# The Safety of Separated Bike Lanes: a Review of the Evidence

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**Abstract**
This study reviews the evidence on the safety of urban separated bike lanes (SBLs). Several studies comparing SBLs to streets without any designated bicycle facilities were identified. After reviewing the studies, it was determined that only four used a valid methodology and had sufficient and relevant data. All of these found a negative effect of SBLs on bicyclist safety. Based on these studies, the best estimate is that one-way urban SBLs increase bicyclist injury crashes by 24% at intersections and by a lesser amount overall. Further research on SBLs is recommended, either by interviewing injured bicyclists or by studying road user conflicts through computer-analysis of videos, rather than relying on official crash reports.

**Keywords**
separated bike lane; cycle track; bicycle safety; cycle path; bicycle; cycling

**Declarations of interest**: none

## 1.0 Introduction

In the past decade, bike paths along urban roads have become increasingly common in the United States and Canada as new design standards have replaced earlier guidelines discouraging their use. These bicycle facilities are distinguished by their incorporation of a physical separator, between the bicycle lane and the general travel lanes. Of necessity, the physical separation is discontinued everywhere motor vehicles are permitted to cross the bicycle facility (chiefly, intersections and driveways). Following the U.S. Department of Transportation-Federal Highway Administration practice, this paper will use the term separated bike lane (SBL) for these facilities. They are also called sidepaths, cycle tracks, or protected bike lanes.

There are a wide variety of designs that are considered “separated bike lanes” in this study, which considers only studies in urban locations. Some SBLs are at the roadway level, some at sidewalk level, and others are separated from the roadway by a low mountable curb. Some are adjacent to on-street parking. Some are one way and some are two-way (bidirectional).

Under earlier U.S. design guidance, bike paths adjacent to roads were discouraged except where there were few intersections (AASHTO, 2012). The prior guidance was based in part on European research showing that there is an increased crash risk of this type of infrastructure, particularly by increasing the number of car-bike collisions at intersections (Elvik et al., 2009). In the past decade, several studies have been published claiming that SBLs, on the contrary, improve bicyclist safety. After the first of these studies with positive safety findings had been published, an OECD review speculated that North American SBLs might be safer than European ones (International Transport Forum, 2013). Design guidelines encouraging the use of SBLs in urban areas have been promulgated, citing the conclusions of the newer studies (FHWA, 2015). Hundreds of urban SBLs have been built in the past decade in North America (People for Bikes, 2018).

## 2.0 Scope of the Review

The aim of the current study is to provide detailed examinations of the evidence on the impact of urban SBLs on bicyclist crashes and injuries. The universe of studies came from those cited in two recent literature reviews or in design guidelines for SBLs, supplemented by searches in journal indexes and Google Scholar.

### 2.1 Previous Literature Reviews

Thomas and DeRobertis conducted a literature review of the safety of urban SBLs including 23 sources published between 1987 and 2012, all from Northern Europe with the exception of one from Montreal (Thomas and DeRobertis, 2013). Only 10 of the 23 sources compared SBLs to roads without any designated bicycle facilities. These ten sources contained two pairs that refer to the same study so that there were only eight distinct studies. One study was a meta-analysis of earlier studies that are not readily available (Gårder et al., 1994). Another was clearly deficient in that it controlled for the length of the facilities but not for the amount of bicycle or car travel (Lüder, 1987). The remaining studies are included in the current review.

A Cochrane systematic review of the effects of all types of bicycle infrastructure on bicyclist injuries has been published (Mulvaney et al., 2015). The authors found 343 seemingly relevant full-text references of which 41 could be ascertained to be examining the effect of bicycle infrastructure on bicyclist injuries and used a “study design of interest.” Allowable study designs were randomized controlled trial, controlled before-after studies (CBA), and interrupted time series studies (ITS). The 41 references were based on 21 unique studies, of which 20 used CBA and one used ITS. Only one of the 21 studies compared SBLs to roads without bicycle infrastructure (Agerholm et al., 2008). The Cochrane review concluded that this study found “no changes” in police-reported injury crashes. The Cochrane review excluded the Trafitec Copenhagen study because it did not include “outcomes of interest.” Given that this study reports bicyclist crashes and bicyclist injuries, it seems that this finding was in error. Another study reviewed here (Welleman and Dijkstra, 1988) was excluded from the Cochrane review because it was “not cycling infrastructure,” but this judgment is erroneous, since the primary study specifically compares SBLs to roads without designated bicycling infrastructure.

## 2.2 Data and Methodological Issues

Randomized controlled trial (RCT) is considered the best evaluation method in epidemiology. RCT has been used to study differences in bicyclist behavior, such as use of running lights and bright clothing (Lahrmann et al., 2018). However, it is rarely used to evaluate road design “because highway agencies are generally reluctant to use random selection in assigning treatments” (AASHTO, 2010). Therefore road safety researchers rely on second-best methods described below.

*Before-After Method*

This method compares crash and/or injury outcomes at the same location before and after improvements to a site. There are a number of potential confounding factors that must be accounted for in order to produce a valid study:

- Local Changes. Because this study design inevitably compares outcomes over time, there may have been other changes at the location, including physical changes that are not part of the studied treatment and changes to traffic volume that may or may not be due to the treatment.
- Long-term Trends. There may be area-wide changes in road safety (e.g. changes to laws, enforcement, or demographics) or in crash reporting that affect crash outcomes in the entire jurisdiction.
- Regression to the Mean (RTM). Crash events at a single location (e.g, one intersection) are rare. By chance the number of crashes in one time period can be higher than average. The best guess is that in the subsequent year the number of crashes will decline toward the long-term mean. If sites are selected for intervention based on high crash numbers, the evaluation will be biased toward finding a safety effect of the intervention unless RTM is accounted for in the methodology.

The Empirical Bayes (EB) method was developed to account for all three of these types of possible confounding factors. The safety effect of a treatment is estimated by comparing the expected number of crashes that would have occurred in the after period without the treatment to the actual number of reported crashes in the after period. The effects of local changes, long-term trends, and regression to mean are explicitly accounted for through the use of a statistical model to produce the estimate of expected crashes based on traffic volume, site geometry, and other relevant factors.

Other before-and-after study methods may not account for all three confounding factors. Some studies use a comparison or control group, consisting of non-treated sites that are otherwise comparable to the treated ones. Crash and traffic volume data must be collected for the same time period for both the treated sites and the comparison group.  The Highway Safety Manual recommends minimums of 10 to 20 treated sites, 10 to 20 comparison sites, 650 crashes in the comparison group, and three to five years of both before and after crash data (AASHTO, 2010).

*Cross-Sectional Method*

Cross-sectional studies are attractive because they do not require “before” data. Instead they rely on a comparison with control sites. Some studies attempt to find control sites that are like the treated sites in all significant ways. However, “In practice, it is difficult to collect data for enough locations that are alike in all factors affecting crash risk. Hence, cross-sectional analyses are often accomplished through multiple variable regression models. In these models an attempt is made to account for all variables that affect safety” (Banks et al., 2014). The Highway Safety Manual recommends the use of a statistical model and a minimum of 10 to 20 treatment sites, 10 to 20 control sites, and three to five years of crash data. However, even a modeling approach may not produce good results: effects “estimated from cross-section studies could be inaccurate for a number of reasons, including inappropriate functional form, omitted variable bias, or correlation of variables” (Banks et al., 2014). Also, cross-sectional methods, lacking before data, cannot account for RTM.

Results from cross-sectional models vary considerably across different studies, leading some to question the possibility of controlling for all confounding factors (Hauer, 2010). Cross-sectional studies may still provide some value if they include a large enough sample and if they control for potentially confounding factors. However, cross-sectional studies based on a single paired control for each treatment are clearly insufficient.

*Case-Crossover Method*

The case-crossover method is a variant of the case-control method where those experiencing a negative outcome (e.g., traffic crash) are used as their own controls by comparing them at the time of the crash and at another time period. By using the subject as his or her own control, the case-crossover method eliminates confounding factors related to the characteristics of the individual. The method has been used to study the effect of mobile phone use on traffic crashes by interviewing drivers involved in a crash and comparing the likelihood of phone use around the time of the crash to the likelihood of phone use during three control periods (1, 3, and 7 days before the crash) (Gariazzo et al., 2018). It has been used in two of the studies of SBLs discussed below.

A study comparing different evaluation methods for road safety improvements found that in general approaches that are not designed to control for all three sources of bias produce significantly different results from superior methods such as Empirical Bayes. “Second-best studies, relying on the traditional designs for before-and-after studies, can be defended if it can be shown that neither regression-to-the-mean, long-term trends nor changes in traffic volume are likely to confound study results” (Elvik, 2012). RTM is unlikely to be a problem in SBL evaluation, since in most cases SBLs are installed based on a policy decision rather than based on an analysis of bicyclist crash locations; the one study of SBLs that checked for RTM found no evidence of it (Jensen, 2006). However, it is important to account for the other two factors in before-and-after studies of SBLs.

## 3.0 Review of Studies

### 3.1 Bach et al. Traffic safety effects of cycle paths in Danish cities.

This study examined the changes in police-reported crashes before and after the construction of 64 km of SBLs on 105 road segments in Copenhagen and other Danish cities between 1978 and 1981 (Bach et al., 1985). The study controlled for general trends in bicycling over time through the use of a control group of roads without SBLs in the same cities, with data from both before and after implementation of the projects. The study did not control for local changes inbicycling associated with construction of the projects since the authors were unable to obtain before and after bicycle counts at the 105 specific sites.

The study found that bicyclist crashes increased 35% and bicyclist injuries increased 30% after the construction of SBLs compared to what would be expected given long-term trends; these changes were statistically significant based on chi-squared tests. For moped riders, crashes increased 45% and slight injuries increased 55% (the change in serious injuries was not statistically significant). The number of pedestrian crashes increased 35%, due completely to an increase in pedestrian crashes involving bicyclists and mopeds.

The study examined changes in specific crash types. Between intersections, the effect of SBLs was to reduce the number of overtaking and head-on bicycle-motor vehicle (BMV) crashes. Completely offsetting this reduction was an increase in bicycle-pedestrian crashes and crashes “where motor vehicles turn into driveways etc. in front of bicycles going straight ahead,” so that overall there was no net change in the number of bicyclist crashes between intersections. At intersections, there were no crash types that decreased and there was a “surprising increase” in crashes involving motor vehicles turning left in front of oncoming bicyclists, bicycle-pedestrian crashes, and single bicycle or single moped crashes. Both bicyclist crashes and bicyclist injuries at intersections increased 50%.

The study was robust in that it included a large number of both “treatment” and control locations. Its major deficiency was the lack of a control for changes in bicyclist volumes due to project construction. The authors estimate that this factor was not large: they write that “no ‘traffic leap’ [increase] has occurred where cycle tracks have been constructed,” with the exception of “a few high flow sections in Copenhagen [where] a leap [of] about 20-25% in bicycle flows have occurred.” Even if one does not accept the authors’ judgment that there was no significant increase in bicyclist volumes due to the projects, the increase in pedestrian crashes cannot be dismissed, since the projects did not improve pedestrian facilities. Further, the pattern of changes in crash types shows that SBLs reduced overtaking crashes between intersections but increased crashes involving turning motor vehicles and those not involving motor vehicles.

### 3.2 Welleman and Dijkstra, Safety Aspects of Urban Cycle Tracks

This study by the Institute for Road Safety Research (SWOV) analyzed 5,763 injury crashes recorded by police between 1973 and 1977 in the urbanized portions of 14 Dutch municipalities with a population of 50,000 or greater (Welleman and Dijkstra, 1988). The key results are summarized in English in a separate report (Wegman and Dijkstra, 1988).

The study used a cross-sectional comparison including all roads in the municipalities, although without a statistical model accounting for potential confounding factors. In order to account for exposure, the share of cyclists and moped users from average 24-hour weekday road traffic counts was compared to the share of cyclist and moped-rider police-reported injuries, according to location (intersection or road segment) and according to facility type (bike lane, SBL, or mixed traffic). The traffic counts were multiplied by road length for the segments and by the number of intersections for the intersections. The method was able to produce separate estimates of the change in risk for segments and for intersections, but not for both combined.

Comparing bicycling on SBLs to riding in mixed traffic, Welleman and Dijkstra found there was a 24% decrease in the odds of a bicyclist injury crash along road segments between intersections, but a 32% increase in the odds of an injury crash at an intersection. All of the statistics cited were found to be significant at the 95% confidence level. The same comparison, but for moped riders, revealed a 28% increase in risk on segments and a 66% increase in risk at intersections. For moped riders, SBLs clearly increase overall risk. Wegman and Dijkstra explain that “junctions are safer for cyclists if the connecting stretch of road has no facility or bicycle lanes [instead of a separated bicycle lane]. This can be explained by the fact that (turning) traffic is confronted too suddenly with cyclists, because they were not ‘seen’ on the previous stretch of road. Another explanation may be that the car speed is higher when cyclists have separate facilities.” Unfortunately the study provided no details on the types of crashes or the severity of injuries.

### 3.3 Trafitec Copenhagen Study

This study examined police-reported traffic incidents before and after separated bike lanes were constructed in Copenhagen during the 1978-2003 period. The study, in Danish, was conducted by a consulting firm on behalf of the City of Copenhagen (Jensen, 2006); Jensen also wrote an article in English summarizing the results (Jensen, 2007).

In order to reduce confounding factors, the study was limited to the subset of SBLs that had no subsequent traffic improvement schemes, and for which sufficient years of before and after data was available: 23 segments totaling 20.6 km, all one-way. Danish SBLs are generally six to eight feet wide and separated from the roadway by a low mountable curb. (The study also looked at 11 projects where non-separated bicycle lanes had been installed.) The length of the comparison periods, although equal for “before” and “after,” was determined by the years of crash data available for each study road segment, ranging from one to five years.

The observed number of crashes and injuries for several years post-construction was compared to the expected number of crashes, where the latter was calculated by starting with the “before” numbers and applying adjustments for 1) citywide trends and 2) changes in bicycle and motor vehicle traffic at the site level. A third adjustment was considered to account for regression-to-mean effects. However, there was no RTM effect observed for the projects in the study when considered collectively. There was also an adjustment for heterogeneity among the sites in the sample. The study combined bicycles and mopeds together in all results, but bicyclists represented 90-95% of the total.

The published results show that comparing expected to counted values, there was a 10% increase in bicyclist injuries due to the construction of SBLs. In total this increase was not statistically significant. However, Jensen found that there was a statistically significant 24% increase (C.I. +5% to +46%) at intersections and a statistically insignificant 13% decrease (C.I. -30% to +10%) on road segments.

**Table 1. Changes in Crashes Involving Bicyclists on SBLs in Copenhagen**

| | | **Before Observed** | **After Expected** | **After Observed** | **Observed vs. Expected Change (%)** |
|---|---|---|---|---|---|
| **Crashes without MV** | | | | | |
| | Single Bicycle | 36 | 23 | 31 | n.s. |
| | Bicycle-Bicycle | 36 | 27 | 72 | 167 |
| | Bicycle-Pedestrian | 91 | 77 | 198 | 157 |
| | Subtotal without MV | 163 | 127 | 301 | 137 |
| **Bicycle-MV Crashes** | | | | | |
| | Rear end | 173 | 164 | 57 | **-63 (-73, -49)** |
| | Head on | 10 | 10 | 9 | n.s. |
| | Right angle | 200 | 210 | 204 | n.s. |
| | MV right turn | 81 | 104 | 282 | **+129 (+57, +253)** |
| | MV left turn | 120 | 119 | 161 | **+48 (+4, +110)** |
| | Bicycle left turn | 89 | 80 | 43 | **-41 (-59, -15)** |
| | Parked MV | 94 | 78 | 46 | **-38 (-57, -11)** |
| | Other | 0 | 1 | 2 | n.s. |
| | Subtotal | 767 | 766 | 804 | 5 |

| | | | | | |
|---|---|---|---|---|---|
| | Bicycle-MV | | | | |
| **All Crashes** | | 930 | 893 | 1,105 | 24 |

Notes: "bicycle" includes mopeds. MV = motor vehicle. n.s. = not significant. Changes in bold were calculated by Jensen using a meta-analysis method to account for heterogeneity in the study sites and were statistically significant (95% confidence interval in parentheses). Other percent changes are subtotals not calculated by Jensen and are the simple percent change in the After Expected and After Observed columns.

Jensen did not report the total change in bicyclist crashes, but did report bicyclist crashes by crash type. From his data (see Table 1), one can calculate that the number of crashes counted on SBLs involving bicyclists was 24% higher than expected (this does not account for heterogeneity among the sites). The study found that there were statistically significant decreases in three types of bicycle-motor vehicle collisions: rear end, bicyclist left turn, and parked motor vehicle. The changes in these crashes were calculated using a meta-analysis model that accounts for heterogeneity in the specific sites, which explains why the "observed vs. expected" for these crash types is not a simple percent change. Note that the "rear end" collisions include bicyclists hitting a slow or stopped motor vehicle in addition to motor vehicles colliding with bicyclists in front. One likely reason for the reduction in bicycle-left turn collisions is that bicyclists in Denmark are not allowed to merge to the left side of the road before making a left turn, but must make a two-stage turn. It is possible that some bicyclists took risky shortcut left turns from the right-side of the road prior to the deployment of SBLs, which makes this maneuver impossible.

Jensen found statistically significant increases in BMV crashes involving turning motor vehicles (especially right-turning, but also left-turning), as shown in Table 1. Jensen also found statistically significant increases in bicycle-bicycle rear-end crashes, and bicycle-pedestrian crashes involving pedestrians coming from the right or entering or exiting a bus. He does not provide estimates and confidence intervals for *all* bicycle-bicycle crashes or *all* bicycle-pedestrian crashes, but undoubtedly the increases are statistically significant, as was the increase in all crashes not involving motor vehicles. The simple percent increase in these crash types are shown in Table 1. Overall, there was a 5% increase in bicycle-motor vehicle crashes, and a 157% increase in crashes not involving motor vehicles. Jensen observes that "The crash composition has changed markedly after construction of" SBLS and concludes that "Bicyclists' safety has worsened due to these facilities."  Pedestrian injuries increased 19% (C.I. 2, 28), mostly at intersections, and there was no change in motorist injuries.

One might think that even though the number of collisions increased, the number of serious injuries and fatalities would have declined due to the SBLs. This was not the case.  Serious bicyclist injuries increased 11% (not statistically significant). Bicyclist fatalities increased from three counted before (and three predicted after) to ten counted after (Jensen 2006, Table 7.2, p. 97).

As with the Welleman and Dijkstra study, the crash risk increased more for moped riders than for bicyclists: intersection injuries increased 37% for moped riders compared to 22% for bicyclists (both results were statistically significant at the 95% level) (Jensen, 2006).

### 3.4 Aalborg University Study

A second Danish SBL study was completed by a team from Aalborg University. The results were first published in a Danish journal (Agerholm et al., 2006) and later presented at a conference in English (Agerholm et al., 2008). The study examined police-reported crashes from the beginning of 1986 to the end of 2004 before and after construction of one-way SBLs on larger urban roads in 17 towns in western Denmark. The 46 segments, totaling 40 km, were constructed between January 1989 and December 2000. The study design included a control group of roads with no changes in facilities for bicyclists in urban areas in the 19 municipalities which were included in the study (two municipalities provided only control data).

The change in bicyclist injuries was not found to be statistically significant, but there was a statistically significant increase in injury crash reports when crashes involving bicyclists are counted together with those involving moped users and pedestrians. The combined number of bicyclist, moped, and pedestrian injury crashes increased 25% compared to the expected number, and the share at intersections increased 34%. Both of these increases were statistically significant at the 95% level. The increase was 21% for bicyclists, 41% for pedestrians, and 51% for moped riders, but only the latter was statistically significant (at the 90% level). Overall, the number of injury crashes, including crashes involving cars and trucks only, increased 14% compared to what was expected, but the change was not statistically significant.
The study did not control for any changes in the number of bicyclists or motor vehicles on the study segments, other than controlling for the time trend by use of the comparison group. However, the authors collected daily traffic counts for motor vehicles “for most segments” and for bicyclists “in a few cases.” They concluded that there was no clear trend in the traffic counts on the study segments for either bicyclists or motor vehicles and therefore, “As there has been no increase in the number of users, this cannot be the reason for the increase in the number of accidents.” This conclusion would be more robust if they had bicycle traffic counts for more than 5 of the 46 segments (two of which showed decreases in bicycling and three of which showed increases). However, it is highly unlikely that the construction of SBLs would induce more *pedestrian* activity--and the study found that pedestrian crashes increased even more than bicyclist crashes.

Because this study did not provide detailed data on the type of crashes, it is not possible to compare it to Jensen’s results about the specific types of crashes that increased and decreased. However, they did confirm two important findings of the earlier studies: the negative effects of SBLs are most pronounced at intersections, and faster two-wheelers (moped riders) experience a more negative effect.

The authors proposed an explanation of the negative safety effects: “Bike paths, especially on urban roads, complicate the traffic situation for all road users: there are now three separate traffic areas instead of two. This would be ideal if it were not for the fact that on urban roads the three traffic groups, cars, cyclists, and pedestrians, frequently cross one of the other road users' areas . . .  When a road gets a bike path, cars and bicycles are differentiated on each area at the intersections, and the attention of the opposing party is weakened, and then it's a problem when the two sides of the intersections suddenly have to be integrated in the same area. On urban roads, there are many intersections where cars turn in and out of the area of cyclists. In addition, the duty to yield is imposed on motorists who in a number of situations, for example, in accidents between right-turning cars and straight-through cyclists, actually have poor physical prerequisites for compliance with the duty because the view to the rear can be difficult and, in some cases, impossible” (Agerholm et al., 2006 translated from Danish).

### 3.5 Nosal et al. Montreal Study

Montreal is the only city in North America that had a significant number of urban SBLs prior to 2008 and has therefore been a subject of several studies. The first of these used a cross-sectional method to compare bicyclist injury crash rates on Montreal streets with and without SBLs (Nosal et al., 2011). The test group consisted of nine street segments with two-way SBLs (11.8 km); the control group consisted of five street segments without designated bicycling facilities (6.9 km).  (The study also included four street segments with ordinary bike lanes.) Instead of using a large control group, the study selected a small number of segments that would serve similar routes as the “treatment” streets. This approach is not likely to produce a satisfactory comparison, and it did not in this study. For example, bicyclists on streets with SBLs can cross major streets with traffic signals while those on some of the parallel control streets face stop signs (Rue Boyer vs. Rue de la Roche; Rue Brebeuf vs. Rue Boyer).

The poor study design would be sufficient to invalidate the results. However there were also serious data problems. There was only seven months of injury data for the de Maisonneuve SBL, which accounted for more than one-third of the length of the test SBL segments. In total there was a very small amount of injury data: 222 injuries attributed to SBLs and 50 attributed to the control streets. When making crash rate comparisons, the authors did not report statistical significance or confidence intervals.

Equally problematic, the injury data “does not provide a description of whether or not the cyclist involved in a particular intersection injury was on the cross street or on the actual test street.” This is a fatal flaw because 80% of the crashes reported on both the control and test segments occurred at intersections. All of the crashes are attributed to the studied street, even though they could have occurred to cyclists using the intersecting street. The denominator of the calculated crash rate includes only the volume of bicyclists on the studied street. Because bicyclist counts were much lower for the control segments than the treated segments, inclusion of injuries relating to bicyclists using intersecting streets inflates the crash rates for the control segments more than for the study segments.

### 3.6 Bicycling Injuries and the Cycling Environment (BICE)

A team of public health researchers at the University of British Columbia studied bicyclists treated in hospitals for their “Bicyclists’ Injuries and the Cycling Environment” (BICE) study and published key results in two journal articles (Teschke et al., 2012); (Harris et al., 2013). The team identified 690 injured adult bicyclists treated in Toronto and Vancouver hospitals between May 2008 and November 2009 and interviewed the bicyclists to collect detailed information about the place where the injury occurred and the route they intended to take on the day of the injury. The “case crossover” study design compares the characteristics of the location where the bicyclist was injured to the characteristics of two randomly selected locations along the bicyclist’s intended route. For example, if 20% of the injury locations were on a downhill grade, but only 10% of the random points were downhill, the authors would report that the odds of injury are twice as high when riding downhill. This methodology compares bicyclists at injury and non-injury locations whereas the usual case-crossover method compares injury and non-injury times. The approach is valid only to the extent that itineraries on the day of injury are representative of all bicycle travel.

With regard to separated bike lanes (referred to as “cycle tracks” in the study), the authors concluded that “Cycle tracks had the lowest injury risk, about one ninth the risk of the reference route type.” However this conclusion is not supported by their data. Only four facilities, all in Vancouver and none in Toronto, were categorized as “cycle tracks.” Two of these four are portions of Vancouver’s Seaside Bicycle Route, a bike path with no intersections, in places where it is near a roadway. The longest “cycle track” segment included in the study (1 km) was the Burrard Street Bridge, which has intersections only at each end. Only the Carrall Street Greenway, with five intersections over its 0.6 km length, is a separated bike lane, and it was incomplete for most of the study period. A study subsequently released by the City of Vancouver confirms these observations:

“The facilities that were defined as protected bicycle lanes at the time of the BICE study, including the Burrard Bridge and Stanley Park Drive, have no or very few intersections along their length, including the Burrard Bridge which only has intersections at its start and end points, and also do not all satisfy the definition of a protected bicycle lane provided by NACTO. The types of separated bicycle lanes that the City of Vancouver has constructed since the time of the BICE study, are very different than those that were included in the BICE study, and have generally been constructed in areas with high concentrations of intersections. As such, the results of the cycling injury crash analysis on protected bicycle lanes from the BICE data should be interpreted with caution, as they do not necessarily reflect the typical characteristics of protected bicycle lanes that have been constructed in the City of Vancouver since the time of the BICE study, nor do they all necessarily reflect the City of Vancouver's definition of what constitutes a separated bicycle lane" (Urban Systems, 2015, pp. 49-50).

Given these facts, the reported risk reduction, based on data from a single short SBL commingled with intersection-free bicycle paths, provides no evidence about the safety of SBLs.

### 3.7 Lusk et al. Montreal Study

A follow-up study to the previously-cited Nosal et al. study of two-way SBLs in Montreal was published as a journal article (Lusk et al., 2011). Like the earlier publication, this cross-sectional study was fatally flawed because the authors attempted to identify a single comparison street for each of the "treatment" segments rather than using a large group of controls and a statistical model. The authors chose control *s*treets that could be used as alternatives by bicyclists going to a similar destination, rather than choosing streets that are similar in terms of potentially confounding factors such as traffic speed and volume, number and type of intersections, number of trucks, grades, and on-street parking. The study compared EMS-reported bicyclist injuries on six segments of SBLs totaling 13.4 km and on an equal length of control streets. (In a published correction, the authors reported that the length of the Rachel segment was 1.7 km, not 3.5 km as given in the paper.)

There is internal evidence that the study method does not adequately control for confounding factors. The crash rate calculated by the authors was lower on streets with SBLs in only three of the six pairs of treatment and control streets. Moreover, the crash rates differ dramatically among the six segments with SBLs: two (Brebeuf and de Maisonneuve) have a crash rate that is only one-tenth to one-quarter of the rate calculated for the other SBLs. Differences in traffic volumes and speeds seem to explain the lower crash rates: both segments are one-way, low-volume residential streets with low speed limits: 30 km/h for de Maisonneuve and 40 km/h for Brebeuf. The control streets are wider and have higher speed limits. Berri was the third SBL street found to have a lower crash rate than its control street (St. Denis). Although Berri is a higher-volume, higher-speed street, it is significantly different from St Denis. Along the studied segment Berri has seven intersections whereas St. Denis has twelve. Berri has no on-street parking or parking separated by a 1 m buffer. St Denis has on-street parking and dimensions that may encourage bicyclists to ride within range of opening car doors.

In response to a comment, the study authors acknowledged "that we did not control for all of the differences in road geometry and building typologies because there are no ideal matched streets" (Lusk et al., 2012). A cross sectional study relying on a few matched cases is an unsound method, and one can have no confidence in the results.

### 3.8 Lusk et al. U.S. Cycle Tracks

This study calculated crash rates for 19 sites in the U.S. with bicycle facilities that the authors describe as “cycle tracks” (Lusk et al., 2013). The study has methodological and data problems that invalidate the authors’ conclusions, at least with regard to urban SBLs. With regard to the latter, the outcome data consisted of police-reported BMV crashes reported by many different jurisdictions for periods ranging from a few months to 8.5 years. Eight of the facilities had zero reported crashes, and all but two had 5 or fewer. The bicycle volume data were likewise produced by unspecified and varying methods, with count periods as short as one hour. Most of the facilities (14 of 19 sites) had very few intersections because they were located either in suburban or rural areas or along a shore or a park. The five SBLs on typical city streets (First Avenue North in Minneapolis, and First, Second, Eighth, and Ninth Avenues in New York City) averaged 11.3 intersections per km whereas the 14 other segments averaged only 1.7 intersections per km (Schimek, 2013). These five combined also had a much higher crash rate, using the authors’ estimates, than the other 14 combined.

The study’s methodology has ever greater problems than its data. It was not a before and after study and there was no control group used for comparison. Instead the authors compared their results with published data on bicyclist crash rates. Three of the four sources cited were based on self-reports of all injuries, not on police reports of BMV collisions. Since most bicyclist injuries are not included in police reports, it is invalid to compare police data and self-reported data. The fourth source calculated national estimates of police-reported crashes per bicycle distance traveled. However, this is not a valid basis for comparison: the crash rate on any particular street or facility may differ from a national average due to any of the many ways the particular location may vary from the national average (e.g., traffic speed and volume).

### 3.9 Lessons from the Green Lanes

A team of researchers evaluated SBLs in five U.S. cities: Austin, Chicago, Portland, San Francisco, and Washington (Monsere et al., 2014). Using analysis of video recordings, the study counted conflicts at signalized intersections along the facilities. The study also asked intercepted bicyclists in two locations to report on collisions and near-collisions. However, in neither case was there a comparison to a control group, nor was there a before-and-after comparison. Without either before data or a control group there is no way to determine if there was a change in safety outcomes due to the SBLs.

### 3.10 FHWA Separated Bike Lane Crash Analysis Report

A team of consultants created a Crash Analysis Report as part of the FHWA Separated Bike Lanes Planning and Design Guide (FHWA, 2015). The authors solicited bicycle count and bicycle crash data from U.S. cities that had constructed SBLs. They identified 17 SBL projects for which they were able to obtain both bicyclist crash and count data before and after SBL implementation.

The study has both methodological and data quality problems. Although the before and after method is superior to the cross-sectional method used in almost all the other recent SBL studies, this study did not use a control group and thus was unable to account for long term trends. Also, the study used crash data only for the years for which bicyclist counts were available, which in all but four cases meant that there was only a single year of data for either the before or after period or both, making most of the data statistically unreliable. For 8 of the 17 sites, there were no more than two bicyclist crashes counted during this single reporting year.

There were also problems with the count data. There was no consistency in counting methods: "This analysis relied on whatever data could be provided. Bicycle volume data may have been provided as peak hour bicycle count or average hourly bicycle count for a period ranging from 2 hours to 24 hours." The counting periods were very short: "in most cases, where bicycle volume is available, it only includes one to four hour counts, rather than more accurate longer term counts" (emphasis added). The authors acknowledged the deficiency: "Challenges associated with obtaining bicycle volume data make it difficult to understand the true impacts on safety of separated bike lanes." There was no calculation of statistical significance of differences in crash rates. The authors expressed a lack of confidence in their own results: "The inconsistent nature of data collection, especially bicycle volumes, makes analyses – especially before and after analyses – difficult. . . . [T]here are limits to interpreting these data because of issues such as sample size, confounding variables, lack of statistical testing, what constitutes a crash, and other factors."

### 3.11 Zangenehpour et al. Montreal Conflicts Study

A team from McGill University and Polytechnique Montréal conducted a study of Montreal's two-way separated bike lanes using conflict analysis (Zangenehpour et al., 2016). Like the two earlier Montreal studies, the method was to compare the street with the SBL to a reference street without any designated bicycle facility that was nearby and parallel and so could be used as an alternative route. According to the authors, "the control sites were selected to have similar vehicle traffic conditions." However no data was provided to support that assertion. Instead of using police- or EMS-reported incidents, the study used video recordings to identify bicyclist-motorist conflicts at 23 signalized intersections: 8 without any designated bicycle facility, 8 with a two-way SBL on the right side of the road, and 7 on Boulevard de Maisonneuve, a one-way street with a two-way SBL on the left side. For unspecified reasons, the authors decided to consider "only the cyclists riding parallel to the motor-vehicles, in the same direction (prior to turning)." Therefore the study results are not representative of actual conditions on two-way SBLs because bicyclists operating counter to the flow of traffic were not included.

The metric used to identify a potentially unsafe condition was post-encroachment time (PET): the time difference between the instant a cyclist and a turning motor vehicle each pass through the point where their trajectories intersect. For each observed bicyclist, the PET value used was the minimum for any motor vehicle turning *either before or after* the cyclist crossed the intersection. This definition is problematic: on a street with no bicycle facilities, right-turning motorists can see a bicyclist ahead, and once the bicyclist has cleared the intersection, the motorist can turn without any further delay. If on the other hand there is a bicycle facility to the right of the right-turning motorist, it is essential that the turning driver wait to be sure that no more bicyclists are approaching from behind on the right (and also approaching from the opposite direction, in the case of two-way SBLs). One would therefore expect that turning drivers allow more time *after* a bicyclist clears an intersection when there is a bike lane. Leaving insufficient time *before* a bicyclist clears the intersection--a motorist turning across the path of an approaching bicyclist--is a safety concern. However, the method used in this study provides no means of distinguishing between the two cases, each of which has very different safety implications.

The authors used a statistical model of the number of conflicts, as defined by PET, using a logit model controlling for bicycle volume, turning motor vehicle volumes, and the number of lanes on the main and intersecting roads. They found that SBLs on the right side reduced the number of all conflicts by 40%, but only because of a reduction of PETs between 3 and 5 seconds duration. There was no change in the number that were considered “dangerous” (less than 3 seconds between the time the bicyclist crossed through the intersection and the nearest car before or after). With the SBL on the left side, there was no significant difference between intersections with and without SBL.

The authors posit an explanation for the fewer conflicts observed with cycle tracks on the right: “At intersections with cycle tracks on the right, the lateral distance between a cyclist and a vehicle in the same direction is greater than at intersections with cycle tracks on the left. This means that cyclists and drivers have a greater chance of seeing one another and avoiding dangerous interactions.” In other words, where the SBL is on the right, motorists have to cross the halfway point of the SBL before they connect with a same-direction bicyclist. Unfortunately, the converse is true: when the SBL is on the right, the geometry is unfavorable to opposite-direction bicyclists and more favorable to them when the SBL is on the left—but opposite-direction cyclists were inexplicably excluded from the study.

The authors found that there was a decrease in “conflicts” with SBLs, but only conflict types that they did not consider to be dangerous, and only when the SBL was on the right. But even these conclusions are invalid: the authors’ definition of “conflict” is not necessarily correlated with crash risk, given their indifference to motorists turning either before or after the bicyclist had passed the intersection, and half the potential conflicts--those between motorists and bicyclists approaching from the “wrong” direction—were excluded from the study.

### 3.12 Seville Study

This study (Marqués and Hernández-Herrador, 2017) examined the safety effects of 152 km of SBLs constructed in Seville, Spain between 2007 and 2013. The authors note that there was an increase in bicyclist injuries not involving motor vehicles “due to the concentration of cyclists in the cycle paths and the location of many cycle paths on or next the sidewalks,” but do not believe that the police accurately report these crash types. Their solution was to limit the comparison to BMV crash reports only. However, this may significantly underestimate the safety effects of the infrastructure.

Although the study used data from both before and after construction of the SBLs, the authors did not have any separate data for either bicycle crashes or bicycle usage on streets with and without SBLs. Instead they estimated city-wide BMV crash rates before and after project implementation and assumed that all of the change was due to the SBLs. There may be other explanations for reduced crash rates other than a direct effect of SBLs. Was there, the authors write, “a direct relation between the implementation of the network and the decrease of risk, or it worked through the increase of the number of bicycle trips through a safety in numbers effect?” They acknowledge that “increasing the number of cyclists in the bikeways also implies increasing the number of cyclists in the ordinary carriageway (not all streets have cycle paths and intersections are unavoidable), and that this increase of the number of cyclists in the carriageway also may produce an increase in the safety of cycling.” They argue that the direct effect was more important because their model of crash rates fits better, in terms of higher $R^2$, with a variable representing the size of the SBL network than with a variable representing the number of bicycle trips made. However it is the size and significance of the coefficient of a variable in a multivariate model that is the proper measure of effect size, not the change in $R^2$. Without data measuring crash rates on roads with and without SBLs, it is impossible to determine the actual effect of SBLs on safety.

### 3.13 IIHS Study

IIHS study has been published in Accident Analysis --
https://www.sciencedirect.com/science/article/abs/pii/S000145751931098X

### 3.14 Toronto Study

https://www.sciencedirect.com/science/article/abs/pii/S000145751930658X

They compared police-reported car-bike crashes in Toronto for 2 years before and 2 years after adding 6 cycle tracks (3 had previously been bike lanes). They found that the number of reported crashes went up 2-fold, but after accounting for the increase in bike trips there was a 38% reduction in the crash rate. On the nearby control streets, there was a 35% reduction in the crash rate. Instead of concluding that there may be other factors that account for the change in rate other than the cycle tracks, the authors conclude there was "'a 'safety halo' effect' suggesting an area-wide safety effect of cycle track implementation."

### 3.15 Berlin Study

Evaluation of contributory factors' effects on bicycle-car crash risk at signalized intersections
Peipei Liu & Stefanie Marker
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https://www.tandfonline.com/doi/abs/10.1080/19439962.2019.1591555

## 4.0 Discussion and Conclusions

The four oldest studies all found negative effects of SBLs on safety; the eight newer studies all claim to have found positive safety effects (Table 2). These conflicting findings can be reconciled. The four oldest studies used either a before-and-after method with a control group or, in one case, a cross-sectional method using area-wide data. All of the newer studies have serious deficiencies in method, data, or both that invalidate their conclusions. Most were cross-sectional studies, and none of them used a robust control group. Two were before-and-after studies, but neither used a control group and one considered only city-wide data rather than the actual locations where SBLs were constructed. The single case-crossover study used a reasonable method but did not actually study SBLs.

**Table 2: Summary Evaluation of Studies**

| Study Scope | Study Design | Crash or Injury Data | Source |
|---|---|---|---|
| Cities in Denmark, including Copenhagen, 1978 to 1981 | Before and After, with control group to account for long-term trends | Police reports | (Bach et al., 1985) |
| 14 municipalities in the Netherlands | Cross Sectional, all data | Police reports | (Welleman and Dijkstra, 1988) |
| Copenhagen, 1978 to 2003 | Before and After with model controlling for local and regional trends and check for RTM | Police reports | (Jensen, 2006) |
| 17 municipalities in Denmark, 1989 to 2000 | Before and After, with control group to account for long-term trends | Police reports | (Agerholm et al., 2006) |
| Montreal two-way SBLs | Cross Sectional with comparison group | EMS reports | (Nosal et al., 2011) |

| | | | |
|---|---|---|---|
| Injured bicyclists interviewed in Vancouver and Toronto hospitals | Case-Crossover | Self reports | (Teschke et al., 2012) |
| Montreal two-way SBLs | Cross-sectional, single paired control | EMS reports | (Lusk et al., 2011) |
| Urban and non-urban SBLs in various U.S. places | Cross-sectional, no controls | Police reports and other sources | (Lusk et al., 2013) |
| Two SBLs in Chicago | Cross-sectional, no comparison group | Video of conflicts, self reports of conflicts and near conflicts | (Monsere et al., 2014) |
| 11 SBLs in New York, DC, San Francisco, and Long Beach | Before and After with control for changes in bicyclist volume | Police reports and other sources | (FHWA, 2015) |
| Montreal two-way SBLs | Cross Sectional using statistical model | Video recording of conflicts | (Zangenehpour et al., 2016) |
| Seville SBLs | Before and After with no control group | Police reported crashes | (Marqués and Hernández-Herrador, 2017) |

In summary, there are no reliable studies that show an overall decrease in bicyclist crashes or injuries due to SBLs. The four with the strongest methods and data all point to an increase (Table 3). These studies provide strong evidence that SBLs increase the risk of bicyclist crashes and injuries at intersections and weaker evidence that they increase the risk overall. These studies only considered one-directional SBLs; there is substantial evidence that two-directional SBLs are more dangerous (Schepers et al., 2011); (Jensen and Buch, 2015).

Estimating the safety effect of SBLs is not simple. A robust study using police- or EMS-reported crash and injury data requires a substantial number of SBLs in place over a significant amount of time, as well as good traffic count data before and after implementation, and well-selected controls. However even the best studies may not reveal the true effect, since there is evidence that SBLs lead to a disproportionate increase in bicyclist crashes not involving motor vehicles, and these crashes are severely underreported in official sources. In recent years (2009 to 2017), only 5% of bicyclists treated by an emergency department in Denmark were involved in a crash reported to police, and only 8% of those reported to police were single-bicycle crashes, compared to 72% of those reported by hospital and emergency departments (Statistics Denmark, n.d.). Similarly, nearly three-quarters of hospitalized bicyclists in the Netherlands were involved in single-bicycle crashes (Schepers et al., 2015). A survey of bicyclists from 17 countries found that only 19% of crashes where the bicyclist needed medical attention were reported to police and only 23% of these crashes involved a motor vehicle (Shinar et al., 2018).

**Table 3: Effect of urban SBLs on Crashes and Injuries Involving Bicyclists**

| **Study Location** | **Intersection Crashes** | **Intersection Injuries** | **Total Crashes** | **Total Injuries** | **Source** |
|---|---|---|---|---|---|

| | | | | | |
|---|---|---|---|---|---|
| Cities in Denmark, including Copenhagen | 50% | - | 35% | 30% | (Bach et al., 1985) |
| 14 municipalities in the Netherlands | - | 32% | - | - | (Welleman and Dijkstra, 1988) |
| Copenhagen | - | 24% | 24% | 10% (n.s.) | (Jensen, 2006) |
| 17 municipalities in Denmark | - | 18% (n.s.) | - | 21% (n.s.) | (Agerholm et al., 2006) |

Notes: n.s. = not statistically significant. - = not reported.
(Agerholm et al., 2006) reported statistically significant increases, of slightly greater magnitude, in combined bicyclist and pedestrian crashes. (Bach et al., 1985) did not control for changes in bicycle volume, which they estimate to be a 20% increase on some Copenhagen SBLs.

Future studies of SBL safety should not rely on police-reported data to understand the full safety impact of bicycle infrastructure. One option is to record and analyze traffic behavior to identify conflicts that are predictive of crash outcomes, such as has been done in the Zangenehpour et al. study and others (Madsen and Lahrmann, 2017). This method could in principle measure bicycle-pedestrian and bicycle-bicycle conflicts in addition to bicycle-motor vehicle conflicts, but it cannot be used to analyze single-bicycle incidents. Another method is to interview injured bicyclists, as in the BICE study. The sample of injured bicyclists could be supplemented by a representative survey of bicyclists in the same area to provide a basis for adjusting the results to better represent the behaviors and route choices of all bicyclists.

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