Papers

Effectiveness of speed cameras in preventing road traffic collisions and related casualties: systematic review

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Abstract

Objectives To assess whether speed cameras reduce road traffic collisions and related casualties.
Design Systematic review.
Data sources Cochrane Injuries Group Specialised Register, Cochrane Central Register of Controlled Trials, Medline, Embase, Social Science Citation Index, TRANSPORT database, ZETOC, the internet (including websites of road safety and motoring organisations), and contact with key individuals and organisations.
Main outcome measures Road traffic collisions, injuries, and deaths.
Inclusion criteria Controlled trials and observational studies assessing the impact of fixed or mobile speed cameras on any of the selected outcomes.
Results 14 observational studies met the inclusion criteria; no randomised controlled trials were found. Most studies were before-after studies without controls (n = 8). All but one of the studies showed effectiveness of cameras up to three years or less after their introduction; one study showed sustained longer term effects (4.6 years after introduction). Reductions in outcomes across studies ranged from 5% to 69% for collisions, 12% to 65% for injuries, and 17% to 71% for deaths in the immediate vicinity of camera sites. The reductions over wider geographical areas were of a similar order of magnitude.
Conclusions Existing research consistently shows that speed cameras are an effective intervention in reducing road traffic collisions and related casualties. The level of evidence is relatively poor, however, as most studies did not have satisfactory comparison groups or adequate control for potential confounders. Controlled introduction of speed cameras with careful data collection may offer improved evidence of their effectiveness in the future.

Introduction

Road traffic collisions are an important cause of death and disability worldwide. Every year around the world 1.2 million people are killed and up to 50 million are injured or disabled as a result of road traffic collisions.[1] Morbidity from road traffic collisions is expected to increase in future years, and it is estimated that road traffic collisions will move from ninth to third place in the global burden of disease ranking, as measured in disability adjusted life years.[2 3]

Measures to reduce traffic speed are considered essential to reducing casualties on the road.[1 4 5] Speed cameras are increasingly used to help to reduce traffic speeds in the belief that this will reduce road traffic collisions and casualties, and an expansion in the use of speed cameras is under way in many countries, most notably the United Kingdom.[6] The use of speed cameras is controversial, however. Vociferous opponents, including some motoring associated organisations, oppose their use, and cameras are often criticised in the media.[7–9] The lack of readily available evidence of the effectiveness of cameras has made it difficult for road safety and health professionals to engage in an informed debate about the effectiveness of speed cameras.

A previous small non-systematic review of six studies found a 17% reduction in collisions after introduction of speed cameras.[10] Non-systematic reviews can, however, be limited by bias. We aimed, therefore, to systematically assess the evidence for the effectiveness of speed cameras in reducing road traffic collisions and related casualties.

Methods

We specified the protocol before undertaking the review, and we made no deviations from the protocol.

Study selection

Controlled trials and observational studies assessing the impact of fixed or mobile speed cameras on any or all of three outcomes (collisions, injuries, and deaths) were eligible for inclusion. We considered all published and unpublished material, with no restrictions on date or language. As the effect of co-intervention is difficult to exclude in interventions such as this, studies that did not have speed cameras as the major intervention were not eligible for inclusion.

Identification of primary studies

We searched the following electronic databases: Medline (1966 to February 2004), Embase (1988 to

An appendix, two tables and a figure are on bmj.com

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February 2004), Cochrane Controlled Trials Register (February 2004), Cochrane Injuries Group Specialised Register (February 2004), Social Science Citation Index (1981 to February 2004), TRANSPORT database (1988 to February 2004), and ZETOC British Library database (February 2004). The Medline and Embase search strategies were of the general structure "Intervention synonyms" AND "Outcome synonyms" AND "Study methodology synonyms" (see appendix on bmj.com for terms used). We translated the Medline search strategy into comparable search strategies for other databases. We then searched the bibliographies of studies identified by electronic searches to identify additional studies. We searched the internet by using the Google search engine (February 2004) (see appendix for terms used). We also searched the websites of road safety and motoring organisations (see appendix for list of websites searched). Key individuals and organisations contacted included every police force in England and Wales, the Faculty of Public Health Transport and Health e-group, several road safety organisations, and key experts in the field.

Data extraction and analysis

One reviewer (PP) searched for studies by using the search strategy outlined earlier and, together with a second reviewer (SK), selected studies to obtain for possible inclusion in the review, on the basis of titles and abstracts (where available). Both reviewers then independently extracted data from each study by using a data extraction form that was piloted before use. We extracted details of the study design, aim of the study, setting of the study and nature of the roads, study period, measurement of exposure, outcome and relevant confounders, and results. We also assessed the quality of the studies with a predefined quality scale, which, in the absence of pre-existing scales, we developed and piloted ourselves. The quality scale rated studies on the basis of representativeness of study areas to general population; control areas being representative of intervention areas; objective and valid outcome(s); results provided with estimates of uncertainty; main conclusions based on primary study hypotheses; and important confounders measured and controlled for. For each of the six quality criteria, we rated the studies on a three point scale (0-2). We rated studies scoring a total of 9-12 as good quality, 6-8 as average quality, and 0-5 as poor quality (see appendix for full data extraction form and quality scale). We combined the results from the data extraction forms of the two reviewers and made decisions on inclusion in the review. We resolved disagreements by consensus.

We also extracted data on the actual number of cases in the intervention and control areas for each time period and where appropriate combined them to produce summary statistics. We calculated risk ratios with confidence intervals for before-after and experimental-control comparisons where possible.

Meta-analysis

Owing to the differing nature of the studies, we decided that meta-analysis would not be appropriate. The camera operations evaluated in the studies differed in terms of the nature of camera operation (types of cameras used, intensity of camera usage, nature of punishments for motorists caught speeding). Studies also used a range of outcome measures to assess camera effectiveness and assessed these outcomes over varying time periods. Risk ratios could not be calculated for five out of 14 studies owing to lack of relevant data. We could not use funnel plot analysis to test for publication bias for the same reasons.

Results

We selected 92 studies to review, on the basis of the title or abstract of the report. After reviewing the full articles, we identified 21 studies that were potentially suitable for inclusion. Of these, two studies did not consider the intervention or outcome of interest,[11 12] one study reported only secondary results without details of the methods,[13] two studies did not look at the effectiveness of the introduction of cameras,[14 15] and two studies were preliminary reports that were updated in later publications.[16 17] After excluding these studies, we included 14 studies in the final review (see figure on bmj.com).

All the studies were observational studies; we found no randomised controlled trials. Five studies had control areas distinct from the areas where the cameras were introduced.[18–22] One study used the same areas at times when cameras were not operating as a control,[23] and eight studies used the same areas before introduction of cameras as the comparison group (before-after studies).[10 24–30] The studies were published between 1992 and 2003. All studies were in high income countries. Six studies assessed the effect of fixed cameras,[10 18–20 29 30] four studied the effect of mobile cameras,[21–23 26] and four studied the effect of a combination of fixed and mobile cameras.[24–28] Outcome measures in the studies were diverse and included various measures of collisions, deaths, and injuries. Three studies had a follow up period of one year following the introduction of cameras,[22 26 29] nine studies had a follow up period of one to three years,[18–21 23–25 27 28] and one study had a follow up period of four years.[10] One study stated only that follow up data of at least one year were used.[30] See table A on bmj.com for details of the studies. In terms of methodological quality, we classified no studies as being good quality, seven as average, two as average-poor, and five as poor.

All studies reported a reduction in road traffic collisions and casualties. The reduction in adverse outcomes in the immediate vicinity of camera sites varied considerably across studies, with ranges of 5-69% for collisions, 12-65% for injuries, and 17-71% for deaths at camera sites. Smaller reductions in adverse outcomes were seen over a wider area. See table B on bmj.com for full results.

Discussion

Research conducted so far consistently shows that speed cameras are an effective intervention in reducing road traffic collisions and related casualties. The level of evidence is relatively poor, however, as most studies did not have satisfactory comparison groups or adequate control for potential confounders.

Strengths and weaknesses of the review

This is the first systematic review on the effectiveness of speed cameras. The main strengths of this review are the thoroughness of the search carried out to find relevant publications and the independent extraction of data by the reviewers.

Despite our best efforts we may not have been able to identify all relevant publications, as road safety research is often published as reports and other forms of grey literature. However, owing to the highly controversial nature of the debate about speed cameras in high income countries, we would expect any published negative studies to be highly publicised.

Although it is plausible that findings could have been withheld from publication, we could not test formally for publication bias because of the varied nature of study designs and outcome measures used. Studies (positive or negative) from low income and middle income countries were notably absent. We are unclear whether this represents a lack of research from such countries or their unavailability in published form. We were unable to pool the results and arrive at a summary estimate owing to the multiplicity of interventions, study designs, and outcomes (often lacking explicit case definitions).

Road safety interventions are often multifaceted. Introduction of speed cameras may have been accompanied by other road safety initiatives such as traffic calming and education campaigns against speed and drink driving. Temporal changes such as improvements in car safety, changes in traffic volume, trends in drink driving, and changes in risk taking behaviour can also influence the frequency of road traffic collisions. Speed cameras are generally introduced at sites identified on the basis of high rates of speed related collisions. However, as a rise in traffic collisions could be due to chance, any subsequent reduction could merely be indicative of normal variation ("regression to the mean").[31] All these factors could result in an underestimate or overestimate of the effectiveness of cameras, and most studies only controlled for a few of these factors, if any.

Implications of the research

This review has highlighted the limited nature of the evidence base underpinning the large scale introduction of speed cameras and the need for further robust evidence. Two possibilities exist for improving this evidence base. Randomised controlled trials offer the highest form of evidence. In countries where a large scale introduction of speed cameras is planned and the subject is not politicised, speed cameras could be introduced in a controlled fashion, randomising the allocation of cameras within a larger sampling framework of high risk sites (with remaining sites serving as controls). However, this approach may not be feasible in most settings because of political and other local pressures. In such settings, an alternative may be to carry out any planned introduction of speed cameras in a phased manner spread over a few years with careful collection of data on collisions and injuries, hence producing a natural comparison group (wedge shaped study design). In either case, the research needs to be conducted as soon as possible, before the widespread introduction of cameras results in a permanent loss of such opportunities.

This review was limited to studying the effectiveness of the introduction of speed cameras in preventing collisions and injuries. Although some evidence exists to suggest that the effectiveness of speed cameras varies according to type of camera (visible or hidden),[14 15] questions remain about how the effectiveness of cameras is affected by location criteria (restricting cameras to collision black spots or not) and use of educational initiatives alongside enforcement. Speed cameras may also change the culture of speeding over a longer period of time. Further research is needed into how these other factors may influence the effectiveness of speed cameras.

What is already known on this topic

Speed cameras are used increasingly as a means of reducing road traffic collisions and related casualties

Opinions about the effectiveness of speed cameras in reducing road traffic collisions and related casualties are conflicting

What this study adds

Existing research consistently shows that speed cameras are an effective intervention in reducing road traffic collisions and related casualties

However, the level of evidence is relatively poor, and most studies lack adequate comparison groups

Controlled introduction of speed cameras with careful data collection is needed to improve the evidence base for the effectiveness of speed cameras

Conclusion

Published research consistently shows the effectiveness of speed cameras in preventing road traffic collisions and injuries. However, the level of evidence is relatively poor, and better data need to be collected to improve the evidence base.

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Contributors: PP and SK developed the protocol. PP searched for studies and, together with SK, selected studies to obtain. PP and SK independently extracted data from each study. PP collated the data and drafted the report, with input from SK at all stages, including the calculation of relative risks. SG, BY, and LT made comments on the draft report. PP and SK finalised the review. PP is the guarantor.

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Rectal artemether versus intravenous quinine for the treatment of cerebral malaria in children in Uganda: randomised clinical trial

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Abstract

Objective To compare the efficacy and safety of rectal artemether with intravenous quinine in the treatment of cerebral malaria in children.

Design Randomised, single blind, clinical trial.

Setting Acute care unit at Mulago Hospital, Uganda's national referral and teaching hospital in Kampala.

Participants 103 children aged 6 months to 5 years with cerebral malaria.

Intervention Patients were randomised to either intravenous quinine or rectal artemether for seven days.

Main outcome measures Time to clearance of parasites and fever; time to regaining consciousness, starting oral intake, and sitting unaided; and adverse effects.

Results The difference in parasitological and clinical outcomes between rectal artemether and intravenous quinine did not reach significance (parasite clearance time 54.2 (SD 33.6) hours *v* 55.0 (SD 24.3) hours, $P = 0.90$; fever clearance time 33.2 (SD 21.9) hours *v* 24.1(SD 18.9 hours, $P = 0.08$; time to regaining consciousness 30.1 (SD 24.1) hours *v* 22.67 (SD 18.5) hours, $P = 0.10$; time to starting oral intake 37.9 (SD 27.0) hours *v* 30.3 (SD 21.1) hours, $P = 0.14$). Mortality was higher in the quinine group than in the artemether group (10/52 *v* 6/51; relative risk 1.29, 95% confidence interval 0.84 to 2.01). No serious immediate adverse effects occurred.

Conclusion Rectal artemether is effective and well tolerated and could be used as treatment for cerebral malaria.

Introduction

The recommended treatment of cerebral malaria is intravenous quinine, but alternative drugs are necessary where intravenous treatment is not possible. Studies comparing rectal artemether with intravenous quinine have been carried out in adults,[1 2 3] but results were variable, and information in children is limited.[3] A single dose of rectal artesunate has been found to be associated with rapid reduction in parasite density in children and adults with moderately severe malaria.[4]

We compared the efficacy and safety of rectal artemether with that of intravenous quinine in the treatment of children, aged 6 months to 5 years, with cerebral malaria.

Methods

This study was carried out in Mulago Hospital, Uganda's national referral and teaching hospital, from July 2002 to February 2003. We recruited patients from the acute care unit and followed them for seven days.

This is the abridged version; the full version is on bmj.com