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EVALUATION OF THE CRASH EFFECTS OF STRIP SHOPPING CENTRE TREATMENTS IN VICTORIA

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by

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Abstract:

This report presents results of an evaluation of the Strip Shopping Centre Program that commenced in 2003. It involved the treatment of eighteen strip shopping centres that were identified based on their poor crash history. The purpose of the study was to establish the effectiveness of the program in terms reductions in all types of casualty crashes and casualty crashes involving pedestrians that could be attributed to treatments. Economic savings due to reducing the number of casualty crashes at treated sites were also estimated. Poisson regression was used to compare the difference in before-treatment and after-treatment crash counts at treated sites with those at suitably chosen controls.

The evaluation estimated that the Strip Shopping Centre Program reduced casualty crashes of all types at treated sites by 8.1%, however this estimate of effectiveness was not statistically significant (p=.200). The evaluation also found that reduction in casualty crashes involving a pedestrian was estimated to be 16.9%, but this estimate was also not statistically significant (p=0.167). The economic savings associated with an 8.1% reduction in all casualty crashes over the life of the program would result in a benefit-cost ratio (BCR) of 7.4. This suggests that the benefits of applying an effective treatment at locations with poor crash histories can be maximised by applying them at locations that receive large volumes of traffic.

Further economic assessment of the program revealed that the Strip Shopping Centre program would only need to result in a 1% reduction in casualty crashes of all types, or around 2.5 casualty crashes per annum in total across all 18 treated sites to return positive economic benefits to the community. This is a reflection of the relatively low cost of implementing each Strip Shopping Centre treatment. On this basis, cautious further implementation of similar treatment types seems warranted from the results obtained here along with further evaluation of the current and any expanded program.

Key Words:
strip shopping centre, evaluation, accident analysis, traffic engineering, statistical analysis, economic analysis, speed limit, variable speed limit

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Preface

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EXECUTIVE SUMMARY

In 2003 VicRoads identified several strip shopping centres as being particularly in need of treatment to improve their safety. Since July 2005, $3.085M has been spent treating eighteen strip shopping centres. Information provided by VicRoads indicated that the types of treatments implemented at these locations involved the introduction of variable speed limits. The aim of this study was to evaluate the effect of the eighteen strip shopping centre treatments on casualty crashes using a rigorous statistical methodology. The study tested whether the treatments reduced all types of casualty crashes as well as casualty crashes involving pedestrians.

VicRoads supplied MUARC with the accident numbers of crashes that occurred at the treated sites. These accident numbers were matched with casualty crash data from the Road Crash Information System (RCIS) so that crashes that occurred at the treated sites from 1st February 2001 to 31st October 2007 could be identified from all crashes occurring on Victorian roads during this period. Only crashes occurring after 1st February 2001 were eligible to be included in the analysis because during the month of January 2001, the default urban speed limit of local roads in Victoria was reduced from 60 km/h to 50 km/h.

The method used to assess the effectiveness of the Strip Shopping Centre Program in reducing crashes at the eighteen treated sites was similar to that employed in the evaluation of the Accident Blackspot Component of the $240M Statewide Blackspot Program (Scully, Newstead, Corben & Candappa, 2006). This study used a quasi-experimental study design and Poisson regression models to establish whether changes in the number of casualty crashes at treated sites were significantly different to changes in the number of casualty crashes at non-treated sites. The control sites for each treated site were untreated sections of road with a 60 km/h speed limit in the same Local Government Area (LGA). This group of roads did not include local roads since the speed limit of such roads was reduced from 60 km/h to 50 km/h in January 2001. Therefore, control sites were limited to untreated arterials such as collector roads and main roads within the same LGA as the treated site. Such roads were consistent with the types of roads on which the strip shopping centre treatments were completed.

Providing control sites are chosen carefully, comparing casualty crash reductions at treated sites against those at non-treated control sites enables the effect of treatments on casualty crash counts to be isolated from other factors that may affect casualty crash counts in the post-treatment period (such as general trends in the State's road toll, effects of socio-economic factors and the effect of other road safety programs). Importantly for the present project, comparing crash counts at treated sites with crash counts at carefully-chosen control sites also enabled control of the potential bias caused by the discontinuity in data for crashes occurring prior to 2006 compared with data for crashes occurring from 2006 onwards.

In order to apply this methodology, it was necessary to not only disaggregate the sample of crashes by whether they occurred at treated sites or control sites, but also whether they occurred in the pre-treatment period or the post-treatment period of the treated sites or the control site to which each was matched. This was done using the date that the particular treatment first became operation (as indicated by VicRoads) and the variable relating to the date of the crash in the RCIS database. The minimum length of time for a before treatment period was 2.5 years, with the average being 3.6 years, while the minimum length of an after treatment period was also 2.5 years, with the average being 3.2 years. The use of control sites to adjust the percent reduction in crashes at treated sites means that it was not necessary for before treatment periods to be the same duration as after treatment periods.

Once the casualty crash data was disaggregated into treatment and control pairs and into before treatment and after treatment groups within each of these treatment-control pairs, a log-linear model with Poisson error structure was fitted to the data. The percentage casualty crash reduction at each treated site was derived from one of the model parameters and the statistical significance of the estimated reduction in casualty crashes due to the treatment was equal to the statistical significance for this parameter. Subtle modifications of the model were employed to estimate the average treatment effect for the overall program.

Analysis estimated an 8.1% reduction in casualty crashes at treated sites associated with the introduction of the treatment. Due primarily to the limited number of sites treated under the program this was not statistically significant (p=.200, 95% C.I. = {-4.5% 19.1%}). It was also estimated that the program resulted in a 16.9% reduction in pedestrian crashes, however this result was also not statistically significant (p=0.167). Based on the point estimate of an 8.1% reduction in casualty crashes, measures of economic worth were calculated even though the estimated crash reduction was not significant. If the true effect of the Strip Shopping Centre Program was an 8.1% reduction in casualty crashes, the BCR of the program would be 7.4. This suggests that the benefits of applying an effective treatment at locations with poor crash histories can be maximised by applying them at locations that receive large volumes of traffic. The discussion section of the report provides caveats that the reader should consider when interpreting the estimates of economic worth. Furthermore, it should be noted that the economic measures presented in the report did not account for the effect of treatments on non-crash related factors, such as changes in travel times, fuel consumption, local property prices or the profitability of local businesses.

Further economic assessment of the program revealed that the Strip Shopping Centre program would only need to result in a 1% reduction in casualty crashes of all types, or around 2.5 casualty crashes per annum in total across all 18 treated sites to return positive economic benefits to the community. This is a reflection of the relatively low cost of implementing each Strip Shopping Centre treatment. On this basis, cautious further implementation of similar treatment types seems warranted from the results obtained here along with further evaluation of the current and any expanded program.

A number of assumptions were made to obtain the results presented in this report and interpretation of the results is subject to a number of qualifications. These assumptions and qualifications can be viewed in Appendix C.

EVALUATION OF THE CRASH EFFECTS OF STRIP SHOPPING CENTRE TREATMENTS IN VICTORIA

1.0 INTRODUCTION

The Strip Shopping Centre Program began in 2003 and was aimed at improving the safety of strip shopping centres. VicRoads identified that many of these shopping centres posed a safety problem as they fulfilled conflicting roles. On the one hand, these shopping centres are usually located on arterial roads and so are exposed to a great deal of through traffic; on the other hand, they are also sites of significant pedestrian activity. VicRoads identified several strip shopping centres as being particularly in need of treatment. Information provided by VicRoads indicated that the types of treatments implemented at these locations involved the introduction of variable speed limits. As of July 2005, eighteen strip shopping centres had been treated and no further sites were treated under the program since this time.

1.1 PRELIMINARY EVALUATION OF THE PROGRAM

VicRoads commissioned a preliminary analysis of the effectiveness of these eighteen treatments in reducing the risk of casualty crashes at the treated sites. However, the methodology employed in the evaluation had a number of deficiencies. Firstly, the evaluation did not compare crash data at treated sites with crash data at specific matched control locations. Instead, the evaluation controlled for long term trends in road safety by correcting by the overall reduction in the number of casualty crash occurring on arterial roads in Melbourne over the study period. This method did not control for other factors that may account for changes in crash numbers in pre- and post-treatment periods, particularly those that occurred local to the treated sites. In addition, the method did not control for a potential bias in the data caused by changes that occurred at the beginning of 2006 in the way crash data were collected in Victoria.

Furthermore, the length of time in which post-treatment crash data were collected for each treated strip shopping centre was very short (an average of only 1.8 years). By completing the analysis with an additional year of crash data, the average length of post-treatment periods would be comparable to that used in previous evaluations of improvements to the road network. For example, the average length of post-treatment periods for the recent evaluation of the Accident Blackspot Component of the $240M Statewide Blackspot Program was 2.8 years (Scully, Newstead et al., 2006).

1.2 STUDY AIMS AND HYPOTHESES

The aim of this study was to evaluate the effect of the eighteen strip shopping centre treatments on casualty crashes using a rigorous statistical methodology.

Several null hypotheses were tested, the first of which was that the combined effect of the eighteen treatments was that they had no effect on casualty crash frequency at treated sites. This hypothesis was tested against a two-side alternative hypothesis that the combined effect of the treatments resulted in a change, either increase or decrease, in casualty crash frequency at treated sites.

A second null hypothesis tested was that the combined effect of the eighteen treatments was that they had no effect on the number of casualty crashes involving pedestrians at treated sites. This hypothesis was also tested against a two-sided alternative hypothesis: that the combined effect of the eighteen treatments was an increase or decrease in casualty crashes involving pedestrians at treated sites.

Almost all previous evaluations of black spot treatment effectiveness, including those previously carried out by MUARC, have assessed the study null hypothesis against a two sided alternative hypothesis. Use of a two sided hypothesis is a conservative option as is makes no a-priori assumption about whether the treatment will decrease or increase crash frequency at the treated site. Previous black spot evaluations have shown some instances where treatments have actually increased crash rates due to unintended consequences of the treatment. It is possible to test each null hypothesis against a one-sided alternative hypothesis that assumes the program will result in a reduction in casualty crashes at treated sites. However this would not allow formal statistical significance testing of any identified crash increases. Adopting a one sided alternative hypothesis should only be done when there is clear evidence that a treatment will only lead to crash reductions at a site. For the present evaluation, it was deemed more appropriate to use a more-conservative two-sided alternative hypothesis since there was no clear evidence that crash reductions would necessarily result from the Strip Shopping Centre treatments. The reader can change from a two-sided alternative hypothesis to a one-side alternative hypothesis by simply halving the statistical significance values presented for the two-sided test. Changing from a two-sided alternative hypothesis to a one-sided hypothesis only affects the calculated statistical significance values and does not alter the point estimates of the program effects on casualty crash frequency although all confidence limits quoted in this study relate to the two-sided test.

As well as testing these null hypotheses, the report also presented an economic evaluation of the eighteen strip shopping centre treatments. Estimates of indicative reductions in economic costs due to reductions in crashes at strip shopping centres attributable to the treatments were converted into estimates of economic savings. Using data provided by VicRoads on the cost of each treatment, the estimates of economic savings were used to estimate the benefit-cost ratio (BCR) and net present worth (NPW) of the program as a whole.

2.0 DATA

2.1 TREATMENT DATA

VicRoads provided MUARC with data on each of the eighteen sites treated as part of the Strip Shopping Centre Program. The data provided by VicRoads indicated that the type of treatment completed at each of the eighteen treated sites involved the installation of variable speed limit signs. Further information on each treated site included:

- Name of the road on which the treatment was completed;
- Names of the intersecting roads that formed the boundaries of the treated road length;
- Melway map reference of the treated site;
- Road classification (local, main or highway) of the treated site;
- Hours of operation of the variable speed limit signs;
- Date on which the variable speed limit signs were first "switched on";
- Start and finish dates of treatment works;
- Capital cost of treatment works;
- Estimated annual maintenance and operating costs; and
- Estimated treatment life.

The treatment data provided by VicRoads are shown in Appendix A.

2.2 CASUALTY CRASH DATA

VicRoads also provided MUARC with accident numbers of the 1,655 police-reported casualty crashes that occurred at the eighteen treated sites in the period from 1st of April 2000 to 31st October 2007. In Victoria, a casualty crash is defined as a crash that results in at least one road user being injured or killed. These accident numbers were matched to casualty crash data from VicRoads' Road Crash Information System (RCIS), which contains information on all casualty crashes reported to police. Crashes occurring prior to 1st February 2001 were excluded from the sample of crashes to be analysed because during the month of January 2001, the Government of Victoria reduced the default speed limit of local streets from 60 km/h to 50 km/h which may have biased the study results. As explained in Section 3.2, the speed limit of the road on which a crash occurred was used to match crashes at treated sites with crashes at control sites. Restricting the analysis period to the period after this transition enabled the definition of control sites to be consistent over time.

Critical RCIS data fields used in this study were:

- Crash date;

- Crash severity;
- Crash number;
- Local Government Area (LGA) of the crash; and
- Speed limit of the road on which the crash occurred.

2.2.1 Disaggregation of casualty crashes by treatment/control and before/after groups

The Method section of this report describes how the casualty crash data supplied by VicRoads were disaggregated according to whether crashes occurred at treated sites or controls sites (Section 3.2) and whether they occurred in the pre or post-treatment period (see Section 3.4). The results of categorising casualty crash data in this manner are described in Table 2.1. An analogous table restricted to crashes involving a pedestrian can be found in Appendix D.

Table 2.1: Summary of casualty crash data used in the analysis

Before or After	Treated Site	Control	Not a Treated or a Control Site	Total
Prior to February 2001	204	7,822		8,026
During treatment installation period	26	215		241
Not associated with a Strip site	0	0	116,498	116,498
Subtotal of Omitted Crashes	230	8,037	116,498	124,765
After treatment period	440	5,357		5,797
Before treatment period	896	8,766		9,662
Subtotal of Case/Control Crashes	1,336	14,123		15,459
Total	1,566	22,160	116,498	140,224

From the RCIS dataset provided by VicRoads, it was found that 140,224 casualty crashes occurred during the period 1st January 1999 to 31st October 2007. The previous section explained how VicRoads provided accident numbers of 1,655 crashes occurring at treated sites during the period from 1st of April 2000 to 31st December 2007, however only 1,566 of these accident numbers could be matched to accident numbers in the RCIS dataset.

Of the 1,566 crashes identified as occurring at treated sites, 204 occurred prior to February 2001, making them ineligible for inclusion in the data to be analysed. Another twenty-six occurred in the periods in which it was assumed that treatment works were being completed at the sites. This meant that the analysis of the effect of the treatments on casualty crashes at treated sites would be based on how the remaining 1,336 crashes at treated sites were distributed into either the before treatment period or the after treatment period when compared to the distribution for control sites. Of the crashes at treated sites, 896 occurred in the before treatment periods, while 440 occurred in the after treatment periods.

A total of 22,160 crashes occurred at control sites. The remaining 116,498 casualty crashes occurred at sites that were not associated with treated sites or control sites. For the 22,160 casualty crashes at control sites, 8,766 occurred in before treatment periods, while 5,357 occurred in the after treatment periods.

2.3 CASUALTY CRASH COST DATA

In order to assess the economic benefits of the Strip Shopping Centre Program, it was necessary to estimate the cost of casualty crashes. VicRoads provided MUARC with estimates of the cost of crashes of different severities by the speed zone of the crash location. These estimates were in June 2005 Australian dollar values and are presented in Table 2.2 below. VicRoads advised that these costs were derived using movements in the Consumer Price Index (CPI) to update costs previously endorsed by Austroads in June 2002.

Table 2.2: Crash cost values used for economic assessment

Speed Zone	Ave. cost of serious casualty crash	Ave. cost of other injury crash
60 km/h or less	$467,000	$19,100
70, 80 & 90 km/h	$557,000	$20,800
100 & 110 km/h	$629,000	$20,300

In Table 2.2, a *serious casualty crash* was defined as a crash in which the most seriously injured road user was killed within 30 days as a result of the crash or transported to hospital as a result of the crash, while an *other injury crash* was defined as a crash in which the most seriously injured road user was not killed and did not require transportation to hospital.

3.0 METHOD

3.1 STUDY DESIGN

The method used to assess the effectiveness of the Strip Shopping Centre Program in reducing crashes at the eighteen treated sites was similar to that employed in the evaluation of the Accident Blackspot Component of the $240M Statewide Blackspot Program (Scully, Newstead et al., 2006) which used a quasi-experimental study design. Poisson regression was used to establish whether changes in the number of casualty crashes at treated sites were significantly different to changes in the number of casualty crashes at non-treated sites. This Poisson regression approach to analysing quasi-experimental designs was originally proposed by Bruhning and Ernst (1985) and modifications of the original method have been used by MUARC (e.g. Newstead & Corben, 2002) and other road safety researchers (BTE, 2001) to estimate the effectiveness of road safety programs across all treated sites.

This study design required the identification of the number of casualty crashes occurring at each site before treatment works were undertaken and after the treatment works were completed. It was also necessary to match each treated site to a set of non-treated control sites. The numbers of casualty crashes in the pre-treatment and post-treatment periods at each of the control sites were used to adjust the percentage reduction in the number of casualty crashes at treated sites in the post-treatment period when compared with the pre-treatment period so that it represented the reduction in crashes due to the treatments alone.

Provided control sites are chosen carefully, comparing casualty crash reductions at treated sites against those at non-treated control sites enables the effects of treatments on casualty crash counts to be isolated from other factors that may affect casualty crash counts in the post-treatment period. Factors that are not related to treatments that may affect casualty crash counts include general trends in the State’s road toll due to broad road safety program effects, economic trends, changes in traffic flow in the local area and the effect of other road safety programs such as speed enforcement (Newstead, Cameron, Gantzer & Vulcan, 1995). Importantly for the present project, comparing crash counts at treated sites with crash counts at carefully-chosen control sites also enabled control of the potential bias caused by the discontinuity in data for crashes occurring prior to 2006 compared with data for crashes occurring from 2006 onwards. This discontinuity occurred because of changes in the way police collected crash data.

3.2 CHOICE OF CONTROLS

In order to maximise the statistical power of the analyses, the before and after casualty crash counts of each treated site were compared with aggregated casualty crash counts at a group of control sites. It was proposed that controls would consist of all casualty crashes occurring in the same Local Government Area (LGA) and of the same classification as the road on which the strip shopping centre is located. It was also proposed that only roads that have a speed limit of 60 km/h would be eligible to be used as controls, for allowing high-speed roads to be controls would bias the evaluation because the speed limits at most strip shopping centres would have been 60 km/h prior to treatment. Initially, the extensions of the roads on which treated strip shopping centres were located were also deemed ineligible to be included as control sites. However, it was later decided that lengths of roads extending from treated shopping strips should be eligible for inclusion as control sites. This decision was made when it was found that including crashes from these sites in the control group did not increase the ratio of crashes in the control group occurring during the after treatment period to the number of crashes at control sites in the before treatment period. This indicated that treatments were not causing more crashes to occur on roads extending from treated sites.

A further amendment to the control site eligibility criteria was made on inspection of the crash data provided by VicRoads. The crash data provided by VicRoads did not enable crashes to be categorised by road class. Therefore, it was decided that for treated sites in a particular LGA, controls would consist of all casualty crashes occurring on untreated sections of road with a 60 km/h speed limit in the same LGA. Prior to February 2001, such a group of roads would have included minor local roads. However after February 2001 the default speed limit of such roads was reduced from 60 km/h to 50 km/h. Therefore, limiting all before treatment periods to periods after 31st January 2001 would mean that control groups would exclude crashes on local roads and be limited to crashes on arterials such as collector roads and main roads, which was consistent with the types of roads on which the strip shopping centre treatments were completed.

As can be seen from Appendix A, the eighteen strip shopping centre sites treated as part of the program were distributed across ten different LGAs. This meant that in the present study, some treated sites were matched to the same group of control sites. When evaluating the effectiveness of the program as a whole, the aggregated before and after casualty crash counts of treated sites in the same LGA were compared with the casualty crash counts at the control sites. For example, if there were two treated sites in the one LGA, the summed crash counts of these two sites would be compared against the summed crash counts at non-treated sites occurring on roads with speed limits of 60 km/h in the same LGA. However, this design does not prevent the separate evaluation of separate treated sites in the one LGA, as explained in Section 3.4.

3.3 BEFORE AND AFTER TREATMENT PERIODS

Before and after treatment periods for each treatment-control pair were determined using treatment data provided by VicRoads (see Appendix A). Specifically, the "switch-on" date of each treatment was used to determine the before treatment and after treatment periods. For treatment-control pairs in each LGA, the pre-treatment period was defined as the period from the 1st February 2001 to a week before the earliest switch-on date of each of the treated sites in the particular LGA. The post-treatment period for each LGA was defined as the period from a day after the latest switch-on date of treatments in the particular LGA to the 31st October 2007. VicRoads was unable to provide information on the date that treatments works commenced at each treated site, so it was assumed that for each treated site, treatment work was undertaken and completed in the week prior to the switch-on date. Table 3.1 shows the switch-on date of each of the eighteen treated sites as well as the dates defining the before and after periods for each of the ten Local Government Areas.

As the earliest switch-on date for the eighteen treated sites was 1st August 2003, the minimum length of time for a pre-treatment period was 2.5 years, with the average being 3.6 years. The most recent switch-on date was 29th June 2005, so the minimum length of a post-treatment period was also 2.5 years, with the average being 3.2 years.

Table 3.1: Dates of the before treatment and after treatments

Site name	Suburb	LGA	Switch-on date	Before period		After period	
				Start	Finish	Start	Finish
Whitehorse Rd	Balwyn	Boroondara	7/2/04	1/2/01	31/1/04	8/2/04	31/10/07
Springvale Rd	Springvale	Dandenong	8/6/05	1/2/01	1/6/05	9/6/005	31/10/07
High St	Preston	Darebin	9/8/04	1/2/01	2/8/04	10/8/04	31/10/07
Dorset Rd	Boronia	Knox	5/9/03	1/2/01	29/8/03	6/9/03	31/10/07
Clayton Rd	Clayton	Monash	1/8/03	1/2/01	25/7/03	2/8/03	31/10/07
Racecourse Rd	Kensington	Moonee Valley	27/7/04	1/2/01	20/7/04	17/3/05	31/10/07
Mt Alexander Rd	Moonee Ponds	Moonee Valley	16/3/05	1/2/01	20/7/04	17/3/05	31/10/07
Pascoe Vale Rd	Glenroy	Moreland	29/6/05	1/2/01	22/6/05	30/6/05	31/10/07
Fitzroy St	St Kilda	Port Phillip	21/7/04	1/2/01	14/7/04	22/7/04	31/10/07
Chapel St	South Yarra	Stonnington	21/3/05	1/2/01	14/3/05	1/6/05	31/10/07
Commercial / Malvern Rd	Prahran	Stonnington	21/3/05	1/2/01	14/3/05	1/6/05	31/10/07
Toorak Rd	South Yarra	Stonnington	21/3/05	1/2/01	14/3/05	1/6/05	31/10/07
Glenferrie Rd	Armadale	Stonnington	31/5/05	1/2/01	14/3/05	1/6/05	31/10/07
Bridge Rd	Richmond	Yarra	10/8/04	1/2/01	3/8/04	13/8/04	31/10/07
Brunswick St	Fitzroy	Yarra	12/8/04	1/2/01	3/8/04	13/8/04	31/10/07
Johnston St	Fitzroy	Yarra	12/8/04	1/2/01	3/8/04	13/8/04	31/10/07
Smith St	Fitzroy	Yarra	12/8/04	1/2/01	3/8/04	13/8/04	31/10/07
Swan St	Richmond	Yarra	10/8/04	1/2/01	3/8/04	13/8/04	31/10/07

As pre-treatment periods were on average nearly six months longer than the post-treatment periods, it is likely that even if a treatment didn't have an effect on casualty crash frequency at a treated site, the number of crashes in the post-treatment period would be fewer than that in the pre-treatment period simply because the post-treatment period was of a shorter duration. In such circumstances, reporting the percent reduction of crashes in the post-treatment period compared to the pre-treatment period as the effect of the treatment in reducing the frequency of casualty crashes at the treatment site would be incorrect and misleading.

However, the fact that each treated site was matched to a set of non-treated control sites enabled comparison of post-treatment casualty crash counts at treated sites with pre-treatment casualty crash counts. For each treatment and control site pair, the pre-treatment period for the treated site covers the same time-span as the pre-treatment period for the control site. The same is true for the post-treatment periods of the treated site and the control site. If the crash count at a treated site is likely to be biased because the post-treatment period is of a shorter duration than the pre-treatment period, the same bias will exist for the matched control sites. Therefore, the percentage reduction in the casualty crashes at control sites can be used to normalise the percentage reduction for the treated site, which will give a measure of the effect of the treatment on casualty crash counts at the treated site.

3.4 STATISTICAL ANALYSIS METHODS

This section describes the theory behind the Poisson regression analysis method in detail. The analysis approach is the same as that used in the evaluation of the Accident Blackspot Component of the $240M Statewide Blackspot Program (Scully, Newstead et al., 2006).

Count data assembled for analysis in a quasi-experimental before/after treatment/control design define a two by two contingency table. The aim of the statistical analysis is to estimate the percentage change in casualty crash frequency from before treatment to after treatment at the treated sites relative to that at the control sites. Apart from the lack of treatment and control group site randomisation, this is the same analysis framework used in analysis of clinical trials where a randomised treatment-control structure is used.

Medical literature shows the most appropriate means of analysing count data from trials to estimate net treatment effects relative to a control, is via a log-linear analysis with a Poisson error structure (Breslow & Day, 1987). The estimate resulting from the analysis in the case of the casualty crash data being analysed here is not a relative risk of an outcome, such as cancer in a clinical trial, but the relative casualty crash change in treatment group compared to control. The distribution assumptions about casualty crash frequency made in the use of this method are consistent with those proposed by Nicholson (1986a, 1986b).

Bruhning and Ernst (1985) demonstrated the application of log-linear Poisson models to the analysis of quasi-experimental road safety evaluation designs. Demonstrated as part of the application were techniques for estimating aggregate treatment effectiveness across subsets of treated sites in an analysis, as well as tests for homogeneity of treatment effectiveness across selections of treated sites. Newstead and Corben (2001) also successfully demonstrated use of these methods in evaluating the TAC-funded accident blackspot program implemented in Victoria during 1992 to 1996.

The Poisson log-linear analysis method can be described as follows. Data defined by the quasi-experimental study design with before- and after treatment data in each of L treatment and control pairs can be summarised in a series of L 2x2 contingency tables, represented in Table 3.2.

Table 3.2: Contingency table format used in the analysis method

Site	Control Group		Treatment Group	
No.	Before	After	Before	After
1	n_{111}	n_{112}	n_{121}	n_{122}
...	...	...	...	...
L	n_{L11}	n_{L12}	n_{L21}	n_{L22}

A log-linear model with Poisson error structure, appropriate for the variability in the casualty crash data, is then fitted to the data, with the model form given by Equation 1. The log-linear model form of Equation 1 can easily be fitted in common statistical software packages such as SAS.

$$\ln(n_{ijk}) = \beta_0 + \beta_i + \beta_{ij} + \beta_{ik} + \beta_{ijk} \quad \ldots(Eqn.\ 1)$$

In Equation 1, i is the site number, j is the treatment or control group index, k is the before- or after treatment index, the β values are the model parameters and n_{ijk} is the cell casualty crash count. The percentage casualty crash reduction at site i attributable to the treatment, adjusted for the corresponding change in casualty crash frequency at the control site is equal to

$$\Delta_i = 100 \times (1 - \exp(\beta_{i00}))\% \quad \ldots(Eqn.\ 2)$$

where β_{i00} represents the parameter for the before treatment period at treated site i.

Statistical significance of Δ_i is equal to the statistical significance of β_{i00}, obtained directly from the fitted log-linear model. Confidence limits for Δ_i are computed in the normal way using the estimated standard error of β_{i00} obtained from the fitted log-linear model and using the transformation given by Equation 2. Subtle modifications of the above model can be made to estimate the average treatment effect across a number of treated sites. These modifications were detailed in Bruhning and Ernst (1985) and were used to estimate the overall program effect of the eighteen sites treated as part of the Strip Shopping Centre Program.

3.5 ACCIDENT MIGRATION AND REGRESSION-TO-THE-MEAN

The road safety literature has identified two important issues that should be considered when using a quasi-experimental study design to evaluate road safety programs. These issues are *accident migration* and *regression-to-the-mean*. Both of these issues are discussed below in the context of the methodology used to complete the evaluation of the Strip Shopping Centre Program. Parts of the following sections also appeared in the evaluation of the Accident Blackspot Component of the Statewide Blackspot Program (Scully, Newstead et al., 2006).

3.5.1 Accident Migration

One possible outcome of treating sites on the road network is accident migration, which involves the casualty crash risk being moved, either entirely or partly, from the treated site to another site nearby or vice versa. If not considered in an analysis, accident migration effects can lead to an incorrect estimate of the net benefit of a treatment program to the community. The most likely cause of an accident migration effect in this study would be through a treatment altering traffic volume at the treated site. To assess this likelihood, traffic volume data at the treated sites and at neighbouring sites would be needed. In addition, it would not only have been necessary to examine changes in traffic flows to the sites neighbouring the treated sites but records of casualty crashes at the neighbouring sites would also need to be analysed. This is because the neighbouring sites may be inherently safer than the treated sites so that the net risk across the network can still decrease when the extra traffic volume is diverted to the neighbouring site.

VicRoads did not provide traffic volume data for all the treated sites and neighbouring sites that might have been affected by accident migration for analysis in this project. In addition, it was beyond the scope of the project to analyse casualty crash risk changes at all sites neighbouring the treated sites, which would be required to properly assess the migration of casualty crash risk. Consequently, no results presented from this study have compensated for the effects of accident migration.

Accident migration effects are unlikely if treatments do not lead to substantial shifts in traffic volumes. Unfortunately, it is not clear how the treatments completed as part of the Strip Shopping Centre programs are likely to affect traffic volumes. Further comments regarding accident migration are provided in the discussion section of this report.

3.5.2 Regression-to-the-Mean

Regression-to-the-mean is another potentially confounding influence on estimated treatment effectiveness. It is caused by selecting sites for treatment based on a high casualty crash frequency measured over a narrow window in time due to the expression of an extreme in random variation but which have the same underlying crash rate as sites not selected for treatment. Selecting sites for treatment on such a basis means that there is a high likelihood that the casualty crash frequency at the selected site will reduce in the period immediately following treatment when the underlying crash rate remains the same merely due to the effects of random variation. If the treatment effect at the site is evaluated using the same inadequate casualty crash data from which the site was selected for treatment, the results of the evaluation will be biased to some degree.

Numerous analysis methods have been proposed to estimate the bias in road safety program outcome evaluation results caused by regression-to-the-mean, such as Abbess, Jarrett and Wright (1981). Many of these methods, however, make restrictive assumptions about the likely distribution of accident frequencies both at individual sites and, more importantly, between sites. The estimates of regression-to-the-mean effects, and hence treatment effects, are potentially largely a product of the assumptions made about these distributions. The authors feel that it is potentially dangerous and misleading to use such methods that rely so heavily on the assumptions made. A further problem with these methods is that they require comparison of crash rates across treated sites as well as across all untreated sites in a region on a common basis such as crashes per weekly traffic volume. Generally, such data are not available on the scale needed to achieve this, a problem acknowledged by the Bureau of Transport Economics (BTE) in its evaluation of the Federal Blackspot Program (BTE, 2001).

Other methods recommend more reliable strategies for minimising the potential for regression-to-the-mean problems. These include the use of adequate pre-treatment casualty crash histories to give an accurate estimate of the true pre-treatment casualty crash frequency at the chosen site, and not analysing exactly the same data period that was used to select the treatment site. In the study described here, each treated site that was evaluated had at least 2.5 years of pre-treatment casualty crash data, with the average pre-treatment period being 3.6 years. Furthermore, an analysis technique was used that properly recognised the level and distribution of random variation in the data and compute confidence limits and significance probability levels that properly reflect this.

Table 3.3 shows the number of crashes per month in the before treatment period for each of the eighteen treated sites compared to the number of crashes per month in the ten months prior to the before treatment period. The data available for the study did not allow identification of crashes occurring at treated sites more than ten months prior to the beginning of the before treatment periods. It can be seen from this table that there were an average of 1.16 casualty crashes per month in the before treatment periods of the eighteen treated sites, compared with 1.13 crashes per month in the ten months prior to the beginning of the before treatment periods. Therefore, there was only a small difference in the number of crashes per month in the before treatment period when compared to the period immediately before it. The number of crashes per month for each treated site also indicated that there was only minimal variation in the number of crashes per month between the before treatment periods and the ten months prior to the before treatment periods. This suggested that the crash counts in the before treatment periods used in the present analyses were fairly stable and that any significant reductions in crash rates in the after treatment periods were unlikely to be due to regression-to-the-mean.

Comparing crash rates for the before treatment periods to crash rates at the treated sites over a longer period of time immediately prior to the before treatment periods could confirm the limited influence of regression-to-the-mean provided the data was not confounded by any other influencing factor. However, since trends in the data in the prior period are potentially confounded by the implementation of the default 50km/h urban speed limit in Melbourne, use of this data for assessing potential regression to the mean effects is problematic. For this reason, the comparisons made in Table 3.3 should also be treated with some care since at least some of the prior period analysed will overlap the time before the 50km/h implementation.

Table 3.3: Casualty crashes per month in before treatment periods

	Prior period			Before period			Change in Crashes/Month (prior to before)
	# Crashes	# Months	Crashes /Month	# Crashes	# Months	Crashes /Month	
Bridge Rd	18	10.05	1.79	61	42.02	1.45	-0.34
Brunswick St	14	10.05	1.39	53	42.02	1.26	-0.13
Chapel St	26	10.05	2.59	110	49.35	2.23	-0.36
Clayton Rd	5	10.05	0.50	14	29.70	0.47	-0.03
Commercial Rd / Malvern Rd	19	10.05	1.89	75	49.35	1.52	-0.37
Dorset Rd	4	10.05	0.40	10	30.85	0.32	-0.07
Fitzroy St	16	10.05	1.59	51	41.36	1.23	-0.36
Glenferrie Rd	6	10.05	0.60	21	49.35	0.43	-0.17
High St	17	10.05	1.69	77	41.99	1.83	0.14
Johnston St	29	10.05	2.89	124	42.02	2.95	0.07
Mt Alexander Rd	7	10.05	0.70	43	41.56	1.03	0.34
Pascoe Vale Rd	3	10.05	0.30	28	52.63	0.53	0.23
Racecourse Rd	7	10.05	0.70	35	41.56	0.84	0.15
Smith St	3	10.05	0.30	21	42.02	0.50	0.20
Springvale Rd	8	10.05	0.80	43	51.94	0.83	0.03
Swan St	8	10.05	0.80	37	42.02	0.88	0.08
Toorak Rd	9	10.05	0.90	68	49.35	1.38	0.48
Whitehorse Rd	5	10.05	0.50	25	35.94	0.70	0.20
All Sites	204	180.9	1.13	896	775.03	1.16	0.01

3.6 EVALUATION OUTPUT MEASURES

In order to test the null hypotheses discussed in the introduction, this evaluation examined changes in crash frequencies at treated sites in the after treatment period compared with the before treatment period. Changes in crash frequencies were measured for all types of casualty crashes as well as for crash frequencies limited to crashes involving pedestrians. Crashes involving pedestrians were identified as those that had *Definitions for Classifying*

Accident (DCA) codes from 100 to 109. For each analysis, the entire Strip Shopping Centre Program was evaluated along with separate evaluations of each of the eighteen treated sites.

Economic measures of effectiveness were also used to assess the effectiveness of the program as a whole. The measures of economic effectiveness used to evaluate the program were:

- benefit-cost ratio; and
- net present worth.

Brief explanations of these economic measures have been provided in Appendix B.

Discount rates of 4%, 6% and 8% were assumed in these economic evaluations.

4.0 RESULTS

This section presents the main results of evaluating the effectiveness and economic worth of the Strip Shopping Centre Program. Section 4.1 describes the effectiveness of the program in terms of estimated reductions in the frequency of casualty crashes at treated sites. This is followed by an economic evaluation of the program in Section 4.2.

4.1 CHANGES IN CASUALTY CRASH FREQUENCY

This section presents estimates in the reduction in casualty crash frequency at the eighteen treated sites relative to casualty crash frequencies at chosen control sites. Section 4.1.1 presents estimates that were calculated at the program level (i.e. aggregated across all eighteen sites), while Section 4.1.2 presents casualty crash reduction results for each of the eighteen sites separately.

Throughout the results section of this report, the $p<0.05$ significance level has been used to determine whether results are statistically significant. However, the reader may interpret the results presented throughout the report using a less conservative level of significance if they feel it is appropriate for their purposes.

4.1.1 Program Level Effects

Table 4.1 shows the estimated reduction in the number of casualty crashes occurring at eighteen treated strip shopping centre sites relative to control sites. Estimates have been given for all casualty crashes as well as for casualty crashes involving a pedestrian. Measures presented include the estimated percent reduction in the number of crashes, as well as the estimated annual number of crashes prevented associated with implementation of the treatment program, which was determined using the annual numbers of crashes at treated sites before treatments were implemented. Upper and lower 95% confidence intervals have been given for each estimated crash reduction factor. These 95% confidence intervals give the range in which real crash savings due to the program lie with 95% probability. Statistical significance values of the estimated percentage reduction for each type of crash have also been given. These values give the probability that the estimated crash reduction was due to chance, rather than the effect of the program.

Table 4.1: Estimated crash reductions at the 18 sites treated under the Strip Shopping Centre Program for all casualty crashes and pedestrian casualty crashes

Types of Casualty Crashes	Estimated Crash Reduction (%)	Statistical Significance	Lower 95% Confidence Limit (%)	Upper 95% Confidence Limit (%)	Annual Crash Frequency at Treated Sites Before Treatment	Annual Casualty Crash Saving
All Casualty	8.1	0.1976	-4.5	19.1	245	20†
Pedestrian Crashes	16.9	0.1665	-8.0	36.1	73.5	12*

† Assuming a reduction in casualty crashes of 8.1%
* Assuming a reduction in casualty crashes involving pedestrians of 16.9%

It can be seen that the program was estimated to result in an 8.1% reduction in the number of casualty crashes of all types relative to the crash frequencies at matched control sites.

However, this result was not statistically significant, with the estimated casualty crash reduction ranging from a 4.5% increase to a 19.1% reduction with 95% certainty. As the aggregated annual number of casualty crashes occurring in the pre-treatment periods for the eighteen treated sites was estimated to be 245, assuming an estimated reduction of 8.1%, the treatments would have saved of 20 casualty crashes per annum over the life of the program (15 years).

Of the 15,459 casualty crashes that occurred in the before or after period of a treated site or a control site, 2,110 involved a pedestrian. Of these pedestrian crashes, 387 (18.3%) occurred at treated sites. The estimated 16.9% reduction in pedestrian crashes was not statistically significant (p=0.1337). If the program did result in a significant 16.9% reduction in casualty crashes involving a pedestrian, it would be expected that approximately sixteen pedestrian crashes were prevented across the eighteen sites each year, or some 180 casualty crashes over the life of the treatments.

As the expected life of each of the eighteen treatments was fifteen years (see Appendix A), the casualty crash savings expected over the life of the program was estimated to be fifteen times the estimated annual crash saving, i.e. it was estimated that approximately 300 casualty crashes would be prevented.

4.1.2 Effectiveness of each treatment

Table 4.2 shows the estimated casualty crash reduction for each individual site treated as part of the Strip Shopping Centre Program. The statistical significance of the estimated crash reduction associated with each treatment is shown in the fourth column. It can be seen there were no sites in which it was estimated that treatments reduced the number of casualty crashes with statistical significance.

Like the program as a whole, the lack of statistical significance at the individual treatment site level was due to insufficient crash numbers at each treated site relative to the estimated size of the treatment effect. If the post-treatment periods were of a longer duration, it is likely that some of the sites would have shown significant reductions. Similarly, if a one-sided test of significance were used instead of the more-conservative two-sided test of significance, Mt. Alexander Rd. would have shown a significant reduction in casualty crashes as a result of the treatment. It can also be noted that for some of the eighteen sites, the estimated casualty crash was less than zero, indicating that the treatments at these sites actually increased the incidence of casualty crashes. However, the reader is reminded that these negative casualty crash reduction estimates were not significant, although the estimated increase in crashes at Smith St was almost significant (p=0.0716).

Table 4.2: Estimated casualty crash reductions at each of the eighteen sites treated under the Strip Shopping Centre Program

Site name	LGA	Estimated Crash Reduction (%)	Statistical Significance	Lower 95% Confidence Limit (%)	Upper 95% Confidence Limit (%)
Whitehorse Rd	Boroondara	26.1	0.3400	-37.6	60.3
Springvale Rd	Dandenong	46.1	0.1126	-15.7	74.9
High St	Darebin	29.3	0.1097	-8.1	53.7
Dorset Rd	Knox	15.8	0.6998	-101.3	64.8
Clayton Rd	Monash	35.3	0.2851	-43.7	70.9
Racecourse Rd	Moonee Valley	33.9	0.2245	-28.9	66.1
Mt Alexander Rd	Moonee Valley	46.2	0.0625	-3.29	72.0
Pascoe Vale Rd	Moreland	-21.3	0.5912	-145.6	40.1
Fitzroy St	Port Phillip	3.8	0.8587	-48.0	37.5
Chapel St	Stonnington	4.9	0.7946	-38.8	34.8
Commercial Rd / Malvern Rd	Stonnington	-16.2	0.4839	-77.2	23.7
Toorak Rd	Stonnington	23.1	0.3094	-27.6	53.6
Glenferrie Rd	Stonnington	-42.3	0.3371	-192.7	30.8
Bridge Rd	Yarra	-40.6	0.0968	-110.2	6.0
Brunswick St	Yarra	-37.2	0.1510	-111.3	10.9
Johnston St	Yarra	17.3	0.2748	-16.3	41.2
Smith St	Yarra	-77.6	0.0716	-231.7	4.9
Swan St	Yarra	9.3	0.7404	-61.5	49.0

Table 4.3 shows the estimated reduction in casualty crashes involving a pedestrian for each individual site treated as part of the Strip Shopping Centre Program. It can be seen that none of the eighteen treated sites had statistically significant estimates for changes in the frequency of casualty crashes involving a pedestrian. This was not a surprise given that a relatively small proportion of the casualty crashes analysed involved an impact with a pedestrian. It was not possible to derive a point estimate of the reduction in casualty crashes involving a pedestrian for Mt. Alexander Rd due to there being no pedestrian crashes at this site in the after treatment period.

Table 4.3: Estimated reduction in casualty crashes involving pedestrians at each of the eighteen sites treated under the Strip Shopping Centre Program

Site name	LGA	Estimated Crash Reduction (%)	Statistical Significance	Lower 95% Confidence Limit (%)	Upper 95% Confidence Limit (%)
Whitehorse Rd	Boroondara	58.9	0.1960	-58.1	89.3
Springvale Rd	Dandenong	14.0	0.8019	-179.5	73.5
High St	Darebin	42.3	0.3531	-84.2	81.9
Dorset Rd	Knox	-4.2	0.9558	-340.8	75.4
Clayton Rd	Monash	32.9	0.4971	-112.3	78.8
Racecourse Rd	Moonee Valley	53.5	0.3511	-132.5	90.7
Mt Alexander Rd	Moonee Valley	79.7	0.1387	-67.5	97.5
Pascoe Vale Rd	Moreland	-122.2	0.1996	-652.8	34.4
Fitzroy St	Port Phillip	33.7	0.2697	-37.6	68.1
Chapel St	Stonnington	-14.8	0.6899	-126.2	41.7
Commercial Rd / Malvern Rd	Stonnington	-33.6	0.4586	-187.4	37.9
Toorak Rd	Stonnington	15.6	0.7585	-148.6	71.3
Glenferrie Rd	Stonnington	-31.3	0.6421	-314.2	58.4
Bridge Rd	Yarra	14.5	0.7444	-119.0	66.6
Brunswick St	Yarra	2.3	0.9604	-141.1	60.4
Johnston St	Yarra	39.9	0.1826	-27.1	71.6
Smith St	Yarra	-55.8	0.4335	-372.2	48.6
Swan St	Yarra	-10.8	0.8274	-177.9	55.8

4.2 ECONOMIC WORTH OF SAFETY BENEFITS

Analysis of the casualty crash effects of the Strip Shopping Centre Program was presented in Section 4.1. In this section, casualty crash reductions attributable to the program have been translated into measures of economic worth. These measures of economic worth estimate the economic benefits of the treatments due to the prevention of all types of casualty crashes, including crashes involving pedestrians. The economic measures discussed in this section do not take into account the effect of the treatments on travel time, emissions, noise, fuel consumption, changes in property values in the local area or changes in the profitability of local businesses, which may all be affected by the treatments.

Measures of economic worth specifically related to casualty crashes involving pedestrians have not been given because such measures omit most of the economic benefits of the program since they do not count the benefits due to the prevention of crashes that did not involve pedestrians, which comprise over 70% of the crashes occurring at treated sites.

In all the analyses presented in this section, it has been assumed that the Strip Shopping Centre Program resulted in an 8.1% reduction in casualty crashes at treated sites even though the previous section described how this estimated crash reduction was not statistically significant. The impact of this assumption is discussed in greater detail in the Discussion section of this report

In order to complete this task, it was necessary to translate crash cost data by crash severity, shown in Table 2.2, into an average cost per casualty crash. Table 4.4 shows the distribution by crash severity of casualty crashes occurring at treated sites during the before treatment periods. Crash severity was defined according to the level of injury of the most-seriously injured road user involved in the crash. Using this distribution in a weighted sum of the cost of crashes occurring in 60 km/h zones (see Table 2.2), it was estimated that the average cost of casualty crashes occurring at treated sites was $177,064.73.

Table 4.4: Crash severity of casualty crashes occurring at treated sites in the before treatment periods

Crash Severity	Frequency	Percent
Fatal or Serious	316	35.27
Other Injury	580	64.73
Total	896	100.00

All the economic measures presented in this section have been given in June 2005 Australian dollars values.

Assuming an average casualty crash cost of $177,064.73 along with an estimated casualty crash reduction of 8.1%, economic assessment of the entire program was undertaken. Table 4.5 shows how the cost of completing the eighteen treatments was $3,085,000. The expected aggregate cost of maintaining these treatments over the fifteen years of their expected lives was $154,250 per year, which had a present value of $1,498,114 if a discount rate of six per cent was assumed. Therefore, the present value of the total costs over the life of the project would be $4,583,114. If a discount rate of four per cent was assumed, the present value of the total life time costs of the program was estimated to be $4,800,011, while if a discount rate of eight per cent was assumed the present value of the total life time costs was estimated to be $4,405,300.

Table 4.5: Summary of the costs of the Program

Discount Rate	4%	6%	8%
Capital Costs ($)	$3,085,000	$3,085,000	$3,085,000
Annual Maintenance Costs ($)	$154,250	$154,250	$154,250
Present Value Maintenance Costs ($)	$1,715,011	$1,498,114	$1,320,300
Present Value Total Life Time Costs ($)	$4,800,011	$4,583,114	$4,405,300

Table 4.6 presents the economic evaluation of the Strip Shopping Centre Program assuming that the program resulted in an average reduction in casualty crashes of 8.1% across all eighteen sites for each year of the 15 year life of the program. The measures of economic worth considered were benefit-cost ratio (BCR) and net present worth (NPW) as well as the estimated present value savings due to a reduction in the frequency of casualty crashes for the entire program. These measures of economic worth were derived using discount rates of four, six and eight per cent.

Table 4.6 also presents data used to derive the measures of economic effectiveness, such as the average annual number of crashes at treated sites in the before treatment period, which was used to estimate the number of casualty crashes that the program prevented each year across all eighteen sites.

It can be seen from Table 4.6 that, at a discount rate of six per cent the estimated present value crash savings from an 8.1% reduction in the number of casualty crashes attributed to the program was approximately $34M, compared with a cost of $4.6M. Therefore, using a six per cent discount rate, the BCR of the program was 7.4 and the estimated NPW was approximately $29M. Assuming a discount rate of four per cent increased the BCR to 8.1 and the NPW to $34M, while increasing the discount rate to eight per cent reduced the BCR to 6.8 and the NPW to $26M.

Table 4.6: Economic assessment of the Strip Shopping Centre Program

Discount Rate	4%	6%	8%
Average annual number of casualty crashes at treated sites in before treatment period	245	245	245
Casualty crash reduction	8.1%	8.1%	8.1%
Number of casualty crashes prevented annually	20	20	20
Average cost per casualty crash	$177,065	$177,065	$177,065
Annual crash saving	$3,498,752	$3,498,752	$3,498,752
Project life	15 years	15 years	15 years
Casualty crashes prevented over project life	296	296	296
Present value of savings due to crashes prevented over the project life	$38,900,477	$33,980,748	$29,947,491
Present value of total project life costs	$4,800,011	$4,583,114	$4,405,300
Benefit to cost ratio	8.1	7.4	6.8
Net present worth	$34,100,466	$29,397,634	$25,542,191

5.0 DISCUSSION

5.1 CASUALTY CRASH SAVINGS

Using the best-available statistical methods analysis in this study has estimated that the Strip Shopping Centre Program (SSCP) implementation at 18 sites across Melbourne was associated with an 8.1% average reduction in casualty crashes across the eighteen treated sites. However, the estimated reduction was not statistically significant at the 5% level with the estimated 95% confidence limit on the result ranging from a 4.5% increase to a 19.1% reduction in casualty crashes at treated sites. The lack of statistical significance was caused by insufficient crash numbers at the treated sites for the magnitude of program effectiveness estimated and clearly highlights the problem of evaluating outcome effectiveness of road engineering treatments at relatively few sites. It is acknowledged that from a statistical perspective, the results of the crash analysis obtained from this study appear inconclusive. However, before making conclusions about the overall effectiveness and value of the SSCP from the results obtained here it is important to consider also the results of the economic evaluation which are discussed further below.

The present evaluation also employed a methodology in which crash counts at treated sites were matched to crash counts at similar non-treated sites in the same local government area. This methodology improves the robustness of the analysis compared to simply aggregating the data across all sites for analysis which potentially leads to based estimates of treatment effectiveness. As explained by Bruhning & Ernst (1985), aggregating the data is “scientifically inadmissible” (p. 291) as differences in treatment effects between different treated sites were not considered. The model employed in the present analysis did account for variation in treatment effects between different treated sites, resulting in a more robust overall measure of effectiveness.

When evaluating the effectiveness of preventing casualty crashes involving a pedestrian, the present evaluation found that the point estimate of the reduction in such crashes was greater than the point estimate for all casualty crashes (i.e. 16.9% compared with 8.1%). This is as expected given pedestrian involved crashes are a specific target of the SSCP and that speed related countermeasures are often most effective for vulnerable road users. However, the estimate for the reduction in casualty crashes involving a pedestrian also did not reach a level of significance to have high confidence that the effect of the program on the frequency of casualty crashes involving pedestrians at treated sites was not simply an artefact of random variation in sampling.

When evaluating individual treatments, it was found that none of the 18 individual treatments resulted in statistically significant reductions in casualty crashes at the strip shopping centres where these treatments were employed. Again, this highlights the lack of sufficient crash history at sites where the program was implemented to allow rigorous scientific evaluation.

5.2 ECONOMIC EVALUATION

Section 4.2 translated casualty crash reductions attributable to the Strip Shopping Centre Program into measures of economic worth. In discussing the estimates of economic worth, it should be noted that the results presented were based on the estimated 8.1% reduction in casualty crashes of all types at treated sites which was not statistically significant. Hence the estimates of economic worth should be treated with some caution.

The present value of the cost of completing the eighteen treatments and maintaining them over their expected project life (fifteen years) was estimated to be $4,583,114 if a discount rate of six percent was assumed ($4,800,011 for a discount rate of 4% and $4,405,300 for a discount rate of 8%). Based on an estimated casualty crashes reduction of 8.1% it was estimated that 20 casualty crashes would be prevented across the eighteen sites for each year of the life of the project. The average cost of a casualty crash was assumed to be $177,065, which means that in the first year of the completion of the eighteen treatments, approximately $3.5M would be saved due to the prevention of the 20 crashes across the eighteen treatments. This means that even if only crash savings in the first year of operation were considered, the benefit-cost ratio (BCR) of the project would be approaching one. As the expected life of each of the eighteen treatments was fifteen years, an 8.1% reduction in casualty crashes at treated sites would be expected to result in savings over the program's expected life with a present value ranging from $30M (assuming a discount rate of eight per cent) to $39M (using a discount rate of four percent). This translated to BCR estimates ranging from 6.8 to 8.1 when savings over the total life of the program were considered. Similarly, the estimated net present worth when savings over the total life of the program were considered would range from $26M to $34M.

If the true effect of the program was an 8.1% reduction in casualty crashes, these BCR estimates are very encouraging when compared with the BCRs estimated for some other road safety programs aimed at improving road infrastructure. For example, in the evaluation of the Accident Blackspot Component of the $240M Statewide Blackspot Program (Scully, Newstead et al., 2006) it was estimated that treatments resulted in a 31.1% reduction in casualty crashes at the 804 treated sites, which translated into BCR estimates ranging from 3.3 to 4.2 depending on the discount rate assumed (ranging from four percent to eight percent). Similarly, the evaluation of the $85M TAC funded blackspot program completed in Victoria in the period 1992 -1996 estimated that the treatments would result in an estimated casualty crash reduction of 26.4% at the 559 treated sites, which translated into a BCR of 4.1 when a discount rate of eight per cent was assumed.

There are several possible reasons why the present program would result in much greater BCR estimates than previous programs despite having a lower point estimate for the reduction in casualty crashes at treated sites. Firstly, it is possible that the present evaluation used an inflated estimate of the average cost of casualty crashes. However, Section 4.2 explained how the average cost of a casualty crash at treated sites was estimated using the weighted average of the cost of crashes of different injury severity levels occurring in 60 km/h zones. This was similar to the method for calculating the average cost of casualty crashes used in previous evaluations of road safety programs. The variation between the average costs of casualty crashes occurring in 60 km/h zones estimated in the current evaluation to the analogous costs of casualty crashes used in previous evaluations was not large enough to account for the differences in the BCR estimates.

One likely reason why the BCR estimates for the Strip Shopping Centre Program were much greater than expected given BCR estimates from previous blackspot evaluations is that the average annual number of casualty crashes occurring in the pre-treatment period of sites treated under the Strip Shopping Centre Program was greater than sites treated under previous blackspot programs. For sites treated as part of the Strip Shopping Centre Program, an average of 13.9 casualty crashes per site occurred each year in the before treatment period compared with only 2.1 for sites treated as part of the Accident Blackspot Component of the $240M Statewide Blackspot Program (Scully, Newstead et al., 2006). This means that if treatments delivered an 8.1% reduction in casualty crashes, the Strip

Shopping Centre Program would prevent more crashes per year per site than previous blackspot programs (2.9 casualty crashes prevented each year per site compared with 0.7). This highlights the economic benefits of providing effective treatments at sites where the number of crashes occurring each year is high.

A further reason for the high BCR estimated is that the cost of implementing the Strip Shopping Centre Program treatments was relatively low compared to other black spot treatments at just over $171,000 per treated site on average. This compares with an average treatment cost of just over $250,000 for sites treated under the $240M Statewide Blackspot Program (Scully, Newstead et al., 2006). Low treatment costs combined with well targeted treatment sites lead to high BCR values even for the modest 8.1% total crash reduction estimated in this study.

As noted previously, the BCR estimates obtained in this study are based on an estimated crash reduction of 8.15 associated with implementation of the Strip Shopping centre Program, even though this crash reduction estimate was not statistically significant and hence may have been obtained purely through chance variation within the crash data. Some would interpret the non-significance of the estimated crash reduction as not providing support for the effectiveness of the program casting doubt as to whether future implementations of the same type could be supported. However, further examination of the BCR estimates provides vital information in relation to such a conclusion. Based on the cost information used in this study, obtaining an estimated BCR of 1 for the program, the point at which crash cost savings and program implementation costs are equal, a crash reduction of only around 1% would be required. This is the equivalent of a net absolute crash saving of only 2.5 casualty crashes per annum across all 18 sites treated. Whilst it is possible that the treatments produced no real crash reduction, this is considered unlikely due to the magnitude of the treatment effect estimated, the nature of the treatment works and the historical crash reduction effectiveness of other speed reduction countermeasures in urban areas. For example, the 50km/h default urban speed limit in Victoria produced and estimated 13% casualty crash reduction. As the economic evaluation has shown, the Strip Shopping Centre treatments would only have to produce very small crash reduction to return positive economic benefits to the community. On this basis, further evaluation of the program and cautious further implementation of similar treatment types seems warranted from the results obtained here.

5.3 LIMITATIONS AND POSSIBLE FURTHER RESEARCH

This evaluation of the Strip Shopping Centre Program employed the best-available statistical techniques that could be used with the data available. In order to employ the methodology, a number of assumptions had to be made. These assumptions are presented in Appendix C and should be referred to when drawing conclusions based on the results of the evaluation.

After completing the evaluation, a number of limitations were noted. These limitations are discussed below along with some ways in which the study could be extended to address these limitations.

5.3.1 Serious Casualty Crashes

The main finding of this evaluation was that the Strip Shopping Centre Program produced an estimated 8.1% reduction in casualty crashes at the treated sites although this result was not statistically significant. This included reductions in crashes of all severities, not just crashes in which a road user was killed or seriously injured. Given that the explicit target set for Victoria's "*arrive alive!*" road safety strategy (VicRoads, 2002) was to reduce serious casualties by 20% by the end of 2006, it would be useful if evaluations of programs designed to reduce road trauma present estimates of effectiveness in terms of reductions in serious casualties at treated sites.

Such an evaluation could be presented for the Strip Shopping Centre Program. However, as serious casualty crashes are less numerous than casualty crashes of all severities, it is possible post-treatment periods of greater duration than that currently available will be required before a significant result is likely. This is especially true if an estimate of the effect of the program on serious casualty crashes involving pedestrians was required. One viable option might be to analyse the changes in the ratio of fatal and serious crashes to all casualty crashes, an approach that has higher statistical power than analysis serious casualty crashes alone and has proved successful in other road safety countermeasure evaluation.

5.3.2 Crash Reductions during Hours of Operation of Variable Speed Limit Signs

From Appendix A, it can be seen that the speed limits at treated sites were reduced for all hours of the day and for each day of the week for only five of the eighteen treated sites. For the remaining thirteen sites, speed limits were reduced only during high risk periods of the day. For some sites, speed limits were reduced for most of the day, while for other sites the speed limit was only reduced for a few hours of the day. For example, the speed limit along Johnston Street Fitzroy was reduced from 7AM to 3AM (20 hours of each day), while for the strip shopping centre at Mt Alexander Road Moonee Ponds the speed limit was only reduced for 8 hours (7PM to 3AM) on Wednesday to Saturday nights. Like the strip shopping centre along Mt Alexander Road, speed limits at some treated sites were only reduced for certain days of the week. In addition to this, for many sites, the hours in which reduced speed limits were in operation changed depending on the day of the week. For example, the speed limit along Glenferrie Road Armadale was reduced from 8AM to 9PM on weekdays, but from 8AM to 5PM on weekends.

If the treatments did have an effect on casualty crash frequency, it would be expected that for sites in which speed limits were only reduced for parts of the week the effect on the frequency of casualty crashes would be less during the periods in which the variable speed limit signs were not in operation. Therefore, if the present analysis were restricted to comparing the frequencies of crashes during the periods of the week in which the variable speed limit signs were in operation there might be an increased likelihood of obtaining a significant estimate of effectiveness depending on the amount of crash data remaining and the size of the crash effects specific to hours of operation. However, it should also be noted that the treatments may have an effect on crash frequencies at treated sites outside of the hours of operation of the variable speed limits signs, as the mere presence of the sign hardware may change drivers' behaviour.

In the present report, crashes occurring at any time of the day and week contributed to casualty crash counts in the before treatment periods and after treatment periods. The reason that crash counts were not restricted to those occurring during the hours of operation (or the future hours of operation for before treatment crash counts) was that there were concerns that such restrictions could cause the number of crashes at treated and control sites to be prohibitively low. Further evaluation based on longer after treatment periods would improve the prospects of obtaining statistically significant point estimates of effectiveness under the existing study design and for consideration of effects specifically at times of speed sign restriction. This would be an interesting subject for future research.

5.3.3 Regression-to-the-mean

Section 3.5.2 provided some discussion of the way regression-to-the-mean could inflate the estimated levels of effectiveness of evaluations that employed the current methodology. Comparisons of crash rates per month in the before treatment period and the ten months preceding this period were made to determine whether the number of casualty crashes reported in the before treatment periods were representative of long-term crash trends at the site or were an expression of an extreme in random variation at treated sites. It was found that there was very little difference in the crash rates during the before treatment periods and the ten months prior to the before periods of the eighteen treated sites, which suggested that the overall measures of effectiveness reported were not inflated by the effects of regression-to-the-mean.

The fact that the routes chosen for treatment under the Strip Shopping Centre Program had been recognised as sites with poor safety records over many years also means that the likelihood of them being chosen as a result of randomly high values was remote. In order to know with greater certainty that regression-to-the-mean was not inflating the reported measures of effectiveness, it would be necessary to compare crash rates at the treated sites standardised by traffic volumes to standardised data at all other similar sites across the metropolitan area. As noted in many previous Australian black spot evaluations, volume data with which to standardised crash data is either extremely limited or not available to allow the breadth of comparison required for an adequate regression-to-the-mean assessment on this basis. Furthermore, as noted, the prior data period for this study was confounded by the implementation of the urban default 50km/h speed limit and hence would not have been amenable for use in further regression-to-the-mean assessments. Consequently, the measures taken in this study to reduce regression-to-the-mean bias were considered to be the most thorough practicable.

5.3.4 A last word on Accident (or Traffic) Migration

Section 3.2 explained that for the present analysis, crashes occurring on lengths of roads extending from treated shopping strips were included in the counts of casualty crashes occurring at control sites. It was found that of the 14,123 crashes occurring at control sites, 1,117 (7.9%) occurred on untreated lengths of road extending from treated shopping strips. Of these 1,117 crashes, 766 occurred in before treatment periods while 351 occurred in the after treatment period. Therefore, the ratio of these crashes at the untreated extensions of treated strip shopping centres that occurred in after treatment periods compared to the before treatment periods was 0.46:1. By comparison, the ratio for all crashes occurring at control sites was 0.61:1 (see Table 2.1).

If the treatments completed as part of the Strip Shopping Centre Program had no effect on the number of crashes occurring at non-treated lengths of road extending from treated shopping strips, it would be expected that the ratio of crashes after treatment to before treatment would be the same for these sections of road as the analogous ratio for all other controls. The fact that the two ratios were not equal suggested that the treatments at strip shopping centres could have had an effect on crash frequencies in the after treatment periods on untreated lengths of road extending from treated shopping strips. However, the significance of the effect of treatments along lengths of road extending from treated strip shopping centres was not tested in the present analysis.

There are a number of possible reasons why the frequency of crashes in the after treatment period at untreated extensions of treated strip shopping centres was less than that expected from changes in crash counts at other control sites. Firstly, treatments could have reduced traffic flow through treated strip shopping centre sites and also reduced traffic flow on lengths of road extending from the treatment locations. If road users wished to avoid the variable speed limit zones, they may take an alternative route which would cause them not just to avoid the treated strip shopping centre but also part or all of the untreated sections of the road on which the strip shopping centre was located. This would have increased traffic flow to other sections of the road network. If this increased traffic flow to other sections of the road network had the effect of increasing the number of crashes in the after treatment periods along these alternative routes, the estimated reduction in the number of casualty crashes at treated sites would not represent the reduction in the number of casualty crashes across the entire network. That is, some or all of the crashes that would have occurred at the strip shopping centres if they were not treated would have occurred at other locations on the road network instead. This phenomenon is known as accident migration.

As previously mentioned VicRoads did not provide traffic volume data for treated strip shopping centre sites and neighbouring sites, possibly because such data are not available. These data would be necessary to determine whether the demonstrated reduction in casualty crash risk at the treated sites was accompanied by an increased crash risk at untreated neighbouring sites.

An alternative reason why the frequency of crashes in the after treatment period of untreated lengths of roads extending from treated strip shopping centres was lower than what was expected is that the treatments could have had an effect on road safety beyond the treated regions. This could occur if the treatments caused drivers to drive in a safer manner when approaching and/or after passing through a treated strip shopping centre. If this was the reason for the lower than expected number of crashes at the untreated extensions of roads on which a treated strip shopping centre was located, the number of casualty crashes estimated to be prevented at treated sites also represents the number of casualty crashes prevented across the entire network.

Finally, it is possible that the before to after crash ratios for the extensions of the strip shopping centre roads are different to other controls as a result of random variation in the crash data. It is difficult to determine which of these explanations best accounts for the reduced crash frequency along untreated lengths of road extending from treated strip shopping centres. Consequently it was difficult to determine how to treat these road lengths in the study design with respect to including or excluding them from the control data. If the sites were excluded from the control data and traffic migration had occurred, the after period of the control data would be biased high and the estimates of treatment effect would be over stated. Including these roads as controls in the presence of traffic migration would produce a less biased result, particularly if crash risk on these roads and on the roads to which the traffic migrated was the same. Including these road lengths in the control data if the treatment effect carried over to these roads potentially produced a conservative estimate of the treatment effect at the treated sites. Of there was no spill-over treatment effect, the treatment effect estimates would not be biased.

Given these potential scenarios, it was decided to include the contiguous road lengths either side of the treated sections in the control data hence producing potentially conservative treatment effect estimates. This was considered better than potentially over stating the treatment effects. If this decision did introduce bias to the effect estimates it is likely to be only small considering the total control crash data at the sites in question was less than 8% of the total control data.

5.4 CONCLUSION

This report estimated that, overall, implementation of the Strip Shopping Centre Program at 18 sites across metropolitan Melbourne was associated with an 8.1% reduction in all casualty crashes and 16.9% reduction in casualty crashes involving pedestrians. Although these results were derived using the best-available statistical techniques, neither of these estimates were statistically significant, primarily because of a lack of sufficient crash history at treated sites resulting from the relatively small number of sites treated in conjunction with the limited after treatment crash history available.

The economic savings associated with an 8.1% reduction in all casualty crashes over the life of the program would result in a benefit-cost ratio (BCR) that was substantially greater than those estimated for evaluations of previous blackspot programs. For example, the evaluation of the of the Accident Blackspot Component of the $240M Statewide Blackspot Program by Scully, Newstead et al. (2006) revealed that the 31% reduction in casualty crashes at treated sites resulted in a BCR of 3.7 when a discount rate of 6% was assumed. By comparison, if the true effect of the Strip Shopping Centre Program was an 8.1% reduction in casualty crashes, the BCR of the program would be 7.4. This suggests that the benefits of applying an effective treatment at locations with poor crash histories can be maximised by applying them at locations that receive large volumes of traffic.

Further economic assessment of the program revealed that the Strip Shopping Centre program would only need to result in a 1% reduction in casualty crashes of all types, or around 2.5 casualty crashes per annum in total across all 18 treated sites to return positive economic benefits to the community. This is a reflection of the relatively low cost of implementing each Strip Shopping Centre treatment. On this basis, cautious further implementation of similar treatment types seems warranted from the results obtained here along with further evaluation of the current and any expanded program.

It should also be noted each of the eighteen sites treated as part of the Strip Shopping Centre Program were treated by installing variable speed limits. The report can therefore be considered to be an evaluation of the effectiveness of such treatments in reducing the frequency of casualty crashes at locations similar to those in the present study. The conclusions the present report therefore highlight the need for further evaluation of the effectiveness of these types of treatments at other strip shopping centres across the State.

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APPENDIX A – TREATMENT DATA PROVIDED BY VICROADS

Table A.1: Treatment data provided by VicRoads

Road	Location	Melway Ref.	Class of Road	Hours of Operation	Switch-on date	Type of works	Capital Cost	Annual Maintenance Cost	Project Life (yrs)
Bridge Rd, Richmond	Burnley St to Punt Rd	2G H5 - 2H E6	Main	7am to midnight	10-Aug-04	Electronic VSL signs	$125,000	$6,250	15
Brunswick St, Fitzroy	Gertrude St to Alexandra Pde	2C B5 – 2C A10	Local	Permanent	12-Aug-04	Electronic VSL signs	$100,000	$5,000	15
Chapel St, South Yarra	Dandenong Rd to Toorak Rd	2L J5 – 2P H3	Local	Permanent	21-Mar-05	Electronic VSL signs	$300,000	$15,000	15
Clayton Rd, Clayton	Centre Rd to Clayton Station	79 C3 – 79 C2	Main	Mon-Thurs: 9am-5pm Friday: 9am-9pm Sat-Sun: 9am-5pm	01-Aug-03	Electronic VSL signs	$155,000	$7,750	15
Commercial Rd / Malvern Rd, Prahran	Porter Rd to Chatsworth Rd	2L G9 – 2M E10	Main	Permanent	21-Mar-05	Electronic VSL signs	$300,000	$15,000	15
Dorset Rd, Boronia	Boronia Rd to Elsie Av	64 K9 – 64 K8	Main	Mon-Thurs: 8am-7pm Friday: 8am-9pm Sat-Sun: 9am-5pm	05-Sep-03	Electronic VSL signs	$75,000	$3,750	15
Fitzroy St, St Kilda	Jacka Blvd to S. of Lakeside Dve	2N J6 – 2P B4	Main	6am to 2am	21-Jul-04	Electronic VSL signs	$90,000	$4,500	15
High St, Preston	Dundas St to Queen St	30 F3 – 18 G8	Local	Mon-Sat: 7am to 7pm	09-Aug-04	Electronic VSL signs	$200,000	$10,000	15
Johnston St, Fitzroy	Brunswick St to Mason La	2B K7 – 2C K8	Main	7am to 3am	12-Aug-04	Electronic VSL signs	$200,000	$10,000	15
Racecourse Rd, Kensington	Smithfield Rd to Rankins Rd	2T H1 – 2T K1	Highway	8am to 11pm	27-Jul-04	Electronic VSL signs	$50,000	$2,500	15

Table A.1 (continued): Treatment data provided by VicRoads

Road	Location	Melway Ref.	Class of Road	Hours of Operation	Switch-on date	Type of works	Capital Cost	Annual Maintenance Cost	Project Life (yrs)
Smith St, Fitzroy	Langridge St to Johnston St	2C D11 – 2C D7	Local	Permanent	12-Aug-04	Electronic VSL signs	$65,000	$3,250	15
Swan St, Richmond	Church St to Punt Rd	2L E5 – 2M F6	Main	7am to midnight	10-Aug-04	Electronic VSL signs	$95,000	$4,750	15
Toorak Rd, South Yarra	Punt Rd to Grange Rd Av	2G K10 – 2G F9	Main	Permanent	21-Mar-05	Electronic VSL signs	$165,000	$8,250	15
Whitehorse Rd, Balwyn	Austin St to Brenbeal	46 C7 – 46 E8	Main	8am-10pm	07-Feb-04	Electronic VSL signs	$210,000	$10,500	15
Glenferrie Rd, Armadale	Dandenong Rd to High St	59 B10 – 59 C8	Main	Mon-Fri: 8am-9pm Sat-Sun: 8am-5pm	31-May-05	Electronic VSL signs	$275,000	$13,750	15
Springvale Rd, Springvale	Virginia St to Mainehey Gv	80 A8 – 79 K10	Highway	Mon-Fri: 8am-9pm Sat-Sun: 8am-5pm	08-Jun-05	Electronic VSL signs	$315,000	$15,750	15
Mt Alexander Rd, Moonee Ponds	Dean St to Mascoma St	28 J7 - 28 K10	Main	Wed-Sun: 7pm-3am	16-Mar-05	Electronic VSL signs	$165,000	$8,250	15
Pascoe Vale Rd, Glenroy	Finchley Ave to Grandview St	16 G2 - 16 G3	Main	Mon-Fri: 9am-7pm Sat: 9am-3pm	29-Jun-05	Electronic VSL signs	$200,000	$10,000	15

APPENDIX B – DEFINITION OF ECONOMIC MEASURES

Benefit-cost ratio (BCR) is the ratio of the present value of casualty crash savings over the life of the project to the present value of costs incurred over the life of the project to achieve these savings. As a BCR is a ratio of monetary values, it has no unit of measurement. If an estimate exceeds the value one, it is assumed to be worthwhile.

Net present worth (NPW) is the present value of the casualty crash savings over the life of the project minus the present value of the costs incurred over the life of the project to achieve these savings. NPW estimates are expressed in units of dollars, and the project is deemed worthwhile as long as the estimate exceeds zero. NPW represents the net return of a particular investment. So that NPW estimates for one program can be compared with estimates for other programs of a different scale, it is useful to express NPW values on a per treatment basis.

APPENDIX C – ASSUMPTIONS AND QUALIFICATIONS

In evaluating the effectiveness of the Strip Shopping Centre Program, a number of assumptions were made. These were as follows.

- VicRoads were unable to provide information on the date that treatments works commenced at each treated site, so it was assumed that for each treated site, treatment work was undertaken and completed in the week prior to the switch-on date.

- Descriptions of the treatments provided by VicRoads were accurate with respect to the cost of completing treatments and the dates on which treatments were “switched on”. Similarly, the descriptions enabled accurate identification of the location of sites where treatments were implemented. No independent audit was undertaken to verify the information supplied by VicRoads regarding the type of treatments completed or the location of sites.

- Extraction of crash data at treated sites carried out by VicRoads was accurate.

- Control sites selected for the analysis accurately and fully represented the effects of non-treatment related factors that may affect casualty crash frequency and casualty crash counts in the before or after period at treated sites.

- The form of the statistical models and error structures chosen was the most appropriate for the analysis and provided accurate and unbiased estimates of program effectiveness.

- Statistical analysis presented in this report cannot prove unequivocally that the treatments led to the attributed crash reductions. It is possible that other unrelated but concurrent events led to the effects observed, although this is considered unlikely considering the analysis design employed.

- Casualty crash costs used in the analysis appropriately reflected the real cost of casualty crashes to the community.

The following qualification should also be noted

- The average after treatment period of time in the present evaluation was 3.2 years, which was longer than that used for the recent evaluation of the Accident Blackspot Component of the $240M Statewide Blackspot Program (Scully, Newstead et al., 2006). However the reader should be aware that with further accumulated after treatment experience, the estimated crash effects at treated sites could change.

- All estimates of the economic benefits of the program presented in the report were based on the assumption that the program resulted in an 8.1% reduction in casualty crashes at treated sites despite the evaluation not rejecting the null hypothesis had no effect on the number of casualty crashes at treated sites.

APPENDIX D – DISAGGREGATION OF CASUALTY CRASHES INVOVLING A PEDESTRIAN BY TREATMENT/CONTROL AND BEFORE/AFTER GROUPS

Of the 140,224 casualty crashes eligible to be included in the present analysis, 13,091 involved a pedestrian. Table D.1 shows the results of disaggregating these pedestrian crashes according to whether they occurred at treated sites or controls sites and whether they occurred in the pre or post-treatment period. The methods used to determine control sites and before and after treatment periods were the same as those described in Sections 3.2 and 3.3 respectively.

Table D.1: Summary of casualty crash data used in the analysis (pedestrian crashes only)

Before or After	Treated Site	Control	Not a Treated or a Control Site	Total
Prior to February 2001	61	1,108		1,169
During treatment installation period	6	25		31
Not associated with a Strip site	0	0	9,781	9,781
Subtotal of Omitted Crashes	67	1,133	9,781	10,981
After treatment period	121	652		773
Before treatment period	266	1,071		1,337
Subtotal of Case/Control Crashes	387	1,723		2,110
Total	454	2,856	9,781	13,091