WHAT WORKS: CRIME REDUCTION SYSTEMATIC REVIEW SERIES

No 8. SPEED CAMERAS TO REDUCE SPEEDING TRAFFIC AND ROAD TRAFFIC INJURIES

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January 2016
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This research was co-funded by the College of Policing and the Economic and Social Research Council (ESRC); Grant title: 'University Consortium for Evidence-Based Crime Reduction'. Grant Ref: ES/L007223/1.
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Abstract

Background
Exceeding the speed limit is one of the most common criminal offenses committed in the UK and can engender tremendous social harm. Speed limits on roads regulate traffic speeds by establishing a safe upper limit on vehicle speeds. Measures aimed at enforcing traffic speed limits include the use of speed cameras.

Objectives
To update and expand a Cochrane systematic review of traffic speed enforcement cameras.

Search strategy
We searched the following electronic databases: OVID Transport database (1988 to June 2015); National Police Library (to June 2015), Cochrane Injuries Group Specialised Register (to 16/03/2015), Cochrane Library CENTRAL database (to 16/03/2015); Ovid MEDLINE(R), Ovid MEDLINE(R) In-Process & Other Non-Indexed Citations, Ovid MEDLINE(R) Daily and Ovid OLDMEDLINE(R) (1946 to 16th March 2015); Embase Classic+Embase (OvidSP) (1947 to 16th March 2015); ISI WOS: SCI-EXPANDED (1970) & CPCI-S (1990) to 16th March 2015); PROQUEST (to 12/06/2015); EBSCO (to 12/06/2015); Web of Knowledge (to 12/06/2015); Heritage (to 12/06/2015).

Selection criteria
Randomised controlled trials, interrupted time series and controlled before-after studies that assessed the impact of speed cameras on traffic speeding, road crashes, crashes causing injury and fatalities, were eligible for inclusion.

Data collection and analysis
We independently screened studies for inclusion, extracted data from full text reports, assessed methodological quality; we reported study authors’ outcomes and calculated standardised results based on the information available in each study.

Main results
In this updated systematic review an additional 16 evaluations met the inclusion criteria and were added to the 35 studies in the previous Cochrane review. Across both newly and previously identified studies, the implementation of speed camera programmes was associated with a reduction in average speed of 7% (95% CI 0-13%), in percentage of vehicles exceeding the speed limit of 57% (95% CI 50-64%), in crashes of 19% (95% CI 14-24%), in injury crashes of 18% (95% CI 13-23%) and in severe or fatal crashes of 21% (95% CI 13-29%). There was little evidence that the effect of interventions differed by type of speed camera, whether overt or covert, fixed or mobile, or that effects differed in urban and rural areas. However, there was some evidence to suggest that fixed cameras had a slightly greater effect on all road traffic crashes and those resulting in fatalities or severe injuries, than mobile cameras.

Authors’ conclusions
This review provides evidence that speed cameras are an effective intervention for reducing speeding behaviour, and can help combat some of the negative consequences of speeding such as fatalities and injury crashes. Considering continuing increases in traffic volumes, speed cameras appear to be a worthwhile intervention to protect public safety.
Background

Description of the problem
Exceeding the speed limit is one of the most common criminal offenses committed in the UK and can engender tremendous social harm. Consequences of speeding can be severe with both direct and indirect effects on health and wellbeing. There is a direct relationship between speeding vehicles and increased crash risk. Reaction and braking distances are longer at higher speeds which can increase the likelihood of road traffic crashes. Further, vehicles travelling at different speeds (both above and below average speeds) can create more interaction between vehicles which can increase the number of crashes. In 2012 exceeding the speed limit was a contributory factor in 5% of casualties and 12% of fatalities in Great Britain (Department for Transport 2013). Indirectly, fear of speeding vehicles can discourage people from walking and cycling, or discourage parents from allowing their children to play outdoors, or travel on their own.

Speed limits on roads are intended to regulate traffic speeds by establishing a safe upper limit on vehicle speeds. Speed limits are usually assigned by category, type, and design of the road (Chin 1999). Despite the very real consequences of exceeding the speed limit, researchers have argued that speeding has been socially constructed as not a ‘real’ crime (Corbett 2000). British Social Attitudes survey found that 90% of people agreed that motorists should drive within the speed limit (NatCen Social Research 2013). However, according to an RAC survey, over 40% of motorists admit to breaking the speed limits on 20 mph and 30 mph roads in urban areas and on 50 mph and 60 mph roads in rural areas. Sixty-seven percent of motorists admitted to breaking the speed limit of 70 mph on motorways (RAC 2015).

Description of the intervention
Efforts to reduce speeding behaviour have traditionally focused on the “3Es”: Education, Engineering and Enforcement. Educational interventions such as Safe Community Programmes aim to heighten driver awareness of speeding-related safety issues. Engineering interventions, such as speed humps or chicanes, change the road environment to physically slow traffic speeds. Enforcement measures focus on ensuring that the public adhere to the posted speed limits through the automated or manual monitoring of traffic speeds. This review concentrates on the implementation of automatic traffic speed enforcement using fixed and mobile cameras to enforce speed limits.

How the intervention might work
Speed cameras have the potential to reduce speed-related crime by increasing offenders’ perceptions of the risk of being caught and facing consequences if they exceed the speed limit. Successful sanctions (including licence points, driving bans, fines, and driver awareness courses) and prosecution of offenders using evidence collected by speed cameras might also impact on recidivism rates.

The use of speed cameras is often publically divisive: Delaney and colleagues (2005) describe five recurring controversies:

1- Within some jurisdictions speed cameras raise revenue for local governments so there are concerns that in some instances they are implemented for financial reasons.

2- Concerns about fairness, a failure to notify offenders on the spot, and a lack of opportunity to explain circumstances.

3- Speeding is not always perceived as a safety problem (as 1 above).
4- Concerns about accuracy of cameras to detect speed within enforcement tolerances.

5- Concerns that photographic traffic law enforcement is ‘Spying’ on law-abiding citizens (invasion of privacy) and that camera data may be used for other purposes.

Despite these concerns, many countries worldwide have expanded the use of speed cameras over time, based on research evidence that speed cameras reduce both traffic speeds and road traffic crashes. This updated detailed review of the research evidence on speed cameras is therefore timely.

Importance of review

The pandemic of road traffic deaths and injuries continues unabated. Each year almost 1.2 million people die and between 20 and 50 million people are injured or disabled worldwide as a result of road traffic crashes (Peden 2004; WHO 2015). Worldwide, for people under 44 years, road traffic crashes are a leading cause of death and disablement second only to HIV and AIDS (Krug 2002). The continuing advance of motorisation in many developing countries is likely to further exacerbate the problem. It is estimated that by 2020 road traffic crashes will have moved from ninth to third in the world ranking of burden of disease, as measured in disability adjusted life years (Murray 1997). In Britain in 2014 there were 1,775 people killed and 22,807 people seriously injured in road traffic collisions.

Data on fatalities from USA suggests that excessive speed is implicated in about 30% of all fatal crashes (U.S. Department of Transportation 2004). The annual costs of road traffic crashes in developed countries are of the order of 2-3% of GDP (Elvik 2000; Jacobs G 2000). Thus, the identification of effective strategies for the prevention of road traffic injuries is of public health and public finance importance globally.

Braking distances are greater at higher vehicle speeds because greater distances are travelled as a driver reacts to any situation that requires slowing or stopping. Traffic speed limits are thus intended to regulate traffic speeds by establishing an upper speed limit, and to increase road safety by reducing the variability of vehicle speeds. In the event of a collision, higher traffic speeds will result in greater exchanges of energy and momentum, imposing greater forces (and therefore more severe injuries) on the bodies of the people involved (Allsop 2010). Therefore, any reduction in the upper centiles of vehicle speeds should reduce the number of deaths and severe injuries in traffic collisions that do occur. A reduction in the number of drivers who exceed the speed limit should therefore lead to a reduction in the severity of crashes (Pilkington 2002).

The enforcement of traffic speed limits needs to be such that drivers believe that they will be caught if they exceed speed limits; yet police cannot be present on all roads at all times. In many countries there is therefore an increased use of automatic speed enforcement, using speed detection devices such as cameras. Cameras may be manned or unmanned, mobile or fixed, overt or covert. Older speed camera systems used film which meant that the films had to be removed from the camera

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1 In England and Wales, the law does not require drivers to be warned about the presence of road safety cameras for the enforcement of offences but best practice guidelines published by the Department for Transport advocate visible enforcement to give drivers maximum opportunity to comply. The guidelines advise that camera signs should be co-located with speed limit signs and enforcement should be overt (with minimum visibility distances) for both fixed (including yellow camera housing, warning signs within 1km of housing and visibility) and mobile speed cameras (including marked vehicles and operators in high visibility clothing with signs at intervals of around 1km throughout enforcement area). The guidance does not restrict police discretion to operate covertly.
units to be developed. Modern systems use digital and video cameras and are able to transmit information over data networks. Speed cameras nowadays use low-powered Doppler radar speed sensors to trigger the camera to capture an image of any vehicle (and its number plate) travelling above a pre-set speed. Automated speed enforcement cameras may also monitor the average speed of vehicles over a stretch of road using several measurements (Decina 2007).

A previous systematic review for the Cochrane Collaboration assessed the effectiveness of speed cameras for prevention of road traffic injuries and deaths in 2010 (Wilson et al., 2010). The review found that after implementation of speed cameras, the relative reduction in average speed ranged from 1-15% in the 35 studies included in the review; the reduction of proportion of speeding vehicles ranged from 14-65%; and the reduction in road traffic crashes ranged from 8-49%. The authors chose not to conduct a meta-analysis of results due to heterogeneity between and within the included studies. The aim of this review was to update and expand the Cochrane systematic review of traffic speed enforcement cameras, and to explore under which circumstances speed cameras may, or may not work, and to assess whether any effects may differ by type of device (i.e. covert versus overt, fixed versus mobile cameras). We also aimed to investigate the time over which any effects may be seen after cameras are removed (‘time-halo’) and the distance from the sites of speed cameras that any effects may be seen (‘distance-halo’).

EMMIE Framework
This systematic review was conducted in support of the What Works Centre for Crime Reduction, hosted by the UK College of Policing. One aim of the UK College of Policing is to promote and facilitate evidence-based policing, defined as: “a method of making decisions about ‘what works’ in policing: which practices and strategies accomplish police missions most cost-effectively” (Sherman, 2013, p.377). Systematic reviews of the research literature lie at the heart of the ‘what works’ movement. The results of this review will be incorporated in an online toolkit devised by researchers at the UCL Jill Dando Institute of Security and Crime Science. To help structure the toolkit, the consortium produced the ‘EMMIE’ framework for assessing 5 dimensions of evidence (Johnson 2015; Johnson et al. 2015). The EMMIE acronym refers to:

- Effect size (how effective is the intervention?)
- Mechanism (how does the intervention work?)
- Moderators (in which contexts does the intervention work?)
- Implementation (what is needed to implement the intervention?)
- Economics (how much might the intervention cost?).

Objectives
Our objective was to update and expand a Cochrane systematic review to provide a comprehensive account of automatic speed enforcement devices evaluated worldwide. The update included studies published after 2010 (the date of the last Cochrane update) and has been expanded by including information under the EMMIE framework (see above) on mechanisms, moderators, implementation and economic costs of speed camera interventions. For each study we have described the setting (e.g., nature of roads), theoretical basis for the intervention, characteristics (i.e., mobile or fixed; manned or unmanned; covert or overt), and outcomes (including traffic law violations). Where sufficient numbers of well-designed controlled evaluations were identified, we have included estimates of the effect of interventions on the defined primary outcome (speeding vehicles) and secondary outcomes (e.g., road traffic crashes) to assess the effectiveness of interventions. We have
also sought to investigate potential moderators of intervention effects, and to summarise the different aspects of implementation of speed enforcement devices and their respective costs.

Methods

Criteria for considering studies for this review

Types of studies

Broad inclusion criteria were used for considering studies, in order to include programmes that have undergone controlled evaluation, as well as those that have been assessed descriptively or qualitatively. Studies were included for evidence of effectiveness if they used any of the following research designs:

- randomised controlled trials (RCTs, including clustered RCTs and quasi-RCTs);
- controlled before-after (CBA) studies;
- interrupted time series (ITS) studies.

The definition of CBA and ITS designs are based on that used by the Cochrane Effective Practice and Organisation of Care group (EPOC) as follows:

- CBA: A design where there is contemporaneous data collection before and after the intervention and an appropriate control site or activity;
- ITS: A design where there is a clearly defined point in time when the intervention occurred and at least three data points before and three after the intervention.

Types of participants

Inclusion criteria:

- Roads subject to any type of automated or semi-automated speed enforcement intervention.

Exclusion criteria:

- Interventions using red light traffic signal cameras at signalized junctions.

Types of interventions

This review covers all types of automated or semi-automated speed enforcement measures. This includes speed cameras (photo radar), laser and other radar devices, as well as ancillary equipment such as road embedded electromagnetic loops. These interventions are further categorised as manned or unmanned cameras, mobile or fixed, overt or covert, or combinations of these. This review does not include red light cameras at signalised junctions which is addressed by another systematic review in the ‘What Works?’ series.

Types of outcome measures

Primary outcome measures

The primary outcome variables are the percentage of drivers who exceed the speed limit (or a designated speed threshold), and the average speeds driven in areas with and without cameras:

- The pre/post change in absolute speed, and the pre/post change in percentage speeding in areas with and without cameras.

Where data are available we will investigate the duration of any reduction in either speed, or in the percentages speeding (i.e. time and distance ‘halos’), to investigate the local and widespread effects of the enforcement.
Secondary outcome measures
The secondary outcome variables include the number of traffic crashes, number of fatalities from crashes, and number of injuries from crashes.

Other data
We also sought data on economic outcomes (including costs of providing the intervention and income generated by the intervention) and process outcomes (e.g. data on implementation).

Search methods for identification of studies
Our search methods comprised four parts: first, a search of electronic bibliographic databases for published work (see below for electronic databases searched); secondly, a search of the grey literature for unpublished work; thirdly, a search of trials registers for ongoing and recently completed trials; finally, a search of reference lists of published studies, including contacting authors and specialist groups to enquire about unpublished studies (see Appendix 1: Full search strategy for a selected database). In order to reduce publication and retrieval bias we did not restrict our search by language, date or publication status, although the main sources are predominantly English.

Electronic searches
The Cochrane Injuries Group Trials Search Co-ordinator searched the following:

- Cochrane Injuries Group Specialised Register (to 16/03/2015)
- Cochrane Library CENTRAL database (to 16/03/2015)
- Ovid MEDLINE(R), Ovid MEDLINE(R) In-Process & Other Non-Indexed Citations, Ovid MEDLINE(R) Daily and Ovid OLDMEDLINE(R) 1946 to 16th March 2015
- Embase Classic+Embase (OvidSP) 1947 to 16th March 2015

The librarian at the College of Policing Library searched the following:

- PROQUEST (to 12/06/2015)
- EBSCO (to 12/06/2015)
- Web of Knowledge (to 12/06/2015)
- Heritage (to 12/06/2015)
- OVID Transport database (1988 to June 2015)
- National Police Library (to June 2015)

Searching other resources
Hand-searches
The previous update of the Cochrane review additionally included a hand-search of the journals *Accident Analysis and Prevention* (1974 to April 2010) and *Injury Prevention* (1995 to April 2010). No further hand-searching was undertaken in this review as it was not considered necessary, due to the prospective indexing of new studies by the Cochrane Injuries Group.

Data collection and analysis
Selection of studies
All studies identified through the search process were first exported to the EndNote bibliographic database for de-duplication. Once duplicate records had been removed, the records were imported into EPPI-Reviewer 4 software for screening and coding. This allowed the team to manage coding tasks, assess inter-rater reliability, and share the results (within the project team and externally).
Two review authors independently examined the titles, abstracts, and keywords of electronic records for eligibility according to the inclusion criteria above. Results of this initial screening were cross-referenced between the two review authors, and full-texts obtained for all potentially relevant reports of studies. The full-text reports of potentially eligible studies were independently assessed for final inclusion in the review by two review authors using screening codes in EPPI-Reviewer 4. Any disagreements were resolved by discussion with a third review author. Reference lists of all eligible trials were inspected for further eligible studies.

Data extraction and management
Data were coded independently by two review authors in EPPI Reviewer, using a standardised data coding set (see Appendix 2: EPPI Reviewer standardised data coding set). Corresponding authors of studies were contacted if the required data were not reported in the published manuscript.

Data synthesis and analysis

Descriptive analysis
We described all studies that met the inclusion criteria, including the following:

1. Study design
   - Study design and quality (risk of bias)
   - Data collection methods, modes, and techniques; validity of tools
   - Statistical and other analyses
2. Participants (intervention and control if relevant)
   - Study setting (country, urban/rural location)
   - Nature of roads (Road type: motorway, major, minor, and speed limit)
3. Components of programme
   - Mobile or fixed cameras
   - Manned or unmanned cameras
   - Covert or overt cameras
   - Single point or average speed
   - Theoretical basis used in the design of the intervention components
4. Outcomes
   - Primary outcomes
     (e.g. Percentage of vehicles travelling above the speed limit, average speeds)
   - Secondary outcomes
     (e.g. road traffic crashes, deaths and injuries).

Statistical analysis
To facilitate comparisons of studies we defined a standardised summary measure for each outcome. Summary measures were based on relative effects, rather than differences in effects, where the outcome after intervention was divided by the outcome before intervention, as an expression of the proportional change in outcome. We calculated summary measures for all studies where possible (i.e. where required information was reported or adequate data were available for the calculation).

We estimated rate ratios by dividing the count of the outcome post- and pre-intervention in the intervention area by the corresponding ratio in the control area.

For example, the estimated rate ratio for road traffic collisions was:

\[
\frac{\text{collisions after/collisions before in intervention area}}{\text{collisions after/collisions before in control area}}
\]
Assuming that traffic volume remains the same on the roads post intervention in the control and intervention areas, this rate ratio estimates the change in the collision rate in intervention areas compared to that in control areas. For outcomes expressed as counts or rates we estimated the intervention effect using rate ratios with a 95% confidence interval (CI), calculated assuming a Poisson distribution for the number of collisions in each area and time period.

Unadjusted data provided by study authors in tables, text or graphs were also used to present the results of studies in relation to speed, numbers of vehicles speeding, road traffic crashes, road traffic injuries and fatalities.

**Synthesis**

We pooled the results in a meta-analysis where three or more studies reported the same outcome. We pooled the logarithm of the rate ratio and its standard error (calculated assuming a Poisson distribution for the number of collisions in each area and time period). If there were too few studies for a meta-analysis the results of individual studies were described in a narrative.

Heterogeneity (variability) among the effect estimates was assessed using a chi-squared test at a 5% significance level and quantified using the $I^2$ statistic, the percentage of between-study variability that is due to true differences between studies (heterogeneity) rather than due to sampling error. We considered an $I^2$ value greater than 50% to reflect substantial heterogeneity. Substantial heterogeneity would mean that the results of different studies vary substantially more than would be expected if the effects of speed cameras on the speed and traffic crash outcomes were the same in each setting. Where substantial variation in results was identified, we investigated its source by conducting subgroup analyses according to type of camera and by urban or rural setting. When assessing for differences of effect by subgroups, visual inspection of the forest plots was made and consideration to the widths of the confidence intervals for each subgroup was given. Where the confidence intervals of summary estimates for subgroups do not overlap, or do so very little, a difference in effect between the groups may be indicated, and follow-up tests were conducted.

Details of each intervention are presented in a table of study characteristics. We used Stata statistical software (version 14) to conduct the meta-analyses.

**Assessment of risk of bias/quality of included studies**

To assess the risk of bias and rate the quality of included studies, we followed the approach adopted by the authors of the 2010 Cochrane review. This approach relied on the Data Collection Checklist described by the Cochrane Effective Practice and Organisation of Care Review Group (EPOC). The process was based on four of the seven criteria used for the quality assessment for CBA designs, and the two additional criteria for ITS designs, used by EPOC. Broadly, study quality was based on:

1) Matching of intervention and control areas (e.g. the comparability of the areas; whether control areas are adjacent to the intervention area)

2) Blinding of data collection and analyses

3) Lengths of data collection time period pre- and post-intervention

4) Selective reporting of results by study authors

5) Control of confounders (e.g. was there an assessment of the distribution of confounders between intervention and control groups?)

6) Any other potential sources of bias.
The criteria chosen were those that are relevant to community trial designs and specifically determine the appropriateness of baseline measurements, characteristics of the control site, protection against contamination between sites, and reliability of the outcome measures, including; objectivity of measurement, results being reported with random variability and p-values, important potential confounders and risk of bias described and controlled for, and discussion of study generalisability. ITS studies were also scrutinised against the EPOC quality criteria for a clearly defined point in time when the intervention occurred, and at least three data collection points before and after the intervention. All indicators were scored on whether the criteria were done, not clear, or not done.

For CBA studies, quality scores were based on 12 indicators:

- High (Done=10, Not clear=2, Not done=0)
- Moderate (Done=8, Not clear=2, Not done=2)
- Low (<8 done)

For time series studies, quality scores were based on 9 indicators:

- High (Done=7, Not clear=2, Not done=0)
- Moderate (Done=6, Not clear=2, Not done=1)
- Low (<6 done)

For the 35 studies included in the 2010 Cochrane review quality assessment and scoring was performed independently by three review authors (CMW, CW, RLB). For the studies identified in the update, two review authors independently assessed the quality of the included studies (RS, PE) and any discrepancies were resolved by deferment to a third review author (CP). All disagreements were resolved by consensus.

**Results**

**Description of studies**

**Results of the search**

Records from all searches were imported, screened and coded using the EPPI Reviewer 4 software. Initial screening was shared between three review authors, who screened the titles, abstracts and keywords of the records for potential eligibility according to the inclusion criteria. This screening resulted in the exclusion of a total of 4,098 records. The review authors identified a total of 125 records to be assessed for eligibility using the full text reports. After reviewing the full text of all potentially relevant studies, 17 studies were identified that met the inclusion criteria, in addition to the 35 studies identified in the previously published systematic review. Two of the 17 newly identified studies were reports of the same evaluation (Skubic, 2013; 2014), therefore a total of 16 studies were analysed. The full text reports for all eligible studies were coded in detail in EPPI Reviewer. The numbers of records excluded with reasons are shown in figure 1.

In total, 45 were controlled before-after (CBA) studies with a distinct control group (15 newly identified CBA studies plus 30 previously identified) and 6 were interrupted time series (ITS) studies (1 newly identified ITS study plus 5 previously identified). No randomised controlled trials were found.

The characteristics of the 16 newly included studies are shown in Appendix 3. The table of characteristics of the 35 studies included in the previously published Cochrane systematic review is
Methods of analysis used by study authors varied. Some used simple ratios (as we have proposed in this review), whilst others used Empirical Bayes or Propensity Score Matching methods. In the Empirical Bayes method, the change in outcomes for any given camera site is the difference between the expected number of outcomes that would have occurred in the after period without intervention and the observed number of reported outcomes in the after period. In the Propensity Score Matching methods, treatment and control sites are matched using scores based on the characteristics of each site (e.g. road types, speed limits, numbers of junctions, average traffic volumes, etc.). To check matching, the distributions of propensity scores are compared between camera and control sites. In our analysis, we have used the reported results (e.g. rate ratios with 95% confidence intervals) if the raw numbers of outcomes were not reported in each study report.

For the previous Cochrane review (Wilson, 2010) a naming convention using official country codes (International Organisation for Standardisation, ISO-3166) was applied to the 35 included studies. For the studies identified in this updated review, first author names and years of publication have been retained to identify each study.
Outcome measures covered a range of definitions and measures of speed, crashes, and injuries. Speed outcomes were mainly based on average traffic speeds and the percentage of drivers who exceeded the posted speed limit. Road traffic crash outcomes resulting in injury to passengers or other road users were usually given in numbers rather than rates. Crash outcomes that included injury were usually given for all injuries, or numbers of crashes that resulted in fatalities or serious injuries. For some studies, the numbers of crashes also included damage to property. A summary of outcome measures covered by the current and previous reviews is provided in Table 1.

Table 1: Summary of studies

<table>
<thead>
<tr>
<th>Outcomes</th>
<th>2010 review</th>
<th>Current</th>
</tr>
</thead>
<tbody>
<tr>
<td>speed and crash</td>
<td>9</td>
<td>1</td>
</tr>
<tr>
<td>crash only</td>
<td>9</td>
<td>12</td>
</tr>
<tr>
<td>speed only</td>
<td>7</td>
<td>3</td>
</tr>
<tr>
<td>halo effects, of which:</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Distance halo effects</td>
<td>6</td>
<td>2</td>
</tr>
<tr>
<td>Time halo effects</td>
<td>3</td>
<td>2</td>
</tr>
<tr>
<td>Time and distance halo effects</td>
<td>3</td>
<td>1</td>
</tr>
<tr>
<td>Interventions</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Fixed camera</td>
<td>17</td>
<td>13</td>
</tr>
<tr>
<td>(16 overt, 1 covert &amp; overt)</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Mobile camera</td>
<td>15</td>
<td>3</td>
</tr>
<tr>
<td>(8 overt, 3 covert, 4 overt &amp; covert)</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Fixed and mobile</td>
<td>3</td>
<td></td>
</tr>
<tr>
<td>(2 overt, 1 overt &amp; covert)</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Country</td>
<td></td>
<td></td>
</tr>
<tr>
<td>1 study in:</td>
<td>Denmark, Finland, Germany, Spain, Hong Kong</td>
<td>Spain, Australia, Norway, South Korea, Canada</td>
</tr>
<tr>
<td>2 studies in:</td>
<td>Netherlands, New Zealand, Norway</td>
<td>Italy</td>
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<td>3 studies in:</td>
<td>Canada</td>
<td>UK, Belgium, USA</td>
</tr>
<tr>
<td>5 or more:</td>
<td>USA (5), UK (6), Australia (10)</td>
<td></td>
</tr>
<tr>
<td>Urban/rural</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Urban</td>
<td>13</td>
<td>8</td>
</tr>
<tr>
<td>Rural</td>
<td>6</td>
<td></td>
</tr>
<tr>
<td>Rural &amp;/or semi rural</td>
<td>7</td>
<td>1</td>
</tr>
<tr>
<td>Mixed urban, rural, semi rural</td>
<td>9</td>
<td></td>
</tr>
<tr>
<td>Range of speed limits</td>
<td>Range</td>
<td>60 - &gt;100km/h</td>
</tr>
<tr>
<td>40 - 110km/h</td>
<td>(7 studies did not specify speed limits)</td>
<td></td>
</tr>
<tr>
<td>Study period</td>
<td>&lt;= 2 years</td>
<td>12</td>
</tr>
<tr>
<td></td>
<td>2-4 years</td>
<td>7</td>
</tr>
<tr>
<td></td>
<td>4-6 years</td>
<td>9</td>
</tr>
<tr>
<td></td>
<td>6-8 years</td>
<td>1</td>
</tr>
<tr>
<td></td>
<td>&gt;8</td>
<td>6</td>
</tr>
</tbody>
</table>

In 2010, nineteen studies had reported both speed and crash outcomes (AU NSW 2; AU NSW 3; AU Tasmania; AU VIC 1; AU VIC 2; CA British Columbia; CA Vancouver B.C.; DE Germany; DK Denmark; FI Finland; GB 30mph Roads Nationwide; GB Nationwide; HK Hong Kong; NL Friesland; NL Netherlands; NZ Christchurch; NZ Nationwide; US Arizona 1; US North Carolina), of the newly identified studies in this review, only Vanlaar (2014) reported both. Nine of the previously identified studies reported crash outcomes only (AU QLD 1; AU QLD 2; AU VIC 3; ES Barcelona; GB Cambridge; GB Norfolk; GB South Wales; GB West London; NO Nationwide) while 12 of the 16 studies identified in this updated review did so (De Pauw, 2014; Novoa, Skubic; Thorpe, 2012; Budd, 2011; Gorell, 2004; Montella, 2015; Hoye, 2015; Shim, 2015; Moon, 2010; Montella, 2012; Li, 2013). Reporting speed outcomes only were seven previously identified studies (AU NSW 1; AU South Australia; CA Toronto; NO Oslo;
US Arizona 2; US Maryland; US Washington DC) and three newly identified studies (De Pauw, 2014d; De Pauw, 2014b; Medina, 2009). Of the 35 previously identified studies, six studies reported halo effects (AU NSW1; AU NSW2; AU Tasmania; CA Toronto; FI Finland; NO Oslo), three newly identified and two previously identified studies reported distance halo effects (De Pauw, 2014d; Montella, 2015; Li, 2013; AU Tasmania; FI Finland), and three previously identified studies reported time halo effects (AU NSW1; CA Toronto; NO Oslo). Only one study reported both time and distance halo effects (AU NSW 2).

Types of interventions varied between studies. Speed cameras were categorised according to whether they were overt or covert, fixed or mobile, or combinations of these. Fixed cameras were defined as those operating from a fixed mounted position on permanent poles. Speed cameras operating from police patrol cars or on temporarily installed poles were considered to be mobile cameras (although fixed for a period, these cameras may be moved from place to place). Similarly, with newly identified studies, it was decided that interventions could only be considered covert when studies included deliberate design features to conceal them (e.g. from unmarked patrol cars or hidden cameras). This review also included average speed enforcement interventions which measure the average speed of vehicles over a set distance to determine speeding. These interventions are referred to as ‘average speed section control’, ‘point to point’ or ‘time over distance’ cameras (Skubic, 2013).

Seventeen previously identified studies evaluated the effect of fixed cameras, of which, 16 studies comprised overt cameras and one included both overt and covert cameras. Thirteen of the 16 newly identified studies assessed fixed cameras, all of which were overt (De Pauw, 2014; Novoa, 2010; De Pauw, 2014d; Budd, 2011; Gorell, 2004; Montella, 2015; Hoye, 2015; Shim, 2015; Montella, 2012; Li, 2013; Vanlaar, 2014; Skubic; De Pauw, 2014b). Of these, three studies evaluated average speed enforcement interventions (Montella, 2012; Montella, 2015; De Pauw, 2014b). Fifteen previously identified studies, and three newly identified studies evaluated the effect of mobile speed cameras. Of previously identified studies, eight comprised overt and three covert cameras, and four studies included combinations of overt and covert cameras. All three of the newly identified mobile camera studies comprised overt interventions (Thorpe, 2012; Moon, 2010; Medina, 2009). Two previously identified studies reported the effects of a combination of fixed and mobile overt cameras, and one reported the effects of a combination of fixed, mobile, overt and covert cameras.

All of the previously identified and the newly identified studies were conducted in high income countries. Previously identified studies were conducted in Denmark, Finland, Germany, Spain and Hong Kong (one study in each of these settings), Netherlands, New Zealand and Norway (two studies in each of these settings), Canada (three studies), USA (five studies), UK (six studies), and Australia (10 studies). The newly identified studies included one each from Spain (Novoa, 2010), Australia (Budd, 2011), Norway (Hoye, 2015), South Korea (Shim, 2015) and Canada (Vanlaar, 2014), two from Italy (Montella, 2012; Montella, 2015), and three each from UK (Thorpe, 2012; Gorell, 2004; Li, 2013), Belgium (De Pauw, 2014; De Pauw 2014d; De Pauw, 2014b) and USA (Skubic; Moon 2010; Medina, 2009).

Of the previously identified studies 13 were conducted within an urban setting, six in a rural setting, seven in a rural and/or a semi-rural location and nine studies within a mixed urban/rural/semi-rural environment. Of the newly identified studies, eight were undertaken in an urban setting (Novoa, 2010; Skubic; Gorell, 2004; Montella, 2015; Moon, 2010; Montella, 2012; Vanlaar, 2014; Medina, 2009), 7 were conducted in a mixed urban/rural/semi-rural setting (De Pauw, 2014; De Pauw, 2014d; Thorpe, 2012; Budd, 2011; Shim, 2015; Li, 2013; De Pauw, 2014b) and one study was conducted only
in a rural/semi-rural context (Hoye, 2015). The range of study locations was reflected in the variation of speed limits in the studies. The previously identified studies were in settings with speed limits ranging from 40 km/h to 110 km/h. Of the newly identified studies, seven did not specify the speed limits while the limits in the remaining studies ranged from 60 km/h to over 100 km/h.

All previously identified studies were published between 1984 and 2009, with the newly identified studies published between 2004 and 2015. While fourteen of the newly identified studies were published after the previous review, two additional studies were identified for inclusion during the period covered by the previous review (Gorrell, 2004; Medina, 2009). Of the previously identified studies, 12 had study periods of less or equal to 24 months, compared to seven newly identified studies (De Pauw, 2014d; Hoye, 2015; Shim, 2015; Moon, 2010; Medina, 2009; Skubic; De Pauw, 2014b). Seven previously identified studies and five newly identified studies (Novoa, 2010; Thorpe, 2012; Gorrell, 2004; Montella, 2015; Li, 2013) had study periods of 2 to 4 years. Nine previously identified studies had study periods of 4 to 6 years compared to two newly identified studies (Budd, 2011; Vanlaar, 2014). One previously identified study had a study period of 6 to 8 years while the two newly identified studies covering the longest study periods were also in this category (De Pauw, 2014; Montella, 2012). Of the previously identified studies, a further three covered study periods of 8 to 10 years and three covered study periods greater than or equal to 10 years.

More details about the individual studies may be found in Appendix 3: characteristics of included studies.

Risk of bias in included studies
Of the total 51 included studies, 20 received a quality assessment rating of high (8 newly identified studies plus 12 previously identified), 17 received a quality assessment rating of moderate (5 newly identified studies plus 12 previously identified), and 14 received a quality assessment rating of low (3 newly identified studies plus 11 previously identified). Table 2 provides a list of the included studies with their individual quality rating.

Table 2: Quality of the evidence of included studies

<table>
<thead>
<tr>
<th>Study ID</th>
<th>Study design</th>
<th>Rating</th>
</tr>
</thead>
<tbody>
<tr>
<td>Newly identified studies</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Budd 2011</td>
<td>CBA</td>
<td>High</td>
</tr>
<tr>
<td>De Pauw 2014</td>
<td>CBA</td>
<td>Moderate</td>
</tr>
<tr>
<td>De Pauw 2014 b</td>
<td>CBA</td>
<td>Moderate</td>
</tr>
<tr>
<td>De Pauw 2014 d</td>
<td>CBA</td>
<td>Moderate</td>
</tr>
<tr>
<td>Gorell 2004</td>
<td>CBA</td>
<td>High</td>
</tr>
<tr>
<td>Hoye 2015</td>
<td>CBA</td>
<td>High</td>
</tr>
<tr>
<td>Li 2013</td>
<td>CBA</td>
<td>High</td>
</tr>
<tr>
<td>Medina 2009</td>
<td>CBA</td>
<td>Low</td>
</tr>
<tr>
<td>Montella 2012</td>
<td>CBA</td>
<td>High</td>
</tr>
<tr>
<td>Montella 2015</td>
<td>CBA</td>
<td>High</td>
</tr>
<tr>
<td>Moon 2010</td>
<td>CBA</td>
<td>Moderate</td>
</tr>
<tr>
<td>Novoa 2010</td>
<td>ITS</td>
<td>High</td>
</tr>
<tr>
<td>Shim 2015</td>
<td>CBA</td>
<td>Moderate</td>
</tr>
<tr>
<td>Skubic 2013</td>
<td>CBA</td>
<td>Moderate</td>
</tr>
<tr>
<td>Thorpe 2012</td>
<td>CBA</td>
<td>Low</td>
</tr>
<tr>
<td>Vanlaar 2015</td>
<td>CBA</td>
<td>Low</td>
</tr>
</tbody>
</table>

Previously Identified Studies
<table>
<thead>
<tr>
<th>Study ID</th>
<th>Study design</th>
<th>Rating</th>
</tr>
</thead>
<tbody>
<tr>
<td>AU NSW 1</td>
<td>CBA</td>
<td>Low</td>
</tr>
<tr>
<td>AU NSW 2</td>
<td>CBA</td>
<td>High</td>
</tr>
<tr>
<td>AU NSW 3</td>
<td>CBA</td>
<td>Low</td>
</tr>
<tr>
<td>AU QLD 1</td>
<td>CBA</td>
<td>Low</td>
</tr>
<tr>
<td>AU QLD 2</td>
<td>CBA</td>
<td>High</td>
</tr>
<tr>
<td>AU South Australia</td>
<td>CBA</td>
<td>Moderate</td>
</tr>
<tr>
<td>AU Tasmania</td>
<td>CBA</td>
<td>Moderate</td>
</tr>
<tr>
<td>AU VIC 1</td>
<td>ITS</td>
<td>High</td>
</tr>
<tr>
<td>AU Vic 2</td>
<td>CBA</td>
<td>High</td>
</tr>
<tr>
<td>AU Vic 3</td>
<td>CBA</td>
<td>Moderate</td>
</tr>
<tr>
<td>CA British Columbia</td>
<td>CBA</td>
<td>Low</td>
</tr>
<tr>
<td>CA Toronto</td>
<td>CBA</td>
<td>High</td>
</tr>
<tr>
<td>CA Vancouver</td>
<td>CBA</td>
<td>Moderate</td>
</tr>
<tr>
<td>DE Germany</td>
<td>CBA</td>
<td>Low</td>
</tr>
<tr>
<td>DK Denmark</td>
<td>CBA</td>
<td>Low</td>
</tr>
<tr>
<td>ES Barcelona</td>
<td>ITS</td>
<td>High</td>
</tr>
<tr>
<td>FI Finland</td>
<td>CBA</td>
<td>Low</td>
</tr>
<tr>
<td>GB 30 MPH Roads Nationwide</td>
<td>CBA</td>
<td>High</td>
</tr>
<tr>
<td>GB Cambridge</td>
<td>ITS</td>
<td>Moderate</td>
</tr>
<tr>
<td>GB Nationwide</td>
<td>ITS</td>
<td>Moderate</td>
</tr>
<tr>
<td>GB Norfolk</td>
<td>CBA</td>
<td>High</td>
</tr>
<tr>
<td>GB South Wales</td>
<td>CBA</td>
<td>Low</td>
</tr>
<tr>
<td>GB West London</td>
<td>CBA</td>
<td>Low</td>
</tr>
<tr>
<td>HK Hong Kong</td>
<td>CBA</td>
<td>Low</td>
</tr>
<tr>
<td>NL Friesland</td>
<td>CBA</td>
<td>High</td>
</tr>
<tr>
<td>NL Netherlands</td>
<td>CBA</td>
<td>Low</td>
</tr>
<tr>
<td>NO Nationwide</td>
<td>CBA</td>
<td>High</td>
</tr>
<tr>
<td>NO Oslo</td>
<td>CBA</td>
<td>High</td>
</tr>
<tr>
<td>NZ Christchurch</td>
<td>CBA</td>
<td>Moderate</td>
</tr>
<tr>
<td>NZ Nationwide</td>
<td>ITS</td>
<td>Moderate</td>
</tr>
<tr>
<td>US Arizona 1</td>
<td>CBA</td>
<td>High</td>
</tr>
<tr>
<td>US Arizona 2</td>
<td>CBA</td>
<td>Moderate</td>
</tr>
<tr>
<td>US Maryland</td>
<td>CBA</td>
<td>Moderate</td>
</tr>
<tr>
<td>US North Carolina</td>
<td>CBA</td>
<td>Moderate</td>
</tr>
<tr>
<td>US Washington DC</td>
<td>CBA</td>
<td>Moderate</td>
</tr>
</tbody>
</table>

Effects of interventions

Speed

Relative speed ratios were estimated in five studies, one of which estimated effects on speed during the day and at night. The effects of speed cameras on average speeds were highly heterogeneous, meaning that the individual study results varied more than should be expected if the effects of cameras on speed were generally the same in each setting, and differed only by chance (i.e. sampling error). The direction of the estimated effects was, however, consistent. The pooled effect estimate is shown in Figure 2 as the Effect Size (ES) with a 95% confidence interval, and is represented by a diamond shape in the final row of Figure 2. This is the overall estimate of the relative change in average speed with speed cameras compared to control sites without speed cameras. The 95% confidence interval is a range that we may be reasonably confident contains the
true relative change in average speed. Where the confidence intervals for an estimate and the reference line (at one on the x-axis) overlap, this indicates that an observed effect is not reliable. The pooled effect estimate from a random effects (which provide more conservative estimates of effect than fixed effects models, and assume that the variation observed cannot be explained by sampling error alone) meta-analysis was therefore a 7% reduction (i.e. \((1 - 0.93) \times 100\%\)) in average speed (95% confidence interval: 0% to 13% reduction in average speed).

**Table 1:**

<table>
<thead>
<tr>
<th>Study ID</th>
<th>ES (95% CI)</th>
<th>Weight</th>
</tr>
</thead>
<tbody>
<tr>
<td>AU Tasmania</td>
<td>0.98 (0.89, 1.08)</td>
<td>16.44</td>
</tr>
<tr>
<td>NL Friesland</td>
<td>0.97 (0.92, 1.03)</td>
<td>21.41</td>
</tr>
<tr>
<td>NL Netherlands</td>
<td>0.94 (0.93, 0.94)</td>
<td>25.76</td>
</tr>
<tr>
<td>US North Carolina (Day)</td>
<td>0.99 (0.77, 1.28)</td>
<td>5.32</td>
</tr>
<tr>
<td>US North Carolina (Night)</td>
<td>0.98 (0.76, 1.26)</td>
<td>5.32</td>
</tr>
<tr>
<td>US Washington DC</td>
<td>0.85 (0.85, 0.86)</td>
<td>25.76</td>
</tr>
<tr>
<td>Overall (I-squared = 99.0%, p = 0.000)</td>
<td>0.93 (0.87, 1.00)</td>
<td>100.00</td>
</tr>
</tbody>
</table>

**NOTE:** Weights are from random effects analysis

**Figure 2:** Effects of speed cameras on average speeds

Note – the statistical measure used to quantify the variation in effect size across studies is called ‘I-squared’. Values of I-squared over 50% indicate substantial heterogeneity. In this case, the value was 99% (figure 2). The column ES shows the Effect Size and confidence intervals (CI), which are also represented visually in the figure. The % Weight column indicates the contribution of each study to the overall estimate of ES – studies with larger confidence intervals, for which there is less certainty in the estimates, are assigned smaller weightings. The weighting applied to each study is also conveyed by the size of the boxes used to illustrate the ES estimates.

**Mobile vs Fixed**

All studies other than one (NL Netherlands) were of mobile cameras. The estimates of effect of mobile speed cameras on average speeds were highly heterogeneous (I-squared: 86.1%, figure not shown) meaning they varied more than would be expected on a chance basis.
Two studies assessed the effects of covert cameras on average speed. As shown in Figure 3, the estimates were highly heterogeneous (I-squared: 94.6%), illustrating meaningful variation across studies, and the overall effect of covert cameras was non-significant. The estimates of the effect of overt cameras were, however, consistent (I-squared: 0%); the pooled estimate was a 6% reduction in average speed (95% confidence interval: 6% to 7% reduction in average speed).
Urban vs Rural

As shown in Figure 4, the effects of speed cameras on average speeds were fairly consistent in rural settings (I-squared: 11.4%). The pooled estimate of effect in rural settings was a 6% reduction in average speed, with a 95% confidence interval of between a 4% reduction and an 8% reduction (ratio 0.94; 95% CI 0.92 to 0.96).

There was more heterogeneity in the results on average speeds in urban settings (I-squared: 19.6%, p=0.288), albeit non-significant. The pooled estimate of effect in urban settings was a 13% reduction in average speed, with a 95% confidence interval of between a 5% reduction and a 19% reduction (ratio 0.87; 95% CI 0.81 to 0.95).

Figure 4: Effects of speed cameras on average speeds - urban vs rural settings
Percentage speeding
Nine studies reported a total of 12 estimates of effect on the percentage of drivers exceeding the speed limit. Only three studies reported these estimates with confidence intervals (see Figure 5), for the remaining studies confidence intervals were incalculable. There was substantial heterogeneity between the three study results (I-squared 74.6%). The overall pooled estimate of the effect of speed cameras on the percentage of drivers exceeding the speed limit was a 59% reduction, with a 95% confidence interval of between 50% to 77% (ratio 0.41; 95% CI 0.33 to 0.50).

<table>
<thead>
<tr>
<th>Study</th>
<th>ES (95% CI)</th>
<th>Weight</th>
</tr>
</thead>
<tbody>
<tr>
<td>NL Friesland</td>
<td>0.68 (0.33, 1.40)</td>
<td>6.64</td>
</tr>
<tr>
<td>NL Netherlands</td>
<td>0.43 (0.41, 0.45)</td>
<td>52.55</td>
</tr>
<tr>
<td>US Washington DC</td>
<td>0.35 (0.30, 0.41)</td>
<td>40.82</td>
</tr>
<tr>
<td>Overall (I-squared = 74.6%, p = 0.020)</td>
<td>0.41 (0.33, 0.50)</td>
<td>100.00</td>
</tr>
</tbody>
</table>

Figure 5: Effects of speed cameras on percentage of drivers exceeding the speed limit

To examine the results from all nine studies graphically, the missing standard errors were imputed by using the maximum of the known standard errors (i.e. using SE=0.37 from the NL Friesland study). The standard errors in the other studies were: 0.024 (NL Netherlands) and 0.079 (US Washington DC).
The pooled effect estimate based on the 12 estimates of effect is more than a halving of the percentage of drivers who exceeded the speed limit (ratio 0.43, 95% CI 0.36 to 0.50). Thus the overall estimate of effect is similar when including the results from these other studies. As an exploratory sensitivity analysis, missing standard errors were replaced by the median of the known SEs (i.e. 0.079, from the US Washington DC study). The pooled effect estimate and confidence interval remained similar (ratio 0.53; 95% CI 0.42 to 0.66).

One study (De Pauw, 2014d) measured the change on the percentage of drivers who exceeded the speed limit at the camera site and at further locations downstream (1km and 3.3-3.8km). The data collected at the camera site was used in the primary meta-analysis. However, substituting the ‘at camera’ data with data from the downstream locations did not materially alter the overall effect size (1km downstream ratio 0.51, 95% CI 0.42-0.62 and 3km downstream ratio 0.50, 95% CI 0.41-0.60).
### Figure 7: Effects of speed cameras on the percentage of drivers exceeding the speed limit (imputed confidence intervals) – fixed vs mobile cameras.

The effects of mobile speed cameras on the percentages of drivers exceeding the speed limit were heterogeneous (I-squared: 75.4%, p=0.007). The effects of fixed speed cameras on percentages of drivers exceeding the speed limit were more consistent (I-squared: 17.8%, p=0.298). Visual inspection of the forest plot (figure 7), showed no evidence to suggest that effects on the percentages of drivers exceeding the speed limit differed between mobile (ratio 0.59; 95% CI 0.34-1.02), mixed (ratio 0.46; 95% CI 0.27-0.76) or fixed speed cameras (ratio 0.41; 95% CI 0.33-0.50), with overlap in summary effect estimate confidence intervals between all subgroups.
The effects of covert speed cameras on the percentages of drivers exceeding the speed limit were heterogeneous (I-squared: 67.5%). The effects of overt speed cameras on the percentages of drivers exceeding the speed limit were consistent (I-squared: 0.0%). There was insufficient evidence in the forest plot to suggest that effects on the percentages of drivers exceeding the speed limit differed between overt or covert speed cameras with significant overlap between the subgroup confidence intervals (ratio 0.44; 95% CI 0.24-0.82 – covert, ratio 0.43; 95% CI 0.41-0.45 - overt).

Figure 8: Effects of speed cameras on the percentage of drivers exceeding the speed limit (imputed confidence intervals) – overt vs covert cameras
Urban vs Rural

The effects of speed cameras on the percentages of drivers exceeding the speed limit were heterogeneous in urban and rural settings (I-squared: 48.9% - urban, 60% - rural). The effects of speed cameras on the percentages of drivers exceeding the speed limit were consistent in mixed settings (I-squared: 0.0%). There was insufficient evidence (Figure 9) to suggest that effects on the percentages of drivers exceeding the speed limit differed between urban (ratio 0.47; 95% CI 0.33-0.67) or rural settings (ratio 0.56; 95% CI 0.35-0.89), with substantial overlap in subgroup confidence intervals.

**Figure 9:** Effects of speed cameras on the percentage of drivers exceeding the speed limit (imputed confidence intervals) – urban vs rural settings

<table>
<thead>
<tr>
<th>Study ID</th>
<th>ES (95% CI)</th>
<th>Weight</th>
</tr>
</thead>
<tbody>
<tr>
<td>AU NSW1</td>
<td>0.78 (0.38, 1.61)</td>
<td>4.44</td>
</tr>
<tr>
<td>FI Finland</td>
<td>0.66 (0.32, 1.36)</td>
<td>4.44</td>
</tr>
<tr>
<td>US Maryland (using Maryland controls)</td>
<td>0.52 (0.25, 1.07)</td>
<td>4.44</td>
</tr>
<tr>
<td>US Maryland (using Virginia controls)</td>
<td>0.40 (0.19, 0.83)</td>
<td>4.44</td>
</tr>
<tr>
<td>US Washington DC</td>
<td>0.35 (0.30, 0.41)</td>
<td>24.74</td>
</tr>
<tr>
<td>Subtotal (I-squared = 48.9%, p = 0.098)</td>
<td>0.47 (0.33, 0.67)</td>
<td>42.50</td>
</tr>
<tr>
<td>NL Friesland</td>
<td>0.68 (0.33, 1.40)</td>
<td>4.44</td>
</tr>
<tr>
<td>NL Netherlands</td>
<td>0.43 (0.41, 0.45)</td>
<td>30.85</td>
</tr>
<tr>
<td>NZ Nationwide</td>
<td>0.86 (0.42, 1.78)</td>
<td>4.44</td>
</tr>
<tr>
<td>Subtotal (I-squared = 60.0%, p = 0.082)</td>
<td>0.56 (0.35, 0.89)</td>
<td>39.73</td>
</tr>
<tr>
<td>Depauw 2014 d (E19 Brasschaat)</td>
<td>0.23 (0.11, 0.47)</td>
<td>4.44</td>
</tr>
<tr>
<td>Depauw 2014 d (E40 Boutersem)</td>
<td>0.26 (0.13, 0.54)</td>
<td>4.44</td>
</tr>
<tr>
<td>DePauw 2014b (Brussels direction)</td>
<td>0.41 (0.20, 0.85)</td>
<td>4.44</td>
</tr>
<tr>
<td>DePauw 2014b (Ghent direction)</td>
<td>0.40 (0.19, 0.83)</td>
<td>4.44</td>
</tr>
<tr>
<td>Subtotal (I-squared = 0.0%, p = 0.590)</td>
<td>0.31 (0.22, 0.45)</td>
<td>17.76</td>
</tr>
<tr>
<td>Overall (I-squared = 46.4%, p = 0.039)</td>
<td>0.43 (0.36, 0.50)</td>
<td>100.00</td>
</tr>
</tbody>
</table>

NOTE: Weights are from random effects analysis.
Relative crashes ratios were estimated in 15 studies, providing 18 estimates of effect. The pooled estimate is shown as a guide to the overall average effect but should be treated with some caution due to variation between studies in their design and conduct. It suggests that speed cameras reduced crashes by 19% with a 95% confidence interval from 14% to 24% (ratio 0.81; 95% CI 0.76 to 0.86).

![Figure 10](image-url): Effects of speed cameras on all road traffic crashes
Mobile vs Fixed

![Figure 11: Effects of speed cameras on all road traffic crashes – fixed vs mobile cameras](image)

The effects of mobile speed cameras on road traffic crashes were consistent (I-squared 0.0%) but the effects of fixed speed cameras on road traffic crashes were heterogeneous (I-squared 61.6%). There was some evidence to suggest that effects of mobile cameras on road traffic crashes differed to those of fixed cameras, with only a small overlap in confidence intervals shown between the subgroups (ratio 0.85; 95% CI 0.81-0.89 – mobile, ratio 0.74; 95% CI 0.65-0.84 – fixed). A follow-up test, using the Q statistic to estimate between group (mobile versus fixed cameras) variance, confirmed that the difference was statistically significant (Q=5.72, p=0.017). Consequently, it appears that while both types of camera reduce traffic crashes, the effects associated with fixed cameras were generally larger, but also more variable.
Overt vs Covert

**Figure 12:** Effects of speed cameras on all road traffic crashes – overt vs covert cameras

The effects of overt speed cameras on road traffic crashes were heterogeneous (I-squared 57.2%). The forest plot shows insufficient evidence to suggest that effects of overt cameras (ratio 0.81; 95% CI 0.75-0.87) on traffic crashes differed to those of covert cameras (ratio 0.79; 95% CI 0.68-0.91), with substantial overlap occurring in between the subgroup confidence intervals.
Urban vs Rural

Figure 13: Effects of speed cameras on all road traffic crashes – urban vs rural cameras

The effects of speed cameras on road traffic crashes were heterogeneous in urban settings (I-squared: 72.3%), but consistent in rural and mixed settings (I-squared: 0% - rural and mixed). There was insufficient evidence in the forest plot to suggest that the effects on road traffic crashes differed between urban (ratio 0.82; 95% CI 0.74-0.92) or rural settings (ratio 0.82; 95% CI 0.75-0.89).
Injury crashes
Relative injury crashes ratios were estimated in 13 studies, providing 15 estimates of effect. The estimates of effect on road traffic crashes resulting in injuries were fairly consistent (I-squared 11.5%). The overall pooled estimate of effect suggests that speed cameras reduced road traffic crashes resulting in injury by 18% with a 95% confidence interval from 13% to 23% (ratio 0.82; 95% CI 0.77 to 0.87).

Other studies used different injury outcomes and are not included in this pooled analysis. These tended to estimate larger reductions: Gorell (2004) estimated the effect of speed cameras on the percentage change in injury crashes and found a 12.4% (95% CI 4.2% to 20.6%) reduction in crashes resulting in any injury. Montella (2012) also estimated the effect of speed cameras on the percentage change in injury crashes and found a 31% (95% CI 24% to 38%) reduction in all injury crashes. Thorpe (2012) estimated the effect of speed cameras on numbers of people injured in road traffic crashes and found a 39% reduction in casualties. Li (2013) additionally estimated the effect of speed cameras on the change in the number of crashes per kilometre, and found a reduction of 1.07 (SE 0.168) injury crashes per kilometre.

Figure 14: Effects of speed cameras on road traffic crashes resulting in injuries
Figure 15: Effects of speed cameras on road traffic crashes resulting in injuries – fixed vs mobile cameras

The effects of mobile speed cameras on road traffic crashes resulting in injury were consistent (I-squared 0.2%). The effects of fixed speed cameras were fairly consistent (I-squared 10.6%). There was insufficient evidence that the effects of mobile cameras on road traffic crashes resulting in injury differed to those of fixed cameras, with only minimal overlap present in the confidence intervals of the summary effect estimates (ratio 0.88; 95% CI 0.78-0.98 – mobile, ratio 0.80; 95% CI 0.74-0.86 – fixed). A follow-up test, using the Q statistic to estimate between group variance, confirmed that the difference between the fixed and mobile cameras was not statistically significant (Q=1.74, p=0.187).
Figure 16: Effects of speed cameras on road traffic crashes resulting in injuries – overt vs covert cameras

The effects of overt speed cameras on road traffic crashes resulting in injuries were consistent (I-squared 0.0%). The overall pooled estimate of effect of overt speed cameras on road traffic crashes resulting in injuries was similar to the estimate for all studies combined.
The effects of speed cameras on road traffic crashes causing injuries were consistent in urban and rural settings (I-squared: 0% - urban and rural). The pooled estimate of effect in urban settings was a 27% reduction in injury crashes, with a 95% confidence interval of 10% to 40% (ratio 0.73; 95% CI 0.60 to 0.90). The pooled estimate of effect in rural settings was a 21% reduction in injury crashes, with a 95% confidence interval of 14% to 28% (ratio 0.79; 95% CI 0.72 to 0.86).
Fatal and severe injury crashes

The effects of speed cameras on road traffic crashes resulting in fatalities or severe injuries were estimated in 5 studies; the estimated effects were consistent (I-squared 11.3%). The overall pooled estimate of effect on road traffic crashes resulting in fatalities or severe injuries was a 21% reduction with a 95% confidence interval from 13% to 29% (ratio 0.79; 95% CI 0.71 to 0.87).

Other studies used different injury outcomes and are not included in this pooled analysis. These tended to estimate larger reductions: Gorell (2004) estimated the effect of speed cameras on the percentage change in injury crashes and found a 20.6% (3.8% to 37.3%) reduction in crashes resulting in fatal or severe injuries. Montella (2012) also estimated the effect of speed cameras on the percentage change in injury crashes and found a 56% (95% CI 42% to 69%) reduction in crashes resulting in fatal or severe injuries. Li (2013) additionally estimated the effect of speed cameras on the change in the number of crashes per kilometre, and found a reduction of 0.13 (SE 0.046) fatal or severe injury crashes per kilometre.

<table>
<thead>
<tr>
<th>Study</th>
<th>ES (95% CI)</th>
<th>Weight</th>
</tr>
</thead>
<tbody>
<tr>
<td>AU Tasmania</td>
<td>0.72 (0.36, 1.43)</td>
<td>2.05</td>
</tr>
<tr>
<td>GB 30 MPH Roads Nationwide &lt;=500m</td>
<td>0.69 (0.57, 0.84)</td>
<td>21.13</td>
</tr>
<tr>
<td>GB 30 MPH Roads Nationwide &lt;=1km</td>
<td>0.73 (0.53, 1.00)</td>
<td>9.22</td>
</tr>
<tr>
<td>HK Hong Kong</td>
<td>0.33 (0.01, 10.80)</td>
<td>0.08</td>
</tr>
<tr>
<td>Moon 2010</td>
<td>0.86 (0.78, 0.95)</td>
<td>55.74</td>
</tr>
<tr>
<td>De Pauw 2014</td>
<td>0.71 (0.54, 0.93)</td>
<td>11.77</td>
</tr>
<tr>
<td>Overall (I-squared = 11.3%, p = 0.343)</td>
<td>0.79 (0.71, 0.87)</td>
<td>100.00</td>
</tr>
</tbody>
</table>

**Figure 18**: Effects of speed cameras on road traffic crashes resulting in fatalities or severe injuries

NOTE: Weights are from random effects analysis
Overall  (I-squared = 11.3%, p = 0.343)
The effects on road traffic crashes resulting in fatalities or severe injuries were consistent for mobile and fixed cameras (I-squared 0.0% for both mobile and fixed). There was weak evidence, shown by some overlap in the confidence intervals of the subgroups, to suggest that fixed cameras had a greater effect on road traffic crashes resulting in fatalities or severe injuries (ratio 0.7; 95% CI 0.61 to 0.81) than mobile cameras (ratio 0.86; 95% CI 0.78 to 0.94). A follow up test using the Q statistic to estimate between group (mobile versus fixed cameras) variance, confirmed that the difference was statistically significant (Q=5.11, p=0.024). It seems that the fixed cameras are associated with a slightly greater reduction in crashes resulting in fatalities or severe injuries compared to mobile cameras.

Overt vs Covert
All of the studies that assessed the effects of speed cameras on road traffic crashes resulting in fatalities or severe injuries were of overt cameras only.

Urban vs Rural
It was not possible to assess evidence for differences in the effects on crashes resulting in fatalities or severe injuries in urban or rural settings, as most were in mixed settings.
Halo effects
As shown in Figure 20, Li (2013) found that speed cameras were most effective in reducing crashes at 200 metres from the location, where they are most effective, but continued to be very effective up to 500m. The cumulative reduction in fatal or serious crashes (FSCs) increased up to 500 metres and then declined to 1 km. Crash reduction due to speed cameras is negatively correlated to the distance from cameras but it is unknown how far the relationship holds over larger distances.

![Figure 20: Cumulative reduction in annual incidence of personal injury crashes (PICs) and fatal/serious injury crashes (FSCs). Reproduced from source: Figure 3, Li, 2013.](image)

De Pauw (2014d) found that speed cameras were most effective in reducing average speeds and percentage of speeding vehicles at camera locations, but observed significant effects as far away as 1 km upstream or downstream of cameras. In an investigation of average speed cameras, Montella (2015) found that crashes were reduced by 32% on the motorway with average speed cameras, but also noted a statistically significant reduction of 21% in crashes in parts of the motorway where the intervention was not active (i.e. a significant ‘spill-over’ effect of the intervention). The study is not clear whether this is because there were no cameras or because the cameras that were there were not active.
EMMIE framework

In addition to extracting information to determine the effect of speed cameras, the included studies were coded to provide supplementary information around the EMMIE framework (Johnson et al., 2015). In addition to measures of the ‘effect’ of interventions, this framework provides a structure within which to detail the mechanisms, moderators, implementation and economic factors of speed camera programmes. Information has been extracted from the included studies and additional studies identified during screening, where details are pertinent to the EMMIE framework.

Mechanisms

The main mechanism through which speed detection camera programmes are considered to act is described by ‘Deterrence Theory’, which is based on ‘Rational Choice Theory’. Deterrence Theory posits that the punishment associated with committing an offence should be sufficient to deter offenders and others, from committing the same offence. In relation to speeding, Naatanena and Summala (1971) suggest that when determining their speed, drivers make a rational choice and balance the risks (e.g. of sanctions) against their motivation for speeding. The principal objective of speed detection measures is to increase the perceived risk of the speeding behaviour (Keall, 2001) by increasing the likelihood of being caught and subsequent punishment through sanctions, which may include licence points, driving bans, fines and speed awareness courses.²

There are two relevant components to Deterrence Theory that may explain how, and why, speed detection programmes might work:

- ‘Specific Deterrence Theory’ is focused primarily on the individual offender, and assumes that once caught and brought to justice, they will avoid re-offending as a consequence (Belin, 2010);

- ‘General Deterrence Theory’ is focused on the wider population, and is described by Ross (1982) as: “the effect of threatened punishment upon the population in general, influencing potential violators to refrain from a prohibited act through a desire to avoid the legal consequences.”

Specific Deterrence

Under Specific Deterrence Theory, a speed camera programme might be aimed at detecting the highest number of offenders as possible, in order that offenders refrain from re-offending. It may therefore be beneficial to operate ‘covert’ (i.e. concealed) speed camera systems, which may be more effective at detecting a higher proportion of speeding drivers (Diamantopolou, 2002). Keall (2002) also suggests that the concealment of cameras would result in drivers passing cameras at faster speeds due to unawareness of their presence, therefore increasing detection rates.

Deterrence Theory also suggests that the penalties imposed for an offence will influence the avoidance of further offences: higher fines and increased numbers of penalty points issued in connection with an offence may act as additional deterrents for an individual offender. Tay (2010) suggested that monetary fines and supplementary costs (e.g. increased car insurance) imposed after being caught for traffic speeding may be likely to increase the ‘deterrent’ effect. In this way, fines

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² Speed awareness courses are commonly offered in England and Wales as an alternative to fines or penalty points. In 2014, around 1.2 million people who had been caught speeding opted to take a speed awareness course when it was offered to them (https://ndors.org.uk/trends-stats/).
may have both revenue-raising and safety benefits. A deterrent effect, however, may also occur if police were to issue a ticket, with associated increase in insurance costs, with no fine. Studies included in this review have not collected data on the rate at which fines are issued and further research in this area could help to assess whether this mechanism is at play.

General Deterrence

It has been proposed across a number of studies that a speed camera programme based on General Deterrence Theory will have the greatest impact on speeding behaviours (Chen, 2000; Chen, 2002; Vanlaar, 2014; Diamantopolou, 2002). Chen (2000) hypothesised that covert mobile radar speed enforcement with an accompanying publicity campaign would not only reduce speeds at deployment sites, but also more widely across a province. This would be due to increased perceptions of risk of detection from the unknown and unpredictable locations of photo-radar units.

Fixed average speed cameras were installed in Scottsdale, Phoenix with accompanying media coverage (Retting, 2008). In a subsequent survey of drivers in Phoenix (conducted 8 months after camera installation), 44% of drivers said that the cameras had caused them to reduce their speed, and 60% of these drivers also said that the cameras caused them to reduce their speeds on other roads. Diamantopolou (2002) suggested that using marked cars, or a mix of marked and unmarked cars, in addition to a media campaign, could further enhance the General Deterrent effect, as enforcement becomes known and the effects become more widespread.

Newstead (2003) intended to maximise General Deterrence by carrying out mobile enforcement at random times and locations (across 2,500 pre-approved sites) using overt cameras. This approach would make drivers aware of the cameras, whilst keeping enforcement locations and activity unpredictable. Drivers would then be required to reduce their speeds over a wider area in order to avoid detection. Such positive spill-over effects on speeds and collisions away from the specific locations of enforcement, if real, may be due to General Deterrence Theory.

A number of studies have tested the plausibility of this mechanism through their assessment of the halo or spillover effects (Li, 2013; De Pauw 2014d; Montella 2015), highlighting deterrence occurring both at the camera locations and beyond. While speed cameras have been reported to be most effective in crash reduction at the camera location (Li, 2013) but significant spillover effects have also been reported up to 1km upstream and downstream from camera sites (De Pauw, 2014d). One study of an average speed camera enforcement programme (Montella, 2015), reported significant crash reduction of 21% in a 22.3km section of motorway with no camera enforcement, indicating that a significant spillover effect from the enforced area was generated.

Moderators

Possible moderators of effect were reported in some of the included study reports, these are understood to be pre-existing conditions that may affect the outcomes observed and could include factors such as road type, speed limits, setting (urban or rural) and country level effects. Subgroup analysis of rural and urban studies in the meta analysis found no evidence that effects differed between urban and rural areas. Sufficient data was not available to further analyse other potential moderators in this way, although further moderating factors have been discussed across the studies.

Time of day and day of week, have both been suggested as possible moderators of the effects of speed cameras. Perez (2007) argued that as heavy weekday peak-time traffic volumes reduce the opportunities to drive at higher speeds, greater effects of cameras might be expected during times of lighter traffic flows. De Pauw (2014) found a greater effect during the week, compared with at the
weekend, and a greater effect during the day, compared with at night. These results were supported by Montella (2015) who also found greater effects during the day than at night, and greater effects on weekdays than at weekends. Diamantapolou (2002), however, found greater reductions in average speeds at weekends compared to week days; during the week the effects were greatest in the afternoons during peak traffic.

Weather conditions have also been proposed as a likely modifier of effect of cameras. Montella (2012) initially did not find that weather conditions influenced effectiveness, but subsequently found greater reductions in traffic crashes for rainy and wet conditions (Montella, 2015). The greatest estimated reductions were in single vehicle crashes, which is suggested to be due to reduced speeds reducing the occurrence of skidding. (Montella, 2015)

Tay (2010) reported that the issuing of enforcement penalty tickets is highly seasonal, with the highest rates in summer, when roads tend to be dry. The type of road may also influence the effects of cameras. Greater effects have been observed in tunnels than on open roads (Gorell 2004). This, however, may be more a reflection of the restricted movement in a tunnel, than a moderating effect of cameras.

Implementation
There are a number of key features identified in the implementation of speed camera installations, including: whether they are covert or overt systems, manned (i.e. by police presence) or unmanned (automatic), and whether they are mobile or fixed.

The different methods of implementation of speed detection programmes may alter the way in which speeding behaviour is modified by influencing drivers’ perceptions of the certainty of detection, or their perception of the severity or likelihood of punishment. With respect to the former, it is recognised that drivers adapt their behaviour in response to enforcement to reduce the risk of detection (including reducing speed and learning enforcement locations; Soole, 2013). Consequently, enforcement is required that encourages a generalised deterrence effect and more widespread enforcement to reduce detection avoidance. Soole (2013) hypothesized that mobile enforcement can increase the general deterrent effect as drivers are unsure whether cameras are operating in isolation at a single site, or as part of an average speed detection programme. Montella (2015) also identified that in a programme utilising average speed cameras on a section of motorway, a large spillover and generalised deterrence effect was generated. Average speed enforcement also has reportedly wider public acceptance as it is thought to be more indicative of a drivers’ typical speed behaviour (van Schagen, 2004; Stefan, 2005 and Malenstein, 1997 all in Soole, 2013).

Cameras can be installed so as to be overt or covert. They can be made overt in different ways, including the presence of police officers, signs to indicate their presence, or painted different colours (e.g. yellow or grey) to make them more or less obvious. While there was little discussion of issues associated with the ease with which implementation varied for overt or covert cameras, the available evidence suggested no difference in effect size. No studies were identified that discussed implementation issues (or the effect on crime) associated with the colour of the cameras.

With respect to punishment (or the actual enforcement of law breaking), the scale of speed camera programmes and the levels of enforcement (i.e. camera operation hours and penalty tickets issued) may also influence the effectiveness of speed camera programmes. If so, this would represent an
important lesson for the implementation of such interventions. However, we identified no evidence on differences in effects associated with differing levels of enforcement of payment of fines.

Economic analyses vary in complexity, from the collection of the costs of implementation to the estimation monetized benefits of interventions, of the primary studies reviewed data was typically collected on estimates of crash costs (by severity), overall capital costs of scheme implementation, annual operating and maintenance costs, fine costs and ticketing revenue. More detailed costs relevant to implementation (for example of speed camera units, personnel costs of ticketing and processing) were not widely reported and due to wide variations in the implementation of schemes were not easily comparable.

Only one of the newly included studies reported on economic factors associated with speed detection programmes. Thorpe (2012) conducted a study to determine the cost of treatment saved (from casualties prevented\(^3\)) in a study of 56 mobile safety cameras in the Northumbria Police force area of the UK. They estimated between £25,600 and £30,900 was saved in treatment costs alone over the two years of the study.

A further look at the EMMIE coded data extraction from studies included in the previous speed camera review (Wilson, 2010) found that of the studies that conducted an economic analysis, all reported positive outcomes.

In Norway, Elvik (1997) reported capital investment of USD $49,000 per road segment for box mounted photo radar units, with annual operating costs of USD £32,000 with a total annual operating costs of USD $2.45 million for the 64 sections of road with cameras installed. A benefit cost ratio of 7.95 was calculated using annual operational costs (total of USD $2.45 million for 64 road segments) and benefits (USD $19.54million from an estimated 62 injuries prevented per year) excluding capital costs and possible costs of increased travel times.

Goldenbeld (2005) reported an economic analysis for speed enforcement using mobile radar devices in unmarked cars on 28 road sections. They reported total project costs of around 1 million euros per year and a cost of 5 million euros over the study period (with 130,000 euros being spent on publicity). An estimated two fatalities were assumed to have been saved over the same period and a more modest benefit cost ratio (BCR) of 3.1 was estimated.

In Queensland, Australia, a speed camera programme was deployed using overt mobile cameras with 15 units. Newstead (2003) used crash costs estimated by the Bureau of Transport Economics with a human capital approach (equating the value of a human life to the value of output produced over expected lifetime) to calculate total crash costs savings over 4 years of AUD $2,865.8 million. Total running costs of the intervention were AUD $60.8 million with the study reporting a BCR of 47 (excluding the negligible fine revenue).

Shin (2009) evaluated a speed camera programme comprising six fixed speed detection stations (with 3 cameras at each location to cover both directions) along 6.5 miles of urban freeway in Arizona, USA. While not conducting a full cost-benefit analysis, Shin (2009) estimated an annual economic net benefit (based on the costs of crashes only) of their speed enforcement programme to be between $16.5 and $17.1 million.

\(^3\) UK Government uses values the prevention of a road fatality at £1.84m and an injury at £54k (https://www.gov.uk/government/publications/reported-road-casualties-great-britain-annual-report-2014)
Soole (2013) conducted a meta-analysis of average speed enforcement and detailed a number of studies that conducted cost benefit analyses. The cost of implementing a ‘typical’ ASE programme was not detailed as this was deemed inherently difficult due to the numerous possible differences in site configuration and scheme characteristics. However, it was noted that compared to the more traditional speed enforcement approaches, average speed enforcement is relatively more expensive partly as a result of operational costs (Soole, 2013). While Soole (2013) only found limited evidence from the studies of average speed enforcement, it was concluded that these schemes can be associated with long term economic benefits, even when analysis does not consider revenue raised.

Cameron (2008) estimated the economic costs and benefits of a recommended fixed average speed enforcement programme (referred to as point-to-point speed camera systems) in Perth, Western Australia. The economic analysis included crash costs (by severity), capital costs to set up the scheme, operational and processing costs and fine revenue. The report identified 40 road sections where the benefit cost ratios of an ASE system would be 10 or greater, reporting an estimated BCR of 27.9 for the top 10 of these sites.

Cost estimation comparisons are difficult to make between speed camera enforcement schemes due to the number of potential variations in their implementation (for example the area covered, the number of cameras, whether cameras are fixed or mobile, average speed enforcement or single location cameras, manned or automatic cameras). The heterogeneity in the speed camera programmes and in the reporting of economic factors has meant it has not been possible to extract comparable costs for each specific type of intervention. Table 4 summarises the estimated benefit cost ratios, which have been reported across a number of studies.

Table 3: Benefit cost ratios (BCR)

<table>
<thead>
<tr>
<th>Author</th>
<th>Intervention</th>
<th>Setting</th>
<th>Estimated costs</th>
<th>Estimated savings</th>
<th>BCR</th>
</tr>
</thead>
<tbody>
<tr>
<td>Thorpe (2012)</td>
<td>56 mobile road safety cameras</td>
<td>Northumbria, UK</td>
<td>£25,600-£30,900 saved treatment costs over 2 years</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Lynch (2010)</td>
<td>Average speed enforcement</td>
<td>Australia</td>
<td>USD $16.5M-USD $17.1M/year</td>
<td></td>
<td>7.4-12.5</td>
</tr>
<tr>
<td>Shin (2009)</td>
<td>6 fixed locations (18 automatic cameras) over 6.5 miles of urban freeway</td>
<td>Scottsdale, Arizona, USA</td>
<td>USD $16.5M-USD $17.1M/year</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Cameron (2008)</td>
<td>Average speed enforcement, on high traffic urban freeways, highways &amp; high crash rural highways</td>
<td>Perth, Western Australia</td>
<td>USD $16.5M-USD $17.1M/year</td>
<td></td>
<td>10-27.9</td>
</tr>
<tr>
<td>Andersson (2005)</td>
<td>Fixed mounted camera boxes – 30 sections, 225 cameras</td>
<td>Sweden</td>
<td>USD $16.5M-USD $17.1M/year</td>
<td></td>
<td>4:1 (3.7)</td>
</tr>
<tr>
<td>Goldenbeld (2005)</td>
<td>Mobile radar, unmarked cars – 28 road sections</td>
<td>Friesland, Netherlands</td>
<td>Total project costs 1 million euros/year</td>
<td>2 fatalities saved over 5 years</td>
<td>3:1</td>
</tr>
<tr>
<td>Newstead (2003)</td>
<td>Overt mobile cameras</td>
<td>Queensland, Australia</td>
<td>Total over 5 years AUS $60.8M</td>
<td>Total of AUS $2,865.8M over 5 years</td>
<td>47:1</td>
</tr>
<tr>
<td>Elvik (1997)</td>
<td>Box mounted photo radar units – 64 road sections</td>
<td>Norway</td>
<td>USD $49,000 capital costs and $32,000 annual operation costs per road segment,$2.45M total annual maintenance</td>
<td>62 injuries prevented annually, USD $19.54M saved</td>
<td>7.95</td>
</tr>
</tbody>
</table>
Discussion

Summary of main results
Results from this review show consistent reductions in both speed and crash outcomes, across a number of different subgroups. Across all studies, the implementation of speed camera programmes was associated with a reduction in average speed of 7% (95% CI 0 to 13%), in percentage of vehicles exceeding the speed limit of 57% (95% CI 50 to 64%), in crashes of 19% (95% CI 14 to 24%), in injury crashes of 18% (95% CI 13 to 23%) and in severe or fatal crashes of 21% (95% CI 13 to 29%). Where there were enough data to generate comparisons across subgroups we found only limited evidence that effects differed by type of speed camera (fixed or mobile), and no evidence for difference of effect between overt or covert cameras. There was no evidence that effects on percentage of speeding vehicles or injury crashes differed by whether cameras were fixed or mobile. There was some evidence to suggest that fixed cameras had a greater effect on all road traffic crashes and those resulting in fatalities or severe injuries. We found no evidence that effects differed between urban and rural areas. There was some evidence that effects were greater within a short distance of camera sites compared to the wider areas.

How did results change since previous Cochrane review?
The estimates of the effects of speed cameras are now more precise due to more studies being included in this updated review. The estimated reductions in average speed is now between 0% and 13% (previously 1% to 15%); reduction in percentage of vehicles exceeding the speed limit is now between 50% and 64% (previously 8% to 70%); reduction in all road traffic collisions is now between 14% and 24% (previously 8% to 49%); reduction in injury collisions is now between 13% and 23% (previously 8% to 50%); and reduction in fatal or serious collisions is now between 13% and 29% (previously 11% to 44%). Unlike the previous Cochrane review, we have presented all results graphically using forest plots.

Limitations
Our meta-analyses of effects of speed cameras on speed and crash outcomes need to be interpreted with caution. Studies differed widely in terms of study quality, differences in study periods and settings, periods of data collection, types of cameras evaluated, as well as by factors that were not reported, such as other crime prevention or safety interventions used in conjunction with the speed cameras. These differences between studies (i.e. study heterogeneity) can mean that any pooling of studies, to produce an overall average estimate of effect, is therefore inappropriate. However, in our report we have grouped broad outcomes on speeding and traffic crashes in order to produce overall estimates of effect for illustration. For example, the speeding outcomes were the relative change in average speeds and the reduction in the percentage of drivers who exceed the speed limit. For both of these speeding outcomes we pooled the results in a statistical meta-analysis to estimate likely overall effects of cameras on speed and speeding. We could not, however, account for the different speed limits on the roads included in each study in this analysis, and so if the relative effects of cameras on speeding differ at different posted speed limits, then our pooled estimates of effect may be biased.

The results from the included studies were also often heterogeneous, as indicated in the statistical meta-analyses by the ‘I-squared’ statistic, which was often greater than 50%. This means that the results vary substantially more than would be expected if the effects of speed cameras on the speed and traffic crash outcomes were the same in each setting. We have attempted to understand this heterogeneity (variability) in results by conducting subgroup analyses by type of camera and by
urban or rural setting. Some of the heterogeneity was explained by these subgroups (the I-squared statistic was greatly reduced in some subgroups). For instance, effects of speed enforcement using overt cameras on average speed were consistent (I-squared: 0%), as were effects on injury crashes in urban (I-squared: 0%) and rural (I-squared: 0%) areas. We also examined subgroups by study quality (not presented in the results) but this did not reveal any consistent patterns between results of higher versus lower quality studies.

While evidence from this review on the exact size of the effects of speed cameras on speeding and crash outcomes must be interpreted with a degree of caution, we can be fairly confident that the implementation of speed cameras is associated with reductions in speeding and crash outcomes. The magnitudes of the estimated effects were somewhat variable, however the vast majority of studies included in this review suggest reductions in either speed, or in crash outcomes. In addition, the results on reductions in crash outcomes associated with speed camera enforcement are broadly consistent with previous research (Elliot 2005; Wegman 2006), including greater reductions in serious injury crashes and fatalities compared to other crashes. Consistent with previous studies, the newly identified studies included in this review reported some evidence of halo effects: greater reductions in speed and crash outcomes in the vicinity of cameras.

Generally, the methodological quality of studies included in this review was moderate. We found no randomised control trials, 7 interrupted time series studies, and 35 controlled before and after studies. More recent studies tended to be of higher quality. Of the previously identified studies in the Cochrane review; 32% were of low quality, 34% of moderate quality and 34% were high quality. Of the newly identified studies; 18% were of low quality, 35% of moderate quality and 47% were high quality. This review only included studies with acceptable control or comparison areas. Most studies examining crash related outcomes collected at least one year of ‘before’ data and one year of ‘after’, with 24 studies having ‘before’ and ‘after’ periods of at least two years. There are a number of potential limitations in interrupted time series and controlled before and after studies that may impact on results including: regression to the mean, traffic and/or crash migration (see below) and confounding variables.

The number of crashes that occur on particular roads randomly fluctuates over the long-term. However, speed camera enforcement strategies are often introduced on particular sites based on a recent history of high crashes. It is possible that crash frequencies at these sites were at the high end of naturally occurring random fluctuations and that in subsequent years, crash numbers would decline (regress back to the mean), even without any intervention. For a study to account for regression to the mean would require control sites to be chosen in exactly the same way as the treated sites (e.g. roads with the same levels of traffic exceeding the speed limit). In practice, however, it is difficult to find such matched control sites and to justify not treating them. Studies which do not account for regression to the mean may therefore overestimate the effects of speed camera enforcement on crashes. Some studies included in this review were able to take account of regression to the mean by using Empirical Bayes statistical methods, while others were able to control for long term trends in crash rates. The majority of studies, however, were unable to account for this possibility.

Another limitation of speed enforcement studies is the possibility of traffic (or crash) migration. The introduction of speed cameras in particular location may lead vehicles to use alternative non-enforced routes. We would expect fewer crashes to occur on routes with fewer vehicles. This may be more likely to occur with fixed overt speed cameras compared to other types of speed enforcement intervention. Very few studies included in this review collected information on traffic volumes. There
is a need for improved data collection on traffic volumes over time, so that any fluctuations in traffic volumes on roads may be incorporated into analysis of the effects of road safety programmes.

Finally, other factors such as season, time of day, changes in road design, speed limits, and road safety publicity may also have affected crashes. Most studies only controlled for a few of these, if any. The extent to which these factors may have impacted on the overall estimates is difficult to determine, given the number of included studies and other sources of heterogeneity discussed above. However, as we have suggested, although the magnitude of effects of speed cameras may be uncertain, we may be reasonably certain of the directions of effects, which indicate a reduction in speeding and crash outcomes.

The limited evidence for differences in effects by either type of camera (fixed or mobile; overt or covert) or by urban or rural settings, limits the further understanding of possible mechanisms of action of speed cameras. For example, while both covert and overt cameras led to a reduction in speeding when compared to no cameras, there was no evidence that the effects differed between camera types. It may seem counterintuitive that covert cameras, of which drivers are unaware, would reduce speeding but at this time we can only speculate about the mechanism. Additionally, no study provided empirical information on the effects of camera programmes on speeding and crash outcomes in the wider areas within which speed cameras are implemented, in order to assess whether General Deterrence theory might be supported. If the estimated effects on speeding and crashes found in this review are seen elsewhere in the community, then cameras may well have a wider general deterrence effect. The estimated effects may, however, support Specific Deterrence theory where the specific deterrent effect of cameras is the perceived risk by drivers of being caught and penalised. No studies reported on the sizes of fines or penalties issued to offenders, so any further assessment of the specific deterrence effects of different types or levels of punishment was not possible.

One included study reported on economic factors associated with speed detection programmes. In the Northumbria Police force area of the UK an estimated saving of around £30,000 was made in treatment costs from casualties prevented over two years. The costs of speed camera implementation were not provided, making an economic assessment difficult. Data on the costs of implementation are needed in future reports of evaluations of camera programmes. Cost-benefit ratios were estimated in some of the included studies. These estimated that the benefits exceed the estimated costs of speed camera programmes by at least 3:1, and were estimated to be far larger when the time horizons considered were 5 years or more. Comparisons of costs between speed camera programmes are difficult to make, however, due to large variations in implementation.

Further research
Further research is needed to evaluate the extent to which enforcement of traffic speeding violations results in compliance with speed limits at speed camera locations, and whether any effects are seen more generally in the neighbourhoods. Indeed, to better understand the mechanisms and moderators of action, and to assess effects more reliably, larger national studies such as that by Allsop (2010) may be needed. These studies are possible to conduct without large research costs, as police and local authorities regularly implement (and indeed remove) speed camera programmes over time. Such ‘natural experiments’ are possible providing that the locations (e.g. postcodes) of streets, as well as the month and year of implementation of enforcement the programmes, are well documented by the police and local authorities. These studies would have a
large number of areas and thus sufficient statistical power to reliably detect effects, and to explore whether any effects vary over distance and over time. A large study would also be able to assess possible displacement of speeding and traffic crashes to neighbouring streets, should drivers decide to take alternative routes to avoid the speed cameras. It may also be possible to link data from police crime reports and criminal justice outcomes to evaluate whether the nature of punishment is important. For example, if linkages could be made between the sizes of fines or penalties issued, and the specific road and speed camera where the driver had offended, it would be possible to assess whether larger fines and penalties are more effective. It may be that ‘persuasive’ letters to offenders once caught speeding are equally effective a deterrent as being caught and brought to justice.

In the future, the arrival of autonomous, self-driving cars may make speed enforcement redundant. As digital motoring technologies develop, cars equipped with radar, GPS, and computer vision may be capable of sensing and navigating the road environment without human input (and human error). Such vehicles will sense other road users and proceed at safe speeds, thereby reducing risks to pedestrians, cyclists, and other road users. In future ‘smart’ cities, traffic congestion might also be monitored centrally by computer, using GPS information from the fleet of vehicles to regulate traffic at safer and efficient speeds, via signalling and speed control over the road network. However, the automobile industry would be required to participate in such schemes, to be sure that autonomous cars are not programmed to exceed speed limits, just because surrounding vehicles are doing so.

Implications
This review provides evidence that speed cameras are an effective intervention for reducing a common criminal offence, speeding behaviour, and can help combat some of the negative consequences such as fatalities and injury crashes. Considering continuing increases in traffic volumes, speed cameras appear to be a worthwhile intervention to protect public safety. To help mitigate recurring controversies over the use of speed cameras, public education and publicly available data to ensure transparency of operations may be needed.

Acknowledgements
This research was conducted as part of the University Consortium for Evidence-Based Crime Reduction and was funded by the Economic and Social Research Council (ESRC); RC Grant reference: ES/L007223/1.
References

REFERENCES TO NEWLY IDENTIFIED STUDIES INCLUDED IN THIS REVIEW


Skubic (2013,2014)


REFERENCES TO STUDIES INCLUDED IN PREVIOUS COCHRANE REVIEW

AU NSW 1

AU NSW 2

AU NSW 3

AU QLD 1

AU QLD 2
WALSH, D. & WESSLING, G. Speed Cameras: Queensland last off the blocks but is it leading the race? 3rd National Conference on Injury Prevention and Control, 1999 Queensland, Australia, 79-83.

AU South Australia
CAIRNEY, P. T. & FACKRELL, H. 1993. The effects of a 40 km/h local area speed limit on traffic behaviour and community perceptions and opinions. Australian Road Research Board (ARRB). Research Note 243

AU Tasmania

AU VIC 1

AU VIC 2
AU VIC 3


CA British Columbia

CA Toronto

CA Vancouver B.C.

DE Germany

DK Denmark

ES Barcelona

FI Finland

GB 30mph Roads Nationwide


GB Cambridge


GB Nationwide


GB Norfolk

GB South Wales

GB West London

HK Hong Kong

NL Friesland

NL Netherlands


NO Nationwide

NO Oslo

NZ Christchurch

NZ Nationwide


US Arizona 1

US Arizona 2
US Maryland

US North Carolina

US Washington D.C.

BACKGROUND REFERENCES


Appendices

Appendix 1: Full search strategy for a selected database

Medline search strategy

1. Photography/
2. (enforc* adj3 program*).ab,ti.
3. Law Enforcement/
4. Social Control Policies/
5. ("road safety" adj3 legislation).ab,ti.
6. ("red light" adj5 (running or camera*)).ab,ti.
7. camera*.ti,ab.
8. (camera* adj5 (traffic or intersection* or junction* or automat*)).ab,ti.
9. (speed adj5 (camera* or control or enforc* or limit* or measurement*)).ab,ti.
10. ((photo* or automat*) adj3 enforc*).ab,ti.
11. (radar adj3 gun).ab,ti.
12. (radar adj3 (gun* or laser*)).ab,ti.
13. 1 or 2 or 3 or 4 or 5 or 6 or 7 or 8 or 9 or 10 or 11 or 12
14. "Wounds and Injuries"/
15. Accidents, Traffic/
16. ((accident* or colli* or fatal* or injur* or crash* or speed*) adj3 (traffic or road or vehicle or automobile)).ab,ti.
17. 14 or 15 or 16
18. 13 and 17
Appendix 2: EPPI Reviewer standardised data coding set

Study design
- Meta-analysis
- RCT
- Controlled interrupted time series
- Controlled before and after
- Before/after not controlled
- Cross sectional
- Case study
- Qualitative
- Commentary

Study length
- Dates of before period
- Dates of after period

Data collection details
- Data sources
- Creation of variables

Characteristics of intervention sites

Characteristics of control sites

Study setting and nature of roads
- Country
- Urban/rural
- Road type (motorway, major road, etc.)

Study aims

Intervention type:
- Mobile
- Fixed
- Manned
- Unmanned
- Covert
- Overt
- Average Speed
- Single point

Intervention components
- Number of cameras
- Size of area covered

Implementation (what is needed to implement speed cameras)

Mechanism
- Theory or mechanism of change

Measures of exposure to speed cameras

Outcome measures:
- Percentage of speeding drivers above the speed limit
- Average speed in areas with and without cameras
- Duration of speed reduction
• Road user deaths
• Road user injuries
• Road traffic crashes
• Total numbers of crimes

Statistical Methods

Description and treatment of bias and confounding

• Matching of intervention and control areas (e.g. the comparability of the areas; whether control areas are adjacent to the intervention area)
• Blinding of data collection and analyses
• Lengths of data collection time period pre- and post-intervention
• Control of confounders (e.g. was there an assessment of the distribution of confounders between intervention and control groups?)
• Adjustment for time trends
• Any other potential sources of bias (regression to the mean, adjustments for seasonality)
• Selective reporting of results by study authors

Results - where in full text are quantitative results

• Difference between groups (include CI)
• Interpretation

Cost information
### Appendix 3: Characteristics of included studies

<table>
<thead>
<tr>
<th>Methods</th>
<th>Participants</th>
<th>Intervention</th>
<th>Comparator/control</th>
<th>Outcomes</th>
<th>Results</th>
</tr>
</thead>
</table>
| **Budd (2011)** | | Fixed speed (and red light) cameras at treated sites consisted of 87 camera units placed at 76 discrete intersections. | 1189 intersections, from rural and metropolitan areas within the state of Victoria over the years 2000 to 2009. | • Casuality crash observations from 10,245 crashes.  
• Casualty crash frequency  
• Severe injury crashes (resulting in hospitalisation or death)  
• Before treatment crashes, at least 1 month before treatment within postcode.  
• After treatment crashes, at least 1 day after activation of treatment site within postcode. | For crashes involving vehicles travelling from the approach intersection leg where the camera was placed: estimated casualty crash reduction was 47% (95% CI: (36, 56), p<0.0001).  
Considering crashes involving vehicles from all approaches: the estimated casualty crash reduction was 26% (95% CI: (16, 35), p<0.0001).  
A 44% reduction (95% CI: (31, 64), p<0.0001) in right angle and right turn against crashes, those particularly targeted by red light enforcement, was also estimated.  
Estimates of effects not been inflated by regression to the mean. |
| **De Pauw (2014)** | | Speed cameras in 65 locations.  
All crashes within 500 m upstream and downstream of cameras. Crashes selected at distance bands of 250 m, to examine whether effects differ by distance from cameras. | The comparison group only included total crash numbers for Flanders, and it was not possible to calculate the over-dispersion. | • All injury crashes  
• Severe injury crashes, including persons who needed more than 24 h of hospitalization or who died within 30 days | 0–500 m  
All injury crashes: RR 0.92 [0.82;1.02]  
Severe injury crashes: RR 0.71 [0.54; 0.92]  
500–750 m  
All RR 1.08 [0.93;1.27]  
Severe RR 1.14 [0.81; 1.62]  
750–1000 m  
All RR 1.02 [0.88;1.19]  
Severe RR 1.27 [0.86; 1.87] |
| **De Pauw (2014b)** | | Automatic section speed control (ASSC) systems in Flanders, Belgium installed March 2013.  
7.4km with max speed limit of 120km/h, should take at least 222 seconds to travel. | 2 locations on same motorway were controls; similar traffic volumes and types of vehicles but without ASSC  
Possible confounders controlled for included widely implemented traffic measures, seasonal factors and weather conditions. | Effect on driving speeds:  
• average speed  
• odds of drivers exceeding speed limit  
• odds of drivers exceeding speed limit by >10% | ASSC led to decrease in average speed (by 5.84km/h); 74% reduction in odds of drivers exceeding speed limit; 86% reduction in odds of drivers exceeding speed limit by >10%.  
Decreases in average speeds (2.32-6.59 km/h, and odds of speed limit violations (40-72%) also found up to 6km upstream and downstream of... |
| **downstream of entrance and exit points.** | **ASSC devices not clearly visible for drivers; installed at entrance and exit of section.** | **ASSC and further a decrease in speed variability.** |
| **De Pauw (2014d)** | **Comparison locations selected to control for: weather, seasonality & other implemented traffic safety measures. 3 control locations, one on same road as each site (between 15-25 km from site), and one on a similar road.** | **The effects on average speed, after adjusting for trend effects are graphically displayed in Fig. 3 of study report; more detailed results are shown in Table 3. Fig. 4 and Table 4 of study report show effect on odds of drivers exceeding the speed limit of 120 km/h.** |
| Quasi experimental. | | **The effect on the odds of drivers exceeding the speed limit by more than 10% is shown in Fig. 5 and Table 5 of study report.** |
| Speeds at 2 Flemish motorway sites (120 km/h posted limits). Speed data recorded for 11-13 months before installation and compared to 10-18 month period after installation. | | |
| At each motorway, speed data were collected at 5 locations: ● 2.5-3km upstream ● At information sign, 0.25-0.7 km upstream ● at speed camera ● 1km downstream ● 3-4km downstream | | |
| **Medina (2009)** | **General traffic stream and free-flowing vehicles used for comparison. The speed of every 5th vehicle was systematically sampled and measured (representative sample from all vehicles in works area).** | **For free-flowing traffic:** ● SPE reduced average downstream speed by 2 to 3.8 mph for cars and by 0.8 to 5.3 mph for trucks. ● SPE reduced speeding cars by 7.1% to 23.4% and speeding trucks by 4.2% to 48.3% For the general traffic stream: ● SPE reduced average downstream speed by 1.1 to 2.9 mph on cars and by 0.9 to 3.3 mph on trucks. ● SPE reduced the percentage of speeding cars and trucks in the general traffic stream by 2.9% to 28.6%, and by 7.5% to 36.1%, respectively. |
| Data collected 1.5 miles downstream from intervention. Time-stamped videos were used to determine vehicle speeds between 2 road markers (200 ft apart). | | |
| **Gorell (2004)** | | |
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| | | |
| | | |
| | | |
### Controlled before-after study on London roads

The study focused on camera sites with at least 4 KSI collisions within 36 months of installation.

Effect of safety cameras on collision numbers at the 77 sites with 4 or more KSI collisions in the 3 years before camera installation. A subset of 68 sites was selected from the overall pool of 271 control sites that had experienced at least 4 KSI collisions in the three year period, and used as the control sites. Sites were matched on: total "before" injury collisions at camera sites; road characteristics, vehicle flow, lengths and geographical area (same or neighbouring London borough).

- All personal injury collisions
- Collisions within 400m on either side of a camera

At sites where there had been four or more KSI collisions in the previous three years, cameras were estimated to reduce KSI collisions by 20.6%, statistically significant (p=0.05 level). Table 1 shows approximate 95% confidence interval for this estimate.

The second (EB) approach produced a very similar result, the 95% confidence interval being -5% to -32%.

The results of the analysis for all personal injury collisions indicate that there was a 12.4% reduction in all injury collisions attributed to the introduction of cameras where there had been 4 or more KSI collisions in the previous 3 years.

### Hoye (2015)

Before after evaluation using Empirical Bayes method. Did not compare crashes per million vehicle km travelled in the before and after periods.

All sites in Norway with section control installed by 2012.

14 sites in evaluation, speed limit 80km/h at 12 sites and 90-70km/h at 2 sites, mostly with 2 lanes. Outside urban areas, and undersea.

Section control site comprised road between 2 speed cameras (4 if bidirectional), average speed between cameras.

All sites signposted upstream of first camera and cameras are overt.

An Empirical Bayes evaluation was conducted as there were no control roads with similar crash history.

<table>
<thead>
<tr>
<th>Injury and KSI crashes</th>
<th>Crash data until 2013</th>
</tr>
</thead>
<tbody>
<tr>
<td>Injury crashes reduced by 12%.</td>
<td></td>
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<tr>
<td>Greater effect in tunnels than open roads but results not statistically significant.</td>
<td></td>
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<tr>
<td>KSIs reduced by 49% (statistically significant).</td>
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</tbody>
</table>

### Moon (2010)
| Li (2013) | Camera sites from 8 English districts included in treatment group: Cheshire, Dorset, Greater Manchester, Lancashire, Leicester, Merseyside, Sussex and West Midlands. Study period 1999-2007 to capture 3 years before and after camera installation (between 2002 and 2004) for every site. The before/after periods used for control sites were same as those of closest camera sites. | Propensity score matching method (PSM) used to evaluate the effect of speed cameras on the reduction of road traffic accidents. | 
| • Collision data from Traffic Engineering Accident Analysis System (complete record of all collision reports sent by police)  
• Collision data categorised as total, fatal injury, property damage only.  
• Fatal and injury collisions were pooled as very few fatal collisions occurred. | Speed cameras most effective up to 200m where reduction in annual injury collisions per km was approximately 1.35 (27.5% in percentage). Effect decreases as the distance from camera site increases:  
• up to 500m: 1.135 (26.4% in percentage)  
• up to 1km: 0.570 (18.5% in percentage). |  
| Montella (2012) | Study sites in Italy on motorway with 3 lanes in each direction and 130 km/h speed limit. The study site was 79.88 km (39.94 km in each direction). Enforcement determined average speed over 500 m up to several km. Speed control activated in July 2007 over a length that covers almost all the study sections. | Empirical Bayes method using a before-and-after study. Analysis over 2001-2009 with a before period of 6.5 years and an after period of 2.5 years. Models were fitted to crash types: total, run off the road, rear end, sideswipe, other crash types, | 
| • Numbers of KSI collisions (<4 KSIs per km in last 3 years.  
• Number of injury collisions (<8 PICs per km in last 3 years. | Average yearly crashes per km decreased from 4.2 before to 2.2 after intervention. Total crashes reduced by 31.2%, with a lower 95% confidence limit of 24.3%. All crash types reductions except rear-end crashes, which showed a non-significant reduction (13.8%). Night-time crashes reduced: 38.1% versus 26.3% day-time. |  
| Before and after evaluation including control sites. | 14 sites with speed cameras in Charlotte, North Carolina (total 57.6 miles). Sites selected for high collision history. 3 vans with cameras operated on one of the 14 sites each day. Warning signs at entrance to sites to notify drivers of speed enforcement, accompanying aggressive media campaign to publicise programme. | Comparison sites were 11 corridors geographically scattered and similar to treatment sites, with lower collision rates. Sample odds ratio test showed that collisions at comparison and control sites were similar. | 
| • Numbers of KSI collisions (<1 KSI per km in last 3 years.  
• Average yearly crashes per km decreased from 26.3% before to 13.8% after intervention. Total crashes reduced by 31.2%, with a lower 95% confidence limit of 24.3%. All crash types reductions except rear-end crashes, which showed a non-significant reduction (13.8%). Night-time crashes reduced: 38.1% versus 26.3% day-time. | The first period of intervention led to slight decreases in total, property damage only and fatal injury collisions of 8 to 10%. The second period of intervention led to a decrease total collisions of 15 to 18%. In the post-intervention period there was a decrease in collisions of between 17 to 21%. |
<p>| Montella (2015) | Urban motorway in southern Italy, 3 lanes in each direction, with access control and interchanges. Total length 40.4 km (20.2 km per carriageway), divided into 82 segments in each carriageway (lengths 40 to 1387 m). Speeding up to 10km/h (plus 5% tolerance) fined 41-168 Euros. 10-40km/h over limit fined 168-674 Euros and 3 penalty points. 40-60km/h over, driving ban between 1-3 months, fine of 527-2108 Euros. &gt;60km/h over, driving ban 6-12 months, fine of 821-3287 Euros. | Motorway in Campania Region used as control, divided into 652 segments (lengths 62 to 3509 m). Traffic data provided by motorway management agency. Annual average daily traffic from 2006 to 2011 ranged between 7885 and 18,370 vehicles per day. Crash count was 1873 in the before period and 1445 in the after period. | Crash data collected through police reports and integrated with detailed site inspections. Crashes classified by alignment, collision type, severity, weather, pavement conditions, time of day. Crash data for 2006 to 2011, with before period of 3.08 years and after period of 2.91 years. Crash count for all treatment sites was 559 before and 279 after intervention. | Crash reduction in daylight crashes 33.4% (lower 95% CI limit 22.4%). Crash reduction in night-time crashes 28.5% (lower 95% CI limit 10.3%). Mean speed reduction greater in day: 9.9 vs. 9.2% for light vehicles, 5.3 vs. 1.8% for heavy. Effectiveness decreased over time: crash reduction 37.3% in first year, 29.9% in second and 27.9% in third. Crash reduction for total crashes 32.0% (lower 95% CI limit 22.3%). Greater effectiveness in curves (43.4 vs. 28.4% crash reduction). |
| Novoa (2010) | Quasi-experimental design using arterial roads (two to four lanes that cross the city) in Barcelona. 8 fixed speed cameras installed in March 2003. Operating in 22 locations, stretches of arterial roads encompassing 500 m before and after location of speed cameras were considered the enforced. Injury crashes and people injured between January | On enforced stretches the risk of crashes was similar in both periods (RR 0.98; 95% CI 0.75 to 1.27). There was a non-significant increase | Single vehicle crashes showed greatest reductions: 43.7% (lower 95% CI limit 31.2%). Reduction in rainy crashes 57.3% (vs. 29.6% non-rainy, lower 95% CI limit 34.8%). Reduction in wet crashes 50.9% (vs. 29.4% dry, lower 95% CI limit 30.1%). Reduction in daytime crashes 33.4% (lower 95% CI limit 22.4%). Reduction in night-time crashes 28.5% (lower 95% CI limit 10.3%). Crash reduction in working days 37.6% (lower 95% CI limit 26.9%). Crash reduction not significant at weekends. Effectiveness decreased over time: crash reduction 37.3% in first year, 29.9% in second and 27.9% in third. Crash reduction for total crashes 32.0% (lower 95% CI limit 22.3%). Greater effectiveness in curves (43.4 vs. 28.4% crash reduction). |</p>
<table>
<thead>
<tr>
<th>Shim (2015)</th>
<th>Empirical Bayes method.</th>
<th>ASES automatically measures vehicle speeds and photographs registration numbers of vehicles that exceed the speed limit. Information on enforcement locations is mandated by law to be open to the public.</th>
<th>Crash prediction model used data from 2007 to 2010 and annual average daily traffic data from 1128 roadway segments of Korean expressways.</th>
<th>All crash types included in the analysis.</th>
<th>Overall impact was a 7.6% reduction in the number of crash occurrences.</th>
</tr>
</thead>
<tbody>
<tr>
<td>Speed limit 50 km/h (31.1 m/h) crossed by intersections regulated by traffic lights. Mean speed during study period 21 km/h.</td>
<td>randomly moved so that motorists’ perceptions are that speed is being enforced throughout the beltway.</td>
<td>stretches of arterial roads, the remaining stretches of arterial roads being considered as the control group.</td>
<td>2002 and December 2007 for the arterial roads</td>
<td>observed in risk of injury (RR 1.37; 95% CI 0.79 to 2.38). On non-enforced stretches risks were similar in both periods for collisions (RR 0.99; 95% CI 0.93 to 1.05) and for injury (RR 1.00; 95% CI 0.91 to 1.10).</td>
<td></td>
</tr>
<tr>
<td>Shim (2015)</td>
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<td>Overall impact was a 7.6% reduction in the number of crash occurrences.</td>
</tr>
<tr>
<td>Skubic (2013)</td>
<td>Fixed and mobile speed cameras across Arizona in 2008. Controversy meant cameras were removed in 2010. Evaluation of 20 mile bidirectional (40miles) segment of interstate i-10 in urban Phoenix, speed limit 65mph. Collision data from Arizona Dept of Transport for 2008-2011. Cameras deployed in Oct 2008 until Jul 2010. Three 9-month periods of analysis, pre-cameras (Jan-Sept 2008), cameras (Jan-Sept 2009) and post cameras (Jan-Sept 2011)</td>
<td>26 miles ’experimental segments’ with cameras every 2 miles along its length during evaluation period</td>
<td>14 miles with no cameras (control segment).</td>
<td>Total numbers of collisions during each time period.</td>
<td>Speed cameras did not statistically contribute to increase or decrease in number of MVC 26 mile experimental segment: 1.5% (341 vs. 346, p=0.8) increase in MVC when cameras were placed and 28% increase when removed. 14 mile control segment (no cameras): 3.6% and 39% increases in MVC during same time periods.</td>
</tr>
<tr>
<td><strong>Thorpe (2012)</strong></td>
<td>56 mobile camera sites</td>
<td>67 control sites. To test comparability in explanatory variables between control and camera sites, a Monte Carlo permutation test was conducted on site characteristics which confirmed sites were comparable.</td>
<td>Casualties that did not occur in ‘after’ period.</td>
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</tr>
<tr>
<td>Before and after study of mobile speed cameras. ‘Before’ period was financial years 2001/02-2002/03 and after period 2004/05-2005/06.</td>
<td></td>
<td></td>
<td>In the ‘after’ period, casualties fell by 132 (32%) to 298 at the mobile camera sites.</td>
<td></td>
<td></td>
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<tr>
<td>Empirical Bayes method.</td>
<td></td>
<td></td>
<td>Empirical Bayes analysis suggests casualties would have fallen to 321 casualties without cameras, limiting their effect to preventing 23 casualties.</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Casualty data from representative sample of control sites used to predict expected numbers of casualties in after period.</td>
<td></td>
<td></td>
<td>This figure could be expected to fall further if trend is taken into account although this is not straightforward within the Empirical Bayes framework. A crude solution is to reduce the 23 casualties by 4.7% annually for the 2 years in the ‘after’ period based on the available casualty data for the region.</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Data from Northumbria road network not covered routinely by mobile camera enforcement used to estimate trends at treated sites.</td>
<td></td>
<td></td>
<td>Results from fully Bayesian analysis suggests an expected mean and median number of casualties in the ‘after’ period of 327 and 313, respectively and, after taking trend into account, this reduces to 319 and 306 casualties.</td>
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<tr>
<td></td>
<td></td>
<td></td>
<td>The median is most appropriate statistic to use suggesting that the 56 mobile cameras prevented eight casualties.</td>
<td></td>
<td></td>
</tr>
</tbody>
</table>

<table>
<thead>
<tr>
<th><strong>Vanlaar (2014)</strong></th>
<th>To detect speeding at intersections, activated sensors triggered and calculates speed.</th>
<th>Comparison group data from comparable crashes at comparable times in New Brunswick where there was no camera enforcement during study period. Data were cross-referenced and checked for accuracy. The same selection criteria</th>
<th>Injury crashes</th>
</tr>
</thead>
<tbody>
<tr>
<td>Time series analyses of red light running and speed related crashes at intersections.</td>
<td></td>
<td></td>
<td>Average speed: little differences in average speed before and after installation of cameras, at treatment and control sites for: 50 km/h winter; 60 km/h winter and summer.</td>
</tr>
<tr>
<td></td>
<td></td>
<td></td>
<td>Speeding violations (&gt;1 km/h over speed limit): at 50 km/h locations in winter,</td>
</tr>
</tbody>
</table>
for crashes are applied as those used with the data from Winnipeg.

| decreases in speeding violations apparent at intervention and comparison locations, but decrease at intervention locations significantly greater (57% decrease at intervention, 19% at comparison location). |
| For low traffic density observations, there was a decrease of 57% at the location and 14% at the comparison locations. |
| For serious speeding violations (13 km/h over speed limit) at the 50 km/h locations in the winter, a decrease of 54% in serious violations was found at intervention locations and 37% at comparison. |
| For serious speeding violations at low traffic density observations: decrease of 54% at the treatment locations versus 34% at comparison. |
| Data from 60 km/h location in winter showed a significant decrease of 12% in speeding violations (>1 km/h over speed limit) at treatment site. At comparison sites there was a significant increase in speeding violations of 13%. |
| Low traffic density, an increase in speeding violations was 40% at treatment sites and 57% at comparison locations. |
| Serious speeding violations (13 km/h over speed limit) at 60 km/h locations in winter there was an increase of 83% at treatment locations but significantly lower 13% at comparison locations. |
| Low traffic density only, an increase of 114% at treatment sites versus increase of 15% at comparison site. |
Regarding 60 km/h locations in summer, a significant 22% decrease in speeding violations (>1 km/h over speed limit) at the treatment site and a significant 13% increase at comparison sites.

Low traffic density only, a significant 27% decrease at treatment location and significant 23% increase at comparison locations.

The increase in speeding violations at 60 km/h location in winter was no longer apparent in summer.