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ERRATA

- p 14, last para, 5th line: "take into account the influence" for "take into the influence"
- p 17, last para, 2nd line: "they achieve" for "they achieves"
- p 20, 1st para, 7th line: "Some diagnostic tools for interpreting results" for "Some results diagnostic tools"
- p 40, 2nd para, 1st line: "during eight years before and four years after" for "during years before and after"
- p 40, 3rd para, 9th line: "naïve" for "naiive"
- p 44, 4th para, 8th line: "operation" for "operations"

**STATISTICAL EVALUATION OF ROAD TRAUMA
COUNTERMEASURES**

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3. M.H. Cameron, "The effect of seat belts on minor and severe injuries measured on the Abbreviated Injury Scale".
4. M.H. Cameron and P.M. Strang, "Effect of intensified random breath testing in Melbourne during 1978 and 1979".
5. M.O. Haque and M.H. Cameron, "Effect of the Victorian Zero BAC legislation on serious casualty accidents : July 1984 - December 1985".
6. A.P. Vulcan, M.H. Cameron and W.L. Watson, "Mandatory bicycle helmet use: experience in Victoria, Australia".
7. M.H. Cameron, T. Mach, D. Neiger, A. Graham, R. Ramsay, M. Pappas and J. Haley, "Vehicle crashworthiness ratings in Australia".
8. M.H. Cameron, T. Mach, D. Neiger, A. Graham, R. Ramsay, M. Pappas and J. Haley, "Vehicle crashworthiness ratings from Victoria and New South Wales crash data".
9. M.H. Cameron, A. Cavallo and G. Sullivan, "Evaluation of the random breath testing initiative in Victoria 1989-1991: Multivariate time series approach".
10. A.P. Vulcan, M.H. Cameron and L. Heiman, "Evaluation of mandatory bicycle helmet use in Victoria, Australia".
11. T. Thoresen, M.H. Cameron, T. Fry and L. Heiman, "Estimating the relative influences of accident countermeasures and socio-economic factors on the Victorian road toll 1985-1990".
12. M.H. Cameron, A. Cavallo and A. Gilbert, "Crash-based evaluation of the speed camera program in Victoria 1990-1991. Phase 1: General effects. Phase 2: Effects of program mechanisms".
13. M.H. Cameron and S.V. Newstead, "Evaluation of mass media publicity as support for enforcement".
14. M.H. Cameron, A.P. Vulcan, C.F. Finch and S.V. Newstead, "Mandatory bicycle helmet use following a decade of helmet promotion in Victoria, Australia - An evaluation".
15. M.H. Cameron, S.V. Newstead and A.P. Vulcan, "Analysis of reductions in Victorian road casualties, 1989 to 1992".
16. M. H. Cameron, "Economic analysis - An essential tool".
17. M.H. Cameron, C.F. Finch, S.V. Newstead, T. Le, A. Graham, M. Griffiths, M. Pappas and J. Haley, "Measuring crashworthiness: Make/model ratings and the influence of Australian Design Rules for motor vehicle safety".
18. M.H. Cameron, K. Diamantopoulou, N. Mullan and S. Gantzer, "Evaluation of the country random breath testing and publicity program in Victoria 1993-1994".
19. M.H. Cameron, S.V. Newstead and C.M. Le, "Rating the aggressivity of Australian passenger vehicles towards other vehicle occupants and unprotected road users".
20. K. Diamantopoulou and M.H. Cameron, "Localised effects on crashes of the country random breath testing and publicity program in Victoria".

Summary

This thesis is based on twenty papers which have the common theme of statistical evaluation of road trauma countermeasures. The papers represent the development of the candidate's ideas in this field over three decades. The ultimate aim has been to develop analysis methods to reach definitive conclusions about the effectiveness of countermeasures to road trauma.

As required, additional material is given in the thesis to show how the papers are linked thematically. The structure of the thesis is as follows: identification of the papers, presentation of an evaluation framework, discussion of the analysis methods in the papers, concluding remarks, statements from co-authors, summaries of the papers, and finally, the papers themselves.

Initially in the evaluation framework, the nature of road trauma countermeasures is defined in terms of the chain of events leading to traumatic events on the road. Countermeasures aim to break the chain before a traumatic event occurs, or at least to reduce the risk of one or more steps occurring. Impact evaluation is an assessment of whether these risks have been reduced and whether the resulting road trauma has fallen. The papers in this thesis focus on impact evaluation.

The emphasis of the framework is on evaluation designs to reach reliable conclusions, and in particular on the strengths and threats to the validity of each design. The most critical threats to the validity of evaluations of road trauma countermeasures are considered to be:

1. "History" (other causes operating at the same time)
2. "Maturation" (trends in the impact criterion over time)
3. Regression artefacts (over-estimation of the mean value of the impact criterion due to chance occurrence of high values of the criterion in earlier periods)
4. Instability in the impact criterion.

The framework outlines the methods which have been developed and applied in the papers to overcome the threats of history and maturation. The threat of regression artefacts was not considered to be relevant in any of the evaluations in the papers. Instability in the evaluation criteria was considered to be due to the inherent chance variation in road trauma and appropriate statistical techniques were used to take this threat into account.

The evaluation framework is further developed by categorising the impact evaluation methods. The categories are defined by the method used to estimate the expected level of the impact criterion had the countermeasure not existed, and by the method used to adjust the criterion for the influence of other factors. To complete the framework, the thesis lists a number of general and specific assumptions which must be made if the evaluation method in each category is used.

The analysis methods used in the twenty papers are classified into the categories which were defined in the evaluation framework. The actual applications of the method in each category are then summarised, with emphasis on the limitations and on the threats to validity which each method aimed to overcome or to which each is susceptible. The candidate aimed to use the evaluation framework developed in this thesis to clarify the principles and assumptions behind each suggested method of impact evaluation.

Preface

This thesis has been prepared for submission as a staff candidate for the degree of Doctor of Philosophy from Monash University. The thesis is based on the candidate's previously published and unpublished work which is linked thematically. Staff candidature at Monash University is primarily intended for those who wish to prepare a thesis in this way.

The common theme of the papers in this thesis is the statistical evaluation of road trauma countermeasures. The candidate has been involved in research in this field for more than three decades. The papers represent the on-going development of the candidate's ideas in the field with the aim of reaching more definitive conclusions about the effectiveness of countermeasures.

This thesis was prepared while the candidate was a member of staff of the Accident Research Centre at Monash University. Most of the papers were prepared in that role, in many cases in co-authorship with other staff members. The extent to which the candidate claims the intellectual development of the theoretical concepts in each paper is detailed in the thesis. On average, the candidate claims to be responsible for 80% of the intellectual content of the papers. Nevertheless, the candidate is greatly indebted to his co-authors, where relevant, in helping bring each paper to fruition.

I declare that:

- this thesis contains no material which has been accepted for the award of any other degree or diploma in any university or other institution
- to the best of my knowledge, the thesis contains no material previously published or written by another person, except where due reference is made in the text of the thesis
- where the work in the thesis is based on joint research or publications, the relative contribution of the candidate and the other authors was as disclosed in the previous paragraph and in Appendix A of the thesis.

Finally, I wish to thank my wife Sonya most sincerely for continually encouraging me to commence and complete this thesis while I was pressured by many other responsibilities.



Maxwell Hugh Cameron
February 2000

STATISTICAL EVALUATION OF ROAD TRAUMA COUNTERMEASURES

The Papers

The thesis is based on the candidate's previously published and unpublished work in the field of statistical evaluation of road trauma countermeasures. The papers represent the on-going development of the candidate's ideas in the field with the aim of reaching more definitive conclusions about the effectiveness of countermeasures.

Identification

Twenty papers have been identified which adequately represent the development of the candidate's ideas and which provide the basis for a framework for conclusive evaluation of road trauma countermeasures. Sixteen of the papers have been accepted for publication in peer review journals or peer review conference proceedings. Two further papers were invited to be presented at major conferences, without a requirement for peer review, and were published in conference proceedings. The remainder are two Monash University Accident Research Centre (MUARC) reports which include relevant evaluation research which has not been published in full elsewhere. These reports were reviewed by the Centre's Director and sponsor representatives before publication.

The twenty papers submitted as part of this thesis are listed in chronological order below (the titles of peer review journals and conference proceedings are underlined):

1. M.H. Cameron, "Accident rate analysis and confidence limits". Proceedings, Fifth Conference, Australian Road Research Board, Canberra, August 1970.
2. J.E. Cowley and M.H. Cameron, "Prediction of motor vehicle occupant fatality trends following seat belt wearing legislation". Proceedings, Eighth Conference, Australian Road Research Board, Perth 1976.
3. M.H. Cameron, "The effect of seat belts on minor and severe injuries measured on the Abbreviated Injury Scale". Proceedings, Eighth International Conference of the International Association for Accident and Traffic Medicine, Aarhus, Denmark, June 1980. Also in Accident Analysis and Prevention, Vol. 13, no. 1, March 1981.
4. M.H. Cameron and P.M. Strang, "Effect of intensified random breath testing in Melbourne during 1978 and 1979". Proceedings, Eleventh Conference, Australian Road Research Board, August 1982.
5. M.O. Haque and M.H. Cameron, "Effect of the Victorian Zero BAC legislation on serious casualty accidents : July 1984 - December 1985". Journal of Safety Research, Vol. 20, No. 3, Fall 1989.

6. A.P. Vulcan, M.H. Cameron and W.L. Watson, "Mandatory bicycle helmet use: experience in Victoria, Australia". Special issue on "Road Trauma and its Prevention", World Journal of Surgery, No. 16, pp. 389-397, 1992.
7. M.H. Cameron, T. Mach, D. Neiger, A. Graham, R. Ramsay, M. Pappas and J. Haley, "Vehicle crashworthiness ratings in Australia". Proceedings, International IRCOBI Conference on the Biomechanics of Impacts, Verona, Italy, September 1992. Also in: Accident Analysis and Prevention, Vol. 26, No. 4, 1994.
8. M.H. Cameron, T. Mach, D. Neiger, A. Graham, R. Ramsay, M. Pappas and J. Haley, "Vehicle crashworthiness ratings from Victoria and New South Wales crash data". Road and Transport Research, Vol. 1, No. 3, September 1992.
9. M.H. Cameron, A. Cavallo and G. Sullivan, "Evaluation of the random breath testing initiative in Victoria 1989-1991: Multivariate time series approach". Report No. 38, Monash University Accident Research Centre, ISBN 0 7326 0038 3, September 1992.
10. A.P. Vulcan, M.H. Cameron and L. Heiman, "Evaluation of mandatory bicycle helmet use in Victoria, Australia". Proceedings, 36th Annual Conference of the Association for the Advancement of Automotive Medicine, Portland, Oregon, October 1992.
11. T. Thoresen, M.H. Cameron, T. Fry and L. Heiman, "Estimating the relative influences of accident countermeasures and socio-economic factors on the Victorian road toll 1985-1990". Proceedings, 16th Australian Road Research Board Conference, Perth, November 1992.
12. M.H. Cameron, A. Cavallo and A. Gilbert, "Crash-based evaluation of the speed camera program in Victoria 1990-1991. Phase 1: General effects. Phase 2: Effects of program mechanisms". Report No. 42, Monash University Accident Research Centre, ISBN 0 7326 0042 1, December 1992.
13. M.H. Cameron and S.V. Newstead, "Evaluation of mass media publicity as support for enforcement". Invited paper in: Proceedings, Australasian Drink-Drive Conference, Melbourne, November 1993.
14. M.H. Cameron, A.P. Vulcan, C.F. Finch and S.V. Newstead, "Mandatory bicycle helmet use following a decade of helmet promotion in Victoria, Australia - An evaluation". Accident Analysis and Prevention, Vol. 26, No. 3, 1994.
15. M.H. Cameron, S.V. Newstead and A.P. Vulcan, "Analysis of reductions in Victorian road casualties, 1989 to 1992". Proceedings, 17th Australian Road Research Board Conference, Gold Coast, August 1994.
16. M. H. Cameron, "Economic analysis - An essential tool". Invited paper in: Proceedings, First National Conference on Injury Prevention and Control, Sydney, February 1995.
17. M.H. Cameron, C.F. Finch, S.V. Newstead, T. Le, A. Graham, M. Griffiths, M. Pappas and J. Haley, "Measuring crashworthiness: Make/model ratings and the influence of

- Australian Design Rules for motor vehicle safety". Proceedings, International IRCOBI Conference on the Biomechanics of Impact, Brunnen, Switzerland, September 1995.

18. M.H. Cameron, K. Diamantopoulou, N. Mullan and S. Gantzer, "Evaluation of the country random breath testing and publicity program in Victoria 1993-1994". Proceedings, ROADS 96 Conference, Christchurch, New Zealand, September 1996.
19. M.H. Cameron, S.V. Newstead and C.M. Le, "Rating the aggressivity of Australian passenger vehicles towards other vehicle occupants and unprotected road users". Proceedings, International IRCOBI Conference on the Biomechanics of Impact, Gothenborg, Sweden, September 1998. Also in: Journal of Crash Prevention and Injury Control, Vol. 1, No. 2, 1999.
20. K. Diamantopoulou and M.H. Cameron, "Localised effects on crashes of the country random breath testing and publicity program in Victoria". Proceedings, 19th ARRB Transport Research Conference, Investing in Transport, Sydney, December 1998.

The papers were chosen from the 90 published papers and 120 major reports which the candidate has written or co-authored during a period spanning more than three decades in the field of research and evaluation of road trauma countermeasures. In choosing the papers for submission, preference was given for those which had been subject to peer review before publication. A number of papers were also based on a technical report which itself had been reviewed by the MUARC Director (Professor Peter Vulcan) or a specialist external reviewer.

Fifteen of the twenty papers have been completed since the candidate became a member of the MUARC staff in May 1990. This satisfies the Monash University requirement that at least 75% of the thesis research work must have been completed while the candidate is a member of staff. The other five papers complement these fifteen to fully represent the development of the candidate's ideas.

Authorship

All but three of the papers were written in conjunction with co-authors. The thesis of a staff candidate may contain joint or multi-authored papers, provided that the papers are prefaced with a statement signed by the authors disclosing their respective contributions.

The papers submitted as part of this thesis are prefaced by signed letters providing the co-author's estimate of the candidate's proportional contribution to the intellectual development of the theoretical concepts on which each paper was based (Appendix A). While some co-authors have not been contacted or contactable, in every case a letter has been provided by at least one senior co-author of the paper.

The co-authors' estimates of the candidate's proportional contribution to the intellectual content of the evaluation research in each paper ranged from 50% to 95%, and was 100% in the three cases of sole authorship. The average proportion over all twenty papers was 80%. Thus the submitted papers could be considered equivalent to sixteen papers prepared solely by the candidate, at least so far as intellectual content is concerned.

What the papers represent

The common theme of the papers in this thesis is the statistical evaluation of road trauma countermeasures. The papers represent the on-going development of the candidate's ideas in the field. The subjects covered by the papers include seat belt wearing, random breath testing, speed cameras, zero blood alcohol laws for novice drivers, bicycle helmet wearing, and vehicle crashworthiness and aggressivity towards other road users.

Some of the analysis designs used in earlier work could be considered to be deficient by modern standards, but were considered acceptable by the candidate and his peers at the time. The lessons learnt from critiques of the earlier work were important for developing a framework for conclusive evaluation of road trauma countermeasures. The framework reveals that, under certain assumptions, the earlier analysis designs are adequate and can be applied with confidence if the assumptions can be considered true.

While statistical methods have been used to analyse road trauma data in the papers, the emphasis of this thesis is on appropriate evaluation designs, their strengths and the threats to their validity. In many situations, a range of statistical methods can be used, but the more critical issues for the conclusiveness of the evaluation are usually those related to the analysis design. The candidate considers that this is the appropriate emphasis in this field.

Evaluation Framework

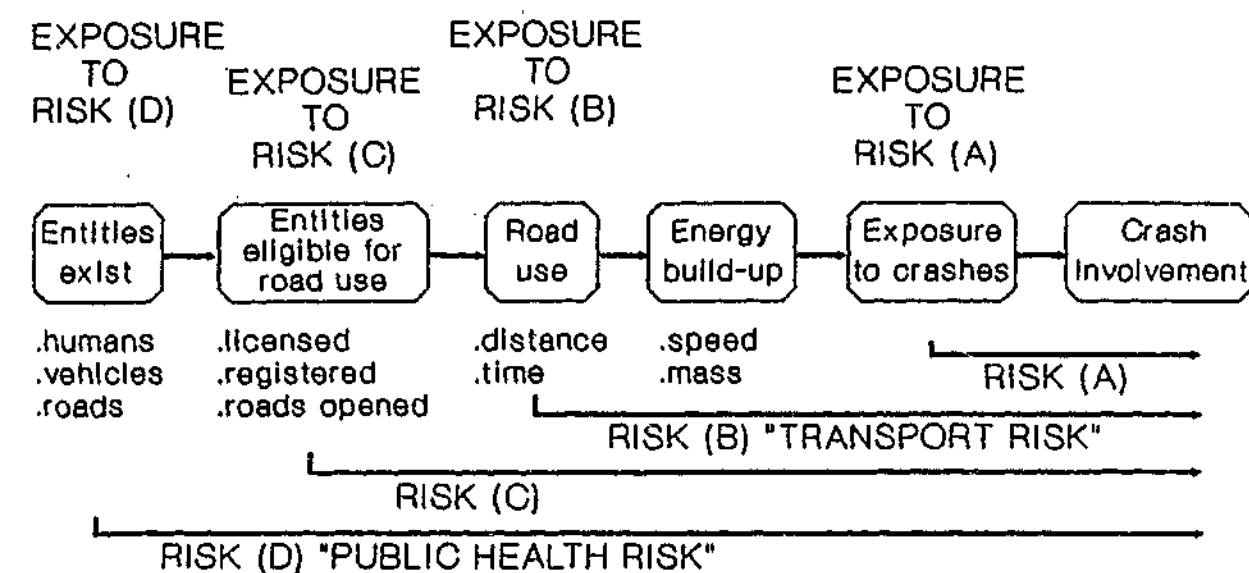
The nature of road trauma countermeasures

A road trauma countermeasure is a measure which aims to break the chain of events before a traumatic event occurs on the road (ie. personal injury or death). A schematic representation of these events is illustrated by the "road trauma chain" (Cameron 1991; see also Papers 7 and 8), shown in Figure 1. All but one of the papers are focused on measures associated with personal injury or death, or crashes resulting in these outcomes. There are also road safety measures aimed at preventing crash involvement, and the methods described in this thesis are relevant to their evaluation as well.

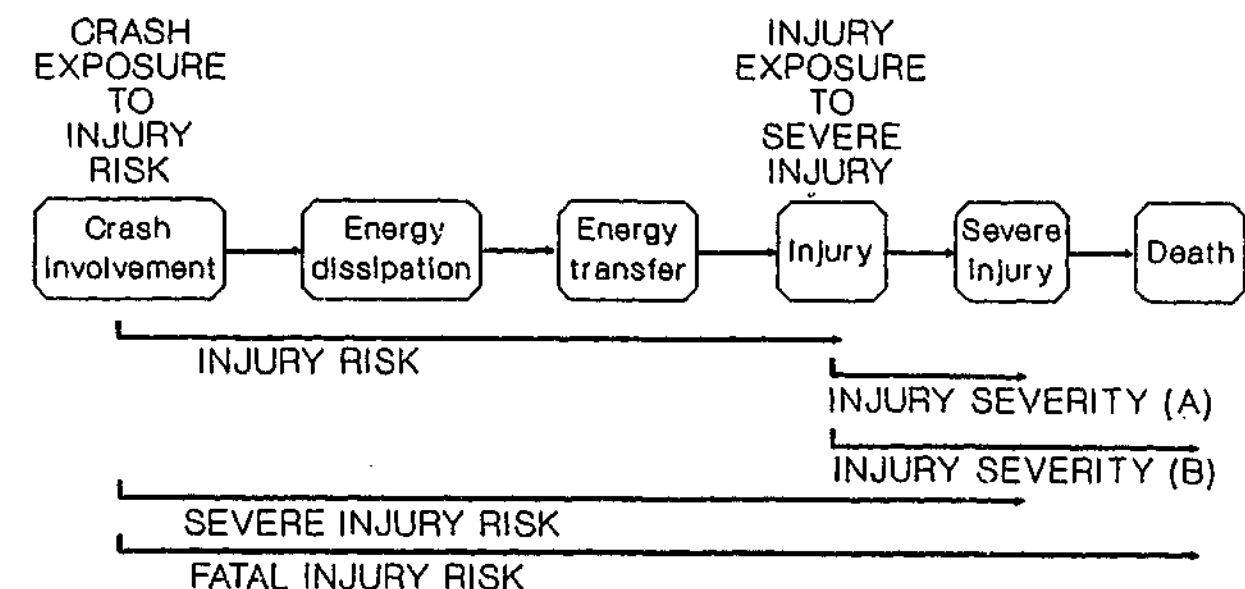
Associated with each event in the chain is a probability (or "risk") that the next step or steps will occur, leading on to later events in the chain. Evaluation of road trauma countermeasures is often based, explicitly or implicitly, on assessing whether these probabilities have changed. A number of important risks (some spanning more than one event) commonly estimated in practice are shown below the chain of events. In practice most countermeasures, because of lack of full coverage or because they can only ever be partially effective, are aimed at reducing one or more risks, rather than totally eliminating risk.

Figure 1:

THE ROAD TRAUMA CHAIN (1. PRE-CRASH)



THE ROAD TRAUMA CHAIN (continued) (2. CRASH and POST-CRASH)



An important concept underlying the estimation of each risk is the "exposure" to that risk, ie. the number and type of events occurring, to which the risk is applied (Cameron and Oxley 1995). As may be expected, there is frequently good data on the extent of occurrence of the undesirable and traumatic outcomes (crashes, injuries and deaths), but data on pre-crash exposure events is less commonly available (apart from data on the human, vehicle and road "populations"). An evaluation based solely on outcome frequencies, in the absence of complementary exposure data, may result in false conclusions about the effect of the

countermeasure on the risk at which it is aimed. It may not be clear whether the countermeasure has reduced risk or whether exposure has been reduced (this situation may be acceptable if the countermeasure was also aimed at exposure, but a poorer understanding would result from the evaluation).

Evaluation processes

The evaluation of road trauma countermeasure programs can be approached from two points of view (Cameron 1994). *Process evaluation* aims to measure whether the program was implemented as planned and covers issues like how, when, where and the quality of the implementation. *Impact evaluation* (or outcome evaluation) aims to assess whether the program (as implemented) was actually effective in having an impact on the road trauma it intended to reduce.

The focus of this thesis is on impact evaluation and methods to achieve valid conclusions in this area. It is not the intention to cover methods for process evaluation, but some good examples of evaluation research in this area are given in Papers 6, 9, 12, 14, and 18.

Impact evaluation criteria

Criteria for use in impact evaluations generally fall in one of three groups (Drummond 1992):

1. **Ultimate criteria.** The injury incidence or severity of the specific target group which is the aim of the road trauma countermeasure (eg. deaths and injuries from road crashes involving illegal blood alcohol levels, in the case of a drink-driving prevention program).
2. **Surrogate criteria.** A substitute group of injuries which may be a sub-set or super-set of the ultimate criterion (if this is unavailable) and/or evidence exists of a strong relationship between the two (eg. crashes during "high alcohol hours" of the week, which have been found from previous research to be those periods when the highest proportions of seriously injured drivers had illegal blood alcohol levels).
3. **Intermediate criteria.** Measures of behaviour, attitudes or knowledge (in that order of preference) for which there is prior evidence of a link with the incidence of the ultimate criterion (eg. number of drivers on the road with illegal blood alcohol levels, measured from random road-side surveys). The strength of this prior evidence needs to be reviewed before accepting an intermediate criterion.

All of the papers in this thesis have used ultimate or surrogate criteria. Some have also used intermediate criteria (eg. seat belt and bicycle helmet wearing rates in Papers 2 and 14, respectively), but these criteria have been used to explain variations in the ultimate criteria rather than used as stand-alone measures. An example of research in which intermediate criteria have played a parallel role with surrogate criteria, and assisted cause-and-effect conclusions, is given in Cameron and Vulcan (1998).

The papers in this thesis have analysed ultimate and surrogate criteria to evaluate the impacts of countermeasures on pre-crash risks and post-crash risks (Figure 1). The estimates of pre-

crash risks are usually based on the frequency of crash involvement divided by an estimate of pre-crash exposure (eg. the size of the population exposed to risk, or the amount of road use by the population). In a number of studies involving comparisons over time, it has been necessary to assume that the pre-crash exposure has remained relatively constant when making inferences about pre-crash risks from analysis of crash involvement frequencies.

The estimates of post-crash risks have been based on the frequency of injury or death divided by the number of crashes. As indicated in Figure 1, crash frequencies represent the exposure to injury so far as post-crash risks are concerned. It has been convenient in some studies to conceive of injury as representing the exposure to severe injury or death. This has been useful in providing estimates of the risk of serious injury in a crash via a two step process: (i) risk of injury in a crash, multiplied by (ii) risk of serious injury for injured persons (see Papers 7,8, 17 and 19).

Hypotheses for test in an impact evaluation

In scientific terms, an impact evaluation is a formal test of the hypothesis that the road trauma countermeasure did not change the ultimate criterion, versus the alternative hypothesis that there was a change (with a reduction being the change direction of most interest). In practice, it is common to measure the impact on:

- sub-sets of the ultimate criterion (if this is available) as well as this criterion in total, or
- on different types of surrogate and intermediate criteria, in total and within sub-sets.

For example, a state-wide drink-driving program may be evaluated in terms of its impact on crashes in the major city, in the provincial cities and towns, and on the rural highways separately. Each of these represents a test of a specific hypothesis.

When a large number of hypotheses is tested it can become confusing regarding their relationship to each other and their relative importance to testing the principal hypothesis. In addition, it is possible that some of the tests will appear to support the hypothesis that the program is effective, due entirely to chance among the large number of tests.

To minimise these problems, an important discipline in impact evaluation is to make formal statements of the *general* and *specific* hypotheses regarding the effect of the program on each of the ultimate, surrogate and intermediate criteria (as available). The general hypothesis may relate to the effect of the program overall, whereas the specific hypotheses would relate to sub-sets of the criteria (eg. region, time of week, age group). The overlap of the hypotheses and the implications of their test results for the overall conclusion should be obvious from the formal structure of the hypotheses, provided they are stated explicitly.

Evaluation by comparison in time or space

Most impact evaluations of road trauma countermeasures are based on comparisons of the injury criteria across time. Periods before the countermeasure was introduced (or changed substantially) are compared with periods after the change. (The relevance of data from the before period to measurement of the countermeasure impact will be discussed below.)

In some situations, the countermeasure has not been newly introduced and exists in a variety of forms, and/or appropriate data does not exist prior to the introduction of the countermeasure. In this case, the impact evaluation may be based on comparisons of the criterion across space, where space may be different types of road users, vehicles and road sections. Of course, there would be a need to ensure that such comparisons provide a valid measure of the (differences in) impact, but in fact this is no less of an issue for the time-based comparisons (see discussion below).

The papers in this thesis cover impact evaluations which make either *time-related* comparisons or *cross-section* comparisons in space. A number of the studies have made comparisons in both time and space, but the motive of the space comparisons has been to represent the influence of other factors, not to measure any differential impacts of the countermeasure in space. It is conceivable and feasible to measure a range of impacts across space in a single analysis, but this has not been attempted in any of the papers.

Measuring the impact

The impact of the road trauma countermeasure on a chosen criterion is measured by the difference between:

- (a) the level of that criterion (eg. injuries to the target group) which actually occurred, when the countermeasure was present, and
- (b) the level which was expected to occur, if there had been no countermeasure.

Determining the second of these components is the major challenge because it is not actually observable among the target group. (As commented earlier, there is frequently good data on the actual occurrence of injuries, thus providing the first component.) The second component can only be estimated from comparisons across time or space, but a number of issues arise when this is done.

In time-related evaluations, time has passed during and since the implementation of the countermeasure, and the injury level of the target group may have changed due to a number of other, perhaps unknown, factors independent of the presence of the measure. In cross-section evaluations, the comparison group (road users, vehicles or road sections) is not the same as the target group, and its injury level may be influenced by many factors.

Reaching valid conclusions about the impact of the countermeasure on the criterion requires attention to be given to a range of possible influences on it, apart from the countermeasure's effect. These influences represent threats to the validity of the analysis comparisons, and will be discussed in turn in the following section.

Threats to the validity of the evaluation

There can be threats to both the external and internal validity of an evaluation of a road trauma countermeasure (Cook and Campbell 1979; Council, Reinfurt, Campbell et al 1980).

External validity relates to the ability to generalise from the results of the evaluation to the population in general. This ability may be compromised if the study is based on a sample of the population (eg. road sites) and attention has not been given to whether the sample is representative. An evaluation based on criteria measured for the whole population in a jurisdiction is relatively immune from questions of external validity. Most of the countermeasures evaluated in the papers in this thesis were assessed using broad criteria without sampling, so questions of external validity did not arise.

Internal validity concerns the ability of the evaluation to provide an estimated impact which measures the effect of the countermeasure and not anything else. To a large extent, internal validity is related to the validity of the estimate of the level of the criterion which was expected to occur, if the countermeasure had not operated. The papers in this thesis represent a range of methods aimed at providing such estimates. The validity of the estimates depends on the validity of the assumptions underlying each method.

Cook and Campbell (1979) have defined thirteen general types of rival explanations for the estimated apparent impact of a countermeasure not being (entirely) due to the countermeasure. Council et al (1980) considered that four of these were the most critical threats to the internal validity of evaluations of road trauma countermeasures. These threats are:

1. "History" (other causes operating at the same time)

This threat arises from the possibility that specific causes other than the countermeasure being evaluated resulted in all or part of any observed difference between the actual (a) and expected (b) levels of road trauma [where (a) and (b) are the components of the measured impact, defined above]. Such causes may be known or unknown, and the known causes may or may not be measured in ways which enables their effects to be taken into account explicitly; this of course would not be possible with unknown causes.

2. "Maturation" (trends in the criterion over time)

In time-related evaluations, the criterion variable may be "maturing" with time for a variety of reasons which has created a time trend before the countermeasure was implemented. It may be reasonable to expect that such a trend would have continued in the absence of the countermeasure. Failure to account for a trend would represent a threat to the validity of the expected level (b) of road trauma.

3. Regression artefacts

This threat arises if specific units of the road transport system (humans, vehicles or road sections) are chosen for application of the countermeasure on the basis of recent high levels of road trauma associated with the specific units. Since at least part of road trauma occurrence is due to chance processes, it would be expected that the criterion variable would be closer to the mean value of its chance process during subsequent periods (ie. "regression to mean") than during the period from which the specific units were chosen for treatment. Failure to take this phenomenon into account in time-related evaluations would result in an overestimate of the expected level (b) of the criterion variable.

4. Instability in the criterion

This threat arises if the criterion variable has uncertainty in its measurement of the phenomenon which the countermeasure aims to reduce. A difference between the actual level (a) and a validly estimated expected level (b) of road trauma may not mean that the countermeasure has been effective in reducing risk if there is uncertainty in (a). The major source of uncertainty in (a) in impact evaluations based on ultimate or surrogate criteria is chance variation. Failure to take this into account can result in invalid conclusions about the meaning of differences between (a) and (b).

The following sections discuss general methods to overcome three of these four threats to the internal validity of evaluations of road trauma countermeasures. Specific methods to address each threat, alone and together, have been developed for the evaluations described in the papers in this thesis. However, regression artefacts as a threat did not arise in any of the evaluations in the papers. It is possible to address a regression artefact through some methods aimed at the "history" threat to validity, and there is also a set of specific statistical methods which provide adjustments for the expected level (b) for the "regression to mean" effect (Hauer 1980; Abbess, Jarrett and Wright 1981).

Methods for taking into account trends over time ("maturation")

The simplest method to estimate the expected level (b) of road trauma which would have occurred had there been no countermeasure, is to use the observed level in a period of the same duration before the countermeasure was implemented. However, this method relies on the validity of the strong assumption that there was no trend in the criterion variable before the implementation. Data on the criterion variable needs to be gathered and analysed over a sufficiently long period to establish if the assumption is true before this method is used.

The trend can include regular and cyclical components. If the criterion variable is aggregated over the period of the cyclical component, then this component can be ignored (eg. in yearly observations). However, if the countermeasure effects are to be examined on more frequent observations of the criterion (eg. quarterly or monthly), then there will be a need for time series analysis methods which address both components of the trend.

The simplest family of methods for representing a trend in the criterion variable is univariate time series models, where terms in the series are expressed as functions of earlier terms and time itself, ie. the series depends only on itself and time. *Auto-Regressive Integrated Moving Average* (ARIMA) models (Box and Jenkins 1976) are an example of such methods. A model is fitted to data on the criterion variable during the pre-implementation period, and the model is then extrapolated to estimate the expected level (b) during the period after the countermeasure has been implemented.

An extension of this approach is to add to a univariate model a hypothetical term representing the effect of the countermeasure, but with unknown parameter(s) specifying the magnitude of the effect and which are to be estimated from the data. The "intervention" term can have functional forms ranging from a simple step to relatively complex combinations of steps, slopes, and accelerating and decaying effects. The form of the intervention effect is a relatively critical assumption which must be made using this approach. The extended

univariate model is then fitted to data on the criterion variable spanning both the pre- and post-implementation periods. *Intervention Analysis* (Box and Tiao 1975) is such an approach, involving adding an intervention term to an ARIMA model. Due to the increased data thus available to calibrate the model, compared with the simple univariate model fitted to pre-implementation data, the model may be better estimated. The parameter(s) of the intervention term provide an estimate of the impact of the countermeasure (which can be immediate or relatively complex over time, depending on the form of the intervention term).

The estimation of the models may be further improved, and the precision of the estimated expected level or the estimated impact also improved, if there are known measured factors having an influence on the criterion variable which can be included in the model. These *covariates* add to the complexity of the model because it is then not just a function of time and past terms in the series (and perhaps an intervention term), and multivariate methods must be used to fit the model. Multivariate time series models can be fitted either to pre-implementation values of the criterion and the covariates, or to pre- and post-implementation values of these data series plus an intervention term. The relative advantages and assumptions behind these two approaches are the same as those outlined in the previous paragraph with regard to univariate models with and without intervention terms. If the covariates are linked to variations in the criterion variable, then a multivariate model including the covariates may provide a better estimate of the countermeasure impact.

In summary, the methods for taking into account the threat of "maturation" (trends in the criterion over time) include:

- estimating the expected level (b) from the observed level during the pre-implementation period (following prior investigation establishing no trend exists),
- estimating the expected level (b) by extrapolation of a univariate time series model of pre-implementation levels of the criterion,
- estimating the expected level (b) by extrapolation of a multivariate time series model (including covariates) of pre-implementation levels of the criterion, or
- estimating the impact from the magnitude of an intervention term in either a univariate or multivariate time series model representing the pre- and post-implementation levels of the criterion.

Methods for taking into account other factors ("history")

The traditional scientific method for overcoming the threat of "history" is to randomise the assignment of the countermeasure to two groups, and then compare (changes in) the criterion variable between the *treated* group and the other (*control*) group. Because of the random assignment, it is assumed that any influence of other factors, apart from the countermeasure, is (at least approximately) equal for the two groups. This very strong evaluation method is known as an *experimental design*, but is seldom available for evaluations of road trauma countermeasures.

A more practical alternative in social settings like road trauma prevention is to seek a group which is considered to *respond to all other factors in the same way as the target group of the countermeasure*, but is unaffected by the countermeasure being evaluated. Such a group is defined as the *comparison* group and the evaluation is known as a *quasi-* or *pseudo-experimental design*. In practice it is impossible to know whether the comparison group

response to all other factors is the same as the target group, but the chances are maximised by choosing it to be as similar as possible. The evaluator also has an onus to be proactive in checking the assumption, unlike in the classical experimental design where randomisation guarantees that the two groups are the same (within chance variation).

Comparison groups are more relevant for overcoming the threat of "history" in time-related evaluations than in cross-section evaluations. (Comparisons of groups are an integral part of cross-section evaluations, but the group definitions are seldom in the control of the evaluator and other methods must be used to take other factors into account - see below.) If a successful comparison group can be defined, the changes in the criterion variable measured for the comparison group can be considered to measure the changes in the criterion variable for the target group due to all other factors apart from the countermeasure.

The simplest way in which the comparison group data can be used in an evaluation is to use the ratio of the criterion variable, post-implementation to pre-implementation, to adjust the pre-implementation value of the target group data to provide the expected level (b) post-implementation. In a sense, this method discounts the change in the target group data, pre- to post-implementation, for the change in the comparison group data, which in turn is considered to measure the effect of all other factors. The net change in the target group data is then considered to measure the effect of the countermeasure alone.

This philosophy regarding the use of the comparison group data and its separate analysis to discount an initial estimate of the countermeasure impact, based on the target group data, carries over to the other methods of estimating the impact described in the previous section. Thus a univariate or multivariate model could be fitted to the comparison group data during the pre-implementation period (in close parallel to the target group analysis), the fitted model extrapolated into the post-implementation period, and the difference between the actual and extrapolated values for the criterion variable considered to measure the effect of all other factors operating at the same time as the countermeasure. Alternatively, a univariate or multivariate model with an intervention term (with the same form and timing as the target group analysis) could be fitted to the comparison group data spanning both the pre- and post-implementation periods, and the apparent "intervention effect" in this case considered to measure the effect of all other factors. In the case of each alternative, the estimated effect of all other factors is discounted (usually by subtraction) from the initial estimate of the countermeasure impact based on the target group data, to provide a final estimate of the impact after the other factors have been taken into account.

All of the above methods for taking into account "history" (with the exception of the multivariate models incorporating influential covariates) have treated the "other factors" as unknown. Even the multivariate models have assumed that there may be other, unknown or unmeasured factors, apart from the covariates, which represent the threat of history. While the inclusion of covariates was discussed in the previous section in the context of improving the precision of the impact estimates, the explicit inclusion of factors which represent alternative explanations apart from the countermeasure is an obviously desirable attribute of the multivariate approach. However, there are further methods which can capitalise on knowledge of other influential factors to overcome this threat, which depend on whether such factors are measured on categorical or interval scales. These methods are most relevant to cross-section evaluations, for which comparison groups are seldom available to attempt to address the threat of history.

If the known influential variable or variables are categorical in nature, the simplest method to take these factors into account is stratification of the evaluation. In other words, the evaluation-analysis is conducted separately for each unique level of the variable (or variables in combination), forming the strata of the data. This approach results in a number of separate estimates of the impact of the evaluation, in those strata for which there is sufficient data, but potentially no reliable estimate for some strata. Because of the risk of unreliable estimates, there is a disincentive to stratify the evaluation by all of the known influential variables, or at the lowest level of disaggregation available. However, in some circumstances it is possible to overcome the problem of multiple estimates with poor precision by combining them in a way that each stratum is given equal weight (*normalisation*), producing a single estimate whose precision can be calculated from that of the separate estimates (and can be expected to be better than each of them).

An alternative to full stratification (and then normalisation) is to develop a *generalised linear model* (Nelder and Wedderburn 1972) of the criterion variable as a function of the countermeasure impact and the known influential variables simultaneously. Because the influential variables are included with the impact in the model, the estimate of the impact has automatically taken any confounding due to these variables into account. In a generalised linear model the influential variables can be either categorical or interval scaled. *Logistic regression* is a generalised linear modelling procedure which is appropriate when the criterion variable is a proportion bounded by zero and unity (as are many road trauma risk estimates). It has been applied to take into account other influential factors in the countermeasure evaluations described in Papers 12, 14, 17 and 19.

In summary, the methods for taking into account the threat of "history" (other causes operating at the same time) include:

- adjusting the expected level (b) of the target group by the ratio of the post- to pre-implementation levels of the criterion variable measured in the comparison group,
- discounting the initial estimate of countermeasure impact, from univariate or multivariate time series analysis of target group data, by the apparent impact of all other factors estimated by parallel time series analysis of the same type, to provide a final estimate of the impact,
- stratification of the evaluation data and analysis by categories of the known influential variables and, where appropriate, normalisation of the separate impact estimates to provide a single overall estimate, or
- developing a generalised linear model of the criterion variable as a function of the countermeasure impact and the known influential variables simultaneously.

Methods for taking into account chance variation ("instability")

The major source of the threat of instability in ultimate or surrogate criteria used in road trauma countermeasure evaluations is chance variation. This is because road trauma is the outcome of a very large number of traffic events ("exposure"), each with a relatively small probability ("risk") of crash and injury outcome. It can be shown that under the condition of the independence of the outcomes of each exposure event on the risk for subsequent events, the number of crashes has approximately the Poisson statistical distribution with mean "risk" times "exposure" (Feller 1968). It has also been shown that under circumstances in which the

rate of learning (reduction of risk) due to past crashes of an individual is relatively low, the number of crashes has approximately a Poisson distribution (Cameron 1969). Further, if independent individuals have different levels of risk between themselves and at different times, the total number of crashes across all individuals is also Poisson distributed. Thus the Poisson distribution appears to be a relatively robust representation of the statistical distribution of crash numbers.

Apart from this observation, which has been capitalised on in some of the papers in this thesis, the major methods for taking into account chance variation are traditional statistical methods in which the variance of the criterion variable is measured from the data rather than assumed from a theoretical distribution. Many statistical methods assume that the criterion variable has a Normal distribution with constant variance. While the Normal distribution is an adequate approximation to the Poisson distribution when the Poisson mean is high (greater than 100; Feller 1968), the fact remains that the Poisson variance increases with the mean. The square root transformation has been recommended to stabilise the variance of Poisson variables (Kempthorne 1962). Other transformations, such as the logarithm, may achieve similar effects and have other advantages when fitting certain forms of statistical model (eg. fitting a multiplicative model by linear multiple regression after first taking logarithms of the dependent and independent variables).

Crash frequencies may also be considered to be counts of independent events. When cross-tabulated by categorical variables in contingency tables, the chance variation in each frequency can be represented by a range of contingency table analysis techniques (Bishop, Fienberg and Holland 1975; Christensen 1990). *Log-linear categorical models* fitted using the GLIM procedure are a recent powerful development in this area (Aitkin et al 1990).

When the criterion variable is a proportion (such as a risk estimate), it is important to recognise that the data on which it is based constrains it to lie between zero and unity, and not to use a statistical method which assumes otherwise (such as the Normal distribution, because in theory a Normal variable is unbounded in either direction). *Logistic regression* is an appropriate statistical method for developing models for criterion variables of this type which reflect their bounded variation.

Categorisation of impact evaluation methods

Impact evaluation methods can be classified according to their approach to estimating the expected level of the criterion variable in the absence of the countermeasure (and overcoming the threat of "maturation" in time-related evaluations), and their approach to taking into account the influence of other factors apart from the countermeasure (and overcoming the threat of "history"). Table 1 is a categorisation of two general methods for cross-section evaluations and ten general methods for time-related evaluations.

Methods 0A and 1A are conceptually the simplest evaluation designs, where the injury level of the countermeasure target group is compared, respectively, with that of a comparison group (adjusted by the relative exposure) or with that of the target group during the pre-implementation period. Cross-section evaluation methods which fall in category 0B explicitly take into the influence of known other factors by using either categorisation (stratification) or covariate analysis.

Table 1: General methods for impact evaluation of road trauma countermeasures

Method of estimating level expected to have occurred	Extension to adjust for "other factors" using comparison group
Cross-section comparisons	
0A. Injury level of comparison group (adjusted by relative exposure of target and comparison group)	0B. As 0A, adjusted for known other factors by either: <ul style="list-style-type: none"> • (0B+) categorisation, or • (0B++) covariate analysis
Time-related comparisons	
1A. Injury level of target group during the pre-implementation period	1B. As 1A, multiplied by ratio of post- to pre-implementation period injury levels of the comparison group
2A. Extrapolation of a univariate time series model of pre-implementation injury levels	2B. As 2A, adjusted for the change in injury level of the comparison group compared with an extrapolation of its past injury levels using a univariate time series model
3A. Magnitude of an intervention term* in an (otherwise) univariate time series model representing the pre- and post-implementation injury levels	3B. As 3A, adjusting the intervention term by a term measured in the same way from an (otherwise) univariate time series model of the injury levels of the comparison group
4A. Extrapolation of a multivariate time series model (including covariates) of pre-implementation injury levels	4B. As 4A, adjusted for the change in injury level of the comparison group compared with an extrapolation of its past injury levels using a multivariate time series model
5A. Magnitude of an intervention term* in a multivariate time series model (including covariates) representing the pre- and post-implementation injury levels	5B. As 5A, adjusting the intervention term by a term measured in the same way from a multivariate time series model of the injury levels of the comparison group

* Coefficient of a dummy variable representing the countermeasure intervention, or coefficient of time-related measure of countermeasure operation (the latter not being applicable to the intervention term of the comparison group in methods 3B and 5B)

Methods 2A and 3A extend method 1A by using univariate time series models to overcome the threat of "maturation". Methods 4A and 5A further extend these two methods by using multivariate time series models, incorporating known and measured influential factors as covariates, to overcome the threat of "history" (from these factors) as well as the threat of maturation. Methods 3A and 5A are subtle variations on their immediate predecessors, making use of an intervention term to measure the impact of the countermeasure rather than aiming to estimate the expected level in the absence of the countermeasure.

Methods 1B to 5B further extend the time-related methods, 1A to 5A, by adding parallel analysis of a comparison group to overcome the threat of "history" from unknown influential

factors. It is usual, but not necessary, for the analysis of the comparison group to mirror that of the target group so that the results of the former can be discounted from the latter.

Table 1 does not categorise the methods by approaches which take into account chance variation in ultimate or surrogate criteria (aiming to overcome the threat of "instability"). A range of appropriate methods exist, as illustrated in the papers in this thesis. There has been rapid development of statistical methods for this purpose, especially in the area of time series analysis. The emphasis of this thesis is on appropriate evaluation designs and their strengths relative to the threats of "maturation" and "history".

In Table 1, the definition of an intervention term has been extended to include the coefficient of a time-related measure of the countermeasure operations (ie. a measure of the amount of activity taking place under the banner of the countermeasure, assuming it varies, in each period - including zero values during the pre-implementation phase in the case of new countermeasures). This extension of the intervention term beyond relatively simple step and slope functions, when appropriate for the countermeasure, offers a potentially more powerful model for linking the countermeasure with variations in the criterion variable.

Table 1 includes a relatively rare family of methods (types 4B and 5B) which combine two methods to take into account the threat of "history" in the one evaluation analysis. The multivariate time series method is aimed at including those influential variables which are known and measured, and which potentially represent alternative explanations for changes in the criterion variable apart from the countermeasure. The use of a comparison group whose criterion variable data is analysed in the same multivariate way as the target group (including the same covariates) is aimed at measuring the effect of all other factors, additional to the covariates, and apart from the countermeasure. Because the two methods complement each other, the second aimed at covering the influential factors not covered by the first, there is less onus on the multivariate models needing to include all important influential factors. This contrasts with methods 4A and 5B where the need to cover all important factors is critical.

Assumptions implicit in the use of each method

There are a number of general assumptions which apply to all of the methods defined in Table 1. These include whether the criterion variable has been appropriately chosen as the focus of the countermeasure (especially in the case of surrogate criteria when ultimate criteria are not available) and whether the target group is correct, exhaustive of all countermeasure influences, and inclusive of groups for which there may be unexpected effects.

Another general assumption implicit usually in time-related evaluations is that there are no discontinuities in the exposure trend over the period being analysed. Most time-related evaluations use crash or injury frequencies as the criterion variable, whereas the implicit hypothesis (usually not stated) being tested is that the countermeasure has or has not reduced the risk of crash or injury per unit exposure. This occurs in many cases because reliable data on exposure trends, for use in conjunction with the frequencies to estimate risk trends, is usually not available. However it is an important assumption which, where possible, should be given careful attention. In most cross-section evaluations, where differences in exposure between the groups being compared are to be expected, it is usual to avoid such an assumption by taking exposure differences explicitly into account in the analysis.

Table 2 lists the specific assumptions which are implicit in the use of each of the individual methods defined in Table 1. In general, the more complex the method, the more prescriptive the assumptions which need to be made. In some cases the assumptions are desirable in order to increase the statistical power of the method and thus make the estimated impacts on the criterion variable more reliable. In other cases the assumptions are essential to overcome an evaluation design difficulty, without which the evaluation could not proceed. During the discussion of each paper submitted as part of this thesis, the key assumptions made in employing the evaluation method will be highlighted together with efforts made to establish the validity of each assumption where possible.

Economic analysis of measured impacts

The previous sections have outlined methods for evaluating the impact of a countermeasure on ultimate or surrogate criteria, the threats to validity of such evaluations, methods for overcoming those threats, and the assumptions implicit in the use of each method. However, this framework for the candidate's papers would be remiss if it did not point out that some evaluation studies do not finish with measuring the impact of the countermeasure on road trauma.

While reduction in road trauma is an absolute goal in itself, all countermeasure programs have their costs. In environments with finite resources for investment in road safety programs, evaluators are often asked whether the program's benefits justify its costs. Decision-makers faced with alternative investments in effective road safety programs often ask whether the excess of the benefits over costs for one program is greater than for another.

These questions put emphasis on the economic analysis of the benefits and costs of countermeasures for which impact evaluation has shown that they achieve road trauma reductions. Paper 16 outlines methods for doing this in a road safety context, so they will not be repeated here. The paper also illustrates how the impact evaluation results of another paper (Paper 15) can be extended to complete the economic analysis of benefits and costs.

Table 2: Implicit assumptions for each method

Method of estimating level expected to have occurred	Extension to adjust for "other factors" using comparison group
<i>Cross-section comparisons</i>	
0A <ul style="list-style-type: none"> Exposure of target and comparison groups has been measured appropriately and accurately 	0B+ (categorisation) <ul style="list-style-type: none"> Stratifying variables must be categorical Extent of stratification is adequate 0B++ (covariate analysis) <ul style="list-style-type: none"> Functional form of model of countermeasure impact and covariates is correct Available covariates cover all important influential variables
<i>Time-related comparisons</i>	
1A <ul style="list-style-type: none"> No trend in criterion variable for target group before implementation 	1B <ul style="list-style-type: none"> No trend in criterion variable for comparison group Change in comparison group measures effect of all other factors
2A <ul style="list-style-type: none"> Functional form of univariate time series model is correct No time-related influential factors 	2B <ul style="list-style-type: none"> Functional form of univariate time series model for comparison group is correct No time-related influential factors
3A <ul style="list-style-type: none"> As 2A Functional form of intervention term is correct 	3B <ul style="list-style-type: none"> As 2B Functional form of "intervention" term (same as 3A) measures effect of other factors appropriately
4A <ul style="list-style-type: none"> Available covariates cover all important influential factors Functional form of multivariate time series model is correct 	4B <ul style="list-style-type: none"> Functional form of multivariate time series model for comparison group is correct No assumption that covariates cover <i>all</i> important influential factors (some desirable)
5A <ul style="list-style-type: none"> As 4A Functional form of intervention term is correct 	5B <ul style="list-style-type: none"> As 4B Functional form of "intervention" term (same as 5A) measures effect of other factors appropriately

The Methods developed in the Papers

The twenty papers have been summarised with a view to categorising the evaluation methods used in each according to the categories defined in Table 1. The summary of each paper describes the evaluation criterion (including the target group chosen), the method used to estimate the expected level of the criterion for the target group (in the absence of the countermeasure), and the method used to take into account the influence of "other factors" (if applicable). The summaries, including the category assigned to each method, are tabulated in Appendix B.

Twenty three evaluation methods are covered in the papers because some papers have used two methods. Where a paper used more than one method, they have each been categorised as defined in Table 1. The following sections outline the papers whose methods have been included in each category. The discussion of each method covers the evaluation strategy and its limitations, the threats to validity which it aimed to overcome or to which it is susceptible, and its contribution to the aim of reaching more definitive conclusions about the effectiveness of countermeasures. A summary of the application of the method in the relevant papers is tabulated at the end of each section.

Cross-section comparison methods

None of the papers submitted as part of this thesis have used evaluation method 0A for cross-section comparisons. It is theoretically possible that the injury levels of two or more comparison groups (after adjustment for their relative exposures) could have been directly compared in a valid way. In practice the nature of road trauma comparisons across a factor of interest is seldom one where the factor can be considered independent of other factors which influence injury levels. As such, method 0B must be used to take into account the influence of the other factors, either by categorisation (method 0B+) or by covariate analysis (method 0B++).

Method 0B+ : Adjustment for known other factors by categorisation

Papers no. 1, 3, 7 and 8 have all used method 0B+ for evaluation studies making use of cross-section comparisons (Table 3). In each study the threat of the influence of an "other" (known) factor influencing the criterion variable has been recognised, and the other factor has been included in the analysis in categorical form. In the case of interval- or ordinal-scaled factors, the categories have been formed by grouping the data in ranges of the factor.

Paper 1 analyses the number of drivers involved in reported crashes, using their estimated mean daily miles driven to provide their expected crashes, and also to provide estimates of their crash risk per unit of road use (ie. Risk (B) in Figure 1 of this thesis). The risk estimates are referred to as "accident rates" in the paper.

The paper presents theory and evidence to support a Poisson model for accident processes, ie. that the number of accidents during a given period follows the Poisson distribution. Thus the paper also recognises the threat of "instability" in the criterion variable due to chance variation and aims to take this into account by an explicit model of its distribution. From this

basis a method of contingency table analysis is developed which allows a statistical test of the equality of crash risks across the categories defined by one attribute (factor), and tests of first- and second-order interactions between two and three attributes, respectively. Subsequently the paper addresses the very high power (very low Type II error probabilities) when the analysis method is applied to the typically very large accident data files available and when conventional significance levels (Type I error probabilities) are used, and recommends empirical methods to overcome problems which may occur. Some results diagnostic tools and a method for calculating confidence limits for the risk estimates are also presented.

In application of the contingency table analysis method to evaluate the effect of driver sex on crash risk, the paper illustrates the use of method OB+ to take into account the influence of another factor (driver age) available in the data and likely also to affect crash risk. (In fact, the paper focuses on driver sex and age simultaneously, and the remarks made here would apply almost equally if the two factors were reversed.) The data is stratified into categories reflecting ranges of driver age, in addition to the two categories representing the focus of this cross-section comparison, driver sex. The analysis method allows a statistical test of the hypothesis that there was no interaction between driver sex and the other factor (driver age) so far as crash risk is concerned. The paper found that there was a statistically significant interaction. The appropriate conclusion from this illustration of evaluation method OB+ is that the effect of driver sex is related to the driver age, and that the effect should be estimated within each category of driver age defined.

A weakness of method OB+ illustrated in Paper 1 is that the available data on driver age, an interval-scaled variable, has not been used fully. Driver age has been grouped, somewhat arbitrarily, into six ranges chosen to avoid small crash frequencies, and the order of the age groups is ignored by the analysis method used. The broad age groups may hide some effects which would have been apparent had finer groups been used. Furthermore the effects of driver age may appear stronger if the analysis could make full use of the original scale on which it was measured. Method OB++ (see below) offers the potential to use driver age as an interval-scaled covariate in a cross-section comparison of the two driver sexes.

Paper 3 analyses the proportion of injured car occupants who sustained specific injury (defined on a six-point injury severity scale within each of six body regions). At the higher end of the injury severity scale, the proportions provide estimates of the probability of a severe injury given that the occupant is injured (ie. Injury Severity (A) in Figure 1). The paper focuses on the injury severity of injured occupants who were seat belted and makes a cross-section comparison with unbelted injured occupants in an evaluation of the effects of seat belt wearing on specific injury patterns. The important role of the entry criterion for inclusion in the data (ie. injury to some degree) in limiting conclusions about injury risk was recognised.

Difference in crash severity is a known "other factor" which threatens the validity of the cross-section comparison of the seat belt wearers and non-wearers. However, information on the severity of crash experienced by the injured occupants was not available, only the crash location (open road or built-up area) which in turn was considered likely to be associated with travel and impact speeds and hence with crash severity. As a proxy for crash severity, the crash location was taken into account by stratifying the data by the two location categories. Separate comparisons of the injury patterns of the belted and unbelted occupants were made

for each location category, providing two independent sets of estimates of the effects of seat belt wearing.

Paper 3 illustrates a weakness of method OB+ which was not apparent in Paper 1. The method potentially produces more than one estimate of the effect of the countermeasure, the number of estimates being related to the number of categories defined by the "other" factor (or factors) considered in the analysis. Unlike Paper 1, Paper 3 provided no test of whether there is evidence that the countermeasure effects were really different between the categories. (Ordered multiple choice models could be applied to the ordered injury severity scale and other categorical variables included in the analysis to test this; eg. O'Donnell and Connor 1994. However, the test may still reveal that separate estimates are warranted.)

For the purpose of evaluating differences in "crashworthiness" between makes/models of cars, Papers 7 and 8 analyse the risk of driver involved in a crash being killed or severely injured (admitted to hospital) in two steps:

- (i) rate of injury for drivers involved in crashes (estimating the Injury Risk in Figure 1)
- (ii) rate of serious injury for injured drivers (estimating Injury Severity in Figure 1).

The papers differ in that Paper 7 compares (i) with an alternative measure of relative injury risk based on two-car crashes, and Paper 8 examines the effect of vehicle mass on (i) and (ii) and their combined rate. The combined rate, (i) x (ii), is considered to provide estimates of the risk of a driver being killed or severely injured when involved in a crash.

Both papers include the "road trauma chain" (Figure 1 of this thesis), the second part of which provides the conceptual basis for the two step approach outlined in the previous paragraph. This chain of events includes the concept that "exposure-to-risk" can be extended beyond events in the pre-crash phase (ie. exposures A to D in Figure 1) to include exposure to the risk of injury when involved in a crash ("crash exposure") and exposure to the risk of severe injury or death when injured in a crash ("injury exposure"). This conceptualisation opens the door to methods of evaluation which have traditionally been applied to pre-crash risks being applied to evaluate countermeasures aimed at post-crash risks.

The papers recognise that many other factors apart from the car make/model can affect the risks measured by the rates (i) and (ii) defined above. The known factors available in the data files included the driver age, sex and seat belt use, the car mass, crash type, point of impact, and speed limit at the crash location. The method of analysis chosen at the time to take other factors into account was categorisation. Although relatively large data sets were available, the need to make cross-section comparisons of as many makes/models as possible limited the number of available other factors (and their levels) which could be considered. A prior analysis (outlined in the papers) found that driver sex and speed limit zone were the two factors most strongly related to (i) and (ii) and which differed enough between individual makes/models to make a substantial difference to the specific estimates for each make/model (Cameron, Mach and Neiger 1992).

The data is then stratified into four categories defined by the two driver sexes and two ranges of speed limits. Within each category, rates (i) and (ii) are calculated for each make/model. To provide a global estimate of, for example rate (i), the four estimates are then *normalised* following Armitage (1971), ie. a weighted average is calculated with weights (fixed for each make/model) representing the proportion of drivers in the four age by speed limit categories which was observed for all makes/models in total. This method treats each make/model of

car as if they had all had the same mix of male and female drivers and crashes in high and low speed limit zones, for the purpose of providing a global estimate of (i). A similar approach is used to provide a global estimate of rate (ii) for each make/model. The normalisation approach overcomes a weakness identified in Paper 3 in that only one estimate is ultimately produced (for each of (i) and (ii)) of the effect of each make/model, rather than four estimates. As a final step, the global estimates of (i) and (ii) are combined to provide a single estimate of the crashworthiness effect of each make/model.

Papers 7 and 8 illustrate the major weakness of method OB+ in that, in practice, it is capable of taking into account only a limited number of other factors to address the threat of "history". Use of a method including the less-important other known factors, while it may estimate essentially the same crashworthiness effects, is likely to produce more reliable estimates with narrower confidence limits. It was recognised that the limitations of the method OB+ could be overcome by the use of logistic regression models for analysis of rates (i) and (ii). These models allow a greater number of other factors to be included in the analysis as well as make/model, and thus their influence could be taken into account in this way. The suggested method falls in evaluation method category OB++ and was taken up (as described in Paper 17) to overcome the major weakness of method OB+ described above.

Paper 8 includes material examining the relationships between car mass and each of the risk estimates, rates (i) and (ii), as well as the combined rate, which itself was conceptualised as a risk estimate. Simple linear regressions were fitted between each risk estimate and the mass of the 41 makes/models for which reliable estimates had been obtained, in order to test the statistical significance of the relationship with mass. The assumption of linear regression, that the error terms are Normally distributed, is inappropriate for dependent variables which are proportions bounded by zero and unity. The recognition that logistic regression models could be used in this context had the additional advantage that they represent the chance variation in the rates (i) and (ii) in an appropriate way.

Table 3: Papers using cross-section comparison method OB+ : Adjustment for known other factors by categorisation

Paper No. and Identification	Description	Evaluation criteria	Expected level method	Other factors method
1. M.H. Cameron, "Accident rate analysis and confidence limits". <u>Proceedings, Fifth Conference, Australian Road Research Board</u> , Canberra, August 1970.	1. Poisson model for accident process 2. Method for contingency table analysis of accident rates 3. Statistical testing errors for large data sets 4. Method for detecting specific cells with significantly different risks 5. Confidence limits for risk	No. of drivers involved in reported accidents in Brisbane, 1961	Exposure adjustment: Mean daily miles driven, estimated from a travel survey	1. When driver age was analysed, driver sex was taken into account by categorisation 2. Vice versa when driver sex was analysed 3. Their interaction was also tested
3. M.H. Cameron, "The effect of seat belts on minor and severe injuries measured on the Abbreviated Injury Scale". <u>Proceedings, Eighth International Conference of the International Association for Accident and Traffic Medicine</u> , Aarhus, Denmark, June 1980. Also in <u>Accident Analysis and Prevention</u> , Vol. 13, no. 1, March 1981.	1. Classification of injuries of injured car occupants on six-point threat-to-life scale (AIS) within six body regions 2. Comparison of injury severity distributions of seat belted and unbelted occupants, categorised by crash location	Percentage of total seat belted injured occupants who sustained injury at specific AIS level	Percentage of total unbelted injured occupants who sustained injury at specific AIS level	Categorisation by crash location (dichotomy) to take into account differences in vehicle speed and hence crash severity
7. M.H. Cameron, T. Mach, D. Neiger, A. Graham, R. Ramsay, M. Pappas and J. Haley, "Vehicle crashworthiness ratings in Australia". <u>Proceedings, International IRCOBI Conference on the Biomechanics of Impacts</u> , Verona, Italy, September 1992. Also in: <u>Accident Analysis and Prevention</u> , Vol. 26, No. 4, 1994.	1. Crashworthiness measured in two components (i) driver injury risk in tow-away crashes in NSW, (ii) injury severity of injured drivers in Victoria and NSW 2. Comparison of (i) with relative injury risk estimated from two-car injury crashes in Victoria	Estimated risk of death or hospital admission for drivers involved in tow-away crashes [derived from separate estimates of (i) and (ii)]	Exposure adjustment: Number of tow-away crash involvements ("crash exposure"), and injured drivers ("injury exposure"), by make/model	Categorisation of crash and injury exposures by driver sex and speed limit zone. Normalisation of exposure distributions so that each make/model is compared on same basis, by weighted averages of (i) and (ii) using the universal exposure distributions.
8. M.H. Cameron, T. Mach, D. Neiger, A. Graham, R. Ramsay, M. Pappas and J. Haley, "Vehicle crashworthiness ratings from Victoria and New South Wales crash data" <u>Road and Transport Research</u> , Vol. 1, No. 3, September 1992.	1. Analysis as per paper no. 7 2. Investigation of relationship between crashworthiness (and each component) and vehicle mass	As per paper no. 7	1. As per paper no. 7 2. Linear regression of crashworthiness estimate against mass	As per paper no. 7

Method OB++ : Adjustment for known other factors by covariate analysis

Papers no. 17 and 19 have extended the evaluations of relative safety of cars by make/model described in Papers 7 and 8, but have made use of method OB++ to better take into account the influence of other factors during the cross-section comparisons (Table 4). Paper 17 is focused on evaluating the crashworthiness of cars, ie. their ability to protect their own occupants from injury, whereas Paper 19 is focused on evaluating their "aggressivity", ie. the threat of injury which a car make/model represents to other road users with which it impacts.

In a similar way to Papers 7 and 8, Paper 17 analyses rates (i) and (ii) separately to estimate the effect of car make/model on the Injury Risk and Injury Severity probabilities, respectively. It then combines the two estimates of risk probability to provide a measure of the crashworthiness of each make/model of car. A logistic regression model for each of these risks, P , with the following functional form is fitted to the data on driver crash involvements and injured drivers, respectively:

$$\text{logit}(P) = \ln\left(\frac{P}{1-P}\right) = \beta_0 + \beta_1 X_1 + \dots + \beta_k X_k = f(X).$$

The car make/model is one of the factors, X_i . The remaining factors, X_j , are other known factors available in the data which also affect the risk probabilities. By including them in the logistic regression model as covariates together with the car make/model, it was assumed that their influence was taken into account and that the threat of "history" (from at least these factors) was removed from the evaluation of car make/model.

In Paper 17, the other factors included in the regression model are all categorical variables, namely the driver sex, driver age (three categories), speed limit at the crash location, and number of vehicles involved in the crash (two categories for each). The analysis also included the statistically significant first-order interactions between these four variables. Interval-scaled variables could have been included as covariates, if available and appropriate. Consideration was given to including driver age in its original interval-scale form, but its known non-monotonic influence on injury outcome militated against its inclusion in this way. The interval-scaled variable, car mass, could have been included as a covariate to be taken into account when measuring crashworthiness, if that had been considered appropriate. However, in Paper 17 the mass is considered to be an ingredient of the crashworthiness performance of each make/model and it would have been inappropriate to remove its effects. In another context, this was considered appropriate and was successfully achieved using a generalised linear model (Cameron, Gantzer and Carr 1996).

As was expected, the use of evaluation method OB++ which could take into account a larger number of "other" factors as covariates, compared with method OB+ based on categorisation, resulted in more precise estimates of the crashworthiness effects. The logistic regression method used in Paper 17 was also applied to the same data set as was used in Papers 7 and 8. For each car make/model, the width of the confidence interval for the crashworthiness estimate based on the logistic regression method was narrower than the width resulting from the stratification and normalisation method (Cameron, Finch and Le 1994).

While method OB++ has advantages over method OB+ in this context, it was noted that it requires the assumption that the functional form of the logistic regression model (see above) is correct. Method OB+ does not require this assumption, but implicitly fits a more complex model with many more parameters, which in turn results in the crashworthiness estimates being less precise for those makes/models for which there is relatively few data. Method OB++ continues to be subject to the threat of "history" from other influential factors which are not included as covariates or are not highly correlated with the factors which are included. In the context of Paper 17, an important influential factor which was not available in the data, and hence could not be considered for inclusion, was the crash severity (measured, for example, by impact speed or vehicle deformation). While crash severity may be correlated with the speed limit and the number of vehicles involved in the crash, the absence of this covariate from the analysis was the basis of an important qualification of the results which was stated in Paper 17.

Notwithstanding the assumptions and qualifications outlined in the previous paragraph, the method developed in Paper 17 has been used to produce four further evaluations of the crashworthiness of Australian cars by make/model (Cameron, Newstead and Skalova 1996; Newstead, Cameron and Skalova 1996; Newstead, Cameron and Le 1997, 1998, 1999). On each occasion, the project sponsors have widely distributed the results in the form of a brochure to provide advice to potential car purchasers on relative crashworthiness. The results have been considered to have high face validity by consumers and other commentators, suggesting that the assumptions of the method are reasonable and that the absence of a measure of crash severity has not led to misleading results.

Paper 19 extends the method used in Paper 17 to evaluate the aggressivity of cars by make/model. Aggressivity is measured by the risk of the other driver being killed or severely injured in a two-car crash with the specific make/model, and by the injury severity of unprotected road users (pedestrians, bicyclists, motorcyclists) injured in collisions with the specific make/model. The first of these risk probabilities is estimated in two steps:

- (i) rate of injury of other drivers involved in two-car crashes (Injury Risk)
- (ii) rate of serious injury for other drivers injured in two-car crashes (Injury Severity).

In a similar way to Paper 17, a logistic regression model is developed for each of these risk estimates, rates (i) and (ii), and the injury severity of the unprotected road users. As the focus of the cross-section evaluation, the make/model of the impacting car is included as one factor, X_i . Other factors available in the data were considered for inclusion as covariates because of their potential influence on each risk. The factors considered were all categorical in nature and included the speed limit at the crash location; the age and sex of the driver of the impacting car; the age and sex of the other driver or the unprotected road user; and the type of unprotected road user, when appropriate. All possible interactions of these factors were also considered and included if statistically significant. The age and sex of the driver of the impacting car were not statistically significant and are not included in the final logistic regression models.

The assumptions and qualifications associated with the method and results in Paper 19 are almost identical with those in Paper 17. The functional form of the regression model is assumed, and the threat of "history" from other factors not included (especially differences in crash severity) remains. [However, a peripheral study on the same data using the injury outcome of the driver of the impacting car as a proxy for crash severity when included as a

covariate found that its omission made little difference to the results.] At the time of writing the results have not been published in a way which identifies make/model to test their face validity. However, the general finding of higher levels of aggressivity associated with Australian four-wheel-drive vehicles has been considered to be consistent with expectation by commentators on the paper ("The Open Road" 1998).

Table 4: Papers using cross-section comparison method OB++ : Adjustment for known other factors by covariate analysis

Paper No. and Identification	Description	Evaluation criteria	Expected level method	Other factors method
17. M.H. Cameron, C.F. Finch, S.V. Newstead, T. Le, A. Graham, M. Griffiths, M. Pappas and J. Haley, "Measuring crashworthiness: Make/model ratings and the influence of Australian Design Rules for motor vehicle safety". <u>Proceedings, International IRCOB Conference on the Biomechanics of Impact</u> , Brunnen, Switzerland, September 1995.	1. Crashworthiness measured in two components (i) driver injury risk in tow-away crashes in NSW, (ii) injury severity of injured drivers in Victoria and NSW 2. Investigation of relationship between crashworthiness and year of manufacture of vehicle	Estimated risk of death or hospital admission for drivers involved in tow-away crashes [derived from separate estimates of (i) and (ii)]	Coefficient of car model in separate logistic regression models of (i) injury risk, and (ii) injury severity	Influential crash and injury exposure factors (driver age and sex, speed limit zone, number of vehicles involved) were included in the models as covariates
19. M.H. Cameron, S.V. Newstead and C.M. Le, "Rating the aggressivity of Australian passenger vehicles towards other vehicle occupants and unprotected road users". <u>Proceedings, International IRCOB Conference on the Biomechanics of Impact</u> , Gothenborg, Sweden, September 1998. Also in: <u>Journal of Crash Prevention and Injury Control</u> , Vol. 1, No. 2, 1999.	1. Aggressivity to other drivers measured by components (i) injury risk in two-car crashes, (ii) injury severity of injured drivers 2. Aggressivity to unprotected road users measured by their injury severity 3. Investigation of relationships between aggressivity, crashworthiness and mass	1. Estimated risk of death or hospital admission for other drivers involved in tow-away crashes with subject cars [derived from separate estimates of (i) and (ii)] 2. Estimated risk of death or hospital admission for unprotected road users who were injured in collisions with subject cars	Coefficient of car model in separate logistic regression models of (i) injury risk, and (ii) injury severity of other drivers; and logistic regression model of injury severity of injured unprotected road users	Influential crash and injury exposure factors (other driver age and sex, speed limit zone, number of vehicles involved, unprotected road user age, sex and type) were included in the relevant logistic regression models as covariates

Time-related comparison methods

None of the papers submitted as part of this thesis have used evaluation methods 1A, 2B, 3A or 4B for time-related comparisons. While method 1A has been commonly used in traffic engineering safety evaluations ("before-after" studies), it is generally considered to be very susceptible to the threat of "maturation" and thus has been avoided in the papers.

Method 3A, while it overcomes the threat of maturation, is susceptible to the threat of "history" due to other unknown factors affecting the criterion variable. Even where univariate time series methods were considered appropriate, method 3A has been avoided in favour of method 3B because of its greater strength. Similar remarks apply to method 2A, which has been used in only one paper, and method 4A, which can include known other factors as covariates but is still susceptible to the threat from unknown factors.

Methods 2B and 4B have been avoided in favour of the corresponding methods 3B and 5B, respectively, because of the potential additional statistical power to be gained from fitting a time series model to the pre- and post-implementation data combined, rather than to the pre-implementation data alone. The disadvantage of needing to assume the specific functional form for the intervention term was considered to be out-weighed by the potentially greater reliability (smaller error variance) of the impact estimate for the countermeasure being evaluated.

Method 1B: Adjustment for other factors by pre- to post- change in comparison group

Papers no. 4, 5, 18 and 20 have all used method 1B for time-related evaluation studies (Table 5). Papers 5 and 18 have each used two methods of evaluation, but only one method is relevant to the current discussion. Each paper has made use of the value of the criterion variable in a pre-implementation period to estimate the expected level for the target group during a period after the countermeasure had been implemented. The influence of "other" factors (not explicitly known or defined) has been taken into account by considering the parallel change in the criterion variable measured for a comparison group.

Paper 4 analyses serious casualty crashes at night which occurred in five specific sectors of Melbourne during weeks influenced by Police random breath test (RBT) operations. Weeks were considered to be influenced by the direct effects of the operations and by residual effects in subsequent weeks. During three periods of up to eight weeks in 1978 and 1979, the Police had rotated their RBT operations between the sectors for usually two weeks at a time. The expected level of crashes in the influenced sector-weeks was estimated by the average of the number of crashes which occurred in the same sector-weeks during two previous years. The effects of other factors was measured by the change in the level of day-time serious casualty crashes in the same sector-weeks over the same years. The day-time change was used to adjust the night-time change in order to take the effects of other factors into account. The statistical significance of the adjusted (net) change was tested by a 2 x 2 chi-square test.

An alternative measure of the effect of other factors could have been the change in night-time crashes in the sectors of Melbourne during weeks outside the influenced sector-weeks. This alternative was rejected because it was hypothesised that the RBT may have had

contamination effects beyond the direct and residual influence. The measure chosen for this purpose was favoured because the day-time crashes were considered to involve relatively low levels of alcohol involvement compared with night-time crashes, and alcohol-involved crashes are the focus of the countermeasure under evaluation. However, a weakness of this decision is the possibility that RBT, as a highly visible form of Police enforcement, may also have an influence of day-time crashes, including those not involving illegal alcohol levels. The use of the change in day-time crashes to adjust the change in night-time crashes may have caused an under-estimate of the effect of RBT.

Another weakness is highlighted by a critique by Ross (1981) on an earlier analysis of the effects of the RBT operations during the first of the three periods (Cameron, Strang and Vulcan 1980). During that analysis, only one previous year's crashes were used to estimate the expected level in the influenced sector-weeks. Ross criticised this as being inadequate to overcome the threat of "maturation" and favoured the use of time series analysis over a large number of years to provide the estimates by extrapolation, ie. method 2A or 2B in Table 1. As Homel (1988) has also pointed out, one year of pre-implementation crash data would also be inadequate to overcome the threat of "regression to mean" if this had been present. The RBT operations were assigned to the sectors of Melbourne without reference to the crash levels during the same weeks in the previous year. However, the possibility remains that with the relatively small number of sectors considered, the RBT may have been assigned to sector-weeks with relatively high crash levels during the previous year by chance, thus producing an over-estimate of the expected level during the RBT operations. While Paper 4 aimed to overcome these criticisms by the use of two previous years' crash data, the paper acknowledges that the use of method 2A or 2B would have been superior, had resources permitted their use.

Paper 5 uses method 1B to support a parallel analysis using method 3B to evaluate the effects of the zero blood alcohol concentration (BAC) requirement for novice drivers which was introduced in Victoria in May 1984. Both methods described in the paper analyse serious casualty crashes involving novice drivers in "alcohol times" of the week¹ (the target group) during periods before and after the legislation was implemented. The effect of other factors were taken into account by identical analysis of the crashes of three different comparison groups. These were (1) novice drivers crashing during the non-alcohol times, (2) standard licence holders (who were not subject to the zero BAC requirement) crashing during alcohol times, and (3) standard licence holders crashing during non-alcohol times.

When method 1B is used in Paper 5, the expected level of crashes for the target group during an 18 month post-implementation period was the actual number of crashes for the target group during a matched pre-implementation period of the same duration. Thus the change in the number of crashes between these two periods could be considered to be an initial estimate of the effect of the legislation. The parallel changes in the crashes of the comparison group provide three estimates of the effect of other factors, which in turn needs to be discounted from the initial estimate of the countermeasure effect. In this paper, unlike Paper 4, the effect

¹ The "alcohol times" of the week (ie. 4pm Sunday to 4am Monday, 6pm to 6am on Monday to Thursday nights, 6pm Friday to 8am Saturday, and 2pm Saturday to 8am Sunday) were those periods when the percentage of serious casualty crashes involving drivers with a blood alcohol content exceeding 0.05% exceeded 9% and averaged 24.5% of crashes during 1977-82 (South 1986). The "non-alcohol times" are the converse of these periods, during which 3.3% of the crashes involved drivers with blood alcohol content exceeding 0.05%.

of other factors is measured in three different ways, offering greater potential to overcome the threat of "history".

In Paper 5, the crashes of the target group and the three comparison groups provide a 2 x 2 contrast. Thus the change in target group crashes was initially discounted ("netted") by the change in novice driver crashes during non-alcohol times, and then further discounted by the net change in the standard licence holder crashes (alcohol time crashes netted for non-alcohol time crashes). The net net change is the final estimate of the countermeasure effect, after taking into account the influence of other factors which influenced novice drivers specifically and also drivers in general. The statistical significance of the net net change was judged by a 2 x 2 x 2 contingency table analysis test, where the third contrast was the time period (pre-versus post-implementation). The test is essentially a test of the presence of an interaction between licence type and time of week affecting the change in crashes.

While the application of method 1B in Paper 5 had greater power than its application in Paper 4, due to the availability of three comparison groups, nevertheless it was recognised in the paper that the method was still likely to be subject to the threat of "maturation". Elsewhere in the paper it was identified that there was a substantial downward trend in the crashes of the target group (and also, but less so, in the standard licence holder crashes during alcohol-times). This made method 3B the preferred method of analysis in Paper 5, with the results of method 1B being included for comparative and illustrative purposes. The application of method 3B in the paper will be discussed later in this thesis.

Paper 18 uses method 1B to evaluate the localised effects on crashes due to a program of increased random breath testing in country Victoria during November 1993 to December 1994. Earlier, the paper also evaluates the general effect of the program, using method 5B, which will also be discussed later in this thesis. Paper 20 extends the evaluation of localised effects using method 1B to test the hypothesis that some forms of RBT operations may have produced differential effects on crashes related to the type of road on which they occurred.

The papers analyse serious casualty crashes which occurred during "high alcohol hours"² in 70 regions of country Victoria during weeks in which RBT operations were present, covering a 58 week period. The expected level of crashes in the enforced region-weeks was estimated by the number of crashes in the same region-weeks during the previous year. The effects of other factors were measured by the change in the number of "low alcohol hour" crashes in the same region-weeks over the same periods. Residual effects on crashes during periods up to three weeks after the RBT operations ceased were considered (Cameron, Diamantopoulou, Mullan et al 1997), but the results of this analysis are not given in the papers.

Consideration was given to using the change in high alcohol hour crashes in the region-weeks uninfluenced by the RBT operations to measure the effects of other factors, but it turned out that these region-weeks were too sparse and unrepresentative to satisfy that role. However, the broad coverage of region-weeks in which the RBT operations occurred, out of the total of

² The country "high alcohol hours" of the week were 6pm Sunday to 6am Monday, 6pm to 4am on Monday to Wednesday nights, 6pm Thursday to 6am Friday, 6pm Friday to 8am Saturday, and 4pm Saturday to 10am Sunday (Gantzer 1995). The remaining hours of the week were the country "low alcohol hours". The percentage of drivers killed or admitted to hospital with a blood alcohol content exceeding 0.05% from country crashes during the high alcohol hours was found to be nearly seven times the percentage from crashes during the low alcohol hours in 1990-94.

70 x 58 = 4060 possibilities, had the advantage that the threat of "regression to mean" due to chance assignment of the operations to region-weeks with relatively high numbers of crashes during the previous year was unlikely. The breadth of coverage of the relatively large number of possibilities resulted in this threat being of less concern in Papers 18 and 20 compared with Paper 4, where the RBT operations were rotated for two weeks at a time between no more than five sectors of Melbourne during study periods totalling no more than eight weeks.

The effect of the RBT operations was estimated by the change in the high alcohol hour crashes in the enforced region-weeks, discounted by the change in the low alcohol hour crashes in the same region weeks. To test the specific effects of RBT under various circumstances, the crash data was stratified into categories defined by the style of RBT operation used (five categories), the level of drink-driving publicity awareness (three categories) and, in the case of Paper 20, the type of road (two categories). This stratification was not considered necessary to address the threat of "history", rather the aim was to measure the effects specific to each category. To test whether there was evidence of real differences of RBT effects related to the categories, higher-order contingency table analysis methods were used. If such differences were judged to exist, the statistical significance of each specific effect was tested by a 2 x 2 chi-square test of the crash numbers in the specific category.

The discussion of Papers 4 and 5 has emphasised that a key weakness in the use of method 1B for time-related evaluations is the possibility of the threat of "maturation". It is considered that in the evaluations described in Papers 18 and 20, this threat is less likely, though still possible. The broad dispersion and relatively large number of region-weeks used, in aggregate, to measure the change in crashes of the target group and the comparison group militates against any general trend in crashes being an alternative explanation for the measured effect of the RBT operations. The increased confidence that the threat of "maturation" is reduced in such circumstances is important (if valid), because a large number of region-weeks would prohibit the use of the following evaluation methods which require time series models to be developed for the crashes in each region-week.

Table 5: Papers using time-related comparison method 1B: Adjustment for other factors by pre- to post- change in comparison group

Paper No. and Identification	Description	Evaluation criteria	Expected level method	Other factors method
4. M.H. Cameron and P.M. Strang, "Effect of intensified random breath testing in Melbourne during 1978 and 1979". <u>Proceedings, Eleventh Conference, Australian Road Research Board</u> , August 1982.	1. Random breath testing (RBT) scheduled in sectors of Melbourne on Thursday to Saturday nights for up to two weeks at a time 2. Other sectors/weeks kept RBT-free to facilitate pseudo-experimental design 3. Analysis of relative changes in accidents	Serious casualty accidents <i>at night</i> in RBT sectors during weeks the RBT operated (plus subsequent weeks to measure residual effects)	Average of serious casualty accidents <i>at night</i> in the same sectors and weeks, <i>in two previous years</i>	Change in serious casualty accidents <i>during day-time</i> in RBT sectors during weeks influenced by the RBT operations, compared to same sectors and weeks in the two previous years
5. M.O. Haque and M.H. Cameron, "Effect of the Victorian Zero BAC legislation on serious casualty accidents : July 1984 – December 1985". <u>Journal of Safety Research</u> , Vol. 20, No. 3, Fall 1989. (Method 2)	Analysis of accident involvements of novice car drivers (the target group) who were subject to the zero blood alcohol content (BAC) requirement in Victoria from May 1984	Serious casualty accidents involving target drivers which occurred during "alcohol times" of the week in post-legislation period	Level of target group accidents during (relatively short) pre-legislation period	Change in target drivers' accidents during "non-alcohol times". Also compared with net change ("alcohol time" relative to "non-alcohol time") in accidents involving standard licensed drivers not subject to zero BAC
18. M.H. Cameron, K. Diamantopoulou, N. Mullan and S. Gantzer, "Evaluation of the country random breath testing and publicity program in Victoria 1993-1994". <u>Proceedings, ROADS 96 Conference</u> , Christchurch, New Zealand, September 1996. (Method 2)	Analysis of changes in crashes in influenced areas to measure localised effects of country RBT enforcement operations	Serious casualty crashes during HAHs in country Victoria areas and weeks influenced by RBT operations	HAH crashes in same areas and weeks during the previous year	Change in LAH crashes in same areas and weeks (RBT period compared with previous year). Categorisation to measure specific effects
20. K. Diamantopoulou and M.H. Cameron, "Localised effects on crashes of the country random breath testing and publicity program in Victoria". <u>Proceedings, 19th ARRB Transport Research Conference, Investing in Transport</u> , Sydney, December 1998.	Extension of analysis in paper no. 18 to test hypothesised differential effects by road type. Re-analysis of changes in crashes in influenced areas to measure localised effects of enforcement operations on major and minor roads	Serious casualty crashes during HAHs in country Victoria during Nov 1993 to Dec 1994 in areas and weeks influenced by RBT operations	HAH crashes in same areas and weeks during the previous year	Change in LAH crashes in same areas and weeks (RBT period compared with previous year). Categorisation by road type to measure specific effects

Method 2A: Extrapolation of univariate time series model of pre-implementation levels

Paper no. 6 uses method 2A to evaluate the initial effect of the compulsory bicycle helmet wearing legislation in Victoria on the number of injured cyclists with head injuries (Table 6). The paper recognises that a number of other road safety initiatives had been introduced broadly around the same time as the helmet law in July 1990 (see Papers 9, 11, 12 and 15 for details) which may also have reduced the risk of bicyclist crashes and injuries. Attempting to overcome this threat of "history" due to the other initiatives, the paper defines the proportion of injured cyclists who sustained a head injury as the criterion variable, ie. a measure of Injury Severity in Figure 1, where severe injury is defined as a head injury in this context.

Three data sets are used to evaluate the initial effect of the law on such criteria: two sets of data on cyclist hospital admissions (to December 1990) and one on cyclist presentations to a sub-sample of Victorian hospitals (to June 1991). In each case, the data was available for a number of years prior to the law (nine years in one case), so a time series model of the pre-legislation trend in the criterion variable was developed in an attempt to overcome the threat of "maturation". A simple linear regression was fitted to the proportions with head injury during the pre-legislation period, then extrapolated against time to estimate the expected level (with confidence limits) during the post-legislation period. The effect of the helmet law was estimated by the difference between the actual post-legislation proportion and the expected level, and statistical significance was judged by comparison of the actual proportion with the confidence limits.

A weakness of method 2A displayed in Paper 6 is that it did not take into account the likely influence on the criterion variable, in developing the pre-legislation model, of a known factor for which relevant data was available, namely helmet wearing rates surveyed since 1985. While the substantially increased post-legislation wearing rate was associated with the law and hence inappropriate for use in method 2A, the pre-legislation wearing rates had the potential to act as a covariate in the time series model. This may have reduced the apparent chance variation in the criterion variable, thus reducing the width of the confidence limits of the estimated level during the post-legislation period and providing a more sensitive test of the statistical significance of the estimated effect of the law. Such an approach would, however, use method 4A and will be discussed later in this context in Papers 10 and 14.

Apart from the definition of the criterion variable (discussed above), the analysis did not attempt to overcome the threat of "history" from other factors, eg. by consideration and parallel analysis of a comparison group. The paper relied on the assumption that all other factors, apart from the helmet law, affected the risk of cyclist injury other than to head in the same way as they affected cyclist injury to the head. However, it is possible that there may have been greater reductions in the risk of head injury, relative to the risk of a cyclist sustaining non-head injury, associated with a reduction in vehicle speeds due to Victoria's speed camera program which was expanded in 1990 (see Paper 12). Robinson (1996) found that the proportion of injured *pedestrians* who sustained a head injury fell during the first two post-legislation years in much the same way as the bicyclist proportion, even though the helmet law did not pertain to them. If the pedestrian proportion could be considered to adequately represent the influence of other factors on, say, the risk of head injury to unprotected road users in general, then a better method for the analysis would have been method 2B in which pedestrians are used as the comparison group.

Table 6: Paper using time-related comparison method 2A: Extrapolation of univariate time series model of pre-implementation levels

Paper No. and Identification	Description	Evaluation criteria	Expected level method	Other factors method
6. A.P. Vulcan, M.H. Cameron and W.L. Watson, "Mandatory bicycle helmet use: experience in Victoria, Australia". Special issue on "Road Trauma and its Prevention", World Journal of Surgery, No. 16, pp. 389-397, 1992.	1. Presentation of time series of helmet wearing rates during pre- and post-legislation periods 2. Time series analysis of proportion of killed and hospital admitted cyclists who sustained head injuries	Proportion with head injuries during the post-legislation period	Extrapolation of linear regression model against time, fitted to proportions during pre-legislation periods	None [other factors affecting head injury incidence were assumed to affect equally the incidence of cyclist death or hospital admission from injuries other than to the head]

Method 3B: Intervention term in univariate time series model, adjusting for other factors by univariate analysis of comparison group

Paper 5 uses method 3B as its principal method of analysis in order to overcome the threat of "maturation" which may have applied when method 1B was used to evaluate the effects of the zero BAC requirement for novice drivers (Table 7). The target group, the evaluation criterion and the three comparison groups used in the evaluation were the same as for method 1B previously described.

Univariate ARIMA time series models were fitted to monthly levels of the criterion crash involvements of the target group and each comparison group during the 7½ year pre-intervention period. For the month of the introduction of the zero BAC law, an intervention term was added to each time series model and the model fitted again to the pre- and post-legislation period covering nine years. The intervention term was formulated as a step change in the crash involvement rate per month for the target group. The same formulation was used for each comparison group series except that, in these cases, the estimated "intervention effect" was considered to measure the influence of other factors and thus overcome the threat of "history". For the final model fitting, the method of *Intervention Analysis* (Box and Tiao 1975) was used to provide estimates of the intervention effects and the standard deviation of each estimate.

Following a similar approach to that when method 1B was used in Paper 5, the estimated intervention effect for novice drivers during alcohol times was initially discounted ("netted") by their intervention effect estimated for non-alcohol times (by subtraction of the second estimate from the first), and then further discounted by subtracting the net intervention effect for standard licence holders (ie. their intervention effect on alcohol time crashes minus the intervention effect on non-alcohol time crashes). The net net intervention effect was considered to provide the final estimate of the countermeasure effect, after the influence of other factors were taken into account. Its standard deviation was also estimated from the four individual standard deviations, thus allowing the statistical significance of the estimated countermeasure effect to be judged.

While the use of method 3B in Paper 5 aimed at overcoming the threat of "maturation" which method 1B was susceptible to, nevertheless it required two assumptions to be made. The first of these was that the functional form of the ARIMA models (including the step function form of the intervention term) was correct for the target group crash involvements and for each of the comparison groups. The diagnostic checking associated with the fitting of the ARIMA models during the pre-intervention period suggested that this assumption was reasonable, but was not necessarily water-tight.

The second assumption was that the effects of all time-related influential factors affecting the target group had been taken into account by parallel analysis of the comparison groups. A *post hoc* analysis described in the paper found that the number of novice drivers in Victoria increased substantially during the post-legislation period. Whether this resulted in a differential increase in exposure-to-risk during the alcohol times and non-alcohol times is unknown. (If not, the adjustment of the estimated intervention effect for novice drivers during alcohol times by the intervention effect estimated for non-alcohol times would take the

increase in exposure into account.) Nevertheless, a fundamental general assumption of time-related evaluation studies of this type, namely that there were no discontinuities in the exposure trend over the period analysed, is questionable in the case of Paper 5.

If monthly data on exposure levels of the target and comparison groups had been available throughout the period analysed, this factor could have been included in the time series models as a covariate in order that its specific influence on crashes could have been taken into account. Such an approach may use method 5B which is to be illustrated later in this thesis.

Table 7: Paper using time-related comparison method 3B: Intervention term in univariate time series model, adjusting for other factors by univariate analysis of comparison group

Paper No. and Identification	Description	Evaluation criteria	Expected level method	Other factors method
5. M.O. Haque and M.H. Cameron, "Effect of the Victorian Zero BAC legislation on serious casualty accidents : July 1984 - December 1985". <u>Journal of Safety Research</u> , Vol. 20, No. 3, Fall 1989. (Method 1)	Analysis of accident involvements of novice car drivers (the target group) who were subject to the zero blood alcohol content (BAC) requirement in Victoria from May 1984	Serious casualty accidents involving target drivers which occurred during "alcohol times" of the week in post-legislation period	Intervention term in ARIMA model of target group driver accidents during pre- and post-legislation periods	Change in target drivers' accidents during "non-alcohol times". Also compared with net change ("alcohol time" relative to "non-alcohol time") in accidents involving standard licensed drivers not subject to zero BAC

Method 4A: Extrapolation of multivariate time series model (including influential covariates) of pre-implementation levels

Papers no. 2, 10 and 14 have all used method 4A for time-related evaluation studies (Table 8). In each case, a multivariate time series model has been built incorporating at least one influential factor to explain the trend in the criterion variable during the period prior to the measure being implemented. In method 4A, the model is then extrapolated using values of the explanatory factors in the post-implementation period to estimate the expected value of the criterion variable during this period. Apart from those included in the model, the influence of other influential factors is not taken into account, for example, by inclusion of a comparison group in the analysis.

Paper 2 analyses vehicle occupant fatality trends in Victoria during years before and after the introduction of compulsory seat belt wearing legislation in December 1970 for the purpose of providing a base for evaluating other measures introduced during 1971-1974 (specifically the absolute speed limit introduced in 1974). Apart from a linear function against time, the model includes estimates of seat belt wearing rates actually achieved each year during 1971-1974, rates expected in the absence of the legislation, and the relative risk of death for seat belt non-wearers compared with wearers. The paper estimates the relative risk in two ways: (i) from a special survey of road deaths and hospitalisations matched with ambulance officers' reports, and (ii) by fitting the multivariate model to the pre- and post-legislation occupant fatality data.

Neither method was totally satisfactory from the point of view of the objective of evaluating the measures introduced during 1971-1974. The available data on seat belt wearing of killed occupants was probably not sufficiently reliable to provide accurate estimates of the relative risk of death using the first method. The second method was subject to bias in the estimate of the relative risk because the other measures introduced during 1971-1974 may have contributed to the apparent lower risk of being killed associated with the increased wearing rates. In addition, both methods involved the implicit assumption that increased seat belt wearing was the only factor affecting occupant fatality risks during 1971-1973. Later studies have shown this assumption to be naïve because a large number of Australian Design Rules for Motor Vehicle Safety aimed at occupant protection were applied to new cars sold during this period and have been shown to reduce the risk of driver death and serious injury substantially (Cameron et al 1995; Paper 17 in this thesis).

Paper 10 builds on the approach in Paper 2 to address a weakness of Paper 6 in that it did not make use of available data on a known influential factor (namely, helmet wearing rates) when developing a time series model of the criterion variable (proportion of injured bicyclists who sustained a head injury) during the period before the compulsory bicycle helmet wearing law commenced in Victoria. Helmet wearing rates had increased substantially over the years prior to the law and Paper 10 finds a statistically significant negative relationship between the wearing rate and the proportion head injured. A simple linear regression is fitted between these two variables and the estimated relationship is used to estimate the expected proportion head injured (with confidence limits) if the helmet wearing rate had not increased further from its immediate pre-legislation level as a result of the law.

As suggested in the preceding discussion on Paper 6, the inclusion of helmet wearing rate data as a covariate in the model for the proportion head injured was apparently beneficial to the sensitivity of the statistical test of the effect of the law. The confidence limits of the estimated proportion during the post-legislation year were sufficiently narrow that a statistically significant difference between it and the actual proportion was found. While Paper 10 represents a methodological improvement compared with Paper 6, it makes the inappropriate assumption when applying linear regression to the proportion head injured that the error terms are Normally distributed. A more appropriate method is used for this purpose in Paper 14.

Paper 14 extends the use of evaluation method 4A applied in Paper 10 by, first, developing a logistic regression model of helmet wearing rates against time, and, second, developing a logistic regression model of the proportion head injured against wearing rate. The first of these models is used to estimate the expected values of the covariate (wearing rate) during the post-legislation years in the absence of the law, under the assumption that it would have continued to increase to a limited extent. The second model is used to estimate the expected proportion head injured (with confidence limits) in the absence of the law, but using the modelled limited increase in wearing rates, for comparison with the actual proportions during the post-legislation years. The use of logistic regression models for rates/proportions bounded by zero and unity was a more appropriate choice of model form in this context.

However, neither Paper 10 nor Paper 14 overcomes the threat of "history" from other factors which was identified in the comments on Paper 6. In the absence of parallel analysis of a comparison group, evaluation method 4A cannot take into account the influence of other influential factors apart from those factors included in the multivariate time series model. Thus any other factor which started operating in Victoria at or about the same time as the bicycle helmet wearing law, and which reduced the bicyclist head injuries to a greater extent than injuries other than to the head (and hence reduced the proportion head injured), may be an alternative explanation for the apparent effect of the law. As discussed in the context of Paper 6, one possibility is the reduction in vehicle speeds associated with the expanded speed camera program in Victoria during 1990.

Table 8: Papers using time-related comparison method 4A: Extrapolation of multivariate time series model (including influential covariates) of pre-implementation levels

Paper No. and Identification	Description	Evaluation criteria	Expected level method	Other factors method
2. J.E. Cowley and M.H. Cameron, "Prediction of motor vehicle occupant fatality trends following seat belt wearing legislation". <u>Proceedings, Eighth Conference, Australian Road Research Board</u> , Perth 1976.	1. Model for occupant fatality trends as function of seat belt wearing rates and seat belt effectiveness 2. Estimation of wearing rates 3. Estimation of effectiveness by (a) special survey data (b) regression 4. Estimation of fatality trends 1971-1974	Driver and passenger fatality trends in Victoria, 1971-1974 [specifically 1974 when absolute speed limit introduced]	Model for occupant fatality trends as function of seat belt wearing rates and seat belt effectiveness	None [assumed that difference between actual and expected occupant fatalities in 1974 was due only to absolute speed limit and associated factors]
10. A.P. Vulcan, M.H. Cameron and L. Heiman, "Evaluation of mandatory bicycle helmet use in Victoria, Australia". <u>Proceedings, 36th Annual Conference of the Association for the Advancement of Automotive Medicine</u> , Portland, Oregon, October 1992.	1. Presentation of time series of helmet wearing rates 2. Linkage of pre-legislation wearing rates with proportion of killed and hospital admitted cyclists who sustained head injuries 3. Time series analysis of proportion with head injuries	Proportion with head injuries during the post-legislation period	Extrapolation of linear regression model of head injured proportion against wearing rate (the covariate), assuming pre-legislation rate continued in post-legislation period	None [other factors affecting head injury incidence were assumed to affect equally the incidence of cyclist death or hospital admission from injuries other than to the head]
14. M.H. Cameron, A.P. Vulcan, C.F. Finch and S.V. Newstead, "Mandatory bicycle helmet use following a decade of helmet promotion in Victoria, Australia - An evaluation". <u>Accident Analysis and Prevention</u> , Vol. 26, No. 3, 1994.	1. Time series analysis of pre- and post-legislation helmet wearing rates 2. Linkage of wearing rates with proportion of killed and hospital admitted cyclists who sustained head injuries 3. Time series analysis of proportion with head injuries	Proportion with head injuries during the post-legislation period	Extrapolation of logistic regression model of head injured proportion against wearing rate (the covariate), assuming pre-legislation trend in the rate continued in post-legislation period	None [other factors affecting head injury incidence were assumed to affect equally the incidence of cyclist death or hospital admission from injuries other than to the head]

Method 5A: Intervention term in multivariate time series model (including influential covariates)

Papers no. 11, 12, 13 and 15 have all used method 5A for time-related evaluation studies (Table 9). Each paper has developed multivariate time series models of a criterion variable (usually monthly road trauma level) as a function of a number of explanatory factors. The explanatory factors could include an intervention dummy variable representing the commencement of a countermeasure, or, as in the case of the four papers here, the explanatory factor of interest is a time-varying measure of the countermeasure operation. In either case, the magnitude, sign and statistical significance of the estimated coefficient of the countermeasure variable are the principal indicators of the effect of the countermeasure on the criterion variable when evaluation method 5A is used. Apart from those factors included in the multivariate model, the influence of other influential factors is not taken into account, for example, by inclusion of a comparison group in the analysis.

Paper 11 aimed to overcome the threat of "history" due to the omission of important influential factors by a systematic review of the key variables related to monthly fluctuations in road fatalities in Victoria. The "road trauma chain" (Figure 1 of this thesis) was used to identify relevant factors related to each step in the chain of events leading to road death. Available data limited the inclusion of some likely influential factors (eg. the Transport Accident Commission road safety advertising), but care was also taken to minimise collinearity problems by not including more than one factor measuring essentially the same influence.

Multivariate models were developed for monthly road fatalities during 1985-1990 for Victoria in total, and in three major road user type groups. The literature review indicated that a multiplicative functional form would be most appropriate (Hakim et al 1991), and this model was fitted by multiple linear regression after logarithm transformations of the dependent and independent variables. Initially, when the fatalities in relatively small sub-groups were considered, Poisson or Negative Binomial distributions were assumed for the fatality counts. After pooling the data for the final analysis, it was found that the ordinary least squares (OLS) method of regression produced comparable results, even though it involved the implicit assumption of Normally distributed error terms.

Because of the functional form of the models fitted, the regression coefficients were considered to be estimated "elasticities" for the corresponding factor (ie. the percentage change in expected monthly fatalities due to a 1% increase in the factor). This approach was used to estimate the effect of each included factor on the fatality reductions during 1990 (relative to 1989) due to its change between 1989 and 1990 (while holding each other factor constant). While no one road safety measure was the focus of this analysis, the procedure illustrates how evaluation method 5A is used to estimate the impact of a countermeasure when a time-related measure of the countermeasure operations is included in the multivariate time series model.

The fundamental assumptions underlying the application of evaluation method 5A include that the available covariates cover all important influential factors and that the functional form of the multivariate time series model (including the intervention effect of the

countermeasure under evaluation) is correct. Paper 11 includes evidence which indicates that the first of these assumptions is sound in this case. When the estimated impact of each of the countermeasures and other included factors during 1990 was added up, the total was only slightly greater (within 10%) of the actual total reduction in fatalities between 1989 and 1990. This indicates that the model includes all influential factors directly or by proxy (ie. other factors not included may have been so closely related to another included factor that the apparent effect of the factor in fact measures their combined effect). The second assumption is difficult to check (except by the standard diagnostic tests of the residuals from the statistical models), except to say that in this case the findings related to the first assumption were unlikely if the functional form had been wrong.

Paper 12 uses method 5A to evaluate the relative impact of the components of the speed camera program (including enforcement operations and supporting mass media publicity) introduced in Victoria in 1990. Method 5B had previously been used in the first part of the paper to measure the impact of the program as a whole (see later discussion in this thesis). Method 5A was then used in an attempt to understand the key mechanisms through which the program appeared to achieve its overall effect.

The criterion variables were the number of casualty crashes in Melbourne during the "low alcohol hours" of the week, and the severity of those crashes (defined as the proportion which resulted in death or serious injury). The low alcohol hours are the converse of the "high alcohol hours"³. Available monthly data on the speed camera program activity was limited to hours of camera operation, number of speeding tickets (Traffic Infringement Notices) issued for camera-detected speeding offences, level of Transport Accident Commission (TAC) road safety television advertising (measured by TARPs⁴) with a speeding theme, and level of advertising covering all safety themes. Monthly unemployment rate was also considered as a covariate following the finding that it was an important explanatory factor of variations in the criterion variables in the analysis in the first part of Paper 12.

Following the approach in Paper 11, multiplicative form models were fitted to monthly casualty crashes during 1983-1991, after logarithm transformations of the dependent and independent variables, by multiple linear regression using the OLS method. In contrast, the analysis of monthly crash severity recognised that this is a proportion bounded by zero and unity and used logistic multiple regression to fit models of the independent variables (after logarithm transformation).

While the paper finds relationships between the criterion variables and monthly speeding tickets, camera hours and advertising levels, the discussion of these findings points out that there were other hypothesised influential factors that could not be considered in the models. The absence of available data on potentially influential factors is an important limitation of

³ The "high alcohol hours" of the week (ie. 4pm Sunday to 6am Monday, 6pm to 6am on Monday to Thursday nights, 4pm Friday to 8am Saturday, and 2pm Saturday to 10am Sunday) were those periods when the percentage of drivers killed or admitted to hospital with a blood alcohol content exceeding 0.05% was about 38% during 1988-89 (Harrison 1990). The "low alcohol hours" are the converse of these periods, during which less than 4% of driver serious casualties had blood alcohol content exceeding 0.05%.

⁴ TARPs (Target Audience Rating Points) is the summation of the Rating Points (ie. the percentage of persons in the viewing area estimated to be watching the specific television channel at the time the advertisement was shown) for the particular Target Audience of the advertisement. It measures the intensity and audience reach of the advertising.

method 5A. However, in the context of Paper 12, it is possible that the monthly variations in the included covariates may have represented the variations in the missing factors to some extent. This indicates the desirability of a conceptual model of the process being modelled, at least at a general level, to assist decisions about the criticality of absent covariates.

In contrast with Paper 11, Paper 12 does include variables measuring levels of TAC road safety television advertising, which was hypothesised and found to be an important influential factor, at least so far as the speed camera program is concerned. There are theories which suggest that traffic enforcement and supporting publicity interact in a synergistic way to produce an effect on crashes which is greater than their separate effects would predict. The functional forms of the models used in Paper 12 do not include any representation of this potential interaction effect. For the functional form of the models to be considered correct, there is a need to assume that synergistic interactions are small or non-existent.

Paper 13 is based on the analysis approach and general context of Papers 11 and 12, except that focus is on the specific evaluation of the TAC road safety advertising. First, it reviews the previous evaluations of the general effects of the speed camera program and the random breath test "booze bus" initiative (see Paper 12, part 1, and Paper 9, respectively), highlighting the publicity activities supporting the Police enforcement in each case.

The paper then summarises the research in Paper 12 in which monthly "low alcohol hour" casualty crashes in Melbourne were linked with a number of factors, including levels of TAC road safety television advertising measured by TARPs. Paper 13 extends this analysis and finds that the strongest relationship (highest elasticity estimate) is that with TARPs achieved by advertising with either "speeding" or "concentration" themes. Following the focus of the paper, it displays the estimated functional relationship (assuming a multiplicative form) between the number of crashes and the level of advertising with these themes (while holding all other factors constant). This illustrates the use of method 5A to evaluate the effect of one specific countermeasure. Other countermeasures are included in the analysis because of their role as covariates, not because an evaluation of their effects is the principal objective.

The remainder of Paper 13 seeks relationships between advertising levels and monthly "high alcohol hour" serious casualty crashes in Melbourne and country Victoria separately. In this case the advertising levels are measured by "Adstock" (Broadbent 1979), a function of TARPs which represents the current and retained awareness of current and past advertising. Awareness of advertising messages is considered to be potentially the first step in changes in attitudes and behaviours, and perhaps reductions in road trauma related to the messages. The analysis follows the crash-based analysis in the second part of Paper 12 (and extended in the first part of Paper 13), except that monthly alcohol sales were considered as an additional socio-economic factor for the "high alcohol hour" crashes. The evaluation results are displayed as the estimated relationships between the number of crashes and the monthly Adstock of advertising with the "drink-driving" theme.

In its discussion section, Paper 13 cautions that the analysis methods have involved a number of assumptions. These are listed explicitly at the end of the paper, and include assumptions about the inclusion of all important influential factors as covariates, the correctness of the functional form of the multivariate time series model, and the functional form of the intervention effect of the countermeasure being evaluated (eg. the Adstock function of TARPs, and its parameters). Of the four papers which make use of evaluation method 5A,

Paper 13 makes the most explicit statements of the key assumptions on which it is based (see Table 2 of this thesis).

Paper 15 commences with summaries of evaluations of the general effects of the speed camera program, random breath test program, and a number of other road safety initiatives introduced in Victoria during 1989-1990. Evaluation method 5A is then used as the first step in producing estimates of the relative contributions of each of the key road safety programs and socio-economic factors to the substantial reductions in serious casualty crashes (in total) in Victoria during 1990-1992.

The first step is the fitting of multivariate time series models of monthly variations in serious casualty crashes during 1983-1992 in four strata: high alcohol hour crashes in Melbourne, high alcohol hour crashes in country Victoria, low alcohol hour crashes in Melbourne, and low alcohol hour crashes in country Victoria. These models use essentially the same methods and influential factors as covariates as used in Papers 12 and 13, except that the monthly TAC television advertising levels are represented by Adstock in each stratum. The regression coefficients (elasticity estimates) are the evaluation method 5A results, representing the estimated impact of a 1% increase in the specific road safety program variable to which the coefficient refers.

The second step uses a method developed by a co-author of Paper 15, Stuart Newstead, to take these evaluation results, estimate the total impact of each road safety program on crashes in the stratum due to the increase in the program activity, and then combine these estimates across the four strata to estimate the impact on all crashes in Victoria during each of the years 1990 to 1992 (Cameron and Newstead 1993a). This method is also applied to estimate the effects of the changes in the socio-economic factors included in the separate models. The second step illustrates how evaluation method 5A can be extended to provide an aggregate estimate of the effect of a countermeasure, after relationships have been found with a time-varying measure of the countermeasure operations.

While not said explicitly in Paper 15, its estimates of the countermeasure impacts are based on the same assumptions as Papers 11, 12 and 13. The paper does note that the interactions of enforcement and associated publicity levels were not considered in the models and could have been substantial. Assuming that such interactions were not present was a specific element of the general assumption regarding the functional form of the models. The need to make this and other assumptions, many of which cannot be adequately tested, limits the use of evaluation method 5A in practice, except in situations where another, preferred method cannot be used.

Table 9: Papers using time-related comparison method 5A: Intervention term in multivariate time series model (including influential covariates)

Paper No. and Identification	Description	Evaluation criteria	Expected level method	Other factors method
11. T. Thoresen, M.H. Cameron, T. Fry and L. Heiman, "Estimating the relative influences of accident countermeasures and socio-economic factors on the Victorian road toll 1985-1990". <u>Proceedings, 16th Australian Road Research Board Conference</u> , Perth, November 1992.	1. Development of time series regression models linking road fatalities with road safety measures, socio-economic factors, and seasonality 2. Estimation of contribution (impact) of each measure and factor to 1990 fatality reduction	Monthly road fatalities in Victoria, for all road users, and for vehicle occupants, pedestrians, and motorcyclists and bicyclists in particular	Coefficient of road safety measure (multiplied by 1989-1990 change) in multiple regression multiplicative model of fatalities 1985-1990	Influential socio-economic factors (and other road safety measures not the current focus of assessing their impact) were included in the model as covariates. Held constant in assessment of impact of specific measure
12. M.H. Cameron, A. Cavallo and A. Gilbert, "Crash-based evaluation of the speed camera program in Victoria 1990-1991. Phase 1: General effects. Phase 2: Effects of program mechanisms". Report No. 42, Monash University Accident Research Centre, ISBN 0 7326 0042 1, December 1992. (Method 2)	Linkage of crash outcomes in Melbourne with program measures of new speed cameras plus intensive mass media publicity	Casualty crashes, and their injury severity outcome, which occurred during the "low alcohol hours" of the week during 1983-1991	Coefficients of road safety measures in (b) multiple regression model of crashes, (c) logistic regression model of injury severity [unemployment rates as covariate in each case]	Influential socio-economic factor (unemployment rates) was included as covariate
13. M.H. Cameron and S.V. Newstead, "Evaluation of mass media publicity as support for enforcement". Invited paper in: <u>Proceedings, Australasian Drink-Drive Conference</u> , Melbourne, November 1993.	1. Extension of time series regression models found in paper no. 12 (part 2) for "low alcohol hour" crashes 2. Development of similar models linking "high alcohol hour" crashes with relevant road safety measures and socio-economic factors	1. Casualty crashes during LAHs in Melbourne during 1983-1991 2. Serious casualty crashes during HAHs in Melbourne and country Victoria during 1983-1992	Coefficient of road safety advertising measure in multiple regression multiplicative model of crashes	Influential socio-economic factors (and other road safety measures not the current focus of assessing their impact) were included in the model as covariates

Table 9 (cont.): Papers using time-related comparison method 5A: Intervention term in multivariate time series model (including influential covariates)

Paper No. and Identification	Description	Evaluation criteria	Expected level method	Other factors method
15. M.H. Cameron, S.V. Newstead and A.P. Vulcan, "Analysis of reductions in Victorian road casualties, 1989 to 1992". Proceedings, 17th Australian Road Research Board Conference, Gold Coast, August 1994.	1. Summary of previous evaluations of Victorian programs 2. Development of models for "low alcohol hour" serious casualty crashes 3. Estimation of contribution (impact) of each measure & factor to total crash reductions in Victoria during 1990 to 1992	Serious casualty crashes during HAHs and LAHs in Melbourne and country Victoria during 1990-1992 (four evaluation strata considered; final estimates aggregated)	Coefficient of each measure and factor (multiplied by its change relative to 1988) in each multiple regression model Multiplicative model of crashes 1983-1992. Estimated impacts aggregated over time of week and area of Victoria.	Influential socio-economic factors (and other road safety measures not the current focus of assessing their impact) were included in the model as covariates

Method 5B: Intervention term in multivariate time series model (including influential covariates), adjusting for other factors by multivariate analysis of comparison group

Papers no. 9, 12 (method 1) and 18 (method 1) have all used method 5B for time-related evaluation studies (Table 10). This method aims to overcome the principal weakness of method 5A (which must assume that all important influential factors have been included as covariates) by making use of a comparison group to adjust for the influence of unknown or unmeasured factors. However, because in practice the comparison group is selected because it is not influenced by the countermeasure under evaluation, evaluation method 5B does not need to seek relationships between the impact criteria and a measure of the countermeasure operation (unlike method 5A). Method 5B makes use of a dummy variable (or variables) representing the countermeasure intervention, the coefficient of which so far as the comparison group is concerned measures the influence of the factors not specifically included as covariates.

Paper 9 aims to evaluate the impact on crashes of a new random breath testing initiative (the use of high profile "booze buses" as testing stations) which was implemented initially in Melbourne in December 1989 and expanded into rural Victoria. This, together with state-wide mass media publicity about the new program, made it difficult to find within-Victoria comparison areas, so comparable regions of New South Wales were used instead (this state used only car-based random breath testing at the time of the study). The criterion variables were monthly fatal crashes and serious casualty crashes in each region of Victoria during the "high alcohol hours" of the week.

Victoria experienced an economic recession in the early 1990's and unemployment rates increased rapidly from about mid-1990. A milder economic recession was experienced in New South Wales and its unemployment rates began to increase about a year later. This important difference between the target and comparison areas used for the evaluation required a method to take this into account. Past research had suggested that unemployment rate is a good indicator of both the quantitative and qualitative (ie. propensity for high risk road use) dimensions of road travel. As a known and measured factor potentially related to "high alcohol hour" serious crashes, the monthly unemployment rates in the regions of the two states were considered as covariates in the multivariate time series models.

The multivariate models, incorporating step function intervention terms for December 1989, all 1990, and all 1991, were developed for the monthly crashes in each region (including those in New South Wales) during 1983-1991. Two time series analysis methods were used:

- (1) ARIMA Intervention Analysis, including unemployment rates as a covariate, and
- (2) Multiple regression analysis, including a linear trend component, monthly dummy variables, unemployment rates, and the three intervention terms.

Multiplicative functional forms were considered most appropriate, following the literature review and findings of Paper 11, so logarithm transformations of the dependent and independent variables were performed before the analysis methods (1) and (2) were applied.

The intervention effect found by fitting these models to the crashes in each region of Victoria was considered to be initial estimates of the impact of the "booze bus" initiative. The coefficient of the same type of intervention term fitted to the crashes in the corresponding

comparison region of New South Wales was considered to measure the effects of other unknown factors which may have operated at the same time as the initiative, in both Victoria and New South Wales. (These factors did not include changes in unemployment rates which, even though they increased close to the introduction of the initiative, were included as covariates in the analysis.) The final estimate of the impact of the initiative on crashes in Victoria was calculated by subtracting the coefficient of the New South Wales intervention term from the Victorian coefficient, thus taking into account any effects of the unknown factors.

A previous evaluation of the same initiative had considered a region of rural Victoria to be relatively untreated (during 1990) and suitable for use as a comparison area (Drummond, Sullivan and Cavallo 1992). Evaluation method 2A using univariate ARIMA models fitted to weekly crashes during the pre-implementation years was used to estimate the expected crashes in Melbourne and the treated rural region, in the absence of the initiative, during 1990. The use of multivariate ARIMA models in Paper 9, incorporating unemployment rates as a covariate, was potentially a more powerful approach. However, it was found that the specific ARIMA models fitted to the data were such that they adapted to the crash series one annual cycle after the initiative was implemented (ie. from December 1990) and produced under-estimates of the impact during 1991. For this reason, in Paper 9 the estimated impacts during 1991 derived from the multiple regression time series analysis approach were considered the more reliable estimates for that year compared with the ARIMA method.

A fundamental assumption in the use of evaluation method 5B is that the functional form of the multivariate time series model (including the form of the intervention term) is correct. In Paper 9, two different methods of multivariate time series analysis were used (though they were both based on essentially the same multiplicative model structure) and they produced similar results, at least for the estimated impacts during 1990 where they could be compared. The form of the intervention was modelled in a conservative way by separately representing the periods December 1989 (when the program was launched and "introductory" effects were expected), 1990 (before all rural Victoria was covered) and 1991 (when stronger effects in rural Victoria were expected; and were found in Paper 9 in comparison with 1990). This appears to be a useful strategy when the shape of the intervention effect over time is unknown or poorly understood. However, more complex intervention effects of random breath testing programs can be and have been modelled, involving initial step effects followed by exponential decay to a less extreme level (Henstridge, Homel and Mackay 1997).

However, unlike method 5A, method 5B is less reliant on the multivariate time series models including all of the important influential factors as covariates if valid conclusions are to be reached. While it is desirable that the models include the available influential variables, because their presence has the potential to reduce the unexplained variation in the models, the use of the comparison group in method 5B aims to take into account the influence of the variables which are not included. In the case of Paper 9, however, it was found that an available influential variable (unemployment rates) changed in different ways in the two regions being compared. Hence it was essential that this particular factor be used as a covariate in the analysis for valid conclusions to be reached. This also illustrates the need for evaluators to be proactive in checking that the target and comparison groups are similar, especially in regard to available relevant measures, and being prepared to take the differences into account in the analysis if they are not (Cameron 1995).

The first part of Paper 12 uses method 5B to evaluate the general effects of a new speed camera program in Victoria (including supporting mass media publicity) which was announced in December 1989 and the increased cameras gradually became operational during 1990. A major launch of the program took place in April 1990. As discussed in the previous section outlining the application of method 5A to the second part of Paper 12, the criterion variables were the number of casualty crashes in Melbourne during the "low alcohol hours" of the week, and the injury severity of those crashes. The analysis methods and comparison areas were identical to those used in Paper 9, except that logistic regression was used to fit the multivariate time series models to the crash injury severity criterion.

The criterion variables were constrained to "low alcohol hour" crashes because most of the speed camera activity took place during those times and because these crashes, involving relatively small proportions of drivers with illegal blood alcohol content, were relatively immune from the effects of the "booze bus" initiative which commenced in December 1989 (see Paper 9). It was not assumed that there was no effect of the speed camera program during the "high alcohol hours", but it was decided not to attempt to measure it. Given the times of speed camera operations, the effects were expected to be small, however any effects on "high alcohol hour" crashes may have been confounded with the apparent effects of the "booze bus" initiative which had been measured as described in Paper 9.

The choice of the form of the intervention terms reflected the staged introduction of the program after its announcement and the absence of knowledge about the expected effects of this relatively unique program (at least the manner and intensity of the camera operations, compared with that in other jurisdictions). Intervention effects were separately included in the time series models to represent December 1989 to March 1990 (the period after the announcement, when camera activity was still low), April 1990 to June 1990 (the period from the launch, when the publicity continued but camera activity remained low), and July 1990 onwards (after camera activity increased rapidly, and the supporting publicity remained at high levels). Introductory effects of new speeding initiatives, immediately following their announcement, have been found in previous research (Wagenaar, Streff and Schultz 1990). Subsequently the third intervention effect was split into two to separately measure the effect during March to December 1991 because New South Wales introduced its own speed camera program during March 1991. It was considered that method 5B evaluated the *relative* impact of the Victorian program compared with the New South Wales program during that period, whereas the intervention effects measured during the earlier periods were more likely to be absolute in nature.

As in Paper 9, a fundamental assumption behind the use of method 5B in the first part of Paper 12 is that the functional form of the multivariate time series models was correct. Again like Paper 9, this was confirmed somewhat (at least as far as the crash analysis was concerned) by noting that the two time series analysis methods produced essentially the same results for the estimated impacts of the program during the intervention periods in 1990. To a large extent the assumption of correct functional form relies on the satisfactory representation of reality which had been found, in previous research, to be provided by the multiplicative models (Hakim et al 1991, and Paper 11).

The first part of Paper 18 uses method 5B to evaluate the general effect of a program of increased random breath testing in country Victoria during November 1993 to December

1994. The localised effects of the program were subsequently evaluated, using method 1B, in the second part of Paper 18 and in Paper 20, as discussed earlier in this thesis.

The criterion variable was the monthly number of serious casualty crashes which occurred in country Victoria during the country "high alcohol hours" previously defined. In this study the comparison areas were Melbourne and country New South Wales (outside Sydney), but neither area was considered totally satisfactory for this purpose. Multivariate time series models with a multiplicative structure were fitted to the crash series during 1984-1994 using OLS multiple regression after logarithm transformations. Based on previous research, the covariates included the unemployment rates and an index of alcohol sales in each region. The intervention effect was represented by a step function commencing in November 1993.

The results are presented in Paper 18 as the estimated "intervention effect" in each region, calculated from the estimated coefficient of the step function. The paper stops short of completing method 5B by subtracting the Melbourne or country New South Wales "intervention effect" from the Victorian estimate. This was because of doubts about whether either region could adequately represent the influence of all unknown factors which would have affected crashes in country Victoria in the absence of the country random breath testing program. The emphasis of the research described in Paper 18 was on evaluating the localised effects of the program using the more definitive evaluation method 1B for this purpose. For this reason the first part of Paper 18 stopped short of stating any strong conclusions based on method 5B. This section of Paper 18 illustrates the need for the evaluator to be proactive in checking that the target and comparison groups are similar in terms of relevant measures (except for factors included as covariates, however) if the evaluation is heavily reliant on the use of method 5B.

Table 10: Papers using time-related comparison method 5B: Intervention term in multivariate time series model (including influential covariates), adjusting for other factors by multivariate analysis of comparison group

Paper No. and Identification	Description	Evaluation criteria	Expected level method	Other factors method
9. M.H. Cameron, A. Cavallo and G. Sullivan, "Evaluation of the random breath testing initiative in Victoria 1989-1991: Multivariate time series approach". Report No. 38, Monash University Accident Research Centre, ISBN 0 7326 0038 3, September 1992.	Time series analysis of crashes in Melbourne and rural Victoria to measure the impact of new random breath test (RBT) "booze buses" introduced in December 1989 together with intensive mass media supporting publicity	Fatal and serious casualty crashes which occurred during the "high alcohol hours" of the week during each of the years 1990 and 1991	1. For 1990, intervention term in ARIMA model of crashes in each Victorian area during pre- and post-implementation periods, including unemployment rates as covariate 2. For 1991, intervention term in multiple regression model of the same factors as 1.	Intervention term in same type of model of crashes in corresponding areas of New South Wales (Sydney & rural NSW). Adjustment of Victorian intervention estimates for the corresponding NSW estimates, to take into account factors not included as covariates.
12. M.H. Cameron, A. Cavallo and A. Gilbert, "Crash-based evaluation of the speed camera program in Victoria 1990-1991. Phase 1: General effects. Phase 2: Effects of program mechanisms". Report No. 42, Monash University Accident Research Centre, ISBN 0 7326 0042 1, December 1992. (Method 1)	Time series analysis of crash outcomes in Victorian areas to measure the impact of new speed cameras introduced in December 1989, plus intensive mass media publicity commencing April 1990	Casualty crashes, and their injury severity outcome, which occurred during the "low alcohol hours" of the week in relevant program implementation periods during 1989-1991	Intervention term in (a) ARIMA model of crashes, (b) multiple regression model of crashes, (c) logistic regression model of injury severity [unemployment rates as covariate in each case]	Intervention terms in same types of models of crashes and injury severity for corresponding areas of New South Wales. Adjustment of Victorian for NSW estimates.
18. M.H. Cameron, K. Diamantopoulou, N. Mullian and S. Gantzer, "Evaluation of the country random breath testing and publicity program in Victoria 1993-1994". Proceedings, ROADS 96 Conference, Christchurch, New Zealand, September 1996. (Method 1)	Time series analysis of crashes to measure general impact of country RBT program	Serious casualty crashes during HAHs in country Victoria during Nov 1993 to Dec 1994	Intervention term in multiple regression model of crashes 1984-1994 (with covariates)	Intervention terms in same types of models of crashes in Melbourne and country NSW

Economic analysis methods

Paper 16 differs from the previously described papers by outlining methods of economic analysis of the results of impact evaluations of road trauma countermeasures, and then applying the methods to the results given in Paper 15 (based on evaluation method 5A). The economic analysis methods are described in the context of injury prevention programs generally, but the concepts emanated from the road trauma field where they were more advanced in their application at the time.

Following the classification of previous evaluation methods in Tables 3-10, the evaluation criterion in economic analysis is the value of the road trauma savings found from an impact evaluation of the countermeasure. It is usual to value this benefit by weighting the road trauma savings by the average value, in monetary terms, for each unit of road trauma saved (Table 11). If the impact evaluation produced road trauma savings classified by, say, injury severity, then the average values for each class of road trauma should be used. The values assigned can depend upon a number of perspectives, as outlined in Paper 16. To complete the economic analysis, the value of the benefit needs to be comparable with the program costs. This usually means that the aim is to value both in monetary terms, and usually in terms of present values if the benefits and costs are spread over many years.

The expected level of the benefit, under the hypothesis that the countermeasure has no economic value (analogous to the hypothesis of no effect, used in impact evaluation), is the cost of the countermeasure program (Table 11). It is possible for an effective countermeasure to have no economic value, because the valued benefits do not exceed the costs. This is unusual among road trauma countermeasures because the unit values assigned to road trauma savings (even the most conservative unit values) usually result in the benefits far outweighing the costs. However, it is important that benefits and costs be assessed within global social systems because, in the context of some sub-systems, the benefits within the sub-system may be less than its costs. The presence or absence of transfer payments between sub-systems can lead to this type of conclusion.

Paper 16 builds on the results of impact evaluation of the overall effects (total reductions in serious casualty crashes over 1990-1993) due to the random breath testing "booze bus" and speed camera programs (including supporting mass media publicity) in Victoria (Paper 15). It is presumed in Paper 16 that the influence of other factors which may distort the estimate of the savings in crashes has been taken into account during the impact evaluation (Table 11). It is not usual during economic analysis to employ methods to overcome this threat to validity of the estimates; that is correctly left to the province of impact evaluation using the appropriate methods described earlier in this thesis.

Paper 16 illustrates the process of valuing the estimated savings in serious casualty crashes by their average social cost (based on the "human capital" method), which is considered to be a conservative approach to road trauma valuation (Bureau of Transport and Communications Economics; undated). In conjunction with Paper 15, it illustrates the complete countermeasure evaluation process of impact evaluation followed by economic analysis. However, to a large extent, its validity is dependent on the assumptions and analysis of Paper 15 rather than those made in Paper 16.

Table 11: Paper using economic analysis methods

Paper No. and Identification	Description	Evaluation criteria	Expected level method	Other factors method
16. M. H. Cameron, "Economic analysis - An essential tool". Invited paper in: Proceedings, First National Conference on Injury Prevention and Control, Sydney, February 1995.	1. Methods of valuing the injury savings measured in impact evaluations 2. Role of program costs and benefit-cost criteria in economic assessment of programs 3. Application of economic analysis to RBT and speed camera impacts measured as in paper no.15	Estimated savings in serious casualty crashes weighted by the average social cost per crash	Program costs	Not applicable [presumed that other factors have been taken into account in the impact evaluation of injury savings]

Conclusion

This thesis represents the development of the candidate's ideas in the field of statistical evaluation of road trauma countermeasures. Throughout the development, the aim has been to reach more definitive conclusions about the effectiveness of countermeasures. As well as presenting twenty papers that represent the development process, another aim of this thesis has been to create a framework of evaluation methods.

It is hoped that the framework has clarified the principles and assumptions behind each suggested method of impact evaluation based on ultimate or surrogate criteria. The aim was to clarify the threats to validity to which some of the simpler methods are exposed, and to illustrate methods that aim to overcome those threats. All methods involve assumptions, whether it be the absence of real threats or the functional forms of methods designed to cope with their presence, and this thesis aims to make them overt.

The breadth of road trauma countermeasures covered by the evaluation studies has been beneficial in generalising from them to develop the evaluation framework. The "road trauma chain" (Figure 1) is part of this framework. It suggests that impact evaluation is essentially an assessment of whether one or more risks in the chain have changed, no matter whether the risk lies in either the pre-crash or post-crash domain. There should be no conceptual barrier to the use of each evaluation method in either domain.

Realisation of this general principle has assisted the candidate to recognise the relevance of evaluation methods traditionally applied in one area to other areas of road safety. It is hoped that readers of this thesis will accept this principle for use in their own evaluation work.

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**APPENDIX A: Statements from co-authors
regarding the candidate's intellectual
contribution to co-authored papers**



AUSTRALIA

ACCIDENT RESEARCH CENTRE
Director: Professor A. P. Vulcan

6 January 1998

Ms A. Cavallo
Road Safety Department
VicRoads
60 Denmark Street
Kew VIC 3101

Dear Antonietta


CO-AUTHORSHIP OF PAPERS TO BE SUBMITTED FOR PhD

I have registered as a staff candidate for the degree of Doctor of Philosophy at Monash University where, as you know, I am working as a seconded Senior Research Fellow in the Accident Research Centre. I plan to submit a thesis based on a selection of my published and unpublished papers, some of which were written in conjunction with co-authors, including yourself. The field of study for my thesis is "Statistical evaluation of road trauma countermeasures".

I have identified 20 papers representing my development during the last 27 years of the theoretical concepts for undertaking scientific and conclusive research in the field. Most of these developments have emanated from the challenges faced in evaluating the major road safety initiatives in Victoria, such as random breath testing, speed cameras and mass media advertising, but some have emanated from earlier and concurrent work on other initiatives.

The University regulations allow the thesis to contain joint or multi-authored papers, provided they are prefaced by a signed statement disclosing the respective contributions by the candidate and the other authors. I am writing to ask you to provide such a statement regarding my proportional contribution to the intellectual development of the theoretical concepts on which the following papers were based:

Paper identification	Max Cameron's intellectual contribution to theoretical concepts		Co-author's signature	Co-author identification (name, address, telephone number - please print)
	Suggested proportion	Co-author's estimate		
M.H. Cameron, A. Cavallo and G. Sullivan, "Evaluation of the random breath testing initiative in Victoria 1989-1991: Multivariate time series approach". Report No. 38, Monash University Accident Research Centre, September 1992.	90%	90%		ANTONIETTA CAVALLO VICROADS 4th floor 60 Denmark St Kew VIC 3101

Paper identification	Max Cameron's intellectual contribution to theoretical concepts		Co-author's signature	Co-author identification (name, address, telephone number - please print)
	Suggested proportion	Co-author's estimate		
M.H. Cameron, A. Cavallo and A. Gilbert, "Crash-based evaluation of the speed camera program in Victoria 1990-1991. Phase 1: General effects. Phase 2: Effects of program mechanisms". Report No. 42, Monash University Accident Research Centre, December 1992.	90%	90%		Antoniella Cavallo Vic Roads 60 Denmark St. Kew VIC 3101

Thank you for your assistance in providing this statement on my behalf. It was a pleasure to work with you and I hope my estimates of my contributions to the intellectual content are in accordance with your recollection. Please feel free to contact me on (03) 9905 4373 at Monash University or on (03) 9877 3862 at home if you wish to discuss the matter.

It would be appreciated if you could return the original of this letter, with your estimate, signature and identification details completed, to me at your earliest convenience.

Yours sincerely



Max Cameron
Senior Research Fellow



AUSTRALIA

ACCIDENT RESEARCH CENTRE
Director: Professor A. P. Vulcan

6 January 1998

Mr J. E. Cowley



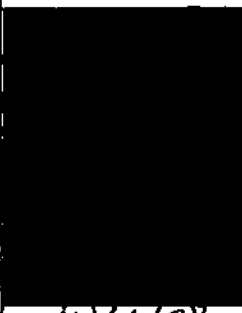

Dear Jim

CO-AUTHORSHIP OF PAPER TO BE SUBMITTED FOR PhD

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	Suggested proportion	Co-author's estimate		
J.E. Cowley and M.H. Cameron, "Prediction of motor vehicle occupant fatality trends following seat belt wearing legislation". <u>Proceedings, Eighth Conference, Australian Road Research Board, Perth 1976.</u>	50%	50%	 10/1/98.	 07 5495 8211. 07 5496 7706.

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Yours sincerely

A solid black rectangular box used to redact the signature of Max Cameron.

Max Cameron
Senior Research Fellow



ACCIDENT RESEARCH CENTRE
Director: Professor A. P. Vulcan

6 January 1998

Ms K. Diamantopoulou
Research Fellow
Accident Research Centre
Monash University
Clayton VIC 3168


Dear Kathy

CO-AUTHORSHIP OF PAPER TO BE SUBMITTED FOR PhD

I have registered as a staff candidate for the degree of Doctor of Philosophy at Monash University where, as you know, I am working as a seconded Senior Research Fellow in the Accident Research Centre. I plan to submit a thesis based on a selection of my published and unpublished papers, some of which were written in conjunction with co-authors, including yourself. The field of study for my thesis is "Statistical evaluation of road trauma countermeasures".

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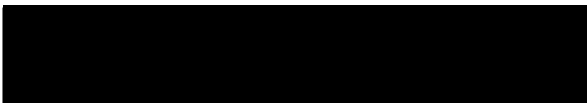
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Paper identification	Max Cameron's intellectual contribution to theoretical concepts		Co-author's signature	Co-author identification (name, address, telephone number - please print)
	Suggested proportion	Co-author's estimate		
M.H. Cameron, K. Diamantopoulou, N. Mullan and S. Gantzer, "Evaluation of the country random breath testing and publicity program in Victoria 1993-1994". <u>Proceedings, ROADS 96 Conference</u> , Christchurch, New Zealand, September 1996.	80%	80%		KATHY DIAMANTOPOULOU ACCIDENT RESEARCH CENTRE MONASH UNIVERSITY WELLINGTON ROAD CLAYTON, VIC 3168 Ph: (03) 9905 4378

Thank you for your assistance in providing this statement on my behalf. It was a pleasure to work with you and I hope my estimate of my contribution to the intellectual content is in accordance with your recollection. Please feel free to contact me on (03) 9905 4373 at Monash University or on (03) 9877 3862 at home if you wish to discuss the matter.

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Yours sincerely

A solid black rectangular box used to redact the signature of Max Cameron.

Max Cameron
Senior Research Fellow



28 August 1998

Ms K. Diamantopoulou
Research Fellow
Accident Research Centre
Monash University
Clayton VIC 3168

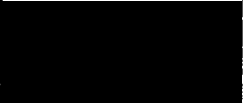
Dear Kathy

CO-AUTHORSHIP OF PAPER TO BE SUBMITTED FOR PhD

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Paper identification	Max Cameron's intellectual contribution to theoretical concepts		Co-author's signature	Co-author identification (name, address, telephone number - please print)
	Suggested proportion	Co-author's estimate		
K. Diamantopoulou and M.H. Cameron, "Localised effects on crashes of the country random breath testing and publicity program in Victoria". <u>Proceedings, 19th ARRB Transport Research Conference, Investing in Transport</u> , Sydney, December 1998.	. 75%	75%		KATHY DIAMANTOPOULOU ACCIDENT RESEARCH CENTRE MONASH UNIVERSITY WELLINGTON ROAD CLAYTON, 3168 VIC. Ph: (03) 9905-4378

Thank you for your assistance in providing this statement on my behalf. It was a pleasure to work with you and I hope my estimate of my contribution to the intellectual content is in accordance with your recollection. Please feel free to contact me on (03) 9905 4373 at Monash University or on (03) 9877 3862 at home if you wish to discuss the matter.

It would be appreciated if you could return the original of this letter, with your estimate, signature and identification details completed, to me at your earliest convenience.

Yours sincerely

A solid black rectangular box used to redact the signature of Max Cameron.

Max Cameron
Senior Research Fellow



AUSTRALIA

ACCIDENT RESEARCH CENTRE
Director: Professor A. P. Vulcan

6 January 1998

Dr C. F. Finch
School of Human Movement
Deakin University
221 Burwood Highway
Burwood VIC 3125


Dear Caroline


CO-AUTHORSHIP OF PAPERS TO BE SUBMITTED FOR PhD

I have registered as a staff candidate for the degree of Doctor of Philosophy at Monash University where, as you know, I am working as a seconded Senior Research Fellow in the Accident Research Centre. I plan to submit a thesis based on a selection of my published and unpublished papers, some of which were written in conjunction with co-authors, including yourself. The field of study for my thesis is "Statistical evaluation of road trauma countermeasures".

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Paper identification	Max Cameron's intellectual contribution to theoretical concepts		Co-author's signature	Co-author identification (name, address, telephone number - please print)
	Suggested proportion	Co-author's estimate		
M.H. Cameron, A.P. Vulcan, C.F. Finch and S.V. Newstead, "Mandatory bicycle helmet use following a decade of helmet promotion in Victoria, Australia - An evaluation". <u>Accident Analysis and Prevention</u> , Vol. 26, No. 3, 1994.	80%	80%		Dr Caroline Finch. School of Human Movement Deakin University 221 Burwood Highway Burwood 3125 Ph: 03 9251 7084

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It would be appreciated if you could return the original of this letter, with your estimate, signature and identification details completed, to me at your earliest convenience.

Yours sincerely



Max Cameron
Senior Research Fellow



ACCIDENT RESEARCH CENTRE
Director: Professor A. P. Vulcan

6 January 1998

Dr T. Fry
Faculty of Econometrics
Monash University
Clayton VIC 3168

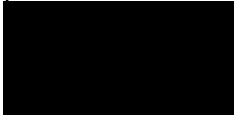
Dear Tim

CO-AUTHORSHIP OF PAPER TO BE SUBMITTED FOR PhD

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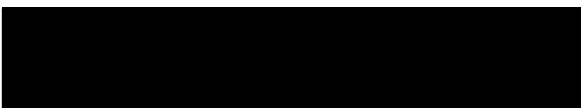
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T. Thoresen, M.H. Cameron, T. Fry and L. Heiman, "Estimating the relative influences of accident countermeasures and socio-economic factors on the Victorian road toll 1985-1990". <u>Proceedings, 16th Australian Road Research Board Conference, Perth, November 1992.</u>	50%	50%		Dr. T.R.L. Fry Department of Econometrics and Business Statistics, Monash University, Clayton, Vic. 3168 (03) 9905 2415

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Max Cameron
Senior Research Fellow



AUSTRALIA

ACCIDENT RESEARCH CENTRE
Director: Professor A. P. Vulean

6 January 1998

Ms S. Gantzer
Transport Accident Commission
GPO Box 2751Y
Melbourne VIC 3001



Dear Sandra

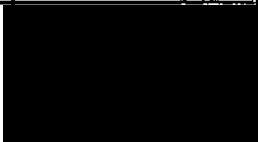
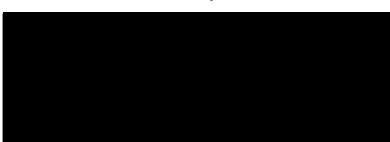
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M.H. Cameron, S.V. Newstead and S. Gantzer, "Effects of enforcement and supporting publicity programs in Victoria, Australia". Proceedings of Conference, Road Safety in Europe and Strategic Highway Research Program, Prague, Czech Republic, September 1995.	90%	90%		SANDRA GANTZER  BH: 0664 6853 AH: 9720 9226

Paper identification	Max Cameron's intellectual contribution to theoretical concepts		Co-author's signature	Co-author identification (name, address, telephone number - please print)
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M.H. Cameron, K. Diamantopoulou, N. Mullan and S. Gantzer, "Evaluation of the country random breath testing and publicity program in Victoria 1993-1994". <u>Proceedings, ROADS 96 Conference</u> , Christchurch, New Zealand, September 1996.	80%	80%		SANDRA GANTZER  BH: 9664 6353 AH: 9720 9226

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Yours sincerely



Max Cameron
Senior Research Fellow



AUSTRALIA

ACCIDENT RESEARCH CENTRE
Director: Professor A. P. Vulcan

6 January 1998

Dr M. O. Haque



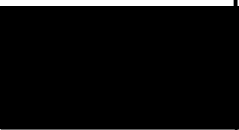
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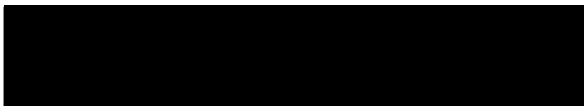
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Paper identification	Max Cameron's intellectual contribution to theoretical concepts		Co-author's signature	Co-author identification (name, address, telephone number - please print)
	Suggested proportion	Co-author's estimate		
M.O. Haque and M.H. Cameron, "Effect of the Victorian Zero BAC legislation on serious casualty accidents : July 1984 - December 1985". <u>Journal of Safety Research</u> , Vol. 20, No. 3, Fall 1989.	66%	50%		DR. MOHAMMED OHIDUL HAQUE 9 COLLIER COURT BURWOOD, VIC - 3125 Ph. 9808-5013(W) 9248-1042(W)

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Max Cameron
Senior Research Fellow



AUSTRALIA

ACCIDENT RESEARCH CENTRE
Director: Professor A. P. Vulcan

6 January 1998

Ms N. Mullan
Road Accident Prevention Research Unit
Department of Public Health
University of Western Australia
Nedlands WA 6907


Dear Narelle

CO-AUTHORSHIP OF PAPER TO BE SUBMITTED FOR PhD

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Max Cameron
Senior Research Fellow



ACCIDENT RESEARCH CENTRE
Director: Professor A. P. Vulcan

6 January 1998

Ms D. Neiger
ABS Statistical Support Section
GPO Box 2796Y
Melbourne VIC 3001


Dear Dina


CO-AUTHORSHIP OF PAPERS TO BE SUBMITTED FOR PhD

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M.H. Cameron, T. Mach, D. Neiger, A. Graham, R. Ramsay, M. Pappas and J. Haley, "Vehicle crashworthiness ratings from Victoria and New South Wales crash data". <u>Road and Transport Research</u> , Vol. 1, No. 3, September 1992.	95%	95%		DINA NEIGER Statistical Support Section ABS GPO BOX 2796Y MELBOURNE VIC 3001 03 9615 7406

Paper identification	Max Cameron's intellectual contribution to theoretical concepts		Co-author's signature	Co-author identification (name, address, telephone number - please print)
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M.H. Cameron, T. Mach, D. Neiger, A. Graham, R. Ramsay, M. Pappas and J. Haley, "Vehicle crashworthiness ratings in Australia". <u>Proceedings, International Conference on the Biomechanics of Impacts</u> , Verona, Italy, September 1992. Also in: <u>Accident Analysis and Prevention</u> , Vol. 26, No. 4, 1994	95%	95%.		DINA NEIGER STATISTICAL SUPPORT SECTION ABS 6PD BOX 2796Y MELBOURNE VIC 3001 03 9615 7406

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Yours sincerely



Max Cameron
Senior Research Fellow



ACCIDENT RESEARCH CENTRE
Director: Professor A. P. Vulcan

6 January 1998

Mr S. V. Newstead
Research Fellow
Accident Research Centre
Monash University
Clayton VIC 3168

Dear Stuart

CO-AUTHORSHIP OF PAPERS TO BE SUBMITTED FOR PhD


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Paper identification	M.H. Cameron's intellectual contribution to theoretical concepts		Co-author's signature	Co-author identification (name, address, telephone number - please print)
	Suggested proportion	Co-author's estimate		
M.H. Cameron and S.V. Newstead, "Evaluation of mass media publicity as support for enforcement". Proceedings, Australasian Drink-Drive Conference, Melbourne, November 1993.	90%	90%		STUART NEWSTEAD ACCIDENT RESEARCH CENTRE MONASH UNIVERSITY CLAYTON, 3168 PH: 9905-4364

Paper identification	Max Cameron's intellectual contribution to theoretical concepts		Co-author's signature	Co-author identification (name, address, telephone number - please print)
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M.H. Cameron, A.P. Vulcan, C.F. Finch and S.V. Newstead, "Mandatory bicycle helmet use following a decade of helmet promotion in Victoria, Australia - An evaluation". <u>Accident Analysis and Prevention</u> , Vol. 26, No. 3, 1994.	80%	80%		STUART NEWSTEAD ACCIDENT RESEARCH CENTRE MONASH UNIVERSITY CLAYTON, 3168 PH: 9905-4364
M.H. Cameron, S.V. Newstead and A.P. Vulcan, "Analysis of reductions in Victorian road casualties, 1989 to 1992". <u>Proceedings, 17th Australian Road Research Board Conference</u> , Gold Coast, August 1994.	80%	80%		STUART NEWSTEAD ACCIDENT RESEARCH CENTRE MONASH UNIVERSITY CLAYTON, 3168 PH: 9905-4364
M.H. Cameron, C.F. Finch, S.V. Newstead, T. Le, A. Graham, M. Griffiths, M. Pappas and J. Haley, "Measuring crashworthiness: Make/model ratings and the influence of Australian Design Rules for motor vehicle safety". <u>Proceedings, International IRCOBI Conference on the Biomechanics of Impact</u> , Brunnen, Switzerland, September 1995.	80%	80%		STUART NEWSTEAD ACCIDENT RESEARCH CENTRE MONASH UNIVERSITY CLAYTON, 3168 PH: 9905-4364
M.H. Cameron, S.V. Newstead and S. Gantzer, "Effects of enforcement and supporting publicity programs in Victoria, Australia". <u>Proceedings of Conference, Road Safety in Europe and Strategic Highway Research Program</u> , Prague, Czech Republic, September 1995.	90%	90%		STUART NEWSTEAD ACCIDENT RESEARCH CENTRE MONASH UNIVERSITY CLAYTON, 3168. PH: 9905-4364

Paper identification	Max Cameron's intellectual contribution to theoretical concepts		Co-author's signature	Co-author identification (name, address, telephone number - please print)
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M.H. Cameron and S.V. Newstead, "Mass media publicity supporting police enforcement and its economic value". Invited paper in: Proceedings, Symposium on Mass Media Campaigns in Road Safety, Public Health Association of Australia, Perth, September 1996.	95%	95%.		STUART NEWSTEAD ACCIDENT RESEARCH CENTRE. MONASH UNIVERSITY SWAYTON, 3168 PH: 9905-4364.

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Yours sincerely



Max Cameron
Senior Research Fellow



28 August 1998

Mr S. V. Newstead
Research Fellow
Accident Research Centre
Monash University
Clayton VIC 3168

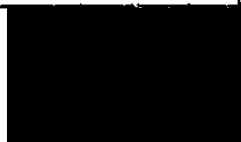

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M.H. Cameron, S.V. Newstead and C.M. Le, "Rating the aggressivity of Australian passenger vehicles towards other vehicle occupants and unprotected road users". <u>Proceedings, International IRCOBI Conference on the Biomechanics of Impact, September 1998.</u>	80%	80%		STUART NEWSTEAD  PH: (03) 9879-3697

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Max Cameron
Senior Research Fellow



ACCIDENT RESEARCH CENTRE
Director: Professor A. P. Vulcan

6 January 1998

Mr P. M. Strang
[REDACTED]

Dear Peter

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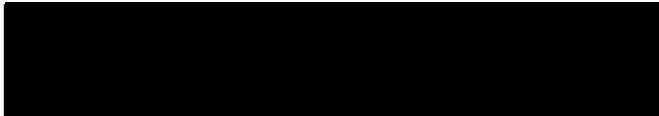
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M.H. Cameron and P.M. Strang, "Effect of intensified random breath testing in Melbourne during 1978 and 1979". <u>Proceedings, Eleventh Conference, Australian Road Research Board, August 1982.</u>	75%	75%	[REDACTED]	Peter McKenzie Strang TEL: (02) 62706707 (W) : (02) 62491181 (H)

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Max Cameron
Senior Research Fellow



AUSTRALIA

7 JAN 1998

ACCIDENT RESEARCH CENTRE
Director: Professor A. P. Vulcan

6 January 1998

Mr T. Thoresen
ARRB Transport Research Ltd.
500 Burwood Highway
Nunawading VIC 3133

Dear Thorolf

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Note: My involvement was concentrated at the data collection and modelling stages.

Paper identification	Max Cameron's intellectual contribution to theoretical concepts		Co-author's signature	Co-author identification (name, address, telephone number - please print)
	Suggested proportion	Co-author's estimate		
T. Thoresen, M.H. Cameron, T. Fry and L. Heiman, "Estimating the relative influences of accident countermeasures and socio-economic factors on the Victorian road toll 1985-1990". <u>Proceedings, 16th Australian Road Research Board Conference, Perth, November 1992.</u>	50%	50%		Thorolf Thoresen Principal Economist ARRB Transport Research 500 Burwood Highway Vermont South 3133

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Max Cameron
Senior Research Fellow



AUSTRALIA

ACCIDENT RESEARCH CENTRE
Director: Professor A. P. Vulcan

6 January 1998

Professor A. P. Vulcan
Director
Accident Research Centre
Monash University
Clayton VIC 3168

Dear Peter

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	Suggested proportion	Co-author's estimate		
A.P. Vulcan, M.H. Cameron and W.L. Watson, "Mandatory bicycle helmet use: experience in Victoria, Australia". Special issue on "Road Trauma and its Prevention", <u>World Journal of Surgery</u> , No. 16, pp. 389-397, 1992.	66% of evaluation component of paper	66%		Prof. A. P. VULCAN ACCIDENT RESEARCH CENTRE MONASH UNIVERSITY CLAYTON, 3168

Paper identification	Max Cameron's intellectual contribution to theoretical concepts		Co-author's signature	Co-author identification (name, address, telephone number - please print)
	Suggested proportion	Co-author's estimate		
A.P. Vulcan, M.H. Cameron and L. Heiman, "Evaluation of mandatory bicycle helmet use in Victoria, Australia". <u>Proceedings, 36th Annual Conference of the Association for the Advancement of Automotive Medicine</u> , Portland, Oregon, October 1992.	75% of evaluation component of paper	75%		A.P. VULCAN. AS ABOVE
M.H. Cameron, A.P. Vulcan, C.F. Finch and S.V. Newstead, "Mandatory bicycle helmet use following a decade of helmet promotion in Victoria, Australia - An evaluation". <u>Accident Analysis and Prevention</u> , Vol. 26, No. 3, 1994.	80%	80%		A.P. VULCAN. AS ABOVE
M.H. Cameron, S.V. Newstead and A.P. Vulcan, "Analysis of reductions in Victorian road casualties, 1989 to 1992". <u>Proceedings, 17th Australian Road Research Board Conference</u> , Gold Coast, August 1994.	80%	80%		A.P. VULCAN AS ABOVE

Thank you for your assistance in providing this statement on my behalf. It was a pleasure to work with you and I hope my estimates of my contributions to the intellectual content are in accordance with your recollection. Please feel free to contact me on (03) 9905 4373 at Monash University or on (03) 9877 3862 at home if you wish to discuss the matter.

It would be appreciated if you could return the original of this letter, with your estimate, signature and identification details completed, to me at your earliest convenience.

Yours sincerely

[Redacted signature]

Max Cameron
Senior Research Fellow



ACCIDENT RESEARCH CENTRE
Director: Professor A. P. Vulcan

6 January 1998

Ms W. L. Watson
Research Fellow
Accident Research Centre
Monash University
Clayton VIC 3168


Dear Wendy

CO-AUTHORSHIP OF PAPER TO BE SUBMITTED FOR PhD

I have registered as a staff candidate for the degree of Doctor of Philosophy at Monash University where, as you may recall, I am working as a seconded Senior Research Fellow in the Accident Research Centre. I plan to submit a thesis based on a selection of my published and unpublished papers, some of which were written in conjunction with co-authors, including yourself. The field of study for my thesis is "Statistical evaluation of road trauma countermeasures".

I have identified 20 papers representing my development during the last 27 years of the theoretical concepts for undertaking scientific and conclusive research in the field. Most of these developments have emanated from the challenges faced in evaluating the major road safety initiatives in Victoria, such as random breath testing, speed cameras and mass media advertising, but some have emanated from earlier and concurrent work on other initiatives.

The University regulations allow the thesis to contain joint or multi-authored papers, provided they are prefaced by a signed statement disclosing the respective contributions by the candidate and the other authors. I am writing to ask you to provide such a statement regarding my proportional contribution to the intellectual development of the theoretical concepts on which the following paper was based:

Paper identification	Max Cameron's intellectual contribution to theoretical concepts		Co-author's signature	Co-author identification (name, address, telephone number - please print)
	Suggested proportion	Co-author's estimate		
A.P. Vulcan, M.H. Cameron and W.L. Watson, "Mandatory bicycle helmet use: experience in Victoria, Australia". Special issue on "Road Trauma and its Prevention", <u>World Journal of Surgery</u> , No. 16, pp. 389-397, 1992.	66% of evaluation component of paper	66% of evaluation component of paper		WENDY WATSON MONASH UNIVERSITY ACCIDENT RESEARCH CENTRE (BUILDING 70) WELLINGTON RD. CLAYTON 9905 1813

Thank you for your assistance in providing this statement on my behalf. It was a pleasure to work with you and I hope my estimate of my contribution to the intellectual content is in accordance with your recollection. Please feel free to contact me on (03) 9905 4373 at Monash University or on (03) 9877 3862 at home if you wish to discuss the matter.

It would be appreciated if you could return the original of this letter, with your estimate, signature and identification details completed, to me at your earliest convenience.

Yours sincerely

A solid black rectangular box used to redact the signature of Max Cameron.

Max Cameron
Senior Research Fellow

APPENDIX B: Summaries of the papers in chronological order

IDENTIFICATION NUMBERS OF PAPERS IN THIS THESIS

(Peer review journals and conference proceedings are underlined)

1. M.H. Cameron, "Accident rate analysis and confidence limits". Proceedings, Fifth Conference, Australian Road Research Board, Canberra, August 1970.
2. J.E. Cowley and M.H. Cameron, "Prediction of motor vehicle occupant fatality trends following seat belt wearing legislation". Proceedings, Eighth Conference, Australian Road Research Board, Perth 1976.
3. M.H. Cameron, "The effect of seat belts on minor and severe injuries measured on the Abbreviated Injury Scale". Proceedings, Eighth International Conference of the International Association for Accident and Traffic Medicine, Aarhus, Denmark, June 1980. Also in Accident Analysis and Prevention, Vol. 13, no. 1, March 1981.
4. M.H. Cameron and P.M. Strang, "Effect of intensified random breath testing in Melbourne during 1978 and 1979". Proceedings, Eleventh Conference, Australian Road Research Board, August 1982.
5. M.O. Haque and M.H. Cameron, "Effect of the Victorian Zero BAC legislation on serious casualty accidents : July 1984 - December 1985". Journal of Safety Research, Vol. 20, No. 3, Fall 1989.
6. A.P. Vulcan, M.H. Cameron and W.L. Watson, "Mandatory bicycle helmet use: experience in Victoria, Australia". Special issue on "Road Trauma and its Prevention", World Journal of Surgery, No. 16, pp. 389-397, 1992.
7. M.H. Cameron, T. Mach, D. Neiger, A. Graham, R. Ramsay, M. Pappas and J. Haley, "Vehicle crashworthiness ratings in Australia". Proceedings, International Conference on the Biomechanics of Impacts, Verona, Italy, September 1992. Also in: Accident Analysis and Prevention, Vol. 26, No. 4, 1994.
8. M.H. Cameron, T. Mach, D. Neiger, A. Graham, R. Ramsay, M. Pappas and J. Haley, "Vehicle crashworthiness ratings from Victoria and New South Wales crash data". Road and Transport Research, Vol. 1, No. 3, September 1992.
9. M.H. Cameron, A. Cavallo and G. Sullivan, "Evaluation of the random breath testing initiative in Victoria 1989-1991: Multivariate time series approach". Report No. 38, Monash University Accident Research Centre, ISBN 0 7326 0038 3, September 1992.
10. A.P. Vulcan, M.H. Cameron and L. Heiman, "Evaluation of mandatory bicycle helmet use in Victoria, Australia". Proceedings, 36th Annual Conference of the Association for the Advancement of Automotive Medicine, Portland, Oregon, October 1992.

11. T. Thoresen, M.H. Cameron, T. Fry and L. Heiman, "Estimating the relative influences of accident countermeasures and socio-economic factors on the Victorian road toll 1985-1990". Proceedings, 16th Australian Road Research Board Conference, Perth, November 1992.
12. M.H. Cameron, A. Cavallo and A. Gilbert, "Crash-based evaluation of the speed camera program in Victoria 1990-1991. Phase 1: General effects. Phase 2: Effects of program mechanisms". Report No. 42, Monash University Accident Research Centre, ISBN 0 7326 0042 1, December 1992.
13. M.H. Cameron and S.V. Newstead, "Evaluation of mass media publicity as support for enforcement". Invited paper in: Proceedings, Australasian Drink-Drive Conference, Melbourne, November 1993.
14. M.H. Cameron, A.P. Vulcan, C.F. Finch and S.V. Newstead, "Mandatory bicycle helmet use following a decade of helmet promotion in Victoria, Australia - An evaluation". Accident Analysis and Prevention, Vol. 26, No. 3, 1994.
15. M.H. Cameron, S.V. Newstead and A.P. Vulcan, "Analysis of reductions in Victorian road casualties, 1989 to 1992". Proceedings, 17th Australian Road Research Board Conference, Gold Coast, August 1994.
16. M. H. Cameron, "Economic analysis - An essential tool". Invited paper in: Proceedings, First National Conference on Injury Prevention and Control, Sydney, February 1995.
17. M.H. Cameron, C.F. Finch, S.V. Newstead, T. Le, A. Graham, M. Griffiths, M. Pappas and J. Haley, "Measuring crashworthiness: Make/model ratings and the influence of Australian Design Rules for motor vehicle safety". Proceedings, International IRCOBI Conference on the Biomechanics of Impact, Brunnen, Switzerland, September 1995.
18. M.H. Cameron, K. Diamantopoulou, N. Mullan and S. Gantzer, "Evaluation of the country random breath testing and publicity program in Victoria 1993-1994". Proceedings, ROADS 96 Conference, Christchurch, New Zealand, September 1996.
19. M.H. Cameron, S.V. Newstead and C.M. Le, "Rating the aggressivity of Australian passenger vehicles towards other vehicle occupants and unprotected road users". Proceedings, International IRCOBI Conference on the Biomechanics of Impact, Gothenborg, Sweden, September 1998. Also in: Journal of Crash Prevention and Injury Control, Vol. 1, No. 2, 1999.
20. K. Diamantopoulou and M.H. Cameron, "Localised effects on crashes of the country random breath testing and publicity program in Victoria". Proceedings, 19th ARRB Transport Research Conference, Investing in Transport, Sydney, December 1998.

Summaries of the papers in chronological order

Paper No.	Description	Evaluation criteria	Expected level method	Other factors method	Type	Notes
1	1. Poisson model for accident process 2. Method for contingency table analysis of accident rates 3. Statistical testing errors for large data sets 4. Method for detecting specific cells with significantly different risks 5. Confidence limits for risk	No. of drivers involved in reported accidents in Brisbane, 1961	Exposure adjustment: Mean daily miles driven, estimated from a travel survey	1. When driver age was analysed, driver sex was taken into account by categorisation 2. Vice versa when driver sex was analysed 3. Their interaction was also tested	0B+	Illustrates limitations of categorisation for taking other factors into account (can't make use of value of interval or ordinal scale; sub-divides the data)
2	1. Model for occupant fatality trends as function of seat belt wearing rates and seat belt effectiveness 2. Estimation of wearing rates 3. Estimation of effectiveness by (a) special survey data (b) regression 4. Estimation of fatality trends 1971-1974	Driver and passenger fatality trends in Victoria, 1971-1974 [specifically 1974 when absolute speed limit introduced]	Model for occupant fatality trends as function of seat belt wearing rates and seat belt effectiveness	None [assumed that difference between actual and expected occupant fatalities in 1974 was due only to absolute speed limit and associated factors]	4A	Illustrates naivety of assumption and need for methods which take other factors into account
3	1. Classification of injuries of injured car occupants on six-point threat-to-life scale (AIS) within six body regions 2. Comparison of injury severity distributions of seat belted and unbelted occupants, categorised by crash location	Percentage of total <i>seat belted</i> injured occupants who sustained injury at specific AIS level	Percentage of total <i>unbelted</i> injured occupants who sustained injury at specific AIS level	Categorisation by crash location (dichotomy) to take into account differences in vehicle speed and hence crash severity	0B+	Illustrates limitations of categorisation for taking other factors into account (can't make use of value of interval or ordinal scale; sub-divides the data)
4	1. Random breath testing (RBT) scheduled in sectors of Melbourne on Thursday to Saturday nights for up to two weeks at a time 2. Other sectors/weeks kept RBT-free to facilitate pseudo-experimental design 3. Analysis of relative changes in accidents	Serious casualty accidents <i>at night</i> in RBT sectors during weeks the RBT operated (plus subsequent weeks to measure residual effects)	Average of serious casualty accidents at night in the same sectors and weeks, <i>in two previous years</i>	Change in serious casualty accidents <i>during day-time</i> in RBT sectors during weeks influenced by the RBT operations, compared to same sectors and weeks in the two previous years	1B	Illustrates inadequacy of pre-post method of measuring change in time series due to intervention (see Ross 1981, commenting on Cameron et al 1980 using one previous year)

Summaries of the papers in chronological order

Paper No.	Description	Evaluation criteria	Expected level method	Other factors method	Type	Notes
5	1. Analysis of accident involvements of novice car drivers (the target group) who were subject to the zero blood alcohol content (BAC) requirement in Victoria from May 1984 2. Discussion of omitted covariates (new licences and learner permits issued)	Serious casualty accidents involving target drivers which occurred during "alcohol times" of the week in post-legislation period	1. Intervention term in ARIMA model of target group driver accidents during pre- and post-legislation periods 2. Level of target group accidents during (relatively short) pre-legislation period	Change in target drivers' accidents during "non-alcohol times" (calculated using each method 1. and 2.). Also compared with net change ("alcohol time" relative to "non-alcohol time") in accidents involving standard licensed drivers not subject to zero BAC	Method 1 3B Method 2 1B	Illustrates limitations of univariate times series analysis methods for taking into account influential covariates (eg. exposed population) which vary rapidly
6	1. Presentation of time series of helmet wearing rates during pre- and post-legislation periods 2. Time series analysis of proportion of killed and hospital admitted cyclists who sustained head injuries	Proportion with head injuries during the post-legislation period	Extrapolation of linear regression model against time, fitted to proportions during pre-legislation periods	None [other factors affecting head injury incidence were assumed to affect equally the incidence of cyclist death or hospital admission from injuries other than to the head]	2A	Analysis did not take into account the known time trends in helmet wearing rates, a likely highly influential covariate. Also relied on the assumption being true (now questionable). Inappropriate analysis method for a proportion. Robinson 1996 used 2B (peds)
7	1. Crashworthiness measured in two components (i) driver injury risk in tow-away crashes in NSW, (ii) injury severity of injured drivers in Victoria and NSW 2. Comparison of (i) with relative injury risk estimated from two-car injury crashes in Victoria	Estimated risk of death or hospital admission for drivers involved in tow-away crashes [derived from separate estimates of (i) and (ii)]	Exposure adjustment: Number of tow-away crash involvements ("crash exposure"), and injured drivers ("injury exposure"), by make/model	Categorisation of crash and injury exposures by driver sex and speed limit zone. Normalisation of exposure distributions so that each make/model is compared on same basis, by weighted averages of (i) and (ii) using the universal exposure distributions.	0B+	Conceptual framework (the Road Trauma Chain) extends pre-crash exposure concepts into crash phase. In crash phase, defines conditional probabilities forming components of crashworthiness risk. Illustrates limits of categorisation.

Summaries of the papers in chronological order

Paper No.	Description	Evaluation criteria	Expected level method	Other factors method	Type	Notes
8	1. Analysis as per paper no. 7 2. Investigation of relationship between crashworthiness (and each component) and vehicle mass	As per paper no. 7	1. As per paper no. 7 2. Linear regression of crashworthiness estimate against mass	As per paper no. 7	0B+	New material (compared with paper no. 7) on investigation of mass effect. Illustrates inappropriate regression analysis of proportions (corrected in later CWR paper no. 17 using logistic regression).
9	Time series analysis of crashes in Melbourne and rural Victoria to measure the impact of new random breath test (RBT) "booze buses" introduced in December 1989 together with intensive mass media supporting publicity	Fatal and serious casualty crashes which occurred during the "high alcohol hours" of the week during each of the years 1990 and 1991	1. For 1990, intervention term in ARIMA model of crashes in each Victorian area during pre- and post implementation periods, including unemployment rates as covariate 2. For 1991, intervention term in multiple regression model of the same factors as 1.	Intervention term in same type of model of crashes in corresponding areas of New South Wales (Sydney & rural NSW). Adjustment of Victorian intervention estimates for the corresponding NSW estimates, to take into account factors not included as covariates.	5B	Specific ARIMA models fitted were unsuitable for measuring intervention effect during 1991 because they adapted to (tracked) the intervention effect during 1990, causing under-estimation.

Summaries of the papers in chronological order

Paper No.	Description	Evaluation criteria	Expected level method	Other factors method	Type	Notes
10	<p>1. Presentation of time series of helmet wearing rates</p> <p>2. Linkage of pre-legislation wearing rates with proportion of killed and hospital admitted cyclists who sustained head injuries</p> <p>3. Time series analysis of proportion with head injuries</p>	Proportion with head injuries during the post-legislation period	Extrapolation of linear regression model of head injured proportion against wearing rate (the covariate), assuming pre-legislation rate continued in post-legislation period	None [other factors affecting head injury incidence were assumed to affect equally the incidence of cyclist death or hospital admission from injuries other than to the head]	4A	<p>Analysis relied on the assumption being true (now questionable).</p> <p>Inappropriate analysis method for a proportion (wearing rate). Robinson 1996 used comparison group (peds) and 2B.</p>
11	<p>1. Development of time series regression models linking road fatalities with road safety measures, socio-economic factors, and seasonality</p> <p>2. Estimation of contribution (impact) of each measure and factor to 1990 fatality reduction</p>	Monthly road fatalities in Victoria, for all road users, and for vehicle occupants, pedestrians, and motorcyclists and bicyclists in particular	Coefficient of road safety measure (multiplied by 1989-1990 change) in multiple regression multiplicative model of fatalities 1985-1990	Influential socio-economic factors (and other road safety measures not the current focus of assessing their impact) were included in the model as covariates. Held constant in assessment of impact of specific measure	5A	<p>Collinearity of measures and factors could lead to under- or over-estimation of impact of specific measure.</p> <p>Contribution of missing measures/factors not represented (eg. advertising levels).</p> <p>Interactions not represented.</p>

Summaries of the papers in chronological order

Paper No.	Description	Evaluation criteria	Expected level method	Other factors method	Type	Notes
12	<p>1. Time series analysis of crash outcomes in Victorian areas to measure the impact of new speed cameras introduced in December 1989, plus intensive mass media publicity commencing April 1990</p> <p>2. Linkage of crash outcomes in Melbourne with program measures</p>	<p>Casualty crashes, and their injury severity outcome, which occurred during the "low alcohol hours" of the week in relevant program implementation periods during 1989-1991</p>	<p>1. Intervention term in (a) ARIMA model of crashes, (b) multiple regression model of crashes, (c) logistic regression model of injury severity [unemployment rates as covariate in each case]</p> <p>2. Coefficients of road safety measures in crash/severity models</p>	<p>1. Intervention terms in same types of models of crashes and injury severity for corresponding areas of New South Wales. Adjustment of Victorian for NSW estimates.</p> <p>2. Influential socio-economic factor (unemployment rates) was included as covariate</p>	<p>Method 1 5B</p> <p>Method 2 5A</p>	<p>1. Specific ARIMA models fitted were unsuitable for measuring intervention effect during 1991 because they adapted to (tracked) the intervention effect during 1990, causing under-estimation.</p> <p>2. Advertising levels included, but interactions not represented</p>
13	<p>1. Extension of time series regression models found in paper no. 12 (part 2) for "low alcohol hour" crashes</p> <p>2. Development of similar models linking "high alcohol hour" crashes with relevant road safety measures and socio-economic factors</p>	<p>1. Casualty crashes during LAHs in Melbourne during 1983-1991</p> <p>2. Serious casualty crashes during HAHs in Melbourne and country Victoria during 1983-1992</p>	<p>Coefficient of road safety advertising measure in multiple regression multiplicative model of crashes</p>	<p>Influential socio-economic factors (and other road safety measures not the current focus of assessing their impact) were included in the model as covariates</p>	5A	<p>Collinearity of measures and factors could lead to under- or over-estimation of contribution of advertising. Interactions not represented.</p>

Summaries of the papers in chronological order

Paper No.	Description	Evaluation criteria	Expected level method	Other factors method	Type	Notes
14	<ol style="list-style-type: none"> 1. Time series analysis of pre- and post-legislation helmet wearing rates 2. Linkage of wearing rates with proportion of killed and hospital admitted cyclists who sustained head injuries 3. Time series analysis of proportion with head injuries 	Proportion with head injuries during the post-legislation period	Extrapolation of logistic regression model of head injured proportion against wearing rate (the covariate), assuming pre-legislation trend in the rate continued in post-legislation period	None [other factors affecting head injury incidence were assumed to affect equally the incidence of cyclist death or hospital admission from injuries other than to the head]	4A	<p>Analysis relied on the assumption being true (now questionable).</p> <p>Appropriate analysis method for proportions (wearing rate and percentage head injured). Robinson 1996 used comparison group (peds) and 2B.</p>
15	<ol style="list-style-type: none"> 1. Summary of previous evaluations of Victorian programs 2. Development of models for "low alcohol hour" serious casualty crashes 3. Estimation of contribution (impact) of each measure & factor to total crash reductions in Victoria during 1990 to 1992 	Serious casualty crashes during HAHs and LAHs in Melbourne and country Victoria during 1990-1992 (four evaluation strata considered; final estimates aggregated)	Coefficient of each measure and factor (multiplied by its change relative to 1988) in each multiple regression multiplicative model of crashes 1983-1992. Estimated impacts aggregated over time of week and area of Victoria.	Influential socio-economic factors (and other road safety measures not the current focus of assessing their impact) were included in the model as covariates	5A	<p>Collinearity of measures and factors could lead to under- or over-estimation of contribution of specific measure. Interactions not represented.</p>

Summaries of the papers in chronological order

Paper No.	Description	Evaluation criteria	Expected level method	Other factors method	Type	Notes
16	1. Methods of valuing the injury savings measured in impact evaluations 2. Role of program costs and benefit-cost criteria in economic assessment of programs 3. Application of economic analysis to RBT and speed camera impacts measured as in paper no.15	Estimated savings in serious casualty crashes weighted by the average social cost per crash	Program costs	Not applicable [presumed that other factors have been taken into account in the impact evaluation of injury savings]	Economic analysis	Program costs are the expected program benefit under the hypothesis of no economic value (but not loss) due to program
17	1. Crashworthiness measured in two components (i) driver injury risk in tow-away crashes in NSW, (ii) injury severity of injured drivers in Victoria and NSW 2. Investigation of relationship between crashworthiness and year of manufacture of vehicle	Estimated risk of death or hospital admission for drivers involved in tow-away crashes [derived from separate estimates of (i) and (ii)]	Coefficient of car model in separate logistic regression models of (i) injury risk, and (ii) injury severity	Influential crash and injury exposure factors (driver age and sex, speed limit zone, number of vehicles involved) were included in the logistic regression models as covariates	0B++	Overcomes limits of categorisation, however limited by available covariate factors. Logistic regression appropriate method for proportions.
18	1. Process evaluation of the program 2. Time series analysis of crashes to measure general impact of program 3. Analysis of changes in crashes in influenced areas to measure localised effects of enforcement operations	1. Serious casualty crashes during HAHs in country Victoria during Nov 1993 to Dec 1994 2. Crash sub-set in areas and weeks influenced by RBT operations	1. Intervention term in multiple regression model of crashes 1984-1994 (with covariates) 2. HAH crashes in same areas and weeks during the previous year	1. Intervention terms in same types of models of crashes in Melbourne and country NSW 2. Change in LAH crashes in same areas and weeks (RBT period compared with previous year). Categorisation to measure specific effects	Method 1 5B Method 2 1B	1. Attribution questionable. No adjustment of intervention term because of doubts about appropriateness of comparison group 2. Categorisation not necessary for validity, only specificity. Road type used in later analysis



Summaries of the papers in chronological order

Paper No.	Description	Evaluation criteria	Expected level method	Other factors method	Type	Notes
19	<p>1. Aggressivity to other drivers measured by components (i) injury risk in two-car crashes, (ii) injury severity of injured drivers</p> <p>2. Aggressivity to unprotected road users measured by their injury severity</p> <p>3. Investigation of relationships between aggressivity, crashworthiness and mass</p>	<p>1. Estimated risk of death or hospital admission for other drivers involved in tow-away crashes with subject cars [derived from separate estimates of (i) and (ii)]</p> <p>2. Estimated risk of death or hospital admission for unprotected road users who were injured in collisions with subject cars</p>	<p>Coefficient of car model in separate logistic regression models of (i) injury risk, and (ii) injury severity of other drivers; and logistic regression model of injury severity of injured unprotected road users</p>	<p>Influential crash and injury exposure factors (other driver age and sex, speed limit zone, number of vehicles involved, unprotected road user age, sex and type) were included in the relevant logistic regression models as covariates</p>	0B++	<p>Correction for other factors is limited by available covariate factors.</p> <p>Peripheral study (see Cameron et al 1998) indicated that omission of crash severity, measured by subject driver injury, did not bias results.</p>
20	<p>Extension of analysis in paper no. 18 to test hypothesised differential effects by road type.</p> <p>Re-analysis of changes in crashes in influenced areas to measure localised effects of enforcement operations on major and minor roads</p>	<p>Serious casualty crashes during HAHs in country Victoria during Nov 1993 to Dec 1994 in areas and weeks influenced by RBT operations</p>	<p>HAH crashes in same areas and weeks during the previous year</p>	<p>Change in LAH crashes in same areas and weeks (RBT period compared with previous year). Categorisation by road type to measure specific effects</p>	1B	<p>Categorisation not necessary for validity. Specific analysis by road type explained counter-intuitive results of paper no. 18.</p>



APPENDIX C: The submitted papers



Paper No. 1

M.H. Cameron,
"Accident rate analysis and confidence limits".

Proceedings, Fifth Conference,
Australian Road Research Board,
Canberra, August 1970.

ACCIDENT RATE ANALYSIS AND CONFIDENCE LIMITS

(Paper No. 613)

The problem of detecting real differences in risk from accident rates alone is examined. A model for accident processes is presented, and accident risk defined. Empirical support for the model is critically examined. A consequence of the model is a method of statistical analysis of the variation of risk. Significance tests for the interaction of attributes in the risk space are also presented. The statistical testing errors are discussed and some recommendations made. A method of detecting the particular risks which differ from the hypotheses is described. Finally, the procedure for calculating confidence limits for accident risk is derived from the accident process model.

INTRODUCTION

1. A survey of motor vehicle usage in Queensland conducted by the Australian Road Research Board has provided detailed data of mileage performance (Ref. 1 and 2). Questionnaires were mailed to a sample of vehicle owners, during 1963 and 1964, calling for mileage to be recorded on a specified day after receipt of the questionnaire. From this survey, accurate estimates of annual vehicle mileage were obtained, broken down into a large number of characteristics of considerable interest. The survey is probably the first of its type to achieve such accuracy, and to provide such comprehensive data of mileage performance. These data have been used to calculate accident rates, in terms of accidents per mile travelled. Accident rates have been calculated for the various levels of a number of attributes, including driver age, driver sex, day of week, vehicle age, and car occupancy (Ref. 3, 4, 5 and 6). It is thought that accident rates calculated in this way provide a comparable measure of the inherent danger in each situation. The technique of calculating accident rates is not new, but seldom in the past have accurate estimates of mileage performance been available to make the rates meaningful.

2. However it is apparent that some users of this technique are not really sure what accident rates are or mean. Some think of an accident rate as actually being a definitive parameter of inherent danger, whereas others appreciate that an accident rate is an estimate of such a parameter. Even then there are still difficulties in comparing accident rates. How does one judge when the difference between two accident rates is so large that it must be ascribed to a difference in the underlying parameters, rather than to chance alone? Without a probabilistic model for accident processes it is not obvious how this can be done. It is intended to present now a model for the accident processes of individual drivers, and show how this can be used to compare accident rates of various attribute-levels. The model also leads to tests for the presence of an interaction between attributes, which should be of some value in accident causation research.

ACCIDENT PROCESS MODEL

3. A suitable model for the accident process of each individual driver may be a Poisson process. Sichel (Ref. 7) proposed this model and demonstrated that the accident processes of each of two coloured drivers of a South African bus company do not reject the hypothesis of a Poisson process with stationary parameter. A stationary Poisson accident process is generated by an individual if, at mileage m ,

- (a) $\Pr\{\text{one accident in the mileage interval } (m, m + dm)\} = \lambda \cdot dm + o(dm)$, where $o(dm)$ is a quantity such that $o(dm)/dm \rightarrow 0$ as $dm \rightarrow 0+$,
- (b) $\Pr\{\text{more than one accident in } (m, m + dm)\} = o(dm)$, and
- (c) the number of accidents in non-overlapping mileage intervals represent independent random variables.

Then it can be shown that the number N of accidents sustained by that individual in M miles is such that:

$$\Pr\{N = n\} = \exp(-\lambda M) \cdot (\lambda M)^n / n! \quad (1)$$

Even if λ varies with the mileage m , say according to the function $f(m)$ which depends on m only, but not on the number of prior accidents at mileage m , then eqn (1) is still valid, but where:

$$\lambda = M^{-1} \int_0^M f(m) dm$$

If λ in (a) depends on the number of prior accidents at mileage m ; then eqn (1) is strictly not valid, but remains a good approximation if the effect of this learning (or deterioration, perhaps) is small, as has been shown elsewhere (Ref. 8).

4. While Sichel found that for two individuals their accident processes resembled stationary Poisson processes, it seems unlikely that these accident processes were in fact stationary Poisson. Almost certainly λ in (a) varied slightly with mileage m , and almost certainly each individual's process was effected by past accidents, though apparently this effect was small. Eqn (1) above should be approximately true for these two bus drivers. It will also be approximately true for drivers who are little effected by past accidents in their accident process. The interpretation of the parameter λ for a particular one of these drivers will depend on whether his process is stationary throughout the M miles driven, or otherwise. In practice this will be not known, although Sichel's data indicate that λ should be nearly stationary. In either case, if eqn (1) is true, then the parameter λ is a measure of the inherent danger suffered by that driver within the particular M miles of driving, and henceforth λ will be called the *risk*.

5. For eqn (1) to apply even approximately to each of the population of drivers at large, each driver should be subject to little or no learning, or deterioration, from past events in his accident process. Shaw (Ref. 9) further investigated drivers at the same bus company as did Sichel. She reports that in general the accident processes of individual drivers were characterized by a short learning period of quite high accident occurrence, followed by a process with stable accident occurrence rate. Hence it seems that apart from a short learning period, a stationary Poisson process is a suitable accident process model, and that eqn (1) will be approximately true. It is intended to adopt this model henceforth, but it is emphasised that some care should be exercised in investigations of drivers who may still be in their learning period. In such cases eqn (1)

may not be true, but if it is still used to draw some tentative inferences it is worth noting that in accident processes where learning occurs, the distribution of N is usually characterised by having variance less than its mean, whereas variance and mean are identical if N has the Poisson distribution.

RISK ANALYSIS

6. If T drivers each have mutually independent Poisson accident processes, or approximately so, and if the t -th driver sustains N_t accidents in M_t miles with risk λ_t , then:

$$\Pr\{N_t = n\} = \exp(-\lambda_t M_t) \cdot (\lambda_t M_t)^n / n!$$

the Poisson distribution with mean $\lambda_t M_t$. The distribution of the total accidents:

$$N = \sum_{t=1}^T N_t$$

is again Poisson with mean $\sum \lambda_t M_t$. Defining:

$$\lambda = M^{-1} \sum \lambda_t M_t, \text{ where } M = \sum M_t$$

then the mean of the distribution of N is λM . Here λ is a mileage-weighted average of the λ_t 's. As such it represents an overall measure of the risk for the *group* of T drivers. However it is not correct to interpret λ as a measure of the risk for any one individual of the group, unless it is known that λ_t is constant for all t . An unbiased estimator of λ is N/M , the *accident rate*.

ONE-ATTRIBUTE CASE

7. If the T drivers can be described by an attribute with mutually exclusive levels, e.g. the attribute 'sex', with levels 'male' and 'female', then the drivers corresponding to each such level represent a group; and a group-risk may be defined as in para. 6. For an attribute A with I mutually exclusive levels, let M_i be the total miles driven and λ_i the group-risk corresponding to the group of drivers described by the i -th level of attribute A . Then the total number N_i of accidents sustained by these drivers in the M_i miles is such that:

$$\Pr\{N_i = n\} = \exp(-\lambda_i M_i) \cdot (\lambda_i M_i)^n / n! \quad (2)$$

The expectation of the accident rate N_i/M_i is λ_i , and hence it can be seen that with the model presented above, the accident rate is indeed an appropriate measure of the inherent danger associated with the i -th level of attribute A . However, given accident rates N_i/M_i and N_k/M_k , specific to different levels i and k of the attribute A , how does one judge the truth of the null hypothesis $H_0: \lambda_i = \lambda_k$ against the general alternative hypothesis $H_1: \lambda_i \neq \lambda_k$. Without a model such as eqn (2), criteria for testing H_0 against H_1 are not obvious.

8. Let

$$p_i = M_i/M = M_i / \left(\sum_{i=1}^I M_i \right),$$

since

$$\sum_{i=1}^I M_i = M$$

$$\pi_i = \lambda_i M_i / \left(\sum_{i=1}^I \lambda_i M_i \right) = \lambda_i M_i / (\lambda M)$$

since

$$\lambda = M^{-1} \sum_{i=1}^I \lambda_i M_i = (\lambda_i / \lambda) p_i = \mu_i p_i$$

say, where:

$$\mu_i = \lambda_i / \lambda$$

It follows (Ref. 8) from eqn (2) and the mutual independence of the N_i that the set of variates $\{N_i\}$ can be considered as from a multinomial distribution with parameters $\{\pi_i\}$ and N . If $\pi_i > 0$ for all i , it further follows (Ref. 10, p. 355) that the random variable

$$X^2 = \sum_i (N_i - N\pi_i)^2 / (N\pi_i)$$

is asymptotically (as $N \rightarrow \infty$) distributed like a χ^2 variate on $(I - 1)$ degrees of freedom. Under the hypothesis $H_0: \lambda_i = \lambda$, all i (which may alternatively be stated as $H_0: \mu_i = 1$, all i), it follows that $\pi_i = p_i$, all i . Hence when N is large, the criterion:

$$X^2 = \sum_i (N_i - Np_i)^2 / (Np_i) \quad (3)$$

can be compared with a chi-square variate on $(I - 1)$ degrees of freedom to test H_0 ; rejecting H_0 for values of X^2 greater than the upper critical value of χ^2 . It is worth noting that the criterion eqn (3) is not concerned with the absolute size of the mileage M_i , but only with the apportionment $\{p_i\}$ of the mileage over the attribute A . Neither is the criterion concerned with the absolute value of the risk λ_i , and it is quite appropriate to phrase the hypothesis H_0 in terms of the *relative risk* $\mu_i = \lambda_i / \lambda$, rather than the λ_i themselves.

9. It is advantageous at this point to examine further evidence in support of the probabilistic accident model presented earlier. Reference 11 describes an investigation of a sample of drivers for whom the number of accidents and miles travelled per year were known from a questionnaire survey. From this total sample, drivers were drawn with equal chance and their corresponding accidents and miles added, until the cumulative miles was as near as possible to 100,000 miles. This process was then repeated for the remaining drivers in the sample. If S such sub-samples were formed in this way, and if each driver in the total sample had mutually independent Poisson accident process, then the total number N_s of accidents in the s -th sub-sample would be such that:

$$\Pr\{N_s = n\} = \exp(-\lambda_s M_s) \cdot (\lambda_s M_s)^n / n! \quad (4)$$

where $M_s \approx 100,000$ for all s , and λ_s is the group-risk for the s -th sub-sample. If $\lambda_s = \lambda$, a constant for all s , then $\{N_1, N_2, \dots, N_S\}$ could be considered as a sample of size S from a Poisson distribution with mean $100,000\lambda$. In Ref. 11 it was found in general for drivers operating under similar risk conditions, i.e., drivers of the same sex and driving the same type of vehicle, that the samples $\{N_1, N_2, \dots, N_S\}$ did not reject the hypothesis of coming from a Poisson distribution. To some extent these findings support the Poisson accident process model presented earlier, or at least support the truth of eqn (4) for groups of drivers.

10. Further relevant evidence has also been given in Ref. 12. Data were obtained on injury accidents, and total vehicle mileage estimated, for each day during a period in Sweden when three different levels of vehicle speed restriction were tried. The days were divided into three groups: Mondays to Fridays, Saturdays, and Sundays. For the days within each speed-group and day-group, the criterion X^2 in eqn (3) was calculated; the attribute A in this case having each of the appropriate days as its levels. Consequently, if the probabilistic model eqn (2) is correct for these accident data, then each criterion X^2 represents a test for a constant risk over the particular days for which it was calculated. It is perhaps somewhat surprising that, apart from the days when the speed limits were unrestricted, the criteria X^2 are in general not significant. To some extent this suggests not only that the model eqn (2) is correct, but also that the risk remained almost constant within each of the three day-groups, while speed limits were in force. However, it should be noted that other, perhaps less simple, explanations of the insignificant X^2 criteria exist.

TWO-ATTRIBUTE CASE

11. Consider now a second attribute B , with J mutually exclusive levels, which may be employed to describe each of the T drivers. Let M_{ij} be the total miles driven, λ_{ij} be the group-risk, and N_{ij} be the total accidents corresponding to the group of drivers described by both the i -th level of attribute A and the j -th level of attribute B . Then:

$$\Pr\{N_{ij} = n\} = \exp(-\lambda_{ij} M_{ij}) \cdot (\lambda_{ij} M_{ij})^n / n! \quad (5)$$

Define:

$$\begin{aligned} N_{i.} &= \sum_j N_{ij} & N_{.j} &= \sum_i N_{ij} & N &= \sum_i \sum_j N_{ij} \\ M_{i.} &= \sum_j M_{ij} & M_{.j} &= \sum_i M_{ij} & M &= \sum_i \sum_j M_{ij} \\ \lambda_{i.} &= M_{i.}^{-1} \sum_j \lambda_{ij} M_{ij} & \lambda_{.j} &= M_{.j}^{-1} \sum_i \lambda_{ij} M_{ij} & \lambda &= M^{-1} \sum_i \sum_j \lambda_{ij} M_{ij} \\ \mu_{ij} &= \lambda_{ij} / \lambda & \mu_{i.} &= \lambda_{i.} / \lambda & \mu_{.j} &= \lambda_{.j} / \lambda \\ p_{ij} &= M_{ij} / M & p_{i.} &= M_{i.} / M & p_{.j} &= M_{.j} / M \\ \pi_{ij} &= \lambda_{ij} M_{ij} / (\sum_i \sum_j \lambda_{ij} M_{ij}) = \lambda_{ij} M_{ij} / (\lambda M) = \mu_{ij} p_{ij} \\ \pi_{i.} &= \sum_j \pi_{ij} = \sum_j \lambda_{ij} M_{ij} / (\sum_i \sum_j \lambda_{ij} M_{ij}) = \lambda_{i.} M_{i.} / (\lambda M) = \mu_{i.} p_{i.} \\ \pi_{.j} &= \sum_i \pi_{ij} = \sum_i \lambda_{ij} M_{ij} / (\sum_i \sum_j \lambda_{ij} M_{ij}) = \lambda_{.j} M_{.j} / (\lambda M) = \mu_{.j} p_{.j} \end{aligned}$$

$$\begin{aligned} \text{Hence} \quad \sum_i \sum_j p_{ij} &= 1 = \sum_i p_{i.} = \sum_j p_{.j} \\ \text{and} \quad \sum_i \sum_j \pi_{ij} &= 1 = \sum_i \pi_{i.} = \sum_j \pi_{.j} \end{aligned}$$

12. It follows from eqn (5) that:

$$\Pr\{N_{i.} = n\} = \exp(-\lambda_{i.} M_{i.}) \cdot (\lambda_{i.} M_{i.})^n / n!$$

$$\text{and} \quad \Pr\{N_{.j} = n\} = \exp(-\lambda_{.j} M_{.j}) \cdot (\lambda_{.j} M_{.j})^n / n!$$

Comparison of these equations with eqn (2) is all that is necessary to indicate that the results given for the one-attribute case are sufficient to provide tests for each of the hypotheses $H_0^{(A)}: \mu_{i.} = 1$, all i , and $H_0^{(B)}: \mu_{.j} = 1$, all j . The appropriate criteria are:

$$X_A^2 = \sum_i (N_{i.} - Np_{i.})^2 / (Np_{i.})$$

and

$$X_B^2 = \sum_j (N_{.j} - Np_{.j})^2 / (Np_{.j})$$

compared with chi-square variates on $(I - 1)$ and $(J - 1)$ degrees of freedom, respectively.

13. A hypothesis of no interaction between the attributes A and B in the risk space is $H_0^{(AB)}: \mu_{ij} = \mu_{i.}\mu_{.j}$, all i, j . Subject to the conditions:

$$\sum_i \hat{\mu}_{i.} p_{i.} = 1, \quad \sum_j \hat{\mu}_{.j} p_{.j} = 1$$

maximum likelihood estimators of $\mu_{i.}$ and $\mu_{.j}$ are

$$\hat{\mu}_{i.} = N_{i.} / (Np_{i.}) \text{ and } \hat{\mu}_{.j} = N_{.j} / (Np_{.j})$$

respectively (Ref. 8). Hence it follows (Ref. 13, p. 423) that under $H_0^{(AB)}$, the criterion:

$$X_{AB}^2 = \sum_i \sum_j (N_{ij} - N\hat{\pi}_{ij})^2 / (N\hat{\pi}_{ij})$$

where

$$\hat{\pi}_{ij} = \hat{\mu}_{i.}\hat{\mu}_{.j}p_{ij} = [N_{i.}/(Np_{i.})][N_{.j}/(Np_{.j})]p_{ij}$$

is asymptotically (as $N \rightarrow \infty$) distributed like a chi-square variate on $(I - 1)(J - 1)$ degrees of freedom. Hence when N is large, the criterion X_{AB}^2 can be compared with a chi-square variate on $(I - 1)(J - 1)$ degrees of freedom to test $H_0^{(AB)}$. The use of this technique to analyse accident risks appears to be new in the literature (as also does the technique to be described in the next section). A similar and well-known technique (Ref. 13, p. 556) has often been used to test $H_0: \pi_{ij} = \pi_{i.}\pi_{.j}$, all i, j , the hypothesis of no interaction between A and B in the accident space, but this hypothesis is of limited interest, and a test of the hypothesis $H_0^{(AB)}: \mu_{ij} = \mu_{i.}\mu_{.j}$, all i, j , as provided by the new criterion X_{AB}^2 should be of much greater value in accident research.

THREE-ATTRIBUTE CASE

14. The results of the previous section are appropriate to risks in the $A \times B$ attribute space. The analysis is performed by a criterion applied to each of the two margin attribute spaces (A ignoring B, and B ignoring A), and a criterion for testing the interaction between the attributes. Consider now a third attribute C, with K mutually exclusive levels, which may be employed in a like manner to attributes A and B to describe each of the T drivers. Let M_{ijk} be the total miles driven, λ_{ijk} be the group-risk, and N_{ijk} be the total accidents corresponding to the group of drivers described by the i -th level of attribute A, j -th level of attribute B, and k -th level of attribute C. Then:

$$\Pr\{N_{ijk} = n\} = \exp(-\lambda_{ijk}M_{ijk}) \cdot (\lambda_{ijk}M_{ijk})^n / n! \quad (6)$$

Now a whole host of definitions can be given for lower-order quantities in terms of the basic quantities M_{ijk} , λ_{ijk} and N_{ijk} , but in principle they are the same as the definitions given at the beginning of the previous section, and so they will be taken as understood. It follows from eqn (6) that:

$$\Pr\{N_{ij.} = n\} = \exp(-\lambda_{ij.}M_{ij.}) \cdot (\lambda_{ij.}M_{ij.})^n / n!$$

Comparison of this equation with eqn (5) is sufficient to indicate that all the results of the previous section hold in the $A \times B$ (ignoring C) attribute space. Similarly the results of the previous section can be shown to hold in each of the $A \times C$ (ignoring B) and $B \times C$ (ignoring A) attribute spaces.

15. A hypothesis of no second-order interaction between the attributes A, B and C in the risk space is:

$$H_0^{(ABC)}: \mu_{ijk} = (\mu_{ij.}\mu_{.k}) / (\mu_{i.}\mu_{.j}\mu_{.k}), \text{ all } i, j, k$$

Maximum likelihood estimators of the first and second order relative risks are (Ref. 8):

$$\begin{aligned} \hat{\mu}_{i.} &= N_{i.} / (Np_{i.}), & \hat{\mu}_{.j} &= N_{.j} / (Np_{.j}), & \hat{\mu}_{.k} &= N_{.k} / (Np_{.k}) \\ \hat{\mu}_{ij.} &= N_{ij.} / (Np_{ij.}), & \hat{\mu}_{i.k} &= N_{i.k} / (Np_{i.k}), & \hat{\mu}_{.jk} &= N_{.jk} / (Np_{.jk}) \end{aligned}$$

Hence it follows that under $H_0^{(ABC)}$, the criterion:

$$X_{ABC}^2 = \sum_i \sum_j \sum_k (N_{ijk} - N\hat{\pi}_{ijk})^2 / (N\hat{\pi}_{ijk})$$

where:

$$\hat{\pi}_{ijk} = [\hat{\mu}_{ij.}\hat{\mu}_{i.k}\hat{\mu}_{.jk}] / (\hat{\mu}_{i.}\hat{\mu}_{.j}\hat{\mu}_{.k}) p_{ijk}$$

is asymptotically (as $N \rightarrow \infty$) distributed like a χ^2 variate on $(I - 1)(J - 1)(K - 1)$ degrees of freedom. Hence when N is large, the criterion X_{ABC}^2 can be compared with a chi-square variate on $(I - 1)(J - 1)(K - 1)$ degrees of freedom to test $H_0^{(ABC)}$.

STATISTICAL TESTING ERRORS

16. With any statistical test of a hypothesis H_0 , there are two possible errors which may be made, namely:

(I) reject H_0 when it is true, or

(II) not reject H_0 when it is false.

The probability of a type I error is usually denoted by α . The probability of a type II error is, of course, a function of the alternative hypothesis (say, H_1) considered, and is usually denoted by β . The power of the test is defined as $1 - \beta$ and equals the probability of rejecting H_0 when H_1 is true.

17. The methods of risk analysis described earlier each employ a criterion X^2 which, if the total accidents N is large, has approximately the chi-square distribution on appropriate degrees of freedom ν when the appropriate hypothesis H_0 is true. For arbitrary α , but it is usual to choose α small (say, 0.05, 0.01, or 0.001), it is possible to find a critical value $\chi_\alpha^2(\nu)$ such that:

$$\int_{\chi_\alpha^2(\nu)}^{\infty} d\chi^2(\nu, 0) = \Pr\{Y \geq \chi_\alpha^2(\nu)\} = \alpha,$$

where $\chi^2(\nu, 0)$ is the distribution function of the central chi-square distribution with ν degrees of freedom, and where the variate Y has this distribution. If X^2 exceeds $\chi_\alpha^2(\nu)$, then this may be explained in one of two ways, namely:

(a) H_0 is false, or

(b) a rare, unlikely event has occurred.

In practice, the first of these explanations is always chosen. Occasionally the second explanation may be correct and H_0 is in fact true, but α is the probability of this occurring, and hence this justifies the choice of a small α . When X^2 exceeds $\chi_\alpha^2(\nu)$, it is said to be significant, and α is often called the significance level.

18. Consider now the situation when H_0 is false. Then X^2 has approximately the non-central chi-square distribution on ν degrees of freedom and non-centrality parameter γ , and with distribution function $\chi^2(\nu, \gamma)$. If the hypotheses H_0 and H_1 are phrased in terms of the accident frequencies $\pi_i (= \mu_i p_i)$, and $\pi_i^{(0)}$ and $\pi_i^{(1)}$ are the

hypothetical values of π_i in H_0 and H_1 , respectively, then the non-centrality parameter is (Ref. 13, p. 436)

$$\gamma = N \sum_{i=1}^I (\pi_i^{(1)} - \pi_i^{(0)})^2 / \pi_i^{(0)}$$

It is convenient to define:

$$\kappa = \left[\sum_{i=1}^I (\pi_i^{(1)} - \pi_i^{(0)})^2 / \pi_i^{(0)} \right]^{\frac{1}{2}}$$

Consider the various hypotheses tested by the methods of risk analysis described earlier. For $H_0^{(A)} : \mu_i = 1$, all i , then $\pi_i^{(0)} = 1$, $p_i = p_i$. The alternative hypothesis is the general alternative $H_1^{(A)} : \mu_i \neq 1$, some i , so without a more explicit hypothesis take $\pi_i^{(1)} = \mu_i p_i$. Then:

$$\kappa = \left[\sum_{i=1}^I (\mu_i p_i - p_i)^2 / p_i \right]^{\frac{1}{2}} = \left[\sum_{i=1}^I p_i (\mu_i - 1)^2 \right]^{\frac{1}{2}}$$

Hence in the one-attribute case, κ is the root mean square difference of the relative risks and unity. By a similar argument, for

$H_0^{(AB)} : \mu_{ij} = \mu_i \mu_j$, all i, j , then:

$$\kappa = \left[\sum_{i=1}^I \sum_{j=1}^J p_{ij} (\mu_{ij} - \mu_i \mu_j)^2 / (\mu_i \mu_j) \right]^{\frac{1}{2}}$$

and for $H_0^{(ABC)} : \mu_{ijk} = (\mu_{ij} \mu_{i,k} \mu_{j,k}) / (\mu_i \mu_j \mu_k)$ all i, j, k , then:

$$\kappa = \left[\sum_{i=1}^I \sum_{j=1}^J \sum_{k=1}^K p_{ijk} (\mu_{ijk} - \mu_{ijk}^{(0)})^2 / \mu_{ijk}^{(0)} \right]^{\frac{1}{2}}$$

where $\mu_{ijk}^{(0)} = (\mu_{ij} \mu_{i,k} \mu_{j,k}) / (\mu_i \mu_j \mu_k)$

19. For a particular critical value $\chi_a^2(v)$, the probability of a type II error is:

$$\beta = \Pr\{X^2 < \chi_a^2(v)\} \approx \int_0^{\chi_a^2(v)} d\chi^2(v, \gamma)$$

Hence the power of the test is approximately:

$$P = \int_{\chi_a^2(v)}^{\infty} d\chi^2(v, \gamma) \quad (7)$$

It can be seen that α , β and γ are all intimately related. For fixed α and κ , and noting that $\gamma = N\kappa^2$, it also can be seen that P tends to 1 as N increases (Ref. 13, p. 231). Hence for very large N , the power P will be close to 1 even for small, perhaps trivial, values of κ . However this will only be the case if α is kept at conventional, fixed levels, irrespective of N . There is a case for reducing α instead of β at times of gain in test sensitivity due to large N , particularly in the accident research context. If the real risk situation is little different from H_0 , then the type II error is of little consequence, since if H_0 were rejected and a counter-measure introduced, there would be little room for improvement of the risk situation anyway. However a type I error may result in an extensive counter-measure program being introduced which would be quite useless, would waste money, and could only bring the science of accident research into disrepute.

20. In an investigation (Ref. 6) in which the present writer was involved and where the risk analysis methods were used, it was decided that α should not exceed 0.001, and that where possible the tests should have power at least 0.999 when $\kappa \geq 0.15$, but not at the expense of α exceeding 0.001. All the χ^2 tests involved had degrees of freedom between 1 and 36 inclusive. By an approximation to the non-central χ^2 distribution given in (Ref. 13, p. 228), eqn (7) is approximately:

$$P = \int_{\chi_a^2(v)}^{\infty} \chi_a^2(v) \cdot (v + \gamma) / (v + 2\gamma) d\chi^2[(v + \gamma)^2 / (v + 2\gamma), 0] \quad (8)$$

Since P increases for decreasing v , it is sufficient to find γ such that:

$$\int_{\chi_{0.001}^2(36)}^{\infty} d\chi^2[(36 + \gamma)^2 / (36 + 2\gamma), 0] = 0.999$$

The solution is $\gamma = 85$, to the nearest integer. Hence for $\alpha = 0.001$, $P \geq 0.999$ whenever $\gamma \geq 85$ and $1 \leq v \leq 36$. However, whenever $N < 85 / (0.15)^2 = 3778$, the power is less than 0.999 at $\kappa = 0.15$ for some v in the range 1 to 36. This can only be avoided if α is allowed to exceed 0.001, which was not permissible. Whenever $N \leq 3778$, α was taken as 0.001 and hence the critical value $C_{v,N}$ for χ^2 criteria on v degrees of freedom was:

$$C_{v,N} = \chi_{0.001}^2(v)$$

21. For $N > 3778$ and α fixed at 0.001, there is some $\kappa < 0.15$ for which $P \geq 0.999$ whenever $1 \leq v \leq 36$. Since values of κ less than 0.15 were considered trivial in the investigation cited, it was decided to choose α for $N > 3778$ such that the tests would have identical power at $\kappa = 0.15$ as for when $N = 3778$. The power of the tests at $N = 3778$ is given by the set $\{P_v; v = 1, 2, \dots, 36\}$ where:

$$P_v = \int_{\chi_{0.001}^2(v)}^{\infty} d\chi^2(v, 85) \\ \approx \int_{\chi_{0.001}^2(v) \cdot (v+85)/(v+170)}^{\infty} d\chi^2[(v+85)^2/(v+170), 0]$$

These integrals were determined with the aid of tables of the incomplete gamma function (Ref. 14). Since $(v + \gamma)^2 / (v + 2\gamma) > 30$ for all $\gamma \geq 85$, eqn 8 can be replaced with the approximation:

$$P = \Phi\{[2 \cdot \chi_a^2(v) \cdot (v + \gamma) / (v + 2\gamma)]^{\frac{1}{2}} - [2 \cdot (v + \gamma)^2 / (v + 2\gamma) - 1]^{\frac{1}{2}}\}$$

where

$$\Phi\{x\} = (2\pi)^{-\frac{1}{2}} \int_x^{\infty} \exp(-\frac{1}{2}y^2) dy$$

Hence:

$$P \approx \Phi\{[2(v + \gamma) / (v + 2\gamma)]^{\frac{1}{2}} [\chi_a^2(v)]^{\frac{1}{2}} - [v + \gamma]^{\frac{1}{2}}\}$$

Solving for $\chi_a^2(v)$:

$$\chi_a^2(v) \approx ([v + \gamma]^{\frac{1}{2}} + \Phi^{-1}(P) \cdot [(v + \gamma) / (v + 2\gamma)]^{\frac{1}{2}})^2$$

where Φ^{-1} is the inverse function of Φ . For $N > 3778$, the critical value $C_{v,N}$ for chi-square criteria on v degrees of freedom was taken as:

$$C_{v,N} = ([v + \gamma]^{\frac{1}{2}} + \Phi^{-1}(P_v) \cdot [(v + \gamma) / (v + 2\gamma)]^{\frac{1}{2}})^2 \quad (9)$$

where $\gamma = N \cdot (0.15)^2$

22. In theory it should be possible to determine α for any critical value $C_{v,N}$ by setting $\chi^2_{\alpha}(v) = C_{v,N}$. In practice this cannot always be done, since as N increases beyond 3778, α quickly falls below values tabulated in conventional tables of $\chi^2_{\alpha}(v)$. Some preliminary investigations suggest that although α is not identical for all $C_{v,N}$ at each particular N , it is approximately so, at least for $N \leq 6000$ and $1 \leq v \leq 36$. An approximate measure of α can be obtained via the identity:

$$\chi^2_{\alpha}(1) = [\Phi^{-1}(\frac{1}{2}\alpha)]^2$$

by taking:

$$\alpha = 2\Phi([C_{1,N}]^{\frac{1}{2}})$$

with the aid of tables in Ref. 15, p. 104 and p. 111.

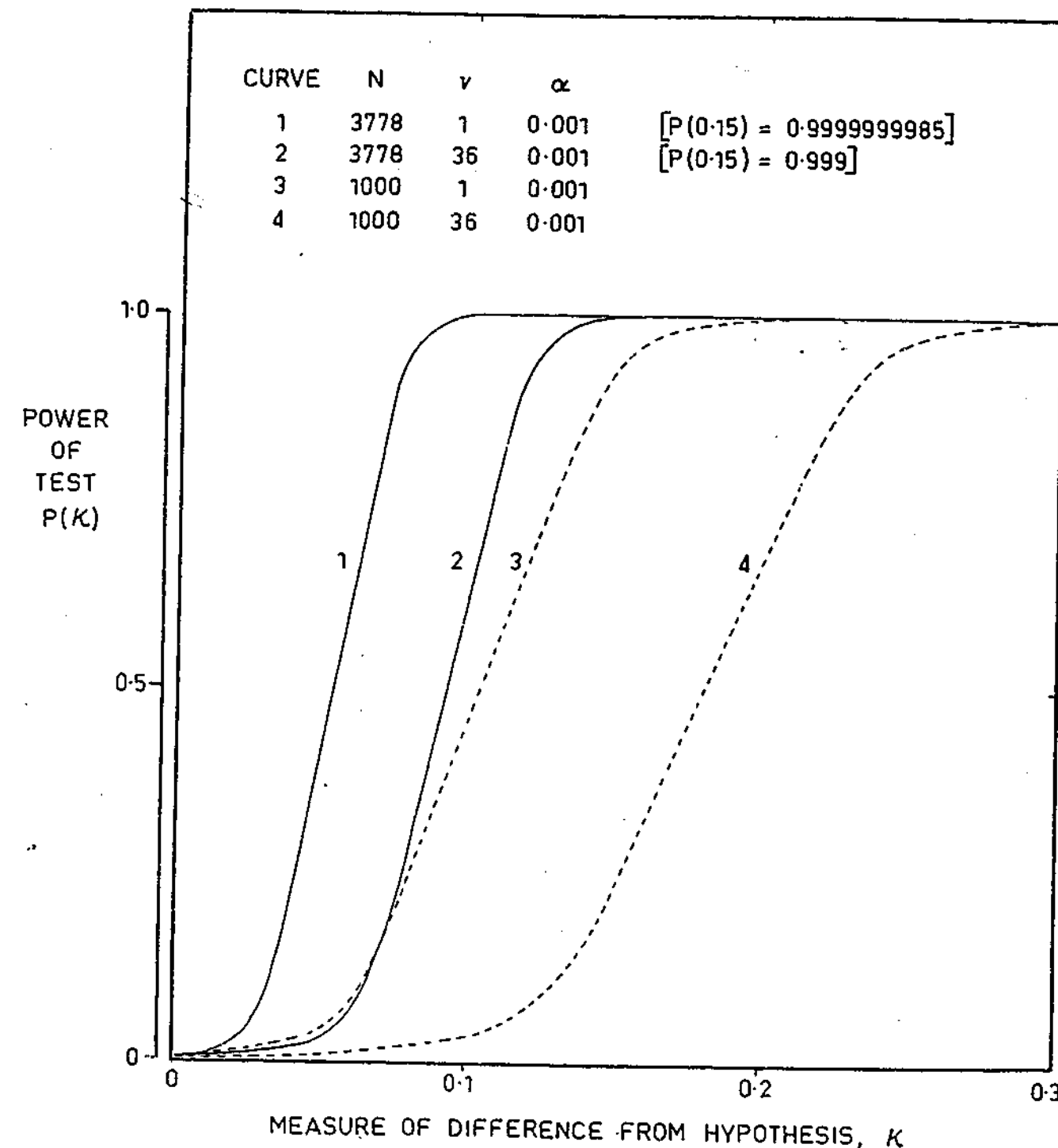


Fig. 1 — Power of chi-square test as a function of difference from hypothesis

23. Fig. 1 and 2 illustrate the power of the χ^2 tests as a function of κ . In Fig. 1, curves 1 and 2 are the power curves when $N = 3778$ and $\alpha = 0.001$, for 1 and 36 degrees of freedom, respectively. The power curves for degrees of freedom between 1 and 36 lie between curves 1 and 2. It can be seen that the power is 0.999 at $\kappa = 0.15$ (the smallest non-trivial value of κ) when $v = 36$, and increases with decreasing v to 0.9999999985 when $v = 1$. The dashed curves in Fig. 1 are the power curves when $N = 1000$ and $\alpha = 0.001$. At $\kappa = 0.15$, the maximum power is 0.93 when $v = 1$, and falls to 0.23 when $v = 36$. While it is possible when $N = 1000$ to choose tests with higher power at $\kappa = 0.15$, this could only be done at the expense of α exceeding 0.001. In Fig. 2, curves 1 and 2 are the power curves when $N = 10,000$ and when the critical values have been chosen as described in para. 21. As can be seen, the critical values have been chosen so that the tests have identical power at $\kappa = 0.15$ as for the tests

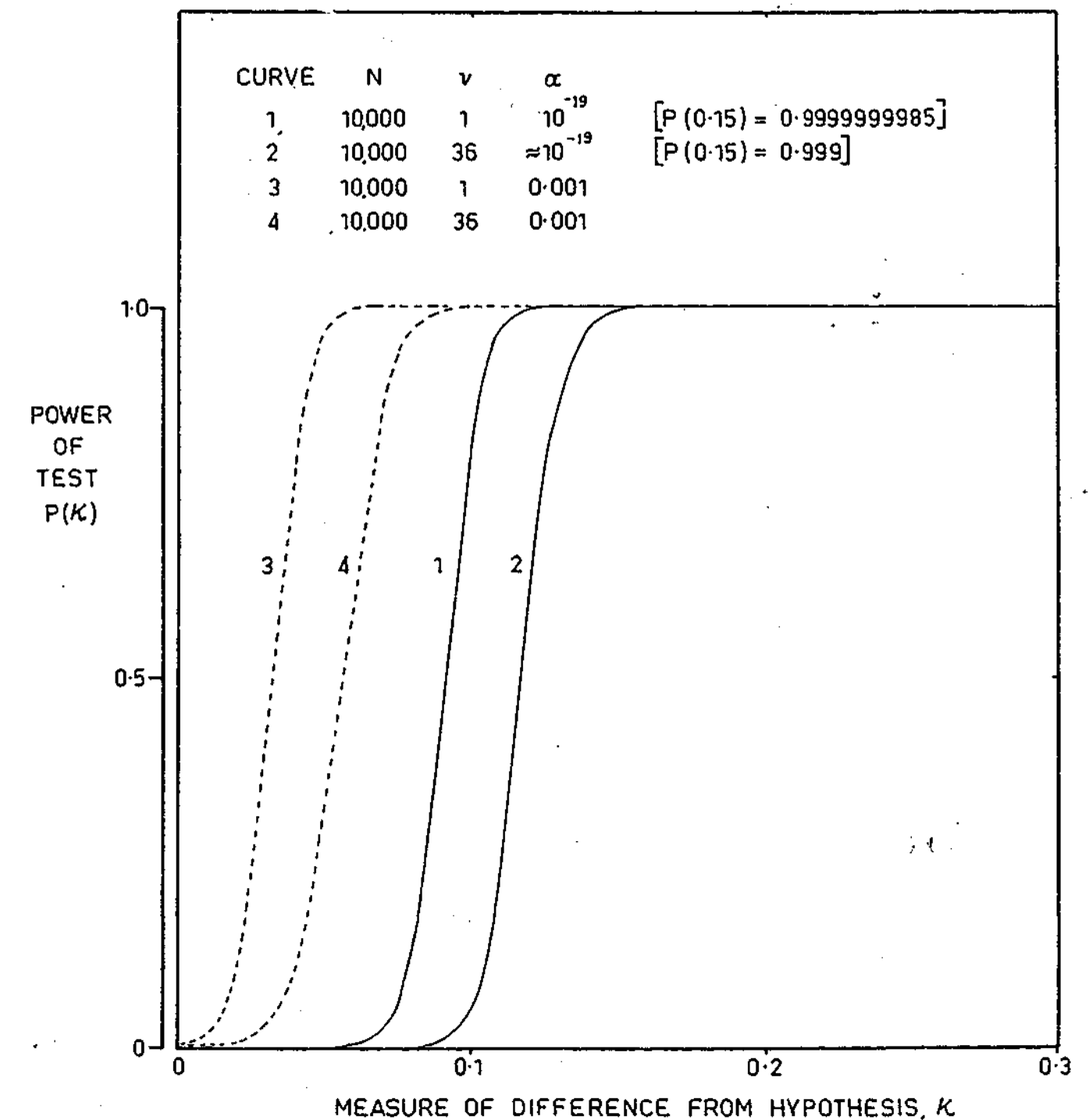


Fig. 2 — Power of chi-square test as a function of difference from hypothesis

when $N = 3778$ and $\alpha = 0.001$. For these tests, α is approximately 10^{-19} . The dashed curves in Fig. 2 are the power curves when $N = 10,000$ and $\alpha = 0.001$. For these tests, the power is already at least 0.999 when $\kappa = 0.092$. Hence trivial values of κ less than 0.15 would have been detected with power close to 1 if α had remained fixed at 0.001 when N exceeded 3778.

24. If the economic consequences of the type I and type II errors can be well determined in a particular investigation of accident risk, then Ref. 16 describes an objective method of determining the balance between α , β and κ at any particular N . However if, as is usual, the consequences (whether economic or otherwise) are not clearly apparent, then it is recommended that critical values for χ^2 be chosen along the lines of that described in the last four paragraphs. While these four paragraphs are phrased in terms of the empirical values used in a particular investigation, the philosophy of the procedure should be obvious and capable of application in other investigations.

HIGH AND LOW RISKS

25. If, in applying the risk analysis methods described earlier, a particular χ^2 criterion proves significant, then at times it is desired to then find the position in the risk space of departure from the corresponding null-hypothesis. When the criterion has only one degree of freedom, the position and direction of departure are obvious from a comparison of the observed and hypothetical accident frequencies. However consider the situation where:

$$X_A^2 = \sum_{i=1}^I (N_i - Np_i)^2 / (Np_i)$$

is significant, and I , the number of levels of attribute A, exceeds 2. For a particular level 'i' of A, the appropriate criterion to test the hypothesis $H_0^{(i)} : \mu_i = 1$, is

$$X_{(i)}^2 = (N_i - Np_i)^2 / (Np_i) + [(N - N_i) - N(1 - p_i)]^2 / [N(1 - p_i)] \quad (10)$$

$$= (N_i - Np_i)^2 / [Np_i(1 - p_i)] \text{ on 1 degree of freedom} \quad (11)$$

The quantity $X_{(i)}^2$ is a further calculation in addition to X_A^2 , whereas $(N_i - Np_i)^2 / (Np_i)$ is usually available as a by-product in the calculation of X_A^2 , for every i . For this reason in Ref. 8 it was recommended that $(N_i - Np_i)^2 / (Np_i)$ be compared with the χ^2 critical value for 1 degree of freedom as a test of $H_0^{(i)} : \mu_i = 1$, since if $(N_i - Np_i)^2 / (Np_i)$ exceeds the critical value $C_{1,N}$, then this necessarily implies that $X_{(i)}^2$ does also.

26. In practice it has been found that this test procedure severely lacks power for rejecting $H_0^{(i)}$ when $H_0^{(i)}$ is false. For fixed $|\mu_i - 1| > 0$, the power is higher for smaller p_i . The test appears to have reasonable power when $(N_i - Np_i)^2 / (Np_i)$ is compared with critical value $\frac{1}{2}C_{1,N}$. This is not unexpected when it is recalled that $(N_i - Np_i)^2 / (Np_i)$ is one of two components of $X_{(i)}^2$ in eqn (10). With critical value $\frac{1}{2}C_{1,N}$, the test is still biased in that the power is higher for smaller p_i at fixed $|\mu_i - 1| > 0$. There is no claim here that $(N_i - Np_i)^2 / (Np_i)$ compared with $\frac{1}{2}C_{1,N}$ is a rigorous test procedure for testing $H_0^{(i)}$, but it is suggested that the criterion $(N_i - Np_i)^2 / (Np_i)$ is a very appropriate criterion, and that the problem lies in choosing a suitable critical value. The present writer is aware that the strictly correct critical

value is $(1 - p_i)C_{1,N}$, which is obvious from eqn (11). In testing $H_0^{(i)}$, this critical value is operationally little more difficult to use than $\frac{1}{2}C_{1,N}$. If $H_0^{(i)}$ is rejected by a significant criterion $(N_i - Np_i)^2 / (Np_i)$, then the problem still remains of deciding whether the appropriate conclusion is $\mu_i > 1$ or $\mu_i < 1$. If the maximum likelihood estimator of μ_i , namely $\hat{\mu}_i = N_i / (Np_i)$, exceeds 1, then the conclusion is that the i -th level of attribute A is associated with a *high risk*. If $\hat{\mu}_i < 1$, the conclusion is a *low risk*.

27. The situation is a little more complex when X_{AB}^2 is significant, and I or J or both exceed 2. For particular levels 'i' of A and 'j' of B, it was recommended in Ref. 8 that the hypothesis $H_0^{(ij)} : \mu_{ij} = \mu_i \mu_j$ could be tested by the criterion:

$$X_{(ij)}^2 = (N_{ij} - N\hat{\pi}_{ij})^2 / (N\hat{\pi}_{ij})$$

where:

$$\hat{\pi}_{ij} = \hat{\mu}_i \hat{\mu}_j p_{ij} = [N_i / (Np_i)] [N_j / (Np_j)] p_{ij}$$

with critical value $C_{1,N}$. Again it has been found in practice that this test procedure severely lacks power, but this is not the case if the critical value is instead chosen as $\frac{1}{4}C_{1,N}$. For this test the strictly correct critical value for the criterion $X_{(ij)}^2$ is not obvious and has not been found by the present writer. Hence $\frac{1}{4}C_{1,N}$ appears to be the best critical value available. Hence to be consistent it may be appropriate to choose $\frac{1}{4}C_{1,N}$ as the critical value for $(N_i - Np_i)^2 / (Np_i)$ when testing $H_0^{(i)}$. If $H_0^{(ij)}$ is rejected by a significant $X_{(ij)}^2$ and $\hat{\mu}_{ij} = N_{ij} / (Np_{ij})$ exceeds the product $\hat{\mu}_i \hat{\mu}_j$, then it is appropriate to conclude that *when appearing together* the i -th level of A and the j -th level of B are associated with a high risk relative to the hypothesis $H_0^{(AB)} : \mu_{ij} = \mu_i \mu_j$, all i, j . If $\hat{\mu}_{ij} < \hat{\mu}_i \hat{\mu}_j$, the conclusion is a low risk relative to $H_0^{(AB)}$.

28. The situation when X_{ABC}^2 is significant and any of I, J and K exceed 2, is probably obvious. In Ref. 8 it was recommended that the hypothesis $H_0^{(ijk)} : \mu_{ijk} = (\mu_{ij} \mu_{i,k} \mu_{j,k}) / (\mu_i \mu_j \mu_{i,k})$

could be tested by the criterion:

$$X_{(ijk)}^2 = (N_{ijk} - N\hat{\pi}_{ijk})^2 / (N\hat{\pi}_{ijk})$$

where:

$$\hat{\pi}_{ijk} = [(\hat{\mu}_{ij} \hat{\mu}_{i,k} \hat{\mu}_{j,k}) / (\hat{\mu}_i \hat{\mu}_j \hat{\mu}_{i,k})] p_{ijk}$$

with critical value $C_{1,N}$. Here also a severe lack of power has been found for this test procedure, but not if the critical value is instead chosen as $1/8 C_{1,N}$. If $H_0^{(ijk)}$ is rejected by a significant $X_{(ijk)}^2$, then a high risk relative to $H_0^{(ABC)} : \mu_{ijk} = (\mu_{ij} \mu_{i,k} \mu_{j,k}) / (\mu_i \mu_j \mu_{i,k})$, all i, j, k , is concluded if $\hat{\mu}_{ijk} > (\hat{\mu}_{ij} \hat{\mu}_{i,k} \hat{\mu}_{j,k}) / (\hat{\mu}_i \hat{\mu}_j \hat{\mu}_{i,k})$, or a low risk relative to $H_0^{(ABC)}$ if the inequality is reversed.

CONFIDENCE LIMITS FOR RISK

29. If the model eqn (1) is true and the total mileage M is known, then confidence limits can be calculated for the risk λ . A $100(1 - \alpha)$ per cent confidence interval for λ is a (random variable) interval which covers the fixed λ with probability $(1 - \alpha)$. The terminals of the interval are known as confidence limits. Rewriting eqn (1) as:

$$\Pr\{N = n \mid \lambda M\} = \exp(-\lambda M) \cdot (\lambda M)^n / n!$$

then, given a realization N^* of N , a $100(1 - \alpha)$ per cent confidence interval for λM is $[L, U]$, where:

$$\sum_{n=N}^{\infty} \Pr\{N = n \mid L\} + \sum_{n=0}^{N^*} \Pr\{N = n \mid U\} = \alpha \quad (12)$$

For definiteness, it is usual to choose:

$$\sum_{n=N}^{\infty} \Pr\{N = n \mid L\} = \frac{1}{2}\alpha = \sum_{n=0}^{N^*} \Pr\{N = n \mid U\} \quad (13)$$

when N^* is such that this is possible. When eqn (12) holds, then:

$$\Pr\{L \leq \lambda M \leq U\} = 1 - \alpha$$

and hence:

$$\Pr\{L/M \leq \lambda \leq U/M\} = 1 - \alpha$$

i.e. $[L/M, U/M]$ is a $100(1 - \alpha)$ per cent confidence interval for λ .

30. The determining of L and U which satisfy eqn (12) or (13) is possible via tables like Table 7 in Ref. 15, but the procedure is complex and may lack accuracy. However there exists a simple approximation for the confidence limits when λM is large. It has been shown in Ref. 17, (p. 245) that as $\lambda M \rightarrow \infty$, the Poisson distribution approaches the normal distribution with the same mean and variance. Since the mean and variance of N are both λM , for large λM it follows that approximately:

$$\Pr\{|N - \lambda M|/(\lambda M)^{\frac{1}{2}} \leq u\} = 1 - \alpha \quad (14)$$

where $u = \Phi^{-1}(\frac{1}{2}\alpha)$. The event in the braces in eqn (14) may be rewritten after manipulation of the inequality as:

$$N + \frac{1}{2}u^2 - (Nu^2 + \frac{1}{4}u^4)^{\frac{1}{2}} \leq \lambda M \leq N + \frac{1}{2}u^2 + (Nu^2 + \frac{1}{4}u^4)^{\frac{1}{2}}$$

Consequently, an approximate $100(1 - \alpha)$ per cent confidence interval for λ is $[L'/M, U'/M]$:

where:

$$L' = N + \frac{1}{2}u^2 - (Nu^2 + \frac{1}{4}u^4)^{\frac{1}{2}} \quad (15)$$

and:

$$U' = N + \frac{1}{2}u^2 + (Nu^2 + \frac{1}{4}u^4)^{\frac{1}{2}} \quad (16)$$

31. The midpoint of the interval $[L'/M, U'/M]$ is $(N + \frac{1}{2}u^2)/M$, which has expectation $\lambda + \frac{1}{2}u^2/M$. Hence the interval is not symmetrical about an unbiased estimator of λ , but this will be approximately so when N is large compared with $\frac{1}{2}u^2$. An unbiased estimator of λ is N/M , and

$$(N + \frac{1}{2}u^2)/M = (N/M)(1 + \frac{1}{2}u^2/N)$$

is asymptotic with N/M as N increases. The explicit form of eqn (15) and (16), in contrast with eqn (12) and (13), allows the algebraic expression of an important quantity, the relative width of the confidence interval $[L'/M, U'/M]$, namely:

$$H = (U'/M - L'/M)/\{\frac{1}{2}(U'/M + L'/M)\} = 2(Nu^2 + \frac{1}{4}u^4)^{\frac{1}{2}}/(N + \frac{1}{2}u^2)$$

H is independent of M , and it can be seen that H approaches zero as N increases. From another direction, $\frac{1}{2}H$ measures the proportionate error in N/M as a point estimator of λ , with confidence $100(1 - \alpha)$ per cent. In Ref. 2, Foldvary has adopted the principle that, provided the mileage M is adequately determined, when:

(a) $N \geq 30$, the calculated rate N/M is a sufficiently accurate estimator of λ ,

(b) $10 \leq N < 30$, the rate should be treated with caution, and

(c) $N < 10$, the rate would be so suspect that it should not even be calculated.

If a 95 per cent level of confidence is adopted, $\frac{1}{2}H$ is 0.54 and 0.34 when N equals 10 and 30 respectively. In other words, even for N as large as 30, there is only 95 per cent confidence that λ will be covered by the interval $[0.66 N/M, 1.34 N/M]$, i.e. $N/M \pm 34$ per cent.

ILLUSTRATIVE EXAMPLE

32. An illustrative example of the application of the techniques previously described will now be given. This will be based on some data given in Ref. 3, which allow an investigation of the risk of drivers of private vehicles being involved in accidents. It is assumed that the number of accidents in which each driver participates satisfies eqn (1). Two attributes with mutually exclusive levels will be considered, namely driver sex (A), an obvious dichotomy, and driver age (B), with six age-classes as levels. It was considered prudent not to give a three-attribute example of risk analysis at this time since in practice three attributes are seldom studied together, and the oft-used two-attribute case is sufficient to illustrate the methods. TABLE I gives the number of participations of drivers in accidents, classified according to the attributes A and B defined above. For comparison with these accident data, the total mileage driven by all drivers during the same period, classified by A and B, is ideally required, but the cost of obtaining such information is prohibitive. An alternative approach is to estimate these distances by a sample survey, a technique employed in Ref. 3. TABLE II contains estimates of the mean daily mileage driven by all drivers, also

TABLE I

DRIVERS INVOLVED IN ACCIDENTS. PRIVATE VEHICLES
(BRISBANE 1961)

Driver Age	Driver Sex		
	Male	Female	Total
< 20	1028	53	1081
20 - 29	2370	224	2594
30 - 39	1617	180	1797
40 - 49	1260	164	1424
50 - 59	823	99	922
≥ 60	459	49	508
Total	7557	769	8326

TABLE I: Drivers of private vehicles involved in reported accidents in the Brisbane Metropolitan area during 1961, classified according to driver sex and age. In Queensland it is compulsory to report accidents involving personal injury requiring medical attention and accidents resulting in property damage of value \$50 or more. From Ref. 3, Table IIIA.

TABLE II

ESTIMATED MEAN DAILY MILES DRIVEN*

Driver Age	Driver Sex		
	Male	Female	Total
< 20	48	7	55
20 - 29	250	45	295
30 - 39	289	69	358
40 - 49	300	47	347
50 - 59	226	25	251
≥ 60	152	17	169
Total	1265	210	1475

TABLE II: Estimated mean daily miles (in thousands) driven by all drivers of private vehicles in the Brisbane Metropolitan area during 1961, classified according to driver sex and age. From Ref. 3, Table IIIA.

*Figures given are estimated miles divided by 1000.

TABLE III

ESTIMATED RISK* (ACCIDENT RATE)

Driver Age	Driver Sex		
	Male	Female	Total
< 20	21.42	7.57	19.65
20 - 29	9.48	4.98	8.79
30 - 39	5.60	2.61	5.02
40 - 49	4.20	3.49	4.10
50 - 59	3.64	3.96	3.67
≥ 60	3.02	2.88	3.01
Total	5.97	3.66	5.64

TABLE III: Number of drivers of private vehicles involved in reported accidents per 1000 mean daily miles driven in the Brisbane Metropolitan area during 1961, classified according to driver sex and age. Calculated from Tables I and II.

*Figures given are estimated risk multiplied by 1000.

classified by A and B, as given in Ref. 3. These data should all be multiplied by some fixed constant to estimate the total mileages driven during the accident period, but since the scale of these mileages is not important, this has not been done. The fact that the mileage data has only been estimated rather than known absolutely will be ignored henceforth, but in such circumstances it is imperative that the mileage be estimated precisely if subsequent analysis is to be meaningful, and even then any inferences drawn should be treated with caution. A method of inference which allows for estimation of the mileage would be more appropriate in the case of the data of Ref. 3, but such a method is sorely lacking in the literature.

33. TABLE III gives the quotients of the corresponding cells in TABLES I and II, i.e. the estimated risk, or accident rate as it is usually known. The scale of risk is also not important. The hypothetical accident frequencies for the two-attribute risk analysis appear in TABLE IV. The frequencies in the two margins are of the form $Np_{i.}$ and $Np_{.j}$, since they relate to tests of the hypotheses $H_0^{(A)}: \mu_{i.} = 1$, all i , and $H_0^{(B)}: \mu_{.j} = 1$, all j . The frequencies in the body of TABLE IV are of the form

$$N\hat{\pi}_{ij} = N\hat{\mu}_{i.}\hat{\mu}_{.j}p_{ij} = N[N_{i.}/(Np_{i.})][N_{.j}/(Np_{.j})]p_{ij}$$

since they relate to a test of the hypothesis $H_0^{(AB)}: \mu_{ij} = \mu_{i.}\mu_{.j}$, all i, j . The three chi-square criteria which together perform the risk analysis are:

$$X_A^2 = \sum_i (N_{i.} - Np_{i.})^2 / (Np_{i.}) = 170.55, \text{ on 1 d.f.} \quad (17)$$

$$X_B^2 = \sum_j (N_{.j} - Np_{.j})^2 / (Np_{.j}) = 2982.54, \text{ on 5 d.f.} \quad (18)$$

$$X_{AB}^2 = \sum_i \sum_j (N_{ij} - N\hat{\pi}_{ij})^2 / (N\hat{\pi}_{ij}) = 85.74, \text{ on 5 d.f.} \quad (19)$$

34. In a like manner as in Ref. 6, it was decided that critical values should be chosen after adopting the principles that α should not exceed 0.001 and that values of κ less than 0.15 are trivial. The consequences of exactly these principles have been discussed earlier. Since the total accidents (8326) exceeds 3778, it was necessary to choose the critical values as in eqn (9). Consequently, the critical values are:

$$C_{1;8326} = 60.88 \text{ and } C_{5;8326} = 71.74$$

Comparison of each of the three chi-square criteria in the previous paragraph with the appropriate critical value indicates that each criterion is significant. It is concluded that all three hypotheses, $H_0^{(A)}$, $H_0^{(B)}$ and $H_0^{(AB)}$, are false. It remains to determine the positions in the risk space which provide the major contributions towards rejection of these hypotheses.

35. Since the criterion X_A^2 for testing the difference in risk between the sexes has only 1 degree of freedom, the full conclusion from its significance is easily determined. Comparing the observed total involvements for male drivers (7557) in TABLE I with the hypothetical total involvements (7140.6) in TABLE IV under the hypothesis $H_0^{(A)}$ of no risk difference between sexes, it is apparent that $H_0^{(A)}$ is rejected because of a high risk for male drivers. The situation is more complex when the significant criterion has more than 1 degree of freedom. The right-hand margin of TABLE V contains the age-specific χ^2 criteria:

$$X_{(j)}^2 = (N_{.j} - Np_{.j})^2 / (Np_{.j})$$

TABLE IV
HYPOTHETICAL NUMBER OF DRIVERS INVOLVED IN ACCIDENTS

Driver Age	Driver Sex		
	Male	Female	Total
< 20	998.4	89.3	310.5
20 - 29	2326.5	256.7	1665.2
30 - 39	1535.2	224.7	2020.8
40 - 49	1302.9	125.1	1958.7
50 - 59	878.6	59.6	1416.8
≥ 60	483.5	33.2	954.0
Total	7140.6	1185.4	8326.0

TABLE IV: Hypothetical numbers of drivers involved in accidents, under the null-hypotheses of the two-attribute risk analysis.

TABLE V
RISK ANALYSIS CHI-SQUARE CRITERIA

Driver Age	Driver Sex		
	Male	Female	Total
< 20	0.88	14.73	1912.42
20 - 29	0.81	4.17	518.06
30 - 39	4.35	8.89	24.79
40 - 49	1.41	12.08	145.98
50 - 59	3.52	26.09	172.82
≥ 60	1.25	7.58	208.48
Total	24.28	146.27	0

TABLE V: Risk analysis χ^2 criterion for each cell, defined as follows: if A is the cell entry in Table I and E is the corresponding entry in Table IV, then the corresponding entry in Table V is $(A-E)^2/E$.

TABLE VI
LOWER AND UPPER 95 PER CENT CONFIDENCE LIMITS FOR RISK

Driver Age	Driver Sex		
	Male	Female	Total
< 20	20.15	5.79	18.52
	22.77	9.90	20.86
20 - 29	9.11	4.36	8.46
	9.87	5.67	9.14
30 - 39	5.33	2.25	4.79
	5.87	3.02	5.26
40 - 49	3.97	2.99	3.90
	4.44	4.07	4.32
50 - 59	3.40	3.25	3.44
	3.90	4.82	3.92
≥ 60	2.76	2.18	2.76
	3.31	3.81	3.28
Total	5.84	3.41	5.52
	6.11	3.93	5.77

TABLE VI: Lower and upper 95 per cent confidence limits for risk, calculated using the normal distribution approximation to the Poisson distribution.

which are the components of X_B^2 in eqn (18.) As mentioned earlier, the criterion $X_{(j)}^2$ with critical value $\frac{1}{2}C_{1,N}$ provides a suitable test of the hypothesis $H_0^{(j)}: \mu_{.j} = 1$. It turns out that all criteria $X_{(j)}^2$ exceed $\frac{1}{2}C_{1,8326} = 30.44$, with the exception of the criterion specific to the age-class 30-39. Comparison of the age-specific observed and hypothetical total involvements in TABLES I and IV leads to the conclusion that the age-classes: (a) <20, and (b) 20-29, are each associated with a high risk, and that the age-classes: (a) 40-49, (b) 50-59, and (c) ≥60, are each associated with a low risk.

36. The body of TABLE V contains the χ^2 criteria

$$X_{(ij)}^2 = (N_{ij} - N\hat{\pi}_{ij})^2 / (N\hat{\pi}_{ij}),$$

which are the components of X_{AB}^2 in eqn (19). It was recommended earlier that the criterion $X_{(ij)}^2$ with critical value $\frac{1}{2}C_{1,N}$ provides a suitable test of the hypothesis $H_0^{(ij)}: \mu_{ij} = \mu_{i.}\mu_{.j}$. The only criterion $X_{(ij)}^2$ which exceeds $\frac{1}{2}C_{1,N} = 15.22$ is that corresponding to female drivers of age 50-59. Comparison of the observed involvements (99) for these drivers with the hypothetical involvements (59.6) under the hypothesis $H_0^{(AB)}$ indicates that $H_0^{(AB)}$ is rejected because the risk for female drivers of age 50-59 is higher than expected if $H_0^{(AB)}$ were true. It is concluded that female drivers of age 50-59 have a high risk relative to the hypothesis $H_0^{(AB)}: \mu_{ij} = \mu_{i.}\mu_{.j}$, all i, j , and that the major contribution to the interaction between the attributes driver sex and driver age in the risk space is due to these drivers.

37. TABLE VI contains the upper and lower 95 per cent confidence limits for the risk of accident involvement, classified by driver sex and age. Point estimates of these risks have previously been given in TABLE III. Since the smallest number of accident involvements (49) in TABLE I is still fairly large, the normal distribution approximation to the Poisson distribution is an adequate basis for calculating the confidence limits. Consequently, the confidence limits in TABLE VI were calculated on the basis of eqn (15) and (16).

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DISCUSSIONS

D. C. ANDREASSEN,
B.Sc., M.Tech., M.H.F.S., Chief Engineer, Traffic Commission of Victoria.

38. I read, with personal agreement, the points raised by the author in para. 1 and 2 about the use of 'rates'; in particular the author says "... some users ... are not really sure what accident rates are or mean". The author has raised this and other important questions in his introduction but as far as I could see has not answered them, unless he has disposed of them by saying "Without a probabilistic model ...". Would the author like to comment on this?

R. J. VAUGHAN,
B.Sc., M.Eng.Sc., M.E., A.R.R.B. Research Fellow, University of Adelaide.

39. With reference to para. 3, Goulbergh and I (Paper 723) found that λ varies with road geometry and furniture.

40. With reference to para. 4, in sampling for λ we must note that most drivers use the same roads but some classes of drivers may not.

41. TABLE III exhibits a downward trend in λ (i.e. a leaning curve) indicating that $\lambda(t)$ over a long period is not stationary.

42. Could the author form a test along the lines of the points demerit system?

AUTHOR'S CLOSURE

To J. C. LANE
(See Introductory Remarks)

43. The writer mentions two new distributions described by Cresswell and Froggatt, and I take it that this refers to the Long Distribution and Short Distribution in Chapter XIII of the reference. In practice I do not think that the use of either of these distributions rather than a Poisson distribution would make a great deal of difference because they are both essentially Poisson in nature, and have the added disadvantage of relying on the difficult concept of randomly occurring 'spells' during which accidents can occur. In the paper it is not claimed that the accident process of each individual driver is a Poisson process, but that evidence of other accident researchers suggests that a Poisson process is a sufficiently good and simple model for useful inferences to be made.

44. I cannot agree with the writer that there is no conflict between deterministic models and stochastic models of accident processes. A deterministic model can give only mean accident risks or numbers, whereas a stochastic model also gives variances for these estimates as well as allowing the use of the very powerful methods of statistical inference. A stochastic model need not be limited to individuals or

small groups; in fact the Poisson model is such that its application to groups of any size does not cause any difficulty.

To D. C. ANDREASSEN

45. A probabilistic model (and a stochastic model is probabilistic) may provide a conceptual framework within which accident rates can be understood. A Poisson accident process is one such probabilistic model. It turns out that N , the number of accidents in M miles, has a Poisson distribution and the distribution of N/M can be found. Hence it emerges that the accident rate N/M is in fact a *random variable*; the mean of its distribution is *estimated* by a realization of N/M with all the usual errors of estimation. With the Poisson process model the mean of the distribution of N/M is an important quantity which I have called the *risk*.

To R. J. VAUGHAN

46. The form of eqn 1 in para. 3 of the paper can be invalidated if the accident process of the individual driver is such that the risk at mileage m is a function of that driver's accident experience in the mileage interval $(0, m)$. If this is not the case and eqn 1 is valid, then λ in eqn 1 represents a mileage-weighted average of all the various levels of risk experienced by that particular driver in the particular M miles driven, including any different levels of risk due to road geometry and furniture.

47. The writer comments that in sampling for λ we should note that some drivers may not always use the same roads. This may cause further problems in interpreting λ (and the problems of interpretation are not insignificant) but does not in itself invalidate the Poisson process model.

48. I agree with the writer that over a long period (especially a driver's lifetime) $\lambda(t)$ is not stationary. However, this need not invalidate the Poisson process model for an individual driver over a short period (say, at most a year or two) of driving. Sichel's meagre data indicate that over a short period λ is approximately stationary. Hence the non-stationarity of $\lambda(t)$ over a driver's lifetime would not be a problem in contemporary accident studies usually based on one year's accident data.

49. The formulation of a test along the lines of the points demerit system escapes me without further indication of what the writer has in mind.

Paper No. 2

J.E. Cowley and M.H. Cameron,
"Prediction of motor vehicle occupant fatality trends
following seat belt wearing legislation".

Proceedings, Eighth Conference,
Australian Road Research Board,
Perth 1976.

PREDICTION OF MOTOR VEHICLE OCCUPANT FATALITY TRENDS FOLLOWING SEAT BELT WEARING LEGISLATION*

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ABSTRACT

One of the most important road safety measures introduced in Australia in recent years has been the legislation on compulsory wearing of seat belts in motor vehicles. As a result of this measure, road traffic accident fatalities are now some 10 to 20 per cent below the pre-legislation trend. An accurate analysis of the post-legislation casualty trends is not straightforward because of the small amount of post-legislation data available, and because new road safety measures are being continually introduced. It is important to quantify the effect of the legislation so that accurate assessments of new road safety measures can be carried out. The paper describes an initial attempt at developing a model for the predicted reduction in occupants killed resulting from a large increase in seat belt wearing rates, and at calibrating the model using Victorian data.

INTRODUCTION

1. It has been recognised for many years now that seat belts — particularly lap/sash seat belts — are effective in reducing injuries to motor vehicle occupants in road traffic collisions. Australia therefore took a positive step forward when the fitting of lap/sash belts was made compulsory for front (outer) seating positions in cars (and car derivatives) first registered in or after January 1969 (note that South Australia introduced equivalent legislation two years earlier), and appropriate belts for all car seating positions were made compulsory for cars (and car derivatives) registered in or after January 1971. Recent accounts of the relevant Australian Design Rules (ADR) are given in Vulcan, Ungers and Milne (1975) and McKenzie and Milne (1976), which include information on legislation requiring the retro-fitting of seat belts to older vehicles.

2. The problem of getting motorists to wear available seat belts was overcome in Victoria by the introduction of compulsory wearing legislation in December 1970. The remaining States and Territories introduced similar legislation nine to 12 months later, so that by January 1972 compulsory seat belt wearing was in force throughout Australia. The Australian legislation generally applies to motor vehicle occupants aged eight years or older, but

Victoria has now introduced legislation covering restraint wearing for occupants under eight years of age who occupy front seating positions.

3. The wearing legislation resulted in a large increase in overall seat belt wearing rates throughout Australia, and appeared to have a dramatic effect on reported casualties as shown by figures from the Australian Bureau of Statistics (ABS) given in Figs 1 to 3. These graphs show motor vehicle occupants killed and injured in New South Wales, Victoria and the rest of Australia during 1963 to 1974, for drivers, passengers and all motor vehicle occupants respectively. The legislation is shown on these graphs as having been introduced at the end of 1970 in Victoria and the end of 1971 in all other States and Territories. Linear regression trends are shown for the pre-legislation periods.

4. Fig. 4 shows the equivalent data for all non-motor vehicle occupants — pedestrians, motor cyclists, pedal cyclists and others — which generally depict increases in casualties during the 1970 to 1974 post-legislation period; these are mainly due to motor cycle accidents following the recent rapid rise in popularity of the motor cycle in Australia.

5. Figs 1 to 3 generally show discontinuities in the casualty trends — particularly for fatalities — which could be attributed to the seat belt wearing legislation; indeed, a large number of papers have shown that the legislation has been effective. Andreassend (1972) and Foldvary and Lane (1974) compare casualty data for Victoria and other States during the first year of operation of the Victorian legislation with pre-legislation data, using mainly contingency table analysis; the latter reference shows the importance of examining metropolitan area statistics separately. Crinion, Foldvary and Lane (1975) report an analysis of South Australian casualties during the year preceding and following the wearing legislation, using contingency table analysis, in which account was taken of exposure to risk by analysing numbers of vehicles involved in all reported accidents, as well as casualties. Henderson and Wood (1973) and Vaughan, Wood and Croft (1974) use regression analysis to compare one to three years'

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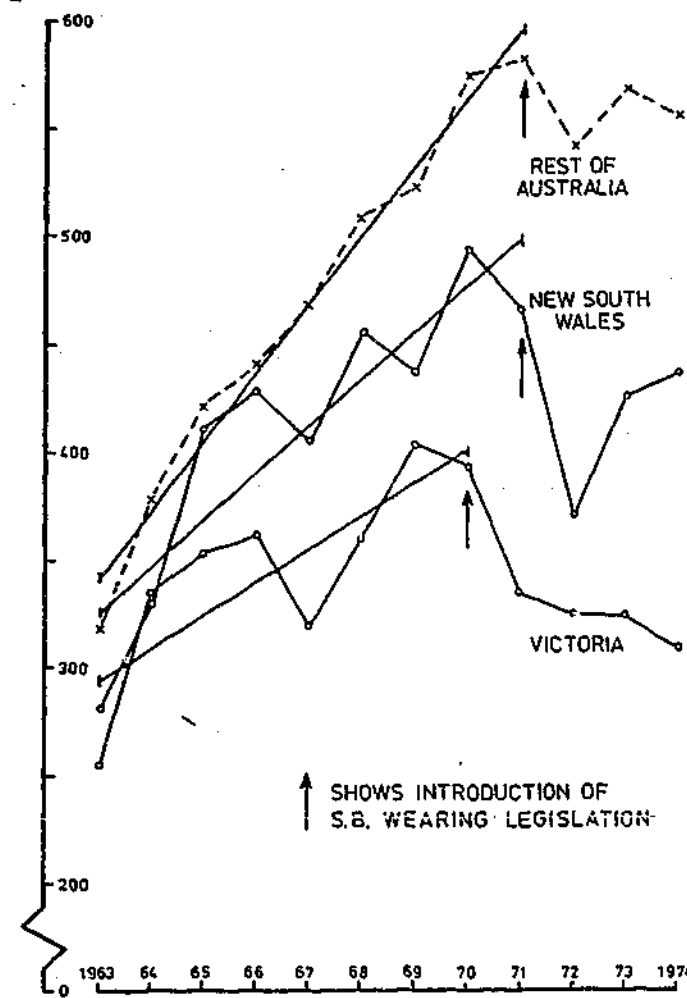


Fig. 1(a) — Motor vehicle drivers killed in Australia — 1963 to 1974

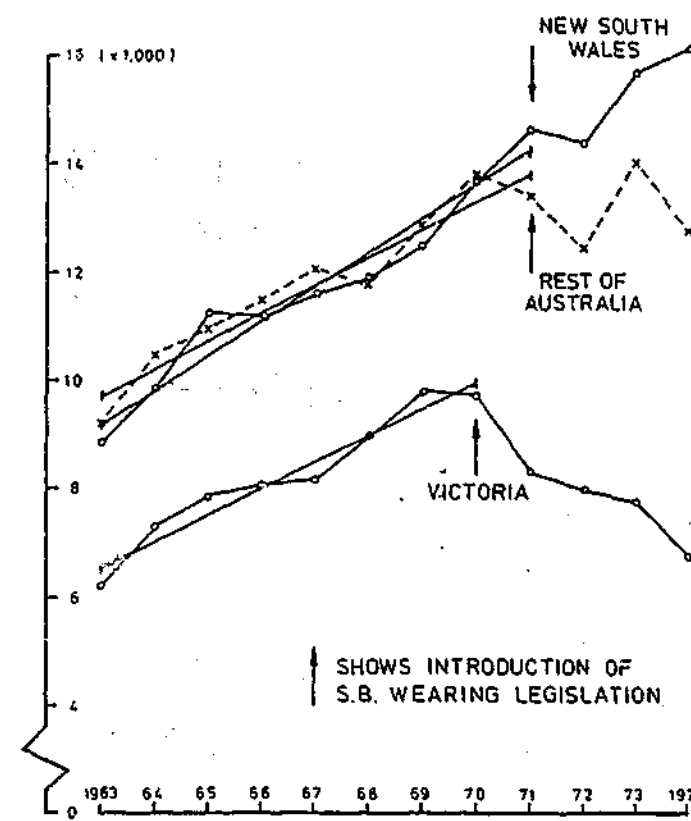


Fig. 1(b) — Motor vehicle drivers injured in Australia — 1963 to 1974

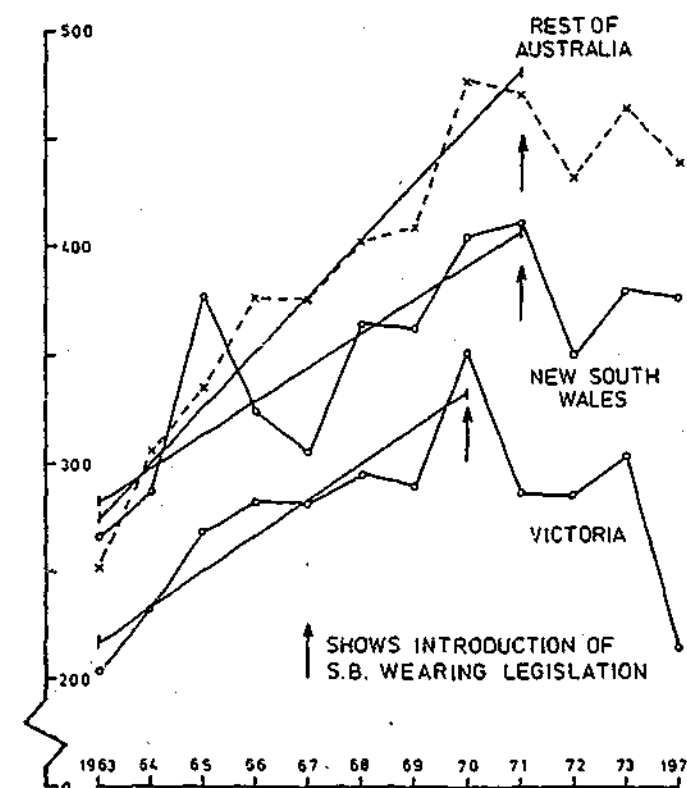


Fig. 2(a) — Motor vehicle passengers* killed in Australia — 1963 to 1974
(* Includes motor cycle pillion passengers)

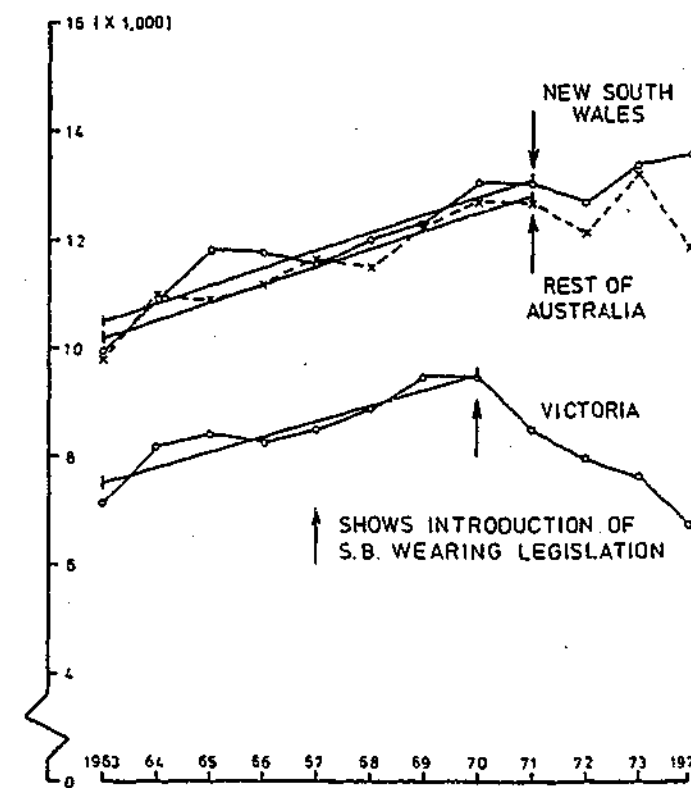


Fig. 2(b) — Motor vehicle passengers* injured in Australia — 1963 to 1974
(* Includes motor cycle pillion passengers)

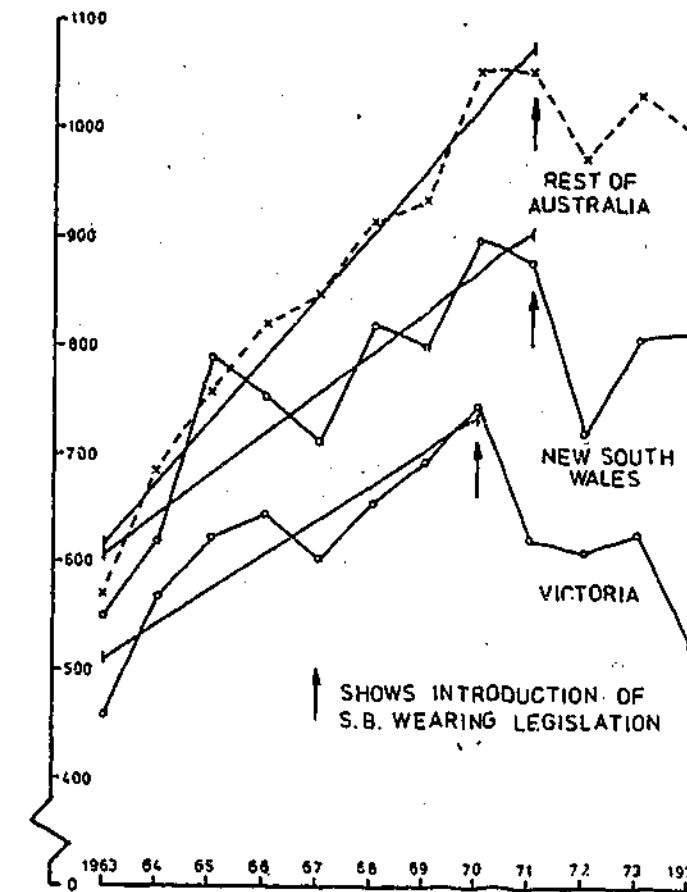


Fig. 3(a) — Motor vehicle occupants killed in Australia — 1963 to 1974 (Total from Figs 1(a) and 2(a))

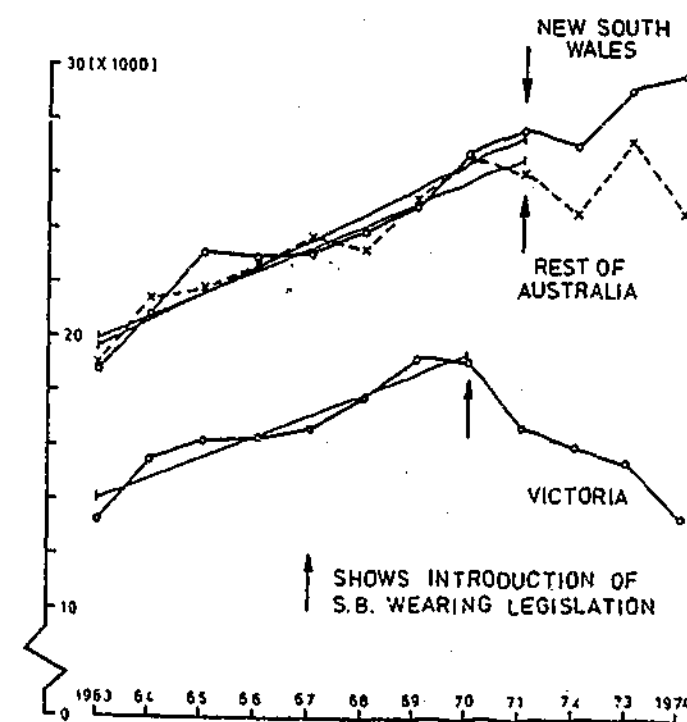


Fig. 3(b) — Motor vehicle occupants injured in Australia — 1963 to 1974 (Total from Figs 1(b) and 2(b))

post-legislation casualty data in New South Wales with pre-legislation trends; this analysis includes possible exposure measures such as time series of vehicles registered, driving licences issued and consumption of motor spirit. The most recent analysis of the effectiveness of wearing

legislation is given in Vulcan *et al.* (1975), in which post-legislation casualty data up to and including 1974 is compared by regression analysis with pre-legislation trends.

6. The references summarised above have clearly demonstrated the effectiveness of the wearing legislation in Australia. However, now that casualty data are available for three to four post-legislation years, it is appropriate to check whether all these data are consistent with a sharp increase, and subsequent rise, in seat belt wearing rates alone or whether they represent evidence of the effectiveness of other road safety measures. Methods for predicting post-legislation trends in fatalities due to increased seat belt wearing are presented in this paper.

7. An analysis using such methods is necessary for an evaluation of new road safety measures which are being continually introduced. Examples are: the numerous Australian Design Rules for motor vehicles, speed limits and speed zoning, and (proposed) legislation on 'random' breath testing. The effectiveness of some of these later measures may be small or short-lived, but as it is necessary to determine their effectiveness in many cases, a clearer understanding of the outcome of the seat belt legislation is required. An example of a study where an accurate measurement of the effect of the legislation alone was necessary is given in Cowley (1975), which examines possible reasons for the sharp reduction in Victorian vehicle occupant fatalities in 1974 (see Figs 1 to 3). This may have been due to the introduction of an absolute speed limit, a change in motor vehicle occupancy, a decrease in travel, or other changes. Other examples would include the differences in trends in occupants injured between New South Wales and other States during 1971-74, and differences in trends in casualties between Victoria and other States during the latter half of 1971-74, as shown in Figs 1 to 3.

8. In addition, the analysis methods presented in this paper illustrate the need for accurate information on seat belt wearing rates, in vehicles on the roads and in vehicles involved in accidents, as well as fatality rates for persons wearing and not wearing seat belts, both before and after the introduction of seat belt wearing legislation. The methods presented should therefore be of interest to other Governments and authorities who are considering the introduction of seat belt wearing legislation and who wish to measure its effectiveness.

9. The main aim of this paper is therefore to predict the changes which should occur in the time series of motor vehicle occupants killed resulting from the introduction of seat belt wearing legislation. It should be regarded as an initial attempt at developing suitable post-legislation trends, because the analysis is restricted to readily available data on road traffic accident casualties; possible refinements to the analysis requiring more detailed casualty data are discussed later. The methods are developed using Victorian data, because much of the information relevant to the problem is available for this State only.

FATALITY REDUCTION FACTOR, k

10. As stated earlier, the seat belt wearing legislation led to a large increase in wearing rates. The effect of a change in wearing rate on occupant fatalities can be examined in the following way:

Let a = the total number of road traffic accidents (defined by some property damage criterion (desirably zero), and assumed to cover all casualty accidents) within a given region,
 n = the average number of cars and car derivatives involved in such accidents,

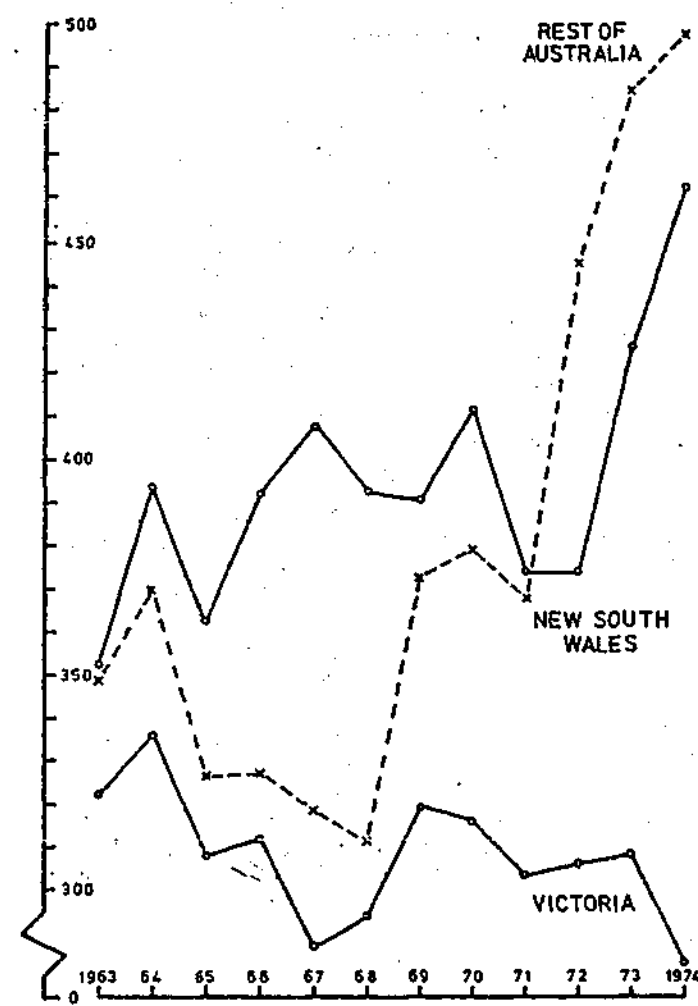


Fig. 4(a) — Non motor vehicle occupants killed in Australia — 1963 to 1974

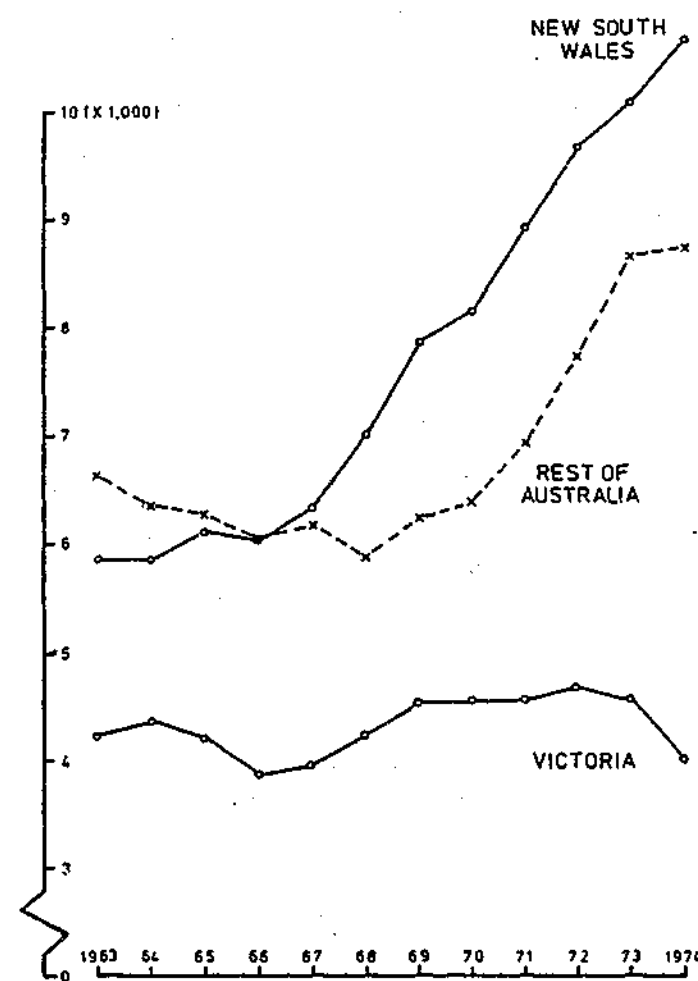


Fig. 4(b) — Non motor vehicle occupants injured in Australia — 1963 to 1974

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n_0 = the average number of occupants of a given type (e.g. rear seat passengers) involved in such vehicles,
 r = the overall seat belt wearing rate of these occupants — hereinafter referred to as wearing rate — defined as the proportion of such occupants wearing belts; hence, r will be given by the product of seat belt fitting rate — the proportion of occupant seating positions fitted with seat belts, and seat belt usage rate — the proportion of available belts in given seating positions which are being used,
 p_b = the probability of such an occupant being killed if involved in an accident and wearing a seat belt,
 p_{nb} = the corresponding kill probability whilst not wearing a seat belt, and
 s = the probability ratio, p_{nb}/p_b , which will generally exceed unity.

Then the number of these occupants killed within the region will be given by:

$$an_0(p_b r + p_{nb}(1-r))$$

11. Thus if the seat belt wearing rate is changed from a hypothetical rate r_1 (without legislation) to a rate r_2 (with legislation), then for given values of a , n , and n_0 , the ratio of fatalities with the higher wearing rate, r_2 , to those with the lower wearing rate, r_1 , will be given by:

$$k = \frac{p_b r_2 + p_{nb}(1-r_2)}{p_b r_1 + p_{nb}(1-r_1)} \\ = \frac{r_2 + s(1-r_2)}{r_1 + s(1-r_1)}$$

provided that p_b and p_{nb} do not change as a result of the legislation *per se*.

12. The fatality reduction factor, k , is thus a measure of the effect of a change only in wearing rate. The assumption of constant p_b and p_{nb} (and hence s) is by no means straightforward because it is possible that p_b will increase as a result of the legislation, due to belts being worn in an improper or insecure manner.

13. For $r_2 > r_1$ and $s > 1$, the fatality ratio k is always less than unity. This simple model therefore states that the post-legislation trend, with wearing rate r_2 , will be that of the hypothetical trend, with wearing rate r_1 , but factored down by k .

14. The factor k is sensitive to the values of r_1 , r_2 and s . For example, for $r_1 = 0$, $r_2 = 1$ (which would be the extreme values of wearing rate), then for $s = 2$ and 4, the respective values of k are 0.50 and 0.25. If, however, r_1 and r_2 are changed to 0.25 and 0.75 respectively (which may be closer to the rates achieved in practice), then for $s = 2$ and 4, the values of k are 0.71 and 0.54 respectively. Thus it is obvious that the parameters r_1 , r_2 and s need to be estimated accurately and the following sections examine this problem in some detail using Victorian data.

15. Such estimation must include the time variation of r_1 and r_2 as these can be slowly varying functions. Fig. 5 illustrates this point by sketching typical time series for occupant fatalities for various wearing rate functions r_1 and r_2 . In the 'steady-state', when both the hypothetical wearing rate r_1 and the post-legislation wearing rate r_2 have reached constant values, then k is a constant and the asymptotic post-legislation trend in fatalities is a constant times the hypothetical trend. However, during the 'transient' stage the time variations of r_1 and r_2 can lead to occupant fatality curves which have negative slopes with time. As Australia is currently in this transient stage, the time variation of r_1 and r_2 is important, as is discussed in the following sections.

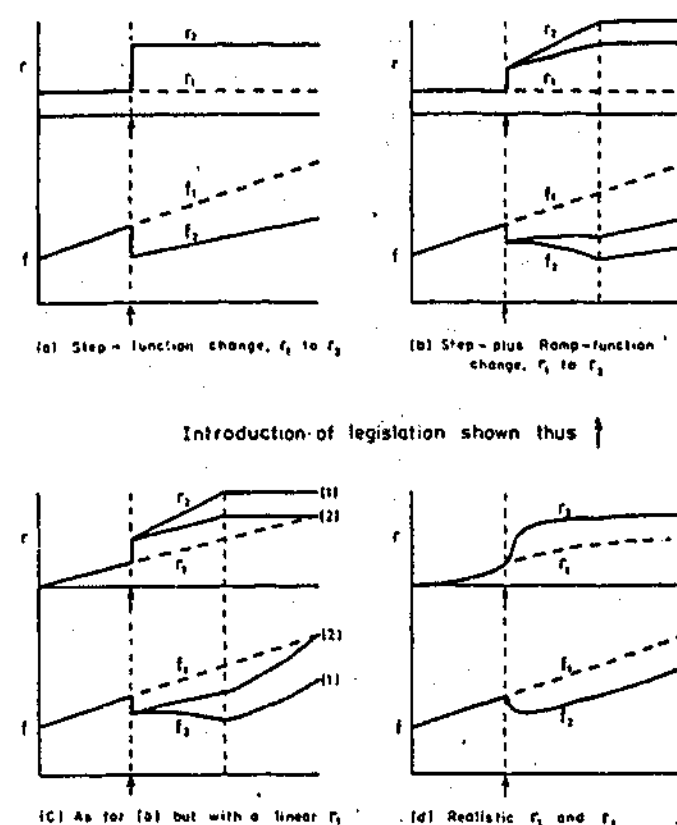


Fig. 5 — Sketches of fatality trends (f) for various overall seat belt wearing rates (r) before and after introduction of seat belt wearing legislation

REALISTIC r_1 AND r_2

16. The Road Safety and Traffic Authority (RoSTA) have collected comprehensive data on the Victorian post-legislation fitting, usage and wearing rates. Fig. 6 shows the data for lap/sash belts for drivers of cars and car derivatives, taken from a RoSTA table by Andreassend (quoted in Vulcan *et al.* 1975). For Melbourne, the value of r_2 has risen from 55 per cent in mid 1971 (six months after the legislation came into force), to 79 per cent in early 1975; the corresponding rural values are lower at 44 per cent and 77 per cent, but indicate that the rural rate has nearly caught up with the urban rate. The Victorian usage rate may be approaching an asymptote between 80 per cent and 90 per cent, implying that the asymptotic value of the wearing rate, r_2 , will fall within this band. Fig. 6 shows unexpected downturns in fitting and usage rates in mid 1973. As the sample sizes used in the RoSTA surveys are large — typically six to 13 thousands during 1972 to 1974 — the mid 1973 dip could be significant.

17. The data in Vulcan *et al.* (1975) are for cars only. Andreassend (1972) describes preliminary results from an on-going study of seat belt wearing at the time of his writing, presumably late 1971/early 1972. He found that the left front passenger usage rate (where lap/sash belts were fitted) was 8 per cent absolute less than the driver rate, in both metropolitan and rural areas. In a sub-study of 1322 cars occupied by both drivers and front left passengers, Andreassend found that 1.4 per cent of the cars had a seat belt fitted to only one of those seating positions.

18. Since it is likely that the driver's seat is always fitted in preference to the passenger seat in such cars, the passenger fitting rate was taken as 1.4 per cent absolute less than the driver rate. These reductions in fitting and usage rate were applied (by subtraction) to the corresponding driver rates in Fig. 6 to estimate front left pas-

senger fitting, usage and, finally, wearing rates by calculation.

19. As the wearing rates shown in Fig. 6 have reasonably linear trends during the 1971-74 period, least squares linear trend lines were fitted to the data separately for metropolitan and rural areas for drivers and front left passengers. These four trend lines were fitted to smooth the wearing rate estimates. The trend lines were then used to estimate wearing rates at the mid-points of each of the years 1971-74, as given in Table 1. Finally, the metropolitan and rural wearing rates were averaged (i.e. assuming equal occupant miles in each area) to estimate Victoria-wide rates, also shown in Table 1. These results were taken as the best estimates of r_2 for drivers and front left passengers, respectively, for the four year period; these regression lines should not be extrapolated beyond 1974.

20. Estimates of r_1 , the hypothetical wearing rates in the absence of legislation, must be less exact since they cannot be observed directly. Andreassend (1972) gives fitting rates of lap/sash belts in the driver and front left passenger positions (combined) of cars observed in Melbourne car-parks in surveys conducted during 1965-71. These fitting rates are shown in Fig. 7, together with the fitting rates of Melbourne drivers for 1971-75 given in Fig. 6. There is strong indication of a logistic type trend in front seat fitting rates. Also shown is the effective date of Australian Design Rule No. 4 (ADR 4), which requires that cars and car derivatives first registered on or after 1 January 1969 must have lap/sash belts fitted to the front outboard seating positions. ADR 4 and the subsequent

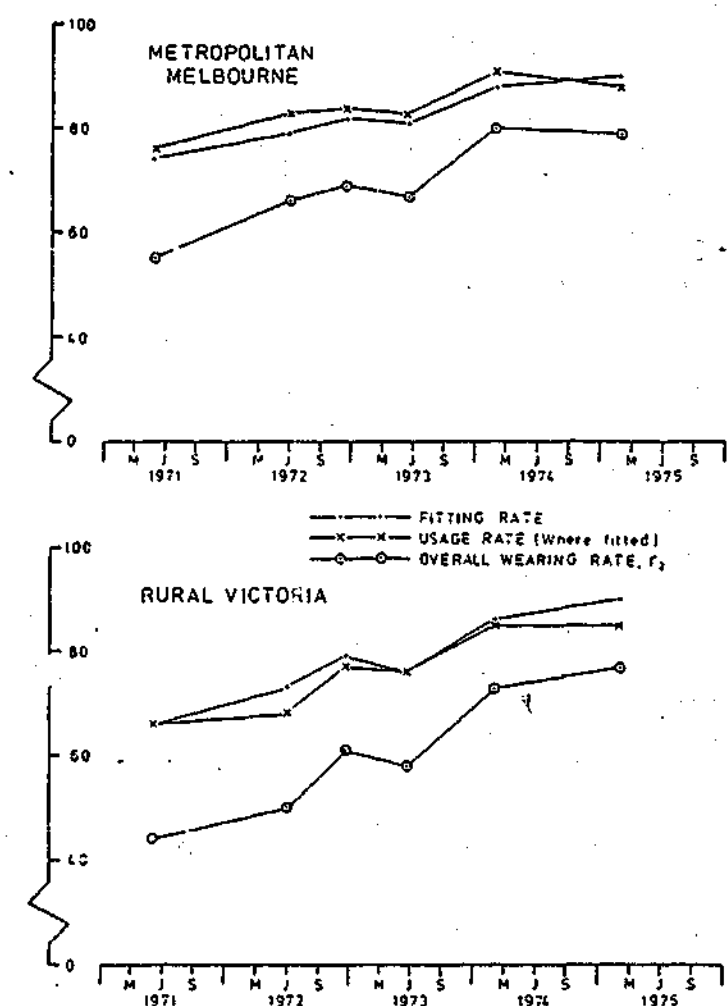


Fig. 6 — Fitting, usage and overall wearing rates (per cent) of lap/sash belts by drivers of cars and car-derivatives in Victoria after introduction of seat belt wearing legislation

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TABLE I
ESTIMATES OF ACTUAL WEARING RATES (r_2) FOR DRIVERS AND FRONT LEFT PASSENGERS IN VICTORIA, 1971 TO 1974 (PER CENT)

Year (Mid-point):	1971	1972	1973	1974
Drivers				
Metropolitan Melbourne	58	64	71	78
Rural Victoria	44	54	63	72
All Victoria	51	59	67	75
Front Left Passengers				
Metropolitan Melbourne	51	57	64	70
Rural Victoria	38	47	55	64
All Victoria	45	52	59	67

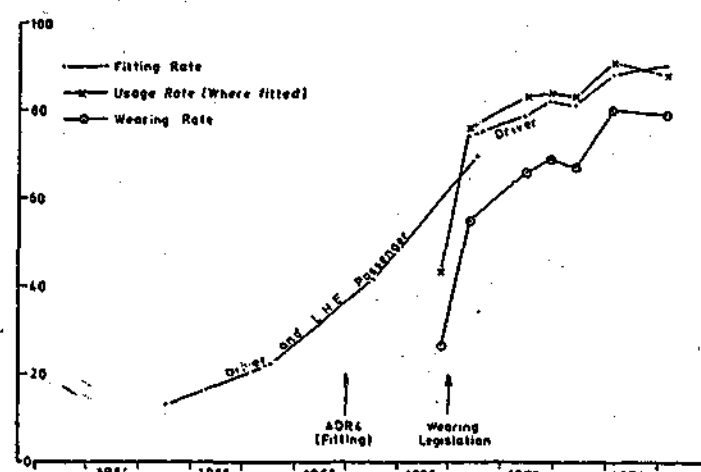


Fig. 7—Fitting, usage and overall wearing rates (per cent) of lap/sash belts by drivers of cars and car-derivatives in metropolitan Melbourne before and after introduction of seat belt wearing legislation

retro-fitting legislation will lead eventually to 100 per cent fitting of lap/sash belts in these seating positions.

21. Data on the likely usage rates in the absence of legislation are sparse. Some measurements of Melbourne drivers in late 1970 (Australian Road Research Board 1971) indicate a wearing rate of 26 per cent. Fig. 7 indicates a fitting rate of about 60 per cent at that time. These two figures suggest a usage rate of about 43 per cent just before the seat belt wearing legislation came into force. It is therefore assumed that the usage rate of Melbourne drivers would have remained at 43 per cent throughout 1971-74 in the absence of legislation; however it is possible that more refined estimates could be made.

22. The fitting rates for drivers shown in Fig. 6 have reasonably linear trends during 1971-74, though it is likely they are part of logistic trends. Hence least squares linear trend lines were fitted to the data separately for metropolitan and rural areas. Fitting rates for the mid-points of the years 1971-74 were estimated from the trend lines.

23. The hypothetical wearing rates, r_1 , for Melbourne drivers were calculated by applying the 43 per cent usage rate to the estimated fitting rates for 1971-74. The May 1971 figures given by Vulcan *et al.* (1975) suggest a 10 per cent absolute lower usage rate for rural drivers. Hence r_1 for rural drivers was calculated with a usage rate of 33 per cent. Hypothetical wearing rates for front left passengers were calculated using the same rules employed for r_2 , viz. fitting rate 1.4 per cent absolute less than drivers and usage rate 8 per cent absolute less than drivers. The results are shown in Table II, with Victoria-wide wearing rates calculated by averaging the metropolitan and rural wearing rates. These results were taken as the best esti-

mates of r_1 for drivers and front left passengers, respectively.

SENSITIVITY TO s

24. The sensitivity of the model to s , the ratio of probabilities of death in an accident with and without seat belts, is demonstrated by applying it to occupant fatalities for 1963-74 in Victoria. The actual and hypothetical wearing rates in the post-legislation period were taken as estimated in the previous section.

25. Fig. 8 shows that the trends in occupant fatalities were approximately linear during 1963-70 (pre-legislation), but with considerable variation about the trend. The trend is really composed of trends in mileage driven, accident risk, occupancy, and pre-legislation seat belt wearing, but a simple linear trend seems an adequate representation over the period. Accordingly, a least squares linear trend line was fitted to the pre-legislation data and then projected to estimate the occupant fatalities in 1971-74 in the absence of legislation.

26. The fatality ratio, k , described in para. 11, was calculated for each of the years 1971-74 with s taking values 2, 3, 4 and 5 in turn. The ratio is identically one for s equals one. The ratio is the expected proportion of the hypothetical fatalities in the absence of legislation (given by the projected trend line) which actually occur if s is correct. Hence the projected number of fatalities were multiplied by the ratios k , for each s in turn to produce the dashed lines in Fig. 8.

27. It can be seen that the predicted occupant fatalities in the post-legislation period are sensitive to s . This implies that the estimation of s must be accurate to adequately reflect the effects of the legislation. Note that a value of s slightly below 3, while producing trends in good agreement with the actual fatalities in Fig. 8, should not be taken as a final estimate of s since it is known that many other injury-reducing countermeasures (e.g. ADRs) were introduced during 1971-74.

ESTIMATION OF s

28. The parameter s can be estimated from a knowledge of seat belt wearing rates among killed occupants and those in accidents (defined by some property damage criterion). In a specific region during a specific time interval, let:

N_b = number of occupants of a given type (e.g. drivers) killed in such accidents and wearing seat belts, and

M_b = number of occupants of a given type involved in such accidents and wearing seat belts.

Let N_{nb} and M_{nb} be similar numbers for occupants not wearing seat belts. Then N_b is a random variable with

TABLE II

ESTIMATES OF HYPOTHETICAL WEARING RATES (r_1) FOR DRIVERS AND FRONT LEFT PASSENGERS IN VICTORIA IN THE ABSENCE OF SEAT BELT WEARING LEGISLATION, 1971 TO 1974 (PER CENT)

Year (Mid-point):	1971	1972	1973	1974
Drivers				
Metropolitan Melbourne	32	34	36	38
Rural Victoria	22	25	27	29
All Victoria	27	29	31	33
Front Left Passengers				
Metropolitan Melbourne	26	27	29	31
Rural Victoria	17	18	20	22
All Victoria	21	23	25	26

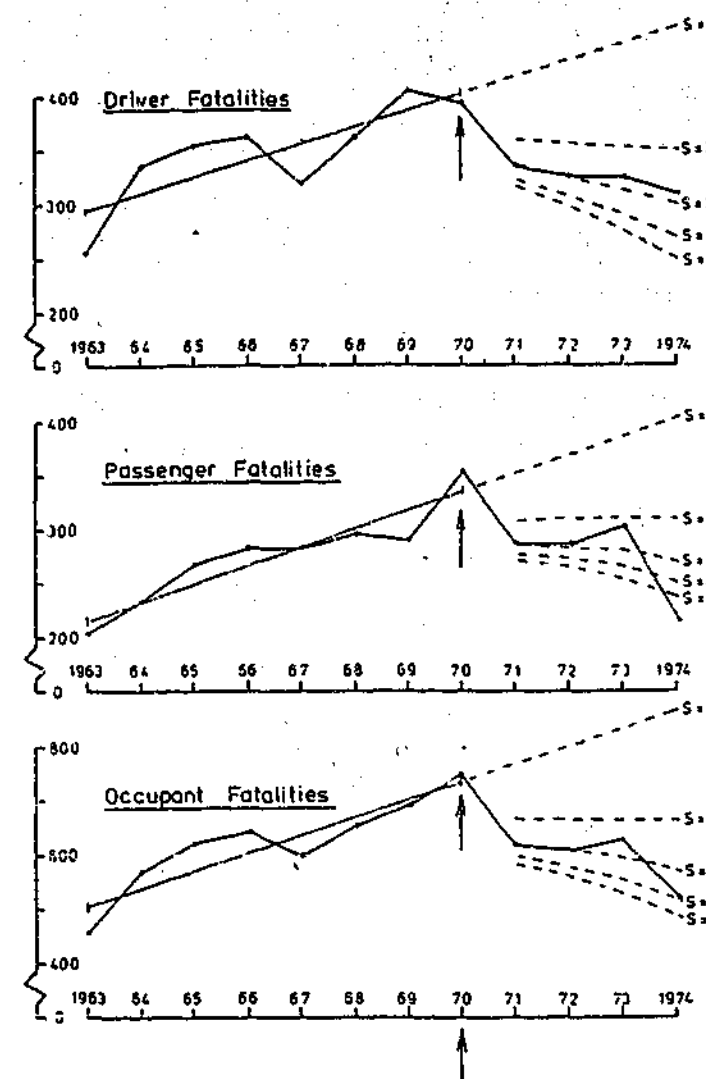


Fig. 8—Motor vehicle occupants killed in Victoria—1963 to 1974. Sensitivity of post-legislation model to value of s for 1971 to 1974

distribution conditional on M_b and p_b , the kill probability while wearing a seat-belt, defined in para. 10. The expectation of N_b is $p_b M_b$; similarly for N_{nb} . It follows that, approximately:

$$N_{nb}/N_b = (p_{nb}/p_b)(M_{nb}/M_b)$$

i.e. $p_{nb}/p_b = (N_{nb}/N_b)(r/(1-r))$ approximately, where r is the seat belt wearing rate of occupants in accidents and estimated by $M_b/(M_b + M_{nb})$. Define a seat belt wearing rate of killed occupants.

$$r' = N_b/(N_b + N_{nb})$$

Then, approximately:

$$s = p_{nb}/p_b = ((1-r')/r)(r/(1-r)) \quad (1)$$

29. Accurate seat belt wearing rates in accidents and among killed occupants are difficult to obtain in the post-legislation period. This is because official accident data in all States and Territories stem from police reports. An upward bias in wearing rates in official data is suspected because occupants involved in accidents are reluctant to report non-wearing to the police and it is likely that the police themselves are similarly reluctant, to avoid the chore of prosecution. Accident data in the pre-legislation period were considered, but rejected in favour of more recent data given by Nelson (1974).

30. Nelson gives results from the Pattern of Injury Survey carried out by the Royal Australasian College of Surgeons (RACS) in Victoria during June 1971 to May 1973, inclusive. Data at the scene of the accident, including seat belt wearing of injured occupants, were collected by ambulance officers on a road crash report form (RCR).

There was a considerable amount of under-reporting of these data, resulting in a strong rural bias. However, it is considered that the reporting of seat belt wearing by ambulance officers is likely to lack most of the bias in police reports.

31. Detailed data on road user injuries were collected by hospitals and coroners on a road trauma report form (RTR). Data collection was compulsory by legislation, so reporting levels were high. For Nelson's analysis, a file of matched RCRs and RTRs was created. The matching criteria were stringent which, together with the under-reporting of RCRs, resulted in the matched file containing only 44 per cent of RCRs and 19 per cent of RTRs.

32. From Appendix C9 in Nelson's report, it was calculated that the wearing rate among fatalities in the matched file was 26.7 per cent for drivers. Unpublished data from Nelson's study indicate that this figure is only slightly affected by the rural bias in the data. Nelson does not give sufficient data to calculate a similar figure for front left passengers.

33. Wearing rates of occupants in accidents defined by a property damage criterion cannot be obtained for Victoria because, in general, only casualty accidents are reportable to the police. Nelson's data cover only vehicles in which at least one occupant was injured sufficiently for an ambulance to be called and police reports cover only accidents in which at least one person was injured sufficiently to require medical treatment. Seat belt wearing rates observed in road-side surveys were considered as a proxy for wearing rates in accidents defined by a property damage criterion. It was suggested that drivers who do not wear seat belts have characteristics which may make them more likely to be involved in accident situations, i.e. wearing rates in accidents may be lower than observed on the road. However, data given by Knaff and Ziegler (1976) suggest that this is not the case. Table III, derived from Knaff and Ziegler, shows wearing rates observed in road-side surveys and tow-away accidents in the U.S.A., for each of three model years of cars. There is no consistent direction of difference between the two series of wearing rates and the difference between the two overall wearing rates is small. Accordingly, it was decided that road-side observations of wearing rates are a sufficiently good approximation to those in accidents.

34. Vulcan *et al.* (1975) give overall wearing rates of drivers observed in road-side surveys in Melbourne and rural Victoria during June 1972, nearly exactly the mid-point of Nelson's data collection. The metropolitan and rural wearing rates were averaged to estimate a Victoria-wide rate, viz. 58 per cent. Using $r' = 26.7$ per cent and

TABLE III

WEARING RATES OF LAP ONLY AND LAP-AND-SHOULDER SEAT BELTS OBSERVED IN ROADSIDE SURVEYS AND IN TOW-AWAY ACCIDENTS, U.S.A. (PER CENT)

Model Year of Car	Roadside Surveys, Drivers and Front Passengers, 19 American Cities, December 1974 to June 1975*	Tow-away Accidents, all Occupants, 5 Widely Separated American Localities, Period presumably 1975†
1973	23.7	35.9
1974	46.7	51.7
1975	48.6	46.6
Total	39.7	43.7

* From Knaff and Ziegler (1976), Fig. 2.

† From Knaff and Ziegler (1976), Table 1, based on 10 758 vehicle occupants in approximately 8000 tow-away accidents.

$r = 58$ per cent, s for drivers was calculated as 3.79 using eqn (1). This may be compared with American values of s for lap/sash belts of 1.7 to 2.5 derived from figures in Joksch and Wuerdemann (1972) and 2.5 derived from Table 2 in Knaff and Ziegler (1976).

35. The absence of a wearing rate among fatally injured front left passengers in Nelson's analysis prevents a similar estimation of s for these occupants. However, sufficient data are given in Nelson's report to estimate via another approach for both drivers and front left passengers. This second approach also illustrates the use of the model for predicting numbers of casualties (i.e. persons killed or injured).

36. The probability of an occupant of a given type with a seat belt being killed in an accident, p_b , can be expressed as the product of two probabilities, viz.

p'_b = probability of being killed, if injured and wearing a seat belt, and

q_b = probability of being injured, if involved in an accident and wearing a seat belt.

p'_{nb} and q_{nb} be the corresponding probabilities for occupants not wearing seat belts. Then, it follows that:

$$s = p_{nb}/p_b = (p'_{nb}/p'_b)(q_{nb}/q_b) = s' \cdot t, \text{ say.}$$

The parameter t has an analogous role for occupant casualty numbers as s has for fatality numbers. However, fatalities are well-defined in most accident reporting systems, since most fatalities are killed outright or die soon after. Injuries can be defined at one of a number of different severity levels and it is important to estimate t for a severity level appropriate to the casualty numbers being predicted.

37. Let 'injury' be defined by entry to the RCR/RTR matched file in Nelson's data, i.e. sufficiently severe for an ambulance to be called, the occupant probably admitted to hospital, and both forms filled out accurately enough to achieve a match. Then Nelson (1974, p. 108) gives estimates of p'_b and p'_{nb} for drivers and front passengers separately (the figure for front passengers with seat belts should be 0.043, not 0.046, by calculation from Nelson's Appendix C9). These data lead to the following estimates of s' :

$$\begin{array}{ll} \text{Drivers} & \text{Front Passengers} \\ 0.084/0.039 = 2.15 & 0.061/0.043 = 1.42 \end{array}$$

The estimate for front passengers was taken as being applicable to both front left and front centre passengers.

38. Nelson (1974, p. 130) also gives wearing rates in the matched file for each occupant position. There is little difference in these wearing rates in rural and urban areas, so the rural bias was ignored. Since the following estimation procedure is sensitive to inaccuracy in the wearing rates, the driver rate was calculated from Nelson's Appendix C9, i.e. 44.1 per cent. The front left passenger rate was taken as the average of the rural and urban rates, i.e. 40.5 per cent. These data were used in conjunction with wearing rates from June 1974 road-side surveys reported by Vulcan *et al.* (1975) to estimate t in a similar manner to para. 34 for s . The wearing rate, r , for front left passengers was derived from observations of driver fitting and usage using the same rules as in para. 23. The following estimates for t were found:

$$\begin{array}{ll} \text{Drivers} & \text{Front Left Passengers} \\ 1.75 & 1.51 \end{array}$$

Estimates of s were found by multiplying the corresponding estimates of s' and t , viz.

$$\begin{array}{ll} \text{Drivers} & \text{Front Left Passengers} \\ 3.77 & 2.14 \end{array}$$

This second estimate of s for drivers agrees well with the estimate given in para. 34. The difference is thought to be due to rounding errors only.

39. Since this model can be used to predict post-legislation casualty numbers (as distinct from fatality numbers) also, consideration was given to an estimate of t appropriate for casualties reported to the police. Using further data from Nelson (1974) and the methods of para. 38 estimates of t were found as follows:

$$\begin{array}{ll} \text{Drivers} & \text{Front Left Passengers} \\ 1.82 & 1.46 \end{array}$$

These values should be viewed with caution because of difficulties in defining a casualty. It should be noted that these values do not yield post-legislation casualty trends in agreement with the actual post-legislation casualty data.

PREDICTED POST-LEGISLATION TREND LINES

40. The estimates of s found in the previous section were used in conjunction with the estimates of r_1 and r_2 for 1971-74 to predict post-legislation trends for occupant fatalities. Fig. 9 shows driver, passenger and all occupant fatalities for 1963-74. The predicted fatalities for all occupants is the sum of the driver and passenger predictions.

41. It can be seen that the predicted trend for driver fatalities underestimates the achieved fatality data and that the reverse is true for the passenger fatality graph. These trends (using $s = 3.8$ for drivers and 2.1 for pas-

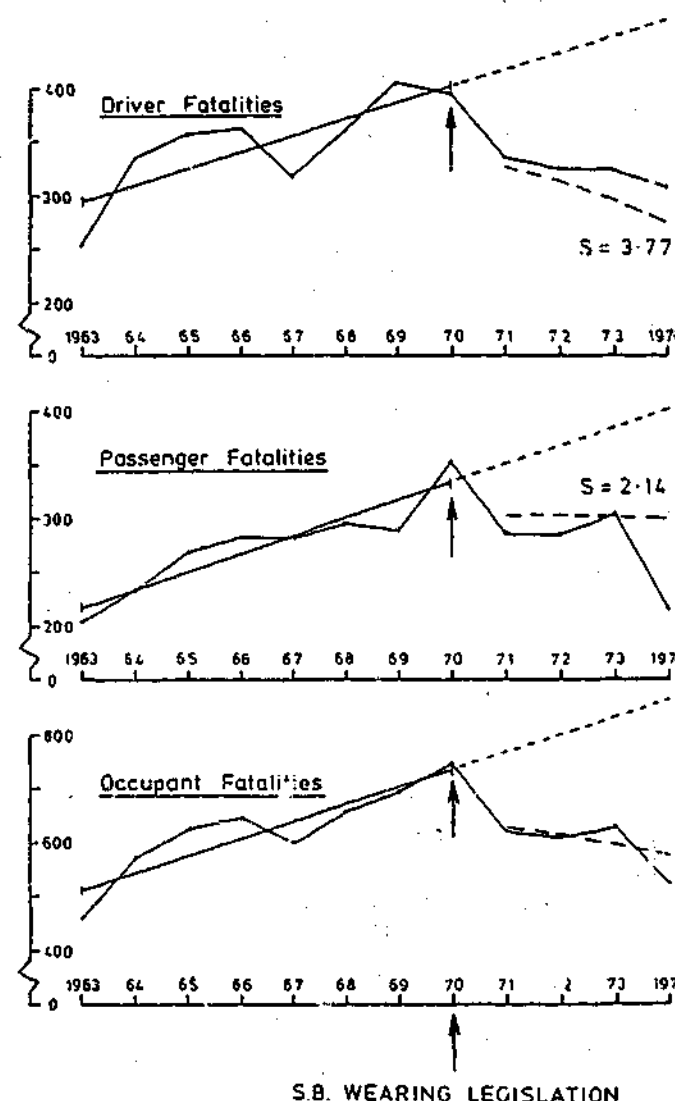


Fig. 9 — Motor vehicle occupants killed in Victoria — 1963 to 1974. Predicted post-legislation fatality trends for 1971 to 1974 (Extrapolation beyond 1974 is invalid)

sengers) can be compared with other possible values of s shown in Fig. 8. It must be emphasised that these predicted trends only apply to the four post-legislation years, 1971-74, and that the expected fatality curves for later years will show eventual upward trends in accordance with the sketches shown in Fig. 5.

REGRESSION METHODS

42. The methods described above estimate the three parameters r_1 , r_2 and s using Victorian information which is independent of the actual post-legislation casualty data. However, if there is no independent information available for estimating the model parameters (as e.g. in other Australian States), it is instructive to fit the model to the accident data using regression methods. These methods are described more fully in Appendix A.

43. A suitable method would be to fit a simple trend (e.g. linear) to the pre-legislation data and then apply the model described earlier to the post-legislation data in order to estimate parameter values. This estimation would generally be one of two types:

- s and the parameters comprising r_1 and r_2 all unknown, and
- s unknown, but the functions r_1 and r_2 completely specified.

The latter is examined below for Victorian data.

44. Eqn (7) in Appendix A gives the solution for an unknown s in which the following assumptions are made:

- r_1 is a constant,
- r_2 is a linearly-increasing function, and
- the pre-legislation trend is a linearly-increasing function.

When this is applied to Victorian data, taking $r_1 = 30$ per cent (drivers) and 24 per cent (passengers) (see Table II) and r_2 as given in Table I, the following estimates of s are obtained:

	1971-1973	1971-1974
Drivers killed	2.86	2.70
Passengers killed	2.51	3.65

In both cases, separate estimates are given for the periods 1971-73 and 1971-74, because it is likely that other measures or factors contributed to the sharp downturn in fatalities in Victoria in 1974 (see Cowley 1975). As expected, the inclusion of the 1974 data value in the regression analysis makes a large difference in the regression estimate of s for passengers.

45. The comparison between the three-year regression estimates of s and the independent estimates of s from previous sections can be summarised for Victoria as follows:

	Regression	Independent
Driver fatalities	2.86	3.77
Passenger fatalities	2.51	2.14

The agreement between these estimates is not unreasonable and is discussed later. However, the equivalent comparison of t values for casualties, shown below, shows poor agreement and highlights the problems associated with making predictions from the various existing forms of casualty (as distinct from fatality) data:

	Regression	Independent
Driver casualties	3.36	1.82
Passenger casualties	2.71	1.46

DISCUSSION

46. Earlier sections have developed a simple model to represent reductions in fatalities following the introduction of compulsory seat belt wearing, and have attempted to estimate the model parameters. The success of these procedures can be measured by comparing the post-legis-

lation trends with the actual data in Figs 8 and 9 and by comparing the different estimates of s given in para. 45.

47. The agreement between predicted and actual fatalities, shown in Fig. 9, is not unreasonable at first sight; but the differences between the regression estimates and independent estimates of s , given in para. 45, are large. If the latter estimates of s are valid, then Fig. 9 indicates that some other measures or factors were operating during the post-legislation period, which tended to increase driver fatalities and decrease passenger fatalities. Further research is required to itemise the road safety measures and other changes in the road system which have occurred in Victoria since the wearing legislation came into force. The effect of these may explain the differences between the predicted and actual fatalities.

48. However, further work is also required to check the validity of the parameter estimates computed from Nelson (1974). Such work should include the estimation of confidence regions for s — these could be large — and for r_1 and r_2 , the latter covering both the variability in r_2 shown in Fig. 6 and the assumption that wearing rates in accidents are the same as wearing rates in the driving population.

49. The work described in this paper must be regarded as an initial attempt at estimating the effects of the seat belt wearing laws, because many assumptions have been made and because the analysis has been restricted to aggregated data for the whole of Victoria.

50. However, the work has demonstrated that the nature of the post-legislation trends is not at all obvious. In Victoria, where r_2 has continued to increase after an initial step up, and where s is fairly high (2 to 4) for drivers and passengers, a downward trend in occupant fatalities is to be expected, at least in the initial years following legislation. However, in New South Wales, for example, occupant fatalities seem to be on an upward trend after an initial step down following legislation (see Fig. 3). The sketches in Fig. 5 demonstrate that the New South Wales experience is still consistent with a positive effect of the legislation, but with somewhat different variation in r_2 and perhaps also an s different to that in Victoria.

51. For definitive results, further work on this topic should include the following considerations:

- validity of linear pre-legislation trends (and their extrapolation to estimate hypothetical casualties in the absence of legislation), compared with the use of variations in the pre-legislation wearing rates and of exposure measures such as petrol delivered, registered vehicles etc.;
- precision of the estimates of r_1 and r_2 , especially r_1 , in light of evidence from South Australia of a downward trend in pre-legislation usage rates as fitting rates increased;
- prediction of asymptotic trends in r_1 and r_2 , which could apply in, say, five years' time;
- estimation of s from police accident reports in the pre-legislation period, when a bias in the reporting of seat belt wearing may not have existed; and
- estimation of s from data collected in Australian in-depth accident studies, in which it is likely that seat belt wearing will be objectively determined.

52. Further work should also include some partitioning of the accident data into:

- urban/rural, defined by speed regions or by the presence of streetlighting or buildings;
- type of accident, e.g. single and multi-vehicle, and impact direction if possible;
- front/rear passengers; and
- type of motor vehicle, e.g. cars and car derivatives versus other vehicles.

Of these, item (a) would be the most important because of different accident patterns and known differences in wearing rates in urban and rural areas. Desirably, such an analysis should examine changes in vehicle occupancy and allow for this factor in estimating s for drivers, front passengers and rear passengers.

53. The analysis to date has been restricted to Victoria mainly because the data from Nelson (1974) have been used to estimate s . Similar data are not available in other States and Territories, so it would be necessary to resort to regression techniques or to use the estimates of s found here. Regression methods could be applied separately to the other States and Territories which have monitored post-legislation wearing rates with the aim of estimating s across Australia.

54. It is considered that compulsory wearing legislation has been the most effective road safety measure adopted recently in Australia, and that the effectiveness of subsequent measures should be quantified separately. Thus it is important that the results of the wearing legislation are analysed in some detail in order that its effectiveness can be better understood. Therefore, it is hoped that Australian States and Territories will continue to monitor seat belt fitting and usage rates to contribute to such analyses.

SUMMARY AND CONCLUSION

55. A description has been given of a simple model for predicting fatality reductions (and hence fatality trends) following the Australian seat belt wearing laws, and an initial calibration of the model:

- (a) directly from Victorian injury-pattern data given by Nelson (1974) and Victorian seat belt wearing data from RoSTA (i.e. by methods independent of the actual post-legislation fatality data); and
- (b) by regression techniques applied to the actual post-legislation fatality data in Victoria.

The calibration has been reasonably successful, but could be improved if more detailed fatality data were available — particularly data partitioned into urban and rural regions for the whole period 1963 to 1974. The calibration has demonstrated the need for continuous measurements or estimates of seat belt wearing rates in the accident population and in the driving population.

56. Australia was the first motorised country to introduce seat belt wearing legislation and now that data for three to four post-legislation years are available, it is important to analyse its effect on the road toll. The methods described in this paper should therefore lead to a better understanding of this important legislation, and allow new road safety measures which are being continually introduced to be assessed more accurately. On the basis of this initial analysis, it is concluded that further research is required to obtain:

- (a) more accurate and consistent fatality parameters for the model in Victoria,
- (b) model parameters and post-legislation fatality trends in all States and Territories, and
- (c) equivalent model parameters for casualties (or persons injured only) — as distinct from fatalities.

APPENDIX A

POST-LEGISLATION TRENDS FROM REGRESSION ANALYSIS

57. The following methods are developed for motor vehicle occupants of a given type (e.g. rear seat passengers) who are killed; a similar argument applies for occupants who are injured.

58. Using the notation of para. 10, the number of fatalities with the lower (hypothetical) wearing rate, r_1 , is:

$$F = an_0n_0p_{0r_1} + p_{0b}(1 - r_1)$$

and the number with the higher (actual) wearing rate, r_2 , is:

$$f = an_0n_0(p_{0r_2} + p_{0b}(1 - r_2))$$

where r_1 , r_2 and a can be represented by given functions of time, t , and n , n_0 , p_0 and p_{0b} are assumed to be constant. Thus, the ratio of fatalities with the higher wearing rate to those with the lower rate is given by:

$$k(t) = \frac{f(t)}{F(t)} = \frac{r_2(t) + s(1 - r_2(t))}{r_1(t) + s(1 - r_1(t))}$$

which can be rewritten as:

$$f(t) = k(t) \cdot F(t) \quad (2)$$

59. If $F(t)$ is known, the post-legislation trend $f(t)$ can be computed by applying regression techniques to eqn (2), with all summations being applied to the post-legislation data points only. The problem will generally be one of two types:

Case (1) — s and the parameters comprising $r_1(t)$ and $r_2(t)$ all unknown, or

Case (2) — s unknown, but the functions $r_1(t)$ and $r_2(t)$ completely specified.

The structure and complexity of the resulting Normal Equations will depend upon the assumptions made.

60. The above methods utilise the pre- and post-legislation data separately, in that $F(t)$ is computed from the pre-legislation data alone, and the estimates of the s , $r_1(t)$ and $r_2(t)$ parameters are then obtained from least squares methods applied to the post-legislation data. More general methods can be applied in which the pre- and post-legislation data are not separated, and 'broken' regression curves are fitted to the complete data set. The more general methods should lead to more accurate results, but the additional complexity may not be warranted. For example, in Victoria with eight pre-legislation annual data points (1963-70) and three post-legislation points (1971-73) — or four points if 1974 is included — it is doubtful whether the more general methods would yield significantly better results. However, it is likely that the more general methods would be necessary if the ratio of post-legislation to pre-legislation data points were increased significantly.

61. The methods above apply regression techniques to the variable f . However, if the dependent variable is taken instead to be the fatality ratio $k = f/F$, the resulting Normal Equations would be simpler. Preliminary calculations indicate that this simplification would lead to sufficiently

accurate results (with the current 8:3 data ratio for Victoria), but this has not been checked in detail.

62. The methods of paras 58 and 59 are applied below to the particular case:

- (a) r_1 is a constant value,
- (b) r_2 is a linearly-increasing function of time, given by $r_{20} + r_{21}t$, and
- (c) a is a linearly-increasing function of time, given by $a_0 + a_1t$.

This simple case is chosen for comparison with the main results presented in the text; in particular, the combination of (a) and (c) leads to a linear function for the hypothetical post-legislation trend, which has been assumed throughout the text.

63. In this case, the hypothetical fatalities equation becomes:

$$F = F_0 + F_1t \quad (3)$$

where $F_0 = a_0n_0p_{0b}(1 - (1 - 1/s)r_1)$
 $F_1 = a_1n_0p_{0b}(1 - (1 - 1/s)r_1)$

are positive constants, and the post-legislation fatality numbers are given by:

$$f = (F_0 + F_1t)(k_0 - k_1t) \quad (4)$$

where $k_0 = (1 - (1 - 1/s)r_{20})/(1 - (1 - 1/s)r_1)$
 $k_1 = ((1 - 1/s)r_{21})/(1 - (1 - 1/s)r_1)$
are positive constants.

64. If data for evaluating k_0 and k_1 are not readily available (Case (1) of para. 59) they can be estimated

from eqn (4), by regression techniques, using the following pair of Normal Equations:

$$\Sigma(F_0 + F_1t) = k_0\Sigma(F_0 + F_1t) - k_1\Sigma(F_0 + F_1t)t$$

$$\Sigma t(F_0 + F_1t) = k_0\Sigma t(F_0 + F_1t) - k_1\Sigma t^2(F_0 + F_1t) \quad (5)$$

where the summations are taken over the f and t data points for the post-legislation period only.

65. If however, the overall wearing rates r_1 and $r_2 = r_{20} + r_{21}t$ are known, then regression techniques can be used to evaluate the single unknown parameter, s (Case (2) of para. 59). In this case k_0 and k_1 are related by the following:

$$(1 - k_0)/(r_{20} - k_0r_1) = 1 - 1/s = k_1/(k_1r_1 + r_{21})$$

and eqn (4) can be written in terms of a single unknown, b , as follows:

$$f = b((F_0 + F_1t) - (F_0 + F_1t)(r_{20} + r_{21}t)/r_1) + (F_0 + F_1t)(r_{20} + r_{21}t)/r_1 \quad (6)$$

where $b = 1/(1 - (1 - 1/s)r_1)$

66. The value of b (and hence s) can be obtained from the Normal Equation:

$$\Sigma(f(F_0 + F_1t)(1 - (r_{20} + r_{21}t)/r_1)) = b\Sigma(F_0 + F_1t)^2(1 - (r_{20} + r_{21}t)/r_1)^2 + \Sigma(F_0 + F_1t)^2(r_{20} + r_{21}t)(1 - (r_{20} + r_{21}t)/r_1) \quad (7)$$

where again the summations are taken over the f and t data points for the post-legislation period only. This method has been used to obtain estimates of s for Victoria and these are described in paras. 44 and 45.

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Paper No. 3

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THE EFFECT OF SEAT BELTS ON MINOR AND SEVERE INJURIES MEASURED ON THE ABBREVIATED INJURY SCALE†

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Abstract—Following the implementation of seat belt wearing legislation in Victoria in December 1970, the Royal Australasian College of Surgeons established a survey to collect detailed injury and crash data from car accidents in that State. An analysis of the effect of seat belt wearing on severe injuries sustained by car occupants during the first two years of the survey was reported by Cameron and Nelson[1977]. Minor injuries were ignored in that analysis. Further work extended the file to cover 8537 occupants injured during the first three years of the survey and the injuries (including minor injuries) were coded on the Abbreviated Injury Scale. This paper examines the effect of seat belt wearing on both minor and severe injuries. Some comparisons of injury severity distributions in the Victorian data and in data collected by North American MDAI teams are also made. The paper concludes that the wearing of static three-point lap/sash belts by front outboard seat occupants of cars and car derivatives is associated with: (a) Reduced likelihood of severe-to-fatal injury to the head-face, thorax, lower torso and lower extremities when injured and not ejected in crashes in built-up areas and, for some body regions, in open road crashes. (b) Increased likelihood of minor injury to the thorax and lower torso when injured and not ejected in crashes in all locations and of minor injury to the neck (i.e. whiplash) when injured and not ejected in crashes in built-up areas. There are suggestions that the increased likelihoods of the minor injuries are not artefacts of the injury criterion for inclusion, nor of the reduced likelihood of severe injury to the trunk when seat belts are worn, but are due to the wearing of the seat belt.

INTRODUCTION

To assess the effect of seat belt wearing on the injury pattern of injured car occupants, Cameron and Nelson[1977] analysed a matched file of 6526 trauma and crash reports collected during the first two years of the Royal Australasian College of Surgeons' Pattern of Injury Survey. From June 1971, the survey collected details of injuries of Victorian road users treated at hospital or killed. These details, recorded on the Road Trauma Report (RTR) form shown in Appendix A, were matched with information on the crash circumstances of car occupants provided by ambulance officers. Unfortunately, the ambulance officer return was incomplete and biased toward rural crashes[Nelson, 1974].

Cameron and Nelson found that the number of injuries recorded on the RTR form was too great for individual study. They concentrated on a particular subset of the injuries recorded, chosen as being those injuries commonly occurring among fatally-injured vehicle occupants. This selection method had the disadvantage of missing uncommon severe injuries. A more objective method of selecting severe injuries, say based on the Abbreviated Injury Scale [Joint Committee on Injury Scaling, 1976], was not available in the matched file at the time. Nor was it possible to select minor injuries in the absence of an objective injury scale. This meant that the effect of seat belt wearing on minor injuries could not be fully considered.

Following recent additions and improvements to the matched file[Cameron, 1977], it has become possible to study the effect of seat belt wearing on both minor and severe injuries measured on the Abbreviated Injury Scale (AIS). The addition of a further year's data has extended the file to cover 8537 injured occupants of cars and car derivatives. Each injury, including soft/surface tissue injuries, has been assigned an AIS score and grouped into six body regions defined by Huelke *et al.* (1977). The highest AIS score was calculated for each region, and the maximum AIS score (MAIS) over all regions found for each injured occupant. The AIS scoring and the body regions used are described in greater detail below.

The objective of the present study was to examine the effect of seat belt wearing on the injuries of car occupant casualties; in particular, the effect of belts on minor injuries was explored as this was not fully considered by Cameron and Nelson. The effect on severe injuries

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defined by the AIS was also considered for comparison with the earlier results. In addition comparisons were made with related results based on data collected by North American in-depth accident investigation teams [Huelke *et al.*, 1977].

DATA

The data collected during the first two years of the Royal Australasian College of Surgeons' (RACS) Pattern of Injury Survey have been described elsewhere [Nelson, 1974; Cameron and Nelson, 1977]. A third year's data were collected in the same way and incorporated in the matched file analysed here [Cameron, 1977]. It remains to describe the assignment of AIS scores to injuries recorded on the RTR form and the grouping of injuries into body regions, to consider the completeness with which minor injuries were recorded and to characterize the crash locations.

Assignment of AIS scores

The AIS score assigned to each injury on the RTR form is shown in Appendix A. These scores were based on Nelson [1974, Appendix F] who in turn based his assignment on the original nine-point AIS system [States, 1969]. Nelson did not make use of scores 6 to 9 of the original AIS scale, which relate to various degrees of fatal consequences of the injury. The 1976 version of AIS [Joint Committee on Injury Scaling, 1976] used a six-point scale where a score of 6 was reserved for currently untreatable (necessarily fatal) injuries. Only one injury on the RTR form was considered to be an AIS = 6 injury, namely the joint occurrence of primary severe brain damage and secondary intracranial compression. Further details are given in Cameron [1977].

Body regions

Injuries recorded on the RTR form were grouped into six body regions based on those used by Huelke *et al.* (1977), shown in Table 1. Huelke *et al.* referred to the "head-face" region as "head" only. All soft tissue injuries recorded in the Head and Neck section of the RTR form were assigned to the head-face region, in order to leave whiplash injuries (also AIS = 1) uncontaminated in the neck region.

In each body region the AIS score corresponding to the most severe injury was found. The

Table 1. Structures of the body regions (after Huelke *et al.* 1977)

<u>Head-Face:</u>	Includes brain, calvarium and oral-facial structures.
<u>Neck:</u>	Includes the cervical spine and musculature, anterior throat structures and cervical blood vessels and nerves.
<u>Thorax:</u>	Includes all of the structures of the internal thoracic area from the base of the neck to the respiratory diaphragm, the ribs, vertebrae, sternum and overlying musculature and skin.
<u>Lower Torso:</u>	Includes the abdominal wall musculature, the lumbar spine and associated musculature, the abdominal organs, the respiratory diaphragm, the bony pelvis, the pelvic organs as well as the skin over the iliac areas, the buttocks and side portions of the pelvis overlying the hip articulation.
<u>Upper Extremities:</u>	Includes the shoulder girdle and joints, and all structures of the arm, elbow, forearm, wrist, hands and fingers.
<u>Lower Extremities:</u>	Includes all structures of the thigh, knee, leg, ankle, foot and toes.

maximum AIS score (MAIS) over all regions was also calculated for each injured occupant. MAIS is not the same as the more commonly used overall AIS (OAIS) which is a clinical judgement of the AIS of a single injury which by itself would be equivalent in terms of overall severity to the cumulative effect of multiple injuries [Joint Committee on Injury Scaling, 1976].

Recording of minor injuries

Nelson [1974] compared a sample of RTR forms with corresponding hospital histories and found a substantial omission of injuries, possibly mainly minor injuries. To establish whether minor injuries were seriously under-recorded in comparison with similar crash injury data files, a comparison of RACS data with data collected by North American in-depth accident investigation teams was made (results not shown). Except for the head-face region, there was some evidence of under-recording of minor injuries (AIS = 1) in the RACS data. However, it was concluded that sufficient minor injuries were recorded in the RACS data to make worthwhile an evaluation of the effect of seat belt wearing on these injuries.

Crash location

Cameron and Nelson [1977] attempted to control for crash severity differences by the use of crash location (metropolitan Melbourne versus non-metropolitan). To some extent this choice of the crash location was historical [Nelson, 1974] and was made because the variable described crash location with very little missing data (Table 2). Another descriptor of crash location was also provided by ambulance officers, namely "open road" versus "built-up area." Although this variable has a higher level of missing data in the matched file, it was considered to represent a better indicator of vehicle speed and hence crash severity. Accordingly, it was chosen as the control for crash severity in the analysis reported here.

Occupant casualties in crashes in built-up areas were spread over both metropolitan and non-metropolitan areas (Table 2). The strong association between the two crash location variables may explain some of the apparent contradictions between the detailed results presented here and those given by Cameron and Nelson [1977] when controlling for crash location. The most important of these apparent contradictions relates to the association between neck injury and seat belt wearing. This is discussed further below.

ANALYSIS

Analysis of RACS file

For comparison of the results with Cameron and Nelson [1977], the analysis was restricted to non-ejected front outboard seat occupant casualties in the RACS matched file. Since the period in which the crashes occurred (June 1971-May 1974) was before the effective date of Australian Design rule 4B requiring inertia reel seat belts to be fitted to the front outboard seats of new vehicles, the restrained occupant casualties considered were almost exclusively wearing static three-point lap/sash belts.

The analysis was further restricted to occupant casualties of known age 16 years or older so that the results could be compared with those of Huelke *et al.* [1977]. Such occupants represent about 93% of the non-ejected front outboard seat occupant casualties in the matched file [Cameron and Nelson, 1977].

When comparing injured seat belt wearers and non-wearers, Cameron and Nelson found

Table 2. Number of occupant casualties in the RACS matched file, described by two descriptors of crash location

	Open Road	Built-up Area	Unknown	TOTAL
Metropolitan Melbourne	152	3483	108	3743
Non-metropolitan (rest of Victoria)	2780	1956	38	4774
Unknown	3	10	7	20
TOTAL	2935	5449	153	8537

important differences of seating position, crash location (metropolitan Melbourne versus non-metropolitan), impact direction, vehicle size, ejection from vehicle, and occupant age and sex. Since each of these variables was potentially related to crash severity (as experienced by the occupant) or injury susceptibility, they attempted to control for these differences by initially restricting the analysis to non-ejected front outboard occupants and then sub-setting the data by each of the remaining variables in turn. In general, the controlled analyses confirmed the differences in injury patterns found for all wearers compared with all non-wearers, as far as the severe injuries were concerned. The absence of crash severity information in the Survey data prevented a definitive evaluation of the effect of seat belt wearing on severe injuries of occupant casualties, but the consistent results from controlled analyses lead the authors to suggest that the observed injury differences were substantially due to seat belt wearing alone.

The consistent results also suggest that controlled analyses may not be necessary during any subsequent comparison of the injuries of seat belt wearing and non-wearing occupant casualties in the same data file. This was taken as being the case during this study of the effect of seat belt wearing on minor and severe injuries, except that crash location (open road versus built-up area) was retained as a controlling variable both because of its likely association with crash severity and the known rural bias of the data file [Nelson, 1974].

Results from North American studies

Huelke *et al.* [1977] analysed the injuries of 5103 occupants in frontal collisions and 994 occupants in roll-overs investigated by North American in-depth accident investigation teams. They separately considered the 765 non-ejected occupants in roll-overs. Most of the restrained occupants wore lap only belts, but 215 of the occupants in frontal collisions and 57 of the non-ejected occupants in roll-overs wore lap/shoulder belts. Thus measures of the difference in injury pattern of unrestrained and lap/shoulder belted occupants were available for comparison with the results based on the RACS matched file. However the RACS results include occupants in all crash types, about 42% of which involved frontal impacts or the vehicle overturning.

RESULTS

The following sections describe differences in AIS distributions when unrestrained front outboard seat occupant casualties aged 16 years or older are compared with restrained occupant casualties of the same seats and age-group. RACS results for non-ejected occupants in crashes on the open road and in built-up areas separately are also compared with Huelke's results for occupants in frontal crashes and non-ejected occupants in rollovers. AIS distributions for the whole body are first considered, using Maximum AIS (MAIS) for the RACS results and Overall AIS (OAS) for Huelke's results, and then the individual body regions.

Whole body

There was a statistically significant ($p < 0.001$) difference in the frequency distributions of MAIS when unrestrained occupants in the RACS file who were involved in crashes in built-up areas were compared with restrained occupants in like crashes (results not shown). Restrained occupants were less likely to have sustained injuries with maximum AIS greater than one and were more likely to have no injury (MAIS = 0). There was no significant difference in the distribution of MAIS between belted and unbelted occupants involved in open road crashes.

About 11% of the occupants included had a maximum AIS of zero. This does not necessarily imply that they were uninjured, only that they failed to score a tick in one of the AIS boxes on the RTR form shown in Appendix A. They may have sustained more minor injuries recorded in the General section of the RTR. In contrast, Huelke's data relate to severe injury accidents but may include uninjured occupants involved in crashes resulting in injury.

Table 3 summarizes the differences in MAIS distribution of unrestrained compared with restrained occupants in terms of a measure of "belt effectiveness", defined in the footnote to the table. Belt effectiveness is the percentage change in the frequency of each AIS level (or group of levels) when seat belt wearers are compared with non-wearers. A negative sign implies that seat belt wearing was associated with a reduction in the proportion of occupant casualties sustaining injuries of the given level of severity.

The belt effectiveness measures were not tested separately for statistical significance;

Table 3. Belt effectiveness* of front outboard seat occupant casualties aged 16 years or older in (a) RACS matched file (non-ejectees only) and (b) results given by Huelke *et al.* [1977]

Whole Body				
AIS	RACS (non-ejectees): Maximum AIS		Huelke (CPIR data): Overall AIS	
	Open Road Crashes	Built-up Area Crashes	Frontal Crashes	Rollovers (non-ejectees)
0	+26.8	+36.9	+48.4	+20.6
1	+10.2	+ 8.5	+ 6.2	+43.9
2	-23.7	-16.1	- 3.1	-38.2
3+	- 7.7	-32.4	-58.0	-70.4
No. not belted	812	1970	3950	566
No. belted (lap/shoulder belt)	919	1831	215	57

$$* \text{ Belt effectiveness (\%)} = \left(\frac{\text{Proportion of belted with AIS} = 1}{\text{Proportion of unbelted with AIS} = 1} - 1 \right) \times 100$$

overall tests were made using the Chi-square test. Also shown in Table 3 is the belt effectiveness of lap/shoulder belts in the results given by Huelke *et al.* [1977]; here however, the injury severity is measured by OAS, described earlier.

Body regions

The next step was to compare the AIS frequency distributions of unrestrained and restrained occupants in the RACS file for each of the six body regions in turn. In the following cases there were statistically significant differences (maximum significance level $p = 0.1$):

- head-face injury in open road crashes and in built-up area crashes ($p < 0.001$ in both cases),
- neck injury in built-up area crashes ($p < 0.1$),
- thorax injury in open road crashes ($p < 0.1$) and in built-up area crashes ($p < 0.01$),
- lower torso injury in built-up area crashes ($p < 0.001$), and
- lower extremity injury in built-up area crashes ($p < 0.01$)

The detailed results are summarized in terms of belt effectiveness (defined earlier) in Tables 4-9. In all of the significant cases above, except neck injury in built-up area crashes, restrained casualties were less likely to have sustained severe-to-fatal injury (AIS ≥ 3) in the particular body region compared with unrestrained occupant casualties, confirming the results of Cameron and Nelson [1977]. In the case of neck injury in built-up area crashes, restrained casualties were more likely to have sustained minor-to-severe injury compared with unrestrained occupant casualties, partially confirming the findings of Cameron and Nelson for this body region. They found increases in the frequency of whiplash injury (AIS = 1) among belted occupants involved in crashes in both metropolitan Melbourne and the rest of Victoria, as well as increases in the frequency of more severe neck injuries in non-metropolitan areas. The disparity in the results may be due to the different crash location variables used in the two studies.

Turning now to minor injuries (AIS = 1), the frequency of their occurrence appeared unchanged or slightly reduced in the head-face and extremities regions, when seat belt wearers were compared with non-wearers. However, the wearing of seat belts was associated with substantial increases in the frequency of:

- AIS 1 injuries in the thorax region, for both open road and built-up area crashes,
- AIS 1 injuries in the lower torso region, for both open road and built-up area crashes, and
- AIS 1 (whiplash) injuries in the neck region, for built-up area crashes only (as discussed earlier).

Table 4. Belt effectiveness in the head-face region of front outboard seat occupant casualties aged 16 years or older in (a) RACS matched file (non-ejectees only) and (b) results given by Huelke *et al.* [1977]

Head-Face Region				
	RACS (non-ejectees)		Huelke (CPIR data)	
	Open Road Crashes	Built-up Area Crashes	Frontal Crashes	Rollovers (non-ejectees)
<u>AIS</u>				
0	+35.2	+45.5	+71.3	+12.9
1	- 6.4	-13.2	-29.5	+15.7
2	-39.4	-41.5	-35.4	-26.7
3+	- 9.5	-51.1	-74.7	-81.4
No. not belted	812	1970	3950	569
No. belted (lap/sash belt)	919	1831	215	57

Table 5. Belt effectiveness in the neck region of front outboard seat occupant casualties aged 16 years or older in (a) RACS matched file (non-ejectees only) and (b) results given by Huelke *et al.* [1977]

Neck Region				
	RACS (non-ejectees)		Huelke (CPIR data)	
	Open Road Crashes	Built-up Area Crashes	Frontal Crashes	Rollovers (non-ejectees)
<u>AIS</u>				
0	-1.0	-1.4	-14.9	-5.5
1	-18.4	+51.6	+113.1	+90.1
2+	+89.3	+25.5	+27.6	-100.0
No. not belted	812	1970	3950	570
No. belted (lap/sash belt)	919	1831	215	57

Table 6. Belt effectiveness in the thorax region of front outboard seat occupant casualties aged 16 years or older in (a) RACS matched file (non-ejectees only) and (b) results given by Huelke *et al.* [1977]

Thorax Region				
	RACS (non-ejectees)		Huelke (CPIR data)	
	Open Road Crashes	Built-up Area Crashes	Frontal Crashes	Rollovers (non-ejectees)
<u>AIS</u>				
0	-4.2	-3.0	-2.5	+18.3
1	+34.8	+23.8	+36.8	-30.1
2	-5.0	+28.8	-56.3	-12.5
3+	-13.2	-36.6	-36.3	-64.7
No. not belted	812	1970	3950	569
No. belted (lap/sash belt)	919	1831	215	57

Table 7. Belt effectiveness in the lower torso region of front outboard seat occupant casualties aged 16 years or older in (a) RACS matched file (non-ejectees only) and (b) results given by Huelke *et al.* [1977]

Lower Torso Region				
	RACS (non-ejectees)		Huelke (CPIR data)	
	Open Road Crashes	Built-up Area Crashes	Frontal Crashes	Rollovers (non-ejectees)
<u>AIS</u>				
0	- 2.3	- 1.7	-10.1	- 1.4
1	+49.5	+73.4	+113.5	+ 43.0
2	-29.3	-13.9	+ 16.7	-100.0
3+	- 9.0	-36.4	- 55.8	- 40.4
No. not belted	812	1970	3950	570
No. belted (lap/sash belt)	919	1831	215	57

Table 8. Belt effectiveness in the upper extremities of front outboard seat occupant casualties aged 16 years or older in (a) RACS matched file (non-ejectees only) and (b) results given by Huelke *et al.* [1977]

Upper Extremities				
	RACS (non-ejectees)		Huelke (CPIR data)	
	Open Road Crashes	Built-up Area Crashes	Frontal Crashes	Rollovers (non-ejectees)
<u>AIS</u>				
0	+ 0.1	+ 1.2	-11.5	+ 0.6
1	- 1.9	+ 2.7	+32.1	+14.1
2	-20.9	-10.0	+18.6	-36.9
3+	+16.1	-33.5	-79.2	-53.8
No. not belted	812	1970	3950	570
No. belted (lap/sash belt)	919	1831	215	57

Table 9. Belt effectiveness in the lower extremities of front outboard seat occupant casualties aged 16 years or older in (a) RACS matched file (non-ejectees only) and (b) results given by Huelke *et al.* [1977]

Lower Extremities				
	RACS (non-ejectees)		Huelke (CPIR data)	
	Open Road Crashes	Built-up Area Crashes	Frontal Crashes	Rollovers (non-ejectees)
<u>AIS</u>				
0	+ 2.3	+ 6.6	+20.4	+ 21.9
1	- 5.5	- 7.1	-13.0	- 15.1
2	+62.0	- 3.7	-21.7	-100.0
3+	- 2.6	-34.7	-81.3	- 66.0
No. not belted	812	1970	3950	570
No. belted (lap/sash belt)	919	1831	215	57

DISCUSSION

Car occupants must have been injured (and brought to hospital at least) to appear in the RACS matched file on which this evidence was based; it must therefore be emphasised that nothing can be said about the likelihood of severe injury *ab initio* for car occupants in crashes.

It must also be emphasised that it is not known whether the increase in minor injury to the trunk was an artefact of the reduction in severe injury in the same body region when seat belts were worn, since seat belt wearers must have been injured somewhere to ultimately appear in the RACS matched file. However, in contrast, the reductions in severe injury to the head-face region and in the lower extremities were not accompanied by increases in minor injuries when belts were worn. These findings suggested that the increased likelihood of minor injury to the trunk, the body region contacted by a lap/sash belt, among injured belt wearers was in fact due to the presence of the seat belt.

The increase in likelihood of minor injury to the neck may also have been an artefact of the need for seat belt wearers to have been injured to appear in the RACS matched file. However, whiplash injury *per se* is not an injury requiring immediate treatment at hospital and it is likely that such injuries were accompanied by more severe injury. This suggests that the increased likelihood of minor injury to the neck among injured belt wearers was in fact due to the wearing of the seat belt.

In general, the results of Huelke *et al.* [1977] for lap/shoulder belt effectiveness were in agreement with the RACS results when allowance was made for the relatively small number of lap/shoulder belted occupants in Huelke's data. Huelke *et al.* also found evidence of increases in the likelihood of minor injury to the neck and lower torso in both frontal crashes and roll-overs, and to the thorax in frontal crashes (the type of crash in which lap/shoulder belted occupants would be expected to have had significant contact with the upper torso part of their belts).

CONCLUSIONS

1. The wearing of static three-point lap/sash belts by front outboard seat occupants of cars and car derivatives is associated with: (a) Reduced likelihood of severe-to-fatal injury to the head-face, thorax, lower torso and lower extremities among victims who were injured and not ejected in crashes occurring in built-up areas and, for some body regions, in open road crashes. (b) Increased likelihood of minor injury to the thorax and lower torso among victims who were injured and not ejected in crashes occurring in all locations and of minor injury to the neck (i.e. whiplash) among victims who were injured and not ejected in crashes occurring in built-up areas.

2. There are suggestions that the increased likelihoods of the minor injuries are not artefacts of the injury criterion for inclusion, nor of the reduced likelihood of severe injury to the trunk when seat belts are worn, but are due to the wearing of the seat belt.

The absence of crash severity information in the RACS data prevented a definitive evaluation of the effect of seat belt wearing on the injuries of car occupant casualties. However, this and an earlier study [Cameron and Nelson, 1977] suggest that the observed differences in injury, when seat belt wearers are compared with non-wearers, are substantially due to seat belt wearing alone.

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APPENDIX A

ROAD TRAUMA REPORT

NAME..... Sex..... Age.....

Vehicle Registration No. [] Seat Belt Worn ☐ Yes ☐ No

Date of Accident...../...../..... Time of Accidenta.m.p.m.

Locality of Accident.....

Hospital..... Casualty No. UR No.

Time of Hospital Examinationa.m.p.m. CAR OCCUPANT ☐

SOURCE OF INFORMATION ☐ 1. CASUALTY ☐ 2. WARD ☐ 3. CORONER PEDESTRIAN ☐

[Please place tick in relevant box (✓)] CYCLIST ☐

A. GENERAL LOSS OF CONSCIOUSNESS YES <input type="checkbox"/> NO <input type="checkbox"/> 1. Transient <input type="checkbox"/> 2. Conscious on Arrival <input type="checkbox"/> Unconscious on Arrival YES <input type="checkbox"/> NO <input type="checkbox"/> 1. From Time of Accident <input type="checkbox"/> 2. Lucid Interval <input type="checkbox"/> 3. Recovery Rapid <input type="checkbox"/> 4. Delayed <input type="checkbox"/> BLOOD LOSS YES <input type="checkbox"/> NO <input type="checkbox"/> 1. <500 ML. <input type="checkbox"/> 2. >500 ML. <input type="checkbox"/> VOMIT YES <input type="checkbox"/> NO <input type="checkbox"/> 1. Inhaled <input type="checkbox"/> 2. Not Inhaled <input type="checkbox"/> SHOCK YES <input type="checkbox"/> NO <input type="checkbox"/> 1. Moderate <input type="checkbox"/> 2. Severe <input type="checkbox"/> CONTINUING HAEMORRHAGE YES <input type="checkbox"/> NO <input type="checkbox"/> 1. Head and Neck <input type="checkbox"/> * 2. Trunk <input type="checkbox"/> * 3. Intraabdominal AIS = 1 <input type="checkbox"/> 4. Intrathoracic 1 <input type="checkbox"/> 5. Limbs <input type="checkbox"/> * (* See body region)		FACIAL BONE FRACTURE YES <input type="checkbox"/> NO <input type="checkbox"/> 1. Malar AIS = 3 <input type="checkbox"/> 2. Middle $\frac{1}{3}$ 3 <input type="checkbox"/> 3. Mandible 3 <input type="checkbox"/> 4. Nasal 2 <input type="checkbox"/> CERVICAL SPINE FRACTURE YES <input type="checkbox"/> NO <input type="checkbox"/> 1. Body Stable 3 <input type="checkbox"/> 2. Body Unstable 5 <input type="checkbox"/> 3. Accessory Process 3 <input type="checkbox"/> NON SPECIFIC (WHIPLASH) 1 <input type="checkbox"/> NO <input type="checkbox"/> SPINAL CORD DAMAGE YES <input type="checkbox"/> NO <input type="checkbox"/> 1. Transient 2 <input type="checkbox"/> 2. Paraplegia - Arms 4 <input type="checkbox"/> both 3. Paraplegia - Legs 4 <input type="checkbox"/> 5 EYE DAMAGE YES <input type="checkbox"/> NO <input type="checkbox"/> 1. Major 3 <input type="checkbox"/> 2. Minor 2 <input type="checkbox"/> BRAIN DAMAGE YES <input type="checkbox"/> NO <input type="checkbox"/> 1. Concussion 2 <input type="checkbox"/> 2. Primary Severe Brain Damage 5 <input type="checkbox"/> both 3. Secondary Intracranial Compression 5 <input type="checkbox"/> 6 TREATMENT YES <input type="checkbox"/> NO <input type="checkbox"/> 1. Operative - Major <input type="checkbox"/> 2. Operative - Minor <input type="checkbox"/> 3. Conservative <input type="checkbox"/>	
B. HEAD AND NECK YES <input type="checkbox"/> NO <input type="checkbox"/> 1. Major <input type="checkbox"/> 2. Minor <input type="checkbox"/>		C. CHEST YES <input type="checkbox"/> NO <input type="checkbox"/> 1. Major <input type="checkbox"/> 2. Minor <input type="checkbox"/>	
SOFT TISSUE YES <input type="checkbox"/> NO <input type="checkbox"/> 1. Laceration AIS = 1 <input type="checkbox"/> 2. Abrasion 1 <input type="checkbox"/> 3. Bruising 1 <input type="checkbox"/> 4. Penetrating 1 <input type="checkbox"/> 5. Loss of Tissue 1 <input type="checkbox"/> (+1 AIS if continuing haemorrhage)		SURFACE TISSUE YES <input type="checkbox"/> NO <input type="checkbox"/> 1. Laceration AIS = 1 <input type="checkbox"/> 2. Abrasion 1 <input type="checkbox"/> 3. Bruising (+1 AIS if continuing) 1 <input type="checkbox"/> 4. Penetrating (haemorrhage) 1 <input type="checkbox"/> 5. Loss of Tissue 1 <input type="checkbox"/> FRACTURE YES <input type="checkbox"/> NO <input type="checkbox"/> 1. Ribs <input type="checkbox"/> Minor 2 <input type="checkbox"/> Flail 4 <input type="checkbox"/> 2. Clavicle 2 <input type="checkbox"/> 3. Sternum 2 <input type="checkbox"/> 4. Scapula 2 <input type="checkbox"/>	

SEE BACK PAGE FOR INSTRUCTIONS

ROAD TRAUMA REPORT

(Please place tick in relevant box (✓))

CHEST (Cont.)		DAMAGED INTERNAL ORGANS (Cont.)	
PNEUMOTHORAX 1. Right - Open 2. Right - Closed 3. Right - Tension 4. Left - Open 5. Left - Closed 6. Left - Tension AIS = 4		11. Kidney - Left 12. Duodenum 13. Diaphragm 14. Bowel - Large 15. Bowel - Small 16. Mesentery 17. Major Vessel 18. Stomach 19. Other AIS = 4	
HAEMOTHORAX 1. Right 2. Left		TREATMENT 1. Operative - Major 2. Operative - Minor 3. Conservative	
LUNG DAMAGE 1. Right 2. Left		E. SPINE AND PELVIC BONES	
AORTA DAMAGE 1. Major 2. Minor		FRACTURE SPINE BODY	
TRACHEA DAMAGE 1. Major 2. Minor		1. Thoracic 2. Lumbar 3. Sacral	
ESOPHAGUS DAMAGE 1. Major 2. Minor		Stable 1. Thoracic 2. Lumbar 3. Sacral AIS = 3	
HEART DAMAGE 1. Major 2. Minor		Unstable 1. Thoracic 2. Lumbar 3. Sacral	
MAJOR VEIN DAMAGE 1. Major 2. Minor		ACCESSORY PROCESS 1. Thoracic 2. Lumbar 3. Sacral	
TREATMENT 1. Operative - Major 2. Operative - Minor 3. Conservative		FRACTURE PELVIS	
D. ABDOMEN AND PELVIS		1. Pubic Rami 2. Ischial Rami 3. Sacro Iliac Joint 4. Acetabulum (Central Dislocation) 5. Other	
SURFACE TISSUES 1. Laceration 2. Abrasion 3. Bruising 4. Penetrating 5. Loss of Tissue (haemorrhage) AIS = 1		SPINAL CORD DAMAGE 1. Transient 2. Paraplegia 3. Cauda Equina	
DAMAGED INTERNAL ORGANS 1. Spleen 2. Liver 3. Bladder - Intraperitoneal 4. Bladder - Extraperitoneal 5. Urethra Membranous 6. Urethra Extramembranous 7. Ureter - Right 8. Ureter - Left 9. Pancreas 10. Kidney - Right		TREATMENT 1. Operative - Major 2. Operative - Minor 3. Conservative	

(Please place tick in relevant box (✓))

F. EXTREMITIES		DISLOCATION	
UPPER LIMBS 1. Major 2. Minor		1. Hip 2. Knee 3. Ankle 4. Toes AIS = 3	
SURFACE TISSUE 1. Laceration 2. Abrasion 3. Bruising 4. Penetrating 5. Loss of Tissue (haemorrhage) AIS = 1		NERVE INJURY 1. Major 2. Minor	
FRACTURES 1. Arm 2. Forearm 3. Wrist 4. Fingers		MAJOR VESSEL INJURY	
DISLOCATION 1. Acromioclavicular 2. Shoulder 3. Elbow 4. Wrist 5. Fingers		TREATMENT 1. Operative - Major 2. Operative - Minor 3. Conservative - Plaster 4. Conservative - Traction 5. Conservative - Manipulation 6. Other	
NERVE INJURY 1. Major 2. Minor		G. DISPOSAL	
MAJOR VESSEL INJURY		TREATED IN CASUALTY 1. Observation 2. Minor treatment	
TREATMENT 1. Operative - Major 2. Operative - Minor 3. Conservative - Plaster 4. Conservative - Traction 5. Conservative - Manipulation 6. Other		WARD ADMISSION 1. Operative treatment in Theatre 2. Conservative	
LOWER LIMBS 1. Major 2. Minor		TIME IN HOSPITAL (No. of Days)	
SURFACE TISSUE 1. Laceration 2. Abrasion 3. Bruising 4. Penetrating 5. Loss of Tissue (haemorrhage) AIS = 1		DIED FROM INJURIES 1. In Hospital 2. Not Admitted to Hospital	
FRACTURES 1. Thigh 2. Knee/Patella 3. Leg 4. Ankle 5. Foot		MAJOR CAUSE OF DEATH (Specify) 1. 2. 3.	
SECONDARY OR CONTRIBUTING CAUSE (Specify) 1. 2. 3.		DIED FROM UNRELATED CAUSE	

Paper No. 4

Paper No. 4

M.H. Cameron and P.M. Strang,
"Effect of intensified random breath testing in
Melbourne during 1978 and 1979".

Proceedings, Eleventh Conference,
Australian Road Research Board,
August 1982.

M.H. CAMERON, B.Sc., M.Sc., F.S.S., Principal Safety Officer, Royal Automobile Club of Victoria
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ABSTRACT

During three periods in 1978 and 1979, intensified random breath testing (RBT) was applied in turn to sections of Melbourne for up to two weeks at a time. The periods of operation ranged from four weeks to several months, with up to 100 hours of RBT per week. An evaluation of the effects of intensified RBT on serious casualty accidents at night was carried out. Night-time serious casualty accidents are a good surrogate for alcohol-involved accidents. Direct effects were measured in the areas and weeks of operation, residual effects were measured in the same areas during following weeks, and contamination effects were measured in nearby areas (apart from those directly influenced). The study concluded that the intensified RBT reduced the risk of alcohol-related accidents (as measured by changes in the risk of night-time serious casualty accidents) in the areas and weeks of operation and in the same areas for at least two weeks after operations ceased. Contamination effects in nearby areas were also observed. The paper includes estimates of the magnitudes of these effects on night-time serious casualty accidents.

INTRODUCTION

1. The Motor Car (Breath Testing Stations) Act was introduced in Victoria in July 1976 to allow Police to carry out random breath testing (RBT) at designated Preliminary Breath Testing Stations at the roadside. RBT is, in fact, not strictly random, as times and places of operation are chosen to maximise the likelihood of detecting drinking drivers and of influencing other potential drink-drivers. At the breath testing stations motorists are stopped at random by a Police Officer in uniform and required to take a preliminary breath test using the Drager Alcotest. If the test is positive the driver is given an evidentiary test using the Breathalyzer 900.

2. In the period from the introduction of the legislation up to October 1978, there was an average of 8 hours of RBT per week in the Melbourne metropolitan area, except for two study periods with an average of 32 hours of operation per week on Tuesday, Thursday, Friday and Saturday nights during February/March 1977 (6 weeks) and May/June 1977 (7 weeks).

3. These two periods of "increased" RBT were followed by three periods during 1978 and 1979 in which "intensified" RBT was carried out in the Melbourne metropolitan area on Thursday, Friday and Saturday nights:

- (a) October-December 1978 (7 weeks), with an average of 100 hours of RBT per week,
- (b) March/April 1979 (4 weeks), with an average of 93 hours of RBT per week,
- (c) September-December 1979 (8 weeks of testing spread over the period), with an average of 74 hours of RBT per week of operation.

4. Evaluations of the effect of the relatively low level of RBT operations during the first six months of the legislation (July to December 1976), and of the effect of the periods of increased RBT in 1977, have been previously reported briefly in Cameron, Strang and Vulcan (1980) and in more detail in Cameron (1978a,b). An evaluation of the first period of intensified RBT has also been previously carried out (Cameron et al 1980).

5. This paper describes an evaluation of the second and third periods of intensified RBT in terms of serious casualty accidents at night, together with a re-evaluation of the first intensified RBT period based on later data. Because this study was able to combine the results from three periods of intensified RBT, it produced accurate estimates of the magnitude of the effects of intensified RBT on serious casualty accidents. These estimates allow the cost-effectiveness of RBT operations to be assessed, for comparison with other Police activities aimed at reducing road

ACKNOWLEDGEMENTS: The work described in this paper was carried out while the first author was a consultant to the Road Safety and Traffic Authority, Victoria. The views expressed are those of the authors and not necessarily those of the Road Safety and Traffic Authority or the Royal Automobile Club of Victoria.

accidents.

AREAS AND TIMES OF INTENSIFIED RBT

6. Fig. 1 shows the six sectors of the Melbourne Statistical Division (MSD) used in the three studies of the effect of intensified RBT. As can be seen, the MSD includes the Melbourne metropolitan area (in which the intensified RBT was applied), as well as non-urban areas. In collaboration with the Victoria Police, the Road Safety and Traffic Authority nominated areas of Melbourne in which RBT should be confined during specific weeks. Figs. 2-4 show the actual schedules of intensified RBT during the three study periods. Subsequent weeks hypothesised as sustaining residual RBT effects are also shown in each case (double and single hatching).

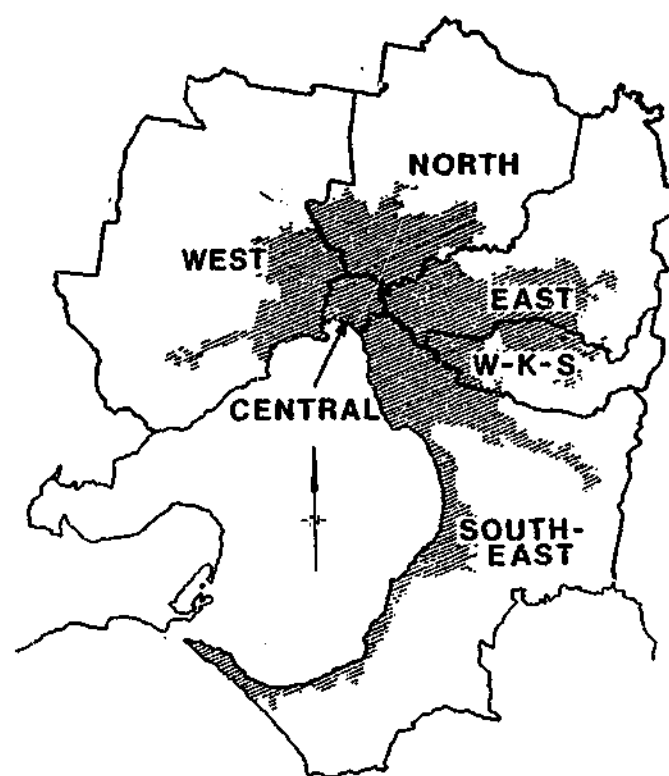


Fig. 1 - Melbourne Statistical Division, showing the six sectors used in this study. (W-K-S: Waverley, Knox and Sherbrooke LGAs)

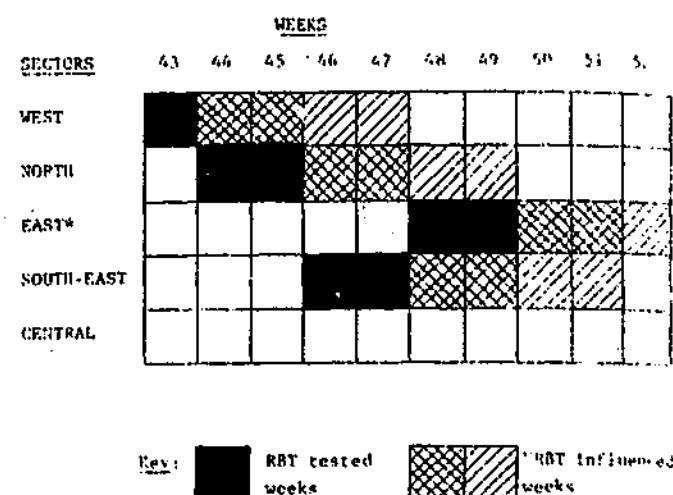


Fig. 2 - Schedule of intensified RBT during October-December 1978 study period. (*Includes Waverley, Knox and Sherbrooke LGAs)

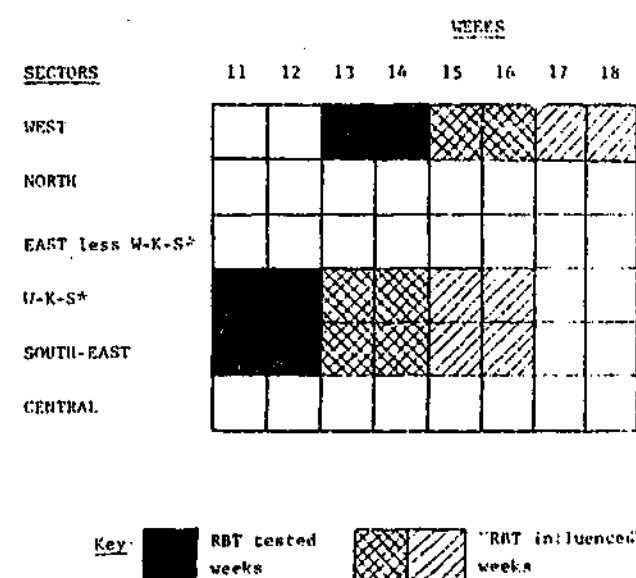


Fig. 3 - Schedule of intensified RBT during March-April 1979 study period. (*Waverley, Knox and Sherbrooke LGAs)

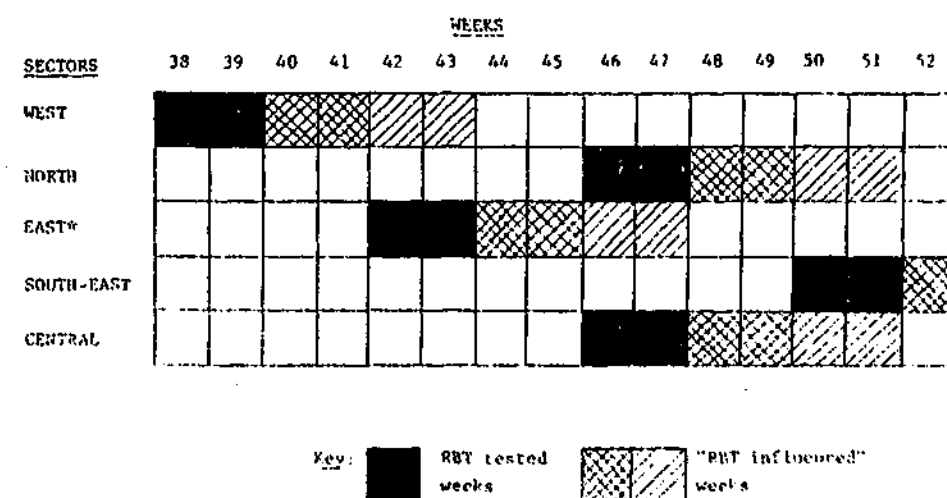


Fig. 4 - Schedule of intensified RBT during September-December 1979 study period. (Includes Waverley, Knox and Sherbrooke LGAs)

7. The data used for this study were serious casualty accidents (SCAs) reported to the Police during 1976 to 1979. SCAs were defined as those casualty accidents in which there was at least one person killed, or injured and admitted to hospital. At times fatal accidents were considered separately when their numbers permitted statistically meaningful conclusions.

8. The data files for 1978 were "final", but the data file for 1979 was "preliminary" and did not contain some accident records received late. As a guide to the effect of the "preliminary" status of the 1979 data file, in 1978 the final file included an additional 63 (1.3%) SCAs scattered throughout the year compared with the preliminary file of the same type.

9. In 1977 a revised version of the casualty accident reporting form 513A was introduced which may have caused an increase in the reporting rate of injury (non-fatal) accidents. There is evidence that there may have been an increase in the reporting rate of non-fatal SCAs as well as in the reporting rate of non-serious casualty accidents (Cameron 1981).

10. The possible under-reporting of SCAs in 1976 and the preliminary (and hence incomplete) status of SCA records for 1979 had implications for the analysis. Considerations of year-to-year changes in the accident experience of areas and times influenced by intensified RBT (in 1978 or 1979) needed to take into account these deficiencies in the 1976 and 1979 data by considering in parallel the change in accident experience of areas or times not influenced by RBT.

ANALYSIS

HYPOTHESES TESTED

11. There were four hypotheses tested in this study:

- (I) Intensified RBT decreased the risk of alcohol-related accidents in the areas and weeks in which RBT operations took place,
- (II) Intensified RBT had residual effects in the areas of operation after operations had ceased,
- (III) Intensified RBT had contamination effects in nearby areas apart from those directly influenced by the RBT operations,
- (IV) The direct effects of intensified RBT (including the residual effect, if any) were greater than the contamination effects.

12. Serious casualty accidents which occurred at night were taken as a surrogate for alcohol-involved accidents. Night was defined as the period from 6 p.m. to 3.59 a.m. next day. Table 1 shows that 46% of drivers who were killed or taken to hospital as a result of accidents at night in the MSD during 1977, had a blood alcohol concentration (BAC) reading over 0.05%, compared with only 11% resulting from day-time (4 a.m. to 5.59 p.m.) accidents. These findings justified the choice of night-time SCAs as a surrogate for alcohol-involved accidents. Night-time

TABLE 1: Driver-rider casualties in the Melbourne Statistical Division who were killed or taken to hospital during 1977. Proportion with BAC readings over 0.05%, by time of day and day of week.

	Night*		Day**
	Sunday to Wednesday	Thursday to Saturday	
Total driver-rider casualties	1070	1639	3574
No. matched with BAC readings	672	1000	1989
No. with BAC readings over 0.05%	256	513	213
Percentage with BAC over 0.05%	38.1	51.3	10.7

* "Night" was defined as 6.00 p.m. to 3.59 a.m. next day

** "Day" was defined as 4.00 a.m. to 5.59 p.m.

fatal accidents are known to be an even better surrogate for alcohol-related crashes and these accidents were considered separately where possible in this study.

PERIODS INFLUENCED BY RBT

13. A range of periods was hypothesised as being directly influenced by the RBT operations applied in each sector of Melbourne (see Figs. 2-4). These were:

- (a) RBT tested weeks: full shading in the Figures,
- (b) RBT tested weeks plus two subsequent weeks (RBT+2): full and double-hatched shading,
- (c) RBT tested weeks plus four subsequent weeks (RBT+4): full, double- and single-hatched shading.

14. Periods (b) and (c) were considered to study the extent of any residual effects of intensified RBT. In previous evaluations, only periods (a) and (b) have been studied. Further, in contrast with previous evaluations, accidents which occurred on Monday to Wednesday of the first week of RBT operations in each sector have been excluded from periods (a), (b) and (c) in this study. These accidents occurred in the pre-RBT period and could not have been directly influenced by RBT operations.

EXPECTED ACCIDENTS IN RBT-INFLUENCED PERIODS

15. For comparison with the actual night-time SCAs which occurred in each sector during the periods influenced by RBT operations, there was a need to estimate the expected accidents which would have occurred in the same time and areas in the absence of RBT. Cameron et al (1980) did this by referring to the accident experience in the previous year (1977) in the case of the intensified RBT operations in October-December 1978.

16. Ross (1981) and other commentators have criticised the use of one previous year as being an inadequate basis for estimating the expected accidents in the RBT-influenced periods. Ross appears to favour the use of a rigorous time series analysis over a large number of years prior to the periods of RBT operations, in the tradition of the evaluations of the U.S. Alcohol Safety Action Projects (NHTSA 1979) and drink-driving laws in Great Britain (Ross 1973) and Canada (Chambers, Roberts and Voelker 1976).

17. The implications of Ross' proposed time series analysis approach for testing the hypotheses of this study were considered. It was apparent that to test the hypotheses adequately it would be necessary to establish trends in accidents within a large number of

small slices of area and time, because of the localised and short-term duration of RBT operations in Melbourne during 1978 and 1979. An alternative would have been to establish prior trends in accidents in the whole MSD. However, this approach would not have allowed separate consideration of the direct, residual and contamination effects of RBT operations.

18. While Ross' proposed method of analysis was clearly desirable, the resources available for this study were insufficient to allow it to be carried out. However, to avoid the danger of using only one previous year to indicate expected accidents, it was decided that this approach would be extended. The accident experience during each of the three periods of intensified RBT was compared with experience during two preceding base years. The night-time SCAs in the same times and areas as those influenced by RBT operations, but during two base years in each case, were averaged to estimate the expected accidents in the RBT-influenced periods.

The base years used for each intensified RBT period were:

- (a) October-December 1978: base years 1976 and 1977,
- (b) March/April 1979: base years 1976 and 1978 (February/March 1977 was contaminated by the increased RBT period),
- (c) September-December 1979: base years 1976 and 1977 (October-December 1978 was contaminated by the 1978 intensified RBT period).

OTHER FACTORS OPERATING

19. Apart from the (possible) under-reporting and preliminary status of SCA records in 1976 and 1979, respectively, there were a number of factors which may have influenced SCA reports from the areas and times of RBT operations apart from RBT itself.

20. During the October-December 1978 period, there was a petrol tanker driver's strike for one week which produced petrol shortages and a possible reduction in vehicle travel. A similar strike in April 1977 was associated with a reduction in vehicle speeds (McLean, 1977), which in turn may have reduced the likelihood of serious casualties from crashes. There was also Melbourne-wide publicity associated with the operation of the RBT units (without specifying locations), and publicity before increased penalties for drink-driving offences were introduced on 20th December.

21. Just before and during the March/April 1979 period, there was an intensive drink-driving publicity campaign which had a high level of recall. At that time, the major Victorian brewing company introduced a low-alcohol-content beer.

22. During part of the September-December 1979 period, there was a further road safety publicity campaign, which included drink-driving as one of its themes.

CONTROL FOR OTHER FACTORS

23. In an attempt to measure the influence of identified and unidentified factors apart from RBT operations, year-to-year changes in day-time SCAs in the areas and periods influenced by RBT were also considered.

24. Day-time SCAs are less frequently alcohol-related than those occurring at night (see Table I), and hence it was expected that they would be less sensitive to the effect of an alcohol-related countermeasure, especially as RBT operations were confined to night-time hours during the three studies. In addition, because the accidents considered occurred in the same areas and weeks as the night-time

SCAs used to measure the primary effect of RBT, it was expected that they would be sensitive to the influence of all other factors (apart from RBT) operating, except any which operated during the night hours only.

25. The use of day-time accidents as a control for other factors is a common analysis strategy in evaluations of drink-driving countermeasures (Ross 1973; Chambers et al 1976; NHTSA 1979). The referenced evaluations all make use of night-time fatal accidents as the primary criterion, and use day-time fatal accidents as a control for other influences.

RESULTS

SERIOUS CASUALTY ACCIDENTS

26. In general, only the combined results from the three separate periods of intensified

TABLE II: Serious casualty accidents in the Melbourne Statistical Division (MSD) during three periods of intensified RBT in 1978 and 1979

SERIOUS CASUALTY ACCIDENTS					
	First Base Year (1976)	Second Base Year	Average of Base Years	RBT Year	Change (%)
NIGHT-TIME					
1. Treated (RBT) areas/periods					
(a) RBT sectors/RBT weeks	149	186	167.5	142	-15.2
(b) RBT sectors/1+2 weeks after	173	208	190.5	166	-12.9
TOTAL (a)+(b) (RBT+2)	322	394	358.0	308	-14.0
(c) RBT sectors/3+4 weeks after	140	176	158.0	151	-4.4
TOTAL (a)+(b)+(c) (RBT+4)	462	570	516.0	459	-11.0
2. Contaminated areas/periods during total study weeks					
(a) MSD, less RBT+2	995	1073	1034.0	968	-6.4
(b) MSD, less RBT+4	855	897	876.0	817	-6.7
DAYTIME					
3. Treated (RBT) areas/periods					
(a) RBT sectors/RBT weeks	195	185	190.0	212	+11.6
(b) RBT sectors/1+2 weeks after	224	201	212.5	240	+12.9
TOTAL (a)+(b) (RBT+2)	419	386	402.5	452	+12.3
(c) RBT sectors/3+4 weeks after	198	186	192.0	195	+1.6
TOTAL (a)+(b)+(c) (RBT+4)	617	572	594.5	647	+8.8
4. Contaminated areas/periods during total study weeks					
(a) MSD, less RBT+2	1259	1209	1234.0	1292	+4.7
(b) MSD, less RBT+4	1061	1023	1042.0	1097	+5.3

RBT are given in this paper. Detailed results for each separate period are given in Cameron (1981). Within each study period, the duration of the residual effects of RBT operations appeared to be at least two weeks after operations had ceased in each sector.

27. The combination of area and time from the start of RBT operations to the end of the apparent residual effect, within each sector, was designated as a "treated area/period". When the treated areas/periods for each study period were considered as a whole, the remainder of the MSD during the study period was designated as "contaminated areas/periods".

28. There appeared to be sufficient similarity between the results in the treated areas/periods to justify combining the three studies to produce overall estimates of the effect of intensified RBT. The results from the three separate studies were summed to form Table II.

29. It should be noted that, in some instances, accidents in the base years have been counted twice. This applied particularly to those accidents in the parts of the base years used for comparison with the RBT periods at the ends of 1978 and 1979, because the base years were the same in each case (1976 and 1977) and the two study periods overlapped. However, there was no overlap of the RBT weeks in 1978 and 1979 (Figs. 2 and 4) and hence there was no double-counting of accidents in these areas/periods (rows 1(a) and 3(a) in Table II).

TABLE III: Changes in serious casualty accidents during three periods of intensified RBT in 1978 and 1979.

SERIOUS CASUALTY ACCIDENTS

	Change (%): Base years to RBT year		Net* Change (%)	Chi- square test (1 df)	One-tailed test signif. level (p)
	NIGHT	DAY			
<u>Treated areas/periods</u>					
(a) RBT sectors/RBT weeks	-15.2	+11.6	-24.0	4.084	p < 0.025
(b) RBT sectors/1+2 weeks after	-12.9	+12.9	-22.8	4.189	p < 0.025
TOTAL (a)+(b) (RBT+2)	-14.0	+12.3	-23.4	8.520	p < 0.005
(c) RBT sectors/3+4 weeks after	- 4.4	+ 1.6	- 5.9	0.155	p > 0.1
TOTAL (a)+(b)+(c) (RBT+4)	-11.0	+ 8.8	-18.3	7.159	p < 0.005
<u>Contaminated areas/periods during total study weeks</u>					
(a) MSD, less RBT+2	- 6.4	+ 4.7	-10.6	4.535	p < 0.02
(b) MSD, less RBT+4	- 6.7	+ 5.3	-11.4	4.490	p < 0.02

* NIGHT change corrected for DAY change.

30. There was minimal overlap of the periods formed by the weeks of RBT operations plus the subsequent two weeks (RBT+2), but extensive overlap of the full treated areas/periods (RBT+4) and of the contaminated areas/periods during the studies at the ends of 1978 and 1979. Double-counting does not distort the estimates of RBT effects, but does cause the statistical tests for the existence of a true effect to be anti-conservative (i.e., more likely to produce a significant test when in fact there was no real RBT effect).

Treated Areas/periods

31. When night-time SCAs in the treated areas/periods of all three studies were compared with that expected from experience in the base years, the apparent effect of intensified RBT was a 15% reduction during the period of operations, followed by a 13% reduction during the next two weeks and a 4% reduction during the two weeks after that (Table II, part 1). When these reductions were corrected by parallel changes in day-time SCAs in the treated areas/periods (Table II, part 3), the net reductions were 24% during the RBT periods, 23% during the first and second weeks after, and 6% during the third and fourth weeks after operations had ceased (Table III). These first two net reductions were statistically significant but the third was not.

Contaminated Areas/periods

32. Weaker effects were apparent in the contaminated areas/periods of the MSD during

TABLE IV: Summary of changes in serious casualty accidents during the three periods of intensified RBT.

SERIOUS CASUALTY ACCIDENTS

Study Period	Change: Base years to RBT year (%)	Chi- square test (1 df)	One-tailed test signif. level (p)
<u>1. OCTOBER-DECEMBER 1978</u>			
<u>Treated areas/periods (RBT+2)</u>			
(a) NIGHT	-20.3	4.672	p < 0.02
(b) DAY	+10.4		
Net change : (a) rel. to (b)	-27.8		
<u>Contaminated areas/periods (MSD/weeks 43-52, less RBT+2)</u>			
(c) NIGHT	- 5.9	0.664	p > 0.7
(d) DAY	-13.7		
Net change : (c) rel. to (d)	+ 9.0		
<u>2. MARCH/APRIL 1979</u>			
<u>Treated areas/periods (RBT+4)</u>			
(a) NIGHT	- 8.0	1.947	0.1 > p > 0.05
(b) DAY	+16.0		
Net change : (a) rel. to (b)	-20.7		
<u>Contaminated areas/periods (MSD/weeks 11-18, less RBT+4)</u>			
(c) NIGHT	- 1.0	0.586	p > 0.1
(d) DAY	+ 8.7		
Net change : (c) rel. to (d)	- 8.9		
<u>3. SEPTEMBER-DECEMBER 1979</u>			
<u>Treated areas/periods (RBT+4)</u>			
(a) NIGHT	-14.7	5.470	p < 0.01
(b) DAY	+13.5		
Net change : (a) rel. to (b)	-24.8		
<u>Contaminated areas/periods (MSD/weeks 38-52, less RBT+4)</u>			
(c) NIGHT	- 8.7	7.079	p < 0.005
(d) DAY	+13.2		
Net change : (c) rel. to (d)	-19.4		

the study periods. When the treated areas/periods were taken to be the periods of operation plus the subsequent two weeks in the tested sectors (i.e., RBT+2), the contaminated areas/periods displayed a 6% reduction in night-time SCAs compared with the base years (Table II, part 2). When corrected for changes in day-time SCAs in the contaminated

areas/periods, the net reduction in night-time SCAs was 11% (Table III). This net reduction was statistically significant.

Treated versus Contaminated Areas/periods

33. To measure the differential in the direct effect of intensified RBT over and

above its contamination effect, the 23% net reduction in night-time SCAs in the treated areas/periods (RBT+2) was compared with the 11% net reduction in the contaminated areas/periods. The differential effect, 14% reduction in night-time SCAs, was judged for significance by a 2x2 Chi-square test (Bishop, Fienberg and Holland 1975) and found to be weakly statistically significant (Chi-square = 2.217, df = 1; $0.1 > p > 0.05$, one-tailed).

Individual Study Periods

34. Summary results from each of the three individual periods of intensified RBT are given in Table IV. These results are given to support the significance tests in Table III, without the disadvantage of being based on "double-counted" accidents. During the two 1979 study periods, residual effects of RBT operations were apparent up to four weeks after operations ceased. Hence, the treated

areas/periods were considered to be two weeks longer than during the 1978 study period.

FATAL ACCIDENTS

35. Results related to fatal accidents, a component of the serious casualty accidents already described, are given separately because of their relative importance and the high level of alcohol involvement in this type of accident occurring at night. However, even when the results from the three periods of intensified RBT were combined, there were insufficient fatal accidents to reach statistically meaningful conclusions regarding the effects of RBT operations.

36. Notwithstanding the lack of conclusiveness from the results pertaining to fatal accidents, consideration was given to whether the apparent effects of intensified RBT on fatal accidents were consistent with

TABLE V: Fatal accidents in the Melbourne Statistical Division (MSD) during three periods of intensified RBT in 1978 and 1979.

FATAL ACCIDENTS					
	First Base Year (1976)	Second Base Year	Average of Base Years	RBT Year	Change (%)
NIGHT-TIME					
<u>1. Treated (RBT) areas/periods</u>					
(a) RBT sectors/RBT weeks	15	27	21.0	14	-33.3
(b) RBT sectors/1+2 weeks after	18	24	21.0	18	-14.3
TOTAL (a)+(b) (RBT+2)	33	51	42.0	32	-23.8
(c) RBT sectors/3+4 weeks after	24	16	20.0	15	-25.0
TOTAL (a)+(b)+(c) (RBT+4)	57	67	62.0	47	-24.2
<u>2. Contaminated areas/periods during total study weeks</u>					
(a) MSD, less RBT+2	115	119	117.0	103	-12.0
(b) MSD, less RBT+4	91	103	97.0	88	- 9.3
DAYTIME					
<u>3. Treated (RBT) areas/periods</u>					
(a) RBT sectors/RBT weeks	16	20	18.0	19	+ 5.6
(b) RBT sectors/1+2 weeks after	16	18	17.0	16	- 5.9
TOTAL (a)+(b) (RBT+2)	32	38	35.0	35	0.0
(c) RBT sectors/3+4 weeks after	14	19	16.5	14	-15.2
TOTAL (a)+(b)+(c) (RBT+4)	46	57	51.5	49	- 4.9
<u>4. Contaminated areas/periods during total study weeks</u>					
(a) MSD, less RBT+2	111	83	97.0	97	0.0
(b) MSD, less RBT+4	97	64	80.5	83	+ 3.1

the observed effects on all serious casualty accidents.

Treated Areas/periods

37. For the three study periods combined (Table V), there was evidence of fatal accident reductions of greater or similar magnitude to the reductions in SCAs in the treated areas/periods. When night-time fatal accidents in the treated areas/periods were compared with the base years, there was a 33% reduction during the period of operations, followed by a 14% reduction during the next two weeks and a 25% reduction during the two weeks after that. When corrected for parallel changes in day-time fatal accidents in the treated areas/periods, the net reductions in night-time fatal accidents were 37%, 9% and 12% respectively (Table VI). None were statistically significant.

Contaminated areas/periods

38. In the contaminated areas/periods of the MSD during the study periods, there was a 12% net reduction in night-time fatal accidents relative to changes in day-time fatal accidents (Table VI). This net reduction was also not statistically significant.

DISCUSSION

39. There was strong evidence to support the first hypothesis, namely that intensified RBT decreased the risk of alcohol-related accidents in the areas and weeks in which RBT operations took place. There was a 24% reduction in night-time SCAs in the areas and periods of RBT operations, relative to changes in day-time SCAs in the same areas and periods. There was also a 37% reduction of the same type in the fatal accident component of these SCAs. The reduction in night-time SCAs was statistically significant, whereas the reduction in night-time fatal accidents was not. However, the fatal accidents were insufficient in number to yield a statistically significant test on a reduction of the same magnitude as that observed among SCAs. Hence the data pertaining to fatal accidents were inconclusive regarding the truth of the hypothesis, but were not inconsistent with it (i.e. the net change in fatal accidents was in the hypothesised direction).

40. There was also strong evidence for the second hypothesis, namely that intensified RBT had residual effects in the areas of operation after operations had ceased. There was a 23% reduction in night-time SCAs, relative to changes in day-time SCAs, in the tested areas during the two weeks after

TABLE VI: Changes in fatal accidents during three periods of intensified RBT in 1978 and 1979.

FATAL ACCIDENTS					
	Change (%): Base years to RBT year		Net * Change (%)	Chi- square test (1 df)	One-tailed test signif. level (p)
	NIGHT	DAY			
<u>Treated areas/periods</u>					
(a) RBT sectors/RBT weeks	-33.3	+ 5.6	-36.8	0.796	p > 0.1
(b) RBT sectors/1+2 weeks after	-14.3	- 5.9	- 8.9	0.0004	p > 0.1
TOTAL (a)+(b) (RBT+2)	-23.8	0.0	-23.8	0.611	p > 0.1
(c) RBT sectors/3+4 weeks after	-25.0	-15.2	-11.6	0.004	p > 0.1
TOTAL (a)+(b)+(c) (RBT+4)	-24.2	- 4.9	-20.3	0.657	p > 0.1
<u>Contaminated areas/periods during total study weeks</u>					
(a) MSD, less RBT+2	-12.0	0.0	-12.0	0.432	p > 0.1
(b) MSD, less RBT+4	- 9.3	+ 3.1	-12.0	0.352	p > 0.1

* NIGHT change corrected for DAY change

testing ceased, but only a 6% reduction of the same type during the two weeks after that. The reduction during the first two weeks was statistically significant, whereas that during the second two weeks was not. Thus intensified RBT appeared to have a residual effect for at least two, but less than four, weeks after operations had ceased. Once again, the changes in fatal accidents were not statistically significant, but were not inconsistent with the hypothesis.

41. There appeared to be evidence to support the third hypothesis, namely that intensified RBT had contamination effects in nearby areas apart from those directly influenced by RBT operations. In the MSD during the study periods, apart from in the tested sectors during the period of operation plus the subsequent two weeks, there was an 11% reduction in night-time SCAs, relative to changes in day-time SCAs. This reduction was statistically significant, giving support to the third hypothesis. Changes in fatal accidents were not statistically significant, but were not inconsistent with the hypothesis.

42. An alternative to all three hypotheses exists, namely that another factor (apart from intensified RBT) operated over the whole MSD during the study periods to reduce the risk of alcohol-related accidents (or, more specifically, to reduce night-time SCAs). This hypothesis was tested by comparing the net reduction in night-time SCAs in the areas and periods directly influenced by RBT with the net reduction in the areas and periods hypothesised as having received contamination effects. The difference was weakly statistically significant in the direction of a greater net reduction in the directly influenced areas and periods. This result gave support for rejection of the alternative hypothesis. This result also represents evidence for the fourth hypothesis proposed in this study, namely that the direct effects of intensified RBT (including the residual effect, if any) were greater than the contamination effects.

43. During the March/April 1979 study, the direct effects of intensified RBT were smaller than during the other two study periods. This result suggests that a four week campaign period of intensified operation is insufficient to reach the maximum level of effectiveness of intensified RBT. The duration of a campaign necessary to reach the maximum level is not clear from the results of the longer study periods.

44. This study has provided reasonably stable (and hence apparently accurate) estimates of the effects of intensified RBT operations on night-time SCAs, namely a 23% reduction in the tested areas during the periods of operation and the following two weeks, and an 11% reduction in nearby areas. These estimates allow some preliminary remarks to be made about the cost-effectiveness of intensified RBT.

45. Applying the estimates to data given in Table II, it was estimated that the direct

effects resulted in a saving of 82 SCA's and the contamination effects saved 114 SCA's, a total of 196 SCAs saved during the three periods of intensified operations. Some of these savings may have been due to the associated publicity and other factors which occurred at the same time. These other influences may explain some part of the apparent contamination effects, but at the very least, a 14% reduction (i.e., 50) in SCA's is directly ascribable to intensified RBT operations in the tested areas (reference para. 33). These two estimates of the savings due to intensified RBT may be described as "liberal" and "conservative" estimates, respectively.

46. To achieve these savings, the Victoria Police committed resources to carry out 1664 hours of testing activity during the three study periods. Each testing station was manned by on average four Police Officers. Hence, up to 6656 man-hours were deployed to achieve the above estimated savings of SCAs. Based on a total societal cost of \$20,000 per SCA, and recent work by Somerville (1981) suggests that this may be a gross underestimate, it is possible to calculate the value to the community of each of the 6656 man-hours of Police effort committed to intensified RBT in 1978 and 1979.

47. Thus, the estimated value of each man-hour was in the range from at least \$150 (conservative estimate) to \$589 (liberal estimate) or greater. It follows that intensified RBT in Melbourne during 1978 and 1979 was apparently a highly cost-effective countermeasure.

48. Against this background, it should be noted that the Victoria Police claim that they would have operational problems in sustaining the "intensified" level of RBT operations committed during the campaigns in 1978 and 1979. This appears to be due to the exposed night duty associated with RBT, and the short-fall in other areas due to the need to re-assign Police Officers to assist in intensified operations (Martin 1982), but the primary objection appears to be that RBT is a relatively ineffective use of Police resources for detecting drink-driving (and other) offences.

49. This latter objection is not surprising when it is realised that the real objective of an alcohol countermeasure of the RBT type is to deter drink-driving (Maxted 1974; Ennis 1977; Johnston 1979). The apparent purpose of RBT operations is to detect drivers who have been drinking to an illegal extent, but since drivers are stopped essentially "at random" from the traffic stream during the period of operations, it is a relatively ineffective method of detecting drink-drivers compared with other Police procedures. Some Police believe that these other procedures are more effective in detecting drink-drivers and, by implication, are more effective in reducing alcohol-involved crashes. No evidence has been presented to support the claimed effectiveness (or cost-effectiveness) of these alternative Police procedures in reducing road accidents.

50. In the absence of evidence of superior cost-effectiveness (or even superior effectiveness) of alternative Police procedures compared with intensified RBT, in terms of realising the objective of reducing serious road accidents, then higher priority should be given to the use of intensified RBT in those Police activities aimed at road accident reduction.

51. However, it should be particularly noted that the accident reduction benefits of RBT operations have been clearly demonstrated only when applied at an intensified level, i.e. at the rate of 74 to 100 hours per week on Thursday, Friday and Saturday nights, concentrated within a sector of Melbourne representing about one-quarter of its urban area. Furthermore, the benefits have been demonstrated only when applied on a campaign basis (with associated mass media publicity), i.e. the benefits of intensified RBT may be realisable only when testing operations contrast with the normal, low level. Thus, intensified RBT should not be seen, on the evidence to date, as being a road accident countermeasure suitable for continuous, unbroken application.

52. This evaluation of intensified RBT has been carried out only in terms of its effect on serious casualty accidents at night. When Cameron et al (1980) conducted their initial evaluation of the 1978 intensified RBT operations, they employed casualty accident reports with which blood alcohol readings had been matched, in addition to the criterion of serious casualty accidents. They found that intensified RBT was associated with a reduction in alcohol-involvement in single-vehicle casualty accidents, but were unable to be conclusive about the effect on alcohol-involvement in multi-vehicle casualty accidents because of deficiencies in the matching process.

53. For a full understanding of whether intensified RBT achieves its objective, as an alcohol-related countermeasure, of reducing alcohol-involved accidents, it is essential that an evaluation be carried out in terms of accidents for which alcohol involvement has been identified. Analysis problems associated with the incomplete matching of blood alcohol readings with Police accident reports should be overcome and the analysis described in this paper repeated in terms of alcohol-involvement in accidents of all types.

54. An alternative explanation of the observed effects of intensified RBT on serious casualty accidents may be that mere Police presence, in a road-block type situation, is effective in reducing accidents of various types, not just those which are alcohol-related. Support for this possibility is given by Maisey and Saunders (1981) who evaluated a large-scale traffic enforcement blitz in Western Australia, following closely the methodology of Cameron et al (1980). The enforcement blitz involved road blocks at various locations primarily at night and on weekends. Traffic officers examined drivers' licences and vehicle roadworthiness, and alcohol-impaired drivers were detected. The

campaign received considerable mass media publicity, apparently with a "drink-driving" emphasis (Boulton 1981). Maisey and Saunders found that the enforcement blitz and associated publicity resulted in a reduction in night-time casualty accidents in Perth comparable in magnitude to the reduction in night-time serious casualty accidents observed in Melbourne by Cameron et al (1980) during the 1978 intensified RBT campaign.

55. These findings tend to suggest that intensified RBT may be, predominantly, a deterrent to road user behaviours which lead to accidents of various types, with an additional effect of deterring drink-driving behaviour because of the special, alcohol-related nature of the countermeasure. The proposed extension of this study, to evaluate the effect of intensified RBT in Melbourne in 1978 and 1979 in terms of alcohol-involvement in accidents, would test this hypothesis.

CONCLUSIONS

56. The conclusions of this study were as follows:

- (a) Intensified RBT in Melbourne during 1978 and 1979 decreased the risk of alcohol-related accidents (measured by changes in the risk of night-time serious casualty accidents) in the areas and weeks in which RBT operations took place.
- (b) Intensified RBT had residual effects in the areas of operation for at least two weeks (but less than four weeks) after operations ceased.
- (c) Intensified RBT had contamination effects in nearby areas apart from those directly influenced by RBT operations.
- (d) The estimated effects of intensified RBT on the risk of night-time serious casualty accidents were as follows:
 - i) in the areas and weeks of RBT operations: 24% reduction;
 - ii) in the areas of RBT operations during the two weeks after operations ceased: 23% reduction;
 - iii) in nearby areas apart from those directly influenced by RBT operations and their residual effect (but in a period from the start of operations to at most four weeks after operations ceased): 11% reduction.
- (e) Four week campaign periods of intensified operation appear to be insufficient to reach the maximum level of effectiveness of intensified RBT.

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Paper No. 5

M.O. Haque and M.H. Cameron,
"Effect of the Victorian Zero BAC legislation
on serious casualty accidents :
July 1984 - December 1985".

Paper No. 5

Journal of Safety Research,
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Effect of the Victorian Zero BAC Legislation on Serious Casualty Accidents: July 1984-December 1985

Mohammed Ohidul Haque and Max Cameron

The zero BAC legislation was introduced in Victoria on May 22, 1984, to reduce the alcohol-related accidents for novice drivers and riders. The accident effects of the legislation were examined only for *car drivers* involved in serious casualty accidents (SCAs), using intervention time series analysis and the pre-post method. "Alcohol times," that is, times during which the proportions of SCAs involving alcohol have been shown to be higher, were used as a surrogate for alcohol involvement. The analysis indicated a reduction of about 4% in the number of learner, first-year probationary, unlicensed, and disqualified drivers involved in SCAs at alcohol times over the 18-month postlegislation period. The statistical power of the analysis was very poor. Hence, it was not possible to determine conclusively whether the result was due to chance variation or to the legislation which did not achieve statistical significance because of poor analytical power.

Zero BAC legislation was introduced in Victoria on May 22, 1984. Under this legislation no learner (L), first-year probation-

ary (P), disqualified, or unlicensed driver or motorcycle rider is allowed to drive or ride with any alcohol in his or her blood.

In June 1983, new arrangements for the training and testing of motorcycle learner permit applicants were introduced. The proximity of this change to the introduction of the zero BAC legislation prevented an evaluation of the effect of the latter on novice motorcyclists. Thus, the present report evaluates the effect of the zero BAC legislation for learner, first-year probationary, disqualified, and unlicensed *drivers* only.

The effect of the legislation is examined using two approaches. First, time series techniques incorporating the expected form

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of intervention (legislation) effects are applied to analyze the effect of the legislation on target group drivers involved in serious casualty accidents (SCAs). Second, accidents in periods before and after the introduction of the legislation are compared to investigate the effects of the legislation on the numbers of drivers from the target and control groups involved in SCAs.

The time series method was considered statistically the more appropriate but, for comparative purposes, both approaches were used to assess the effects of the zero BAC law in the 18-month postlegislation period July 1984 to December 1985.

REVIEW OF PREVIOUS STUDIES

A surrogate for alcohol involvement was necessary, because no direct measure of BAC level for all drivers involved in SCAs was available. Cameron and Strang (1982) pointed out that SCAs are known to have a high probability of alcohol involvement, especially at night. Sloane and South (1985) showed that the distribution of alcohol-related accidents is not uniform over the hours of the day and days of the week. They observed that the proportion of accidents which are alcohol-related is higher on Thursday to Saturday nights than at other times. These observations were also made by Haque, Strang, and Crabb (1986) in their interim analysis of the effect of the zero BAC legislation.

Since then, South (1986) has indicated that there are "alcohol times" (certain hours of a week) during which the proportion of SCAs involving alcohol is higher than at other times (the complement of the alcohol times is taken as the so-called "nonalcohol times"). Any reduction in the incidence of alcohol-related accidents that might occur due to the implementation of the zero BAC legislation could be expected to be greater at alcohol times. Thus, SCAs during the alcohol times were used in this study as surrogates for alcohol-involved accidents.

At a later date, Haque and Cameron (1987) made a comprehensive study and presented more detailed information about the effect of the Victorian zero BAC legislation, using serious casualty accident data.

They also present the findings of the surveys of P-plate usage (see Bowen, 1985), Melbourne on-road driver exposure (see Drummond, Cave, & Healy, 1986), and public awareness of the zero BAC legislation in Victoria (see Monk & South, 1985).

METHOD

Data Source

At the time of the study, accident data from the Victoria Road Traffic Authority (RTA) accident file were available up to December 1985. This provided an opportunity to measure the effectiveness of the zero BAC legislation for the first 18 months after its implementation.

Accident data for all drivers of cars and car derivatives who were involved in SCAs were considered in the present study. A SCA is defined as an accident in which at least one person is killed or is injured and admitted to hospital.

This criterion was chosen to provide a larger data set than fatal accidents. This results in the statistical test being more powerful, enabling smaller effects to be detected.

Driver Groups and Times

For the purpose of the analysis, learner, first-year probationary, disqualified, and unlicensed drivers were the "target" group. The standard license holders were the control group drivers, since they were not subject to the zero BAC legislation.

Further, the accident involvements of the target and control group drivers were disaggregated into "alcohol" and "nonalcohol" times of accident occurrence. The effects of the zero BAC legislation were considered likely to be greatest for the target group during the alcohol times.

Thus, the following combinations of drivers and times of their accident involvement were analyzed in the study:

1. Target group drivers during alcohol times;
2. Target group drivers during nonalcohol times;
3. Control group drivers during alcohol times; and

4. Control group drivers during nonalcohol times.

The first combination (target group drivers during alcohol times) was the primary focus of the study. The remaining combinations of drivers and times were included to measure and account for the effects of other influences that may have affected the first combination in addition to the zero BAC legislation.

Statistical Methods

There are two basic types of statistical methods that can be used to evaluate road safety countermeasures. These are the pre-post method and the time-series method. The pre-post method attempts to evaluate the effect of road safety countermeasures by comparing the relative changes in the numbers of target and control group drivers involved in SCAs between the pre- and post-legislation periods. The time series method attempts to analyze the effect of road safety countermeasures by predicting the expected number of target group drivers involved in SCAs during the postlegislation period, using the available prelegislation data. The relative merits of these two approaches are given in Haque and Cameron (1987).

Because prelegislation downward trends were identified by Haque et al. (1986) in the accident involvements of the target group and standard license holders, especially at night and especially for the target group, it was decided that time series analysis methods were more likely to give valid conclusions in this study. In addition, it was decided to use the pre-post method for comparative and illustrative purposes. When the results of the two methods are in conflict, however, the time series results should be preferred in this study.

ANALYSIS AND RESULTS

Time Series Analysis

The usual Box-Jenkins (1976) ARIMA models were fitted to the prelegislation data (i.e., January 1977 to May 1984) for all four driver/time combinations, or series. Projections were then made on the basis of the best available model for each separate series.

Next, intervention techniques (Box & Tiao, 1975) were applied to those best models that were used for ARIMA forecasts, using all SCA data from January 1977 to December 1985. The intervention approach involved (a) formulating a model of the expected effect of the intervention (i.e., the zero BAC legislation), (b) incorporating this in the ARIMA model previously found for each series, and (c) fitting the combined (intervention) model to the pre- and post-legislation data simultaneously. The presence of an intervention effect is tested statistically by a test of whether the intervention coefficient in the model is significantly different from zero.

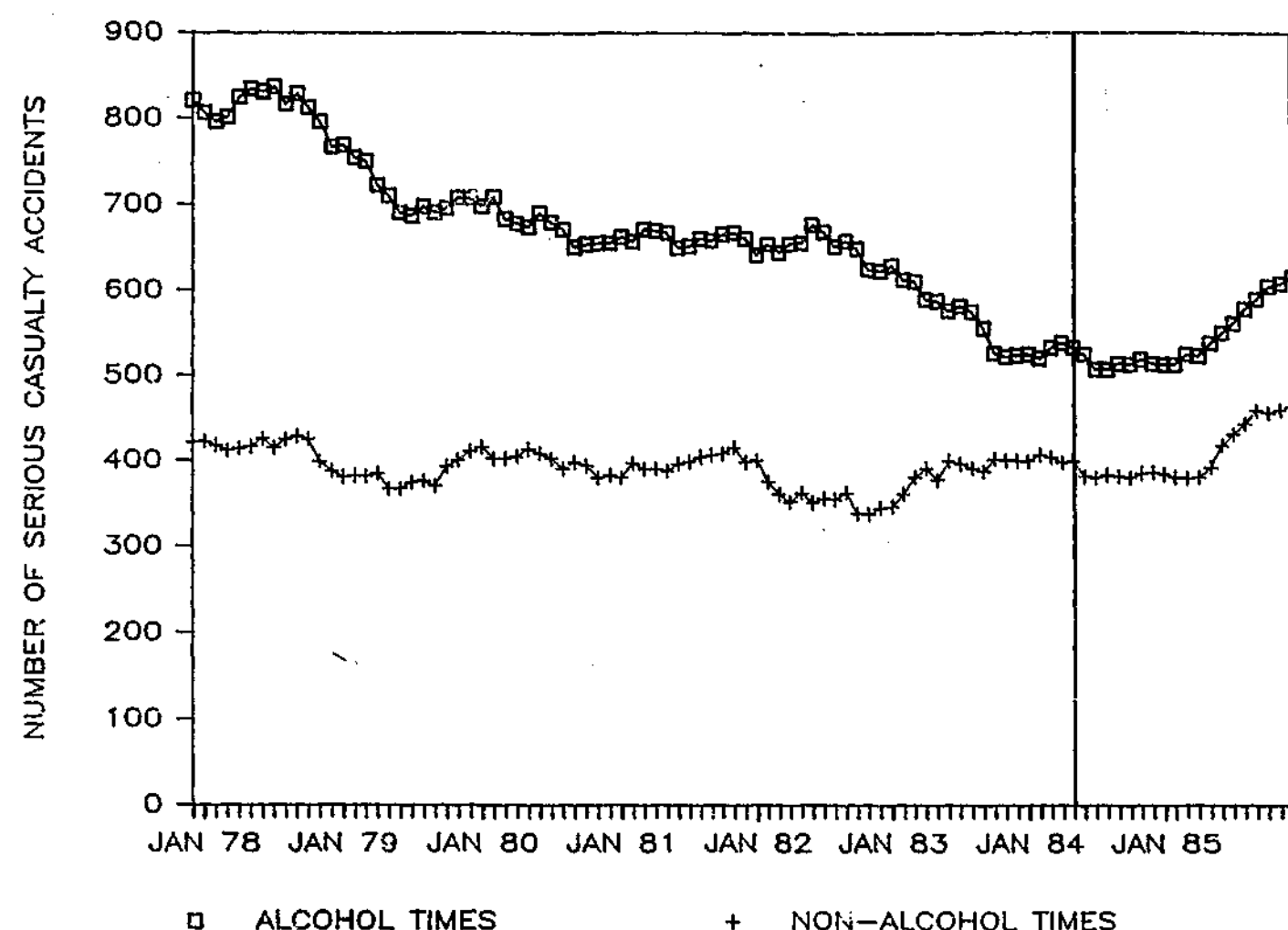
This approach was adopted for each of the four series of data, even though there was no hypothesized intervention effect for the three control series, Series 2, 3, and 4. For these three series, any measured "intervention" effect is a measure of the effect of other influences that should be taken into account in assessing the effect of the zero BAC legislation on Series 1, target group drivers during alcohol times.

Correction for the effect of other influences was made by subtracting the estimated intervention coefficient of the control series from that of Series 1. Before this was done, the intervention effects were standardized by dividing the intervention coefficients by the monthly average SCA involvements forecast by the ARIMA model in the postlegislation period. The effect of the zero BAC legislation can then be tested by the statistical significance of the corrected intervention coefficient for Series 1.

Prelegislation Trends

Figure 1 indicates that the 12-month moving total trends for target group drivers involved in SCAs at alcohol (and nonalcohol) times have stabilized or increased since early 1984, which is contrary to the hypothesis. On the other hand, it is apparent from Figure 2 that the 12-month moving totals for control group drivers involved in SCAs both at alcohol and nonalcohol times have increased from early 1983. At this stage of the analysis, it was not known for what group of drivers the rate of increase of SCAs

FIGURE 1
TIME SERIES ANALYSIS: 12-MONTH MOVING TOTALS OF SERIOUS CASUALTY ACCIDENTS FOR
TARGET GROUP DRIVERS



was faster. In order to investigate this, the intervention coefficients for target and control group drivers involved in SCAs (standardized for the monthly average forecasts in the postlegislation period) have been tested for differences following the fitting of the intervention models. Only standardized coefficients have been used for such comparisons.

Results of the Time Series Analysis

The results of the ARIMA and intervention models together with the actual numbers of target and control group drivers involved in SCAs during alcohol and nonalcohol times are given in Table 1.

There were 866 target group drivers involved in SCAs during the alcohol times in the postlegislation period, compared with 803.3 forecast by the ARIMA model, a difference of 7.8%. In contrast, the number of

target group drivers involved in SCAs during the nonalcohol times was 11.3% greater than that forecast by the ARIMA model; this difference was assumed to have been due to influences on the target group drivers other than the zero BAC legislation. It could be expected that a similar 11.3% difference for the target group would have occurred during the alcohol times in the absence of the legislation. Hence, these results indicate that the net effect of the legislation was a 3.1% reduction in the target group's involvement in SCAs during the alcohol times (the net difference shown in Table 1).

The standard license holders' SCA involvement during the alcohol times is another indicator of the effect of other influences. In this case, their postlegislation SCA involvements were 7.4% greater than forecast by the ARIMA model (Table 1). This was similar to the difference for the target group

during the alcohol times. However, during the nonalcohol times the standard license holders were involved in only 6.6% more SCAs than forecast. Thus, the pattern of differences for the standard license holders is different from that of the target group. The net difference for standard license holders is a 0.7% increase.

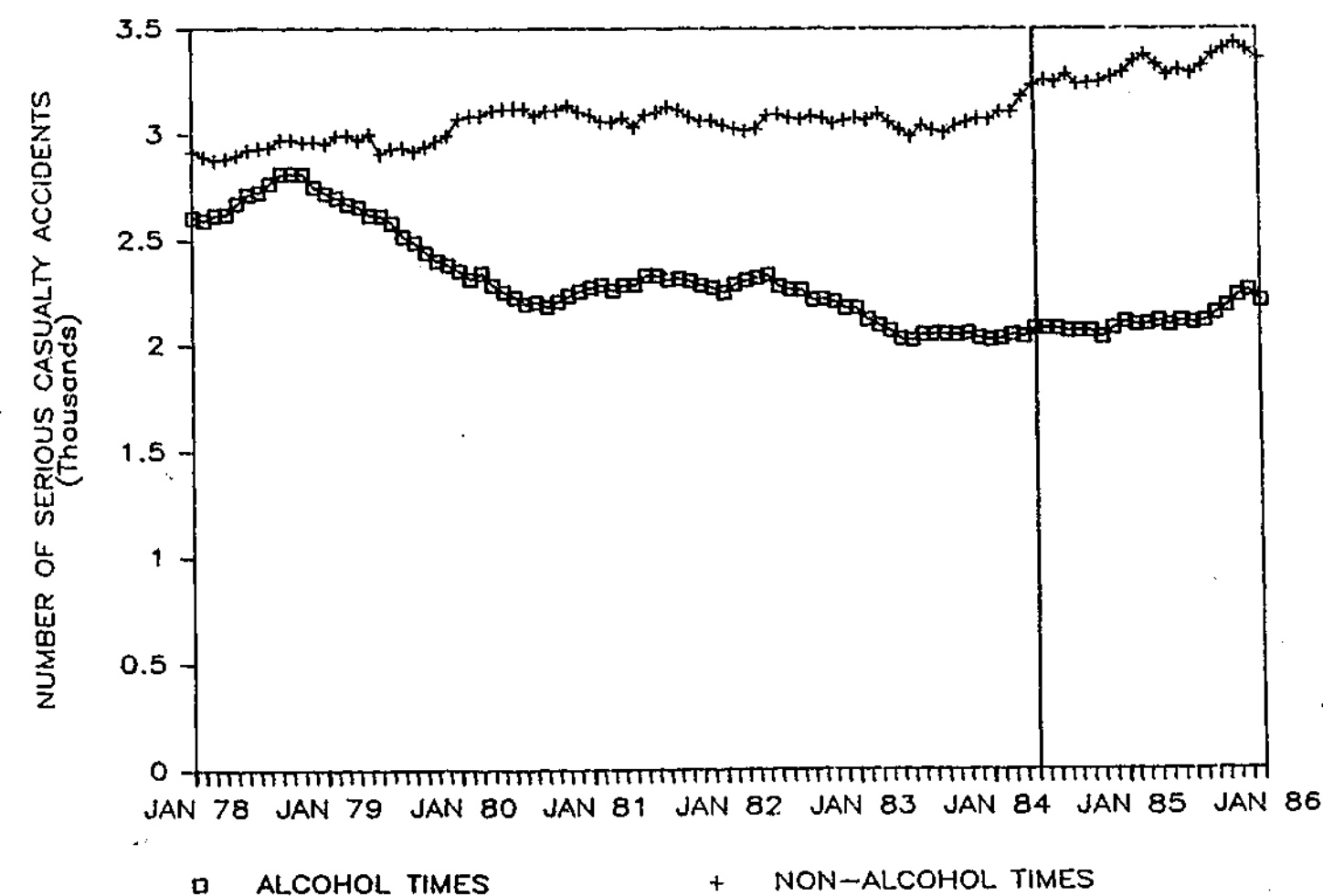
To fully take into account the effects of influences on the target group drivers other than the zero BAC legislation, the net difference for the target group needs to be adjusted for the net difference for the standard license holders (although the latter is small in this instance). The adjusted net difference for the target group was a 3.8% reduction (Table 1). This is the best estimate of the effect of the zero BAC legislation on the SCA involvements of target group drivers during the alcohol times in the postlegislation period.

The statistical significance of this estimat-

ed effect was judged by appropriate differencing of the standardized intervention coefficients for each of the four series, following the procedure above for calculating the adjusted net percentage difference. The adjusted net difference of the intervention coefficients is directly related to the adjusted net percentage difference, but has the advantage that a statistical test can be made to test whether it is real or due to chance variation.

A positive intervention coefficient (+1.7%) was observed for target group drivers when the step function was estimated for alcohol times. This coefficient was not statistically significant ($t=1.43$). On the other hand, a positive intervention coefficient (+9.2%) was observed for target group drivers when a step function was estimated at nonalcohol times. This coefficient was also not statistically significant ($t=1.23$). Further, the difference between these

FIGURE 2
TIME SERIES ANALYSIS: 12-MONTH MOVING TOTALS OF SERIOUS CASUALTY ACCIDENTS FOR
CONTROL GROUP (STANDARD LICENSE) DRIVERS



two standardized intervention coefficients was not statistically significant ($t = -0.99$).

For standard license holders during alcohol times, the intervention coefficient was positive (+0.6%), although not statistically significant ($t = 1.14$). However, the intervention coefficient was +8.2% ($t = 3.65$) for standard license holders at nonalcohol times. This implies that the increase in standard license holders' SCAs at nonalcohol times was significantly above that expected from the ARIMA model forecasts.

When the adjusted net difference of the four estimated intervention coefficients was calculated, it was not statistically significant ($t = -0.004$). Thus, the estimated effect of the zero BAC legislation (i.e., 3.8% reduction

in target group driver involvements during the alcohol times) must be concluded as due to chance variation.

Pre-Post Comparisons

The effects of the zero BAC legislation were also examined by comparing the post-legislation period with a comparable 18-month prelegislation period (July 1982 to December 1983). However, it should be noted that this method of analysis does not take into account the prelegislation trends and hence the results should only be used for comparative purposes with the more definitive time series analysis results.

Table 2 shows that there were 366 target group drivers involved in SCAs during alcohol times in the postlegislation period compared to 837 in the prelegislation period, an increase of about 3.6%. However, there was an 11.9% increase in SCA involvements for the target group in the nonalcohol times between the same two periods. As it could be expected that a similar 11.9% increase in the target group's SCAs would have occurred during the alcohol times in the absence of the legislation, these results indicate that the net effect of the legislation was a 7.4% reduction in SCA involvements for target group drivers during the alcohol times. This net reduction was not statistically significant.

A 1.5% net reduction in SCAs also occurred for the standard license holders between the same periods when alcohol times were compared with nonalcohol times. Thus, compared to standard license holders, target group drivers had an overall 6.0% net reduction in SCAs during alcohol times in the first 18-month postlegislation period. A one-tailed $2 \times 2 \times 2$ contingency table test shows that this reduction was not statistically significant (Bishop, Fienberg, & Holland, 1978).

The results in Table 2 are in good agreement with those in Table 1 where the time series results are summarized. The largest difference is that the forecast postlegislation SCA involvement of the target group in the alcohol times is only 803.3 (Table 1) compared with 837 during the 18-month prelegislation period (Table 2); this illustrates the

TABLE 2
DRIVERS INVOLVED IN SERIOUS CASUALTY
ACCIDENTS 18 MONTHS BEFORE AND AFTER
ZERO BAC LEGISLATION

Group	Before (July 1982- Dec. 1983)	After (July 1984 Dec. 1985)	% Change
<u>Target Group</u>			
Alcohol times	837	866	+3.6
Nonalcohol times	586	655	+11.9
Net change ^a			-7.4
<u>Standard License Holders</u>			
Alcohol times	3,095	3,288	+6.3
Nonalcohol times	4,593	4,958	+7.9
Net change ^a			-1.5
Target group net change adjusted for standard license holders net change			-6.0

^aAlcohol times relative to nonalcohol times.

prelegislation trend which the pre-post method is unable to take into account.

Nevertheless, the critical net changes in the two tables are in the same direction and approximately the same magnitude. Thus, the results in Table 2 provide confirmation of the more conclusive results in Table 1.

DISCUSSION

Power of the Statistical Tests

The results of the analyses show the absence of a statistically significant effect of the legislation during its first 18 months. The most definitive test performed was that based on the adjusted net difference of the intervention coefficients, which showed that the adjusted net percentage reduction (3.8%) in the target group drivers' SCAs

during the alcohol times was not statistically significant.

However, if it is assumed that the actual effect was, in fact, the measured effect of 3.8% reduction, the power of this test is only 0.21. The power of a statistical test of a null hypothesis is the probability that it will lead to the rejection of the null hypothesis when it is false, i.e., the probability that it will result in the conclusion that the phenomenon exists. Power analysis has been carried out to determine the minimum effect of the legislation which could have been detected in the target group.

The results show that the power of the test was not sufficient for the 18-month postlegislation data unless a 20% SCA reduction could be expected, which is possible but unlikely. Past experience suggests that drink-driving countermeasures are likely to achieve no more than 10% to 20% reduction in the accidents they are aimed at—those involving alcohol. Thus the zero BAC legislation could be expected to achieve no more than 2.5% to 10% reduction in the target group's SCA involvement during the alcohol times.

Further power analysis revealed that if the actual effect of the legislation was a 10% reduction, then this effect would be detected with adequate power only if 40 months' postlegislation data are used. The analysis to date suggests that an effect of this magnitude is possible. However, premature analysis would have a high probability of inconclusive results because a 10% reduction is the maximum effect that appears possible, and smaller effects would require a longer postlegislation period before conclusive results could be reached.

Possible Factors Diluting the Effect of the Zero BAC Legislation

Notwithstanding the lack of power of the statistical tests, the results of the analyses indicate the absence of a substantial effect of the legislation during its first 18 months. This absence may be explained by at least four factors:

1. The apparent absence of specific en-

TABLE 1
DRIVERS INVOLVED IN SERIOUS CASUALTY
ACCIDENTS DURING JULY 1984-DECEMBER
1985 (18 MONTHS POSTLEGISLATION)

Group	ARIMA Forecasts ^a	Actual Numbers	% Difference
<u>Target Group</u>			
Alcohol times	803.3 (751.6)	866	+7.8
Nonalcohol times	588.5 (642.5)	655	+11.3
Net difference ^b			-3.1
<u>Standard License Holders</u>			
Alcohol times	3,061.7 (3,078.9)	3,288	+7.4
Nonalcohol times	4,650.7 (4,977.4)	4,958	+6.6
Net difference ^b			+0.7
Target group net difference adjusted for standard license holders net difference			-3.8

^aIntervention model forecasts in parentheses.
^bAlcohol times relative to nonalcohol times.

forcement procedures supporting the legislation;

2. The absence of mass media publicity supporting enforcement of the legislation, in particular, publicity aimed specifically at deterring the target group;

3. The existence of decreasing trends in SCAs during alcohol times in the prelegislation period, especially for the target group of the legislation, apparently due to a package of drinking-driving countermeasures introduced progressively since 1976 (see South, Swan, & Vulcan, 1983, and RACV, 1983) which has meant that any additional countermeasure such as the zero BAC legislation could only be expected to have a relatively small incremental effect at the margin;

4. Some insensitivity in the method of using alcohol times as a proxy for alcohol involvement in serious casualty accidents, and nonalcohol times as a proxy for the absence of alcohol involvement. (Neither category is pure and this dilutes the measured effect).

No other factor is apparent to explain the absence of a substantial effect. Random breath testing activity has continued at a similar or greater level during the period up to December 1985. This measure is not specifically aimed at the target group drivers, but, coupled with mass media publicity aimed at drinking and driving in general, is currently the principal mechanism by which the target group is expected to be deterred from committing the zero BAC offense.

Postlegislation Trends

Throughout the preceding analysis, there was a general pattern of increased SCA involvement during the postlegislation period (compared with ARIMA forecasts or with SCA involvement in the prelegislation period) for all categories of driver and especially for the target group drivers during the non-alcohol times (see Tables 1 and 2 and Figures 1 and 2). Although the analysis method was able to take this pattern into account while assessing the effect of the zero BAC

legislation, the postlegislation trends still demand some explanation.

The patterns of increased SCA involvements may be explained by the following factors:

1. Up to June 1984, there appear to have been generally downward trends in new issues of car learner permits and car probationary licenses per month (see Figures 8 and 9 in Haque & Cameron, 1987). Since then, the probationary license issue rate increased by 5.7% in July to December 1984 (relative to 1983-1984), by 11.2% in January to June 1985, and by 18.7% in July to December 1985. These increases in new licensing rate took some time to affect the number of current first-year probationary licenses, but by July to December 1985 this number was 9.5% greater than the 1983-1984 average. Thus, these increases probably explain a substantial proportion of the target group's SCA involvements during the post-legislation period, especially in the nonalcohol times.

2. Introduction of Sunday bar trading may also have had an effect on the number of SCAs involving all categories of drivers. Sunday bar trading was first introduced in July 1983 for 4 hours in 2 periods on Sundays. In November 1984, this was relaxed and extended to 8 hours. In the interim analysis of the zero BAC legislation, Haque et al. (1986) found that there was an increasing trend of SCAs involving standard license holders on Sundays since late 1983. Experiences from other states show that Sunday bar trading has increased the number of SCAs (Lind, 1982; Smith, 1980). These findings suggest that the introduction of Sunday bar trading and its later relaxation may have caused an increasing number of driver involvements in SCAs since late 1983.

CONCLUSIONS

The analysis showed that there was no significant accident reduction due to the implementation of the zero BAC legislation in Victoria, probably due to the absence of

specific enforcement procedures and a mass media publicity campaign. Hence, in order to achieve a significant accident reduction, it is essential to implement the various specific procedures and mass media publicity supporting enforcement of the legislation.

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Paper No. 6

A.P. Vulcan, M.H. Cameron and W.L. Watson,
"Mandatory bicycle helmet use:
experience in Victoria, Australia".

Paper No. 6

Special issue on "Road Trauma and its Prevention",
World Journal of Surgery,
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Mandatory Bicycle Helmet Use: Experience in Victoria, Australia

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On July 1, 1990, the legislation requiring wearing of an approved bicycle (safety) helmet by all pedal cyclists, unless exempted, came into effect in Victoria, Australia. The paper describes the more important activities which paved the way for this initiative and presents some preliminary information about the effect of the legislation on wearing rates and head injuries. Since 1980 there has been promotion of helmet use through bicycle education in schools, mass media publicity, support by professional organizations and community groups, bulk purchase schemes, and government rebates for helmet purchases. The Australian Standard for bicycle safety helmets has also been changed to meet community demands for lighter helmets with more provision for ventilation. There has been a steady increase in voluntary helmet use in Melbourne from 1983 to March 1990, as follows: 5% to 70% in primary school children; 2% to 20% in secondary students; and 27% to 40% in adults. In the period after the legislation, with relatively little enforcement, these three groups have shown substantial increases in helmet use rates, rising to 70-90% in most cases. Preliminary data show that the numbers of bicyclists with a head injury have dropped in the period since the legislation came into effect. The possible contributions to this reduction, of less bicycle use and lower risk of head injury in an accident, are discussed.

On July 1, 1990 the legislation requiring wearing of an approved bicycle (safety) helmet by all pedal cyclists, unless exempted, came into effect in Victoria. It is understood that Victoria was the first state in the world to introduce such a requirement. This paper describes the more important activities during the past decade which paved the way for this groundbreaking initiative and provides preliminary information about the effect of the legislation on wearing rates and injuries.

The Legislation

The requirement to wear a helmet when bicycling in Victoria is implemented as a regulation under the Road Safety Act 1986. The Road Safety (Bicycle Helmets) Regulations 1990 amends the Traffic Regulations to insert *inter alia*:

"Bicycle Helmets to be Worn: A person must not drive a bicycle on a highway unless he or she is wearing a securely fitted bicycle helmet of a type approved by the Roads

Corporation. A person must not use a bicycle to carry another person on a highway unless that person is wearing a securely fitted bicycle helmet of a type approved by the Roads Corporation [VICROADS]. Penalty: 1 penalty unit."

The maximum penalty of 1 penalty unit (currently \$100) is rarely applied as offenders are not normally taken to court, but rather a Traffic Infringement Notice for \$15 is issued. For children, a Bicycle Infringement Notice (no monetary penalty) may be sent to the parents.

At the time of the announcement (September 1989) of the introduction of the requirement, the Australian Standard for bicycle helmets was under review [1]. VICROADS approval was introduced as an interim measure pending amendment of Standard AS 2063.2 in April 1990. This system allowed the newer, lighter-weight style helmets to be approved, without compromising safety for cyclists. Helmets satisfying the existing standard received automatic approval. However, from April 1992, certification to the new Australian Standard replaced the need for VICROADS approval.

The definition of a highway includes any area provided for separate vehicular traffic or use by the public for passage with vehicles (a bicycle is defined as a vehicle in Victorian law). In practical terms, this means that all cyclists are required to wear a helmet whilst cycling on the road, separate bicycle path, shared footway, or in a public park [1].

The regulation also provides for the following exemptions to be granted:

1. "a person participating in a race that is declared by the Roads Corporation by notice in writing served on the race organisers to be an authorised race for the purposes of this regulation"; to avoid discouraging overseas participants in international bicycle races from competing in Victoria;
2. any person who "would find it extremely difficult to comply with those requirements"; to cover medical conditions not anticipated at the time of the regulation;
3. until 1 July 1991, a person who "is a practising member of an organised religion who is wearing a headdress customarily worn by members of that religion and that headdress makes it impracticable for the person to wear a bicycle helmet";

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specifically to exempt (at least temporarily) the Sikh community in Victoria, who cannot accommodate a turban with a bicycle helmet; and

4. until 1 July 1991, a person who "has a physical condition or characteristic that makes it impracticable for the person to wear a bicycle helmet"; to cover the small percentage of the population with heads larger than the maximum helmet size available.

While the regulation does not specify so, an exemption has also been granted to Postal Delivery Officers riding bicycles while delivering mail, but not while cycling to and from the Post Office [1].

Setting the Scene

A climate which favored promotion of bicycle helmets with an ultimate goal of compulsory use had been created in Victoria during the 1960's and 1970's. A law which required all motorcyclists to wear an approved helmet had come into effect in Victoria in 1961. During the 1970's the Victorian government had achieved considerable success in reducing deaths and injuries to all road users through restrictive legislation supported by publicity and enforcement, e.g., compulsory seat belt (1970) and child restraint use laws (1975), random breath testing (1976), higher penalties and licence cancellation directed against drink-driving (1978), and engine capacity limits for novice motorcyclists [2].

By the mid-1970's, a few cyclists were wearing bicycle helmets of unknown protective performance, although some imported helmets met overseas standards. In 1977 the Standards Association of Australia published its standard AS2063-1977 "General Purpose Protective Helmets (for use in pedal cycling, horse riding and other activities requiring similar protection)," but it was not until 1981 that the first helmet was certified as meeting the Standard.

An important study of the differences between the pattern of injury for motorcyclists, nearly all of whom wore helmets, and bicyclists, very few of whom wore helmets, showed significant differences. "The frequency of fractured vault of skull was significantly higher in pedal cyclists than in motorcyclists sustaining only head injury. Although motorcyclist casualties overall outnumbered pedal cyclist casualties two to three times, there were about twice as many pedal cyclists as motorcycle riders with sole head injuries" [3].

A later study examined the effectiveness of bicycle helmets for a sample of wearers on the basis of self-reported accidents. It concluded that the better hard shell helmets gave greater levels of protection than inferior hard shell helmets, soft helmets, or no helmets [4].

Helmet Promotion

There was a wide variety of activities to promote helmet use including education, publicity, support by professional and interested organizations, bulk purchase schemes, and helmet rebates [1, 5]. These culminated in a statement in Parliament by the Hon. Steve Crabb, Minister for Transport, in October 1984 that the government would move towards compulsory helmet

wearing legislation [6]. Some of these activities are described below and in greater detail elsewhere [5, 7].

Education

In 1980, a bicycle safety education unit (Bike-Ed) for use in primary and lower post-primary schools (students aged 9-13 years) was developed. Promotion of bicycle helmets was achieved through the use of this unit and an associated film on safer riding.

A decision by the Education Department that from January 1983 helmets must be worn when participating in all school cycling activities, including Bike-Ed, was a major turning point in the acceptance of helmets. At the time however, it was considered by some to be the death-knell for Bike-Ed as a voluntary activity because "most students would not participate if helmet wearing was compulsory." It was clear that means would have to be found to promote helmets and make them more readily available to schools if Bike-Ed was to survive and expand.

Improving Helmet Availability

During the second half of 1982, the Road Safety & Traffic Authority established a bulk helmet purchase scheme, with the co-operation of one Education Department region, which enabled parents to order helmets through schools at \$30 (approximately 33% discount). All 1,000 helmets underwritten by the scheme were sold and there was demand for more. This pilot scheme clearly demonstrated that demand for helmets could be generated by such discount schemes.

Guidelines for a modified bulk purchase scheme involving discount through arrangements with retailers were developed during 1983 and assistance was provided to a number of organizers of such schemes through Education Department Regions and individual schools. Schools were also encouraged to seek support from local service clubs for the purchase of helmets and some succeeded. Several Education Department Regions acquired trailers which contained sufficient helmets and other equipment for a Bike-Ed course, which were loaned to schools.

Publicity

In preparation for a major publicity campaign, the Road Traffic Authority commissioned research on the attitudes of cyclists to helmet wearing [8]. This concluded, first, that there are strong negative reactions by the older boys to bicycle helmets, and they have heightened sensitivity to peer group pressure. Second, the reactions of younger boys and girls, although still negative, are predicated to a much lesser degree by peer group pressure. Third, promotional activity should be targeted at the mothers of primary school age children. The key points to be communicated are the availability of helmets that meet approved safety standards; the seriousness of the head injury problem, both in collisions with cars and in single vehicle bicycle accidents; the degree of protection provided by the helmets; and the importance of the helmets for younger, less skilled bicycle riders.

Two television commercials were developed, based on this

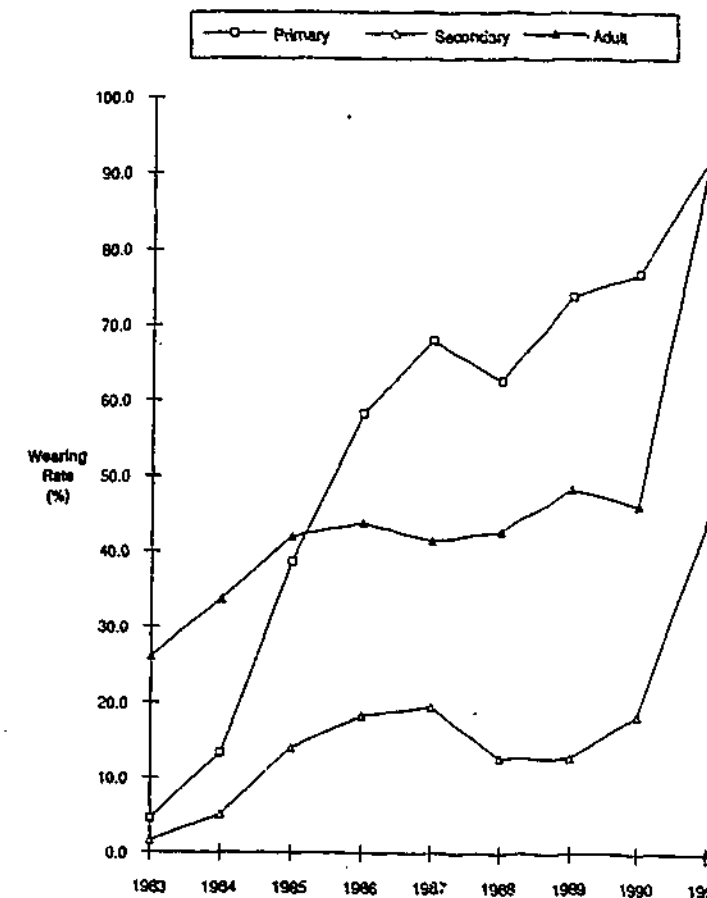


Fig. 1. Helmet wearing rates for metropolitan commuting cyclists, Victoria, Australia, 1983-1991.

and further research. In March 1984 the commercials were launched using TV and radio, supported by a pamphlet. The initial campaign lasted about two months, but the commercials continued to appear for many more months.

Further Helmet Promotion Activities

In order to coordinate and support a wide range of further helmet promotion activities, the Road Traffic Authority established a helmet promotion task force whose membership included representatives from the Bicycle Institute of Victoria, Brain Foundation, Child Accident Prevention Foundation of Australia, Education Department, Police Department, Royal Australasian College of Surgeons, Royal Automobile Club of Victoria, State Bicycle Committee, bicycle retailers, helmet importers, and helmet manufacturers. This group had been built up in part from those organizations and individuals who had attended earlier meetings on helmet promotion convened by the Royal Australasian College of Surgeons, which had been actively promoting both helmet rebate schemes and compulsory use legislation for several years.

The main helmet promotion activity during 1984 was further provision of assistance to organizers of bulk helmet purchasing schemes, mainly in Education Department Regions, but generally using designated retailers. At least 20,000 helmets were sold through these schemes at savings of about \$10 each.

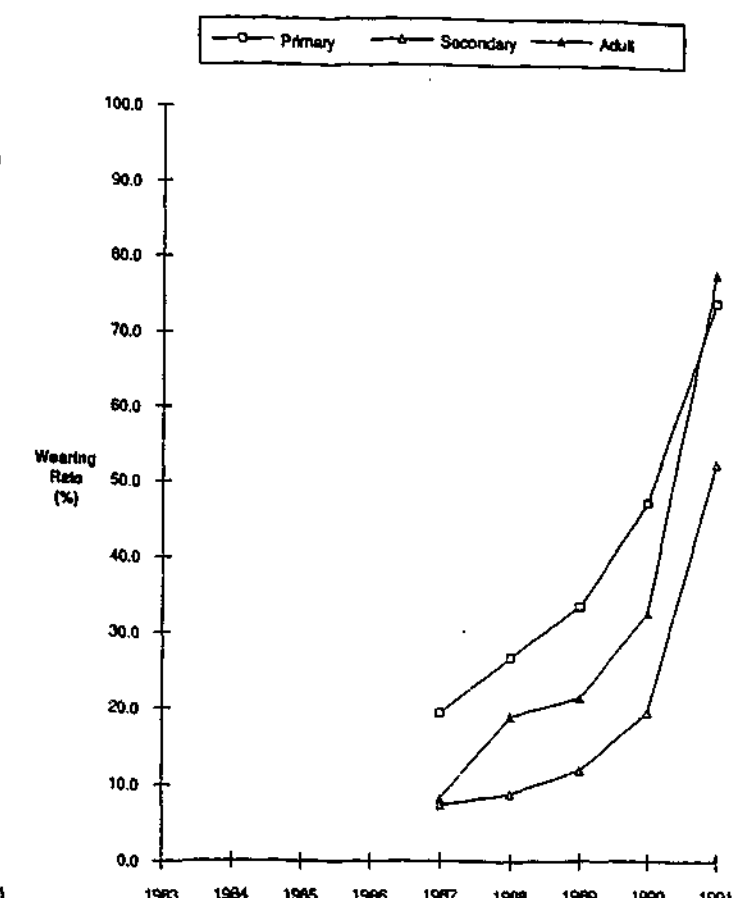


Fig. 2. Helmet wearing rates for metropolitan recreational cyclists, Victoria, Australia, 1987-1991.

Guidelines for operating such bulk purchase schemes were provided to assist organizers [9].

Helmet Rebate Scheme

Early in December 1984, the Minister for Transport announced that the government would pay a rebate of \$10 to all purchasers of an Australian made, Standards Association approved bicycle helmet. The scheme was to operate for purchases made from December 11 to 29, which was traditionally the peak time for helmet sales. The television and radio publicity campaign was re-run during this period with the mention of the \$10 rebate added to the TV commercials. A total of more than 37,000 rebates were paid and many others purchased helmets but did not bother to claim.

At the end of 1984, four imported helmets had received Standards Association approval but these helmets were not included in the rebate scheme. The importers were vehemently opposed to the initial scheme and there was public demand for extension of it. A \$5 rebate scheme began on February 25, 1985 covering all approved helmets purchased between the end of the previous scheme (i.e., December 29, 1984) and March 9. Over 5,000 rebates of \$5 were paid under this scheme.

Subsequent lobbying by Australian manufacturers and importers about which helmets should be covered, plus the views of some bicycle groups that no helmet rebates should be offered

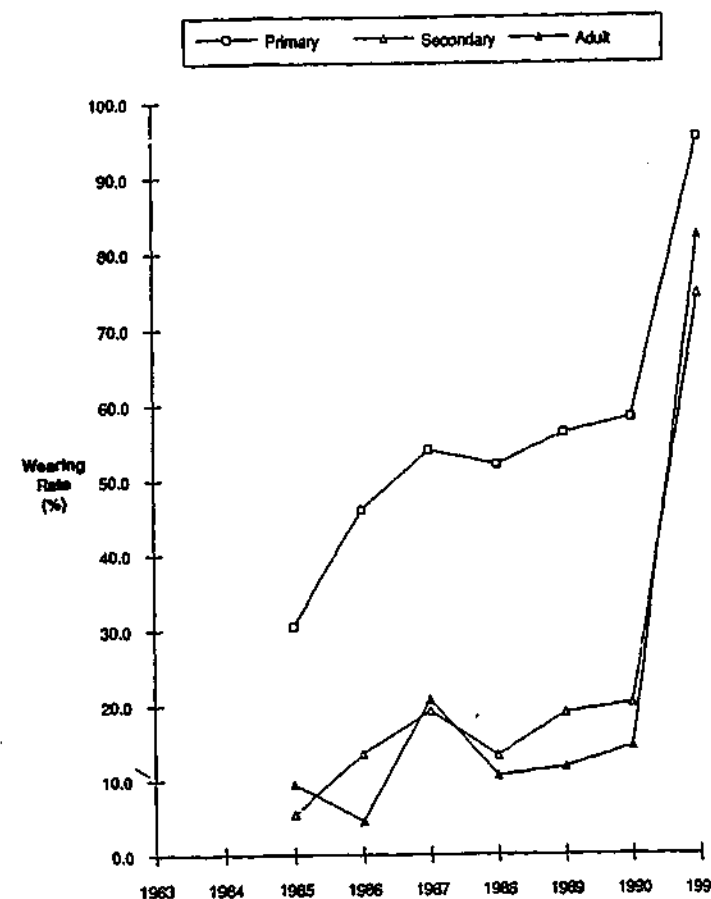


Fig. 3. Helmet wearing rates for country commuting cyclists, Victoria, Australia, 1985-1991.

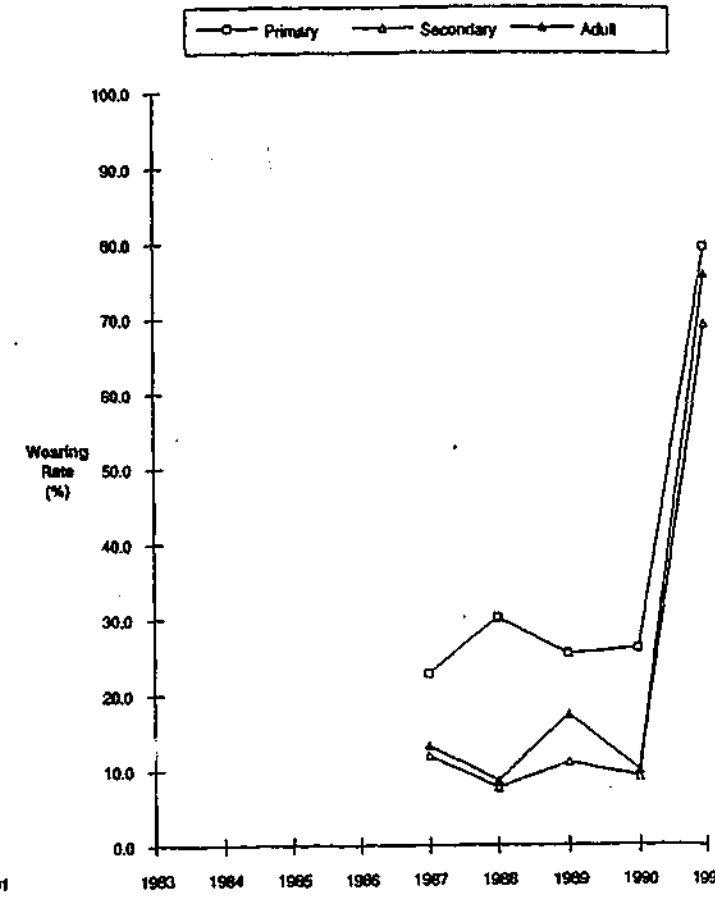


Fig. 4. Helmet wearing rates for country recreational cyclists, Victoria, Australia, 1987-1991.

until lighter, cooler helmets with better retention systems were available, almost prevented the launch of any further schemes. Ultimately compromises were reached and further \$10 rebate schemes were undertaken in December 1985, 1986 and 1987, and from December to February in 1988 and 1989 resulting in a total of more than 168,000 helmet rebates from all schemes [1].

The Australian Standard was amended in 1990 by deleting the penetration test, which allowed soft shell helmets and larger holes to enable better cooling air circulation.

Increased Helmet Wearing

In order to measure progress in helmet wearing a series of observation surveys were carried out initially in the Melbourne metropolitan area and later in several regional cities in Victoria. The metropolitan surveys were of adult commuter cyclists on arterial roads near the central business district and of primary and secondary school students on the approaches to a sample of schools. Hence the student surveys could be biased towards higher wearing rates [10]. Later, recreational riding surveys were also included [11]. The results are shown in Figures 1 and 2. The country surveys began in 1985 in 10 regional cities and showed considerable variation between cities [11]. The results are shown in Figures 3 and 4.

The surveys shown in Figures 1-4 were generally taken in March each year, so the 1991 results reflect the situation some

eight months after the helmet wearing law was introduced. Some surveys in July and October 1990 reported even higher wearing rates, particularly for secondary school students, but these surveys have not been included because of their limited size. Nevertheless, the increases in helmet wearing rates after introduction of the law are remarkable, especially as up to the time of the surveys there had been relatively little enforcement. They confirm that if the community understands the benefits of a safety measure and a reasonable proportion have been persuaded to adopt it voluntarily, then compulsory use can be achieved, if proper foundations have been laid.

Although the Hon. Steve Crabb, Minister for Transport, in launching the second helmet rebate scheme said in February 1985, "We are now aiming to get usage rates to at least 40% in all categories so that it will be possible to make the use of bicycle helmets compulsory. The Government hopes to be in that position by July 1," it took a further 5 years for the law to be introduced. During this period there were additional multimedia publicity campaigns, helmet rebate schemes, "Safe Cycle" promotions by the Victoria Police and promotion of bicycle safety in schools [1]. There were also additional studies which provided further information about the effectiveness of helmets in reducing injuries [12-15]. All of these were important, but probably the single most significant contribution was the report of the Parliamentary Social Development Committee.

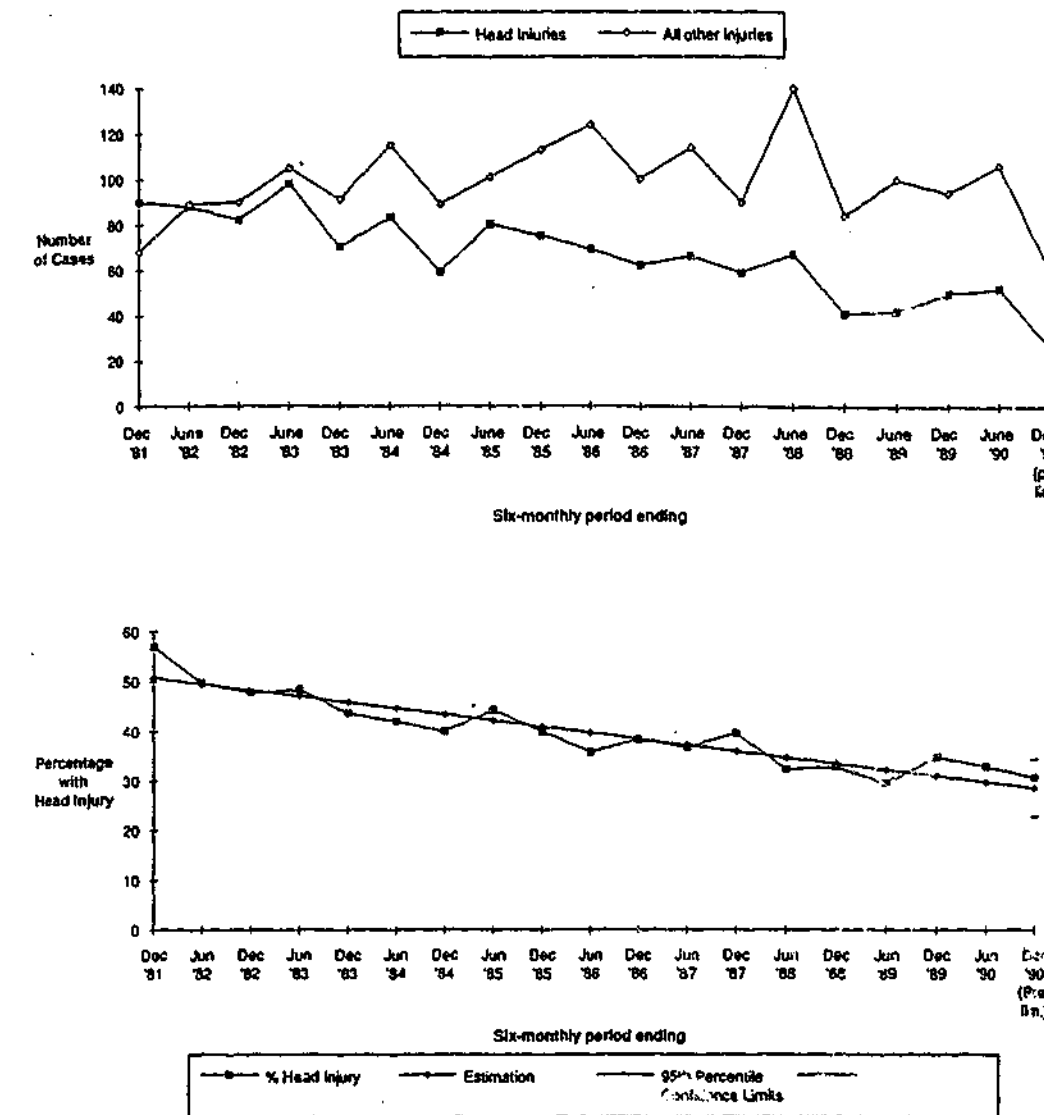


Fig. 5. Number of severe bicyclist casualties registered with the Transport Accident Commission, Victoria, Australia, July 1981-December 1990.

Fig. 6. Percentage of severe bicyclist casualties with head injury, Victoria, Australia, July 1981-December 1990.

Parliamentary Social Development Committee

During 1986 and 1987, the Social Development Committee of the Parliament of Victoria conducted an inquiry into child pedestrian and bicycle safety. In their submission to the inquiry, the Road Traffic Authority and the Victoria Police supported compulsion as a long-term solution to the problem of encouraging all bicyclist groups to wear helmets. The submission pointed out that the prerequisites for a successful law include a specific bicycle helmet standard which is agreed to by the user groups and able to be enforced at law, a sufficient range of styles and sizes in approved helmets to accommodate a wide range of head sizes, a reduction in overall price levels if undue hardship is to be avoided, and a significant increase in wearing rates. Other submissions also supported compulsory wearing.

The Committee's First Report on the Inquiry, tabled in December 1986, included eleven recommendations associated with bicycle helmet use, of which the key ones were:

"That mandatory helmet use by bicyclists be introduced as soon as possible", and

"That the RTA [Road Traffic Authority] should report to the Minister of Transport within six months of the tabling of this report with a detailed and comprehensive strategy, including a phased timetable, to introduce mandatory helmet usage. Such a report should consider: the impact of such legislation on the diverse categories of bicycle users; methods of reducing costs of bicycle helmets to users." [16]

Following extensive canvassing of comment from cyclists and the community generally, and detailed review of the key issues, the Road Traffic Authority developed a strategy in December 1987 which recommended that legislation be introduced to require the mandatory wearing of bicycle helmets while cycling, from January 1, 1989. After further deliberation, the Minister for Transport and the Minister for Police and Emergency Services announced in September 1989 as part of a package of Road Safety Initiatives that a new regulation requiring cyclists to wear bicycle helmets would take effect in Victoria from July 1, 1990.

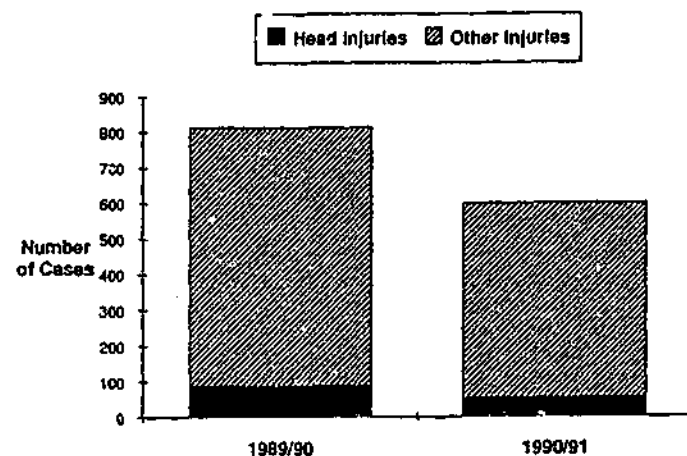


Fig. 7. Child bicyclist presentations to Victorian Injury Surveillance System hospitals, Melbourne, Australia, 1989-90 versus 1990-91.

Effect on Bicyclist Injuries

The initial effects of the law on bicyclist head injuries have been measured by examining data from three sources: Transport Accident Commission (TAC) claims for "no fault" injury compensation from cyclists who were killed or hospitalised following collision with a motor vehicle in Victoria; Victorian Injury Surveillance System (VISS) records of child cyclist presentations at the Emergency Departments of three hospitals in Melbourne (Royal Children's Hospital, Western Hospital Sunshine and Footscray campuses, and the Preston and Northcote Community Hospital); these records cover cyclists treated in Emergency Departments as well as those admitted, and also cover cyclist injuries from crashes not involving a motor vehicle; and Health Department records of acute presentations by bicyclists to public hospitals in Victoria resulting in admission (preliminary data provided in April 1991); admissions from crashes not involving a motor vehicle are included, but private hospital admissions are not.

Transport Accident Commission Claims

Bicyclist claimants were classified into those sustaining a head injury, and those sustaining any other injury, on the basis of up to five injury codes recorded using the ICD-9 system. Figure 5 shows that the number of cyclists killed or admitted to hospital with head injuries has fallen consistently between July 1981 and June 1990 as the usage of helmets has increased. In the 6 months following the introduction of the mandatory wearing law (July to December 1990), the number of cyclists with head injuries has decreased by 46% relative to the corresponding period during 1989.

The possible explanations for the recent decreases are: the number of cyclists on the road, and hence at risk of involvement in crashes, has decreased, possibly having been discouraged from cycling by the requirement to wear a helmet; the wearing of helmets has made cyclists more conspicuous, or the helmets and associated publicity have made cyclists ride more carefully, thus reducing their risk of collision; or the presence of the helmet has reduced the risk of serious head injury for cyclists involved in collisions.

These alternative explanations were addressed by examining the proportion of killed and admitted cyclists who sustained a head injury (Fig. 6). The downward trend in the proportions before the law was introduced was statistically significant ($p < 0.001$). The trend line was extrapolated to predict the proportion which would have been expected in July-December 1990 in the absence of the law and 95% confidence limits for the prediction were calculated. The actual proportion of seriously injured cyclists with a head injury is inside the prediction limits.

The progressive reduction in bicyclist head injuries relative to cyclists with other injuries is also shown in Figure 5. Since the early 1980's, the number of head injuries has fallen consistently, while the number of other injuries has increased, then remained essentially constant. The exception is the period July-December 1990, when the cyclists with other injuries decreased by 35% (and the number with head injuries by 46%) relative to the corresponding period in 1989. This is tentative evidence that the number of cyclists involved in collisions, either due to a reduction in bicycle use or a reduction in risk, has decreased during the post-law period. However, it should be noted that the total number of road traffic injuries in 1990 was about 20% below the 1989 level.

Victorian Injury Surveillance System Records

Figure 7 shows that the number of cyclists <15 years of age presenting to VISS hospitals with a head injury has decreased substantially between 1989/90 and 1990/91. During the post-law period (July 1990 to June 1991), there were only 52 child cyclist presentations with head injuries. This is a 40% reduction compared to the previous year (July 1989 to June 1990).

As above, there may be alternative explanations for these decreases. However, Figure 8 shows that the proportion of cyclist presentations who had a head injury during each month in the post-law period was generally well below the trend line fitted to January 1989 to June 1990 proportions. While none of these post-law proportions fell below the lower 95% prediction limit, collectively their deviations from the trend line were consistently below the line to a statistically significant degree (testing the sum of the deviations, $p < 0.01$). This represents evidence that there was a real reduction in the risk of injury to the head of child cyclists involved in crashes in the catchment area of the VISS hospitals during the post-law period. The lowest quarterly percentage with head injury occurred during the January-March 1991 quarter, averaging 5%.

It is also apparent that the number of child cyclists involved in crashes and presenting to the VISS hospitals has decreased (Fig. 7). The total number of child cyclists presenting decreased by 26% between July 1989 to June 1990 and the corresponding twelve month period after the law. Some of this decrease was due to the reduction in cyclists with head injuries, but even the number who did not sustain a head injury fell by 25% during the post-law period.

Admissions to Public Hospitals

Acute admissions by bicyclists were classified into those sustaining a head injury, and those sustaining any other injury, on the basis of up to five injury codes recorded using the ICD-9 system. Figure 9 shows that the number of bicyclist admissions

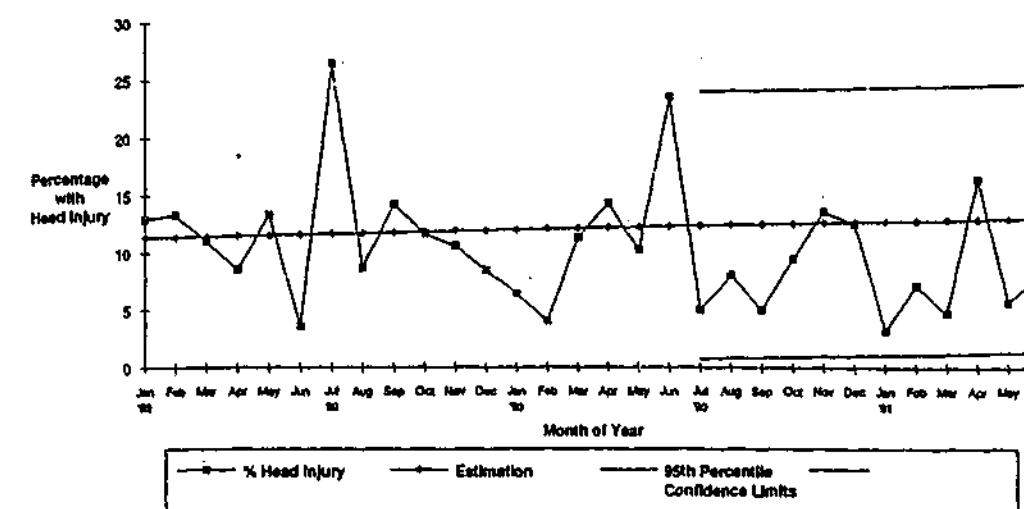


Fig. 8. Percentage of child bicyclist presentations with head injury to Victorian Injury Surveillance System hospitals, Melbourne, Australia, January 1989-June 1991.

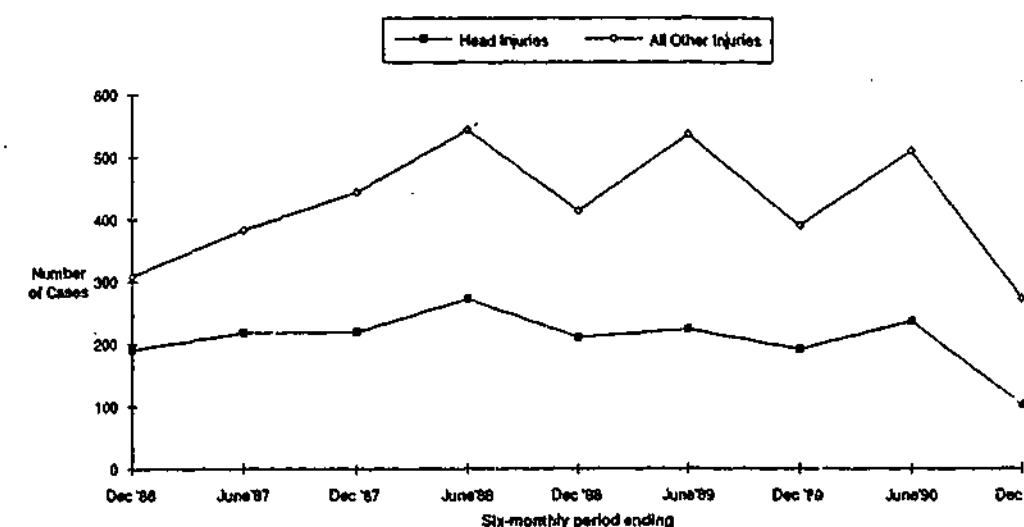


Fig. 9. Bicyclist admissions to public hospitals, July 1986-December 1990.

with head injury fell substantially during the post-law period, as did the number with other injuries. When July-December 1990 was compared with the corresponding months in 1989, head injuries decreased by 47% and cyclists with other injuries decreased by 30%.

Figure 10 shows that the proportion of bicyclist admissions who sustained a head injury has fallen during the period July 1986 to December 1990. The proportion during July-December 1990 was compared with the pre-law trend and was found to lie below the trend line but within the 95% prediction limits. The deviation of the actual proportion below the trend was not statistically significant.

Discussion

The initial results show a reduction in the number of bicyclists with head injuries following the introduction of the mandatory helmet wearing law in Victoria on July 1, 1990. However, the mechanisms by which this reduction was achieved seem to be two-fold: a reduction in the number of cyclists involved in crashes resulting in injury requiring hospital treatment, and a

reduction in the risk of head injury for cyclists who were injured.

The extent of the reduction in bicyclist claims to TAC for injuries other than to the head (35%) supports the first mechanism. This mechanism is also supported by the 25% decrease in presentations to VISS hospitals by child cyclists without head injury, and the 30% reduction in hospital admission of cyclists with injuries other than to the head. There is anecdotal evidence that some cyclists have been discouraged from cycling because they do not own or do not wish to wear a helmet.

In addition, there is evidence that the risk of head injury to cyclists involved in crashes has been reduced (the second mechanism), at least for child cyclists. There was a statistically significant decrease in the proportion of child cyclist presentations to VISS hospitals who sustained a head injury in crashes during the 12 month post-law period, with the lowest proportion occurring during the March 1991 quarter. For cyclists of all ages, the proportion of those admitted to hospital who sustained a head injury was below the trend line during the first 6 months of the post-law period, although the fall was not statistically significant. However, the proportion of TAC claims from se-

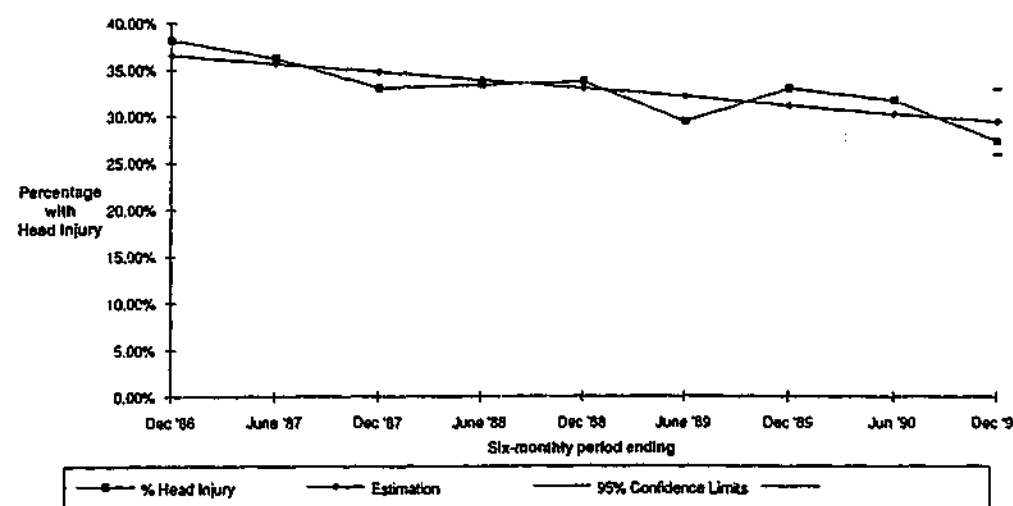


Fig. 10. Percentage of bicyclist admissions with head injury, Victoria, Australia, July 1986–December 1990.

verely injured cyclists who sustained a head injury in collisions during the same 6 months was slightly above the trend estimate.

The use of a full year of post-legislation data of cyclist injuries, when available, is necessary to confirm these results with higher levels of statistical significance. The additional data will also allow disaggregation by the cyclist age group and region of Victoria, so that the changes in cyclist injuries can be related to the specific increases in helmet wearing rates shown in Figures 1–4.

The successful implementation of the bicycle helmet use laws is the latest in a series of mandatory use laws during the past three decades, where the Victorian Government has taken a lead. The strategies and activities necessary to achieve the ultimate goal were multi-faceted and involved support from a wide range of community and professional organizations. It is important in seeking the introduction of similar measures to recognize the need for such a widely based approach and the need for patience and perseverance.

Conclusions

The mandatory bicycle helmet wearing law implemented in Victoria on July 1, 1990 has been successful in building on past efforts to promote helmet use by bringing helmet wearing rates to new high levels for all cyclist age groups throughout Victoria. The introduction of the law has been accompanied by an immediate reduction in the number of bicyclists with head injuries. Apparently this has been achieved through a reduction in the number of cyclists involved in crashes (possibly through a decrease in bicycle use or increased conspicuousness) and a reduction in the risk of head injury of child cyclists involved in crashes. Further analyses to confirm these initial trends will be done as additional data become available.

Résumé

A partir du 1er Juillet, 1990, la législation rendant obligatoire le port d'un casque (dit de sécurité) pour toute personne se déplaçant avec un deux-roues à pédale, sauf exceptions, a pris effet dans le province de Victoria, Australie. Cet article décrit les actions principales qui ont amené à créer cette loi ainsi que

quelques résultats de l'effet de cette législation comme son respect et son influence sur le taux des accidents. Depuis 1980, on a enseigné l'utilité du casque dans les lycées, à travers les médias, les clubs de rencontre professionnels et locaux. Il y a eu aussi une publicité promotionnelle importante avec possibilité d'achat en grand nombre ainsi que des prix de réductions sous certaines conditions. Le standard de casque en Australie a évolué ces derniers temps-ci de façon à répondre aux demandes de la communauté pour des casques plus légers, et mieux aérés. L'utilisation volontaire des casques n'a cessé d'augmenter à Melbourne entre 1983 jusqu'en Mars 1990: 5 à 70% chez les élèves en primaire, 2 à 20% chez les élèves en secondaires, et 27 à 40% chez les adultes. Dans la période suivant cette législation, sans contrôle draconien, ces taux sont passés à 70–90% dans la plupart des cas. Les résultats préliminaires montrent que le pourcentage des traumatismes crâniens a diminué depuis que cette loi est passée. Les contributions de l'utilisation du casque, de la diminution de l'utilisation du vélo, ainsi que le risque moindre de traumatisme crânien en cas d'accident sont discutés.

Resumen

El 1 de julio de 1990 entró en vigencia en Victoria, Australia, la legislación que requiere el uso de un casco de seguridad por todos los ciclistas, a menos que estén exentos. El presente artículo describe las principales actividades que prepararon el terreno para esta iniciativa legal y presenta información preliminar sobre el efecto de la legislación sobre las tasas de uso del casco y las tasas de lesiones craneoencefálicas. A partir de 1980 se ha hecho promoción al uso de cascos a través de programas educacionales sobre ciclismo en las escuelas, a través de los medios masivos de publicación, con el soporte de las organizaciones profesionales y los grupos comunitarios, programas de compra al por mayor y reembolsos gubernamentales por la compra de cascos. El estándar Australiano de seguridad de los cascos de seguridad ha sido modificado con el objeto de acceder a las demandas de la comunidad sobre cascos más livianos y mejor ventilados. Se ha registrado un sostenido incremento en el uso voluntario de cascos en Melbourne entre 1983 y marzo de 1990: 5% a 70% en niños de escuela primaria; 2% a 20% en

estudiantes de secundaria; y 27% a 40% en adultos. En el período siguiente a la legislación, con relativa menor necesidad de imposición autoritaria, estos grupos han demostrado sustanciales incrementos en las tasas de uso del casco, con ascenso a 70–90% en la mayoría de los casos. Los datos preliminares demuestran que el número de ciclistas con trauma craneoencefálico ha descendido en el tiempo transcurrido desde la vigencia de la legislación. Se revisan las eventuales contribuciones a este fenómeno por menor uso de bicicletas y menor riesgo de lesión craneana en el accidente.

Acknowledgments

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Paper No. 7

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VEHICLE CRASHWORTHINESS RATINGS IN AUSTRALIA*

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Abstract—The paper reviews the published vehicle safety ratings based on mass crash data from the United States, Sweden, and Great Britain. It then describes the development of vehicle crashworthiness ratings based on injury compensation claims and police accident reports from Victoria and New South Wales, the two most populous states in Australia. Crashworthiness was measured by a combination of injury severity (of injured drivers) and injury risk (of drivers involved in crashes). Injury severity was based on 22,600 drivers injured in crashes in the two states. Injury risk was based on 70,900 drivers in New South Wales involved in crashes after which a vehicle was towed away. Injury risk measured in this way was compared with the "relative injury risk" of particular model cars involved in two car crashes in Victoria (where essentially only casualty crashes are reported), which was based on the method developed by Folksam Insurance in Sweden from Evans' double-pair comparison method. The results include crashworthiness ratings for the makes and models crashing in Australia in sufficient numbers to measure their crash performance adequately. The ratings were normalised for the driver sex and speed limit at the crash location, the two factors found to be strongly related to injury risk and/or severity and to vary substantially across makes and models of Australian crash-involved cars. This allows differences in crashworthiness of individual models to be seen, uncontaminated by major crash exposure differences.

INTRODUCTION

There is a need to inform consumers of the relative safety of cars offered for sale as a way of encouraging manufacturers to improve the crash performance of their products (Social Development Committee 1990; Dowdell 1990a, 1990b). Consumer advice on vehicle safety performance can provide vehicle make/model ratings of two types:

1. Crashworthiness ratings (measuring the relative safety of vehicles in preventing injury and/or severe injury in crashes)
2. Crash involvement ratings (measuring attributes which assist or prevent vehicles from being involved in crashes).

Previous research has shown that vehicle factors play a large role in whether a car occupant is

severely injured in a crash (other key factors are the impact speed, point of impact, seating position, restraint use, and the occupant's age and sex). International evidence shows that there are considerable differences between makes and models related to vehicle crashworthiness (Campbell and Reinfurt 1973; Gustafsson et al. 1989).

In contrast, vehicle factors have been estimated in several studies (summarised by Johnston 1984) to be the cause of about 10% of crash involvements (road user factors cause about 90% and environmental factors cause about 30%; multiple causes are common). Thus there is much less potential for make/model differences related to crash involvement.

The development of crashworthiness ratings should be given priority in vehicle safety ratings because of their greater potential to find significant differences between makes and models of cars.

This paper summarises the data and analysis methods used in a Monash University Accident Research Centre (MUARC) project to develop crashworthiness ratings and presents ratings for

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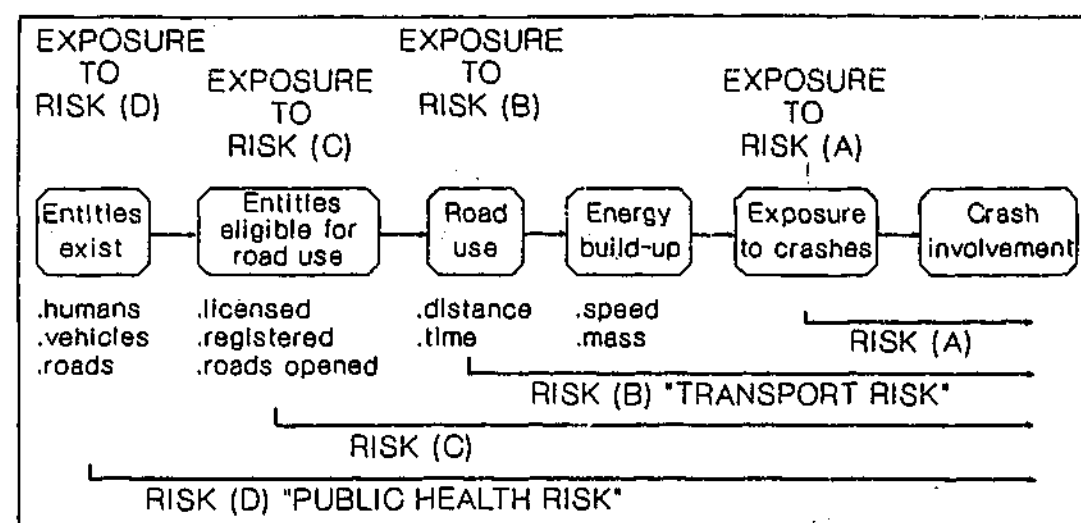


Fig. 1. The road trauma chain. 1. Precrash.

1982–1990 model vehicles based on crash data from Victoria and New South Wales (NSW) combined. Further details are given in the technical report from the project (Cameron, Mach, and Neiger 1992a), which covers the concepts developed, preparation of the data used, preliminary investigations to determine analysis methods, details of the adjustment procedures, and investigations of the separate results from Victoria and NSW.

TERMINOLOGY

Vehicle safety performance can be measured in many ways. Basically we are interested in the ability of the vehicle to assist in breaking the road trauma chain shown in Figs. 1 and 2. The vehicle ratings can reflect the various risks shown in the figures and we are interested in knowing the vehicles for which these risks are relatively high or low.

Crash involvement ratings measure one of the

precrash risks shown in Fig. 1. Which of these risks is measured depends on the type of exposure used as the denominator of the involvement rate. For mass accident data comparisons of makes/models, it is most common to use numbers of vehicles registered as the denominator and to measure risk (C) by the involvement rate. It would be preferable to measure risk (B), but road use data by make/model is not always available to calculate the appropriate rate. Risk (A) may be even more preferable, as it would measure the ability of the vehicle to assist the driver to recover from some critical pre-crash situations (e.g. skidding out of control).

Crashworthiness ratings measure the risks shown in Fig. 2. The starting event is the crash involvement and there are various risks of injury depending on the severity level specified. Injury risk is measured by the injury rate, which is the number of persons killed or injured divided by the number involved in crashes. Since many of our data sources

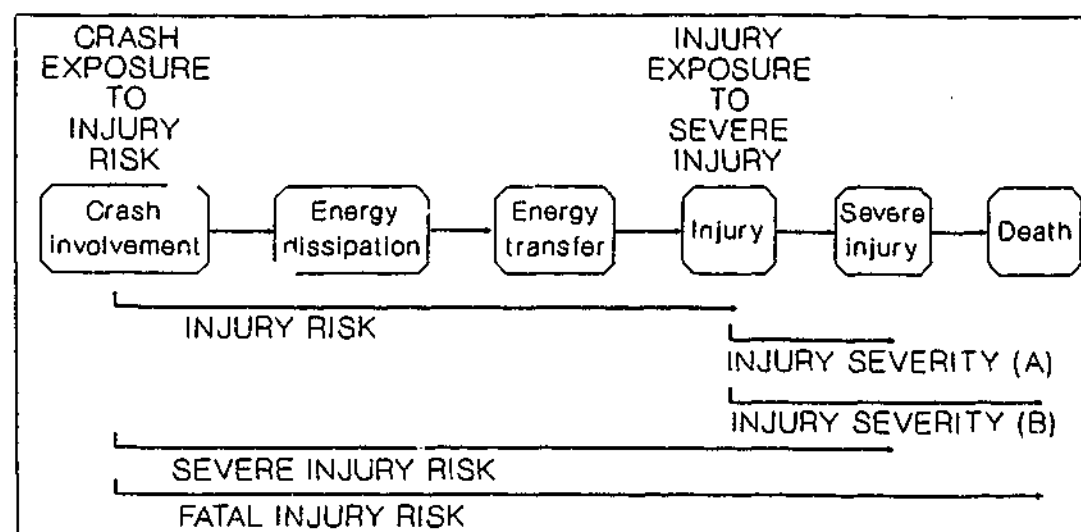


Fig. 2. The road trauma chain. 2. Crash and postcrash.

have some minimum level of injury as the entry criterion, it is useful to define injury severity as the risk of severe injury (given that the vehicle occupant is injured). Injury severity can also relate to the risk of death, for occupants who are injured. The number of crash involvements forms the exposure to injury risk and is known as "crash exposure". In injury data collections, the event of being injured (to a level providing entry to the data system) represents the "injury exposure" to the risk of severe injury.

Vehicle ratings for different makes/models can be developed to measure each of the risks defined above. Whether the observed differences measure differences in vehicle safety characteristics can depend on how the ratings are calculated. Differences in the driver and passenger characteristics, in the crash speed, in environmental factors, and in the crash type, could potentially hide any vehicle design differences. If the aim of vehicle ratings is to measure true differences in vehicle safety, then the analysis needs to take into account these other differences. This can be done by normalisation, i.e. by calculations which attempt to make the exposure distribution the same for all makes/models; or by estimating the expected rating of a specific make/model (taking exposure into account) for use as a reference figure against which the actual rating should be compared.

LITERATURE REVIEW

Vehicle safety ratings based on mass crash data have been published as consumer advice by the Highway Safety Research Centre (Campbell and Reinfurt 1973), the Insurance Institute for Highway Safety (IIHS 1991), and the Highway Loss Data Institute (HLDI 1991) in the United States; Folksam Insurance (Gustafsson et al 1989; Folksam, undated) in Sweden; and the U.K. Department of Transport (DOT 1991). These are summarised in Table 1 and detailed reviews are given elsewhere (Cameron 1991; Cameron, Mach, and Neiger 1992a). Vehicle safety ratings based on sources other than mass crash data have also been published, such as those using results from barrier crash tests (Gillis 1991) or those from assessments of the presence of a number of occupant protection features in each model (Vehicle Safety Consultants 1989).

The five published vehicle safety ratings based on mass crash data have each used different measures of vehicle safety performance. While the general tendency is for the measures to cover crashworthiness aspects (perhaps reflecting a perception that the biggest differences between cars should emerge in this dimension), many of the measures

embody the risk of crash involvement as well. Only the vehicle safety ratings published by the Highway Safety Research Centre and by Folksam Insurance can be considered to measure crashworthiness exclusively.

In some cases an organisation appears to recognise that its measure includes crash involvement risks, and it takes steps to correct the differences in risk between models by adjusting or categorising its vehicle ratings by factors such as driver age and sex and the type of car (e.g. body style, age, performance, and whether privately or company owned). It is usually not known or not stated whether these factors adequately account for the differences in crash involvement risk. It is possible that the vehicle safety ratings published by IIHS, HLDI, and the U.K. DOT continue to reflect differences in driver types and usage patterns as well as differences in crashworthiness between the models of cars that they compare.

CRASH DATA USED

Victorian crashes

Detailed injury data have been collected by the Transport Accident Commission (TAC) and its predecessor, the Motor Accidents Board, as part of their responsibilities to provide road transport injury compensation in Victoria. Details of the vehicle occupied were added from the VIC ROADS vehicle registration system.

TAC claims from drivers of cars and station wagons manufactured after 1981 who were involved in crashes in the period 1983 to 1990, and whose medical expenses exceeded a threshold that was indexed from year to year (\$317 in 1989), were matched with police accident reports. The police reports covered all drivers involved in accidents, no matter whether the police officer recorded the person as injured or uninjured (it was possible for an injury claim to be made in circumstances where injury was not apparent at the time of the accident). Accidents are reported to the police in Victoria if a person was killed or injured, if property was damaged but names and addresses were not exchanged, or if a possible breach of the road traffic regulations has occurred (Green 1990).

The merged file covered 15,876 drivers of 1982–1990 model cars and station wagons crashing during 1983–1990. Of these drivers involved in reported crashes, 12,867 (81%) were injured (i.e. TAC claimants), and 3,158 (24.5%) of the injured were killed or admitted to hospital (i.e. formally admitted and staying at least one night).

Table 1. Summary of published vehicle safety ratings based on mass crash data

Publishing organisation	Rating measures used in the publication	Dimensions covered by the measure: Crash involvement (CI) Crashworthiness (CW)	Factors used to adjust the ratings before comparison between models	Factors used to categorize the [adjusted] ratings into car groups
Highway Safety Research Centre (USA)	Rate of driver death or serious injury per involvement in crash with damage exceeding \$100	CW	Impact speed Point of impact on car Accident type	Crash type (single-vehicle versus car-to-car crashes)
Insurance Institute for Highway Safety (USA)	Occupant death rate per 10,000 registered cars	CI and CW	Car wheelbase Driver age Driver sex	Car wheelbase Body style
Highway Loss Data Institute (USA)	1. Occupant injury rate per insured vehicle year a) any injury b) injury costs > \$500	1. CI and CW	1. Driver age	Car wheelbase Body style
	2. Vehicle damage payments per insured vehicle year	2. CI	2. Driver age Deductible amount (excess)	
Folksam Insurance (Sweden)	1. Relative risk of driver injury in two-car crashes	1. CW	1. Car weight (contra to weight of "other" car)	Car weight
	2. Risk of death or permanent disability to occupants who were injured	2. CW	2. Nil	
	3. Combination: 1 by 2	3. CW	3. Car weight (see above)	
Department of Transport (UK)	1. Injury accident involvement rate per 10,000 registered cars	1. CI and CW	Nil	Age of car Performance of car Size of car Owner of car (private or company)
	2. Car occupant casualty rate per 10,000 registered cars	2. CI and CW		
	3. Driver casualty rate per 100 accidents involving impact with another vehicle or other hard object	3. CW		

New South Wales crashes

The NSW Roads and Traffic Authority (RTA) supplied a file covering 75,860 light passenger vehicles involved in police-reported crashes during 1989–1990 that resulted in death or injury or towing

of a vehicle. The National Roads and Motorists' Association (NRMA) had added the make, model, and year of manufacture to these vehicles after matching with the NSW vehicle register via registration number. The file supplied covered vehicles man-

ufactured during the period 1982–1990. The file not only covered cars and station wagons, but also covered four-wheel drive vehicles, passenger vans, light trucks, and other commercial vehicles (these could be identified by their model).

The vehicle file (which also contained driver age and sex) was merged with files supplied by NSW RTA covering details of the person casualties (killed and injured persons) and the reported crashes for the same years. Each vehicle/driver matched uniquely with the corresponding crash information, but only injured drivers could match with persons in the casualty file. A driver who did not match was considered to be uninjured. When the unoccupied vehicles were excluded, the injury details of 73,399 drivers involved in crashes were available. According to the data supplied about these drivers, 10,097 (13.8%) were injured and 2,045 (20.3%) of the injured were killed or admitted to hospital (i.e. as in the Victorian data, staying at least one night).

The presence of uninjured drivers in the merged data file meant that it was suitable for measuring the risk of driver injury (in cars sufficiently damaged to require towing). This contrasted with the Victorian data file, which could not be used to measure injury risk directly because not all uninjured drivers were included.

DERIVATION OF MODELS OF CARS

The Victorian vehicle register provided the make and year of manufacture of the crashed vehicle but not the model. Model was initially derived for 1982–1988 model cars using logic developed and supplied by the Royal Automobile Club of Victoria (RACV) based on the make, year, and power-mass units. Power-mass units (PMU) are the sum of RAC horsepower units (PU) and the vehicle mass in units of 50 Kg (MU). Refined logic was developed by MUARC based on make, year, PMU, PU, MU, and body type and extended to cover 1989–1990 models. Both logics were applied to the combined Victorian data to derive passenger car models for the model years 1982–1990.

The NRMA had decoded the chassis number (obtained from the NSW vehicle register) to determine the models of light passenger vehicles that crashed in NSW. The decoding identified some light-truck and other commercial models that were not considered further. In addition, because the Victorian data were limited to cars and station wagons, the four-wheel drive and passenger van models in the NSW data were analysed separately.

All but 8% of the NSW vehicles had a model identified; in these cases the make of the vehicle was

used as the model in both states' data. Comparison between makes that contain more than one model should be made with care and may not be legitimate, because some manufacturers have a broad span of masses in their model range.

Expert advice was obtained regarding the models, years, and body types that needed to be kept separate, or could be aggregated, because of dissimilarity/similarity of crashworthiness aspects (this advice was used in the presentation of results). However, some models have undergone substantial change during 1982–1990 and their aggregate rating for all the year models released in this period needs to be interpreted with caution.

MEASURES TO RATE CRASHWORTHINESS

Crashworthiness ratings measure the risk of serious injury to the drivers of each specific model car when it is involved in a crash. This risk can be measured in two components:

1. risk of injury for drivers involved in crashes (*injury risk*), and
2. risk of serious injury for drivers who are injured (*injury severity*).

Following the method used by Folksam Insurance (Gustafsson et al. 1989), it is then possible to calculate an overall crashworthiness rating, defined as:

$$\text{Combined rate} = \text{Injury severity} \times \text{Injury risk}.$$

The combined rate defined in this way can be interpreted as measuring the risk of serious injury to drivers involved in a crash.

Serious injury can be variously defined, but in this study, serious injury was taken as death, or injury requiring hospital admission. In Australia, injuries such as "whiplash" (alone) seldom result in immediate hospital admission, but can often lead to long-term disability, whereas persons are frequently retained in hospital overnight for observation following blunt trauma to the head even though they may be subsequently considered uninjured.

Aldman et al. (1984) recommended that "when individual car models are studied the possible influence of the age of the driver, speed limit at the scene of the accident, belt usage rate, weight of the struck car, (and) impact direction . . . must be taken into account". Major differences in crash patterns between models of cars have the potential to hide any effects of the vehicle design on injury risk or injury

severity. It is necessary to take account of these differences if valid comparisons of the crashworthiness of cars are to be made.

The variables in the data files that described the crash patterns included speed zone, crash type, point of impact on the car, car mass, and restraint use. The driver age and sex are related to injury susceptibility. The first question was whether any of these variables had a significant relationship with the injury severity and injury risk measures selected for rating crashworthiness. The second question was whether the significant variable differed enough between makes/models for this to make a substantial difference to the rating scores. These questions were investigated separately for each measure (Cameron et al. 1992a) and the findings are summarised in the following sections.

However, it should be noted that other factors not collected in the data (e.g. crash speed) may have had substantial effects on injury severity or injury risk. These effects may have remained (at least partially) after the major available factors were taken into account by the analysis method.

Injury severity of injured drivers

The data on injured drivers from Victoria and NSW were pooled to measure injury severity by model of car, i.e. to provide the first component of the combined rate. In the pooled data, 5,193 of the 22,964 injured drivers were killed or admitted to hospital, representing an injury severity of 22.6 per 100 injured drivers. This was considered sufficiently similar to the separate injury severities from Victoria (24.5%) and NSW (20.3%) to justify combining the two states' data. The standard deviation of the pooled injury severity (0.27) was substantially lower than that for either state alone. Thus the pooled data have greater sensitivity and reliability than the separate data sets.

In both the Victorian and NSW data files on injured drivers, it was found that the driver sex and the speed zone at the crash location (in two categories: up to 75 km/h; 80 km/h and above) were both strongly related to injury severity and they both varied substantially across makes and models of crash involved cars. Other factors influencing injury severity were either associated with speed zone or, like driver age above 60, varied so little across models that their overall effect was relatively small compared with driver sex and speed zone.

The differences in driver sex and speed zone between models of cars were taken account by normalisation, a method used by HLDI (1991) following Armitage (1971). This was achieved by calculating the injury severity for drivers within each of the four

categories of sex by speed zone, then combining the four figures using a constant mix of these categories for each model (the mix was, in fact, the average mix for all models combined). Thus, every model was treated as if it had the same mix of male and female drivers and crashes in the high and low speed zones. This was essential to allow comparisons which related to vehicle differences rather than to injury susceptibility and other differences in the crash circumstances.

In the pooled data from the two states, 22,626 (or 98.5%) of the injured drivers had known sex and speed zone. These data formed the basis for calculating the normalised injury severity for each car model.

Injury risk of drivers involved in crashes

Because the Victorian data file did not include all uninjured drivers involved in crashes, it was not used to calculate injury risk for the main results. The file of 73,399 drivers of 1982–1990 model cars crashing in NSW during 1989–1990 included 63,302 who were not recorded as injured on the police report. Some of their crashes may have been reported because other persons had been injured, but most were reported because a vehicle was towed away. This means that the injury rate is essentially an unbiased measure of the driver's injury risk in tow-away crashes. Thus the driver injury rate was used for the second component of the combined rate. The overall injury rate of the NSW drivers was 13.8 per 100 involved drivers with a standard deviation of 0.13.

The influence of crash patterns and driver characteristics on the comparisons of injury rates between models of cars was also investigated. Like injury severity, it was found that the driver sex and the speed zone were each strongly related to injury rate (and each varied substantially across makes and models of crash-involved cars). Other factors had relatively small effects. Accordingly, the driver injury rate for each model car was normalised by driver sex and speed zone in a way similar to the driver injury severity described above.

Among the crash-involved drivers from NSW, 70,916 (or 96.6%) had known sex and speed zone. These data formed the basis for calculating the normalised injury rate for each car model.

Relative injury risk in two-car crashes

As part of the study, there was a need for an independent assessment of driver injury risk derived from Victorian data alone, so that crashworthiness ratings from the two states' data could be calculated separately and compared (Cameron et al. 1992a). However, as noted above, this could not be done

in the same way as for drivers involved in crashes in NSW, due to the incomplete coverage of uninjured drivers in the Victorian data.

The method developed by Folksam Insurance (Gustafsson et al. 1989) for measuring injury risk from data in which essentially only injury crashes are recorded, was applied to the Victorian data. The method was derived by Folksam from Evans' (1986) double-pair comparison method. For two-car crashes, the method calculates the relative risk of injury to drivers of a specific model of car, relative to the injury risk of drivers of other model cars. The method is applicable only to two-car crashes, and this crash type covers about 60% of the drivers recorded in the Victorian data files.

In calculating relative injury risk, driver injury was defined as making a TAC claim. In the Victorian data on crashes prior to 1987, the claim status of drivers of only the 1982–1986 model cars was known (and not that of drivers of earlier model cars colliding with them), so these crashes were ignored in the calculation. In the remaining data there were 8,743 two-car crashes during 1987–1990 in which at least one of the cars was a 1982–1990 model and the TAC claim status of both drivers was known.

For the drivers of cars with identifiable models, the relative injury risk was biased towards high values because the matching of the police report information on an occupant (not necessarily the driver) with a TAC claim was likely to be correlated with driver injury. However this bias was the same for all of the car models being compared.

The relative injury risk was normalized for driver sex differences by calculating separate relative risks for the female and male drivers of each model and then calculating the normalised figure by giving each sex the weighting observed on average for all models. There were too few two-car crashes in speed zones 80 km/h and above to justify normalisation by speed zone as well.

Victorian relative injury risk compared with NSW injury rate

While the NSW driver injury rate may appear to be a simpler and more definitive method of estimating driver injury risk, in practice many jurisdictions record only injury crashes in their mass data and the relative-injury-risk method must be considered instead. For this reason it is important to know whether the relative injury risk is an adequate predictor of the injury rate. It should be noted that the two measures of injury risk are on a different scale: the Victorian relative injury risk is relative to an all-model average (and biased towards high values),

whereas the NSW injury rate is an absolute measure of the risk of injury in tow-away crashes.

The two measures of driver injury risk were compared for those car models for which the coefficient of variation (standard deviation divided by the estimate) of each measure was less than 50% (Fig. 3). There was a highly significant ($p < 0.01$) correlation between the measures ($R = 0.503$).

In principle, the Victorian relative injury risk was not directly comparable with the NSW driver injury rate because, in addition to their different scales, the former pertained to two-car crashes only and the latter was normalised by speed zone as well as driver sex. To address this issue, the relative injury risk was compared with two alternative versions of the NSW driver injury rate, in each case limited to drivers involved in two-car crashes (73% of the total drivers involved), but using two different methods of normalisation. Each comparison displayed a highly significant correlation, with the following correlation coefficients:

1. $R = 0.489$ when the NSW injury rate was normalised for driver sex only (i.e. the closest match to the Victorian method),
2. $R = 0.523$ when the NSW injury rate was normalised for driver sex and speed zone.

The significant correlation between each of the comparisons suggests that both general methods, in fact, measure the risk of injury to drivers involved in crashes. It was also concluded that, while the measures are correlated, the strength of the association between them is such that the relative-injury-risk method is apparently not an adequate substitute for the injury rate (of drivers involved in tow-away crashes) in terms of measuring driver injury risk.

The NSW driver injury rate was used to measure driver injury risk in the principal results of the project described.

Combined rate

The combined rate for each model was calculated by multiplying the driver injury severity (based on Victorian and NSW data) by the driver injury rate (based on NSW data). Thus the combined rate was normalised by driver sex and speed zone, because each of its components was separately normalised. The two components, respectively, measure:

1. the risk of death or hospital admission for drivers who were injured in a crash, and
2. the risk of injury for drivers involved in tow-away crashes.

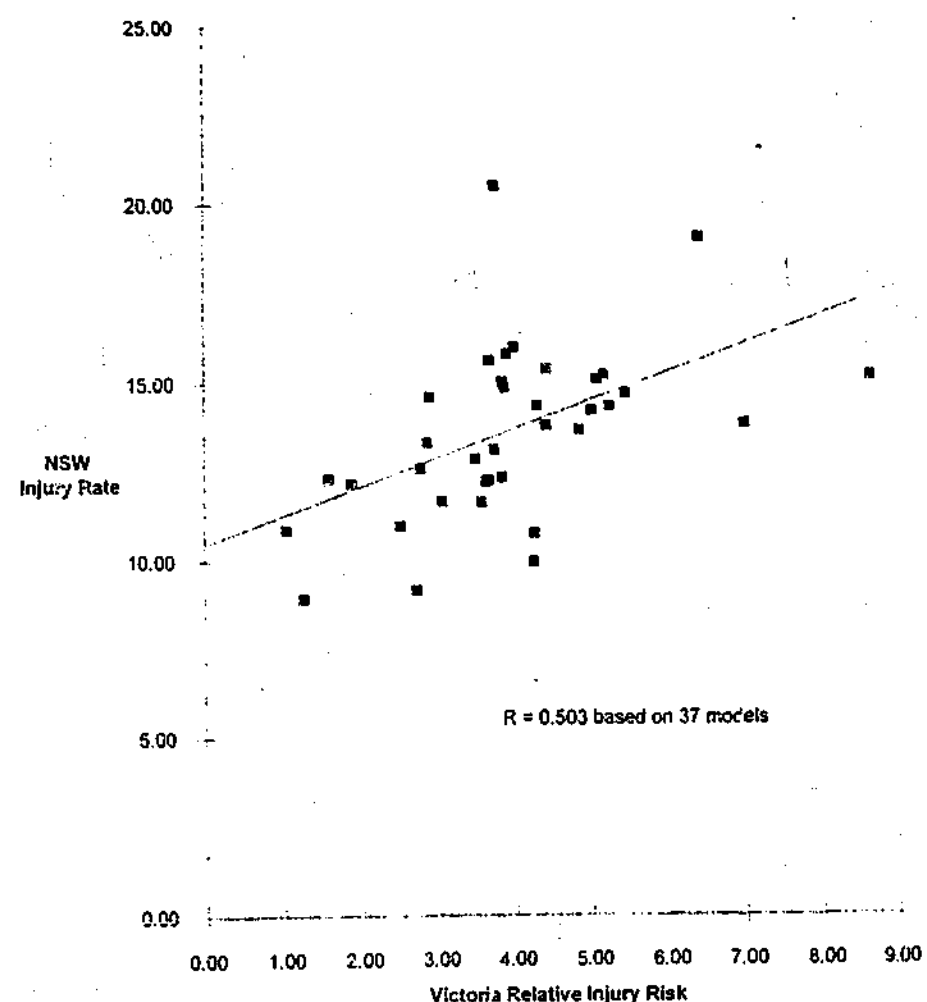


Fig. 3. NSW injury rate versus Victoria relative injury risk.

The combined rate can thus be interpreted as measuring the risk of death or hospital admission for drivers involved in tow-away crashes. The overall combined rate was 3.14 per 100 involved drivers with a standard deviation of 0.05.

CRASHWORTHINESS RATINGS BY MODEL OF VEHICLE

Crashworthiness ratings based on the combined rate defined above were calculated for each model of passenger car and station wagon when there were sufficient data to calculate each normalised component of the combined rate. These calculations made use of data from Victoria and NSW crashes.

Separate ratings were calculated for four-wheel-drive vehicles and passenger vans because the models of these types of vehicles were available only in the NSW data. The raw figures were not directly comparable with those for cars and station wagons, because the overall combined rate (for all types of vehicles) based on NSW data alone was only 2.80 per 100 involved drivers. To facilitate a direct comparison, the combined rates for the four-wheel-drive

and passenger van models were rescaled (by the ratio 3.14/2.80).

Rating scores

The crashworthiness rating score for each model of car, station wagon, four-wheel-drive vehicle, and passenger van is presented in Table 2 (in bold type), sorted in ascending order within market group. These groups reflect the general market categories within which consumers typically make a decision about which model to purchase in Australia. The rating scores for the makes that were not separated into models are presented in Table 3.

The rating score for each pair of models that could be aggregated (because the models are believed to be essentially the same so far as crashworthiness aspects are concerned) is their aggregate rating, i.e. the same value is assigned to each of the models. In no case was there a statistically significant difference between their individual rating scores; this is an indication of the validity of the analysis method, since it produced similar rating scores for vehicles that appear to have the same crashworthiness design.

The standard deviation is a measure of the accuracy of the rating score in estimating the crashworthiness of a specific model car. The standard deviation is a function of the number of involved and injured drivers in the data files. The true risk of driver death or hospital admission in a crash could be expected to be within two standard deviations of the rating score with approximately 95% confidence. Thus each rating score has error limits spanning two standard deviations on each side of the score, also shown in Tables 2 and 3.

It was decided that the rating score should not be reported if it does not provide an accurate estimate of the crashworthiness of the specific model car (or aggregate of two models). The results in Tables 2 and 3 exclude those makes and models where:

1. there were insufficient involved or injured drivers to calculate both of the components of the rating score (i.e. combined rate) for the specific model,
2. the standard deviation of the rating score exceeded 1.5, or
3. the coefficient of variation of the rating score exceeded 70% (this criterion was also necessary because small standard deviations tended to occur for the lower rating scores, but the standard deviations were relatively high in proportionate terms).

The error limits can be used to judge whether the rating score is sufficiently different from the all make/model average (3.14 per 100 involved drivers) for this to be unlikely to be due to chance. An upper limit below the average is indicative of superior crashworthiness, whereas a lower limit above the average suggests inferior crashworthiness. This could occur by chance only about 5% of the time. Of the 62 makes and models for which the rating score could be calculated accurately, eight displayed an upper limit below the average and one displayed a lower limit greater than the average.

DISCUSSION

The rating scores given in Tables 2 and 3 measure the risk of death or hospital admission of drivers of specific makes and models of vehicles involved in crashes. The question is whether the figures represent the crashworthiness of the vehicle alone or whether they also reflect other differences between vehicles related to the crashes in which they were involved or to physiological and other characteristics of the driver.

The analysis has recognised that a number of

factors available in the data could affect the rating scores. The most important of these factors were the driver sex and speed zone, and the variations in these factors between makes and models were taken into account. Other factors that were strongly associated with the high speed zones (e.g. single-vehicle crashes, fixed-object collisions, and rollovers) were also taken into account with speed zone due to the strong association. Driver age, which could be expected to affect injury susceptibility, did not appear to vary sufficiently between models of cars to have a substantial effect on the rating scores.

However, the analysis was not able to take into account other potentially important factors that were not measured in the data, e.g. the crash speed of the vehicle (to the extent that variations in this factor were not reflected in the speed zone at the crash location). The absence of such information from police accident reports and injury compensation claim records is a limitation of this type of data. However, the large number of cases available in these files provide the opportunity to measure accurately the risks of serious injury to crash-involved drivers.

The crashworthiness of vehicles sold and crashing in Victoria and NSW would not be expected to differ between the states. The technical report demonstrated a statistically significant correlation between the rating scores derived for the two states independently (even though they used different methods of analysis for a component of the figures, i.e. driver injury risk) (Cameron et al. 1992a). As the vehicles were the only common factor between the two states, the presence of a correlation suggests that both sets of rating scores are measuring the same thing, i.e. the crashworthiness of the vehicles alone.

It follows that the rating scores based on both states' data combined are also likely to measure crashworthiness alone, and presumably more accurately. The reliability of the rating scores is indicated by the similar scores produced for each pair of models with apparently the same crashworthiness design. Cameron et al. (1992b) reported a strong relationship between the rating scores and the mass of the passenger car models listed in Table 2; thus the ability of the rating method to confirm this expected effect is a further indication of the reliability of the scores.

The accuracy of the rating scores is indicated by the standard deviations and error limits given in Tables 2 and 3. Comparison of the rating scores can be made for each pair of models, within the limits of their individual levels of accuracy. The most reliable comparisons are for those pairs of models where the

Table 2. Crashworthiness ratings of 1982-1990 model vehicles involved in crashes

MAKE	MODEL OF CAR	YEARS OF MANUFACTURE INVOLVED IN CRASHES	RISK OF DEATH OR HOSPITALISATION FOR DRIVERS INVOLVED IN CRASHES			ERROR LIMITS OF THE RATING (2 Std. Devs.)	
			Rating Per 100 Drivers	Standard Deviation	Coeff. of Variation	Lower Limit	Upper Limit
All Make/Model Average			3.14	0.05	1.51%		
LARGE CARS							
Ford	FALCON E SEDAN	1988-90	1.85	0.45	24.16%	0.96	2.74
Holden	COMMODORE VN	1988-90	2.39	0.41	17.18%	1.57	3.21
Toyota	LEXCEN	1988-90					
Mitsubishi	MAGNA	1984-90	2.40	0.25	10.26%	1.90	2.89
Nissan	SKYLINE	1982-90	2.62	0.48	18.35%	1.66	3.58
Ford	FALCON X SEDAN	1982-88	2.65	0.16	5.85%	2.34	2.96
Ford	FALCON X WAGON	1982-88	2.75	0.31	11.40%	2.12	3.37
Ford	FALCON E WAGON	1988-90	2.75	0.93	33.74%	0.89	4.60
Holden	COMMODORE VH-VL	1982-88	2.99	0.16	5.40%	2.67	3.32
Large Car Average			2.69				
MEDIUM CARS							
Holden	APOLLO	1989-90	2.55	0.66	25.74%	1.24	3.87
Toyota	CAMRY (89-90)	1989-90					
Nissan	PINTARA	1986-90	2.71	0.56	20.83%	1.58	3.83
Toyota	CAMRY (83-88)	1983-88	2.97	0.43	14.48%	2.11	3.83
Toyota	CORONA	1982-87	3.16	0.30	9.51%	2.56	3.77
Mitsubishi	SIGMA/SCORPION	1982-87	3.25	0.28	8.47%	2.70	3.80
Nissan	BLUEBIRD	1982-86	3.28	0.31	9.45%	2.66	3.90
Mazda	MAZDA 626	1982-90	3.37	0.25	7.52%	2.86	3.87
Ford	TELSTAR	1983-90					
Holden	CAMIRA	1982-89	3.53	0.25	7.15%	3.03	4.04
Mitsubishi	NIMBUS	1984-90	3.73	1.34	36.04%	1.04	6.42
Nissan	GAZELLE	1983-86	4.11	1.35	32.84%	1.41	6.81
Medium Car Average			3.27				
SMALL CARS							
Mitsubishi	LANCER	1988-90	2.18	0.91	41.92%	0.35	4.00
Toyota	COROLLA	1982-90	3.38	0.23	6.81%	2.92	3.84
Holden	NOVA	1989-90					
Mazda	MAZDA 323	1982-90	3.39	0.46	13.66%	2.46	4.32
Ford	METEOR/LASER	1982-90	3.46	0.20	5.67%	3.07	3.85
Holden	ASTRA	1984-89	3.52	0.28	8.06%	2.95	4.09
Nissan	VECTOR/PULSAR	1982-90					
Mitsubishi	COLT	1982-90	3.53	0.42	11.81%	2.70	4.37
Mitsubishi	CORDIA	1982-88	4.05	0.89	22.06%	2.26	5.84

Honda	CIVIC	1982-90	4.08	0.67	16.40%	2.74	5.42
Holden	GEMINI	1982-88	4.21	0.41	9.77%	3.38	5.03
Hyundai	EXCEL	1986-90	4.27	0.90	21.05%	2.47	6.06
Holden	BARINA	1984-90	4.52	0.73	16.23%	3.05	5.98
Suzuki	SWIFT	1984-90					
Daihatsu	CHARADE	1982-90	4.69	0.82	17.42%	3.06	6.32
Small Car Average			3.57				
SPORTS CARS							
Toyota	CELICA	1982-90	3.03	0.66	21.76%	1.71	4.35
Mazda	RX7	1982-90	3.72	1.01	27.10%	1.70	5.74
Sports Car Average			3.24				
LUXURY CARS							
Volvo	VOLVO 700 SERIES	1984-88	1.31	0.76	58.19%	0.00	2.84
Holden	STATESMAN	1982-86	1.51	0.52	34.39%	0.47	2.54
Ford	FAIRLANE N & LTD D	1988-90	2.05	0.85	41.20%	0.36	3.74
Honda	ACCORD	1982-90	2.58	0.68	26.33%	1.22	3.95
Honda	PRELUDE	1982-90	2.63	0.77	29.41%	1.08	4.17
Toyota	CRESSIDA/CROWN	1982-90	2.70	0.59	21.73%	1.53	3.88
Volvo	VOLVO 200 SERIES	1982-90	2.88	0.75	26.07%	1.38	4.38
Ford	FAIRLANE Z & LTD F	1982-87	3.13	0.50	15.89%	2.14	4.13
Mazda	MAZDA 929	1982-90	3.56	0.72	20.25%	2.12	5.00
Luxury Car Average			2.49				
4 WHEEL DRIVE VEHICLES							
Nissan	PATROL/PATHFINDER	1982-90	1.98	0.75	37.97%	0.48	3.48
Holden	JACKAROO	1983-90	2.08	0.72	34.88%	0.63	3.53
Daihatsu	ROCKY	1984-90	2.15	1.46	68.06%	0.00	5.07
Toyota	4RUNNER/LAND CRUISE	1982-90	2.70	0.60	22.31%	1.50	3.91
Mitsubishi	PAJERO	1983-90	3.03	1.24	41.02%	0.54	5.52
Nissan	NAVARA	1984-90	4.17	1.38	33.13%	1.41	6.94
Suzuki	SIERRA	1982-90	4.85	1.37	28.24%	2.11	7.58
4 Wheel Drive Average			2.63				
PASSENGER VANS							
Toyota	HILUX/HI & LITEACE	1982-90	3.42	0.54	15.79%	2.34	4.49
Ford	COURIER	1982-89	3.90	1.04	26.69%	1.82	5.99
Mitsubishi	MITSUBISHI VANS	1982-90	3.96	0.80	20.27%	2.36	5.57
Toyota	TARAGO	1983-85	4.10	1.40	34.06%	1.31	6.89
Passenger Van Average			3.67				

Table 3. Crashworthiness ratings of 1982-1990 model cars (by make) involved in crashes

Table of Crashworthiness Ratings of 1982-1990 model cars (of make) involved in crashes							
MAKE	MODEL OF CAR	YEARS OF MANUFACTURE INVOLVED IN CRASHES	RISK OF DEATH OR HOSPITALISATION FOR DRIVERS INVOLVED IN CRASHES			ERROR LIMITS OF THE RATING (2 Std. Devs.)	
			Rating Per 100 Drivers	Standard Deviation	Coeff. of Variation	Lower Limit	Upper Limit
CARS BY MAKE ONLY							
SAAB		1982-90	1.77	0.68	38.39%	0.41	3.12
Rover		1982-89	1.78	0.73	40.82%	0.33	3.24
B M W		1982-90	1.93	0.49	25.60%	0.94	2.92
Mercedes		1982-90	2.07	0.52	24.97%	1.04	3.10
Renault		1982-89	2.39	1.07	44.69%	0.25	4.53
Fiat		1982-87	2.70	1.30	48.15%	0.10	5.30
Alfa Romeo		1982-90	2.80	0.74	26.45%	1.32	4.28
Peugeot		1982-90	2.94	1.00	34.06%	0.94	4.94
Subaru		1982-90	3.40	0.41	12.09%	2.57	4.22
Range Rover		1982-89	3.45	1.25	36.36%	0.94	5.96

Note. The results in this table, which represent several different models of different mass, are not directly comparable with those in Table 2, for a single make/model.

error limits do not overlap. The limited reliability of the relative comparisons is obvious when the general width of the error limits is examined.

CONCLUSIONS

1. The rating scores in Tables 2 and 3 apparently measure the crashworthiness of the makes and models of vehicles, free from the effects of driver sex and speed zone differences between models (though the scores may be affected by other factors not collected in the data). The analysis suggests that the different rating scores were predominantly due to vehicle factors alone.
2. Each rating score apparently reliably indicates the crashworthiness of the vehicle, with a level of accuracy indicated by the error limits of the score.
3. The rating scores can be used to make reliable comparisons of the crashworthiness of pairs of models when the error limits do not overlap.
4. The Folksam "relative injury risk" of drivers involved in two-car crashes, while significantly correlated with the driver injury rate in tow-away crashes, is apparently not an adequate substitute for this rate in order to measure driver injury risk.

ASSUMPTIONS AND QUALIFICATIONS

The results and conclusions presented in this paper are based on a number of assumptions and warrant a number of qualifications, which should be noted.

Assumptions

1. TAC claims records and NSW Police accident reports accurately recorded driver injury, hospital admission and death.
2. There was no bias in the merging of TAC claims and Victorian police accident reports related to crash exposure factors and model of car.
3. Crashed-vehicle registration numbers were recorded accurately on police accident reports, and they correctly identified the crashed vehicles in the Victorian and NSW vehicle registers.
4. The adjustment for driver sex and speed zone was sufficient to remove the influences of the main factors available in the data that affected crash severity and injury susceptibility. (Other factors examined had smaller effects or injury severity or injury risk, and/or varied by relatively small amounts between models.)

Qualifications

1. Only driver crash involvements and injuries have been considered. Passengers occupying the same model cars may have had different injury outcomes, which may have suggested a different assessment of the crashworthiness of the cars in terms of protecting all their occupants from injury.
2. The makes of cars that could not be disaggregated into models may include a range of models with a broad span of masses or other factors affecting crashworthiness. The rating score calculated for these makes should not be interpreted as applying to each model of these manufacturers.

3. Some models with the same name through the 1982-1990 years of manufacture varied substantially in their construction and mass. The rating score calculated for these models may give a misleading impression and should be interpreted with caution.
4. Other factors not collected in the data (e.g. crash speed) may differ between the makes and models and may affect the results.

Acknowledgement and disclaimer—This paper is based on a Monash University Accident Research Centre technical report which used information supplied by VIC ROADS, the Transport Accident Commission in Victoria, the Roads and Traffic Authority in New South Wales, the Royal Automobile Club of Victoria, and the National Roads and Motorists' Association. The paper may be reproduced only in its entirety. Any republication of the findings of this paper whether by way of summary or reproduction of the tables or otherwise is prohibited unless prior written consent is obtained from the Monash University Accident Research Centre and any conditions attached to that consent are satisfied.

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Paper No. 8

M.H. Cameron, T. Mach, D. Neiger,
A. Graham, R. Ramsay, M. Pappas and J. Haley,
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Paper No. 8

Vehicle crashworthiness ratings from Victoria and New South Wales crash data

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ABSTRACT

The paper outlines the concepts and terminology of vehicle safety ratings and summarises the published ratings based on mass crash data from the United States, Sweden and Great Britain. It then describes the development of vehicle crashworthiness ratings based on injury compensation claims and police accident reports from Victoria and New South Wales.

Crashworthiness was measured by a combination of injury severity (of injured drivers) and injury risk (of drivers involved in crashes). Injury severity was based on 22,600 drivers injured in crashes in the two States. Injury risk was based on 70,900 drivers involved in crashes in New South Wales where a vehicle was towed away.

The results include crashworthiness ratings for the makes and models crashing in sufficient numbers to measure their crash performance adequately. The ratings were normalised for the driver sex and speed limit at the crash location, the two factors found to be strongly related to injury risk and/or severity, and to vary substantially across makes and models of Australian crash-involved cars. This allows differences in crashworthiness of individual models to be seen, uncontaminated by major crash exposure differences.

The relationship between the rating scores and the mass of individual passenger car models is also examined.

There is a need to inform consumers of the relative safety of cars offered for sale as a way of encouraging manufacturers to improve the crash performance of their products (Social Development Committee 1990; Dowdell 1990a, 1990b). Consumer advice on vehicle safety performance can provide vehicle make/model ratings of two types:

- crashworthiness ratings (measuring the relative safety of vehicles in preventing injury and/or severe injury in crashes);
- crash involvement ratings (measuring attributes which assist or prevent vehicles from being involved in crashes).

Previous research has shown that vehicle design factors play a large role in whether a car occupant is severely injured in a crash (other key factors are the impact speed, point of impact, seating position, restraint use, and the occupant's age and sex). International evidence shows that there are considerable differences between makes and models related to vehicle crashworthiness (Campbell and Reinfurt 1973; Gustafsson *et al.* 1989).

In contrast, vehicle factors have been estimated in several studies (summarised by Johnston 1984) to be the cause of about 10 per cent of crash involvements (road user factors cause about 90 per cent and environmental factors cause about 30 per cent; multiple causes are common). At least some of these vehicle-related causal factors are due to vehicle condition rather than to vehicle design. Thus there is much less potential for finding make/

model differences in vehicle design related to crash involvement.

The development of crashworthiness ratings should be given priority in vehicle safety ratings because of their greater potential to find significant differences in vehicle design between makes and models of cars. Crash involvement ratings are constrained by the relatively small role that vehicle design plays in causing crashes (assuming that its effects can be separated from the many other factors affecting the risk of crashes).

This paper summarises the data and analysis methods used in a Monash University Accident Research Centre project to develop crashworthiness ratings and presents ratings for 1982-90 model vehicles based on crash data from Victoria and New South Wales (NSW) combined. Further details are given in the technical report from the project (Cameron *et al.* 1992) which covers the preparation of the data used, preliminary investigations to determine analysis methods, details of the adjustment procedures, and investigations of the separate results from Victoria and New South Wales. This paper goes on to examine the relationship between the rating scores and the mass of the individual passenger car models.

Terminology

Vehicle safety performance can be measured in many ways. Basically, we are interested in the ability of the vehicle to assist in breaking the road trauma chain shown in Figs 1 and 2. The vehicle

ratings can reflect the various risks shown in the Figures and we are interested in knowing the vehicles for which these risks are relatively high or low.

Crash involvement ratings measure one of the pre-crash risks shown in Fig. 1. Which of these risks is measured depends on the type of exposure used as the denominator of the involvement rate. For mass accident data comparisons of makes/models, it is most common to use numbers of vehicles registered as the denominator and to measure risk (C) by the involvement rate. It would be preferable to measure risk (B), but road use data by make/model are not always available to calculate the appropriate rate. Risk (A) may be even more preferable as it would measure the ability of the vehicle to assist the driver to recover from some critical pre-crash situations (e.g. skidding out of control).

Crashworthiness ratings measure the risks shown in Fig. 2. The starting event is the crash involvement and there are various risks of injury depending on the severity level specified. Injury risk is measured by the injury rate, which is the number of persons killed or injured divided by the number involved in crashes. Since many of our data sources have some minimum level of injury as the entry criterion, it is useful to define injury severity as the risk of severe injury (given that the vehicle occupant is injured). Injury severity can also relate to the risk of death, for occupants who are injured. The number of crash involvements forms the exposure to injury risk and is known as 'crash exposure'. In injury data collections, the event of being injured (to a level providing entry to the data system) represents the 'injury exposure' to the risk of severe injury.

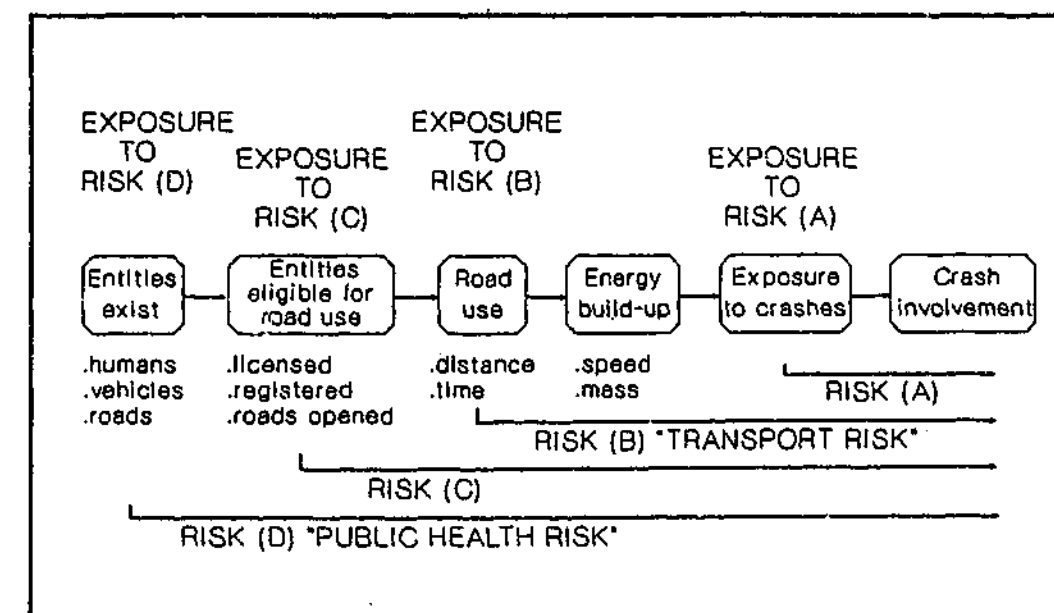


Figure 1

The road trauma chain
(1. pre-crash)

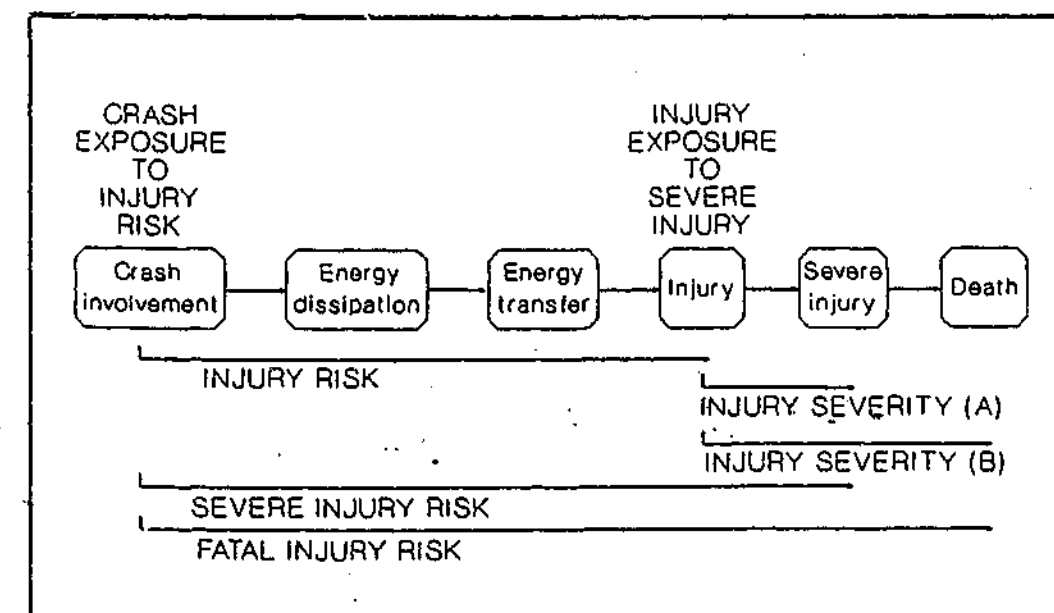


Figure 2

The road trauma chain
(continued) (2. crash and
post-crash)

Vehicle ratings for different makes/models can be developed to measure each of the risks defined above. Whether the observed differences measure differences in vehicle safety characteristics can depend on how the ratings are calculated. Differences in the driver and passenger characteristics, in the crash speed, in environmental factors, and in the crash type, could potentially hide any vehicle design differences. If the aim of vehicle ratings is to measure true differences in vehicle safety, then the analysis needs to take into account these other differences. This can be done by normalisation, ie. making the exposure distribution the same for all makes/models, or by estimating the expected rating of a specific make/model (taking exposure into account) for use as a reference figure against which the actual rating should be compared.

Literature review

Vehicle safety ratings based on mass crash data have been published as consumer advice by the Highway Safety Research Centre (Campbell and Reinfurt 1973), the Insurance Institute for Highway Safety (IIHS 1991), and the Highway Loss Data Institute (HLDI 1991) in the USA; Folksam Insurance (Gustafsson *et al.* 1989; Folksam, undated) in Sweden; and the UK Department of Transport (DOT 1991). These are summarised in Table 1 and detailed reviews are given elsewhere (Cameron 1991, Cameron *et al.* 1992). Vehicle safety ratings based on sources other than mass crash data have also been published, such as those using results from barrier crash tests (Gillis 1991) or from assessments of the presence of a number of occupant protection features in each model (Vehicle Safety Consultants 1989, Consumers' Association 1991).

TABLE 1
Summary of published vehicle safety ratings based on mass crash data

Publishing organisation	Rating measures used in the publication	Dimensions covered by the measure: Crash involvement (CI) Crashworthiness (CW)	Factors used to adjust the ratings before comparison between models	Factors used to categorise the [adjusted] ratings into car groups
Highway Safety Research Centre (USA)	Rate of driver death or serious injury per involvement in crashes with damage exceeding \$100	CW	Impact speed Point of impact on car Accident type	Crash type (single-vehicle versus car-to-car crashes)
Insurance Institute for Highway Safety (USA)	Occupant death rate per 10,000 registered cars	CI and CW	Car wheelbase Driver age Driver sex	Car wheelbase Body style
Highway Loss Data Institute (USA)	1. Occupant injury rate per insured vehicle year (a) any injury (b) injury costs > \$500	1. CI and CW	1. Driver age	Car wheelbase Body style
	2. Vehicle damage payments per insured vehicle year	2. CI	2. Driver age Deductible amount (excess)	
Folksam Insurance (Sweden)	1. Relative risk of driver injury in two-car crashes	1. CW	1. Car weight (contra to weight of 'other' car)	Car weight
	2. Risk of death or permanent disability to occupants who were injured	2. CW	2. Nil	
	3. Combination: 1 by 2	3. CW	3. Car weight (see above)	
Department of Transport (UK)	1. Injury accident involvement rate per 10,000 registered cars	1. CI and CW	Nil	Age of car Performance of car Size of car Owner of car (private or company)
	2. Car occupant casualty rate per 10,000 registered cars	2. CI and CW		
	3. Driver casualty rate per 100 accidents involving impact with another vehicle or other hard object	3. CW		

The five published vehicle safety ratings based on mass crash data have each used different measures of vehicle safety performance. While the general tendency is for the measures to cover crashworthiness aspects (perhaps reflecting a perception that the biggest differences between cars should emerge in this dimension), many of the measures embody the risk of crash involvement as well. Only the vehicle safety ratings published by the Highway Safety Research Centre and by Folksam Insurance can be considered to measure crashworthiness exclusively.

In some cases, the organisations appear to recognise that their measure includes crash involvement risks, and they take steps to correct the differences in risk between models by adjusting or categorising their vehicle ratings by factors such as driver age and sex and the type of car (e.g. body style, age, performance, and whether privately or company owned). It is usually not known or not stated whether these factors adequately account for the differences in crash involvement risk. It is possible that the vehicle safety ratings published by IIHS, HLDI, and the UK DOT continue to reflect differences in driver types and usage patterns as well as differences in crashworthiness between the models of cars which they compare.

Crash data used

Victorian crashes

Detailed injury data have been collected by the Transport Accident Commission (TAC) and its predecessor, the Motor Accidents Board, as part of their responsibilities to provide road transport injury compensation in Victoria. Details of the vehicle occupied were added from the VIC ROADS vehicle registration system.

TAC claims from drivers of cars and station wagons manufactured after 1981 who were involved in crashes in the period from 1983 to 1990, and whose medical expenses exceeded a threshold which was indexed from year to year (\$317 in 1989), were matched with Police accident reports. The Police reports were on all drivers involved in accidents, no matter whether the Police officer recorded the person as injured or uninjured (it was possible for an injury claim to be made in circumstances where injury was not apparent at the time of the accident). Accidents are reported to the Police in Victoria if a person was killed or injured, if property was damaged but names and addresses were not exchanged, or if a possible breach of the Road Traffic Regulations has occurred (Green 1990).

The merged file covered 15,876 drivers of 1982-90 model cars and station wagons crashing during 1983-90. Of these drivers involved in reported crashes, 12,867 (81 per cent) were injured (i.e. TAC claimants), and 3158 (24.5 per cent) of the injured were killed or hospitalised.

New South Wales crashes

The New South Wales Roads and Traffic Authority (RTA) supplied a file covering 75,860 light passenger vehicles involved in Police reported crashes during 1989-90 which resulted in death or injury or a vehicle being towed away. The National Roads and Motorists' Association (NRMA) had added the make, model and year of manufacture to these vehicles after matching with the New South Wales vehicle register via registration number. The file supplied covered vehicles manufactured during the period 1982-90. The file not only covered cars and station wagons, but also covered four-wheel drive vehicles, passenger vans, light trucks and other commercial vehicles (these could be identified by their model).

The vehicle file (which also contained driver age and sex) was merged with files supplied by RTA covering details of the person casualties (killed and injured persons) and the reported crashes for the same years. Each vehicle/driver matched uniquely with the corresponding crash information, but only injured drivers could match with persons in the casualty file. A driver who did not match was considered to be uninjured. When the unoccupied vehicles were excluded, the injury details of 73,399 drivers involved in crashes were available. According to the data supplied about these drivers, 10,097 (13.8 per cent) were injured and 2045 (20.3 per cent) of the injured were killed or hospitalised.

The presence of uninjured drivers in the merged data file meant that it was suitable for measuring the risk of driver injury (in cars sufficiently damaged to require towing). This contrasted with the Victorian data file, which could not be used to measure injury risk directly because not all uninjured drivers were included.

Derivation of models of cars

The Victorian vehicle register provided the make and year of manufacture of the crashed vehicle but not the model. Model was initially derived for 1982-88 model cars using logic developed and supplied by the Royal Automobile Club of Victoria (RACV) based on the make, year and power-mass

units. Power-mass units (PMU) are the sum of RAC horsepower units (PU) and the vehicle mass in units of 50 kg (MU). Refined logic was developed by MUARC based on make, year, PMU, PU, MU and bodytype, and extended to cover 1989-90 models. Both logics were applied to the combined Victorian data to derive passenger car models for the model years 1982-90.

The NRMA had decoded the chassis number (obtained from the New South Wales vehicle register) to determine the models of light passenger vehicles which crashed in New South Wales. The decoding identified some light truck and other commercial models which were not considered further. In addition, because the Victorian data was limited to cars and station wagons, the four-wheel drive and passenger van models in the New South Wales data were analysed separately.

All but 8 per cent of the NSW vehicles had a model identified; in these cases the make of the vehicle was used as the model in both States' data. Comparison between makes which contain more than one model should be made with care and may not be legitimate, because some manufacturers have a broad span of masses in their model range.

Expert advice was obtained regarding the models, years and bodytypes which needed to be kept separate, or could be aggregated, because of dissimilarity/similarity of crashworthiness aspects (this advice was used in the presentation of results). However some models have undergone substantial change during 1982-90 and their aggregate rating for all the year models released in this period needs to be interpreted with caution.

Measures to rate crashworthiness

Crashworthiness ratings measure the risk of serious injury to the drivers of each specific model car when it is involved in a crash. This risk can be measured in two components:

- risk of injury for drivers involved in crashes ('injury risk'), and
- risk of serious injury for drivers who are injured ('injury severity').

Following the method used by Folksam Insurance (Gustafsson *et al.* 1989), it is then possible to calculate an overall crashworthiness rating, defined as:

Combined Rate = Injury Severity x Injury Risk

The combined rate defined in this way can be interpreted as measuring the risk of serious injury to drivers involved in a crash. Serious injury can be variously defined, but in this study, serious injury was taken as death, or injury requiring hospital admission.

Aldman *et al.* (1984) recommended that 'when individual car models are studied the possible influence of the age of the driver, speed limit at the scene of the accident, belt usage rate, weight of the struck car, (and) impact direction... must be taken into account'. Major differences in crash patterns between models of cars have the potential to hide any effects of the vehicle design on injury risk or injury severity. It is necessary to take account of these differences if valid comparisons of the crashworthiness of cars are to be made.

The variables in the data files which described the crash patterns included speed zone, crash type, point of impact on the car, car mass, and restraint use. The driver age and sex are related to injury susceptibility. The first question was whether any of these variables had a significant relationship with the injury severity and injury risk measures selected for rating crashworthiness. The second question was whether the significant variable differed enough between makes/models for this to make a substantial difference to the rating scores. These questions were investigated separately for each measure (Cameron *et al.* 1992) and the findings are summarised in the following sections.

Injury severity of injured drivers

The data on injured drivers from Victoria and New South Wales was pooled to measure injury severity by model of car, i.e. to provide the first component of the combined rate. In the pooled data, 5193 of the 22,964 injured drivers were killed or hospitalised, representing an injury severity of 22.6 per 100 injured drivers. This was considered sufficiently similar to the separate injury severities from Victoria (24.5 per cent) and New South Wales (20.3 per cent) to justify combining the two States' data. The standard deviation of the pooled injury severity (0.27) was substantially lower than that for either State alone. Thus the pooled data has greater sensitivity and reliability than the separate data sets.

In both the Victorian and New South Wales data files on injured drivers, it was found that driver sex and the speed zone at the crash location in two categories: up to 75 km/h; 80 km/h and above were

both strongly related to injury severity and they both varied substantially across makes and models of crash involved cars. Other factors influencing injury severity were either associated with speed zone or, like driver age above 60, varied so little across models that their overall effect was relatively small compared with driver sex and speed zone.

The differences in driver sex and speed zone between models of cars were taken into account by normalisation, a method used by HLDI (1991) following Armitage (1971). This was achieved by calculating the injury severity for drivers within each of the four categories of sex by speed zone, then combining the four figures using a constant mix of these categories for each model (the mix was in fact the average mix for all models combined). Thus every model was treated as if it had the same mix of male and female drivers and crashes in the high and low speed zones. This was essential to allow comparisons which related to vehicle differences rather than to injury susceptibility and other differences in the crash circumstances.

In the pooled data from the two States, 22,626 (or 98.5 per cent) of the injured drivers had known sex and speed zone. These data were the basis for calculating the normalised injury severity for each car model.

Injury risk of drivers involved in crashes

Because the Victorian data file did not include all uninjured drivers involved in crashes, it was not used to calculate injury risk for the main results. The file of 73,399 drivers of 1982-90 model cars crashing in New South Wales during 1989-90 included 63,302 who were not recorded as injured on the Police report. Some of their crashes may have been reported due to other persons being injured, but most were reported because a vehicle was towed away. This means that the injury rate is essentially an unbiased measure of the driver's injury risk in tow-away crashes. Thus the driver injury rate was used for the second component of the combined rate. The overall injury rate of the NSW drivers was 13.8 per 100 involved drivers with a standard deviation of 0.13.

The influence of crash patterns and driver characteristics on the comparisons of injury rates between models of cars was also investigated. Like injury severity, it was found that the driver sex and the speed zone were each strongly related to injury rate (and each varied substantially across makes and models of crash involved cars). Other factors had relatively small effect. Accordingly,

the driver injury rate for each model car was normalised by driver sex and speed zone in a similar way as the driver injury severity described above.

Among the crash-involved drivers from New South Wales, 70,916 (or 96.6 per cent) had known sex and speed zone. These data were the basis for calculating the normalised injury rate for each car model.

Combined rate

The combined rate for each model was calculated by multiplying the driver injury severity (based on Victorian and New South Wales data) by the driver injury rate (based on New South Wales data). Thus the combined rate was normalised by driver sex and speed zone, because each of its components was separately normalised. The two components, respectively, measure:

- the risk of death or hospitalisation for drivers who were injured in a crash, and
- the risk of injury for drivers involved in tow-away crashes.

The combined rate can thus be interpreted as measuring the risk of death or hospitalisation for drivers involved in tow-away crashes. The overall combined rate was 3.14 per 100 involved drivers with a standard deviation of 0.05.

Crashworthiness ratings by model of vehicle

Crashworthiness ratings based on the combined rate defined above were calculated for each model of passenger car and station wagon, where there was sufficient data to calculate each normalised component of the combined rate (this required at least one injured and one involved driver in each of the four categories of sex by speed zone, for the specific model of car). These calculations made use of data from Victoria and New South Wales crashes.

Separate ratings were calculated for four-wheel drive vehicles and passenger vans because the models of these types of vehicles were available only in the New South Wales data. The raw figures were not directly comparable with those for cars and station wagons, because the overall combined rate (for all types of vehicle) based on the New South Wales data alone was only 2.80 per 100

involved drivers. To facilitate a direct comparison, the combined rates for the four-wheel-drive and passenger van models were rescaled (by the ratio 3.14/2.80).

Rating scores

The crashworthiness rating score for each model of car, station wagon, four-wheel drive vehicle and passenger van is presented in *Table II* (in bold type), sorted in ascending order within market group. These groups reflect the general market categories within which consumers typically make a decision about which model to purchase in Australia. The rating scores for the makes which were not separated into models are presented in *Table III* (the Range Rover was recorded as a make in the Victorian and New South Wales vehicle registers, however, a reviewer has pointed out that only one four-wheel drive model has been produced under that 'make' during the period).

The rating score for each pair of models which could be aggregated (because the models are believed to be essentially the same so far as crashworthiness aspects are concerned) is their aggregate rating, i.e. the same value is assigned to each of the models (in no case was there a statistically significant difference between their individual rating scores).

The standard deviation is a measure of the reliability of the rating score in estimating the crashworthiness of a specific model car. The standard deviation is a function of the number of involved and injured drivers in the data files. The true risk of driver death or hospitalisation in a crash could be expected to be within two standard deviations of the rating score with approximately 95 per cent confidence. Thus each rating score has error limits spanning two standard deviations on each side of the score, also shown in *Tables II* and *III*.

It was decided that the rating score should not be reported if it does not provide a reliable estimate of the crashworthiness of the specific model car (or aggregate of two models). The results in *Tables II* and *III* exclude those makes and models where:

- there were insufficient involved or injured drivers to calculate both of the components of the rating score (i.e. combined rate) for the specific model,
- the standard deviation of the rating score exceeded 1.5, or

- the coefficient of variation of the rating score exceeded 70 per cent (this criterion was also necessary because small standard deviations tended to occur for the lower rating scores, but the standard deviations were relatively high in proportionate terms).

The error limits can be used to judge whether an individual rating score is sufficiently different from the all make/model average (3.14 per 100 involved drivers) for this to be more than chance variation. An upper limit below the average is indicative of superior crashworthiness, whereas a lower limit above the average suggests inferior crashworthiness. This could occur by chance only about 5 per cent of the time. Of the 62 makes and models for which the rating score could be calculated reliably, eight displayed an upper limit below the average. These were (in order of lowest ratings score):

Models

Volvo 700 Series (1984-88 years of manufacture)

Holden Statesman (1982-86)

Ford Falcon E-series Sedan (1988-90)

Mitsubishi Magna (1984-90)

Ford Falcon X-series Sedan (1982-88)

Makes (representing several different models of different mass)

SAAB (1982-90)

BMW (1982-90)

Mercedes (1982-90)

The Holden Gemini (1982-88 years of manufacture) displayed a lower limit greater than the average.

Relationship with car mass

Krafft *et al.* (1991) found a statistically significant inverse relationship between the Folksam combined rate and the weight of 47 car models ($R = -0.50$). However, they also found that the relationship is not homogeneous, and that a number of small car models had very low combined rates, and that some large car models displayed rates similar to small cars. This suggests that car weight is an important factor in determining crashworthiness, but that there is also a residual component which may be explainable by other factors such as vehicle design.

TABLE II
Crashworthiness ratings of 1982-90 model vehicles involved in crashes

Make	Model of car	Years of manufacture involved in crashes	Risk of death or hospitalisation for drivers involved in crashes			Error limits of the rating (2 Std Devs)	
			Rating per 100 drivers	Standard deviation	Coeff. of variation (%)	Lower limit	Upper limit
All Make/Model Average			3.14	0.05	1.51		
LARGE CARS							
Ford	FALCON E SEDAN	1988-90	1.85	0.45	24.16	0.96	2.74
Holden	COMMODORE VN	1988-90	2.39	0.41	17.18	1.57	3.21
Toyota	LEXCEN	1988-90					
Mitsubishi	MAGNA	1984-90	2.40	0.25	10.26	1.90	2.89
Nissan	SKYLINE	1982-90	2.62	0.48	18.35	1.66	3.58
Ford	FALCON X SEDAN	1982-88	2.65	0.16	5.85	2.34	2.96
Ford	FALCON X WAGON	1982-88	2.75	0.31	11.40	2.12	3.37
Ford	FALCON E WAGON	1988-90	2.75	0.93	33.74	0.89	4.60
Holden	COMMODORE VH-VL	1982-88	2.99	0.16	5.40	2.67	3.32
Large Car Average			2.69				
MEDIUM CARS							
Holden	APOLLO	1989-90	2.55	0.66	25.74	1.24	3.87
Toyota	CAMRY (89-90)	1989-90					
Nissan	PINTARA	1986-90	2.71	0.56	20.83	1.58	3.83
Toyota	CAMRY (83-88)	1983-88	2.97	0.43	14.48	2.11	3.83
Toyota	CORONA	1982-87	3.16	0.30	9.51	2.56	3.77
Mitsubishi	SIGMA/SCORPION	1982-87	3.25	0.28	8.47	2.70	3.80
Nissan	BLUEBIRD	1982-86	3.28	0.31	9.45	2.66	3.90
Mazda	MAZDA 626	1982-90	3.37	0.25	7.52	2.86	3.87
Ford	TELSTAR	1983-90					
Holden	CAMIRA	1982-89	3.53	0.25	7.15	3.03	4.04
Mitsubishi	NIMBUS	1984-90	3.73	1.34	36.04	1.04	6.42
Nissan	GAZELLE	1983-86	4.11	1.35	32.84	1.41	6.81
Medium Car Average			3.27				
SMALL CARS							
Mitsubishi	LANCER	1988-90	2.18	0.91	41.92	0.35	4.00
Toyota	COROLLA	1982-90	3.38	0.23	6.81	2.92	3.84
Holden	NOVA	1989-90					
Mazda	MAZDA 323	1982-90	3.39	0.43	13.66	2.46	4.32
Ford	METEOR/LASER	1982-90	3.46	0.20	5.67	3.07	3.85
Holden	ASTRA	1984-89	3.52	0.28	8.06	2.95	4.09
Nissan	VECTOR/PULSAR	1982-90					
Mitsubishi	COLT	1982-90	3.53	0.42	11.81	2.70	4.37
Mitsubishi	CORDIA	1982-88	4.05	0.89	22.06	2.26	5.84
Honda	CIVIC	1982-90	4.08	0.67	16.40	2.74	5.42
Holden	GEMINI	1982-88	4.21	0.41	9.77	3.38	5.03
Hyundai	EXCEL	1986-90	4.27	0.90	21.05	2.47	6.06
Holden	BARINA	1984-90	4.52	0.73	16.23	3.05	5.98
Suzuki	SWIFT	1984-90					
Daihatsu	CHARADE	1982-90	4.69	0.82	17.42	3.06	6.32
Small Car Average			3.57				
SPORTS CARS							
Toyota	CELICA	1982-90	3.03	0.66	21.76	1.71	4.35
Mazda	RX7	1982-90	3.72	1.01	27.10	1.70	5.74
Sports Car Average			3.24				
LUXURY CARS							
Volvo	VOLVO 700 SERIES	1984-88	1.31	0.76	58.19	0.00	2.84
Holden	STATESMAN	1982-86	1.51	0.52	34.39	0.47	2.54
Ford	FAIRLANE N & LTD D	1988-90	2.05	0.85	41.20	0.36	3.74
Honda	ACCORD	1982-90	2.58	0.68	26.33	1.22	3.95
Honda	PRELUDE	1982-90	2.63	0.77	29.41	1.08	4.17
Toyota	CRESSIDA/CROWN	1982-90	2.70	0.59	21.73	1.53	3.88
Volvo	VOLVO 200 SERIES	1982-90	2.88	0.75	26.07	1.38	4.38
Ford	FAIRLANE Z & LTD F	1982-87	3.13	0.50	15.89	2.14	4.13
Mazda	MAZDA 929	1982-90	3.56	0.72	20.25	2.12	5.00
Luxury Car Average			2.49				

TABLE II (Continued)
Crashworthiness ratings of 1982-90 model vehicles involved in crashes

Make	Model of car	Years of manufacture involved in crashes	Risk of death or hospitalisation for drivers involved in crashes			Error limits of the rating (2 Std Devs)	
			Rating per 100 drivers	Standard deviation	Coeff. of variation (%)	Lower limit	Upper limit
4-WHEEL-DRIVE VEHICLES							
Nissan	PATROL/PATHFINDER	1982-90	1.98	0.75	37.97	0.48	3.48
Holden	JACKAROO	1983-90	2.08	0.72	34.88	0.63	3.53
Daihatsu	ROCKY	1984-90	2.15	1.46	68.06	0.00	5.07
Toyota	4RUNNER/LAND CRUISER	1982-90	2.70	0.60	22.31	1.50	3.91
Mitsubishi	PAJERO	1983-90	3.03	1.24	41.02	0.54	5.52
Nissan	NAVARA	1984-90	4.17	1.38	33.13	1.41	6.94
Suzuki	SIERRA	1982-90	4.85	1.37	28.24	2.11	7.58
4 Wheel Drive Average			2.63				
PASSENGER VANS							
Toyota	HILUX/HI LITEACE	1982-90	3.42	0.54	15.79	2.34	4.49
Ford	COURIER	1982-89	3.90	1.04	26.69	1.82	5.99
Mitsubishi	MITSUBISHI VANS	1982-90	3.96	0.80	20.27	2.36	5.57
Toyota	TARAGO	1983-85	4.10	1.40	34.06	1.31	6.89
Passenger Van Average			3.67				

TABLE III
Crashworthiness ratings of 1982-90 model cars (by make) involved in crashes

Make	Model of car	Years of manufacture involved in crashes	Risk of death or hospitalisation for drivers involved in crashes			Error limits of the rating (2 Std Devs)	
			Rating per 100 drivers	Standard deviation	Coeff. of variation (%)	Lower limit	Upper limit
CARS BY MAKE ONLY							
SAAB		1982-90	1.77	0.68	38.39	0.41	3.12
Rover		1982-89	1.78	0.73	40.82	0.33	3.24
BMW		1982-90	1.93	0.49	25.60	0.94	2.92
Mercedes		1982-90	2.07	0.52	24.97	1.04	3.10
Renault		1982-89	2.29	1.07	44.69	0.25	4.53
Fiat		1982-89	2.70	1.30	48.15	0.10	5.30
Alfa Romeo		1982-90	2.80	0.74	26.45	1.32	4.28
Peugeot		1982-90	2.94	1.00	34.06	0.94	4.94
Subaru		1982-90	3.40	0.41	12.09	2.57	4.22
Range Rover		1982-89	3.45	1.25	36.36	0.94	5.96

Note: The results in this table, which represent several different models of different mass, are not directly comparable with those in Table II for a single make/model.

The ratings scores of the 41 models of cars and station wagons listed in Table II were compared with their average masses obtained from the VIC ROADS vehicle registration system for those vehicles crashing in Victoria (Fig. 3). Car makes which could not be disaggregated into specific models (Table III) were

ignored because their average mass was not considered sufficiently representative. The correlation was very highly significant ($p < 0.001$; $R = -0.74$). A linear regression fitted to the data suggested that the rating score falls by 6.0 per cent per 100 kg increase in the mass of the car.

The rating score is a combination of the injury risk and the injury severity of the drivers of each car model involved in crashes (see above). The correlation between the driver injury risk and car mass (Fig. 4) was very highly significant ($p < 0.001$; $R = -0.75$). The relationship between these two variables appeared almost linear with injury risk falling by 4.7 per cent per 100 kg increase in car mass. This apparent effect of car mass in crashes in general should be compared with that observed in multi-vehicle crashes. When analysing Victorian crashes alone using Folksam's method to estimate the driver relative risk of injury in two-car crashes (following Gustafsson *et al.* 1989), there was a 9.1 per cent decrease in injury risk per 100 kg increase in mass (Cameron *et al.* 1992). An effect of this magnitude is confirmed from results given by Gustafsson *et al.* (10.2 per cent decrease per 100 kg increase, from their Table 7).

However, when the driver injury severity of the car models listed in Table II was considered, the correlation with car mass (Fig. 5) was only weakly statistically significant ($p < 0.1$; $R = -0.29$) and injury severity fell by a relatively small 2.3 per cent per 100 kg increase in mass. The large amount of residual variation in injury severity after the regression against car mass was fitted suggested

that other factors (e.g. vehicle design) operate to explain variations in injury severity to a greater extent than they potentially explain variations in injury risk.

Discussion

The ratings scores given in Tables II and III measure the risk of death or hospitalisation of drivers of specific makes and models of vehicles involved in crashes. The question is whether the figures represent the crashworthiness of the vehicle alone, or whether they also reflect other differences between vehicles related to the crashes in which they were involved or to physiological and other characteristics of the driver.

The analysis has recognised that a number of factors available in the data could affect the rating scores. The most important of these factors were the driver sex and speed zone, and the variations in these factors between makes and models were taken into account. Other factors which were strongly associated with the high speed zones (e.g. single-vehicle crashes, fixed object collisions, and rollovers) were also taken into account with speed

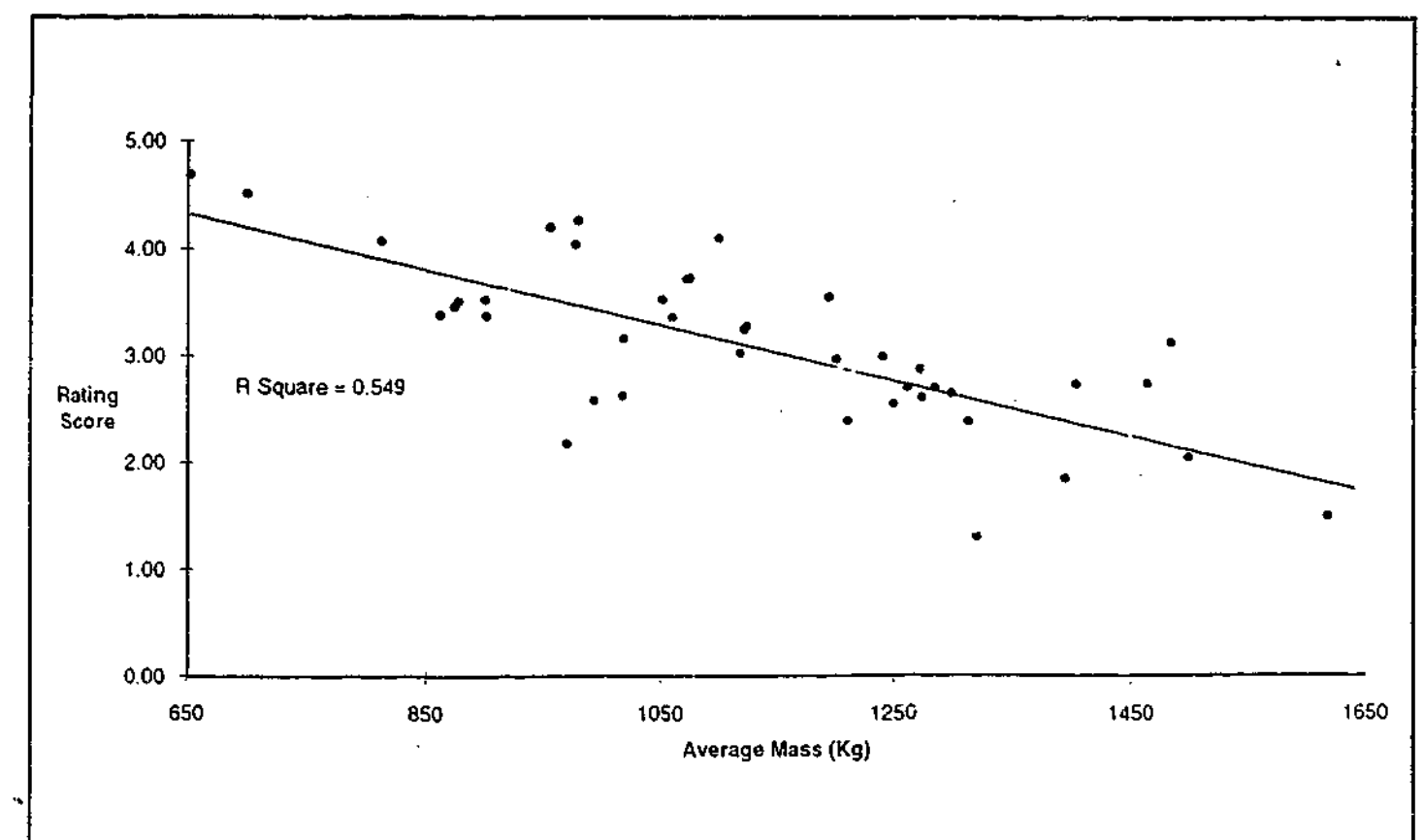


Figure 3

Crashworthiness rating score versus average mass of car model

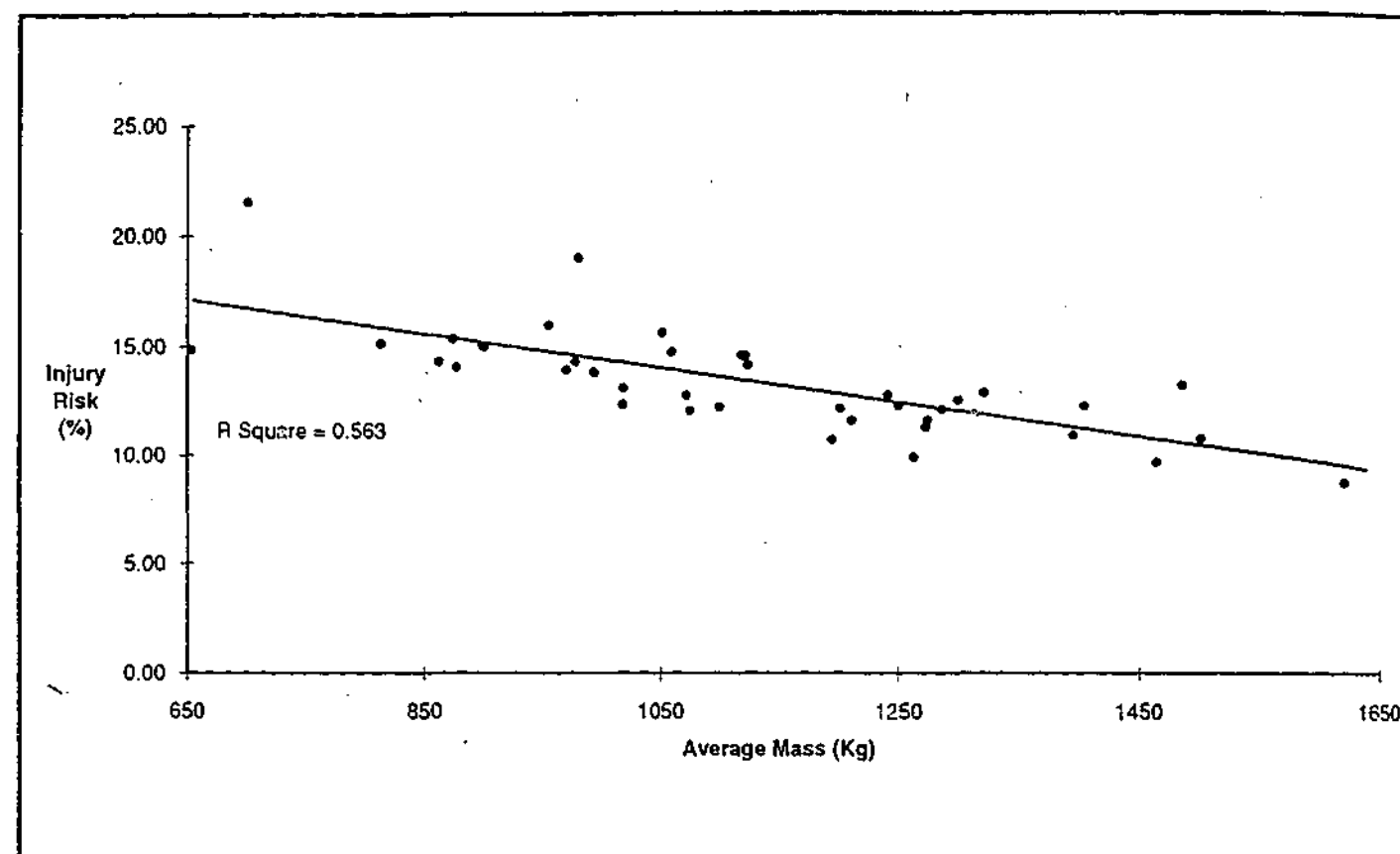


Figure 4

Injury risk versus average mass of car model

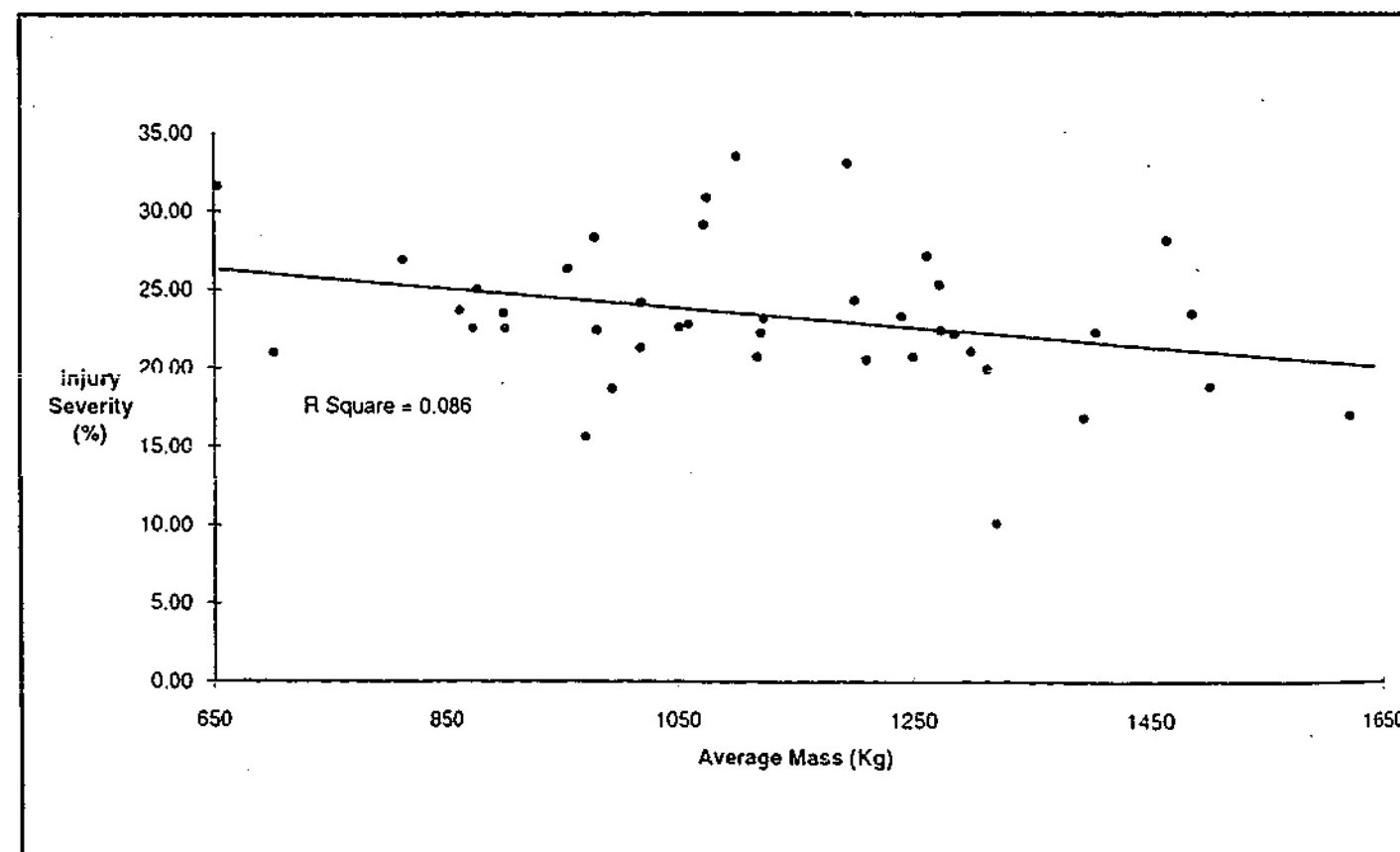


Figure 5

Injury severity versus average mass of car model

zone due to the strong association. Driver age, which could be expected to affect injury susceptibility, did not appear to vary sufficiently between models of cars to have a substantial effect on the rating scores.

However, the analysis was not able to take into account other potentially important factors which were not measured in the data, e.g. the crash speed of the vehicle (to the extent that variations in this factor were not reflected in the speed zone at the crash location). The absence of such information from Police accident reports and injury compensation claim records is a limitation of this type of data. However, the large number of cases available in these files provide the opportunity to measure the risks of serious injury to crash involved drivers reliably.

The crashworthiness of vehicles sold and crashing in Victoria and New South Wales would not be expected to differ between the States. The technical report demonstrated a statistically significant correlation between the rating scores derived for the two States independently (even though they used different methods of analysis for a component of the figures, i.e. driver injury risk) (Cameron *et al.* 1992). As the vehicles were the only common factor between the two States, the presence of a correlation suggests that both sets of rating scores are measuring the same thing, i.e. the crashworthiness of the vehicles alone.

It follows that the rating scores based on both States' data combined are also likely to measure crashworthiness alone, and presumably more reliably. The reliability of the current rating scores is indicated by the standard deviations and error limits given in *Tables II* and *III*.

Comparison of the rating scores can be made for each pair of models, within the limits of their individual levels of reliability. The most reliable comparisons are for those pairs of models where the error limits do not overlap. The limited reliability of the relative comparisons is obvious when the general width of the error limits is examined.

There is apparently a strong relationship between the ratings scores and the mass of the passenger car models, with the lowest risks of driver death or hospitalisation occurring in the cars of greatest mass. This apparent effect of car mass was strongest in its effect on the risk of driver injury (particularly in multi-vehicle crashes) compared with the effect on the injury severity of injured drivers.

After the apparent effect of car mass is taken into account, the residual variation in driver injury severity (and, to a lesser extent, in driver injury risk) may be due to vehicle design factors. However, Koch *et al.* (1991) suggested that, at least in the car models crashing in Sweden which they analysed, there is a correlation between cars with greater mass and those with superior designs which reduce the risk of driver injury. A correlation of the same type may also exist among Australian cars. This would result in the apparent effect of car mass on driver injury risk being overestimated in this paper, leaving a greater residual which is potentially explainable by vehicle design factors.

Conclusions

1. The rating scores in *Tables II* and *III* measure the crashworthiness of the makes and models of vehicles, free from the effects of driver sex and speed zone differences between models. The analysis suggests that the different rating scores were predominantly due to vehicle factors alone.
2. Each rating score is reliable in indicating the crashworthiness of the vehicle specified to the extent indicated by the error limits of the score.
3. The rating scores can be used to make reliable comparisons of the crashworthiness of pairs of models when the error limits do not overlap.
4. In general the rating scores of the passenger car models are inversely related to their mass. This may, in part, be due to superior crashworthiness design being correlated with greater mass among cars on Australian roads.

Assumptions and qualifications

The results and conclusions presented in this paper are based on a number of assumptions and warrant a number of qualifications which should be noted.

Assumptions

- TAC claims records and New South Wales Police accident reports accurately recorded driver injury, hospitalisation and death.
- There was no bias in the merging of TAC claims and Victorian Police accident reports related to crash exposure factors and model of car.

- Crashed vehicle registration numbers were recorded accurately on Police accident reports and that they correctly identified the crashed vehicles in the Victorian and New South Wales vehicle registers.
- The adjustment for driver sex and speed zone was sufficient to remove the influences of the main factors available in the data which affected crash severity and injury susceptibility. (Other factors examined had smaller effects on injury severity or injury risk, and/or varied by relatively small amounts between models.)

Qualifications

- Only driver crash involvements and injuries have been considered. Passengers occupying the same model cars may have had different injury outcomes which may have suggested a different assessment of the crashworthiness of the cars in terms of protecting all their occupants from injury.
- The makes of cars which could not be disaggregated into models may include a range of models with a broad span of masses or other factors affecting crashworthiness. The rating score calculated for these makes should not be interpreted as applying to each model of these manufacturers.
- Some models with the same name through the 1982-90 years of manufacture varied substantially in their construction and mass. The rating score calculated for these models may give a misleading impression and should be interpreted with caution.
- Other factors not collected in the data (e.g. crash speed) may differ between the makes and models and may affect the results.

The next steps

The technical report from the project (Cameron *et al.* 1992) recommended that the rating scores should be published widely as advice to consumers. A brochure titled 'How does your car rate in a crash?' was jointly produced and distributed by VIC ROADS, RACV, NSW Roads and Traffic Authority, NRMA and TAC Insurance during 1992. The brochure illustrated the rating scores in a graphical form which allowed the error limits to be shown, and listed the assumptions and qualifications on

which the scores were based. A summary of the technical report was also prepared to support the brochure.

The brochure was launched by State Ministers responsible for road safety in Victoria and New South Wales during April 1992. At the same time, the May issue of *Royalauto* journal published the rating scores for those vehicles included in Table II. These scores were widely republished in daily newspapers throughout Australia. The apparent absence of scores for the makes not included in the Table II led to adverse comments from some manufacturers and increased the media exposure of the results. (The scores in Table III for the makes which were not separated in models were portrayed in a separate chart in the brochure.)

Some manufacturers and importers have responded to the rating scores in the advertising of their products. Volvo importers were quick to point out that 'a Volvo has just topped an independent study on safety by Monash University' in advertisements for their 940 model (which has replaced the 700 Series models rated in the project). Ford Australia in their advertisements for the EB Falcon have noted that 'recently released government statistics reveal Falcon's superiority in terms of crash safety. In an accident, you are safer in a Falcon sedan than in any other Australian-made large car on the market today'. SAAB importers, reacting to the absence of Australian rating scores for their specific models, have pointed out in their advertisements that the SAAB 9000 displayed the best ratings in the systems published by Folksam (undated) and Great Britain's Consumers' Association (1991). These reactions suggest that the car industry has recognised that at least some consumers are taking crashworthiness rating scores into consideration in choosing a car to buy.

The technical report also recommended that an on-going system to provide crashworthiness ratings should be established. This is clearly necessary to provide updated rating scores for new model series as they appear on the Australian market, as well as more reliable ratings for existing models. Release of new scores on a regular basis is necessary to attract media and consumer attention, and to encourage the car industry to react in their advertising and by improving their products. The Monash University Accident Research Centre (in collaboration with its partners in the project) plans to update the rating scores by including crash data from Victoria and New South Wales during 1991. The new data should also allow additional makes to have rating scores for their individual models.

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Paper No. 9

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"Evaluation of the random breath testing
initiative in Victoria 1989-1991:
Multivariate time series approach".

Report No. 38,
Monash University Accident Research Centre,
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Paper No. 9

**EVALUATION OF THE RANDOM BREATH
TESTING INITIATIVE IN VICTORIA
1989-1991**

MULTIVARIATE TIME SERIES APPROACH

by

Max Cameron
Antonieta Cavallo
Glen Sullivan

Monash University
Accident Research Centre

September 1992

Report No. 38

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Author(s)	Type of Report & Period Covered
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Transport Accident Commission
222 Exhibition Street
Melbourne VIC 3000

Abstract:

A quasi-experimental time series evaluation of the effect a random breath testing (RBT) initiative, introduced late in 1989 in Victoria, on crashes during 1990 was undertaken (Report no. 37). This report presents an additional evaluation study which uses an alternative method of estimating the effect of the initiative on crashes in 1990, and also attempts to assess the effect during 1991.

The RBT initiative involved a substantially different method of RBT enforcement compared with past operations, with bus-based RBT stations replacing car-based stations and a multi-million dollar, Statewide anti-drink driving publicity campaign through all mass media.

Multivariate time series modelling of high alcohol hour serious casualty and fatal crashes was undertaken to estimate the change relating to the RBT initiative during 1990 and 1991, taking into account changes in unemployment rate and changes in the same crash types in NSW. A form of time series modelling known as ARIMA Intervention Analysis was used to estimate effects during 1990, whilst a multiple regression approach was used to estimate effects during 1991.

The findings of the present study indicate that the RBT initiative (in its entirety) resulted in an 18% reduction in high alcohol hour serious casualty crashes and a 24% reduction in high alcohol hour fatal crashes in metropolitan Melbourne in 1990, but no statistically significant effect during 1991. In rural Victoria, high alcohol hour serious casualty crashes decreased by 13% in 1990 and by 24% in 1991, whilst there were no statistically significant effects in high alcohol hour fatal crashes in rural Victoria in 1990 nor 1991. The conclusiveness of these findings depends on the adequacy of unemployment rate as an indicator of changes in travel during high alcohol hours, the appropriateness of NSW as a comparison area to take into account the effects of "other" factors (other than unemployment rate) influential in Victoria during the intervention period, and the assumption of minimal effects of concurrent speed camera operations in Victoria during high alcohol hours.

Key Words:
(IRRD except where marked*)
evaluation (assessment), collision,
road trauma, publicity, enforcement,
Blood Alcohol Content, alcohol*,
drink driving*, Random Breath Testing*

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EXECUTIVE SUMMARY

INTRODUCTION

The Monash University Accident Research Centre (MUARC) recently completed an evaluation of the impact of an anti-drink driving and random breath testing (RBT) initiative on crashes in Victoria during 1990 (and the last few weeks of 1989). This extension to the initial study provides an alternative method of estimating the effect of the initiative on crashes in 1990, and also attempts to assess the effect during 1991.

THE INITIATIVE

The initiative involved a substantially different method of RBT enforcement compared with past operations, with bus-based RBT stations replacing car-based stations. A multi-million dollar, Statewide anti-drink driving publicity campaign through all mass media, was launched in mid December 1989, and reinforced throughout 1990 and 1991. This campaign was the cornerstone of public perceptions of the program, designed to both heighten perceptions of extended enforcement and sensitise the public to the consequences of drink driving. A media launch and publicity for the introduction of new "Booze Buses" also occurred in April and September 1990, respectively.

The change to bus-based operations more than doubled the number of drivers tested per unit time from the passing traffic stream, thus increasing the proportion of drivers tested who approached RBT stations. The initiative has also changed the quality of exposure to RBT by the clearly identifiable and highly visible "Booze Bus" designed solely for that purpose. In the metropolitan area there was no change in the number of RBT sessions conducted, and gradual but smaller increases in session hours/duration. In rural Victoria, the number of sessions decreased, whilst session hours dropped mainly throughout 1990.

The late 1989 and 1990 period of the initiative involved the launch of the initiative and publicity, the change-over to the use of buses in enforcement operations, the phased introduction of 13 new buses to replace 4 existing buses, but relatively small increases in hours of RBT operation (in the metropolitan area). The 1991 period represents a more stable period of new bus operations and publicity, and larger increases (relative to 1989) in both hours of RBT operation and tests conducted in the metropolitan area.

These different patterns were assessed by testing effects in the 1990 and the 1991 periods separately. The introduction of the initiative differed between metropolitan Melbourne and rural Victoria, with (relatively) smaller, more gradual changes in rural areas. This also indicated that effects for these two parts of the State should be separately evaluated.

In brief the key aspects of the initiative were:

- A major multi-million dollar, multi-media publicity campaign
- Thirteen new, high profile 'Booze Buses', largely replacing car-based testing, especially in Melbourne
- A strike force using 'Probationary Constables In Training' (PCIT's) on monthly roster to operate buses
- More than doubling the number of drivers tested, mostly in Melbourne

METHOD

Time series modelling of high alcohol hour serious casualty and fatal crashes was undertaken to estimate the change in these crashes relating to the RBT initiative during 1990 and 1991, taking into account changes in unemployment rate and changes in the same crash types in NSW.

Multivariate time series models of high alcohol hour serious casualty and fatal crashes were developed for Melbourne (treatment area) and Sydney (comparison area), so that the changes beginning from the first month of the initiative could be estimated for each series. Percentage changes in each area could then be contrasted allowing the net percentage change in Melbourne to be estimated, ie. estimated percentage changes in target crashes in Melbourne during each of the post-intervention years (1990 and 1991) were adjusted by the parallel estimated percentage changes in Sydney during the same year. The net change provided an estimate of the percentage change in high alcohol hour crashes that is attributable to the RBT initiative. The same process was undertaken for the respective rural areas of Victoria and NSW.

Unemployment rate was used in the models in order to account for different trends in the two States in estimated total vehicle travel and economic activity (especially its influence on night-time travel) over the intervention period. In order to check that unemployment rate was a valid indicator, models were fitted for each State with one using estimated vehicle travel (based on fuel sales) and the other using the unemployment rate. The results obtained were virtually identical regardless of whether estimated travel or unemployment rate was used.

A form of time series modelling known as ARIMA Intervention Analysis was used to estimate effects during 1990, whilst a multiple regression approach was used to estimate effects during 1991.

RESULTS

Table 1 below shows the estimated changes in high alcohol hour serious casualty crashes for Melbourne and rural Victoria separately during 1990 and 1991.

Table 1

Net percentage changes in high alcohol hour serious casualty crashes in Melbourne and in rural Victoria during 1990 & 1991

Intervention Period	Net change in Melbourne# & 90% confidence interval	Net change in rural Victoria# & 90% confidence interval
1990 (ARIMA model)	-17.9%* (-7.9% to -26.8%)	-12.8%* (-3.7% to -21.1%)
1991 (Regression model)	-13.4% ns (9.0% to -31.2%)	-24.3%* (-13.2% to -34.0%)

ONE-TAILED SIGNIFICANCE TESTING FOR NET REDUCTIONS IN MELBOURNE, CORRESPONDING TO 90% CONFIDENCE INTERVALS

* Statistically significant at $p < 0.05$ level ns not significant at the 0.05 level

Using Sydney as a comparison area for Melbourne (after taking into account the different changes in unemployment rate in the two cities), the changes in Sydney were used to estimate the changes that would have occurred in Melbourne had the intervention not taken place. The netted percentage changes show that the initiative was associated with a significant 17.9% reduction in high alcohol hour serious casualty crashes in Melbourne in 1990, but no statistically significant effect in 1991 (non significant 13.4% reduction).

Using rural NSW as a comparison area for rural Victoria (after taking into account the different changes in unemployment in the two rural areas), the changes there were used to estimate the changes that would have occurred in rural Victoria had the intervention not taken place. The netted percentage changes show that the initiative was associated with a significant 12.8% reduction in high alcohol hour serious casualty crashes in 1990 and a significant 24.3% reduction in 1991.

Fatal crashes during high alcohol hours were used as a secondary criterion in this evaluation because of their lower numbers, higher chance variation, and hence reduced sensitivity for measuring the effect of the RBT initiative compared with serious casualty crashes. The results are shown in Table 2 below.

Table 2

Net percentage changes in high alcohol hour fatal crashes in Melbourne and in rural Victoria during 1990 & 1991

Intervention Period	Net change in Melbourne# & 90% confidence interval	Net change in rural Victoria# & 90% confidence interval
1990 (ARIMA model)	-24.2%* (-1.4% to -41.8%)	2.6%ns (32.8% to -20.7%)
1991 (Regression model)	-3.6%ns (66.4% to -44.1%)	-1.2%ns (42.0% to -31.3%)

ONE-TAILED SIGNIFICANCE TESTING FOR NET REDUCTIONS IN MELBOURNE, CORRESPONDING TO 90% CONFIDENCE INTERVALS

* Statistically significant at $p < 0.05$ level ns not significant at the 0.05 level

Using Sydney as a comparison area for Melbourne, the netted percentage changes show that there was a statistically significant 24% decrease in high alcohol hour fatal crashes in Melbourne in 1990, but no statistically significant effect in 1991.

Using rural NSW as a comparison area for rural Victoria, the netted percentage changes show that there was no statistically significant effect of the initiative on high alcohol hour fatal crashes in 1990 or in 1991. The confidence limits were, however, very wide for these estimates which reduces the chances of showing a significant effect.

CONCLUSIONS

The findings of the present study indicate that the RBT initiative (in its entirety) resulted in an 18% reduction in high alcohol hour serious casualty crashes and a 24% reduction in high alcohol hour fatal crashes in Melbourne in 1990, but no statistically significant effect during 1991.

In rural Victoria, high alcohol hour serious casualty crashes decreased by 13% in 1990 and by 24% in 1991, whilst there were no statistically significant effects in high alcohol hour fatal crashes in rural Victoria in 1990 nor 1991.

The conclusiveness of these findings depends on the adequacy of unemployment rate as an indicator of changes in travel during high alcohol hours, the appropriateness of NSW as a comparison area to take into account the effects of "other" factors (other than unemployment rate) influential in Victoria during the intervention period, and the assumption of minimal effects of speed camera operations in Victoria during high alcohol hours.

1.0 INTRODUCTION

The Monash University Accident Research Centre (MUARC) has recently completed an evaluation to determine the impact of an anti-drink driving and random breath testing (RBT) initiative on crashes in Victoria during 1990 (and the last few weeks of 1989). The initiative involved a qualitatively and quantitatively different method of RBT enforcement with bus-based RBT stations replacing car-based operations. A multi-million dollar, Statewide multi-media publicity campaign "If you drink then drive, you're a bloody idiot" was launched in mid December 1989, and intensively reinforced intermittently throughout 1990 and 1991. A media launch and publicity for the introduction of new "Booze Buses" also occurred in April and September 1990, respectively.

A detailed description of the initiative, operational data and the methodology used in the initial evaluation of effects up to the end of 1990 is reported by Drummond, Sullivan, Cavallo & Rumbold (1992) and in a detailed report of operations by Sullivan, Cavallo, Rumbold & Drummond (1992). This extension to the initial study provides an alternative method of estimating the effect of the initiative on crashes in 1990, and also attempts to assess the effect during 1991.

2.0 OVERVIEW OF THE INITIATIVE - WHAT IS BEING EVALUATED ?

2.1 RBT enforcement

Changes in RBT began in metropolitan Melbourne in late 1989, with a large-scale shift to bus-based RBT operations from the traditional car-based method (buses had been used at a relatively low level over previous years). Existing buses (4 operational Toyota Coaster converted buses) were used initially, being gradually replaced with a fleet of 13 new purpose-built and highly visible "Booze Buses" between April and November 1990. There was a much smaller and delayed use of bus-based RBT in rural Victoria, although rural areas (generally) closer to Melbourne received bus-based testing earlier and more frequently than rural areas farther from Melbourne (Figures 1- 4). This is not surprising given that the initiative and new buses were first obvious in Melbourne, and the deployment of RBT operations was centrally controlled for metropolitan police districts.

Quantitatively, the main changes associated with the use of bus-based RBT stations was that a greater number of personnel can conduct tests at any one time. The sampling fraction from the passing traffic stream was therefore greater, resulting in more than a doubling in the testing rate (per hour). This is reflected in the doubling in tests conducted since 1989. The level of testing increased in 1990 and again in 1991 (Figure 5). Of course, the number of personnel required to undertake such operations increased substantially.

Figure 1
RBT Tests: Car v. Bus - Metropolitan Area

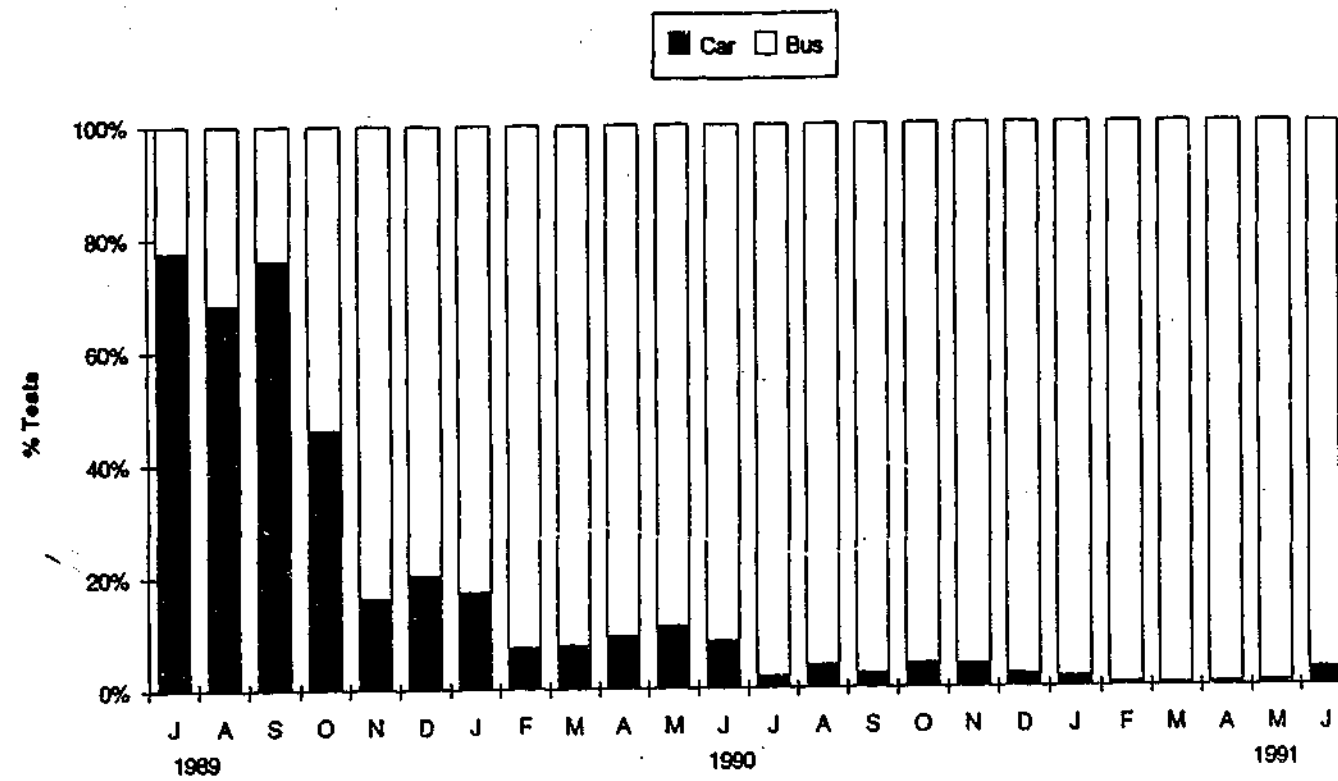


Figure 2
RBT Hours Testing: Car v. Bus - Metropolitan Area

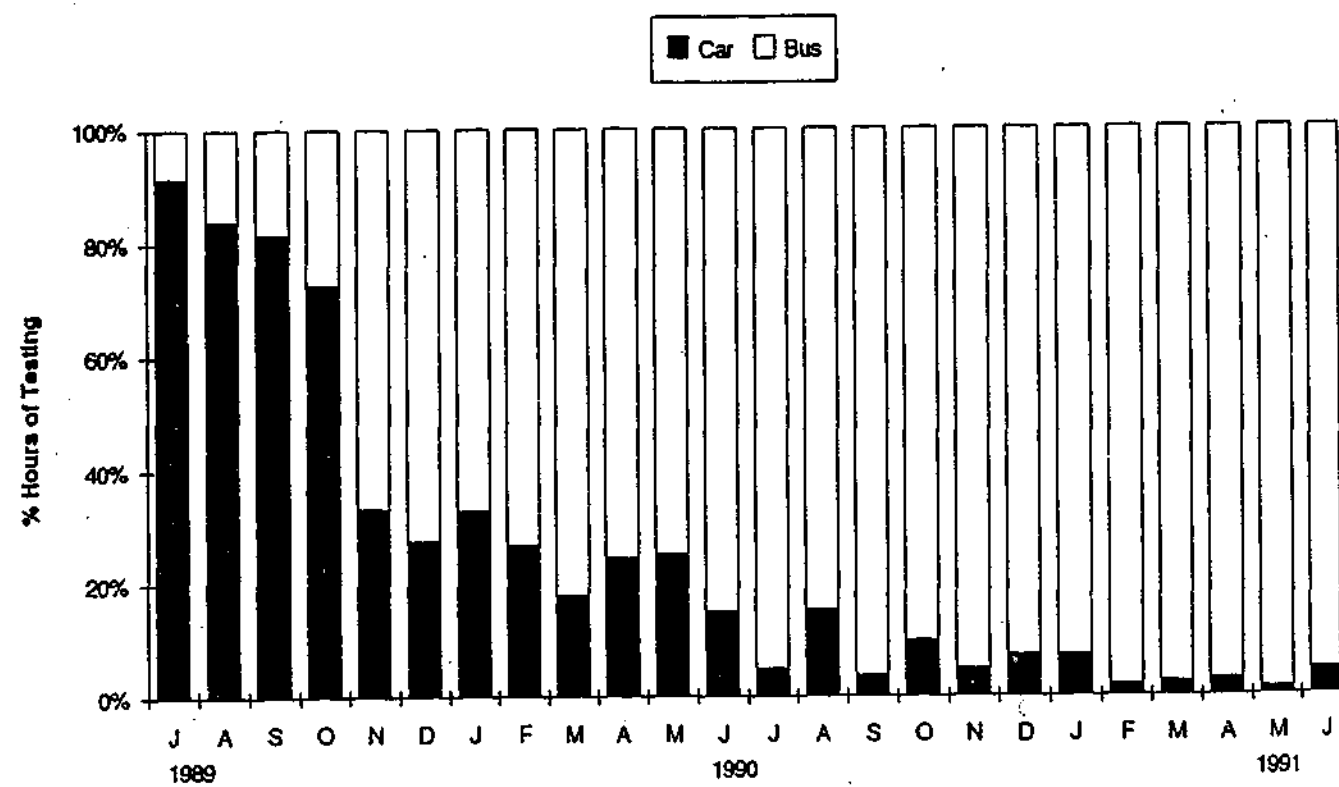


Figure 3
RBT Tests: Car v. Bus - Rest of State

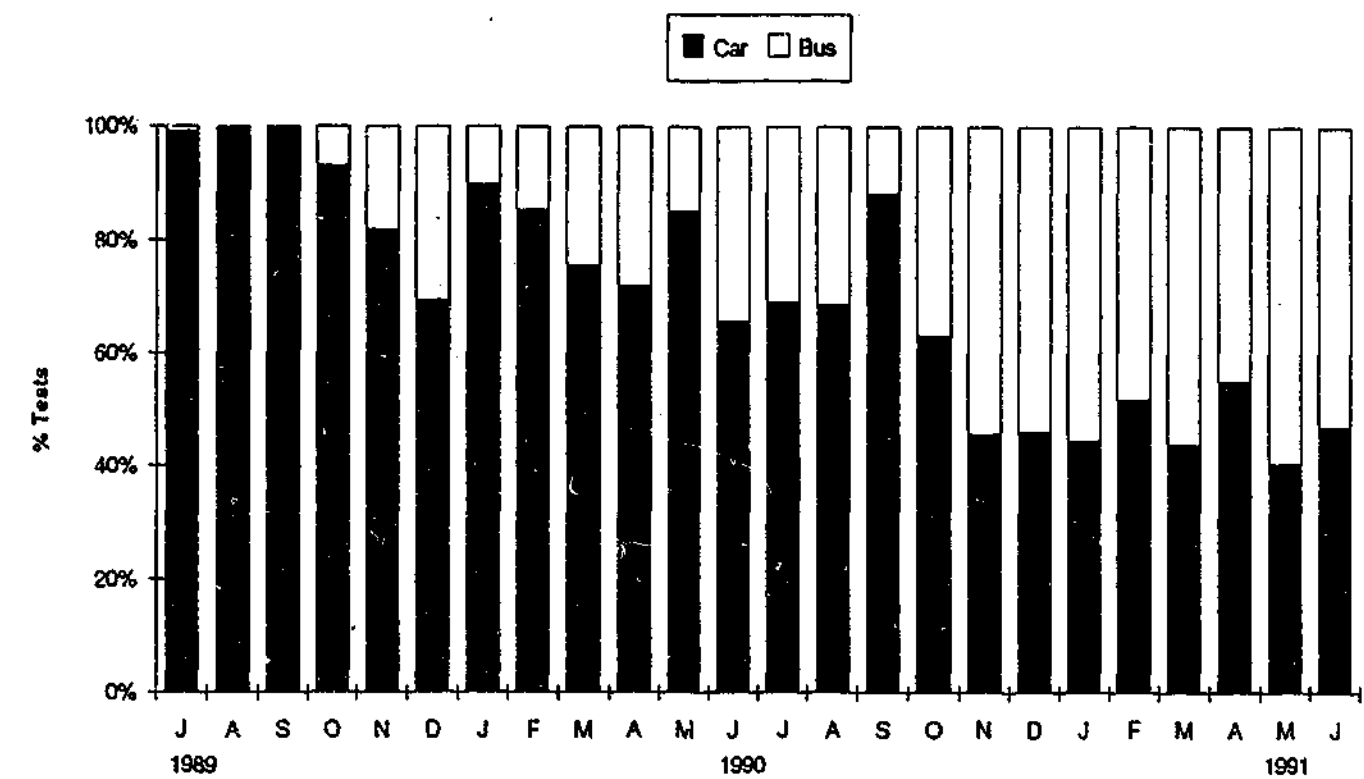


Figure 4
RBT Hours Testing: Car v. Bus - Rest of State

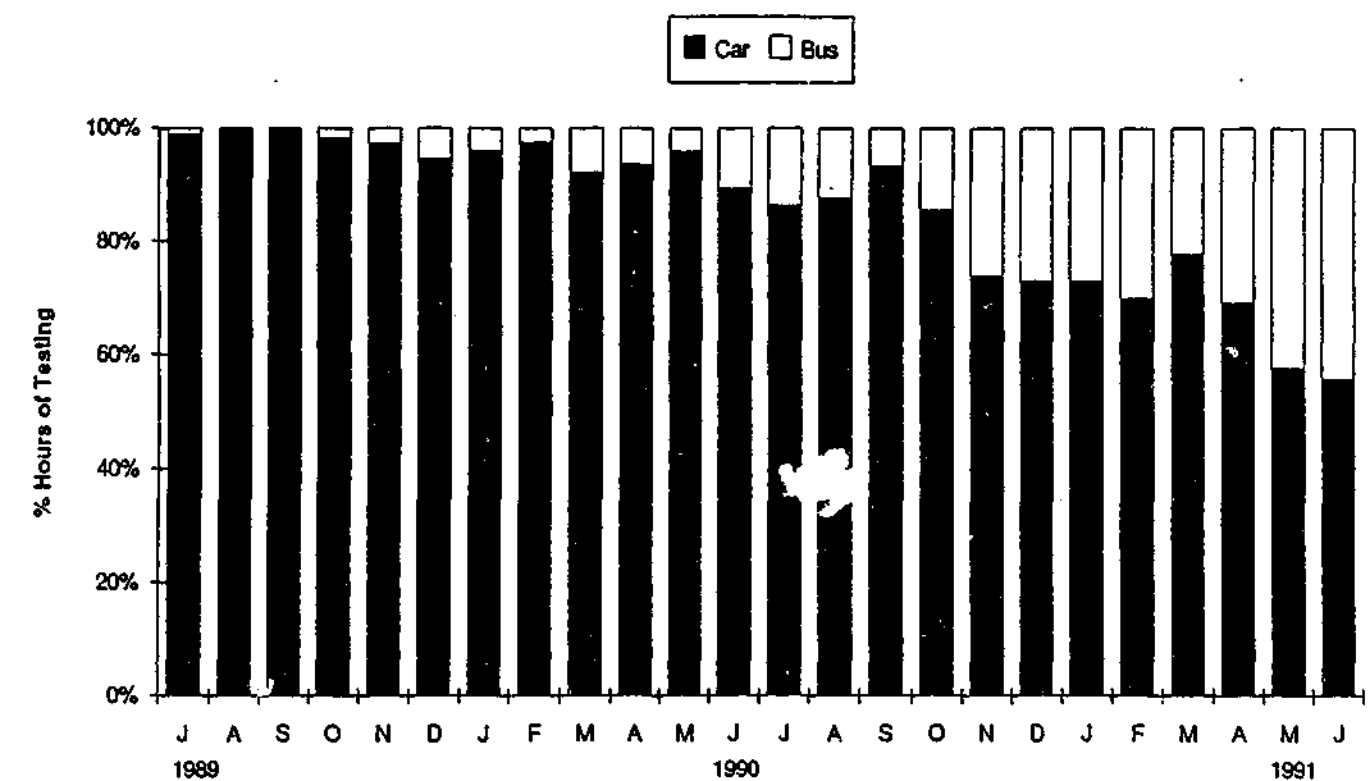
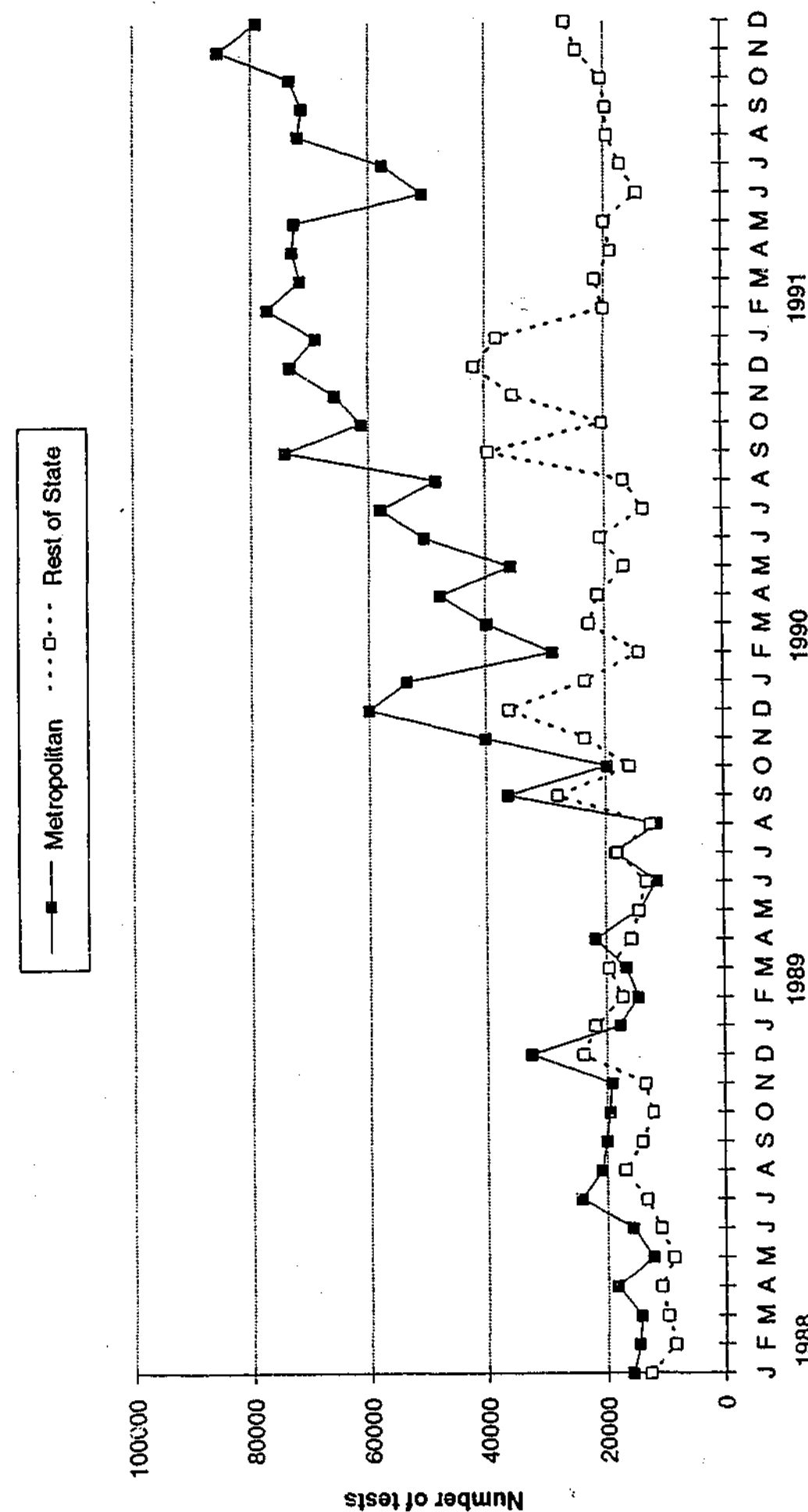


Figure 5
Monthly Number of RBT Tests Conducted, 1988-1991



In contrast, there was little change in the number of RBT sessions conducted in the metropolitan area, and decreases in the rural area (Figures 6 & 7). The duration of sessions became slightly longer, reflected as small and gradual increases in the total number of hours of RBT operations in the metropolitan area (an increase by about $\frac{1}{3}$ in 1990 and $\frac{2}{3}$ in the first half of 1991, relative to 1989; Figure 8). Therefore, in this sense, there has been only a modest expansion of the RBT program in the metropolitan area, especially in 1990, relative to the more drastic and large increase in tests conducted.

In reality, the probability of being tested when driving past an RBT station has greatly increased, but the chances of actually passing an RBT station has only slightly increased. However, the relative deterrent value of increasing the former as opposed to the latter is not known (the incremental deterrent value of actually being tested rather than just driving past an RBT station has not been assessed) and also the perceptions of drivers may not reflect this reality (the initiative may have exaggerated the presence of RBT stations in the minds of drivers).

At a **qualitative** level, bus-based testing is considered to be more conspicuous than car-based operations, particularly for the new "Booze Buses" which are purpose-built for maximum visibility. It is therefore probable that the proportion of drivers recognising the bus-based RBT station as they pass it or travel in its vicinity, increases. This assumes that a large proportion of those passing a traditional car-based session do not recognise its purpose. It should be noted however that there are no data to validate this expected outcome. An additional change has been the use of Police Constables In Training (PCITs) to undertake the testing at RBT stations since November 1989.

On the basis of the data provided, it would seem that this initiative has sought to increase the deterrent value of RBT, largely by improving the quality of delivery, visibility and increasing testing rate per hour of operation, rather than by markedly increasing the duration of RBT operation, per se.

2.2 Publicity

A publicity campaign was launched on 12 December 1989 and has been presented through all types of media (television, print and radio). Graphic post-crash scenarios are presented in which vehicle passengers are severely injured and the driver has a positive BAC reading. The television advertisements are particularly emotive showing scenes with distressed relatives and hospital staff, and the severity of the personal injuries resulting from the crash. Some advertising also publicised RBT enforcement through the new "Booze Buses". There were also a considerable number of media articles published about the initiative. The level of publicity was extremely intensive relative to levels of previous initiatives, with public recall of the advertisements at over 90%.

Figure 6
Metropolitan Area - Number of RBT Sessions Conducted

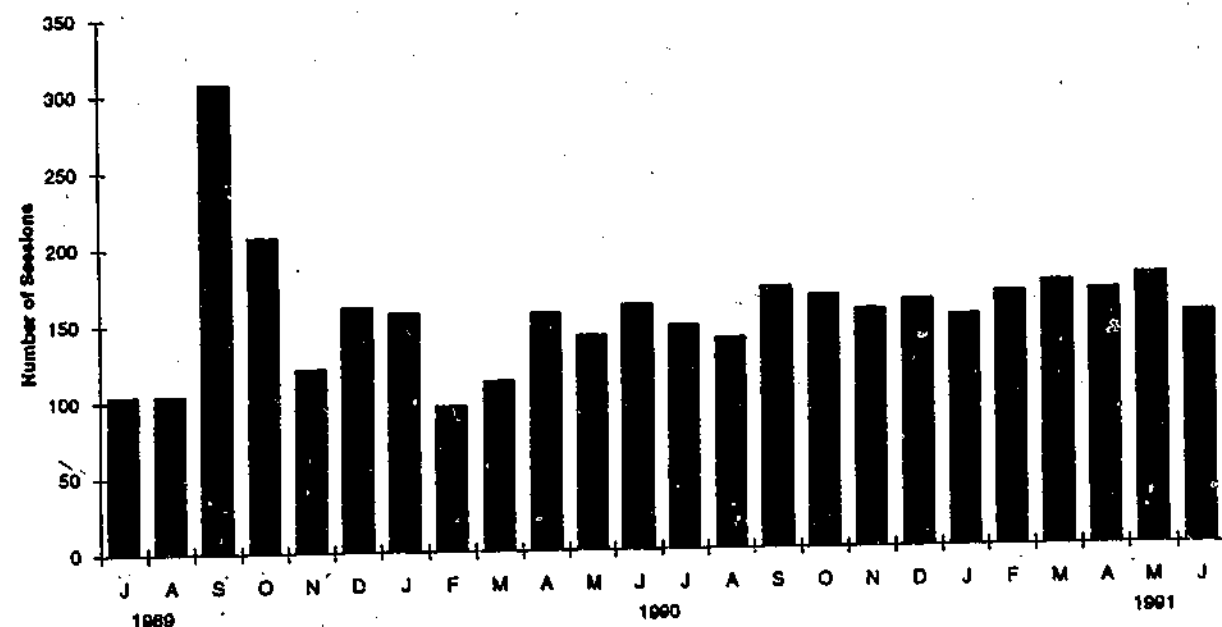


Figure 7
Rural Area - Number of RBT Sessions Conducted

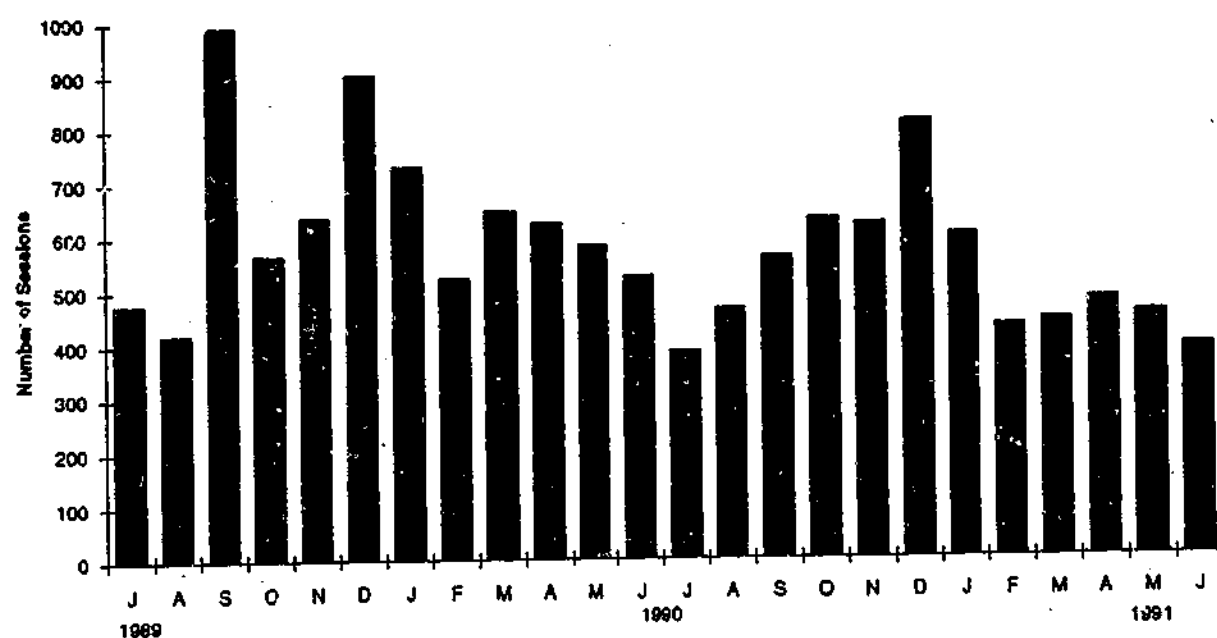
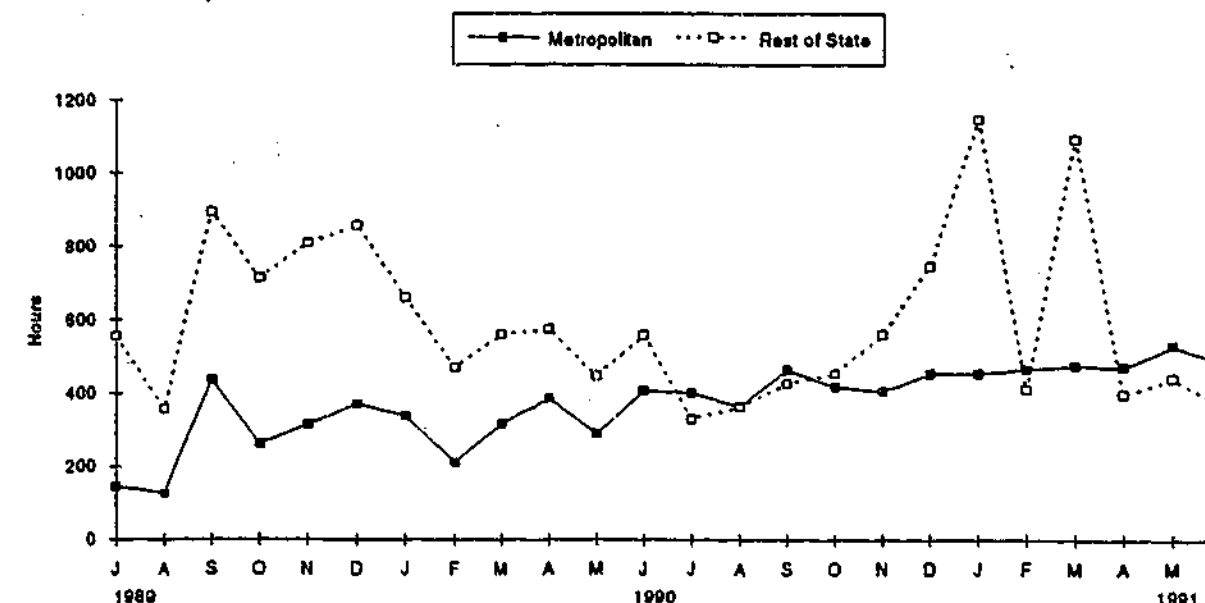


Figure 8
RBT - Monthly Number of Hours of Testing



2.3 The program being evaluated

In essence, the program being evaluated:

- builds on a history of mainly car-based RBT, related drink driving publicity and other anti-drink driving countermeasures which have been introduced since the late 1970's;
- has more than doubled the number of drivers tested per unit time from the passing traffic stream, thus changing the relative mix of non-tested vs tested motorists in the passing traffic stream.
Actually taking a test and/or an awareness that there is a greater chance of being tested when passing a bus-based station, may have enhanced deterrence value over only seeing the operation and/or perceiving that there is a lower probability of being tested when passing¹;
- has increased the number of drivers exposed to RBT to the maximum potential that can be achieved from:
 - (i) increased visibility (for the same number of sessions), and
 - (ii) the gradual, but relatively smaller increments in the length of sessions/hours of operation;
- has changed the quality of exposure to RBT by the clearly identifiable and highly visible "Booze Bus" designed solely for that purpose;
- has used an intensive publicity campaign to raise community awareness of drink driving, crash consequences and, to a lesser extent, the introduction and operation of "Booze Buses".

¹ It is not known to what extent drivers are aware of this change, nor what the relative deterrent value is of actually being tested as opposed to seeing the operation

3.0 METHODS OF EVALUATION

3.1 Quasi-experimental time series analysis

The initial evaluation (Drummond *et al.*, 1992) used univariate time series analysis to predict, on the basis of pre-intervention trends, the incidence of crashes that would have occurred within a treated 'group' in the study period. This was then compared to the actual number of crashes to determine whether there had been changes. Changes in a comparison 'group', not affected by the treatment, were used to account for the changes due to the influence of other factors (for example, abrupt changes in road use, or the introduction of another countermeasure) which coincided with the timing of the initiative.

This evaluation process is displayed diagrammatically in Figure 9 below.

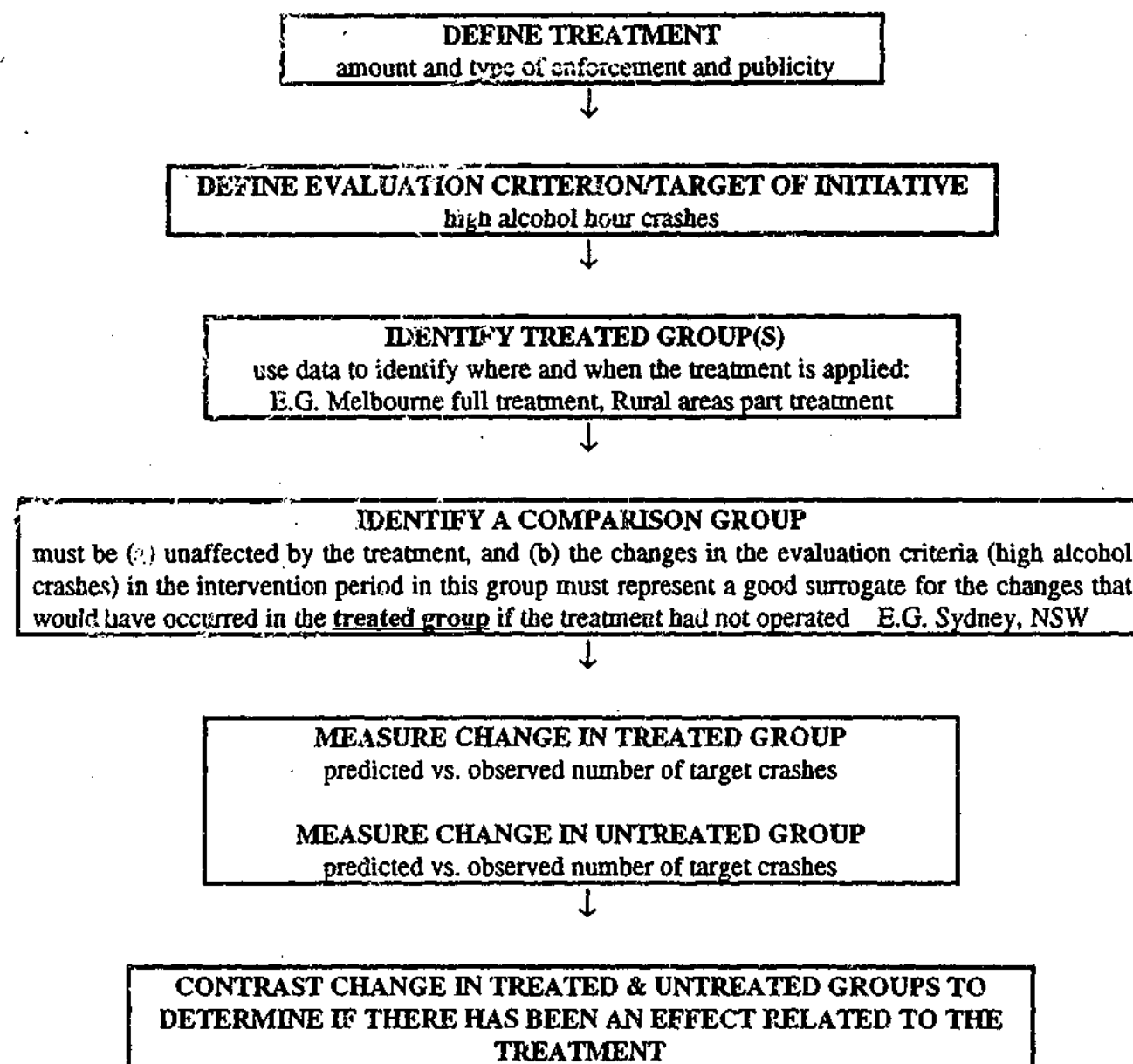


Figure 9 Flow chart describing evaluation process

In order to satisfy the comparison group requirement (b) above, it is widely held that the treatment and comparison groups be similar in as many ways as possible, to avoid confounding (Hauer, 1991 offers an alternative view on this subject, however). However, it is not possible, in a strict sense, to have complete similarity between treatment and comparison groups, unless the treatment is randomly allocated to matched pairs of groups. What needs to be kept in mind is that the changes in crashes in the comparison group needs to represent the effects of 'other factors' on the target crashes in the treatment group, so that changes in the comparison group can be used to estimate the changes that would have occurred in the treatment group.

Differences between the treated and comparison groups can confound the estimation of change due to other factors in the treatment group. At times, a valid comparison group is not available, or it may meet some criteria but not others, or it is difficult to assess its adequacy given the range of factors that could be important and might differ, but for which data are not available to undertake such an assessment. The challenge in the evaluation of this countermeasure was to find an adequate comparison group to serve the above purpose (that is, to account for other factors which have also influenced the treatment group) thus allowing inferences ascribing observed changes to the initiative.

In the initial evaluation a quasi-experimental approach was used, in which inter-State comparisons (metropolitan Melbourne vs. Sydney) and intra-rural comparisons provided a form of "control" (ie. the comparison area is used to account for the effects of other factors, which are presumed to affect the treated and comparison area equally). Such interstate comparisons cannot provide perfect control since economic and other factors are not identical in both areas, and each State has conducted some type of drink driving countermeasure over previous years (Homel, 1988). However, this provided the best available comparison group and the best evaluation method within the framework of univariate time series analysis.

Econometricians (Phillips, Ray & Votey, 1984; Votey, 1984) have argued that this approach does not take account of the many exogenous factors which "influence" accident levels (eg. vehicle distance travelled, alcohol consumption, vehicle mix, law enforcement levels, etc.). Econometric models of casualties attempt to statistically control for and take account of the many factors that might influence accident levels.

3.2 Multivariate time series analysis

Because it was believed that it is likely that "other factors" (eg. alcohol sales and economic activity, especially the influence of the latter on night-time travel) may have had different effects in treated and comparison areas, an alternative method of time series analysis was used in this evaluation study.

This extension to the initial study used the same quasi-experimental areas for comparison and, in many ways, built on the initial work, for instance, by using the same evaluation criteria (that is, serious casualty crashes and fatal crashes in high alcohol hours) and the data-driven approach to defining the nature of the enforcement and identifying areas and times which are affected by it. The major conceptual differences in the two methods are:

1. A Melbourne vs. Sydney comparison is still used in a time-series framework, but an attempt is made to account for the differential effect of "other" (ie. economic) factors on the incidence of crashes in the two areas, which may disturb the comparison.

In order to do this, a multivariate time series model is developed and the effects of the economic measure are separated from the effects of the initiative. Instead of using a univariate time series model to make predictions for comparisons with actual crash frequencies, an explanatory model was developed within which the initiative and economic measures are included as variables in the model and tested for their relationships with the crash data.

2. The use of time series modelling to discern relationships between crashes and independent variables, rather as a tool for making predictions, allows statistical assessment of effects in post-intervention periods longer than 12 months after the introduction of the initiative. This is because most univariate forecasting methods can only predict one seasonal cycle ahead.

This allowed the extension study to attempt to estimate the effects of the initiative during 1991, in addition to providing alternative estimates during 1990. (The initial study did not attempt to measure effects beyond the end of 1990, which was recognised as the limit of the univariate time series analysis methods used.)

3. The use of time series modelling in this way theoretically allows testing of the separate relationships between measures of elements of the initiative (eg. the enforcement and publicity elements) and the crash criteria, to provide indications of the relative effects of each component. The problems with attempting this are listed in footnote 2.

4.0 DETAILS OF THIS EVALUATION

4.1 Scope of the evaluation

As noted in Section 2, the initiative is comprised of different elements. The initial change-over in RBT enforcement from use of cars to use of existing buses, the phased introduction of the new buses over an eight month period (April to November 1990), and the presence of publicity over all phases, means that the effects of the various components of the initiative cannot be assessed independently, in a strict technical sense².

However, quantitatively and qualitatively different treatment periods can be assessed by testing effects in the 1990 and the 1991 periods separately. The former period represents the effects of the launch of the initiative, the change-over to the use of buses in enforcement, the phasing in of new buses and related publicity, and relatively small increases in hours of RBT operation; the latter period represents a more stable period of new bus operations and publicity, and larger increases (relative to 1989) in both hours of RBT operation and tests conducted³. However, obtaining

² A multivariate time series model could be used to dissociate effects by enforcement and publicity over the intervention period, however this approach has the following problems:

- (i) a measure of the publicity over time is required, such as TARPS (Target Audience Rating Points);
- (ii) the quantified measure of publicity may not reflect its expected effect on crash risk; ie. the numerical values representing the amount of publicity over time does not necessarily represent its effect on crash risk because the nature of the relationship is more complex, with residual and decremental effects over time to be taken into account, as well as the nature of the publicity and the characteristics of its communication, eg. intensity and duration of the campaign (Johnston & Cameron, 1979);
- (iii) an appropriate measure of RBT enforcement needs to be selected for use in the model to represent the amount of enforcement over time, however, as discussed earlier, the relationship between RBT hours of operation and tests conducted has been altered by the new method of RBT, and the relative deterrence value of tests as opposed to hours of operation is not known, therefore selection of the appropriate measure is difficult and directly influences the results that would be obtained;
- (iv) if the measures of publicity and enforcement correlate, then it is not possible to dissociate their individual correlations with crash criteria;
- (v) if it is possible to obtain individual correlations for publicity and enforcement with crashes, there is always the possibility that this is an artefact of these measures' association with another (unmeasured) factor, that is, the correlations only provide a measure of association but not causation;
- (vi) correlations obtained only relate to those produced when the other components of the program were operating, and not the expected effect of that component when operating on its own.

³ Although the post-intervention period could be segmented into smaller, more specific treatment periods when different mixes of the components were operating, the problem with interpreting results from such analyses makes this technically indefensible for the following reasons: (a) public perceptions of the initiative are unlikely to be compartmentalised into short, discrete treatment periods in chronological order (eg. old bus RBT only, publicity and old bus RBT, publicity and mix of old/new bus RBT (at varying levels over time), publicity and new bus RBT only) (b) that small time periods (eg. a few months) reduce the specific crash frequencies analysed and therefore reduce statistical power (c) the possible effect of preceding time periods/treatment mixes could contaminate the assessment of a subsequent treatment mix (d) the effects of preceding treatment(s) cannot be discounted, ie. treatment effects may be ongoing and merge in with different treatment types. The latter two issues are also a problem for assessing the effects of the 1991 period of the initiative.

statistically significant results for 1991 requires larger changes than for 1990, because the confidence limits of the estimated effects are likely to be much wider in the second year.

An evaluation of a program attempts to provide an accurate indication of whether the particular program achieved a desired effect. It does not necessarily answer questions about *how* the program achieved its effects (if any) or whether a different type of program will or will not produce a particular effect. This study evaluated the safety outcome of the initiative, but is not a study of the *how* the initiative, or its respective components, achieved the outcome. This is discussed in section 8.0 Recommendations.

4.2 Hypotheses

The study aimed to test and quantify the following hypotheses:

- (a) a reduction in the risk of high alcohol hour serious casualty crashes in Melbourne and the rest of Victoria (separately) during 1990 through the introduction of the RBT initiative, relative to the pre-intervention level;
- (b) the reduction in the risk of high alcohol hour serious casualty crashes in Melbourne and the rest of Victoria (separately) during 1991 through the maintenance and expansion of the RBT initiative, relative to the pre-intervention level;
- (c) the reduction in the risk of high alcohol hour fatal crashes in Melbourne and the rest of Victoria (separately) during 1990 through the introduction of the RBT initiative, relative to the pre-intervention level;
- (d) the reduction in the risk of high alcohol hour fatal crashes in Melbourne and the rest of Victoria (separately) during 1991 through the maintenance and expansion of the RBT initiative, relative to the pre-intervention level.

From past experience of consistent reductions in crash risk associated with RBT, in Victoria and NSW, it was hypothesised that a reduction or no effect due to the new Victorian initiative during 1990 and 1991 was the only conceivable possibility. Accordingly, one-tailed significance tests for a reduction in risk have been carried out.

4.3 Evaluation criteria

The primary criterion for assessment of the initiative was serious casualty crashes (defined as crashes in which one or more person(s) is killed or admitted to hospital), whilst fatal crashes were considered secondary. This is because although fatal crashes are associated with more traumatic outcome and are more likely to be alcohol related than serious casualty crashes, their smaller numbers hamper the power of statistical tests for changes. Serious casualty crashes display chance variation of about one-third the level (in proportion terms) of that of fatal crashes.

Serious casualty crashes during high alcohol times provide a powerful evaluation criterion because 38% of these crashes have been shown to involve drivers with BACs over 0.05% (Harrison, 1990). In addition, past experience suggests that RBT can affect other types of high alcohol hour crashes other than those which are alcohol related (Homel, 1981; Cameron & Strang, 1982). Therefore the even higher alcohol involvement of fatal crashes is not an important issue affecting the priority which should be given to this crash type.

Crashes defined as those occurring in the Melbourne metropolitan area are those which occurred in Melbourne metropolitan police districts (A - J), consistent with the presence of the RBT initiative in these areas. Rural Victoria refers to all other areas in the State. Sydney metropolitan crashes are defined as those which occurred within the Sydney Statistical Division boundary, whilst rural NSW refers to all other areas of the State.

4.4 Research design

The estimated percentage change in crashes in Melbourne during each of the post-intervention years (1990 and 1991) was adjusted by the parallel estimated percentage change in Sydney during the same year. Multivariate time series models of high alcohol hour serious casualty and fatal crashes were developed for Melbourne (treatment area) and Sydney (comparison area), so that the changes beginning from the first month of the initiative could be estimated for each series. Percentage changes in each area could then be contrasted allowing the **net percentage change** in Melbourne to be estimated. The net change provided an estimate of the percentage change in high alcohol hour crashes that is attributable to the RBT initiative. Comparison area(s) were also used to increase confidence that the initiative is responsible for observed changes in the treatment area. The same process was undertaken for the respective rural areas of Victoria and NSW.

The time series method can control for long term trends, seasonal cycles and other regular patterns in the outcome variables. Road safety measures which have gradually decreased high risk factors related to the incidence and severity of crashes (eg. gradual decreases in drink driving, seat belt non-wearing, and excessive speeding; safer vehicles, improved road environments and traffic management) are represented as longer term, "smooth" trends in crash criteria over time. Such trends in a crash series were taken into account through a trend component in the model. This eliminated the need to include specific measures of such variables in the time series models to take them into account, and allowed a better assessment of the real effect of a new or abrupt change to the crash series in time.

The research design, however, involved the assumption that the influences of extraneous factors (ie. factors not included in the models) on high alcohol hour serious casualty and fatal crashes in NSW during the intervention were similar to those for Victoria. Whilst a full evaluation of all factors influencing crashes in NSW was beyond the scope of the study, an examination of the major factors that could have affected the incidence of high alcohol hour crashes prior to and during the intervention period was made. Factors considered to confound the role of NSW as a comparison area were introduced as covariates in the multivariate time series models.

4.5 Covariates

4.5.1 Controlling for exposure using economic indicators

In order to make valid contrasts between models fitted for Victoria and those for NSW it was deemed necessary to include a measure of driving exposure during high alcohol hours in the models so that possible differences in the two States would be taken into account. It is well established that driving exposure is directly related to crash risk, and it was considered that although the interstate comparison provides some level of control for national changes in travel exposure, the different economic changes in the two States (eg. higher unemployment rate in Victoria compared to that in NSW since 1990) could have led to a greater reduction in vehicle travel in Victoria, and perhaps even greater reductions in vehicle travel in high alcohol hours (mainly discretionary travel).

Direct measures of total vehicle travel, or vehicle travel for different times of the day and days of the week (eg. through traffic counting programs) are not available and such information is expensive to collect. The lack of such data is a problem, given its central role in the monitoring the success or failure of road trauma countermeasures and its role in developing traffic models and strategies to meet transport needs.

However, correlates such as fuel sales and unemployment figures are readily available. Fuel sales data has been adjusted by fuel consumption rates and corrected to equate with the Australian Bureau of Statistics triennial Survey of Motor Vehicle Usage (SMVU) to give an estimate of total travel in each of the States (Lambert, 1992). This estimate is only available on a Statewide basis whereas unemployment figures are available for metropolitan and rural areas separately. Previous research both in the United States (Partyka, 1984) and Victoria (Thoresen, Fry, Heiman & Cameron, 1992) has demonstrated significant correlational relationships between fatalities and unemployment measures over time.

An examination of the data for Victoria and NSW shows that the trends in estimated total vehicle kilometres travelled since 1983 for the two States were essentially parallel, increasing until 1990 when Victorian travel appears to decrease whilst travel in NSW remains steady (Figures 10 & 11). Similarly, unemployment rates for Melbourne and Sydney (Figures 12 & 13), rural areas for both States (Figures 14 & 15), and the total State (Figures 16 & 17) showed decreasing parallel trends over time until late 1989/1990 when rates in both States increased and that for Melbourne and rural Victoria overtook those in Sydney and rural NSW, respectively.

These differences in trends in both estimated total travel and unemployment rate just prior to and during the intervention period suggests that there could well be differences in changes in high alcohol hour travel between the two States. It was decided to include unemployment rate as a covariate for each time series developed to control for the differential effects of this factor in the treatment and control areas. This measure was preferred to estimated total vehicle travel, given its availability for the metropolitan and rural areas of each State, providing more appropriate control for the two different types of areas.

Figure 10
Estimated Total Vehicle Kilometres Travelled
(from Fuel Sales Data)
Victoria v. NSW

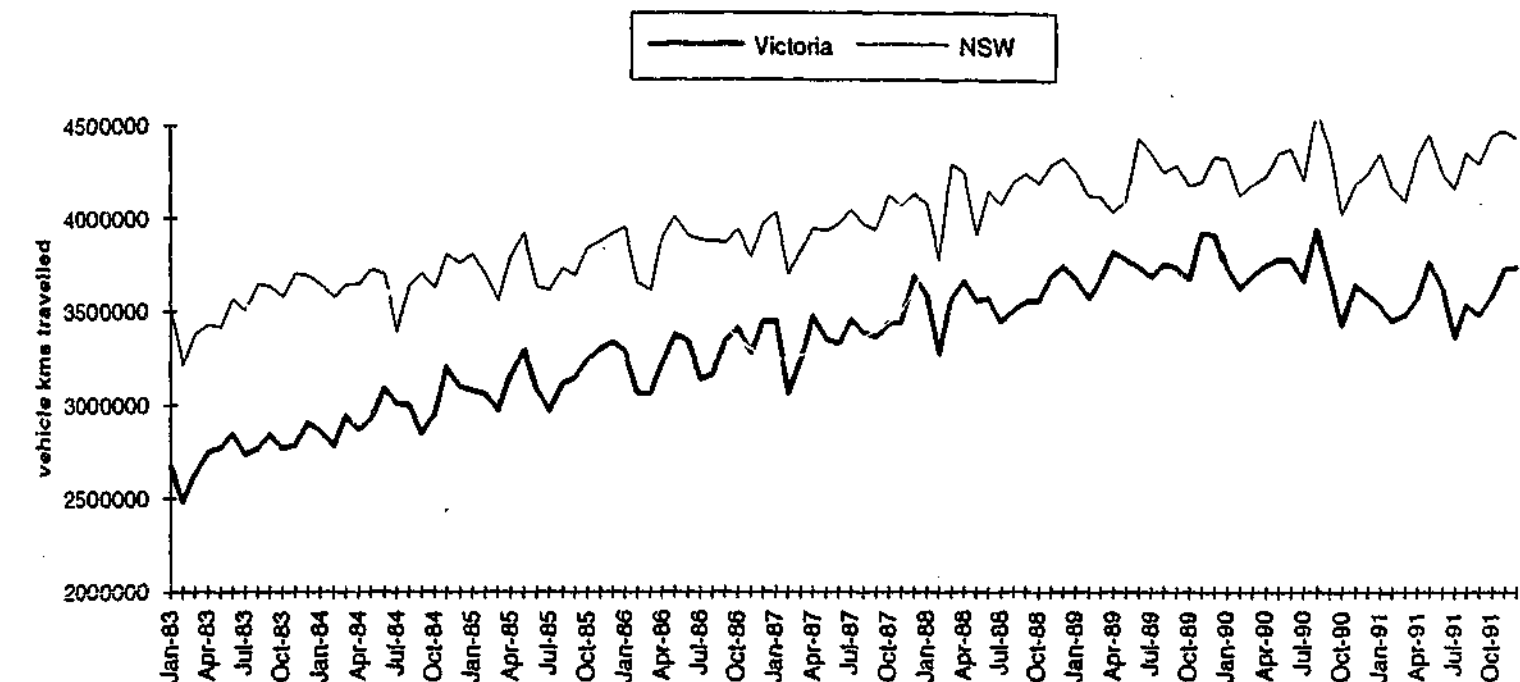


Figure 11
Estimated Total Vehicle Kilometres Travelled
(from Fuel Sales Data) 12 Month Moving Averages
Victoria v. NSW

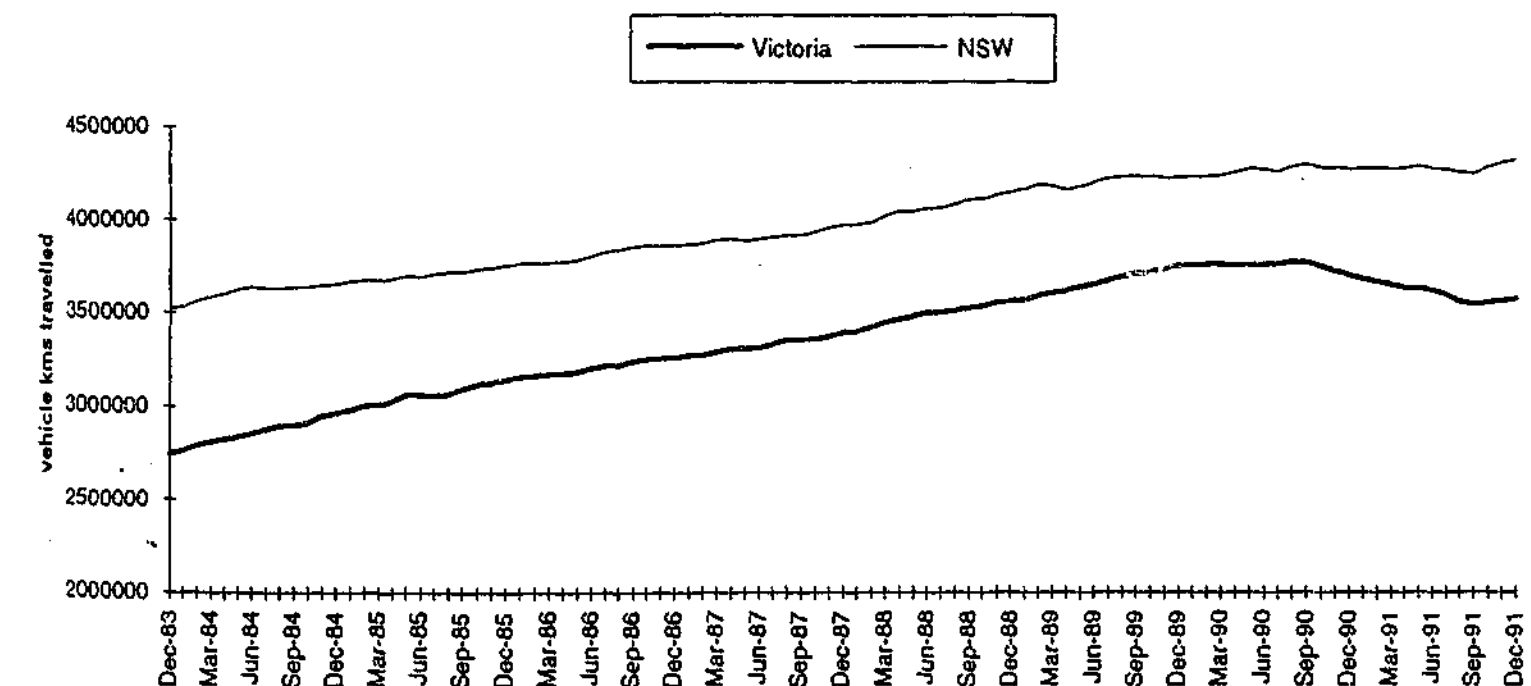


Figure 12
Unemployment Rates - Melbourne v. Sydney

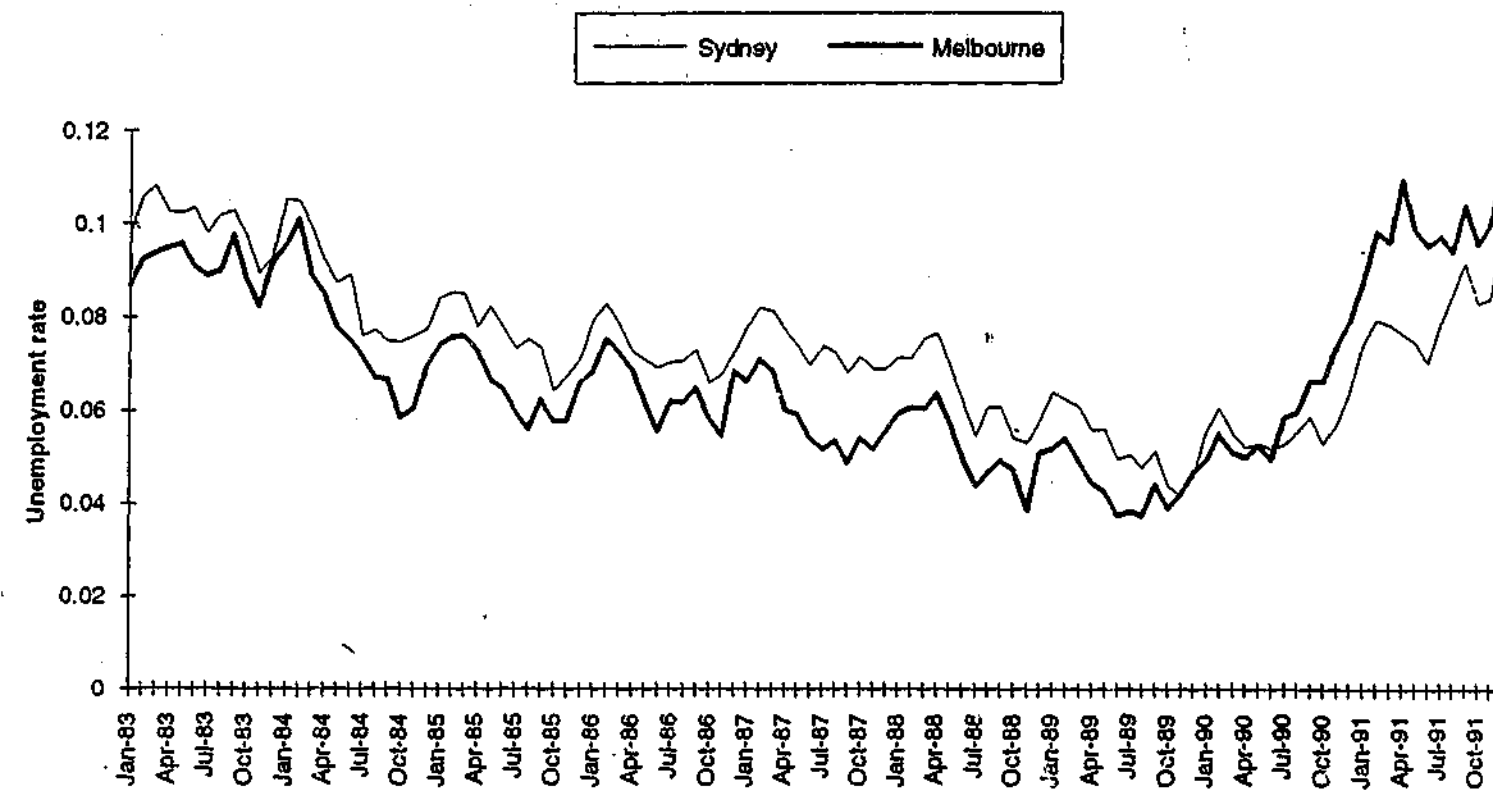


Figure 13
Unemployment Rate - Melbourne v. Sydney
12 Month Moving Averages

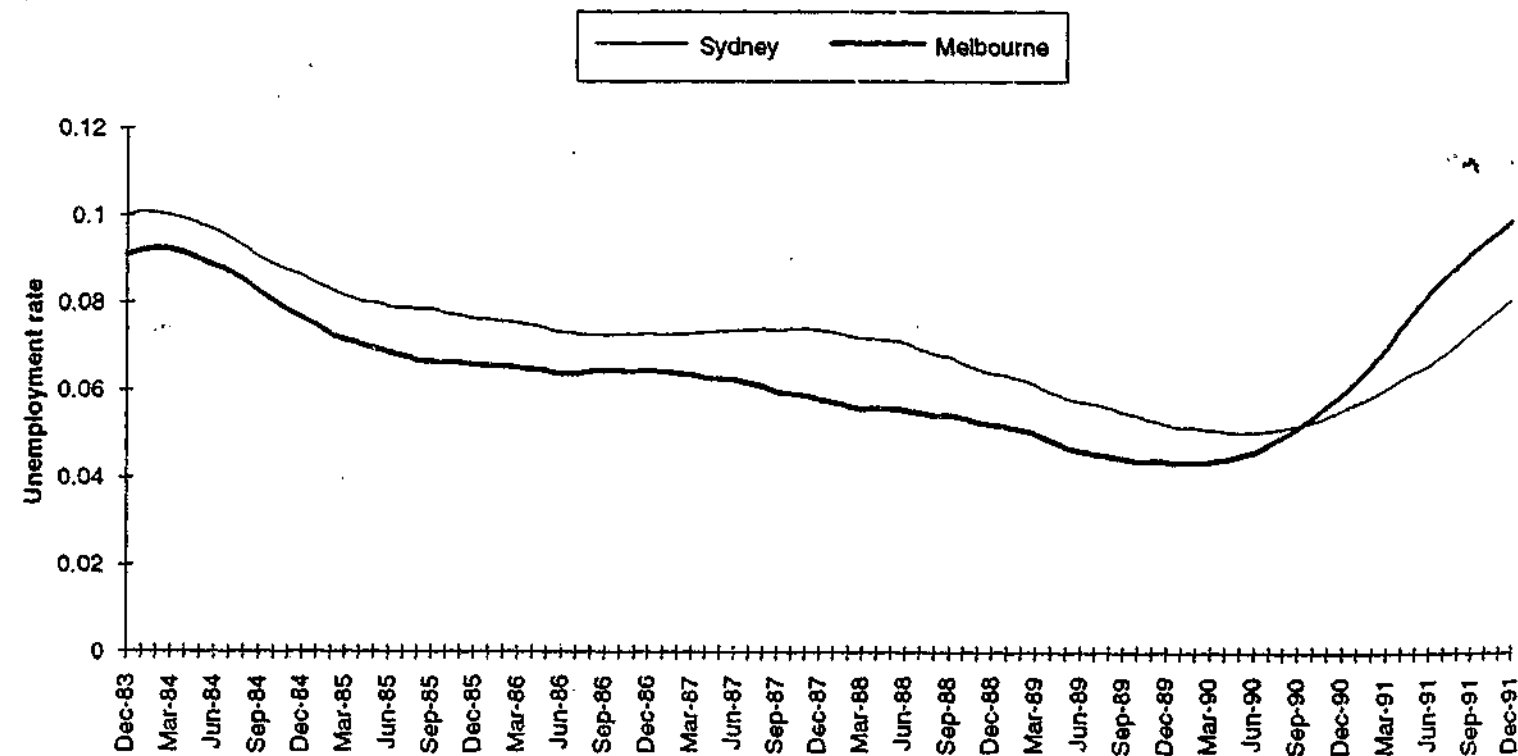


Figure 14
Unemployment Rates - Rural Victoria v. Rural NSW

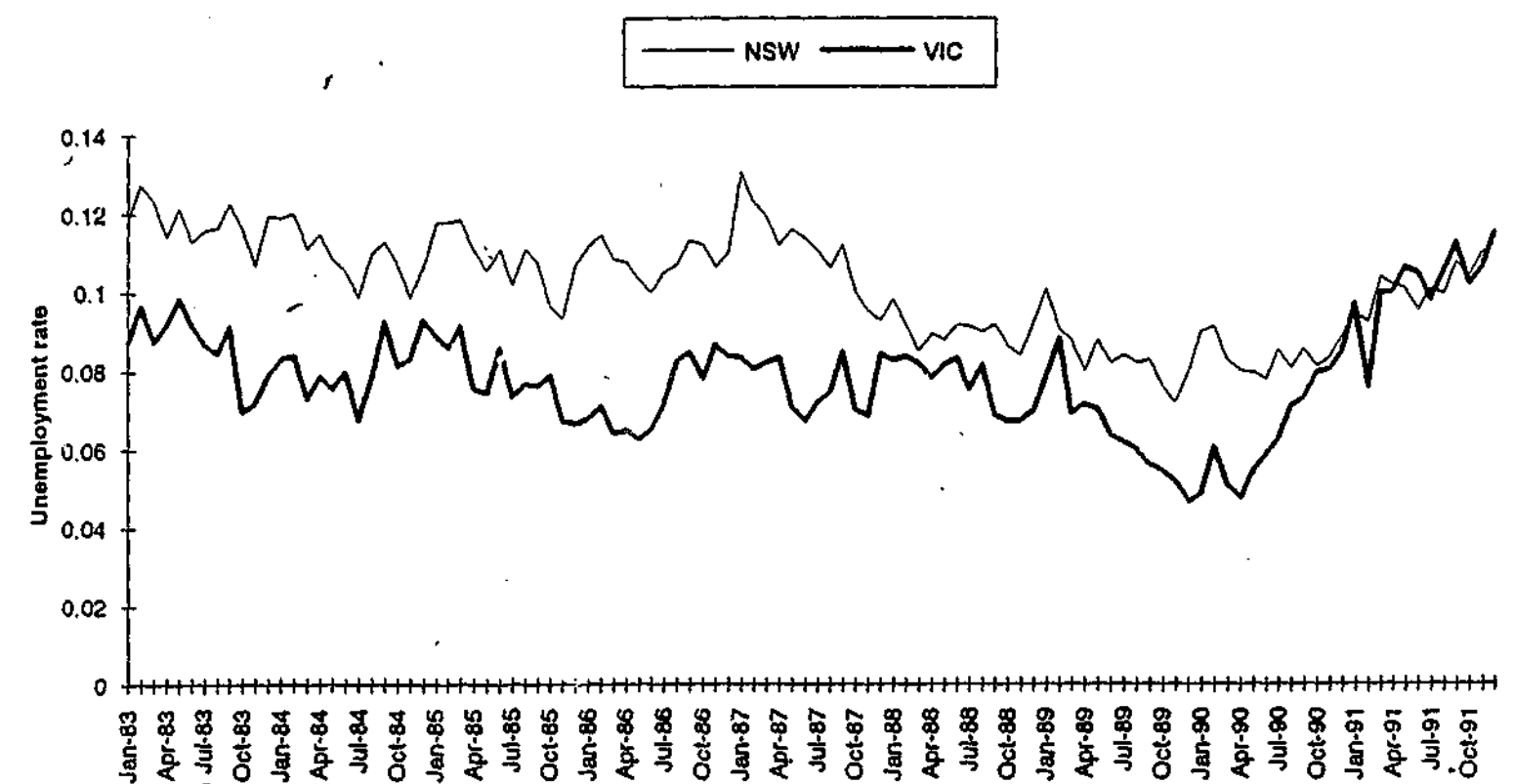


Figure 15
Unemployment Rates - Rural Victoria v. Rural NSW
12 Month Moving Averages

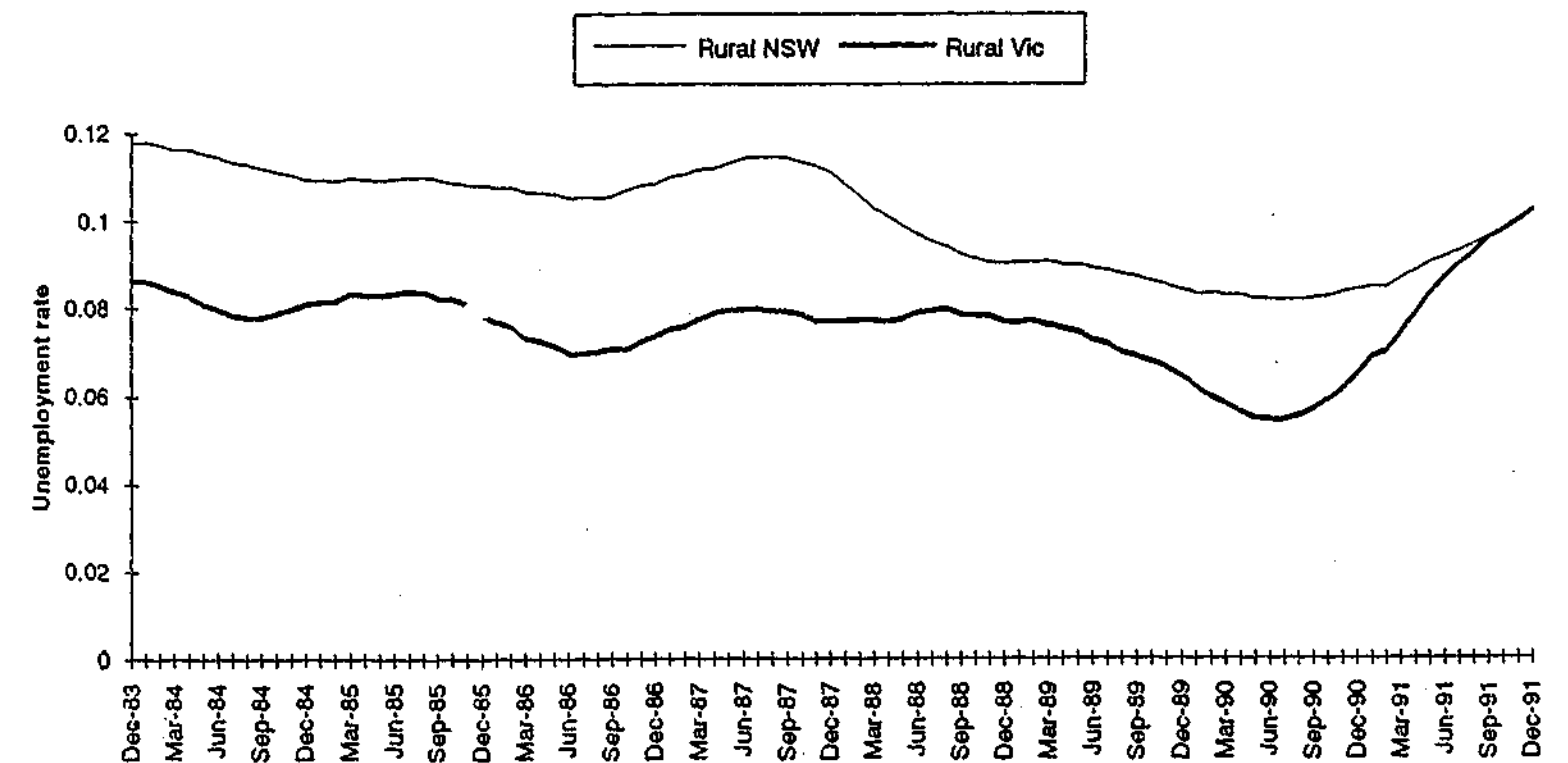


Figure 16
Unemployment Rates - Victoria v. NSW

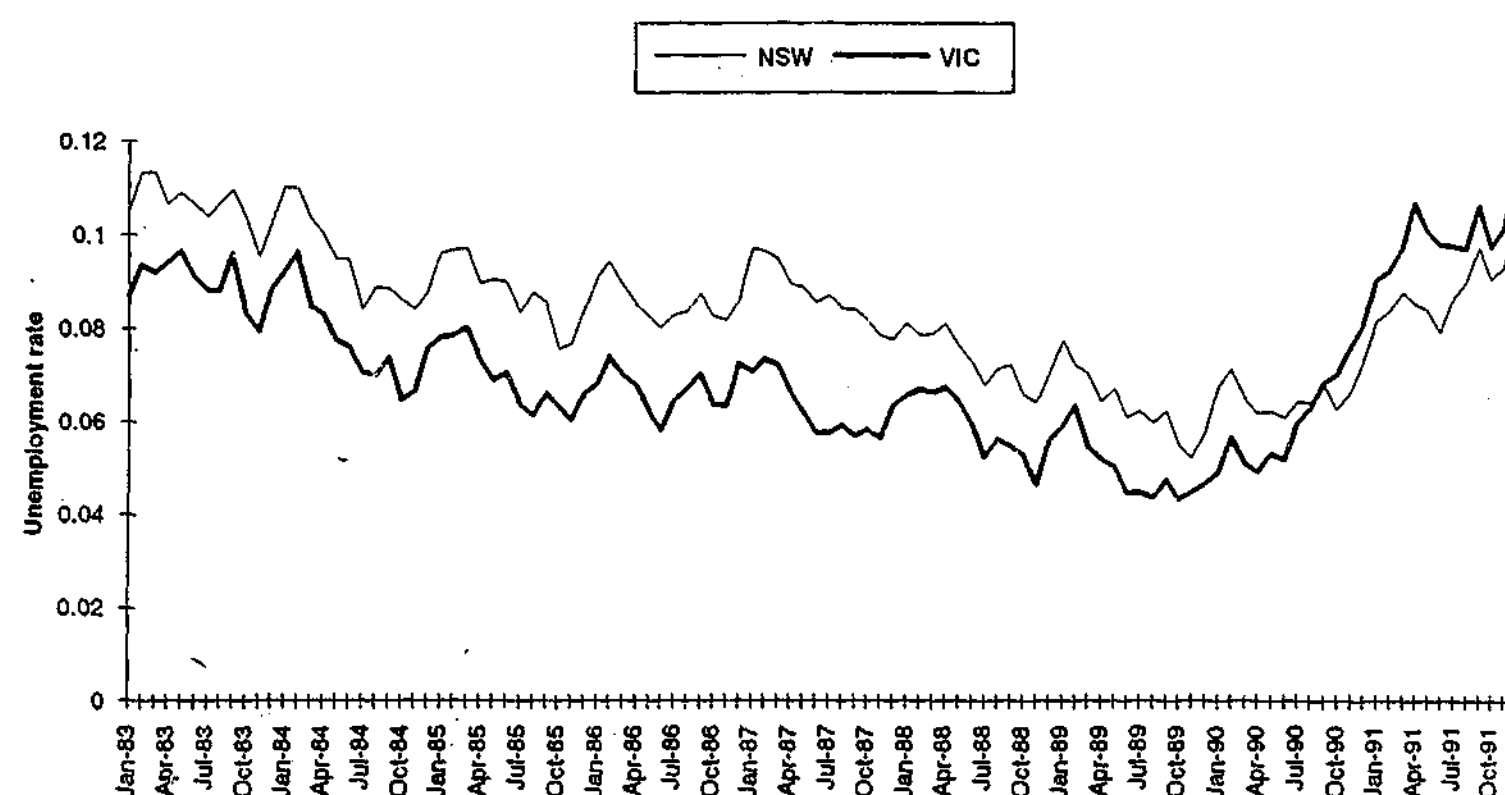
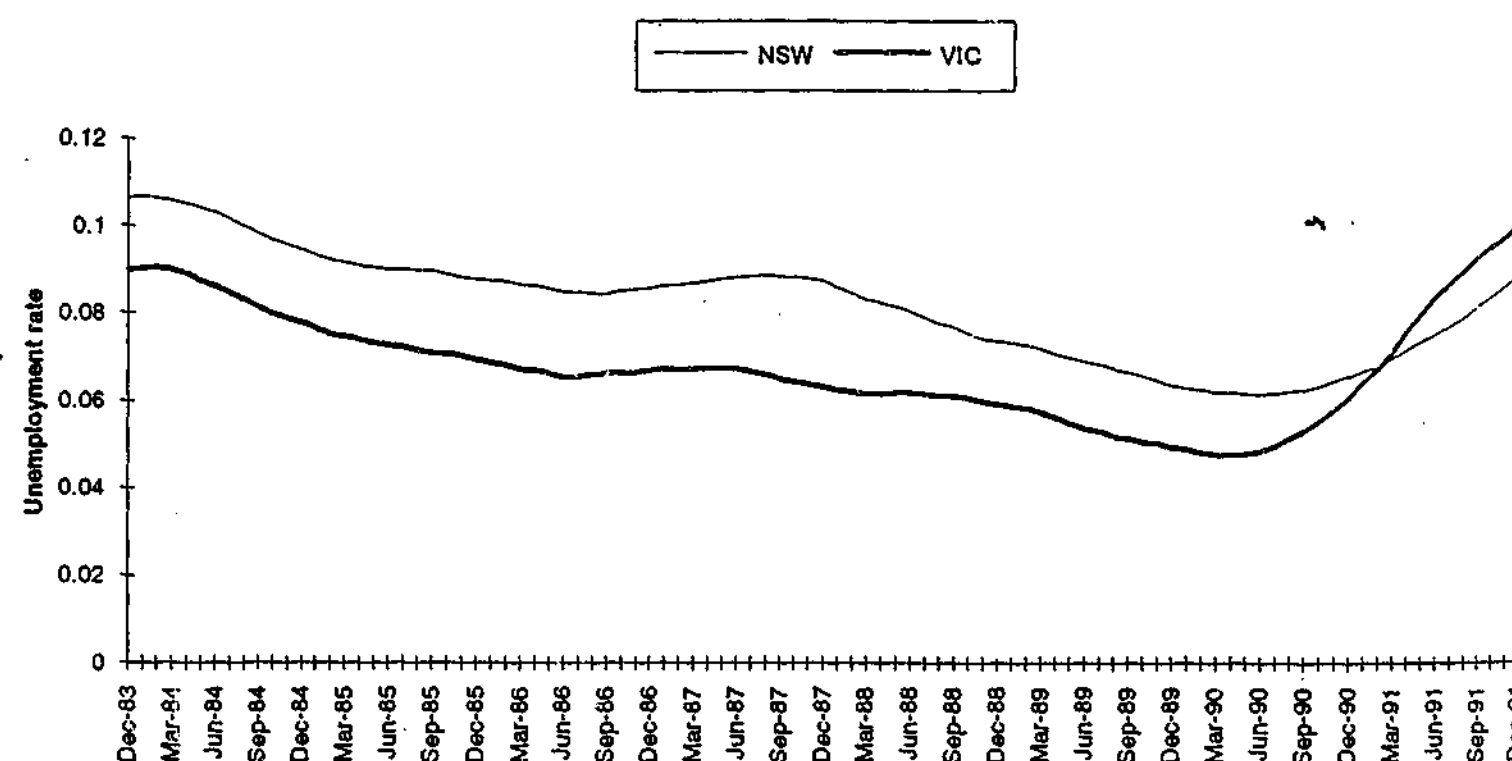


Figure 17
Unemployment Rates - Victoria v. NSW
12 Month Moving Averages



In addition, correlations between estimated total travel and total unemployment, in each State, revealed significant associations between the two measures over the time period to be used in the time series modelling, particularly in the pre-intervention period (see Appendix 1). Unemployment rate provided the higher correlations than number of unemployed persons. It was decided that unemployment rate was the more appropriate measure, controlling for fluctuations in the numbers in the labour force at any one time. Unemployment numbers were highly correlated with rates which means that either measure could be used without changing analysis outcomes. A study of correlations between total estimated travel and unemployment rate with various lags incorporated, showed that zero lag provided the best correlation between the two measures.

In attempting to control for exposure, however, there was a concern that changes in high alcohol hour exposure levels may either be a reflection of other factors (eg. economic), or a product of the initiative⁴, or a combination of both. For this study, it was considered that economic factors could have affected vehicle travel, regardless of the additional effect of the initiative (if any) on travel, and so it was considered necessary to attempt to control for changes in exposure due to changes in economic activity. At the very least, this study assesses reductions (or otherwise) in the risk of serious crash involvement per kilometre travelled, if not crash risk in a more global sense. The RBT initiative could be expected to reduce this risk, by reducing drinking before driving, as well as reducing driving after drinking (both of these behaviour changes would contribute to reductions in the global risk).

The assumption made in using unemployment rate in this study is that this reflects the changes in exposure, due to economic factors, during the high alcohol hours of the week. The relationship between unemployment and travel patterns for different times of day and days of the week is uncertain. Previous research linking unemployment and fatalities does so globally, ie. for all times and not for a subset of times.

4.5.2 Controlling for alcohol consumption patterns

Alcohol consumption was also viewed as a potentially contaminating "other" factor which alters the incidence of drink driving, and therefore the risk of alcohol related crashes. State-wide alcohol sales data provide a surrogate measure of consumption. These data were obtained from ABS publications detailing Liquor Retail Turnover. The data were corrected for inflation using the Alcoholic Beverages sub index of the

⁴ Detering drink driving is by definition an "exposure reducing" approach to this problem. To achieve reductions in drink driving, means to either drive but not drink or drink but not drive. In the latter, some of the options open to a person result in a decrease in travel on the road; for example: a person can get a lift with someone who hasn't been drinking, thereby increasing vehicle occupancy rates and decreasing vehicle travel; alternatively the person can opt to not travel at all by not going out, or staying the night at the place he/she has been drinking or at the non-drinker's home or travel by foot or public transport. Obviously not all deterred drivers will take options which reduce vehicle travel (eg. they may decide to not drink in order to continue to drive, or they may take a taxi). The point is that vehicle travel can change in response to the initiative as well as to other factors influencing the road transport system, and so this should be kept in mind when looking at the issue. Part of the "success" of a drink driving count measure, therefore, might be to reduce high alcohol hour vehicle travel.

Consumer Price Index, thus providing a series reflecting total alcohol consumption (volume of consumption).

Examination of the data for Victoria and NSW since 1983 (Figures 18 & 19) show that the two States have different trends in alcohol consumption. Victorian alcohol consumption has been gradually decreasing over the whole period, particularly since 1988 when the downward trend is more noticeable. On the other hand, alcohol consumption in NSW has not varied greatly, being relatively steady until 1988 and increasing slightly since then.

Whilst the trends are different for the two States, the changes in each are gradual and occur much earlier than the beginning of the initiative. This means that a trend component in the modelling should account for this. On the other hand, given that the trends are different for the two States and the data was available, it was decided to consider it as a variable in each model. Inspection of correlations (Appendix A) with unemployment measures and estimated travel revealed that for Victoria the measures were significantly correlated whilst extremely low correlations were found in NSW, although only during the pre-intervention period. For the combined pre- and post-intervention periods, unemployment no longer correlated with alcohol "consumption" in Victoria.

The correlations found in Victoria between alcohol sales and unemployment rate could violate the assumption of independence between variables and disturb the models developed. Although it was decided to include this variable in the initial modelling, it was recognised that this posed problems. Its ultimate exclusion would not be critical, given that its effects could be accounted for by the trend component of the model.

Figure 18
Alcohol Sales - Victoria v. NSW

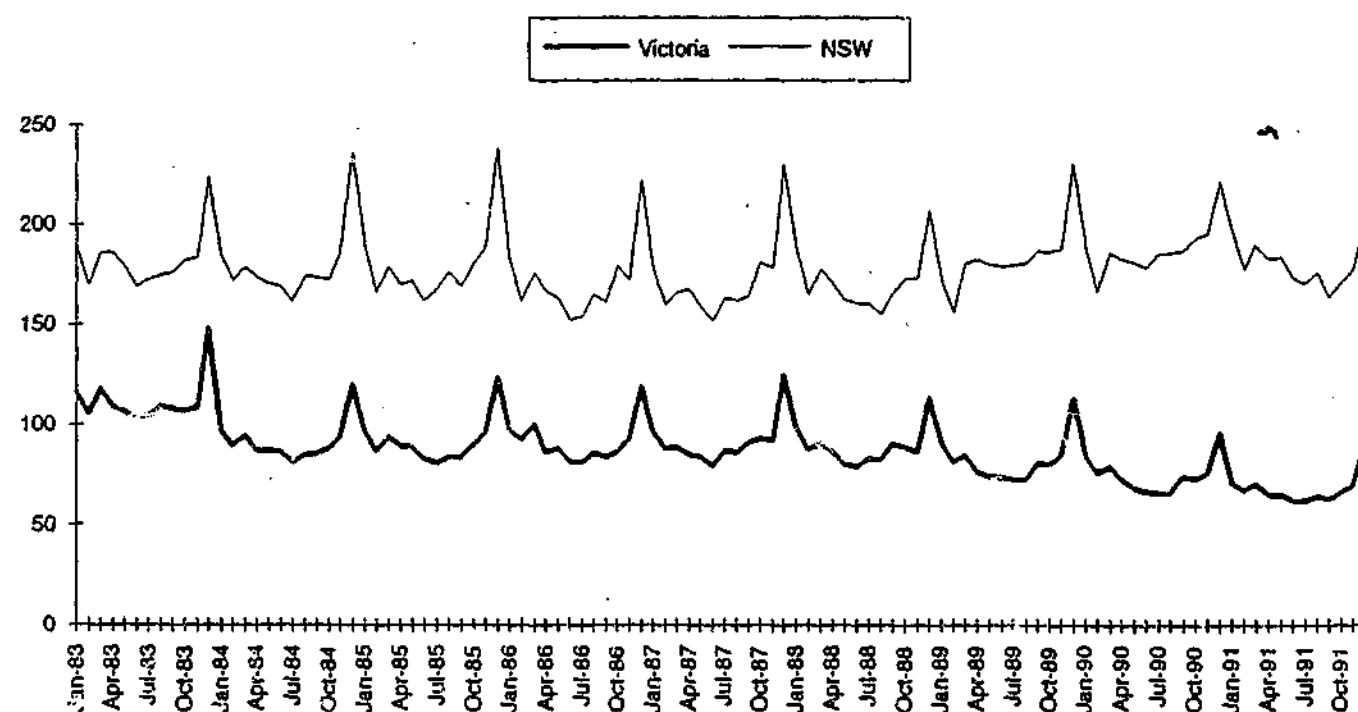
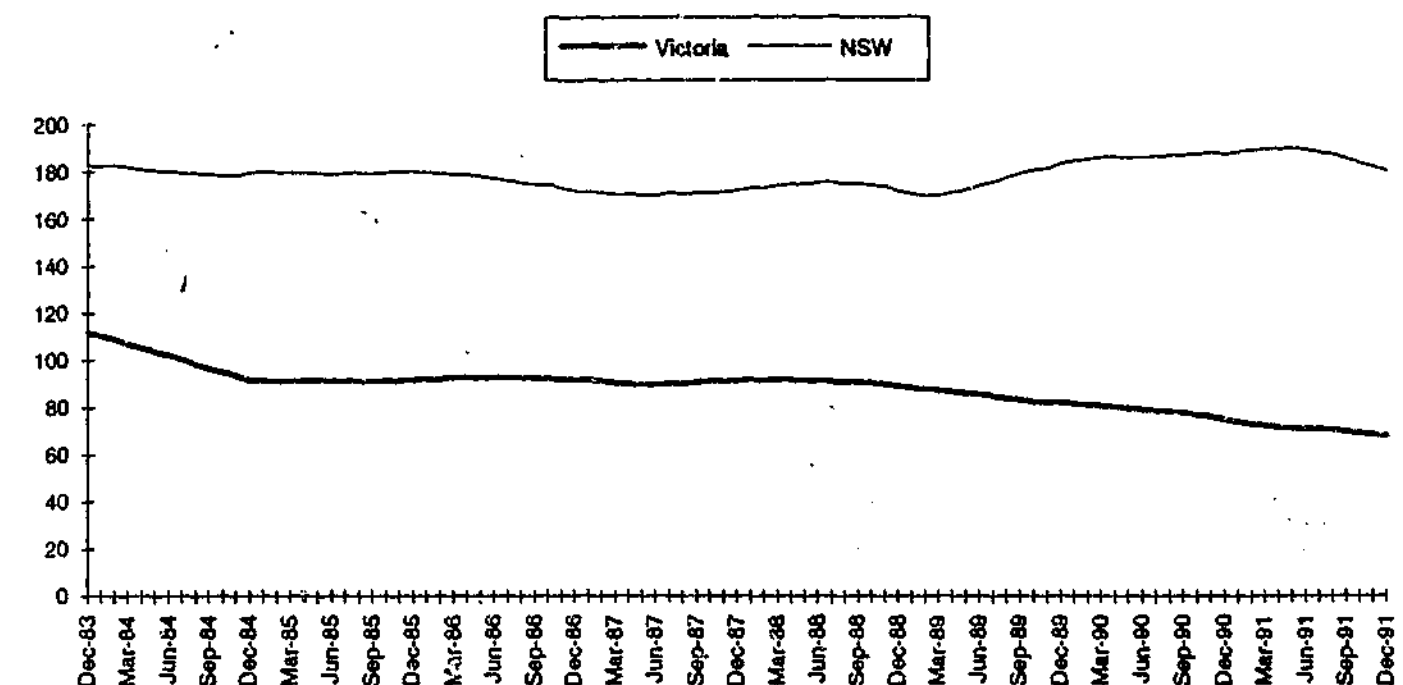


Figure 19
Alcohol Sales - Victoria v. NSW
12 Month Moving Averages



4.5.3 Random Breath Testing activities in NSW

Available data and published material describing RBT in NSW was used to determine whether significant changes had occurred just prior to or during the intervention period so that its role as a comparison area for Victoria could be assessed. Homel (1990) provides a brief chronological description of RBT in NSW indicating that the only major changes have been the introduction of low levels (relative to standard RBT) of mobile RBT since late 1987. RBT is only undertaken from police vehicles and, apart from some bus testing in 1983, has remained a car-based program.

The monthly number of tests conducted by stationary and mobile RBT in NSW since 1986 for the metropolitan rural areas separately, is shown in Figures 20-23. The numbers of tests conducted have remained relatively constant over time. The presence of mobile RBT can be seen from November 1987, increasing gradually over time to a level of around 13,000 tests per month in both the metropolitan and rural areas, by the end of 1991. At this point however they only account for around 1/5 of stationary RBT tests, and far less in earlier periods. There has also been a gradual increase in stationary RBT tests conducted beginning in the last half of 1988, by around 10,000 tests by the end of 1991. A publicity schedule was also obtained (Appendix B) which shows that annual spending on publicity has been around \$600,000 since 1986/87, except for 1989/90 when \$1.4 million dollars was spent. However, the amount spent can reflect the higher cost of the advertising, both in terms of development, rather than more advertising, per se. The schedule also shows that a series of laws relating to drink driving have been introduced over time.

Based on the evidence presented it seems that no major changes took place in NSW, and that any changes in enforcement seem to be gradual over time, and thus should be taken into account by the trend component in the modelling.

Figure 20
NSW Metropolitan RBT
Tests conducted between 1986 to 1991

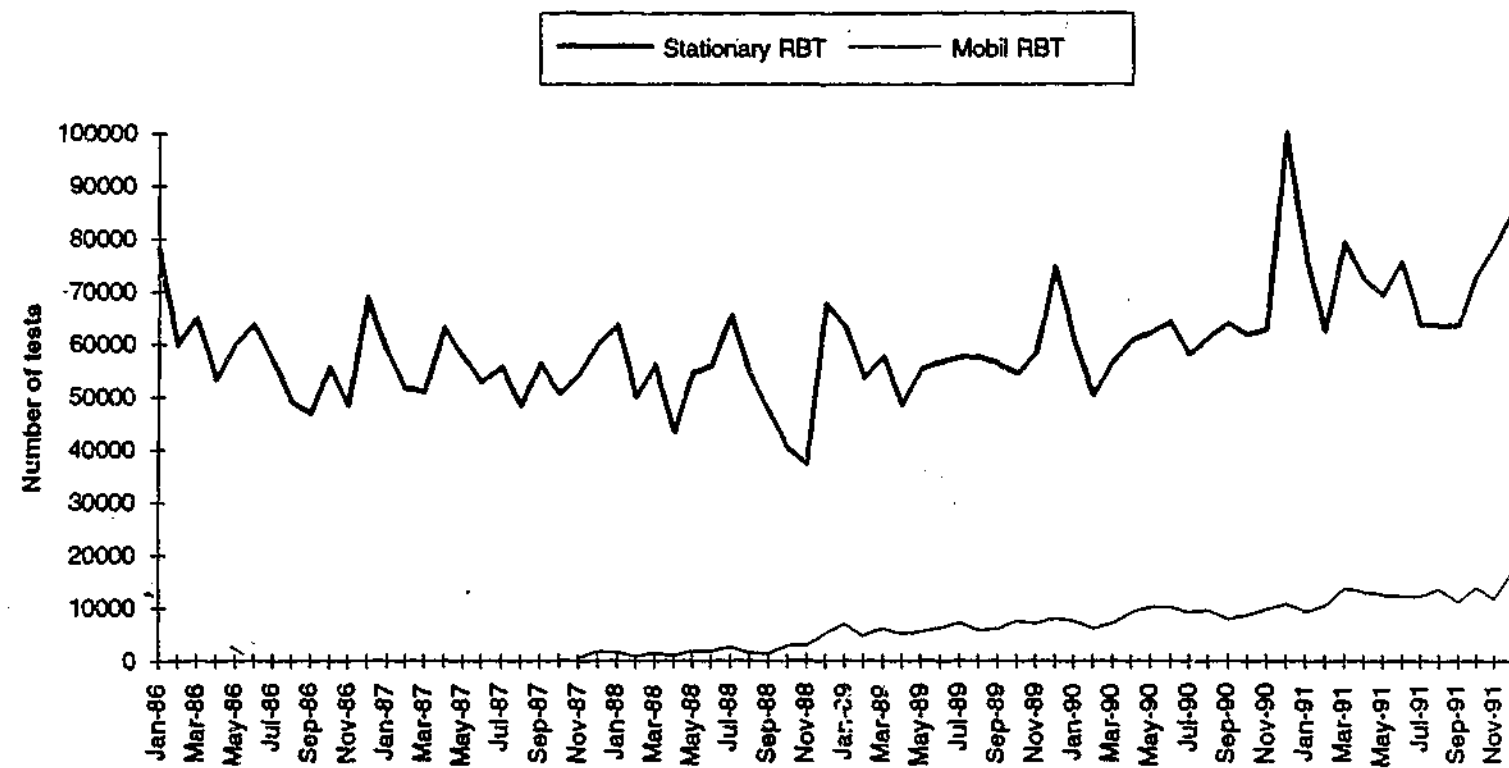


Figure 21
NSW Rural RBT
Tests conducted between 1986 to 1991

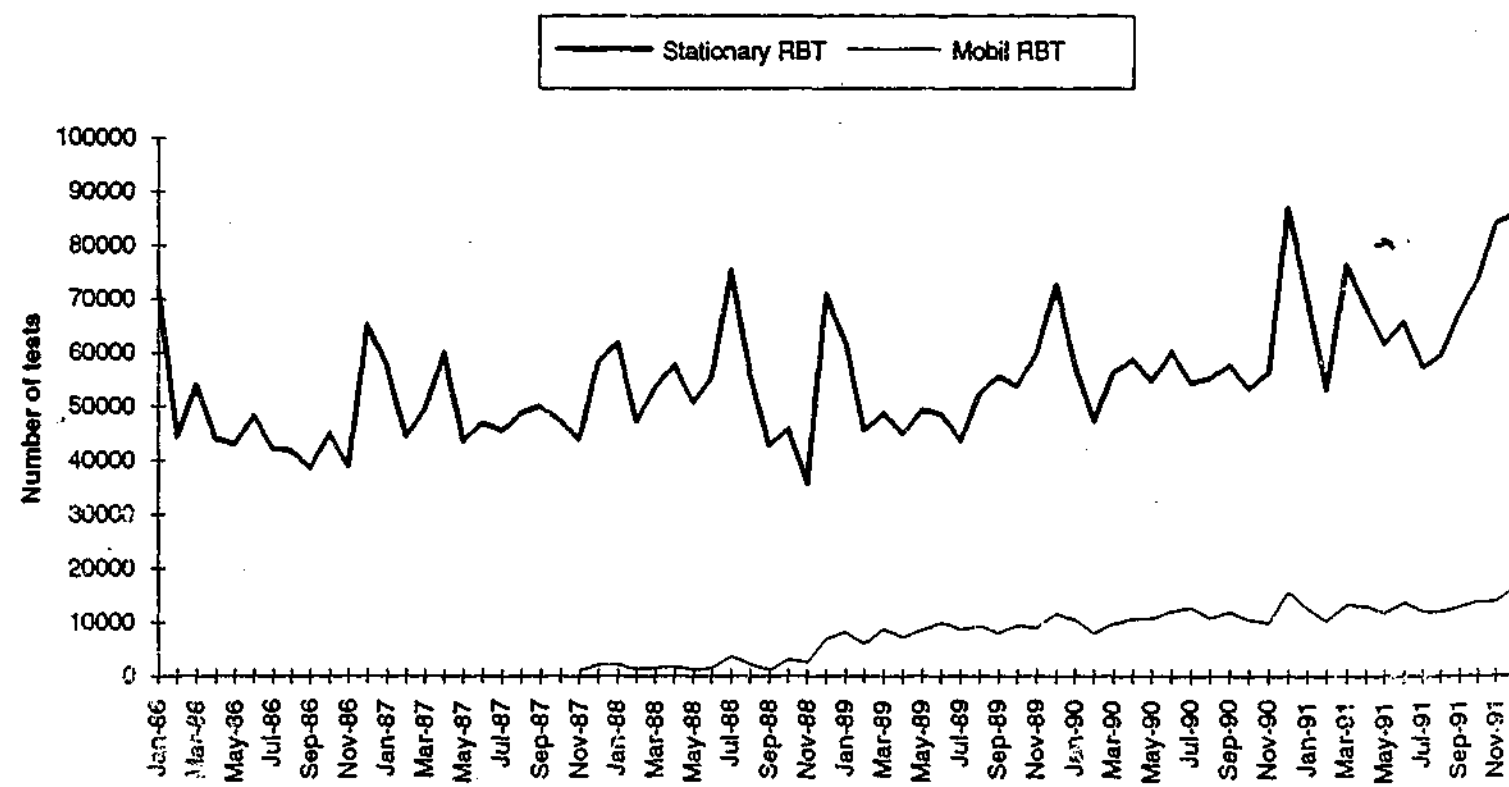


Figure 22
NSW Metropolitan RBT - 12 Month Moving Averages

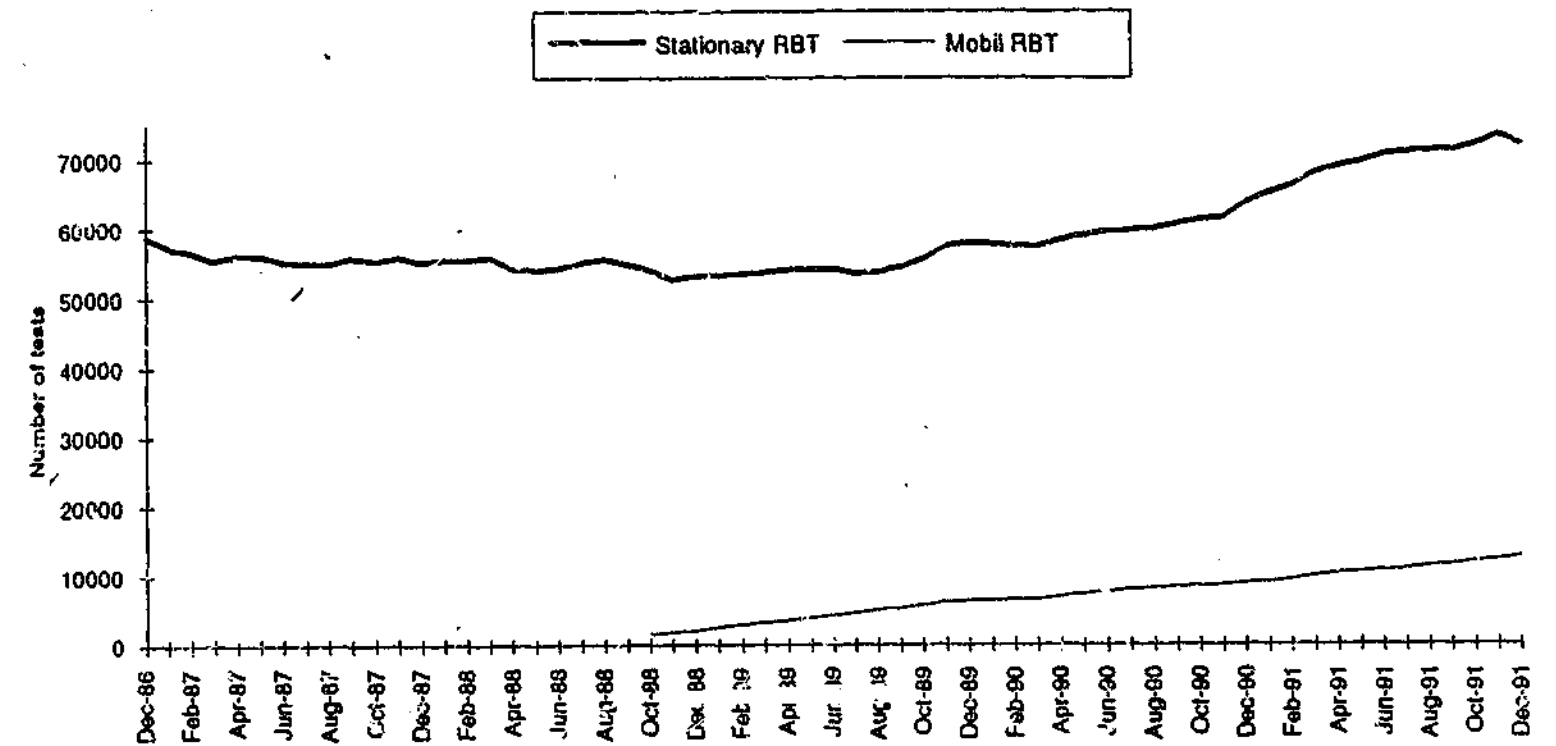
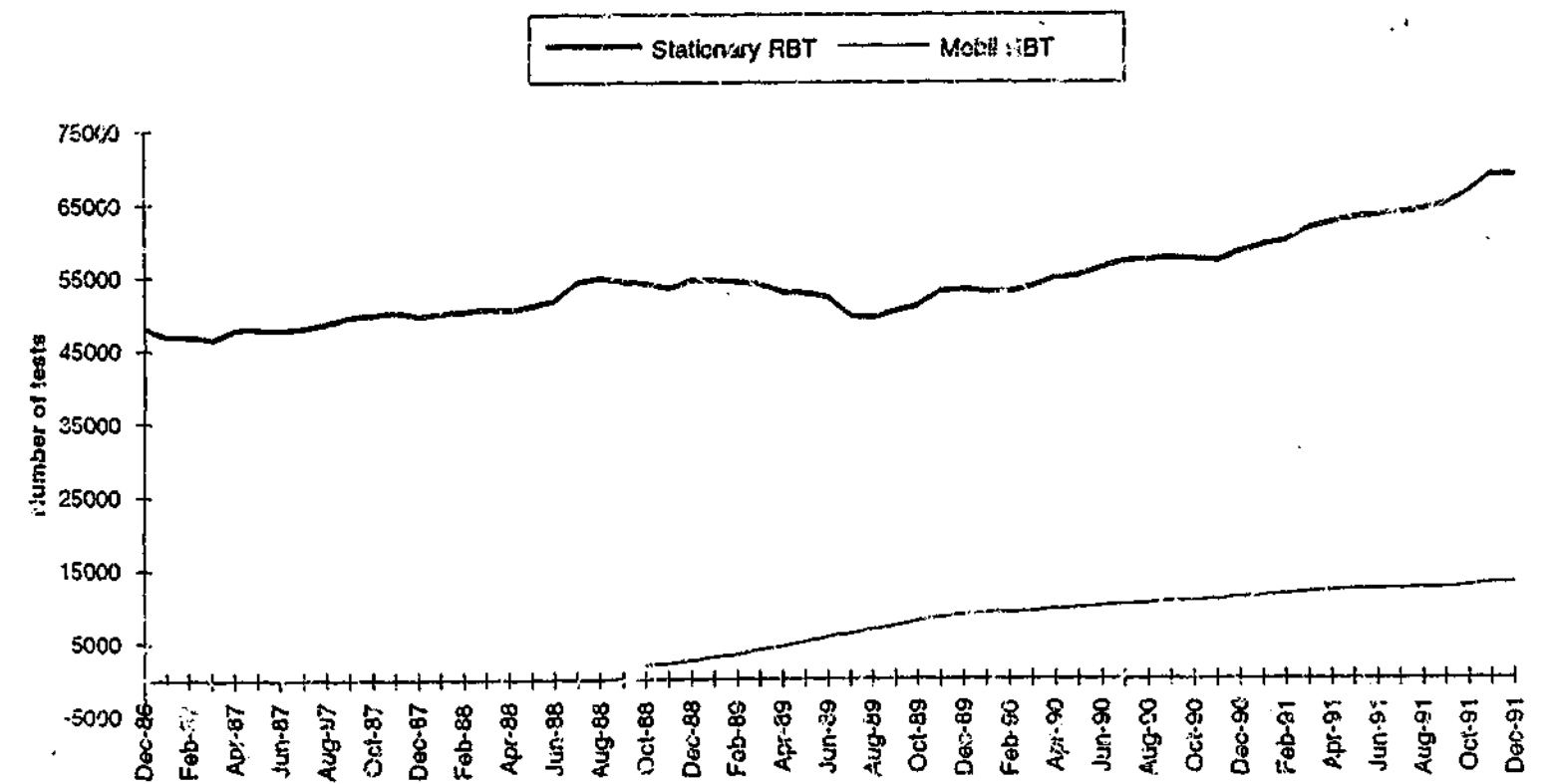


Figure 23
NSW Rural RBT - 12 Month Moving Averages



4.6 Time series methodology for modelling crashes

In this study, the aim of the modelling process was to develop models that closely fit the number of high alcohol hour serious casualty and fatal crashes over the period January 1983 to December 1991, for each series, and to estimate the relationships between the variables in the model (the intervention, covariates) and the crash criteria. A monthly time series was used because monthly data only was available for covariates to be included in the modelling.

It was initially decided to model the time series data using a form of ARIMA modelling known as *Intervention Analysis* (Box and Tiao 1975). This method allows the use of covariates in models that make use of ARIMA techniques for trend and seasonality. The ARIMA methodology provides a good method for estimating the effect of seasonality. A wide variety of trend and other recurring patterns can also be modelled by ARIMA. A sufficient number of data points were available for this type of modelling (a minimum of 5-6 seasons, or 60-72 observations, are required for multivariate ARIMA and multiple regression models (Makridakis, Wheelwright & McGee, 1983) and in the current study 108 observations are available for the years 1983-1991 inclusive).

The approach used was similar to that used by Wagenaar, Streff & Schultz (1990) in estimating the effects of raising the speed limit, from 55 to 65 mph, on Michigan's rural interstate highways. In order to estimate the effect of the intervention, a step function, represented by dummy variables with values 0 (no intervention) and 1 (presence of the intervention) for appropriate months, was used in the model.

4.6.1 Selection of ARIMA models

Treatment of covariates

The data series to be modelled covered the period from January 1983 to December 1991 with the intervention beginning in December 1989. Since the intervention took different forms over the two year period (see section 4.1), it was decided that three dummy variables would be used to cover different parts of that period, so that the different effects could be seen. The three dummy variables used for this purpose were for the periods:

- December 1989 (representing a partial effect, since the intervention commenced in mid-December)
- January to December 1990
- January to December 1991

Before including the covariates in the ARIMA models their correlations were examined. As detailed in section 4.5, estimated Statewide travel (converted fuel sales) correlated highly with unemployment in all areas, but with higher correlations in metropolitan areas, as opposed to rural areas, and for the pre-intervention period. It was also found that serious casualty crashes correlated negatively with unemployment in Melbourne, but showed no real correlation in Sydney or in rural Victoria, and a positive correlation in rural NSW. Low, negative correlations were

found between fatal crashes and unemployment in Melbourne and Sydney, but were again uncorrelated in rural Victoria and positively correlated in rural NSW (although to a lesser extent).

Model structure

It has been found in the past that, in part because of the nature of crash numbers being counts of events, that multiplicative models best model crash data (Hakim, Shefer, Hakkert & Hocherman, 1991; Thoresen et al, 1992). In order to create multiplicative models, natural logarithms of the dependent variable, covariates and dummy variables were taken.

The dummy variables were defined as "1" ($\log_e 1 = 0$) in the non-intervention period and the exponential constant "e" ($\log_e e = 1$) in the intervention period.

The modelling process began with the identification of univariate ARIMA models of the serious casualty crash data, for the **pre-intervention period** (January 1983 to November 1989), in each of the six areas of interest (Victoria, Melbourne, rural Victoria, NSW, Sydney and rural NSW). This was accomplished in two stages. The first stage involved the analysis of autocorrelation and partial autocorrelation functions to estimate possible models. The next stage required the over fitting of those models followed by model reduction to arrive at the best model for each set of crash data.

The models initially identified for each of the six areas all required seasonal components. There were variations by area in the non-seasonal part of the models.

Whilst a model structure with non-seasonal component might be best for making predictions, it was considered inappropriate for explanatory models covering the post-intervention period. The purpose of the covariates in the models was for them to describe the post-intervention changes.

It was also considered necessary for all six models to be identical in structure so that the results would be consistent and comparable. The resultant model structure decided upon was the seasonable ARIMA (0,0,1) (0,1,1)¹² model. In modelling fatal crashes, the diagnostic procedure used for identifying models for serious casualty crashes was repeated. For models of fatal crashes in each of the six areas, the same ARIMA structure as that for serious casualty crashes was found to be the best or very close to the best fit. Therefore the same model structure ARIMA (0,0,1) (0,1,1) was chosen.

The modelling process indicated that the parameter estimates for the alcohol sales covariate was highly correlated with the estimates for unemployment rate. This indicated that the two estimates were describing the same thing and, therefore, that the model had been overfitted. It was decided that the alcohol sales covariate should be excluded from the models, for reasons noted in section 4.5.2.

The final models were ARIMA (0,0,1) (0,1,1)¹² with unemployment rate, and dummy variables for December 1989, January to December 1990 and January to December 1991 as covariates.

4.6.2 Multiple regression models

Since the final ARIMA models included seasonal differencing of the crash data, and therefore used crashes at a lag of 12 months, they model 1991 by taking into account the 1990 decrease in the number of crashes. Thus the estimated impact of the RBT initiative during 1991, using this approach, was under-estimated due to the influence of the initiative during 1990. In order to resolve this problem it was decided to consider alternative techniques to model the data in order to estimate the 1991 change.

In keeping with the previous ARIMA modelling, a multiple regression model with a multiplicative structure was developed for each crash series. This was achieved, again, by taking natural logarithms of each variable in the model. The dependent variable in each model was the number of crashes while the independent variables in the regression equations were made up of:

- a linear trend component
- monthly dummy variables
- the unemployment rate
- a dummy variable for December 1989
- a dummy variable for January to December 1990
- a dummy variable for January to December 1991

The similarity of the 1990 results for the regression models with the corresponding ARIMA model results served as a validation of the ARIMA modelling. As expected, the coefficients of the 1991 dummy variables were all larger in magnitude using the regression models, than for the ARIMA models, since the regression equations do not use lagged crashes in modelling current crashes.

4.6.3 Net effects relative to NSW

Estimated percentage changes in the crash criteria associated with the periods of the initiative were calculated for each of the six areas. The final step was to estimate the net effect for Melbourne and rural Victoria, by calculating the differences between Melbourne vs Sydney and rural Victoria vs rural NSW. This was accomplished by discounting the Victorian percentage change associated with the initiative, by the corresponding NSW percentage change for the same period. (The NSW percentage change represents the effect of other factors not included in the models, and assumed to operate equally in Victoria). Confidence intervals for the net percentage change were also determined.

Figures 24-27 show the models fitted for serious casualty crashes in Melbourne, Sydney, rural Victoria and rural NSW over time. For the post-intervention period, two additional trends are shown: the model for crashes if unemployment rate had remained at 1987-1989 levels, and the model for crashes also including the step functions for the intervention. These charts illustrate the models fitted and the differential role of unemployment in the post-intervention period in Melbourne compared with Sydney.

Figure 24
Model of High Alcohol Hour Serious Casualty Crashes in Melbourne

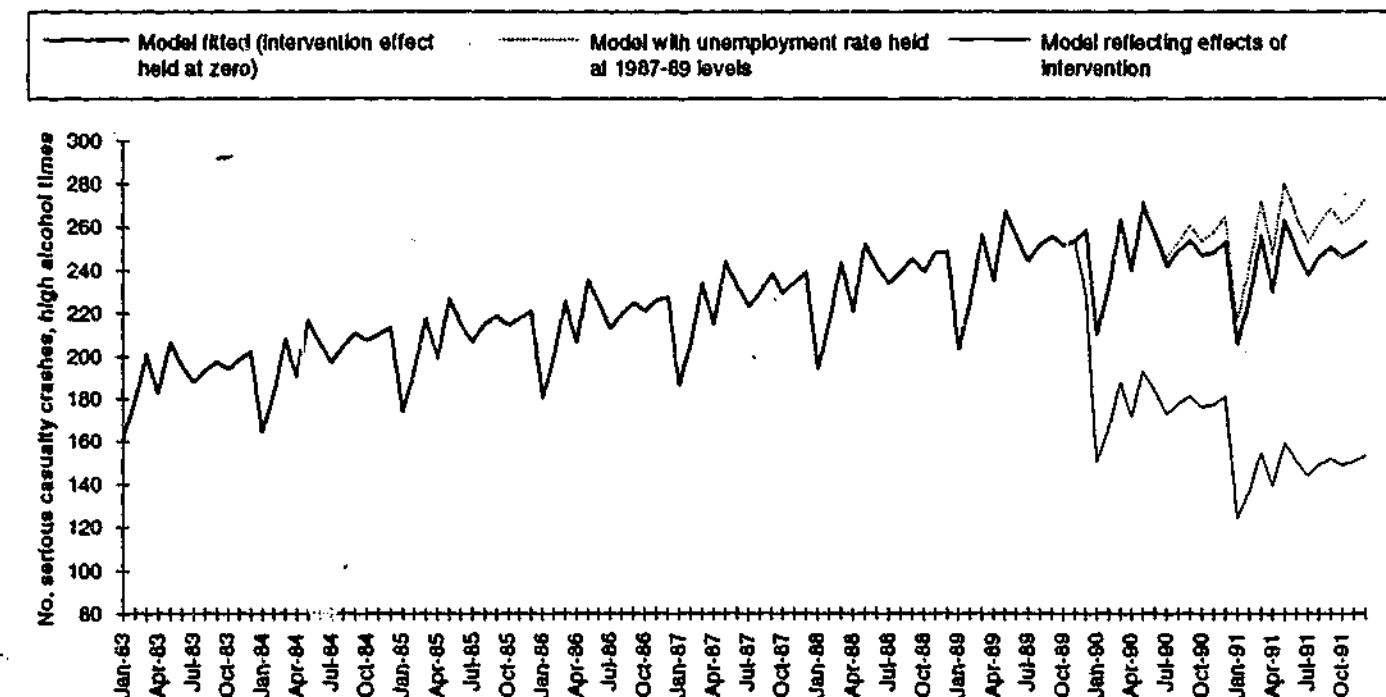


Figure 25
Model of High Alcohol Hour Serious Casualty Crashes in Sydney

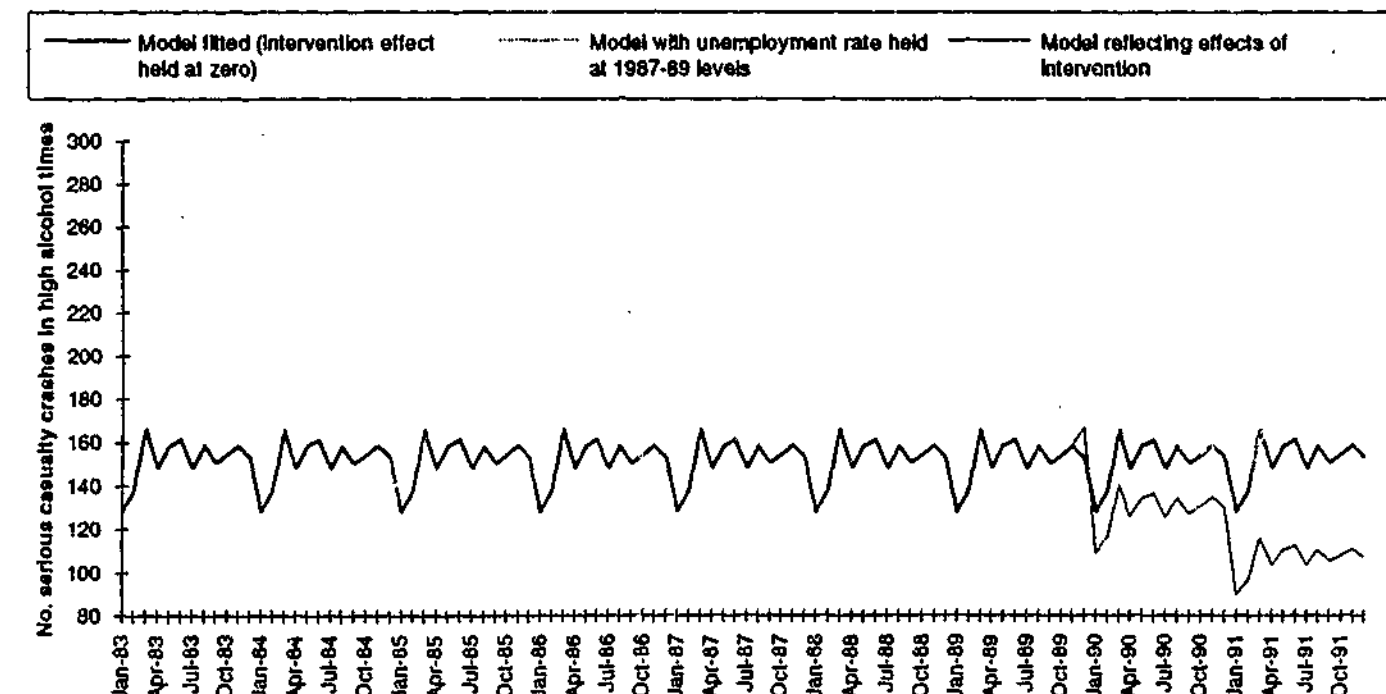


Figure 26
Model of high alcohol hour Serious Casualty Crashes in rural Victoria

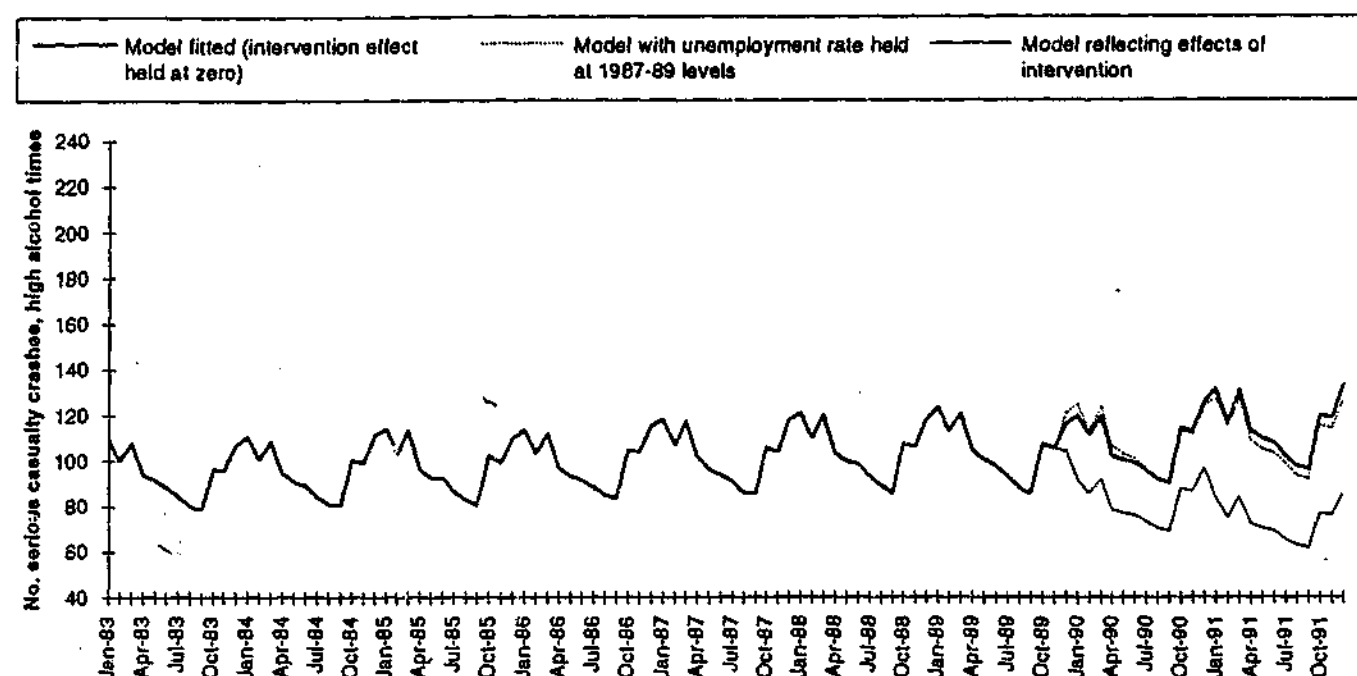
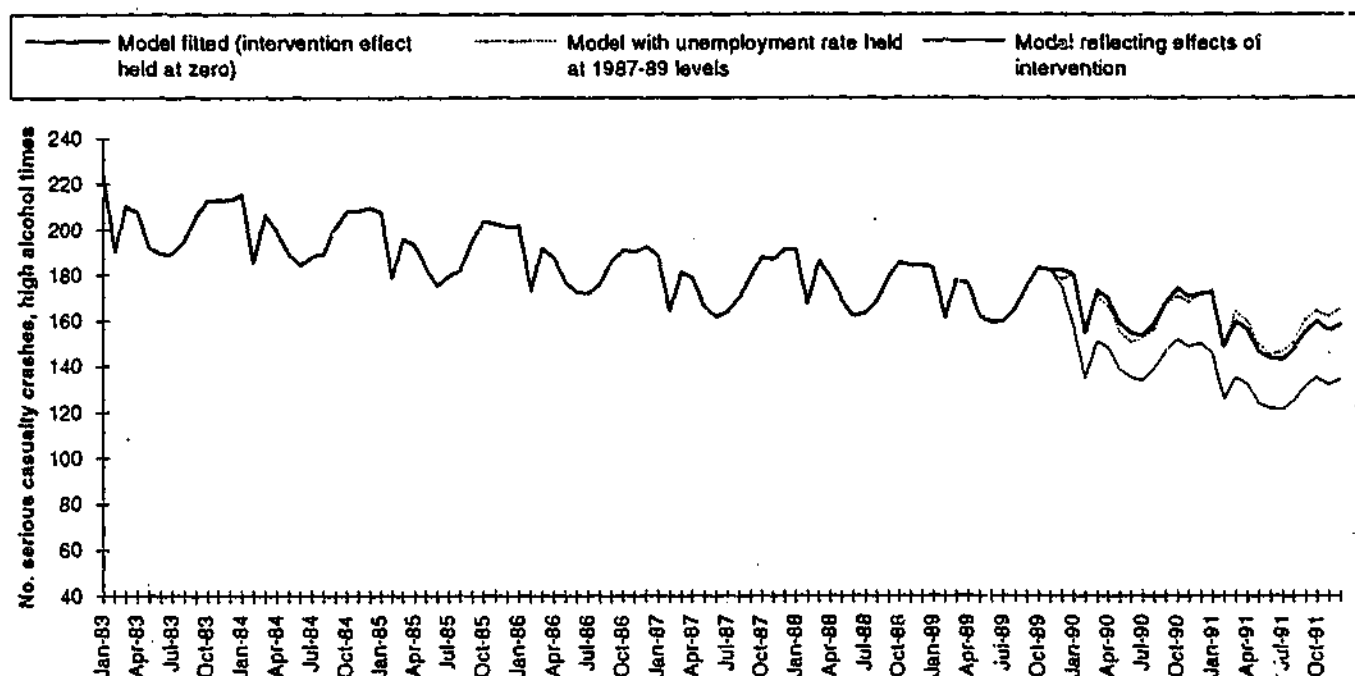


Figure 27
Model of high alcohol hour Serious Casualty Crashes in rural NSW



4.6.4 State-wide analysis

In both the ARIMA and regression modelling, the Statewide Victorian and NSW models were used to validate the use of the unemployment rate as an indicator of vehicle travel. Two models were fitted for each State with one using estimated travel (based on fuel sales) and the other using the unemployment rate. The results obtained were virtually identical regardless of whether estimated travel or unemployment rate was used.

5.0 RESULTS

The final models developed to assess the effects of the RBT initiative in 1990 and 1991 on high alcohol hour serious casualty and fatal crashes in Melbourne and rural Victoria, are shown in Appendix C. Tables 1 and 2 below summarise the results for high alcohol hour serious casualty crashes for each comparison.

Table 1

Percentage changes in high alcohol hour serious casualty crashes in Melbourne, Sydney and net percentage changes in Melbourne in 1990 & 1991

Intervention Period	Percentage change in Melbourne & 95% confidence interval	Percentage change in Sydney & 95% confidence interval	Net change in Melbourne# & 90% confidence interval
1990 (ARIMA model)	-30.6%** (-24.2% to -36.4%)	-15.5%** (-6.2% to -23.9%)	-17.9%* (-7.9% to -26.8%)
(Regression model)	-28.6%** (-21.2% to -35.4%)	-15.3%** (-6.4% to -23.2%)	-15.8%* (-5.3% to -25.1%)
1991 (Regression model)	-39.6%** (-26.3% to -50.4%)	-30.2%** (-15.6% to -42.3%)	-13.4% ns (9.0% to -31.2%)

* Statistically significant at $p < 0.05$ level ** Statistically significant at $p < 0.01$ level

ns not significant at the 0.05 level

ONE-TAILED SIGNIFICANCE TESTING FOR NET REDUCTIONS IN MELBOURNE, CORRESPONDING TO 90% CONFIDENCE INTERVALS

Table 2

Percentage changes in high alcohol hour serious casualty crashes in rural Victoria, rural NSW and net percentage changes in rural Victoria in 1990 & 1991

Intervention Period	Percentage change in rural Victoria & 95% confidence interval	Percentage change in rural NSW & 95% confidence interval	Net change in rural Victoria# & 90% confidence interval
1990 (ARIMA model)	-23.1%** (-15.0% to -30.5%)	-11.8%** (-6.1% to -17.2%)	-12.8%* (-3.7% to -21.1%)
(Regression model)	-23.3%** (-16.9% to -30.3%)	-12.8%** (-6.9% to -18.3%)	-12.0%* (-3.0% to -20.2%)
1991 (Regression model)	-35.9%** (-26.5% to -44.2%)	-15.3%** (-7.5% to -22.5%)	-24.3%* (-13.2% to -34.0%)

* Statistically significant at $p < 0.05$ level ** Statistically significant at $p < 0.01$ level

ns not significant at the 0.05 level

ONE-TAILED SIGNIFICANCE TESTING FOR NET REDUCTIONS IN RURAL VICTORIA, CORRESPONDING TO 90% CONFIDENCE INTERVALS

The results in table 1 show that there have been statistically significant reductions in both Melbourne and Sydney during 1990 and during 1991, over and above what is accounted for by changes in unemployment rate in both cities.

Using Sydney as a comparison area for Melbourne (after controlling for differential changes in unemployment in the two cities), the changes in Sydney were used to estimate the changes that would have occurred in Melbourne had the intervention not taken place. The netted percentage changes show that the initiative was associated with a significant 17.9% reduction in high alcohol hour serious casualty crashes in Melbourne in 1990, but no statistically significant effect in 1991 (non significant 13.4% reduction).

Table 2 shows that there have been statistically significant reductions in both rural Victoria and rural NSW during 1990 and during 1991, over and above what is accounted for by changes in unemployment rate in both areas.

Using rural NSW as a comparison area for rural Victoria (after controlling for differential changes in unemployment in the two cities), the changes there were used to estimate the changes that would have occurred in rural Victoria had the intervention not taken place. The netted percentage changes show that the initiative was associated with a significant 12.8% reduction in 1990 and a significant 24.3% reduction in 1991 in high alcohol hour serious casualty crashes.

Tables 3 and 4 show the results for high alcohol hour fatal crashes for each comparison.

Table 3

Percentage changes in high alcohol hour fatal crashes in Melbourne, Sydney and net percentage changes in Melbourne in 1990 & 1991

Intervention Period	Percentage change in Melbourne & 95% confidence interval	Percentage change in Sydney & 95% confidence interval	Net change in Melbourne# & 90% confidence interval
1990 (ARIMA model)	-37.6%** (-26.8% to -46.8%)	-17.6% ns (8.0% to -37.2%)	-24.2%* (-1.4% to -41.8%)
(Regression model)	-37.6%** (-21.0% to -50.8%)	-14.8%ns (7.4% to -32.4%)	-26.8%* (-3.5% to -44.5%)
1991 (Regression model)	-45.2%* (-12.1% to -65.9%)	-43.2%* (-11.3% to -63.6%)	-3.6%ns (66.4% to -44.1%)

* Statistically significant at $p < 0.05$ level ** Statistically significant at $p < 0.01$ level

ns not significant at the 0.05 level

ONE-TAILED SIGNIFICANCE TESTING FOR NET REDUCTIONS IN MELBOURNE, CORRESPONDING TO 90% CONFIDENCE INTERVALS

Table 4

Percentage changes in high alcohol hour fatal crashes in rural Victoria, rural NSW and net percentage changes in rural Victoria in 1990 & 1991

Intervention Period	Percentage change in rural Victoria & 95% confidence interval	Percentage change in rural NSW & 95% confidence interval	Net change in rural Victoria# & 90% confidence interval
1990 (ARIMA model)	-13.9%ns (12.3% to -33.9%)	-16.1%* (-2.0% to -28.1%)	2.6%ns (32.8% to -20.7%)
(Regression model)	-15.4%ns (9.5% to -34.6%)	-17.9%* (-3.3% to -30.3%)	3.1%ns (33.3% to -20.2%)
1991 (Regression model)	-31.0%ns (0% to -52.4%)	-30.2%** (-12.8% to -44.1%)	-1.2%ns (42.0% to -31.3%)

* Statistically significant at $p < 0.05$ level ** Statistically significant at $p < 0.01$ level

ns not significant at the 0.05 level

ONE-TAILED SIGNIFICANCE TESTING FOR NET REDUCTIONS IN RURAL VICTORIA, CORRESPONDING TO 90% CONFIDENCE INTERVALS

The results in table 3 show that there have been statistically significant reductions in both Melbourne and Sydney during 1990, and in Sydney only during 1991, over and above what is accounted for by changes in unemployment rate in both cities.

Using Sydney as a comparison area for Melbourne (after controlling for differential changes in unemployment in the two cities), the netted percentage changes show that there was a statistically significant 14% decrease in high alcohol hour fatal crashes in Melbourne in 1990, but no statistically significant effect in 1991.

Table 4 shows that there have been statistically significant reductions in high alcohol hour fatal crashes in rural NSW only during both 1990 and 1991, over and above what is accounted for by changes in unemployment rate in both area.

Using rural NSW as a comparison area for rural Victoria (after controlling for differential changes in unemployment in the two cities), the netted percentage changes show that there was apparently no effect of the initiative on high alcohol hour fatal crashes in 1990 or in 1991 (non significant 3% and -1% change, respectively).

5.1 Statewide results

Tables 5 and 6 below show Statewide results for high alcohol hour serious casualty crashes in Victoria and NSW from models which use estimated total vehicle travel and unemployment rate, alternatively, as covariates. Tables 7 and 8 show the same results for high alcohol hour fatal crashes.

Table 5

Percentage changes in high alcohol hour serious casualty crashes in Victoria, NSW and net percentage changes in Victoria in 1990 & 1991, based on models with travel as the covariate

Intervention Period	Percentage change in Victoria & 95% confidence interval	Percentage change in NSW & 95% confidence interval	Net change in Victoria# & 90% confidence interval
1990 (ARIMA model)	-28.4%** (-23.0% to -33.3%)	-13.6%** (-8.3% to -18.7%)	-17.0%* (-10.2% to -23.3%)
(Regression model)	-29.4%** (-24.0% to -34.4%)	-12.8%** (-7.2% to -18.0%)	-19.1%* (-12.5% to -25.3%)
1991 (Regression model)	-42.1%** (-35.4% to -48.2%)	-22.5%** (-16.9% to -27.7%)	-25.3%* (-16.7% to -33.1%)

* Statistically significant at $p < 0.05$ level ** Statistically significant at $p < 0.01$ level

ns not significant at the 0.05 level

ONE-TAILED SIGNIFICANCE TESTING FOR NET REDUCTIONS IN VICTORIA, CORRESPONDING TO 90% CONFIDENCE INTERVALS

Table 6

Percentage changes in high alcohol hour serious casualty crashes in Victoria, NSW and net percentage changes in Victoria in 1990 & 1991, based on models with unemployment rate as the covariate

Intervention Period	Percentage change in Victoria & 95% confidence interval	Percentage change in NSW & 95% confidence interval	Net change in Victoria# & 90% confidence interval
1990 (ARIMA model)	-28.4%** (-23.0% to -33.4%)	-13.3%** (-7.9% to -18.5%)	-17.4%* (-10.5% to -23.8%)
(Regression model)	-30.0%** (-23.5% to -34.0%)	-13.6%** (-8.2% to -18.7%)	-17.8%* (-10.9% to -24.1%)
1991 (Regression model)	-41.3%** (-31.6% to -49.7%)	-22.5%** (-13.4% to -30.6%)	-24.3%* (-11.2% to -35.5%)

* Statistically significant at $p < 0.05$ level ** Statistically significant at $p < 0.01$ level

ns not significant at the 0.05 level

ONE-TAILED SIGNIFICANCE TESTING FOR NET REDUCTIONS IN VICTORIA, CORRESPONDING 90% CONFIDENCE INTERVALS

A comparison of tables 5 and 6 shows that the estimated percentage changes in high alcohol hour serious casualty crashes for the two States and for each time period, which are provided by models using estimated travel are almost identical to those provided by models using unemployment rate as the covariate.

Table 7

Percentage changes in high alcohol hour fatal crashes in Victoria, NSW and net percentage changes in Victoria in 1990 & 1991, based on models with travel as the covariate

Intervention Period	Percentage change in Victoria & 95% confidence interval	Percentage change in NSW & 95% confidence interval	Net change in Victoria# & 90% confidence interval
1990 (ARIMA model)	-28.9%** (-19.7% to -37.1%)	-16.8%** (-5.8% to -26.6%)	-14.6%* (-1.1% to -26.2%)
(Regression model)	-27.3%** (-15.0% to -37.7%)	-15.3%* (-3.9% to -25.4%)	-14.1%ns (1.7% to -27.4%)
1991 (Regression model)	-35.1%** (-18.0% to -48.7%)	-32.2%** (-21.7% to -41.4%)	-4.3%ns (18.8% to -27.5%)

* Statistically significant at $p < 0.05$ level ** Statistically significant at $p < 0.01$ level

ns not significant at the 0.05 level

ONE-TAILED SIGNIFICANCE TESTING FOR NET REDUCTIONS IN VICTORIA, CORRESPONDING TO 90% CONFIDENCE INTERVALS

Table 8

Percentage changes in high alcohol hour fatal crashes in Victoria, NSW and net percentage changes in Victoria in 1990 & 1991, based on models with unemployment rate as the covariate

Intervention Period	Percentage change in Victoria & 95% confidence interval	Percentage change in NSW & 95% confidence interval	Net change in Victoria# & 90% confidence interval
1990 (ARIMA model)	-29.0%** (-19.7% to -37.2%)	-16.5%* (-5.3% to -26.5%)	-14.9%* (-1.4% to -26.6%)
(Regression model)	-27.3%** (-15.0% to -37.8%)	-17.4%** (-6.3% to -27.1%)	-12.0%ns (4.1% to -25.6%)
1991 (Regression model)	-33.7%* (-8.1% to -52.2%)	-37.7%** (-21.7% to -50.5%)	6.4%ns (39.7% to -27.3%)

* Statistically significant at $p < 0.05$ level ** Statistically significant at $p < 0.01$ level

ns not significant at the 0.05 level

ONE-TAILED SIGNIFICANCE TESTING FOR NET REDUCTIONS IN VICTORIA, CORRESPONDING TO 90% CONFIDENCE INTERVALS

Estimated changes in high alcohol hour fatal crashes are similar for models using travel and models using unemployment rate as a covariate. These findings support the decision to use unemployment rate as a covariate for analyses for metropolitan and rural parts of each State.

6.0 DISCUSSION

Time series modelling of high alcohol hour serious casualty and fatal crashes was undertaken to estimate the change in these crashes relating to the RBT initiative during 1990 and 1991, controlling for unemployment rate and changes in the same crash types in NSW.

The results suggest that the initiative resulted in:

- an estimated 18% reduction (between 8% and 27% with 90% confidence) in high alcohol hour **serious casualty crashes in Melbourne** in 1990, but no statistically significant effect during 1991 (the apparent effect was a 13% reduction, but this may have been due to chance);
- an estimated 13% reduction (between 4% and 21% with 90% confidence) in high alcohol hour **serious casualty crashes in rural Victoria** in 1990, and a 24% reduction (between 13% and 34%) in 1991;
- an estimated 24% reduction (between 1% and 42% with 90% confidence) in high alcohol hour **fatal crashes in Melbourne** in 1990, but no statistically significant effect during 1991;
- no statistically significant reduction in high alcohol hour **fatal crashes in rural Victoria** in 1990 or in 1991.

Serious casualty crashes-

The significant reductions in high alcohol hour serious casualty crashes suggest that the initiative was effective in both periods for the rural area, but only effective in 1990 for the metropolitan area, despite an increase in enforcement levels in terms of both tests and hours of operation, and the exclusive use of the new "Booze Buses", in 1991. However, the findings for the 1991 period are less conclusive given that: (a) the effects are those achieved after a year of operation of the initiative in 1990, and cannot reflect the effects that would have been achieved if this part of the initiative had immediately been introduced in 1990, and (b) other (new or changing) factors may mask or weaken the effects of the treatment, further away in time from the beginning of the intervention.

Fatal crashes-

The results for fatal crashes are less informative than those for serious casualty crashes. The larger chance variation (in proportionate terms) for fatal crashes, due mainly to their smaller numbers, makes them more difficult to model. This leads to less precise estimates of changes, as reflected in the extremely wide confidence limits around the estimates. This means that for small effects in particular, it is not possible to detect a statistically significant change even if there has actually been one. It would not be correct to interpret the results as suggesting that for Melbourne in 1991 and for rural Victoria in both 1990 and 1991, fatal crashes remained unaffected by the initiative, compared with the apparently large effect in 1990 in Melbourne.

The pattern of results for both serious casualty and fatal crashes in the metropolitan and rural areas, suggest the following effects of the initiative:

- in Melbourne the initiative was effective in reducing both fatal and serious casualty crashes in 1990 only
- in rural Victoria the initiative was effective in reducing serious casualty crashes in both 1990 and 1991, even though in 1990 the increased use of buses was mainly in the metropolitan area. Changes in actual enforcement were smaller in rural Victoria during this period. This suggests that either low level changes in enforcement and/or that publicity is very effective in such areas for reducing high alcohol hour serious casualty crashes.
- the relatively large reductions, in both fatal and serious casualty crashes in high alcohol hours in Melbourne (in 1990 only) and in serious casualty crashes in rural Victoria suggests that perhaps non-alcohol related crashes during these hours also are affected by the initiative.

Evidence for this interpretation can be found in examining the trend for drivers and riders killed (Drummond, *et al*, 1992). Drivers killed with lower BACs (0.05-0.15%) decreased over the intervention period (and may have been decreasing earlier than this), but those with higher BACs (> 0.15%) remained a stable proportion of the total group of killed drivers and riders. In addition, the small number of drivers/riders killed in the lower BAC group, means that if the initiative had only affected this group, there would have been limited potential for reductions in fatalities. This is not to say that RBT and related drink driving deterrence mechanisms are not necessary to maintain current levels of alcohol related crashes, but large additional benefits in that area may not be achievable.

6.1 Limitations of this research

Effects of the Speed Camera Program

An alternative interpretation of the results is that at least some of the reduction in high alcohol hour crashes was due to the Speed Camera initiative, given its almost simultaneous introduction in Victoria. Low levels of local publicity began in December 1989/January 1990 with the trialling of 4 models of speed cameras at a small number of sites in Melbourne, whilst the official mass media publicity launch occurred in April 1990. Speed cameras were used at substantial levels by July 1990 when the automated TIN processing system commenced. Although the majority of speed camera sessions have been in daylight hours, there was some overlap with high alcohol hours (17.6% for the whole week) particularly on weekends (around 50% in high alcohol hours). Additionally, it is not known if driver responses to speed cameras (if any) are limited to their times of operation or whether speed behaviour has also been modified at night (which is encompassed by high alcohol times). Although there is yet no evidence for generalisation of behaviour beyond specific

times and locations of speed enforcement, the current program is very different to previous enforcement programs.

Unfortunately, the interstate comparison cannot take this into account because speed cameras were not operating in NSW until March 1991. Therefore, the 1990 effect cannot totally be ascribed to the RBT initiative, with a high level of certainty. The estimated effect of the RBT initiative in 1991 might provide a better indication from this point of view, because speed cameras were operating in NSW for most of 1991 (although the use of cameras was completely different there to deployment in Victoria).

Unemployment rate as a measure of high alcohol hour (discretionary) travel

The use of unemployment rate as a covariate to control for changes in high alcohol hour travel cannot be directly validated. Nevertheless Figures 10 and 11 show that there were substantial changes in the trends in travel almost coinciding with the RBT initiative, and that these changes differed between Victoria and NSW. These changes made it imperative that the analysis methods took changes in travel into account. Unemployment rates were considered to affect and be related to changes in discretionary travel (ie. mainly travel during high alcohol hours) and had the additional advantage for the analysis that they were available for metropolitan and rural areas separately.

Interstate control

The absence of an untreated area, time or other crash group in the respective Victorian metropolitan and rural areas to provide for a control group, meant the use of an interstate comparison group was unavoidable. Although the most similar State to Victoria was chosen for this role, it is not known how well it provided control for other (exogenous) factors present over time in Victoria. The most obvious relevant differences between the two States, manifested in unemployment rates and alcohol consumption, were considered in the analysis. Changes in unemployment rates were explicitly taken into account in the analysis of each States' crash data.

However, even after unemployment rates were taken into account, the interstate comparison area experienced statistically significant reductions in serious casualty crashes in 1990 and 1991 (both in Sydney and rural NSW), and statistically significant reductions in fatal crashes in 1990 and 1991 in rural NSW and in 1991 only in Sydney. It is not known the extent to which these reductions were related to safety measures particular to NSW or to "other" factors which were also applicable to Victoria.

7.0 CONCLUSIONS

The findings of the present study indicate that the RBT initiative (in its entirety) resulted in an 18% reduction in high alcohol hour serious casualty crashes and a 24% reduction in high alcohol hour fatal crashes in Melbourne in 1990, but no statistically significant effect during 1991. In rural Victoria, high alcohol hour serious casualty crashes decreased by 13% in 1990 and by 24% in 1991, whilst there was no statistically significant effect for high alcohol hour fatal crashes both in 1990 and 1991.

The conclusiveness of these findings depends on the adequacy of unemployment rate as an indicator of changes in travel during high alcohol hours, the appropriateness of NSW as a comparison area to take into account the effects of "other" factors (other than unemployment rate) influential in Victoria during the intervention period, and the assumption of minimal effects of speed camera operations in Victoria during high alcohol hours.

8.0 RECOMMENDATIONS

Although the evaluation of this program suggests that it has been effective in reducing target crashes, the major questions which will still remain unanswered are:

1. Which parts of the initiative are mainly responsible for increasing deterrence, that is, how did the initiative achieve its effects; for example, was it the real changes in particular aspects of enforcement, and/or the influence of publicity, either on its own, or in conjunction with the changes in enforcement;
2. The influence of the initiative on driver perceptions; what are drivers' perception about the changes in RBT enforcement and do perceived changes in enforcement correspond at all to the reality; do drivers have exaggerated perceptions of the likelihood of being tested;
3. The relative and/or combined value of varying number of hours of operation, tests conducted, frequency of sessions, duration of sessions, distributions of operations over geographical locations and times of day, different levels of RBT conspicuity, and the presence/absence of publicity.

These are important questions given that the quality and nature of RBT enforcement has been changed. An understanding of the mechanisms by which the deterrent effect has been achieved is crucial to identifying further action required. An optimisation study could be undertaken, as described by Drummond *et al.* (1992), to attempt to answer these questions.

As noted in the report, direct measures of vehicle travel for different times of the day and days of the week (eg. through traffic counting programs) are not available and yet such information is central to monitoring the success or failure of road trauma countermeasures. The availability of such data should be improved if more effective road safety evaluations are to be undertaken.

Greater co-ordination between the implementation and evaluation of future road safety programs is required if more definitive assessments are to be made of such initiatives.

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APPENDICES

APPENDIX A**CORRELATIONS BETWEEN VARIABLES**

APPENDIX A

CORRELATIONS BETWEEN VARIABLES

Correlations between estimated total vehicle travel, number of persons unemployed, unemployment rate, alcohol sales and high alcohol hour fatal and serious casualty crashes for Melbourne, Sydney, rural Victoria and rural NSW for:

- pre-intervention period only (January 1983 - November 1989)
- pre- & post-intervention period (January 1983 - December 1990)

Correlations were conducted on log transformed data as this was the form of the data to be used in the modelling (to achieve a multiplicative structure).

Correlations between estimated total vehicle travel and unemployment rates, incorporating monthly lags, are also shown.

GLOSSARY of VARIABLE CODES

LOGSCC	Natural Logarithm of high alcohol hour serious casualty crashes
LOGFC	Natural Logarithm of high alcohol hour fatal crashes
LOGUNEMP	Natural Logarithm of number of unemployed persons
LOGURATE	Natural Logarithm of unemployment rate
LOGTRAV	Natural Logarithm of estimated total vehicle travel
LOGALCS	Natural Logarithm of alcohol sales
LOGD89	Natural Logarithm of December 1989 Intervention (Dummy) Variable = e in Dec 1989 and 1 elsewhere
LOGD90	Natural Logarithm of 1990 Intervention (Dummy) Variable = e in 1990 and 1 elsewhere
LOGD91	Natural Logarithm of 1991 Intervention (Dummy) Variable = e in 1991 and 1 elsewhere

Correlation

[illegible]

RBT Covariate Correlations - Rural Victoria - Jan 1983 to Nov 1989

Correlation

[illegible]

RBT Covariate Correlations - Sydney - Jan 1983 to Nov 1989

Correlation

[illegible]

RBT Covariate Correlations - Rural NSW - Jan 1983 to Nov 1989

Correlation

[illegible]

RBT Covariate Correlations - Melbourne - Jan 1983 to Dec 1991

Correlation									
CORR	LOGSCC	LOGFC	LOGUNEMP	LOGURATE	LOGTRAV	LOGALCS	LOGD89	LOGD90	LOGD91
LOGSCC	1	0.6752	-0.6896	-0.6259	0.0741	0.3326	0.0605	-0.2646	-0.6124
LOGFC	0.6752	1	-0.3745	-0.3062	-0.0893	0.3242	0.1538	-0.3168	-0.4083
LOGUNEMP	-0.6896	-0.3745	1	0.9738	-0.3552	-0.0391	-0.0962	-0.0411	0.6612
LOGURATE	-0.6259	-0.3062	0.9738	1	-0.5427	0.1073	-0.116	-0.1442	0.5327
LOGTRAV	0.0741	-0.0893	-0.3552	-0.5427	1	-0.5331	0.1502	0.3523	0.2409
LOGALCS	0.3326	0.3242	-0.0391	0.1073	-0.5331	1	0.1522	-0.3214	-0.5183
LOGD89	0.0605	0.1538	-0.0962	-0.116	0.1502	0.1522	1	-0.0342	-0.0342
LOGD90	-0.2646	-0.3168	-0.0411	-0.1442	0.3523	-0.3214	-0.0342	1	-0.125
LOGD91	-0.6124	-0.4083	0.6612	0.5327	0.2409	-0.5183	-0.0342	-0.125	1

RBT Covariate Correlations - Rural Victoria - Jan 1983 to Dec 1991

Correlation									
CORR	LOGSCC	LOGFC	LOGUNEMP	LOGURATE	LOGTRAV	LOGALCS	LOGD89	LOGD90	LOGD91
LOGSCC	1	0.5068	-0.2447	-0.1483	-0.1262	0.5597	0.0513	-0.252	-0.4505
LOGFC	0.5068	1	-0.0999	-0.0223	-0.0772	0.3301	-0.0403	-0.1072	-0.2408
LOGUNEMP	-0.2447	-0.0999	1	0.9125	0.0253	-0.2427	-0.2176	-0.1764	0.7012
LOGURATE	-0.1483	-0.0223	0.9125	1	-0.335	0.0044	-0.2599	-0.3703	0.5339
LOGTRAV	-0.1262	-0.0772	0.0253	-0.335	1	-0.5331	0.1502	0.3523	0.2409
LOGALCS	0.5597	0.3301	-0.2427	0.0044	-0.5331	1	0.1522	-0.3214	-0.5183
LOGD89	0.0513	-0.0403	-0.2176	-0.2599	0.1502	0.1522	1	-0.0342	-0.0342
LOGD90	-0.252	-0.1072	-0.1764	-0.3703	0.3523	-0.3214	-0.0342	1	-0.125
LOGD91	-0.4505	-0.2408	0.7012	0.5339	0.2409	-0.5183	-0.0342	-0.125	1

RBT Covariate Correlations - Sydney - Jan 1983 to Dec 1991

Correlation									
CORR	LOGSCC	LOGFC	LOGUNEMP	LOGURATE	LOGTRAV	LOGALCS	LOGD89	LOGD90	LOGD91
LOGSCC	1	0.7213	-0.175	-0.0489	-0.3059	-0.0408	0.0825	-0.2134	-0.5893
LOGFC	0.7213	1	-0.2359	-0.1443	-0.1494	-0.1094	0.1097	-0.0659	-0.4896
LOGUNEMP	-0.175	-0.2359	1	0.9734	-0.5119	-0.0859	-0.1927	-0.3533	0.3624
LOGURATE	-0.0489	-0.1443	0.9734	1	-0.6658	-0.1121	-0.1996	-0.417	0.1878
LOGTRAV	-0.3059	-0.1494	-0.5119	-0.6658	1	0.173	0.1157	0.3496	0.4043
LOGALCS	-0.0408	-0.1094	-0.0859	-0.1121	0.173	1	0.2787	0.1972	0.042
LOGD89	0.0825	0.1097	-0.1927	-0.1996	0.1157	0.2787	1	-0.0342	-0.0342
LOGD90	-0.2134	-0.0659	-0.3533	-0.417	0.3496	0.1972	-0.0342	1	-0.125
LOGD91	-0.5893	-0.4896	0.3624	0.1878	0.4043	0.042	-0.0342	-0.125	1

RBT Covariate Correlations - Rural NSW - Jan 1983 to Dec 1991

Correlation									
CORR	LOGSCC	LOGFC	LOGUNEMP	LOGURATE	LOGTRAV	LOGALCS	LOGD89	LOGD90	LOGD91
LOGSCC	1	0.6717	0.1449	0.3695	-0.5868	0.1569	0.0197	-0.3836	-0.5995
LOGFC	0.6717	1	0.1768	0.2892	-0.4147	0.1069	-0.001	-0.2875	-0.4652
LOGUNEMP	0.1449	0.1768	1	0.9095	-0.4031	-0.1894	-0.1664	-0.4433	0.2385
LOGURATE	0.3695	0.2892	0.9095	1	-0.6946	-0.1776	-0.1635	-0.4716	0.0262
LOGTRAV	-0.5868	-0.4147	-0.4031	-0.6946	1	0.173	0.1157	0.3496	0.4043
LOGALCS	0.1569	0.1069	-0.1894	-0.1776	0.173	1	0.2787	0.1972	0.042
LOGD89	0.0197	-0.001	-0.1664	-0.1635	0.1157	0.2787	1	-0.0342	-0.0342
LOGD90	-0.3836	-0.2875	-0.4433	-0.4716	0.3496	0.1972	-0.0342	1	-0.125
LOGD91	-0.5995	-0.4652	0.2385	0.0262	0.4043	0.042	-0.0342	-0.125	1

Travel v. Unemployment Rate Correlations - Melbourne - Jan 1983 to Dec 1990

1

CORR	KMTRAVEL	LAG_0	LAG_1	LAG_2	LAG_3	LAG_4	LAG_5	LAG_6
KMTRAVEL	1	-0.8336	-0.7808	-0.7196	-0.6552	-0.5811	-0.5316	-0.4886
LAG_0	-0.8336	1	0.9445	0.8805	0.8098	0.7394	0.6845	0.6137
LAG_1	-0.7808	0.9445	1	0.9435	0.8806	0.8068	0.74	0.6857
LAG_2	-0.7196	0.8805	0.9435	1	0.944	0.882	0.809	0.7428
LAG_3	-0.6552	0.8098	0.8806	0.944	1	0.9422	0.8826	0.8105
LAG_4	-0.5811	0.7394	0.8068	0.882	0.9422	1	0.941	0.8818
LAG_5	-0.5316	0.6845	0.74	0.809	0.8826	0.941	1	0.9417
LAG_6	-0.4886	0.6137	0.6857	0.7428	0.8105	0.8818	0.9417	1

Travel v. Unemployment Rate Correlations - Rural Victoria - Jan 1983 to Dec 1990

2

CORR	KMTRAVEL	LAG_0	LAG_1	LAG_2	LAG_3	LAG_4	LAG_5	LAG_6
KMTRAVEL	1	-0.6077	-0.5467	-0.5073	-0.4791	-0.4317	-0.3766	-0.3497
LAG_0	-0.6077	1	0.7885	0.6745	0.6433	0.5286	0.4202	0.3053
LAG_1	-0.5467	0.7885	1	0.7738	0.6846	0.6532	0.544	0.4393
LAG_2	-0.5073	0.6745	0.7738	1	0.7546	0.6614	0.6184	0.5167
LAG_3	-0.4791	0.6433	0.6846	0.7546	1	0.7632	0.6763	0.6339
LAG_4	-0.4317	0.5286	0.6532	0.6614	0.7632	1	0.77	0.6868
LAG_5	-0.3766	0.4202	0.544	0.6184	0.6763	0.77	1	0.78
LAG_6	-0.3497	0.3053	0.4393	0.5167	0.6339	0.6868	0.78	1

Travel v. Unemployment Rate Correlations - Sydney - Jan 1983 to Dec 1990

3

CORR	KMTRAVEL	LAG_0	LAG_1	LAG_2	LAG_3	LAG_4	LAG_5	LAG_6
KMTRAVEL	1	-0.8479	-0.8174	-0.8015	-0.7877	-0.7701	-0.7509	-0.7457
LAG_0	-0.8479	1	0.9557	0.8949	0.849	0.8211	0.7936	0.7632
LAG_1	-0.8174	0.9557	1	0.9551	0.8929	0.8447	0.8159	0.7875
LAG_2	-0.8015	0.8949	0.9551	1	0.953	0.8879	0.8374	0.8054
LAG_3	-0.7877	0.849	0.8929	0.953	1	0.9508	0.8823	0.827
LAG_4	-0.7701	0.8211	0.8447	0.8879	0.9508	1	0.9485	0.8759
LAG_5	-0.7509	0.7936	0.8159	0.8374	0.8823	0.9485	1	0.9456
LAG_6	-0.7457	0.7632	0.7875	0.8054	0.827	0.8759	0.9456	1

Travel v. Unemployment Rate Correlations - Rural NSW - Jan 1983 to Dec 1990

4

CORR	KMTRAVEL	LAG_0	LAG_1	LAG_2	LAG_3	LAG_4	LAG_5	LAG_6
KMTRAVEL	1	-0.8007	-0.7563	-0.7635	-0.7452	-0.7289	-0.7495	-0.7525
LAG_0	-0.8007	1	0.8938	0.8167	0.7743	0.7845	0.7534	0.7205
LAG_1	-0.7563	0.8938	1	0.8941	0.8117	0.7703	0.7799	0.7511
LAG_2	-0.7635	0.8167	0.8941	1	0.8871	0.8092	0.7619	0.7799
LAG_3	-0.7452	0.7743	0.8117	0.8871	1	0.8871	0.8041	0.758
LAG_4	-0.7289	0.7845	0.7703	0.8092	0.8871	1	0.8866	0.8011
LAG_5	-0.7495	0.7534	0.7799	0.7619	0.8041	0.8866	1	0.8855
LAG_6	-0.7525	0.7205	0.7511	0.7799	0.758	0.8011	0.8855	1

APPENDIX B

NEW SOUTH WALES DRINK DRIVING (RBT) CAMPAIGN
DOCUMENTATION OF ADVERTISEMENTS AND LEGISLATION

ROADS AND TRAFFIC AUTHORITY OF NSW
DRINK DRIVING (RBT) CAMPAIGN:
DOCUMENTATION OF ADVERTISEMENTS.

1982-1984.

The random breath testing advertising campaign began in December 1982. RBT aims to reduce drink-driving primarily through deterrence. The underlying theme of the advertising up until Easter 1984 was the probability of arrest and its consequences. This was captured in the slogan 'Will you be under 0.05 or under arrest?' It was concentrated into three major bursts- Christmas launch 1982; Christmas 1983; and an Easter 'booster' 1984.

The total budget for December 1983 to June 1984 was \$1,500,000.

Date	Medium	Name of Ad	Content
Christmas 1982	TV	How will you go? (Please accept our apology)	Introduction to the campaign/Apology for inconvenience of RBT/Rationale for its purpose/Detection of drink-driving
		One in Three	Probability of detection
		Nightmare	More effective and numerous enforcement/High fear
Christmas 1983	TV	One in Three (1)	Probability of detection
		One in Three (2)	as above
		One in Three (3)	as above
		Human Graph	Illustrating graphically how RBT saves lives/benefits/compliments the citizens
	TV + Cinema	Nightmare	More effective and numerous enforcement/High fear
	Radio	One in Three	Probability of detection
Easter 1984	TV	Remember how much RBT? (Pre-Easter)	Twice as much testing this Easter.
		Remember how much RBT? (Post-Easter)	Every police patrol car is operating as a booze bus
		Nightmare (Pre-Easter)	More effective and numerous enforcement ("over 300 vehicles")
		Nightmare (Post-Easter)	"

1982-1984 continued

Date	Medium	Name of Ad	Content
		Driveway	-
		Pub	-
		Disco	-
	Radio	Nightmare	"
		Driveway	-
		Pub	-
		Disco	-
	Press	-	-
	24-sheet poster	Every Highway Patrol Car	-
	Poster	Over 300 Booze Buses	-
	Bus Strips	One in Three	-
		Every Highway Patrol Car	-
	Taxi Backs	Over 300 Booze Buses	-

1984-1987.
A revised advertising strategy took place from Christmas 1984. It had a lower fear appeal and involved the community in promoting social responsibility. The new slogan was 'Stay under 0.05 or get off the road.'
The total budget for 1986-1987 was \$600,000.

Date	Medium	Name of Ad	Content
Christmas 1984	TV	Inevitability	Every police car acting as a booze bus/Inevitability of being tested/To increase perception of RBT activity
		Consequences	Extent of damages to other property a drunk driver can potentially incur/'A \$10,000 drink'
Easter 1985	TV	Family	Unscripted testimonial interviews with real drink drive victims/family of victims. "Social responsibility" theme.
		Ruth	as above
		Rachel	as above
Christmas 1985	TV	Confessions of a drinking driver	Emphasises that you can have more 'fun' if you don't drink-drive home
Easter 1986	TV	Family	as above
		Ruth	as above
		Rachel	as above
Christmas 1986	TV	Inevitability	as above
	Radio	Christmas Present	The Xmas present of a \$1000 fine + possible disqualification for 1 Yr if drive at 0.05 or over
		Suburbs	-
		No Thanks I'm Driving	Advice on strategy
	Press	End of the road for drink driver	-
	Outdoor	All over summer they're all over Sydney	RBT/chance of being caught
		All over summer they're all over the country	as above

1987-1988

This program was based on the dual strategy of deterrence and attitude change.

Deterrence- Informing the public that RBT is even more effective and If you break the law you will get caught.

Attitude Change- Advertising less reliant on police and designed to work more on changing attitudes so as to reinforce the behavioural change that RBT has encouraged.

Also, special communication was aimed at young drivers highlighting the 0.02 limit which applies to them.

The total budget for November 1987 to June 1988 was \$577,400.

1988-89

This campaign particularly addressed the issues of deterrence-through RBT and mobile RBT- and peer group pressure to drink. The total budget for 1988-1989 was \$665,700.

Date	Medium	Name of Ad	Content
September 1987	Bus Backs	Drink drivers queue here	Illustrating penalty for drink-driving - loss of licence
October 1987	TV	Drink Drive 1	RBT/penalties
		Drink Drive 2	
		Drink Drive 3	
	Radio	Drink Drive 1	
		Drink Drive 3	
November 1987	Print	"200 get life"	Success of RBT
	Radio	"On your own"	-
		"Yeah"	-
December 1987	Radio	-	If you leave the car at home you'll have a lot more fun.
		Family	Unscripted testimonial interviews with real drink drive victims/family of victims
		Ruth	as above
		Rachel	as above
		Mobile	Introduction of mobile RBT - added risk
		No thank you	-
	Press	Don't drink and drive	-
		200 get life	-

1987-1988 and 1988-89 continued

Date	Medium	Name of Ad	Content
1987 (non-specific)	Billboards	All over Summer they're all over Sydney	RBT
		You'll have a lot more fun if you leave the car at home	-
	Press + Poster	It's the end of the road for the drunk driver	-
	Leaflet	Don't blow it-A guide to staying under 0.05	Advice and information on drinks/time
	TV	Family	Unscripted testimonial interviews with real drink drive victims/family of victims
		Ruth	as above
		Rachel	as above
Easter 1988	TV	Barbeque	Drink driver/slow reflexes/To address the credibility of 0.05 and correct the misapprehension that it is safe to drive after a few drinks
		Inevitability	Every police car acting as a booze bus/Inevitability of being tested/To correct the misapprehension that there is less RBT
		Mobile RBT	Mobile RBT units/Idea of inevitability of being tested
1988-89 (main periods Xmas + Easter)	TV	Barbeque	as above
	Radio	Mobile Strike Rate	Added risk of being caught drink-driving
		Raspberries	RBT
	Outdoor	"Hand", "Street Sign"	RBT/Mobile RBT
	Press	"Street Sign" (ethnic press)	-

1989-1990

This program carried over some advertising from 1988, the theme being that a driver should not succumb to social pressure, and should say "no" to drink driving. A very different advertising campaign was developed for 1990. T.V and print advertising was based on the concept of "senses poems"-that is, based on an emotion rather than logical sequential thinking. Each word conjures up an image which results in a series of mind images. The message clearly was the dire consequences of drink driving.

The total budget for 1989-1990 was \$1,453,000.

1990-1991

The campaign, on television, focussed on RBT and penalties for being caught drink-driving. Reference was also made to the role of RBT in "keeping people safe".

The total budget for 1990-91 was \$750,000.

Date	Medium	Name of Ad	Content
September 1989	TV	Mobile RBT	Mobile RBT units/Inevitability of being tested
		Drinks Party	"Say no to drink driving"/Provides a role model for resisting peer group pressure
		Barbeque	Don't have to drink much to be impaired, in an emergency
	Radio	Raspberries	RBT
	24 sheet poster	All over Summer:If you think you can avoid RBT	Risk of being caught
	Bus Back	Side Street -Back Lane	Mobile-RBT
	Press	200 Get Life	Success of RBT in reducing the road toll.
1989-1990 (main periods Xmas + Easter)	TV	Dead Kid	Chance of crashing
		Dead Mate	Chance of crashing
		Cop	Mobile RBT + immediate licence suspension for .15 offence
		Cop	RBT and immediate licence suspension for .15 offence
		You Won't Survive	More chance of being caught drink-driving

1989-1990 continued

Date	Medium	Name of Ad	Content
	Radio	Police Sergeant	Penalties for drink driving
		Heapsa Tears	.15 offence - licence cancellation for 3 years
		Girlfriend	Emphasises harsher penalties \$1500 fine + licence cancellation for 3 years if 0.15.
		Thanks	Emphasises that people are realising the importance of not drinking and driving, hence accidents are coming down
		Girl Talk	Danger and stupidity of getting into a car with a drink driver
		Cab	Don't drink and drive-take a cab, b's etc.
	Posters	Dead Mate	as above
		Dead Kid	as above
		Bad Dream	Crash risk
		Paraplegic	Crash risk
	Press	It's not only dangerous to Drink + Drive, it can also cost you a lot	Information on penalties
	Taxi Backs	Stay under 0.05 or catch a cab	Reminder about the risks of being caught drink-driving.
	Superbus	Stay Under 0.05 or catch the bus	as above

Date	Medium	Name of Ad	Content
Dec 1990-Feb 91 March 91	TV	Drink Drive You'll Be Sorry	Chances of being caught by RBT /mobile RBT, penalties "Keeping people safe"
		Don't Drink and Drive	as above
		Drink Drive 0.05 and you'll be sorry	Penalties for driving at .05
		Drink Drive 0.15 and you'll be sorry	Penalties for driving at .15
	Press	Extension of .02 Limit	.02 limit extended to young drivers, and some professional drivers
	Brochure	Cheers without Fears	A guide to party givers
January 1991	Brochure	A guide to staying under the limit	Updated brochure with information about drink-driving and RBT and advice on drinks/time for staying under .02 and .05
April 1991	Brochure	.02 Limit	Brochures for young drivers and professional drivers

APPENDIX 2

LEGISLATION

ALCOHOL

16 December 1968

Introduction of a prescribed concentration of alcohol (PCA) offence of 0.08 grams of alcohol per 100 millilitres of blood. Introduction of chemical testing by police. Police were allowed to test drivers suspected of being under the influence of alcohol, drivers involved in crashes or committing offences.

December 1979

Minimum of 3 months disqualification (1st offence).

July 1980

Police were directed to breath-test crash-involved drivers and drivers committing 4 point offences.

15 December 1980

The PCA was lowered to 0.05.

17 December 1982

Three ranges of PCA offences were introduced, with different penalties for each range. The ranges were low range: .05 to under .08; middle range: .08 to under .15; and high range: .15 and over.

Random Breath Testing was introduced on a trial basis between 17 December 1982 and 16 December 1985.

Legislation was introduced to allow for the compulsory blood testing of drivers and riders of motor and other vehicles, and of pedestrians aged 15 years and over admitted to or treated at a public hospital as the result of a motor accident.

May 1985

A PCA of 0.02 was introduced for learner and provisional licence holders. A special range PCA offence of .02 to under .05 was introduced.

November 1987

Mobile Random Breath Testing was introduced on a statewide basis, after a trial period in parts of NSW.

18 December 1989

Police were given the power to immediately suspend the licence of a motorist charged with a high range PCA offence, refusing a breath test, or refusing to allow a blood sample to be taken.

The .02 PCA was extended to a person who is not the holder of a licence, as defined in the Traffic Act. The penalties for low, middle and high range PCA offences were extended to apply to drivers supervising learners.

January 1991

The .02 PCA was extended to drivers and riders for their first three years of provisional/unconditional licensing while they are under 25; and to drivers of heavy vehicles, public passenger vehicles and vehicles carrying hazardous loads.

July 1991- Under existing provisions of the Motor Traffic Regulations

Introduction of the Driver Assessment Program for high range PCA offenders, on a trial basis, initially in limited areas of NSW.

Offenders within the selected areas will be required to satisfy a medical assessment on alcohol dependency and problems associated with alcohol use before being eligible for re-licensing following their disqualification period.

MODELLING OUTPUT

DRUGS AND DRIVING

3 November 1967

The Poisons Act (1966) amended the definition in the Motor Traffic Act of the word "drug" as referring to the driver of vehicles whilst under the influence of intoxicating liquor or a drug. The new definition included, in addition to certain drugs listed in the Poisons Act, any substance which may be prescribed in the Motor Traffic Regulations. (The relevant drugs were subsequently listed under the Drug Misuse and Trafficking Act.)

On the advice of a committee of experts on this subject, Schedule N was inserted in the Regulations prescribing a number of substances as "drugs".

1 December 1987

Provisions were included for blood and urine testing for drugs.

Under this legislation, a driver suspected of being under the influence of a drug could be stopped and a breath-alcohol test conducted. If this test was negative, the Police could then conduct a roadside assessment. If the driver was still suspected of being impaired by a drug, the driver

Modelling Output Glossary

- ARIMA (0,0,1) (0,1,1)

MA1	Non-seasonal moving average component
SMA1	Seasonal moving average component
LOGURATE	Natural Logarithm of the Unemployment Rate
LOGD_89	Natural Logarithm of December 1989 Dummy Variable = e in Dec 1989 and 1 elsewhere
LOGD_90	Natural Logarithm of 1990 Dummy Variable = e in 1990 and 1 elsewhere
LOGD_91	Natural Logarithm of 1991 Dummy Variable = e in 1991 and 1 elsewhere

- Regression Equation

INTERCEP	Regression Equation Intercept
TR	Linear Trend Component of Regression Equation = 1 in Jan '83, 2 in Feb '83 etc.
LOGURATE } LOGD_89 } LOGD_90 } LOGD_91 }	Same as with ARIMA modelling
LOGFEB	Natural Logarithm of February Dummy Variable = e in February and 1 in other months
LOGMAR	Natural Logarithm of March Dummy Variable = e in March and 1 in other months
LOGAPR	Natural Logarithm of April Dummy Variable = e in April and 1 in other months
LOGMAY	Natural Logarithm of May Dummy Variable = e in May and 1 in other months
LOGJUN	Natural Logarithm of June Dummy Variable = e in June and 1 in other months
LOGJUL	Natural Logarithm of July Dummy Variable = e in July and 1 in other months
LOGAUG	Natural Logarithm of August Dummy Variable = e in August and 1 in other months
LOGSEP	Natural Logarithm of September Dummy Variable = e in September and 1 in other months
LOGOCT	Natural Logarithm of October Dummy Variable = e in October and 1 in other months
LOGNOV	Natural Logarithm of November Dummy Variable = e in November and 1 in other months
LOGDEC	Natural Logarithm of December Dummy Variable = e in December and 1 in other months

e is the exponential constant and $\text{LOG}_e(e)=1$, $\text{LOG}_e(1)=0$.

Melbourne Serious Casualty Crashes

- ARIMA (0,0,1) (0,1,1)

FINAL PARAMETERS:

Number of residuals 96
 Standard error .10297146
 Log likelihood 79.119557
 AIC -146.23911
 SBC -130.85303
 R-squared 0.6160
 Durbin-Watson 1.97
 SSE 1.0657

Variables in the Model:

	B	SEB	T-RATIO	APPROX. PROB.
MA1	-.28162615	.09760473	-2.8853741	.00489229
SMA1	.79376670	.14272139	5.5616521	.00000027
LOGURATE	-.01393053	.00254926	-5.4645500	.00000042
LOGD_89	-.05845129	.07282619	-.8026136	.42431277
LOGD_90	-.36498292	.04476140	-8.1539659	.00000000
LOGD_91	-.21132918	.04742749	-4.4558377	.00002410

- Regression Equation

Root MSE	0.09722	R-square	0.7734
Dep Mean	5.31204	Adj R-sq	0.7336
C.V.	1.83025		
Durbin-Watson	1.55	SSE	0.8602

Parameter Estimates

Variable	DF	Parameter Estimate	Standard Error	T for H0: Parameter=0	Prob > T
INTERCEP	1	4.851564	0.22078753	21.974	0.0001
TR	1	0.002560	0.00095695	2.675	0.0089
LOGURATE	1	-0.093037	0.09179309	-1.014	0.3135
LOGD_89	1	-0.124700	0.10487449	-1.189	0.2375
LOGD_90	1	-0.337535	0.05047094	-6.688	0.0001
LOGD_91	1	-0.503778	0.10107021	-4.984	0.0001
LOGFEB	1	0.105038	0.04635152	2.266	0.0258
LOGMAR	1	0.222649	0.04600058	4.840	0.0001
LOGAPR	1	0.126614	0.04594508	2.756	0.0071
LOGMAY	1	0.247096	0.04589213	5.384	0.0001
LOGJUN	1	0.188336	0.04646864	4.053	0.0001
LOGJUL	1	0.141167	0.04636718	3.045	0.0030
LOGAUG	1	0.168791	0.04637357	3.640	0.0005
LOGSEP	1	0.194715	0.04597226	4.235	0.0001
LOGOCT	1	0.165183	0.04625558	3.571	0.0006
LOGNOV	1	0.178745	0.04632775	3.858	0.0002
LOGDEC	1	0.202990	0.04813676	4.217	0.0001

Sydney Serious Casualty Crashes

- ARIMA (0,0,1) (0,1,1)

FINAL PARAMETERS:

Number of residuals 96
 Standard error .12785761
 Log likelihood 52.677356
 AIC -93.354711
 SBC -77.968622
 R-squared 0.3941
 Durbin-Watson 1.88
 SSE 1.9435

Variables in the Model:

	B	SEB	T-RATIO	APPROX. PROB.
MA1	-.12408873	.09495636	-1.3067974	.19461060
SMA1	.95089431	.57098498	1.6653578	.09931881
LOGURATE	-.00063956	.00290771	-.2199537	.82640535
LOGD_89	-.17259360	.09068436	-1.9032346	.06020639
LOGD_90	-.16815698	.05326165	-3.1571866	.00216794
LOGD_91	-.18687484	.05689664	-3.2844617	.00145669

- Regression Equation

Root MSE	0.12801	R-square	0.5777
Dep Mean	4.96376	Adj R-sq	0.5034
C.V.	2.57891		
Durbin-Watson	1.56	SSE	1.4912

Parameter Estimates

Variable	DF	Parameter Estimate	Standard Error	T for H0: Parameter=0	Prob > T
INTERCEP	1	4.867056	0.33822135	14.390	0.0001
TR	1	0.000004807	0.00126870	0.004	0.9970
LOGURATE	1	0.005359	0.14828649	0.036	0.9713
LOGD_89	1	0.087833	0.14103640	0.623	0.5350
LOGD_90	1	-0.165501	0.05045617	-3.280	0.0015
LOGD_91	1	-0.359762	0.09704526	-3.707	0.0004
LOGFEB	1	0.073670	0.06066677	1.214	0.2278
LOGMAR	1	0.257914	0.06051505	4.262	0.0001
LOGAPR	1	0.145396	0.06042641	2.406	0.0181
LOGMAY	1	0.212481	0.06052980	3.510	0.0007
LOGJUN	1	0.230807	0.06123311	3.769	0.0003
LOGJUL	1	0.144508	0.06166185	2.344	0.0213
LOGAUG	1	0.211855	0.06086683	3.481	0.0008
LOGSEP	1	0.160971	0.06062606	2.655	0.0094
LOGOCT	1	0.186346	0.06215334	2.998	0.0035
LOGNOV	1	0.214063	0.06207093	3.449	0.0009
LOGDEC	1	0.179202	0.06233680	2.875	0.0050

Rural Victoria Serious Casualty Crashes

- ARIMA (0,0,1) (0,1,1)

FINAL PARAMETERS:

Number of residuals 96
 Standard error .13215406
 Log likelihood 48.838958
 AIC -85.677917
 SBC -70.291827
 R-squared 0.4741
 Durbin-Watson 1.99
 SSE 2.0760

Variables in the Model:

	B	SEB	T-RATIO	APPROX. PROB.
MA1	-.03416333	.09338311	-.3658406	.71534222
SMA1	.96413282	.82279905	1.1717719	.24438120
LOGURATE	-.00642710	.00285052	-2.2547109	.02657748
LOGD_89	.01987308	.09488912	.2094347	.83458221
LOGD_90	-.26293670	.05112312	-5.1432050	.00000158
LOGD_91	-.12456866	.05496856	-2.2661802	.02583769

- Regression Equation

Root MSE	0.12744	R-square	0.6379
Dep Mean	4.54120	Adj R-sq	0.5743
C.V.	2.80633		
Durbin-Watson	1.95	SSE	1.4780

Parameter Estimates

Variable	DF	Parameter Estimate	Standard Error	T for H0: Parameter=0	Prob > T
INTERCEP	1	4.943257	0.25760539	19.189	0.0001
TR	1	0.001846	0.00065291	2.828	0.0058
LOGURATE	1	0.104072	0.10306375	1.010	0.3153
LOGD_89	1	-0.110478	0.14507963	-0.761	0.4483
LOGD_90	1	-0.264972	0.04891857	-5.417	0.0001
LOGD_91	1	-0.445009	0.07033762	-6.327	0.0001
LOGFEB	1	-0.097642	0.06013423	-1.624	0.1079
LOGMAR	1	-0.010189	0.06012452	-0.169	0.8658
LOGAPR	1	-0.159236	0.06018086	-2.646	0.0096
LOGMAY	1	-0.195556	0.06017381	-3.250	0.0016
LOGJUN	1	-0.217173	0.06015604	-3.610	0.0005
LOGJUL	1	-0.263914	0.06037013	-4.372	0.0001
LOGAUG	1	-0.316589	0.06025719	-5.254	0.0001
LOGSEP	1	-0.342374	0.06049065	-5.660	0.0001
LOGOCT	1	-0.112268	0.06035880	-1.860	0.0661
LOGNOV	1	-0.129195	0.06041056	-2.139	0.0351
LOGDEC	1	-0.025279	0.06257426	-0.404	0.6872

Rural NSW Serious Casualty Crashes

- ARIMA (0,0,1) (0,1,1)

FINAL PARAMETERS:

Number of residuals 96
 Standard error .08907047
 Log likelihood 86.361983
 AIC -160.72397
 SBC -145.33788
 R-square 0.3985
 Durbin-Watson 1.92
 SSE 0.9350

Variables in the Model:

	B	SEB	T-RATIO	APPROX. PROB.
MA1	.05673160	.0924947	.6133496	.54119229
SMA1	.97078906	1.0628306	.9133996	.36347309
LOGURATE	.01269558	.0020027	6.3393036	.00000000
LOGD_89	-.07793201	.0643810	-1.2104808	.22926372
LOGD_90	-.12562488	.0319727	-3.9291334	.00016723
LOGD_91	-.07438148	.0343464	-2.1656261	.03298429

- Regression Equation

Root MSE	0.08687	R-square	0.7803
Dep Mean	5.15838	Adj R-sq	0.7416
C.V.	1.68402		
Durbin-Watson	2.12	SSE	0.6867

Parameter Estimates

Variable	DF	Parameter Estimate	Standard Error	T for H0: Parameter=0	Prob > T
INTERCEP	1	5.013106	0.24419988	20.529	0.0001
TR	1	-0.003215	0.00062537	-5.141	0.0001
LOGURATE	1	-0.188479	0.11808772	-1.596	0.1139
LOGD_89	1	-0.039844	0.09471408	-0.421	0.6750
LOGD_90	1	-0.137033	0.03316417	-4.132	0.0001
LOGD_91	1	-0.166306	0.04509418	-3.688	0.0004
LOGFEB	1	-0.145710	0.04097379	-3.556	0.0006
LOGMAR	1	-0.048839	0.04119011	-1.186	0.2388
LOGAPR	1	-0.072767	0.04165106	-1.747	0.0840
LOGMAY	1	-0.134416	0.04157392	-3.233	0.0017
LOGJUN	1	-0.162222	0.04200706	-3.862	0.0002
LOGJUL	1	-0.151705	0.04175116	-3.634	0.0005
LOGAUG	1	-0.120914	0.04156580	-2.909	0.0046
LOGSEP	1	-0.053060	0.04111239	-1.291	0.2001
LOGOCT	1	-0.025626	0.04204447	-0.609	0.5437
LOGNOV	1	-0.037069	0.04291336	-0.864	0.3900
LOGDEC	1	0.013687	0.04234902	0.323	0.7473

Melbourne Fatal Crashes

- ARIMA (0,0,1) (0,1,1)

FINAL PARAMETERS:

Number of residuals 96
 Standard error .25122377
 Log likelihood -6.5148663
 AIC 25.029733
 SBC 40.415822
 R-squared 0.4262
 Durbin-Watson 1.73
 SSE 6.8620

Variables in the Model:

	B	SEB	T-RATIO	APPROX. PROB.
MA1	.08361508	.09463138	.8835873	.37927350
SMA1	.79586628	.15950568	4.9895795	.00000296
LOGURATE	-.00870133	.00448542	-1.9399136	.05552162
LOGD_89	.12773307	.18617896	.6863454	.49425995
LOGD_90	-.47121228	.08114683	-5.8069092	.00000010
LOGD_91	-.10940379	.08645189	-1.2654876	.20896250

- Regression Equation

Root MSE	0.23207	R-square	0.5163
Dep Mean	2.77072	Adj R-sq	0.4313
C.V.	8.37587		
Durbin-Watson	2.05	SSE	4.9010

Parameter Estimates

Variable	DF	Parameter Estimate	Standard Error	T for H0: Parameter=0	Prob > T
INTERCEP	1	2.585245	0.52701682	4.905	0.0001
TR	1	0.002384	0.00228422	1.044	0.2994
LOGURATE	1	0.041193	0.21910885	0.188	0.8513
LOGD_89	1	0.300022	0.25033398	1.198	0.2338
LOGD_90	1	-0.472210	0.12047344	-3.920	0.0002
LOGD_91	1	-0.601776	0.24125321	-2.494	0.0144
LOGFEB	1	0.190053	0.11064045	1.718	0.0892
LOGMAR	1	0.507707	0.10980276	4.624	0.0001
LOGAPR	1	0.219912	0.10967027	2.005	0.0479
LOGMAY	1	0.402478	0.10954390	3.674	0.0004
LOGJUN	1	0.334745	0.11092002	3.018	0.0033
LOGJUL	1	0.305925	0.11067783	2.764	0.0069
LOGAUG	1	0.382969	0.11069309	3.460	0.0008
LOGSEP	1	0.369130	0.10973516	3.364	0.0011
LOGOCT	1	0.153938	0.11041144	1.394	0.1666
LOGNOV	1	0.248427	0.11058371	2.247	0.0271
LOGDEC	1	0.298530	0.11490178	2.598	0.0109

Rural Victoria Fatal Crashes

- ARIMA (0,0,1) (0,1,1)

FINAL PARAMETERS:

Number of residuals 96
 Standard error .354081
 Log likelihood -45.795183
 AIC 103.59037
 SBC 118.97645
 R-squared 0.3422
 Durbin-Watson 1.09
 SSE 13.7349

Variables in the Model:

	B	SEB	T-RATIO	APPROX. PROB.
MA1	-.02051423	.09445483	-.2171856	.82855535
SMA1	.96453639	.85334459	1.1303012	.26135254
LOGURATE	.00134100	.00754084	.1778320	.85925466
LOGD_89	.05596892	.25451572	.2163675	.82919097
LOGD_90	-.14949753	.13537852	-1.1036280	.27269664
LOGD_91	-.13658163	.14559656	-.9380828	.35071275

- Regression Equation

Root MSE	0.34303	R-square	0.2478
Dep Mean	2.45377	Adj R-sq	0.1155
C.V.	13.97981		
Durbin-Watson	2.05	SSE	10.7018

Parameter Estimates

Variable	DF	Parameter Estimate	Standard Error	T for H0: Parameter=0	Prob > T
INTERCEP	1	2.956044	0.69339498	4.263	0.0001
TR	1	0.000509	0.00175743	0.290	0.7727
LOGURATE	1	0.129781	0.27741613	0.468	0.6410
LOGD_89	1	-0.400752	0.39051002	-1.026	0.3075
LOGD_90	1	-0.166995	0.13167384	-1.268	0.2079
LOGD_91	1	-0.371101	0.18932739	-1.960	0.0530
LOGFEB	1	-0.272575	0.16186297	-1.684	0.0956
LOGMAR	1	0.081079	0.16183683	0.501	0.6176
LOGAPR	1	-0.028074	0.16198848	-0.173	0.8628
LOGMAY	1	-0.073368	0.16196952	-0.453	0.6516
LOGJUN	1	-0.237881	0.16192167	-1.469	0.1453
LOGJUL	1	-0.300256	0.16249793	-1.848	0.0679
LOGAUG	1	-0.217255	0.16219394	-1.339	0.1838
LOGSEP	1	-0.186966	0.16282233	-1.148	0.2539
LOGOCT	1	-0.381857	0.16246745	-2.350	0.0209
LOGNOV	1	-0.086951	0.16260678	-0.535	0.5941
LOGDEC	1	0.101351	0.16843078	0.602	0.5488

Sydney Fatal Crashes

- ARIMA (0,0,1) (0,1,1)

FINAL PARAMETERS:

Number of residuals 96
Standard error .29781436
Log likelihood -30.310752
AIC 72.621505
SBC 88.007594
R-squared 0.3711
Durbin-Watson 1.89
SSE 10.9162

Variables in the Model:

	B	SEB	T-RATIO	APPROX. PROB.
MA1	-.25115886	.0891458	-2.8173955	.00595055
SMA1	.98463885	2.1228344	.4638321	.64388770
LOGURATE	-.00290666	.0076017	-.3823714	.70308709
LOGD_89	.12120425	.2085524	.5811693	.56257892
LOGD_90	-.19416218	.1385615	-1.4012708	.16457181
LOGD_91	-.42385646	.1477904	-2.8679573	.00514555

- Regression Equation

Root MSE 0.30000 R-square 0.3999
Dep Mean 2.73201 Adj R-sq 0.2943
C.V. 10.98082
Durbin-Watson 1.54 SSE 8.1899

Parameter Estimates

Variable	DF	Parameter Estimate	Standard Error	T for H0: Parameter=0	Prob > T
INTERCEP	1	2.303173	0.79263106	2.906	0.0046
TR	1	0.000041050	0.00297325	0.014	0.9890
LOGURATE	1	-0.056623	0.34751347	-0.163	0.8709
LOGD_89	1	0.301826	0.33052268	0.913	0.3636
LOGD_90	1	-0.159990	0.11824543	-1.353	0.1794
LOGD_91	1	-0.565579	0.22742824	-2.487	0.0147
LOGFEB	1	0.211691	0.14217425	1.489	0.1400
LOGMAR	1	0.350170	0.14181868	2.469	0.0154
LOGAPR	1	0.370383	0.14161096	2.615	0.0104
LOGMAY	1	0.455184	0.14185324	3.209	0.0018
LOGJUN	1	0.461603	0.14350147	3.217	0.0018
LOGJUL	1	0.354546	0.14450625	2.453	0.0161
LOGAUG	1	0.437114	0.14264309	3.064	0.0029
LOGSEP	1	0.502100	0.14207884	3.534	0.0006
LOGOCT	1	0.383795	0.14565807	2.635	0.0099
LOGNOV	1	0.385396	0.14546493	2.649	0.0095
LOGDEC	1	0.352698	0.14608800	2.414	0.0178

Rural NSW Fatal Crashes

- ARIMA (0,0,1) (0,1,1)

FINAL PARAMETERS:

Number of residuals 96
Standard error .23483396
Log likelihood -1.0602232
AIC 14.120446
SBC 29.506536
R-squared 0.3076
Durbin-Watson 2.03
SSE 5.5306

Variables in the Model:

	B	SEB	T-RATIO	APPROX. PROB.
MA1	.06287234	.09563401	.6574266	.51258463
SMA1	.83308715	.16550012	5.0337556	.00000248
LOGURATE	.01331322	.00523944	2.5409633	.01276713
LOGD_89	-.15745519	.17118089	-.9198176	.36012715
LOGD_90	-.17504982	.07888673	-2.2190021	.02900118
LOGD_91	-.11004343	.08418432	-1.3071726	.19448370

- Regression Equation

Root MSE 0.21865 R-square 0.4952
Dep Mean 3.09408 Adj R-sq 0.4065
C.V. 7.06673
Durbin-Watson 2.14 SSE 4.3505

Parameter Estimates

Variable	DF	Parameter Estimate	Standard Error	T for H0: Parameter=0	Prob > T
INTERCEP	1	3.595526	0.61466040	5.850	0.0001
TR	1	-0.001634	0.00157408	-1.038	0.3021
LOGURATE	1	0.202824	0.29723129	0.682	0.4967
LOGD_89	1	-0.182552	0.23839893	-0.766	0.4458
LOGD_90	1	-0.197735	0.08347548	-2.369	0.0200
LOGD_91	1	-0.358864	0.11350378	-3.162	0.0021
LOGFEB	1	-0.009549	0.10313258	-0.093	0.9264
LOGMAR	1	0.156050	0.10367708	1.505	0.1357
LOGAPR	1	-0.010111	0.10483731	-0.096	0.9234
LOGMAY	1	0.074432	0.10464315	0.711	0.4787
LOGJUN	1	0.041951	0.10573338	0.397	0.6925
LOGJUL	1	0.123029	0.10508926	1.171	0.2448
LOGAUG	1	0.075548	0.10462270	0.722	0.4721
LOGSEP	1	0.258096	0.10348145	2.494	0.0144
LOGOCT	1	0.210392	0.10582753	1.988	0.0498
LOGNOV	1	0.154499	0.10801455	1.430	0.1560
LOGDEC	1	0.327562	0.10659410	3.073	0.0028

Victoria (Unemployment) Serious Casualty Crashes

- ARIMA (0,0,1) (0,1,1)

FINAL PARAMETERS:

Number of residuals 96
 Standard error .08872184
 Log likelihood 92.494862
 AIC -172.98972
 SBC -157.60363
 R-squared 0.6671
 Durbin-Watson 1.97
 SSE 0.8254

Variables in the Model:

	B	SEB	T-RATIO	APPROX. PROB.
MA1	-.21289078	.09818086	-2.1683532	.03276980
SMA1	.82855705	.16857966	4.9149289	.00000400
LOGURATE	-.01176104	.00210534	-5.5862957	.00000025
LOGD_89	-.02945531	.06281369	-.4689315	.64025235
LOGD_90	-.33411480	.03725477	-8.9683774	.00000000
LOGD_91	-.18488777	.03965006	-4.6629887	.00001084

- Regression Equation

Root MSE	0.08298	R-square	0.7825
Dep Mean	5.69623	Adj R-sq	0.7443
C.V.	1.45668		
Durbin-Watson	1.67	SSE	.6267

Parameter Estimates

Variable	DF	Parameter Estimate	Standard Error	T for H0: Parameter=0	Prob > T
INTERCEP	1	5.581733	0.20541153	27.173	0.0001
TR	1	0.002821	0.00071789	3.929	0.0002
LOGURATE	1	-0.002642	0.08461583	-0.031	0.9752
LOGD_89	1	-0.139084	0.09036632	-1.539	0.1272
LOGD_90	1	-0.342026	0.03751654	-9.117	0.0001
LOGD_91	1	-0.533164	0.07857491	-6.785	0.0001
LOGFEB	1	0.027182	0.03949572	0.688	0.4931
LOGMAR	1	0.134583	0.03918104	3.435	0.0009
LOGAPR	1	0.021932	0.03915273	0.560	0.5767
LOGMAY	1	0.096068	0.03917977	2.452	0.0161
LOGJUN	1	0.054787	0.03946675	1.388	0.1685
LOGJUL	1	0.005742	0.03951978	0.145	0.8848
LOGAUG	1	0.011412	0.03936593	0.290	0.7726
LOGSEP	1	0.021655	0.03925588	0.552	0.5826
LOGOCT	1	0.067952	0.03943635	1.723	0.0883
LOGNOV	1	0.075050	0.03947081	1.901	0.0604
LOGDEC	1	0.117450	0.04112489	2.856	0.0053

Victoria (Travel) Serious Casualty Crashes

- ARIMA (0,0,1) (0,1,1)

FINAL PARAMETERS:

Number of residuals 96
 Standard error .08754391
 Log likelihood 92.824879
 AIC -173.64976
 SBC -158.26367
 R-squared 0.6786
 Durbin-Watson 1.97
 SSE 0.8208

Variables in the Model:

	B	SEB	T-RATIO	APPROX. PROB.
MA1	-.20251970	.09743429	-2.0785260	.04050788
SMA1	.85895673	.19923626	4.3112470	.00004159
LOGTRAV	.00219089	.00037518	5.8396024	.00000008
LOGD_89	-.03060649	.06150100	-.4976585	.61993837
LOGD_90	-.33340582	.03668760	-9.0876967	.00000000
LOGD_91	-.19082689	.03953517	-4.8267625	.00000569

- Regression Equation

Root MSE	0.08295	R-square	0.7827
Dep Mean	5.69683	Adj R-sq	0.7445
C.V.	1.45613		
Durbin-Watson	1.67	SSE	0.6262

Parameter Estimates

Variable	DF	Parameter Estimate	Standard Error	T for H0: Parameter=0	Prob > T
INTERCEP	1	6.774675	4.47298941	1.515	0.1333
TR	1	0.003167	0.00129291	2.450	0.0162
LOGTRAV	1	-0.080002	0.30156510	-0.265	0.7914
LOGD_89	1	-0.139014	0.08948700	-1.553	0.1238
LOGD_90	1	-0.347915	0.03734029	-9.317	0.0001
LOGD_91	1	-0.547227	0.05649175	-9.687	0.0001
LOGFEB	1	0.022462	0.04270307	0.526	0.6002
LOGMAR	1	0.132218	0.04006187	3.300	0.0014
LOGAPR	1	0.021919	0.03912151	0.560	0.5767
LOGMAY	1	0.096752	0.03920795	2.468	0.0155
LOGJUN	1	0.054803	0.03915471	1.400	0.1650
LOGJUL	1	0.002914	0.04075858	0.071	0.9432
LOGAUG	1	0.010004	0.03961122	0.253	0.8012
LOGSEP	1	0.019407	0.04010121	0.484	0.6296
LOGOCT	1	0.065727	0.04023291	1.634	0.1058
LOGNOV	1	0.074910	0.03930315	1.906	0.0598
LOGDEC	1	0.117866	0.04046355	2.913	0.0045

New South Wales (Unemployment) Serious Casualty Crashes

- ARIMA (0,0,1) (0,1,1)

FINAL PARAMETERS:

Number of residuals 96
 Standard error .08400461
 Log likelihood 97.602259
 AIC -183.20452
 SBC -167.81843
 R-squared 0.4637
 Durbin-Watson 1.86
 SSE 0.7429

Variables in the Model:

	B	SEB	T-RATIO	APPROX. PROB.
MA1	-.04491588	.10084979	-.4453741	.65711862
SMA1	.83399600	.18069815	4.6154097	.00001305
LOGURATE	.00628568	.00189749	3.3126310	.00133213
LOGD_89	-.10763259	.06090237	-1.7672973	.08056820
LOGD_90	-.14289392	.03115903	-4.5859558	.00001463
LOGD_91	-.12814432	.03314174	-3.8665532	.00020849

- Regression Equation

Root MSE	0.08063	R-square	0.7641
Dep Mean	5.76254	Adj R-sq	0.7226
C.V.	1.39915		
Durbin-Watson	1.67	SSE	0.5916

Parameter Estimates

Variable	DF	Parameter Estimate	Standard Error	T for H0: Parameter=0	Prob > T
INTERCEP	1	5.744279	0.24450799	23.493	0.0001
TR	1	-0.001700	0.00077212	-2.201	0.0302
LOGURATE	1	-0.051375	0.11154414	-0.461	0.6462
LOGD_89	1	0.021411	0.08874400	0.241	0.8099
LOGD_90	1	-0.146223	0.03104942	-4.709	0.0001
LOGD_91	1	-0.254694	0.05671177	-4.491	0.0001
LOGFEB	1	-0.055220	0.03805254	-1.451	0.1502
LOGMAR	1	0.082490	0.03801500	2.170	0.0326
LOGAPR	1	0.020107	0.03828746	0.525	0.6007
LOGMAY	1	0.016637	0.03838595	0.433	0.6657
LOGJUN	1	0.013334	0.03890020	0.343	0.7326
LOGJUL	1	-0.022723	0.03902490	-0.582	0.5618
LOGAUG	1	0.021710	0.03854083	0.563	0.5746
LOGSEP	1	0.033708	0.03820897	0.882	0.3800
LOGOCT	1	0.062202	0.03933520	1.581	0.1173
LOGNOV	1	0.070867	0.03978073	1.781	0.0782
LOGDEC	1	0.080132	0.03925859	2.041	0.0441

New South Wales (Travel) Serious Casualty Crashes

- ARIMA (0,0,1) (0,1,1)

FINAL PARAMETERS:

Number of residuals 96
 Standard error .08454747
 Log likelihood 97.374236
 AIC -182.74847
 SBC -167.36238
 R-squared 0.4628
 Durbin-Watson 1.86
 SSE 0.7469

Variables in the Model:

	B	SEB	T-RATIO	APPROX. PROB.
MA1	-.04730381	.10123979	-.4672452	.64145350
SMA1	.82059164	.17011982	4.8236100	.00000576
LOGTRAV	-.00101456	.00031713	-3.1991769	.00190359
LOGD_89	-.10755667	.06159174	-1.7462840	.08417374
LOGD_90	-.14663363	.03085563	-4.7522490	.00000764
LOGD_91	-.12785600	.03323681	-3.8468197	.00022341

- Regression Equation

Root MSE	0.07966	R-square	0.7698
Dep Mean	5.76254	Adj R-sq	0.7293
C.V.	1.38229		
Durbin-Watson	1.71	SSE	0.5774

Parameter Estimates

Variable	DF	Parameter Estimate	Standard Error	T for H0: Parameter=0	Prob > T
INTERCEP	1	-1.478251	4.68579543	-0.315	0.7531
TR	1	-0.002633	0.00087611	-3.005	0.0034
LOGTRAV	1	0.486367	0.31072881	1.565	0.1210
LOGD_89	1	0.033866	0.08596596	0.394	0.6945
LOGD_90	1	-0.136634	0.03126795	-4.370	0.0001
LOGD_91	1	-0.255027	0.03571011	-7.142	0.0001
LOGFEB	1	-0.027644	0.04170778	-0.663	0.5091
LOGMAR	1	0.102782	0.03973030	2.587	0.0113
LOGAPR	1	0.031689	0.03805497	0.833	0.4072
LOGMAY	1	0.026061	0.03784726	0.689	0.4928
LOGJUN	1	0.023355	0.03780990	0.618	0.5383
LOGJUL	1	-0.000819	0.03932573	-0.021	0.9834
LOGAUG	1	0.029842	0.03779842	0.790	0.4319
LOGSEP	1	0.043990	0.03811190	1.154	0.2514
LOGOCT	1	0.078159	0.03841118	2.035	0.0448
LOGNOV	1	0.082532	0.03795975	2.174	0.0323
LOGDEC	1	0.082084	0.03879782	2.116	0.0371

Victoria (Unemployment) Fatal Crashes

- ARIMA (0,0,1) (0,1,1)

FINAL PARAMETERS:

Number of residuals 96
 Standard error .18984644
 Log likelihood 21.131141
 AIC -30.262282
 SBC -14.876193
 R-squared 0.4479
 Durbin-Watson 1.92
 SSE 3.6564

Variables in the Model:

	B	SEB	T-RATIO	APPROX. PROB.
MA1	.04912827	.09998637	.4913496	.62437510
SMA1	.76387717	.15329102	4.9831826	.00000304
LOGURATE	-.00542457	.00367699	-1.4752757	.14362812
LOGD_89	.12627255	.14324603	.8815082	.38039124
LOGD_90	-.34255655	.06269362	-5.4639775	.00000042
LOGD_91	-.13423916	.06651254	-2.0182535	.04654223

- Regression Equation

Root MSE	0.17594	R-square	0.5394
Dep Mean	3.34095	Adj R-sq	0.4584
C.V.	5.26614		
Durbin-Watson	2.11	SSE	2.8169

Parameter Estimates

Variable	DF	Parameter Estimate	Standard Error	T for H0: Parameter=0	Prob > T
INTERCEP	1	3.088082	0.43549988	7.091	0.0001
TR	1	0.000822	0.00152203	0.540	0.5905
LOGURATE	1	-0.074826	0.17939686	-0.417	0.6776
LOGD_89	1	-0.014195	0.19158867	-0.074	0.9411
LOGD_90	1	-0.318511	0.07954006	-4.004	0.0001
LOGD_91	1	-0.411401	0.16658931	-2.470	0.0154
LOGFEB	1	-0.009660	0.08373621	-0.115	0.9084
LOGMAR	1	0.295439	0.08306904	3.557	0.0006
LOGAPR	1	0.095071	0.08300902	1.145	0.2551
LOGMAY	1	0.167609	0.08306635	2.018	0.0466
LOGJUN	1	0.077805	0.08367478	0.930	0.3549
LOGJUL	1	0.014581	0.08378720	0.174	0.8622
LOGAUG	1	0.107672	0.08346103	1.290	0.2003
LOGSEP	1	0.110856	0.08322771	1.332	0.1862
LOGOCT	1	-0.090923	0.08361033	-1.087	0.2797
LOGNOV	1	0.087518	0.08368340	1.046	0.2984
LOGDEC	1	0.211793	0.08719026	2.429	0.0171

Victoria (Travel) Fatal Crashes

- ARIMA (0,0,1) (0,1,1)

FINAL PARAMETERS:

Number of residuals 96
 Standard error .18973004
 Log likelihood 21.148271
 AIC -30.296542
 SBC -14.910453
 R-squared 0.4493
 Durbin-Watson 1.92
 SSE 3.6558

Variables in the Model:

	B	SEB	T-RATIO	APPROX. PROB.
MA1	.04946636	.09995126	.4949048	.62187320
SMA1	.76578040	.15416064	4.9674183	.00000324
LOGTRAV	.00100452	.00067400	1.4903842	.13961947
LOGD_89	.12575344	.14303119	.8792029	.38163292
LOGD_90	-.34175057	.06225858	-5.4892128	.00000037
LOGD_91	-.13690536	.06674744	-2.0510955	.04316509

- Regression Equation

Root MSE	0.17593	R-square	0.5395
Dep Mean	3.34095	Adj R-sq	0.4585
C.V.	5.26578		
Durbin-Watson	2.11	SSE	2.8165

Parameter Estimates

Variable	DF	Parameter Estimate	Standard Error	T for H0: Parameter=0	Prob > T
INTERCEP	1	-0.826691	9.48630982	-0.087	0.9307
TR	1	0.000229	0.00274201	0.083	0.9338
LOGTRAV	1	0.276040	0.63955885	0.432	0.6670
LOGD_89	1	-0.002147	0.18978392	-0.011	0.9910
LOGD_90	1	-0.318170	0.07919123	-4.018	0.0001
LOGD_91	1	-0.432887	0.11980764	-3.613	0.0005
LOGFEB	1	0.001244	0.09056462	0.014	0.9891
LOGMAR	1	0.301557	0.08496316	3.549	0.0006
LOGAPR	1	0.093992	0.08296884	1.133	0.2602
LOGMAY	1	0.166776	0.08315217	2.006	0.0479
LOGJUN	1	0.082569	0.08303925	0.994	0.3227
LOGJUL	1	0.029415	0.08644075	0.340	0.7344
LOGAUG	1	0.115989	0.08400743	1.381	0.1708
LOGSEP	1	0.117482	0.08504658	1.381	0.1705
LOGOCT	1	-0.079668	0.08532590	-0.934	0.3529
LOGNOV	1	0.091570	0.08335406	1.099	0.2749
LOGDEC	1	0.202749	0.08581504	2.363	0.0203

New South Wales (Unemployment) Fatal Crashes

- ARIMA (0,0,1) (0,1,1)

FINAL PARAMETERS:

Number of residuals 96
 Standard error .16798148
 Log likelihood 24.587983
 AIC -37.175966
 SBC -21.789876
 R-squared 0.3868
 Durbin-Watson 1.95
 SSE 3.2980

Variables in the Model:

	B	SEB	T-RATIO	APPROX. PROB.
MA1	-.00659011	.0918726	-.0717309	.94297546
SMA1	.98661746	2.0836330	.4735083	.63699692
LOGURATE	.00599049	.0036883	1.6241733	.10783613
LOGD_89	-.05373107	.1218713	-.4408837	.66035420
LOGD_90	-.18082237	.0644857	-2.8040680	.00618116
LOGD_91	-.23902109	.0690978	-3.4591731	.00083014

- Regression Equation

Root MSE	0.16603	R-square	0.6108
Dep Mean	3.64030	Adj R-sq	0.5424
C.V.	4.56084		
Durbin-Watson	1.95	SSE	2.5085

Parameter Estimates

Variable	DF	Parameter Estimate	Standard Error	T for H0: Parameter=0	Prob > T
INTERCEP	1	3.850318	0.50349749	7.647	0.0001
TR	1	-0.000545	0.00158996	-0.343	0.7324
LOGURATE	1	0.119523	0.22969473	0.520	0.6041
LOGD_89	1	0.026220	0.18274404	0.143	0.8862
LOGD_90	1	-0.190688	0.06393780	-2.982	0.0037
LOGD_91	1	-0.473360	0.11678242	-4.053	0.0001
LOGFEB	1	0.044352	0.07835883	0.566	0.5728
LOGMAR	1	0.209004	0.07828153	2.670	0.0090
LOGAPR	1	0.114974	0.07884258	1.458	0.1482
LOGMAY	1	0.205770	0.07904538	2.603	0.0108
LOGJUN	1	0.209917	0.08010435	2.621	0.0103
LOGJUL	1	0.193120	0.08036114	2.403	0.0183
LOGAUG	1	0.190789	0.07936431	2.404	0.0182
LOGSEP	1	0.329445	0.07868096	4.187	0.0001
LOGOCT	1	0.264395	0.08100011	3.264	0.0015
LOGNOV	1	0.223033	0.08191755	2.723	0.0078
LOGDEC	1	0.316244	0.08084236	3.912	0.0002

New South Wales (Travel) Fatal Crashes

- ARIMA (0,0,1) (0,1,1)

FINAL PARAMETERS:

Number of residuals 96
 Standard error .16790836
 Log likelihood 24.509216
 AIC -37.018432
 SBC -21.632343
 R-squared 0.3871
 Durbin-Watson 1.95
 SSE 3.3024

Variables in the Model:

	B	SEB	T-RATIO	APPROX. PROB.
MA1	-.00837092	.0918022	-.0911842	.92754912
SMA1	.98874004	2.4758658	.3993512	.69958025
LOGTRAV	-.00096662	.0006093	-1.5863271	.11617243
LOGD_89	-.05481092	.1218366	-.4498724	.65388387
LOGD_90	-.18450011	.0637872	-2.8924314	.00479298
LOGD_91	-.23880638	.0692932	-3.4463169	.00086577

- Regression Equation

Root MSE	0.16477	R-square	0.6167
Dep Mean	3.64030	Adj R-sq	0.5493
C.V.	4.52640		
Durbin-Watson	1.94	SSE	2.4707

Parameter Estimates

Variable	DF	Parameter Estimate	Standard Error	T for H0: Parameter=0	Prob > T
INTERCEP	1	-8.917023	9.69301143	-0.920	0.3600
TR	1	-0.003393	0.00181231	-1.872	0.0644
LOGTRAV	1	0.829418	0.64277196	1.290	0.2002
LOGD_89	1	0.014667	0.17782873	0.082	0.9344
LOGD_90	1	-0.166401	0.06468072	-2.573	0.0117
LOGD_91	1	-0.389048	0.07386975	-5.267	0.0001
LOGFEB	1	0.094736	0.08627649	1.098	0.2751
LOGMAR	1	0.243584	0.08218589	2.964	0.0039
LOGAPR	1	0.126401	0.07872031	1.606	0.1118
LOGMAY	1	0.212250	0.07829064	2.711	0.0080
LOGJUN	1	0.212015	0.07821335	2.711	0.0080
LOGJUL	1	0.214569	0.08134899	2.638	0.0098
LOGAUG	1	0.193824	0.07818961	2.479	0.0150
LOGSEP	1	0.342254	0.07883808	4.341	0.0001
LOGOCT	1	0.273848	0.07945716	3.446	0.0009
LOGNOV	1	0.222241	0.07852334	2.830	0.0057
LOGDEC	1	0.318617	0.08025697	3.970	0.0001

Paper No. 10

A.P. Vulcan, M.H. Cameron and L. Heiman,
"Evaluation of mandatory bicycle helmet use
in Victoria, Australia".

Proceedings, 36th Annual Conference of the
Association for the Advancement
of Automotive Medicine,
Portland, Oregon, October 1992.

Paper No. 10

EVALUATION OF MANDATORY BICYCLE HELMET USE IN VICTORIA, AUSTRALIA

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ABSTRACT

On July 1, 1990 a law requiring wearing of an approved safety helmet by all bicyclists (unless exempted) came into effect in Victoria, Australia. Some of the more important steps which paved the way for this important initiative (believed to be the first statewide legislation of its type in the world) are described and its effects are analysed. There was an immediate large increase in helmet wearing rates, although teenagers continued to show lower rates than younger children and adults. Considerable reductions in the number of bicyclists killed or admitted to hospital after sustaining head injuries were found in the first year of the law. Analysis of the injury data also showed smaller reductions in the number of bicyclists killed or admitted to hospital who did not sustain head injuries. Limited surveys indicated a considerable reduction in bicycle use by children since introduction of the law.

THE STATE OF VICTORIA has a population of about 4.4 million and 605 registered vehicles per 1,000 population. Melbourne is the largest city in Victoria, with a population of about 3 million. In 1991 there were 503 persons killed on the road, which represents a fatality rate of 1.2 deaths per 100 million km travelled. Normally bicyclists have represented about 4% of road deaths, although in 1991 only 8 (or 1.6%) were killed.

BACKGROUND TO THE LAW

A climate which favoured promotion of bicycle helmets with an ultimate goal of compulsory use had been created in Victoria during the 1960's and 70's. All motorcyclists were required by law to wear an approved helmet since 1961. The Government had achieved considerable success in reducing road trauma through restrictive legislation supported by publicity and enforcement, eg. compulsory seat belt (1970) and child restraint use laws (1975 and 1981), random breath testing (1976) and engine capacity limits for novice motorcyclists (1979) (Vulcan 1990).

By the mid-1970's, a few pedal cyclists were wearing bicycle helmets of unknown protective performance, although some imported helmets met overseas standards. It was not until 1981 that the first helmet was certified as meeting the 1977 Australian Standard for protective helmets.

Introduction of the mandatory bicycle helmet use law in July 1990 was preceded by a decade of helmet promotion, involving education, mass media publicity, support by professional associations and community groups, consultation with bicycle groups, and financial incentives. Some of these activities are discussed below and in greater detail elsewhere (Wood and Milne 1988, Leicester et al 1991, Vulcan et al 1992).

School Support - In 1980, a bicycle safety education unit (Bike-Ed) for use by students aged 9 - 13 years was developed. Promotion of helmets was achieved through the use of this unit in schools and an associated film on safer riding. From 1983 the Education Department required a helmet to be worn in all state school cycling activities. A few private schools required helmets to be worn when cycling to school.

In 1982, a bulk helmet purchase scheme was established, in one Education Department region, which enabled parents to order helmets through schools at \$30 (approximately 33% discount). All 1,000 helmets underwritten by the scheme were sold and there was demand for more. The pilot scheme clearly demonstrated that demand for helmets could be generated by such discount schemes. It led to many other bulk purchase schemes during the next two years, resulting in at least 20,000 helmets being purchased.

Mass Media Publicity - A publicity campaign was undertaken during 1984, using two television commercials supported by radio and a pamphlet. The campaign was based on extensive research and was targeted at parents of primary school aged children. It emphasised the seriousness of head injury among bicyclists and the protection provided by helmets.

Consultation and Promotion - In order to coordinate and support a wide range of further helmet promotion activities, the Road Traffic Authority established a helmet promotion task force with membership including representatives of bicyclists, motorists, police, education, community safety organisations, helmet manufacturers and retailers, and doctors. In fact the group grew from an earlier one initiated by the Royal Australasian College of Surgeons which had been actively promoting mandatory helmet use and helmet subsidies for several years.

Two Australian studies about the protective aspects of helmets were also useful in promotion (McDermott and Klug 1982, Dorsch et al 1984). Later studies provided further useful information about the effectiveness of helmets in reducing injuries (Wood and Milne 1988, Thompson et al 1989, Williams 1991).

Helmet Rebate Scheme - Early in December 1984, the Minister for Transport announced that the Government would pay a rebate of \$10 to all purchasers of an Australian made, Standards Association approved bicycle helmet. The scheme was to operate for purchases made from 11 to 29 December, which was traditionally the peak time for helmet sales. The television and radio publicity campaign was re-run during this period with the mention of the \$10 rebate added. A total of more than 37,000 rebates were paid and many others purchased helmets but did not bother to claim.

Further \$10 rebate schemes were undertaken in December 1985, 1986 and 1987, and from December to February in 1988 and 1989 resulting in a total of more than 168,000 helmet rebates from all schemes (Leicester et al 1991).

The Parliament - Although the Minister for Transport said in February 1985,

"We are now aiming to get usage rates to at least 40% in all categories so that it will be possible to make the use of bicycle helmets compulsory. The Government hopes to be in that position by July 1."

it took a further 5 years for the law to be introduced. During this period there were many additional promotional activities but probably the single most significant contribution was the report on an Inquiry into Child Pedestrian and Bicycle Safety by the Social Development Committee of the Victorian Parliament.

In their First Report on the Inquiry, tabled in December 1986, the Committee recommended, *inter alia* "that mandatory helmet use by bicyclists be introduced as soon as possible." The Committee also recommended that a report be prepared with a detailed strategy to introduce mandatory helmet usage, including consideration of "the

impact of such legislation on the diverse categories of bicycle users" and "methods of reducing costs of bicycle helmets to users" (Social Development Committee 1986).

The Final Steps - Following extensive canvassing of comment from cyclists and the community generally, and detailed review of the key issues, the Road Traffic Authority developed a strategy in December 1987 which recommended that legislation be introduced to require the mandatory wearing of bicycle helmets, from 1 January 1989.

After further deliberation, the Minister for Transport and the Minister for Police and Emergency Services announced in September 1989, as part of a package of road safety initiatives, that a new regulation requiring cyclists to wear approved helmets would take effect in Victoria from 1 July 1990.

Helmet Standards - Bicycle groups had been pressing for several years for lighter, cooler helmets. The Australian Standard was amended in 1990 by deleting the penetration test which allowed the lighter, soft-shell helmets and larger holes to enable better cooling air circulation (Leicester et al 1991). This overcame another important objection to the widescale use of helmets at all times.

THE LAW

The law has been implemented through the Road Safety Bicycle Helmets Regulations 1990, under the Road Safety Act 1986. In effect, it requires all persons cycling on the road, footpath, separate bicycle path or in a public park to wear a securely fitted approved bicycle helmet. It also applies to bicycle passengers.

Exemptions are available mainly on the basis of extreme difficulty of complying, but in practice they are difficult to obtain and it is understood that fewer than 50 have been granted to date.

The maximum penalty of \$100 is rarely applied as offenders are not normally taken to court, but rather a Bicycle Offence Penalty Notice for \$15 is issued. For children, a Bicycle Offence Report (no monetary penalty) may be sent to the parents.

ENFORCEMENT

The number of Bicycle Offence Penalty Notices increased from 2836 to 19,229 between 1989/90 and 1990/91 (ie. 1 July to 30 June), while Bicycle Offence Reports increased from 1743 to 5028. While it is likely that most of this increase relates to helmet wearing offences, the extent to which other bicycling offences also increased is not known. In the context of a bicyclist population of over 2 million, this could be regarded as a relatively moderate level of enforcement.

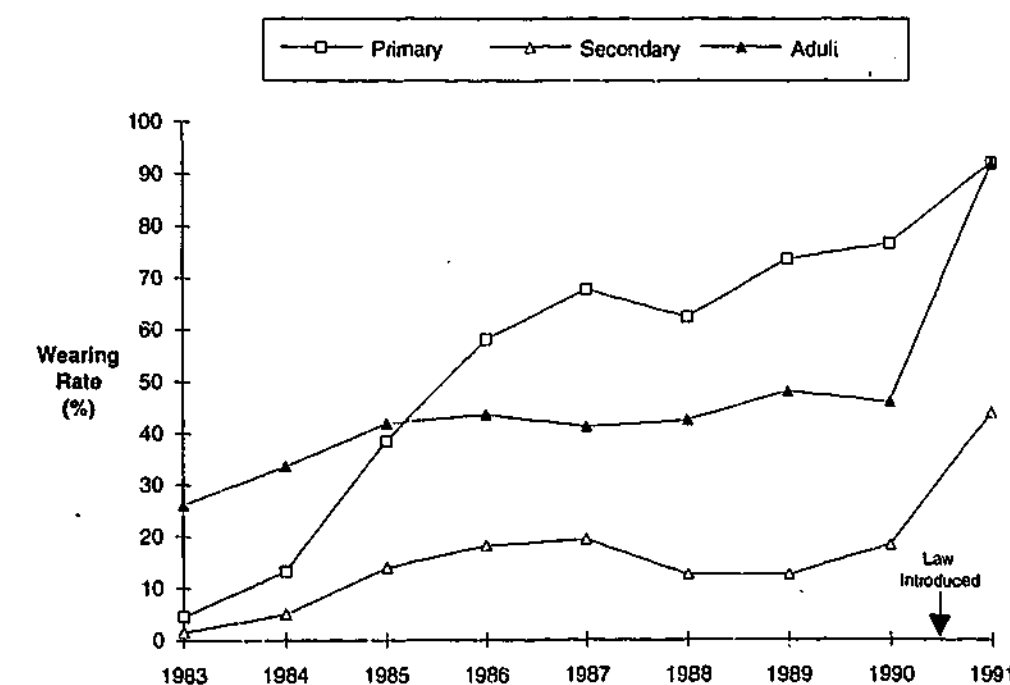
HELMET WEARING

In order to measure progress in helmet wearing, VIC ROADS conducted a series of observation surveys initially in the Melbourne Metropolitan area and later in several regional cities in Victoria.

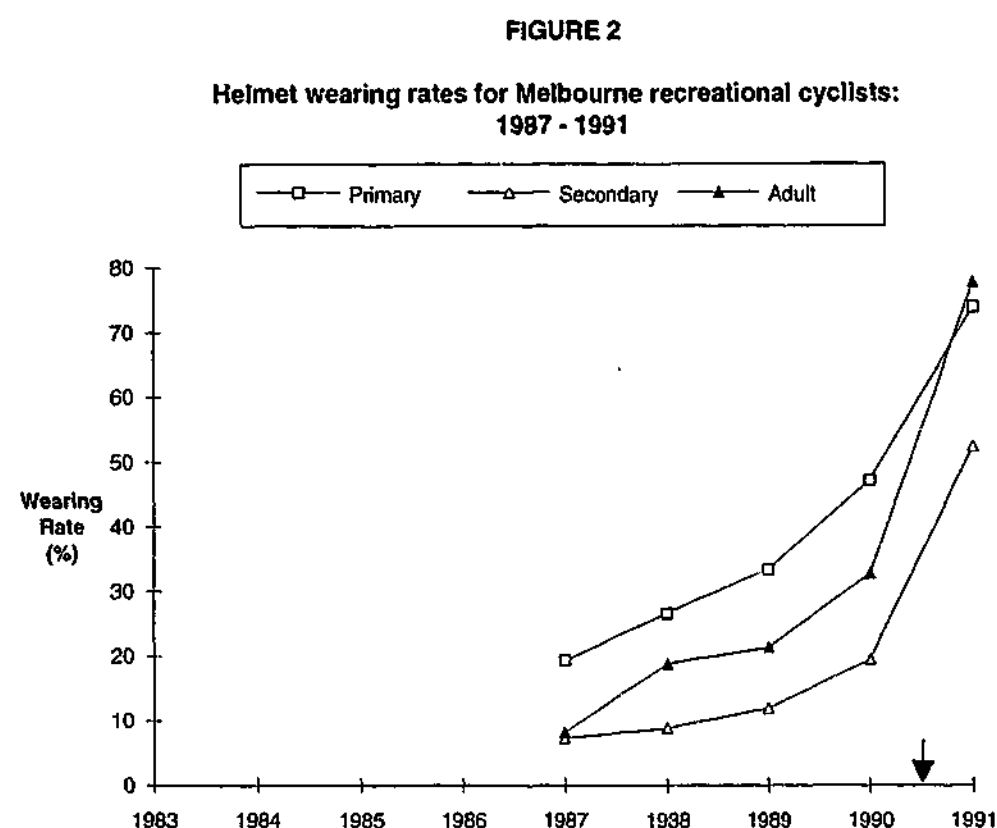
The Melbourne surveys were of adult commuter cyclists on arterial roads near the Central Business District and of primary (aged 5-11) and secondary (aged 12-17) school students on the approaches to a sample of schools. The student surveys could be biased towards higher wearing rates (Heiman 1987) as it is known that some students only wear their helmets when leaving home and on approaching school. Later, recreational riding surveys were also included (Sullivan and Wise 1990, Morgan et al 1991). The results are shown in Figures 1 and 2.

FIGURE 1

Helmet wearing rates for Melbourne commuting cyclists:
1983 - 1991



The surveys were generally taken in March each year, so the 1991 results reflect the situation some eight months after the helmet wearing law was introduced. Some surveys in July and November 1990 reported even higher wearing rates, particularly for secondary school students, but these surveys have not been included because of their limited size.



Estimates of overall wearing rates were produced from the separate survey results in Melbourne by combining the commuting and recreational rates according to the proportions found in observational surveys of bicycle use in Melbourne during 1987/88 and 1991, and combining age groups according to the proportions in these same surveys. Recreational wearing rates prior to 1987 were estimated from the commuter cyclist rates by assuming that the ratio of recreational to commuting wearing rates during 1983-86 was the same as that observed during 1987.

The estimates indicated that helmet use rates by cyclists in Melbourne rose gradually from 6% in March 1983 to 36% in March 1990, and then increased after introduction of the law to 73% in March 1991. The trends in overall wearing rates were used in the interpretation of the changes in cyclist head injuries described in the following section.

The country surveys began in 1985 in 10 regional cities and showed considerable variation between cities (Sullivan and Wise 1990). However the general pattern of wearing rates was similar to that in

Melbourne, with large increases in wearing rates for all age groups following introduction of the law (Table 1).

Table 1: Helmet wearing rates (%) in regional cities of Victoria, during March 1990 and 1991 (from Morgan et al 1991).

	Commuting Cyclists			Recreational Cyclists		
	Age 5-11	Age 12-17	Age 18+	Age 5-11	Age 12-17	Age 18+
1990	58	20	13	26	9	10
1991	95	75	82	80	70	76

The increases in helmet wearing rates after introduction of the law are remarkable, especially as up to the time of the surveys there had been relatively moderate enforcement. They confirm that if the community understands the benefits of a safety measure and a reasonable proportion have been persuaded to adopt it voluntarily, then considerably increased use can be achieved through a law, even with relatively moderate levels of enforcement (as discussed earlier).

BICYCLIST INJURIES

The initial effects of the law on bicyclist head (excluding face) injuries have been measured by examining data from the following sources:

1. Claims for injury compensation from cyclists who were killed or hospitalised (ie. severely injured) following collision with a motor vehicle. The Transport Accident Commission (TAC) is the sole insurer for such claims in Victoria.
2. Health Department records of acute presentations by bicyclists to all public hospitals in Victoria resulting in admission, including admissions from crashes not involving a motor vehicle.

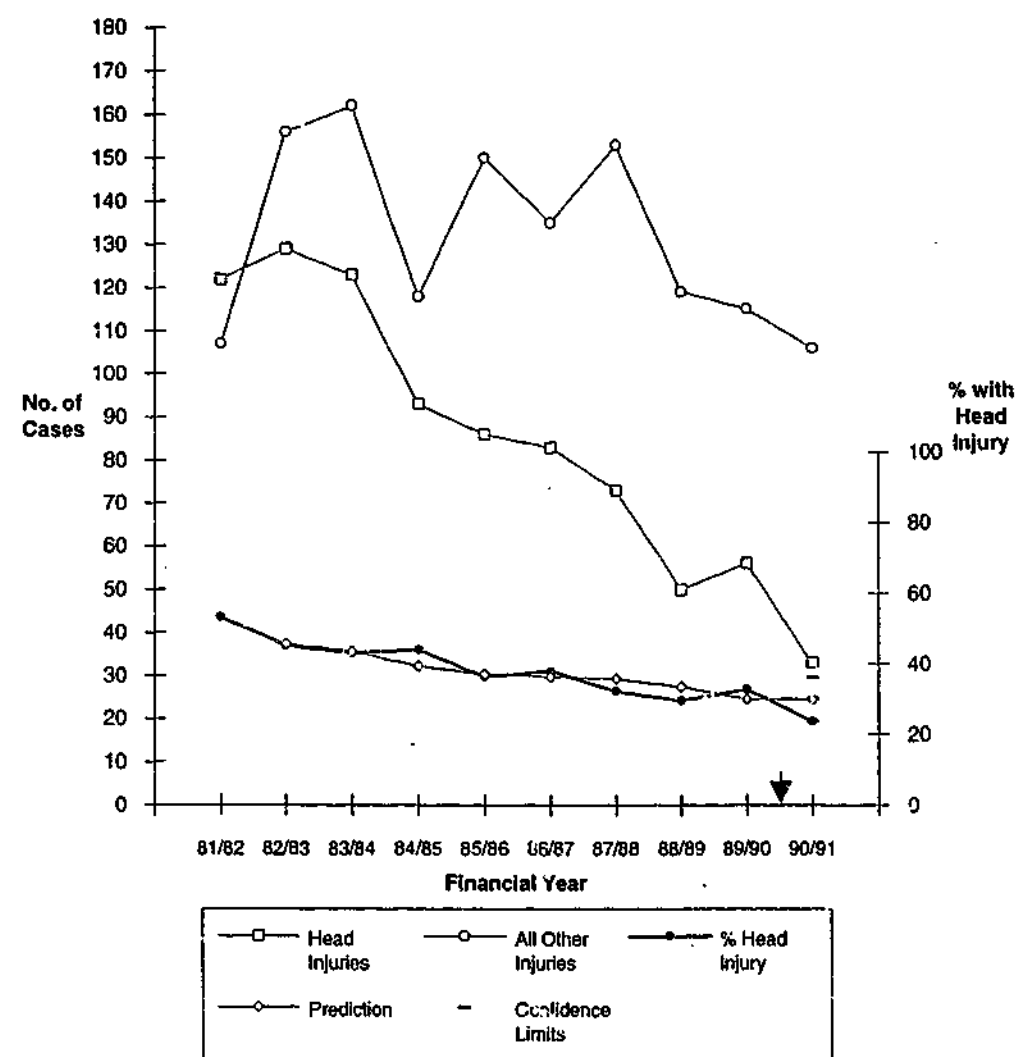
Transport Accident Commission Claims - Bicyclist claimants were classified into those sustaining a head injury (with or without other injuries), and those sustaining any other injury, on the basis of up to five injury codes recorded using the ICD-9 system. N-codes 800, 801, 803, 850-854, 872, 873.0, 873.1, 873.8 and 873.9 were classified as head injuries, following Healy (1986). Data on the number of bicyclists in Victoria each year during the 1980's were not available to calculate injury rates.

Figure 3 shows that the number of cyclists killed or admitted to hospital with head injuries in Melbourne fell progressively between July

1981 and June 1990 as the usage of helmets increased. In the 1990/91 financial year following the introduction of the mandatory wearing law (July 1990 to June 1991), the number with head injuries decreased by 41% relative to the corresponding period during 1989/90. This decrease suggests that, other things being unchanged, the substantially increased level of helmet use due to the law has reduced the risk of head injury to cyclists.

FIGURE 3

Severe Bicyclist Casualties Registered with TAC
Metropolitan Melbourne: July 1981 - June 1991



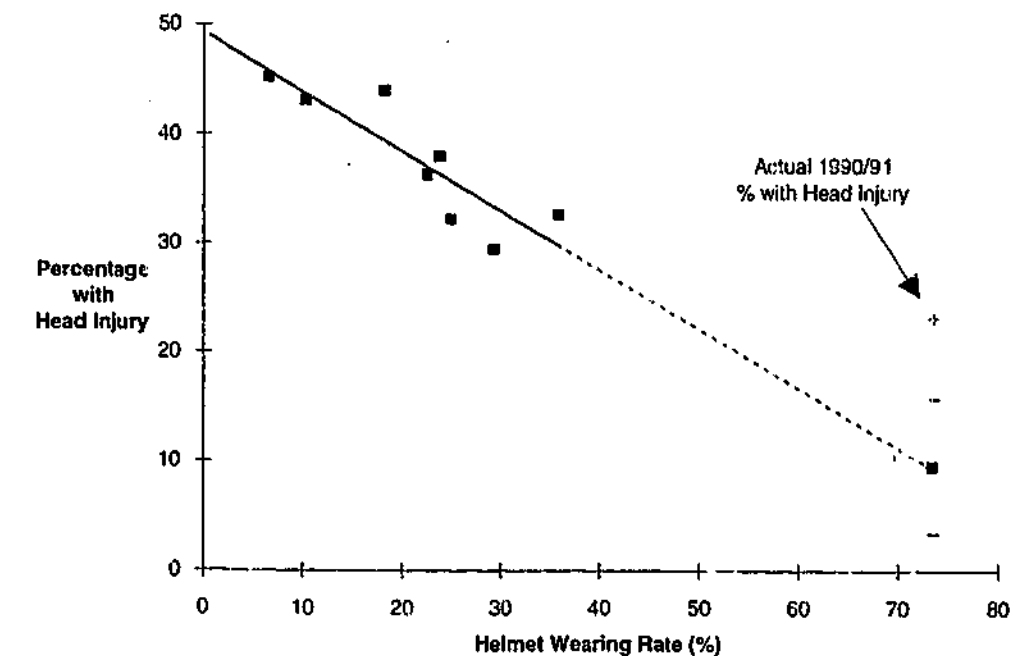
The number of Melbourne cyclists sustaining severe injuries other than to the head increased during the early 1980's, then fluctuated about a constant value (Figure 3). However during 1990/91, the number decreased by 8% relative to 1989/90. This decrease was somewhat unexpected, because a reduction in head injuries through helmet use would have led to some cyclists with multiple body region injuries now being classified as "other than head injuries". It suggests that the number of cyclists involved in crashes with motor vehicles has decreased during the post-law period, either due to a reduction in bicycle

use or a reduction in the risk of crash involvement. The first of these possible explanations will be examined explicitly later. The second possibility is consistent with the general reduction in all road deaths and hospital admissions in Victoria, which fell by 12% between 1989/90 and 1990/91.

Because of the fall in non-head injuries as well as head injuries, the effect of helmet use for cyclists involved in crashes was addressed by examining the percentage of killed and admitted cyclists who sustained a head injury (Figure 3). It was found that there was a statistically significant correlation ($R = 0.87$) between these percentages and the estimated overall helmet wearing rates for Melbourne during the years prior to the law (Figure 4).

FIGURE 4

Percentage with Head Injury v. Helmet Wearing Rate
Severe Bicyclist Casualties: Melbourne: July 1981 - June 1991



A linear regression was fitted and used to predict the expected percentage of cyclists with a head injury (assuming the relationship was unchanged) in the case where the helmet wearing rate was 73%, the actual post-law level estimated for all cyclists in Melbourne. The predicted percentage head injured was 9.6%, but the actual percentage (23.7%) was higher and above the upper 95% confidence limit for the prediction. This finding suggested that the percentage head injured had not decreased by as much as would be expected if the helmets had continued to be equally effective as they appeared to be during the pre-law period. However the fitted linear relationship itself must be questioned when extrapolated to high wearing rates, because it suggests that the percentage head injured would fall to zero before helmet wearing rates reach 100%.

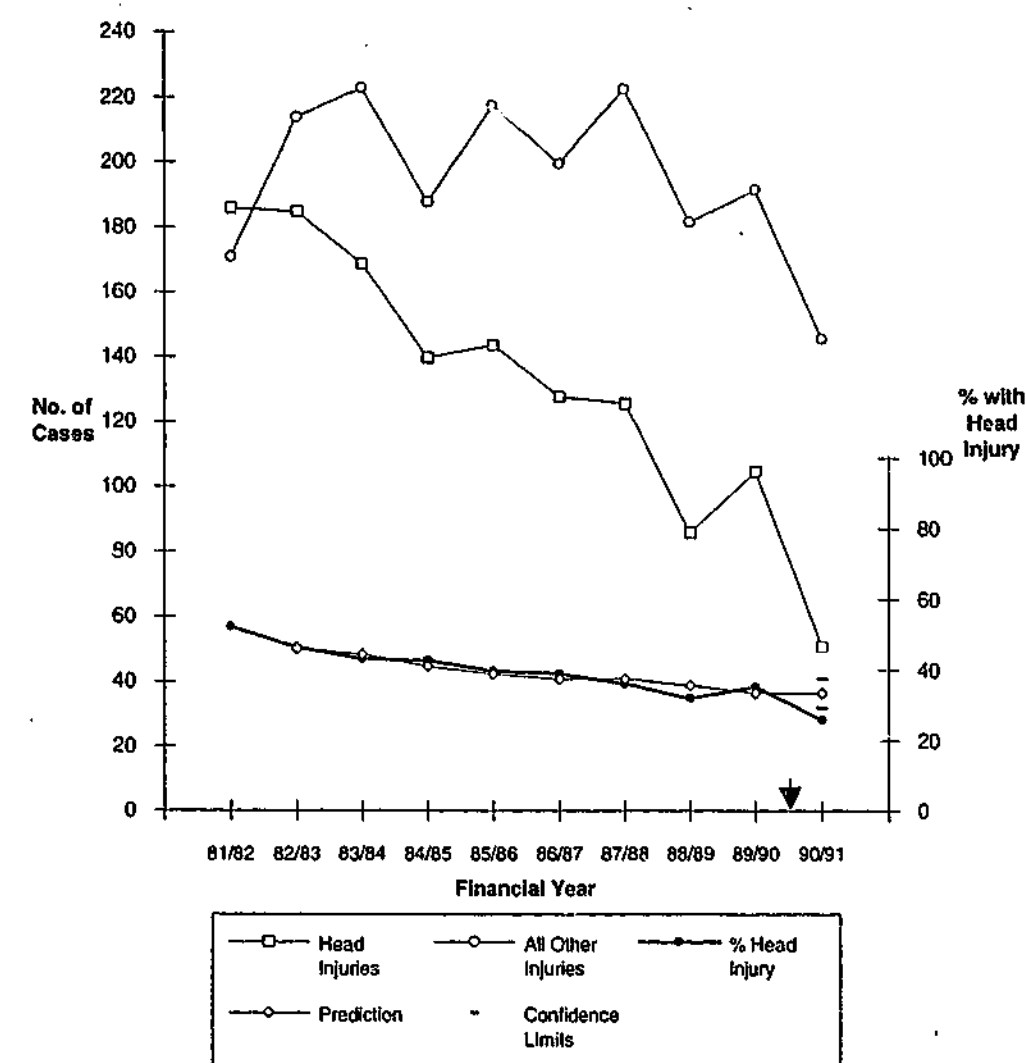
The linear regression was also used to predict the percentage head injured which would have been expected in 1990/91 in the absence of the law, assuming that the 1989/90 wearing rate had continued unchanged (Figure 3). In addition, 95% confidence limits for the prediction were calculated. The percentage head injured during 1990/91 (23.7%) was below the percentage predicted (29.9%) and fell just within the confidence limits. However the actual percentage was statistically significantly below the prediction when a decrease in head injury risk was tested ($p < 0.05$; one-tailed test).

Similar findings were observed when the TAC claims from severely injured cyclists involved in crashes in the whole of Victoria were examined (Figure 5). The number of killed or hospitalised cyclists with head injury fell by 51% between 1989/90 and 1990/91, and the number with severe injuries other than to the head fell by 24% over the same period.

Overall helmet wearing rates for cyclists in Victoria were estimated by combining the country commuting and recreational rates in the same way as those for Melbourne (ie. using the ratio of commuting to recreational riding in Melbourne as a proxy for regional cities), then combining the overall Melbourne and country rates in proportion to the population distribution. Country rates prior to 1984/85 were estimated from the Melbourne rates by assuming that the ratio of country to Melbourne rates during 1982/83 and 1983/84 was the same as that calculated during 1984/85. The results of an interview survey of bicycle use throughout Victoria in 1989 confirmed that the method of weighting the country wearing rates and combining them with those for Melbourne is likely to produce accurate results (State Bicycle Committee 1991). This process yielded estimated overall wearing rates for Victoria of 31% in 1989/90 and 75% in 1990/91.

A statistically significant correlation ($R = 0.91$) between these estimated helmet wearing rates and the percentage of severely injured cyclists who sustained a head injury was found and a linear regression fitted. The actual percentage of severely injured cyclists with a head injury during 1990/91 in Victoria (25.9%) was compared with the percentage predicted (33.5%) if the 1989/90 helmet wearing rate had continued unchanged (Figure 5). It was below the lower 95% confidence limit for the prediction and the decrease was very highly statistically significant ($p < 0.001$; one-tailed test). However, as was found for Melbourne, the actual percentage was above the expected 12.0% with head injury which was predicted from the linear regression for a 75% wearing rate.

FIGURE 5
Severe Bicyclist Casualties Registered with TAC
Victoria: July 1981 - June 1991



Admissions to Public Hospitals - Acute admissions by bicyclists were classified into those sustaining a head (excluding face) injury, and those sustaining any other injury, on the basis of up to five injury codes recorded using the ICD-9 system (using the same N-codes to classify head injuries as those used in the analysis of TAC claims). E-codes were used to select the bicyclist admissions. Some 75% of the admissions arose from crashes not involving a motor vehicle (mainly E-code 826.1), whereas the remainder did (mainly E-code 813.6). Information was not available prior to July 1986.

Figure 6 shows that the number of bicyclist admissions with head injury fell substantially during the post-law period, as did the number with other injuries. When the 1990/91 figures were compared with 1989/90, cyclists with head injuries decreased by 37% and the number with other injuries decreased by 21%.

FIGURE 6

Bicyclist Admissions to Public Hospitals
Victoria: July 1986 - June 1991

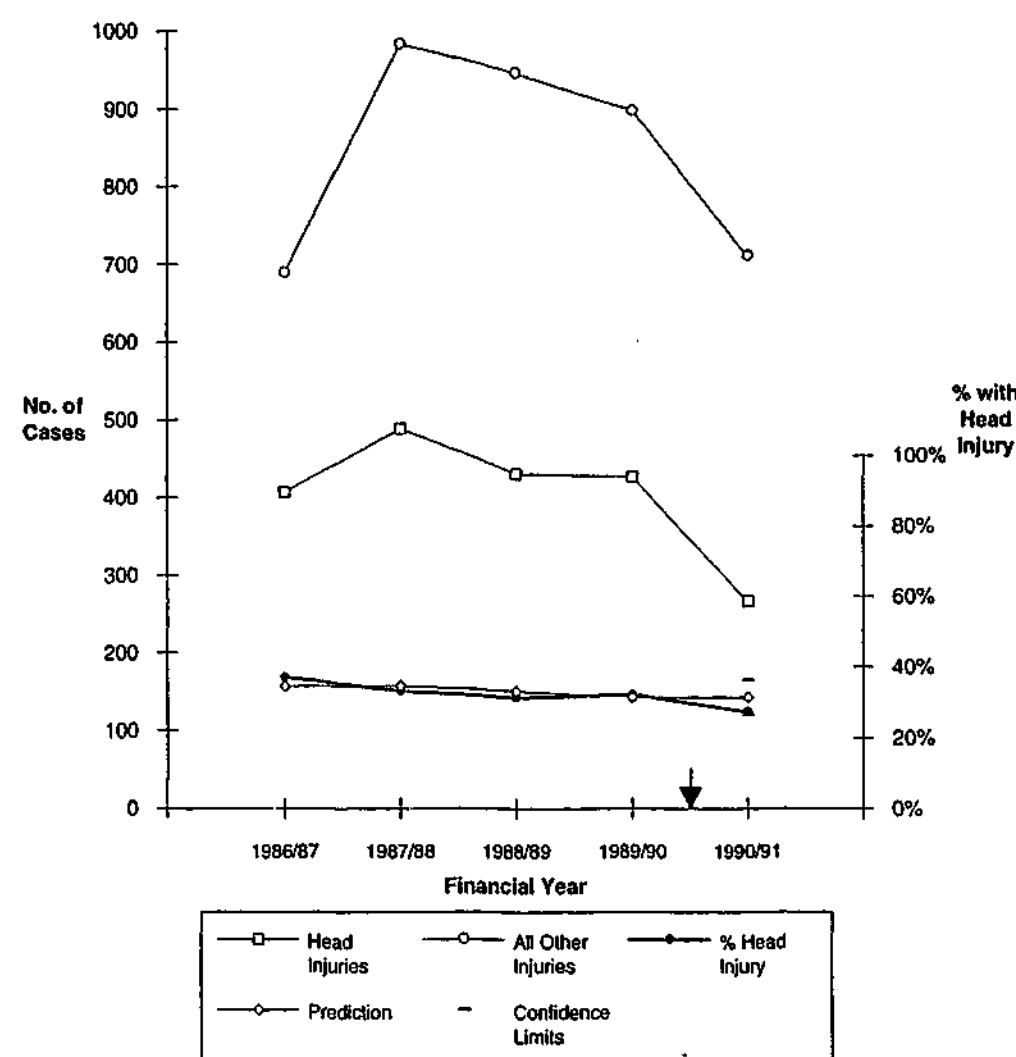


Figure 6 shows that the percentage of bicyclist admissions who sustained a head injury has fallen during the period July 1986 to June 1990, in line with the increase in the estimated helmet wearing rates during this period, and a satisfactory regression line linking these two variables was able to be fitted ($R = 0.59$). The percentage head injured during 1990/91 (27.3%) was compared with the percentage predicted (31.4%) if the 1989/90 helmet wearing rate had continued unchanged. The actual percentage head injured fell just within the 95% confidence limits for the prediction. The decrease in the actual percentage was almost statistically significant ($p < 0.06$; one-tailed test).

These findings were consistent with those found based on TAC claims from severely injured cyclists after collisions with motor vehicles, indicating that the mandatory helmet use law had similar effects on cyclist collisions not involving motor vehicles as well.

BICYCLE USE

The reduction in the number of severely injured cyclists with injuries other than to the head (and some of the reduction in those with head injuries) during the post-law period may have been due to a reduction in bicycle use as well as due to other factors affecting the risk of accident involvement.

In May/June 1991, an observational survey was conducted at a representative sample of 64 sites in Melbourne to determine whether bicycle use (on roads and footpaths) had decreased since the introduction of the new law. In analysing the effects, comparisons were made with the results of a very similar survey of child cycling conducted in May/June 1990 just prior to the mandatory wearing law (Drummond and Ozanne-Smith 1991). Total bicycle use by children had decreased by 36%, with the largest decrease (44%) being exhibited by the 12 - 17 year old group, compared with a 15% decrease for the 5-11 year old group. Because the 1990 survey did not cover adult cyclists, it was not possible to examine the change in their bicycle use over the same period, although there was an increase in adult cycling of about 58% when compared with a survey in November 1987-January 1988 (Drummond and Jee 1988).

Because of the changed pattern of bicycle use by age group after the law, it is planned to examine the injury trends and their relationships with helmet wearing rates for each age group of cyclists separately.

DISCUSSION

The results show a large reduction (37-51%) in the number of bicyclists with head injuries during the first year after the introduction of the mandatory helmet wearing law in Victoria on 1 July 1990. However, the mechanisms by which this reduction was achieved seem to be two-fold:

- a reduction in the number of cyclists involved in crashes resulting in severe injury (ie. killed or admitted to hospital), and
- a reduction in the risk of head injury for cyclists who were severely injured.

The extent of the reduction in bicyclist claims to TAC for severe injuries other than to the head (24%) supports the first mechanism. This mechanism is also supported by the 21% reduction in hospital admission of cyclists with injuries other than to the head. In addition, there is evidence that bicycle use by children fell by 36% during the post-law period. Hence reduction in bicycle use appears to be an important contributor to the reduction in crash involvement, though it is also possible that the wearing of helmets has made cyclists more

conspicuous, or that the helmets and associated publicity have made cyclists ride more carefully.

In addition, major initiatives directed at drink/driving and speeding were introduced in Victoria in December 1989 and March 1990, respectively. The total number of persons killed and admitted to hospital resulting from all road trauma during the year commencing July 1990 was 12% below the number for the previous year. This could account for some of the reductions in bicyclist trauma during this period.

There is also evidence that the risk of head injury to cyclists involved in crashes has been reduced (the second mechanism). The percentage of severely injured bicyclist claimants to TAC who suffered a head injury during the post-law period was statistically significantly below that which would have been expected had pre-law helmet wearing rates continued unchanged. This result was found both for Melbourne alone and for the whole State of Victoria. Essentially similar results were found when the percentage of bicyclist admissions to public hospitals who sustained a head injury was examined in the same way, though in this case the reduction was not quite statistically significant.

Based on the relationship (assumed linear) between the percentage of severely injured cyclists who sustained a head injury, and the helmet wearing rates established in the pre-law period, there is an indication that increased helmet wearing in the post-law period has not been as effective in reducing the risk of head injury to crash-involved cyclists as would have been predicted. If true, this could be due to helmets being less securely adjusted or fastened by those cyclists who did not previously wear them, or possibly to the greater proportion of lighter, soft-shell helmets being worn as a result of the amendment to the Australian Standard for bicycle helmets in 1990. It may also be a result of the assumptions which had to be made in combining a range of helmet wearing data, and fitting and extrapolating the linear relationship with head injury, to make the predictions. Further investigations are being undertaken to clarify these comments.

CONCLUSIONS

The mandatory bicycle helmet wearing law implemented in Victoria on 1 July 1990 has been successful in building on past efforts to promote helmet use by bringing helmet wearing rates to new high levels for all cyclist age groups, both in Melbourne and regional cities.

The introduction of the law has been accompanied by an immediate large reduction in the number of bicyclists with head injuries. Apparently this has been achieved through a reduction in the number of cyclists involved in crashes (at least partly through a decrease in bicycle use) and a reduction in the risk of head injury of cyclists involved in crashes.

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Paper No. 11

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Paper No. 11

ESTIMATING THE RELATIVE INFLUENCES OF ACCIDENT COUNTERMEASURES AND SOCIO ECONOMIC FACTORS ON THE VICTORIAN ROAD TOLL 1985-1990

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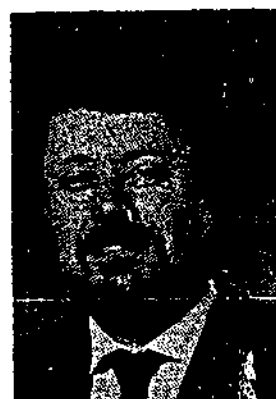
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SUMMARY

While road accident fatalities in Victoria have displayed considerable variability over the last decade, analysts have been divided as to relative degree to which this was attributable to the effectiveness of accident countermeasures, as opposed to demographic and socio economic factors. The significant decline in road mortalities since 1989 is of particular interest in this context. This paper describes the results of an investigation of the issue conducted by the Monash University Accident Research Centre during 1991. Analysis was undertaken using monthly data for the period January 1985 to December 1990, and was directed at accident sub groups such as pedestrians and car occupants as well as at overall road fatalities. Relationships between possible causal factors and fatalities were modelled via multiple regression techniques. Despite the expected volatility in fatality figures and collinearity between explanatory variables a satisfactory model was developed for fatalities as a single group. The model indicated positive relationships between fatality levels and number of persons of driving age, and real expenditure on alcohol. Negative relationships were indicated for a range of factors including random breath test and speed camera activities, seat belt wearing rates, and unemployment numbers. Similar models were developed for pedestrians, occupants and motorcyclist/bicyclists as separate groups, albeit with slightly less explanatory power. An implication of the investigation is that an economic upturn would make it harder to continue or sustain the reduced fatality rates observed since 1989.



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1. BACKGROUND

1. During 1990, the number of road accident fatalities for the State of Victoria fell by some 29 per cent compared with the previous year. This trend which continued into 1991 when the state road toll stood at 503 the lowest level since 1953, appears to be a national phenomena. As the second half of 1990 witnessed the onset of the most serious economic downturn since the nineteen sixties, this occurrence raised questions as to the relative extent to which the decline in fatalities was attributable to economic factors, accident countermeasures, or other factors. As resolution of this issue has important implications for measuring the general effectiveness of current road safety countermeasures and the formulation of future strategies, the Monash University Accident Research Centre (MUARC) assembled a multidisciplinary team comprising several of its own staff plus specialist drawn from the Monash University Department of Econometrics and the Australian Road Research Board. MUARC was responsible for the overall management of the project.

2. The project had two primary aims; to link the Victorian road toll with economic, road safety, social and other factors, and if feasible to forecast the road toll for future periods. The primary expected outcome of the project was the development of an econometric model or series of models which could be used to assist policy makers with decisions involving resourcing road safety programs and in assessing the impact of such programs at an aggregate level.

3. Analysis was confined to road accident fatalities; injuries and property damage outcomes were not covered. While fatality based analysis presents some statistical problems because fatalities reflect accident severity as well as frequency, this line of investigation was pursued partly because of a past preference displayed by policy makers and the public for analysis of this type, and partly because of the available accident series for Victoria that relating to fatalities is the most reliable and consistent over time. The period of analysis initially considered was 1980 to 1990; with 1991 data becoming available only after primary analysis had been completed. Analysis was also directed at monthly figures not only for statistical reasons but also as a reflection of a general interest in monthly changes in accident levels.

2. METHODOLOGY

4. The methodology adopted had four discrete though not necessarily sequential parts. These comprised; a review of the relevant Australian and overseas literature and local work in progress, review of the current local knowledge base regarding the causation of accidents and their severity, compilation and construction of the necessary data base of dependent and explanatory variables, and finally a modelling phase which involved testing alternative estimation procedures and modelling structures in order to derive models with the best possible explanatory and predictive power.

2.1 Literature Review

5. Review of the Australian literature in addition to providing valuable background information on how the study should be conducted, indicated that the link between levels of economic

activity and the road toll had been the subject of ongoing investigation for at least a decade. Studies reviewed dated from work undertaken by Filmer and Mannion in the early 1980's through studies undertaken by Thomson (1981) and Pascoe (1988) to recent studies commissioned by the Roads Corporation of Victoria (Haque 1991) and the Federal Office of Road Safety (Pettit 1992). In summary these studies indicated that the following points should be born in mind by future investigators. Model specification was considered to be crucial by several authors, as model design should be aimed at trapping interactions between potential explanatory variables as well as their more gross contribution. A fairly common conclusion was that data should be analysed on a more frequent basis than permitted by annual data; quarterly or monthly data was considered more appropriate. Other common suggestions were that investigations should be carried out on a state by state basis rather than at a national level, and if possible should be executed for distinct road user groups as opposed to the all road users level. Many of these suggestion were taken up by Haque (1991). In addition to attempting to build on and improve on the last study cited, the MUARC project varied substantially in terms of fatality sub-groups tested, the range of explanatory variables investigated, and the variety of alternative statistical models trialled. In particular by careful model specification and choice of variables the authors sought to minimise the effects of multi-collinearity between explanatory variables in response to which Haque had found it necessary to undertake a series of complex statistical adjustments.

6. Much of the international literature relevant to this project was found to have been previously reviewed by S. Hakim et al (1991). While confirming many of the observations made in the Australian literature a number of additional important points were made. In particular, it was suggested that models should be based upon an appropriate theoretical construct relating to accident causation. It was also recommended that the dependent variable used be formulated as a number (e.g. number of fatalities), rather than as a rate (e.g. fatalities per capita), thus enabling the exposure factor (e.g. population) to be validly included as an explanatory variable in the model. The review also suggested the use of count distribution methods, such as the Poisson or Negative Binomial models, as opposed to methods which treat fatalities as continuous variables and which permit fractional outcomes (e.g. 1.5 fatalities). It should be noted that overseas studies typically included only one variable to describe variations in economic conditions, this usually being some measure of unemployment. Finally the review indicated that other explanatory variables found to be significant in overseas work have included alcohol consumption and a variety of countermeasures such as reduction of speed limits and increase in seat belt usage.

2.2 Links with Road Safety Practice

7. To assist with the selection of potential causal factors affecting road fatalities, a conceptual model (the "Road Trauma Chain") showing the general steps leading to the road death of an individual was used (Cameron 1990). The chain was originally developed to assist with countermeasure development by identifying links where the chain can be broken if opportunities and effective measures are available. For this project, attention was focused on individual humans as the entities who may pass through the different steps in the chain and ultimately contribute to the Victorian road toll. The chain proved of considerable value in identifying key groups of factors, changes in which could be expected to produce substantial changes in the road toll. These factors included measures of road use, population "licensing" rates, and separate identification of countermeasures aimed at reducing risks of accident

involvement and countermeasures aimed at reducing severities once accidents had occurred. On the basis of the insights generated by the road trauma chain and the literature review fatality data was disaggregated into the following twelve groups for analytic purposes:

- (i) Male drivers aged 17 - 24 years
- (ii) Male drivers over 24 years
- (iii) Female drivers - all ages
- (iv) Motor cyclists - both sexes
- (v) Bicyclists - both sexes
- (vi) Pedestrians - both sexes 16 years and under
- (vii) Pedestrians - 17- 59 years
- (viii) Pedestrians 60 years and over
- (ix) Passengers - both sexes 24 years and under
- (x) Passengers - both sexes over 24 years
- (xi) All other fatalities
- (xii) All Fatalities

2.3 Data

8. Significant effort was devoted to assembling an adequate data base. While this was relatively straight forward for the dependent variable fatalities, assembly of monthly series for potential explanatory variables proved less than straightforward. Details of data series assembled and used in the modelling process are shown in Table I. It should be noted however that when reviewing possible dependent variables, inconsistencies were encountered in the Victorian serious injuries series an important factor which contributed to the decision to concentrate analysis on fatalities only. For the independent or explanatory variable group, data was assembled for thirty-six variables (excluding monthly dummy variables and lagged transformations), covering socio-demographic factors, economic variables, measures relating to road use, and road safety countermeasures. A feature of countermeasure series developed is the level of quantification, a factor which permitted modelling of their impacts directly, instead of as "dummy" variables or generalised indexes of countermeasure effort as was done in some previous studies.

9. Data on three countermeasures affecting the risk of accident involvement, namely random breath tests, red light and speed camera activities were assembled on a monthly basis. For random breath test information on monthly numbers of tests performed was only available from January 1987 onwards. In order to generate monthly series for the period 1980 to that date monthly patterns were applied to annual data both calendar and financial. Post 1987 data was obtained from the Traffic Accident Commission, with information prior to that date being provided by the Victoria Police. Data on monthly numbers of speed camera infringement

TABLE I
DESCRIPTION OF EXPLANATORY VARIABLES EVALUATED
VICTORIAN ROAD FATALITY MODELLING 1980-90

GROUP/SUB-GROUP	VARIABLE NAME	DESCRIPTION	UNITS OF MEASUREMENT
ECONOMIC FACTORS	Level of activity		
	rgdp	Expenditure on Real gross domestic product (Aust)	\$m 1980/81 prices
	rgos	Real Gross Operating Surplus-trading enterprises (Aust)	\$m 1980/81 prices
	nce	Real total private final consumption expenditure (Aust)	\$m 1980/81 prices
	Impact		
	rawe	Real average weekly earnings-males (Vic)	\$ 1980/81 prices
	interest	Reserve Bank short term bond rate (Aust 2yr)	%
	mu1524	Male unemployment - 15 to 24 year age group (Vic)	(000's)
	mu25up	Male unemployment - 25 year and over age group (Vic)	(000's)
	mutotal	Male unemployment - total (Vic)	(000's)
	fu1524	Female unemployment - 15 to 24 year age group (Vic)	(000's)
	fu25up	Female unemployment - 25 year and over age group (Vic)	(000's)
	futotal	Female unemployment - total (Vic)	(000's)
	tu1524	Total unemployment - 15 to 24 year age group (Vic)	(000's)
	tu25up	Total unemployment - 25 year and over age group (Vic)	(000's)
	tutotal	Total unemployment - total (Vic)	(000's)
	malepar	Male participation rate in the workforce (% Vic)	%
	fempar	Female participation rate in the workforce (Vic)	%
ROAD USE & ACCESS FACTORS	ncars	monthly new registrations-cars and stations wagons (Vic)	number
	novch	monthly new registrations-other vehicles (Vic)	number
	nmcs	monthly new registrations-motor cycles (Vic)	number
	nvttotal	monthly new registrations-all vehicles (Vic)	number
	Fuel	Monthly volumes of fuel purchased (Victoria)	megalitres
	ls	Monthly of learners permits issued (Vic)	number
	ps	Monthly probationary licenses issued (car-truck-bus) Vic	number
	mcls	Monthly motor-cycle learners permits issued (Vic)	number
	mcps	Monthly motor-cycle probationary licenses issued (Vic)	number
ROAD SAFETY COUNTER MEASURES			
	Driver behaviour		
	rbi	Monthly random breath test infringements (Vic)	number
	speed	Monthly speed camera infringement notices issued (Vic)	number
	red	Monthly number of red light camera infringements (Vic)	number
	Severity limiting		
	dsbelt	Monthly seat belt wearing rate (Vic)	%
SOCIO-DEMOGRAPHIC FACTORS	fsbelt	Monthly seat belt wearing rate (Vic)	%
	rsbelt	Monthly seat belt wearing rate (Vic)	%
	bikehel	Monthly bicycle helmet wearing rate (Vic)	%
	malepop	Civilian male population over 15 years old (Vic)	(000's)
	fempop	Civilian female population over 15 years old (Vic)	(000's)
	totpop	Civilian population over 15 years old (Vic)	(000's)
	Nalc	Value of monthly sales of liquor outlets (Vic)	\$ m 1980/81 prices

notices issued was provided by the Traffic Camera Office of the Victoria Police from May 1986 onwards. It should be noted that speed cameras became operational in the State from March 1986. In contrast red light cameras became operational in August 1983 and MUARC was able to obtain details of monthly red light infringement notices issued from January 1984 onwards. A potentially significant factor not included in this group of variables was the TAC media promotions campaign directed at factors such as drink driving, excessive speed, and fatigue. This exclusion reflected an inability to quantify these campaigns in a fashion comparable to other variables considered.

10. Regarding severity reduction countermeasures, data was collected on seat-belt wearing rates, and bicycle helmet wearing rates. For seat-belt wearing rates series were built up describing driver, left front seat passenger, and rear seat passenger wearing rates. As data on seat-belt and bicycle helmet rates was collected on an annual basis monthly estimates were derived via a process of linear interpolation. Data was also collected describing monthly numbers of learner permits and probationary licenses issued for motorcycles and cars. These series contain both a "licensing" element and a countermeasure element in the form of the training component contained in the licensing process which sought to promote safety awareness.

11. A range of problems were encountered in assembling an appropriate collection of economic, population, and socio economic variables. As can be seen from the description of variables assembled as contained in Table I, data for a whole range of National Accounting aggregates measure in constant price or "real" terms was assembled (ABS 1991). While it was possible either to get this data in a monthly format or to convert it into that format from quarterly basis this data was available on only a national rather than an individual state basis. The sole series of this type available for Victoria was real monthly average weekly earnings, and even this had to be converted from a quarterly basis. As monetary policy was a feature of economic management in the period an appropriate interest rate indicator was also included in the independent variable series.

12. In contrast it was possible to assemble data showing monthly numbers of unemployed not only specifically for the state of Victoria, but also for the full range of age and gender groups listed in the fatality groups it had been decided to investigate. Male and female participation rates were also obtained from ABS.

13. A range of "population" data was also collected. In addition to collecting monthly data for the civilian population of Victoria (split in to male, female, and total) data was also collected for new vehicle registrations (motorcycles, cars, and other vehicles) in order to represent changes in the vehicle fleet impacting on travel. The latter was included as an imperfect proxy for summary statistics relating to the vehicle fleet in use; more specifically to measures relating to changes in vehicle crashworthiness and vintage/usage relationships.

14. Finally data on two further variables, expenditure on alcohol and mega litres of fuel consumed in the Victorian marketing area, was also assembled. This data was available only from January 1985 onwards. Fuel data was collected as a proxy for road use data which was not available on a monthly basis. Alcohol usage was obtained by first obtaining from ABS published and unpublished figures on the monthly turnover (sales) figures of Victorian Hotels, Liquor Stores and Licensed Clubs for the period May 1982 onwards. Information for previous

months was estimated from quarterly data. To eliminate inflationary elements this data was converted into constant price terms using the alcoholic beverages sub-index of the Consumer Price Index.

2.4 Modelling

15. Because monthly fuel sales data, the sole proxy for road usage was available only from January 1985 onwards, it was decided to use this month as the starting date for the analysis. In all 72 monthly observations terminating December 1990 were included in the analysis. This decision also reduced the number of monthly observations of explanatory variables which had been estimated from quarterly and annual data.

16. To develop suitable single equation models for each of the eleven fatality sub-groups and for fatalities overall, both ordinary least squares multiple regression (OLS) and "count" model methodologies based on maximum likelihood methods were employed. For the former two alternative data transformations were explored; the first involved expressing both independent and dependent variables in terms of natural logs (double log models), the second involved transforming only the dependent variables into natural log form (semi log models). Both these approaches generated models where independent variables had multiplicative impacts, and differed significantly from Haque's approach in which a simple additive structure was employed. For "count" models two alternative methodologies were trialled, a Poisson variant and a Negative Binomial variant. It should be noted that the particular version of the LIMDEP software (Greene 1990) utilised, only permitted the user to invoke the latter when its internal checking facility confirmed that data could be considered to be "over-dispersed".

17. For both the "count" and ordinary least squares approach monthly dummies were added to the list of explanatory variables. An alternative set of dummies was also calculated for the main annual holiday periods and was tested as a substitute for the full range of monthly dummies. Minor problems were experienced for the holiday dummy series due to the transition from a three term to a four term educational year with a consequent change in timing and duration of school holiday periods. With the exception of these dummy variables, explanatory variables were also evaluated both "as observed" and lagged one period. More complex lagged structures were not evaluated primarily because of a lack of a strong causal hypotheses for most variables. For semi-log OLS models, explanatory variables were selectively trialled with exponents dictated by sensitivity analysis.

18. Actual model building with some exceptions involved limiting selection to one factor from each explanatory variable sub group (excluding monthly and holiday dummies) as a means of limiting multi-collinearity problems generated by correlation between variables. As could be expected stronger levels of correlation were generally encountered for variables within sub-groups as compared to correlations between sub-groups. While the initial choice of which variables to include in individual models was based on underlying causal hypotheses, actual incorporation was influenced by a number of factors. Inclusion was determined according to whether the variable had the "expected" sign, was statistically significant (as indicated by the t ratio), and made a positive contribution to the overall statistical significance and, or, "fit" of the model as measured by the appropriate test statistics.

19. Using the above methods and available data base, with the exception of the "All Fatalities" group, it was not possible to derive acceptable equations for any one of the other fatality sub-groups. While in many cases tests indicated that statistically significant relationships had been formulated these were not strong enough to generate models capable of adequately explaining or tracking observed fatalities. For the OLS derived models typically better equations accounted for 30 percent or less of observed variations in fatalities, while "count" models performed even less satisfactorily.

20. The poorer than anticipated performance of models at an individual fatality sub-group level appears to reflect data characteristics as opposed to modelling methodologies. In many cases explanatory variables were poorly targeted, that is they were not specific to the individual fatality groups being investigated. Problems also arose where interpolation had filtered out volatility in explanatory variables, and where variables used were inadequate proxies for factors for which full information was not available. The latter included using new vehicle registrations as a proxy for the use profile of the vehicle stock. Much of the unexplained variation is also likely to reflect local accident specific variables previously described. The performance of count models in addition to being affected by the above, also appears to have been affected by the "whole of state" aggregation intrinsic in fatality series which de-emphasized the count nature of fatalities. As a consequence frequency distributions associated with dependent variables are more consistent with the assumptions of statistical normality which underpin OLS compared with count models. In terms of relative performance the count model methodology returned near comparable results to OLS for motorcycle and bicycle fatalities, accident sub-groups where aggregation appeared to have minimal impact on the essential count nature of the accident type.

21. To test the hypothesis that the effects of non modelled and imperfectly represented explanatory variables would tend to be mutually self cancelling and that targeting of variables would improve with aggregation, the eleven individual fatality sub-categories were amalgamated into three categories and models fitted to each. The three broad categories were as follows:

- all vehicle occupants, comprising male and female drivers and passengers of both sexes
- all pedestrians, comprising all age groups of pedestrians.
- riders and passengers of motorcycles and bicycles.

22. Of the three groupings, the "cycle" group is the least homogeneous as while both components are highly vulnerable to severity factors, factors such as vehicle performance, risk and type of accident involvement vary substantially. Models fitted to these groups are detailed in Table II. As expected better outcomes were obtained from fitting single equation models to the three fatality amalgamations. Models developed for each broad group plus the overall total fatalities grouping are summarised in Table 2. It should be noted that single tailed tests of significance have been employed for variables other than monthly dummies.

23. With the exception of the equation derived for motorcyclists and bicyclists, the models developed account for either just under, or over, fifty percent of the variation displayed by observed fatality rates, a favourable result given the volatility of monthly fatality statistics. The model developed for bicyclists and motorcyclists which accounted for a little less than 40 percent of variations observed was been included because it is deemed to be statistically significant according to the appropriate statistical test of the overall equation. Each of the three

TABLE II

VICTORIAN ROAD ACCIDENT FATALITIES 1985-90
DETAILS OF MODELS AND PARAMETER VALUES ESTIMATED

(s = sign of parameter, v = value, p = pr($t \geq t_{calc}$) (1))

Variables included and Transformation Utilised	Victorian Road Fatality Sub-Groups			
	All vehicle Occupants s =, v =, (p =,)	All Pedestrians s =, v =, (p =,)	Motorcyclists and Bicyclists s =, v =, (p =,)	Total fatalities all Road Users s =, v =, (p =,)
Equation type	OLS (double log)	OLS (double log)	OLS (double log)	OLS (double log)
Constant	+ 24.3572 (0.01)	- 1.3064 (0.39)	+ 0.6013 (0.38)	- 5.4839 (0.36)
Alcohol sales	+ 0.3985 (0.01)	+ 1.2116 (0.06)	+ 0.8114 (0.01)	+ 0.5438 (0.01)
Random Breath Tests (lagged one period)	- 0.07120 (0.04)	- 0.17769 (0.10)		- 0.1684 (0.01)
Speed Camera				- 0.0069 (0.03)
Drivers Seat Belt	- 4.7854 (0.01)			- 2.9831 (0.05)
Total Population (un lagged)				+ 2.7768 (0.03)
Probationary Licenses issued (unlagged)	+ 0.1783 (0.05)			
Probationary Licenses issued (lagged one period)				+ 0.1288 (0.09)
Total Unemployment (all age groups)	- 0.3342 (0.02)	- 0.5043 (0.06)	- 0.5350 (0.04)	- 0.2609 (0.05)
Seasonal dummies				
- January		- 0.3994 (0.09)	+ 0.4925 (0.01)	
- February		- 0.5906 (0.01)	+ 0.5977 (0.01)	
- March			+ 0.4892 (0.01)	
- May		+ 0.2882 (0.08)	+ 0.5081 (0.01)	+ 0.1811 (0.01)
- June	- 0.1545 (0.05)	+ 0.2862 (0.10)	+ 0.2036 (0.20)	
- July	- 0.2605 (0.01)			- 0.1512 (0.02)
- August	- 0.1984 (0.01)			- 0.1118 (0.09)
- October		- 0.5793 (0.01)		- 0.0816 (0.19)
- November	- 0.2249 (0.01)	- 0.3602 (0.04)	+ 0.1511 (0.32)	- 0.2111 (0.01)
- December		- 0.4052 (0.24)		
R squared	0.47	0.51	0.37	0.58
F value (v1 v2)	6.23 (9, 62)	6.37 (10, 61)	4.62 (8, 63)	6.95 (12, 59)

(1) For seasonal dummies a two tailed "t" test is employed, i.e. $p = \text{pr}(|t| \geq t_{calc})$

models generate estimates which track observed fatalities satisfactorily, including the downward trend in fatalities observed for 1990. This is illustrated in Figure 1 where observed and predicted values are plotted for the all fatalities group.

Number of fatalities

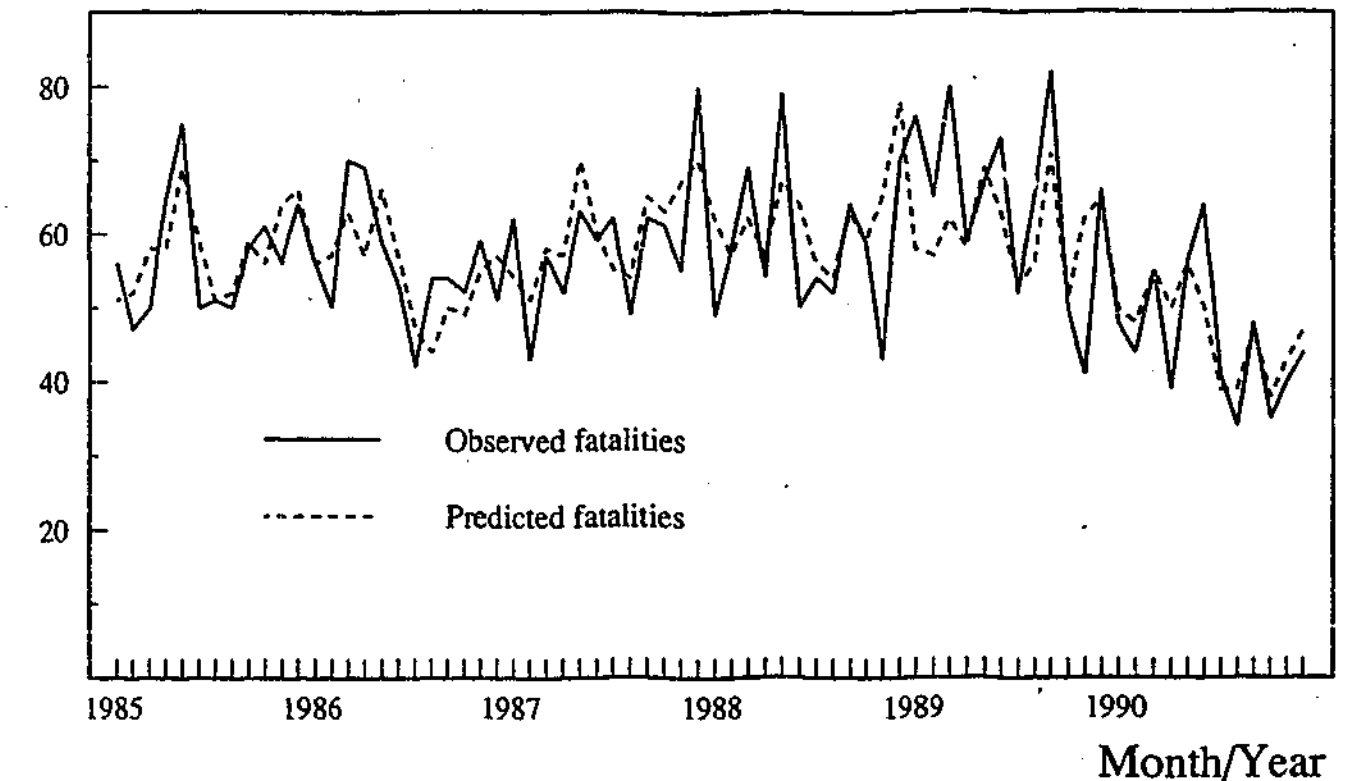


Figure 1

3. DISCUSSION OF RESULTS

3.1 Explanatory Variables and Model Structure

24. Examination of the variables included in Table 2 shows that only a small sub-set of the explanatory variables considered were included in the final models. Excluding monthly dummies and the constant term, the only explanatory variables common to equations developed for the fatality amalgamations shown in Table 2 are alcohol sales and unemployment. Given that some 66 potential explanatory variables were considered, a number which was increased when the same set of variables lagged one period were trialled, numbers of explanatory variables finally chosen are small. Excluding monthly dummies, numbers of explanatory variables included in final equations ranged from two for the motorcycle/bicycle group to seven for fatalities overall. Lagging of variables yielded a consistently better result for the random breath test variable across all models. Lagging one period implies that a variable will have maximum impact one period after it takes a given value. In the case of random breath tests this may indicate that the effect of this countermeasure is at a maximum a short time after such tests take place rather than immediately.

25. Development of non statistical reasons for inclusion or exclusion of potential explanatory variables was an important part of the overall exercise. In some cases non appearance of some variables such as seat-belt wearing rates and probationary licenses issued is logical, for example where target groups such as pedestrian and cycle riders were considered. In such cases these factors were not expected to directly affect fatality rates. Some sub classes of variables such as economic variables reflecting Australia wide variations in economic activity, and new vehicle purchases do not appear in any equations partly due to their imprecision. With the exception of motorcycles, new vehicle sales provided a poor proxy for changes in overall vehicle stock characteristics and usage patterns, and were not included.

26. Specific variables expected to be included, but subsequently dropped out of the modelling process included fuel sales, real average weekly earnings, and red light camera infringements. As the fuel sales variable was the only variable directly related to road use, a detailed account of the reasons for its exclusion is warranted. On statistical grounds it was found that the significance of factors remaining in the models after it's exclusion improved substantially. On conceptual grounds it was reasoned that fuel sales comprise an intermediate variable lying between factors generating travel (e.g. population and economic activity) and the fatal outcomes of such travel. Consequently its inclusion in models where travel generating factors were also present could be interpreted as double counting. Causal factors should be counted only once. This reasoning appears to be supported by statistical analysis. Fuel sales could however be validly included in multi-equation models in which road use and accident outcomes were estimated in separate though linked equations.

27. For many of the other factors dropped from the modelling process, the reasoning was different. For example red light camera infringements were in fact never included at any stage in the process due to lack of statistical significance and, or observance of a "wrong sign" associated with parameter estimates, suggesting a positive relationship between the counter-measure and fatalities. In contrast the elimination of real average weekly earnings from the modelling process occurred at a late stage, and while statistically justified was unfortunate as falling real wages appeared to be correlated with the fall in fatality levels which occurred prior to the onset of significant levels of unemployment.

28. Regarding the monthly dummies two points can be made. Firstly there is no consistent pattern of occurrence in terms of strength, direction (sign) or statistical significance between the models developed. The only monthly dummy common to all models is the November dummy, and then there is no consistency across models in terms of direction, strength and statistical significance. Secondly, it can be seen that some monthly dummies have "p" values which imply that they are not statistically significant. However they were included as their omission reduced the significance of overall models and often resulted in instability in other terms included.

29. Because excluding monthly dummies, only a relatively few explanatory variables were finally included in each model care should be taken when interpreting results. For example it is likely that with the exception of the model for overall fatalities, that the random breath test countermeasure variable captures some of the effect of other countermeasures such as speed camera operations. This follows as it was possible to develop models either including the pair of variables alcohol sales and random breath tests, or only including the less powerful single variable speed camera variable. Only in the case of the model developed for all fatalities was it possible to include all three variables. This appears to have occurred because the process of

amalgamating the various fatality sub-groups eliminated much of the variability in fatality rates in part leading to an apparent strengthening of measurable underlying trends. The same process did not occur for seat belt wearing variables, and as a consequence the variable "drivers seat belt" is best interpreted as standing for all seat belt wearing categories, and does not imply for example that improved rear passenger seat belt wearing rates had no appreciable impact on vehicle occupant fatalities.

3.2 Estimated Impacts of Key Variables on the 1990 Road Toll

30. In Table III the impacts of individual variables contained in each model (excluding monthly dummies) have been calculated separately. A *ceteris paribus* assumption has been made; that is only the variable of interest has been allowed to vary. Thus, for example, the increase in speed camera activity recorded in 1990 is estimated to have reduced fatalities in the All Road User group by 50. The figures in brackets represent the percentage impact on the 1989 level of fatalities whilst the total decrease for 1990 represents the percentage drop in fatalities in 1990 compared with 1989 in overall terms. For reasons given previously (e.g. multicollinearity) it is wise not to assume that the estimated impacts tell the whole story. It is indeed possible that as many of these variables move together and also proxy missing information, such as the TAC advertising campaign, consequently the estimated impact of say RBT is likely to reflect the changes in a package of measures.

TABLE III

ESTIMATED IMPACTS OF EXPLANATORY VARIABLES IN 1990

Variable	All User Groups	All Vehicle Occupants	Total Pedestrians	Motorcyclists & Bicyclists
Alcohol sales	-39 (-5.0)	-19 (-3.7)	-18 (-11.3)	-7 (-7.5)
Random Breath Tests (lagged one month)	-100 (-12.9)	-28 (-5.4)	-22 (-13.8)	X
Speed Cameras	-50 (-6.4)	X	X	X
Driver Seat Belt Belt Wearing Rate	-45 (-5.8)	-48 (-9.3)	X	X
Total Population	+32 (+4.1)	X	X	X
P - Plates Issued	+6 (+0.8)	+6 (+1.2)	X	X
Total Unemployment	-53 (-6.8)	-46 (-8.9)	-21 (-13.2)	-13 (-14.0)
Total Estimated Impact	-249	-135	-61	-20
1989 Total	776	517	159	93
1990 Decrease Number (%)	-228 (-29.4)	-141 (-27.3)	-66 (-41.5)	-19 (-20.4)

31. As the model are all double-log linear regression models it is also possible to add down each column in Table 3 and produce an estimate of the joint impact of all the changes in 1990. For the All Fatality Group this process indicates a reduction of 249 fatalities (actual reduction = 228), for Vehicle Occupants 135 fatalities (actual = 141), for Pedestrians 61 fatalities (actual = 66) and for Motorcyclists and Bicyclists 20 fatalities (actual = 19). The relative accuracy of these overall impact calculations is encouraging.

32. Since the modelling exercise did not constrain the 3 user sub groups to sum to the total generated by the all fatalities model, it is not surprising that the sum of the estimated impacts for the fatality sub groups does not equal that estimated from the overall model. Given that the All User Groups also includes the "Other Fatality category" (not included elsewhere) and the varying precision of our estimates, as reflected in the p-values in Table 2, the consistency of the overall results remains encouraging. For example if the totals estimated for the 3 user groups are summed an overall reduction in fatalities of 216 is indicated which is reasonably close to the actual reduction of 228 fatalities.

33. A further way of evaluating the sensitivity of fatalities to variations in explanatory variables is to compare fatality levels predicted by a particular model as a whole as opposed to elements of that model, under different circumstances. Such a process can yield additional insights into the role of particular explanatory variable groups. For example total fatalities predicted for 1990 with observed values for all explanatory variables could be compared with values predicted assuming 1989 unemployment rates had applied in 1990, all other explanatory variables taking on their 1990 values. This comparison generates an estimate of the impact of deteriorating economic conditions on fatality levels, which is less effected by inter-correlation of explanatory variables than partial methods. This was undertaken as alternative to projecting the model forward into 1991 given that the necessary data for explanatory variables was unavailable for that year at the time of writing.

34. This particular calculation indicates that if the lower average levels of unemployment observed in 1989 had applied in 1990, fatalities would have been about 5 per cent higher for the whole year. However it should be noted that the annual figure can be misleading given that unemployment rates in Victoria did not increase over and above their 1989 levels until May 1990, with consistent increases from June 1990 onwards. Taking account of this timing, the aggregate model predicted that fatalities would have been 9 percent higher for the September Quarter, and thirteen percent higher for the December Quarter than would have been predicted had 1990 unemployment levels applied during these quarters.

35. Assuming that variation in predicted values can be applied to variations in observed fatality levels, the above calculations imply that about 14 percent of the reduction in overall Victorian road accident fatalities for the whole year 1990 reflected a net deterioration in the economy as measured by the increase in numbers unemployed.

4. SUMMARY AND CONCLUSIONS

36. As a result of the work reported above, a series of plausible models have been developed which link a range of countermeasures, economic factors, and other factors with recent trends in Victorian road fatalities. In particular the models indicated that a significant proportion of the change in fatality levels was attributable to economic factors as well as to countermeasures.

37. The quality of the models developed in this project is considered to be adequate in that they explain about 50 percent of the variations in the monthly Victorian road toll during the period 1985-90. The models also generated plausible indications of the relative impacts on the 1990 road toll due to countermeasures (ie. random breath testing, speed camera activity, and increased seat belt use), the economy (measured by unemployment) and social factors (ie. alcohol sales, population numbers, and probationary licences issued).

38. However the models cannot be used as substitutes for in depth controlled evaluations of the impact of individual countermeasures and other factors as they imperfectly reproduce selected elements of the full road trauma chain. Neither should the models be viewed as the output of a curve fitting exercise which has minimal power in explaining or accounting for trends in fatalities recently witnessed. The models in effect lie midway between these two possible interpretations, and provide a means of sorting out the relative contribution to the reduction in the Victorian Road Toll of whole groups of factors in approximate rather than exact terms.

39. The work undertaken suggests three areas in which improvements could be made; firstly the predictive power of the models could be improved, secondly the range of possible explanatory variables could be enlarged, and finally the predictive power of models developed should be subject to further evaluation.

40. Predictive abilities could be improved in three ways. Firstly fatality trends could be analysed at a finer level although model development would be contingent on significant improvements in data. By finer level is meant analysis of trends by area (urban/rural), time of day, day of week, and purpose of trip. Secondly models could be developed using quarterly as opposed to monthly data. Data availability with respect to explanatory variables is more abundant, and quarterly level analysis would screen out "random" monthly variations. Finally sub-models and the model for overall fatalities could be made more consistent. This could be done in two ways; sub models could be required to have the full set of explanatory variables contained in the overall model (irrespective of statistical significance) and could be constrained to generate estimates which when summed gave a result similar to that generated by the overall fatalities model.

41. The second area could be addressed as follows. Effort could be devoted to developing more precise explanatory variables specific to particular road using sub-groups and specific to Victoria only. In this regard the development of multiple equation models should be considered as it permits the inclusion of more variables than the current single equation structure, and provides a closer statistical approximation to the road trauma chain. Finally statistical methods designed to minimise the effects of multi-collinearity should be evaluated. The final area could be addressed by using the models developed for the 1985-90 period to predict monthly 1991 totals, which could then be compared with actual fatalities.

42. In addition to the above three further actions should be considered. Firstly, development of similar models should be considered for other States and Territories as opposed to a whole of Australia model. Secondly a parallel set of models could be developed for statistical series covering serious injuries resulting from road accidents. Finally, some effort should be devoted to maintaining and enlarging the data base built in the course of the project described in this report.

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Phase 2: Effects of program mechanisms".

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Paper No. 12

CRASH-BASED EVALUATION OF THE SPEED CAMERA PROGRAM IN VICTORIA 1990-1991

Phase 1 : General Effects

Phase 2 : Effects of Program Mechanisms

by

Max Cameron

Antonietta Cavallo

Adrian Gilbert

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Abstract:

A major speed camera program was launched in April 1990 in Victoria which involved a dramatic increase in the detection of speeding offenders and a multi-million dollar, Statewide publicity campaign through all mass media. This report describes Phases 1 and 2 of an evaluation study examining its effects on crashes. Phase 1 examined the general effects Victoria-wide, in Melbourne, and in the rest of the State separately, and Phase 2 attempted to link the effects to the various speeding deterrence mechanisms (both specific and general deterrence) associated with the program.

The results of Phase 1 indicated that the speed camera program in Victoria has been associated with decreases in the frequency of reported casualty crashes which occur in low alcohol times of the week and also decreases in their injury severity. These decreases have been calculated as departures from expected values based on past trends and seasonal patterns, changes in unemployment rate, and using the comparable areas of New South Wales as a control. The program appears to have had its greatest effects (in terms of decreases in the frequency of casualty crashes) on arterial roads in Melbourne and on 60 km/h roads in rural Victoria, where the majority of the speed camera operations have taken place within the respective Melbourne and country areas. This pattern of results provides further evidence that the observed effects relate to the speed camera program. The results of Phase 2 suggested that the reductions in the frequency of casualty crashes (in low alcohol hours) appeared to be linked with speed camera TINs (Traffic Infringement Notices) issued to detected drivers, Transport Accident Commission (TAC) road safety publicity in general and TAC speed related publicity in particular (lower level of significance). Reductions in the injury severity of casualty crashes (in low alcohol hours) appeared to be associated with speed camera TINs issued and hours of camera operation.

It is planned to undertake two further Phases of evaluation: Phase 3 will attempt to examine the localised effects in time and space related to the camera operations, while Phase 4 will attempt to link the observed effects with changes in speed behaviour.

Key Words:

(IRRD except where marked *)
speed & evaluation (assessment), injury,
collision, road trauma, traffic regulations,
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camera*, speed enforcement*, time series analysis*

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EXECUTIVE SUMMARY

A speed camera program was gradually introduced in Victoria since December 1989 to reduce traffic speeds and hence road trauma. The program involved the progressive introduction of 54 speed cameras between December 1989 and January 1991 across the State, and was supported by an intensive Statewide mass media publicity campaign. The publicity aimed to increase the perception of the level of camera operations and their legitimacy, as well as building a community agenda about speeding and safety. The major launch of the enforcement and publicity occurred in April 1990.

The current evaluation study attempts to quantify the effects of the program on the incidence and severity of road crashes between December 1989 and December 1991. This report describes Phase 1 of the evaluation, which examined the general effects Victoria-wide, in Melbourne, and in the rest of the State separately, and Phase 2 which links the effects to the various speeding deterrence mechanisms (both specific and general deterrence) associated with the program. It is planned that Phase 3 will examine the localised effects in time and space related to the camera operations. Phase 4 will link the observed effects with changes in speed behaviour.

PROGRAM DESCRIPTION

Speed cameras were developed to detect and photograph speeding vehicles. A slant radar measures the speeds of passing vehicles, while a camera control unit provides photographic evidence of the vehicle at the scene of the offence and records the time, date, location and speed of travel. Speed cameras can be set so that both receding and/or approaching vehicles can be monitored. The speed cameras are capable of taking two photographs per second and can be operated at any time of the day (or night).

Cameras can be mounted on tripods outside the vehicle or used in a less obvious manner from inside the vehicle, as is generally the case. Generally unmarked police vehicles have been used to reduce the visibility of operations. Warning signs are not displayed specifically in relation to camera operations to alert drivers of the camera's presence. However, some general warning signs were erected at a number of locations, including every major road coming into Victoria. Through such operations, the program aimed to increase drivers' perceptions about the chances of being caught, and hence increase compliance with speed limits generally, by detecting a large proportion of offenders.

An automated Traffic Infringement Notice (TIN) penalty system was also introduced to allow efficient processing of offences and issuing of TINs, given the dramatic increase in detection rates. A TIN informs the vehicle owner of the details of the offence such as the date, place and time of the offence and the penalties for the level of the offence. Penalties increase with the level of speeding over the posted speed limit, with combinations of fines, licence demerit points, and immediate licence suspension, depending on the severity of the offence. The registered owner of the vehicle is liable for these penalties unless he/she nominates the driver of the vehicle at the time of the offence.

Low levels of local publicity began in December 1989/January 1990 with the trialing of 4 speed cameras at a small number of sites in Melbourne. The official mass media publicity launch occurred in April 1990. Speed cameras were used at substantial levels from July 1990 when the automated TIN processing system commenced. The program resulted in a dramatic increase in all speeding offenders detected from a level of around 20,000 per month before July 1990 to 40,000 - 80,000 per month over the operation of the program. Speed cameras have been used mainly on arterial roads in 60 km/h speed zones in both the metropolitan and country areas. There has also been greater use of speed cameras in the metropolitan area.

PHASE 1: GENERAL EFFECTS

Phase 1 of the evaluation attempted to determine the area-wide effects of the program on casualty crash frequency and injury severity (evaluation criteria), as it operated in different periods during 1990 and 1991. This approach was undertaken because it was considered possible that the program could have generalised or dispersed effects beyond immediate speed camera sites and times, given that speed cameras are often not obvious, are used at many sites, use has been maintained at high levels (especially relative to previous speed enforcement), and related publicity has also been intensive.

The program was characterised by three distinct periods to the end of 1991:

T1a: a period of low level camera trialing and localised low level publicity from December 1989 to March 1990

T1b: a period characterised by a high profile media launch of speed cameras and intensive publicity relating to speed-related crash risk, but little speed camera enforcement (ie. after an intense 9 day burst of camera use, negligible levels of speed camera use, offence detection and hence TINs issued) between April and late June 1990,

T2: the subsequent period in which enforcement increased dramatically, and thus also the detection and punishment of speed offenders, and was maintained at high levels, as was speed publicity, from July 1990 onwards. (This period was later split into T2a and T2b for analysis reasons).

Speed cameras have been used to a different extent in the Melbourne metropolitan area and rural areas in the State, and to differing extents on different zoned roads, therefore analyses were undertaken for each of the following treated areas:

- Victoria
 - Melbourne
 - arterial roads (all zones)
 - arterial roads (60 km/h zones)
 - Rural Victoria
 - arterial (open) roads (100km/h limit)
 - roads in rural towns (60 km/h zones)

Method

The estimated percentage change in the evaluation criteria (casualty crash frequency and casualty crash severity) in each area during each of the post-intervention periods (T1a, T1b, and T2) was adjusted by the parallel estimated percentage change in the comparable area in New South Wales (comparison areas) for the same periods. Taking into account parallel changes in comparison areas is a method for excluding the effects of extraneous influences on evaluation criteria in the treatment areas, so that estimates reflect the impact solely of the program.

Multivariate time series models for each of the evaluation criteria were developed for each of the treatment and comparison areas, so that the changes (relative to what was expected) during each of the post-intervention periods could be estimated for each series. Casualty crash severity was measured by the ratio of fatal plus serious injury crashes to crashes involving minor injury only. The respective unemployment rates in each area were included in the models fitted, to take into account the differential changes in treatment and comparison areas, and because it was considered that unemployment rates provided a measure for differential changes in driving exposure (vehicle travel).

Percentage changes in each area of Victoria were then contrasted with the corresponding area of NSW, allowing the *net percentage changes* in each treatment area to be estimated. The net change provided an estimate of the percentage change in each of the two crash measures that is attributable to the speed camera program, after other factors have been taken into account (measured by experience in NSW).

Periods T1a and T1b essentially represent an introductory period prior to the major change in speed enforcement practices, and as such are a "one-off" occurrence for the program. The T2 period was divided into the time between July 1990 and February 1991 (T2a) and March to December 1991 (T2b) because of the commencement of a speed camera program in NSW in March 1991. Net percentage changes in T1a, T1b and T2a therefore represent the effects of the Victorian program, whilst the net percentage change for T2b represents the effects of the Victorian program relative to that in NSW.

Only casualty crashes in "low alcohol hours" of the week (mainly daytime) were examined given the parallel operation of a major Random Breath Testing (RBT) initiative primarily in high alcohol times. Thus the results pertain to the effects of the speed camera program during "low alcohol hours" only (ie. Monday - Thursday 6am to 6pm, Friday 6am to 4pm, Saturday 8am to 2pm, Sunday 10am to 4pm). During "low alcohol hours" in 1988 and 1989, the percentage of drivers killed or admitted to hospital with a blood alcohol content exceeding 0.05%, was below 4%. Hence, even a large reduction in drink driving could have only a very small effect on the frequency of serious crashes in those "low alcohol hours".

Results

Figures 1 and 2 show the actual values of the two evaluation criteria in each month of 1983-1991, for Victoria as a whole. The Figures also show the expected post-intervention trends (from the models) if the speed camera program had not been

introduced. Two expected trends are displayed; one based on the model for Victoria, excluding the effect of other factors not implicit in the model, and the second trend including the effect of other factors, as measured by the experience in NSW. The difference between the expected trend (including other factors) and the actual represents the apparent effect of the program.

Figure 1
Number of Casualty Crashes per month in low alcohol times 1983-1991
Victoria

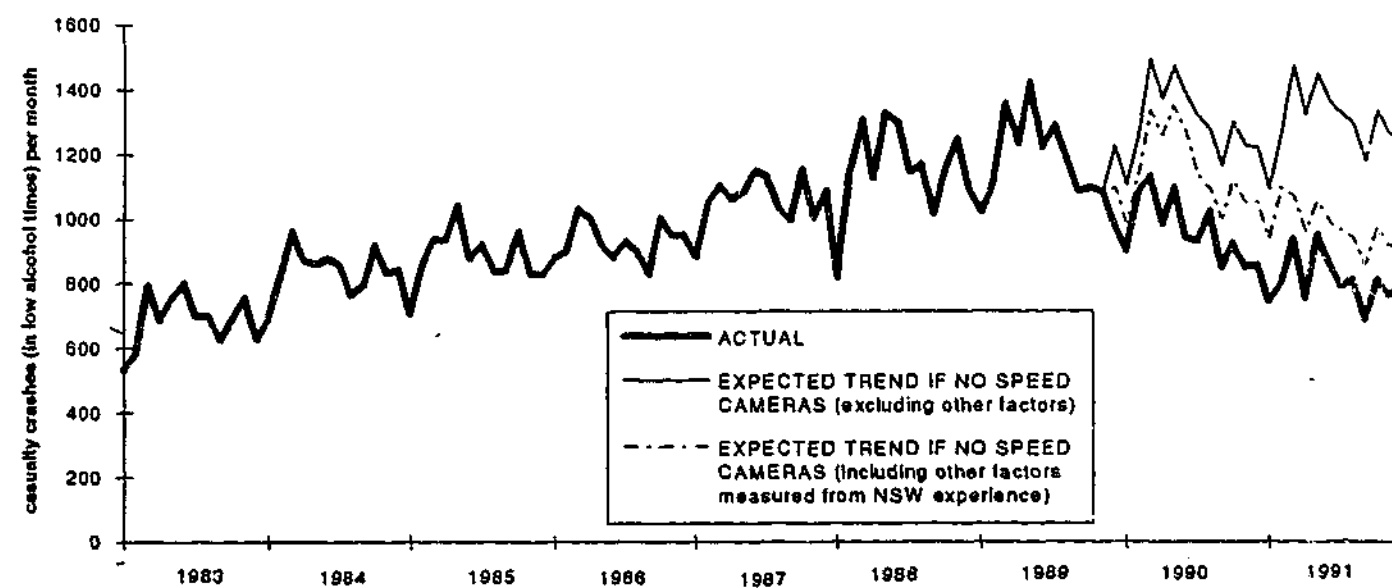
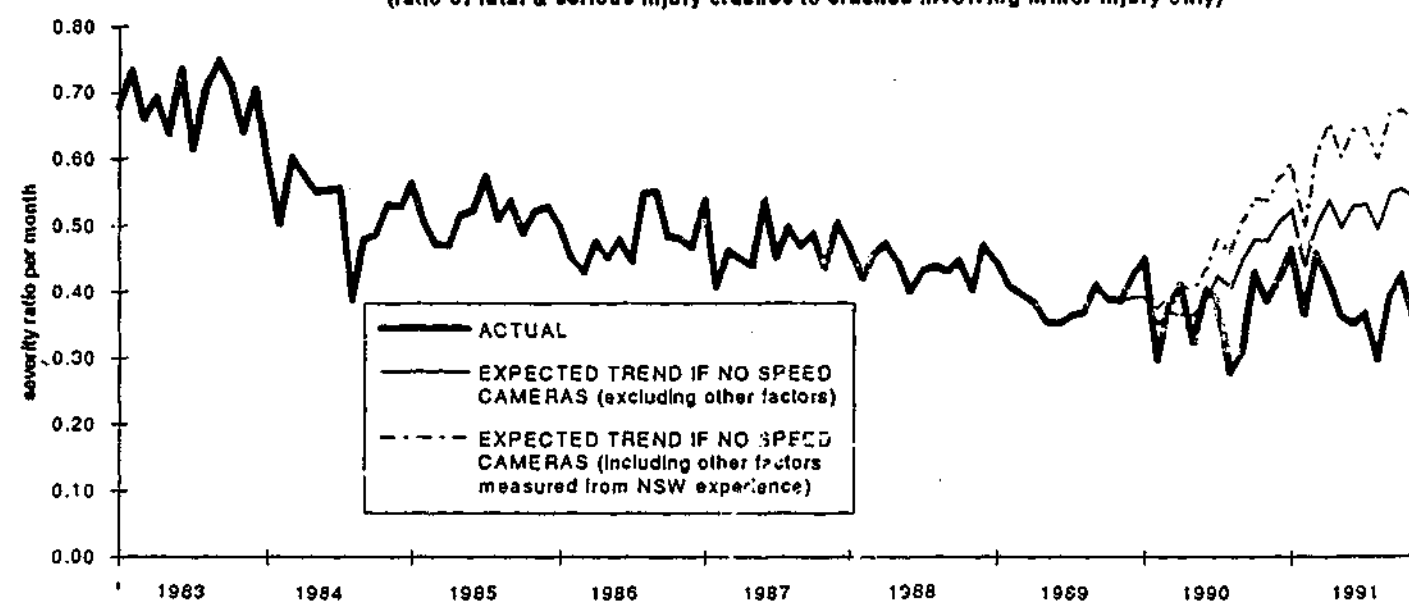


Figure 2
Casualty Crash Severity per month in low alcohol times 1983-1991, Victoria
(ratio of fatal & serious injury crashes to crashes involving minor injury only)



Figures 3-6 provide a summary of the percentage reductions measured for the two evaluation criteria, together with the 95% confidence limits on each estimate. Both statistically significant (shaded) and non-significant estimates are shown, because the non-significant reductions indicate time trends in the effects, although individually they do not represent scientific evidence of an effect in the specific phase of the speed camera program.

The effects on Melbourne's 60 km/h arterial roads are reflected on all Melbourne arterial roads, so only the results for the latter roads are given in Figure 3. Figure 4 shows the effects over all Melbourne roads, which are generally smaller than on the arterial roads (reflecting relatively smaller effects on residential roads). The apparent effects on rural 100 km/h highways were generally non-significant and disparate in magnitude and direction, so for these reasons they are not presented in a Figure. The effects on rural 60 km/h roads were more consistent over the program phases than those on the 100 km/h rural roads and are presented in Figure 5 (however because of the inconsistency between the results for these two types of rural roads, the apparent effects for rural Victoria as a whole are not presented in a Figure). Although the Victoria-wide effects are obviously an amalgam of a number of different effects operating on different roads during different months, they are summarised in Figure 6 to provide an overall view of effects of the program.

Frequency of casualty crashes (in low alcohol hours)

A consistent significant drop from what would have been expected in the **number of (low alcohol hour) casualty crashes** across all treated areas was observed for the periods corresponding to the publicity launch (T1b) and the increase in speed camera enforcement (T2a), except for 100 km/h rural open roads where there was a significant drop in T2a only. The largest of these reductions (around 30%) occurred on arterial roads in Melbourne. During T1a the reduction (14%) was confined only to arterial roads in Melbourne. Rural 60 km/h roads experienced a statistically significant drop relative to the respective rural NSW areas in T2b, when NSW also had a speed camera program operating.

Injury severity of casualty crashes (in low alcohol hours)

The Melbourne area (as a whole and on arterial roads specifically) experienced increasing reductions over time from what would have been expected in the **severity of (low alcohol hour) casualty crashes**. No significant changes were observed on 60 km/h rural roads, although 100 km/h rural roads experienced a significant increase in casualty crash severity during T1b (this unexpected result may in fact be a chance anomaly which has arisen because of the large number of significance tests conducted in this study). The overall changes in severity for Victoria as a whole were significant reductions in the T2a (28%) and T2b (40%) periods.

It should be noted that the estimated percentage reductions in the injury severity of casualty crashes apply to the severity ratio defined earlier (page iii) and not to the more traditional measure of injury severity, namely the ratio of fatal plus hospitalisation crashes to all casualty crashes (ie. those involving death or any level of injury). The former ratio is likely to show larger changes than the latter; for example, the estimated 28% reduction in the Victoria-wide severity ratio during T2a is equivalent to a 21% reduction in the traditional measure.

In addition, the estimated percentage reductions in the injury severity of casualty crashes are derived from models which take into account the effects of changes in unemployment rate. In practice this means that the estimated net effects are based on the expectation that casualty crash severity (in low alcohol hours) would have increased in Melbourne and Victoria, given the increase in unemployment rate that

occurred during the intervention period, had the speed camera program not been introduced. Therefore, the estimated percentage reductions are not reflected in the actual trend in the casualty crash severity ratio during the intervention period.

Pattern of effects corresponding to the level of speed camera enforcement

In terms of **casualty crash frequency in low alcohol times**, the pattern of results suggested that there were stronger reductions on 60km/h arterial roads and all arterial roads in Melbourne (around 30%) as compared with Melbourne as a whole and in rural 60km/h zones (around 20%) and also as compared with the weakest effect observed in rural 100km/h zones (14% drop in one period only). This pattern of effects corresponds with the level of speed camera enforcement in these different areas:

- speed camera operations were conducted almost exclusively on arterial roads,
- a greater number speed camera operations were conducted in Melbourne than in rural areas (70% of total sessions on average have been conducted in Melbourne),
- most (80-90%) camera sessions have been conducted in 60 km/h zones in both the respective metropolitan and rural parts of the State.

The pattern of results therefore provided greater confidence that the observed reductions in the frequency of casualty crashes (in low alcohol hours) were related to the speed camera program.

In terms of the **injury severity of casualty crashes**, reductions were observed in the Melbourne areas but not in any of the rural areas, again corresponding to the greater level of speed camera enforcement in Melbourne compared with country Victoria. There was no substantial difference between the effects on Melbourne arterial roads only and all Melbourne roads, except that they appeared somewhat greater on the arterials (as could be expected).

Figure 3
Estimated Percentage Reductions (with 95% confidence limits) in Casualty Crash Frequency and Injury Severity in Low Alcohol Hours during Phases of the Speed Camera Program - Melbourne Arterial Roads - Dec 89 - Dec 91

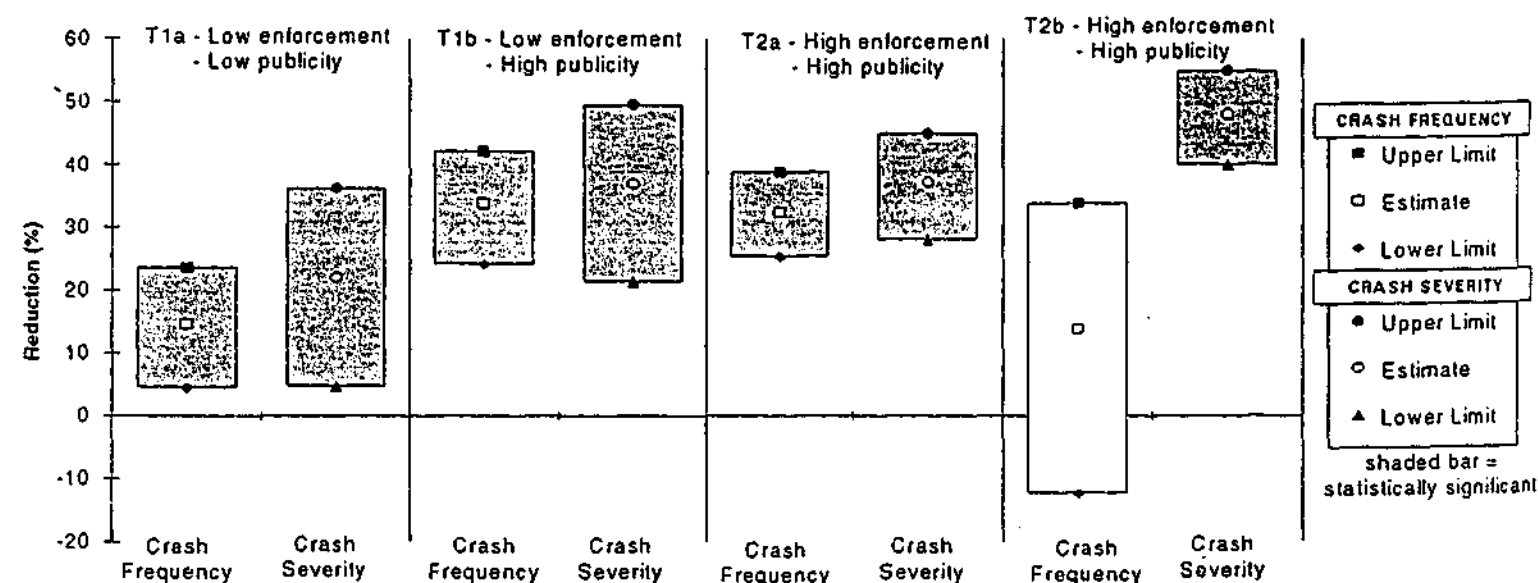


Figure 4
Estimated Percentage Reductions (with 95% confidence limits) in Casualty Crash Frequency and Injury Severity in Low Alcohol Hours during Phases of the Speed Camera Program - Melbourne - Dec 89 - Dec 91

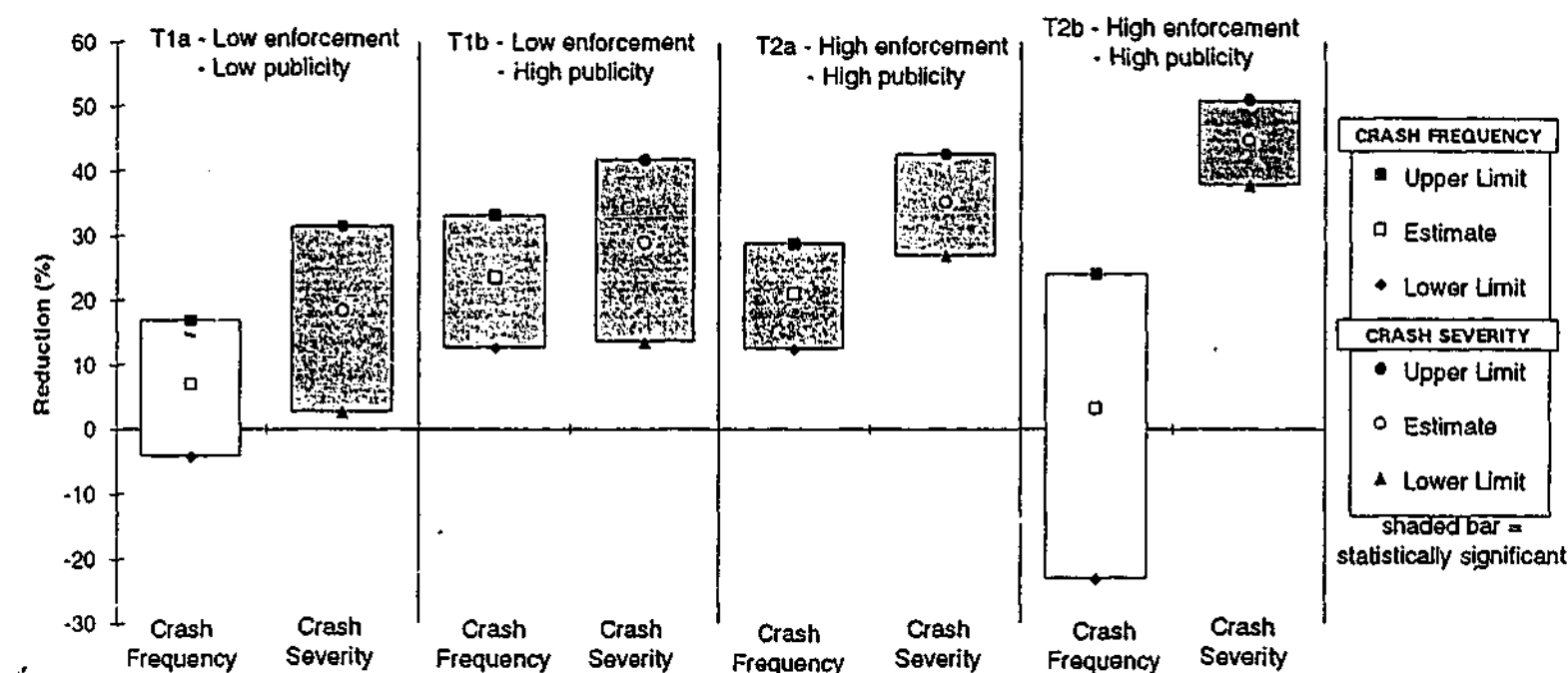


Figure 5
Estimated Percentage Reductions (with 95% confidence limits) in Casualty Crash Frequency and Injury Severity in Low Alcohol Hours during Phases of the Speed Camera Program - Rural Victoria 60kmh - Dec 89 - Dec 91

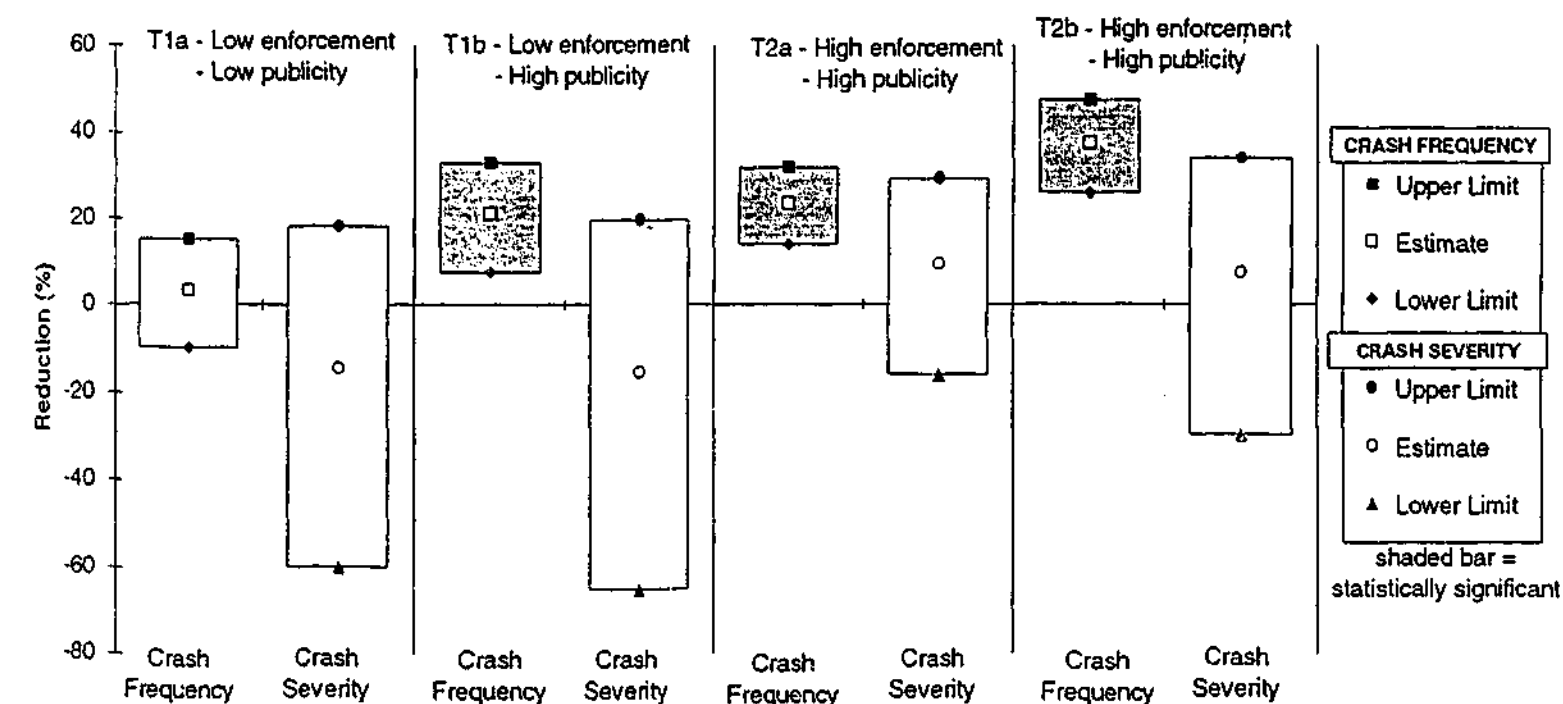
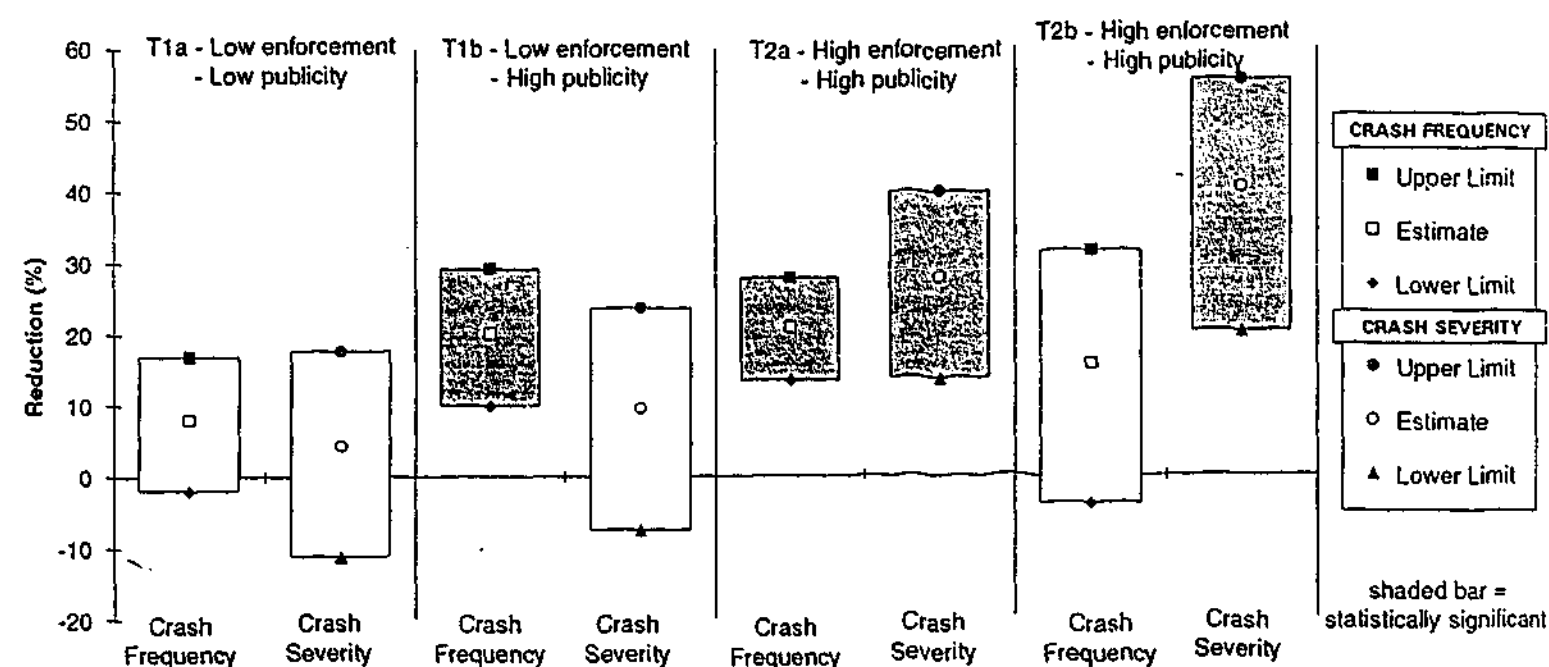


Figure 6
Estimated Percentage Reductions (with 95% confidence limits) in Casualty Crash Frequency
and Injury Severity in Low Alcohol Hours during Phases of the Speed Camera Program -
Victoria - Dec 89 - Dec 91



This phase of the evaluation tested for simultaneous reductions in casualty crash frequency and their injury severity (in low alcohol times) with the introduction of the various phases of the program (represented by intervention variables). Reductions were estimated after taking into account the effects of other influences; namely long-term trends in casualty crash risk and severity, seasonal trends and unemployment, and also changes in the evaluation criteria in NSW, which served as a comparison area. The effects observed in Phase 1 are strongly suggestive of a coincidental effect of the program on the evaluation criteria. However, it does not directly link the program to the observed effects, and hence it is possible that other influences, which are not accounted for, could have caused these observed effects.

PHASE 2: EFFECTS OF PROGRAM MECHANISMS

Phase 2 of this evaluation attempts to further understand and estimate the impact of the program by modelling the various deterrence mechanisms of the program against the evaluation criteria (casualty crash incidence and severity during low alcohol times of the week). The measured mechanisms are represented quantitatively and are assessed for their relative impact on the evaluation criteria. The purpose of this is two-fold:

1. to attempt to link more directly the program mechanisms with observed reductions in crash criteria, and hence better establish the "cause-and-effect" of the program,
2. to attempt to indicate which of the program mechanisms appear to be most strongly associated with changes in the crash criteria.

A range of mechanisms which potentially affected drivers' perceptions and hence speed behaviour have operated through the speed camera program. Measures of speed camera TINs issued, TAC speed related publicity only, all TAC road safety publicity, and hours of camera operation were available on a monthly basis, whilst other measures were not. Therefore an attempt was made to determine the relationship (if any) between the evaluation criteria and the quantified deterrence mechanisms.

Method

The relationships between the evaluation criteria (casualty crash incidence and severity during low alcohol times of the week) in Melbourne and the different program measures were determined using multivariate time series analysis models.

Results

Significant inverse relationships were found between some of the measures of the program and the frequency and injury severity of casualty crashes which occurred in low alcohol times, in 1990 and 1991. These were:

Frequency of casualty crashes (low alcohol times)	Injury severity of casualty crashes (low alcohol times)
<ul style="list-style-type: none"> • Speed camera TINs issued to drivers • All TAC road safety publicity • TAC Speed related publicity (sig. at 0.07 level only) 	<ul style="list-style-type: none"> • Speed camera TINs issued to drivers • Speed camera operation hours

The effects of speed camera TINs issued to drivers was consistently related to both crash frequency and injury severity, with stronger effects observed on arterial roads for both criteria. The direct effects of speed camera operations themselves (basically the immediate effects of the cameras operating) appeared to be confined to casualty crash injury severity only. This may indicate that:

- drivers are not aware of cameras operating because they are generally not obvious and hence cannot react to operations directly, or
- exposure to camera operations is not sufficient to modify speed behaviour to the degree required to reduce the risk of a casualty crash occurring but receipt of a TIN and punishment for speeding is effective in reducing crash risk.

Alternatively, the high intercorrelations between variables and estimates for the variables, especially for camera operations and other significant variables, may have masked the relationship between camera operations and casualty crash frequency. In addition, the casualty injury severity measure may be a more specific and more sensitive criterion related to speed than casualty crash frequency.

In relation to the findings for publicity, similar comments apply for the relationship between speed related publicity and casualty crash frequency. The estimate for this variable was correlated with the estimates for other significant variables. On the other hand, the stronger significant effect of all road safety publicity rather than speed related publicity on casualty crash frequency may reflect that casualty crash

frequency is a general measure of the risk of all types of (low alcohol hour) crashes and it is not confined to "speed-related" crashes (which could reasonably be expected to be affected by the speed-related publicity).

It appears that publicity (speed related only or all publicity) was not related to casualty crash injury severity.

QUALIFICATIONS OF THIS RESEARCH

The research design involved a number of assumptions. These include:

Interstate control (Phase 1)

The conceptualisation of the effects of this program as being generalised across broad areas for phase 1 of the evaluation led to the absence of any untreated area, time or other crash group in the respective Victorian metropolitan and rural areas to provide an acceptable comparison group. This meant that the use of an interstate comparison group was unavoidable. Although the most similar State to Victoria was chosen for this role, it is not known how well it provided control for other (exogenous) factors present over time in Victoria (although unemployment rates were explicitly taken into account in the analysis of each State's crash data). It is therefore assumed that net changes in T1a, T1b and T2a represent the effects of the program, rather than other factors. The net changes in T2b are assumed to represent the effects of the program in Victoria relative to the effects of speed cameras introduced in NSW.

Unemployment rate as a measure of low alcohol hour vehicle travel (Phases 1 & 2)

The use of unemployment rate as a covariate to control for changes in low alcohol hour travel cannot be directly validated. There were substantial changes in the trends in total travel almost coinciding with the program and these changes differed between Victoria and NSW. Explicitly controlling for changes in exposure was considered to be important for estimating effects of the program on crash frequency.

A significant statistical relationship was found between unemployment rate and injury severity of casualty crashes in Melbourne and in Sydney, but not in rural Victoria nor rural NSW, suggesting that increases in unemployment rate is associated with increases in the injury severity of crashes. One possible explanation for this is that when traffic congestion is reduced in times of economic recession, the opportunity to travel at higher speeds also increases (especially in urban areas) which leads to greater injury severity of those crashes which occur. This warranted the retention of unemployment rate in the models so it could be accounted for in estimating the effects of the program on crash severity. The estimates of the program in Melbourne and State-wide Victoria are therefore influenced by the presence of the unemployment variable in these models, because of its statistically significant role in these models of casualty crash severity.

Efficacy of the Phase 1 models

The validity of the results for casualty crash severity are tentative given that the models fitted may be inadequate from a statistical perspective. These models were

characterised by high intercorrelations between estimates for the predictors, in particular between those for unemployment rate, trend and T2b, which may have affected the estimates of the program variables. It was also assumed that the correlations between some parameter estimates in the (ARIMA) modelling of casualty crash frequency did not affect the estimates for the intervention periods.

The approach used in the modelling was such that a specific test was made for a step change or reduction with the introduction of the program. This assumes that the nature of the effects of the program are immediate and maintained. If, for example, effects were initially small and increased incrementally over time, the current estimates will only be an average of these varying effects. Although this is a shortcoming, it is because of the unclear nature of the effects of the program over time that alternative frameworks were not used.

Efficacy of the Phase 2 models

The models were found to fit the crash data well (around 84% and 66% of the variance in the casualty crash frequency and casualty crash severity criteria, respectively, was accounted for by these models). However, intercorrelations between coefficient estimates for casualty crash frequency models (in particular) and casualty crash injury severity models indicated that there were high correlations between many estimates. Such intercorrelations may mean that the estimated relationships reported are unreliable, particularly that indicated for the relationship between camera operations (and to a lesser extent that for speed publicity) and casualty crash frequency, given its high correlations with the estimates for other significant variables (ie. TINs issued, unemployment rate and trend).

Low alcohol hour crashes (Phases 1 & 2)

The results described here apply only to casualty crashes occurring during the low alcohol hours of the week (mainly daytime). This is because of the influence of a concurrent RBT initiative in high alcohol times.

Difficulties in linking program mechanisms (Phase 2)

One of the major practical difficulties with attempting to model the specific aspects of the program is the reliance on available quantified measures of the program and representing it validly. The lack of monthly time series data for other program mechanisms meant that a full model was not developed. On the other hand, while this was theoretically desirable, in practice it may have been impossible to develop a full model and disaggregate each mechanism's sole contribution to observed reductions in the crash criteria, given that the mechanisms are unavoidably interrelated.

Problems with the feasibility of this aim include the statistical problems of collinearity between the measures, and between the measures and other influences, the appropriate representation and quantification of the mechanisms within the model, and the fact that correlational relationships between the program and crashes does not prove causality absolutely. For these reasons, statistically significant effects which seem related to a particular program mechanism cannot definitively be attributed to

that particular part of the program only, given that it operated in the presence of the other mechanisms (quantified and unquantified) and other unknown factors. Indeed, the relative importance of the various mechanisms in drivers' perceptions, speed choice and speed behaviour and the extent to which these have altered cannot be understood through this quantitative approach.

FURTHER RESEARCH

It is now planned to undertake Phase 3 of this evaluation, as outlined in section 5.0. This should provide more definitive evidence of a cause-and-effect link between the speed camera operations and the general effects observed in Phase 1 of the evaluation. This phase should also provide further understanding of the key factor affecting the deterrent effect of the program, so that future camera operations of the program can be optimised. An additional Phase which attempts to examine existing traffic speed data over time to provide additional supporting evidence linking the observed effects to the speed camera program will also be attempted. It may not, however, replace the evidence that should be provided by the proposed Phase 3 of the evaluation, given the paucity of traffic speed data over the relevant time period. Additional research areas and data requirements emerged from the study and are outlined in the report.

CONCLUSIONS

The introduction of the speed camera program and supporting publicity in Victoria has been associated with decreases in the frequency of reported casualty crashes which occur in low alcohol times of the week and also decreases in their injury severity. These decreases have been calculated as departures from expected values based on past trends and seasonal patterns, changes in unemployment rate, and using the comparable areas of NSW as a control. The magnitudes of these decreases have varied with the region of the State and nature of the program operated during different periods. The program appears to have had its greatest effects (in terms of the frequency of casualty crashes) on arterial roads in Melbourne and on 60 km/h roads in rural Victoria, where the majority of the speed camera operations have taken place within the respective Melbourne and country areas. This pattern of results provides further evidence that the observed effects relate to the speed camera program.

The results of Phase 2 suggest that the reductions in the frequency of casualty crashes (in low alcohol hours) appeared to be linked with speed camera TINs issued to detected drivers, TAC road safety publicity in general and TAC speed related publicity (lower level of significance). Reductions in the injury severity of casualty crashes (in low alcohol hours) appeared to be associated with speed camera TINs issued and hours of camera operation.

On the basis of the analyses in this report, it is clear that the speed camera program (enforcement and supporting publicity) has been effective.

CRASH-BASED EVALUATION OF THE SPEED CAMERA PROGRAM IN VICTORIA 1990-1991

PHASE 1: GENERAL EFFECTS

PHASE 2: EFFECTS OF PROGRAM MECHANISMS

1.0 INTRODUCTION

A speed camera program was gradually introduced in Victoria since December 1989 to reduce traffic speeds and hence road trauma. The program involved the progressive introduction of 54 speed cameras between December 1989 and January 1991 across the State, and was supported by an intensive Statewide mass media publicity campaign. The publicity aimed to increase the perception of the level of camera operations and their legitimacy, as well as building a community agenda about speeding and safety. The major launch of the enforcement and publicity occurred in April 1990.

Up until this time speed enforcement was conducted at a relatively low level given the high resource demands required without an automated surveillance system. A few speed cameras were used and trialed in the late 1980's. Speed cameras were developed to detect and photograph speeding vehicles. A slant radar is used to measure speeds of passing vehicles, while a camera control unit provides photographic evidence of the vehicle at the scene of the offence and records the time, date, location and speed of travel. A full record of each session of operation is therefore available with information, such as the number of vehicles checked, the number of vehicles travelling at different speeds, the time and date of the operation. Speed cameras can be set so that both receding and/or approaching vehicles can be monitored. The speed cameras are capable of taking two photographs per second and can be operated at any time of the day (or night).

Cameras can be mounted on tripods outside the vehicle or used in a less obvious manner from inside the vehicle, as is generally the case. Generally unmarked police vehicles are used to reduce the visibility of operations. Warning signs are not displayed specifically in relation to camera operations to alert drivers of the camera's presence. However, some general warning signs were erected at a number of locations, including every major road coming into Victoria. This type of speed enforcement is predicated on the underlying belief that such operations (which result in larger proportions of drivers being detected and punished) are the best way to increase drivers' perceptions about the chances of being caught, and thus to increase compliance with speed limits generally.

An automated Traffic Infringement Notice (TIN) penalty system was also introduced through the establishment of a speed camera office to allow efficient processing of offences and issuing of TINs, given the dramatic increase in detection rates. A TIN informs the vehicle owner of the details of the offence such as the date, place and time of the offence and the penalties for the level of the offence. Penalties are commensurate with the level of speeding over the posted speed limit, with combinations of fines, licence demerit points, and licence suspension, depending on the severity of the offence. The registered owner of the vehicle is liable for these penalties unless he/she nominates the driver of the vehicle at the time of the offence.

The current evaluation study attempts to quantify the effects of the program on the incidence and severity of road crashes for different periods between December 1989 and December 1991.

This report outlines:

- available research relating to the role of speed and speed enforcement on crashes
- the mechanisms which could lead to changes in speed behaviour
- quantification of the mechanisms and description of the program
- evaluation study of the effects on crashes of the program

This report describes Phase 1 of the evaluation, which examined the general effects Victoria-wide, in Melbourne, and in the rest of the State separately, and Phase 2 which links the effects to the various speeding deterrence mechanisms (both specific and general deterrence) associated with the program. It is planned that Phase 3 will examine the localised effects in time and space related to the camera operations. Phase 4 will link the observed effects with changes in speed behaviour.

2.0 THE ROLE OF SPEED IN CRASHES

Reviews of studies which have investigated the relationships between travel speed and crashes (eg. Cowley, 1980, 1983, 1987; OECD, 1982; Sanderson & Cameron, 1982; Social Development Committee, 1991) have found that the nature of the relationship between crash incidence and speed is still not well understood. This is mainly because of the difficulty in estimating the pre-crash speeds of vehicles, and the dependency of involvement rate on different crash types, traffic and road characteristics (Sanderson & Cameron, 1982). On the other hand, basic data on crash dynamics as well as those studies which address secondary safety issues (Cowley, 1987) consistently demonstrate that the severity of injury in the event of a crash increases with crash speed.

Research conducted to date has almost exclusively been undertaken overseas and is mainly limited to rural highway environments. The studies fall into two methodological groups (Cowley, 1987):

- a) those which examine changes in traffic speed distribution characteristics (average speed, speed variance, 85th percentile speed, skewness, etc.) and corresponding changes in both numbers of crashes and injury levels, generally after a change in posted speed limits
- b) those which examine the crash involvement rates for different vehicle speeds, compared with the speeds of vehicles in the (corresponding) traffic stream, to identify speeds which are associated with higher crash risk, (with only Solomon [1964] giving attention to the relationship between speed and crash severity).

Generally, European studies have used the first approach whilst the second approach has mainly been used in early US studies (mainly in the 1960's). The literature is summarised below.

2.1 Studies of speed limit changes

In reviewing studies of speed limit changes the general conclusions have been that reductions in speeds are associated with reductions in crash rates and crash severities whilst inversely, increasing speeds generally increases crash occurrences (OECD, 1982; Cowley, 1987). Nilsson (1981) describes Swedish studies since the 1960's of the effects of lowering speed limits and concludes that decreases in speed limits led to decreases in traffic speeds and consequently decreases in crash and casualty rates. Other Scandinavian and Danish studies confirm these findings (Cowley, 1987). The OECD (1982) provided a quantification of these effects outside built-up areas (based on the findings of Swedish research to that date, and taking into consideration the estimates from other countries): the percentage reduction in crash rates equals 'n' times the percentage drop in mean speed, where 'n'=4 for fatal crashes, 'n'=3 for personal injury crashes and 'n'=2 for all crashes.

A recent study of the speed limit reduction from 110km/h to 90km/h during the summer period of 1989 in Sweden, estimated that personal injury crashes decreased by 27% and the number of persons killed or injured fell by 21% (Nilsson, 1990). Motorways experienced the largest reductions. The estimates are based on comparisons with the relative change on 70 km/h roads, to provide a base, although it was stated that speeds on these roads also fell and so the estimates may be conservative. A Danish evaluation of the effect of the introduction of a 50km/h limit in built-up areas, found that injury crashes dropped by 9% and fatalities by 24% (Engel & Thomson, 1988).

In the US, Wagenaar, Streff & Schultz (1990) evaluated, using a time series approach, the effects of raising the speed limit, from 55 to 65 mph, on Michigan's rural interstate highways in 1988. Using time series analyses, including covariates, they found an increase of 48.4% in fatalities, 31.8% in serious injuries, 30.3% in moderate injuries, no change in minor injuries and a 27.3% increase in property damage only crashes. There were no changes on rural highways on which speed limits did not change, except for an increase in fatalities on 55 mph limited access freeways. Garber & Graham (1990) evaluated the increase from 55 to 65 mph on rural interstate highways in all states, and found conflicting results for different states. On the whole, however, they concluded that the increase in speed limit led to a greater number of fatalities.

In addition, in reviewing European studies of speed limit/travel speed changes, OECD (1982) found evidence of a relationship between speed dispersion and crash risk, for homogeneous traffic only, that is, not where there is a mix of heavy and light vehicles.

2.2 Studies of relative crash risk for varying travel speeds

Studies conducted in the US, which have used approach (b) (section 2.0) to examine travel speed and crash risk, support the finding that speed variance is related to crash risk but do not find average or median travel speeds important in determining crash involvement. Solomon (1964) found a U shaped relationship between speed dispersion and crash risk: crash involvement increasing with increasing departure from average traffic speeds, particularly for night-time crashes. Solomon's work uses

mainly third party estimates of pre-crash speeds. Research Triangle Institute (1970) and West & Dunn (1971) reported a flatter U shaped relationship between crash risk and departures from average traffic speeds, with departures of 25 km/h or more only related to an eight-fold increase in crash risk. These studies, particularly that by West & Dunn (1971), estimate the pre-crash speeds of crash-involved vehicles better, although the samples studied are smaller. These studies, however, are extremely dated and it has been questioned whether they apply to the current situation, particularly in Australia.

A more recent US study (Garber & Gardirau, 1988) examined the inter-relationships between highway types, road characteristics (in particular design speeds), travel speed characteristics (average travel speeds and speed variance), and crash rates. Thirty-six sites were used consisting of a sample of Interstates (rural, urban, expressways/freeways), urban and rural Arterials, and rural Collectors, which had minimal differences in posted speed limits, generally at 55 miles per hour.

Unlike previous studies which examined the relationship between speed and crashes by estimating the pre-crash speed of crash-involved vehicles and quantifying the crash risk of each speed category relative to its occurrence in the traffic stream, this study examines the relationship between overall crash rates with the characteristics listed above. The assumption in this approach is that the speed characteristics of the traffic stream cause the observed crash rates, without directly determining the specific crash risk of each speed category. In addition, mathematical models using regression analysis were used to determine the relationships between the different factors. However, as noted in the report, there was some correlation between factors, namely design speed and average speed, and average speed and speed variance. Therefore in attempting to dissociate the independent relationships between these characteristics and crash risk, some of the results may actually represent the importance of the other factor(s). Nevertheless, the following results can be drawn from this study:

- average travel speeds increase as the design speed of the roadway increases, suggesting that drivers travel faster on highways with better geometric characteristics (regardless of the posted speed limit)
- speed variance decreases as average travel speed increases (although the inter-relationship between average speed and design speed means that design speed could be the important factor, although this is not tested in the study)
- speed variance is lowest when the difference between the design speed and posted speed limit is between 5 and 10 miles per hour
- the relationship between average speed and crash rate could not be assessed because of the inter-relationship between design speed (ie. geometric characteristics) and average speed
- crash rate increases as speed variance increases for each highway type in the study (varying design speeds and average speeds were not taken into account in this analysis and therefore these other factors may have confounded the analysis, although separate analyses for each road type were undertaken).

2.3 Other evidence for the relationship between injury levels and speed

According to the law of physics, speed and energy dissipation in a crash are directly related: for a given vehicle mass, the kinetic energy to be dissipated in a crash increases by the square of the impact speed; that is, if speed is doubled, four times the energy will be absorbed in the crash. For this reason, it is said that increased speed leads to increased crash severity, and probably increased injury severity to the vehicle occupants.

Studies of speed limit changes, outlined in section 2.1, have found effects on injury rates with changes in travel speeds. In addition, Solomon's (1964) study also found that higher travel speeds are associated with greater injury risk. Bohlin (1967) reported a curvilinear relationship between crash speed and injury risk to drivers from a comprehensive Swedish study. The rate of increase in injury risk was greater at higher impact speeds. The results also showed that the probability of injury was greater for unbelted drivers although the relationship was still clear for drivers wearing seat belts. Data on crash dynamics and findings from other studies conducted in the US in the 1960's clearly showed the trend of increasing probability of fatality occurrence with increased vehicle speed (Cowley, 1987).

Some reports warn of the applicability of the results of these studies to current Australian conditions, given high seat belt wearing rates and some improvements in vehicle design, such as collapsible steering columns and impact-absorbing chassis (Social Development Committee Inquiry, 1991). The consistent findings of the various types of studies, however (including more recent speed limit evaluation studies in the US and Sweden), as well as basic principles of crash dynamics indicate the importance of speed in injury outcome, when a crash occurs.

As was pointed out in the Social Development Committee Inquiry (1991), injury or death can occur for unprotected road users at relatively low speeds, given that motorcyclists, pedestrians and bicyclists account for around 60% of deaths in 60 km/h zones. Forty-five percent of all fatal crashes occur in 60 km/h zones. Yet such statistics often confuse the possible dual role of travel speed as a factor in crash incidence (particularly the possible importance of speed dispersion) and crash severity, and is not adjusted for any exposure data to test injury risk by vehicle crash speeds. Additionally, factors other than speed and/or speed characteristics are important in crash risk. The other point made by the Social Development Committee (1991) is that freeways have substantially lower casualty rates than undivided roads, regardless of their higher speed limits and travel speeds. However, this may indicate their inherent safety due to geometric properties and low interaction and conflict levels with other traffic and road users, as opposed to the travel speeds characteristic of these environments.

2.4 Australian studies

Australian studies have generally attempted to quantify the proportion of crashes in which speed is a factor through analysis of mass crash data and other sources. Such estimates are derived from subjective judgements and data which are thought to be speed related. Objective measures of speeds are not available.

It would appear that, on the basis of the weight of the evidence available from existing studies, vehicle speed is an important factor in both primary and secondary safety. However, it must be realised that the results of these studies may not be applicable to Australian conditions, they are almost exclusively based on rural roads and highways, and that the precise role speed has in contributing or causing crashes and their severity is still unclear. As stated in the Social Development Committee report (1991): "There is no doubt that speed is a contributing factor in road crashes. Whether or not speeding, however defined, is an important factor is yet to be proven." (p.175).

3.0 EFFECTS OF ENFORCEMENT ON SPEED BEHAVIOUR & CRASH RISK

There are four published trials of speed cameras available. Dreyer & Hawkins (1979) reported on a 3 month trial on 4 different roads (residential, rural, urban and urban thoroughfare) in Arlington Texas using a van-mounted ORBIS III photomechanical device. They reported that the greatest effect was on urban, densely travelled roadways during 'high' levels of enforcement where the proportion of "speeders" decreased throughout the 0.5 mile test segments. Reductions at residential and rural sites were less dramatic, but were detected at the van and post-ORBIS III locations; lower levels of enforcement appeared to be effective at these sites. Speed distributions were not greatly affected however, with small shifts in average travel speed of the order of 1 mph. They also reported that the effects on travel speeds were maintained for some time after the device was removed in the immediate area.

A study of photo radar on a West German autobahn to increase compliance with a 100km/h limit in the late 1970's (Blackburn & Glanz, 1984, cited in Freedman, Williams & Lund, 1990) reported increased compliance with the limit and resulted in a reduction from 300 crashes, 80 injuries and 7 deaths to 9 crashes, 5 injuries and no deaths.

In Victoria, a trial of speed cameras used from manned, unmarked stationary vehicles, with and without warning signs, was undertaken between 1985 and 1987. The study found that greater reductions in travel speeds resulted with the presence of highly visible warning signs compared with sites without warning signs (Portans, 1988). The data also indicated that the "halo" effect around the sites was limited, although no indication is given as to the size of the halo achieved. The "halo effect" is a term used to describe the effect on traffic behaviour beyond the point where enforcement is applied (Dart & Hunter, 1976, cited in Portans, 1988). Portans (1988) also concluded that media publicity is also an important factor for reducing speeds.

Speed enforcement studies have generally found that drivers respond to highly visible Police vehicles by suppressing vehicle speeds for up to a maximum of 5 km after the enforcement symbol, and this can occur regardless of whether the initial vehicle speed was over the limit (Portans, 1988). Leggett (1988) demonstrated that on 3 rural highways in Tasmania, low levels of highly visible but *random* placement of marked Police vehicles can increase the halo effect to 21 km and result in significant reductions in crash rates. Brackett & Edwards (1978, cited in Leggett, 1988) found a similar effect with such randomised enforcement. Most of these studies have been undertaken on highway stretches with little attention to effects of speed enforcement

in urban areas. Most studies indicate however, that the effects of highly visible and limited speed enforcement are highly localised and temporary.

Armour (1984) reported an experiment at 2 urban sites in Sydney primarily to investigate memory effects of speed enforcement. A memory effect is defined as reduction of travel speeds at sites where enforcement symbols had previously been operating, even when it is longer present. A memory effect of at least 2 days after the enforcement symbol was removed was found for commuter drivers, whilst no memory effect was found for afternoon (non-commuter) situations. The study also suggested that drivers returned to their normal driving behaviour soon after passing a police vehicle, and a halo effect was achieved only while the police operation was visible to the driver. A Canadian study (Hauer, Ahlin & Bowser, 1982) reported a 3 day effect after one enforcement period and that this increased to 6 days after repeated enforcement. They found no difference between drivers who occasionally passed the site and those who passed it daily. A Swedish study (Engdahl & Nilsson, 1983; cited in Bjornskau & Elvik, 1990) claimed an effect of about 14 days when passing the same (enforcement) site. The effects did not differ if drivers had passed more than one enforcement site. No lasting effects were found for enforcement from unmarked vehicles.

The current program under evaluation is somewhat similar to the Victorian speed camera trial (1985 to 1987) to the extent that unmarked police vehicles and no warning signs are used, but is on a much larger scale and is supported by much more extensive publicity than any previous enforcement program in Victoria. The current type of enforcement program leads to a much larger number of drivers being detected and punished (specific deterrence), compared to highly visible enforcement which attempts to deter exposed drivers by the fear of being detected by increasing their belief that they will be caught (general deterrence). Traditional deterrence theory postulates that deterrence to committing offences occurs through either of these two mechanisms.

The assumption is that if enough drivers are detected speeding and there is widespread awareness of the program through publicity, a generalised effect on crashes is possible; that is, through a high level of specific deterrence supplemented by publicity enough offending drivers who have been punished will change behaviour elsewhere, and indirectly deter other non-punished drivers from speeding through word of mouth and publicity. To date there is no evidence in the research literature that speed enforcement effects generalise to other than the immediate vicinity of the actual enforcement (Portans, 1988), but the authors of this report are unaware of a program of this scale having been evaluated previously. In addition, the postulation of two global deterrence concepts, specific and general, needs to take into account the more common response to risk of detection and punishment, particularly in relation to speed enforcement, of *restrictive* deterrence where drivers curtail rather than entirely refrain from their violations of the law, to avoid getting caught (Gibbs, 1975; cited in Homei, 1980).

Deterrence literature generally is concerned with understanding the process of deterrence, whether deterrence truly occurs and if so how. The process is complex involving theories of decision-making processes and the various factors (psychological and social) which impinge upon it. The general model underlying

modern deterrence theory is that legal threat (perceived likelihood of being detected and punished) is one of many factors which influence drivers' risk taking behaviour, and that these factors do not necessarily pertain, as thought previously, to rational cost benefit weighting of options. Personal and extra legal factors can weaken the impact of legal factors. Many factors other than laws, enforcement and punishment, which are not necessarily 'rational', may intervene to encourage or discourage propensity to offend.

The question however, still remains as to how to enhance legal sanctions and programs to give this factor the maximum possible impact on the maximum number of drivers. Past experience in other road safety programs suggest that in order to obtain maximum effect publicity campaigns should accompany legal programs, enforcement should be maintained over time and be deployed in such a way as to maximise (exaggerate) drivers' perceptions of being detected and punished (the best example of this is Random Breath Testing). It is theorised that such programs pave the way for social attitudes and beliefs to change about the behaviour, which in turn reinforce the negative consequences of this behaviour and act to maintain behavioural changes.

On the surface this may appear simple but the transient nature and pervasiveness of speeding, the widely held belief that this behaviour is not necessarily unsafe, and the fact that speeds are often selected to match the surrounding road and traffic conditions means that speeding is not easy to deter, except temporarily, as suggested by past experience and research. A recent review of law enforcement evaluations and studies (Bjornskau & Elvik, 1990) did not reveal evidence of programs with long-term effects. Additionally, they estimated that actual enforcement needs to be increased substantially to impact upon drivers' perceptions of enforcement levels and the risk of being detected.

4.0 PROGRAM MECHANISMS INFLUENCING SPEED BEHAVIOUR

The speed camera program in Victoria may have generated several mechanisms via which drivers could have responded to reduce travel speeds. These include:

1. Receipt of a speeding TIN and associated demerit points (specific deterrence)
2. Receipt of multiple TINs, demerit points and/or licence suspension (specific deterrence)
3. Knowledge of other drivers who have received TINs (general deterrence)
4. Knowledge of actual speed camera operations (general deterrence)
5. Publicity about speed cameras and the risks of speeding (general deterrence)

At the very least this information helps to describe the characteristics of the program, and to interpret the results of the formal evaluation study. The information can also be used as described under section 5.0, phase 2 of the evaluation study to link the effects on primary and secondary safety of the program mechanisms. The program mechanisms are described in turn below.

4.1 Specific deterrence

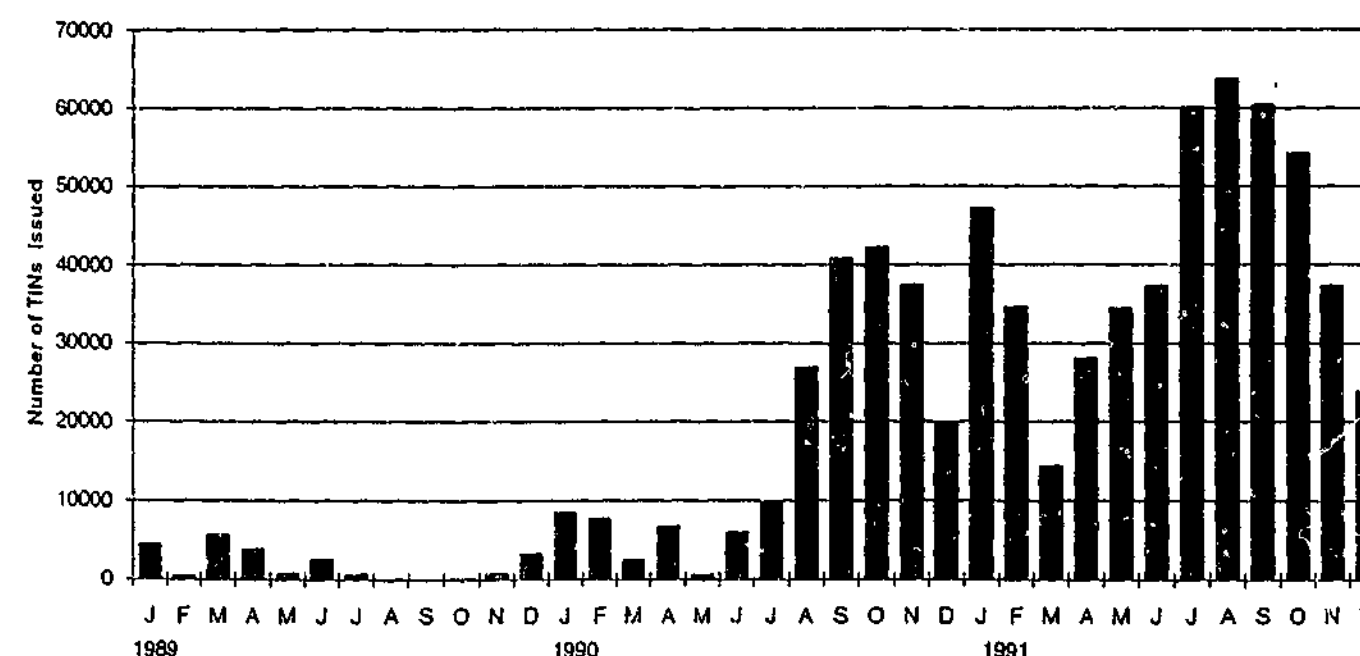
4.1.1 Receipt of a TIN

Specific deterrence refers to the modification of behaviour by direct punishment so that the individual avoids re-punishment. Drivers who have received TINs are possibly deterred from "speeding" to avoid further penalties.

Figure 1 shows the number of speed camera offences Victoria-wide over time. Figure 2 shows the number of all other speed offences Victoria-wide over time. As shown in Figure 1, punishment of drivers for exceeding speed limits overall has increased dramatically relative to pre-program levels (speed cameras had been operating at extremely low levels since 1987, and trialed between 1985 and 1987). The monthly level of offences through the Speed Camera Program rose 2 to 4 times the level achieved by traditional speed enforcement methods, with around 20,000 speeding offenders in all detected per month before July 1990 to 40,000 - 80,000 per month over the operation of the program.

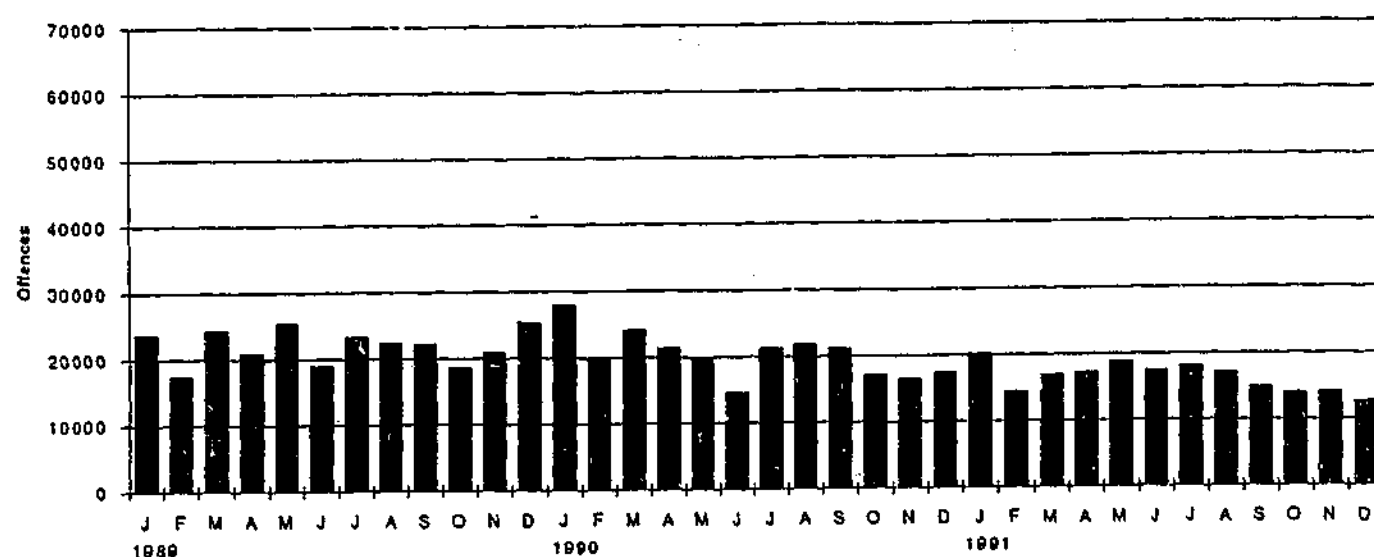
It should be noted however, that whilst speed camera operations have been conducted across the whole of Victoria, the level of camera operations have not been uniform across regions and speed zones; a far greater number of operations have been conducted in Melbourne compared with the rest of the State (refer section 4.2.2).

Figure 1
Monthly number of Traffic Infringement Notices Issued
for Speeding Offences Detected by Speed Cameras
Victoria 1989-1991



It should also be noted that the monthly number of TINs issued via speed camera detection (Figure 1) between April and June 1990 are not directly reflected by the monthly level of speed camera operations in this period, given that negligible use of speed cameras occurred between mid April to late June.

Figure 2
Non Speed Camera Speeding Offences,
1989-1991 Victoria



4.1.2 Receipt of multiple TINs

Drivers who have received multiple TINs may be further deterred given the approaching increase in penalty severity (ie. licence suspension) as licence demerit points are accumulated; and at a particular point re-offenders and/or excessive speeders are 'incapacitated' by licence suspension.

A maximum of 12 points in any three year period is permitted, after which a driver can choose between the following options: a 3 month licence suspension or an attempt at no extra points in the following 12 month period (if breached a six month licence suspension is invoked). More severe offences also incur higher penalties which results in faster accumulation of demerit points or even direct licence suspension. The penalties are as follows:

Offence	Fine	Demerit Points	Licence Suspension
Speed in excess of that permitted:			
1 - 15 km/h	\$105	1	
16 - 29 km/h	\$165	3	
30 - 39 km/h	\$220	4	1 month
40 - 44 km/h	\$300	4	4 months
45 - 49 km/h	\$300	6	4 months
50 km/h or more	\$360	6	6 months

Three sources of information are available which indicate the receipt of multiple TINs in the driving population.

Self-report data

An interview study (VIC ROADS, 1991, unpublished) was conducted in May 1991 and repeated in December 1991, in which a random sample of 1,050 licensed drivers were asked if they had received a TIN from a speed camera operation in the last 12

months, and if so how many. The responses indicated that 12 -14% of licensed drivers had been detected for speeding offences, and that the majority had had one offence only (particularly by December 1991). Furthermore, the number who had received three or more in the previous 12 month period appeared to have decreased in the second survey.

Table 1

Proportion of licensed drivers detected for a speeding offence by a speed camera

Number of speeding offences	May 1991 % drivers	December 1991 % drivers
One	6.2%	11.2%
Two	1.5%	1.8%
Three	4.0%	0.7%
Four	-	0.1%
Total one or more	11.7%	13.8%

(Source: VIC ROADS interview survey)

Traffic Camera Office TINs Issued Data

The TCO has undertaken an examination of the proportion of multiple offenders detected through the speed camera program in January and July 1992, using both driver licences and vehicle registrations as the basis for the calculations. This is summarised in table 2 below. The TCO data indicated that over 20% of drivers had been detected for speed camera offences since the beginning of the program (the interview survey referred to the percentage of drivers detected within a 12 month period only). The majority have only one offence although a significant proportion have had more than one TIN.

Table 2

Proportion of licensed drivers and registered vehicle owners issued a speeding TIN for an offence detected by a speed camera, Victoria

Number of TINs issued	January 1992		July 1992	
	% vehicle registrations	% driver licences	% vehicle registrations	% driver licences
One	13%	12%	12%	11%
Two	6%	4%	5%	3%
Three	2%	1%	2%	1%
Four	1%	1%	1%	-
Total one or more	22%	25%*	19%	22%*

(Source: TCO) * 7% do not have a licence and/or omitted to provide their licence details

Demerit points for licensed drivers

Table 3 shows six monthly figures since June 1990 of the proportion of licensed drivers with demerit points in Victoria (not exclusively for a speeding offence

detected by a speed camera). Drivers with one or more demerit points have increased fourfold between June 1990 and December 1991. Increases have occurred across all levels of demerit points. The largest groups are those with one or with three demerit points.

Table 3

Proportion of licensed drivers with demerit points in Victoria (not exclusively for a speeding offence detected by a speed camera)

Number of demerit points	June 1990 % drivers	Dec 1990 % drivers	June 1991 % drivers	Dec 1991 % drivers
one	1.5	4.8	6.7	8.2
two	0.7	0.9	1.3	1.7
three	3.3	5.0	6.6	7.3
four	0.4	1.0	1.6	2.1
five	0.1	0.2	0.4	0.6
six	0.3	0.6	1.1	1.3
seven	0.07	0.2	0.4	0.5
eight	0.03	0.07	0.1	0.2
nine	0.04	0.1	0.2	0.3
ten	0.01	0.04	0.1	0.2
eleven	0.01	0.02	0.05	0.07
twelve	0.01	0.02	0.05	0.08
more than twelve	0.01	0.03	0.09	0.2
Total one or more	6.5	12.4	18.7	22.8

(Source: VIC ROADS)

4.2 General deterrence mechanisms

General deterrence (in its classical sense) refers to the modification of behaviour by fear and avoidance of legal punishment. To be deterred by legal sanctions, drivers must perceive that they have a good chance of being detected and punished if they offend. Perceptions of being detected can be enhanced by indirect exposure to the program.

4.2.1 Knowledge of drivers caught

The VIC ROADS interview study (unpublished, 1991) indicated that 57% of licensed drivers knew someone who had been detected speeding by a speed camera, both in May 1990 and December 1991.

4.2.2 Knowledge of speed camera operations

In terms of actual speed camera operations, a high level of use of speed cameras was found Statewide (Sullivan, Cavallo, Rumbold & Drummond, 1992), increasing from around 250 hours per month in June 1990 to around 1500 hours or more per month throughout 1991. In total, in the period from July 1990 and December 1991, there has been 20,000 hours of total speed camera operation and 9,000 individual sessions.

Over 11.5 million vehicles have passed a speed camera session. However, the number of drivers who saw these operations is not known. Sessions tend to last around 2-3 hours and are conducted on all days of the week (although weekdays have a greater number of sessions). Sessions are mainly conducted in the morning and afternoon with none before 6am in the morning and only around 4% after 8pm in the evening.

Whilst speed camera operations have been conducted in all areas of the State, the majority have been conducted in Melbourne (70% of total sessions on average have been conducted in Melbourne). Furthermore, 80-90% of camera sessions have been conducted in 60 km/h zones in both of the respective metropolitan and rural parts of the State. The differences between Melbourne and rural speed camera operations until the end of 1991 are outlined in Sullivan *et al* (1992) and are illustrated by the data below.

Melbourne:

- a total of around 6,300 speed camera sessions, with about 97% in 60 and 75 km/h speed zones
- 60,000 vehicles per month detected over set speed threshold
- non-speed camera speed offences declined from 10,000 - 12,000 per month to 6,000 - 7,000 per month

Rural Victoria:

- around 2,700 speed camera sessions in all, with 85% in 60 and 75 km/h speed zones
- 12,000 vehicles per month detected over set speed threshold
- non-speed camera speed offences dropped from 10,000 - 12,000 per month to 6,000 - 7,000 per month

The analysis of speed camera operations indicates that speed offences through enforcement measures other than speed cameras have reduced equally in Melbourne and rural Victoria, but speed camera enforcement is much greater in Melbourne than in rural Victoria. This suggests that there has been a much greater net increase in speed enforcement in Melbourne than in the rest of the State, which in fact appears to have experienced little change in actual net speed enforcement.

4.2.3 Mass media publicity

TAC mass media speed camera and speed related crash publicity with the theme "Don't fool yourself - speed kills" began in April 1990 and was maintained at intensive levels, particularly in 1990. TARPs (Target Audience Rating Points) is a measure relating to the estimated percentage of people (in the target group) who were likely to have seen the advertising, and thus provides a measure of the amount and intensity of television advertising. Monthly TARPs data revealed intensive periods of TAC speed publicity for most months in 1990 and around 4 months in 1991. A total of 5280 and 1772 TARPs relating to speed were achieved in 1990 and 1991, respectively on Melbourne television, which is a high level of publicity relative to previous levels of road safety publicity in Victoria.

5.0 EVALUATION PLAN FOR ASSESSING THE PROGRAM'S EFFECTS ON CRASHES

The paucity of relevant information about the effects of different types of speed enforcement on speed behaviour and speed-related crash risk is obvious from the review of existing research literature in the preceding sections of this report. The deterrence mechanisms underlying behavioural changes are also not well understood. Such information is important for guiding the structure of an evaluation of a program which is directed at behavioural change, such as the speed camera program. For example, information about where and for how long travel speeds (and therefore speed-related crash risk) are most likely to be affected by this type of enforcement allows identification of where and when the testing for crash effects of the program should be undertaken. Without this information, knowing where and when to look and test for effects is difficult to define.

Consequently, testing for effects in this evaluation has been conceived at a number of levels so that the evaluation is both as comprehensive and definitive as possible. The effects of the program on crashes was evaluated at three levels:

1. General effects Victoria-wide, in the Melbourne metropolitan and rest of State areas, and specifically on arterial roads in the respective areas
2. Effects through the specific and general deterrence mechanisms associated with the program (defined in section 4.0)
3. Localised effects in time and space related to the speed camera operations.

These are outlined below.

1. General Effects: Victoria-wide, Melbourne metropolitan, and rest of State areas, and on arterial roads in the respective areas

At this level, the evaluation attempted to determine the area-wide effects (if any) of the program on the evaluation criteria (casualty crash frequency and crash severity), as it operated during different periods during 1990 and 1991. This approach was taken because it was considered possible that the program could have generalised or dispersed effects beyond the immediate speed camera sites and times, given that speed cameras are generally not obvious, are used at many sites, use has been maintained at high levels (especially relative to previous speed enforcement) and related publicity has also been pervasive and intensive.

However, it is recognised that such an approach does not necessarily provide:

- the best test of the effects of the program (particularly if the effects are small and/or highly specific to camera locations), nor
- information about the how the program achieved its effects so that decisions can be made for future strategies for the program.

This phase of the evaluation was likely to provide equivocal conclusions regarding cause-and-effect. Two extra levels of analysis were undertaken to address these needs.

2. Effects through specific and general deterrence mechanisms

An attempt was made to develop a more detailed model with the various mechanisms generated by the program represented quantitatively. The measured mechanisms generated by the program were then be assessed for their relative impact on the evaluation criteria.

Time series data is available relating to:

1. number of TINs issued to offenders,
2. proportion of drivers with one or more TINs
3. proportion of drivers with a varying number of demerit points,
4. proportion of drivers who are aware of others who have been 'punished',
5. amount of paid television publicity, and
6. hours of speed camera operation.

Measures 1, 5, and 6 are available on a monthly basis, 3 is available on a quarterly basis, whilst 2 and 4 are available for particular dates only (shown and discussed in section 4.0). Thus, measures 2 and 4 cannot be used in the statistical analyses, but provide useful descriptive information about the program. Measure 3 was also problematic because it was available only Statewide and on a quarterly basis, and the appropriate quantitative representation of it in the model was not obvious.

Therefore an attempt was made to determine the relationship (if any) between deterrence mechanisms 1, 5, and 6 and the evaluation criteria.

Problems with this approach have been outlined previously in an evaluation of the Random Breath Testing (RBT) initiative in Victoria during 1989-91, where the feasibility of dissociating the effects of enforcement and publicity was discussed (Cameron, Cavallo & Sullivan, 1992). These include the statistical problems of collinearity between the measures and between the measures and other influences, the appropriate representation and quantification of the mechanisms within the model (eg. does a measure of enforcement, such as number of TINs issued, represent its effects on the public and therefore does this provide a fair test of its relationship with crashes?), and the possible non-linear relationships between the mechanisms and crash risk.

In addition, some of the mechanisms which may act to influence speed behaviour, would also at the same time be measures of behavioural change: eg. the level of TINs issued reflects both the mechanism influencing speed behaviour, and if the program is successful, an outcome indicator. The modelling of variables which could be both

measures of treatment (independent variable) and of response to treatment (dependent variable) as independent variables, will diminish the power of this approach, particularly for later periods in the intervention when driver responses, if any, are most likely to be manifested in such measures.

The advantage of this approach is that it allows the form of the program to be directly represented in time by the changes and variations in intensity of the respective elements of the program, thus the variations in the program can be tested against changes and variations in crash risk. The difficulty is validly representing the form of the program, and the reliance on available quantified measures of the program for the model.

3. Localised effects in time and space related to speed camera operations

As noted earlier, the likely distribution of effects of the enforcement is not known. There are good reasons for testing, however, effects in varying times and areas related to the times and locations of operations. One of the most compelling reasons to do this is the transient nature of the target behaviour, that is, drivers can very quickly vary their travel speeds in response to a number of factors, including perceived enforcement presence (whether based on seeing operations, previous experience of enforcement at a location via receipt of a TIN, warnings from other drivers, or familiarity with and predictability in camera deployment).

The question is how long and how far are these effects maintained, because the greater the suppression of high risk travel speed, the greater the chance of decreasing speed related crashes and/or decreasing the severity of crashes which do occur. Operations which maximise the 'suppression' will achieve the greatest effects, and hence recommendations can be made for optimising enforcement strategies.

The advantage of this approach is that it provides the most definitive assessment of the cause-and-effect connection of the program if effects are found, compared with phases 1 and 2. This is because effects can be directly linked to the cause (operations) and by definition, generalised effects cannot occur if there are no local effects. The added advantage of this detailed analysis is that the effects of the program over time may be determined.

This approach, however, relies on knowledge of speed 'suppression' distributions across locations and times of driving, which is currently lacking, although some indications can be found from research into different speed enforcement techniques in different settings. Additionally, the selection of an appropriate research design could also pose problems (as has been the case for all phases of this evaluation), particularly the selection of appropriate comparison or untreated crash subsets.

This phase will be completed during 1993.

6.0 PHASE 1: EVALUATION OF GENERAL CRASH EFFECTS

6.1 Temporal structure of crash data corresponding to the program

As noted in Section 4.0, the program to date has been characterised by three distinct periods¹:

T1a a period of low level camera trialing and localised low level publicity from December 1989 to March 1990,

T1b a period characterised by a high profile media launch of speed cameras and intensive publicity relating to speed-related crash risk, but little speed camera enforcement (ie. after an intense 9 day burst of camera use, negligible levels of speed camera use, offence detection and hence TINs issued) between April and late June 1990,

T2 the subsequent period in which enforcement increased dramatically, and thus also the detection and punishment of speed offenders, and was maintained at high levels, as was supporting publicity starting from July 1990 onwards.

Periods T1a and T1b essentially represent an introductory period prior to the large changes in speed enforcement practises, and as such is a "one-off" occurrence for the program. The T2 period had to be divided into the time between July 1990 and February 1991 (T2a) and March to December 1991 (T2b), to enable the most valid estimates of effect to be determined, for reasons outlined in section 6.5.2.

6.2 Spatial structure of crash data corresponding to the program

Speed cameras have been used to a different extent in the Melbourne metropolitan area and rural areas in the State, and to differing extents on different zoned roads (section 4.0), which may or may not correspond to relative driver exposure (distance travelled) in these zones. Therefore, analyses were undertaken for each of the following treated areas:

Victoria	Melbourne	arterial roads (all zones)
		arterial roads (60 km/h zones)
	Rural Victoria	arterial (open) roads (100km/h)
		roads in rural towns (60 km/h zones)

¹ The periods identified are based, more or less, on the presence or absence of parts of the package, ie publicity or enforcement. Further distinctions of qualitatively and quantitatively different program periods could be identified, however, the evidence for these in the data is not great and the assumptions would need to be made that differing amounts of enforcement and/or publicity (eg. 1,500 hours per month as opposed to 2,000 hours per month of camera operation) result in different outcomes or effects.

6.3 Hypotheses

Phase 1 of the evaluation study aimed to test and quantify the following hypotheses relating to the introduction of the speed camera program:

- (a) a change in the evaluation criteria in Melbourne and the rest of Victoria (separately) and Victoria (as a whole) during T1a, T1b, T2a and T2b
- (b) a change in the evaluation criteria on arterial roads (all speed zones) in Melbourne during T1a, T1b, T2a and T2b
- (c) a change in the evaluation criteria on arterial roads zoned 60km/h in Melbourne during T1a, T1b, T2a and T2b
- (d) a change in the evaluation criteria on rural open roads (zoned 100 km/h) during T1a, T1b, T2a and T2b
- (e) a change in the evaluation criteria in country towns (60km/h zones) during T1a, T1b, T2a and T2b
- (f) a relative change in the evaluation criteria on arterial roads in Melbourne relative to residential streets in Melbourne during T1a, T1b, T2a and T2b

The hypotheses are all bi-directional and hence two-tailed significance tests were applied throughout the study, as there is no precedent of such a program and therefore research establishing any directional effects of the program.

6.4 Evaluation criteria

The possible dual role of vehicle speed in crash occurrence and crash outcome (injury severity) indicated in previous studies required that both aspects be assessed as possible outcomes of the program.

Crashes reported to Police in Victoria were used in the study. Casualty crashes only were examined. Property-damage-only crashes were not included in the analysis since they have not been coded since January 1991, and prior to this only approximately 20% were actually reported to Police. Thus, property-damage-only crashes may not be representative of all such crashes in the population. Casualty crashes consist of fatal crashes (those in which one or more persons is killed), serious injury crashes (those in which one or more persons is seriously injured and probably admitted to hospital) and minor injury crashes (in which one or more persons requires medical treatment).

Casualty crash frequency

Changes in the frequency of casualty crashes (defined above) as a whole were assessed.

Although it would be more desirable to have a measure of the incidence of speed-related crashes over time, it was not possible to identify speed-related and non speed-

related crash types (Appendix A). The consequence of using all casualty crashes for this criterion is the possible dilution of effects if speed-related crashes are only a small sub-set of all casualty crashes, making detection of an effect more difficult under this scenario. It is not clear, however, how large the target group of crashes might be and therefore whether this is a problem for the analysis. On the other hand, the arbitrary grouping of crashes into speed-related and non speed-related categories could result in incorrect categorisation and therefore an incorrect target group for assessment.

Casualty crash injury severity

The change in the injury severity of crashes was assessed by testing for changes in the relative proportion of crashes in each of the police reported crash severity levels of:

1. crashes which resulted in one or more fatalities or one or more persons admitted to hospital or seriously injured (serious casualty crashes)
2. crashes resulting in one or more persons with minor injury (minor injury crashes)

The ratio of fatal plus serious injury crashes to crashes involving minor injury only was used as the dependent variable representing crash injury severity.

Fatal and serious injury crashes were combined because of the lower numbers (statistically) of fatal crashes. If the program was successful, fewer crashes resulting in death or serious injury over time should have occurred.

6.5 Research design

The estimated percentage change in the evaluation criteria (casualty crash injury severity and casualty crash incidence) in each area described in section 6.2 during each of the post-intervention periods (T1a, T1b, T2a, and T2b) was adjusted by the parallel estimated percentage change in the comparable area in New South Wales (comparison areas) for the same periods. Taking into account parallel changes in comparison areas is a method for excluding the effects of extraneous influences on the evaluation criteria in the treatment areas, so that estimates reflect the impact solely of the program.

Multivariate time series models for each of the crash measures were developed for each of the treatment and comparison areas, so that the changes during each of the post-intervention periods of the initiative could be estimated for each series.

Percentage changes in each area could then be contrasted allowing the *net percentage changes* in each treatment area to be estimated. The net change provided an estimate of the percentage change in each of the crash measures that is attributable to the speed camera program.

6.5.1 Time series analysis

The time series method can control for long term trends, seasonal cycles and other regular patterns in the outcome variables. Road safety measures which have gradually decreased high risk factors related to the incidence and severity of crashes (eg. gradual decreases in drink driving, seat belt non-wearing, and excessive speeding; safer vehicles, improved road environments and traffic management) are represented as longer term, "smooth" trends in crash criteria over time. Such trends in a crash series were taken into account through a trend component in the time series models. This eliminated the need to include specific measures of such variables in the models to take them into account, and allowed a better assessment of the real effect of a new or abrupt change to the crash series in time.

The aim of the modelling process was to develop models that closely fit the number and severity of casualty crashes over the period January 1983 to December 1991, for each series (treatment and comparison areas), and to estimate the relationships between the variables in each model and the crash criteria.

6.5.2 Confounding factors

RBT in Victoria

The almost simultaneous introduction in Victoria of the RBT initiative late in 1989 meant that the effect on crashes of the two initiatives could confound estimations ascribed to the speed camera program only. RBT operates and targets crashes in high alcohol times (generally night-time) when 38.4% of serious casualty crashes (fatal and serious injury crashes) are alcohol related. Serious casualty crashes in low alcohol times (daylight hours) only include 4% where alcohol is a factor. Low alcohol times of the week are defined as Monday - Thursday 6am to 6pm, Friday 6am to 4pm, Saturday 8am to 2pm, and Sunday 10am to 4pm (Harrison, 1990).

The majority of speed camera sessions have been in daylight hours, although there was some overlap with high alcohol hours (17.6% averaged across the whole week) particularly on weekends when high alcohol times start earlier (around 50% in high alcohol hours). It is not known if driver responses to speed cameras (if any) are limited to their times of operation or whether speed behaviour has also been modified at night (which is encompassed by high alcohol times). Although there is yet no evidence for generalisation of behaviour beyond specific times and locations of speed enforcement, the current program is very different to previous enforcement programs.

Whilst it would have been desirable to estimate the effects (if any) of the program for crashes at night, it is only possible to do this for low alcohol hour crashes, when RBT effects are considered to be minimal. The proposed analyses are therefore limited to casualty crash severity and incidence in low alcohol hour times (42% of all times of the week).

Speed Cameras in NSW

Speed cameras were introduced in NSW in March 1991, although the use of cameras was different from their deployment in Victoria. This meant that the use of NSW as a comparison area for estimating the absolute effects in Victoria beyond this period was not possible. Therefore the proposed analyses are separated into comparisons of the T2 period between July 1990 and February 1991 (T2a) and March to December 1991 (T2b). T2b represents an estimation of the effects in Victoria over and above the effects of 'other factors' as well as the effects (if any) of the speed cameras in NSW.

6.5.3 The role of interstate comparisons

The interstate comparison areas provided a form of 'control' for the influence of other factors that may have also affected the crash measures in this period. The assumption made is that the changes in the comparison areas represent the effects of 'other factors' on the target crashes in the treatment areas, so that changes in comparison areas can be used to estimate the changes that would have occurred in treatment areas in the absence of the treatment (ie. the speed camera program).

Such interstate comparisons cannot provide perfect control since economic and other factors are not identical in both areas. However, it provided the best available comparison area for a post-hoc evaluation design of a pervasive Statewide intervention (whose effect has been conceptualised at the broadest area-wide level) for this phase of the evaluation.

New South Wales was considered the most appropriate comparison State for Victoria because it is the most comparable in terms of urbanisation, economic activity, population, size and other characteristics than other Australian States. If there are statistically significant reductions in NSW comparison areas for the periods being contrasted, however, it would be difficult to determine whether these in fact reflect the effect of other factors or whether they represent the effect of other (road safety) countermeasures in that State. Therefore, its adequacy as a comparison area is difficult to assess.

An examination of the availability of information on the major factor influencing crash levels, exposure to crash risk generally, prior to and during the intervention period was made for Victoria and NSW. This factor was considered to confound the role of NSW as a comparison area and so a proxy measure (unemployment rate) was introduced as a covariate in the multivariate time series models (see section 6.5.4).

6.5.4 Covariates-controlling for exposure using economic indicators

It was considered necessary to include a measure of driving exposure during low alcohol hours (essentially day time) in the models fitted for Victoria and NSW so that possible differences in the two States would be taken into account. It is well established that driving exposure is directly related to crash risk, and it was considered that although the interstate comparison provides some level of control for national changes in travel exposure, the different economic changes in the two States

(eg. higher unemployment rate in Victoria compared to that in NSW since 1990) may have led to a greater reduction in vehicle travel in Victoria than in NSW.

Unfortunately, direct measures of total vehicle travel, or vehicle travel for different times of the day and days of the week (eg. through traffic counting programs) over time for the areas of interest were not available.

However, correlates with total vehicle travel such as fuel sales (Lambert, 1992) and unemployment figures (ABS, 1991,a) were available. Fuel sales data has been adjusted by fuel consumption rates and corrected to equate with the Australian Bureau of Statistics triennial Survey of Motor Vehicle Usage (ABS, 1991,b) to give an estimate of total travel in each State (Lambert, 1992). This estimate is only available on a Statewide basis whereas unemployment figures are available for metropolitan and rural areas separately. Both are gross measures and the extent to which they reflect vehicle travel for particular times of the day and week is not known. However, low alcohol times correspond to commuter travel times and would make up the bulk of total vehicle travel, making the use of gross proxy measures of total travel for low alcohol time exposure appropriate. In addition, previous research both in the United States (Partyka, 1984) and Victoria (Thoresen, Fry, Heiman & Cameron, 1992) has demonstrated significant correlational relationships between overall fatalities and unemployment measures over time.

An examination of the data for Victoria and NSW shows that the trends in estimated total vehicle kilometres travelled since 1983 for the two States were essentially parallel, increasing until 1990 when Victorian travel appears to decrease whilst travel in NSW remains steady (Figures 3 & 4). Similarly, unemployment rates for Melbourne and Sydney (Figures 5 & 6), rural areas for both States (Figures 7 & 8), and the total State (Figures 9 & 10) showed decreasing parallel trends over time until late 1989/1990 when rates in both States increased and those for Melbourne and rural Victoria overtook those in Sydney and rural NSW, respectively.

These differences in trends in both estimated total travel and unemployment rate just prior to and during the intervention period suggest that there could be differences in changes in low alcohol hour travel between the two States. It was decided to include unemployment rate as a covariate for each time series developed to control for the differential effects of this factor in the treatment and comparison areas. This measure was preferred to estimated total vehicle travel, given its availability for the metropolitan and rural areas of each State, providing more specific control for the two different types of areas.

Figure 3
Estimated Total Vehicle Kilometres Travelled
(from Fuel Sales Data)
Victoria v. NSW

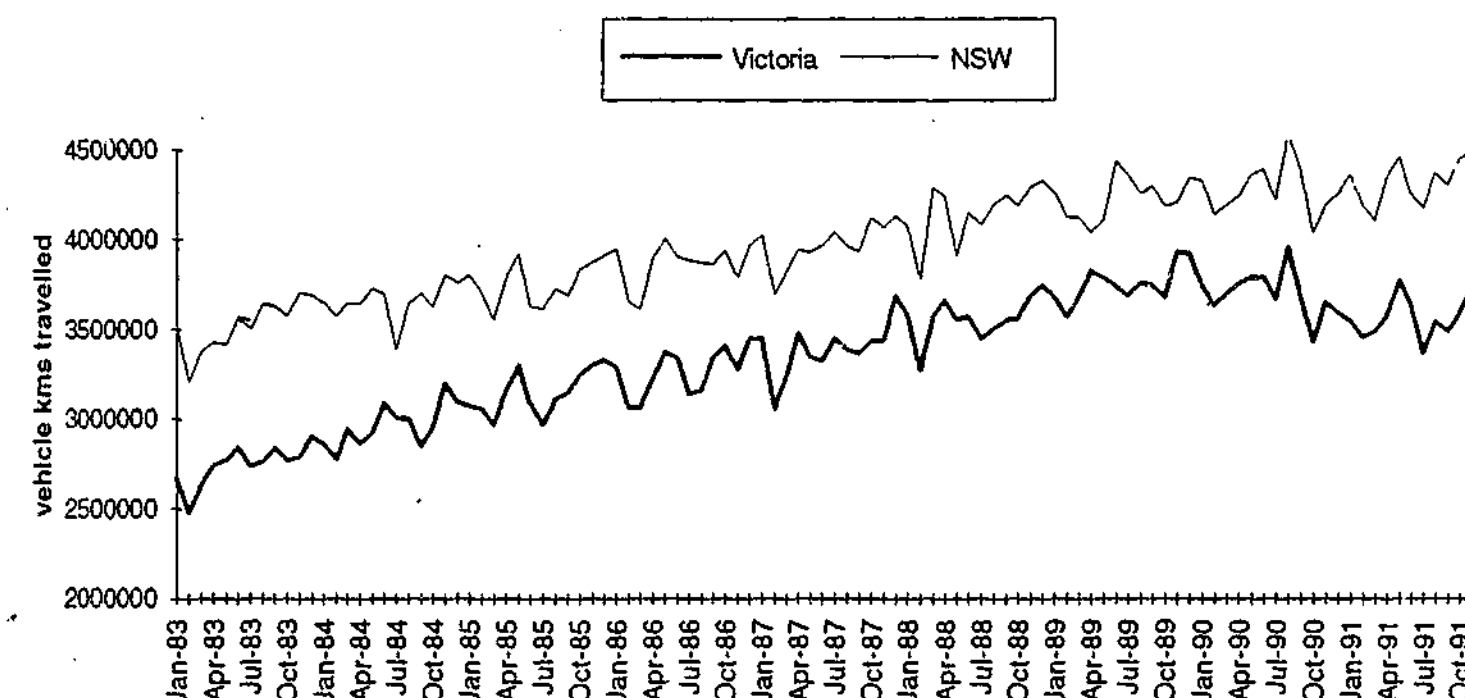


Figure 4
Estimated Total Vehicle Kilometres Travelled
(from Fuel Sales Data) 12 Month Moving Averages
Victoria v. NSW

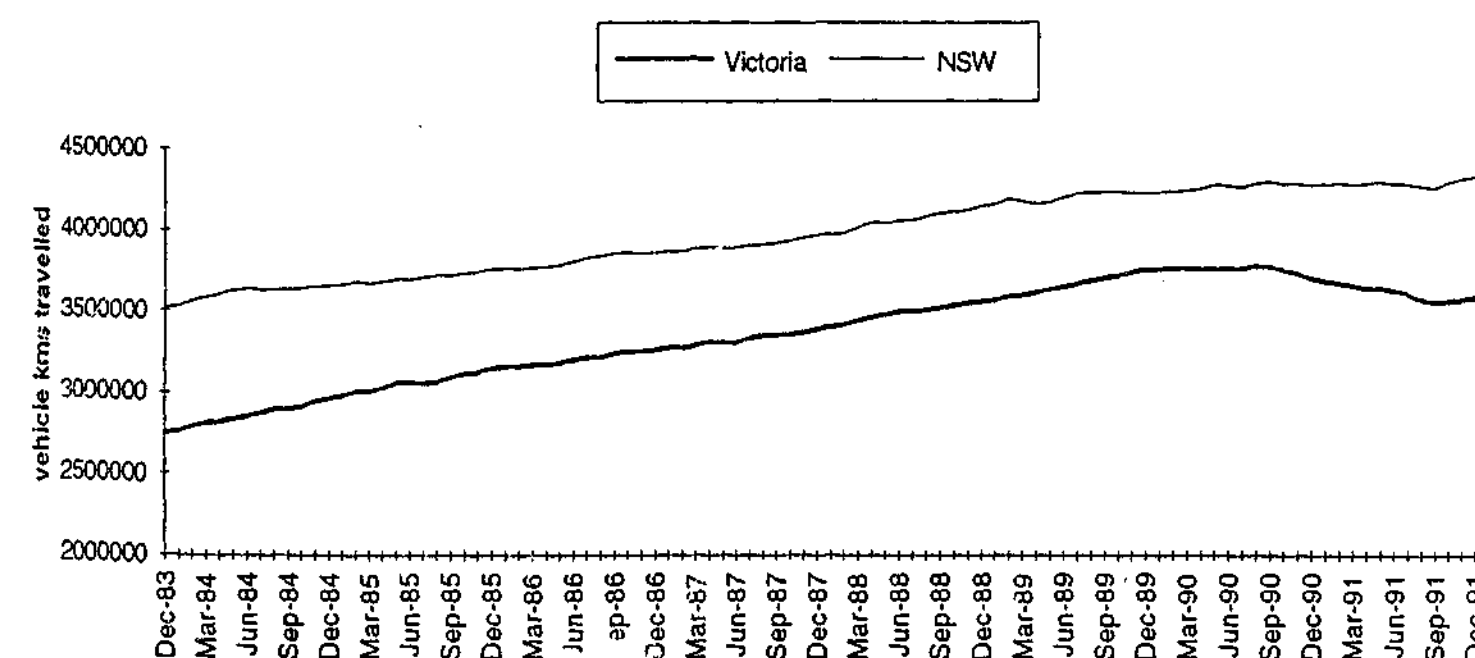


Figure 5
Unemployment Rates - Melbourne v. Sydney

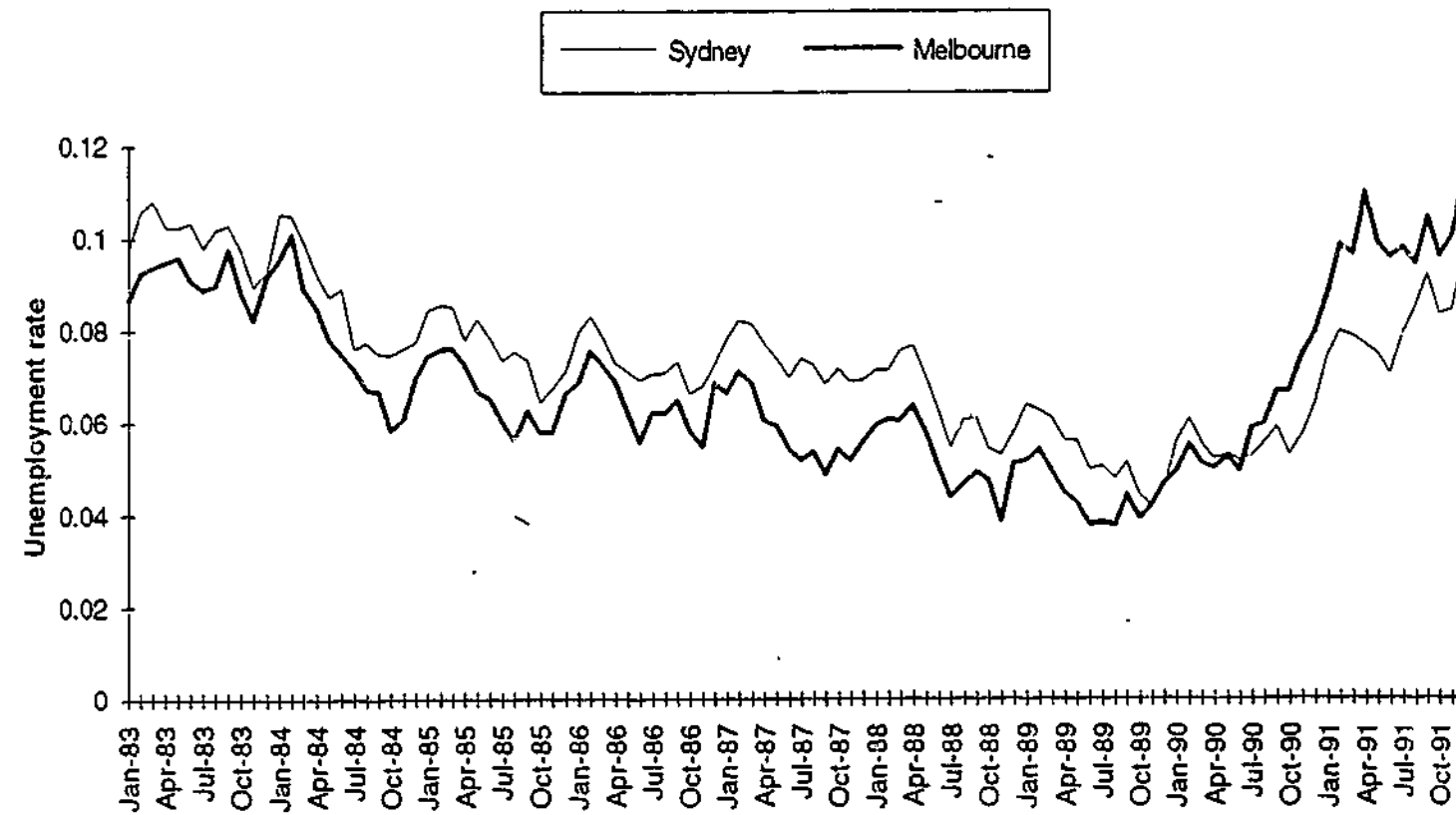


Figure 6
Unemployment Rate - Melbourne v. Sydney
12 Month Moving Averages

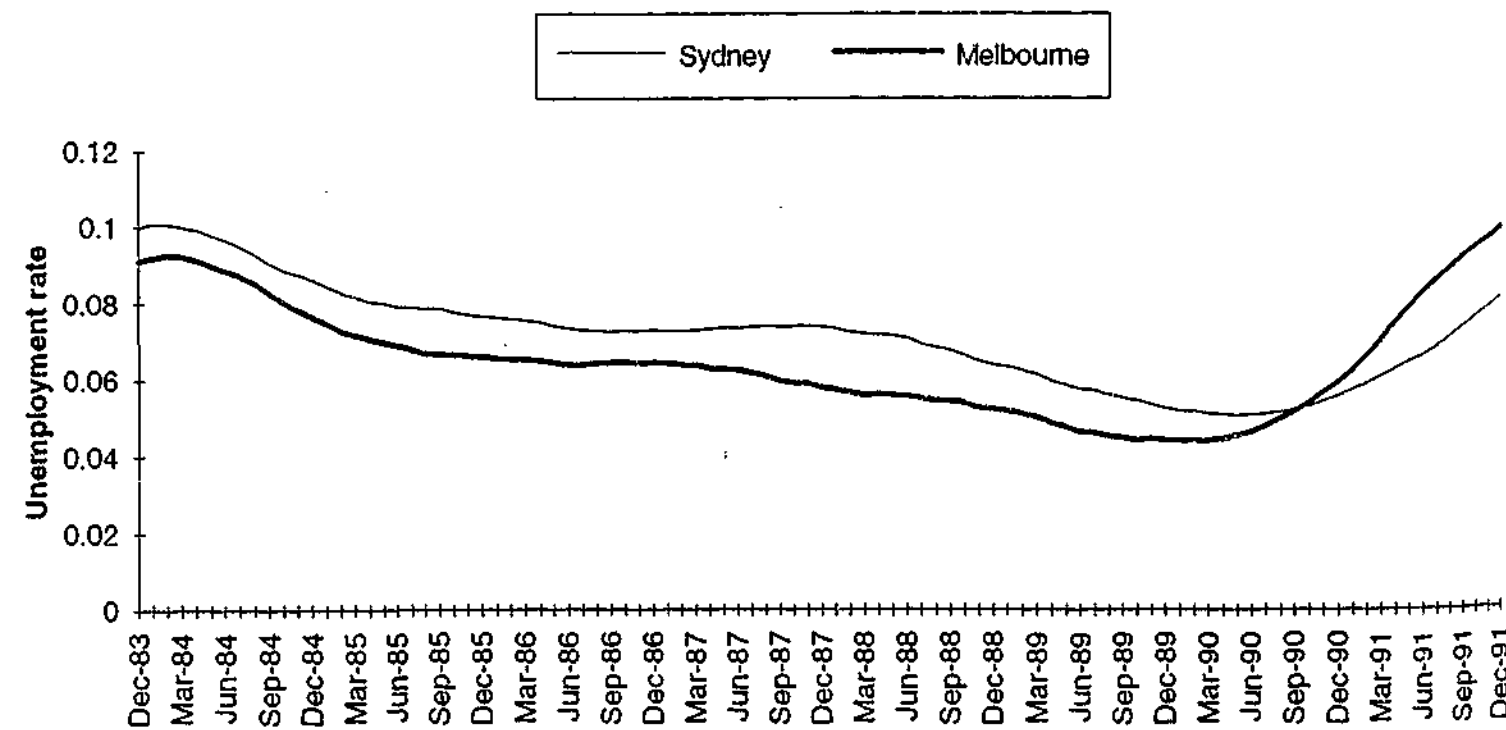


Figure 7
Unemployment Rates - Rural Victoria v. Rural NSW

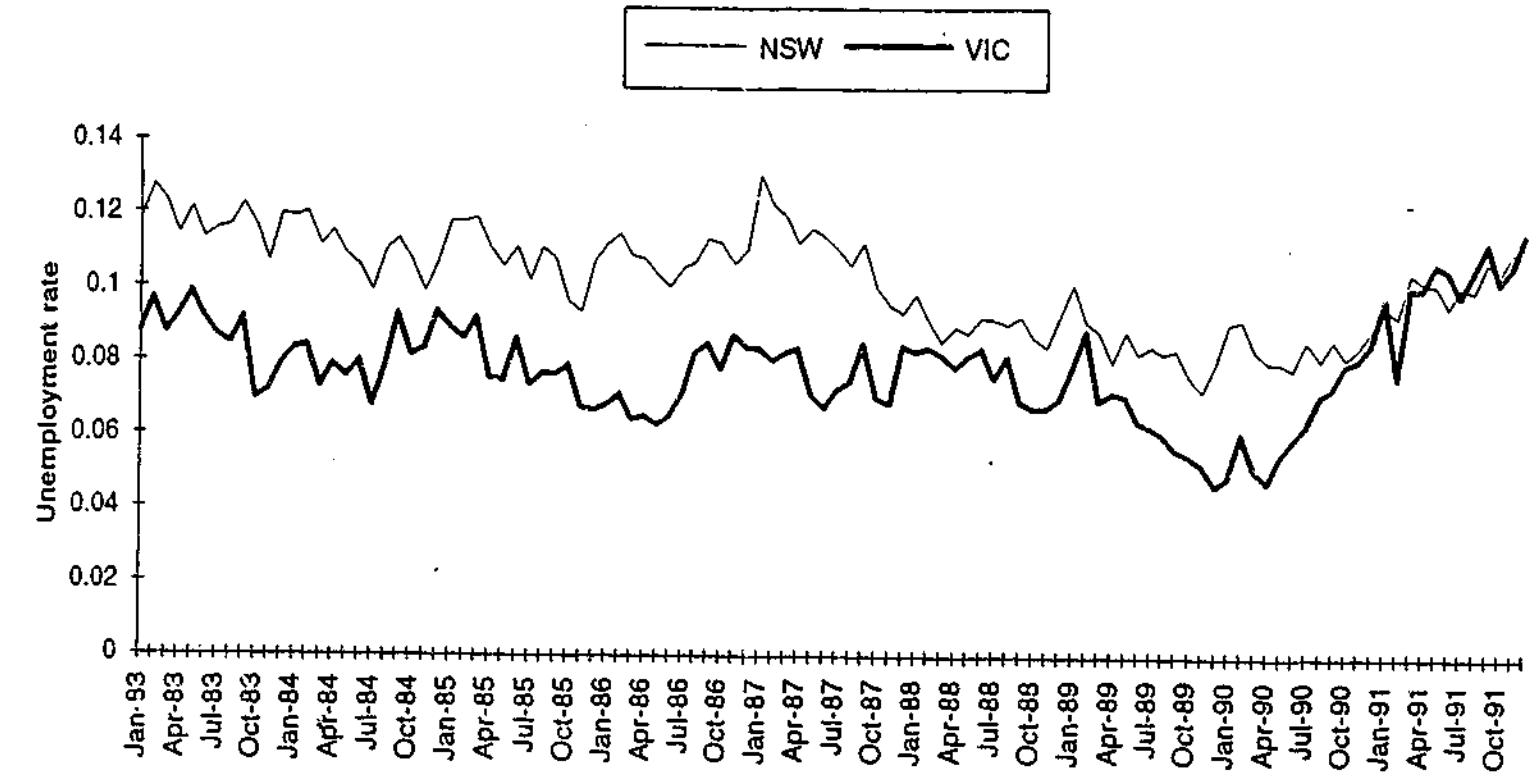


Figure 8
Unemployment Rates - Rural Victoria v. Rural NSW
12 Month Moving Averages

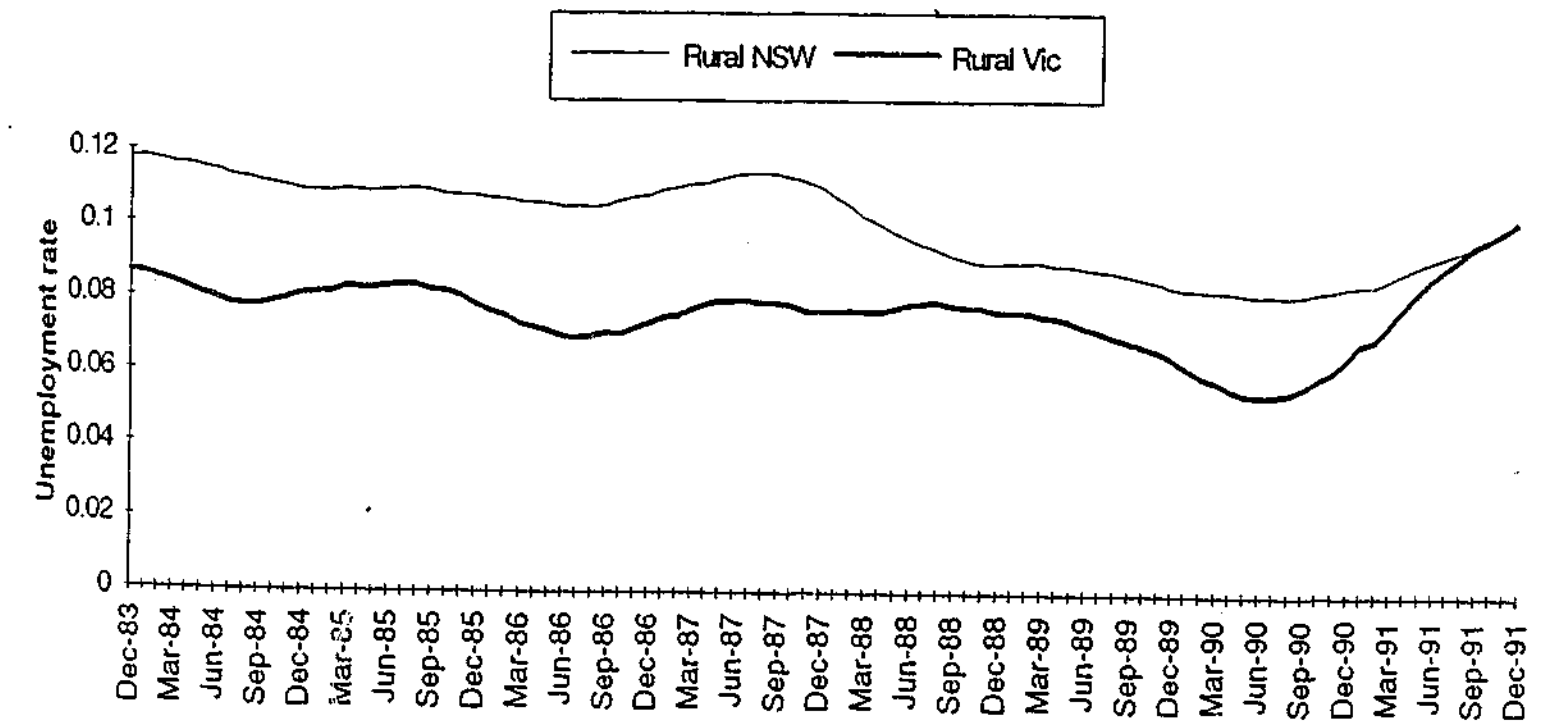


Figure 9
Unemployment Rates - Victoria v. NSW

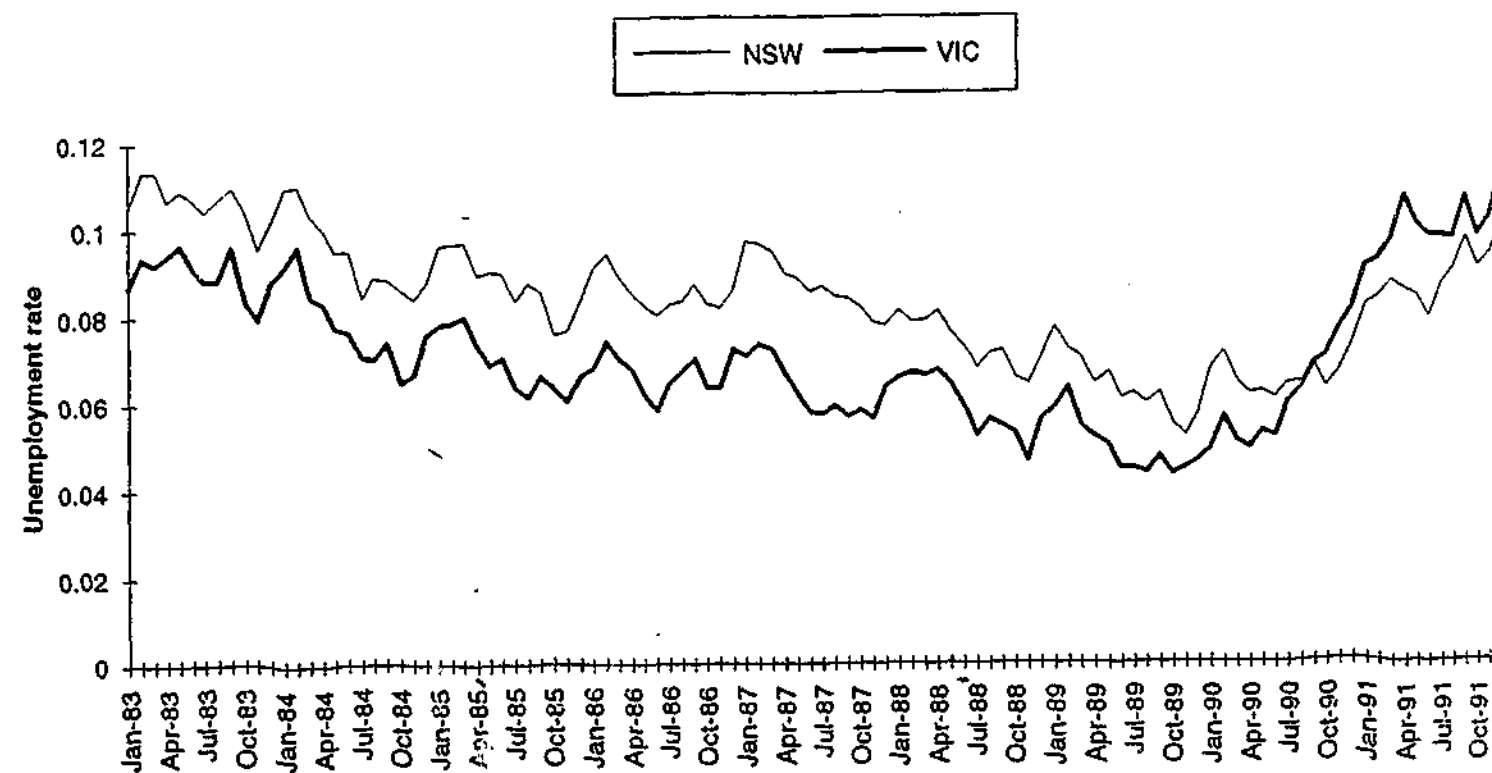
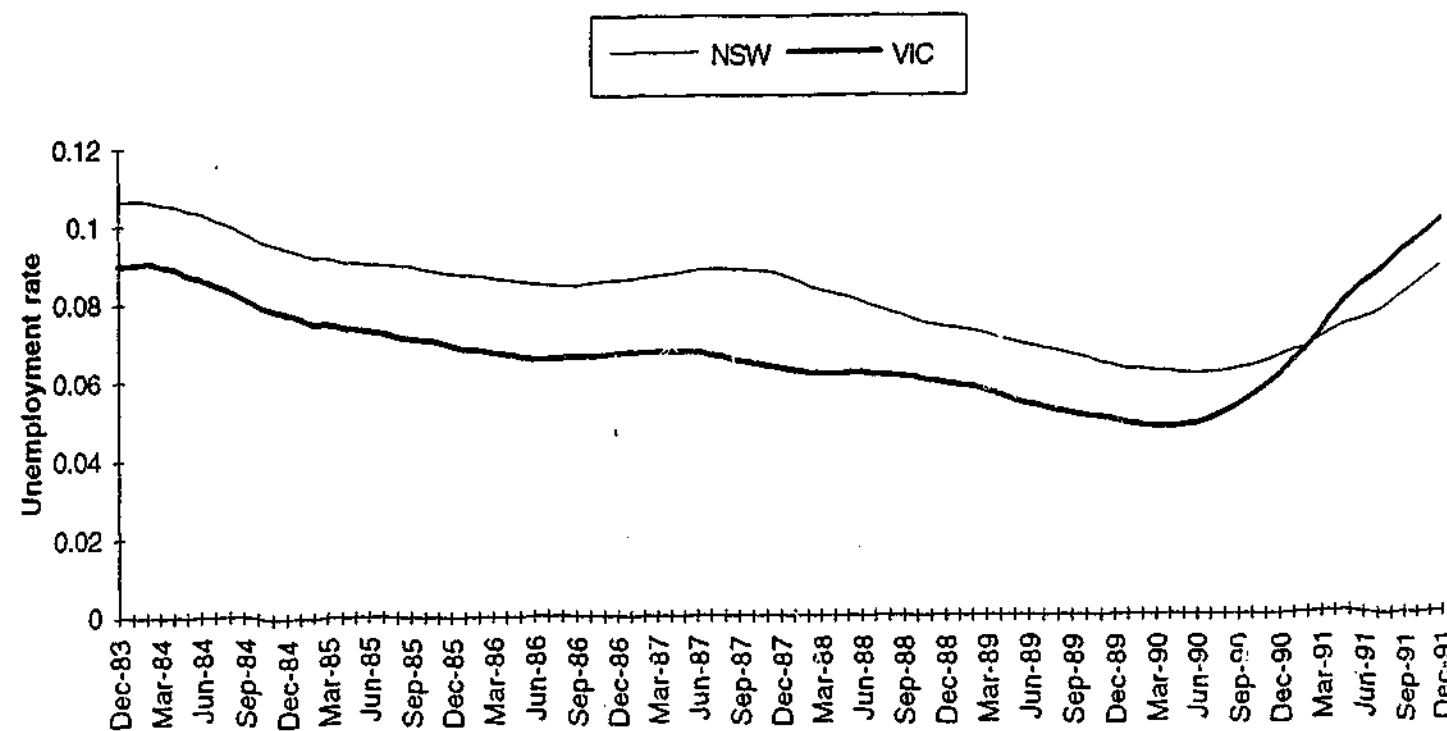


Figure 10
Unemployment Rates - Victoria v. NSW
12 Month Moving Averages



The correlation coefficient between estimated total travel and total unemployment rate in Victoria was -0.52 and in NSW was -0.69. Significant correlations (between -0.3 and -0.7) were also found between Statewide vehicle travel and unemployment rates for metropolitan and rural parts of each State over the time period to be used in the time series modelling, with higher correlations in NSW areas (Appendix B).

Unemployment rate provided the higher correlations than number of unemployed persons. It was decided that unemployment rate was the more appropriate measure, controlling for fluctuations in the numbers in the labour force at any one time. Unemployment numbers were highly correlated with rates which means that either measure could be used without changing analysis outcomes. A study of correlations between total estimated travel and unemployment rate with various lags incorporated, showed that zero lag provided the best correlation between the two measures.

The assumption made in introducing an exposure measure (such as unemployment rate) as a covariate is that the differences in patterns and levels of total vehicle travel and unemployment rate in the two States are large and important enough to have a different degree of impact on crash risk over the intervention period.

The second assumption made in using a global measure of exposure (unemployment rate in this study) is that this reflects the changes in exposure over time during the low alcohol hours of the week. The relationship between unemployment and travel patterns for different times of day and days of the week is uncertain. Previous research linking unemployment and fatalities does so globally, that is for all times and not for a subset of times.

6.6 Time series methodology for modelling crashes

All models were undertaken using a monthly time series because monthly data only were available for unemployment to be included in the modelling.

6.6.1 Casualty crash frequency

The time series data were modelled using a form of ARIMA (Auto Regressive Integrated Moving Average) modelling known as *Intervention Analysis* (Box and Tiao, 1975) and multiple regression models.

ARIMA models

This method allows the use of covariates in models that make use of ARIMA techniques to represent trend and seasonality. The ARIMA methodology provides a general method for estimating a wide range of seasonal effects. A variety of trend and other recurring patterns can also be modelled by ARIMA. A sufficient number of data points were available for this type of modelling (a minimum of 5-6 seasons, or 60-72 observations, are required for multivariate ARIMA and multiple regression models [Makridakis, Wheelwright & McGee, 1983] and in the current study 108 observations are available for the years 1983-1991 inclusive).

The approach used was similar to that used by Wagenaar, Streff & Schultz (1990) in estimating the effects of raising the speed limit, from 55 to 65 mph, on Michigan's

rural interstate highways. In order to estimate the effect of the intervention, a series of step functions, represented by dummy variables with values 0 (no intervention) and 1 (presence of the intervention) for appropriate months, were used in the model.

-Treatment of covariates

The data series to be modelled covered the period from January 1983 to December 1991 with the intervention beginning in December 1989. Since the intervention took different forms over the two year period (see section 6.1), it was decided that four dummy variables would be used to cover different parts of that period, so that the effects in each period could be estimated. The four dummy variables used for this purpose were for the periods:

- December 1989 to March 1990 (T1a)
- April to June 1990 (T1b)
- July 1990 to February 1991 (T2a)
- March to December 1991 (T2b)

Before developing the ARIMA models, the correlations between independent variables and between these and the dependent variable were examined (Appendix B). It was found that low alcohol hour casualty crashes correlated negatively with unemployment rates in Victoria (-0.73), Melbourne (-0.73), and rural Victoria (-0.44) but no significant correlation in the NSW areas.

Low to moderate significant correlations were found between unemployment rate and the four intervention variables, particularly for the T2b variable and unemployment rate in the Victorian areas (correlation 0.5). The reliability of the modelling procedure to provide valid estimates of effects of each variable (parameter estimates) relies on the condition that the variables are essentially uncorrelated; there is some indication that they are not. The modelling procedure also provides correlations between parameter estimates; these will be reviewed with the results in order to establish any intercorrelation problems.

-Model structure

It has been found in the past that, in part because of the nature of crash numbers being counts of events, that multiplicative models best model crash data (Hakim, Shefer, Hakkert & Hocherman, 1991; Thoresen *et al.*, 1992). In order to fit multiplicative models, natural logarithms of the dependent variable, covariates and independent variables were taken.

The dummy variables were defined as "1" ($\log_e 1 = 0$) in the non-intervention period and the exponential constant "e" ($\log_e e = 1$) in the intervention period.

The modelling process began with the identification of univariate ARIMA models of the casualty crash data, for the **pre-intervention period** (January 1983 to November 1989), in each of the treatment and comparison areas. This was accomplished in two stages. The first stage involved the analysis of autocorrelation and partial autocorrelation functions to estimate possible models. The next stage required the

over fitting of those models followed by model reduction to arrive at the best model for each set of crash data.

The models identified for each of the six areas all required seasonal components. Models with non-seasonal components were not selected. This is because a model structure with a non-seasonal component might be best for making predictions, but it was considered inappropriate for explanatory models covering the post-intervention period. The purpose of the covariates in the models was for them to describe the post-intervention changes.

It was also considered necessary for all models to be identical in structure so that the results would be consistent and comparable. The resultant model structure decided upon was the seasonal ARIMA (0,0,1) (0,1,1)¹² model. In all cases, this ARIMA structure was found to be the closest or the next closest fit.

The final models were ARIMA (0,0,1) (0,1,1)¹² with unemployment rate and intervention variables for T1a, T1b, T2a, and T2b included as covariates.

Multiple regression models

ARIMA models with Intervention Analysis are appropriate for estimating changes up to 12 months after the beginning of the intervention. Beyond this the model takes account of 1990 changes in the number of crashes. Thus the estimated impact of the program during T2b (and to some extent T2a), using this approach, could be underestimated if the initiative was effective during 1990. Multiple regression provided an alternative technique to model the data in order to estimate the T2b change, and also to validate the estimates for the earlier intervention periods.

In keeping with the previous ARIMA modelling, a multiple regression model with a multiplicative structure was developed for each crash series. This was achieved, again, by taking natural logarithms of each variable in the model. The dependent variable in each model was again the number of casualty crashes in low alcohol hours while the independent variables in the regression equations were made up of:

- a linear trend component
- monthly dummy variables (to represent seasonality)
- the unemployment rate
- a dummy variable for December 1989 to March 1990 (T1a)
- a dummy variable for April to June 1990 (T1b)
- a dummy variable for July 1990 to February 1991 (T2a)
- a dummy variable for March to December 1991 (T2b)

6.6.2 Casualty crash injury severity

Logistic (multiple) regression models of casualty crash severity (in low alcohol times) were developed for each treatment and comparison area from January 1983 to December 1991.

Casualty crash severity was represented as the ratio:

$$\frac{\text{fatal \& serious injury crashes}}{\text{minor injury crashes}}$$

in order to determine whether crashes have become less severe, corresponding to the intervention periods. The same covariates were initially used in these analyses as those for casualty crash frequency. Intercorrelations between parameter estimates were examined and new models without some covariates developed. Some of the final models exclude the trend covariate.

6.6.3 Net effects in treated areas relative to respective comparison areas in NSW

Estimated percentage changes in the crash criteria associated with the periods of the initiative were calculated for each of the treated and comparison areas. The final step was to estimate the net effect for treated areas by calculating the differences between the respective treated and comparison areas. To do this the Victorian percentage changes in each intervention period were discounted by the corresponding NSW percentage changes for the same periods. (The NSW percentage change represents the effect of other factors not included in the models, and assumed to operate equally in Victoria). Confidence intervals for the net percentage change were also determined. The method for these calculations is illustrated in Appendix C.

6.6.4 State-wide analysis: unemployment rate vs. vehicle travel covariate

In both the ARIMA and regression modelling for casualty crash frequency (in low alcohol times), the State-wide Victorian and NSW models were used to validate the use of the unemployment rate as an indicator of vehicle travel. Two models were fitted for each State with one using estimated travel (based on fuel sales) and the other using the unemployment rate. The results obtained were virtually identical for models of casualty crash frequency regardless of whether estimated travel or unemployment rate was used. There were some differences for models of casualty crash injury severity depending on whether unemployment rate or travel was used. These are presented in Appendix D.

6.7 Results

The results of each model and the net effects for each comparison are shown in Appendix E. The output from each model and corresponding diagnostic information is presented in Appendix F.

The following sections present the estimates of the effects of the speed camera program for the intervention periods on the frequency and injury severity casualty crashes (in low alcohol times) in the various treated areas.

Figures 11 and 12 below show the actual and fitted values of the two crash criteria in each month of 1983-91, for Victoria as a whole.

Figure 11
Number of Casualty Crashes in low alcohol times
Victoria

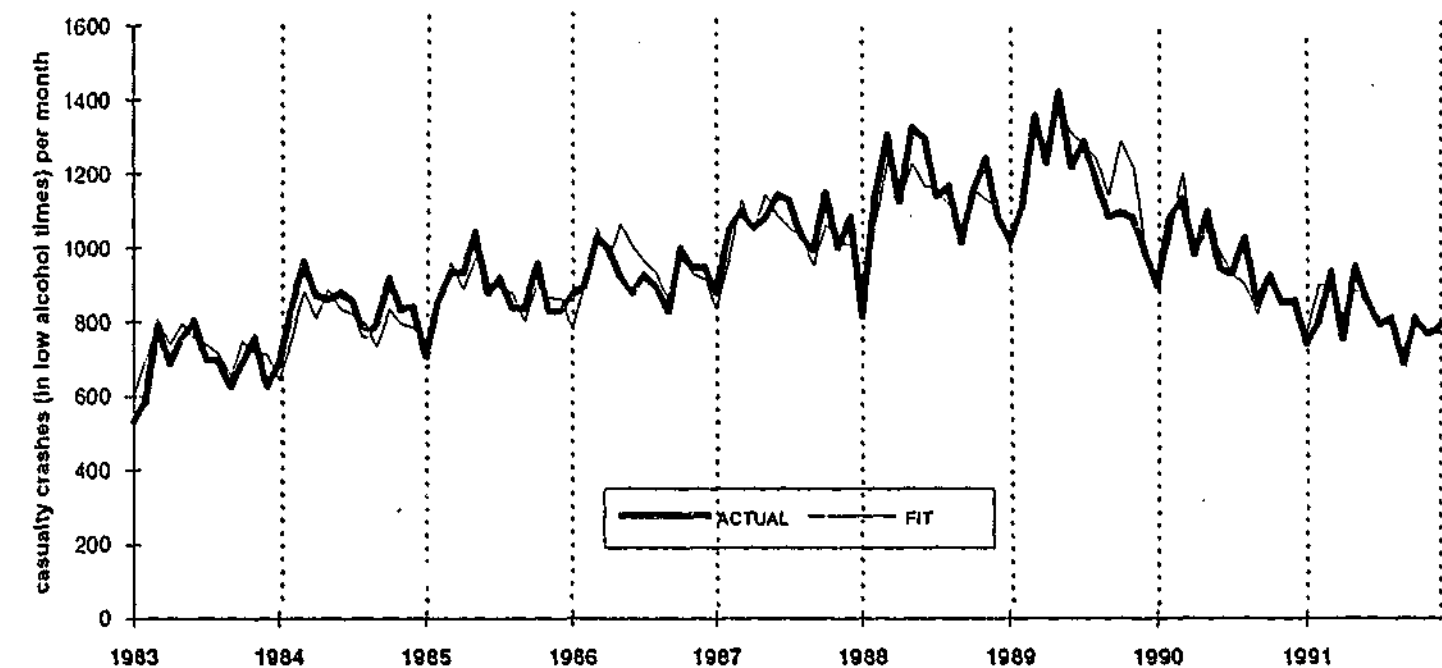
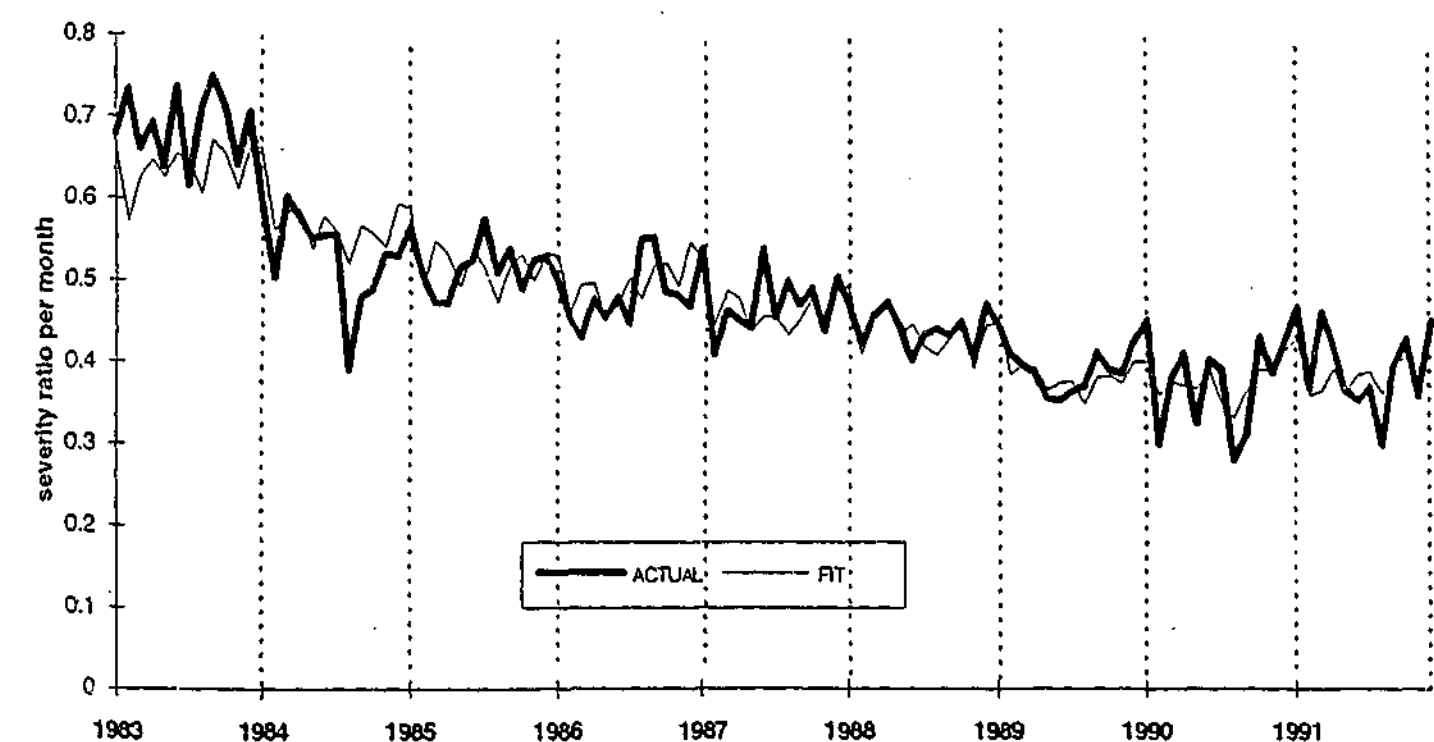


Figure 12
Casualty Crash Severity in low alcohol times, Victoria
(ratio of fatal & serious injury crashes to crashes involving minor injury only)



6.7.1 Effects on casualty crash frequency

Figure 13 shows the actual values of the frequency of casualty crashes (in low alcohol hours) in each month of 1983-1991, for Victoria as a whole. This Figure also shows the expected post-intervention trends (from the models) if the speed camera program had not been introduced. Two expected trends are displayed; one based on the model for Victoria, excluding the effect of other factors not implicit in the model, and the second trend including the effect of other factors, as measured by the experience in NSW. The difference between the expected trend (including other factors) and the actual represents the apparent effect of the program.

Figure 13
Number of Casualty Crashes per month in low alcohol times 1983-1991
Victoria

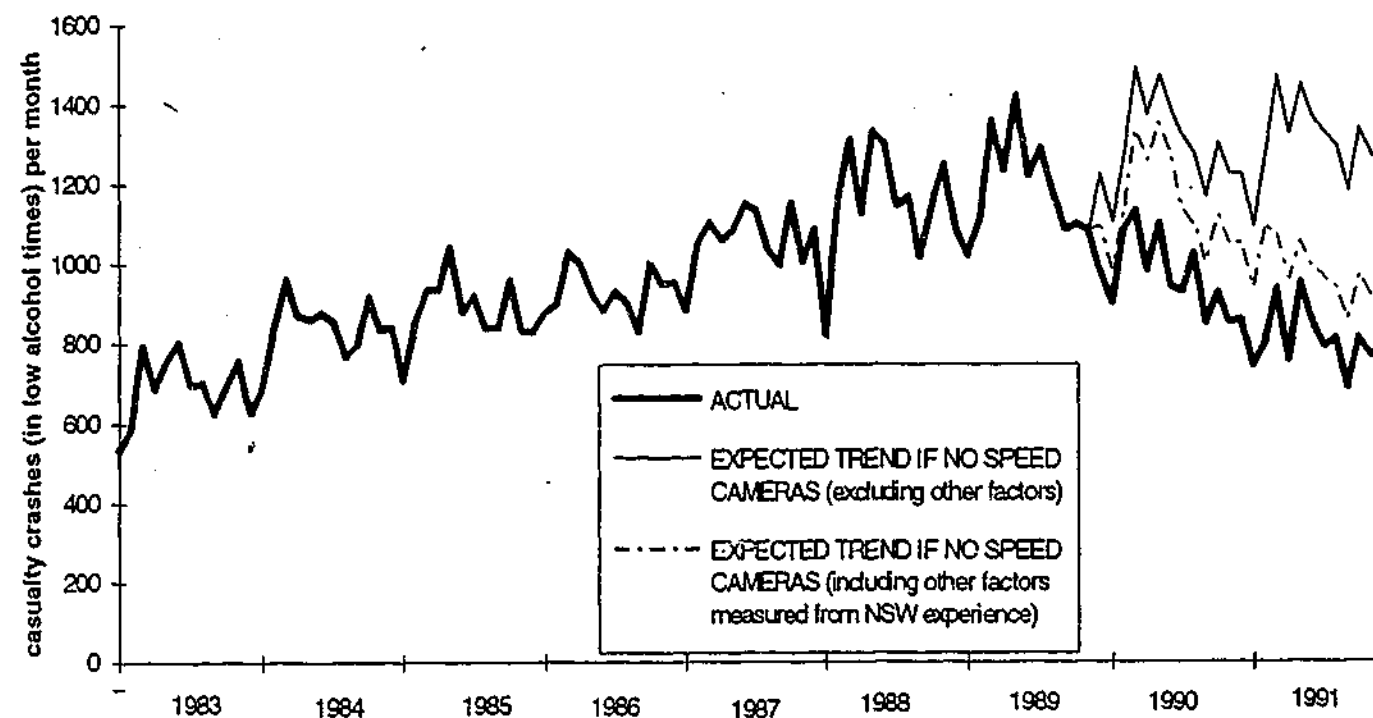


Table 4 shows the estimates of the net effects for each intervention period, after discounting changes in the Victorian areas by changes in the respective NSW areas, and after controlling for differential changes in unemployment rate. The changes in NSW were used to estimate the changes that would have occurred in Victoria had the intervention not taken place.

Area-wide effects

The pattern of results for the ARIMA and regression models are similar, particularly for estimates of T1a and T1b in Victoria and Melbourne. Confidence limits are also of similar magnitude for all estimates, except for T2b in Victoria and Melbourne which are larger (expectedly) for the regression models. Confidence intervals are of similar magnitude for all intervention estimates for ARIMA and regression models for rural Victoria. As noted in section 6.6.1, estimates for T2b would be larger for

regression models than ARIMA models providing estimates which are not influenced by any earlier changes. For this reason, results for T2b are taken from the regression models. Results for T1a, T1b and T2a are taken from the ARIMA models, given the assessment of the efficacy of the models outlined below. Bolded figures in Table 4 refer to the results from the preferred models.

The netted percentage changes show that there were statistically significant reductions in low alcohol hour casualty crashes in T1b (April 1990) and T2a (July 1990) in Melbourne, rural Victoria and the whole of the Victoria coinciding with the major launch of the program and the increase in speed camera enforcement. The reductions were of the order of 20%. There was also a statistically significant reduction in T2b in rural Victoria only, although the wider confidence limits for T2b estimates in Melbourne and Victoria indicated that it was not possible to provide precise estimates in these areas. T2b represents the effects of the Victorian speed camera program over and above any effects of the program operating in NSW, and does not provide an estimate for the net effect of the Victorian program *per se*.

Effects on arterial roads and by speed zone

The pattern of results for the ARIMA and regression models were again similar. Confidence limits for all intervention estimates were generally larger for the regression models, particularly for T2b estimates, in spite of the fact that the regression models tended to fit the data series better.

The netted percentage changes show that there were statistically significant reductions in low alcohol hour casualty crashes in T1a, T1b, and T2a, but not in T2b, on all arterial roads and specifically on 60 km/h arterial roads in Melbourne compared with those in Sydney. In T1a the reduction was around 13%, whilst much higher reductions of around 30% were observed in T1b and T2a.

No statistically significant effect was observed on arterial roads compared to residential streets in Melbourne (although the estimate of a 9% reduction in T2a approached statistical significance). The individual models for the two road types indicate that there reductions on both arterials and residential streets. This relative comparison can be interpreted in two ways; either the program's effects extended to residential streets (although speed cameras did not operate in these streets) or the change in residential streets do not reflect the effects of the program but the changes in casualty crash risk in this period due to other influences. Without supporting evidence of speed behaviour monitored over time in residential streets, it is not possible to determine which interpretation is correct.

Open rural 100km/h roads in country Victoria experienced a 14% drop in low alcohol hour casualty crashes in the T2a period only, whilst in 60 km/h country zones (country towns) there were statistically significant reductions in the T1b (21%), T2a (23%) and T2b (37%) periods. This pattern of results appears to follow the amount of speed camera use in country areas where the vast majority (around 80-90%) of sessions were conducted in 60 km/h zones.

Table 4

Estimated net effects on the frequency of casualty crashes in treatment areas

TREATMENT AREA	ARIMA MODEL (with unemployment rate, no constant)		REGRESSION MODEL (with unemployment rate)	
	Net effects	95% C.I. *	Net effects	95% C.I. *
Victoria				
T1a	-8.0% ns	-16.9% ~ +2.0%	-9.7% ns	-19.0% ~ +0.8%
T1b	-20.1% *	-29.2% ~ -9.8%	-22.2% *	-31.3% ~ -11.8%
T2a	-20.9% *	-27.9% ~ -13.3%	-17.9% *	-27.9% ~ -6.6%
T2b	-7.4% ns	-15.9% ~ +2.0%	-15.6% ns	-31.5% ~ +4.0%
Melbourne				
T1a	-7.0% ns	-16.8% ~ +4.3%	-6.7% ns	-17.7% ~ +5.9%
T1b	-23.5% *	-33.1% ~ -12.5%	-20.2% *	-31.1% ~ -7.6%
T2a	-21.1% *	-28.9% ~ -12.4%	-12.7% ns	-25.4% ~ +2.1%
T2b	-4.5% ns	-14.3% ~ +6.6%	-3.3% ns	-24.0% ~ +23.1%
Rural Victoria				
T1a	-3.7% ns	-14.0% ~ +7.8%	-12.8% ns	-24.3% ~ +0.4%
T1b	-16.4% *	-27.4% ~ -3.8%	-20.2% *	-31.6% ~ -7.0%
T2a	-19.5% *	-27.5% ~ -10.7%	-20.4% *	-28.4% ~ -11.6%
T2b	-11.9% *	-21.1% ~ -1.7%	-27.7% *	-37.3% ~ -16.6%
All arterials Melbourne vs all arterials Sydney				
T1a	-14.6% *	-23.6% ~ -4.5%	-13.4% *	-23.8% ~ -1.6%
T1b	-33.8% *	-42.2% ~ -24.1%	-27.4% *	-37.5% ~ -15.7%
T2a	-32.4% *	-38.8% ~ -25.3%	-23.2% *	-35.2% ~ -9.1%
T2b	-10.6% *	-19.0% ~ -1.3%	-13.8% ns	-33.8% ~ +12.3%
60km/h arterials Melbourne vs 60km/h arterials Sydney				
T1a	-13.8% *	-23.0% ~ -3.4%	-12.7% *	-23.3% ~ -0.6%
T1b	-31.5% *	-40.4% ~ -21.3%	-27.4% *	-37.7% ~ -15.4%
T2a	-30.6% *	-37.4% ~ -23.0%	-25.1% *	-37.0% ~ -11.0%
T2b	-5.6% ns	-15.0% ~ +4.7%	-13.2% ns	-33.8% ~ +13.8%
All arterials Melbourne vs residential streets Melb.				
T1a	-1.6% ns	-14.6% ~ +13.3%	+13.4% ns	-2.5% ~ +32.0%
T1b	-3.2% ns	-18.5% ~ +14.9%	+2.1% ns	-14.9% ~ +22.5%
T2a	-9.1% ns	-19.8% ~ +2.9%	+1.7% ns	-19.6% ~ +28.8%
T2b	-0.1% ns	-11.8% ~ +13.7%	+16.2% ns	-18.8% ~ +66.2%
100 km/h roads Rural Victoria				
T1a	+0.2% ns	-14.4% ~ +17.3%	-14.9% ns	-30.3% ~ +3.0%
T1b	-8.1% ns	-24.3% ~ +11.6%	-11.5% ns	-28.8% ~ +9.9%
T2a	-14.1% *	-25.4% ~ -1.1%	-9.6% ns	-22.1% ~ +4.9%
T2b	-9.1% ns	-21.5% ~ +5.2%	-11.9% ns	-28.0% ~ +7.6%
60 km/h roads Rural Victoria				
T1a	-3.3% ns	-15.0% ~ +9.9%	-2.6% ns	-17.9% ~ +15.5%
T1b	-21.0% *	-32.7% ~ -7.3%	-23.0% *	-35.9% ~ -7.4%
T2a	-23.3% *	-31.8% ~ -13.7%	-25.7% *	-34.6% ~ -15.7%
T2b	-17.8% *	-27.3% ~ -7.0%	-37.4% *	-47.3% ~ -25.7%

* 95% C.I.- true reduction in casualty crash risk lies within the specified range with 95% confidence
(ie. there is a 5% chance of the true reduction in casualty crash risk being outside this range)

* Statistically significant at $p < 0.05$ level ns not significant at the 0.05 level

Bolded figures refer to the results derived from the preferred models for each intervention period

Efficacy of the models

An examination of the adjusted R^2 statistic for each of the final models (Appendix E) showed that in each case the regression models fitted better, as did all models for Victorian areas compared with the NSW areas for both ARIMA and regression models (higher adjusted R^2 values). Almost all models however, were of acceptable fit (adjusted R^2 values generally varied between 0.5 - 0.8). Examination of figure 11 shows a brief divergence between the model and the actual values in the third quarter of 1989. It has been suggested that 'Operation 100' Police activity and the high profile fatal bus crashes during that period could have been responsible for the lower number of crashes during this period than was expected by the model. These "one-off" events were not explicitly taken into account in the model because it is not possible to take into consideration every possible variable of interest, and their effect appear to be relatively brief as reflected by the convergence between the model and the actual data from late 1989.

The constant terms were removed for the final ARIMA models. This was because the correlations between the parameter estimates (see Appendix F) for the constant and unemployment rate was found to be extremely high (0.99) for metropolitan and rural models in both Victoria and NSW, and also for the Statewide models, which indicated that the two estimates were describing the same thing and that therefore the model had been overfitted.

An examination of the correlations between parameter estimates in the final ARIMA models (Appendix F) indicated that there were moderate but statistically significant correlations (between 0.35 and 0.5) between estimates for unemployment rate and intervention variables T1a, T1b, and T2a for Victorian and NSW models both Statewide and metropolitan and rural. It is assumed that the statistical technique was robust enough to cope with these intercorrelations and that therefore the estimates for the intervention variables are reliable.

A similar examination for the final regression models (Appendix F) revealed extremely high intercorrelations in all areas (Victoria, NSW, Melbourne, Sydney, rural Victoria and rural NSW) and between parameter estimates for unemployment rate, trend and the intercept, (generally 0.9). The estimates for unemployment rate, trend and the intercept were almost always highly correlated with estimates for intervention variables T2a and T2b, particularly T2b (generally around 0.8). These high intercorrelations suggest that the estimates given by these models for the intervention may be unreliable. Thus, although the regression models tended to fit the data series better, the higher intercorrelations between the estimates means that those provided by the regression models were more likely to be suspect compared with the ARIMA estimates.

It is interesting to note that, as reflected in the earlier correlations between variables, unemployment rate was not a statistically significant variable in models of NSW low alcohol hour casualty crashes, but was statistically significant in Victorian models of low alcohol hour casualty crashes. Statewide, total estimated vehicle travel in NSW appeared to be uncorrelated with low alcohol casualty crashes whilst in Victoria total vehicle travel was statistically related. This suggests that the relationship between exposure to risk and crash involvement is not always manifested, or that other factors

are more important than this factor, or mediate its effect, in some places, or alternatively that the available measures are not adequate.

The primary purpose for including the unemployment rate covariate in the models was to improve the interstate comparisons to be made, by directly taking into account differences in changes in this measure in the two States. For the intra-Melbourne comparison between arterial roads and residential streets, the role of the unemployment covariate in this sense is redundant. However, given that unemployment rate was consistently found to be statistically significant in models of low alcohol hour casualty crashes in Victoria and its parts, it was included in the models for the intra-Melbourne comparison.

6.7.2 Effects on casualty crash injury severity

Figure 14 again illustrates the methodology for estimating the effects of the speed camera program. Shown are the actual values of the injury severity of casualty crashes (in low alcohol hours) in each month of 1983-1991, for Victoria as a whole. Two expected post-intervention trends (from the models) if the speed camera program had not been introduced are also shown; one based on the model for Victoria, excluding the effect of other factors not implicit in the model, and the second trend including the effect of other factors, as measured by the experience in NSW. The difference between the expected trend (including other factors) and the actual represents the apparent effect of the program on casualty crash injury severity (in low alcohol times).

Figure 14
Casualty Crash Severity per month in low alcohol times 1983-1991, Victoria
(ratio of fatal & serious injury crashes to crashes involving minor injury only)

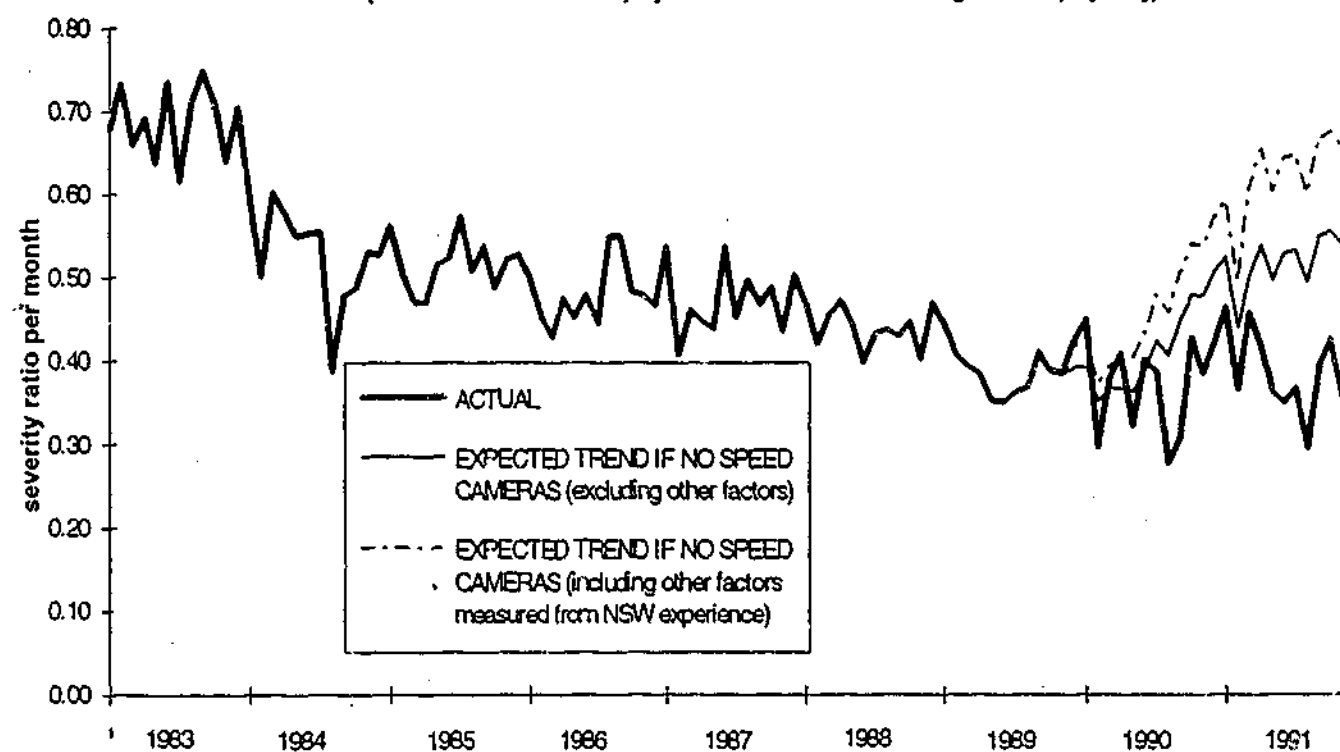


Table 5 shows the estimates of the net effects for each intervention period, after discounting changes in the Victorian areas by changes in the respective NSW areas, after controlling for differential changes in unemployment rate. The changes in NSW were used to estimate the changes that would have occurred in Victoria had the intervention not taken place.

Area-wide effects

There were statistically significant reductions in the severity of low alcohol hour casualty crashes in T2a (28%) and T2b (40%) in Victoria as a whole. Similarly, there were significant reductions in severity in Melbourne over each of the intervention periods, increasing in size over time (18% - 45%). In contrast no such reductions were observed in country Victoria, with a statistically significant increase in severity estimated in T1b (31%). The validity of these results are in question, however, given that the models fitted may be inadequate from a statistical perspective as outlined in the section below regarding the efficacy of the models.

Effects on arterial roads and by speed zone

Statistically significant reductions in the severity of low alcohol hour casualty crashes in all intervention periods (increasing from 22% to 48%) was observed for all Melbourne arterials, whilst reductions in T1b, T2a and T2b (increasing from 33% to 49%), but not in T1a, were observed on 60 km/h arterial roads in Melbourne.

A statistically significant effect was observed on arterial roads compared to residential streets in Melbourne in T2b only (19%), although the estimates for T1b and T2a appeared to approach statistical significance. The individual models for the two road types indicate that there were statistically significant reductions on both arterials and residential streets in T2a and T2b. The statistically significant reductions in residential streets in T2a and T2b can be interpreted in two ways; either the program's effects extended to residential streets (although speed cameras did not operate in these streets) or the change in residential streets do not reflect the effects of the program but the changes in casualty crash severity in this period due to other influences. Without supporting evidence of speed behaviour monitored over time in residential streets, it is not possible to determine which interpretation is correct.

Open rural 100km/h roads in country Victoria experienced no changes in the intervention periods, except for a significant 66% increase in low alcohol hour casualty crashes in T1b. In 60 km/h country zones (country towns) there were no statistically significant changes during the intervention periods.

Again, the validity of these results are in question given that the models fitted may be inadequate from a statistical perspective as outlined in the section below regarding the efficacy of the models.

Table 5
Estimated net effects on the injury severity of casualty crashes in treatment areas

TREATMENT AREA	LOGISTIC REGRESSION MODELS	
	Net effects	95% C.I.♦
Victoria (including unemployment rate and trend)		
T1a	-4.4% ns	-17.7% ~ +11.1%
T1b	-9.5% ns	-23.7% ~ +7.5%
T2a	-27.9% *	-39.9% ~ -13.6%
T2b	-40.3% *	-55.4% ~ -20.1%
Melbourne (including unemployment rate, no trend)		
T1a	-18.3% *	-31.5% ~ -2.6%
T1b	-29.0% *	-41.7% ~ -13.5%
T2a	-35.2% *	-42.6% ~ -26.9%
T2b	-44.6% *	-50.8% ~ -37.6%
Rural Victoria (including unemployment rate and trend)		
T1a	+9.2% ns	-13.2% ~ +37.4%
T1b	+30.7% *	+2.0% ~ +67.6%
T2a	-0.8% ns	-16.3% ~ +17.6%
T2b	+3.2% ns	-18.1% ~ +30.1%
All arterials Melbourne vs all arterials Sydney (including unemployment rate, no trend)		
T1a	-22.1% *	-36.3% ~ -4.7%
T1b	-36.9% *	-49.5% ~ -21.3%
T2a	-37.1% *	-45.0% ~ -28.0%
T2b	-47.9% *	-54.9% ~ -39.9%
60km/h arterials Melbourne vs 60km/h arterials Sydney (including unemployment rate, no trend)		
T1a	-15.7% ns	-32.5% ~ +5.3%
T1b	-33.1% *	-47.7% ~ -14.5%
T2a	-38.4% *	-46.9% ~ -28.5%
T2b	-49.2% *	-56.6% ~ -40.4%
All arterials vs residential streets in Melbourne (including unemployment rate, no trend)		
T1a	-10.3% ns	-31.5% ~ +17.4%
T1b	-24.9% ns	-44.5% ~ +1.7%
T2a	-16.0% ns	-30.3% ~ +1.3%
T2b	-19.2% *	-33.8% ~ -1.5%
100 km/h roads Rural Victoria (including unemployment rate and trend)		
T1a	+1.7% ns	-27.7% ~ +43.0%
T1b	+65.6% *	+14.7% ~ +139.3%
T2a	+8.4% ns	-15.8% ~ +39.6%
T2b	+15.3% ns	-18.2% ~ +62.6%
60 km/h roads Rural Victoria (including unemployment rate and trend)		
T1a	+14.4% ns	-18.1% ~ +59.8%
T1b	+15.4% ns	-19.5% ~ +65.2%
T2a	-9.3% ns	-29.1% ~ +16.1%
T2b	-7.3% ns	-33.8% ~ +29.8%

♦ 95% C.I.- true reduction in casualty crash severity lies within the specified range with 95% confidence (ie. there is a 5% chance of the true reduction in casualty crash severity being outside this range)

* Statistically significant at $p < 0.05$ level ns not significant at the 0.05 level

Bolded figures refer to the results derived from the preferred models for each intervention period

Efficacy of the models

The confidence limits for net estimates in these models were wider than for those for casualty crash frequencies.

An examination of the adjusted R^2 statistic for each of the final models (Appendix E) showed that whilst the models developed for Victoria fitted well (R^2 values ranged between 0.45 - 0.78), the models for NSW areas were poor (adjusted R^2 values ranged between 0.01 - 0.48), particularly for Sydney models. The residential street model for Melbourne also fitted quite low (0.24).

An examination of the correlations between parameter estimates in all of the initial models fitted indicated that there were extremely high correlations (consistently around 0.9) between estimates for the unemployment rate, trend and intercept parameters for all the models. These 3 parameters were each also highly correlated with intervention variables T2a and T2b (0.8 - 0.9) for Victorian and Melbourne (area-wide, arterial and residential road) models and with T2b for all rural Victoria and NSW, Sydney and rural NSW models (area-wide and arterials) with correlations always between 0.7-0.9. These high intercorrelations suggest that the estimates given by such models for the intervention may be unreliable.

In order to improve these models, new models were fitted without the trend parameter. For the Melbourne and Sydney models the new models appeared to improve while State-wide and rural models did not. Therefore the final models adopted for Melbourne and Sydney excluded the trend parameter. The high intercorrelations between the intercept and unemployment parameters remained but these were no longer correlated with T2a and only moderately with T2b (0.55) for all Melbourne and Sydney models. The remaining high intercorrelations for these models and those for the other models may still affect estimates for the intervention estimates, and therefore these results are tentative.

The primary purpose for including the unemployment rate covariate in the models of casualty crash frequency was to improve the interstate comparisons to be made, by directly taking into account differences in changes in this measure (as a proxy for vehicle exposure) in the two States. However, there was no prior evidence for its importance in crash severity. Given that unemployment rate was found to be statistically significant in models of low alcohol hour casualty crash frequency in Victoria and its parts, it was included in all the models of casualty crash severity. It was found to be a statistically significant variable in Melbourne, Sydney and Victoria-wide models but not in rural or NSW-wide models of casualty crash severity, suggesting that increases in unemployment rate is associated with increases in the injury severity of crashes.

Its inclusion in models of casualty crash severity means that the estimated net effects are based on the premise that casualty crash severity (in low alcohol hours) would have increased in Melbourne and Victoria, given the increase in unemployment rate that occurred during the intervention period, had the speed camera program not been introduced. The estimated net effects therefore are not simply reflected in the actual trend in the casualty crash severity ratio over time.

The graphs of each data series (Appendix F), show that the recent trend in casualty crash severity in NSW is different to that in Victoria, with the severity of low alcohol hour casualty crashes increasing in the latter part of the series in NSW. This is reflected in the estimates of changes in NSW which indicate statistically significant increases in severity of rural NSW crashes in T2a (+12%) and T2b (+25%) and Sydney metropolitan crashes in T1b (+14%). In using NSW areas as comparison areas, it needs to be assumed that this would have been the trend in Victorian areas if the program had not operated. This needs to be kept in mind when judging these results.

6.8 Discussion

The estimated effects for the various treated areas during the four intervention periods are summarised in Tables 6 and 7.

6.8.1 Frequency of casualty crashes (in low alcohol hours)

A consistent drop from what would have been expected in the number of (low alcohol hour) casualty crashes across all treated areas was observed for the periods corresponding to the publicity launch (T1b) and the increase in speed camera enforcement (T2a), except for 100 km/h rural open roads where there was a drop in T2a only. The largest of these reductions (around 30%) occurred on arterial roads in Melbourne. A smaller reduction (14%) during T1a was confined to Melbourne arterial roads only. Rural 60 km/h roads (and therefore rural Victoria as a whole) experienced a statistically significant drop relative to the respective rural NSW areas in T2b, when NSW also had a speed camera program operating.

6.8.2 Injury severity of casualty crashes (in low alcohol hours)

The Melbourne area (as a whole and on arterials roads specifically) experienced increasing reductions over time from what would have been expected in the severity of (low alcohol hour) casualty crashes. No changes were observed on 60 km/h rural roads, although 100 km/h rural roads and therefore the rural area as a whole experienced a substantial increase in casualty crash severity corresponding to the launch of the speed camera program (T1b). Given the different trends in the rural and metropolitan parts of the State, the averaged change for Victoria as a whole were significant reductions in the T2a (28%) and T2b (40%) periods.

It should be noted that the estimated percentage reductions in the injury severity of casualty crashes apply to the severity ratio defined earlier (page 19) and not to the more traditional measure of injury severity, namely the ratio of fatal plus hospitalisation crashes to all casualty crashes (ie. those involving death or any level of injury). The former ratio is likely to show larger changes than the latter; for example, the estimated 28% reduction in the Victoria-wide severity ratio during T2a is equivalent to a 21% reduction in the traditional measure.

These estimated net effects on casualty crash severity in Melbourne and Victoria-wide are influenced by the inclusion of unemployment rate in these models. Unemployment rate was included in the models of crash severity because a significant statistical relationship was found between unemployment rate and injury

severity of casualty crashes in urban areas (Melbourne, Sydney and Victoria-wide, but not in rural areas of either State nor in NSW overall) suggesting that increases in unemployment rate is associated with increases in the injury severity of crashes².

Thus, these estimated net effects on casualty crash severity are derived from models which take into account the effects of changes in unemployment rate. In practice this means that the estimated net effects are based on the expectation that casualty crash severity (in low alcohol hours) would have increased in Melbourne and Victoria, given the increase in unemployment rate that occurred during the intervention period, had the speed camera program not been introduced. Therefore estimated net percentage reductions are not simply reflected in the actual trend in the casualty crash severity ratio during the intervention period.

Table 6

Estimated net effects on the incidence and injury severity of casualty crashes (in low alcohol times) in treatment areas (relative to NSW comparison areas)

TREATMENT AREA	Low level enforcement & publicity Dec '89-Mar '90 (T1a)	Low level enforcement & intense publicity April-June 1990 (T1b)	High level enforcement & intense publicity July '90-Feb '91 (T2a)	High level enforcement & intense publicity Mar-Dec 1991 (T2b)▲
Melbourne 60km/h arterials -casualty crashes -severity	-13.8% *	-31.5% * -33.1% *	-30.6% * -38.4% *	-49.2% *
Melbourne all arterials -casualty crashes -severity	-14.6% * -22.1% *	-33.8% * -36.9% *	-32.4% * -37.1% *	-47.9% *
Melbourne -casualty crashes -severity	-18.3% *	-23.5% * -29.0% *	-21.1% * -35.2% *	-44.6% *
Rural Victoria 60 km/h roads -casualty crashes -severity		-21.0% *	-23.3% *	-37.4% *
Rural Victoria 100 km/h roads -casualty crashes -severity		+65.6% *	-14.1% *	
Rural Victoria -casualty crashes -severity		-16.4% * +30.7% *	-19.5% *	-27.7% *
Victoria -casualty crashes -severity		-20.1% *	-20.9% * -27.9% *	-40.3% *

* Statistically significant at $p < 0.05$ level ▲ effects relative to NSW speed camera program
(only statistically significant results are shown)

² A possible explanation for this correlation is that decreased traffic volumes (related to economic recession) leads to higher travel speeds and hence greater crash severity. However, there was no prior evidence to suggest that vehicle exposure is related to the injury severity outcome of crashes.

6.8.3 Pattern of effects across areas and corresponding levels of speed camera operations

In terms of **casualty crash frequency in low alcohol times**, the pattern of results suggested that there were stronger reductions on 60km/h arterial roads and all arterial roads in Melbourne (around 30%) as compared with Melbourne as a whole and in rural 60km/h zones (around 20%) and also as compared with the weakest effect observed in rural 100km/h zones (14% drop in one period only). This pattern of effects appeared to correspond with the level of speed camera enforcement in these different areas:

- speed camera operations were conducted almost exclusively on arterial roads,
- a greater number speed camera operations were conducted in Melbourne than in rural areas (70% of total sessions on average have been conducted in Melbourne),
- most (80-90%) camera sessions have been conducted in 60 km/h zones in both the respective metropolitan and rural parts of the State.

The pattern of results therefore provided greater confidence that the observed reductions in the frequency of casualty crashes (in low alcohol hours) were related to the speed camera program.

In terms of the **injury severity of casualty crashes**, reductions were observed in the Melbourne areas but not in any of the rural areas, again corresponding to the greater level of speed camera enforcement in Melbourne compared with country Victoria. There was no substantial difference between the effects on Melbourne arterial roads only and all Melbourne roads, except that they appeared somewhat greater on the arterials (as could be expected).

6.8.4 Relative comparison

Table 7 shows the relative comparison of effects on arterial compared to local roads in Melbourne. A 19% reduction in the **severity of casualty crashes** during low alcohol times was observed on arterial roads compared to residential streets in Melbourne in T2b only.

Table 7

Relative effects on the incidence and severity of casualty crashes (in low alcohol times) on arterial roads compared to residential streets in Melbourne

TREATMENT AREA	Low level enforcement & publicity Dec '89-Mar '90 (T1a)	Low level enforcement & intense publicity April-June 1990 (T1b)	High level enforcement & intense publicity July '90-Feb '91 (T2a)	High level enforcement & intense publicity Mar-Dec 1991 (T2b)▲
Melbourne all arterials vs residential streets -casualty crashes -severity				-19.2% *

* Statistically significant at $p < 0.05$ level ▲ effects relative to NSW speed camera program (only statistically significant results are shown)

6.8.5 Limitations of Phase 1

This research design involved a number of assumptions. These are considered in turn.

Interstate control

The conceptualisation of the effects of this program as being generalised across broad areas for this phase of the evaluation (partly because of the pervasiveness and intensity of the enforcement and publicity) led to the absence of an untreated area, time or other crash group in the respective Victorian metropolitan and rural areas to provide for a control group. This meant the use of an interstate comparison group was unavoidable. Although the most similar State to Victoria was chosen for this role, it is not known how well it provided control for other (exogenous) factors present over time in Victoria. The most obvious relevant differences between the two States, manifested in unemployment rates, were considered in the analysis. Changes in unemployment rates were explicitly taken into account in the analysis of each States' crash data.

However, even after unemployment rates were taken into account, the interstate comparison area experienced statistically significant reductions in low alcohol hour casualty crashes and in some instances increases in low alcohol hour casualty crash severity. It is not known the extent to which these reductions were related to safety measures particular to NSW or to "other" factors which were also applicable to Victoria.

The validity of the results therefore depends on whether the influences of extraneous factors (ie. factors not included in the models) on the evaluation criteria in NSW during the intervention were similar to those for Victoria.

It should also be noted that T2b represents the effects of the Victorian speed camera program over and above any effects of the program operating in NSW, and does not provide an estimate for the net effect of the Victorian program *per se*.

Relative control

The greater relative reduction in (low alcohol hour) casualty crash severity on Melbourne arterial roads compared with residential streets in T2b can be interpreted in one of two ways:

1. the program had a greater effect on arterial roads than residential streets, which were also affected by the program, or
2. that the changes in residential streets represents the base from which any additional effects can be ascribed to the program, ie. 19% drop in T2b

These interpretations are also subject to the assumption that the expected effect of the program and the effects of 'other factors' on the severity of casualty crashes on the two different types of roads would be expected to be the similar. That is, severity of crashes on both road types are equally susceptible to change.

Unemployment rate as a measure of low alcohol hour vehicle travel

Casualty crash frequency

The use of unemployment rate as a covariate to control for changes in low alcohol hour travel cannot be directly validated. There were substantial changes in the trends in total travel almost coinciding with the program and these changes differed between Victoria and NSW.

The assumption made in introducing an exposure measure (such as unemployment rate) as a covariate is that the differences in patterns and levels of total vehicle travel and unemployment rate in the two States are large and important enough to have a different degree of impact on crash risk over the intervention period.

The second assumption made in using a global measure of exposure (unemployment rate in this study) is that this reflects the changes in exposure over time during the low alcohol hours of the week. The relationship between unemployment and travel patterns for different times of day and days of the week is uncertain. Previous research linking unemployment and fatalities does so globally, that is for all times and not for a subset of times. Explicitly controlling for changes in exposure was considered to be important for estimating effects of the program on crash frequency.

Casualty crash injury severity

In the case of injury severity of crashes there was no theoretical reason or evidence to suggest that vehicle exposure is related to the injury severity outcome of crashes. Nevertheless, a significant statistical relationship was found between unemployment rate and injury severity of casualty crashes in Melbourne and in Sydney, Victoria-wide but not NSW-wide, and not in rural Victoria nor rural NSW. This suggested that increases in unemployment rate could be associated with increases in the injury severity of crashes, particularly in urban areas. One possible explanation for this is that when traffic congestion is reduced in times of economic recession, the opportunity to travel at higher speeds also increases (especially in urban areas) which leads to greater injury severity of those crashes which occur.

There is no evidence for this theory, but the strong negative correlation between unemployment rate and injury severity of low alcohol hour casualty crashes was considered to warrant the retention of unemployment rate in the models so it could be accounted for in estimating the effects of the program on crash severity. The estimates of the program in Melbourne and State-wide Victoria are therefore influenced by the presence of the unemployment variable in these models, because of its statistically significant role in these models of casualty crash severity.

Effects of the RBT Initiative

Although the majority of RBT sessions have been in high alcohol hours, there was a small proportion of RBT sessions occurring during low alcohol hours. It is not known if this overlap has resulted in some observed effects reflecting (in part) RBT effects.

Efficacy of the models

The validity of the results for casualty crash severity are tentative given that the models fitted may be inadequate from a statistical perspective as outlined in the results section regarding the efficacy of the models. These models were characterised by high intercorrelations between estimates for the predictors, in particular between those for unemployment rate, trend and T2b, which may have affected the estimates of the program variables. It was also assumed that the removal of the constant term and the correlations between some parameter estimates in the ARIMA modelling of casualty crash frequency did not affect the estimates for the intervention periods.

The approach used in the modelling was such that a specific test was made for a step change or reduction with the introduction of the program. This assumes that the nature of the effects of the program are immediate and maintained. If, for example, effects were initially small and increased incrementally over time, the current estimates will only be an average of these varying effects. Although this is a shortcoming, it is because of the unclear nature of the effects of the program over time that alternative frameworks were not used.

Low alcohol hour crashes

It is not known to what extent driver responses to speed cameras (if any) are limited to their times of operation or whether speed behaviour has also been modified at night (which is encompassed by high alcohol times). Although there is yet no evidence for generalisation of behaviour beyond specific times and locations of speed enforcement, the current program is very different to previous enforcement programs. Whilst it would have been desirable to test for night time effects of the program, this was not possible due to the concurrent operation of an intensive RBT initiative. The current results therefore only apply to low alcohol times, almost all of which are in daylight hours.

7.0 PHASE 2: EFFECTS OF PROGRAM MECHANISMS

Phase 2 of this evaluation attempts to further understand and estimate the impact of the program by modelling the various deterrence mechanisms of the program against the evaluation criteria (casualty crash incidence and severity during low alcohol times of the week). The measured mechanisms are represented quantitatively and are assessed for their relative impact on the evaluation criteria. The purpose of this is two-fold:

1. to attempt to link more directly the program mechanisms with observed reductions in crash criteria, and hence better establish the "cause-and-effect" of the program,
2. to attempt to indicate which of the program mechanisms appear to be most associated with changes in the speed-related crash criteria.

7.1 Rationale for estimating the effects of program mechanisms on crash criteria

Models developed in Phase 2 test the changes and variations in intensity of the respective elements of the program against the changes and variations in the evaluation criteria. This provides a more rigorous test of whether the speed camera program has caused observed crash effects than the Phase 1 evaluation.

Phase 1 tested for simultaneous reductions in the evaluation criteria with the introduction of the various phases of the program (represented by intervention variables), after taking into account the effects of other influences; namely long-term trends in casualty crash risk and severity, seasonal trends and unemployment, and also changes in the evaluation criteria in NSW, which served as a comparison area. The effects observed in Phase 1 are strongly suggestive of a coincidental effect of the program on the evaluation criteria. However, it does not directly link the program to the observed effects, and hence it is possible that other influences, which are not accounted for, have caused these observed effects.

Phase 2 is more rigorous in testing the cause-and-effect link. Direct measures of the speed camera program operations are tested against changes in the evaluation criteria on a monthly time series basis, across the two year post-intervention period. Hence, a more specific and direct relationship is required in Phase 2 if it is to be concluded that the effects are attributable to the program, rather than on the basis of the presence or absence of the treatment in different time blocks, as in Phase 1.

An additional reason for modelling the various mechanisms of the program is to allow an understanding of how the program effects changes in the evaluation criteria, that is, which are the critical elements and ways in which the program could be improved to maximise road safety benefits.

One of the major practical difficulties with this approach, however, is validly representing the form of the program, and the reliance on available quantified measures of the program in a monthly time series.

7.2 Data available relating to program mechanisms

Time series data relating to the program (see section 4.0) is available relating to:

1. number of speed camera TINs issued (monthly; Statewide only)
2. proportion of drivers issued with one or multiple speed camera TINs (two measurements from TCO in 1992 only)
3. proportion of drivers with a varying number of demerit points (quarterly only; Statewide only; demerit points from all types of traffic offences)
4. proportion of drivers who are aware of others who have been caught by a speed camera (two measurements in 1991 only)
5. amount of paid television publicity (in terms of TARPs³) (monthly; metropolitan television only; both speed related TAC publicity and all TAC road safety publicity)
6. hours of speed camera operation (monthly; Statewide, metropolitan and rural areas).

7.3 Selection of program variables and crash criteria to be modelled

Program variables

Measures of TINs issued (1), publicity (5), and camera operations (6) are available on a monthly basis, demerit points (3) is available on a quarterly basis, whilst multiple TINs (2) and awareness of others caught (4) are available for two dates only (shown in section 4.0). Thus, measures 2 and 4 cannot be used in the statistical analyses.

Whilst it would be possible to interpolate the demerit points measure (3) to provide a monthly series from the quarterly data points available, the appropriate representation of this measure is not immediately obvious. For instance, the average number of demerit points over time may not be a sufficiently sensitive measure of change, given that the distribution of demerit points is highly skewed with relatively low proportions in the higher ranges. Selecting the proportion or number of drivers with demerit points above a particular level is also not satisfactory; that is, it raises the question which is the level of accumulated demerit points which most deters drivers from speeding and committing further offences, and hence reduces speed-related crash risk? For these reasons this measure was not included in the analysis for Phase 2.

Publicity data (5) is available monthly but for the metropolitan area only. Publicity, however, represents a major component of the program and was used at unprecedented levels, particularly in relation to speeding.

³ Target Audience Rating Points is a measure of the proportion of people in a nominated target group who have been exposed to the advertising and thus provides a measure of its amount and intensity

Therefore an attempt was made to determine the relationship (if any) between the evaluation criteria and deterrence mechanisms relating to TINs issued (1), publicity (both speed related and total road safety publicity [5]) and camera operations (6).

Evaluation criteria

Crash criteria chosen as evaluation outcomes were those selected in Phase 1, that is, casualty crash frequency and casualty crash injury severity (see section 6.4). These are police reported casualty crashes which include fatal crashes (those in which one or more persons is killed), serious injury crashes (those in which one or more persons is seriously injured and probably admitted to hospital) and minor injury crashes (in which one or more persons requires medical treatment).

Only casualty crashes in low alcohol hours of the week (mainly daytime) were examined given the parallel operation of a major Random Breath Testing (RBT) initiative primarily in high alcohol times. Thus the results pertain to the effects of the program mechanisms on crashes during low alcohol hours only (ie. Monday to Thursday 6am to 6pm, Friday 6am to 4pm, Saturday 8am to 2pm, Sunday 10am to 4pm).

Because of the availability of only Melbourne television publicity data, it was decided to undertake the analysis for crashes in the Melbourne area only. Data for speed camera operations in the Melbourne area only were used for comparability. Whilst it would have been desirable to use TINs issued data which related specifically to the Melbourne area only as well, this data was not available in disaggregated form for this Phase of the evaluation. Victoria-wide TINs issued were therefore used, on the assumption that it resembled the trends and changes for those related to the Melbourne area only. In addition, the greater usage of cameras in Melbourne would suggest that a large proportion of these TINs would emanate from the Melbourne area.

Crashes on Melbourne arterial roads specifically were also analysed given the larger reductions observed in the incidence and severity of crashes on these roads in Phase 1.

In summary, the following predictors and measures of speed-related crash risk were selected for the Phase 2 analyses:

Crash Variables:

1. casualty crash frequency in low alcohol times in Melbourne
2. casualty crash severity in low alcohol times in Melbourne
3. casualty crash frequency in low alcohol times on Melbourne arterial roads
4. casualty crash severity in low alcohol times on Melbourne arterial roads

Program Variables:

1. Number of speed camera TINs issued (Victoria)
- 2.a. Publicity (Melbourne television: speed-related TAC advertising only)
- 2.b. Publicity (Melbourne television: all TAC road safety advertising)
3. Hours of camera operations (Melbourne)

These data are displayed graphically in Figures 15 to 19.

Figure 15
Monthly number of casualty crashes in low alcohol hours
Melbourne, 1983-1991

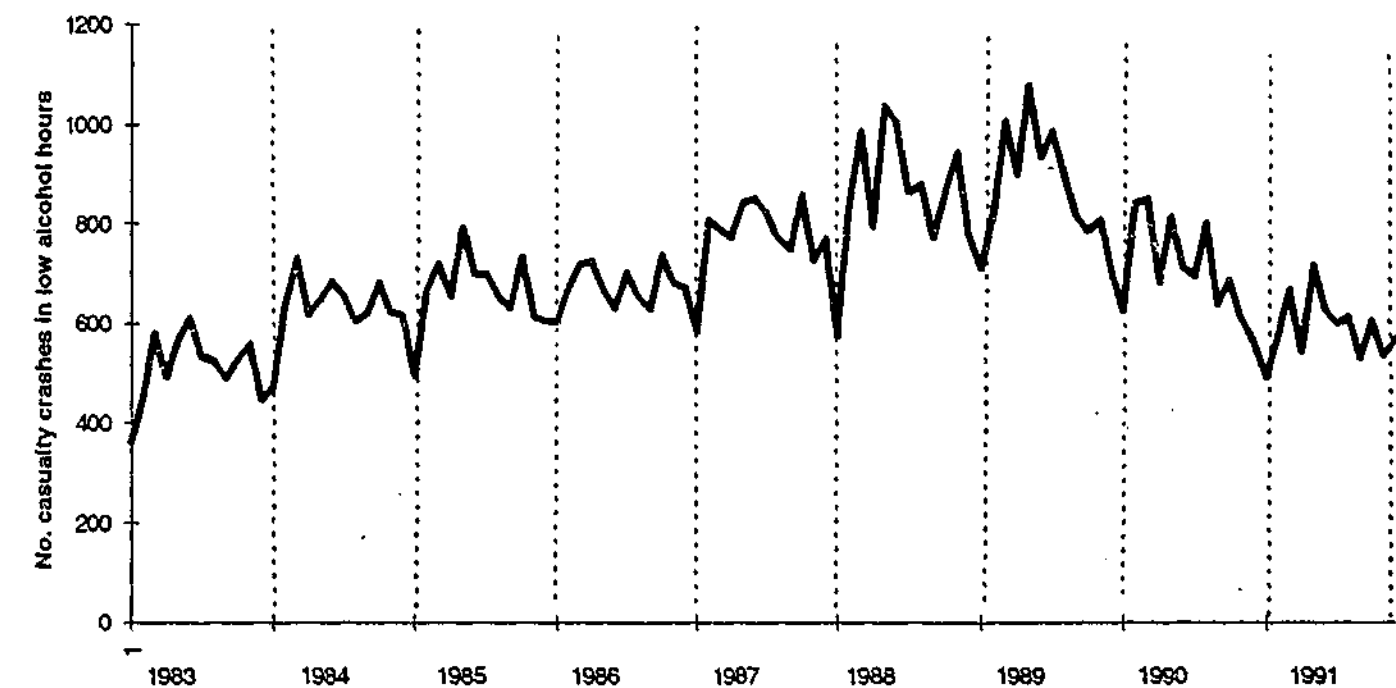


Figure 16
Casualty Crash Severity in low alcohol times during month
Melbourne 1983-1991
(ratio of fatal & serious injury crashes to crashes involving minor injury only)

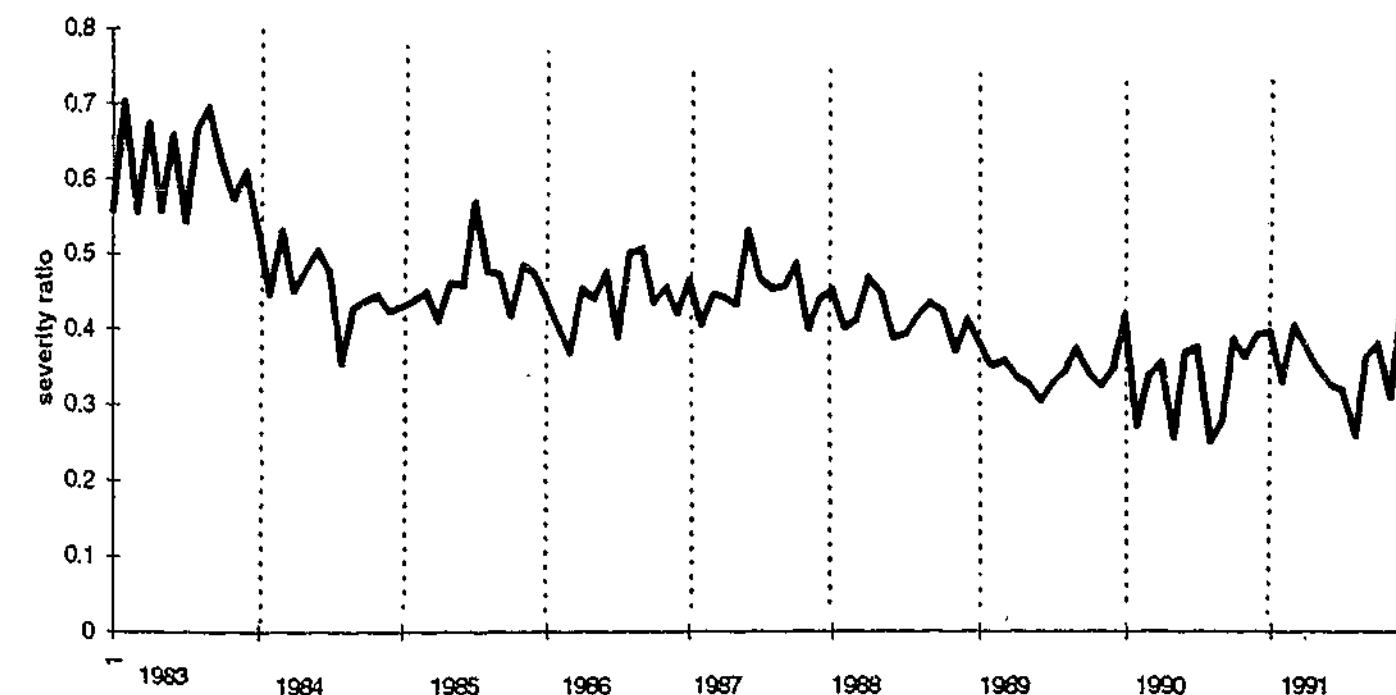


Figure 17
Monthly number of Traffic Infringement Notices Issued
for Speeding Offences Detected by Speed Cameras
Victoria 1989-1991

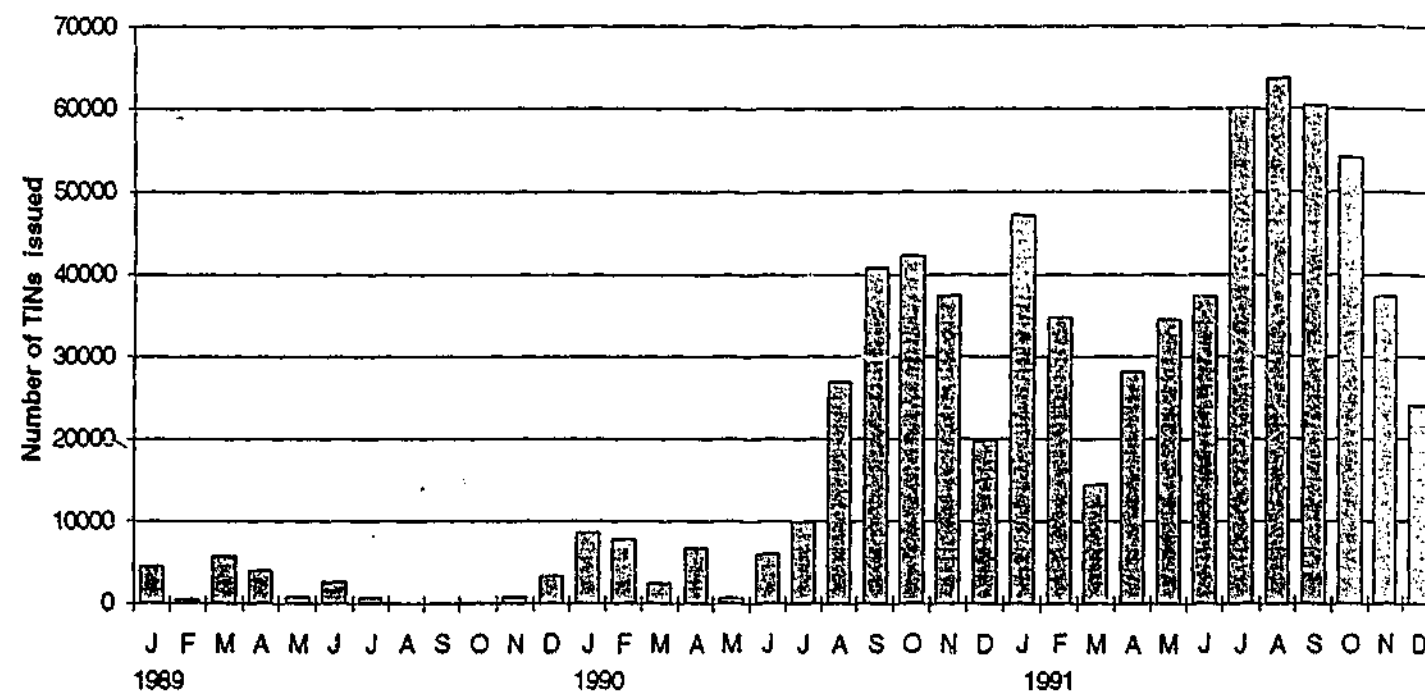


Figure 18
Melbourne television advertising: monthly number of TARPs
Speed related publicity and other TAC road safety publicity

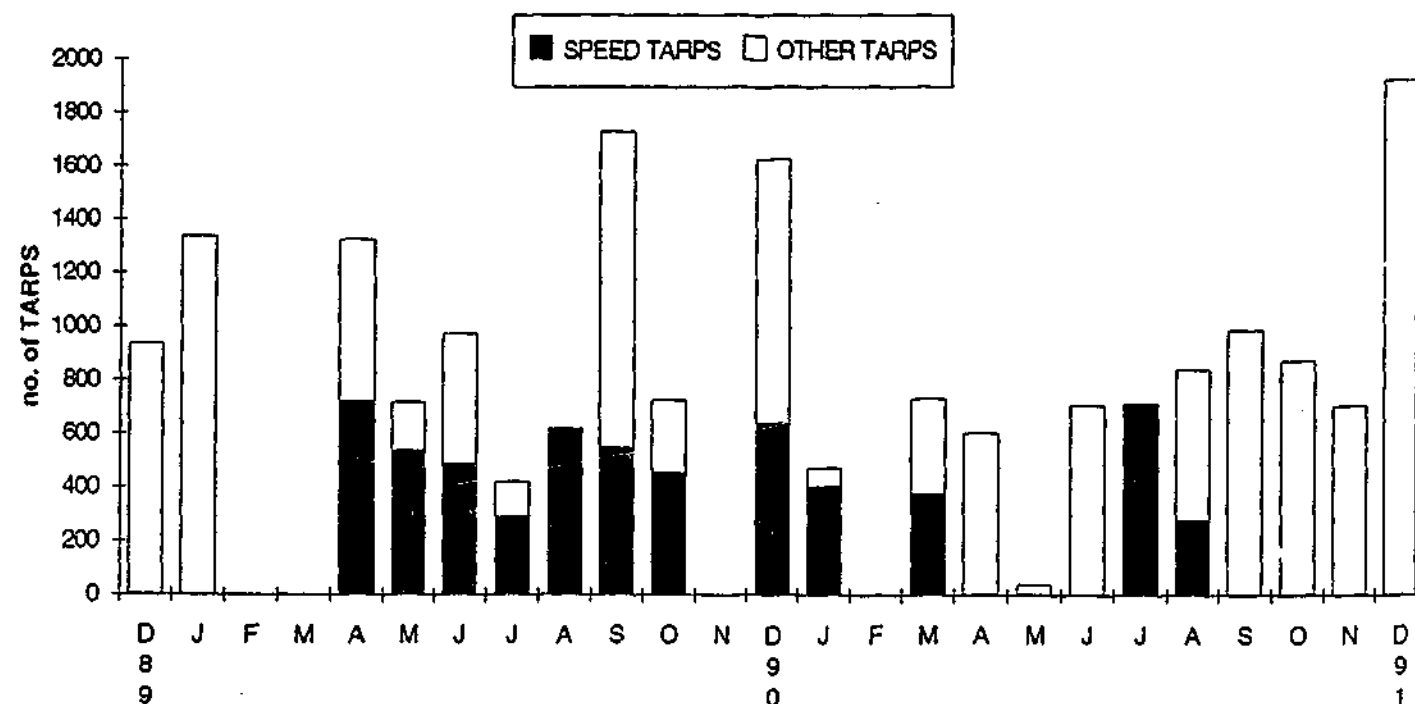
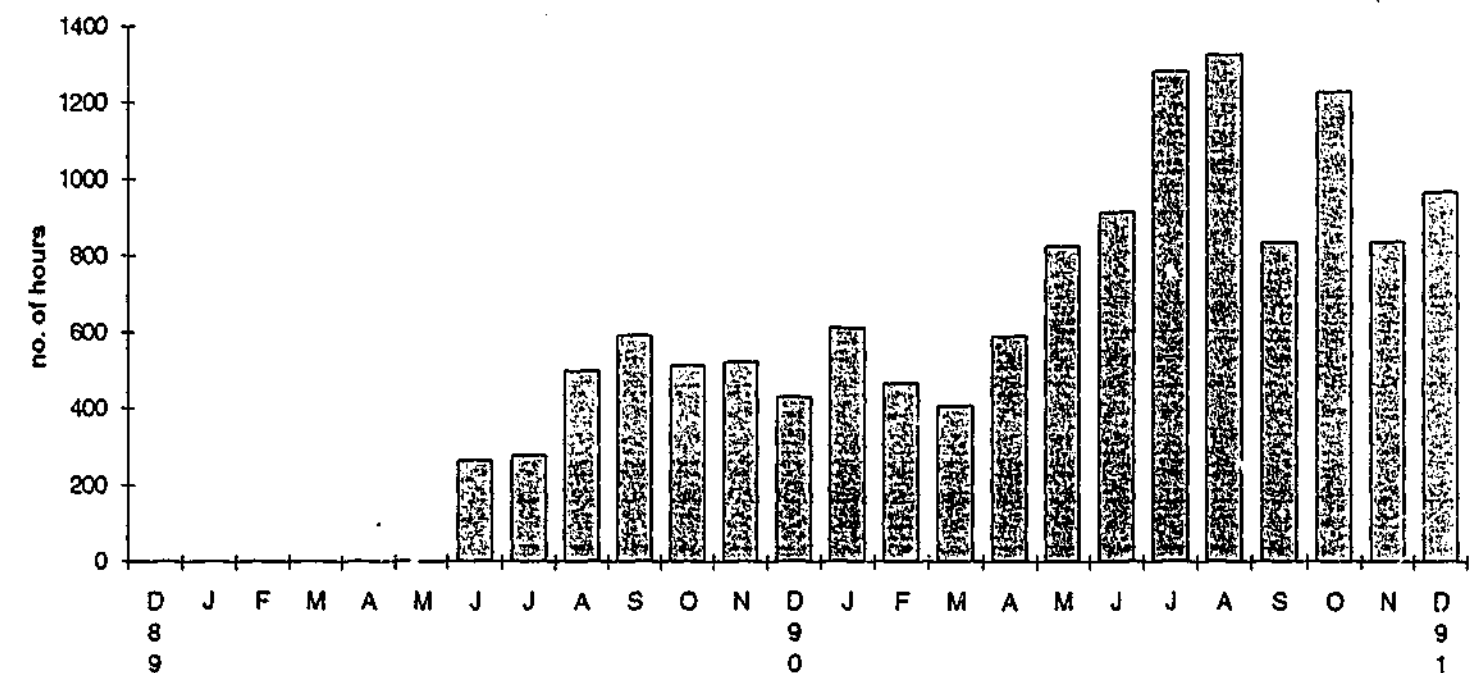


Figure 19
Monthly number of hours of camera operations
Melbourne, June 1990-December 1991



7.4 Hypotheses

Phase 2 aimed to test the following hypotheses relating to the program.

A reduction in the frequency and injury severity of low alcohol hour casualty crashes in Melbourne related to:

- the number of speed camera TINs issued to drivers Victoria-wide
- the level of the TAC Melbourne television speed publicity campaign (Don't fool yourself, speed kills)
- the level of all TAC Melbourne television road safety publicity
- the number of hours of speed camera operations in the metropolitan area

A reduction in the frequency and injury severity of low alcohol hour casualty crashes on Melbourne arterial roads related to:

- the number of speed camera TINs issued to drivers Victoria-wide
- the level of the TAC Melbourne television speed publicity campaign
- the level of all TAC Melbourne television road safety publicity
- the number of hours of speed camera operations in the metropolitan area

The hypotheses are all uni-directional and hence one-tailed significance tests were applied throughout this part of the study, as the results in Phase 1 indicate a directional effect (reduction) of the program in Melbourne.

7.5 Methodology

The relationships between the evaluation criteria (the frequency and injury severity of casualty crashes which occurred in low alcohol times) in Melbourne and the different program measures were determined using multivariate time series analysis models.

7.5.1 Time series analysis

Time series analysis can control for long term trends, seasonal cycles and other regular patterns in the crash variables. Longer term, "smooth" trends (due to a variety of factors including road safety programs) in the crash criteria were taken into account through a trend component in the time series models if necessary. This eliminated the need to include specific measures of such variables in the models, and allowed a better assessment of the real effect of a new or abrupt change to the crash series in time.

The aim of the modelling process was to develop models that closely fit the number and severity of casualty crashes over the period January 1983 to December 1991, for each series, and to estimate the relationships between the program variables in each model and the crash criteria. All models were undertaken using monthly data.

Frequency of casualty crashes

The time series data were modelled using multiple regression methods. The dependent variable in each model was again the monthly number of casualty crashes in low alcohol hours while the independent variables in the regression equations were made up of:

- a linear trend
- monthly dummy variables (to represent seasonal variations)
- unemployment rate in Melbourne
- number of TINs issued in Victoria
- number of TAC speed publicity TARPs or number of all TAC road safety publicity TARPs on Melbourne television
- number of speed camera hours of operation in Melbourne

Unemployment rate was included in the models because it was found to play a significant role in those fitted for Melbourne in Phase 1. It was also considered important for the models in Phase 2 to be compatible with those developed in Phase 1. Similarly, for compatibility with Phase 1, multiplicative models were fitted; natural logarithms of the dependent variable, covariates and independent variables were taken.

Before developing the multiple regression models, the correlations between independent variables were examined because the reliability of the modelling procedure to provide valid estimates of effects of each variable (parameter estimates) relies on the condition that the variables are not highly correlated (Appendix G). An examination of correlations between independent variables showed that the unemployment rate was significantly correlated with hours of camera operation (0.5) and all TAC road safety publicity (0.3), but not with TINs issued nor speed related

TAC publicity. Both hours of camera operation and number of speed camera TINs issued correlated significantly with speed related TAC publicity (both 0.6) and all TAC road safety publicity (both 0.8). Number of speed camera TINs issued and hours of camera operation were also significantly correlated (0.8) with each other. These latter correlations suggest that the program variables are highly correlated with one another. The modelling procedure also provides correlations between parameter estimates; these will be reviewed with the results in order to establish any intercorrelation problems.

Correlations between dependent and independent variables also revealed that low alcohol hour casualty crashes in Melbourne were significantly correlated with the unemployment rate (-0.75), with hours of camera operations (-0.26) and with all TAC road safety publicity (-0.20).

Casualty crash injury severity

Logistic (multiple) regression models of casualty crash severity (in low alcohol times) were developed for crashes in Melbourne from January 1983 to December 1991. Casualty crash injury severity was represented as the ratio:

$$\frac{\text{fatal \& serious injury crashes}}{\text{minor injury crashes}}$$

to provide a sensitive measure of changes in the injury severity of crashes. The same covariates were used in these analyses as those for casualty crash frequency, except for the trend variable which was omitted from the models (as in Phase 1) to reduce problems of intercorrelations between parameter estimates. Thus, the severity models are also compatible with those developed in Phase 1.

7.6 Results

The output from each model and corresponding diagnostic information is presented in Appendix H. The following tables present the estimated relationships between the modelled speed camera program mechanisms and changes in the frequency and injury severity of casualty crashes (in low alcohol hours), specifically on Melbourne arterials and also on all roads in Melbourne.

7.6.1 Models assessing monthly TINs issued, camera operations and speed related TAC publicity only

Table 8 below shows the estimated relationship between TINs issued from speed camera offences, speed publicity and hours of camera operations, and casualty crash frequency and injury severity in low alcohol times of the week, after accounting for the variance related to unemployment rate, seasonal trends and trend over time (for casualty crash frequency only in the case of trend).

The estimated effect of each variable is given by the coefficient for that variable, its T distribution value and its level of statistical significance (the two-tailed significance levels given in Appendix H have been halved because one-tailed directional tests of the hypotheses were made; see section 7.4). The coefficient can be interpreted as the percentage change per month in crash frequency or severity, respectively, given a one percent change in the predictor variable.

Table 8

Estimated effect of monthly speed camera TINs issued, speed related publicity and hours of camera operations on the incidence and severity of casualty crashes (in low alcohol times) on arterial roads in Melbourne and all roads in Melbourne

TREATMENT AREA	Estimated coefficient of logged variable	T value (>90 d.f.)	Significance level (one-tailed)
Melbourne arterial roads			
-casualty crashes in low alcohol times			
• no. of speed camera TINs issued during month	-0.0132**	-3.096	0.0013
• hours of speed camera operations during month	-0.0079	-0.636	0.2632
• speed related TAC publicity during month	-0.0091	-1.538	0.0638
-severity of casualty crashes in low alcohol times			
• no. of speed camera TINs issued during month	-0.0233***	-3.990	0.0001
• hours of speed camera operations during month	-0.0339**	-2.694	0.0042
• speed related TAC publicity during month	-0.0050	-0.602	0.2744
Melbourne all roads			
-casualty crashes in low alcohol times			
• no. of speed camera TINs issued during month	-0.0078***	-3.692	0.0002
• hours of speed camera operations during month	-0.0033	-0.628	0.2660
• speed related TAC publicity during month	-0.0040	-1.504	0.0681
-severity of casualty crashes in low alcohol times			
• no. of speed camera TINs issued during month	-0.0188***	-3.297	0.0007
• hours of speed camera operations during month	-0.0380**	-3.099	0.0013
• speed related TAC publicity during month	0.0004	0.055	0.4783

* Statistically significant at $p < 0.05$ level; ** Statistically significant at $p < 0.01$ level; *** Statistically significant at $p < 0.001$ level

The results in Table 8 indicate that there is a significant relationship between **casualty crash frequency (in low alcohol times)** and speed camera TINs issued to drivers, for crashes on arterial roads specifically and on all roads in Melbourne, with a stronger effect observed on arterial roads. The estimated effect of speed related publicity on the frequency of casualty crashes approached statistical significance at the 0.05 level. Hours of camera operation did not appear to be associated with reductions in the frequency of casualty crashes.

Casualty crash injury severity (in low alcohol times) was also significantly related to speed camera TINs issued to drivers, for crashes on arterial roads specifically and on all roads in Melbourne, with a stronger effect observed on arterial roads. In addition, hours of camera operation was related to casualty crash injury severity for all Melbourne roads and for arterial roads only. There was no statistically significant relationship between speed related publicity and casualty crash severity.

Efficacy of the models

Examination of all the models indicated that they fit the crash data well with around 84% and 66% of the variance in casualty crash frequency and casualty crash severity, respectively, accounted for by these models.

Inspection of intercorrelations between coefficient estimates for **casualty crash frequency models** indicated however, that there were high correlations (around 0.8) between estimates for the intercept term, unemployment rate, and trend; between the estimate for hours of camera operation and estimates for the intercept, unemployment rate and trend (around -0.7). Moderate correlations (around 0.4) were also observed between the estimate for speed publicity and those for the intercept, trend and unemployment rate. As indicated by the correlations between variables in section 7.5.1, hours of camera operations was significantly correlated with unemployment rate, speed publicity and TINs issued; TINs issued was also significantly correlated with speed publicity. Such intercorrelations may mean that the estimated relationships reported in Table 8 are unreliable, particularly that indicated for camera operations (and to a lesser extent that for speed publicity) given its high correlations with the estimates for other significant variables (ie. TINs issued, unemployment rate and trend).

Inspection of intercorrelations between coefficient estimates for **casualty crash injury severity models** also indicated high (0.6 - 0.9) but fewer correlations than for the casualty crash models mainly because the trend variable was omitted from these models for comparability with Phase 1 models. The estimated coefficient for unemployment rate was correlated with those for the intercept term, hours of camera operation and TINs issued; the estimate for TINs issued was also correlated with the estimate for hours of camera operations. These intercorrelations may also have affected the estimated relationships derived from the models of casualty crash injury severity.

7.6.2 Models assessing monthly TINs issued, camera operations and all TAC road safety publicity

Table 9 shows the estimated relationships when all TAC road safety publicity is included in the models rather than only speed related publicity.

These results confirm the relationships observed in Table 8, but also indicate that there is a statistically significant relationship between casualty crash frequency and total TAC road safety publicity. The estimated size of the effects of the other program variables (TINs issued and camera operation hours) varied only slightly when the models are fitted with total publicity data, except for the estimate of the

effect of TINs issued on casualty crash frequency on arterial roads which halved in size (but remained statistically significant).

Table 9

Estimated effect of monthly camera TINs issued, all TAC road safety publicity and hours of camera operations on the incidence and severity of casualty crashes (in low alcohol times) on arterial roads in Melbourne and all roads in Melbourne

TREATMENT AREA	Estimated coefficient of logged variable	T value (>90 d.f.)	Significance level (one-tailed)
Melbourne arterial roads			
-casualty crashes in low alcohol times			
• no. of speed camera TINs issued during month	-0.0065**	-2.754	0.0036
• hours of speed camera operations during month	-0.0069	-1.367	0.0876
• all TAC road safety publicity during month	-0.0056*	-1.669	0.0493
-severity of casualty crashes in low alcohol times			
• no. of speed camera TINs issued during month	-0.0228***	-3.620	0.0003
• hours of speed camera operations during month	-0.0354**	-2.814	0.0030
• all TAC road safety publicity during month	-0.00270	-0.258	0.3983
Melbourne all roads			
-casualty crashes in low alcohol times			
• no. of speed camera TINs issued during month	-0.0065**	-3.000	0.0018
• hours of speed camera operations during month	-0.0043	-0.921	0.1797
• all TAC road safety publicity during month	-0.0077**	-2.499	0.0071
-severity of casualty crashes in low alcohol times			
• no. of speed camera TINs issued during month	-0.0183**	-2.981	0.0019
• hours of speed camera operations during month	-0.0371**	-3.029	0.0016
• all TAC road safety publicity during month	-0.0019	-0.188	0.4256

*Statistically significant at $p < 0.05$ level; ** Statistically significant at $p < 0.01$ level; *** Statistically significant at $p < 0.001$ level

The intercorrelations between estimated coefficients present in the models of casualty crash frequency and injury severity using only speed publicity are again present in these models. However, whilst there had been moderate correlations between the speed publicity estimate and the intercept, trend and unemployment rate estimates in casualty crash frequency models, this was not the case when all TAC road safety publicity was used. This may explain why all TAC publicity appears to have a statistically significant relationship with casualty crash frequency, whilst speed publicity does not achieve statistical significance.

7.7 Discussion

Significant relationships were found between some of the measures of the program and the frequency and injury severity of casualty crashes which occurred in low alcohol times, in 1990 and 1991. These were:

Frequency of casualty crashes (low alcohol times)	Injury severity of casualty crashes (low alcohol times)
<ul style="list-style-type: none"> • Speed camera TINs issued to drivers • All TAC road safety publicity • Speed related TAC publicity (sig. at 0.07 level only) 	<ul style="list-style-type: none"> • Speed camera TINs issued to drivers • Speed camera operation hours

The effects of speed camera TINs issued to drivers was consistently related to both crash frequency and injury severity, with stronger effects observed on arterial roads for both criteria. The direct effects of speed camera operations themselves (basically the immediate effects of the cameras operating) appeared to be confined to casualty crash injury severity only. This may indicate that:

- drivers are not aware of cameras operating because they are generally not obvious and hence cannot react to operations directly, or
- exposure to camera operations is not sufficient to modify speed behaviour to the degree required to reduce the risk of a casualty crash occurring but receipt of a TIN and punishment for speeding is effective in reducing crash risk.

Alternatively, the high intercorrelations between variables and estimates for the variables, especially for camera operations and other significant variables, may have masked its relationship with casualty crash frequency. In addition, the casualty injury severity measure may be a more specific and more sensitive criterion related to speed than casualty crash frequency.

In relation to the findings for publicity, similar comments apply for the relationship between speed related publicity and casualty crash frequency. The estimate for this variable was correlated with the estimates for other significant variables. On the other hand, the stronger significant effect of all TAC road safety publicity rather than speed related publicity on casualty crash frequency may reflect that casualty crash frequency is a general measure of the risk of all types of (low alcohol hour) crashes and it is not confined to "speed-related" crashes (which could reasonably be expected to be affected by the speed-related publicity).

It appears that TAC publicity (speed related only or all publicity) was not related to casualty crash injury severity.

As noted in relation to the results of Phase 1, the results of Phase 2 are also based on the inclusion of unemployment rate in the models of casualty crash frequency as well as for the models of casualty crash injury severity. Explicitly controlling for changes in exposure was considered to be important for estimating the effects of the program

on crash frequency⁴. Although there was no prior evidence of the importance of exposure/unemployment rate on crash severity, a significant statistical relationship was found between unemployment rate and the injury severity of casualty crashes (in urban areas in particular) which led to its inclusion in models of crash severity as well.

Thus, estimated effects of program variables on casualty crash severity in Melbourne are based on models which take into account the effects of changes in unemployment rate. This means that these estimated effects are based on the premise that casualty crash severity (in low alcohol hours) would have increased in Melbourne (given the increase in unemployment rate which occurred during the intervention period) had the speed camera program not been introduced.

7.8 Limitations of Phase 2

Difficulties inherent in this approach

One of the major practical difficulties with attempting to model the specific aspects of the program is the reliance on available quantified measures of the program and representing it validly. The lack of monthly time series data for other program mechanisms (eg. knowledge of other drivers who have been caught, number of drivers issued with multiple TINs, number of drivers with increasing demerit points) meant that a full model of the program mechanisms and therefore its effects was not developed. On the other hand, while this was theoretically desirable, in practice it may have been impossible to develop a full model and disaggregate each mechanism's sole contribution to observed reductions in the crash criteria, given that the mechanisms are unavoidably interrelated.

Problems with the feasibility of this aim have been outlined previously in an evaluation of the Random Breath Testing (RBT) initiative in Victoria during 1989-1991, where the feasibility of dissociating the effects of enforcement and publicity was discussed (Cameron, Cavallo & Sullivan, 1992). These include the statistical problems of collinearity between the measures, and between the measures and other influences, the appropriate representation and quantification of the mechanisms within the model (eg. does a measure of enforcement, such as number of TINs issued, represent its effects on the public and therefore does this provide a fair test of its relationship with crashes?), the possible non-linear relationships between the mechanisms and crash risk, and the fact that correlational relationships between the program and crashes does not prove causality absolutely. To properly dissociate the effects of the parallel elements of the program a longer post-intervention period (3-4 years) is required. For these reasons, statistically significant effects which seem related to a particular program mechanism cannot definitively be attributed to that particular part of the program only, given that it operated in the presence of the other mechanisms (quantified and unquantified) and other unknown factors. Indeed, the relative importance of the various mechanisms in driver perceptions, speed choice

⁴ It was considered that unemployment was an adequate proxy measure of vehicle travel over time given some prior evidence of a correlational relationship between unemployment rate and fatalities and relatively high correlations between State-wide estimated total vehicle travel (based on fuel sales) and unemployment rate measures.

and speed behaviour and the extent to which these have altered cannot be understood through this quantitative approach.

Additionally, some of the mechanisms which may act to influence speed behaviour, would also at the same time be measures of behavioural changes; eg. the level of TINs issued reflects both the mechanism influencing speed behaviour, and if the program is successful, an outcome indicator. The modelling of variables which could be both measures of treatment (independent variable) and of response to treatment (dependent variable) as independent variables, will diminish the power of this approach, particularly for later periods in the intervention when driver responses, if any, are most likely to be manifested in such measures.

Efficacy of the models

The models were found to fit the crash data well (around 84% and 66% of the variance in the casualty crash frequency and casualty crash severity criteria, respectively, was accounted for by these models). However, intercorrelations between coefficient estimates for **casualty crash frequency models** (in particular) and **casualty crash injury severity models** indicated that there were high correlations between many estimates. Such intercorrelations may mean that the estimated relationships reported are unreliable, particularly that indicated for the relationship between camera operations (and to a lesser extent that for speed publicity) and casualty crash frequency, given its high correlations with the estimates for other significant variables (ie. TINs issued, unemployment rate and trend).

Unemployment rate as a measure of low alcohol hour vehicle travel

The same assumptions that applied to Phase 1 apply to this part of the study (see section 6.8.1).

8.0 FURTHER RESEARCH

It is now planned to undertake Phase 3 of this evaluation, as outlined in section 5.0. This should provide more definitive evidence of a cause-and-effect link between the speed camera operations and the general effects observed in Phase 1 of the evaluation. This phase should also provide further understanding of the key factor affecting the deterrent effect of the program, so that future camera operations of the program can be optimised.

An additional Phase which attempts to examine existing traffic speed data over time to provide additional supporting evidence linking the observed effects to the speed camera program will also be attempted. It may not, however, replace the evidence that should be provided by the proposed Phases 3 of the evaluation, given the paucity of traffic speed data over the relevant time period.

Additional research areas and data requirements emerged from the study to date.

These relate to:

- an urgent need for better vehicle exposure/traffic volume information over time, given the importance of taking into account changes in exposure when assessing the effects of road safety programs, particularly for post-hoc, quasi-experimental evaluations where the availability of adequate comparison areas is questionable and given that statistical forms of control using surrogate measures also poses problems;
- an investigation of the risk of varying levels of injury outcomes in crashes under different levels of vehicle exposure/traffic volumes, given the finding that unemployment rate in urban areas was found to correlate with the severity of casualty crashes in low alcohol hours;
- a research program which investigates the process of changes in driver perceptions, speed choice and speed behaviour and the factors involved, given that the process by which such changes may have occurred are poorly understood and would be useful for improving behavioural change programs; in particular, the extent and consistency of modifications in speed behaviour amongst drivers in relation to a number of contingencies (given that speed behaviour is transient and is influenced by a variety of factors), the nature of speed deterrence and the critical elements which enact and maintain desired behavioural changes.

9.0 CONCLUSIONS

The introduction of the speed camera program and supporting publicity in Victoria has been associated with decreases in the frequency of reported casualty crashes which occur in low alcohol times of the week and also decreases in their injury severity. These decreases have been calculated as departures from expected values based on past trends and seasonal patterns, changes in unemployment rate, and using the comparable areas of NSW as a control. The magnitudes of these decreases have varied with the region of the State and nature of the program operated during different periods. The program appears to have had its greatest effects (in terms of the frequency of casualty crashes) on arterial roads in Melbourne and on 60 km/h roads in rural Victoria, where the majority of the speed camera operations have taken place within the respective Melbourne and country areas. This pattern of results provides further evidence that the observed effects relate to the speed camera program.

The results of Phase 2 suggest that the reductions in the frequency of casualty crashes (in low alcohol hours) appeared to be linked with speed camera TINs issued to detected drivers, TAC road safety publicity in general and TAC speed related publicity (lower level of significance). Reductions in the injury severity of casualty crashes (in low alcohol hours) appeared to be associated with speed camera TINs issued and hours of camera operation.

On the basis of the analyses in this report, it is clear that the speed camera program (enforcement and supporting publicity) has been effective.

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APPENDICES

APPENDIX A

EVALUATION CRITERIA: SPEED-RELATED CRASHES

APPENDIX A

EVALUATION CRITERIA: SPEED-RELATED CRASHES

A sound impact evaluation uses an evaluation criterion (or criteria) which is directly related to the program to ensure that it is as valid and as statistically powerful (within the constraints of the sample size available) as possible. In most road safety program evaluations the target group is clearly defined (eg. young drivers) and/or there are direct measures of the target crash type available in mass data (eg. intersection crashes, alcohol-involved crashes) or very good surrogate measures exist (serious casualty crashes in high alcohol hours).

In contrast, "speeding" is a pervasive and transient behaviour making it difficult to identify its presence in crash types or characteristics. Given the difficulties in studying the relationship between travel speed and crashes, its role as a factor in contributing to or causing crash risk is not well understood. There is therefore no empirical evidence pointing to a target group. Notwithstanding this, an attempt has been made to identify target group(s) (available in the mass crash data) which might be related to speed-related crashes. A review of available information and literature on "speeding" was undertaken to do this, with emphasis placed upon Australian studies/data. The balance between the need to identify a target group and maintaining the validity of the evaluation was discussed in the main part of the report. It was decided that the available evidence was insufficient to identify speed-related crashes.

Selected studies pointing to potential target group(s)

Solomon (1964)- Elevated crash risk found for significant departures from average traffic speeds for night-time crashes (U shaped relationship), whilst the trend for daytime crashes was much weaker. Crash types that showed this trend were (mainly) night-time single vehicle ran off road collisions (only for higher speeds) and head-on collisions. Solomon found no evidence of an interaction between driver age or sex and travel speed for accident involvement rates at lower speed ranges. For the highest speeds there appeared to be a difference between younger and older drivers, with younger drivers over represented in higher speeds.

Accident data, speed profiles and vehicle volumes (used to estimate vehicle-miles of travel) were collected on selected 2-lane and 4-lane US main rural highways (freeways were not included) during the day and night. Six thousand crashes and 10,000 vehicle-involvements were used in the study. However, most of the speed estimates of crash-involved vehicles were made by police or third parties, and averaged estimates of vehicle-miles travelled across speed distributions were used. The data relate to the late 1950's.

Research Triangle Institute Study (1970) & West & Dunn (1971)- Flat U shaped relationship found between crash risk and departure from average traffic speeds. Small sample sizes did not allow disaggregation by crash types or other characteristics. There was a tendency, however, for younger drivers (≤ 24 years) to be involved in crashes involving higher speeds and older drivers (≥ 40 years) in crashes involving lower speeds.

These studies are considered to better estimate the speeds of crash involved vehicles prior to crashing, and provide matched speed distributions of the traffic flow for the times and locations of the individual crashes investigated. Sample sizes are much smaller than for Solomon's study. The four State highways in Indiana were used in the study. Both these and Solomon's study use the concept of comparing speed distribution of vehicles involved in crashes, for comparison with the speeds of vehicles in the traffic stream, to estimate those parts of the speed distribution which are associated with high crash rates.

Cowley (1983) Characteristics of the speeding driver- review and analysis of ARRB rural journey speed data (1979) suggested that young, male drivers, and younger vehicles were associated with higher travel speeds. However, these type of data provide characteristics related to travel speeds and these may have little bearing on "speeding" in crashes. Comparisons between "speeders" and "non-speeders", and "speeding" and "non-speeding" factors only are possible using such data.

Casualty crash data were also analysed for 1979 and although speed estimation of crash involved vehicles is fraught with problems, the data are directly related to casualty crash frequency. Cowley found that "serious speeding" (defined as travelling at 25 km/h over the posted speed limit) was more likely to be associated with: young (< 25 years) drivers; male drivers; drivers with less than two years driving experience; drink driving; night-time; weekend driving; high crash severity. The advantages of this study are that the results are based on Victorian data and use casualty crash involvement to determine factors related to speeding in crashes.

Cowley also undertook a review of available Australian and overseas literature. He concluded that:

- there appears to be some association between speeding and alcohol in crashes
- young drivers and possibly male drivers are more likely to speed or travel at high speeds
- newer, high-powered cars are probably associated with speeding or high speeds
- longer trips are possibly associated with high speeds or speeding.

Fildes, Rumbold & Leening (1991) Speed behaviour and driver attitudes to speeding- Younger drivers (< 34 years) were likely to be "excessive fast travellers" whilst drivers aged 45 years or more were more likely to be "excessively slow" travellers. Sex of the driver was unrelated to travel speed. The number of vehicle occupants was also significantly related to travel speeds.

This study provides information relevant to the recent Victorian context for both rural and urban locations, during daylight hours. It focuses on the characteristics of and differences between drivers who travel at differing speeds. There is no information relating to the characteristics of those who are involved in "speed-related" crashes. Therefore, the assumption has to be made that age group involvement in varying travel speeds is mirrored in crash travel speeds, assuming a direct exposure-crash involvement relationship exists.

CORRELATIONS BETWEEN VARIABLES

APPENDIX B

CORRELATIONS BETWEEN VARIABLES

1. Correlations between estimated total vehicle travel and unemployment measures for Melbourne, Sydney, rural Victoria and rural NSW for the period January 1983-December 1991 are presented. Correlations between estimated total vehicle travel and unemployment rates, incorporating monthly lags, are also shown.
2. Correlations between the variables used in each model are presented. The variables are unemployment rate, the intervention variables (T1a to T2b) and the dependent variables number of low alcohol hour casualty crashes and severity of low alcohol hour casualty crashes.

Correlations were conducted on log transformed data as this was the form of the data to be used in the modelling (to achieve a multiplicative structure).

Glossary of variable codes

LOGUNEMP	Natural Logarithm of number of unemployed persons
LOGURATE	Natural Logarithm of unemployment rate
LOGTRAV	Natural Logarithm of estimated total vehicle travel
LOGCNT	Natural Logarithm of number of low alcohol hour casualty crashes
LOG_T1A	Natural Logarithm of December 1989-March 1990 (T1a) Intervention (Dummy) Variable = e in Dec. 1989-March 1990 and 1 elsewhere
LOG_T1B	Natural Logarithm of April 1990-June 1990 (T1b) Intervention (Dummy) Variable = e in April 1990-June 1990 and 1 elsewhere
LOG_T2A	Natural Logarithm of July 1990-February 1991 (T2a) Intervention (Dummy) Variable = e in July 1990-February 1991 and 1 elsewhere
LOG_T2B	Natural Logarithm of March 1991-December 1991 (T2b) Intervention (Dummy) Variable = e in March-December 1991 and 1 elsewhere

Covariate Correlations - Melbourne - Jan 1983 to Dec 1991

Correlation Coefficients

	LOGUNEMP	LOGURATE	LOGTRAV
LOGUNEMP	1	0.9738	-0.3552
LOGURATE	0.9738	1	-0.5427
LOGTRAV	-0.3552	-0.5427	1

Covariate Correlations - Rural Victoria - Jan 1983 to Dec 1991

Correlation Coefficients

	LOGUNEMP	LOGURATE	LOGTRAV
LOGUNEMP	1	0.9125	0.0253
LOGURATE	0.9125	1	-0.335
LOGTRAV	0.0253	-0.335	1

Covariate Correlations - Sydney - Jan 1983 to Dec 1991

Correlation Coefficients

	LOGUNEMP	LOGURATE	LOGTRAV
LOGUNEMP	1	0.9734	-0.5119
LOGURATE	0.9734	1	-0.6658
LOGTRAV	-0.5119	-0.6658	1

Covariate Correlations - Rural NSW - Jan 1983 to Dec 1991

Correlation Coefficients

	LOGUNEMP	LOGURATE	LOGTRAV
LOGUNEMP	1	0.9095	-0.4031
LOGURATE	0.9095	1	-0.6946
LOGTRAV	-0.4031	-0.6946	1

Travel v. Unemployment Rate Correlations - Melbourne - Jan 1983 to Dec 1990

1

CORR	KMTRAVEL	LAG_0	LAG_1	LAG_2	LAG_3	LAG_4	LAG_5	LAG_6
KMTRAVEL	1	-0.8336	-0.7808	-0.7196	-0.6552	-0.5811	-0.5316	-0.4886
LAG_0	-0.8336	1	0.9445	0.8805	0.8098	0.7394	0.6845	0.6137
LAG_1	-0.7808	0.9445	1	0.9435	0.8806	0.8068	0.74	0.6857
LAG_2	-0.7196	0.8805	0.9435	1	0.944	0.882	0.809	0.7428
LAG_3	-0.6552	0.8098	0.8806	0.944	1	0.9422	0.8826	0.8105
LAG_4	-0.5811	0.7394	0.8068	0.882	0.9422	1	0.941	0.8818
LAG_5	-0.5316	0.6845	0.74	0.809	0.8826	0.941	1	0.9417
LAG_6	-0.4886	0.6137	0.6857	0.7428	0.8105	0.8818	0.9417	1

Travel v. Unemployment Rate Correlations - Rural Victoria - Jan 1983 to Dec 1990

2

CORR	KMTRAVEL	LAG_0	LAG_1	LAG_2	LAG_3	LAG_4	LAG_5	LAG_6
KMTRAVEL	1	-0.6077	-0.5467	-0.5073	-0.4791	-0.4317	-0.3766	-0.3497
LAG_0	-0.6077	1	0.7885	0.6745	0.6433	0.5286	0.4202	0.3053
LAG_1	-0.5467	0.7385	1	0.7738	0.6846	0.6532	0.544	0.4393
LAG_2	-0.5073	0.6745	0.7738	1	0.7546	0.6614	0.6184	0.5167
LAG_3	-0.4791	0.6433	0.6846	0.7546	1	0.7632	0.6763	0.6339
LAG_4	-0.4317	0.5286	0.6532	0.6614	0.7632	1	0.77	0.6868
LAG_5	-0.3766	0.4202	0.544	0.6184	0.6763	0.77	1	0.78
LAG_6	-0.3497	0.3053	0.4393	0.5167	0.6339	0.6868	0.78	1

Travel v. Unemployment Rate Correlations - Sydney - Jan 1983 to Dec 1990

3

CORR	KMTRAVEL	LAG_0	LAG_1	LAG_2	LAG_3	LAG_4	LAG_5	LAG_6
KMTRAVEL	1	-0.8479	-0.8174	-0.8015	-0.7877	-0.7701	-0.7509	-0.7457
LAG_0	-0.8479	1	0.9557	0.8949	0.849	0.8211	0.7936	0.7632
LAG_1	-0.8174	0.9557	1	0.9551	0.8929	0.8447	0.8159	0.7875
LAG_2	-0.8015	0.8949	0.9551	1	0.953	0.8879	0.8374	0.8054
LAG_3	-0.7877	0.849	0.8929	0.953	1	0.9508	0.8823	0.827
LAG_4	-0.7701	0.8211	0.8447	0.8879	0.9508	1	0.9485	0.8759
LAG_5	-0.7509	0.7936	0.8159	0.8374	0.8823	0.9485	1	0.9456
LAG_6	-0.7457	0.7632	0.7875	0.8054	0.827	0.8759	0.9456	1

Travel v. Unemployment Rate Correlations - Rural NSW - Jan 1983 to Dec 1990

4

CORR	KMTRAVEL	LAG_0	LAG_1	LAG_2	LAG_3	LAG_4	LAG_5	LAG_6
KMTRAVEL	1	-0.8007	-0.7563	-0.7635	-0.7452	-0.7289	-0.7495	-0.7525
LAG_0	-0.8007	1	0.8938	0.8167	0.7743	0.7845	0.7534	0.7205
LAG_1	-0.7563	0.8938	1	0.8941	0.8117	0.7703	0.7799	0.7511
LAG_2	-0.7635	0.8167	0.8941	1	0.8871	0.8092	0.7619	0.7799
LAG_3	-0.7452	0.7743	0.8117	0.8871	1	0.8871	0.8041	0.758
LAG_4	-0.7289	0.7845	0.7703	0.8092	0.8871	1	0.8866	0.8011
LAG_5	-0.7495	0.7534	0.7799	0.7619	0.8041	0.8866	1	0.8855
LAG_6	-0.7525	0.7205	0.7511	0.7799	0.758	0.8011	0.8855	1

Correlations between Intervention Variables & Unemployment Rate
Correlations between number of low alcohol hour casualty crashes & Unemployment Rate

Correlation Coefficients					
AREA	LOG_T1A	LOG_T1B	LOG_T2A	LOG_T2B	LOGCNT
Melbourne				0.5	-0.75
Sydney	-0.26	-0.25	-0.21	0.19	
Rural Victoria	-0.42	-0.33		0.54	-0.44
Rural NSW	-0.23	-0.3	-0.31		
Victoria	-0.25	-0.21		0.54	-0.73
NSW	-0.26	-0.28	-0.26		

METHOD FOR CALCULATING CONFIDENCE INTERVALS FOR
NET PERCENTAGE CHANGES

APPENDIX C

APPENDIX C

METHOD FOR CALCULATING CONFIDENCE INTERVALS FOR NET PERCENTAGE CHANGES

Parameter estimates and related standard errors are used to calculate net percentage changes and confidence intervals. All calculations are undertaken in log space, then the final figures are converted.

Parameter estimates for a treated area and its respective comparison area are subtracted to provide the net parameter estimate, which is then converted from log space and expressed as a percentage. Its confidence interval is calculated by simply adding the variances of the two areas and taking their square root*, to provide a variance for the net difference, from which a confidence interval can be calculated in the standard way.

An illustration is presented showing net percentage changes and their confidence intervals derived from individual models.

* Theorem 5.8(b) Mendenhall, Scheaffer & Wackerly "Mathematical Statistics with Applications"

ARIMA.XLS

	Coefficient	Standard Error	T-Ratio	Approx. Probability	Percent Change	95% Conf. Interval Lower	Interval Upper
Victoria (Unemployment) - Crashes 1, 2, 3							
ARIMA(0,0,1 (0,1,1)'							
MA1	-0.1932	0.1019	-1.8960	0.06			
SMA1	0.7170	0.1281	5.5963	0.00			
LOGURATE	-0.0308	0.0020	-15.6100	0.00			
LOGD_T1A	-0.1709	0.0396	-4.3118	0.00	-15.7%	-22.0%	-8.9%
LOGD_T1B	-0.3416	0.0474	-7.2027	0.00	-28.9%	-35.2%	-22.0%
LOG_T2A	-0.3361	0.0342	-9.8252	0.00	-28.5%	-33.2%	-23.6%
LOG_T2B	-0.2454	0.0346	-7.0993	0.00	-21.8%	-26.9%	-16.3%
R SQUARE ADJ							
0.7014							
N.S.W (Unemployment) - Crashes 1, 2, 3, 4							
ARIMA(0,0,1 (0,1,1)'							
MA1	-0.5150	0.0828	-6.2187	0.00			
SMA1	0.9991	30.0155	0.0333	0.97			
LOGURATE	-0.0007	0.0019	-0.3443	0.73			
LOGD_T1A	-0.0881	0.0339	-2.5994	0.01	-8.4%	-14.3%	-2.1%
LOGD_T1B	-0.1173	0.0400	-2.9361	0.00	-11.1%	-17.8%	-3.8%
LOG_T2A	-0.1013	0.0325	-3.1159	0.00	-9.6%	-15.2%	-3.7%
LOG_T2B	-0.1689	0.0347	-4.8706	0.00	-15.5%	-21.1%	-9.6%
R SQUARE ADJ							
0.4986							
Net Percentage Changes							
LOGD_T1A					-8.0%	-16.9%	2.0%
LOGD_T1B					-20.1%	-29.2%	-9.8%
LOGD_T2A					-20.9%	-27.9%	-13.3%
LOGD_T2B					-7.4%	-15.9%	2.0%

STATEWIDE RESULTS

APPENDIX D

ARIMASP.XLS

	Coefficient	Standard Error	T-Ratio	Approx. Probability	Percent Change	95% Conf. Interval	
						Lower	Upper
Victoria (Travel) - Crashes 1, 2, 3							
<i>ARIMA(0,0,1) (0,1,1)</i>							
MA1	-0.1603	0.1011	-1.5736	0.12			
SMA1	0.7465	0.1347	5.5418	0.00			
LOGTRAV	0.0057	0.0003	17.1169	0.00			
LOGD_T1A	-0.1747	0.0373	-4.6846	0.00	-16.0%	-21.9%	-9.7%
LOGD_T1B	-0.3371	0.0447	-7.5272	0.00	-28.6%	-34.6%	-22.1%
LOGD_T2A	-0.3366	0.0324	-10.3815	0.00	-28.6%	-33.0%	-23.9%
LOGD_T2B	-0.2593	0.0331	-7.8215	0.00	-22.8%	-27.7%	-17.7%

R SQUARE ADJ 0.7168

Victoria (Unemployment) - Crashes 1, 2, 3

ARIMA(0,0,1) (0,1,1)

MA1	-0.1932	0.1019	-1.8960	0.06			
SMA1	0.7170	0.1281	5.5963	0.00			
LOGURATE	-0.0308	0.0020	-15.6100	0.00			
LOGD_T1A	-0.1709	0.0396	-4.3118	0.00	-15.7%	-22.0%	-8.9%
LOGD_T1B	-0.3416	0.0474	-7.2027	0.00	-28.9%	-35.2%	-22.0%
LOGD_T2A	-0.3361	0.0342	-9.8252	0.00	-28.5%	-33.2%	-23.6%
LOGD_T2B	-0.2454	0.0346	-7.0993	0.00	-21.8%	-26.9%	-16.3%

R SQUARE ADJ 0.7014

ARIMASP.XLS

	Coefficient	Standard Error	T-Ratio	Approx. Probability	Percent Change	95% Conf. Interval	
						Lower	Upper
N.S.W (Travel) - Crashes 1, 2, 3, 4							
<i>ARIMA(0,0,1) (0,1,1)</i>							
MA1	-0.5144	0.0833	-6.1762	0.00			
SMA1	0.9904	2.9786	0.3325	0.74			
LOGTRAV	0.0002	0.0003	0.4913	0.62			
LOGD_T1A	-0.0899	0.0339	-2.6517	0.01	-8.6%	-14.5%	-2.3%
LOGD_T1B	-0.1193	0.0397	-3.0059	0.00	-11.2%	-17.9%	-4.1%
LOGD_T2A	-0.1032	0.0322	-3.2075	0.00	-9.8%	-15.3%	-3.9%
LOGD_T2B	-0.1701	0.0346	-4.9135	0.00	-15.6%	-21.2%	-9.7%
R SQUARE ADJ		0.5001					

N.S.W (Unemployment) - Crashes 1, 2, 3, 4							
<i>ARIMA(0,0,1) (0,1,1)</i>							
MA1	-0.5150	0.0828	-6.2187	0.00			
SMA1	0.9991	30.0155	0.0333	0.97			
LOGURATE	-0.0007	0.0019	-0.3443	0.73			
LOGD_T1A	-0.0881	0.0339	-2.5994	0.01	-8.4%	-14.3%	-2.1%
LOGD_T1B	-0.1173	0.0400	-2.9361	0.00	-11.1%	-17.8%	-3.8%
LOGD_T2A	-0.1013	0.0325	-3.1159	0.00	-9.6%	-15.2%	-3.7%
LOGD_T2B	-0.1689	0.0347	-4.8706	0.00	-15.5%	-21.1%	-9.6%
R SQUARE ADJ		0.4986					

REGSP.XLS

	Coefficient	Standard Error	T-Ratio	Approx. Probability	Percent Change	95% Conf. Interval	
						Lower	Upper
Victoria (Travel) - Crashes 1, 2, 3							
REGRESSION							
INTERCEP	-5.9610	3.6422	-1.6370	0.11			
IDNUM	0.0039	0.0010	3.7000	0.00			
LOGTRAV	0.8324	0.2457	3.3870	0.00			
LOGD_T1A	-0.1897	0.0381	-4.9860	0.00	-17.3%	-23.2%	-10.9%
LOGD_T1B	-0.3035	0.0450	-6.7460	0.00	-26.2%	-32.4%	-19.4%
LOGD_T2A	-0.3372	0.0380	-8.8690	0.00	-28.6%	-33.7%	-23.1%
LOGD_T2B	-0.4875	0.0472	-10.3270	0.00	-38.6%	-44.0%	-32.6%
R SQUARE ADJ		0.8813					
Victoria (Unemployment) - Crashes 1, 2, 3							
REGRESSION							
INTERCEP	6.0465	0.2330	25.9550	0.00			
IDNUM	0.0063	0.0008	8.0670	0.00			
LOGURATE	-0.1416	0.0993	-1.4260	0.16			
LOGD_T1A	-0.2148	0.0397	-5.4030	0.00	-19.3%	-25.4%	-12.8%
LOGD_T1B	-0.3388	0.0457	-7.4200	0.00	-28.7%	-34.8%	-22.1%
LOGD_T2A	-0.3504	0.0572	-6.1280	0.00	-29.6%	-37.0%	-21.2%
LOGD_T2B	-0.4896	0.0924	-5.2990	0.00	-38.7%	-48.9%	-26.5%
R SQUARE ADJ		0.8691					

REGSP.XLS

	Coefficient	Standard Error	T-Ratio	Approx. Probability	Percent Change	95% Conf. Interval	
						Lower	Upper
N.S.W (Travel) - Crashes 1, 2, 3, 4							
REGRESSION							
INTERCEP	1.8504	4.0278	0.4590	0.65			
IDNUM	-0.0007	0.0008	-0.9290	0.36			
LOGTRAV	0.3446	0.2671	1.2900	0.20			
LOGD_T1A	-0.1130	0.0388	-2.9150	0.00	-10.7%	-17.2%	-3.6%
LOGD_T1B	-0.0857	0.0442	-1.9400	0.06	-8.2%	-15.8%	0.1%
LOGD_T2A	-0.1318	0.0316	-4.1750	0.00	-12.4%	-17.6%	-6.8%
LOGD_T2B	-0.2747	0.0320	-8.5940	0.00	-24.0%	-28.6%	-19.1%

R SQUARE ADJ 0.6670

N.S.W (Unemployment) - Crashes 1, 2, 3, 4

REGRESSION							
INTERCEP	7.1911	0.2124	33.8490	0.00			
IDNUM	0.0006	0.0007	0.8830	0.38			
LOGURATE	0.0679	0.0987	0.6880	0.49			
LOGD_T1A	-0.1132	0.0393	-2.8800	0.01	-10.7%	-17.3%	-3.6%
LOGD_T1B	-0.0881	0.0444	-1.9830	0.05	-8.4%	-16.1%	-0.1%
LOG_T2A	-0.1529	0.0330	-4.6390	0.00	-14.2%	-19.5%	-8.4%
LOG_T2B	-0.3203	0.0530	-6.0460	0.00	-27.4%	-34.6%	-19.5%

R SQUARE ADJ 0.6626

LOG.XLS

	Coefficient	Standard Error	T-Ratio	Approx. Probability	Percent Change	95% Conf. Interval	
						Lower	Upper
Victoria(Travel)							
LOGISTIC REGRESSION							
INTERCEP	16.1405	5.5898	2.887	0.00			
IDNUM	-0.0019	0.0016	-1.167	0.25			
LOGTRAV	-1.1125	0.3771	-2.95	0.00			
LOGD_T1A	-0.0296	0.0584	-0.507	0.61	-2.9%	-13.4%	8.9%
LOGD_T1B	-0.0176	0.0691	-0.254	0.80	-1.7%	-14.2%	12.5%
LOGD_T2A	-0.0605	0.0583	-1.037	0.30	-5.9%	-16.0%	5.5%
LOGD_T2B	-0.0272	0.0725	-0.376	0.71	-2.7%	-15.6%	12.2%

R SQUARE ADJ 0.7628

Victoria (Unemployment)

LOGISTIC REGRESSION							
INTERCEP	0.8845	0.3317	2.667	0.01			
IDNUM	-0.0026	0.0011	-2.388	0.02			
LOGURATE	0.5296	0.1413	3.747	0.00			
LOGD_T1A	0.0183	0.0566	0.324	0.75	1.9%	-8.8%	13.8%
LOGD_T1B	0.0045	0.0650	0.069	0.95	0.4%	-11.6%	14.1%
LOGD_T2A	-0.2080	0.0814	-2.555	0.01	-18.8%	-30.8%	-4.7%
LOGD_T2B	-0.3236	0.1315	-2.461	0.02	-27.6%	-44.1%	-6.4%

R SQUARE ADJ 0.7750

LOG.XLS

	Coefficient	Standard Error	T-Ratio	Approx. Probability	Percent Change	95%Conf. Interval Lower	Upper
N.S.W.(Travel)							
LOGISTIC REGRESSION							
INTERCEP	-7.8977	5.2959	-1.491	0.14			
IDNUM	-0.0041	0.0010	-4.148	0.00			
LOGTRAV	0.4628	0.3512	1.317	0.19			
LOGD_T1A	0.0710	0.0510	1.392	0.17	7.4%	-2.9%	18.6%
LOGD_T1B	0.1105	0.0581	1.902	0.06	11.7%	-0.3%	25.2%
LOGD_T2A	0.1295	0.0415	3.119	0.00	13.8%	4.9%	23.5%
LOGD_T2B	0.1923	0.0420	4.574	0.00	21.2%	11.6%	31.6%

R SQUARE ADJ 0.3223

N.S.W.(Unemployment)							
LOGISTIC REGRESSION							
INTERCEP	-1.0236	0.2800	-3.656	0.0004			
IDNUM	-0.0032	0.0009	-3.622	0.0005			
LOGURATE	-0.0483	0.1301	-0.371	0.7114			
LOGD_T1A	0.0630	0.0518	1.217	0.2268	6.5%	-3.8%	17.9%
LOGD_T1B	0.1041	0.0585	1.778	0.0788	11.0%	-1.1%	24.5%
LOGD_T2A	0.1195	0.0434	2.751	0.0072	12.7%	3.5%	22.7%
LOGD_T2B	0.1929	0.0698	2.763	0.0069	21.3%	5.8%	39.1%

R SQUARE ADJ 0.3103

MODELS OF CASUALTY CRASH FREQUENCY AND THEIR INJURY SEVERITY FOR REGIONS ACROSS VICTORIA & NSW AND NET EFFECTS FOR EACH COMPARISON

Modelling Output Glossary

- ARIMA (0,0,1) (0,1,1)

MA1	Non-seasonal moving average component
SMA1	Seasonal moving average component
LOGURATE	Natural Logarithm of the Unemployment Rate
LOGTRAV	Natural Logarithm of the Total Vehicle Kilometers Travelled
LOGD_T1a	Natural Logarithm of December 1989 to March 1990 Dummy Variable = e in time period and 1 elsewhere
LOGD_T1b	Natural Logarithm of April 1990 to June 1990 Dummy Variable = e in time period and 1 elsewhere
LOGD_T2a	Natural Logarithm of July 1990 to February 1991 Dummy Variable = e in time period and 1 elsewhere
LOGD_T2b	Natural Logarithm of March 1991 to Dec 1991 Dummy Variable = e in time period and 1

- Regression Equation

INTERCEP	Regression Equation Intercept
IDNUM	Linear Trend Component of Regression Equation = 1 in January '83, 2 in February '83 etc.

LOGURATE	}	
LOGTRAV	}	
LOGD_T1a	}	Same as with ARIMA modelling
LOGD_T1b	}	
LOGD_T2a	}	
LOGD_T2b	}	

LOGFEB	Natural Logarithm of February Dummy Variable = e in February and 1 in other months
--------	---

LOGMAR	Natural Logarithm of March Dummy Variable = e in March and 1 in other months
--------	---

LOGAPR	Natural Logarithm of April Dummy Variable = e in April and 1 in other months
--------	---

LOGMAY	Natural Logarithm of May Dummy Variable = e in May and 1 in other months
--------	---

LOGJUN	Natural Logarithm of June Dummy Variable
--------	--

= e in June and 1 in other months

LOGJUL Natural Logarithm of July Dummy Variable
= e in July and 1 in other months

LOGAUG Natural Logarithm of August Dummy Variable
= e in August and 1 in other months

LOGSEP Natural Logarithm of September Dummy Variable
= e in September and 1 in other months

LOGOCT Natural Logarithm of October Dummy Variable
= e in October and 1 in other months

LOGNOV Natural Logarithm of November Dummy Variable
= e in November and 1 in other months

LOGDEC Natural Logarithm of December Dummy Variable
= e in December and 1 in other months

e is the exponential constant and $\text{LOG}_e(e)=1$, $\text{LOG}_e(1)=0$

MODELS FOR PHASE 1: **GENERAL EFFECTS OF THE PROGRAM**

Models for Casualty Crashes

1. ARIMA time series model with covariates

CASUALTY CRASHES = a x MAb (non-seasonal moving average)
x SMAC (seasonal moving average)
x URATED (unemployment rate)
x T1A^e (dummy for Dec 1989 to March 1990)
x T1B^f (dummy for April 1990 to June 1990)
x T2A^g (dummy for July 1990 to Feb 1991)
x T2B^h (dummy for March 1991 to Dec 1991)

ARIMA.XLS

	Coefficient	Standard Error	T-Ratio	Approx. Probability	Percent Change	95% Conf. Interval Lower	Interval Upper
Victoria (Unemployment) - Crashes 1, 2, 3							
ARIMA(0,0,1 (0,1,1)'							
MA1	-0.1932	0.1019	-1.8960	0.06			
SMA1	0.7170	0.1281	5.5963	0.00			
LOGURATE	-0.0308	0.0020	-15.6100	0.00			
LOGD_T1A	-0.1709	0.0396	-4.3118	0.00	-15.7%	-22.0%	-8.9%
LOGD_T1B	-0.3416	0.0474	-7.2027	0.00	-28.9%	-35.2%	-22.0%
LOG_T2A	-0.3361	0.0342	-9.8252	0.00	-28.5%	-33.2%	-23.6%
LOG_T2B	-0.2454	0.0346	-7.0993	0.00	-21.8%	-26.9%	-16.3%
R SQUARE ADJ		0.7014					
N.S.W (Unemployment) - Crashes 1, 2, 3, 4							
ARIMA(0,0,1 (0,1,1)'							
MA1	-0.5150	0.0828	-6.2187	0.00			
SMA1	0.9991	30.0155	0.0333	0.97			
LOGURATE	-0.0007	0.0019	-0.3443	0.73			
LOGD_T1A	-0.0881	0.0339	-2.5994	0.01	-8.4%	-14.3%	-2.1%
LOGD_T1B	-0.1173	0.0400	-2.9361	0.00	-11.1%	-17.8%	-3.8%
LOG_T2A	-0.1013	0.0325	-3.1159	0.00	-9.6%	-15.2%	-3.7%
LOG_T2B	-0.1689	0.0347	-4.8706	0.00	-15.5%	-21.1%	-9.6%
R SQUARE ADJ		0.4986					
Net Percentage Changes							
LOGD_T1A					-8.0%	-16.9%	2.0%
LOGD_T1B					-20.1%	-29.2%	-9.8%
LOGD_T2A					-20.9%	-27.9%	-13.3%
LOGD_T2B					-7.4%	-15.9%	2.0%

ARIMA.XLS

	Coefficient	Standard Error	T-Ratio	Approx. Probability	Percent Change	95% Conf. Interval Lower	Interval Upper
Melbourne (Unemployment) - Crashes 1, 2, 3							
<i>ARIMA(0,0,1)(0,1,1)</i>							
MA1	-0.2166	0.1024	-2.1146	0.04			
SMA1	0.7692	0.1466	5.2469	0.00			
LOGURATE	-0.0294	0.0020	-14.5627	0.00			
LOGD_T1A	-0.1677	0.0423	-3.9651	0.00	-15.4%	-22.2%	-8.1%
LOGD_T1B	-0.3555	0.0512	-6.9469	0.00	-29.9%	-36.6%	-22.5%
LOGD_T2A	-0.3507	0.0375	-9.3563	0.00	-29.6%	-34.6%	-24.2%
LOGD_T2B	-0.2363	0.0384	-6.1537	0.00	-21.0%	-26.8%	-14.9%
R SQUARE ADJ	0.6932						
Sydney (Unemployment) - Crashes 1, 2, 3, 4							
<i>ARIMA(0,0,1)(0,1,1)</i>							
MA1	-0.5429	0.0815	-6.6643	0.00			
SMA1	0.9611	0.7124	1.3489	0.18			
LOGURATE	0.0013	0.0021	0.6331	0.53			
LOGD_T1A	-0.0966	0.0394	-2.4514	0.02	-9.2%	-16.0%	-1.9%
LOGD_T1B	-0.0878	0.0458	-1.9161	0.06	-8.4%	-16.3%	0.2%
LOGD_T2A	-0.1136	0.0378	-3.0032	0.00	-10.7%	-17.1%	-3.9%
LOGD_T2B	-0.1907	0.0403	-4.7297	0.00	-17.4%	-23.6%	-10.6%
R SQUARE ADJ	0.4794						
Net Percentage Changes							
LOGD_T1A					-7.0%	-16.8%	4.3%
LOGD_T1B					-23.5%	-33.1%	-12.5%
LOGD_T2A					-21.1%	-28.9%	-12.4%
LOGD_T2B					-4.5%	-14.3%	6.6%

ARIMA.XLS

	Coefficient	Standard Error	T-Ratio	Approx. Probability	Percent Change	95% Conf. Interval Lower	Interval Upper
Rural Victoria (Unemployment) - Crashes 1, 2, 3							
<i>ARIMA(0,0,1)(0,1,1)</i>							
MA1	-0.0779	0.0944	-0.8253				
SMA1	0.9403	0.4154	2.2636	0.41			
LOGURATE	-0.0342	0.0021	-16.0867	0.02			
LOGD_T1A	-0.1532	0.0438	-3.4920	0.00	-14.2%	-21.3%	-6.5%
LOG_T1B	-0.2991	0.0547	-5.4622	0.00	-25.9%	-33.4%	-17.5%
LOGD_T2A	-0.3091	0.0402	-7.6728	0.00	-26.6%	-32.2%	-20.6%
LOGD_T2B	-0.2772	0.0423	-6.5479	0.00	-24.2%	-30.2%	-17.7%
R SQUARE ADJ		0.5670					
Rural N.S.W (Unemployment) - Crashes 1, 2, 3, 4							
<i>ARIMA(0,0,1)(0,1,1)</i>							
MA1	-0.2087	0.0938	-2.2249	0.03			
SMA1	0.9928	3.6364	0.2730	0.79			
LOGURATE	-0.0041	0.0021	-1.9423	0.06			
LOGD_T1A	-0.1157	0.0372	-3.1130	0.00	-10.9%	-17.2%	-4.2%
LOG_T1B	-0.1202	0.0464	-2.5942	0.01	-11.3%	-19.0%	-2.9%
LOGD_T2A	-0.0920	0.0349	-2.6334	0.01	-8.8%	-14.8%	-2.3%
LOGD_T2B	-0.1501	0.0368	-4.0788	0.00	-13.9%	-19.9%	-7.5%
R SQUARE ADJ		0.4617					
Net Percentage Changes							
LOGD_T1A					-3.7%	-14.0%	7.8%
LOGD_T1B					-16.4%	-27.4%	-3.8%
LOGD_T2A					-19.5%	-27.5%	-10.7%
LOGD_T2B					-11.9%	-21.1%	-1.7%

ARIMA.XLS

	Coefficient	Standard Error	T-Ratio	Approx. Probability	Percent Change	95% Conf. Interval Lower	Interval Upper
Melbourne Arterials (Unemployment) - Crashes, 1, 2, 3							
<i>ARIMA(0,0,1)(0,1,1)</i>							
MA1	-0.2059	0.1020	-2.0187	0.05			
SMA1	0.7663	0.1478	5.1835	0.00			
LOGURATE	-0.0323	0.0022	-14.9136	0.00			
LOGD_T1A	-0.1782	0.0454	-3.9274	0.00	-16.3%	-23.4%	-8.5%
LOGD_T1B	-0.3627	0.0549	-6.6073	0.00	-30.4%	-37.5%	-22.5%
LOGD_T2A	-0.3791	0.0401	-9.4488	0.00	-31.6%	-36.7%	-26.0%
LOGD_T2B	-0.2545	0.0411	-6.1988	0.00	-22.5%	-28.5%	-16.0%
R SQUARE ADJ	0.6355						
Sydney Arterials (Unemployment) - Crashes 1, 2, 3, 4							
<i>ARIMA(0,0,1)(0,1,1)</i>							
MA1	0.0710	0.1174	0.6050	0.55			
SMA1	0.8789	0.5832	1.5069	0.14			
LOGURATE	0.0218	0.0030	7.1404	0.00			
LOGD_T1A	-0.0209	0.0343	-0.6079	0.55	-2.1%	-8.4%	4.7%
LOGD_T1B	0.0494	0.0424	1.1646	0.25	5.1%	-3.3%	14.2%
LOGD_T2A	0.0125	0.0310	0.4025	0.69	1.3%	-4.7%	7.6%
LOGD_T2B	-0.1428	0.0292	-4.8839	0.00	-13.3%	-18.1%	-8.2%
R SQUARE ADJ	0.4242						
Net Percentage Changes							
LOGD_T1A					-14.6%	-23.6%	-4.5%
LOGD_T1B					-33.8%	-42.2%	-24.1%
LOGD_T2A					-32.4%	-38.8%	-25.3%
LOGD_T2B					-10.6%	-19.0%	-1.3%

ARIMA.XLS

	Standard Coefficient	Error	T-Ratio	Approx. Probability	Percent Change	95% Conf. Interval Lower	Upper
Melbourne Arterials 60 Km/h Speed Zone (Unemployment) - Crashes 1, 2, 3							
ARIMA(0,0,1)(0,1,1)							
MA1	-0.2269	0.0970	-2.3381	0.02			
SMA1	0.8448	0.1953	4.3250	0.00			
LOGURATE	-0.0283	0.0021	-13.3958	0.00			
LOGD_T1A	-0.1709	0.0450	-3.7972	0.00	-15.7%	-22.8%	-7.9%
LOGD_T1B	-0.3491	0.0553	-6.3175	0.00	-29.5%	-36.7%	-21.4%
LOGD_T2A	-0.3650	0.0411	-8.8745	0.00	-30.6%	-36.0%	-24.8%
LOGD_T2B	-0.2425	0.0428	-5.6620	0.00	-21.5%	-27.8%	-14.7%
R SQUARE ADJ	0.5961						
Sydney Arterials 60 Km/h Speed Zone (Unemployment) - Crashes 1, 2, 3, 4							
ARIMA(0,0,1)(0,1,1) ok							
MA1	-0.0037	0.1167	-0.0314	0.98			
SMA1	0.9128	0.7801	1.1701	0.25			
LOGURATE	0.0232	0.0033	7.0595	0.00			
LOGD_T1A	-0.0230	0.0363	-0.6336	0.53	-2.3%	-9.0%	4.9%
LOGD_T1B	0.0295	0.0448	0.6587	0.51	3.0%	-5.7%	12.5%
LOGD_T2A	0.0003	0.0333	0.0087	0.99	0.0%	-6.3%	6.8%
LOGD_T2B	-0.1846	0.0315	-5.8673	0.00	-16.9%	-21.8%	-11.6%
R SQUARE ADJ	0.5128						
Net Percentage Changes							
LOGD_T1A					-13.8%	-23.0%	-3.4%
LOGD_T1B					-31.5%	-40.4%	-21.3%
LOGD_T2A					-30.6%	-37.4%	-23.0%
LOGD_T2B					-5.6%	-15.0%	4.7%

ARIMA.XLS

	Standard Coefficient	Error	T-Ratio	Approx. Probability	Percent Change	95% Conf. Interval Lower	Upper
Melbourne Arterials (Unemployment) - Crashes, 1, 2, 3							
ARIMA(0,0,1)(0,1,1)							
MA1	-0.2059	0.1020	-2.0187	0.05			
SMA1	0.7663	0.1478	5.1835	0.00			
LOGURATE	-0.0323	0.0022	-14.9136	0.00			
LOGD_T1A	-0.1782	0.0454	-3.9274	0.00	-16.3%	-23.4%	-8.5%
LOGD_T1B	-0.3627	0.0549	-6.6073	0.00	-30.4%	-37.5%	-22.5%
LOGD_T2A	-0.3791	0.0401	-9.4488	0.00	-31.6%	-36.7%	-26.0%
LOGD_T2B	-0.2545	0.0411	-6.1988	0.00	-22.5%	-28.5%	-16.0%
R SQUARE ADJ	0.6355						
Melbourne Residential (Unemployment) - Crashes 1, 2, 3							
ARIMA(0,0,1)(0,1,1)							
MA1	-0.1200	0.1038	-1.1558	0.25			
SMA1	0.7626	0.1430	5.3338	0.00			
LOGURATE	-0.0221	0.0026	-8.4297	0.00			
LOGD_T1A	-0.1616	0.0563	-2.8732	0.01	-14.9%	-23.8%	-5.0%
LOGD_T1B	-0.3301	0.0683	-4.8312	0.00	-28.1%	-37.1%	-17.8%
LOGD_T2A	-0.2832	0.0493	-5.7429	0.00	-24.7%	-31.6%	-17.0%
LOGD_T2B	-0.2559	0.0503	-5.0913	0.00	-22.6%	-29.8%	-14.6%
R SQUARE ADJ	0.4940						
Net Percentage Changes							
LOGD_T1A					-1.6%	-14.6%	13.3%
LOGD_T1B					-3.2%	-18.5%	14.9%
LOGD_T2A					-9.1%	-19.8%	2.9%
LOGD_T2B					-0.1%	-11.8%	13.7%

ARIMA.XLS

	Coefficient	Standard Error	T-Ratio	Approx. Probability	Percent Change	95% Conf. Interval Lower	95% Conf. Interval Upper
Rural Victoria 100 Km Speed Zones (Unemployment) - Crashes 1, 2, 3							
ARIMA(0,0,1)(0,1,1)							
MA1	-0.0435	0.0935	-0.4650	0.64			
SMA1	0.9583	0.6401	1.4970	0.14			
LOGURATE	-0.0275	0.0028	-9.8816	0.00			
LOGD_T1A	-0.0839	0.0580	-1.4464	0.15	-8.0%	-17.9%	3.0%
LOGD_T1B	-0.1944	0.0725	-2.6811	0.01	-17.7%	-28.6%	-5.1%
LOG_T2A	-0.2548	0.0531	-4.7942	0.00	-22.5%	-30.2%	-14.0%
LOG_T2B	-0.1835	0.0559	-3.2843	0.00	-16.8%	-25.4%	-7.1%
R SQUARE ADJ		0.3749					
Rural N.S.W. 100 Km Speed Zones (Unemployment) - Crashes 1, 2, 3, 4							
ARIMA(0,0,1)(0,1,1)							
MA1	-0.1090	0.1009	-1.0803	0.28			
SMA1	0.7661	0.1228	6.2399	0.00			
LOGURATE	0.0024	0.0032	0.7384	0.46			
LOGD_T1A	-0.0859	0.0556	-1.5458	0.13	-8.2%	-17.7%	2.3%
LOGD_T1B	-0.1098	0.0675	-1.6262	0.11	-10.4%	-21.5%	2.3%
LOGD_T2A	-0.1027	0.0488	-2.1043	0.04	-9.8%	-18.0%	-0.7%
LOGD_T2B	-0.0876	0.0497	-1.7624	0.08	-8.4%	-16.9%	1.0%
R SQUARE ADJ		0.2321					
Net Percentage Changes							
LOGD_T1A					0.2%	-14.4%	17.3%
LOGD_T1B					-8.1%	-24.3%	11.6%
LOGD_T2A					-14.1%	-25.4%	-1.1%
LOGD_T2B					-9.1%	-21.5%	5.2%

ARIMA.XLS

	Coefficient	Standard Error	T-Ratio	Approx. Probability	Percent Change	95% Conf. Interval Lower	95% Conf. Interval Upper
Rural Victoria 60 Km Speed Zones (Unemployment) - Crashes 1, 2, 3							
ARIMA(0,0,1)(0,1,1)							
MA1	-0.0135	0.0946	-0.1430	0.89			
SMA1	0.9328	0.4384	2.1279	0.04			
LOGURATE	-0.0367	0.0025	-14.4234	0.00			
LOGD_T1A	-0.1905	0.0532	-3.5784	0.00	-17.3%	-25.5%	-8.3%
LOGD_T1B	-0.3259	0.0665	-4.8985	0.00	-27.8%	-36.6%	-17.8%
LOG_T2A	-0.3445	0.0485	-7.1038	0.00	-29.1%	-35.6%	-22.1%
LOG_T2B	-0.3818	0.0509	-7.5063	0.00	-31.7%	-38.2%	-24.6%
R SQUARE ADJ	0.5374						
Rural N.S.W. 60 Km Speed Zones (Unemployment) - Crashes 1, 2, 3, 4							
ARIMA(0,0,1)(0,1,1)							
MA1	-0.0462	0.0951	-0.4860	0.63			
SMA1	0.9793	1.3665	0.7166	0.48			
LOGURATE	-0.0024	0.0021	-1.1400	0.26			
LOGD_T1A	-0.1565	0.0379	-4.1282	0.00	-14.5%	-20.6%	-7.9%
LOGD_T1B	-0.0896	0.0475	-1.8847	0.06	-8.6%	-16.7%	0.4%
LOGD_T2A	-0.0795	0.0351	-2.2678	0.03	-7.6%	-13.8%	-1.1%
LOGD_T2B	-0.1863	0.0368	-5.0679	0.00	-17.0%	-22.8%	-10.8%
R SQUARE ADJ	0.4372						
Net Percentage Changes							
LOGD_T1A					-3.3%	-15.0%	9.9%
LOGD_T1B					-21.0%	-32.7%	-7.3%
LOGD_T2A					-23.3%	-31.8%	-13.7%
LOGD_T2B					-17.8%	-27.3%	-7.0%

MODELS FOR PHASE 1: **GENERAL EFFECTS OF THE PROGRAM**

Models for Casualty Crashes (cont.)

2. Multiple regression model

$$\text{CASUALTY CRASHES} = a + x(\text{TREND})^b + (1 \text{ in Jan 1983, } -2 \text{ in Feb 1983, } \dots)$$

$x \text{ URATE}^c$
 $x \text{ T1A}^d$
 $x \text{ T1B}^e$
 $x \text{ T2A}^f$
 $x \text{ T2B}^g$
 $x \text{ FEB}^h$ (dummy for February months)
 $x \text{ DEC}^i$ (dummy for December months)

REG.XLS

	Coefficient	Standard Error	T-Ratio	Approx. Probability	Percent Change	95% Conf. Interval	
						Lower	Upper
Victoria (Unemployment) - Crashes 1, 2, 3							
REGRESSION							
INTERCEP	6.0465	0.2330	25.9550	0.00			
IDNUM	0.0063	0.0008	8.0670	0.00			
LOGURATE	-0.1416	0.0993	-1.4260	0.16			
LOGD_T1A	-0.2148	0.0397	-5.4030	0.00	-19.3%	-25.4%	-12.8%
LOGD_T1B	-0.3388	0.0457	-7.4200	0.00	-28.7%	-34.8%	-22.1%
LOGD_T2A	-0.3504	0.0572	-6.1280	0.00	-29.6%	-37.0%	-21.2%
LOGD_T2B	-0.4896	0.0924	-5.2990	0.00	-38.7%	-48.9%	-26.5%
R SQUARE ADJ 0.8691							
N.S.W (Unemployment) - Crashes 1, 2, 3, 4							
REGRESSION							
INTERCEP	7.1911	0.2124	33.8490	0.00			
IDNUM	0.0006	0.0007	0.8830	0.38			
LOGURATE	0.0679	0.0987	0.6880	0.49			
LOGD_T1A	-0.1132	0.0393	-2.8800	0.01	-10.7%	-17.3%	-3.6%
LOGD_T1B	-0.0881	0.0444	-1.9830	0.05	-8.4%	-16.1%	-0.1%
LOGD_T2A	-0.1529	0.0330	-4.6390	0.00	-14.2%	-19.5%	-8.4%
LOGD_T2B	-0.3203	0.0530	-6.0460	0.00	-27.4%	-34.6%	-19.5%
R SQUARE ADJ 0.6626							
Net Percentage Changes							
LOGD_T1A					-9.7%	-19.0%	0.8%
LOGD_T1B					-22.2%	-31.3%	-11.8%
LOGD_T2A					-17.9%	-27.9%	-6.6%
LOGD_T2B					-15.6%	-31.5%	4.0%

REG.XLS

	Standard		Approx.		Percent	95% Conf. Interval	
	Coefficient	Error	T-Ratio	Probability	Change	Lower	Upper
Melbourne (Unemployment) - Crashes 1, 2, 3							
REGRESSION							
INTERCEP	5.4319	0.2157	25.1880	0.00			
IDNUM	0.0048	0.0009	5.2530	0.00			
LOGURATE	-0.2500	0.0929	-2.6910	0.01			
LOGD_T1A	-0.1661	0.0436	-3.8080	0.00	-15.3%	-22.2%	-7.7%
LOGD_T1B	-0.3088	0.0526	-5.8690	0.00	-26.6%	-33.8%	-18.6%
LOGD_T2A	-0.2774	0.0679	-4.0870	0.00	-24.2%	-33.7%	-13.4%
LOGD_T2B	-0.3519	0.1031	-3.4130	0.00	-29.7%	-42.5%	-13.9%
R SQUARE ADJ	0.8610						
Sydney (Unemployment) - Crashes 1, 2, 3, 4							
REGRESSION							
INTERCEP	6.4336	0.2173	29.6040	0.00			
IDNUM	-0.0002	0.0008	-0.2460	0.81			
LOGURATE	0.0193	0.0974	0.1980	0.84			
LOGD_T1A	-0.0973	0.0471	-2.0670	0.04	-9.3%	-17.3%	-0.5%
LOGD_T1B	-0.0826	0.0534	-1.5480	0.13	-7.9%	-17.1%	2.2%
LOGD_T2A	-0.1413	0.0420	-3.3680	0.00	-13.2%	-20.0%	-5.7%
LOGD_T2B	-0.3183	0.0671	-4.7450	0.00	-27.3%	-36.2%	-17.0%
R SQUARE ADJ	0.6828						
Net Percentage Changes							
LOGD_T1A					-6.7%	-17.7%	5.9%
LOGD_T1B					-20.2%	-31.1%	-7.6%
LOGD_T2A					-12.7%	-25.4%	2.1%
LOGD_T2B					-3.3%	-24.0%	23.1%

REG.XLS

	Coefficient	Standard Error	T-Ratio	Approx. Probability	Percent Change	95% Conf. Interval	
						Lower	Upper
Rural Victoria (Unemployment) - Crashes 1, 2, 3							
REGRESSION							
INTERCEP	5.1627	0.2081	24.8080	0.00			
IDNUM	0.0077	0.0005	16.6190	0.00			
LOGURATE	-0.0133	0.0858	-0.1560	0.88			
LOGD_T1A	-0.2739	0.0570	-4.8050	0.00	-24.0%	-32.0%	-15.0%
LOGD_T1B	-0.3213	0.0599	-5.3630	0.00	-27.5%	-35.5%	-18.4%
LOGD_T2A	-0.3800	0.0413	-9.2080	0.00	-31.6%	-36.9%	-25.9%
LOGD_T2B	-0.6071	0.0575	-10.5620	0.00	-45.5%	-51.3%	-39.0%
R SQUARE ADJ 0.8198							
Rural N.S.W (Unemployment) - Crashes 1, 2, 3, 4							
REGRESSION							
INTERCEP	6.3954	0.2264	28.2530	0.00			
IDNUM	0.0010	0.0006	1.8100	0.07			
LOGURATE	0.0439	0.1109	0.3950	0.69			
LOGD_T1A	-0.1365	0.0442	-3.0890	0.00	-12.8%	-20.0%	-4.9%
LOG_T1B	-0.0952	0.0504	-1.8890	0.06	-9.1%	-17.6%	0.4%
LOGD_T2A	-0.1514	0.0343	-4.4100	0.00	-14.1%	-19.6%	-8.1%
LOGD_T2B	-0.2829	0.0444	-6.3670	0.00	-24.6%	-30.9%	-17.8%
R SQUARE ADJ 0.5315							
Net Percentage Changes							
LOGD_T1A					-12.8%	-24.3%	0.4%
LOGD_T1B					-20.2%	-31.6%	-7.0%
LOGD_T2A					-20.4%	-28.4%	-11.6%
LOGD_T2B					-27.7%	-37.3%	-16.6%

REG.XLS

	Standard	Approx.	Percent	95% Conf. Interval			
Coefficient	Error	T-Ratio	Probability	Change	Lower	Upper	
Melbourne Arterials (Unemployment) - Crashes 1, 2, 3							
REGRESSION							
INTERCEP	5.0273	0.2347	21.4210	0.00			
IDNUM	0.0050	0.0010	5.0480	0.00			
LOGURATE	-0.2905	0.1011	-2.8730	0.01			
LOGD_T1A	-0.1425	0.0475	-3.0020	0.00	-13.3%	-21.0%	-4.8%
LOGD_T1B	-0.3010	0.0572	-5.2570	0.00	-26.0%	-33.8%	-17.2%
LOGD_T2A	-0.2824	0.0739	-3.8230	0.00	-24.6%	-34.8%	-12.9%
LOGD_T2B	-0.3433	0.1121	-3.0600	0.00	-29.1%	-43.1%	-11.6%
R SQUARE ADJ 0.8570							
Sydney Arterials (Unemployment) - Crashes 1, 2, 3, 4							
REGRESSION							
INTERCEP	6.5265	0.2173	30.0360	0.00			
IDNUM	-0.0041	0.0012	-3.5290	0.00			
LOGURATE	0.1170	0.0999	1.1720	0.25			
LOGD_T1A	0.0013	0.0443	0.0290	0.98	0.1%	-8.2%	9.2%
LOGD_T1B	0.0192	0.0506	0.3800	0.71	1.9%	-7.7%	12.6%
LOGD_T2A	-0.0179	0.0444	-0.4040	0.69	-1.8%	-10.0%	7.1%
LOGD_T2B	-0.1948	0.0747	-2.6070	0.01	-17.7%	-28.9%	-4.7%
R SQUARE ADJ 0.8177							
Net Percentage Changes							
LOGD_T1A					-13.4%	-23.8%	-1.6%
LOGD_T1B					-27.4%	-37.5%	-15.7%
LOGD_T2A					-23.2%	-35.2%	-9.1%
LOGD_T2B					-13.8%	-33.8%	12.3%

REG.XLS

	Standard		Approx.	Percent	95% Conf. Interval	
Coefficient	Error	T-Ratio	Probability	Change	Lower	Upper
Melbourne 60 Kmh Arterials (Unemployment) - Crashes 1, 2, 3						
REGRESSION						
INTERCEP	5.0129	0.2421	20.7060	0.00		
IDNUM	0.0048	0.0010	4.6500	0.00		
LOGURATE	-0.2227	0.1043	-2.1360	0.04		
LOGD_T1A	-0.1448	0.0490	-2.9560	0.00	-13.5%	-21.4%
LOGD_T1B	-0.3270	0.0591	-5.5380	0.00	-27.9%	-35.8%
LOGD_T2A	-0.3143	0.0762	-4.1250	0.00	-27.0%	-37.1%
LOGD_T2B	-0.3904	0.1157	-3.3730	0.00	-32.3%	-46.1%
R SQUARE ADJ	0.8340					
Sydney Arterials 60 Kmh Speed Zone (Unemployment) - Crashes, 1, 2, 3, 4						
REGRESSION						
INTERCEP	6.4178	0.2188	29.3290	0.00		
IDNUM	-0.0044	0.0012	-3.8010	0.00		
LOGURATE	0.1146	0.1006	1.1390	0.26		
LOGD_T1A	-0.0091	0.0447	-0.2030	0.84	-0.9%	-9.2%
LOG_T1B	-0.0065	0.0510	-0.1270	0.90	-0.6%	-10.1%
LOGD_T2A	-0.0248	0.0447	-0.5560	0.58	-2.5%	-10.6%
LOGD_T2B	-0.2488	0.0752	-3.3080	0.00	-22.0%	-32.7%
R SQUARE ADJ	0.8532					
Net Percentage Changes						
LOGD_T1A					-12.7%	-23.3%
LOGD_T1B					-27.4%	-37.7%
LOGD_T2A					-25.1%	-37.0%
LOGD_T2B					-13.2%	-33.8%

REG.XLS

	Coefficient	Standard Error	T-Ratio	Approx. Probability	Percent Change	95% Conf. Interval	
						Lower	Upper
Melbourne Arterials (Unemployment) - Crashes 1, 2, 3							
REGRESSION							
INTERCEP	5.0273	0.2347	21.4210	0.00			
IDNUM	0.0050	0.0010	5.0480	0.00			
LOGURATE	-0.2905	0.1011	-2.8730	0.01			
LOGD_T1A	-0.1425	0.0475	-3.0020	0.00	-13.3%	-21.0%	-4.8%
LOGD_T1B	-0.3010	0.0572	-5.2570	0.00	-26.0%	-33.8%	-17.2%
LOGD_T2A	-0.2824	0.0739	-3.8230	0.00	-24.6%	-34.8%	-12.9%
LOGD_T2B	-0.3433	0.1121	-3.0600	0.00	-29.1%	-43.1%	-11.6%
R SQUARE ADJ		0.8570					
Melbourne Residential (Unemployment) - Crashes 1, 2, 3							
REGRESSION							
INTERCEP	4.2080	0.3012	13.9690	0.00			
IDNUM	0.0047	0.0013	3.6620	0.00			
LOGURATE	-0.0975	0.1298	-0.7510	0.45			
LOGD_T1A	-0.2686	0.0609	-4.4090	0.00	-23.6%	-32.2%	-13.9%
LOGD_T1B	-0.3217	0.0735	-4.3780	0.00	-27.5%	-37.2%	-16.3%
LOGD_T2A	-0.2995	0.0948	-3.1590	0.00	-25.9%	-38.4%	-10.7%
LOGD_T2B	-0.4934	0.1440	-3.4270	0.00	-38.9%	-54.0%	-19.0%
R SQUARE ADJ		0.7350					
Net Percentage Changes							
LOGD_T1A					13.4%	-2.5%	32.0%
LOGD_T1B					2.1%	-14.9%	22.5%
LOGD_T2A					1.7%	-19.6%	28.8%
LOGD_T2B					16.2%	-18.8%	66.2%

REG.XLS

	Coefficient	Standard Error	T-Ratio	Approx. Probability	Percent Change	95% Conf. Interval	
						Lower	Upper
Rural Victoria 100 Km/h Speed Zones - Crashes 1, 2, 3							
REGRESSION							
INTERCEP	4.0661	0.2923	13.91	0.00			
IDNUM	0.0057	0.0007	8.75	0.00			
LOGURATE	-0.2117	0.1205	-1.757	0.08			
LOGD_T1A	-0.2965	0.0801	-3.704	0.00	-25.7%	-36.5%	-13.0%
LOGD_T1B	-0.3043	0.0842	-3.616	0.00	-26.2%	-37.5%	-13.0%
LOGD_T2A	-0.2378	0.0580	-4.102	0.00	-21.2%	-29.6%	-11.7%
LOGD_T2B	-0.3343	0.0807	-4.14	0.00	-28.4%	-38.9%	-16.1%
R SQUARE ADJ 0.7456							
Rural N.S.W 100 Km/h Speed Zones - Crashes 1, 2, 3, 4							
REGRESSION							
INTERCEP	5.3097	0.3218	16.499	0.00			
IDNUM	-0.0005	0.0008	-0.587	0.56			
LOGURATE	-0.0480	0.1577	-0.304	0.76			
LOGD_T1A	-0.1353	0.0628	-2.154	0.03	-12.7%	-22.8%	-1.2%
LOGD_T1B	-0.1816	0.0717	-2.535	0.01	-16.6%	-27.5%	-4.0%
LOGD_T2A	-0.1367	0.0488	-2.8	0.01	-12.8%	-20.7%	-4.0%
LOGD_T2B	-0.2070	0.0632	-3.276	0.00	-18.7%	-28.2%	-8.0%
R SQUARE ADJ 0.6409							
Net Percentage Changes							
LOGD_T1A					-14.9%	-30.3%	3.0%
LOGD_T1B					-11.5%	-28.8%	9.9%
LOGD_T2A					-9.6%	-22.1%	4.9%
LOGD_T2B					-11.9%	-28.0%	7.6%

	Standard		T-Ratio	Approx. Probability	Percent Change		
	Coefficient	Error			Lower	Upper	
Rural Victoria 60 Km Speed Zones - Crashes 1, 2, 3							
REGRESSION							
INTERCEP	4.6148	0.2662	17.337	0.00			
IDNUM	0.0086	0.0006	14.355	0.00			
LOGURATE	0.1285	0.1097	1.171	0.24			
LOGD_T1A	-0.1916	0.0729	-2.629	0.01	-17.4%	-28.4%	
LOGD_T1B	-0.3031	0.0766	-3.955	0.00	-26.1%	-36.4%	
LOGD_T2A	-0.4653	0.0528	-8.813	0.00	-37.2%	-43.4%	
LOGD_T2B	-0.8133	0.0735	-11.063	0.00	-55.7%	-61.6%	
R SQUARE ADJ	0.7709						
Rural N.S.W 60 Km Speed Zones - Crashes 1, 2, 3, 4							
REGRESSION							
INTERCEP	5.9908	0.2448	24.469	0.00			
IDNUM	0.0012	0.0006	1.984	0.05			
LOGURATE	0.1695	0.1200	1.413	0.16			
LOGD_T1A	-0.1651	0.0478	-3.454	0.00	-15.2%	-22.8%	
LOGD_T1B	-0.0419	0.0545	-0.769	0.44	-4.1%	-13.8%	
LOGD_T2A	-0.1677	0.0371	-4.515	0.00	-15.4%	-21.4%	
LOGD_T2B	-0.3446	0.0481	-7.169	0.00	-29.2%	-35.5%	
R SQUARE ADJ	0.5853						
Net Percentage Changes							
LOGD_T1A					-2.6%	-17.9%	
LOGD_T1B					-23.0%	-35.9%	
LOGD_T2A					-25.7%	-34.6%	
LOGD_T2B					-37.4%	-47.3%	

MODELS FOR PHASE 1: GENERAL EFFECTS OF THE PROGRAM

Model for Injury Severity of Casualty Crashes

(Logistic multiple regression model)

$$\text{Severity Ratio} = \frac{\text{Fatal plus Serious Injury Crashes}}{\text{Minor Injury Crashes}}$$

$$\text{SEVERITY RATIO} = a \times (e^{\text{TREND}})^b \times \text{URATE}^c \times \text{T1Ad} \times \text{T1Be} \times \text{T2Af} \times \text{T2Bg} \times \text{FEBh} \times \text{DECi}$$

$$\text{Note: Injury Severity} = \frac{\text{Fatal plus Serious Injury Crashes}}{\text{All Injury Crashes}} = \frac{\text{SEVERITY RATIO}}{1 + \text{SEVERITY RATIO}}$$

REGLOG.XLS

	Coefficient	Standard Error	T-Ratio	Approx. Probability	Percent Change	95% Conf. Interval	
						Lower	Upper
'pvic.reg							
Victoria Total (Unemployment & Trend)							
REGRESSION							
INTERCEP	0.8845	0.3317	2.667	0.01			
IDNUM	-0.0026	0.0011	-2.388	0.02			
LOGURATE	0.5296	0.1413	3.747	0.00			
LOGD_T1A	0.0183	0.0566	0.324	0.75	1.9%	-8.8%	13.8%
LOGD_T1B	0.0045	0.0650	0.069	0.95	0.4%	-11.6%	14.1%
LOGD_T2A	-0.2080	0.0814	-2.555	0.01	-18.8%	-30.8%	-4.7%
LOGD_T2B	-0.3236	0.1315	-2.461	0.02	-27.6%	-44.1%	-6.4%
'R SQUARE ADJ 0.7750							
'PNSW.REG							
N.S.W. Total (Unemployment & Trend)							
REGRESSION							
INTERCEP	-1.0236	0.2800	-3.656	0.00			
IDNUM	-0.0032	0.0009	-3.622	0.00			
LOGURATE	-0.0483	0.1301	-0.371	0.71			
LOGD_T1A	0.0630	0.0518	1.217	0.23	6.5%	-3.8%	17.9%
LOGD_T1B	0.1041	0.0585	1.778	0.08	11.0%	-1.1%	24.5%
LOGD_T2A	0.1195	0.0434	2.751	0.01	12.7%	3.5%	22.7%
LOGD_T2B	0.1929	0.0698	2.763	0.01	21.3%	5.8%	39.1%
'R SQUARE ADJ 0.3103							
Net Percentage Changes							
LOGD_T1A					-4.4%	-17.7%	11.1%
LOGD_T1B					-9.5%	-23.7%	7.5%
LOGD_T2A					-27.9%	-39.9%	-13.6%
LOGD_T2B					-40.3%	-55.4%	-20.1%

REGLOG.XLS

	Coefficient	Standard Error	T-Ratio	Approx. Probability	Percent Change	95% Conf. Interval	
						Lower	Upper
Melbourne Total (Unemployment) - Crash Severity							
REGRESSION ok							
INTERCEP	0.9019	0.1541	5.8510	0.00			
LOGURATE	0.6185	0.0561	11.0330	0.00			
LOGD_T1A	-0.0919	0.0667	-1.3790	0.17	-8.8%	-20.0%	3.9%
LOGD_T1B	-0.1984	0.0749	-2.6480	0.01	-18.0%	-29.2%	-5.0%
LOGD_T2A	-0.3744	0.0463	-8.0930	0.00	-31.2%	-37.2%	-24.7%
LOGD_T2B	-0.5640	0.0492	-11.4680	0.00	-43.1%	-48.3%	-37.3%
R SQUARE ADJ 0.6733							
Sydney Total (Unemployment) - Crash Severity							
REGRESSION ok							
INTERCEP	-0.9749	0.1466	-6.6500	0.00			
LOGURATE	0.1616	0.0570	2.8340	0.01			
LOGD_T1A	0.1106	0.0600	1.8440	0.07	11.7%	-0.7%	25.6%
LOGD_T1B	0.1442	0.0671	2.1490	0.03	15.5%	1.3%	31.8%
LOGD_T2A	0.0592	0.0406	1.4580	0.15	6.1%	-2.0%	14.9%
LOGD_T2B	0.0267	0.0360	0.7420	0.46	2.7%	-4.3%	10.2%
R SQUARE ADJ 0.0370							
Net Percentage Changes							
LOGD_T1A					-18.3%	-31.5%	-2.6%
LOGD_T1B					-29.0%	-41.7%	-13.5%
LOGD_T2A					-35.2%	-42.6%	-26.9%
LOGD_T2B					-44.6%	-50.8%	-37.6%

REGLOG.XLS

	Coefficient	Standard Error	T-Ratio	Approx. Probability	Percent Change	95% Conf. Interval	
						Lower	Upper
Rural Victoria (Unemployment & Trend) - Crash Severity							
REGRESSION							
INTERCEP	0.0613	0.3557	0.1720	0.86			
IDNUM	-0.0086	0.0008	-10.7630	0.00			
LOGURATE	0.0151	0.1466	0.1030	0.92			
LOGD_T1A	0.1134	0.0974	1.1640	0.25	12.0%	-7.5%	35.6%
LOGD_T1B	0.3058	0.1024	2.9860	0.00	35.8%	11.1%	65.9%
LOGD_T2A	0.1160	0.0705	1.6450	0.10	12.3%	-2.2%	29.0%
LOGD_T2B	0.2775	0.0982	2.8260	0.01	32.0%	8.9%	60.0%
R SQUARE ADJ 0.6735							
Rural N.S.W (Unemployment & Trend)							
REGRESSION							
INTERCEP	-0.9080	0.3345	-2.7150	0.01			
IDNUM	-0.0053	0.0008	-6.3140	0.00			
LOGURATE	-0.1786	0.1639	-1.0900	0.28			
LOGD_T1A	0.0254	0.0653	0.3890	0.70	2.6%	-9.8%	16.6%
LOGD_T1B	0.0378	0.0745	0.5070	0.61	3.8%	-10.3%	20.2%
LOGD_T2A	0.1242	0.0507	2.4480	0.02	13.2%	2.5%	25.1%
LOGD_T2B	0.2459	0.0657	3.7450	0.00	27.9%	12.4%	45.4%
R SQUARE ADJ 0.4834							
Net Percentage Changes							
LOGD_T1A					9.2%	-13.2%	37.4%
LOGD_T1B					30.7%	2.0%	67.6%
LOGD_T2A					-0.8%	-16.3%	17.6%
LOGD_T2B					3.2%	-18.1%	30.1%

REGLOG.XLS

	Coefficient	Standard Error	T-Ratio	Approx. Probability	Percent Change	95% Conf. Interval	
						Lower	Upper
'pmlba_io.reg							
Melbourne Arterials (Unemployment)							
REGRESSION							
INTERCEP	0.8561	0.1628	5.258	0.00			
LOGURATE	0.6171	0.0592	10.422	0.00			
LOGD_T1A	-0.1239	0.0704	-1.759	0.08	-11.7%	-23.0%	1.4%
LOGD_T1B	-0.2625	0.0792	-3.316	0.00	-23.1%	-34.1%	-10.2%
LOGD_T2A	-0.4268	0.0489	-8.735	0.00	-34.7%	-40.7%	-28.2%
LOGD_T2B	-0.5920	0.0519	-11.396	0.00	-44.7%	-50.0%	-38.7%
'R SQUARE ADJ 0.6823							
'psyda_io.reg							
Sydney Arterials (Unemployment)							
REGRESSION							
INTERCEP	-1.3937	0.3017	-4.619	0.00			
LOGURATE	0.0140	0.1142	0.123	0.90			
LOGD_T1A	0.1253	0.0745	1.68	0.10	13.3%	-2.1%	31.2%
LOGD_T1B	0.1983	0.0808	2.453	0.02	21.9%	4.1%	42.9%
LOGD_T2A	0.0368	0.0483	0.763	0.45	3.8%	-5.6%	14.0%
LOGD_T2B	0.0599	0.0515	1.162	0.25	6.2%	-4.0%	17.4%
'R SQUARE ADJ 0.0652							
Net Percentage Changes							
LOGD_T1A					-22.1%	-36.3%	-4.7%
LOGD_T1B					-36.9%	-49.5%	-21.3%
LOGD_T2A					-37.1%	-45.0%	-28.0%
LOGD_T2B					-47.9%	-54.9%	-39.9%

REGLOG.XLS

	Coefficient	Standard Error	T-Ratio	Approx. Probability	Percent Change	95% Conf. Interval	
						Lower	Upper
'pmlb60io.reg							
MELBOURNE ARTERIALS 60KM/H SPEED ZONES (UNEMPLOYMENT)							
REGRESSION							
INTERCEP	0.9138	0.1796	5.089	0.00			
LOGURATE	0.6472	0.0653	9.911	0.00			
LOGD_T1A	-0.1310	0.0777	-1.687	0.10	-12.3%	-24.7%	2.1%
LOGD_T1B	-0.2127	0.0873	-2.436	0.02	-19.2%	-31.9%	-4.1%
LOGD_T2A	-0.4466	0.0539	-8.287	0.00	-36.0%	-42.4%	-28.9%
LOGD_T2B	-0.6180	0.0573	-10.788	0.00	-46.1%	-51.8%	-39.7%
'R SQUARE ADJ 0.6524							
'psyd60_io.reg							
SYDNEY ARTERIALS 60KM/H SPEED ZONES (UNEMPLOYMENT)							
REGRESSION							
INTERCEP	-1.4251	0.3352	-4.252	0.00			
LOGURATE	0.0277	0.1268	0.219	0.83			
LOGD_T1A	0.0395	0.0828	0.477	0.64	4.0%	-11.6%	22.4%
LOGD_T1B	0.1894	0.0898	2.109	0.04	20.8%	1.3%	44.1%
LOGD_T2A	0.0373	0.0536	0.695	0.49	3.8%	-6.6%	15.3%
LOGD_T2B	0.0590	0.0572	1.03	0.31	6.1%	-5.2%	18.7%
'R SQUARE ADJ 0.0051							
Net Percentage Changes							
LOGD_T1A					-15.7%	-32.5%	5.3%
LOGD_T1B					-33.1%	-47.7%	-14.5%
LOGD_T2A					-38.4%	-46.9%	-28.5%
LOGD_T2B					-49.2%	-56.6%	-40.4%

REGLOG.XLS

	Coefficient	Standard Error	T-Ratio	Approx. Probability	Percent Change	95% Conf. Interval	
						Lower	Upper
'pmlba_io.reg							
Melbourne Arterials (Unemployment)							
REGRESSION							
INTERCEP	0.8561	0.1628	5.258	0.00			
LOGURATE	0.6171	0.0592	10.422	0.00			
LOGD_T1A	-0.1239	0.0704	-1.759	0.08	-11.7%	-23.0%	1.4%
LOGD_T1B	-0.2625	0.0792	-3.316	0.00	-23.1%	-34.1%	-10.2%
LOGD_T2A	-0.4268	0.0489	-8.735	0.00	-34.7%	-40.7%	-28.2%
LOGD_T2B	-0.5920	0.0519	-11.396	0.00	-44.7%	-50.0%	-38.7%
'R SQUARE ADJ 0.6823							
'pmlbr_io.reg							
Melbourne Residential Streets (Unemployment)							
REGRESSION							
INTERCEP	0.8907	0.2729	3.264	0.00			
LOGURATE	0.5808	0.0992	5.852	0.00			
LOGD_T1A	-0.0149	0.1180	-0.126	0.90	-1.5%	-21.8%	24.2%
LOGD_T1B	0.0233	0.1327	0.175	0.86	2.4%	-21.1%	32.8%
LOGD_T2A	-0.2528	0.0819	-3.087	0.00	-22.3%	-33.9%	-8.8%
LOGD_T2B	-0.3784	0.0871	-4.347	0.00	-31.5%	-42.2%	-18.8%
'R SQUARE ADJ 0.2423							
Net Percentage Changes							
LOGD_T1A					-10.3%	-31.5%	17.4%
LOGD_T1B					-24.9%	-44.5%	1.7%
LOGD_T2A					-16.0%	-30.3%	1.3%
LOGD_T2B					-19.2%	-33.8%	-1.5%

REGLOG.XLS

	Coefficient	Standard Error	T-Ratio	Approx. Probability	Percent Change	95% Conf. Interval	
						Lower	Upper
'prov100.log							
Rural Vic 100Kmh Speed Zones (Unemployment & Trend)							
REGRESSION							
INTERCEP	0.6669	0.5356	1.245	0.22			
IDNUM	-0.0096	0.0012	-8.029	0.00			
LOGURATE	0.1056	0.2208	0.478	0.63			
LOGD_T1A	0.0925	0.1467	0.63	0.53	9.7%	-17.7%	46.2%
LOGD_T1B	0.4221	0.1542	2.737	0.01	52.5%	12.7%	106.3%
LOGD_T2A	0.1331	0.1062	1.253	0.21	14.2%	-7.2%	40.7%
LOGD_T2B	0.2541	0.1479	1.718	0.09	28.9%	-3.5%	72.3%
'R SQUARE ADJ 0.5255							
Rural N.S.W. 100Kmh Speed Zones (Unemployment)							
REGRESSION							
INTERCEP	-0.2993	0.4802	-0.623	0.53			
IDNUM	-0.0038	0.0012	-3.163	0.00			
LOGURATE	-0.0878	0.2353	-0.373	0.71			
LOGD_T1A	0.0757	0.0938	0.807	0.42	7.9%	-10.2%	29.6%
LOGD_T1B	-0.0825	0.1069	-0.772	0.44	-7.9%	-25.3%	13.5%
LOGD_T2A	-0.0522	0.0728	0.717	0.48	5.4%	-8.7%	21.5%
LOGD_T2B	0.1116	0.0943	1.184	0.24	11.8%	-7.1%	34.5%
'R SQUARE ADJ 0.2260							
Net Percentage Changes							
LOGD_T1A					14.4%	-18.1%	59.8%
LOGD_T1B					15.4%	-19.5%	65.2%
LOGD_T2A					-9.3%	-29.1%	16.1%
LOGD_T2B					-7.3%	-33.8%	29.8%

REGLOG.XLS

	Coefficient	Standard Error	T-Ratio	Approx. Probability	Percent Change	95% Conf. Interval	
						Lower	Upper
'prov60.log							
Rural Vic 60 Kmh Speed Zones (Unemployment & Trend)							
REGRESSION							
INTERCEP	-0.4466	0.5353	-0.834	0.41			
IDNUM	-0.0081	0.0012	-6.733	0.00			
LOGURATE	-0.0222	0.2207	-0.101	0.92			
LOGD_T1A	0.0928	0.1466	0.633	0.53	9.7%	-17.7%	46.2%
LOGD_T1B	0.2435	0.1541	1.58	0.12	27.6%	-5.7%	72.6%
LOGD_T2A	0.0419	0.1062	0.394	0.69	4.3%	-15.3%	28.4%
LOGD_T2B	0.2395	0.1478	1.62	0.11	27.1%	-4.9%	69.8%
'R SQUARE ADJ 0.4475							
'pron60.log							
Rural N.S.W. 60 Kmh Speed Zones (Unemployment & Trend)							
REGRESSION							
INTERCEP	-1.3703	0.4455	-3.076	0.00			
IDNUM	-0.0063	0.0011	-5.597	0.00			
LOGURATE	-0.2324	0.2183	-1.065	0.29			
LOGD_T1A	-0.0421	0.0870	-0.484	0.63	-4.1%	-19.1%	13.7%
LOGD_T1B	0.1007	0.0992	1.016	0.31	10.6%	-8.9%	34.3%
LOGD_T2A	0.1391	0.0676	2.059	0.04	14.9%	0.7%	31.2%
LOGD_T2B	0.3156	0.0875	3.608	0.00	37.1%	15.5%	62.7%
'R SQUARE ADJ 0.3985							
Net Percentage Changes							
LOGD_T1A					14.4%	-18.1%	59.8%
LOGD_T1B					15.4%	-19.5%	65.2%
LOGD_T2A					-9.3%	-29.1%	16.1%
LOGD_T2B					-7.3%	-33.8%	29.8%

Paper No. 13

M.H. Cameron and S.V. Newstead,
"Evaluation of mass media publicity
as support for enforcement".

Invited paper in:
Proceedings, Australasian Drink-Drive Conference,
Melbourne, November 1993.

Paper No. 13

AUSTRALASIAN DRINK-DRIVE CONFERENCE
Melbourne, November 1993

EVALUATION OF MASS MEDIA PUBLICITY AS SUPPORT
FOR ENFORCEMENT

Max Cameron and Stuart Newstead
Monash University Accident Research Centre

ABSTRACT

The Transport Accident Commission (TAC) in Victoria has launched a number of publicity campaigns with drink-driving themes to support Police enforcement, especially the program of increased random breath testing (RBT) using highly visible "booze buses" which commenced in September 1989. The TAC has also supported the Victorian speed camera program with mass media publicity, especially on television. An evaluation of the speed camera program found statistically significant relationships between levels of TAC publicity and reductions in casualty crashes in Melbourne when other key factors were taken into account (speed camera operating hours, numbers of speeding tickets issued, and unemployment rates). The methods used in that study were employed to examine the effects of TAC publicity with drink-driving themes on serious casualty crashes during those times of the week when drink-driving is most common. Monthly variations in RBT activity, alcohol sales, unemployment rates and seasonal factors were taken into account. The strongest effects were found when the publicity was measured by a function which represents cumulative awareness due to current and previous advertising levels.

1. INTRODUCTION

The research described in this paper was carried by the Monash University Accident Research Centre (MUARC) as part of evaluations of the Victorian speed camera program (including supporting publicity) and the Transport Accident Commission's (TAC) road safety television advertising. The TAC has made a major investment in road safety advertising in Victoria since 1989. Up to the end of 1992, nearly \$23 million had been spent on advertising, with about 70% on television, and about \$2.5 million spent to create the television advertisements. Many of the advertisements have aimed to support Police enforcement activities, including operations aimed at drink-driving such as random breath testing.

However it would not be correct to view the television advertisements as TAC's only contribution to road safety. Since its creation in 1987, the TAC has sponsored a number of other major road safety programs in Victoria, including:

- 13 new "booze buses" for Police random breath testing operations
- 54 new slant radar speed cameras for Police speed enforcement
- support for establishment of the Trauma Centre and Helipad at the Alfred Hospital in Melbourne
- an expanded program of accident "black spot" treatments

- support for the other road safety programs, including school-based and community-based public education activities
- support for the road safety research program at MUARC.

This paper describes the second part of the MUARC evaluation of the road safety benefits of TAC's television advertising campaigns (Cameron et al 1993). The first part attempted to estimate the general effects of the advertising campaigns on Victoria's road safety performance, but was unable to do this satisfactorily because of the absence of evaluations of some major road safety programs, the availability of only average effects over a number of months for those programs which had been evaluated, and the inclusion of publicity effects (including TAC road safety advertising) in the measured effects of some key programs in Victoria.

The second part built on recent MUARC evaluations of two major road safety programs which combined TAC advertising campaigns with increased enforcement efforts by the Victoria Police (i.e. the speed camera program and the program of increased random breath testing using "booze buses", both commencing near the end of 1989). Further details of the enforcement activities, supporting publicity, and assessment of their combined and individual effects are given later in this paper.

The third part of the project attempted to evaluate the *Concentrate or Kill* television campaign conducted by TAC. *Concentrate or Kill* differs from the speed and drink-driving advertising campaigns in that it was not designed to support enforcement. Thus the details of this part of the project are not given here, but the results are described briefly by way of contrast with the findings for the publicity campaigns supporting enforcement.

2. TAC ROAD SAFETY PUBLICITY CAMPAIGNS

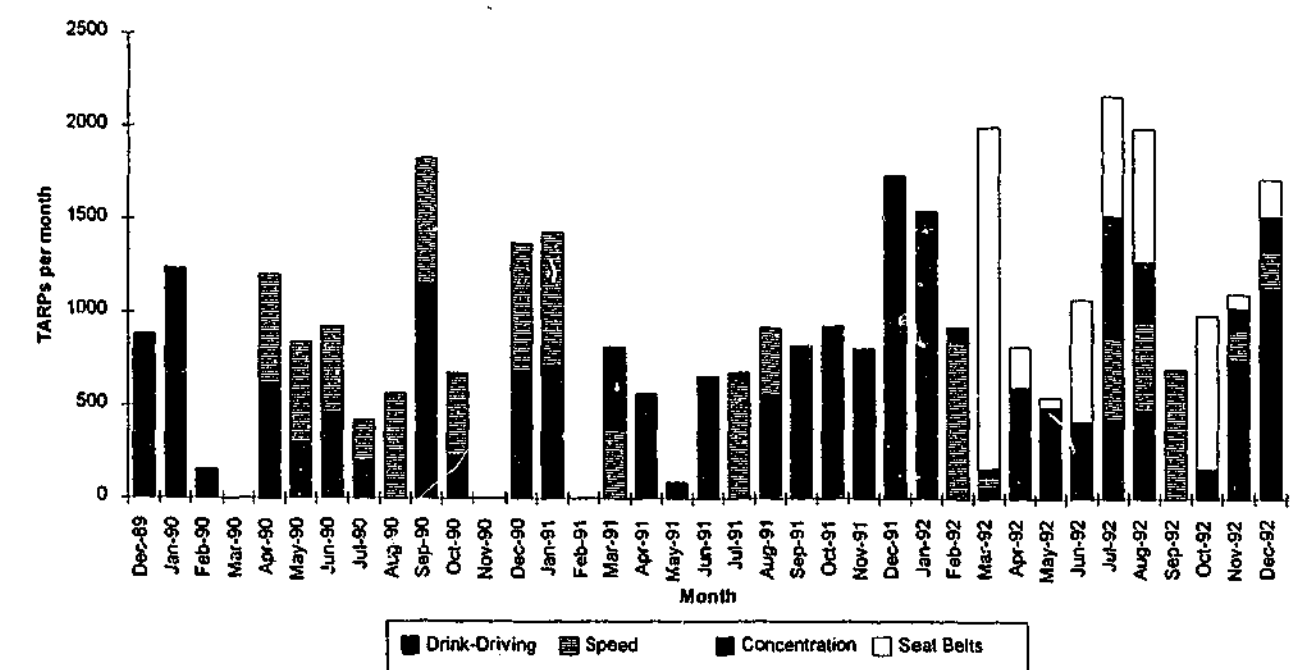
The TAC launched its first major road safety advertising campaign on television in December 1989. Each television advertisement was launched under the banner of a general theme related to the road safety problem area targeted, and each theme had an associated slogan phrase which typically was included at the end of the corresponding advertisements (Table 1).

Table 1: Theme, slogan, name and launch date for each television advertisement launched by TAC during December 1989 to December 1992.

Theme	Slogan	Name	Launch Date
Drink-driving	"If you drink, then drive, you're a bloody idiot"	Lost Family	December 1989
		Girlfriend	December 1989
		Booze Bus	September 1990
		Friends	August 1991
		Joey	November 1992
Speed	"Don't fool yourself, speed kills"	Speed Cameras	April 1990
		Beach Road	April 1990
		Tracey	September 1990
Concentration	"It's in your hands, concentrate or kill"	Country Kids	March 1991
		The Morgue	October 1991
Seat Belts	"Belt up, or suffer the pain"	Bones	March 1992

The intensity of TAC television advertising (TARPs) by theme and month to December 1992 is shown in Figure 1. TARPs (Target Audience Rating Points) is a measure of audience reach. It is a summation of the Rating Points (ie. the percentage of persons in the viewing area estimated to be watching the specific television channel at the time the advertisement was shown) for the particular Target Audience of the advertisement (ie. persons aged 18-39). However it does not describe the distribution of the number of viewings of the advertisement by each member of the Target Audience (eg. percentage having seen it zero times, once, twice, three times, etc.).

FIGURE 1
TAC Road Safety Television Advertising : TARPs per month by theme



There was considerable monthly variation in the level and content of TAC television advertising (Figure 1). This variation represented an opportunity to examine the link between the TAC advertising and the monthly road safety performance in Victoria.

Television was the principal medium used by TAC for its road safety advertising throughout 1990-92. Other media used were radio, press, outdoor advertising (including mobile billboards), Sky Channel and cinema. Television advertising TARPs were planned and bought at the same level in the Melbourne and Victorian regional television areas. This implies that the intensity of the television advertising, shown in Figure 1, was approximately the same for all Victorian viewing areas during 1990-92. It was estimated that 25% of media placements of the TAC advertisements have been at no charge each year, ie. the actual number of showings was a third higher than the purchased number (however the audience reach, measured by TARPs, may not be a third higher because the free showings may not have occurred during high rating programs).

3. PUBLICITY SUPPORTING POLICE ENFORCEMENT

3.1 Publicity supporting the Speed Camera Program

New slant radar speed cameras were progressively introduced in Victoria commencing with four in December 1989 and building to 54 by January 1991. The program included the high-exposure publicity campaign "Don't fool yourself - speed kills" launched in April 1990. From mid-1990 to December 1991 the number of Traffic Infringement Notices (TINs) issued per month for offences detected by the speed cameras varied from 14,000 to 64,000, averaging 16,100 during the second half of 1990 and 43,100 during 1991.

MUARC has evaluated the effect of the program on changes in "low alcohol hour"¹ casualty crashes and their severity during four periods from December 1989 to December 1991 in which there were different combinations of levels of publicity and camera activity (Cameron et al 1992a). The evaluation included a phase from April to June 1990 when there were negligible levels of camera use (after an intense 9 day burst at the beginning of April) but the television publicity continued at a high level. During this period the study showed statistically significant reductions in casualty crashes and their injury severity of 24% and 29%, respectively, in Melbourne during the low alcohol hours of the week, and a 21% reduction in the low alcohol hour casualty crashes in rural towns (Cameron et al 1992a). These reductions were measured relative to crash experience in NSW during the same period, and after taking into account the differential changes in unemployment rates (which were found to affect travel patterns) and crash seasonality and trends in each State.

A second stage of the evaluation linked the monthly levels of TAC television advertising (TARPs) with the variations in casualty crashes in Melbourne during low alcohol hours in 1990-91, and found statistically significant relationships for all TAC publicity and for the speed-related publicity in particular (lower level of significance). This analysis took into account the monthly variations in speed camera operating hours (Figure 2) and the speed camera TINs issued (Figure 3), the two other program variables potentially having a large effect on crashes, as well as variations in Melbourne unemployment rates and crash seasonality and trend. Thus a by-product of this evaluation was the identification of a relationship between the monthly level of TAC advertising and casualty crash reductions in Melbourne, but this had been sought and found only for low alcohol hour crashes (Cameron et al 1992a). Further details of this relationship are given in Section 4.

3.2 Publicity supporting the Random Breath Testing Program

The program of increased random breath testing (RBT) using highly visible "booze buses" commenced in September 1989. Initially there was a considerable increase in testing using four existing buses (Figure 4), which were supplemented during 1990 by the progressive introduction of 13 new buses provided by TAC to the Victoria Police for this purpose. This initiative was supported by the first of the TAC high-exposure publicity campaigns, "If you drink then drive - you're a bloody idiot", launched in December 1989. Television

¹ The "low alcohol hours" of the week (ie. Monday-Thursday 6am to 6pm, Friday 6am to 4pm, Saturday 8am to 2pm, Sunday 10am to 4pm) are those periods when the percentage of drivers killed or admitted to hospital with a blood alcohol content exceeding 0.05%, was below 4%. The "high alcohol hours" are the converse of these periods, during which about 38% of driver serious casualties had blood alcohol content exceeding 0.05% (Harrison 1990).

advertising with drink-driving themes has been a consistent part of the TAC publicity program during 1990-92. Further details of this enforcement program and supporting publicity are given in another paper to this Conference (Cavallo 1993).

FIGURE 2
Monthly number of Hours of Speed Camera Operations
Melbourne, June 1990-December 1991 (earlier data not available)

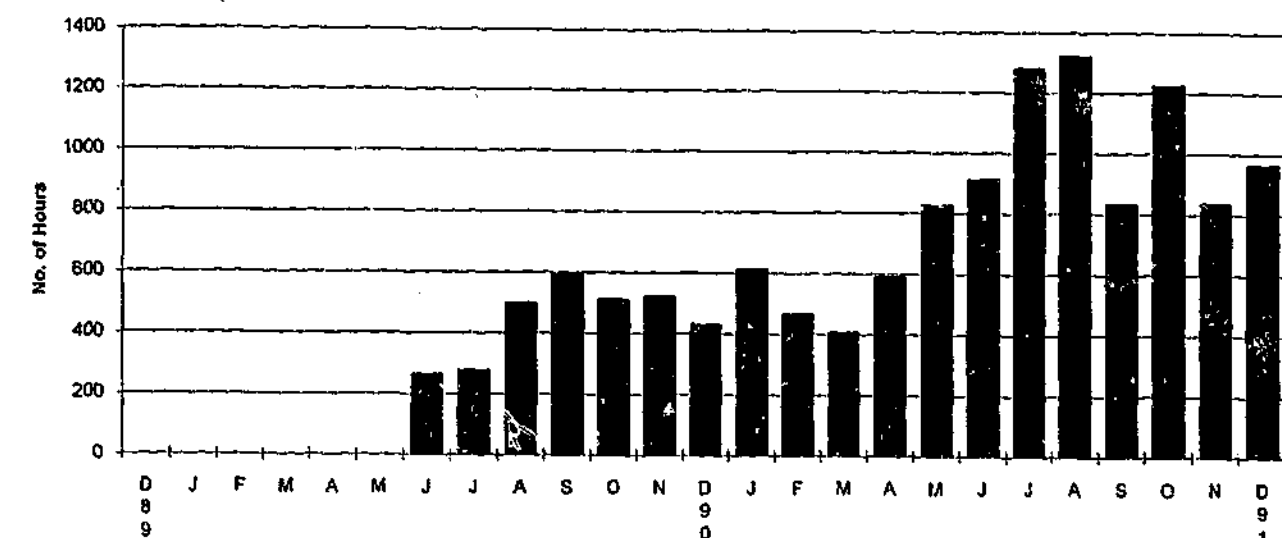
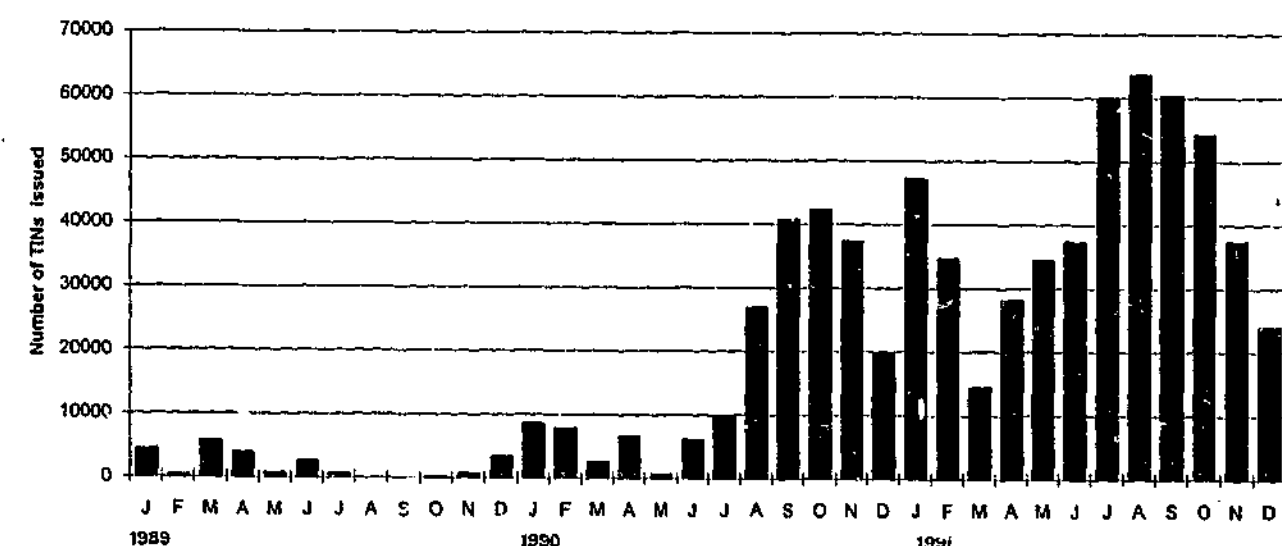
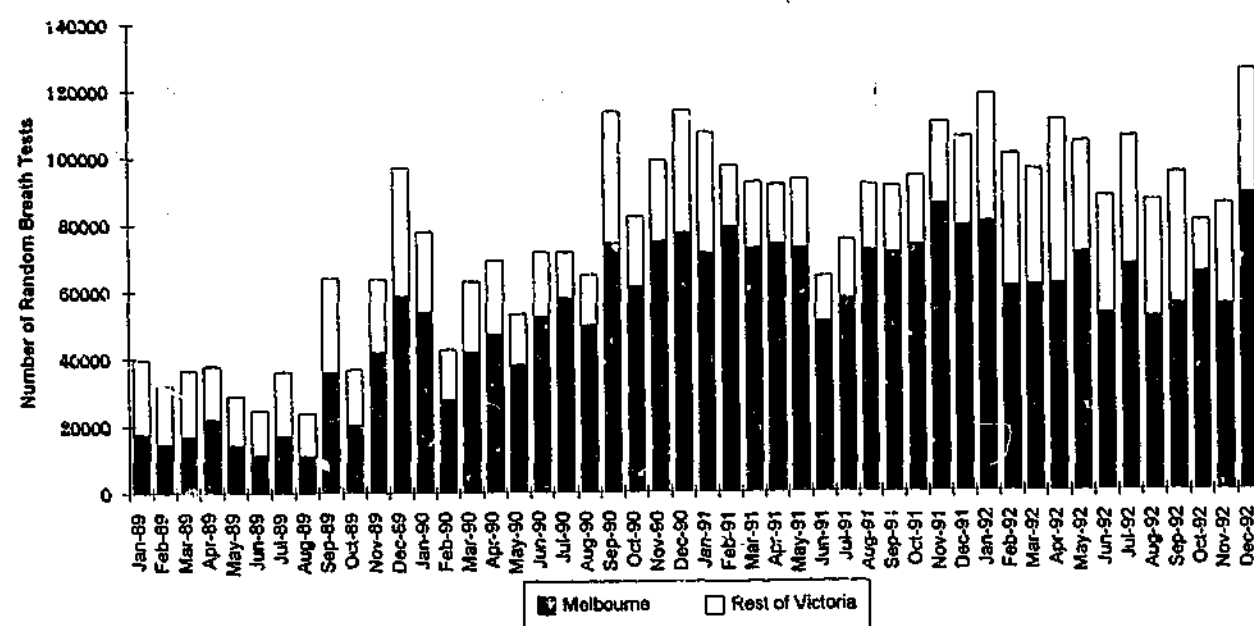


FIGURE 3
Monthly number of Traffic Infringement Notices Issued
for Speeding Offences Detected by Speed Cameras
Victoria 1989-1991



A MUARC study examined changes in "high alcohol hour" (HAH) serious casualty crashes occurring in Melbourne, and in the rest of Victoria (where the program was slower to develop), during 1990 and 1991 separately (Cameron et al 1992b). Serious casualty crashes (SCCs) are those resulting in death or serious injury (usually hospital admission). The changes were adjusted for parallel changes in corresponding areas of NSW, to account for "other" factors affecting crashes apart from the effects of economic conditions, which were taken into account using trends in unemployment rates in each State. Using multivariate regression models, the analysis found the statistically significant reductions in HAH SCCs of 18% in Melbourne and 13% in rural Victoria during 1990. There was also a statistically significant reduction of 24% in rural Victoria during 1991, but the apparent 13% reduction in HAH SCCs in Melbourne during 1991 was not statistically significant.

FIGURE 4
Number of Random Breath Tests per month
Victoria 1989 - 1992



The analysis method linked monthly variations in the number of HAH SCCs during 1983-91 with variables representing the following:

- step effects during December 1989, during 1990, and during 1991 (the December 1989 effects were not statistically significant in any area)
- monthly unemployment rates in the corresponding area
- seasonal variation (monthly steps effects)
- long-term trend.

A measure of alcohol consumption per month was also initially considered but ultimately was excluded from the models. Unlike the Speed Camera Program Evaluation, the analysis did not include a stage where operational measures of the RBT program activity (eg. number of breath tests and levels of TAC drink-driving publicity) were included in the models instead of the step effects representing the influence of the program in Victoria for a period (eg. the step for 1990). This extension of the analysis of HAH SCCs has since been carried out and is described in Section 5.

4. FORM OF THE RELATIONSHIPS USED FOR "LOW ALCOHOL HOURS"

The relationship between low alcohol hour casualty crashes in Melbourne per month and TAC advertising TARP had been found by fitting the model for casualty crashes shown in Appendix A over the period 1983-91 (Cameron et al 1992a). It has been found in a number of studies that multiplicative (rather than additive) models fitted to the explanatory variables are best to represent road trauma series (Pascoe 1988, Hakim et al 1991, Thoresen et al 1992).

In the context of this paper, the specific model was able to take into account the monthly fluctuations (some very substantial) in those explanatory variables which were found to have significant effects on casualty crashes, so that the separate effect of the TAC advertising

could be assessed. However the process assumed that the form of the model was correct, and the analysis was confined to estimating the magnitude of the exponent of each explanatory variable. In practice, the exponents were estimated by first taking logarithms of the number of casualty crashes and each of the explanatory variables, and then fitting the relationship using multiple linear regression.

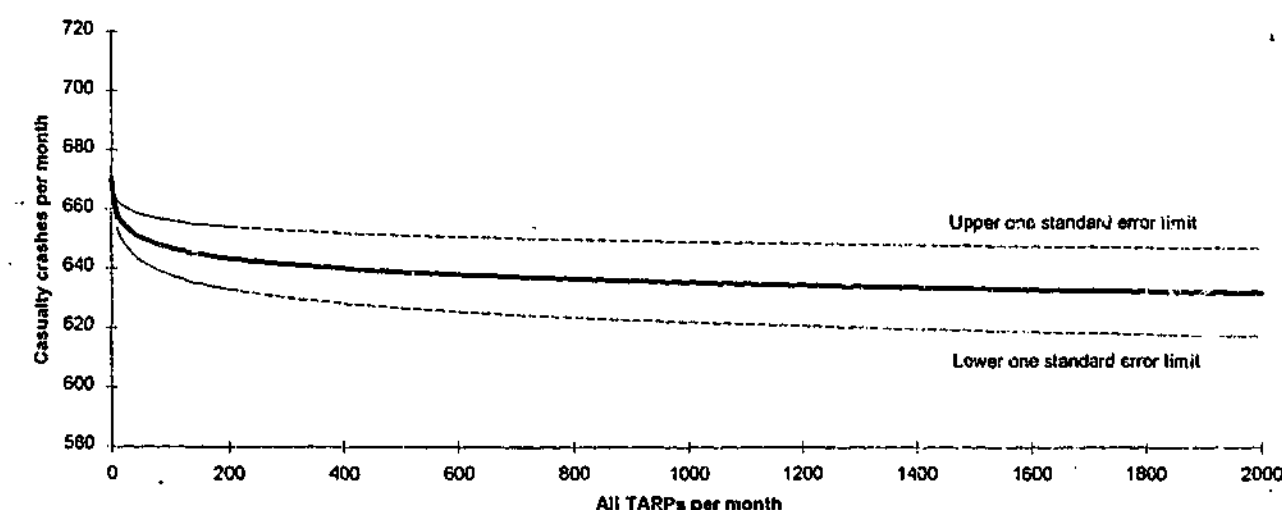
4.1 Relationship with all TAC advertising TARP

The exponent of all TAC advertising TARP per month was estimated to be -0.0077 with a standard error of 0.0031 (Cameron et al 1992a, Table 9) and was statistically significant ($p < 0.05$; two-tailed test). When all other explanatory variables in the fitted model were held constant at their average levels during December 1989 to December 1991, the form of the relationship with low alcohol hour casualty crashes in Melbourne was:

$$\text{CASUALTY CRASHES PER MONTH} = 670.5 (\text{All TARP})^{-0.0077}$$

This estimated relationship is shown in Figure 5. A relationship of this form is to be expected in this context because it is consistent with the likely diminishing returns, ie. the crash reduction per month increases at a diminishing rate as the amount of advertising increases. The range in which the true relationship could lie (with approximate 68% confidence) is indicated by the lower and upper limits calculated by adding and subtracting one standard error from the estimated exponent. The relationship in Figure 5 may not be realistic for non-zero TARP in the range below 400 because there was only one month in 1990-91 with a TARP value in this range.

FIGURE 5
REDUCTION IN CASUALTY CRASHES v.
ALL T.A.C. PUBLICITY (TARPs)
Melbourne roads during low alcohol hours (all other effects held constant)



4.2 Relationship with "Speeding" and "Concentration" TARPs

The previously described relationship, found by Cameron et al (1992a), was between low alcohol hour casualty crashes and total TARPs from all advertising themes used by TAC, including drink-driving. It was considered unlikely that the drink-driving publicity influenced the low alcohol hour crashes, since the proportion of such crashes involving

alcohol is relatively small. If so, the inclusion of the drink-driving TARPs may have masked a stronger relationship between the remaining TARPs and low alcohol hour casualty crashes.

To check this possibility the same analysis method employed by Cameron et al was used to fit the model in Appendix A, but considering only TARPs from advertising with speeding or concentration themes. The total TARPs per month with one of these themes ranged from 280 to 1150, with one month of 40 and six months with zero.

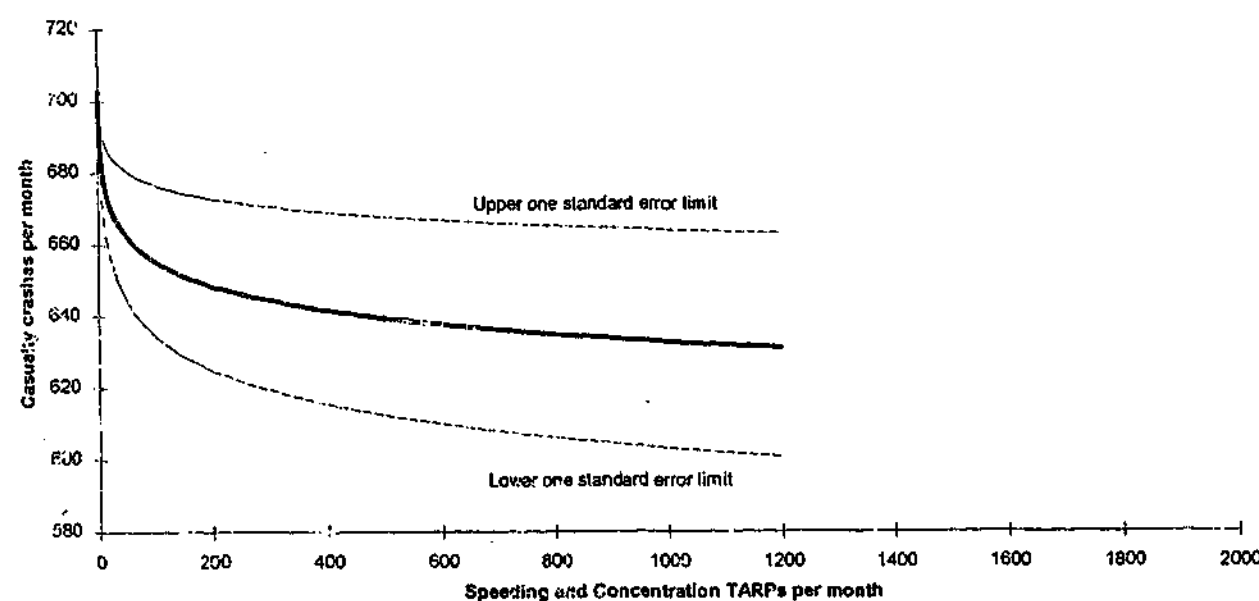
A statistically significant exponent of the speeding plus concentration TARPs was found when the model was fitted to the monthly low alcohol hour casualty crashes in Melbourne. The estimated exponent was -0.0152 with a standard error of 0.0070 (statistical significance level $p < 0.05$). The greater magnitude of the exponent compared with that observed when TARPs from all publicity themes were considered (Section 4.1) indicates a stronger relationship (though subject to a higher level of uncertainty because of the larger standard error of the estimate).

When the other explanatory variables were held constant at their average levels, the form of the relationship was:

$$\text{CASUALTY CRASHES PER MONTH} = 702.7(\text{Speeding \& Concentration TARPs})^{-0.0152}.$$

This relationship is shown in Figure 6, together with the likely range for the true relationship. The relationship is not shown for TARPs above 1200 per month, since this is outside the range of the speeding plus concentration publicity levels observed during 1990-91, and may not be realistic for non-zero TARPs below 280.

FIGURE 6
REDUCTION IN CASUALTY CRASHES v.
T.A.C. SPEEDING AND CONCENTRATION PUBLICITY (TARPs)
Melbourne roads during low alcohol hours (all other effects held constant)



The relationships in Figures 5 and 6 were used to estimate the point of diminishing returns for the investment in TAC television publicity per month, on average. The process

compared the value of the estimated crash savings per month at each level of monthly TARPs with the cost of investing in those TARPs (Cameron et al 1993).

5. FORM OF THE RELATIONSHIPS TESTED FOR "HIGH ALCOHOL HOURS"

The form of the relationships tested between high alcohol hour (HAH) crashes and levels of TAC publicity followed that developed in the Speed Camera Program Evaluation (see Section 4). In addition, the potential effect of the publicity in each month was represented in two ways, the second method representing possible carry-over effects to subsequent months. The analysis initially considered only serious casualty crashes, because of the known high involvement of drink-driving in these crashes during HAHs, and analysed crashes in Melbourne and the rest of Victoria separately.

5.1 Multivariate regression models

Based on previous experience in evaluating the RBT program, it was decided to fit the multiplicative models shown in Appendix B to the monthly numbers of HAH crashes during 1983-92. The explanatory variables included:

- monthly unemployment rates in the corresponding area (Figure 7)¹
- number of random breath tests conducted in the area (Figure 4)
- an index of alcohol sales in Victoria (Figure 8, following Cameron et al 1992b)
- effect of TAC publicity (TARPs placed on television during the month, or the Adstock² of TAC television placements)
- seasonal variation (monthly steps, positive or negative)
- long-term trend.

Thus the models were able to take into account the monthly fluctuations (some very substantial) in those explanatory variables which were found to have significant effects on HAH crashes, so that the separate effect of the TAC advertising could be assessed. However the process assumed that the form of the model was correct, and the analysis was confined to estimating the magnitude of the exponent of each explanatory variable.

5.2 Mechanisms of advertising effects

Television advertising placed in a given week does not necessarily produce its full effects in that week nor do its effects stop at the end of that week. Studies of advertising effects, principally of awareness of the main messages, have shown that there are delayed and carry-over effects.

Broadbent (1979) found that there is a delayed growth in awareness to peak levels when advertising is conducted at low constant levels. There is a "trigger threshold" of exposure to

¹ Following Cameron et al (1992a,b), unemployment rates in the two areas of Victoria were used as a measure of economic activity in preference to an estimate of total travel in Victoria based on fuel sales. This was because of the need to model crashes in the two areas separately and because the previous research had found statistically significant relationships with unemployment rates.

² "Adstock" is a measure of the effect of current and past levels of advertising, which will be described in Section 5.3.

the advertising before most people are fully aware of the message. The consensus is that a minimum of three exposures is needed (Elliott, in preparation). This could be achieved with a minimum of 300 TARPs if they were perfectly distributed over the target audience. In practice the distribution of number of exposures per person is uneven.

FIGURE 7
Unemployment Rates in Melbourne (MSD) and the rest of Victoria (ROV)
1983-92

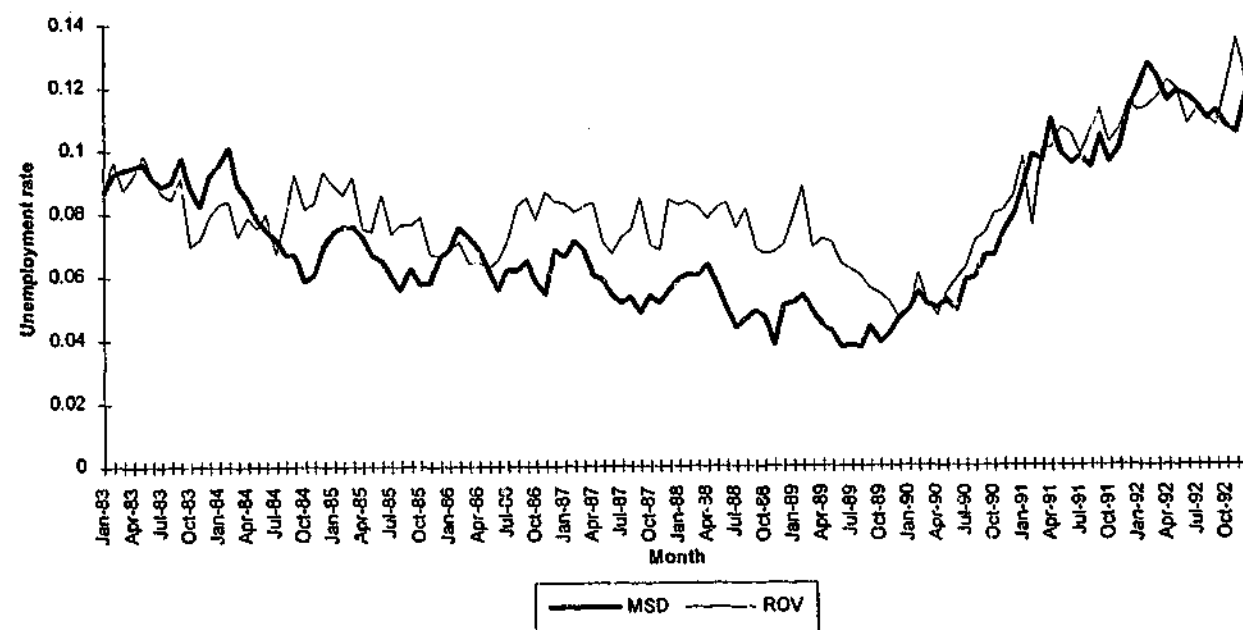
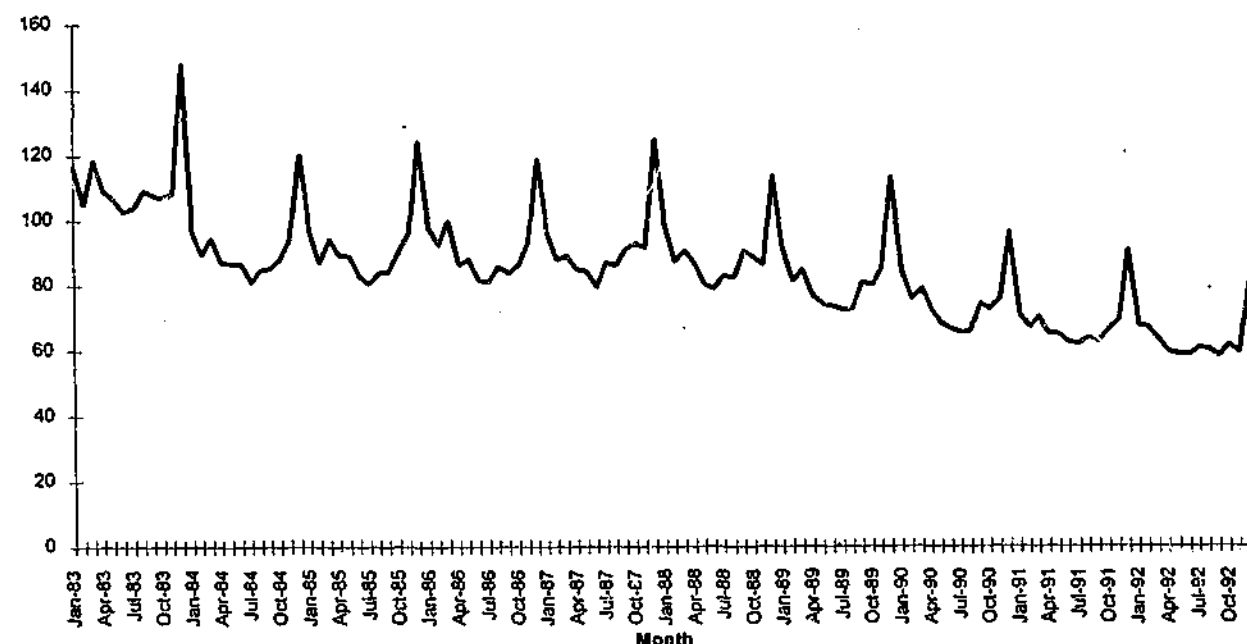


FIGURE 8
Index of Alcohol Sales in Victoria, 1983-92



Most advertisers aim to break through the trigger threshold quickly by placing a high level of advertising at the beginning of a campaign. TAC launches of a new television advertisement nearly always exceeded 300 TARPs in the first week. Thus no delayed effects of TAC publicity were expected and it was assumed that any effects on crashes commenced immediately in the week in which the advertisements were placed.

There is also quite a high level of retention of the awareness of the message of the advertising to the next and subsequent weeks, and Broadbent (1979) has found this to decay exponentially with time, ie. a constant retention factor (eg. 87%) represents the proportion of the target audience retaining awareness of the message in the next week, and then the same proportion of them in the next week, and so on. Broadbent expressed this decay function in terms of its "half-life", ie. the number of weeks after which only half the audience is aware of the message. A number of studies of "brand" advertising have found that a half-life of five weeks is very common (Broadbent 1979; Brown 1988, quoted in Broadbent 1990); this corresponds to a retention factor of approximately 87% per week.

A high level of advertising can lead to satiation of the message or "wear-out". This can occur for some individuals at even modest levels of advertising because of the imperfect distribution of exposure. For a typical weekly placement of advertising which achieves 300 TARPs, some 16% of the target audience will be exposed to the message six or more times in the week (Elliott, in preparation). One way of minimising wear-out is to create more than one advertisement and to rotate them in a pool.

4.3 Adstock

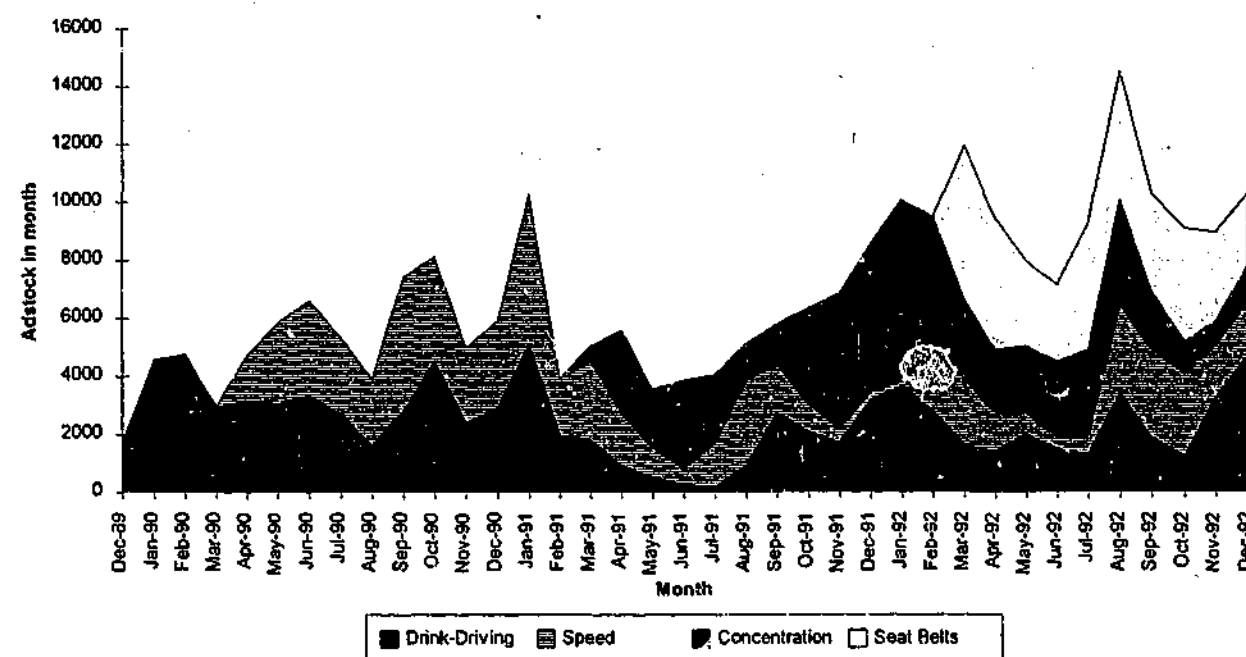
"Adstock" is a concept developed by Broadbent (1979) to represent the effects on awareness of current and past advertising. It is the cumulation of the TARPs in the current week with the decayed effects (applying a specific retention factor) of the TARPs placed in each previous week. Thus Adstock is "cumulative, decayed ratings ... and so represents the amount of advertising current at the time" (Broadbent 1990).

Adstock (using a half-life of five weeks) was calculated from the weekly levels of advertising placed by TAC to represent the total effect in each week of TAC's current and past advertising. In essence, it represents the total TARP effect on awareness considered to be given to or retained by the target audience during a specific week.

The calculations were made for each theme of the TAC advertisements as well as the total, and then grouped into each month during December 1989 to December 1992 (Adstock during a week spanning two months was distributed pro-rata over the months). Total Adstock showed an increasing but fluctuating trend during 1990-92 (Figure 9), with no month having a zero level (compare with raw TARPs per month in Figure 1).

VIC ROADS (in conjunction with TAC) placed a substantial level of anti-drink-driving advertisements on Victorian television during November 1989, achieving 430 TARPs for audiences in the 16-24 year old age group in Melbourne. For the purpose of the analysis described in the following section, the decayed levels of these drink-driving TARPs were added to those achieved by TAC placements commencing in December 1989.

FIGURE 9
TAC Road Safety Television Advertising - Monthly Adstock by theme



5.4 Crash types analysed

The initial analysis considered only serious casualty crashes (SCCs) during HAHs because of the high involvement of drink-driving in these crashes, the known effects of the Random Breath Test Program on this crash type (Cameron et al 1992b) and the likelihood that previously developed models could be extended (see Section 3.2). Subsequently the analysis was extended to non-serious casualty crashes and then to all casualty crashes during HAHs (Cameron et al 1993), but this will not be described here.

Separate analyses were conducted for HAH crashes which occurred in Melbourne and in the rest of Victoria. The unemployment rates and monthly random breath tests used as explanatory variables were those which occurred in each specific area. The index of alcohol sales was available only for total Victoria and was used in each of the two areas. The intensity of TAC television advertising, measured by TARPs per week, was considered to be equally applicable in the two areas (see Section 2).

The effects of the television publicity were represented both in terms of raw TARPs in each month and by the calculated Adstock for the month (see Section 5.3). TARPs and Adstock specifically for drink-driving themes were considered as well as those for all themes. The drink-driving publicity was considered likely to have had direct effects (if any) on crashes during the high alcohol hours, when crashes involving drink-driving are most common. However, monthly TARPs from publicity of all themes had been shown to have a statistically significant relationship with low alcohol hour crash levels in the Speed Camera Program Evaluation, whereas for the speed-related TARPs the relationship was only marginally significant. For this reason, levels of TAC publicity of all themes were also specifically considered in the analysis.

6. RELATIONSHIPS FOUND FOR HIGH ALCOHOL HOUR CRASHES

6.1 Crashes in Melbourne

The model given in Appendix B was fitted to the explanatory variables to explain the monthly variations in HAH SCCs in Melbourne during 1983-92. This was done by taking logarithms of the crash numbers and each explanatory variable, and then fitting the model by multiple linear regression. The four models fitted each explained 83-84% of the monthly variation in HAH SCCs. The estimated exponents of the TAC publicity variables were all statistically significant (Table 2).

Table 2: MELBOURNE SERIOUS CASUALTY CRASHES (High alcohol hours of the week). Effects of TAC Publicity.

PUBLICITY THEME	Estimated Exponent of TAC Publicity Level (standard error of estimate)	
	TARPs in month	Adstock
All publicity themes	-0.0261*** (0.0066)	-0.0251*** (0.0061)
Drink-driving theme	-0.0131* (0.0055)	-0.0249*** (0.0061)

* Statistically significant at $p < 0.05$ level; *** Very highly statistically significant at $p < 0.001$ level.

These estimated exponents, which measure the elasticity of HAH SCCs to the TAC publicity, should be compared with the estimated exponents found for low alcohol hour casualty crashes in Melbourne (namely -0.0077 for all TAC publicity and -0.0152 for speeding and concentration themes only; see Section 4). These were estimated using similar models, but only for TAC publicity measured by the TARP level in the month.

The apparent effects of TAC publicity of any theme on HAH SCCs in Melbourne were greater than those measured on low alcohol hour casualty crashes in the Speed Camera Program Evaluation. The drink-driving TARPs appeared to have had similar levels of effect as the combined speeding and concentration TARPs on the corresponding types of crash (ie. high and low alcohol hour crashes, respectively).

The effects on Melbourne HAH SCCs of TAC publicity of any theme, compared with the specific drink-driving publicity, appear similar, especially when the publicity levels are measured by Adstock (Table 2). This may be explained by the very high correlation in the monthly levels of the general and specific themes, as can be seen in Figure 9 (correlation between raw TARPs was 0.77 and between the Adstocks was 0.82). Thus it is unclear whether the specific drink-driving publicity, or the TAC publicity in general, may be related to the reductions in HAH SCCs measured by the exponents in Table 2.

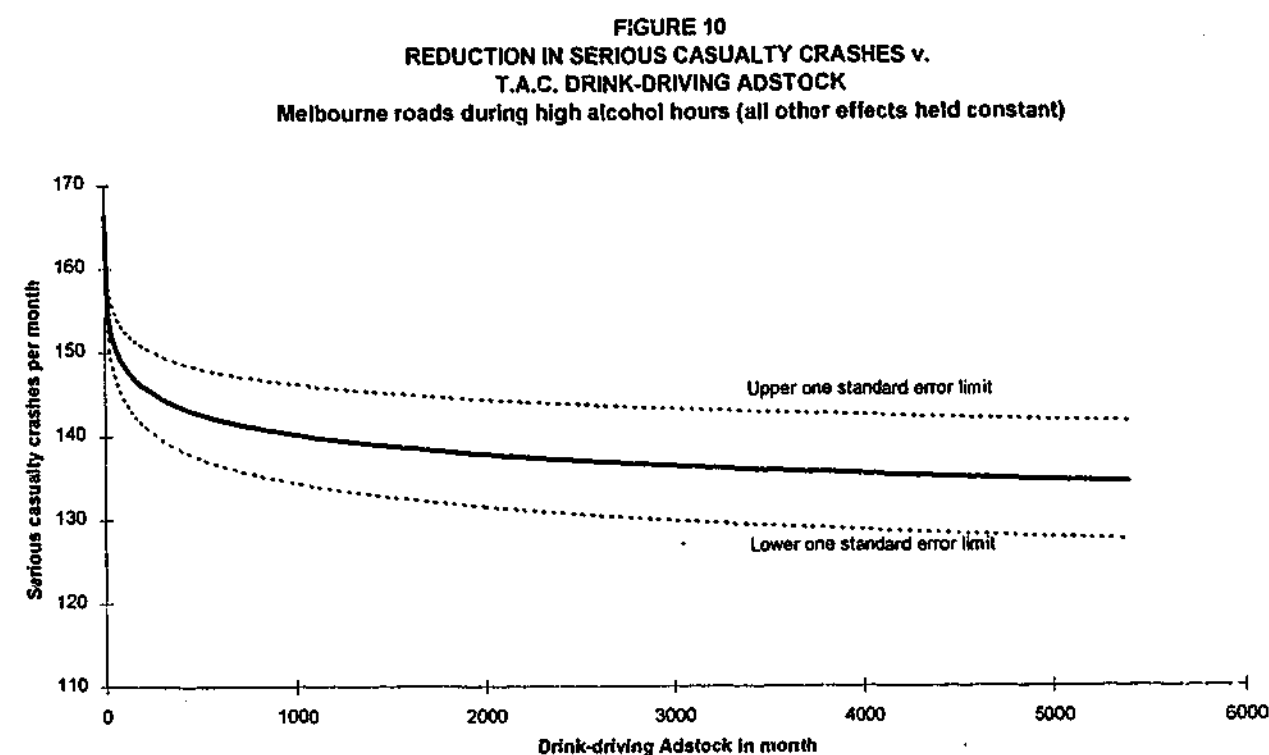
The general TARPs were strongly correlated with the number of random breath tests in the same month (correlation 0.81) and moderately correlated with the unemployment rate (0.52)

and alcohol sales (-0.50). In contrast, the drink-driving TARPs were only moderately correlated with the random breath tests (0.63) and had correlation coefficients less than 0.25 in magnitude for the unemployment rate and alcohol sales. Similar remarks apply to the correlations with the general and specific Adstocks. As relatively low correlation between different explanatory variables is desirable to improve model reliability, the models which include the drink-driving TARPs or Adstock are likely to be more reliable than those for the general publicity levels, and hence the estimated exponents of the drink-driving publicity in Table 2 are likely to be more reliable.

The form of the relationship between HAH SCCs in Melbourne and drink-driving Adstock, when all of the other explanatory variables in the fitted model were held constant at their average levels during December 1989 to December 1992, was:

$$\text{MELBOURNE HAH SCCs PER MONTH} = 166.4 (\text{Drink-Driving Adstock})^{-0.0249}.$$

This relationship is shown in Figure 10, together with the likely range in which it lies.



6.2 Crashes in the rest of Victoria outside Melbourne

The four models fitted each explained 61-67% of the monthly variation in HAH SCCs in country Victoria. Except for the drink-driving TARPs in each month, the estimated exponents relating TAC publicity levels with reductions in HAH SCCs were all statistically significant (Table 3). In particular, the Adstock of the drink-driving publicity was very highly statistically significant, even though the raw drink-driving TARPs were not.

Table 3: COUNTRY VICTORIA SERIOUS CASUALTY CRASHES (High alcohol hours of the week). Effects of TAC Publicity.

PUBLICITY THEME	Estimated Exponent of TAC Publicity Level (standard error of estimate)	
	TARPs in month	Adstock
All publicity themes	-0.0345*** (0.0080)	-0.0327*** (0.0067)
Drink-driving theme	-0.0132 ns (0.0078)	-0.0316*** (0.0074)

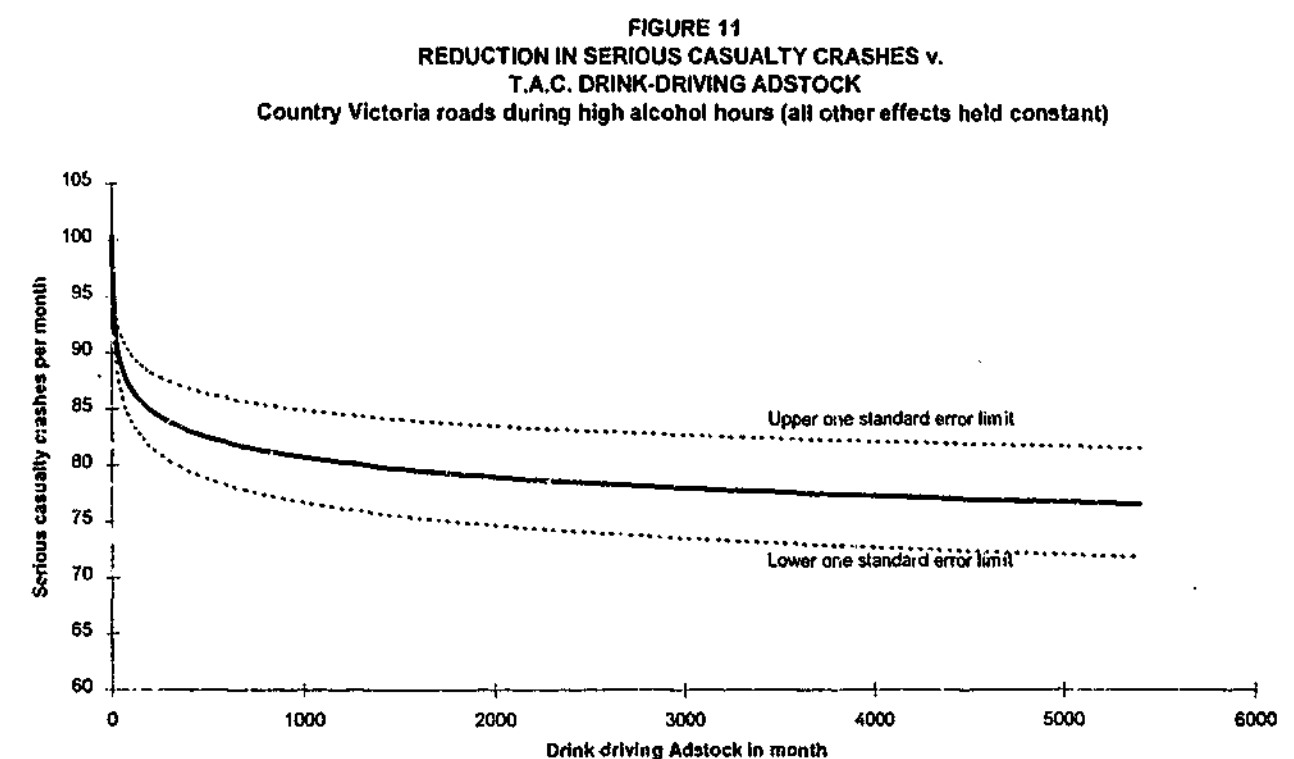
*** Very highly statistically significant at $p < 0.001$ level.

As in Melbourne, there were relatively high correlations between the general TARPs and the country random breath tests (0.81) and unemployment rate (0.47). The drink-driving TARPs had lower correlations with the monthly random breath tests (0.63) and unemployment rate (0.19). [The correlations with alcohol sales in Victoria were unchanged and have been described with the Melbourne results.] Thus the estimated exponents for the drink-driving publicity in Table 3 are likely to be more reliable than those for the general publicity.

The form of the relationship between HAH SCCs in country Victoria and drink-driving Adstock, when all of the other explanatory variables in the fitted model were held constant at their average levels during December 1989 to December 1992, was:

$$\text{COUNTRY HAH SCCs PER MONTH} = 100.4 (\text{Drink-Driving Adstock})^{-0.0316}.$$

This relationship is shown in Figure 11, together with the likely range in which it lies.



6.3 Other factors included in the relationships

The models fitted indicated the importance of a number of other factors, in addition to TAC television publicity, in explaining variations in the monthly numbers of HAH SCCs during the period to the end of 1992. The regression coefficients of each of the (logged) explanatory factors included in the models with drink-driving Adstock, excepting the coefficients for the monthly step variables and the intercept, are summarised in Table 4. The "goodness-of-fit" of the models over the period 1983-92 is shown in Figures 12 and 13.

Table 4: Effects of unemployment rate, number of random breath tests, alcohol sales (all Victoria), and Adstock of TAC drink-driving publicity on serious casualty crashes during high alcohol hours of the week. Melbourne and country Victoria 1983-92.

AREA OF VICTORIA	Estimated coefficient of logged variable	T value (103 d.f.)	Significance level (two-tailed)
MELBOURNE CRASHES			
• trend	0.0037***	4.515	0.0001
• unemployment rate in month	-0.3300***	-8.541	0.0001
• no. of random breath tests during month	-0.0111**	-2.655	0.0092
• alcohol sales during month	0.6522***	3.914	0.0002
• drink-driving Adstock in month	-0.0249***	-4.092	0.0001
CRASHES IN COUNTRY VICTORIA			
• trend	0.0024*	2.261	0.0258
• unemployment rate in month	-0.1649*	-2.387	0.0188
• no. of random breath tests during month	0.0055	0.912	0.3640
• alcohol sales during month	0.7500**	3.138	0.0022
• drink-driving Adstock in month	-0.0316***	-4.282	0.0001

* Statistically significant at $p < 0.05$ level; ** Highly statistically significant at $p < 0.01$ level;

*** Very highly statistically significant at $p < 0.001$ level.

The monthly unemployment rate was very highly significantly and inversely related to the crashes in Melbourne, and significantly and inversely related to the crashes in country Victoria. The strength of the relationships were greater than those observed in previous studies of total Victorian fatalities to the end of 1990 (Thoresen et al 1992) and of HAH SCCs to the end of 1991 (Cameron et al 1992b).

The monthly number of random breath tests was highly significant and inversely related to the crashes in Melbourne, but was not statistically significantly related to monthly variations in country Victoria HAH SCCs. Relationships of this type had not previously been tested in the evaluation of the RBT program (Cameron et al 1992b).

Monthly variations in the index of alcohol sales in Victoria were significantly and positively related to the HAH SCCs in Melbourne (very highly significant) and in country Victoria (highly significant).

FIGURE 12
SERIOUS CASUALTY CRASHES IN MELBOURNE DURING HIGH ALCOHOL HOURS 1983-92
Fit of models using Drink-Driving Adstock (R-square = 0.84)

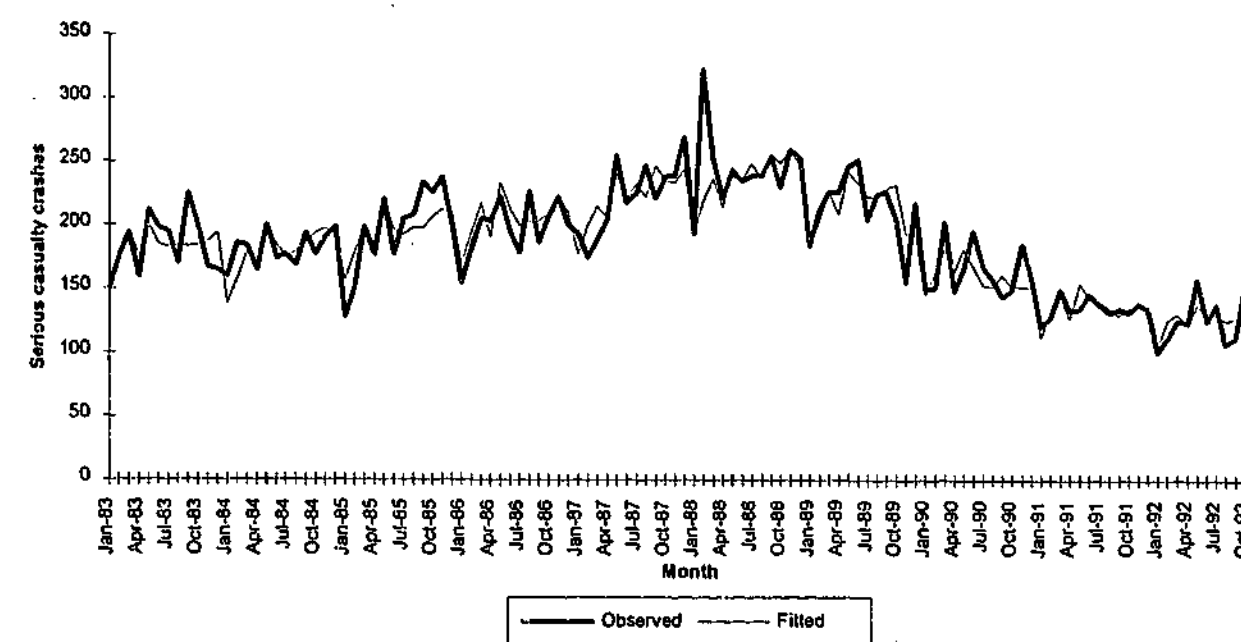
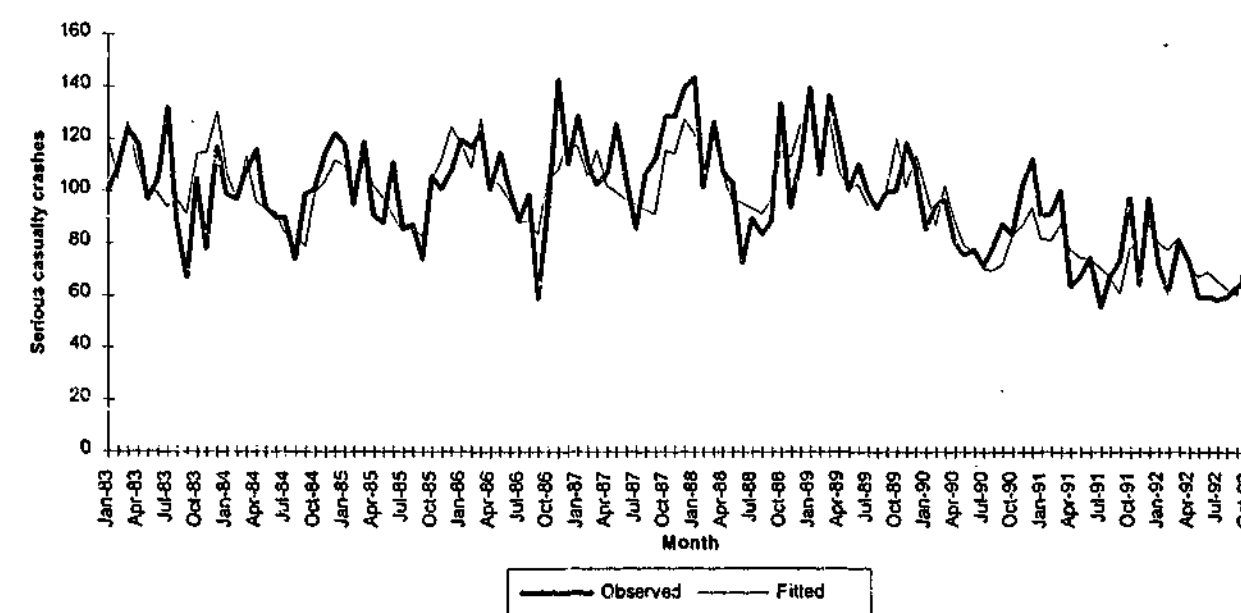


FIGURE 13
SERIOUS CASUALTY CRASHES IN COUNTRY VICTORIA DURING HIGH ALCOHOL HOURS 1983-92
Fit of models using Drink-Driving Adstock (R-square = 0.66)



7. THE CONCENTRATE OR KILL CAMPAIGN NOT LINKED TO ENFORCEMENT

The campaign with the theme "Its in your hands, concentrate or kill" aimed to encourage drivers, especially the young accompanied by their peers on country roads, to concentrate while driving. As such there was no direct enforcement activity associated with this advertising campaign. Details of that part of the project which attempted to evaluate this campaign are given in Cameron et al (1993).

The project identified a link between the Adstock of publicity with the *Concentrate or Kill* theme and levels of awareness of the need for drivers to concentrate. It was envisaged that the statistical analysis would link numbers of crashes with advertising exposure levels to measure the extent to which the *Concentrate or Kill* advertising led to a reduction in the number (or severity) of the group of crashes that were targetted. However it did not appear possible to identify valid target groups of drivers or crashes which were large enough to make this link satisfactorily. Therefore, the analysis was restricted to a comparison of crash rates of target and control groups before and after the introduction of each of the advertisements.

For both advertisements using the concentration theme, there was no reliable evidence of reductions in the risk of serious casualty crashes involving the target groups of the advertisements after the commencement of the advertising campaigns. These findings could have resulted from the crash numbers being too small to show statistically significant reductions or from the effect of the advertisements being relatively small. A number of characteristics of the *Concentrate or Kill* campaign which may have affected its effectiveness are outlined in the third part of Cameron et al (1993).

8. DISCUSSION

8.1 Low alcohol hour crashes in Melbourne

A relationship between the monthly levels of TAC television advertising (all themes) and the reductions in low alcohol hour casualty crashes in Melbourne was originally found as part of an evaluation of the Speed Camera Program (enforcement and publicity). The study reported here found a stronger relationship between the same type of crash reductions and the monthly levels of publicity with "speeding" or "concentration" themes, using the same methods of analysis as the earlier evaluation. It is possible that the earlier inclusion of "drink-driving" publicity may have partially masked the effect on low alcohol hour crashes, which are likely to have relatively low levels of alcohol involvement and hence are unlikely to be influenced by this type of publicity.

While the relationship with the monthly levels of "speeding" or "concentration" publicity is stronger, it is also subject to higher levels of uncertainty. Nevertheless, the mechanism of the effects on the low alcohol hour crashes appears more likely to be related to these themes than to the TAC publicity of any type.

8.2 High alcohol hour crashes in Melbourne and country Victoria

The experience developed in gaining an understanding of the effect mechanisms of the Speed Camera Program suggested that the same analysis methods may be used to investigate

the existence of relationships between high alcohol hour crashes and TAC advertising levels, especially advertising with drink-driving themes. While the methods involve a number of assumptions, in general they have been found to be reasonable ones in the context of trends in road trauma numbers.

Two independent analyses have demonstrated a link between monthly levels of TAC drink-driving advertising on television (measured by the Adstock of current and previous advertising) and reductions in crashes during the high alcohol hours of the week. Statistically significant relationships were found with serious casualty crashes in both Melbourne and country Victoria, after monthly variations in a number of other major explanatory variables were taken into account. This replication of essentially the same finding suggests that the evidence for a link is robust.

However it should be noted that a high level of television advertising at the beginning of a new campaign may be necessary to achieve these effects. The TAC launches of each new advertisement had this characteristic, which may be an essential part of establishing in real life the relationships between publicity levels and casualty crash reductions observed in the study.

In the regression models of monthly numbers of serious casualty crashes during high alcohol hours, the level of alcohol sales was highly to very highly statistically significant, but the number of random breath tests was (highly) statistically significant only in Melbourne. The contribution of reduced alcohol sales to reductions in this type of crash is not surprising given its almost direct relationship with drink-driving. The question is whether increased random breath testing led to the reduced alcohol sales (at least in part), and whether this was an indirect effect of the enforcement activity in addition to the direct effect measured by its regression coefficients (see Table 4). It was not possible to address this question using the analysis methods described here, but it may be possible using alternative methods.

8.3 General remarks

The results of the project confirm those of many other studies which have examined road safety programs which combine Police enforcement and publicity regarding the enforcement activity or the danger of the behaviour being enforced. Elliott (1993), when reviewing available literature on the effects of road safety mass media campaigns, found that the combination of publicity and enforcement is more effective than other campaigns. It would seem that the simultaneous enforcement of the behaviour being publicised adds to the incentive of the audience to respond to the publicity and to change their behaviour appropriately.

The project was unable to find reliable evidence of a positive effect of the *Concentrate or Kill* campaign, except that it appeared to increase awareness of the need for drivers to concentrate. Notwithstanding the difficulties in conducting an evaluation of this campaign, there is a suggestion that its effects were relatively small. This campaign has the disadvantage that the behaviour it aims to promote is not one that can be legislated nor enforced.

9. CONCLUSIONS

In general, the research indicates clear links between levels of TAC publicity supporting the drink-driving and speeding enforcement programs and reductions in casualty crashes when other major factors are held constant.

The road safety effects of TAC publicity with themes not related to enforcement (ie. concentration) is less clear. The *Concentrate or Kill* advertisements appear to raise awareness of the issue, but there is no conclusive evidence at this stage that they have reduced the crash involvement of the specific target group of the advertisements, namely young drivers on country roads.

10. ASSUMPTIONS

The results described in this paper were based on the following major assumptions:

- (a) The form of the relationships fitted to monthly numbers of casualty crashes occurring in Victoria during the "low" and "high" alcohol hours, respectively, in 1983-92 was correct (see Appendices A and B, respectively).
- (b) The explanatory variables included (where appropriate, considering the time of week of the crashes being modelled) in the fitted relationships in addition to levels of TAC road safety television publicity (measured by TARPs and Adstock), namely monthly levels of speed camera operating hours, speed camera TINs issued, random breath tests, alcohol sales, unemployment rates, trend and seasonality variables, did not omit any other major variables with substantial effects on casualty crashes during the corresponding periods of the week.
- (c) The correlations between monthly levels of TAC television publicity and other variables included in the fitted relationships did not have a major effect on the estimates of the publicity impacts.
- (d) The decay in awareness of the road safety messages from the television advertising follows a negative exponential function with a half-life of five weeks.
- (e) The cumulative awareness of current and previous advertising, considered to be measured by Adstock, did not reach a level where satiation of the messages or "wear-out" occurred in any month during 1989-92, resulting in a relatively low level of effectiveness of the advertisements compared with expected.

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APPENDIX A

EVALUATION OF SPEED CAMERA PROGRAM PHASE 2: EFFECTS OF PROGRAM MECHANISMS MODELS FITTED TO MONTHLY LOW ALCOHOL HOUR CASUALTY CRASHES

1. Model for Casualty Crashes

$$\text{CASUALTY CRASHES} = a \begin{matrix} x (e^{\text{TREND}})^b & (1 \text{ in Jan 1983, 2 in Feb 1983, } \dots) \\ x \text{ URATE}^c & (\text{unemployment rate during month}) \\ x \text{ TINS}^d & (\text{speed camera TINs issued}) \\ x \text{ HOUR}^e & (\text{hours of camera operations}) \\ x \text{ TARPS}^f & (\text{TAC road safety publicity TARPs}) \\ x \text{ FEB}g & (\text{dummy for February months}) \\ : \\ : \\ x \text{ DEC}h & (\text{dummy for December months}) \end{matrix}$$

2. Model for Injury Severity of Casualty Crashes

$$\text{Severity Ratio} = \frac{\text{Fatal plus Serious Injury Crashes}}{\text{Minor Injury Crashes}}$$

$$\text{SEVERITY RATIO} = a \begin{matrix} x (e^{\text{TREND}})^b \\ x \text{ URATE}^c \\ x \text{ TINS}^d \\ x \text{ HOUR}^e \\ x \text{ TARPS}^f \\ x \text{ FEB}g \\ : \\ : \\ x \text{ DEC}h \end{matrix}$$

APPENDIX B

MODELS FITTED TO MONTHLY HIGH ALCOHOL HOUR SERIOUS CASUALTY CRASHES (SCCs)

1. Model for TAC Publicity TARPs

$$\text{MONTHLY SCCs} = a \begin{matrix} x (e^{\text{TREND}})^b & (1 \text{ in Jan 1983, 2 in Feb 1983, } \dots) \\ x \text{ URATE}^c & (\text{unemployment rate during month}) \\ x \text{ RBT}^d & (\text{RBT tests}) \\ x \text{ ALC}^e & (\text{alcohol sales}) \\ x \text{ TARPS}^f & (\text{TAC publicity TARPs}) \\ x \text{ FEB}g & (\text{dummy for February months}) \\ : \\ : \\ x \text{ DEC}h & (\text{dummy for December months}) \end{matrix}$$

2. Model for Adstock of TAC Publicity

$$\text{MONTHLY SCCs} = a \begin{matrix} x (e^{\text{TREND}})^b & (1 \text{ in Jan 1983, 2 in Feb 1983, } \dots) \\ x \text{ URATE}^c & (\text{unemployment rate during month}) \\ x \text{ RBT}^d & (\text{RBT tests}) \\ x \text{ ALC}^e & (\text{alcohol sales}) \\ x \text{ ADSTOCK}^f & (\text{TAC publicity Adstock}) \\ x \text{ FEB}g & (\text{dummy for February months}) \\ : \\ : \\ x \text{ DEC}h & (\text{dummy for December months}) \end{matrix}$$

Paper No. 14

M.H. Cameron, A.P. Vulcan, C.F. Finch and S.V. Newstead,
"Mandatory bicycle helmet use
following a decade of helmet promotion
in Victoria, Australia
- An evaluation".

Accident Analysis and Prevention,
Vol. 26, No. 3, 1994.

Paper No. 14



MANDATORY BICYCLE HELMET USE FOLLOWING A DECADE OF HELMET PROMOTION IN VICTORIA, AUSTRALIA—AN EVALUATION

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Abstract—On July 1, 1990, a law requiring wearing of an approved safety helmet by all bicyclists (unless exempted) came into effect in Victoria, Australia. Some of the more important steps that paved the way for this important initiative (believed to be the first statewide legislation of its type in the world) are described, and the initiative's effects are analysed. There was an immediate increase in average helmet-wearing rates from 31% in March 1990 to 75% in March 1991, although teenagers continued to show lower rates than younger children and adults. The number of insurance claims from bicyclists killed or admitted to hospital after sustaining a head injury decreased by 48% and 70% in the first and second years after the law, respectively. Analysis of the injury data also showed a 23% and 28% reduction in the number of bicyclists killed or admitted to hospital who did not sustain head injuries in the first and second post-law years, respectively. For Melbourne, where regular annual surveys of helmet wearing have been conducted, it was possible to fit a logistic regression model that related the reduction in head injuries to increased helmet wearing. Surveys in Melbourne also indicated a 36% reduction in bicycle use by children during the first year of the law and an estimated increase in adult use of 44%.

INTRODUCTION

The state of Victoria has a population of about 4.4 million and 605 registered vehicles per 1,000 population. Melbourne is the largest city in Victoria, with a population of about 3 million. In 1992, 396 people were killed on the road, corresponding to a fatality rate of 0.91 deaths per 100 million km travelled. Generally, bicyclists represent about 4% of road deaths.

Background to the law

A climate that favoured the promotion of bicycle helmets with an ultimate goal of compulsory use had been created in Victoria during the 1960s and 70s. All motorcyclists were required by law to wear an approved helmet since 1961. The government had achieved considerable success in reducing road trauma through restrictive legislation supported by publicity and enforcement, e.g. compulsory seat belts (1970), child-restraint use laws (1975 and 1981), random breath testing (1976), and engine capacity limits for novice motorcyclists (1979) (Vulcan 1990).

By the mid-1970s, a few pedal bicyclists were

wearing bicycle helmets of unknown protective performance, although some imported helmets met overseas standards. It was not until 1981 that the first helmet was certified as meeting the 1977 Australian Standard for protective helmets.

Introduction of the mandatory-bicycle-helmet-use law in July 1990 was preceded by a decade of helmet promotion involving education, mass media publicity, support by professional associations and community groups, consultation with bicycle groups, and financial incentives. Some of these activities are discussed below and in greater detail elsewhere (Wood and Milne 1988; Leicester, Nassau, and Wise et al. 1991; Vulcan, Cameron, and Watson 1992b).

School support. In 1980, a bicycle safety education unit (Bike-Ed) for use by students aged 9–13 years was developed. Promotion of helmets was achieved by this unit in schools and an associated film on safer riding. From 1983, the Education Department required a helmet to be worn in all state school bicycling activities. A few private schools required that students wear helmets when bicycling to school.

In 1982, a bulk helmet purchase scheme was established in one Education Department region, and parents were able to order helmets through schools at a cost of \$30 (approximately 33% discount). All 1,000 helmets underwritten by this scheme were sold and there was demand for more. The pilot scheme clearly demonstrated that a demand for helmets could be generated by such discount schemes. It led to many other bulk purchase schemes during the next two years, resulting in the purchase of at least 20,000 helmets.

Mass media publicity. A mass media publicity campaign was undertaken during 1984, using two television commercials supported by radio and a pamphlet. The campaign was based on extensive research and was targeted at parents of primary-school-aged children. It emphasised the seriousness of head injury among bicyclists and the protection provided by helmets.

Consultation and promotion. In order to coordinate and support a wide range of further helmet promotion activities, the Road Traffic Authority established a helmet promotion task force with membership including representatives of bicyclists, motorists, police, education, community safety organisations, helmet manufacturers and retailers, and doctors. The group grew from an earlier one initiated by the Royal Australasian College of Surgeons, which had been actively promoting mandatory helmet use and helmet subsidies for several years.

Two Australian studies describing the protective aspects of helmets were also useful in promotion (McDermott and Klug 1982; Dorsch *et al.* 1984). Later studies provided further useful information about the effectiveness of helmets in reducing injuries (Wood and Milne 1988; Thompson, Rivara, and Thompson 1989; Williams 1991).

Helmet rebate scheme. Early in December 1984, the Minister for Transport announced that the Government would pay a rebate of \$10 to all purchasers of an Australian-made, Standards Association-approved bicycle helmet. The scheme was to operate for purchases made from 11 to 29 December, which was traditionally the peak time for helmet sales. The television and radio publicity campaign was rerun during this period with mention of the \$10 rebate added. A total of more than 37,000 rebates were paid, and many others purchased helmets but did not bother to claim.

Further \$10-rebate schemes were undertaken in December 1985, 1986, and 1987 and from December to February in 1988 and 1989. These resulted in a total of more than 168,000 helmet rebates from all schemes (Leicester *et al.* 1991).

The Parliament. Although the Minister for Transport in October 1984 said during Parliament that

the Government embarked on a course of action to encourage substantial numbers of school children to wear safety helmets so that in due course the wearing of safety helmets can be made compulsory, firstly, because many people will already be using them and, secondly, because of a much larger production run safety helmets will be considerably cheaper than they are at present. (Hansard 1984),

it took a further five-and-a-half years for the law to be introduced. During this period there were many additional promotional activities, but probably the single most significant contribution was the report of an Inquiry into Child Pedestrian and Bicycle Safety by the Social Development Committee of the Victorian Parliament.

In their first report on the Inquiry, tabled in December 1986, the committee recommended, *inter alia*, "that mandatory helmet use by bicyclists be introduced as soon as possible." The committee also recommended that a report be prepared with a detailed strategy to introduce mandatory helmet usage, including consideration of "the impact of such legislation on the diverse categories of bicycle users" and "methods of reducing costs of bicycle helmets to users" (Social Development Committee 1986).

The final steps. Following extensive canvassing of comment from bicyclists and the community in general, and a detailed review of the key issues, the Road Traffic Authority developed a strategy in December 1987 that recommended that legislation be introduced to require the mandatory wearing of bicycle helmets, from 1 January 1989.

After further deliberation, the Minister for Transport and the Minister for Police and Emergency Services announced, as part of a package of road safety initiatives in September 1989, that a new regulation requiring bicyclists to wear approved helmets would take effect in Victoria from 1 July, 1990.

Helmet standards. Bicycle groups had been pressing for several years for lighter, cooler helmets. The Australian Standard was amended in 1990 by deleting the penetration test which allowed the lighter, soft-shell helmets and larger holes to enable better cooling air circulation (Leicester *et al.* 1991). This overcame another important objection to the wide scale use of helmets at all times.

The law

The mandatory helmet wearing law has been implemented through the Road Safety Bicycle Helmets Regulations 1990, under the Road Safety Act of 1986. It requires all persons bicycling on the road,

on a footpath, on a separate bicycle path, or in a public park to wear a securely fitted, approved bicycle helmet. It also applies to bicycle passengers (e.g. children in child seats).

Exemptions are available mainly on the basis of extreme difficulty in complying. In practice, they are difficult to obtain and it is understood that fewer than 50 were granted during the first year after the law.

The maximum penalty of \$100 for not wearing a helmet is rarely applied, as offenders are not normally taken to court, but rather a Bicycle Offence Penalty Notice for \$15 is issued. For children, a Bicycle Offence Report (no monetary penalty) may be sent to the parents.

Enforcement. The number of Bicycle Offence Penalty Notices increased from 2,836 to 19,229 between 1989/90 and 1990/91 (i.e. 1 July to 30 June), while Bicycle Offence Reports increased from 1,743 to 5,028 over the same period. Whilst it is likely that most of this increase relates to helmet-wearing offences, the extent to which other bicycling offences also increased is not known. In the context of a bicyclist population of over 2 million, this could be regarded as a relatively moderate level of enforcement.

METHODS AND MATERIALS

Surveys of helmet wearing

In order to measure progress in helmet wearing due to various educational and promotional activities during the 1980s, VIC ROADS (the trading name of the Roads Corporation of Victoria, the state agency responsible for road and traffic matters, including safety) conducted a series of observation surveys at specific sites in February/March each year. Surveys of commuting cyclists were initially conducted in the metropolitan Melbourne area and several country cities were added later (Sullivan and Wise 1990; Morgan, Peberdy, and Rogerson 1991). The Melbourne metropolitan surveys were of adult commuter bicyclists on arterial roads near the central business district and of primary (aged 5–11) and secondary (aged 12–17) school students on the approaches to a sample of schools. It is possible that the student surveys could be biased towards higher wearing rates (Heiman 1987) as it is known that some students only wear their helmets when leaving home and on approaching school. In 1987, the surveys were extended to include recreational cycling, namely in residential streets between 4 p.m. and 6 p.m. on weekdays, and in places and during periods that varied from survey to survey on weekends. Full details of the methodology used in the annual

surveys conducted during 1983–1991 are given in Wise (1989) and Morgan *et al.* (1991).

Table 1 summarises the overall (all ages combined) helmet-wearing rates observed during each of the annual VIC ROADS surveys. It should be noted that because these surveys were generally conducted in March of each year, the 1991 results reflect the situation some eight months after the helmet wearing law was introduced. Some additional surveys in July and November 1990 reported even higher post-law wearing rates, particularly for secondary-school students, but these surveys have not been included here because of their limited size.

A series of observational surveys of bicycle usage and helmet wearing was also conducted by the Monash University Accident Research Centre (MUARC) before and after the introduction of the law. Each survey collected data on bicycle use and helmet wearing from a representative sample of bicyclists observed in metropolitan Melbourne during a two-week, nonholiday period over 8 A.M.–6 P.M., seven days a week. Both commuter and recreational bicycling were included, although data were not collected in rural Victoria. Identical survey methodology was adopted during each of the surveys and full details are available in Finch *et al.* (1993). Table 2 summarises the MUARC series of surveys.

Estimates of overall helmet-wearing rates in metropolitan Melbourne

The VIC ROADS helmet-wearing rates had been collected annually during 1983–1991 and covered a longer period than the MUARC surveys. For this reason, estimates of overall helmet-wearing rates in metropolitan Melbourne were calculated from the separate VIC ROADS survey results in Melbourne by combining the commuting and recreational rates. This was achieved by calculating weighted averages of helmet-wearing rates amongst commuter and recreational bicyclists based on the relative proportions of commuter and recreational bicyclists from the 1987/1988 (Drummond and Jee 1988) and 1991 (Finch *et al.* 1992) MUARC exposure surveys. The proportions in the 1987–1988 survey were applied to each of the prelaw years. Recreational wearing rates before 1987 were estimated from the commuter bicyclist rates by assuming that the ratio of recreational to commuting wearing rates during 1983–1986 was the same as that observed during 1987 (Cameron, Heiman, and Neiger 1992).

Estimation of helmet wearing rates in 1992. Because the series of VIC ROADS surveys finished in 1991, an estimate of helmet-wearing rates in 1992 was available only from the MUARC data. However, for consistency, an estimate of the "likely"

Table 1. Number of bicyclists (all ages) and helmet wearing rates (%) observed in the VIC ROADS surveys in metropolitan Melbourne and country cities: 1983-1991

Year	Metropolitan Melbourne				Country cities			
	Commuter		Recreational		Commuter		Recreational	
	n	Helmet wearing rate (%)	n	Helmet wearing rate (%)	n	Helmet wearing rate (%)	n	Helmet wearing rate (%)
1983	2957	3						
1984	1728	14						
1985	1698	28			4498	16		
1986	2926	30			4408	25		
1987	2865	34	537	13	4242	32	977	15
1988	5925	35	5406	16	7849	21	2723	15
1989	5444	39	7274	20	7504	23	4279	17
1990	6855	43	8777	31	11598	23	7453	13
1991	5781	79	7604	70	11464	81	9068	74

VIC ROADS helmet-wearing rate needed to be obtained.

Comparison of the VIC ROADS and MUARC estimates for the years 1990 and 1991 consistently found higher rates for the VIC ROADS surveys. This may be explained partly by the fact that the VIC ROADS surveys of adult commuter cycling were conducted on arterial roads near Melbourne's central business district, generally in March, while the MUARC surveys were taken at randomly selected sites throughout Melbourne, generally in May. An estimate of the 1992 VIC ROADS helmet-wearing rate was obtained by adjusting the MUARC 1992 rate by a factor representing the average difference between the two surveys' rates in earlier years.

Estimates of Victoria-wide helmet wearing rates. Commuting and recreational surveys for rural cities (those Victorian cities outside the Melbourne metropolitan area) were combined in the same way as those for Melbourne to obtain an overall estimate of helmet-wearing rates in rural Victoria (Cameron et al. 1992). Rural rates prior to 1985 were estimated from the Melbourne rates by assuming that the ratio of rural to Melbourne rates during 1983 and 1984 was the same as that calculated during 1985.

The estimated wearing rates for Melbourne and

Table 2. Number of bicyclists (all ages) and helmet-wearing rates (%) observed in the MUARC surveys in metropolitan Melbourne: 1987/1988-1992

Survey	No. of observation sites	No. of cyclists	Helmet-wearing rate %
Nov-Jan 1987/1988	105	5837	24
May 1990	80	3709	31
May 1991	64	2011	64
May 1992	64	2477	76

rural cities were combined in the ratio 70:30 to produce overall wearing rate estimates for Victoria from 1983 to 1991. This ratio corresponds to the population distribution of the state during 1986-1990 and also to the distribution of bicycle use estimated from a telephone survey in 1989 (State Bicycle Committee 1991). A Victoria-wide estimate of helmet wearing could not be obtained for 1992 because of the lack of rural surveys in that year.

Bicycle use

The observational surveys conducted by MUARC provided estimates of bicycle use in metropolitan Melbourne. Bicycle use was assessed by timed measurements of individual bicyclist exposure scaled up to provide an estimate of total bicycle use in metropolitan Melbourne (in seconds of bicycling) (Drummond and Jee 1988; Drummond and Ozanne-Smith 1991; Finch et al. 1993).

Bicyclist injuries

The effects of the law on bicyclist head (excluding face) injuries were measured by examining claims for "no fault" injury compensation from bicyclists who were killed or hospitalised (i.e. severely injured) after a collision with a motor vehicle. The Transport Accident Commission (TAC), the sole insurer for such claims in Victoria, provided this information.

The claims records for killed or hospitalised bicyclists in Victoria were classified by TAC according to injury type on the basis of up to five recorded injuries using the ICD-9 system. Bicyclists were broadly classified as those who sustained a head injury (whether or not there were other injuries present), those who did not sustain a head injury (referred to as "other injury") and those with unknown injury information. Head injuries were those

assigned N-codes 800, 801, 803, 850-854, 872, 873.0, 873.1, 873.8 and 873.9, following Healy (1986).

Vulcan et al. (1992a) analysed acute presentations by bicyclists to all public hospitals in Victoria resulting in admission, including admissions from collisions not involving a motor vehicle. The results were consistent with those found for TAC claims, indicating that the increase in helmet use had similar effects on bicyclist collisions not involving motor vehicles. This suggests that the omission of non-motor-vehicle/bicyclist collisions from the present analysis is not likely to lead to misleading conclusions about the influence of increased helmet wearing.

A limitation of the available annual injury data is that the corresponding number of bicyclists in Victoria is not available. This means that injury rates per bicyclist population could not be computed. Assessment of changes over time was therefore based on an examination of the proportion of head-injury cases amongst those with known injury information.

Statistical methods

Wearing rates. The effects of the introduction of mandatory helmet use in 1990 on helmet-wearing rates and the risk of head injury were assessed by logistic regression techniques (BMDP 1988). One property of the logistic model is that it constrains the dependent variable (in this first case, the helmet-wearing rate) to be between 0% and 100%. For this reason, it is the most appropriate technique to apply to data of the type analysed here.

For assessing the influence of the law on helmet-wearing rates, the model included two independent variables: one was a dummy variable indicating the postlaw period, and the other represented the year of the survey of helmet wearing. The logistic regression procedure also considered the interaction between these two variables to assess whether the rate of change in helmet-wearing rates increased or decreased after the introduction of mandatory helmet use in 1990. In addition, the logistic model was used to provide an estimate of the helmet-wearing rate in the absence of the law. By comparing this with the actual helmet-wearing rates in 1991 and 1992, an estimate of the additional benefit of the law, on top of the ongoing helmet promotion activity, was obtained.

Head injuries. A second logistic regression model was used to describe the relationship between head-injury risk and helmet-wearing rates. The dependent variable, in this case, was taken to be the proportion of all cases with head injury amongst the group of severely injured crash-involved bicyclists with known injuries. In addition to the use of estimated helmet-wearing rates as an explanatory variable, a dummy variable indicating the postlaw period

was considered in the model. The purpose of this was to provide an assessment of whether the relationship between head injuries and helmet wearing was different before and after the law, independent of the magnitude of helmet wearing in the community, or whether other factors associated with the law might be related to head-injury trends. An interaction term between these two explanatory variables was also considered.

To assist the interpretation of the logistic regression findings, the estimated helmet-wearing rates that would have occurred in the absence of the law were used to estimate the proportion of head-injury cases in the postlaw period. Comparison of these estimates with the observed proportion of head-injury cases was then used to assess the influence of increased helmet wearing on bicyclist head injuries.

RESULTS

Helmet wearing rates

Figures 1 and 2 show the helmet-wearing rates observed in the VIC ROADS surveys in Melbourne, during 1983-1991, for commuting (including to/from school) and recreational bicyclists, respectively. The substantial increase in helmet wearing over this period by all age groups on both types of trips is clearly shown.

Estimates of overall helmet use by bicyclists in Melbourne, during 1983-1992 are indicated in Fig. 3 (heavy solid line). Overall helmet-wearing rates rose gradually from 6% in March 1983 to 36% in March 1990. After introduction of the law, they had increased to 73% by March 1991 and are estimated to have risen further to 83% by May/June 1992. These trends in overall wearing rates are used in the interpretation of the changes in bicyclist head injuries described in the following section.

Figure 3 also displays the logistic regression model fitted to the observed data (solid line). The level of agreement between the two solid lines indicates how well the logistic model describes the observed data. The logistic model included a term representing the survey year ($p < 0.001$), as well as a term measuring the influence of the law's introduction in July 1990 ($p < 0.001$). An interaction term between these variables was also fitted and found to be statistically significant ($p < 0.001$) suggesting that the rate of increase in helmet-wearing rates after the law was greater than before the law. The dashed line in Fig. 3 indicates the increasing trend in helmet wearing that would have been expected if the law had not been introduced and if the logistic model before that time represented the true trend. Clearly, the actual helmet-wearing rates far exceed those that

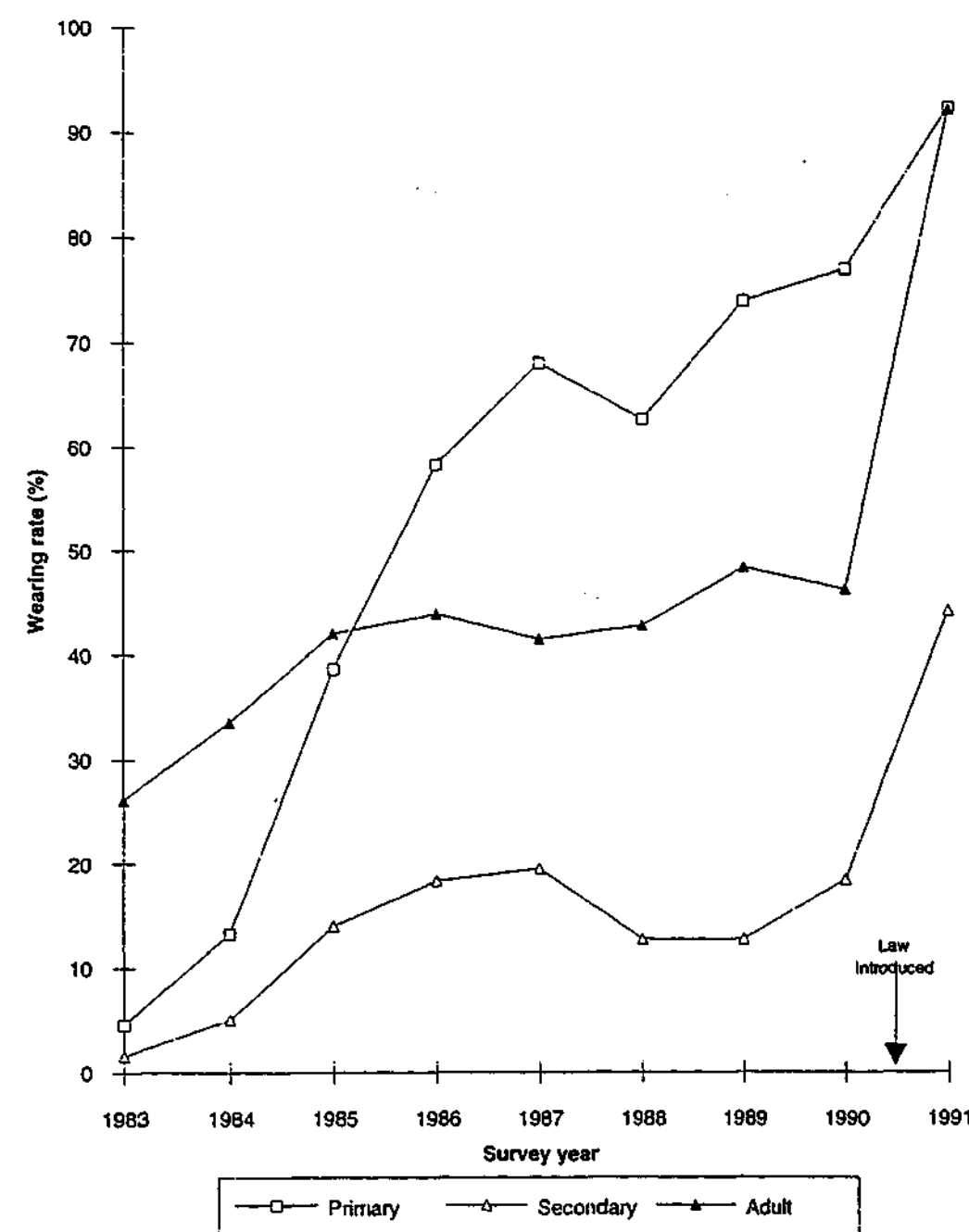


Fig. 1. Age-specific helmet-wearing rates amongst commuting bicyclists in metropolitan Melbourne: 1983-1991.

would have been expected if the 1990 mandatory use law had not been introduced—actual helmet wearing rates in 1991 and 1992 were 1.8 times those expected.

Victoria-wide helmet-wearing rates rose from 31% in 1990 to 75% in 1991 following introduction of the helmet-wearing law. Helmet wearing throughout Victoria could not be estimated for 1992, however, because of the lack of rural surveys in that year.

Taken together, the increases in helmet-wearing rates after introduction of the law in Victoria are remarkable, especially since there had been relatively moderate enforcement up to the time of the surveys. They confirm that if the community understands the benefits of a safety measure, and a reasonable proportion has already been persuaded to adopt

it voluntarily, then considerably increased use can be achieved through a law, even with relatively moderate levels of enforcement.

Bicyclist injuries in Melbourne

Figure 4 shows that, based on TAC injury claims, the number of bicyclists killed or admitted to hospital with a head injury in Melbourne fell progressively between July 1981 and June 1990 as the use of helmets increased. In the 1990/1991 financial year (July 1990 to June 1991), following the introduction of the mandatory wearing law, the number of cases with a head injury decreased by 36% relative to the corresponding period during 1989/1990. By 1991/1992, there were 66% fewer head injury cases

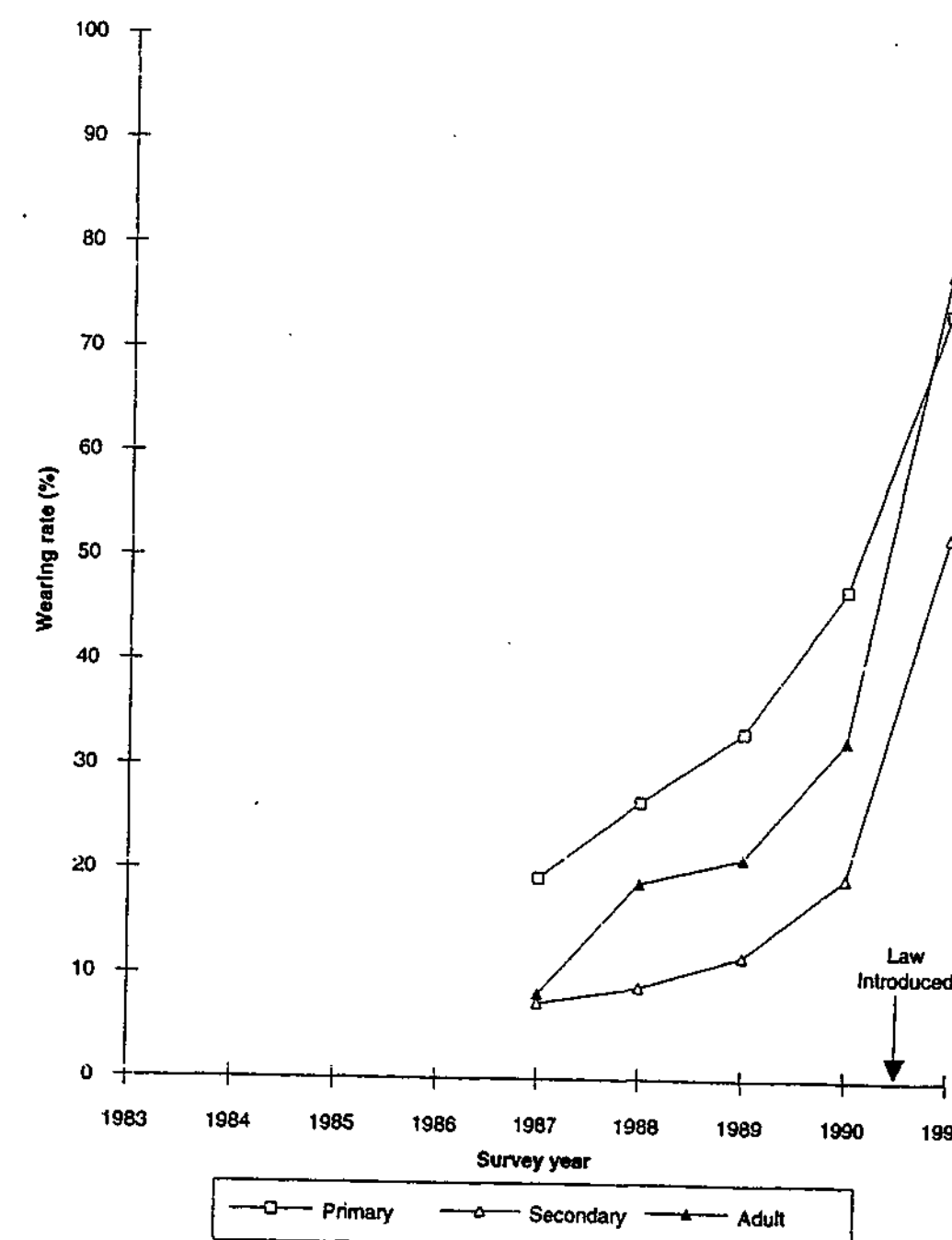


Fig. 2. Age-specific helmet-wearing rates amongst recreational bicyclists in metropolitan Melbourne: 1987-1991.

than in the year before the law. This decrease suggests that, other things being unchanged, the substantially increased level of helmet use due to the law has reduced the risk of head injury to bicyclists.

The trend in the number of Melbourne bicyclists sustaining severe injuries other than to the head increased during the early 1980s, then declined slowly, particularly after 1988 (Fig. 4). During 1990/1991, the number of injured bicyclists without head injuries decreased by 6%, relative to 1989/1990. By 1991/1992, non-head injured cases had declined further to 17% fewer than prelaw. These decreases were somewhat unexpected, because a reduction in head injuries through helmet use would have led to classification of some bicyclists with multiple body-region

injuries as "other injury". They suggest that the number of bicyclists involved in crashes with motor vehicles has decreased during the postlaw period, either due to a reduction in bicycle use or a reduction in the risk of crash involvement. The first of these possible explanations will be examined explicitly later. The second possibility is consistent with the general reduction in police-reported road deaths and serious injuries in Victoria, which fell by 18% between 1989/1990 and 1990/1991 and by 26% between 1989/1990 and 1991/1992.

Because of the fall in non-head injuries, as well as head injuries, the effect of helmet use on bicyclists involved in crashes was addressed by examining the percentage of killed and admitted bicyclists with

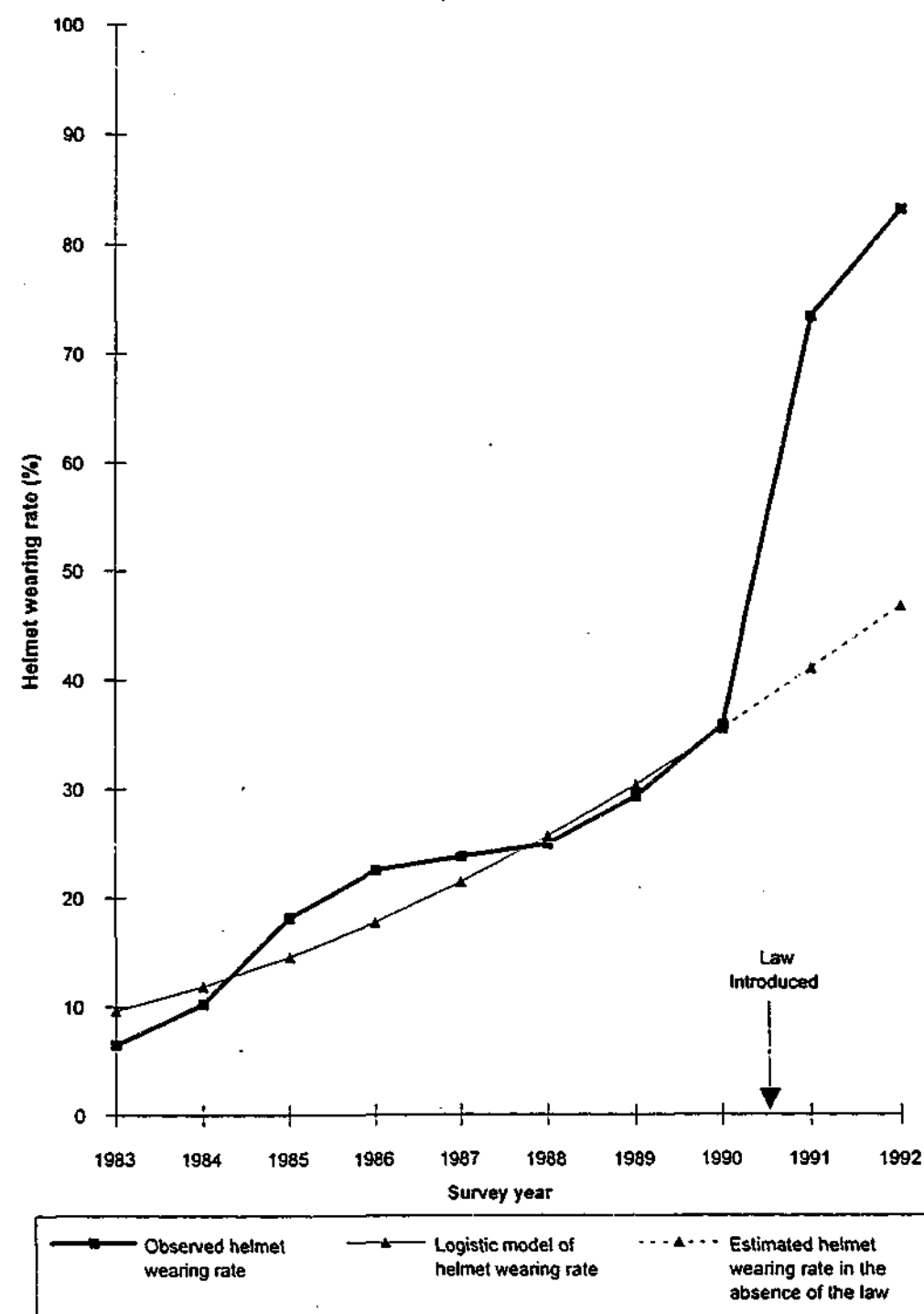


Fig. 3. The influence of the law on overall helmet-wearing rates in metropolitan Melbourne. (All age groups and commuter and recreational bicyclists combined.)

known injuries, who sustained a head injury (Fig. 5). As expected, there was a clear inverse relationship between the proportion of head-injured cases and helmet-wearing rate. The logistic regression model (solid line) describes the relationship with increasing helmet wearing rates (p -value < 0.001). The effect of other factors relating to the 1990 law on the proportion of cases with a head injury, over and above the increased helmet-wearing rates, was assessed by incorporating a dummy variable representing the postlaw years into the logistic regression model. However, neither this variable, nor its interaction

with the helmet-wearing rate, was statistically significant. Both terms were therefore removed from the logistic model shown in Fig. 5.

The proportion of head-injured cases that would have been expected if the law had had no effect on helmet-wearing rates (Fig. 3) and if the prelaw head-injury versus wearing rate trend continued, is also shown in Fig. 5. For 1991, this proportion was estimated to be 28%, compared to the observed proportion of 25%; this difference was not statistically significant. In 1992, on the other hand, the observed proportion of head-injured cases (17%)

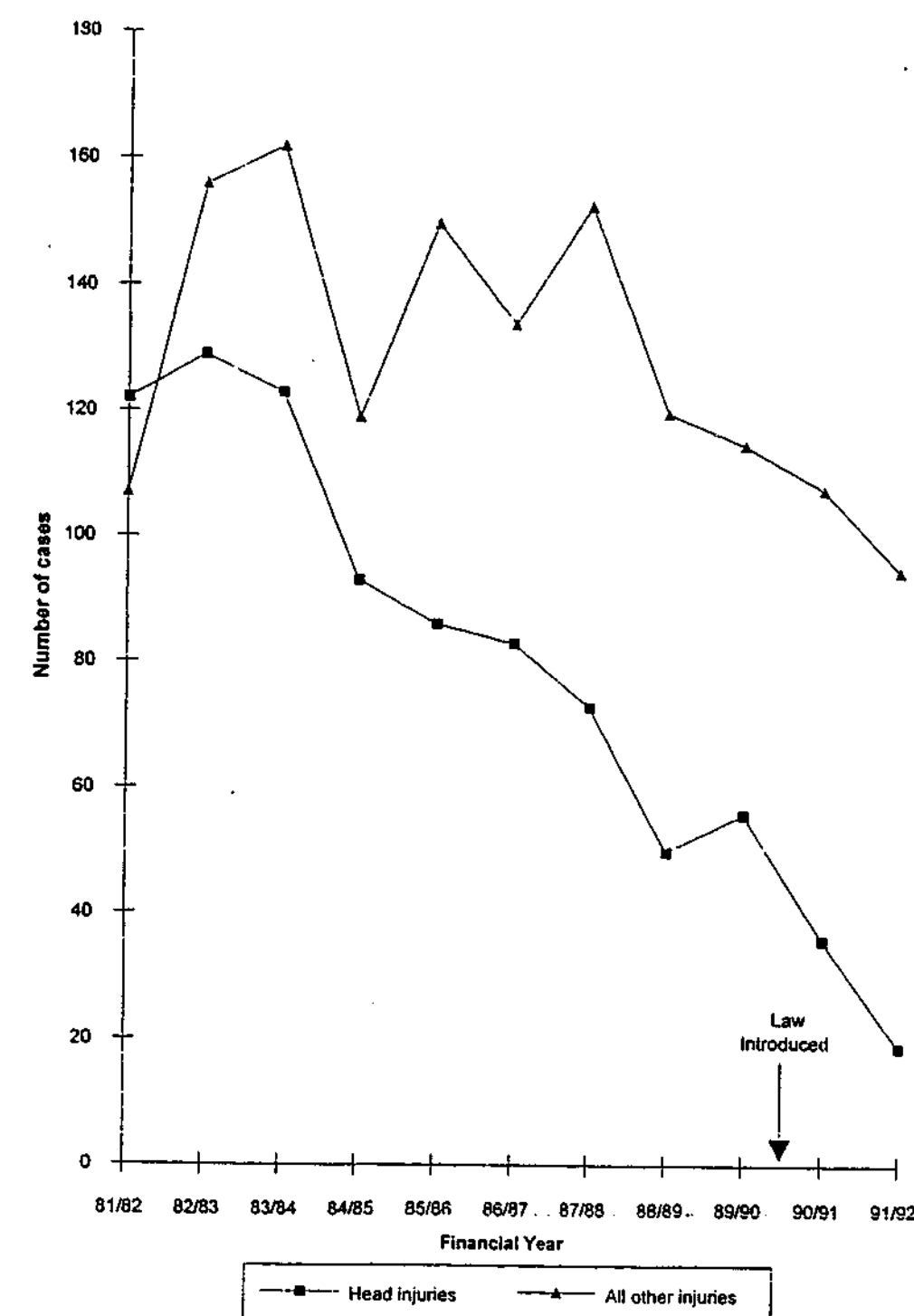


Fig. 4. Severe bicyclist casualties registered with TAC: Metropolitan Melbourne: July 1981–June 1991.

was significantly less (one-sided test, $p = 0.03$) than the estimate based on no effect of the law on wearing rates (25%) (Fig. 6). Figure 6 also suggests that increased helmet wearing during the first postlaw year may not have been as effective in reducing head injuries as it appeared to be during the second postlaw year.

Bicyclist injuries in Victoria

When TAC injury claims for the whole of Victoria were examined, similar, but even larger, reductions were found. The number of killed or hospitalised bicyclists with a head injury fell by 48% between 1989/1990 and 1990/1991 and by 70% between 1989/

1990 and 1991/1992. In addition, the number of cases with severe injuries other than to the head fell by 23% between 1989/1990 and 1990/1991 and by 28% over 1989/1990 to 1991/1992.

No detailed analysis of the relationship between the Victoria-wide risk of head injury and postlaw helmet-wearing rates could be conducted due to the lack of rural surveys of helmet wearing in 1992.

Bicycle use

The reduction in the number of severely injured bicyclists with injuries other than to the head (and some of the reduction in those with head injuries) during the postlaw period may have been due to a

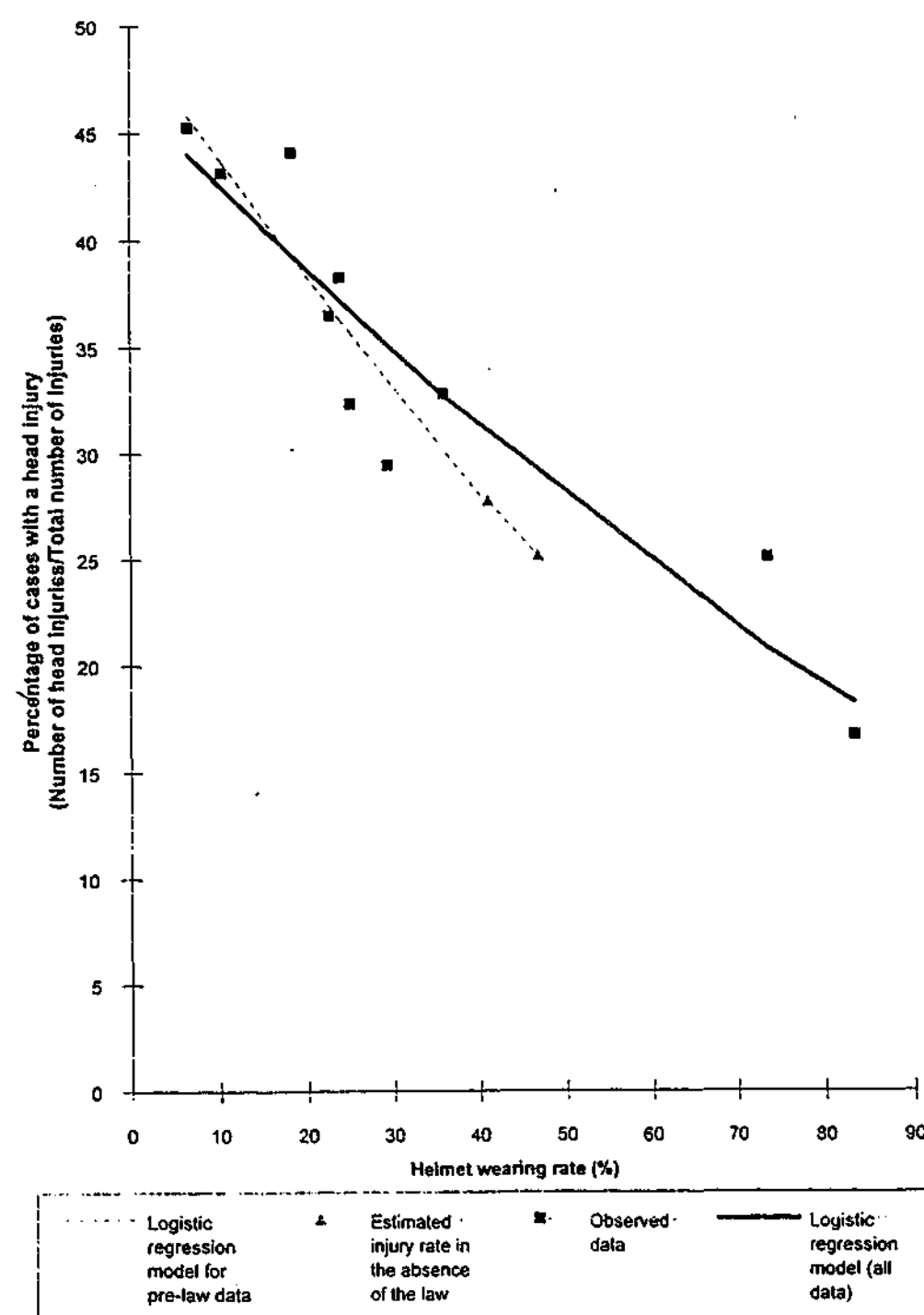


Fig. 5. Relationship between the proportion of cases with a head injury and the helmet-wearing rate (%) in metropolitan Melbourne: July 1981–June 1991.

reduction in bicycle use as well as other factors affecting the risk of accident involvement.

In May of 1991 and 1992, observational surveys were conducted at a representative sample of 64 sites in Melbourne to determine whether bicycle use had decreased since the introduction of the new law. In analysing the effects, comparisons were made with the results of a very similar survey of child bicycling at the same 64 sites conducted in May 1990 just before the mandatory wearing law (Finch et al. 1993).

Figure 7 shows that bicycle usage among teenagers had decreased by 43% by 1991 and 46% by

1992, relative to 1990. Bicycle use among children aged 5–11 years also decreased over the same period—by 3% in 1991 and 11% in 1992, compared to 1990. Because the 1990 survey did not cover adult bicyclists, it was not possible fully to examine the change in their bicycle use. However, there was an increase in adult bicycling of 86% by 1991 and a doubling of usage in 1992 when compared with a survey in November 1987–January 1988 (Finch et al. 1993). When data for all age groups are combined, the total bicycle use in 1991 was 9% greater than in 1987/1988, and in 1992 it increased a further 3%. A

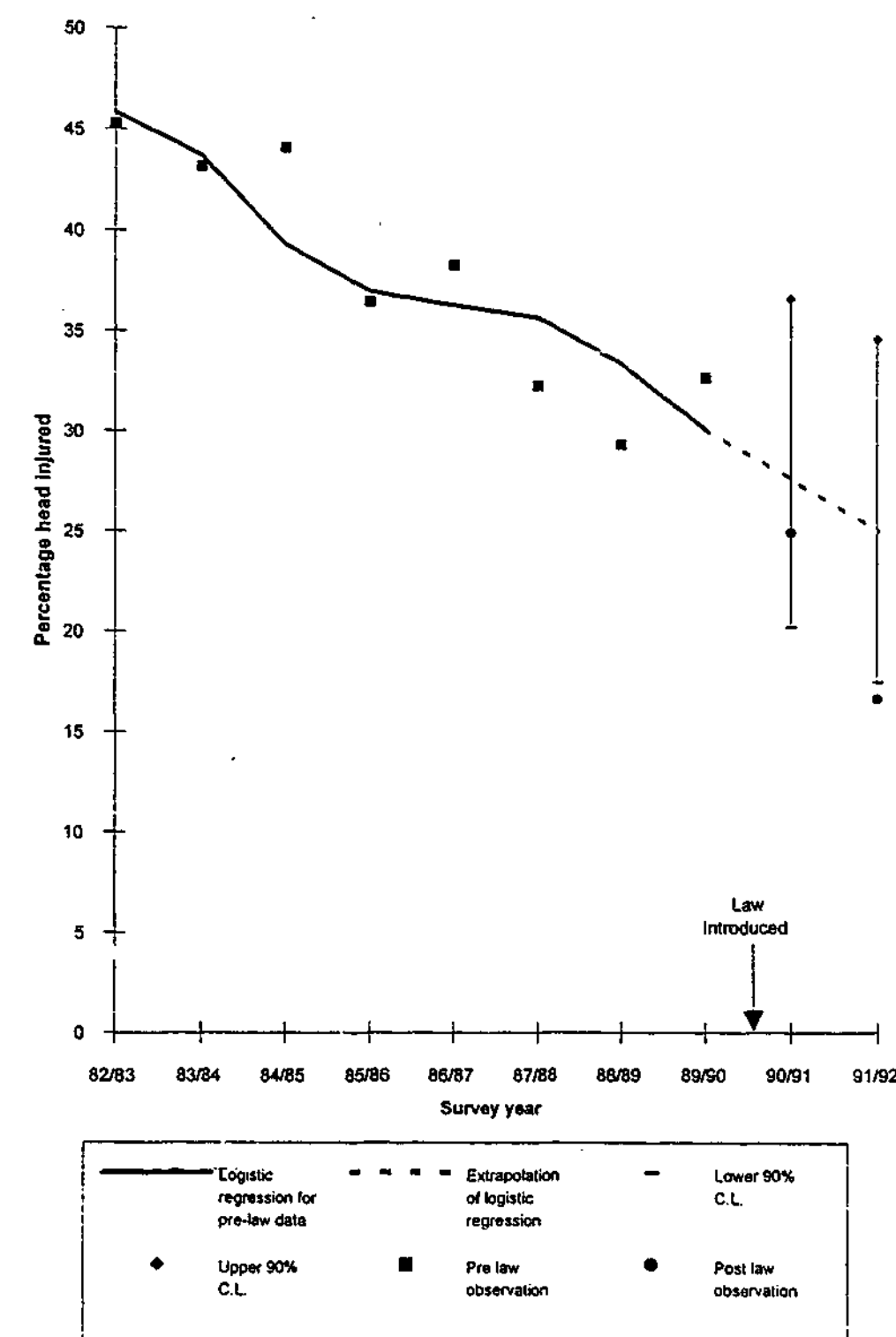


Fig. 6. Logistic regression of cyclist head injuries based on prelaw data. Percentages of head injured cases versus survey year in metropolitan Melbourne. (90% confidence intervals for the postlaw estimates also indicated).

more direct measure of the effect of the law on bicycle use can not be made because adults were not included in the 1990 surveys.

DISCUSSION

There was a progressive increase in helmet-wearing rates amongst commuter and recreational bicyclists of all ages during the prelaw period. Rural and metropolitan Melbourne surveys indicate that these

increases occurred throughout Victoria. In the two postlaw years, the wearing rates were significantly higher than would have been predicted if the previous decade's efforts towards increasing helmet wearing had continued their effect in the absence of the law.

The results show a large reduction (48%) in the number of bicyclists with head injuries during the first year after the introduction of the mandatory-helmet-wearing law in Victoria on 1 July 1990. During the second year after the law's introduction, there were

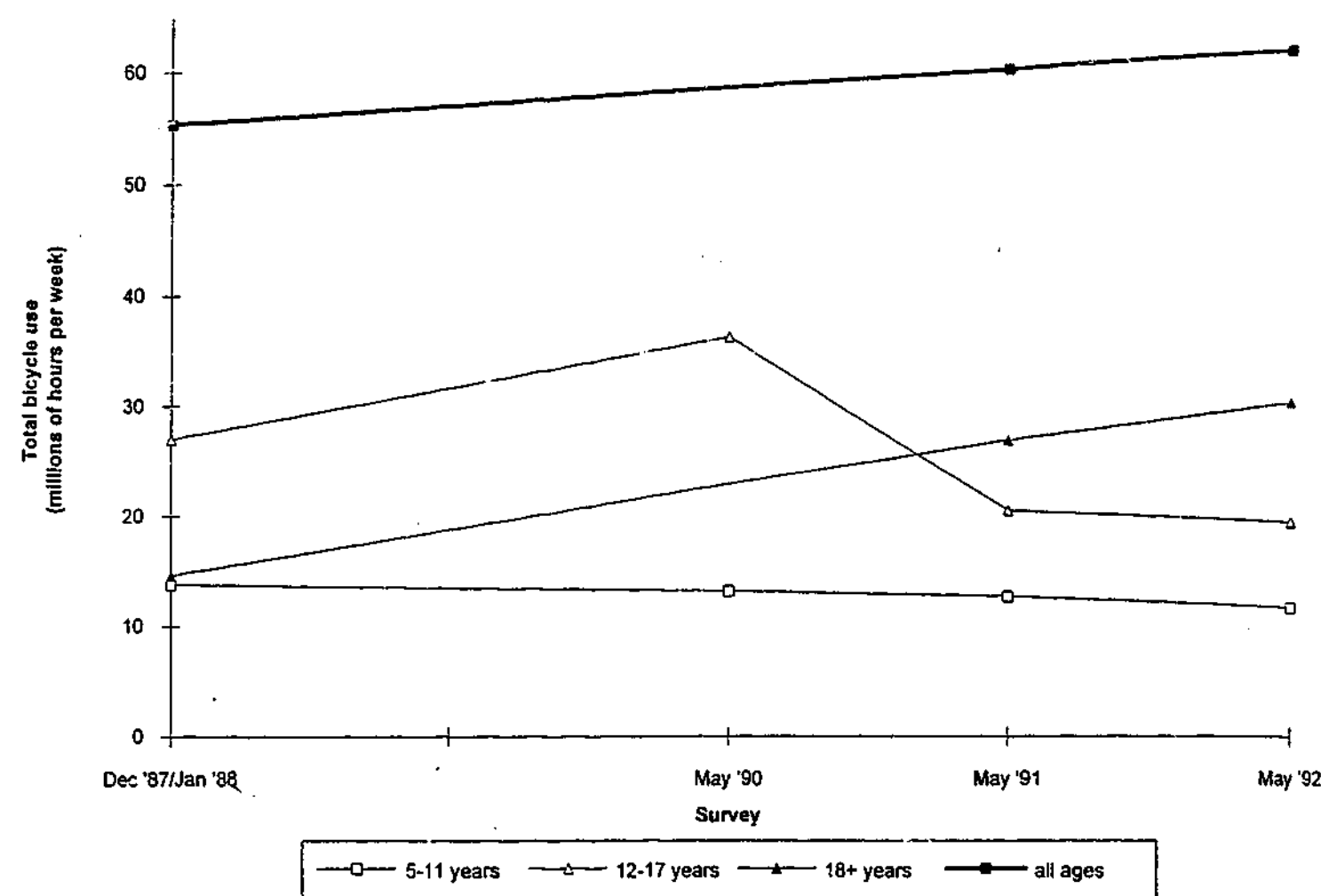


Fig. 7. Age-specific estimated bicycle use in metropolitan Melbourne: 1987–1988–1992. (The symbols indicate where data are available.)

70% fewer head injuries, compared to the 1989/1990 period. However, the mechanisms by which this reduction was achieved seem to be two-fold:

1. a reduction in the number of bicyclists involved in crashes resulting in severe injury (i.e. killed or admitted to hospital), and
2. a reduction in the risk of head injury for bicyclists who were severely injured.

The extent of the reduction in bicyclist claims to TAC for severe injuries other than to the head (23% in 1990/91; 28% in 1991/92; each compared to 1989/1990) supports the first mechanism. In addition, there is evidence that bicycle use by children and teenagers fell by an average of 36% during the first two postlaw years, although use by adults increased markedly. Hence, reduction in bicycle use appears to be a contributor to the reduction in crash involvement, though it is also possible that the wearing of helmets has made bicyclists more conspicuous or that the helmets and associated publicity have made bicyclists ride more carefully.

In addition, major initiatives directed at drink/driving and speeding were introduced in Victoria in

December 1989 and March 1990, respectively. The police-reported total number of people killed or seriously injured resulting from all road trauma during July 1990 to June 1991 was 18% below that for the previous 12 months. During July 1991–June 1992, there were 26% fewer cases than during the 1989/1990 period. This could account for some of the reductions in bicyclist trauma during this period.

There is also evidence that the risk of head injury to bicyclists involved in crashes has been reduced (the second mechanism). The percentage of severely injured bicyclist claimants to TAC who suffered a head injury during the postlaw period was considerably lower than that of prelaw levels. This result was found both for Melbourne alone and for the whole of Victoria.

On the basis of the logistic regression model describing the relationship between the percentage of severely injured bicyclists who sustained a head injury and the helmet-wearing rates, established for the prelaw period, there is an indication that increased helmet wearing in the postlaw period was not as effective in reducing the risk of head injury to crash-involved bicyclists during the first postlaw year compared with the second year.

Finch et al. (1993) found that the proportion of helmet owners wearing their helmets, rather than carrying them, increased between 1991 and 1992, particularly amongst teenagers. In addition, fewer hard-shell helmets were being worn in 1991, compared to 1992, particularly by adults. This may reflect initial enthusiasm following the amendment to the Australian Standard for bicycle helmets in 1990, which allowed lighter, soft-shell helmets. There was no evidence that bicyclists were less likely to secure their helmets in 1991 than in 1992, although it is possible that helmets were less securely adjusted or fastened by those bicyclists who had not previously worn them. Helmeted bicyclists may also have been engaging in more behaviours resulting in more severe impacts in 1991 than in 1992. These observations may help to explain the differences in the apparent effectiveness of helmets in 1991 and 1992. Alternatively, they may also be a result of the assumptions that had to be made in combining a range of helmet-wearing data. Further investigations are being undertaken to clarify these comments.

CONCLUSIONS

The mandatory bicycle-helmet wearing law implemented in Victoria on 1 July 1990 has been successful in building on past efforts to promote helmet use by bringing helmet wearing rates to new high levels for all bicyclist age groups, both in Melbourne and rural cities.

The introduction of the law has been accompanied by an immediate, large reduction in the number of bicyclists with head injuries. Apparently this has been achieved through a reduction in the number of bicyclists involved in crashes (at least partly through a decrease in bicycle use) and a reduction in the risk of head injury of bicyclists involved in crashes. Increases in helmet-wearing rates and decreases in head-injury risk have continued for two years after the introduction of the law.

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Paper No. 15

M.H. Cameron, S.V. Newstead and A.P. Vulcan,
"Analysis of reductions in Victorian
road casualties, 1989 to 1992".

Proceedings, 17th Australian Road
Research Board Conference,
Gold Coast, August 1994.

Paper No. 15

ANALYSIS OF REDUCTIONS IN VICTORIAN ROAD CASUALTIES, 1989 TO 1992

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SUMMARY

Road accident fatalities in Victoria fell from 776 in 1989 to 396 in 1992. The latter figure corresponds to 1.5 deaths per 10,000 registered vehicles or 1.0 deaths per 100 million vehicle km travelled, both of those rates being comparable with the lowest recorded among motorised countries. During the same period, serious injuries dropped 36.9% (from 9356 to 5905).

A review of countermeasures which have been shown to be associated with these reductions in deaths and serious injuries is undertaken. They include:

- increased random breath testing, supported by publicity;
- new speed cameras, supported by publicity;
- bicycle helmet wearing law;
- lowering of 110 km/h freeway speed limit;
- improvements to the road system; and
- various other measures.

The effect of the downturn in the economy and reduced alcohol sales over the same period is also considered.

The contributions of each of the major countermeasures and other factors to the reductions in serious casualty crashes during each of the years 1990 to 1992 are estimated.



Max Cameron is a Senior Research Fellow in the Accident Research Centre at Monash University. He holds an M.Sc. in mathematical statistics and is a Fellow of the Royal Statistical Society. He has worked in the road safety field in Australia since 1965, with extensive experience in road safety research and its management, and in road safety policy formulation and strategic planning. He has special skills in road crash data analysis and countermeasure evaluation in the behavioural, vehicle and road environment safety areas.



Stuart Newstead is a Research Fellow at the Monash University Accident Research Centre and holds an M.Sc. in mathematical statistics. He has only recently entered the field of road safety research after time spent working in the field of AIDS research. His current research focus is in the area of road crash data and countermeasure evaluation with particular focus on statistical methodology.



Peter Vulcan has been Director of the Monash University Accident Research Centre since its establishment in 1987. Previously he was Chief General Manager in the Road Traffic Authority for four years and Chairman of the Road Safety and Traffic Authority in Victoria for seven years. Peter is a mechanical engineer with a Ph.D. in engineering mechanics.

INTRODUCTION

1. After 1989 there was a remarkable reduction in deaths and injuries on Victorian roads. The drop in fatalities from 776 in 1989 to 396 in 1992 was the largest continuous reduction ever experienced in Victoria and brought the total to below 400 for the first time since 1948, when there were 87% fewer registered vehicles.
2. Progress in road safety is usually measured in terms of fatalities per 10,000 registered vehicles. Figure 1 shows this rate for Australia, Victoria and those countries which have the best road safety records.

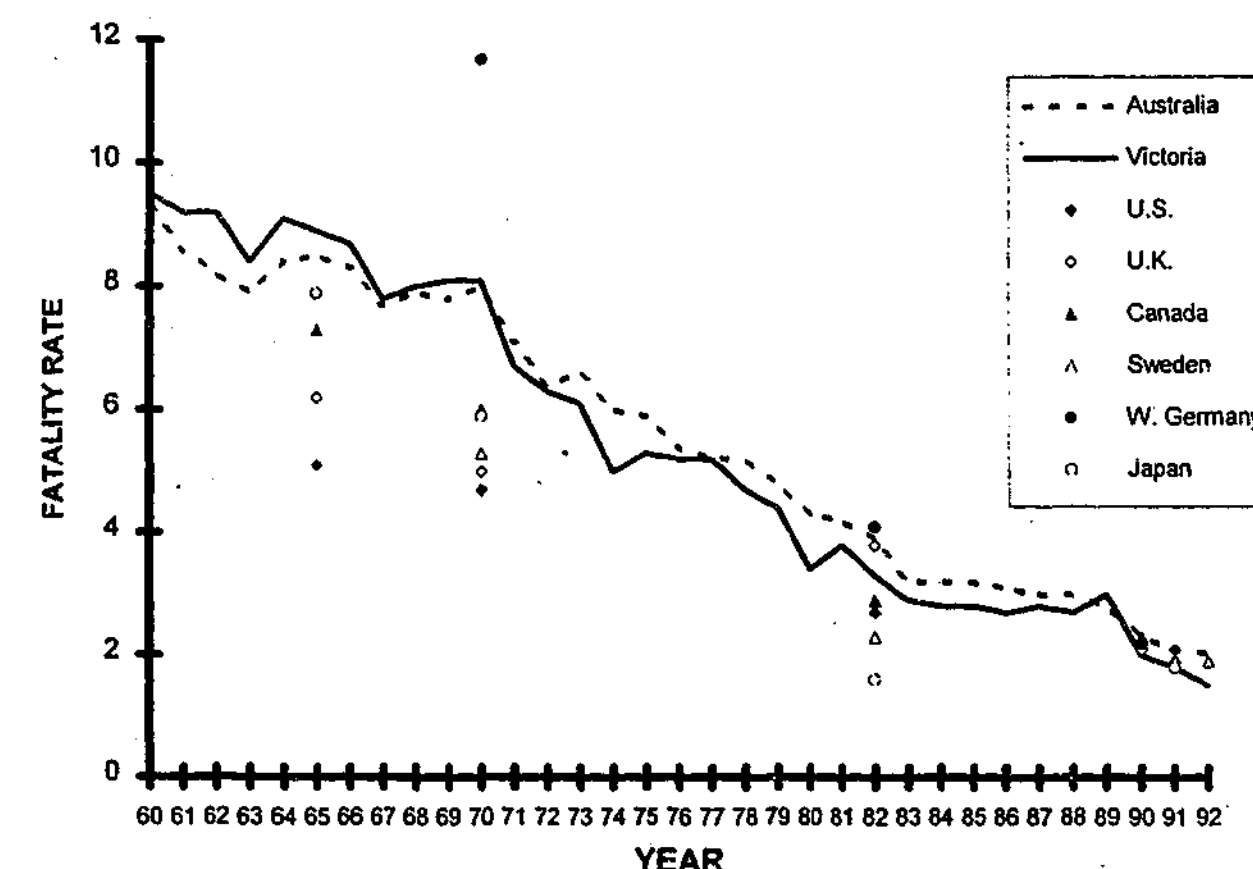


Fig. 1 - Fatalities per 10,000 vehicles 1960-1992

3. It can be seen that up to 1970 progress was relatively slow, but the sustained reductions from 1970 to about 1983 brought Victoria and Australia close to the levels of those countries with the best rates. Then, after a period of relatively little progress, the large drop in Victoria during the three years 1990-92 is shown with the 1992 rate of 1.5 deaths/10,000 registered vehicles being below the latest rates available for other countries. (Care should, however, be taken in comparing the Victorian rate with those of other countries, because there are undoubtedly states/regions in those countries with fatality rates considerably below their national average, just as in Australia the ACT rate is below that for Victoria.)

4. As well as the substantial decrease in road fatalities in Victoria since 1989, there have also been large reductions in serious injuries and other injuries (Figure 2). It should be noted that comparisons of injuries may not be as reliable because of changes in reporting practices with time.

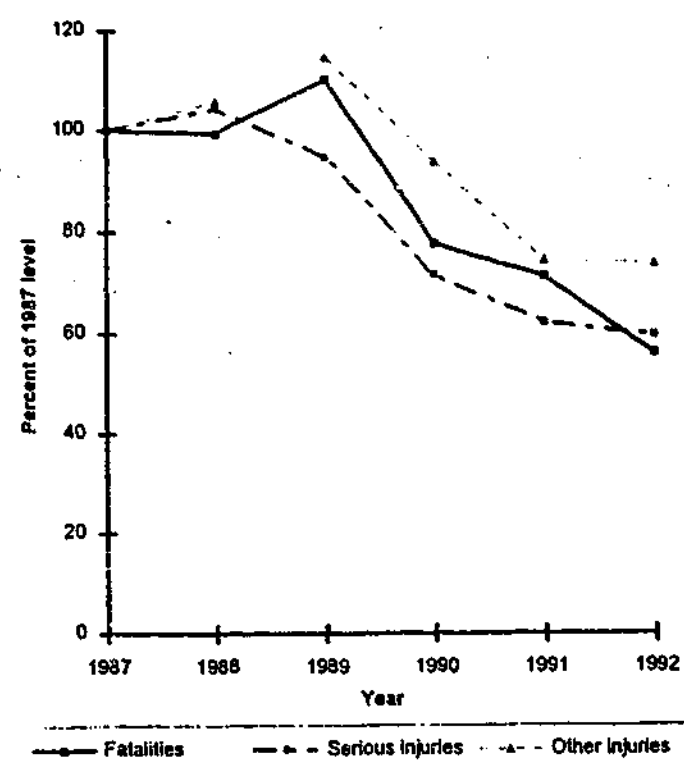


Fig. 2 - Percentage change from 1987.
Fatalities, serious injuries and other injuries

5. These large reductions in deaths and injuries have been attributed to the implementation of various safety programs which were introduced commencing in September 1989, and to the downturn in the economy which occurred during the period under consideration.

6. It is important to estimate the contribution of each of these programs and if possible the mechanism by which they achieved their reductions, so that they can be fine tuned for further gains and allocation of future resources can be made on the basis of the best available information. Some of the measures which are considered to have contributed to the reductions include:

- New Speed Cameras, supported by publicity;
- Increased Random Breath Testing, supported by publicity;
- Bicycle Helmet Wearing Law;
- Lowering of 110 km/h freeway speed limit;
- Improvements to the road system through treatment of accident black spots; and
- Special enforcement campaigns.

7. Unfortunately the periods of implementation for many of these measures coincided and most were applied throughout the State, making evaluation more difficult. Nevertheless, evaluations covering the relevant period have been done on all but the last measure and key aspects of these will be presented in this paper.

SPEED CAMERA PROGRAM

8. New slant radar speed cameras were progressively introduced commencing with four in December 1989 and building to 54 by January 1991. The monthly numbers of speeding tickets (Traffic Infringement Notices) issued following detection by speed cameras are shown in Figure 3. The program included an intensive statewide mass media publicity campaign "Don't fool yourself - speed kills" which aimed to increase the perception of the level of camera operations and their legitimacy. This multi-media cam-

paign by the Transport Accident Commission (TAC) involved much larger expenditure than previous road safety campaigns. It was launched in April 1990 and maintained at high levels for most of 1990, four months in 1991 and seven months in 1992.

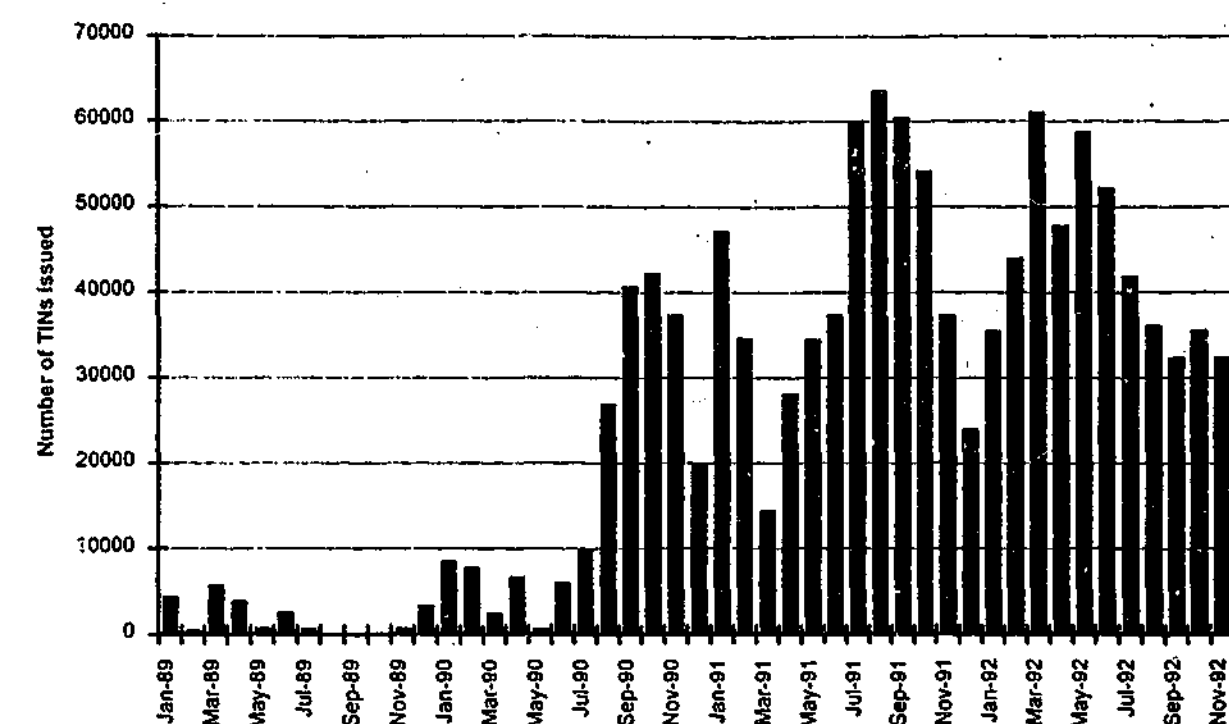


Fig. 3 - Monthly number of Traffic Infringement Notices issued for speeding offences detected by speed cameras - Victoria 1989-1992

9. An evaluation has been undertaken of the effect of the program on crashes during "low alcohol hours", on the basis that the coincident random breath testing (RBT) program could have had only a very small effect on these crashes (Cameron et al. 1992a). During low alcohol hours (Monday - Thursday 6 a.m. to 6 p.m., Friday 6 a.m. to 4 p.m., Saturday 8 a.m. to 2 p.m., Sunday 10 a.m. to 4 p.m.) an average of less than 4% of drivers in serious casualty crashes exceed 0.05% blood alcohol content. Furthermore the majority of RBT operations occurred during high alcohol hours and over 82% of speed camera operations were in low alcohol hours.

10. The analysis was divided into four periods:

- T 1(a) Low level camera trialing and localised low level publicity, December 1989 to March 1990 (average below 5,000 speed camera offences per month).
- T 1(b) High profile media publicity but little change in speed camera use after an intense nine day burst at the start of the period, April to June 1990.
- T 2(a) High levels of camera operations and supporting publicity (14-64,000 offences per month) before the New South Wales speed camera program began, July 1990 to February 1991.
- T 2(b) Same as T2(a) but after the New South Wales speed camera program began, March 1991 - December 1991. (As New South Wales was used as a comparison area, any effect measured during T 2 (b) would represent the difference between this program in the two States.)

11. Speed cameras had been used mainly (80-90% of sessions) on arterial roads in 60 km/h speed zones in both metropolitan and country areas. Their use was also greater in the metropolitan area (70% of sessions) than in the country.

12. Multivariate time series models were developed to predict expected casualty crash frequency and injury severity for Melbourne and Sydney as a comparison area. The respective unemployment rates in each city were included as co-variables in each model to take account of differential impacts of the downturn in the economy on vehicle use. Similar models were also developed for the "rest of Victoria" and the "rest of NSW" as a comparison area.

13. The percentage changes from the model prediction in each area of Victoria were then contrasted with those for the corresponding area in NSW, the difference providing the estimated percentage change which was considered to be attributable to the Victorian speed camera program (after allowing for differences in unemployment rates between the two States and any "other factors" which are assumed to have affected low alcohol hour crashes in both States equally).

14. Figure 4 shows the net percentage reductions in casualty crash frequency and injury severity (defined as the ratio of fatal plus serious injury crashes to crashes involving only minor injury) in low alcohol hours on Melbourne arterial roads when compared with Sydney arterial roads for each of the four periods.

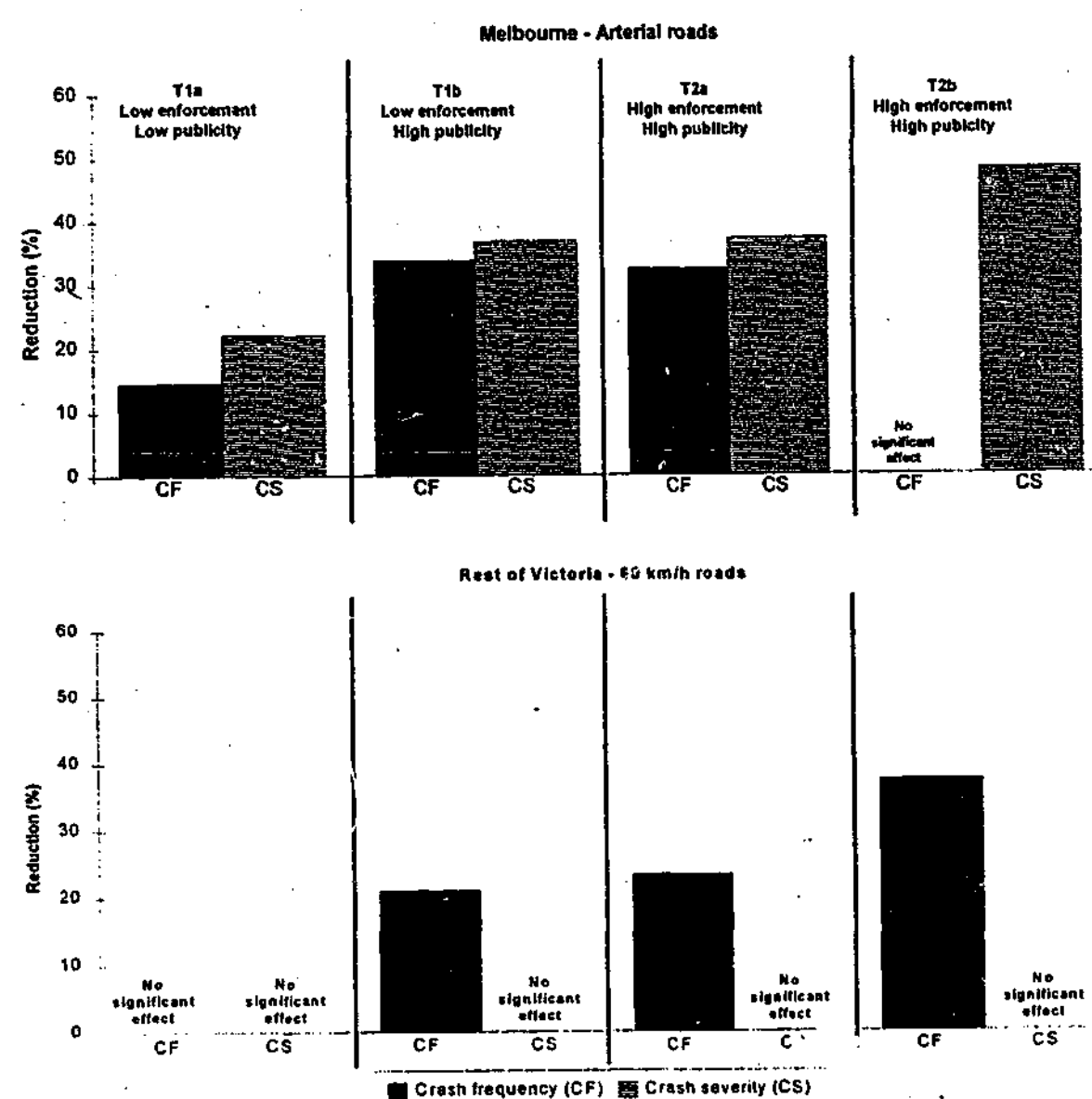


Fig. 4 - Estimated Percentage Reductions in Casualty Crash Frequency and Injury Severity in Low Alcohol Hours

15. Figure 4 also shows the net percentage reductions on 60 km/h roads in the rest of Victoria when compared with 60 km/h roads in the rest of NSW.

16. It can be seen that there were statistically significant reductions in casualty crash frequency (in low alcohol hours) in both areas in three of the four periods, but the reductions in severity were confined to Melbourne. The fact that in period T 2(b) the frequency of casualty crashes in Melbourne was not significantly different from Sydney might be explained by the fact that the NSW speed camera program had started in Sydney at the beginning of this period, and there was a significant reduction in such crashes in Sydney during T 2(b).

17. Other results are given in the full report (Cameron et al. 1992a). When results for the whole of Victoria were considered there was a statistically significant reduction in the frequency of low alcohol hour casualty crashes of 20% during period T 1(b) and 21% during T 2(a). Similarly there were reductions in severity of 28% in T 2(a) and 40% in T 2(b). There were, however, no significant reductions for 100 km/h zones.

18. A second phase of the study established relationships between the frequency of casualty crashes in Melbourne and various components of the program (numbers of speeding tickets issued, and levels of supporting publicity) (Cameron et al. 1992a). The third phase of the study examined the localised effects in time and space related to the camera operations in Melbourne and the receipt of speeding tickets which resulted from each camera session (Rogerson et al. 1994). This phase has found a significant reduction in casualty crashes within one kilometre of a camera site as a result of the receipt of a TIN, for two weeks after receiving the penalty (the effect may have been longer). However, there was no statistically significant reduction in casualty crashes which occurred during the week starting on the actual day of camera operations.

RANDOM BREATH TESTING PROGRAM

19. Commencing in September 1989 the use of buses for random breath testing (RBT) was gradually increased, initially in the metropolitan area using four existing buses and progressively throughout the State as 13 new purpose-built highly visible "booze buses" became available during 1990. Increased resources for operating the buses were provided through the use of Probationary Constables in Training. This resulted in the number of RBT tests increasing from around half a million in 1989 to over 900,000 in 1990 and over 1,100,000 in 1991. The number of tests per month are shown in Figure 5 for Melbourne and rural Victoria. Buses had become the primary form of RBT in the metropolitan area by November 1989, while in the rural areas the shift to greater use of buses occurred after October 1990.

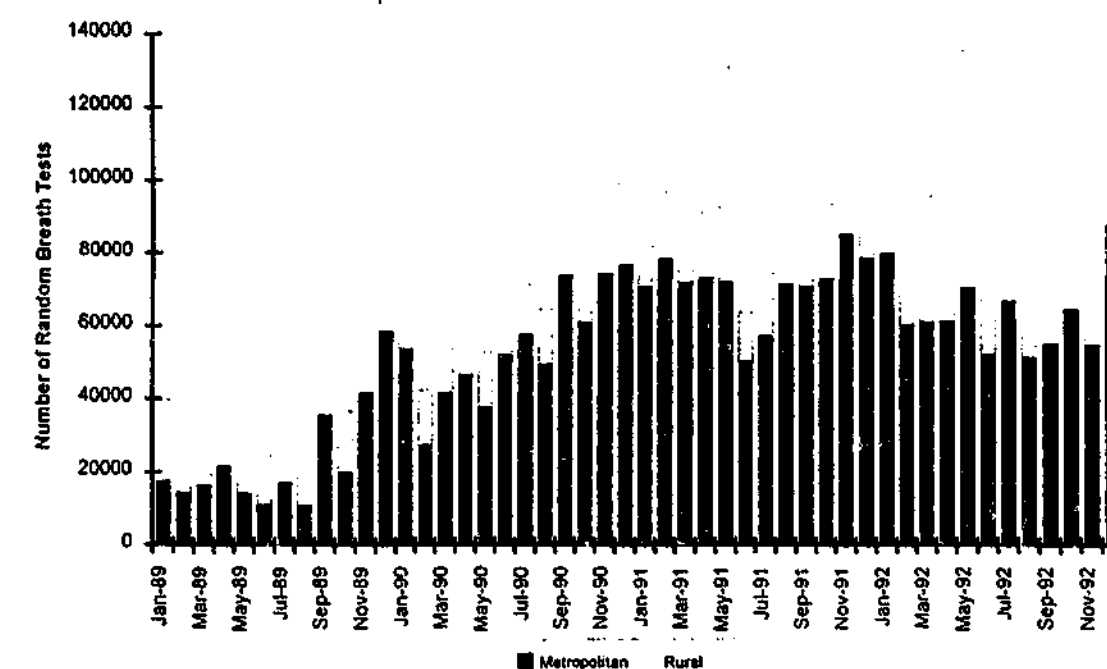


Fig. 5 - Number of Random Breath Tests per month Victoria 1989 - 1992

20. In December 1989 a major statewide multi-million dollar publicity campaign "If you drink then drive - you're a bloody idiot" was launched to support the new RBT operations. Further media launches and publicity for the new booze buses occurred in April and September 1990.

21. Two different analyses of the effect of the RBT initiative have been conducted (Drummond et al. 1992; Cameron et al. 1992b; Cavallo and Cameron 1992). The first was a quasi-experimental time series design which used the more remote rural areas of Victoria (R2), which had received less RBT, as a comparison area for Melbourne and for the remaining rural areas, which were closer to Melbourne (R1). Sydney was also used as a comparison area for Melbourne.

22. The second was a multivariate time series approach, with Sydney as a comparison area for Melbourne and the "rest of NSW" as a comparison area for the "rest of Victoria". Unemployment rate was used as a co-variate (as in the speed camera program evaluation) to account for the effect of the economic downturn on vehicle use.

23. Both studies limited their evaluation to fatal crashes and serious casualty crashes in "high alcohol hours". This is reasonable because an average of about 38% of serious casualty crashes in these hours involve a driver with a BAC over 0.05%, while the corresponding percentage during the remainder of the week (low alcohol hours) is below 4%. Furthermore it had to be assumed that the speed camera program has little effect during these high alcohol hours.

24. Figure 6 shows the results from these two studies. It should be noted however, that while the results shown for Melbourne use Sydney as the comparison area, the rural results for the quasi-experimental study are for Rural area R1 (the area with greater RBT bus operations) compared with Rural area R2, whereas the second study compares the whole of rural Victoria with rural NSW.

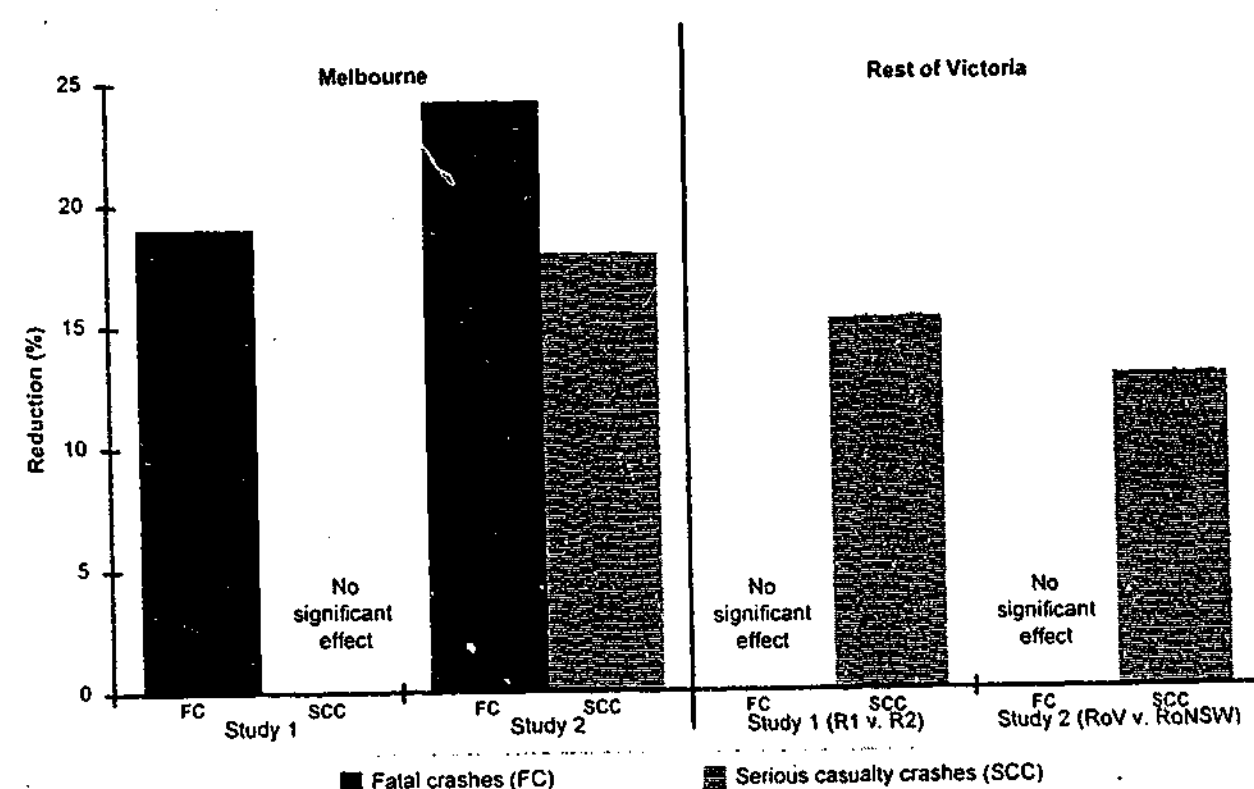


Fig. 6 - Net reductions in high alcohol hour crashes 1990

25. It can be seen that even through the treatment and comparison areas for "rural" are different in the two studies, three of the four estimated net reductions are similar, namely:

Melbourne, fatal crashes	(19-24%)
Rural, serious casualty crashes	(13-15%)
Rural, fatal crashes	no significant effect

26. The different results for Melbourne serious casualty crashes appear to result from the different analysis methods used (Cavallo and Cameron 1992). The multivariate time series analysis (Study 2) was capable of being extended to 1991 using regression modelling and found a 24% significant reduction in rural serious casualty crashes (high alcohol times).

27. As shown in Table 1, the RBT program was also accompanied by progressive reductions in the proportion of drivers and motorcyclists killed (and tested) with a Blood Alcohol Content (BAC) exceeding .05%.

TABLE 1
DRIVER/MOTORCYCLIST FATALITIES BY BAC

	Number below .05% BAC	Number exceeding .05% BAC	Percent exceeding .05% BAC
1988	279	105	37.6
1989	349	113	32.4
1990	246	73	29.7
1991	243	70	28.8
1992 (prelim)	181	37	20.4

BICYCLE HELMET LAW

28. A law which required all bicyclists to wear an approved bicycle safety helmet came into effect in Victoria on 1 July 1990. This resulted in an increase in the estimated overall helmet wearing rate from 31% to 75%. Based on TAC claims, in the first 12 months after the introduction of the law there was a reduction of 51% in the number of bicyclists with a head injury killed or admitted to hospital and the corresponding number without a head injury dropped 24%. As shown in Figure 7, in the second year the corresponding reductions were 70% and 28% respectively. While the large reduction in head injuries was a clear indication of the effect of the law, the reduction among bicyclists without a head injury was shown to be at least partly a result of reduced cycling, particularly among teenagers (Finch et al. 1992). A portion of both drops may of course be attributable to the economic downturn, or the speed camera and RBT programs.

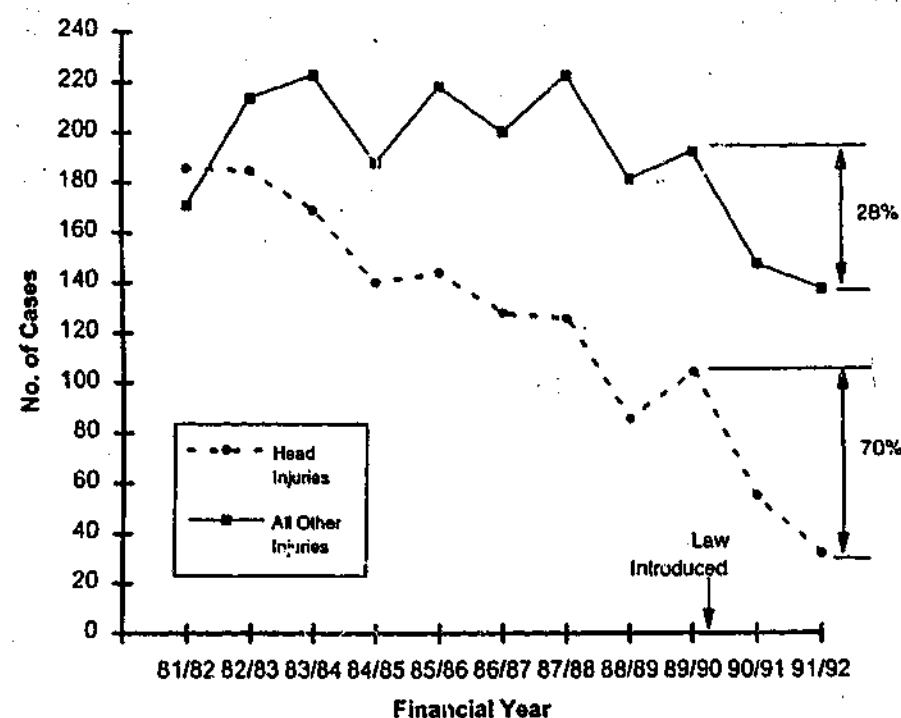


Fig. 7 - Number of severe bicyclist casualties, Victoria

FREEWAY SPEED LIMIT

29. The 110 km/h speed limit on high standard rural and outer metropolitan freeways was lowered to 100 km/h at the end of September 1989. An analysis showed a 19.3% drop in casualty accident rate, when adjusted for changes for a control group of roads over the 814 days until the end of 1991 (Sliogeris 1992). This corresponds to a drop of 84 casualty accidents per annum of which some 39 per annum were fatal or serious injury accidents.

IMPROVEMENTS TO THE ROAD SYSTEM

30. In regard to progressive improvements to the road system, particularly at locations or along routes with a bad accident record, earlier studies have shown that reductions in casualty accidents of 10-50% have been achieved, yielding very high benefit cost ratios (Corben et al. 1990; Tziotis and Bui 1992; Ogden 1992). However in any one year, available resources have limited works to sites with total casualty accidents of less than 1,500 per annum. Assuming a 30% average reduction, the additional annual reduction from one year's program would be up to 450 casualty accidents, ie. about 4% of the total drop in casualty accidents.

31. Such road improvements are important over the longer term, because they continue to provide these reductions each year for the life of each treatment. In fact, VIC ROADS has estimated total savings of 9,105 casualty accidents (cumulative) resulting from the treatment of 1,998 sites under the accident blackspot program during the period 1982 to 1992 (Anderson 1993). However, their contribution to the large downturn since 1989 could only be relatively small.

ECONOMIC ACTIVITY

32. Various overseas and Australian studies have shown that it is possible to construct models which link downturns in economic activity with reductions in road fatalities, taking into account changes in road safety measures and other factors where relevant (Hakim

et al. 1991; Haque 1991; Pettit 1992). After an extensive examination of relevant variables, a study developed a model which explained about 50% of the variations in monthly Victorian road fatalities during the period 1985 to 1990. (Thoresen et al. 1992)

33. The model estimated that in 1990 a reduction of 53 fatalities could be associated with the increase in unemployment. This represents a reduction of 6.8% of the 1989 road toll or about one quarter of the total drop in 1990. For 1991 the model indicated that perhaps one third of the reduction from 1989 could be associated with unemployment.

SPECIAL ENFORCEMENT PROGRAMS

34. The Victoria Police have implemented various Statewide enforcement programs of high intensity but limited duration, often directed at certain classes of road use and supported by publicity. There have also been special campaigns undertaken in certain areas in accordance with the road safety calendar directed at increased seat belt wearing and driver fatigue. It is likely that they each made a contribution to the overall reductions but to date no specific evaluations of their effects on casualties have been published.

OVERALL EFFECTS ON ROAD CASUALTIES IN VICTORIA SINCE 1989

35. The separate studies described above have reported various reductions in fatal or casualty crashes associated with specific programs implemented during the period 1990 to 1992. While all of these studies have covered 1990, not all have extended into 1991 or 1992. Furthermore, the reductions have been expressed in different ways. A need was identified to combine the results to determine the relative contributions of the various measures and other factors to the overall reduction in road casualties in Victoria since 1989 (Vulcan 1993).

36. Estimates of the contributions of the RBT and speed camera programs (and their supporting publicity) have been given priority because of their potential to have broad effects since these programs are not focused on crashes involving only particular road users or occurring only on particular parts of the road system.

37. The estimates are based on models linking serious casualty crashes (SCCs) with monthly variations in operational measures of the programs' activity, such as numbers of random breath tests conducted, numbers of speed camera TIRs issued, and levels of TAC road safety television advertising. The models also considered links with monthly unemployment rates and, where appropriate, monthly alcohol sales. These models were developed as part of an evaluation of the separate effects of the TAC television advertising supporting the enforcement programs (Cameron et al. 1993; Cameron and Newstead 1993a) and in subsequent work (Cameron and Newstead 1993b).

MODELS FOR MONTHLY SERIOUS CASUALTY CRASHES IN VICTORIA

High Alcohol Hour Crashes and the Random Breath Testing Program

38. A multiplicative model was fitted to the monthly numbers of high alcohol hour (HAH) SCCs in Melbourne and country Victoria (separately) during 1983-92. A number of studies have found that multiplicative (rather than additive) models fitted to explanatory variables are best to represent road trauma series (Hakim et al. 1991; Pettit 1992). The explanatory variables considered for HAH crashes included:

- monthly unemployment rates in the corresponding area (Figure 8)
- number of random breath tests conducted in the area (Figure 5)

- an index of alcohol sales in Victoria (Figure 9)
- effect of TAC television advertising with drink-driving themes (see below)
- seasonal variation
- long-term trend.

39. The index of alcohol sales was based on the monthly sales turnover figures of hotels, liquor stores and licensed clubs in Victoria. This data was converted into constant price terms using the alcohol beverages sub-index of the Consumer Price Index. The index is considered to represent monthly variations in the volume of alcohol beverage consumption (Thoresen et al. 1992). However it does not take into account any changes in alcohol content, except to the extent that this was reflected in price per unit volume.

40. The effect of TAC television advertising was measured by a function "Adstock" (Broadbent 1979; Cameron et al. 1993) which represents the audience's retained awareness of current and past levels of advertising. The Adstock of TAC television advertising during each month in 1990-92, by theme of the message, is shown in Figure 10.

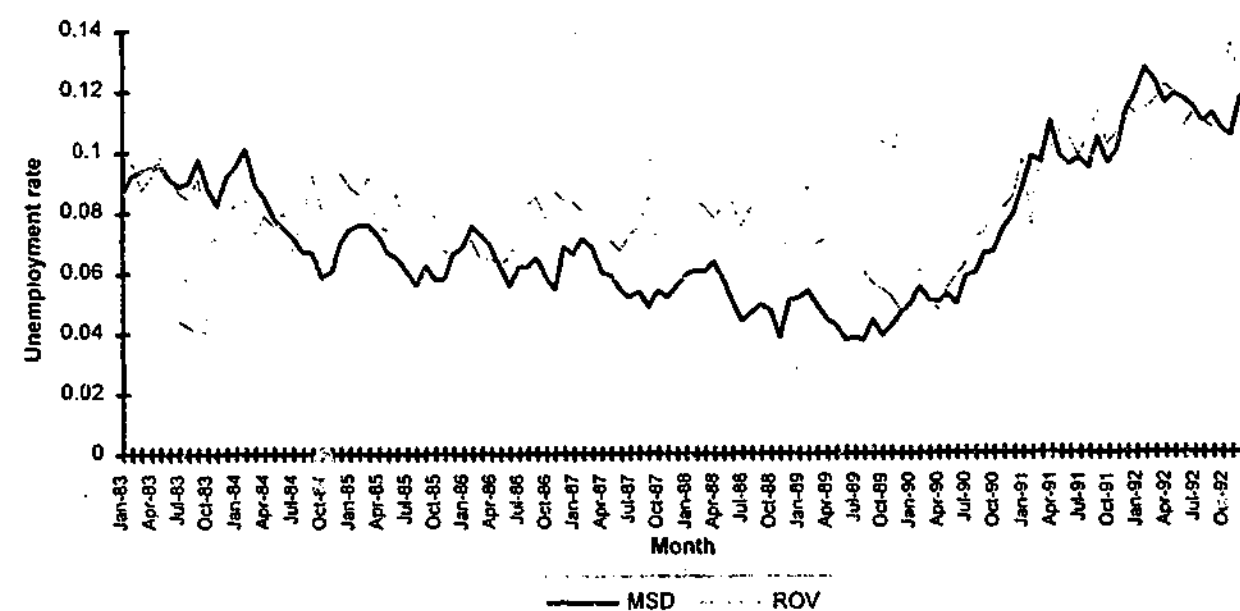


Fig. 8 - Unemployment rates in Melbourne (MSD) and the rest of Victoria (ROV) 1983-92

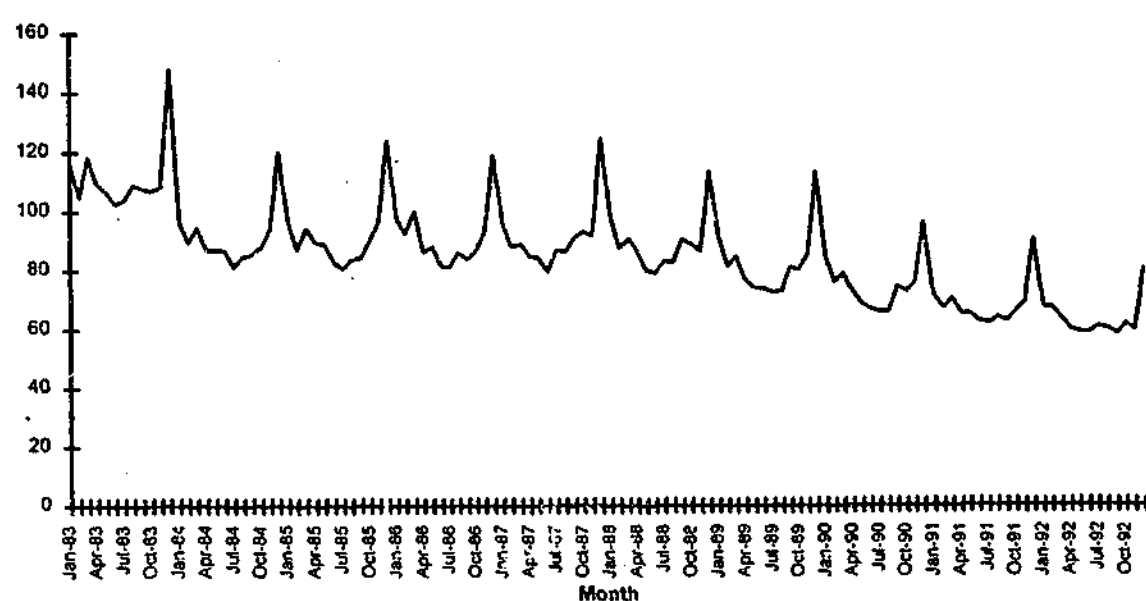


Fig. 9 - Index of alcohol sales in Victoria, 1983-92

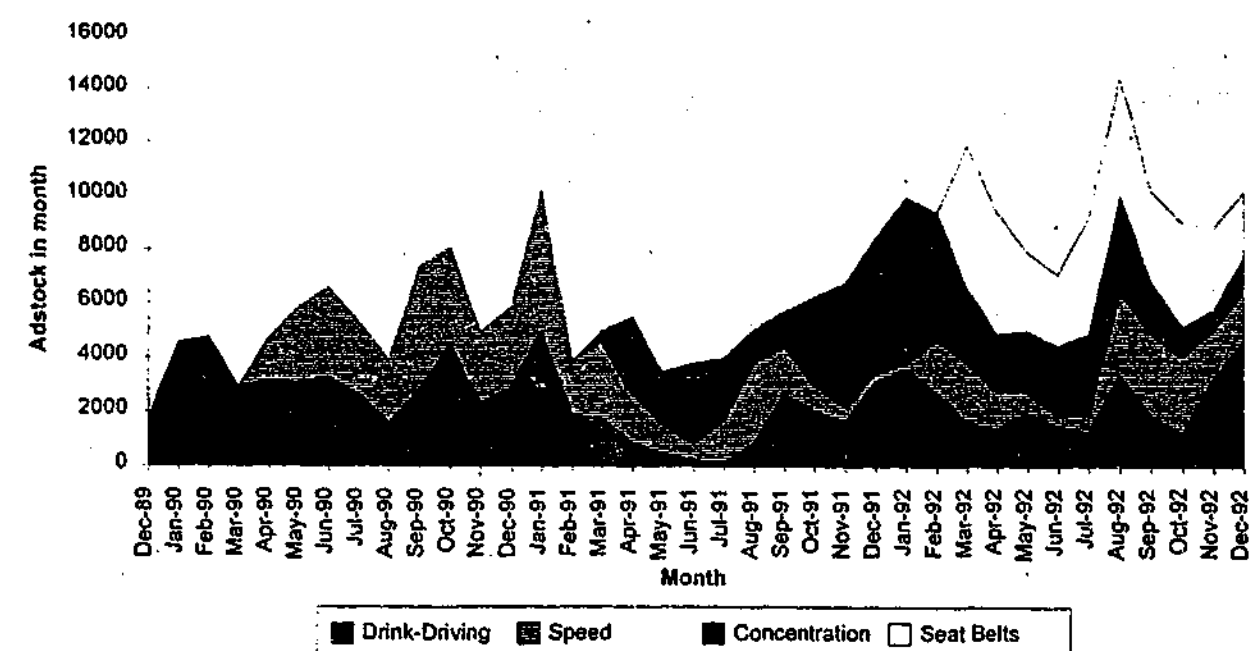


Fig. 10 - TAC road safety television advertising - Monthly Adstock by theme

41. The model was fitted by first taking logarithms of the number of HAH SCCs and each of the explanatory variables, and then fitting the relationship using multiple linear regression. The regression coefficients of each of the (logged) explanatory factors included in the models, excepting the coefficients for the monthly step variables and the intercept, are summarised in Table 2. The model for Melbourne crashes explained 84% of the monthly variation and the model for crashes in country Victoria explained 66%.

TABLE 2

EFFECTS OF EXPLANATORY VARIABLES ON SERIOUS CASUALTY CRASHES DURING HIGH ALCOHOL HOURS OF THE WEEK. MELBOURNE AND COUNTRY VICTORIA 1983-92.

AREA OF VICTORIA Explanatory variable	Estimated coefficient of logged variable	T value (103 d.f.)	Significance level (two-tailed)
MELBOURNE CRASHES			
• trend	0.0037***	4.515	0.0001
• unemployment rate in month	-0.3300***	-8.541	0.0001
• no. of random breath tests during month	-0.0111**	-2.655	0.0092
• alcohol sales during month	0.6522***	3.914	0.0002
• drink-driving Adstock in month	-0.0249***	-4.092	0.0001
CRASHES IN COUNTRY VICTORIA			
• trend	0.0024*	2.261	0.0258
• unemployment rate in month	-0.1649*	-2.387	0.0188
• no. of random breath tests during month	0.0055	0.912	0.3640
• alcohol sales during month	0.7500**	3.138	0.0022
• drink-driving Adstock in month	-0.0316***	-4.282	0.0001

* Statistically significant at $p < 0.05$ level; ** Highly statistically significant at $p < 0.01$ level; *** Very highly statistically significant at $p < 0.001$ level.

42. The monthly unemployment rate was very highly significantly and inversely related to the crashes in Melbourne, and significantly and inversely related to the crashes in country Victoria. The strength of the relationships were greater than those observed in previous studies of total Victorian fatalities to the end of 1990 (Thoresen et al. 1992) and of HAH SCCs to the end of 1991 (Cameron et al. 1992b).

43. The monthly number of random breath tests was highly significant and inversely related to the crashes in Melbourne, but was not statistically significantly related to monthly variations in country Victoria HAH SCCs. Relationships of this type had not previously been tested in the evaluation of the RBT program.

44. Monthly variations in the index of alcohol sales in Victoria were significantly and positively related to the HAH SCCs in Melbourne (very highly significant) and in country Victoria (highly significant). It is possible that the reductions in alcohol sales may be related to the increases in RBT and associated publicity. This will be discussed later.

45. Awareness of TAC drink-driving television advertising, measured by Adstock, was very highly significantly and inversely related to the monthly number of HAH SCCs in both areas of the State.

46. Because of the multiplicative structure of the models fitted, the regression coefficients in Table 2 are estimates of the "elasticity" of HAH SCCs to a change in the explanatory factor. Thus, for example, a 1% decrease in alcohol sales in Victoria is estimated to result in a 0.65% decrease in the number of HAH SCCs in Melbourne, if all other major factors affecting these crashes remained constant. However it should be noted that these "elasticities" may only be valid over the range of the explanatory variables actually considered when developing the models, and should not be assumed to apply when the explanatory factors fall outside these ranges.

Low Alcohol Hour Crashes and the Speed Camera Program

47. A multiplicative model was fitted to monthly numbers of low alcohol hour (LAH) SCCs in Melbourne and country Victoria (separately) during 1983-92. The explanatory variables included:

- monthly unemployment rates in the corresponding area (Figure 8)
- number of TINs issued from speed cameras in Victoria (Figure 3)
- Adstock of TAC television advertising with speeding and concentration themes (see Figure 10)
- seasonal variation
- long-term trend.

48. The regression coefficients of each of the (logged) explanatory factors included in the models, excepting the coefficients for the monthly step variables and the intercept, are summarised in Table 3. The model for Melbourne crashes explained 78% of the monthly variation and the model for crashes in country Victoria explained 67%.

49. As in the models for HAH crashes (Table 2), the monthly unemployment rate was significantly and inversely related to the LAH serious casualty crashes in both Melbourne and country Victoria.

TABLE 3

EFFECTS OF EXPLANATORY VARIABLES ON SERIOUS CASUALTY CRASHES DURING LOW ALCOHOL HOURS OF THE WEEK. MELBOURNE AND COUNTRY VICTORIA 1983-92.

AREA OF VICTORIA Explanatory variable	Estimated coefficient of logged variable	T value (104 d.f.)	Significance level (two-tailed)
MELBOURNE CRASHES			
• trend	0.0032***	4.849	0.0001
• unemployment rate in month	-0.1181*	-2.271	0.0252
• no. of speed camera TINs issued	-0.0242***	-4.811	0.0001
• speeding and concentration Adstock	-0.0288***	-3.771	0.0003
CRASHES IN COUNTRY VICTORIA			
• trend	0.0019**	3.048	0.0029
• unemployment rate in month	-0.1946**	-3.040	0.0030
• no. of speed camera TINs issued	-0.0097	-1.619	0.1085
• speeding and concentration Adstock	-0.0266***	-3.689	0.0004

* Statistically significant at $p < 0.05$ level; ** Highly statistically significant at $p < 0.01$ level;

*** Very highly statistically significant at $p < 0.001$ level.

50. The monthly number of speed camera TINs issued was very highly significantly and inversely related to LAH SCCs in Melbourne, but was not statistically significant so far as the link with LAH crashes in country Victoria was concerned. A relationship of this type had not previously been tested for country crashes in the evaluation of the speed camera program (Cameron et al. 1992a). The absence of a link with levels of activity of the speed camera program, which has been predominantly operated in Melbourne, is consistent with the generally weaker general effects found in country towns and on rural highways in that study.

51. Awareness of TAC "speeding" and "concentration" television advertising, measured by Adstock, was very highly significantly and inversely related to the monthly number of LAH SCCs in both areas of the State.

COMBINING THE RESULTS OF THE MODELS

52. A method was developed to decompose the models to estimate the separate effects of the countermeasures and other factors, and then to combine the results from the four models (covering two periods of the week and two regions of Victoria) to estimate the overall contribution of each measure/factor to the reduction in serious casualty crashes in Victoria during each of the years 1990 to 1992 (Cameron and Newstead 1993b). The method estimates each contribution as the percentage reduction in serious casualty crashes apparently due to the measure/factor. It should be noted that these percentages cannot simply be added up to estimate the total contribution. If more than one contributor is being considered, the percentage reduction of each must be applied in turn.

ESTIMATING THE IMPACTS ON TOTAL ROAD TRAUMA IN VICTORIA

53. The method outlined in the previous section was used to estimate the contributions of the speed camera and RBT programs (and their supporting publicity), reduced economic activity and reduced alcohol sales, to the reductions in serious casualty crashes (Table 4). The expected levels of crashes were those which the models predicted if the road safety programs had not been operating and if the unemployment rate and alcohol sales had remained at their 1988 levels. The total contribution of the four road safety programs has been estimated as described in the previous section and is not simply the addition the four percentage reductions estimated for each program.

TABLE 4

ESTIMATED REDUCTIONS IN SERIOUS CASUALTY CRASHES ATTRIBUTABLE TO VARIOUS FACTORS - VICTORIA, ALL HOURS, 1990-92

	1990	1991	1992
Modelled actual serious casualty crashes (actual serious casualty crashes)	6147 (6156)	5350 (5371)	5207 (5156)
Expected* serious casualty crashes	9041	9380	9737
Reduction in serious casualty crashes	32.0%	43.0%	46.5%
Contribution of increased unemployment	0.8%	10.8%	13.5%
Contribution of reduced alcohol sales	5.6%	8.5%	10.6%
Contribution of speed camera TINs	8.2%	9.2%	9.3%
Contribution of speed and concentration publicity	7.4%	10.2%	10.3%
Contribution of RBT	3.9%	4.1%	4.1%
Contribution of drink-driving publicity	9.9%	8.8%	9.5%
Contribution of above four road safety programs	26.4%	28.7%	29.3%

*Expected if the road safety initiatives and other factors had remained at base levels

DISCUSSION

54. It is possible that the speed camera program may have had an effect on HAH serious casualty crashes (as well as LAH crashes). However, as most of the camera operations were during LAHs and in order to try to separate the effects of the RBT and speed camera programs, the speed camera operations (ie. camera hours and TINs issued) were excluded from the HAH crash models. Similarly, the number of RBTs, drink-driving publicity levels, and the index of alcohol sales were excluded from the LAH crash models because drivers have BACs over 0.05% in less than 4% of LAH serious casualty crashes.

55. Based on the models fitted to monthly variations in serious casualty crashes in Victoria to the end of 1992, it appears that the major road safety programs combining Police enforcement and mass media publicity have contributed to most of the reduction in these crashes during 1990-92 compared with levels expected. These road safety programs are estimated to have contributed reductions in serious casualty crashes of 26-29% during these three years. It is estimated that the speed camera operations, principally through the

issuing of speeding TINs, have contributed 8-9%, and that television advertising with a combination of "speeding" and "concentration" themes (which apparently supported the speed cameras) has contributed 7-10%. It is estimated that the increased random breath testing has contributed about 4% each year and that the supporting "drink-driving" advertising on television has contributed 9-10%. However the effects of interactions of these enforcement and associated publicity activities were not considered in the models and these may have been substantial.

56. In addition to these major programs, it appears that reduced economic activity and reduced alcohol sales have also contributed to the reduction in serious casualty crashes. The contribution of increased unemployment rates is estimated to be 1% reduction during 1990 (principally in the second half of the year), 11% during 1991 and 14% during 1992. The relatively small contribution of unemployment rates during 1990 should be compared with an earlier estimate that increased unemployment contributed to a decrease of about 7% in Victorian road fatalities during that year (Thoresen et al. 1992). If increased unemployment during 1990 led to a reduction in certain types of vehicle use (eg. discretionary travel at night), it may be that this vehicle use was associated with a higher risk of fatal outcome than serious injury.

57. The contribution of reduced alcohol sales to the reduction in serious casualty crashes was estimated to rise from 6% in 1990 to 9% in 1991 and 11% in 1992. In the regression models of monthly numbers of serious casualty crashes during HAHs, the level of alcohol sales was highly to very highly statistically significant, but the number of random breath tests was (highly) statistically significant only in Melbourne. The contribution of reduced alcohol sales to reductions in this type of crash is not surprising given its relationship with drink-driving. The question is whether the increased random breath testing and supporting publicity led to the reduced alcohol sales (at least in part), and whether this was an indirect effect of the enforcement activity in addition to the direct effect measured by its regression coefficients (see Table 2). It was not possible to address this question using the analysis methods described here, but it may be possible using alternative methods.

58. Together the road safety programs, reduced economic activity and reduced alcohol sales appeared to have produced reductions in serious casualty crashes of 32% during 1990, 43% during 1991 and 47% during 1992, compared with expected levels.

CONCLUSIONS

59. Victoria has experienced unprecedented reductions in road fatalities and injuries during the three years 1990 to 1992. The bulk of the reduction can reasonably be attributed to the two major road safety programs, the random breath testing program and the speed camera program, each supported by publicity. The major contributors and the apparent percentage reduction in serious casualty crashes during 1990-92 due to each measure/factor are shown in Table 4. The anti-speeding and drink-driving programs together are estimated to have contributed reductions in serious casualty crashes of at least 26-29% during these three years.

60. It appears that the reduced economic activity and reduced alcohol sales have also contributed to the reduction in serious casualty crashes. It is unclear whether the reduction in alcohol sales may have been, at least in part, due to the increased random breath testing and hence its apparent effect on crashes may partially represent an indirect effect of the increased testing.

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Paper No. 16

M. H. Cameron,
"Economic analysis - An essential tool".

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Economic analysis: an essential tool

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Economic analysis of injury prevention programs is essential for decisions about the best allocation of resources to specific projects and as a means of attracting additional resources to injury prevention areas. In these times of severe resource constraints for all social programs, it is necessary (and feasible) for injury prevention programs to be subjected to economic assessment.

The key issues in economic analysis which will be outlined in this paper are:

- valuing the benefits;
- valuing the costs;
- transfer costs or payments; and
- economic assessment, in which benefits are compared with costs.

As an example, the paper will include an economic assessment of the road trauma benefits of the Victorian random breath testing and speed camera programs (including the supporting mass media publicity) during 1990-93.

Valuing the benefits

The valuation of the benefits of an injury prevention program usually requires two steps: an evaluation of the impact of the program on injuries, and estimation of the value of the injuries saved.

Impact evaluation

Because of the social environment in which injury prevention programs are placed, it is seldom possible to use classical scientific methods to measure their impact. Thus it may be difficult to separate the effects of the program from a number of other social changes which may reduce the injuries targeted by the program. Methods which aim to overcome these difficulties and thereby produce reliable conclusions about causation and the level of impact of injury prevention programs are outlined by Cameron (1).

Valuing the injuries saved

Impact evaluation provides an estimate of the number of injuries saved by the program. Valuing these benefits in monetary terms, for comparison with the costs of the program, requires estimates of the average cost or value of each injury saved.

The costs of injury seldom fall in one sector of the economy and many of the costs of severe injury can be intangible and long-lasting (for example, serious spinal injuries can have large intangible costs to quality of life as well as long-term treatment costs). The full costs of injury are seldom aggregated for use by those responsible for allocating resources for injury prevention (for consideration as an alternative to the allocation of resources to the all-too-apparent needs of injury treatment and compensation). Studies which embrace the broad range of social costs of injury provide this aggregation.

Steadman and Bryan (2) define the two basic approaches to injury costing:

- human capital or ex-post approach, which examines the costs of injuries which have already occurred; and
- willingness to pay or ex-ante approach, which seeks to determine the amount the community would pay to reduce injuries in the future.

The human capital approach involves aggregating all of the costs of an injury, each cost being a loss which is of value to society as well as to the injured person. Steadman and Bryan identify three broad types of cost:

- loss or partial loss of the victim (the most common way of valuing this cost is to estimate the victim's production capacity lost as a result of the injury, measured as forgone income);

- costs of accident-generated activities (for example, hospital, medical and rehabilitation costs and costs of legal and court proceedings); and
- pain and suffering (usually valued on the basis of court awards).

Steadman and Bryan provide a basis for human capital estimates of the injury *per person*, given in Table 1, derived from Monash University Accident Research Centre (MUARC) (3).

Using the same basis, MUARC derived estimates of the average cost *per injury*, categorised by body region and the Abbreviated Injury Scale (AIS) (4), given in Table 2.

The estimates in Table 2 relate to an individual injury to the specified body region at the specified level of severity. Thus they should be applicable to other fields of human injury, not just road trauma.

Table 1 – Average cost of injury to car occupants, by injury level (1991 \$A) (3)

Injury Level	Average cost per injured
Killed	596 000
Hospital admission	59 000
Medical treatment	15 500
Unreported injury	2 300

Table 2 – Average cost per injury (in 1991 \$A '000s) (4)

Body Region	Injury Severity						Unknown
	Minor (AIS = 1)	Moderate (AIS = 2)	Serious (AIS = 3)	Severe (AIS = 4)	Critical (AIS = 5)	Maximum (AIS = 6)	
External	1.5	8.3	23.2	37.7	54.7	332.3	1.5
Head	2.1	9.8	40.3	92.9	328.2	332.3	1.5
Face	2.1	9.8	40.3	53.2	108.9	332.3	1.5
Neck	2.1	9.8	40.3	53.2	108.9	332.3	1.5
Chest	1.5	8.3	23.2	37.7	54.7	332.3	1.5
Abdomen-Pelvis	1.5	8.3	23.2	37.7	54.7	332.3	1.5
Spine	1.5	8.3	54.2	467.0	558.4	332.3	1.5
Upper extremity	2.1	14.4	34.1				1.5
Lower extremity	1.5	14.4	43.3	64.0	108.9		1.5

The 'willingness to pay' approach involves estimation of the amounts individuals are willing to pay to prevent serious injury. Generally, the estimates produced vary considerably. Steadman and Bryan (2) argued that 'willingness to pay' estimates must exceed human capital costs, since a rational individual must at the very least value life by the expected value of his/her future income. 'Willingness to pay' estimates could be up to three times larger than those produced by the human capital approach. Further details of both approaches to valuing injuries are given by Cameron (5).

Valuing the costs

A process evaluation of an injury prevention program aims to measure whether the program was implemented as planned. A key output from the process evaluation should be information on the costs of the program. The costs of the program are the value of the resources consumed by the program. These may be fixed costs (usually associated with development and/or implementation) or variable costs (per unit service delivered by the program, or per unit time for which the program runs). If the program has the option of varying its level or duration, then the variable costs are useful for economic assessment of the value of increasing or decreasing the program at the margins.

Transfer costs or payments

Some costs paid as part of an injury prevention program are transfer payments and do not reflect resources actually consumed by the program. Examples are traffic fine payments or user payments for the costs of the program (for example, part-payments, perhaps at a token level). While these payments may represent a real benefit to one sector (for example, the Government) and a real cost to another (for example, the private sector), and may make the funding of a program feasible, they do not represent the real consumption of resources and should not be included as costs or benefits of the program.

Economic assessment

Economic assessment is the final step in judging whether the injury prevention program is economically worthwhile, for instance, whether the

benefits of the program exceed its resource costs. Common criteria for economic assessment are:

- benefit/cost ratio (total benefits divided by total costs); and
- net present value (total benefits minus total costs).

The former criterion is best for judging the relative merits of programs of different size, whereas the second criterion indicates the absolute magnitude of the net benefits of the program. Intangible benefits or costs which can be valued may be included, if warranted.

Many injury prevention programs return benefits over a number of future years; many also incur on-going costs in future years. These streams of benefits and costs cannot simply be totalled because of the time value of money, independent of inflation (a dollar available now is worth more than a dollar available in the future). Each stream should be discounted to present values before totalling. This raises questions about the appropriate discount rate, which may be related to the opportunities for alternative investment of the program costs.

If the impact evaluation can link the injury reduction benefits to the size of the program (and its marginal costs), then the economic assessment can establish the break-even point of the program, beyond which the additional programs costs exceed the additional benefits. An example of this type of analysis in the context of the road safety television advertising is given by Cameron and co-workers (6).

Random breath testing and speed camera programs

Increased random breath testing, using highly visible 'booze buses', and the introduction of new speed cameras both commenced in Victoria late in 1989. Each of these enforcement programs have been supported by high level multi-media advertising placed by the Transport Accident Commission. Recent work using methods described by Cameron, Newstead and Vulcan (7) provided estimates of the percentage reductions in crashes involving death or hospital admission in Victoria due to these programs during each of the years 1990 to 1993. The estimated total reduction in serious casualty crashes during the four years was 10 820.

In addition, there were probably considerable savings in the less serious injury crashes, but these have been omitted from this assessment.

The average social cost of a serious casualty crash, using the human capital approach, has been estimated as \$200 000 in 1992 prices. Thus the total benefits from the two programs from 1990 to 1993 are estimated to be \$2 160 million.

From the years 1989/90 to 1992/93, the total cost of the increased random breath testing and speed camera programs, including the supporting publicity and establishment of the Traffic Camera Office and its operating costs, is estimated to have been \$98 million over the same four years (8). The total estimated benefits of \$2 160 million from the two programs from 1990 to 1993 can be compared with the total program costs of \$98 million from the years 1989/90 to 1992/93 (note the slightly different timings of the four year period considered in each case, and that neither the benefits nor costs over the four years have been discounted in this illustrative example). When this comparison is made, it can be seen that there is a benefit/cost ratio of 22:1.

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Paper No. 17

M.H. Cameron, C.F. Finch, S.V. Newstead,
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"Measuring crashworthiness:

Make/model ratings and the influence of
Australian Design Rules for motor vehicle safety".

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**MEASURING CRASHWORTHINESS:
MAKE/MODEL RATINGS AND THE INFLUENCE OF AUSTRALIAN
DESIGN RULES FOR MOTOR VEHICLE SAFETY**

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ABSTRACT

This paper updates and extends the methods in a previous paper which gave crashworthiness ratings for makes/models of Australian cars manufactured during 1982-90. The new results used logistic regression to take into account a larger number of factors which were found to be strongly related to the injury risk and injury severity of drivers. The data covered 220,000 drivers involved in tow-away crashes in New South Wales during 1987-92, and a total of 45,000 drivers injured in crashes in Victoria and New South Wales during the same period. The crashworthiness ratings measured the risk of a driver being killed or admitted to hospital when involved in a tow-away crash. The ratings were able to identify 26 makes/models manufactured during 1982-92 which have superior or inferior crashworthiness compared with the average vehicle.

The relationship between crashworthiness and year of manufacture was investigated using the same analysis methods and an expanded data set including pre-1982 vehicles. The data covered 424,000 drivers involved in tow-away crashes and 102,000 injured drivers. The crashworthiness of passenger cars in Australia showed the greatest improvements for those manufactured over the years 1970 to 1979 during which a number of new Australian Design Rules aimed at occupant protection took effect. There was a 47% reduction in the risk of serious injury for drivers involved in tow-away crashes while driving the post-1979 cars compared with those manufactured during the 1960's.

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CRASHWORTHINESS is the ability of a vehicle to protect its occupants against injury in the event of a crash. There is considerable interest in the ratings of makes and models of passenger cars which reflect their relative crashworthiness.

A previous paper reviewed methods used internationally to rate the safety of cars, based on mass crash data (Cameron, Mach, Neiger, Graham, Ramsay, Pappas and Haley, 1992a). Since that time, there have been a number of methodological advances which are reflected in the internationally published ratings (Folksam 1992, Broughton 1994, Highway Loss Data Institute 1994, Insurance Institute for Highway Safety 1994, U. K. Department of Transport 1995).

In 1992, the first crashworthiness ratings of Australian passenger cars were produced based on crash and injury data from Victoria during 1983-90 and New South Wales (NSW) during 1989-90 (Cameron et al 1992a, 1994). The ratings were based on data for 22,964 drivers injured in crashes in the two States, plus data for 73,399 drivers involved in tow-away crashes in NSW. Crashworthiness was measured in two components:

1. Rate of injury for drivers involved in tow-away crashes (injury risk)
2. Rate of serious injury (death or hospital admission) for injured drivers (injury severity).

The crashworthiness rating was formed by multiplying these two rates together; it then measured the risk of serious injury for drivers involved in crashes. Measuring crashworthiness in this way was first developed by Folksam Insurance (Gustafsson, Hagg, Krafft, Kullgren, Malmstedt, Nygren and Tingvall 1989).

The rating figures were widely distributed throughout Australia in the form of a "Driver Protection Ratings" brochure. Cameron et al (1992b) reported the Australian car manufacturers' and importers' responses to the rating scores in the subsequent advertising of their products. The paper also examined the relationship between the rating score and the mass of individual passenger car models.

These ratings took into account the speed zone of the crash and the driver sex. Since these ratings were published, an improved analysis method has been developed to take into account a broader range of factors affecting the risk of severe injury, thereby improving the reliability and sensitivity of the results. Based on a comparison of methods applied to the previous data, the new method produces crashworthiness ratings with proportionately smaller variability.

This paper describes the data and analysis methods used to update the previously published crashworthiness ratings. The new ratings cover makes

and models of passenger vehicles manufactured during 1982-92 and crashing in Victoria or NSW during 1987-92. The results, given in the appendix, rate passenger vehicles in terms of the risk of the driver being killed or admitted to hospital when involved in a tow-away crash.

This paper also describes an investigation of the relationship between crashworthiness and year of manufacture of passenger cars sold in Australia during 1964 to 1992. These years of manufacture were of interest because a number of Australian Design Rules aimed at car occupant protection came into effect during the 1970's. The results are displayed in Figure 1 given later in this paper.

It should be noted that none of the analysis described in this paper addresses the "aggressivity" of specific models of passenger cars, ie. the threat of injury to pedestrians or occupants of other cars in the event of a collision (Broughton 1994, U. K. Department of Transport 1995).

DATA

The crash and injury data used for the update of the crashworthiness ratings were identical in format to those used and described previously (Cameron et al 1992a, 1994), but also covered crashes during 1991 and 1992. The Victorian data were derived from 13,943 Transport Accident Commission claims for injury compensation by drivers of 1982-92 model cars and station wagons which crashed during 1987-92, and whose claim records could be matched with Victoria Police accident reports.

The NSW data covered 221,971 drivers of 1982-92 model light passenger vehicles involved in Police reported crashes during 1987-92 which resulted in death or injury or a vehicle being towed away. As well as cars and station wagons, the files covered four-wheel drive vehicles, passenger vans, and light commercial vehicles. Of the 221,971 drivers involved in tow-away crashes, 31,127 were injured. When the data on the injured drivers from the two States were combined, there were data on 45,070 injured drivers available for analysis.

The makes and models of the crashed vehicles manufactured during 1982-92 were derived by processes which involved matching the crash records with the State vehicle register using the registration number, and then decoding information held on the register. The processes were the same as those outlined previously (Cameron et al 1992a, 1994).

For the investigation of the relationship between crashworthiness and year of manufacture, cars manufactured prior to 1982 were also considered. For this analysis, the data covered 423,612 drivers of cars, station wagons and taxis manufactured in 1964-92 who were involved in tow-away crashes in NSW during 1987-92. The data also covered 101,955 drivers of these vehicles who were injured in crashes in Victoria or NSW during the same years.

ANALYSIS

OVERVIEW - The crashworthiness rating (C) is a measure of the risk of serious injury to a driver of a car when it is involved in a crash. This risk can be considered to be the product of two probabilities:

- i) the probability that a driver involved in a crash is injured (injury risk), denoted by R;
- ii) the probability that an injured driver is killed or admitted to hospital (injury severity), denoted by S.

That is

$$C = R \times S.$$

To produce the updated crashworthiness ratings, each of the two components of the rating were obtained by logistic regression modelling techniques. Such techniques are able to simultaneously adjust for the effect of a number of factors (such as driver age and sex, number of vehicles involved, etc.) on probabilities such as the injury risk and injury severity.

LOGISTIC MODEL - The logistic model of a probability, P, is of the form:

$$\text{logit}(P) = \ln\left(\frac{P}{1-P}\right) = \beta_0 + \beta_1 X_1 + \dots + \beta_k X_k = f(X).$$

That is, the log of the odds ratio is expressed as a linear function, f, of k associated variables, $X_i, i=1, \dots, k$. Estimates of the parameter coefficients of the logit function, ie. the $\hat{\beta}_i$, can be obtained by maximum likelihood estimation (Hosmer & Lemeshow, 1989). The extension of this model to include interaction terms is straightforward.

LOGISTIC MODELS FOR EACH COMPONENT - To obtain crashworthiness ratings reflecting vehicle factors alone, it was necessary to develop logistic models of each of the crashworthiness components separately to identify possible factors, other than vehicle design, that might have influenced the crash outcomes. This was done initially without considering the type of car in the models as the aim was to determine which other factors were most likely to have an influence across a broad spectrum of crashes.

Logistic models were obtained separately for injury risk (R) and injury severity (S) because it was likely that the various factors would have different levels of influence on these two probabilities. The factors considered during this stage of the analysis for both injury risk and injury severity were:

- sex: driver sex (male, female)
- age: driver age (≤ 25 years; 26-59 years; ≥ 60 years)
- speedzone: speed limit at the crash location (≤ 75 km/h; ≥ 80 km/h)
- nveh: the number of vehicles involved (one vehicle; >1 vehicle)

These variables were available in both the Victorian and NSW crash data. Other variables (eg. whether or not the vehicle collided with a fixed object) were highly correlated with these variables, or were only available from one source and their inclusion would have drastically reduced the number of cases that could have been included in the analysis.

All data were analysed using the LR procedure of the BMDP statistical package (BMDP, 1988). Estimates of the coefficients of the logit function, $\hat{\beta}_i, i=1, \dots, k$, together with their associated standard errors, were obtained by maximum likelihood estimation. For both injury risk and injury severity, a stepwise procedure was used to identify which factors and their interactions made a significant contribution to these probabilities. All possible first order interactions were considered. A hierarchical structure was imposed so that if an interaction between two variables was included in the model then the corresponding main effects would also be included. The resultant logistic regression models were referred to as the "covariate" models.

ASSESSING CAR MODEL DIFFERENCES - Injury risk and injury severity for individual vehicle models were estimated after adding a variable representing car model to the respective logistic "covariate" models. The car model variable was forced into the logistic equation and individual car model coefficients were computed to represent deviations of that car from the average.

It was important to ensure that the logistic model adequately described the data and did not yield individual car model coefficients that were imprecise or unstable. For this reason, individual car models with small frequencies were pooled with similar car models, if appropriate, or they were excluded from the analysis. Car models were excluded if, after pooling models, either there were less than 100 involved vehicles or there were less than 30 injured drivers.

After exclusion, the regression analyses were performed on 87 individual car models (or pooled similar models). The variable representing car model was therefore categorical with 87 nominal levels. The choice of the design for the logistic model allowed the injury risk and injury severity estimates for each car model to be compared with the overall (average) rating for all models considered.

COMBINING INJURY RISK AND INJURY SEVERITY COMPONENTS - For a given model of car, j, the crashworthiness rating, C_j , was calculated as:

$$C_j = R_j \times S_j$$

where

R_j denotes the injury risk for car model j

S_j denotes the injury severity for car model j.

Each of the crashworthiness components can be thought of as a binomial probability, which is estimated by the logistic regression procedure as described above. If we let \hat{R}_j and \hat{S}_j denote the logistic estimates of injury risk and injury severity, respectively, then it is straightforward to show that

$$E(\hat{C}_j) = \hat{R}_j \cdot \hat{S}_j$$

and

$$\text{Var}(\hat{C}_j) = \text{Var}(\hat{R}_j \hat{S}_j) = \frac{\hat{R}_j(1-\hat{R}_j)}{n_j} \cdot \frac{\hat{S}_j(1-\hat{S}_j)}{m_j} + \frac{\hat{R}_j(1-\hat{R}_j)}{n_j} \cdot \hat{S}_j^2 + \frac{\hat{S}_j(1-\hat{S}_j)}{m_j} \cdot \hat{R}_j^2$$

where

n_j is the number of cars of model j used to estimate R_j

m_j is the number of cars of model j used to estimate S_j .

The 95% confidence interval for the combined rate is

$$\hat{R}_j \hat{S}_j \pm 1.96 \cdot \sqrt{\text{Var}(\hat{R}_j \hat{S}_j)}$$

Because each of the two estimated crashworthiness components have been adjusted for the effect of other factors by logistic regression prior to their incorporation into the combined ratings, the resultant crashworthiness rating is also adjusted for the influence of these factors.

RELATIONSHIPS WITH YEAR OF MANUFACTURE - For this analysis, the year of manufacture of the cars, station wagons and taxis replaced the car model in the logistic modelling process. There were considerable variations across years of manufacture of the crash-involved vehicles in terms of the mix of driver ages and, to a somewhat lesser extent, of driver sex. There were also considerable variations in terms of the mix of speed zones and the crash type (number of vehicles involved). These variables were considered in the separate logistic models for injury risk and injury severity when investigating the relationship of crashworthiness with year of manufacture.

RESULTS

ESTIMATING INJURY RISK - The logistic regression model of the injury risk component of the crashworthiness ratings of 1982-92 model vehicles was estimated from the data on 221,971 drivers involved in tow-away crashes in NSW. Because of missing values of some of the factors to be included in the logistic regression, analysis was performed on data relating to only 182,616 involved drivers.

Both driver sex and speedzone were significantly associated with injury risk and were included in the logistic regression as both main effects and an interaction term. No other variable or interaction term significantly improved the fit of the logistic model. The final "covariate" model for injury risk was

$$\log\left(\frac{R}{1-R}\right) = -1.41 + 0.281 \cdot \text{sex} + 0.492 \cdot \text{speedzone} - 0.028 \cdot \text{sex} \cdot \text{speedzone}$$

ESTIMATING INJURY SEVERITY - The injury severity component was based on 45,070 drivers of 1982-92 model vehicles who were injured in crashes in Victoria or NSW during 1987-92. Because of missing values of some of the associated crash factors, logistic regression was performed on data relating to only 33,543 injured drivers.

The analysis identified a number of important factors - driver sex, driver age, speedzone and *nveh*. In addition, significant interactions were found between sex and age, sex and *nveh*, and age and *nveh*. No other variable or interaction term significantly improved the fit of the logistic model. The final "covariate" model for injury severity was

$$\begin{aligned} \log\left(\frac{S}{1-S}\right) = & -0.93 + 0.092 \cdot \text{sex} - 0.249 \cdot \text{age}(1) + 0.414 \cdot \text{age}(2) + 0.338 \cdot \text{speedzone} \\ & + 0.229 \cdot \text{nveh} + 0.117 \cdot \text{sex} \cdot \text{age}(1) - 0.151 \cdot \text{sex} \cdot \text{age}(2) - 0.005 \cdot \text{age}(1) \cdot \text{nveh} \\ & - 0.060 \cdot \text{age}(2) \cdot \text{nveh} - 0.208 \cdot \text{speedzone} \cdot \text{nveh} \end{aligned}$$

The driver age variable had three categories and so two contrasts were set up in the regression analysis to handle this variable. Age(1) refers to the comparison of drivers aged 26-59 years with those aged <26 years whereas age(2) compares older drivers (>59 years) to the 26-59 year olds.

CRASHWORTHINESS RATINGS - The crashworthiness rating for each car model was obtained by multiplying the individual injury risk and injury severity estimates. The appendix gives the crashworthiness ratings and the associated 95% confidence intervals for the 87 car models included in the analyses. The appendix also indicates the overall ranking of the crashworthiness ratings from 1 (lowest or best crashworthiness rating) to 87 (highest or worst crashworthiness rating).

Each crashworthiness rating is an *estimate* of the true risk of a driver being killed or admitted to hospital in a tow-away crash and, as such, each estimate has a level of uncertainty about it. This uncertainty is indicated by the confidence limits. There is 95% probability that the confidence interval will cover the true risk of serious injury (death or hospital admission) to the driver of the particular model of vehicle.

The ratings in the appendix exclude those models where the width of the confidence interval exceeded 7, or the ratio of the confidence interval width to the rating score exceeded 2.1. This second criterion was necessary because smaller confidence intervals tended to occur for the lower rating scores, but the confidence intervals were relatively wide in proportionate terms.

COMPARISONS WITH THE ALL MODEL AVERAGE RATING - The confidence limits can be used to judge whether the true risk of death or hospital admission for a driver of a specific model car involved in a tow-away crash is significantly different from the overall average for all models, ie. 2.66 per 100 involved drivers. An upper limit below the average is indicative of superior crashworthiness, whereas a lower limit above the average suggests inferior crashworthiness. Other models also have crashworthiness ratings at the low or high end of the scale, but their confidence limits overlap the all model average. Although such models may also have superior or inferior crashworthiness characteristics, the data base did not contain sufficient numbers of these models for the data to represent scientific evidence that this is the case.

Fifteen models had ratings representing evidence of superior crashworthiness because their upper confidence limits were less than the average rating. Five of these were large cars and a further six were luxury models. Two were classified as medium cars and one was a relatively old small car. The remaining model was a commercial panel van based on one of the large passenger car models displaying superior crashworthiness. The specific models were (in order of estimated risk of serious driver injury in a crash, from lowest to highest):

- BMW 5 Series (1983-92 years of manufacture)
- Saab 900 Series (1983-92)
- Peugeot 505 (1983-92)
- Honda Accord (1986-89)
- Volvo 200 Series (1982-92)
- Toyota Crown/Cressida (1982-85)
- Honda Prelude (1983-92)
- Ford Falcon Panel Van (1982-92)
- Ford Telstar / Mazda 626 (1988-91)
- Ford Falcon EA Sedan (1988-91)
- Holden Commodore VN/VP (1988-92) / Toyota Lencen (1989-92)
- Ford Falcon X-series Wagon (1982-88)
- Mitsubishi Magna (1985-90)
- Ford Falcon X-series Sedan (1982-88)
- Toyota Corolla (1982-84).

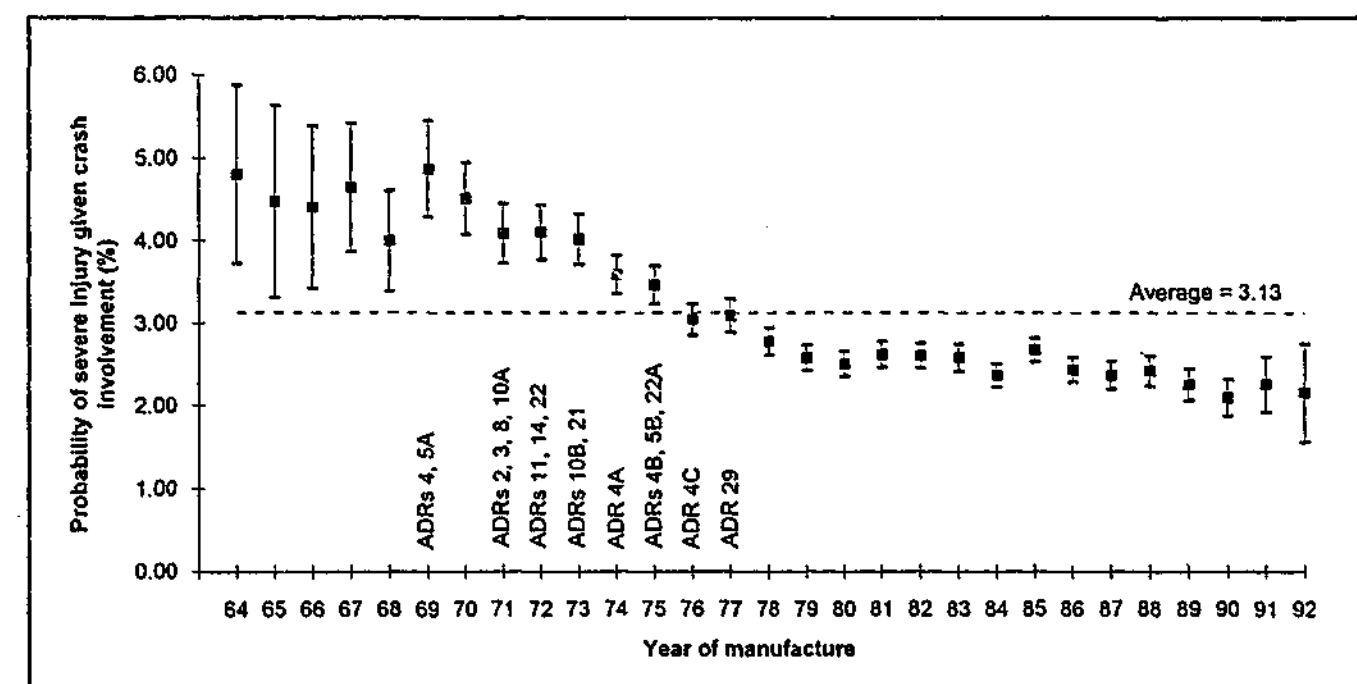
Eleven models had ratings representing evidence of inferior crashworthiness because their lower confidence limits were greater than the average rating. Seven were small cars, three were light commercial vehicles, and the remaining model was a pooled family of passenger vans. The specific models were (in order of estimated risk of serious driver injury in a crash, from highest to lowest):

- Subaru Sherpa/Fiori (1989-92)
- Suzuki Mighty Boy (1985-88)
- Holden Carry (1985-90) / Suzuki Scurry (1985-87)
- Subaru Brumby (1982-92)

- Daihatsu Handivan (1982-90)
- Suzuki Hatch (1982-89)
- Daihatsu Charade (1982-86)
- Holden Barina (1985-88) / Suzuki Swift (1985-88)
- Honda Civic (1984-87)
- Nissan Pulsar/Vector (1982-86) / Holden Astra (1982-86)
- Mitsubishi passenger vans (1982-92).

CRASHWORTHINESS RELATED TO YEAR OF MANUFACTURE - The analysis was performed on 423,612 drivers of 1964-92 model vehicles involved in tow-away crashes in NSW, and 101,955 injured drivers of the same models from Victoria and NSW. The "covariate" models from this larger set of data were somewhat more complex than those obtained when crashes and injuries from the 1982-92 model vehicles were considered (see earlier results), perhaps reflecting the effects of the considerable variations in driver age and sex, speed zone at the crash location, and crash types in vehicles manufactured over the full period 1964 to 1992.

Fig. 1 - Crashworthiness by year of manufacture (with 95% confidence limits), and commencement years of Australian Design Rules (ADRs) for motor vehicle safety aimed at occupant protection



There was a general and significant improvement in vehicle crashworthiness with increasing year of manufacture over the years considered (Figure 1). Specifically however, little improvement can be seen in the years 1964 to 1969 followed by rapid improvement over the period 1970 to 1979 with a plateau from 1980 to 1985. There is some visual evidence of a decreasing trend in the period after 1985 although this trend is not statistically significant. The relatively wide confidence intervals observed on the estimates for the years of

manufacture 1964 to 1969 and 1992 are a reflection of the smaller numbers of crashes involving vehicles manufactured in these years appearing in the data.

To summarise the magnitude of the improvement in crashworthiness seen in vehicles during the 1970's, the average crashworthiness estimate for the 1980-92 year vehicles was compared with the average for those manufactured during 1964-69. This showed a 47% reduction in the risk of serious injury for drivers involved in tow-away crashes while driving the post-1979 vehicles compared with those manufactured during the 1960's.

Drivers of vehicles manufactured during 1970 to 1979 could be expected to have benefited from the commencement of a number of Australian Design Rules (ADRs) for motor vehicle safety which previous research has shown to be effective in providing occupant protection (Cameron 1987), namely:

- ADR 4 (seat belts fitted in front seats) from 1/1/69
- ADR 2 ("anti-burst" door latches and hinges) from 1/1/71
- ADR 10A ("energy-absorbing" steering columns) from 1/1/71
- ADR 22 (head restraints) from 1/1/72
- ADR 10B (steering columns with limited rearward displacement) from 1/1/73
- ADR 4B (inertia reel seat belts fitted in front seats) from 1/1/75
- ADR 22A (minimum-height adjustable head restraints) from 1/1/75
- ADR 29 (side door strength) from 1/1/77.

In addition, the following ADRs introduced over the same period could also be expected to have provided increased injury protection for drivers:

- ADR 5A (seat belt anchorage points for front seats) from 1/1/69
- ADR 3 (strengthened seat anchorages) from 1/1/71
- ADR 8 (safety glass in windscreens and side windows) from 1/7/71
- ADR 11 ("padded" sun visors) from 1/1/72
- ADR 14 ("breakaway" rear vision mirrors) from 1/1/72
- ADR 21 ("padded" instrument panels) from 1/1/73
- ADR 4A (improved seat belt buckles), effective from 1/4/74
- ADR 5B (improved location of seat belt anchorages) from 1/1/75
- ADR 4C (dual-sensing locking retractor inertia reel seat belts) from 1/1/76.

The years of implementation of these ADRs are shown on Figure 1 for comparison with the crashworthiness estimates for the vehicles manufactured during the 1970's. Legislation for compulsory wearing of fitted seat belts, which was introduced in all Australian States and Territories during the early 1970's, was an important adjunct to the ADRs which required the fitting of seat belts in new cars. The years after 1978 were characterised by relatively few new ADRs for occupant protection in passenger vehicles, so it is not surprising that crashworthiness appeared to plateau after 1979.

CONCLUSIONS

The expansion of the crash data base through the addition of four years crash data from NSW and two years from Victoria, and the use of an improved method of analysis, has resulted in more up-to-date, extensive and reliable estimates of the crashworthiness of individual vehicle models than those published previously. The crashworthiness ratings and their associated confidence limits have identified 26 models of passenger cars, four-wheel drive vehicles, passenger vans and light commercial vehicles which have superior or inferior crashworthiness characteristics compared with the average vehicle.

The crashworthiness of passenger cars in Australia has improved over the years of manufacture from 1964 to 1992. The improvements have not been regular each year and showed the greatest gains in those cars manufactured over the years 1970 to 1979 during which a number of new Australian Design Rules aimed at occupant protection took effect. There was an estimated 47% reduction in the risk of serious injury for drivers involved in tow-away crashes while driving the post-1979 vehicles compared with those manufactured during the 1960's.

The results and conclusions presented in this paper are based on a number of assumptions and qualifications. In particular it should be noted that other factors not collected in the data (eg. crash speed) may have differed between the models and across years of manufacture, and thus may have affected the results. In addition, vehicle safety was measured only in terms of the injury protection afforded to drivers occupying the cars of specific models and years of manufacture, and did not take into account the threat of injury to pedestrians or occupants of other cars.

ACKNOWLEDGMENTS AND DISCLAIMER

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The work described in the second part of this paper related to crashworthiness and year of manufacture was funded by a grant from the Royal Automobile Club of Victoria Ltd. No conditions have been attached to the republication of the results related solely to year of manufacture.

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APPENDIX
Crashworthiness Ratings of 1982-92 Models of Cars Involved in Crashes During 1987-92

Make	Model of car	Year of manufacture	Serious injury rate per 100 drivers involved	Overall rank order	Lower 95% confidence limit	Upper 95% confidence limit
ALL MODEL AVERAGE			2.66	2.60	2.73	
Large cars			3.11	3.00	3.27	
Ford	FALCON EX/EB WAGON	88-92	2.17	23	1.40	2.94
Ford	FALCON EA SEDAN	88-92	2.19	25	1.78	2.59
Ford	FALCON EB SEDAN	82-84	2.19	26	0.25	4.13
Holden	COMMODORE VN/VP	90-92	2.25	28	0.91	3.61
Toyota	LEXCEN	89-92	2.72	45	1.60	3.84
Ford	FALCON X SERIES WAGON	82-88	2.77	47	2.34	3.20
Mitsubishi	MAGNA	85-90	2.79	49	2.15	3.43
Ford	FALCON X SERIES SEDAN	82-88	2.80	50	1.64	3.97
Nissan	SKYLINE	82-90	2.86	51	2.64	3.09
Holden	COMMODORE VH-VL	82-88	2.89	53	1.67	4.13
Medium cars			2.95	58	2.51	3.41
Pugeot	505	83-92	2.99	60	1.97	4.01
Ford	TELSTAR	88-91	3.02	61	2.49	3.54
Mazda	626	88-91	3.07	62	1.66	4.48
Nissan	PINTARA	89-92	3.28	65	2.33	4.23
Ford	CORSAIR	88-92	3.29	66	2.34	4.24
Toyota	CAMRY	87-92	3.33	68	1.58	5.08
Holden	APOLLO	89-92	3.61	73	3.12	4.10
Mitsubishi	NIMBUS	82-91	3.80	74	2.72	4.88
Nissan	STANZA	82-83	3.87	75	2.51	5.23
Toyota	CORONA	83-87	4.03	77	1.94	6.13
Subaru	1800/LEONE	82-90	4.17	80	3.27	5.07
Mitsubishi	SIGMA/SCORPION	82-87	4.40	81	3.54	6.05
Nissan	PINTARA	86-88	4.56	82	3.35	6.97
Ford	TELSTAR	83-87	4.62	83	3.14	8.35
Mazda	626	83-86			3.73	9.51
Nissan	BLUEBIRD	82-86				
Holden	CAMIRA	83-88				
Mazda	626	82				
Nissan	PRAIRIE	83-86				
Toyota	CAMRY	83-86				
Small cars			2.33	2.22	2.44	
Toyota	COROLLA	89-92	1.79	12	0.88	2.70
Holden	NOVA	89-92	1.94	15	1.43	2.44
Toyota	COROLLA	82-84	1.99	17	0.72	3.26
Mazda	323	90-92	2.11	18	1.74	2.49
Mitsubishi	LANCER	88-92	2.15	20	1.78	2.53
Mitsubishi	CORDIA	83-89	2.15	21	1.85	2.46
Toyota	COROLLA	85-88	2.16	22	1.98	2.35
Nissan	PULSAR/VECTRA	88-90	2.37	35	1.71	3.03
Holden	ASTRA	88-90	2.50	39	2.30	2.70
Ford	LASER/METEOR	90				
Ford	LASER/METEOR	82-89				
Mazda	323	82-88				
Holden	BARINA	89-92				
Suzuki	SWIFT	89-92				
Mitsubishi	COLT	82-90				
Nissan	PULSAR/VECTRA	87				
Holden	ASTRA	87				
Holden	GEMINI	82-84				
Honda	CIVIC	88-92				
Holden	GEMINI	85-87				
Hyundai	EXCEL	86-92				
Honda	CIVIC	82-83				
Nissan	PULSAR/VECTRA	82-86				
Holden	ASTRA	84-86				
Honda	CIVIC	84-87				
Daihatsu	CHARADE	88-92				
Ford	LASER/METEOR	91-92				
Holden	BARINA	85-88				
Suzuki	SWIFT	85-88				
Daihatsu	CHARADE	82-88				
Suzuki	HATCH	82-89				
Daihatsu	HANDICAN	82-90				
Subaru	SHERPA/FIORI	89-92				

Crashworthiness Ratings of 1982-92 Models of Cars Involved in Crashes During 1987-92

Make	Model of car	Year of manufacture	Serious injury rate per 100 drivers involved	Overall rank order	Lower 95% confidence limit	Upper 95% confidence limit
ALL MODEL AVERAGE			2.66		2.60	2.73
Passenger vans			3.41		2.87	3.94
Toyota	TARAGO	83-90	2.89	52	2.12	3.87
Mitsubishi	PASSENGER VANS	82-92	3.59	72	2.88	4.31
Commercial vehicles (GVM <= 3000kg)			3.02		2.69	3.34
Ford	FALCON PANEL VAN	82-92	1.83	13	1.04	2.62
Nissan	NAVARA	86-92	2.18	24	1.24	3.11
Ford	FALCON UTE	82-92				
Nissan	XFN UTE	88-92	2.27	28	1.68	2.87
Holden	RODEO	82-92	2.59	41	1.55	3.83
Nissan	720 UTE	82-85	2.90	55	1.76	4.04
Toyota	HIACE/LITEACE	82-92	2.97	59	2.37	3.58
Holden	SHUTTLE	82-91	3.07	63	0.85	5.29
Subaru	BRUMBY	82-92	5.79	84	3.77	7.81
Holden	CARRY	85-90				
Suzuki	SCURRY	85-87	5.90	85	2.86	8.95
Suzuki	MIGHTY BOY	85-88	6.57	86	3.77	9.37

Sporta cara			1.07	2.26	3.88
Toyota	CELICA	82-85	2.90	54	1.83
Mazda	RX7	82-85	3.34	69	1.41
Alfa Romeo	33	82-92	3.58	71	1.24

4-Wheel drive vehicles			2.85	2.34	2.98
Nissan	PATROL	82-92			
Ford	MAVERICK	88-92	1.94	18	1.15
Mitsubishi	PAJERO	83-92	2.24	27	1.22
Toyota	LANDCRUISER	82-92	2.47	37	1.84
Toyota	4RUNNER/HILUX	82-92	2.59	40	2.13
Daihatsu	ROCKY F70/75	87-92	3.63	70	1.08
Suzuki	SIERRA	82-92	3.87	76	2.62

Paper No. 18

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EVALUATION OF THE COUNTRY RANDOM BREATH TESTING AND PUBLICITY PROGRAM IN VICTORIA 1993-1994

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SUMMARY

In November 1993, the Victoria Police in conjunction with the Transport Accident Commission launched a major program in country Victoria in an effort to increase the number of random breath tests (RBT) to at least 700,000 tests in a twelve month period, and supported the enforcement operations with mass-media publicity. The objectives of the research project are to evaluate the program in terms of implementation characteristics, effects on road trauma, and cost-effectiveness, and to provide information to optimise the mix and levels of the major components of RBT programs (enforcement activities and supporting publicity) in the future.

From the introduction of the program in November 1993 to the end of 1994, 790,445 random breath tests were conducted in country Victoria. There was substantial variation in RBT activity patterns between the country Police Districts. The intensity of the supporting publicity was also measured, both directly and in terms of awareness levels in country areas. There was a 9% reduction in 'high alcohol hour' serious casualty crashes below expected levels in country Victoria during the program. Crashes in smaller areas of country Victoria influenced by the RBT activity were analysed so that the effects of styles of RBT operations, and the interactions of these effects with the levels of publicity awareness, could be seen.

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Narelle Mullan has a B.App.Sc. in Mathematics and Computing from Curtin University and is currently a Research Assistant at Monash University Accident Research Centre. Her involvement at MUARC is in the area of road accident data analysis, countermeasure evaluation and, more recently, spatial analysis using desktop mapping.



Sandra Gantzer was a Research Assistant at the Monash University Accident Research Centre and holds a B.Sc.(Hons) in mathematical statistics. Her research at the Accident Research Centre was in the area of road crash data analysis and countermeasure evaluation with particular focus on statistical methodology. She is currently employed by the Transport Accident Commission as a Research Analyst and continues research in the road safety field with a focus on data analysis.

INTRODUCTION

1. In November 1993, the Victoria Police, in conjunction with the Transport Accident Commission (TAC), launched a major program aimed at increasing random breath testing (RBT) in country Victoria and supporting these enforcement operations with mass-media publicity. The aim was to increase the number of random breath tests conducted in country Victoria to at least 700,000 during a 12 month period, while maintaining the high levels of testing activity built up in Melbourne during 1990-93. The program was subsequently extended beyond November 1994 after the initial 12 months. This paper covers an evaluation of the program to the end of 1994 to include the substantial levels of RBT activity and drink-driving publicity during December 1994.
2. To achieve the goal of at least 700,000 tests, the TAC contracted the Victoria Police to send additional Melbourne-based "booze buses" to country Police districts to supplement the six buses placed permanently in country areas. These buses are purpose built, highly visible vehicles introduced in Victoria during 1989, from which the Police can conduct breath testing operations. They are staffed by a greater number of personnel conducting tests than car-based operations. Police cars were also used as RBT stations in country Victoria during the program, sometimes in conjunction with bus-based operations in the same area. The cars are equipped with an illuminated roof-top sign which can display an RBT message during testing operations (Moloney 1994).
3. The TAC also launched a Country RBT advertising campaign through rural media (television, radio, press and billboards) during November/December 1993, designed to raise the awareness of country residents to the new level of RBT activity. Other mass media publicity with drink-driving themes was placed in these same media during the second half of 1994.

EVALUATION OBJECTIVES

4. The evaluation has the following objectives:
 - To evaluate the country RBT and publicity program in terms of implementation characteristics, its effects on road trauma, and the cost-effectiveness of the program and its components.
 - To provide information to optimise the mix and levels of the major components of RBT programs (enforcement activities and supporting publicity) in the future.
5. The project is being carried out in three stages, namely the process evaluation (documentation of the RBT program), the crash-based evaluation, and the economic analysis of the program cost-effectiveness. This paper describes the methods and results of the first two stages.
6. The first stage of the project summarised the levels and nature of the RBT operations and supporting publicity in country Victoria during late 1993 to December

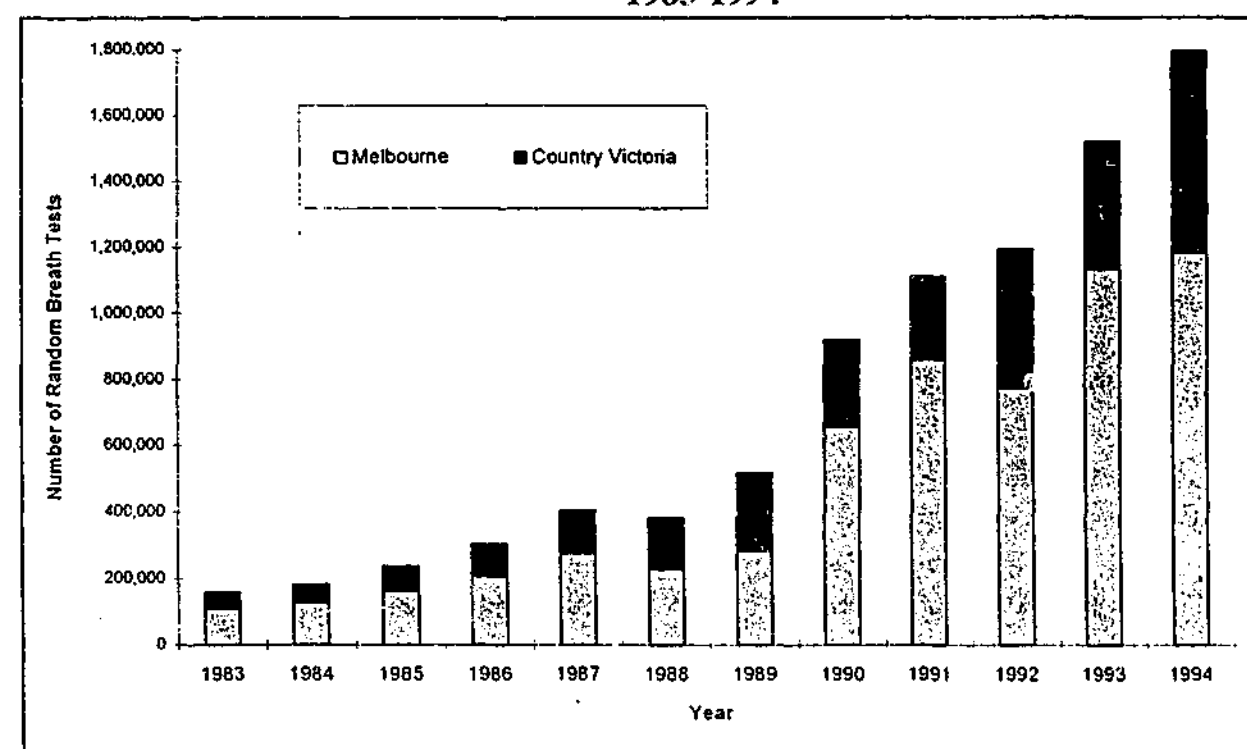
1994. The second stage (crash-based evaluation) was carried out in two parts aiming to measure (1) the general effects of the program across the whole of country Victoria, and (2) the localised effects in the areas and periods directly influenced by the program, so that the differential effects of the styles of RBT operations and levels of publicity support can be seen.

PROGRAM OPERATIONS DURING 1993-1994

Background

7. Victoria Police introduced random breath testing of drivers for blood alcohol levels in July 1976. Random breath testing (RBT) was predominantly conducted from Police cars from that time although four relatively small Toyota Coaster buses were used as well (Sullivan, Cavallo & Drummond, 1992). In late 1989, custom-built buses were gradually introduced in Victoria to increase the number of drivers who were random breath tested and to enhance the visibility of RBT operations. These are now commonly known as "booze buses". By exposing more drivers to random breath testing, it was suggested that buses would likely act as a greater deterrent to drink-driving than car-based tests. The shift from traditional car-based to bus-based RBT operations predominantly occurred in Melbourne. Country areas of Victoria had a smaller and delayed use of bus-based tests during 1990 (Cavallo & Cameron, 1992). The total numbers of tests conducted each year in these two regions of Victoria during 1983-1994 are shown in Figure 1.

Figure 1: Annual Number of RBTs in Melbourne and Country Victoria, 1983-1994



Method

8. RBT activity in country Victoria during November 1993-December 1994 was examined by location and drink-driving deterrence related factors. These factors included the style of RBT operation, frequency of RBT sessions, number of tests, and hours of testing. In addition, average rates such as the average tests per session and average tests per hour were determined to assess if there was any variation in RBT operations conducted in each Police District. The style of operation was defined by the type of Police vehicle used in each RBT session. The styles considered were a TAS Bus (fully-staffed Melbourne-based booze bus sent by the Traffic Alcohol Section, TAS, of the Police to country Victoria primarily on weekends), a District Bus (country-based booze bus operated by Police Districts in country Victoria) and a Police car (generally cars operated by Police based in country Victoria).

9. At the same time as the Melbourne-based buses commenced country operations on weekends, a country RBT publicity campaign was launched in a 4 week burst at the end of 1993. All advertisements were targeted at drink-driving males aged 18-29 living and driving in rural areas and being exposed to rural media. There were also other drink-driving bursts, with rural themes, during July, September/October and November/December 1994. Television exposure was measured weekly using TARPs (Target Audience Rating Points), a measure of audience reach, during November 1993 to December 1994. Drink-driving advertising awareness for country Victoria television viewers was measured by a function of TARPs related to "Adstock" (Broadbent, 1979) which represents the audience's retained awareness of current and past levels of advertising. The function used will be defined later in this paper.

Results

10. The number of random breath tests conducted in country Victoria during late November 1993 to December 1994 increased to 790,445 tests of which 424,545 were completed by Melbourne-based TAS booze buses. The monthly average number of tests in country Victoria during December 1993 to December 1994 rose to 59,839, a 74% increase compared with the average of 34,321 tests during January to November 1993. In Melbourne the relatively high level of RBT activity achieved in 1993 (1,088,330 tests) was maintained with 1,093,355 tests completed in 1994 after implementation of the program. Figure 2 displays the monthly number of random breath tests conducted during 1993-1994 by each style of country RBT enforcement.

11. There was considerable variation in testing operations related to the style used, with TAS booze buses conducting fewer RBT sessions (8%) than both cars (81%) and District buses (10%) but random breath testing a greater proportion of the drivers tested (54%) than the other two styles (20% and 26% for District buses and cars, respectively). There were also substantial variations across Police Districts in the mix of different styles, in part related to a tendency for TAS buses not to be sent to Districts furthest from Melbourne. Figure 3 shows the total number of testing hours conducted by each style in each Police District.

Figure 2: Monthly Number of RANDOM BREATH TESTS conducted by CARS and BOOZE BUSES in Country Victoria, 1993-1994

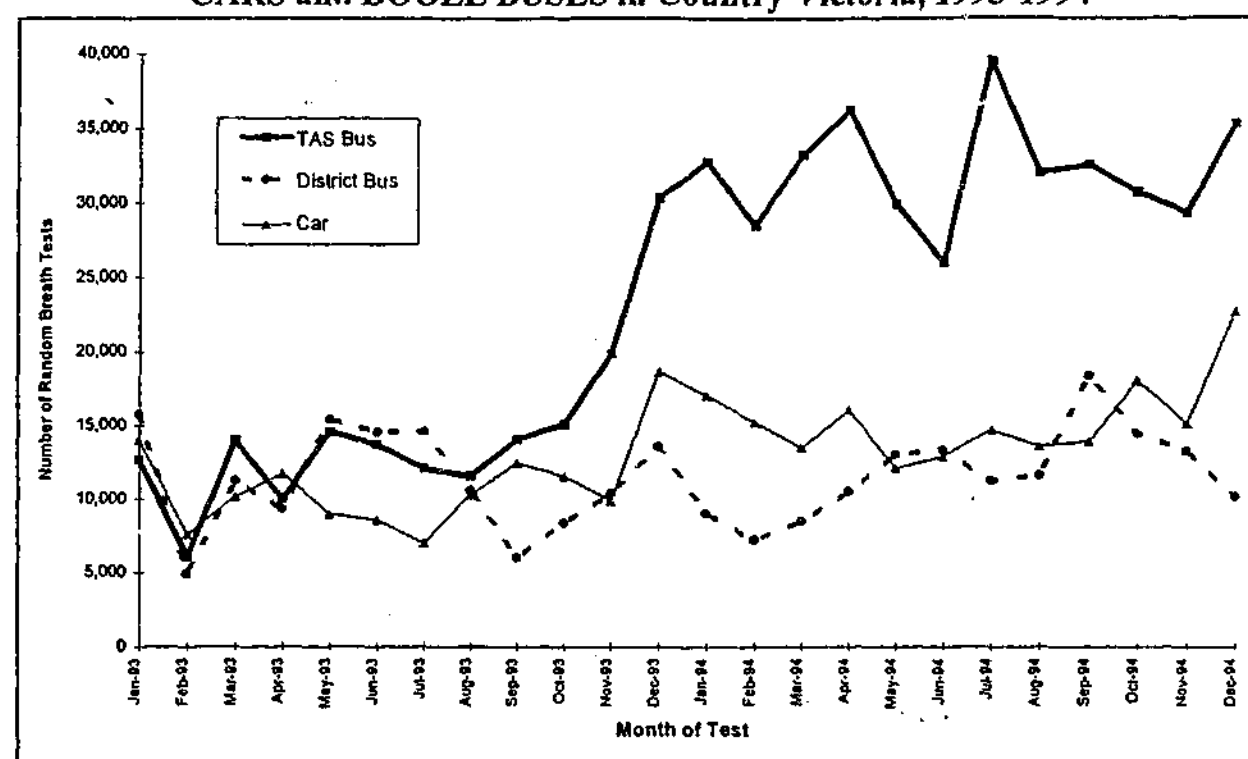
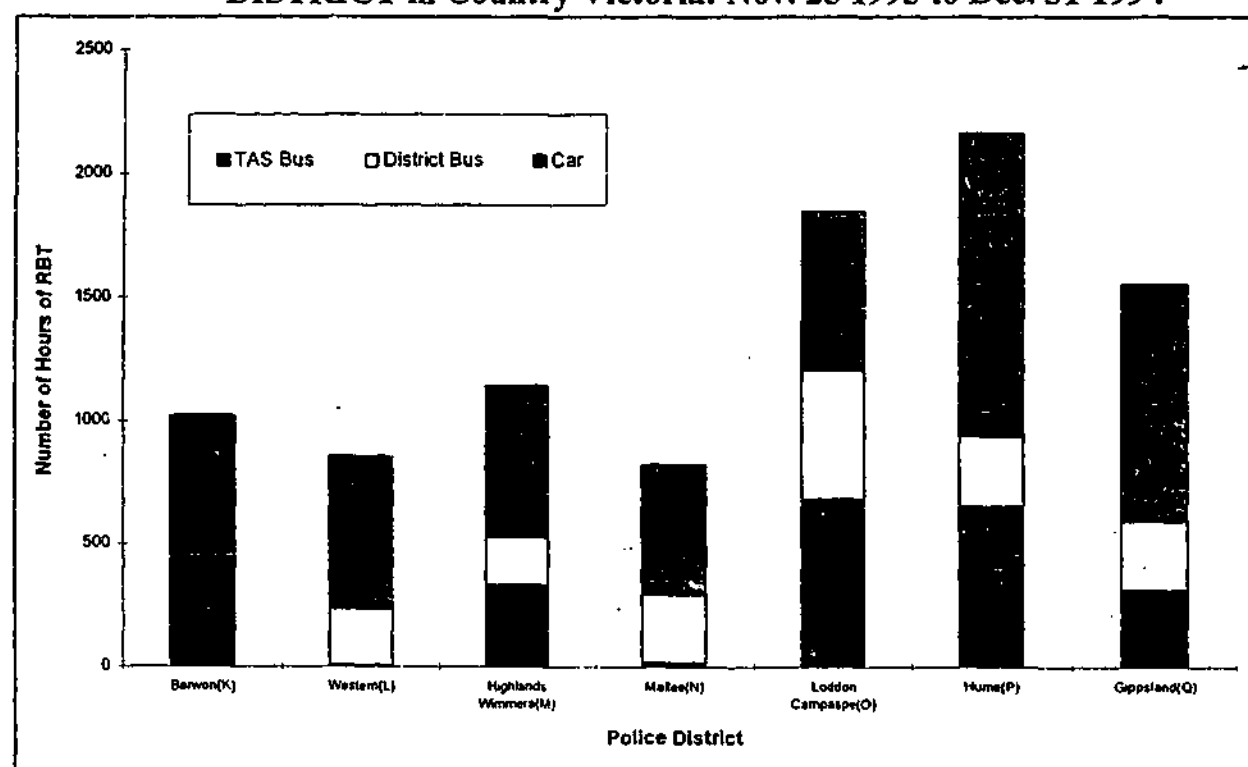


Figure 3: Number of HOURS of RBT by STYLE OF OPERATION and DISTRICT in Country Victoria: Nov. 25 1993 to Dec. 31 1994

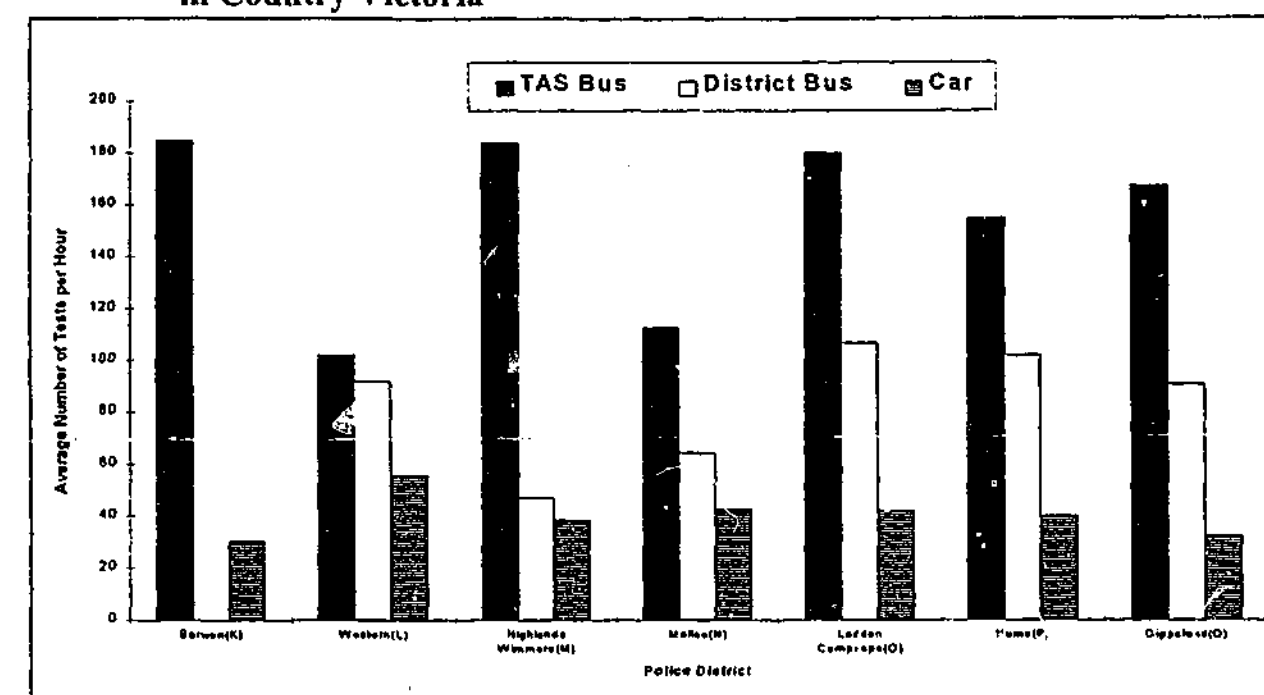


12. TAS buses were able to conduct more tests per hour than the other styles of operation, testing nearly twice as many as District buses and more than four times as many as cars. The number of tests per hour completed by TAS buses, District buses and cars on average were 172, 89 and 40 tests respectively. The TAS buses appeared to test greater numbers per hour than District buses in country Victoria because:

- TAS buses were manned by more personnel than District buses;
- Often TAS buses were sent to higher traffic volume locations than District buses;
- TAS bus personnel achieved a greater level of training than those operating District buses.

13. The average number of tests per hour also varied by Police District for each style of operation, perhaps also reflecting lower traffic volumes in some Districts (Figure 4). Note that there were no District buses used in Barwon District during the program.

Figure 4: Average Number of Tests per Hour by Style of Operation and District in Country Victoria

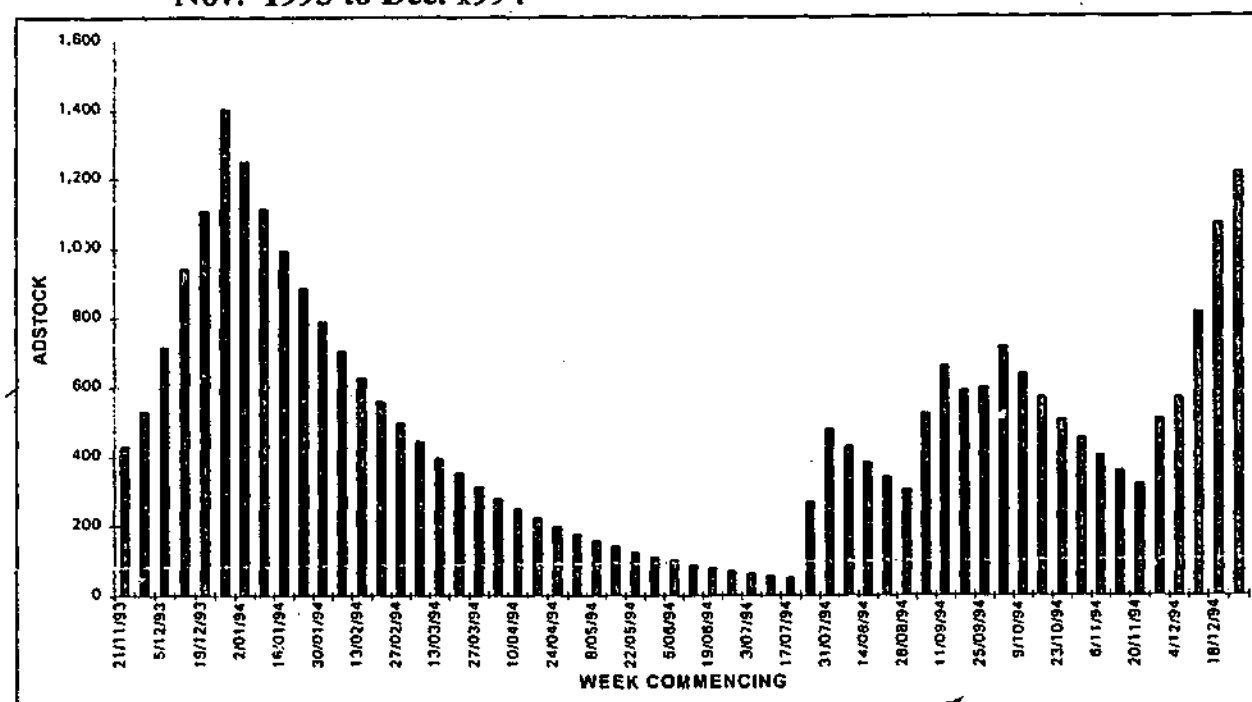


14. Surveys of unaided recall of road safety television advertising with drink-driving themes conducted for TAC were compared with weekly levels of television exposure during 1990-1994. Broadbent (1979) has found that the retained awareness of a burst of advertising appears to decay exponentially at a constant rate. He defined a time-related function "Adstock" which is the weighted cumulative sum of current and past levels of advertising, with decay factors applied to past levels. He rescaled the decay factors so that the weights total one, but no rescaling was done in the Adstock function used here. A range of decay factors, covering decay "half-lives" (Fry 1996) from 2 to 12 weeks, were considered. The Adstock based on each half-life was regressed against the level of awareness of drink-driving advertising measured in the surveys, and the one with the highest correlation coefficient was considered the best estimate. For country Victorian television viewers, the awareness of drink-driving advertising appeared to decay to half the audience 6 weeks after the showing of the advertisement.

15. Figure 5 represents the estimated awareness levels (measured by Adstock, as defined) of the drink-driving related publicity which accompanied the increased country RBT operations, for each week during the period of the program. This figure provides a better indication of the time variation of the publicity awareness than the five surveys conducted at irregular times during the period. It can be seen that the publicity support

varied considerably during the program period, falling to relatively low levels of awareness during April - July 1994. It should be noted that high levels of awareness occurred only during the Christmas holiday period, when there is typically subtle changes to enforcement practices, and other unrelated factors operate to affect crashes (such as increased recreational travel).

Figure 5: Drink-Driving Advertising Adstock per Week in Country Victoria, Nov. 1993 to Dec. 1994



GENERAL EFFECTS ON CRASHES IN COUNTRY VICTORIA

Background

16. This part of the crash-based evaluation aimed to link the program with reductions in serious casualty crashes (those crashes resulting in death or serious injury) during the "high alcohol hours" of the week in country Victoria, ie. those periods when illegal drink-driving is more likely to occur. A previous definition of these high alcohol hours (Harrison 1990) had been based on the analysis of blood alcohol concentration (BAC) readings from drivers who were killed or treated at hospital as a result of crashes throughout Victoria during 1988 and 1989. This concept was updated and made more specific to the purposes of this study by analysing the BAC readings of killed and seriously injured drivers during 1990-94, for country Victoria and Melbourne separately.

17. The country high alcohol hours (HAH) which resulted from this re-analysis (Gantzer 1995) were 6pm Sunday to 6am Monday, 6pm to 4am on Monday to Wednesday nights, 6pm Thursday to 6am Friday, 6pm Friday to 8am Saturday, and 4pm Saturday to 10am Sunday. The remaining hours of the week were described as the country "low alcohol hours" (LAH). Gantzer (1995) suggested that the proportion of drivers killed or treated at hospital from country Victoria crashes during HAHs who had a BAC exceeding 0.05% was nearly seven times the proportion during LAHs (however,

due to bias in the data during 1990-94, the absolute proportions estimated from this data are not reliable).

Method

18. The estimated percentage change in crashes in country Victoria during the RBT program was compared with the estimated percentage change in the Melbourne area and in country NSW for the same period. Multivariate time series modelling of monthly numbers of high alcohol hour serious casualty crashes during 1984-94 was undertaken to estimate the change in these crashes related to the country RBT program, following methods similar to Cameron et al (1992a). The models were multiplicative functions of a number of factors considered to have affected the crashes, including a step function for the November 1993 to December 1994 period. A double-log transformation of the dependent and independent variables was carried out before the models were fitted by multivariate linear regression. The estimated change in crashes was derived from the coefficient of the step function. Thus this estimate took into account factors such as unemployment rate, alcohol sales and drink-driving publicity, which had been found from previous research to have strong links with high alcohol hour crashes (Newstead et al 1995a).

Results

19. The time series modelling of crashes in country Victoria found a statistically significant reduction of 9.5% in serious casualty crashes during the high alcohol hours of the week in the period November 1993 to December 1994. This was derived from the estimated coefficient of the step function included in the model of monthly variations of these crashes during 1984-94. The coefficient was found to be statistically significant and necessary to fully explain the crash variations over the period. Parallel analyses of crashes in Melbourne and country NSW found no statistically significant evidence of changes in HAH crashes during the same period, but there were indications of a reduction in HAH crashes in Melbourne (4.3%). The changes in high alcohol crashes are presented in Table 1. The reduction in crashes in country Victoria during the high alcohol hours of the week in the period November 1993 to December 1994 may have been due to the country RBT and publicity program. More definitive conclusions may be reached following an examination of the localised effects of the enforcement operations on crashes.

Table 1: Percentage Changes in High Alcohol Hour Serious Casualty Crashes in Country Victoria, Melbourne and Country NSW during November 1993 to December 1994

	Percentage Change and 95% Confidence Interval		
	Country Victoria	Melbourne	Country NSW
High alcohol hour serious casualty crashes	-9.48% * (-0.42%, -17.71%)	-4.26% (2.30%, -10.07%)	+5.54% (-2.61%, 14.37%)

* Statistically significant at $p < 0.05$ level

LOCALISED EFFECTS ON CRASHES IN COUNTRY VICTORIA

20. The aim of the second part of the crash-based evaluation was to measure the effects of the program on crashes in smaller areas of country Victoria influenced by the RBT activity, during periods of time when the enforcement was operating. This analysis was carried out so that the effects of the different styles of RBT operations, and the interaction of these effects with different levels of publicity awareness, could be seen. However, it should be noted that the nature of the analysis was such that only the effects of the changes in RBT activity, due to the program, could be seen, not the absolute effects of country RBT activity. In addition, because there was RBT activity in country Victoria before the program, the analysis relied upon the assumption that the aggregation of crashes across a number of areas had the effect of averaging out the between-area variations in RBT activity and styles of operation. For this reason, the most reliable results will be those based on aggregations of crashes over broad areas.

Method

21. Data had been obtained on RBT sessions conducted by Police from 25 November 1993 to 31 December 1994. Included in the data were the number of tests and hours of RBT operation, as well as the LGA of country Victoria where each session operated. A total of 153 different LGAs had some form of RBT activity during the program.

22. Data on the numbers of high alcohol hour serious casualty crashes that occurred in country Victoria during the RBT program were collected. As a comparison, there was a need to estimate the expected number of accidents which would have occurred in the same week and area in the absence of RBT activity. Earlier studies have used accident experience in previous years (Cameron and Strang 1982). For this study, the crashes occurring from late November 1992 to December 1993 were used. However, since the period of late November 1993 to December 1993 was part of the RBT program, the crashes that occurred in the previous year's corresponding period were used as a proxy for these crashes, which meant that the crashes occurring from late November 1992 to December 1992 were used as a comparison twice in the analysis.

23. The percentage change in the number of HAH serious casualty crashes in the regions influenced by RBT activity, during the weeks of influence compared with the same weeks in the previous year, was calculated. This represented the first estimate of the effect of the particular style of RBT operation. The possible effects of other factors affecting crashes in the same regions during the weeks were taken into account by analysing the change in LAH serious casualty crashes in parallel. Other factors may have included changes in the speed camera program, which previous research has shown to reduce both HAH and LAH casualty crashes (Cameron et al 1992b, Newstead et al 1995b, 1996). However, LAH crashes were considered to be relatively unaffected by a drink-driving program such as the country RBT and publicity program. The percentage change in the LAH crashes was considered to measure the effects of the other factors, if any.

24. The difference between the percentage change in HAH crashes and the percentage change in LAH crashes was considered to provide a better estimate of the effect of the particular RBT style under consideration. The net percentage change was calculated to estimate this effect. However, it may be a conservative measure of the effect of this component of the country RBT and publicity program if, as is possible, the presence of enforcement had an effect on LAH crashes as well as HAH crashes.

25. Before analysing the crashes in this way, there was a need to define the regions influenced by the RBT activity in each week. Identification of RBT-influenced regions involved grouping the existing LGAs into amalgamated areas. Initial examination of the RBT data showed that some LGA boundaries occurred in areas of high RBT activity and high traffic flow which could lead to a "spillage" effect of RBT activity from one LGA to a neighbouring LGA. Such spillage effects would be overlooked in the analysis if the 153 LGAs were treated separately. It was therefore decided to group the LGAs into areas that ideally had boundaries of low RBT activity and low traffic flow to minimise the spillage effect. Within each Police District, each LGA was methodically examined to determine if it could stand alone or if it needed to be amalgamated with another LGA. Whether RBT activity in one LGA was able to influence crash frequencies in another LGA (either by word of mouth or by the testing of drivers) depended on various factors. These factors included the terrain and size of the LGA, whether the LGA contained or bordered a large rural city, whether a major highway passed through the LGA, and the amount of RBT activity that had occurred in the LGA during the program. When all these factors were considered, the 153 LGAs were amalgamated into 70 new regions.

26. The RBT program period was then divided into 58 weeks, and for each week and each region, the amount of activity (in hours and tests) was determined. Initially, an RBT-influenced region was considered to be one which had any activity in a given week. Other thresholds were also considered, ie. at least one hour's testing or at least 150 tested drivers. Similar processes were undertaken for each style of operation (TAS Bus, District Bus and Car) and combination of styles (TAS Bus/Car and District Bus/Car) to determine whether RBT activity was to be considered present or absent for each style or combination. A conditional selection process was then used to identify the crashes which occurred when an RBT influence was considered to be present, for each style of operation considered.

27. Consideration was given to using HAH crashes in the uninfluenced areas and weeks as an alternative basis for measuring the effects on crashes in country Victoria by factors other than the country RBT program. It transpired that a large proportion of country Victoria, and a large proportion of the 58 weeks during the program, were covered by the influence of RBT activity. The geographical composition and population sparseness of the uninfluenced areas made them unrepresentative of country areas of Victoria, so they were likely to be dissimilar from the areas where RBT influence reached. For these reasons the uninfluenced areas and weeks were rejected as an appropriate comparison for the influenced regions and weeks.

28. The interaction of the effect of the RBT operations with levels of awareness of drink-driving advertising was examined by calculating the net percentage change in

HAH serious casualty crashes during each of three periods when different levels of awareness were suggested by the "Adstock" function. Adstock (as defined) had previously been shown to represent awareness for each week during the period. The levels of publicity awareness considered were 'low' (under 200 Adstock units), 'medium' (200-800 Adstock units) and 'high' (above 800 Adstock units). It should be noted that the changes in HAH crashes calculated in this way do not represent an estimate of the effects of drink-driving advertising *per se*. They represent an estimate of the effect of each RBT style, conditional on the level of publicity awareness at the time the RBT activity was operating.

29. The standard two-way chi-square test of the crash frequencies, categorised by alcohol time of week (HAH v. LAH) and period (pre-RBT program v. during RBT program), was used to test the statistical significance of the net percentage change for each style of operation (Ryan et al 1985). The statistical significance of differences in the net percentage change for each style was tested by analysing the three-way interaction in contingency tables of crash frequencies categorised by time of week, period and style (Bishop et al 1975, Christensen 1990). Log-linear categorical models were fitted to the crashes in each contingency table to assess the statistical significance of this interaction, using the statistical package GLIM (Aitkin et al 1990). A similar method of analysis was used to test the statistical significance of the interaction of the effects of the RBT operations with the levels of publicity awareness.

30. Consideration was given to the increased chances of Type 1 statistical errors when a number of hypothesis tests were carried out with each judging statistical significance at conventional levels of statistical significance (say, $p < 0.05$ or lower). There were at least eight independent hypothesis tests considered, testing the effects of five styles of operation and testing the effects of the RBT activity during three periods of publicity awareness. This suggests that a maximum significance level of approximately $p = 0.006$ should be used before any one test is judged to be statistically significant. Against this is the increased risk of a Type 2 statistical error, ie. that evidence of an effective form of country RBT activity will be judged to be due to chance and hence ignored. Given the importance of finding effective countermeasures to drink-driving in country areas, this suggests that higher significance levels than $p = 0.006$ should be considered. The reader should bear these considerations in mind while examining the statistical significance of the results.

Results

Crashes in the regions and weeks of enforcement

31. Table 2 shows the comparison of serious casualty crashes in regions and weeks during which cars *or* District buses *or* TAS buses (all styles) conducted at least one random breath test in the week.

Table 2: Serious Casualty Crashes during the Weeks of Enforcement in Regions and Weeks Influenced by ALL STYLES of Operation, by Alcohol Time and Program Period.

ALCOHOL TIME	PERIOD		% Change	Net % Change
	Pre-RBT program	During RBT program		
High Alcohol Hour	578	546	-5.5	-6.8
Low Alcohol Hour	846	857	1.3	

32. In areas and weeks where there was RBT activity, there was a 5.5% decrease in HAH serious casualty crashes during the RBT program relative to the pre-RBT period, when any style of operation was present. The corresponding change for LAH serious casualty crashes was a 1.3% increase. To estimate the difference between the HAH and LAH percentage changes, the net percentage change was calculated, producing a net percentage decrease of 6.8%. Thus an apparent 6.8% decrease in HAH serious casualty crashes (relative to LAH crashes) occurred during the program (relative to before the program) in RBT influenced areas during the weeks of RBT activity. A chi-square test found that this net percentage decrease was not statistically significant ($p=0.3634$).

33. A log-linear model fitted to the crashes to test for a three-way interaction between alcohol time, period and style found a statistically significant interaction ($p=0.0166$). Hence there appeared to be differences between the effects of each style during the weeks of enforcement. Table 3 presents the serious casualty crashes when each individual style was operating in the RBT-influenced areas.

Table 3: Serious Casualty Crashes during the Weeks of Enforcement in Regions and Weeks Influenced by RBT Operations, by Style of Operation, Alcohol Time and Program Period.

STYLE OF OPERATION	ALCOHOL TIME	PERIOD		Net % Change
		Pre-RBT program	During RBT program	
Cars alone	High Alcohol Hour	371	300	-22.3%*
	Low Alcohol Hour	494	514	
District buses alone	High Alcohol Hour	26	24	-19.6%
	Low Alcohol Hour	27	31	
TAS buses alone	High Alcohol Hour	16	7	-42.4%
	Low Alcohol Hour	25	19	
Cars & District buses together	High Alcohol Hour	50	65	12.0%
	Low Alcohol Hour	81	94	
Cars & TAS buses together	High Alcohol Hour	96	138	46.9%**
	Low Alcohol Hour	184	180	

* Statistically significant decrease ($p=0.0116$)

** Statistically significant increase ($p=0.0227$)

34. All three individual styles produced net percentage reductions in HAH serious casualty crashes during the RBT program (as compared to the pre-RBT program). TAS bus operations produced the largest reduction of 42.4%, however this reduction was not statistically significant ($p=0.3093$), possibly due to the low number of crashes. Similarly, a non-significant 19.6% net reduction ($p=0.5722$) in crashes resulted when District buses were solely conducting random breath testing in country areas. However, when cars were operating alone, a statistically significant 22.3% net reduction in HAH serious casualty crashes occurred in regions influenced by these operations ($p=0.0116$).

35. In contrast there was evidence of net *increases* in high alcohol hour (relative to low alcohol hour) serious casualty crashes during the RBT period (relative to the pre-period) in areas where combinations of cars and District or TAS buses were operating together.

36. The 12% net increase for car/District bus operations was not statistically significant ($p=0.6384$), so it could have been due to chance. However, the 46.9% increase occurring for the car/TAS bus combination was significant ($p=0.0227$). This was largely due to the 44% increase in HAH serious casualty crashes that occurred during these operations, with an accompanying 2% decrease in LAH crashes.

Interactions with publicity awareness

37. The effects of the RBT operations on HAH serious casualty crashes were investigated for each level of drink-driving publicity awareness present at the time. The crashes occurring during the week of enforcement (Table 2) were analysed separately for weeks when low, medium or high levels of publicity awareness, respectively, were present in country Victoria (Table 4).

Table 4: Serious Casualty Crashes during the Weeks of Enforcement in Regions and Weeks Influenced by ALL STYLES of Operation, by Level of Publicity Awareness, Alcohol Time and Program Period.

PUBLICITY AWARENESS	ALCOHOL TIME	PERIOD		Net % Change
		Pre-RBT program	During RBT program	
Low	High Alcohol Hour	117	122	-12.7
	Low Alcohol Hour	165	197	
Medium	High Alcohol Hour	350	305	-14.3*
	Low Alcohol Hour	485	493	
High	High Alcohol Hour	111	119	25.8
	Low Alcohol Hour	196	167	

*Statistically significant decrease at 15% level ($p=0.1277$)

38. A log-linear model was fitted to the crashes to test for the presence of a three-way interaction between publicity awareness, program period and alcohol time during the week of enforcement. There was some evidence of a three-way interaction ($p=0.1354$),

suggesting that the effects of the RBT program were related to the level of awareness of the drink-driving publicity.

39. When low levels (<200 Adstock) of publicity awareness were operating, a 12.7% net reduction ($p=0.4173$) occurred for HAH serious casualty crashes in regions and weeks where any style of RBT was present. A slightly larger net reduction of 14.3% occurred during the weeks when the publicity awareness was at a medium level (200-800 Adstock). This reduction was statistically significant at the 15% level ($p=0.1277$).

40. Conversely, there was no evidence of a reduction in HAH serious casualty crashes during weeks when the RBT activity was accompanied by high levels of publicity awareness. A net increase in crashes of 25.8% resulted, but this was not statistically significant ($p=0.1734$).

41. The crashes that occurred in the regions and weeks of "car only" enforcement at each level of publicity awareness were further analysed. A statistically significant net crash reduction of 33% ($p=0.0024$) occurred during weeks of medium publicity awareness. There was also evidence of a 22% reduction during weeks of low publicity awareness ($p=0.2873$), but no indication of a reduction when high publicity awareness supported the RBT activity.

Minimum threshold for RBT activity

42. The above analyses were based on regions and weeks in which any RBT activity (for a specific style of operation, in some instances) was considered possible to have influenced the HAH crashes. In order to discount the situations where little enforcement activity occurred in a particular week, the crashes were also analysed for regions and weeks in which at least one hour of testing, or at least 150 tests, were achieved by each style of RBT operation (ie. by both forms of operation, if a combined style of operation).

43. Only the analyses based on a minimum threshold of 150 tests produced substantially different results to those found when any level of RBT activity was considered. There was evidence of crash reductions occurring when car/District bus and car/TAS bus combinations were operating together. It should be noted that the crash frequencies in the RBT-influenced areas and weeks are relatively small in these circumstances, because combinations of cars and buses where both forms of operation achieved 150 tests in a week appeared to be relatively infrequent.

44. For this reason it was decided to pool the crashes influenced by each car/bus combination to provide a more reliable estimate of the crash reductions during the weeks of enforcement when cars and buses operated together (Table 5). A highly significant 49.2% net reduction in HAH serious casualty crashes ($p=0.0157$) was observed in regions and weeks during which RBT was conducted by a combination of a car and a booze bus.

Table 5: Serious Casualty Crashes during the Weeks of Enforcement in Regions and Weeks Influenced by CARS AND BUSES OPERATING TOGETHER and EACH ACHIEVED 150 TESTS, by Alcohol Time and Program Period.

ALCOHOL TIME	PERIOD		Net % Change
	Pre-RBT program	During RBT program	
High Alcohol Hour	46	38	-49.2*
Low Alcohol Hour	51	83	

* Statistically significant decrease ($p=0.0157$)

DISCUSSION

45. Introduction of the country RBT program in late November 1993 saw 790,445 tests conducted in country Victoria to the end of 1994, with the relatively high level of RBT activity achieved in Melbourne in 1993 being maintained during 1994. The TAS booze buses appeared to display considerable deterrent value for drink-driving by random breath testing a greater number of drivers per hour than either District buses or cars across country Victoria. However, there was substantial variation in RBT activity patterns between the country Police Districts.

46. A significant reduction in high alcohol hour serious casualty crashes was found in country Victoria during the program, after taking into account other factors influencing trends in these crashes. This was greater than the non-significant reduction in crashes of the same type in Melbourne during the period, and can be contrasted with the (non-significant) increase in such crashes in NSW when analysed in the same way. The reduction in HAH crashes in country Victoria may have been due to the country RBT program, but detailed analysis of the localised effects of the enforcement operations was considered necessary to establish cause-and-effect.

47. Localised effects were measured by analysing the changes in HAH crashes, in the regions and weeks influenced by the enforcement operations, compared with the year before. Changes in LAH crashes, in the same areas and weeks, were used as the comparison for the changes in HAH crashes in the regions and weeks influenced by the program. This method could be considered a conservative way of estimating the program effects, if the program is considered likely to have affected LAH crashes as well as HAH crashes. It also relies on the assumption that the effects of other factors, separate from the RBT program, would have been the same on both HAH and LAH crashes.

48. The analysis of localised effects found evidence of reductions in HAH serious casualty crashes in regions influenced by certain styles of RBT operations. These styles include car only testing, and combinations of cars and buses, where both the cars and the buses conducted at least 150 tests per week in a region. There were too few crashes covered by enforcement operations involving buses operating alone, or in combination with cars achieving less than 150 tests per week, to reach reliable conclusions about the

effects of these styles. There was little evidence of crash reductions during weeks when the RBT activity was accompanied by high levels of awareness of drink-driving publicity, however there may also have been too few crashes covered during these weeks to reach reliable conclusions.

49. Notwithstanding the above qualifications, some of these findings were surprising given previous findings relating crash reductions in Melbourne to the visibility and intensity of RBT operations and to levels of supporting publicity (Cameron, Newstead and Gantzer 1995). An explanation may lie in the possible reactions of country drink-drivers when faced with intense enforcement, heightened by intense publicity.

50. A possible reaction to a perception of intense drink-driving enforcement in a country region may have been for some drink-drivers to change their driving behaviour after drinking, rather than to change their drinking behaviour. This could have taken the form of travelling on minor roads, unfamiliar to them, rather than using the major roads and highways they normally use. Since minor roads in country areas are likely to be of lower standard than major roads, due to poorer maintenance, lower delineation and additional roadside hazards, it is likely that drink-drivers would have experienced increased risks of crashing and resulting injury on such roads. Such a change in travel behaviour could be expected to reflect in a greater proportion of alcohol-related crashes having occurred on minor roads, rather than major roads, during the country RBT program (at least in those regions and weeks influenced by intense enforcement and publicity). Further research to explore this possible mechanism of the country RBT program has been recommended.

CONCLUSIONS

51. There was evidence indicating reductions in high alcohol hour serious casualty crashes due to the following styles of RBT activity in the regions of country Victoria influenced by the enforcement:

- RBT conducted by cars operating alone, during the weeks when enforcement was present (estimated 22% reduction)
- Combinations of cars and buses, when each component conducted at least 150 tests per week, during the enforcement weeks (estimated 49% reduction).

52. However, there were too few crashes covered by other styles of RBT operations, such as buses operating alone, to reach reliable conclusions about their effects.

53. There was some evidence of an interaction between the effects of the enforcement operations and the levels of awareness of drink-driving television advertising in country Victoria. Medium levels of awareness appear to increase the effects of the "car only" enforcement operations (estimated 33% reduction). However, there were apparently too few crashes covered by high levels of awareness to reach reliable conclusions.

54. There was a 9% reduction in high alcohol hour serious casualty crashes below expected levels in country Victoria during the operation of the country RBT and publicity program to December 1994. This reduction in crashes was probably at least partly due to the increased level of RBT activity due to the program.

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Paper No. 19

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Rating the Aggressivity of Australian Passenger Vehicles Towards Other Vehicle Occupants and Unprotected Road Users

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Two measures of aggressivity of Australian passenger vehicles have been developed. The first measures the aggressivity to occupants of other cars. This type of aggressivity rating is based on two-car crashes between passenger vehicles and measures the injury risk each make/model in the collisions poses to the drivers of the other vehicles. The second measures aggressivity to unprotected road users. These aggressivity ratings reflect the threat of severe injury to pedestrians, bicyclists and motorcyclists by the make/model of vehicle colliding with them. This analysis was based on nearly 102,000 drivers involved in tow-away crashes with the makes/models which were the focus of the study and on nearly 22,000 injured pedestrians, bicyclists, and motorcyclists. The results suggest that crashworthiness and aggressivity are two different aspects of a vehicle's safety performance, with good performance on one dimension not necessarily being associated with good performance on the other.

Keywords: Injury, aggressivity, compatibility, passive safety, statistics, vehicle occupant, pedestrian, bicyclist, motorcyclist

INTRODUCTION

Since 1990, Monash University Accident Research Centre (MUARC) has conducted a series of studies to provide consumer advice on the crashworthiness of individual makes and models of Australian passenger cars. Crashworthiness was defined as the relative safety of vehicles in preventing severe injury to their occupants in crashes. Ratings of

crashworthiness, measured by the rate of serious driver injury in tow-away crashes, were produced for individual models (Cameron *et al.*, 1992; 1995; 1996).

This paper extends MUARC's previous work in this area by adding measures of the "aggressivity" of individual car models. Aggressivity ratings measure the risk of injury which a vehicle poses to occupants of other vehicles which it impacts and

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to other, unprotected road users such as pedestrians, bicyclists, and motorcyclists. The availability of aggressivity ratings represents further consumer advice which purchasers of cars could take into account when choosing a car in addition to its crashworthiness rating. The distinction between crashworthiness and aggressivity in the area of crash compatibility of vehicles was noted by Gabler and Hollowell (1998).

DEVELOPMENTS IN EUROPE

Broughton (1996) has defined an aggressivity index which is calculated for each car model (M) from mass data on two-car crashes in which at least one driver is injured. The index was based on the same type of data used by Folksam Insurance, Sweden (Gustafsson *et al.* 1989) and the UK Department of Transport (DoT) (1995) to calculate their respective crashworthiness indices (Table I). In both Sweden and the UK, non-injury crashes are not reported, so the number of two-car crashes in which neither driver is injured (n_4) is not known and, hence, cannot be used in either a crashworthiness or aggressivity index.

Broughton's aggressivity index is:

$$A = (n_2 + n_3) / (n_1 + n_2 + n_3),$$

where n_1 , n_2 , and n_3 are the frequencies of two-car crashes with different injury outcomes, as defined in Table I. Broughton has also pointed out that the DoT crashworthiness index,

$$D = (n_1 + n_2) / (n_1 + n_2 + n_3),$$

TABLE I Number of two-car crashes between specific make/model (M) and other makes/models (O)

Drivers of other makes/models O	Drivers of make/model M	
	Injured	Not injured
Injured	n_2	n_3
Not injured	n_1	n_4

and the Folksam crashworthiness index,

$$R = (n_1 + n_2) / (n_2 + n_3),$$

are related through the relationship $D/A = R$.

Broughton shows that both D and R are influenced by the aggressivity of the specific model (i.e., independent of their occupant protection capabilities, the models which inflict more injuries on "other" drivers will have lower crashworthiness indices), but also that D comes closer than R to the ideal of being independent of the casualties in the "other" car. Thus it appears that the Folksam index, R , is particularly likely to be a measure of not only the crashworthiness of a car model, but also its aggressiveness (the above relationship suggests that a more aggressive model will appear to be more crashworthy, if R is used as the measure of crashworthiness).

Turning to Broughton's aggressivity index, A , and the DoT crashworthiness index, D , both Broughton and DoT have reported a strong inverse relationship between them when each is calculated for a range of car models included in the UK ratings. Relatively few models departed substantially from the curvilinear relationship fitted to the data by Broughton. This suggests that either UK cars are truly characterized by an inverse relationship between aggressivity and crashworthiness, or that the apparent relationship is, at least in part, an artefact of the constraints on the data (i.e., crashes with at least one injured driver) used to produce the indices for each model. It is hoped to avoid these constraints by the use of Australian data on crashes based on the tow-away criterion for collection.

DEVELOPMENTS IN THE UNITED STATES

Hollowell and Gabler (1996) describe a NHTSA research program to measure vehicle aggressivity and compatibility and then to link these to vehicle design characteristics. They calculated an

"aggressivity metric", defined as

$$\text{Aggressivity metric} = \frac{\text{Deaths in other vehicle}}{(\text{Total registrations in subject vehicle}) / 1,000,000}$$

For each make and model of car, light truck, and van under 10,000 pounds, the metric was calculated from data on two-car collisions involving a fatality, and recorded in the Fatal Accident Reporting System (FARS) database for 1991-93. This metric showed that the most aggressive vehicles were light trucks (pickups and sports utility vehicles) and vans and that, among cars, the heavier models were the most aggressive. However, the detailed results also showed that weight is not always the over-riding contributor to aggressivity, as some heavy cars had relatively low scores on the metric.

Hollowell and Gabler recognized that a poor aggressivity rating may reflect characteristics of the driver and how the vehicle is driven (e.g., speed), as well as any structural or weight factor. At the very least, their metric is influenced by the crash rate per million registrations of each model, and variations in this rate should not be included in an aggressivity index used to measure the threat to other road users involved in a crash with the subject model. They also calculated two other metrics to partially overcome these problems:

- the other vehicle fatalities divided by the subject vehicle fatalities, and
- the other vehicle fatalities divided by the subject vehicle fatal accidents.

These two metrics reduced the initial aggressivity ranking of a model which presumably had a relatively high crash rate. The first of these alternative metrics is similar to Broughton's aggressivity index, but "injury" is redefined as death.

In a subsequent paper, Gabler and Hollowell (1998) gave preference to a metric defined as:

$$\text{Aggressivity metric} = \frac{\text{Driver deaths in other vehicle}}{(\text{No. of two-light-vehicle-collisions involving the subject vehicle})}$$

They suggested that normalizing by the number of (police reported) crashes rather than vehicle registrations focuses the metric more on vehicle performance and less on driver behavior. Nevertheless, this metric produced results by vehicle category that were similar to the principal metric used in their 1996 paper.

AGGRESSIVITY RATINGS FOR AUSTRALIAN PASSENGER VEHICLES

The investigation of the feasibility and methods of providing aggressivity ratings for Australian passenger vehicles was studied in terms of the threat which each subject model represents to:

- (1) Occupants of other cars colliding with the subject model cars, and
- (2) Pedestrians, bicyclists and motorcyclists impacted by the subject model cars.

The aggressivity ratings were based on one of the data sets used to produce crashworthiness ratings (Newstead *et al.*, 1997), namely police reports of crashes in New South Wales (NSW) resulting in death or injury or a vehicle being towed away.

Crashes involving pedestrians, bicyclists, and motorcyclists are seldom reported to the police in NSW unless someone is killed or injured (usually the unprotected road user). This means that an estimate of the risk of injury was not calculable for the unprotected road users for inclusion in the second type of aggressivity rating (a measure of injury severity was). This problem did not occur for drivers of other cars, for whom the available data allowed estimates of both the risk of injury and of their injury severity.

Aggressivity Towards Occupants of Other Cars

As in Europe and the United States, this type of aggressivity rating has been based on two-car

crashes between light vehicles (i.e., heavy vehicle collisions were excluded). The subject vehicles were the passenger cars, station wagons, "four-wheel drive" vehicles, passenger vans, and light commercial vehicles manufactured during 1982-95, whose makes and models have been identified in the NSW crash data.

In Australia, "four-wheel drive" designates a category of passenger vehicles known as sports utility vehicles in other countries, which are characterized by relatively high mass, frontal stiffness, and wheel ride height compared with passenger cars, and featuring four-wheel drive transmissions. These vehicles are not based on passenger car or station wagon designs.

The NSW data on two-car crashes involving each model of the subject vehicle has been extracted in the same form as Table I. In this case, the number of crashes in which neither driver was injured (i.e., n_4) was available, at least so far as tow-away crashes are concerned. The measure of the risk of injury of the other drivers colliding with the subject model, unadjusted for any other factors, was:

$$\text{Injury risk of other drivers} = RO \\ = (n_2 + n_3) / (n_1 + n_2 + n_3 + n_4).$$

The injury severity of other drivers could be measured in a number of ways from the information on injury recorded on NSW police reports (namely, killed; admitted to hospital; or injury requiring medical treatment). The measure of injury severity, similar to that used in the crashworthiness ratings project, was:

$$\text{Injury severity of other drivers} = SO \\ = \text{proportion of injured drivers who were} \\ \text{killed or admitted to hospital.}$$

The aggressivity measure for each subject car model, assuming RO and SO are independent, was then calculated as:

$$\text{Aggressivity to other car drivers} = AO \\ = RO \times SO.$$

This measured the risk of a driver of other cars being killed or admitted to hospital when involved in collisions with the subject model cars.

Before this aggressivity measure was calculated, consideration was given to taking into account the differences between the crash circumstances of the subject car models which may result in a distorted view of its aggressivity only partly related to the characteristics of the subject cars. Factors available in the data to consider such differences included:

- speed limit at the crash location
- subject driver age (younger drivers may be driving at relatively fast speeds not fully represented by the speed limit)
- subject driver sex (male drivers may be driving at relatively fast speeds)
- other car driver age (older drivers are more susceptible to injury)
- other car driver sex (female drivers are more susceptible to injury, but males appear to be associated with relatively high injury severities).

Aggressivity Towards Pedestrians, Bicyclists, and Motorcyclists

The aggressivity ratings reflecting the threat to pedestrians, bicyclists and motorcyclists have been based on NSW data on collisions between these road user types and the subject model vehicles with identified makes and models. The aggressivity measure was based on injured pedestrians, bicyclists and motorcyclists and reflects the injury severity of their outcome. The measure of the aggressivity of the subject models towards unprotected road users was:

$$\text{Aggressivity to unprotected road users} = AU \\ = \text{proportion of unprotected road users} \\ \text{injured in collisions with subject car} \\ \text{models who were killed or admitted} \\ \text{to hospital.}$$

As with the measure of aggressivity to drivers of other cars, consideration was given to taking into account any major differences in the crash

circumstances related to the following factors which may distort the results:

- speed limit at the crash location
- subject driver age
- subject driver sex
- unprotected road user age
- unprotected road user sex
- type of unprotected road user.

DATA

The NSW crash data available for estimation of vehicle aggressivity ratings was the same as that used by Newstead *et al.* (1997) to produce crashworthiness ratings, namely police reports of crashes during 1987-95 resulting in death or injury or a vehicle being towed away. They have described the method of assembly of these data, including the means by which vehicle models were identified. Subsets of these data were taken in order to estimate the two aggressivity measures. The methods of selecting appropriate cases for each purpose are described below.

Data from NSW used to estimate the crashworthiness ratings covered 350,740 drivers of 1982-95 model vehicles involved in crashes resulting in at least one of the vehicles being towed, over the period 1987-95. Of these 350,740 vehicles, 250,762 were coded as being involved in crashes with one other vehicle (i.e., the crash involved a total of two vehicles). In order to compare occupant injury levels between the two vehicles involved in the crash, it was necessary to match the crash and occupant injury information for the each of the two vehicles.

The data used for calculation of the crashworthiness ratings covered only vehicles manufactured from 1982 to 1995. Matching of the data of crashes in which both vehicles were manufactured from 1982 to 1995 identified 101,916 vehicles. Of the drivers of these vehicles, 12,311 were injured. These records were used for calculation of vehicle aggressivity ratings toward drivers of other vehicles.

For calculation of vehicle aggressivity ratings towards unprotected road users, the data from NSW was interrogated to identify single vehicles crashing with one pedestrian, bicyclist, or motorcyclist. 21,899 crashes of this type were identified involving vehicles manufactured over the years 1982-95. All the unprotected road users involved in these crashes were injured to some degree.

METHODS

Aggressivity Towards Occupants of Other Cars

As described above, the measure of aggressivity to drivers of other cars was:

$$AO = RO \times SO.$$

Each of the two components of the aggressivity rating, RO and SO, were estimated by logistic regression modelling techniques (Hosmer and Lemeshow, 1989). Such techniques are able to adjust simultaneously for the effect of a number of factors, discussed below, on the injury risk and injury severity probabilities.

A peripheral investigation included an adjustment of the measure of aggressivity towards drivers of other cars which took into account the injury outcome of the drivers of the subject model cars, hence, providing an indication of the crash severity. The logistic regression techniques employed allowed adjustment for the injury risk or injury severity of the driver of the subject vehicle by including this as a covariate in each of the logistic models for RO and SO, respectively. The subject driver injury outcome was a statistically significant predictor of the injury outcome of the driver of the other car, for each component RO and SO. However the inclusion of the subject driver injury outcome as a covariate made little difference to the overall aggressivity rating, AO, compared with that obtained when it was omitted. The results also suggest that the other factors included as covariates (see below) provide an adequate substitute for a direct measure of crash severity.

Aggressivity Towards Unprotected Road Users

As described above, all unprotected road users were injured in the reported crashes, hence the concept of injury risk was redundant when considering aggressivity towards these road user types. The aggressivity measure for unprotected road users was equivalent to the injury severity component of the aggressivity measure for other drivers described above. That is:

Aggressivity to unprotected road users = AU.

Logistic regression modelling techniques were used to obtain estimates of AU adjusting for the effect of other factors, discussed below, on the aggressivity rating.

Logistic Regression Analysis

Logistic regression was applied to each of RO, SO, and AU in turn to estimate the contribution of the subject model vehicle to variations in these probabilities. The methods of analysis and the calculation of the confidence limits for the estimates were identical to those used for the crashworthiness ratings and described by Newstead *et al.* (1997). Besides the subject vehicle model, other influential factors were included in the analysis to take their effects into account.

Before the final analysis could be undertaken, it was necessary to develop logistic models of each component (RO, SO and AU) to identify possible factors, other than vehicle design, that might have influenced the injury outcome of the other driver or unprotected road user. This was initially done without considering the type of car (make/model or market group) in the logistic regression model, as the aim was to determine which other factors were most likely to have an influence across a broad spectrum of crashes.

Aggressivity Towards Occupants of Other Cars

Logistic models were obtained separately for RO and SO because it was likely that the various factors

would have different levels and directions of influence on these two component probabilities of the aggressivity measure. The factors considered in the models for both injury risk and injury severity were:

- speed limit at the crash location (< 80 km/h, ≥ 80 km/h)
- age of driver of subject car (≤ 25 years, 26–59 years, ≥ 60 years)
- sex of driver of subject car
- other car driver age (≤ 25 years, 26–59 years, ≥ 60 years)
- other car driver sex.

Aggressivity Towards Unprotected Road Users

The influential factors considered in the logistic regression model for AU were:

- speed limit at the crash location
- age of driver of subject car (≤ 25 years, 26–59 years, ≥ 60 years)
- sex of driver of subject car
- unprotected road user age (≤ 25 years, 26–59 years, ≥ 60 years)
- unprotected road user sex
- type of unprotected road user (bicyclist, motorcyclist, pedestrian).

For all analyses, a stepwise procedure was used to identify which factors and their interactions made a significant contribution to the probabilities. All possible first and higher order interactions were considered. A hierarchical structure was imposed so that if an interaction between two variables was included in the model then the corresponding main effects would also be included. The resultant logistic regression models were referred to as the "covariate" models or equations.

Assessing Differences in Aggressivity between Specific Car Models

Aggressivity injury risk, where relevant, and injury severity for individual vehicle models were estimated after adding a variable representing the subject car model to the respective logistic "covariate"

models. The car model variable was added to the logistic equation and individual car model coefficients were computed to represent deviations of that car from the average.

It was important to ensure that the logistic model adequately described the data and did not yield individual car model coefficients that were imprecise or unstable. In a similar manner to the exclusion of models for calculation of crashworthiness ratings (Newstead *et al.* 1997), car models were excluded for the calculation of aggressivity towards other vehicle drivers if:

- (i) there were less than 100 vehicles with which they had crashed, or
- (ii) there were less than 40 injured drivers in the other vehicles;

or models were excluded for calculation of aggressivity towards unprotected road users if there were less than 30 injured persons with which they had crashed.

Assessing Differences in Aggressivity between Broad Market Groups

A similar approach to that for individual car models was used to assess car market group aggressivity averages. A variable representing the different market groups (large, medium, small, luxury, sports, four-wheel-drive, passenger vans, and commercial vehicles with GVM ≤ 3000 kg) was added to each of the "covariate" models. Deviations of each market group, from the average, were assessed.

Combining the Injury Risk and Injury Severity Components

For aggressivity towards drivers of other vehicles, the final ratings were given by multiplying the estimates, RO and SO, for each individual subject car model (or market group). It was assumed that the probabilities estimated by these two components are independent. The influence of modern trends in barrier crash testing and other aspects of vehicle design on this assumption are unknown.

Because each of the two estimated aggressivity components had been adjusted for the effect of other factors by logistic regression prior to their combination, the resultant aggressivity rating was also adjusted for the influence of these factors. Since the aggressivity measure for unprotected road users was estimated as a single component, no combination of components was necessary.

RESULTS

Aggressivity Towards Other Car Drivers

Using the methods described above, logistic regression models of the injury risk and injury severity of the focus driver (i.e., the driver of the "other" vehicle) were built separately as functions of, firstly, model and, secondly, the market group of the subject vehicle colliding with the vehicle of the focus driver.

The logistic regression models of the injury risk of focus drivers showed a number of factors to be statistically significant predictors. These were focus driver age and sex and the speed zone, along with the interaction between focus driver age and sex. In addition, the model of the colliding vehicle was also a statistically significant predictor of focus driver injury risk when added to the logistic regression model. This indicated that there is differential performance between vehicle models in terms of their aggressivity towards drivers of other vehicles so far as injury risk is concerned. In the same manner, when vehicle market group was substituted for vehicle model in the logistic regression equation, it was also a significant predictor of focus driver injury risk.

The logistic regression models of the injury severity of focus drivers showed the factors focus driver age and the speed zone to be statistically significant predictors. The model of the colliding vehicle was also a statistically significant predictor of injury severity as was the vehicle market group when substituted for vehicle model in the logistic regression equation.

Final estimates of vehicle aggressivity towards the drivers of other vehicles, formed by multiplying the estimated injury risk and injury severity components, were obtained for 56 different vehicle models.

Analysis by Market Groups

Table II summarizes the estimated injury risk, injury severity and aggressivity ratings by the 8 broad market groups, along with the estimated confidence limits on the aggressivity ratings. The estimated aggressivity rating is the expected number of vehicle drivers killed or seriously injured per 100 involved in two-car tow-away collisions where their vehicle impacts with one of the designated market group. Table II shows the four-wheel-drive vehicles to be the most aggressive towards drivers of other vehicles, with an average of 3.18 drivers being killed or seriously injured for every 100 tow-away crashes with a four-wheel-drive. Similarly, Table II shows sports cars to be the least aggressive towards drivers of other vehicles, with an aggressivity rating of 1.02.

Statistically Significant Makes and Models

The estimated aggressivity ratings towards drivers of other vehicles for the 56 individual vehicle models rated ranged from a minimum of 0.23 to a maximum of 5.25 serious injuries per 100 tow-away crashes.

Of the 56 individual vehicle models for which an aggressivity rating was calculated, seven models had an aggressivity rating which was significantly less (better) than the overall average of 1.53 serious driver injuries per 100 tow-away crashes. These seven vehicle models comprised three small car models, three medium car models and one sports car.

Four models had an aggressivity rating which was significantly greater (worse) than the overall average. These four models comprised one large car model, two four-wheel-drive models, and one make of passenger van.

Aggressivity Towards Unprotected Road Users

A logistic regression model of the injury severity of unprotected road users was built as a function of, firstly, the model, and, secondly, the broad market group of the vehicle colliding with the unprotected road user. Variations in the other influential factors listed above were adjusted in the model by including them as predictors of unprotected road user injury severity along with colliding vehicle model or market group.

The logistic regression model of the injury severity of unprotected road users showed a number of factors to be statistically significant predictors. These factors were the unprotected road user age and sex, the speed zone and the type of unprotected road user, along with interactions between speed

TABLE II Estimated vehicle aggressivity towards other drivers, by market group

Market group	No. of two-car crashes	Other driver injury risk (%)	Other driver injury severity (%)	Aggressivity rating*	Overall rank order	Lower 95% confidence limit	Upper 95% confidence limit	Width of confidence interval
Four-wheel-drive	3539	16.0	19.9	3.18	8	2.63	3.73	1.10
Commercial	4200	15.5	14.0	2.17	6	1.75	2.59	0.85
Large	29829	11.6	16.0	1.86	5	1.71	2.01	0.30
Luxury	2728	11.8	15.1	1.77	4	1.29	2.26	0.97
Medium	17652	10.4	14.3	1.49	3	1.31	1.66	0.35
Passenger vans	2177	14.7	18.3	2.69	7	2.03	3.35	1.32
Small	19698	8.7	14.8	1.29	2	1.14	1.44	0.31
Sports	751	11.3	9.0	1.02	1	0.31	1.74	1.43

*Serious injury rate per 100 drivers of other vehicles involved in collisions with vehicles from the given market group.

zone and type of unprotected road user, speed zone and age of unprotected road user, and unprotected road user age and sex. In addition, the model of the colliding vehicle was also a statistically significant predictor of unprotected road user injury severity. As with the aggressivity towards drivers of other vehicles, the results indicated that there is differential performance between vehicle models in terms of their aggressivity towards unprotected road users so far as injury severity is concerned. In the same manner, when vehicle market group was substituted for vehicle model in the logistic regression equation, it was also a statistically significant predictor of unprotected road user injury severity.

As mentioned previously, it was not possible to estimate injury risk for unprotected road users as all those involved in reported crashes in the data available were injured. Hence the aggressivity rating for the unprotected road users was limited to the injury severity estimate. On this basis, aggressivity ratings toward unprotected road users were obtained for 85 different vehicle models.

Analysis by Market Groups

Table III summarizes the aggressivity ratings by the eight broad market groups along with the estimated confidence limits on the aggressivity ratings. The aggressivity rating is the expected number of unprotected road users killed or seriously injured per 100 injured in impacts with one of the vehicles from

each designated market group. Table III shows the four-wheel-drive vehicles to be the most aggressive towards unprotected road users, with an average of 41.5 unprotected road users being killed or seriously injured for every 100 injured by these vehicles. Similarly, Table III shows medium cars to be the least aggressive towards unprotected road users, with an aggressivity rating of 31.2%.

In general, within each market group, the average injury severity for unprotected road users is more than double that of vehicle drivers injured in impacts with the subject vehicles, highlighting the vulnerability of this road user group when involved in a crash.

Statistically Significant Makes and Models

The estimated aggressivity ratings towards unprotected road users for the 85 individual vehicle models rated ranged from a minimum of 14.6 to a maximum of 46.6 serious injuries per 100 injured unprotected road users.

Three models had an aggressivity rating which was significantly less (better) than the overall average for the 85 models. These comprised two large car models and one medium car.

Eight models had an aggressivity rating which was significantly greater (worse) than the overall average. These comprised two small car models, four four-wheel-drive models and two makes of commercial vans.

TABLE III Estimated vehicle aggressivity towards unprotected road users, by market group

Market group	No. of injured unprotected road users	Aggressivity towards unprotected road users (%)*	Overall rank order	Lower 95% confidence limit	Upper 95% confidence limit	Width of confidence interval
Four-wheel-drive	886	41.5	8	38.3	44.6	6.3
Commercial	1190	36.8	7	34.1	39.5	5.4
Large cars	5989	31.3	2	29.8	32.8	3.0
Luxury	810	34.0	5	30.9	37.1	6.2
Medium cars	3532	31.2	1	29.4	32.9	3.5
Passenger vans	674	36.6	6	33.2	40.1	6.9
Small cars	3948	32.4	3	30.7	34.1	3.4
Sports	258	32.8	4	27.9	38.1	10.2

*Serious injury rate per 100 unprotected road users injured in collisions with vehicles from the given market group.

Relationships between Aggressivity, Crashworthiness and Vehicle Mass

Broughton (1996) and Cameron *et al.* (1995) have noted a strong relationship between vehicle crashworthiness and mass, with heavier vehicles tending to display better crashworthiness. A theoretical analysis by Wood (1997) suggests that a mass relationship might exist for aggressivity. Figure 1 plots the estimated aggressivity rating towards other drivers against vehicle mass for the 56 models rated and shows that vehicles with higher mass tend to be more aggressive towards drivers of other vehicles.

The dotted line in Figure 1 illustrates the relationship between aggressivity and vehicle mass. Some classes of vehicle tend to exhibit particularly high aggressivity even after accounting for mass effects (i.e., those vehicles with points lying above the dotted line in Figure 1). The four-wheel-drive,

passenger vans and commercial vehicles generally fall into this category.

The results also indicate that the subject vehicle mass (or the ratio of the mass of the subject vehicle to the mass of the other vehicle) could be included as a covariate in a logistic regression analysis similar to that described above. This analysis would allow the effects of vehicle design on aggressivity to be seen more clearly.

Aggressivity and Crashworthiness

In assessing the British vehicle safety indices, Broughton (1996) found a strong inverse relationship between the indices for crashworthiness and aggressivity. Figure 2 shows aggressivity towards other drivers plotted against crashworthiness for those vehicle models with both ratings (for both

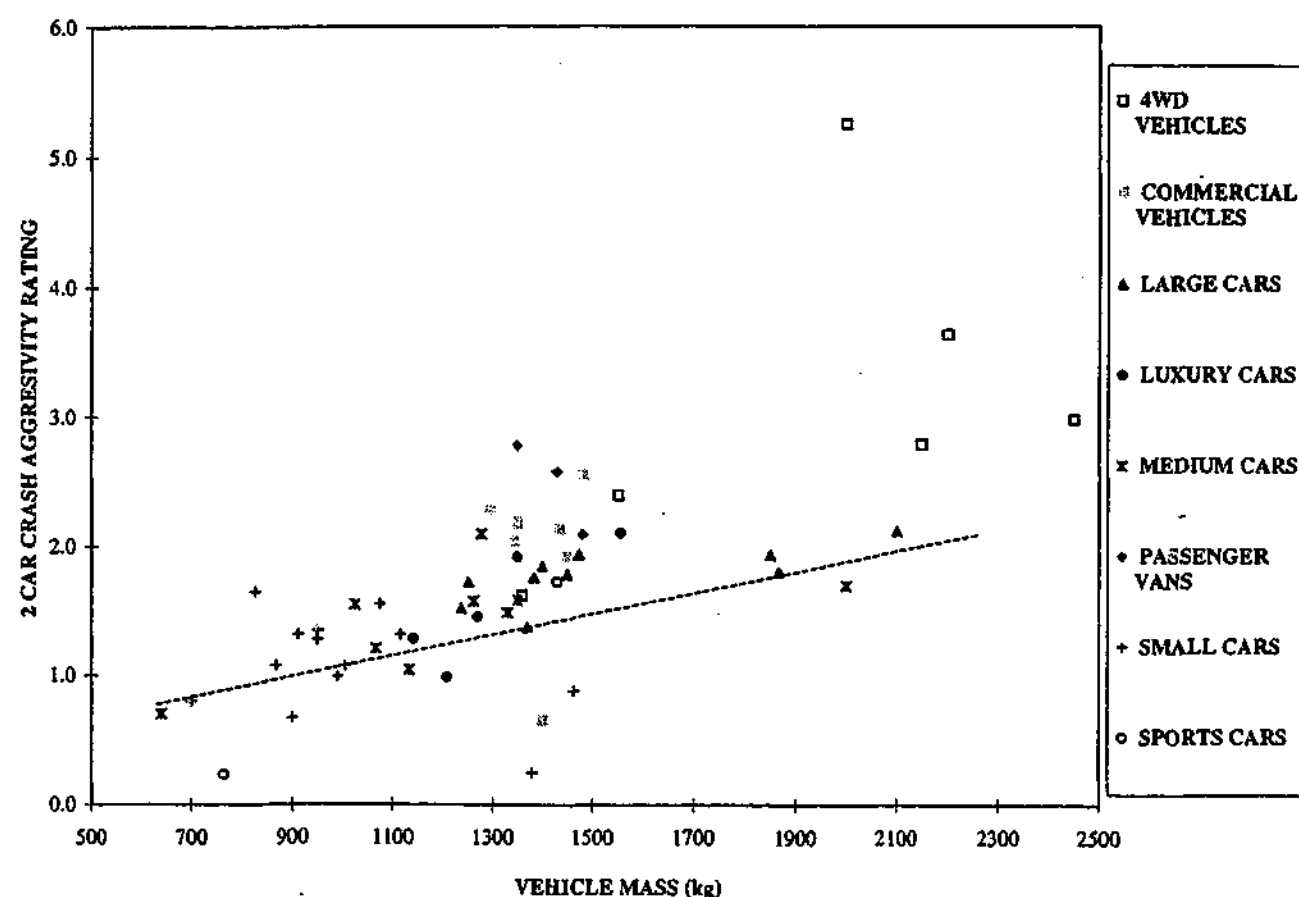


FIGURE 1 Estimated vehicle aggressivity towards other drivers vs. vehicle mass.

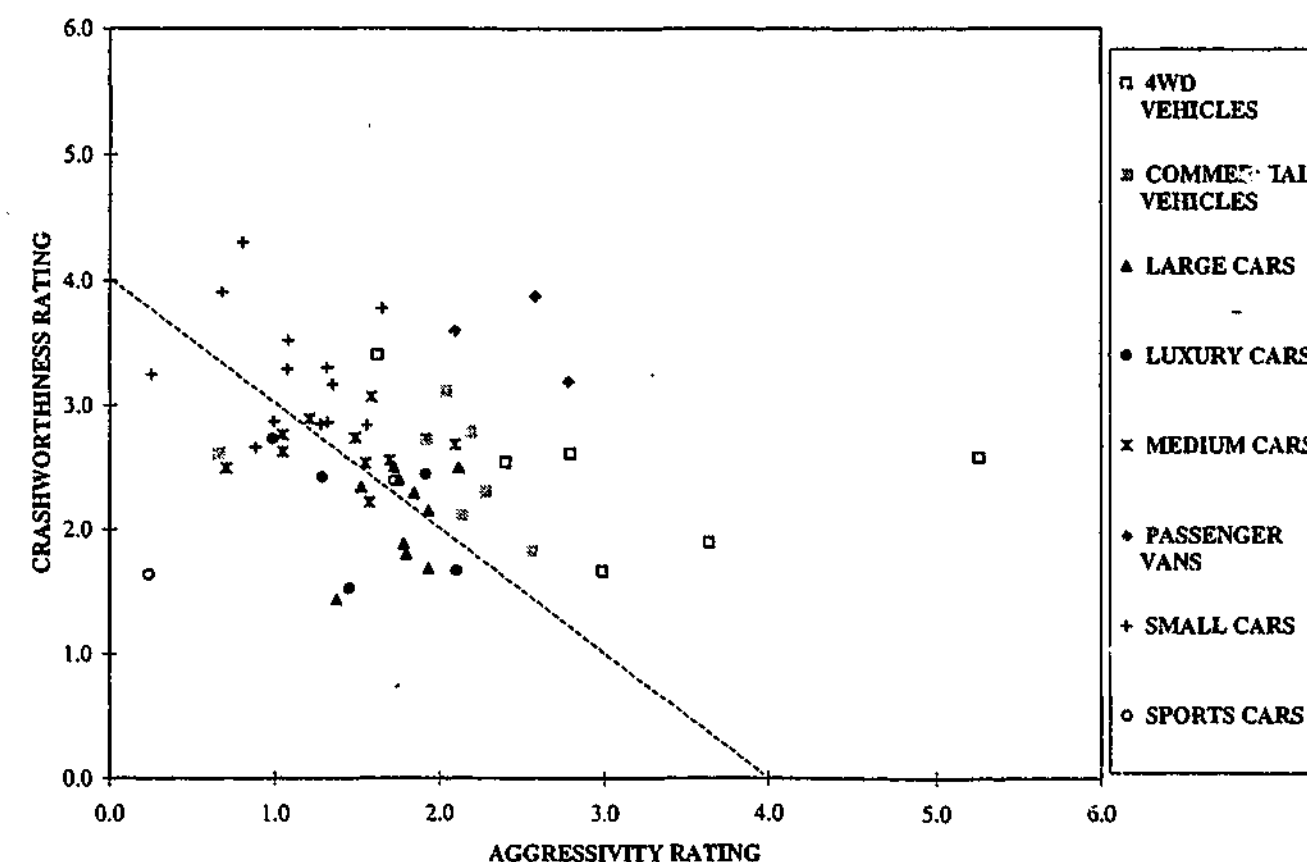


FIGURE 2 Estimated vehicle aggressivity towards other drivers vs. crashworthiness rating.

measures, high values are indicative of high risks of serious injury to a driver involved in a crash). As Figure 2 shows, the inverse relationship between the two measures is not particularly strong. The dotted line in Figure 2 represents the nominal inverse relationship between aggressivity and crashworthiness ratings with points above the line representing vehicles with relatively high aggressivity for their level of crashworthiness and points below the line representing vehicles with relatively low aggressivity for their crashworthiness performance. Four-wheel-drives, passenger vans and commercial vehicles are again the groups of vehicles which generally show relatively high levels of aggressivity for their level of crashworthiness.

Absence of a strong relationship between the measures of aggressivity and crashworthiness suggests that the two quantities considered here are measuring two different aspects of a vehicle's safety performance. Whilst one would expect some rela-

tionship between the two measures given their common but opposite relationships with mass, the lack of a strong relationship suggests vehicle mass is only playing a small part in the aggressivity dimension relative to total vehicle safety design. The independence of these two measures does not seem to have been achieved to the same degree under other systems (UK Department of Transport, 1995; Broughton, 1996).

DISCUSSION

The methods developed and applied in this paper have allowed estimation of vehicle aggressivity ratings for Australian passenger vehicles with respect to both drivers of other vehicles and unprotected road users. Aggressivity is an important measure as, in conjunction with crashworthiness ratings, it enables assessment of the total safety of the vehicle

fleet from the perspective of the protecting not only its own occupants in a crash but also road users external to that vehicle. Vehicle safety ratings in Australia to date have concentrated primarily on estimating the relative protection a vehicle provides to its own occupants. Consumer information has typically recommended that people purchase vehicles which offer maximum safety benefits to them as occupants without recourse to the risk the specific vehicles may pose to other road users. This study has demonstrated that this advice may not necessarily provide a net gain to society as a whole. One example is a recommendation for people to buy large four-wheel-drive vehicles based on their occupant protection performance without noting that these vehicles pose a high injury risk to other road users in a collision, a point particularly relevant if the vehicle is to be used in an urban environment where the likelihood of a collision with another road user is high. Whilst the issue of aggressivity may not be a high priority for vehicle consumers, the information should be valuable to both legislators and those promoting vehicle safety generally. The availability of vehicle aggressivity ratings in conjunction with crashworthiness ratings will allow these groups to focus legislation and consumer advice to achieve better total vehicle safety performance.

Whilst similar in concept to the aggressivity ratings developed overseas, the ratings developed here appear to be superior in a number of areas. One of the major advantages of the aggressivity ratings developed here, in comparison to particularly those described in Broughton (1996), is their apparent independence from the crashworthiness ratings. A high level of inverse correlation between crashworthiness and aggressivity ratings would diminish the additional information on safety provided by the aggressivity measure. The aggressivity ratings developed here, however, appear to provide largely independent information on vehicle safety. The reason for the independence of the two measures found in this study is possibly linked to the availability of non-injury crash data due to the tow-away crash reporting criteria in NSW. Non-injury crash data allows the estimation of the injury risk components of both crashworthiness and aggressivity, a mea-

sure not directly available from the data on injury crashes only described by Broughton (1996).

One drawback aggressivity ratings have in comparison to the crashworthiness ratings of Newstead *et al.* (1997) is that they cover far fewer individual vehicle models. Aggressivity ratings for other drivers and unprotected road users estimated here cover only 56 and 85 vehicle models, respectively, whilst crashworthiness ratings based on almost the same data cover 120 individual models. The reason for the reduced model coverage in comparison to crashworthiness ratings stems from the fact that the aggressivity ratings, for reasons described above, are calculated from subsets of the total data used for crashworthiness calculation, namely crashes between two passenger vehicles manufactured later than 1982 and crashes with unprotected road users. Smaller quantities of data also compromise the precision of the aggressivity measures resulting in far fewer vehicles which can be differentiated as better or worse than the overall average in comparison to the differentiation obtained from crashworthiness ratings. These deficiencies highlight the need for further updates of the aggressivity ratings when additional data becomes available.

With the addition of the two measures of vehicle aggressivity developed in this paper, there are now three independent measures of the safety performance of Australian passenger vehicles derived from analysis of mass crash data. For simplicity of presentation and interpretation, particularly in the area of consumer safety advice, effort needs to be made to find a method of consolidating this information into a single measure of total vehicle safety, or some other cohesive method of summary presentation, which reflects overall vehicle safety. Combination measures could include an average of the three measures weighted by the relative involvement of the road user type to which each relates in past crashes involving the vehicle model concerned, treating crashworthiness as a safety benefit and aggressivity as a safety disbenefit. There are other approaches which could also be feasible. Further research needs to be undertaken to establish the most appropriate method of consolidation.

CONCLUSIONS

This paper has discussed the history and principles behind the estimation of vehicle aggressivity ratings. It has used these concepts to develop a framework for estimation of vehicle aggressivity ratings for Australian passenger vehicles. These methods have been successfully applied to estimate two different types of aggressivity ratings.

Firstly, ratings of aggressivity towards drivers of other passenger vehicles, measuring the risk of serious injury a vehicle poses to drivers of other cars with which it impacts, were calculated for 56 models of Australian passenger vehicles. Secondly, ratings of aggressivity towards unprotected road users, namely pedestrians, motorcyclists and bicyclists, measuring the risk of serious injury a vehicle poses to these road users when injured in a crash, were estimated for 85 different vehicle models.

Estimated vehicle aggressivity towards drivers of other vehicles was found to have an increasing relationship with subject vehicle mass. It was also found to have little or no relationship with ratings of vehicle crashworthiness, demonstrating the independence of the two complementary measures.

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Investing in Transport,
Sydney, December 1998.

Paper No. 20

LOCALISED EFFECTS ON CRASHES OF THE COUNTRY RANDOM BREATH TESTING AND PUBLICITY PROGRAM IN VICTORIA

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ABSTRACT

Crashes occurring on major and minor roads in areas of country Victoria influenced by random breath testing (RBT) activity during 1993-1994 were analysed, so that the effects of styles of RBT operations, and the interactions of these effects with the levels of publicity awareness, could be seen. The major findings suggest that at times of intense RBT enforcement and high publicity awareness in rural areas of Victoria, some drink-drivers changed their travel behaviour and used relatively unsafe minor roads. Under these circumstances statistically significant increases in high alcohol hour serious casualty crashes occurred on minor roads but not on major roads. These findings provide the basis for a decision to schedule RBT operations on minor roads as well as on major roads.

INTRODUCTION

In November 1993, the Victoria Police, in conjunction with the Transport Accident Commission (TAC), launched a major program aimed at increasing random breath testing (RBT) in country Victoria and supporting these enforcement operations with mass-media publicity. The aim was to increase the number of random breath tests conducted in country Victoria to at least 700,000 during a 12 month period commencing late November 1993, while maintaining the high levels of testing activity built up in Melbourne during 1990-1993. The program was subsequently extended beyond November 1994 after the initial 12 months.

To achieve the goal of at least 700,000 tests, the TAC contracted the Victoria Police to send additional Melbourne-based "booze buses" to country Police districts to supplement the six buses placed permanently in country areas. Police cars were also used as RBT stations in country Victoria during the program, sometimes in conjunction with bus-based operations in the same area. The cars are equipped with an illuminated roof-top sign which can display an RBT message during testing operations (Moloney, 1994). The TAC also launched a country RBT campaign through rural media (including television, radio, press and billboards) during November-December 1993. This campaign was used to raise awareness of country residents to the new level of RBT activity. Other mass-media publicity with drink-driving themes were placed in these same media during the second half of 1994.

Objectives

Monash University Accident Research Centre (MUARC) was contracted by TAC to carry out the evaluation. The objective was to evaluate the program in terms of implementation characteristics and its effects on road trauma. The evaluation was carried out in the following four stages:

- i. The first stage was a documentation of the program (a process evaluation), which summarised the levels and nature of the RBT operations and supporting publicity in country Victoria during late 1993 to December 1994.
- ii. The second stage, a crash-based evaluation, aimed to measure the general effects of the program across the whole of country Victoria.

- iii. The third stage (also a crash-based evaluation) measured the localised effects in the areas and periods directly influenced by the program, so that the differential effects of the styles of RBT operations accompanied by different levels of publicity support could be seen.
- iv. The fourth stage arose from the findings of the third stage, and aimed to investigate the locations of alcohol-related crashes within the regions influenced by the RBT operations, possibly due to changes in drink-drivers' travel behaviour.

Stages i, ii and iii were summarised in Cameron et al (1996), whilst this paper focuses on the results of the fourth stage. It aims to test the hypothesis that a greater proportion of alcohol-related crashes occurred on minor roads, rather than major roads, during the country RBT program because of changes in drink-drivers' choice of route. A full report on all stages of the study can be found in Cameron et al (1997).

METHODOLOGY

Random Breath Test and Crash Data

Data had been obtained on RBT sessions conducted by Police from 25 November 1993 to 31 December 1994. Included in the data were the number of tests and hours of RBT operation, as well as the Local Government Area (LGA) of country Victoria where each session operated. A total of 153 different LGAs had some form of RBT activity during the program. In addition, for each RBT session, the style of operation as defined by the type of Police vehicle used was recorded. The styles considered were a TAS Bus (fully-staffed Melbourne-based booze bus sent by the Traffic Alcohol Section, TAS, of the Police to country Victoria primarily on weekends), a District Bus (country-based booze bus operated by Police Districts in country Victoria) and a Police car (generally cars operated by Police based in country Victoria).

Data on the numbers of high alcohol hour (HAH) serious casualty crashes that occurred in country Victoria during the RBT program were collected. High alcohol hours of the week are those periods when illegal drink-driving is more likely to occur. The country high alcohol hours were defined as 6 p.m. Sunday to 6 a.m. Monday, 6 p.m. to 4 a.m. on Monday to Wednesday nights, 6 p.m. Thursday to 6 a.m. Friday, 6 p.m. Friday to 8 a.m. Saturday, and 4 p.m. Saturday to 10 a.m. Sunday (Gantzer, 1995). The remaining hours of the week were described as the country "low alcohol hours" (LAH).

For comparison with the crashes which occurred during the RBT program, there was a need to estimate the expected number of accidents which would have occurred in the same week and area in the absence of RBT activity. Earlier studies have used accident experience in previous years (Cameron and Strang, 1982). For this study, the crashes occurring from late November 1992 to December 1993 were used. However, since the period of late November 1993 to December 1993 was part of the RBT program, the crashes that occurred in the previous year's corresponding period were used as a proxy for these crashes. This meant that the crashes occurring from late November 1992 to December 1992 were used as a comparison twice in the analysis. It was assumed that the crashes in late 1992 represented those in late 1993. Only a relatively small proportion of "before" crashes were counted twice (ie. 10.5%), therefore the double-counting would not unduly bias the results.

It should be noted that the nature of the analysis was such that because there was some RBT activity in country Victoria before the program, only the effects of the *changes* in RBT activity due to the program could be measured, not the *absolute* effects of country RBT activity. In addition, the analysis relied upon the assumption that the aggregation of crashes across a number of areas had the effect of averaging out the between-area variations in RBT activity and styles of operation. For this reason, the most reliable results were those based on aggregations of crashes over broad areas.

The crashes that occurred during the pre-RBT and RBT periods in rural Victoria were categorised as major or minor road crashes depending on the type of road they occurred on. A major road was defined to be a freeway, state highway or a main road (using VicRoads route number classifications), whilst a minor road was a local, forest, tourist or other road.

Analysis Strategy

For each road type, the change in the number of HAH serious casualty crashes in the regions influenced by RBT activity, during the weeks of influence compared with the same weeks in the previous year, was calculated. This represented the first estimate of the effect of the particular style of RBT operation. The possible effects of other factors affecting crashes in the same regions during the weeks were taken into account by analysing the change in LAH serious casualty crashes in parallel. Other factors may have included changes in the speed camera program, which previous research has shown to reduce both HAH and LAH casualty crashes (Cameron, Cavallo and Gilbert, 1992; Newstead, Mullan and Cameron, 1995; Newstead, Gantzer and Cameron, 1996). Newstead, Gantzer and Cameron (1996) found statistically significant reductions in serious casualty crashes during both low alcohol and high alcohol hours of the week in Melbourne as a result of an increase in the number of speed-camera traffic infringement notices (TINs) issued. A significant reduction in crashes during LAHs of the week in country Victoria was also found, whilst Newstead, Mullan and Cameron (1995) found evidence of a statistically significant localised speed camera effect on rural arterial roads during HAHs when a 15km radius of influence was considered. However, LAH crashes were considered to be relatively unaffected by a drink-driving program such as the country RBT and publicity program. The change in the LAH crashes was considered to measure the effects of the other factors, if any.

The difference between the percentage change in HAH crashes and the percentage change in LAH crashes was considered to provide a better estimate of the effect of the particular RBT style under consideration. The net percentage change was calculated to estimate this effect. However, it may be a conservative measure of the effect of this component of the country RBT and publicity program if, as is possible, the presence of enforcement had an effect on LAH crashes as well as HAH crashes.

Before analysing the crashes in this way, there was a need to define the regions influenced by the RBT activity in each week. Identification of RBT-influenced regions involved grouping the existing LGAs into amalgamated areas. Initial examination of the RBT data showed that some LGA boundaries occurred in areas of high RBT activity and high traffic flow which could lead to a "spillage" effect of RBT activity from one LGA to a neighbouring LGA. Such spillage effects would be overlooked in the analysis if the 153 LGAs were treated separately. It was therefore decided to group the LGAs into areas that ideally had boundaries of low RBT activity and low traffic flow to minimise the spillage effect. Within each Police District, each LGA was methodically examined to determine if it could stand alone or if it needed to be amalgamated with another LGA. Whether RBT activity in one LGA was able to influence crash frequencies in another LGA (either by word of mouth or by the testing of drivers) depended on various factors. These factors included the terrain and size of the LGA, whether the LGA contained or bordered a large rural city, whether a major highway passed through the LGA, and the amount of RBT activity that had occurred in the LGA during the program. When all these factors were considered, the 153 LGAs were amalgamated into 70 new regions.

The RBT program period was then divided into 58 weeks, and for each week and each region, the amount of activity (in hours and tests) was determined. An RBT-influenced region was considered to be one which had any activity in a given week. Similar processes were undertaken for each style of operation (TAS Bus, District Bus and Car) and combination of styles (TAS Bus/Car and District Bus/Car), to determine whether RBT activity was to be considered present or absent for each style or combination. A conditional selection process was then used to identify the crashes which occurred when an RBT influence was considered to be present, for each style of operation considered.

Supporting Levels of Publicity Awareness

At the same time as the Melbourne-based buses commenced country operations on weekends, a country RBT publicity campaign was launched in a four week burst at the end of 1993. All advertisements were targeted at drink-driving males, aged 18-29 years, living and driving in rural areas and being exposed to rural media. There were also other drink-driving bursts, with rural themes, during July, September/October and November/December 1994. Television exposure was measured weekly using TARPs (Target Audience Rating Points), a measure of audience reach, during November 1993 to December 1994. Drink-driving advertising awareness for country Victoria television viewers was measured by a function of TARPs related to "Adstock" (Broadbent, 1979) which represents the audience's retained awareness of current and past levels of advertising.

Country Victoria audience's retained awareness of current and past levels of advertising in each week during the program was represented in a chart (see Figure 1). This chart was used to categorise the publicity support into levels of "Adstock" units. The levels of publicity awareness considered were 'low' (under 200 Adstock units), 'medium' (200-800 Adstock units) and 'high' (above 800 Adstock units). For each road type, the interaction of the effect of the RBT operations with levels of awareness of drink-driving advertising was examined by calculating the net percentage change in HAH serious casualty crashes during periods when different levels of awareness were suggested by the "Adstock" function. It should be noted that the changes in HAH crashes calculated in this way do not represent an estimate of the effects of drink-driving advertising *per se*. They represent an estimate of the effect of each RBT style, conditional on the level of publicity awareness at the time the RBT activity was operating.

Statistical Methodology

The standard two-way chi-square test of the crash frequencies, categorised by alcohol time of week (HAH v. LAH) and period (pre-RBT program v. during RBT program), was used to test the statistical significance of the net percentage change for each road type and for each style of operation (Ryan et al, 1985). For each road type (major or minor), the statistical significance of differences in the net percentage change for each style was tested by analysing the three-way interaction in contingency tables of crash frequencies categorised by time of week, period and style (Bishop et al, 1975 and Christensen, 1990). Log-linear categorical models were fitted to the crashes in each contingency table to assess the statistical significance of this interaction, using the statistical package GLIM (Aitkin et al, 1990). A similar method of analysis was used to test the statistical significance of the interaction of the effects of the RBT operations with the levels of publicity awareness.

Consideration was given to the increased chances of Type 1 statistical errors when a number of hypothesis tests were carried out with each judging statistical significance at conventional levels of statistical significance (say, $p < 0.05$ or lower). There were at least eight independent hypothesis tests considered, testing the effects of five styles of operation and testing the effects of the RBT activity during three periods of publicity awareness. This suggests that a maximum significance level of approximately $p = 0.006$ should be used before any one test is judged to be statistically significant. Against this is the increased risk of a Type 2 statistical error, i.e. that evidence of an effective form of country RBT activity will be judged to be due to chance and hence ignored. Given the importance of finding effective countermeasures to drink-driving in country areas, this suggests that higher significance levels than $p = 0.006$ should be considered. The reader should bear these considerations in mind while examining the statistical significance of the results.

RESULTS

Crashes during the weeks of enforcement

Table 1 compares the serious casualty crashes in regions and weeks during which cars or District buses or TAS buses (i.e. any style of operation) conducted at least one hour of RBT operations in the week of enforcement. The crashes are categorised by road type (major v. minor), alcohol time (HAH v. LAH) and program period (pre-RBT program v. during RBT program). As a further comparison, the serious casualty crashes that occurred on all roads are given.

Table 1. Serious casualty crashes during the weeks of enforcement in regions and weeks influenced by any style of operation, by alcohol time, program period and road type

Type Of Road	Alcohol Time	Period		% Change	Net % Change
		Pre-RBT program	During RBT program		
Major	HAH	336	299	-11.0	-10.1
	LAH	485	480	-1.0	
Minor	HAH	241	247	+2.5	-1.6
	LAH	361	376	+4.2	
All Roads	HAH	578	546	-5.5	-6.8
	LAH	846	857	+1.3	

In areas and weeks where there was any RBT activity, there was an 11% decrease in HAH serious casualty crashes on *major* roads during the RBT program relative to the pre-RBT period. The corresponding change for LAH serious casualty crashes was a 1% decrease. To estimate the difference between the HAH and LAH percentage changes, the net percentage change was calculated, producing a net percentage decrease of 10.1%. Thus an apparent 10.1% decrease in HAH serious casualty crashes (relative to LAH crashes) occurred during the program (relative to before the program) in RBT influenced areas during the weeks of RBT activity on major roads of country Victoria.

On *minor* roads, the corresponding change for HAH serious casualty crashes was a 2.5% increase, and for LAH serious casualty crashes a 4.2% increase. This resulted in an apparent net 1.6% decrease in HAH serious casualty crashes relative to LAH serious casualty crashes during the program in RBT influenced areas during the weeks of enforcement. The 6.8% decrease in HAH serious casualty crashes that resulted on all roads in RBT influenced regions was therefore, primarily due to the 10.1% decrease that occurred on major roads. However, it should be noted that the net percentage reduction found for major roads was not statistically significant ($p = 0.2987$), nor was the smaller reduction found on minor roads ($p = 0.8902$). Furthermore, a log-linear model fitted to the crashes to test for a three-way interaction between alcohol time, program period and road type (major v. minor) did not find a statistically significant effect ($p = 0.5612$).

Style of RBT operation

For major roads, a log-linear model fitted to the crashes to test for a three-way interaction between style of RBT operation (as defined), alcohol time and program period found a statistically significant interaction ($p = 0.0433$). Therefore, there appears to be a style of operation effect on major roads during the weeks of enforcement in the RBT-influenced regions. The corresponding three-way interaction for minor road crashes was statistically significant at the 9% level ($p = 0.0802$). The major and minor road crashes that occurred during the weeks of enforcement are presented in Table 2 for each style of operation.

Table 2. Serious casualty crashes occurring during the weeks of enforcement in regions and weeks influenced by RBT operations, by style of operation, road type, alcohol time and program period

MAJOR ROADS				
Style of Operation	Alcohol Time	Period		Net % Change
		Pre-RBT program	During RBT program	
Cars alone	HAH	214	165	-24.0*
	LAH	288	292	
District or TAS buses alone	HAH	26	20	-14.8
	LAH	31	28	
Cars & buses together	HAH	80	108	+35.0**
	LAH	147	147	
MINOR ROADS				
Style of Operation	Alcohol Time	Period		Net % Change
		Pre-RBT program	During RBT program	
Cars alone	HAH	157	135	-20.2
	LAH	206	222	
District or TAS buses alone	HAH	16	11	-34.4
	LAH	21	22	
Cars & buses together	HAH	65	95	+36.9***
	LAH	118	126	

*Statistically significant decrease at the 4% level ($p=0.0390$)

**Statistically significant increase at the 12% level ($p=0.1102$)

***Statistically significant increase at the 13% level ($p=0.1266$)

Cars operating alone or buses operating alone produced net percentage reductions in HAH serious casualty crashes on major roads during the RBT program (as compared to the pre-RBT program). Cars produced the larger reduction of 24%; this reduction was also statistically significant ($p=0.0390$). Buses operating alone produced a smaller non-significant reduction (this may possibly be due to the relatively small number of crashes). Conversely, when cars and buses were operating together there was evidence of a 35% net increase in HAH (relative to LAH) serious casualty crashes during the RBT period (relative to the pre-RBT period). This increase was statistically significant at the 12% level ($p=0.1102$).

There was evidence of a net increase in HAH (relative to LAH) serious casualty crashes on minor roads during the RBT period in areas where combinations of cars/District buses or cars/TAS buses were operating together. This increase, however, was only statistically significant at the 13% level ($p=0.1266$). The magnitude of the increase was similar to that found for major road crashes. In contrast when cars or buses were operating alone, net percentage reductions in HAH serious casualty crashes occurred on minor roads. Bus operations produced the largest reduction of 34.4% (this compares with a 14.8% reduction in major road crashes), however the reduction was not statistically significant ($p=0.3952$), and hence could have been due to chance.

Interactions with publicity awareness

To test the hypothesis that in rural areas, drink-driving behaviour in reaction to intense enforcement and/or intense publicity is different to that found in urban areas, the interactions with publicity for each road type were examined. The aim was to determine if at times of intense enforcement and intense drink-driving publicity, drivers in rural areas tend to travel (and crash) more frequently on minor roads after drinking, and avoid major roads where they may believe that RBT stations are more likely to be located. This behaviour could be expected to result in a significant increase in crashes on the minor roads.

The crashes occurring during the weeks of enforcement on major and on minor roads were analysed separately for weeks when low, medium or high levels of publicity awareness (as defined) were present in country Victoria. A log-linear model was fitted to the crash data to test for the presence of a significant interaction between publicity, program period, alcohol time and road type during the weeks of enforcement when any style of RBT was operating. The test for the four-way interaction was significant at the 11% level ($p=0.1046$), indicating that there was some evidence of an interaction between road type and publicity awareness. The major and minor road crashes that occurred during the weeks of enforcement are presented in Table 3 for each level of publicity awareness. As a comparison the corresponding crash frequencies for crashes occurring on all roads are also given.

Table 3. Serious casualty crashes occurring during the weeks of enforcement in regions and weeks influenced by any style of operation, by road type, level of publicity awareness, alcohol time and program period

ALL ROADS				
Publicity Awareness	Alcohol Time	Period		Net % Change
		Pre-RBT program	During RBT program	
Low	HAH	117	122	-12.7
	LAH	165	197	
Medium	HAH	350	305	-14.3
	LAH	485	493	
High	HAH	111	119	+25.8
	LAH	196	167	
MAJOR ROADS				
Publicity Awareness	Alcohol Time	Period		Net % Change
		Pre-RBT program	During RBT program	
Low	HAH	66	75	-1.1
	LAH	97	109	
Medium	HAH	205	168	-15.6
	LAH	274	266	
High	HAH	65	56	-6.5
	LAH	114	105	
MINOR ROADS				
Publicity Awareness	Alcohol Time	Period		Net % Change
		Pre-RBT program	During RBT program	
Low	HAH	51	47	-28.8
	LAH	68	88	
Medium	HAH	144	137	-11.2
	LAH	211	226	
High	HAH	46	63	+81.1*
	LAH	82	62	

*Statistically significant increase at the 3% level ($p=0.0202$)

Crashes on all roads

When low levels (<200 Adstock) of publicity awareness were operating, a 12.7% net reduction ($p=0.4173$) occurred for HAH serious casualty crashes on all roads influenced by the RBT program in regions and weeks where any style of RBT was present. A slightly larger net reduction of 14.3% occurred during the weeks when the publicity awareness was at a medium level (200-800 Adstock). This reduction was statistically significant at the 13% level ($p=0.1277$). Conversely, there was no evidence of a reduction in HAH serious casualty crashes during weeks when the RBT activity was accompanied by high levels of publicity awareness. A

net increase in crashes of 25.8% resulted, but this was not statistically significant ($p=0.1734$). The question was did similar net percentage changes in HAH serious casualty crashes occur on major and on minor roads at each level of publicity awareness?

Crashes on major roads

For each level of publicity awareness there were reductions in crashes on major roads during the weeks of enforcement in the regions influenced by RBT. The largest reduction of 15.6% occurred at times of medium drink-driving publicity awareness. However, none of the crash reductions were statistically significant. At times of high publicity awareness it can be seen from Table 3 that there was a net *increase* in crashes of 25.8% on all roads but a net *decrease* of 6.5% on major roads, suggesting that the increase was predominantly on minor roads.

Crashes on minor roads

A log-linear model was fitted to the crashes on minor roads to test for the presence of a three-way interaction between publicity awareness, program period and alcohol time during the weeks of enforcement. There was evidence of a significant three-way interaction ($p=0.0210$), indicating a publicity awareness effect on the net changes in HAH serious casualty crashes that occurred on minor roads in RBT-influenced areas during the weeks of enforcement.

When low levels of publicity awareness (<200 Adstock units) were operating, a 28.8% net reduction ($p=0.1889$) occurred for HAH serious casualty crashes on *minor* roads in regions and weeks where any style of RBT was present. A smaller net reduction of 11.2% ($p=0.4385$) occurred during the weeks when publicity awareness was at a medium level (200-800 Adstock units). Neither of these reductions, however, were statistically significant. Conversely, there was evidence of an increase in HAH serious casualty crashes on minor roads during weeks when the RBT activity was accompanied by high levels of publicity awareness (>800 Adstock units). A statistically significant net increase of 81.1% resulted ($p=0.0202$). In comparison, for HAH serious casualty crashes occurring on *major* roads a net *decrease* of 6.5% resulted, although this was not statistically significant.

Interactions between publicity awareness and style of operation

The remaining question was whether there were also indications of increases in minor road crashes when intense enforcement was operating in conjunction with high publicity awareness levels in country Victoria? To answer this question interactions between publicity awareness and style of operation for minor road crashes were investigated.

The crash frequencies for individual bus-only and individual car/bus combinations were relatively small on minor roads for each publicity awareness level in the RBT-influenced areas and weeks as a result of the infrequency of these operations compared to cars. Therefore, it was decided to pool the minor road crashes in regions influenced by booze buses (TAS or District bus) and by car/booze bus combinations (car/District bus or car/TAS bus). These types of operation included a high-profile booze bus operating in the region and hence it could be expected that the RBT would be perceived as being more intense than car-only operations. Table 4 gives the HAH and LAH serious casualty crashes that occurred on minor roads during the weeks of enforcement in regions and weeks influenced by each style of operation at each level of publicity awareness.

Car operations

The three-way interaction between publicity awareness, alcohol time and program period was only statistically significant at the 18% level for car-only operations ($p=0.1785$), indicating little significant effect of different levels of publicity on minor road crashes influenced by car

operations. However, when *medium* levels of publicity awareness were operating, a statistically significant 33.3% net *reduction* occurred for HAH serious casualty crashes on minor roads in regions and weeks where cars were operating ($p=0.0445$). There was also a smaller net reduction of 28.4% at times of low publicity awareness but this was not statistically significant ($p=0.3549$). At times of high publicity where car operations were present, a net increase in minor road crashes of 30.2% was found; this again was not statistically significant.

Table 4. Serious casualty crashes occurring on minor roads during the weeks of enforcement in regions and weeks influenced by RBT operations, by style of operation, level of publicity awareness, alcohol time and program period

CARS OPERATING ALONE				
Publicity Awareness	Alcohol Time	Period		Net % Change
		Pre-RBT program	During RBT program	
Low	HAH	29	27	-28.4
	LAH	30	39	
Medium	HAH	90	70	-33.3*
	LAH	120	140	
High	HAH	38	38	+30.2
	LAH	56	43	
BUS or CAR/BUS COMBINATIONS				
Publicity Awareness	Alcohol Time	Period		Net % Change
		Pre-RBT program	During RBT program	
Low	HAH	21	19	-31.6
	LAH	34	45	
Medium	HAH	52	62	+13.6
	LAH	80	84	
High	HAH	8	25	+311.2**
	LAH	25	19	

*Statistically significant decrease at the 5% level ($p=0.0445$)

**Statistically significant increase at the 1% level ($p=0.0043$)

Bus or car/bus operations

The test for the presence of a three-way interaction between publicity awareness, alcohol time and program period for bus or bus/car combinations was statistically significant ($p=0.0144$) for minor road crashes. This suggests that in areas of country Victoria when buses or car/bus combinations were in operation, the effects of the RBT activity on crashes on minor roads were related to the level of awareness of the drink-driving publicity.

When low levels of publicity were operating, a 31.6% net *reduction* ($p=0.3282$) occurred for HAH serious casualty crashes in regions and weeks when booze buses or a combination of cars and buses were present. However when medium levels were operating a 13.6% net *increase* ($p=0.6033$) resulted. Neither of these net changes in crash frequencies was statistically significant. An even greater net *increase* of 311.2% occurred at times of high levels of publicity awareness for HAH serious casualty crashes on minor roads. This increase was statistically significant ($p=0.0043$). Thus when high levels of drink-driving publicity were operating, a significant increase in HAH serious casualty crashes on minor roads occurred in the regions and weeks when buses or car/bus combinations were present. It is worth noting, however, that the increase in crashes in *absolute* terms was relatively small. The net increase was estimated to represent an additional 19 serious casualty crashes that would have occurred on minor roads when the enforcement was accompanied by high levels of publicity awareness during the RBT program.

These results suggest that, in regions and weeks of intense RBT activity at times of high publicity awareness in rural areas of Victoria, significant increases in HAH serious casualty crashes occurred on minor roads but not on major roads. This may be because drink-drivers change their driving habits after drinking when they are aware that there is intense RBT in the area, highlighted by the high level of publicity, and thus travel on minor roads to a greater extent. Crash frequencies may therefore have increased because of increased traffic volumes on these roads and because of the generally lower standard of minor roads in country areas (compared to major roads). Harrison (1996) also supported the suggestion that drink-driving is likely to differ in rural areas, through a study that was based on survey results of hotel patrons in rural regions of Victoria. The study found that almost half of the respondents surveyed indicated that they would choose to drive home from a hotel by an alternative route if they knew about the location of a booze bus.

DISCUSSION

Localised effects of the program

Localised effects were measured by analysing the changes in HAH crashes, in the regions and weeks influenced by the enforcement operations, compared with the year before. Changes in LAH crashes, in the same areas and weeks, were used as the comparison for the changes in HAH crashes in the regions and weeks influenced by the program. This method could be considered a conservative way of estimating the program effects, if the program is considered likely to have affected LAH crashes as well as HAH crashes. It also relies on the assumption that the effects of other factors, separate from the RBT program, would have been the same on both HAH and LAH crashes.

The analysis of localised effects found evidence of reductions in HAH serious casualty crashes in regions influenced by certain styles of RBT operations and/or when certain levels of publicity accompanied the enforcement (Cameron et al, 1996). The circumstances when reductions occurred were when *cars alone* were operating, and especially when *cars* were operating at times of *medium publicity* awareness. There was little evidence of crash reductions during weeks when the RBT activity was accompanied by high levels (more than 800 Adstock units) of awareness of drink-driving publicity, or when combinations of cars and buses were operating together. Conversely, in regions and weeks influenced by *car and bus* combinations, a statistically significant net percentage *increase* in HAH serious casualty crashes occurred when *high publicity* awareness accompanied the enforcement.

Some of these findings were surprising given previous findings relating crash reductions in Melbourne to the visibility and intensity of RBT operations and to levels of supporting publicity (Cameron, Newstead and Gantzer 1995). An explanation may lie in the possible reactions of country drink-drivers when faced with intense enforcement, heightened by intense publicity.

Localised effects on crashes on major and minor roads

A possible reaction to a perception of intense drink-driving enforcement in a country region may have been for some drink-drivers to change their driving behaviour after drinking, rather than to change their drinking behaviour. This could have taken the form of travelling on minor roads, unfamiliar to them, rather than using the major roads and highways they normally use. Since minor roads in country areas are likely to be of lower standard than major roads, due to poorer maintenance, lower delineation and additional roadside hazards, it is likely that drink-drivers would have experienced increased risks of crashing and resulting injury on such roads. Such a change in travel behaviour could be expected to reflect in a greater proportion of alcohol-related crashes having occurred on minor roads, rather than major roads, during the country RBT program (at least in those regions and weeks influenced by intense enforcement and publicity).

Evidence found to support the hypothesis that during the 1993-1994 country RBT and publicity program, some drink-drivers faced with intense enforcement, heightened by intense publicity, changed their travel behaviour and used relatively unsafe minor roads, resulting in increased serious casualty crashes in these circumstances, included:

- i. RBT conducted by *cars* operating alone during the weeks when enforcement was present produced a statistically significant *24% reduction* in HAH serious casualty crashes on *major* roads in the enforced regions. A *20% reduction* also occurred for minor road HAH serious casualty crashes, however this reduction was not statistically significant and may have been due to chance.
- ii. *Cars and buses* operating together during the weeks of enforcement produced an estimated *37% increase* in HAH serious casualty crashes on *minor* roads in the enforced regions. The magnitude of the increase was similar for major road crashes. Neither increase, however, was statistically significant.
- iii. There was some evidence of an interaction between the type of road on which serious casualty crashes occurred and the levels of awareness of drink-driving television advertising in country Victoria.
 - When *low* levels of publicity (less than 200 Adstock units) awareness were operating a *29% net reduction* occurred for HAH serious casualty crashes on *minor* roads in regions and weeks where any style of RBT was present. A smaller net reduction of *11%* occurred during the weeks when publicity awareness was at a medium level (200-800 Adstock units). Neither of these reductions, however, was statistically significant.
 - Conversely, there was evidence of an increase in HAH serious casualty crashes on *minor* roads during weeks when the RBT activity was accompanied by *high* levels of publicity awareness (more than 800 Adstock units). A statistically significant net *increase* of *81%* resulted. In comparison, for HAH serious casualty crashes occurring on *major* roads a net *decrease* of *6.5%* resulted; however this was not statistically significant.
- iv. For *minor* road crashes, there was evidence of an interaction between the style of RBT operation and the levels of awareness of drink-driving television advertising in country Victoria.
 - When *medium* levels of publicity awareness were operating, a statistically significant *33% net reduction* occurred for HAH serious casualty crashes on *minor* roads in regions and weeks where *car-only* operations were present.
 - When *low* levels of publicity awareness were operating, a *32% net reduction* occurred for HAH serious casualty crashes on *minor* roads in regions and weeks when *booze buses* or a *combination of cars and buses* were present. However when medium levels were operating a *14% net increase* resulted. Neither of these net changes in crash frequencies was statistically significant.
 - Conversely, a statistically significant net *increase* of *311%* occurred at times of *high* levels of publicity awareness for HAH serious casualty crashes on *minor* roads in regions and weeks when *buses* or *car/bus combinations* were present. This increase is estimated to represent an additional 19 crashes occurring under these circumstances.

CONCLUSION

There was evidence of a reduction in the risk of high alcohol hour serious casualty crashes in country Victoria during the period November 1993 to December 1994 (Cameron et al, 1996). The extent to which it could be concluded that this reduction was a general effect of the country RBT and publicity program was related to the findings regarding the localised effects in the regions and weeks where the RBT operated.

The initial analysis of the localised effects on crashes of the country RBT and publicity program found evidence of a statistically significant reduction in HAH serious casualty crashes when RBT was conducted by cars operating alone, during the weeks and in the regions when enforcement was present. There was also some evidence of an interaction between the effects of the enforcement operations and the levels of awareness of drink-driving television advertising in country Victoria. Medium levels of awareness appear to increase the effects of the "car only" enforcement operations. Conversely, in regions and weeks influenced by car and bus combinations, a statistically significant net percentage increase in HAH serious casualty crashes occurred when high publicity awareness accompanied the enforcement.

These somewhat surprising findings led to the final stage of the evaluation, the results of which provided evidence to support the hypothesis that some drink-drivers faced with intense enforcement, heightened by intense publicity, changed their travel behaviour and used relatively unsafe minor roads. It was found that at times of intense enforcement and high publicity awareness in rural areas of Victoria, significant increases in high alcohol hour serious casualty crashes occurred on minor roads but not on major roads.

IMPLICATIONS OF THE FINDINGS

These findings provide the basis for a decision to schedule RBT operations on minor roads as well as on major roads. On 15 October 1997, the Victoria Police, in conjunction with the TAC, launched a new initiative targeted at drivers who think they can drink-drive by avoiding the conspicuous booze buses usually operated on major roads. Prior to this time, most Police cars were equipped with breath-testing equipment, however, as part of the campaign, operational orders were re-issued emphasising the requirement for Police to breath-test drivers apprehended for other offences. Two new TAC television advertisements were also launched at the same time alerting the community to this enforcement. Other enforcement practices that have been receiving more emphasis in rural areas in recent times include patrol cars "sitting off" hotels and clubs, with satellite cars working on back roads in concert with booze buses. These initiatives may have implications for drink-drivers who use minor roads in rural areas thinking they can avoid booze buses, because they may now have a greater perceived risk of being detected by other Police vehicles on any type of road.

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FIGURES

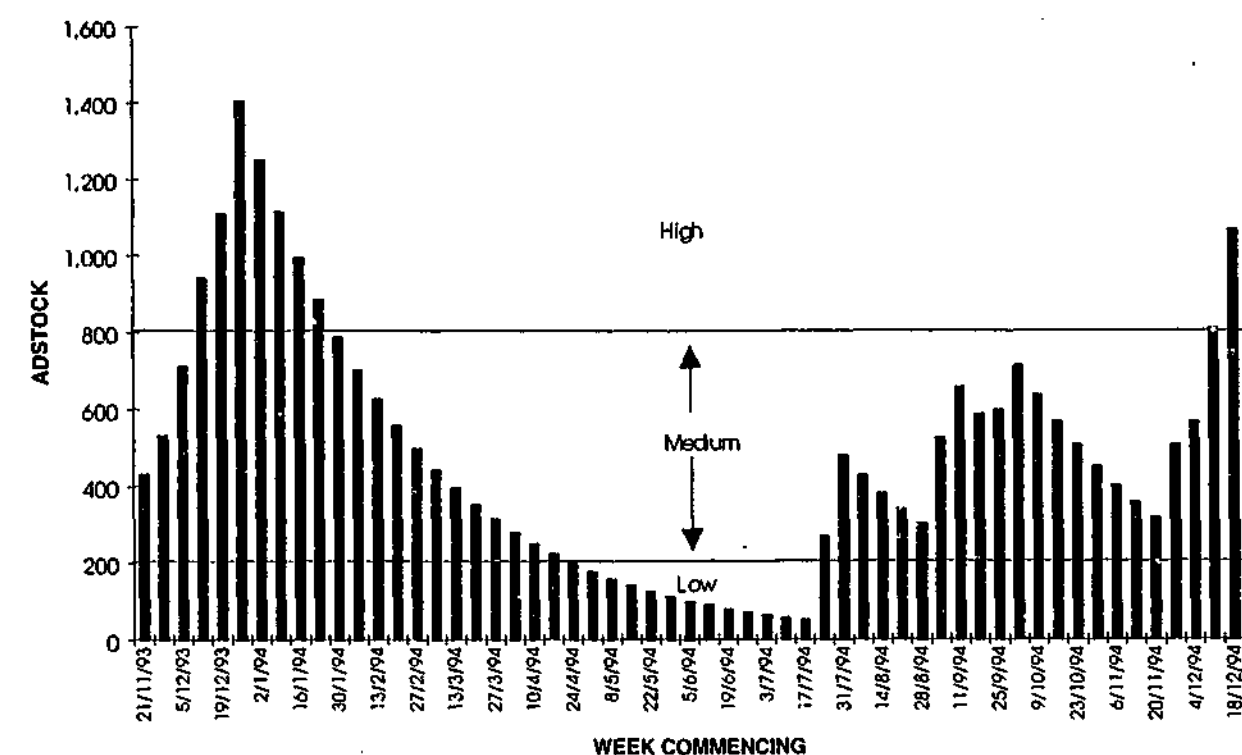


Figure 1. Drink-driving advertising Adstock per week in country Victoria, November 1993 to December 1994