Safety Effects of Automated Speed Enforcement Programs

Critical Review of International Literature

Libby J. Thomas, Raghavan Srinivasan, Lawrence E. Decina, and Loren Staplin

Automated speed enforcement programs were evaluated worldwide to ascertain the effectiveness of such programs at achieving safety benefits. Unlike previous reviews on this topic, a critical review process was used to determine the most likely range of probable safety effects of fixed and mobile automated speed enforcement programs. Among the 90 studies from 16 countries that were initially identified as potential safety evaluation studies, 13 met the criteria for detailed methodological review. On the basis of evidence from the best-controlled evaluation studies, injury crash reductions in the range of 20% to 25% appear to be a reasonable estimate of site-specific safety benefit from conspicuous, fixed-camera, automated speed enforcement programs. No conclusions were reached regarding site-specific effects of mobile enforcement programs. Estimates of systemwide crash reductions likely attributable to covert, mobile speed enforcement programs were based on different subsets of crashes (daytime casualty crashes and daytime speed-related crashes) and were limited to two studies, but also were in the range of 20% to 25%.

The detrimental effects of excessive speed on traffic safety are 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. National Highway Traffic Safety Administration (NHTSA) estimates that speeding was a contributing factor in 31% of 2006 fatal crashes in the United States, resulting in 13,543 lives lost (*1*). Several prior reviews have discussed the evidence for crash severity and crash frequency associations with excessive speed and speed variance (*2–4*).

Research also confirms that many drivers do not comply with established speed limits (*5–7*). When drivers make inappropriate speed choices that result in crashes, the human, social, and economic costs are considerable. Pedestrians and bicyclists are particularly vulnerable to increasingly severe injuries with incrementally higher speeds.

The goal of automated enforcement is to complement conventional law enforcement by significantly increasing the objective and perceived chances of being caught, thereby creating a reduction in speeding that will lead to crash reductions. Automated enforcement systems incorporating cameras and speed-measuring devices have been widely applied to aid speed enforcement efforts in more than a dozen countries since the early 1990s. Use of automated speed enforcement is likely more extensive; the Insurance Institute for Highway Safety estimated in 1999 that "about 75 countries rely on cameras to enforce speed limits" (*8*). Automated speed enforcement has thus far been slower to gain acceptance and widespread use in the United States. A number of concerns have arisen, among them the issue of whether the technology improves safety or is primarily a revenue-producing tool. Other issues such as constitutional and privacy concerns have largely been resolved through case law, but the legal environment and public opinion remain important factors, both in the United States and abroad, in establishing effective and sustainable automated enforcement programs.

The intent of the current study was to examine the evidence from around the world as to the effectiveness of automated speed enforcement at improving safety. Therefore, only evaluation studies that examined safety outcomes in the form of crashes or injuries were included in this review. The best quality studies were given the most weight in determining probable ranges of effectiveness of automated speed enforcement programs.

Two recently published systematic reviews of automated enforcement and speed enforcement were identified (*9, 10*). Pilkington and Kinra reviewed 14 studies of safety effects of all quality levels, with the objective of determining whether speed cameras reduce road traffic collisions and injuries (*9*). Seven of the 14 studies were reviewed in the current study (*11–17*) in addition to an interim report (*18*) to one reviewed here (*19*). Studies were given numerical quality ratings on the basis of the sum of their scores on individual measures.

Wilson et al. reviewed 26 papers that evaluated speed enforcement using any type of speed detection device including speed cameras, radar, and laser detection (*10*). Thus, this review included both automated and nonautomated (as the term is used in the current paper) enforcement programs. Studies included those comparing various types of speed enforcement treatments, not just introductions of the enforcement. Nine of the studies reviewed were included in the current study (*11–17, 19, 20*). Descriptive study quality ratings (fair, poor, and good) were determined on the basis of the number of quality criteria met.

Pilkington and Kinra concluded that although the evidence is weak, the research consistently shows that speed cameras are an effective intervention in improving road safety (*9*). Wilson et al. similarly concluded that because of the consistency of reported speed and crash reductions, speed enforcement detection devices are effective at reducing traffic crashes and injuries (*10*). Although both reviews provided quality assessments of the reviewed studies, neither attempted to provide guidance in determining the range of effectiveness of

L. J. Thomas and R. Srinivasan, Highway Safety Research Center, University of North Carolina, 730 Martin Luther King, Jr. Boulevard, Suite 300, Chapel Hill, NC, 27599-3430. L. E. Decina and L. Staplin, TransAnalytics, LLC, 1722 Sumneytown Pike, Box 328, Kulpsville, PA 19443. Corresponding author: L. Thomas, Libby_Thomas@unc.edu.

Transportation Research Record: Journal of the Transportation Research Board, No. 2078, Transportation Research Board of the National Academies, Washington, D.C., 2008, pp. 117–126.
DOI: 10.3141/2078-16

various types of automated speed enforcement programs (*9*) or of speed enforcement detection devices in general (*10*) for reducing crashes and injuries.

In the current paper, probable ranges of crash reduction benefits of introducing automated speed enforcement (involving image capture) were determined by considering study quality factors and type of automated speed enforcement program. In addition, several recently published papers that were not included in the prior reviews, among them one of the best controlled studies, lend support to the estimates provided herein.

DESCRIPTION OF AUTOMATED SPEED ENFORCEMENT

Automated speed enforcement generally falls into two broad types: fixed camera and mobile systems. Fixed cameras are typically mounted in boxes at fixed locations, similar to red-light camera installations at intersections, and can continually monitor traffic speeds without a human operator if digitally connected to an electronic system. If local data storage or wet film is used, systems may not be operating at all times, but this fact may not be obvious to the traveling public. Speeds are typically measured using Doppler or laser radar systems.

Mobile camera operations may be deployed in police vehicles, marked or unmarked, and are usually tended by enforcement officers or other trained officials. Some programs also operate mobile deployments mounted on tripods by the side of the road. Mobile deployments may be rotated among sites, so enforcement is not typically continuous at any one location. There is wide variation among countries and jurisdictions in the division between covert and overt mobile operations. Some mobile operations appear nearly as overt as fixed deployments in space if not in time, with signed enforcement zones and marked deployment vehicles. Other jurisdictions have used unmarked and hidden vehicles and unsigned enforcement zones or more general warning signs in efforts to create a more generalized deterrent effect. Public information and awareness campaigns typically accompany the enforcement efforts.

A third type of automated speed enforcement, less frequently used at this time, is speed-over-distance systems that photograph vehicles at both start and end points and determine whether an infraction has occurred based on the calculated average speed. This type of deployment may be used most often where speeding and speed-related crashes are a problem over some distance and may be perceived as fairer because speeds are not determined at a single point location. The type of deployment mode has a number of ramifications for the design and thoughtful conduct of safety evaluation studies, in addition to the deterrence or safety objectives.

EVALUATIONS OF ROAD SAFETY CHANGES RELATED TO AUTOMATED SPEED ENFORCEMENT

Most evaluations of automated speed enforcement systems to date have been performed in countries other than the United States. The science of evaluating road safety treatments is ever evolving but remains largely dependent on opportunistic situations. As a result, most studies are post hoc before-and-after evaluations of treatments implemented at problem (high crash or speeding) locations. In conducting such evaluations, considerations include accuracy and consistency of data measures over time and location; the need to control for traffic volume or other exposure measures and the nonlinear relationship between traffic volumes and crash frequencies; confounding enforcement or other countermeasures; other unknown or unmeasured confounding effects; and regression toward the mean (*21*). Crash severity also should be considered to ensure that the treatment is not having counteractive effects, whereby reductions in one crash type are offset by increases in different types, or where a reduced crash frequency is accompanied by an increased severity of crashes.

Regression toward the mean (RTM) is typically a significant effect when high-crash sites are selected for treatment, as appears to be the case in many of the studies reviewed here. RTM describes the statistical tendency for high-crash trends to decrease toward the mean in subsequent time periods independent of any treatment (*15, 21, 22*). If not controlled for, RTM may explain a significant portion of observed changes, possibly resulting in an overstatement of safety improvement attributed to the treatment. Hauer et al. (*23*) argued that the empirical Bayes method (EB), should be the standard of professional practice to address the RTM effect in before-and-after safety studies, as well as to increase the precision of the estimates developed. The EB method includes the use of reference groups (which ideally should be similar to the treatment group but unaffected by the treatment) to account for RTM bias and the development of yearly factors in the regression modeling to account for general time-related trends and unknown effects.

In evaluating treatments that are applied to high-crash locations or "black spots," possible spillover effects to untreated locations must be assessed through the careful selection and analysis of comparison groups. Both positive and negative spillover might occur. Positive spillover may be an issue in widely publicized automated speed enforcement programs, especially if there is a perception of widespread enforcement. Even with fixed camera programs, the traveling public may be unsure of exact camera locations. In the case of fixed or conspicuous mobile automated speed enforcement, possible unintended negative consequences such as crash migration from treated to nontreated sites could also occur. 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 is sometimes characterized as the "kangaroo" effect (*15*). It is also possible that motorists may choose alternate routes to avoid treated locations, contributing to an increase in crashes at alternate sites and a decrease in crashes at the camera sites (*24*). Some jurisdictions have widely used both fixed and mobile systems, so spillover from one area to the other may occur, making the selection of comparison groups especially problematic.

A final issue in safety treatment evaluation studies is establishing the mechanism, or causal link, by which the treatment is hypothesized to reduce crashes (*3*). In the case of automated speed enforcement, the objective is to reduce speeding—and perhaps speed dispersion—and consequently crashes associated with excessive or inappropriate speeds for conditions. Ideally, then, speed effects as well as crash effects should be determined in evaluation studies, lending support for any safety benefits attributed to the treatment.

This review of evaluations of automated speed enforcement programs used the issues previously mentioned to develop study evaluation criteria. These criteria were then used to assess study quality and draw conclusions regarding the effectiveness of fixed and mobile automated speed enforcement programs at improving safety.

METHODS

To identify studies, four sources were used: (*a*) electronic subject databases, (*b*) electronic technical library databases, (*c*) Internet sources including government and international research agency

Web sites, and (*d*) professional associations. English-language studies completed before September 2005 were acquired and reviewed to determine whether they did in fact describe an evaluation of an automated speed enforcement program that included safety-related outcome measures (crashes or injuries) and provided detailed descriptions of study methods and detailed results, rather than a summary only.

Although studies reporting effects on speed without crash or injury outcomes were not of interest in the current study, this measure—treatment effects on speed—was a criterion for crash-related studies to be included in this review, along with other criteria described later. On the basis of the issues raised previously, an a priori methodological review of automated speed enforcement studies was developed to include assessment of the following study characteristics:

1. 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)?
2. Did the study account for crash severity?
3. Did the study methods and analysis control for or account for changes in traffic volumes before and after the implementation?
4. Did the study design and analysis account for possible time trend effects (e.g., general trends in crashes, seasonal changes, or changes in the motoring population, vehicle fleet, weather)?
5. Did the study account for other possible confounding factors such as concurrent treatments or enforcement, changes in data measures (such as reporting thresholds), or other factors that may overlap with before and after periods?
6. Did the study examine possible crash migration caused by the treatment, either to nonenforced sections of the same roadways or to nonenforced alternate roads?
7. Did the study account for RTM?

Among the 90 studies from 16 countries that were initially identified as potential evaluation studies of safety effects, 39 proved to be evaluation studies available in English. Of the 39 studies, 13 English language studies were identified for detailed methodological review. The remaining 26 studies were eliminated for one of the following reasons:

- Crash effects were not reported.
- Study or analysis methods were not available.
- Introduction of an automated enforcement program was not evaluated in some studies; examples are studies that compared other treatments with automated speed enforcement or combinations of treatments.
- Later versions using the same data or more complete data and analysis techniques were available for some of the evaluations.

DISCUSSION OF STUDY RESULTS

Table 1 provides an overview of the 13 studies from seven countries included in this review. Four studies reported on fixed camera enforcement programs, whereas 8 studies evaluated mobile enforcement programs. One study was of a comprehensive program involving multiple types of automated enforcement. All of the identified studies were before-and-after assessments. Two studies reported on systemwide effects using time-series models; the remainder studied aggregate crash effects at treatment sites of varying description. Other program parameters, such as treatment and sample area size or length, road types, speed limits, enforcement thresholds, penalties, publicity, and enforcement conspicuity and intensity, also varied. Some of these parameters were not described in detail in many of the papers. A variety of crash and injury outcome measures were used, with some papers reporting results for several measures. All of the studies provided aggregate results across study sites. Some provided additional detailed analyses by location type, road type, user type, or speed limit, which are not discussed in the current review.

All of the studies estimated significant reductions in outcome measures of crashes or injuries at camera sites or systemwide after-program implementations. Because fixed and mobile deployments differ in ways that may affect outcomes and, possibly, driver behavioral adaptations with implications for study design and analysis, results were assessed separately for these two general types of automated speed enforcement. Tables 2 through 4 provide summaries of treatment and comparison groups, study limitations (based on review of the study quality factors), and key reported significant crash outcomes, respectively. Seven of the studies measured effects on

TABLE 1 Reviewed Studies, Locales, and General Deployment Type in Chronological Order

Reference	Location of Intervention	General Location Types	Type of Deployment	Measured Speed Effects
11, Cameron et al., 1992	Victoria, Australia	Rural and urban	Mobile	No
15, Elvik, 1997	Norway	Rural and urban	Fixed	No
12, Chen et al., 2000	British Columbia, Canada	Rural and urban	Mobile	Yes
17, Tay, 2000	Christchurch, New Zealand	Urban	Mobile	Summary trends
13, Chen et al., 2002	British Columbia	Rural	Mobile	Yes
14, Christie et al., 2003	South Wales, United Kingdom	Rural and urban	Mobile	No
20, Newstead and Cameron, 2003	Queensland, Australia	Rural and urban	Mobile	No
19, Gains et al., 2004	United Kingdom	Rural and urban	Fixed, mobile, and speed over distance	Yes
16, Hess, 2004	Cambridgeshire, United Kingdom	Rural and urban	Fixed	No
24, Mountain et al., 2004	Great Britain, United Kingdom	Rural and urban	Fixed	Yes
26, ARRB group, 2005	New South Wales, Australia	Rural and urban	Fixed	Yes
28, Cunningham et al., 2005	Charlotte, North Carolina, United States	Urban	Mobile	Yes
27, Goldenbeld and van Schagen, 2005	Friesland Province, Netherlands	Rural	Mobile	Yes

TABLE 2 Fixed, Conspicuous, Automated Speed Enforcement: Study Overviews and Significant Results

Treatment and Comparison	Study Period and Limitations	Key Reported Outcomes
26, ARRB Group, 2005		
28 camera sites of 111 implemented; selected from high crash rate or high crash severity locations to represent the range of treated environments. 17 local government areas served as comparison group for 14 sites. 13 sections matched for roadway characteristics, used as comparison for 10 sites.	Before = 3 years; after = 2 years Matched control sites may account for general traffic volume effects (if trends the same); before/after volumes not considered. No discussion of other treatments or potential confounders; may have been controlled using comparison groups. Crash migration due to traffic flow changes not considered. RTM not controlled.	At camera lengths (1 to 3.3 km) –19.7% (p = .0001) all crashes –20.1% (p = .0164) injury crashes –22.8% (p = .0051) casualty (inj. + fatal) crashes –89.8% (–22.1, –98.7)[a] fatal crashes –16.9% (p = .0116) non-injury crashes Crash increases at some adjacent sites to camera lengths; crash reductions achieved over combined camera and adjacent lengths were in the range of 6–8%, except for fatal crashes (~58%) and nonsignificant.
15, Elvik, 1997		
64 sections on variety of roads/speed limits. Later sections added to the study had to meet both higher than normal accident density and higher than normal accident rate warrants for the road type, and have mean speeds above the posted speed limit. Earlier sections did not necessarily meet all of these requirements. Comparison using EB procedures and county crashes for each location to account for general trends and volumes and RTM.	Before = 3.94 years (avg.); after = 4.61 years (avg.) Effects on speed not determined. Apparently, no direct accounting for before or after traffic volume changes. Expected crashes may also have been estimated assuming a linear relationship with vehicle miles traveled. To adjust for time trends, used the ratio of comparison group crashes in after period to comparison group crashes in before period. More recent studies have estimated annual factors for each year in the study period to more accurately account for time trends. No discussion of other potential confounders but may have been controlled with procedures used. Crash migration not examined.	Average segments of 5.2 km: –20% (–26%, –13%) injury crashes Sections conforming to crash rate and crash density warrants: –26% (–36%, –16%) injury crashes Sections not conforming to either warrant: –5% (–28%, +24%) injury crashes Data for only one section: –12% (–38%, +26%) property damage only crashes
16, Hess, 2004		
49 sites on rural trunk roads and urban roads; major (A roads) and minor roads (non-A); speed limits not described. Time-dependent coefficients were derived using all crashes in the county (including at camera sites). These were then used to remove time-dependent components including RTM, trend, and seasonality.	Before = varied; after = varied, minimum 1 year. Total study period 13 years Effects on speed not determined. Fatal and injury crashes considered by developing weights based on likelihood for each crash injury level (except no injury). No explicit examination of traffic volume. Crash migration due to traffic flow changes not considered. Time-dependent coefficients were intended to control for factors that have regional effects. Unclear whether time-dependent coefficients sufficiently account for regression to the mean.	Area within 250 m (if linked by road) of camera sites: –45.7% weighted injury crashes Area within 500 m of camera sites: –41.3% weighted injury crashes; effects higher on major roads and trunk roads Area within 1,000 m: –31.6% weighted injury crashes Area within 2,000 m: –20.9% weighted injury crashes (Confidence intervals/significance levels not reported.)
24, Mountain et al., 2004		
62 sites on 48 km/h (30 mph) roads with reported severe speeding problems throughout country. Comparison using EB procedures and comparison group of national crashes and traffic flows used to account for general trends and traffic flow changes.	Before = 3 years; after = 2.3 years (avg.) Other potential confounders may have been controlled using study methods–national crashes as comparison group. However, treatment spillover or effects of other speed camera programs may have affected estimates of effects. Time trend adjustments were used to compensate for using older safety performance functions in EB procedures.	Within 500 m either direction –25% (–35%, –14%) injury crashes –20% attributed to speed/behavior changes, and –5% attributed to traffic diversion Within 1 km either direction –24% (–33%, –13%) injury crashes –19% attributed to changes in speed –5% to traffic diversion.

[a]95% confidence intervals.

TABLE 3 Mobile, Overt, Automated Speed Enforcement: Study Reviews and Significant Results

Treatment and Comparison	Study Period and Limitations	Key Reported Outcomes
Signed Treatment Zones and Conspicuous Vehicles *28*, Cunningham et al., 2005		
14, 35–50 mph (56–80 km/h) high volume, urban corridors. Comparison group of 11 corridors within the City (lower volume but similar crash trends).	Before = 4 years; after = 4 months; short after period—consider results preliminary. Severity reporting changes during the study; separate analyses by severity confounded. Before/after traffic volume not explicitly considered. Crash migration not considered. RTM not explicitly controlled—some analyses to examine possible RTM effects.	Corridorwide: –12% +/– 4% all crashes
20, Newstead and Cameron, 2003		
1,500 high crash zones throughout state. Zones could include multiple marked camera sites; defined as high crash area within 6-km boundary (polygon). Comparison group of sites outside the 6 km boundaries intended to reflect effects of other enforcement programs (started before the speed camera program) and general trends.	Before = 5 years; after = 4.5 years Effects on speed not determined. No explicit control for traffic volume. Long-term trends accounted for. Seasonality was not considered. No examination of crash migration. RTM not directly controlled, but relatively long study periods may reduce RTM effect.	Within 2-km area –17.5% ($p < .0001$) all severity crashes –15.6% ($p = .0002$) fatal & medically treated crashes –21.9 ($p = .0001$) hospitalization crashes –20.3% ($p < .0001$) no-injury crashes Also reported significant crash reductions in various categories within 2 to 4 km, and 4 to 6 km. Significant effects of program intensity (number of zones and hours of operation per site on all severity crash reductions)
Signs at or Near Deployment Sites *14*, Christie et al., 2003		
101 sites; majority on 30 mph (48.3 km/h) roads; about 1/4th on higher speed roads. Matched comparison sites from neighboring police enforcement district.	B = 38 months (avg.); A = 17 months (avg.) Effects on speed not determined. Effects on severity not examined. Traffic volume data not available. Unclear if trends in crashes were same for treatment and comparison group. Cameras introduced in the region prior to the study. Data precision (crash location) improved in the after period. Crash migration not considered. RTM not controlled.	Within 500 m either direction –51% injury crashes [0.49 rate ratio (0.42, 0.57)[a]] all roads –51% injury crashes [49 rate ratio (0.42, 0.58), 30 mph (48.3 kph)] roads –59% injury crashes [0.41 rate ratio (0.27, 0.62), 60–70 mph (96.6–112.7 kph)] roads
27, Goldenbeld and van Schagen, 2005		
28 segments on 80 and 100 kph (50–62 mph), rural single carriageway roads. Comparison group included all other rural roads in the province, approximately 5,200 km total length.	Before = 8 years; after = 5 years Traffic volumes were substantially lower on comparison roads and not explicitly considered in analyses. Possible confounding from engineering measures implemented during study period. Possible confounding due to spillover effects on unenforced segments (extensive media campaign). Crash migration considered only indirectly. RTM not controlled, but long study period may mitigate RTM effect.	Average of 4.1-km segments –21% injury crashes [0.79 odds ratio (0.66, 0.95, 95% CI)] –21% serious traffic casualties [0.79 odds ratio (0.63, 0.99 95% CI)]
17, Tay, 2000		
24 sites in city of Christchurch. Cameras increased from 3 to 24 during the study period. Comparison group included all nonspeed camera zones in the city.	Total before/after study period 1993–1995; B and A periods not described but varied by site. Only citywide mean and 85th percentile speed trends were described. Traffic volume not explicitly considered. Crash migration not considered. RTM not controlled.	–9.2% +/– 5.9% reduction in all crashes (treatment length not reported) –32.3% +/– 12.5% reduction in serious injury crashes

[a]95% confidence intervals.

TABLE 4 Mobile, Covert, Automated Speed Enforcement: Study Reviews and Significant Results

Treatment and Comparison	Study Period and Limitations	Key Reported Outcomes
13, Chen et al, 2002		
12 radar locations along a single 22-km segment of an 80 to 90 km/h (50 or 56 mph) rural, divided highway. Comparison group of 3 police jurisdictions in study area.	Before = 2 years; after = 2 years Effects on varying severity of crashes not determined. Unclear if before/after traffic volumes explicitly considered. Comparison group may have been affected by province-wide program and publicity. Traffic flow and crash migration to other routes deemed unlikely since no alternate routes. Empirical Bayes methods used to control for RTM, general volume effects.	Corridorwide: –16% +/– 7% all (police-reported) crashes At treated locations (within 1 km either direction): –14% +/– 11% all crashes At non-treated inter-leaving sites along corridor (> 1 km from camera sites): –19% +/– 10% all crashes (not significantly different from treated segments)
Systemwide Effects *11*, Cameron et al, 1992		
54 cameras used on 60 and 100 km/h (37 and 62 mph) urban (Melbourne, 70%) and rural (30%), mostly arterial, roads. Comparison group: Comparable areas from neighboring state (NSW). (Cameras introduced into comparison areas during after period; analyses took this into account.) Time series models controlled for seasonal and time trend.	Before = 7 years; after = 18 months (full implementation) Effects on speed not determined. Economic indicators used to control for exposure. Daytime (low alcohol hour) crashes used to reduce confounding with alcohol-related program. Program was covert and widely publicized so traffic diversion and crash migration not considered likely; examination not possible with system-wide analysis. RTM not likely a factor due to using system-wide crashes and long before period.	System- (state)-wide (full intervention period, before cameras introduced into NSW) –20.9% (–27.9%, –13.3%)[a] daytime casualty crashes (injury and fatality), –27.9% in crash severity (ratio of fatal + serious injury–minor injury crashes) Citywide (Melbourne): –21.1% (–28.9%, –12.4%) daytime casualty crashes Rural areas (Victoria): –19.5% (–27.5%, –10.7%) daytime casualty crashes
12, Chen et al., 2000		
Provincewide deployment of 30 cameras operated primarily during daytime at high crash sites or sites with perceived speeding problem. No comparison group. Time series models to control for seasonal and time-trend effects.	Before = 5 years; after = 1 year (following 5-months phase-in) Gasoline sales used as proxy covariate to account for exposure (in lieu of traffic volume). Use of daytime-only unsafe speed-related collisions to reduce confounding with alcohol interventions introduced during the study period. No comparison group to account for other/unknown trends. Program was covert and widely publicized so traffic diversion and crash migration not considered likely; examination not possible with systemwide analysis. RTM not likely a factor due to using systemwide crashes.	System (province) wide –25% daytime unsafe speed-related crashes (significance level presumed $p = .05$) –11% day-time traffic collision injured carried by ambulance (sign. level presumed $p = .05$) –17% daytime traffic collision fatalities ($p = .10$, one tailed)

[a]95% confidence intervals.

driving speeds as well as crashes [see Decina et al. (*25*) for detailed speed results].

Fixed, Conspicuous Enforcement

Results of the critical review and key study results for the four evaluations of fixed speed camera enforcement at the targeted enforcement areas are shown in Table 2. Three of these studies used comparison groups in estimating procedures to account for volume and general trends (*15, 24, 26*); Mountain et al. (2004) and Elvik (1997) also controlled for RTM using EB procedures (*24, 15*). Two of the three studies documented reductions in speeds and percentage exceeding limits (results not shown; *24, 26*). A variety of road types and speed limits were represented. Each of these studies reported aggregate injury crash reductions in the range of 20% to 25% for treatment lengths of 1 km up to 5.2 km, whereas the fourth study reported very different results (*16*).

The fourth study provided estimates for weighted (by severity) injury crash reductions that were greater than those reported previously for lengths of 1 km centered on camera sites (*16*). To account for trend, seasonality, and RTM effects, multiplicative coefficients were computed, rather than using EB methods. It is unclear whether the time-dependent coefficients sufficiently accounted for RTM and how the weights for crash severity may have affected results reported. The estimates are not directly comparable to those for varying severity levels from other studies.

Only one study reported significant reductions when fatal crashes were examined independently (*26*), but sample sizes of fatal crashes are typically small, decreasing the likelihood of detecting significant

effects. The RTM effect also tends to be high for fatal and serious crashes as in Mountain et al. (*24*), confirming the importance of controlling for this factor to obtain accurate estimates of treatment safety effects.

Only one study of fixed or mobile programs directly controlled for before-and-after traffic flow and determined effects on crashes (*24*). Traffic diversion because of the treatment was estimated to contribute 5% of the overall 25% treatment-related reduction in injury crashes. It is unknown whether traffic diversion contributed to an increase in crashes (crash migration) on the alternate routes. The remaining 20% treatment effect was attributed to desired behavior changes, including speed reductions. According to the authors, there was no evidence of localized crash migration (kangaroo effect) because of sudden braking or changes in speed near the enforced zones.

The Australian Road Research Board reported evidence of a possible kangaroo effect on crashes (*26*). In addition to reporting crash reductions at the treated segments, this group documented increases in speeds and crashes over 2 years at some nonenforced sites adjacent to treated segments. Their results suggest the possibility that driver adaptations to the treatment over time may have contributed to crash migration. The authors argued that the slight increases in crashes were more than offset by decreases along the treated segments, but when camera length and adjacent length results were combined, none of the crash reductions was statistically significant.

None of the studies of fixed camera enforcement discussed the possibility of positive spillover on nearby or comparison groups because of the treatment. There was also little discussion of the details of the comparison groups and whether they may have been affected by extensive use of speed cameras surrounding the study areas in some studies, and the effect this would have on reported outcomes.

Mobile Enforcement

Reported safety estimates from the eight studies of mobile enforcement programs were more variable than those of fixed cameras. This variation may reflect marked differences between programs with respect to factors such as conspicuity and intensity, varying sampling and study methodologies, different outcome measures, and potentially, the larger number of studies.

Conspicuous, Mobile Enforcement

Studies of overt, mobile deployments reported reductions from 21% to 51% in injury crashes (*14, 27*) and of approximately 9% to 18% in all crashes at the treated locations (*17, 20, 28;* Table 3). In addition to differing outcome measures, these results may reflect differences in conspicuity (e.g., signs posted at, versus in the general vicinity, of treated sites; or the use of marked versus unmarked vehicles), enforcement intensity, site differences, and other program features, as well as RTM, traffic volume changes, and other possible confounding and spillover effects.

In the cases of signed, mobile camera enforcement zones and otherwise conspicuous, mobile enforcement, motorists may learn to 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 directly examined the possibility that traffic or crash migration to nonenforced routes or segments accounted for some of the crash reductions. The possibility of positive spillover on untreated or comparison sites also did not appear to be examined by any of the studies.

Inconspicuous, Mobile Enforcement

Only one study of mobile enforcement controlled for RTM as well as general trends. This study of a covert enforcement program found a 16% corridorwide reduction in all crashes (effects similar at interleaving nondeployment and 12 deployment locations) along a single 22-km corridor (*13*, Table 4). Mean speeds were reported to decrease to below the posted limit at deployment locations and by 2 km/h at a single monitoring (nonenforced) location. It is possible, however, that effects of the same widely publicized, automated enforcement program that had been previously reported to have a provincewide effect, may have affected the comparison group and resulted in the treatment effect being underestimated.

Inconspicuous, Mobile Enforcement: Systemwide Effects

A more generalized deterrent effect over a wider area may be a goal of more covert, automated enforcement. One of the earlier studies from Victoria, Australia (*11*), and a more recent study from British Columbia, Canada (*12*), examined systemwide (province or state) effects of covert, mobile enforcement with extensive publicity (Table 4). Cameron et al. reported reductions of approximately 20% in daytime casualty crashes statewide during full program implementation (*11*), and Chen et al. reported reductions of 25% in daytime, unsafe speed-related crashes (*12*). Both studies used daytime-only crashes to reduce confounding with concurrent alcohol-related enforcement programs. Although there may be unknown confounding factors, and it is more difficult to definitively attribute systemwide crash reductions to the programs, both studies used time-trend analyses to account for general crash trends and proxy measures to adjust for overall travel exposure. Relatively long before study periods and the use of state- or provincewide crashes appears to reduce the likelihood of RTM playing a large role in these results. Unfortunately, the studies used different outcome measures, so results cannot be compared. Although effects on traffic speeds were not documented in the Australian study, effects of program intensity were examined. Crashes and crash severity in Melbourne were significantly related to number of traffic infringement notices mailed (systemwide), and crash severity was related to hours of operation, providing more support for a program effect.

Comprehensive Automated Enforcement Program

A large-scale study of the national safety camera program in the United Kingdom evaluated the speed and casualty effects, public acceptance, costs and benefits, and program administration of a combined fixed (including red light), speed-over-distance, and mobile camera enforcement program under the cost-recovery system (*19*). The program was begun with eight counties in 2000 and expanded to 24 partnership areas by April 2002, which was the focus of the evaluation. The program allowed local road safety partnerships to recover the costs of administering automated enforcement programs, subject to strict criteria. Guidelines for the partnerships, management and expenditures, public communication, site selection (includes collision

warrants and review for other engineering solutions), fixed camera conspicuity, and monitoring of results were established.

The authors reported reductions in speeds and reductions in percentages of speeders and excessive speeders across camera sites, with greater reductions at fixed camera installations. Speed-over-distance cameras were, however, most effective at reducing the percentage of drivers at more than 15 mph above the limit. Successive speed measures suggested that fixed sites have a more immediate but sustained impact, with mobile sites taking somewhat longer to achieve the full speed effect. Estimated reductions in personal injury crashes were 33%; numbers killed and seriously injured were reduced by 40% on average, with higher reductions at fixed camera sites and in urban locations. These reported outcomes may reflect not only the influence of camera introductions but also entry of existing camera sites into the cost-recovery program and increases in conspicuity at fixed camera sites that were mandated by the program, as well as RTM and traffic volume effects. Comparison areas, used to account for long-term and seasonal trends, also included nonprogram, automated speed enforcement, so spillover may have occurred in both directions. The authors argued that RTM should not be a factor because number of crashes was not the sole criterion for site selection. It was, however, a primary factor in the cost-recovery program.

RECOMMENDATIONS FOR FUTURE RESEARCH

Future implementations of automated speed enforcement programs should include collaborations between traffic enforcement authorities and researchers to conduct controlled, randomized experiments of the safety effects. Sites with high crash frequencies caused by problem speeding could be randomly assigned to treatment and nontreatment groups, obviating the need for controlling for general trends, RTM, and other confounders. If random assignment is not feasible, EB procedures should be used in before-and-after studies of safety effects at high crash locations to control for RTM and to properly account for changes in traffic volume and other changes over time. Standardized outcome measures would also significantly aid interpretation.

A comparison group that best represents the treatment group crash trends but is unaffected by the treatment being evaluated should be used. If the entire area may be affected by the treatment, as might be expected in widespread and publicized automated speed enforcement programs, the best comparison group may be from the most comparable neighboring area. In addition, negative spillover potential should be assessed to ensure that unintended consequences do not result from the treatment, particularly in studies of overt, fixed, or mobile automated enforcement programs. Council et al. (*29*), in an assessment of red-light camera enforcement using EB procedures, used multiple comparison groups and separate analyses to examine the possibility of spillover effects on the reference group of untreated, signalized intersections. Multiyear, before-treatment crash trends and possible confounding treatments should be examined to determine whether the comparison groups are representative of the study group.

Monitoring of traffic flow, speeds, and crashes on nearby routes should be performed to ensure that the treatment does not adversely affect other areas, whether or not part of the comparison group. Limited evidence from reviewed studies of fixed camera enforcement suggests that negative spillover in the form of crash migration to unenforced segments or traffic diversion and possible crash migration to alternate routes may occur in some situations. Ali et al. (*30*) also found that speeds increased upstream and downstream of camera sites, but they did not examine effects on crashes. Currently, there are no data to indicate whether such undesired driver responses might also occur with deployments of mobile systems of varying conspicuity. Even if covert, the deployment can likely be detected by some motorists and may result in sudden speed changes near enforced sites. There may, however, be less opportunity for behavioral adaptations to covert, randomly allocated mobile enforcement activities to negatively impact crashes at nontreated locations; again, no data were found that address this question.

Many questions remain with regard to both distance and time halo effects of various types or combinations of enforcement and program factors and the relationship between intensity of mobile enforcement and outcomes. It is also unclear how the size, shape, and other characteristics of the sample area itself affect reported outcomes. Although some studies provided some examination, a justification or explanation of the treatment sample area was not always provided. The treatment sample area should be carefully determined and described, and, if possible, a theoretical basis for the expected zone of effect should be provided on the basis of the enforcement and site parameters. Conversely, site factors, enforcement intensity, and program parameters such as timing of citation issuance, publicity, and signing could be incorporated into models of effects for larger studies of automated speed enforcement with variation in such factors.

Although there is a general consensus in the safety community that higher speeds result in more severe crashes, there is less agreement about the relationship between changes in speed and changes in crash frequencies. The current review did not attempt to assess the relationship between reported speed changes and estimated crash reductions, but some research uncovered touched on the subject. Hirst et al. (*31*), reported on analyses of the relationship between speed reductions and crash reductions in a comparison study of engineering changes and automated enforcement treatments. They argued, on the basis of results of models, that each 1 mph speed reduction will result in approximately a 4% reduction in crashes for sites treated with cameras (with speeds expected on 30 mph roads). The relationship between speed reductions and crash reductions varied, however, on the basis of initial speeds and, depending on the countermeasure used, automated speed enforcement, or vertical or horizontal traffic calming measures (*32*). Recently, Elvik (*3*) 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 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 acknowledged limitations in the use of the power model. For example, the power model predicted 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, which is very unlikely to be the case. There is clearly also a need for more work on the relationship between changes in speed and changes in crashes.

CONCLUSIONS

On the basis of the best controlled, existing research in English-language reports, all from highly developed countries, it appears highly likely that automated fixed speed enforcement programs will

result in safety improvements at high-crash locations. The best estimates of injury crash reductions attributable to fixed camera systems fall in the range of 20% to 25% at treated locations. This range may, however, include effects caused by traffic diversion in addition to speed reductions resulting from the treatment. There may also be some shifting of crashes because of a kangaroo effect of speed reductions at enforced zones but increases elsewhere in some situations. Programs should be designed to minimize the potential for undesirable driver behaviors, and evaluations should examine the evidence for negative spillover. Effects on fatal and other severity crashes are less certain but also declined in general. Only time and more research will reveal whether the results of these studies are generalizable to other environments or programs. The similarity of reported effects of fixed cameras for high crash or problem speeding sites across a variety of road types, speed limits, and countries, is promising, although based on a limited number of better controlled studies.

On the basis of the existing evidence, covert, mobile enforcement programs with extensive supporting publicity also appear likely to result in significant systemwide safety improvements. Because of differences in outcome measures, an estimate of the range of expected improvement is less certain, but daytime casualty (injury and fatal) crash reductions reported by a statewide Australian study were around 20%, whereas daytime speed-related collisions were reduced by 25% in a provincewide Canadian study. Again, evidence is less abundant for fatal crashes, but trends were in general downward for all severity of crashes.

A wider range of reported aggregate, site-specific crash reductions is associated with mobile enforcement programs in the reviewed studies. It is not known to what extent various effects estimates reflect differences in intensity of enforcement, conspicuity and other program parameters, site differences, and methodological and statistical differences of the studies. In addition to controlling for confounders, care should be taken in future studies of mobile as well as fixed automated enforcement programs to examine the potential for both positive and negative spillover by measuring traffic flow changes and examining speed and collision trends on alternate routes or untreated adjacent areas as well as in study areas. Multiple comparison groups may be needed. There may be greater potential for negative spillover among more conspicuous versus less conspicuous mobile enforcement programs, but data are needed to test this hypothesis.

Additional well-controlled speed and safety evaluations are needed that account for enforcement intensity levels, accompanying publicity, signing, and other program parameters for varying site conditions. Although some research has been conducted into these factors, particularly in Australia, more well-controlled research is needed, ideally using comparable crash and speed outcome measures. Until that occurs, understanding of how site and program factors affect outcomes and how to most efficiently realize safety objectives is limited. The evidence is building, however, that automated speed enforcement can be an effective tool to reduce speeding and crashes when properly used. The next steps are to build well-designed programs and continue to evaluate these tools.

ACKNOWLEDGMENTS

NHTSA supported this study, conducted under the direction of Douglas Gurin, James Onder, and Kathy Sifrit, contract technical managers. The authors acknowledge Mary Ellen Tucker, Highway Safety Research Center (HSRC) librarian; Robert Sarmiento, Kay Geary, and Joe Ellison of Northwestern University Transportation Library; and Kathy Lococo and Michael Mercadante, TransAnalytics LLC for study assistance. Forrest Council and Jane Stutts from HSRC, reviewed and provided valuable comments on earlier manuscripts.

REFERENCES

1. Speeding, *Traffic Safety Facts: 2006 Data.* DOT-HS-810-814. National Highway Traffic Safety Administration, TRB National Center for Statistics and Analysis, Washington, D.C., 2007.
2. *Special Report 254: Managing Speed: Review of Current Practice for Setting and Enforcing Speed Limits.* TRB, National Research Council, Washington, D.C., 1998. http://onlinepubs.trb.org/Onlinepubs/sr/sr254.pdf. Accessed July 7, 2008.
3. Elvik, R. Speed and Road Safety: Synthesis of Evidence from Evaluation Studies. In *Transportation Research Record: Journal of the Transportation Research Board, No. 1908,* Transportation Research Board of the National Academies, Washington, D.C., 2005, pp. 59–69.
4. Aarts, L., and I. van Schagen. Driving Speed and the Risk of Road Crashes: A Review. *Accident Analysis and Prevention,* Vol. 38, 2006, pp. 215–224.
5. Fitzpatrick, K., P. Carlson, M. A. Brewer, M. D. Wooldridge, and S.-P. Miaou. *NCHRP Report 504: Design Speed, Operating Speed, and Posted Speed Practices.* TRB, National Research Council, Washington, D.C., 2003.
6. Royal, D. *National Survey of Speeding and Unsafe Driving Attitudes and Behaviors: 2002, Volume II: Findings Report.* DOT-HS-809-68. National Highway Traffic Safety Administration, U.S. Department of Transportation, Washington, D.C., 2003.
7. Williams, A. F., S. Y. Kyrychenko, and R. A. Retting. Characteristics of Speeders. *Journal of Safety Research,* Vol. 37, 2006, pp. 277–232.
8. Insurance Institute for Highway Safety. Cameras to Control Speed. *Status Report,* Vol. 34, 1999, pp. 1–3.
9. Pilkington, P., and S. Kinra. Effectiveness of Speed Cameras in Preventing Road Traffic Collisions and Related Casualties: Systematic Review, *British Medical Journal,* Vol. 330, 2005, pp. 331–334 and appendices.
10. Wilson, C., C. Willis, J. K. Hendrikz, and N. Bellamy. Speed Enforcement Detection Devices for Preventing Road Traffic Injuries (Review). *Cochrane Database of Systematic Reviews,* Vol. 2, 2006, CD004607.
11. Cameron, M., A. Cavallo, and A. Gilbert. *Crash-Based Evaluation of the Speed Camera Program in Victoria 1990–91. Phase 1: General Effects, Phase 2: Effects of Program Mechanisms.* Monash University Accident Research Centre Report No. 42, Monash University Accident Research Centre, Victoria, Australia, 1992.
12. Chen, G., J. Wilson, W. Meckle, and P. Cooper. Evaluation of Photo Radar Program in British Columbia, *Accident Analysis and Prevention,* Vol. 32, 2000, pp. 517–526.
13. Chen, G., J. Wilson, and P. Meckle. Speed and Safety Effect of Photo Radar Enforcement on a Highway Corridor in British Columbia. *Accident Analysis and Prevention,* Vol. 34, 2002, pp. 129–138.
14. Christie, S. M., R. A. Lyons, F. D. Dunstan, and S. J. Jones. Are Mobile Speed Cameras Effective? A Controlled Before and After Study, *Injury Prevention,* Vol. 9, 2003, pp. 302–306.
15. Elvik, R. Effects on Accidents of Automatic Speed Enforcement in Norway. In *Transportation Research Record 1595,* TRB, National Research Council, Washington, D.C., 1997, pp. 14–19.
16. Hess, S. Analysis of the Effects of Speed Limit Enforcement Cameras: Differentiation by Road Type and Catchment Area. In *Transportation Research Record: Journal of the Transportation Research Board, No. 1865,* Transportation Research Board of the National Academies, Washington, D.C., 2004, pp. 28–34.
17. Tay, R. Do speed cameras improve road safety? In *Traffic and Transportation Studies, Proceedings of ICTTS 2000,* (K. C. P. Wang, G. Xiao, and J. Ji, eds.) 2000, pp. 44–51.
18. Gains, A., and R. Humble. *A Cost Recovery System for Speed and Red-light Cameras: Two Year Pilot Evaluation.* PA Consulting Group Research Paper prepared for Road Safety Division, Department of Transport, London, 2003.
19. Gains, A., B. Heydecker, J. Shrewsbury, and S. Robertson. *The National Safety Camera Program: Three-Year Evaluation Report.* PA Consulting Group, London, 2004.

20. Newstead, S., and M. Cameron. *Evaluation of Crash Effects of the Queensland Speed Camera Program.* Monash University Accident Research Centre Report No. 204, Monash University Accident Research Centre, Victoria, Australia, 2003.
21. Hauer, E. *Observational Before—After Studies in Road Safety: Estimating the Effect of Highway and Traffic Engineering Measures on Road Safety.* Pergamon Press, Oxford, United Kingdom, and Tarrytown, N.Y., 1997.
22. Elvik, R. The Importance of Confounding in Observational Before-and-After Studies of Road Safety Measures. *Accident Analysis and Prevention,* Vol. 34, 2002, pp. 631–635.
23. Hauer, E., D. W. Harwood, F. M. Council, and M. S. Griffith. Estimating Safety by the Empirical Bayes Method: A Tutorial. In *Transportation Research Record: Journal of the Transportation Research Board, No. 1784,* Transportation Research Board of the National Academies, Washington, D.C., 2002, pp. 126–131.
24. Mountain, L., W. Hirst, and M. Maher. Costing Lives or Saving Lives? A Detailed Evaluation of the Impact of Speed Cameras on Safety, *Traffic Engineering and Control,* Vol. 45, 2004, pp. 280–287.
25. Decina, L. E., L. Thomas, R. Srinivasan, and L. Staplin. *Automated Enforcement: A Compendium of Worldwide Evaluations of Results.* Office of Research and Technology, Behavioral Technology Research Division, National Highway Traffic Safety Administration, Washington, D.C., 2007.
26. ARRB Group Project Team. *Evaluation of the Fixed Digital Speed Camera Program in NSW, for Roads and Traffic Authority.* NSW, RC2416. 2005. www.rta.nsw.gov.au/roadsafety/downloads/2005_05_speedcamera_evaluation.pdf. Accessed July 24, 2008.
27. Goldenbeld, C., and I. van Schagen. 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 and Prevention,* Vol. 37, 2005, pp. 1135–1144.
28. Cunningham, C. M., J. E. Hummer, and J.-P. Moon. *An Evaluation of the Safety Effects of Speed Enforcement Cameras in Charlotte, NC.* Final report presented to NC Governor's Highway Safety Program, 2005.
29. Council, F. M., B. Persaud, K. Eccles, C. Lyon, and M. S. Griffith. *Safety Evaluation of Red-Light Cameras.* ITS Joint Program Office and Office of Safety Research and Development, FHWA, McLean, Va., 2005.
30. Ali, S. Y., O. Al-Saleh, and P. A. Koushki. Effectiveness of Automated Speed-Monitoring Cameras in Kuwait. In *Transportation Research Record 1595,* TRB, National Research Council, Washington, D.C., 1997, pp. 20–26.
31. Hirst, W. M., L. J. Mountain, and M. J. Maher. Are Speed Enforcement Cameras More Effective Than Other Speed Management Measures? An Evaluation of the Relationship between Speed and Accident Reductions. *Accident Analysis and Prevention,* Vol. 37, 2005, pp. 731–741.
32. Mountain, L. J., W. M. Hirst, and M. J. Maher. Are Speed Enforcement Cameras More Effective Than Other Speed Management Measures? The Impact of Speed Management Schemes on 30 mph Roads. *Accident Analysis and Prevention,* Vol. 37, 2005, pp. 742–754.

The opinions, findings, and conclusions expressed in this paper are those of the authors and are not necessarily those of NHTSA.

The Traffic Law Enforcement Committee sponsored publication of this paper.