WHAT WORKS: CRIME REDUCTION SYSTEMATIC REVIEW SERIES

No 11. RED LIGHT ENFORCEMENT CAMERAS TO REDUCE TRAFFIC VIOLATIONS AND ROAD TRAFFIC INJURIES

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April 2017
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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.
Figure 1: PRISMA flow diagram .............................................................. 19
Figure 2: Effects of red-light cameras on total crashes ....................... 23
Figure 3: Effects of red-light cameras on total crashes (imputed confidence intervals) 24
Figure 4: Effects of red-light cameras on total crashes - stratified by study quality .... 25
Figure 5: Effects of red-light cameras on total crashes - stratified by country ........ 26
Figure 6: Effects of red-light cameras on total road traffic crashes resulting in injury .. 27
Figure 7: Effects of red-light cameras on total road traffic crashes resulting in injury - stratified by study quality .......................................................... 28
Figure 8: Effects of red-light cameras on total road traffic crashes resulting in injury - stratified by country ................................................................................. 29
Figure 9: Effects of red-light cameras on total property damage only crashes ........... 30
Figure 10: Effects of red-light cameras on total road traffic crashes resulting directly from red-light running ................................................................. 31
Figure 11: Effects of red-light cameras on right angle crashes ..................... 32
Figure 12: Effects of red-light cameras on right angle crashes - stratified by study quality. ........................................................................................................... 33
Figure 13: Effects of red-light cameras on right angle crashes - stratified by country ... 34
Figure 14: Effects of red-light cameras on right angle crashes resulting in injury .......... 35
Figure 15: Effects of red-light cameras on right angle crashes resulting in injury - stratified by study quality. ................................................................. 36
Figure 16: Effects of red-light cameras on right angle crashes resulting in injury - stratified by country ................................................................................. 37
Figure 17: Effects of red-light cameras on turning, same roadway crashes ............ 38
Figure 18: Effects of red-light cameras on turning, same roadway crashes resulting in injury (imputed confidence intervals) ......................................................... 39
Figure 19: Effects of red-light cameras on total rear end crashes ..................... 40
Figure 20: Effects of red-light cameras on rear end crashes - stratified by study quality .. 41
Figure 21: Effects of red-light cameras on rear end crashes - stratified by country ...... 42
Figure 22: Effects of red-light cameras on total rear crashes resulting in injury ........ 43
Figure 23: Effects of red-light cameras on total rear crashes resulting in injury - stratified by study quality ................................................................. 44
Figure 24: Effects of red-light cameras on total rear crashes resulting in injury- stratified by country ................................................................................. 45
Figure 25: Effects of red-light cameras on total rear end crashes from red-light running. ............................................................................................................ 45
Figure 26: Effects of red-light cameras on red-light violations (imputed confidence intervals) ........................................................................................................... 46
Figure 27: Effects of red-light camera on total road traffic crashes – stratified by the use of warning signs ......................................................................................... 53
Figure 28: Effects of red-light camera on total road traffic crashes resulting in injury – stratified by the use of warning signs ......................................................... 54

Table 1: Summary of outcome measures ................................................. 20
Table 2: Summary of the included studies and quality of evidence ............ 21
Table 3: Summary of overall effect estimates* ......................................... 47
Abstract

Background
Road traffic crashes are a major and increasing cause of injury and death around the world. Many crashes occur as a result of red-light running (RLR), which occurs when a driver enters an intersection after the traffic light has turned red. While many drivers obey traffic signals, the possibility for violations exists due to issues such as driver distraction, aggressive driving behaviours, or a deliberate decision to ignore the traffic signal. RLR may result in collisions that cause damage to property as well as injuries and fatalities. Red-light cameras (RLCs) are an enforcement mechanism that permit police to remotely enforce traffic signals. They may serve as a deterrent to drivers who intentionally engage in RLR. The previous Cochrane systematic review of RLCs found that they were effective in reducing total casualty crashes but found that evidence on the effectiveness of cameras on red-light violations, total crashes, or specific types of casualty crashes was inconclusive. However, this review searched only a small number of electronic databases and was limited to a handful of studies published in 2002 or earlier.

Objectives
This report updates and expands upon the previous Cochrane systematic review of red-light cameras. It presents a systematic review and meta-analysis of the effectiveness of RLCs on the incidence of red-light violations and the incidence and severity of various types of traffic crashes.

Search Strategy
This report uses a four-part search strategy that involves: (1) Searching 25 online electronic bibliographic databases for published and unpublished evaluations of RLCs; (2) Searching the websites of 44 international institutes and research agencies focusing on transportation issues for reports and other grey literature; (3) Searching the reference lists of published studies to identify additional published and unpublished works; and (4) conducting a keyword search using Google and Google Scholar to search for additional grey literature.

Selection Criteria
Studies that assessed the impact of RLCs on red-light violations and/or traffic crashes were eligible for inclusion if they used randomized controlled trials, interrupted time series, or controlled before-after studies. Studies that involved additional interventions, such as speed cameras, were excluded. Both published and unpublished reports were included. Qualitative, observational or descriptive studies that did not include formal comparisons of treatment and control groups were excluded from this research.

Data Collection and Analysis
The researchers independently screened studies for inclusion, assessed methodological quality, and extracted data from full text reports using standardized instruments. Pooled meta-analyses were carried out when at least three studies reported the same outcome; otherwise, the results of individual studies were described in a narrative. EMMIE framework data were coded using the EPPIE Reviewer database.

Main Results
After searching and screening, 28 were identified that could be included in this review, in addition to the 10 identified in the prior Cochrane review. Because several reported on multiple independent study areas, this report evaluates 41 separate analyses. The meta-
analyses of these 41 evaluations show some statistically significant reductions in both red-light violations and crash outcomes. RLC programmes were associated with a 24% decrease in right angle crashes and a 29% decrease in right angle injury crashes, as well as a 20% decrease in total injury crashes and a 61% decrease in red light violations. Conversely, RLCs were also associated with a 19% increase in rear-end crashes. There was also some statistically nonsignificant evidence that RLCs were associated with a reduction in crashes due to RLR.

Authors’ Conclusions
The evidence suggests that RLCs can be effective in reducing red-light violations and some types of traffic crashes, although they also appear linked to an increase in rear end crashes. The primary policy implication of this review is that RLC programmes need to be evaluated based on a cost-benefit analysis that considers the fiscal viability and economic impact of RLC effects and that weighs the benefits of reduced traffic violations and some types of crashes against the increased risk of other crash types.
Background

The Problem, Condition, or Issue

Road traffic crashes are a major and increasing cause of injury and death around the world, with almost 1.25 million people dying annually and between 20 and 50 million more suffering non-fatal injuries, including permanent disabilities. Global estimates suggest that these crashes cost countries between three and five % of their gross income (World Health Organization, 2016).

According to the Department for Transport (2016), there were approximately 140,000 traffic crashes in Great Britain in 2015. As a result of these crashes, over 186,000 people were injured; including 23,874 persons killed or seriously injured (KSI) and of these there were 1,730 traffic fatalities. Approximately 60% of those injured and 44% of traffic crash fatalities were drivers or passengers in cars; the remainder were primarily pedestrians, motorcyclists, and pedal cyclists/bicyclists.

Traffic violations occur when a driver enters an intersection after the traffic light has turned red. While most drivers obey traffic signals, the possibility for violations does exist, due to either driver distraction, aggressive driving behaviours, or a deliberate decision to ignore the signal. Traffic light violations appear to be fairly common. In the UK, over 67,000 motorists were given points for failing to stop at traffic lights in 2015. A recent survey found that about 25% of motorists admit to running a red-light in the past year, which is equivalent to 9.3 million motorists. (Massey, 2016). A recent national telephone survey in the USA found that while the vast majority of drivers (93.5%) consider red-light running (RLR) to be unacceptable, almost 39% admitted to having driven through a red traffic light in the past month and over 25% had done this more than once, although very few (2.5%) reported running red lights regularly or fairly often (AAA, 2016).

RLR can have severe consequences when it results in collisions that cause damage to vehicles and road users. While such traffic crashes may cause damage to property only, they can be serious, particularly when colliding at speed into the sides of other vehicles (TRB, 2003). Drivers of vehicles approaching traffic lights may also decide that they have time to cross on a yellow or amber light, resulting in a rear-end collision if the vehicle in front has already slowed and stopped. According to Zahl (1994), eliminating traffic violations could reduce road injury crashes by as much as 40%.

The use of Red-Light Cameras as an Intervention

Intersections are locations on roads that have the potential to create conflict for drivers and pedestrians and increase the risk of crashes. Although intersections make up only a small proportion of roadway systems, a considerable proportion of crashes occur at intersections (Choi, 2010). One way to reduce this conflict is through the use of a traffic control device such as a traffic signal. Traffic signals are designed to identify which vehicles and/or pedestrians have the right of way to pass through an intersection at any given time, thus ensuring orderly movement of traffic, reducing delays for waiting vehicles, and reducing the frequency of vehicular crashes (FHA, 2004a).

Motorists run red lights for a variety of reasons. However, survey research suggests that many drivers consider RLR too frequently be an intentional act that has few legal consequences (FHA, 2004b). There are a number of engineering countermeasures, which
focus on engineering design to reduce RLR. One increasingly popular method of enforcing compliance with traffic signals is through the use of red-light cameras (RLCs).

RLCs are a fully automated photo detection system that includes three key elements: cameras, sensors or triggers, and a computer. The cameras may take still or video images, or both; modern systems generally use digital cameras but some older systems may use 35-mm wet film cameras. In the UK, the cameras are usually placed on one arm of an intersection where a red light running problem has been identified. In the USA the cameras generally are located on all four corners of an intersection, so that vehicles coming from any direction may be photographed from multiple angles. Cameras are activated if the vehicle is moving over the triggers at a predetermined speed; if the vehicle has stopped on an induction loop, or activates only the first of the two triggers, the computer will not signal the cameras. Most systems take at least two photographs and superimpose the date and time of the violation, the location of the intersection, the speed at which the vehicle was travelling and the amount of time that elapsed between the light turning red and the vehicle entering the intersection (FHA, 2004b).

After the RLCs capture images of vehicles as they violate a red traffic signal and the evidence is reviewed, penalty tickets are sent to the address where the violating vehicle is registered. RLCs thus have the potential to reduce traffic law offenses by increasing offenders’ perceptions of the risks of being caught and being brought to justice if they run a red light.

RLCs permit police to remotely enforce traffic signals. Unlike traditional manual enforcement methods which are resource intensive and high risk, RLCs operate continuously and without human intervention, freeing up officers to engage in other activities. They do not lead to potentially dangerous high-speed pursuits and they provide a physical record of all violations (Bochner, 2010). Their mechanical nature also reduces the possibility of accusations of human bias, discrimination, or selective enforcement (Aeron-Thomas, 2005). Studies have shown that drivers who intentionally engage in RLR appear to be most likely to be influenced by countermeasures of this type (FHA, 2004b).

The use of RLCs, however, remains somewhat controversial, particularly in the USA. Some departments have had difficulty sustaining the financial viability of RLC programmes, there have been a number of legal challenges to the use of RLCs, and their effectiveness in reducing RLR and vehicular crashes has been questioned (Langland-Orban, 2008; IIHS, 2016). In the UK, RLCs are generally more accepted as bringing about positive road safety benefits with a rapid growth in their numbers since initial use began in the 1990s (54 red light cameras in use by 1994, rising to 254 by 1996; Hooke, 1996).

Prior Reviews
One previous Cochrane systematic review of the effect of RLCs on the incidence of red-light violations as well as the incidence and severity of road crashes and casualties has been conducted, examining research published in or before 2002 (Aeron-Thomas, 2005). Although no randomized controlled studies were located, a number of controlled before-after studies were identified. The study concluded that RLCs were effective in reducing the total number of casualty crashes but also found that evidence regarding the effect of RLCs on red-light violations, total collisions, or specific types of casualty crashes was inconclusive. The review concluded that larger and better controlled studies were needed.

The Use of EMMIE within Systematic Reviews
This review was conducted in support of the efforts of the What Works Centre for Crime Reduction, which is hosted by the UK College of Policing. The What Works Centre emphasizes the development of an evidence-based approach to policing by coordinating collaborations
among academics and practitioners and creating a programme to foster systematic reviews of research into policing and crime reduction practices. The Centre focuses on providing “robust and comprehensive evidence that will guide decision-making on public spending” (CoP, 2016).

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 and hosted by the What Works Centre. The toolkit uses the EMMIE framework, which assesses interventions based on five key dimensions: effect, mechanism, moderators, implementation, and economic cost (Johnson, 2015).

Effect refers to the effectiveness or impact of the intervention and assesses whether or not the evidence suggests that the intervention led to a change in crime, either an increase or a decrease. Mechanism refers to how the intervention works and what element of the intervention process brought about the effect. Moderators are the context in which the intervention works; this dimension considers the conditions or circumstances that must exist for the intervention to be effective. Implementation focuses on what must be done to put the intervention into practice. Finally, Economics considers both direct and indirect costs associated with the intervention as well as any possible cost benefits to the implementing agency (CoP, 2015).

Contribution of this Review
This systematic review has expanded and updated the previous Cochrane systematic review (Aeron-Thomas, 2005), which only searched a small number of electronic databases and only included a small number of studies published in 2002 or earlier. Since this study was conducted, RLC technology has continued to improve and the use of RLCs has expanded. This updated review involves broader and more extensive searches and incorporates more recent studies from as many countries as possible, as well as carrying out more detailed and extensive meta-analyses and examining economic data when available. Additionally, the review has been expanded to include information from the EMMIE framework. The results of this review have the potential to inform police and government policies and procedures to increase traffic safety.
Objectives

The main objective of this review was to assess the impact of RLCs on the incidence of red-light violations and the incidence and severity of traffic crashes by locating and examining all the major empirical studies on the effect of RLCs on traffic patterns. The update has been expanded by including information under the EMMIE framework (see above) on mechanisms, moderators, implementation and economic costs of RLC interventions. For each study, we have described the setting (e.g., nature of roads), theoretical basis for the intervention, characteristics, 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 (number of red-light violations) and secondary outcomes (e.g., road traffic crashes) to assess the effectiveness of interventions. In addition to examining the impact of RLCs on road traffic crashes overall, the effect on different types of traffic crashes, such as rear end and right angle crashes, was evaluated separately. We have also investigated potential moderators of intervention effects, and summarized the different aspects of implementation of traffic enforcement devices and their respective costs.

Methods

Criteria for Considering Studies for This Review

We considered studies that employed experimental study designs to provide evidence of effectiveness. These included controlled-before-after (CBA) studies, controlled interrupted time series, and randomized controlled trials.

Types of studies

To be eligible for inclusion, a study must have measured the effectiveness of RLCs by comparing intersections that received the treatment (the treatment condition) with intersections that did not (the control condition). Studies were eligible for inclusion if they involved one of the following research designs:

1. Experimental design/randomized controlled trials (RCT): This category included studies that used random assignment to assign intersections to the treatment and control groups.
2. Quasi-experimental design/quasi-RCT: This category included studies that allocated the treatment and control conditions using quasi-random processes, rather than truly randomizing treatment allocation.
3. Controlled before/after (CBA) design: This category included studies in which data are collected for both treatment and control conditions before and after the treatment was initiated.
4. Controlled interrupted time series: This category included studies in which data was collected at multiple separate time points before and after the treatment was initiated.

Qualitative, observational or descriptive studies that did not include formal comparisons of treatment and control groups were excluded from this research.

For all research designs, the non-intervention control condition did not exclude normal routine traffic enforcement by criminal justice system personnel; police still could issue citations for traffic violations at control intersections.
Types of participants
RLCs do not have participants in the standard sense. Essentially, the participants were signalized intersections in the area under study. Intersections where additional interventions were in operation, such as speed cameras or enhanced police enforcement, were excluded. For treatment intersections, RLC enforcement must have applied to all motorists passing through the intersections where the cameras were installed.

Types of interventions
Eligible studies must have tested the effect of RLCs on traffic violations or crashes. An RLC is considered to be a digital or film still and/or video camera that is used to detect red-light violators and identify them so that they may be charged with their violations. Studies that examined RLCs as part of a larger traffic enforcement initiative, specifically those that examined the joint effect of red-light and speed cameras, were excluded.

Studies were included when the interventions included cameras at intersections or junctions that were designed to detect red-light violators. Studies examining area-wide programmes where RLCs operated at some but not all signalized intersections in the community were also included.

Types of outcome measures
Eligible studies had to have measured and reported data on at least one of the following outcome measures:

- Red-light violations, based on the number of vehicles passing through a junction after entering on a red light. Vehicles that enter a junction on a yellow/amber light but do not clear the intersection before the light changes to red are not considered violators.
- Number, severity, and type of road traffic crashes. This may include the number of total crashes, the number of crashes resulting in injury, the number of property damage-only crashes, and the number of specific types of crashes (e.g., rear-end crashes; right-angle crashes).

Data on economic outcomes, including the costs of providing the intervention and the income it generated), and process outcomes (e.g., implementation data) was collected when available.

Types of settings
Studies were eligible regardless of the country in which they were conducted or the form in which they were published. When studies were not published in English, efforts were made to obtain translations. There was only one possibly eligible study for which this was not possible (Giaever, 1998).

No date restrictions were placed on this study. However, RLCs have only been used for traffic enforcement since the 1960s (Retting, 2003); therefore, no research prior to that time could have been conducted.

Search Methods for Identification of Studies

Search strategy
A four-part search strategy was used to locate research meeting the criteria for eligibility.

1. Online electronic bibliographic databases were searched for published evaluations of RLCs (see Appendix A for a list of electronic databases).
2. The websites of a large number of international institutes and research agencies focusing on transportation issues were searched for reports and other grey literature (see Appendix B for a list of websites).
3. The reference lists of published studies were searched to identify additional published and unpublished research studies.
4. A keyword search using Google and Google Scholar was conducted to search for additional grey literature. The first 100 non-sponsored hits of each search were examined.

Electronic searches
For all electronic database and agency website searches conducted by Florida International University, the following keywords were used.

- Red AND light AND camera(s)
- Red-light AND camera(s)
- Red AND light AND violation(s)
- Red-light AND violation(s)
- Traffic AND camera(s)
- Traffic AND violation(s)

These were adapted when necessary to meet specific requirements of individual search engines or to conform to international terminology variations and spelling conventions. Search terms were intentionally general in nature to ensure that searches cast the broadest possible net and that relevant background material was identified.

All electronic databases searched by London School of Hygiene and Tropical Medicine were conducted using the full search strategy as outlined in Appendix C and adapted where required for each database. Lynn O’Mahony at the College of Policing’s National Police Library conducted additional specialised searching.

Data Collection and Analysis
Screening and review process
All studies identified through the LSHTM and National Police Library search process were first exported to the EndNote bibliographic database for de-duplication. Once duplicate records had been removed, records were combined with FIU search results in a spreadsheet to identify and remove further duplications. Once duplicate records were removed, each study was screened to determine if it met basic inclusion criteria, specifically:

1. The study dealt with the use of RLCs to reduce/prevent traffic light violations and/or traffic crashes.
2. The study included both a treatment and a comparison/control group.
3. The study reported results on at least one of the following outcome measures: incidence of red-light violations, incidence of road traffic crashes, severity of road traffic crashes
4. ExTRANeOUS variables were controlled by at least one of the following methods: randomization, matching, or pre- and post-test measures of violations and/or crashes

At least 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 authors and full-texts were then obtained for
all potentially relevant reports of studies. The publication status of the study (unpublished vs. published) did not affect study eligibility.

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
All studies were managed using the EPPI Reviewer 4 software. For the EMMIE framework data extraction, data were coded independently by two review contributors (CP and David Colas Aberg) in EPPI Reviewer, using a standardised data coding set (see Appendix D: EPPI Reviewer standardized data coding set for EMMIE framework). The remaining coding of studies (including study characteristics, study quality and assessment of bias, measurement of effect) was conducted using Microsoft Excel.

Details of study coding categories
All eligible studies were coded on a variety of criteria, including details related to the source of the study (e.g., publication source, title, authors, etc.), study characteristics (methodological type, dates of data collection, etc.), sample characteristics (e.g., size, location); study methods and procedures (selection process, characteristics of treatment and control areas, associated publicity campaigns, etc.), descriptions of the independent and dependent variables (e.g., construct and operationalization), effect size data (if any), adjustment for bias (e.g., regression to the mean, spillover/diffusion), and study conclusions.

Every eligible study was coded by two review authors, using a standardized data extraction instrument (see Appendix E for data items). All disagreements were identified and resolved by discussion with a third review author.

Descriptive analysis
The review includes all studies meeting the inclusion criteria. Descriptive statistics extracted from each study included:

- Study design: This describes study design and quality; risk of bias; data collection methods, types of statistical analyses
- Participants: This describes intervention and controls; setting of study; nature of roads used
- Programme components: This describes type of camera; camera signing practices; publicity campaigns
- Study outcomes: This describes incidence of red-light violations and traffic crashes

Statistical analysis
To facilitate comparisons of studies we defined a standardized 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:
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. 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.

Data synthesis

Results were pooled 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 RLCs were the same in each setting. Where substantial variation in results was identified, we attempted to investigate its source by conducting subgroup analyses. Stratification of the outcome data was only possible where sufficient information on studies was available for grouping. Subgroup analysis was conducted based on study quality and country (USA, Australia or other). 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 tables of the characteristics of studies (Appendix G). We used Stata statistical software (version 14.2) to conduct the meta-analyses.

Assessment of risk of bias/quality in included studies

Spillover (diffusion of benefits)

Spillover occurs when the treatment has an effect outside the targeted area or population. In the case of traffic enforcement, spillover occurs when a safety measure such as an RLC that is placed at one intersection affects driver behaviour at other intersections that do not have RLCs. This may occur because RLC programmes frequently involve not only the placement of cameras but also the posting of warning signs and widespread publicity campaigns. As a result, driver behaviour may be affected throughout the area, rather than just at those intersections that have cameras. To reduce spillover effects, control and comparison sites should be located outside the area affected by RLC programme publicity.

Regression to the mean
Regression to the mean (RTM) refers to a statistical phenomenon that appears when making repeated measurements of the same variable. In general, observations that increase or decrease rapidly over a short period to produce extremely high or low values tend to be followed by values that are closer to the mean (see e.g., Barnett 2005). In the case of traffic enforcement research, safety measures such as RLCs tend to be placed where they can be most effective, which means that they usually are located at intersections with very high rates of traffic violations and/or traffic crashes. These intersections are likely to show lower rates of violations and/or crashes upon later measurement, regardless of whether or not RLCs have been installed, due to the tendency of RTM.

The effect of RTM can be reduced at the study design stage by randomly allocating subjects (intersections) to treatment or control groups. At the data analysis stage, statistical methods such as Empirical Bayes may be used to estimate the size of the effect and to adjust observed measurements for RTM.

**Study quality assessment**

Study quality analysis was based on six dimensions that focused on the design of the study, the analysis of the data, and the contents of the study report. These six dimensions, which include criteria for risk of bias and conform to the requirements set forth by the UK Economic and Social Research Council (ESRC), are:

1. Selection and matching of intervention and control areas
2. Blinding of data collection and analysis
3. Pre- and post-intervention data collection periods
4. Reporting of results
5. Control of confounders
6. Control of other potential sources of bias

See Appendix F for a list of the 17 specific criteria included in each dimension. Each individual criterion statement was scored on whether it was True, False or Unclear. Quality scores were then derived from these indicators (17 in total), which studies categorized as high, moderate, or low quality, as follows:

- **High quality studies** had to meet *all* of the following criteria:
  - True>=14 (must include accounting for both RTM and spillover, and before and after periods of >=1 year)
  - Unclear<=3
  - False=0
- **Moderate quality studies** had to meet all of the following criteria:
  - True>=10 (must include accounting for either RTM or spillover, and before and after periods of >= 1 year)
  - Unclear<=4
  - False<=4
- **Low quality studies** were those that did not meet the criteria above.

Three review authors (EC, SK and CP) performed quality assessment and scoring independently. For the studies identified in the previous review, the same three review authors independently assessed the quality of the included studies. Any discrepancies were resolved by deferment to further review authors (RS and PE). All disagreements were resolved by consensus.
Results

Introduction
This section presents the results of the systematic review and meta-analysis of studies examining RLCs. It is organized around the EMMIE framework which includes measures of effect as well as discussions of the mechanisms through which RLCs are believed to work, the moderating factors that may influence the activation of these mechanisms, various elements that may affect the successful implementation of RLC programmes, and the economic costs and benefits associated with the use of RLCs.

Description of Studies

Results of the search
The search strategy produced a total of 5,708 records after duplicates were removed. Title and abstract screening resulted in the exclusion of 5,587 records, leaving a total of 121 references that were potentially eligible for inclusion in this study. Full-text review of these works identified 28 primary studies that met the inclusion criteria, in addition to the 10 studies that had been identified in the prior Cochrane review. Eight of these newly identified studies (and one study from the previous review) had associated publications, which were subsumed in the primary studies (Cunningham, 2010; Fitzsimmons, 2007; Garber, 2007; Miller, 2006; Persaud, 2005; Sayed, 2007; Shin, 2007; and Retting, 2002 from the previous review). Two newly identified studies reported on more than one independent study area; Fitzsimmons (2007) included separate analyses of RLCs in two cities in Iowa (Council Bluffs and Davenport) and Shin (2007) evaluated independent programmes in two cities in Arizona (Phoenix and Scottsdale). Kloeden (2009) evaluated two RLC programmes in Adelaide that occurred at different times (1988 and 2001).

The full text reports for the 28 newly identified studies (along with their subsumed publications) were coded in detail in EPPI Reviewer and Excel for the EMMIE analysis and are included in the tables of characteristics of included studies (Appendix G). For the previous Cochrane review (Aeron-Thomas, 2005) a naming convention combining authors and location of study was applied to the 10 included studies. For the additional studies identified in this updated review, first author names and years of publication have been retained to identify each study, with a city or year suffix where required. From the 28 additional primary studies, there were 31 separate analyses evaluated in this review.

The search process is diagrammed in Figure 1, which also shows the number of records excluded, with a summary of reasons (further details of some of the excluded studies are also available in Appendix G).
A total of 38 studies were included in this analysis. Of these, 37 were controlled before-after (CBA) studies with a distinct control group (27 newly identified studies plus 10 previously identified in the prior Cochrane review). One of the newly identified studies employed a controlled interrupted time series (ITS) design. No randomized controlled trials were found. See Appendix G for information on the studies included in and excluded from this review. The characteristics of the studies in the previously published Cochrane systematic review are not duplicated in this report (for copyright reasons).

Like the previous review, the majority of the newly identified studies were from the USA (20) and Australia (5); the remainder were from Canada (2) and Singapore (1). Fifteen of the newly identified studies were published in academic journals (seven also had other associated publications); the rest were published as technical reports and/or Master’s theses. The publication dates of the newly identified studies ranged from 1981 to 2016 inclusively; two studies (Andreassen, 1995 and Maisey, 1981) included data from the 1970s. The methods of analysis used by study authors varied greatly. Most studies used simple ratios (as we have proposed in this review), while 12 included Empirical Bayes analyses.

The outcome measures from the 38 primary studies covered a range of definitions and measures of red-light violations, crashes, and injuries. Only three studies reported red-light violations and these studies reported no other outcomes (Chin, 1989; Arup, 1992 and Retting 1999). The rest examined various types of crashes. Most studies reported crash outcomes by crash type and collected crash statistics from official databases of police reports. The types of crashes examined included total crashes (all types combined), rear end crashes, and right angle crashes (or similar outcomes such as turning same roadway crashes; turning different roadway crashes; angle crashes; and turning and right angle crashes). In many studies, crash types were further disaggregated into total number of crashes of a given type (including those involving property damage only), injury crashes, and red-light -running only crashes (identified through police reports where a red-light violation occurred).
Fourteen of the newly-identified studies reported crash outcomes resulting in injury to passengers or other road users (AECOM, 2014; Ahmed, 2015; Burkey, 2004; City of Lubbock, 2008; Cunningham, 2010; Garber 2007; Kloeden, 2009; Kull, 2014; Llau, 2015; Maisey, 1981; Miller, 2006; Sayed, 2007 and Sharpnack, 2009) as did five of the previously-identified studies (Hillier 1993; Mann 1994; Ng 1997; Retting 2002; South 1988). In most cases, injury crashes included total injuries and fatalities. However, Llau (2015) included ‘possible injuries’ in the injury crash counts while Persaud (2005) specifically excluded crashes classified as ‘possible injury’ from the injury class counts. Burkey (2004) examined injury and possible injury crashes separately.

A summary of outcome measures covered by the current and previous reviews is provided in Table 1.

Table 1: Summary of outcome measures

<table>
<thead>
<tr>
<th>Outcome</th>
<th>Number of studies reporting</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>Current review</td>
</tr>
<tr>
<td>Total crashes (inc PDO)</td>
<td>Total</td>
</tr>
<tr>
<td>Right angle</td>
<td>13</td>
</tr>
<tr>
<td>Turning, same roadway</td>
<td>5</td>
</tr>
<tr>
<td>Turning, different roadway</td>
<td>1</td>
</tr>
<tr>
<td>Turning and right angle</td>
<td>3</td>
</tr>
<tr>
<td>Rear end</td>
<td>17</td>
</tr>
<tr>
<td>Property damage only</td>
<td>5</td>
</tr>
<tr>
<td>Violations</td>
<td>2</td>
</tr>
<tr>
<td>Hit pedestrian</td>
<td>2</td>
</tr>
<tr>
<td>Injury outcomes</td>
<td>Total</td>
</tr>
<tr>
<td>Right angle</td>
<td>4</td>
</tr>
<tr>
<td>Turning, same roadway</td>
<td>2</td>
</tr>
<tr>
<td>Turning, different roadway</td>
<td>0</td>
</tr>
<tr>
<td>Turning and right angle</td>
<td>1</td>
</tr>
<tr>
<td>Rear end</td>
<td>4</td>
</tr>
<tr>
<td>Red-light running</td>
<td>Red-light running</td>
</tr>
<tr>
<td>Red-light running right angle</td>
<td>1</td>
</tr>
<tr>
<td>Red-light running rear end</td>
<td>3</td>
</tr>
<tr>
<td>Red-light running injury crashes</td>
<td>2</td>
</tr>
</tbody>
</table>

Risk of Bias in Included Studies

Quality assessment

Four of the 38 studies received a quality assessment rating of high (all newly identified studies), eight received a quality assessment rating of moderate (six newly identified studies plus two previously identified), and 26 received a quality assessment rating of low (18 newly identified studies plus eight previously identified). Table 2 provides a list of the included studies with their quality ratings.
Table 2: Summary of the included studies and quality of evidence

<table>
<thead>
<tr>
<th>Study</th>
<th>Quality</th>
<th>Study design</th>
<th>Spillover</th>
<th>RTM</th>
</tr>
</thead>
<tbody>
<tr>
<td>AECOM 2014</td>
<td>low</td>
<td>CBA, EB</td>
<td>Yes</td>
<td>Yes</td>
</tr>
<tr>
<td>Ahmed 2015</td>
<td>high</td>
<td>CBA, EB</td>
<td>Yes</td>
<td>Yes</td>
</tr>
<tr>
<td>Andreassen 1995</td>
<td>low</td>
<td>CBA</td>
<td>Yes</td>
<td>No</td>
</tr>
<tr>
<td>Arup 1992</td>
<td>low</td>
<td>CBA</td>
<td>No</td>
<td>No</td>
</tr>
<tr>
<td>Burkey 2004</td>
<td>moderate</td>
<td>ITS</td>
<td>No</td>
<td>Yes</td>
</tr>
<tr>
<td>Chin 1989</td>
<td>low</td>
<td>CBA</td>
<td>Yes</td>
<td>No</td>
</tr>
<tr>
<td>City of Garland 2006</td>
<td>low</td>
<td>CBA</td>
<td>No</td>
<td>No</td>
</tr>
<tr>
<td>City of Lubbock 2008</td>
<td>low</td>
<td>CBA</td>
<td>No</td>
<td>No</td>
</tr>
<tr>
<td>Cunningham 2010</td>
<td>moderate</td>
<td>CBA</td>
<td>Yes</td>
<td>Yes</td>
</tr>
<tr>
<td>Fitzsimmons 2007b -Council Bluffs -Davenport</td>
<td>moderate*</td>
<td>CBA</td>
<td>No</td>
<td>No</td>
</tr>
<tr>
<td>Garber 2007</td>
<td>high</td>
<td>CBA, EB</td>
<td>Yes</td>
<td>Yes</td>
</tr>
<tr>
<td>Hobeika 2006</td>
<td>low</td>
<td>CBA</td>
<td>Yes</td>
<td>No</td>
</tr>
<tr>
<td>Hu 2011</td>
<td>low</td>
<td>CBA</td>
<td>Yes</td>
<td>Yes</td>
</tr>
<tr>
<td>Klooden 2009 -1988</td>
<td>low</td>
<td>CBA</td>
<td>No</td>
<td>No</td>
</tr>
<tr>
<td>-2001</td>
<td>low</td>
<td>CBA</td>
<td>No</td>
<td>No</td>
</tr>
<tr>
<td>Ko 2013</td>
<td>moderate</td>
<td>CBA, EB</td>
<td>Yes</td>
<td>Yes</td>
</tr>
<tr>
<td>Kull 2014</td>
<td>low</td>
<td>CBA</td>
<td>Yes</td>
<td>No</td>
</tr>
<tr>
<td>Llau 2015</td>
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<td>Yes</td>
<td>Yes</td>
</tr>
<tr>
<td>Maisey 1981</td>
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<td>CBA</td>
<td>Unclear</td>
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<tr>
<td>Miller 2006</td>
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<td>CBA, EB</td>
<td>Yes</td>
<td>Yes</td>
</tr>
<tr>
<td>Persaud 2005</td>
<td>moderate</td>
<td>CBA, EB</td>
<td>Yes</td>
<td>Yes</td>
</tr>
<tr>
<td>Porter 2013</td>
<td>low</td>
<td>CBA</td>
<td>Yes</td>
<td>No</td>
</tr>
<tr>
<td>Pulugarthu 2014</td>
<td>high</td>
<td>CBA, EB</td>
<td>Yes</td>
<td>Yes</td>
</tr>
<tr>
<td>Richardson 2003</td>
<td>low</td>
<td>CBA, EB</td>
<td>No</td>
<td>Yes</td>
</tr>
<tr>
<td>Ross 2011</td>
<td>low</td>
<td>CBA</td>
<td>No</td>
<td>No</td>
</tr>
<tr>
<td>Sayed 2007</td>
<td>moderate</td>
<td>CBA, EB</td>
<td>Yes</td>
<td>Yes</td>
</tr>
<tr>
<td>Sharpnack 2009</td>
<td>low</td>
<td>CBA</td>
<td>No</td>
<td>No</td>
</tr>
<tr>
<td>Shin 2007 -Scottsdale</td>
<td>low</td>
<td>CBA, EB</td>
<td>Yes</td>
<td>Yes</td>
</tr>
<tr>
<td>-Phoenix</td>
<td>low</td>
<td>CBA</td>
<td>Yes</td>
<td>Yes</td>
</tr>
<tr>
<td>Sohn 2012</td>
<td>low</td>
<td>CBA</td>
<td>No</td>
<td>No</td>
</tr>
</tbody>
</table>

Previously identified studies:

<table>
<thead>
<tr>
<th>Study</th>
<th>Quality</th>
<th>Study design</th>
<th>Spillover</th>
<th>RTM</th>
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</thead>
<tbody>
<tr>
<td>CA SA LA 2002</td>
<td>low</td>
<td>CBA</td>
<td>No</td>
<td>No</td>
</tr>
<tr>
<td>CA SA Oxnard 2002</td>
<td>low</td>
<td>CBA</td>
<td>No</td>
<td>No</td>
</tr>
<tr>
<td>CA SA Sacramento 2002</td>
<td>low</td>
<td>CBA</td>
<td>No</td>
<td>No</td>
</tr>
<tr>
<td>CA SA San Diego 2002</td>
<td>low</td>
<td>CBA</td>
<td>No</td>
<td>No</td>
</tr>
<tr>
<td>Hillier Sydney 1993</td>
<td>low</td>
<td>CBA</td>
<td>No</td>
<td>Yes</td>
</tr>
<tr>
<td>Mann Adelaide 1994</td>
<td>low</td>
<td>CBA</td>
<td>No</td>
<td>No</td>
</tr>
<tr>
<td>Ng Singapore 1997</td>
<td>moderate</td>
<td>CBA</td>
<td>No</td>
<td>Yes</td>
</tr>
<tr>
<td>Retting Fairfax 1999 (1999a)</td>
<td>low</td>
<td>CBA</td>
<td>Yes</td>
<td>No</td>
</tr>
<tr>
<td>Retting Oxnard 2002</td>
<td>moderate</td>
<td>CBA</td>
<td>No</td>
<td>Yes</td>
</tr>
<tr>
<td>South Melbourne 1988</td>
<td>low</td>
<td>CBA</td>
<td>No</td>
<td>Yes</td>
</tr>
</tbody>
</table>

*Only one quality rating was given for each study, even if multiple locations were examined separately with variable methodologies.
EMMIE

Searches found few studies focused on the UK that met the review inclusion criteria. This is considered to be due to the general acceptance of red light cameras as a safety measure in this country. Red light cameras were introduced earlier to the UK (than the USA where the majority of included studies were found) and studies in the 1990s (in particular Hooke, 1996) quickly suggested they were successful in reducing injury collisions cost effectively. This influenced the Department for Transport, police forces and local authorities to utilise this early evidence base to implement red light cameras. Additionally RLC in the UK are now often implemented in conjunction with speed cameras, as traffic safety cameras and as the effects of these functions could not be separated studies on such cameras were excluded from the review. The EMMIE section primarily focuses on the elements present in studies that met inclusion criteria for the review. However, additional relevant sources have been added in some sections to provide supplementary information to ensure relevance for the UK based reader.

Effects of Interventions

Total crashes

24 studies reported 27 estimates of effect on the total number of all types of crashes (including property damage only). Twenty of these studies (accounting for 23 estimates of effect) reported these estimates with confidence intervals or standard errors.

The effects of RLCs on the total number of crashes at intersections were highly heterogeneous ($I^2=90.4$, $p=0.000$). The direction of the estimated effects was also inconsistent. Figure 2 shows the effect sizes (ES) and 95% confidence intervals (CI) for each individual study. When the CI overlaps the reference line (a vertical line located at one on the x-axis), this indicates that the observed effects of RLCs may be due to chance. An overall estimate of the relative change in total crashes at locations with RLCs compared to control sites is represented by a diamond shape at the bottom of Figure 2. This pooled effect estimate of 0.98 indicates a 2% reduction in total crashes. However, as the 95% CI crosses the reference line, ranging from a 9% reduction to a 7% increase in total crashes, the effect is uncertain.
To examine results from all 24 studies graphically, the missing standard errors for the remaining effect estimates were imputed by using the median of the known standard errors (i.e. 0.090). The pooled effect estimate based on the 27 estimates of effect (shown in Figure 3) has little impact on the overall estimate, heterogeneity remained high ($I^2$ 89.0%, $p=0.000$) and the overall reduction of 1% in total crashes remained uncertain (95% CI; 8% reduction to 6% increase).

Figure 2: Effects of red-light cameras on total crashes
Figure 3: Effects of red-light cameras on total crashes (imputed confidence intervals)

Total crashes stratified by study quality

Only one high quality study (Garber, 2007) estimated the effect of RLCs on total crashes, reporting a 29% increase (95% CI; 22-36% increase). A meta-analysis of the moderate-quality studies revealed a 2% increase in total crashes (95% CI: 20% decrease -31% increase) with strong evidence of heterogeneity ($I^2 90.9\%$, $p=0.000$). A meta-analysis of low quality studies reported a 3% decrease in total crashes (95% CI 9% decrease - 3% increase), again with strong evidence of heterogeneity ($I^2 68.3\%$, $p=0.000$).
Figure 4: Effects of red-light cameras on total crashes - stratified by study quality

Total crashes stratified by country

As shown in Figure 5, the subgroup analysis by country indicate no effects of RLCs on total crashes in Australia (95% CI; 6% decrease-7% increase) and uncertain effects in the USA (1% decrease; 95% CI; 11% decrease- 9% increase). In both cases, the CIs overlap the reference line. In Australia, the effect estimates across studies are consistent ($I^2$ 0%, p=0.000) while in the USA, there was much greater heterogeneity ($I^2$ 86.3%, p=0.000).
Injury crashes were examined in 17 studies, providing 18 estimates of effect. Fifteen of these studies (16 estimates) reported confidence intervals. Figure 6 shows that the estimates of effect on total injury crashes were also highly heterogeneous ($I^2 = 93.1\%$). The overall pooled estimate of effect suggests that RLCs reduced total injury crashes by 20% with a 95% CI (32% to 5% decrease). In Llau (2015), the number of injury crashes specifically included possible injuries, this distinction was not made for other studies.
Using the median standard error from these studies (0.112), the additional two estimates (from Kull, 2014 and Sayed, 2007) were added to the pooled analysis which had only a marginal impact on the overall estimate of effect (18% decrease; CI 95% 29%-5% decrease; forest plot not shown).

Several other studies used different criteria to measure injuries and therefore are not included in the pooled analysis. Burkey (2004) reported nonsignificant increases in crashes resulting in severe injuries (10% increase; CI 95%; 20% decrease-50% increase), and significant increases for possible injuries (50% increase; CI 95%; 30%-70% increase). Hu (2011) reported a borderline significant reduction in all fatal crashes at RLC intersections per million population of 17% (CI 95%; 0-30% reduction). Richardson (2003) reported a 29% decrease in the number of people injured at red-light intersections per year (CI incalculable).

**Total injury crashes stratified by study quality**

As there were only two high-quality studies examining total injury crashes, a meta-analysis could not be conducted. These two studies produced very different findings; Garber (2007) reported an increase of 19% (95% CI; 8-30% increase) and Llau (2015) reported a nonsignificant decrease of 12% (95% CI; 23% decrease - 3% increase). A meta-analysis of moderate quality studies suggested a nonsignificant decrease in total injury crashes of 41% (95% CI; 70% decrease to 14% increase) with high heterogeneity ($I^2$ 96.2%, $p=0.000$). A meta-analysis of low quality studies indicated a significant decrease in injury crashes of 16% (95% CI; 27-2% decrease), with evidence of significant heterogeneity ($I^2$ 80.9%, $p=0.000$). These results are shown in Figure 7.
Figure 7: Effects of red-light cameras on total road traffic crashes resulting in injury - stratified by study quality

Total injury crashes stratified by country

As seen in Figure 8, a meta-analysis of studies from the USA reported a decrease in injury crashes of 19%, though the effect was uncertain (95% CI; 40% decrease - 9% increase) and there was evidence of significant heterogeneity ($I^2$ 94.3%, $p=0.000$). A meta-analysis of studies from Australia reported a decrease in injury crashes of 15% (95% CI; 23-6% decrease) with no evidence of heterogeneity ($I^2$ 4.8%, $p=0.000$).
Total property damage only crashes

Seven studies examined property damage only (PDO) crashes, six of which included CIs or standard errors. These are shown in Figure 9. Overall, there was a nonsignificant increase of 5% in PDO crashes (95% CI; 8% decrease to a 20% increase). Using the median standard error of these studies (0.072), one additional study (Sayed, 2007) was added to the pooled analysis and this marginally reduced the magnitude of the overall effect size to a 2% increase which remained nonsignificant (95% CI; 10% decrease to 15% increase; forest plot not shown). There were not enough studies to permit any subgroup analyses by study quality or country.
Figure 9: Effects of red-light cameras on total property damage only crashes.

Total crashes from red-light running

Six studies looked specifically at RLR crashes (producing seven estimates of effect). RLR crashes were identified as those caused directly by a driver running a red-light or failing to yield during a turn on red, or any crash where a red-light violation ticket was issued. The pooled estimates showed a 47% overall reduction in the total number of RLR crashes, although this was nonsignificant (95% CI; 72% decrease - 2% increase; $I^2$ 98.4%; p=0.000)

Figure 10 identifies Fitzsimmons’ Council Bluffs study (2007) as a visual outlier due to the extremely large reductions in red-light running crashes found in this study. As a sensitivity analysis, the meta-analysis was repeated excluding this study; the direction of the overall estimate of effect was unchanged but the estimated reduction in crashes dropped by over half to 18% which was now significant (95% CI; 25-11% decrease). Removing this study also reduced heterogeneity ($I^2$ 14.9%; p=0.320).

When City of Lubbock (2008) was also removed from the pooled analysis, RLR crashes were significantly reduced by 19% (95% CI; 25-11% decrease) and heterogeneity remained low ($I^2$ 17.5%, p=0.304).

Two further studies reported estimates of effect of RLCs on road traffic crashes from RLR. However, these limited ‘total’ crashes to only specific types, rather than all crashes due to RLR. Therefore, they were not considered comparable and were excluded from the pooled analysis. Andreassen (1995) reported a 7% increase in total RLR crashes, which were defined as the sum of pedestrian, right angle, turning (same roadway) and rear end crashes. Cunningham (2010) similarly reported a 5% increase in RLR crashes, which were defined as the sum of turning (both same roadway and different roadways), right angle and rear end crashes.
Total injury crashes from red-light running

Two studies (not enough for a pooled analysis) specifically examined RLR crashes that resulted in injuries. Miller (2006) reported a 34% reduction (95% CI 64-5% decrease) while Garber (2007) reported a nonsignificant increase of 7% (95% CI 18% decrease - 31% increase).

Right angle crashes

Total right angle crashes

Fifteen studies evaluated the effect of RLCs on total right angle crashes (including property only crashes), providing 17 estimates of effect. Twelve of these studies (14 estimates of effect) included confidence intervals. Garber (2007) and Persaud (2005) provided both overall estimates of effect and estimates broken down by jurisdiction; the combined overall effects reported in each of these studies have been included in the pooled analysis as it was considered that the different geographic regions would not be independent. Figure 11 indicates a significant reduction in right angle crashes of 24% at intersections with RLCs compared to those without (95% CI; 35-10% decrease) with evidence of heterogeneity between studies ($I^2$ 94%, p=0.000).

Three studies reported rate ratios without CIs. When these studies were added to the pooled meta-analysis (using the median SE of 0.117), the magnitude and direction of the effect remained similar, with an overall estimate of a 21% reduction (95% CI; 32-9% decrease), and continued high heterogeneity ($I^2$ 92.9%, p=0.000).
Garber (2007) was the only high quality study reporting effects of RLCs on right angle crashes, reporting a 20% increase (95% CI; 9%-32% increase) right angle crashes. However, this study also appears consistently as an outlier reporting higher increases across outcomes. A pooled estimate of three moderate quality studies suggested a nonsignificant decrease of 23% (95% CI; 33% decrease to 13% increase) while a pooled estimate of low quality studies suggested a 31% decrease in right angle crashes (95% CI; 44-16% decrease). These are shown in Figure 12.
**Figure 12:** Effects of red-light cameras on right angle crashes - stratified by study quality.

**Total right angle crashes stratified by country**

As shown in Figure 13, a subgroup analysis by country suggested a nonsignificant decrease of 13% in right angle crashes in the USA (95% CI; 27% decrease - 4% increase) and 37% in Australia (95% CI; 64% decrease - 9% increase). There was evidence of significant heterogeneity in both countries ($I^2$ 91.4%, p=0.000 in the USA and $I^2$ 94%, p=0.000 in Australia).
Figure 13: Effects of red-light cameras on right angle crashes - stratified by country

### Right angle injury crashes

Seven studies reported eight estimates of effect; one of these studies (Kull, 2014) reported effect estimates without CIs. The pooled analysis of studies with reported CIs, which is shown in Figure 14, estimates a significant overall reduction of 29% in right angle injury crashes (95% CI; 42-14% decrease), with moderate evidence of heterogeneity ($I^2$ 59.1%, $p=0.023$).

When Kull (2014) was added to the pooled analysis using the median of known SEs (0.206), there was a similar overall estimated reduction in right angle injury crashes of 28% (95% CI; 39-14% decrease) with moderate heterogeneity ($I^2$ 53.7%, $p=0.02$; forest plot not shown).

<table>
<thead>
<tr>
<th>Study ID</th>
<th>ES (95% CI)</th>
<th>Weight</th>
</tr>
</thead>
<tbody>
<tr>
<td>USA</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Burkey 2004</td>
<td>1.12 (0.95, 1.32)</td>
<td>8.46</td>
</tr>
<tr>
<td>Persaud 2005</td>
<td>0.75 (0.71, 0.80)</td>
<td>9.05</td>
</tr>
<tr>
<td>Cunningham 2010</td>
<td>0.77 (0.61, 0.98)</td>
<td>7.85</td>
</tr>
<tr>
<td>Garber 2007</td>
<td>1.20 (1.09, 1.32)</td>
<td>9.90</td>
</tr>
<tr>
<td>Sharpnack 2009</td>
<td>0.76 (0.49, 1.10)</td>
<td>5.92</td>
</tr>
<tr>
<td>Shin 2007-Phoenix</td>
<td>0.86 (0.68, 1.00)</td>
<td>7.88</td>
</tr>
<tr>
<td>Shin 2007-Scottsdale</td>
<td>0.83 (0.71, 0.97)</td>
<td>8.52</td>
</tr>
<tr>
<td>Sohn 2012</td>
<td>0.69 (0.56, 0.80)</td>
<td>8.04</td>
</tr>
<tr>
<td>Subtotal (I-squared = 91.4%, $p = 0.000$)</td>
<td>0.87 (0.73, 1.04)</td>
<td>63.60</td>
</tr>
<tr>
<td>AUS</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Koedoen 2009-1988</td>
<td>0.82 (0.66, 1.03)</td>
<td>7.95</td>
</tr>
<tr>
<td>Koedoen 2009-2001</td>
<td>0.79 (0.62, 1.01)</td>
<td>7.77</td>
</tr>
<tr>
<td>Maisey 1981</td>
<td>0.17 (0.12, 0.25)</td>
<td>6.62</td>
</tr>
<tr>
<td>Hillier Sydney 1993</td>
<td>0.79 (0.55, 1.13)</td>
<td>6.60</td>
</tr>
<tr>
<td>Mann Adelaide 1994</td>
<td>1.08 (0.74, 1.57)</td>
<td>8.45</td>
</tr>
<tr>
<td>Subtotal (I-squared = 94.0%, $p = 0.000$)</td>
<td>0.83 (0.63, 1.00)</td>
<td>35.40</td>
</tr>
<tr>
<td>Overall (I-squared = 92.5%, $p = 0.000$)</td>
<td>0.78 (0.66, 0.92)</td>
<td>100.00</td>
</tr>
</tbody>
</table>

NOTE: Weights are from random effects analysis
Figure 14: Effects of red-light cameras on right angle crashes resulting in injury.

*Right angle injury crashes stratified by study quality*

No high quality studies and only two moderate quality studies estimated the effects of RLCs on right angle injury crashes. Of the two moderate quality studies, Persaud (2005) provided evidence of a significant decrease of 16% in crashes (95% CI; 25%-5% decrease) while Ng (1997) suggested a nonsignificant decrease of 10% (95% CI; 40% decrease - 35% increase). A pooled estimate of low quality studies, shown in Figure 15, indicated a 40% reduction in right angle crashes (95% CI; 50-29% decrease) with no evidence of heterogeneity ($I^2$ 0%, $p=0.491$).
Figure 15: Effects of red-light cameras on right angle crashes resulting in injury - stratified by study quality.

**Right angle injury crashes stratified by country**

There were not enough studies to estimate the pooled effect of RLCs on right angle crashes in the USA. In Australia, a pooled estimate indicated a significant 43% decrease (95% CI; 53-32% decrease) with no evidence of heterogeneity ($I^2$ 0%, p=0.706), as shown in Figure 16.
Effects of red-light cameras on right angle crashes resulting in injury - stratified by country.

Right angle crashes from red-light running

Only Ko (2013) reported right angle crashes that resulted directly from RLR (including property damage only crashes). This study estimated a significant reduction of 24% (95% CI; 28 to 19% reduction).

Other turning crashes

A number of studies examined different crash types, such as turning, same roadway crashes (where cars approaching from opposite directions on the same road crash when one turns across the path of the other); turning, different roadway crashes; total direct and indirect right angle crashes; and the sum of all turning and right angle crashes at intersections. Most of these outcomes were unique to individual studies and could not be compared to other reported outcomes; however, a few studies reported turning, same roadway crashes. These have been pooled to give an overall estimate of effect.

Total turning, same roadway crashes

Five studies reported turning, same roadway crashes as an outcome with seven estimates of effect. Two of these studies reported effect estimates without CIs (Andreassen, 1995 and Richardson, 2003).

The pooled analysis, shown in Figure 17, estimates no effect of RLCs on turning, same roadway crashes (95% CI; 28% decrease - 40% increase, Figure 17), with substantial heterogeneity ($I^2$ 94.1%, p=0.000).
The addition of the two studies without CI/SE, did not greatly change the overall effect estimate, suggesting a nonsignificant 1% increase (95% CI; 22% decrease - 30% increase).

**Turning, same roadway injury crashes**

Only Kloeden (2009) reported turning, same roadway crashes that resulted in injury with CIs (producing two estimates of effect). However, South (1988) and Kull (2014) also reported estimates of effect for this outcome, so the median SE was imputed (0.256, from Kloeden, 2009) and they were added to a pooled estimate of effect. The meta-analysis indicated a nonsignificant 6% increase in turning, same roadway injury crashes (95% CI; 17% decrease - 35% increase) with no evidence of heterogeneity between studies ($I^2$ 0%, $p=0.000$). This is shown in Figure 18.
Figure 18: Effects of red-light cameras on turning, same roadway crashes resulting in injury (imputed confidence intervals).

Rear end crashes

*Total rear end crashes*

Nineteen studies evaluated the effect of RLCs on total rear end crashes, reporting 21 estimates of effect. 16 of these studies (18 estimates of effect) reported CI/SE. Overall, these 16 studies showed significant evidence for an increase of 19% in rear end crashes with RLCs (95% CI; 9-31% increase). This is shown in Figure 19.
With imputed SEs added for the remaining five estimates of effect (using the median of 0.091), the overall pooled estimate of effect was largely comparable, with an estimated 17% increase in crashes (95% CI; 8%-27% increase; \( I^2 \) 85.7%).

**Total rear end crashes stratified by study quality**

A meta-analysis of three high quality studies found a significant 33% increase in rear end crashes (95% CI; 17%-51% increase). Meta-analyses of moderate and low quality studies both found nonsignificant increases in rear-end crashes. For low quality studies, there was an increase of 9% (95% CI; 2%-22% increase) with evidence of heterogeneity (\( I^2 \) 75.3%, \( p=0.000 \)); for moderate quality studies, there was an increase of 41% (95% CI; 1% decrease - 102% increase) with substantial heterogeneity (\( I^2 \) 94.7%, \( p=0.000 \)).
Figure 20: Effects of red-light cameras on rear end crashes- stratified by study quality.

**Total rear end crashes stratified by country**

A meta-analysis of studies in the USA found a significant 22% increase in rear-end crashes (95% CI: 8-39% increase), with evidence of heterogeneity ($I^2$ 88.1%, $p=0.000$). Pooled estimates from Australia indicated a significant increase of 10% (95% CI: 0-22% increase), with no evidence of heterogeneity ($I^2$ 0%, $p=0.622$). This is shown in Figure 21.
Figure 21:  Effects of red-light cameras on rear end crashes - stratified by country.

**Total rear end injury crashes**

Eight studies (nine estimates of effect) examined total rear end crashes that resulted in injury and of these; seven studies reported eight estimates of effect with CI/SE. The pooled analyses of these eight estimates, which is shown in Figure 22, suggested a nonsignificant decrease in rear end injury crashes of 1% (95% CI; 20% decrease - 23% increase) and moderate heterogeneity ($I^2$ 55.5%, $p=0.028$).

When Kull (2014) was added, using the median of known SEs (0.251), the direction of effect changed to a nonsignificant increase of 1% (95% CI; 17% decrease - 23% increase) and the amount of heterogeneity decreased ($I^2$ 50.1%, $p=0.042$).
Figure 22: Effects of red-light cameras on total rear crashes resulting in injury.

Rear end injury crashes stratified by study quality

One high quality study (Ahmed, 2015) reported a significant 23% increase in rear end injury crashes (95% CI; 23% - 47% increase). The two moderate quality studies presented contradictory results; Persaud (2005) reported a significant decrease of 24% (95% CI; 39% - 5% decrease) while Ng (1997) reported a nonsignificant increase of 6% (95% CI; 36% decrease to 77% increase). A meta-analysis of low quality studies, which is shown in Figure 23, found a nonsignificant 1% reduction in rear-end injury crashes (95% CI; 25% decrease - 31% increase) with no evidence of heterogeneity ($I^2$ 18.6%, p=0.296).
Effects of red-light cameras on total rear crashes resulting in injury- stratified by study quality.

Rear end injury crashes stratified by country

There were not enough studies from the USA to estimate a pooled effect on rear-end injury crashes. In Australia, a meta-analysis found a nonsignificant 1% reduction in rear-end injury crashes (95% CI; 27% decrease - 36% increase) with no evidence of heterogeneity ($I^2 = 39\%$, p. 0.178). This is shown in Figure 24.
Figure 24: Effects of red-light cameras on total rear crashes resulting in injury - stratified by country.

Total rear end crashes from red-light running

Three studies estimated the effect of RLCs on total rear end crashes that were specifically identified as resulting from a red-light violation (four estimates of effect). As seen in Figure 25, a pooled estimate indicated a nonsignificant 6% increase in rear-end crashes from RLR (95% CI; 34% decrease - 69% increase) with evidence of heterogeneity ($I^2$ 82.5%, p=0.000).

Figure 25: Effects of red-light cameras on total rear end crashes from red-light running.
Red light violations

Three studies estimated the effect of RLCs on the number of red-light violations, although only two of these (Arup, 1992 and Retting, 1999) reported CI/SEs. The median of the known SEs (0.315) was used to include an additional study (Chin, 1989). The overall pooled estimate of effect based on the three studies suggested that the presence of RLCs significantly reduced the occurrence of red-light violations by 61% (95% CI; 64% - 56% decrease). There was no evidence of heterogeneity ($I^2$ 0%; p=0.462). This is shown in Figure 26.

![Figure 26: Effects of red-light cameras on red-light violations (imputed confidence intervals).](image)

Summary of effects

This systematic review shows that RLCs resulted in a significant reduction of 61% in red-light violations (95% CI 64-56% reduction), of 24% in right angle crashes (95% CI 35-10% reduction), of 29% in right angle injury crashes (95% CI 42-14% reduction), and of 20% in total injury crashes (95% CI 32-5% reduction; see Table 3). However, RLCs are also associated with a significant increase in rear end crashes of 19% (95% CI 9-31% increase). There was no evidence to suggest that study heterogeneity was consistently explained by either country or study quality, although results were more consistent across outcomes for studies conducted in Australia.
Table 3: Summary of overall effect estimates*

<table>
<thead>
<tr>
<th>Outcome</th>
<th>Overall ES</th>
<th>95% CI</th>
<th>I²</th>
<th>p-value</th>
<th>No. studies (no. estimates)</th>
</tr>
</thead>
<tbody>
<tr>
<td>Total crashes</td>
<td>↓ 0.98</td>
<td>0.91-1.07</td>
<td>90.4</td>
<td>0.000</td>
<td>20 (23)</td>
</tr>
<tr>
<td>Total injury crashes</td>
<td>↓ 0.80</td>
<td>0.68-0.95</td>
<td>93.1</td>
<td>0.000</td>
<td>15 (16)</td>
</tr>
<tr>
<td>PDO crashes</td>
<td>↑ 1.05</td>
<td>0.92-1.20</td>
<td>86.6</td>
<td>0.000</td>
<td>6 (6)</td>
</tr>
<tr>
<td>Total RLR crashes</td>
<td>↓ 0.53</td>
<td>0.28-1.02</td>
<td>98.4</td>
<td>0.000</td>
<td>6 (7)</td>
</tr>
<tr>
<td>Right angle crashes</td>
<td>↓ 0.76</td>
<td>0.65-0.90</td>
<td>94.0</td>
<td>0.000</td>
<td>12 (14)</td>
</tr>
<tr>
<td>Right angle injury crashes</td>
<td>↓ 0.71</td>
<td>0.58-0.86</td>
<td>59.1</td>
<td>0.023</td>
<td>6 (7)</td>
</tr>
<tr>
<td>Rear end crashes</td>
<td>↑ 1.19</td>
<td>1.09-1.31</td>
<td>85.9</td>
<td>0.000</td>
<td>16 (18)</td>
</tr>
<tr>
<td>Rear end injury crashes</td>
<td>↓ 0.99</td>
<td>0.80-1.23</td>
<td>55.5</td>
<td>0.028</td>
<td>7 (8)</td>
</tr>
<tr>
<td>Rear end RLR crashes</td>
<td>↑ 1.06</td>
<td>0.66-1.69</td>
<td>82.5</td>
<td>0.000</td>
<td>3 (4)</td>
</tr>
<tr>
<td>Red light violations</td>
<td>↓ 0.39</td>
<td>0.36-0.44</td>
<td>97.7</td>
<td>0.462</td>
<td>3</td>
</tr>
</tbody>
</table>

*Arrows denote direction of effect, with bold indicating a significant result.

Additional sub group analyses based on whether or not studies accounted for regression to the mean and, whether or not RLC programmes mentioned the use of warning signs, did not consistently explain the heterogeneity between study results (forest plots not shown). However, studies accounting for regression to the mean tended to report more moderate decreases for right angle and right angle crashes resulting in injury outcomes, than studies not accounting for regression to the mean (forest plots in Appendix H).

Mechanisms

Mechanisms focus on how and why an intervention works. There are clearly a number of reasons why drivers may intentionally run red lights, including being in a rush and needing to save time, impatience, and frustration with having to stop (see e.g. Wissinger, 2000; Porter, 2001). A review of the relevant research suggests that the primary mechanism by which RLCs may reduce RLR is deterrence, which involves the use of punishment, or the threat of punishment to discourage people from offending.

Deterrence theory

According to Gibbs (1975), deterrence is “the omission or curtailment of a crime from the fear of legal punishment” (p. 39). Deterrence theory posits that fear of punishment encourages potential wrongdoers to comply with the law. Empirical research examining the effectiveness of traffic lights and traffic regulations on traffic violations suggests that deterrence may result when violation is associated with penalty and the potential for penalty (Cramton, 1969). This suggests that other mechanisms, such as RLCs, may also affect driver behaviour and deter drivers from running red lights.

Porter and Berry (2001) conducted a survey of drivers and found that respondents did not believe that the police would catch most red-light runners, so they felt there were few consequences for this behaviour. However, surveys of communities in which RLC enforcement has been implemented show different results. For example, a survey conducted by Retting and Williams (2000) found that in cities with RLCs, 61% of respondents considered it likely that someone would get a ticket for running a red-light in their city, compared to only 46% of respondents in cities without camera enforcement. Additionally, respondents in cities with cameras were more than twice as likely to know someone who had received a traffic ticket for running a red-light as those in cities without cameras. RLCs not only increase the risk of punishment, and thus the negative consequences of RLR, but
also increase the perception of risk among the general population. This suggests that the mechanism by which RLCs may reduce RLR is deterrence, which involves the use of punishment, or the threat of punishment to discourage people from offending.

It is important to note that deterrence theory assumes that the target behaviour is committed intentionally: drivers may deliberately run red lights or may intentionally try to “beat the light” by speeding up when the light turns yellow. In the latter case, even though drivers may not have actually intended to run the red light, they were aware that their attempt to beat the yellow light created the potential to do so. Deterrence assumes that drivers’ behaviour can be influenced by RLCs, however some drivers may be unable to stop or clear the junction safely at the end of the green signal phase (during the yellow signal phase), and this is referred to as the dilemma zone. This has implications for safety as driver behaviour may be more unpredictable, one driver may choose to stop and become at risk for a rear end collision, while a driver who continues may be at risk of crossing on the red light and at greater risk of a collision at the intersection and potential prosecution (Maxwell, 2006). Enforcement mechanisms that attempt to deter behaviours such as RLR are unlikely to affect situations in which the behaviour is committed inadvertently or unintentionally, such as when a driver was unable to see the signal, was inattentive and did not realise that the traffic light was red, or was unable to stop. Therefore, it is likely that RLCs will be more effective in reducing RLR among drivers who deliberately engage in this behaviour.

Types of deterrence

There are two main types of deterrence. Specific deterrence focuses primarily on punishing apprehended offenders, with the assumption that they will be deterred from reoffending out of a desire to avoid future punishment. General deterrence focuses on the population at large and assumes that the threat of punishment will deter people from initial law violations. The greater the perception of risk of punishment, the greater the likelihood that general deterrence will be effective. To be effective, traffic enforcement policies need to do both, so that a sanction not only impacts the individual who is being punished but also others who do not directly experience the sanction.

As a specific deterrent, RLCs focus on reducing repeat offending by punishing those individuals who offend. Because RLCs are fully automated and operate remotely, without requiring human intervention, drivers who run red lights are more likely to be detected and punished. One purpose of this punishment is to deter these drivers from running red lights again in the future.

General deterrence is achieved through increasing the risk of apprehension and punishment. This may include both the actual or objective risk of being detected and the perceived or subjective risk, which reflects the driver’s belief about the likelihood that they will be detected in a violation. Overall, perceived risk appears to be most likely to influence driving behaviour (Zahl, 1994). RLCs are designed to create what Belin (2010) calls a “feeling of continued surveillance” that suggests a greater risk of apprehension and punishment for running red lights.

In many cases, the primary aim of enforcement strategies such as RLC programmes is general rather than specific deterrence - the focus is on preventing red-light violations rather than on offender detection (see, e.g., MacLean 1985; Zahl 1994). South (1988) suggests that increased enforcement, obtained through the use of RLCs, may deter potential offenders and Retting (2002) also found that the effects of RLCs were not limited to only the specific intersections with cameras, providing further support for a general deterrent effect. Retting (1999, p.173) suggests that “The presence of cameras may promote a general readiness to stop at red lights.”
Spillover: The halo effect

According to Hu (2011, p.277), “A high likelihood of apprehension helps convince motorists to comply with traffic laws.” RLCs are designed to increase the actual risk of apprehension as well as increasing the perception of increased risk, to create a general deterrent effect. However, their impact may not be limited to only those intersections with cameras. RLCs may also create spillover, so that they have a more general effect on driver behaviour at signalized intersections, reducing red-light violations not only at those locations where cameras are placed but also at surrounding non-camera intersections. Retting (2002, p. 1825) suggests that the potential for positive spillover is a key element of RLC programmes because “the goal of highly conspicuous traffic enforcement is to produce generalized changes in driver behaviour with respect to traffic safety laws.”

Attitudinal surveys suggest that RLCs do create an overall perception of increased risk. As discussed above, Retting and Williams found that in cities with RLC enforcement programmes significantly more respondents believed it was likely that someone who runs a red-light would get a ticket. Arup (1992) surveyed drivers in Brisbane, Australia before and after the implementation of an RLC programme. He found that prior to the introduction of RLCs, most drivers did not consider it likely that they would be caught disobeying a red traffic light (only 38% of drivers rated their chances of being caught as “likely” or “very likely”); this increased to 52% after the RLC programme was implemented. He also stated that a survey in Adelaide, Australia after the implementation of RLCs found that 85% of people surveyed believed that RLCs “will make it more likely for drivers to get caught running a red light” and 72% believed RLCs “would change their driving habits.” (p.27)

South (1988) has pointed out that for RLC enforcement to have an effect, motorists must be aware that cameras are in operation. He discussed a study of RLCs in Victoria, Australia in which RLCs were installed and monitored prior to media publicity of the programme. During the period when the public was unaware of the programme, approximately 300 violators per week were photographed by the RLCs; this decreased to about 20 violators per week after the media began publicizing the RLC programme.

To increase the potential for spillover, and therefore increase the effectiveness of the enforcement programme, Ross (2011) suggests the use of jurisdictional boundary signs warning drivers about the use of photo enforcement mechanisms, rather than posting signs at the specific traffic intersections where cameras are installed. Shin (2007) examined the effect of RLCs in two cities in Arizona. In Phoenix, warning signs were placed at all approaches at RLC intersections, while in Scottsdale signs were only placed on some of the intersections where cameras were deployed. The researchers found significantly greater spillover effects in Scottsdale; they suggested that the drivers in Phoenix knew which intersections and approaches had cameras and modified their behaviour accordingly, while drivers in Scottsdale were not as certain of where the cameras were placed and therefore were more likely to be deterred from traffic violations citywide.

In general, empirical tests of the spillover effect of RLCs have yielded inconsistent results. Retting (1999, 2002) found red-light violations in Fairfax and Oxnard decreased at both camera and non-camera intersections; they argued that in both cities the decline at non-camera sites was due to spillover (see e.g., Retting 1999b, p.173). Similarly, Kull (2014) found a decline in angle crash rates in Chicago at intersections with and without RLCs (36 % and 27 %, respectively), and concluded that the decrease in crashes at the non-camera sites was due to spillover. AECOM (2014) reported relatively large spillover effects as well, suggesting that the reduction in collisions at non-RLC intersections was due to changes in driver behaviour that may have resulted from the widespread publicity of the RLC programme throughout the
jurisdiction as well as the public’s lack of knowledge as to which specific intersections were equipped with cameras. Chin (1989) found that the installation of RLCs at intersections in Singapore not only reduced RLR on those approaches covered by the cameras but on other approaches as well.

Conversely, while Hobeika (2006) found a spillover effect of RLCs on property-damage-only crashes in Fairfax, they also concluded that there was no spillover effect for injury crashes. Garber (2007) found that there were no significant changes in various types of traffic crashes at non-camera sites in Virginia that were used to test for spillover.

Moderators

Moderators are “variables that may explain outcomes across different studies” (Johnson, 2015, p.462). Possible moderators could include factors such as road type, intersection geometry, traffic flow, number of lanes, speed limits, weather conditions and country or area level effects. In the meta-analysis, studies were stratified by country (USA and Australia) to examine country-level effects and by study quality. However, there was insufficient data to allow further analysis of other potential moderators in this manner. Some of the individual studies did note, discuss, and in some cases control for a number of possible moderating factors. However, there was only limited evidence regarding their effect on RLC programme outcomes.

Time of day and day of week

Time of day and day of week have been suggested as possible moderators for RLC effects. Retting (1998), Porter (1999), and Kamyab (2000) all found that the incidence of red-light violations is greater on weekdays than weekends. They also found that red-light violations varied by time of day; Retting (1998) and Porter (1999) indicated the average number of red-light runners was greater during the day than at night while Kamyab (2000) reported a higher incidence of red-light violations specifically between 3:00pm and 5:00pm. Fitzsimmons (2007a, p. 10) suggested that these may be related to traffic volumes and congestion levels, pointing out that drivers who are on the road during peak hours may experience delays which may lead to adverse behaviour such as red-light violations, particularly if traffic signals are improperly timed. Conversely, Arup (1992, p.20) found that prior to the installation of RLCs, the number of violations per hour was lower during peak periods (defined as 7:30am to 9:30am and 4:00pm to 6:00pm), regardless of traffic flows. However, they also stated that during these peak periods, signal coordination was designed to favour major traffic flows, reducing delays and decreasing the potential for violations.

It should be noted that some of the studies in the meta-analysis did not collect data around the clock; Chin (1989), for example, only collected data on weekdays and only during specific periods of the day that fell between 8:00am and 7:00pm.

Signal timing

Another possible moderator is signal timing. Retting (1995, p.6) found that longer change intervals, such as by increasing the length of the yellow signal interval, may reduce red-light violations and traffic crashes. Bonneson (2003) reported that RLR rates are higher when the duration of the yellow signal interval is shorter than suggested by traffic engineering equations and Bonneson (2004) found that after increasing the yellow interval duration, there was a decrease in the frequency of red-light-related crashes. Retting (2008) found that longer yellow signal lengths were associated with a 36 % decrease in red-light violations.

Speed limit variations

Variations in the speed limit on the roads used in RLC studies may also serve as a moderator
variable. Some studies in the meta-analysis did control for this, such as Miller (2006) and Fitzsimmons (2007). Miller (2006) only used roads with speed limits ranging from 35 to 55 mph; they pointed out that the effects of RLCs on roads with different characteristics, including speed limits, could vary.

Other potential moderators
Other factors that could influence the results include average daily traffic (ADT), the presence and/or number of left turn lanes (equivalent to a right turn lane in the UK), the size or openness of the intersections (typically larger in the USA and Australia in comparison to the UK), road gradient, and the percentage of heavy trucks in the traffic stream. Additionally, whether the signals at the intersection were actuated, pre-timed, or coordinated may affect driver behaviour. An actuated traffic signal will change its phase if a vehicle is detected while pre-timed signals are pre-programmed to maintain constant signal intervals remain regardless of traffic flow. Coordinated signals are found along major roads with multiple traffic signals and are timed to allow drivers travelling the road at the posted speed limit to have all green signal phases.

A UK study conducted on RLR in Birmingham (Lawson, 1991) reported some ‘average’ characteristics associated with increased RLR, these features may also moderate the effect of RLCs across different studies. It found RLR crashes were relatively common late at night and in the early hours compared with non RLR crashes, particularly on Saturday night and Sunday morning. The study reported that a combination of high speeds, large traffic volumes, and a sense of openness at the intersection could all contribute to increased RLR crashes. A sense of openness could be created by a combination of a downhill gradient towards an intersection, clear visibility through a stretch of road on the intersection exit or the presence of multiple lanes. The report author also noted a great variation in crash patterns between intersections, and within approaches of intersections, and suggested crash history at individual sites, rather than general intersection characteristics, will be the best indicator of where to place RLCs.

Implementation
Implementation involves the context in which RLCs programmes are put into practice. Variations in implementation may influence programme effectiveness, affecting the level of deterrence. The structure and organization of RLC programmes vary greatly by jurisdiction and a number of these variations may affect how driver behaviour is influenced by RLCs, although most were not tested directly by the included studies.

Public awareness and programme publicity
Public knowledge of the implementation of a photo enforcement programme is considered essential for programme success. The use of publicity campaigns to enhance public awareness, as well as posted warning signs (discussed below), are believed to increase the general deterrent effect of the cameras and create spillover from intersections with cameras to the wider area (see e.g., Ross 2011; Shin, 2007). Retting (2002) stated that the primary mechanism for preventing red-light violations is driver awareness and stressed the importance of publicizing RLC enforcement programmes.

In the USA, the Federal Highway Administration (FHA 2004b) states that a critical element for proper implementation of RLCs is an ongoing public education programme. Similarly, in the UK, the Department for Transport (2007, p.9) states, “Every effort should be made to publicize the use of cameras in an area.” The Department suggests that such publicity will enhance the deterrent effect of camera programmes and improve public compliance with
traffic laws.

Many of the included studies specifically mentioned the use of public awareness programmes and media publicity regarding operation of RLCs. Some jurisdictions mailed written notices to local residents to alert them to the new photo enforcement programme. In many cases, the programmes began publicizing the programmes months before the cameras were installed and some even provided specific information as to which intersections were selected for RLC monitoring. To further increase awareness, and possibly enhance specific deterrence, many jurisdictions implemented a warning period prior to beginning formal RLC enforcement. During this period, violators were issued warnings rather than being ticketed. Warning periods, when used, generally lasted 30 days or less (see, e.g., Fitzsimmons 2007; Llau 2015; Porter 2013; Retting 1999; Retting, 2002; Ross 2011).

While none of the included studies specifically examined the impact of publicity programmes on camera effectiveness, many emphasized the importance of these programmes. As Ross (2011, p.9) pointed out, “To maximize the impact of red-light camera enforcement, drivers must be aware of the enforcement...”.

Warning signs

Many of the included studies reported that the jurisdictions under study posted signs to inform drivers that RLCs are in use. Along with publicity campaigns, warning signs are designed to increase driver awareness of the automated enforcement programmes and enhance their deterrent effect.

Warning signs may be posted either at or near the specific intersections at which cameras are installed or at major entrance points to the city (or both). In some cases, the use and placement of signage may be affected by legislative requirements; in the USA, this varies by state. In North Carolina, for example, state statutes required warning signs to be posted on all approaches of any intersection at which an RLC was installed, regardless of which approaches were actually monitored by RLCs (Cunningham, 2010). Conversely, in Oregon, cities operating RLCs are required to post signs on all major routes into the city; Ross (2011) reported that Salem also posted warning signs on each approach where a camera was operating to help increase driver awareness of photo enforcement. In California, warning signs are required but local governments are given the option of placing signs either at each monitored intersection or at all major city or county entrances (see e.g., Retting 1999; Retting, 2002; CASA 2002). However, using the latter option does mean that there is a risk that some major entrances may be overlooked. CASA (2002) reported that in 1999, approximately 700 citations for red-light violations were dismissed in Sacramento after a traffic court ruling found the city had failed to fully comply with the law when installing warning signs. Following this ruling, the city placed warning signs at each RLC-monitored intersection.

The location of warning signs may affect their utility. CASA (2002) suggested that placing warning signs at major entrance points rather than at treated intersections would enhance the deterrent effect of the RLCs across the entire jurisdiction. Kloeden (2009) reported that in Adelaide (Australia) an informal survey found that many people were unaware of the location of RLCs and suggested that the location of the signs, which in Adelaide were frequently placed well back from the intersection being monitored and off to the side of the road, may have contributed to the lack of knowledge about the programme and to the lack of effectiveness of RLCs in reducing traffic crashes in 2001. They suggested that placing warning signs on the far side signal poles might improve the effects of cameras.

Only a small number of the studies examined the impact of signage on RLC effectiveness.
Shin (2007; as discussed in Washington, 2005) stated that angle and left-turn crash reduction benefits from RLCs appeared to be greater at intersections where warning signs were installed than at intersections without signage. Persaud (2005; as discussed in Council, 2005) reported that the use of warning signs at intersections was associated with a smaller benefit than warning signs at both intersections and city limits.

A meta-analysis was conducted to explore the effect of warning signs on total crashes (Figure 27) and total injury crashes (Figure 28) in the primary studies. There was no significant difference in the effect of RLCs on total road traffic crashes, or total road traffic crashes resulting in injury, between the studies that mentioned the use of warning signs and those that did not. However, for both crash types, studies reporting the use of warning signs do show a slightly greater trend towards a reduction in crashes than studies that did not mention the use of warning signs (albeit insignificant).

![Figure 27: Effects of red-light camera on total road traffic crashes – stratified by the use of warning signs](image-url)
Figure 28: Effects of red-light camera on total road traffic crashes resulting in injury—stratified by the use of warning signs

Obstacles to implementation

Very few of the included studies provided any detailed discussion of the types of implementation issues and obstacles that RLC programmes face. However, several key obstacles that were mentioned included problems with contract vendors, issues involved in operating a legally compliant RLC programme, and public attitudes and concerns.

Vendor-related concerns

A vendor is the agency that supplies and operates an RLC system for a community. Sharpnack (2009) reported that numerous issues with the vendor threatened the success of the RLC programme in Costa Mesa, California. Ongoing issues requiring multiple contacts every week with the vendor included inaccurate record keeping, billing errors, technology malfunctions, problems with traffic signal phasing, and performance issues. One serious issue was the inability of the vendor to ensure that the cameras were obtaining clear images of drivers at the RLC intersections; this problem led to a large number of citations being dismissed. Similar issues were reported by other studies (Fitzsimmons, 2007; CASA, 2002).

Legal challenges

A number of studies reported issues relating to the legalities involved in operating an RLC programme, particularly relating to issuing citations. CASA (2002) reported that several local governments in California, including San Diego, Beverly Hills, and San Francisco, have been sued by red-light violators who challenged programme operations.
In the United States, state statutes generally require traffic citations be issued by law enforcement officers. If tickets resulting from RLC enforcement are sent out by a third party, such as the vendor operating the system, this may be a violation of the law. For example, a Florida state statute mandates that only law enforcement officers may issue violations. In 2015, traffic judges in Broward County, Florida, dismissed 24,000 pending RLC tickets, with a value of over $6.3 million. The judges stated that because the videos from the cameras were sent out of state for screening by an Arizona-based vendor, the programme violated Florida law (Ballou, 2015). Similar challenges in other cities and counties around the state have led many jurisdictions to shut down their RLC programmes (Sweeney, 2017a) and in March 2017, the Florida House voted for a state-wide ban of RLCs. The bill still has to pass the Senate before becoming law (Sweeney, 2017b). The same month, the Texas Senate voted to ban the use of RLCs as a method of traffic enforcement in the state; this bill is now pending in the Texas House (Lopez, 2017).

A number of due process concerns have also been raised. ACLU (2011) pointed out that citations generated by RLC enforcement hold the owner or lessee of the motor vehicle liable for the alleged violation regardless of who was actually driving the vehicle at the time, stating “Guilt is presumed over innocence.” Additionally, because of the delay between the actual violation and the receipt of the ticket, which is sent by mail, the owner of the vehicle may have difficulty remembering the event, or even who was actually driving the vehicle on that day. This may make it extremely difficult, if not impossible, to make any reasonable defence to the citation.

In Chicago, Illinois, a class-action lawsuit recently was brought against the city alleging that the city violated due process in the method used to notify drivers of violations and of late fees for failure to pay fines on time. This is part of a larger scandal in the city alleging corruption and mismanagement of the programme, including bribery, inconsistent enforcement, and the use of unfair criteria to issue tickets (Kidwell, 2016).

Public concerns about privacy

A number of studies have cited public concerns regarding the possible invasion of personal privacy as a significant obstacle to RLC compliance (CASA, 2002; Garber, 2007, as discussed in Garber, 2005; Hobeika, 2006 and Ross, 2011). RLCs collect an enormous amount of personal information about the vehicle and the driver, including the precise location of the vehicle at one or more specific dates and times. This information could be used to track an individual’s location, with serious potential legal or even personal repercussions.

While police officers engaged in traditional traffic enforcement would gather the same information, the data would be controlled and protected by the police department. RLCs are operated by for-profit third-party vendors, creating serious public privacy concerns. The ACLU (2011) stressed the “the need for strict controls on what data is collected, how long it is held and by whom, and access to the data by third parties.” The concern is that the data may become accessible not only to traffic enforcement agencies but also to other governmental agencies and officers and to non-governmental entities, and may even become public record. This could result in a serious violation of personal privacy.

Another concern is possible misuse of the photographs taken by the RLCs. While California law prohibits the use of RLC photographs for any purpose other than the prosecution of the motorist for the traffic violation, CASA (2002) reported that nearly all of the local California governments studied had or would use these photos for other purposes as well, including the investigation of unrelated criminal activities and as evidence in court.
Another issue that has been raised is that of the purpose of RLC programmes and the belief that generating revenue for the local government may take priority over improved public safety (see, e.g., Hobeika, 2006). Simons (2016) states that while they may be intended as safety programmes, “increasingly, the public sees them as money-making scams that can actually make roads less safe.” Essentially, to many they are viewed as ‘cash cows’ and money machines. Cunningham (2010; as discussed in Cunningham, 2004) argues that this is a legitimate concern because if conflicts arise between revenue and safety, a system that is revenue-focused will not be able to maximize safety.

Because the vendors are for-profit businesses, they have an incentive to increase ticket revenue, regardless of public safety implications. Similarly, local and state governments may rely on the revenue generated by these programmes. In 2016, a California state senator proposed a bill to reduce the base fine for rolling turn violations in the state. The bill resulted from growing concerns that after installing RLCs in several California cities, these violations, which result in only 0.5% of all traffic crashes, were accounting for nearly all red-light citations. These tickets generated huge sums for the cities; the amount of money cleared by Rancho Cordova from red-light infractions increased by over 100% between 2012 and 2016 (from approximately $73,000 to approximately $742,000) because of the installation of four RLCs. The high fine and associated fees and other penalties, which were primarily intended to apply to much more dangerous violations, have caused severe hardship to lower-income drivers and the bill was not only approved by the state Senate but was supported by the state ACLU, the National Motorists Association, and the California Association of Highway Patrolmen. However, the bill died in the Assembly appropriations committee “after it was determined that it would cause a significant loss of revenue at the state and local levels.” (Flynn, 2016) Programmes that appear to be revenue-driven rather than a mechanism to improve road safety may generate public opposition, making implementation and continued operation more difficult.

Other implementation factors

While included studies lacked contributions from the UK, there are well-established guidelines in this country to aid in the implementation of traffic enforcement camera schemes. The Department for Transport published best practice and guideline documentation in 2007 that includes stipulations and advice for implementing RLCs in the UK (as well as speed cameras and combined red light and speed cameras, known as traffic safety cameras). The guidance is, in part, based on an influential study (Hooke, 1996) conducted in Birmingham, which cited at least four issues that should be considered in local consultation prior to implementing red light cameras schemes;

- Siting - ideally crash history would be reviewed over the preceding three to five years and deployment of RLCs should be based at intersections with an accident history that includes at least one killed or seriously injured accident in the most recent 36 months. Enforcement cameras should only be deployed when other countermeasures are considered inappropriate or not cost effective. Siting of cameras should be agreed between the local authorities and police.
- Signing and publicity - well co-ordinated efforts between local authorities and police with a joint strategy should ensure broadest coverage and consistent message. RLCs should be deployed with appropriate signage and these must be positioned so that they are visible at all times and not obscured. Partners should make every effort to publicise the use of cameras and their road safety purpose. Ongoing publicity is useful to counteract habituation and to maintain drivers’ awareness of detection.
• Operation – cost effectiveness should be measured and while local authorities often bear brunt of capital costs, the resource costs for police should also be considered.
• Enforcement - potential volume of cases arising should be estimated and planned for in advance to ensure they can be dealt with appropriately. In processing offences, fixed penalty notices were found to be significantly more cost effective than court summons.

Economics
Clearly, the various costs and benefits of operating a RLC programme are a key consideration for local communities and there are a variety of economic analyses that may be conducted. The most basic is a simple estimate of the costs required to implement and operate a programme. Communities may also be concerned about the programme’s fiscal viability, which involves comparing implementation and operation costs to the revenue generated by the programme. Additionally, analysts may examine the economic impact of changes in the number of traffic crashes.

Many of the studies did not include any economic information; those that did varied widely in the amount of detail provided. Only one report (South, 1988) considered monetized cost and crashes, but the brief analysis that was conducted excluded revenue from fines. None of the studies conducted a full cost-benefit analysis that included both fiscal viability and societal benefits (including crash costs), and there was insufficient comparable information available to permit anything other than a qualitative synthesis of economic information from the included studies.

Implementation and operational costs
Only a small number of studies provided information on implementation costs, which can include factors such as the capital cost of the RLC equipment, the costs involved in setting up an RLC programme, and operational or running costs.

According to Fitzsimmons (2007), a typical RLC system can cost USD $50,000 or more, depending on both the type of intersection and the number of cameras to be installed. Washington (2005) also estimated the cost of a 35-mm wet film camera system to be around USD $50-60,000, including the camera and housing, pole, and loop detectors, as well as installation, and pointed out that digital systems are significantly more expensive, costing up to USD $100,000.

Operational costs include the ongoing costs involved in running the RLC programme. One of these is the fee charged by the RLC vendor. This can be either a flat monthly fee or a cost per citation issued. Washington (2005) stated that monthly fees tend to be approximately $5,000 per camera system and Fitzsimmons (2007) reported similar monthly fees per intersection. Fitzsimmons (2007) also noted that in Iowa, multiple possible payment structures were implemented. While a flat monthly operating fee was one option, cities could elect to pay the vendor a fee per citation. Depending on the vendor, the fee was either fixed or decreased as the number of citations per day increased.

Shin (2007; as discussed in Washington, 2005) suggested that RLC programmes could be operated as revenue neutral programmes, so that the operating costs are equal to the fines generated from the programme. However, they also pointed out the difficulty in locating published estimates of installation and operating costs.

Fiscal viability
The studies that included economic information focused either on the programme’s fiscal
viability, which involved comparing implementation and operation costs to revenue generated), or the safety and social benefits of the programme, which focused on cost changes resulting from changes in the number of traffic crashes. None of the studies conducted a complete analysis that included both operational costs and societal benefits (including crash costs).

Of the studies that examined fiscal viability, three reported negative economic outcomes (Andreassen, 1995; City of Lubbock, 2009 and Sharpnack, 2009). Andreassen (1995) reported capital costs of at least AU $1.8 million and annual running costs of AU $285,000 to operate 15 RLC sites in Sydney with five cameras. Annual revenue in 1989 was AU $272,000, which did not even cover programme operating costs. As Andreassen (1995) did not report any significant reduction in crashes from the RLC programme, it is clear that the programme was not fiscally viable. Sharpnack (2009) found that between 2003 and 2009, the RLC programme in Costa Mesa, California raised approximately USD $5.7 million. During this period, the city paid approximately USD $6 million to the contract vendor, for a net cost to the city of about USD $300,000, not counting the cost of civilian police personnel who devoted approximately 30 hours per week to RLC programme tasks. Similarly, City of Lubbock (2009) found that between July and December 2007, their RLC programme, which included 11 RLC intersections, reported a monthly deficit ranging from USD $18,858 to USD $47,419.94.

Two studies reported both positive and negative economic outcomes when comparing programme costs and revenue. CASA (2002) examined six RLC programmes in California and reported that three (Fremont, Oxnard and San Diego) were generating cumulative net revenues and three (Los Angeles, Sacramento and San Francisco) were operating with a cumulative deficit. The researchers noted that while vendors provide similar services, they receive different amounts from each local government, which may account for some of the variation in revenue and expenditure. Garber (2005) also found wide variations when they examined the fiscal feasibility of six RLC programmes in Virginia. Two programmes (Arlington and Fairfax City) reported a net revenue, three (Alexandra, Fairfax County and Vienna) reported net deficits, and one (Falls Church) was operating at a break-even point. Fairfax County had the largest annual deficit (an annual net loss of USD $97,811) while Arlington had the largest annual gain (an annual net revenue of USD $12,499). Overall, the researchers concluded that in general the jurisdictions are not generating net revenue from RLC programmes.

Crash safety benefits

Five studies (Garber, 2007; Persaud, 2005, as discussed in Council, 2005; Shin, 2007; South, 1988 and Ross, 2011) examined the economic effect of changes in the number of traffic crashes that resulted from the use of RLCs, separate from the installation and operational costs of the programmes.

Ross (2011) examined the overall cost of crashes at one intersection in Salem, Oregon before and after an RLC was installed and found that the average monthly cost of crashes increased by about 70% during the post-installation period, rising from USD $16,296 to USD $27,738. They noted that during the period prior to camera installation there was a higher percentage of injury crashes and that most of the crashes occurring after the cameras were installed were rear-end crashes, which tend to produce less severe injuries. However, despite this, the overall increase in the average number of crashes per month resulted in an overall greater cost after the cameras were installed.

Garber (2007) focused on the safety impact of cameras in the six programmes in Virginia. They reported that injury crashes increased after camera installation but pointed out that not all crash types are equally severe. To measure the effect of RLCs on net injury severity,
they examined comprehensive crash costs, monetizing rear-end and angle crashes. They found that when looking at injury-only crashes, cameras were associated with increased crash costs in some jurisdictions and decreased costs in others. However, the results of the analysis varied greatly based on the underlying assumptions and the jurisdictions examined.

Three studies (Persaud, 2005, as discussed in Council, 2005; South, 1988 and Shin 2007) found positive economic impacts after RLC implementation. South (1988) reported that crash costs at RLC intersections in Victoria, Australia were 13.8% lower than expected when compared to control sites. This was equivalent to an annual net savings of AUD $30,253 per site (in 1987 dollars). Shin (2007) examined crash costs in Phoenix and Scottsdale, Arizona before and after RLC programme implementation, using national crash cost estimates for various types of crashes. They estimated a mean annual safety benefit of USD $4,504 for the 10 target approaches in Phoenix and USD $684,134 for the 14 target approaches in Scottsdale. The difference is reportedly due to the differential impact of the cameras in the two cities; in Phoenix, the cameras primarily contributed to a decrease in PDO crashes while in Scottsdale, they had a greater impact on reducing injury crashes. Council (2005) conducted cost comparisons on crash changes in seven jurisdictions around the USA. When the jurisdictions were combined, they found an overall economic benefit of about USD $39,000 per intersection per year when PDO crashes were included and $50,000 per intersection per year when PDO crashes were excluded from analysis.

Other economic factors

Early cost benefit analysis studies in the UK identified significant benefits year for RLCs. Hooke (1996) reported the average cost of RLC per site was over £9,000, with average annual costs of £5,600. This study further compared capital, maintenance and prosecution costs with savings in crash reduction and fixed penalty revenues and found seven out of ten areas were able to obtain net benefit within one year, with returns increasing to around twelve times the investment after 5 years. Additional wider reported cost benefits of RLCs included the release of traffic police for other duties and a saving of 1% of each officer's time was reported to be equivalent to a national saving of up to £4million.

**Discussion**

**Summary of Main Results**

This systematic review shows that RLCs can be effective in reducing red-light violations and some types of traffic crashes, particularly right angle crashes, right angle injury crashes, and total injury crashes. However, RLCs also appear to be linked to an increase in rear end crashes. There was no evidence to suggest that study heterogeneity was explained by either country or study quality. The presence or absence of warning signs did not appear to have an impact on RLC effectiveness. There was some evidence that studies accounting for RTM report more moderate decreases for right angle and right angle crashes resulting in injury than studies not accounting for RTM.

**Comparisons with the Previous Cochrane Review**

The original systematic review (Aeron-Thomas, 2015) included only ten studies which were all published during 2002 or earlier and which came from only three countries (the USA, Australia, and Singapore). This updated review has expanded the search to a more comprehensive list of databases and websites and has increased the number of included studies to 38. Additionally, the research located by the updated search tends to be of higher quality. The updated review includes four studies, which have been classified as high quality, while the original review had none; conversely, 80% of the previously identified studies were
of low quality, compared to 64% of the newly identified studies.

The increased number of studies included in the updated review, combined with the addition of a more extensive meta-analysis, has increased the precision of the effect estimates. The findings of this updated review support those of the original review in some areas, but not in all. As reported in the original review, the updated review found that RLCs were effective in reducing total injury crashes. However, while the original review did not find any significant effect of RLCs on various types of crashes, the updated review did report differential effects of RLCs on specific crash types. RLCs were found to be associated with a reduction in right angle crashes and right angle injury crashes and with an increase in rear end crashes.

Additionally, the updated review has incorporated the EMMIE coding system. Although the original review discussed effect sizes, the other dimensions of the EMMIE scheme were not addressed. In contrast, the updated review includes discussions of the underlying mechanisms, potential moderators and implementation factors that may influence RLC effectiveness, and economic costs and benefits of RLC programmes. EMMIE coding additionally found that while spillover (or diffusion of benefits) is reported in a number of studies, the magnitude of this effect is not established and factors that trigger the underlying mechanism of general deterrence (e.g. warning signs and publicity campaigns) have not been featured widely when devising measures of effectiveness of RLC programmes. Practitioners must undertake careful implementation of schemes to ensure legal compliance. Full cost benefit analysis including capital, maintenance, operational costs and revenue alongside societal costs and benefits (including costs/savings associated with increase or decreases in crashes) are lacking in the current literature.

Comparisons with Prior Meta-Analyses

Two previous meta-analyses were conducted on the effects of RLCs on traffic crashes (Erke, 2009; Høye, 2013; note that Høye was an update and replication of Erke). A comparison of the results of this study with the prior meta-analyses was inconsistent.

When including all studies, regardless of quality level, both this study and Erke (2009) reported a nonsignificant decrease in total crashes associated with RLCs while Høye (2013) found a nonsignificant increase. Total injury crashes cannot be compared because the earlier meta-analyses analysed injury and fatal crashes separately while this study merged the two. This study and Høye (2013) both observed a small nonsignificant increase in property-damage-only crashes while Erke (2009) reported no effect of RLCs on this type of crash.

All three studies found that RLCs were associated with a decrease in right angle crashes (nonsignificant in Høye, 2013); similarly, right angle injury crashes were found to decrease in all three studies, although this result was nonsignificant in Erke (2009). In all three meta-analyses, rear end crashes increased with the use of RLCs (nonsignificant in Erke), as did rear end injury crashes (significant in Erke only).

Høye (2013) suggested some evidence that warning signs improve total crash outcomes when used with RLCs. However, this study did not find any consistent pattern or significant differences between those studies that reported the use of warning signs and those that did not.

Both Erke (2009) and Høye (2013) found that those studies that control for RTM report less favourable effects of RLCs; this study found some evidence of this when examining right angle and right angle injury crashes. Erke (2009, p.903) suggests that failing to control for RTM “can lead to an overestimation of the effects of RLCs.” Erke (2009) also reported the likely presence of other moderators of effects of RLC that could not be accounted for in meta-analysis, similar to the findings in this research.
Overall, the comparison of this research with the previous meta-analyses suggests that future research needs to look specifically for other possible factors that may cause RLCs to increase or decrease crashes at particular intersections.

Limitations of the Review Process

One limitation of this review is the lack of high quality studies assessing the effectiveness of RLCs. There were no randomized controlled trials. While the number of studies in the updated review is almost four times that of the original review, only four were classified as high quality and eight were rated as moderate quality. Two-thirds of the studies examined in the updated review were assessed as being of low quality. There is limited geographic coverage, particularly among the high-quality studies. All four of the high quality studies were conducted in the south-eastern portion of the United States (two in Florida, one in North Carolina, one in Virginia). The original review included research from only three countries (the USA, Australia, and Singapore). Aside from the addition of two studies conducted in Canada (Sayed, 2007; AECOM, 2014), the updated review was still limited to research conducted in these countries. Additionally, the lack of methodological rigor, particularly the failure of many studies to account for both spillover and regression to the mean, affects the quality of the evidence in those studies.

The restricted geographical coverage is particularly limiting for practitioners looking for insight into the use of RLCs in the UK. There are a number of differences between roadways in the USA or Australia and the UK, which mean UK practitioners should carefully interpret results from studies included in this review which are largely based in the USA and Australia. Significant differences include the size and geometry of intersections, which tend to cover a greater area and incorporate longer distances from the stop line to intersection exit on the other side. As noted by Lawson (1991), this may contribute to a greater feeling of openness at an intersection which, in turn could increase the propensity of drivers to run red lights. In accordance with UK guidelines it is clear that careful consideration should be given to crash history and specific site characteristics prior to implementation at each potential RLC location. Further differences between the UK and the USA or Australia include the differences between speed limits in urban areas and the potential for cultural responses to violations and enforcement to differ. While these factors have not been discussed in the included studies, the UK guidelines recommend consultation at a local level with all relevant partners to ensure that such locally relevant factors will be recognised and considered prior to implementation. While the geographical coverage is limited in this review, arguably, the overall effect of RLCs is comparable and when implemented in line with rigorous UK guidelines, further benefit could be derived.

Another limitation was that not all included studies examined all outcome measures of interest. Most noticeably, only a small number of studies examined the impact of RLCs on RLR or on crashes that only involved property damage. The operationalization of the outcome measures also varied; for example, whether or not “possible injury” crashes were included in injury crash counts. Not all studies clearly defined the various crash types, making comparisons more difficult. Importantly, due to the limited occurrences, few studies reported fatal and more severe injury crashes as separate outcomes, although these are likely to be of most interest in terms of crash and injury reduction.

Additionally, the results of the meta-analysis may be affected by the high levels of heterogeneity found among the results of the included studies. The measure of heterogeneity was significant for all main outcome measures except red-light violations. This means that the effect sizes within a given outcome lacked consistency. While subgroup analyses were carried out, in most cases heterogeneity remained high; the only exception
was that for some outcome measures, studies conducted in Australia showed little heterogeneity and strong agreement regarding effect sizes.

Suggestions for Future Research

The design of future evaluations of RLC programmes should consider the results of this review. The results of the majority of studies examined in this review were limited by methodological weaknesses, including a failure to adjust for the effect of regression to the mean or spillover. Future research should address these factors. The use of randomized controlled trials also would be an advantage for future studies. Additionally, the possible moderators identified in this review should be controlled for when possible. The effect of variations in implementation of RLC programmes should also be considered.

In the world of crash and injury reduction there is increasingly an emphasis on (fatal and) the severe life-changing crashes as the ones that really matter, rather than on those crashes that result in less severe injuries or damage-only. As crashes resulting in fatal or severe injuries occur more rarely, current primary studies often do not distinguish between different crash types. Further studies that look at the distinction between fatal and severe crashes and minor injury and damage only crashes would be of particular interest.

Particularly in the UK, red light cameras have been accepted as being a cost effective means to reduce injury crashes and as such have been widely introduced. Some examples of current practice in London include the installation (by Transport for London and the Metropolitan Police) of a network or 250 cameras at signalised junctions. These are safety cameras designed to enforce against both red light running and vehicles breaking speed limits on green, to gain maximum safety benefit at high risk junctions. Studies examining effects of such safety cameras could not be included in this review as the effects of speed and red light running enforcement could not be separated. Further studies to look at the benefit of these combined safety cameras could be of interest to examine their potential for additional road safety benefits over and above red light cameras. Additionally, as the numbers and use of red light cameras and safety cameras are increasing, studies undertaking extrapolation to estimate the potential benefits of these measures if cameras were more widespread would be interesting.

Another example of current practice in the UK includes the London Traffic Light Awareness Scheme. This includes education workshops for drivers about the dangers of violating traffic signals to inform and encourage safer driving in the future. Studies to evaluate similar educational strategies that target driver behaviour could be helpful to determine the effectiveness of such schemes.

Implications for Policy and Practice

Several implications for criminal justice professionals, policymakers, and others involved in traffic engineering and enforcement have emerged from this research.

Consideration needs to be given regarding whether the benefits of RLCs outweigh the costs. This review suggests that RLCs are an effective method for reducing red-light violations and some types of traffic crashes, including total crashes involving injury. Given continued increases in traffic volume and increasing limited police budgets, which restrict the amount of time police devote to traffic enforcement, RLCs appear to be a viable way of protecting public health and safety. However, RLCs also are associated with an increase in rear end crashes. The potential benefits of a reduction in traffic violations and in some types of injury crashes should be weighed against the increased risk of other crash types.

In addition, policymakers must consider the economic implications of operating an RLC
programme, including not only the basic costs of installation and operation but also the fiscal viability of the programme and the economic impact of the effects that RLCs have on traffic violations and traffic crashes.

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.

We would like to thank the following:

- Andrew Hutchings, Ph.D. at the London School of Hygiene and Tropical Medicine for statistical advice and assistance
- Phyllis Schultz at Rutgers University and Melissa del Casto at Florida International University for assistance with specialist searching
- Lynn O’Mahony at the College of Policing for searching of National Police Library, and
- David Colas Arberg for assistance with EMMIE coding.
References

References to Studies Included in Updated Review

*AECOM 2014 (Alberta)


*Ahmed 2015 (Florida)*


*Andreassen 1995 (Melbourne)*


*Arup 1992 (Brisbane)*


*Burkey 2004 (Greensboro)*


*Chin 1989 (Singapore)*


*City of Garland 2006 (Garland)*


*City of Lubbock 2008 (Lubbock)*


*Cunningham 2010 (Raleigh)*


*Fitzsimmons 2007 (Council Bluffs and Davenport)


*Garber 2007 (Multi-jurisdiction)*


Hobeika 2006 (Fairfax)


Hu 2011 (Multi-jurisdiction)

*Kloeden 2009 (Adelaide)*

*Ko 2013 (Texas)*

*Kull 2014 (Chicago)*

*Llau 2015 (Miami-Dade County)*

*Maisey 1981 (Perth)*

*Miller 2006 (Fairfax)*


*Persaud 2005 (Multi-jurisdiction)*

Porter 2013 (Virginia)

*Pulugurtha 2014 (Charlotte)*

*Richardson 2003 (Sydney)*

*Ross 2011 (Salem)*

*Sayed 2007 (Edmonton)*


*Sharpnack 2009 (Costa Mesa)*

*Shin 2007 (Phoenix and Scottsdale)*


*Sohn 2012 (Las Cruces)*
References to Studies Included in Previous Review

*CASA 2002 (Los Angeles, Oxnard, Sacramento, San Diego)*


*Hillier 1993 (Sydney)*


*Mann 1994 (Adelaide)*


*Ng 1997 (Singapore)*


*Retting 1999 (Fairfax)*


*Retting 2002 (Oxnard)*


*South 1988 (Melbourne)*


*Indicates the study was included in the meta-analysis*

References to Studies Excluded From This Review

Blais 2015 (France)

**Budd 2011 (Victoria)**


**Dahnke 2008 (Houston)**


**De Pauw 2014 (Flanders)**


**Golub 2002 (San Diego)**


**Guerin 2010 (Albuquerque)**


**Hebert Martinez 2006 (Virginia)**


**Kent 1995 (Melbourne)**


**Loftis 2011 (Houston)**


**Lum 2003 (Singapore)**


**McCart 2014 (Arlington County)**


**McFadden 1999 (Multijurisdiction)**


**MVA 1995 (Strathclyde)**


**Obeng 2008 (Greensboro)**


**Radalj 2001 (Perth)**


**Shah 2014 (Chicago)**


**Smith 2000 (Multijurisdiction)**

Sun 2012 (Shanghai)

STC 2003 (Ontario)

Vanlaar 2014 (Winnipeg)

Wang 2015 (Florida)

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Yoo 2012 (Unknown)

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Aeron-Thomas 2005


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Belin (2010)


Bochner 2010


Bonneson 2003


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Choi 2010


CoP 2015


CoP 2016

Cramton 1969


Department for Transport 2007


Department for Transport 2016


Erke 2009


FHA 2004a


FHA 2004b


FHA 2012


Flynn 2016


Giæver 1998


Gibbs 1975

Høye 2013

Hooke 1996

IIHS 2016

Johnson 2015

Kamyab 2000

Kidwell 2016

Langland-Orban 2008

Lawson 1991

Lopez 2017

MacLean 1985
Massey 2016

Maxwell 2006

NHTSA 2015

Porter 1999

Retting 1995

Retting 1998

Retting 2000

Retting 2003

Retting 2008

Simons, S.-A. 2016

Sweeney 2017a


Sweeney 2017b


TRB 2003


WHO 2016


Wissinger 2000


Zahl 1994

Appendices

Appendix A - Electronic Databases Searched in this Study

The Cochrane Injuries Group Trials Search Co-ordinator (DB) searched the following:

- Cochrane Injuries Group Specialised Register (to 16/03/2016)
- Cochrane Library CENTRAL database (to 16/03/2016)
- Ovid MEDLINE(R), Ovid MEDLINE(R) In-Process & Other Non-Indexed Citations, Ovid MEDLINE(R) Daily and Ovid OLDMEDLINE(R) 1946 to 16th March 2016
- Embase Classic+Embase (OvidSP) 1947 to 16th March 2016

The librarian at the National Police Library searched the following:

- PROQUEST (to 12 June 2015)
- EBSCO (to 12 June 2015)
- Web of Knowledge (to 12 June 2015)
- Heritage (to 12 June 2015)
- National Police Library (to June 2015)

The Florida International University research group (EGC & SK) searched the following (all through December 31, 2015):

- Criminal Justice Abstracts
- Dissertation Abstracts
- EconLit
- EThOS (UK E-Theses Online Service)
- Health and Safety Science Abstracts
- Hein Online
- Interuniversity Consortium for Political and Social Research (ICPSR)
- JSTOR
- LexisNexis: Academic
- National Criminal Justice Reference Service (NCJRS)
- PsycINFO
- ProQuest CJ
- Sociological Abstracts
- Transport Research and Innovation Portal (TRIP)
- Web of STRID - an integrated database combining records from the Transportation Research Board’s Transportation Research Information Services (TRIS) Database and the Organization for Economic Cooperation and Development’s Joint Transport Research Centre’s International Transport Research Documentation SITRD database
- Web of Science
Appendix B - Specialized Websites Searched in this Study
The Florida International University research group searched the following during February 2016:

● AA Foundation for Traffic Safety, USA - www.aaafoundation.org
● American Transportation Research Institute - http://atri-online.org/
● Australia and New Zealand Society of Evidence Based Policing - http://www.anzsebp.com/
● Australian Transport Safety Bureau (ATSB) - https://www.atsb.gov.au
● Center for Evidence-Based Crime Policy, USA - http://cebcp.org/evidence-based-policing/
● Center for Problem-Oriented Policing, USA - http://www.popcenter.org/
● Centers for Disease Control and Prevention (CDC), USA – http://www.cdc.gov/
● CROW, Netherlands – http://www.crow.nl/english-summary
● Department of Transport Planning and Engineering, National Technical University of Athens, Greece - http://www.civil.ntua.gr/departments/transport/
● European Road Safety Observatory (ERSO) - http://ec.europa.eu/transport/wcm/road_safety/erso/index-2.html
● Federal Highway Administration (FHWA), USA - http://www.fhwa.dot.gov/
● Federal Highway Research Institute (BASt), Germany - www.bast.de/EN/Home/
● French Institute of Science and Technology for Transport, Development, and Networks (IFSTTAR) - http://www.ifsttar.fr/en/welcome/
● Institute for Road Safety Research (SWOV), Netherlands – http://www.swov.nl/index_uk.htm
● Institute for Transport Sciences (KTI), Hungary - http://www.kti.hu/index.php/home
● Institute of Transport Economics (TOI), Norway - https://www.toi.no/?lang=en_GB
● Institute of Transportation Engineers (ITE), USA – http://www.ite.org/
● Justice Research and Statistics Association, USA - http://www.jrса.org
● Laboratoire d’Économie des Transports (LET), France - http://www.let.fr/?lang=en
● Police Executive Research Forum, USA - http://www.policeforum.org/
● Police Foundation, USA - http://www.policefoundation.org/
● Rand Corporation, USA - http://www.rand.org/
● Scottish Institute for Policing Research - http://www.sipr.ac.uk/
● Society of Evidence Based Policing, UK - http://www.sebp.police.uk/
● Swedish National Road and Transport Research Institute (VTI) - http://www.vti.se/en/
● Transport Canada (TC) - www.tc.gc.ca/eng/
● Transport Research Board (TRB), USA - http://www.trb.org
● Transport Research Laboratory (TRL), UK - http://trl.co.uk/
● Transport Safety Research Centre (TSRC), UK - http://www.lboro.ac.uk/departments/lds/research/groups/tsrc/
● Vera Institute of Justice, USA - http://www.vera.org/
● VTT Technical Research Centre of Finland, LTD (VTT) - http://www.vttresearch.com/
● World Health Organization (WHO) – http://www.who.int/en/
Appendix C - A Sample of a Full Search Strategy: Medline

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
<table>
<thead>
<tr>
<th>Mechanism</th>
<th>Were any of these mechanisms mentioned?</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>Were any mentioned mechanisms tested?</td>
</tr>
<tr>
<td></td>
<td>Were any intermediate outcome measures taken to assess the activation of the causal mechanism(s) judged to underpin the intervention?</td>
</tr>
<tr>
<td>Moderators</td>
<td>What was the country of implementation?</td>
</tr>
<tr>
<td></td>
<td>What was the setting of the intervention?</td>
</tr>
<tr>
<td></td>
<td>What were the road and traffic characteristics?</td>
</tr>
<tr>
<td></td>
<td>Was the intervention implemented in isolation? Was it stand-alone, as part of a package or unclear?</td>
</tr>
<tr>
<td></td>
<td>Was monitoring/intervention undertaken at specific times? -times of day, season or constant</td>
</tr>
<tr>
<td></td>
<td>What type of technology was used?</td>
</tr>
<tr>
<td>Implementation</td>
<td>What (if any) implementation factors were mentioned?</td>
</tr>
<tr>
<td></td>
<td>Which (if any) implementation factors were stated as necessary for success?</td>
</tr>
<tr>
<td></td>
<td>Who was responsible for deciding the eligibility and location of intervention sites?</td>
</tr>
<tr>
<td></td>
<td>Who was responsible for setting up operating/maintain/ticketing intervention?</td>
</tr>
<tr>
<td></td>
<td>What (if any) obstacles to implementation existed?</td>
</tr>
<tr>
<td></td>
<td>What resources were required?</td>
</tr>
<tr>
<td></td>
<td>What personnel requirements (training, etc.) existed?</td>
</tr>
<tr>
<td>Economics</td>
<td>What was the cost of the programme set-up?</td>
</tr>
<tr>
<td></td>
<td>What were the annual running costs of the program?</td>
</tr>
<tr>
<td></td>
<td>What is the cost of a red light camera?</td>
</tr>
<tr>
<td>Question</td>
<td></td>
</tr>
<tr>
<td>-------------------------------------------------------------------------</td>
<td></td>
</tr>
<tr>
<td>What is the cost of a crash?</td>
<td></td>
</tr>
<tr>
<td>What are the personnel/hours costs?</td>
<td></td>
</tr>
<tr>
<td>What costs are related to direct and indirect benefits?</td>
<td></td>
</tr>
<tr>
<td>What is the cost-benefit ratio? (Cost benefit analysis)</td>
<td></td>
</tr>
</tbody>
</table>
Appendix E - Standardised data extraction form

<table>
<thead>
<tr>
<th>Queries</th>
<th>Responses</th>
</tr>
</thead>
<tbody>
<tr>
<td>Study</td>
<td></td>
</tr>
<tr>
<td>Short name of study (first author/date)</td>
<td></td>
</tr>
<tr>
<td>Linked studies (if any)</td>
<td></td>
</tr>
<tr>
<td>Location (city/country)</td>
<td></td>
</tr>
<tr>
<td>Methodology</td>
<td></td>
</tr>
<tr>
<td>Participant junctions</td>
<td></td>
</tr>
<tr>
<td>Interventions (RLCs)</td>
<td></td>
</tr>
<tr>
<td>Outcome measures</td>
<td></td>
</tr>
<tr>
<td>Signage/publicity (if any)</td>
<td></td>
</tr>
<tr>
<td>Distance used for identifying RLC crashes</td>
<td></td>
</tr>
<tr>
<td>Method of identifying RLR</td>
<td></td>
</tr>
<tr>
<td>Notes</td>
<td></td>
</tr>
<tr>
<td>Final decision (include/exclude)</td>
<td></td>
</tr>
</tbody>
</table>
### Appendix F - Assessment of Quality of Studies

<table>
<thead>
<tr>
<th>Dimension</th>
<th>Criteria</th>
</tr>
</thead>
</table>
| 1) Selection and matching of intervention and control areas | The characteristics of the study and control sites were the same/similar  
There were no changes in the control sites during the study period  
The control sites were not adjacent to the intervention sites  
It is unlikely that the control group received the intervention |
| 2) Blinding of data collection and analysis    | The outcome data were obtained from routine reporting systems (not originally collected for study)  
The analysis was conducted blind to intervention and control groups |
| 3) Lengths of data collection time period pre- and post-intervention | Length of before period is at least 1 year  
Length of after period is at least one year |
| 4) Reporting of results                        | The main findings of the study are clearly described  
The authors report uncertainty due to random variability (confidence intervals)  
Appropriate statistical tests were used to assess the main outcomes reported (p-values) |
| 5) Control of confounders                     | The authors describe potential confounders  
The distributions of confounders in intervention and control sites were assessed and similar  
Do the authors discuss the effect of confounders on the results? |
| 6) Any other potential sources of bias         | Did the study control for potential bias due to regression to the mean?  
Did the study report, or control for ‘spill-over’ effects (e.g. use control sites located away from red-light camera sites and associated publicity)? |
| Were any other sources of bias addressed? |  |
Appendix G - Characteristics of Studies

Included studies
Description of 28 newly identified studies (with additional subsumed studies where appropriate).

### AECOM, 2014 (Alberta, Canada)

<table>
<thead>
<tr>
<th>Methods</th>
<th>CBA with Empirical Bayes analysis. Study period 1995-2008, RLC implementation dates varied between municipalities.</th>
</tr>
</thead>
<tbody>
<tr>
<td>Participants</td>
<td>217 signalized and 47 unsignalized intersections across 5-7 municipalities in Alberta.</td>
</tr>
<tr>
<td>Interventions</td>
<td>76 signalized intersections with RLCs, 141 non camera signalized intersections, 47 non camera unsignalized intersections.</td>
</tr>
<tr>
<td>Outcomes</td>
<td>Total crashes, property damage only crashes, right angle crashes, rear end crashes.</td>
</tr>
<tr>
<td>Notes</td>
<td></td>
</tr>
</tbody>
</table>

### Ahmed, 2015 (Florida, USA)

<table>
<thead>
<tr>
<th>Methods</th>
<th>CBA with Empirical Bayes analysis, minimum of 3 year before and 3 year after period.</th>
</tr>
</thead>
<tbody>
<tr>
<td>Participants</td>
<td>75 signalised intersections in Florida, USA</td>
</tr>
<tr>
<td>Interventions</td>
<td>25 RLC equipped intersections, 50 adjacent non camera spillover sites.</td>
</tr>
<tr>
<td>Outcomes</td>
<td>Total right angle and left turn crashes, total rear end crashes, injury right angle and left turn crashes, injury rear end crashes</td>
</tr>
<tr>
<td>Notes</td>
<td>Safety performance function was calculated using 950 signalized intersections across Florida. Data were extracted only for intersection-related crashes, and crashes associated with drugs and alcohol were excluded. Data presented ‘target approaches’ at intersections and ‘all approaches’.</td>
</tr>
</tbody>
</table>

### Andreassen, 1995 (Melbourne, Australia)

<table>
<thead>
<tr>
<th>Methods</th>
<th>CBA with 5 years before data (1979-1983) and 5 years after data (1985-1989).</th>
</tr>
</thead>
<tbody>
<tr>
<td>Participants</td>
<td>All signalized intersections in the Melbourne metropolitan area.</td>
</tr>
<tr>
<td>Interventions</td>
<td>41 RLC sites (installed in 1984), crashes at all signalized Melbourne Metropolitan intersections used as comparison.</td>
</tr>
<tr>
<td>Outcomes</td>
<td>Pedestrians hit crossing roads, right angle crashes, rear end</td>
</tr>
<tr>
<td>Notes</td>
<td>Selection of sites was from a list of signals ranked by right angle casualty crashes. 75% of sites had initial average frequencies of 2 or less crashes/year. Total crashes used in comparison includes the crashes at RLC sites.</td>
</tr>
</tbody>
</table>
### Arup, 1992 (Brisbane, Australia)

<table>
<thead>
<tr>
<th><strong>Methods</strong></th>
<th>CBA. Before data collected between 19th-30th November 1990, after data collected between 27th November – 10th December 1991. Data collected during peak (7.30-9.30am and 4-6pm weekdays) and off peak (all other times) hours.</th>
</tr>
</thead>
<tbody>
<tr>
<td><strong>Participants</strong></td>
<td>12 signalized intersections around Brisbane.</td>
</tr>
<tr>
<td><strong>Interventions</strong></td>
<td>6 RLC sites, 6 non camera control sites.</td>
</tr>
<tr>
<td><strong>Outcomes</strong></td>
<td>Red light violations</td>
</tr>
<tr>
<td><strong>Notes</strong></td>
<td>Before/after data periods very short</td>
</tr>
</tbody>
</table>

### Burkey, 2004 (North Carolina, USA)

<table>
<thead>
<tr>
<th><strong>Methods</strong></th>
<th>Controlled interrupted time series over 57 months, including at least 26 months of before data and 22-31 months of after data.</th>
</tr>
</thead>
<tbody>
<tr>
<td><strong>Participants</strong></td>
<td>303 signalized intersections in Greensboro, North Carolina</td>
</tr>
<tr>
<td><strong>Interventions</strong></td>
<td>18 RLC equipped intersections, 285 non camera signalized control intersections.</td>
</tr>
<tr>
<td><strong>Outcomes</strong></td>
<td>Total crashes, severe injury crashes, possible injury crashes, property damage only crashes, right angle crashes, left turning different roadway crashes and rear end crashes.</td>
</tr>
<tr>
<td><strong>Notes</strong></td>
<td>Crashes only included if they result in either an injury or USD $1,000 worth of property damage.</td>
</tr>
</tbody>
</table>

### Chin, 1989 (Singapore)

<table>
<thead>
<tr>
<th><strong>Methods</strong></th>
<th>CBA. Before data collected between 19th January-17th February 1987, after data collected between 14th April-26th May 1987 for one day at each intersection.</th>
</tr>
</thead>
<tbody>
<tr>
<td><strong>Participants</strong></td>
<td>16 intersections in Singapore, with appropriate vantage point (e.g. tall building) from which to operate video camera of intersection.</td>
</tr>
<tr>
<td><strong>Interventions</strong></td>
<td>11 RLC intersections and 5 non camera intersections.</td>
</tr>
<tr>
<td><strong>Outcomes</strong></td>
<td>Red light violations.</td>
</tr>
<tr>
<td><strong>Notes</strong></td>
<td>Before/after data periods very short. One day survey at each intersection only, data only collected on dry weekdays.</td>
</tr>
</tbody>
</table>

### City of Garland, 2006 (Texas, USA)

<table>
<thead>
<tr>
<th><strong>Methods</strong></th>
<th>CBA with 31 months before data and 31 months after data, RLCs implemented on 16th September 2003.</th>
</tr>
</thead>
<tbody>
<tr>
<td><strong>Participants</strong></td>
<td>4 high volume intersections in Garland, Texas.</td>
</tr>
</tbody>
</table>
### Interventions
- 4 RLC intersections with wet film cameras on one approach road, 6 similar control intersections in Garland.

### Outcomes
- Total crashes, red light runner total crashes, rear end crashes

### Notes

#### City of Lubbock, 2008 (Texas, USA)

<table>
<thead>
<tr>
<th>Methods</th>
<th>CBA with data collected Jul-Dec for 2005, 2006 and 2007. Two periods of 6 months before data (Jul-Dec 2005 and 2006), and one 6 month period of after data (Jul-Dec 2007).</th>
</tr>
</thead>
<tbody>
<tr>
<td>Participants</td>
<td>17 intersections in Lubbock City Council, Texas.</td>
</tr>
</tbody>
</table>

### Interventions
- 11 RLC intersections, 6 non camera comparison intersections.

### Outcomes
- Total crashes, RLR crashes (resulting from red light violations), rear end crashes, total injury crashes.

### Notes
Based on a City Council Worksession presentation with few study details.

#### Persuad, 2005 (also Council 2005; CA, MD, NC, USA)

<table>
<thead>
<tr>
<th>Methods</th>
<th>CBA with Empirical Bayes analysis. Average before period of 6 years and average after period of 2.76 years.</th>
</tr>
</thead>
<tbody>
<tr>
<td>Participants</td>
<td>836 intersections across 7 jurisdictions around the USA (El Cahon, San Diego, San Francisco, CA; Howard County, Montgomery County and Baltimore, MD; and Charlotte, NC) with identified RLC problems in mid-late 1990s.</td>
</tr>
</tbody>
</table>

### Interventions
- 132 RLC treatment sites (between 6-31 in each jurisdiction), 296 unsignalized non camera comparison sites (around 40-50 in each jurisdiction) similar to RLC sites and 408 signalized non camera reference sites (to calibrate safety performance functions and investigate spillover, 34-86 in each jurisdiction).

### Outcomes
- Total right angle crashes (right angle, broadside or turning crashes from perpendicular approaches), right angle definite injury crashes (exclude ‘possible’ injury), rear end crashes, rear end definite injury crashes.

### Notes
Injury crashes include fatal and definite injury, exclude possible injury crashes

#### Cunningham, 2004 (North Carolina, USA)

<table>
<thead>
<tr>
<th>Methods</th>
<th>CBA with 5 years of before data and less than one year of after data.</th>
</tr>
</thead>
<tbody>
<tr>
<td>Participants</td>
<td>24 signalized intersections in Raleigh, North Carolina</td>
</tr>
</tbody>
</table>

### Interventions
- 12 intersections equipped with RLCs

### Outcomes
- Total crashes, angle crashes, rear end crashes
### Notes

Results may be affected by seasonality as there is not a full year of data in the after period. Comparison group with halo effect methodology also included to control for spillover (using subset of comparison sites on same roadway as treated sites).

### Cunningham, 2010 (North Carolina, USA)

<table>
<thead>
<tr>
<th>Methods</th>
<th>CBA with 5 years of before data and 46 months of after data.</th>
</tr>
</thead>
<tbody>
<tr>
<td>Participants</td>
<td>14 intersections in Raleigh, North Carolina</td>
</tr>
<tr>
<td>Interventions</td>
<td>6 intersections equipped with RLCs</td>
</tr>
<tr>
<td>Outcomes</td>
<td>Total crashes, angle crashes, rear end crashes, red light running related crashes (sum of right angle, rear end and left turn crashes), injury crashes</td>
</tr>
<tr>
<td>Notes</td>
<td>Comparison group with halo effect methodology also included to control for spillover (using subset of comparison sites on same roadway as treated sites).</td>
</tr>
</tbody>
</table>

### Fitzsimmons 2007a, 2007b (Council Bluffs and Davenport, Iowa, USA)

#### Council Bluffs

<table>
<thead>
<tr>
<th>Methods</th>
<th>CBA with variable amounts of before and after data.</th>
</tr>
</thead>
<tbody>
<tr>
<td>Participants</td>
<td>9 signalized intersections in Council Bluffs, Iowa</td>
</tr>
<tr>
<td>Interventions</td>
<td>5 RLC intersections (7 intersection approach roads)</td>
</tr>
<tr>
<td>Outcomes</td>
<td>Total crashes, rear end crashes, RLR crashes (identified from crash report as ran a red signal)</td>
</tr>
<tr>
<td>Notes</td>
<td>Further cross sectional analysis in Clive, Iowa not extracted</td>
</tr>
</tbody>
</table>

#### Davenport

<table>
<thead>
<tr>
<th>Methods</th>
<th>CBA with Empirical Bayes analysis, variable amounts of before and after data.</th>
</tr>
</thead>
<tbody>
<tr>
<td>Participants</td>
<td>9 signalized intersections in Davenport, Iowa</td>
</tr>
<tr>
<td>Interventions</td>
<td>4 RLC intersections (6 intersection approach roads)</td>
</tr>
<tr>
<td>Outcomes</td>
<td>Total crashes, rear end crashes, RLR crashes (identified from crash report as ran a red signal)</td>
</tr>
<tr>
<td>Notes</td>
<td>Further cross sectional analysis in Clive, Iowa not extracted</td>
</tr>
</tbody>
</table>

### Hallmark, 2010 (Davenport, Iowa, USA)

<p>| Methods         | CBA with Empirical Bayes analysis, variable amounts of before and after data. |</p>
<table>
<thead>
<tr>
<th>Participants</th>
<th>9 signalized intersections in Davenport, Iowa</th>
</tr>
</thead>
<tbody>
<tr>
<td>Interventions</td>
<td>4 RLC intersections (6 intersection approach roads), cameras installed in 2004, 5 non camera control intersections in Davenport with similar traffic volumes, crash frequencies and roadway types.</td>
</tr>
<tr>
<td>Outcomes</td>
<td>Total crashes, RLR crashes, RLR rear end crashes (identified from crash reports as ran a red signal)</td>
</tr>
<tr>
<td>Notes</td>
<td></td>
</tr>
</tbody>
</table>

Garber, 2007 (also Garber 2005; North Virginia, USA)

<table>
<thead>
<tr>
<th>Methods</th>
<th>CBA with Empirical Bayes analysis of 3500 crashes over 7 years.</th>
</tr>
</thead>
<tbody>
<tr>
<td>Participants</td>
<td>72 signalized intersections over 6 jurisdictions in Northern Virginia</td>
</tr>
<tr>
<td>Interventions</td>
<td>28 RLC intersections and 44 control intersections, with an additional 15 spillover sites (within 0.6 miles of RLC intersection)</td>
</tr>
<tr>
<td>Outcomes</td>
<td>Total crashes, injury crashes, angle crashes, rear-end crashes, RLR crashes (driver charged with RL violation), injury RLR crashes</td>
</tr>
<tr>
<td>Notes</td>
<td>Employed 4 difference analyses with only EB extracted (paired t-test, analysis of variance, generalized linear modelling and Empirical bayes)</td>
</tr>
</tbody>
</table>

Hobeika, 2006 (also Yaungyai, 2004; Fairfax County, Virginia, USA)

<table>
<thead>
<tr>
<th>Methods</th>
<th>CBA with 1-2 years of before data and 1-2 years of after data.</th>
</tr>
</thead>
<tbody>
<tr>
<td>Participants</td>
<td>14 intersections in Fairfax County, Virginia.</td>
</tr>
<tr>
<td>Interventions</td>
<td>13 RLC intersections, 4 non camera control intersections (away from RLC zone with high numbers of crashes).</td>
</tr>
<tr>
<td>Outcomes</td>
<td>Total crashes, fatal crashes, injury crashes, property damage only crashes</td>
</tr>
<tr>
<td>Notes</td>
<td>(Yaungyai 2004 is MSc Thesis by 2nd author). No appropriate data for extraction, with some unexpected % changes e.g. Table VI PDOS at site 9, would require more data to include</td>
</tr>
</tbody>
</table>

Hu, 2011 (USA)

<table>
<thead>
<tr>
<th></th>
<th></th>
</tr>
</thead>
<tbody>
<tr>
<td>Participants</td>
<td>62 large cities (&gt;200,000 residents in 2008) with and without RLC programs in USA</td>
</tr>
<tr>
<td>Interventions</td>
<td>14 cities identified with RLC enforcement for all of after period and none of before, 48 cities identified with no RLC enforcement in before or after periods.</td>
</tr>
</tbody>
</table>
| Outcomes | Citywide per capita rate of fatal red light running crashes (one driver assigned ‘failure to obey traffic
<table>
<thead>
<tr>
<th>Methods</th>
<th>CBA with 4 years before and 4 years after data for RLCs installed in 1988 and 1 year before and 1 year after data for RLCs installed in 2001.</th>
</tr>
</thead>
<tbody>
<tr>
<td>Participants</td>
<td>All signalized intersections in Adelaide metropolitan area from mid-1988.</td>
</tr>
<tr>
<td>Outcomes</td>
<td>Total crashes, total injury crashes, right turn crashes, right turn injury crashes, right angle crashes, right angle injury crashes, rear end crashes, rear end injury crashes, other injury crashes.</td>
</tr>
<tr>
<td>Notes</td>
<td></td>
</tr>
</tbody>
</table>

**Ko, 2013 (Texas, USA)**

<table>
<thead>
<tr>
<th>Methods</th>
<th>CBA with Empirical Bayes analysis. Before data collected between 1 and 4 years before and after RLC implementation. Total of 516 and 663 intersection years before and after data respectively.</th>
</tr>
</thead>
<tbody>
<tr>
<td>Participants</td>
<td>311 signalized intersections across 32 jurisdictions in Texas.</td>
</tr>
<tr>
<td>Interventions</td>
<td>RLCs at 245 signalized intersections, 66 reference non camera intersections (at least 2 miles from treated site to minimize spillover).</td>
</tr>
<tr>
<td>Outcomes</td>
<td>Total RLR crashes, rear end RLR crashes, right angle RLR crashes (RLR crashes identified from crash reports if it included one of following factors: disregard of stop-go signal, failure to yield the right-of-way during a turn on red).</td>
</tr>
<tr>
<td>Notes</td>
<td>Outcomes are all specific to RLR crashes identified from crash reports (not crash type).</td>
</tr>
</tbody>
</table>

**Kull, 2014 (Chicago, Illinois, USA)**

<table>
<thead>
<tr>
<th>Methods</th>
<th>CBA conducted between 2005-2012 with minimum of 1 years before and 1 year after data. Median RLC ‘go live’ data of 31st July 2007, use for before/after distinction for control group.</th>
</tr>
</thead>
<tbody>
<tr>
<td>Participants</td>
<td>All signalized intersections in Chicago.</td>
</tr>
<tr>
<td>Interventions</td>
<td>170 RLC signalized intersections, all other signalized non camera intersections in Chicago used as control.</td>
</tr>
<tr>
<td>Outcomes</td>
<td>Angle injury crashes, rear end injury crashes, turning injury crashes, total injury crashes.</td>
</tr>
<tr>
<td>Notes</td>
<td>Changes in reporting in 2009 raised the minimum cost for PDO reporting from $500 to $1,500; therefore only crashes featuring at least one injury were used.</td>
</tr>
</tbody>
</table>
### LLau, 2015 (Florida, USA)

**Methods**
CBA with Empirical Bayes analysis using 3 years before data (2008-2010) and 2 years after data (2011-2012).

**Participants**
60 signalized intersections in Miami-Dade County, Florida

**Interventions**
20 intersections with RLCs and 40 non camera comparison sites. 2 comparison sites matched for each treated site, all at least 2 miles away.

**Outcomes**
Right angle and turning crashes, rear end crashes, all injury crashes (includes possible injuries), injury right angle and turning crashes. (RLR related crashes defined as the sum of right angle and turning crashes).

**Notes**

### Maisey, 1981 (Perth, Australia)

**Methods**
CBA, with two 1 year periods of before data (Jul 1977-Jul 1979) and 1 year of after data (Jul 1979-Jul 1980).

**Participants**
10 signalized intersections in Perth.

**Interventions**
1 RLC signalized intersection (4 way approach) and 9 non camera, signalized, 4 way control intersections.

**Outcomes**
Total crashes, rear end crashes, right angle and indirect (turning) right angle crashes, injury crashes.

**Notes**
Study also looked separately at total crashes during daylight vs darkness.

### Miller, 2006 (also Khandelwal, 2005; Fairfax County, Virginia, USA)

**Methods**
CBA with Empirical Bayes analysis. Data collected between 1998 and 2003. RLC installation began in 2000, varied before and after periods with a minimum of 2.8 years before and 0.8 years after.

**Participants**
36 sites in Fairfax County, Virginia

**Interventions**
13 RLC sites and 33 non camera sites selected based on recommendations from Virginia Department of Transportation.

**Outcomes**
Total crashes, injury crashes, injury crashes attributable to RLR, rear end crashes attributable to RLR, total crashes attributable to RLR. (A crash was categorized as RLR if collision type was coded as ‘disregarded stop-go light’ or if the narrative in crash report clearly states a driver ran red light).

**Notes**
Crash data for RLC sites obtained from routine databases, crash data for comparison sites from manual examination of crash reports forms.

### Porter, 2013 (Virginia, USA)
Methods | CBA with two time points before RLCs switched off and 4 points after switch off.
Participants | 8 signalized intersections in Virginia.
Interventions | 4 RLC equipped intersections, 2 non camera spillover controls in Virginia Beach, and 2 non camera controls in Newport News, Virginia.
Outcomes | Proportion of last drivers (those entering the intersection before oncoming traffic) who ran red lights.
Notes | This study estimated the effect of turning RLCs off, rather than turning them on, and is therefore not included in the data extraction.

Pulugurtha, 2014 (Charlotte, North Carolina, USA)

Methods | CBA with Empirical Bayes analysis. Before data varied from 1.5-3.5 years, after data varied between
Participants | 80 signalized intersections in Charlotte, North Carolina.
Interventions | 32 RLC intersections and 48 signalized non camera intersections (similar to treated intersections).
Outcomes | Total crashes
Notes | Includes some after data for different crash types, only total crashes included in EB analysis and extraction.

Retting, 1999b (Oxnard, CA, USA) - subsumed in Retting, 2002 included in previous review

Methods | CBA. Around 24 hours before and 24 hours after data collected for non camera sites and between 120 and 241 hours of both before and after data collected at RLC sites.
Participants | 12 intersections in Oxnard and 2 control intersections in Santa Barbara.
Interventions | 9 RLCs and 3 non camera spillover sites in Oxnard, with 2 non camera controls in Santa Barbara.
Outcomes | Red light running violations.
Notes | Very short before and after periods of data collection. Data collected using video cameras deployed by researchers at sites. Data extracted for control sites.

Richardson, 2003 (Sydney, Australia)

Methods | CBA with Empirical Bayes analysis. RLCs installed between Jan 1988 and Oct 1995, before data collected to 5 years prior to RLC installation through to 2001.
Participants | 16 intersections in Sydney.
Interventions | 8 RLC and 8 control intersections. Control sites located close to RLC locations.
| Outcomes | Annual total crashes, right angle crashes (perpendicular approaches), turning crashes (turning, same roadway), rear end crashes, other crashes. |
| Notes |   |

**Ross, 2011 (Salem, Oregon, USA)**

| Methods | CBA with 50 months of before data (Jan 2004 – Feb 2008) and 21 months after data (Apr 2008 – Dec 2009) with RLCs installed March 2008 (excluded from analysis). |
| Participants | 3 intersections in Salem, OR. |
| Interventions | 1 RLC intersection (with cameras at 2 approaches), 2 non camera intersections nearby. |
| Outcomes | Average monthly total crashes (all within 300ft of intersection analysed). |
| Notes | Other analyses conducted but before and after data only available for average monthly crashes. |

**Sayed, 2007 (also Sayed, 2006; Edmonton, Canada)**

| Methods | Collision prediction models and Empirical Bayes analysis. 3 years of before data and 2-3 years of after data, RLCs implemented between 1999 and 2002. |
| Participants | 25 RLC sites, 26 comparison group locations, 100 reference group sites. |
| Interventions | 25 RLCs at typical 4 way intersections (3-4 lanes on each approach and 50km/hr speed limit). 46 non camera control sites (with same traffic and environmental conditions as treated sites), some located close to treated sites so could not account for spillover. 100 reference group sites (similar characteristics to treated sites) used to develop collision prediction model. |
| Outcomes | Total crashes, severe crashes (fatal and injury crashes), property damage only crashes, right angle head on crashes, rear end crashes. |
| Notes | Analysis carried out on 25 sites, one site appeared to be an outlier and analysis was also conducted on 24 sites, extracted only data for the evaluation of all 25 sites. |

**Sharpnack, 2009 (Costa Mesa, California, USA)**

| Participants | 6 intersections in Costa Mesa. |
| Interventions | 2 RLCs and 4 non camera control intersections. |
| Outcomes | Total crashes, injury crashes, broadside crashes (right angle), rear end crashes |
| Notes |   |
Shin, 2007 (also Washington, 2005; Arizona, USA)

Four different methodologies used in 2 cities in AZ; simple before/after study, before/after study with traffic flow correction, before/after study with comparison group and Empirical Bayesian before/after study. CBA method used in Phoenix and CBA with EB in Scottsdale:

Phoenix, AZ - CBA

<table>
<thead>
<tr>
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</thead>
<tbody>
<tr>
<td>Participants</td>
<td>23 intersections in Phoenix</td>
</tr>
<tr>
<td>Interventions</td>
<td>10 RLC intersections and 13 non camera comparison intersections in Phoenix. Comparisons selected on basis of similarity to treated intersections.</td>
</tr>
<tr>
<td>Outcomes</td>
<td>Angle crashes, left turn crashes, rear end crashes.</td>
</tr>
<tr>
<td>Notes</td>
<td>Crashes involving factors such as alcohol, drugs, illness or fatigue were removed. Target crashes within 30m from intersection determined to be red light related. Results reported for target approaches and all approaches, data extracted for all approaches.</td>
</tr>
</tbody>
</table>

Scottsdale, AZ - EB

<table>
<thead>
<tr>
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</thead>
<tbody>
<tr>
<td>Participants</td>
<td>14 intersections in Scottsdale</td>
</tr>
<tr>
<td>Interventions</td>
<td>14 RLC intersections</td>
</tr>
<tr>
<td>Outcomes</td>
<td>Angle crashes, left turn crashes, rear end crashes.</td>
</tr>
<tr>
<td>Notes</td>
<td>Crashes involving factors such as alcohol, drugs, illness or fatigue were removed. Target crashes within 30m from intersection determined to be red light related. Results reported for target approaches and all approaches, data extracted for all approaches.</td>
</tr>
</tbody>
</table>

Sohn, 2012 (City of Las Cruces, New Mexico, USA)

<table>
<thead>
<tr>
<th>Methods</th>
<th>CBA. Data collected between Jan 2004 and April 2011 with RLC program initiating in March 2009.</th>
</tr>
</thead>
<tbody>
<tr>
<td>Participants</td>
<td>10 intersections in City of Las Cruces, NM.</td>
</tr>
<tr>
<td>Interventions</td>
<td>4 RLC intersections (cameras on multiple approaches) and 6 non camera intersections in Las Cruces.</td>
</tr>
<tr>
<td>Outcomes</td>
<td>Total crashes, angle crashes, rear end crashes, PDO crashes, angle injury crashes, rear end injury crashes.</td>
</tr>
<tr>
<td>Notes</td>
<td></td>
</tr>
<tr>
<td>Study</td>
<td>Reason for Exclusion</td>
</tr>
<tr>
<td>------------------------</td>
<td>------------------------------------------------------------------------------------------------------------------------------------------------------</td>
</tr>
<tr>
<td>Blais 2015</td>
<td>RLCs not primary intervention of interest. Study examines the French Automated Speed Enforcement Program (ASEP) - cannot separate effect of RLCs from ASEP</td>
</tr>
<tr>
<td>Budd 2011</td>
<td>All intersections without RLCs located within the same postcode as a treatment sites.</td>
</tr>
<tr>
<td>Dahnke 2008</td>
<td>No independent control group, non-monitored approaches of RLC intersection used as controls.</td>
</tr>
<tr>
<td>De Pauw 2014</td>
<td>Comparison group is database of all crashes in Flanders to control for trend, includes treated intersections.</td>
</tr>
<tr>
<td>Golob 2002</td>
<td>No independent control group, used non-monitored/unenforced approaches of RLC intersection as comparison.</td>
</tr>
<tr>
<td>Guerin 2010</td>
<td>Speed cameras and RLCs activated at the same time - cannot separate effect of RLCs from speed cameras.</td>
</tr>
<tr>
<td>Hebert Martinez 2006</td>
<td>Focus on identifying and predicting characteristics of red light runner drivers after implementation of RLC program. Outcome measures not of interest.</td>
</tr>
<tr>
<td>Kent 1995</td>
<td>Ordinal logistic model - not a CBA, no before data.</td>
</tr>
<tr>
<td>Loftis 2011</td>
<td>No non-RLC control intersection used. RLC intersections not selected at random. Study used simulation approach to model collisions with and without RLCs installed.</td>
</tr>
<tr>
<td>Lum 2003a</td>
<td>No independent control group - non-monitored approaches of RLC intersection used as control. No examination of collision, only red light running. Study examines impact of RLCs on after-red times - does RLC effect mean after-red times on camera vs non-camera approaches.</td>
</tr>
<tr>
<td>Lum 2003b</td>
<td>Same data as Lum 2003a, some of the same results reported. No independent control group - non-monitored approaches of RLC intersection used as control.</td>
</tr>
<tr>
<td>Lum 2003c</td>
<td>Same data as Lum 2003a, some of the same results reported. No independent control group - non-monitored approaches of RLC intersection used as control.</td>
</tr>
<tr>
<td>McCarrt 2014</td>
<td>No before data. Only data during warning period at which time cameras and signs had been installed and public announcement made.</td>
</tr>
<tr>
<td>McFadden 1999</td>
<td>Report is a synthesis of other RLC studies. No new data provided.</td>
</tr>
<tr>
<td>MVA Consultancy 1995</td>
<td>Some non-camera approaches of RLC intersections used as controls (only 19 hours after monitoring).</td>
</tr>
<tr>
<td>Obeng 2008</td>
<td>Focus is on offsetting driver behavior - RLC intervention not the primary intervention of interest.</td>
</tr>
<tr>
<td>Radalj 2001</td>
<td>No control before after crash data. Excluded from previous review 'no base data provided on control'.</td>
</tr>
<tr>
<td>Shah 2010</td>
<td>Not a controlled before after study. Mentions a report of a controlled before after study that was acquired but only reports percentage decreases and none of the data is included</td>
</tr>
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<td>Shah 2014</td>
<td>Not a controlled before after study. Mentions a report of a controlled before after study that was acquired but only reports percentage decreases and none of the data is included</td>
</tr>
<tr>
<td>Smith 2000</td>
<td>Report is a synthesis of other RLC studies. No new data provided.</td>
</tr>
<tr>
<td>Sun 2012</td>
<td>No before after comparison. Data collected using video-based system set up by researchers. Intersections located 800m apart (approx 1/2 mile).</td>
</tr>
<tr>
<td>Synectics Trans Consult 2003 (red light camera systems and stepped-up police enforcement)</td>
<td>Study examines both RLCs and &quot;stepped up police enforcement&quot; (not clearly defined). Publicity campaign on RLR also conducted prior to and during the study. No signage used.</td>
</tr>
<tr>
<td>Synectics Trans Consult 2003 (Red light running evaluation)</td>
<td>Not clear whether data was collected on more than one day during each phase of the study. During the interim period, violation data were collected twice at the distant comparison sites but only once at the other locations</td>
</tr>
<tr>
<td>Vanlaar 2014</td>
<td>Study examines combined speed and RLC programme - cannot separate effect of RLCs from speed cameras.</td>
</tr>
<tr>
<td>Wang 2015</td>
<td>Study examines CMFs (crash modification factors), analysed trends of CMF. RLCs not primary intervention of interest.</td>
</tr>
<tr>
<td>Walden 2011</td>
<td>No independent control. Non red light crashes at treated intersections used as comparison group.</td>
</tr>
<tr>
<td>Wong 2014</td>
<td>Signal phasing changed concurrently with installation of RLCs so exclude as combined intervention.</td>
</tr>
<tr>
<td>Yoo Sung Jun 2010</td>
<td>No control group – location of study not specified.</td>
</tr>
</tbody>
</table>
Appendix H - Subgroup analyses based on RTM

Effects of red-light cameras on right angle crashes - stratified by whether or not studies account for regression to the mean.
Effects of red-light cameras on right angle crashes resulting in injury - stratified by whether or not studies account for regression to the mean.