Cycling infrastructure for reducing cycling injuries in cyclists (Review)

Mulvaney CA, Smith S, Watson MC, Parkin J, Coupland C, Miller P, Kendrick D, McClintock H

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[Intervention Review]

Cycling infrastructure for reducing cycling injuries in cyclists

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ABSTRACT

Background

Cycling is an attractive form of transport. It is beneficial to the individual as a form of physical activity that may fit more readily into an individual's daily routine, such as for cycling to work and to the shops, than other physical activities such as visiting a gym. Cycling is also beneficial to the wider community and the environment as a result of fewer motorised journeys. Cyclists are seen as vulnerable road users who are frequently in close proximity to larger and faster motorised vehicles. Cycling infrastructure aims to make cycling both more convenient and safer for cyclists. This review is needed to guide transport planning.

Objectives

To:

1. evaluate the effects of different types of cycling infrastructure on reducing cycling injuries in cyclists, by type of infrastructure;

2. evaluate the effects of cycling infrastructure on reducing the severity of cycling injuries in cyclists;

3. evaluate the effects of cycling infrastructure on reducing cycling injuries in cyclists with respect to age, sex and social group.

Search methods

We ran the most recent search on 2nd March 2015. We searched the Cochrane Injuries Group Specialised Register, CENTRAL (*The Cochrane Library*), MEDLINE (OvidSP), Embase Classic + Embase(OvidSP), PubMed and 10 other databases. We searched websites, handsearched conference proceedings, screened reference lists of included studies and previously published reviews and contacted relevant organisations.

Selection criteria

We included randomised controlled trials, cluster randomised controlled trials, controlled before-after studies, and interrupted time series studies which evaluated the effect of cycling infrastructure (such as cycle lanes, tracks or paths, speed management, roundabout design) on cyclist injury or collision rates. Studies had to include a comparator, that is, either no infrastructure or a different type of infrastructure. We excluded studies that assessed collisions that occurred as a result of competitive cycling.

Data collection and analysis

Two review authors examined the titles and abstracts of papers obtained from searches to determine eligibility. Two review authors extracted data from the included trials and assessed the risk of bias. We carried out a meta-analysis using the random-effects model where at least three studies reported the same intervention and outcome. Where there were sufficient studies, as a secondary analysis we accounted for changes in cyclist exposure in the calculation of the rate ratios. We rated the quality of the evidence as 'high', 'moderate', 'low' or 'very low' according to the GRADE approach for the installation of cycle routes and networks.

Main results

We identified 21 studies for inclusion in the review: 20 controlled before-after (CBA) studies and one interrupted time series (ITS) study. These evaluated a range of infrastructure including cycle lanes, advanced stop lines, use of colour, cycle tracks, cycle paths, management of the road network, speed management, cycle routes and networks, roundabout design and packages of measures. No studies reported medically-attended or self-reported injuries. There was no evidence that cycle lanes reduce the rate of cycle collisions (rate ratio 1.21, 95% CI 0.70 to 2.08). Taking into account cycle flow, there was no difference in collisions for cyclists using cycle routes and networks compared with cyclists not using cycle routes and networks (RR 0.40, 95% CI 0.15 to 1.05). There was statistically significant heterogeneity between the studies ($I^2 = 75\%$, $Chi^2 = 8.00$ df = 2, P = 0.02) for the analysis adjusted for cycle flow. We judged the quality of the evidence regarding cycle routes and networks as very low and we are very uncertain about the estimate. These analyses are based on findings from CBA studies.

From data presented narratively, the use of 20 mph speed restrictions in urban areas may be effective at reducing cyclist collisions. Redesigning specific parts of cycle routes that may be particularly busy or complex in terms of traffic movement may be beneficial to cyclists in terms of reducing the risk of collision. Generally, the conversion of intersections to roundabouts may increase the number of cycle collisions. In particular, the conversion of intersections to roundabouts with cycle lanes marked as part of the circulating carriageway increased cycle collisions. However, the conversion of intersections with and without signals to roundabouts with cycle paths may reduce the odds of collision. Both continuing a cycle lane across the mouth of a side road with a give way line onto the main road, and cycle tracks, may increase the risk of injury collisions in cyclists. However, these conclusions are uncertain, being based on a narrative review of findings from included studies. There is a lack of evidence that cycle paths or advanced stop lines either reduce or increase injury collisions in cyclists. There is also insufficient evidence to draw any robust conclusions concerning the effect of cycling infrastructure on cycling collisions in terms of severity of injury, sex, age, and level of social deprivation of the casualty.

In terms of quality of the evidence, there was little matching of intervention and control sites. In many studies, the comparability of the control area to the intervention site was unclear and few studies provided information on other cycling infrastructures that may be in place in the control and intervention areas. The majority of studies analysed data routinely collected by organisations external to the study team, thus reducing the risk of bias in terms of systematic differences in assessing outcomes between the control and intervention groups. Some authors did not take regression-to-mean effects into account when examining changes in collisions. Longer data collection periods pre- and post-installation would allow for regression-to-mean effects and also seasonal and time trends in traffic volume to be observed. Few studies adjusted cycle collision rates for exposure.

Authors' conclusions

Generally, there is a lack of high quality evidence to be able to draw firm conclusions as to the effect of cycling infrastructure on cycling collisions. There is a lack of rigorous evaluation of cycling infrastructure.

P L A I N   L A N G U A G E   S U M M A R Y

Cycling infrastructure (changes to the road environment) for reducing cycling injuries in cyclists

Review question

This review aimed to answer the question "what effect do different types of cycling infrastructure have on cycling injuries and collisions? ". Cycling infrastructure involves changes which are made to the road design or management of the road for cyclists. We aimed to include studies which looked at the effects of three types of cycling infrastructure:

1. that which aims to manage the shared use of the road space for both motor vehicles and cyclists, for example, cycle lanes and shared use of a bus lane;

2. that which separates cycle traffic from motorised traffic and may include special routes just for cycle traffic, for example, cycle tracks and cycle paths. These may be shared with pedestrians;

3. management of the roads to include separation of motor vehicle and cycle traffic (for example, traffic rules that ban certain types of traffic from making particular turns) and cycle turns at traffic signals.

Comparisons were made with either routes or crossings that either did not have cycling infrastructure in place or had a different type of infrastructure. We were interested in studies with both adults and children. The primary outcome of interest was cycling injuries suffered as a result of a cycling collision. Secondary outcomes were collision rates for cyclists; and cycle counts, that is the number of cyclists using the infrastructure.

Background

Cycling infrastructure involves making changes to the road environment to provide special facilities for cyclists. These may include putting in cycle lanes or giving cyclists right of way at junctions, or separating cyclists from fast-moving or high-volume traffic. Speed limits may be introduced which means cyclists share the road with vehicles moving more slowly. This review is important because if we want to get more people cycling, we need to know whether cycling infrastructure helps to keep cyclists safe.

Search date

We searched world-wide research literature up to March 2015.

Study characteristics

The types of studies that could be included in this review are randomised controlled trials, cluster randomised controlled trials, controlled before-after studies, and interrupted times series studies. We found 21 studies looking at the effects of 11 different types of cycling infrastructure. No studies reported self-reported injuries or medically attended injuries. Fourteen studies reported police-reported ‘cycle crashes’ or ‘accidents’ or ‘injury crashes’ and the other studies reported outcomes such as number of “cycle accidents” or “crashes involving cyclists”. Nine studies reported collisions by severity; seven studies reported on age of casualty; and two studies reported on sex. One study reported on the level of social deprivation. Cycle flow was collected in 14 studies.

Key results

Generally we found a lack of evidence that the types of cycling infrastructure we looked at affects injuries or collisions in cyclists. Cycle routes and networks do not seem to reduce the risk of collision. Speed limits of 20 mph, changing parts of the road network to some designs of roundabouts and changing busy parts of a cycle route may reduce the risk of collision. In terms of severity of injury, sex, age and level of social deprivation of the area, there is a lack of evidence to draw any conclusions concerning the effect of cycling infrastructure on cycling collisions.

Quality of the evidence

We carried out a thorough search for relevant papers. The quality of the evidence was low with 20 of the included 21 studies using a controlled before-after study design. Few studies considered how factors such as weather and volume of traffic may affect collision rates. Few studies considered how changes in cycle rates seen as a result of installing infrastructure may affect changes in collision rates.

SUMMARY OF FINDINGS FOR THE MAIN COMPARISON [Explanation]

Cycle routes and networks for the prevention of cycling injuries in cyclists

Patient or population: cyclists
Settings: road environment
Intervention: cycle routes and networks
Comparison: no cycle routes or network

Outcomes	Illustrative comparative risks* (95% CI)		Relative effect (95% CI)	Number of sites (studies)	Quality of the evidence (GRADE)	Comments
	Assumed risk	Corresponding risk				
	No cycle routes or network	Cycle routes and networks				
Self-reported injuries or medically-attended injuries						No data available
Collision rates for cyclists Follow-up: 1 to 2 years	The mean collision rates for cyclists in the control groups was **0.024 collisions per 100 cyclists in one year**[1]	The mean collision rates for cyclists in the intervention groups was **0.016 lower** (0.001 to 0.025 higher)	Rate ratio 0.68 (95% CI 0.31 to 1.47)	18 (4 studies[2])	⊕○○○ **very low**[3]	

*The basis for the **assumed risk** (e.g. the median control group risk across studies) is provided in footnotes. The **corresponding risk** (and its 95% confidence interval) is based on the assumed risk in the comparison group and the **relative effect** of the intervention (and its 95% CI).
CI: Confidence interval;

GRADE Working Group grades of evidence
High quality: Further research is very unlikely to change our confidence in the estimate of effect.
Moderate quality: Further research is likely to have an important impact on our confidence in the estimate of effect and may change the estimate.
Low quality: Further research is very likely to have an important impact on our confidence in the estimate of effect and is likely to change the estimate.
Very low quality: We are very uncertain about the estimate.

[1] Based on accident data in 3 control sites prior to opening the cycle route, taken from Harland 1993. Data taken from the route after it was opened for use has not been used, as it is likely to be affected by changes in cycling rates/behaviour as a result of opening of the cycle route.

[2] Based on data from 9 cycle routes/networks and 9 control groups/comparison areas (6 interventions from Harland 1993, 1 each from Carlson 1975, Nicholson 1979 and Quenault 1981 and corresponding control groups).

[3] Evidence downgraded one level from low quality to very low quality due to serious risk of bias, inconsistency in results (I^2 = 71%).

BACKGROUND

Description of the condition

Cycling is promoted widely as a form of physical activity for improving the public's health (NICE 2012), and is an attractive form of transport and of exercise, for several reasons. Firstly, cycling is an activity that may fit more readily into an individual's daily routine: for example, cycling to work or to the shops, as opposed to finding time to visit the gym (Hillsdon 1996; Cavill 2008). Secondly, while individual cyclists enjoy the consequent health benefits of cycling (Oja 2011), the wider general public also benefit as a result of fewer car journeys resulting in reduced emissions (Lindsay 2011), and improvement in the local environment through reduced congestion and community severance (McClintock 2002). Thirdly, cycling is a relatively cheap form of transport. Therefore, it may be accessible to people who are socially disadvantaged and are less likely to have access to a car. Physical activity has many health benefits including a reduced risk of cardiovascular disease (Schnohr 2006; Oja 2011), cancer (Schnohr 2006; Inoue 2008) and type 2 diabetes (Hu 1999; Pucher 2010). Physical activity is also beneficial for psychological health, reducing depressive symptoms in people with depression (Cooney 2013). Yet data for England indicates that fewer than half of all adults and less than a third of children meet the recommended guidelines of physical activity levels (Department of Health 2011).

The physical environment is known to play an important role in influencing levels of cycling (NICE 2008; Fraser 2010). Cyclists are seen as vulnerable road users who are frequently in close proximity to larger and faster motorised vehicles which offer the occupants some protection if a collision occurs, unlike the cyclist. Cyclists report fear of injury from lack of segregated cycling routes, the volume and speed of traffic and driver behaviour (TfL 2008). In 2013, in England there were 109 pedal cyclist fatalities and 3143 reported seriously injured casualties (DfT 2014). Inequalities exist in cyclist injuries with a risk of cyclist injury being 20% to 30% higher in lower socioeconomic groups than higher socioeconomic groups (Hasselberg 2001; Engström 2002). In 2008, cyclist casualty rates were around 29 per 100,000 in the most deprived 10% of areas of England compared to 20 per 100,000 in the least deprived (DfT 2010). Thus to maximise the public health benefits of increased cycling it is necessary to minimise the risk of cycling injuries and people's fear of cycling. For this review 'cycling' refers to two-wheeled, non-powered cycling and includes commuter, work and leisure cycling.

Description of the intervention

One key approach to reducing the fear and risk of injury for vulnerable road users such as cyclists is through engineering and, in particular, through transport infrastructure. Transport infrastructure refers to physical measures within the built environment that are in place to enable traffic to flow safely and thus allow society to function fully. Transportation infrastructure generally develops over time and is frequently designed with the needs of the motorised-vehicle user being of most importance (WHO 2004). Within this, infrastructure specific to cycling includes measures to manage cycle traffic and motorised traffic, to varying degrees, in mixed traffic conditions. It generally takes one of three main forms. Firstly, there is infrastructure that manages the road space for shared use by both motor vehicles and cyclists, and this includes cycle lanes within the carriageway. Secondly, there is cycling infrastructure that separates cycle traffic from motorised traffic. This may include special routes for use exclusively by cyclists but which may also be shared with pedestrians. Management of the traffic network represents a third form of cycling infrastructure and includes traffic regulations that ban certain types of traffic from making particular turns and imposed speed management.

How the intervention might work

The role of infrastructure in reducing the fear of cycling is evidenced by research that has found that changes in infrastructure can positively influence cycling rates with cyclists choosing to use routes serviced by cycle facilities (Garrard 2008; Winters 2010; Yang 2010). In terms of injury prevention, research also indicates that infrastructure may be effective at reducing injuries (Rodgers 1997; Moritz 1998; Lusk 2011b). Reducing the risk of cycling injury may also reduce the social inequalities seen in cycle injuries. As an injury prevention strategy, cycling infrastructure is particularly potent for several reasons. Firstly, it is population based and thus can reach large numbers of the population; secondly its mainly passive mode requires no actions from individuals; and thirdly, changes are made only once, thus requiring no reinforcement (Reynolds 2009).

Why it is important to do this review

The promotion of cycling and walking is highly topical with recently published reports on schemes to promote cycling in the United Kingdom (UK) (TfL 2008; Sloman 2009). With much on-going research in this area new results are frequently being published. There have been a number of literature reviews and summaries of evidence of the effectiveness of cycling infrastructure at reducing cycling injuries (Clarke 1995; Reynolds 2009; Pucher 2010; Reid 2010; Karsch 2012; Road Safety Observatory 2013; Thomas 2013). Few have performed a systematic search of the literature or have focused on study designs which include a control group; and, to our knowledge, none have pooled study findings. There is no Cochrane review of this topic as previous cycling-related Cochrane reviews have focused on the use of cycle helmets

(Thompson 1999; Macpherson 2008), and cyclist visibility (Kwan 2006) to reduce cyclist injuries. The recent review of measures to promote cycling and walking by NICE did not assess infrastructure (NICE 2012). There is, therefore, an urgent public health need for a Cochrane review to assess the effectiveness of cycling infrastructure on cycling injuries and to identify types of cycling infrastructure which are most effective at reducing injuries.

OBJECTIVES

The objectives of this review are to:

1. evaluate the effects of different types of cycling infrastructure on reducing cycling injuries in cyclists, by type of infrastructure;

2. evaluate the effects of cycling infrastructure on reducing the severity of cycling injuries in cyclists;

3. evaluate the effects of cycling infrastructure on reducing cycling injuries in cyclists with respect to age, sex and social group.

METHODS

Criteria for considering studies for this review

Types of studies

Studies were considered for inclusion in the review if they were one of the following study types:

- randomised controlled trials (RCT)
- cluster randomised controlled trials
- controlled before-after studies (CBA)
- interrupted time series studies (ITS).

Types of participants

Studies were included which involved adult or child cyclists, or both. A number of included studies present data on age and sex of cyclist casualties; we have indicated these studies in Characteristics of included studies under 'Participants'.

For this review, under 'Participants' we also describe intervention areas where cycling infrastructure has been implemented, including stretches of road, junctions, roundabouts and routes, and control areas with either no infrastructure or a different type of infrastructure, and within which injuries and collisions may occur. Where the term 'casualty' is used, this refers to someone who was either injured or killed.

Types of interventions

Interventions included in the review took one of three main forms.

1. Cycling infrastructure that aims to manage the shared use of the road space for both motor vehicles and cyclists, including:

i) cycle lanes - these are part of the road (carriageway) and are indicated, often by a white line and a cycle icon painted on the lane, and appropriate signage. Cycle lanes are to be used exclusively by cyclists. This includes contraflow cycle lanes where two-way cycling is allowed on a street that allows motorised traffic to travel only one way. Cycle lanes may be advisory or mandatory. If mandatory, motor traffic is excluded by regulation;

ii) shared use of a bus lane - these are often defined by appropriate road markings and signage;

iii) advanced stop lines (ASLs) - provide a second stop line at traffic signals beyond that of the regular stop line at a junction and, extending across the width of the road, they allow cyclists to wait in front of the queuing traffic while the signal is red and to leave the intersection ahead of the motorised traffic when the signal turns green. The area between the two lines may be surfaced with a coloured material. Cyclists access the area by use of a cycle lane filtering cycle traffic. An ASL may also be called an advanced stop box or bike box.

iv) cycle routes - cyclists share the road with motorised vehicles but the route is signed as a preferred route and may avoid particularly busy roads;

v) any of the above where the lanes and stop lines have been painted in colour to make them more noticeable to other road users.

2. Cycling infrastructure which separates cycle traffic from motorised traffic and may include special routes exclusively for cycle traffic, but which may be shared with pedestrians either in mixed or segregated conditions. These include:

i) cycle tracks - these lie alongside a road but cyclists are separated from motorised vehicles perhaps by a kerb or other physical barrier such as bollards. They may be one-way or, more frequently, two-way;

ii) cycle paths - these are paths which are separate from the road and may be marked to segregate cycle traffic from pedestrian traffic, or they may be shared with pedestrians.

3. Management of the transport network represents a third form of cycling infrastructure. This includes:

i) separation of traffic movements - through direction signage differentiated by vehicle type, or through regulatory means: for example, traffic regulations that ban certain types of traffic from making particular turns;

ii) cycle phases at traffic signals - these operate at a junction and allow cyclists to cross an intersection at a separate time from motorised vehicles;

iii) speed management - achieved either by physical measures, such as the use of narrowed roads or speed bumps, or by the imposition of speed limits including widespread 20 mph zones.

Examples of cycling infrastructure may be placed on continuous sections of roads or at intersections. In some situations there will be more than one infrastructure feature in place and different examples of cycling infrastructure may be installed as appropriate over a larger geographical area such as in the case of cycle routes and networks. The effect of individual features will be determined where possible.

The terminology for cycle infrastructure varies across continents and so we have categorised the type of infrastructure installed based on the authors' description of the infrastructure, rather than just relying on the authors' terminology.

Types of outcome measures

Primary outcomes

Studies were eligible for inclusion if they included a measure of injuries sustained as a result of cycling, that is, either self-reported or medically attended injuries.

Secondary outcomes

Secondary outcomes of interest were:

- collision rates for cyclists, expressed as collisions per million bike-km.
- cycle counts, as cycling infrastructure may promote cycling thus benefiting the wider public health.

The terms 'crash', 'collision' and 'accident' are used interchangeably in the literature but we have used the term 'collision' in this review as this is the word currently used in UK road safety engineering practice. Collisions may involve another vehicle and may not necessarily result in injury. In Characteristics of included studies we have replicated the terms used by the authors in their original article.

We included injuries and collisions sustained as a result of maintenance issues of infrastructure, such as uneven surfaces. We did not include studies that reported only injuries sustained while racing; mountain biking (that is, cycling off-road, frequently over rough terrain); or playing (that is, cycling a non-directional course).

Search methods for identification of studies

In order to reduce publication and retrieval bias we did not restrict our search by language, date or publication status.

Search strategies were developed iteratively. Results from the initial search were assessed to determine whether the strategy identified relevant reports and were adjusted as required.

For the website searches and some of the smaller databases, the search terms were wide to increase the sensitivity of the search. The search strategy, which was formulated in MEDLINE, was adapted as necessary for use in each of the other databases. All search strategies are reported in Appendix 1.

We undertook original searches in 2013 and reran the searches in 2015 when some changes were made to the databases and sites searched as detailed below. GEOBASE was dropped from the updated search (2015) as the original search only identified one relevant paper, among a large number of irrelevant papers, that was also found from searches of other databases.

Electronic searches

The Injuries Group Trials Search Co-ordinator searched the following electronic databases:

1. Cochrane Injuries Group Specialised Register (2 March 2015);
2. Cochrane Central Register of Controlled Trials (CENTRAL) (*The Cochrane Library*, 2 March 2015);
3. MEDLINE (OvidSP) (1946 to 2 March 2015);
4. PubMed (www.ncbi.nlm.nih.gov/sites/entrez/) (2 March 2015);
5. Embase Classic and Embase (OvidSP) (1947 to 2 March 2015);
6. ISI Web of Science: Science Citation Index Expanded (SCI-EXPANDED) (1970 to 2 March 2015);
7. ISI Web of Science: Conference Proceedings Citation Index-Science (CPCI-S) (1990 to 2 March 2015).

The review authors searched the following electronic databases:

1. OpenSIGLE (System for Information on Grey Literature in Europe) (http://opensigle.inist.fr/) (1 February 2015);
2. GEOBASE (1980 to 8 April 2013);
3. Index to Theses (1970 to 25 February 2013), replaced by EThOS (e-theses online service) (http://ethos.bl.uk/Home.do) (2013 to 14 March 2015);
4. SafetyLit (1800 to 3 Febuary 2015);
5. Institution of Civil Engineers virtual library (1836 to 18 February 2015);
6. TRANweb (1976 to 3 February 2015);
7. Transport Research International Documentation (TRID) (1923 to 18 February 2015);
8. Transport Research Laboratory database (1966 to 18 February 2015);
9. Sustrans database (1972 to 18 March 2013 and website 2013 to 18 February 2015).

Searching other resources

A systematic search of the Internet was performed, recording details of websites searched, the date, search terms used and results. The following websites were searched in February and March 2013 and searched again in February and March 2015:

- Pedestrian and Bicycle Information Center (www.bicyclinginfo.org/);
- Cycling Embassy of Great Britain (www.cycling-embassy.org.uk/);

- AAA Foundation for Traffic Safety (www.aaafoundation.org/);
- Australian Road Research Board (www.arrb.com.au/home.aspx);
- Swedish National Road and Transport Research Institute (www.vti.se/en/);
- Transport Canada (www.tc.gc.ca/eng/menu.htm);
- Transportation Research Board (www.trb.org/Main/Home.aspx);
- Injury Control Resource Information Network (www.injurycontrol.com/icrin/);
- Harborview Injury Prevention and Research Center (http://depts.washington.edu/hiprc/);
- CTC: the national cycling charity (UK) (www.ctc.org.uk/);
- American Society of Civil Engineers (www.asce.org/);
- Google Scholar (http://scholar.google.co.uk/).

The Swedish National Road and Transport Research Institute was not searched in the updated search as a large number of studies were found in the original search but none were of relevance.
Abstracts of the following conferences were searched by hand:

- World Conference on Injury Prevention and Safety Promotion (1st to 11th);
- Australian Cycling Conference (1st to 5th);
- NZ Cycling Conference (1st to 8th). Now the 2WALKandCYCLE conference (2012 and 2014)
- Velo-city Conference (1980 to 2014).

The following organisations in the UK were contacted by e-mail in March 2013 to ask if their members could identify any unpublished or ongoing relevant work.

- Universities' Transport Study Group: approximately 1250 members
- Cycling and Society Research Group: approximately 350 members
- Local Authorities Cycle Planning Group: approximately 350 members
- London Cycling Research Group: approximately 20 members.

Additionally, the reference lists of included studies were searched, as well as reference lists of previously published reviews. Key individuals were contacted, such as those who have previously published relevant work and interest groups to ask if they could identify any unpublished or ongoing research. Contacts of team members were asked for information on any relevant studies.

Data collection and analysis

The search results were imported into a reference management program and duplicates removed. Two authors (CM and SS) independently screened titles and abstracts of articles for relevance according to the pre-determined criteria for study inclusion. Details of papers retained at this stage were entered into a database and full text reports were sought.

Selection of studies

We attempted to retrieve full text reports retained at this stage. Full text reports were sought by requesting interlibrary loans; searching the Internet; contacting authors by e-mail and where no reply was received, sending a reminder; using contacts of review authors and asking staff from Cochrane centres in other countries to search their resources, particularly for papers published in a non-UK database or website, or not published in English. Where it was not possible to obtain the full paper, screening for inclusion was done on the title/abstract. For 26 papers published in a language other than English we sought speakers of each language from staff based in Cochrane centres in the relevant countries who screened the papers for inclusions, and extracted data if necessary.
Two authors independently assessed full text papers for eligibility using the inclusion criteria. Reasons for exclusion were based on the following hierarchy:

- was not an intervention of interest
- was not a study design of interest
- did not report outcomes of interest
- did not provide sufficient information to make a decision.

Papers were excluded according to the first reason encountered in the hierarchy. Disagreement was dealt with by deferment to a third author (JP, MW, CC, PM). If the decision remained unclear, advice was sought from team members and the Cochrane Injuries Group. Reasons for exclusion of a paper were recorded. Papers retained at this second review stage are included studies in the final review.

Data extraction and management

Two authors independently extracted data using a data extraction form designed for this review and pre-tested. Authors extracted data on:

- study design;
- year of study;
- country of origin;
- proximity of control areas to intervention areas and any matching undertaken;
- characteristics and number of intervention and control areas such as urban or rural environment, residential or commercial or industrial or educational, higher or lower capacity roads in terms of vehicle numbers;
- nature of the intervention such as length of cycle lane, position at an intersection or continuous road;
- details of the comparator such as no intervention or a different type of infrastructure;
- length of follow up for data collection;

- data on outcome measures of interest;
- author's conclusions.

Where data were missing or needed to be queried, authors were contacted in an effort to obtain the relevant information. Again, any disagreements on data were deferred to a third author (CC or DK).

Assessment of risk of bias in included studies

Two authors independently undertook critical appraisal of the included studies to assess their quality. For RCTs we planned to assess the following sources of risk of bias:

- random sequence generation (selection bias);
- allocation concealment (selection bias);
- blinding (performance bias and detection bias);
- blinding of participants and personnel (performance bias);
- blinding of outcome assessment (detection bias);
- incomplete outcome data (attrition bias);
- selective reporting (reporting bias); other bias.

As no RCTs were included in the review we assessed quality based on the Cochrane Handbook approach described in section 13.5.2.1:

- how the intervention and control areas were matched based on the comparability of the control and intervention areas, assessed according to the extent to which control areas were chosen based on characteristics similar to those of the intervention area, and their proximity to each other, assessed according to whether the control area was adjacent or in close proximity to the intervention area or whether the control area was distant to the intervention area (selection bias);
- whether data collection and analyses were performed by a researcher blind to the study intervention and control areas, and source of outcome data; for example, a database maintained by an organisation external to the study team (detection bias);
- the length of time of data collection pre- and post-installation of the intervention with a minimum of one year considered acceptable (attrition bias);
- whether authors appeared to be selective in terms of the results they reported (reporting bias);
- whether there was an assessment of the distribution of confounders between treatment groups (bias due to confounding);
- any other potential sources of bias.

The review authors gave a brief description of possible sources of each type of bias for each included study and rated the risk of bias as high risk, low risk, or unclear. The findings of the two authors were examined for discrepancies and any discrepancies were resolved by deferment to a third review author (CC or DK). As all studies were CBAs, with one ITS, the results of the appraisal of quality has been presented graphically. An assessment of the quality of the data was made using Grading of Recommendations Assessment, Development and Evaluation guidelines (GRADE) (Guyatt 2008).

Measures of treatment effect

We planned to account for variations in exposure by express self-reported or medically-attended injuries as injuries per million bike-km, where sufficient data were provided. Alternatively, we planned to report the number of injuries per hour of cycle use or number of injuries per cyclist, depending on how injuries were reported in the included studies. The majority of studies we identified were controlled before and after studies. All studies reported collisions in intervention areas before and after the intervention was installed and for comparable time periods in control areas. Thus, for each study we calculated a rate ratio corresponding to the ratio of collision counts post- and pre-intervention in the intervention area(s) divided by the corresponding ratio in control area(s), i.e.: (collisions after collisions before in intervention area) (collisions after collisions before in control area). If changes in cycling exposure comparing post- to pre-intervention periods are the same in control and intervention areas, the rate ratio estimates the change in the collision rate in intervention areas compared to that in control areas. Some studies provided information on cyclist counts in intervention and control areas before and after the intervention was installed. Where there were sufficient studies, as a secondary analysis, we incorporated changes in cyclist exposure in the calculation of the rate ratios for collisions, to account for different changes in cycling exposure in intervention and control areas. To account for cyclist exposure in each study for which we had data, we multiplied the ratio of collisions by the percentage change in cycle flow in control areas (after versus before)/percentage change in flow in intervention areas (after versus before). For outcomes expressed as counts or rates we assessed treatment effect using rate ratios with a 95% confidence intervals (CI). For studies that provided data on subcategories of the road network, that is, intersections and road segments, we undertook statistical analysis for each subcategory. We produced a 'Summary of findings' table (Summary of findings for the main comparison), which examines data on collisions occurring on cycle routes and networks, prepared using GRADEpro (GRADEpro 2007).

Dealing with missing data

In our protocol we stated that we would assess the number of dropouts for each included study. However, the study designs of the included articles did not include individual study participants but rather counts of cyclist collisions at intervention and control sites so we were not able to assess the number of participant dropouts. We assessed completeness of data in terms of whether data were presented for all intervention and control sites and our findings are presented in the 'Risk of bias' tables.

Assessment of heterogeneity

For the studies that were combined in a meta-analysis, we assessed heterogeneity using forest plots; statistical tests of heterogeneity were undertaken using the Chi^2 test, with significance defined as a P value of < 0.1, and the I^2 statistic. I^2 values above 30% suggest that moderate heterogeneity exists. Too few studies were included in the meta-analyses to undertake subgroup analyses to explore possible reasons for heterogeneity.

Assessment of reporting biases

The meta-analyses we undertook did not use 10 or more studies and as a consequence we did not test for asymmetry using Egger's test nor did we assess publication bias by inspection of funnel plots for symmetry.

Data synthesis

Where three or more studies reported the same outcomes for the same type of infrastructure we performed a meta-analysis. We used random-effects models to combine the log rate ratios and their corresponding standard errors using the generic inverse variance method using Review Manager 5 (RevMan) (Review Manager 2014). Standard errors for logarithms of rate ratios, used to calculate 95% CIs for rate ratios, were calculated assuming that the number of collisions in each area in each period followed a Poisson distribution. Where there were insufficient studies to undertake a meta-analysis the results from individual studies have been combined in a narrative review and the key characteristics and findings of the studies are presented.

Subgroup analysis and investigation of heterogeneity

If three or more studies reported relevant data, we planned to undertake several subgroup analyses. Firstly, we aimed to consider the effectiveness of the infrastructure at reducing injuries of differing severity by undertaking a subgroup analysis according to fatal injury, serious injury and slight injury. Secondly, we aimed to undertake subgroup analyses comparing effect sizes between countries with and without cycle helmet legislation as compulsory wearing of a cycle helmet may affect the severity of injuries sustained. Thirdly, we aimed to evaluate the effectiveness of cycling infrastructure in reducing cycling injuries with respect to age, sex and social group and planned to undertake subgroup analyses based on age (child versus adult), sex (male versus female) and social group (disadvantaged versus non-disadvantaged). However, there were insufficient studies to undertake subgroup analyses and we have reported the findings narratively.

Sensitivity analysis

We planned to undertake sensitivity analyses by rerunning the analyses and including only RCTs considered to be at low risk of selection bias in terms of adequate allocation concealment, detection bias in terms of blinded outcome assessment and attrition bias due to follow-up of fewer than 80% of participants in each arm. However, there were too few studies in each meta-analysis to run sensitivity analyses based on risk of biases such as risk of selection bias, detection bias and attrition bias.

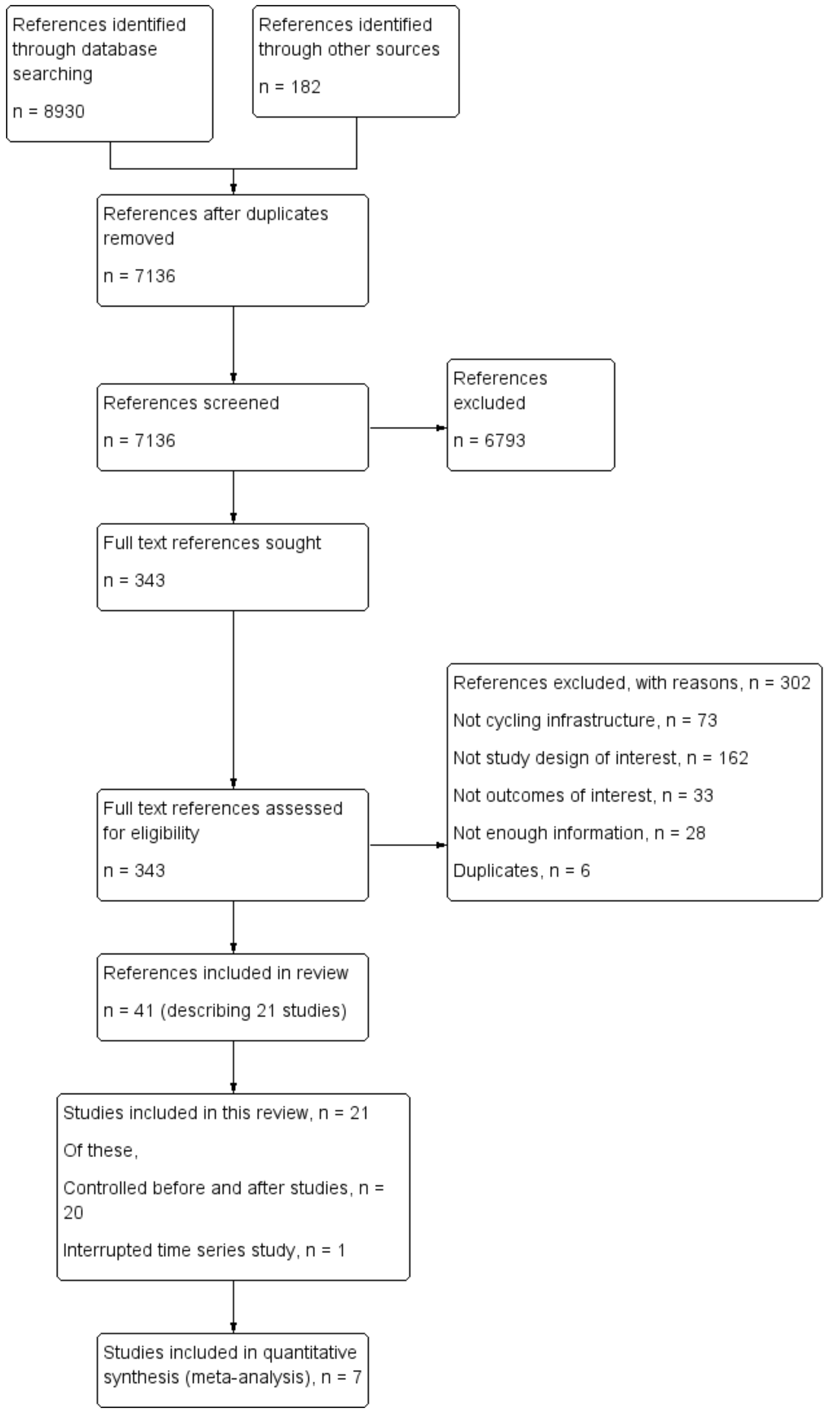
R E S U L T S

Description of studies

Results of the search

From our initial searches we found 8930 references from searching bibliographic databases. A further 182 references were found from other sources such as websites and conference abstracts resulting in a total of 9112 records (Figure 1). Following removal of duplicates, 7136 references remained, of which 6793 were excluded after preliminary screening of their titles and abstracts. We attempted to obtain the full text of the remaining 343 papers. We contacted 28 authors for further information and received responses from 11. Authors were unable to supply missing data but responded to requests for further details of the intervention or sent copies of papers. All 28 studies are listed in the Characteristics of excluded studies, with the explanation 'Not enough information available'. We assessed the 343 potentially relevant articles for eligibility. Of these, 302 were excluded. The remaining 41 papers describing 21 studies met our inclusion criteria and were included in the review (Characteristics of included studies). The PRISMA flow diagram (Figure 1) shows the process of study selection.

Figure 1. Study flow diagram.



Of the 21 studies included in this review, eight are reported in multiple references with, in some cases, the additional references providing further details on the methodology and intervention. Thus, the study by Bovy 1988 is described in three other included papers (Wilmink 1987; ten Grotenhuis 1989; van Goeverden 2011); the study by Daniels 2009a by three other papers (Daniels 2007; Daniels 2008; Daniels 2009); Gårder 1998 by three other papers (Leden 1997a; Leden 1997b; Leden 1997c); Grundy 2009 by three other papers (Grundy 2008a; Grundy 2008b; Steinbach 2011); and Jensen 1997 by one other paper (Nielsen 1996).
In addition, three of the four studies of cycle routes and networks are described in a total of 10 included papers. One paper by Harland 1993 presents the results from an evaluation of cycle routes in six cities but a further five papers also describe the study or present results for individual cities: Dean 1993 (Stockton only); Gercans 1991; Harbidge 1993 (Kempston only); Harland 1993; Shipley 1994 (Southampton only). The results from the study of the cycle route in Portsmouth are presented in one paper by Nicholson 1979 but a detailed description of the intervention is described in a second included paper (Quenault 1977). Quenault 1981 reports the findings from a study evaluating the Peterborough cycle route but further details of the evaluation are given in a second paper, the interim report (Quenault 1979). Where studies are described in more than one paper, where a secondary paper provides data not provided in the primary paper, then the secondary paper is referred to in the review where appropriate. The primary paper for each study is indicated by an asterisk in the Characteristics of included studies. Of the 343 potentially relevant papers, there were 26 papers which we could only find in a language other than English. We sought speakers of each language from staff based in Cochrane centres in the relevant countries who screened the papers for inclusion and extracted data if necessary. Of the included papers, three were published in a language other than English (Wilmink 1987; Bovy 1988; Nielsen 1996).

Included studies

Types of studies

Of the 21 included studies, 20 were controlled before-after studies (Carlson 1975; Nicholson 1979; Quenault 1981; Wheeler 1987; Bovy 1988; Smith 1988; Williams 1989; Harland 1993; Jensen 1997; Gårder 1998; Buckley 2000; Webster 2003; Allen 2005; Mountain 2005; König 2006; Agerholm 2008; Daniels 2009a; Chen 2012; Jensen 2013; Parsons 2013); and one was a controlled ITS study (Grundy 2009). No RCTs are included. Nine studies were from the UK (Nicholson 1979; Quenault 1981; Wheeler 1987; Williams 1989; Harland 1993; Webster 2003; Allen 2005; Mountain 2005; Grundy 2009); three from the United States (Carlson 1975; Smith 1988; Chen 2012); three from Denmark (Jensen 1997; Agerholm 2008; Jensen 2013); two from Sweden (Gårder 1998; König 2006); two from New Zealand (Buckley 2000; Parsons 2013); and one each from Belgium (Daniels 2009a); and the Netherlands (Bovy 1988).

Types of interventions

Four studies examined the safety effect of cycle lanes on cycle collisions (Smith 1988; Buckley 2000; Chen 2012; Parsons 2013). One study assessed the effects of advanced stop lines on cycle collisions (Allen 2005); and one study examined the use of coloured intersection crossings (König 2006). Two studies assessed the safety effects on cycle collisions of cycle tracks (Gårder 1998; Agerholm 2008); and one evaluated the safety effects on cycle collisions of cycle paths (Williams 1989). One study assessed the safety effects on cycle collisions of cycle lanes along main roads crossing side roads at signalised versus priority junctions (Jensen 1997). Three studies examined the effects of speed management on cycle collisions of 20 or 30 mph zones (Webster 2003; Mountain 2005; Grundy 2009). We identified three types of infrastructure which did not readily fit into our typology of infrastructures, as detailed in our description of interventions of interest (Criteria for considering studies for this review), because they did not examine only one type of infrastructure: we have called this group 'Combination of cycling infrastructure'. Five studies within this group assessed the safety effects of cycle routes and networks on cycle collisions where cycle routes and networks may include more than one type of infrastructure (Carlson 1975; Nicholson 1979; Quenault 1981; Bovy 1988; Harland 1993); for example, sharing the road with motorised vehicles and separate cycle paths. One study evaluated the introduction of a package of infrastructure measures, such as a white-line segregated cyclist or pedestrian paths and signal controlled cycle crossings, at two points on a cycle route (Wheeler 1987); and two studies examined the safety effects on cycle collisions of various designs of cycling infrastructure at roundabouts (Daniels 2009a; Jensen 2013). The comparators for each study are given in Table 1.

Types of outcome measures

No studies specifically reported self-reported injuries or medically attended injuries. The outcomes reported by authors were described in a variety of terms, making it difficult to identify studies reporting identical outcomes (Table 2). Fourteen studies (Nicholson 1979; Quenault 1981; Wheeler 1987; Bovy 1988; Williams 1989; Harland 1993; Gårder 1998; Allen 2005; Mountain 2005; König 2006; Agerholm 2008; Daniels 2009a; Grundy 2009; Chen 2012) reported police-reported cycle crash/accident/injury crashes. Of these, five studies (Nicholson 1979;

Williams 1989; Harland 1993; Allen 2005; Grundy 2009) in the UK reported using STATS19 data which is police-reported data on any road collision involving human injury or death, categorised as fatal, serious or slight. The remaining studies did not specify precisely the type of outcome data they analysed, other than reporting outcomes such as number of "cycle accidents" or "crashes involving cyclists". Nine primary papers (Nicholson 1979; Quenault 1981; Williams 1989; Jensen 1997; Webster 2003; Allen 2005; Daniels 2009a; Grundy 2009; Jensen 2013) reported collisions by severity and in addition, three papers (Dean 1993; Harbidge 1993; Shipley 1994) also reported injury severity for the cycle routes described in Harland (Harland 1993). Six primary papers reported the age of the person injured (Williams 1989; Jensen 1997; Webster 2003; Allen 2005; Mountain 2005; Grundy 2009); with two additional papers (Dean 1993; Harbidge 1993) reporting age of cyclists using the cycle routes described in Harland 1993. Two studies reported on sex of cyclists (Allen 2005; Shipley 1994); and one study (Grundy 2009) reported on the level of social deprivation of road segments where infrastructure was installed, based on the 2004 Index of Multiple Deprivation (Noble 2004).

Of the 21 included studies, 14 reportedly collected data on cycle counts (Carlson 1975; Nicholson 1979; Quenault 1981; Wheeler 1987; Bovy 1988; Smith 1988; Williams 1989; Harland 1993; Gårder 1998; Allen 2005; König 2006; Agerholm 2008; Chen 2012; Parsons 2013) (Table 3); however, Chen 2012 did not present data in their paper. The metrics used to present cycle flow data included absolute numbers, annual average daily traffic and percentage change in cycle flows. In addition, the schedules used to count cycle flow varied widely between studies. While some authors adjusted collision data for changes in cycle flow, more frequently cycle flow data was discussed either alongside trends in collision data or separately from collision data.

Excluded studies

There were 302 studies excluded from the review with reasons given in Characteristics of excluded studies. We used a hierarchy of reasons for study exclusion as follows: study did not relate to cycle infrastructure; study design not one of interest; study did not present data on outcomes of interest; we were unable to obtain a full copy of the paper and we had insufficient information to decide whether it met the inclusion criteria. Data on our reasons for exclusion relate to the first reason encountered. Papers were excluded for the following reasons: 73 (24%) papers did not relate to cycling infrastructure, 162 (54%) were not a study design of interest, 33 (11%) did not report outcomes of interest and we had insufficient information for 28 (9%) papers. Six papers were duplicates and were removed.

Risk of bias in included studies

Included studies were assessed for quality using the criteria described in the section 'Assessment of risk of bias in included studies'. The results are presented narratively in the table 'Characteristics of included studies'. The results are also presented graphically across all studies (Figure 2) and for individual studies (Figure 3). The majority of studies were at high or unclear risk of bias for selection bias and bias due to confounding. In terms of how well intervention and control areas were matched and their proximity to each other, six (29%) studies were rated as well matched and proximal, 13 (62%) were rated as poorly matched and proximity was either unknown or distal, and for two (10%) studies it was unclear. In terms of detection bias, the majority of studies (n = 19, 90%) analysed routinely collected collision data taken from databases maintained by organisations external to the study team. For three (14%) studies the source of collision data was unclear. In terms of attrition bias, over half the studies (n = 14, 67%) used data collection periods both before and after installation of the cycling infrastructure of at least one year. The length of time of data collection pre- and post-installation of infrastructure varied widely from one to five years pre-instalment to one to 9.5 years post-instalment. For four studies (19%) the data collection periods were unclear and three studies (14%) used data collection periods after the intervention of less than one year. In terms of selective reporting, one study excluded data from the analysis collected at an intervention site which had not been successful in reducing collisions (Mountain 2005); and a second study reported little data making it difficult to draw conclusions (Buckley 2000).

Figure 2. Risk of bias graph: review authors' judgements about each risk of bias item presented as percentages across all included studies. Twenty-one studies are included in this review.

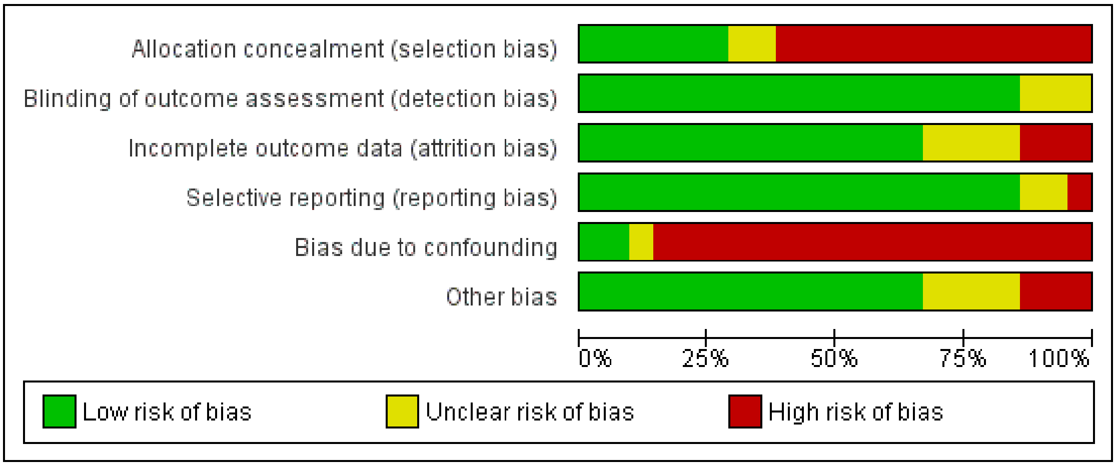
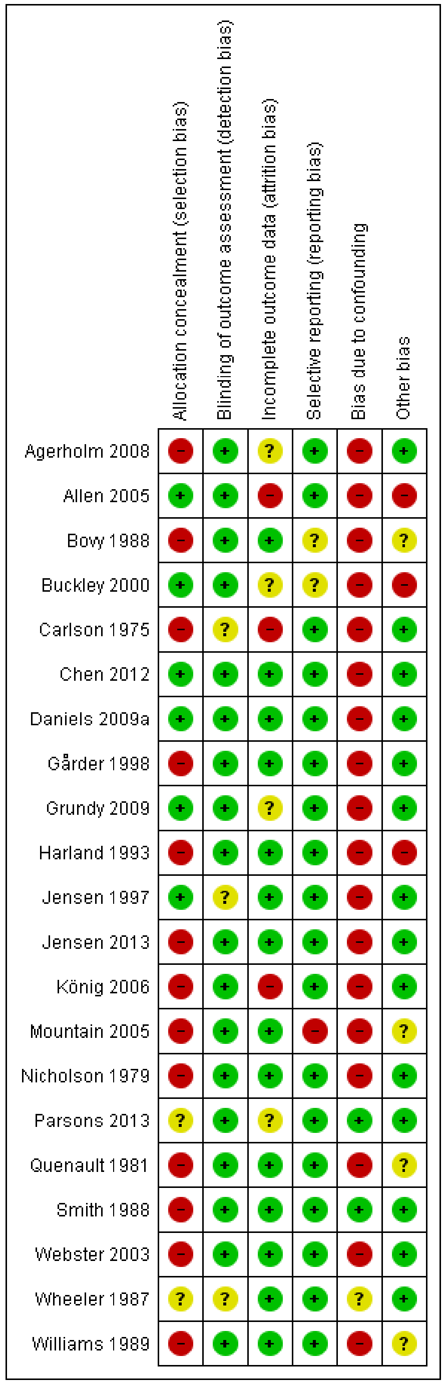


Figure 3. Risk of bias summary: review authors' judgements about each risk of bias item for each included study.



The majority of studies (n = 18, 86%) did not consider the effect of confounders between the intervention and control sites on their findings. While 13 studies (62%) measured cycle flow, only four studies adjusted collision rates by cycle flow. These studies failed to take into account confounding factors in their analyses or to discuss possible confounding factors. Three studies (14%) were at risk of other potential biases due to lack of reporting some outcomes for all intervention and control sites.

Effects of interventions

See: **Summary of findings for the main comparison** Cycle routes and networks for the prevention of cycling injuries in cyclists

Findings for primary and secondary outcomes are presented for each type of cycling infrastructure included in this review, according to the typology of cycling infrastructure presented in the section 'Types of interventions', with the additional category of 'Combination of cycling infrastructure'. All analyses of outcomes are limited to data which were available in the included papers.

1. Shared use of the road space for both motor vehicles and cyclists

Cycle lanes

Injuries

No included studies examined the effects of cycle lanes on self-reported or medically attended injuries.

Collisions

Four studies reported the effects of cycle lanes on cycle collisions. Three studies reported numbers of cycle collisions at both intervention and control sites and thus we pooled data to perform a meta-analysis (Smith 1988; Chen 2012; Parsons 2013). Buckley 2000 only reported confidence intervals for a mean effectiveness of treatment (mean effectiveness calculated from collision rates before and after at intervention and control sites). Chen 2012 reported cycle collisions on road segments and at intersections; for this first meta-analysis we used total collisions on both segments and at intersections. Chen 2012 used "bicycle trip density" in his modelling but does not present this data thus the meta-analysis does not take into account cycle flow.

Findings from this analysis (Analysis 1.1) show no statistically significant difference in the ratio of cycle collisions comparing intervention (cycle lanes) and control areas (no cycle lanes) (rate ratio 1.21, 95% confidence interval (CI) 0.70 to 2.08; Chi^2 = 3.56, 2 degrees of freedom (df), P = 0.17; I^2 = 44%). The I^2 value of 44% indicates there is some heterogeneity between the three studies. Using data from Chen 2012 and Smith 1988 on collisions at intersections only, the findings show no statistical difference in the number of cycle collisions at sites where there are cycle lanes and control sites (rate ratio (RR) 1.20, 95% CI 0.91 to 1.59; Chi^2 = 2.43, 2 df, P = 0.30; I^2 = 18%) nor for collisions on road segments only (RR 1.27, 95% CI 0.79 to 2.05; Chi^2 = 2.46, 2 df, P = 0.29; I^2 = 19%).

Smith 1988, using estimates of the total daily cycle trips, reported an increase of 20% in the collision rate per 1000 daily trips for the control area from before to after installation of cycle lanes in intervention areas (no CI or statistical significance data presented). Smith 1988 does not present corresponding collision rates for the roads where cycle lanes were installed but using their data for estimated daily trips it is possible to calculate that the cycle collision rate per 1000 daily trips on streets where cycle lanes were installed increased by 25% from before to after installation.

Parsons 2013 took into account cycle flow to report a 43% decrease (no CI or statistical significance reported) in cycle collision rates per million vehicle (cycle) kilometres travelled (MVKT) on streets where cycle lanes were installed while a decrease of 25% (no statistical significance reported) was observed on roads acting as a control. Allowing for collision reductions observed at control sites, the authors state that an average reduction of 23% in collision rates was observed at intervention sites following installation of cycle lanes. However, it is not clear how this figure was calculated.

Buckley 2000 calculated confidence intervals for the mean effectiveness of the treatment, where a negative effect indicates a possible increase in cyclist collision rate and a positive effect indicates a possible decrease in collision rate. The results show that the installation of cycle lanes did not change cycle collision rates when compared to control sites (intervention site: CI −123% to 89% (not statistically significant), compared with the control site: CI −30% to 97% (not statistically significant)). The authors did not take into account cycle flow.

Subgroup analysis of collision casualties

Authors of included papers assessing the effect of cycle lanes on injuries and collisions did not present data on any outcomes of interest for subgroup analysis, that is, severity of collision casualty, age, sex or level of social deprivation.

Cycle counts

Of the four studies reporting the safety effects of cycle lanes, three studies measured cycle count (Smith 1988; Chen 2012; Parsons 2013). Chen 2012 stated in their paper that they assessed cycle flow but they did not present cycle flow data in their paper. Parsons 2013 examined the cycle counts taken at the intersections on cycle lanes. In the intervention routes, cycle numbers per annum from pre-installation to post-installation rose by 236% with changes at individual sites varying from −400% to +542% with seven sites showing an increase and five sites showing a decline in cycle numbers. Over the study period cycle counts per annum recorded at the three control routes were +3%, +6% and −22%. Smith 1988 used cycle count data only to produce estimated daily trips (in thousands) for the whole of the city of Madison, Wisconsin, USA. Thus there is insufficient evidence to suggest that greater numbers of cyclists may be attracted to use roads with cycle lanes once installed.

Collisions involving cyclists adjusted for cycle flow

Of the three studies (Smith 1988; Chen 2012; Parsons 2013) included in the meta-analysis of the effects of cycle lanes on cycle collisions only two (Smith 1988; Parsons 2013) reported exposure and thus there was insufficient data to undertake a meta-analysis of cycle lanes taking into account exposure.

Advanced Stop Lines

Injuries

No included studies examined the effects of advanced stop lines on self-reported or medically attended injuries.

Collisions

One included study assessed the effects of advanced stop lines (ASLs) on collisions (Allen 2005). The study reported five sets of before and after data for five intervention sites, of which two showed an increase in cycle casualty rate per year, one site showed no change and two sites showed a decrease in cycle casualty rates per year (Allen 2005). No data on statistical significance were presented. No before-after data are presented for control sites. Cycle flows were observed only once, at the end of the study, and thus changes in cycle counts cannot be examined or taken into account when examining before and after casualty rates.

Subgroup analysis of collision casualties

Severity of collision

Using STATS19 data from Allen 2005 we undertook a Chi^2 test of independence to examine the relation between severity of injury and the installation of ASLs. The relation between these variables was statistically significant ($Chi^2 = 5.0391$, 1 df, $P = 0.02$) with cyclists' injuries being more likely to be slight rather than serious following installation of ASLs.

Sex of collision casualty

Using data from Allen 2005 a Chi^2 test of independence was undertaken to examine the relationship between the installation of ASLs and the sex of casualties which was found to be non-significant ($Chi^2 = 0.0342$, 1 df, $P = 0.853$).

Age of collision casualty

Allen 2005 reported on age of casualties. Of casualties where age was reported, one casualty was age 15 years (in an intervention area, post-installation of ASLs), the remaining 122 casualties were all age 18 years and over.

Cycle counts

Allen 2005 undertook cycle counts but only compared intervention and control sites at the end of the study; no before-data are presented. It is not possible to assess changes in cycle counts as a result of the installation of ASLs.

Use of colour in road infrastructure

Injuries

No included studies examined the effects of use of colour on self-reported or medically attended injuries.

Collisions

One study assessed the safety effects on collisions of coloured cycle crossings at intersections (König 2006). There were few cycle collisions during the study period (three cycle collisions at reconstructed sites before reconstruction and four post reconstruction with no cycle collisions at non-reconstructed intersections). König 2006 did not take into account cycle flow when considering changes in collision numbers.

Subgroup analysis of collision casualties

Authors did not present data on outcomes of interest for subgroup analysis.

Cycle counts

König 2006 undertook cycle counts at only two reconstructed and two control junctions on one occasion only, thus the effect of installing coloured cycle crossings at intersections on cycle flow cannot be assessed.

2. Separation of cycle traffic from motorised traffic

Cycle tracks

Injuries

No included studies examined the effects of cycle tracks on self-reported or medically attended injuries.

Collisions

Two studies evaluated the safety effects on collisions of installing cycle tracks (Gårder 1998; Agerholm 2008). The study by Agerholm 2008 was classified as a study of cycle tracks, described as being separated from the road by a kerb and elevated by 7 to 12 cm. The authors reported an increase of 21% in cycle injury collisions on cycle tracks that was not statistically significant (no P values given) with an 18% increase at intersections and 17% increase on road sections (both non-statistically significant, no P values presented) from before to after installation. The authors collected data on Annual Daily Traffic (ADT) for cyclists for a few sections but report no change in ADT.

Gårder 1998 (Leden 1997a; Gårder 1998) investigated the safety effects of coloured and raised cycle crossings that cross minor roads. Gårder 1998 used collision data from untreated intersections to calculate an index of effectiveness (IE) score, based on the ratio of collisions in the before/after period in the comparison areas to the ratio of collisions at the treated intersections in the before/after period. In one article describing this study, the authors focus on two locations within the treatment group (Leden 1997a). For one location Leden 1997a reports an IE of 0.63 (standard deviation (SD) = 0.22), indicating a 37% decrease in collisions and for the second site, Leden reports an IE of 1.15 (SD = 0.24), indicating a 15% increase in collisions. However, these calculations are based on the assumption of no regression-to-mean effect.

By studying just the treated intersections at the two location sites, rather than the whole length of the routes, Leden 1997a calculated an IE of 0.69 (SD = 0.31) (statistical significance not reported) suggesting a 31% decrease in cycle collisions, and for cycle collisions involving motor vehicles only, Leden 1997a reported an IE of 0.81 (SD = 0.34) (statistical significance not reported) again suggesting a likely decrease in the collisions at treated intersections. The authors (Leden 1997a) state that there may be biases in the choice of which intersections to treat and thus they took into account the effect of regression-to-mean to calculate an IE score of 1.08 (SD = 0.22) (statistical significance not reported) indicating a likely risk increase in the number of cycle collisions involving cars in the intervention area of 8% (Gårder 1998).

Gårder 1998 reported an increase in cycle flow from before to after reconstruction of at least 50% on experimental sections. The authors suggest that as an increase in collisions of approximately 8% was observed, the findings indicate that the interventions were effective in promoting the safety of cyclists. However, as numbers of collisions are small it is difficult to draw firm conclusions of the effects of reconstruction on reported collisions at the treated intersections. Furthermore, the authors consider that the speed of cyclists on reconstructed roads may have increased. The authors conclude that “the most likely effect of raising the cycle crossing is a risk reduction of 30%”.

Subgroup analysis of collision casualties

Neither of the two studies of cycle tracks provided data on severity of collision casualties, or age or sex of casualty, or level of social deprivation.

Cycle counts

Agerholm 2008 collected data on Annual Daily Traffic for cyclists but the authors did not present the data, only reporting that “no clear change in ADT was found” for cyclists. Gårder 1998 reported increases in cycle flows of 75%, 79% and 100% at the intervention sites. In contrast, cycle flows on the control sections increased by approximately 20% (no P values presented). The authors conclude that this indicates cycle flow increased by at least 50% on the reconstructed roads, “probably as a result of the ’better’ layout”. Thus there is insufficient evidence to assess changes in cycle count as a result of the intervention.

Cycle paths

Injuries

No included studies examined the effects of installing cycle paths on self-reported or medically attended injuries.

Collisions

For the purposes of this review we discuss cycle routes on the pavement under the generic heading of ’cycle paths’. One study was identified for inclusion in the review. Williams 1989 examined the injury collisions involving cyclists on footways (pedestrian area behind a kerb adjacent to the carriageway for traffic) converted to joint cycle pedestrian use. Williams 1989 used the ratios of before/after collisions in the control sites to calculate an expected number of collisions in the after period at the experimental sites. The expected total number of collisions was very similar to the observed number (no P values presented). Williams 1989 reports that changes in the number of collisions occurring at junctions in the middle of a scheme, at junctions at the beginning or end of a scheme or not at a junction were small and not statistically significant (no P values presented).

Subgroup analysis of collision casualties

Severity of collision

Williams 1989 reported a slight, non-statistically significant decrease in the proportion of killed or seriously injured (KSI) cyclists on converted footways from before to after installation (no P values presented).

Age of collision casualty

Williams 1989 reported a non-statistically significant increase (no P values presented) in the number of children involved in collisions following the implementation of converted footways.

Cycle count

Williams 1989 reported that generally, following conversions of footpaths to footways, the number of cyclists increased and that a greater percentage of cyclists used the footway than the road (no P values presented). However, these numbers do not take into account general trends in cycle volumes over the period of cycle flow data collection. There is insufficient evidence to assess the effect of cycle paths on cycle counts.

3. Management of the road network

Cycle lanes through signalised and priority junctions

Injuries

No included studies examined the effects of signalised and priority junctions on cycle lanes on self-reported or medically attended injuries.

Collisions

Jensen 1997 examined the safety effect on cycle collisions of cycle lanes continued through signalised junctions and on main roads across side roads with a 'give way' line. Jensen 1997 used data from a control group to calculate the expected number of collisions in the after period at the intersections on roads with a cycle lane. For signalised junctions there were more observed collisions post-treatment of intersections than expected but it was not a statistically significant difference (no P value presented). At junctions where cyclists have priority, there were statistically significantly more collisions observed than expected post-treatment of the junction (no P value presented). The increase in cycle collisions was mainly due to an increase in the number of collisions involving a cyclist and a car.

Subgroup analysis of collision casualties

Severity of collision

At junctions where a cycle lane continues on the main road past the mouth of a side road and the cyclist has priority, Jensen (Nielsen 1996; Jensen 1997) reports a slight decrease in the proportion of injuries that were fatal and an increase in the number of injuries that were slight (no change in the proportion of severe injuries) from pre- to post-treatment (no P values presented), but casualty numbers were small. There was no change in the proportions of injuries that were fatal, severe, or slight for signalised junctions.

Age of collision casualty

On cycle lanes at signalised junctions Jensen 1997 reports no change in age distribution of casualties from pre- to post-treatment (no P values presented) (Nielsen 1996; Jensen 1997). At priority junctions with cycle lanes continuing on the main road there were proportionally fewer child casualties (aged 0 to 17 years) post-treatment than observed in the pre-treatment period (no P values presented).

Cycle count

Jensen 1997 did not present data on cycle count.

Speed management

Injuries

No included studies examined the effects of speed management on self-reported or medically attended injuries.

Collisions

Two studies examined collision rates following the installation of 20 mph zones. Webster 2003 reported a statistically significant decrease of 32.9% ($P < 0.01$) in cyclist casualties in the 20 mph zones and a decrease of 15.2% on unclassified roads (statistical significance not reported). The authors took the reductions on unclassified roads into account to suggest that the reductions in cyclist casualties for 20 mph zones can be revised to 21% (Webster 2003). They state that the revised figure assumes that the reductions on unclassified roads have been brought about by factors other than the introduction of a 20 mph zone, such as cycle training and education. However, the authors continue by stating that the 20 mph zones will have contributed to casualty reductions on unclassified roads and thus the reduction in cyclist casualties on 20 mph zones should be considered as being between 21% and

33%. The authors did not take into account changes in cycle flows either as a result of the 20 mph zones or changes over time.

Grundy 2009 examined the effects on casualty rates of 20 mph schemes using a time series regression analysis. Some of the schemes in the analysis by Grundy 2009 were also used in the analysis by Webster 2003. Grundy 2009 found that in areas with a 20 mph zone installed, there was a reduction in all cyclists casualties of 16.9% (95% CI 4.8% to 29.0%) while on roads in areas adjacent to the 20 mph zones, a reduction of 4.6% (95% CI −2.5% to 11.7%) was observed in all cyclist casualties.

Mountain 2005 assessed the safety effects of speed cameras and engineering interventions such as vertical and horizontal deflections, carriageway narrowing and speed-activated signs. At sites where cameras were installed there was a decrease in all cyclist collisions of 6% (95% CI −33% to 23%) while at sites where engineering schemes were installed there was an observed decrease in all cyclist collisions of −34% (95% CI of −56% to −7%). Mountain 2005 did not collect data on cycle flows. The authors did not collect data for control sites and thus were not able to correct the observed changes in collision rates for regression to mean effects or trend (Mountain 2005).

Subgroup analysis of collision casualties

Severity of collision

From an evaluation of 20 mph zones, Webster 2003 reported a statistically significant decrease of 49.6% ($P < 0.05$) in the number of KSI (killed or seriously injured) cyclist casualties. On unclassified roads, the authors report a decrease of 28.3% (statistical significance not reported) in the number of KSI cyclist casualties (Webster 2003). The authors (Webster 2003) took the reductions on unclassified roads into account to state that the reductions in cyclist casualties for 20 mph zones can be revised to 30%. However, this revised figure assumes that the reductions on unclassified roads have been brought about by factors other than the introduction of a 20 mph zone and thus, while there is evidence that the 20 mph zones will have contributed to KSI casualty reductions on unclassified roads, the authors state that the reduction in KSI cyclist casualties on 20 mph zones should be considered as being between 30% and 50% (Webster 2003). Similarly, Grundy 2009 reported a greater decrease in severity of injuries seen in the 20 mph zones (−37.6%, 95% CI 14.4% to 60.9%) compared to the control areas (+2.1, 95% CI −19.5% to 15.2%).

Age of collision casualty

Webster 2003 reports a non-statistically significant decrease ($P > 0.05$) of 60.1% in the number of KSI child cyclist casualties in 20 mph zones from before to after installation. Grundy 2009 reported that for cyclists aged 0 to 15 years, the introduction of the 20 mph zones resulted in a 27.7% reduction (95% CI 6.3% to 49.1%) in casualties and for cyclists aged 16 years and over the 20 mph zones led to a 7.3% reduction (95% CI −10.3% to 24.9%) in casualties. On roads in adjacent areas for cyclists aged 0 to 15 years there was a 6.2% reduction (95% CI −10.8% to 23.2%) in casualties and for cyclists aged 16 years there was a 7.2% reduction (95% CI −0.11% to 4.6%) in casualties. Mountain 2005 reported a decrease in child cyclist collisions of 2% (95% CI −42% to 43%) where speed cameras were installed while at sites where engineering schemes were installed, there was an observed decrease in child cyclist collisions of 37% (95% CI of −69% to 7%). Thus these findings suggest that 20 mph may be effective at reducing collisions in child cyclists.

Cycle count

Of the three studies assessing the safety effect of reduced speeds and speed management, none presented cycle count data (Webster 2003; Mountain 2005; Grundy 2009).

4. Combination of cycling infrastructure

Cycle routes and networks

Injuries

No included studies examined the effects of cycle routes and networks on self-reported or medically attended injuries.

Collisions

Four included studies examining the safety effects of cycle routes in cities were considered similar enough to pool their data in a meta-analysis (Carlson 1975; Nicholson 1979; Quenault 1981; Harland 1993). Carlson 1975 presents data for collisions that occurred at three places: on the cycle route; the area surrounding the cycle route and serviced by the cycle route from which cyclists will be attracted to the cycle route; and the areas outside the zone serviced by the cycle route (comparison group). Using collision data for areas outside the zone serviced by the cycle route for the Carlson 1975 control area data, the forest plot indicates heterogeneity between the studies (Analysis 2.1). Similar results were seen using collision data for the area surrounding the cycle route and serviced by the cycle route for the Carlson 1975 control area data. We produced Summary of findings for the main comparison which summarises data on cyclists' collisions on cycle routes and networks. We judged the quality of the evidence to be very low and we are very uncertain about the estimate.

In terms of cycle count, Harland 1993 presents cycle flow data on and off the cycle route before and after opening the cycle route for three of the six cities examined. Nicholson 1979 does not present cycle flow data post implementation of the cycle route. Thus, a meta-analysis of cycle collisions adjusted for cycle count data was undertaken for three studies (Carlson 1975; Quenault 1981; Harland 1993) using data from Carlson 1975 for areas outside the zone serviced by the cycle route; and data from three cities in the evaluation by Harland 1993. The findings from this meta-analysis show that while collision rates decreased, there was no statistically significant difference in collision rates between cyclists using the cycle routes and those not using the cycle routes (rate ratio 0.40, 95% CI 0.15 to 1.05, $Chi^2 = 8.00$, df = 2, P = 0.02; $I^2 = 75\%$). An I^2 value of 75% suggests that heterogeneity between the studies was high. While the studies by Quenault 1981 and Harland 1993 were undertaken in the UK, the study by Carlson 1975 was carried out in the US and thus there will be differences in the road transport system which may influence both use of installed cycle networks and collision rates. While Quenault 1981 and Harland 1993 used police reported collision data, the source of data used by Carlson 1975 is unclear. In addition, Quenault 1981 and Harland 1993 used longer data collection periods post-installation of the cycle network than Carlson 1975.There are too few studies in the meta-analysis to include in a funnel plot for evidence of publication bias. Shipley 1994 discusses before-after changes in the number of cycle collisions on the carriageway (a decrease of 53.8%) and at roundabout junctions (an increase of 34.9%) but none were statistically significant.

Bovy 1988 examined the effects of implementing a cycle network covering a large area, in contrast to the previously discussed studies which installed a smaller number of cycle routes. The authors examined collisions involving cyclists in five districts; that is, the intervention and control areas and three other areas. Of the 161 collisions that occurred pre-construction of the cycle network, 39 (24%) occurred in the intervention area and 19 (12%) in the control area. In the post-construction data collection period, of the 145 collisions that occurred, 27 (19%) occurred in the intervention area and 18 (12%) in the control area. Using the data from the intervention and control areas we undertook a Chi^2 test of independence to examine the relation between collisions and installation of the cycle network. The relation between these variables was not statistically significant ($Chi^2 = 0.5773$, 1 df, P = 0.447).

Subgroup analysis of collision casualties

Severity of collision

Using data from Dean 1993, we calculated that there was no statistically significant change in the severity of injuries on the cycle route between before and after opening of the cycle route (1-sided Fisher's exact test, P = 0.571). Similarly there was no statistically significant change in severity of injuries from before the opening of the cycle route to after its opening (1-sided Fisher's exact test, P = 0.464).

Nicholson 1979 reported no significant change in the number of KSI cyclists casualties from before to after opening of the cycle route, or between those on the cycle route and off the cycle route (no P values presented). They collected after-data for one year. The numbers of casualties on the cycle route were small, making it difficult to draw statistical conclusions.

Similarly, Quenault 1981 also reported severity of injuries on and off the cycle route both before and after opening of the route. Post-installation data was collected for 18 months. Numbers of injuries on the cycle route are too small to draw statistical conclusions. Harbidge 1993 reports collisions by severity for both intervention and control areas but only for the after period. Shipley 1994 presents data on severity of injuries for cycle collisions along the cycle route pre- and post-opening of the route but does not provide similar data for the control areas.

Sex of collision casualty

Dean 1993 and Harland 1993 reported sex of casualties but only for cycle routes and not for comparison areas.

Age of collision casualty

Using data from Dean 1993, we undertook a Chi^2 test of independence to examine the relationship between changes in the age of casualties (0 to 19 years versus 20 years and over) on the cycle route from pre- to post-opening of the route. While a greater proportion of all casualties were aged under 19 years post-opening of the cycle route, the relationship was found to be non-significant ($Chi^2 = 0.9867$, 1 df, P = 0.321). Similarly, off the cycle route a greater proportion of all casualties were aged under 19 years post-opening of the cycle route compared to pre-opening; however, a Chi^2 test of independence found no significant relationship between age of casualties off the cycle route from pre- to post-opening of the route ($Chi^2 = 1.0195$, 1 df, P = 0.313). Harbidge 1993 reported a statistically significant decrease of 50% in the number of collisions involving children in Kempston (intervention) compared to Bedford (control) ($Chi^2 = 4.35$, $P < 0.05$). Harland 1993 collected data on age of cyclist but for the experimental areas only.

Cycle count

Harland 1993 presents observations on cycle counts on and off cycle routes before and after construction for three towns in the UK: Kempston, Nottingham and Stockton. By combining this data, a Chi^2 test of independence was performed to examine the relation between cycle counts and opening of the cycle route. The relation between these variables was significant ($Chi^2 = 241.64$, 1 df, $P < 0.01$) with cyclists being more likely to use the cycle routes than other roads once the cycle routes were opened.

From cycle counts undertaken for the evaluation of the Southampton Western Approach Cycle Route (Shipley 1994), overall a decrease of 13.6% in the number of cyclists was observed at the control sites from before to after opening of the cycle route while an increase of 28.2% in the number of cyclists was observed at the experimental sites (no P values presented). There was a "highly significant" increase of 346% in the number of women cyclists on the footway at two experimental sites (no significant change at the third site) while one control site showed a "statistically significant" change in the number of women cyclists, two control sites showed no significant change (P values not presented). Shipley 1994 reported a non-statistically significant increase of 20% in children cycling along the route and no change in flow at control sites (P values not presented).

Data from cycle counts at two intervention sites on the Portsmouth cycle route showed increases of 24.6% and 68.4% cycles/hour from before to after opening of the cycle route (no P value presented) (Nicholson 1979). No data is provided for cycle flow off the cycle route post-opening of the cycle route. The authors conclude that the introduction of the cycle routes was followed by a "significant increase in their use by cycles". Quenault 1981, evaluating the Peterborough cycle route, examined changes in cycle flow on the cycle route adjusted for trends in cycle flow observed at control sites and reported an observed average increase of 19% at primary cycle count sites ($P < 0.01$). At the two secondary cycle count sites, just off the cycle route, cycle counts showed an average decline of 5% (not statistically significant, no P value presented). Quenault 1981 examined the effect of the cycle route on the percentages of adults and children using the route but present data of cycle counts by age for three sites only. Two sites showed a statistically significant increase in the proportion of children using the cycle path relative to adults ($P < 0.01$) but one site showed no change.

Carlson 1975 counted cycle flow at three locations on the cycle route and observed an average increase in cycle flow of 12.6% from pre- to post-opening of the cycle route. In contrast, counts at two stations on roads with no cycle facilities showed an average decrease of 14.4% (no P values presented).

Bovy 1988 assessed annual cycle flow pre- and post-construction of the cycle network and found that pre-construction in the five areas examined, 20% of total cycle flow occurred in the intervention area and 18% in the control area while post construction there was little change with 20% of total cycle flow observed in the intervention area and 17% in the control area.

Collisions involving cyclists adjusted for cycle count

For the meta-analysis of cycle routes and networks, of the four included studies three presented detailed cycle count data (Carlson 1975; Quenault 1981; Harland 1993). Carlson 1975 undertook cycle flow counts at three sites on the cycle route prior to and post-opening and thus we calculate a mean increase of 12.6% on the cycle route, while off the cycle route there was a mean decrease in cycle flow from before to after opening of the cycle route of 14.4%. Harland 1993 presents for three cities cycle count data both on and off a cycle route, before and after opening of the route. Using these data, we calculated a mean increase in cycle flow on the cycle route of 18.2% from before to after opening and a mean decrease in cycle flow of 42.1% off the cycle route. Quenault 1981 presents 4-hour cycle count data in 4 months immediately before opening of a cycle route versus 4 months immediately after opening (unadjusted for observed annual trend at control sites) for primary sites on the cycle route. These sites showed an increase of 5% while a decrease of 14% was observed at control sites "well away from cycle routes". Nicholson 1979 presents change in cycle flow on a cycle route and on other parts of the road network before and during implementation, but not after.

Thus by adjusting the ratio of cycle collisions by the ratio of cycle flow we undertook a meta-analysis of the effects of cycle routes and networks on cycling collisions adjusted for cycle flow but using three cities from Harland 1993 for which we had cycle flow and collision data (Analysis 2.2). The results show no statistically significant difference in the number of collisions for cyclists using cycle routes and networks and those not using cycle routes (RR 0.40, 95% CI 0.15 to 1.05, $Chi^2 = 8.00$ df = 2, $P = 0.02$; $I^2 = 75\%$).

Package of infrastructure

Injuries

No included studies examined the effects of a package of infrastructure on self-reported or medically attended injuries.

Collisions

Wheeler 1987 examined the effects of two cycle schemes, the Albert Gate and Albion Gate on the Ambassador Cycle Route in central London, where a package of cycling infrastructure measures were implemented. Wheeler 1987 reported collisions involving cyclist casualties for an area of 12 km^2 enclosing the whole length of the route and the surrounding area from which cyclists might be attracted. Data were collected for 3 years before and after implementation of the cycle schemes. Wheeler 1987 reported non-statistically significant decreases in the number of collisions causing injury of 30% and 0%, respectively, from before to after installation. In terms of areas in the vicinity of the cycle schemes there was a non-statistically significant decrease in collisions of 13%, and for the whole surrounding area enclosing the cycle route, the authors report an increase of 4% (statistical significance not reported). The authors conclude that there was a slight increase in collisions on the cycle route, possibly due to more cyclists using it. Using cycle flow data, Wheeler reports that collision rates (total collisions x 100/cycle flow) for the whole area decreased by 24.9% (statistical significance not reported). Using the cycle flow data in a similar way, it is possible to calculate that reported 'collisions causing injury' rates (x 100/cycle flow) for the Albert Gate and Albion Gate screenlines (point at which passing cycles are counted) decreased by 47.1% and 26.7% respectively.

Subgroup analysis of collision casualties

Wheeler 1987 did not present data on severity of collision casualties, or age or sex of the casualty.

Cycle count

Wheeler 1987 assessed cycle flows following the introduction of the Albert Gate and Albion Gate schemes. At Albert Gate and six associated screenline (counting) sites there was an overall 13% increase (P < 0.001) in cycle counts; however, cycle counts at each of the seven sites monitored varied considerably. An increase of 59% (P < 0.001) was observed at Albert Gate and increases of 20% (P < 0.01) were recorded at two other sites. However, decreases of 21% and 12% (P < 0.05) were reported for two other screenline sites, both of which are closer to Albert Gate suggesting that cyclists may have been attracted to use the new Albert Gate scheme. Using data on cycle flow from control sites, Wheeler estimated flows that could have been expected across the Albert Gate and the other six screenlines had the scheme had no effect on cycle flows and found that generally cycle flows were significantly lower than expected for each site, except at Albert Gate where cycle flow was significantly higher than expected (P < 0.01).

At the Albion Gate and six associated screenline (counting) sites, an overall 35% decrease (P < 0.05) in cycle flow was observed. On closer inspection of cycle flows at each of the seven sites, cycle flows at Albion Gate increased by 2% (not statistically significant) while cycle flows at three gates nearest to Albion Gate decreased by 63% (P < 0.001), 62% (P < 0.00.1) and 44% (P < 0.01), respectively. Once again using data from control sites to calculate estimated cycle flow post-implementation of the Albion Gate scheme, cycle flows at three sites were statistically significantly lower than expected (P < 0.01 to 0.05) while flow at Albion Gate was 47% higher than expected (P < 0.01). This suggests that cyclists may have been attracted to use the new Albion Gate in preference to other gates. The authors (Wheeler 1987) report that overall changes in cycle flows at control sites may be due to changes in London Transport fares over the study period which were reduced by 32%, increased by 96% and then reduced once again by 25% in an 18 month period. The authors conclude that there is evidence that cyclists used the Albert Gate and Albion Gate schemes following their introduction in preference to other gates to Hyde Park but there was "less transfer than was hoped from Park Lane and its gyratories". Cycle flows (taken from London Transport whole cordon total) increased by 32% from pre- to post-intervention (statistical significance not reported).

Collisions involving cyclists adjusted for cycle count

Using cycle flow data taken from London Transport, Wheeler 1987 reports that collision rates (total collisions x 100/cycle flow) for the whole area decreased by 24.9% (statistical significance not reported). Using the cycle flow data in a similar way, it is possible to calculate that reported collision rates (x 100/cycle flow) for the Albert Gate and Albion Gate screenlines decreased by 47.1% and 26.7% respectively.

Roundabout design

Injuries

No included studies examined the effects of a roundabout design on self-reported or medically attended injuries.

Collisions

Daniels 2009a examined the safety effect of four categories of roundabout design: mixed traffic; with cycle lanes in the circulating carriageway; separate cycle paths; and grade separated (multi-level). Cycle lanes were divided further into four categories based on the presence or absence of a line marking between the carriageway and cycle lane or barrier, where a barrier may be a curbstone, small concrete elements, verdure or an elevation between the carriageway and cycle lane. Separate cycle paths were divided further into two designs based on whether cyclists were given priority or not. To compare the safety effects of different roundabout designs on collisions for cyclists, Daniels 2009a calculated an effectiveness index (EI) expressed as an odds ratio of the change in collision numbers in the treatment group after conversion to a roundabout from an intersection, compared to the change in collision numbers in the comparison group in the same period. An EI greater than 1 indicates an increase in the number of crashes while an EI less than 1 indicates a decrease in crashes in the intervention group. Overall, for all roundabouts examined, the EI was 1.27 (95% CI 1.00 to 1.61, P = 0.05) suggesting that the roundabouts installed at intervention sites may increase the odds of collisions compared to the odds of collisions at the control sites. For roundabouts with cycle lanes of varying designs within the circulating carriageway, the EI was 1.93 (95% CI 1.38 to 2.69, P < 0.01), demonstrating a significant worsening effect due to their presence. In particular, for roundabouts with cycle lanes marked by a barrier and a line, the EI was 2.06 (95% CI 1.24 to 3.44, P = 0.01), which was reduced slightly for cycle lanes with lines and no barriers to 1.85 (95% CI 1.16 to 2.94, P = 0.01), both being statistically significant. For all other roundabout designs there was no statistically significant change in the number of collisions, as indicated by the EI.

Jensen 2013 examined the safety effects of converting intersections to roundabouts with varying treatments for cyclists. Trends in collision rates observed in comparison areas were used to calculate a correction factor for general collision trends, which the authors employed, along with a correction factor for regression to the mean, to produce the expected number of collisions in the after period if the reconstructions had not taken place. The number of cycle collisions increased by 65% post conversion to roundabouts, while the number of injuries increased by 40% (no P values presented). In terms of individual cycle treatment at junctions converted to roundabouts, at roundabouts with cycle lanes and priority to cyclists there was a statistically significant increase in cycle collisions of 113%. For roundabouts converted to include a coloured cycle lane marked as part of the circulating lane (priority to cyclists) there was a statistically significant increase in cycle collisions of 246% (no P value presented). For roundabouts with a cycle path and no priority to cyclists, a statistically significant decrease in cycle collisions of 81% was observed (no P value presented). For roundabouts with no structural facility for cyclists with priority to cyclists, for cycle tracks with priority to cyclists, and for cycle tracks with blue cycle crossings and priority to cyclists, there was no statistically significant change in the number of collisions. Collision rates on coloured cycle lanes and blue cycle crossings were higher than collision rates on the same facilities but without colour. The roundabouts included 3- and 4-armed designs and speed limits varied from 40 to 10 km/h.

These studies provide some evidence that roundabouts may increase the odds of collisions for a cyclist.

Subgroup analysis of collision casualties

Severity of collision

Daniels 2009a investigated the design of roundabouts and reported that while there were no statistically significant differences in the number of cyclist casualties KSI compared to the comparison group, all designs showed an increase in the number of fatal and serious collisions compared to the comparison group. Jensen 2013 found that following conversion of intersections to roundabouts, the number of cycle collisions increased by 65% post conversion of all roundabouts while the number of injuries increased by 40%, with a 49% decrease in fatalities, a 10% increase in severe injuries and an 80% increase in slight injuries (no P values presented).

Cycle count

Neither of the two studies that assessed the safety effects of roundabout design on cycle collisions presented data on cycle count (Daniels 2009a; Jensen 2013).

Social Deprivation

Speed management

Grundy 2009 reported that 20 mph zones appear to have smaller effects on cyclist casualties with increasing social deprivation of the area in which the collision occurred but that the numbers of cyclist casualties were insufficient to allow reliable comparisons to be made. On adjacent roads there was no evidence that cyclist casualty rates differed by level of social deprivation. However, the authors report that annual reductions in cyclist casualties decreased with increasing social deprivation ($P < 0.001$). In terms of ethnicity, the percentages of cyclist casualties by ethnic group changed little from before to after implementation of 20 mph zones and there were no statistically significant differences in reductions in cyclist casualties by ethnic group in 20 mph zones and adjacent roads.

Type of collision

Some studies presented collision data by type of collision, such as ’right turning’ and ’automobile turns in front of bicycle’. Dean 1993 presents data such as ’cyclists runs into rear of braking vehicle’ that does not appear to be related to design of infrastructure. No studies provide data on collisions where the cyclist may have fallen due to an obstruction in the road.

Cycle helmet legislation

While the countries in which the included studies took place varied in terms of cycle helmet legislation, for each type of infrastructure there were too few studies that provided data on severity of injury to allow for a subgroup analysis of injury severity by cycle helmet legislation.

DISCUSSION

Summary of main results

This review found a lack of evidence that the cycling infrastructure assessed reduces cyclists’ collisions. Meta-analyses of study findings relating to cycle lanes on stretches of road, and cycle routes and networks found no evidence that either are effective at reducing cycle collisions or injuries. These meta-analyses were conducted on studies employing a CBA trial design and are thus subject to a high risk of bias. When examining the effects of cycle routes and networks on collisions we assessed the quality of the evidence using GRADE and rated the quality of the evidence as very low.

Generally, there is a lack of high quality evidence to be able to draw firm conclusions as to the effect of cycling infrastructure on cycling collisions. From data presented narratively, the use of 20 mph speed restrictions in urban areas may be effective at reducing cyclist casualties. Redesigning specific parts of cycle routes that may be particularly busy or complex in terms of traffic movement may be beneficial to cyclists in terms of reducing the risk of collision. Generally, the conversion of intersections to roundabouts may increase the number of cycle collisions. In particular, the conversion of intersections to roundabouts with cycle lanes marked as part of the circulating carriageway increased cycle collisions. However, the conversion of intersections with and without signals to roundabouts with cycle paths may reduce the odds of collision. Both continuing a cycle lane across the mouth of a side road with a 'give way' line onto the main road, and cycle tracks, may increase the risk of injury collisions in cyclists. However, these conclusions are uncertain, being based on a narrative synthesis of findings from included studies. There is a lack of evidence that cycle paths or advanced stop lines either reduce or increase injury collisions in cyclists.

There is insufficient evidence to draw any robust conclusions concerning the effect of cycling infrastructure on cycling collisions in terms of severity of injury, sex, age, and level of social deprivation of the casualty. The introduction of 20 mph speed limits may reduce the number of KSI cyclist casualties. The introduction of ASL may result in cyclist casualties being more likely to suffer slight rather than serious injuries. The introduction of 20 mph speed limits and the installation of cycle routes and networks may reduce the number of child cyclist casualties.

No studies reported collisions by cause such as 'fell off' due to an obstruction in the road. This was probably due to studies generally using police reported collision data, which is more likely to contain data on collisions involving another vehicle. There were too few studies that provided data on severity of injury to allow for a subgroup analysis of injury severity by the existence of legislation requiring the use of cycle helmets.

In terms of cycle flow, cycle routes and networks may be effective at encouraging more cycling. In addition, re-designing complex parts of a cycle route may encourage more cycle use, particularly over parts of the network that have been re-designed. For all other types of infrastructure, there was insufficient evidence that cycle flow increased, or authors failed to measure or report cycle flow. The schedules used to count cycle flow varied widely between studies. Few studies examined the effectiveness of cycling infrastructure while controlling for exposure. While some authors adjusted collision data for changes in cycle flow, more frequently cycle flow data was discussed either alongside trends in collision data or separately from collision data. These findings are based on studies employing designs that are at greater risk of bias than RCTs and thus should be viewed with caution. There were too few studies to conduct sensitivity analyses to examine the impact of study quality on outcomes.

Overall completeness and applicability of evidence

We undertook a thorough search for relevant studies. Our searches identified over 8000 possibly relevant studies in the initial stages of the review. All the included studies were conducted in higher income countries.

Many of the included studies were undertaken a number of years ago and will have been investigating the effects of interventions installed earlier than the date of publication. The measures implemented may not comply with current standards or with what is now considered best practice. Eleven (52%) of the studies were published more than 10 years ago, that is, before 2004; given the changes in traffic volumes and transport trends, and the developing approaches to the design and implementation of infrastructure for cycle traffic both in the UK and in Europe, the evidence from the older studies may not be very relevant to today's road conditions.

No studies reported data on the condition and maintenance of the cycling infrastructure and thus the effect of this on subsequent cycle collision rates is unknown. In addition, installation of cycling infrastructure does not necessarily mean that it was used in the way it was intended and its installation may lead to other unexpected behaviour that may result in changes in the risk of collision; for example, the speed of cyclists may have increased on reconstructed roads.

Quality of the evidence

We set out to find studies designed either as RCTs, CBAs or ITS. We did not find any RCTs that met our inclusion criteria; the majority of the included studies were CBAs. This issue is likely to affect the results of the review as potential biases are greater for studies employing a CBA design than RCT; for example, the baseline characteristics of the intervention and control groups may not be comparable and given that we have included CBA studies in our meta-analyses, our findings should be viewed with caution. Given the number of evaluations of cycling infrastructure we identified from our searches, there are relatively few that have used a scientifically robust study design.

In general, there was little matching of intervention and control sites. The control site most frequently chosen was the whole of a city, minus the area of the intervention site. For these studies where the control area was defined as the whole of the city, it was not always explicitly stated whether the collision data presented for the whole of the city also included collisions data from intervention sites. In many studies, the comparability of the control area to the intervention site was unclear, and control and intervention sites were likely to be dissimilar. Few studies provided information on other cycling infrastructures that may be in place in the control and intervention areas.

In terms of outcomes of interest, and the use of the terms collision, crash and accident by researchers, it was not always clear whether the collision, crash or accident data referred to all events or only those resulting in an injury. Some researchers explicitly stated that the data referred to injury collisions but for studies where this was not clear, an assumption has been made that the data refer only to all collisions, with or without an injury. The majority of studies analysed data routinely collected by organisations external to the study team, most frequently, police-reported data. Analysing data that has been collected by a source external to the study team reduces the risk of bias in terms of systematic differences in assessing outcomes between the control and intervention groups. While there have been some questions raised as to the completeness of such databases, with under-reporting of collisions, the more serious injuries are more likely to be reported than more minor injuries. However, it is worth noting that collisions not involving a motor vehicle are less likely to be reported than those involving a motor vehicle (Buckley 2000).

In terms of outcome data, for some studies the numbers of collisions were too low to draw any statistically significant conclusions; sometimes this was due to an inadequate period of data collection post-installation of the infrastructure. Given the seasonal and time trends observed in traffic volume a minimum of one year data collection pre- and post-instalment is necessary. Furthermore, some authors did not take regression-to-mean effects into account when examining changes in collisions. Regression to the mean is a statistical phenomenon that can occur in studies assessing the effectiveness of transport interventions aimed at reducing collisions because by nature, sites for an intervention may be chosen based on higher than average collision rates. However, regression to the mean may also occur because high collision rates in one year may be followed by lower rates in the proceeding year due to random fluctuation alone, and hence may result in an overestimate of reductions in collisions. Thus longer data collection periods pre- and post-installation are recommended.

Few studies considered the effects of confounding factors on their results. A key confounding factor, changes in cycle flow over the period of the study, was measured by over half of the included studies but few adjusted cycle collision rates for exposure. It is anticipated that installation of cycling infrastructure may result in an increase in the number of cyclists using the installation, and thus studies not adjusting cycle collisions for exposure may underestimate reductions in cycle collision rates. A number of studies discussed how timing of cycle flow counts in terms of season (winter versus summer) and the general weather conditions over a period of several months or years (which can be quite variable, especially in temperate climates such as the UK) can affect cycle flow. For many of these studies, confounding factors such as weather and traffic volume, are as equally likely to influence both the intervention and control sites. In addition, implementation of cycling infrastructure may raise awareness of safety issues surrounding cycling and may alter the wearing of helmets and visibility aids which may in turn influence collision rates and severity of injuries.

Potential biases in the review process

We undertook a comprehensive search for relevant studies. However, identifying, and then locating, potentially relevant studies for this review was challenging. This is due to many of the reports of engineering/transport interventions being published as grey literature. Only eight of the 21 included articles were published in journals. In addition, searching for engineering/transport related papers is not as straightforward as searching for medically focused papers that use a system of keywords for cataloguing articles in databases and which thus supports searching for articles using relevant search terms (Wentz 2001). Given the number of articles and conference presentations we identified from a broad range of sources, it is possible that we may have failed to identify some potentially relevant reports.

Injury rates and collisions were the primary and secondary outcomes of interest for this review. However, for many of the included studies collisions and injury rates were just one of many outcomes assessed such as video analysis of cyclist and driver behaviour, vehicle speed, cyclist interviews and conflicts on the road network between cyclists and motorised vehicles and/or pedestrians where no actual collision occurs. Thus injury rates and collisions were often not the main outcome assessed in the study. This may mean that potentially relevant papers were excluded when there was no indication from the title or abstract of the paper that collision data was collected. However, we attempted to circumvent this problem by seeking the full text for any paper that appeared to evaluate a cycling infrastructure and thus exclude it at the full text stage if it did not report injuries or collisions.

Many of the evaluations of infrastructure are written as reports that are generally much longer and less formally set out than reports of studies published in journals. This inevitably makes the task of identifying and extracting relevant data more difficult and may lead to key pieces of information being overlooked. In addition, some papers generated much discussion on whether they met the inclusion criteria. Decisions on inclusion/exclusion have to be based on whether the reported study meets the inclusion criteria, even if papers subsequently do not report the data in a useable form, or fail to report all the data. Where there was uncertainty, we attempted to contact authors or find other papers reporting the same study.

Several included studies have been reported in more than one paper. In such cases we sought all reports relevant to that study. However, some papers were then subsequently excluded either because they reported outcomes other than those of interest, such as people's attitudes to the cycle routes, or they reported insufficient data to meet the inclusion criteria.

The findings of the quality of studies should be viewed with some caution as we appraised study quality using criteria designed for the appraisal of RCTs and applying it, with some adaptations, to the appraisal of non-RCTs. When examining the effects of cycle routes and networks on collisions we assessed the quality of the evidence using GRADE and rated the quality of the evidence as very low (Summary of findings for the main comparison).

A number of included studies used the Empirical Bayes method which uses data collected from a control group to provide data on a general trend in traffic safety and controls for effects of regression to the mean. Using this data, an expected rate of collisions post-installation of the infrastructure in the experimental areas is determined and compared with the observed rates post-installation. Such studies were included if they met the definition of a CBA (EPOC). A weakness of these studies is that if the control area is not very comparable to the intervention area, the expected rates of collisions will be inaccurate.

For this review we adopted the methodology of a Cochrane review, designed originally to evaluate medical interventions, and thus the methodology may not be considered the most appropriate for evaluating transport interventions. However, given the clear health impact of transport interventions on injuries, use of a mechanism such as a Cochrane review may help to highlight the need for improving the design of transport intervention evaluation studies and their subsequent reporting.

Agreements and disagreements with other studies or reviews

While there have been a number of literature reviews and summaries of evidence of the effectiveness of cycling infrastructure at reducing injuries, for example Clarke 1995, Reynolds 2009, Pucher 2010, Reid 2010, Karsch 2012, Road Safety Observatory 2013, Thomas 2013, few have systematically searched for relevant studies or restricted their included studies to those with a robust study design that includes a control group.

The most relevant previous reviews to this current review are those of Thomas 2007 and Reid 2010. Thomas 2013 assessed the safety effects of urban cycle tracks and concluded from the 23 relevant papers identified that "one way cycle tracks are generally safer than two-way and that when effective intersection treatments are employed, constructing cycle tracks reduces collisions and injuries". However, these conclusions are based on a review of study findings some of which report a statistically significant reduction in cycle collisions while others do not report statistical significance. More importantly, results from individual studies are not pooled. Thomas 2013 presents a table summarising included studies and while this indicates whether the study findings were controlled for exposure and whether it was a before-after study, there is no indication whether a control group was included. Thus the quality of some studies is unclear.

Elvik 2009 undertook a comprehensive search for studies evaluating road safety measures including measures aimed at improving the safety of cyclists. They aimed to identify studies that have quantified "the effect of one or more road safety measures on the number of accidents, accident rate and the number of injuries or risk of injuries". The authors summarised the results using meta-analysis. From an analysis of study findings evaluating the effects on accidents of cycle lanes Elvik 2009 reported a reduction in accidents involving cyclists on roads with cycle lanes, with the greatest reductions seen at junctions. A meta-analysis of findings from studies evaluating cycle tracks indicated a non-statistically significant increase in accidents involving cyclists, with greatest increases seen at junctions. For tracks aimed at pedestrians and cyclists Elvik 2009 identified no change in the number of accidents involving cyclists. Elvik 2009 also investigated the effects of different cycle facilities at junctions. They found a non-statistically significant reduction in cycle accidents for ASLs and a statistically significant reduction in the number of accidents for coloured cycle lanes. For continuing cycle paths at junctions Elvik 2009 found a non-statistically significant reduction in the number of accidents. Our findings were in agreement with those of Elvik 2009 regarding no change in the number of accidents on cycle tracks designed for use by both pedestrians and cyclists, and a reduction in the number of accidents for cycle paths continuing along a junction. For all other road safety measures previously mentioned our findings were in disagreement. However, the inclusion criteria adopted by Elvik 2009 were not as strict as our criteria, and thus their meta-analyses include study designs that do not require a control group. Reid 2010 concludes that "the evidence is strong that reducing the general speed of motorised traffic confers a safety benefit for cyclists". In addition they report that roundabouts are particularly risky for cyclists but that signalising may reduce the risk. The findings from our review support those of Reid (Reid 2010).

AUTHORS' CONCLUSIONS

Implications for practice

This review has found a lack of evidence of the effectiveness of the cycling infrastructure evaluated at reducing cycling injuries; thus we are unable to give implications for practice in terms of installation of specific types of cycling infrastructure to promote the safety of cyclists. However, given that there is insufficient evidence, it would be inappropriate and premature to imply that cycling infrastructure should not be installed. While cycling infrastructure may have the potential to reduce cyclist collisions, it can only be effective if road users use the cycling infrastructure as planned. Installation of cycling infrastructure may attract new or inexperienced cyclists to use both the infrastructure and the wider road system. This suggests that installation of cycling infrastructure alone may not be the most appropriate approach to reduce collisions.

Implications for research

This review highlights the lack of rigorous evaluations of the many forms of cycling infrastructure. Given the lack of well designed, recent evaluation studies of cycling infrastructure and that the prevention of road traffic injuries worldwide is becoming increasingly important (WHO 2011), there is a pressing need for transport injury prevention interventions such as cycling infrastructure to be evaluated using well-designed studies. Evaluating cycling infrastructure is difficult, particularly given the range of factors local to any intervention site, many of which will be subtle and hard to measure but will undoubtedly affect collision rates. Future studies should include a comparable control area, and pre- and post-installation data collection periods of sufficient length to allow for seasonal and temporal changes in traffic volume. Interventions and outcome data should be described clearly to allow for precise comparisons and pooling of findings from studies assessing similar interventions. Researchers and planners need to provide detailed descriptions of the infrastructure under evaluation so any future evaluations and transport plans can make most effective use of the findings from previously researched infrastructure. Future studies should include some evaluation of road users' behaviour to assess whether the cycling infrastructure is being used as planned. Examination of changes in collision rates should take into account contemporaneous changes in cycle flow, especially as the infrastructure may attract a greater number of cyclists than it can safely accommodate. In addition, it is necessary for these evaluation studies to be reported and disseminated in such a way that ensures the findings can be most readily accessed and put into practice by the health-related professions who support transport injury prevention and promote physical activity to improve public health.

The complexity of reducing conflicts between different types of users on the road system cannot be overlooked. The purpose of road safety engineering is to manage the multiplicity of risks in such a way as to minimise the risk posed to all users. Any change in a layout will result in the potential for some risks to be reduced while other risks may be increased; for example, changing a roundabout to signal control may reduce the number of collisions at the junction with the roundabout, but could create more rear-end shunt type collisions at the end of queues approaching the roundabout. Thus, future studies of cycling infrastructure need to consider the wider ranging impacts that any change in infrastructure may have on the behaviour of all road users and potential collisions.

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Yang L, Sahlqvist S. McMinn A. Griffin SJ, Ogilvie D. Interventions to promote cycling: systematic review. *British Medical Journal* 2010;**341**:c5293. [DOI: 10.1136/bmj.c5293]

* *Indicates the major publication for the study*

CHARACTERISTICS OF STUDIES

Characteristics of included studies *[author-defined order]*

Agerholm 2008

Methods	CBA Before data: 3 to 5 years After data: 3 to 5 years
Participants	I: 46 sections of road totaling 40 km in 17 towns in Western Denmark (19 of the largest municipalities and 8 counties were asked for data on relevant cycle projects) C: all roads in built up areas in the 19 municipalities which were originally part of the study. Only roads where there was no registered change in the facilities for cycle users were included Age of cyclists: no data.
Interventions	I: cycle paths: a one-way path next to the traffic lane and in the same direction as the traffic lane. It is separated from the lane by a kerb and elevated 7 to 12 cm. Cycles and unregistered mopeds with a speed limit of 30 km/h are allowed. On larger roads there were cycle paths on both sides. Paths were constructed between 1st January 1989 and 31st December 2000 C: no changes in the facilities for cycle users were registered
Outcomes	Police reported injury accidents: taken from the official Danish accident database. General accident data from the 19 municipalities originally included in the study. Accident data from the beginning of 1986 to the end of 2004 were included. Absolute numbers reported ADT for cyclists: collected only for a few studied segments.
Notes	-

Risk of bias

Bias	Authors' judgement	Support for judgement
Allocation concealment (selection bias)	High risk	There was no matching as the control group was all roads in built up areas that had not had changes for cyclists implemented The proximity of the intervention and control sites is unclear
Blinding of outcome assessment (detection bias) All outcomes	Low risk	Data came from the official Danish accident database based on police-reported accidents
Incomplete outcome data (attrition bias) All outcomes	Unclear risk	Exact time periods of 'before and after' accident data were unclear. Authors reported that they "should be 3 to 5 years"

Agerholm 2008 *(Continued)*

Selective reporting (reporting bias)	Low risk	Reported both increases and decreases in accident rates.
Bias due to confounding	High risk	Data on ADT were collected for the study sections; however, cyclist ADT was only available for some sections. No data presented. The author states that "No clear change in ADT was found for motorised vehicles nor for cyclists and moped riders. As there has been no increase in the number of users, this cannot be the reason for the increase in the number of accidents". There was no discussion of other possible confounders. Casualty rates were not adjusted for cycle flow
Other bias	Low risk	No other bias.

Allen 2005

Methods	CBA Before data: range 4 to 6 years After data: range 2 to 6 years
Participants	I: 12 sites from a range of locations within Greater London, UK. The sites had to be expected to have 100 cyclists passing over two days C: 2 sites within Greater London. Age of cyclists: reported on all ages.
Interventions	I: a range of layouts: • two carriageway lanes with a combined ahead and left turn lane on nearside (4 sites) • one carriageway lane allowing left, ahead and right turn lane movements (4 sites) • one carriageway lane onto a signalised roundabout (1 site) • three entry lanes with a separate left turn lane, no central feeder lane (1 site) • ASL feeder lane between a left turn lane and right turn/ahead carriageway lanes (2 sites). ASLs installed between 1997 and 2001. C: sites without an ASL with the right hand lane for straight over and/or right turn, and a left hand lane for straight over and/or left turn (2 sites)
Outcomes	Casualty rate: STATS19 data provided by Transport for London. Using data from 1992 to 2003 Cycle flows: were surveyed on 2 days at each site from 0700 to 1800 hours in 2003 (January to September), but at three sites observations were capped once 400 cyclists had passed (200 in the peak period (07:00 to 10:00) and 200 in the off peak (10:00 to 13:00)

Notes	Of the 12 intervention sites, only five sets of 'before and after' data are presented for the following reasons: • the installation data for four ASLs are unknown; and for one ASL site, installation occurred at the end of the data analysis period so for these five sites casualty rate data are presented for before-installation only. • 'after' data are "not available" for one site. • 'before and after' data are presented for six intervention sites, however, for two of these sites the data are presented as one figure.

Risk of bias

Bias	**Authors' judgement**	**Support for judgement**
Allocation concealment (selection bias)	Low risk	Intervention sites chosen based on feasibility of collecting video footage at the site and predicted number of cyclists using the arm of the junction across the day. Control sites were selected on the same basis as the intervention sites but without an ASL, that is, the number of cyclists passing the point of interest (the target being 100 over two days at each site) and the layout of the site in terms of the number and set-up of approach lanes and feeder lane. The control sites were also selected on the basis that ASLs may be installed at a later date Intervention and control sites were both selected from an area of Greater London. One control site was very close to an intervention site and one was within the same general area as the intervention sites
Blinding of outcome assessment (detection bias) All outcomes	Low risk	Casualty data from STATS19 data used from Transport for London's Road Safety Unit
Incomplete outcome data (attrition bias) All outcomes	High risk	The installation date was not known for some of the intervention sites so no 'after' data was presented for these sites or for the two control sites. The time periods before and after installation varied but the minimum was 4 years before and 2 years after The authors state that for the rate of casualties in the year before and the year after installation, the data "should be treated with caution due to the sheer variation in years of data applied before and after" 'Before and after' data only presented for five intervention sites

Allen 2005 *(Continued)*

Selective reporting (reporting bias)	Low risk	No bias.
Bias due to confounding	High risk	No assessment of confounders such as traffic mix, weather or land use Casualty rates were not adjusted for cycle flow.
Other bias	High risk	Of 12 intervention sites, 'before and after' data are presented for six sites only and of these, 2 sites form part of the same junction so only 5 sets of data are presented. For 2 intervention sites the date of installation of the ASL is unknown and for a further 2 sites, the date of installation is at the end of the study period. For control sites, only 1 figure is presented; 'before and after' data is not presented

Bovy 1988

Methods	CBA Before data: 3 years. After data: 2 years.
Participants	I: Area covering 9600 m² of Delft, Netherlands C: Area covering 4300 m² of Delft. Age of cyclists: no data.
Interventions	I: building new cycle paths, reconstructing bicycle paths, making short cuts, defining bicycle lanes, widening roads, installing and phasing traffic lights, abolishing one-way traffic for bicycles, permitting cyclist a free right turn at traffic lights, reconstructing intersections, providing crossover, building bridge, reconstructing bridge, building tunnel, providing bicycle stands at transit stops C: no changes in the facilities for cycle users (changes were made post collection of 'after' data)
Outcomes	Police-reported accidents. Bicycle travel: persons using a bike, trips per person per day, travel time per person per day (minutes), travel distance per person per day (km)
Notes	Study described in 4 included papers.

Risk of bias

Bias	Authors' judgement	Support for judgement

Allocation concealment (selection bias)	High risk	There was no matching; the control group was one area of the city where changes were yet to be implemented The proximity of the intervention and control sites is unclear
Blinding of outcome assessment (detection bias) All outcomes	Low risk	Accident data from police-reported accidents database.
Incomplete outcome data (attrition bias) All outcomes	Low risk	Data collection 3 years pre-construction and 2 years post construction
Selective reporting (reporting bias)	Unclear risk	No bias.
Bias due to confounding	High risk	Authors discussed changes in cycle flow with changes in collision rates but did not adjust collisions rates by exposure. Confounders such as time of day and season for cycle flow counts were discussed
Other bias	Unclear risk	No other bias.

Buckley 2000

Methods	CBA Before data: “period was of similar length to the after period” After data: “approximately 2 years in length”.
Participants	I: one road leading from the edge of the Central Business District (CBD) to the urban areas, Christchurch, New Zealand C: one control road with similar geometry (carriageway widths), traffic volumes and location to the intervention road Age of cyclists: no data.
Interventions	I: cycle lanes: street marked with cycle lanes since 1993. C: no details given of any changes.
Outcomes	Crashes involving injuries. Data from the Accident Investigation System (AIS)
Notes	The authors examine four intervention sites but one, Tuam Street, was observed to be “subject to vastly different environmental factors” and was therefore analysed separately from the other streets. Manchester Street acted as a control for Tuam Street The authors state that the AIS database contains only crashes involving injury which are required to be reported and by law only crashes involving motorised vehicles are required to be reported so some crashes, such as cycle vs cycle, are not included

Risk of bias		
Bias	**Authors' judgement**	**Support for judgement**
Allocation concealment (selection bias)	Low risk	The control area has a similar geometry (carriageway widths), traffic volumes and location
Blinding of outcome assessment (detection bias) All outcomes	Low risk	Accident Investigation System (AIS) database managed by Land Transport Safety Authority (LTSA)
Incomplete outcome data (attrition bias) All outcomes	Unclear risk	Exact time periods of 'before and after' accident data were unclear. Authors reported that 'after' data collection was "approximately 2 years in length" and the before period "was of similar length to the after period"
Selective reporting (reporting bias)	Unclear risk	Little data presented.
Bias due to confounding	High risk	Crash rates were not adjusted for cycle flow. Authors discuss the need to use a control site to minimise the variation in crash rates caused by factors such as change in reporting rate, changes in vehicle flow and general change in crash rates. However, the authors do not appear to have taken confounders into account in their analysis
Other bias	High risk	The authors state that following data analysis it was clear that the control site may have "some unique problems of its own"

Carlson 1975

Methods	CBA Before data: 3.5 years After data: 10 months
Participants	I: A route in Seattle, USA, with a known high cycle usage. C: (1) areas immediately outside the cycle route and (2) surrounding area Age of cyclists: no data.
Interventions	I: 'Demonstration Bikeway One': 2 miles long, linking Greenlake Park to the University of Washington Campus, established August 1973. It included: • 'strong yellow/green' boundary lines • motor vehicles must yield right-of-way to cycles when turning left

	• where the roadway width narrowed removing the possibility of a designated cycle lane, signing indicated that cycles have preference over motor vehicles on the entire roadway • traffic control buttons for cyclists at some intersections. C: no details given of any changes.
Outcomes	Reported cycle/motor vehicle accidents. Cycle counts: taken at 5 locations, 3 on the bikeway and 2 at non-bikeway locations. Morning peak period (08:00 to 09:30) cycle counts taken pre-intervention May 1973 and post-intervention December 1973. An additional count was also made post-intervention only on the bikeway. Presents percentage change in cycle flows only
Notes	-

Risk of bias

Bias	Authors' judgement	Support for judgement
Allocation concealment (selection bias)	High risk	No matching of intervention and control areas. Control areas are the surrounding area but actual proximity unknown
Blinding of outcome assessment (detection bias) All outcomes	Unclear risk	No information provided on the source of accident data.
Incomplete outcome data (attrition bias) All outcomes	High risk	For accident data 3.5 years pre-intervention but only 10 months post-intervention
Selective reporting (reporting bias)	Low risk	No bias.
Bias due to confounding	High risk	No discussion of confounding. The authors do not take exposure into account when considering changes in accident rates
Other bias	Low risk	No other bias.

Chen 2012

Methods	CBA Before data: 5 years After data: 2 years
Participants	I: roadway segments on 61 streets, totaling 43 miles in the 5 boroughs of New York City, USA C: locations with segment or intersection-level characteristics comparable to those of the intervention group

Interventions	I: on-street cycle lanes (not protected by a parking lane), installed over 10 years from 1996 to 2006 C: no cycle lane. Age of cyclists: no data.
Outcomes	Police-reported cyclist crashes on roadway segments and at intersections
Notes	As the cycle lanes were installed at different times over 10 years the actual 5 years before and 2 years after for each intervention site comprised different calender years. Thus the treatment group was divided into multiple subsets defined by the year of installation and for each subset, the authors selected untreated locations by applying frequency-matching techniques to resemble the distribution of segment-level and geographic distribution of the treatment group The control group was divided into 2 subgroups: segments and intersections. Selection of control segments was based on 3 segment-level factors that have significant impact on crashes: 1-way versus 2-way roads; divided versus undivided roads (if they are 2-way roads); and number of travel lanes. Untreated segments were more likely to be scattered around the city, therefore segments that were parallel to the segments in the treatment group were manually selected

Risk of bias

Bias	Authors' judgement	Support for judgement
Allocation concealment (selection bias)	Low risk	Matching of control and intervention sites. "Many of the bicycle lane segments were part of long corridors" on the same road "whereas those in the comparison group were more likely to be scattered around the city". "We manually selected roadway segments that were parallel to those in the treatment group and added them to the comparison group"
Blinding of outcome assessment (detection bias) All outcomes	Low risk	Police-reported crash data but no name given for data source
Incomplete outcome data (attrition bias) All outcomes	Low risk	Data collection period before installation was 5 years and after 2 years
Selective reporting (reporting bias)	Low risk	No bias.
Bias due to confounding	High risk	The authors state that they controlled for exposure and conflicts by entering neighbourhood-level and site-level (segment versus intersection) variables into the model. At the neighbourhood level the authors

		used daytime population density, retail density and bicycle trip density. Site-level variables included the presence of bus stops or parking on road segments, whether the segment was a truck route, control type (signalised or not) and the number of arms at the intersection. Authors state they did not control for before-after differences in cyclist volumes because the data were not available
Other bias	Low risk	No other bias.

Daniels 2009a

Methods	CBA Before data: at least 3 years After data: 1 year
Participants	I: 90 roundabouts in Flanders, Belgium. C: 172 intersections on regional roads in the neighbourhood of the roundabouts. To avoid possible interaction effects between the control and observed roundabout locations, control locations had to be at least 500 m away from the observed location roundabouts Age of cyclists: no data.
Interventions	I: roundabouts with different designs: • mixed traffic • cycle lanes within the roundabout • separate cycle paths • grade-separated cycle paths where cycle traffic flows independently of the motorised traffic and whether priority is given to cyclists or not. Additionally acquired data included: • the presence of line markings between the carriageway and cycle lane • the presence of a physical barriers between the roundabout and the cycle facility (for example, a curbstone) or an elevation between the carriageway and cycle lane. • pavement colour. Constructed between 1994 and 2000. C: no details given of changes.
Outcomes	Accidents involving at least one cycle. Data from the National Statistics Institution. Data used from 1991 to 2001
Notes	This is a controlled before-after study using the Empirical Bayes method to control for general trends in traffic safety and possible effects of regression to the mean Study described in four included papers: • Daniels B, Nuyts E, Wets G. Design types of cycle facilities at roundabouts and their effects on traffic safety: some empirical evidence. Velo-City Conference 2009. • *Daniels S, Brijs T, Nuyts E, Wets G. Injury crashes with bicyclists at roundabouts: influence of some location characteristics and the design of cycle

	facilities. Journal of Safety Research 2009;40(2):141-8. • Daniels S, Brijs T, Nuyts E, Wets G. The effects of roundabouts on accidents with bicyclists: influence of the design type of cycle facilities. XXth ICTCT workshop 25/10/2007-26/10/2007;Valencia, Spain. • Daniels S, Nuyts E, Wets G. The effects of roundabouts on traffic safety for bicyclists: an observational study. Accident Analysis and Prevention 2008;40:518-26.

Risk of bias

Bias	Authors' judgement	Support for judgement
Allocation concealment (selection bias)	Low risk	Control group intersections were on regional roads in the neighbourhood of the intervention roundabouts. "Preference for comparison locations was given to intersections on the same main road as the nearby roundabout location with the same type of crossing road" The intersection was at least 500 m from the roundabout to reduce interaction effects
Blinding of outcome assessment (detection bias) All outcomes	Low risk	Crash data came from the National Statistical Institution.
Incomplete outcome data (attrition bias) All outcomes	Low risk	'Before' period is at least 3 years and the 'after' period is 1 year
Selective reporting (reporting bias)	Low risk	No bias.
Bias due to confounding	High risk	Bias due to confounding factors: authors state that "confounding factors might exist that were not controlled for". The authors do not take exposure into account when considering changes in accident rates
Other bias	Low risk	No other bias.

Gårder 1998

Methods	CBA Before data: minimum 67 months before. After data: varying but 34 months for most intersections.
Participants	I: 44 intersections along 18.7 km of road sections in Gothenburg, Sweden. 6 intersections (4 reconstructed and 2 control) were studied in detail for the accident analysis. "Practically all" completed by 1993

	C: 1898 intersections in central Gothenburg. Cyclists ride either in the roadway or on separate paths parallel to the roadway. The paths then end with short ramps or curb cuts at each cross street and cyclists use non-elevated, marked cycle crossings, similar to pedestrian crosswalks but delineated by painted white rectangles rather than zebra stripes Age of cyclists: no data.
Interventions	I: Raised cycle crossings at intersections and accentuated with red pavement. Cycle crossings were elevated to a level similar to sidewalks. Some crossings were elevated by as much as 12 cm having 20 cm long ramps for crossing motor vehicles, while others were raised by only 4 to 6 cm and having almost vertical mountable kerbs. All the cycle paths run parallel to major streets (minor arterials) and the raised crossings cross minor roads C: no changes.
Outcomes	Police- and hospital-reported accidents: from the city of Gothenburg, using data from January 1988 to September 1996 Cycle flow counts: taken at 2 intervention and 2 control sites, 2 weeks before and 2 weeks after reconstruction
Notes	Study described in four included articles: • *Gårder P, Leden L, Pulkkkinen U. Measuring the safety effect of raised bicycle crossings using a new research methodology. Transportation Research Record 1998;1636:64-70. • Leden L, Claesson A, Gårder P, Näsman P, Pulkkinen U, Thedén T. Metodik för före-/-efterstudier. Tillämpat på cyklisters trafiksäkerhet. KFB-Rapport 1997:145 1997;Stockholm, Sweden. • Leden L. Has the City of Gothenburg found the concept to encourage bicycling by improving safety for bicyclists? In: Velo-City. Barcelona, 1997. • Leden L. Safer junction design encouraged bicycling in Gothenburg. In: Proceedsings of the Conference. Traffic Safety on two continents. Lisbon, Portugal, VTI konferens 9A, Part 8,1997.

Risk of bias

Bias	Authors' judgement	Support for judgement
Allocation concealment (selection bias)	High risk	No matching of intervention and control sites. Central Gothenburg was used as the control group so some roads proximal and some distal
Blinding of outcome assessment (detection bias) All outcomes	Low risk	City of Gothenburg database containing police-reported and hospital-reported accidents
Incomplete outcome data (attrition bias) All outcomes	Low risk	Before data: minimum 67 months before. After data: varying but 34 months for most intersections.

Selective reporting (reporting bias)	Low risk	No bias.
Bias due to confounding	High risk	Authors state that before/after data reflects "the effect of changes in other factors such as traffic volume, weather, vehicle fleet, driver behavior, inclination to report accidents, and so forth". Cycle flow data not taken into account
Other bias	Low risk	No other bias.

Grundy 2009

Methods	Time series regression study Time periods before and after for each road varied. The date of implementation of each zone is known and roads were classified as pre-intervention, under construction or post-intervention for each financial year Maximum 'after' period of 15 years (1991 to 2006).
Participants	I: roads that were in, or would become part of, a 20 mph zone. Interventions were introduced from 1991 to 2008. Information was available on 385 out of 399 20 mph zones (Grundy 2009). In terms of kilometres of road in 20 mph zones, 115 km (2.5%) were in the least deprived quintile, 238km (5.9%) were in the second least deprived, 362 km (9.7%) were in the third least deprived, 561 km (17.7%) were in the fourth least deprived and 731 km (27.5%) were in the most deprived quintile In terms of social deprivation, 43 of 399 zones (11%) were in the least deprived quintile, 54 zones (14%) were in the second least deprived, 84 zones (21%) were in the third least deprived, 94 zones (23%) were in the fourth least deprived and 124 zones (31%) were in the most deprived quintile C: areas 'adjacent' to a 20 mph zone, i.e. all road segments that connected to road junctions within 150 m of a 20 mph zone, and all other roads Age of cyclists: 0 to 15 years, 16 years and over.
Interventions	I: 20 mph zones. C: no 20 mph zone.
Outcomes	Police-reported cyclist casualties, by severity and age. Data from 1986 to 2006. STATS19 data
Notes	Study described in four included articles: • Grundy C, Steinbach R, Edwards P, Wilkinson P, Green J. 20 mph zones and road safety in London. A report to the London Road Safety Unit. 2008;London: London School of Hygiene and Tropical Medicine. • Grundy C, Steinbach R, Edwards P, Wilkinson P, Green J. The effect of 20 mph zones in inequalities in road casualties in London. A report to the London Road Safety Unit. London: London School of Hygiene and Tropical Medicine 2008. • *Grundy C, Steinbach R, Edwards P, Green J, Armstrong B, Wilkinson P. Effect

	of 20mph traffic speed zones on road injuries in London, 1986-2006: controlled interrupted time series analysis. BMJ 2009;339(b4469 doi:10.1136). • Steinbach R, Grundy C, Edwards P, Wilkinson P, Green J. The impact of 20 mph traffic speed zones on inequalities in road casualties in London. J Epidemiol Community Health 2011;65:921-6. doi:10.1136/jech.2010.112193.

Risk of bias

Bias	**Authors' judgement**	**Support for judgement**
Allocation concealment (selection bias)	Low risk	20 mph zones were compared to adjacent areas and all other roads in London
Blinding of outcome assessment (detection bias) All outcomes	Low risk	STATS19 data used and linked to road segment data.
Incomplete outcome data (attrition bias) All outcomes	Unclear risk	Used outcome data covering a period of 20 years; dates of introduction of the 20 mph zones reported but how many years of 'before' and 'after' is not reported clearly
Selective reporting (reporting bias)	Low risk	No bias.
Bias due to confounding	High risk	The authors state "we could not take into account the potential impact of other road safety initiatives such as road safety cameras". It might be expected that such confounding in 20 mph zones would also be seen on other roads. The authors do not take exposure into account when considering changes in accident rates
Other bias	Low risk	No other bias.

Harland 1993

Methods	CBA Before data: 2 years After data: 2 years
Participants	I: Six cities: Exeter, Kempston, Nottingham, Stockton, Cambridge and Southampton, UK. (Originally Canterbury was to be included too but opposition to the cycle route prevented its construction at that time.) C: Adjoining urban areas in the cities. Age of cyclists: no data - (Dean 1993 and Harbidge 1993: 0 to 19 years, 20 years and over).

Interventions	I: • Exeter: 6 km long route connecting the city centre and housing to the north and south with a trading estate across the river. A cycle bridge was constructed across the River Exe. Constructed from 1987 to 1988. Opened June 1988. • Kempston: a fine grid cycle network connecting to a new route approximately 5km long which connected Kempston, a community on the south west outskirts of Bedford, to Bedford town centre. Constructed from 1986 to 1990. Opened June 1986. • Nottingham: 12 km of new cycle routes to add to an existing cycle route bringing the total to 30 km in the south west part of Nottingham. It links housing and employment centres in the south west of the city to the city centre and was considered useful for the journey to work. Constructed from 1986 to 1990. Opened 1990. • Stockton: approximately 4 km cycle route linking residential districts, schools and colleges with the town centre. Provides an alternative to heavily trafficked roads. Constructed 1985. Opened May 1985 • Cambridge: The South East Cambridge Cycle Route linking two busy radial roads, including a new bridge 237 m long over the railway line. Constructed from 1988 to 1989. Opened 1989. • Southampton: 8 km long cycle route linking the city centre with commercial and residential districts to the west, providing an alternative to cycling on dual carriageway roads with large roundabouts and slip roads. Constructed 1987. Opened November 1987. C: no details given of any changes.
Outcomes	Police reported cyclist casualties: taken from STATS19 data, 1983 to 1991 but only two years before and two years after for each city Cycle flow: presented as average weekday cycle flow in each city (1984 to 1990) and 'before and after' data, on and off the cycle route (three cities only)
Notes	Study described in six included articles that report either on the evaluation of all six cycle routes or on individual cycle routes • Dean JD. The Stockton Cycle Route after study. Transport Research Laboratory, Department of Transport 1993;334. • Gercans R, Harland G. Cycle-routes. In: Velo-city. Milano, Italy, 1991:114-20. • Harbidge J, Henley S, Jones RB. Kempston Urban cycle route project after study. Transport Research laboratory 1993. • *Harland DG, Gercans R. Cycle Routes. Transport Research Laboratory 1993;Project Report 42 H5/11B. • Harland G. Cycle routes research in the UK. In: Velo-city. Nottingham, England, 1993:529-36. • Shipley F. The Southampton Western Approach Cycle Route. Cyclist Flows and Accidents. Transport Research Laboratory 1994; Project report 93.

Risk of bias

Bias	Authors' judgement	Support for judgement
Allocation concealment (selection bias)	High risk	No matching. Control areas were based on adjoining urban areas. "The experimental

		areas include the entire extent of the experimental cycle routes together with related features and also any main roads from which the routes were expected to attract cyclists. The control areas were urban areas adjoining the experimental areas in all the towns except Exeter" where the routes and connected traffic calming "gave a fairly extensive coverage of much of the city and so the area affected by the routes was defined as the whole of Exeter City district and the villages and towns of the surrounding three districts provide the control casualties". Therefore some control areas were proximal to intervention areas and some distal
Blinding of outcome assessment (detection bias) All outcomes	Low risk	STATS19 data used.
Incomplete outcome data (attrition bias) All outcomes	Low risk	Data collection period was 2 years pre- and 2 years post-construction
Selective reporting (reporting bias)	Low risk	No bias.
Bias due to confounding	High risk	Modelling took into account the effect of town, area, road and period (before or after) . Cycle flow was not used in the modelling
Other bias	High risk	Does not report cycle flow on and off the cycle route before and after opening of the cycle routes for all 6 cities

Jensen 1997

Methods	CBA Before data: between 24 and 60 months (average 50 months) After data: between 24 and 60 months (average 50 months)
Participants	I: 251 junctions on 37 stretches of roads with cycle lanes (1.0 m to 1.6 m wide), marked by cycle symbols and separated from the vehicle lane by a continuous white line, established 1985 to 1990 in Denmark. Of the 251 junctions, 34 were signal controlled, 217 right-of-way, 209 were a 3-arm junction and 42 were a 4-arm junction C: 262 control junctions where no cycle path is present. The control crossings are chosen so that they are as similar to the crossings with cycle lanes as possible. Of the 262 junctions 35 were signal controlled, 227 right-of-way, 195 were a 3-arm junction and 67 were a 4-arm junction

	Age of cyclists: 0 to 17 years, 18to 24 years, 25to 69 years, 70 years and over
Interventions	I: signals C: priority
Outcomes	Cycle accidents with casualties: occurring at junctions (signalised and priority junction treatment)
Notes	Study is described in two included articles: • *Jensen SU, Andersen KV, Nielsen ED. Junctions and cyclists. In: Velo-City conference proceedings. Barcelona, 15-19 September 1997. • Nielsen ED, Andersen KV, Lei KM. The safety effect of cycle lanes [Trafiksikkerhedseffekten af cykelbaner ibyområder Rapport nr. 50]. VejdirektoratetTrafiksikkerhed- og Miljøafdelingen 1996.

Risk of bias

Bias	Authors' judgement	Support for judgement
Allocation concealment (selection bias)	Low risk	Matching of sites on proximity and number of arms at intersection
Blinding of outcome assessment (detection bias) All outcomes	Unclear risk	Source of accident data not given.
Incomplete outcome data (attrition bias) All outcomes	Low risk	The same length of time was used pre- and post-intervention for each junction, minimum 2 years and maximum 5 years
Selective reporting (reporting bias)	Low risk	No bias.
Bias due to confounding	High risk	No assessment of confounding factors reported. The authors do not take exposure into account when considering changes in accident rates
Other bias	Low risk	No other bias.

Jensen 2013

Methods	CBA Before data: 5 (calendar) years After data: 1 to 5 years
Participants	I: 332 intersections in 61 municipalities in Denmark. C: The rest of the municipality. Age of cyclists: no data.

Interventions	I: Conversion of intersections to roundabouts with the number of roads on the intersection being the same as the number of roundabout arms. A range of cycle facilities: • none, priority to cyclists (n = 28) • cycle lane with priority to cyclists (n = 55) • coloured cycle lane with priority to cyclists (n = 16) • cycle track with priority to cyclists (n = 17) • cycle track with blue cycle crossings and priority to cyclists (n = 8) • cycle path with no priority to cyclists (n = 18). Conversions took place 1995 to 2009. C: no details of any changes given.
Outcomes	Cycle crashes including PDO crashes (crashes recorded by Danish police). Data used from 1985 to 2010
Notes	-

Risk of bias

Bias	**Authors' judgement**	**Support for judgement**
Allocation concealment (selection bias)	High risk	No matching. The control area was the whole area minus the intersections so some proximal some distal
Blinding of outcome assessment (detection bias) All outcomes	Low risk	Police-reported data (Danish police).
Incomplete outcome data (attrition bias) All outcomes	Low risk	5 years pre-intervention. 1 to 5 years post-intervention.
Selective reporting (reporting bias)	Low risk	No bias.
Bias due to confounding	High risk	The methodology used accounts for crash trends and regression to the mean but not traffic volumes. The authors state that "it is not possible to account for changes in traffic volume"
Other bias	Low risk	No other bias.

König 2006

Methods	CBA Before data: 1 to 4 years After data: 0 to 4 years
Participants	I: 71 intersections on arterial streets in Lund, Sweden. C: 15 intersections on arterial streets in Lund. Age of cyclists: no data.
Interventions	I: reconstruction of cycle crossings, consisting of 3 parts: • two ramps either side • one flat part between the ramps for pedestrians coloured grey • one flat part between the ramps for cyclists coloured red. The specific design depends on the characteristics of individual intersections. Some are combined with a refuge. Reconstructed 1998 to 2004 C: combined crossings for cyclists and pedestrians. The pedestrians crossing is a zebra crossing. The cycle crossing has borders of white squared markings on one side and zebra markings on the other. It is not marked in any colour Age of cyclists: no data.
Outcomes	Police-reported and hospital-attended accidents: 1999 onwards Cycle flow data for 2 pairs of intervention-control intersections only and on one occasion only (not before-after)
Notes	The author examined fatal road accident rates in Sweden, Skåne and Lund, representing national, regional and municipality rates respectively, to assess whether changes in accident rates in Lund mirror changes nationally and regionally

Risk of bias

Bias	**Authors' judgement**	**Support for judgement**
Allocation concealment (selection bias)	High risk	There was no matching for the controlled 'before and after' accident analysis Proximity of the reconstructed and non-reconstructed intersections not described but as all the intersections are in Lund some will be proximal and some distal
Blinding of outcome assessment (detection bias) All outcomes	Low risk	Accident data was from the Swedish Traffic Accident Data Acquisition (STRADA), a traffic accident registration programme containing data from both police stations and hospitals, especially casualty departments
Incomplete outcome data (attrition bias) All outcomes	High risk	'Before' data collection varied from 1 to 4 years and 'after' data collection varied from 0 to 4 years

Selective reporting (reporting bias)	Low risk	No bias.
Bias due to confounding	High risk	No assessment of confounding factors for analysis of cycle accident data. The authors do not take exposure into account when considering changes in accident rates
Other bias	Low risk	No other bias.

Mountain 2005

Methods	CBA Before data: 3 years After data: up to 3 years (average 2.5)
Participants	150 speed management schemes at various locations throughout Great Britain on roads with 30 mph speed limits I1: 79 schemes using speed enforcement cameras. I2: 71 schemes using engineering measures. Age of cyclists: child, adult.
Interventions	I1: 79 speed enforcement cameras (fixed n = 62 and mobile n = 17) I2: 71 engineering measures were any form of vertical deflection (with or without narrowing or horizontal deflections) and those with narrowing or horizontal deflections only: • vertical deflections include any measure that alters the vertical profile of the carriageway such as roads humps and speed cushions; • narrowing includes any measure used as part of a speed management scheme to reduce the carriageway width available to moving traffic, for example, pinch points, central hatching, traffic islands; • horizontal deflections include measures that alter the horizontal alignment of the carriageway such as mini roundabouts, build-outs, and chicanes (with either one- or two-way working). Four schemes used speed-activated signs to control speeds (grouped with horizontal deflections for analysis) and one site used 30 mph speed warning roundels painted on the carriageway. This latter scheme was subsequently dropped from the analysis. Thus, of 70 schemes, 31 had horizontal deflections, narrowing or speed-activated signs and 39 had vertical deflections
Outcomes	All accidents occurring at the schemes (obtained from various Local Authorities and Police Forces)
Notes	-

Risk of bias

Bias	**Authors' judgement**	**Support for judgement**

Allocation concealment (selection bias)	High risk	30 mph roads were selected where speeding was a significant problem. No information given on how the decision was made as to which roads received which type of intervention (cameras or engineering measures)
Blinding of outcome assessment (detection bias) All outcomes	Low risk	Accident data obtained from various local authorities and police forces
Incomplete outcome data (attrition bias) All outcomes	Low risk	3 years 'before' data and 3 years 'after'.
Selective reporting (reporting bias)	High risk	One scheme was excluded from the analysis: "The scheme with painted roundels on the road was not successful in reducing accidents and, as it does not fit naturally into any other group, was excluded from the analysis."
Bias due to confounding	High risk	Confounding discussed but not taken into account for cycle accidents. The change in accident rates will be affected by trend and regression to the mean. "The absence of predictive models for cyclist and pedestrian accidents or data for control sites, meant that it was not possible to correct the observed changes in accidents involving vulnerable road users for RTM (regression to the mean) effects". The authors do not take exposure into account when considering changes in accident rates
Other bias	Unclear risk	"As the effects of the 4 speed-activated signs were found to be similar to horizontal deflections and narrowing, these were grouped together for subsequent analysis."

Nicholson 1979

Methods	CBA Before data: 5 years After data: 1 year
Participants	I: Residential roads in Portsmouth, UK. C: Roads without the intervention in Portsmouth. Age of cyclists: no data.

Interventions	I: An experimental cycle route in operation between November 1975 and June 1976 6.3 km of cycle routes, one essentially north-south and the second east-west following the general direction of popular cycle flows and giving access to shopping areas and schools. The routes were laid in relatively quiet roads, mainly fronted by residential property Details of the route include: • Carriageway markings: a central 2-way cycle lane marked by 4 m lengths of white line with 2 m gaps making 'hazard lines'. At intervals along these lanes a cycle symbol was painted to remind motorists that it was a designated cycle route. • Restriction of motor traffic to those requiring access to properties on the routes only. • Of 54 junctions on the cycle route, 49 had a special layout compelling motor traffic to turn left at the junction although cyclists on the cycle route were allowed to continue straight across the junction. • Road signs: road signs to designate cycle routes, a new red-bordered triangular warning sign containing a cycle symbol to warn motorists of a cycle route crossing the junction ahead and a blue-bordered flag-type direction sign to indicate to cyclists the destination of a branch of the cycle route (both approved by Department of Evnrionment for experimental use). • Junction layout: ○ 42 cross roads or 'T' junctions where priority was given to the cycle route by use of 'GIVE WAY' road signs and markings, and by having the road markings delineating the cycle route marked across the junction. In addition there was one right-left stagger junction and one left hand stagger junction. ○ 5 special junctions of which 4 displayed 'TURN LEFT' road signs and markings at all approaches for all traffic except cyclists and aimed to prevent motor traffic from interfering with cyclists and intended to dissuade traffic from using the route or crossing it. In addition one 'T' junction displayed a 'NO RIGHT TURN' road sign to prevent traffic crossing the cycle route. ○ 5 cross roads with priority against the cycle route. The cycle route stopped short of the junction and continued in the road opposite, in effect breaking the cycle route where major roads crossed the cycle route. C: no details given of any changes.
Outcomes	1. Police-reported cycle accidents: using STATS19 (January 1970 to June 1977) 2. Cycle flows. 13-hour counts (0600 to 1900 hours) were undertaken approximately every 3 months starting June 1975 at 20 sites: • 6 on the cycle route • 2 on a major road parallel to the cycle route • 2 on a residential road parallel to the cycle route • 3 on a link route • 2 on a major road parallel to the link route • 5 at the entrance to the Naval Base. Regular counts were made between 1530 and 1715 hours at two sites on the cycle route. They were conducted approximately every 3 months before, every two weeks during and every month after the experiment
Notes	Study described in two included articles: • *Nicholson FJ. Cycle routes in Portsmouth. II - Traffic studies. Transport and Road Research Laboratory 1979;TRRL Laboratory report 874.

	• Quenault SW, Head TV. Cycle routes in Portsmouth. I - Planning and implementation. Transport and Road Research Laboratory 1977; Supplementary report 317.

Risk of bias

Bias	Authors' judgement	Support for judgement
Allocation concealment (selection bias)	High risk	No matching of control and intervention areas. The control area was all Portsmouth so some areas proximal and some distal to intervention
Blinding of outcome assessment (detection bias) All outcomes	Low risk	STATS19 data used, compiled by Hampshire County Council from accident report forms supplied by Hampshire Constabulary
Incomplete outcome data (attrition bias) All outcomes	Low risk	'Before' data collection was 5 years and 'after' data collection was 1 year
Selective reporting (reporting bias)	Low risk	No bias.
Bias due to confounding	High risk	No assessment of possible confounders. Cycle flow was not taken into account when considering accidents
Other bias	Low risk	No other bias.

Parsons 2013

Methods	CBA Pre-intervention: typically 5 years Post-intervention: typically 5 years
Participants	I: arterial routes in Christchurch, New Zealand. C: arterial routes in Christchurch. Age of cyclists: no data.
Interventions	I: cycle lanes, marked by a continuous solid line (Personal communication with author) installed on 12 routes in Christchurch (4 major arterials, 7 minor arterials, 1 collector), located across the city, varying in length from 800 m to 3800 m, varying rates of annual average daily traffic (000s) from 5 to 8 up to 32 to 39 C: 3 routes "treated well before this period", pre-2000, marked by dashed line (Personal communication with author), all minor arterials, located in 3 different geographical areas of the city, varying in length from 1100 m to 3150 m, varying rates of annual average daily traffic (000s) from 12 to 14 up to 20 to 25

Outcomes	Cycle crash data (1999 and 2009). Cycle counts.
Notes	-

Risk of bias

Bias	Authors' judgement	Support for judgement
Allocation concealment (selection bias)	Unclear risk	No information on matching. No information on proximity of control areas to intervention areas but some control streets located in same parts of the city as intervention streets
Blinding of outcome assessment (detection bias) All outcomes	Low risk	Data from Crash Analysis System (CAS) database, police traffic crash reports
Incomplete outcome data (attrition bias) All outcomes	Unclear risk	Data collection pre- and post-intervention "typically 5 years pre-treatment, 5 years post treatment and one treatment affected year" but not reported for each route
Selective reporting (reporting bias)	Low risk	No bias.
Bias due to confounding	Low risk	Underlying trends in cycle and crash numbers were accounted for. The authors state that "changes to the traffic conditions and types of traffic may have influenced some of the sample sites in the study". These were not accounted for in the analysis
Other bias	Low risk	No other bias.

Quenault 1981

Methods	CBA Before data: 3.5 years After data: 1.5 years
Participants	I: A route from Peterborough city centre to Bretton, on the north-western outskirts, UK C: all of Peterborough. Age of cyclists: no data.
Interventions	I: A cycle route to provide an interrupted route for cyclists, segregated from motor traffic wherever possible. Opened July 1977 Consisting of:

	• Contra-flow cycle lane marked by a continuous white line, cycle symbols on the carriageway and traffic signs. The lane line must not be crossed by motor vehicles except to gain access to premises. The lane is 1.3 m wide. Cyclists travel in the opposite direction to the general traffic flow. • Link roads: roads with low traffic flow designated as an advisory route for cyclists, signed by advisory cycle route signs and possibly direction signs for cyclists. On this route most of the link roads are closed at one end to all traffic except pedestrians and cyclists. Junctions may be controlled by traffic lights or priority given to the link road. • Cycle tracks: purpose-built ways for cyclists with or without adjacent footways. Generally cyclists may cycle in either direction on cycle tracks. Cycle tracks may be signed by mandatory or advisory signs and are sometimes marked with cycle symbols on the surface. There are two lengths of 3 m wide, two-way cycle tracks with adjacent pedestrian path, separated from a raised 2 m wide pedestrian path by a sloping brick course, and running parallel to roads. Other cycle tracks on the route are 2 m wide and are without adjacent pedestrian paths. • With-flow advisory cycle lane: an unregulated lane marked by a hazard line (broken white line), cycle symbols and traffic signs. The lanes may be crossed by motor vehicles if safe to do so. The 1 m advisory with-flow lane runs east-west on a bridge and a segregated one-way cycle track (2.4 m wide) runs west-east on the bridge. The cycle track is reinforced by a ban on cyclists travelling west-east on the road. Advisory with-flow cycle lanes run east-west and west-east along another road, lanes are 1.0 to 1.3 m wide and are discontinued across junctions where there is a side street. • Shared cyclists/pedestrian path: several un-segregated paths were installed, measuring 2 to 3 m wide. These are controlled by either mandatory or advisory ‘pedestrian and cyclists only’ signs and indicated by cycle direction signs. No special markings are provided on the path itself. There is also a short stretch of a segregated path, where both the cyclist and pedestrian parts are 1 m wide and are divided by a continuous white line. The part of the path used by cyclists may be marked with symbols and the path may be controlled either by mandatory or advisory ‘pedestrian and cyclists only’ signs. • Cycle junctions: both cycle junctions had three traffic phases and one phase solely for cycles, conventionally signalled with System D detection. • Cycle crossings: two crossings used conventional Systems D detection. One crossing contained no special features for cyclists. Four road crossings gave priority to motor traffic. C: no details given of any changes.
Outcomes	1. Police-reported cyclist accidents: including severity (slight, serious, fall) (January 1974 to October 1978) 2. Cycle counts: • at primary sites located on the cycle route; • at secondary sites close to, but not on, the cycle route; • at control sites well away from the cycle route. Some counts included adults versus children. (March 1977 to October 1978)
Notes	Study described in two included articles: • Quenault SW, Morgan JM. Cycle routes in Peterborough: interim report. Transport and Road Research Laboratory 1979;904. • *Quenault SW. Peterborough experimental cycle route. Transport and Road

	Research Laboratory 1981; TRRL Laboratory report 975.

Risk of bias

Bias	Authors' judgement	Support for judgement
Allocation concealment (selection bias)	High risk	No matching of control and intervention areas. The control group comprised all Peterborough, with some areas distal and some proximal to intervention route
Blinding of outcome assessment (detection bias) All outcomes	Low risk	All road accidents reported to police and obtained from Cambridgeshire County Council
Incomplete outcome data (attrition bias) All outcomes	Low risk	Before data collection 3.5 years. After data collection 1.5 years.
Selective reporting (reporting bias)	Low risk	No bias.
Bias due to confounding	High risk	Exposure was not taken into account when examining accident rates. Considered the effect of weather, population, employment, shopping floor space and road conditions on cycle flows
Other bias	Unclear risk	Some missing data in cycle counts.

Smith 1988

Methods	CBA Before data: 4 years After data: 4 years
Participants	I: one-way arterial street pair in the City of Madison, Wisconsin, USA C: all of the City of Madison. Age of cyclists: no data.
Interventions	I: cycle lanes for the 1.3 mile one-way street-pair, Johnson and Gorham, to provide a continuous and convenient cycle facility through the corridor to the north of the Capitol. Installed in September 1977. An exclusive cycle lane was placed on the left hand side of Johnson Street (unconventional placement) and on the right hand side of Gorham Street (conventional placement) • On Johnson Street west of Butler Street the 39-foot wide pavement was re-striped to provide a 6-foot cycle lane on the left, two narrowed through traffic lanes and a right hand curb parking lane with no parking during peak hours. East of Butler Street the 44-foot wide pavement was striped for a 13-foot combined parking and cycle lane on

	the left hand side, a 10-foot through lane and a 21-foot combined through and parking lane. • On Gorham Street the combined 13-foot cycle and parking lane was located on the right hand side and the two through vehicle lanes on the left. Preferential lane signing and pavement markings were used to designate the intended uses of the bike lane, for example, "Left Lane - Bicycles and Left Turns Only" signs were placed in areas where parking was prohibited. C: no details given of any changes/cycling infrastructure.
Outcomes	Cycle accidents: using data from the Madison Department of Transportation Cycle counts: taken originally at 3 intersections within central Madison. Counts at the intersections east and west side of the central area continued but were dropped at the third intersection on the university campus due to large variation in the count volumes. All cycles entering the intersection were counted for 2 hours during the morning and afternoon peak periods once a month. The counts were then factored to produce an estimate of total daily cycle trips citywide based on a 1974 survey of cycle travel
Notes	In effect 3 interventions were examined: cycle lane, cycle lane with parking, and cycle lane with parking placed on the 'wrong' side of the street. Author used time series analysis

Risk of bias

Bias	Authors' judgement	Support for judgement
Allocation concealment (selection bias)	High risk	No matching of control and intervention areas. The control group comprised the whole of Madison, to account for trends in accidents. Control area encompassed intervention area so some proximal and some distal
Blinding of outcome assessment (detection bias) All outcomes	Low risk	Accident data from Madison Department of Transportation.
Incomplete outcome data (attrition bias) All outcomes	Low risk	'Before' data collection 4 years. 'After' data collection 4 years.
Selective reporting (reporting bias)	Low risk	No bias.
Bias due to confounding	Low risk	Discuss threats to validity of history (other causes at the same time), regression to the mean, maturation (trends over time) and instability because of chance or random fluctuations in data, although authors state that only the latter two may threaten validity in this study and consider accident rates in the light of these. No discussion on how other factors such as environmental or geo-

		graphical may influence accident rates. The authors took exposure into account when considering changes in accident rates
Other bias	Low risk	No other bias.

Webster 2003

Methods	CBA Before data: 5 calendar years After data: Average 3 years (range 1 to 5 years)
Participants	I: London Boroughs, UK. Boroughs provided detailed information on 115 of a possible 137 20 mph zones. Implemented between December 1989 and May 2003. Of the 115 zones, 78 had been in long enough to provide 12-months post-intervention data C: All unclassified roads in London for zone roads, all A and B roads in London for boundary roads Age of cyclists: 0 to 15 years, 16 years and over.
Interventions	I: 20 mph zones in London. 102 (89%) zones were area zones and 13 (11%) were linear zones; that is, they consisted of one road. There were 40 (35%) purely residential zones, 70 zones (61%) containing schools and 5 zones (4%) which were town/city centres or mainly commercial zones Types of speed reduction measures used include (information available for 95 zones): • road humps: flat-top humps in 70 zones (74%), round-top humps in 49 zones (52%), the most common height for the humps was 75 mm, used in 80% of zones with road humps, the remainder used humps in the range 75 to 100 mm high • raised junctions are a form of flat-top road hump covering the whole junction, used in 26 zones (27%). The degree to which they extend into the side roads depends on local factors at each site. They are often combined with road humps • speed cushions, used in 42 zones (44%), mainly combined with road humps and raised junctions. Speed cushions were developed to reduce the discomfort and delay to large vehicles such as buses and fire appliances. The most common height was 75 mm (94%), the others were 80 mm high (6%). The most common width of the cushions was 1.6 m (64%), other widths of cushions were 1.7 m (17%), 1.8 m (11%) and 1.9 m (8%) • chicanes: only used at 2 (2%) of the zones. • raised footway, a form of flat-top hump which is used to raise the footway at an entrance to a zone. Other measures used rarely included mini-roundabouts, rumble strips, and VMS sign (Variable message sign) The maximum length of road comprising one zone was 14.5 km and the minimum was 0.15 km. The average length of road was 3.4 km. The total length of all roads in the zones was 391 km; of this, 253.4 km of road was used for the accident analysis. The largest 20 mph zone covered 2.08 km^2 while the smallest (excluding linear zones) was 0. 02 km^2. The average size of a zone was 0.35 km^2 C: no details given of any changes.

Outcomes	Reported injury accidents (slight, serious, fatal). Data provided for all cyclists and child cyclists only. Data from London Accident Analysis Unit
Notes	-

Risk of bias

Bias	**Authors' judgement**	**Support for judgement**
Allocation concealment (selection bias)	High risk	No matching. 20 mph zones compared with all unclassified roads in London as control data and "all 'A' and 'B' roads in London as 'control' data for boundary roads" so some roads proximal and some distal
Blinding of outcome assessment (detection bias) All outcomes	Low risk	Data supplied by London Accident Analysis Unit.
Incomplete outcome data (attrition bias) All outcomes	Low risk	Data collected 5 years pre-intervention and from 1 to 5 years post-intervention
Selective reporting (reporting bias)	Low risk	No bias.
Bias due to confounding	High risk	Does not take into account confounding factors.
Other bias	Low risk	No other bias.

Wheeler 1987

Methods	CBA Before data: 3 years. After data: 3 years.
Participants	I. Albert Gate and Albion Gate, two main elements of the 5 km Ambassador cycle route in London, a north-south route through a congested area of the West End of London, UK, mainly avoiding busy roads and the gyratories at Hyde Park Corner and Marble Arch C. The surrounding area of the Ambassador cycle route within a 12 km² perimeter Age of cyclists: no data.
Interventions	I: • Albert Gate: two-way cycle route, signalled controlled crossings, red asphalt, inclined granite kerbs to protect cyclists from motor vehicles, opened 1st July 1982. • Albion Gate: central protected cycle lane, white-line segregated cyclist/pedestrian path, adjacent signal controlled cycle crossings, a short two-way cycle lane surfaced in

	red asphalt, opened 30th June 1983. C: No details of any changes reported.
Outcomes	Reported accidents involving cyclist casualties: measured at two gates, at three areas in vicinity of schemes and elsewhere in the area. Data from July 1979 to June 1986 Cycle counts: taken at Albert Gate and 6 other sites across the south side of Hyde Park, at Albion Gate and 6 other sites on the north side of Hyde Park. Counts undertaken on weekdays (Monday to Friday) in both directions at each site over two-hour peak periods in the morning (08:00 to 10:00), lunchtime (12:00 to 14:00) and evening (16:30 to 18: 30). Counts taken on 3 occasions: September 1981, September 1982 and September 1983. Cycle counts were made for 3 control sites
Notes	-

Risk of bias

Bias	Authors' judgement	Support for judgement
Allocation concealment (selection bias)	Unclear risk	No matching of intervention and control areas although accidents measured at other points along the cycle route and cycle flows measured at other gates on the route Cycle accident data collected for the area enclosing the whole of the Ambassador route and for the surrounding area from which cyclists might be attracted
Blinding of outcome assessment (detection bias) All outcomes	Unclear risk	The source of reported accidents is not given.
Incomplete outcome data (attrition bias) All outcomes	Low risk	3 years 'before' data and 3 years 'after' data.
Selective reporting (reporting bias)	Low risk	No bias.
Bias due to confounding	Unclear risk	No description of other confounding factors.
Other bias	Low risk	No other bias.

Williams 1989

Methods	CBA Before data: 3 years After data: 3 years (2 years for 5 sites)

Participants	I: 18 footways (pedestrian rights-of-way beside a road carriageway) in 7 counties in England C: all County roads Age of cyclists: 0 to 15 years, 16 to 65 years, over 65 years
Interventions	I: 18 footways converted to joint cycle pedestrian use, implemented between March 1980 and November 1984 • Bedfordshire: 2 sites (1.7 and 0.2 km) • Berkshire: 4 sites (3.3, 0.7, 1.8 and 1.6 km) • Cambridgeshire: 6 sites/8 sections (1.2, 0.5, 0.5, 1.3, 1.2, 2.3, 0.6, 1.6 km) • Hertfordshire: 2 sites (0.6 and 3.8 km) • Humberside: 1 site/2 sections (1.5 and 1.0 km) • Shropshire: 1 site (1.5 km) • Tyne and Wear: 2 sites/4 sections: (5.0, 1.6, 1.6 and 0.8 km) 16 sites are on A-class (main) roads, 15 are on single-carriageway roads and 12 are on roads with 30 or 40 mph speed limit. 8 sites had low density housing frontage and 6 were rural, 1 was river and businesses, 1 was mixed housing and light industry, 1 was mixed, industry, and 1 was undeveloped 4 schemes had some form of segregation between the pedestrian and cyclist paths: 1 is wholly segregated by a white line, 1 has a short section segregated by a white line, 2 have short sections segregated by level The widths of conversion ranged from 0.9 m to 3.7 m. C: no details of any changes/cycling infrastructure.
Outcomes	Injury accidents involving cyclists: using STATS19 data. Cycle flows: counted by Local Highways Authorities after implementation of the scheme at 10 sites and before and after at 3 sites, made at different times of the year
Notes	-

Risk of bias

Bias	**Authors' judgement**	**Support for judgement**
Allocation concealment (selection bias)	High risk	No matching of control and experimental areas. Some roads proximal, some distal.
Blinding of outcome assessment (detection bias) All outcomes	Low risk	STATS19 data from Local Highway Authorities.
Incomplete outcome data (attrition bias) All outcomes	Low risk	3 years pre-intervention and 3 post-intervention (although 5 sites have 2 years only post-intervention)
Selective reporting (reporting bias)	Low risk	No bias.

Williams 1989 *(Continued)*

Bias due to confounding	High risk	No reported assessment of confounders. Exposure was not taken into account when examining accident rates
Other bias	Unclear risk	No information is presented on other cycling infrastructures in place on other roads in the counties

* = primary reference, where several papers report the same study
ADT = annual daily traffic
ASL = advanced stop lines
CBA = controlled before-after study
ITS = interrupted time series studies
I: Intervention
C: Control

Characteristics of excluded studies *[ordered by study ID]*

Study	Reason for exclusion
Abdul Rahimi 2013	Not study design of interest
ADAC 1985	Not study design of interest
Agustsson 1997	Not study design of interest
Agustsson 2001	No outcomes of interest
Allat 2012	Not enough information
Angenendt 1989	Not enough information
Anon 1996	Not study design of interest
Anon 2011	Not study design of interest
Asmus 2012	Not study design of interest
Atkins 2005	Not study design of interest
Bakaba 2013	Not cycling infrastructure
Barker 1997	Not cycling infrastructure

(*Continued*)

Bertelmann 1979	Not study design of interest
Bracher 1989	Not study design of interest
Brannolte 1990	Not cycling infrastructure
Briglia 2009	Not cycling infrastructure
Brilon 1988	Not cycling infrastructure
Brilon 1990a	Not study design of interest
Brilon 1990b	Not cycling infrastructure
Brindle 1983	Not enough information
Brindle 1985	Not study design of interest
Brindle 1996	Not study design of interest
Bristol City Council 2012	Not study design of interest
Buckby 2013	Not study design of interest
Burbidge 2015	Not study design of interest
Campbell 1987	Not enough information
Campbell 1989	Not enough information
Carter 2006	Not cycling infrastructure
Carter 2007a	Not cycling infrastructure
Carter 2007b	Not cycling infrastructure
Chatfield 1991	Not enough information
Chen 2009	Not study design of interest
Cherry 2012	Not study design of interest
Cheyne 2003	Not study design of interest
Christie 2006	Not cycling infrastructure

(*Continued*)

Clarke 1995	Not study design of interest
Cleary 1993	Not study design of interest
Coates 1999	Not study design of interest
Crampton 1992	Not study design of interest
Croft 1996	Not cycling infrastructure
Cumming 2012	Not study design of interest
Cynecki 1992	Not cycling infrastructure
Danaher 2005	Not study design of interest
Daniel 2005	Not study design of interest
Daniels 2007	Not cycling infrastructure
Daniels 2010	Not study design of interest
Daniels 2011	Not study design of interest
Davies 1997a	Not study design of interest
Davies 1997b	Not study design of interest
Davies 1998	Not cycling infrastructure
Davies 1999	Not study design of interest
Davies 2001	Not study design of interest
De Brabander 2005	Not cycling infrastructure
De Brabander 2007	Not cycling infrastructure
De Pauw 2014	Not cycling infrastructure
DeRobertis 1998	Not study design of interest
Dill 2012	No outcomes of interest
Do 2011	No outcomes of interest
Edquist 2012	Not cycling infrastructure

(*Continued*)

Ekman 1995	Not study design of interest
Elvik 2000	Not study design of interest
Elvik 2001	Not study design of interest
Enns 2014	Not study design of interest
Environmental & Transport 1991	Not study design of interest
Fager 1984	Not study design of interest
Fairlie 1990	Not enough information
Ferigo 2005	Not study design of interest
FHWA 1993	Not study design of interest
FHWA 1994	Not study design of interest
FHWA 1999	Not study design of interest
FHWA 2004a	No outcomes of interest
FHWA 2004b	No outcomes of interest
FHWA 2006	Not study design of interest
FHWA 2010	No outcomes of interest
FHWA 2012	Not study design of interest
Fischer 2010	Not study design of interest
Fitzpatrick 2011	Not study design of interest
Forbes 2009	Not study design of interest
Franklin 1999	Not study design of interest
Franklin 2002	Not cycling infrastructure
Frith 1986	Not cycling infrastructure
Gerlach 2009	Not study design of interest
Gibbard 2004	Not study design of interest

(*Continued*)

Goodno 2013	Not study design of interest
Gorman 1989	Not study design of interest
Grana 2008	Not study design of interest
Granefelt 1997	Not cycling infrastructure
Gray 2004	Not study design of interest
Grontmij 1991	Not study design of interest
Gross 2012	Not cycling infrastructure
Guthrie 2001	Not study design of interest
Hamann 2013	Not study design of interest
Hamelynck 1994	Not study design of interest
Hansen 1988	Not enough information
Harris 2013	Not study design of interest
Hartl 1995	Not study design of interest
Hass-Klau 1991	Not study design of interest
Haworth 2001	Not study design of interest
Hedman 1991	Not study design of interest
Henson 1992a	Not cycling infrastructure
Henson 1992b	Not cycling infrastructure
Herrstedt 1989	Not study design of interest
Herrstedt 1992	Not study design of interest
Hoareau 2002	No outcomes of interest
Holmes 1993	Not study design of interest
Huang 2002a	No outcomes of interest

(*Continued*)

Huang 2002b	No outcomes of interest
Huang 2003	No outcomes of interest
Hunter 1997	Not study design of interest
Hunter 1998	Not study design of interest
Hunter 1999a	No outcomes of interest
Hunter 1999b	Not study design of interest
Hunter 1999c	Not study design of interest
Hunter 2000a	Not study design of interest
Hunter 2000b	Not study design of interest
Hunter 2000c	Not study design of interest
Hunter 2000d	Not study design of interest
Hunter 2001	Not study design of interest
Hunter 2009	Not study design of interest
Hunter 2011	Not study design of interest
Hunter 2012	Not study design of interest
Hurwitz 2014	Not study design of interest
Hyden 2000	Not study design of interest
Jaarsma 2011	Not cycling infrastructure
Jadaan 1988	Not cycling infrastructure
Janssen 1984a	Not cycling infrastructure
Janssen 1984b	Not cycling infrastructure
Janssen 1985	No outcomes of interest
Jayadevan 2006	Not cycling infrastructure

(*Continued*)

Jensen 2007a	Not study design of interest
Jensen 2007b	Not outcomes of interest
Jensen 2008	No outcomes of interest
Jensen 2010	Not enough information
Johannessen 1982	Not enough information
Johansson 2006	Not enough information
Johansson 2007	Not enough information
Kahrmann 1988	No outcomes of interest
Kallberg 1982	Not study design of interest
Kay 2013	No outcomes of interest
Kerr 2013	Not cycling infrastructure
Ketteridge 1993	Not study design of interest
Kim 2001	Not study design of interest
Kirchknopf 1994	No outcomes of interest
Kjemtrup 1993	Not cycling infrastructure
Klein 1991	Not cycling infrastructure
Knapp 2001	Not study design of interest
Knoflacher 1980	Not enough information
Knoflacher 2002	Not enough information
Koehle 1981	No outcomes of interest
Koehler 1991	Not study design of interest
Kortegast 2012	Not study design of interest
Kulmala 1994	Not cycling infrastructure
Lange 1992	Not enough information

(*Continued*)

Lawton 2001	Not study design of interest
Layfield 2005	Not study design of interest
Leden 2006	No outcomes of interest
Leutzbach 1986	Not enough information
Levine 1988	Not cycling infrastructure
Liabo 2003	Not cycling infrastructure
Lindqvist 2001	Not cycling infrastructure
Lines 1995	Not study design of interest
Lings 2004	Not cycling infrastructure
Ljungberg 1984	Not enough information
Lott 1976	Not study design of interest
Louisse 1994	Not study design of interest
Lusk 2011a	Not study design of interest
Lusk 2011b	Not study design of interest
MacBeth 2001	Not study design of interest
Mackie 1988	Not cycling infrastructure
Mackie 1990	Not cycling infrastructure
Marshall 2011	Not study design of interest
McClintock 1996	Not study design of interest
Meredith 2001	Not cycling infrastructure
Meyers 2008	No outcomes of interest
Michael 2005	Not study design of interest
Miller 2011	Not study design of interest

(*Continued*)

Millot 2008	Not study design of interest
Minikel 2012	Not study design of interest
Moeller 1992	No outcomes of interest
Monsere	Not study design of interest
Monsere 2011a	No outcomes of interest
Monsere 2011b	Not study design of interest
Monsere 2012	Not study design of interest
Najjar 2002	Not cycling infrastructure
Nash 2005	Not study design of interest
Natarajan 2008	Not study design of interest
Nicholson 2004	Not study design of interest
Niedra 2005	Not study design of interest
Nilsson 2001	Not study design of interest
Nilsson 2003	Not study design of interest
Noel 1995	Not study design of interest
Noland 2003	Not cycling infrastructure
Noordzij 1996	Not study design of interest
Nosal 2012	Not study design of interest
O'Connor 1987	Not study design of interest
Oei 1996	Not cycling infrastructure
Oppe 1983	Not cycling infrastructure
Osland 2012	Not study design of interest
Owens 2005	Not cycling infrastructure
PBIC	Not study design of interest

(*Continued*)

Pegrum 1972	Not cycling infrastructure
Persaud 2001	Not cycling infrastructure
Persaud 2003	Not cycling infrastructure
Peters 2013	Not cycling infrastructure
Portsmouth City Council 2010	Not study design of interest
Prato 2015	Not study design of interest
Quenault 1982	Not study design of interest
Ragland 2011	Not study design of interest
Raisman 2009	Not cycling infrastructure
Ram 1992	Not enough information
Rasanen 1998	No outcomes of interest
Rasmussen 2007	Not study design of interest
Retting 2002	Not cycling infrastructure
Rifaat 2011	Not cycling infrastructure
Rodgers 2005	Not study design of interest
Rogerson 1991	No outcomes of interest
Rosales 2005	Not study design of interest
RTA 1999	No outcomes of interest
Ruest 2012	Not cycling infrastructure
Russell 2010	Not study design of interest
Ruwenstroth 1987	Not enough information
Ryley 1996	No outcomes of interest
Rystam 1995a	Not cycling infrastructure
Rystam 1995b	Not cycling infrastructure

(*Continued*)

Sach 1984a	Not study design of interest
Sach 1984b	Not study design of interest
Sadek 2007	No outcomes of interest
Sakshaug 2010	Not study design of interest
Sammer 1993	Not cycling infrastructure
Schepers 2011	Not study design of interest
Schepers 2014	Not study design of interest
Schnull 1992	Not study design of interest
Schoon 1994	Not study design of interest
Schuster 2001	Not enough information
Simpson 1999	Not enough information
Slinn 1993	Not study design of interest
Sliwinski 2006	Not study design of interest
SNRTRI 2000	Not study design of interest
Sorton 1987	Not study design of interest
Stidger 2002	Not cycling infrastructure
Stout 2006	No outcomes of interest
Strauss 2013	Not study design of interest
Sumner 1978	No outcomes of interest
Suzuki 2012	Not study design of interest
Takacs 1978	Not enough information
Tan 2011	Not study design of interest
Tefe 2008	Not study design of interest
TemaNord 1994	Not study design of interest

(*Continued*)

Teschke 2012a	Not cycling infrastructure - linked to Harris 2013
Teschke 2012b	Not cycling infrastructure - linked to Harris 2013
Teschke 2012c	Not study design of interest - linked to Harris 2013
Thomas 2007	Not cycling infrastructure
Tilly 2005a	Not study design of interest
Tilly 2005b	Not study design of interest
Tollazzi 2007	Not enough information
Towliat 2000	No outcomes of interest
Towliat 2001	Not study design of interest
Toy 2001	Not enough information
Trevelyan 1989	Not study design of interest
Turner 1981	Not cycling infrastructure
Turner 2009	Not study design of interest
Turner 2010a	Not enough information
Turner 2010b	Not enough information
Turner 2011	Not study design of interest
van der Meer 1982	Not study design of interest
Van Laarhoven 1983	Not study design of interest
Van Minnen 1994	Not study design of interest
van Minnen 1995	Not study design of interest
Van Minnen 1999	No outcomes of interest
Vandenbulcke 2014	Not study design of interest
Varma 2008	Not study design of interest
Veling 1988	Not study design of interest

(*Continued*)

Visa 1992	No outcomes of interest
Vulcan 1993	Not enough information
Walker 1989a	Not cycling infrastructure
Walker 1989b	Not cycling infrastructure
Wang 2009	Not cycling infrastructure
Ward 1982	Not cycling infrastructure
Ward 1989	Not cycling infrastructure
Weber 2001	Not study design of interest
Weber 2007	Not cycling infrastructure
Webster 1993	Not study design of interest
Wegmann 1988	Not study design of interest
Welleman 1988	Not cycling infrastructure
Wheeler 1995	No outcomes of interest
Wheeler 2005a	Not study design of interest
Wheeler 2005b	Not study design of interest
Wiedemann 1989	Not enough information available
Williams 1987	Not study design of interest
Wynne 1992	Not study design of interest
Xu-Mei 2000	Not cycling infrastructure
Yannis 2014	Not cycling infrastructure
Yee 1986	Not cycling infrastructure
Yuan 2001	Not cycling infrastructure
Zayerzadeh 2008	Not cycling infrastructure

(*Continued*)

Zegeer 1992	Not study design of interest

DATA AND ANALYSES

Comparison 1. Cycle lanes vs. no cycle lanes (not adjusted for cycle flow)

Outcome or subgroup title	No. of studies	No. of participants	Statistical method	Effect size
1 Collisions (not adjusted for cycle flow)	3		Rate Ratio (Random, 95% CI)	1.21 [0.70, 2.08]

Comparison 2. Cycle routes and networks vs. no cycle routes and networks

Outcome or subgroup title	No. of studies	No. of participants	Statistical method	Effect size
1 Collisions (not adjusted for cycle flow)	4		Rate Ratio (Random, 95% CI)	Totals not selected
2 Collisions (adjusted for cycle flow, collision data for 3 cities from Harland)	3		Rate Ratio (Random, 95% CI)	0.40 [0.15, 1.05]

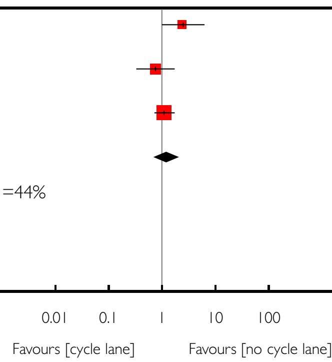
Analysis 1.1. Comparison 1 Cycle lanes vs. no cycle lanes (not adjusted for cycle flow), Outcome 1 Collisions (not adjusted for cycle flow).

Review: Cycling infrastructure for reducing cycling injuries in cyclists

Comparison: 1 Cycle lanes vs. no cycle lanes (not adjusted for cycle flow)

Outcome: 1 Collisions (not adjusted for cycle flow)

Study or subgroup	log [Rate Ratio] (SE)	Rate Ratio IV,Random,95% CI	Weight	Rate Ratio IV,Random,95% CI
Chen 2012	0.90336 (0.47367)		23.5 %	2.47 [0.98, 6.24]
Parsons 2013	-0.27541 (0.42908)		26.7 %	0.76 [0.33, 1.76]
Smith 1988	0.10185 (0.2242)		49.8 %	1.11 [0.71, 1.72]
Total (95% CI)			**100.0 %**	**1.21 [0.70, 2.08]**



Heterogeneity: $Tau^2 = 0.11$; $Chi^2 = 3.56$, df = 2 (P = 0.17); I^2 =44%
Test for overall effect: Z = 0.68 (P = 0.50)
Test for subgroup differences: Not applicable

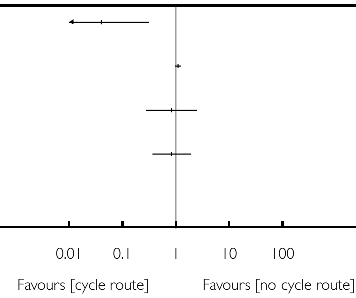
Analysis 2.1. Comparison 2 Cycle routes and networks vs. no cycle routes and networks, Outcome 1 Collisions (not adjusted for cycle flow).

Review: Cycling infrastructure for reducing cycling injuries in cyclists

Comparison: 2 Cycle routes and networks vs. no cycle routes and networks

Outcome: 1 Collisions (not adjusted for cycle flow)

Study or subgroup	log [Rate Ratio] (SE)	Rate Ratio IV,Random,95% CI	Rate Ratio IV,Random,95% CI
Carlson 1975	-3.26012 (1.07219)		0.04 [0.00, 0.31]
Harland 1993	0.08528 (0.06632)		1.09 [0.96, 1.24]
Nicholson 1979	-0.17035 (0.55688)		0.84 [0.28, 2.51]
Quenault 1981	-0.19539 (0.42313)		0.82 [0.36, 1.89]



Analysis 2.2. Comparison 2 Cycle routes and networks vs. no cycle routes and networks, Outcome 2 Collisions (adjusted for cycle flow, collision data for 3 cities from Harland).

Review: Cycling infrastructure for reducing cycling injuries in cyclists

Comparison: 2 Cycle routes and networks vs. no cycle routes and networks

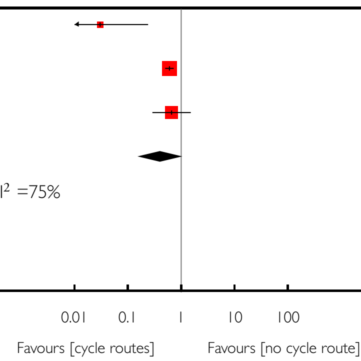
Outcome: 2 Collisions (adjusted for cycle flow, collision data for 3 cities from Harland)

Study or subgroup	log [Rate Ratio] (SE)	Rate Ratio IV,Random,95% CI	Weight	Rate Ratio IV,Random,95% CI
Carlson 1975	-3.53428 (1.07219)		14.9 %	0.03 [0.00, 0.24]
Harland 1993	-0.5081 (0.10638)		48.6 %	0.60 [0.49, 0.74]
Quenault 1981	-0.39501 (0.423129)		36.5 %	0.67 [0.29, 1.54]
Total (95% CI)			**100.0 %**	**0.40 [0.15, 1.05]**

Heterogeneity: $Tau^2 = 0.49$; $Chi^2 = 8.00$, df = 2 (P = 0.02); $I^2 = 75\%$

Test for overall effect: Z = 1.85 (P = 0.064)

Test for subgroup differences: Not applicable



ADDITIONAL TABLES

Table 1. Comparators and outcomes for included studies (as reported by study author)

Cycle infrastructure	Study comparator	Outcomes relating to collisions (source of data)(as reported by study author)	Outcomes relating to cycle count
1. Shared use of the road space for both motor vehicles and cyclists			
Cycle lanes			
Buckley 2000	No details	Crashes involving cyclists from Accident Investigation Systems	-
Chen 2012	No cycle lane	Police-reported cyclist crashes	Bicycle trip density (did not present data)
Parsons 2013	Dashed lines - old	Cycle crash data (from CAS database)	Annual average daily traffic
Smith 1998	No details	Cycle accidents (Madison Department of Transportation)	Estimated daily trips

Table 1. Comparators and outcomes for included studies (as reported by study author) *(Continued)*

Advanced stop lines (ASL)			
Allen 2005	No ASL	Casualty rate (STATS 19)	Number of cyclists
Use of colour			
König 2006	No changes	Police-reported and hospital-attended accidents (STRADA)	Number of cyclists
2. Separation of cycle traffic from motorised traffic			
Cycle tracks			
Agerholm 2008	No changes	Police-reported injury accidents	Annual Daily Traffic
Gårder 1998	No changes	Police- and hospital-reported accidents	Cycle flow
Cycle paths			
Williams 1989	No details	Injury accidents involving cyclists (STATS19)	Number of cyclists
3. Management of the road network			
Cycle lanes through signalised and priority junctions			
Jensen 1997	No cycle lanes	Cycle accidents with casualties	-
Speed management			
Grundy 2009	No 20 mph zone	Police-reported cyclist casualties (STATS19)	-
Mountain 2005	Camera vs. engineering measures	All accidents (obtained from various local authorities and police forces)	-
Webster 2003	No 20 mph zone	Reported injury accidents (London Accident Analysis Unit)	-
4. Combination of cycling infrastructure			
Cycle routes and networks			
Bovy 1988	No changes	Police-reported accidents	Cycle flow

Table 1. Comparators and outcomes for included studies (as reported by study author) (*Continued*)

Carlson 1975	No details	Police-reported cycle/motor accidents	Cycle flow
Harland 1993	No details	Police-reported cyclists casualties (STATS19)	Number of cyclists
Nicholson 1979	No changes	Police-reported cycle accidents (STATS19)	Number of cyclists
Quenault 1981	No details	Police-reported cycle accidents	Number of cyclists
Package of infrastructure			
Wheeler 1987	No details	Reported accidents involving cyclist casualties	Number of cyclists
Roundabout design			
Daniels 2009	No details	Accidents involving at least one cycle (National Statistics Institution)	-
Jensen 2013	No details	Cycle crashes (recorded by police)	-

Table 2. Cycle collision data

Study author, year	Cycle collision/ injury data presented and subcategories	Country Cycle helmet legislation	Source of crash data	Date of installation of cycling infrastructure	Data collection periods: Years and length pre/ post installation	Cycle flow data taken into account in collision analysis	Notes	Effect of cycle infrastructure on cycle collision data
1. Shared use of the road space for both motor vehicles and cyclists								
Cycle lanes								
Buckley 2000	Cyclist crashes No subcategories	New Zealand Mandatory since 1994 with fines.	Accident Investigation System (AIS) database maintained by Land Transport Safety Authority	1993 onwards	'Before and after' data collection period unclear but states "the recommended observation is both 5 years	No cycle flow data	Presented minimum and maximum effect	Inconclusive

Table 2. Cycle collision data *(Continued)*

					before and after implementation of a treatment" "in some cases the after period was approximately two years in length"			
Chen 2012	Police reported cycle crashes (vehicle-cyclist collisions) No subcategories	USA In New York, children aged 1 to 14 years must wear an approved helmet from 1994		1996 to 2006		Used in model, but did not control for before-after differences in cyclist volumes because data not available	Used generalised estimating equation (GEE) methodology comparison group	Cycle crashes increased
Parsons 2013	Cycle crash rates No subcategories	New Zealand Mandatory since 1994	CAS	2003 to 2006	1999 to 2009 Typically 5 years pre-treatment and 5 years post-treatment and one treatment-affected year	Yes	Cycle crash rates per million vehicle kilometres travelled	Decrease in crash rates
Smith 1998	Cycle accidents How accident occurred and where cyclist was positioned	USA Not mandatory in Madison	Madison Department of Transportation	1977	1974 to 1981 4 years before, 4 years after	Yes	Presented total accident rate per 1000 daily trips. Used a time-series analysis	Cycle crashes increased, state exposure provides a partial explanation for the increase
Advanced stop lines								

Table 2. Cycle collision data *(Continued)*

Allen 2005	Cyclist casualties Severity, age, sex	UK Not mandatory	STATS19 data Transport for London's (TfL) Safety Unit	1997 to 2001 (some unknown)	1990 to 2003 (varied by site) Before ranged from 4 to 6 years, after ranged from 2 to 6 years	No, discussed separately from casualty data	Presented as casualty rate per year (not by subcategories)	Inconclusive
Use of colour								
König 2006	Police-reported and hospital-attended accidents No subcategories	Sweden Mandatory for children under 15	Swedish Traffic Accident Data Acquisition (STRADA) Police- and hospital-reported	2000 and 2004	2000 to 2004 (varied by site) Before ranged from 1 to 4 years, after ranged from 0 to 4 years	No, discussed separately from collision data		Inconclusive
2. Separation of cycle traffic from motorised traffic								
Cycle tracks								
Agerholm 2008	Police reported cyclist injury crashes No subcategories	Denmark Not mandatory	Danish accident database	1989 to 2000	1986 to 2004	Discussed narratively with cycle accident data		Increased cycle accidents
Gårder 1998	Police- and hospital-reported accidents No subcategories	Sweden Mandatory for children under 15	City of Gothenburg database of police and hospital reported accidents	1993	1988 to 1996	Discussed narratively with cycle accident data	Reported cycle collisions	Cycle accidents grew by 8% but cycle flow increased by 50%, therefore some evidence of effectiveness
Cycle paths								

Table 2. Cycle collision data *(Continued)*

Williams 1989	Cyclists' injury accidents Severity, age	UK Not mandatory	STATS19 from Local Highway Authorities	1980 to 1984	3 year before and after	Discussed narratively with cycle accident data	Reported injury accidents involving cyclists	No effect
3. Management of the road network								
Cycle lanes through signalised and priority junctions								
Jensen 1997	Cycle collisions with casualties Severity, age	Denmark Not mandatory		1985 to 1990	Before and after period the same, varies from 24 to 60 months, average 50			Increased
Speed management								
Grundy 2009	Police-reported cyclist casualties Severity, age, social deprivation, ethnicity	UK Not mandatory	Police STATS19 data	1991 to 2008	1986 to 2006	No	Used time series regression analysis	Decreased
Mountain 2005	All accidents Age	UK Not mandatory	Local authorities and police forces	Not given	Before 3 years and up to 3 years after, (average after of 2.5 years)	No account of cycle flow	Used Empirical Bayes methodology	Decreased, but treat with caution
Webster 2003	Reported injury accidents Severity, age	UK Not mandatory	London Accident Analysis Unit	1989 to 2003	Before 5 years, after period was minimum 1 year and maximum 5 years	No	Collisions per year per site	Decreased
4. Combination of cycling infrastructure								
Cycle routes and networks 0.95 (0.87 to 1.04)								

Table 2. Cycle collision data *(Continued)*

Bovy 1988	Police-reported accidents No subcategories	Netherlands Not mandatory	Not clear	1982 to 1984	Before 3 years, after 2 years	No account of cycle flow		Decreased
Carlson 1975	Cycle/ motor vehicle accidents No subcategories	USA In Seattle mandatory for all ages since 2003	Not clear	1973	1970 to 1973, 10 months before, 10 months after	Discussed narratively with cycle accident data		Decreased on cycle route, numbers small
Harland 1993	Police-reported cyclist casualties No subcategories	UK Not mandatory	STATS19	1985 to 1990	1983 to 1991. Before 2 years, after 2 years	Discussed narratively with cycle accident data		No change
***Dean 1993 Stockton only**	Pedal cycle casualties Severity, age	UK Not mandatory	Cleveland County Council	1985	1983 to 1986 18 months before, 18 months after	No, discussed separately from accident data		Reduction in serious, increase in fatal, inconclusive
***Harbidge 1993 Kempston only**	Personal injury accidents involving pedal cycles Severity, age	UK Not mandatory	Bedfordshire County Council	1986	1982 to 1990 4 years before and 5 years after	No, discussed separately from accident data		No change except reduction in child collisions
***Shipley 1994 Southampton only**	Pedal cycle accidents Severity, sex	UK Not mandatory		1987	1984 to 1989 3 years before, 2 years after	Discussed narratively with cycle accident data		No change
Nicholson 1979	Police-reported cycle accidents Severity	UK Not mandatory	STATS19 from Hampshire County Council	1975 to 76	1970 to 1977 5 years before, 2 years during, 1 year after	No, discussed separately from accident data		No change
Quenault 1981	Police-reported cycle accidents	UK Not mandatory	Cambridge County	1977	1974 to 1978	No, discussed sepa-		No change

Table 2. Cycle collision data *(Continued)*

	Severity		Council		3.5 years before, 1.5 years after	rately from accident data		
Package of infrastructure								
Wheeler 1987	Reported accidents involving cyclist casualties No subcategories	UK Not mandatory		1982 and 1983	Before period 3 years, after period 3 years	Yes, accidents x 100/ cycle flow	Accidents x 100/ cycle flow	Generally decreased
Roundabout design								
Daniels 2009	All registered accidents causing injury involving at least one cycle Severity	Belgium Not mandatory	National Statistics Institution	1994 to 2000	1991 to 2001	No		Increase on roundabouts with cycle lanes
Jensen 2013	Cycle crashes and injuries Severity	Denmark Not mandatory	Police-reported data	1995 to 2009	1985 to 2010 'Before' period is 5 years, 'after' period is 1 to 5 years	No	Cycle crashes with cyclists injuries	Increased

Table 3. Cycle flow data

Study author, year	Cycle flow data measured	Source of cycle flow data	Location	Day and time of day	Time of year	Cycle flow data taken into account in accident analysis	How cycle flow data is presented	Notes
1. Shared use of the road space for both motor vehicles and cyclists								
Cycle lanes								
Buckley 2000	No data							

Table 3. Cycle flow data *(Continued)*

Chen 2012	Cycle trip density (BTD)	No details given				Yes, used in model		BTD calculated as number of cycle commuters divided by total census tract road length No data presented, just used in model
Parsons 2013	Annual average daily traffic (AADT) (000s)	Christchurch City Council	Cycle count data obtained for intersections of routes. 12 intervention and 3 control.	Two types of data exist. Pre-2004: 07:30 to 09:00 and 16:15 to 17:45 (manually collected) Post-2004: typically counted between 07:00 to 09:00 and 16:00 to 18:00 (collected either manually or electronically)	1999 to 2009	Yes, crashes per million vehicle kilometres travelled	Presents % change in cycle counts from pre- to post-implementation for each intervention and each control site. "Overall, the longer-term changes in cycle counts post-treatment have been strong positive, with an average increase in cycle count trends of over 200% across all routes"	AADT calculated using a method based on time of day, day of week, time of year (e.g. school term or not) to scale short-term cycle counts in to AADT. Based on the assumption that cycle numbers follow a common cycling profile, determined by whether the route is considered a commuter (more cyclical with morning and evening peaks) or non- commuter

Table 3. Cycle flow data *(Continued)*

Smith 1998	Estimated daily trips (000s)	Madison Department of Transportation	Originally at 3 intersections in central Madison. Counts were dropped at the third intersection on the university campus due to large variation in the count volumes	2 hours during the morning and afternoon peak periods	Once a month, 1974 to 1981	Yes, total accident rate per 1000 daily trips	Presents total estimated daily trips for each of the 4 years before and each of the 4 years after intervention for all Madison, so change in cycle flow identical for both intervention and control areas	The counts are factored to produce an estimate of total daily cycle trips citywide based on a 1974 survey of cycle travel. All cycles entering the intersection were counted
Advanced stop lines								
Allen 2005	Numbers	Authors	12 intervention and 2 control sites	07:00 to 18:00 hours, weekdays, for 2 days	Jan to Sept 2003	No, discussed separately from accident data	Number of cyclists over 2 days, intervention and control sites on one occasion (not before and after)	Counts capped at 3 sites (2 intervention and 1 control) once a sample of 400 cyclists obtained
Use of colour								
König 2006	Number of cyclists	No details given	For 2 pairs of intervention/control intersections (71 intervention and 15 control intersections in total)	07:30 to 09.30 and 16:00 to 18:00 in 10 minute intervals	Counted on one occasion (not before and after)	No, discussed separately from accident data	Number of cyclists counted once at intervention and control	
2. Separation of cycle traffic from motorised traffic								
Cycle tracks								

Table 3. Cycle flow data (*Continued*)

Agerholm 2008	Annual Daily Traffic (ADT)	Not given	Cycle ADT available for only a few studied segments			Discussed narratively with cycle accident data		No data given just states "no clear change"
Gårder 1998	Percentage change in cycle flows	No details given	At 2 (of 44) intervention and at 2 (of 1898) control sites		For 2 weeks before and 2 weeks after construction	Discussed narratively with cycle accident data	Cycle flows increased by 75% on one side and by 79% on the other side, and by almost 100% on the second intervention street. On the control sections there was a growth of around 20%	Nu-Metrics Countmates at one site and a permanent loop detector sensitive at a second point
Cycle paths								
Williams 1989	Numbers	JMP Consultants and Local Authority	At 3 of 18 intervention sites on footway and on road (before and after)	Site 1: 08:00 to 09:00 eastbound, 15:30 to 16:30 westbound Site 2: 08:00 to 09:00 northbound, 17:00 to 18:00 southbound Site 3: 07:00 to 09:00 and 15:30 to 18:30 both directions	Site 1: March 1983 (before), Sept 1983 (after) Site 2: Sept to Nov 1979 (before), Oct 1983 (after) Site 3: Oct 1978 (before), Sept 1983 (after)	Discussed narratively with cycle accident data	Numbers before and after, on footway and on road (intervention and control sites)	
3. Management of the road network								
Cycle lanes through signalised and priority junctions								

Table 3. Cycle flow data (*Continued*)

Jensen 1997	No data							
Speed management								
Grundy 2009	No data							
Mountain 2005	No data							
Webster 2003	No data							
Cycle routes and networks								
Bovy 1988	Percentage of cycle flow in intervention and control areas in 5 districts	No details given	Before and after in 5 districts including intervention and control areas			Discussed narratively with cycle accident data	No change observed.	
Carlson 1975	Percentage change in cycle flows	Not clear	5 locations: 3 on the Bikeway, 2 not on the Bikeway. Also one count made at a location on the Bikeway that was not monitored previously	Morning peak period 08:00 to 09:30.	May 1973 (pre- intervention) Dec 1973 (post- intervention)	Discussed narratively with cycle accident data	Percentage increases seen along Bikeway of 3.6, 8.4 and 25.7. Percentage decreases seen on non-Bikeway roadways of 4.4 and 24.4	Manual counts, no numbers presented only change in percentage
Harland 1993	Numbers, average weekly cycle flow	Consultancies working with local authorities	Before and after, on and off cycle route for 3 cities only. Average weekday cycle flow for 6 cities		1984 to 1990	Discussed narratively with cycle accident data	Presents percentage change from before to after for 3 cities only for both intervention and control sites	Some data missing for average weekday counts. Data collection periods for before and after data not clear

Table 3. Cycle flow data (*Continued*)

***Dean 1993 Stockton only**	Numbers	No details given	5 screenline and 3 cycle route stations. Before and after daily cycle counts are presented for each of the 7 stations on the screenline	For 1 weekday (09:00 to 19:00) at monthly intervals, Oct 1985 to Sept 1986 6 days (Monday to Saturday) for 12 hours (07:00 to 19:00) in. It is not reported when 'before' cycle count data was collected	October 1986	No, discussed separately from accident data	Presents before and after cyclist volumes and percentage changes for 5 intervention and 2 control sites	For 'after' data, cycle counts were taken at 4 sites on the route itself. Presents cycle count data by age and sex for the cycle route but does not report similar data for the catchment area
***Harbidge 1993 Kempston only**	Average daily number		11 stations	16-hour (06:00 to 22:00) cycle counts. 6-day period (Monday to Saturday).	1985, 1986, 1987 and 1990	No, discussed separately from collision data	Presents numbers and percentage change in cycle flow before and after for 3 experimental and 1 control site	It is not clear whether the 1986 data is before or after opening of the cycle route, which occurred in June 1986
***Shipley 1994 Southampton only**	Cycle counts, average daily flow	Transport Research Laboratory 1987, 1988 and 1990	At 3 experimental sites (along the cycle route) and 3 control sites (away from the cycle corridor)		Counts were made on 5 successive weekdays from 07:00 to 19:00 during March or April in 1987 (before) , 1988 and 1990 (after)	Discussed narratively with cycle collision data	Presents average daily flow before and after for 3 experimental and 3 control sites. Control sites decreased by 13.6% from 1987 to 1990, cycle flow on experimental sites	

Table 3. Cycle flow data *(Continued)*

							increased by 28.2% from 1987 to 1990	
Nicholson 1979	Numbers for 13-hour count and mean flow (cycles/hour) for 2-hour count	No details given	20 sites: 6 on the cycle route 2 on a major road parallel to the cycle route 2 on a residential road parallel to the cycle route 3 on a link route 2 on a major road parallel to the link route 5 at the entrance to the Naval Base.	1. Regular counts made between 15:30 and 17:15 at two sites on the cycle route 2. 13-hour counts (06:00 to 19:00)	1. Conducted approximately every 3 months before, every 2 weeks during and every month after the experiment 2. 13-hour counts taken at 20 points in the city (including 6 on the cycle route) at approximately quarterly intervals June 1975 to Nov 1976	No, discussed separately from collision data	Presents change in numbers of cycle flow on cycle route and on other parts of the road network including major road parallel to cycle route before and during implementation, not after. After data only for 2 sites on the cycle route	
Quenault 1981	Numbers, change and percentage change	No details given	15 sites: at 9 primary sites located on the cycle route (5 peak period only, 1 off peak only) at 2 secondary sites close to, but not on, the cycle route, at 4 control sites "well away" from the cycle route. All other sites 16-	Peak period: 2-hour morning and evening (07:00 to 09:00 and 16:00 to 18:00) 16-hour period: 06:00 to 22:00. On cycle routes counts made on weekdays at fortnightly intervals Off the	Mar 1977 to Oct 1978	No, discussed separately from accident data	Percentage change before and after at primary and secondary sites adjusted by observed annual trend at control sites	Some counts included adults versus children. Cyclist recorded as men, women, boys, girls. (latter estimated under 16 years) Some missing data

Table 3. Cycle flow data (*Continued*)

			hour periods	route counts made on weekdays at 4-weekly intervals.				
Package of infrastructure								
Wheeler 1987	Numbers and percentage change	Westminster City Council and Greater London Council	Albert Gate and 6 other sites across the south side of Hyde Park At Albion Gate and 6 other sites on the north side of Hyde Park At 3 control sites.	Weekdays (Monday to Friday) in both directions at each site over two-hour peak periods in the morning (08:00 to 10:00), lunchtime (12:00 to 14:00) and evening (16:30 to 18:30)	Sept 1981 Sept 1982 Sept 1983	Yes, accidents x 100/ cycle flow (but used cycle flow data for whole of cordon area collected by London Transport)	Presents numbers and percentage change from before and after for intervention and control sites	Data presented as total of 2-way flows in 6 peak hours a day summed over 5 week days
Roundabout design								
Daniels 2009	No data							
Jensen 2013	No data							

APPENDICES

Appendix 1. Search strategies

A study filter was not added to the strategies because the review includes non-randomized controlled studies which often do not report sufficient information to classify them accordingly (Reeves 2011).

1. Databases searched by Information Specialist, Cochrane Injuries Group

Cochrane Injuries Group Specialised Register (Cochrane Register of Studies (CRS))

#1 ((bicycl* or cycl* or bike or commute* or pedestrian* or walk* or lane*) OR (speed management) OR (speed reduction) OR (speed control)) AND (INREGISTER) [REFERENCE] [STANDARD]
#2 ((road hump) OR (road cushion) OR (road narrowing)) AND (INREGISTER) [REFERENCE] [STANDARD]
#3 ((speed hump) OR (speed cushion) OR (speed narrowing)) AND (INREGISTER) [REFERENCE] [STANDARD]
#4 #1 OR #2 OR #3 [REFERENCE] [STANDARD]
#5 ((environmental health) OR (safety or control or reduction) OR (infrastructure or management or urbanization or urbanisation) OR (public health)) AND (INREGISTER) [REFERENCE] [STANDARD]
#6 #4 AND #5 [REFERENCE] [STANDARD]
#7 ((injur*) OR (accident* or fatal* or wound* or trauma* or fracture* or lacerat*)) AND (INREGISTER) [REFERENCE] [STANDARD]
#8 #6 AND #7 [REFERENCE] [STANDARD]

CENTRAL (The Cochrane Library)

#1MeSH descriptor: [Bicycling] explode all trees
#2(cycl* or bike or bicycl*):ti,ab,kw (Word variations have been searched)
#3"commute*":ti,ab,kw (Word variations have been searched)
#4(speed near/1 (management or reduction or control)):ti,ab,kw (Word variations have been searched)
#5((cycl* or bicycl* or bike) near/3 (lane* or route* or way or trail or link or track* or road* or path* or symbol* or amenit* or network* or exemption* or street* or stage or box*)):ti,ab,kw (Word variations have been searched)
#6(on-road or off-road):ti,ab,kw (Word variations have been searched)
#7(pedestrian* or walk*):ti,ab,kw (Word variations have been searched)
#8bike or bicycl* or cycl*:ti,ab,kw (Word variations have been searched)
#9#7 and #8
#10roundabout* or junction* or footpath* or footway* or pathway* or sidewalk*:ti,ab,kw (Word variations have been searched)
#11((cycl* or bicycle* or bike) near/3 (segregat* or share or separate)):ti,ab,kw (Word variations have been searched)
#12(Shar* near/1 (path* or footway* or facilit* or pavement* or sidewalk* or lane* or marking*)):ti,ab,kw (Word variations have been searched)
#13traffic calm:ti,ab,kw (Word variations have been searched)
#14((road or speed) near/1 (hump or cushion or narrowing)):ti,ab,kw (Word variations have been searched)
#15#1 or #2 or #3 or #4 or #5 or #6 or #9 or #10 or #11 or #12 or #13 or #14
#16MeSH descriptor: [Environmental Health] this term only
#17MeSH descriptor: [Safety] this term only
#18MeSH descriptor: [Urbanization] explode all trees
#19MeSH descriptor: [Urban Health] explode all trees
#20MeSH descriptor: [Public Health] explode all trees
#21MeSH descriptor: [Accidents, Traffic] explode all trees and with qualifiers: [Prevention & control - PC, Statistics & numerical data - SN]
#22MeSH descriptor: [City Planning] explode all trees
#23MeSH descriptor: [Environment Design] this term only
#24MeSH descriptor: [Urban Renewal] this term only
#25(speed near/1 (management or reduction or control)):ti,ab,kw (Word variations have been searched)
#26(infrastructure near/3 (transport or change or management)):ti,ab,kw (Word variations have been searched)
#27MeSH descriptor: [Accident Prevention] explode all trees
#28((prevention or reduction) near/3 (accident* or conflict* or crash* or fatal* or wound* or injur* or trauma* or fracture* or lacerat*)):ti,ab,kw (Word variations have been searched)
#29conflict* and (cycl* or bike or bicycl*):ti,ab,kw (Word variations have been searched)

#30#16 or #17 or #18 or #19 or #20 or #21 or #22 or #23 or #24 or #25 or #26 or #27 or #28 or #29
#31#15 and #30
#32MeSH descriptor: [Wounds and Injuries] explode all trees
#33(accident* or crash* or fatal* or wound* or injur* or trauma* or fracture* or lacerat*):ti,ab,kw (Word variations have been searched)
#34#32 or #33
#35#31 and #34

MEDLINE (OvidSP)

1. exp Bicycling/
2. (cycl* or bike or bicycl*).ab,ti.
3. “commute*”.ab,ti.
4. (speed adj1 (management or reduction or control)).ab,ti.
5. ((cycl* or bicycl* or bike) adj3 (lane* or route* or way or trail or link or track* or road* or path* or symbol* or amenit* or network* or exemption* or street* or stage or box*)).ab,ti.
6. ((on-road or off-road) adj3 (lane* or path*)).ab,ti.
7. ((pedestrian* or walk*) adj3 (bike or bicycl* or cycl*)).ab,ti.
8. (roundabout* or junction* or footpath* or footway* or pathway* or sidewalk*).ab,ti.
9. ((cycl* or bicycl* or bike) adj3 (segregat* or share or separate)).ab,ti.
10. ((cycl* or bike or bicycl*) adj4 (signal* or facilit*)).ab,ti.
11. (Shar* adj1 (path* or footway* or facilit* or pavement* or sidewalk* or lane* or marking*)).ab,ti.
12. “traffic calm*”.ab,ti.
13. ((road or speed) adj1 (hump or cushion or narrowing)).ab,ti.
14. 1 or 2 or 3 or 4 or 5 or 6 or 7 or 8 or 9 or 10 or 11 or 12 or 13
15. *Environmental Health/
16. *Safety/
17. (speed adj1 (management or reduction or control)).ab,ti.
18. *Accidents, Traffic/pc, sn [Prevention & Control, Statistics & Numerical Data]
19. (infrastructure adj3 (transport or change or management)).ab,ti.
20. exp Accident Prevention/
21. *Urbanization/
22. *Urban Health/
23. *city planning/ or *environment design/ or *urban renewal/
24. *Public Health/
25. ((prevention or reduction) adj3 (accident* or crash* or fatal* or wound* or injur* or trauma* or fracture* or lacerat*)).ab,ti.
26. (conflict* and (cycl* or bike or bicycl*)).ab,ti.
27. 15 or 16 or 17 or 18 or 19 or 20 or 21 or 22 or 23 or 24 or 25 or 26
28. exp *“Wounds and Injuries”/
29. (accident* or crash* or fatal* or wound* or injur* or trauma* or fracture* or lacerat*).ab,ti.
30. 28 or 29
31. 14 and 27 and 30

EMBASE Classic + EMBASE (OvidSP)

1. exp Bicycling/
2. (cycl* or bike or bicycl*).ab,ti.
3. “commute*”.ab,ti.
4. (speed adj1 (management or reduction or control)).ab,ti.
5. ((cycl* or bicycl* or bike) adj3 (lane* or route* or way or trail or link or track* or road* or path* or symbol* or amenit* or network* or exemption* or street* or stage or box*)).ab,ti.
6. ((on-road or off-road) adj3 (lane* or path*)).ab,ti.
7. ((pedestrian* or walk*) adj3 (bike or bicycl* or cycl*)).ab,ti.
8. (roundabout* or junction* or footpath* or footway* or pathway* or sidewalk*).ab,ti.
9. ((cycl* or bicycl* or bike) adj3 (segregat* or share or separate)).ab,ti.
10. ((cycl* or bike or bicycl*) adj4 (signal* or facilit*)).ab,ti.
11. (Shar* adj1 (path* or footway* or facilit* or pavement* or sidewalk* or lane* or marking*)).ab,ti.
12. “traffic calm*”.ab,ti.

13. ((road or speed) adj1 (hump or cushion or narrowing)).ab,ti.
14. 1 or 2 or 3 or 4 or 5 or 6 or 7 or 8 or 9 or 10 or 11 or 12 or 13
15. *Environmental Health/
16. *Safety/
17. (speed adj1 (management or reduction or control)).ab,ti.
18. (infrastructure adj3 (transport or change or management)).ab,ti.
19. exp Accident Prevention/
20. *Urbanization/
21. *Urban Health/
22. *city planning/ or *environment design/ or *urban renewal/
23. *Public Health/
24. ((prevention or reduction) adj3 (accident* or crash* or fatal* or wound* or injur* or trauma* or fracture* or lacerat*)).ab,ti.
25. (conflict* and (cycl* or bike or bicycl*)).ab,ti.
26. 16 or 17 or 18 or 19 or 20 or 21 or 22 or 23 or 24
27. 14 and 26
28. exp *"Wounds and Injuries"/
29. (accident* or crash* or fatal* or wound* or injur* or trauma* or fracture* or lacerat*).ab,ti.
30. 28 or 29
31. 27 and 30
32. limit 31 to exclude medline journals

PubMed

((((((((((((((accident*[Title/Abstract]) OR lacerat*[Title/Abstract]) OR fracture*[Title/Abstract]) OR trauma*[Title/Abstract]) OR injur*[Title/Abstract]) OR wound*[Title/Abstract]) OR fatal*[Title/Abstract]) OR crash*[Title/Abstract])) OR ("Wounds and Injuries"[Majr]))) AND (((((((((((((((((("Bicycling"[Mesh])) OR (((cycl*[Title/Abstract]) OR bike[Title/Abstract]) OR bicycl*[Title/Abstract])) OR (commute*[Title/Abstract])) OR (((speed management[Title/Abstract]) OR speed reduction[Title/Abstract]) OR speed control[Title/Abstract])) OR ((on-road[Title/Abstract]) AND Path*[Title/Abstract])) OR ((off-road[Title/Abstract]) AND lane*[Title/Abstract])) OR ((off-road[Title/Abstract]) AND path*[Title/Abstract])) OR ((on-road[Title/Abstract]) AND lane*[Title/Abstract])) OR (((((((((((((((((lane*[Title/Abstract]) OR path*[Title/Abstract]) OR route*[Title/Abstract]) OR trail*[Title/Abstract]) OR link*[Title/Abstract]) OR road*[Title/Abstract]) OR symbol*[Title/Abstract]) OR amenit*[Title/Abstract]) OR network*[Title/Abstract]) OR exemption*[Title/Abstract]) OR street*[Title/Abstract]) OR stage[Title/Abstract]) OR box*[Title/Abstract]) OR way*[Title/Abstract]) OR track*[Title/Abstract])) AND (((cycl*[Title/Abstract]) OR bike[Title/Abstract]) OR bicycl*[Title/Abstract]))) OR ((((walk*) AND pedestrian*)) AND (((cycl*[Title/Abstract]) OR bike[Title/Abstract]) OR bicycl*[Title/Abstract]))) OR (((((roundabout*[Title/Abstract]) OR junction*[Title/Abstract]) OR footpath*[Title/Abstract]) OR pathway*[Title/Abstract]) OR sidewalk*[Title/Abstract])) OR (((((cycl*[Title/Abstract]) OR bike[Title/Abstract]) OR bicycl*[Title/Abstract])) AND (((segregat*[Title/Abstract]) OR share[Title/Abstract]) OR separate[Title/Abstract]))) OR ((((signal*[Title/Abstract]) OR facilit*[Title/Abstract])) AND (((cycl*[Title/Abstract]) OR bike[Title/Abstract]) OR bicycl*[Title/Abstract]))) OR (((((((((path*[Title/Abstract]) OR footway*[Title/Abstract]) OR facilit*[Title/Abstract]) OR pavement*[Title/Abstract]) OR sidewalk*[Title/Abstract]) OR lane*[Title/Abstract]) OR marking*[Title/Abstract])) AND (shar*[Title/Abstract]))) OR (traffic calm*[Title/Abstract])) OR (((((hump[Title/Abstract]) OR cushion[Title/Abstract]) OR narrowing[Title/Abstract])) AND ((road[Title/Abstract]) OR speed[Title/Abstract])))))) AND ((((((((((speed management[Title/Abstract]) OR speed reduction[Title/Abstract]) OR speed control[Title/Abstract])) OR (("Environmental Health"[Majr]) OR "Safety"[Majr:noexp])) OR ((((("Accident Prevention"[Mesh]) OR ("Accidents, Traffic/prevention and control"[Majr] OR "Accidents, Traffic/statistics and numerical data"[Majr])) OR "Urbanization"[Majr]) OR "Urban Health"[Majr]) OR "Public Health"[Majr:noexp])) OR (((infrastructure[Title/Abstract])) AND (((transport[Title/Abstract]) OR change[Title/Abstract]) OR management[Title/Abstract]))) OR (((city planning[MeSH Major Topic]) OR environment design[MeSH Major Topic]) OR urban renewal[MeSH Major Topic])) OR ((((prevention[Title/Abstract]) OR reduction[Title/Abstract])) AND ((((((((accident*[Title/Abstract]) OR lacerat*[Title/Abstract]) OR fracture*[Title/Abstract]) OR trauma*[Title/Abstract]) OR injur*[Title/Abstract]) OR wound*[Title/Abstract]) OR fatal*[Title/Abstract]) OR crash*[Title/Abstract])))) OR (((((cycl*[Title/Abstract]) OR bike[Title/Abstract]) OR bicycl*[Title/Abstract])) AND (conflict*[Title/Abstract])))) AND ((((((((((accident*[Title/Abstract]) OR lacerat*[Title/Abstract]) OR fracture*[Title/Abstract]) OR trauma*[Title/Abstract]) OR injur*[Title/Abstract]) OR wound*[Title/Abstract]) OR fatal*[Title/Abstract]) OR crash*[Title/Abstract])) OR ("Wounds and Injuries"[Majr]))))) NOT (medline[SB])

ISI Web of Science: Science Citation Index Expanded (SCI-EXPANDED)
ISI Web of Science: Conference Proceedings Citation Index-Science (CPCI-S)

#10 #9 AND #8 AND #4
#9 TS=(accident* or crash* or fatal* or wound* or injur* or trauma* or fracture* or lacerat*)
#8 #7 OR #6 OR #5
#7 TI=((prevention or reduction) AND (accident* or conflict* or crash* or fatal* or wound* or injur* or trauma* or fracture* or lacerat*))
#6 TS=((safety or control or reduction OR (speed management) OR (speed reduction) OR (speed control)) AND (infrastructure or management or urbanization or urbanisation))
#5 TS=(conflict* and (cycl* or bike or bicycl*))
#4 #3 OR #2 OR #1
#3 TS=((speed hump) OR (speed cushion) OR (speed narrowing))
#2 TS=((road hump) OR (road cushion) OR (road narrowing))
#1 TI=((bicycl* or cycl* or bike or commute* or pedestrian* or walk* or lane*))

2. Databases searched by authors

TRANweb (searched to 3^{rd} February 2015)
“cycle AND safety” [in Keyword Anywhere]
“bicycle AND safety” [in Keyword Anywhere]
“cycle AND injuries” [in Keyword Anywhere]
”bicycle AND injuries” [in Keyword Anywhere]
“cycle AND accidents” [in Keyword Anywhere]
“ Bicycle AND accidents” ” [in Keyword Anywhere]
“Cycle AND crash” ” [in Keyword Anywhere]
“bicycle AND crash” ” [in Keyword Anywhere]
“Cycle AND infrastructure” ” [in Keyword Anywhere]
“Bicycle AND infrastructure” ” [in Keyword Anywhere]

SafetyLit (searched to 3^{rd} February 2015)
Searched “bicycle facilities” textword+synonyms
AND “injury” or “injuries” textword(s)
Searched “speed bumps” textword+synonyms AND “cycle” or “bicycle” textwords EXACT AND “injury” or “injuries” textword(s) Exact (search included articles with “bicycle” etc)
Searched “cycle” OR “bicycle” textword EXACT AND “crash” textwords EXACT
Searched “cycle OR bicycle” textword EXACT AND “conflict” textwords EXACT

OpenSigle (searched to 1^{st} February 2015)
bicycl*
Cycl* AND safety

TRID (searched to 3^{rd} February 2015)
(cycl* OR bicycl*) AND safety
((cycl* (NOT “life cycle”)) OR bicycl*) AND (conflict OR conflicts) AND (evaluat* OR trial OR “case control”)
((cycl* (NOT “life cycle”)) OR bicycl*) AND ((conflict OR conflicts) OR ((injur*) OR (accident*) OR (crash*) AND (safety))) AND (evaluat* OR trial OR “case control”)
Searched “cyclist” in All fields
((cycl* (NOT “life cycle”)) OR bicycl*) AND ((conflict OR conflicts) OR (injur*)) AND (evaluat* OR trial OR “case control”)
((cycl* (NOT “life cycle”)) OR bicycl*) AND ((conflict OR conflicts) OR (injur*) OR (accident*) AND (safety))) AND (evaluat* OR trial OR “case control”)
((cycl* (NOT “life cycle”)) OR bicycl*) AND ((conflict OR conflicts) OR (injur*) OR (accident*)) AND (evaluat* OR trial OR “case control”)

Geobase (searched to 8^{th} April 2013)
Bicycling.mp. or cycle transport/ or urban transport/ or transportation policy/ or transportation infrastructure/ or travel behavior/ or transportation safety/2. (cycl* or bike or bicycl*).ab,ti.3. “commute*”.ab,ti.4. (speed adj1 (management or reduction or control)).ab,ti.5. ((cycl* or bicycl* or bike) adj3 (lane* or route* or way or trail or link or track* or road* or path* or symbol* or amenit* or network* or exemption* or street* or stage or box*)).ab,ti.6. ((on-road or off-road) adj3 (lane* or path*)).ab,ti.7. ((pedestrian* or walk*) adj3 (bike or bicycl* or cycl*)).ab,ti.8. (roundabout* or junction* or footpath* or footway* or pathway* or sidewalk*).ab,ti.9. ((cycl* or bicycl* or bike) adj3 (segregat* or share or separate)).ab,ti.10. ((cycl* or bike or bicycl*) adj4 (signal* or facilit*)).ab,ti.11. (Shar* adj1 (path* or footway* or facilit* or pavement* or sidewalk* or lane* or marking*)).ab,ti.12. “traffic calm*”.ab,ti.13. ((road or speed) adj1 (hump or cushion*

or narrowing)).ab,ti.14. 1 or 2 or 3 or 4 or 5 or 6 or 7 or 8 or 9 or 10 or 11 or 12 or 1315. Environmental Health.mp.16. Safety.mp. or "HEALTH AND SAFETY"/ or TRANSPORTATION SAFETY/ or SAFETY/17. (speed adj1 (management or reduction or control)).ab,ti.18. ACCIDENT/ or ACCIDENT PREVENTION/19. TRAFFIC MANAGEMENT/ or Traffic.mp.20. (infrastructure adj3 (transport or change or management)).ab,ti.21. exp Accident Prevention/22. Urbanization.mp. or URBANIZATION/23. Urban Health.mp. or public health/ or urban area/24. (city planning or environment design or urban renewal).mp. [mp=title, abstract, geobase descriptor, heading word, original title, keyword]25. Public Health.mp. or public health/26. ((prevention or reduction) adj3 (accident* or crash* or fatal* or wound* or injur* or trauma* or fracture* or lacerat*)).ab,ti.27. (conflict* and (cycl* or bike or bicycl*)).ab,ti.28. 15 or 16 or 17 or 18 or 19 or 20 or 21 or 22 or 23 or 24 or 25 or 26 or 2729. injury/ or "Wounds and Injuries".mp.30. (accident* or crash* or fatal* or wound* or injur* or trauma* or fracture* or lacerat*).ab,ti.31. 29 or 3032. 14 and 28 and 31
Index to Theses (searched to 25th February 2013)
(bicycl* OR cycle OR cyclist OR cycling OR bike)AND (safety OR accident* OR injur* OR crash)
Replaced by **EThOS** (e-theses online service) (http://ethos.bl.uk/Home.do) (searched 2013 to 14th March 2015)
(bicycl* OR cycle OR cyclist OR cycling OR bike)AND (safety OR accident* OR injur* OR crash)
Institute of Civil Engineers
Searched "cycle" AND "safety" in All fields
Searched "bicycle" AND "safety" in All fields
Searched "cycle" AND "injuries" in All fields
Searched "bicycle" AND "injuries" in All fields
Searched "cycle" AND "accidents" in All fields
Searched "bicycle" AND "accidents" in All fields
Searched "cycle" AND "crash" in All fields
Searched "bicycle" AND "crash" in All fields
Searched "cycle" AND "infrastructure" in All fields
Searched "bicycle" AND "infrastructure" in All fields
Searched all fields "bicycle"
Searched all fields "cycles"
Searched all fields "cycleways"
Searched all fields "cycling"
Institute of Civil Engineers (searched to 18th February 2015)
Searched "cycle" AND "safety" in All fields
Searched "bicycle" AND "safety" in All fields
Searched "cycle" AND "injuries" in All fields
Searched "bicycle" AND "injuries" in All fields
Searched "cycle" AND "accidents" in All fields
Searched "bicycle" AND "accidents" in All fields
Searched "cycle" AND "crash" in All fields
Searched "bicycle" AND "crash" in All fields
Searched "cycle" AND "infrastructure" in All fields
Searched "bicycle" AND "infrastructure" in All fields
Searched all fields "bicycle"
Searched all fields "cycles"
Searched all fields "cycleways"
Searched all fields "cycling"
Transport Research Laboratory (searched to 18th February 2015)
"bicycl* OR cycle OR cyclist OR cycling"
Sustrans Access database (searched to 18th March 2013 and website 2013 to 18th February 2015)
cycl* or bicycle* and "infrastructure"
3. Websites searched by authors
Pedestrian and Bicycling Information Center (searched to 17th February 2015)
Searched "library" on "engineering" "crashes" "health and safety"
Cycling Embassy of Great Britain (searched to 17th February 2015)
Searched "document library"
AAA Foundation (searched to 17th February 2015)

Searched “current projects”
Searched “completed projects” using filter “road safety”
Australian Road and Research Board (searched to 17^{th} February 2015)
Searched under “publications”, subheading “report and manuals”, subheading “Safety & Traffic publications”
Swedish National Road and Transport Research Institute (searched to 5^{th} March 2013)
Searched “publications” and subheading “ Traffic safety “ (2000 to all 2012)
Searched “publications” and subheading “environment “ (2000 to all 2012)
Searched “publications” and subheading “ infrastructure maintenance” ”Traffic safety “
Transport Canada (searched to 18^{th} February 2015)
“Cycl* safety”
“Bicycle”
Transportation Research Board (searched to 18^{th} February 2015)
Searched Pedestrian and Bicyclist Research
Injury Control Resource Information Network (searched to 9^{th} March 2015)
Bicycle* or cycl*
Harborview Injury Prevention and Research Center (searched to 9^{th} March 2015)
“Bicycle”
CTC UK (searched to 9^{th} March 2015)
“infrastructure”
“trials”
American Society of Civil Engineers (searched to 18^{th} February 2015);
Searched using
(accident* “OR” infrastructure “OR” injuries “OR” public safety “OR” collision “OR” conflict) AND (bicycl*) in the all text field
(accident* “OR” infrastructure “OR” injuries “OR” public safety “OR” collision “OR” conflict) AND (cycl*) in the all text field
Google Scholar (searched to 18^{th} February 2015)
Searched “cycle safety”
“Bicycle safety”
“cycle injuries”
“bicycle injuries”
“cycle accidents”
“bicycle accidents”
“cycle crash”
“bicycle crash”
“cycle infrastructure”
“bicycle infrastructure”

Appendix 2. Methods for future updates

Measures of treatment effect

To account for variations in exposure we will express self-reported or medically-attended injuries as injuries per million bike-km, where sufficient data are provided. Alternatively, we will report the number of injuries per hour of cycle use or number of injuries per cyclist, depending on how injuries are reported in the included studies. Where there is sufficient information, we will include a differentiation of injury rates by severity according to fatal, serious injury and slightly injured. We will also differentiate between injuries sustained as a result of a collision with other traffic, for example motor vehicles, other bicycles or pedestrians, and injuries relating to having ‘fallen off’ due to collision with obstacles both within the road and adjacent to the road. For dichotomous outcomes we will assess the treatment effect using relative risk with 95% confidence interval. For continuous outcomes we will assess treatment effect using differences in means and 95% confidence intervals, and for rates we will use rate ratios. For studies that provided data on subcategories of the road network, that is, intersections and road segments, we will undertake statistical analysis for each subcategory. We will produce a ’Summary of findings’ table, which will include data on the primary outcomes and an assessment of the quality of the data, using GRADE.

Unit of analysis issues

For studies using a clustered design that have not adjusted for clustering when reporting their data, we will do this using the intra-class correlation coefficient (ICC) of the study, if available. We will calculate the design effect using the ICC and the average cluster size. Where ICCs are not reported we will use those from similar, published cluster randomised trials. For dichotomous outcomes the number experiencing the event and the number of participants will be divided by the design effect. For continuous outcomes we will divide the number of participants by the design effect. For rate outcomes we will adjust the number of events and the number of kilometres travelled for clustering using the variance inflation factor (Donner 2000).

Dealing with missing data

We will assess the number of dropouts for each included study and will report the number of participants who are included in the final analysis as a proportion of the number of participants who started the study. We will also assess the extent to which studies conformed to an intention-to-treat analysis. Where data are missing we will attempt to obtain the data from the authors.

Assessment of heterogeneity

If there are sufficient studies, that is three or more, describing the same type of infrastructure we will stratify our analyses by type of infrastructure. For studies that have been combined in meta-analyses, we will assess the heterogeneity of studies by inspection of the forest plot and, in particular, the confidence intervals of the individual studies; statistical tests of heterogeneity will be undertaken using the Chi^2 test, with significance defined as a P value of < 0.1, and the I^2 statistic. I^2 values above 30% suggest that moderate heterogeneity exists. In such cases, findings will be interpreted with caution. Subgroup analyses will be undertaken to explore possible reasons for heterogeneity.

Assessment of reporting biases

We will assess publication bias by generating funnel plots and inspecting them for symmetry. For meta-analyses using 10 or more studies, we will test for asymmetry using Egger's test. We will perform this calculation using Stata.

Data synthesis

Where there are three or more studies reporting the same outcomes we will perform meta-analyses. We will estimate pooled rate ratios and 95% confidence intervals for injury rates, pooled relative risks and 95% confidence intervals for dichotomous outcomes and pooled mean differences and 95% confidence intervals for continuous outcomes. Random-effects models will be used to allow for and to quantify the degree of heterogeneity between studies. If the review includes both randomised and non-randomised studies the primary analyses will be based on randomised studies with secondary analyses including both randomised and non-randomised studies. If there are insufficient studies to undertake a meta-analysis the results from individual studies will be combined in a narrative review. For studies included in a narrative review, the key characteristics and findings of the studies will be presented. Difference and similarities between studies will be examined.

Subgroup analysis and investigation of heterogeneity

If there are sufficient data, that is two or more studies reporting relevant data, we plan to undertake several subgroup analyses. Firstly, we aim to consider the effectiveness of the infrastructure at reducing severity of injuries. Thus we will undertake a subgroup analysis according to fatal injury, serious injury and slightly injured. In addition we will undertake subgroup analyses comparing effect sizes between countries with and without cycle helmet legislation as compulsory wearing of a cycle helmet may affect the severity of injuries sustained. Secondly, we aim to evaluate the effectiveness of cycling infrastructure in reducing cycling injuries in cyclists with respect to age, sex and social group. Thus we will undertake subgroup analyses based on age (child versus adult), sex (male versus female) and social group (disadvantaged versus non-disadvantaged).

Sensitivity analysis

Sensitivity analyses will be undertaken by rerunning the analyses and including only RCTs considered to be at low risk of selection bias in terms of adequate allocation concealment, detection bias in terms of blinded outcome assessment and attrition bias due to follow-up of fewer than 80% of participants in each arm.

HISTORY

Protocol first published: Issue 2, 2013

Review first published: Issue 12, 2015

Date	Event	Description
22 February 2013	Amended	Search strategy amended.

CONTRIBUTIONS OF AUTHORS

CM had the original idea for the review, was the lead author for the Cochrane protocol and contributed to all parts of the Cochrane review.

SS contributed to the Cochrane protocol and carried out searches, filtered titles and abstracts, extracted data, input data into RevMan and contributed to the Cochrane review.

MW contributed to the Cochrane protocol and acted as a third reviewer, extracted data and contributed to the Cochrane review.

JP contributed to the Cochrane protocol and acted as a third reviewer, extracted data and contributed to the Cochrane review.

CC contributed to the Cochrane protocol and acted as a third reviewer, extracted data, provided statistical advice and contributed to the Cochrane review.

PM contributed to the Cochrane protocol and acted as a third reviewer, extracted data and contributed to the Cochrane review.

DK contributed to the Cochrane protocol and acted as a third reviewer, extracted data and contributed to the Cochrane review.

HM acted as a third reviewer and contributed to the Cochrane review.

DECLARATIONS OF INTEREST

JP: I undertake consultancy and research work in transport and two relevant clients in this regard are Sustrans, the sustainable transport charity, and the Department for Transport.

SS, MW, CM, CC, DK: None known.

SOURCES OF SUPPORT

Internal sources

- No sources of support supplied

External sources

- This Cochrane review was funded by the National Institute for Health Research Public Health Research (NIHR PHR) Programme (Project: 11/3020/04)., UK.

The views and opinions expressed therein are those of the authors and do not necessarily reflect those of the NIHR PHR Programme or the Department of Health.

DIFFERENCES BETWEEN PROTOCOL AND REVIEW

1. Hugh McClintock is now an author on the review.

2. Throughout this review we have used the term 'collision' to refer to crashes, accidents or collisions, unless we are discussing the results of a paper which specifically uses the term crash or accident.

3. The Background (specifically, 'Description of the condition' and 'Why is it important to do this review') has changed to include newer references.

4. Non-randomised studies (pre-specified as eligible study designs in the protocol) include interrupted time series studies. In such studies, data are collected at multiple time points before and after an intervention to assess whether the intervention has an effect considerably greater or less than the underlying secular trend. We only included studies of this design if they also included a control group. We had not anticipated finding any relevant studies of this design.

5. We have given 'age of cyclists' as a description of the participants in terms of adult and/or child under 'Types of participants' in the 'Characteristics of included studies' table. We also included a description of the intervention and control areas for each study and within which numbers of injuries and/or collisions involving participants are of interest.

6. Following identification of studies which met our inclusion criteria, we identified three types of infrastructure which did not readily fit into our original typology of infrastructures. These studies assessed the installation of more than one type of infrastructure: we have called this group 'Combination of cycling infrastructure'.

7. We undertook original searches in 2013 and updated the searches in 2015. As a result of changes in databases and conferences we searched slightly different sources in 2015. Details of the changes are reported.

8. We planned to produce a 'Summary of findings' table for the primary outcome injuries. However, as studies did not report self-reported or medically attended injuries we produced a 'Summary of findings' table for the secondary outcome collisions.

9. Due to the studies we found we were not able to undertake all the analyses we had proposed in our protocol. Therefore, in the main text we state the analyses we planned and what we actually undertook, and we have presented our proposed analyses for future updates in Appendix 2.

10. For 'Measures of treatment effect', as all studies reported on collisions we examined changes in collision rates before and after installation of the cycling infrastructure in intervention and control areas while accounting for changes in cycle flow.

11. For studies that provided data on subcategories of the road network, that is, intersections and road segments, we undertook statistical analysis for each subcategory.

NOTES

1. We included controlled before-after studies, defined as studies *"in which observations are made before and after the implementation of an intervention, both in a group that receives the intervention and in a control group that does not"* (EPOC). We also included interrupted time series studies (ITS). ITS studies *"can provide a method of measuring the effect of an intervention when randomisation or identification of a control group are impractical"* (EPOC). ITS studies use *"observations collected at multiple time points before and after an intervention. The design attempts to detect whether the intervention has had an effect significantly greater than any underlying trend over time. There is no way to assess the impact of any concurrent events on the outcomes of interest"* (EPOC).

2. One study employed the Empirical Bayes method which is a procedure for statistical inference in which the prior distribution is estimated from the data (Daniels 2009a). Daniels 2009a calculated an expected value for 'accident rates after implementation of the intervention' using the before accident data and general trends in accidents observed in a comparison group. This expected value was then compared to the observed value. According to Hauer (Hauer 2001), this method addresses two issues:

- It increases the precision of estimates beyond when there is only a two to three year history of accidents.
- It takes into account regression to the mean.