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[Intervention Review]

Street lighting for preventing road traffic injuries

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ABSTRACT

Background

Road traffic crashes are a major cause of death and injury, especially in low and middle-income countries. It is estimated that road traffic injuries will have risen from ninth to third in world disease burden rankings by 2020, accounting for 2.3 million deaths per year globally. Street lighting has been suggested as a relatively low-cost intervention with the potential to prevent traffic crashes.

Objectives

To assess the effects of street lighting on injuries caused by road traffic crashes.

Search methods

We searched the Cochrane Injuries Group's Specialised Register, CENTRAL, MEDLINE, EMBASE, TRANSPORT and the Australian Transport Index. We also searched the Internet and checked reference lists of relevant papers. The search was not restricted by language or publication status. The searches were conducted to October 2008.

Selection criteria

Randomised controlled trials, non-randomised controlled trials and controlled before-after studies, comparing new street lighting with unlit roads, or improved street lighting with the pre-existing lighting level.

Data collection and analysis

Two authors screened search results, extracted data, assessed risk of bias and analysed the data.

Main results

We found 17 controlled before-after studies of street lighting, all reporting crash data, of which 15 contributed data to the meta-analysis. Seven trials included a designated control site; the other ten collected data at one site with the day-time data being used as the control. The methodological quality of the trials was generally poor.

Three trials compared street lighting with an area control on total crashes; pooled rate ratio (RR) = 0.45 (95% confidence interval (CI) 0.29 to 0.69). Two trials compared street lighting with an area control on total injury crashes (all severities); RR = 0.78 (95% CI 0.63 to 0.97). No trials compared the number of fatal crashes with an area control.

Eleven trials compared street lighting with a day-time control on total crashes; pooled RR = 0.68 (95% CI 0.57 to 0.82). Six trials compared street lighting with a day-time control on total injury crashes; pooled RR = 0.68 (95% CI 0.61 to 0.77). Four trials compared street lighting with a day-time control on fatal crashes; pooled RR = 0.34 (95% CI 0.17 to 0.68).

Authors' conclusions

The results from this systematic review suggest that street lighting may prevent road traffic crashes, injuries and fatalities. However, further well designed studies are needed to determine the effectiveness of street lighting, particularly in middle and low-income countries.

PLAIN LANGUAGE SUMMARY

Street lighting for preventing road traffic crashes and injuries

Road traffic crashes are a major cause of death and injury, especially in low and middle-income countries. Worldwide, each year over a million people are killed and some ten million people are permanently disabled in road traffic crashes. Furthermore, it is estimated that road traffic injuries will have risen from ninth to third in world disease burden rankings by 2020, and will account for 2.3 million deaths each year globally.

Street lighting has been suggested as a relatively low-cost intervention with the potential to prevent traffic crashes. Street lighting may improve a driver's visual capabilities and ability to detect roadway hazards. However, it is also argued that street lighting could have an adverse effect on road safety; drivers may 'feel' safer because lighting gives them improved visibility which could result in them increasing speed and reducing concentration.

This systematic review was conducted to assess how street lighting affects the occurrence of road traffic crashes and associated injuries. The authors searched for all controlled trials comparing the effects of new street lighting with unlit roads, or improved street lighting with the pre-existing lighting level. They found 17 controlled before-after studies, all of which were conducted in high-income countries. Twelve studies investigated the effects of newly installed street lighting, four the effects of improved lighting and one investigated both new and improved lighting. Five of the studies compared the effects of street lighting with a separate area control, while the remaining 12 used data from a day-time control. The authors were able to pool crash or injury data from 15 of the studies. The risk of bias in these studies was judged to be high.

The results indicate that street lighting can prevent road traffic crashes, injuries and fatalities. This finding might be of particular interest to low and middle-income countries where the policy on street lighting is less developed and the installation of suitable lighting systems is less common than in high-income countries. However, further well designed studies are needed to determine the effectiveness of street lighting in middle and low-income countries.

BACKGROUND

Description of the condition

The global epidemic of road traffic deaths and injuries is only in its infancy and yet, according to the World Health Organization, road traffic injuries (RTIs) are already the leading cause of death by injury, the ninth cause of all deaths and the ninth contributor to the burden of disease worldwide (estimated using disability adjusted life years - DALYs) (Peden 2002). Over a million people die each year on the world's roads, and the number of injuries could be as high as 50 million (Peden 2004).

Crude estimates indicate that RTIs cost anywhere between 1% to 2% of a country's Gross National Product (GNP), depending upon that country's degree of motorisation. The Transport Research Laboratory calculates that this sums to a global bill of $578 billion (Jacobs 2000), but despite these figures average spending in 1990 was only US$1 for every DALY caused by road traffic crashes (Peden 2003). The problem is likely to become more severe as private car ownership continues to rise. By 2020 it is predicted that RTIs will have risen to third in the world disease burden rankings, accounting for 2.3 million deaths annually, over 90% of which will afflict lower and middle-income countries (Peden 2004). As far back as 1991 road deaths in China equalled those in the United States despite there being one hundred times fewer cars per thousand population (five versus 770) (Roberts 1995). The advance of motorisation is likely to further exaggerate this disparity to even more alarming proportions. Few low-income countries have an infrastructure suited for, and capable of coping with, excessive car travel. The roads in such countries are already dangerous; they are only likely to become more so.

Road traffic crashes should not be dismissed as the unfortunate culmination of chance, but are best considered as risk factors dependent upon epidemiological events. Being young, male, poor, or a pedestrian, pedal cyclist or motorist will all increase an individual's risk of involvement in a road traffic crash, as will driving at night (Zwi 1993). The risk of a driver having a crash in the dark is about one and a half to two-times higher than in daylight (Elvik 2004).

Description of the intervention

Public lighting was first introduced in both London and New York in 1882, well before the rise of the motor car. The popularity of street lighting lay in the assistance it gave to the maintenance of social order and the reduction of crime. Scientific interest in injury prevention did not follow for another 70 years or so. During the 1950s and 1960s studies began to be conducted to assess the role that street lighting could play in improving the safety of the ever busier and more dangerous roads of motorised nations. The International Commission on Illumination (CIE) argued in 1960 that lighting reduced crashes on urban traffic routes (original report updated in 1992 (CIE 1992)), and work during the following decade suggested the magnitude of this reduction to be approximately 30%. Since then the provision of street lighting has generally been justified on the basis of cost savings expected from the increased service and safety levels (Macauley 1989).

It is perhaps unfortunate that the readiness with which this figure was accepted has dissuaded researchers from conducting as much research in the field as they might have otherwise done. Although research continued throughout the 1970s, the majority of work is now 30 years or more out of date. Much has changed on the world's roads since then: traffic volume has swelled universally, including in countries where automotive travel was previously rare. As such, the estimates derived from early work may not apply to modern roads and driver behaviour. Furthermore, estimations for the effect of street lighting were never even investigated in many low and middle-income countries where there are more vulnerable road users who are less likely to be segregated from traffic. It is these countries that now carry the greatest burden from road traffic crashes.

How the intervention might work

Street lighting may improve a driver's visual capabilities and ability to detect roadway hazards, especially among older drivers. Rockwell found that when lighting quality was increased, drivers exhibited earlier detection of intersections, earlier gas pedal release and speed reduction suggesting an improvement in driver visual certainty (Rockwell 1976). In addition to these effects, street lighting can reduce the contrast between headlight glare and the environment, preventing loss of visual certainty from contrast adaptation. However, some argue that these improvements could lead to changes in driver behaviour that may have an adverse affect on safety due to the risk compensation effect. This could cause behavioural adaptation to a perceived low-risk situation: if drivers 'feel' safer because lighting gives them improved visibility then they may increase speed and reduce concentration (Assum 1999). If correct, this implies that overall risk remains constant over time, regardless of the intervention, because drivers adapt their behaviour to the reduced risk situation.

A literature review analysed the results of eleven previous investigations, and found that there was not sufficient evidence to support the claim that lighting reduced road traffic crashes (Vincent 1983). Furthermore, almost all of the eleven included studies were found to be inadequate in some respect, including inappropriate site selection and outcome evaluation. Vincent concluded that despite a widespread faith in street lighting, there was scant evidence in support. He recommended that further justification be sought on the basis of better driver reaction to hazards under lit conditions. The literature was re-examined by Elvik, who concluded that street lighting may reduce night-time fatalities by as much as 65% and night-time injuries by 30% (Elvik 1995). Both studies stated that the effect of public lighting varies with crash type and severity, and that numerous other variables may further complicate this effect.

Why it is important to do this review

The contrasting opinions of different authors and the rapidly changing state of global road behaviour, combined with the absence of recent investigation, necessitate a rigorous re-examination of the available evidence which will be provided by this systematic review.

OBJECTIVES

To examine the impact of street lighting on the incidence of injury and death caused by road traffic crashes.

METHODS

Criteria for considering studies for this review

Types of studies

Randomised controlled trials (RCTs) are the gold standard of experimental design but there may be logistical difficulties in carrying

out RCTs of street lighting interventions. A process of randomisation requires many different units to randomise, and whereas in a trial with people as units it is feasible to recruit enough to carry out the randomisation, in a trial with groups of streets as units it is much more likely that investigators will have 'recruited' very few (most likely two) units to compare.
It was vital that the study design incorporated a control and comparison area, and so randomised controlled trials, quasi-randomised controlled trials and controlled before-after studies (CBAs) were eligible for inclusion in this systematic review.

Types of participants

We considered streets or groups of streets as participants, represented by the urban or rural planning authority for the intervention area in question. Streets located in high, middle and low-income countries were eligible.

Types of interventions

The following types of comparisons, occurring on any road type, were eligible:

- areas with street lighting schemes compared to control where street lighting has not been introduced;
- areas where street lighting has been upgraded or improved compared to control with the pre-existing lighting level.

We did not include trials where street lighting was evaluated as one of a group of interventions (some or all of which are not lighting related), since it would be impossible to isolate the effect of street lighting from that of the other intervention(s).

Types of outcome measures

Primary outcomes

- Number of crashes in the control and intervention groups.
- Number of injury crashes in the control and intervention groups.
- Number of fatal crashes in the control and intervention groups.

Secondary outcomes

- Road traffic speed.
- Perceived road user safety.

Search methods for identification of studies

We applied no language restriction.

Electronic searches

We searched the following electronic databases:

- the Cochrane Injuries Group Specialised Register (searched on 16 October 2008);
- the Cochrane Central Register of Controlled Trials (CENTRAL) (*The Cochrane Library* Issue 4, 2008);
- MEDLINE (1966 to October 2008);
- EMBASE (1980 to October 2008);
- TRANSPORT (1988 to July 2007);
- Australian Transport Index (1975 to May 2003); and
- TRIS (Transport Research Information Service) (1960 to October 2008).

The search strategies are presented in Appendix 1.
We conducted an Internet search, beginning with national and international road safety organisations, including the following websites and progressing to general web search engines:

- AAA Foundation for Traffic Safety (USA) - www.aaafoundation.org;
- ARRB Australian Road Research Board - www.arrb.org.au;
- Australian Transport Safety Bureau - www.atsb.org.au;
- Information and Technology Platform for Transport, Infrastructure and Public Space (CROW) (Netherlands) - www.crow.nl;
- Danish Council for Road Safety Research - www.trm.dk/eng/veje/rft;
- Danish Transport Research Institute - www.dtf.dk;
- Department for Transport (UK) - http://www.dft.gov.uk/;
- DVR Deutscher Verkehrssicherheitsrat Road Safety Institute (Germany) - www.dvr.de;
- FINNRA Finnish National Road Administration - www.tieh.fi;
- INRETS Institut National de Recherche sur les Transports et leur Securite The French National Institute for Transport and Safety Research (France) - www.inrets.fr;
- ITE Institute of Transportation Engineers (USA) - www.ite.org;
- LET Laboratoire d'Economie des Transports Transport Economics Laboratory (France) - http://www.let.fr;
- NHTSA National Highway Traffic Safety Administration (USA) - www.nhtsa.dot.gov;
- Swedish National Roads Administration - www.vv.se;
- SWOV Institute for Road Safety Research (Netherlands) - www.swov.nl;
- TOI - Institute of Transport Economics (Norway) - www.toi.no;
- TC Transport Canada - www.tc.gov;
- TRB Transportation Research Board - www.nas.edu/trb;
- TRL Transport Research Laboratory - www.trl.co.uk;
- US Department of Transport - Federal Highway Administration (USA) - www.fhwa.dot.gov;
- VTI Swedish National Road and Transport Research Institute - www.vti.se;

- VTT Technical Research Centre of Finland - www.vtt.fi.

Searching other resources

We sought further potential published or unpublished studies by checking references of relevant papers and literature reviews and by communicating with the authors. We checked references from the relevant chapter of the Handbook of Road Safety Measures (Elvik 2004).

Data collection and analysis

Selection of studies

One author independently examined titles, abstracts and citations of all possible studies identified by the search strategy and identified potentially relevant articles. Two authors retrieved and independently examined the full text of these studies. We resolved disagreement on inclusion status by discussion with a third party.

Data extraction and management

Two authors independently extracted data using a standardised form. We extracted the following information:

- type of study: RCT or CBA, quality markers;
- study setting: type of road, speed of traffic, time of year and/or day outcomes were measured;
- type of intervention: design and colour of lighting installed, corresponding change in luminance or intensity and number of roadside obstacles;
- follow up: duration of follow up (RCT) or data collection in each period (CBA); and
- outcomes: number and type of road user sustaining fatal, serious or minor injury; number of crashes.

Assessment of risk of bias in included studies

The assessment of the risk of bias in non-randomised trials is problematic, and no universally accepted instrument exists for formally scoring the risk of bias in controlled before-after studies. In order to assess the risk of bias in included studies we collected the following information.

1. Matching of control to intervention areas - studies with an area control may be matched by characteristics such as location of the roads (e.g. residential or rural), volume of traffic and speed limit. Alternatively, many of the studies used day-time crash statistics for the intervention area as control data. This is because in theory street lighting does not affect traffic behaviour during the day-time, so the intervention area in day-time may be considered a perfectly matched control area as all else is equal. Furthermore, because the same authority collects data on both the intervention and the control scenarios, there is unlikely to be any systematic bias. However, the assumption that street lighting does not affect day-time behaviour could be incorrect; for this reason studies with day-time and area controls have been treated separately.
2. Data collection time - the length of time over which data are collected in a controlled before-after study is important as the longer the time period, the more likely it is that short-term changes will be less significant. For an intervention like street lighting, it is better for data to have been collected over at least a year to account for seasonal adjustments in daylight at different latitudes.
3. Source of outcome data - this is important for an indication of the accuracy and extent of the blinding of outcome assessment.

Incorporating a description of the study's performance against each of the above domains and our overall judgment, we completed a 'Risk of bias' table for each study as follows:

- 'yes' indicates low risk of bias;
- 'unclear' indicates unclear or unknown risk of bias; and
- 'no' indicates high risk of bias.

Measures of treatment effect

We conducted analyses to assess the effect of street lighting on the incidence of injuries and crashes. The results of each study are expressed as rate ratios. The rate ratio is the ratio of event rates post and pre-intervention in the intervention area divided by the corresponding post to pre-intervention event ratio in the control area. Provided that any changes in the population at risk are the same in both control and intervention areas, the rate ratio gives the reduction in the event rate in the intervention area compared to that in the control area. For example, a rate ratio of 0.8 corresponds to a 20% reduction in events compared to that predicted from the rates in the control area.

We calculated standard errors for logarithms of rate ratios and 95% confidence intervals for rate ratios assuming that the number of events in each area in each period followed a Poisson Distribution.

Assessment of heterogeneity

We assessed statistical heterogeneity between trials using a Chi^2 test, where P less than or equal to 0.1 was taken to indicate significant heterogeneity. We assessed the impact of heterogeneity by investigating the I^2 statistic. We calculated both these measures within Review Manager (RevMan).

Data synthesis

We pooled the log rate ratios and the corresponding standard errors, using the random-effects model, on a logarithmic scale using the generic inverse variance method in Review Manager (RevMan). The assumption of random-effects both allows for the anticipated heterogeneity between effects across studies and provides rigour if the assumption that events follow the Poisson Distribution is violated, for instance by over dispersion. Where there were no events observed in the intervention or control areas, we added 0.5 to each of the cells (intervention before, intervention after, control before, control after) to avoid computational problems caused by dividing by 0.

RESULTS

Description of studies

Results of the search

Searching identified 1308 published and unpublished studies, of which we deemed 145 to be potentially relevant based on the title or abstract, and obtained the full texts.

We found no randomised controlled trials but 17 controlled before-after studies, described in 18 separate reports, satisfied the inclusion criteria (Box 1972; Box 1989; Christie 1958; Cornwell 1972; Huber 1968; Isebrands 2006; Lamm 1985; Lipinski 1976; Pegrum 1972a; Pegrum 1972b; Richards 1981; Sabey 1973; Seburn 1948; Tamburri 1968; Tanner 1958; TVA 1969; Walker 1976).

Included studies

All 17 studies reported data on crashes at night and during the day. Seven included a designated control site or control region (Christie 1958; Cornwell 1972; Huber 1968; Lamm 1985; Lipinski 1976; Richards 1981; Sabey 1973), five of which provided area control data (Christie 1958; Huber 1968; Lamm 1985; Richards 1981; Sabey 1973); the remaining two (Cornwell 1972; Lipinski 1976) were treated as day-time control studies. Ten studies collected data at the intervention site only (in these cases day-time data were used as the control as the intervention was designed to affect only night-time crashes) (Box 1972; Box 1989; Isebrands 2006; Pegrum 1972a; Pegrum 1972b; Seburn 1948; Tamburri 1968; Tanner 1958; TVA 1969; Walker 1976). None of the studies reported data on our secondary outcomes of traffic speed and perceived road user safety.

Ten of the included studies were conducted in the United States of America (California (Tamburri 1968), Chicago (Box 1972; Box 1989), Connecticut (Huber 1968), Illinois (Lipinski 1976), Iowa (Walker 1976), Minnesota (Isebrands 2006), Missouri (Seburn 1948), Tennessee (TVA 1969) and Texas (Richards 1981)); four were conducted in the United Kingdom (two concentrated on the London area (Sabey 1973; Tanner 1958), one in Buckinghamshire (Christie 1958), and one examined sites across the country (Cornwell 1972); two in Perth, Australia (Pegrum 1972a; Pegrum 1972b); and one study was conducted in Germany (Frankfurt (Lamm 1985)). None of the included studies examined the effect of street lighting in either low or middle-income countries.

Intervention

Twelve studies (Box 1972; Christie 1958; Cornwell 1972; Isebrands 2006; Lamm 1985; Lipinski 1976; Pegrum 1972a; Pegrum 1972b; Richards 1981; Sabey 1973; TVA 1969; Walker 1976) compared the installation of street lighting against a 'before' period in which it was absent. Four studies (Box 1989; Huber 1968; Seburn 1948; Tanner 1958) examined locations where previously poor street lighting was improved to a specified standard, and one (Tamburri 1968) investigated both new and improved lighting in different areas.

Ten studies (Box 1972; Christie 1958; Cornwell 1972; Huber 1968; Lamm 1985; Richards 1981; Sabey 1973; Seburn 1948; Tanner 1958; TVA 1969) investigated the effect of continuous lighting along urban or rural roads. Six studies focused on the effect of non-continuous lighting; four of them (Isebrands 2006; Lipinski 1976; Tamburri 1968; Walker 1976) at intersections in the USA and two (Pegrum 1972a; Pegrum 1972b) at pedestrian crossings in Australia. One study (Box 1989) examined intersection and mid-block lighting separately.

Nine studies (Box 1989; Christie 1958; Huber 1968; Lamm 1985; Pegrum 1972a; Pegrum 1972b; Richards 1981; Tamburri 1968; TVA 1969) provided some detail of the intervention lighting design (see 'Characteristics of included studies'), three studies (Cornwell 1972; Sabey 1973; Tanner 1958) referred to standards to which intervention lighting matched, and the remaining five studies (Box 1972; Isebrands 2006; Lipinski 1976; Seburn 1948; Walker 1976) gave no details of intervention lighting.

Participants

A variety of different road types were used in the studies, varying from six-lane urban freeways to small, rural roads.

Further details about all of the included studies can be found in the table 'Characteristics of included studies'.

Risk of bias in included studies

Full details of the risk of bias for individual studies are presented in the 'Risk of bias in included studies' tables; a brief summary follows.

Matching of control to intervention areas

The risk of bias for this item was judged to be low for 15 studies (Box 1972; Box 1989; Cornwell 1972; Isebrands 2006; Lamm 1985; Lipinski 1976; Pegrum 1972a; Pegrum 1972b; Richards 1981; Sabey 1973; Seburn 1948; Tamburri 1968; Tanner 1958; TVA 1969; Walker 1976), unclear for one study (Huber 1968) and high for one study (Christie 1958).

Data collection time

We did not identify a specific length of data collection period above or below which a study could be classified as high or low risk of bias. We therefore judged that the risk of bias for this item was unclear for all studies.

Source of outcome data

The risk of bias for this item was judged to be low for 13 studies (Box 1972; Cornwell 1972; Huber 1968; Isebrands 2006; Lamm 1985; Pegrum 1972a; Pegrum 1972b; Richards 1981; Sabey 1973; Seburn 1948; Tamburri 1968; Tanner 1958; TVA 1969), unclear for two studies (Christie 1958; Walker 1976) and high for two studies (Box 1989; Lipinski 1976).

Effects of interventions

The search process identified 17 trials meeting the inclusion criteria, all of which reported data on subsequent crashes. Data presented in two trials (Lipinski 1976; TVA 1969) were not suitable for inclusion in the meta-analysis and have not been considered further. The pooled analysis is based on data from 15 studies. None of the studies reported usable data on the secondary outcome measures of road traffic speed and perceived road user safety.

Total crashes

(See Table 1).

In total 11 studies (Box 1972; Box 1989; Christie 1958; Isebrands 2006; Lamm 1985; Pegrum 1972a; Pegrum 1972b; Richards 1981; Seburn 1948; Tamburri 1968; Walker 1976) reported data on the subsequent number of total crashes.

All street lighting versus area control

(See Analysis 1.1).

Three trials (Christie 1958; Lamm 1985; Richards 1981) provided data for a comparison of the effectiveness of street lighting with a separate area control of unlit roads in reducing total crashes. All trials were of street lighting installed for the first time. The pooled rate ratio was 0.45 (95% confidence interval (CI) 0.29 to 0.69). There was no statistically significant heterogeneity between trials (Chi^2 = 0.34, df = 2, P = 0.85, I^2 = 0%).

All street lighting versus day-time control

(See Analysis 2.1).

Eleven trials (Box 1972; Box 1989; Christie 1958; Isebrands 2006; Lamm 1985; Pegrum 1972a; Pegrum 1972b; Richards 1981; Seburn 1948; Tamburri 1968; Walker 1976) compared the effectiveness of street lighting with a day-time control in reducing total crashes. The pooled rate ratio was 0.68 (95% CI 0.57 to 0.82). There was borderline evidence of statistically significant heterogeneity between trials (Chi^2 = 15.66, df = 10, P = 0.11, I^2 = 36%).

New street lighting versus area control

(See Analysis 3.1).

Three trials (Christie 1958; Lamm 1985; Richards 1981) provided data for a comparison of the effectiveness of new street lighting with a separate area control of unlit roads in reducing total crashes. The pooled rate ratio was 0.45 (95% CI 0.29 to 0.69). There was no statistically significant heterogeneity between trials (Chi^2 = 0.34, df = 2, P = 0.85, I^2 = 0%).

New street lighting versus day-time control

(See Analysis 4.1).

Four studies (Box 1972; Christie 1958; Lamm 1985; Richards 1981) compared the introduction of continuous street lighting for the first time with a day-time control. The pooled rate ratio was 0.83 (95% CI 0.57 to 1.21). There was borderline evidence of statistically significant heterogeneity between trials (Chi^2 = 5.12, df = 3, P = 0.16, I^2 = 41%).

(See Analysis 5.1).

Four studies (Isebrands 2006; Pegrum 1972a; Pegrum 1972b; Walker 1976) compared the introduction of street lighting for the first time on an intersection or pedestrian crossing (non-continuous) with day-time control. The pooled rate ratio was 0.53 (95% CI 0.40 to 0.71). There was no statistically significant heterogeneity between trials (Chi^2 = 2.46, df = 3, P = 0.48; I^2 = 0%).

Improvement in street lighting versus area control

None of the studies compared the effects of improved street lighting with an area control on total crashes.

Improvement in street lighting versus day-time control

(See Analysis 7.1).

Two studies (Box 1989; Seburn 1948) provided data for a comparison of improved continuous street lighting with pre-existing street lighting as the standard, using day-time control data. The pooled rate ratio was 0.72 (95% CI 0.50 to 1.02). There was borderline evidence of statistically significant heterogeneity between trials (Chi^2 = 1.96, df = 1, P = 0.16; I^2 = 49%).

(See Analysis 8.1).

Two studies (Box 1989; Tamburri 1968) compared an improvement of street lighting with pre-existing lighting on intersections, using day-time control data. The pooled rate ratio was 0.64 (95% CI 0.43 to 0.95). There was no significant heterogeneity between trials (Chi^2 = 1.05, df = 1, P = 0.31; I^2 = 5%).

Total injury crashes

(See Table 2).

In total seven studies (Box 1989; Cornwell 1972; Huber 1968; Isebrands 2006; Sabey 1973; Seburn 1948; Tanner 1958) reported data on the subsequent number of injury crashes (all severities including fatal).

All street lighting versus area control

(See Analysis 1.2).

Two studies (Huber 1968; Sabey 1973) compared the effectiveness of street lighting with an area control of no street lighting in reducing all injury crashes. The pooled rate ratio was 0.78 (95% CI 0.63 to 0.97). There was no significant heterogeneity between trials (Chi^2 = 0.89, df = 1, P = 0.34; I^2 = 0%).

All street lighting versus day-time control

(See Analysis 2.2).

Six studies (Box 1972; Cornwell 1972; Huber 1968; Isebrands 2006; Seburn 1948; Tanner 1958) compared the effectiveness of street lighting with a day-time control in reducing all injury crashes. The pooled rate ratio was 0.68 (95% CI 0.61 to 0.77). There was no statistically significant heterogeneity between trials (Chi^2 = 4.72, df = 5, P = 0.45; I^2 = 0%).

New street lighting versus area control

(See Analysis 3.2).

One trial (Sabey 1973) compared the effect of the introduction of continuous street lighting for the first time with an area control. The rate ratio was 0.75 (95% CI 0.59 to 0.95).

New street lighting versus day-time control

(See Analysis 4.2).

One study (Cornwell 1972) compared the introduction of continuous street lighting for the first time with a day-time control. The rate ratio was 0.63 (95% CI 0.50 to 0.79).

(See Analysis 5.2).

One study (Isebrands 2006) compared the introduction of street lighting for the first time on intersections with a day-time control. The rate ratio was 0.69 (95% CI 0.30 to 1.59).

Improvement in street lighting versus area control

(See Analysis 6.1).

One study (Huber 1968) compared an improvement in street lighting with a separate area control of pre-existing lighting. The rate ratio was 0.98 (95% CI 0.59 to 1.61).

Improvement in street lighting versus day-time control

(See Analysis 7.2).

Four studies (Box 1989; Huber 1968; Seburn 1948; Tanner 1958) compared an improvement in continuous street lighting with the pre-existing street lighting standard, using day-time control data. The pooled rate ratio was 0.72 (95% CI 0.57 to 0.90). There was no statistically significant heterogeneity between trials (Chi^2 = 3.93, df = 3, P = 0.27; I^2 = 24%).

(See Analysis 8.2).

One study (Box 1989) compared an improvement in non-continuous street lighting with the pre-existing street lighting standard, using day-time control data. The rate ratio was 0.58 (95% CI 0.25 to 1.31).

Fatal crashes

(See Table 3).

All street lighting versus area control

None of the studies compared the effects of street lighting with an area control on fatal crashes.

All street lighting versus day-time control

(See Analysis 2.3).

Four studies (Cornwell 1972; Isebrands 2006; Seburn 1948; Tanner 1958) reported data on subsequent fatal crashes; all used day-time control data. The pooled rate ratio was 0.34 (95% CI 0.17 to 0.68). There was no statistically significant heterogeneity between trials (Chi^2 = 2.32, df = 3, P = 0.51; I^2 = 0%).

New street lighting versus area control

None of the studies compared the effects of new street lighting with an area control on fatal crashes.

New street lighting versus day-time control

(See Analysis 4.3).

One study (Cornwell 1972) compared the introduction of continuous street lighting for the first time with a day-time control. The rate ratio was 0.21 (95% CI 0.07 to 0.65).

(See Analysis 5.3).

Three studies (Isebrands 2006; Pegrum 1972a; Pegrum 1972b) compared the introduction of non-continuous street lighting for the first time with a day-time control. The pooled rate ratio was 0.80 (95% CI 0.06 to 10.03). There was no statistically significant heterogeneity between trials (Chi^2 = 0.33, df = 1, P = 0.57; I^2 = 0%).

Improvement in street lighting versus area control

None of the studies compared the effects of improved street lighting with an area control on fatal crashes.

Improvement in street lighting versus day-time control

(See Analysis 7.3).
Two studies (Seburn 1948; Tanner 1958) compared an improvement in street lighting with the pre-existing street lighting standard, using day-time control data. The pooled rate ratio was 0.44 (95% CI 0.18 to 1.04). There was no statistically significant heterogeneity between trials (Chi^2 = 0.78, df = 1, P = 0.38; I^2 = 0%).

DISCUSSION

Summary of main results

This systematic review and meta-analysis of controlled before-after studies suggests that street lighting may prevent road traffic crashes, injuries and fatalities.

Overall completeness and applicability of evidence

The included trials were conducted in the USA, UK, Germany and Australia, hence it may not be appropriate to generalise the findings to other settings. Particular caution is required when making inferences regarding the effectiveness of street lighting in lower-income countries where roads tend to have different characteristics and usage compared to those in high-income countries.
Most studies located for inclusion in this systematic review were published before 1990, with the earliest dating back to 1948. Traffic behaviour on roads across the world has changed dramatically over the past 20 years as private car ownership has become increasingly common. Conclusions based on studies carried out decades ago may no longer be applicable to modern traffic conditions. It seems unlikely that the direction of effect would have changed but the magnitude of the effect should be treated with caution.

Quality of the evidence

The methodological quality of the included studies was generally poor. Whilst the appropriateness of collection periods and blinding of outcome assessment were reasonable, the quality of methods to select the control area were mixed, and there was potential for contamination of the control area in some studies. For example, only one study with an area control specified an intention to match the control roads by speed limit, road type and police district (Sabey 1973), while another two studies (Lamm 1985; Richards 1981) used control areas adjacent to the intervention areas (the intervention and control area in Richards 1981 were the opposite directions of the same road), hence contamination of the control areas may have occurred.
The data collection periods, both before and after, need to be of sufficient lengths to account for short-term fluctuations in traffic behaviour and to provide a reliable estimate of crash frequency. None of the included studies had a data collection period of less than 12 months for either the before periods (range one to three years) or the after periods (range one to eight years), which may be adequate to avoid the problem of confounding due to changes in the background rate of injury.
Regression to the mean, a statistical phenomenon which can occur when looking at a non-random selection of sites, should also be considered. It describes the tendency for an abnormally high (or low) number of events (such as injuries) to return to values closer to the long term mean. Any observed abnormally high (or low) number of events is thus a result of random fluctuation. It is a particular threat to controlled-before-and-after studies and has important implications when the study interest is a change in outcome. In such cases an apparent intervention effect may actually be a result of the number of events returning to the average rate after a random fluctuation. Consequently, these studies should be interpreted with caution.
As with all systematic reviews, the validity of the results is dependent on the validity of the individual included studies. No randomised controlled trials were identified for inclusion, hence the review is based on the findings of 17 controlled before-after studies (CBAs). Such non-randomised studies are susceptible to confounding and bias, and assessment of the risk of bias in such study designs is problematic.

Potential biases in the review process

Publication, dissemination and selection bias are potential threats to all systematic reviews, however they are particular threats to reviews of road safety research, as relevant data tend to be published in the grey literature of road safety organisations. Studies with "positive" or "interesting" results may be more likely to be published and accessible; indeed there may be no intention to publish until such results are discovered. In contrast to medical databases, which can be searched with high sensitivity and positive predictive value, road safety databases typically have a very limited range of index terminology, especially describing study methodology. As a result, for transport databases it is necessary to first apply a more inclusive strategy, involving comprehensive terminology, and then to assess more relevant results. Despite our efforts to identify all eligible studies, published and unpublished and irrespective of language of publication, we cannot exclude the possibility of selection bias.

We calculated rate ratios as the measure of intervention effect. An assumption of using the rate ratio as an effect estimate is that any changes in the population at risk are the same for both control and intervention areas. We cannot be certain that such an assumption was realistic for all of the included studies, thus caution is required in the interpretation of the rate ratios.

Agreements and disagreements with other studies or reviews

Vincent 1983 investigated the literature and considered 29 studies, of which the vast majority were labelled in the paper as methodologically flawed due to the selection of sites, the design of the study, or the statistical analysis. Vincent concluded that the literature did not support most of the study authors' conclusions that street lighting can reduce injuries. Twelve years later, Elvik reassessed the literature and carried out a meta-analysis of 37 studies of newly lit compared to previously unlit roads (Elvik 1995). He found a reduction in fatal injury crashes of 65% and a reduction of injury only crashes of 30%. He found the results robust with respect to different road types (freeway, urban, rural) and study design. This work was updated to include improvements in lighting compared to existing lighting (Elvik 2004). Increasing the level of lighting by up to double the previous level has a limited effect on the number of crashes, but it is not so pronounced as for newly installed lighting compared to a previously unlit road.

We have carried out a similar meta-analysis but have restricted the study designs to controlled before-after studies. A meta-analysis of 15 of these studies gives very similar results to Elvik in terms of total crashes, fatal crashes and improved lighting compared to a previously unlit road.

AUTHORS' CONCLUSIONS

Implications for practice

The results of this review suggest that street lighting may prevent road traffic crashes, injuries and fatalities. The results are unlikely to have a significant impact on the road planning policy of high-income countries. It is already widely accepted that street lighting has a safety function and has become an integral part of road design. However, in the UK, an increasing number of local councils are turning off public street lighting in certain areas in a move to reduce costs and carbon emissions (BBC 2008). The potential adverse road safety impact of such a policy should be carefully considered in light of the findings of this review.

In low and middle-income countries, however the policy on street lighting is less developed and the installation of suitable lighting systems is less common. Therefore the positive findings of this review may further encourage attention to the improvement of street lighting in lower income countries as a road safety measure.

Implications for research

No randomised controlled trials were identified in this review. Additional well designed controlled trials are needed to estimate effects accurately, with a focus on effectiveness in middle and lower-income countries. In particular, further research investigating the impact of street lighting on vulnerable road users is required.

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* *Indicates the major publication for the study*

CHARACTERISTICS OF STUDIES

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

Box 1972

Methods	Controlled before-after study; day-time control
Participants	Chicago, USA: 5.3 miles of 6-lane freeway (interstate 94) between 132nd-167th streets. 12-foot median from 132nd-146th, 33-foot median from 147th-167th
Interventions	New lighting Details not reported
Outcomes	All crashes Fatal/serious injury crashes
Notes	

Risk of bias

Bias	**Authors' judgement**	**Support for judgement**
Matching of control and intervention areas?	Low risk	Day-time control data used
Length of data collection periods?	Unclear risk	2 years before, 1 year after
Source of outcome data?	Low risk	Police reports

Box 1989

Methods	Controlled before-after study; day-time control
Participants	Ogden Ave, Chicago, USA: 2.8 km of 5-lane route from Washington to Naper Boulevard, 18 m wide, running through a commercial area. Average daily traffic 24,000 in before period, 32,000 in after period
Interventions	Improved lighting: mid-block (continuous) and intersection (non-continuous) Before: one block of modern lighting plus a few intersection lights After: 400 W high pressure sodium lamp lighting inside type III cutoff luminaires, installed at an average of 15 lux mounted at 15 m. Spaced at maximum 64 m with 44 m at intersections and 38 m at major/collector intersections
Outcomes	Injury/fatal crashes Property damage only crashes Total crashes
Notes	Economic analysis given

Box 1989 (*Continued)*

Risk of bias		
Bias	**Authors' judgement**	**Support for judgement**
Matching of control and intervention areas?	Low risk	Day-time control data used
Length of data collection periods?	Unclear risk	2 years before and after
Source of outcome data?	High risk	Collected for study

Christie 1958

Methods	Controlled before-after study; area control
Participants	Colnbrook by-pass, Buckinghamshire, UK: half a mile of single, 3-lane carriageway 30 to 33 ft wide
Interventions	New lighting Ten centrally suspended cut-off lanterns introduced at 90-yard spacing, mounted at 25 ft. 400 W mercury in 9 lanterns, 250 W mercury in 1 lantern (adjacent to unlit stretch)
Outcomes	All crashes (including non-injury)
Notes	Part III of this publication only

Risk of bias		
Bias	**Authors' judgement**	**Support for judgement**
Matching of control and intervention areas?	High risk	Control area is the remainder of the road within the county of Buckinghamshire
Length of data collection periods?	Unclear risk	3 years before, 1.5 years after

Cornwell 1972

Methods	Controlled before-after study; day-time and area controls were used, although insufficient data given to utilise the area controls
Participants	UK: 16 urban and 43 rural roads throughout England
Interventions	New lighting Introduction of lighting installed to British Standard CP1004:1963. Control data from West Midlands Constabulary

Outcomes	Fatal crashes Serious injury crashes Slight injury crashes
Notes	

Risk of bias

Bias	Authors' judgement	Support for judgement
Matching of control and intervention areas?	Low risk	Day-time control data used
Length of data collection periods?	Unclear risk	2 years before and after
Source of outcome data?	Low risk	Combination of police/local authority reports and Department of the Environment statistics

Huber 1968

Methods	Controlled before-after study; area and day-time controls
Participants	USA: Connecticut Turnpike
Interventions	Improved lighting Lighting intensity was reduced from 0.6 fc to 0.2 fc and then restored to 0.6 fc Control section: beyond geographical limits of lighting change
Outcomes	Total number of injury crashes
Notes	Single test section of freeway and corresponding control sections

Risk of bias

Bias	Authors' judgement	Support for judgement
Length of data collection periods?	Unclear risk	36 months before data (0.6 fc) 10 months intermediate data (0.2 fc) 12 months after data (0.6 fc)
Source of outcome data?	Low risk	Connecticut Highway Department

Isebrands 2006

Methods	Controlled before-after study; day-time controls
Participants	48 rural intersections, Minnesota, USA
Interventions	New lighting installed at intersections (non-continuous) Details not reported
Outcomes	All within 300 feet of intersection: Total crashes Fatal crashes Personal injury crashes Property damage only crashes
Notes	

Risk of bias

Bias	Authors' judgement	Support for judgement
Matching of control and intervention areas?	Low risk	Day-time control data used
Length of data collection periods?	Unclear risk	Mostly 3 years before and after; occasionally 2 years before or after
Source of outcome data?	Low risk	Minnesota Department of Transport

Lamm 1985

Methods	Controlled before-after study; day-time and area controls
Participants	Frankfurt, Germany: 7.9 km of 4-lane heavily developed freeway (autobahn A648/A66), with 3 m median. Speed limit = 110 km/hr. Average daily traffic 169,600 in before period; 361,900 in total after period
Interventions	New lighting Lighting devices were installed in the following areas: Section 1 (1.9 km): 250 W high-pressure sodium lamps on cable-suspended luminaires 12 m high, 20 to 21 m apart on poles installed in the middle of the median; Section 2 (3.7 km): partially lighted as section 1, then 400 W high mast lighting mounted on poles between 25 to 31m high; Section 3 (2.3 km): unlighted control section.
Outcomes	Total crashes
Notes	

Risk of bias

Lamm 1985 *(Continued)*

Bias	Authors' judgement	Support for judgement
Matching of control and intervention areas?	Low risk	Three adjacent sections of the same freeway
Length of data collection periods?	Unclear risk	1 year before (all subsections unlighted) 5 years after (A1) (subsections 1 and 2 were lighted and subsection 3 was unlighted) 3 years after (A2) (subsection 1 was lighted, subsection 2 was partially lighted, and subsection 3 was unlighted)
Source of outcome data?	Low risk	Police reports

Lipinski 1976

Methods	Controlled before-after study; area and day-time controls, although data from control area are not reported so day-time controls used
Participants	Rural at-grade intersections, Illinois, USA
Interventions	New lighting installed at intersections (non-continuous) Details not reported
Outcomes	Night crashes per year Day crashes per year Total crashes per year Night crash/total crash ratio Night crashes rate Day crash rate Total crash rate
Notes	

Risk of bias

Bias	Authors' judgement	Support for judgement
Matching of control and intervention areas?	Low risk	Day-time control data used
Length of data collection periods?	Unclear risk	Rates per year reported, not number and time period separately
Source of outcome data?	High risk	Collected for study

Pegrum 1972a

Methods	Controlled before-after study; day-time control
Participants	Perth, Australia: 57 pedestrian crossings on main roads
Interventions	New lighting installed at pedestrian crossings (non-continuous) Two 100 W SOX (sodium) in Philips SVA 140 floodlight fittings mounted 17 ft above the carriageway on poles about 12 ft in advance of the pedestrian crossing centre line. The beam is aimed at a point 3 ft above the pavement and one-quarter carriageway width out from the kerb. Nominal output of each 100 W sodium lamp is 11,000 lumens. Assuming a 40 ft carriageway width, for points 3 ft above road pavement at kerb position and 10 ft and 20 ft from kerb the maintained illumination levels on a vertical plane are about 5.5, 8.6, and 3 lm/ft^2 respectively.
Outcomes	Total crashes (pedestrian) Total crashes (vehicle only) Fatal crashes (pedestrian) Fatal crashes (vehicle only)
Notes	

Risk of bias

Bias	**Authors' judgement**	**Support for judgement**
Matching of control and intervention areas?	Low risk	Day-time control data used
Length of data collection periods?	Unclear risk	5 years before and after
Source of outcome data?	Low risk	Main Roads Department

Pegrum 1972b

Methods	Controlled before-after study; day-time control
Participants	Perth, Australia: 6 pedestrian crossings on main roads
Interventions	New lighting installed at pedestrian crossings (non-continuous) Two 100 W SOX (sodium) in Philips SVA 140 floodlight fittings mounted 17 ft above the carriageway on poles about 12 ft in advance of the pedestrian crossing centre line. The beam is aimed at a point 3 ft above the pavement and one-quarter carriageway width out from the kerb. Nominal output of each 100 W sodium lamp is 11,000 lumens. Assuming a 40 ft carriageway width, for points 3 ft above road pavement at kerb position and 10 ft and 20 ft from kerb the maintained illumination levels on a vertical plane are about 5.5, 8.6 and 3 lm/ft^2 respectively.

Outcomes	Total crashes (pedestrian) Total crashes (vehicle only) Fatal crashes (pedestrian) Fatal crashes (vehicle only)
Notes	

Risk of bias

Bias	Authors' judgement	Support for judgement
Matching of control and intervention areas?	Unclear risk	Day-time control data used
Length of data collection periods?	Unclear risk	2 years before and after
Source of outcome data?	Low risk	Mains Roads Department

Richards 1981

Methods	Controlled before-after study; day-time and area controls
Participants	Austin, Texas, USA: 7.2 miles of 4-lane interstate 35 through the city Section 1: 30 ft ditch median Sections 2/3: 20 ft raised median with a semi-rigid barrier. Average daily traffic 116,930. Speed limit of 55 mph was introduced during period of study
Interventions	New lighting Type of lamp not reported Section 1: median-mounted lighting 50 ft high and 300 ft apart Section 2: shoulder-mounted lighting 30 ft high and 175 ft apart (sited opposite) Section 3: shoulder-mounted lighting 30 ft high and 175 ft apart (sited staggered) South-bound lighting in all 3 sections was turned off in response to a power shortage in the area and served as control
Outcomes	Total crashes
Notes	Energy saving and economic analysis given

Risk of bias

Bias	Authors' judgement	Support for judgement
Matching of control and intervention areas?	Low risk	North- and south-bound lanes of the same freeway
Length of data collection periods?	Unclear risk	2 years before and after

Source of outcome data?	Low risk	Austin Transportation Department

Sabey 1973

Methods	Controlled before-after study; area control
Participants	UK: 37 trunk road sites throughout England with speed limits of 30, 40, 50 or 70 mph
Interventions	New lighting Lighting installed to British Standard CP1004:1963. Remainder of trunk and class I roads in the same police district and subject to the same speed limit as intervention sites served as control
Outcomes	Fatal/serious crashes All personal injury crashes
Notes	Economic analysis given

Risk of bias

Bias	Authors' judgement	Support for judgement
Matching of control and intervention areas?	Low risk	Unlit, similar roads with the same speed limit in the same district used as control
Length of data collection periods?	Unclear risk	All data collection periods at least one year and scaled down to be of equal length
Source of outcome data?	Low risk	TRRL computer records

Seburn 1948

Methods	Controlled before-after study; day-time control
Participants	Kansas City, Missouri, USA: major arteries
Interventions	Improved lighting Details not reported
Outcomes	Fatal crashes Injury crashes Property damage only crashes Total crashes
Notes	

Seburn 1948 (*Continued*)

Risk of bias		
Bias	**Authors' judgement**	**Support for judgement**
Matching of control and intervention areas?	Low risk	Day-time control data used
Length of data collection periods?	Unclear risk	Either 6 months or 1 year before and after
Source of outcome data?	Low risk	Kansas City Department of Public Works

Tamburri 1968

Methods	Controlled before-after study; day-time control
Participants	California, USA: 4 urban high-volume intersections
Interventions	Improved lighting at intersections Previous obsolete low-intensity lighting was improved to 20,000 lumen mercury vapour luminaires
Outcomes	Total crashes
Notes	This study includes flashing beacons, safety lighting, delineation devices and protective guardrail. Reported separately; lighting data extracted Safety lighting section included 26 intersection lighting projects where no previous lighting existed, but no control data were reported for these

Risk of bias		
Bias	**Authors' judgement**	**Support for judgement**
Matching of control and intervention areas?	Low risk	Day-time control data used
Length of data collection periods?	Unclear risk	Before and after time is equal but length not specified
Source of outcome data?	Low risk	California Highway District

Tanner 1958

Methods	Controlled before-after study; day-time control
Participants	UK: 64 lengths of road throughout
Interventions	Improved lighting Lighting improved to British Standard CP1004:1952. Mostly 140 W sodium, 240 W fluorescent or 400 W mercury discharge lamps
Outcomes	Fatal/serious/slight injuries involving a pedestrian Fatal/serious/slight injuries not involving a pedestrian Total fatal/serious/slight injuries Property damage crashes
Notes	Economic analysis given

Risk of bias

Bias	Authors' judgement	Support for judgement
Matching of control and intervention areas?	Low risk	Day-time control data used
Length of data collection periods?	Unclear risk	In most cases the before and after periods are 1, 2, or 3 complete years each. In a few cases however, 1 period might be 2 years and the other 1 year, or each period might be only 6 months
Source of outcome data?	Low risk	Metropolitan Police records

TVA 1969

Methods	Controlled before-after study; day-time control
Participants	Nashville-Davidson County, Tennessee, USA 32.1 miles of suburban highway, mostly 4-lane with a 4 ft concrete median strip. Average daily traffic = 69,625 before lighting; 85,945 after lighting (2-year average)
Interventions	New lighting IES type II, medium, cutoff luminaires, 400 W clear mercury, mounted at 30 ft, mast arms and pole setback at 8 to 20 ft, average spacing 105 ft
Outcomes	Total crashes Fatalities Personal injuries Property damage All reported per million vehicular miles

Notes	Economic analysis given	
Risk of bias		
Bias	**Authors' judgement**	**Support for judgement**
Matching of control and intervention areas?	Low risk	Day-time control data used
Length of data collection periods?	Unclear risk	1 year before, 2 years after
Source of outcome data?	Low risk	State of Tennessee Department of Highways, Research and Planning Division

Walker 1976

Methods	Controlled before-after study; day-time control	
Participants	Iowa, USA 47 rural at-grade channelised or unchannelised intersections requiring traffic to stop in 1, 2, 3 or all 4 directions	
Interventions	New lighting at intersections (non-continuous) Details not reported	
Outcomes	Total crashes	
Notes		
Risk of bias		
Bias	**Authors' judgement**	**Support for judgement**
Matching of control and intervention areas?	Low risk	Day-time control data used
Length of data collection periods?	Unclear risk	3 years before and after

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

Study	Reason for exclusion
Al-Balbissi 2000	Not randomised controlled trial or controlled before-after study
Assum 1999	No injury outcomes
Austin 1976	Review of other studies
Bielby 1991	Not randomised controlled trial or controlled before-after studyy
Borel 1958	Review of other studies
Box 1972a	Not randomised controlled trial or controlled before-after study
Bruneau 2001	Not randomised controlled trial or controlled before-after study
Canel 2000	No injury outcomes
Christie 1962	Not randomised controlled trial or controlled before-after study
Christie 1966	Review of other studies
Cleveland 1969	Review of other studies
Cobb 1987	Not randomised controlled trial or controlled before-after study
Corben 1998	Not randomised controlled trial or controlled before-after study
De Clercq 1985	Not randomised controlled trial or controlled before-after study
Fisher 1971	Review of other studies
Fisher 1977	Review of other studies
Fisher 1980	Review of other studies
Fisher 1990	Not randomised controlled trial or controlled before-after study
Green 2003	Not randomised controlled trial or controlled before-after study
Griffith 1994	Not randomised controlled trial or controlled before-after study
Hasson 2002	No injury outcomes
Havard 2002	Not randomised controlled trial or controlled before-after study
Highways Agency 2008	Not randomised controlled trial or controlled before-after study

(*Continued*)

ITE 1966	Review of other studies
Ives 1962	Not randomised controlled trial or controlled before-after study
Janoff 1987	No injury outcomes
Kulmala 1994	Road lighting examined as part of a group of interventions; cannot separate its effect
Polus 1978	Does not differentiate between signage and lighting
Radalj 2003	Lighting is for warning rather than illumination
Schwab 1982	Review of other studies
Taragin 1960	Not randomised controlled trial or controlled before-after study
Victoria 2002	Not randomised controlled trial or controlled before-after study
Vincent 1981	Review of other studies
Wilken 2001	Not randomised controlled trial or controlled before-after study

Characteristics of studies awaiting assessment *[ordered by study ID]*

Brüde 1985

Methods	
Participants	
Interventions	
Outcomes	
Notes	

Harris 1954

Methods	
Participants	
Interventions	
Outcomes	

Notes	

Jackett 1996

Methods	
Participants	
Interventions	
Outcomes	
Notes	

Jamil 1990

Methods	
Participants	
Interventions	
Outcomes	
Notes	

Johnson 1965

Methods	
Participants	
Interventions	
Outcomes	
Notes	Referenced in Tamburri 1968, which is discontinuous lighting version

Jørgensen 1971

Methods	
Participants	
Interventions	
Outcomes	

Jørgensen 1971 *(Continued)*

Notes	

Jørgensen 1980

Methods	
Participants	
Interventions	
Outcomes	
Notes	

Ketvirtis 1977

Methods	
Participants	
Interventions	
Outcomes	
Notes	

NBPR 1978

Methods	
Participants	
Interventions	
Outcomes	
Notes	

Scott 1980

Methods	
Participants	
Interventions	
Outcomes	

Notes	

Talbot 1975

Methods	
Participants	
Interventions	
Outcomes	
Notes	

Walthert 1970

Methods	
Participants	
Interventions	
Outcomes	
Notes	

Witakowski 1980

Methods	
Participants	
Interventions	
Outcomes	
Notes	

DATA AND ANALYSES

Comparison 1. All street lighting versus area control

Outcome or subgroup title	No. of studies	No. of participants	Statistical method	Effect size
1 Total crashes	3		Rate ratio (Random, 95% CI)	0.45 [0.29, 0.69]
2 Total injury crashes	2		Rate ratio (Random, 95% CI)	0.78 [0.63, 0.97]

Comparison 2. All street lighting versus day-time control

Outcome or subgroup title	No. of studies	No. of participants	Statistical method	Effect size
1 Total crashes	11		Rate ratio (Random, 95% CI)	0.68 [0.57, 0.82]
2 Total injury crashes	6		Rate ratio (Random, 95% CI)	0.68 [0.61, 0.77]
3 Fatal crashes	4		Rate ratio (Random, 95% CI)	0.34 [0.17, 0.68]

Comparison 3. New street lighting versus area control

Outcome or subgroup title	No. of studies	No. of participants	Statistical method	Effect size
1 Total crashes	3		Rate ratio (Random, 95% CI)	0.45 [0.29, 0.69]
2 Total injury crashes	1		Rate ratio (Random, 95% CI)	0.75 [0.59, 0.95]

Comparison 4. New street lighting (continuous) versus day-time control

Outcome or subgroup title	No. of studies	No. of participants	Statistical method	Effect size
1 Total crashes	4		Rate ratio (Random, 95% CI)	0.83 [0.57, 1.21]
2 Total injury crashes	1		Rate ratio (Random, 95% CI)	0.63 [0.50, 0.79]
3 Fatal crashes	1		Rate Ratio (Random, 95% CI)	0.21 [0.07, 0.65]

Comparison 5. New street lighting (non-continuous) versus day-time control

Outcome or subgroup title	No. of studies	No. of participants	Statistical method	Effect size
1 Total crashes	4		Rate Ratio (Random, 95% CI)	0.53 [0.40, 0.71]
2 Total injury crashes	1		Rate Ratio (Random, 95% CI)	0.69 [0.30, 1.59]
3 Fatal crashes	3		Rate Ratio (Random, 95% CI)	0.66 [0.38, 1.13]

Comparison 6. Improved street lighting versus area control

Outcome or subgroup title	No. of studies	No. of participants	Statistical method	Effect size
1 Total injury crashes	1		Rate ratio (Random, 95% CI)	0.98 [0.59, 1.61]

Comparison 7. Improved street lighting (continuous) versus day-time control

Outcome or subgroup title	No. of studies	No. of participants	Statistical method	Effect size
1 Total crashes	2		Rate Ratio (Random, 95% CI)	0.72 [0.50, 1.02]
2 Total injury crashes	4		Rate ratio (Random, 95% CI)	0.72 [0.57, 0.90]
3 Fatal crashes	2		Rate Ratio (Random, 95% CI)	0.44 [0.18, 1.04]

Comparison 8. Improved street lighting (non-continuous) versus day-time control

Outcome or subgroup title	No. of studies	No. of participants	Statistical method	Effect size
1 Total crashes	2		Rate Ratio (Random, 95% CI)	0.64 [0.43, 0.95]
2 Total injury crashes	1		Rate Ratio (Random, 95% CI)	0.58 [0.25, 1.31]

Analysis 1.1. Comparison 1 All street lighting versus area control, Outcome 1 Total crashes.

Review: Street lighting for preventing road traffic injuries

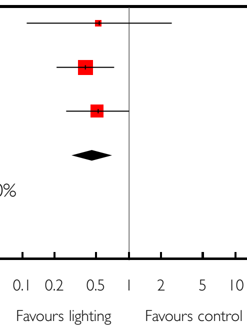
Comparison: 1 All street lighting versus area control

Outcome: 1 Total crashes

Study or subgroup	log [Rate ratio] (SE)	Rate ratio IV,Random,95% CI	Weight	Rate ratio IV,Random,95% CI
Christie 1958	-0.6444 (0.7936)		7.9 %	0.52 [0.11, 2.49]
Lamm 1985	-0.9343 (0.3161)		49.6 %	0.39 [0.21, 0.73]
Richards 1981	-0.6829 (0.3412)		42.6 %	0.51 [0.26, 0.99]
Total (95% CI)			**100.0 %**	**0.45 [0.29, 0.69]**

Heterogeneity: $Tau^2 = 0.0$; $Chi^2 = 0.34$, df = 2 (P = 0.85); $I^2 = 0.0\%$

Test for overall effect: Z = 3.61 (P = 0.00030)



Analysis 1.2. Comparison 1 All street lighting versus area control, Outcome 2 Total injury crashes.

Review: Street lighting for preventing road traffic injuries

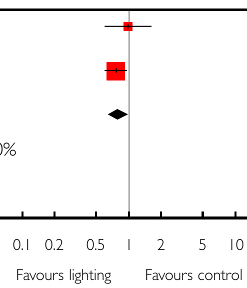
Comparison: 1 All street lighting versus area control

Outcome: 2 Total injury crashes

Study or subgroup	log [Rate ratio] (SE)	Rate ratio IV,Random,95% CI	Weight	Rate ratio IV,Random,95% CI
Huber 1968	-0.0245 (0.2567)		18.7 %	0.98 [0.59, 1.61]
Sabey 1973	-0.2934 (0.1231)		81.3 %	0.75 [0.59, 0.95]
Total (95% CI)			**100.0 %**	**0.78 [0.63, 0.97]**

Heterogeneity: $Tau^2 = 0.0$; $Chi^2 = 0.89$, df = 1 (P = 0.34); $I^2 = 0.0\%$

Test for overall effect: Z = 2.19 (P = 0.028)



Analysis 2.1. Comparison 2 All street lighting versus day-time control, Outcome 1 Total crashes.

Review: Street lighting for preventing road traffic injuries

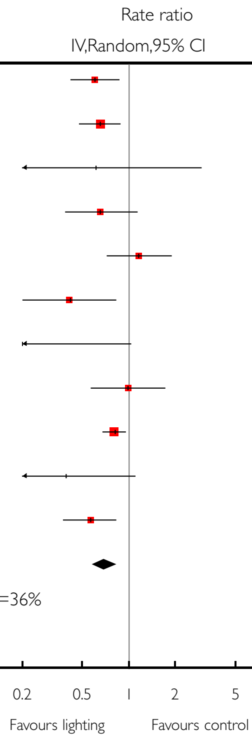
Comparison: 2 All street lighting versus day-time control

Outcome: 1 Total crashes

Study or subgroup	log [Rate ratio] (SE)	Weight	Rate ratio IV,Random,95% CI
Box 1972	-0.518 (0.1942)	13.0 %	0.60 [0.41, 0.87]
Box 1989	-0.438 (0.1582)	16.0 %	0.65 [0.47, 0.88]
Christie 1958	-0.4953 (0.8126)	1.2 %	0.61 [0.12, 3.00]
Isebrands 2006	-0.4243 (0.2828)	7.9 %	0.65 [0.38, 1.14]
Lamm 1985	0.1501 (0.2515)	9.4 %	1.16 [0.71, 1.90]
Pegrum 1972a	-0.8873 (0.3566)	5.5 %	0.41 [0.20, 0.83]
Pegrum 1972b	-1.6094 (0.8367)	1.2 %	0.20 [0.04, 1.03]
Richards 1981	-0.019 (0.2892)	7.7 %	0.98 [0.56, 1.73]
Seburn 1948	-0.2145 (0.0911)	23.3 %	0.81 [0.67, 0.96]
Tamburri 1968	-0.9445 (0.5318)	2.8 %	0.39 [0.14, 1.10]
Walker 1976	-0.5878 (0.2052)	12.1 %	0.56 [0.37, 0.83]
Total (95% CI)		**100.0 %**	**0.68 [0.57, 0.82]**

Heterogeneity: $Tau^2 = 0.03$; $Chi^2 = 15.66$, df = 10 (P = 0.11); $I^2 = 36\%$

Test for overall effect: Z = 4.10 (P = 0.000042)



Analysis 2.2. Comparison 2 All street lighting versus day-time control, Outcome 2 Total injury crashes.

Review: Street lighting for preventing road traffic injuries

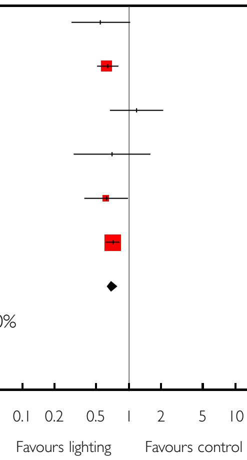
Comparison: 2 All street lighting versus day-time control

Outcome: 2 Total injury crashes

Study or subgroup	log [Rate ratio] (SE)	Rate ratio IV,Random,95% CI	Weight	Rate ratio IV,Random,95% CI
Box 1989	-0.6097 (0.3221)		3.4 %	0.54 [0.29, 1.02]
Cornwell 1972	-0.4668 (0.1162)		25.9 %	0.63 [0.50, 0.79]
Huber 1968	0.1588 (0.2955)		4.0 %	1.17 [0.66, 2.09]
Isebrands 2006	-0.3773 (0.4286)		1.9 %	0.69 [0.30, 1.59]
Seburn 1948	-0.4994 (0.2367)		6.2 %	0.61 [0.38, 0.97]
Tanner 1958	-0.3583 (0.0772)		58.6 %	0.70 [0.60, 0.81]
Total (95% CI)			**100.0 %**	**0.68 [0.61, 0.77]**

Heterogeneity: $Tau^2 = 0.0$; $Chi^2 = 4.72$, df = 5 (P = 0.45); I^2 =0.0%

Test for overall effect: Z = 6.49 (P < 0.00001)



Analysis 2.3. Comparison 2 All street lighting versus day-time control, Outcome 3 Fatal crashes.

Review: Street lighting for preventing road traffic injuries

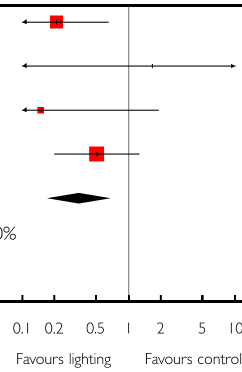
Comparison: 2 All street lighting versus day-time control

Outcome: 3 Fatal crashes

Study or subgroup	log [Rate ratio] (SE)	Weight	Rate ratio IV,Random,95% CI
Cornwell 1972	-1.5686 (0.5769)	36.4 %	0.21 [0.07, 0.65]
Isebrands 2006	0.5108 (2.2509)	2.4 %	1.67 [0.02, 137.34]
Seburn 1948	-1.9095 (1.3017)	7.1 %	0.15 [0.01, 1.90]
Tanner 1958	-0.6848 (0.473)	54.1 %	0.50 [0.20, 1.27]
Total (95% CI)		**100.0 %**	**0.34 [0.17, 0.68]**

Heterogeneity: Tau^2 = 0.0; Chi^2 = 2.32, df = 3 (P = 0.51); I^2 =0.0%

Test for overall effect: Z = 3.06 (P = 0.0022)



Analysis 3.1. Comparison 3 New street lighting versus area control, Outcome 1 Total crashes.

Review: Street lighting for preventing road traffic injuries

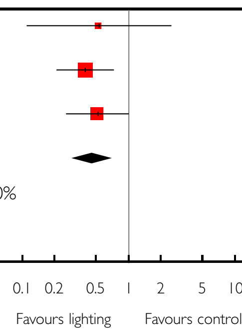
Comparison: 3 New street lighting versus area control

Outcome: 1 Total crashes

Study or subgroup	log [Rate ratio] (SE)	Weight	Rate ratio IV,Random,95% CI
Christie 1958	-0.6444 (0.7936)	7.9 %	0.52 [0.11, 2.49]
Lamm 1985	-0.9343 (0.3161)	49.6 %	0.39 [0.21, 0.73]
Richards 1981	-0.6829 (0.3412)	42.6 %	0.51 [0.26, 0.99]
Total (95% CI)		**100.0 %**	**0.45 [0.29, 0.69]**

Heterogeneity: Tau^2 = 0.0; Chi^2 = 0.34, df = 2 (P = 0.85); I^2 =0.0%

Test for overall effect: Z = 3.61 (P = 0.00030)



Analysis 3.2. Comparison 3 New street lighting versus area control, Outcome 2 Total injury crashes.

Review: Street lighting for preventing road traffic injuries

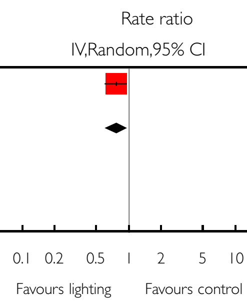
Comparison: 3 New street lighting versus area control

Outcome: 2 Total injury crashes

Study or subgroup	log [Rate ratio] (SE)	Weight	Rate ratio IV,Random,95% CI
Sabey 1973	-0.2934 (0.1231)	100.0 %	0.75 [0.59, 0.95]
Total (95% CI)		**100.0 %**	**0.75 [0.59, 0.95]**

Heterogeneity: not applicable

Test for overall effect: Z = 2.38 (P = 0.017)



Analysis 4.1. Comparison 4 New street lighting (continuous) versus day-time control, Outcome 1 Total crashes.

Review: Street lighting for preventing road traffic injuries

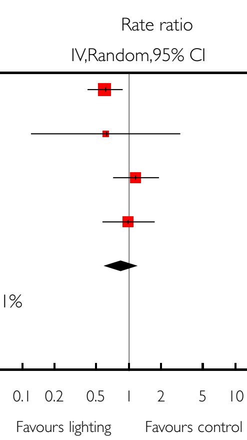
Comparison: 4 New street lighting (continuous) versus day-time control

Outcome: 1 Total crashes

Study or subgroup	log [Rate ratio] (SE)	Weight	Rate ratio IV,Random,95% CI
Box 1972	-0.518 (0.1942)	38.4 %	0.60 [0.41, 0.87]
Christie 1958	-0.4953 (0.8126)	5.2 %	0.61 [0.12, 3.00]
Lamm 1985	0.1501 (0.2515)	30.4 %	1.16 [0.71, 1.90]
Richards 1981	-0.019 (0.2892)	26.0 %	0.98 [0.56, 1.73]
Total (95% CI)		**100.0 %**	**0.83 [0.57, 1.21]**

Heterogeneity: $Tau^2 = 0.06$; $Chi^2 = 5.12$, df = 3 (P = 0.16); $I^2 = 41\%$

Test for overall effect: Z = 0.96 (P = 0.34)



Analysis 4.2. Comparison 4 New street lighting (continuous) versus day-time control, Outcome 2 Total injury crashes.

Review: Street lighting for preventing road traffic injuries

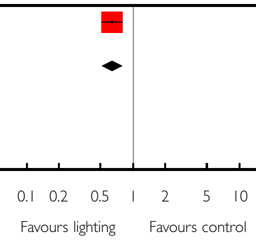
Comparison: 4 New street lighting (continuous) versus day-time control

Outcome: 2 Total injury crashes

Study or subgroup	log [Rate ratio] (SE)	Rate ratio IV,Random,95% CI	Weight	Rate ratio IV,Random,95% CI
Cornwell 1972	-0.4668 (0.1162)		100.0 %	0.63 [0.50, 0.79]
Total (95% CI)			**100.0 %**	**0.63 [0.50, 0.79]**

Heterogeneity: not applicable

Test for overall effect: Z = 4.02 (P = 0.000059)



Analysis 4.3. Comparison 4 New street lighting (continuous) versus day-time control, Outcome 3 Fatal crashes.

Review: Street lighting for preventing road traffic injuries

Comparison: 4 New street lighting (continuous) versus day-time control

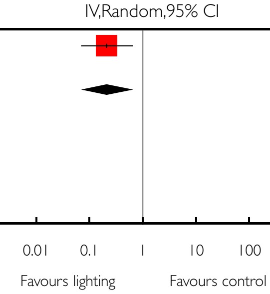
Outcome: 3 Fatal crashes

Study or subgroup	log [Rate Ratio] (SE)	Rate Ratio IV,Random,95% CI	Weight	Rate Ratio IV,Random,95% CI
Cornwell 1972	-1.5686 (0.5769)		100.0 %	0.21 [0.07, 0.65]
Total (95% CI)			**100.0 %**	**0.21 [0.07, 0.65]**

Heterogeneity: not applicable

Test for overall effect: Z = 2.72 (P = 0.0065)

Test for subgroup differences: Not applicable



Analysis 5.1. Comparison 5 New street lighting (non-continuous) versus day-time control, Outcome 1 Total crashes.

Review: Street lighting for preventing road traffic injuries

Comparison: 5 New street lighting (non-continuous) versus day-time control

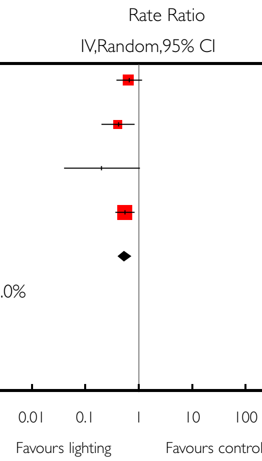
Outcome: 1 Total crashes

Study or subgroup	log [Rate Ratio] (SE)	Weight	Rate Ratio IV,Random,95% CI
Isebrands 2006	-0.4243 (0.2828)	27.5 %	0.65 [0.38, 1.14]
Pegrum 1972a	-0.8873 (0.3566)	17.3 %	0.41 [0.20, 0.83]
Pegrum 1972b	-1.6094 (0.8367)	3.1 %	0.20 [0.04, 1.03]
Walker 1976	-0.5878 (0.2052)	52.1 %	0.56 [0.37, 0.83]
Total (95% CI)		**100.0 %**	**0.53 [0.40, 0.71]**

Heterogeneity: $Tau^2 = 0.0$; $Chi^2 = 2.46$, df = 3 (P = 0.48); $I^2 = 0.0\%$

Test for overall effect: Z = 4.23 (P = 0.000023)

Test for subgroup differences: Not applicable



Analysis 5.2. Comparison 5 New street lighting (non-continuous) versus day-time control, Outcome 2 Total injury crashes.

Review: Street lighting for preventing road traffic injuries

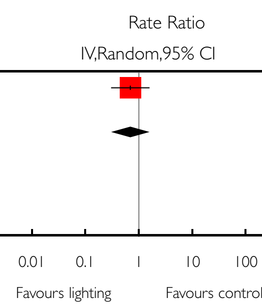
Comparison: 5 New street lighting (non-continuous) versus day-time control

Outcome: 2 Total injury crashes

Study or subgroup	log [Rate Ratio] (SE)	Weight	Rate Ratio IV,Random,95% CI
Isebrands 2006	-0.3773 (0.4286)	100.0 %	0.69 [0.30, 1.59]
Total (95% CI)		**100.0 %**	**0.69 [0.30, 1.59]**

Heterogeneity: not applicable

Test for overall effect: Z = 0.88 (P = 0.38)



Analysis 5.3. Comparison 5 New street lighting (non-continuous) versus day-time control, Outcome 3 Fatal crashes.

Review: Street lighting for preventing road traffic injuries

Comparison: 5 New street lighting (non-continuous) versus day-time control

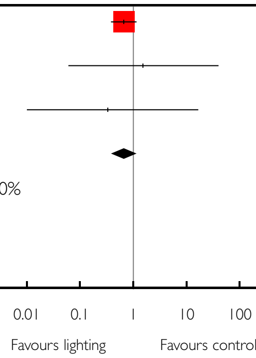
Outcome: 3 Fatal crashes

Study or subgroup	log [Rate Ratio] (SE)	Rate Ratio IV,Random,95% CI	Weight	Rate Ratio IV,Random,95% CI
Isebrands 2006	-0.4243 (0.2828)		95.4 %	0.65 [0.38, 1.14]
Pegrum 1972a	0.4055 (1.6833)		2.7 %	1.50 [0.06, 40.64]
Pegrum 1972b	-1.0986 (2)		1.9 %	0.33 [0.01, 16.80]
Total (95% CI)			**100.0 %**	**0.66 [0.38, 1.13]**

Heterogeneity: $Tau^2 = 0.0$; $Chi^2 = 0.36$, df = 2 (P = 0.84); $I^2 = 0.0\%$

Test for overall effect: Z = 1.50 (P = 0.13)

Test for subgroup differences: Not applicable



Analysis 6.1. Comparison 6 Improved street lighting versus area control, Outcome 1 Total injury crashes.

Review: Street lighting for preventing road traffic injuries

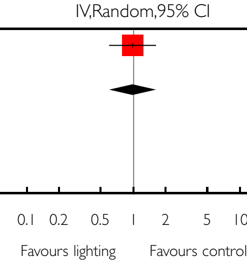
Comparison: 6 Improved street lighting versus area control

Outcome: 1 Total injury crashes

Study or subgroup	log [Rate ratio] (SE)	Rate ratio IV,Random,95% CI	Weight	Rate ratio IV,Random,95% CI
Huber 1968	-0.0245 (0.2567)		100.0 %	0.98 [0.59, 1.61]
Total (95% CI)			**100.0 %**	**0.98 [0.59, 1.61]**

Heterogeneity: not applicable

Test for overall effect: Z = 0.10 (P = 0.92)



Analysis 7.1. Comparison 7 Improved street lighting (continuous) versus day-time control, Outcome 1 Total crashes.

Review: Street lighting for preventing road traffic injuries

Comparison: 7 Improved street lighting (continuous) versus day-time control

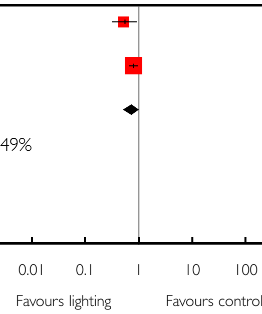
Outcome: 1 Total crashes

Study or subgroup	log [Rate Ratio] (SE)	Rate Ratio IV,Random,95% CI	Weight	Rate Ratio IV,Random,95% CI
Box 1989	-0.6084 (0.2661)		29.9 %	0.54 [0.32, 0.92]
Seburn 1948	-0.2145 (0.0911)		70.1 %	0.81 [0.67, 0.96]
Total (95% CI)			**100.0 %**	**0.72 [0.50, 1.02]**

Heterogeneity: Tau^2 = 0.04; Chi^2 = 1.96, df = 1 (P = 0.16); I^2 =49%

Test for overall effect: Z = 1.84 (P = 0.065)

Test for subgroup differences: Not applicable



Analysis 7.2. Comparison 7 Improved street lighting (continuous) versus day-time control, Outcome 2 Total injury crashes.

Review: Street lighting for preventing road traffic injuries

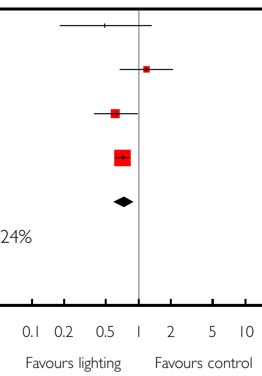
Comparison: 7 Improved street lighting (continuous) versus day-time control

Outcome: 2 Total injury crashes

Study or subgroup	log [Rate ratio] (SE)	Rate ratio IV,Random,95% CI	Weight	Rate ratio IV,Random,95% CI
Box 1989	-0.7304 (0.5095)		5.0 %	0.48 [0.18, 1.31]
Huber 1968	0.1588 (0.2955)		13.3 %	1.17 [0.66, 2.09]
Seburn 1948	-0.4994 (0.2367)		19.1 %	0.61 [0.38, 0.97]
Tanner 1958	-0.3583 (0.0772)		62.7 %	0.70 [0.60, 0.81]
Total (95% CI)			**100.0 %**	**0.72 [0.57, 0.90]**

Heterogeneity: Tau^2 = 0.02; Chi^2 = 3.93, df = 3 (P = 0.27); I^2 =24%

Test for overall effect: Z = 2.86 (P = 0.0042)



Analysis 7.3. Comparison 7 Improved street lighting (continuous) versus day-time control, Outcome 3 Fatal crashes.

Review: Street lighting for preventing road traffic injuries

Comparison: 7 Improved street lighting (continuous) versus day-time control

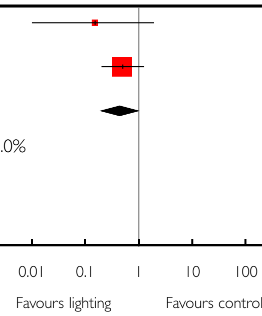
Outcome: 3 Fatal crashes

Study or subgroup	log [Rate Ratio] (SE)	Rate Ratio IV,Random,95% CI	Weight	Rate Ratio IV,Random,95% CI
Seburn 1948	-1.9095 (1.3017)		11.7 %	0.15 [0.01, 1.90]
Tanner 1958	-0.6848 (0.473)		88.3 %	0.50 [0.20, 1.27]
Total (95% CI)			**100.0 %**	**0.44 [0.18, 1.04]**

Heterogeneity: $Tau^2 = 0.0$; $Chi^2 = 0.78$, df = 1 (P = 0.38); I^2 =0.0%

Test for overall effect: Z = 1.86 (P = 0.063)

Test for subgroup differences: Not applicable



Analysis 8.1. Comparison 8 Improved street lighting (non-continuous) versus day-time control, Outcome 1 Total crashes.

Review: Street lighting for preventing road traffic injuries

Comparison: 8 Improved street lighting (non-continuous) versus day-time control

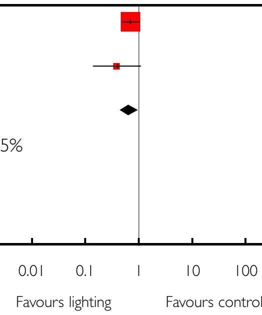
Outcome: 1 Total crashes

Study or subgroup	log [Rate Ratio] (SE)	Rate Ratio IV,Random,95% CI	Weight	Rate Ratio IV,Random,95% CI
Box 1989	-0.3635 (0.1985)		86.1 %	0.70 [0.47, 1.03]
Tamburri 1968	-0.9445 (0.5318)		13.9 %	0.39 [0.14, 1.10]
Total (95% CI)			**100.0 %**	**0.64 [0.43, 0.95]**

Heterogeneity: $Tau^2 = 0.01$; $Chi^2 = 1.05$, df = 1 (P = 0.31); $I^2 = 5\%$

Test for overall effect: Z = 2.21 (P = 0.027)

Test for subgroup differences: Not applicable



Analysis 8.2. Comparison 8 Improved street lighting (non-continuous) versus day-time control, Outcome 2 Total injury crashes.

Review: Street lighting for preventing road traffic injuries

Comparison: 8 Improved street lighting (non-continuous) versus day-time control

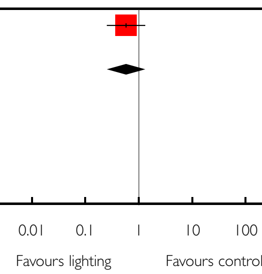
Outcome: 2 Total injury crashes

Study or subgroup	log [Rate Ratio] (SE)	Rate Ratio IV,Random,95% CI	Weight	Rate Ratio IV,Random,95% CI
Box 1989	-0.5527 (0.4202)		100.0 %	0.58 [0.25, 1.31]
Total (95% CI)			**100.0 %**	**0.58 [0.25, 1.31]**

Heterogeneity: not applicable

Test for overall effect: Z = 1.32 (P = 0.19)

Test for subgroup differences: Not applicable



ADDITIONAL TABLES

Table 1. Total crashes

	Intervention after	Intervention before	Control after	Control before	Rate ratio	Log rate ratio	Standard error
Street lighting versus area control							
Christie 1958	3	8	10	14	0.5250	-0.6444	0.7936
Lamm 1985	116	30	187	19	0.3929	-0.9343	0.3161
Richards 1981	26	35	50	34	0.5051	-0.6829	0.3412
Street lighting versus day-time control							
Box 1972	67	151	108	145	0.5957	-0.5180	0.1942
Box 1989	103	116	355	258	0.6453	-0.4380	0.1582
Christie 1958	3	8	8	13	0.6094	-0.4953	0.8126
Isebrands 2006	41	47	68	51	0.6543	-0.4243	0.2828
Lamm 1985	116	30	203	61	1.1619	0.1501	0.2515
Pegrum 1972a	14	34	76	76	0.4118	-0.8873	0.3566
Pegrum 1972b	2	8	30	24	0.2000	-1.6094	0.8367
Richards 1981	26	35	106	140	0.9811	-0.0190	0.2892
Seburn 1948	384	388	715	583	0.8070	-0.2145	0.0911
Tamburri 1968	7	15	30	25	0.3889	-0.9445	0.5318
Walker 1976	46	90	207	225	0.5556	-0.5878	0.2052

Table 1. Total crashes *(Continued)*

New street lighting versus area control							
Christie 1958	3	8	10	14	0.5250	-0.6444	0.7936
Lamm 1985	116	30	187	19	0.3929	-0.9343	0.3161
Richards 1981	26	35	50	34	0.5051	-0.6829	0.3412
New street lighting (continuous) versus day-time control							
Box 1972	67	151	108	145	0.5957	-0.5180	0.1942
Christie 1958	3	8	8	13	0.6094	-0.4953	0.8126
Lamm 1985	116	30	203	61	1.1619	0.1501	0.2515
Richards 1981	26	35	106	140	0.9811	-0.0190	0.2892
New street lighting (non-continuous) versus day-time control							
Isebrands 2006	41	47	68	51	0.6543	-0.4243	0.2828
Pegrum 1972a	14	34	76	76	0.4118	-0.8873	0.3566
Pegrum 1972b	2	84	30	24	0.2000	-1.6094	0.8367
Walker 1976	46	90	207	225	0.5556	-0.5878	0.2052

Table 1. Total crashes (*Continued*)

Improved street lighting (continuous) versus day-time control							
Box 1989	31	44	145	112	0.5442	-0.6084	0.2661
Seburn 1948	384	388	715	583	0.8070	-0.2145	0.0911
Improved street lighting (non-continuous) versus day-time control							
Box 1989	72	72	210	146	0.6952	-0.3635	0.1985
Tamburri 1968	7	15	30	25	0.3889	-0.9445	0.5318

Table 2. Total injury crashes

	Intervention after	Intervention before	Control after	Control before	Rate ratio	Log rate ratio	Standard error
All street lighting versus area control							
Huber 1968	33	36	248	264	0.9758	-0.0245	0.2567
Sabey 1973	57	98	13825	17742	0.7464	-0.2925	0.1670
All street lighting versus day-time control							
Box 1989	23	36	67	57	0.5435	-0.6097	0.3221

Table 2. **Total injury crashes** *(Continued)*

Cornwell 1972	184	283	449	433	0.6270	-0.4668	0.1162
Huber 1968	33	36	61	78	1.1721	0.1588	0.2955
Isebrands 2006	16	18	35	27	0.6857	-0.3773	0.4286
Seburn 1948	42	135	61	119	0.6069	-0.4994	0.2367
Tanner 1958	403	505	1425	1248	0.6989	-0.3583	0.0772
New street lighting versus area control							
Sabey 1973	57	98	13823	11742	0.7464	-0.2925	0.1670
New street lighting (continuous) versus day-time control							
Cornwell 1972	184	283	449	433	0.6270	-0.4668	0.1162
New street lighting (non-continuous) versus day -time control							
Isebrands 2006	16	18	35	27	0.6857	-0.3773	0.4286
Improved street lighting versus area con-							

Table 2. Total injury crashes (Continued)

trol							
Huber 1968	33	36	248	264	0.9758	-0.0245	0.2567
Improved street lighting (continuous) versus day-time control							
Box 1989	8	15	31	28	0.4817	-0.7304	0.5095
Huber 1968	33	36	61	78	1.1721	0.1588	0.2955
Seburn 1948	42	135	61	119	0.6069	-0.4994	0.2367
Tanner 1958	403	505	1425	1248	0.6989	-0.3583	0.0772
Improved street lighting (non-continuous) versus day-time control							
Box 1989	15	21	36	29	0.5754	-0.5527	0.4202

Table 3. Fatal crashes

	Intervention after	Intervention before	Control after	Control before	Rate ratio	Log rate ratio	Standard error
All street lighting versus day-time control							
Cornwell 1972	6	27	16	15	0.2083	-1.45686	0.5769
Isebrands 2006	0.5	1.5	0.5	2.5	1.6667	0.5108	2.2509
Seburn 1948	4	9	3	1	0.1481	-1.9095	1.3017

Table 3. Fatal crashes (*Continued*)

Tanner 1958	15	28	17	16	0.5042	-0.6848	0.4730
New street lighting (continuous) versus day-time control							
Cornwell 1972	6	27	16	15	0.2083	-1.45686	0.5769
New street lighting (non-continuous) versus day-time control							
Isebrands 2006	0.5	1.5	0.5	2.5	1.6667	0.5108	2.2509
Pegrum 1972a	1	1	3	2	0.6667	-0.4055	1.6833
Pegrum 1972b	0.5	1.5	1.5	1.5	0.3333	-1.0986	2.0000
Improved street lighting (continuous) versus day-time control							
Seburn 1948	4	9	3	1	0.1481	-1.9095	1.3017
Tanner 1958	298	354	1164	1008	0.7290	-0.3161	0.0896

APPENDICES

Appendix 1. Search strategies

CENTRAL (The Cochrane Library Issue 4, 2008)

#1 MeSH descriptor Lighting explode all trees
#2 (light* or illuminat* or luminair*) and (road* or street* or highway* or freeway*)
#3 Streetlight* or street light* or street-light*
#4 (#1 OR #2 OR #3)
#5 MeSH descriptor Accident Prevention explode all trees
#6 MeSH descriptor Accidents, Traffic explode all trees with qualifier: PC
#7 (accident* or crash* or injur* or fatal* or disabl* or disabil*) and prevent*
#8 (#5 OR #6 OR #7)
#9 (#4 AND #8)

MEDLINE (1966 to October 2008)

1. exp Lighting/
2. ((light* or illuminat* or luminair*) adj5 (road* or street* or highway* or freeway*)).ab,ti.
3. (streetlight* or street-light*).ab,ti.
4. 1 or 3 or 2
5. exp Accident Prevention/
6. exp Accidents, Traffic/pc [Prevention & Control]
7. ((accident* or crash* or injur* or fatal* or disabl* or disabil*) adj5 prevent*).ab,ti.
8. 5 or 6 or 7
9. 8 and 4

EMBASE (1980 to October 2008)

1. exp illumination/
2. ((light* or illuminat* or luminair*) adj5 (road* or street* or highway* or freeway*)).ab,ti.
3. (streetlight* or street-light*).ab,ti.
4. 2 or 3 or 1
5. exp accident prevention/
6. exp Traffic Accident/
7. ((accident* or crash* or injur* or fatal* or disabl* or disabil*) adj5 prevent*).ab,ti.
8. 7 or 5 or 6
9. 8 and 4

TRANSPORT 1988 to July 2007

#1 STREET-LIGHTING
#2 road*
#3 street*
#4 highway*
#5 freeway*
#6 road* or street* or highway* or freeway*
#7 (#6 in ti) or (#6 in ab)
#8 light*
#9 illuminat*
#10 luminair*

#11 light* or illuminat* or luminair*
#12 (#11 in ti) or (#11 in ab)
#13 #1 or (#7 and #12)
#14 streetlight*
#15 street
#16 light*
#17 street-light*
#18 #13 or streetlight* or street light* or street-light*
#19 ACCIDENT-PREVENTION
#20 accident*
#21 crash*
#22 injur*
#23 fatal*
#24 disabl*
#25 disabil*
#26 accident* or crash* or injur* or fatal* or disabl* or disabil*
#27 (#26 in ti) or (#26 in ab)
#28 prevent*
#29 prevent*
#30 (prevent* in ti) or (prevent* in ab)
#31 #19 or (#27 and #30)
#32 #13 and #31

Cochrane Injuries Group SR (searched 16 October 2008)

(streetlight* or "street light*" or street-light* or (road* or street* or highway* or freeway* and light* or illuminat* or luminair*)) and (accident* or crash* or injur* or fatal* or disabl* or disabil* and (prevent*))

FEEDBACK

Response from PR Marchant, 13 May 2009

Summary

I feel some response is necessary concerning this review as there are considerable problems in accepting the review as it stands. There is a danger that this review might be read as 'street lighting will have a substantial beneficial effect in reducing injuries', in spite of some cautionary words within the review.

The data do not arise from randomised controlled trials and one has to wonder how comparable the comparators are to the treatment areas. It is not possible to judge from the data given. One notices that the counts in the areas being compared are sometimes very different. There is also likely to be specific bias in that the areas are given lighting because of concerns about a stretch of road having a poor accident record. This lack of equivalence between areas is likely to generate a spurious seemingly beneficial effect, purely by chance, due to Regression towards the Mean (Marchant 2008, Baxter and Marchant 2009).

Another major effect is that of dissemination bias (A more general form of publication bias).This is a problem leading to an incomplete and unrepresentative sample of trial results being available for meta-analysis. This is even a problem for conventional trials in health care, where there is usually an intention to produce a written output at the inception of a trial. Matters become more problematic with results from 'trials' in policy, especially when the intervention is regarded as routine. Policy implementations are regrettably not as yet routinely set out as scientific experiments. In policy 'trials' it may be that there is no intention to report prior to the implementation and the decision to publish is made later. This decision is likely to be heavily influenced by the result; one can imagine a number of pressures generating publication in the case of 'good news'. A trial register for policy trials would help reduce dissemination bias.

The results of this review are different from the conclusions of the UK Highways Agency, which manages the UK's strategic road network, based on its research on its roads to assess the value of lighting. The Agency gave revised guidance in March 2007 (Chief Highway Engineer Memorandums 189/07 and 190/07) downplaying the importance of lighting as a cost effective safety measure,. This was after years of implementing lighting based on accepting a 30% accident reduction benefit for lighting.
There are statistical issues which need to be recognised in work of this nature. While it is true that running a random effects meta analysis, as the authors do, will 'get a result' in that it will 'fix' a large heterogeneity statistic, it can not distinguish between heterogeneity of treatment effect in different circumstances and the over dispersion in the Poisson counts. The later may be due to intrinsic factors influencing variation at different sites; this will impact on the weights which should be given to each study in the meta analysis.
Recommendations

- A trial register for policy studies is needed to combat dissemination bias.
- Proper (tamper proof) randomisation needs to be applied in deciding which units are active and which are control, again to counter bias.
- Collecting data from multiple time periods before and after will help separate heterogeneity of effect between studies and variation within areas.

Without such improvements it seems that reviews of studies are likely to be seriously misleading and could lead to expensive false 'solutions' being implemented. False solutions will not help alleviate serious problems.
I state for the purposes of transparency that my interest in this matter is sparked by concerns about the environmental impacts of exterior artificial lighting, see Marchant 2007.
I thank Fiona Beyer for clarifying some issues for me via personal communication.
References
Marchant PR (2008) 'Regression towards the mean', entry for the International Encyclopedia of the Social Sciences, 2nd edition, W. Darity (ed.), Macmillan Reference USA (Thomson Gale), New York
Baxter PD and Marchant PR, (2009) The cross product ratio in the bivariate log normal distribution and gamma distribution, with an application to non-randomised trials. Accepted to appear in the Journal of Applied Statistics.
Marchant PR (2007) Are the claims for lighting benefit true? How can we tell? Chapter 7 of the Proceedings of the 6th European Dark-Skies Symposium 2006 Sept 15-16 Portsmouth UK http://www.britastro.org/dark?skies/cfds2006/proceedings.pdf

Reply

The review authors have responded to each of Paul Marchant's comments in turn.
I feel some response is necessary concerning this review as there are considerable problems in accepting the review as it stands. There is a danger that this review might be read as "street lighting will have a substantial beneficial effect in reducing injuries", in spite of some cautionary words within the review.
We agree that some clarification would benefit the review. We are grateful to Dr Marchant for his comments and have made alterations as described below. However, we disagree that the review is fundamentally flawed, for the following reasons:
This is a high-quality systematic review conducted using gold-standard methodology. We defined a priori the levels of evidence that would be included in the review (randomised controlled trials (RCTs) and controlled before-after studies (CBAs)), and searched exhaustively for appropriate studies to include. Our conclusion ("street lighting may prevent road traffic crashes, injuries and fatalities") follows from the statistically significant results reported in the analysis.
We believe that there is a danger of inappropriately conflating weaknesses in the methodology of the included studies (which have been discussed in the review) with weaknesses in the review methods.
In the absence of good quality studies, there is a risk of implementing ineffective interventions. Our review has highlighted this absence of good-quality evidence and we hope that its publication might help rectify this problem.
The data do not arise from randomised controlled trials and one has to wonder how comparable the comparators are to the treatment areas. It is not possible to judge from the data given. One notices that the counts in the areas being compared are sometimes very different.
Given that our exhaustive search revealed no RCTs of street lighting with injury/fatality outcomes, we used a "next best" study design and included only CBAs. These are acknowledged to be methodologically less robust than randomised controlled trials, but are the most robust available to make any assessment of the impact of street lighting on injuries and fatalities. We assessed the risk of bias of the studies as best we could by collecting information about whether the control and intervention areas were matched, the length of time over which data was collected, and the source of the outcome data (whether it was collected independently of the study). The counts in the areas compared are likely to be different if a daytime control is being used, but the absolute difference between intervention and control arms is less important than the ratio within each arm of the before count compared to the after count.

There is also likely to be specific bias in that the areas are given lighting because of concerns about a stretch of road having a poor accident record. This lack of equivalence between areas is likely to generate a spurious seemingly beneficial effect, purely by chance, due to Regression towards the Mean (Marchant 2008, Baxter and Marchant 2009).

We agree that this is an issue. We omitted to include an explanation of regression towards the mean (despite having discussed it before publication) - this has been added to the discussion section.

Another major effect is that of dissemination bias (A more general form of publication bias).This is a problem leading to an incomplete and unrepresentative sample of trial results being available for meta-analysis. This is even a problem for conventional trials in health care, where there is usually an intention to produce a written output at the inception of a trial. Matters become more problematic with results from trials in policy, especially when the intervention is regarded as routine. Policy implementations are regrettably not as yet routinely set out as scientific experiments. In policy trials it may be that there is no intention to report prior to the implementation and the decision to publish is made later. This decision is likely to be heavily influenced by the result; one can imagine a number of pressures generating publication in the case of good news. A trial register for policy trials would help reduce dissemination bias.

We agree that dissemination bias is a potential issue in this area, and have added a sentence in the discussion section to this effect. We already discussed publication bias in the discussion section of the review, and we made every effort to overcome this by the extensive nature of our search for evidence.

The results of this review are different from the conclusions of the UK Highways Agency, which manages the UK's strategic road network, based on its research on its roads to assess the value of lighting. The Agency gave revised guidance in March 2007 (Chief Highway Engineer Memorandums189/07 and 190/07) downplaying the importance of lighting as a cost effective safety measure. This was after years of implementing lighting based on accepting a 30% accident reduction benefit for lighting.

Whereas the conclusions of our review are based on all existing RCTs and CBAs, the conclusions of the UK Highways Agency (HA) are based on two observational studies. The HA acknowledged in this work that a controlled before/after study would be the optimum in terms of methodology, but resource and time constraints precluded this. We were unable to include these two studies in our review because they do not satisfy the methodological inclusion criteria (i.e. they are not RCTs or CBAs). The first study compares the proportions of personal injury accidents on lit sections of the entire HA network (stratified by road type) compared to unlit sections, and concludes that for the period 1994-2004 10% fewer accidents occurred on lit sections of motorway in the dark compared to unlit sections. The second is a matched case control study, where 30 lit/unlit pairs of similar motorway sections were identified and the number of non-junction accidents occurring in the period 1994-2004 was noted. No statistically significant difference in personal injury between lit and unlit sections in the dark was detected in any of the matched pairs. We have added the HA studies to our list of excluded studies to add transparency about why they were excluded.

There are statistical issues which need to be recognised in work of this nature. While it is true that running a random effects meta-analysis, as the authors do, will get a result in that it will fix a large heterogeneity statistic, it cannot distinguish between heterogeneity of treatment effect in different circumstances and the over dispersion in the Poisson counts. The later may be due to intrinsic factors influencing variation at different sites; this will impact on the weights which should be given to each study in the meta-analysis.

We agree that a simple random effects meta-analysis cannot determine sources of the heterogeneity. However our primary aim in this analysis is to estimate relative risks with confidence intervals that allow for this heterogeneity, whatever its cause. We disagree that there is one correct weighting to use in a meta-analysis of this type. We would argue that the weighting used in a random effects analysis is a reasonable choice and that the confidence intervals we obtain do appropriately allow for heterogeneity in relative risks. We also point that there was little evidence of substantial heterogeneity in our analyses and hence that our results are likely to be robust to choice of analytical method.

Contributors

Fiona Beyer and Katharine Ker

Response from PR Marchant, 19 May 2010

Summary

Introduction

There is a need to respond further to the updated review 'Street Lighting for preventing road traffic injuries' by Fiona R Beyer & Katherine Ker published 17 Feb. 2010.

The authors acknowledge some of the concerns express in my earlier comments but they do not go far enough.

Note the website 'Understanding Uncertainty' understandinguncertainty.org/node/231 associated with David Spiegelhalter (FRS, Prof. of Public Understanding of Risk) and his team give, independently, criticisms very similar to mine.

The problems with the review are severe because of the nature of the studies used. We have no idea how well these studies, i.e. those that have generated a written report and therefore get included in the review, represent the situation in its totality. Also we do not know how much worse studies of the type used are, compared with good RCTs, i.e. ones having protocols and other protection against bias. Indeed the authors rightly state 'The risk of bias in these studies was judged to be high'. They then go on to state, wrongly, that the results (from positive meta-analysis findings) suggest that lighting may be beneficial. Indeed the plain language summary states 'the results indicate that street lighting can prevent road traffic crashes, injuries and fatalities'. (Remember we are discussing the average effect). Lighting might be beneficial, on average, but also it might not and indeed it could make things substantially worse because the quality of these studies means we have no real information. The high risk of bias within the studies means that the naive interpretation of results is suspect. Indeed the high risk of bias implies that the correction conclusion may, in fact, be different from the one the authors have given. Indeed a result indicating a poor safety benefit would be of considerable interest.

The authors go on to say that well designed studies are needed to determine the effectiveness of street lighting in middle and low income countries. I say well designed (and executed) studies are needed to determine the effectiveness of street lighting in all types of country. The studies used in the review are from high income countries but as is clear the existing studies are unreliable and potentially biased. I also would argue that well designed (and executed) epidemiological studies need to be undertaken using unbiased selection, when lighting is changed. Change of lighting is happening in many local authority areas in the UK at the present time at the cost of billions of pounds. Using the recorded accident data (STATS19) for a sufficient period before and after the change can indicate whether there has been a substantial overall benefit. I would argue that this must be done at the earliest opportunity to ensure that the public is getting value.

The authors also refer to 'improved lighting'. Improved is an unscientific term, that is used in marketing. It is imprecise. The authors probably mean brighter when it replaces existing lighting (and of course any lighting when compared with none). Improved, in other circumstances, could mean having well-shielded lighting, which would reduce glare. (This is an issue for the American Medical Association's resolution 516 against light pollution). Indeed having more lighting might not be an improvement in overall terms, when environmental impacts are considered. See the report of the Royal Commission on Environmental Pollution 'Artificial Light in the Environment' Nov. 2009. www.rcep.org.uk/reports/sr?2009?light/documents/RCEPˆartificiallight.pdf

In the 'discussion' section on regression towards the mean one should note that the effect is likely to be particularly strong if the reason the lights were installed was because the road had an unexpectedly high accident rate. In the 'implications for practice' section the review cautions against reducing lighting; 'in the UK an increasing number of local councils are turning off public street lighting in certain areas'. One should note that there might be an increase in the number of road traffic accidents recorded, when lights are turned off, even if lighting has no benefit in safety terms, simply due to regression towards the mean. The reason for this is that because, as the authors state, it is 'widely accepted that street lighting has a safety function' means that through the nervousness of local councils, switch offs only occur in places that are deemed to be particularly safe. My understanding is that the switch-offs have not had bad safety impacts, such as in the Essex County Council trial. Such trials are continuing and being initiated in other areas.

The big problem with the studies in the review is that insufficient is known about them. We don't know in general if there was an intention to produce a report before collecting data. Are the protocols of all the studies available? It would be interesting to know the sources of any funding for the studies as there is the phenomenon that results tend to be produced which the paymaster would welcome. Corporate influence can be problematic, see for example 'Science and the Corporate Agenda' www.sgr.org.uk/SciencePolicy/SGRˆcorpˆscienceˆfull.pdf

Other corrections to note:

1. On Page 5 'One author (PP)', but Philip Pond is not named as an author; he is listed in the Acknowledgements.
2. One reference given previously has now been published. The correct attribution is Baxter PD and Marchant PR (2010) 'The cross product ratio in the bivariate log normal distribution and gamma distribution, with an application to non-randomised trials', Journal of Applied Statistics pp1-8.

The conclusion of the systematic review must surely be that better evidence is needed before any substantive effect of lighting can be determined. The danger of allowing the current conclusion to stand is that new, adequate studies will not be done to provide the evidence so desperately needed.

Below I give annotated comments to the authors' previous reply.

Reply --

The review authors have responded to each of Paul Marchant's comments in turn.

I feel some response is necessary concerning this review as there are considerable problems in accepting the review as it stands. There is a danger that this review might be read as 'street lighting will have a substantial beneficial effect in reducing injuries', in spite of some cautionary words within the review. We agree that some clarification would benefit the review. We are grateful to Dr Marchant for his comments and have made alterations as described below. However, we disagree that the review is fundamentally flawed, for the following reasons:

This is a high-quality systematic review conducted using gold-standard methodology. We defined a priori the levels of evidence that would be included in the review (randomised controlled trials (RCTs) and controlled before-after studies (CBAs)), and searched exhaustively for appropriate studies to include. Our conclusion ('street lighting may prevent road traffic crashes, injuries and fatalities') follows from the statistically significant results reported in the analysis.

PM: Statistical significance means nothing if the data is unreliable.

We believe that there is a danger of inappropriately conflating weaknesses in the methodology of the included studies (which have been discussed in the review) with weaknesses in the review methods.

The reason that one has criteria for study inclusion is because this affects the outcome. The reason one does a systematic review is to find the underlying scientific truth of the real situation. When the review is based on studies which are potentially biased by a large unknown amount the outcome will be potentially biased by a large unknown amount and this needs to be stated strongly up front and in the conclusion. This means we cannot tell anything useful substantively except we need to get some evidence that can be trusted.

In the absence of good quality studies, there is a risk of implementing ineffective interventions. Our review has highlighted this absence of good-quality evidence and we hope that its publication might help rectify this problem.

PM: This is naive. My view is the headline conclusion is likely to be taken at face value, as regrettably most people and particularly salespersons are woefully ignorant of the subtleties of scientific methods and statistics. This danger is not reduced by a somewhat simplistic podcast & summary. Although the summary correctly states 'The risk of bias in these studies was judged to be high', it ought to go further to state explicitly that the result of the meta-analysis therefore also suffers from the risk of high bias.

The data do not arise from randomised controlled trials and one has to wonder how comparable the comparators are to the treatment areas. It is not possible to judge from the data given. One notices that the counts in the areas being compared are sometimes very different.

Given that our exhaustive search revealed no RCTs of street lighting with injury/fatality outcomes, we used a 'next best' study design and included only CBAs. These are acknowledged to be methodologically less robust than randomised controlled trials, but are the most robust available to make any assessment of the impact of street lighting on injuries and fatalities. We assessed the risk of bias of the studies as best we could by collecting information about whether the control and intervention areas were matched, the length of time over which data was collected, and the source of the outcome data (whether it was collected independently of the study). The counts in the areas compared are likely to be different if a daytime control is being used, but the absolute difference between intervention and control arms is less important than the ratio within each arm of the before count compared to the after count.

PM: The issue is quantitatively how much worse are CBA studies than RCTs. This is the issue rather than asserting that CBAs are better than just Before-After studies without any comparator / 'control'. Clearly the estimate of effect from a CBA will be less well-determined than that of an RCT of equivalent size-but how different is it likely to be? Assuming there to be exactly zero difference, as the authors effectively do, is 'optimistic'.

There is also likely to be specific bias in that the areas are given lighting because of concerns about a stretch of road having a poor accident record. This lack of equivalence between areas is likely to generate a spurious seemingly beneficial effect, purely by chance, due to Regression towards the Mean (Marchant 2008, Baxter and Marchant 2009' This should be 2010, Journal of Applied Statistics pp1-8).

We agree that this is an issue. We omitted to include an explanation of regression towards the mean (despite having discussed it before publication) - this has been added to the discussion section.

PM: This addition is welcome.

Another major effect is that of dissemination bias (A more general form of publication bias). This is a problem leading to an incomplete and unrepresentative sample of trial results being available for meta-analysis. This is even a problem for conventional trials in health care, where there is usually an intention to produce a written output at the inception of a trial. Matters become more problematic with results from trials in policy, especially when the intervention is regarded as routine. Policy implementations are regrettably not as yet routinely set out as scientific experiments. In policy trials it may be that there is no intention to report prior to the implementation

and the decision to publish is made later. This decision is likely to be heavily influenced by the result; one can imagine a number of pressures generating publication in the case of good news. A trial register for policy trials would help reduce dissemination bias.
We agree that dissemination bias is a potential issue in this area, and have added a sentence in the discussion section to this effect. We already discussed publication bias in the discussion section of the review, and we made every effort to overcome this by the extensive nature of our search for evidence.
PM: But if the negative studies are just not written up, extensive searching can not find them. 'Note there is a strong possibility that such studies are missing because of their 'uninteresting' or 'unwelcome' findings'; see 16.1.1 V5 Cochrane Handbook. Such absence may be associated with the sponsor.
Former comment from PM: The results of this review are different from the conclusions of the UK Highways Agency, which manages the UK's strategic road network, based on its research on its roads to assess the value of lighting. The Agency gave revised guidance in March 2007 (Chief Highway Engineer Memorandums189/07 and 190/07) downplaying the importance of lighting as a cost effective safety measure. This was after years of implementing lighting based on accepting a 30% accident reduction benefit for lighting.
Former comment from authors: Whereas the conclusions of our review are based on all existing RCTs and CBAs, the conclusions of the UK Highways Agency (HA) are based on two observational studies. The HA acknowledged in this work that a controlled before/after study would be the optimum in terms of methodology, but resource and time constraints precluded this. We were unable to include these two studies in our review because they do not satisfy the methodological inclusion criteria (i.e. they are not RCTs or CBAs). The first study compares the proportions of personal injury accidents on lit sections of the entire HA network (stratified by road type) compared to unlit sections, and concludes that for the period 1994?2004 10% fewer accidents occurred on lit sections of motorway in the dark compared to unlit sections. The second is a matched case control study, where 30 lit/unlit pairs of similar motorway sections were identified and the number of non?junction accidents occurring in the period 1994?2004 was noted. No statistically significant difference in personal injury between lit and unlit sections in the dark was detected in any of the matched pairs. We have added the HA studies to our list of excluded studies to add transparency about why they were excluded.
PM: It is of course right that the inclusion criteria are respected. However, it is worth taking note of the fact that the Highways Agency has little motive to produce biased work. It presumably wants to maximise safety at minimum cost using any means available. In order to obtain the correct understanding it is important not to rely solely on trials but to consider other relevant evidence such as the results of data collected when schemes have been implemented, as pointed out above in connection with the possibilities arising from the large scale changes in UK street lighting.
Former comment from PM: There are statistical issues which need to be recognised in work of this nature. While it is true that running a random effects meta-analysis, as the authors do, will get a result in that it will fix a large heterogeneity statistic, it cannot distinguish between heterogeneity of treatment effect in different circumstances and the over dispersion in the Poisson counts. The latter may be due to intrinsic factors influencing variation at different sites; this will impact on the weights which should be given to each study in the meta-analysis.
Former comment from authors: We agree that a simple random effects meta-analysis cannot determine sources of the heterogeneity. However our primary aim in this analysis is to estimate relative risks with confidence intervals that allow for this heterogeneity, whatever its cause. We disagree that there is one correct weighting to use in a meta-analysis of this type. We would argue that the weighting used in a random effects analysis is a reasonable choice and that the confidence intervals we obtain do appropriately allow for heterogeneity in relative risks. We also point that there was little evidence of substantial heterogeneity in our analyses and hence that our results are likely to be robust to choice of analytical method.
PM: The point remains that we are uncertain of the weights that each study should be given when entering the meta-analysis and this will lead to uncertainty in the effect size.
PM: Some Quotations:
'If we manage to find a selection process that is reasonably plausible in the context of the topic being reviewed but which undermines the result that has been claimed then that result must surely be viewed with considerable caution, however impressive the analysis leading to it might appear to be.' John Copas in the Royal Statistical Society's publication Significance Dec. 2005
'...you should report everything that you think might make it invalid - not only what you think is right about it: other causes that could possibly explain your result' in Cargo Cult Science by Richard Feynman
'Meta-analyst; one who thinks that if enough manure is piled high enough it will smell of roses.' In 'Dicing with Death' by Stephen Senn
Final Conclusion
So I conclude: to meta-analyse when the original studies are seriously problematic is wrong. The systematic review should be left without meta-analysis and simply have a call for sound studies.

Reply

We would like to thank Dr Marchant for his continuing interest in our systematic review.

We have previously answered his comments about publication bias and regression to the mean. We would like to add the following:

Risk of bias: although the CBAs included in this review would tend to have a higher risk of bias than RCTs, we would reiterate that they are the best type of study available to answer the question about the impact of street lighting on injuries. In two of the three specific areas for which we assessed risk of bias (matching of control/intervention areas and source of outcome data), it was judged to be low. Even in the absence of pooling the data, the forest plots demonstrate a tendency to a protective effect. Our conclusion that street lighting *may* prevent road traffic crashes, injuries and fatalities (with several caveats in the review about potential bias), and our call for better-designed studies, follows from the data.

Location of further work: the reason we have mentioned lower and middle income countries (LMCs) in our recommendations is that no RCTs or CBAs have been carried out in LMCs. The road environment is often very different in LMCs with vulnerable road users not separated from traffic in the same way as they tend to be in higher income countries (HMCs). Moreover, many LMICs are at comparatively low levels of motorisation and the risk of road crashes in these countries can only be expected to increase with greater motorisation, it therefore makes them a worthy setting for future research into all road safety interventions.

In the absence of any studies at all in LMCs, we have suggested that "additional well designed controlled trials are needed to estimate effects accurately, with a focus on effectiveness in middle and lower-income countries".

Improved lighting: we acknowledge that improved lighting may be subject to different definitions. We have reported this term as used in the studies. Since they investigated the impact of lighting on injuries, in this context improved means brighter.

Authorship: because Cochrane reviews are dynamic documents which are regularly updated, an author who carried out early work for a review is sometimes removed (with their agreement) from the authorship list on later iterations, when they are no longer able to contribute to the review. In this case, Philip Pond was an author early on in the life of the review, but has now moved on to other things. We have removed the references to specific authors in order to avoid confusion.

The correct attribution for the paper on regression to the mean is as follows:
Baxter, Paul D. and Marchant, Paul R.(2010) 'The cross-product ratio in bivariate lognormal and gamma distributions, with an application to non-randomized trials', Journal of Applied Statistics, 37: 4, 529 - 536.

Contributors

Fiona Beyer and Katharine Ker

WHAT'S NEW

Last assessed as up-to-date: 14 October 2008.

Date	Event	Description
22 July 2010	Feedback has been incorporated	The authors' reply to feedback submitted by Dr. Marchant has been included

HISTORY

Protocol first published: Issue 2, 2004

Review first published: Issue 1, 2009

Date	Event	Description
27 May 2010	Feedback has been incorporated	Feedback was submitted by a reader and has been included in the feedback section. The authors' reply to the feedback will appear on the Cochrane Library on 4 August 2010
12 January 2010	Feedback has been incorporated	The authors' reply to feedback has been included in this review
28 January 2009	New search has been performed	One new study was included in this update. The conclusions of the review have not changed
26 May 2005	New citation required and conclusions have changed	The protocol was revised as a consequence of changes to the review team

CONTRIBUTIONS OF AUTHORS

FB: designed the protocol, devised and conducted the searches, obtained reports, extracted and analysed the data and wrote the review.

KK: helped analyse the data and write the review.

DECLARATIONS OF INTEREST

None known.

INDEX TERMS

Medical Subject Headings (MeSH)

Accidents, Traffic [*prevention & control]; Lighting [*methods]; Wounds and Injuries [mortality; *prevention & control]

MeSH check words

Humans