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Traffic Safety Meta-Analysis of Reversible Lanes

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Traffic Safety Meta-Analysis of Reversible Lanes

by

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A THESIS

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Abstract

In this study, a meta-analysis was performed to review the potential effects of reversible lanes on traffic safety. After a systematic literature review, ten U.S studies were selected for inclusion in the analysis. These studies employed either a simple before-after comparison or a cross-sectional comparison methodology. Study results were extracted by transforming the collision rates and frequencies of these studies into a log-odds ratio.

The results of the meta-analysis suggest that a 30.9% increase in total collisions is expected. Property-damage-only collisions are expected to increase by 16.6% and injury collisions are expected to increase by 34.9%. Peak period collisions are expected to increase by 46.2%, while off-peak period collisions are expected to increase by 12.8%. The results were significant, with the exception of the off-peak period collision result.

A meta-regression was performed, which regress variables related to study type, collision type and operational characteristics of the study locations on the extracted log odds ratio. It was found that peak period operations were positively and significantly correlated with an increase in crashes. Study quality as observed through the study type variable was found to be a significant variable. Presence of left-turn restrictions was negatively correlated and significant, suggesting that application of a left-turn restriction can improve the safety of reversible lanes. The presence of delineator/barrier was generally negatively correlated, and both the presence of dynamic traffic control and static traffic control were generally positively correlated. Topics of time/study age trends and transferability were also discussed.

Preface

This thesis is original, unpublished, independent work by the author, A. Manuel. This thesis builds on earlier work presented at a Canadian Institute of Transportation Engineers conference by *Manuel, de Barros and Tay (2018)*, where an odds ratio analysis based on *Bahar (2010)* was used with seven of 10 of the studies included in this thesis. This thesis has changed considerably since this earlier work, in that additional changes to the methodology and additional studies have been included since, but the work is provided for the reader's reference in case the reader is interested.

Acknowledgements

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Finally, I would like to thank the myriad of people who encouraged me over the course of this journey, from other professionals to my friends in the Transportation Lab and in my personal life. And my family, especially my immediate family including my sister Abigail, my mother Violeta, my father Eradio, Consuelo my grandmother, my uncle Reynaldo, and our pets Rainbow and Glider for their continuous support throughout this degree. Whenever I doubted myself or I questioned my reason for starting this degree, my family was always there to bring me back up. And to my sister: I don't know for sure, but I intend to find out.

Dedication

To a family friend, Hannah, and to an acquaintance in Junior High, Phil. I'm sorry we didn't do more to prevent what happened to you. As transportation engineers and as a society, we can and must do better.

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CHAPTER ONE: INTRODUCTION

1.1 Background

The topic of traffic safety is an issue that has a profound effect on transportation network users from the personal level to the global level; costing more than \$500 billion dollars annually in direct costs, killing 1 million and injuring as high as 50 million people annually (*World Health Organization 2004*). For Calgarians alone, this cost is estimated to be between \$800 million and \$1.7 billion dollars annually (*City of Calgary 2015*).

Given the great societal cost of collisions around the world, the incorporation of traffic safety into transportation engineering processes and planning has been a well studied, researched and practiced field.

Another important topic in transportation engineering and planning is the issue of optimizing existing transportation facilities while minimizing cost and potential land requirements. Reversible lanes are one such traffic engineering tool, which temporarily reverse traffic flow in one or more lanes to respond to heavy directional traffic flow during peak periods (*Wolshon and Lambert 2004, 2006*). Safe operation of this type of transportation facility is achieved by dynamic and/or static signing, special pavement markings, as well as temporal and spatial transitions between off-peak balanced lane configurations and peak period unbalanced configurations (*Wolshon and Lambert 2004, 2006*).

1.2 Problem Statement

The literature on reversible lanes has generally focused on the traffic flow aspect, and this topic has become more well researched in recent years. The available traffic safety literature on reversible lanes, with relation to the traffic flow and traffic optimization aspect, has typically been studied by the way of simple before-after reviews of collisions, typically as one part of an extensive operational before-after review of roads where this treatment has been applied. There also exists cross-sectional studies comparing a collision frequency or rate of a roadway with reversible lanes to a similar roadway without reversible lanes. Although one previous meta-analysis of reversible lanes exists in the literature (*Elvik et al. 2009*), there is a potential to expand on this previous study with the inclusion of additional studies, performing additional exploratory and sensitivity analysis, and the inclusion of meta-regression analysis.

This additional analysis will provide an updated Crash Modification Factor for use by transportation practitioners and researchers to predict the potential change in collisions due to the installation of reversible lanes. This prediction tool is expected to facilitate analysis in terms of roadway functional planning, and cost-benefit analysis where this treatment is an option, especially with relation to the potential collision cost. With relation to the meta-regression analysis, the results will be of use to practitioners and researchers as a first attempt to correlate between collisions and covariates that may affect collisions, such as study quality and differing operational characteristics of reversible lanes from location-to-location. Practitioners can use the results of this analysis to better inform their choice in terms of cost vs. benefit for operational options and

potential mitigations in the implementation of reversible lanes such as use of delineators or left-turn restrictions.

1.3 Scope

The scope of this thesis is limited to observational studies of reversible lanes and analyzing collision data identified through a systematic review of the traffic safety literature, under study exclusion criteria of study age, study locations, study language, study type/methodology, as well as site/facility characteristics. These criteria are to be further defined in the methodology section of this thesis.

1.4 Research Objectives

The main objective of this thesis is to determine an updated measure of the traffic safety effect of reversible lanes by:

- Performing a meta-analysis of previous traffic safety studies on reversible lanes in order to determine an overall crash modification factor.
- Performing a meta-regression analysis to determine the effect of study characteristics and site operational characteristics on traffic safety.

In addition to providing an update to previous meta-analyses on this subject, this study will also attempt to answer the following research questions through the meta-analysis:

- Through an exploratory analysis, can the studies as identified, selected and extracted be combined to determine an aggregate estimate of the effect of reversible lanes on safety?

- What are the operational factors that affect the traffic safety of reversible lanes, and what is their magnitude and direction of effect?
- What is the effect of the study quality and/or study type on the results of the meta-analysis?

In this regard, the results of the meta-analysis will provide a quantifiable measure of the effect of reversible lanes from the existing literature which can be used to inform future research. The effect estimates provided by this study could also potentially assist in future transportation functional planning where reversible lanes are an option.

1.5 Thesis Organization

This thesis is organized into five sections, including the current introduction chapter. The four chapters following are as described briefly below:

CHAPTER TWO – PREVIOUS WORK: This chapter will summarize the characteristics of reversible lanes and summarize the current state of research on reversible lanes as a whole, as well as specific traffic safety research on reversible lanes.

CHAPTER THREE – METHODOLOGY: This chapter will summarize the methodology used for the meta-analysis, including the steps taken to perform the systematic literature search, selection criteria used to include or exclude studies, steps taken to extract study results, and steps taken to perform the meta-analysis itself.

CHAPTER FOUR – RESULTS AND DISCUSSION: This chapter will summarize and discuss the results of the meta-analysis, including the exploratory analysis and main analysis, which is composed of the traditional meta-analysis as well as the meta-regression.

CHAPTER FIVE – CONCLUSION AND FUTURE WORK: This chapter will summarize the study and the key findings of the analysis, as well as suggest future research directions for this research topic.

CHAPTER TWO: PREVIOUS RESEARCH

2.1 Introduction

This chapter will briefly discuss the history and characteristics of reversible lanes. A summary of the previous literature with relation to reversible lanes in general, as well as specifically with relation to traffic safety will follow.

2.2 History and General Characteristics of Reversible Lanes

Applications of reversible lanes in traffic engineering began as early as 1928 in Los Angeles referred to by Dorsey (1948 as cited in Wolshon and Lambert 2004, Guebert et al. 2010) as an off-centre lane movement. Since these early applications, use of reversible lanes grew through the 1940s to the 1960s on urban arterials (Wolshon and Lambert 2004, Guebert 2010). In the 1970s, reversible lanes were increasingly being applied on freeways, tunnels and bridges in the United States, Europe and Australia, and in the 1980's to 1990's were applied in combination with HOV and contraflow bus lanes (Wolshon and Lambert 2004, Guebert 2010). Most recently, reversible lanes have been applied in conjunction with emergency planning and evacuation measures with relation to terrorism and natural disasters (Wolshon and Lambert 2004, Guebert 2010).

In general, existing Canadian and American guidance documents characterize reversible lanes as any transportation facilities where the direction of traffic flow is reversed on one or more lanes for a temporary period of time over the course of the day (Wolshon and Lambert 2004, Guebert 2010). Planning guidance documents (Wolshon and Lambert

2004, ITE 2009, Guebert 2010) generally suggest that reversible lanes are warranted or should be considered when the following characteristics are met:

- Directional split larger than 65/35 percent during peak hours
- Average or operational speed decrease of greater than 25%
- Large proportion of road users using the facility composed of through-traffic
- Constrained right-of-way along the facility in question
- Adequate capacity on either end of the facility in question

2.3 Operational Characteristics of Reversible Lanes

In order to safely transition between periods where lane reversal is in effect and not in effect, reversible lanes are characterized by both spatial and temporal transitions (Wolshon and Lambert 2004, Guebert 2010). Spatially, there are typically five key sections referred to in existing guidance documents (Wolshon and Lambert 2004, Guebert 2010):

- Approach zone: normal mainline traffic is informed of the start of the reversible lane treatment
- Entry or decision zone: traffic must move or merge into and out of the reversible lane or lanes
- Travel zone: traffic moves according to the lane usage as established in earlier zones
- Exit Zone: traffic must move out of the reversible lanes in advance of the departure zone

- Departure zone: traffic leaves the road segments with reversible lanes and follow normal lane usage.

Spatially, the highest risk areas of reversible lanes are the entry and exit zones, given the need for lane-changing and merging in order to move into and out of the affected lanes (Wolshon and Lambert 2004, Guebert 2010).

Figure 1: Reversible Lane Zones (Source: Guebert, 2010)

Temporally, a reversible lane facility has specified frequency and duration (Guebert 2010). The most common reversible lane systems have a “fixed time” starting and ending at the same times, usually morning and evening peak hours on weekdays, although some systems are manually implemented at beneficial times by the traffic system operator (Guebert 2010). Some systems are also automatically implemented once the system has reached certain criteria such as a certain traffic volume, volume/capacity ratio and/or queue length threshold (Guebert 2010).

Reversible lane systems must also provide an adequate transition timing from reversible to default operation, which should be as brief as possible in the interests of efficiency, but long enough for drivers to adapt to the change in lane configuration (*Wolshon and Lambert 2004, Guebert 2010*). Transition timing occurs for most systems according to a calculated transition time, taking into account the length of the facility and the speed of vehicles travelling through the facility (*Guebert 2010*). The transition is then spatially transitioned through use of traffic control devices, or physically through the use of moveable barriers or delineators (*Wolshon and Lambert 2004, Guebert 2010*).

Several types of applications of reversible lanes exist, but can be grouped into two major groups, temporary/infrequent systems and permanent systems (*Guebert 2010*).

Temporary systems are typically used where a traffic volume increase is expected to be infrequent or occasional, such as during sports events, emergency evacuations, or for construction staging in order to utilize the remaining capacity in lanes not currently under construction (*Wolshon and Lambert 2004, Guebert 2010*). Temporary systems typically use some sort of temporary delineator and/or police/traffic operator control to ensure compliance, although there have been examples of locations that use permanent installations for temporary or infrequent systems (*Wolshon and Lambert 2004*).

Permanent systems are usually used as a response to recurring traffic congestion where there is an unbalanced directional flow, especially during peak hours of operation (*Wolshon and Lambert 2004, Guebert 2010*). Permanent systems can be further divided into two subtypes: physically separated systems and off-centre systems (*Guebert 2010*).

Physically separated systems use integrated systems of ramps and barriers in order to convey usage of the reversible lane or lanes to road users (*Guebert 2010*). Off-centre systems typically use traffic control devices to convey the availability of the reversible lane to road users in the peak direction of flow over a specific time (*Guebert 2010*). Typical traffic control devices used are static sign implementations, dynamic implementations such as lane control signals and gantries, as well as delineators and portable barricades (*Wolshon and Lambert 2004*).

An example of such an implementation on Centre Street in Calgary is illustrated in Figure 2 where the lane reversal is operational in the PM peak period and lane configuration is 3:1 in favour of the northbound direction leading away from downtown. In Figure 3, the lane reversal is not in effect during the afternoon off-peak period, and lane configuration is balanced at 2:2.

Figure 2: Centre Street Lane Reversal at 4 Avenue SW/SE Facing North - PM Peak Operational Period

Figure 3: Centre St Lane Reversal at 4 Avenue SW/SE Facing North - Afternoon Off-Peak Period

2.4 Previous General Research – Reversible Lanes

Reversible lanes have generally been a well-researched topic in transportation engineering, with several current guidelines governing or guiding their use in traffic engineering (*e.g. Wolshon and Lambert 2004, Guebert 2010*). Outside of traffic safety, studies of reversible lane operations have appeared to form the majority, if not a large part of the literature. Studies can be grouped into several categories, and these categories include design optimization of reversible lanes, feasibility studies of planned reversible lanes, and field studies of reversible lane performance.

2.4.1 Optimization Studies

Optimization of reversible lanes is a large subset of the literature. Several studies have reviewed optimization of the number of conventional reversible lanes with relation to segments and corridors (*Glickman 1973, Frejo et al. 2016*), as well as an integrated design optimization model for both segments and intersections along a reversible lane corridor (*Zhao et al. 2014*). Other studies include optimization of a network using a combination of reversible lanes and traveller information systems (*Wu et al. 2009*), as well as capacity modelling for contraflow left turn lanes (*Zhao et al. 2018*).

An emerging field of research in reversible lanes is optimization of dynamic lane reversals through optimization of traffic flows of both corridors and small road networks, especially with relation to autonomous vehicles (*Hauskinecht et al. 2011, Duell et al. 2016*). This line of research extends to intersections employing dynamic reversible left turn treatments (*Krause et al. 2015*).

Finally, another major subset of the optimization of reversible lanes is evacuation contraflow both in terms of evaluation of existing plans for evacuation (*Chien et al. 2009*), as well as in the development of an optimized evacuation plan through a given network (*Xie et al. 2010, Zhao et al. 2016*).

2.4.2 Feasibility and Planning Studies

Planning studies reviewing the feasibility of adding reversible lanes or comparing a planned reversible lane facility to other improvement options have also been performed. One study used the Highway Capacity Manual and empirical data from conversion of two-way streets into a one-way street to determine the potential effect on traffic (*Hemphill and Surti 1974*). Another study simulated the traffic flow of several planning options, including reversible lanes (*Markovetz et al. 1995*). Results from these studies have shown reversible lanes to be either the best option (*Markovetz et al. 1995*) or shown to be feasible with significant cost savings (*Hemphill and Surti 1974*).

2.4.3 Observational Studies

One other major subset of the reversible lane literature are observational studies either comparing the before and after states of the upgraded reversible lane facility, or comparing an existing reversible lane facility against other roads. These studies are typically all-encompassing operational reviews of traffic flow and travel time, with many of these studies showing considerable improvements in travel time and traffic volume (*e.g. DeRose 1966, Agent and Clark 1980*), or showed no increase in travel time despite a considerable rise in traffic volume (*Lalani and Baird 1981*).

2.5 Previous Traffic Safety Research – Reversible Lanes

Traffic safety research into reversible lanes can be grouped into two major subgroups.

The first subgroup are studies reviewing reversible lanes through studies of the ergonomics of a component of reversible lane systems, or through driver simulator studies reviewing the ergonomics of several components of reversible lane systems as a whole. The other subgroup of studies are observational studies analyzing traffic safety measures of effectiveness such as collision frequencies, rates or number of observed traffic conflicts. Meta-analyses of such observational studies have also been performed with relation to reversible lanes.

2.5.1 Human Factors and Ergonomics Studies

Earlier ergonomics studies with relation to reversible lanes appear to focus on the lane control component of reversible lanes separately from the rest of the system. Studies did this either through experimental studies of prototype signs and signals (*Strybel and Nassi 1986*), or through analysis of stated preference surveys with relation to participants' comprehension of lane control signal symbols (*Knoblauch et al. 1984, Ullman et al. 1993*). In the identified experimental study of prototype signs (*Strybel and Nassi 1986*) it was found that both the conventional "square lens" and "revolving beacon" devices were recognized except at extreme distances where the prototype revolving beacon device and sign combination were more easily recognized. In the two identified studies employing surveys (*Knoblauch et al. 1984, Ullman et al. 1993*), it was found that the 'red X' and 'green arrow' were universally recognized and interpreted correctly. However, with relation to other symbols, the results were more mixed. In *Ullman et al (1993)*, a diagonal

yellow arrow denoting upcoming closure of the lane was well recognized and interpreted, but a yellow X or yellow vertical arrow was interpreted more widely depending on other nearby lane signals. In *Knoblauch, Parker and Keegel (1984)*, a yellow flashing X used to denote a two-way left turn lane on a reversible lane facility was poorly interpreted when compared to a two-way left turn arrow symbol.

More recently, researchers have been employing driver simulation studies to study several components of reversible lane facilities. In *Valdés and Colucci (2017)*, a study was performed to determine the optimal lane width, and posted speed given varying times of day for a dynamic reversible toll lane in Puerto Rico. *Cruz et al (2018)* reviewed the effect of different exit signage on the exiting behaviour of motorists on the same road section in Puerto Rico. In *Zhao, Yun, Zhang and Yang (2015)*, driver comprehension of a reversible left turn lane, known as an “exit-lane for left-turn” (EFL), was tested by exposing non-familiar and familiar drivers to experimental signage and signal controls and reviewing reaction times, as well as rates of red-light and wrong-way violations.

2.5.2 Observational Studies

The traffic safety of reversible lanes is a generally well-studied and wide-ranging topic. The majority of these studies are not experimental in nature but observational in nature, by observing traffic safety performance of an existing road with a treatment in question. Traffic conflicts studies of reversible lanes review the number of violations or near-collisions rather than collisions, which generally occur more frequently than collisions (*Young et al. 2014*). Traffic conflict measures included encroachment rates into opposing

traffic (e.g. *Dey et al. 2011, Cao et al. 2014*) or left turn violation rates (*DeRose 1966*) during the operational period of the reversible lane. The remainder of studies identified were observational studies comparing and analyzing collision frequency or collision rate. This was done either through a simple before-after study, or through what has been called a non-regression cross-sectional study (*Bahar 2010*) or a comparative study (*Elvik 2003*). These study types will be discussed in more detail in the coming sections.

2.5.2.1 Traffic Conflicts

Traffic conflicts as a traffic safety measure of performance was developed in the 1960s to 1980s as an alternative method to collecting crash data (*Young et al. 2014*). The general methodology of studies employing the traffic conflicts technique is to observe the traffic stream in question for any “observed situation in which two or more road users approach each other in space and time [where] there is a risk of collision if their movement remains unchanged” (*Amundson and Hyden 1977, as cited in Young et al. 2014*). Over time, this research field has developed through the development of several formal surrogate safety measures for integration into video tracking technology and traffic microsimulation programs (*Young et al. 2014*).

Traffic conflicts studies of reversible lanes have generally been on-road observational studies using measures of encroachment into the reversible lane with relation to an arterial road (*Dey et al. 2011, Cao et al. 2014*), as well as aggressive lane changing and illegal parking. In *Cao et al (2014)* they found that the number of observed conflicts increased in the initial minutes of reversal when compared to observations before reversal

on a Chinese application of reversible lanes. *Dey, Ma and Aden (2011)* reviewed a reversible lane corridor in Washington, D.C., where they observed that the encroachments were due to either willing traffic violators, or unfamiliar drivers potentially confused by the pole and post mounted lane control as opposed to overhead signal/sign control. Both studies also found that utilization of the reversible lane is generally lower than in the surrounding concurrent flow lanes, which is in line with studies characterizing traffic flow on reversible lanes (*Lambert and Wolshon 2010*). *Agent and Clark (1980)* reviewed traffic conflicts as a result of congestion and found that conflicts per lane on the reversible lane section Nicholasville Road in Lexington, USA increased overall by 40%, while conflicts per lane in the peak period direction decreased by 30%. *DeRose (1966)* performed a review of left-turn violations and found that left-turn violations were more likely to occur at the extreme ends of the system leading to a review of lane control signal spacing on the project.

As discussed in Section 2.5.1, three studies using traffic conflicts methodologies used driver simulators to conduct their experiments. Two studies analyzed the average lane position of the existing vs proposed signage for a barrier separated reversible toll lane (*Valdés and Colucci 2017, Cruz et al. 2018*). *Zhao, Yun, Zhang and Yang (2015)* reviewed the rates of navigation errors, utilization rates, red light violations and wrong-way violations for an untrained and trained subgroup using an experimental reversible left-turn lane. No microsimulation-based traffic conflict studies were identified in the literature search.

2.5.2.2 Simple Before-After Collision Studies

Simple before-after studies in traffic safety are a comparison of crashes before introduction of a traffic safety treatment to crashes after introduction of a treatment, with the ratio or relative change in collision frequencies of rates from before to after being the traffic safety effect of the treatment (*Hauer 1997*). The key assumption in a simple before-after study is that the ‘before’ period is directly compared to the ‘after’ period assuming that the ‘before’ period is indicative of the ‘after’ period had there not been any treatment (*Hauer 1997*). Specifically, the simple before-after study is typically characterized by the following methodology as summarized from *Hauer (1997)*. The simple before-after measure of safety θ is defined as:

$$\theta = \lambda/\pi \quad (2.1)$$

Where λ is the after-collision count with the installed treatment and π is the ‘after’ collision count in a theoretical scenario where the treatment was not installed at the location (*Hauer 1997*). In this case, the after-collision count λ is simply the observed value after installation, but the theoretical ‘after’ collision count π where the treatment was not installed is assumed to be the observed ‘before’ treatment installation value. In summary this means the following:

$\lambda = L$, where L is the collision frequency during the ‘after’ study period with the treatment, and $\pi = K$, where K is the collision frequency during the ‘before’ study period.

θ greater than 1 indicates that the treatment may be unsafe, while a θ less than 1 indicates that the treatment may potentially be effective (*Hauer 1997*). If the before period varies from the after period, π is adjusted using a ratio of after periods to before periods (*Hauer 1997*).

The traffic safety effect can also be estimated as the difference between the collision frequencies of the after and before collision frequencies, given by:

$$\delta = \pi - \lambda \quad (2.2)$$

The before-after study is sometimes referred to as a naïve before-after study because the assumption made about the ‘after’ collision frequency if the treatment had not been installed (π) does not account for several confounding factors that could affect traffic safety, including (*Hauer 1997, ITE 2009*):

- changes in traffic volume,
- effect of other treatments or programs on subject roadways
- changes in collision reporting thresholds
- selection bias towards locations with high collision frequency
- regression to the mean effects

Hauer (1997) supposes that θ or δ taken from such an analysis may be due to the treatment but may also be due to these and other confounding factors which may not have been accounted for in the analysis. In addition, locations with high collision frequency

would theoretically be more inclined to be selected for treatment, and the phenomenon of regression-to-the-mean could overstate the traffic safety effect of a given treatment, as the collision frequency “regresses” towards the mean collision frequency from the high value (*Hauer 1997*). In general, studies that have methodologies that have limited or no control for confounding factors tend to overestimate the effect of a given treatment to be studied (*Hauer 1997, Elvik 2002*).

The weaknesses of the simple before-after study can be mitigated by using a before and after study period that is sufficiently long enough to capture enough collisions to increase the statistical accuracy, but short enough that the effect can still be attributable to the treatment (*Hauer 1997*). The general rule-of-thumb is to have a study period of around three years before and after treatment (*Hauer 1997*).

Despite the weaknesses of the simple before-after study, the majority of observational studies on the traffic safety effects of reversible lanes have been simple before-after studies, usually as a part of an extensive performance review of other performance measures such as travel time improvements.

Identified before-after studies have typically compared the traffic flow and operational behaviour before and after, as well as the collision frequency or collision rate for varying periods. Periods varied from 5 to 6 months (*e.g. Miller et al. 1979, Bretherton and Elhaj 1996*), 9 months (*e.g. Lalani and Baird 1981*), one year (*e.g. Miller et al. 1979, Agent and Clark 1980*), up to two years (*e.g. DeRose 1966, Miller et al. 1979*) and up to three

years (*Bretherton and Elhaj 1996*). Study periods are provided in the meta-analysis dataset as shown in Appendix B. Study results are generally mixed and range from a decrease (*DeRose 1966, Miller et al. 1979*), to no effect (*Agent and Clark 1980*), to an increase (*Miller et al. 1979, Lalani and Baird 1981, Bretherton and Elhaj 1996*) in collisions, although it appears that more studies report an increase in collisions rather than a decrease.

2.5.2.3 Comparative/Cross-Sectional Studies

Cross-sectional studies in the traffic safety context are studies that compare traffic safety measures of effectiveness of roads with the traffic safety treatment in question, with roads that do not have the treatment in question, over a common timeframe or snapshot in time (*Gross et al. 2010*). These types of studies are particularly useful when there are few before-after scenarios within the road network proposed for study (*Gross et al. 2010*).

Typically, cross-sectional collision data is analysed through multiple regression models which attempt to account for any potential differences between segments which may also affect traffic safety (*Gross et al. 2010*). However, in the context of reversible lanes, only direct comparisons between non-reversible lanes and reversible lanes have been performed, without the use of multiple regression analysis. In one meta-analysis methodology (*Bahar 2010*) these are referred as non-regression cross-sectional studies, and in other relevant meta-analysis, these studies are also referred to as comparative studies (*e.g. Elvik 2003*), or simply cross-sectional studies (*e.g. Elvik 2017*). For the

purposes of this study, the term comparative study and cross-sectional study will be used interchangeably to denote this kind of study.

Several studies were identified over the course of the literature review which were identified to be comparative studies, with generally consistent results trending towards an increase in crashes with installation of reversible lanes. *Dey, Ma and Aden (2011)* conducted their study in Washington, D.C. with collision data spanning over six years, where one road section with reversible lanes was compared to two nearby control/comparison roadway sections of similar length with similar traffic patterns. Their analysis showed that the percentage of peak period crashes was 8-10% higher on the reversible lane section, and the total number of crashes was higher on the reversible lane road section. The crash rate even after normalizing by traffic volume also continued to be higher on the road section with reversible lanes when compared to its nearby non-reversible lane counterparts. A similar and preliminary version of the paper by *Dey and Ma (2007)* shows similar results for three intersections along a reversible and non-reversible segment with similar traffic patterns, with around three times as many crashes overall on the reversible lane intersections.

In *Knoblauch, Parker and Keegel (1984)*, average collision rates for reversible lanes with two-way left-turn lanes for several U.S locations was compared to conventional two-way left-turn lanes without reversible lanes, as well as undivided arterials without any additional treatments from several U.S sources. Their report found that collision rates for reversible lanes were generally higher when compared to both conventional two-way left

turn lanes, as well as undivided arterials with a conventional cross-section, although the difference was not statistically significant.

In *Miller, Deueser, Wattleworth and Wallace (1979)*, comparison of the collision rate with the collision rate on a nearby control base of roads or a network of roads showed that collision rates increased when compared to the control base for two out of the three project areas. In a recent Phoenix area study (*City of Phoenix 2010*), it was found that the three-year crash rates between roadways with reversible lanes and nearby non-reversible lanes did not vary significantly. The collision rate in identified cross-sectional studies was reported as the number of collisions per million vehicle miles travelled for road sections, collisions per million entering vehicles for intersections. The exceptions were *Dey, Ma and Aden (2011)* which reported a frequency over similar segment lengths and adjusted for traffic volume, and *Dey and Ma (2007)*, which is similar to *Dey, Ma and Aden (2011)* but reports frequencies at three intersections each for a reversible lane segment and non-reversible lane segment with similar traffic patterns.

In general, cross-sectional studies suffer from a study design weakness, in that the crash frequency or rate could be due to known or unknown factors (*Gross et al. 2010*).

Although known factors can be controlled for through regression and/or matching of roads based on these factors (*Gross et al. 2010*), the effect of unknown factors, or known factors that have not been measured at the time of the study would not be captured and accounted for (*Gross et al. 2010*). Another weakness of cross-sectional studies is a lack of consistency in cross-sectional studies owing to differences in functional forms and

variables used of the regression equation (*Hauer 2010*). However, a cross-sectional study would be preferred in a case where there are not enough before-to-after cases for comparison to perform a before-after study (*Gross et al. 2010*).

2.5.2.4 Meta-Analysis

Meta-analysis is a statistical analysis of several results from a systematic review of the research literature or an “analysis of analyses” (*Elvik 2005*). Meta-analysis relies on the results of a systematic literature review which differs from a conventional “narrative literature review”, where relevant studies are described and results of studies are summarized in an informal manner (*Elvik 2005*). Systematic literature reviews are characterized by extensive searches with the goal of including all studies, even unpublished or gray literature not available in academic journals (*Elvik 2005*). Data in systematic reviews are extracted according to standardized procedure defined by study inclusion criteria, topic definitions and scope, and specific data extraction procedures (*Elvik 2005*). Such steps are summarized in *Elvik (2005)* and also briefly discussed in this section. These steps will be further developed in the following chapter on methodology.

- Define the topic of the proposed meta-analysis
- Conduct a systematic literature search
- Define criteria for study inclusion or exclusion
- Extract data from included studies, and
- Convert data to a common scale or ‘estimate of effect’

The topic of the meta-analysis should be defined as narrowly and precisely as possible in order to avoid mixing together studies that are too categorically different from each other (*Elvik 2005*). The systematic literature search should be based on several techniques, such as searches on computerized subject databases, contacting researchers who may have access to unpublished reports in the subject area in question, hand searching relevant journals, and by tracing back references in previously selected studies (*Elvik 2005*). These studies are then included or excluded based on inclusion criteria such as study age, language restriction and study type (*Elvik 2005*).

Once the systematic literature review is complete, data from included studies is then extracted to a common ‘estimate of effect’, which in traffic safety may be ‘proportion of target crash types’, comparison of collision rates, odds ratios and expected crash reductions of collision frequencies (*Elvik 2005, Gross et al. 2010*).

Once ‘estimates of effect’ have been collected from the included studies, meta-analysis can take place which usually consists of an ‘exploratory analysis’ and a ‘main analysis’ (*Elvik 2005*). In the exploratory analysis stage, tests are performed on the distribution of estimates to determine its overall behaviour in terms of asymmetry, modality and presence of publication bias (*Elvik 2005*). The main analysis combines the estimates of effect into a summary estimate, which is primarily based on a weighted average of each individual estimate, illustrated by the following general equation (*Elvik 2005, Gross et al. 2010*):

$$\bar{Y} = \frac{\sum W_i Y_i}{\sum W_i} \quad (2.3)$$

where:

- $\bar{Y}$ is the resulting summary estimate
- Y_i is the estimate of effect for each study from the first study to the *i*th study in the meta-analysis,
- W_i is the statistical weighting for each study.

Typically, this statistical weight is a function of the variance or standard error (*Elvik 2005*). This variance or standard error can be based on the following models and assumptions, which will be discussed in depth in future sections:

- Fixed effects model – between-study variance is assumed to be solely random
- Random effects model – between-study variance is systematic but does not identify or model these systematic differences

The choice of using each type of model is based on the homogeneity test, which indicates whether the variance between each estimate of effect is homogeneous, or heterogeneous (*Elvik 2005*). Another methodology that can be used is a mixed-effects model, which combines the fixed-effects model and random-effects model by means of a meta-regression analysis to account for both the systemic variation and random variation (*Thompson and Sharp 1999*).

Although meta-analysis has been used and applied extensively in the medical field since the first analyses performed in the 1970s, meta-analyses in transportation research is

generally a much younger and less extensive field (*Elvik 2005*). However, a cursory review of meta-analyses in transportation and traffic safety shows that this research field is becoming extensive in itself. Meta-analysis techniques have been used to study and quantify the traffic safety effect of red light cameras or speed cameras (*Erke 2009, Høye 2013*), roundabouts (*Elvik 2017*), bypasses (*Elvik et al. 2007*), speed limit changes (*Castillo-Manzano et al. 2019*), drug use and use of drunk driving checkpoints (*Erke et al. 2009, Elvik 2013*), road safety campaigns (*Phillips et al. 2011*), cell phones and texting (*Caird et al. 2014, Simmons et al. 2016*) and many others.

With relation to reversible lanes, one meta-analysis was identified from *Elvik, Vaa, Høye and Sorensen (2009)*, which reviewed collision statistics from five reversible lane studies – one Norwegian study and four U.S studies. Their meta-analysis found that installation of reversible lanes would potentially result in an 18% increase in injury collisions, a 15% increase in collisions occurring during the peak period, and a 4% increase overall for all collision types combined. However, *Elvik et al (2009)* found that the heterogeneity or variation between studies was relatively high and denoted potentially systematic differences between studies. They also found that their results were not statistically significant. Therefore, they cautioned readers from relying heavily on their results, stating that the findings may be misleading due to the available studies' lack of control of confounding factors. A meta-analysis of reversible lanes has not been identified to have occurred since the study by *Elvik et al(2009)*.

Despite the reported increase in collisions from the meta-analysis of reversible lanes performed by *Elvik et al (2009)*, a theoretical cost-benefit analysis in *Elvik et al (2009)*

showed that the cost benefit ratio ranged between 9.71 and 6.49 in favour of reversible lanes. This was because of the larger benefit in road user cost, especially travel time savings, which was largely independent of the increased collision cost associated with a collision increase ranging from 0 to 15%. This ratio is in line with cost-benefit analyses done by others (*Agent and Clark 1980, Lalani and Baird 1981*). However, there have been moves to remove existing reversible lanes at some locations due to safety concerns (*e.g. Fenston 2018*).

2.6 Summary

In this chapter the general characteristics of reversible lanes especially on arterial roadways was discussed, including a brief history, and relevant operational characteristics. Then a conventional narrative literature review of the studies reviewing reversible lanes was performed. In general, the topic of reversible lanes is a well studied field, especially in subtopics of optimization, planning studies and options analysis, as well as observational studies of operations. Especially in the case of observational studies of reversible lanes in terms of traffic volume, travel time and travel speeds, reversible lanes have been shown to considerably improve these performance measures. In the case of planning options analysis, reversible lanes have generally been shown to be the best potential option out of all reviewed options.

Reversible lanes have also been well studied in the field of traffic safety, with studies reviewing the ergonomics of reversible lane systems or components of these systems. Observational studies have also been performed on such facilities, with a focus on either

overall operations by reviewing traffic volume changes, travel time changes and overall utilization rates in addition to traffic safety (*e.g. DeRose 1966, Agent and Clark 1980, Lalani and Baird 1981, Dey et al. 2011*), or traffic safety specifically (*e.g. Bretherton and Elhaj 1996*).

Study methodology for identified studies are either simple before-after studies which review the state of the road before-and-after, or comparison studies which compare the traffic safety of a treatment road to roads that are similar in aspects such as traffic volume, cross-section or geometric design over a common period of time. The majority of identified simple before-after studies generally reviewed traffic safety through comparison of the collision frequencies and in some cases a rate in units of distance travelled or units of entering vehicles of a treated road before and after installation.

The cross-sectional or comparative studies identified do not perform a regression analysis, and control for either a small number of factors or do not appear to control for any factors. These cross-sectional studies compare the collision history of a treated road or set of treatment roads with a comparison set of similar roads except for the treatment in question. In general, these studies make a comparison of the rate or frequency of the reversible lane segment against either a nearby non-treated section of road along the same corridor or close to the corridor, or makes a comparison to an average rate obtained from a network or set of roads. The majority of identified cross-sectional studies reported collision history as a rate with units of exposure in vehicle-distance travelled or units of

entering vehicles, although some studies reported a collision frequency (*e.g. Dey et al. 2011*)

In general, the results are mixed amongst the studies, with a trend towards an overall increase in crashes. *Elvik et al (2009)* performed a meta-analysis of several road safety treatments, including reversible lanes. The results of five studies were combined in a meta-analysis of several road safety treatments, including reversible lanes. In this meta-analysis, it was found that injury collisions would potentially increase by 18%, peak period collisions would potentially increase by 15%, and overall, there would be a 4% increase upon installation of reversible lanes. However, their results were non-significant, and analysis of the between-study variance showed that the estimates of effect were heterogeneous. Therefore, the authors of the study cautioned readers from relying heavily on the effect estimates provided.

Although meta-analyses of reversible lanes have been performed in the past, this study will add to the current body of knowledge by adding additional studies that may have been missed by the previous study by *Elvik et al (2009)*. This study will also build on the previous study by performing a meta-regression analysis. In *Elvik et al (2009)*, a random-effects meta-analysis was performed, but a meta-regression analysis or mixed-effects meta-analysis, which attempts to explain any variation between-studies through the use of relevant variables was not performed. Topics related to the methodology will be further discussed in the upcoming chapter.

CHAPTER THREE: METHODOLOGY

3.1 Introduction

This chapter will discuss and summarize the methodology for the meta-analysis section of this thesis, including the steps taken to perform the systematic literature review, data extractions, as well as the steps taken to perform the exploratory and main analysis parts of the meta-analysis.

This study builds on an earlier attempt to perform a meta-analysis of reversible lane traffic safety studies as outlined in *Manuel, de Barros and Tay (2018)* by using a raw odds ratio methodology with study quality scoring based on *Bahar (2010)*. This study differs considerably from the previous study in that a log-odds ratio methodology without study quality scoring is used on the basis of studies such as *Elvik et al (2009)*, although some elements of the previous analysis are retained such as adjustment of collision data for unequal study periods. This study also elects to perform a meta-regression as opposed to implementing study quality scoring which will be discussed in the following sections.

3.2 Meta-analysis of Previous Studies

The general methodology for the meta-analysis section of this thesis will be based on the general framework for systematic literature review and meta-analysis of traffic safety studies in *Elvik (2005)* and *Gross, Persaud and Lyon (2010)*. The overarching steps required before conducting a meta-analysis are as follows from *Elvik (2005)*, and will be discussed further in the following sections.

- Topic definition and systematic search
- Definition of study inclusion criteria
- Data extraction
- Conversion of estimates of effect to common scale

Once these steps have been taken, exploratory analysis of study data is performed, followed by a main analysis where study results are combined into a mean estimate of effect.

3.2.1 Topic Definition, Systematic Search and Study Selection Criteria

The topic of interest is to determine the effect of reversible lanes from available observational studies analyzing collision data.

The systematic literature search was performed using the following research databases:

- Transport Research International Documentation (TRID) database
- Institute of Transportation Engineers (ITE) Library
- Google Scholar

Search terms used included a combination of the treatment type and the estimate of effect desired. Treatment type search terms included ‘reversible lane’, ‘reversible lanes’, ‘reversible lan*’, ‘lane reversal’, ‘lane reversals’, ‘lane reversal*’, as well as ‘tidal flow’, ‘tidal traffic’, ‘tidal flow traffic’, ‘tidal flow lane’, ‘tidal flow lanes’, ‘tidal flow lane*’. Estimate of effect terms included ‘safety’, ‘accident rate’, ‘accident frequency’, ‘collision

rate’, ‘collision frequency’, ‘crash rate’, ‘crash frequency’, as well as ‘collision’, ‘crash’ and ‘accident’ and relevant word stems for these terms. Appendix A includes a table of the search terms and search engines used for each query performed during the course of the study, as well as estimated or actual times and dates at which the query was executed.

Where possible, an asterisk was used to include results with different stems from a given root word. For example, a query using “lan*” would return lan-e, lan-es, lan-ing, etc.

With relation to Google Scholar, it is generally known that Google Search automatically stems search terms, except with relation to exact match, where the search term is placed inside quotation marks, as per Google Search Help Page on refining web searches (*Google 2019*). In this case, exact match was employed on the search terms used, and stemmed manually on the treatment type search term and estimate of effect search term.

Studies were included or removed from consideration based on the following restrictions:

- Study type: Observational studies reviewing historical collision frequencies and collision rates
- Facility type: Any urban arterial road applying permanent reversible lanes for congestion mitigation without permanent non-traversable median separation between opposing lanes of traffic, a.k.a off-centre application
- Language restrictions: English language only
- Study age, location: No restrictions

In this case, an “off-centre” reversible lane is defined according to the Transportation Association of Canada guideline for reversible lane systems (*Guebert 2010*) which is defined as the temporary taking of a lane or lanes in the off-peak direction for peak direction travel. This temporary taking is typically for the purposes of recurring congestion mitigation, temporary accommodation of construction and special events traffic, as well as emergency evacuation purposes (*Wolshon and Lambert 2004, Guebert 2010*). Physically separated systems as defined in *Guebert (2010)* were not considered in the analysis. Studies reviewing traffic safety of reversible lanes in a temporary application such as for emergency evacuation or construction, or other facility types with limited access such as freeway or expressway applications were also omitted from the analysis.

These restrictions were made in keeping with the need to keep “theoretically distinct categories apart” (*Elvik 2005*). In this case, the inclusion criteria given serve to narrow the scope of observational studies to a subset of reversible lanes that allow some level of access through private accesses and public intersections, with performance indicators narrowed down to the use of collision frequencies and rates.

Other study types that were excluded include studies that reviewed traffic safety effects through simulation or through traffic conflicts methods in keeping with the same reasons for restriction as above in terms of separating distinct categories with relation to key performance indicators. Studies that include a percent reduction but do not include a frequency or collision rate were also excluded from the study in order to facilitate

calculation of the estimated standard error. Simple before-after studies that provide a frequency, but do not provide a clear study period were also excluded. Reasons for exclusion will be developed in later sections.

Ideally, the systematic literature search preceding a meta-analysis should attempt to be comprehensive and attempt to collect all studies on the topic in question, and therefore it is ideal that there be few, if any, restrictions on study age, location or language (*Elvik 2005*). Restrictive selection criteria can also lead to selection bias in some cases (*Sterne et al. 2005*). In addition, design and operation standards for reversible lanes still generally vary from jurisdiction to jurisdiction (*Wolshon and Lambert 2004, Guebert 2010*), so there does not appear to be a clear date to base a study age restriction, such as a date of an introduction of a uniform standard/guideline, or date of an influential technological change affecting reversible lane design. Therefore, no restrictions were placed on study age or location. However, a restriction on language was placed to facilitate understanding of the study material.

3.2.2 Data Extraction

The study author, paper title, publication year, location where the study was conducted, and location or name of the source were extracted from each search result. A preliminary and cursory screening of the search results was performed by reviewing abstracts and scanning the paper if readily available for any discussion of collision/accident/crash rates or frequencies in the body of the paper or abstract, specifically with relation to reversible lanes. The study was then selected on the basis of the study abstract or body matching the

selection criteria. Selected studies were then also traced using the ancestry technique, which is summarized from *Elvik (2005)* as using reference lists from selected studies to retrieve relevant studies for potential inclusion.

3.2.3 Conversion of Estimates to Common Scale

The general methodology for estimating the traffic safety effect and standard error in this study is based on the log-odds ratio method of analysis, which is further explained in *Shadish and Haddock (2009)*, *Elvik et al (2009)* and summarized in this section. In brief summary, the odds ratio is calculated using the following ratio, as per the notation of *Shadish and Haddock (2009)*

$$o_i = \frac{AD}{BC} \quad (3.1)$$

where:

- o_i is the odds ratio for the i th study,
- A is the traffic safety measure in the after period for the treatment group,
- B is the traffic safety measure in the before period for the treatment group.
- C is the measure as observed in the before period for the control or comparison group,
- D is the observed measure in the after period for the comparison group.

The traffic safety effect measures most commonly captured and used are proportions or percentages, collision rates, collision frequencies, as well as odds ratios of these collision rates and frequencies (*Elvik 2005, Elvik et al. 2009*).

When a study employs a simple before-after study design as discussed in the previous chapter, there is no comparison group, so the odds ratio simplifies to a ratio of the observed traffic safety effect after and the effect before for the treated road in question, or $o_i = \frac{A}{B}$. Studies employing a non-regression cross-sectional design as defined in *Bahar (2010)* and as discussed in previous sections would have the same ratio where the observed effect for the treatment road would be A , the expected after-safety effect, while B would be the expected before-safety effect, expressed by the observed effect for the comparison road.

Log transformation of the odds ratio yields the log-odds ratio, l_i . Log transformation of odds ratio is typically performed as the distribution of raw odds ratios is usually skewed, resulting in a mean estimate that may be much larger than the true mean especially where the sample size is small, while a logarithmic transform of the odds ratios is normally distributed in general (*Agresti 2013*).

$$l_i = \ln(o_i) \quad (3.2)$$

The variance of log-odds ratio for study i is then estimated with the following:

$$v_i = \frac{1}{A} + \frac{1}{B} + \frac{1}{C} + \frac{1}{D} \quad (3.3)$$

Again, C and D are excluded if the study is a simple before-after study or non-regression cross-sectional study.

3.2.4 Adjustment of Traffic Safety Effects – Different Study Periods

In most cases in the literature collected, before and after study periods, or the snapshot period of time in cross-sectional studies were generally of equal or near-equal periods. In these cases, the traffic safety effect as calculated using the log-odds ratio would remain unchanged. However, there were some studies that had unequal study periods. In order to include these studies, a method from *Hauer (1997)* was adapted for use in this study, which adjusts the before period estimate-of-effect using a ‘ratio of durations’ r_d , for study i as given below.

$$r_d(i) = \frac{R_a(i)}{R_b(i)} \quad (3.4)$$

Where $R_a(i)$ is the duration of the after period for the study or entity i , and $R_b(i)$ is the duration of the before period for the study or entity i . The adjusted before estimate-of-effect, $B'_{adj,i}$ is then given as the following, where B_i is the original collision rate or frequency during the before study period in the treatment group.

$$B'_{adj,i} = B_i \times r_d(i) \quad (3.5)$$

For any studies that provide a collision frequency, an explicit study duration either in the form of dates or a stated study duration was deemed to be required in order to perform this calculation. For any studies that only provide a collision rate where study duration

was a denominator or assumed denominator, a study duration was not required as the duration has already been normalized for the entity or entities in question. For example, a rate in million vehicle miles or kilometers travelled for segments or corridors, or million entering vehicles for intersections, would include a duration under the commonly known definition for this rate.

3.2.5 Adjustment of Traffic Safety Effects – Traffic Volume Changes

Hauer (1997) provides a methodology for correction or control of traffic flow changes.

Bahar (2010) also provides a methodology for correction of estimates of effect assuming a linear relationship between collision frequencies and traffic flow change if there are traffic volumes available but no known relationship between traffic safety and traffic volume for the treatment. The effect of not controlling for confounding factors such as traffic volume is known in traffic safety to overestimate traffic safety effects (*Elvik 2002*). However, given the variation in study quality in terms of available traffic volumes, variation between studies in the use of rates and frequencies, as well as a lack of information in the collected studies related to the traffic volume to collision frequency/rate relationship, correction of estimates-of-effect due to traffic volume changes was omitted from the analysis.

3.2.6 Adjustment of Traffic Safety Effects – Study Quality

Christensen (2003) summarizes methods to incorporate study quality, these include the following five methods:

1. Define quality threshold and leave out worst studies below threshold
2. Stratify and perform separate meta-analysis based on quality
3. Modifying statistical weight by quality score
4. Use meta-regression
5. Sequential combination of trial results according to study quality or quality score

Method 1 is generally criticized for the arbitrariness of using a quality threshold, while methods 2 and 5 indirectly apply a quality threshold through stratification and reliance on the highest quality stratum in the case of method 2, while method 5 provides a score based on a sequential allowance of lower quality studies (*Christensen 2003*). Method 5 would be an indirect application of method 1 if a mean estimate is chosen which does not include all selected studies (*Christensen 2003*). Methods 3 and 4 are generally preferred methodologies (*Christensen 2003*). Method 3, modification of statistical weight with use of quality score was discounted due to a need to develop a valid and reliable methodology for gauging traffic safety study quality, which is outside the scope of this thesis. However, *Christensen (2003)* recommends use of Method 3 given the method's ability to directly influence the statistical weight based on study quality. Study quality scoring methodologies exist (*Bahar 2010*), but were developed using a raw odds ratio methodology, rather than a log odds ratio methodology. An earlier attempt to apply a

quality score as defined in *Bahar (2010)* for an early version of this study applied to an odds ratio conversion of the data is given in *Manuel, de Barros and Tay (2018)*.

Method 4, use of meta-regression analysis with aspects of study quality as independent variables will be the methodology used to account for quality and will be discussed in the coming sections. Method 2 will be used for exploratory reasons to determine any trends and behaviours due to the two study types and to gauge overall study quality between the study types.

3.2.7 Stratification/Subgroup Analysis

For both the exploratory and main analysis, summary estimates are provided for all estimates from all studies combined. Effect estimates were also stratified into subgroups related to collision severity and time of occurrence, as well as by study methodology.

On the basis of previous meta-analysis for reversible lanes by *Elvik et al (2009)*, selected studies and their estimates of effect will be stratified according to the following collision severities and types in the exploratory and main analysis, with additional subgroups in the PDO, off-peak and study methodology category

Stratification in the main and exploratory analysis will also be performed with relation to the two study methodology types as identified in previous chapters; Simple Before-After, and Comparative/Cross-Sectional studies.

The following is a summary list of the stratifications made for this analysis.

- Collision Severity
 - o Injury – All Collision Types
 - o Property Damage Only (PDO) – All Collision Types
 - o Unspecified Severity – All Collision Types
- Collision Time-of-Occurrence
 - o Unspecified Severity – Collisions Occurring during Peak Period or Reversible Lane Operational Period
 - o Unspecified Severity – Collisions Occurring during Off Peak Period or Reversible Lane Non-Operational Period
- Study Methodology (Simple Before-After, Comparison Study)
- All Estimates Combined

3.3 Exploratory Analysis

Once each study finding is converted to a common estimate of effect and corresponding standard error, an exploratory analysis is then performed in order to determine the feasibility of performing the main analysis, and if feasible, to inform the methodology of the main analysis (*Elvik 2005*). In line with *Elvik (2005)*, an exploratory analysis will be conducted by visual inspection of funnel plots of the estimate of effect against a measure of the quality of the study, such as statistical weight, variance, sample size or standard error for general shapes, trends and other behaviour such as funnel plot asymmetry. In line with recommendations from *Sterne and Egger (2001)*, the standard error will be the vertical axis variable in order to more easily assess smaller studies, as use of the variance

or inverse variance (statistical weight) compresses studies of smaller statistical weight. This is in order to catch what is known in medical meta-analysis as small-study effects where small studies tend to overestimate the effects of a treatment (*Sterne et al. 2001*). The level of asymmetry in the study data will also be quantitatively studied in order to gauge potential levels of bias, which will be discussed in more detail in the coming sections. Other steps include a review of the level of systematic variation/homogeneity in the study results, as well as a review of the level of potential publication bias in the given studies.

A fixed-effects approach will also be used throughout the development of the funnel plot, the exploratory analysis, as any sensitivity analysis due to the random-effect method's tendency to place more statistical weight on smaller, more imprecise studies results with lower standard error, which could affect the likelihood of detecting a potential bias in the collected studies (*Poole and Greenland 1999, Sterne and Egger 2001*).

3.3.1 Funnel Plot Asymmetry

In a literature survey of meta-analysis methods related to responding to asymmetry performed by the Institute for Transportation Economics in Norway (*Christensen 2003*), three additional tests were identified to quantify the level of asymmetry in addition to the trim-and-fill test. These tests were the Egger Linear Regression Test, Begg and Mazumdar Rank Correlation Test, and Sugita et al Method. The Sugita et al Method assumes one missing study which represents the weighted average of the potentially missing effect estimates (*Christensen 2003*). This method was discounted by *Christensen*

(2003) due to the results of an experiment by the Institute for Transportation Economics showing a reasonable estimate of the weighted mean, but a precision or statistical weight that is negative which is not possible. This test was not discussed further in *Christensen (2003)*, while the remaining two tests will be discussed further.

The Begg and Mazumdar Rank Correlation Method measures the level of asymmetry by standardizing the estimates of effect, and then comparing the ranks of the standardized estimates of effect for each study with their corresponding variances of each study (*Sterne and Egger 2005*). The Egger Linear Regression Method performs a regression of the study effect estimate on a measure of the study quality, which is the effect estimate's standard error (*Sterne and Egger 2005*). Both methodologies are further discussed in *Sterne and Egger (2005)*

The Egger Linear Regression method has been shown to be slightly more powerful than the Begg and Mazumdar Rank Correlation Method (*Sterne and Egger 2005*) and generally was more consistent in identifying significance in simulation studies (*Christensen 2003*). Therefore, *Christensen (2003)* recommended its use over other methods. This is despite some identified weaknesses from the literature related to the Egger Test being a biased estimator due to the effect estimate being correlated with its precision/standard error (*Christensen 2003, Lin et al. 2018*). In an empirical study of several asymmetry/publication bias tests, this has potentially manifested itself in a higher rate of statistical significance over other tests, although it is not clear if this is due to

increased sensitivity or due to the higher rate of Type I or false positive error (*Lin et al. 2018*).

Within literature reviews of medical meta-analysis, additional guidance and information exists with relation to tests that are variants of the Begg and Mazumdar Rank Correlation Test or Egger's Test (*Sterne et al. 2011, Jin et al. 2015*) or even newer methodologies such as selection models (*Jin et al. 2015*). However, these literature reviews of asymmetry tests typically refer to randomized control trial studies in medicine which differ from observational studies in terms of presence of a variety of unknown and known confounding factors rather than publication bias or selection bias (*Egger and Schneider 1998, Sterne et al. 2011*). In the case of observational studies, the use of funnel plots and corresponding tests in analysis would be less useful given the potential effect of these confounding factors (*Egger and Schneider 1998*). The simulations that these reviews have referred to have also generally not appeared to simulate situations that would be present in such an observational study (*Sterne et al. 2011*).

Guidance on observational studies is comparatively less available (*Mueller et al. 2018*). One synthesis article of recommendations from meta-analysis of observational studies provided some information as to the need to assess publication bias and heterogeneity through a funnel plot visual inspection or through statistical tests, although these studies did not provide a recommended test (*Mueller et al. 2018*). There did not appear to be any additional guidance on the subject of asymmetry in an observational study setting, especially in traffic safety from the results of the literature review.

Given the absence of recommendations or suggestions on asymmetry tests in observational studies, a cursory survey of relatively recent meta-analysis in traffic safety was performed and showed that analysis of asymmetry was generally done through visual inspection and subjective interpretation of the funnel plot rather than any statistical method, or used a combination of visual inspection and the Trim-and-Fill test (*Erke et al. 2009, Høye 2010, 2014, 2018, e.g. Phillips et al. 2011, Elvik 2013, 2017, Simmons et al. 2016*). Some studies used Egger's Test (*Caird et al. 2014, Castillo-Manzano et al. 2019*) or Begg's Test (*Simmons et al. 2016*) without additional information on justification. One study reported the Fail-Safe N, which is a calculation of the number of hypothetical studies required to reduce the results of the meta-analysis from its calculated value to a null value (*Castillo-Manzano et al. 2019*). However, this method is generally not recommended due to its lack of justifiable statistical criteria and susceptibility to misinterpretation (*Becker 2005*). Given the results of this cursory literature review, this study will use the Egger Linear Regression Test as suggested by *Christensen (2003)*. Other reasons for selection of this test over others is for the test's ease-of-understanding, availability in existing analysis software, and subsequent review in the transportation context through *Christensen (2003)*.

The following is a more detailed discussion of the Egger Linear Regression Method, summarizing the methodology as expressed in (*Egger et al. 1997*), and *Sterne and Egger (2005)*. Egger's Method is a linear regression of the standard normal deviate z_i for each trial i against the precision of each trial. In this case, z_i is defined as the estimate of effect

T_i divided by the standard error s_i ($z_i = \frac{T_i}{s_i}$), and precision p_i is the inverse of the standard error for each study trial ($p_i = \frac{1}{s_i}$). In this case, T_i is the log odds ratio l_i for each trial. The regression equation is then defined as the following from *Sterne and Egger (2005)*:

$$E[z_i] = \beta_0 + \beta_1 p_i \quad (3.6)$$

An identical regression equation that uses the effect estimate as the independent variable instead of the standard normal deviate and regresses based on the standard error is given by the following (*Sterne and Egger 2005*).

$$E[T_i] = \beta_0 s_i + \beta_1 \quad (3.7)$$

Between-study heterogeneity is calculated by multiplying the variance with an overdispersion parameter ϕ (*Sterne and Egger 2005*) as per the definition for Egger's Test. This multiplicative factor method of accounting for heterogeneity is discussed in more depth later in this chapter and in *Thompson and Sharp (1999)*. The effect estimates are assumed to be normally distributed, and regression equation given in Equation (3.7) can also be expressed in the following statistical model from *Sterne and Egger (2005)*.

$$T_i \sim N(\beta_0 s_i + \beta_1, \phi v_i) \quad (3.8)$$

Once the regression is performed, a test on the slope of the resulting line is performed, with the null hypothesis being a lack of funnel plot asymmetry and therefore zero slope

($\beta_0 = 0$), alternative hypothesis being a slope that shows $\beta_0 \neq 0$ signifying the presence of funnel plot asymmetry (*Sterne and Egger 2005*). A negative slope implies asymmetry favouring the left side, positive slope corresponding to asymmetry favouring the right side, and a vertical line corresponding to zero slope on a funnel plot (*Sterne and Egger 2005*).

In general, the two tests mentioned above note that the statistical power is relatively low if the number of effect estimates is less than or equal to 10 (*Sterne and Egger 2005*). In this case, as all subgroups identified had more than 10 studies identified in each group, the funnel plot asymmetry test will be carried out for all subgroups. In *Egger et al (1997)*, given the general low sample size of studies and the relatively low power of the test, they suggest using a 10% two-tailed significance level for this test. This value will be used for this test in this study.

3.3.2 Systematic Variation Between Studies

The systematic variation between studies will be tested using the homogeneity Q-statistic which is defined as the following based on notation in *Shadish and Haddock (2009)*.

$$Q = \sum_{i=1}^n w_i T_i^2 - \frac{(\sum_{i=1}^n w_i T_i)^2}{\sum_{i=1}^n w_i} \quad (3.9)$$

Where T_i is the measure of effect estimate for the i th study, such as the odds ratio or log odds ratio, and w_i is the statistical weight, which is the inverse of the variance v_i .

$$w_i = 1/v_i \quad (3.10)$$

If the homogeneity test statistic is smaller than the critical Chi-square value, the null hypothesis that there is no systematic variation between study results, or that the studies are homogeneous cannot be rejected (*Shadish and Haddock 2009*). If the homogeneity test statistic is larger than the critical Chi-square value, the null hypothesis is rejected, and the estimated effects are not homogeneous (*Shadish and Haddock 2009*). The results of this analysis would then inform the model to be used in the main analysis, which include a fixed-effects, random-effects, or mixed-effects analysis. A 90% confidence level was chosen for the purposes of the analysis due to the low power of the test (*Christensen 2003*). More discussion on the use of random, fixed or mixed effects modeling is discussed in the Main Analysis section of this report.

In addition to the homogeneity test, the I^2 statistic was calculated as per *Higgins and Thompson (2002)*, which is the proportion of the total variation that can be attributed to heterogeneity. The equation is given as follows converted to a percentage from *Shadish and Haddock (2009)*, where the sample size of effect estimates is given by k and Q is the homogeneity statistic given by Equation (3.9).

$$I^2 = 100 \left(\frac{Q - (k - 1)}{Q} \right) \quad (3.11)$$

General guidelines to determine the relative level of heterogeneity given I^2 are provided in *Shadish and Haddock (2009)*, in which an I^2 of 25% would denote a small level of heterogeneity, 50% a medium level of heterogeneity, 75% a high level of heterogeneity.

3.3.3 Publication Bias

The effect of potential publication bias is generally less important than the effect of other sources of heterogeneity in meta-analyses of observational studies when compared to randomized controlled trials due to other confounding factors (*Egger and Schneider 1998*). However, it is still not clear if the presence of publication bias is more or less prevalent in meta-analyses of observational studies compared to randomized control trial studies (*Mueller et al. 2018*), and should still be assessed. Within traffic safety, evidence of potential publication bias was found through a trim-and-fill assessment of a handful of previous meta-analyses, although they could not comment on overall prevalence (*Høye and Elvik 2010*). Their results suggested that publication bias could lead to overestimation of the traffic safety effect, although this could be in addition to or due to other factors such as poor study design (*Høye and Elvik 2010*).

In this regard, potential publication bias will be evaluated through a qualitative assessment of funnel plots of the standard error of each extracted effect estimate against the effect estimate itself. The qualitative assessment is informed by *Sterne, Becker and Egger (2005)*, which includes interpretation information for identifying potential publication bias in funnel plots. Specifically, a funnel plot with noticeable gaps at the extreme end indicates potential suppression of smaller studies of higher standard error where significance is less likely (*Sterne et al. 2005*).

In addition, a more quantitative review of potential publication bias will be performed using trim-and-fill analysis as defined in *Duval and Tweedie (2000)*. In a trim-and-fill

analysis, extreme estimates of effect on the asymmetric side of the funnel plot are “trimmed” and then replaced or “filled” on the opposite side of the plot before the trimmed estimates are included in the calculation of the mean estimate (*Duval and Tweedie 2000*). The resulting mean value can then be compared to the original estimate in a sensitivity analysis to determine the level of publication bias in the studies (*Duval and Tweedie 2000, Duval 2005*).

Specifically, as described in *Duval (2005)* if there are n observed studies, there are k_o suppressed studies due to publication bias. It is assumed that the suppressed studies would have been on the extreme right side of the funnel plot (*Duval 2005*). This is due to the assumption or inference that studies on the right side that are negative or unfavourable to the treatment would be suppressed where the studies are not statistically significant or of lower precision, while studies on the left side favourable to the treatment would be more likely to be published (*Sterne et al. 2005*).

As described in *Duval (2005)* in order to estimate k_o , each observed estimate of effect T_i , is “centred” with respect to the mean estimate of effect $\bar{T}$, given by $X_i = T_i - \bar{T}$. This mean estimate of effect can be calculated using the fixed-effects or random-effects method as will be discussed in later sections. Absolute values of X_i are then assigned a rank from 1 to n , defined as $rank_i$.

The number of suppressed studies k_o can then be estimated with either R_o or L_o summarized below with notation from *Duval (2005)*.

$$R_o = \gamma^* - 1 \quad (3.12)$$

$$L_o = [4S_{rank} - n(n + 1)]/(2n - 1) \quad (3.13)$$

where

- γ^* is the total number of studies n subtracted by the rank of the rightmost, or most negative effect estimate
- S_{rank} is the sum of the ranks for all $X_i > 0$.

The result given by the estimator is then rounded to the nearest integer, and the number of extreme rightmost studies are reflected to the left side of the plot. In the statistical package used, and in the analysis performed in this report, the default option using L_o is used due to its increased robustness under certain funnel plot configurations, although both estimators are generally applicable and useful (*Duval 2005*).

In the statistical package used, the side where publication bias is to be tested can be specified and applied to studies that experience publication bias on either side (*Viechtbauer 2019*). If the side is not specified, the side is chosen with relation to the results of Egger's Regression test which is run during the call to the relevant trim and fill command in the metafor package in R (*Viechtbauer 2019*).

3.4 Main-Analysis

In the main-analysis as per *Elvik (2005)*, the estimates-of-effect from the collected studies are combined into one summary estimate-of-effect and corresponding standard error.

Depending on the results of the exploratory analysis, especially with relation to the homogeneity test, there are primarily three model types as potential options which can be used in the main analysis, as summarized below (*Elvik 2005*). All three options will be studied.

- Fixed-Effects Model: Variation between studies is assumed to be random and not systematic
- Random-Effects Model: Variation between studies is assumed to be systematic in nature
- Mixed-Effects or Meta-regression Model: Variation between studies is assumed to be systematic, and due to independent variables given in a regression model

3.4.1 Fixed-Effects Model

As discussed in *Shadish and Haddock (2009)* and summarized below for the fixed-effects model, a theoretical meta-analysis is planned for k independent studies, with the i th study having an estimate of effect T_i , variance v_i and corresponding true population effect θ_i . In the fixed-effects model, the true effect from each study from θ_1 to θ_k is assumed to have a common value, θ and there is no variation in the true effect. Therefore, the mean estimate of effect $\bar{T}$ is given by the following:

$$\bar{T} = \frac{\sum_{i=1}^k w_i T_i}{\sum_{i=1}^k w_i} \quad (3.14)$$

Given this assumption, $\bar{T}$ would be an unbiased estimator of the common population effect size θ . The corresponding variance v for the mean estimate is then the inverse of the sum of the weights, given by the following:

$$v = \frac{1}{\sum_{i=1}^k \frac{1}{v_i}} = \frac{1}{\sum_{i=1}^k w_i} \quad (3.15)$$

Significance is then determined using the two tailed z-statistic at a 95% confidence interval given by the following.

$$Z = \frac{|\bar{T}|}{\sqrt{v}} \quad (3.16)$$

95% confidence intervals for the estimate $\bar{T}$ are calculated with the following, with $\bar{T}_U$ the upper bound value and $\bar{T}_L$ the lower bound value

$$\bar{T}_U = \bar{T} + 1.96\sqrt{v}, \bar{T}_L = \bar{T} - 1.96\sqrt{v} \quad (3.17)$$

Each T_i will be based on l_i , the log odds ratio calculated for each study finding. Values are then converted from their logarithmic form to odds ratio form by taking the exponential of the summary estimate and corresponding upper and lower bound values of the confidence interval.

Where the study dataset is heterogeneous, the summary effect from the “fixed effects” model would only be applicable to the studies and may not be an unbiased estimator of the overall safety effect of the population of reversible lanes (*Shadish and Haddock*

2009). A random-effects or mixed-effects model should then be used to account for the systematic variation between the studies, and for the calculated summary effects to be indicative of the effects of the treatment in question as a whole (*Shadish and Haddock 2009*).

3.4.2 Random-Effects Model

Also, as discussed in *Shadish and Haddock (2009)* and summarized in this section, a random-effects model differs from a fixed-effects model in that the population or true effect in each study from k studies is not a common value θ , but rather randomly distributed. In this case, the variance v_i^* is due to two components; the within-study variance v_i , and between-study variance σ_θ^2 .

$$v_i^* = \sigma_\theta^2 + v_i \quad (3.18)$$

Several methodologies exist to estimate σ_θ^2 (*Viechtbauer 2005*). The most commonly used estimation methodology is the DerSimonian and Laird (DL) estimate (*Christensen 2003*), and this estimate is also used widely in traffic safety meta-analysis (*e.g. Elvik et al. 2007, Erke 2009*) as well as the most recent meta-analysis on reversible lanes (*Elvik et al. 2009*). In this study, the DL estimate will be used, and the methodology is summarized below based on *Shadish and Haddock (2009)*.

An unbiased estimator for the between-study variance σ_{θ}^2 is given by the following equation, where Q is the homogeneity test statistic as discussed in previous sections, k is the number of studies

$$\sigma_{\theta}^2 = \frac{[Q - (k - 1)]}{c} \quad (3.19)$$

where c is given by the following:

$$c = \sum_{i=1}^k w_i - \left[\frac{\sum_{i=1}^k w_i^2}{\sum_{i=1}^k w_i} \right] \quad (3.20)$$

Variance v_i^* , and the corresponding statistical weight can then be calculated, and the summary estimate and corresponding variance will reflect the between-studies variation.

$$v = \frac{1}{\sum_{i=1}^k \frac{1}{v_i^*}} = \frac{1}{\sum_{i=1}^k w_i^*} \quad (3.21)$$

$$\bar{T} = \frac{\sum_{i=1}^k w_i^* T_i}{\sum_{i=1}^k w_i^*} \quad (3.22)$$

Significance and confidence intervals, and conversion from log-odds to odds ratio is then performed, similar to the fixed effects methodology. Where study variations are heterogeneous, the random effects model allows for this heterogeneity between studies but there is no provision for explaining it, as can be done with a mixed-effects or meta-regression analysis (*Christensen 2003*).

3.4.3 Meta-regression Analysis

The meta-regression analysis will be based on a weighted-normal errors regression analysis as discussed in *Thompson and Sharp (1999)*, and summarized in this section. In this case, the estimate-of-effect such as the log-odds ratio for each trial or study finding is assumed to be normally distributed. In this case, the estimate of effect T_i is assumed to be independent and normally distributed, where α is the intercept, β is the coefficient for variable x_i and v_i is the within-study variance for each T_i :

$$T_i \sim N(\alpha + \beta x_i, v_i) \quad (3.23)$$

In order to incorporate any heterogeneity on the trials captured from each study, either a multiplicative factor, or additive factor can be included to the within-study variance to model the variance between studies. The multiplicative method φ modifies the within-study variance to φv_i and is typically estimated with the Mean Square Error as calculated in the regression analysis. The additive method modifies the variance v_i with τ^2 to give a modified variance $v_i + \tau^2$. τ^2 is typically estimated and several of these estimators exist, which are discussed in more detail in other sources (*Thompson and Sharp 1999*, *Viechtbauer 2005*). For this study, an additive method is used for the meta-regression due to the higher intuitive appeal and consistency of the additive method (*Thompson and Sharp 1999*), as well as the added benefit of the model reducing to a fixed-effects meta-regression if there is negligible between-study variance (*Sterne and Egger 2005*).

Although the multiplicative method is a quick and easy-to-implement approximation of between-study variance (*Thompson and Sharp 1999*), this method is less appealing due to

its lack of intuitive appeal, and generally does not correct a criticism of fixed-effects models, which is that there is dominance of larger studies versus smaller studies (*Thompson and Sharp 1999*). Another weakness is that a multiplicative factor ϕ of less than one given by the Mean Square Error of the regression also implies that there is less heterogeneity between studies than there would be by chance, which is generally considered implausible (*Sterne and Egger 2005*). Therefore, T_i is given by the following as per the additive method:

$$T_i \sim N(\alpha + \beta x_i, v_i + \tau^2) \quad (3.24)$$

The hypothetical model given by the above is assuming there is only one covariate (*Thompson and Sharp 1999*). However, the model can be expanded into a multivariate model with several explanatory variables (*Thompson and Sharp 1999*). The estimator used is the Dersimonian-Laird estimator as per the methodology in the Random-Effects model to maintain consistency with other analysis methodologies previously performed.

The covariates chosen for the purposes of this study are based on the subgroupings developed in the Stratification/Subgroup Analysis section. Each of the subgroups were modeled as dummy variables, with presence or subgroup identification for each observed estimate of effect signified as 1, and absence signified as 0:

- *INJ*: injury collision effect estimate
- *PDO*: PDO collision effect estimate
- *PEAK*: effect estimate of peak period collisions

- *OFFPEAK*: effect estimate of collisions occurring outside of peak period
- *CS*: if cross-sectional study then 1, if simple before-after then 0
- *LT*: if left turn restriction then 1, if no left turn restriction then 0
- *DYN*: presence of dynamic signs and lane control signals
- *STATIC*: presence of static lane control signs
- *DELIN*: presence of manually installed traffic delineator or temporary barrier

In addition to the subgroup related variables, four other variables were added with relation to the interpreted operational characteristics of each reversible lane project as described in and interpreted from the relevant sections of each study where data was extracted. These are *LT*, *DYN*, *STATIC* and *DELIN*.

Model formation was informed by the results of the exploratory analysis. Specifically, there are three models that will be studied. Model 1 includes all effect estimates combined, with all variables as mentioned above were included for inference. Model 2 includes all effect estimates derived from simple before-after studies and all variables with the exception of *CS*, the cross-sectional study variable. Model 3 includes the effect estimates from the cross-sectional studies with all variables included with exception of *CS*, and the crash severity variables for injury and property damage only, *INJ* and *PDO*. Severity was excluded in Model 3 due to the nature of the cross-sectional studies in that an overall rate or frequency was reported in the selected studies.

In addition to the model outputs from R, which will include standard error, z-value, p-value and confidence intervals of the log-odds ratio regressed against the chosen variables, the Variance Inflation Factor (VIF) will also be included for the purposes of monitoring multicollinearity. In short, VIF is given by the following equation (*O'Brien 2007*).

$$VIF = \frac{1}{1 - R_i^2} \quad (3.25)$$

Where R_i^2 is the R-squared value of a regression of the *ith* independent variable on all other independent variables in the model (*Craney and Surles 2002*).

In general, VIF is used as a measure of the amount of collinearity due to the variable, and can be interpreted as the multiplicative factor that the variance of the coefficient would increase by, given the correlation of the variable in question with the rest of the model variables R_i^2 (*O'Brien 2007*). Although a VIF over generally known rules of thumb such as a VIF of 4 or 10 does not necessarily mean an invalid model in general (*O'Brien 2007*), or with relation to transportation models (*Tay 2017*), a VIF of 4 will be used as a rule-of-thumb cutoff for the purposes of monitoring the model for potential multicollinearity. If multicollinearity signified by a VIF of larger than 4 on any model variables is detected, the model variable will be further examined with relation to potential effects on estimation and theoretical importance to the model as suggested in *Tay (2017)*. However, the aim of the study is to explore and estimate the effect of study quality and operational/site variables on the traffic safety of reversible lanes, any

multicollinearity will simply be noted and no removals will occur on any of the identified variables unless otherwise stated as a result of further analysis.

3.5 Outlier Analysis

Based on the results of the exploratory and main-analysis, an outlier analysis may take place.

Several methodologies exist to facilitate the detection of outliers in an objective manner as explained in several sources (*e.g. Seo 2002*). These include the “standard deviation method” where outliers are typically defined as those values outside a range centered around the mean plus or minus a multiple of the standard deviation (*Seo 2002*). There are also methodologies that are more robust to the effect of outliers, such as Tukey’s Method, which defines an outlier as outside of a ‘fence’, and this fence is defined as the range of values less than the first quartile minus a multiple of the interquartile range, or greater than the third quartile plus a multiple of the interquartile range (*Seo 2002*). The interquartile range is the difference between the third quartile and the first quartile, and the multiple chosen is typically 1.5 or 3 (*Seo 2002*).

Use of boxplots and corresponding methods such as Tukey’s Method have been shown to be useful in revealing outlying effect estimates (*Greenhouse and Iyengar 2009, Anzures-Cabrera and Higgins 2010*). However, these methods do not take into account the uncertainty and precision of the estimates, and the effect of skewed distributions may

potentially have less importance when precision is accounted for (*Anzures-Cabrera and Higgins 2010*).

Leave-one-out analysis removes one study and recalculates the mean effect estimate and corresponding statistics for the purposes of sensitivity analysis, specifically, the mean effect estimate is calculated g times with one effect estimate removed, or $g-1$ effect estimates (*e.g. Greenhouse and Iyengar 2009, Elvik 2017*). Recalculation of the weighted mean effect estimate in a leave-one-out analysis allows for the accounting of the estimate's precision and therefore the influence of the removed effect estimate on the result (*Greenhouse and Iyengar 2009*). Given the benefits of this method in terms of accounting for the precision and statistical weight of each estimate, this methodology will be used in any outlier analysis that may be performed in this study.

From a review of guidance literature such as literature review papers, handbooks and textbooks, leave-one-out analysis is described generally and is suggested as a potential method of carrying out a sensitivity analysis on outliers, although these sources generally do not provide a specific recommended methodology for performing the analysis (*e.g. Elvik 2005, Borenstein et al. 2009*). One reference source (*Greenhouse and Iyengar 2009*) performed a comparison of the change between the original estimate and leave-one-out estimates with relation to an example dataset, and deemed those studies where the leave-one-out result differed in comparison to the general trend of the other leave-one-out results to be influential outliers. They also point to the potential versatility of the method in terms of combining or modifying methodologies, such as removal two at a

time, etc, use of leave-one-out on subgroups, and combination with methods like Maximum Likelihood Estimation, although computational and/or general complexity may increase (*Greenhouse and Iyengar 2009*).

A cursory review of recent traffic safety meta-analysis was then performed in light of the absence of information related to specific procedure related to outlier sensitivity analysis. Some studies do not perform a sensitivity analysis with relation to outliers (*Phillips et al. 2011, Caird et al. 2014, Simmons et al. 2016, Høye 2018*), while the remainder of surveyed studies use a leave-one-out analysis or variant (*Erke et al. 2009, Erke 2009, Høye 2010, 2013, 2014, Elvik 2013, 2017, Castillo-Manzano et al. 2019*). Within those studies that perform a leave-one-out analysis, some studies simply state that those studies that change the mean effect estimate significantly are reported and studied further, although this threshold does not appear to be elaborated on (*Erke 2009, Høye 2013, 2014, Castillo-Manzano et al. 2019*). Some studies simply review the studies with the largest deviation from the original estimate (*Høye 2010*). Some studies explicitly state that studies outside of the confidence interval of the original estimate will be considered potential outliers (*Elvik 2013, 2017*). One study removed a group of seven estimates with the largest effect and reviewed the change between the original and new study estimate (*Erke et al. 2009*). None of the studies provided an explicit justification for their methodology decision.

Based on this review, the threshold for identification of an effect estimate using leave-one-out sensitivity analysis will be based on the leave-one-out results falling outside of

the 95% confidence interval for the original mean estimate. This threshold is intuitive in that a study holds significant influence if its removal results in a new estimate that is outside of what the expected value should be based on the stated confidence level. Based on this cursory review, the leave-one-out method of sensitivity analysis also has advantages over other methods in that the precision of each study is incorporated in the calculated weighted average.

3.6 Software Used

Meta-analysis software used includes R version 3.6.0 (*R Core Team 2019*), under the ‘metafor’ package, *version 2.1-0 (Viechtbauer 2010)*. Reference was also made to *Viechtbauer (2010)* and the relevant help files for the package for more details on package functions and plotting options as well as information on model types and model fitting during the analysis. Plotting of the effect estimates grouped by study in the funnel plot was facilitated with the *colorspace* package, *version 1.4.-1 (Zeileis et al. 2019)*, using a colour palette called Berlin to differentiate between each of the studies (*Zeileis et al. 2009*)

CHAPTER FOUR: RESULTS AND DISCUSSION

4.1 Introduction

This chapter will summarize the results of the systematic literature review, extraction of effect estimates, as well as the meta-analysis with relation to the exploratory analysis, main analysis, as well as the outlier sensitivity analysis. A discussion of the exploratory, main and outlier analysis will also be provided in this section.

4.2 Systematic Literature Review - Results

As discussed in the previous chapter, a systematic literature search was executed on transportation research databases such as the TRID and ITE Library databases, as well as Google Scholar. As shown in Appendix A, 34 queries were executed, and around 2723 entries were returned overall. Out of these entries, approximately 1500 of these entries are unique non-duplicates. The vast majority of these entries either included a slight mention of reversible lanes, or discussed reversible lanes but did not include any discussion of any traffic safety statistics and were therefore excluded from the meta-analysis.

Ten studies were selected for inclusion in the study. From the original study, four studies were included, which were *DeRose (1966)*, *Upchurch (1975)*, *Agent and Clark (1980)*, and *Bretherton and Elhaj (1996)*. One study was excluded as the study was in Norwegian, which was *Kirste (1989)*. Table 1 is a summary of the included studies, their publication years, the country where the study was conducted and the study methodology used, as well as the number of effect estimates extracted. 155 effect estimates were

extracted according to the methodology as discussed in Chapter 3. All selected studies were conducted in the US and the methodology used by the selected studies were either simple before-after studies or comparative/cross-sectional studies.

Table 1: List of Included Studies

Authors	Publication Year	Country of Study	Study Methodology Type	Number of Effect Estimates
Agent and Clark*	(1980)	USA	Simple Before-After	12
Bretherton and Elhaj*	(1996)	USA	Simple Before-After	6
City of Phoenix	(2010)	USA	Comparative/Cross-Sectional	16
Derosé*	(1966)	USA	Simple Before-After	5
Dey and Ma	(2007)	USA	Comparative/Cross-Sectional	5
Dey, Ma and Aden	(2011)	USA	Comparative/Cross-Sectional	6
Knoblauch, Parker and Keegel	(1984)	USA	Comparative/Cross-Sectional	52
Lalani and Baird	(1981)	USA	Simple Before-After	36
Miller, Deueser, Wattleworth and Wallace	(1979)	USA	Comparative/Cross-Sectional	8
			Simple Before-After	8
Upchurch*	(1975)	USA	Simple Before-After	1

* denotes study included in *Elvik et al (2009)*

Several studies were identified as a potential candidate for further study, but upon further review of the study, paper or report, was found to be unsuitable for the meta-analysis, given the inclusion/exclusion criteria set forth in Chapter 3. The list of excluded studies and the reason for exclusion is given in Table 2.

Table 2: List of Excluded Studies

Authors	Publication Year	Country of Study	Reason for Exclusion
Agent and Clark	(1982)	USA	Study results previously captured - Agent and Clark 1980
Bhargava et al	(2006)	USA	Facility type out-of-scope - Freeway Reversible Lane
Burnham	(1976)	USA	Operational characteristics of treatment not reported
Cao et al	(2012a)	USA	Facility type out-of-scope - Conversion Freeway Reversible HOV to HOT lane
Cao, Xu and Huang	(2012b)	USA	Facility type out-of-scope - Conversion Freeway Reversible HOV to HOT lane
Cao, Zuo and Xu	(2014)	China	Incompatible traffic safety measure - traffic conflicts study
Christiansen and Lomax	(1990)	USA	Facility type out-of-scope - Freeway Reversible Lane
Cothron et al	(2004)	USA	Facility type out-of-scope - Freeway Reversible Lane
Dougald	(2007)	USA	Facility type out-of-scope - Freeway Reversible Lane
Dowling and Elias	(2013)		No collision rate or frequency reported
Falcocchio, Levinson	(2015)		No collision rate or frequency reported
Gray et al	(1995)	USA	Facility type out-of-scope - Freeway Reversible Lane
Habermann, Schonleiter and Burmeister	(1972)	Germany	No collision rate or frequency reported
Kirste*	(1989)	Norway	Non-English Study
LaPlante	(1967)	USA	No collision rate or frequency reported
LaPlante and Harrington	(1984)	USA	Facility type out-of-scope - Permanent Bus Contraflow Lane
Li, Yang and Xiong	(2007)	China	Incompatible traffic safety measure - traffic conflicts study
McCasland	(1981)	USA	Facility type out-of-scope - Freeway Reversible Lane
Munnich and Buckeye	(2007)	USA	Facility type out-of-scope - Conversion Freeway Reversible HOV to HOT lane
Murray et al	(2000)	USA	Facility type out-of-scope - Freeway Reversible Lane
Parsonson and Thomas	(1976)	USA	Operational characteristics of treatment not reported
Pint et al	(1995)	USA	Facility type out-of-scope - Freeway Reversible Lane
Retting et al	(2001)	USA	Operational characteristics of treatment not reported

Authors	Publication Year	Country of Study	Reason for Exclusion
Rosenbaum	(1986)	USA	Study results previously captured - Knoblauch, Parker and Keegel 1982
Stokes, Christiansen and Ranft	(1988)	USA	Facility type out-of-scope - Freeway Reversible Lane
Ullman and Krammes	(1991)	USA	Facility type out-of-scope - Freeway Work Zone Reversible Lane
Walaczek et al	(2016)	Germany	Facility type out-of-scope - Freeway Work Zone Reversible Lane

*denotes study included in *Elvik et al (2009)*

In general, the reason for exclusion when a traffic safety related study into reversible lanes was identified was due to several factors. The most common reason for exclusion was due to an out-of-scope facility type and/or type of application outside of the inclusion criteria, such as a freeway application or work zone application of reversible lanes. In some cases, traffic conflicts studies reviewed violation rates or observed conflict rates but not a collision rate or frequency, which was outside of the study scope in terms of the traffic safety measure used. Other reasons for exclusion include a lack of discussion in some reports to provide specific operational characteristics of the reversible lane treatment, such as type of traffic control used to convey the reversible lane to drivers. In some of these studies, a time period for the before- and after case was not explicitly or implicitly stated or incorporated into a rate (*e.g. Burnham 1976*). In this case, it was necessary to remove these studies from the meta-analysis in order to conduct a meta-regression analysis where operational characteristics were included as a variable or for the adjustment of unequal periods as per the methodology. Finally, any studies reporting a percent or proportion of change in collisions but no collision rate or frequency were removed from the analysis due to a need to estimate a standard error for each captured CMF. As these excluded entries are typically literature reviews citing other journal papers, as well as harder to locate/procure grey literature (*e.g. Dowling and Elias 2013, Falcocchio and Levinson 2015*), an attempt to locate these studies was made and if successful were included.

4.3 Data Extraction - Assumptions and Additional Calculation Steps

This section will discuss assumptions and additional calculations that were taken for specific studies in addition to the steps taken in Sections 3.2.3 to 3.2.6 to convert the study findings to a common scale.

In *Elvik et al (2009)* the fatal crash summary effect was not reported, as it appears that only two studies from the original meta-analysis explicitly reported fatal crash frequencies or rates (*Agent and Clark 1980, Bretherton and Elhaj 1996*). These two studies also reported either zero or low fatal crash frequencies/rates. New studies identified and included in this study but were not in *Elvik et al (2009)* did not explicitly report or include fatal collision frequencies or rates, and so the sample size of effect estimates would most likely remain the same. Therefore, the fatal crash summary effect is also not reported in this study.

Where injury crash frequencies were reported, but PDO crashes were not reported, PDO crashes were extracted by subtracting injury crashes from the total crash count, under the assumption that injury crashes included fatal, serious and minor injury crashes. This method was used in the calculation of effect estimates from *Agent and Clark (1980)*, *Lalani and Baird (1981)*, and *Bretherton and Elhaj (1996)*. Given that fatal collisions are generally rare and given the short timeframes used in the studies cited, this assumption appears to be sound.

In one study (*Upchurch 1975*), a total number of collisions was reported in the evaluation period after installation of the reversible lane but not for the before period of evaluation. In this case, the report provided a total number of collisions directly attributed to the reversible lane. As this study was included in previous meta-analysis, the effect estimate was calculated by calculating the collision frequency before installation as the total number of collisions subtracted by the total number of collisions directly attributed to the reversible lane, divided by the total number of collisions during the evaluation period.

In order to calculate the effect estimate for the comparative studies from Table 1 reporting a collision rate in million vehicle kilometers or miles travelled (*Miller et al. 1979, Knoblauch et al. 1984, City of Phoenix 2010*), the extraction methodology changed with relation to the change in methodology of the studies. In *Knoblauch, Parker and Keegel (1984)* collision rates from 13 reversible lane sites were compared one-by-one to an aggregated collision rate for four non-reversible lane datasets compiled or found by the investigators of the study. This resulted in 52 CMFs extracted from *Knoblauch, Parker and Keegel (1984)*.

Two reports focusing on a reversible lane segment of Connecticut Avenue in Washington D.C were identified and included in the study (*Dey and Ma 2007, Dey et al. 2011*). Although both studies were similar, upon further review of both papers, *Dey and Ma (2007)* reviewed three intersections along the treatment segment and one comparison segment of similar traffic pattern, while *Dey, Ma and Aden (2011)* reviewed one

reversible lane segment and two non-reversible lane segments of similar length and traffic pattern. Therefore, the frequencies of both studies were included for this study.

In *Miller et al (1979)*, a before- and after frequency and rate for each study area is provided, as well as a summary "control base" rate of a nearby non-reversible section of road or county-wide accident rate. The control base rate is collected over an unknown time period. For the simple before-after part of their study, the rates in million vehicle miles before and after installation of the reversible lane was used to calculate the log-odds ratio, as specific dates for data collection were only reported in terms of months, requiring assumption of some of the start and end dates by day, which complicates calculation using frequencies. The report's methodology also emphasizes comparison using rates. For the comparative/cross-sectional part of their study, the after rate for the reversible lane section as well as the control base rate in million vehicle miles was used to calculate the log-odds ratio.

City of Phoenix (2010) performed a cross-sectional study, where morning and afternoon peak period collision rates over three years were reported for two reversible lane roadway corridors and for two comparison roadway corridors close to the treatment roadways in proximity and characteristics. Rates were reported in per million vehicle miles travelled for segments and per million entering vehicles for intersections. For this study, the average segment and intersection rate for each corridor in the study area was calculated and each reversible lane corridor collision rate was compared to each non-reversible lane

corridor rate. The average morning and afternoon rate for segments and intersections yielded 16 CMFs from this study.

Collision frequencies and rates used in the extraction of the study, in addition to other information including the study location, study type and binary values of the meta-regression variables is provided in Appendix B.

4.4 Exploratory Analysis

In this section, an exploratory analysis will be performed as per the methodology established in Chapter 3. This will include a visual inspection of funnel plots for general trends and overall symmetry, a review of overall and subgroup statistics such as homogeneity, as well as a review of publication bias using a trim-and-fill approach as discussed in the methodology. As discussed in the methodology, a fixed-effects methodology is used to calculate the standard error for the funnel-plot analysis as well as any asymmetry tests and trim-and-fill analysis in order to better capture the effects of small studies (*Poole and Greenland 1999, Sterne and Egger 2001*).

Table 3 includes the some of the quantitative values and statistics of each subgroup with relation to the collision severity and time-of-occurrence subgroups, including number of effect estimates in each subgroup, sum and proportion of statistical weights, and heterogeneity statistics. Table 4 is the corresponding table for the study type subgroups.

Table 3: Subgroup Characteristics by Collision Severity and Time of Occurrence

Severity	Collision Type	Number of Effect Estimates	Sum of Statistical Weights	Proportion of Statistical Weights	Heterogeneity Test Statistic (Q)	p-value	Proportion of Systematic Variation (I ²)
Injury	All	18	437.818	0.077	45.076	0.0002	62.286
PDO	All	17	750.997	0.132	49.062	<.0001	67.388
Unspecified	All	120	4500.934	0.791	726.637	<.0001	83.623
Unspecified	Peak Period	60	753.725	0.132	220.393	<.0001	73.230
Unspecified	Off Peak Period	16	1154.879	0.203	190.938	<.0001	92.144
All Estimates		155	5689.749	1.000	839.772	<.0001	81.662

Table 4: Study Effect Estimates and Statistical Characteristics by Study Type

Study Methodology Type	Number of Effect Estimates	Sum of Statistical Weights	Proportion of Statistical Weights	Heterogeneity Test Statistic (Q)	p-value	Proportion of Systematic Variation (I ²)
Simple Before-After	68	4321.573	0.760	266.345	<.0001	74.845
Comparative/Cross-section	87	1368.176	0.240	271.086	<.0001	68.276
All Estimates	155	5689.749	1.000	839.772	<.0001	81.662

4.4.1 Funnel Plot Visual Inspection

Interpretation of funnel plots are informed partly by a relevant textbook chapter on funnel plot interpretation (*Sterne et al. 2005*) and interpretations presented in other traffic safety meta-analyses (*e.g. Elvik 2017*).

Figure 4 is the funnel plot for all 155 estimates extracted from the ten studies. Figure 5 to Figure 9 are the funnel plots for each of the collision severity and time-of-occurrence

subgroups. Figure 10 and Figure 11 are the funnel plots for each of the study type subgroups for before-after studies and comparative studies respectively.

Figure 4: Funnel Plot - All Effect Estimates

Overall from Figure 4, it appears that most of the extracted CMFs fall within the 95% confidence interval as denoted by the white area in the figure, with some outliers existing on both sides of the mean, especially as the standard error increases. Studies of higher precision outlying the funnel on the right side of the mean were newer studies such as *Bretherton and Elhaj (1996)*, *Dey and Ma (2007)* and *Dey, Ma and Aden (2011)*. While outlying studies of higher precision on the left side were older studies such as *DeRose (1966)*. There did not appear to be any time or study related trends for outliers of lower precision or those studies within the funnel. After a visual inspection of the funnel plot, there appears to be an equal number of effect estimates on both sides of the fixed effects

mean CMF calculated, and there does not appear to be any major visible gap in the extremes of either side of the mean or the gap is symmetrical on both sides.

Figure 5 is the funnel plot for the CMFs for the identified injury collisions. For the CMFs classified as injury collisions, most of the 18 estimates of effect fall within the 95% confidence interval, with only one effect estimate falling outside of the interval on each side, and there is no discernable gap on either extreme side of the plot, given the number of estimates. The outlying study on the right belonged to a newer study (*Bretherton and Elhaj 1996*), while the outlying study on the left belonged to an 80s study (*Lalani and Baird 1981*). There appears to be an equal number of effect estimates on either side of the mean.

Figure 5: Funnel Plot - Injury Collisions

Figure 6 is the funnel plot for the CMFs for the identified PDO collisions. Visual inspection of the PDO collisions funnel plot revealed that there appeared to be equal amounts of effect estimates on both sides of the mean, with the majority of the estimates within the confidence interval. Two estimates on the right and outside the funnel were relatively farther away from the mean, while one study on the left side was outside the funnel but comparatively closer to the mean and overall study grouping. All studies outside the funnel region belonged to *Lalani and Baird (1981)*. No gap denoting potential publication bias was readily apparent on visual inspection.

Figure 6: Funnel Plot - PDO Collisions

A funnel plot of the remainder of the effect estimates of unspecified severity is given in Figure 7. The funnel plot showed relatively equal numbers of studies on both sides of the mean, with slightly more studies on the left side, and outliers on both sides of the mean especially as the standard error decreases. A general trend similar to the trend in the all-

effect-estimates combined funnel plot was observed here in that older studies of higher precision were to the left of the mean, and newer studies of higher precision were to the right of the mean. There appears to be a visible gap on the extreme sides of the funnel plot, but appears to be symmetrical with several high standard error studies hovering around the mean value.

Figure 7: Funnel Plot - Unspecified Severity

Figure 8 is the funnel plot corresponding to the peak period collision subgroup. For the peak period collision subgroup, there appears to be a slightly larger number of studies on the left side of the mean, with relatively equal amounts of effect estimates outside of the 95% confidence interval on both sides of the mean for studies with greater statistical weight or lower standard error. However, it should be noted that studies on the right and outside of the confidence interval funnel are generally farther away from the funnel than those studies on the left that are outside of the funnel. The general trend as observed in

other funnel plots is observed here, in that newer studies such as *Dey and Ma (2007)* and *Dey, Ma and Aden (2011)* were to the right of the mean and outside the funnel region. Older studies such as *DeRose (1966)* were to the left of the mean and outside the funnel. *Lalani and Baird (1981)* was outlying the funnel on both sides. A visible gap is apparent and symmetric on the extreme ends of both sides.

Figure 8: Funnel Plot - Peak Period Collisions

The off-peak period subgroup effect estimates are shown by the funnel plot in Figure 9. This set of effect estimates is characterized by a small set of low standard error studies, with more effect estimates to the left of the mean. Most studies were within the 95% confidence interval, although there were around five estimates outlying the funnel on the left side, while three studies on the right side were outside the funnel and relatively farther away from the funnel when compared with the outlying studies on the left. The outlying studies on the left generally belonged to older studies using a simple before-after

methodology, while the studies outlying on the right generally belonged to recently performed cross-sectional studies. No asymmetry was readily apparent, and no gap was observed which would indicate potential publication bias.

Figure 9: Funnel Plot - Off-Peak Period Collisions

In addition to stratifying the studies by collision severity and time of occurrence, studies were also stratified with relation to study methodology. Figure 10 is the funnel plot for the simple before-after study subgroup. Visual inspection of the funnel plot for before-after studies revealed relatively equal numbers of effect estimates on both sides of the mean, and an increasingly larger numbers of outliers were present outside of the 95% confidence interval line as standard error decreases. There were more outlying estimates on the right side when compared to the left side. The studies outlying on the right side belonged to *Bretherton and Elhaj (1996)*, and *Lalani and Baird (1981)*, while the studies outlying on the left side belonged to *Derose (1966)*, and *Lalani and Baird (1981)*. A

slight gap on the right side was present, which may be an indication of potential publication bias, although more analysis and study is needed.

Figure 10: Funnel Plot - Simple Before-After Studies

Figure 11 is the funnel plot for the comparative study subgroup. Conversely, visual inspection of the funnel plot for cross-sectional studies revealed a significant number of studies with standard error between 0.75 to 0.25 to the left of the mean. This group of studies mostly consisted of estimates from *Knoblauch et al (1984)*. To the right of the mean, a slight majority of the studies were within the funnel but six of these studies were outside of the funnel. Within these six studies on the right, most of them were of low standard error or of high statistical weight. Gaps were symmetrically on both sides of the mean, however due to the band of studies visually present as described before, the plot looks very asymmetric. Studies outlying the funnel on the right side include more recent cross-sectional studies (*Dey and Ma 2007, Dey et al. 2011*), while the studies outlying on

left side consisted of estimates from *Knoblauch et al (1984)* and one estimate from *City of Phoenix (2010)*.

Figure 11: Funnel Plot – Cross-sectional Studies

4.4.2 Heterogeneity

Visual inspection of the funnel plot showed an increasing number of studies outside of the 95% confidence level as the standard error decreases when variation between studies should decrease and converge towards the mean value, according to the interpretation guidance used and mentioned at the beginning of this section. Some measure of this behavior occurred for all studies combined and for all subgroups. For all studies combined and for all subgroups defined in previous sections the heterogeneity statistic was less than 0.02% which is much less than the confidence level threshold of 10% as set in the methodology, suggesting that heterogeneity exists between the studies for all subgroups and overall for all studies combined.

Calculation of I^2 as defined in Chapter 3 revealed that in general, the amount of systematic variation in all identified subgroups was substantial, with proportions ranging between 62% and 92%. Based on *Shadish and Haddock (2009)*, a medium level of heterogeneity was present for most subgroups, and a high level of heterogeneity was found for the unspecified severity subgroup, off-peak-period subgroup, and all estimates combined.

This suggests that the summary effect from the “fixed effects” model would only be applicable to the studies and may not be an unbiased estimator of the overall safety effect of the population of reversible lanes (*Shadish and Haddock 2009*). A random-effects or mixed-effects model should then be used to account for the systematic variation between the studies, and for the calculated summary effects to be indicative of the effects of the treatment in question as a whole (*Shadish and Haddock 2009*).

4.4.3 Asymmetry

As discussed in more detail in Chapter 3, Egger’s Regression Test was used which is a linear regression of the effect estimates extracted from each study against the standard error. Table 5 and Table 6 are the results of the test, including the slope, standard error of the slope, t-statistic and corresponding p-value for the collision type subgroups and study methodology subgroups respectively.

Slopes for the injury, peak period and off-peak period subgroups registered negative, non-significant slope. The negative slope suggests that there may be larger numbers of studies to the left of the mean. The PDO subgroup, unspecified severity subgroup, and all estimates combined registered a positive but non-significant slope, suggesting that there may be larger numbers of studies to the right of the mean. However, looking at the regression test results for collision severity and time-of-occurrence subgroups shown in Table 5, we found that no subgroup had p-values below a 10% significance level, suggesting that there is no statistically significant asymmetry and that asymmetry in general is not a concern.

Table 5: Egger Regression Test Results by Collision Severity and Time of Occurrence

Severity	Collision Type	Slope	Standard Error	t-value	p-value
Injury	All	-0.977	0.753	-1.298	0.213
PDO	All	0.503	0.889	0.566	0.580
Unspecified	All	0.103	0.304	0.338	0.736
Unspecified	Peak Period	-0.146	0.419	-0.350	0.728
Unspecified	Off Peak Period	-1.463	2.222	-0.658	0.521
All Estimates		0.074	0.267	0.277	0.782

Table 6: Egger Regression Test Results by Study Type

Study Methodology Type	Slope	Standard Error	t-value	p-value
Simple Before-After	0.613	0.387	1.581	0.119
Comparative/Cross-section	-1.180	0.233	-5.053	0.000
All Estimates	0.074	0.267	0.277	0.782

Looking at Table 6, we find that the slope for the before-after studies was positive, but p-value showed non-significance. Conversely, the slope value for the cross-sectional studies was -1.18, suggesting asymmetry favouring the left side, with a significant p-value of much less than 0.10. This suggests that there are significantly larger numbers of effect estimates to the left of the mean such that asymmetry is likely. These results are in line with the funnel plot visual inspections performed earlier.

4.4.4 Publication Bias

As discussed in previous sections, publication bias was assessed using a Trim-and-Fill analysis under a fixed-effects model. The results will be presented in two parts. Section 4.4.4.1 will discuss publication bias results with relation to collision severity and time of occurrence, while Section 4.4.4.2 will discuss results with relation to study type.

4.4.4.1 Publication Bias – Collision Severity and Time of Occurrence Subgroups

Table 7 are the results of the trim-and-fill analysis for the collision type subgroups, including the mean estimate and 95% confidence intervals for both the original fixed-effects estimate and the corresponding trim-and-fill estimate. An additional column is provided to show the difference between the original estimate and trim-and-fill estimate, which is the original estimate minus the trim-and-fill estimate. Figure 12 is a forest plot of the original estimate and trim-and-fill estimate for each collision type subgroup, which includes the calculated mean estimate and 95% confidence interval for each subgroup and estimate type.

With relation to the effect estimates stratified according to severity and time-of-occurrence, the trim-and-fill sensitivity analysis showed that three of the six subgroups did not have any ‘missing studies’ to be filled on either side. These subgroups were the PDO subgroups, unspecified severity subgroups, and all estimates combined. The remaining three subgroups’ estimates were generally filled on the right side.

Table 7: Trim-and-Fill Sensitivity Results - Collision Severity and Time of Occurrence

Severity	Collision Type	Original Estimate	95% CI	Trim-and-Fill Estimate	95% CI (lower limit)	Number of Missing Studies	Difference (Original - Trim-Fill)
Injury	All	1.447	(1.318, 1.589)	1.466	(1.336, 1.609)	2R	-0.019
PDO	All	1.132	(1.054, 1.216)	1.132	(1.054, 1.216)	0	0
Unspecified	All	1.302	(1.265, 1.341)	1.302	(1.265, 1.341)	0	0
Unspecified	Peak Period	1.497	(1.394, 1.608)	1.544	(1.440, 1.657)	8R	-0.047
Unspecified	Off Peak Period	1.198	(1.131, 1.269)	1.415	(1.345, 1.488)	5R	-0.217
All Estimates		1.289	(1.256, 1.323)	1.289	(1.256, 1.323)	0	0

Figure 12: Forest Plot - Original vs. Trim-and-Fill Estimate, Collision Type

Subgroups

The differences between the original and trim-and-fill estimates for the injury and peak period subgroups were relatively small, ranging between 0.019 to 0.047. Both of these subgroups' estimates were also within the 95% confidence interval for their neighbouring estimates. On the other hand, the off-peak period subgroup difference was relatively larger compared to the injury and peak period subgroups at a difference of 0.217. The off-peak period subgroups' estimates were also outside of each other's 95% confidence intervals.

Figure 13 to Figure 15 are the funnel plots with trim-and-fill points included for injury, peak period and off-peak period collisions respectively. No trim-and-fill funnel plots

were provided for the subgroups where the number of simulated missing studies was zero.

Figure 13: Trim-and-Fill Funnel Plot – Injury Collisions

Figure 14: Trim-and-Fill Funnel Plot – Peak Period Collisions

A visual inspection of the funnel plot for the injury subgroup in Figure 13 and peak period subgroup in Figure 14 showed that the “missing” studies were filled on the right,

and were generally interspersed with the observed effect estimates for the injury subgroup and peak period subgroup, and did not appear to fill a gap on the extreme ends of the funnel plot that is indicative of a dataset with potential publication bias. Table 7 and Figure 12 confirms this observation for these subgroups as the difference between the original and trim-and-fill estimates were relatively low, and within the 95% confidence intervals of their neighbour estimates. This suggests that if there are unpublished or uncaptured studies that their theoretical effect is not statistically significant based on this sensitivity analysis.

Figure 15: Trim-and-Fill Funnel Plot – Off-Peak Period Collisions

Visual inspection of the trim-and-fill funnel plots for the off-peak period subgroup given by Figure 15 shows interspersed of the trim-and-fill points with the observed values. However, grouping of the trim-and-fill points is centered around the two outlying points to the right of the mean and outside of the funnel. Comparison of the original estimate with the trim and fill estimate for the off-peak period subgroup in Table 7 showed a

relatively large difference between the two estimates. The estimates themselves were also outside of the 95% confidence intervals of their neighbour estimates, as can be seen in Figure 12 and Table 7. This suggests that there may be publication bias under the assumptions of the trim-and-fill test, but may also be due to the presence of outlying points, and more study will be performed to gauge the effect of outliers.

4.4.4.2 Publication Bias – Study Type Subgroups

Table 8 are the results of the Trim and Fill analysis for the study type subgroups, which includes the mean estimate and 95% confidence intervals for both the original fixed-effects estimate as well as the Trim-and-Fill estimates for both the simple before-after and cross-sectional study types. A column showing the difference between the two estimates as the original estimate subtracted by the Trim-and-Fill estimate is also given in the rightmost column. Figure 16 is a forest plot comparing the results of the original estimate with the trim-and-fill estimate for the study type subgroups.

Table 8: Trim-and-Fill Sensitivity Results – Study Type

Study Methodology Type	Original Estimate	95% CI	Trim-and-Fill Estimate	95% CI	Number of Missing Studies	Difference (Original - Trim-Fill)
Simple Before-After	1.132	(1.099, 1.166)	1.121	(1.088, 1.154)	4L	0.011
Comparative/Cross-section	1.941	(1.841, 2.047)	2.122	(2.016, 2.233)	34R	-0.181
All Estimates	1.289	(1.256, 1.323)	1.289	(1.256, 1.323)	0	0

Figure 16: Forest Plot - Original vs. Trim-and-Fill Estimate, Study Type Subgroups

For the simple before-after study subgroup, a 0.25% percent difference and a 1.1% decrease from the original estimate of 1.132 to the trim-and-fill estimate of 1.121 was observed. However, each of the estimates were within their neighbouring estimates' confidence intervals as shown in Table 8. The accompanying percent difference was also substantially larger relative to the before-after studies, and the increase from the original mean estimate is 18% from a CMF of 1.941 to 2.122. This change was determined to be significant, given that each of the original and trim-and-fill estimates were outside of their adjacent confidence intervals.

Figure 17: Trim-and-Fill Funnel Plot – Before-After Studies

Figure 18: Trim-and-Fill Funnel Plot – Cross-Sectional Studies

With relation to the study type subgroups, visual inspection of the before-after trim-and-fill funnel plot illustrated by Figure 17 showed four additional “missing” data points to the left, with most of these points outside of the confidence interval.

Visual inspection of the cross-sectional trim-and-fill funnel plot given by Figure 18 showed a significant amount of asymmetry and a large number of trim-and-fill points reflected on the right side, filling an apparent gap on the extreme end of the right side of the plot. This result and the results from Table 7 and Figure 16 suggests that publication bias may be present in the cross-sectional studies subgroup, although other factors may also be leading to the asymmetry, including outliers and choice of performance indicators used by the studies.

4.4.5 Summary and Discussion – Exploratory Analysis

In this section, the results of the exploratory analysis will be summarized, and discussed with relation to trends, overall feasibility of performing the main-analysis, and changes to the direction of the methodology of the main analysis if the main analysis is feasible.

4.4.5.1 Visual Inspection, Asymmetry and Publication Bias

In general, a visual inspection of the funnel plots for all effect estimates combined as well as the collision type subgroups showed that there were varying levels of asymmetry generally favouring the left side or equal on both sides. For most of the collision type subgroups, asymmetry was not significant given both the results of the Egger Regression Test and trim-and-fill results. For most of the subgroups, there was either no gap at the extreme ends of the funnel plot which would be indicative of a potential suppression of studies (*Sterne et al. 2005*), or a symmetrical gap on both sides of the funnel plot.

One exception to this observation was found among the effect estimates stratified by collision type, and this subgroup was the off-peak period subgroup. Visual inspection of the off-peak period collisions funnel plot revealed slight asymmetry favouring the left side. Egger's test confirmed this with a negative slope on regression, although the p-value was 0.521, suggesting that the null hypothesis of zero-slope could not be rejected. This suggests that any asymmetry is non-significant. Trim and fill results on the other hand showed the exact opposite result, with an increase in the trim-and-fill mean effect that was outside of the 95% confidence interval for the original estimate and vice versa. To further review this discrepancy, the R_o estimator was used and compared to the default L_o estimator in a brief sensitivity analysis. Calculation of R_o to estimate the number of missing studies revealed that there were 0 missing studies under this estimator, compared to five studies filled on the right side under the L_o estimator. This further suggests the significant difference between the trim-and-fill estimate and the original estimate may be more due to the presence of outlying data points and overall low sample size for the subgroup and less due to potential unpublished or uncaptured studies.

Upon closer inspection of the off-peak funnel plot, three outlying points on the right side of the mean with relatively low standard error were identified. One of the points was found to be one of the six effect estimates calculated from *Dey, Ma and Aden (2011)*, with a raw odds ratio of 2.609 corresponding to a log odds ratio of 0.959 with a standard error of around 0.083. The second point was found to belong to *Dey and Ma (2007)* with a raw odds ratio of 2.674, log odds ratio of 0.984, and standard error of 0.173. The next highest value is also a value from *Dey, Ma and Aden (2011)* corresponding to a raw odds

ratio of 1.558, log odds ratio of 0.443, standard error of 0.071. These three outlying estimates were derived from recent cross-sectional studies and were on the right side, while the remainder of the studies were simple before-after studies (*DeRose 1966, Agent and Clark 1980, Lalani and Baird 1981*) and were generally to the left of the mean, as can be observed in Figure 9. The conflicting results between the regression and trim-and-fill tests could be due to these potential outliers, although more study in the form of outlier analysis is required.

Stratification of effect estimates by study type revealed the following with relation to simple before-after studies and comparative/cross-sectional studies. Before-after studies after a visual inspection generally showed equal number of studies overall, with an absence of high standard error studies on the right side, coupled with a relatively large number of low standard error studies outside of the funnel on the right side. Egger's test returned a positive slope, suggesting asymmetry favouring the right side, although the p-value returned was 0.119 which is non-significant but close to the 10% significance level. The trim-and-fill analysis confirmed this asymmetry with four studies filled on the left side, although the original estimate and trim-and-fill estimate were within each other's' confidence intervals. Given the results of the quantitative analysis, asymmetry and asymmetry due to publication bias does not appear to be a major concern for the before-after subgroup. However, the absence of studies in the lower right-hand end of the funnel may potentially be explained by the mixing of rates from Miller et al (1979) and frequencies in the rest of the simple before-after studies.

On the other hand, comparative/cross-sectional studies generally showed asymmetry favouring the left side from the visual inspection and regression test. The trim-and-fill funnel plot showed that missing studies filled the gap on the extreme right at very large numbers. This asymmetry may be due to a mixing of the traffic safety measures, with two studies providing frequencies over a six-year period (*Dey and Ma 2007, Dey et al. 2011*), versus using rates normalized in million vehicle miles which was observed in the rest of the comparative studies. Reviewing the results of *Dey, Ma and Aden (2011)* and *Dey and Ma (2007)* against the rest of the studies, the results of the study generally shows a positive log odds ratio and lower standard error owing to the frequencies reported, compared to the other studies reporting a rate. This suggests a potential reason for the discrepancy.

Visual inspection with relation to the subgroups was also performed in terms of relative study age and study methodology. A general trend was observed in several subgroups, where the oldest studies were situated to the left of the mean, and the newest studies being to the right of the mean in terms of the high precision studies outlying the funnel. Generally, these studies came from the oldest study *DeRose (1966)* on the left, which is a simple before-after study, while the newest studies generally came from recent cross-sectional studies (*Dey and Ma 2007, Dey et al. 2011*) and were situated on the right. There did not appear to be a clear, observed relationship between study age and whether a study was to the left or right of the mean within the funnel region itself or for studies of lower precision.

4.4.5.2 Heterogeneity

Although asymmetry and potential publication bias were not present in the majority of the subgroups analyzed, heterogeneity was detected and inferred through the visual inspection of outliers within the lower standard error region of all of the funnel plots. The heterogeneity test statistic confirmed this inference, with all identified subgroups positing a p-value much less than the 10% significance level set out in the methodology. The I^2 value was computed for all subgroups, and it was found that there were medium to high levels of heterogeneity in the studies, suggesting that a random-effects model as well as a mixed-effects model should be included in order to respond to the non-negligible between-study variance.

Within the collision type subgroups, the subgroups with an I^2 value above 75% were the unspecified collision type subgroup, the off-peak period collision subgroup as well as all estimates combined. For the unspecified collision type subgroup, and for all estimates combined, this result could be due to the large number of studies, study locations, operational differences, collision types and mixing of collision frequencies and rates. Within the off-peak period collision subgroup, a potential reason for the high level of heterogeneity would be potentially due to the small sample size of low-standard-error studies tied with the outlying points as discussed previously. The remainder of the collision type subgroups had varying levels of unexplained heterogeneity below 75%, with injury crashes at the lowest level with an I^2 of 62%, and this may be a function of the number of studies, study locations and also due to the stratification of collisions by collision type.

Within the study type subgroups, the simple before-after subgroup had a higher heterogeneity at 75%, while the comparative/cross-sectional studies subgroup had a relatively lower heterogeneity at around 68%. Although asymmetry was not present at a significant level in the before-after studies and present in the cross-sectional studies, the funnel plot for before-after studies shows an overall larger number of collisions outside of the 95% confidence bounds compared to the cross-sectional studies. This may be also a function of the more varied set of study locations, corresponding differences in operations, and collision types when compared to cross-sectional studies. Cross-sectional studies appear from a cursory review of the dataset to be less varied in terms of study locations, as well as a use of an overall rate of unspecified severity in million vehicle miles, or million entering vehicles for most studies.

4.4.5.3 Exploratory Analysis - Overall Findings

In general, from the visual inspection of the funnel plots, there was some potential asymmetry favouring either the left side or there were generally equal numbers of effect estimates on either side of the mean. However, Egger's test and the trim-and-fill test for publication bias generally found that this asymmetry was not statistically significant for most of the subgroups, with the exception of the cross-sectional subgroup. In this subgroup, asymmetry was present suggesting potential bias overall. The trim-and-fill analysis also filled studies in an apparent gap on the right side, suggesting that there may potentially be publication bias, although there may be other potential reasons for the observed asymmetry, which will be discussed in more detail in later sections. Further review of the off-peak period subgroup yielded contradicting results, and these results

may be due to an outlier coupled with the characteristics of these studies. Heterogeneity overall was also found to be significant and present in relatively high levels.

Given the results of Egger's Regression Test for the cross-sectional studies subgroup, and the trim-and-fill tests for both the off-peak period and cross-sectional studies subgroup, the reader is suggested to take the results from these subgroups with some caution given the observed asymmetry in these subgroups. The trim-and-fill analysis is assessed and simulated as a sensitivity analysis under the assumption that missing studies due to publication bias are included. However, these results are provided as a what-if analysis with relation to the potential for publication bias and its potential effect on the result. No additional correction for publication bias will be performed in the main and meta-regression analysis.

More discussion on the potential sources of asymmetry and heterogeneity and potential mitigations will follow in the coming sections. However, from a visual inspection of the estimates from each of the 10 studies, it appears that study age may potentially play a role, as well as the study methodology, the study location and corresponding operational characteristics. For instance, *DeRose (1966)* may be the oldest study in the dataset, but the study methodology used was simple before-after, and the authors alluded to the effect of parking and left-turn restrictions in this specific implementation. On the other hand, *Dey and Ma (2007)*, as well as *Dey, Ma and Aden (2011)* were the newest studies, but these studies used a cross-sectional study methodology, and the implementation was

distinctly different from other implementations in that pole mounted lane use signals were used, which was a concern for the authors.

Based on the exploratory analysis, the results of the main analysis will continue with the additional random-effects model and mixed-effects model analysis to be used to respond to the heterogeneity and asymmetry present in the study results. In addition, outlier analysis will also be performed as discussed in the previous chapter.

4.5 Main Analysis

This section will discuss the results of the main analysis, which includes a fixed-effects analysis, and a random-effects analysis. A meta-regression analysis or mixed-effects analysis through the inclusion of explanatory variables related to collision severity, study characteristics and operational characteristics of the treatment roadway was also performed.

4.5.1 Traditional Meta-Analysis Results

In this section, a traditional meta-analysis summarizing the results of the fixed-effects and random-effects analysis will be made. Results will be provided as follows. Section 4.5.1.1 will discuss results for the collision severity and time of occurrence sub-groups, while Section 4.5.1.2 will discuss results with relation to the study methodology type subgroups.

Table 9: Mean Effect Estimates and Comparisons Between Models – Collision Severity and Time of Occurrence

Severity	Collision Type	Fixed-Effects			Random-Effects			Elvik et al (2009)	
		Estimated CMF	p-value	95% CI	Estimated CMF	p-value	95% CI	Estimated CMF	95% CI
Injury	All	1.447	< .0001	(1.318, 1.589)	1.349	0.001	(1.129, 1.611)	1.18	(0.84, 1.66)
PDO	All	1.132	0.001	(1.054, 1.216)	1.166	0.038	(1.009, 1.348)	N/A	
Unspecified	All	1.302	< .0001	(1.265, 1.341)	1.327	< .0001	(1.204, 1.462)	1.04	(0.95, 1.13)
Unspecified	Peak Period	1.497	< .0001	(1.394, 1.608)	1.462	< .0001	(1.237, 1.728)	1.15	(0.97, 1.37)
Unspecified	Off Peak Period	1.198	< .0001	(1.131, 1.269)	1.128	0.278	(0.907, 1.402)	N/A	
All Estimates		1.289	< .0001	(1.256, 1.323)	1.309	<.0001	(1.210, 1.415)	N/A	

Table 10: Percent Difference and Change in CMF Value, Random-Effects vs. Fixed Effects and Previous Study – Collision Severity and Time of Occurrence

Severity	Collision Type	Random - Fixed		Random - Elvik et al (2009)	
		Percent Difference	Difference (Random-Fixed)	Percent Difference	Difference (Random - Prev.)
Injury	All	1.75%	-0.098	3.3%	0.17
PDO	All	0.74%	0.034	N/A	
Unspecified	All	0.48%	0.025	6.1%	0.29
Unspecified	Peak Period	0.59%	-0.035	6.0%	0.31
Unspecified	Off Peak Period	1.50%	-0.070	N/A	
All Estimates		0.39%	0.020	N/A	

Table 11: Mean Effect Estimates and Comparisons Between Models – Study Methodology Types

Study Methodology Type	Fixed-Effects			Random-Effects			Percent Difference	Difference (Random-Fixed)
	Estimated CMF	p-value	95% CI	Estimated CMF	p-value	95% CI		
Simple Before-After	1.132	< .0001	(1.099, 1.166)	1.196	< .0001	(1.112, 1.287)	1.37%	0.064
Comparative/Cross-section	1.941	< .0001	(1.841, 2.047)	1.441	< .0001	(1.268, 1.637)	7.39%	-0.500
All Estimates	1.289	< .0001	(1.256, 1.323)	1.309	<.0001	(1.210, 1.415)	0.39%	0.020

4.5.1.1 Meta Analysis Results by Collision Severity and Time of Occurrence

Table 9 is a compiled table of the Fixed-Effects and Random-Effects results stratified by collision type, corresponding p-values and 95% confidence intervals, as well as the corresponding results from *Elvik et al (2009)*. Table 10 lists the percent and absolute differences of the random-effects estimate subtracted by the fixed-effects estimates as well as the estimates from the previous meta-analysis by *Elvik et al (2009)*. Figure 19 is a forest plot of the Fixed-Effects and Random-Effects results by collision type, and includes the mean estimate and 95% confidence intervals.

Figure 19: Forest Plot - Collision Severity and Time of Occurrence Subgroups, Fixed-Effects and Random-Effects Models

Comparing the random-effects and fixed-effects results for the collision severities in Table 10 and Figure 19, overall there were observed decreases from the fixed to the

random effects result ranging from a 9.8% decrease from 1.447 to 1.349 for the injury subgroup, 3.5% decrease from 1.497 to 1.462 for the peak period subgroup, to 7% decrease from 1.198 to 1.128 for the off-peak period subgroup. Observed increases from the fixed-effects to the random-effects result ranged from a 3.4% increase from 1.132 to 1.166 for the PDO subgroup, 2.5% increase from 1.302 to 1.327 for the unspecified severity subgroup, and a 2% increase for all estimates overall from 1.289 to 1.309.

These resulting increases and decreases are in line with the exploratory analysis results, specifically with relation to Egger's test. Most effect estimates were within the confidence intervals of their opposite model except for the off-peak period subgroup, where the fixed effects model result of 1.198 was within the 95% confidence interval of the random effects model, while the random effects model result of 1.128 fell outside of the fixed effects model's confidence interval. The off-peak random effects model was also the only subgroup with a non-significant p-value.

Overall, from the random-effects model, there can be an expected 30-33% increase in collisions overall with relation to installation of reversible lanes. Injury collisions are expected to increase by 35%, while PDO collisions would be expected to increase by 17%. During the peak period, collisions would be expected to increase by 46% while collisions in the off-peak period would be expected to increase by 13%. Within the random-effects models, the estimated mean CMFs calculated were all significant to 5%, with the exception of the off-peak period subgroup where the p-value was calculated to

be 0.278. For this subgroup only, the results under the fixed-effects model were more significant.

Comparison of the random-effects model with the results of the previous model showed that the additional extracted effect estimates resulted in an increased CMF for injury collisions from 1.18 to 1.35, which is a 17% increase from the previous study. The resulting CMFs for the unspecified effect estimates subgroup increased from 1.04 in the previous study to 1.33, a 29% increase from *Elvik et al (2009)*. Peak period collisions increased 31% from 1.15 to 1.46 from the previous study estimates.

4.5.1.2 Meta Analysis Results by Study Methodology Type

Table 11 are the corresponding compiled results and comparisons between the fixed- and random-effects models for the study type subgroups. Figure 20 is the forest plot for the fixed- and random-effects models for each of the study type subgroups. In this case, the simple before-after study subgroup increased by 6.4% from a fixed-effects estimate of 1.132 to 1.196 for the random-effects estimate. This result was in line with the slope direction result in the Egger Test for asymmetry. The fixed-effects estimate was within the random-effects confidence interval but the random effects estimate was outside the fixed-effects confidence interval which is expected given the smaller intervals in a fixed-effects model.

Figure 20: Forest Plot - Study Type Subgroups, Fixed-Effects and Random-Effects Models

Conversely, with the comparative/cross-sectional study type subgroup, there was a major and significant decrease of 50% from the fixed-effects model result of 1.941 to random-effects model of 1.441. The two results were very far apart with relation to their opposing confidence intervals. This is in line with the results of the exploratory analysis.

4.5.2 Meta-Regression Results

This section will discuss the results of the meta-regression analysis. Table 12 are the descriptive statistics for each of the models. Model 1 included all 155 effect estimates, while model 2 included 68 effect estimates corresponding to the number of simple before-after study estimates. Model 3 included 87 estimates corresponding to the number of cross-sectional study estimates. Table 13 to Table 15 are the model outputs for each of

the three models from Model 1, 2 and 3 respectively, including values of the coefficients, z-value, p-value, 95% confidence intervals and Variance Inflation Factors (VIF) for the coefficients.

Table 12: Meta-Regression Model Descriptive Statistics

	Model 1 - All Effect Estimates				Model 2 - Before-After Estimates				Model 3 - Comparative Estimates			
Variables	MIN	MAX	MEAN	SD	MIN	MAX	MEAN	SD	MIN	MAX	MEAN	SD
ln(o _i)	-1.270	2.485	0.212	0.561	-0.693	1.368	0.180	0.437	-1.270	2.485	0.237	0.641
INJ	0	1	0.117	-	0	1	0.265	-	-	-	-	-
PDO	0	1	0.110	-	0	1	0.250	-	-	-	-	-
PEAK	0	1	0.390	-	0	1	0.485	-	0	1	0.310	-
OFFPEAK	0	1	0.097	-	0	1	0.191	-	0	1	0.034	-
CS	0	1	0.565	-	-	-	-	-	-	-	-	-
LT	0	1	0.708	-	0	1	0.735	-	0	1	0.690	-
STATIC	0	1	0.734	-	0	1	0.750	-	0	1	0.724	-
DYN	0	1	0.532	-	0	1	0.441	-	0	1	0.609	-
DELIN	0	1	0.091	-	0	1	0.074	-	0	1	0.103	-

Table 13: Model 1 Results

Model 1: All Effect Estimates Included							
I^2 : 63.08%		R^2 : 54.01%			n = 155		
Variable	Coefficient	Standard Error	z-value	p-value	95% CI lower bound	95% CI upper bound	VIF
α	-0.1744	0.1625	-1.0731	0.2832	-0.493	0.1442	
<i>INJ</i>	0.1735	0.1045	1.6608	0.0968	-0.0313	0.3783	1.3124
<i>PDO</i>	0.0447	0.0988	0.4523	0.651	-0.1489	0.2383	1.3592
<i>PEAK</i>	0.2519	0.0747	3.3711	0.0007	0.1054	0.3983	1.1597
<i>OFFPEAK</i>	-0.0857	0.0887	-0.9659	0.3341	-0.2595	0.0881	1.1897
<i>CS</i>	0.2249	0.0792	2.8389	0.0045	0.0696	0.3802	1.4594
<i>LT</i>	-0.1555	0.0866	-1.7954	0.0726	-0.3252	0.0143	1.8219
<i>STATIC</i>	0.3223	0.086	3.7482	0.0002	0.1538	0.4909	1.5479
<i>DYN</i>	0.2595	0.1046	2.4806	0.0131	0.0545	0.4645	2.6162
<i>DELIN</i>	-0.3268	0.1811	-1.8046	0.0711	-0.6817	0.0281	1.0934

Model 1 is the result of the log odds ratios for all extracted study estimates, regressed on the variables given in Table 13. In general, the I^2 value indicated a relatively high amount of systematic heterogeneity at 63.08%. The R^2 value indicates that approximately 54.01% of the heterogeneity is accounted for in this model. The coefficients in this model are mixed in terms of sign. With relation to collision severity, the coefficient for the injury (*INJ*) variable was a positive and significant variable to 10% significance, while the coefficient for the PDO variable was a smaller positive but non-significant value. With relation to the time-of-occurrence variables, the peak period coefficient was a positive and statistically significant value, while the off-peak period (*OFFPEAK*) coefficient was a negative value but was not statistically significant. The cross-sectional study coefficient (*CS*) was a positive and significant value, suggesting that comparative/cross sectional studies are more likely to observe an increase in crashes as a result of installation of the treatment. With relation to the operational characteristics of

the study locations, the left-turn restriction coefficient (*LT*) was found to have a negative and significant value to 10% significance, while the temporary barrier or delineator variable (*DELIN*) coefficient was also found to have a negative and significant value to 10% significance. On the other hand, the static sign coefficient (*STATIC*) and the dynamic lane signal coefficient (*DYN*) was found to have a positive and significant value to a 5% significance.

Table 14: Model 2 Results

Model 2: Simple Before-After Studies Only							
I^2 : 63.49%		R^2 : 29.62%		n = 68			
Variable	Coefficient	Standard Error	z-value	p-value	95% CI lower bound	95% CI upper bound	VIF
α	0.0965	0.1989	0.485	0.6277	-0.2934	0.4864	
<i>INJ</i>	0.1427	0.0925	1.5419	0.1231	-0.0387	0.324	1.2556
<i>PDO</i>	-0.0075	0.0869	-0.0865	0.9311	-0.1779	0.1629	1.2974
<i>PEAK</i>	0.2326	0.0889	2.6155	0.0089	0.0583	0.4069	1.4333
<i>OFFPEAK</i>	-0.1747	0.0916	-1.9068	0.0565	-0.3543	0.0049	1.388
<i>LT</i>	-0.2015	0.1078	-1.87	0.0615	-0.4127	0.0097	2.443
<i>STATIC</i>	0.2616	0.1061	2.4657	0.0137	0.0537	0.4695	2.2545
<i>DYN</i>	-0.0402	0.1316	-0.3051	0.7603	-0.2982	0.2179	3.8658
<i>DELIN</i>	0.1591	0.294	0.541	0.5885	-0.4171	0.7352	1.1234

Model 2 is the result of the log odds ratios for the study estimates from the simple-before-after studies only, regressed on the variables given in Table 14. I^2 values showed that the amount of systematic heterogeneity was high at 63.49%, while the value of R^2 suggests that 29.62% of the heterogeneity has been accounted for in this model. The coefficient for *INJ*, the injury collision variable was positive but not significant, while *PDO* resulted in a negative slope which was also not significant. For the time of occurrence variables, the

PEAK coefficient resulted in a positive value, while the *OFFPEAK* coefficient was negative. Both the *PEAK* and *OFFPEAK* coefficients were statistically significant at 10% with p-values of 0.0089 and 0.0565 respectively. For the operational variables, *LT* coefficient was negative and significant to 10%, *DELIN* on the other hand was positive but not significant. The *STATIC* coefficient was positive and significant, while the *DYN* variable coefficient was negative, but non-significant.

Table 15: Model 3 Results

Model 3: Cross-Sectional Studies Only							
I^2 : 52.92%		R^2 : 35.48%		n = 87			
Variable	Coefficient	Standard Error	z-value	p-value	95% CI lower bound	95% CI upper bound	VIF
α	0.1317	0.2408	0.5468	0.5845	-0.3403	0.6037	
<i>PEAK</i>	0.4329	0.1499	2.887	0.0039	0.139	0.7268	1.2526
<i>OFFPEAK</i>	0.1923	0.229	0.84	0.4009	-0.2564	0.6411	1.29
<i>LT</i>	-0.3288	0.1403	-2.3438	0.0191	-0.6037	-0.0539	1.4193
<i>STATIC</i>	-0.0698	0.1666	-0.4189	0.6753	-0.3964	0.2568	1.5989
<i>DYN</i>	0.5267	0.1612	3.2674	0.0011	0.2108	0.8426	1.47
<i>DELIN</i>	-0.2501	0.2445	-1.0229	0.3063	-0.7293	0.2291	1.1159

Model 3 is the result of the log odds ratios for the study estimates from the cross-sectional studies only, regressed on the variables given in Table 15. I^2 values showed that there was a medium level of heterogeneity at 52.92%, while R^2 values suggest that 35.48% of the variation has been accounted for in the model. With relation to the time-of-occurrence variable, the peak period variable was found to be a positive and significant value, while the off-peak period variable coefficient was found to also be a positive and smaller value compared to the peak period coefficient, although the value was not

significant. The left-turn restriction coefficient was found to be a negative and significant value, while the delineation coefficient was also found to be a negative but non-significant value. STATIC was a negative, non-significant value while DYN was a positive and significant value.

With relation to multicollinearity, in Model 1, VIF for all model variables ranged between 1.093 and 2.626 with the highest VIF value being the dynamic variable coefficient. In Model 2, the majority of VIF values ranged between 1.123 and 2.443 with the DYN variable having the highest VIF value of 3.866. In Model 3 VIF values ranged between 1.116 and 1.599, with the static variable having the largest VIF value. As VIF values never rose above the rule-of-thumb threshold of 4, multicollinearity was found to not be a major concern in the three models.

4.5.3 Study Age/Time Trends

Given the trends with relation to certain older studies being to the left of the mean effect estimate, and newer studies being to the right of the mean from the exploratory analysis, a meta-regression analysis was performed on the basis of a single variable denoting publication year for all effect estimates combined, for cross-sectional studies only, and for before-after studies only. The results are given in Table 12. Table 12 shows that there is a significant, positive slope for the YEAR variable in all cases.

Table 16: Meta-Regression for Publication Year Only - Results

Subgroup	Variable	Coefficient	Standard Error	z-value	p-value	95% CI lower bound	95% CI upper bound
All Effect Estimates	α	-34.881	5.378	-6.486	<.0001	-45.422	-24.340
	YEAR	0.018	0.003	6.537	<.0001	0.012	0.023
Before-After Studies	α	-26.778	8.951	-2.992	0.003	-44.321	-9.234
	YEAR	0.014	0.005	3.012	0.003	0.005	0.023
Cross-Sectional Studies	α	-36.807	9.301	-3.957	<.0001	-55.036	-18.578
	YEAR	0.019	0.005	4.000	<.0001	0.010	0.028

As mentioned in the exploratory analysis section, a general time related trend was noted for those studies with high precision/low standard error, such as *DeRose (1966)* and *Dey, Ma and Aden (2011)*, although there may be additional factors that may play a role in the result, including study methodology and the operational characteristics of each site.

In this regard, an analysis was performed similar to the previous section, with the addition of a publication year variable, and the results of this meta-regression analysis are provided in Appendix C. Results will be summarized here and will be discussed in the Summary and Discussion section related to meta-regression, Section 4.7.3. In Appendix C, All Effect Estimates Combined will refer to Model A, Before-After Studies only will be referred to as Model B, Cross-Sectional Studies only will be referred to as Model C.

In Model A, the coefficient for year is a positive, significant value with a model coefficient of 0.0132, and p-value of 0.017. The peak period collision coefficient and the dynamic control coefficient is positive and significant; however no other variables are significant. Despite significance, the direction of the coefficients for both Model 1 and

Model A is the same for almost all variables except for LT, where this variable flipped to positive in Model A. LT was significant in Model 1 but not in Model A. The VIF for the YEAR variable was 4.597, which was slightly above the threshold of 4, and additional variables saw an increase in reported VIF from the reported model, including the CS, LT and DYN variable.

In Model B, the coefficient for year is a negative, non-significant value of -0.0108, and p-value of 0.756. PEAK and OFFPEAK were the only variables that were statistically significant in this case. In this case, despite significance, the coefficients for all other variables other than year were in the same direction as Model 2. The VIF for the YEAR variable in this case was a large value of 70.606, as was the LT and STATIC variables, at a VIF of 65.45 and 52.961 respectively. DYN rose above the threshold VIF at 4.586.

In Model C, the coefficient for year is a positive and significant value of 0.0141 and p-value of 0.0623. DYN was also a statistically significant, positive value but no other coefficients were statistically significant. Despite significance, coefficients were consistent in Model 3 and Model C, except for OFFPEAK, which changed sign to negative in Model C. OFFPEAK was not significant in either model. No variable rose above a VIF of 4, although the VIF for YEAR was the largest value at 2.817 in both Model 1 and Model C, where VIF ranged between 1.111 and 1.599 in Model 1, 1.284 and 1.856 in Model C for all other variables.

4.6 Outlier Analysis

In this section, the results of the outlier analysis will be provided. Based on the methodology section of the analysis, outlier analysis was performed for all estimates combined as well as all subgroups. The values reported here are in odds ratios, converted from their log odds ratio values.

The outlier analysis identified potential outliers for all effect estimates combined, for the injury subgroup, for the unspecified collision severity subgroup, peak period subgroup and off-peak period subgroups. Within the study methodology subgroups, outliers were identified for the cross-sectional subgroup only. Overall, for all effect estimates combined, two points were identified, one belonging to *Bretherton and Elhaj (1996)*, which upon removal increased the effect estimate by 4.3% from 1.289 to 1.332, the other belonging to *Dey, Ma and Aden (2011)* which decreased the estimate by 3.8% from 1.289 to 1.250.

For the injury subgroup, one point was found to be outlying, belonging to *Bretherton and Elhaj (1996)*, which decreased the estimate by 14.2% from 1.447 to 1.305. There were no outlying points in the PDO subgroup. For the unspecified severity subgroup, two outlying points increased the mean estimate of 1.302, one belonging to *DeRose (1966)*, which increased the mean estimate by 4.0% to 1.342, and another belonging to *Bretherton and Elhaj (1996)*, which increased the mean estimate by 6.1% to a mean estimate of 1.363. One outlying point decreased the main estimate, belonging to *Dey, Ma and Aden (2011)* by 4.9% to a mean estimate value of 1.254.

For the peak period subgroup, one identified outlying point belonging to *Dey, Ma and Aden (2011)* decreased the main estimate of 1.497 by 11% to a mean value of 1.386, while one outlying point belonging to *DeRose (1966)* increased the estimate by 11.4% to a mean value of 1.611. Finally, for the off-peak subgroup, one outlying point belonging to *DeRose (1966)* increased the mean estimate of 1.198 by 10.2% to a mean value of 1.300, while another point belonging to *Dey, Ma and Aden (2011)* decreased the estimate by 6.5% to a mean value of 1.133.

For the study type subgroups, no outlying points were identified in the before-after study subgroup. However, as identified previously, an outlying point was identified within the cross-sectional subgroup belonging to *Dey, Ma and Aden (2011)*, which decreased the original mean estimate by 13.6% from 1.941 to 1.805.

Overall, the outlying points from this analysis belonged to three studies, *DeRose (1966)*, *Bretherton and Elhaj (1996)*, and *Dey, Ma and Aden (2011)*.

4.7 Summary and Discussion – Main and Outlier Analysis

This section will discuss the results of the main analysis, including the traditional analysis and the meta-regression analysis, as well as potential explanations for behaviour of the data. Discussion of the outlier analysis will also be performed in this section.

4.7.1 Traditional Meta-Analysis

The results of the meta-analysis show that within most of the subgroups as well as all estimates combined, that installation of a reversible lane may result in a 46% increase in peak period collisions, 13% increase in off-peak period collisions, and 35% increase in injury collisions based on the random effects model. PDO collisions would be expected to increase by 17%. Overall, there would be a 30.9% to 32.7% increase in collisions, given all estimates and unspecified collision types respectively. Overall, there has been an increase in the estimated effect from the previous meta-analysis by *Elvik et al (2009)*. All results in the traditional meta-analysis were statistically significant, except for the off-peak subgroup.

The reason for the increases and change in significance from the results of *Elvik et al (2009)* is potentially because of the increase in sample size due to the inclusion of six additional studies and their corresponding effect estimates, as well as removal of a study due to language exclusion criteria. Specifically, the collecting or conducting of additional studies will generally decrease the variance (*Hauer et al. 2012*). Another potential reason for the increase between the results in this study is due to these additional studies belonging to predominately cross-sectional studies. The potential effect of this inclusion of additional studies with few to no control mechanisms, including cross-sectional studies will be discussed further in the coming paragraphs.

Comparing the fixed-effects and random-effects models for the collision type subgroups in Table 10, there were relatively small to modest changes in both directions ranging

from a 2.5% increase for the unspecified severity subgroup, to a 9.8% decrease from the fixed-effects to the random-effects models. In general, these increases and decreases seem to be in line with the results of the exploratory analysis, especially the slope directions returned by Egger's Test. The relatively small changes between the fixed-effects and random-effects models also reflect the lack of statistically significant publication bias and/or asymmetry in general for the collision type subgroups. This suggests that overall, for all of the studies as stratified by collision type, that asymmetry is not a major concern.

The off-peak period subgroup is a potential exception. However, the non-significant random-effects result, Egger's regression test result, combined with the contradictory results of the trim-and-fill analysis when using the R_o and L_o estimator as well as the high level of heterogeneity for this subgroup also suggest that asymmetry and publication bias may not be a concern. In this case, the result could be due to the characteristics of the effect estimates being a combination of low standard error studies, being of smaller sample size overall for the subgroup, and a mix between study types as well as a result of outliers which will be discussed in the coming sections.

Comparing the fixed- and random-effects models for the study type subgroups, the simple before-after study subgroup increased by 6.4% from a fixed-effects estimate of 1.132 to a random-effects estimate of 1.196. A medium to high amount of systematic variation was detected through an I^2 percentage of 74.8%, the corresponding exploratory analysis suggested that asymmetry test results were close to the 10% confidence

threshold but ultimately non-significant. The trim-and-fill sensitivity analysis also suggested that asymmetry due to publication bias was not a concern. However, the difference between the fixed- and random-effects results was such that the random-effects result was outside of the fixed-effects confidence interval for this subgroup. This is expected given the random-effects model's increased emphasis on studies with lower statistical weight versus the fixed-effects model's emphasis on studies with higher statistical weight.

Comparison of the random- and fixed-effects models for the cross-sectional study subgroup on the other hand showed a substantial decrease of 50% from the fixed effects model estimate of 1.941 to the random-effects model estimate of 1.441, as the random-effects model placed greater weight on the smaller studies on the left side. This is despite a smaller amount of systematic variation given by an I^2 of 66.49%, and asymmetry that was generally significant.

Asymmetry could also be due to several other sources as stated in *Egger et al (1997)* and *Sterne, Becker and Egger (2005)* and discussed briefly here. Specifically, with relation to the subgroups exhibiting significant asymmetry, the following may be present as adapted from *Sterne, Becker and Egger (2005)* for this study.

- Selection bias
 - Publication bias - unpublished and hard-to-locate gray literature or suppression of small studies showing large negative effect of reversible lanes
 - Location bias – English language restrictions and inclusion criteria overall may have led to selection of exclusively U.S studies
- True heterogeneity
 - Reporting requirements for PDO crashes changing over time and over jurisdictions (*e.g. Hauer 1997*)
 - Under-reporting of PDO crashes (*e.g. Hauer 1997*)
 - Differing thresholds for injury crashes by jurisdiction
 - Use of collision exposure rates vs collision frequencies between studies
 - Differences in cross-section, geometrics, operational characteristics between reversible lane sites
- Data irregularities
 - Poor methodological design
 - e.g unequal and/or short evaluation periods
 - Inadequate matching of treatment to comparison/control roads
- Chance

In general, the use of trim-and-fill testing is made under the assumption that a gap on one of the extreme sides of the funnel plot signifies suppression of a result against expectations and is of low precision or not statistically significant (*Duval 2005*).

However, in the case of observational studies, the effect of publication bias may potentially be secondary to the effect of potential known and unknown confounding factors, given that there is less control for these factors when compared to randomized controlled trials and studies (*Egger and Schneider 1998, Sterne et al. 2011*).

For example, with relation to use of collision frequencies vs collision rates as a performance indicator, collision rates are generally known to be a less accurate measure when compared to a raw collision frequency (*Bahar 2010*). This is because collision rates are generally a linear approximation of the relationship between traffic volume and collision frequency, which is known empirically to be nonlinear, reflecting the fact that driver behaviour changes according to traffic volume, speed and density (*Hauer 1997*). When comparing roads before and after, or with and without the treatment, this approximation may lead to erroneous conclusions as the effect of traffic volume on collisions is mixed with the effect of the treatment on collisions in a rate calculation (*Hauer 1997*). This difference may have manifested in the asymmetry present in the cross-sectional study subgroup, where the majority of extracted estimates were rates, while a small handful of studies used a frequency.

Other differences between the collected studies could also play a role in the observed asymmetry and heterogeneity. With relation to simple before-after studies, it has been shown that these types of studies generally report a larger improvement after installation of a traffic safety measure when compared to before-after studies with additional controls for confounding factors (*Høye and Elvik 2010*). With relation to cross-sectional studies

using regression, it has been shown that their results vary from study to study, given the variation in functional forms from study to study (*Hauer 2010*). The observations made so far suggest that this variation in methodologies may have occurred here, for cross-sectional studies not using a regression. Operational differences between study locations may also play a role in the overall results. A review of the characteristics of outliers as well as a meta-regression analysis may be able to provide more details as to the potential mechanisms of the observed asymmetry and heterogeneity.

4.7.2 Outlier Analysis

In this regard, the outlier analysis was performed and there were three studies identified where outliers were present and contributing to a large enough change to fall outside of the original estimate's 95% confidence interval. Specifically, these studies were *Bretherton and Elhaj (1996)*, *DeRose (1966)*, and *Dey, Ma and Aden (2011)*. The general trend was that removal of *Bretherton and Elhaj (1996)* outliers, as well as *DeRose (1966)* outliers increased the mean estimate, while *Dey, Ma and Aden (2011)* decreased the mean estimate.

One potential reason for these outlying points based a cursory look at the dataset, is that *Bretherton and Elhaj (1996)* and *DeRose (1966)* had unequal before-after study periods which required extrapolation of the before count using the methodology as developed in Section 3.2.4 - Adjustment of Traffic Safety Effects – Different Study Periods. Other reasons related to study design have been discussed previously.

With relation to *Dey, Ma and Aden (2011)*, this cross-sectional study collected collision data over a six-year period equally over road segments of similar length that the authors state were chosen based on similar traffic patterns. However, the authors do not state any additional characteristics that the road was matched under or controlled for. The study also elected to report collision data under a frequency but not an exposure rate, unlike other cross-sectional studies identified in this study. In general, cross-sectional studies suffer from methodological problems with relation to the effect of unknown or unrecorded factors (*Gross et al. 2010*). In regression cross-sectional studies, this has resulted in a lack of consistent results given the varying model design and variables used (*Hauer 2010*). These reasons, as well as the general finding that confounding factors can seem to overestimate the safety effects of road safety treatments may be reasons for the overall outlying effect for *Dey, Ma and Aden (2011)*. This general finding is based on studies of simple before-after studies when compared to improved methodologies (*Hauer 1997, Elvik 2002*).

4.7.3 Meta-Regression

In order to better model the heterogeneity between the studies in terms of operational characteristics and effects of study quality a meta-regression of all 155 effect estimates was suggested. Three models were devised for the meta-regression portion of the main-analysis, as discussed in Chapter 3. Model 1 is a meta-regression of all log-odds effect estimates against collision severity and time-of-occurrence, traffic control devices used, and the cross-sectional study type. Model 2 is a regression of log-odds from simple-

before-after studies only, while Model 3 is a regression for cross-sectional study effect estimates.

In Model 1, the additional explanatory variables were able to explain 54.01% of the heterogeneity. With relation to the study type, the coefficient for cross-sectional studies was positive at 0.2249, with a p-value of 0.005 suggesting significance to 10%. A potential inference based on this result and the results of the previous analysis is an indication that effects are being overestimated where studies are poorly controlled, as discussed previously. Specifically, cross-sectional studies captured in this analysis where control/comparison methodologies or matching of treatment roads with comparisons roads is limited, are more likely to observe an increase in crashes when compared to a simple before-after study. One other reason for this behaviour is the mix of a large number of effect estimates calculated with rates with two studies that provided frequencies as noted in earlier discussions.

A comparison of the I^2 illustrating the amount of unaccounted heterogeneity, as well as the R^2 for both Model 2 and Model 3 shows that Model 2 still has large amounts of heterogeneity around 63%, and a smaller amount of this heterogeneity is explained by the model variables at 29.62%. Model 3, on the other hand, had a moderate amount of heterogeneity at an I^2 of 52.92% and 35.48% of the heterogeneity explained by the model. As discussed in the exploratory analysis section, this difference in heterogeneity could be due to a larger variation in study methodology, study locations, collision types, and treatment road characteristics in the before-after subgroup as opposed to the cross-

sectional subgroup, coupled with missing explanatory variables. For example, traffic volume change could be a potential measure to be included, but was not included as there was a lack of either reported volumes for some studies, incorporation of traffic volume into a rate, or differences in reporting measures for traffic volumes between studies (e.g. use of AADT, peak hour volume etc.).

Finally, trends across models were made with relation to severity, time of occurrence and type of traffic control. In terms of severity, the injury coefficient over all three models was consistently higher than the PDO coefficient in both Models 1 and 2. This suggests a potentially larger change in potential injury collisions over PDO collisions which is reflected in the traditional meta-analysis. One potential explanation for this result can be inferred from the literature in Chapter 2, in that utilization rates of reversible lanes are lower, and corresponding violation rates or encroachment rates are higher when overall traffic volumes are low and operating speeds are higher, and utilization increases while violations decrease respectively when traffic volumes increase and speeds are lower.

With relation to collisions by time-of-occurrence, the peak period coefficient is consistently higher than the off-peak-period coefficient for all models. This is intuitive given the higher volumes and the time-of-operation of reversible lanes that coincide with the peak period. The peak period coefficient was also statistically significant across all models, while the off-peak coefficient was not significant across any model. This significance is reflected in the previous analyses, and potentially due to the characteristics of the subgroup as discussed previously.

With relation to traffic control and operational characteristics of the study sites, one operational characteristic was found to have a consistently negative and significant effect in the models, which was left-turn restriction. The rest of the variables capturing operational characteristics were found to have conflicting or varying levels of consistency from model to model.

For instance, the *DELIN* coefficient was negative and significant in Model 1, but in Model 2, the coefficient was positive and non-significant, while in Model 3, the coefficient was negative and non-significant.

For the *STATIC* coefficient, the estimate was positive and significant in both Model 1 and 2, while it was negative and non-significant in Model 3. The dynamic variable (*DYN*) coefficient on the other hand was positive and significant in Model 1 and 3, while the coefficient was negative and non-significant in Model 2.

With relation to traffic control and traffic operational characteristics, the overall trend is not completely clear between all three models, with the exception of left-turn restrictions. In general, it appears that left-turn restrictions would be expected to improve the traffic safety of reversible lanes. On the basis of model variables where statistical significance could be attained, it appears that the use of static signs and/or dynamic signs and signals tended to increase collision rates among the study sites, while the use of temporary barriers or delineators tended to decrease collision rates.

Although the input for the dataset for these variables depend on the interpretation of each study location's operational descriptions, these findings are aligned with the general inference that safety improves when complexity is reduced or incompatible road users are physically separated (*Elvik et al. 2009*). For instance, use of delineators or temporary barriers or left-turn restrictions would reduce complexity in the case of restriction, and physically separate users in the case of delineators. Although the traffic control measures used to delineate a reversible lane have been used consistently, reliably and safely in the field, the use of lane control signal gantries and signs would increase driver workload and requirements for interpretation. An opposite sign but non-significant coefficient was found for the *DELIN*, *STATIC* and *DYN* variables in one of three models. However, given that these results are not statistically significant, the results on the whole are consistent with the inferences made above.

4.7.4 Time Trends Discussion

Based on the findings of the exploratory analysis a meta-regression of all effect estimates combined, before-after estimates only, and cross-sectional studies only was performed to determine the relationship between the effect estimates and the year of publication. In all subgroups, the YEAR coefficient was positive and statistically significant, suggesting that collisions on reversible lanes have increased as a function of the publication year.

Publication year was not considered in the initial analysis, as the literature on reversible lanes, and traffic safety in general suggested that other factors may play a bigger role in the observed traffic safety of reversible lanes. As discussed before, the effect of

confounding factors and lack of subsequent control may affect the observed result of an analysis. Within the reversible lane research, there is a great deal of variation with relation to operational characteristics from location to location. However, given the results of the meta-regression of CMFs to the publication year, a brief and cursory analysis was performed similar to the meta-regression reported in Section 4.5.2, with the addition of a YEAR variable, signifying publication year. The results are briefly discussed here, and complete tables are provided in Appendix C.

As summarized in Section 4.5.3, we find that the coefficient is positive and significant in Model A, all estimates combined, and Model C, cross sectional studies only, but was negative and not significant in Model B, simple before-after studies only. This may suggest an overall positive relationship between study age and observed traffic safety, but the observations from the exploratory analysis and outlier sensitivity analysis suggest that the study methodology and study location's characteristics may also be confounding factors that make an analysis including publication year potentially spurious, despite plausible reasons for the relationship. This has been observed in other medical meta-analyses where studies' findings have contradicted each other despite plausible explanations for either finding (*Egger and Schneider 1998*).

There are also varying levels of potential multicollinearity in Models A and B. The YEAR variable in Model A marginally crossed the VIF threshold and VIF was increased marginally in study quality and operational characteristics variables. In Model B, the YEAR variable VIF was very large, and several operational characteristics' variables

increased significantly or marginally in terms of VIF. In Model C, the YEAR variable VIF value was below the threshold value of 4, but was the highest value reported in Model C compared to the other variables. For most of the model coefficients, the sign did not change regardless of change in significance, or the sign changed in a variable that was not significant in both the model with and without the publication year as a covariate. In one case, the LT coefficient was negative but significant in Model 1, while in Model A this coefficient was positive but not significant.

For Model C, VIF never rose past a value of 4, suggesting that multicollinearity was not as large of a problem as in Models A and B. However, in this case, the confounding factors of study design and methodology may still play a larger role, as evidenced by the exploratory and main analysis. Specifically, the outlying nature and influence of recent cross-sectional studies in terms of their high precision and study design may be leading to the significant result in YEAR when compared to simple before-after studies in Model B.

A high VIF does not necessarily mean an invalid model (*O'Brien 2007, Tay 2017*), and both models with and without YEAR as a variable are reported. However, this result suggests that including YEAR as a variable may be collinear with the methodology of the study and the location's characteristics.

Based on Tay (*2017*), Models 1, 2 and 3 would provide a more precise estimate and indication to practitioners as to what mitigations may be effective in improving the safety of reversible lanes as indicated by the significance of the variable, and Models A, B and

C are generally in line with the results of Models 1 and 2 despite the lack of significance in these models including publication year.

In general, study age appears to be a significant factor, suggesting the collisions have increased on reversible lanes over time. One potential explanation for this relation is that traffic overall may have increased year over year, although there is not enough data available to include this as a variable. However, this analysis shows that this relation may also be as a result of study methodology and operational characteristics. In general more study of the effect of including study age or suitable proxy is suggested as a potential study question in future analyses on this topic.

4.7.5 Transferability of Results

A limitation of the study is the predominance of US studies in the selected studies. Traffic and transportation engineering standards in North America are generally more uniform with guidance such as the Manual of Traffic Control Devices (*Federal Highway Administration 2009*) and its Canadian counterpart Manual of Traffic Control Devices for Canada (*Transportation Association of Canada 2014*). In addition, there is voluntary guidance from both US transportation bodies (*Wolshon and Lambert 2004*) and Canadian transportation bodies (*Guebert 2010*), with the Canadian guidance appearing to draw partly from American practice. Given the smaller confidence intervals and generally significant results, there can be some confidence that reversible lanes increase crashes based on these results. With relation to transferability of results, practitioners in Canada should be able to use these results given the similarities in language and technical

guidance, although there may be confounding factors that may affect the applicability of this research in the Canadian context, such as the jurisdiction's familiarity with reversible lanes, or adverse weather conditions. This assumed transferability may be less applicable in other places, for example in Europe where language transferability and jurisdictional differences would be assumed to be more of an issue. There is also the issue of familiarity of the treatment in the jurisdiction among other potentially unknown factors.

In one study examining the foundations of CMFs (*Hauer et al. 2012*), the authors state that transferability will always be an issue and the true CMF will always be unknown and can only be estimated. In order to provide a CMF that will be an adequate predictor of the safety effect, these authors posit that two things can improve the predictive power of any CMF, and these are the inclusion of additional studies, and the inclusion of the factor, in this case location as a variable in any future functions. An attempt has been made through meta-regression to account for some factors, but these results can be improved upon with additional studies, and the accounting for issues of transferability in future research. In the meantime, this research can be used in lieu of such a CMF.

CHAPTER FIVE: CONCLUSIONS AND FURTHER RESEARCH

5.1 Conclusions

In this study, a meta-analysis reviewing the traffic safety effect of reversible lanes was performed to quantify the observed and recorded effects from the literature, building from previous meta-analysis on this topic as well as the wide body of knowledge on this traffic engineering measure. This study builds on this research by including several other studies and performing a meta-regression analysis to gauge the effects of study quality as well as operational characteristics of each study and effect estimate identified.

Overall, ten studies were selected for inclusion in the meta-analysis, characterized as mainly U.S studies published between 1966 and 2011, employing either a simple before-after methodology or cross-sectional methodology without regression analysis. Overall, 155 effect estimates were extracted from these ten studies using the log odds ratio method of meta-analysis.

Extracted effect estimates were analyzed for all estimates combined, then stratified and analyzed by collision severity, collision time of occurrence, as well as by study methodology. Subgroups formed include collision severity by injury, PDO and unspecified severity. Collision time of occurrence subgroups formed include peak-period collisions and off-peak period collisions. Stratification was also made along the lines of the study methodology, specifically studies were grouped into either the simple before-after subgroup or comparative/cross-sectional studies subgroup.

An exploratory analysis of the dataset showed that for most subgroups, there was slight asymmetry favouring either side of the mean depending on the subgroup. Asymmetry in the injury, peak period and off-peak period subgroups were found to favour the left side, while PDO, unspecified severity crashes and all estimates combined were found to favour the right side, although this asymmetry was not significant. One exception to this was the cross-sectional study subgroup, where asymmetry and potential asymmetry due to publication bias was found to favour the left side of the mean to a statistically significant level. Potential reasons for this asymmetry include publication bias, but could also be due to the differences in each study. These differences include the study methodology in terms of confounding factor control and performance indicator chosen with relation to collision frequency or rate, the year of publication, and operational characteristics of the study locations.

A review of heterogeneity suggested a generally significant level of between-study variance was present. The high level of heterogeneity suggests that there are significant differences between the studies owing to potential confounding factors, including those that have been mentioned previously with relation to study methodology and operational characteristics. In this case, a fixed-effects model was determined to be inadequate, and was supplemented with both a random-effects model and meta-regression model in order to account for the heterogeneity between studies. An outlying point in the off-peak period exploratory analysis also highlighted the need for an outlier analysis.

The traditional meta-analysis showed that based on the random-effects model, PDO collisions are expected to increase by 16.6%, while injury collisions were expected to increase by 34.9%. The remainder of the studies reporting unspecified collision severity reported an overall 32.7% increase in collisions. In the peak period, collisions are expected to increase by 46.2%, while off-peak period collisions are expected to increase by 12.8%. Overall, a 30.9% increase is expected based on all 155 estimates. All mean estimates were statistically significant, with the exception of the off-peak period subgroup.

Overall, an increase was noted from this analysis as compared to the previous meta-analysis on this subject. As was noted in the narrative literature review, results on the traffic safety effect of reversible lanes appeared mixed with more studies reporting an increase, so a statistically significant increase would be in line with this observation as the sample size increases. However, the effects of study methodology with poorer control on confounding factors have also been known in traffic safety to exaggerate the safety effect or cause the safety to appear varied with no overall trend. In this case, the inclusion of studies with poorer control, specifically recent cross-sectional studies which are also potentially outlying may have led to this increase from the previous meta-analysis.

Comparison of the random-effects model against the fixed-effects model showed that increases and decreases were generally in line with the direction of asymmetry. This is due to the placing of additional statistical weight on those studies of lower precision, given that the assumption of studies converging toward a true mean as the precision

increases is less applicable where there is significant heterogeneity. Most of the changes were small to modest changes which were within the confidence intervals of their neighbouring models' mean effect estimates. The off-peak period subgroup's random-effects estimate was found to be non-significant and outside of the fixed-effects confidence interval and this could be due to the very heterogeneous nature of this subgroup and relatively small sample size. However, the relative change in this subgroup was relatively small between the fixed-effects and random-effects models. The cross-sectional study subgroup exhibited significant changes between the two models, which was in line with the statistically significant asymmetry in this subgroup.

As discussed in more detail in Chapter 4 and earlier in this chapter, this asymmetry could be due to several reasons. Specifically, these reasons are selection bias due to the restrictions placed by the inclusion/exclusion criteria, selection bias due to unpublished and/or suppressed literature, asymmetry due to true heterogeneity, and due to data irregularities given a potentially poor methodology, as well as chance.

Given the current studies, outlier bias was also analyzed using the process of leaving one effect estimate out, recalculating the mean estimate and comparing the result with the original estimate. Through this analysis, it was found that three studies were responsible for 11 outliers encountered. Two of the studies were before-after studies that were found to have unequal study periods, and one potential reason for the outlying points from these studies was due to the extrapolation of the before collision count based on these unequal study periods. This is in addition to the generally known propensity of a simple before-

after study to potentially overestimate the traffic safety effect of treatments. One cross-sectional study was found that differed from the majority of other cross-sectional studies in that a frequency was reported. Another reason was that the study may have been matched on a limited number of characteristics, and several potential confounding factors may not have been accounted for in the analysis. One final reason is that this particular study was done on a reversible lane road with dynamic lane control side mounted poles rather than overhead lane control gantries, a concern raised by the authors.

A meta-regression was completed, which allowed for inferences to be made based on study quality and characteristics of the reversible lane roadways observed in each study. Below are the general findings of the meta-regression.

- The cross-sectional variable coefficient is positive and significant, suggesting that poorer study control may potentially be leading to a larger result compared to simple before-after studies
- The injury collision coefficient is generally a larger positive number compared to the PDO coefficient, in line with the narrative literature review finding related to encroachment/violation rates as a function of traffic volumes
- Peak period coefficient is generally a significant, positive value, while the off-peak coefficient is generally a smaller positive value or negative value of varying significance
- Presence of left-turn restriction coefficient was generally a negative and significant value

- Presence of delineator coefficient was generally negative, suggesting a decrease in crashes.
- Dynamic traffic control and static traffic control coefficients generally both positive, but no trend favouring one or the other was apparent between the studies.
- With relation to DELIN, DYN and STATIC, the coefficients were generally significant, however the opposite sign was observed from the above findings but these coefficients were generally not significant

With relation to the different traffic control measures taken, the results are in line with the general inference that traffic safety is improved upon installation of a treatment that decreases complexity or separates incompatible road users.

Given the observed relationship between publication year and traffic safety effects from the exploratory analysis, a meta-regression of the collected effect estimates regressed against the year of publication showed that the relationship of publication year to traffic safety was positive, suggesting that collisions would be expected to increase as a function of time.

Given this preliminary result, a meta-regression including publication year as a variable was performed. The results from this meta-regression suggests a mixed result, with a positive and significant effect observed in the model with all estimates combined, and for the model with cross-sectional studies only. A negative, but non-significant coefficient

was observed in the model for before-after studies only. In addition, marginal to significant multicollinearity was observed in two of three of the models. In general, the results of the models with publication year included were of the same sign as the models without publication year included, significance notwithstanding. In general, study age may be a significant confounding factor, although the effect of other confounding factors such as study methodology and operational characteristics may potentially be collinear with the publication year. In general, more study may be needed, and this subject could be a point of future study.

Finally, the transferability of the results from this thesis was discussed. Given the use of reversible lanes in the US and Canada, the results should be transferable to Canada on the basis of the similarities between the two jurisdictions, and may be less transferable as a function of jurisdictional differences and familiarity of road users to the treatment, among other potential factors. Future analyses on this subject would be expected to improve the transferability of this result.

In general, the reader is cautioned from relying too heavily on the results, given the high levels of heterogeneity between the studies. However, in the absence of newer studies using methodologies that control for confounding factors, the general results of this research, with its confidence intervals, can be used to inform planning activities and further research.

5.2 Further Research

In general, there appears to be a substantial amount of research done on the subject of the traffic safety effect of reversible lanes. However, with relation to the meta-analysis, the study is limited on the basis of observed high levels of heterogeneity, some observed asymmetry from the exploratory analysis, and statistical significance of one of the main analysis results. Results from future meta-analysis and the topic in general could be improved by the inclusion of existing grey literature in future meta-analyses as well as the completion of future studies employing methodologies which better control for confounding factors. Specifically, there is much more research to be completed on reversible lanes even in the review of collision frequencies, and can be further expanded to the review of traffic conflicts as well as microsimulations.

Another area of future research is the development of study quality scoring assuming a log-odds ratio method of analysis, as suggested from review articles on the subject of meta-analysis. Although methodology exists for a raw odds ratio as developed for the Highway Safety Manual, and this study attempted to account for study quality with a meta-regression, study quality scoring for log-odds ratios would better reflect the quality of the study in the effect estimate's statistical weight as suggested by others, and would potentially improve the result here. A quality scoring scheme was outside of the scope of this study.

Finally, given the results of the systematic literature review, there does not appear to be any studies that employ newer and widely used study methodologies that better control

for confounding factors, such as Before-After studies using Empirical Bayes, or cross-sectional studies that integrate regression or matching schemes to control for confounding factors. The body of knowledge on reversible lanes would be well served by such additional studies.

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APPENDIX A: LIST OF SEARCH TERMS USED

Table 17: Search Terms, Engines Used, Query Dates and Number of Results

Search Term String	Number of Results	Search Engine Used	Date of Query (Estimated)
reversible lane safety	105	Transport Research International Documentation (TRID)	(October 2017-March 2018)
reversible lane	135	ITE Library	(March to April 2018)
"reversible lanes" OR "reversible lane" OR "lane reversal" "collision rate"	9	Google Scholar	26-Jun-18
"reversible lanes" OR "reversible lane" OR "lane reversal" "accident rate"	133	Google Scholar	26-Jun-18
"reversible lanes" OR "reversible lane" OR "lane reversal" "crash rate"	95	Google Scholar	05-Jul-18
"reversible lanes" OR "reversible lane" OR "lane reversal" "accident rates"	144	Google Scholar	05-Jul-18
"reversible lanes" OR "reversible lane" OR "lane reversal" "collision rates"	13	Google Scholar	05-Jul-18
"reversible lanes" OR "reversible lane" OR "lane reversal" "crash rates"	93	Google Scholar	05-Jul-18
"reversible lanes" OR "reversible lane" OR "lane reversal" "accident frequency"	41	Google Scholar	30-Jul-18
"reversible lanes" OR "reversible lane" OR "lane reversal" "collision frequency"	6	Google Scholar	31-Jul-18
"reversible lanes" OR "reversible lane" OR "lane reversal" "crash frequency"	51	Google Scholar	31-Jul-18
"reversible lanes" OR "reversible lane" OR "lane reversal" "accident frequencies"	26	Google Scholar	01-Aug-18
"reversible lanes" OR "reversible lane" OR "lane reversal" "collision frequencies"	4	Google Scholar	01-Aug-18
"reversible lanes" OR "reversible lane" OR "lane reversal" "crash frequencies"	27	Google Scholar	01-Aug-18
collision OR crash OR accident "reversible lane"	413	Google Scholar	01-Aug-18
collision OR crash OR accident "reversible lanes"	575	Google Scholar	29-Aug-18
collision OR crash OR accident "lane reversal"	218	Google Scholar	14-Sep-18
collision OR crash OR accident "lane reversals"	60	Google Scholar	25-Sep-18

Search Term String	Number of Results	Search Engine Used	Date of Query (Estimated)
lane reversal*	41	ITE Library	27-Sep-18
reversible lan*	1	ITE Library	01-Oct-18
reversible lane OR reversible lanes OR reversible lane*	136	ITE Library	01-Oct-18
tidal flow OR tidal traffic	7	ITE Library	05-Oct-18
collision OR crash OR accident "tidal flow lane"	16	Google Scholar	05-Oct-18
collision OR crash OR accident "tidal flow lanes"	16	Google Scholar	09-Oct-18
collision OR crash OR accident "tidal flow traffic"	11	Google Scholar	09-Oct-18
collision OR crash OR accident "tidal traffic"	45	Google Scholar	09-Oct-18
safety reversible lan*	167	TRID	10-Oct-18
safety lane reversal*	50	TRID	16-Oct-18
collision* crash* accident* reversible lan*	18	TRID	19-Oct-18
collision* crash* accident* lane reversal*	2	TRID	19-Oct-18
safety tidal traffic	44	TRID	19-Oct-18
safety tidal flow lan*	20	TRID	23-Oct-18
collision* crash* accident* tidal traffic	1	TRID	23-Oct-18
collision* crash* accident* tidal flow lan*	0	TRID	23-Oct-18

APPENDIX B: META-ANALYSIS DATA

Table 18: Extracted Effect Estimates’ Study Characteristics

Study No.	Author	Publication Year	Study Location	Study Type	Crash Severity	Crash Type	Study Location 1	Study Location 2
1	Agent, Clark	1980	Lexington, KY	Simple Before-After	Unspecified	AM Peak	Nicholasville Road	-
2	Agent, Clark	1980	Lexington, KY	Simple Before-After	PDO	AM Peak	Nicholasville Road	-
3	Agent, Clark	1980	Lexington, KY	Simple Before-After	Injury	AM Peak	Nicholasville Road	-
4	Agent, Clark	1980	Lexington, KY	Simple Before-After	Unspecified	PM Peak	Nicholasville Road	-
5	Agent, Clark	1980	Lexington, KY	Simple Before-After	PDO	PM Peak	Nicholasville Road	-
6	Agent, Clark	1980	Lexington, KY	Simple Before-After	Injury	PM Peak	Nicholasville Road	-
7	Agent, Clark	1980	Lexington, KY	Simple Before-After	Unspecified	Off Peak	Nicholasville Road	-
8	Agent, Clark	1980	Lexington, KY	Simple Before-After	PDO	Off Peak	Nicholasville Road	-
9	Agent, Clark	1980	Lexington, KY	Simple Before-After	Injury	Off Peak	Nicholasville Road	-
10	Agent, Clark	1980	Lexington, KY	Simple Before-After	Unspecified	All Collisions	Nicholasville Road	-
11	Agent, Clark	1980	Lexington, KY	Simple Before-After	PDO	All Collisions	Nicholasville Road	-
12	Agent, Clark	1980	Lexington, KY	Simple Before-After	Injury	All Collisions	Nicholasville Road	-
13	Bretherton, Elhaj	1996	Atlanta, GA	Simple Before-After	Unspecified	All Collisions	U.S.78 from SR 124 to East Park Place Boulevard	-
14	Bretherton, Elhaj	1996	Atlanta, GA	Simple Before-After	Injury	All Collisions	U.S.78 from SR 124 to East Park Place Boulevard	-
15	Bretherton, Elhaj	1996	Atlanta, GA	Simple Before-After	PDO	All Collisions	U.S.78 from SR 124 to East Park Place Boulevard	-
16	Bretherton, Elhaj	1996	Atlanta, GA	Simple Before-After	Unspecified	All Collisions	U.S.78 from SR 124 to East Park Place Boulevard	-
17	Bretherton, Elhaj	1996	Atlanta, GA	Simple Before-After	Unspecified	All Collisions	U.S.78 from SR 124 to East Park Place Boulevard	-
18	Bretherton, Elhaj	1996	Atlanta, GA	Simple Before-After	Injury	All Collisions	U.S.78 from SR 124 to East Park Place Boulevard	-
19	City of Phoenix	2010	Phoenix, AZ	Comparative/Cross-Sectional	Unspecified	AM Peak	7th Avenue Segments	19th Avenue Segments
20	City of Phoenix	2010	Phoenix, AZ	Comparative/Cross-Sectional	Unspecified	AM Peak	7th Street Segments	19th Avenue Segments
21	City of Phoenix	2010	Phoenix, AZ	Comparative/Cross-Sectional	Unspecified	AM Peak	7th Avenue Segments	16th Avenue Segments
22	City of Phoenix	2010	Phoenix, AZ	Comparative/Cross-Sectional	Unspecified	AM Peak	7th Street Segments	16th Avenue Segments
23	City of Phoenix	2010	Phoenix, AZ	Comparative/Cross-Sectional	Unspecified	AM Peak	7th Avenue Intersections	19th Avenue Intersections
24	City of Phoenix	2010	Phoenix, AZ	Comparative/Cross-Sectional	Unspecified	AM Peak	7th Street Intersections	19th Avenue Intersections
25	City of Phoenix	2010	Phoenix, AZ	Comparative/Cross-Sectional	Unspecified	AM Peak	7th Avenue Intersections	16th Avenue Intersections
26	City of Phoenix	2010	Phoenix, AZ	Comparative/Cross-Sectional	Unspecified	AM Peak	7th Street Intersections	16th Avenue Intersections
27	City of Phoenix	2010	Phoenix, AZ	Comparative/Cross-Sectional	Unspecified	PM Peak	7th Avenue Segments	19th Avenue Segments
28	City of Phoenix	2010	Phoenix, AZ	Comparative/Cross-Sectional	Unspecified	PM Peak	7th Street Segments	19th Avenue Segments
29	City of Phoenix	2010	Phoenix, AZ	Comparative/Cross-Sectional	Unspecified	PM Peak	7th Avenue Segments	16th Avenue Segments
30	City of Phoenix	2010	Phoenix, AZ	Comparative/Cross-Sectional	Unspecified	PM Peak	7th Street Segments	16th Avenue Segments
31	City of Phoenix	2010	Phoenix, AZ	Comparative/Cross-Sectional	Unspecified	PM Peak	7th Avenue Intersections	19th Avenue Intersections
32	City of Phoenix	2010	Phoenix, AZ	Comparative/Cross-Sectional	Unspecified	PM Peak	7th Street Intersections	19th Avenue Intersections
33	City of Phoenix	2010	Phoenix, AZ	Comparative/Cross-Sectional	Unspecified	PM Peak	7th Avenue Intersections	16th Avenue Intersections
34	City of Phoenix	2010	Phoenix, AZ	Comparative/Cross-Sectional	Unspecified	PM Peak	7th Street Intersections	16th Avenue Intersections
35	Derosé	1966	Dearborn, MI	Simple Before-After	Unspecified	All Collisions	Michigan Avenue (U.S 12)	-
36	Derosé	1966	Dearborn, MI	Simple Before-After	Unspecified	AM Peak	Michigan Avenue (U.S 12)	-
37	Derosé	1966	Dearborn, MI	Simple Before-After	Unspecified	PM Peak	Michigan Avenue (U.S 12)	-

Study No.	Author	Publication Year	Study Location	Study Type	Crash Severity	Crash Type	Study Location 1	Study Location 2
38	Derosé	1966	Dearborn, MI	Simple Before-After	Unspecified	Total Peak	Michigan Avenue (U.S 12)	-
39	Derosé	1966	Dearborn, MI	Simple Before-After	Unspecified	Off Peak	Michigan Avenue (U.S 12)	-
40	Dey and Ma	2005	Washington, DC	Comparative/Cross-Sectional	Unspecified	All Collisions	Connecticut Ave (3 Intersections)	Massachusetts Ave (3 Intersections)
41	Dey and Ma	2006	Washington, DC	Comparative/Cross-Sectional	Unspecified	AM Peak	Connecticut Ave (3 Intersections)	Massachusetts Ave (3 Intersections)
42	Dey and Ma	2007	Washington, DC	Comparative/Cross-Sectional	Unspecified	PM Peak	Connecticut Ave (3 Intersections)	Massachusetts Ave (3 Intersections)
43	Dey and Ma	2007	Washington, DC	Comparative/Cross-Sectional	Unspecified	Total Peak	Connecticut Ave (3 Intersections)	Massachusetts Ave (3 Intersections)
44	Dey and Ma	2007	Washington, DC	Comparative/Cross-Sectional	Unspecified	Off-Peak	Connecticut Ave (3 Intersections)	Massachusetts Ave (3 Intersections)
45	Dey, Ma, Aden	2011	Washington, DC	Comparative/Cross-Sectional	Unspecified	All Collisions	Connecticut Ave	Wisconsin Ave (Comparison Road 1)
46	Dey, Ma, Aden	2011	Washington, DC	Comparative/Cross-Sectional	Unspecified	Total Peak	Connecticut Ave	Wisconsin Ave (Comparison Road 1)
47	Dey, Ma, Aden	2011	Washington, DC	Comparative/Cross-Sectional	Unspecified	Off Peak	Connecticut Ave	Wisconsin Ave (Comparison Road 1)
48	Dey, Ma, Aden	2011	Washington, DC	Comparative/Cross-Sectional	Unspecified	All Collisions	Connecticut Ave	Massachusetts Ave (Comparison Road 2)
49	Dey, Ma, Aden	2011	Washington, DC	Comparative/Cross-Sectional	Unspecified	Total Peak	Connecticut Ave	Massachusetts Ave (Comparison Road 2)
50	Dey, Ma, Aden	2011	Washington, DC	Comparative/Cross-Sectional	Unspecified	Off Peak	Connecticut Ave	Massachusetts Ave (Comparison Road 2)
51	Knoblauch et al	1984	USA	Comparative/Cross-Sectional	Unspecified	All Collisions	Fifteenth Avenue-Phoenix, AZ	Urban multilane undivided site in 19 cities
52	Knoblauch et al	1984	USA	Comparative/Cross-Sectional	Unspecified	All Collisions	Grand River Avenue - Detroit, MI	Urban multilane undivided site in 19 cities
53	Knoblauch et al	1984	USA	Comparative/Cross-Sectional	Unspecified	All Collisions	Seventh Avenue - Phoenix, AZ	Urban multilane undivided site in 19 cities
54	Knoblauch et al	1984	USA	Comparative/Cross-Sectional	Unspecified	All Collisions	Washington Street - Phoenix, AZ	Urban multilane undivided site in 19 cities
55	Knoblauch et al	1984	USA	Comparative/Cross-Sectional	Unspecified	All Collisions	Memorial Drive - Atlanta, GA	Urban multilane undivided site in 19 cities
56	Knoblauch et al	1984	USA	Comparative/Cross-Sectional	Unspecified	All Collisions	Georgia Avenue - Montgomery County, MD	Urban multilane undivided site in 19 cities
57	Knoblauch et al	1984	USA	Comparative/Cross-Sectional	Unspecified	All Collisions	Route 202 - Manchester, Maine	Urban multilane undivided site in 19 cities
58	Knoblauch et al	1984	USA	Comparative/Cross-Sectional	Unspecified	All Collisions	Norman Bridge Road/Decatur Street - Montgomery, AL	Urban multilane undivided site in 19 cities
59	Knoblauch et al	1984	USA	Comparative/Cross-Sectional	Unspecified	All Collisions	Michigan Avenue - Dearborn, Michigan	Urban multilane undivided site in 19 cities
60	Knoblauch et al	1984	USA	Comparative/Cross-Sectional	Unspecified	All Collisions	Red River Street - Austin, Texas	Urban multilane undivided site in 19 cities
61	Knoblauch et al	1984	USA	Comparative/Cross-Sectional	Unspecified	All Collisions	Nicholasville Road	Urban multilane undivided site in 19 cities
62	Knoblauch et al	1984	USA	Comparative/Cross-Sectional	Unspecified	All Collisions	N.W Seventh Avenue - Miami, Florida	Urban multilane undivided site in 19 cities
63	Knoblauch et al	1984	USA	Comparative/Cross-Sectional	Unspecified	All Collisions	N.W Seventh Avenue - Miami, Florida	Urban multilane undivided site in 19 cities
64	Knoblauch et al	1984	USA	Comparative/Cross-Sectional	Unspecified	All Collisions	Fifteenth Avenue-Phoenix, AZ	Two-way left-turn sites - North Carolina
65	Knoblauch et al	1984	USA	Comparative/Cross-Sectional	Unspecified	All Collisions	Grand River Avenue - Detroit, MI	Two-way left-turn sites - North Carolina
66	Knoblauch et al	1984	USA	Comparative/Cross-Sectional	Unspecified	All Collisions	Seventh Avenue - Phoenix, AZ	Two-way left-turn sites - North Carolina
67	Knoblauch et al	1984	USA	Comparative/Cross-Sectional	Unspecified	All Collisions	Washington Street - Phoenix, AZ	Two-way left-turn sites - North Carolina
68	Knoblauch et al	1984	USA	Comparative/Cross-Sectional	Unspecified	All Collisions	Memorial Drive - Atlanta, GA	Two-way left-turn sites - North Carolina
69	Knoblauch et al	1984	USA	Comparative/Cross-Sectional	Unspecified	All Collisions	Georgia Avenue - Montgomery County, MD	Two-way left-turn sites - North Carolina
70	Knoblauch et al	1984	USA	Comparative/Cross-Sectional	Unspecified	All Collisions	Route 202 - Manchester, Maine	Two-way left-turn sites - North Carolina
71	Knoblauch et al	1984	USA	Comparative/Cross-Sectional	Unspecified	All Collisions	Norman Bridge Road/Decatur Street - Montgomery, AL	Two-way left-turn sites - North Carolina
72	Knoblauch et al	1984	USA	Comparative/Cross-Sectional	Unspecified	All Collisions	Michigan Avenue - Dearborn, Michigan	Two-way left-turn sites - North Carolina
73	Knoblauch et al	1984	USA	Comparative/Cross-Sectional	Unspecified	All Collisions	Red River Street - Austin, Texas	Two-way left-turn sites - North Carolina
74	Knoblauch et al	1984	USA	Comparative/Cross-Sectional	Unspecified	All Collisions	Nicholasville Road	Two-way left-turn sites - North Carolina
75	Knoblauch et al	1984	USA	Comparative/Cross-Sectional	Unspecified	All Collisions	N.W Seventh Avenue - Miami, Florida	Two-way left-turn sites - North Carolina
76	Knoblauch et al	1984	USA	Comparative/Cross-Sectional	Unspecified	All Collisions	N.W Seventh Avenue - Miami, Florida	Two-way left-turn sites - North Carolina
77	Knoblauch et al	1984	USA	Comparative/Cross-Sectional	Unspecified	All Collisions	Fifteenth Avenue-Phoenix, AZ	Two-way left-turn sites - Texas

Study No.	Author	Publication Year	Study Location	Study Type	Crash Severity	Crash Type	Study Location 1	Study Location 2
78	Knoblauch et al	1984	USA	Comparative/Cross-Sectional	Unspecified	All Collisions	Grand River Avenue - Detroit, MI	Two-way left-turn sites - Texas
79	Knoblauch et al	1984	USA	Comparative/Cross-Sectional	Unspecified	All Collisions	Seventh Avenue - Phoenix, AZ	Two-way left-turn sites - Texas
80	Knoblauch et al	1984	USA	Comparative/Cross-Sectional	Unspecified	All Collisions	Washington Street - Phoenix, AZ	Two-way left-turn sites - Texas
81	Knoblauch et al	1984	USA	Comparative/Cross-Sectional	Unspecified	All Collisions	Memorial Drive - Atlanta, GA	Two-way left-turn sites - Texas
82	Knoblauch et al	1984	USA	Comparative/Cross-Sectional	Unspecified	All Collisions	Georgia Avenue - Montgomery County, MD	Two-way left-turn sites - Texas
83	Knoblauch et al	1984	USA	Comparative/Cross-Sectional	Unspecified	All Collisions	Route 202 - Manchester, Maine	Two-way left-turn sites - Texas
84	Knoblauch et al	1984	USA	Comparative/Cross-Sectional	Unspecified	All Collisions	Norman Bridge Road/Decatur Street - Montgomery, AL	Two-way left-turn sites - Texas
85	Knoblauch et al	1984	USA	Comparative/Cross-Sectional	Unspecified	All Collisions	Michigan Avenue - Dearborn, Michigan	Two-way left-turn sites - Texas
86	Knoblauch et al	1984	USA	Comparative/Cross-Sectional	Unspecified	All Collisions	Red River Street - Austin, Texas	Two-way left-turn sites - Texas
87	Knoblauch et al	1984	USA	Comparative/Cross-Sectional	Unspecified	All Collisions	Nicholasville Road	Two-way left-turn sites - Texas
88	Knoblauch et al	1984	USA	Comparative/Cross-Sectional	Unspecified	All Collisions	N.W Seventh Avenue - Miami, Florida	Two-way left-turn sites - Texas
89	Knoblauch et al	1984	USA	Comparative/Cross-Sectional	Unspecified	All Collisions	N.W Seventh Avenue - Miami, Florida	Two-way left-turn sites - Texas
90	Knoblauch et al	1984	USA	Comparative/Cross-Sectional	Unspecified	All Collisions	Fifteenth Avenue-Phoenix, AZ	Two-way left-turn sites - Virginia
91	Knoblauch et al	1984	USA	Comparative/Cross-Sectional	Unspecified	All Collisions	Grand River Avenue - Detroit, MI	Two-way left-turn sites - Virginia
92	Knoblauch et al	1984	USA	Comparative/Cross-Sectional	Unspecified	All Collisions	Seventh Avenue - Phoenix, AZ	Two-way left-turn sites - Virginia
93	Knoblauch et al	1984	USA	Comparative/Cross-Sectional	Unspecified	All Collisions	Washington Street - Phoenix, AZ	Two-way left-turn sites - Virginia
94	Knoblauch et al	1984	USA	Comparative/Cross-Sectional	Unspecified	All Collisions	Memorial Drive - Atlanta, GA	Two-way left-turn sites - Virginia
95	Knoblauch et al	1984	USA	Comparative/Cross-Sectional	Unspecified	All Collisions	Georgia Avenue - Montgomery County, MD	Two-way left-turn sites - Virginia
96	Knoblauch et al	1984	USA	Comparative/Cross-Sectional	Unspecified	All Collisions	Route 202 - Manchester, Maine	Two-way left-turn sites - Virginia
97	Knoblauch et al	1984	USA	Comparative/Cross-Sectional	Unspecified	All Collisions	Norman Bridge Road/Decatur Street - Montgomery, AL	Two-way left-turn sites - Virginia
98	Knoblauch et al	1984	USA	Comparative/Cross-Sectional	Unspecified	All Collisions	Michigan Avenue - Dearborn, Michigan	Two-way left-turn sites - Virginia
99	Knoblauch et al	1984	USA	Comparative/Cross-Sectional	Unspecified	All Collisions	Red River Street - Austin, Texas	Two-way left-turn sites - Virginia
100	Knoblauch et al	1984	USA	Comparative/Cross-Sectional	Unspecified	All Collisions	Nicholasville Road	Two-way left-turn sites - Virginia
101	Knoblauch et al	1984	USA	Comparative/Cross-Sectional	Unspecified	All Collisions	N.W Seventh Avenue - Miami, Florida	Two-way left-turn sites - Virginia
102	Knoblauch et al	1984	USA	Comparative/Cross-Sectional	Unspecified	All Collisions	N.W Seventh Avenue - Miami, Florida	Two-way left-turn sites - Virginia
103	Lalani, Baird	1981	Phoenix	Simple Before-After	Injury	AM Peak	Seventh Ave from McDowell Rd to Northern Ave	Signalized Intersections
104	Lalani, Baird	1981	Phoenix	Simple Before-After	Injury	AM Peak	Seventh Ave from McDowell Rd to Northern Ave	Midblock and Nonsignalized Intersections
105	Lalani, Baird	1981	Phoenix	Simple Before-After	Injury	AM Peak	Seventh Ave from McDowell Rd to Northern Ave	All Locations
106	Lalani, Baird	1981	Phoenix	Simple Before-After	Unspecified	AM Peak	Seventh Ave from McDowell Rd to Northern Ave	Signalized Intersections
107	Lalani, Baird	1981	Phoenix	Simple Before-After	Unspecified	AM Peak	Seventh Ave from McDowell Rd to Northern Ave	Midblock and Nonsignalized Intersections
108	Lalani, Baird	1981	Phoenix	Simple Before-After	Unspecified	AM Peak	Seventh Ave from McDowell Rd to Northern Ave	All Locations
109	Lalani, Baird	1981	Phoenix	Simple Before-After	PDO	AM Peak	Seventh Ave from McDowell Rd to Northern Ave	Signalized Intersections
110	Lalani, Baird	1981	Phoenix	Simple Before-After	PDO	AM Peak	Seventh Ave from McDowell Rd to Northern Ave	Midblock and Nonsignalized Intersections
111	Lalani, Baird	1981	Phoenix	Simple Before-After	PDO	AM Peak	Seventh Ave from McDowell Rd to Northern Ave	All Locations
112	Lalani, Baird	1981	Phoenix	Simple Before-After	Injury	PM Peak	Seventh Ave from McDowell Rd to Northern Ave	Signalized Intersections
113	Lalani, Baird	1981	Phoenix	Simple Before-After	Injury	PM Peak	Seventh Ave from McDowell Rd to Northern Ave	Midblock and Nonsignalized Intersections
114	Lalani, Baird	1981	Phoenix	Simple Before-After	Injury	PM Peak	Seventh Ave from McDowell Rd to Northern Ave	All Locations
115	Lalani, Baird	1981	Phoenix	Simple Before-After	Unspecified	PM Peak	Seventh Ave from McDowell Rd to Northern Ave	Signalized Intersections
116	Lalani, Baird	1981	Phoenix	Simple Before-After	Unspecified	PM Peak	Seventh Ave from McDowell Rd to Northern Ave	Midblock and Nonsignalized Intersections
117	Lalani, Baird	1981	Phoenix	Simple Before-After	Unspecified	PM Peak	Seventh Ave from McDowell Rd to Northern Ave	All Locations

Study No.	Author	Publication Year	Study Location	Study Type	Crash Severity	Crash Type	Study Location 1	Study Location 2
118	Lalani, Baird	1981	Phoenix	Simple Before-After	PDO	PM Peak	Seventh Ave from McDowell Rd to Northern Ave	Signalized Intersections
119	Lalani, Baird	1981	Phoenix	Simple Before-After	PDO	PM Peak	Seventh Ave from McDowell Rd to Northern Ave	Midblock and Nonsignalized Intersections
120	Lalani, Baird	1981	Phoenix	Simple Before-After	PDO	PM Peak	Seventh Ave from McDowell Rd to Northern Ave	All Locations
121	Lalani, Baird	1981	Phoenix	Simple Before-After	Injury	Off Peak	Seventh Ave from McDowell Rd to Northern Ave	Signalized Intersections
122	Lalani, Baird	1981	Phoenix	Simple Before-After	Injury	Off Peak	Seventh Ave from McDowell Rd to Northern Ave	Midblock and Nonsignalized Intersections
123	Lalani, Baird	1981	Phoenix	Simple Before-After	Injury	Off Peak	Seventh Ave from McDowell Rd to Northern Ave	All Locations
124	Lalani, Baird	1981	Phoenix	Simple Before-After	Unspecified	Off Peak	Seventh Ave from McDowell Rd to Northern Ave	Signalized Intersections
125	Lalani, Baird	1981	Phoenix	Simple Before-After	Unspecified	Off Peak	Seventh Ave from McDowell Rd to Northern Ave	Midblock and Nonsignalized Intersections
126	Lalani, Baird	1981	Phoenix	Simple Before-After	Unspecified	Off Peak	Seventh Ave from McDowell Rd to Northern Ave	All Locations
127	Lalani, Baird	1981	Phoenix	Simple Before-After	PDO	Off Peak	Seventh Ave from McDowell Rd to Northern Ave	Signalized Intersections
128	Lalani, Baird	1981	Phoenix	Simple Before-After	PDO	Off Peak	Seventh Ave from McDowell Rd to Northern Ave	Midblock and Nonsignalized Intersections
129	Lalani, Baird	1981	Phoenix	Simple Before-After	PDO	Off Peak	Seventh Ave from McDowell Rd to Northern Ave	All Locations
130	Lalani, Baird	1981	Phoenix	Simple Before-After	Injury	All Collisions	Seventh Ave from McDowell Rd to Northern Ave	Signalized Intersections
131	Lalani, Baird	1981	Phoenix	Simple Before-After	Injury	All Collisions	Seventh Ave from McDowell Rd to Northern Ave	Midblock and Nonsignalized Intersections
132	Lalani, Baird	1981	Phoenix	Simple Before-After	Injury	All Collisions	Seventh Ave from McDowell Rd to Northern Ave	All Locations
133	Lalani, Baird	1981	Phoenix	Simple Before-After	Unspecified	All Collisions	Seventh Ave from McDowell Rd to Northern Ave	Signalized Intersections
134	Lalani, Baird	1981	Phoenix	Simple Before-After	Unspecified	All Collisions	Seventh Ave from McDowell Rd to Northern Ave	Midblock and Nonsignalized Intersections
135	Lalani, Baird	1981	Phoenix	Simple Before-After	Unspecified	All Collisions	Seventh Ave from McDowell Rd to Northern Ave	All Locations
136	Lalani, Baird	1981	Phoenix	Simple Before-After	PDO	All Collisions	Seventh Ave from McDowell Rd to Northern Ave	Signalized Intersections
137	Lalani, Baird	1981	Phoenix	Simple Before-After	PDO	All Collisions	Seventh Ave from McDowell Rd to Northern Ave	Midblock and Nonsignalized Intersections
138	Lalani, Baird	1981	Phoenix	Simple Before-After	PDO	All Collisions	Seventh Ave from McDowell Rd to Northern Ave	All Locations
139	Miller et al.	1979	USA	Simple Before-After	Unspecified	AM Peak	U.S 1/South Dixie Highway AM - Before/After	-
140	Miller et al.	1979	USA	Simple Before-After	Unspecified	PM Peak	U.S 1/South Dixie Highway PM - Before/After	-
141	Miller et al.	1979	USA	Simple Before-After	Unspecified	All Collisions	U.S 1/South Dixie Highway 24 Hr - Before/After	-
142	Miller et al.	1979	USA	Simple Before-After	Unspecified	AM Peak	Kalaniana'ole Highway Bus Only AM - Before/After	-
143	Miller et al.	1979	USA	Simple Before-After	Unspecified	AM Peak	Kalaniana'ole Highway Bus/Carpool AM - Before/After	-
144	Miller et al.	1979	USA	Simple Before-After	Unspecified	AM Peak	N.W 7th Avenue AM - Before/After	-
145	Miller et al.	1979	USA	Simple Before-After	Unspecified	PM Peak	N.W 7th Avenue PM - Before/After	-
146	Miller et al.	1979	USA	Simple Before-After	Unspecified	All Collisions	N.W 7th Avenue 24 Hr - Before/After	-
147	Miller et al.	1979	USA	Comparative/Cross-Sectional	Unspecified	AM Peak	U.S 1/South Dixie Highway AM - Cross-Sec	Dade County (Miami) County-Wide Accident Summaries
148	Miller et al.	1979	USA	Comparative/Cross-Sectional	Unspecified	PM Peak	U.S 1/South Dixie Highway PM - Cross-Sec	Dade County (Miami) County-Wide Accident Summaries
149	Miller et al.	1979	USA	Comparative/Cross-Sectional	Unspecified	All Collisions	U.S 1/South Dixie Highway 24 Hr - Cross-Sec	Dade County (Miami) County-Wide Accident Summaries
150	Miller et al.	1979	USA	Comparative/Cross-Sectional	Unspecified	AM Peak	Kalaniana'ole Highway Bus Only AM - Cross-Sec	FAP Route 60, Honolulu
151	Miller et al.	1979	USA	Comparative/Cross-Sectional	Unspecified	AM Peak	Kalaniana'ole Highway Bus/Carpool AM - Cross-Sec	FAP Route 60, Honolulu
152	Miller et al.	1979	USA	Comparative/Cross-Sectional	Unspecified	AM Peak	N.W 7th Avenue AM - Cross-Sec	Dade County (Miami) County-Wide Accident Summaries
153	Miller et al.	1979	USA	Comparative/Cross-Sectional	Unspecified	PM Peak	N.W 7th Avenue PM - Cross-Sec	Dade County (Miami) County-Wide Accident Summaries
154	Miller et al.	1979	USA	Comparative/Cross-Sectional	Unspecified	All Collisions	N.W 7th Avenue 24 Hr - Cross-Sec	Dade County (Miami) County-Wide Accident Summaries
155	Upchurch	1975	Memphis, TN	Characteristics Study	Unspecified	All Collisions	Union Ave from East Parkway to Manassas St	-

Table 19: Extracted Effect Estimates and Meta-Regression Variable Values

Study No.	Rate (R) or Freq. (F)	before study duration (months)	after study duration (months)	ratio of durations	original before frequency/ rate	original after frequency /rate	INJ	PDO	PEAK	OFF PEAK	CS	LT	STATIC	DYN	DELIN	CMF	ln(CMF)	v_i	s_i
1	F	12	12	1.00	37	30	0	0	1	0	0	0	0	1	0	0.811	-0.210	0.060	0.246
2	F	12	12	1.00	27	25	0	1	1	0	0	0	0	1	0	0.926	-0.077	0.077	0.278
3	F	12	12	1.00	10	5	1	0	1	0	0	0	0	1	0	0.500	-0.693	0.300	0.548
4	F	12	12	1.00	74	93	0	0	1	0	0	0	0	1	0	1.257	0.229	0.024	0.156
5	F	12	12	1.00	59	70	0	1	1	0	0	0	0	1	0	1.186	0.171	0.031	0.177
6	F	12	12	1.00	15	23	1	0	1	0	0	0	0	1	0	1.533	0.427	0.110	0.332
7	F	12	12	1.00	249	276	0	0	0	1	0	0	0	1	0	1.108	0.103	0.008	0.087
8	F	12	12	1.00	202	210	0	1	0	1	0	0	0	1	0	1.040	0.039	0.010	0.099
9	F	12	12	1.00	47	66	1	0	0	1	0	0	0	1	0	1.404	0.340	0.036	0.191
10	F	12	12	1.00	360	399	0	0	0	0	0	0	0	1	0	1.108	0.103	0.005	0.073
11	F	12	12	1.00	288	305	0	1	0	0	0	0	0	1	0	1.059	0.057	0.007	0.082
12	F	12	12	1.00	72	94	1	0	0	0	0	0	0	1	0	1.306	0.267	0.025	0.157
13	F	24	10	0.42	967	620	0	0	0	0	0	0	1	1	0	1.532	0.426	0.004	0.064
14	F	24	10	0.42	303	278	1	0	0	0	0	0	1	1	0	2.192	0.785	0.011	0.107
15	F	24	10	0.42	663	338	0	1	0	0	0	0	1	1	0	1.218	0.197	0.007	0.081
16	F	6	6	1.00	227	311	0	0	0	0	0	0	1	1	0	1.370	0.315	0.008	0.087
17	R	24	36	1.50	958	1479	0	0	0	0	0	0	1	1	0	1.029	0.029	0.001	0.037
18	R	24	12	0.50	300	234	1	0	0	0	0	0	1	1	0	1.560	0.445	0.011	0.105
19	R	36	36	N/A	0.968	1.000	0	0	1	0	1	1	1	0	0	1.033	0.032	2.033	1.426
20	R	36	36	N/A	0.968	2.035	0	0	1	0	1	1	1	0	0	2.102	0.743	1.524	1.235
21	R	36	36	N/A	0.97	1	0	0	1	0	1	1	1	0	0	1.031	0.030	2.031	1.425
22	R	36	36	N/A	0.97	2.035	0	0	1	0	1	1	1	0	0	2.098	0.741	1.522	1.234
23	R	36	36	N/A	6.506	3.576	0	0	1	0	1	1	1	0	0	0.550	-0.598	0.433	0.658

Study No.	Rate (R) or Freq. (F)	before study duration (months)	after study duration (months)	ratio of durations	original before frequency/rate	original after frequency/rate	INJ	PDO	PEAK	OFF PEAK	CS	LT	STATIC	DYN	DELIN	CMF	ln(CMF)	v _i	S _i
24	R	36	36	N/A	6.506	4.174	0	0	1	0	1	1	1	0	0	0.642	-0.444	0.393	0.627
25	R	36	36	N/A	4.694	3.576	0	0	1	0	1	1	1	0	0	0.762	-0.272	0.493	0.702
26	R	36	36	N/A	4.694	4.174	0	0	1	0	1	1	1	0	0	0.889	-0.117	0.453	0.673
27	R	36	36	N/A	2.453	3.782	0	0	1	0	1	1	1	0	0	1.541	0.433	0.672	0.820
28	R	36	36	N/A	2.453	6.855	0	0	1	0	1	1	1	0	0	2.794	1.028	0.553	0.744
29	R	36	36	N/A	3.022	3.782	0	0	1	0	1	1	1	0	0	1.252	0.224	0.595	0.772
30	R	36	36	N/A	3.022	6.855	0	0	1	0	1	1	1	0	0	2.269	0.819	0.477	0.691
31	R	36	36	N/A	11.606	9.868	0	0	1	0	1	1	1	0	0	0.850	-0.162	0.187	0.433
32	R	36	36	N/A	11.606	14.176	0	0	1	0	1	1	1	0	0	1.221	0.200	0.157	0.396
33	R	36	36	N/A	14.324	9.868	0	0	1	0	1	1	1	0	0	0.689	-0.373	0.171	0.414
34	R	36	36	N/A	14.324	14.176	0	0	1	0	1	1	1	0	0	0.990	-0.010	0.140	0.375
35	F	12	24	2	345	612	0	0	0	0	0	1	0	1	0	0.883	-0.124	0.003	0.055
36	F	12	24	2	31	64	0	0	1	0	0	1	0	1	0	1.028	0.028	0.032	0.178
37	F	12	24	2	84	166	0	0	1	0	0	1	0	1	0	0.984	-0.016	0.012	0.109
38	F	12	24	2	115	230	0	0	1	0	0	1	0	1	0	0.996	-0.004	0.009	0.093
39	F	12	24	2	230	382	0	0	0	1	0	1	0	1	0	0.827	-0.190	0.005	0.069
40	F	72	72	1.00	61	187	0	0	0	0	1	0	1	1	0	3.066	1.120	0.022	0.147
41	F	72	72	1.00	2	24	0	0	1	0	1	0	1	1	0	12.000	2.485	0.542	0.736
42	F	72	72	1.00	13	40	0	0	1	0	1	0	1	1	0	3.077	1.124	0.102	0.319
43	F	72	72	1.00	15	64	0	0	1	0	1	0	1	1	0	4.267	1.451	0.082	0.287
44	F	72	72	1.00	46	123	0	0	0	0	1	0	1	1	0	2.674	0.984	0.030	0.173
45	F	72	72	1.00	460	785	0	0	0	0	1	0	1	1	0	1.707	0.534	0.003	0.059
46	F	72	72	1.00	130	271	0	0	1	0	1	0	1	1	0	2.085	0.735	0.011	0.107
47	F	72	72	1.00	330	514	0	0	0	1	1	0	1	1	0	1.558	0.443	0.005	0.071
48	F	72	72	1.00	262	785	0	0	0	0	1	0	1	1	0	2.996	1.097	0.005	0.071

Study No.	Rate (R) or Freq. (F)	before study duration (months)	after study duration (months)	ratio of durations	original before frequency/rate	original after frequency/rate	INJ	PDO	PEAK	OFF PEAK	CS	LT	STATIC	DYN	DELIN	CMF	ln(CMF)	v _i	s _i
49	F	72	72	1.00	65	271	0	0	1	0	1	0	1	1	0	4.169	1.428	0.019	0.138
50	F	72	72	1.00	197	514	0	0	0	1	1	0	1	1	0	2.609	0.959	0.007	0.084
51	R	N/A	N/A	N/A	8.6	9.7	0	0	0	0	1	0	1	0	0	1.128	0.120	0.219	0.468
52	R	N/A	30	N/A	8.6	9	0	0	0	0	1	1	1	0	0	1.047	0.045	0.227	0.477
53	R	N/A	12	N/A	8.6	6.8	0	0	0	0	1	1	1	0	0	0.791	-0.235	0.263	0.513
54	R	N/A	N/A	N/A	8.6	4.4	0	0	0	0	1	1	1	0	1	0.512	-0.670	0.344	0.586
55	R	N/A	24	N/A	8.6	15.7	0	0	0	0	1	0	0	1	0	1.826	0.602	0.180	0.424
56	R	N/A	24	N/A	8.6	5.4	0	0	0	0	1	1	1	1	0	0.628	-0.465	0.301	0.549
57	R	N/A	9	N/A	8.6	3.2	0	0	0	0	1	0	1	1	0	0.372	-0.989	0.429	0.655
58	R	N/A	36	N/A	8.6	13.1	0	0	0	0	1	1	1	1	0	1.523	0.421	0.193	0.439
59	R	N/A	30	N/A	8.6	13.2	0	0	0	0	1	1	0	1	0	1.535	0.428	0.192	0.438
60	R	N/A	12	N/A	8.6	10.8	0	0	0	0	1	1	0	1	0	1.256	0.228	0.209	0.457
61	R	N/A	12	N/A	8.6	12	0	0	0	0	1	0	0	1	0	1.395	0.333	0.200	0.447
62	R	N/A	39	N/A	8.6	23.6	0	0	0	0	1	1	0	1	0	2.744	1.009	0.159	0.398
63	R	N/A	N/A	N/A	8.6	5.8	0	0	0	0	1	1	0	1	0	0.674	-0.394	0.289	0.537
64	R	N/A	N/A	N/A	7.4	9.7	0	0	0	0	1	0	1	0	0	1.311	0.271	0.238	0.488
65	R	N/A	30	N/A	7.4	9	0	0	0	0	1	1	1	0	0	1.216	0.196	0.246	0.496
66	R	N/A	12	N/A	7.4	6.8	0	0	0	0	1	1	1	0	0	0.919	-0.085	0.282	0.531
67	R	N/A	N/A	N/A	7.4	4.4	0	0	0	0	1	1	1	0	1	0.595	-0.520	0.362	0.602
68	R	N/A	24	N/A	7.4	15.7	0	0	0	0	1	0	0	1	0	2.122	0.752	0.199	0.446
69	R	N/A	24	N/A	7.4	5.4	0	0	0	0	1	1	1	1	0	0.730	-0.315	0.320	0.566
70	R	N/A	9	N/A	7.4	3.2	0	0	0	0	1	0	1	1	0	0.432	-0.838	0.448	0.669
71	R	N/A	36	N/A	7.4	13.1	0	0	0	0	1	1	1	1	0	1.770	0.571	0.211	0.460
72	R	N/A	30	N/A	7.4	13.2	0	0	0	0	1	1	0	1	0	1.784	0.579	0.211	0.459
73	R	N/A	12	N/A	7.4	10.8	0	0	0	0	1	1	0	1	0	1.459	0.378	0.228	0.477

Study No.	Rate (R) or Freq. (F)	before study duration (months)	after study duration (months)	ratio of durations	original before frequency/rate	original after frequency/rate	INJ	PDO	PEAK	OFF PEAK	CS	LT	STATIC	DYN	DELIN	CMF	ln(CMF)	v_i	S_i
74	R	N/A	12	N/A	7.4	12	0	0	0	0	1	0	0	1	0	1.622	0.483	0.218	0.467
75	R	N/A	39	N/A	7.4	23.6	0	0	0	0	1	1	0	1	0	3.189	1.160	0.178	0.421
76	R	N/A	N/A	N/A	7.4	5.8	0	0	0	0	1	1	0	1	0	0.784	-0.244	0.308	0.555
77	R	N/A	N/A	N/A	11.4	9.7	0	0	0	0	1	0	1	0	0	0.851	-0.161	0.191	0.437
78	R	N/A	30	N/A	11.4	9	0	0	0	0	1	1	1	0	0	0.789	-0.236	0.199	0.446
79	R	N/A	12	N/A	11.4	6.8	0	0	0	0	1	1	1	0	0	0.596	-0.517	0.235	0.485
80	R	N/A	N/A	N/A	11.4	4.4	0	0	0	0	1	1	1	0	1	0.386	-0.952	0.315	0.561
81	R	N/A	24	N/A	11.4	15.7	0	0	0	0	1	0	0	1	0	1.377	0.320	0.151	0.389
82	R	N/A	24	N/A	11.4	5.4	0	0	0	0	1	1	1	1	0	0.474	-0.747	0.273	0.522
83	R	N/A	9	N/A	11.4	3.2	0	0	0	0	1	0	1	1	0	0.281	-1.270	0.400	0.633
84	R	N/A	36	N/A	11.4	13.1	0	0	0	0	1	1	1	1	0	1.149	0.139	0.164	0.405
85	R	N/A	30	N/A	11.4	13.2	0	0	0	0	1	1	0	1	0	1.158	0.147	0.163	0.404
86	R	N/A	12	N/A	11.4	10.8	0	0	0	0	1	1	0	1	0	0.947	-0.054	0.180	0.425
87	R	N/A	12	N/A	11.4	12	0	0	0	0	1	0	0	1	0	1.053	0.051	0.171	0.414
88	R	N/A	39	N/A	11.4	23.6	0	0	0	0	1	1	0	1	0	2.070	0.728	0.130	0.361
89	R	N/A	N/A	N/A	11.4	5.8	0	0	0	0	1	1	0	1	0	0.509	-0.676	0.260	0.510
90	R	N/A	N/A	N/A	6.3	9.7	0	0	0	0	1	0	1	0	0	1.540	0.432	0.262	0.512
91	R	N/A	30	N/A	6.3	9	0	0	0	0	1	1	1	0	0	1.429	0.357	0.270	0.519
92	R	N/A	12	N/A	6.3	6.8	0	0	0	0	1	1	1	0	0	1.079	0.076	0.306	0.553
93	R	N/A	N/A	N/A	6.3	4.4	0	0	0	0	1	1	1	0	1	0.698	-0.359	0.386	0.621
94	R	N/A	24	N/A	6.3	15.7	0	0	0	0	1	0	0	1	0	2.492	0.913	0.222	0.472
95	R	N/A	24	N/A	6.3	5.4	0	0	0	0	1	1	1	1	0	0.857	-0.154	0.344	0.586
96	R	N/A	9	N/A	6.3	3.2	0	0	0	0	1	0	1	1	0	0.508	-0.677	0.471	0.686
97	R	N/A	36	N/A	6.3	13.1	0	0	0	0	1	1	1	1	0	2.079	0.732	0.235	0.485
98	R	N/A	30	N/A	6.3	13.2	0	0	0	0	1	1	0	1	0	2.095	0.740	0.234	0.484

Study No.	Rate (R) or Freq. (F)	before study duration (months)	after study duration (months)	ratio of durations	original before frequency/rate	original after frequency/rate	INJ	PDO	PEAK	OFF PEAK	CS	LT	STATIC	DYN	DELIN	CMF	ln(CMF)	v_i	S_i
99	R	N/A	12	N/A	6.3	10.8	0	0	0	0	1	1	0	1	0	1.714	0.539	0.251	0.501
100	R	N/A	12	N/A	6.3	12	0	0	0	0	1	0	0	1	0	1.905	0.644	0.242	0.492
101	R	N/A	39	N/A	6.3	23.6	0	0	0	0	1	1	0	1	0	3.746	1.321	0.201	0.448
102	R	N/A	N/A	N/A	6.3	5.8	0	0	0	0	1	1	0	1	0	0.921	-0.083	0.331	0.575
103	F	9	9	1.00	3	2	1	0	1	0	0	1	1	0	0	0.667	-0.405	0.833	0.913
104	F	9	9	1.00	5	13	1	0	1	0	0	1	1	0	0	2.600	0.956	0.277	0.526
105	F	9	9	1.00	5	15	1	0	1	0	0	1	1	0	0	3.000	1.099	0.267	0.516
106	F	9	9	1.00	11	7	0	0	1	0	0	1	1	0	0	0.636	-0.452	0.234	0.483
107	F	9	9	1.00	14	31	0	0	1	0	0	1	1	0	0	2.214	0.795	0.104	0.322
108	F	9	9	1.00	25	38	0	0	1	0	0	1	1	0	0	1.520	0.419	0.066	0.258
109	F	9	9	1.00	8	5	0	1	1	0	0	1	1	0	0	0.625	-0.470	0.325	0.570
110	F	9	9	1.00	9	18	0	1	1	0	0	1	1	0	0	2.000	0.693	0.167	0.408
111	F	9	9	1.00	20	23	0	1	1	0	0	1	1	0	0	1.150	0.140	0.093	0.306
112	F	9	9	1.00	6	3	1	0	1	0	0	1	1	0	0	0.500	-0.693	0.500	0.707
113	F	9	9	1.00	9	24	1	0	1	0	0	1	1	0	0	2.667	0.981	0.153	0.391
114	F	9	9	1.00	15	27	1	0	1	0	0	1	1	0	0	1.800	0.588	0.104	0.322
115	F	9	9	1.00	16	10	0	0	1	0	0	1	1	0	0	0.625	-0.470	0.163	0.403
116	F	9	9	1.00	23	79	0	0	1	0	0	1	1	0	0	3.435	1.234	0.056	0.237
117	F	9	9	1.00	39	89	0	0	1	0	0	1	1	0	0	2.282	0.825	0.037	0.192
118	F	9	9	1.00	10	7	0	1	1	0	0	1	1	0	0	0.700	-0.357	0.243	0.493
119	F	9	9	1.00	14	55	0	1	1	0	0	1	1	0	0	3.929	1.368	0.090	0.299
120	F	9	9	1.00	24	62	0	1	1	0	0	1	1	0	0	2.583	0.949	0.058	0.240
121	F	9	9	1.00	27	29	1	0	0	1	0	1	1	0	0	1.074	0.071	0.072	0.267
122	F	9	9	1.00	34	30	1	0	0	1	0	1	1	0	0	0.882	-0.125	0.063	0.250
123	F	9	9	1.00	64	59	1	0	0	1	0	1	1	0	0	0.922	-0.081	0.033	0.180

Study No.	Rate (R) or Freq. (F)	before study duration (months)	after study duration (months)	ratio of durations	original before frequency/rate	original after frequency/rate	INJ	PDO	PEAK	OFF PEAK	CS	LT	STATIC	DYN	DELIN	CMF	ln(CMF)	v _i	s _i
124	F	9	9	1.00	63	72	0	0	0	1	0	1	1	0	0	1.143	0.134	0.030	0.173
125	F	9	9	1.00	102	74	0	0	0	1	0	1	1	0	0	0.725	-0.321	0.023	0.153
126	F	9	9	1.00	165	146	0	0	0	1	0	1	1	0	0	0.885	-0.122	0.013	0.114
127	F	9	9	1.00	36	43	0	1	0	1	0	1	1	0	0	1.194	0.178	0.051	0.226
128	F	9	9	1.00	68	44	0	1	0	1	0	1	1	0	0	0.647	-0.435	0.037	0.193
129	F	9	9	1.00	101	87	0	1	0	1	0	1	1	0	0	0.861	-0.149	0.021	0.146
130	F	9	9	1.00	36	34	1	0	0	0	0	1	1	0	0	0.944	-0.057	0.057	0.239
131	F	9	9	1.00	48	67	1	0	0	0	0	1	1	0	0	1.396	0.333	0.036	0.189
132	F	9	9	1.00	84	101	1	0	0	0	0	1	1	0	0	1.202	0.184	0.022	0.148
133	F	9	9	1.00	90	89	0	0	0	0	0	1	1	0	0	0.989	-0.011	0.022	0.149
134	F	9	9	1.00	139	184	0	0	0	0	0	1	1	0	0	1.324	0.280	0.013	0.112
135	F	9	9	1.00	229	273	0	0	0	0	0	1	1	0	0	1.192	0.176	0.008	0.090
136	F	9	9	1.00	54	55	0	1	0	0	0	1	1	0	0	1.019	0.018	0.037	0.192
137	F	9	9	1.00	91	117	0	1	0	0	0	1	1	0	0	1.286	0.251	0.020	0.140
138	F	9	9	1.00	145	172	0	1	0	0	0	1	1	0	0	1.186	0.171	0.013	0.113
139	R	11	11	N/A	5.2	8.8	0	0	1	0	0	1	1	1	1	1.692	0.526	0.306	0.553
140	R	11	11	N/A	9.2	15.4	0	0	1	0	0	1	1	1	1	1.674	0.515	0.174	0.417
141	R	11	11	N/A	7.2	12.1	0	0	0	0	0	1	1	1	1	1.681	0.519	0.222	0.471
142	R	20	25	N/A	1.7	1.7	0	0	1	0	0	1	1	0	1	1.000	0.000	1.176	1.085
143	R	20	16	N/A	1.7	1.3	0	0	1	0	0	1	1	0	1	0.765	-0.268	1.357	1.165
144	R	5	14	N/A	10.8	9.5	0	0	1	0	0	1	1	1	0	0.880	-0.128	0.198	0.445
145	R	5	14	N/A	9.9	14.8	0	0	1	0	0	1	1	1	0	1.495	0.402	0.169	0.411
146	R	5	14	N/A	10.3	12.4	0	0	0	0	0	1	1	1	0	1.204	0.186	0.178	0.422
147	R	N/A	11	N/A	7.5	8.8	0	0	1	0	1	1	1	1	1	1.173	0.160	0.247	0.497
148	R	N/A	11	N/A	7.5	15.4	0	0	1	0	1	1	1	1	1	2.053	0.719	0.198	0.445

Study No.	Rate (R) or Freq. (F)	before study duration (months)	after study duration (months)	ratio of durations	original before frequency/rate	original after frequency /rate	INJ	PDO	PEAK	OFF PEAK	CS	LT	STATIC	DYN	DELIN	CMF	ln(CMF)	v_i	s_i
149	R	N/A	11	N/A	7.5	12.1	0	0	0	0	1	1	1	1	1	1.613	0.478	0.216	0.465
150	R	N/A	25	N/A	2.3	1.7	0	0	1	0	1	1	1	0	1	0.739	-0.302	1.023	1.011
151	R	N/A	16	N/A	2.2	1.3	0	0	1	0	1	1	1	0	1	0.591	-0.526	1.224	1.106
152	R	N/A	14	N/A	7.5	9.5	0	0	1	0	1	1	1	1	0	1.267	0.236	0.239	0.488
153	R	N/A	14	N/A	7.5	14.8	0	0	1	0	1	1	1	1	0	1.973	0.680	0.201	0.448
154	R	N/A	14	N/A	7.5	12.4	0	0	0	0	1	1	1	1	0	1.653	0.503	0.214	0.463
155	F	12	12	1.00	680	817	0	0	0	0	0	1	1	1	0	1.201	0.184	0.003	0.052

Notes:

CMF is the raw odds ratio of the after-to-before collision frequency/rate, adjusted for unequal study periods using the ratio-of-durations

Ln(CMF) is the log-transformed odds ratio

v_i is the within study variance for each study based on the log-odds ratio methodology

s_i is the standard error for each study based on v_i

APPENDIX C: META-REGRESSION ANALYSIS INCLUDING YEAR

Table 20: Meta-Regression Model Descriptive Statistics Including Publication Year

	Model A - All Effect Estimates				Model B - Before-After Estimates				Model C - Comparative Estimates			
Variables	MIN	MAX	MEAN	SD	MIN	MAX	MEAN	SD	MIN	MAX	MEAN	SD
ln(o _i)	-1.270	2.485	0.212	0.561	-0.693	1.368	0.180	0.437	-1.270	2.485	0.237	0.641
YEAR	1966	2011	1986.774	11.394	1966	1996	1980.721	6.123	1979	2011	1991.506	12.287
INJ	0	1	0.117	-	0	1	0.265	-	-	-	-	-
PDO	0	1	0.110	-	0	1	0.250	-	-	-	-	-
PEAK	0	1	0.390	-	0	1	0.485	-	0	1	0.310	-
OFFPEAK	0	1	0.097	-	0	1	0.191	-	0	1	0.034	-
CS	0	1	0.565	-	-	-	-	-	-	-	-	-
LT	0	1	0.708	-	0	1	0.735	-	0	1	0.690	-
STATIC	0	1	0.734	-	0	1	0.750	-	0	1	0.724	-
DYN	0	1	0.532	-	0	1	0.441	-	0	1	0.609	-
DELIN	0	1	0.091	-	0	1	0.074	-	0	1	0.103	-

Table 21: Model A Results

Model A: All Effect Estimates Included – With Publication Year							
I^2 : 63.04%		R^2 : 53.76%			n = 155		
Variable	Coefficient	Standard Error	z-value	p-value	95% CI lower bound	95% CI upper bound	VIF
α	-26.3295	10.9627	-2.4017	0.0163	-47.8161	-4.843	
<i>YEAR</i>	0.0132	0.0055	2.3861	0.017	0.0024	0.0241	4.5965
<i>INJ</i>	0.1602	0.1048	1.5281	0.1265	-0.0453	0.3656	1.3164
<i>PDO</i>	0.0387	0.099	0.3905	0.6961	-0.1553	0.2327	1.3603
<i>PEAK</i>	0.2045	0.0774	2.6425	0.0082	0.0528	0.3562	1.2412
<i>OFFPEAK</i>	-0.1162	0.0898	-1.2942	0.1956	-0.2921	0.0598	1.2146
<i>CS</i>	0.06	0.1051	0.5712	0.5679	-0.146	0.2661	2.5627
<i>LT</i>	0.0114	0.1114	0.1026	0.9183	-0.2069	0.2297	3.0032
<i>STATIC</i>	0.1292	0.1182	1.0932	0.2743	-0.1024	0.3608	2.913
<i>DYN</i>	0.2471	0.1049	2.3567	0.0184	0.0416	0.4526	2.6213
<i>DELIN</i>	-0.208	0.1879	-1.107	0.2683	-0.5763	0.1603	1.1759

Table 22: Model B Results

Model B: Simple Before-After Studies Only - With Publication Year							
I^2 : 62.33%		R^2 : 27.25%			n = 68		
Variable	Coefficient	Standard Error	z-value	p-value	95% CI lower bound	95% CI upper bound	VIF
α	21.5209	69.4018	0.3101	0.7565	-114.504	157.5459	
<i>YEAR</i>	-0.0108	0.035	-0.3087	0.7575	-0.0794	0.0578	70.606
<i>INJ</i>	0.1424	0.0934	1.5248	0.1273	-0.0406	0.3255	1.2572
<i>PDO</i>	-0.009	0.088	-0.1025	0.9183	-0.1815	0.1634	1.3032
<i>PEAK</i>	0.2339	0.0898	2.6044	0.0092	0.0579	0.41	1.4384
<i>OFFPEAK</i>	-0.1751	0.0926	-1.8905	0.0587	-0.3567	0.0064	1.3892
<i>LT</i>	-0.3721	0.564	-0.6598	0.5094	-1.4777	0.7334	65.45
<i>STATIC</i>	0.4195	0.5199	0.8068	0.4198	-0.5995	1.4384	52.9609
<i>DYN</i>	-0.0575	0.1448	-0.3971	0.6913	-0.3414	0.2264	4.5861
<i>DELIN</i>	0.1513	0.2958	0.5113	0.6091	-0.4285	0.731	1.1323

Table 23: Model C Results

Model C: Cross-Sectional Studies Only – With Publication Year							
I^2 : 52.17%		R^2 : 36.36%			n = 87		
Variable	Coefficient	Standard Error	z-value	p-value	95% CI lower bound	95% CI upper bound	VIF
α	-27.8522	15.0137	-1.8551	0.0636	-57.2785	1.5741	
<i>YEAR</i>	0.0141	0.0075	1.8641	0.0623	-0.0007	0.0288	2.8171
<i>PEAK</i>	0.2389	0.1819	1.313	0.1892	-0.1177	0.5954	1.8553
<i>OFFPEAK</i>	-0.0075	0.2513	-0.0298	0.9762	-0.5	0.4851	1.5722
<i>LT</i>	-0.216	0.1526	-1.4156	0.1569	-0.515	0.0831	1.6897
<i>STATIC</i>	-0.1597	0.1732	-0.9222	0.3564	-0.4992	0.1797	1.7365
<i>DYN</i>	0.5454	0.1611	3.3853	0.0007	0.2296	0.8612	1.4754
<i>DELIN</i>	-0.0734	0.2618	-0.2805	0.7791	-0.5866	0.4397	1.2844

Fin