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Evaluation of the Scottsdale Loop 101 automated speed enforcement demonstration program☆

Kangwon Shin[a,*], Simon P. Washington[a,1], Ida van Schalkwyk[b,2]

[a] Department of Civil and Environmental Engineering, Ira A. Fulton School of Engineering, Arizona State University, Tempe, AZ 85287-5306, United States
[b] School of Civil and Construction Engineering, Oregon State University, Corvallis, OR 97331-3212, United States

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A B S T R A C T

Speeding is recognized as a major contributing factor in traffic crashes. In order to reduce speed-related crashes, the city of Scottsdale, Arizona implemented the first fixed-camera photo speed enforcement program (SEP) on a limited access freeway in the US. The 9-month demonstration program spanning from January 2006 to October 2006 was implemented on a 6.5 mile urban freeway segment of Arizona State Route 101 running through Scottsdale. This paper presents the results of a comprehensive analysis of the impact of the SEP on speeding behavior, crashes, and the economic impact of crashes. The impact on speeding behavior was estimated using generalized least square estimation, in which the observed speeds and the speeding frequencies during the program period were compared to those during other periods. The impact of the SEP on crashes was estimated using 3 evaluation methods: a before-and-after (BA) analysis using a comparison group, a BA analysis with traffic flow correction, and an empirical Bayes BA analysis with time-variant safety. The analysis results reveal that speeding detection frequencies (speeds ≥ 76 mph) increased by a factor of 10.5 after the SEP was (temporarily) terminated. Average speeds in the enforcement zone were reduced by about 9 mph when the SEP was implemented, after accounting for the influence of traffic flow. All crash types were reduced except rear-end crashes, although the estimated magnitude of impact varies across estimation methods (and their corresponding assumptions). When considering Arizona-specific crash related injury costs, the SEP is estimated to yield about $17 million in annual safety benefits.

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1. Introduction

Speeding is recognized as a major contributing factor in traffic crashes. In order to reduce speed-related crashes, the city of Scottsdale, Arizona implemented the first fixed photo speed enforcement camera demonstration program (SEP) in the US on a high-speed limited access facility. The 9-month demonstration program spanning from January 2006 through October 2006 was implemented on a 6.5 mile stretch of Arizona State Route 101, an urban freeway in Scottsdale. The SEP consisted of 6 speed detection stations in the enforcement zone, in which three cameras were positioned to enforce speed for each direction of travel (north and south bound). The speed limit on the SR 101 freeway is 65 mph, while the enforcement equipment is triggered to photograph drivers traveling at speeds of 76 mph or greater. This analysis is focused on estimating:

- The impact of the SEP on citable speeding behavior (i.e.. speeds ≥ 76 mph).
- The impact of the SEP on average speeds.
- The effect of the SEP on traffic safety.
- The expected economic safety benefit of the SEP.

This evaluation, sponsored by the Arizona Department of Transportation (ADOT) and the City of Scottsdale, utilizes data from the Arizona Department of Public Safety (crash reports), ADOT (motor vehicle crashes, traffic volumes, and traffic speeds), the City of Scottsdale (traffic volumes and speeds), RedFlex (speeding detections and traffic speeds), and the Arizona Crash Outcome Data Evaluation System (crash details and crash costs).

2. Literature review

Numerous studies have been conducted to elucidate the relationship between speed and safety: detailed reviews of which are

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* Corresponding author at: Department of Civil and Environmental Engineering, Ira A. Fulton School of Engineering, Arizona State University, Tempe, AZ 85287-5306, United States. Tel.: +1 480 965 3001; fax: +1 480 965 0557.
E-mail addresses: Kangwon.Shin@asu.edu (K. Shin), Simon.Washington@asu.edu (S.P. Washington), idavan@engr.oregonstate.edu (I. van Schalkwyk).
[1] Tel.: +1 480 965 2220; fax: +1 480 965 0557.
[2] Tel.: +1 541 737 8874; fax: +1 541 737 3052.

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provided elsewhere (Lave and Lave, 1998; Stuster et al., 1998; Skszek, 2004; Kweon and Kockelman, 2005). In the 1960s, many studies found that the variance of speed is one of the most important factors affecting safety, suggesting a U-shaped relationship between crash rate and variance in speed. The relationship illustrates that the more the speed of driver deviates from the mean speed of traffic, the greater the likelihood of crash involvement.

Lave (1985) revitalized the U-shaped relationship by estimating regression models to test the relationship between the fatality rate, average speed, and the difference between the 85th percentile and average speed (as a proxy for speed variance) with cross-sectional data from 1981 and 1982. Lave concluded that there was no statistical evidence indicating that average speed affects the fatality rate. Consequently, Lave suggested that the focus of speed laws should be changed so that they coordinate speed rather than limit it. Other studies also agreed that crash rates increase with increasing speed variance, but not with average speed (Garber and Gadiraju, 1989; Garber and Ehrhart, 2000).

Synder (1989) re-estimated Lave's model using a fixed effect linear model with 2 measures for speed variability: the difference between the 85th percentile and median speed (for faster drivers) and the difference between the 15th percentile and median speed (for slower drivers). The author concluded that both average speed and speed dispersion are important factors in highway fatalities, but the speed dispersion for faster drivers is related only to fatality rate. Levy and Asch (1989) also concluded that lack of coordination implies greater risk, but average speed also contributes to increasing the fatality risk depending on speed variance. The authors suggested that enforcement efforts are better directed toward slowing down high speed drivers rather than speeding up low speed drivers. In addition, it is evident that a driver's speed is one of the most important factors affecting crash severity, owing to the relationship between vehicle velocity, kinetic energy, and energy absorption upon impact (Joksch, 1993; Moore et al., 1995; Kloeden et al., 2001).

Past research suggests that speed enforcement—through reduction of high speeds and resulting speed variance—is a promising countermeasure for reducing crash frequency and severity. The results of numerous studies that examine the effect of speed enforcement programs on safety and speed have confirmed the validity of the paradigms discussed above (Hauer et al., 1982; Lamm and Kloeckner, 1984; Elvik, 1997; Vaa, 1997; Sisiopiku and Patel, 1999; Chen et al., 2002; Ha et al., 2003; Hess and Polak, 2003; Retting and Farmer, 2003; Hess, 2004; Champness and Folkman, 2005; Cunningham et al., 2005; Goldenbeld and van Schagen, 2005). The studies in general show that speed enforcement programs lead to a significant reduction in speed and crash frequency. Several studies solely evaluated the effect of speed enforcement on speed (Hauer et al., 1982; Vaa, 1997; Sisiopiku and Patel, 1999; Retting and Farmer, 2003; Champness and Folkman, 2005) or on traffic safety (Elvik, 1997; Hess, 2004), while others evaluated both speed and safety (Lamm and Kloeckner, 1984; Chen et al., 2002; Ha et al., 2003; Hess and Polak, 2003; Cunningham et al., 2005; Goldenbeld and van Schagen, 2005). Two studies (Lamm and Kloeckner, 1984; Ha et al., 2003) reported on an enforcement condition similar to that in Scottsdale (i.e., fixed cameras on freeways), but differed with respect to traffic conditions, road users (skills and 'safety culture'), geometric design standards, and weather compared to the SR 101 in Scottsdale.

Although all studies suggest that photo enforcement cameras are effective in reducing speed and crash frequency at photo enforcement camera deployment sites, the estimates of the impact of speed cameras on safety vary considerably. Elvik and Vaa (2004) conducted a meta analysis that combined the effects of automated enforcement on safety reported in Australia, England, Germany, the Netherlands, Norway, Sweden, and United States. The results yield a 19% reduction in total crash frequency and a 17% reduction in injury crash frequency. The reduction in total crash frequency was greater in urban areas (28%) than in rural areas (4%). A recent meta analysis (Pilkington and Kinra, 2005) also examine the effect of speed enforcement cameras on safety using the evaluation results from 14 observational studies, which were selected from 92 studies. The results show that the effects varied across studies: reductions from 5% to 69% in crash frequencies, 12% to 65% in injuries, and 17% to 71% in fatalities.

Although the effect of speed enforcement programs on safety is consistently positive, many studies suffer from one or more non-ideal conditions. Failure to account for regression-to-the-mean may lead to overestimation of the positive benefits, while benefits may be underestimated if spillover effects are ignored. Moreover, the results of some studies may be biased because total crashes in lieu of target crashes (crashes that are materially affected by the photo enforcement speed cameras) were examined. As a general result, the use of total crashes instead of target crashes will lead to inaccurate estimates of the safety impacts.

Given the knowledge gained from the literature on speed and speed variance and their impact on safety, we now turn to the analysis of the Scottsdale 101 SEP. The paper begins with an examination of the impact of the SEP on speeding behavior, with focus both on speeders and average speeds. The safety impact of the SEP is examined next, where three different analysis methodologies are presented. We discuss the economic impacts in the safety section, converting crashes to crash costs. The final section presents conclusions.

3. Speeding behavior impacts

3.1. Impact on ticketed speeding behavior

Speed detection data in the enforcement zone are used to analyze the impact of the SEP on ticketed speeding behavior—vehicles exceeding 75 mph. Motorists driving between 66 and 75 mph are in violation of the posted speed limit but are not ticketed or detected by the SEP. The speed detection data were collected by the 6 enforcement cameras during the following *warning*, *program*, *after*, and *reactivation* periods:

- *Warning* period: 1/22/2006–2/21/2006 (31 days).
- *Program* period: 2/22/2006–10/23/2006 (244 days).
- *After* period: 10/24/2006–12/31/2006 (69 days).
- *Reactivation* period: 2/22/2007–6/29/2007 (128 days).

For the first 31 days of the program, Scottsdale sent warning notices to drivers who exceeded the 75 mph threshold, and began mailing citations after February 21, 2006. The city deactivated the cameras on October 23, 2006, but continued to detect speeding (the cameras were 'bagged' but the detection loops were still active). The cameras were reactivated on February 2007 and have been in operation since. The deactivation and reactivation dates of the SEP were widely advertised in the media.

The results of the preliminary ANOVA tests suggest that speeding detection frequencies are significantly affected by the period of observation as well as the day of the week (e.g., spikes for weekends and holidays). Table 1 shows the summary statistics of the daily speeding detections per camera for the four periods and day of the week (weekdays vs. weekends and holidays). The descriptive statistics show that the mean number of speeding detections is less on weekdays than on weekends and holidays for all 4 periods, indicating that the likelihood of speeding on weekends and holidays is higher due to the relatively lower traffic demand. Moreover, the statistics reveal that the mean number of speeding detections

Table 1
Summary statistics for daily detection frequency per camera during the 4 periods by the day of the week.

Day of the week	Analysis period				F_{BF} (p-value), H_0: equal variance
	Warning	Program	After	Reactivation	
Weekdays	132.02[a] (66.75)[b]	104.68 (68.61)	1200.72 (829.60)	110.50 (56.22)	612.56 (<0.001)
Weekends and holidays	235.98 (111.31)	185.00 (100.32)	2045.66 (1020.53)	194.13 (92.45)	389.13 (<0.001)

[a] Mean.
[b] Standard deviation.

Table 2
Estimates for the difference in daily speeding detection per camera by period and the day of the week.

Day of week	Period pair	Difference in daily speeding detection (p-value)	95% CIs	
			Lower	Upper
Weekdays	Warning–Program	27.33 (<0.001)	15.17	39.49
	After–Program	1096.04 (<0.001)	998.01	1194.06
	Reactivation–Program	5.81 (0.072)	−0.53	12.16
Weekends and holidays	Warning–Program	50.98 (<0.001)	19.86	82.11
	After–Program	1860.66 (<0.001)	1689.91	2031.42
	Reactivation–Program	9.13 (0.241)	−6.14	24.41

in the *after* period markedly increased compared to the *program* period, and the mean number of speeding detections is also less in the *program* period compared to the *warning* period. In addition, the variances of the speeding detections in each period are significantly different at $\alpha = 0.05$ as shown in Table 1, which were tested using the Brown–Forsythe (BF) test (Kutner et al., 2005). Consequently, the impact of the SEP on speeding behavior was estimated using the generalized least square (GLS) estimation, which accounts for the unequal variances. Specifically, the inverse of the variance in each group by period and day of the week was used as a weight in the GLS estimation (Greene, 2003; Washington et al., 2003; Kutner et al., 2005).

Table 2 shows the estimates for the differences in the daily speeding detections per camera by period and the day of the week. The difference in the mean number of speeding detections between the *after* (or warning) period and the *program* period is significant at $\alpha = 0.05$. Specifically, the frequency of speeding detections significantly decreased by 26% on weekdays (or by 28% for weekends and holidays) from the *warning* to the *program* periods. In addition, the speeding detections increased by 1047% on weekdays and 1006% on weekends and holidays from the *program* to *after* period. However, the mean speeding detection frequency between the *program* period and the *reactivation* period is not significant at $\alpha = 0.05$. In summary, the activation of the SEP was an effective countermeasure for reducing speeding, resulting in significant reductions in the number of motorists exceeding 75 mph.

3.2. Impact on mean speeds

The effect of the SEP on mean speeds were analyzed by comparing speed data in the enforcement zone between the *before* and the *program* periods. In contrast with the analysis of speeding detections, the mean speeds during the *after* and *reactivation* period were not compared in this analysis due to a lack of speed data. Instead, speed and traffic flow data during the *before* period were obtained from 6 reference sites in the enforcement zone, situated close to each camera. Examined are the speed and flow data from each lane aggregated to 15-min intervals, on several observation dates: 4/14/2005, 4/20/2005, and 6/28/2005. To control for the day of the week and observation date effects, the speed and traffic flow data from three identical times and days of the week during the program period were selected: 4/13/2006 (Thursday), 4/19/2006 (Wednesday), and 6/27/2006 (Thursday). In addition, speed and traffic flow data collected during congested conditions (e.g., peak periods) were excluded because the SEP does not significantly impact speeds during congested flow conditions. A threshold of '62.2 mph' was used as the average speed for delineating congested and un-congested conditions, which is approximately equivalent to the threshold speed for 'LOS E' when free-flow speed is 75 mph (Transportation Research Board, 2000).

Table 3
Summary statistics for speed (mph) by period.

Period	Mean	S.D.	Minimum	Q1	Q2	Q3	Maximum
Before	73.11	3.528	64.80	70.00	72.90	76.00	82.00
Program	64.36	1.203	62.33	63.67	64.33	65.00	68.33
Total	66.90	4.514	62.33	63.67	65.00	69.20	82.00

To address the question of spillover affect on average speeds, we examined a 6 mile section of Loop 101 about 40 miles away from the enforcement zone near Glendale, Arizona. We compared the average speeds (while accounting for traffic volumes) during the *before* and *program* periods at this comparison site and found a statistically insignificant 1/2 mph reduction in average speeds from the *before* to *program* periods. We concluded that there had not been a spillover effect on the Loop 101 freeway outside of the enforcement zone (for additional detail see Washington et al., 2007).

Table 3 shows the summary statistics for the mean speed in the enforcement zone by period. The statistics show that the mean speed decreased from 73.1 mph (*before* period) to 64.4 mph (*program* period), and the standard deviation of speed also reduced from 3.5 to 1.2 mph respectively. In order to account for traffic flow effects in addition to the impact of the SEP on average speeds, a variance-weighted least squares technique was used to account for group-wise heteroskedasticity in speed (the Brown–Forsythe test statistic = 1193.78; p-value < 0.001). Unlike the GLS estimation used for analyzing the speeding detections, the effect of *traffic flow* is explicitly considered. Eq. (1) shows the GLS estimation results, where s_i is the mean speed for the ith observation, D_i is the dummy variable for period (1 for the *before* period; otherwise 0), F_i is the mean flow rate for the ith observation, and $D_i \cdot F_i$ is an interaction term between D_i and F_i. The standard errors for each estimate are in parentheses (all estimates are significant at $\alpha = 0.05$).

$$s_i = \underset{(0.04701)}{65.39} + \underset{(0.24206)}{10.29}\,D_i - \underset{(0.00005)}{0.0014}\,F_i - \underset{(0.00024)}{0.0016}\,D_i \cdot F_i \quad (1)$$

It is evident that the impact of the SEP on speed increases as traffic flow decreases due to the well-known relationship between speed

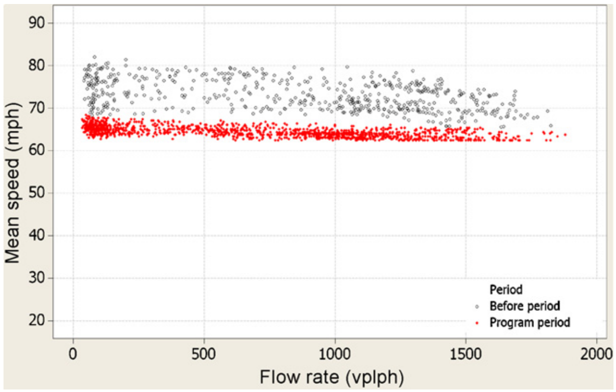

Fig. 1. Change in speed–flow relationship during the *before* and *program* periods.

Table 4
Estimated speed reduction (mph) resulting from the SEP.

Period pair	Speed reduction (S.E.)	95% CIs	
		Lower	Upper
Before–Program (flow = Q1[a])	9.97 (0.201)	9.57	10.36
Before–Program (flow = Q2)	9.04 (0.127)	8.79	9.29
Before–Program (flow = Q3)	8.47 (0.149)	8.17	8.75

[a] Q1, Q2, and Q3 are quartiles for the flow rate: 206 vplph, 800 vplph, and 1169 vplph respectively.

and traffic flow. In addition, the interaction between average speed and flow is significant, suggesting that the speed of a driver in the *program* period is relatively insensitive to changes in traffic flow due to the SEP as shown in Fig. 1, that is, the speed–flow curve is flatter during the *program* period compared to the *before* period. Table 4 shows the estimated impact of the SEP on average speeds for the first, second, and third quartiles of vehicle flows. The mean speed decreased by 9.97 mph when traffic flow was 206 vplph (Q1), by 9.04 mph at 800 vplph (Q2), and by 8.47 mph at 1169 vplph (Q3).

4. Safety impacts

The assessments of safety impacts of the SEP are now presented. Preliminary analysis concepts are followed by a description of three analysis methods, their assumptions, and analysis results.

4.1. Target crashes and evaluation methods

4.1.1. Target crashes

To accurately estimate the impact of the SEP on safety it is necessary to identify which crashes are materially affected by the speed enforcement cameras—referred to as "target" crashes. We define target crashes as those that occur during non-peak periods, since crashes that occur during peak periods are unlikely to be materially affected by the SEP. Time of day (TOD) is used as a proxy for non-peak periods, because the traffic flow conditions at the time of a crash are often unknown. Fig. 2 shows the average daily speeding detection rate by TOD and the day of the week, in which the average daily speeding detection rate is the ratio of the average daily speeding detections per 15-min interval to the average traffic volume per 15-min interval collected from the enforcement sites in the *program* period.

Fig. 2 illustrates that speeding detections decrease during peak hours on weekdays, while they are relatively insensitive to the changes in traffic flow on weekends and holidays. For example, the average daily speeding detection rates during peak hours on weekdays are remarkably low—less than 0.25% between 6:00 AM and 9:00 AM. Since the trend of the speeding detection by TOD suggests that TOD can be used to identify traffic flow regimes, two traffic flow regimes (peak and non-peak periods) are defined: Peak periods (06:00 AM–09:00 AM; 16:00 PM–19:00 PM) and non-peak periods (the remaining 18 h for weekdays; 24 h for weekends and holidays).

Consequently, target crashes in this analysis are crashes that occurred within the enforcement zone (MP 34.51–MP 41.06: 6.5 miles) during the non-peak periods defined by TOD because of the limited expected influence of the cameras on slow moving peak period traffic. Possible spillover effects beyond the enforcement zone are not considered and also not expected due to prior research. In addition, target crashes are "mainline" crashes, which exclude crashes that occurred on SR 101 ramps and frontage roads. The duration of the target crash data are:

- *Before* period: 2/22 to 8/31 (years 2001 through 2005).
- *Program* period: 2/22/2006–10/23/2006 (244 days).

4.1.2. Evaluation methods

The before-and-after (hereafter BA) study was used to estimate the impact of the SEP on safety. The analysis approach used in this study is an enhancement of the generally accepted and widely applied methods described by Hauer (Hauer, 1997; Hauer et al., 2002). The key notations used in the analysis are:

(1) π: Expected number of crashes for the *program* period without the SEP.
(2) λ (L): Expected (observed) number of crashes for the *program* period with the SEP.
(3) κ (K): Expected (observed) number of crashes for the *before* period.
(4) μ (M): Expected (observed) number of crashes in the comparison zone for the *before* period.
(5) ν (N): Expected (observed) number of crashes in the comparison zone for the *program* period.

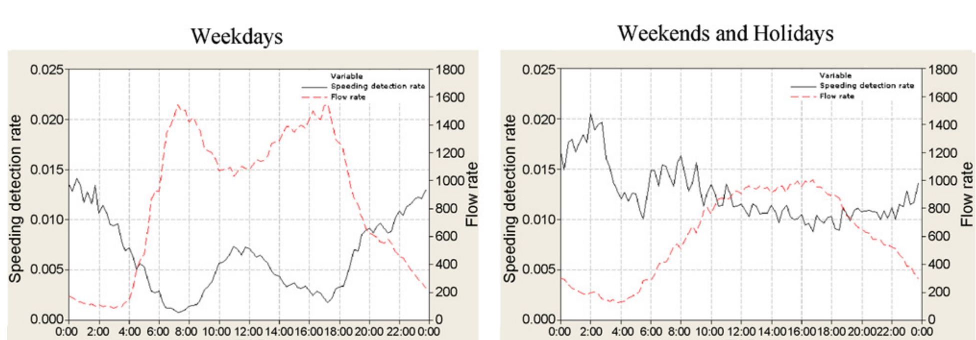

Fig. 2. Average daily speeding detection rate by TOD.

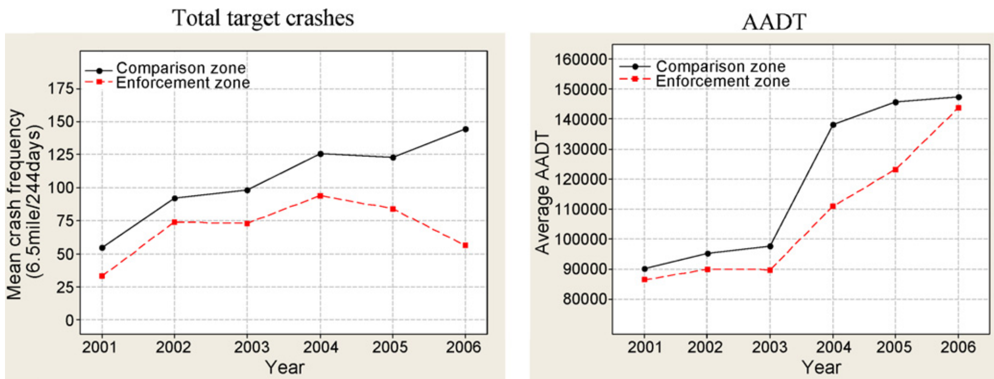

Fig. 3. Change in total target crashes and AADT by year (comparison zone vs. enforcement zone).

(6) $\delta = \pi - \lambda$: Change in safety resulting from the SEP.
(7) $\theta = \lambda/\pi$: Index of the effectiveness of the SEP.

As discussed in Hauer (1997), the parameter λ is generally estimated using the observed number of crashes in the *program* period (L). However, it is often undesirable to predict the parameter π by merely using the observed number of crashes in the *before* period (K) because of the change in safety over time as well as the regression-to-the-mean effect.

In this analysis 3 evaluation methods are used to cope with the aforementioned issues: a BA study with a comparison group, a BA study with traffic flow correction, and an empirical Bayes' BA study. Three different analysis approaches were applied for a couple of important reasons. First and foremost, the empirical Bayes (EB) method is useful and will provide more accurate results only in cases when regression-to-the-mean is present and significant. Second, the different methods make different assumptions, and thus by comparing the results across different methods one can indirectly assess the reasonableness of the method's assumptions. Finally, relying on an approach whose assumptions are invalid will yield incorrect results; thus general agreement across several analysis approaches increases comfort in overall conclusions.

A sequential procedure suggested by Hauer (1997) is used throughout: predict π, estimate λ, estimate θ and δ, and estimate the variances of θ and δ. In the following sections, we briefly discuss the 3 evaluation methods, their assumptions, and the results of the analysis, focusing on obtaining the point estimates (i.e., π, λ, δ, and θ). The mathematical derivations for the estimators and further detail can be found in Hauer (1997) and Washington and Shin (2005), while a full description of the analysis procedures and results for this study are available in Washington et al. (2007).

4.2. Predict expected crashes

4.2.1. BA study with a comparison group

The simple BA study merely based on the observed number of crashes (i.e., K) assumes that no change in factors affecting safety other than the SEP have occurred from the *before* to the *program* period. This assumption is often invalid because numerous factors affecting traffic safety change over time. In order to take into account changes in safety over time, a BA study with a comparison group is routinely used. A key assumption of this approach is that the crash trends in the enforcement zone and the comparison site (or group of sites) are the same in the absence of the SEP. If this assumption is shown to be true, a value of π can be estimated using the change in safety of the comparison group over time. The prediction of π thus becomes:

$$\pi_c = r_d r_c \kappa = r_d \nu \mu^{-1} \kappa, \qquad (2)$$

where r_d is the ratio to adjust for differences in duration between the *program* and *before* periods, r_c is the comparison ratio reflecting the change in safety in the enforcement zone from the *before* to *program* periods, and is commonly estimated using the observed crashes in the comparison group.

In this study, potential comparison sites were limited to sections on SR 101 in order to reduce unwanted spatial variability in crashes. Sections close to the enforcement zone were excluded due to possible spillover effects. The spillover effects of the program upstream and downstream of the enforcement zone are assumed to follow an exponential decay (Hauer et al., 1982), and define a 2.8 mile influence zone. The exponential decay model assumes that the effect of enforcement on speed reduction is reduced by half for every 0.56 mile. Using these influence zones, the spillover effect is eliminated after the 2.8 mile influence zone in each direction. Consequently, the comparison zone used in this analysis, an approximately 48-mile stretch of SR 101 excludes the influence zones in addition to the enforcement zone.

In order to assess the suitability of the comparison zone, past crash trends within the comparison group were compared to crash trends within the enforcement zone using odds ratios, as in prior studies (Hauer, 1997; Wong et al., 2005). The test results reveal odds ratios close to 1, with all 95% CIs containing the expected value 1, indicating that the two crash time series (comparison and enforcement zones) moved together and did not stray apart during the *before* period as shown in Fig. 3 (for more detail see Washington et al., 2007). Consequently, the comparison ratios ($\hat{r}_c$) were estimated from the crash data for the comparison group. In this analysis, annual comparison ratios were used instead of a single comparison ratio to reflect the annual safety change in the comparison group. For example, the comparison ratio in year t is (Hauer, 1997):

$$\hat{r}_c(t) = \frac{N_P}{M_t}\left(1 + \frac{1}{M_t}\right)^{-1}, \qquad (3)$$

where M_t is the observed number of crashes that occurred in the comparison group in year t, and N_P is the observed number of crashes that occurred in the comparison group in the *program* period. Thus, the predicted value of π in the BA study with a comparison group ($\hat{\pi}_c$) is:

$$\hat{\pi}_c = r_d \sum_{t=1}^{B} \hat{r}_c(t) K_t, \qquad (4)$$

where B is the total number of years during the *before* period, and the rest of the notation are as defined previously. Table 5 shows the comparison group evaluation results by crash type and severity. Column L shows the observed crashes during the SEP, while column 1 shows the predicted number of crashes during the SEP using the comparison group methodology. For example, 56 total

Table 5
Observed and predicted crash frequencies by crash type, severity, and BA analysis methods.

Crash type and severity		Observed crash frequencies[a]				Predicted crash frequencies[b] ($\hat{\pi}$)		
		K	L	M	N	1	2	3
All target crashes	Single-vehicle	190	19	290	66	46.53	51.18	50.97
	Side-swipe (same direction)	62	12	118	35	17.68	21.85	22.61
	Rear-end	62	23	278	99	23.36	30.43	30.77
	Other	44	2	53	16	12.47	15.28	15.70
Injury crashes	Single-vehicle	45	6	73	20	9.42	10.37	10.15
	Side-swipe (same direction)	18	2	28	7	3.44	4.83	5.27
	Rear-end	21	8	96	31	6.67	9.30	9.93
	Other	14	1	12	5	3.94	6.61	6.66
PDO crashes	Single-vehicle	145	13	217	47	35.56	39.60	39.64
	Side-swipe (same direction)	44	10	90	28	13.38	16.82	17.14
	Rear-end	41	15	182	68	16.23	21.01	21.13
	Other	30	1	41	11	7.46	9.19	9.36
Total crashes		358	56	739	217	100.03	118.74	120.05
Total injury crashes		98	17	208	63	23.47	31.12	32.02
Total PDO crashes		260	39	530	154	72.63	86.62	87.28

[a] K: observed crashes before SEP; L: observed crashes during SEP; M: observed crashes before SEP, comparison site; N: observed crashes during SEP, comparison site.
[b] 1: BA approach with a comparison group; 2: BA approach with traffic flow correction; 3: empirical BA approach.

crashes were observed in the SEP, whereas 100.03 were expected. Seventeen injury crashes were observed in the SEP, whereas 23.47 were expected.

An alternative analysis approach to using crash trends from a comparison site during the *before* and *program* periods of observation is to use before period data in the enforcement zone, corrected for traffic volume differences over time. This approach is a before-after (BA) study with traffic flow correction, as discussed in the next section.

4.2.2. BA study with traffic flow correction

Traffic flow is one of the most influential factors affecting safety, as it is a direct measure of exposure to risk that drivers face. Therefore, it is necessary to account for the change in traffic flow in the enforcement zone from the *before* to *program* periods in a BA analysis. The effect of changes in traffic flow, which is captured by a traffic flow correction factor, is separate from crash trend effects, which are captured by comparison ratios. If these effects are not estimated appropriately, then double counting may occur as discussed in previous studies (Hauer, 1997; Elvik, 2002).

In this analysis, a significant increase in AADT was observed in the enforcement zone from the *before* to the *program* periods, as shown in Fig. 3 (e.g., 42% AADT increase on average from the *before* to the *program* period). In addition, the average AADT in the enforcement zone increased by 17% from 2005 to 2006, while the average AADT in the comparison zone increased by only 1% from 2005 to 2006. This indicates that in 2006 the increased rate for drivers who drove the Loop 101 sections in the enforcement zone is more rapid than that for drivers who drove the Loop 101 sections in the comparison zone, which may be attributed to the change in travel patterns as a result of regional growth prior to 2006. These differential AADT increases suggest that the increase in traffic flow in the enforcement zone may not be captured by the comparison ratios (applied in the previous section). To relate safety and AADT, safety performance functions (SPFs) using traffic crash data, time-effect covariates, and AADT in a total of 52 sections within the comparison zone from 2001 to 2006 were estimated. These SPFs are then applied to predict the expected number of crashes as a function of traffic flow and time trends using:

$$\kappa_{it} = d_i \gamma_t F_{it}^{\beta}, \tag{5}$$

where κ_{it} is the expected number of crashes for the ith section during the tth year, d_i is the length of the ith section, γ_t is the time-effect parameter capturing the influence of all factors that change from year to year (except for the change in traffic flow), F_{it} is the AADT of the ith section during the tth year, and β is an estimable parameter for the AADT variable. Note that the AADT variables in the SPFs were used as proxy to represent traffic flow during the target periods. The estimation results are summarized in Table 6. Although the model form is quite simple, it is reliable for simultaneously accounting for the change in AADT and time trend effects (Hauer, 1997; Lord and Persaud, 2000; Garber et al., 2006), Applying the estimated SPFs shown in Table 6, the values of π in the BA study with a traffic flow correction (π_F) were calculated using:

$$\hat{\pi}_F = r_d \sum_{i}^{E} \sum_{t}^{B} \frac{\hat{f}(F_{ip})}{\hat{f}(F_{it})} K_{it}, \tag{6}$$

where K_{it} is the observed number of crashes in the ith section during the tth before period, F_{it} is the AADT in the ith section during the tth before period, F_{ip} is the AADT in the ith section during the *program* period, E is the total number of sections in the enforcement zone, B is the total number of years during the *before* period, and r_d is the correction factor for duration differences. These predictions are obtained across crash type and severity, the results of which are summarized in Table 5. For example, column L shows that there were 13 single-vehicle PDO crashes observed in the enforcement zone, whereas the BA with correction for traffic flow predicts that there should have been 39.60 crashes (had the SEP not been implemented). The prediction results show that the estimates of π obtained from the BA approach with traffic correction factor (column 2) are consistently larger than those obtained from the BA approach with a comparison group (column 1). The BA with traffic flow correction methodology reflects both the increase in traffic flow within the enforcement zone from the *before* to *program* periods and time trend effects.

One might note that AADT reflects the Loop 101 use for all time periods, not just off-peak periods. Due to data limitations, the growth in off-peak traffic only was not available. However, a relationship between total AADT growth and off-peak crashes is established in this analysis based on the assumption that a proportionate growth in AADT occurred off-peak and that model beta coefficients—the intercept term and the coefficient for AADT—will correct for scale. Thus, the modeled relationship between AADT and crashes is valid and will play a reasonable and defensible role of forecasting crashes based on AADT changes over time.

Table 6
Estimated safety performance functions used in BA studies [$n = 335$].

Crash type and severity		ln(AADT) (veh/day)	Time-effect parameters						LR test statistic (χ^2)	Dispersion parameter
			$\ln(\gamma_1)$	$\ln(\gamma_2)$	$\ln(\gamma_3)$	$\ln(\gamma_4)$	$\ln(\gamma_5)$	$\ln(\gamma_6)$		
Single-vehicle	All	0.6737***	−5.7872***	−5.3592***	−5.4719***	−5.6184***	−5.7484***	−5.6576***	7754.0***	0.1124***
	Injury	0.5314***	−5.4300**	−5.1525**	−5.2339**	−5.2813**	−5.4479**	−5.1971**	545.1***	0.1147***
	PDO	0.6881***	−6.2459***	−5.7969***	−5.9210***	−6.0780***	−6.2062***	−6.1747***	5361.2***	0.0982***
Side-swipe (same direction)	All	1.1203***	−12.0769***	−11.6352***	−11.5354***	−11.7347***	−11.7751***	−11.6393***	1918.3***	0.0984***
	Injury	0.8854***	−10.7030***	−10.1840***	−10.4307***	−10.3390***	−10.4383***	−10.3948***	22.92***	0.1172**
	PDO	1.1806***	−13.0502***	−12.6626***	−12.4534***	−12.7272***	−12.7591***	−12.5928***	1047.6***	0.1123***
Rear-end	All	1.8012***	−19.3112***	−18.7977***	−18.5895***	−18.7894***	−18.9611***	−18.6873***	2930.9***	0.4491***
	Injury	1.6193***	−18.1867***	−17.6743***	−17.5023***	−17.7472***	−17.8475***	−17.6902***	732.5***	0.4174***
	PDO	1.9240***	−21.1583***	−20.6892***	−20.4826***	−20.6252***	−20.8395***	−20.5186***	2018.5***	0.3997***
Other	All	0.7506***	−8.5236***	−8.0564***	−7.9830***	−8.2254***	−8.2097***	−8.0038***	270.5***	0.0446*
	Injury	1.5941***	−19.5714***	−19.3824***	−19.1445***	−19.3823***	−19.6471***	−19.2705***	88.8***	0.1609*
	PDO	0.4648**	−5.5236**	−4.9989*	−4.9712*	−5.2040*	−5.0953*	−4.9552**	67.83***	0.0680*
Total	All	1.1822***	−10.9089***	−10.4484***	−10.4187***	−10.5924***	−10.7129***	−10.5352***	19047.9***	0.1357***
	Injury	1.1374***	−11.5808***	−11.1901***	−11.1848***	−11.2914***	−11.4240***	−11.2460***	4816.9***	0.1297***
	PDO	1.1791***	−11.2022***	−10.7465***	−10.7139***	−10.8843***	−11.0126***	−10.8378***	15705.5***	0.1161***

* $p < 0.10$.
** $p < 0.05$.
*** $p < 0.01$.

4.2.3. Empirical Bayes BA study

Although the BA study with traffic flow correction accounts for both changes in traffic flow and safety, it does not account for possible regression-to-the-mean (RTM) effects. Empirical Bayes' BA methods are well documented for accounting for possible RTM effects. Due to the questionable assumption that the underlying safety κ is constant over time, as discussed in previous sections, a full EB procedure (Hauer, 1997; Hauer et al., 2002) was used to take into account changes in traffic flow, trend effects, and RTM phenomenon simultaneously.

Assuming that the observed crash frequency in the tth year is Poisson distributed with parameter κ_t and the *prior* distribution of κ_1 (underlying Poisson rate in the base year) is a gamma distribution with parameters α and β, the expected value of κ_1 given all available crash history (K) is expressed (Hauer, 1997; Persaud et al., 2003; Garber et al., 2006):

$$E(\kappa_1|K_1, K_2, \ldots, K_B) = \frac{\alpha + \sum_{t=1}^{B} K_t}{(\alpha/E(\kappa_1)) + \sum_{t=1}^{B} C_t}, \qquad (7)$$

where C_t is the correction ratio to account for the change in safety between the tth and the base years (i.e., $E(\kappa_t)/E(\kappa_1)$), $E(\kappa_1)$ is the expected number of crashes in the base year, and the remaining terms are as defined previously. When the expected crash frequency for each year κ_t is constant over time ($\kappa_t \equiv \kappa$ for $t = 1, 2, \ldots, B$), Eq. (7) reduces to the well-known Bayes estimator with time-invariant κ:

$$E(\kappa|K) = wE(\kappa) + (1 - w)KB^{-1}, \quad w = \frac{E(\kappa)}{E(\kappa) + BV(\kappa)}, \qquad (8)$$

where $E(\kappa)$ is the expected crash frequency of the reference group per year, $V(\kappa)$ is the variation around $E(\kappa)$, w is a weight between 0 and 1. Note that Eq. (8) is a special form of Eq. (7) with time-variant κ, and any year during the *before* period can be used as the base year in Eq. (7), which ultimately leads to the same results as discussed in previous studies (Hauer, 1997; Persaud et al., 2003; Garber et al., 2006). By multiplying the correction ratio by the empirical Bayes estimate in Eq. (7), the expected number of crashes in the tth year is obtained:

$$E(\kappa_t|K_1, K_2, \ldots, K_B) = C_t E(\kappa_1|K_1, K_2, \ldots, K_B). \qquad (9)$$

All components in Eq. (7) are estimated using the estimates from the negative binomial regression models shown in Table 6: $E(\kappa_1)$ and C_t are estimated using the estimates of the expected number of crashes in each year, and the estimate of the dispersion parameter is used for estimating α. Therefore, the predicted value of π for all sections in the enforcement zone is:

$$\hat{\pi}_{EB} = \sum_{i=1}^{E} \sum_{t=B+1}^{P} \hat{C}_{it} \hat{E}(\kappa_{i1}|K_{i1}, K_{i2}, \ldots, K_{iB}), \qquad (10)$$

where P is the total number of years during the *program* period. The estimates of π were also obtained across crash type and severity, and the results are summarized in Table 5. Column 3 shows the predicted crash frequencies in the enforcement zone using the empirical Bayes' BA method. The predicted values of π in the EB BA study are slightly larger than those in the BA study with traffic flow correction (except for single-vehicle injury crashes). This finding suggests that the enforcement zone was not the 'least safe' on SR 101 prior to the SEP program, since the expected number of crashes from the reference group is larger than the observed number of crashes from each section in the enforcement zone. Moreover, the similarity of the predictions between the EB BA study and the BA study with traffic flow correction suggests that potential bias from the regression-to-the-mean is relatively small when estimating the impact of the SEP on safety in the enforcement zone. In other

Table 7
Estimates for the impact of the SEP on safety by evaluation method.

Crash type and severity		BA with a comparison group		BA with traffic flow correction		Empirical Bayes BA	
		$\hat{\theta}$[a]	$\hat{\delta}$[b]	$\hat{\theta}$	$\hat{\delta}$	$\hat{\theta}$	$\hat{\delta}$
Total target crashes	Single-vehicle	0.41 (0.10)^{c,***}	27.53 (5.62)***	0.37 (0.09)***	32.18 (5.88)***	0.37 (0.09)***	31.97 (8.36)***
	Side-swipe (same direction)	0.67 (0.21)*	5.68 (4.19)*	0.54 (0.17)**	9.85 (4.55)**	0.52 (0.16)**	10.61 (5.88)**
	Rear-end	0.96 (0.24)	0.36 (5.85)	0.74 (0.18)*	7.43 (6.41)*	0.74 (0.18)*	7.77 (7.33)*
	Other	0.16 (0.11)***	10.47 (2.40)***	0.13 (0.09)***	13.28 (2.79)***	0.12 (0.09)***	13.70 (4.21)***
Total injury crashes	Single-vehicle	0.62 (0.27)*	3.42 (2.93)*	0.56 (0.24)*	4.37 (3.02)*	0.57 (0.25)*	4.15 (4.02)*
	Side-swipe (same direction)	0.55 (0.39)	1.44 (1.67)	0.39 (0.27)*	2.83 (1.89)*	0.36 (0.25)**	3.27 (2.70)*
	Rear-end	1.14 (0.46)	−1.33 (3.23)	0.82 (0.33)	1.30 (3.57)	0.77 (0.30)	1.93 (4.23)
	Other	0.24 (0.23)**	2.94 (1.48)**	0.14 (0.13)***	5.61 (2.11)**	0.14 (0.13)***	5.66 (2.77)**
Total PDO crashes	Single-vehicle	0.36 (0.10)***	22.56 (4.71)***	0.33 (0.09)***	26.60 (4.97)***	0.33 (0.09)***	26.64 (7.26)***
	Side-swipe (same direction)	0.73 (0.25)	3.38 (3.78)	0.58 (0.20)**	6.82 (4.13)**	0.57 (0.20)**	7.14 (5.21)**
	Rear-end	0.90 (0.27)	1.23 (4.84)	0.69 (0.21)*	6.01 (5.33)*	0.69 (0.20)*	6.13 (6.01)*
	Other	0.13 (0.13)**	6.46 (1.74)**	0.10 (0.10)***	8.19 (2.02)***	0.10 (0.10)***	8.36 (3.22)***
Total target crashes		0.56 (0.08)***	44.03 (8.95)***	0.47 (0.07)***	62.74 (10.50)***	0.46 (0.07)***	64.05 (13.27)***
Total injury crashes		0.72 (0.19)*	6.47 (4.73)**	0.54 (0.14)**	14.12 (5.43)**	0.52 (0.14)**	15.02 (7.00)**
Total PDO crashes		0.54 (0.09)***	33.63 (7.56)***	0.45 (0.08)***	47.62 (8.93)***	0.44 (0.08)***	48.28 (11.24)***

[a] Percent change in crash from the before to the program period is $(\theta - 1) \times 100$.
[b] Positive sign indicates decrease in crash for the program period.
[c] For parameter estimates, the associated standard deviations are in parentheses.
* $p < 0.2$.
** $p < 0.1$.
*** $p < 0.01$ for $H_0: \theta = 1$ or $H_0: \delta = 0$.

words, the SR 101 site was fairly 'average' or slightly above average with respect to safety performance prior to installation of the SEP compared to the remainder of SR 101.

4.3. Impact on safety

Applying the predicted and observed values in Table 5, the safety impacts of the SEP (i.e., θ and δ) were estimated:

$$\hat{\delta} = \hat{\pi} - \hat{\lambda}; \qquad \hat{\theta} = \frac{\hat{\lambda}\hat{\pi}^{-1}}{1 + V(\hat{\pi})\hat{\pi}^{-2}}. \tag{11}$$

The estimators in Eq. (11) are the function of $\hat{\pi}$, $\hat{\lambda}$ or the variance of $\hat{\pi}$, in which the variance of $\hat{\pi}$ is estimated using the well-known delta method assuming that the crash frequency is Poisson distributed or by using the property of the gamma distribution. Again, a full derivation and justification for the variances of $\hat{\pi}$ are available in Hauer (1997) and Washington et al. (2007). The estimation results from the 3 evaluation approaches are summarized in Table 7, in which the significance of the estimates was tested with the conditional binomial test as well as the normal test (Przyborowski and Wilenski, 1940; Sahai and Misra, 1992; Hauer, 1996; Ng and Tang, 2005; Shin and Washington, 2007). The results from the empirical Bayes BA approach show that the total number of target crashes was reduced by 54%, the total number of injury crashes was reduced by 48%, and the total number of PDO crashes decreased by 56%. Although the magnitudes of the impacts vary across the 3 evaluation approaches, the results from all evaluation approaches are in general agreement and suggest that the total number of target crashes was reduced by the SEP. All target crashes were reduced except for the rear-end injury crashes. However, the change in the rear-end injury crashes is insignificant at $\alpha = 0.2$ in all evaluation approaches, suggesting that the estimated increase is not statistically significant.

Table 8
Estimated Arizona-specific crash costs (US $/crash).

Collision type	Crash severity	Final medical cost	Total other cost	Quality of life cost	Total cost
Single-vehicle	K	$162,870	$1,340,063	$2,111,828	$3,614,761
	A	$122,790	$200,291	$361,020	$684,101
	B	$24,104	$61,295	$88,104	$173,503
	C	$13,545	$34,771	$45,343	$93,659
	O	$15,527	$41,402	$50,277	$107,206
Side-swipe (same direction)	K	$119,065	$1,651,039	$2,496,842	$4,266,946
	A	$133,636	$301,959	$442,205	$877,801
	B	$27,504	$80,482	$86,291	$194,277
	C	$16,354	$65,398	$64,673	$146,425
	O	$15,826	$62,247	$50,530	$128,604
Rear-end	K	$71,037	$1,608,206	$2,441,687	$4,120,929
	A	$70,820	$162,469	$239,725	$473,013
	B	$39,899	$100,244	$152,827	$292,971
	C	$28,785	$77,037	$113,695	$219,517
	O	$30,643	$77,278	$117,022	$224,942
Other	K	$77,949	$1,200,900	$1,784,243	$3,063,092
	A	$97,374	$236,524	$310,713	$644,611
	B	$15,431	$62,216	$60,957	$138,604
	C	$8,557	$42,965	$43,917	$95,439
	O	$3,421	$34,919	$11,019	$49,359

Table 9
Change in safety by crash type and severity and annual crash benefits (US $1000).

Analysis method	Collision type	Crash severity					
		Fatal crashes (K)	Disabling injury (A)	Evident injury (B)	Possible injury (C)	Property damage (O)	Total
BA study with traffic flow correction	Single-vehicle	$1503	$134	$1370	−$184	$4266	$7,088
	Side-swipe (same)	$1651	$0	$476	$204	$1312	$3,643
	Rear-end	$0	−$859	$1018	$63	$2021	$2,243
	Other	$1748	$368	$369	$438	$605	$3,529
	Total	$4902	−$358	$3234	$521	$8204	$16,503
EB BA study with time-varying κ	Single-vehicle	$1471	$87	$1341	−$192	$4273	$6,980
	Side-swipe (same)	$1803	$0	$520	$263	$1373	$3,960
	Rear-end	$0	−$822	$1145	$155	$2064	$2,543
	Other	$1762	$371	$372	$443	$618	$3,565
	Total	$5036	−$364	$3379	$669	$8328	$17,048

4.4. Economic analysis

An economic analysis is conducted to quantify the impact of the SEP on safety. Crash costs were derived from extensive national research on the full cost of motor vehicle crashes (Blincoe et al., 2002). Inflation adjusted costs from the National Hospital Discharge Survey, National Health Interview Survey, AZ hospital cost/charge information, CHAMPUS data on physician costs, National Medical Expenditure Survey, National Council on Compensation Insurance, and Crashworthiness Data System were used to derive the final cost estimates.

In this analysis, the crash costs were revised from national averages to reflect Arizona-specific costs such as hospital charges by injury severity category and to reflect crashes on Arizona high-speed limited access urban interstates. Crash costs were estimated from a large sample of crashes that occurred on Arizona high-speed freeways (SR 101, 202, and 51). Table 8 shows the estimated Arizona-specific crash costs for crashes by severity level (KABCO severity scale). The crash costs consist of three categories:

- Medical costs: Professional, hospital, emergency department, drugs, rehabilitation, long-term care.
- Other costs: Police/ambulance/fire, insurance administration, loss of wages, loss of household work, legal/court costs, property damage.
- Quality of life costs: Based on quality adjusted life years.

Crash benefits were obtained by multiplying the estimated crash costs by the estimates of the change in safety (δ). Since it is necessary to estimate the change in safety by crash type as well as severity, the predicted total number of injury crashes for each crash type was divided by crash type. The division was conducted by multiplying the predicted total number of injury crashes by the proportion of crashes with a certain crash type calculated using the observed data. However, the changes in safety for the PDO crashes were directly extracted from the predicted value for the PDO crashes for more reliable results. By multiplying the unit costs by the changes in safety (δ), the economic benefits are obtained. In this paper, the economic benefits from the BA study with a comparison group are not quantified because the estimates do not reflect the change in traffic flow from the *before* to *program* period and thus are thought to be the least accurate of the three methods. Table 9 shows the economic benefits per year of $16.5 M and $17.1 M. The table shows the estimated economic benefits using the BA study with correction for traffic flow and the empirical Bayes BA study. Inspection of the table shows that the expected benefits are similar across all categories, negative benefits (costs) are estimated for rear-end injury crashes, and total crash benefits are quite similar.

5. Conclusions and recommendations

The city of Scottsdale, Arizona demonstrated the first application of a fixed photo SEP in the US on a limited access urban interstate. This study analyzed the impact of the SEP on speeding behavior, traffic safety, and economic impacts of crashes. Three analysis approaches were used, including a before-after analysis with comparison site, a before-after analysis with correction for traffic flow, and an empirical Bayes' analysis. All three methods produced similar results, with slight differences in safety impact estimates among the methods. The comparison site analysis approach suffers from differences in AADT between the comparison site and the enforcement site. The before-after with correction for traffic flow improves upon the comparison site approach with regard to traffic volumes, but does not account for possible regression-to-the-mean effects. The EB method revealed that regression-to-the-mean was small and reverse to what is usually expected, suggesting that the Scottsdale site for the SEP was an above average performing site prior to SEP implementation. As a result, the EB estimates produced the highest estimates of safety effectiveness compared to the other methods.

The major analysis findings of the study are:

- Speeding detection frequency (speeds ≥ 76 mph) increased by a factor of 10.5 after the SEP was temporarily terminated. During this termination the cameras were "bagged" and advertising and news media advertised the end of the program.
- The SEP reduced the average speed at the enforcement camera sites by about 9 mph on average and also contributed to reducing the speed dispersion at the enforcement camera sites.
- The reduction in the mean of speed resulting from the SEP is dependent on traffic flow: the reductions increased as traffic flow decreased due to the well-known relationship between speed and traffic flow.
- The total number of target crashes decreased by 44–54%, depending on the analysis method. In addition, the total number of injury crashes decreased by 28–48%, while the total number of PDO crashes decreased by 46–56%. The empirical Bayes BA approach resulted in the highest estimated reductions, and accounts for AADT, safety trends, and regression-to-the-mean effects.
- All but rear-end crashes types appear to have been reduced. Although the changes in safety for rear-end injury crashes were inconsistent among evaluation methods (and their assumptions), the decrease in rear-end crashes was not statistically significant. It is concluded that the effect of the SEP on rear-end crashes is uncertain, and slight increases or decrease are possible depending on site-specific conditions.
- The total estimated SEP benefits (looking at the costs of crashes only) range from an estimated $16.5 M to $17.1 M per year, depending on the analysis type and associated assumptions. This

estimate does not reflect a cost-benefit analysis, merely an estimate of the annual safety benefit of the program.

A number of more general conclusions are also supported by this research include:

- Swapping of crash types is common for safety countermeasures—many countermeasures exhibit the 'crash swapping' phenomenon observed in this study (left-turn channelization, red-light cameras, conversion of stop signs to signals, etc.), where decreases in some crash types are traded for increases in others. Thus, it is quite expected to see varying magnitudes of reductions across crash categories, and even some increases are possible. The hope is always, of course, that the benefits outweigh the costs, as was the case here.
- In agreement with a substantial body of prior national and international research, reduced speeds and speed dispersion improve safety. The noted reduction speed dispersion may have contributed to safety improvements. The logical defensibility of reduced speed dispersion of course lies in the increased ability of drivers to perform evasive maneuvers, change lanes, merge, etc.

Spillover effects, or general deterrence effects, were subjected to cursory examination in this study due to data and resource limitations. Examination of general deterrence effects of the SEP on average speeds revealed no evidence of an effect at a similar site on the Loop 101 about 40 miles away from the enforcement zone—average speeds were reduced by a statistically insignificant 1/2 mph. Crash spillover effects were examined indirectly through the comparison site analysis, which used the remainder of the Loop 101 as a comparison site. Had crashes increased elsewhere on the Loop 101 as a result of the SEP, the comparison site analysis would have revealed lower estimated benefits of the program compared to the Simple before-after and EB methods. However, this result did not occur, and all analysis methods produced statistically similar results. A more detailed investigation into spillover effects is certainly possible; however this analysis did not delve more deeply into this issue.

Although not studied specifically in this study, anecdotal evidence suggests an increased awareness of drivers in the enforcement zone. It is quite possible, and even likely, that increased awareness has contributed to the improvement in safety. In other words, it is difficult to separate the safety effects of reduced speed and heightened awareness that results from being carefully monitored. This issue remains a topic of future research.

Although it is anticipated that the analysis results are sufficient to draw general conclusions as to the effectiveness of similar SEP programs, the results in this paper should be treated with caution because this study was based on samples from a relatively brief program period. A more comprehensive analysis including the crash data during the reactivation period will yield more reliable results. If possible, investigating the relationship between safety and non-peak period traffic flow is also desirable.

It is also important to recognize that a SEP will have economic impacts other safety, and these impacts have not been addressed in this paper. The SEP will also impact travel times, court costs, personal costs through increased insurance premiums, reduced costs of enforcement on this section of road, to name but a few costs and benefits. Specifically, it is desirable to investigate whether or not the increase in travel times resulting from the SEP is offset by travel time savings from reduced non-recurrent congestion.

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