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**RE-INVESTIGATION OF THE EFFECTIVENESS  
OF THE VICTORIAN  
TRANSPORT ACCIDENT COMMISSION'S  
ROAD SAFETY CAMPAIGNS**

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**Re-Investigation of the Effectiveness of the Victorian Transport Accident  
Commission's Road Safety Campaigns**

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**Abstract:** Monthly numbers of road crashes in Victoria, Australia, increased gradually from 1983 to 1988, before falling sharply over a period of about two years, and remaining at a low level until the end of 1992. In the late 1980s, the Victorian Transport Accident Commission (TAC) greatly increased the level of government funding for road safety television advertising and police enforcement. The TAC-funded countermeasures focused on the reduction of speeding and drink driving. The dramatic improvement in road safety in Victoria was the subject of a major program of research at the Monash University Accident Research Centre (MUARC). MUARC researchers concluded that the TAC-funded campaigns had been very successful. In particular, they concluded that very high levels of television advertising were cost effective. The main MUARC findings for Melbourne are re-investigated in this report. It is noted that crash numbers started to fall well before the TAC-funded countermeasures were implemented. The MUARC regression analyses of observational data were considered to be insufficiently robust to justify the conclusions reached. A parsimonious 'three factor' model was able to explain the Victorian crash trends in terms of gradual, long-term improvements in road safety, seasonal influences, and changes in the economy. The three factor model was replicated against New Zealand crashes from 1990 to 1997; and it provided a better fit than did a model that included a variable representing the Supplementary Road Safety Package (a campaign modelled on the 'Victorian experience').

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**Key Words:** Reanalysis; Evaluation; Trends; Road Crashes; Casualties; Transport Accident Commission; Television Advertising; Mass Media; Campaigns; Enforcement; Speeding; Drink Driving; Unemployment; Leading Index of Economic Indicators; Business Cycle; Econometric Analysis; Victoria; Australia; New Zealand

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**RE-INVESTIGATION OF THE EFFECTIVENESS  
OF THE VICTORIAN TRANSPORT ACCIDENT COMMISSION'S  
ROAD SAFETY CAMPAIGNS**

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**November, 2000**



## TABLE OF CONTENTS

	Page
<b>EXECUTIVE SUMMARY</b>	i-ii
<b>ACKNOWLEDGEMENTS</b>	iii
<b>1 VICTORIAN CRASHES, COUNTERMEASURES AND EVALUATIONS</b>	
1.1 Serious Casualty Crashes in Victoria, Australia, 1983 to 1992	1
1.2 Implementation of Enforcement Countermeasures	1
1.3 Implementation of New Television Advertising Campaigns	1
1.4 Possible Effects of the Downturn in the Victorian Economy	4
1.5 MUARC Evaluations of the Enforcement and Media Campaigns	5
1.6 Recommendations for High Levels of Television Advertising	6
<b>2 THE MUARC RESEARCH METHODOLOGY</b>	
2.1 MUARC Reports Which Identify a Separate Advertising Effect	7
2.2 The Dependent Variables in RN74	8
2.3 The Independent (Predictor, Explanatory) Variables in RN74	8
2.4 Additive and Multiplicative Relationships	10
2.5 The RN74 Regression Models	11
2.6 MUARC Regression Analyses Selected for Re-Investigation	12
<b>3 MUARC FINDINGS FOR MELBOURNE</b>	
3.1 Results from RN74 for Melbourne	13
3.2 Results from Cameron et al. (1993) for Melbourne	15
<b>4 LOW ALCOHOL HOUR (LAH) CRASHES IN MELBOURNE</b>	
4.1 LAH Univariate Descriptive Statistics	19
4.2 LAH Bivariate Correlation Matrices	24
4.3 Replication of the RN74 LAH Regression Analysis	25
<b>5 HIGH ALCOHOL HOUR (HAH) CRASHES IN MELBOURNE</b>	
5.1 HAH Univariate Descriptive Statistics	26
5.2 HAH Bivariate Correlation Matrices	31
5.3 Collinearity Between the Enforcement and Advertising Variables	33
5.4 Replication of the RN74 HAH Regression Analysis	34
<b>6 COUNTERMEASURES AND CRASHES: TEMPORAL RELATIONSHIPS</b>	
6.1 Some Misconceptions About the Timing of the Fall in Crash Numbers	35
6.2 Speed Countermeasures and LAH Crashes	37
6.3 Drink Drive Countermeasures and HAH Crashes	40
6.4 Comparable Findings from Drummond et al. (1992)	43
<b>7 RN74 CRASH MODEL SPECIFICATION</b>	
7.1 Experiments and Observational Studies	45
7.2 Confounding	46
7.3 Data Dredging and the Evolution of the MUARC Models	49
7.4 Problems of Variable Selection and Definition	50



<b>8</b>	<b>ADJUSTMENT FOR ECONOMIC ACTIVITY</b>	
8.1	Unemployment	56
8.2	Leading Index of Economic Indicators	58
8.3	Replacing Unemployment by CMA12 Lead Index in the RN74 Models	60
<b>9</b>	<b>FURTHER RATIONAL PERTURBATIONS OF THE RN74 MODELS</b>	
9.1	Including Speed Countermeasures in the HAH Crash Model	62
9.2	The Starting Date for Drink Drive Advertising	64
<b>10</b>	<b>INTERPRETATION OF THE REGRESSION COEFFICIENTS OF THE COUNTERMEASURE VARIABLES</b>	65
<b>11</b>	<b>A SIMPLE MODEL OF MELBOURNE CRASHES</b>	
11.1	The Three Factor Theory	72
11.2	Applying the Three Factor Theory to Melbourne Crashes	74
<b>12</b>	<b>NEW ZEALAND REPLICATIONS</b>	
12.1	The New Zealand SRSP and its Evaluation	77
12.2	Replicating Victoria's Results in New Zealand	77
12.3	Including a Linear Trend Variable in Time-Series Crash Models	78
12.4	Cameron and Vulcan's (1998) Evaluation of the SRSP	80
12.5	Re-Analyses of the New Zealand Data	86
12.6	Replicating the Three Factor Theory in New Zealand	90
12.7	Discussion	94
<b>13</b>	<b>DISCUSSION AND RECOMMENDATIONS</b>	
13.1	Summary of Main Findings	95
13.2	Large, Ephemeral v Small, Cumulative Countermeasure Effects	96
13.3	Alcohol Sales and the Economy	99
13.4	Conclusions Concerning the TAC Campaigns	101
13.6	Recommendations	102
<b>14</b>	<b>REFERENCES</b>	104
	<b>ATTACHMENT</b>	113

A Critique of Statistical Modelling of Observational Time-Series Data

by Michael White



## EXECUTIVE SUMMARY

Monthly numbers of road crashes in the state of Victoria, Australia, increased from 1983 to 1988, before falling sharply over a period of about two years, and then remaining at a low level until the end of 1992. Fatal crash numbers fell by about 50%; and the numbers of less severe crashes also fell substantially.

In the late 1980s, the Victorian Transport Accident Commission (TAC) greatly increased the level of funding for road safety television advertising and enforcement. These TAC-funded countermeasures focused on speeding and drink driving.

A program of research at the Monash University Accident Research Centre (MUARC) was undertaken to investigate the downturn in crashes. That program has resulted in the publication of at least 30 reports and papers.

Early research undertaken at MUARC by Cameron, Haworth, Oxley, Newstead and Le (1993) had attempted to determine cost-effective levels of road safety television advertising. That research involved the assumption that countermeasure effects were *quantitative* - in the sense that there were *dose-response* relationships between monthly advertising levels and monthly crash numbers. Those assumed quantitative relationships were central to Cameron et al's conclusion that there were additional benefits to be obtained from high, sustained levels of television advertising.

Much of the MUARC research was brought together in a report by Newstead, Cameron, Gantzer and Vulcan (1995). Their analyses of monthly *serious casualty crashes* in the *Melbourne metropolitan area* were taken to be representative; and have provided the main focus of this re-investigation. Newstead et al. separately analysed 'Low Alcohol Hour' (LAH) and 'High Alcohol Hour' (HAH) crashes, by conducting multiple linear regression analyses which used a linear trend, seasonality, unemployment levels, enforcement levels, advertising levels, and, in the case of HAH crashes, alcohol sales, as predictor variables. They used log-transformed values of the variables. Their main findings were that, beyond an appreciable effect of the downturn in the economy (and reduced alcohol sales in the case of HAH crashes), the TAC-funded countermeasures had been successful in reducing crash numbers.

In 1996, Transport SA commissioned MUARC to review the effectiveness of enforcement strategies in South Australia. The MUARC consultants (Vulcan, Cameron, Mullan & Dyte, 1996) insisted that the review be extended to include road safety television advertising. They recommended "That resources be allocated to double the exposure of television advertisements which support the speed camera and random breath testing programs" (p. 23). The main aim of the project reported here is to check the soundness of the advice concerning the effectiveness of high levels of television advertising, through re-analyses of the data originally analysed by Newstead et al. (1995).

One of the first re-investigations undertaken was simply to plot time-series of crash trends and countermeasure levels in the same graphs. It became obvious that crashes started to fall some months before the countermeasures were first implemented. The countermeasures could not, therefore, have been responsible for triggering the fall in crashes in the late-1980s.

Re-analyses of the data failed to confirm that the countermeasure and crash variables were related at the quantitative level. These results indicate that the dose-response findings of Cameron et al. (1993) in favour of high levels of road safety television advertising were not well grounded.

It was discovered that the regression models of Newstead et al. (1995) were not robust. Minor changes in the definitions and combinations of predictor variables resulted in very different conclusions about the strengths of the countermeasure effects (and, therefore, about the numbers of crashes 'saved').

A regression model should be treated cautiously unless its specification is tightly determined by a sound theory, or unless it has been successfully replicated against an independent set of data. The methods employed for the selection of variables in the MUARC modelling process can be described as 'data dredging'. As such, the models should be considered to be 'hypothesis generating' rather than 'hypothesis testing'. While such models may appear to provide accurate *descriptions* of the past, they should not be taken to provide proven *explanations* of the past; nor should they be taken to provide trustworthy *predictions* of the future (either at the place they were developed, or elsewhere). Accordingly, the results of the MUARC regression analyses should not be considered to provide an adequate basis for recommending the implementation of TAC-type countermeasures.

The authors of this report propose a simple, 'three factor' model to explain Melbourne total serious casualty crash trends from 1983 to 1992. The factors are: (1) gradual, long-term improvements in road safety; (2) seasonality; and (3) changing economic circumstances. In its most general form, the three factor model is not controversial: it simply demands that three well-recognised influences on crash trends be taken into account in any statistical modelling of time-series crash data. The specific version of the three factor model employed in this re-investigation involved the use of raw-data versions of the quantitative variables (rather than log-transformed versions, as used by the MUARC researchers). Gradual improvements in road safety were accounted for by a linear trend term. The economy was measured by a smoothed version of the Leading Index of Economic Indicators, which is a broad index of early activity in the 'business cycle' (rather than by Unemployment, as used by the MUARC researchers). The three factor model provided an excellent description of Melbourne total crash trends, without any reference to the TAC-funded countermeasures.

In a replication of the three factor model in New Zealand, quarterly numbers of road crash casualties, from 1990 to mid-1997, were satisfactorily described. These same casualty trends had previously been analysed by Cameron and Vulcan (1998) as part of an evaluation of the New Zealand Supplementary Road Safety Package (SRSP). However, Cameron and Vulcan failed to include a linear trend term in their model. When it was included, there was no longer any evidence that the SRSP (which had been modelled after the 'Victorian approach') had contributed to the fall in New Zealand casualties.

These findings do not necessarily imply that road safety enforcement or television advertising is ineffective. They simply mean that there has been a failure by Newstead et al. (1995) and Cameron and Vulcan (1998) to *demonstrate* any such effectiveness in Victoria or New Zealand. Given that MUARC consultancy advice has been in favour of very high levels of television advertising, that particular advice should now be considered to be unsubstantiated.

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During the course of the project, written commentaries have been provided by Mr Max Cameron; and discussions have been held with Mr Max Cameron, Mr Stuart Newstead and Professor Claes Tingvall, MUARC, and Mr David Healy, TAC.

Drafts of the report have been reviewed by: Dr Michael Henderson, of Michael Henderson Research; Associate Professor Soames Job, Psychology Department, University of Sydney; Professor John Eccleston, Department of Mathematics, University of Queensland; and Dr David Saffron, of David Saffron Pty Ltd.

Comments on draft reports have been provided by Mr Chris Brooks, Research Management and Policy, Australian Transport Safety Bureau; Dr Ernst Boehm, Melbourne Institute of Applied Economic and Social Research; and Mr Barry Elliott, Consultant Psychologist.

Detailed editorial comments were provided by Ms Fiona McEwen.

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## 1 VICTORIAN CRASHES, COUNTERMEASURES AND EVALUATIONS

### 1.1 *Serious Casualty Crashes in Victoria, Australia, 1983 to 1992*

Over the ten-year period from 1983 to 1992 the monthly number of serious casualty crashes in Victoria, Australia increased gradually, with some irregularities, for about six years, peaked, then fell dramatically within a period of about two years, before levelling out and remaining at a low level until the end of 1992 (see Figure 1.1.1 for serious casualty crash trends in Melbourne). Although the overall shape of the time-series distributions were much the same for crashes of different levels of severity, the peaks occurred at different times, with serious casualty crashes peaking a year or more before less severe crashes. The size of the reduction in *fatalities* was quite extraordinary—falling by almost 50% (from 776 in 1989 to 396 in 1992). Although reductions in serious and minor injuries were not as great, they were still substantial.

### 1.2 *Implementation of Enforcement Countermeasures*

The Victorian Transport Accident Commission (TAC) is the sole provider of compulsory third-party transport accident injury insurance in Victoria. In the late-1980s, the TAC greatly increased the level of government funding for police enforcement and road safety television (TV) advertising. The TAC-funded campaigns focused on drink driving and speeding.

Levels of random breath testing (RBT) were increased in late 1989, and the RBT campaign was enhanced in 1990 by the addition of thirteen highly visible 'booze-buses' to the existing fleet of four smaller buses. The emphasis in metropolitan Melbourne changed from car-based operations, which were substantially reduced between 1989 and 1992, to bus-based RBT.

Slant-radar speed cameras were introduced in December 1989, and by January 1991 there were fifty-four in operation. Prior to that, speed enforcement was conducted at lower levels, using equipment that was less effective for detecting large numbers of offenders.

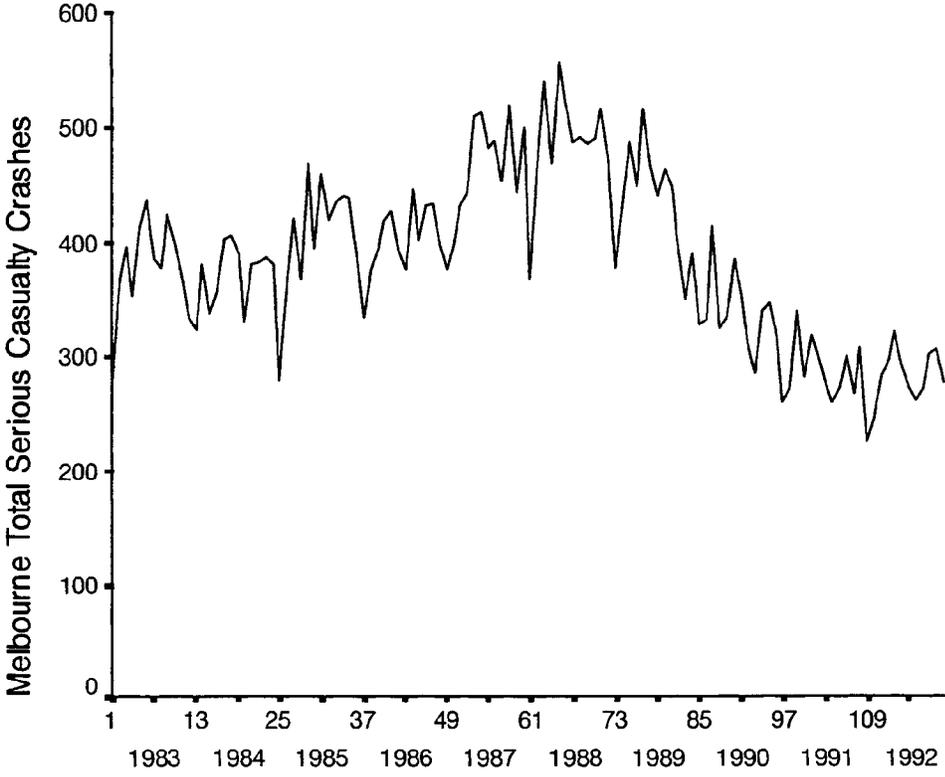
There is sound evidence that well-designed enforcement campaigns can be beneficial (e.g., Zaal, 1994). However, that does not mean that the TAC-funded enforcement campaigns were optimal; nor does it mean that any benefits derived from the TAC-funded enforcement campaigns will necessarily be discernible in the Victorian crash data.

### 1.3 *Implementation of New Television Advertising Campaigns*

The new TAC-funded road safety TV advertising campaigns mainly targeted speeding and drink driving. The drink drive media campaign was launched in December 1989, but the speed media campaign did not commence until April 1990. Prior to December 1989 TV campaigns targeting speeding and drink driving were managed by VicRoads.

Figure 1.1.1

Melbourne Monthly Serious Casualty Crashes  
1983 - 1992



## Level of Advertising

The media strategies adopted by the TAC have been controversial in a number of respects. First, the overall level of TV advertising has been extraordinarily high—both in terms of the number of new advertisements created each year, and the overall levels of exposure achieved (Hendrie & Ryan, 1995). Grey Advertising was the company commissioned by the TAC to develop and place the advertisements. A representative of Grey Advertising (Harper, 1992, p. 209) provided the following argument for the high media weight:

Road safety is an impulse decision that requires constant, high, top-of-mind product promotion to penetrate and motivate behaviour. It's more like buying a McDonald's hamburger. You need to be reminded of who McDonald's are, and what they sell, every day of the year.

Some Australian media researchers have pointed out that the levels of TAC-sponsored advertising are very high and that the consequent overload and wearout has probably resulted in much of the exposure being wasted (Elliott 1998; Fry, 1996; Mitchell & Chard, 1997).

## Cost of Production

The second controversial aspect of TAC advertising is that the executions have been very sophisticated and, as a consequence, the production costs have been high. Subsequent market research (Donovan, Jalleh & Henley, 1996) has challenged the view that there are additional benefits to be obtained from such big production budgets.

## Development of Messages

The third controversy concerns the appropriate means of determining the content and style of advertising messages. The conventional view is that message content should be based on research into the social psychology of human persuasion (e.g., Donovan, 1998; Donovan et al., 1996; Elliott, 1993, 1998; Job, 1988; Walker, 1996). Working from this *theoretical* perspective, the developers of road safety messages have stressed, among other aims, the importance of increasing the perceived risk of detection for offending. In comparison, the TAC approach has been to base advertising messages on *consumer product marketing principles*—guided by market research involving group discussions and telephone interviews (Harper & L'Hullier, 1990; Harper, 1992; Harper & Forsyth, 1994; Senserrick & Harrison, 1999; Sweeney, 1998). The major guiding principles have been expressed as a number of 'Do's' and 'Don'ts' (Harper, 1992, p. 195).

The Do's are to:

- be as shocking as you like
- be as emotional as possible
- ensure that a communication leaves us thinking "This could happen to me"
- emphasise the link between drink/drive, speed and real accidents.

The Don'ts are to:

- concentrate on twisted metal
- bore us with statistics
- lecture us
- threaten us with authority, uniforms or financial penalties
- suggest that we cannot have a drink.

The trust that the TAC has placed in the 'folk wisdom' elicited from focus groups has resulted in a strong emphasis on dramatic crash scenes (albeit without twisted metal) and high emotionality; and in a relative lack of emphasis on the risks of being detected for offending. An emphasis on 'scare tactics' in health promotion has been strongly criticised by some social commentators (Charlton, 1995; Mackay, 1994; Miller, 1992), but endorsed by others (e.g., Chapman, 1999). The TAC portrayals also emphasised the feelings of guilt and remorse of the offending driver.

The TAC TV advertisements are generally considered to have been very well executed (Donovan et al., 1999), and have won some prestigious industry awards (Harper, 1992; Harper & Forsyth, 1994; Harper & L'Hullier, 1990).

#### Environmental v. Behavioural Approaches to Road Safety

The fourth controversial issue is a much broader one: whether or not public safety is best advanced through *environmental* change or through a direct attempt to improve people's *attitudes* (e.g., Haight, 1973). Those who favour environmental change argue that the available evidence shows that campaigns which attempt to change attitudes through education or through TV advertising are generally ineffectual (e.g., Johnston, 1992; Koltzow, 1993; Robertson, 1994). The TAC media campaigns have also been criticised for inculcating a 'blame the victim' ethos in public thinking about road safety matters (Culvenor, 1997a, 1997b; Larsson, 1997).

#### Two Aims of the Media Campaigns

According to Healy and Forsyth (1996, p. 1) the TAC media campaigns had two fairly distinct types of aims. The first was to *directly* reduce the road toll through the immediate effects of advertising on driver behaviour (primarily via an increased fear of the consequences of crashing; but also through a heightened perception of the risk of being detected for offending). The second was to *indirectly* reduce the road toll by producing a climate where high levels of police enforcement of traffic offences would be tolerated by the public. One of the main aims of the research reported here is to evaluate the effectiveness of the TAC TV campaigns with respect to their direct effect on crash numbers. The second stated aim of the TAC advertising would be difficult to evaluate, and no attempt is made to do so.

#### *1.4 Possible Effects of the Downturn in the Victorian Economy*

There is a considerable body of literature on the relationship between the level of economic activity and road crash numbers. In reviewing this literature, Harry (1997) concluded that:

As the level of economic activity rises, so too will the number of road crashes. It has been shown that part of this increase is

attributable to increased exposure (as measured by vehicle kilometres travelled) as more discretionary travel becomes affordable. However, the *quantity* of exposure may not account for all of the rise. It is possible that a further part of the increase may be explained through changes in the *quality* of exposure resulting from changes in the demographics of the driving population. In particular, there will be an increase in the number of young drivers who may be relatively vulnerable to the effects of changes in the economy. When the economy improves, these drivers may be better able to afford discretionary social travel, and greater out-of-residence alcohol consumption, thereby increasing their level of high-risk exposure.

In summary, Harry (1997) has concluded that the relationship between economic activity and crash numbers is mediated through the *quantity* and *quality* of travel.

It is worth noting that some researchers have argued that particular instances of major fluctuations in crash numbers can be explained *entirely* in terms of changing economic conditions. For example, Partyka (1984) provided evidence that major cycles in annual US road crash fatality numbers over the period 1960 to 1982 could be accounted for entirely in terms of fluctuating annual labour force statistics. Similarly, a major downturn in crash numbers in British Columbia in 1982 was attributed entirely to sharply deteriorating economic conditions (Cooper, 1986). More recently, a four-fold increase in road fatalities in East Germany has been attributed to the dramatic improvement in economic conditions following reunification (Winston, Rineer, Menon & Baker, 1999).

Following the stock market crash in late-1987, there was a deep recession in the Victorian economy (Boehm, 1996; Thoresen, Fry, Heiman & Cameron, 1992). Beyond verifying the role of the economic downturn in the reduction of crash numbers, one aim of this paper is to investigate whether the contribution of the economy has been adequately accounted for by using unemployment as the measure of the economy.

### 1.5 MUARC Evaluations of the Enforcement and Media Campaigns

The TAC provided funds to the Monash University Accident Research Centre (MUARC) to undertake a series of evaluations of the TAC-sponsored enforcement and media campaigns.

As a result of the MUARC program of evaluation studies, a number of agency reports were produced (Cavallo & Cameron, 1992; Cameron, Cavallo & Gilbert, 1992; Cameron, Cavallo & Sullivan, 1992; Cameron, Haworth, Oxley, Newstead & Le, 1993; Drummond, Sullivan & Cavallo, 1992; Newstead, Cameron, Gantzer & Vulcan 1995; Newstead, Cameron & Narayan, 1998; Newstead, Mullan & Cameron, 1995; Rogerson, Newstead & Cameron 1994; Thoresen et al., 1992).

MUARC researchers also reported their main findings and some supplementary analyses at a number of conferences and symposiums (Diamantopoulou, Cameron & Shtifelman, 1998; Cameron, 1995; Cameron & Harrison, 1998; Cameron & Newstead, 1993a, 1993b, 1996; Cameron, Newstead & Gantzer, 1995; Cameron, Newstead & Vulcan, 1994; Gantzer, Newstead & Cameron, 1995; Narayan,

Newstead & Cameron, 1998; Shtifelman, Diamond & Cameron, 1999; Vulcan, 1993; Vulcan, Cameron & Newstead, 1995).

In addition, summary papers of the 'Victorian experience' have been published by representatives of the main government agencies involved: the Victoria Police (Bourne & Cooke, 1993; Bodinnar, 1994; Ogden, 1997); the roads authority 'VicRoads' (South, 1994; Ungers & Vincent, 1995); and the TAC (Forsyth & Ogden, 1993; Healy & Forsyth, 1996; Randall, 1995).

Although a number of different statistical techniques were employed by MUARC researchers in their earlier evaluations, multiple linear regression became the favoured technique in their later work.

The main findings from this MUARC research, very briefly, were that, beyond an appreciable effect of the downturn in the economy (and reduced alcohol sales in the case of 'drink drive' crashes), the enforcement and advertising campaigns were very successful in reducing monthly numbers of casualty crashes.

The findings from this research program have formed the basis of recommendations from MUARC consultants to government agencies in various localities (Vulcan, 1998), including: South Australia (Vulcan, Cameron, Mullan & Dyte, 1996); Western Australia (Cameron & Harrison, 1998; Cameron, Harrison, Vulcan, Pronk, Shtifelman & Narayan, 1997; Thompson, 1998); New Zealand (Bliss, Guria, Vulcan & Cameron, 1998; Cameron & Harrison, 1998; Cameron & Vulcan, 1998; Graham, 1998; Macpherson & Lewis, 1998; Tay, 1999); and KwaZulu-Natal in South Africa (Gray & Myers, 1999; KwaZulu-Natal Road Traffic Inspectorate, 1996). The MUARC consultancy to the South Australian Road Safety Consultative Council has been published as an agency report (Vulcan et al., 1996). That consultancy is discussed in more detail below.

### *1.6 Recommendation for High Levels of Television Advertising*

In June 1996, the Enforcement Strategies Working Party of the South Australian Road Safety Consultative Council, through the Safety Strategy Section, Transport SA, commissioned MUARC to review the effectiveness of enforcement strategies in South Australia. MUARC proposed that the enforcement review be extended to include road safety TV advertising, and their proposal was accepted by Transport SA. The extended 'enforcement review' (Vulcan et al., 1996) was completed in September, 1996.

A number of the review's recommendations were relevant to police enforcement activities. Those recommendations were subsequently implemented.

One recommendation from the enforcement review was "That resources be allocated to double the exposure of TV advertisements which support the speed camera and random breath testing programs" (Vulcan et al., 1996, p. 23). That recommendation was made because "Research at MUARC has shown that levels of intensity of TAC advertisements, with themes supporting enforcement aimed at drink-driving and speeding, can be linked with road trauma reductions" (pp. 22-23).

The main aim of the project reported here is to check the soundness of the MUARC consultancy advice concerning the effectiveness of high levels of TV advertising.

## 2 THE MUARC RESEARCH METHODOLOGY

### 2.1 MUARC Reports Which Identify a Separate Advertising Effect

The MUARC researchers conducted separate analyses of 'speed-related' and 'drink-drive related' crashes.

Two of the three MUARC reports on the evaluation of the *speed camera* program (Newstead, Mullan & Cameron, 1995; Rogerson et al., 1994) made no attempt to identify a separate effect of speed TV advertising on speed crash reductions. None of the early evaluations of the *RBT campaign* (Cameron, Cavallo & Sullivan, 1992; Cavallo & Cameron, 1992; Drummond et al., 1992) made any attempt to identify a separate effect of drink drive TV advertising.

The separate effects of speed TV advertising on crash reductions were first reported in Cameron, Cavallo & Gilbert (1992); and the separate effects of drink drive advertising were first reported in Cameron et al. (1993). This latter report also provided some further analyses of the speed advertising effect. Additional analyses of both speed and drink drive advertising effects were reported in Newstead, Cameron, Gantzer & Vulcan (1995). The same statistical methodology (multiple linear regression) was used in all the investigations of advertising effects.

In the two earlier reports covering advertising effects (Cameron, Cavallo & Gilbert, 1992; Cameron et al., 1993) there were some differences between the speed and drink drive analyses. In those reports, only nine years of data (1983 to 1991) were used in the speed analyses, while ten years of data (1983 to 1992) were used in the drink drive analyses. That meant that there were only about two years of non-zero values available for the speed-related advertising and enforcement variables, compared with about three years of non-zero values for the corresponding drink drive variables. Another difference between the speed and drink drive analyses was that advertising levels were measured only in terms of Target Audience Rating Points (TARPs) for speed, but in terms of both TARPs and Adstock for drink driving (those terms are explained below).

In the latest of the three MUARC reports to investigate TV advertising effects (Newstead, Cameron, Gantzer & Vulcan, 1995), both the speed and drink drive analyses were based on ten years of data; and advertising levels were measured only in terms of Adstock. In comparison with the earlier reports which replicated the regression analyses for different levels of crash severity, Newstead et al. analysed only serious casualty crashes. For those reasons, the re-investigations reported in this paper will be based on the data analysed in Newstead et al. As a consequence, only *two* regression models (one for 'speed-related crashes' and one for 'drink-drive related' crashes) will have to be replicated and investigated. For convenience, Newstead et al. (1995) will sometimes be referred to as 'RN74' (MUARC Report Number 74).

[From the References Section, it can be seen that there are two reports that could be abbreviated to 'Newstead et al. (1995)'. From here on, 'Newstead et al. (1995)' should be taken to refer to 'Newstead, Cameron, Gantzer and Vulcan (1995)'. As noted above, that report will also sometimes be referred to as 'RN74'.]

## 2.2 *The Dependent Variables in RN74*

The dependent variables in Newstead et al. (1995) (ie, RN74) were monthly serious casualty crash numbers, measured over a period of ten years (from 1983 to 1992).

HAH and LAH crashes. 'High Alcohol Hour' (HAH) crashes and 'Low Alcohol Hour' (LAH) crashes were analysed separately in RN74. HAH crashes were used in the evaluation of the drink drive campaigns. They correspond roughly to those crashes which occur at night-time. More specifically, they are crashes occurring at the following times: Monday to Thursday 6 pm to 6 am (the following day); Friday 4 pm to 6 am (on Saturday); Saturday 2 pm to 8 am (on Sunday); and Sunday 4 pm to 10 am (on Monday). Those hours were selected on the basis of MUARC research which indicated that they were the times when the majority of serious drink drive crashes happened (Harrison, 1990).

Speed campaigns were evaluated in terms of crashes occurring in 'Low Alcohol Hours' (LAHs)—where LAHs correspond roughly to day-time. More specifically, they are crashes occurring at times that complement the HAHs.

Metropolitan and Rural Crashes. Separate analyses were conducted for crashes occurring in metropolitan Melbourne (the capital of Victoria) and rural Victoria. As the metropolitan results were generally more definitive than the rural results, it is only the former that are further investigated in this report.

Level of Severity of the Crash. 'Serious Casualty Crashes' are those where (based on police reports) at least one road user was admitted to hospital, or killed. 'Non-Serious Casualty Crashes' are those where at least one road user required some medical treatment, but was not admitted to hospital. 'All Casualty Crashes' comprise the previous two categories. Although crashes of various levels of severity were analysed in both of the earlier reports (Cameron, Cavallo & Gilbert, 1992; Cameron et al., 1993), the influential economic analyses of cost-effective levels of advertising in the second of those reports were based entirely on All Casualty Crashes. However, as it was only Serious Casualty Crashes that were analysed in RN74, it is only those crashes that will be further analysed in this re-investigation.

## 2.3 *The Independent (Predictor, Explanatory) Variables in RN74*

Given that the re-analyses to be reported here concern only *metropolitan Melbourne*, variable definitions specific to *rural Victoria* are omitted from this sub-section.

The RN74 models included monthly measures of: trend, individual months (ie, seasonality), unemployment rate (Melbourne), enforcement level and advertising level. The LAH models had no additional predictor variable. The HAH models had one additional predictor variable: alcohol sales. The measures of enforcement and advertising levels were, of course, different for the LAH and HAH models. All of those variables are described in more detail below.

Trend. The trend variable simply took the values 1, 2, 3 etc. up to the total number of months for which the data was analysed. Given that the metropolitan Melbourne LAH and HAH models reported in RN74 were both based on ten years' data, the range of trend values was from 1 to 120. (In fact, the story is a little more complicated than this, as described in sub-section 2.5.)

Month. According to conventional practice, eleven dummy variables were created to code for the twelve months. There was no dummy variable for January. February was coded one (1) in February and zero (0) in all other months; March was coded one (1) in March and zero (0) in all other months, etc.. (Again, the story is a little more complicated than this, as described in sub-section 2.5.)

Unemployment. The monthly unemployment rate (percent unemployed) was for the Melbourne Statistical Division (rather than for the whole of Victoria).

Speed Enforcement. The enforcement measures used in the LAH modelling were 'TINs' and 'Hours'. The TINs variable was defined as the number of speed camera Traffic Infringement Notices issued during that month for the whole of Victoria. (It was assumed that any fluctuations in TIN numbers would be much the same for metropolitan Melbourne as for the whole of Victoria.) The Hours variable was defined as the number of hours of speed camera operation during that month in metropolitan Melbourne. Although the Hours variable was included in some earlier analyses, it was omitted from the analyses reported in RN74 because it was considered inappropriate to include two separate measures of speed enforcement, especially given that they were highly correlated. The Hours variable is therefore not included in the re-analyses reported here.

Drink Drive Enforcement. 'RBTs' was the only measure of enforcement used in the HAH models. This variable was defined as the number of Random Breath Tests conducted per month in metropolitan Melbourne. A distinction must be made between car-based and bus-based random breath testing. Vulcan (1995, p. 83) believed that random breath testing was "re-born" with the introduction of new booze buses, and so only bus-based RBTs were counted. The RBT variable used in the most recent analyses (Newstead et al., 1995, p. 5) was not identical with the corresponding variable used in the earlier analyses. The reason for the difference was that some car-based RBTs had accidentally been included in the earlier analyses. They were removed from the RBT variable before the more recent analyses were conducted. The re-analyses reported here use only the most recent version of the bus-based RBT variable.

Speed and Drink Drive Advertising. The level of advertising is usually measured by the industry in terms of Target Audience Rating Points (TARPs). TARPs are an index of the intensity of advertising defined according to the following rule: 100 TARPs means that everyone in the target audience had the opportunity to see the advertisement once (Fry, 1996). Adstock is an alternative measure of advertising level, derived from TARPs, which aims to reflect the build-up of advertising impact over time (Broadbent & Fry, 1985). The Adstock value for a particular week is a weighted sum of the TARPs in that week and in all previous weeks. The weighting function has a 'half-life' which, for the purposes of the MUARC calculations, was set at five weeks. Further investigations of the decay function for advertising recall are reported in Senserrick & Harrison (1999).

For all of the advertising variables, the values used are those for the whole of Victoria. (It was assumed that any fluctuations would be much the same for metropolitan Melbourne as for the whole of Victoria.)

Speed TARPs were defined so as to also include TARPs pertaining to a completely separate TV campaign on the theme of 'Concentrate or Kill'. For the purpose of the MUARC analyses therefore, 'Speed TARPs' were actually defined as 'Speed &

Concentrate TARPs'. The inclusion of the Concentrate component in the Speed advertising variable will be followed in the re-analyses reported here.

In the earlier MUARC analyses, results were reported either for TARPs only, or for both TARPs and Adstock. However, by 1995 (Newstead et al.), Adstock had become the preferred measure of advertising level. Adstock will therefore be used in the re-analyses reported here.

Alcohol Sales. The 'alcohol sales' variable was defined by Thoresen et al. (1992, p. 14) as the dollar value, in real terms, of monthly Victorian *Retail Trade* for liquor outlets such as hotels, liquor stores and licensed clubs (ABS Catalogue Number 8501.0). The 'real terms' adjustments are made according to the Melbourne *Consumer Price Index* (CPI) for Alcoholic Drinks (this information can only be obtained, for Melbourne, as a data file from ABS Client Services; but it is related to information for the whole of Australia provided in ABS Catalogue Number 6401.0). (It was assumed that any fluctuations in alcohol sales would be much the same for metropolitan Melbourne as for the whole of Victoria.)

## 2.4 Additive and Multiplicative Relationships

In producing their effect (Y), different causes (X1, X2, etc.), when acting jointly, could act in either an *additive* or a *multiplicative* fashion. (Y and X might alternatively be described, respectively, as dependent and independent variables.)

Additive:  $Y = a + b \cdot X1 + c \cdot X2$  (\* means "multiply"; a, b & c are constants)

Multiplicative:  $Y = a \cdot X1^b \cdot X2^c$  (superscripts mean "raised to the power")

Simple examples can be provided of additive and multiplicative relationships between independent variables. If the dependent variable was total crashes per year, and two of the independent variables were the numbers of car and motorcycle registrations, it would be expected that the relationship would be additive:

Additive:  $\text{Crashes} = a + b \cdot \text{CarRegos} + c \cdot \text{MotorcycleRegos} + \text{etc.}$

However, if the dependent variable was pedestrian casualties per crossing, and two of the independent variables were vehicle volume and pedestrian volume, it might be expected that the relationship would be multiplicative:

Multiplicative:  $\text{PedCasualties} = a \cdot \text{VehVol}^b \cdot \text{PedVol}^c + \text{etc.}$

Newstead et al's (1995) use of a multiplicative model to investigate crash trends is fairly conventional.

There is a mathematical problem facing multiplicative relationships: linear regression modelling assumes that there is an additive relationship between the predictor variables. The solution is to convert the multiplicative relationship to an additive relationship by taking logs of both sides of the equation prior to modelling.

That is, Multiplicative:  $Y = a \cdot X1^b \cdot X2^c$  by taking logs, becomes:  
Additive:  $\text{Log}(Y) = \text{Log}(a) + b \cdot \text{Log}(X1) + c \cdot \text{Log}(X2)$

## 2.5 The RN74 Regression Models

### LAH Crashes

The multiplicative model used by Newstead et al. (1995, Appendix D) to describe monthly LAH serious casualty crashes was:

$$\text{Serious LAH Crashes} = a * (e^{\text{Trend}})^b * \text{Unemployment Rate}^c * \text{TINs}^d * \text{Adstock}^e * \text{Feb}^f * \dots * \text{Dec}^p$$

The variables used in this equations have been defined previously. By taking logs (to base e; where e = 2.7182818284) of both sides of the equation, this model can be written, additively, as:

$$\log(\text{Serious LAH Crashes}) = \log(a) + b*\text{Trend} + c*\log(\text{Urate}) + d*\log(\text{TINs}) + e*\log(\text{Adstock}) + f*\log(\text{Feb}) + \dots + p*\log(\text{Dec})$$

Two conventional mathematical techniques are used to simplify the form of the log-transformed, additive version of the equation. First, by starting with the complex trend term:  $(e^{\text{Trend}})^b$  in the original multiplicative version, the resulting term in the additive equation becomes:  $b*\text{Trend}$ .

Second, by defining the value of the individual month variables in the multiplicative equation as “e” for the month in question and one (1) in the ten other months, these values are converted in the additive equation to one (1) for the month in question and zero (0) in the ten other months. This 1,0,0,0,0,0,0,0,0,0 coding for the individual month variables is conventional in regression models.

The LAH crash model provides a predicted value for each month’s LAH serious casualty crashes, and a series of regression coefficients (b, c, d, etc.) for the predictor (explanatory) variables. The output of the regression programs also includes tests of whether the coefficients differ significantly (in the statistical sense) from zero.

### HAH Crashes

The multiplicative model used by Newstead et al. (1995, Appendix B) to describe monthly HAH serious casualty crashes was:

$$\text{Serious HAH Crashes} = a * (e^{\text{Trend}})^b * \text{Urate}^c * \text{RBTs}^d * \text{AlcSales}^e * \text{Adstock}^f * \text{Feb}^g * \dots * \text{Dec}^q$$

By taking logs of both sides the model can be written, additively, as:

$$\log(\text{Serious HAH Crashes}) = \log(a) + b*\text{Trend} + c*\log(\text{Urate}) + d*\log(\text{RBTs}) + e*\log(\text{AlcSales}) + f*\log(\text{Adstock}) + g*\log(\text{Feb}) + \dots + q*\log(\text{Dec})$$

## 2.6 MUARC Regression Analyses Selected for Re-Investigation

There were many original MUARC analyses that could possibly have been selected for replication and re-investigation. However, the re-investigation of a single regression analysis involves a considerable amount of effort; and it was therefore necessary to focus on only a very limited number of original analyses. To this point, various reasons have been provided why only two out of a large number of original MUARC analyses were selected for re-investigation. Those, and other, reasons are summarised below.

In their earliest investigations, the MUARC researchers explored the use of a number of different statistical approaches. These included making interstate comparisons; and operationalising the countermeasures as dichotomous variables. However, from about 1992 onwards, multiple linear regression, involving only Victorian data, and using quantitative countermeasure variables, became the favoured technique.

The MUARC researchers conducted a large number of multiple regression analyses of Victorian crash data—only some of which were reported. And of those that were reported, not all were reported in sufficient detail to know whether an exact replication had been achieved.

Some MUARC analyses involved nine years' data; some ten years' data; and some fourteen years' data. The latter analyses did not all provide separate estimates of the effects of enforcement and advertising.

Some MUARC analyses were of crashes occurring in metropolitan Melbourne; others were of crashes in rural Victoria. The Melbourne analyses usually provided stronger evidence for countermeasure effects; and are generally considered more definitive.

Some MUARC analyses were of fatal crashes; some of serious injury crashes; and some of minor injury crashes. The more recent focus of the MUARC work has been on serious injury crashes.

Over time, the operationalisation of some variables has changed. For example the advertising variables are now measured in terms of Adstock rather than TARPs.

It was considered appropriate to restrict the re-investigation to the MUARC research which resulted in fully-reported multiple linear regression analyses of ten years' metropolitan Melbourne serious casualty crashes, where the countermeasure variables were measured at the interval (quantitative) level, and where TV advertising levels were measured in terms of Adstock. The two metropolitan Melbourne crash analyses reported in RN74 fulfilled all those criteria, and have, therefore, become the foci of this report.

It is considered that those two RN74 analyses are representative of the recent MUARC research that has been influential in shaping government policy on road safety TV advertising levels.

### 3 MUARC FINDINGS FOR MELBOURNE

#### 3.1 Results from RN74 for Melbourne

As noted in the previous section, RN74 provided a consistent set of analyses. It is therefore that report which will be the major focus of the re-investigation undertaken here. As the strongest program effects were found for metropolitan Melbourne, it is only the results for that region which will be considered.

The RN74 regression models for LAH and HAH crashes were reproduced in sub-section 2.5 of this report. Regression coefficients for metropolitan Melbourne LAH and HAH serious casualty crashes were given in RN74 Table 2 (p. 12) and Table 1 (p. 9). Those results are reproduced here in Table 3.1.1.

Table 3.1.1  
*Regression coefficients (B) and associated probabilities (p) for the predictor variables in the RN74 models for LAH and HAH serious casualty crashes in metropolitan Melbourne*

Predictor Variable	LAH		HAH	
	B	p	B	p
Trend	0.003	0.000	0.004	0.000
Unemployment	-0.117	0.026	-0.311	0.000
Speed TINs	-0.024	0.000		
Speed Adstock	-0.029	0.000		
Alcohol Sales			0.665	0.000
RBTs			-0.016	0.009
DD Adstock			-0.016	0.072

Omitted from this table are the regression coefficients for the 11 individual months. It should also be noted that the term 'speed Adstock' is used as short-hand for 'speed and concentrate Adstock'.

It is interesting to note that, while the effect of speed advertising on LAH crashes was statistically significant ( $p = 0.000$ ), the effect of drink drive advertising on HAH crashes was not significant at the conventional 5% level ( $p = 0.072$ ).

Working from these regression coefficients, Newstead et al. (1995) estimated the savings in crash numbers between 1988 and 1992. The procedures involved are described in RN74 *Section 4: Combining the Results of the Models*. The results of those procedures for metropolitan Melbourne are presented in RN74 Appendix E and are reproduced here in Table 3.1.2. (The values for the 1988 Observed crash numbers were not in Appendix E, and have been added here.) The details of how Newstead et al. derived the estimated crash savings from the regression coefficients are not discussed here; only the *outcomes* of those derivations are discussed.

Table 3.1.2  
*Percent reductions in LAH and HAH Serious Casualty Crashes in metropolitan Melbourne between 1988 and 1992*

	<i>LAH Crashes</i>	<i>HAH Crashes</i>	<i>All Crashes</i>
<b>Crash Numbers</b>			
1988 Observed	3016	2852	5868
1992 Observed	1858	1533	3391
1992 Expected	3360	3335	6694
<b>Total % Reduction</b>	44.7%	54.0%	49.3%
<b>Component % Reductions</b>			
Unemployment	8.9%	22.1%	15.5%
Alcohol Sales		20.4%	10.2%
Speed TINS	22.8%		11.4%
Speed Adstock	21.4%		10.7%
RBTs		16.5%	8.2%
Drink Drive Adstock		11.2%	5.6%
<b>Countermeasures</b>	39.3%	25.8%	31.5%

The Total % Reduction values in Table 3.1.2 were obtained by subtracting the 1992 Observed totals from the 1992 Expected totals.

A feature of this table is that the addition of the *Component % Reduction* values produces values that are greater than the corresponding *Total % Reduction* values. For example, the Component % Reductions for LAH crashes (8.9%, 22.8% and 21.4%) add to 53.1%, which is greater than the Total % Reduction value of 44.7%. This method of presentation is related to the fact that the underlying regression models are multiplicative. In the words of Newstead et al. (1995, p. 17):

It should be noted that these percentages cannot simply be added up to estimate the total contribution. ... The proportion of crashes remaining after each contributor needs to be multiplied by the proportions for other contributors to estimate the proportion remaining when all contributors are considered, and thus estimate the overall percentage reduction.

The Total % Reduction in LAH crashes is therefore given by:

$$100 - [(0.911 * 0.772 * 0.786) * 100] = 44.7\%$$

And the Total % Reduction in HAH crashes is given by:

$$100 - [(0.779 * 0.796 * 0.835 * 0.888) * 100] = 54.0\%$$

### 3.2 Results from Cameron et al. (1993) for Melbourne

Of the three MUARC reports that investigated the separate effects of advertising, only one, Cameron et al. (1993), made recommendations for advertising levels based on benefit/cost analyses. It should be noted that Cameron et al. is not the latest of the three reports; nor is it the main focus of the re-investigations reported here. However, the following results could be obtained only from Cameron et al.

It was found that, for the (nine year) period 1983 to 1991, the regression coefficient for the speed advertising variable (in this case, Speed TARPs) was - 0.0152 (SE = 0.0070). Furthermore, when the other explanatory variables were held at their average levels, the form of the relationship between LAH casualty crashes (in this case, *all* casualty crashes) and speed TARPs was:

$$\text{All LAH Casualty Crashes} = 702.7 * (\text{Speed TARPs})^{-0.0152}$$

The graph of this relationship (Figure 3 from Cameron et al., 1993, p. 9) is reproduced below.

It was concluded from this graph (given the known costs of advertising and crashes) that the economically optimal level of speed TARPs per month was between 540 and 1080. No attempt will be made in this report to revisit this benefit/cost analysis. However, some of the assumptions underlying it will be questioned later.

Cameron et al. (1993) also found that, for the (ten year) period 1983 to 1992, the regression coefficient for drink drive Adstock was - 0.0169 (SE = 0.0044); and that when other explanatory variables were held at their average levels, the form of the relationship between HAH casualty crashes (again, *all* casualty crashes) and drink drive Adstock was:

$$\text{All HAH Casualty Crashes} = 470.9 * (\text{Drink Drive Adstock})^{-0.0169}$$

The graph of this relationship (Figure 5 from Cameron et al., 1993, p. 24) is reproduced below.

In their benefit/cost analysis Cameron et al. (1993) concluded that the optimal level of drink drive TARPs per month (converted back from Adstock) was between 800 and 2000. Again, no attempt will be made in this report to scrutinise this benefit/cost analysis. However, as noted above for the LAH analysis, some of the assumptions underlying the analysis will be questioned.

These findings of Cameron et al. (1993) have been very influential. As indicated by the following quote from a MUARC Annual Report (Vulcan, 1998, p. 2), the dose-response relationships have provided the rationale for the recommendation of very high levels of TAC road safety TV advertising in Victoria and elsewhere:

In response to a request by the new TAC Chairman in 1993, time-series analyses were used to estimate the effect of the advertisements alone. It was found that the intensity (audience reach) of those advertisements which supported 'booze bus' and speed camera enforcement were strongly correlated to the relevant

monthly casualties. The resulting relationship between advertising levels and reduction in casualty crashes has been used as a guide for determining the level of advertising placements.

#### Dose-Response Relationship Between Advertising Levels and Crash Numbers

Cameron et al. (1993) have assumed that the 'advertising effects' are quantitative. In other words, they have *effectively* treated the relationship between the advertising and crash variables as a *dose-response relationship* (although they do not use that terminology themselves). The 'quantitative assumption' is challenged in Section 10 of this report. However, given that the re-analyses reported here are on the data analysed by Newstead et al. (1995), the reported results for the examination of the quantitative assumption will be based on *that* data, rather than on the data analysed by Cameron et al. It seemed appropriate to test the quantitative assumption against the more recent set of data. It is conceivable, but unlikely, that the different sets of data could result in different conclusions concerning the validity of the quantitative assumption. That possibility is checked in Section 10.

Figure 3 from Cameron et al. (1993)

FIGURE 3  
REDUCTION IN CASUALTY CRASHES v.  
T.A.C. SPEEDING AND CONCENTRATION PUBLICITY (TARPs)  
Melbourne roads during low alcohol hours (all other effects held constant)

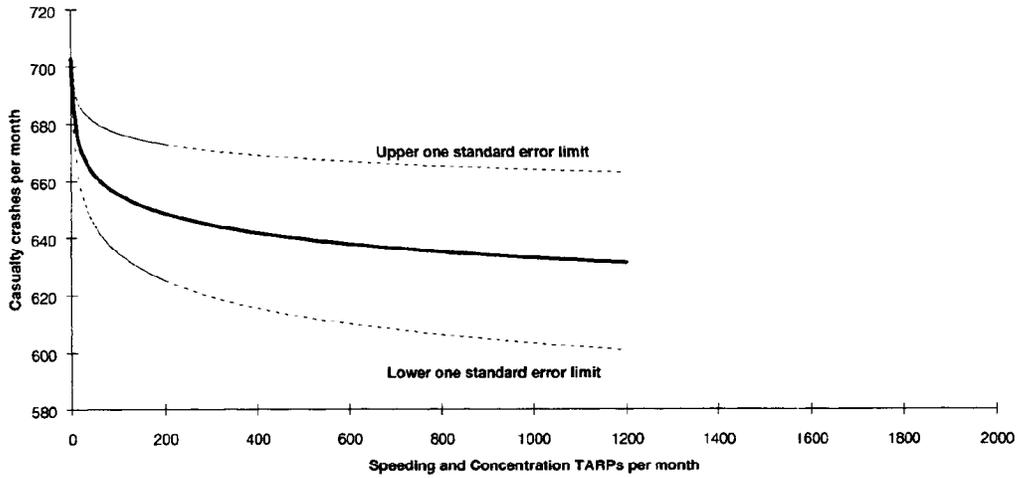
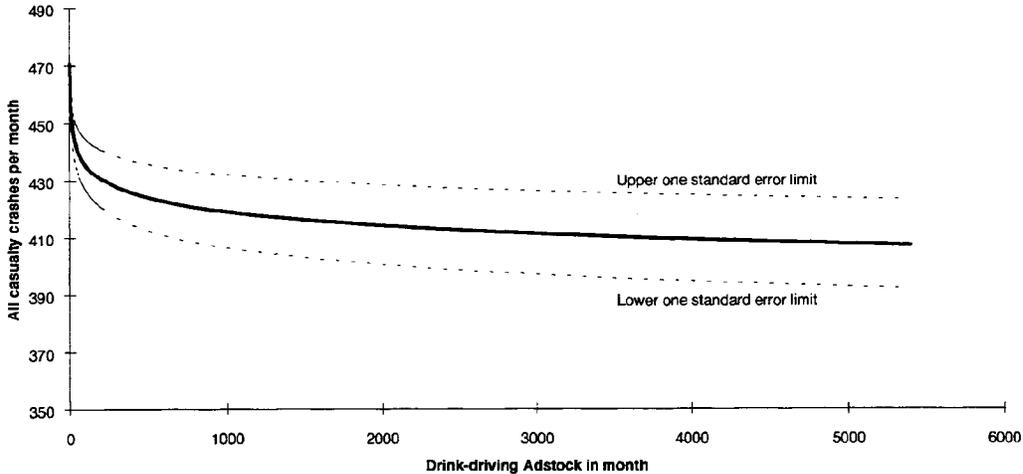


Figure 5 from Cameron et al. (1993)

FIGURE 5  
REDUCTION IN ALL CASUALTY CRASHES v.  
T.A.C. DRINK-DRIVING ADSTOCK  
Melbourne roads during high alcohol hours (all other effects held constant)



## 4 LOW ALCOHOL HOUR (LAH) CRASHES IN MELBOURNE

In this section, RN74 data on monthly Melbourne LAH serious casualty crashes for the ten-year period from 1983 to 1992 will be re-analysed.

The following variables are used:

- LAH serious casualty crashes (metropolitan Melbourne)
- Trend (1 - 120; monthly sequence numbers for ten years)
- Individual months (February, March, etc.)
- Metropolitan Melbourne unemployment rate
- Speed camera TINs (whole of Victoria)
- Speed & concentrate Adstock (whole of Victoria)

### 4.1 LAH Univariate Descriptive Statistics

Figures 4.1.1 to 4.1.4 show how the dependent and independent variables are distributed over the ten-year period. The X-axis in these sequence plots is 'Trend' or 'Month', where month 1 is January 1983 and month 120 is December 1992. All graphs depict raw, rather than log-transformed, data.

It can be seen from Figure 4.1.1 that LAH crashes increase gradually, but irregularly, from 1983 to 1988, when they start to decline. The decline is fairly sharp and levels off from 1990 onwards. Some seasonal (monthly) fluctuations are also detectable.

Figure 4.1.2 shows how Melbourne unemployment rates have changed over the same period. It is interesting to note that this time-series distribution is almost the inverse of that for LAH crashes, with unemployment rates decreasing steadily until 1988-1989, after which there is a sudden, strong turn-around, which then levels off somewhat in 1991 and 1992. Strong seasonal fluctuations are evident.

Figure 4.1.3 presents the results for speed camera TINs. The trend seen here reflects the facts that speed cameras were gradually introduced at very low levels from the end of 1988, but that the program was not fully operational until August-September 1990.

Figure 4.1.4 presents the results for speed & concentrate Adstock. As for the enforcement variable, this variable consists mostly of zero values. The new TAC speed advertising commenced in April 1990, and there were fluctuating levels per month from then on.

It should be noted that the actual variables used in the RN74 regression analyses were the log transformations of the variables described above.

Figure 4.1.1

LAH Serious Casualty Crashes per Month

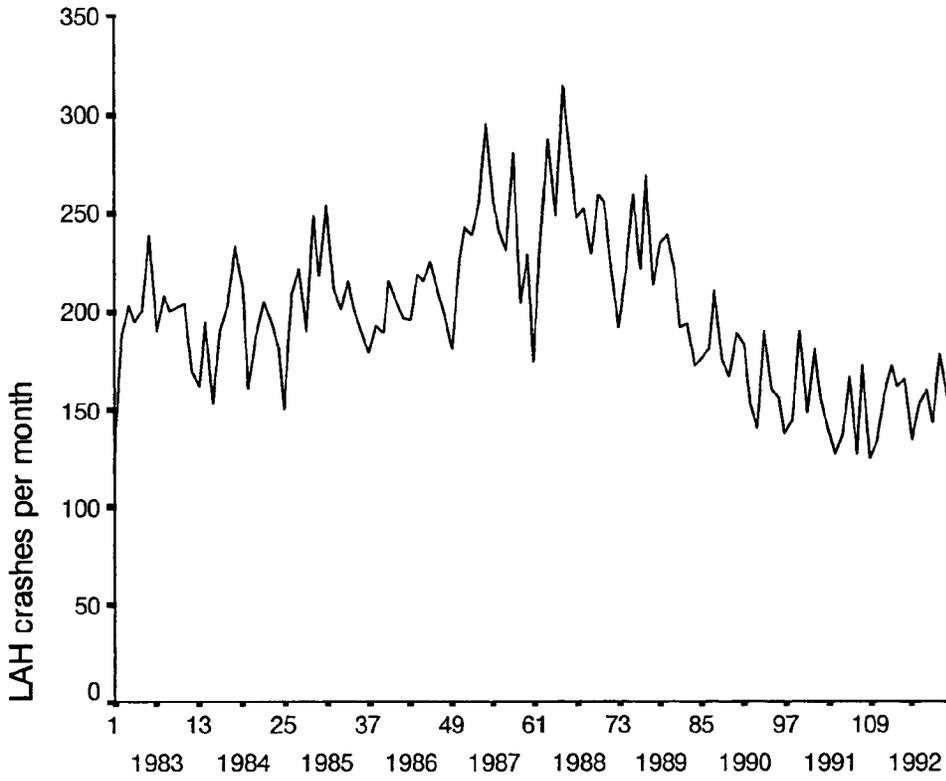


Figure 4.1.2

Melbourne Unemployment Percent per Month

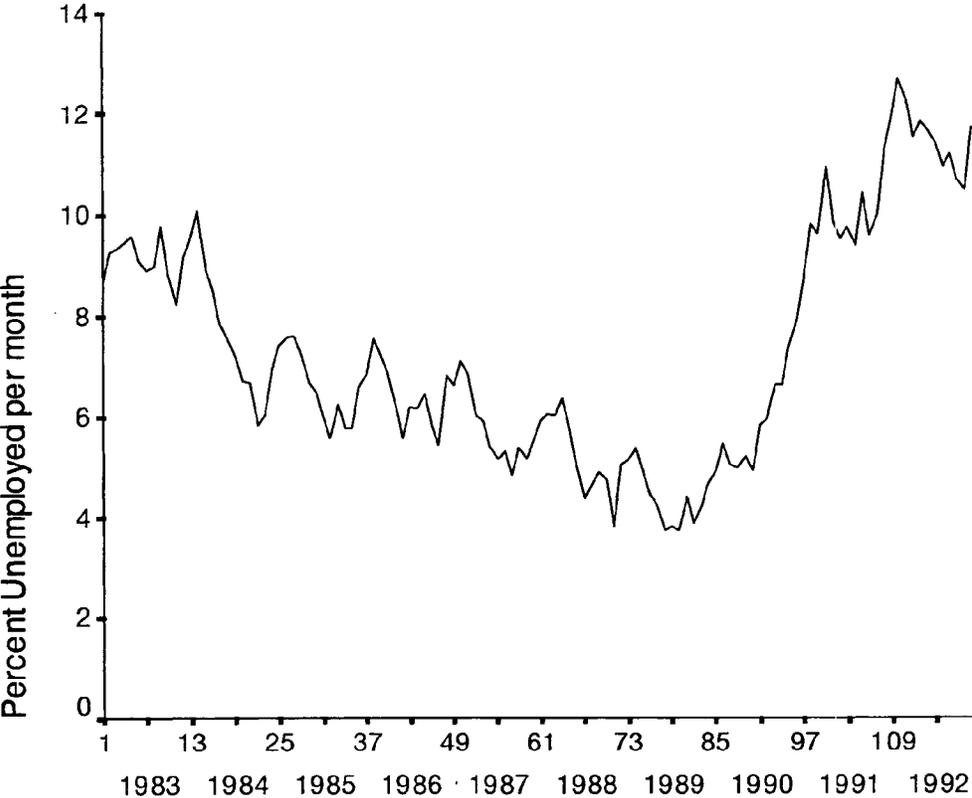


Figure 4.1.3

Speed Camera Traffic Infringement Notices per Month

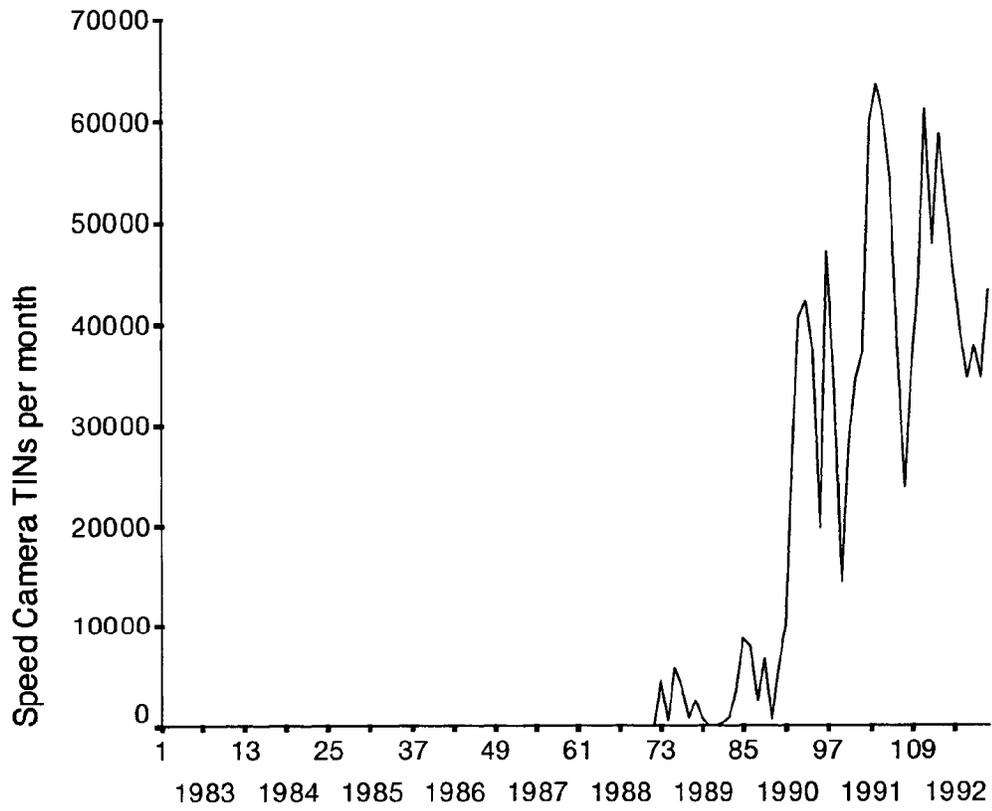
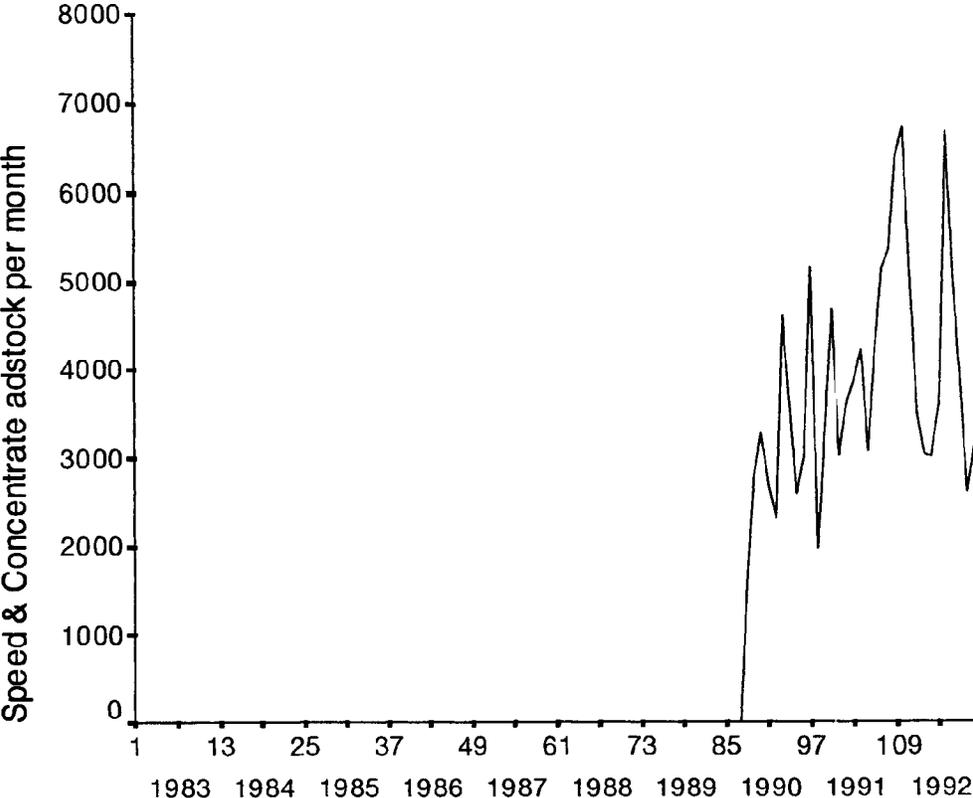


Figure 4.1.4

Speed and Concentrate Adstock per Month



## 4.2 LAH Bivariate Correlation Matrices

Table 4.2.1(a) presents a bivariate correlation matrix (r values and 2-tailed p values) for the raw-data versions of the following variables: LAH serious casualty crashes, trend, unemployment, speed camera TINs, and speed & concentration Adstock. Table 4.2.1(b) presents the same statistics for the log-transformed versions of these variables.

*Table 4.2.1(a)*  
*LAH correlation matrix (r and (p) values) for the raw variables (n = 120)*

	<i>Crashes</i>	<i>Trend</i>	<i>Unemp</i>	<i>TINs</i>	<i>Adstock</i>
<i>Crashes</i>	1.000	-.322 (.000)	-.627 (.000)	-.632 (.000)	-.643 (.000)
<i>Trend</i>	-.322 (.000)	1.000	.194 (.033)	.740 (.000)	.741 (.000)
<i>Unemp</i>	-.627 (.000)	.194 (.033)	1.000	.667 (.000)	.654 (.000)
<i>TINs</i>	-.632 (.000)	.740 (.000)	.667 (.000)	1.000	.871 (.000)
<i>Adstock</i>	-.643 (.000)	.741 (.000)	.654 (.000)	.871 (.000)	1.000

*Table 4.2.1(b)*  
*Correlation matrix (r and (p) values) for the log transformed variables used in the regression model for LAH serious casualty crashes (n = 120)*

	<i>Crashes</i>	<i>Trend</i>	<i>Unemp</i>	<i>TINs</i>	<i>Adstock</i>
<i>Crashes</i>	1.000	-.363 (.000)	-.631 (.000)	-.615 (.000)	-.684 (.000)
<i>Trend</i>	-.363 (.000)	1.000	.105 (.253)	.857 (.000)	.775 (.000)
<i>Unemp</i>	-.631 (.000)	.105 (.253)	1.000	.346 (.000)	.566 (.000)
<i>TINs</i>	-.615 (.000)	.857 (.000)	.346 (.000)	1.000	.865 (.000)
<i>Adstock</i>	-.684 (.000)	.775 (.000)	.566 (.000)	.865 (.000)	1.000

The RN74 multiple linear regression model is based on the relationships between the log-transformed versions of the variables (Table 4.2.1(b)), as well as on relationships which involve the individual month variables.

#### 4.3 Replication of the RN74 LAH Regression Analysis

The following variables were used in the RN74 regression model:

- Log LAH serious casualty crashes (metropolitan Melbourne)
- Trend (1 - 120; monthly sequence numbers for ten years)
- Individual months (February, March, etc.)
- Log metropolitan Melbourne unemployment rate
- Log speed camera TINs (whole of Victoria)
- Log speed & concentrate adstock (whole of Victoria)

The term 'log' will not always be explicitly mentioned when referring to variables in the regression models. Unless otherwise stated it can be assumed that log-transformed variables are being used.

The linear regression equation for LAH serious casualty crashes was presented in sub-section 2.5.

A perfect replication was achieved of the multiple linear regression analysis reported in RN74 (p. 12, Table 2). The regression coefficients for the major variables were presented here in Table 3.1.1 (left), and are reproduced below in Table 4.3.1. (Results for the eleven individual month variables are not given here.) The total proportion of the LAH crash variance accounted for by the full set of independent variables, as measured by R Square, was not very high (R Square = 0.777).

Table 4.3.1  
*Regression coefficients (B), their SEs, and associated probabilities (p) for the predictor variables in the RN74 model for LAH serious casualty crashes in metropolitan Melbourne*

<i>Predictor Variable</i>	<i>B</i>	<i>SE</i>	<i>p</i>
Trend	0.003	0.001	0.000
Log Unemployment	-0.117	0.052	0.026
Log Speed TINs	-0.024	0.005	0.000
Log Speed Adstock	-0.029	0.008	0.000



## 5 HIGH ALCOHOL HOUR (HAH) CRASHES IN MELBOURNE

In this section, RN74 data on monthly Melbourne HAH serious casualty crashes for the ten-year period from 1983 to 1992 will be re-analysed. These analyses for HAH crashes parallel those reported in Section 4 for LAH crashes.

The following variables are used:

- Drink-drive related serious casualty crashes (metropolitan Melbourne)
- Trend (1 - 120; monthly sequence numbers for ten years)
- Individual months (February, March, etc.)
- Metropolitan Melbourne unemployment rate
- Value (\$) of alcohol sales (whole of Victoria)
- RBTs (Melbourne)
- Drink drive Adstock (whole of Victoria)

### 5.1 HAH Univariate Descriptive Statistics

Figures 5.1.1 to 5.1.4 show how the dependent and independent variables are distributed over the ten-year period. All graphs depict raw, rather than log-transformed, data. The unemployment trend was graphed in Figure 4.1.2 and is not reproduced here.

It can be seen from Figure 5.1.1 that HAH crashes increase gradually, but irregularly, from 1983 to 1987, when they start to decline. The decline is gradual at first, but fairly sharp from mid 1989 to early 1991. There is little overall change in crash numbers during 1991 and 1992. Some seasonal (monthly) fluctuations may be visible.

The HAH crash data provided by MUARC contained a single, seriously erroneous value: the number of serious casualty crashes provided for February 1988 was 324, which was 100 higher than the actual number (224). The analyses reported in RN74 used the *incorrect* value. The re-analyses reported here used the *correct* value, and so will not perfectly replicate the findings in RN74. However, because the data sets are identical in all other respects, the differences are small and fairly inconsequential.

Figure 4.1.2 (see Section 4) showed how Melbourne unemployment rates changed over the ten-year period. It is interesting to note that the unemployment distribution is almost the inverse of that for HAH crashes.

Figure 5.1.2 presents the results for alcohol sales. It can be seen that there is a long-term downward trend with strong seasonal fluctuations.

Figure 5.1.1

HAH Serious Casualty Crashes per Month

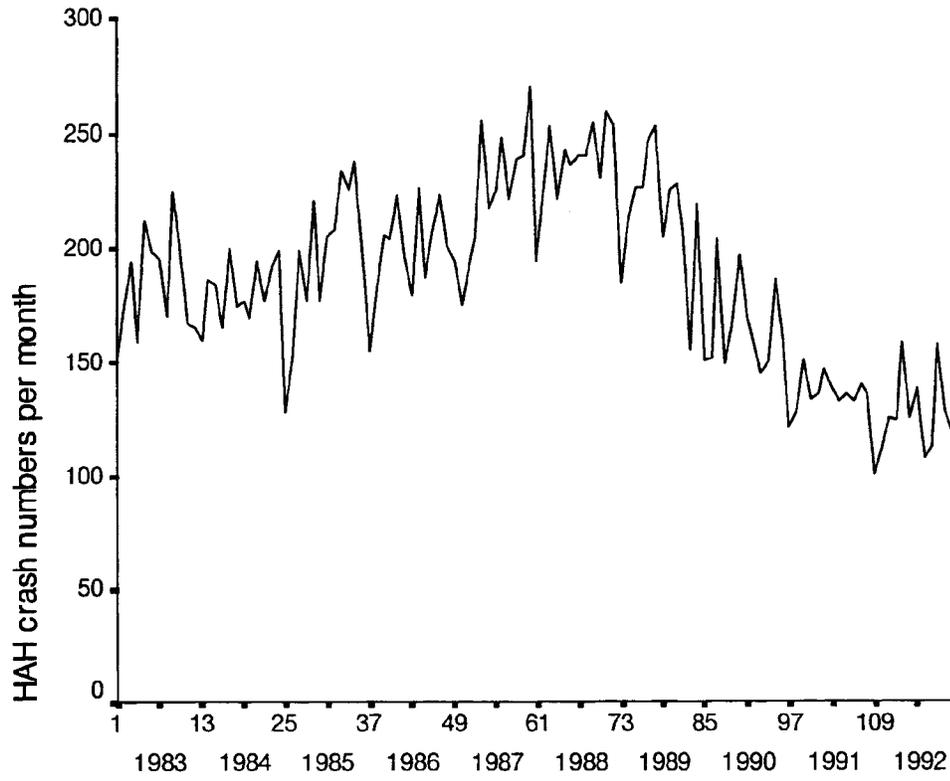


Figure 5.1.2 'Alcohol Sales' per Month

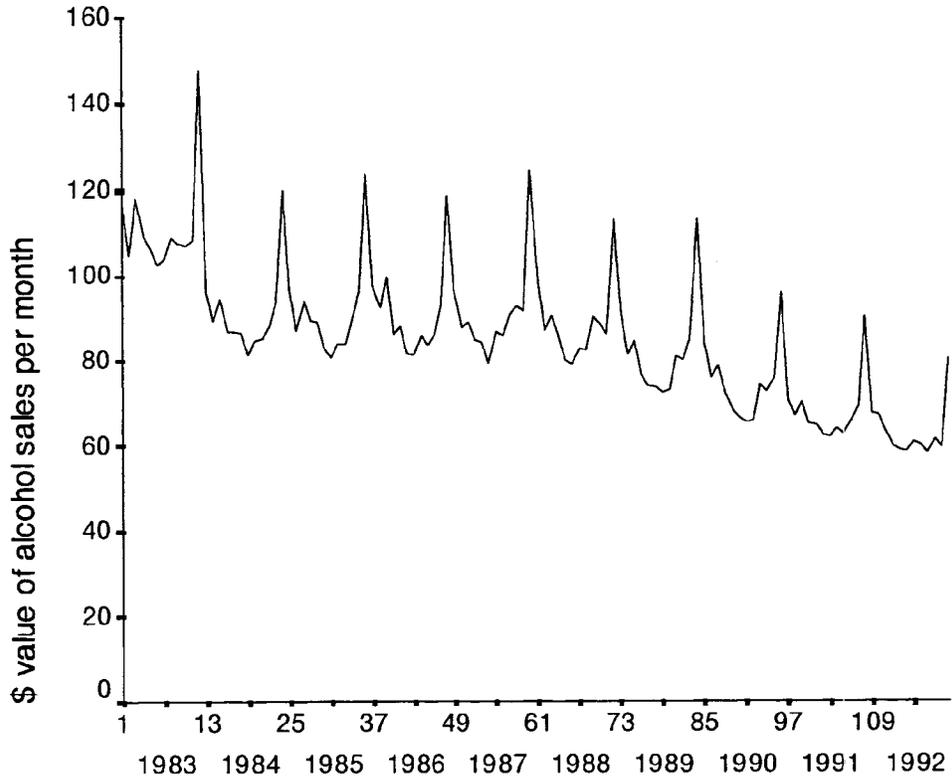


Figure 5.1.3

Bus-Based Random Breath Tests per Month

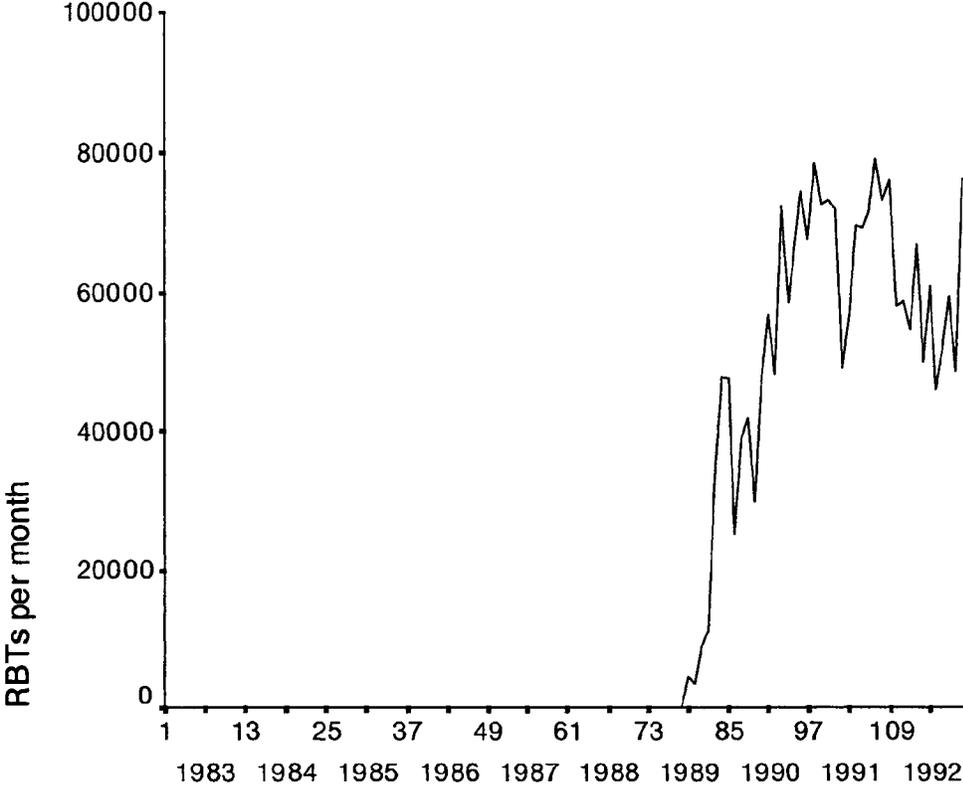


Figure 5.1.4

Drink Drive Adstock per Month

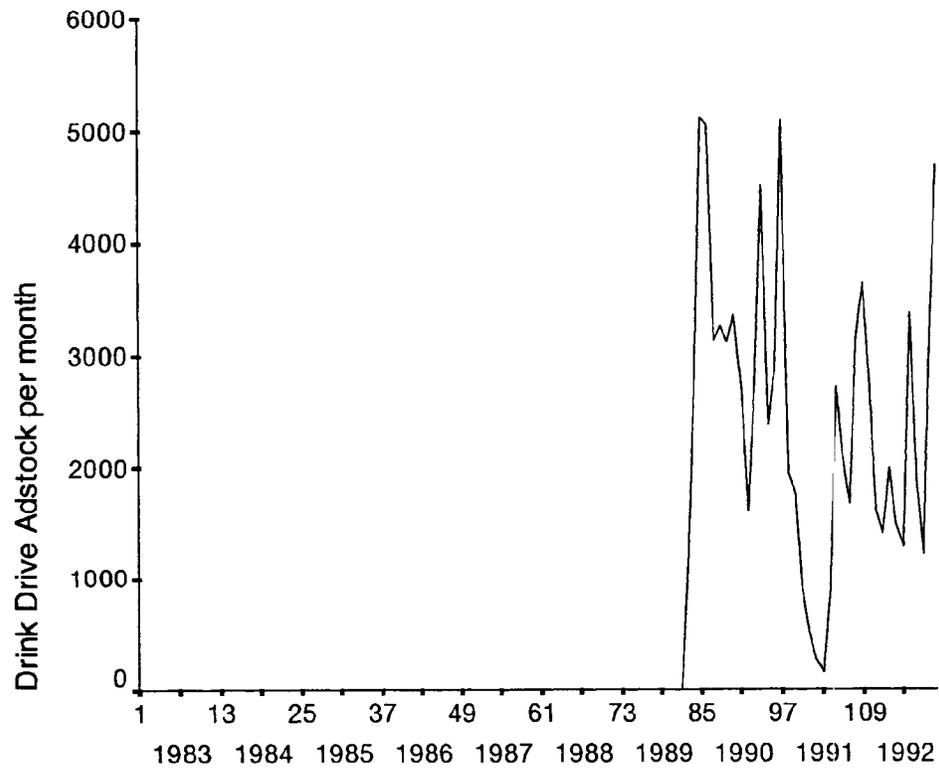


Figure 5.1.3 presents the trend for RBTs. These results are explained by Newstead et al. (1995, p. 3):

The program of increased RBT using highly visible 'booze buses' commenced in September 1989. ... Bus-based RBT showed a considerable increase using the four existing buses in late 1989. Further dramatic increases in bus-based testing were seen during 1990 by the progressive introduction of thirteen new purpose-built, highly-visible booze buses provided by TAC to the Victoria Police.

Although the RBT program may have officially commenced in September 1989 (month 81), Figure 5.1.3 shows that there was some low level of activity during the previous two months (months 79 and 80).

Figure 5.1.4 presents the results for drink drive Adstock. As for the enforcement variable, this variable consists mostly of zero values. The first non-zero value is for November 1989 (month 83). (Some issues concerning the starting date are discussed in sub-sections 7.4 and 9.2 of this report.)

It should be noted that the actual variables used in the RN74 regression analyses were the log transformations of the variables described above.

## 5.2 *HAH Bivariate Correlation Matrices*

Table 5.2.1(a) presents a bivariate correlation matrix (r values and 2-tailed p values) for the raw-data versions of the following variables: drink-drive related serious casualty crashes, trend, unemployment, alcohol sales, RBTs, and drink drive Adstock. Table 5.2.1(b) presents the same statistics for the log-transformed versions of these variables.

The RN74 multiple linear regression model was based on the relationships between the log-transformed versions of the variables (Table 5.2.1(b)), as well as on relationships which involve the individual month variables.

## 5.3 *Collinearity Between the Enforcement and Advertising Variables*

Problems of 'collinearity' can arise when predictor variables are highly correlated (see, for example, Studenmund, pp. 264-268). Collinearity is a complex issue. A well-specified regression model can legitimately include variables that happen to be highly correlated; and the presence of high correlations between predictor variables does not invalidate a regression analysis. However, when high correlations between predictor variables are present, the most commonly used model selection procedures are likely to produce misleading results; and careful analysis is required to determine the extent to which the separate effects of the predictor variables can be identified.

One question that must be asked of the RN74 analyses of Melbourne crash data, is whether the advertising and enforcement variables were so highly correlated as to cast doubt on the straightforward interpretation of the regression coefficients reported.

*Table 5.2.1(a)*  
*HAH correlation matrix (r and (p) values) for the raw variables (n = 120)*

	<i>Crashes</i>	<i>Trend</i>	<i>Unemp.</i>	<i>Alc. Sale</i>	<i>RBTs</i>	<i>Adstock</i>
<i>Crashes</i>	1.000	-.379 (.000)	-.738 (.000)	.408 (.000)	-.707 (.000)	-.583 (.000)
<i>Trend</i>	-.379 (.000)	1.000	.194 (.033)	-.738 (.000)	.802 (.000)	.654 (.000)
<i>Unemp.</i>	-.738 (.000)	.194 (.033)	1.000	-.203 (.027)	.554 (.000)	.293 (.001)
<i>Alc. Sales</i>	.408 (.000)	-.738 (.000)	-.203 (.027)	1.000	-.616 (.000)	-.451 (.000)
<i>RBTs</i>	-.707 (.000)	.802 (.000)	.554 (.000)	-.616 (.000)	1.000	.781 (.000)
<i>DD Adstock</i>	-.583 (.000)	.654 (.000)	.293 (.001)	-.451 (.000)	.781 (.000)	1.000

*Table 5.2.1(b)*  
*Correlation matrix (r and (p) values) for the log transformed variables used in the RN74 regression model for HAH serious casualty crashes (n = 120)*

	<i>Crashes</i>	<i>Trend</i>	<i>Unemp.</i>	<i>Alc. Sale</i>	<i>RBTs</i>	<i>Adstock</i>
<i>Crashes</i>	1.000	-.434 (.000)	-.731 (.000)	.492	-.681 (.000)	-.718 (.000)
<i>Trend</i>	-.434 (.000)	1.000	.105 (.253)	-.773 (.000)	.834 (.000)	.796 (.000)
<i>Unemp.</i>	-.731 (.000)	.105 (.253)	1.000	-.201 (.028)	.338 (.000)	.408 (.000)
<i>Alc. Sales</i>	.492 (.000)	-.773 (.000)	-.201 (.028)	1.000	-.704 (.000)	-.658 (.000)
<i>RBTs</i>	-.681 (.000)	.834 (.000)	.338 (.000)	-.704 (.000)	1.000	.945 (.000)
<i>DD Adstock</i>	-.718 (.000)	.796 (.000)	.408 (.000)	-.658 (.000)	.945 (.000)	1.000

From Table 4.2.1(b) it can be seen that the correlation between the log-transformed versions of the speed Adstock and TINs variables is 0.865. From Table 5.2.1(b) it can be seen that the correlation between the log-transformed versions of the drink drive Adstock and RBT variables was 0.945. Those correlations are at problematic levels.

When considering potential problems of collinearity, Cameron et al. (1993, p. 30) simply *assumed* that:

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 used in the economic analyses.

Newstead et al. (1995, p. 31) acknowledged that there might have been problematic levels of correlation between their advertising and enforcement variables. But, that did not prevent them from providing separate estimates of crash savings attributable to advertising and enforcement.

Narayan, Newstead and Cameron (1998) used much the same approach as Newstead et al. (1995), but incorporated an extra four years' data (from 1993 to 1996) into their regression analyses of metropolitan Melbourne serious casualty crashes. They acknowledged that there was a problem of collinearity for the drink drive countermeasure variables (p. 185):

...estimates of the effect of RBT and drink driving publicity are not given separately because of the high degree of collinearity between these factors in the estimated models. Instead, the effects of these two factors have been combined into a single measure labelled as 'the drink-driving program'.

The issue of collinearity is a complex one. It will not be discussed further in this report. One reason is that any potential problems of collinearity are overshadowed by more serious problems of model specification and model status that are examined in some detail later in this report.

#### 5.4 Replication of the RN74 HAH Regression Analysis

The following variables were used in the RN74 HAH regression model:

- Log drink-drive related serious casualty crashes (metropolitan Melbourne)
- Trend (1 - 120; monthly sequence numbers for ten years)
- Individual months (February, March, etc.)
- Log metropolitan Melbourne unemployment rate
- Log value(\$) of alcohol sales (whole of Victoria)
- Log RBTs (whole of Victoria)
- Log drink drive Adstock (whole of Victoria)

The term 'log' will not always be explicitly mentioned when referring to the variables in the regression models. Unless otherwise stated it can be assumed that log-transformed variables are being used.

The linear regression equation for drink-drive related serious casualty crashes was presented in sub-section 2.5.

A perfect replication was achieved of the multiple linear regression analysis reported in RN74 (p. 9, Table 1), when using the HAH crash variable with the single erroneous value. The regression coefficients from that analysis, for the major variables, were presented here in Table 3.1.1.(right).

Table 5.3.1

*Regression coefficients (B), their SEs and associated probabilities (p) for the predictor variables in the RN74 model for HAH serious casualty crashes in metropolitan Melbourne*

<i>Predictor Variable</i>	<i>B</i>	<i>SE</i>	<i>p</i>
Trend	0.003	0.001	0.000
Log Unemployment	-0.312	0.034	0.000
Log Alcohol Sales	0.628	0.150	0.000
Log RBTs	-0.016	0.006	0.006
Log DD Adstock	-0.015	0.008	0.057

Corresponding regression coefficients from the analysis using the *corrected* crash data are reproduced in Table 5.3.1. (Results for the eleven individual month variables are not reported here.) The total proportion of the drink drive crash variance accounted for by the full set of independent variables, as measured by R Square, was quite high (R Square = 0.866).



## 6 COUNTERMEASURES AND CRASHES: TEMPORAL RELATIONSHIPS

### 6.1 *Some Misconceptions About the Timing of the Fall in Crash Numbers*

Reports from some Victorian and overseas road safety agencies give the clear impression that the agencies believed that the TAC-funded enforcement and advertising campaigns were largely, if not entirely, responsible for *halting the increase* in crash numbers and for *initiating their decline*. Examples of such views are provided below.

A recent (December, 1999) item on 'Road Safety Initiatives' from the TAC internet web site (<http://www.tac.vic.gov.au>), makes the claim that:

By December 1989, the rising road toll and cost of accidents sparked concern for both the number of lives being lost and the number of people being seriously injured. Since this time, the TAC, in conjunction with Victoria's other key road safety agencies, has developed a highly successful accident prevention program. ... The *result of this integrated approach* has been a near halving of Victoria's road toll since 1989, with a corresponding drop in serious injuries of about 35% (1996). [*Italics added.*]

According to the police officer in charge of the Victorian speed camera program (Bodinnar, 1994, p. 206), the TAC-funded campaigns were responsible for *turning around* the worsening crash situation in Victoria:

In 1989, 777 Victorians died and over 6 000 people suffered major injuries through road trauma, with the rate growing by an alarming 5 deaths per month. Something different had to be done to stop the carnage ... Professional strategic partnerships were formed (involving the TAC, the Victoria Police and VicRoads) and a dynamic plan developed and implemented that has saved over 1 200 lives and prevented over 8 000 major injuries, *reversing the upward trend* ... A remarkable 46% reduction in road crash death and injury in five years has proven the effectiveness of the Victorian model. [*Italics added.*]

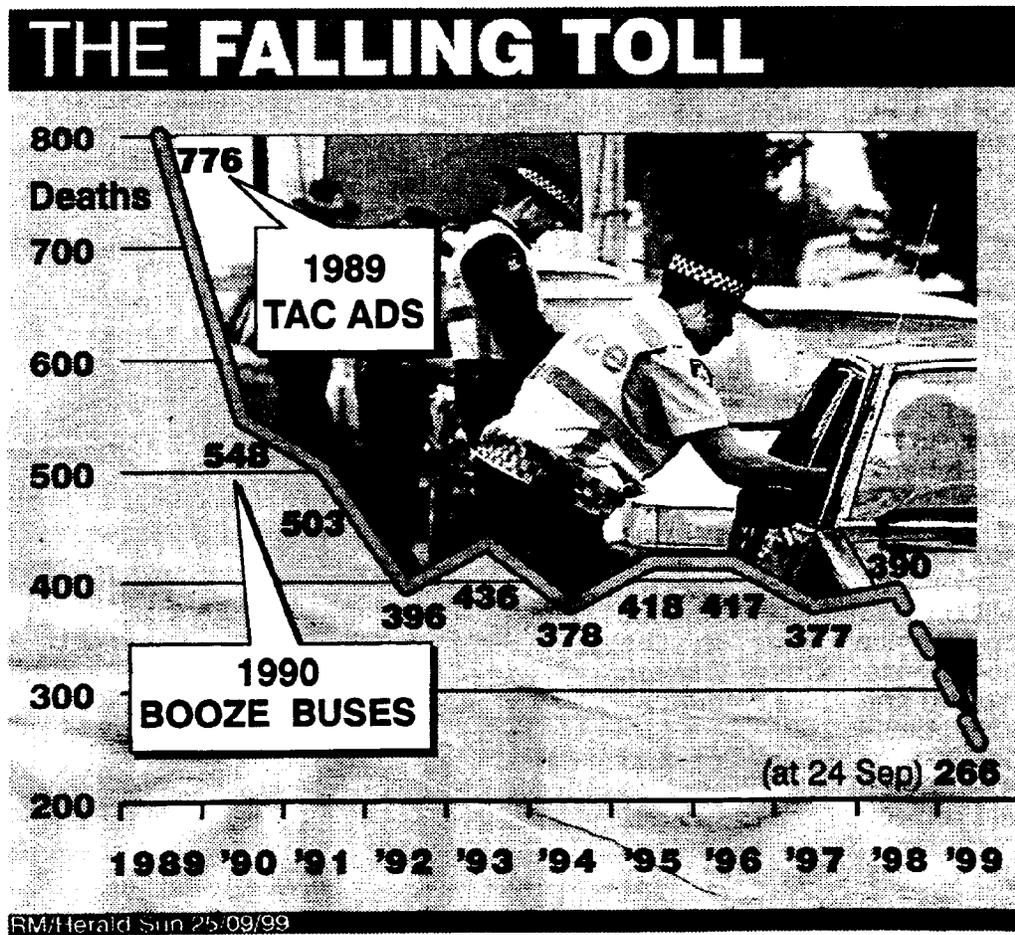
In a similar vein, VicRoads staff (Ungers & Vincent, 1995, p. 13) refer to the notion of 'managing' the road toll:

The trends in road crash figures, and the countermeasures and evaluations described in this paper, demonstrate that ... the extent of road trauma can be *managed* to a level that represents world best performance. [*Italics added.*]

Chief Inspector Mallaby, a representative of the Lancashire Police, is reported by Young (1999, p. 14) as saying that:

Victoria *reduced* the number of crashes by 47% by altering behaviour and attitudes with hard-hitting TV advertising and very robust enforcement. [*Italics added.*]

# The Role of TAC Television Advertising in Reducing the Road Toll



(Melbourne Herald Sun, Saturday 25 September, 1999, p. 20)

MUARC consultants to the Western Australian Government (Cameron et al., 1997) described the effects of the TAC-funded campaigns as follows:

The fatality rate in Victoria had shown little reduction from 1980 to 1988, and in September, 1989 it appeared that if the trend in road deaths for the previous nine months continued, the toll for that year would exceed 800 for the first time in 10 years. A series of measures were introduced commencing at the end of September which saw the road toll peak at 770 deaths in 1989 and drop progressively to 369 in 1992. ...There were similar reductions, though not quite as large, in serious injuries and other injuries. (*Italics added.*)

The belief in the effectiveness of TAC advertising is well illustrated in the 'Falling Toll' figure from the Herald Sun which is reproduced on the previous page.

Causes always precede their effects. Any evidence that crash numbers had started to decline *before* the TAC-funded enforcement or advertising campaigns had been launched would argue against the widespread view that the campaigns had been responsible for halting the increase in crash numbers and initiating the decline.

## 6.2 Speed Countermeasures and LAH Crashes

Some information relevant to the relative timing of the implementation of the speed countermeasures and the turn-around in LAH crash numbers was implicit in Figures 4.1.1, 4.1.3 and 4.1.4. That information is made explicit in Figures 6.2.1 and 6.2.2 by plotting the relevant time-series data in the *same* graphs. Three procedures were used to clarify the visual presentations. First, the LAH crash data was de-seasonalised to reduce its variability (by using the LAH crash residuals which remained after the individual months had been regressed against the original LAH crash variable). Second, the values of the crash, enforcement and advertising variables were converted to the same scale by the conventional standardisation procedure of converting to Z scores. Third, as the first three years of data (1983-1985) were un-informative for this purpose, they were 'filtered out' before the graphs were constructed.

Figure 6.2.1 is the resulting sequence plot for LAH serious casualty crashes and speed camera TINs. Crash numbers peaked at 314 in May 1988 (month 65), and then fell steadily for two years, reaching a low value of 167 in May 1990 (month 89). Crash numbers then remained at low, but fluctuating, levels for the next two and a half years, with some months dipping below the low of May 1990.

Although there had been sporadic and very low levels of speed camera enforcement earlier, Cameron, Cavallo and Gilbert (1992) considered that the speed enforcement campaign did not properly commence until July 1990 (month 91) when the automated TIN processing system became operational. Prior to that time, the camera operations were described (p. ii) as "the trialing of four speed cameras at a small number of sites in Melbourne" from December 1989 to March 1990; and in terms of there being "little speed camera enforcement ( ... after an intense nine day burst)" between April and June 1990. So, the major increase in speed enforcement levels did not occur until at least two years after crash numbers had started to decline; and not until after crashes had reached the first low point of May 1990.

Figure 6.2.1 Coincidence Plot for LAH Crashes and Speed TINs

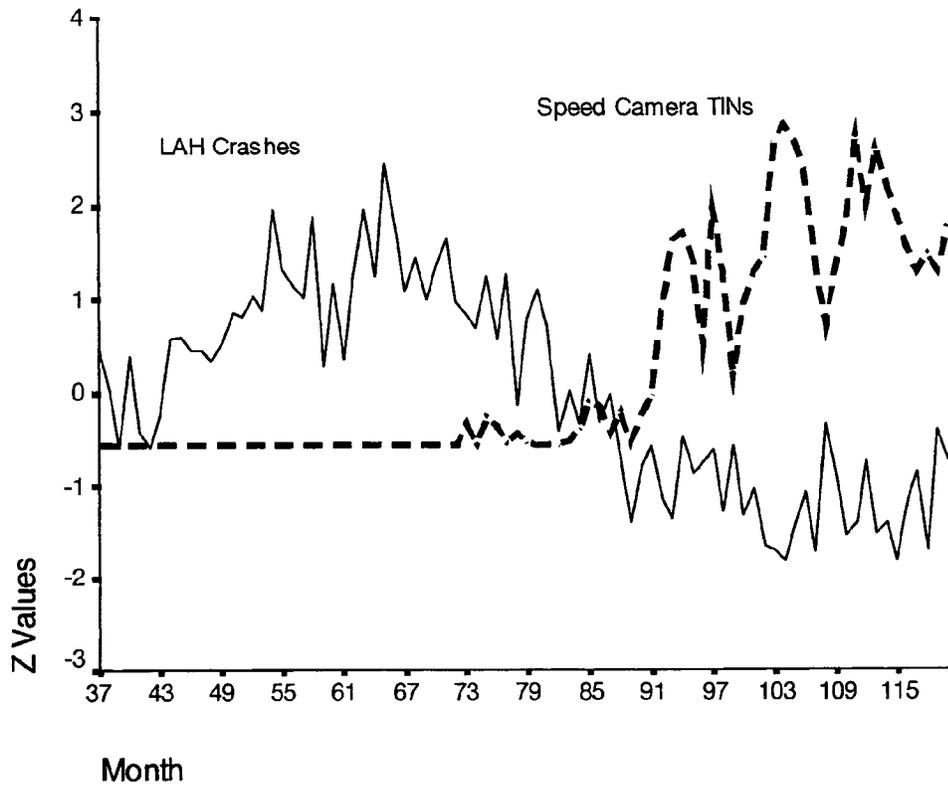
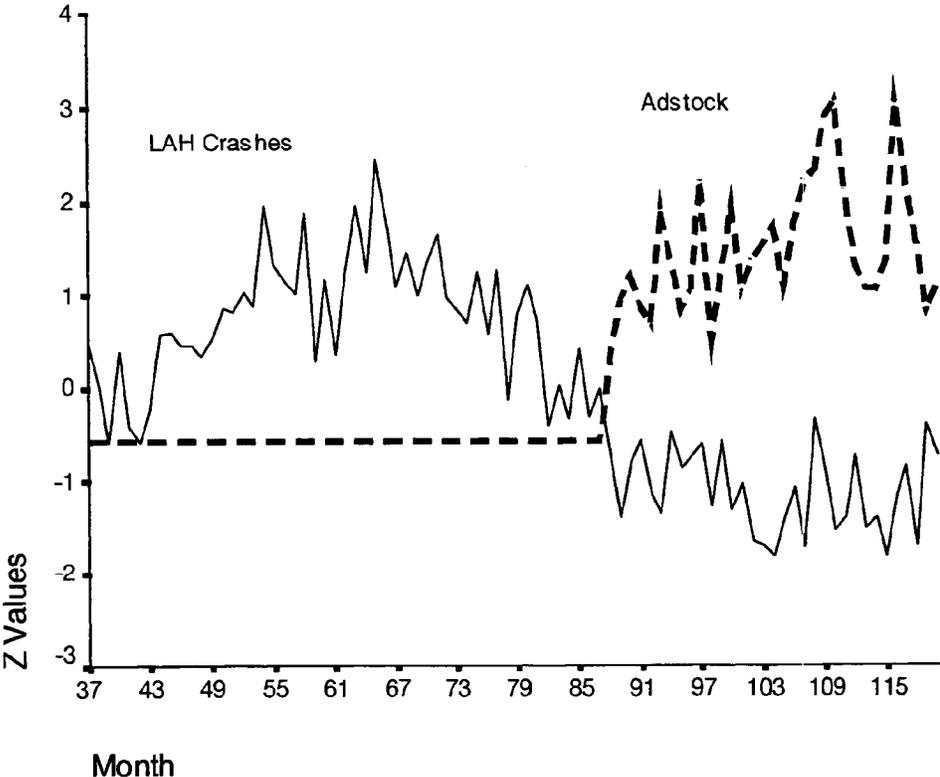


Figure 6.2.2

Coincidence Plot for LAH Crashes and Speed Adstock



In the case of enforcement, it might be possible to argue for the existence of some 'anticipatory' effects, as the enforcement campaign had presumably received some advance attention from the print and TV news media. However, it is still not possible to argue in favour of a causal role for speed enforcement in the *turn-around* of monthly LAH crashes, which occurred before even the low-level trialing of the cameras had commenced.

Figure 6.2.2 is a sequence plot for LAH crashes and speed and concentration TV advertising (Adstock). It is interesting to note that the TAC advertising was not launched until April 1990 (month 88), which was 23 months *after* the start of the decline in crash numbers, and only one month before the first crash low point in May 1990. In the case of TAC advertising, it is not possible to argue for the existence of 'anticipatory' effects. It is also worth noting that a levelling-out of LAH crashes occurred during 1991 and 1992 while TAC speed advertising continued at very high levels. It would therefore not be possible to argue in favour of a causal role for TAC speed advertising in the turn-around of monthly LAH crashes; and it would be very difficult to argue, from this graph, that the advertising had played *any* role at all.

The argument here is not that the sequence plots prove that the speed-related crash countermeasures were totally ineffective, but that the plots are strongly indicative that the countermeasures could not have been involved in halting the increase in crash numbers, or in initiating the sharp fall. It is still quite possible that fluctuations in countermeasure levels in the last three or so years of the study period could underlie the fluctuating crash numbers during that period.

### 6.3 *Drink Drive Countermeasures and HAH Crashes*

Any evidence that HAH crashes had started to decline in number before the enforcement or advertising campaigns had been launched would seriously challenge the view that the TAC-funded drink drive campaigns had been responsible for halting the increase in HAH crash numbers, or for initiating the subsequent decline.

Some relevant information was implicit in Figures 5.1.1, 5.1.3 and 5.1.4. This information is made explicit in Figures 6.3.1 and 6.3.2 by plotting the relevant time-series data in the *same* graphs. The crash figures were de-seasonalised and standardised; and the enforcement and advertising figures were standardised. The first three years' data (1983-1985) are omitted.

Figure 6.3.1 is the sequence plot for HAH serious casualty crashes and RBTs. Crash numbers peaked at 270 in December 1987 (month 60), fell gradually at first, and then fell sharply between mid 1989 and early 1991, reaching a low value of 122 in January 1991 (month 97). (The de-seasonalised minimum in Figure 6.3.1 actually occurs four months later, in May, 1991 (month 101)). Crash numbers then remained at low, but fluctuating, levels for the next two and a half years, with some months dipping below the earlier low point.

The drink drive enforcement campaign commenced in September 1989 (month 81), then quickly gained momentum.

Figure 6.3.1 Coincidence Plot for HAH Crashes and RBTs

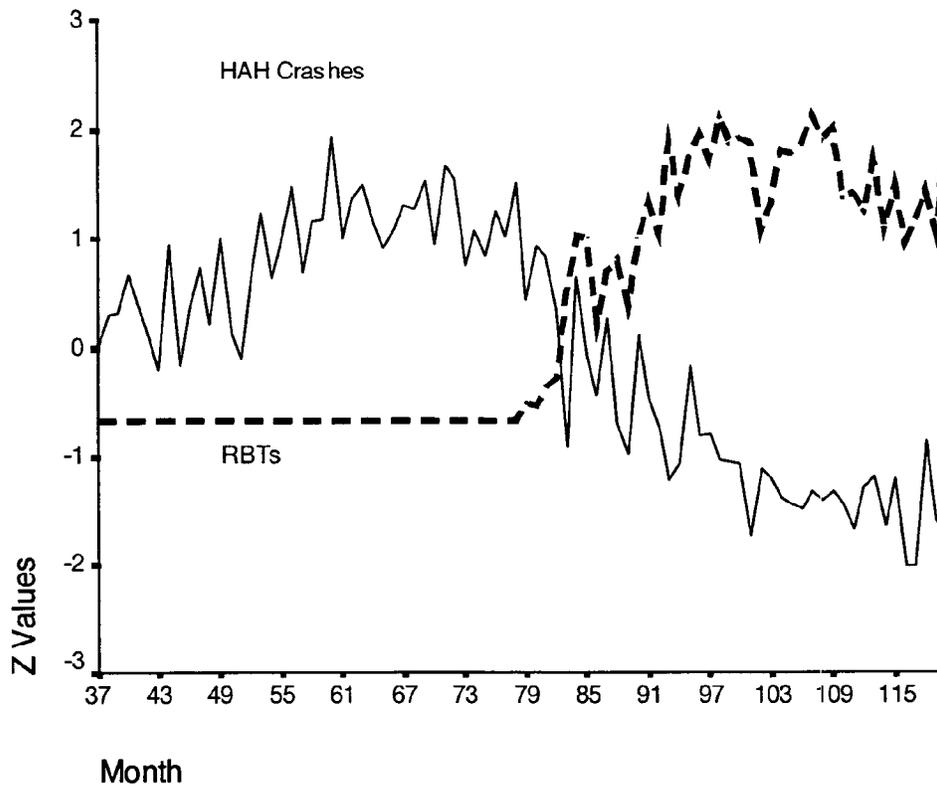
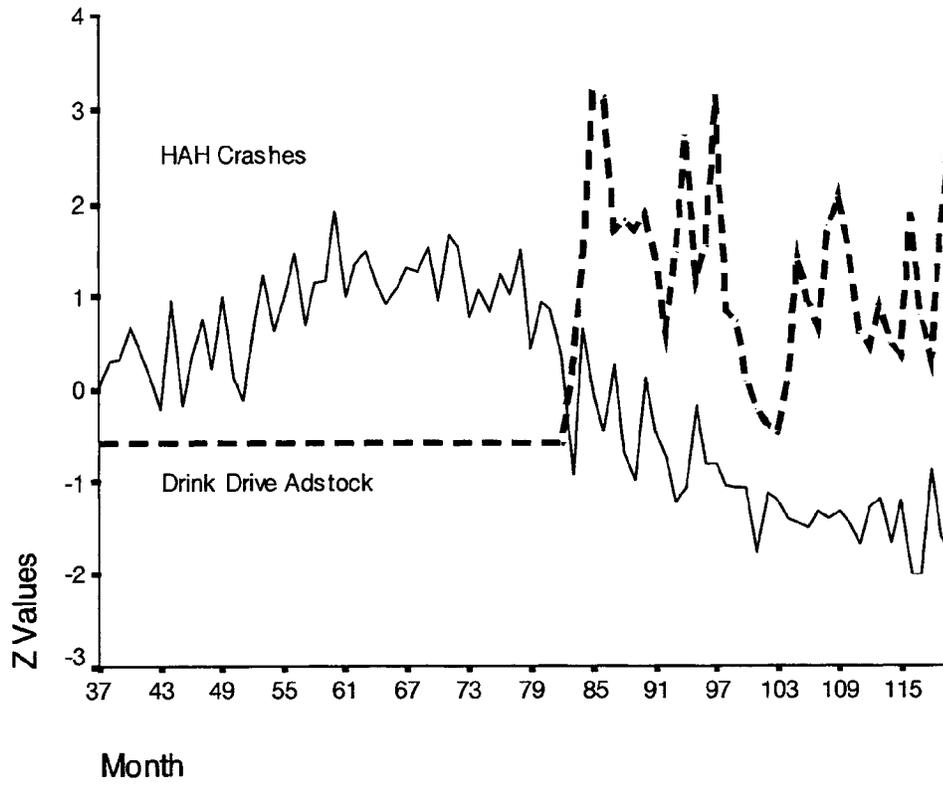


Figure 6.3.2

Coincidence Plot for HAH Crashes and Drink Drive Adstock



In the case of enforcement, it might be possible to argue for the existence of some 'anticipatory' effects, as the enforcement campaign had presumably received some advance attention from the print and TV news media. Nevertheless, it is not possible to argue in favour of a causal role for drink drive enforcement in the *turn-around* of monthly HAH crashes—which occurred well over a year earlier.

Figure 6.3.2 is a sequence plot for HAH crashes and drink drive TV advertising (Adstock). The advertising was considered by Newstead et al. (1995) to have starting in November 1989 (month 83)—at much the same time as the sharp increase in bus-based RBT.

TAC drink drive advertising did not commence until well over a year after the start of the decline in HAH crash numbers. In the case of advertising, it is not possible to argue for the existence of 'anticipatory' effects. It is also worth noting that a levelling-out of HAH crashes occurred during 1991 and 1992 while drink drive advertising continued at very high levels. Based on this evidence, it is obviously not possible to argue in favour of a causal role for TAC drink drive advertising in the turn-around of monthly HAH crashes.

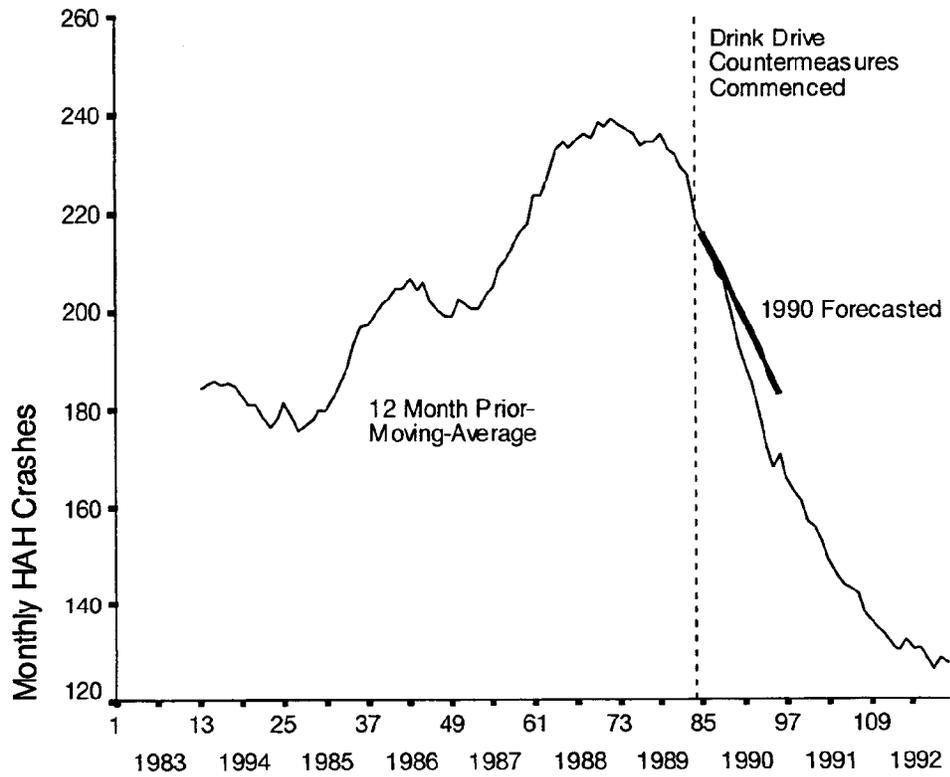
#### 6.4 Comparable Findings from Drummond et al. (1992)

The second MUARC report investigating the downturn in Victorian road crashes, by Drummond et al., was published in September, 1992. They realised that serious HAH casualty crash numbers in Melbourne had started to drop *well before* any countermeasures were introduced.

Drummond et al. (1992) used Auto-Regressive Integrated Moving Average (ARIMA) modelling to generate a forecasted crash line which was sensitive to the *recent* history of the crash variable. In conducting this type of analysis, they first created a 12-month prior-moving-average version of the crash variable. They then identified a single month as the month when the campaign should be considered to have started; and they made predictions about crash numbers for the year following that month. Using that procedure, forecasted crash numbers are very strongly influenced by the crash trend in the twelve months immediately preceding the identified starting date of the campaign. No attempt is made here to more fully describe the forecasting methods of Drummond et al.

Drummond et al. (1992) identified the starting month for the TAC drink drive campaign as December 1989. Their forecasted crash line for 1990 (from their Figure 61, top, p. 67) is re-drawn here as Figure 6.4.1. This line has a strong negative slope, which closely matches observed crash numbers. The match was so close that Drummond et al. concluded that there was nothing about observed HAH crashes in 1990 that required further explanation. Countermeasure effects were not detectable. The results of the Drummond et al. ARIMA analysis are compatible with the results of the 'sequence analyses' presented here in sub-section 6.3.

Figure 6.4.1 HAH Crashes as Forecasted by Drummond et al. (1992)



## 7 RN74 CRASH MODEL SPECIFICATION

### 7.1 *Experiments and Observational Studies*

The analysis of statistical data requires that proper account be taken of the way in which the data arose. In greatly simplified terms, comparative studies may be classified either as designed experiments or as observational studies.

In a simple designed experiment, where the chief aim is to evaluate a particular treatment, the subjects are divided into two groups, usually called the control group and the treatment group. The subjects in the treatment group receive the treatment and those in the control group do not. The response variable is then recorded for each individual, and the distributions of responses in the two groups compared. The key attribute of designed experiments is that the experimenter is able to determine how subjects are allocated to the two groups and, ideally, also to enforce certain protocols for the conduct of the experiment. In well designed experiments, the allocation to treatment and control groups is performed using randomisation; and protocols, such as 'double blind' administration of the experiment, are used. The purpose of these steps is to ensure that there are no systematic differences between the treatment and control groups initially, so that no spurious differences are introduced during the course of the experiment. If this is achieved then we can conclude that any systematic difference in the response must be the result of the treatment. Although various complications can arise, the presence of a substantial difference between treatments and controls in a well designed and executed experiment can be taken as strong evidence of a treatment effect.

Observational studies differ from designed experiments in that the experimenter is unable to control the allocation of subjects to the treatment and control groups. This poses a serious difficulty on the interpretation of data from observational studies. In well designed experiments, randomisation is used to eliminate initial spurious differences between the treatment and control groups. However, in observational studies no such mechanism is available, and there is therefore no logical basis on which to assume that the treatment is the only factor responsible for any differences between the treatment and control groups.

In practice, most studies are typically far more complicated than the designed experiment versus observational study paradigm outlined above. These canonical examples, nevertheless, form a useful point of reference against which to assess the properties of more realistic studies.

The RN74 analyses were concerned with data on Melbourne serious casualty crashes recorded monthly over the 10-year period from 1983 to 1992. In an attempt to reduce the number of casualties resulting from crashes, the following strategies were implemented: a police enforcement campaign targeting drink-driving; a police enforcement campaign targeting speeding; a TV advertising campaign targeting drink-driving; and a TV advertising campaign targeting speeding.

According to the variables that coded for these initiatives, each campaign commenced about 1989, and was maintained at a high level for the remainder of the study period. In this respect, the study can be seen to have certain features of an observational study. In particular, if we take the months prior to the commencement of the campaign as 'control months' and those following the commencement as

'treatment months', then it is clear that the 'allocation' of months to treatment and control is very different from what would be produced by randomisation. In particular, the control months are all at the start of the study period and the treatment months are all at the end.

## 7.2 Confounding

A configuration such as that described above is prone to confounding in the sense that the effects of any other factor that had a systematic effect on the crash numbers over time will be confounded with the advertising and enforcement variables. That is, if such factors exist and are not recorded, then apparent treatment effects will, in fact, be the combined effect of the treatment and the confounding factors and there is no way to obtain separately the actual treatment effects. The fundamental difficulty of an observational study, such as the one in question, is that there is no logical basis on which to infer that *no* such confounding factors are present.

While such considerations may appear at first sight somewhat hypothetical, they are, in fact, crucial. In the present context, we can observe that monthly crash numbers are subject to random variation and seasonal variation together with longer term influences. These longer term influences are not fully understood either theoretically or even empirically. Thus, it is *implausible* that confounding would *not* be present in the study that was reported in RN74.

### The Potential for Confounding in the RN74 Regression Models

Direct evidence of the *potential* for confounding can be seen by examining long-term time series plots of monthly Victorian road crash fatalities for the period from 1970 to 1998.

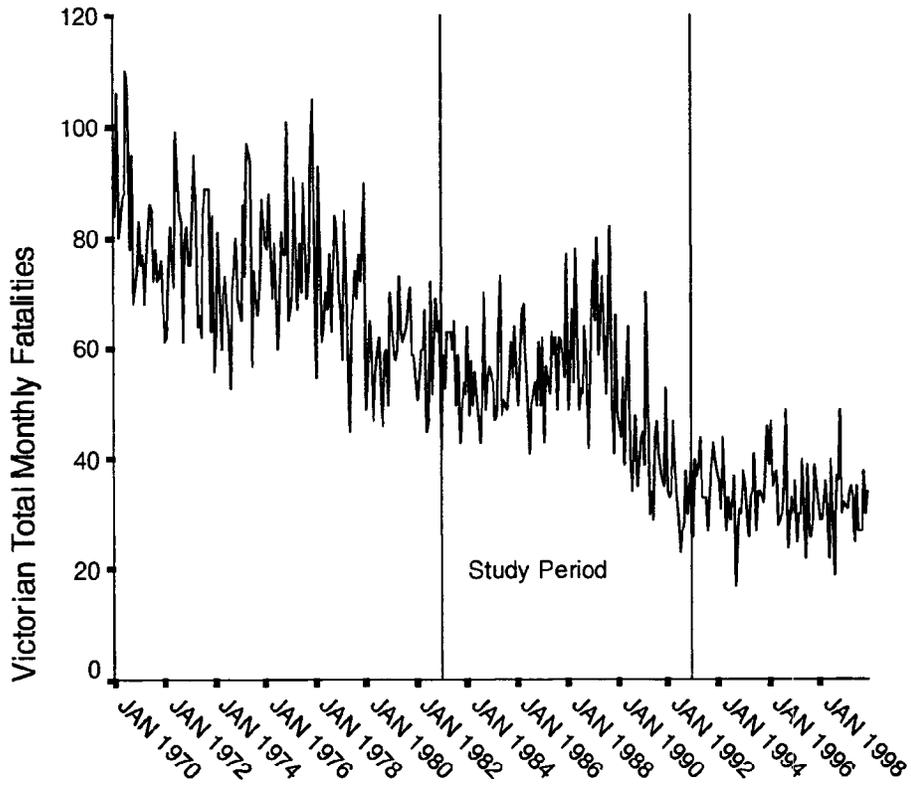
Figure 7.2.1 shows a strong overall downward trend for the 29-year period. This downward trend is consistent with downward crash trends in all developed countries since about 1970 (Beenstock & Gafni, 2000; Broughton, 1991). According to Beenstock and Gafni (p. 72), these long-term downward trends are mainly due to the incremental effects of safer vehicles and safer road infrastructure.

It can also be seen from Figure 7.2.1 that there is a strong *upward deviation* from the downward trend for the first six or so years of the RN74 study period (from 1983 to 1988). (Much the same pattern would have been found for Metropolitan Melbourne serious casualty crashes as is shown here for Victorian fatalities.) The upward deviation is obviously not due to the implementation of the crash countermeasures. Crashes over the six-year period were increasing for reasons that are not well understood. Unless those reasons are unambiguously identified, and accounted for in the RN74 regression models, the values of the regression coefficients for the countermeasure variables will be unreliable.

The main point to be stressed here is that the historical picture shows that fluctuating crash numbers reflect an evolving dynamic process. There is a general downward trend with a number of peaks and troughs not dissimilar from those that occur in the 10-year study period. This establishes clearly that there are other systematic influences in play that must be adjusted for if unbiased estimates of the intervention effects are to be obtained.

Figure 7.2.1

Victorian (Metropolitan + Rural) Monthly Road Crash Fatalities for the Period from 1970 to 1988. The 10-year MUARC Study Period from 1983 to 1992 is Identified.



As an aside, it seems relevant here to quote from Beenstock and Gafni (2000, p. 83):

The fall in the accident rate that occurred in Victoria, Australia in the early 1990s following an intervention package introduced in 1989 (Cameron et al., 1995) (*sic*) may simply have been the renewal of a downward trend in the accident rate that would have occurred without the interventions.

Much the same point was made previously by Henderson (1997). Certainly, inspection of Figure 7.2.1 provides little qualitative evidence of a substantial change in regime resulting from the TAC-funded interventions (implemented in late-1989).

Further direct evidence of the *potential* for confounding can be seen by examining time-series plots of serious casualty crashes and countermeasure levels from 1983 to 1992. Figures 6.2.1 and 6.2.2 show standardised, de-seasonalised crash numbers for the low alcohol hours. Crash numbers appear to increase steadily before levelling out and commencing a rapid decline from about mid-1988. Although speed enforcement was introduced at about the time of the peak, levels of enforcement were very low for the first year or more. Speed advertising did not commence until a year or more after the peak. Figures 6.3.1 and 6.3.2 show standardised, de-seasonalised crash numbers for the high alcohol hours. Again, crash numbers appear to increase steadily before levelling out and commencing a rapid decline from about mid-1988. Drink drive enforcement and advertising did not commence until a number of months after the peak.

These facts strongly suggest that crash numbers were coming down for other reasons before the interventions commenced. While this does not allow us to conclude that the interventions had no effect, it indicates very clearly that *unbiased* estimates of the intervention effects will only be obtained if the other factors driving the crash numbers are correctly identified and adjusted for.

#### Adjusting for Confounding Through Regression Modelling

In seeking to adjust for confounding variables, Newstead et al. (1995) employed various multiple regression models. While regression modelling is frequently applied in such contexts, it is essential to be aware of the serious limitations on its conclusions.

In general terms, a major difficulty is as follows. The purpose of fitting a multiple regression model in the present context is to obtain estimates of coefficients that describe the effects of the treatment variables (that is, advertising and enforcement). Other predictor variables are included in the multiple regression models to account for known sources of variability, and to adjust for possible confounding variables. In terms of the numerical values of the regression coefficients of interest, the precise form of the regression model used is critical. In particular, changing the other predictor variables in the model can have a major impact on those coefficients.

In Sections 8 and 9 of this report, the effects of changing the identity of various predictor variables are investigated. If the RN74 regression models are robust, such changes should have very little effect on the size of the regression coefficients of the countermeasure variables. Conversely, if the RN74 models are not well specified, minor changes to the definitions of the predictor variables could greatly affect the regression coefficients of the countermeasure variables.

### 7.3 Data Dredging and the Evolution of the MUARC Models

The results of a statistical modelling exercise will be valid if the selection of predictor variables is *strongly determined* by an underlying theory which is *fully justifiable on rational grounds*. That will not be the case where variable selection is largely the result of data dredging. When data dredging is involved, the modelling results should be considered to be very tentative, at least until they have been replicated against independent sets of data.

The general point to be made is that the selection of a multiple regression model for observational data where a large number of candidate explanatory variables are available must be regarded as a *purely exploratory* exercise. It will typically be the case that several different well-fitting models, with widely differing interpretations, can be found. The fact that a particular model fits the data well, and has regression coefficients in broad accordance with prior expectations, is no guarantee that the model has any predictive value at all.

The question must be asked, to what extent the RN74 regression models have 'evolved' to better describe the original set of data through the selection of predictor variables on the basis of their ability to improve the overall 'fit' of the model.

The following quotes indicate that the RN74 models have a pre-history that involved such an evolutionary process (Thoresen et al., 1992, p. 17):

... only a small sub-set of the explanatory variables considered were finally included. The principal criteria adopted for inclusion were that the variables should have the right sign and magnitude and should improve the overall statistical significance of the equation as measured by the overall F value for each model.

Given that some 66 potential explanatory variables ... were considered, a number which was increased when the same set of variables lagged one period were trialed, the numbers of truly explanatory variables finally chosen are small.

There was also a move away from the use of TARPs as an operationalisation of advertising levels in the early models to the use of Adstock in the later modelling. The use of Adstock seems to have improved the fit of the models. With respect to 'Rest of Victoria' serious casualty crashes, Cameron et al. (1993, p. 20) noted that "In particular, the Adstock of the drink-driving publicity was very highly statistically significant, even though the raw TARPs were not". And with respect to Melbourne non-serious casualty crashes, they noted (p. 21) that "The estimated exponents of the TAC publicity variables were statistically significant only in the models where the publicity was represented by Adstock".

Similarly, one reason for selecting unemployment rates in preference to fuel sales as a measure of economic activity was that " ... previous research had found statistically significant relationships with unemployment rates" (Cameron & Newstead, 1993b, p. 21).

The MUARC researchers had available to them two versions of the drink drive advertising variable with different starting months. The variable they chose to include in their models provided a stronger advertising effect than the other.

Although the MUARC regression models have been derived from a conceptual model, they have nevertheless undergone considerable evolution within that framework. It would not be inappropriate to describe this process as 'data dredging'. It would seem that, in the absence of validation against an independent set of data, the models should be considered to be very speculative. Some further issues relevant to the evolution of the RN74 models are considered in the following sub-section.

#### *7.4 Problems of Variable Selection and Definition*

To be correctly 'specified', it is not sufficient that a regression model merely include *some* measure of each important variable. It is also necessary that the variables be appropriately defined, or 'operationalised'. This sub-section discusses a number of problems in RN74 associated with the selection of variables, and their operationalisation.

##### Speed Countermeasures Omitted from the HAH Crash Model

The omission of important predictor variables from regression models means that they are incorrectly 'specified' and that, as a consequence, the coefficients obtained from the regression equations are unreliable. Because there is less traffic congestion at night, it seems likely that speeding would be more common at *night*. The omission of speed countermeasure variables from the HAH ('night-time') crash models is, therefore, very problematic. This matter is further explored in sub-section 9.1 of this report.

##### Evaluation of Speed Countermeasures Against Only LAH Crashes

As noted above, it seems likely that speeding, and therefore speed-related crashes, would be relatively more frequent and severe at *night*. The legitimacy of evaluating the speed campaigns against crashes which occurred only during low alcohol hours ('day-time') is, therefore, very questionable. This matter is briefly revisited in sub-section 8.3 of this report.

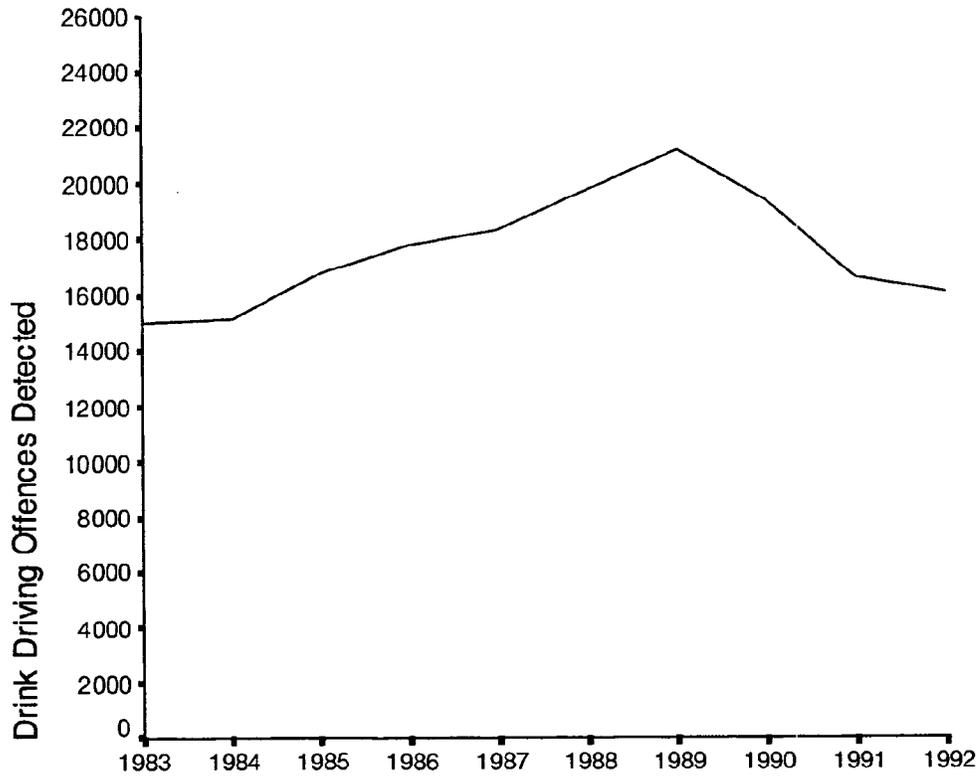
##### The 'General' and 'Specific' Deterrent Effects of RBT

Explanations for the effects of enforcement vary in their emphasis on the relative importance of 'general' and 'specific' deterrence. 'General' deterrence refers to the effects of seeing or knowing that enforcement activities are being carried out, and, therefore, fearing the possibility of being caught for offending. 'Specific' deterrence refers to the punishing effects on an individual of actually being caught, and penalised, for offending.

Random breath testing (RBT) is considered by Henstridge, Homel and Mackay (1997, p. 4) to be "a very pure expression of the theory of general deterrence". The operationalisation of the drink drive enforcement variable in terms of RBTs presumably reflects the belief of the MUARC researchers that drink drive enforcement works mainly through its general deterrent effect.

Figure 7.4.1

Total Annual Detected Drink Driving Offences 1983-1992



However, the widely held view that drink drive enforcement provides its benefits mainly through its general deterrent effects has recently been brought into question by Harrison (1999). Harrison believes that the gains that might be obtained from a greater emphasis on specific deterrence (that is, actually catching and punishing offenders) have probably been under-estimated. There has been a strong emphasis on general deterrence in the recent past in Australia (as exemplified in the use of high-profile Booze Buses—which actually catch very few offenders). This emphasis may have overshadowed the need to apprehend drink drivers.

South (1994, p. 12) provided the annual numbers of Victorian drivers actually detected for drink driving offences from 1977 to 1994. Those numbers changed very little over the 18-year period. Figure 7.4.1 presents the numbers for the 10-year study period from 1983 to 1992. A comparison of Figures 7.4.1 and 5.1.3 shows that the trends for the measures of specific and general drink drive deterrence have behaved very differently over the 10-year period.

There is some indication that drink drive enforcement could be losing its effectiveness in Australia. Results from the 1998 *National Drug Household Survey* (Australian Institute of Health and Welfare, 1999) indicate that the percentage of adults who admit to driving a motor vehicle while under the influence of alcohol has increased from 10.3% in 1995 to 17.5% in 1998. There may be a perception that the current emphasis on general deterrence is like 'crying wolf'. It might be appropriate for future studies of the effectiveness of drink drive enforcement *not* to overlook the role of specific deterrence.

If Newstead et al. (1995) had used a measure of specific deterrence in their HAH crash models, instead of a measure of general deterrence, their findings for the effectiveness of drink drive enforcement would presumably have been very different.

#### The Starting Month for Drink Drive Advertising

The main points to be made (somewhat laboriously) under this heading are: (1) that there was some arbitrariness about the month chosen as the starting date for drink drive TV advertising; (2) that there was some inconsistency in including VicRoads TV advertising for November 1989 while excluding similar previous advertising; and (3) that an implication of including one month's VicRoads TV advertising is that the MUARC researchers were focused on the *level* of advertising rather than its *content*. A further point will be made later (in sub-section 9.2): that an apparently trivial change to the starting date had a profound effect on the contribution of drink drive advertising to the RN74 HAH crash model.

The early evaluations of the drink drive campaigns (Drummond et al., 1992; Cameron, Cavallo & Sullivan, 1992; Cavallo & Cameron, 1992) made no attempt to identify a separate 'advertising effect'. However, those researchers did consider it necessary to identify a starting date for the overall RBT campaign. Drummond et al. (1992, p. 63) noted that "The date of mid-December corresponds to the introduction of publicity". Consequently, they concluded that "The week starting 16 December 1989, the closest week after the formal launch of the initiative, was chosen as the intervention date for the RBT initiative".

Cameron, Cavallo and Sullivan (1992, p. 5) also noted that "A publicity campaign was launched on 12 December 1989". They went on to say that "The level of publicity was extremely intensive relative to levels of previous initiatives". In their

statistical analyses, they had to divide the year 1989 into “pre-intervention” and “intervention” phases. They considered that the intervention started in December 1989. There was no mention in these early reports of any TAC TV advertising occurring before December 1989. There can be no doubt that the TAC drink drive advertising campaign actually commenced in mid-December, 1989.

Cameron et al. (1993) were the first to attempt to identify a separate drink drive ‘advertising effect’. The graphs in their report (Figure 1, p. 3, and Figure 4, p. 18) clearly identified the starting date for drink drive advertising as December 1989. However, their regression models included data from *November* 1989. The use of November 1989 data in their regression models seems inconsistent with their statement (p. 17) that: “The [Adstock] 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”.

Adding to this confusion, Newstead et al. (1995) showed a *November* 1989 starting date for drink drive advertising in their Figure 5 (p. 7), and used *November* advertising data in their regression analyses, but made no written comment about the starting date.

Given that the TAC drink drive advertising campaign actually commenced in December, 1989, it is surprising to find the use of November advertising data in the statistical analyses of Cameron et al. (1993) and Newstead et al. (1995). Cameron et al. (p. 18) stated that:

VicRoads (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.

This statement does not solve the problem. It indicates that December 1989 Adstock levels were inflated by the ‘residue’ from VicRoads advertising in November 1989, but does not explain the use of November 1989 Adstock in the analyses.

Recent discussions with Max Cameron and Stuart Newstead (1999) confirmed that the November 1989 Adstock was entirely derived from VicRoads advertising. They said that the November 1989 levels of advertising were at relatively high levels compared with previous ‘VicRoads’ practice, and claimed that this fact justified the inclusion of the November 1989 TARPs in their analyses. However, at 430 TARPs, the November 1989 level is not very high. It is well below the maintenance level of 1400 TARPs recommended by Cameron et al. (1997) to the Road Safety Council of Western Australia; and is considerably lower than the maximum level of 1236 TARPS achieved under the TAC regime between 1983 and 1992.

If the inclusion of November 1989 VicRoads drink drive TARPs is justifiable, one must ask *why* all the *previous* VicRoads drink drive advertising was ignored. The decision to include one month of VicRoads advertising seems arbitrary, especially in view of the fact that the level of advertising in November 1989 was not very high.

The content of the VicRoads TV campaigns was very different from that of the TAC campaigns. The VicRoads advertisements were made on relatively small budgets, and tended to emphasise the risk of detection for offending rather than the consequences of crashing. The decision to include some VicRoads advertising along with the TAC advertising shows that the MUARC researchers were more concerned with the *level* of advertising than with its *content*.

The failure to explicitly acknowledge the inclusion of the November 1989 VicRoads TV advertising is an unfortunate omission from the various relevant MUARC reports.

### Alcohol Sales

According to Thoresen et al. (1992, p. 14) “... the [Alcohol Sales] series appeared to provide good estimates of variations in monthly consumption”. That claim could *not* be made with respect to *recent* longer-term changes in ‘Alcohol Sales’ (ie, liquor outlet turnover), some of which, since 1992, were due to the introduction of gaming machines (ABS Catalogue Number 8687.0).

The validity of the Alcohol Sales variable is also questionable for the study period (1983 to 1992). There was a 43.7% fall in Alcohol Sales between the first and last years of that period. A reduction of that size in Victoria seems unlikely, especially given that a more direct measurement of alcohol consumption at the national level – the Apparent Per Capita Consumption of Alcohol (ABS Catalogue Number 4306.0) – fell by only 18.3% over the same period. The occasional ABS *Survey of Household Expenditure* (ABS Catalogue Number 6535.0) provides a third means of estimating changes in alcohol consumption: real Weekly Household Expenditure on Total Alcoholic Beverages in Victoria fell by about 17.5% between 1984 and 1993-94. That figure is comparable with the reduction in Apparent Per Capita Consumption of Alcohol in Australia, but is less than half the reduction in alcohol consumption in Victoria as measured by the ‘Alcohol Sales’ variable. It seems likely that less than half of the ten-year reduction in the value of the Alcohol Sales variable is actually due to changes in alcohol consumption. The remainder is presumably due to changes in the levels of the various other activities occurring at ‘clubs, pubs, taverns and bars’.

The MUARC researchers have not always included the Alcohol Sales variable in their regression analyses of drink driving crashes. It was omitted from the analyses conducted by Cameron et al. (1992, p. 20) on the grounds that “The correlations found in Victoria between Alcohol Sales and Unemployment Rate could violate the assumption of independence between variables ...”.

As an aside, it is worth noting that statistical analyses of RN74 data (not reported here in any detail) showed that there was no relationship between Alcohol Sales and HAH crashes for the first five years of the study period (ie, before HAH crash numbers started to fall).

The inaccuracy of the ‘Alcohol Sales’ variable as a measure of alcohol consumption means that its inclusion in the HAH crash model is questionable.

### The Linear Trend Variable

Analyses of the RN74 data (not reported here in any detail; but reported in White (2000)) show that the inclusion of the linear trend variable in the LAH and HAH crash analyses had the effect of substantially *increasing* the contributions of the countermeasure variables to the models.

Cameron and Vulcan (1998) omitted the highly significant trend variable from similar analyses of New Zealand crash trends. Re-analyses of the New Zealand data (reported here in Section 12) show that the exclusion of the linear trend variable had the effect of substantially *increasing* the contribution of the countermeasure variables to the New Zealand crash models.

It would seem that the decision to exclude the trend variable from the New Zealand analyses, while including it in the Victorian analyses, involved some subtlety of judgement. The consequences of those decisions were such that the estimated contributions of the countermeasure variables were much greater than if the opposite pattern of decisions had been made.

### Conclusion

The arbitrariness of the RN74 models is evidenced by: the exclusion of important speed countermeasure variables from the 'night-time' (HAH) crash model; the exclusion of night-time (HAH) crashes from the model evaluating the effects of the speed countermeasures; and the questionable operationalisation of some predictor variables. The coefficients of the predictor variables would probably have been very different if different choices had been made about the selection and definition of the predictor variables. Newstead et al's (1995) estimates of the sizes of crash reductions which are attributable to countermeasure effects should therefore be considered to be unreliable. While the RN74 models might be able to describe the past with apparent accuracy, their ability to predict the future must be questioned. The RN74 models should be considered to be 'hypothesis generating' rather than 'hypothesis testing'.



## 8 ADJUSTMENT FOR ECONOMIC ACTIVITY

It was noted previously that there are serious limitations to the conclusions that can be drawn from regression modelling; and that the precise form of the regression model used is critical. In particular, it was stressed that changing the *other* predictor variables in the RN74 models could have a major impact on the regression coefficients of the countermeasure variables. In this section of the report, the effects of changing the identity of the economic predictor variable will be examined.

### 8.1 Unemployment

Statistical models of road crashes usually incorporate one or more measures of economic activity as a control for driving exposure. According to Wang et al. (1999, p. 2), "The most important explanatory effects associated with safety trends are *economic*, such as personal income, the unemployment rate, number of automobiles manufactured and gross national product".

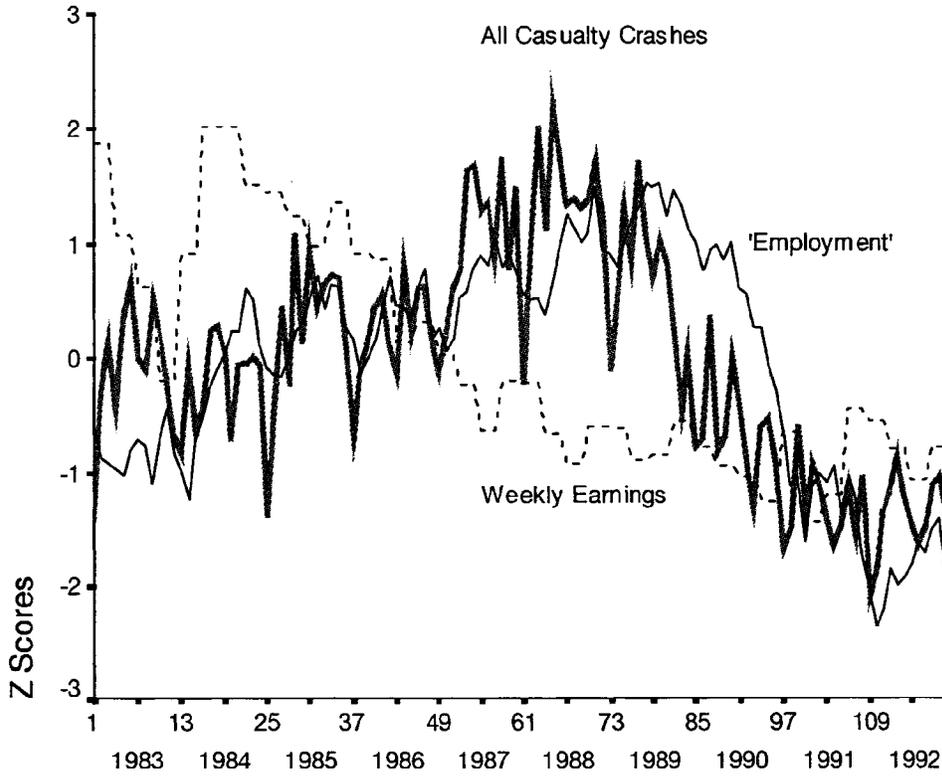
Possible economic measures considered by Thoresen et al. (1992) included: Unemployment, Real Average Weekly Earnings, and Gross Domestic Product. Also mentioned by Thoresen et al. were: New Vehicle Registrations, Fuel Sales and Alcohol Sales; all of which might be *indirect* measures of economic activity. Many of the explanatory variables were evaluated 'as observed' and when lagged by one month. [It is also worth noting that longer lags, of up to six months, were considered by Cameron, Cavallo and Sullivan (1992, Appendix A, p. 50) in their exploratory analyses of the relationship between Unemployment and Kilometres Travelled.] Unemployment was eventually selected by the MUARC researchers as the economic indicator that should provide the best measure of driving exposure in Victoria; and especially, according to Cameron, Cavallo & Sullivan (1992, p. 14), of exposure during high alcohol hours. More recently, in explaining serious casualty crash trends in New Zealand, Cameron and Vulcan (1998) selected New Car Registrations as an explanatory economic variable of equal importance to Unemployment.

Thoresen et al. (1992, p. 18) considered that the *early* deterioration of economic conditions in Victoria (as indicated by the reduction in Real Average Weekly Earnings from early in 1986) was *not* adequately captured by the Unemployment variable. They also realised that the increase in Unemployment occurred about a year *after* the downturn in crashes. They could therefore see some merit in including Real Average Weekly Earnings in the regression model (in addition to Unemployment) to capture the early effect of the deteriorating economy on crash numbers. However, they eventually concluded that they should omit Real Average Weekly Earnings because of its multicollinearity with other variables. They were resigned to the use of Unemployment as the measure of the economy despite the fact that it was not able to capture the early effects of the deteriorating economy on crash numbers.

The inverse of the Melbourne Unemployment variable (referred to here, for convenience, as 'Employment') is plotted against *all* (ie, LAH plus HAH) casualty crashes in Figure 8.1.1. Also plotted in this graph are the *Real Average Weekly Earnings* for Melbourne (ABS Catalogue 6302.0). The values of all variables have been converted to Z scores so that they can be plotted against the same scale. It can be seen that the downturn in crashes occurs *after* the downturn in Real Average Weekly Earnings, but *before* the downturn in 'Employment'.

Figure 8.1.1

Trends in All Casualty Crashes, 'Employment' and Real Average Weekly Earnings



## 8.2 *Leading Index of Economic Indicators*

Not all economic variables fluctuate in unison. Economists (e.g., Frumkin, 1994; Rogers, 1994) differentiate between those variables that are sensitive to *early* changes in the economy (leading indicators), those which respond at much the *same time* as the general economy (coincident indicators), and those which follow *behind* the general economy (lagged indicators). Early changes in the economy are detected by the 'Leading Index', which incorporates: the Consumer Price Index, the Share Price Index, the Demand for New Telephone Services, New Building Approvals, and some other economic variables. Unemployment is considered to be a coincident indicator, and, as such, should lag behind the Leading Index.

Changes in the Australian national and state economies are monitored by the Melbourne Institute of Applied Economic and Social Research. According to their research (Boehm, 1996, pp. 19-20), "The Leading Indexes have fairly consistently displayed relatively long leads at both peaks and troughs ... of mostly at least six months and for some states of around a year". An implication of this fact is that Unemployment will often lag behind the Leading Index by up to a year or so. For the Victorian recession in the late 1980s, the sharp rise in Unemployment started about a year after the peak of the Victorian Leading Index. That peak was identified by Boehm (1996, p. 35) as occurring in August, 1988.

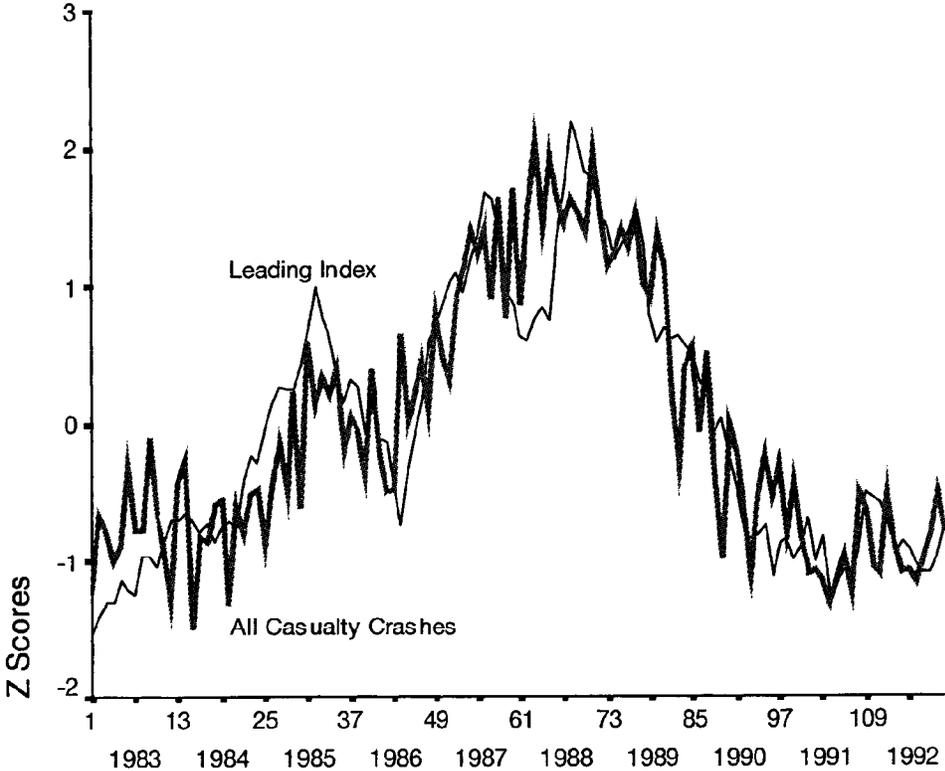
The peak in the Leading Index occurs at much the same time as the peak in all (ie, LAH + HAH) casualty crashes. Therefore, it seems possible that the Leading Index could provide the best-fitting economic variable to use in statistical modelling of crash numbers. Figure 8.2.1 plots standardised values of all casualty crashes and of the Leading Index for the ten-year period from 1983 to 1992. Both variables have been de-seasonalised and de-trended. (It should be noted that de-seasonalising and de-trending occurs automatically in the RN74 regression modelling process as a consequence of the inclusion of the eleven individual month variables and the linear trend variable.) It can be seen that the temporal fit between the Leading Index and all casualty crashes is remarkably close.

In the sub-section 7.2 of this report it was pointed out that there were explanatory gaps to be filled with respect to changes in Melbourne crash numbers between 1983 and 1992: the trend was *upwards* for the first five or so years of this period, reversing the trend of the previous ten years; and the turn-around in crash numbers occurred before the TAC-sponsored countermeasures were implemented. It is obvious that neither of these changes can be explained in terms of the effects of the countermeasures. One intention of this sub-section has been to suggest that the strong improvement in the economy, followed by its strong deterioration, as measured by the Leading Index, could fill those explanatory gaps.

The regression models in RN74 included the natural logarithm of the monthly Unemployment variable as a predictor to adjust for the effect of the economy. However, as noted above, there are several alternative indices of economic activity that could have been used, and there is no theoretical basis to prefer unemployment as the most relevant. The Leading Index is an indicator of economic activity that could have been used by Newstead et al. (1995). The main intention of this section of the report is to see what effect changing the identity of the economic predictor would have on the coefficients of the countermeasure variables.

Figure 8.2.1

The Relationship Between All Crashes and the Leading Index (Both Variables are De-Trended and De-Seasonalised)



### 8.3 Replacing Unemployment by CMA12 Lead Index in the RN74 Models

Because the month-to-month variation in the Leading Index was shown (in analyses not reported here) to be unrelated to the month-to-month variation in Melbourne serious casualty crash numbers, a smoothed version of the Leading Index was created by taking the 12-month centered-moving-average (CMA12). The main effects of substituting CMA12 Lead Index for Unemployment in the RN74 LAH crash model are given in Table 8.3.1.

**Table 8.3.1**  
*Effects of substituting CMA12 Lead Index for Unemployment in the RN74 LAH model.*

	LAH Crash Model			
	Unemployment		CMA12 Lead Index	
	B (SE)	p	B (SE)	p
Trend	0.003 (0.001)	0.000	-0.002 (0.001)	0.192
Economy	-0.117 (0.052)	0.026	1.122 (0.242)	0.000
Enforcement	-0.024 (0.005)	0.000	-0.011 (0.005)	0.045
Advertising	-0.029 (0.008)	0.000	-0.015 (0.007)	0.046

The substitution of the CMA12 Lead Index for the Unemployment variable increased the significance of the contribution of the regression coefficient (B) of the economic variable (from  $p = 0.026$  to  $p = 0.000$ ), while approximately halving the sizes of the regression coefficients of the speed countermeasure variables (from -0.024 to -0.011 for speed TINs; and from -0.029 to -0.015 for speed Adstock). The total percentage of LAH crash variance explained increased from 77.7% to 80.6%.

Newstead, Mullan & Cameron (1995, p. 5) have evaluated the effects of speed crash countermeasures against crashes at *all hours* of the day and night (LAH + HAH crashes), rather than against only *day-time* (LAH) crashes. Table 8.3.2 provides the main results of substituting CMA12 Lead Index for Unemployment in a speed countermeasure model that analyses *all* serious casualty crashes.

**Table 8.3.2**  
*Effects of substituting CMA12 Lead Index for Unemployment in the speed countermeasure model that uses all serious casualty crashes (LAH + HAH crashes)*

	Crash Model Using All Crashes			
	Unemployment		CMA12 Lead Index	
	B (SE)	p	B (SE)	p
Trend	0.002 (0.001)	0.000	-0.003 (0.001)	0.007
Economy	-0.203 (0.041)	0.000	1.293 (0.191)	0.000
Enforcement	-0.022 (0.004)	0.000	-0.006 (0.004)	0.151
Advertising	-0.025 (0.006)	0.000	-0.016 (0.006)	0.006

The substitution of the CMA12 Lead Index for the Unemployment variable has maintained the significance of the regression coefficient of the economic variable (at  $p = 0.000$ ), while substantially decreasing the absolute sizes of the regression coefficients of the speed countermeasure variables (from  $-0.022$  to  $-0.006$  for speed TINS; and from  $-0.025$  to  $-0.016$  for speed Adstock). The total percent of crash variance explained increased marginally from 86.1% to 88.1%. The speed enforcement variable failed to make a significant contribution to the 'all crash' model.

The effects of substituting 12CMA Lead Index for Unemployment in the RN74 HAH crash model are given in Table 8.3.3. The regression coefficient of the economic variable is still highly significant ( $p = 0.000$ ). The sign of the regression coefficient of the RBT variable changed from negative to positive (from  $-0.016$  to  $0.003$ ). The absolute value of the regression coefficient of the drink drive advertising variable decreased a little (from  $-0.015$  to  $-0.013$ ). Neither countermeasure variable makes a significant contribution to the new 'Leading Index' model. The total percent of HAH crash variance explained fell marginally from 86.6% to 84.6%.

**Table 8.3.3**  
*The effects of substituting CMA12 Lead Index for Unemployment in the RN74 HAH crash model*

	HAH Crash Model				
	Unemployment			CMA12 Lead Index	
	B (SE)	p	B (SE)	p	
Trend	0.003 (0.001)	0.000	-0.002 (0.001)	0.134	
Alcohol Sales	0.628 (0.150)	0.000	0.735 (0.159)	0.000	
Economy	-0.312 (0.034)	0.000	1.486 (0.194)	0.000	
Enforcement	-0.016 (0.006)	0.006	0.003 (0.006)	0.676	
Advertising	-0.015 (0.008)	0.057	-0.013 (0.008)	0.129	

Conclusion

There is a wide choice of variables that can be interpreted as general measures of economic activity. Although one might expect that all such variables would be at least loosely correlated, they can not be used interchangeably in the multiple regression analyses of Newstead et al. (1995). The preceding examples show that substituting the Smoothed Leading Index for Unemployment in the analyses changes the conclusions reached about the effectiveness of the countermeasure variables. The consequence of this substitution for the speed campaigns is that the regression coefficients for both speed countermeasures are approximately halved when the speed campaign is evaluated against LAH crashes. And the contribution of speed enforcement is reduced to non-significance when the speed campaigns are evaluated against *all* serious casualty crashes. The consequences of the substitution in the HAH crash model, are that the drink drive enforcement and advertising effects are both reduced to non-significance. It must be concluded that the strong countermeasure effects reported by Newstead et al. are largely a result of their arbitrary decision to use the Unemployment variable as their measure of economic activity.

## 9 FURTHER RATIONAL PERTURBATIONS OF THE RN74 MODELS

It was noted previously that there were serious limitations to the conclusions that could be drawn from regression modelling; and that the precise form of the regression model used was critical. In particular, it was stressed that changing the *other* predictor variables in the model could have a major impact on the regression coefficients of the countermeasure variables. In this section of the report, the effects of including and redefining some predictor variables will be examined.

### 9.1 Including Speed Countermeasures in the HAH Crash Model

It is worth examining the RN74 regression models in more detail to assess the impact of some of the modelling decisions that were taken. One aspect of the modelling that merits close attention is the separation of the evaluations of the speed and drink drive campaigns. In particular, separate regression analyses were performed for the LAH and HAH crashes. The RN74 model for HAH crashes included the background variables: Month (Seasonality), Log Unemployment, Log Alcohol Sales and Trend; together with the enforcement variable Log RBT, and the advertising variable Log Drink Drive Adstock. The countermeasure variables pertaining to the speed campaign were *not* included. However, there is no justification for presuming that the speeding is *not* a contributor to night-time (HAH) crashes. With less traffic congestion at night, the potential for speeding is greater; and mean speeds have been found to be higher at night (Dyson & Woolley, 2000). Furthermore, speed and drink drive enforcement activities have never been confined to the LAH and HAH times of day, respectively. In particular, there has always been some level speed enforcement at night. It would, therefore, seem natural to ask what effects the speed countermeasures might have had on the HAH crashes.

*Table 9.1.1  
Effects of including speed countermeasures in the RN74 HAH crash model*

	HAH Crash Model			
	Original Model		Model Including Speed Countermeasures	
	B (SE)	p	B (SE)	p
Trend	0.003 (0.001)	0.000	0.004 (0.001)	0.000
Alcohol Sales	0.628 (0.150)	0.000	0.606 (0.154)	0.000
Unemployment	-0.312 (0.034)	0.000	-0.301 (0.046)	0.000
DD Enforcement	-0.016 (0.006)	0.006	-0.014 (0.006)	0.012
DD Advertising	-0.015 (0.008)	0.057	-0.011 (0.009)	0.185
Speed Enforcement			-0.006 (0.005)	0.262
Speed Advertising			-0.001 (0.008)	0.848

The columns to the left of Table 9.1.1 show the parameter estimates for the multiple regression model in RN74 for HAH crashes. The columns to the right of the table show the parameter estimates obtained from the same model but with the speed enforcement and advertising variables included. Examination of the parameters of

interest shows that the effect of the drink drive enforcement variable is largely unaffected by the inclusion of the speed-related predictors; but the drink drive advertising variable is attenuated and is no longer statistically significant. Furthermore, neither of the speed campaign coefficients is significant.

Each significance value reported in Table 9.1.1 (right) is for the hypothesis that the given regression coefficient is zero. This estimate is valid only if all other terms are retained in the model. Thus, further analyses are required to reach a simultaneous conclusion about the three non-significant terms (drink drive advertising, speed enforcement and speed advertising). Such analyses (not reported here in detail) show that the speed advertising coefficient is not significant whatever combination of the speed enforcement and drink drive advertising variables is included; and that its removal from the model, as shown in Table 9.1.2 (left) has only a minimal impact on the other coefficients.

**Table 9.1.2**  
*Effects of including various countermeasure combinations in the RN74 HAH crash model*

	<b>HAH Model With Speed Countermeasures</b>			
	<b>Speed Advertising Variable Removed</b>		<b>Both Advertising Variables Removed</b>	
	<b>B (SE)</b>	<b>p</b>	<b>B (SE)</b>	<b>p</b>
Trend	0.003 (0.001)	0.000	0.003 (0.001)	0.000
Alcohol Sales	0.612 (0.151)	0.000	0.613 (0.152)	0.000
Unemployment	-0.307 (0.035)	0.000	-0.317 (0.034)	0.000
DD Enforcement	-0.014 (0.006)	0.012	-0.020 (0.004)	0.000
DD Advertising	-0.012 (0.008)	0.146	removed	
Speed Enforcement	-0.006 (0.005)	0.247	-0.008 (0.005)	0.092
Speed Advertising	removed		removed	

For simplicity, we can, therefore, restrict attention to the model shown in Table 9.1.2 (left). In this context, neither the drink drive advertising variable nor the speed enforcement variable is significant. If speed enforcement is removed, then the original RN74 HAH model, as shown in Table 9.1.1 (left), is the result, and the effect of drink drive advertising becomes marginally statistically significant. However, if, instead, drink drive advertising is removed, then the model shown in Table 9.1.2 (right) is the result. In this context, the speed enforcement variable becomes marginally significant ( $p < 0.1$ ).

Finally, it should be noted that F-tests can also be used to test the hypotheses concerning multiple parameters. In particular, we can test the hypothesis that both the speed enforcement and drink drive advertising variables can be removed from the model in Table 9.1.2 (left). The F-statistic is 2.53, and  $p = 0.084$ . Again, this is only marginally significant; so, in the framework of the model, we could accept the hypothesis that neither speed enforcement nor drink drive advertising had an effect on HAH crashes.

In summary, it would seem fair to say that the purpose of the regression modelling reported in RN74 was specifically to detect and quantify the effects of various countermeasures; and the conclusion reached there was that such effects were present. Given the likelihood that speeding occurs at night-time, it would seem sensible to consider effects of the speed countermeasure variables on the HAH crashes. When those variables are introduced into the regression analysis, it becomes impossible to determine separate, statistically significant effects for the drink drive advertising and speed enforcement variables.

Given these facts, the proper conclusion is simply that the data do not provide sufficient evidence to identify separate effects for those countermeasures. To discard the speed enforcement variable and report the marginally significant effect of the drink drive advertising variable out of context, as it appears on the left of Table 9.1.1, is to grossly overstate the importance of that variable. The corresponding parameter estimate will almost certainly be biased. If the above analyses had been conducted by Newstead et al. (1995), they would have had to conclude that drink drive advertising had no measurable effect on HAH crash numbers.

### 9.2 The Starting Date for Drink Drive Advertising

The starting date for the drink drive advertising campaign was discussed at some length in sub-section 7.4. A conclusion from that discussion is that the choice of December 1989 as the starting date the campaign is at least as plausible as the November 1989 date used by Newstead et al. (1995).

**Table 9.2.1**  
*The effects on the RN74 HAH crash model of starting drink drive advertising in December 1989, rather than November 1989 (as in the original RN74 model)*

	HAH Crash Model			
	November Start to Drink Drive Advertising		December Start to Drink Drive Advertising	
	B (SE)	p	B (SE)	p
Trend	0.003 (0.001)	0.000	0.003 (0.001)	0.000
Alcohol Sales	0.628 (0.150)	0.000	0.634 (0.154)	0.000
Economy	-0.312 (0.034)	0.000	-0.326 (0.036)	0.000
Enforcement	-0.016 (0.006)	0.006	-0.023 (0.005)	0.000
Advertising	-0.015 (0.008)	0.057	-0.002 (0.007)	0.759

The consequences of changing from the November to the December starting date are shown in Table 9.2.1: the drink drive advertising effect becomes non-significant. If the December starting date had been used by Newstead et al. (1995), they would have had to conclude that drink drive advertising had no effect on HAH crash numbers.

The choice to include the November data was made before the RN74 analyses were undertaken. Re-analyses of the data used earlier by Cameron et al. (1993) failed to find such a strong effect of changing the starting month for drink drive advertising.



## 10 INTERPRETATION OF THE REGRESSION COEFFICIENTS OF THE COUNTERMEASURE VARIABLES

In the three previous sections, the validity of the broad approach of Newstead et al. (1995) to regression modelling was seriously questioned. However, in this section, the validity of that broad approach is not questioned; what is questioned is the validity of some specific findings *assuming that the broad approach was valid*.

### The Quantitative Assumption

The following quote from Cameron et al. (1993, p. 7) indicates that they believed that the relationships they had obtained between the advertising and crash variables were mainly due to co-variation at the quantitative (month-to-month) level:

... 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 effects of the TAC advertising could be assessed.

This point was repeated by Cameron and Newstead (1996, p. 31) who were even more explicit in identifying the “monthly fluctuations” in the advertising variable as those which occurred *after the start* of TAC television advertising:

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 TAC advertising and the monthly road safety performance in Victoria.

The “Figure 1” referred to in the above quote is reproduced below. It depicts variations in TARP levels for the various advertising campaigns over the three-year period from December 1989 to December 1992. The zero levels of advertising prior to December 1989 are omitted. While it might seem from a casual inspection of Figure 1 that there is plenty of variation in the advertising variables that might be associated with variation in the crash variables, it should be noted that advertising levels were *not* represented in the RN74 analyses by *raw TARP*s values over *three* years; rather, they were represented by the *log-transformed* values of the *Adstock* versions of the advertising variables over *ten* years (see Figures 10.1 and 10.2). For most of that period, the monthly values of the advertising variables were zero.

Newstead et al. (1995) were less explicit than the researchers quoted above concerning the mechanisms underlying the advertising effects. RN74 included a graph of Adstock levels (p. 7) from which the fore-period of zero levels was omitted.

Inspection of Figures 10.1 and 10.2 naturally leads to the speculation that the advertising effects might *not* relate to crash outcomes *at the quantitative level*. It is easily conceivable that the advertising variables have only one property of relevance to their roles in the crash models: that they commenced in a particular month. In other words, it is conceivable that the role of the advertising variables in the regression models was equivalent to that of a step-function variable with only two effective values: ‘none’ and ‘some’. The same considerations apply to the enforcement variables.

Figure 1 from Cameron & Newstead (1996)

FIGURE 1  
TAC Road Safety Television Advertising : TARP's per month by theme

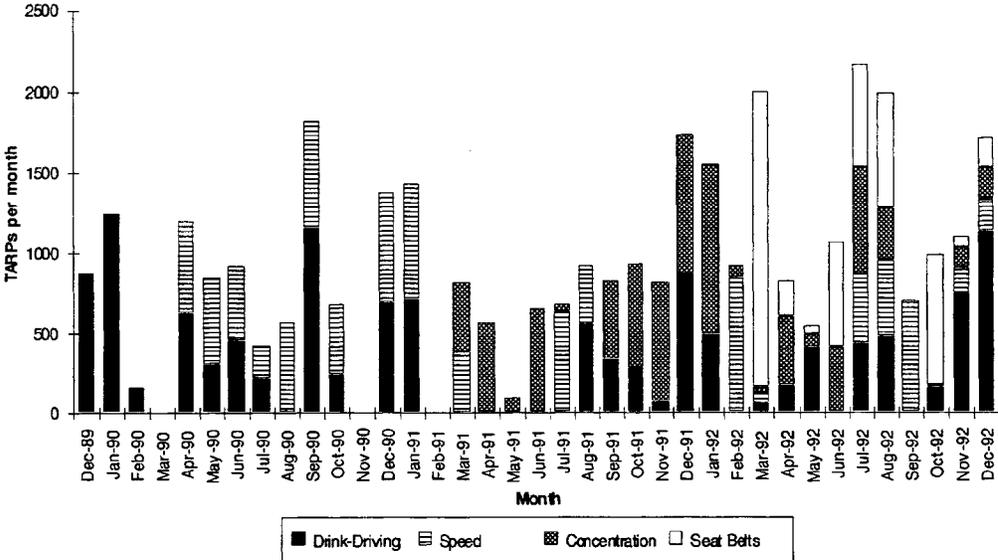


Figure 10.1

Log Speed & Concentrate Adstock 1983-1992

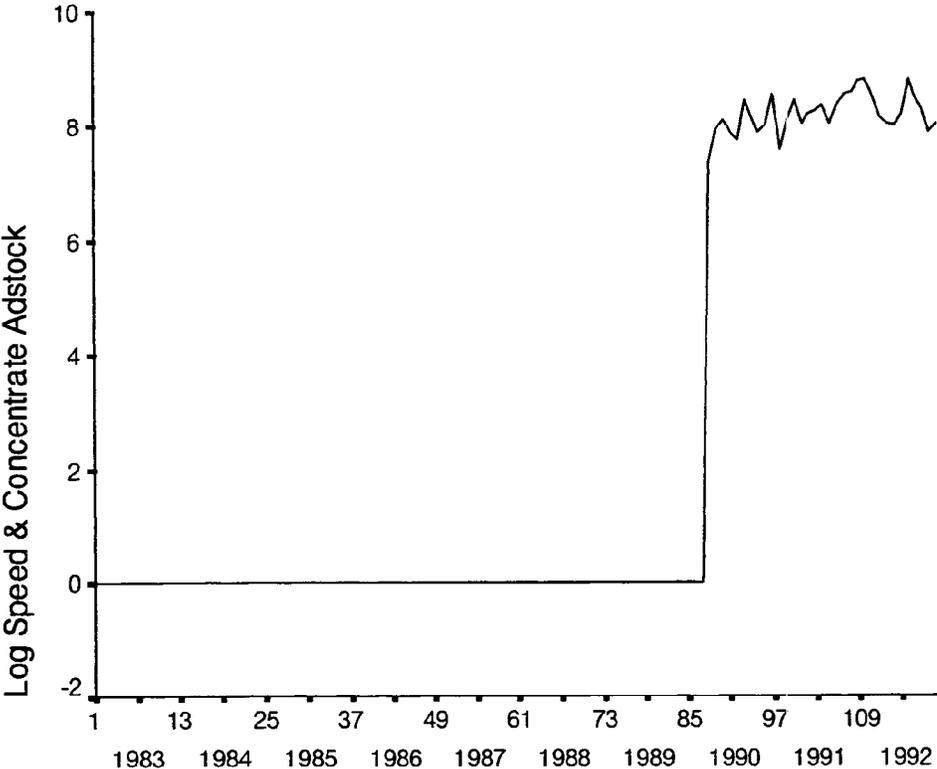
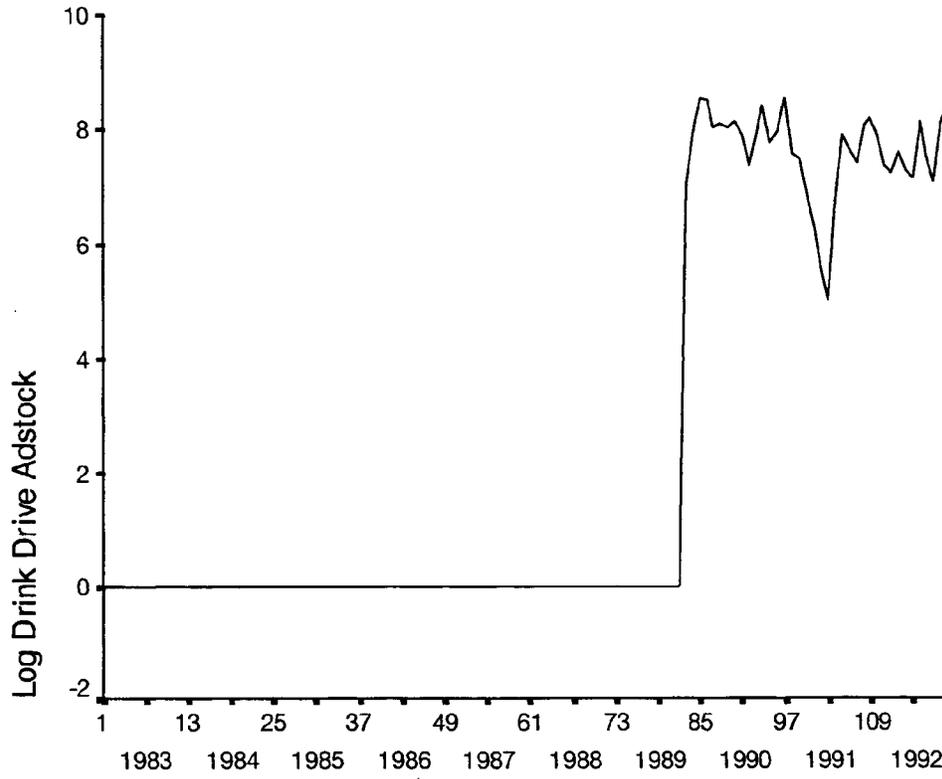


Figure 10.2

Log Drink Drive Adstock 1983-1992



## Two Important Consequences of a Quantitative Relationship

We now examine the regression coefficients for the advertising and enforcement variables as reported by Newstead et al. (1995) and reproduced in the left columns of Tables 10.1 and 10.2 for LAH and HAH crashes, respectively. Consider for example, the value of -0.029 for the estimated coefficient of the speed advertising variable in Table 10.1. Taken on face value, the interpretation of this coefficient is that, if all the other variables were held fixed, then the effect of a 1 unit change in the advertising variable would be a change of -0.029 units in the expected response (the LAH crash variable). This dose-response relationship, as portrayed for the advertising variables in Figures 3 and 5 of Cameron et al. (1993, p. 9 & p. 24), and reproduced here in sub-section 3.2, is an extremely important component of Newstead et al's scientific conclusions for the following two reasons.

First, the demonstration of a dose-response relationship would strengthen the case that the association between the intervention and response was *not* spurious. It was noted in Section 6 of this report that crash numbers appear to peak some time before the introduction of the countermeasures for both the LAH and HAH crashes. An explanation for the significant countermeasure effects reported in RN74 can then be given in terms of *confounding* along the following lines: If a downward trend in crash numbers over the second part of the study was driven by factors *not* accounted for in the MUARC regression models, then the significant countermeasure effects could simply arise because countermeasure levels were zero when crash numbers were relatively high, and were maintained at high positive levels when crash numbers were relatively low. In this case, the negative associations between the countermeasure and crash variables could be purely spurious. Although the countermeasure variables were maintained at high levels throughout the later months, there were nevertheless month to month fluctuations in those variables. In this context, a dose-response relationship between the countermeasure and crash variables at the level of the monthly fluctuations would provide evidence that the associations were *not* spurious.

Second, we contend that any recommendation to alter levels of advertising (for example, in the advice of Vulcan et al. (1996) to the South Australian government) must ultimately rest on the assumption of a dose-response relationship. Although this point is not addressed explicitly in RN74, it is very difficult to see on what basis other than an inferred dose-response relationship could a recommendation to increase advertising in South Australia from a non-zero level to a higher level be justified.

## Testing the Quantitative Assumption

A test for such a dose-response relationship may be performed as follows. Dichotomised versions of the advertising and enforcement variables can be constructed. Such variables take the value zero when the corresponding intervention variable is zero, and 1 otherwise. The dichotomised variables can be introduced into the multiple regression model, and the hypothesis that the original variables can be removed without detriment to the model can then be tested.

The result of fitting this regression model to the LAH crashes is shown in Table 10.1 (right). In this model, neither of the original intervention variable coefficients is significantly different from zero. The F-statistic is 0.58 for the hypothesis that *both* coefficients for the original advertising and enforcement variables are zero, and the

resulting p value is 0.56. Hence, in the context of the regression model reported in RN74 for the LAH crashes, it would appear that there is no real evidence for a quantitative effect of either speed advertising or speed enforcement.

**Table 10.1**

*Regression coefficients (B) for the predictor variables in LAH crash regression models. The original model is on the left. The extended model on the right contains both the original and the dichotomised versions of the countermeasure variables.*

Predictor Variable	Original Model		Model with Dichotomised Countermeasure Variables	
	B (SE)	p	B (SE)	p
Trend	0.003 (0.001)	0.000	0.003 (0.001)	0.000
Unemployment	-0.117 (0.052)	0.026	-0.146 (0.067)	0.032
Speed TINS	-0.024 (0.005)	0.000	-0.005 (0.020)	0.818
Dichotomised TINS	-	-	-0.161 (0.158)	0.311
Speed Adstock	-0.029 (0.008)	0.000	-0.049 (0.062)	0.437
Dichotomised Adstock	-	-	0.141 (0.491)	0.775

**Table 10.2**

*Regression coefficients (B) for the predictor variables in HAH crash regression models. The original model is on the left. The model on the right contains both the original and the dichotomised versions of the countermeasure variables.*

Predictor Variable	Original Model		Model with Dichotomised Countermeasure Variables	
	B (SE)	p	B (SE)	p
Trend	0.003 (0.001)	0.000	0.003 (0.001)	0.000
Unemployment	-0.312 (0.034)	0.000	-0.334 (0.043)	0.000
Alcohol Sales	0.628 (0.150)	0.000	0.624 (0.151)	0.000
RBTs	-0.016 (0.006)	0.006	0.051 (0.057)	0.370
Dichotomised RBTs	-	-	-0.588 (0.502)	0.244
Drink Drive Adstock	-0.015 (0.008)	0.057	-0.011 (0.022)	0.623
Dichotomised Adstock	-	-	-0.160 (0.214)	0.456

The same steps can also be applied to the HAH crashes. The results are shown in Table 10.2 (right). In this model, neither of the original intervention variable coefficients is significantly different from zero. The F-statistic is 0.54 for the hypothesis that *both* coefficients for the original advertising and enforcement variables are zero, and the resulting p value is 0.58. Hence, in the context of the regression model reported in RN74 for HAH crashes, it would likewise appear that

there is no real evidence for a quantitative effect of either drink drive advertising or drink drive enforcement.

#### Replications Using the Data Analysed by Cameron et al. (1993)

The analyses reported above have been replicated using the data analysed by Cameron et al. (1993), from which the original dose-response curves (shown here in sub-section 3.2) were derived. The same findings (not reported here in any detail) were obtained: there was no evidence that the countermeasure variables were effectively quantitative in the context of the regression models.

#### Other Statistical Investigations of the Status of the Countermeasure Variables

Two other approaches to investigating the status of the countermeasure variables were explored. The approach reported above is more rigorous than the other two. However, the other two are briefly described below.

In one approach, the non-zero values of each countermeasure variable were replaced by values from random normal distributions with the same means and SDs. It was found that the 'random number' versions of the countermeasure variables produced much the same results as the original versions.

In another approach, the periods for which the countermeasure variables had zero values were excluded from the analyses, such that the analyses were restricted to the last three, or so, years of the study period. Some decisions had to be made about starting dates, and about which control variables to include in the analyses. The restricted time-frame meant that the effects of seasonality and long-term trends could not reliably be estimated. For those, and other, reasons, that approach was not considered appropriate.

#### Conclusions

In contrast to the assumptions of Cameron et al. (1993), the re-analyses reported above have failed to demonstrate any evidence for a quantitative, or dose-response, relationship between any of the intervention variables and the crash variables, for either the LAH or the HAH crashes. That fact has two important implications.

First, the presence of a dose-response relationship would have provided important evidence that the previously reported associations between the intervention variables and response variables were not spurious. The data do not provide that evidence.

Second, it is very difficult to see how the data can justify a recommendation to increase levels of TV road safety advertising in the absence of any demonstrable dose-response effect.



## 11 A SIMPLE MODEL OF MELBOURNE CRASHES

### 11.1 *The Three Factor Theory*

Dr Michael Henderson, the Chairman of the Australian Advisory Committee on Road Trauma, has expressed serious doubts about the validity of claims made concerning the extent of crash savings that have resulted from the TAC-funded road safety TV advertising and enforcement campaigns (Henderson, 1997). With reference to the Victorian and New South Wales fatality data depicted in Figure 11.1.1, he observed (p. 3) that:

The 1989-1992 period showed a considerable drop in road deaths in Victoria. However, as is also shown, the drop in deaths in New South Wales from 1988 to 1991 was of the same order of magnitude.

He concluded (pp. 4-5) that:

Given the interstate comparison, it may be mistaken to over-estimate the short-term benefits of some specific and widely publicised [TAC] behavioural measures. It may also be a mistake to under-estimate the longer-term effects of the systematic, nationwide application of a national strategy and action plan that comprises a balanced mix of measures directed at human behaviour (including driver risk-taking) as well as the human/environment interface.

As noted earlier, Beenstock and Gafni (2000, p. 83) have hypothesised that the fall in the accident rate that occurred in Victoria in the late 1980s may simply have been the renewal of a long-term downward trend, which was temporarily reversed in the mid-1980s. They postulated that the return to the downward trend would have occurred without the intervention of the TAC-funded countermeasures. (As proposed below, it seems likely that the temporary increase in crash numbers in the mid-1980s was a consequence of the economic boom at the time.)

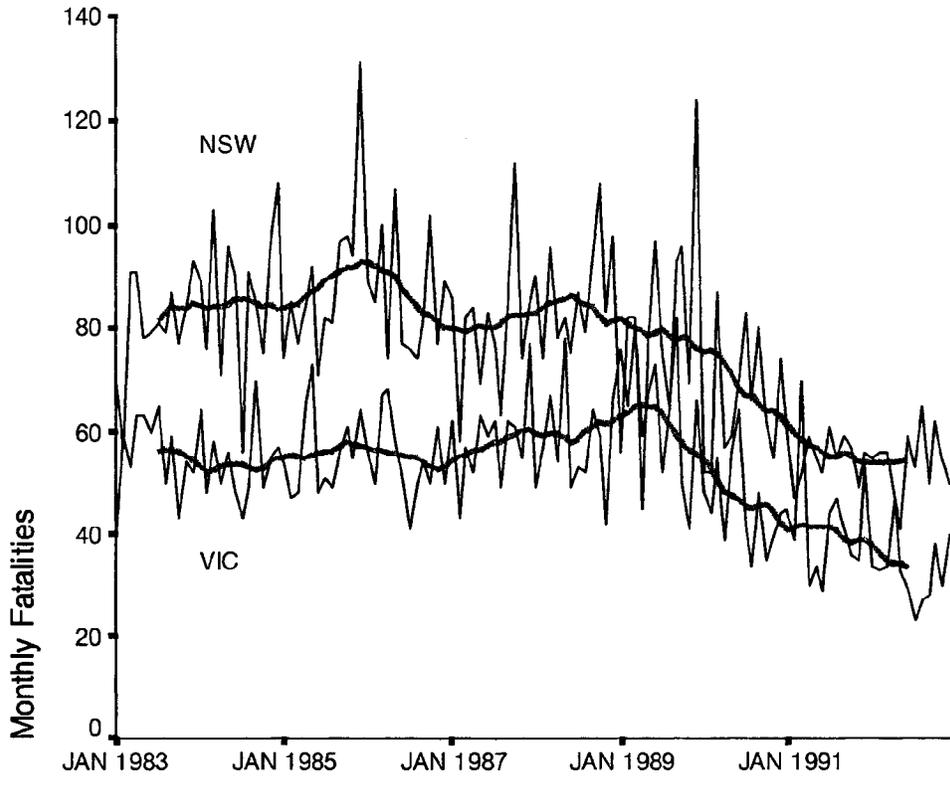
The three factor theory proposed here is broadly consistent with the above views of Henderson (1997) and Beenstock and Gafni (2000).

The three factor theory acknowledges that there has been a long-term downward trend in crash numbers in Australia since the early-1970s. (Australia is not unique in that respect.) The first of the three factors is therefore a linear trend term. This variable will account for gradual improvements in the road infrastructure and the vehicle fleet. This variable will also account for any gradual improvements in road user behaviour due to enforcement, advertising or education. It is expected that the sign of the trend term will be negative. The second factor is seasonality.

The three factor theory postulates that the upward deviation from the long-term downward crash trend in Victoria from 1983 to 1988 was due to a strongly improving economy, and that the sudden drop in crash numbers from 1989 to 1991 was due to a sudden deterioration in the economy. The third factor is therefore a measure of the economy. For reasons given in Section 8, that measure is the smoothed version of the Leading Index of Economic Indicators for Victoria.

Figure 11.1.1

Comparison of New South Wales and Victorian Monthly Fatalities 1983-1992. 12-Month Centered Moving Averages also Shown.



## 11.2 Applying the Three Factor Theory to Melbourne Crashes

It was shown in Section 8 that when the CMA12 Lead Index replaced Unemployment as the measure of the economy, the drink drive countermeasures failed to make any contribution to the HAH crash model, and the speed countermeasures made a much reduced contribution to the LAH crash model. And when the speed countermeasures were evaluated against *all* crashes, speed enforcement made no contribution to the model, while speed advertising made only a weak contribution.

A *single* model for all crashes would obviously be more parsimonious than the two RN74 models (for LAH and HAH crashes) that it would replace. Apart from the trend and seasonality variables, the single model has only *one* additional variable: CMA12 Lead Index. In comparison, the two RN74 models have a total of *six* additional variables: Unemployment, the four countermeasure variables, and Alcohol Sales.

A *simple* crash model could use raw-data versions of all the variables. The 1,0,0,0,0,0,0,0,0,0,0 coding for each of the 11 individual month variables, and the values of the trend variable (1 to 120), can be defined such that they are the same whether the model uses log-transformed or raw-data versions of the other variables.

Detailed results for the *single, simple*, regression analysis of all Melbourne serious casualty crashes are given in Table 11.2.1.

Table 11.2.1  
*Detailed results for the single, simple regression analysis of all Melbourne serious casualty crashes*

Predictors	B	SE	p
Constant (a)	-280.00	30.25	0.000
Trend	-2.15	0.10	0.000
CMA12 Lead Index	5.68	0.25	0.000
Feb	48.92	12.80	0.000
Mar	91.46	12.80	0.000
Apr	63.38	12.80	0.000
May	114.43	12.80	0.000
Jun	99.59	12.80	0.000
Jul	80.77	12.80	0.000
Aug	74.21	12.81	0.000
Sep	75.33	12.81	0.000
Oct	90.48	12.82	0.000
Nov	76.20	12.82	0.000
Dec	67.71	12.82	0.000

All the predictor variables are significant at the  $p = 0.000$  level. The model explains 87.4% of the crash variance.

The prediction equation for raw casualty crash numbers is:

$$\text{All Melbourne Serious Casualty Crashes} = a + b \cdot \text{Trend} + c \cdot (\text{CMA12 Lead Index}) + d \cdot \text{Feb} + \dots + n \cdot \text{Dec}$$

The value of the constant 'a' is -280.00. The unstandardised regression coefficients for all the independent variables (ie, b, c, d, etc. in the formula above) are given as 'B' values Table 11.2.1.

The closeness of the fit between observed and predicted crash numbers can be seen in Figure 11.2.1. This simple regression equation obviously provides a very satisfactory description of the Melbourne serious casualty crash data.

### The Role of Countermeasures Revisited

It is worth asking whether or not the countermeasure variables would have contributed to the simple, raw-data model had they been included. Raw-data versions of the speed TINs and speed TV Adstock variables were added individually to the other predictor variables in the simple raw-data model. Neither the enforcement (B = -0.00045, SE = 0.000; p = 0.180) nor the advertising variable (B = -0.0035, SE = 0.004, p = 0.321) made a significant contribution to the model. Raw-data versions of the drink drive RBT and drink drive TV Adstock variables were also added individually to the other predictor variables. Again, neither the enforcement (B = -0.00014, SE = 0.000; p = 0.679) nor the advertising variable (B = 0.00086, SE = 0.003, p = 0.780) made a significant contribution to the model.

### The Role of the Economy

A measure of the economy (the Leading Index of Economic Indicators) was selected that is one of two commonly used *global* measures of the status of the 'business cycle'. There is a sense in which such a measure is less arbitrary than one selected from amongst hundreds of other more *particular* measures of economic activity. Another reason for selecting this measure was, admittedly, because it peaked at much the same time as the peak in crash numbers. In that respect the measure is a product of 'data dredging', and the results of the regression analysis should be considered to be tentative until replicated against independent sets of crash data.

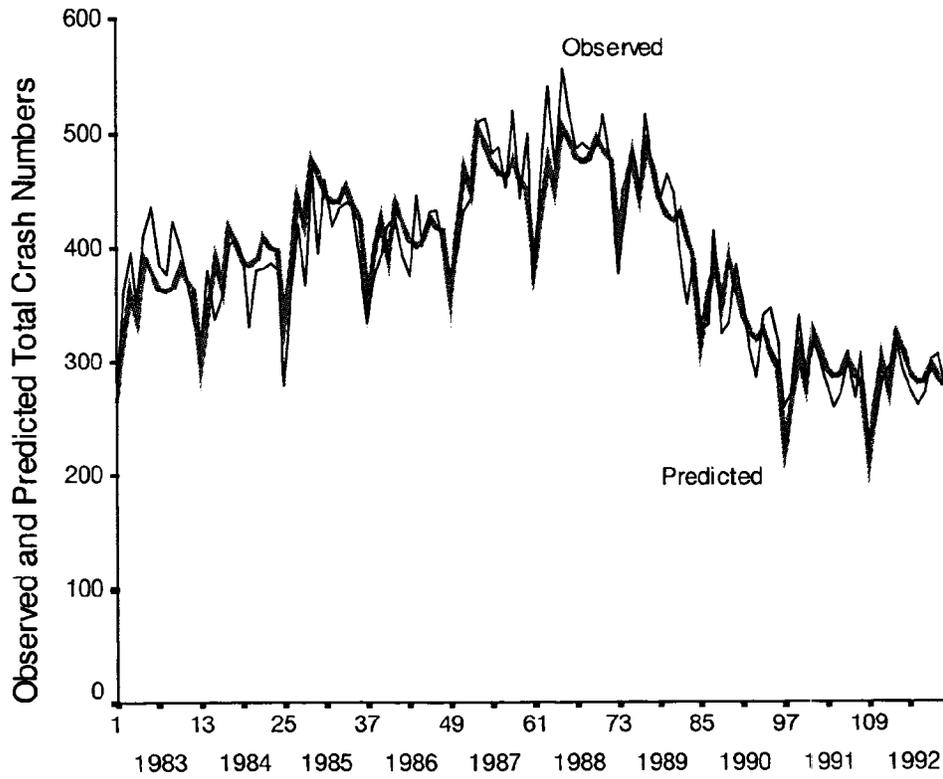
A two-stage regression analysis was performed on the raw crash data with the trend variable and the individual month variables (seasonality) entered in the first stage, and the economic variable included in the second stage. Trend and seasonality accounted for 27.6% of the total crash variance in stage 1, and inclusion of the CMA12 Lead Index in stage 2 accounted for an additional 59.8 % of total crash variance. As noted previously, the total variance explained was 87.4%.

It has been shown that when an indicator of economic conditions is chosen that peaks at much the same time as the Melbourne serious casualty crash series, and when that indicator is stripped of unnecessary micro-variation, the contribution of the economy to the simple, raw-data crash model is very substantial. Under those circumstances, countermeasures have no measurable effect on crash numbers.

As the Leading Index of Economic Indicators was not available at the State level before 1996 (Boehm, 1996), Cameron et al. (1993) and Newstead et al. (1995) cannot be criticised for not using it in their regression analyses.

Figure 11.2.1

Observed and Predicted Melbourne Serious  
Casualty Crashes





## 12 NEW ZEALAND REPLICATIONS

### 12.1 *The New Zealand SRSP and its Evaluation*

The Land Transport Safety Authority (LTSA) of New Zealand implemented a Supplementary Road Safety Package (SRSP) in October, 1995 (Cameron & Harrison, 1998; Graham, 1998). On the advice of MUARC consultants (Cameron, Vulcan, Haworth & Kent, 1994), the SRSP was closely modelled on the TAC-funded campaigns in Victoria. The positive findings from the MUARC evaluations of the effectiveness of the Victorian television advertising campaigns (Cameron et al., 1993; Newstead et al., 1995) provided a justification for the MUARC consultants to recommend high levels of television advertising to the New Zealand Government. The overall rationale for the SRSP was described by Graham (1998, p. 5) as follows:

[The SRSP] was intended to emulate the highly successful strategy employed since 1989 in Victoria, Australia, by the TAC and Police. Realistic, emotionally powerful and shocking advertising linked with increased strategic enforcement measures saw the Victorian road toll halved in the first five years of the campaign.

Graham (1998, p. 5) described the SRSP as comprising:

- improved targeting of speed cameras and compulsory breath testing (CBT)
- the use of laser speed detectors and additional breath testing equipment
- more police hours of speed enforcement
- sustained publicity to support the speed camera and CBT programs

Macpherson & Lewis (1998, p. 41) pointed out that, although the New Zealand Government succeeded in achieving high levels of road safety television advertising, they failed to introduce *hidden* speed cameras or *high-visibility* booze buses. Macpherson and Lewis thought it unlikely that the SRSP could achieve substantial crash reductions without any such radical changes to enforcement practices.

A further aspect of the SRSP (Graham, 1998, p. 6) was that there must be:

- independent evaluations of the safety outcomes achieved

In their role as private consultants, Mr Max Cameron and Professor Peter Vulcan were invited by the LTSA to conduct an independent review of the outcomes of the first two years of the SRSP. Using multiple linear regression analyses, Cameron and Vulcan (1998) concluded that the first two years of the SRSP had successfully reduced the numbers of serious road crash casualties in New Zealand.

### 12.2 *Replicating Victoria's Results in New Zealand*

Un-replicated results should be treated very cautiously. The apparent success of the New Zealand SRSP (Cameron & Vulcan, 1998) could be considered a replication of the MUARC findings for Victoria (e.g., Newstead et al., 1995) in favour of a TAC-type countermeasure program. However, from the re-analyses of the Victorian data presented in this report, it was concluded that the MUARC researchers had failed to properly demonstrate that the TAC-funded campaigns had any effect on Victorian

crash numbers. Given that failure, the positive evaluation by Cameron and Vulcan of the New Zealand SRSP might now seem anomalous; and should be re-examined.

A further reason for re-analysing the New Zealand crash data is to attempt to replicate the successful performance of the three factor model in Victoria (see Section 11) against an independent set of data.

The two aims of this section of the report are, therefore: (1) to re-investigate the analyses conducted by Cameron & Vulcan (1998) from which they concluded that the SRSP had successfully reduced road crash casualties in New Zealand; and (2) to see if the New Zealand casualty data can adequately be described in the terms of the three factor model.

### 12.3 Including a Linear Trend Variable in Time-Series Crash Models

Road crash fatalities have gradually decreased since about 1970 in most of the developed countries of the world (e.g., Beenstock & Gafni, 2000; Broughton, 1991; Oppe, 1991a, 1991b; van Beeck, Borsboom & Mackenbach, 2000; Wang, Hensher & Ton, 1999). Models of crash numbers would normally be considered deficient if they did not make some allowance (e.g., by including a linear trend variable) for the underlying causes of the incremental improvements in road safety. Recent MUARC modelling of Victorian crashes has involved the use of a trend variable to account for the underlying causes of gradual improvements in road safety. Beenstock and Gafni (2000, p. 71) note that those researchers who are not interested in studying the downward crash trend for its own sake, treat the trend “as a nuisance parameter that happens to be essential for model fitting”.

Figure 12.3.1 shows that annual road crash fatalities in New Zealand peaked in 1973. Fatalities have not fallen as steadily in New Zealand as in some other countries. Superimposed on the overall downward trend is a sharper fall in the mid- to late-1970s, followed by a rise in the early- to mid-1980s. A further point to note is the very consistent fall in fatalities over the last thirteen years (from 1987 to 1999).

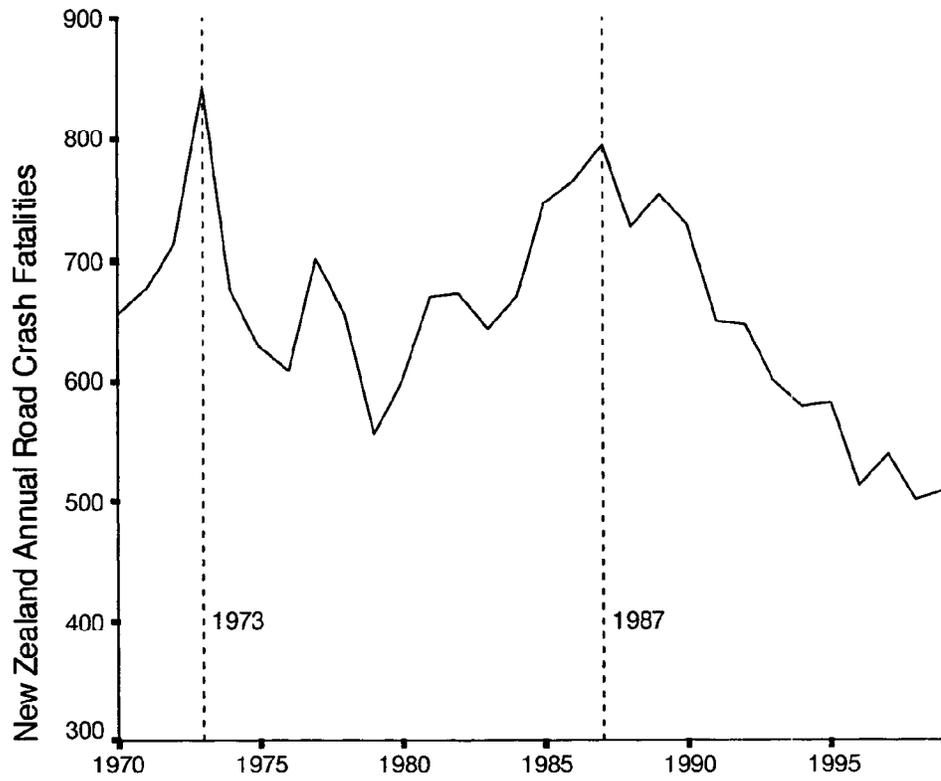
Cameron & Vulcan (1998, p. 11) argued in favour of including a trend variable in the crash models they developed to evaluate the effectiveness of the SRSP:

While it is likely that on-going road improvements including road safety engineering initiatives have made a significant contribution to the downward trend in road trauma, their gradually increasing effect (relatively small changes in any one year) made them unsuitable for *explicit* inclusion in the model. Instead, the effects of road improvements and other smoothly increasing road safety factors (such as improved vehicle design) were considered likely to be represented by the *trend component* of the model. (*Italics added.*)

Here, Cameron & Vulcan (1998) are presenting the sound, conventional argument that a trend term should be included in a time-series crash model to allow for the effects of the gradual improvements in road safety that have occurred in most developed countries since about 1970 (and which, from Figure 12.3.1, would seem to be very evident in New Zealand, especially over the thirteen-year period from 1987 to 1999). However, despite the above argument, Cameron and Vulcan omitted the trend variable from their New Zealand analyses, for reasons discussed below.

Figure 12.3.1

New Zealand Road Crash Fatalities from 1970 to 1999



## 12.4 Cameron and Vulcan's (1998) Evaluation of the SRSP

Cameron and Vulcan's (1998) evaluation of the SRSP comprised two main regression analyses, as well as a number of subsidiary and complementary regression analyses. Only the two main analyses are described and investigated here. First, there is a description of the variables involved.

### Dependent and Independent Variables

All variables were measured at quarterly intervals over the seven-and-a-half year period from the first quarter of 1990 to the second quarter of 1997, giving 30 quarterly values for each variable. The study period encompassed the introduction of CBT in the second quarter of 1993. The first two years of the SRSP (1995/96 and 1996/97) occurred at the very end of the period.

The values of the quantitative variables were subjected to a log-transformation (to base 'e'). The reasons for that procedure relate to the conversion of a multiplicative Serious Casualty equation to an additive version suitable for multiple linear regression analysis.

The dependent variable was:

- Ln(Serious Casualties) (the natural log of deaths and serious injuries)

The independent variables included in both regression models were:

- Q2, Q3 and Q4 (dummy variables for each of the last three quarters of the calendar year, representing seasonal factors)
- CBT, a dummy variable representing the introduction of Compulsory Breath Testing in the second quarter of 1993, and its continuation to Q2 1997
- SRSP-95/96, a dummy variable representing the first year of the SRSP
- SRSP-96/97, a dummy variable representing the second year of the SRSP

A further single independent variable was included in each model to allow for the effects of the economy on crash numbers. The two models differed only in the identity of the economic variable. The two economic variables were:

- Ln(New Cars), the natural log of the number of new cars registered
- Ln(Unemployment), the natural log of the unemployment rate

Time-series plots of the un-transformed values of the quantitative variables (Serious Casualties, New Cars and Unemployment) are provided in Figures 12.4.1, 12.4.2 and 12.4.3.

Figure 12.4.1 shows how the numbers of New Zealand serious road crash casualties have changed over the period from the first quarter in 1990 to the second quarter in 1997. While there were some deviations, there was an overall decrease in casualties over the seven-and-a-half year period. Figure 12.4.1 also identifies the introduction of CBT in the second quarter of 1993. The first (1995/96) and second (1996/97) years of the SRSP are also identified.

Figure 12.4.1

Quarterly New Zealand Road Crash Casualties from Q1 1990 to Q2 1997. Compulsory Breath Testing (CBT) Commenced in Q2 1993. The First and Second Years of the Supplementary Road Safety Package (SRSP) are 1995/96 and 1996/97.

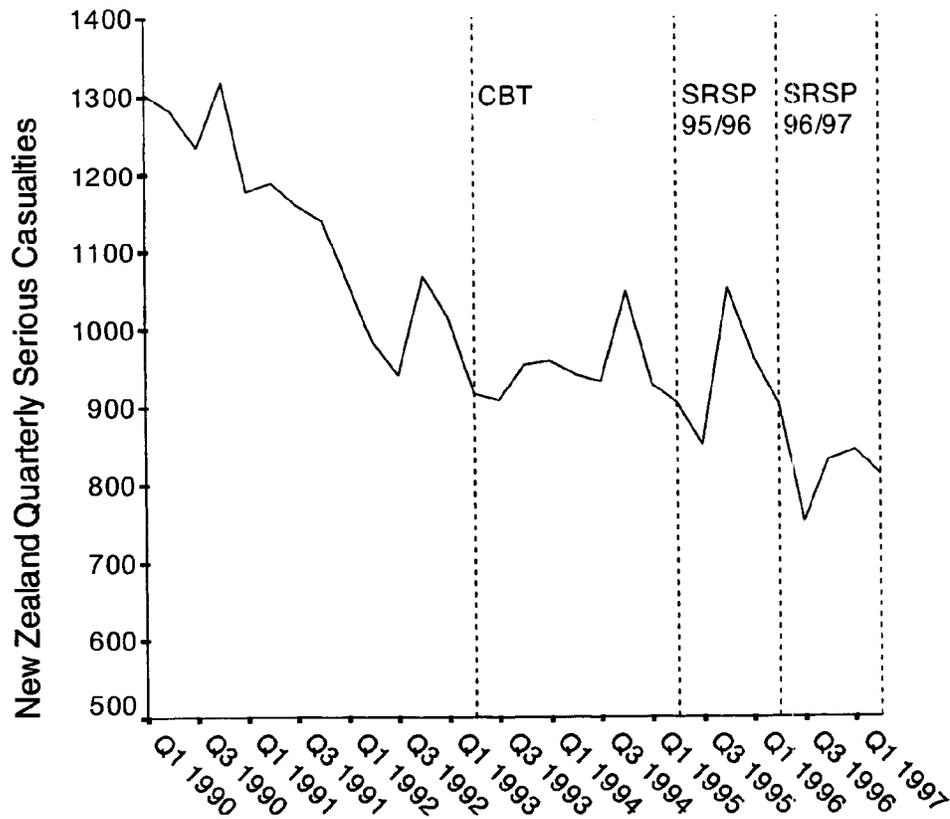


Figure 12.4.2

Quarterly New Zealand New Car Registrations from Q1 1990 to Q2 1997

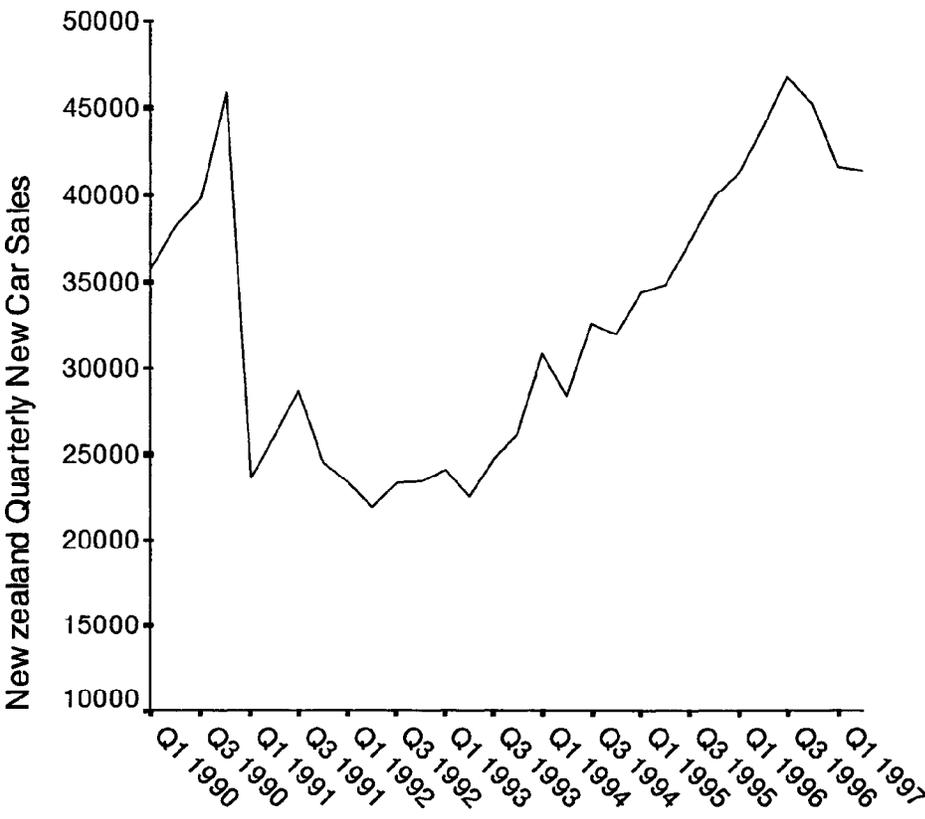


Figure 12.4.3

Quarterly New Zealand Unemployment Rate from Q1 1990 to Q2 1997



Figure 12.4.2 shows how the numbers of new car registrations have changed over the same period. After a sharp fall in 1991, there was a steady recovery from 1993 to 1996. Figure 12.4.3 shows how New Zealand unemployment rates have changed over the same period. After a sharp increase in 1991, there was a steady decrease from 1993 to 1996. It should be noted that Figures 12.4.2 and 12.4.3 describe similar trends in the New Zealand economy. (If the unemployment graph was inverted its shape would be very similar to that for new car registrations.)

It should again be noted that the actual variables used in Cameron and Vulcan's (1998) regression analyses were the log transformed versions of the three quantitative variables described above.

One potential independent variable was excluded by Cameron and Vulcan (1998) from the regression models:

- Trend, a variable that took the values 1, 2, 3 etc. up to the total number of quarters for which the data was analysed. The range of Trend values was, therefore, from 1 to 30.

Arguments for and against excluding the trend variable will be considered later.

### Two Regression Equations

The basic regression equation is:

$$\text{Ln(Serious Casualties)} = \text{Intercept} + b*Q2 + c*Q3 + d*Q4 + e*CBT + f*SRSP - 95/96 + g*SRSP-96/97 + h*\text{Ln(economic variable)}$$

There were two regression equations: one with Ln(New Cars) as the economic variable, the other with Ln(Unemployment).

### Replication of Cameron and Vulcan's Findings

From here on, the term 'log' will not explicitly be used when referring to the quantitative variables in the regression models.

As mentioned above, two similar multiple linear regression analyses were performed by Cameron and Vulcan (1998): one used New Cars as the economic variable; the other used Unemployment. Nearly perfect replications were achieved for both analyses. The reasons for the small differences between the original and replicated analyses are not known. It is possible that some values of some variables were revised before being provided to the authors of this report. However, the differences between the original and replicated analyses are so small that they could be of no consequence for the arguments that follow.

Results for the major variables in the two regression models are presented below. Results are not presented for the intercept, or for the three dummy variables, Q1, Q2 and Q3 (which together represent the effects of seasonality). Table 12.4.1 presents the results for the model that used New Cars as the economic variable, and Table 12.4.2 for the model that used Unemployment.

Table 12.4.1

*Regression coefficients (B), their SEs, and associated t values and probabilities (p) for the main predictor variables in the Serious Casualties regression model which used New Cars as the measure of the economy*

Predictor Variable	B	SE	t	p
CBT	-0.196	0.026	-7.534	.000
SRSP-95/96	-0.100	0.041	-2.438	.023
SRSP-96/97	-0.270	0.043	-6.255	.000
New Cars	0.290	0.061	4.768	.000

Table 12.4.2

*Regression coefficients (B), their SEs, and associated t values and probabilities (p) for the main predictor variables in the Serious Casualties regression model which used Unemployment as the measure of the economy*

Predictor Variable	B	SE	t	p
CBT	-0.227	0.037	-6.210	.000
SRSP-95/96	-0.084	0.055	-1.542	.137
SRSP-96/97	-0.221	0.052	-4.245	.000
Unemployment	-0.278	0.110	-2.517	.020

From Table 12.4.1, it can be seen that all four predictor variables made significant ( $p < .05$ ) contributions to the 'New Cars' model. The total amount of variance in Serious Casualties accounted for by the model was 87.7%. Working from these findings, Cameron and Vulcan (1998, p. 30) concluded that the SRSP was responsible for a 9.5% reduction in serious casualties in 1995/96, and a 23.8% reduction in 1996/97.

From Table 12.4.2, it can be seen that three of the four predictor variables made significant ( $p < .05$ ) contributions to the 'Unemployment' model. The SRSP failed to make a significant contribution in 1995/96. The total amount of variance in Serious Casualties accounted for by the model was 80.6%. Working from these findings, Cameron and Vulcan (1998, p. 30) concluded that the SRSP was responsible for a 8.2% reduction in serious casualties in 1995/96 (despite the non-significance of the SRSP-95/96 variable in this model), and a 20.1% reduction in 1996/97.

From their overall findings, Cameron and Vulcan (1998, p. 30) concluded that "The results are consistent with a road safety initiative which needed to develop during its first year and apparently became more effective during its second year".

Cameron & Vulcan (1998, p. ii) estimated that the first two years of the SPSR saved 109 lives and 1029 serious injuries. According to Graham (1998, p. 7), total savings amounted to a social benefit of at least \$611 million; and a benefit:cost ratio of more than 28:1.

## 12.5 Re-Analyses of the New Zealand Data

### Cameron and Vulcan's Omission of the Linear Trend Term

It was noted above that Cameron and Vulcan (1998) omitted the trend variable from their New Zealand models (despite having provided a good argument for its inclusion). Their main argument (p. 19) for omitting the trend variable was probably that:

During the initial modelling, the trend function appeared to display a strong negative exponent. The analysis revealed that the negative trend apparent in the data may have been an artefact of reductions in road trauma levels due to the introduction of CBT and to the effects of the SRSP.

Cameron and Vulcan (1998) seem to be claiming that there were no genuine, incremental improvements in road safety over the seven-and-a-half year period from 1990 to mid-1997. They propose, instead, that the effects of the introduction of CBT, and the effects of the first two years of the SRSP, were the only factors operating over that period to improve road safety.

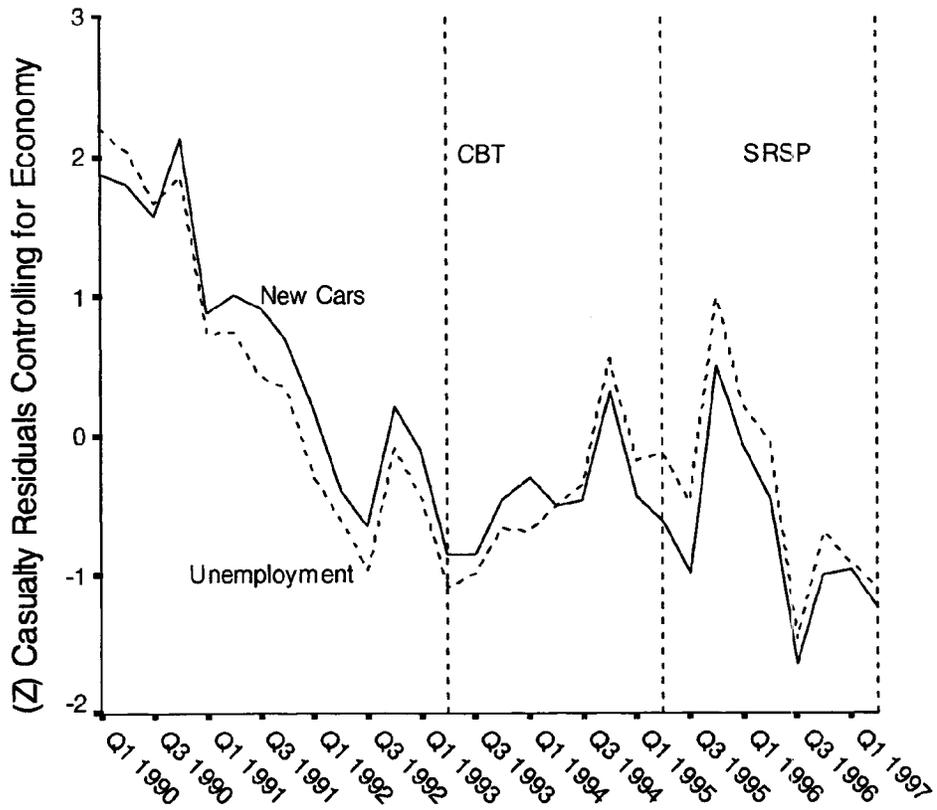
That argument is not very plausible. As can be seen from Figure 12.3.1, road crash fatalities had been falling for about six years prior to the introduction of CBT in mid-1993. Similarly, from Figure 12.4.1, it can be seen that serious road crash casualties had been falling sharply for more than three years before the introduction of CBT. It would seem, contrary to the view of Cameron and Vulcan (1998), that factors such as road and vehicle improvements, and possibly gradual improvements in driver behaviour, were operating to enhance road safety in New Zealand during the early and mid 1990s. Most of those factors would certainly have been operating independently of the CBT and SRSP programs. The inclusion of a trend term would be necessary to account for the effects of such gradual improvements in road safety.

In 1985, the New Zealand government undertook a major, long-term blackspot identification and treatment program. In 1989, a thorough monitoring system for the program was developed. An evaluation of the program (Kraus, 1992) concluded that about 2,400 crashes had been eliminated in the first 30, or so, months of its operation. That program has continued through the 1990s. This example illustrates how inappropriate it would be to overlook the gradual, incremental improvements in road safety that occurred in New Zealand in the early 1990s.

In defending their contention that the only *real* improvements in road safety during the 1990s were due to the introduction of CBT in 1993, and the implementation of the SRSP in 1995, Cameron and Vulcan (1998) are faced with the apparently contradictory fact that casualty numbers fell sharply during 1990, 1991 and 1992 (which was well before the CBT and SRSP initiatives were implemented). However, Cameron and Vulcan could, possibly, have argued that the downward trend over *those three years* was caused by deteriorating economic conditions at that time.

Figure 12.5.1

Standardised (Z values) Quarterly New Zealand Casualty Numbers, Controlling for the Effects of an Economic Variable (either New Cars or Unemployment)



To test that possibility, it would be necessary to see if casualty numbers in the early 1990s still dropped after the effects of economic factors were taken into account. That can be done by examining the Serious Casualty residuals that remain after statistically removing the effects of the economy. The results of that procedure, using either New Cars or Unemployment as a measure of the economy, are presented in Figure 12.5.1.

From Figure 12.5.1, it can be seen that road crash casualties were falling in the early 1990s even after accounting for the effects of economic factors. It would seem that there *was* a genuine improvement in road safety at the time. The appropriate way of accounting for the influence of such factors is to include a trend term in the regression models.

By omitting the trend term, Cameron and Vulcan (1998) have failed to take account of the underlying, incremental improvements in road safety over the full study period.

It would therefore be interesting to see what effects the inclusion of a linear trend variable would have on the results of the two regression analyses.

#### Including a Linear Trend Variable in the Two Regression Analyses

Tables 12.5.1 and 12.5.2 present results for the two regression analyses with the linear trend variable included.

Table 12.5.1

*Regression coefficients (B), their SEs, and associated t values and probabilities (p) for the main predictor variables in the Serious Casualties regression model which used New Cars as the measure of the economy. The trend variable is now included.*

Predictor Variable	B	SE	t	p
CBT	-0.042	0.038	-1.102	.283
SRSP-95/96	0.017	0.039	0.436	.667
SRSP-96/97	-0.093	0.049	-1.874	.075
New Cars	0.205	0.048	4.306	.000
Trend	-0.014	0.003	-4.618	.000

From Table 12.5.1, it can be seen that the CBT and SRSP variables fail to make significant ( $p < .05$ ) contributions to the 'New Cars' model when the linear trend variable is included. In contrast, Trend and New Cars both make highly significant ( $p < .000$ ) contributions. The total amount of variance in Serious Casualties accounted for by the revised model is 90.7% (3.0% higher than for the original model).

From Table 12.5.2, it can be seen that the CBT and SRSP variables failed to make significant ( $p < .05$ ) contributions to the 'Unemployment' model. Again, Trend and Unemployment both make significant contributions. The trend variable is *highly* significant. The total amount of variance in Serious Casualties accounted for by the revised model is 91.2% (10.6% higher than for the original model).

Table 12.5.2

*Regression coefficients (B), their SEs, and associated t values and probabilities (p) for the main predictor variables in the Serious Casualties regression model which used Unemployment as the measure of the economy. The trend variable is included.*

Predictor Variable	B	SE	t	p
CBT	-0.029	0.047	-0.618	.543
SRSP-95/96	0.048	0.046	1.046	.307
SRSP-96/97	-0.025	0.053	-0.471	.643
Unemployment	-0.198	0.077	-2.551	.019
Trend	-0.017	0.003	-5.049	.000

The analyses reported in Tables 12.5.1 and 12.5.2 were both repeated three times, but incorporating only one of the three 'campaign' variables each time. This procedure addresses the question of whether any *single* campaign variable, considered independently of all the other campaign variables, might have reduced casualty numbers. For the 'New Cars' model, CBT on its own made no contribution ( $p = .956$ ). The first year of the SRSP on its own made a significant *positive* contribution ( $p = .015$ ); while the second year of the SRSP on its own made a significant negative contribution ( $p = .008$ ) of about the same magnitude. So, overall, the conclusion would have to be that the campaigns had made no difference to casualty numbers. For the 'Unemployment' model, CBT on its own made no contribution ( $p = .643$ ). The first year of the SRSP on its own made no contribution ( $p = .581$ ); while the second year of the SRSP on its own made a marginally significant *positive* contribution ( $p = .052$ ). Again, the conclusion would have to be that the campaigns had failed to reduce casualty numbers.

In deliberating on their (unreported) findings for the analyses provided here in Tables 12.5.1 and 12.5.2, Cameron and Vulcan (1998, p. 19) said that:

It was decided to exclude the trend function from the final models. This did not result in any deterioration in the *level* of explanation afforded by the models. (*Italics added.*)

Here Cameron and Vulcan (1998) are referring to the fact that the exclusion of the trend variable made little difference to the *total amount* of variance explained. What they failed to mention was that the exclusion of the trend variable radically changed the *pattern* of results. When the trend variable is included in the regression models, the campaign effects fail to materialise.

#### A Second Argument by Cameron & Vulcan for Omitting the Trend Variable

It has not been an easy task to understand Cameron and Vulcan's (1998) justifications for excluding the trend variable. There seems to be a second argument implicit in the following quotes:

Given the apparent growth in the socio-economic system in New Zealand during the 1990s, and attendant growth in road use, it was expected that the underlying trend would be positive. However,

this growth may be tempered by improvements in road trauma rates per kilometre. (p. 18)

It could be concluded ... that the on-going systematic improvements to the road system in New Zealand approximately just offset the general increases in road use, so far as road trauma levels are concerned. (p. 19)

Here, Cameron and Vulcan (1998) seem to be saying that there were two equal and opposite forces that cancelled each other's effects. Gradual improvements to the road system were tending to decrease casualty numbers; while a gradually improving economy was having the opposite effect. If that were so, it might be an argument for excluding *both* variables, but it is certainly not a justification for excluding only the trend variable. However, the trend and economic variables have such different shapes over the full study period that there is really no possibility that they could cancel each other. The only appropriate course of action is to include both variables in the regression models.

### *12.6 Replicating the Three Factor Theory in New Zealand*

In its most basic formulation, the three factor theory is quite uncontroversial. It simply points out that three well-recognised background factors should always be taken into account when modelling time-series crash data. Those factors are: (1) long-term, incremental improvements in the safety of the road environment and vehicles (and possibly also gradual improvements in road user behaviour); (2) seasonality; and (3) major changes in economic conditions. The operationalisation of each of those factors will require some judgement. For example, it might be appropriate for the measurement of economic conditions to be in terms of a broad indicator of the business cycle rather than in terms of some more specific aspect of economic activity. As specified for Victoria (see Section 11), the three factor model used raw-data versions of all quantitative variables; and used the Leading Index of Economic Indicators as the measure of the economy.

The Leading Index for the whole of New Zealand is provided by the National Bank of New Zealand (Rae, 1996). The shapes of the Leading Index and the two economic variables used by Cameron and Vulcan (1998) are compared in Figure 12.6.1. To facilitate this comparison, the Unemployment variable was inverted, and the values of all three variables were standardised using Z-scores.

The three factor model for New Zealand incorporated a linear trend variable to account for gradual improvements in road safety, three dummy variables to account for seasonal factors, and the New Zealand Leading Index to account for changing economic conditions. Raw-data values were used for all quantitative variables.

Results are presented in Table 12.6.1. The trend variable made a highly significant negative contribution to the model, as expected. However, there was little effect of seasonality (Q2, Q3 and Q4). The Leading Index made a highly significant positive contribution to the model, as expected. The total variance explained was 88.8%, which is higher than for either of the Cameron and Vulcan (1998) models (see Tables 12.4.1 and 12.4.2).

Figure 12.6.1

A Comparison of Trends from 1990 to 1997 in the New Zealand Leading Index (NZLI), New Cars and Unemployment (Inverted). The Raw Values Have Been Converted to Z Scores.

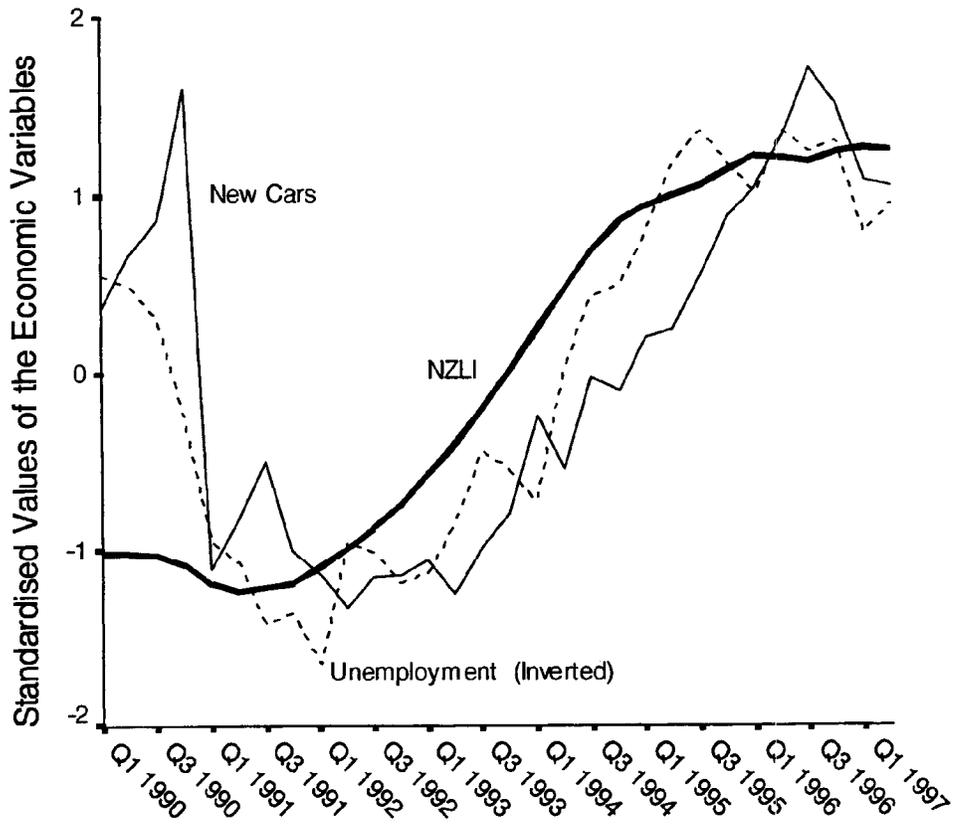


Table 12.6.1

*Regression coefficients (B), their SEs, and associated t values and probabilities (p) for the predictor variables in the three factor model for Serious Casualties*

Predictor Variable	B	SE	t	p
Trend	-29.276	4.074	-7.186	.000
Q2	-19.564	28.108	-0.696	.493
Q3	-59.846	29.037	-2.061	.050
Q4	48.356	29.097	1.662	.110
Leading Index	15.289	4.260	3.589	.001

The closeness of the fit between observed and predicted quarterly serious casualty numbers can be seen in Figure 12.6.2. The three factor model provides a satisfactory description of the New Zealand data.

#### Additional Explanatory Power of CBT and the SRSP

The possibility should be explored that CBT and the SPSP explain some additional casualty variance, over and above the variance explained by the three factor model. Results for the extended model are given in Table 12.6.2.

Table 12.6.2

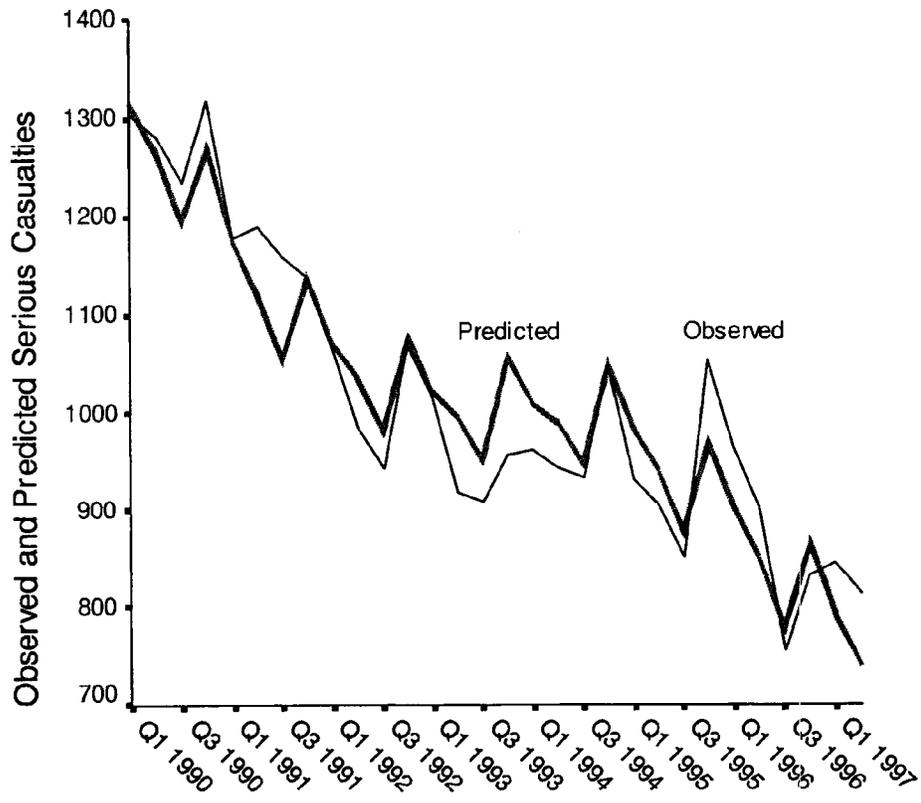
*Regression coefficients (B), their SEs, and associated t values and probabilities (p) for the predictor variables in the extended three factor model for Serious Casualties (which now incorporates the effects of CBT and the SRSP)*

Predictor Variable	B	SE	t	p
Trend	-31.794	3.808	-8.349	.000
Q2	-9.143	23.231	-0.394	.698
Q3	-55.655	24.089	-2.310	.031
Q4	52.057	23.769	2.190	.040
Leading Index	19.338	4.520	4.278	.000
CBT	-80.099	46.475	-1.723	.099
SRSP-95/96	80.423	35.637	2.257	.035
SRSP-96/97	62.612	41.431	1.511	.146

The only countermeasure variable to make a significant contribution (at the 0.05 level) to the model is the first year of the SRSP. However, its contribution is positive, indicating that the first year of the SRSP had the effect of *increasing* casualty numbers. That result is unlikely. The most appropriate interpretation of the data is that the three factor model provides a good account of the casualty data; and that the countermeasures make no further contribution to the model.

Figure 12.6.2

New Zealand Serious Casualties: Observed; and as Predicted by the Three Factor Model



## 12.7 Discussion

### Two Evaluations of the Drink Drive Component of the SRSP

The effectiveness of the drink drive component of the SRSP was evaluated by Macpherson and Lewis (1998). They pointed out that crash numbers were falling in New Zealand well before the SRSP was implemented; and considered that the crash data were insufficient to support a rigorous crash-level evaluation. They argued that the most appropriate outcome against which to evaluate the drink drive campaign was the number of 'evidentiary breath tests' (EBTs) administered following a positive breath sample by a driver on an alcohol screening device. From their analyses of EBT trends, they concluded that the drink drive component of the SRSP had *not* been successful.

Tay (1999) re-analysed Macpherson and Lewis's (1998) EBT data, but came to the opposite conclusion. However, results presented by Tay showed that the rate of EBTs per thousand alcohol screening tests was actually *higher* after the campaign—an indication that drink drive offences had increased, rather than decreased. Tay's conclusion that the drink drive campaign was a success must therefore be seriously questioned.

### The Sign of the Coefficient of the Trend Variable

Arguments presented in this section prescribe a *negative* sign for the coefficient of the trend variable. The sign was negative, as expected, for the results of the Victorian and New Zealand crash models which embodied the three factor theory. However, the sign was positive in the analyses of Victorian crashes conducted by Cameron et al. (1993) and Newstead et al. (1995). White (2000) has argued that the positive sign obtained by the MUARC researchers was a consequence of their models being mis-specified with respect to the identity of the economic variable.

### Conclusions

The re-analyses reported in this section indicate that Cameron and Vulcan (1998) failed to provide any adequate evidence that the first two years of the SRSP reduced road crash serious casualty numbers in New Zealand.

The New Zealand casualties were satisfactorily described by the three factor model; thus providing a replication of the model against an independent set of crash data.

## 13 DISCUSSION AND RECOMMENDATIONS

### 13.1 Summary of Main Findings

Over the ten-year period from 1983 to 1992, monthly numbers of serious casualty crashes in Melbourne first increased gradually, with some irregularities, for about six years, peaked, then fell dramatically within a period of about two years, before levelling out and remaining at a low level until the end of 1992. The statistical analyses of these crash trends reported in RN74 by Newstead et al. (1995) found strong contributions to the crash reductions from TAC-funded police enforcement and road safety TV advertising. Weaker contributions were found from the deteriorating economy. Supporting research by Cameron et al. (1993) provided evidence that high levels of road safety TV advertising were cost effective.

It is commonly believed that the *turn-around* in Melbourne crash numbers in the late 1980s was due to the implementation of the TAC-funded countermeasures. That belief is false. From time-series plots of crash numbers and countermeasure levels, it was shown that the enforcement and advertising campaigns were launched too late to explain the turn-around in monthly crash numbers.

Regression modelling can be valid only if certain assumptions are met. A major assumption is that the model is '*completely specified*'. That involves the identification and correct operationalisation of *all* the relevant predictor variables. The arbitrary nature of the RN74 models is illustrated by: the exclusion of important speed countermeasures from the HAH crash model; the exclusion of night-time crashes from the model that evaluated the speed countermeasures; and the questionable inclusion or operationalisation of a number of the predictor variables.

If the RN74 regression models for Melbourne had been correctly specified, the conclusions concerning the contributions of the countermeasures to fluctuating crash numbers would be robust in the face of rational changes to the definitions of the countermeasure variables. Various such changes were explored; and it was found that the models were far from robust. For example, when a smoothed version of the Leading Index of Economic Indicators was substituted for Unemployment in the LAH and HAH crash models, it was found that most of the crash variance that was previously explained by the enforcement and advertising variables was now able to be explained by fluctuations in the economy. Another example should be mentioned. When a version of the drink drive television advertising variable was used that omitted VicRoads advertising in November 1989, it was found that the advertising variable made no contribution to the HAH crash model. Newstead et al's (1995) estimates of the sizes of crash reductions that are attributable to various countermeasures should be considered very unreliable.

Regression models used by Cameron et al. (1993) identified countermeasure effects that were quantitative in the sense of implying dose-response relationships between advertising levels and the crash numbers. Those inferred quantitative effects are central to their conclusion that there are benefits to be obtained from very high levels of television advertising. Re-analyses presented in this report do not provide a basis for concluding that quantitative countermeasure effects are present. The provision of advice in favour of high levels of road safety TV advertising, cannot, therefore, be justified.

The methods employed for the selection of variables in the RN74 modelling process can be described as 'data dredging' (Eccleston, 2000). As such, the models should be considered to be 'hypothesis generating' rather than 'hypothesis testing'. While such models may appear to provide accurate *descriptions* of the past, they should not be taken to provide proven *explanations* of the past; and they should certainly not be considered to provide *predictions* of the future, either for the place they were developed, or elsewhere. Accordingly, the results of the RN74 regression analyses provide a most inadequate basis for recommending the implementation of TAC-type countermeasures.

A simple, 'three factor' model was proposed by the authors of this report to explain Melbourne total (i.e., LAH + HAH) serious casualty crash trends from 1983 to 1992. The factors were: (1) a long-term downward linear trend; (2) seasonality; and (3) the smoothed Leading Index of Economic Indicators. The model used raw-data versions of the quantitative variables (rather than log-transformed versions, as used by the MUARC researchers). The model parsimoniously explained crash trends over the study period. By selecting a measure of the economy that peaked at much the same time that crashes peaked, an excellent description of crash trends was obtained that did not involve any reference to the TAC-funded enforcement or advertising campaigns.

In a replication of the three factor model in New Zealand, quarterly numbers of road crash casualties, from 1990 to mid-1997, were satisfactorily described. The same casualty trends had previously been analysed by Cameron and Vulcan (1998) as part of an evaluation of the New Zealand SRSP. However, Cameron and Vulcan failed to include a trend variable in their model. When it was included, there was no longer any evidence that the SRSP had contributed to the fall in crash numbers.

### *13.2 Large, Ephemeral v Small, Cumulative Countermeasure Effects*

The RN74 regression models were derived from a conceptual model known as the 'Road Trauma Train' (Thoresen et al., 1992, pp. 6-7, and Figures 1 & 2 following p. 7). That model accounts for the crashes occurring in any one month in terms of causes operating *in that month* (with the possible exception of advertising, when measured in terms of Adstock, where there is some carry-over effect of the previous months' advertising). The model fails to capture any long-term, incremental effects of enforcement or advertising.

As background to this issue, it might be instructive to compare two views on how health-promotion advertising works. The view that advertising does *not* produce its effects immediately is illustrated in the following anecdote (Chapman, 1999):

[I am opposed to the view that] campaigns work in analogous ways to persuasive drugs being injected into populations—with evaluators standing by weeks or months later to count the successes. I was exposed to school anti-smoking campaigns in the 1960s. I stopped smoking after university when I was about 26. Like most ex-smokers, I attribute my decision to stop to a complex imbroglio of reasons, crowned by an awareness that smoking was very unhealthy. Had my teenage anti-smoking lessons been evaluated, I would have counted as one of their many failures. Yet plainly, I had become infected with concerns about smoking that

remained top-of-mind in my assessment of why I quit. Demands that campaigns demonstrate quick, quantifiable results risk losing the understanding that can come from qualitative research into the complexity of how the decision to quit evolves.

Much the same comments could presumably be made about the effects of road safety TV advertising on decisions not to drink drive, or not to speed.

However, even if it were the case that TV viewers responded immediately to road safety advertisements and started to drive more carefully, the question would have to be asked, how long it would take for the crash that would otherwise have happened, to be avoided. Crashes are rare events. A person who regularly drove after drinking, or who regularly drove above the speed limit, might stay accident-free for months or years. It seems unlikely that any conversion to safer driving attitudes as a consequence of viewing road safety advertisements would translate into a crash saving in the same month that the advertisements were viewed. The complementary question is, how long it would take after *not* seeing a road safety advertisement for the crash that would otherwise have been avoided, to occur. Even if attitudinal recidivism was immediate, the resulting crash might not occur for a number of months, or years.

The view expressed above is that TV advertisements do *not* immediately translate into crash savings. In contrast, the view of Newstead et al. (1995) is presumably much the same as that of Harper (1992, p. 209)—which is that “Road safety is an impulse decision that requires constant, high, top-of-mind product promotion to penetrate and motivate behaviour”. That view of advertising is unrealistic in postulating immediate crash savings; and is unduly pessimistic in its failure to acknowledge that long-term, incremental learning can take place.

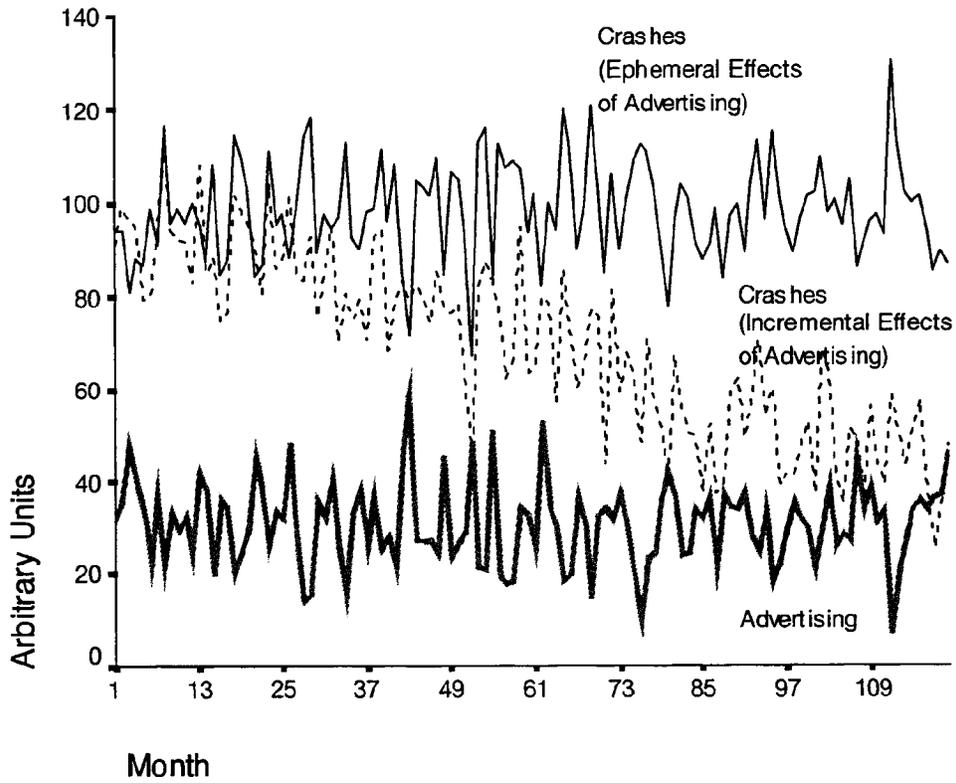
The inadequacies of the Thoresen et al. (1992) conceptual model can be clarified by considering the hypothetical scenarios depicted in Figure 13.2.1. An on-going situation is described where road safety TV advertising is maintained at the same average level, but with considerable month-to-month variation around that average. (Although ten years of data are depicted here, it is assumed that this situation is on-going, and that the pattern of advertising was the same prior to this ten-year period.) Two very different scenarios are depicted. In the ‘ephemeral effects’ scenario, advertising has a very strong, immediate effect on crash numbers, but the average level of crashes remains the same. For the data used here (with ‘Advertising’ as the only independent variable), an RN74-type regression model detects a *strong* advertising effect (R Square = 0.768; Beta = -0.876; p = 0.000).

In the ‘incremental effects’ scenario, advertising again (let us assume) has a very strong effect on crash numbers. In this scenario, although there is no immediate effect of advertising on crashes, there is a long-term, gradual, incremental, cumulative effect of advertising, such that the average level of crashes reduces steadily. However, in this situation, the RN74-type regression model fails to detect *any* effect of advertising on crashes (R Square = 0.000; Beta = 0.015; p = 0.873).

It can be seen from these two hypothetical scenarios that RN74-type regression models will detect strong, immediate, ephemeral effects of countermeasures on crash numbers (should they exist); but they will not detect the types of effects that one might hope for: long-term, incremental, possibly delayed effects.

Figure 13.2.1

Hypothetical Example of Different Types of Advertising Effect on Crash Reductions: An Instantaneous, Ephemeral Effect (Without Long-Term Learning), and a Gradual, Incremental Long-Term Learning Effect



It should be pointed out that the RN74 models place as much value on short-term *recidivism* after reductions in countermeasure levels as on short-term behavioural improvements after increases in countermeasure levels. There is, therefore, a sense in which the detection of a strong countermeasure effect by the RN74 models could be considered to be a *criticism* of that countermeasure, because such a finding would constitute evidence that the countermeasure's effect on crash numbers was ephemeral.

It is argued above that the RN74 models should be considered implausible on the grounds of *a priori* considerations. The question might then be asked, why the re-analyses reported here were undertaken. There are two answers. The first is that, although the RN74 models are considered *implausible*, they are not considered *impossible*. The second is that the RN74 regression analyses actually *did* produce significant countermeasure effects, and an explanation for those effects is required. The explanation proffered here is summarised below.

The re-analyses in Section 10 revealed that the countermeasure effects reported in RN74 were *not* the result of month-to-month correlations between countermeasure and crash levels towards the end of the ten-year period; but, were, instead, a consequence of the facts that countermeasure levels were zero when crash numbers were relatively high (for the first seven, or so, years), and were high when crash numbers were lower (in the last three, or so, years). Such confounding between the countermeasure and crash variables can easily be misconstrued as causal relationships. White (2000) has presented a detailed account of how the confounding might have created very strong *apparent* countermeasure effects.

### 13.3 Alcohol Sales and the Economy

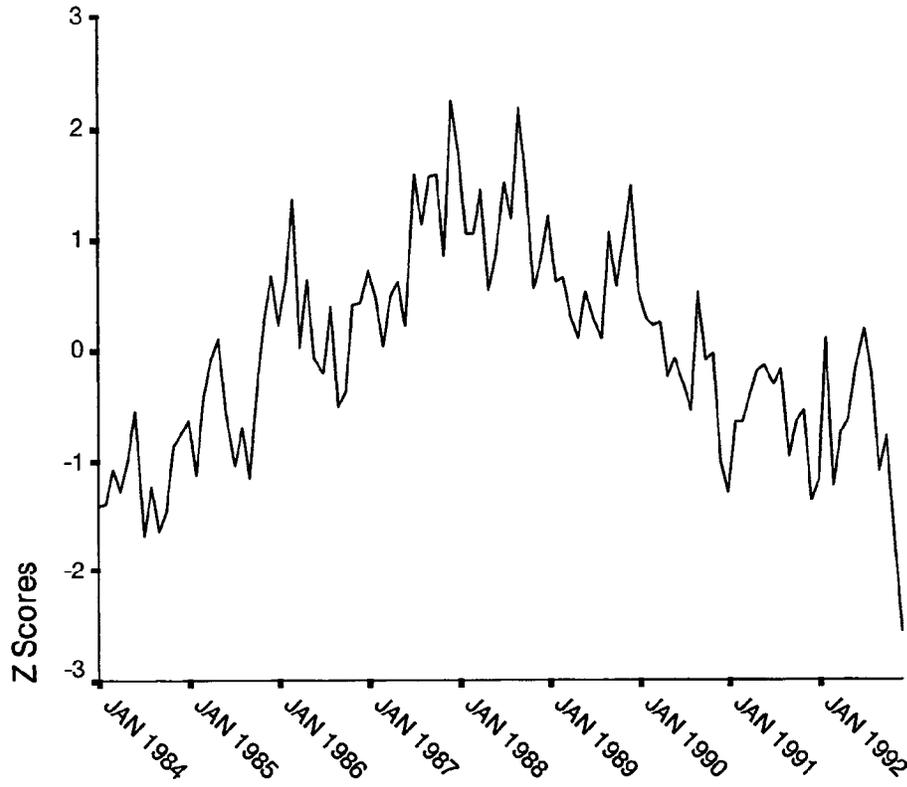
A number of studies have found that overall levels of drinking are lower under poorer economic circumstances (Brenner, 1975; Crawford, Plant, Kreitman & Latcham, 1987; Lester, 1996; Winton, Heather & Robertson, 1986). There is also some evidence that alcohol consumption is 'pro-cyclical', which means that levels of consumption increase during peaks in the business cycle and decrease during troughs (Freeman, 1999).

Some problems with the operationalisation of the alcohol consumption variable ('Alcohol Sales') were discussed in sub-section 7.4. However, if the data *can* be interpreted meaningfully, the temporal pattern should be further investigated. With reference to the raw Alcohol Sales data in Figure 5.1.2, it can be seen that, after an exceptionally high level in 1983, the trend was generally downwards for next nine years. It can also be seen that there were strong seasonal fluctuations. It is not clear why 1983 was exceptional; and it is conceivable that some measurement artefact was involved. The shape of the distribution of the Alcohol Sales variable over the last nine years of the study period deserves further scrutiny.

Figure 13.3.1 gives standardised values for the de-trended and de-seasonalised raw Alcohol Sales data for the nine-year period from 1984 to 1992. This version of the variable will be referred to as 'modified Alcohol Sales'. (Variables entering the RN74 regression analyses were effectively de-trended and de-seasonalised through the incorporation in the regression equation of the linear trend variable and the monthly dummy variables.) It can be seen that modified Alcohol Sales increased during the early- and mid-1980s and decreased thereafter. The peak was in 1988.

Figure 13.3.1

Standardised Values (Z Scores) of the De-Trended and De-Seasonalised Alcohol Sales Variable



All serious casualty crashes peaked in 1988 (see Figure 1.1.1); as did LAH crashes (see Figure 4.1.1). HAH crashes peaked in 1987-1988 (see Figure 5.1.1). The Leading Index of Economic Indicators peaked in 1988 (see Figure 8.2.1). So, there are three variables that moved together: modified Alcohol Sales, a measure of the economy, and serious casualty crashes. As far as the causal picture is concerned, it seems likely that the improving-then-deteriorating economy had both a *direct* effect on all crash numbers, and an *indirect* effect on HAH crash numbers through reduced alcohol sales.

#### 13.4 Conclusions Concerning the TAC Campaigns

Recent estimates of Victorian crash savings attributable to the TAC-funded countermeasure campaigns are provided by Bliss et al. (1998, p. 4):

Road safety publicity supporting the anti-speeding and drink-driving enforcement programs in Victoria has operated with essentially the same styles and levels of advertising for eight years, and the combined enforcement and publicity programs have continued to contribute around a 25% reduction in serious casualty crashes each year.

Based on such estimates of crash savings, the TAC (March, 2000) claims that there have been very high financial savings:

Since 1989, the TAC has embarked on an aggressive accident prevention campaign (in conjunction with Police and VicRoads) which has seen the State's road toll almost halved. This has resulted in savings for the community in a number of areas. For example, based on the 1989 data, the TAC has saved in excess of \$1 billion through reduced claims payouts. The TAC claims costs, however, are not the only economic costs borne by Victorians as a result of road crashes. [When these other costs are taken into account] the reduction in accidents since 1989 (to December 1996) is estimated to have saved the Victorian Community more than \$2.9 billion.

From the re-analyses of the data of Newstead et al. (1995) it is concluded that the above estimates of crash and financial savings attributable to the TAC-funded countermeasures are not supportable. In particular, it is concluded that any regression analysis of the RN74 data can only be considered to be exploratory rather than definitive; and that no estimate of crash savings based on the RN74 regression analyses can be taken on face value.

These conclusions do not necessarily imply that TAC-funded road safety campaigns have been ineffective. They simply mean that there has been a failure by Newstead et al. (1995) to appropriately *demonstrate* any such effectiveness.

More particularly, the re-analyses have failed to support the claims of Cameron et al. (1993) concerning the crash reductions that can be achieved through high levels of TAC-funded road safety TV advertising. MUARC consultancy advice (e.g., Vulcan et al., 1996) has been in favour of advertising levels that are exceptionally high. That advice is not supported by the re-analyses reported here.

## 13.5 Recommendations

### 1 Experimentation as the Preferred Research Method

This re-investigation is relevant to a very broad debate about the usefulness of econometric modelling in the evaluation of public policy initiatives. On the one hand, the OECD (1997, p. 47) believes that “Accident data lend themselves to econometric modelling like few other phenomena within the realm of behavioural science”. On the other hand, there has been a long-standing view that econometric modelling of observational time-series data is rarely conducted in such a way as to be of any practical value (e.g., Brownlee, 1967; Evans, 1997; Evans, 1999; Hendry, 1980; Leamer, 1983; Leontief, 1971; Mayer, 1980; McKim & Turner, 1997; Phelps-Brown, 1972; Redman, 1994; Summers, 1991; Turner, 1997). (An introduction to this debate, from a sceptical perspective, is provided in the Attachment to this report.) The re-analyses reported here have demonstrated the fragility of econometric modelling when applied to the evaluation of crash countermeasures. It is therefore recommended that:

*The developers of an innovative road crash countermeasure should always first consider the possibility of implementing the countermeasure in such a way that it can be evaluated experimentally. Reliance on the post-hoc analysis of observational time-series data should never be the first option considered.*

### 2 Scrutiny of Modelling Research that Underlies Policy Advice

Given the fragility of post-hoc analyses of observational time-series data, the ‘informed purchaser’ of policy advice based on econometric modelling should scrutinise the overall research process for indications that the work has been conducted according to the highest standards. It is recommended that:

*The following questions should be asked of any evaluation of a road crash countermeasure based on econometric modelling:*

- . *How sound is the underlying theoretical model?*
- . *To what extent have the inclusion and operationalisation of predictor variables been based on improvements to the overall fit of the model?*
- . *Have the findings been replicated against completely independent data sets?*
- . *Has the research been subjected to peer review? And,*
- . *if so, have the peer reviewers been provided with the raw data for checking?*

### 3 The Importance of Data Sharing

This re-investigation could not have been undertaken without the provision of data by MUARC. Such studies should be seen to be important components of a nation’s or state’s road safety research program. The US National Research Council endorsed a 1985 report by the Committee on National Statistics entitled *Sharing Research Data*. The third recommendation from that report is reproduced below (Fienberg, Martin & Straf, 1985, p. 27).

*Scientists have a special responsibility to share data as quickly and widely as possible when data are, or will become, relevant to public policy. Withholding such data risks the use of wrong results, or of ineffective analysis of important issues.*

#### **4 Further Research on Crash Trends to Test the Three Factor Theory**

It was hypothesised that the fall in crash numbers in Melbourne in the late 1980s was attributable to worsening economic conditions at the time. It is interesting to note that similar falls, although not so marked, occurred at much the same time in other Australian States. The findings reported here are consistent with the theory that major changes in monthly crash numbers during peaks or troughs in the business cycle can be satisfactorily explained in terms of only three factors: changing economic circumstances, a long-term downward crash trend, and seasonality. It is recommended that:

*The 'three factor' theory of crash trends be thoroughly tested against 1983 – 1992 crash numbers from all Australian States. Further testing should also be conducted for the Australian States, and possibly also for other countries, during other periods affected by major peaks or troughs in the business cycle.*

#### **5 Advice on Levels of Road Safety TV Advertising**

It is considered that the research of Cameron et al (1993) which linked advertising levels to crash reductions was seriously flawed. Given that MUARC consultancy advice has been in favour of advertising levels that are exceptionally high, it is recommended that:

*MUARC consultancy advice in favour of very high levels of road safety television advertising should not be taken into consideration when determining appropriate levels of such advertising.*



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**ATTACHMENT**

**A CRITIQUE OF STATISTICAL MODELLING  
OF OBSERVATIONAL TIME-SERIES DATA**

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This brief essay provides an entry point, from a fairly sceptical perspective, into the long-standing debate on the scientific validity of statistical modelling (using multiple regression techniques) of observational time-series data.

### *The Dubious Scientific Status of Econometric Modelling*

There has been a long-running debate about the scientific status of statistical (or, 'econometric') modelling of observational time-series data (e.g., Brownlee, 1967; Evans, 1997; Evans, 1999; Hendry, 1980; Leamer, 1983; Leontief, 1971; Mayer, 1980; McKim & Turner, 1997; Phelps-Brown, 1972; Redman, 1994; Summers, 1991; Turner, 1997). The extent of the scepticism about the validity of such modelling is indicated by the use of terms such as "soothsaying", "scientific illusion", "wishful thinking", "alchemy" and "con" in the titles of some of these papers.

The basic arguments against statistical modelling of observational time-series data are that the only sound way of discovering causal relationships between dependent and independent variables is through the conduct of properly controlled experiments; and that post-hoc regression modelling of merely 'observational' data will normally provide only equivocal results. This point of view was stated emphatically by Brownlee (1965, pp. 453-454):

The application of multiple regression techniques to observational data can be ... treacherous and misleading. ... In most circumstances, therefore, any indications produced by a multiple regression analysis of observational data are merely a good hint to try for confirmation by a proper experiment.

The justification sometimes advanced that a multiple regression analysis on observational data can be relied upon if there is an adequate theoretical background is utterly specious, and disregards the unlimited capacity of the human intellect for producing plausible explanations by the carload lot.

In a similar vein, Phelps Brown (1972, p. 6), in a presidential address to the Royal Economic Society, concluded that:

... running regressions between time series is only likely to deceive.

### *Problems with Model Specification*

Those who support the 'real-world' value of road crash time-series modelling (e.g., OECD, 1997), still point out that such modelling can be valid *only if certain assumptions are met*. The central question, then, is how likely it is that model assumptions are actually met in any instance of social-science research.

The model assumptions referred to above can be found in any text on econometrics (e.g., Studenmund, 1997). They are generally expressed in mathematical terms, and will not be discussed here in any detail. The main assumption for practical purposes is probably that the model is '*completely specified*'. Specification involves the identification and operationalisation of *all* the relevant independent variables, so that they can be included in the model equation. No relevant variable can be

omitted. Once a variable has been identified, it must be operationalised (that is, defined in such a way that it can be measured). The identification and operationalisation of all the relevant independent variables is a very difficult task in most real-world situations. As a consequence, most of the statistical models used in social science research are of questionable validity.

If it is so rare for statistical models to be correctly specified, the question must be asked why modelling continues to thrive. Leontief's (1971, p. 2) answer was simply that violations of the model assumptions are widely ignored:

By the time it comes to the interpretation of the substantive conclusions, the assumptions on which the model has been based are easily forgotten.

### *The Common Practice of Data Dredging*

According to Studenmund (1997, p. 186):

Almost surely the worst way to choose a specification is to simultaneously try a whole series of possible regression formulations and then choose the equation that conforms the most to what the researcher wants the results to look like.

Furthermore, such 'data mining' and 'fishing expeditions' to obtain desired results for the final regression equation are potentially unethical methods of empirical research. ... if enough alternatives are tried, the chances of obtaining the results desired by the researcher are increased tremendously, but the final result is virtually worthless.

The widespread practice of data dredging is discussed by Leamer (1983, pp. 36-37):

The econometric art as it is practiced at the computer terminal involves fitting many ... statistical models. One or several that the researcher finds pleasing are selected for reporting purposes. This searching for a model is often well-intentioned, but there can be no doubt that such a specification search invalidates the traditional theories of inference. ... This is a sad and decidedly unscientific state of affairs we find ourselves in. Hardly anyone takes data analyses seriously. Or perhaps more accurately, hardly anyone takes anyone else's data analyses seriously.

### *The Superficial Appeal of Technical Skills*

One reason for the popularity of statistical modelling of time-series data might be that some of the people supporting the research are over-impressed with the demonstration of technical skills. In the words of Summers (1991), p. 146):

All too often researchers, referees and editors fail to ask the scientific questions. Instead they ask the same questions that jugglers' audiences might ask: Have virtuosity and skill been

demonstrated? Was something difficult done? Often these questions can be answered favourably, even when no substantive contribution is being made. It is much easier to demonstrate technical virtuosity than to make a contribution to knowledge.

Just as not all demonstrations of virtuosity contribute to knowledge, most empirical work that actually contributes to knowledge does not display the author's capacity for statistical pyrotechnics. Good empirical evidence tells its story regardless of the precise way in which it is analysed.

### *Lack of Contribution to the Advancement of Knowledge*

One criticism of econometric modelling is that it has contributed very little to the advancement of knowledge. For example, with respect to the role of econometrics in the development of economic theory, Leontief (1971, p. 3), in a presidential address to the American Economic Association, lamented that:

In no other field of empirical inquiry has so massive and sophisticated a statistical machinery been used with such indifferent results.

Twenty years later, Summers (1991, pp. 132) agreed that "formal econometric work" had a "negligible impact" on the "development of economic science". He also noted that "The major writings of leading economic theorists ... contain almost no reference to econometric studies".

### *Model Validation*

The results of a theory-based econometric modelling exercise are valid only when the selection of predictor variables is *strongly determined* by the underlying theory which is *fully justifiable on rational grounds*. In practice, the underlying theoretical model is rarely definitive, and variable selection frequently involves data dredging.

The results of an exploratory econometric modelling exercise should be considered to be merely speculative until the model has been successfully replicated against completely independent data sets. All too frequently, exploratory analyses are reported as though they had some predictive validity.

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