Automated Enforcement: A Compendium of Worldwide Evaluations of Results

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16. Abstract
This compendium details automated enforcement systems (AES) implemented around the world and characterizes the safety impacts of such deployments, based on available scientific evaluations of the outcome measures. A systematic literature search was conducted. Criteria for selecting key evaluation studies were developed and applied for two AES technologies: speed camera and red light camera (RLC) enforcement systems.

For automated speed enforcement, key studies reported significant reductions in estimated crashes following program implementation, but only a few studies were well-controlled. The degree to which reported safety improvements are related to the treatment and desired behavioral change (decrease in speeds) as opposed to regression to the mean (RTM), other confounders, and behavior changes such as choosing alternate routes, cannot be stated with certainty. Most studies attempted to account for unknown and time-related crash trends through the use of comparison groups. Four studies considered RTM, while only one study accounted directly for traffic flow changes and effects on crashes. Speed reductions documented in about half of the studies provided support for a relationship between the treatments and crash reductions. Enforcement intensity of mobile enforcements, site-specific differences (e.g., sample area, roadway characteristics), and statistical anomalies may play a role in AES effects. Recommendations for future studies include the use of empirical Bayes (EB) procedures to control for RTM, careful selection and examination of comparison groups, and consideration of traffic flow effects and possible crash migration due to the treatment, in order to improve the accuracy of treatment safety effect estimates.

For red light running enforcement, the key studies support the assertion that RLCs can reduce crash severity at high red light running intersections. Consistent with previous reports, this review attributes a decrease in right-angle crashes with an increase in rear-end crashes to RLC implementations, together with a decrease in red light running violations. Recommendations for future studies of RLC effects include the use of controlled and randomized experiments whenever feasible. Otherwise, adjustments in before- and after- designs with matched control locations (or jurisdictions) to take legal, ethical, and economic barriers to random assignment of RLC installations into account, plus the use of EB procedures to control for RTM and to improve the precision of estimates of treatment safety effects, are strongly recommended.

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Red light running, speeding, camera, automated enforcement systems (AES)

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SI* (MODERN METRIC) CONVERSION FACTORS

APPROXIMATE CONVERSIONS TO SI UNITS

Symbol	When You Know	Multiply By	To Find	Symbol
		LENGTH		
in	inches	25.4	millimeters	mm
ft	feet	0.305	meters	m
yd	yards	0.914	meters	m
mi	miles	1.61	kilometers	km
		AREA		
in^2	square inches	645.2	square millimeters	mm^2
ft^2	square feet	0.093	square meters	m^2
yd^2	square yards	0.836	square meters	m^2
ac	acres	0.405	hectares	ha
mi^2	square miles	2.59	square kilometers	km^2
		VOLUME		
fl oz	fluid ounces	29.57	milliliters	mL
gal	gallons	3.785	liters	L
ft^3	cubic feet	0.028	cubic meters	m^3
yd^3	cubic yards	0.765	cubic meters	m^3
NOTE: Volumes greater than 1000 l shall be shown in m^3.				
		MASS		
oz	ounces	28.35	grams	g
lb	pounds	0.454	kilograms	kg
T	short tons (2000 lb)	0.907	megagrams (or "metric ton")	Mg (or "t")
		TEMPERATURE (exact)		
°F	Fahrenheit temperature	5(F-32)/9 or (F-32)/1.8	Celcius temperature	°C
		ILLUMINATION		
fc	foot-candles	10.76	lux	lx
fl	foot-Lamberts	3.426	candela/m^2	cd/m^2
		FORCE and PRESSURE or STRESS		
lbf	poundforce	4.45	newtons	N
lbf/in^2	poundforce per square inch	6.89	kilopascals	kPa

APPROXIMATE CONVERSIONS FROM SI UNITS

Symbol	When You Know	Multiply By	To Find	Symbol
		LENGTH		
mm	millimeters	0.039	inches	in
m	meters	3.28	feet	ft
m	meters	1.09	yards	yd
km	kilometers	0.621	miles	mi
		AREA		
mm^2	square millimeters	0.0016	square inches	in^2
m^2	square meters	10.764	square feet	ft^2
m^2	square meters	1.195	square yards	yd^2
ha	hectares	2.47	acres	ac
km^2	square kilometers	0.386	square miles	mi^2
		VOLUME		
mL	milliliters	0.034	fluid ounces	fl oz
L	liters	0.264	gallons	gal
m^3	cubic meters	35.71	cubic feet	ft^3
m^3	cubic meters	1.307	cubic yards	yd^3
		MASS		
g	grams	0.035	ounces	oz
kg	kilograms	2.202	pounds	lb
Mg (or "t")	megagrams (or "metric ton")	1.103	short tons (2000 lb)	T
		TEMPERATURE (exact)		
°C	Celcius temperature	1.8C + 32	Fahrenheit temperature	°F
		ILLUMINATION		
lx	lux	0.0929	foot-candles	fc
cd/m^2	candela/m^2	0.2919	foot-Lamberts	fl
		FORCE and PRESSURE or STRESS		
N	newtons	0.225	poundforce	lbf
kPa	kilopascals	0.145	poundforce per square inch	lbf/in^2

* SI is the symbol for the International System of Units. Appropriate rounding should be made to comply with Section 4 of ASTM E380.

(Revised September 1993)

PREFACE

The objective of this project was to produce a compendium detailing automated enforcement approaches and systems implemented around the world and to characterize the impacts of such deployments, based on available research literature. The study was not designed to yield a "best practice" document, to project the impacts of untried concepts, or to discuss the feasibility, logistics, or other challenges associated with automated enforcement system deployments.

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EXECUTIVE SUMMARY

This National Highway Traffic Safety Administration (NHTSA) project produced a compendium of automated enforcement system (AES) approaches and implementations worldwide, with a critical review of studies that have attempted to evaluate the impacts of such deployments. The current research was not designed to yield a "best practice" document; to project the impacts of untried concepts; or to address the feasibility, logistics, or other challenges associated with the actual deployment of automated enforcement systems. Two specific technologies – speed enforcement and red light cameras – emerged as the focus of this review. Recommendations developed in this project concentrate on strategies to improve the scientific validity and precision of future AES evaluation activities.

BACKGROUND

An initial review sought AES evaluation studies with a description of intervention sites, evidence of pre- and post-intervention outcome measure data, and identification of statistical analysis methods. A systematic information and literature search of national and international sources generated a voluminous quantity of reports and articles describing systems to combat speeding and red light running. This was followed by a more in-depth filtering process that ranked studies according to (1) study methodology, targeting research designs incorporating a controlled pre- and post-intervention period for evaluation; (2) outcome measures (e.g., crash or violation frequency and/or rates, plus injury, fatality, property damage); (3) number of treatment and comparison sites; (4) other treatment characteristics (e.g., warning signs, publicity); (5) other evaluation features (e.g., cost-benefits); and (6) confounding variables (e.g., spillover effect, regression to the mean). These evaluation factors were applied in a checklist procedure to yield a final selection of studies for inclusion in this compendium.

Automated enforcement systems addressed in this report use image capture technology to monitor and/or enforce traffic control laws. Automated speed enforcement systems include fixed cameras that can continually monitor traffic speeds without a human operator, and/or mobile camera operations, usually deployed in vehicles by law enforcement agents; and "speed-over-distance" systems that photograph vehicles and measure speeds at both starting and ending points on roadways. Photographs of the speeding vehicle and license plate number are reviewed by jurisdictions, and the owner of the vehicle may receive a citation. This technology has been widely deployed, most extensively in Australia, Canada, Europe, and the United States.

Red light cameras (RLCs) are set up to photograph vehicles entering intersections after signals have turned red. Detection of an offense is made by sensors buried in the pavement and tied to a timing system integrating the traffic signal and pole-mounted camera. Photographs of a vehicle entering an intersection illegally and the license plate number are taken and then reviewed by the jurisdiction. The owner of the vehicle then may receive a citation. RLCs are used worldwide, most heavily in Australia, Canada, Europe, Singapore, and the United States.

RESEARCH FINDINGS

Thirteen evaluation studies met our criteria for full review in the area of *automated speed enforcement*. All reported decreases in estimated injury crashes, all crashes, or speed-related crashes at camera sites, or system-wide (State or province) following implementation of automated speed enforcement using cameras. About half of the studies also documented reductions in speeds, suggesting a relationship between the treatment and the observed safety effect. Because of confounding factors and differences in study methodologies, no single number is thought to best represent the safety effect of automated speed enforcement.

Four studies evaluated local effects of fixed speed camera enforcement. Estimates of injury crash reductions at treated sites (of varying lengths) from three of these studies, including two of the best controlled, were in the range of 20 to 25%. Two studies controlled for volume and regression to the mean (RTM), and two documented reductions in speeds as well as crashes. Using empirical Bayes (EB) procedures which correct for RTM, traffic volume, and changes due to new treatment, one study also documented significant and sizable crash reduction effects due to RTM: up to half of observed fatal and serious crashes. This result highlights the importance of controlling for RTM. The same study also estimated that 5% of the reported 25% overall reduction in the personal injury crash rate was the result of changes in traffic flow, such that beneficial effects at the enforced sites were accompanied by a negative impact on alternate routes. Another study documented increases in speeds and crashes over two years at non-enforced sites adjacent to treatment sites, also suggesting the possibility of driver adaptation and crash migration following the treatment. These outcomes did not totally offset reductions in crashes at camera sites, however.

Reported reductions for mobile enforcement programs were more variable, perhaps reflecting in part the variable nature of mobile enforcements themselves. Only one study of mobile treatment effects controlled for RTM, as well as general trends. This study found a 16% corridor-wide reduction in all crashes along a single 22 km corridor with 12 specific treatment sites. The enforcement program was covert; and, the comparison group may also have been affected by a province-wide program, resulting in an understatement of effect in this study.

Other studies of signed mobile camera enforcement zones and otherwise more overt, conspicuous mobile enforcement programs found a wide range of crash reductions – from about 9 to 18% for all crashes, and from 21 to 51% for injury crashes. Confidence in these results is limited based on lack of control for RTM, short study periods for some studies, issues with comparison groups, and other factors. None of these studies directly examined the possibility of traffic flow changes or crash migration to non-enforced routes or segments.

Two of the studies examined system-wide effects of mobile enforcement. One reported a 25% reduction in daytime unsafe speed-related crashes province-wide and the other reported a reduction of 30% in daytime injury crashes State-wide. One study documented generalized speed reductions, and the other found a relationship between crash reductions and program intensity. While there may be confounding factors, both studies used time-trend analyses to account for general trends, proxy measures to adjust for travel exposure, and one also used a neighboring similar State as a comparison group.

For *red light running enforcement,* findings across the reviewed studies revealed crash effects that were consistent in the direction of those found in many previous RLC evaluation studies. That is, a decrease in right-angle crashes, and an increase in rear-end crashes. When violation frequencies were evaluated, the results were also consistent with findings of previous studies showing that red light running violations decrease at both treatment and non-treatment intersection sites. It may be noted, however, that challenges to data quality in these studies included errors introduced when recording data in the field, and in data entry; variance in red light gap acceptance; the presence of turning vehicles in the red light phase; and the percentage of violations/citations dismissed in court on appeal.

Most of the studies compared treatment sites with comparison sites that were matched for characteristics such as operations (e.g., traffic volume); speed; traffic control (e.g., duration of amber signal); and geometry (e.g., number of lanes). These characteristics varied substantially across all of the reports. Most, if not all, of the traffic control operations and RLC installations were under the control of the participating jurisdictions, leading to inconsistencies in the experimental designs and what measures, if any, the researchers were able to control. In addition, while the majority of sites incorporated RLC warning signs and public outreach efforts, neither the details of sign placement (and the associated visibility/conspicuity) nor the amount and duration of media exposure were documented.

Several studies conducted an economic effects analysis based on an aggregation of right-angle and rear-end crash costs for various injury severity levels. These studies revealed that RLCs do provide a modest aggregate crash-cost benefit. RLCs contributed more to decreasing fatal and injury angle- and left-turn crashes than to decreasing property-damage-only crashes.

CONCLUSIONS AND RECOMMENDATIONS

Existing research indicates that automated enforcement systems can result in measurable safety improvements at high crash locations. The magnitude of the effect, and how much is due to the desired behavior change (decrease in speed or red light running) versus other behavior changes (e.g., choosing alternate routes), remains uncertain.

Recommendations for future implementations of automated speed enforcement systems stress the need for authorities and researchers to collaborate on studies of safety effects using controlled, randomized experiments. Where observational before/after studies are performed, they should document changes in speeds as well as crashes, and control for general trends and regression to the mean. Selecting treatment groups that adequately encompass the extent of expected effects and comparison groups that are unaffected by the treatment, but adequately account for other factors such as general trends and other countermeasures, is essential. Traffic flow and events outside of the immediate treatment zone and on alternate routes must be monitored to determine possible positive and negative spillover effects; negative effects may be more likely for conspicuous and fixed treatments, while positive spillover appears more likely for covert or inconspicuous enforcements, and could affect comparison groups.

Parallel recommendations apply to studies of automated enforcement programs using red light camera (RLC) installations. Such evaluations ideally will be carried out using controlled, randomized experiments. Where legal, ethical, and economic barriers to random assignment lead to the use of before/after designs to gauge safety effects, the selection of treatment and matched control locations (or jurisdictions) is critical. Empirical Bayes methods applied to the calculation of treatment effects can be helpful in guiding the selection of comparison sites. The goal is to establish reference functions with variables that incorporate all other known influences on driving behavior; on exposure, and on crash reporting practices and other factors affecting data quality within a jurisdiction, to permit more reliable estimates of RLC treatment effects.

INTRODUCTION

This chapter presents a problem statement for the study; background information on automated enforcement system technology; the project objectives; and scope of work.

PROBLEM STATEMENT

Automated enforcement is the use of image capture technology to monitor and/or to enforce traffic control laws. It can be used to combat aggressive driving behaviors such as speeding or running red lights. Testimony by the National Highway Traffic Safety Administration (NHTSA) administrator (Martinez, 1997) several years ago pointed out that about one-third of all crashes and two-thirds of the resulting fatalities in the United States are attributed to aggressive driving behaviors. Aggressive driving is often manifest in irresponsible driving behaviors such as speeding, running red lights, and tailgating. Increased enforcement of traffic laws is viewed as a potential solution for aggressive driving, but in reality, conventional law enforcement has not been able to keep pace with increased traffic volumes and vehicle mileage. This concern has prompted the traffic safety community and public entities in the United States and around the world to promote and use automated enforcement to improve general deterrence of aggressive driving and compliance with traffic laws; to reduce resource-intensive traditional enforcement; and to decrease high-risk enforcement methods associated with high-speed pursuits of traffic control law violators.

While there are many issues surrounding the use of automated enforcement systems (AES), including: legal (e.g., privacy, distribution of ticket revenue, ticketing procedures, legislative enactment of laws, etc.); public education and community support; and technology performance and efficiencies, the purpose of this study is to validate the effectiveness (and impact) of AES deployments which have been scientifically evaluated and documented in the research literature.

BACKGROUND

Excessive speeding, red light running, and other aggressive and high-risk driving behaviors are leading causes of crash fatalities and injuries in the United States (Retting, 1999). Traditional law enforcement alone is not enough to deter violators. The use of AES is a potentially effective way to deter high-risk driving behaviors. The goal of AES is to significantly increase the objective and perceived chances of being caught, thus creating a change in behavior that will translate into a crash reduction, whether it applies to running red lights, speeding, or other aggressive driving behaviors. While AES technology has been used in many applications, the two most common AES applications are fixed and mobile cameras for speeding, and fixed cameras at intersections for red light running violations.

Speed Enforcement

The relationship between speed and traffic safety is generally well accepted by researchers and public safety officials. That higher speeds are associated with increasing crash severity is particularly difficult to deny, both from highway safety research and physical laws. Correlational studies also suggest that the probability of a crash is related to speed, both on an individual level and on an average traffic speed level, although the evidence is not as conclusive (TRB, 1998; Elvik, 2005). The incidence of single vehicle (run-off-road) crashes increases sharply with higher speeds, providing additional suggestive evidence of a relationship between crash frequency and speed.

There is some evidence for a relationship between traffic speed dispersion and crash incidence on some road classes (particularly limited access and rural highways). Early studies found that motorists traveling at both significantly higher- and significantly lower-than-average speeds had greater crash risk, although later analysts found that the relationship with lower speeds was related primarily to crashes involving turning and slowing vehicles (reviews by TRB, 1998; and Aarts and van Schagen, 2006). Larger speed variance for particular road sections has also been associated with higher crash rates, but Aarts and van Schagen (2006) point out that in most studies, these variances reflect differences over 24 hours and thus speed differences between peak- and low-flow periods. They argue that to understand the meaning of these variances, it is important to examine speed and crash data on a more disaggregated level.

More recently, Elvik (2005) performed a meta-analysis of 98 treatment evaluation studies incorporating changes in traffic speed and road safety estimates (different severity of crashes or injuries) as outcome measures. He used the Power Model proposed by Göran Nilsson of Sweden to estimate power function relationships between speed and road safety. While combining the results from different studies as part of the meta-analysis, he assigned weights proportional to the inverse of the variance of the estimate. He performed the analysis using both well-controlled studies and all studies, and with and without very small (< 2.5%) changes in speed, and found a strong statistical relationship between changes in speed and changes in both the number of crashes and severity of injuries. Despite these results, Elvik acknowledges limitations in the use of the power model. For example, the power model predicts that the effects on fatalities for a reduction of speed from 100 to 50 km/h would be the same as the reduction from 10 to 5 km/h, very unlikely to be the case. An attempt to consider the effect of initial speed through logistic regression using the power model, showed a tendency for the power to become lower as initial speed became lower, but the changes in the relationship were not very systematic.

Fatality data (NHTSA, FARS, 2004) suggest that speed is associated with about 30% of all fatal crashes in the United States. These data are based on police-reported codes for “exceeding the posted speed limit” or “driving too fast for conditions,” not on investigations showing whether speed was the primary cause of the crash (TRB, 1998). A review of detailed causal crash studies suggests that excessive speed is a causal factor in 7 to 11% of all crashes (in one study, second only to inattention as the second most frequent cause), and a probable cause in 13 to 16% more. The association with casualty crashes is even higher (TRB, 1998). Thus, detailed crash studies suggest that speed is a causal or at least a contributing factor in nearly as high a proportion of all reported crashes as is generally reported based on fatal crash data.

Therefore, there are compelling reasons to attempt to regulate speed, and to enforce limits since many drivers do not comply with established limits. For example, Fitzpatrick et al. (2003), reports that a large majority (68 to 78%) of vehicles exceeded the speed limit on urban and suburban roadways. Others have documented similar findings and significant percentages of excessive speeding as well.

Red Light Running Enforcement

Intersection safety is a serious problem in the United States. Approximately 40% of all crashes are intersection related. NHTSA reported that in 2004 alone, more than 9,000 people died and another 1.5 million people were injured in intersection-related crashes. The number of fatal motor vehicle crashes at traffic signals is rising faster than any other type of fatal crash nationwide (FHWA, 2006).

The FHWA reports that red light running causes more than 180,000 crashes every year, resulting in approximately 1,000 deaths and 90,000 injuries annually. In 2004, data from NHTSA's Fatality Analysis Reporting and General Estimates Systems, found that crashes caused by red light running resulted in as many as 854 fatalities and more than 168,000 injuries. (FHWA, 2006). Survey research has explored this issue with the public. Surveys conducted by USDOT and the American Trauma Society, have reported that almost two-thirds of Americans reported seeing someone running a red light at least a few times a week (FHWA, 2002).

Red light running involves a driver entering an intersection after the traffic signal has turned red. Enforcement has typically involved a law enforcement agent parked in a vehicle near the intersection observing for violations. This method is unsafe for the officer, expensive, and requires a lot of staff resources for the law enforcement agency. Automated enforcement of red light running violators using red light cameras offers an opportunity to efficiently and effectively reduce this unlawful driving behavior.

PROJECT OBJECTIVES AND SCOPE OF WORK

The objective of this study was to produce a compendium detailing automated enforcement approaches and systems implemented around the world and to characterize the impacts of such deployments, based on available research literature. It was not designed to yield a "best practices" document; to project the impacts of untried concepts; to report on products and product development; or to discuss the feasibility, logistics, or other challenges associated with automated enforcement system (AES) deployments.

To meet the study's goals, the scope of work included:

(1) A comprehensive search and document acquisition of technical literature.

(2) A description of AES deployments around the world.

(3) The identification, review and synthesis of critically important research studies that have evaluated the AES technology using scientifically valid methods.

(4) The development of recommendations for future deployments of AES that will facilitate rigorous evaluations of the impacts of AES on highway safety. An annotated bibliography of the selected AES research reports included in this study was also prepared.

RESEARCH APPROACH AND METHODS

INFORMATION AND LITERATURE SEARCH

A systematic approach was used to conduct an international and National information and literature search. Four search methods were used: (1) electronic subject databases; (2) electronic technical library databases; (3) Internet sources; and (4) professional associations.

Search Methods

Use of Electronic Subject Databases. Transportation, engineering, human factors/psychological, and general science electronic databases were searched. These were accessed through staff accounts and free online services. Keyword terms searched included: red light running, speed(ing), aggressive driving, violation(s), crash(es), collision(s), accident(s), injury severity, fatality, automated enforcement system(s), automated enforcement technology, red light camera(s), photo camera, photo radar, laser, detection, detector, adaptive speed, railroad crossing, and following too closely. Other terms were used after reviewing abstracts and other references.

Subject databases and their producers used for the search included:

- Compendex (Engineering Abstracts Inc.)
- IRRD (International Road Research Documentation - Organization for Economic
- Cooperation and Development)
- NTIS (National Technical Information Service)
- PsychInfo (American Psychological Abstracts)
- TRANSDOC (European Conference of Ministers of Transport)
- TRANSPORT – IRTD, and TRANSDOC
- TRIS – (Transportation Research Information Services – Transportation Research Board)

Use of Electronic Technical Library Databases. The following electronic technical library databases were used: Northwestern University Transportation Library (NWUTL) (Evanston, Illinois); Temple University (Philadelphia, Pennsylvania); and University of North Carolina Highway Safety Research Center (UNC/HSRC) (Charlotte, North Carolina). Staff reference librarians were contacted and provided with a description of the project and literature search task, including the search terms. NWUTL and UNC/HSRC used several of the commercially available electronic databases, as well as their own electronic library collections.

The initial searches in these electronic databases revealed well over 500 published articles and research reports relating to AES technology. Most of these references referred to red light cameras and speed enforcement technology. Very few references were related to other AES technologies.

Internet Sources. Government and research agency Web sites were searched. These sites were identified from the Federal Highway Administration's *International Guide to Highway Transportation Information* (Decina & Lococo, 2001), Aeron-Thomas and Hess' *Red-light*

Cameras for the Prevention of Road Traffic Crashes (2005), and sources from the Highway Safety Research Center Library at University of North Carolina.

National Web sites searched included:

- AAA Foundation for Traffic Safety
- Centers for Disease Control
- Federal Highway Administration
- Institute of Transportation Engineers
- National Campaign to Stop Red Light Running
- National Highway Traffic Safety Administration
- National Safety Council
- Transportation Research Board

International Web sites searched included:

- Asociacion Argentina de Technologia Espacial
- Australian Road Research Board
- Australian Transport Safety Bureau
- Chilean Comisión Nacional de Seguridad de Transito
- Danish Council for Road Safety Research; Danish Tránsport Research Institute; Department for Environment and Transport (DOET), United Kingdom
- Deutscher Verkenrssichereitsrat Road Safety Instiute (DVR), Germany
- European Transport Safety Council
- Finnish National Road Administration
- Inform. and Technology Centres for Transport and Infrastructure (CROW), Netherlands
- Institut National de Recherche sur les Transports et leur Sécurité (INRETS), France
- Interamerican Development Bank
- International Cooperation on Theories and Concepts in Traffic Safety
- Laboratoire d'Economie des Transports (LET), France
- Monash University of Accident Research Centre (MUARC), Australia
- Norway Institute of Transport Economics (TOI)
- Pan American Highways Organization
- Swedish National Roads and Transport Research Institute (VTI)
- Transport Canada
- Transport Research Laboratory (TRL), United Kingdom
- Wetenschappelijk Onderzoek Verkersveiligheid (SWOV), Netherlands
- World Health Organization (WHO)

In addition, a general Internet search was conducted, using Yahoo and Google search engines. A list of 181 independent Nations were selected from the United Nations member list and combined with the phrase "automated enforcement." Relevant material was selected from the first ten results of each hit.

Professional Associations. Chairs from several Transportation Research Board (TRB) committees, including: Safety Data, Analysis, and Evaluation (ANB20); Traffic Law Enforcement (ANB40); Vehicle-Highway Automation (AHB30); and Traffic Control Devices

(AHB45) were contacted by e-mail and asked to circulate an e-mail to members and friends of the committee inquiring about their knowledge of currently completed research projects on automated enforcement. Several leads were obtained for new reports, as well as information on projects currently in progress. Over a dozen reference sources were revealed from the inquiries to professional associations.

Scanning References and Abstracts

The initial scan for references involved broad selection criteria for screening titles and abstracts of relevant articles to assess their eligibility for inclusion in the document acquisition task. The title and abstract needed to demonstrate some evidence of an automated enforcement evaluation study with some description of the number of installation sites, evidence of pre- and post-intervention outcome measure data, and identification of statistical testing methodology.

Document Acquisition

Documents were acquired from the following libraries and document delivery services: NWUTL; UNC/HSRC; Temple University; and TRB Publications Sales Office. Documents were also retrieved from electronic full-text Web sites; research author correspondence; and the project team's in-house collections.

CRITERIA FOR SELECTION OF EVALUATION STUDIES

Automated Speed Enforcement

Automated speed enforcement studies were preliminarily reviewed for meeting the first- and second-level study criteria:

- 1st level - Studies were reviewed to determine if they described an evaluation of an automated speed enforcement program issuing traffic infringement citations. Program data elements were extracted and included: country and location information, road types targeted, fixed or mobile deployment, speed limits enforced, enforcement threshold, penalty assessment information, public awareness information, general program description (intensity, hours, selection of sites, etc.).

- 2nd level - If studies met the above criterion, they were reviewed to determine if the evaluation included safety-related outcome measures (crashes or injuries). Data elements extracted included: study period including before/after periods, number and description of treatment units, use and description of comparison group, study design and analyses, whether traffic volume was addressed, presence of other treatment confounders (and whether addressed), outcome measures, and key outcomes.

Checklists with these characteristics were developed in early task activities.

In order to be considered for full review, the report or journal article had to provide detailed methodological descriptions, analyses, and results. Studies of effects on speed without crash or injury outcomes were not further reviewed. Neither technology and feasibility studies, nor perception and self-report survey studies were included in this review.

Based on the issues raised in the foregoing introductory discussion, the methodological review of automated speed enforcement studies included assessment of the following study characteristics:

- Did the study design and analysis document changes in driving speeds as well as crashes to provide a causal link between the treatment and effect (safety outcome)?
- Did the study account for crash severity (to ensure that the treatment is not having counteractive effects on different types or severity of crashes)?
- Did the study methods and analysis control/account for changes in traffic volumes before/after the implementation?
- Did the study design and analysis account for possible time trend effects (such as general trends in crashes, or changes in the motoring population, vehicle fleet, weather, etc.)?
- Did the study account for other possible confounding factors such as concurrent treatments/enforcement, or changes in data measures (such as reporting thresholds), or other factors that may overlap with before/after periods?
- Did the study examine possible crash migration due to the treatment, either to non-enforced sections of the same roadways, or to non-enforced alternate roads?
- Did the study account for regression toward the mean?

About 90 studies from 16 countries were identified as potential evaluation studies of automated speed enforcement implementations. English-language studies only were obtained for detailed screening based on titles or abstracts.

Of the screened studies, 39 were determined to be reports of evaluation studies of automated speed enforcement and preliminarily reviewed for program description and evaluation elements. Seven studies reported effects on traffic speeds, but not crash effects and were not further reviewed. Others were excluded because they did not provide detailed study methods and results. Other evaluation studies may be of interest to the reader, but were excluded from this review if they did not report primarily on the effects of introduction of speed cameras. For example, several studies compared the effectiveness of one or more treatments with automated speed enforcement (Bloch, 1998; Hirst, Mountain, & Maher, 2005; and Mountain, Hirst, & Maher, 2005) or compared the effectiveness of covert and overt photo-enforcement after camera enforcement had already been introduced (Keall, Povey, & Frith, 2001, 2002). Three studies were excluded because later versions using the same data and/or more complete data and analysis techniques were reviewed (Carseldine, 2004, reported on by the ARRB group project team in 2005; Gains & Humble, 2003; Hess & Polak, 2003). Thirteen studies were identified for detailed methodological review. Table 1 identifies this list. Detailed descriptions of the 13 key studies are provided in Part I of the annotated bibliography in Appendix A of this report. Appendix B presents a bibliography of the 26 screened studies that did not meet the detailed review criteria.

Table 1. Automated Speed Enforcement Studies for Detailed Evaluation

Citation	Location of Intervention	Type of Deployment
ARRB Group (2005)	New South Wales, Australia	fixed
Cunningham, Hummer, & Moon (2005)	Charlotte, North Carolina, United States	mobile
Goldenbeld & van Schagen (2005)	Friesland Province, the Netherlands	mobile
Gains, Heydecker, Shrewsbury, & Robertson (2004)	Nationwide, United Kingdom	fixed and mobile
Hess (2004)	Cambridgeshire, United Kingdom	fixed
Mountain, Hirst, & Maher (2004)	Great Britain, United Kingdom	fixed
Christie, Lyons, Dunstan, & Jones (2003)	South Wales, United Kingdom	mobile
Newstead & Cameron (2003)	Queensland, Australia	mobile
Chen, Meckle, & Wilson (2002)	British Columbia, Canada	mobile
Chen, Wilson, Meckle, & Cooper (2000)	British Columbia, Canada	mobile
Tay (2000)	Christchurch, New Zealand	mobile
Elvik (1997)	Norway	fixed
Cameron, Cavallo, & Gilbert (1992)	Victoria, Australia	mobile

The global geographic distribution of the reviewed studies was as follows:

- United Kingdom – four studies
- Australia – three studies
- Canada – two studies
- The Netherlands, New Zealand, Norway, and the United States – one study each

All of the studies were before/after assessments; most used a comparison group to control for trend factors. Most of the studies reported effects on injury crashes, some with varying severity levels. Several used all crashes as the primary safety measure; some included fatalities and injuries or provided some examination of severity level.

Seven of the studies measured effects on driving speeds as well as crashes (Mountain, Hirst, & Maher, 2004; Goldenbeld & van Schagen, 2005; Chen et al., 2000; Chen, Wilson, & Meckle, 2002; ARRB Group 2005; Gains et al., 2004; and Cunningham, Hummer, & Moon, 2005). One of the earlier studies, although not documenting driving speed changes, assessed program factors such as hours of operation, numbers of traffic infringement notices mailed, and publicity level, and the relationship to crash outcomes (Cameron, Cavallo, & Gilbert, 1992).

Automated Red Light Running Enforcement

Red light camera (RLC) enforcement studies were preliminarily reviewed for meeting the first- and second-level study criteria:

- 1st level - Studies were reviewed to determine if they described an evaluation of an RLC enforcement program. Program data elements were extracted and included: country and location information, type of technology (e.g., 35mm still photo, digital still); enforcement parameters; program traits; intersection characteristics; evaluation study considerations; and research design.

- 2nd level - If studies met the above criterion, they were reviewed to determine if the evaluation included: treatment characteristics; concurrent treatments (warning signs, publicity, engineering countermeasures); study design (type of study – e.g., before and after with matched comparisons); number of treatment and comparison sites; duration of study periods; impacts reported (e.g., type of crash, type of injury, and violation characteristics); and other outcome measures (e.g., cost savings).

Checklists with these characteristics were developed in early task activities.

In order to be considered for full review, the study had to provide detailed methodological descriptions, analyses, and results. Neither technology and feasibility studies, nor perception and self-report survey studies were included in this review. The methodological review of automated red light running enforcement studies included assessment of the following study characteristics:

- Did the study design and analysis document changes in crashes (and or violations) to provide a causal link between the treatment and effect (safety outcome)?
- Did the study account for crash severity (e.g., fatality, injury, property damage only) to ensure that the treatment is not having counteractive effects on different types or severity of crashes)?
- Did the study design and analysis account for possible time trend effects (such as general trends in crashes, or changes in the motoring population, vehicle fleet, weather, etc.)?
- Did the study account for other possible confounding factors such as concurrent treatments/enforcement, or changes in data measures such as reporting thresholds, or other factors that may overlap with before/after periods?
- Did the study account for variation in signal timing differences of sites?
- Did the study account for effects of other treatments (e.g., warning signs)?
- Did the study account for regression toward the mean? Spillover effects?

About 75 studies from 9 countries were identified as potential evaluation studies of automated red light running enforcement implementations. Countries of origin for the preliminary list of reports and documents acquired included: Australia, Canada, Kuwait, the Netherlands, Singapore, South Korea, Sweden, the United Kingdom, and the United States. Of the screened studies, 44 were determined to be reports of evaluation studies of red light running automated enforcement and preliminarily reviewed for program description and evaluation elements. Many studies were excluded because they did not provide detailed study methods and results. In addition, literature review and the meta-analyses were not included in this evaluation list. However, such reviews were read and evaluated in the study to offer insights into methodological issues. Table 2 identifies the studies used in the full evaluation.

Table 2. Automated Red Light Running Enforcement Studies for Detailed Evaluation

Citation	Location of Intervention
Council, Persaud, Eccles, Lyon, Griffith (2005)	7 jurisdictions, United States
Washington & Shin (2005)	Phoenix, Arizona, United States Scottsdale, Arizona, United States
Cunningham & Hummer (2005)	Raleigh, North Carolina, United States
Garber, Miller, Eslambolchi, Khandelwal, Mattingly, Sprinkle, & Wachendorf (2005)	8 cities, Virginia, United States
Burkey & Obeng (2004)	Greensboro, North Carolina, United States High Point, North Carolina, United States
Synectics (2003)	6 cities, Ontario, Canada
Butler (2001)	Howard County, Maryland, United States Bucks County, Pennsylvania, United States

All of the studies were before/after assessments; most used a comparison group to control for trend factors. Most matched treatment and comparison sites with intersection features (e.g., traffic counts, car and truck ratio, amber timing phase, presence of warning signs).

While this study was primarily aimed at reviewing evaluations of prospective studies with current methodological considerations, there were a multitude of retrospective studies (i.e., literature reviews, meta-analyses, and systematic reviews) from the past two decades which could not be ignored. Table 3 identifies the retrospective studies reviewed.

Table 3. Retrospective Studies Reviewed on Automated Red Light Running Enforcement

Citation	Studies	Countries
Persuad, Council, Lyon, Eccles, & Griffith (2005)	15	Australia, Singapore, United Kingdom, United States
Aeron-Thomas & Hess (2005)	10	Australia, Singapore, United States
Hakkert & Gittleman (2004)	21	Australia, Canada, Singapore, United Kingdom, United States
Retting, Ferguson, & Hakkert (2003)	7	Australia, Canada, the Netherlands, Singapore, United Kingdom, United States
McGee & Eccles (2003)	14	Australia, Singapore, United Kingdom, United States
Flannery & Maccubbin (2002)	7	United Kingdom, United States
Maccubbin, Staples, & Salwin (2001)	30	Australia, Canada, Hong Kong, Singapore, United Kingdom, United States

Other Automated Enforcement Technology

While other AES applications were identified in the search, there were no substantive, scientific evaluations in the research literature that warranted further investigations. Instead, the published literature was limited to case studies and brief synopses of the location of the deployment and the purpose of the application. Product reviews and anecdotal reports of other AES applications found in the literature searches included: monitoring illegal railway crossings (United States); HOV/Bus Lane (Australia, United Kingdom, United States); tailgating (Israel); and toll booth violators (United States) (Peterson, 1995; Stoddard, 1996; Turner & Polk, 1998; Gaunt & Stevens, 1998; Turner, 1999, Retting, 1999; Institute for Transportation Research and Education, 2000; Transportation Development Centre, 2005). No studies were found that evaluated the use of automated technologies for enforcing aggressive driving violations.

RESEARCH FINDINGS

DESCRIPTION OF AUTOMATED ENFORCEMENT SYSTEM DEPLOYMENTS

Automatic enforcement systems (AES) cover a broad range of electronic devices and systems that detect and photograph vehicle actions defined as traffic violations. The most common uses are for speeding and red light running violations. Other less common uses include railroad crossing violations, aggressive driving and tailgating; HOV and bus lane violations, toll booth violations, and stopped school bus violation.

The technologies can include radar or laser detection devices, electromagnetic loops embedded in the road, pole-mounted or portable cameras, microprocessors, and networking devices. Most of the older systems rely upon 35mm film, which must be routinely extracted from the units, while the newer systems employ digital and video cameras and send the information over data networks. The systems rely on a vehicle tripping the electronic sensors (e.g., after a traffic light changes to red or when a vehicle's speed exceeds the posted limit by a certain designated amount) and photographs are taken of the front and rear of the vehicle, capturing the license plate number. Photographs are reviewed by the jurisdictional authorities. AES equipment is subject to scrutiny and reasonable calibration procedures (Retting, 1999).

Automated Speed Enforcement

Automated speed enforcement cameras take single spot or average vehicle speeds over several measurements. Most speed cameras use a low-powered Doppler radar speed sensor that triggers the camera to photograph vehicles traveling above a preset speed as they pass a specified point. The camera records the date, time, and vehicle speed, and usually is set to activate only when a vehicle is traveling significantly faster than the posted limit. Photo radar often is accompanied by a visible law enforcement presence to maximize the deterrence effect, but cameras can also be deployed unattended. Photographic evidence is reviewed and a citation is issued to the owner of the vehicle (Turner & Polk, 1998; Retting, 1999; Stidger, 2003).

Automated speed enforcement generally falls into two broad categories – fixed cameras and mobile systems. Fixed cameras are typically mounted in boxes at fixed locations and can continually monitor traffic speeds (without a human operator), depending on whether they are digitally connected to an electronic system or using local data storage or wet film (which would need to be replaced periodically). Even if the camera is not operating during certain periods, the traveling public would likely be unaware of this fact.

Mobile camera operations are deployed from police vehicles, typically unmarked, but in some cases vehicles are marked and operated overtly. Programs in the United Kingdom also operate some mobile deployments mounted on tripods by the side of the road. Mobile deployments may be rotated among sites so enforcement is not continuous at any one location. There is wide variation among countries and jurisdictions in the covertness of automated enforcement operations. Some mobile operations seem nearly as overt as fixed deployments (in space if not in time), while some jurisdictions have employed unmarked and hidden vehicles and unsigned enforcement zones in efforts to generate a more generalized deterrent effect.

A third type of automated speed enforcement, less frequently used at this time, is speed-over-distance systems. These systems photograph vehicles and measure speeds at both start and end points, and then determine whether an infraction has occurred based on the calculated average speed. The type of deployment mode has a number of ramifications for the design and thoughtful conduct of safety evaluation studies.

Automated speed measuring devices have been applied (and documented) to aid police efforts in enforcing compliance with speed limits in more than a dozen countries since the early 1990s. The Insurance Institute for Highway Safety (IIHS) estimated in 1999 that about 75 countries have deployed cameras to enforce speed limits. By the late 1990s, at least eight large cities and numerous smaller jurisdictions in the United States had piloted camera speed enforcement (Turner & Polk, 1998). In addition, several States plus the District of Columbia have enacted laws which allow the use of automated speed enforcement. Several of these laws restrict the use of camera speed enforcement to specific situations (e.g., work zones, school zones, specific hours) (IIHS, 2006).

Automated Red Light Running Enforcement

Red Light Cameras (RLCs) are set up to photograph vehicles entering intersections after traffic signal indications have turned red. Detection of an offense is made by sensors (electromagnetic loops) that are buried in the pavement and tied into the timing system of a traffic signal and a pole-mounted camera (either 35 mm or digital). Because the camera's position is fixed, only one direction of traffic flow is monitored at an intersection. Once the signal changes to red, the system is generally programmed with a small enforcement tolerance of 0.1 to 0.3 seconds, after which any vehicle crossing the loops will trigger the camera unit to take two photographs (Burkey & Obeng, 2004). The first photograph is taken of the front of the vehicle when it enters the intersection, and the second is taken when the vehicle is in the intersection. An image is captured that includes the license plate as well as the driver and often other passengers in the front and rear seats. Upon review of photographic evidence usually by a qualified law enforcement agent, a citation is issued to the owner of the vehicle, who is then sent a citation. Those charged with traffic offenses have the opportunity for judicial review (USDOT/FHWA, 2006).

RLCs are being used all over the world in such countries as Australia, Canada, Europe, Israel, Singapore, South Africa, the United Kingdom, and the United States. While RLCs have been used in other countries for more than 20 years, it has been only in the last 10 years or so that they have been used extensively in the United States (McGee & Eccles, 2003). Currently about a dozen States in the United States are using RLC applications.

IMPACTS AND EVALUATIONS (AUTOMATED SPEED ENFORCEMENT)

Most of the peer-reviewed evaluation work of automated speed enforcement has been done in countries other than the United States, including Australia and New Zealand, the United Kingdom, Denmark, the Netherlands, Norway, Sweden, and more recently, Canada. The science of evaluating road safety treatments is ever evolving but still depends on opportunistic situations, resulting in most studies being post-hoc before/after evaluations of treatments implemented at (high crash or perceived speeding) problem locations. Hauer's (1997) landmark study, entitled, "Observational Before-After Studies in Road Safety," attempted to provide methodological guidance to improve the accuracy and precision of estimated safety changes attributed to safety treatments. He described study design considerations and statistical methods to account for a number of known confounding factors in this type of study, where treatment and sampling methods are non-random.

Hauer (1997) demonstrated why safety should be measured in terms of crash frequencies over some time period, not crash rates per vehicle volume (or other measure of exposure). The use of crash rates assumes that the number of crashes increases at a constant rate with increases in volume. This has generally been proven wrong. It is possible that crash rates may decrease with increases in volume, even though nothing has been done to improve 'safety.' Nevertheless, volume does have an impact on crashes, and Hauer also argues that effects of traffic flows should ideally be accounted for directly in model estimates, by using performance function coefficients for similar road classes.

A variety of other trends, including crash reporting limit changes, improving vehicle fleet safety, increasing use of seat belts and helmets, and so forth, affect the counts of crashes and crash severity recorded over time. Property-damage-only or non-injury crashes are particularly variable (over time as well as space) as they are dependent on individuals' ability to estimate costs. For example, rising costs result in more non-injury crashes being recorded over time, and periodic adjustments in reporting thresholds cause sudden corrections. Still, for the purposes of road safety studies, Hauer recommends police-reported crash numbers as the standard outcome measure, but with consideration of the time trends and the uncertainty due to reporting and other errors (including location information) that exist in the data. The types of crashes to use depend on the target of the treatment. With treatments aimed at reducing speeds as the intermediate outcome toward improving safety, one would expect effects on crash severity and on speed-related crashes in general. Defining speed-related crashes from crash report data is often difficult, however, and there may also be effects on what would apparently be non-speed-related crashes, if speeds and speed variance are lowered.

Other possible confounders, including overlapping enforcement or other safety treatments, should be considered on a case-by-case basis as they exist. Study design and analyses should control for the effects of other safety countermeasures through the selection of treatment and comparison data samples and analyses.

Ideally, comparison groups are areas that should represent overall (unmeasured or unknown) trends, but should not be affected by the treatment at all (Hauer, 1997). Elvik (2002, p. 631) maintains that "In general, it is reasonable to assume that a large comparison group (i.e., one in which the annual count of accidents is at least several hundred) includes the effects of all factors that may produce changes over time in the long-term expected number of accidents." In addition to affecting overall outcomes of the treatment, spillover effects from the treatment program may have positive or negative safety effects on the comparison group if not carefully selected, potentially skewing results. For fixed (or conspicuous) enforcement, possible unintended negative consequences such as crash migration from treated to non-treated sites could occur. There are two mechanisms posited by which crashes might increase at other locations due to site-specific, conspicuous speed enforcement. If the enforcement zone is conspicuous or otherwise widely known, motorists may decrease speeds near the treated sites, perhaps abruptly, and increase their speeds before or after the treated zones to make up for lost time; this effect is sometimes characterized as the ‘kangaroo’ effect. The other hypothesis suggests that motorists may choose alternate routes to avoid treated locations. Thus, traffic flows may change due to the treatment and could be partially responsible for any decreases in crashes at the camera sites, but could contribute to crashes at alternate sites. Conversely, with covert enforcement, positive spillover might affect comparison groups, if not carefully chosen, and if there is a perception of widespread speed enforcement.

Another key issue with many before/after safety studies utilizing non-random site selection (i.e., when high-crash sites are chosen for the treatment), is regression toward the mean (RTM), the statistical tendency for high crash trends to shift toward the mean in subsequent time periods independent of any treatment (Hauer, 1997; Elvik, 1997; Elvik, 2002). Unusually low crash trends would similarly be expected to increase toward the mean, but since high-crash sites are typically chosen for treatment, this is not an issue in most non-randomized evaluation studies. If not controlled, RTM may explain a significant portion of observed changes, possibly resulting in an over-statement of safety improvement attributable to the treatment. Hauer et al. (2002) argued that the EB method, first applied to road safety by Abbess et al. (1981), should be the standard of professional practice to address the RTM effect, as well as increase precision of the estimates developed. Using this method, the weights of the safety performance functions for similar roads are reduced as more years of data are available for the subject site. Empirical Bayes procedures, as described in Hauer, also account for general volume trends in the performance functions.

A final issue in safety treatment correlation evaluation studies is establishing the mechanism, or causal link by which the treatment is hypothesized to reduce crashes (Elvik, 2004, 2005). In the case of speed enforcement, the objective is to reduce speeds (and perhaps speed dispersion) and crashes associated with excessive or inappropriate speeds for conditions. Ideally, then, speeds as well as safety effects will be determined in evaluation studies of automated speed enforcement, lending support for any positive safety effects found.

In this review of evaluations of automated speed enforcement programs, we used the issues introduced above to develop study evaluation criteria. These criteria were then used to assess study quality and draw conclusions regarding the effectiveness of automated enforcement programs at improving safety.

A synthesis of key study results follows. The extent to which studies addressed the factors described above guided the level of confidence that was placed in the safety change estimates reported. (Detailed descriptions of the studies, methods, and outcomes are provided in Appendix A - Annotated Bibliography – Part I: Speed). The programs evaluated fell into two broad categories based on whether the enforcement was by fixed cameras or mobile. Because these two types of deployments differ in ways that may affect driver behavioral adaptations with implications for study design and analysis and safety outcomes, results are described separately for these two broad types of automated speed enforcement.

In addition, the details of public information and awareness programs, the extent of operations, such as number of hours of enforcement, numbers of citations issued, etc., was not consistently described across all of the key studies. Type of penalty scheme also varied; and was not always adequately described. And finally, an enforcement threshold was reported in only a single paper (Goldenbeld & van Schagen, 2005).

Key Studies in Automated Speed Enforcement

The studies all reported decreases in estimated injury crashes or all types of crashes at camera sites or system-wide following implementation of automated speed enforcement using cameras. Table 4 provides an overview of these key studies and reported safety outcomes, along with our general rating of study quality. Studies are in order of decreasing conspicuity of enforcement. Results are summarized and discussed in the following paragraphs.

Table 4. Key Study Summaries – Automated Speed Enforcement

Reference	General Location	Target Roads/Speed Limits	Key Reported Outcomes	Study Quality Rating
1 – Fixed cameras, signs and or other alerts (brightly painted camera housings) at/near enforcement sites				
ARRB Group (2005)	Rural and urban (?)	Not described	-20% injury crashes at camera sites -23% casualty crashes (injury and fatal) (within 1 – 2.7 km)	Medium
Elvik (1997)		50, 60, 70, 80, and 90 km/h (31, 37, 43, 50, 56 mi/h) roads	-20% injury crashes (within average of 5.2 km)	Med- high
Hess (2004)	Rural and urban	Rural trunk roads and urban roads; major (A-roads) and minor roads (non-A) – speed limits not described.	-41% weighted injury crashes (within 500m either direction); effects higher on major roads and trunk roads	Medium
Mountain, Hirst, & Maher (2004)	Rural (?) and urban	48 km/h (30 mi/h)	-25% injury crashes - (within 500m either direction) with ~20% due to speed/behavior changes and ~5 % due to traffic diversion -24% injury crashes (within 1 km either direction) with ~19% being attributed to changes in speed and 5% to traffic diversion.	High

Table 4 (Cont'd). Key Study Summaries – Automated Speed Enforcement

Reference	General Location	Target Roads/Speed Limits	Key Reported Outcomes	Study Quality Rating
2 – Mobile cameras, signs at/near enforcement sites, marked deployment vehicles				
Cunningham, Hummer, & Moon (2005)	Urban	56 – 80 km/h (35 – 50 mi/h) high volume corridors	-12% all crashes (corridor)	Medium
Newstead & Cameron (2003)	Rural and urban	Multiple	-17.5% all severity crashes (within 1 km) -16% fatal & medically treated. (within 1 km)	Medium
3 – Mobile cameras, signs at or near deployment sites				
Christie et al. (2003)	Rural and urban	A majority of 48 km/h (30 mi/h) roads, Some 97– 113 km/h (60 – 70 mi/h) roads, A few 64– 80 km/h (40 – 50 mi/h) roads	-51% injury crashes – all roads (within 500m either direction) -51% injury crashes – 48 km/h (30 mi/h) roads -49% injury crashes – 97-113 km/h (60 – 70 mi/h roads	Medium
Goldenbeld & van Schagen (2005)	Rural	80 and 100 km/h (50 – 62 mi/h), single carriageway	-21% injury crashes (within avg. of 4 km)	Medium
Tay (2000)	Urban	Not described	-9.2% reduction in all crashes (treatment length not reported) -32.3% reduction in serious injury crashes	Medium
4 – Mobile cameras, general use of signs in jurisdiction				
Cameron, Cavallo, & Gilbert (1992)	Rural and urban	60 and 100 km/h (37 and 62 mi/h)	-30% day time injury crashes, (system-wide)	Medium
5 – Mobile cameras, no use of signs mentioned				
Chen et al. (2000)	Rural and urban	Multiple	-25% speed-related crashes (system-wide)	Med-high
Chen, Wilson, & Meckle (2002)	Rural	80 or 90 km/h (50 or 56 mi/h)	-16% all crashes (corridor-wide)	Med-high
6 – All types, fixed (including red light), mobile, speed over distance				
Gains et al. (2004)	Rural and urban	Multiple	-33% injury crashes (mobile and fixed sites) -40% killed and seriously injured (mobile and fixed sites) Fixed cameras associated with greater reductions	Medium

Fixed, Conspicuous Enforcement – Effects at Target Sites. Four of the reviewed studies evaluated local effects of fixed speed camera enforcement at the targeted enforcement areas (ARRB Group, 2005; Elvik, 1997; Hess, 2004; and Mountain et al., 2004). The study by Mountain et al. (2004) of fixed, conspicuous enforcement on 30 mi/h roads throughout Great Britain, controlled for changes in traffic flow, time trends, and RTM using comparison groups for crash and volume trends, and by using the EB method for estimating crash frequency changes. This study also provided results of speed studies documenting decreases in mean and 85th percentile speeds and percentage exceeding the limit. An earlier study by Elvik (1997) reanalyzed data used in a previous report, and also controlled for RTM using EB methods to estimate crash frequencies (expected and predicted). It is unclear if the other two studies adequately controlled for RTM.

Mountain et al. (2004) indicate that fixed camera speed enforcement reduced frequencies of injury crashes by about 25% up to 500 m from camera sites and 24% on aggregate up to 1 km upstream and downstream of camera sites over an average 2.3-year after-period. The authors therefore suggest that monitoring for longer distances can document greater overall crash savings and appears not to mask effects by including areas not affected by the cameras according to the authors. Cameras could have benefits beyond 1 km, but this study could not assess that outcome.

There was no evidence that sudden braking or speed changes resulted in increases in crashes over the distances examined. There was, however, evidence for shifting of traffic to other routes, sufficient to contribute 5% of the overall treatment-related decrease in crashes.

Significant RTM effects were observed for both personal injury crashes, and fatal and serious crashes. RTM accounted for around half of observed fatal and serious injury crash reductions, resulting in non-significant treatment effects on serious crashes.

The earlier study from Norway also found a significant reduction in estimated injury collisions at treated locations after applying methods to control for RTM and general crash trends (Elvik 1997). From the description of EB methods used, it is not entirely clear whether traffic volume was properly accounted for. Elvik reported a 20% reduction in the number of injury crashes at treated locations and a 12% reduction in property-damage-only crashes (data from one section), but this latter result was not statistically significant. Sections that conformed to both a crash rate warrant (crash rate of the site was higher than the crash rate for that type of road) and crash density warrant (the segment had at least 0.5 injury accidents per kilometer of road per year) introduced during the study period experienced a 26% reduction in injury crashes (statistically significant), comparable to the results reported by Mountain et al. (2004). Sections that did not conform to either of these warrants experienced a 5% reduction (not statistically significant). The largest effects were found for roads with speed limits of 60 and 70 km/h limits compared with lower- and higher-limit roads (but there was only one road section with a speed limit of 90 km/h). Unfortunately, effects on speed could not be determined.

The study from New South Wales, Australia, by the ARRB Group (2005) did not assess traffic flow changes, but did measure speeds and crash effects at adjacent sites up and downstream of the treated segments in addition to the treated segments. In addition to reporting reductions of about 20% in injury crashes and all crashes and of about 23% in casualty crashes (combined fatal and injury crashes), and sustained reductions in speeds at camera sites, their results suggest the possibility that driver behaviors contributed to crash migration. Proportions of excessive speeding increased by up to 40% in unenforced segments adjacent to treatment sites during the 24 months following implementation, and crashes increased in adjacent segments for some locations. The researchers argue that these slight increases in crashes were more than offset by decreases along the treated segments, but when camera length and adjacent length results were combined, crash reductions were non-significant. The length of camera segments ranged from a minimum of 1 km to 3.3 km (no average length reported). Up- and down-stream adjacent segments also varied more considerably in length. RTM was not controlled in this evaluation of effects at high-crash locations.

The study from Cambridgeshire, United Kingdom, did not assess effects of the fixed camera program on traffic speeds, and used weights based on expected frequencies of various crash severities in the estimating procedure for crashes. To account for trend, seasonality, and RTM effects, multiplicative coefficients to remove time-dependent components were computed, rather than using EB procedures. Traffic volumes were not explicitly accounted for, although the researchers indicate that "no significant changes in traffic levels over time have been reported for the sites under consideration other than those also observed on other sites (i.e., the regional trend) which is [sic] accounted for in the overall time-dependent coefficients (Hess, 2004, p. 31)."

Crashes over differing distances from the camera sites were examined, and authors argue that since highest reductions were found in the immediate surroundings of the cameras (within 250 – 500 m), no evidence was found for an increase in crashes resulting from abrupt braking due to the treatment. Crash migration to alternate routes was not examined. It is unclear to the present reviewers whether the time-dependent coefficients sufficiently accounted for RTM and how the weights for crash severity might have affected these results. Confidence intervals and significance levels were not reported for estimates of aggregate weighted injury crash reductions of 41 percent for up to 500 m, and 21% for up to 2 km from the camera sites (Hess, 2004).

<u>Mobile Enforcement – Effects at Target Sites</u>. The remaining studies addressed mobile camera enforcement, but there were varying levels of conspicuity associated with the programs.

Two studies addressed effects of mobile enforcement deployed in marked vehicles with signs or alerts at treated sites—seemingly the most overt mobile deployment described and thus most comparable to the fixed, conspicuous programs. Newstead and Cameron (2003) assessed changes within a 6 km area around camera sites in Queensland, Australia. Based on the overt speed camera operations, it was felt that effects would be primarily localized. Due to the way in which zones of camera operation were defined, however, (5 km in rural areas and 1 km in urban areas with multiple sites per zone), effects of up to a 6 km area of potential influence were used. Following treatment, aggregate crash reductions were highest in the 2 km area closest to the camera sites, but reductions were found in the 2 to 4 km areas and the 4 to 6 km areas as well (of varying severity levels). Estimated reductions of about 18% in all severity crashes and of 22 percent in hospitalization crashes were significant for up to 2 km. This study used a comparison group of all areas outside the defined 6 km areas of influence to account for potential confounders and general trends, but seasonality was not accounted for. Traffic volume also was not accounted for directly. Neither potential traffic migration nor crash migration were considered. However, camera sites, though signed and enforced using conspicuous vehicles, were selected randomly every day, and so treatment was at least unpredictable in time. Whether this type of deployment limited the potential for adaptive behavior by drivers is unknown. Additionally there were significant reductions in all-severity crashes in the 4 to 6 km area along with reductions in varying severities of crashes up to 4 and 6 km. RTM was not accounted for, although researchers discuss mitigating factors regarding site selection and implementation and a long (six-year) before period. Effects on speed were not studied.

In the only study examined from the United States—a short-term follow-up of a pilot program in Charlotte, North Carolina—the researchers assessed effects of an apparently similar, conspicuous, mobile enforcement program to that in Queensland. Fourteen high-volume, high-crash corridors were the treatment group; non-treated corridors in the city were the comparison group, although these tended to be lower-volume corridors (Cunningham et al., 2005). This study found aggregate effects of a 12% reduction in all crashes in the four-month after period for the treated corridors, and slight, but significant reductions in mean and 85th percentile speeds (<1 mi/h). Confidence in these results is somewhat limited due to the program having been in operation for only four months for this pilot study after period. Furthermore, results should be interpreted as a short-term impact.

Three studies assessed targeted effects of mobile camera enforcement (unmarked or covert) along with the use of informational/warning signs advising of the enforcement in the immediate area (Christie et al., 2003; Goldenbeld and van Schagen, 2005; Tay, 2000). Each used comparison sites to control for general trends, but did not directly control for traffic volumes or RTM effects. Tay (2000) reported less than 10% reduction in all crashes and a 32% reduction in serious injury crashes in the urban setting of Christchurch, New Zealand. Goldenbeld and van Schagen (2005) reported a 21% reduction in injury crashes along treated segments (average length of 4 km) in the rural areas of Friesland province, the Netherlands. Although not controlled directly, long before- and after-study periods in Goldenbeld and van Schagen may reduce the likelihood of RTM effects. Christie et al. (2003) estimated a 51% reduction in injury crashes for sections 500 m in either direction along camera sites in South Wales, United Kingdom. None of these studies examined possible effects on traffic flow, or crash reductions that might have been due to traffic flow changes as opposed to speed reductions.

At the opposite end of the spectrum from fixed, conspicuous enforcement, was a Canadian program utilizing covert, mobile enforcement. Two papers by Chen et al. explored the effects of mobile enforcement province-wide in British Columbia (2000), and along one highway corridor on Vancouver Island (2002). The results of the study of province-wide effects are reported below under system-wide effects.

Chen et al. (2002) examined corridor-wide safety effects using a comparison group and EB methods to control for trends and RTM. Results indicate that the mobile-radar program resulted in reduced traffic speeds at the treated sites and at a non-treated location along the corridor for two years following implementation (Chen et al., 2002). Results suggest generalized crash reductions of 16% (± 7%) throughout the corridor with no evidence of crash migration to non-deployment locations (non-photo-radar influenced) to compensate for time loss (kangaroo effect). An estimated decline of 14% (± 11%) in collisions (all severities) occurred at the treated locations (1 km either direction) and a 19% (± 10%) reduction in collisions occurred at the interleaving non-treated locations (> 1 km from camera site). An examination of crash severity, was not, however, provided. The researchers maintained that no realistic alternate corridors were available, so traffic and crash migration to other non-enforced roadways was assumed not to have occurred. Enforcement was unpredictable in time and space and relatively frequent in space (rotated among the 12 sites along the 22 km corridor). Results may not be generalizable to other corridors with different enforcement parameters or other characteristics. Effects of a publicized, province-wide automated enforcement program may also have contributed to spillover effects on the reference group, possibly resulting in understatement of effects.

A study by the PA Consulting Group of the National safety camera program in the United Kingdom evaluated the speed and casualty effects, along with public acceptance, and costs and benefits and program administration, of fixed (including red light), speed-over-distance, and mobile camera enforcement under the cost-recovery system (Gains et al., 2004). The program, begun with eight counties in 2000 and expanded to 24 partnership areas by April 2002 (the focus of this study), allows local road safety partnerships to recover their costs of administering automated enforcement programs, subject to strict criteria. Guidelines for the partnerships, management and expenditures, public communication, site selection (includes collisions warrants and review for other engineering solutions), fixed camera conspicuity, and monitoring of results were established. The study included effects of new installations, as well as effects of increased conspicuity, and joining the cost recovery program.

On average, across all new camera sites, there were reductions in speeds and reductions in percentages of speeders and excessive speeders, with greater reductions at fixed-camera installations. Successive speed measures suggested that fixed sites also have a more immediate, but sustained impact, with mobile sites taking somewhat longer for the full speed effect (reduction). Personal injury crashes were reported reduced by 33%; numbers killed and seriously injured were reduced by 40% on average with higher reductions at fixed-camera sites and in urban locations. Thus, there appears to be a reduction in the number of injury, serious injury crashes, and speeds following the introduction of cameras. RTM was not, however controlled, but the authors argue that it should not be a factor because the number of crashes was not the sole criterion for site selection. It was, however, a primary factor in the cost-recovery program. The effects on traffic flow were also not accounted for. Especially in the use of the highly conspicuous, fixed cameras, which showed the highest local reductions in injury crashes and serious injuries and fatalities, effects on traffic diversion and potential crash migration could be an issue.

Mobile Enforcement – System-Wide Effects. The more covert the speed enforcement, the more generalized the effects that might be expected. Two studies attempted to assess general area-wide effects of automated camera enforcement on safety. Both of the programs utilized covert enforcement. The British Columbia, Canada, ASE program apparently used no warning signs advising of the automated speed enforcement although there was general publicity of the program (Chen et al., 2000). The Victoria, Australia, program likewise publicized the campaign and in addition, used general warning signs of the program in the region (for example at all major entrances to the city of Melbourne), but did not sign specific automated enforcement zones (Cameron et al., 1992).

In British Columbia, a significant reduction in mean speeds at monitoring sites lacking photo radar enforcement suggests a generalized province-wide decrease in speeding following program implementation (Chen et al., 2000). The estimated 25% reduction in daytime, unsafe speed-related collisions province-wide and reductions in victims carried by ambulances (-11%) and in fatalities (-17%, trend) concurrent with reductions in speed indicates that the program was effective at improving safety. The study did not examine possible effects on other-injury or no-injury crashes. The study could not rule out effects of confounding programs (such as an overlapping alcohol enforcement program), but through the research design (use of daytime only, speed-related collisions, and daytime serious injuries and fatalities, even though the program had some nighttime hours of operation), attempted a conservative approach to estimating the safety effect. It is unclear how the use of speed-related crashes compared with all crashes or all-injury crashes might change the estimates of safety effects. The evaluation covered only the first year of operation and thus, longer-term impact or sustainability, was not addressed. A long before-study period (five years) and the use of province-wide crashes would seem to reduce the likelihood of RTM playing a role in these results. The possibility of crash migration could not be examined in this study of province-wide effects, nor in the following study, but is thought to be unlikely due to the covert and mobile nature of the deployments.

A study of the state- and citywide effects of automated photo-enforcement from Victoria and Melbourne, Australia, was one of the earlier studies of automated speed enforcement (Cameron et al., 1992). This study, using time-trend analyses, found that estimated system-wide, daytime casualty (fatality and injury) crashes were reduced by about 20% statewide from before to after full implementation of the speed camera program and associated publicity. Crashes were reduced by about 21% in the urban area of Melbourne (where more enforcement took place) and by a little less in the rural areas of Victoria. Crash reductions were highest on arterial streets in Melbourne, and 60 km/hr rural roads in the province. Again, since the study examined system-wide effects, RTM may not be a serious issue. Effects on traffic speeds were not documented, but the study found evidence of effects of program intensity. Crashes and crash severity in Melbourne were significantly related to number of traffic infringement notices mailed (system-wide), and crash severity was also related to hours of operation, providing more support for a program effect.

One systematic review of automated speed enforcement evaluations was identified. Pilkington and Kinra (2005) provided a systematic review of 14 studies, a number of the same papers were included in this review. They established six study quality measures and established a three-point scale, from 0-2, for each measure. The quality criteria established were: "representativeness of the study areas to general population; control areas being representative of intervention areas; objective and valid outcome(s); results provided with estimates of uncertainty; main conclusions based on primary study hypotheses; and important confounders measured and controlled for." Studies were given numerical ratings based on the sum of their scores on individual measures, with scores of 9-12 rated as good, 6-8 rated as average, and 0-5 as poor. They concluded that, although study quality was in general poor (half of the studies received the lowest average rating of 6, the remainder were lower), that the research consistently shows that speed cameras are an effective intervention in improving road safety.

IMPACTS AND EVALUATIONS (AUTOMATED RED LIGHT RUNNING ENFORCEMENT)

Most of the peer-reviewed evaluation work of red-light running automated enforcement (using cameras) has been done in Australia, Canada, the Netherlands, Singapore, the United Kingdom, and the United States; and like speed enforcement evaluations, it is dependent on opportunistic situations, resulting in the studies being post-hoc before/after evaluations of treatments implemented at high red light running crash and violation intersection locations.

Retrospective Studies

A broad concern over the study methodologies that have been used to evaluate the impact of red light camera (RLC) systems on violations and crashes at intersections is reflected in the number of research reviews, syntheses, and meta-analyses recently published on this topic (Flannery & Maccubbin, 2002; McGee & Eccles, 2003; Retting, Ferguson, & Hakkert, 2003; Hakkert & Gitelman, 2004; Aeron-Thomas & Hess, 2005; and Persaud, Council, Lyon, Eccles, & Griffith, 2005). This body of work, generally, has identified sources of variance in outcome measures that have not been controlled and/or have not been adjusted for in the statistical analysis methods used to quantify RLC treatment effects in research to date. After acknowledging such shortcomings, recent syntheses have applied criteria to select the most rigorous evaluations described by existing literature, then have focused on this subset of studies to arrive at estimates of the direction and magnitude of changes in safety related to RLC installations. Recommendations for improved research designs to obtain more reliable measures of RLC effectiveness have also been suggested.

As noted in these literature reviews, another difficulty with past studies has been the likely exaggeration of positive effects (reductions in crashes and/or violations) due to a "regression to the mean" at RLC treatment sites. Since RLC installations are typically located at sites described by the highest frequencies of events—instead of, for example, by random assignment—part of any reduction observed at these sites after RLC installation may simply reflect data that fall more in line with area-wide averages for the outcome(s) of interest, which would be expected with *or* without the RLC treatment. Many of these studies also failed to account for "spillover effects," where the installation of RLC systems at isolated locations may (hypothetically) influence motorist behavior within an entire jurisdiction. This phenomenon, if real, is highly desirable from a cost-effectiveness standpoint; at the same time, ignoring it will lead to an *under*estimate of RLC benefits.

These retrospective literature reviews are usually very limited in describing the explicit details of previous RLC evaluation studies. Even the most comprehensive reviews that can be found in the technical literature today ignore aspects of intersection operations that profoundly influence motorist behavior. Setting aside the rare cases where individuals completely fail to detect a red signal, drivers who choose to enter an intersection after red onset include those who are caught in a "dilemma zone" during the yellow phase, and are genuinely uncertain about their ability to safely stop in the distance available, and those who are able to stop comfortably in the distance available but deliberately continue into the intersection after the yellow phase has timed out. Conventional wisdom has it that traffic engineering measures (e.g., the all red clearance interval) target the former group while RLC treatments (and other types of enforcement) target the latter group (see Hakkert & Gitelman, 2004). While this may be true, the impact of traffic engineering measures are nevertheless demonstrated on a *population basis*; a shift across the entire distribution of motorists, for any number of behaviors upstream of or at an intersection—including but not limited to red light infractions—may be expected as a function of RLC warning sign placement, RLC sign conspicuity, length of the signal cycle, duration of the yellow phase, etc. These influences *must* be formally accounted for in continuing efforts to evaluate RLC effectiveness.

Given all of the methodological concerns and limited detailed descriptions of study design and characteristics, the retrospective studies (i.e. literature reviews, meta-analyses, and systematic reviews) from the past two decades should not be ignored. Many of these reviews identified violations as the key outcome measure. Studies of United States RLC installations identified violation reductions from 20% to 87%, but more than half of the studies reported a reduction in the range of 40 to 60% (Maccubbin et al., 2001). Studies from around the world reported similar results (Winn, 1995; Mullen, 2001; Lum and Wong, 2003; Zaal, 1994; Chin, 1989). One study reported reductions in violations for nearby signalized intersections without the RLC installations. The researchers suggested some sort of improved driver compliance or "spillover" effect at these other locations in the vicinity of the treatment (Retting et al., 1999).

In addition, the retrospective studies which also included crash frequency outcome measures reported, for the most part, a decrease in angle crashes with a slight increase in rear-end crashes (Hillier et al., 1993; Mann et al., 1994; Fox 1996; Retting & Kyrychenko, 2002; McGee & Eccles, 2003). The longer-term impact studies of RLC effectiveness (three- to five-year range) showed mixed results in crash reduction. Hillier et al. (1993) and Fox (1996) showed a reduction in crashes, but Andreassen (1995) actually showed an increase. Retting and Kyrychenko (2002) reported a reduction in injury crashes in cities with RLCs installed compared with nearby cities without the RLC treatment.

Key Studies in Red Light Running Enforcement (Prospective Studies)

A synthesis of key study results follows. The extent to which studies addressed the factors described above, guided the level of confidence that was placed in the safety change estimates reported. (Detailed descriptions of the studies, methods, and outcomes are provided in Appendix A - Annotated Bibliography – Part II: Red Light Running). Like the speed enforcement evaluation studies, details of public information and awareness programs, placement of warning signs (e.g., approach to intersection, jurisdiction border), the extent of operations (e.g., RLC installations completed over the course of a few months, not simultaneously), amber signal timing length, etc., were not consistently described across all of the key studies. Type of crashes, and injury severity accounts also varied across the key studies. When violation outcome measures were used in the study, it was not always clear whether these were all paid citations or if a percentage were dismissed upon judicial review and hearing.

In general, the key studies focused primarily on crash frequencies and rates; type of crash (e.g., angle, rear-end); and severity of the crash (e.g., property damage only, injury, fatality). In general, RLC treatments contributed to a small but statistically significant decrease in total right-angle crashes (Council, Persaud, Eccles, Lyon, & Griffith, 2005; Cunningham & Hummer, 2005; Washington & Shin, 2005), yet were associated with an increase in total rear-end crashes (Council et al 2005; Garber, Miller, Eslambolchi, Khandelwal, Mattingly, Sprinkle, & Wachendorf, 2005; Burkey & Obeng, 2004; Butler, 2001; Washington & Shin, 2005). In addition, only two studies varied from these general results (Garber et al., 2005; Cunningham &

Hummer, 2005; Butler, 2001). Two of the selected studies reported a significant reduction in injury right-angle crashes (Council et al., 2005; Synectics, 2003). Other studies reported insignificant increases in total-injury crashes (Garber et al., 2005); and insignificant increase in severity crashes, but significant increase with property-damage-only crashes (Burkey & Obeng, 2004; Synectics, 2003). Only two of the studies looked at frequency of citations before and after treatments; these researchers found significant reductions in citations (Garber et al., 2005; Cunningham & Hummer, 2005).

Table 5 provides an overview of these key studies and reported safety outcomes, along with our general rating of study quality. A brief summary and evaluation of the impacts of the key studies follows. They are presented in chronological order by the most recent date of publication.

One of the most statistically defensible studies was recently conducted by Council, Persaud, Eccles, Lyon, and Griffith (2005) for the Federal Highway Administration (FHWA). They used an empirical Bayes (EB) before-and-after (B/A) approach with a large selection of signalized treatment intersection sites (132), signalized comparison intersection sites (408), and unsignalized comparison intersection sites (296) across 7 jurisdictions in the United States. Data on amber signal timing phases; traffic volume; and publicity were also collected for each intersection. They found crash effects that were consistent in the direction of those found in many previous studies. That is, a decrease in right-angle crashes, yet an increase in rear-end crashes. They also conducted an economic effects analysis based on an aggregation of right-angle and rear-end crash costs for two severity levels (property-damage-only and all other injury-related). They found that RLCs do provide a modest aggregate crash-cost benefit. In addition, further disaggregate analyses of the economic effects were conducted to try to isolate the factors that would favor or discourage the installation of RLCs. It was found that RLCs should be considered at intersections with any of the following operating conditions:

- A high ratio of right-angle crashes to rear-end crashes
- Higher proportion of entering average daily traffic (ADT) on the major road
- Shorter cycle lengths and green timing phase periods
- One or more left-turn protected phases

The researchers also concluded that the presence of warning signs at both RLC intersections and city limits *and* the application of high publicity levels would be effective strategies to enhance the benefits of RLC systems.

In Arizona, a study funded by the Arizona Department of Transportation and FHWA, used 24 treatment sites in Phoenix and Scottsdale, Arizona. The researchers used four different statistical analysis approaches (simple before and after [B/A] with ratio of durations, B/A with traffic flow corrections, B/A with comparison group, and EB B/A approach) to deal with the technical challenges and assess the sensitivity of results to analytical assumptions. They analyzed crash type and severity; amber signal timing phase; approach speeds; left-turn phasing;

Table 5. Key Study Summaries – Red Light Running Enforcement

Reference	Study Locations	Study Period (in months) Total and Pre-intervention/Post-intervention Periods	Number of Intersections Evaluated (Test/Control *)	Key Reported Outcomes
Council, Persaud, Eccles, Lyon, & Griffith (2005)	Baltimore, MD Charlotte, NC El Cajon, CA Howard County, MD Montgomery County, MD San Diego, CA San Francisco, CA	72 (on avg.) 60/12 (on avg.)	132/408 and 296 unsignalized control	Aggregate results showed decrease in right angle crashes and reduction in injury-related; increase in rear-end crashes and injury-related; positive cost-benefit found
Washington & Shin (2005)	Phoenix, AZ Scottsdale, AZ	72 (36/36) 180 (72/72 on avg)	11/249 15/925	Reduction in angle and left-turn crashes outweighed increase in rear-end crashes. Net crash benefit relatively large in Scottsdale
Cunningham & Hummer (2005)	Raleigh, NC	74 (67/7)	7/10	Reduction in total crashes, angle and rear-end crashes
Garber, Miller, Eslambolchi, Khandelwal, Mattingly, Sprinkle, & Wachendorf (2005)	Arlington, VA Fairfax City, VA Falls Church, VA Vienna, VA	72 (36/36) (crash data) 9 (6/3) (violation data)	12/33 (Fairfax Cty) 11/0 (other cities)	Reduction in number of violations; net decrease in red-light running crashes (angle versus rear-end)
Burkey & Obeng (2004)	Greensboro, NC High Point, NC	57 (26/21)	18/285	Increase of most intersection crash types
Synectics (2003)	Toronto, Ontario Hamilton, Ontario Ottawa, Ontario Hamilton, Ontario Peel, Ontario Waterloo, Ontario	84 (60/24)	48/12/17	Reduction in fatal and injury crashes. Cost-effective treatment for reducing injury and fatalities related to intersection crashes

Table 5. (Cont'd) Key Study Summaries – Red Light Running Enforcement

Reference	Study Locations	Study Period (in months) Total and Pre-intervention/Post-intervention Periods	Number of Intersections Evaluated (Test/Control *)	Key Reported Outcomes
Butler (2001)	Howard County, MD Bucks County, PA	60 (30/30)	2/8	Reduction in angle crashes at test sites, not significant

* Control sites defined as those intersections without a red light camera installation

traffic volume; and presence of warning signs and publicity. The results were as follows: both cities showed reduction in angle crashes by 14 and 20% for both treatment and control sites, respectively. However, only one of the cities showed a reduction in left-turn crashes (45%). Rear-end crashes increased in both cities by 20 and 41%, respectively. The total number of crashes was unchanged in Phoenix, but in Scottsdale they were slightly reduced (11%). It was found that RLCs appear to systematically reduce the frequency of angle- and left-turn crashes at intersections; however, the frequency of rear-end crashes increases (Washington & Shin, 2005).

The Arizona study found that when crash severities and costs were considered and intersections were analyzed as an aggregate (and not by individual sites), RLCs provided modest to relatively large benefits. Phoenix showed only a net safety benefit of $4,504 per year (for the 10 target approaches) and Scottsdale showed a net safety benefit of $684,134 per year (for the 14 target approaches), primarily based on the fact that in Scottsdale the RLCs contributed more to decreasing fatal and injury angle- and left-turn crashes than to decreasing property-damage-only (PDO) crashes (Washington & Shin, 2005).

The researchers warned that examination of crash frequencies alone was not sufficient to understand the impact of RLCs. It was apparent through close examination that the severity of crashes was affected by RLCs, and this outcome was an important study consideration in the adoption and or implementation of such RLC programs. The small sample sizes (of installation sites) did not provide statistical evidence on the effects of warning signs. The researchers speculated that drivers seemed to be less likely to run red lights but more likely to rear-end a lead vehicle when they were warned that a RLC was present. In addition, other study limitations were revealed, including: variations in amber signal phasing across installations; differences in traffic volume across intersection sites; and differences in general publicity programs across each city (Washington & Shin, 2005).

A recent North Carolina study (Cunningham & Hummer, 2005) used matching treatment (7) and comparison sites (10) in two cities (Raleigh and Chapel Hill), and conducted three types of before and after crash analyses (causal factors, a comparison group analysis, and an improved comparison group analysis accounting for the halo effect) applying a chi-square test of independence. For the causal factors (time and traffic flow) analysis, total crashes, red light running (RLR)-related crashes, and angle crashes decreased by 30 to 51%, with only a very slight increase in rear-end crashes (2%). For the comparison group analysis, where comparison sites were chosen in a near random fashion, total crashes, RLR-related crashes, angle crashes, and rear-end crashes decreased by 17 to 42%. For the comparison group accounting for the halo effect, total crashes, RLR-related crashes, angle and rear-end crashes all decreased by 14 to 35%. The smallest decrease was shown for total crashes at the comparison sites (accounting for the halo effect); however, this crash type still showed a substantial decrease associated with the installation of the cameras (a 14% decrease). The researchers noted that seasonality effects could have influenced the results of the analysis, because weather and driving patterns change over time and the study did not have a full year of data in the "after" period. The "before" period of study included several years of data. The chi-square test revealed a significant reduction in the frequency of "dangerous" red light running violations (entry into the intersection two or more seconds into the red-light signal phase).

A study of the effects of RLC treatments on red light running violations and crashes was conducted in northeastern Virginia (Fairfax City, Fairfax County, Falls Church, and Vienna). Violation data were collected at 22 RLC treatment intersections over the course of a 6-month period. The study compared the first three months of post-installation data with the second three months of post-installation data. The number of citations for red light running varied substantially by intersection; but citations decreased by an average of 21% per intersection from the first 3 months of study versus the latter 3 months (Garber, Miller, Eslambolchi, Khandelwal, Mattingly, Sprinkle, & Wachendorf, 2005).

Crash data were collected at 12 RLC sites with 33 non-camera (comparison) intersections in Fairfax County; 2 RLC sites in Falls Church; 6 RLC sites in Vienna; and 3 RLC sites in Fairfax City. Six years of crash data were collected (three pre- and three post-). Basic crash analyses (using *t*-tests and paired sample tests) showed that there were some statistically significant decreases in the number of crashes in two of the four jurisdictions; however, there were mixed results for total crashes and total angle crashes, and non-significant increases in total injury crashes. At this level of analysis, the researchers suggested that RLC enforcement reduced the number of crashes attributable to red light running (i.e., crashes where one or more drivers were charged with failure to yield to a stop light). Further analyses (using Analysis of Variance and EB methods) indicated that RLCs were contributing to a definite increase in rear-end crashes, combined with a net decrease in injury crashes attributable specifically to red light running; a possible decrease in angle crashes, and an increase in total injury crashes was also demonstrated. Therefore, RLCs lead to a net improvement in safety only if the severity of the eliminated red-light-running crashes is greater than that of the induced rear-end crashes (Garber et al., 2005).

In addition, when comparing crash frequencies with site characteristics, it was found that, despite an overall increase in crashes, RLCs lead to a decrease in crashes under the following circumstances: higher speed limits; higher traffic volume; fewer through lanes; an amber phase that exceeds the ITE standard; and lower truck percentages (Garber et al., 2005).

In North Carolina, a large study funded by the U.S. DOT Research and Special Programs Administration examined 18 treatment and 285 control sites in two small urban areas (Greensboro and High Point). In addition to crash type, severity, and frequency, and citation frequency data, the following operational and geometric characteristics were identified for analysis: amber signal timing phases; number of lanes; traffic volume; presence of median; posted speed limit; and average daily traffic volume. A randomized trial design was not possible; instead, the researchers used signalized intersections without cameras in the same communities as the treatment sites as controls. The RLCs were installed at intersections with crash rates at least twice as high as other intersections in the cities (Burkey & Obeng, 2004).

RLC camera installation resulted in a broad range of results with respect to crashes, from a 30.8% decrease to a 68.8% increase. There was an increase in rear-end crashes, a small increase in angle crashes, and a decrease in left-turn/same-roadway and left-turn/different-roadway crashes. A Poisson Regression Model controlling for weather, traffic volume, and intersection characteristics was applied. The model estimated a very large impact of RLCs on rear-end crashes (a 78% increase). For crash severity, RLCs were found to have a large and statistically significant effect on property-damage-only and possible-injury crashes. There was a positive but not statistically significant effect on severe crashes. The researchers concluded that the results did not support a conclusion that RLCs reduce crashes. In fact, they pointed out that the installation of these AESs are a detriment to safety (Burkey & Obeng, 2004). This study has been critiqued by researchers from the Insurance Institute for Highway Safety (Kyrychenko & Retting, 2005), who have questioned the study methodology with respect to the comparison site locations (same community suggests a possible spillover effect) and the lack of random assignment of RLC sites.

In Canada, a recent study in six Ontario municipalities assessed the combined effect of two red-light-running countermeasures (RLCs and stepped-up police enforcement) for intersections with a high incidence of red-light-running-related crashes. There were 19 RLC treatment sites, 17 police enforcement sites, and 12 comparison sites. Sites were matched according to duration of amber and red signal phases, number and type of lanes, traffic volume and truck volume, posted speed limit, and placement/visibility of signal heads. An EB method was used for deriving estimates of the overall effectiveness of the two treatments. That is, a comparison between the expected number of crashes that would have occurred if the treatments had not been implemented, and the observed number of crashes that actually occurred with the treatments implemented provided the basis for the safety effectiveness evaluation at the 48 study sites. RLC treatments contributed to a 6.8% decrease in fatal and injury crashes; and an 18.5% increase in property-damage-only (PDO) crashes. For angle crashes, RLC treatments contributed to a 25.3% decrease in fatal and injury angle crashes; and a 17.9% decrease in PDO angle crashes. For rear-end crashes, RLC treatment crashes contributed to a 4.95% increase in fatal and injury rear-end crashes; and a 49.9% increase in PDO crashes (Synectics, 2003).

A final study evaluated the effectiveness of RLCs in Howard County, Maryland, which had been implementing automated enforcement for red-light-running violators for over eight years at the time of the study. A chi-square analysis was performed in a before-treatment versus after-treatment study, using two treatment sites. Four sites in a neighboring State (Pennsylvania) were chosen as control sites, to assess spillover effects; traffic volume and signalization characteristics were matched at the treatment and control sites. A third site type ("non-treatment") consisted of four intersections adjacent to the treatment sites in the same county, with non-operable RLC camera mounts. While there were no significant differences in the rate of right-angle crashes between the treatment sites and control sites, or between the non-treatment sites (non-operable cameras) and the control sites, there was a significant difference in right-angle crashes between the before- and after-camera-installation time periods at the two treatment sites, indicating that the difference can be attributed to the presence of RLCs (Butler, 2001).

INTERPRETATION OF FINDINGS AND METHODOLOGICAL CONCERNS

AUTOMATED SPEED ENFORCEMENT

The reviewed studies evaluated a mix of fixed and mobile, varied conspicuity, automated speed enforcement programs over a variety of urban/rural locations, speed limits, and road types. Program parameters such as penalties, enforcement thresholds, publicity, and enforcement intensity also varied among the programs; therefore, generalizing conclusions among the programs was not attempted. All of the studies attempted to control for general trends that may affect crash frequencies, although in some cases comparison groups may have been affected by the treatment or by other enforcement programs or treatments. In other instances, the comparison group may have been outside the jurisdiction and it is often not clear whether before period general crash trends, possible confounding countermeasures, and other factors were the same between treatment and comparison groups.

The studies using EB methods attempted to control for general traffic volumes. Most of the remaining studies controlled only indirectly for traffic volume/exposure (through the use of comparison groups) although the two system-wide studies used proxies for overall travel. Elvik (2002) argues that general trends in traffic volume are adequately controlled using a comparison group, and that statistically estimating effects due to traffic volume will result in double counting. This conclusion seems, however, not to take into account that traffic flows may change differently for the treatment and comparison groups, due to the treatment itself or to other confounding factors. If traffic before and after traffic flows are not measured and accounted for in models, such information would remain unknown. One would presumably also want to know what proportion of the safety effect is due to traffic diverting to alternate routes since that may impact safety and operations on the other routes. One of the reviewed studies (Mountain et al., 2004) examined “before” and “after” traffic flow. The differences in national trends and site trends were attributed to the treatment since changes due to other causes were ruled out, and amounted to 5% of the overall 25% treatment effect.

Only four of the studies attempted to control for regression to the mean (RTM); three of these used EB procedures. These confounding factors are all difficult issues to address in studies utilizing ‘natural experiments;’ but if not controlled, may introduce inaccuracies in safety improvement estimates. All of the studies—including those studies accounting for trends, RTM, and traffic flow—found significant reductions in estimated crashes following program implementations; about half of the studies also documented reductions in speeds that suggested a plausible link between the treatment and the safety effect.

The treatment area or segment length for which an improvement was reported was variously defined, particularly with respect to mobile enforcement zones. A few of the studies examined camera treatment effects over different distances or areas in an effort to ascertain what ‘catchment area’ size (and shape) best captured safety effects (Chen et al., 2002; Christie et al., 2003; Mountain et al., 2004; Hess, 2004). Two of the reports that examined effects for different distances from camera sites found benefits were greatest nearest to camera sites, but Mountain et al., (2004) found proportional effects when they extended the area up to 1 km in either direction that were comparable to those observed when they included only 250 m or 500 m. Thus, they were able to capture larger overall reductions in crashes. Chen et al. (2002) found that crash reductions were actually somewhat greater (not significantly) at interleaving, non-enforced zones more than 1 km from covert, mobile camera sites. Other researchers examined even larger treatment zones, up to entire corridors. It is unknown whether effects would have been lower or higher had they examined effects over different distances.

Estimates of injury crash reductions at treated sites (of varying lengths) from three studies, two of which were among the best-controlled studies of the effects of fixed cameras were in the range of 20 to 25 percent (ARRB Group, 2005; Elvik, 1997; and Mountain et al., 2004). Two of these controlled for volume and RTM (Elvik, 1997; and Mountain et al., 2004) and two documented reductions in speeds as well as crashes (ARRB Group, 2005; and Mountain et al., 2004). Using EB procedures, the latter group also documented significant and sizable crash-reduction effects due to RTM—up to half of observed fatal and serious crashes—highlighting the importance of controlling for RTM. While program effects on fatal and serious crashes were non-significant with RTM controlled, the authors argue that this result is not unexpected, due to generally lower levels of fatal and serious crashes. The authors suggest that this result is not unexpected due to the relatively low numbers of serious injury crashes on the treated segments and argue that sites with large numbers of less serious crashes can also benefit from increased enforcement and perhaps proactively prevent more serious crashes.

One would not expect fixed speed enforcement to have a pronounced beneficial effect outside of the specific treated locations and none of the studies examined system-wide effects of fixed speed camera deployments. There may, however, be negative system-wide effects if adaptations become extreme enough to offset improvements at treated sites. Whereas fixed deployments may operate more continuously at a specific location, perhaps resulting in more rapid and larger site improvements, the conspicuity and 'dependability' of this type of enforcement means that drivers may adapt more readily to its presence and change routes or modify speed only over a certain distance, and perhaps even increase speeds at non-enforced segments.

A couple of studies found evidence of traffic diversion and speed adaptations to fixed speed cameras. Mountain et al. (2004) estimated the effects of the treatment due to changes in traffic flow to be about 5% of the 25% overall reduction due to the treatment; presumably, the remaining 20% was due mostly to speed reductions. While the traffic flow changes had a beneficial effect at the enforced sites, this result suggests the possibility of a negative impact on alternate routes or locations. The ARRB Group (2005) documented increases in speeds and crashes over a two-year period at some non-enforced sites adjacent to treatment sites. Their results also suggest the possibility of driver adaptation and of crash migration resulting from the treatment. In this case, the likely scenario is that drivers increased speeds at non-enforced locations in advance of and following the enforced sites to make up time. They found, however, that the increases did not offset the reductions in crashes at camera sites, but crash reductions across treated and adjacent segments combined were non-significant. Both of these studies underscore the importance of evaluating crash effects over distances beyond the perceived impact zone, and of monitoring traffic flow around conspicuous or fixed camera sites and alternate routes.

Reported reductions for mobile enforcement programs were more variable, perhaps reflecting in part the variable nature of the enforcement including conspicuity and intensity, as well as differing sampling and study methodologies. Only one study of mobile enforcement controlled for RTM as well as general trends. This study of a covert enforcement program found a 16% corridor-wide reduction in all crashes (effects similar at inter-leaving non-deployment and 12 deployment locations) along a 22 km corridor (Chen et al., 2002). It is possible that effects of the same widely-publicized automated enforcement program, which had been previously reported to have a province-wide effect, may have influenced the comparison group and resulted in the treatment effect being underestimated.

Other studies of more overt mobile deployments found a wide range of crash reductions—from about 9% to 18% in all crashes, and from 21% to 51% in injury crashes at the treated locations. Confidence in these results is limited based on lack of control for RTM, short study periods for some studies, issues with comparison groups, and other factors. In the cases of signed mobile camera enforcement zones and otherwise conspicuous mobile enforcement, motorists might also avoid the area or adapt speeds over short distances, even though cameras are not present at all times. None of the studies of overt, mobile enforcement operations has directly examined the possibility of traffic or crash migration to non-enforced routes or segments.

At the opposite end of the spectrum, there may be less opportunity for behavioral adaptations to covert mobile enforcement to negatively impact crashes at non-treated locations. A more generalized deterrent effect over a wider area might also be a goal of the treatment. Two of the studies examined system-wide (entire province/State) effects of mobile enforcement and found reductions of 20% in daytime casualty crashes, and of 25% in daytime, unsafe speed-related crashes, respectively (Cameron et al., 1992; Chen et al., 2000). Chen et al. (2000) also documented generalized speed reductions. While there may be unknown confounding factors, both studies used time-trend analyses to account for general trends, proxy measures to adjust for travel exposure, and Cameron et al. (1992) also used a neighboring State as a comparison group until automated speed enforcement was also implemented in that State.

Since mobile cameras are not used continuously at a single site, site-specific effects of mobile cameras may also take longer to be realized, or appear to be lower than for fixed sites, as reported by Gains et al. (2004). It is possible, however, that benefits are spread over a wider area, and are not being fully captured in the studies that examined effects only over limited areas surrounding treated sites.

While program and study differences prevent a generalized conclusion about the safety improvement that may be expected from automated speed enforcement programs, Hirst et al. (2005a) analyzed the relationship between speed reductions and crash reductions in a comparison study of engineering and automated enforcement treatments. They argue, based on results of models, that each 1 mi/h speed reduction on 30 mi/h roads will result in about a 4% reduction in crashes for sites treated with cameras. While larger percentage crash reductions are predicted for lower speed roads, the larger speed reductions achieved on higher speed roads should result in overall greater safety improvement for those roads; the data were all from 30 mi/h roads, however, so the generalizability of the model to predict impacts on higher speed roads is unknown.

These same authors also found that both horizontal and vertical deflections (traffic calming measures) resulted in larger improvements. An 8% reduction in crashes was predicted for each 1 mi/h speed reduction for horizontal deflections; and, interestingly, the impact of vertical deflections was independent of the impact on speed reductions. A 44% reduction in injury crashes was predicted with vertical deflections regardless of the impact on traffic speeds. The overall reduction in personal injury crashes achieved with speed cameras was on the order of 22%, and the reduction achieved with horizontal treatments was about 29%, for 30 mi/h roads (Hirst et al., 2005b).

AUTOMATED RED LIGHT RUNNING ENFORCEMENT

General findings across the key studies were consistent with those found in earlier studies. That is, a decrease in right-angle crashes occurred; with a concomitant increase in rear-end crashes. When violation frequencies were considered, the results were consistent with previous studies that found decreases in red light running violations—even at non-treatment intersection sites. In addition, several studies conducted an economic effects analysis based on an aggregation of right-angle and rear-end crash costs for various injury severity levels. The studies revealed that RLCs provide a modest aggregate crash-cost benefit. RLCs contributed more to decreasing fatal and injury-producing angle and left-turn crashes than to decreasing PDO crashes.

All of the key studies evaluated red light camera effectiveness as it related to frequency of crashes and crash types. In addition, with the exception of one study, all of them compared treatment with comparison sites, matched for such characteristics as operations (volumes, degree of saturation, presence of large vehicles; speed [posted speed limit]); traffic control (fixed times or actuated, duration of amber, and red phase, cycle time); geometry (number and type of lanes); and warning signs (approach placement). The detail of these characteristics varied substantially from one deployment to another and often from one time to another during the research period. For example, one study reported that at some sites the amber signal phase lengths were changed during the study period. Missing information was also a problem: none of the evaluation studies identified for review addressed the issue of traffic signal visibility (i.e., type, number, and placement of signal heads); from a human factors standpoint, the ability to see the signal could be a strong influence on motorist behavior, and thus could impact the effectiveness of photo enforcement.

A number of studies used an EB approach to derive estimates of the overall effectiveness of RLC treatments, permitting a more accurate comparison of the expected number of crashes if the treatment(s) had not been implemented versus the observed number of crashes that actually occurred with the treatment(s) in place, in before-and-after research designs. In the absence of random assignment of AES treatments, future evaluations of RLC systems should consistently employ this technique.

The choice of safety outcome measures also deserves scrutiny in the interpretation of findings-to-date. While there are many compelling reasons to use crash and violation data to gauge the effects of RLC interventions, there are some quality-of-data issues that should be mentioned. In the case of crash data, how accurate is the information recorded on the crash incident form by the law enforcement agent at the scene of the crash, and by the data-entry staff at the jurisdiction? What criteria determine that a crash is recorded as a red-light-running incident, and for which vehicle? Crashes involving a left-turning and a through vehicle from opposite approaches to the same intersection could be coded to indicate both vehicles running the red signal, either before, after, or during a protected signal phase. One recommendation is that all crashes (including angle, right- or left-turning) involving two vehicles entering the intersection from perpendicular approaches, that occur within 20 ft of the intersection, should be considered as red-light-running incidents in RLC studies. Consistency in the definition of a rear-end crash across reporting sources is also a concern (e.g., with respect to the proximity of the intersection to be included in the RLC studies). Reporting requirements for severity-of-crash data should be established and applied consistently across studies.

Violation data are also problematic as outcome measures. Depending on the jurisdiction, violations varied according to a grace period (lag time) from 0.1 to 0.4 seconds through the red-light phase. Do violation data reflect people who appeal the citation, and the number of citations that are overturned? How does this number affect the overall event counts used in a RLC evaluation study? There is no question that researchers are limited in their analyses by the availability of the information provided by the jurisdictions participating in the studies. When violation data are used in an evaluation, actual review of the tapes (or digital media) may be warranted.

Finally, inconsistencies in signal operations and signing practices cannot be overlooked. Many studies failed to mention differences in the length of the yellow signal phase. While there are ITE standards (based on approach speeds) for the amber phase, the studies that mentioned signal timing revealed treatment and comparison sites with different yellow signal lengths. Since longer amber times have been associated with reductions in crashes, this factor must be taken into account during study design and the interpretation of results.

Certain studies selected for this compendium mention the use of warning signs, plus area-wide publicity, associated with the RLC treatment evaluated in the project. Unfortunately, the location of warning signs (e.g., near the intersection, upstream a specific distance, on the edge of the municipalities' boundaries) is not consistent or is not identified in these studies. It is not sufficient merely to mention the presence or absence of warning signs—the number, placement, distance from intersection, and direction of traffic for which they are intended should also be documented. Mention of whether the warning signs used at the sites conform to MUTCD standards should also be included.

With regard to publicity campaigns, previous evaluation studies have similarly failed to provide details about the number of PSAs on TV/radio, newspaper articles, billboards, flyer distribution, etc., that provide the reviewer with an understanding of the depth/saturation, the extent, and the duration of the publicity associated with RLC treatments. Additional study of the influence of differences in publicity campaigns on RLC effectiveness would clearly benefit future evaluation efforts.

GENERAL DISCUSSION

Beginning with AES activities keyed to speed reduction, existing research indicates that most carefully-implemented, fixed automated speed enforcement programs are likely to result in aggregate safety improvements at high-crash locations. Covert, mobile enforcement programs also seem likely to result in system-wide safety improvements, based on the limited existing evidence. The exact magnitude of the safety gain, how far it will extend, and how much will reflect a change in the targeted behavior (a decrease in speed) rather than another behavior change (e.g., choosing an alternate route) cannot be stated with certainty at this point.

Ideally, in future implementations of automated speed enforcement, authorities and researchers will collaborate to enable and design controlled, randomized experiments to measure safety effects. Sites with high crash frequencies due to problem speeding could be randomly assigned to treatment and non-treatment groups, eliminating the need for controlling for general trends, regression to the mean, and other confounding variables; these would presumably be equivalent or randomly allocated across treatment and control sites. A stratified randomization may be appropriate, depending on the numbers of sites to be treated and similarities/differences among sites. Even randomization may not solve all problems due to possible spillover on untreated sites.

Realistically, given the pressing need perceived by most jurisdictions to maximize safety and treat the highest crash locations first, observational before/after studies will continue to be used. These must be carefully conducted, with documentation of changes in speeds as well as crashes. Such studies must control for trends, confounding factors, and RTM. Use of the EB procedures to control for RTM, a comparison group that represents general crash trends but is unaffected by the treatment being evaluated, are steps needed to improve the precision as well as accuracy of estimates of treatment effects.

In a number of the countries from which the reviewed studies originated, use of speed cameras or photo-radar is so extensive, that it was difficult in some cases, and will become increasingly so, to conduct studies using comparison groups to control for general trends that are not affected by camera enforcement. In the United States, since automated speed enforcement is not yet widespread, there is still an opportunity to reduce confounding influences or effects of spillover on comparison groups. Choosing comparison groups that are unaffected by the treatment, but adequately account for other factors is imperative.

With speed camera enforcement, examination of possible spillover effects should be carefully considered. Issues and study considerations may vary depending on whether the enforcement is mobile or fixed, and overt or covert. Depending on how aware the traveling public is of the enforcement sites, negative spillover in the form of crash migration may be more likely for fixed and conspicuous treatments, not only resulting in unintended consequences but possibly negatively affecting comparison groups and resulting in an over-statement of benefits. There is limited evidence from the reviewed studies that motorists do learn over time and adjust to fixed, conspicuous cameras by perhaps changing routes or adapting speeds over short intervals. These issues should also be borne in mind when considering the type of deployment to use, as well as the study design.

Positive spillover from the program may more likely affect comparison groups in covert automated speed enforcement programs. Sites within the jurisdiction may be more likely to experience spillover, while those outside may not be as comparable in terms of general trends. Studies need to consider such possibilities and perhaps use multiple comparison groups and analyses to examine possible effects, as done in the red-light camera study by Council et al. (2005). Monitoring of traffic flow should also be done, particularly for conspicuous forms of enforcement, and before- and after-traffic flow data should ideally be incorporated into analyses to determine what proportion of effects are due to traffic flow changes.

In the present studies, a justification or explanation of the size of the treatment zone was not always provided. The size of the treatment zone should be carefully determined and described. If possible, a justification based on the enforcement parameters (when, where, signing/conspicuity, sight distance, etc.) for the expected zone of effect should be provided. Pilot studies to determine the extent of the area of AES effects may be needed.

In observational studies, it is also important to collect 'before' and 'after' speed data so that a causal link can be inferred. Elvik (2003) discusses how analysis of causal chains can help assess validity of safety evaluation studies and help detect the presence of confounding variables not controlled for by the study. As data are collected on both speed and safety effects over a wide range of road types and speeds, it may be possible in time to develop models to predict the impact on speeds, and subsequently crashes, following the lead of Hirst et al. (2005a and 2005b). Their models were developed with data from fixed camera installations, which presumably operate 24 hours a day, 7 days a week (or appear to), and only from 30 mi/h roads. It is unknown if their model predictions will hold up over a range of pre-existing speeds and program factors.

More work is needed, particularly regarding mobile systems. While the early research by Cameron et al., (1992) suggests that there is a dose-dependent effect of program factors (extent of enforcement), it would help to maximize efficiency of mobile systems if more research were done into the nature of the relationship, including determining if there is a minimal threshold of mobile enforcement needed to realize significant safety effects across varying conspicuity levels, publicity, and other factors. To that end, consistent methods of measuring and documenting enforcement and program intensity, such as through hours of enforcement per site, should be adopted and modeled. Other factors such as conspicuity/use of signs should be evaluated. For example, if it is true, that the most dangerous speeders are also the most intractable, does the use of warning signs help or hinder efforts to affect such speeders' behavior or do they have the greatest effect on deterring mild speeders? Controlled studies examining such specific factors as the effect of signs, as well as enforcement threshold, penalties (amount and timing), and others still need to be performed since the studies to date have generally reported on the effects of the overall program. The studies from Australia have gone farthest toward examining effects of other program factors.

In addition to program factors, work is needed to understand the site-specific differences in effects of automated speed enforcement. Virtually all of the studies reported aggregate improvements in safety effects. But among those that also provided data on individual sites, effects varied widely, with most sites showing improvement but some showing no improvement, based on observed results. It is not known whether this variation reflects differences in enforcement intensity, site-specific differences, statistical anomalies, or a combination of all three. For example, Cunningham et al. (2005) reported an extensive range in the numbers of citations issued by site and by corridor, but analyses did not compare effects by corridor with intensity of enforcement. Whether effects vary as a function of speed limit of roads enforced is also unclear from results presented by the present studies. A number of geometric factors could also affect the safety outcomes, suggesting that more, or large-scale, studies utilizing a variety of sites are needed to incorporate these factors into models, and to be able to predict at which sites the use, and the mode, of camera speed enforcement is most effective.

Turning to a consideration of red light running automated enforcement programs, there are several challenges to accurately estimating the safety impacts of RLC treatments, based on existing research. First, many safety-related factors such as traffic volumes, crash reporting thresholds, approach speeds, cycle lengths for signal timing, weather conditions, and law enforcement practices are uncontrolled and/or confounded during the periods of observation. Second, spillover effects caused by drivers reacting to non-RLC equipped intersections make the selection of comparison sites difficult when designing an evaluation study. Third, sites selected for RLC installations may not really be as randomly selected as intended by the study; and as a result may suffer from a regression-to-the-mean effect. Finally, evaluation studies should but often fail to consider the use of crash severity data to gauge the safety impacts of RLCs.

These methodological concerns have been addressed in several of the key studies included in this compendium. Persaud et al. (2005) noted the effects of regression to the mean, as well as changes in traffic volumes and other factors (e.g., weather) from one time to another, and reported that these concerns can be addressed statistically through the application of safety performance functions (SPFs). Specifically, an SPF calibrated to locations without RLCs is used in an EB analysis, to calculate an expected number of crashes at an RLC site. This serves as the benchmark to which the number of events observed in the period after RLC installation is compared, resulting in a more meaningful percent change value. Potential spillover effects of RLC installations on motorists' behavior at nearby intersections may also be addressed, depending upon the temporal and spatial domains in which intersections are sampled to acquire the data used to calibrate the SPF.

Reanalysis of data from previous RLC studies using the SPF approach revealed effects that were in the same direction as reported in other recent reviews and syntheses: a modest reduction in angle crashes coupled with a slight increase in rear-end crashes at RLC-equipped intersections. The magnitude of effects and associated benefits of RLCs were lower, however, which the researchers authors speculate may reflect the failure in prior evaluations to account for regression to the mean. Spillover effects also remain an issue in the reanalysis of past studies, as the retrospective nature of the data (obtained over varying intervals) in such work precludes calibration of the SPF at a point in time that allows derivation of valid expected crash frequencies at the RLC evaluation site(s).

In the Council et al. (2005) study of effects of red light cameras, non-signalized intersections from each jurisdiction were used to account for general trends. The assumption was that enforcement of red lights would not spill over to affect non-signalized locations. In addition, 'before' data from at least three untreated signalized intersections per treatment intersection (in the same jurisdiction) were used to update safety performance functions used in the analyses. These sites were also used to investigate possible spillover effects. Separate before/after analyses using the red light camera program start-date were conducted for the signalized reference group, assuming that any spillover would occur after that time.

There is a need for certain adjustments in the before/after study design with matched control locations (or jurisdictions) that has been the *de facto* 'gold standard' in this area, given the legal, ethical, and economic barriers to random assignment of RLC installations. Specifically, using the EB methodology to establish a benchmark for the calculation of treatment effects will affect the selection of comparison sites. More reliable estimates of RLC treatment effects also may be expected from *prospective* evaluations, where reference functions can be derived that incorporate all manner of other (contemporary) influences on behavior, exposure, crash reporting practices, and other distinguishing factors within a jurisdiction.

It could be argued that future RLC studies that seek to more precisely quantify changes in red light running will never completely eliminate these methodological concerns; if so, investigating strategies to maximize the benefits of RLCs could be a more fruitful research focus. This is not to diminish the contributions of Persaud et al. (2005) or others who apply advanced analysis techniques to this problem. But as there is a general consensus that red light camera installations lead to a modest reduction in the most serious, angle crashes at intersections—regardless of the precise magnitude of this effect—there is also an attribution of this safety benefit to a change in driver behavior (red light running), and an acknowledgement that this behavior in turn depends upon driver perceptions. In particular, the spillover effect is presumed to be mediated by drivers generalizing their perceptions of an increased likelihood of a traffic citation/conviction from a few to many locations.

In this context, learning about the extent to which other (between- and within-subjects) factors influence the perceived likelihood of apprehension becomes a research priority. Stokes, Russel, and Rys (2003), in assessing the feasibility of automated traffic signal enforcement in Kansas, state that planners should "pay special attention to the public acceptance and public education aspects of RLR programs." The results of polls conducted by the Insurance Institute for Highway Safety (reported by Blakey, 2003) showing, consistently, that three-fourths and more of U.S. drivers favor red light cameras, suggest that the public would be receptive, rather than resistant, to a range of perceptual countermeasures.

Differences in driver perceptions may vary nationally, regionally, or locally with changes in public education messages (McGee & Eccles, 2003). The conspicuity of camera installations, the characteristics of advance warnings, or the pairing of RLCs with other, discriminative stimuli that could be added cheaply (at non-instrumented intersections) also could significantly influence driver perceptions in the desired direction. Laboratory or simulation studies could do much to explore these relationships, laying the groundwork for surveys, focus groups, and opinion sampling to identify the most promising manipulations, with subsequent behavioral field observations leading to practical guidelines for maximizing RLC effectiveness.

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APPENDIX A:

ANNOTATED BIBLIOGRAPHY OF AUTOMATED ENFORCEMENT SYSTEMS

PART I: SPEED

PART II: RED LIGHT RUNNING

PART I: SPEED

Reference	ARRB Group Project Team. (2005). *Evaluation of the Fixed Digital Speed Camera Program in NSW* (RC2416). New South Wales, Australia: Roads and Traffic Authority.
Study Objective	The objective of the study was to identify changes in driver speed behavior, changes in crash frequency and severity, economic value of the program, and community attitudes, knowledge, and beliefs. The objective of the safety evaluation was to identify and measure the impact of 28 unattended fixed, digital speed cameras at urban and rural locations. [Only the safety outcomes are reported here.]
Study Location	New South Wales, Australia
Program Traits reported	• Fixed cameras (permanent – not rotated among sites) • Conspicuous • Three large, prominently placed signs used at/near sites informing motorists that cameras are in operation • Enforcement threshold not reported • Owner assessed (unless supplies driver's identity) • Fixed fines and license points; automatic suspensions of 3 months for exceeding the speed limit by > 30 km/h and of 6 months for exceeding by > 45 km/h
Study Period	Not described Before period = 3 years After period = 2 years
Description of Treatment/Comparison Sites	• Initially, the evaluation included 20 (of 25 initial) unattended, fixed, conspicuous digital speed camera sites. The number of installations increased over the study period and was at 81 by October 2002, when 8 more sites were added to the study for a total of 28 study sites, 1 to 3.3 km in length. Digital cameras were primarily installed in locations with high crash rates or high severity crashes that subsequently underwent speed analyses, site review, and consideration of other potential safety treatments. "A camera site was defined as the 'camera length'; the black length section of road in which the camera was located." Data were also collected for upstream and downstream adjacent sections (but lengths not documented for either sites or adjacent sites). • Camera sites were matched with controls based on speed limit, number of lanes, and roadway cross-section (divided/undivided).

Reference (Cont'd)	ARRB Group Project Team. (2005). *Evaluation of the Fixed Digital Speed Camera Program in NSW* (RC2416). New South Wales, Australia: Roads and Traffic Authority.
Research Design & Statistics	Before/after observational study of speed effects and crash effects at treated locations. Speed surveys were undertaken prior to commencement of program and at 6, 12, and 24 months following at the fixed camera sites and at adjacent sites. Speed data were also collected at eight additional sites for at least two time periods subsequent to deployment. Matched non-intervention sites were also used to account for general trends in vehicle speeds in statistical analyses of changes at camera sites. Mean speed was analyzed using the log-normal model and the proportions exceeding the speed limit (by varying margins) were modeled using Poisson regression. Control ratios were applied to determine the proportion of the changes attributable to cameras. For safety/crash effects, Poisson regression analysis was used. Crash data from control sites were included to account for changes in the system during the evaluation period.
Other Interventions	None described
Outcome Measures	<u>Changes in Speed behaviors:</u> • Proportions speeding above pre-determined thresholds • Mean speeds • 85th percentile speeds <u>Changes in Crashes:</u> • Number and severity of crashes at camera sites and adjacent sites Economic value to the community Changes in community attitudes, knowledge, beliefs, and reported behaviors
Research Design Shortcomings, Confounding Variables, and Other Issues	• The analysis seems to assume that changes in traffic volumes in the before and after periods were the same in the treatment and control groups. • Control sections were introduced to account for trends or effects due to other confounding factors. • It is clear that sites with high accident numbers were selected. Hence, it is possible that results are biased due to regression to the mean.

Reference (Cont'd)	ARRB Group Project Team. (2005). *Evaluation of the Fixed Digital Speed Camera Program in NSW* (RC2416). New South Wales, Australia: Roads and Traffic Authority.
Major Findings	<u>Changes in Mean speeds</u> • 6.3 km/h reduction in mean speeds at camera sites, 12 months following • 5.8 km/h reduction in mean speeds at camera sites 24 months after • Generally, no significant changes in speeds at adjacent lengths, but • 1.5 km/h increase at upstream adjacent lengths at 24 months after <u>Changes in Percentage exceeding limit</u> • 5.3% reduction at upstream sections, 12 months after • 70% reduction at camera sections, 12 months after • 20.9% reduction at downstream sections, 12 months after • At 24 months, reductions remained for camera and downstream lengths, but upstream length increased by 3.4 km/h (significant at $p < .0001$) <u>Percentage exceeding by at least 10 km/h</u> • Statistically significant reductions of 86% and 88% along camera lengths at 12 and 24 months respectively, but by 12 and 24 months, increases in percentages speeding excessively were posted for upstream and downstream segments. Effects on 85th percentile speeds were on average -5.7 km/h (6 months after) in 100 km/h zones to -22.3 km/h in 90 km/h zones. Some downstream lengths showed increases for some speeds limits by 12 and 24 months after. <u>Aggregate Changes in Crashes</u> • 19.7% reduction in all reported crashes along camera segments • 22.8% reduction in casualty crashes (fatal and injury) at camera segments • 16.9 % reduction in tow-away crashes along camera segments • 20.1% reduction in injury crashes along camera segments • 89.8% reduction in fatal crashes along camera segments Reductions in all reported crashes, fatal, casualty, injury, and tow-away crashes along combined camera and adjacent segments not statistically significant. All changes reported above were statistically significant.
Conclusions	Estimates of total crashes, injury crashes, and non-injury, tow-away crashes on the order of 20% were reported for the camera sites in the after period, but increases at upstream and downstream sites were documented, along with higher speeds and percentages of speeders upstream and downstream of camera sites. Additionally, high crash sites were selected for treatment and analyses did not control for regression toward the mean.

Reference	Cameron, M.H., Cavallo, A., & Gilbert, A. (1992). *Crash-based Evaluation of the Speed Camera Program in Victoria 1990-91. Phase 1: General Effects. Phase 2: Effects of Program Mechanisms* (Report No. 42). Victoria, Australia: Monash University, Accident Research Centre.
Study Objective	This study evaluated system-wide crash effects of an urban and rural speed camera enforcement and publicity program. The study also examined effects on crashes of some program intensity measures (enforcement level and publicity level).
Study Location	Melbourne and rural Victoria, Australia
Program Traits	• Mobile cameras • Inconspicuous • General warning signs, especially on major roads entering city • High profile media campaign • Enforcement threshold not reported • Unclear if owner/driver assessed • Graduated fines and license points; immediate license suspension possible, depending on severity
Study Period	1983 – 1991 Before period = 7 years After periods were divided into: ▪ T1a: period of low level camera trialing and localized low level publicity from December 1989 to March 1990 ▪ T1b: period characterized by a high profile media launch of speed cameras and intensive publicity related to speed-related crash risk, but little speed camera enforcement ▪ T2: period during which enforcement increased dramatically and also the detection and punishment of speed offenders – starting from July 1990. T2 was further divided into: ▪ T2a (between July 1990 and February 1991), and ▪ T2b (between March 1991 and December 1991), when cameras were introduced into New South Wales, the comparison area. The total T2 after period was 18 mos.

Reference (Cont'd)	Cameron, M.H., Cavallo, A., & Gilbert, A. (1992). *Crash-based Evaluation of the Speed Camera Program in Victoria 1990-91. Phase 1: General Effects. Phase 2: Effects of Program Mechanisms* (Report No. 42). Victoria, Australia: Monash University, Accident Research Centre.
Description of Treatment/Comparison Sites	• Fifty-four mobile speed cameras were introduced on 60 and 100 km/h urban and rural roads in the state of Victoria from December 1989 to January 1991. In 1990, a major enforcement and publicity campaign was launched. • Comparable areas in the neighboring State of New South Wales, considered most similar to Victoria in terms of population, economic activity and other characteristics, were used to control for general trends in the absence of treatment. Camera enforcement was introduced in New South Wales in March 1991, so the comparisons after time T2a reflect changes over and above effects of cameras in New South Wales.
Research Design & Statistics	Before/after study using time trend analyses to analyze crash effects of camera introduction over three after-phase periods. ARIMA models were used to account for trend and other time effects. Phase II of the study attempted to better understand the 'cause and effect' of the program by trying to relate the crash measures with specific program mechanisms. The elements of the program mechanisms considered included the number of Traffic Infringement Notices (TINs), the amount of paid television publicity, and hours of speed camera operation. Multiple regression analysis (with a linear trend component and monthly dummy variables to account for seasonality) was used to model casualty crashes to (natural logarithms of the dependent and independent variables were taken in order to fit multiplicative models), while logistic regression was used to model the crash injury severity.
Other Interventions	• An alcohol enforcement program was introduced in Victoria at the same time as the speed enforcement program.
Outcome Measures	▪ Casualty crashes (included fatal crashes, serious injury crashes, and minor injury crashes) occurring during low-alcohol hours (day time) ▪ Casualty crash injury severity - ratio of fatal plus serious injury crashes to crashes involving minor injury only

Reference (Cont'd)	Cameron, M.H., Cavallo, A., & Gilbert, A. (1992). *Crash-based Evaluation of the Speed Camera Program in Victoria 1990-91. Phase 1: General Effects. Phase 2: Effects of Program Mechanisms* (Report No. 42). Victoria, Australia: Monash University, Accident Research Centre.
Research Design Shortcomings, Confounding Variables, and Other Issues	• This study did not examine changes in driving speeds. • Instead of modeling the ratio of fatal plus serious injury crashes to minor injury crashes, it may have been more useful to model the number of fatal plus serious injury crashes using negative binomial/Poisson regression. • Unemployment rate (for both comparison and treatment States) was used as a proxy for overall exposure since volume data were unavailable, but traffic volumes or changes in traffic due to the treatment were not explicitly considered. • It is not clear if crash trends between the comparison and treatment areas were examined. • Since the analysis focused on system-wide effects, it was not possible to determine if there was any accident migration resulting from the treatment. • To avoid the confounding effect due to the alcohol program, only crashes that occurred during low-alcohol periods (mainly daytime) were included. • The focus of the analysis reported in this study was to look at system-wide effects. In addition, it is not known if the speed cameras were necessarily implemented in high crash locations. Hence, bias due to regression to the mean is probably not a significant concern in this analysis.
Major Findings	Results indicated statistically significant reductions in low-alcohol casualty crashes in T1a, T1b, and T2a, on all arterial roads and specifically on 60 km/h arterial roads in Melbourne (Melbourne is the largest metropolitan area in Victoria, and most of the enforcement occurred in Melbourne). <u>Aggregate Changes in Crashes:</u> • 20% reduction in daytime casualty crashes (injury and fatality) statewide during T1b and T2a (at full implementation, and before cameras introduced in NSW) • 32% reduction on arterials • 27.9% reduction in crash severity statewide Results from phase 2 analyses (program factors/level of enforcement and publicity): • Casualty crash frequency and crash injury severity were both found to be significantly related to the number of speed camera TINs on all Melbourne area roads and Melbourne arterial roads. • Crash injury severity was also found to be significantly related to hours of camera operation for Melbourne area roads and arterial roads. • Speed related publicity was not significant in the crash injury models but approached significance (with a small effect) for frequency of crashes. • Hours of camera operation and TINs were also significantly correlated.

Reference (Cont'd)	Cameron, M.H., Cavallo, A., & Gilbert, A. (1992). *Crash-based Evaluation of the Speed Camera Program in Victoria 1990-91. Phase 1: General Effects. Phase 2: Effects of Program Mechanisms* (Report No. 42). Victoria, Australia: Monash University, Accident Research Centre.
Conclusions	Estimated system-wide crashes were reduced significantly from before to after implementation of the speed camera program and associated publicity. Since the study examined system-wide effects, regression toward the mean may not be a serious issue. A comparison group was used to control for general trends, but it is not clear how similar crashes were, absent treatment, between the two groups. Effects on traffic speeds were not documented, but the study found evidence of effects of program intensity - a dose-dependent effect on crashes. Crashes were significantly related to number of traffic infringement notices mailed and hours of operation providing more support for a program effect.

Reference	Chen, G., Meckle, W., & Wilson, R.J. (2002). Speed and safety effect of photo radar enforcement on a highway corridor in British Columbia. *Accident Analysis and Prevention 34*, 129-138.
Study Objective	To evaluate corridor-wide effects of a photo-radar enforcement program by examining effect on speeds and speed variance and traffic collisions at both enforced and non-enforced locations along the study corridor. The study attempted to answer the question: Is there evidence of traffic collision migration due to the treatment or evidence of spillover effects resulting from the program?
Study Location	Vancouver Island, British Columbia, Canada
Program Traits	• Mobile cameras • Presumably covert (deployed in unmarked minivans) • No use of warning signs mentioned • Enforcement threshold not reported • "Major" PI & E program • Owner assessed • Graduated fines • Usually operated daytime at high accident sites or sites with perceived speeding problem • [Program information from Chen et al., 2000]
Study Period	1994 – 1998 Before period = 2 years After period = 2 years The after period started from April 1996, when the warning letter phase of the program started.
Description of Treatment/Comparison Sites	Study evaluated effect of mobile photo radar enforcement on a 22 km Vancouver Island segment of Highway 17 (Pat Bay Highway) in British Columbia. This is a four-lane divided highway with speed limits 80 km/h or 90 km/h, primarily rural. On this corridor, there were 12 individual photo radar locations. There were no “realistic alternate routes, making traffic migration highly unlikely.” And police-reporting of collisions was thought to be consistent over the study period. • For this study, a photo-radar influenced (PRP) location was a 2 km section of the highway, 1 km in each direction from the photo radar enforcement site. The remaining highway sections, interleaved between the PRP locations, were denoted as non-PRP locations. The length of the non-PRP locations varied between 0.4 and 5.9 km, depending on the proximity of the adjacent treatment locations. Traffic speed data at treatment sites was collected using the photo radar device. Speed data at the one non-PRP location (2 km south of the southern most PRP location) were collected by induction loops. For the safety study, 2 years of data both before and after the implementation of the photo radar program were used. • A comparison group (three police-jurisdictions in the study area) was also used to control for trend effects.

Reference (Cont'd)	Chen, G., Meckle, W., & Wilson, R.J. (2002). Speed and safety effect of photo radar enforcement on a highway corridor in British Columbia. *Accident Analysis and Prevention 34*, 129-138.
Research Design & Statistics	Before/after empirical Bayes (EB) study using interleaving, non-treatment influenced sites along the same corridor in order to examine the impact of spillover effects and traffic/accident migration. For the safety effect, EB method to control for RTM and time effects was used. Crash data for about 650 km of four-lane divided highways in British Columbia (except urban roads in Greater Vancouver) were compiled to form a reference group to develop safety performance functions (SPF) as part of the EB procedure. A simple before/after comparison was used to examine effects on speed.
Other Interventions	▪ No mention of additional programs (including those described in Chen et al., 2000) Data collection period ended after two years due to introduction of a new speed enforcement program on the same corridor.
Outcome Measures	▪ Traffic speeds ▪ Police-reported collisions
Research Design Shortcomings, Confounding Variables, and Other Issues	▪ Only total number of crashes was examined; changes in severity of crashes was not examined. ▪ There is no discussion of other concurrent programs. However, since there was a comparison group to account for trends, the effects of any programs that were implemented system-wide were hopefully accounted for. ▪ The three comparison groups used may not have been ideal, given that a general effect from the system-wide enforcement had been documented (Chen et al., 2000). The adjustment based on these groups may therefore have resulted in an understatement of the treatment effect. ▪ The paper indicates that there are no realistic alternate routes for motorists traveling on this corridor. Hence, this setting does not allow a determination of the effect of photo radar on traffic diversion. ▪ Regression-to-the-mean was accounted for using the EB method. However, the authors seem to argue that the before and after periods need to be equal – they indicate that data after April 1998 were not considered because of the introduction of a new speed enforcement program. This limited the after period to 2 years. Consequently, the before period was also limited to 2 years. Based on the discussion that was presented for the time effect analysis, data were available for at least 5 years before the implementation of the photo radar and could have been used for estimating the SPFs.

Reference (Cont'd)	Chen, G., Meckle, W., & Wilson, R.J. (2002). Speed and safety effect of photo radar enforcement on a highway corridor in British Columbia. *Accident Analysis and Prevention 34*, 129-138.
Major Findings	<u>Traffic speed and variance</u> decreased following the introduction of the program. At the PRP locations, the main reduction occurred starting from the warning letter phase (in May 1996). ▪ Mean speeds dropped to below the posted speed limit and remained stable throughout the 2-year after period. Speed and speed variance decreased at the non-PRP (single) monitoring location after August 1996. ▪ Mean speed decreased by about 2 km/h (3% reduction) ▪ Standard deviation decreased by about 0.5 km/h (6% reduction) <u>Aggregate Changes in Crashes:</u> ▪ 14% $\pm$ 11% reduction in collisions at the PRP locations (1 km either direction) ▪ 19% $\pm$ 10% reduction in collisions at the non-PP locations (beyond 1 km from camera site either direction) ▪ 16% $\pm$ 7% reduction for the corridor as a whole
Conclusions	Results indicate that the mobile-radar program resulted in reduced traffic speeds at the treated sites and at a non-treated location along the study corridor for two years following implementation. A significant decline of 16% in estimated crashes (all severities) was found corridor-wide, including at non-treated locations (> 1 km from camera site). An examination of crash severity, was not, however, provided. Results suggest generalized improvements throughout the corridor with no evidence of accident migration to non-deployment locations (non-PRP locations) to compensate for time loss (kangaroo effect). Authors maintained that no realistic alternate corridors were available, so traffic and crash migration to other non-enforced roadways was assumed not to occur. Enforcement was unpredictable in time and space and relatively frequent in space (rotated among the 12 sites along the 22 km corridor). Results may not be generalizable to other corridors with different enforcement parameters or other characteristics.

Reference	Chen, G., Wilson, R.J., Meckle, W., & Cooper, P. (2000). Evaluation of photo radar program in British Columbia. *Accident Analysis and Prevention*, *32*, 517-526.
Study Objective	To assess whether increased enforcement due to photo radar resulted in a system- (province-) wide decreased traffic speeds through deterrence, and subsequently decreased traffic collisions and casualties system-wide.
Study Location	British Columbia, Canada
Program Traits	• Mobile cameras • Presumably covert (deployed in unmarked minivans) • Use of warning signs not mentioned • Enforcement threshold not reported • Major PI & E program • Owner assessed • Graduated fines • Usually operated daytime at high accident sites or sites with perceived speeding problem • First year, about 30,000 hours of operation, province-wide; approx. 250,000 citations mailed. • Public support measured at about two-thirds.
Study Period	1991 – 1997 Deployment began March 1, 1996, followed by four-month warning letter phase; ticketing began August 2, 1996 For crashes Before period = 5 years Intermediate/implementation startup period = 5 months After period = 12 months For speed, September 1995 – November 1996 at monitoring sites May 1996 – July 1997 at enforcement sites

Reference (Cont'd)	Chen, G., Wilson, R.J., Meckle, W., & Cooper, P. (2000). Evaluation of photo radar program in British Columbia. *Accident Analysis and Prevention*, *32*, 517-526.
Description of Treatment/Comparison Sites	▪ Study evaluated a province-wide deployment of 30 mobile speed cameras deployed in unmarked minivans. Units were typically operated during daytime at sites with high crash history or perceived speeding problem. In the first year, there were 30,000 hours of operation, province-wide; approximately 250,000 tickets were mailed to vehicle owners. Public support was measured through a pre-implementation survey at about two-thirds, and public awareness of the program at about 95%. For speed effects, speed induction loops were installed at 16 sites (19 site-direction combinations) on selected highways and streets across the province where photo radar deployment was not operating – the intent was determine if the photo radar affected speeds at these locations due to the heightened perception of detection (these are called the monitoring sites as opposed to the deployment sites where there was deployment of photo radar). Site selection was non-random in that locations with free-flowing conditions were used. Data were collected for an equivalent 8-day period each month (same number of weekdays and weekend days). Speed data from the treated sites were collected by the photo-radar installations when the units were in operation. ▪ For safety effects, traffic collision and injury data were collected from the police and from the BC ambulance services of the Ministry of Health and monthly totals were summarized and used in the models.
Research Design & Statistics	Before/after observational study using interrupted time series ARIMA models was used to study the effect on province-wide crashes.
Other Interventions	Two other enforcement interventions occurred ruing the last 4 months of the study period • enhanced impaired driving road checks • administrative driving prohibition/vehicle impoundment. Since impaired driving and related programs occur primarily at night, and the speed cameras operated primarily during the daytime, the authors decided to use daytime crash data for the analysis.
Outcome Measures	Changes in province-wide crashes: • daytime unsafe speed related collisions (as determined from police crash reports) • daytime traffic collision injuries carried by ambulances • and daytime traffic collision fatalities Changes in speeds in the enforced zones and province-wide: • percentage of vehicles exceeding the speed limit in the deployment areas and monitoring sites over time

Reference (Cont'd)	Chen, G., Wilson, R.J., Meckle, W., & Cooper, P. (2000). Evaluation of photo radar program in British Columbia. *Accident Analysis and Prevention*, *32*, 517-526.
Research Design Shortcomings, Confounding Variables, and Other Issues	• Traffic volumes (AADT) per se were not considered. However, in the ARIMA analysis, gasoline sales were used as a proxy for vehicle miles traveled. The ARIMA analysis also takes into account time trend effects. • The authors indicate that two traffic safety interventions, the enhanced impaired driving road checks and the administrative driving prohibition/vehicle impoundment legislation were introduced during the last 4 months of the study period. Since impaired driving and related programs occur primarily at night, and the speed cameras operated primarily during the daytime, the authors decided to use daytime crash data for the analysis. • Speed cameras were installed at sites with high number of accidents and sites with a perceived speeding problem. Speed results suggest, however, that there was a higher percentage of excessive speeders (exceeding by more than 16 km/h) at comparison sites than at treatment sites before deployment. It also isn't known with certainty how much other enforcement may have occurred at the monitoring sites. • Since the crash analysis was based on numbers of crashes in the province, the presence/absence of accident migration could not be determined. • Since the safety analysis was based on total number of speed-related crashes province-wide, regression toward the mean phenomenon is likely not a serious issue in this analysis.
Major Findings	<u>Speed effects:</u> • The reduction in the percentage of vehicles exceeding the speed limit following deployment was higher in the deployment sites compared to the monitoring sites. • In the deployment sites, the percentage of vehicles exceeding the speed limit dropped from 66% in May 1996 (during the warning phase) to 33% at the end of the year (a 50% decrease) with the percentage remaining below 40%. • For the same time period, the percentage of vehicles exceeding the speed limit dropped from 69% to about 61% at the monitoring (non-treatment) sites. • At the deployment sites, the percentage of vehicles exceeding the speed limit by at least 16 km/h, dropped from about 10.5% in May 1996 to about 3% at the end of 1996 and beginning of 1997, a 75% reduction. • Again, during the same period, at the non-deployment sites, the percentage of vehicles exceeding the speed limit by at least 16 km/h decreased from 24% to 14%. • A pooled cross-section time series analysis was conducted with speed data from the monitoring sites – results indicated a 2.4 km/h reduction in the average speed following the introduction of photo radar, suggesting a generalized province-wide decrease in speeding. <u>Aggregate Province-wide Safety effects</u>: • 25% reduction in daytime unsafe speed-related collisions (statistically significant), • 11% reduction in daytime injuries carried by ambulances (statistically significant), and • 17% reduction in daytime fatalities. (The reduction in fatalities was significant at p=0.10, one-tailed test)

Reference (Cont'd)	Chen, G., Wilson, R.J., Meckle, W., & Cooper, P. (2000). Evaluation of photo radar program in British Columbia. *Accident Analysis and Prevention, 32*, 517-526.
Conclusions	The reduction in mean speeds at monitoring sites suggests a generalized province-wide decrease in speeding resulting from the program. The reduction in day-time unsafe speed-related collisions province-wide and reductions in victims carried by ambulances and in fatalities concurrent with reductions in speed suggests the project was effective at improving safety. The study could not completely rule-out confounding effects, but through the research design (use of day-time only speed-related collisions and day time serious injuries and fatalities, even though the program had some night-time hours of operation), attempted a conservative approach to estimating the safety effect. The evaluation covers only the first year of operation and thus, longer-term impact or sustainability was not addressed. A long before-study period (5 years) and the use of province-wide crashes may reduce the likelihood of RTM playing a role in these results. It is unclear how the use of speed-related crashes compared with all crashes or all injury crashes might affect the estimates of safety effects. The study also looked at injuries (carried by ambulance) and fatalities, but did not examine possible effects on other injury or no injury crashes.

Reference	Christie, S.M., Lyons, R.A., Dunstan, F.D., & Jones, S.J. (2003). Are mobile speed cameras effective? A controlled before and after study. *Injury Prevention 9*, 302-306.
Study Objective	To compare two methods for determining local effectiveness of mobile speed cameras in reducing road traffic related injuries and use the most appropriate method to assess effectiveness of speed cameras by time after intervention, time of day, speed limit, and type of road user injured.
Study Location	South Wales, United Kingdom [region is 2,121 square kilometers]
Program Traits	• Mobile camera enforcement • Enforcement threshold not reported • Signs used at/near enforcement sites • Publicity not described • Driver assessed with penalty • Fixed fines and license points • Began participation in cost recovery program (with crash warrants for implementation) in 2000; the most sites, 41, were added in 2000
Study Period	1996 – 2000 with number of sites growing throughout the study period. Before period = 38 months (average per site, no minimum given) After period = 17 months (average per site, no minimum given) Data for the three months immediately before and immediately after deployment at each site were excluded since precise implementation dates were not always known.
Description of Treatment/Comparison Sites	• 101 mobile speed camera sites throughout region at rural and urban locations. A majority of sites were on 30 mi/h speed limit roads (76), with 5 on 40 – 50 mi/h roads, and 20 on 60 – 70 mi/h roads. • Matched control sites were selected from Gwent, a neighboring police enforcement district, with only one mobile and no static cameras. Gwent was chosen because its injury rate per population was similar to South Wales. Control sites were matched for posted speed limit, road class, and injurious crashes history (+/- 20% the number of injurious crashes within 500 meters radius in the before-intervention period).
Research Design & Analyses	A matched control, before/after study of effects at treated sites was conducted using two methods for examining the effectiveness of mobile speed cameras – circular zones of 100, 300, 500, and 1,000 meters around the camera site and a route based method using segments of 100, 300, 500, and 1,000 meters in either direction from camera sites (but stopping short of any junction that would cause traffic to slow or stop. The 500 meter route method was then used to investigate the effects within strata of time after intervention, time of day, speed limit, and type of road user injured. Rate ratios of injurious crashes for treated / control sites were estimated, adjusting for unequal before period crashes.
Other Interventions	None described.

Reference (Cont'd)	Christie, S.M., Lyons, R.A., Dunstan, F.D., & Jones, S.J. (2003). Are mobile speed cameras effective? A controlled before and after study. *Injury Prevention 9*, 302-306.
Outcome Measures	The dependent variable was the ratio of injury crashes at intervention and control sites. This measure was used to determine the expected number of injury crashes in the after period at the treatment sites had the intervention not taken place.
Exposure Data	Use of matched comparison sites to indirectly control for general volume trends; traffic volume data not available
Research Design Shortcomings, Confounding Variables, and Other Issues	▪ Cameras had been introduced in the region prior to the study period. ▪ Effects on speed were not determined. ▪ Only injury crashes were considered, possibly to avoid problems due to reporting of property damage only crashes. Authors maintain that police categorization of injury levels was unreliable, and also that there was evidence that reporting practices for severe injury changed during the study period. Thus all injury crashes were examined, rather than by injury severity. ▪ Traffic volume data were not available and authors had to assume no changes in traffic volumes over time. Authors argue however, that since the intervention and control sites were very well matched, time-related changes in traffic volume and other time trends are automatically considered. However, although the total number of injury crashes could be comparable between the treatment and control sites, the trends over time could be different between the two. It is not clear if trends in crashes were examined before the control sites were selected. ▪ There was no specific discussion of effects of cameras on crashes due to traffic diversion to other roads without speed cameras. On the other hand, since they looked at mobile speed cameras which could be moved around among 101 camera sites, there may not have been significant traffic diversion resulting from the treatment. ▪ There is no explicit discussion of possible accident migration due to 'kangaroo' effect (accelerating before or after the camera sites to compensate for time loss). (Camera sites were typically marked with warning signs.) Decreases in crashes were also found, however, for the band between 300 and 500 meters distance from camera sites. ▪ The authors argue that "the control and intervention sites were so well matched for all injurious crashes (matching ratio 0.99) that if any regression to the mean occurred it should have had an equal effect at camera and control sites". It is not clear if this is a reasonable assumption. Also, the before time period is also limited (38 months) and may not be sufficient to exclude the possibility of regression to the mean. ▪ Precision of crash location measurement varied across police divisions; some recorded to nearest 10 meters, others to the nearest 100 meters. The proportions of crashes imprecisely located decreased over the study period so a before/after association exists. The authors indicate that this may have led to underestimations of the benefits of cameras by as much as 3–4%.

Reference (Cont’d)	Christie, S.M., Lyons, R.A., Dunstan, F.D., & Jones, S.J. (2003). Are mobile speed cameras effective? A controlled before and after study. *Injury Prevention 9*, 302-306.
Major Findings	Camera sites had lower than expected number of injury crashes up to 300 meters using the circles (polygon around the camera site) and up to 500 meters using routes. The routes method indicated a larger effect than the circles except in the 100 meters nearest sites. Rate ratio injury crash effects using routes method (aggregate results) were: • 0 – 500 m: 0.49 (0.42, 0.57) (estimated 51% reduction) • 0 – 100 m: 0.30 (0.20, 0.42), (estimated 70% reduction) • 100 – 300 m: 0.55 (0.43, 0.68) (estimated 45% reduction) • 300 – 500 m: 0.59 (0.46, 0.76) (estimated 41% reduction) • 500 – 1,000 m: no significant change Overall, the number of injury crashes after intervention was substantially reduced for both slower and faster speed limit roads (within 500 m): • 30 mi/h roads: 0.49 rate ratio (0.42, 0.58), (51% reduction) • 60 – 70 mi/h roads: 0.41 rate ratio (0.27, 0.62), (49% reduction) Over time, the rate ratio of injurious crashes estimated over all treatment sites (within 500 m) was: • 3 – 6 months: 0.47 (0.31, 0.68) (88 sites) • 6 – 18 months: 0.54 – 0.64 (reported per each 3 month span; most confidence intervals < 1; (66 decreasing to 41 sites over the time period) • 18 – 21 months: 0.18 (0.06 to 0.42) (38 sites) • 21 – 24 months: no significant change (34 sites); sample sizes too small to detect effects per authors. The rate ratio for daytime expected crashes was 0.46 (0.38 to 0.56) and for nighttime expected crashes was 0.55 (0.42 to 0.71). Significant decreases were observed for crashes that injured pedestrians (0.22 rate ratio, 0.14 – 0.32), motorcycle users (0.37 rate ratio, 0.13 – 0.8), and car occupants (0.48 rate ratio, 0.40 to 0.59). Sample sizes were too small to detect effects on pedalcyclist crashes.
Conclusions	Estimates of crashes for camera sites were lower than expected (if no treatment) in the after period (average 17 months) following implementation. Improvements were greatest nearer the camera sites and were sustained up to 21 months following the interventions. Effectiveness was found to be sensitive to the type of metric or defined treatment area surrounding the sites with a 500 meter linear route-based method capturing the most improvement. Although effects due to volume changes and other trends may have been controlled somewhat through the use of comparison groups, regression toward the mean was not controlled. At least 40% of the sites were selected after the cost-recovery program was implemented, meaning that high crash sites were selected. It is not clear how earlier sites were selected. Although the treatment and comparison groups were matched for numbers of injurious crashes during the before period, it is not clear whether trends in crashes were examined and could have been different between these two groups. Additionally, other enforcement and safety programs could have been different in the treatment and comparison group, which was a neighboring police district. Therefore estimated crash reductions may be due to multiple causes, including regression toward the mean and possibly traffic flow changes.

Reference	Cunningham, C.M., Hummer, J.E., & Moon, J.-P. (2005). *An Evaluation of the Safety Effects of Speed Enforcement Cameras in Charlotte, NC.* Presented to the North Carolina Governor's Highway Safety Program. Raleigh, North Carolina: North Carolina State University, Institute for Transportation Research and Education.
Study Objective	To address whether automated speed enforcement cameras provide significant safety benefits to the driving public.
Study Location	Charlotte, North Carolina, United States
Program Traits	• Mobile, conspicuous (marked vans) enforcement • Enforcement on designated corridors only (approved by legislation) • Warning signs required within 1,000 feet (304.8 meters) of enforcement (mobile van parking area) sites • City added “photo enforced” warning beneath all speed limit signs on enforced corridors • Portable signs also used in advance of enforcement vehicle • Media and public information campaign (flyers went out to citizens) • Owner assessed with penalty • Fixed fine • Enforcement threshold not reported.
Study Period	January 2000 – December 2004 <u>Crashes</u> Before period = 4 yrs After period = 4 months
Description of Treatment/Comparison Sites	• Fourteen high volume corridors in an urban area were targeted by the enforcement and included in the evaluation. Speed limits were: 8 corridors at 45 mi/h; 2 with 35 and 45 mi/h sections; 2 with 40 and 45 mi/h sections; 1 with 35, 40, and 45 mi/h sections; and 1 with 55 mi/h limit. More than 75% of enforcement was during daylight hours • Eleven corridors in the same city comprised a comparison group to control for general crash trends, seasonality, and other time trends.

Reference (Cont'd)	Cunningham, C.M., Hummer, J.E., & Moon, J.-P. (2005). *An Evaluation of the Safety Effects of Speed Enforcement Cameras in Charlotte, NC.* Presented to the North Carolina Governor's Highway Safety Program. Raleigh, North Carolina: North Carolina State University, Institute for Transportation Research and Education.
Research Design & Statistics	Before/after study with comparison sites to control indirectly for trend biases (time trends, seasonality) in analyses of crash data. Although not specifically matched (most of the highest volume corridors in the city were in the treatment group), collisions trends between comparison sites and treatment sites were compared using odds ratios and found to be similar. Collisions were however "more frequent, and more frequent per mile at treatment sites than at comparison sites" probably because these were high volume sites. Speed data were collected using induction loops at 80 locations along treatment corridors and 40 locations along comparison corridors ten months before and three months following implementation. Individual speed observations were grouped into fourteen speed intervals; the lowest (0 – 15 mi/h) was dropped as representing mostly turning vehicles. Speed data for each corridor (treatment and comparison) and overall were modeled separately using linear regression models for mean, median, and 85^{th} percentile speeds, with independent variables for time of day (7 am to 7 pm or 7 pm to 7 am), period (before/after), treatment or comparison site, speed limit, and site. Two-tailed F-tests with significance level of 0.05 were used to test for differences in speeds between before and after periods and day versus night. To study the change in the proportion of vehicles exceeding the speed limit by >10 mi/h, logistic regression models were used. The analysis also looked at the variance in speeds before and after the implementation of speed cameras.
Other Interventions	None described
Outcome Measures	<u>Crashes</u> • Total collisions <u>Speeds</u> • Mean speeds • Median speeds • 85^{th} percentile speeds • Percentage of vehicles exceeding limit by > 10 mi/h.

Reference (Cont'd)	Cunningham, C.M., Hummer, J.E., & Moon, J.-P. (2005). *An Evaluation of the Safety Effects of Speed Enforcement Cameras in Charlotte, NC.* Presented to the North Carolina Governor's Highway Safety Program. Raleigh, North Carolina: North Carolina State University, Institute for Transportation Research and Education.
Research Design Shortcomings, Confounding Variables, and Other Issues	• The short after period and limited number of after period crashes for study. • The main focus of the study was on total crashes. However, there were some supplemental analyses of crash severity and crash types. • The authors indicate that reliable traffic volumes could not be obtained in the treatment and comparison sites. • The study did not examine possible crash migration due to the treatment • Authors argue that regression to the mean bias was not significant for several reasons: - The before period was quite long (about 4.5 years), and the treatment sites are high volume corridors. Hence, the random fluctuation due to regression-to-the-mean was probably damped down due to these large magnitudes. - In order to determine if the results in this study are biased due to regression-to-the-mean, authors conducted another set of analyses by including data from January 2003 to July 2004 as the before period. The City of Charlotte decided on the treatment sites based on crash data from 1999 to 2001, and the authors argued that by including data after 2001, it would be possible to determine the extent to which bias due to regression-to-the-mean was present in this study. The results of the second analyses indicated an 11% reduction in crashes in the treatment group following the introduction of speed cameras (this reduction was statistically significant). The authors concluded that regression-to-the-mean bias was probably not significant in this data set. This approach implicitly assumes that randomly high crash frequencies due to regression-to-the-mean usually last only 2 or 3 years – previous research has shown that this is not necessarily the case. - The authors indicate that it would have been very difficult to get a good reference group for conducting an EB analysis. The treatment sites in Charlotte included almost all of the highest volume corridors. The authors argued that it would have been necessary to collect data from high volume corridors in other cities without knowing how comparable they are.

Reference (Cont'd)	Cunningham, C.M., Hummer, J.E., & Moon, J.-P. (2005). *An Evaluation of the Safety Effects of Speed Enforcement Cameras in Charlotte, NC.* Presented to the North Carolina Governor's Highway Safety Program. Raleigh, North Carolina: North Carolina State University, Institute for Transportation Research and Education.
Major Findings	Changes in Speeds Mean speeds at the treatment sites, after the introduction of speed cameras: • 0.91 mi/h decrease (this reduction was statistically significant). • 0.29 mi/h decrease at comparison sites (not statistically significant). Similarly, the 85th percentile speeds: • 0.99 mi/h decrease at the treatment sites (statistically significant) • 0.30 mi/h decrease at the comparison sites (not statistically significant) The percentage of vehicles exceeding the speed limit by 10 mi/h or more decreased by about 55% at the treatment sites (statistically significant), whereas at the comparison sites there was very little change. There was considerable difference in the speed variance at the different treatment and comparison sites. However, following the introduction of speed cameras, more of the treatment sites experienced a reduction in the speed variance compared to the comparison sites (48% of treatment sites versus 30% of comparison sites). Aggregate Change in Crashes • 12% reduction for treated corridors compared to expected number following the introduction of speed cameras (statistically significant).
Conclusions	Camera enforcement appears to have been effective at reducing speeds and overall number of collisions below expected values (without treatment). Confidence in these results is somewhat limited, however, due to the program having been in operation for only four months for this pilot study after period. Furthermore, results should be interpreted as a short-term impact; it is unknown if results would hold up over a longer-term.

Reference	Elvik, R. (1997). Effects on accidents of automatic speed enforcement in Norway. *Transportation Research Record, 1595,* 14-19.
Study Objective	This study re-analyzed data used in an earlier report, controlling for general trends in crashes and regression toward the mean to examine effects on crashes.
Study Location	Norway
Program Traits	• Fixed cameras • Signs ("automatic traffic enforcement") at/near camera sites • Enforcement threshold not specifically reported, but evidently some margin above posted limit • Owner assessed (or required to identify driver if not the driver) • Graduated fines • Public information/media efforts not described
Study Period	Study covered sites where implementation occurred prior to the end of 1995. Before period = 3.94 years (average); minimum per site, 1 year After period = 4.61 years (average); minimum per site, 1 year (The paper provided a secondary analysis of data originally analyzed in a 1996 report.)
Description of Treatment/Comparison Sites	▪ This study evaluated the effect of automatic speed enforcement introduced in Norway in 1988. A total of 64 sections (total length of 336.3 km), average length 5.25 km (0.56 to 20 km range) were used for the evaluation; speed limits were 50, 60, 70, 80, and 90 km/h. ▪ A comparison group (total number of accidents in the county before and after) was used to account for general trends. The EB approach was used to estimate expected numbers of crashes in the absence of enforcement for the post-implementation period. Automatic speed enforcement was performed by means of photo radar units mounted in fixed boxes. Although the photo radar units operated only when there was film in the cameras, motorists did not know whether it was operating or not. In August 1993, three warrants were issued in order to determine if sites could qualify for the installation of these units: • Accident rate warrant – the accident rate of the sites is higher than the accident rate for that type of road • Accident density warrant – the road should have at least 0.5 injury accidents per kilometer of road per year • Speed warrant – the mean speed of traffic should be above the posted speed limit Since automatic speed enforcement was first introduced in 1988 (before the warrants), it is possible that many sites where these units were installed, did not satisfy the warrants introduced in 1993.

Reference (Cont'd)	Elvik, R. (1997). Effects on accidents of automatic speed enforcement in Norway. *Transportation Research Record, 1595,* 14-19.
Research Design & Statistics	Before/after study using EB methods to control for regression toward the mean and comparison group (total numbers of crashes in the country before and after) was used to account for general trends.
Other Interventions	None described
Outcome Measures	<u>Changes in Crashes</u> ▪ fatal and injury crashes ▪ property-damage-only crashes
Research Design Shortcomings, Confounding Variables, and Other Issues	▪ Unfortunately, the authors report that speed data were not available for the before period for most road sections, so effects on speed could not be assessed. ▪ There were no data available on the frequency, duration, or other measures of program intensity. ▪ There is no discussion of other programs or possible confounders. ▪ The authors indicate that they could not test crash migration, due to the tendency of drivers to slow down when they are close to a photo radar unit and speed up elsewhere, or else due to diversion of traffic/crashes to other routes. ▪ The study also accounted for regression to the mean by using the EB approach. However, based on the information presented in this paper, the approach is not as sophisticated compared to more recent studies that have used the EB method. For example, the expected accidents were estimated by multiplying the vehicle miles of travel by the appropriate normal accident data – use of accident rates assume a linear relationship between crash frequency and VMT; several studies have shown that this is not a correct assumption.
Major Findings	<u>Aggregate Changes in Crashes:</u> ▪ 20% reduction in the number of injury accidents at treated locations ▪ 12% reduction in property damage only accidents (data from one section) but this was not statistically significant. ▪ Sections that conformed to the accident rate warrant and accident density warrant experienced a 26 percent reduction in injury accidents (statistically significant). ▪ Sections that did not conform to any of these warrants experienced a 5% reduction (not statistically significant). Largest effects were found for roads with speed limits of 60 and 70 km/h limits (but there was only one road section with a speed limit of 90 km/h)
Conclusions	The study found a significant reduction in injury collisions at treated locations after applying methods to control for RTM and general crash trends. A non-significant decline in non-injury crashes was also reported. Sections that conformed with both high crash rate and crash density warrants introduced to guide implementation showed greater decreases than sections that did not comply with either. Effects on speed could not be determined.

Reference	Gains, A., Heydecker, B., Shrewsbury, J., & Robertson, S. (2004). *The National Safety Camera Program: Three-year Evaluation Report*. London, United Kingdom: Department for Transport.
Study Objective	To evaluate the effectiveness of the U.K. national safety camera, cost-recovery program in terms of effects on speed and effects on casualties at the camera sites, and public acceptance and satisfactory working of the funding and partnership arrangements. [Only the effects on speed and safety are reported here.] Components for introduction of a camera, introduction of the cost-recovery program (applied to that camera), and increase in conspicuity (fixed cameras only) were incorporated into the analyses.
Study Location	24 partnership regions throughout United Kingdom (under the Cost Recovery Program)
Program Traits	• Rural and urban locations • Multiple road types and speed limits • Mobile, fixed (conspicuous), and average over distance automated speed enforcement • Driver assessed • “Notice of Intended Prosecution” must be received by offender within 14 days • Fixed fine and penalty points • Enforcement threshold not described
Study Period	variable, depending on introduction - 2003 Before period = 3 years After period = at least 1 year

Reference (Cont'd)	Gains, A., Heydecker, B., Shrewsbury, J., & Robertson, S. (2004). *The National Safety Camera Program: Three-year Evaluation Report*. London, United Kingdom: Department for Transport.
Description of Treatment/Comparison Sites	Four kinds of cameras were used under the cost-recovery program: standard fixed cameras, digital cameras (speed over distance), mobile speed cameras, and red light cameras. Speed data were obtained from more than 11,600 speed surveys from across each partnership region, from prior to the introduction of the cameras or for pre-existing camera sites. Average readings from 2002/03 were used. Speed results for 1,000 new camera sites (introduced under the cost-recovery program) in 19 partnership areas were available. For crash data: • 1,073 urban fixed, 857 urban mobile, 170 rural fixed, and 275 rural mobile camera sites over 24 partnership regions were incorporated into the study and provided at least one year's data. Fixed sites were defined as being between 400 and 1,500 m in length; mobile sites are defined as being between 400 and 3,000 m; speed over distance sites were between 3,000 and 10,000 m. Each met different types/density of crash warrants. Unsure if these affected data collection – the length for which crashes were assigned to a treated site. Data for mobile sites were collected continuously, irrespective of the frequency of enforcement. • Two types of comparison groups were included: data (PICs and KSIs) were obtained for each police-force, non-partnership area (before April 2003) of Great Britain. There could have been camera enforcement outside of the cost-recovery program in these areas. In addition, comparison with sites within the partnership areas lacking camera intervention were made.
Research Design & Statistics	Before/after study of speed effects, and the number of monthly injury crashes and number of serious and fatal injuries per site. A general comparison group from all of Great Britain was used to account for seasonal variation and general long-term trends. Log-linear modeling using Poisson regression was used for analyses of crash effects (models for injury collisions and number seriously injured and killed) of introduction of cameras with factors accounting for implementation of the cost recovery program, and increase in camera conspicuity incorporated into the models.
Other Interventions	None described

Reference (Cont'd)	Gains, A., Heydecker, B., Shrewsbury, J., & Robertson, S. (2004). *The National Safety Camera Program: Three-year Evaluation Report*. London, United Kingdom: Department for Transport.
Outcome Measures	<u>Changes in Speed</u> • mean speed • 85^{th} percentile speed, • percentage of vehicles exceeding the speed limit • percentage of vehicles exceeding the speed limit by more than 15 mi/h. Sites that had data 'before' the introduction of the cameras and 'after' data from 2002 - 2003 were included. <u>Changes in Crashes</u> • personal injury collisions (PICs) • number seriously injured and killed (KSIs)
Research Design Shortcomings, Confounding Variables, and Other Issues	• RLC sites were included in the fixed camera group, so crash effects reported for fixed and all camera sites presumably include effects resulting from behaviors affected by both types of cameras - speed and red-light running. Almost all of the RLCs were in urban areas. • The comparison group apparently included camera sites not part of the cost recovery program. There is little or no discussion about concurrent programs and whether these may be confounding the results. • The study methods and analysis did not explicitly account for changes in traffic volumes before/after the implementation, and comparison sites were not matched for volume; unknown if crash trends were same before treatment**.** • The analysis did account for time trend and seasonality. • There is very little discussion about accident migration issues. • RTM was not accounted for. Authors argue that RTM bias was not a factor because the number of crashes was not the sole criterion for site selection. • Models of the effects of changes to sites apparently incorporate factors accounting for entry into cost recovery program, and changes in conspicuity, as well as effects of camera introduction per se, but unsure whether model results include these factors combined with camera introduction.

Reference (Cont'd)	Gains, A., Heydecker, B., Shrewsbury, J., & Robertson, S. (2004). *The National Safety Camera Program: Three-year Evaluation Report*. London, United Kingdom: Department for Transport.
Major Findings	<u>Speed Changes</u> Overall, across all new cameras sites: • 2.4 mi/h reduction (about 7% reduction) in the average speed • 3.2 mi/h reduction in 85^{th} percentile speed • 32% reduction (from 45% before to 31% after) in vehicles exceeding the speed limit • 43% reduction (2.9% before to 1.6% after) in vehicles exceeding the speed limit by 15 mi/h. In general, cameras were more effective in urban areas than rural areas at reducing speeds and the percentage of excessive speeders. Fixed camera sites also experienced a much larger reduction in speeds (-5.3 mi/h) compared to mobile camera sites (-1.6 mi/h). <u>Aggregate Changes in Crashes and Injuries</u> Overall, the number of PICs and KSIs decreased at camera sites. Effects on PICs: • 33% (-36%, 029%) overall reduction in PICs • 42% (-45%, -37%) reduction at fixed camera sites • 23% (-26%, -19%) reduction at mobile camera sites • 35% (-37%, -32%) reduction at urban sites • 24% (-31%, -16%) reduction at rural sites Effects on KSIs: • 40% (-42%, -32%) overall reduction in KSIs • 49% (-53%, -44%) reduction at fixed, urban sites (985 sites) • 60% (-68%, -51%) reduction at fixed, rural sites (170 sites) • 29% (-33%, -25%) reduction at mobile, urban sites (782 sites) • 24% (-30%, -17%) reduction at mobile, rural sites (259 sites) Fixed cameras were associated with larger percentage reductions in KSIs (injuries and fatalities) at urban and rural sites, but higher frequency of KSI casualties at mobile, rural sites results in greater frequency reduction. • Pedestrian casualties also decreased: 23% reduction in PICs and 35% in KSIs.

Reference (Cont'd)	Gains, A., Heydecker, B., Shrewsbury, J., & Robertson, S. (2004). *The National Safety Camera Program: Three-year Evaluation Report*. London, United Kingdom: Department for Transport.
Conclusions	• There appears to be a reduction in the number of injury, serious injury crashes, and speeds following the introduction of cameras. RTM was not, however controlled, but the authors argue that it should not be a factor because the number of crashes was not the sole criterion for site selection. It was, however, a primary factor in the cost-recovery program. The effects on traffic flow were also not accounted for. Especially in the use of the highly conspicuous, fixed cameras, which showed the highest local reductions in injury crashes and serious injuries and fatalities, effects on traffic diversion and potential crash migration could be an issue.

Reference	Goldenbeld, C., & van Schagen, I. (2005). The effects of speed enforcement with mobile radar on speed and accidents: An evaluation study on rural roads in the Dutch province of Friesland. *Accident Analysis & Prevention, 37*,1135-1144.
Study Objective	To conduct an independent, well-documented evaluation study of the speed and safety effects of mobile, inconspicuous speed cameras on enforced sections of rural roads.
Study Location	Friesland Province, Netherlands
Program Traits	Mobile cameras (mostly) Inconspicuous Warning signs used in general area/corridors, present at all times Ticketing threshold = > 7 km/h above limit Extensive PI & E campaign involving media (weekly coverage), logo, and dedicated publicity officer Owner assessed Graduated fines, license revocation possible only for offenses > 50 km/h above limit 2 – 3 weeks average time between offense and receipt of notice 1 – 2 hours of enforcement per week per site; most by inconspicuous, mobile radar, with other added later in project. Monitoring of speeding used to re-distribute enforcement effort. For the first three years, there were around 4,000 – 5,500 operational hours; increasing to over 14,000 hours in the fourth year and reported by authorities to remain similar in the fifth year.
Study Period	1990-2002, initial implementation (8 police regions) in 1998, all 25 police regions by January, 2001. For crashes: Before period = 8 yrs After period = 5 yrs For speed: Before period = 1 yr After = 5 yrs
Description of Treatment/Comparison Sites	• Treatment was directed at rural 80 and 100 km/h single carriageway roads with high injury crashes in the before period of 1992 – 1996; 28 road segments with a total length of 116 km (average length of about 4 km) encompassing about 11% and 15% of 100 and 80 km/h roads under provincial road authority supervision. • For speed effects, the treatment group included 12 enforced road sections with speed limit of 80 km/h; 60 km total length. The comparison group was 15 comparable road sections within the province, also with 80 km/h speed limit; 51 km total length. The comparison group had, however, substantially lower traffic volumes (3,800 versus 7,200 vehicles per hour in 1997). • For road safety effects, the experimental group included all 28 speed camera enforced road sections (average length of about 4 km), the majority (23) with speed limit of 80 km/h and 5 with speed limit of 100 km/h. The comparison group included all other rural roads in the province of Friesland; approximately 5,200 km total length.

Reference (Cont'd)	Goldenbeld, C., & van Schagen, I. (2005). The effects of speed enforcement with mobile radar on speed and accidents: An evaluation study on rural roads in the Dutch province of Friesland. *Accident Analysis & Prevention, 37*,1135-1144.
Research Design & Statistics	Before/after study with comparison group on effects on speeds and crashes. Annual mean speeds and percentage of speed violators were analyzed by an analysis of variance for repeated measures with time (T) as an independent within-subjects variable, the presence or absence of speed enforcement (E) as an independent between subjects variable, and the interaction, T X E, as a within-subjects effect. Subjects were the treated or non-treated road segments. Annual average mean speeds or percentage of offenders were the repeated, dependent measures. Overall change over the period, and also contrasts between years were conducted. The road safety effect was assessed by calculating odds ratios for injury crashes and number of serious casualties. Data were compiled yearly resulting in 13 data points, one for each year of the study period, and assigned to either treatment group or comparison group.
Other Interventions	In addition to the speed campaign, enforcement programs on drunk driving, red-light running, seat belt use, helmet use by moped riders, were tailored for each region. Twenty-eight police officers added to each police region to carry out all of these enforcement tasks. According to the authors, however, no other large-scale programs focused on speeding or dangers on rural roads.
Outcome Measures	<u>Speed:</u> • Annual mean speeds per road section • Annual percentage of speed violators (7 km/h or more above limit, target threshold) per sections <u>Crashes and Injuries:</u> • Annual injury crashes per road section • Annual injuries and fatalities per section

Reference (Cont'd)	Goldenbeld, C., & van Schagen, I. (2005). The effects of speed enforcement with mobile radar on speed and accidents: An evaluation study on rural roads in the Dutch province of Friesland. *Accident Analysis & Prevention, 37*,1135-1144.
Research Design Shortcomings, Confounding Variables, and Other Issues	• Traffic volume trends were not explicitly incorporated into analyses, but authors argue that a trend of increasing traffic volumes was only slightly smaller for the experimental group compared to the comparison group. The average traffic flow was, however, substantially lower before the study period in the control sections compared to the experimental sections (3,800 versus 7,200 vehicles). • The authors also tried to consider trend effects through the use of comparison groups. Again, the much lower traffic volumes on the control sections may have limited the validity of the comparison groups. • Authors argue that "traffic flow data showed that there is no reason to assume migration of traffic from enforced to non-enforced roads explains the positive safety development at the enforced roads." • Effects of other treatments such as road engineering methods cannot be ruled out, but authors indicate that only a few engineering measures were taken during the study period. They argue that many of these measures would have affected the experimental and comparison sections to the same extent. • Although sites with high injury rates were selected, the selection period was based on 5 years of data. They also found that in the two years preceding the selection period and the year following the selection period, the number of accidents did not change significantly. The authors conclude that "since the accidents were at the same level for a period of 8 years, it is in unlikely that regression-to-the-mean played a crucial role". • Could not rule out spillover effects from enforced to non-enforced road sections, particularly in light of extensive regional media campaign (and decrease in speeds noted on unenforced segments).
Major Findings	<u>Speed effects</u> • 4 km/h reduction in mean speed on the enforced roads and 1.5 km/h reduction on the control roads from 1997 to 2002 (difference between the two not statistically significant, possibly due to small sample size). • 12% reduction in the percentage of offenders on enforced roads (i.e., exceeding 87 km/h) compared to 5% reduction in the control sections. Further analysis indicated that the years of the start of the enforcement project (i.e., 1998) and of the intensification of the enforcement project (i.e., 2001) were associated with the largest decrease in mean speed and percentage of offenders. <u>Injury Crashes effects</u> • Before/after odds ratio for injury accidents 0.79 (0.66, 0.95), indicating a 21% reduction for enforcement period compared to before period on the signed, enforced sections (average length of about 4 km). • Odds ratio for serious traffic casualties was also 0.79 (0.63, 0.99).

Reference (Cont'd)	Goldenbeld, C., & van Schagen, I. (2005). The effects of speed enforcement with mobile radar on speed and accidents: An evaluation study on rural roads in the Dutch province of Friesland. *Accident Analysis & Prevention, 37*,1135-1144.
Conclusions	Mean speeds and percentage of violators decreased during the program, but not conclusively due to the program. The numbers of injury crashes and casualties decreased by 21% at the enforced road segments during the program, but odd ratios were estimated based on relatively small numbers per the authors. High crash sites were also the target of the treatment and RTM was not accounted for; effects of other possible confounders including program spillover and other countermeasures could not be ruled out.

Reference	Hess, S. (2004). Analysis of the effects of speed limit enforcement cameras: differentiation by road type and catchment area. *Transportation Research Record, 1865*, 28-34.
Study Objective	To assess the effects of speed enforcement cameras on injury crash numbers (weighted for severity), free of trend, seasonality, and regression to the mean effects.
Study Location	Cambridgeshire, United Kingdom
Program Traits	• Fixed cameras • Conspicuous • Warning signs used at/near deployment sites • Enforcement threshold not reported • News media campaign • Other information (owner/driver assessed; penalties) not reported
Study Period	1990 – 2002 Before period varied, depending on implementation date, no minimum described. After period also varied, but a minimum of 12 months.
Description of Treatment/Comparison Sites	▪ Program evaluation involved 49 fixed camera sites installed incrementally over the study period, presumably over various road types and speed limits (not described). All speed limit enforcement cameras are highly visible, and signing is used within a radius of 1 km of the camera site. ▪ No comparison sites were used per se, but time dependent coefficients (see below) were derived using all crashes [in Cambridgeshire] including those at camera sites.

Reference (Cont'd)	Hess, S. (2004). Analysis of the effects of speed limit enforcement cameras: differentiation by road type and catchment area. *Transportation Research Record, 1865*, 28-34.
Research Design & Statistics	Before/after study of injury crash effects, using time-dependent coefficients to account for time-trends and RTM. Estimates for changes in accident numbers were produced for different distances from the camera site (250 m, 500 m, 1,000 m, and 2,000 m polygons, excluding locations not connected to the treatment site). All injury crashes over the study period were geo-coded and assigned to differing camera site catchment areas. In the case of overlapping catchment areas which occurred at a low level with the larger distances, crashes were assigned to each camera site. In order to account for differences in severity, weights were used: 1 for slight injury accidents, 5.58 for serious injury accidents, and 41.46 for fatal injury accidents. These weights were calculated based on the likelihood of these accidents from 1990 to 2002. In order to account for trend, seasonality, and regression-to-the-mean, a multiplicative coefficient to remove the time dependent components was computed. The multiplicative coefficient for a month and severity level was defined as the ratio of mean monthly accident count to the accident count for that month and severity level. This multiplicative coefficient was then multiplied to the accident count in each site for the corresponding month and severity level.
Other Interventions	None described
Outcome Measures	Injury crashes, weighted according to expected frequencies for the different severity levels
Research Design Shortcomings, Confounding Variables, and Other Issues	▪ This study did not assess program effects on driving speeds. ▪ The assigning of some crashes to more than one camera site would tend to underestimate the safety effect. ▪ The study methods and analysis did not explicitly account for changes in traffic volumes before/after the implementation. The authors indicate that "no significant changes in traffic levels over time have been reported for the sites under consideration other than those also observed on other sites, (i.e., the regional trend) which are accounted for in the overall time-dependent coefficients…" ▪ Based on looking at the accident reductions at difference distances from the camera site, authors argue that there is very little evidence of accident migration. There is very little discussion about possible changes in traffic patterns due to deployment. ▪ It is not clear if the use of time dependent coefficients sufficiently account for regression to the mean and other confounders. ▪ Confidence intervals or significance levels were not reported. Results showing the effects due to RTM and other trends were not provided.

Reference (Cont'd)	Hess, S. (2004). Analysis of the effects of speed limit enforcement cameras: differentiation by road type and catchment area. *Transportation Research Record, 1865*, 28-34.
Major Findings	Aggregate Changes in Crashes: ▪ 45.7% reduction in weighted injury accidents for the 250-meter range ▪ 41.3% reduction for the 500-meter range ▪ 31.6% reduction for the 1000-meter range ▪ 20.9% reduction for the 2000-meter range. Results were also reported for different distances on A (major) and non-A roads, urban roads and trunk roads, and multi-camera versus stand-alone sites. Based on the results of the analysis that was conducted for different roadway types, authors argue that the biggest reduction in accident numbers can be obtained on roads with a higher incidence of speeding offenses.
Conclusions	Results suggest that conspicuous, fixed-camera speed enforcement had positive safety benefits, net of RTM and other trend effects at the treated locations, with greater benefits closer to the camera sites, but it is not clear if the use of time-dependent coefficients properly accounted for RTM. In addition, confidence intervals/statistical significance were not reported. Authors postulate that these results suggest that sudden braking near camera sites is not increasing crashes since crash rates closer to the treated sites dropped most. Effects on driving speeds were not documented by the study.

Reference	Mountain, L., Hirst, W., & Maher, M. (2004). Costing lives or saving lives? A detailed evaluation of the impact of speed cameras on safety. *Traffic Engineering and Control*, *45*(8), 280-288.
Study Objective	To assess the separate impacts of camera enforcement due to effects on speed and traffic flow at various distances from the camera sites, controlling for both RTM and trend effects. The study sought to assess whether effects on speeds upstream or downstream of camera sites affect crash outcomes and whether diversion of traffic to other routes is sufficient to contribute to crashes.
Study Location	Great Britain, United Kingdom
Program Traits	• Fixed camera sites • Conspicuous enforcement • Warning signs and brightly colored camera housings required since 2001 • Publicity not described • Enforcement threshold not reported • Penalty (probably to driver, not reported)
Study Period	The study period varied, depending on time of implementation across sites, exact start and end dates not provided For crashes Before period = 3 years prior to implementation for each site After period = 2.3 years, average, and up to 3 years post-implementation for each site. For speed Not described
Description of Treatment/Comparison Sites	• Treatment sites included 62 fixed speed camera sites on 30 mi/h speed limit roads with reported severe speeding problems throughout the United Kingdom. Data were collected for up to 2 km centered on the camera site. • The EB approach was used to estimate expected numbers of crashes in the absence of enforcement for the post-implementation periods. U.K. national accident totals and traffic flows were also used for comparison to camera sites and incorporated into analyses. All traffic flow changes at the sites different from national trends were assumed to be due to the cameras.

Reference (Cont'd)	Mountain, L., Hirst, W., & Maher, M. (2004). Costing lives or saving lives? A detailed evaluation of the impact of speed cameras on safety. *Traffic Engineering and Control*, *45*(8), 280-288.
Research Design & Statistics	Before/After observational study using EB methods to control for RTM and time trends for safety (collision) effect. Study also estimated reductions in crashes related to 'traffic diversion.' At least one measure of traffic flow was obtained before and after the start of enforcement (at each site). Time trend effects were considered at two levels: (1) A crash risk time trend correction was applied to the predictive model to account for changes over time (since model estimates were derived from the period 1980 - 1991. (2) A comparison group that included U.K. national accident totals and traffic flows. This approach assumes that traffic volumes at these fixed camera sites would have followed the national trend if cameras had not been installed. Authors stated that no changes in flow due to other causes were anticipated at any of the sites. Simple before/after comparison for speed effect.
Other Interventions	None are described
Outcome Measures	<u>Changes in Speeds</u> • Mean speeds • 85^{th} percentile speeds • Percentage exceeding speed limit <u>Changes in Crashes</u> • Personal injury crashes at different distances from the camera site. • Results for fatal and severe injury crashes were also provided. • Results due to changes in traffic flow, trend, and RTM also provided

Reference (Cont'd)	Mountain, L., Hirst, W., & Maher, M. (2004). Costing lives or saving lives? A detailed evaluation of the impact of speed cameras on safety. *Traffic Engineering and Control*, *45*(8), 280-288.
Research Design Shortcomings, Confounding Variables, and Other Issues	Analyses incorporated local and national traffic and crash trends into predictive models, used EB procedures to account for RTM, and examined possible effects on crash migration due to traffic flow changes or kangaroo effect. It is not clear to what extent the time crash trend correction factor incorporated into EB model estimates is adequate to account for changes over time (this may be explained in Hirst et al. 2004a and 2004b). Another approach that has been used recently in other studies is to estimate a safety performance function using data for the study period, and estimate annual factors to account for trend and other year-to-year changes.
Major Findings	Mean speeds decreased by an average of 4.4 mi/h and 85th percentile speeds fell by 5.9 mi/h. There was also a 35% reduction in the percentage exceeding the speed limit. Results for monitoring lengths of up to 250 m, 500 m and 1 km were similar. Aggregate Changes in Personal Injury Crashes • 25% reduction (-35, -14 CI), 500 m monitoring length: 20% attributed to the impact of cameras on speed (and possibly other aspects of driver behavior), and about 5% of the reduction attributed to diversion of traffic to other routes. • 24% reduction (-33, -13 CI), 1 km monitoring length: 19% attributed to changes in speed and 5% to traffic diversion. For up to 500 m and 1 km monitoring lengths, the overall average effect on fatal and serious injury crashes were lower than expected, but not significantly. Estimates of changes in crashes attributable to non-program effects including trend effects and RTM were also provided. Positive crash trend effects, if not controlled, would have made cameras appear less effective in reducing personal injury crashes for distances up to 1 km, whereas RTM effects of up to 18% accounted for over half of the overall observed decrease in fatal and serious crashes for 500 m monitoring length. Authors also provide estimates of treatment effects (RTM and trend effects excluded) on changes in crashes / km / year.

Reference (Cont'd)	Mountain, L., Hirst, W., & Maher, M. (2004). Costing lives or saving lives? A detailed evaluation of the impact of speed cameras on safety. *Traffic Engineering and Control*, *45*(8), 280-288.
Conclusions	Estimates suggest that effects of cameras reduced proportions of crashes up to 1 km upstream and downstream of camera sites by about 25%. Thus monitoring for longer distances can document more crash savings and appears not to mask effects by including areas not affected by the cameras, at least in this study. Cameras could have benefits beyond 1 km, but this study could not assess that outcome. There was no evidence that sudden braking or speed changes resulted in increases in crashes. There was evidence for significant shifting of traffic to other routes which contributes to the decrease in crashes resulting from cameras on the treated segments, but could have a detrimental impact on alternate routes. Traffic flows as well as speeds and crashes should therefore be monitored for signs of traffic diversion, and if detected, alternate routes should be monitored. Significant RTM effects were observed for both personal injury crashes, and fatal and serious crashes. RTM accounted for around half of observed fatal and serious injury crash reductions, resulting in non-significant treatment effects on more serious crashes. Authors suggest that this result is not unexpected due to the relatively low numbers of serious crashes on the treated segments and argue that sites with large numbers of less serious crashes can also benefit from increased enforcement and perhaps proactively prevent more serious crashes.

Reference	Newstead, S.V., & Cameron, M.H. (2003). *Evaluation of Crash Effects of the Queensland Speed Camera Program* (Report No. 204). Victoria, Australia: Monash University, Accident Research Center.
Study Objective	The objective was to assess the before/after changes in crashes within 6 km of speed camera sites in a Queensland speed camera enforcement program. Based on the overt speed camera operations, along with the allowable areas of operation, the researches thought that effects would be primarily localized. Due to the way in which zones of camera operation were defined (5 km in rural areas and 1 km in urban areas), effects of up to 6 km area of potential influence were used.
Study Location	Queensland, Australia
Program Traits	• Mobile cameras • Conspicuous (marked vans/vehicles used in deployment) • Signs used at/near enforcement sites • Media and other publicity campaign including paid advertising, began December 1996. • Enforcement threshold not reported • Penalty information not reported (owner or driver; types of penalties) • Average hours of speed camera operation were: 1,000 hours per month early in the program; 4,000 hours by April 2001. Three camera units initially, building to 15 by June 1997.
Study Period	1992 – 2001 Before period = 5 years After period = 4.5 years
Description of Treatment/Comparison Sites	A conspicuous (marked vans), mobile radar program was introduced in Queensland in May 1997. Initially treatment was at 500 high crash locations (zones) where a speed limit review had also been done. Speed camera zones were 1 km in diameter in urban areas and 5 km in rural areas. Up to nine specific camera sites may be operated within a zone. There were 500 zones during early stages of program; 1,500 zones (with an average of 1.5 sites /zone) by April 2001. On a particular day, specific camera sites were selected at random within a particular area. By June 2001, there were more than 2,500 camera sites. Sites outside the 6 km boundaries were used as comparison/control sites and were intended to reflect general effects of the other enforcement programs (started before the speed camera program) and general trends.
Research Design & Statistics	Before/after observational study of crash effects including comparison group. Crash effects (of varying severity) were estimated using a Poisson, log-linear model with monthly crash counts as the dependent variable. Independent variables included a linear trend component, dummy variables to distinguish between control and treatment sites (treatment sites were divided into 3 categories – areas within 2 km of the camera site, areas from 2 km to 4 km of a camera sites, and areas from 4 km to 6 km of a camera sites), and dummy variable to distinguish between before and after conditions.

Reference (Cont'd)	Newstead, S.V., & Cameron, M.H. (2003). *Evaluation of Crash Effects of the Queensland Speed Camera Program* (Report No. 204). Victoria, Australia: Monash University, Accident Research Center.
Other Interventions	• Speed limit review of about 20% of the State-controlled road network undertaken prior to the program. About 5% of the reviewed roads underwent speed limit change, including introduction of 110 km/h limits on some 100 km/h limits on over 1,100 km of roads. An internal evaluation showed no crash effects, little change in average travel speeds, and reduced speed variation on the rezoned roads. • Queensland Random Road Watch program, covering the whole of Queensland with varying density from before start of speed camera program. • Implementation of 50 km/h local street speed limit in southeast Queensland.
Outcome Measures	• All reported collisions by severity level, police region, speed zone and distance from nearest camera site
Research Design Shortcomings, Confounding Variables, and Other Issues	• This study did not examine effects on driving speeds. • Traffic volume was not considered directly in this study. General trends may have been accounted for using comparison group if trends were the same between groups, but effects due to the treatment were not considered. • In the multiple regression analysis, a linear trend component was introduced to account for long-term trends, but seasonality was not considered. • The inclusion of the comparison group seems to account for other confounding factors, as long as confounders would have been the same within the 6 km treatment zones and the comparison zones outside these boundaries. • There is no discussion of accident migration in the report, either near treated sites or traffic diversion to other routes. The camera operation was overt, but the sites were selected randomly everyday. It is difficult to speculate if this would have led to changes in travel patterns. There is no evidence of the kangaroo effect – even locations between 4 and 6 km from the camera sites did not experience an increase in crashes that were statistically significant. • The authors argue that regression to the mean would not have a significant effect on the results for the following reasons: - Three years passed between the first consideration of the program and implementation - 85% of the crashes occurred within 4 km of camera sites - The before period was quite long (six years) Despite these arguments about the lack of bias due to RTM, it would have been better to use EB or similar methods so that any possible effects due to RTM are accounted for.

Reference (Cont'd)	Newstead, S.V., & Cameron, M.H. (2003). *Evaluation of Crash Effects of the Queensland Speed Camera Program* (Report No. 204). Victoria, Australia: Monash University, Accident Research Center.
Major Findings	Results indicated that crash reductions associated with the speed camera program were greatest in the 0 to 2 km area around the camera sites. For the total post-implementation period (January 1997 to June 2001), Aggregate Changes in Crashes (within 2 km around camera sites): • 17.5% reduction in crashes, all severity (p<.0001) • 15.7% reduction in fatal crashes (not significant) • 21.9% reduction in hospitalization crashes (p=.0001) • 15.6% reduction in fatal to medically treated crashes (p=.0002) • 14.7% reduction in other injury crashes (trend, p =.0986) • 20.3% reduction in no-injury crashes (p<.0001) There were also some significant crash reductions in the larger areas surrounding camera sites: • 11.4% reduction in all severity crashes within the 2 to 4 km area • 10.7% reduction in all severity crashes in the 4 km to 6 km area • 14.7% reduction in hospital admission crashes in the 2 to 4 km area • 11.2% reduction in fatal to medically treated in the 2 to 4 km area • significant reductions in no injury crashes at 2 to 4 and 4 to 6 km Injury reductions • 12% in fatal and medically treated injuries in first year of operation • 5% in fatal and medically treated injuries by 2000 – 2001 Again, the reductions were higher within the 2 km area nearest to the cameras. There were some increases in fatal crashes in some years (non-significant) in the 2 to 4 and 4 to 6 km areas around camera sites.
Conclusions	Reductions in estimated crashes were documented up to 6 km radius from the treated sites relative to comparison group, but reductions were greatest within 2 km of treated sites. The reductions due to changes in speed versus possible changes in traffic flows were not ascertained. Regression toward the mean was not specifically controlled.

Reference	Tay, R. (2000). Do speed cameras improve road safety? In K.C.P. Wang, G. Xiao, and J. Ji (Eds.), *International Conference on Traffic and Transportation Studies* (2nd ed.) (pp. 41-51). Beijing, China: China Association for Science and Technology.
Study Objective	To examine the effects of speed cameras on road safety in Christchurch, New Zealand, using data from 24 speed camera zones in the city.
Study Location	Christchurch, New Zealand
Program Traits	• Mobile cameras (a small proportion were fixed) • Signed enforcement locations • Enforcement threshold not reported • Owner assessed • Penalties not described
Study Period	October 1993 – January 1995
Description of Treatment/Comparison Sites	• 24 camera sites in city of Christchurch, New Zealand. First three cameras were introduced in October 1993. Number of cameras grew to 24 over the next two years. • Comparison Group included all non-camera areas throughout Christchurch.
Research Design & Statistics	Before/after study using comparison group. Analyses accounted for different time periods based on when cameras were introduced.
Other Interventions	None described.

Reference (Cont'd)	Tay, R. (2000). Do speed cameras improve road safety? In K.C.P. Wang, G. Xiao, and J. Ji (Eds.), *International Conference on Traffic and Transportation Studies* (2nd ed.) (pp. 41-51). Beijing, China: China Association for Science and Technology.
Outcome Measures	• All crashes • Serious injury crashes (fatal and serious injury combined) • [Speed trends, mean and 85th percentile, were reported for the period 1989 – 1997, but it isn't clear how the data were collected.]
Research Design Shortcomings, Confounding Variables, and Other Issues	• Speed trends were reported (but data collection not described). • Treatment length or area was not described; unknown how were crashes assigned to treatment versus comparison group. • Volume trends not directly accounted for; use of citywide comparison group may control for volume and general crash trends. • Possibility of crash migration (or other spillover effects) not discussed. • Treatment sites selected at least partly on basis of crash histories; possible RTM not accounted for. • Relatively short study period; before/after periods not described.
Major Findings	Aggregate change in crashes: • 9.2% (+/- 5.9%) reduction in all crashes • 32.3% (+/- 12.5%) reduction in serious injury crashes.
Conclusions	There was a small decreasing trend in the estimate of all severity crashes in the after periods over all sites combined and a significant decrease in the estimate of serious injury crashes. Effects due to regression toward the mean cannot be ruled out.

PART II: RED LIGHT RUNNING

Reference	Burkey, M., & Obeng, K. (2004). *A detailed investigation of crash risk reduction resulting from red light cameras in small urban areas*. Washington, DC: U.S. Department of Transportation, Research and Special Programs Administration.
Study Objective	Objective of research is to analyze the impact of RLCs on red light violations and crashes in a holistic framework by accounting for the roles of intersection characteristics and environmental variables in such crashes, and that also account for changes in crashes at intersections with no RLCs.
Study Locations	Greensboro, North Carolina; High Point, North Carolina
Study Period	57 months
Description of Treatment/Comparison Sites	18 treatment RLC intersections 285 control sites (non RLC) Before RLC treatment – 26 months After RLC treatment – 21 months
Research Design & Statistics	Expanded before/after study, with treatment and control sites. Poisson and negative binomial time-series regression models were developed to relate the characteristics of signalized intersections (including road characteristics and traffic volumes) and environmental conditions (snow, liquid precipitation) to the type and severity of crashes. Estimates for types and severities of crashes included three equality constraints involving six coefficients and three pairs of variables. The three constraints were with respect to amber time for each road, all-red time, and one-way roads at intersections. For each pair of variables, the estimated coefficients were set to be equal, so that each pair of variables have equal effects at intersections.
Other Interventions	None
Outcome Measures	10,721 crashes over the 57-month study at 303 intersections. Five crash types were studied, plus an "Other" category: • angle crashes • rear ending a slowing or stopped vehicle • left turning vehicle struck by a second vehicle on the same roadway • left turning vehicle struck by a second vehicle on a different roadway • sideswipe by a vehicle on same roadway • "Other" Crash Severity: • Severe (Type A "fatalities and severe injuries" plus Type B "evident non-disabling injuries") • Possible Injuries (Type C) • Property damage only (no injuries)

Reference (Cont'd)	Burkey, M., & Obeng, K. (2004). *A detailed investigation of crash risk reduction resulting from red light cameras in small urban areas*. Washington, DC: U.S. Department of Transportation, Research and Special Programs Administration.
Exposure Data	Average Daily Volumes
Research Design Shortcomings, Confounding Variables, and Other Issues	Study was not a randomized trial, thus there is no certainty of a causes/effect relationship with the RLC treatment and non-treatment intersections. They used signalized intersections without cameras as controls in the same communities as the treatment sites (which ignores the effect of spillover). Also, the cameras were installed at intersections with higher crash rates (more than twice as many crashes as at other intersections in the city before the cameras were installed). Threshold for reporting crashes was $1,000, which was likely to affect the number of reported rear-end crashes and the number of sideswipe/same direction crashes.
Major Findings	<u>Before/After at Treatment Sites Only</u>: RLC camera installation resulted in a range of results across the 18 intersections, from a 30.8% decrease in total crashes to a 68.8% increase. On average, the results show a 2.5% decrease. There was an increase in rear-end crashes, a small increase in angle crashes, and a decrease in left turn/same roadway and left turn/different roadway. <u>Treatment versus Control Sites</u> <u>with Poisson Regression Model Controlling for Weather, Volume, and Intersection characteristics</u>: Signalized intersections had higher crash rates following the implementation of RLCs when compared to similar intersections, for all crash categories studied with the exception of "left turn/different roadway." Since this crash type accounts for less than 3.6% of crashes overall, and less than 2.5% of crashes at the RLC sites, this will have little effect overall. • Total crashes: Increase associated with placement of RLC. Model estimated that had an RLC not been placed at an intersection, there may have been a 42% decrease in crash rate, holding other factors constant. • Angle crashes: Controlling for the effects of weather, road and traffic light characteristics, and traffic signs, RLCs were associated with no changes in angle crashes. A trend was seen for increases in angle crashes at both RLC and non RLC intersections. • Rear end crashes: increased at RLC sites and decreased elsewhere. The model estimated a very large impact of RLC on this crash type (78% increase). • Left turning vehicles on different roadways: RLC sites associated with a reduction in this crash type (model estimates 60% decrease). • Sideswipe a vehicle moving in the same direction: RLC associated with an increase. • Left turning vehicles on same roadway: marginally significant increase in crashes associated with RLC. • Other crashes: trend toward increased crashes associated with RLC, but not significant For severity of crashes, RLCs were found to have a statistically significant and large effect on property damage only and possible injury crashes. There was a positive, but statistically insignificant estimated effect on severe (fatal, evident, and disabling) crashes. • Severe crashes: Increase in severe crashes with RLC, but not significant. RLCs therefore did not produce a benefit by reducing the severity of crashes relative to other intersections. • Possible injury crashes: High statistically-based increase with the addition of RLC, relative to other intersections (40 to 50%). • Property damage: addition of RLC associated with significant increase in PDO crashes, relative to other intersections (40 to 50%).
Conclusions	Results do not support a conclusion that RLCs reduce crashes. The evidence pointed to the installation of RLCs as a detriment to safety. At a minimum, the researchers conclude that *there is no evidence that the RLC program is decreasing crashes.*

Reference	Butler, P.C. (2001). A *quantifiable measure of effectiveness of red light running cameras at treatment and non-treatment sites.* Master's thesis, Howard University, Washington, DC.
Study Objective	This research evaluated the safety benefits of the red light running cameras along two arterials in Howard County, Maryland. The study examined whether the red light running cameras are effective in reducing angle crashes at camera-equipped intersections (treatment) and at non-camera-equipped intersections (comparison).
Study Location	Howard County, Maryland
Study Period	30 months before – August 15, 1995 to February 17, 1998 30 months after – February 18, 1998 to August 15, 2000
Description of Treatment/Comparison Sites	Howard County, Maryland (6 intersections) • 2 camera sites (treatment) • 4 non-camera sites at intersections adjacent to treatment sites (not operable camera mount) Bucks County, Pennsylvania (4 intersections) • Control sites to account for spillover effect. Similar population and ADT to treatment sites. No experience in this county or in neighboring Philadelphia with RLCs
Research Design & Statistics	Chi-squared analysis before and after with similar treatment and control sites based on ADT and geographical separation.
Other Interventions	1998 National RLR campaign. 1993 Howard County began media campaign to alert drivers to dangers of running red lights.
Outcome Measures	Right angle crashes
Exposure Data	Treatment and control intersections have similar ADT on study approaches (40,000 ADT at treatment sites and 32,000 ADT at control sites).

Reference (Cont'd)	Butler, P.C. (2001). *A quantifiable measure of effectiveness of red light running cameras at treatment and non-treatment sites.* Master's thesis, Howard University, Washington, DC.
Research Design Shortcomings, Confounding Variables, and Other Issues	The RLCs were only in operation 3 years at the treatment sites. This may not be sufficient period of time to attribute the reduction in angle crashes at non camera intersections to the presence of the RLC**.**
Major Findings	Before/After within site type: • At the treatment sites, there was a 50% reduction in angle crashes in the period after the cameras were installed. Chi-squared analysis showed this result to be significant at the .10 alpha level, but not at the .05 alpha level. • Angle crashes at the adjacent non camera sites were reduced by 23% in the same time period, however this difference was not significant. • At the control sites, there was a 19% reduction in angle crashes in the after period, which was significant at the .05 alpha level. This was due to a large increase in angle crashes at one particular intersection that may be considered an anomaly Non camera sites versus control sites: • No significant difference in angle crashes in before versus after period Camera sites versus control sites: • No significant difference in angle crashes in before versus after period Camera sites versus non-camera sites: • No significant difference in angle crashes in before versus after period There were no significant differences between the treatment sites and the control sites or between the non-treatment sites (not operable cameras) and the control sites. It may be due to the fact that all three of the sites experienced angle crashes reductions during this test period. (One intersection in the control group had a large increase in angle crashes while the other three intersections had angle crash reductions. This anomaly caused the large significant differences in the before and after angle crashes.)
Conclusions	The two intersections equipped with RLCs showed a significant difference in angle crashes between the before and after camera installation time period at the 91.7% confidence level, indicating that the difference can be attributed to the presence of the RLCs, There were no signalization or geometric changes made at these intersections during this time period**.**

Reference	Council, F.M., Persaud, B., Eccles, K., Lyon, C., & Griffith, M.S. (2005). *Safety evaluation of red-light cameras* (FHWA-HRT-05-048). Washington, DC: Federal Highway Administration.
Study Objective	To determine effectiveness of RLC systems in reducing crashes, and the associated economic effects of RLC systems.
Study Location	Baltimore, Maryland Charlotte, North Carolina El Cajon, California Howard County, Maryland Montgomery County, Maryland San Diego, California San Francisco, California
Study Period	Roughly 1994 to 2002: Encompasses 4 to 9 years of before-period data, with an average before-period of 6 years; length of the after- period data varied from less than 1 year (approximately 8% of the sites) to 5 years, with an average after- period of approximately 2.76 years for all sites
Description of Treatment/Comparison Sites	• 132 treatment sites • 408 signalized intersections, not RLC-equipped (reference group for calibration of safety performance functions in EB analysis and spillover analyses) • 296 unsignalized comparison sites
Research Design & Statistics	EB before/after research design. Accounts for regression to the mean and tries to overcome the difficulties by using crash rates in normalizing for volume differences between the before and after periods. Economic analysis examined the extent to which the increase in rear end crashes negates the benefits of right angle crashes. An aggregate crash cost benefit of RLC was developed.
Other Interventions	• RLC warning signs • Publicity
Outcome Measures	Crashes and Types: • right-angle (side impact) • left-turn (two vehicles turning) • left-turn (one vehicle oncoming) • rear end (straight ahead) • rear end (while turning) • other (crashes identified as red-light running)
Exposure Data	Average Annual Daily Traffic (AADT)

Reference (Cont'd)	Council, F.M., Persaud, B., Eccles, K., Lyon, C., & Griffith, M.S. (2005). *Safety evaluation of red-light cameras* (FHWA-HRT-05-048). Washington, DC: Federal Highway Administration.
Research Design Shortcomings, Confounding Variables, and Other Issues	• Definition of "red-light-running" and "rear-end" crashes (e.g., distance of crash within intersection) slightly different in some jurisdictions, in crash data files, as no "red light running" crash category is available on most police crash forms • Modest spillover effect on right-angle crashes at non-RLC signalized intersections (decrease in right-angle crashes without an accompanying increase in rear-end crashes).may be attributed to study type (retrospective as opposed to prospective), or other programs and treatments at non-RLC signalized intersections over the years
Major Findings	Aggregate results in terms of crash reduction: • 25% decrease in total right-angle crashes • 16% reduction in injury right-angle crashes • 15 % increase in total rear-end crashes • 24 % increase in injury rear-end crashes Net economic effects: RLCs can save society $39,000 to $50,000 annually at each intersection where they are installed. Costs considered include hospital bills, property damage to vehicles, insurance expenses, value of lost quality of life, and other costs. Greatest economic benefits are at locations with the following characteristics: high ratio of right-angle to rear-end crashes, a higher proportion of entering AADT on the major road, and the presence of left-turn protected phases. Univariate analysis revealed: • high publicity associated with a > benefit than medium publicity levels. • fine plus demerit point penalty associated with a > benefit than a fine-only penalty. • warning signs at intersections only is associated with < benefit than these signs at intersections and city limits. • indications that the aggregate economic benefits > with total entering AADT, increasing proportions of total traffic being on major road, and increasing ratio of right-angle to rear end crashes. • indications that the aggregate economic benefits > with shorter cycle lengths and shorter intergreen periods
Conclusions	The analysis based on an aggregate of rear end and right-angle crash costs for various severity levels, showed that RLC systems do indeed provide a modest aggregate crash-cost benefit. Although rear-end crashes increased with the installation of RLC, they are lower in severity and unit cost than right-angle crashes, which decreased with the installation of the cameras. Thus the increase in rear-end crashes did not negate the effects of the decrease in right-angle crashes.

Reference	Cunningham, C.M., & Hummer, J.S. (2005). *Evaluating the use of red light running photographic enforcement using collisions and red light running violations*. Raleigh, NC: North Carolina Governor's Highway Safety Program.
Study Objective	One of the objectives of this study was to conduct a before/after crash study for various types of crashes related to red light running violations in the city of Raleigh, North Carolina, and to analyze the frequencies of RLR violations to identify driver behavior changes at photo-enforced intersections. Other task activities included a literature review; and focus groups sessions with the public to gather information on attitudes, opinions, and beliefs about the use of AES to enhance traffic laws.
Study Location	Before/after collision study: Raleigh, North Carolina
Study Period	67 months before installation (Jan 98 to July 03) 7 months after
Description of Treatment/Comparison Sites	• Seven treatment intersection sites: high angle collision locations, selected from a pool of intersections provided by the city. In order to disperse cameras throughout the city, some high-collision sites were passed over. • Ten comparison sites: chosen from the same pool as the treatment pool (for halo effect comparison, seven comparison sites were used, each within 1 mile of a treatment site and on the same corridor as the treated location). Site selection process allowed sites to be chosen in a "nearly random" fashion. Sample odds ratio used to determine if comparison sites followed similar crash patterns as the treatment sites. Mean odds ratio= .9608, indicating that treatment and comparison sites acted in a similar manner with respect to total collisions lading up to the inception of RLCs.
Research Design & Statistics	Crash Analyses: • Two comparison group before/after studies ("Comparison Group Study" and "Comparison Group Study for Halo Effect") • One simple before/after study ("Causal Factor Study": only treatment sites to analyze causal factors) Violation Analysis: • Chi-squared test of independence was used to see whether camera implementation had an effect on decreasing RLR violations greater than 2 seconds after the onset of the red ball indication (expected collision time)
Other Interventions	None
Outcome Measures	Four crash types studied: • Total crashes • RLR related (angle, rear-end, left turn same roadway, left turn different roadway) • Angle crashes • Rear-end crashes
Exposure Data	Traffic volume over time

Reference (Cont'd)	Cunningham, C.M., & Hummer, J.S. (2005). *Evaluating the use of red light running photographic enforcement using collisions and red light running violations*. Raleigh, NC: North Carolina Governor's Highway Safety Program.
Research Design Shortcomings, Confounding Variables, and Other Issues	Comparison group analysis to account for halo effect appears counter-intuitive and may be related to small sample sizes or removal of downtown sites on a pre-timed signal system grid where the majority of the treatment sites were located.
Major Findings	Estimated Effect of RLC: Simple Before/After Study Using Treatment Sites Only: (does not account for regression to the mean and seasonality effects) Total crashes 30% reduction RLR-related crashes 32% reduction Angle crashes 51% reduction Rear-end crashes 2% increase Estimated Net Effect of RLC : Prediction using a Comparison Group (Accounts for flaws such as seasonality factors and effects of regression to the mean): Total crashes 17% reduction RLR-related crashes 22% reduction Angle crashes 42% reduction Rear-end crashes 25% reduction Estimated Net Effect of RLC: Accounting for halo-effect using only comparison sites within same corridor and 1 mile of treatment site ("improved comparison group analysis"): Total crashes 14% reduction RLR-related crashes 19% reduction Angle crashes 35% reduction Rear-end crashes 27% reduction Chi-squared test of independence used to determine whether the frequency of violations greater than 2 seconds reduced when cameras where in place showed that RLR violations decreased significantly when cameras were in place (p value < .0010)
Conclusions	The first comparison group study appears to be the best choice for analysis, as it accounts for seasonality effects and regression to the mean. Based on this comparison group study, collisions appear to decrease substantially with total, RLR related, angle, and rear-end decreasing by 17%, 22%, 42%, and 25% respectively. RLCs appeared to have a positive affect on driver behavior. Focus groups indicated that there is a positive perception of RLCs as countermeasures to deter red light running.

Reference	Garber, N.J., Miller, J.S., Eslambolchi, S., Khandelwal, R., Mattingly, K.M., Sprinkle, K.M., & Wachendorf, P.L. (2005). *An evaluation of red light camera (photo-red) enforcement programs in Virginia: a report in response to a request by Virginia's Secretary of Transportation* (VTRC 05-R21). Charlottesville, Virginia: Virginia Transportation Research Council.
Study Objective	Study evaluated Virginia's RLC enforcement programs using three tests of feasibility: technical (program met legal standards, performs with accuracy to be accepted, and enjoys public support); fiscal (program revenues and costs are balanced); and operational feasibility (improves safety).
Study Location	Charlottesville, Virginia* Alexandria, Virginia*‡ Arlington, Virginia*‡ Fairfax City, Virginia*† Fairfax County, Virginia*‡ † Falls Church, Virginia*† Vienna, Virginia*‡ † Virginia Beach, Virginia* * operational & technical info ‡ 6 month citation data † crash data
Study Period	• Approximately six years of crash data for 4 sites (1998 – 2003), before RLC 1998 - 2000, after RLC 2001- 2003 • 6 months of citation data (all post-installation at 22 intersections). Number of citations in 4th, 5th, and 6th months after installation (early installation period) compared to most recent 3-mo of operation (latest installation period).
Description of Treatment/Comparison Sites	Fairfax County: Before/After at 12 RLC intersections. 33 non-camera intersections were also used as control sites (Levels 3 and 4 analyses) Falls Church : Before/After – 2 RLC intersections Vienna: Before/After – 6 RLC intersections Fairfax City: Before/After – 3 RLC intersections
Research Design & Statistics	Basic Crash Analysis at 4 jurisdictions (Levels 1 and 2, using t-tests and paired sample t-tests; do not consider confounding effects such as yellow time, lag time, percentage of heavy trucks. Level 1: compare crashes/year at each intersection before and after installation of camera. Level 2: Same as 1, except crash rates are compared; rates are crashes/million entering vehicles on major road) Analysis of Variance Results in 1 jurisdiction (Fairfax Co) (Level 3: compare crashes/year at intersections with and without cameras for before/after periods, but stratify for volumes, difference between actual and desired yellow time, and heavy truck percentages) Advanced Crash Analysis Results in 1 jurisdiction (Fairfax Co) – EB Method/Paired t-Test (Level 4: crashes that "would have occurred" are estimated from models based on data and then compared to crashes that did occur)
Other Interventions	None

Reference (Cont'd)	Garber, N.J., Miller, J.S., Eslambolchi, S., Khandelwal, R., Mattingly, K.M., Sprinkle, K.M., & Wachendorf, P.L. (2005). *An evaluation of red light camera (photo-red) enforcement programs in Virginia: a report in response to a request by Virginia's Secretary of Transportation* (VTRC 05-R21). Charlottesville, Virginia: Virginia Transportation Research Council.

Outcome Measures	<u>Summary citation data collected at four jurisdictions</u>.
	• 6 months violation (citation) data for red light running in 4 jurisdictions (Alexandria, Arlington, Fairfax County, Vienna) – all post RLC installations – 22 intersections
	
	<u>Summary crash information was collected at four jurisdictions (Fairfax City, Fairfax County, Falls Church, Vienna)</u>
	• Total number of crashes at each intersection during study period (1998-2003)
	• Number of crashes per intersection year (before and after RLCs)
	• Modified crash rate – number of crashes per year divided by the AADT on the major road (units of millions of vehicle miles traveled – VMT)
	• Significance testing based on before/after comparisons of the number of intersection crashes per year
	    ◦ Total crashes
	    ◦ Rear-end crashes attributable to signal
	    ◦ Total injury crashes
	    ◦ Injury crashes attributable to red light running
	    ◦ Red light running crashes
	
	<u>Detailed crash information was used for only one jurisdiction (Fairfax County) covering a six-year time span (three year before and three years after RLC treatments).</u>
	• All crashes
	• All red light running crashes
	• Rear-end crashes
	• Injury crashes due to red light running only
	• All injury crashes

Exposure Data	AADT
Research Design Shortcomings, Confounding Variables, and Other Issues	RE operational feasibility: more precise index of crash severity necessary for comparison of decreases in injury crashes attributable to red light running, and increase in total injury crashes. Also, a modified crash rate (based on major road volumes) rather than a full intersection crash rate (based on mainline and minor road volumes) was used.

Reference (Cont'd)	Garber, N.J., Miller, J.S., Eslambolchi, S., Khandelwal, R., Mattingly, K.M., Sprinkle, K.M., & Wachendorf, P.L. (2005). *An evaluation of red light camera (photo-red) enforcement programs in Virginia: a report in response to a request by Virginia's Secretary of Transportation* (VTRC 05-R21). Charlottesville, Virginia: Virginia Transportation Research Council.
Major Findings	Fairfax County only (Detailed Analyses) • Total crashes increased 12% • Total injury crashes increased 18% • Rear-end crashes increased 57% • Red light running crashes decreased 28% • Injury crashes attributable to red light running decreased 28% • Average reduction in citations was 19% Alexandria, Arlington, Fairfax County, and Vienna (Summary Analyses) • Decrease in number of crashes directly attributable to red light running where one driver is charged with the offense of "failure to yield to stop light" (decrease not statistically significant at two sites, and statistically significant at two sites) • Increase in the number of rear-end crashes (increases significant at one site, increases not significant at two sites, insignificant decrease at one site) • Mixed results for total angle crashes (insignificant increase at one site, significant decrease at another site) • Mixed results for total crashes (insignificant increases at two sites; insignificant decreases at two sites) • Insignificant increase in total injury crashes at four sites. • Average reduction in citations across four jurisdictions (where each intersection carries equal weight) = 19% and where each citation carries equal weight = 33%
Conclusions	Operational Feasibility (impact of program on crashes and citations) • Cameras reduced the number of violations. • Photo enforcement reduced the number of crashes directly attributable to red-light running. • There was an increase in rear-end crashes, a decrease in angle crashes, a net decrease in red-light running injury crashes, and an increase in total injury crashes. • Photo enforcement resulted in a net improvement in safety, if the severity of the eliminated red-light running crashes was greater than that of the induced rear-end crashes. More detailed analysis of the injury crashes is needed.

Reference	Synectics Transportation Consultants, Inc. (2003). *Evaluation of the red light camera enforcement pilot project*. Ontario, Canada: Ontario Ministry of Transportation.
Study Objective	Purpose of pilot study was to conduct a before and after study to assess the combined effect of two red light running countermeasures for intersections with a high incidence of red light running related collisions: use of RLC systems and stepped-up police enforcement. The study included both a safety evaluation and benefit/cost analysis.
Study Location	Six municipalities in the province of Ontario, Canada: • Toronto • Hamilton • Ottawa • Hamilton • Peel • Waterloo
Study Period	Before – 1995 to 1999 After - November 2000 to November 2002
Description of Treatment/Comparison Sites	95 sites evenly distributed across the municipalities were chosen because they had the highest number of collisions normally associated with red light running. Sites with the highest collision counts were assigned a red light camera and the remaining were selected to have stepped up police enforcement. 18 red light cameras were rotated among 68 intersections, and 27 intersections were assigned stepped-up police enforcement for 20 hours in the before period and 20 hours in the after period. 48 sites were selected from the 95 sites for the evaluation. Sites were one of three types: • 19 red light camera sites • 17 stepped-up police enforcement sites • 12 local comparison sites (to evaluate spillover effects of RLC and stepped up police enforcement) The 48 sites all had 4 legs, and 44 had 2-way traffic on all approaches. They were matched in terms of the following factors: • Traffic control – fixed time or actuated, duration of amber and red phase, cycle time. • Geometry – number and type of lanes. • Operations – volume, degree of saturation, presence of large vehicles. • Speed – posted speed limit. • Visibility – type, number, and placement of signal heads.

Reference (Cont'd)	Synectics Transportation Consultants, Inc. (2003). *Evaluation of the red light camera enforcement pilot project*. Ontario, Canada: Ontario Ministry of Transportation.
Research Design & Statistics	Collision history from 179 signalized intersections (48 in the evaluation study, 121 in the evaluation study area, and 10 located outside of the study area) was used to develop safety performance functions (SPFs) for the 6 collision outcome measures (fatal and injury collisions; PDO collisions; fatal and injury angle collisions; PDO angle collisions; fatal and injury rear-end collisions; PDO rear-end collisions). EB Method was used for deriving estimates of the overall effectiveness of the two treatments (together). Crash data were available for the evaluation study locations during the before and after periods of treatment implementation. A comparison between the expected number of crashes if the treatment had not been implemented (EB estimator) and the actual (observed) number of crashes that actually occurred with the treatments implemented provided the basis for the safety effectiveness of the two treatments assessed at each of the 48 sites for the 2 years.
Other Interventions	• Stepped-up police enforcement • Publicity Campaign ("It Won't Kill You To Stop – Don't Run a Red")
Outcome Measures	**For Safety Evaluation:** Crash type • All crash types combined • Angle crashes • Rear-end crashes Injury Severity • Fatal and injurious crashes • Property damage only
Exposure Data	Yearly collision frequency and AADT on both intersecting roads

Reference (Cont'd)	Synectics Transportation Consultants, Inc. (2003). *Evaluation of the red light camera enforcement pilot project*. Ontario, Canada: Ontario Ministry of Transportation.
Research Design Shortcomings, Confounding Variables, and Other Issues	The evaluation study was not able to make any claim to the effectiveness of the RLC on their own. It was impossible to isolate the effect of the red light cameras from the stepped-up enforcement deployments and the publicity campaign. The results of this study represented the combined effect of the treatments.
Major Findings	Note: Observed and expected crashes were compared in this study combining the 2 treatments and the local comparison group performance, so all 48 sites were included as a "RLC treatment". <u>Across crash types</u>: RLC treatments contributed to a 6.8% decrease in fatal and injury crashes. RLC treatments contributed to an 18.5% increase in PDO crashes. <u>For angle collisions</u>: RLC treatments contributed to a 25.3% decrease in fatal and injury angle crashes. RLC treatments contributed to a 17.9% decrease in PDO angle crashes. <u>For rear-end collisions</u>: RLC treatments contributed to a 4.9% increase in fatal and injury rear-end crashes. RLC treatments contributed to a 49.9% increase in a PDO rear-end crashes. Total net cost benefits associated with the 2 years of the project at 48 evaluation sites showed: Total net benefits $1,613,766 * Total net costs $1,026,805 Benefit-to-cost ratio 1.57 * The $3,775,425 in fatal and injury collisions avoided is offset by a gain in property damage only collisions of $2,161,659, yielding a total net benefit of $1,613,766.
Conclusions	The RLC project was judged to be economically viable, given that the societal cost of collisions avoided exceeds the amount invested in the treatments at the 48 evaluation sites. The EB analysis shows that an estimated 47 fatal and injury crashes were avoided as a result of the treatments, valued at $3,775,425. The study showed to be an effective tool in reducing fatal and injury crashes, thereby preventing injuries and saving lives. Ontario will continue to monitor and examine on a yearly basis to validate that the trend continues to be cost-effective.

Reference	Washington, S., & Shin, K. (2005). *The impact of red light cameras (automated enforcement) on safety in Arizona* (Final Report 550). Arizona Department of Transportation.
Study Objective	To compare and contrast (1) the impact of RLCs on the safety at approaches with installed cameras; and (2) the impact of the RLC on safety at all approaches, testing for spillover effect of the RLCs on non-camera approaches. Four different methodologies were employed to cope with the technical challenges and to assess the sensitivity of results to analytical assumptions.
Study Location	Phoenix, Arizona Scottsdale, Arizona
Study Period	Phoenix – October 1998 to September 2003 Scottsdale – January 1991 to December 2003
Description of Treatment/Comparison Sites	Scottsdale 14 camera intersections • Six working (treatment) • Nine "dummy"/non operational/de-activated, used in analysis of spillover effects. Phoenix 11 treatment sites and 14 comparison sites. Comparison sites sampled from intersections with the highest number of intersection-related crashes Treatment sites in both cities selected because they are high crash locations with a history of RLR crashes, and for city-wide coverage.
Research Design & Statistics	Four methods used in the research design and analysis Simple or Naïve Before/After Study with Ratio of Durations Before/After Study with Correction for Traffic Flow Before/After Study with Comparison Group EB Before/After Study
Other Interventions	• Warning signs placed on target approaches at two intersections in Scottsdale and all approaches at RLC intersections in Phoenix. • Level of public program high in the old system and limited in the new system in Scottsdale, and medium in Phoenix.
Outcome Measures	Crashes, Types, all approaches and target approaches Angle Left turn Rear-end Total
Exposure Data	AADT

Reference (Cont'd)	Washington, S., & Shin, K. (2005). *The impact of red light cameras (automated enforcement) on safety in Arizona* (Final Report 550). Arizona Department of Transportation.
Research Design Shortcomings, Confounding Variables, and Other Issues	
Major Findings	The discussion of the four evaluation methods in the report makes it clear that, on conceptual and theoretical grounds, the EB approach is the most defensible of the four approaches. Only the EB approach accounts for regression to the mean, and it involves corrections for traffic and other factors. Estimation results for Scottsdale are summarized using EB before-and-after study, while the results for of the comparison group method are used for Phoenix (the EB was not available for Phoenix, and the comparison group method is an improvement over the naive approach). <u>Phoenix – target approaches (approaches equipped with RLC)</u> • Angle crashes – 42% decrease • Left-turn crashes – 10% decrease • Rear-end crashes 51% increase • Estimated net crash benefit is $4,504/year for the 10 target approaches <u>Phoenix – all approaches (approaches with and without cameras)</u> • Angle crashes – 14% decrease • Left-turn crashes – 1% decrease • Rear-end crashes – 20% increase • Estimated net crash cost is $324,836/year for the 10 intersection approaches (more costs than benefits) <u>Scottsdale – target approaches (approaches equipped with RLC)</u> • Angle crashes – 20% decrease • Left-turn crashes – 45% decrease • Rear-end crashes - 41% increase • Estimated net crash benefit is $684,134/year for the 14 target approaches <u>Scottsdale – all approaches (approaches with and without cameras)</u> • Angle crashes – 17% decrease • Left-turn crashes – 40% decrease • Rear-end crashes – 45% increase • Estimated net crash benefit is $836,460/year for the 14 intersection approaches Summary of RLC effects on safety: **Phoenix** • Angle and left-turn crashes are reduced, and rear end crashes increase. • Increases in angle/left turn and decreases in rear-end crashes on target approaches are significantly greater than those on all approaches, indicating that spillover effects were not observed. • The net crash benefit is relatively small because the RLCs in Phoenix contribute more to reducing angle crashes and left-turn PDO crashes than to reducing fatalities and injuries associated with these crashes. **Scottsdale** • Angle and left-turn crashes are reduced and rear-end crashes increase. • The magnitude of reduction or increase for each crash type on target approaches are slightly greater than those on all approaches, indicating the spillover effects are present, but relatively smaller than the effect on target approaches. • The crash net benefit is relatively large because the RLCs in Scottsdale contribute more to reducing the costs of fatality and injury crashes associated with angle and left-turn crashes than to increasing the costs associated with PDO rear-end crashes.

Reference (Cont'd)	Washington, S., & Shin, K. (2005). *The impact of red light cameras (automated enforcement) on safety in Arizona* (Final Report 550). Arizona Department of Transportation.
Conclusions	• Examination of crash frequencies alone is not sufficient to understand the impact of RLCs. RLC installation affects severity of crashes, which is an important consideration in the adoption and/or implementation of such programs. • RLCs appear to systematically reduce the frequency of angle and left-turn crashes at intersections. This reduction results from fewer drivers entering the intersection on the red indication and collision with perpendicular traffic. • The frequency of rear-end crashes increase at RLCs intersections, presumably due to a relatively larger number of drivers braking suddenly to avoid a possible violation and fine. • The extent to which severity is reduced for angle and left-turn crashes determines whether the RLC program yields a net positive benefit. Increases in rear-end crashes as a result of RLCs tend to yield increases in property damage only crashes, and thus do not significantly impact the economic analysis. • Analysis of differences across intersections and jurisdictions shed some preliminary insights into RLC effectiveness at individual intersections. Observed general trends, some statistical results from this research, and engineering logic suggest that high approach speeds and left-turn phasing are important considerations when installing RLCs. High approach speeds lead to more severe crashes, and when reduced by RLCs, lead to more significant benefits of the RLC program. Lagging left-turn phasing seems to be more significantly impacted by RLCs with respect to left-turn crashes, which tend to be relatively severe. When RLCs are installed at intersections with leading left-turn phasing in contrast, angle crashes tend to be reduced more significantly (compared to left-turn crashes). [Left-turn related crashes are more likely to be reduced (as a result of RLCs) in the lagging-phase condition, whereas angle crashes are more likely to be reduced in the leading left-turn phase condition.] • Warning signs benefit a RLC program, in that drivers are less likely to run a red light and get involved in a subsequent angle or left-turn crash (e.g., avoiding both types of crashes increases the net benefit of the RLC program). • Engineering countermeasures (other than RLCs) may be considered to deal with RLR problems. It may be prudent to exhaust simpler, and less costly engineering countermeasures to combat a red light running problem prior to adopting a RLC program, particularly when some of the previous "ideal" conditions do not exist.

APPENDIX B: BIBLIOGRAPHY OF SCREENED AUTOMATED SPEED ENFORCEMENT PAPERS NOT MEETING DETAILED REVIEW CRITERIA

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