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Safety-in-numbers: a systematic review and meta-analysis of evidence

Rune Elvik and Torkel Bjørnskau
Institute of Transport Economics
Gaustadalleen 21, 0349 oslo, Norway

ABSTRACT

This paper presents a systematic review and meta-analysis of studies that have estimated the relationship between the number of accidents involving motor vehicles and cyclists or pedestrians and the volume of motor vehicles, cyclists and pedestrians. A key objective of most of these studies has been to determine if there is a safety-in-numbers effect. There is safety-in-numbers if the number of accidents increases less than proportionally to traffic volume (for motor vehicles, pedestrians and cyclists). All studies reviewed in the paper are multivariate accident prediction models, estimating regression coefficients that show how the number of accidents depends on the conflicting flows (pedestrians, cyclists, motor vehicles), as well as (in some of the models) other factors that influence the number of accidents. Meta-analysis of regression coefficients involves methodological problems, which require careful consideration of whether the coefficients are sufficiently comparable to be formally synthesised by means of standard techniques of meta-analysis. The comparability of regression coefficients was assessed. It was concluded that a formal synthesis of regression coefficients in studies of the safety-in-numbers effect is defensible. According to a random-effects inverse-variance meta-analysis, the summary estimates of the regression coefficients for traffic volume are 0.50 for motor vehicle volume, 0.43 for cycle volume and 0.51 for pedestrian volume. Estimates are highly consistent between studies. It is concluded that a safety-in-numbers effect exists. It is still not clear whether this effect is causal, nor, if causal, which mechanisms generate the effect.

Key words: safety-in-numbers; meta-analysis; regression coefficients; causality

1 INTRODUCTION

Sustainable transport is an increasingly important objective of transport policy. In Norway, a policy objective in the current national transport plan (Samferdselsdepartementet 2013) is that any growth in traffic in major cities should be by means of public transport or non-motorised transport. Car traffic should not grow. Non-motorised transport is, however, associated with a higher risk of injury per kilometre of travel than most forms of motorised transport (Bjørnskau 2011). An increase in walking or cycling may therefore be associated with an increased number of injured road users.

On the other hand, a number of studies indicate that there is a so-called safety-in-numbers effect for pedestrians and cyclists. This means that when the number of pedestrians and cyclists increases, there is a less than proportional increase in the number of accidents involving them. However, the number of accidents involving pedestrians or cyclists and motor vehicles depends both on the volume of pedestrians or cyclists and on the volume of motor vehicles. To determine if there is a safety-in-numbers effect, one therefore needs data on all conflicting flows (motor vehicles, pedestrians, cyclists).

The objective of this paper is to systematically review studies of the safety-in-numbers effect and synthesise their findings by means of meta-analysis. Studies that use the number of injury accidents involving both a motor vehicle and a cyclist or pedestrian as dependent variable were treated as relevant. Studies of accidents involving pedestrians or cyclists exclusively were not included.

2 STUDY RETRIEVAL AND CLASSIFICATION

A literature search was performed to identify relevant studies. The search employed "safety in numbers" as search term and searched the Transport database provided by Ovid and the Web of Science database provided by Thomson Reuters. Details of the literature survey are described elsewhere (Bjørnskau 2013). Studies judged as relevant for the analyses reported in this paper are listed in Table 1.

Table 1 about here

Table 1 lists 26 studies. All these studies are multivariate accident modelling studies. Table 1 identifies four types of studies. These four types differ with respect to the number of variables included and the information given about the regression coefficients. The four types are defined as follows:

1. Studies that included traffic volume variables only and did not report the standard errors of regression coefficients.
2. Studies that included traffic volume variables only and reported the standard errors of regression coefficients.
3. Studies that included both traffic volume variables and at least one additional independent variable (potentially confounding factor), but did not report the standard errors of regression coefficients.
4. Studies that included both traffic volume variables and at least one additional independent variable (potentially confounding factor) and reported standard errors of regression coefficients.

A synthesis was made of the findings of studies in all four groups, but only studies in groups 2 and 4 were included in the inverse-variance meta-analysis. In most of the studies listed, the accident models have the following form:

$$\text{Number of accidents} = e^{\beta_0} MV^{\beta_1} CYCL^{\beta_2} e^{(\sum_n^{i=1} \beta_n x_n)} \qquad (1)$$

Where e denotes the exponential function, i.e. the base of the natural logarithms (2.71828) raised to the power of a regression coefficient *β*. The first term is the constant term. The next two terms refer to traffic volume. MV denotes motor vehicles, CYCL denotes cyclists (PED for pedestrians in models including pedestrian volume). Traffic volume typically enters models in the form of average daily traffic (AADT). The final term ($e^{(\Sigma\beta nXn)}$) is a set of predictor variables (X) other than traffic volume, which may influence the number of accidents. Please note that the following formulations are mathematically identical:

$$MV^{\beta_1} = e^{(\beta_1 \cdot \ln(MV))} \qquad (2)$$

Thus, the terms for traffic volume, given as power terms in equation 1, can be reformulated as exponential terms. All terms in equation 1 may then be expressed as a single exponential function. If a model of the form shown in equation 1 has been fitted to the data, a regression coefficient for traffic volume (MV, CYCL or PED) with a value less than one indicates that the number of accidents increases less than proportionally to traffic volume.

It deserves to be mentioned that models of the form presented in equation 1 have a long history in road safety research. Smeed (1974), as well as Hakkert and Mahalel (1978), discuss the use of such models to predict the number of accidents in junctions, mentioning studies from the early nineteen-fifties employing a model identical to the one in equation 1. The two terms for traffic volume in these models represent vehicles entering from the major and minor approaches to a junction.

Some of the studies listed in Table 1 have employed a different model of the relationship between traffic volume and the number of accidents, first proposed by Jacobsen (2003):

$$\text{Injury rate} = \frac{Injuries}{Km\ travelled} = \alpha \cdot \left(\frac{Km\ travelled}{Number\ of\ inhabitants}\right)^{(\beta - 1)} \qquad (3)$$

Studies relying on this type of model were not included in the formal synthesis of study findings. The reason for omitting these studies is that the definitions of risk (injuries/km travelled) and exposure (km travelled/number of inhabitants) may give rise to a spurious negative relationship between the variables (Knowles et al. 2009, Elvik 2013), which looks like a safety-in-numbers effect, but is merely a statistical artefact.

Table 1 indicates for each study the type of locations that were studied. According to the type of location, a distinction can be made between three levels for the study units:

1. Micro-level studies, in which typical study units are pedestrian crossings or junctions.
2. Meso-level studies, in which typical study units are street networks or urban traffic zones. Each network or zone consists of several streets and junctions.
3. Macro-level studies, in which study units are municipalities, cities or states.

Most studies are micro-level studies. The studies were assessed with respect to the possibility of including them in a meta-analysis. To be included in meta-analysis, a study should state:

1. One or more estimates of regression coefficients for motor vehicle volume, cyclist volume or pedestrian volume.
2. The standard error of each regression coefficient.

Studies that merely state regression coefficients, not their standard errors, can be included in simpler forms of synthesis (see section 5). Eight of the twenty-six studies were classified as unsuitable for meta-analysis. Only studies that report regression coefficients both for motor vehicle volume and for pedestrians or cyclists (or both) have been included. This inclusion criterion was adopted because previous studies, in particular Jonsson (2005), has found that there can be serious omitted variable bias if only one of the conflicting types of traffic is included in a model. In view of the potentially great importance of omitted variable bias in regression models, this issue is examined in greater detail in section 4 of the paper.

3 PROBLEMS OF META-ANALYSIS OF REGRESSION COEFFICIENTS

Previous surveys of the safety-in-numbers literature (Elvik 2009, 2013, Bjørnskau 2013) did not include a formal meta-analysis. The reason for this was that it was not obvious that the standard inverse-variance method of meta-analysis used in many other studies was applicable to the safety-in-numbers studies. Meta-analysis of regression coefficients involves a number of problems not encountered in meta-analyses of road safety evaluation studies stating estimates of effect as simple odds, odds ratios or accident rate ratios.

Card (2012) discusses this issue and states: “A critical limiting factor in using these effect sizes from multiple regression analyses is that every study should include the same covariates (Zs) in analyses from which results are drawn. In other words, it is meaningful to compare the independent association between X and Y only if every study included in the meta-analysis controls for the same Z or set of Zs. If different studies include fewer or more, or simply different, covariates, then it makes no sense to combine the effect sizes of the type described here (i.e. regression coefficients, semipartial or partial correlations) from these studies.”

He continues: “If different studies do use different covariates, then you have two options, both of which require access to basic, bivariate correlations among all relevant variables (Y, X, and all Zs). The first option is to compute the desired effect sizes from these bivariate correlations for each study and then meta-analyse these now-comparable effect sizes. The second option is to meta-analyse the relevant bivariate correlations from each study in their bivariate form and then use these meta-analysed bivariate correlations as sufficient statistics for multivariate analysis.”

These options are not feasible in this study, as there is no access to the bivariate correlations in all the original studies. In the ensuing discussion, Card uses correlation coefficients exclusively to illustrate his points. Although correlation coefficients are related to regression coefficients, they are not the same. Moreover, it is likely that Card refers to regression models fitted to continuous data. The regression coefficients estimated in safety-in-numbers studies are based on countregression models, in particular negative binomial regression, which differs from ordinary least-squares regression in many ways (Washington, Karlaftis and Mannering 2011).

When reviewing the literature, it is easy to find examples of meta-analyses that do not adhere to the restrictions advocated by Card. Thus, Ewing and Cervero (2010) report a meta-analysis of how characteristics of the built environment influence travel demand. The influence of the built environment on travel demand is stated in terms of elasticities; however not all of these were estimated the same way, nor did they all control for exactly the same confounding variables. Moreover, the standard errors of many of the estimated elasticities were not known. Despite these limitations, the meta-analysis reported interesting and useful findings, showing a systematic pattern indicating that the results were not merely artefacts of the limitations of the analysis. It is therefore fruitful to investigate whether it makes sense to try to formally synthesise regression coefficients even if the studies producing the coefficients are not identical in all respects.

Hauer (2010) is somewhat more optimistic about the comparability of regression coefficients although he warns against an uncritical comparison of such coefficients. He offers the following guidance: "Suppose … that two regressions that differ in some variables yield roughly the same (coefficient) for a treatment (read: variable). The interpretation of such a consistency depends on the "state of nature". If the coefficient depends only weakly on all the variables included in one regression but not the other, then the consistency could be viewed as genuine. However, if the coefficient depends strongly on the not-in-common variables, then the noted consistency should carry little causal weight."

To clarify the interpretation of this point of view, suppose that one study found a regression coefficient for cycle volume of 0.53 (standard error 0.07), another study found a regression coefficient of 0.49 (standard error 0.11). These coefficients are roughly the same. The confidence intervals obtained from the standard errors overlap. From a statistical point of view, one cannot conclude that the coefficients are different. They could reasonably be interpreted as independent estimates of the same regression coefficient, differing by sampling error only.

Suppose further that the first study included variables A, B, C, and D. The second study included variables C, D, E, and F. Only the variables C and D are common in both studies. If it can be shown that:

(A) The estimate of the regression coefficient in the first study (0.53) does not depend strongly on variables A and B,

(B) The estimate of the regression coefficient in the second study (0.49) does not depend strongly on variables E and F, and

(C) Both coefficients depend strongly (and in same direction) on the common variables C and D, then:

The fact that the coefficients are close in value indicates that they are both produced by the same influencing (or causal) factors. It then makes sense to compare the coefficients and combine them formally.

This means that if it is possible to determine which of the independent variables in a model most strongly influence the regression coefficients for traffic volume, one may perform an exploratory analysis for the purpose of assessing the comparability of regression coefficients and thereby their amenability to meta-analysis. The guidelines proposed by Hauer have been applied in this paper.

4 EXPLORATORY META-ANALYSIS

A two-stage exploratory analysis has been performed. The first stage consists of assessing, as far as available studies permit, the comparability of regression coefficients. Provided this exploratory analysis concludes that regression coefficients are comparable, the second stage of exploratory meta-analysis is to examine funnel plots and assess the possible presence of publication bias.

4.1 Regression coefficient comparability

To determine whether regression coefficients for the traffic volume variables are comparable, one must assess their sensitivity to the specification of the regression model. More specifically, if including additional variables in a model changes the values of the regression coefficients, that may indicate omitted variable bias, meaning that the estimated coefficients reflect the effects not only of traffic volume, but of other variables, not included in the regression model. Table 2 lists studies that have compared regression coefficients for different specifications of the regression model.

Table 2 about here

Summersgill and Layfield (1996) developed one regression model containing variables for traffic volume only and one model containing five covariates in addition to the traffic volume variables. As can be seen from Table 2, the values of the regression coefficients for traffic volume did not change much when the covariates were added to the model.

Jonsson (2005) made more extensive comparisons between several versions of the regression models. The largest change in the values of the regression coefficients for traffic volume were found when models containing only one variable for traffic volume, i.e. only motor vehicles, only pedestrians or only cyclists were compared to models that included two variables (motor vehicles and pedestrians or motor vehicles and cyclists) for traffic volume. In the other model specifications, the values of the regression coefficients for traffic volume varied somewhat, but all were consistent with a safety-in-numbers effect.

More limited comparisons reported by Turner et al. (2006), found highly consistent values of the regression coefficients in the models for cyclist accidents, a little more variation in the models for pedestrian accidents. The three model versions compared by Miranda-Moreno et al. (2011) had highly consistent values of the regression coefficients for traffic volume. The two models compared by Schepers et al. (2011) did not differ much with respect to the values of the regression coefficients for the traffic volume variables. Finally, re-analyses of the model presented by Elvik et al. (2013), omitting the interaction term included in the original model, produced very stable values of the regression coefficients for the traffic volume variables.

It was, unfortunately, not possible to conduct similar comparisons of different model specifications for all the studies included in this paper. The comparisons reported in Table 2 are therefore indicative only, rather than conclusive. Nevertheless, the comparisons do not give strong support to the view that regression coefficients are incomparable, because models including different variables are associated with large differences in the values of the regression coefficients for the traffic volume variables. On the contrary, regression coefficients appear to be rather robust across different model specifications. Based on these results, it was decided to proceed to the second stage of the exploratory analysis.

4.2 Analysis of funnel plots

According to the inverse-variance method of meta-analysis, a summary estimate is developed as follows (Elvik 2005):

$$\text{Summary estimate} = \boldsymbol{\bar{Y}} = \frac{\sum \boldsymbol{W_g Y_g}}{\sum \boldsymbol{W_g}} \tag{4}$$

Y_g denotes the coefficient estimate in study g, W_g is the statistical weight assigned to study g and $\bar{Y}$ is the summary estimate, i.e. weighted mean estimate of a coefficient. The statistical weight assigned to each coefficient estimate is proportional to the inverse of its sampling variance:

$$\text{Statistical weight} = \boldsymbol{W} = \frac{1}{\boldsymbol{SE}^2} \tag{5}$$

SE is the standard error of a coefficient. The standard error of a regression coefficient is routinely produced by any commercial statistical software package used to fit accident prediction models. Table 3 lists studies that have been included in the formal synthesis of findings. As can be seen, standard errors were not stated for all these studies. Studies for which standard errors were not stated were not included in the meta-analysis applying the inverse-variance method.

Table 3 about here

The studies listed in Table 3 form the basis for developing funnel plots. A funnel plot is a graphical tool that can be used in exploratory meta-analysis. It is a scatter plot of results, in which the estimate is plotted on the horizontal axis and its statistical precision on the vertical axis. There are many ways of developing funnel plots, but in this paper the guidelines of Sterne and Egger (2001) have been followed. Figure 1 shows a funnel plot of estimates of the regression coefficient for motor vehicle volume.

Figure 1 about here

The weighted mean regression coefficient is shown by the solid vertical line. Dotted lines indicate the contours of the funnel (turned upside-down, since the most precise estimates are plotted in the top of the diagram). Data points are found in all regions of the diagram. The trim-and-fill technique (Duval and Tweedie 2000A, 2000B, Duval 2005) was applied to test for the possible presence of publication bias. Publication bias denotes a tendency not to publish findings that are not statistically significant or that go against prior expectations and are therefore regarded as difficult to interpret. Bias against unexpected or unwanted findings will show up as a missing tail of the funnel plot. The trim-and-fill analysis gave no indication of publication bias for the data points shown in Figure 1. One data point was identified as outlying. The data point was retained in the analysis.

Figure 2 shows a funnel plot of coefficient estimates for pedestrian volume. There are fifteen data points in Figure 2. The data points indicate a bimodal distribution of coefficients. There is one group of data points located between -1.2 and -0.8 on the horizontal scale (the natural logarithm of coefficient estimates) and another group of data points located between -0.6 and -0.2 on the horizontal scale. The weighted mean coefficient is located midway between these two groups of data points (solid vertical line).

Figure 2 about here

It is not known why coefficients for pedestrian volume are bimodal. Application of the trim-and-fill method did not indicate publication bias. Two data points were identified as outlying. These data points were located at opposite ends of the distribution of coefficient estimates and had nearly the same statistical weights. The data points thus tended to cancel each other and were retained in the analysis.

Figure 3 shows a funnel plot of coefficient estimates for cyclist volume. There are eleven data points in the Figure. A test found that one of the data points was outlying.

Figure 3 about here

The significance of the outlying data point for cyclist volume will be further discussed later. At this point, it is noted that when the outlying data point was included in a trim-and-fill analysis, massive publication bias was indicated. However, when the outlying data point was omitted, there was no indication of publication bias. The data point was no longer classified as outlying when a random-effects model of meta-analysis (see below) was performed. It is therefore concluded that the regression coefficients, as far as it can be tested, are robust with respect to model specification and that the distribution of the regression coefficients does not indicate the presence of publication bias. Based on these results an inverse-variance meta-analysis of the coefficients was performed.

5 MAIN ANALYSIS AND RESULTS

A formal meta-analysis was feasible for 25 regression coefficients for motor vehicle volume, 15 regression coefficients for pedestrian volume and 11 regression coefficients for cycle volume. Table 4 reports the results.

Table 4 about here

There was significant heterogeneity in coefficient estimates both for motor vehicles, pedestrian volume and cycle volume. A random-effects model of meta-analysis was therefore adopted. The summary estimates of the regression coefficients were very similar in the fixed-effects and random-effects analysis. The best estimates are 0.50 for motor vehicle volume, 0.43 for cycle volume and 0.51 for pedestrian volume. All summary estimates of the coefficients are statistically highly significant.

In addition to the meta-analysis, Table 4 reports two other summary estimates of the model coefficients. These were developed in order to include all studies, since it was not possible to include all studies in the formal meta-analysis. A simple mean of the regression coefficients was estimated and is reported in the first row of Table 4. The standard error of the mean was estimated as follows:

Standard error of the simple mean = $\boldsymbol{SE} = \frac{\boldsymbol{S}}{\sqrt{\boldsymbol{N}}}$ (6)

S is the standard deviation of the coefficients and N is the number of coefficients. The simple means were based on 34 regression coefficients for motor vehicle volume, 20 for pedestrian volume and 14 for cycle volume. Mean values of the regression coefficients using the number of accidents as weights were also estimated. These are shown in the second row of Table 4. For these coefficients, the standard error was estimated as (Hauer 1997):

Standard error of coefficients weighted by the number of accidents =

$$\boldsymbol{SE} = \frac{\sqrt{\sum \boldsymbol{A}}}{\sum \boldsymbol{A}} \tag{7}$$

A is the number of accidents, the sigma ($\sum$) denotes the sum of the number of accidents forming the basis of the weighted mean estimate.

The results presented in Table 4 are highly consistent. All summary regression coefficient estimates are consistent with a safety-in-numbers effect. All summary coefficient estimates are statistically highly significant. The studies summarised in the meta-analysis thus very clearly indicate the existence of a safety-in-numbers effect.

Subgroup analysis was performed for studies relying on study units at the micro-, meso- and macro-levels. The results of these analyses are also reported in Table 4. The analysis was somewhat hampered by the fact that most studies are at the micro level. For pedestrian accidents, there were no studies at the macro level.

Nevertheless, as far as they go, the estimated summary regression coefficients are all consistent with a safety-in-numbers effect. There was no consistent tendency for the safety-in-numbers effect to be weaker or stronger at the meso- and macro-levels than at the micro level.

6 DISCUSSION

Based on the studies reviewed in this paper, it seems clear that a safety-in-numbers effect exists both for pedestrians and cyclists. An increasing number of pedestrians or cyclists is therefore unlikely to be associated with a proportional increase in the number of accidents. The summary regression coefficients, which are all close to the value of 0.50, indicate that the increase in the number of accidents associated with an increase in the number of pedestrians and cyclists will be far less than proportional to the increase in the number of pedestrians and cyclists. An accident elasticity (coefficient) of 0.50 is equal to the square root. Thus, if the number of pedestrians or cyclists increases by 100 percent (a doubling), the number of accidents can be expected to increase by the square root of two, i.e. by 41 percent ($\sqrt{2} = 1.41$).

The results of the individual studies are very consistent. All regression coefficients for pedestrian volume and all regression coefficients for cycle volume indicate a safety-in-numbers effect. 31 of the 34 regression coefficients for motor vehicle volume indicate a safety-in-numbers effect. Such a high degree of consistency is rare in accident studies.

On the other hand, it is still not possible to rule out methodological interpretations of study findings. In the first place, it is noteworthy that eight of the 26 studies could not be included at all in the meta-analysis. A majority of these studies show statistical relationships that could be spurious. A further three studies did not report the standard errors of the regression coefficients. These studies could not be included in the inverse-variance meta-analysis, but their findings could be summarised by means of simpler techniques of analysis. Thus, in total ten out of twenty-six studies had methodological shortcomings that prevented their inclusion in the meta-analysis.

In the second place, all the accident models are of the same form. The models specify a monotonic functional relationship between traffic volume and the number of accidents. A monotonic function rises or falls throughout its range. It does not have turning points. However, as pointed out by Elvik (2009), it is not unthinkable that there could be limits to, or even a turning point, for the safety-in-numbers effect for cyclists. When a large number of cyclists travel close to each, it would not be surprising if the probability that they un-intentionally encroach on each other would increase. Further studies are needed to determine whether a safety-in-numbers effect could have a turning point or not.

In the third place, no study has controlled sufficiently for potentially confounding factors. There are at least two potentially confounding factors that have not been fully controlled for in the studies reported so far. One of them is that the characteristics of cyclists and pedestrians could differ depending on the number of cyclists or pedestrians. It is well-known that many people hesitate to cycle, or abstain entirely from it, in complex city traffic that they regard as hazardous and difficult to handle. In such traffic environments, one would expect there to be a selective recruitment of cyclists, possibly also pedestrians. In the most demanding traffic environments, only those cyclists or pedestrians who tolerate the risk would opt for cycling or walking. Although one might expect recruitment to become gradually less selective as the number of cyclists and pedestrians increase, it is unlikely that it would be entirely eliminated.

A second potentially confounding factor that is at best rather crudely controlled for in some studies, is the quality of the infrastructure provided for cyclists and pedestrians. This point is discussed by Bhatia and Wier (2011). It is not unlikely that a two-way causal chain is involved here: If you provide high-quality infrastructure, it will make walking or cycling more attractive. This may induce more people to walk or cycle, which in turn would make it cost-effective to improve the quality of infrastructure even more. The variables that have been included in some models are at best crude indicators of infrastructure quality and do not capture all aspects of it. The sensitivity of the regression coefficients to the number of confounding factors controlled for in a study is shown in Figure 4.

Figure 4 about here

The maximum number of potentially confounding variables controlled for is 16; the next highest number is 8. There is no clear tendency for the values of the regression coefficients to move systematically closer to one as the number of confounding factors controlled for increases. Had there been such a tendency, it would suggest that the safety-in-numbers effect might disappear entirely when enough confounding factors were controlled for. The fact that all studies find a safety-in-numbers effect could then be attributed to the fact that the studies did not control adequately for confounding factors. However, no consistent tendency for this to be the case can be seen in Figure 4.

In the fourth place, the reporting of accidents in official statistics is incomplete, in particular for accidents involving cyclists or pedestrians only. Indeed, single pedestrian accidents are not defined as a reportable accident in any country. Single bicycle accidents leading to injury are, in general, reportable, but the level of reporting is extremely low, in the order of 1-10 percent. Could it be the case that the safety-in-numbers effect applies to the reported accidents only, but is not found when unreported accidents are included? In theory, such a possibility cannot be ruled out. There are few studies that shed light on the issue. In his study of single-bicycle accidents, Schepers (2012) found a regression coefficient of 0.52 for fatalities, 0.76 for those admitted to hospital as in-patients and 0.80 for self-reported minor injuries not treated at hospitals. This suggests that the safety-in-numbers effect is weaker for minor injuries than for serious injuries. Schepers and Heinen (2013), however, did not find a similar tendency. One possible reason for the safety-in-numbers effect found for single-bicycle accidents by Schepers (2012) could be that skills improve as the amount of cycling increases.

In the fifth place, the causal mechanism generating a safety-in-numbers effect is unknown. It could be that the quality of the interaction between cyclists and pedestrians on the one hand and motorists on the other improves as the number of cyclists and pedestrians increases. However, quality of interaction is a somewhat vague and catch-all concept that needs to be specified in some detail to become operational. Studies observing behaviour in detail are going on and will hopefully shed more light on the issue.

In the sixth place, a few anomalous results were found in the meta-analysis that deserve discussion. One of these results is the detection of an outlying data point in coefficient estimates for cycle volume. When a trim-and-fill analysis to detect publication bias was run, including the outlying data point, it indicated a massive publication bias, as shown in Figure 5.

Figure 5 about here

When the outlying data point was omitted, there was no indication of publication bias. Moreover, the data point was not classified as outlying when random-effects statistical weights were adopted. Still, this shows that the presence of an outlying data point may have a great influence on the results of a meta-analysis. It was decided to retain the data point, in view of the fact that it was not outlying in the random-effects model. All results reported above therefore include this data point.

Two data points (of a total of fifrteen) were classified as outlying according to the fixed-effects model of coefficients for pedestrian volume. No data point was classified as outlying according to the random-effects model. Besides, the two outlying data points according to the fixed-effects model balanced each other, one being at the upper end of estimates, the other being at the lower end. The data points were therefore retained.

One outlying data points was found with respect to coefficient estimates for motor vehicle volume. The data point was included in the analysis.

7 CONCLUSIONS

The main conclusions from the research reported in this paper can be summarised as follows:

1. A total of 26 studies of the safety-in-numbers effect were identified. Only 15 studies could be included in a formal meta-analysis. The remaining 11 studies had various methodological shortcomings that prevented them from being included in the meta-analysis.
2. Meta-analysis of regression coefficients estimated by means of regression models presents unique challenges. Meta-analysis of regression coefficients should only be performed when the coefficients are robust, i.e. not sensitive to variations in model specification.
3. It was to a limited extent possible to test the robustness of regression coefficients, but the tests indicated that the coefficients were sufficiently robust for a meta-analysis to make sense.
4. The meta-analysis found no evidence of publication bias and indicated the existence of a clear safety-in-numbers effect. Summary estimates of accident elasticities were 0.50 for motor vehicle volume, 0.43 for cycle volume and 0.51 for pedestrian volume.
5. It is still not possible to determine whether the safety-in-numbers effect is a causal relationship or merely a statistical relationship not generated by any plausible causal mechanism.

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LIST OF FIGURES AND TABLES

Figure 1:

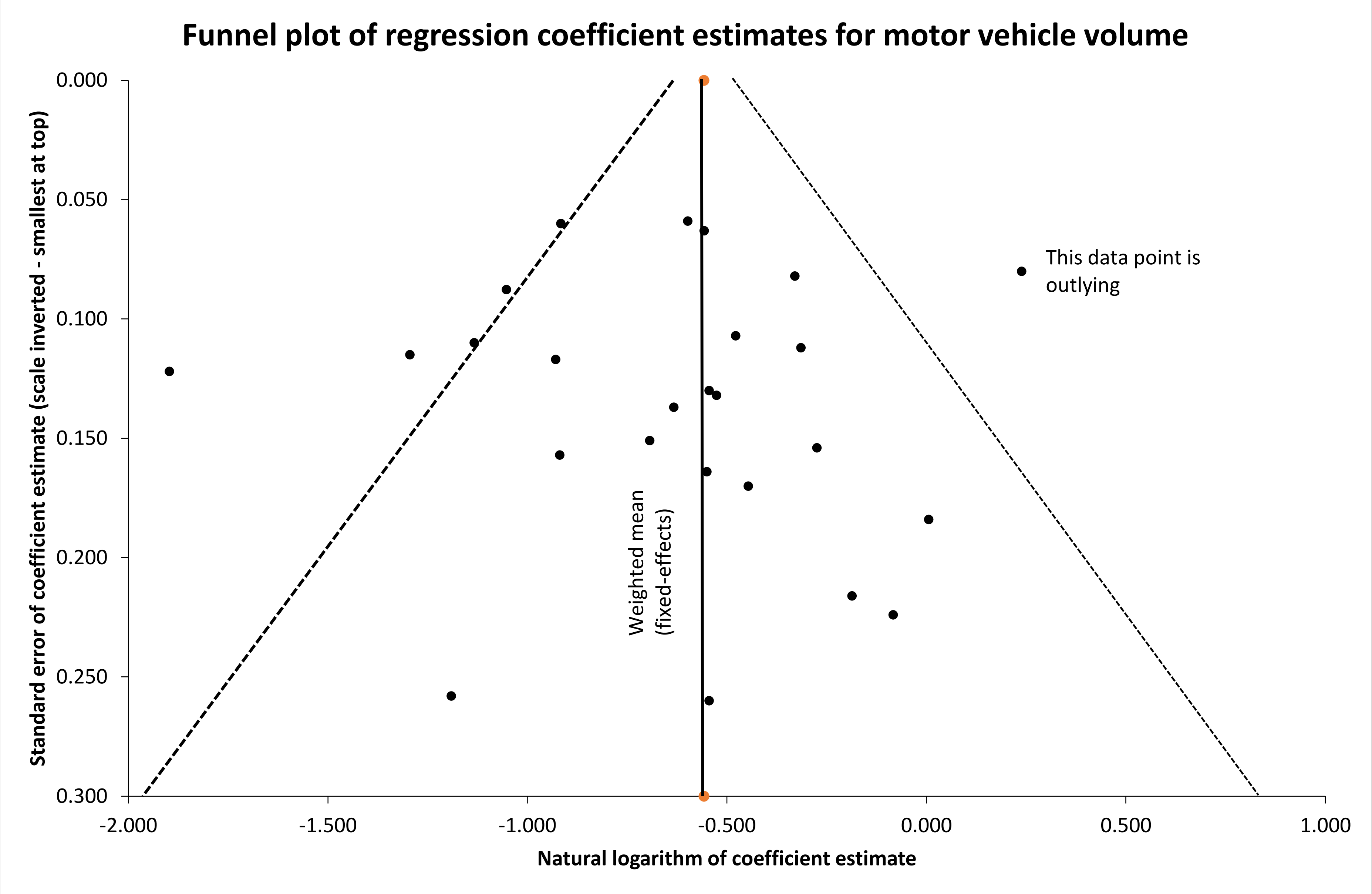
Funnel plot of regression coefficient estimates for motor vehicle volume
Standard error of coefficient estimate (scale inverted - smallest at top)
0.000
0.050
0.100
0.150
0.200
0.250
0.300
Natural logarithm of coefficient estimate
-2.000
-1.500
-1.000
-0.500
0.000
0.500
1.000
Weighted mean (fixed-effects)
This data point is outlying

Figure 2:

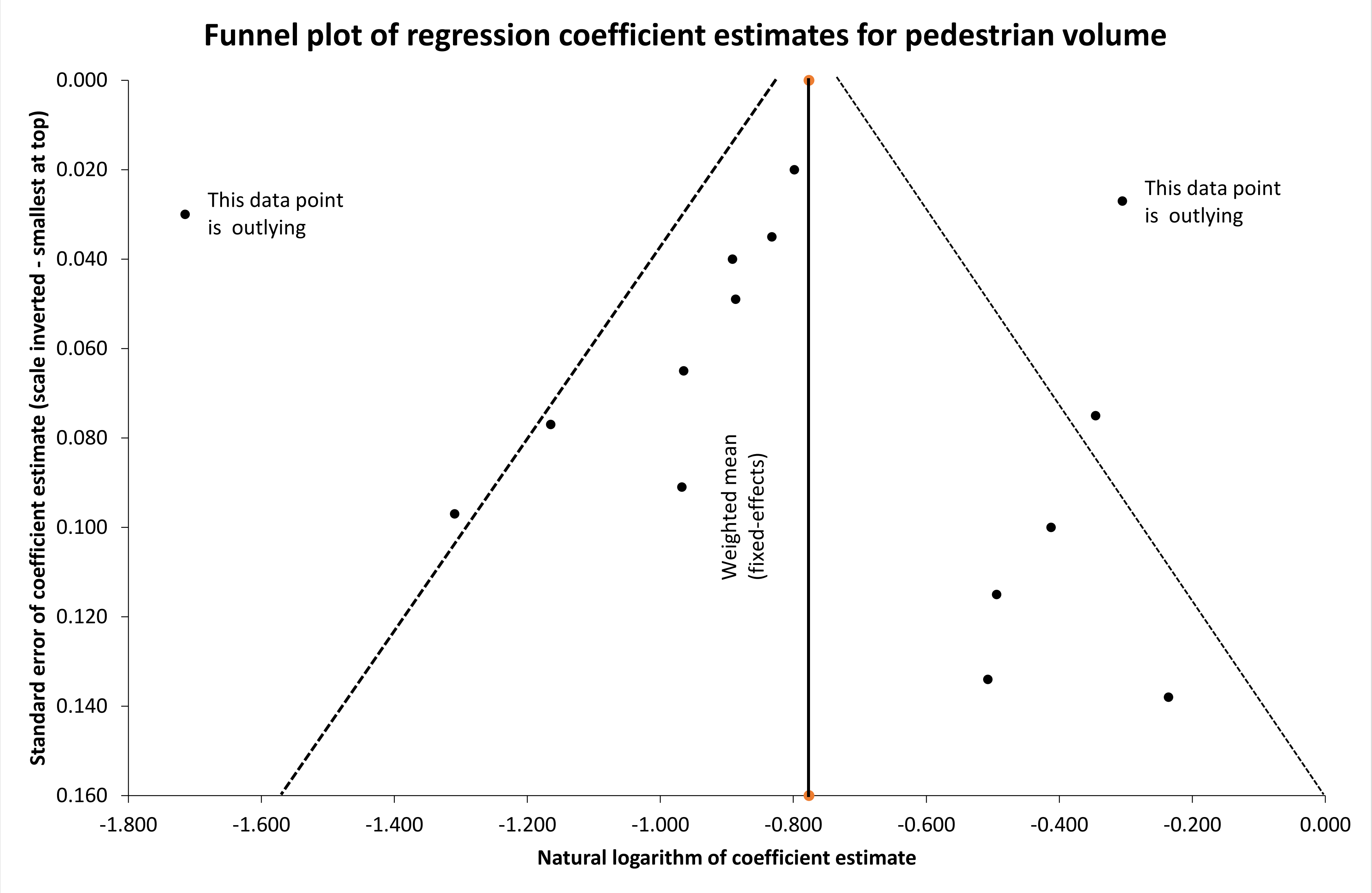
Funnel plot of regression coefficient estimates for pedestrian volume
Standard error of coefficient estimate (scale inverted - smallest at top)
0.000
0.020
0.040
0.060
0.080
0.100
0.120
0.140
0.160
This data point is outlying
This data point is outlying
Weighted mean (fixed-effects)
-1.800
-1.600
-1.400
-1.200
-1.000
-0.800
-0.600
-0.400
-0.200
0.000
Natural logarithm of coefficient estimate

Figure 3:

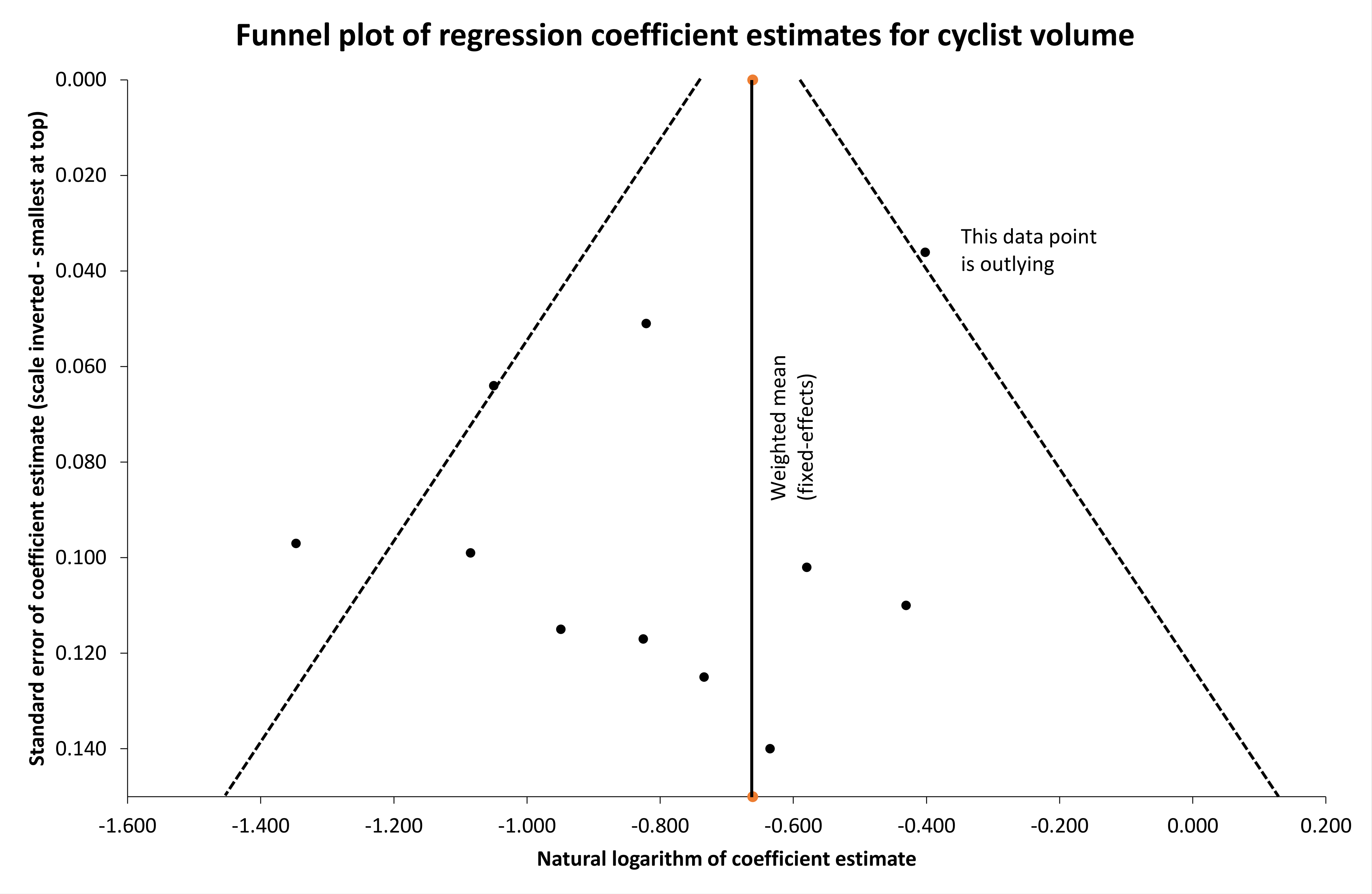
Funnel plot of regression coefficient estimates for cyclist volume
Standard error of coefficient estimate (scale inverted - smallest at top)
0.000
0.020
0.040
0.060
0.080
0.100
0.120
0.140
Natural logarithm of coefficient estimate
-1.600
-1.400
-1.200
-1.000
-0.800
-0.600
-0.400
-0.200
0.000
0.200
Weighted mean (fixed-effects)
This data point is outlying

Figure 4:

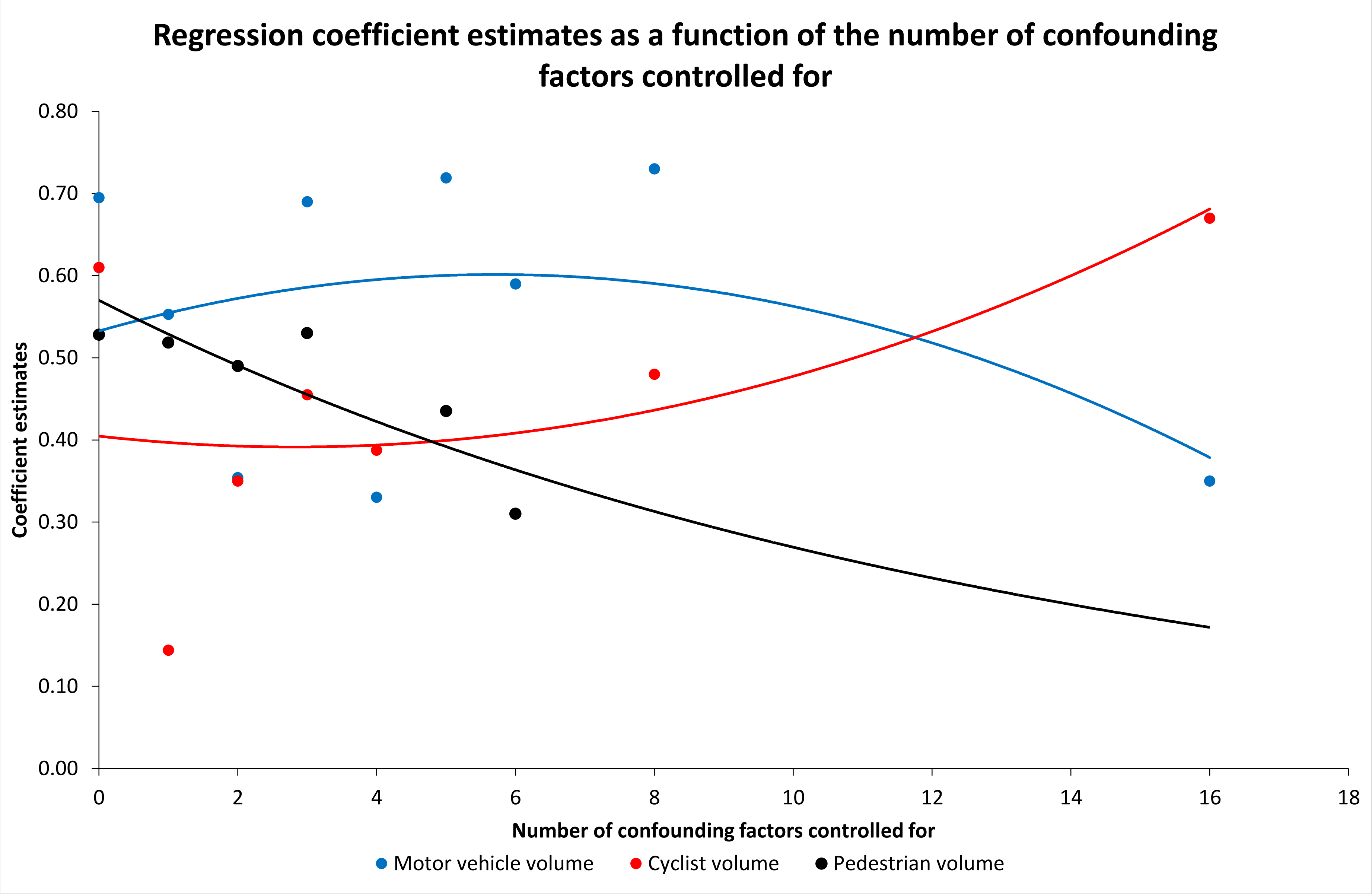
Regression coefficient estimates as a function of the number of confounding factors controlled for
Coefficient estimates
0.80
0.70
0.60
0.50
0.40
0.30
0.20
0.10
0.00
0
2
4
6
8
10
12
14
16
18
Number of confounding factors controlled for
● Motor vehicle volume
● Cyclist volume
● Pedestrian volume

Figure 5:

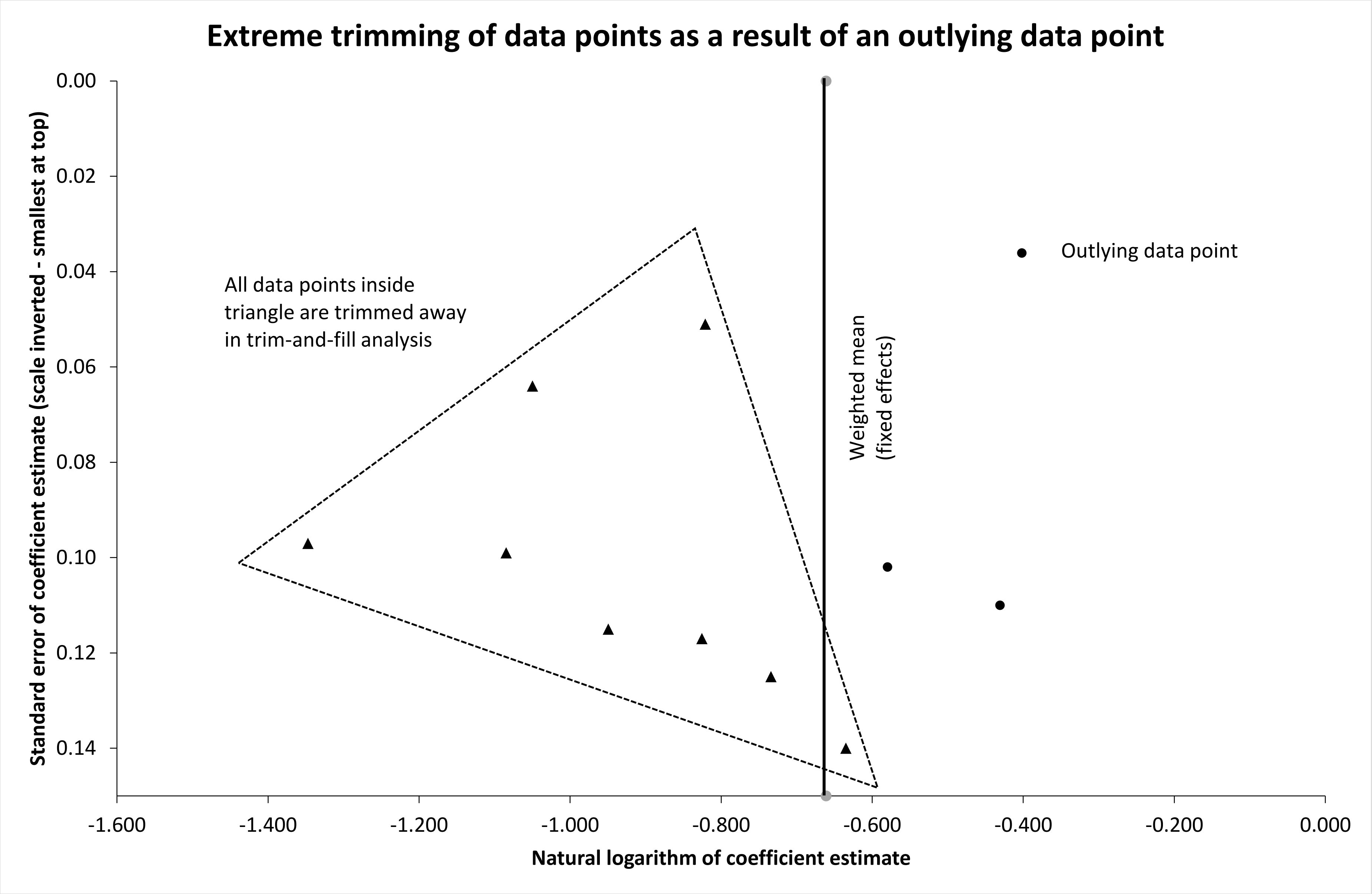
Extreme trimming of data points as a result of an outlying data point
Standard error of coefficient estimate (scale inverted - smallest at top)
0.00
0.02
0.04
0.06
0.08
0.10
0.12
0.14
All data points inside triangle are trimmed away in trim-and-fill analysis
Weighted mean (fixed effects)
● Outlying data point
-1.600
-1.400
-1.200
-1.000
-0.800
-0.600
-0.400
-0.200
0.000
Natural logarithm of coefficient estimate

Table 1:

Study	Type of location studied	Inclusion in synthesis	Study classification; reason for exclusion from meta-analysis
Inwood and Grayson 1979	Pedestrian crossings	Included	Type 4: Several independent variables; standard errors reported
Maycock and Hall 1984	Roundabouts	Excluded	States coefficient only for cross-product of flows, not for cars and pedestrians
Hall 1986	Signalised junctions (urban)	Included	Type 2: Traffic volume only; standard errors reported
Brüde and Larsson 1993	Urban junctions (mixed)	Included	Type 1: Traffic volume only; no standard errors reported
Summersgill and Layfield 1996	Urban road links	Included	Type 4: Several independent variables; standard errors stated
Leden, Gårder and Pulkkinen 1998	Urban junctions	Excluded	Contains data on cyclist volume only
Leden 2002	Signalised junctions (urban)	Included	Type 1: Traffic volume only; no standard errors reported
Lyon and Persaud 2002	Signalised junctions (urban)	Included	Type 2: Traffic volume only; standard errors reported
Jacobsen 2003	Cities in many countries	Excluded	Statistical relationship may be an artefact; data on cyclist volume only
Jonsson 2005	Urban road links	Included	Type 4: Several independent variables; standard errors reported
Robinson 2005	Australian states	Excluded	Statistical relationship may be an artefact; data on cyclist volume only
Zegeer et al. 2005	Pedestrian crossings	Included	Type 4: Several independent variables; standard errors reported
Bonham et al. 2006	Urban traffic zones	Excluded	Contains cyclist volume only; data not presented
Geyer et al. 2006	Urban junctions (mixed)	Included	Type 4: Several independent variables; standard errors reported
Turner, Roozenburg and Francis 2006	Signalised junctions (urban)	Included	Type 3: Several independent variables; no standard errors reported
Harwood et al. 2008	Signalised junctions (urban)	Included	Type 4: Several independent variables; standard errors reported
Knowles et al. 2009	British municipalities	Excluded	Statistical relationship may be an artefact; data on cyclist volume only
Vandenbulcke et al. 2009	Belgian municipalities	Excluded	Statistical relationship may be an artefact; data on cyclist volume only
Miranda-Moreno et al. 2011	Signalised junctions (urban)	Included	Type 4: Several independent variables; standard errors derived from p-values
Schepers et al. 2011	Junctions (mostly suburban)	Included	Type 4: Several independent variables; standard errors reported
Schepers 2012	Dutch municipalities	Excluded	Contains data on cyclist volume only
Buch and Jensen 2013	Junctions (mixed rural and urban)	Included	Type 4: Several independent variables; standard errors reported
Elvik, Sørensen and Nævestad 2013	Pedestrian crossings (urban)	Included	Type 4: Several independent variables; standard errors reported
Schepers and Heinen 2013	Dutch municipalities	Included	Type 4: Several independent variables; standard errors reported
Nordback, Marshall and Janson 2014	Signalised junctions (urban)	Included	Type 4: Several independent variables; standard errors reported
Prato et al. 2014	Urban traffic zones	Included	Type 4: Several independent variables; standard errors reported

Table 2:

		Estimated regression coefficients			
Model	**Variables included in model**	**Motor vehicles**	**Cyclists**	**Motor vehicles**	**Pedestrians**
	Models developed by Summersgill and Layfield (1996)				
1	Motor vehicles, pedestrians (volume only)			0.75	0.51
2	Motor vehicles, pedestrians, one-way street, speed limit, marked crossing, signalised crossing, one interaction term			0.72	0.44
	Models developed by Jonsson (2005)				
1	Motor vehicles, land use, street function, speed limit (cyclist model), type of crossing (pedestrian model)	0.93		0.92	
2	Cyclist volume only or pedestrian volume only		0.47		0.66
3	Motor vehicles and cyclists or motor vehicles and pedestrians (volume only models)	0.52	0.42	0.53	0.60
4	Motor vehicles, cyclists, land use, street function, visibility (cyclist model)	0.76	0.35		
5	Motor vehicles, pedestrians, land use, street function, existence of formal crossing, junctions per km (pedestrian model)			0.83	0.38
6	Motor vehicles, cyclists (cyclist model), pedestrians (pedestrian model), speed limit	0.59	0.39	0.50	0.59
7	Motor vehicles, cyclists (cyclist model), pedestrians (pedestrian model), actual mean speed	0.56	0.35	0.67	0.41
8	Motor vehicles, cyclists, land use, street function, actual speed with parameter pre-set to 2 (cyclist model)	0.74	0.41		
9	Motor vehicles, pedestrians, land use, street function, actual speed with parameter pre-set to 2 (pedestrian model)			0.73	0.59
	Models developed by Turner et al. (2006)				
1	Motor vehicles, cyclists (volume only)	0.29	0.09		
2	Motor vehicles, cyclists, lane width	0.28	0.08		
3	Motor vehicles, pedestrians (volume only)			0.63	0.40
4	Motor vehicles, pedestrians, compliance with traffic signals			0.80	0.63

Table 2, continued:

		Estimated regression coefficients			
Model	Variables included in model	Motor vehicles	Cyclists	Motor vehicles	Pedestrians
	Models developed by Miranda-Moreno et al. (2011)				
1	Motor vehicles, cyclists (total volume only)	0.34	0.44		
2	Motor vehicle flow by turning movements, cyclists (volume only)	0.46	0.44		
3	Motor vehicle flow by turning movements, cyclists, number of legs in junction, parking regulation, presence of median, bus stops	0.40	0.44		
	Models developed by Schepers et al. (2011)				
1	Motor vehicles, cyclists, two-way or one-way cycle track, separation distance, use of red colour, raised bicycle crossing, visibility, number of lanes, left-turn lane, number og legs in junction	0.73	0.48		
2	Motor vehicles, cyclists, type of cycle facility, use of red pavement, type of road markings	0.70	0.44		
	Models based on data analysed by Elvik et al. (2013)				
1	Motor vehicles only			0.65	
2	Pedestrians only				0.24
3	Motor vehicles, pedestrians (volume only)			0.59	0.20
4	Motor vehicles, pedestrians, number of lanes			0.60	0.20
5	Motor vehicles, pedestrians, number of lanes, number of legs in junction			0.59	0.20
6	Motor vehicles, pedestrians, number of lanes, number of legs in junction, traffic signals			0.60	0.20
7	Motor vehicles, pedestrians, number of lanes, number of legs in junction, traffic signals, percentage crossing outside			0.61	0.23
8	Motor vehicles, pedestrians, number of lanes, number of legs in junction, traffic signals, percentage crossing outside, mean speed			0.55	0.25
9	Motor vehicles, pedestrians, number of lanes, number of legs in junction, traffic signals, percentage crossing outside, mean speed, warrant for pedestrian crossing			0.55	0.24

Table 3:

				Coefficients (standard errors in parentheses)			
Study	**Country**	**Number of locations**	**Number of accidents**	**Motor vehicles**	**Cyclists**	**Pedestrians**	**Confounders controlled**
Inwood, Grayson 1979	Great Britain	140	166	0.92 (0.224)		0.27 (0.097)	3
Inwood, Grayson 1979	Great Britain	140	55	0.58 (0.260)		0.79 (0.138)	3
Hall 1986	Great Britain	177	510	1.27 (0.080)		0.18 (0.030)	0
Brüde, Larsson 1993	Sweden	377	432	0.52	0.65		0
Brüde, Larsson 1993	Sweden	285	165	0.50		0.72	0
Summersgill, Layfield 1996	Great Britain	970	693	0.72 (0.082)		0.44 (0.035)	5
Leden 2002	Canada	749	39	0.86		0.48	0
Leden 2002	Canada	126	27	1.19		0.33	0
Lyon, Persaud 2002	Canada	684	5280	0.57 (0.063)		0.74 (0.027)	0
Lyon, Persaud 2002	Canada	263	1065	0.40 (0.157)		0.41 (0.049)	0
Lyon, Persaud 2002	Canada	122	159	0.53 (0.137)		0.66 (0.100)	0
Lyon, Persaud 2002	Canada	123	319	0.58 (0.164)		0.71 (0.075)	0
Jonsson 2005	Sweden	393	143	0.76 (0.154)	0.35 (0.064)		3
Jonsson 2005	Sweden	393	130	0.83 (0.216)		0.38 (0.091)	3
Zegeer et al. 2005	United States	1000	188	1.01 (0.184)		0.38 (0.065)	1
Zegeer et al. 2005	United States	1000	41	0.30 (0.258)		0.60 (0.134)	1
Geyer et al. 2006	United States	247	185	0.15 (0.122)		0.61 (0.115)	2
Turner et al. 2006	New Zealand	446	61	0.29	0.09		1
Turner et al. 2006	New Zealand	351	35	0.36	0.20		1
Turner et al. 2006	New Zealand	176	52	0.80		0.63	1
Turner et al. 2006	New Zealand	351	39	0.56		0.46	1
Harwood et al. 2008	United States	450	728	0.05		0.41 (0.040)	2
Harwood et al. 2008	United States	1433	4824	0.40 (0.060)		0.45 (0.020)	2

Table 3, continued:

				Coefficients (standard errors in parentheses)			
Study	**Country**	**Number of locations**	**Number of accidents**	**Motor vehicles**	**Cyclists**	**Pedestrians**	**Confounders controlled**
Miranda-Moreno et al. 2011	Canada	753	787	0.40 (0.117)	0.44 (0.117)		4
Schepers et al. 2011	Netherlands	490	183	0.73 (0.112)	0.48 (0.125)		8
Schepers et al. 2011	Netherlands	524	156	0.50 (0.151)	0.56 (0.102)		3
Buch, Jensen 2013	Denmark	332	191	0.27 (0.115)	0.34 (0.099)		4
Buch, Jensen 2013	Denmark	709	305	0.32 (0.110)	0.39 (0.115)		4
Elvik 2013	Norway	159	316	0.59 (0.132)		0.31 (0.077)	6
Schepers, Heinen 2013	Netherlands	387	412	0.62 (0.107)	0.26 (0.097)		2
Schepers, Heinen 2013	Netherlands	387	7411	0.55 (0.059)	0.44 (0.051)		2
Nordback et al. 2014	United States	105	198	0.64 (0.170)	0.53 (0.140)		0
Nordback et al. 2014	United States	106	285	0.58 (0.130)	0.65 (0.110)		0
Prato et al. 2014	Denmark	289	5349	0.35 (0.088)	0.67 (0.036)		16

Table 4:

		Mean values of coefficients (95% confidence limits in parentheses)		
Method of synthesis	**Level of study**	**Motor vehicle volume**	**Cyclist volume**	**Pedestrian volume**
Simple mean (not weighted)	All	0.579 (0.488; 0.670)	0.431 (0.339; 0.523)	0.498 (0.422; 0.574)
Mean weighted by number of accidents	All	0.494 (0.483; 0.505)	0.519 (0.503; 0.535)	0.545 (0.529; 0.561)
Fixed-effects meta-analysis mean	All	0.573 (0.531; 0.615)	0.516 (0.473; 0.560)	0.460 (0.438; 0.482)
Random-effects meta-analysis mean	All	0.499 (0.383; 0.615)	0.432 (0.333; 0.530)	0.511 (0.395; 0.627)
Simple mean (not weighted)	Micro	0.567 (0.459; 0.675)	0.432 (0.318; 0.546)	0.508 (0.426; 0.590)
Mean weighted by number of accidents	Micro	0.517 (0.501; 0.533)	0.472 (0.429; 0.515)	0.551 (0.534; 0.568)
Fixed-effects meta-analysis mean	Micro	0.570 (0.519; 0.621)	0.479 (0.395; 0.563)	0.464 (0.441; 0.488)
Random-effects meta-analysis mean	Micro	0.563 (0.421; 0.706)	0.479 (0.395; 0.563)	0.491 (0.376; 0.607)
Simple mean (not weighted)	Meso	0.665 (0.453; 0.877)	0.510 (0.196; 0.824)	0.408 (0.353; 0.463)
Mean weighted by number of accidents	Meso	0.409 (0.384; 0.434)	0.661 (0.635; 0.687)	0.426 (0.358; 0.494)
Fixed-effects meta-analysis mean	Meso	0.590 (0.485; 0.696)	0.592 (0.530; 0.654)	0.428 (0.364; 0.492)
Random-effects meta-analysis mean	Meso	0.640 (0.381; 0.898)	0.514 (0.201; 0.826)	0.428 (0.364; 0.492)
Simple mean (not weighted)	Macro	0.585 (0.516; 0.654)	0.350 (0.174; 0.526)	No results
Mean weighted by number of accidents	Macro	0.554 (0.532; 0.576)	0.431 (0.408; 0.454)	No results
Fixed-effects meta-analysis mean	Macro	0.566 (0.465; 0.668)	0.401 (0.313; 0.489)	No results
Random-effects meta-analysis mean	Macro	0.566 (0.465; 0.668)	0.369 (0.196; 0.541)	No results