Statement before the Ohio House Committee on Transportation, Public Safety, and Homeland Security

Red Light Camera Research

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The Insurance Institute for Highway Safety is a nonprofit research and communications organization that identifies ways to reduce the deaths, injuries, and property damage on our nation's highways. We are supported by the nation's automobile insurers. I am submitting for the record information from the Institute about the use of red light cameras to reduce crashes in urban areas.

Red Light Running

The deliberate running of red lights is a common — and a serious — violation. A study conducted at five busy intersections in Fairfax, Virginia, indicates that motorists frequently run red lights. On average, a motorist ran a red light every 20 minutes.[1] During peak travel times, red light running was more frequent.

Such violations may seem trivial to the violators, but the safety consequences are real. An Institute study found that, compared with all other types of urban crashes, those involving signal violations are the most likely to cause injuries. Researchers reviewed police reports of crashes in four urban areas during 1990-91, finding that running red lights and other traffic controls is the most common cause of all urban crashes (22 percent) and the leading cause of injury crashes in urban areas (27 percent).[2] On a national basis, Institute research found that drivers who ran red lights were responsible for more than 200,000 crashes in 2003, which resulted in nearly 176,000 injuries and 934 deaths.[3]

Red Light Cameras

Red light cameras used for enforcement are effective in modifying driver behavior. Institute evaluations of camera programs in two U.S. cities — Oxnard, California, and Fairfax City, Virginia — found that violation rates decreased by about 40 percent during the first year of enforcement.[1,4] Increases in driver compliance were not limited to camera-equipped sites but spilled over to nonequipped intersections as well.

The key question is, would wide use of such cameras improve the safety of our urban streets? Findings from Institute research indicate they do. Significant citywide crash reductions followed the introduction of red light cameras in Oxnard, California. This is the major finding of the first U.S. research on the effects of camera enforcement on intersection crashes.[5] Injury crashes at intersections with traffic signals were reduced by 29 percent after camera enforcement began in Oxnard in 1997. Front-into-side collisions — the crash type that is most closely associated with red light running — were reduced by 32 percent overall, and front-into-side crashes involving injuries were reduced 68 percent. Crashes declined throughout Oxnard even though only 11 of the city's 125 intersections with traffic signals were equipped with cameras. Previous studies of red light running violations in Oxnard and elsewhere found similar spillover effects. That is, the violations dropped in about the same proportions at intersections with and without cameras, attesting to the strong deterrent value of red light cameras and their ability to change driver behavior.

Institute research based on a review of the international literature provides further evidence that red light cameras can significantly reduce violations and related injury crashes.[6] A detailed assessment of international studies of camera effectiveness indicates that red light camera enforcement generally reduces violations by an estimated 40-50 percent and reduces overall injury crashes by 25-30 percent.

A 2005 study sponsored by the Federal Highway Administration evaluated red light camera programs in seven communities (El Cajon, San Diego, and San Francisco, California; Howard County, Montgomery County, and Baltimore, Maryland; and Charlotte, North Carolina).[7] The study found that, overall, right-angle crashes decreased by 25 percent while rear-end collisions increased by 15 percent. Because the types of crashes prevented by red light cameras tend to be more severe and more costly than the additional rear-end crashes that can occur, the study found a positive societal benefit of more than $14 million. The authors concluded that the increase in rear-end crash frequency does not offset the societal benefit resulting from the decrease in right-angle crashes targeted by red light cameras.

A 2003 report conducted for the Ontario Ministry of Transportation evaluated a two-year pilot program using red light cameras in six communities in Ontario.[8] The study found a 6.8 percent decrease in fatal and injury collisions and a 18.5 percent increase in property-damage-only collisions. As with the Federal Highway Administration study, the researchers found that the positive societal benefits resulting from the decrease in fatal and injury crashes was not offset by the increase in property-damage-only crashes. The report concluded that the program "has been shown to be an effective tool in reducing fatal and injury collisions, thereby preventing injuries and saving lives" and recommended its continuation. Based on the results of the pilot program, Ontario's Transportation Minister authorized the use of red light cameras throughout Ontario.

The Cochrane Collaboration, an international nonprofit organization that conducts systematic reviews of the scientific literature on public health issues, has just released a new study summarizing its review of 10 controlled before-after studies of red light camera effectiveness from Australia, Singapore, and the United States.[9] The authors reported that those studies showed a 16 percent reduction in all types of injury crashes and a 24 percent reduction in right-angle crashes. Looking at rear-end crashes, the review did not find a statistically significant increase or decrease.

A 2004 report by the Virginia Transportation Research Council evaluated red light camera programs in seven Virginia communities (Alexandria, Arlington, Fairfax City, Fairfax County, Falls Church, Vienna, and Virginia Beach).[10] The authors found that the "number of crashes attributable to red light running has decreased, although the number of rear-end crashes has increased." The authors also cautioned that additional information was required to determine the net safety impact. Nevertheless, the report recommended continuation of the red light camera program.

A 2004 report by the Institute for Transportation Research and Education of the North Carolina State University examined the effectiveness of the red light camera program in Raleigh, North Carolina.[11] The study found that red light running crashes were reduced by 22 percent; angle collisions were reduced by 42 percent, and rear-end collisions were reduced by 25 percent.

In contrast to these analyses, a study by Burkey and Obeng[12] has reported an overall increase in crashes associated with the implementation of red light cameras in Greensboro, North Carolina. However, because this study has been cited in testimony given before this committee, I wanted to address its shortcomings. The Institute has conducted a detailed critique of the study as well as a response to subsequent comments by Burkey about our criticisms of the study.[13,14] Both documents are attached to this testimony. A major flaw of the study is that Burkey and Obeng treated data from intersections with and without cameras as if the cameras had been randomly assigned to their location. In fact, Greensboro officials generally installed cameras at intersections with higher crash rates — the camera intersections had more than twice as many crashes as other intersections in the city before cameras were installed. The methods used by Burkey and Obeng failed to properly account for this important difference. As a result they concluded that, because crashes at intersections with cameras outnumbered those at the comparison sites, the cameras were to blame. But this simply reflects the far higher number of crashes at the camera sites to begin with. As discussed earlier in this testimony, in contrast to the flawed Burkey and Obeng study, well-designed studies by the Institute and the Federal Highway Administration, as well as the comprehensive review by the Cochrane Collaboration, have shown consistently the effectiveness of red light cameras in reducing injury crashes.

Privacy Issue

Photographing vehicles whose drivers run red lights does not violate anyone's protected privacy interest. Most red light cameras record only the rears of vehicles, not the occupants. Besides, driving is a regulated activity on public roads. Neither the law nor common sense suggests that drivers should not be observed on the road or that their violations should not be recorded.

Public Support

Like other government policies and programs, red light camera enforcement requires acceptance and support from the public and elected leaders. Although the "big brother" issue is raised by some opponents of automated enforcement technology, public opinion surveys consistently reveal wide acceptance and strong public support for red light cameras. Telephone surveys in many U.S. cities have found more than 75 percent of drivers support red light cameras.[15] As part of its 2004 report, the Virginia Transportation Research Council conducted a public opinion survey at six locations throughout Virginia. Almost two-thirds of the respondents supported red light camera programs.[10] Similar public opinion surveys in Europe and Canada revealed that the majority of drivers support red light cameras.[16]

Research conclusively demonstrates the effectiveness of automated red light camera enforcement in reducing red light violations and related serious injury crashes, especially right-angle injury crashes. The citizens of Ohio will benefit from the continuation of these programs.

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Review of "A Detailed Investigation of Crash Risk Reduction Resulting from Red Light Cameras in Small Urban Areas" by M. Burkey and K. Obeng

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Mark Burkey and Kofi Obeng (2004) of the Economics Department at North Carolina Agricultural and Technical State University conducted a study on the red light camera program in Greensboro, North Carolina. The study is not published in a scientific peer-reviewed journal but is available publicly online (http://www.ncat.edu/~traninst/Burkey_Obeng_Updated_Report_2004.pdf). The following comments address major flaws in the methodology that invalidate the study and the authors' conclusions.

Description of the Report

Officials in Greensboro, North Carolina, installed the first red light camera in February 2001. As of May 2003 cameras were operating at 18 intersections. Violators are fined $50.

The purpose of the Burkey and Obeng (2004) study was to estimate the crash reduction or increase, if any, associated with red light cameras. Crash data for the study were obtained from a North Carolina database, TEAAS (Traffic Engineering Accident Analysis System), that includes injury crashes and those involving more than $1,000 damage. All such crashes occurring within 100 feet of a signalized intersection in Greensboro from January 1999 until September 3, 2003 were included. This produced 57 months of data at 303 intersections, for a total of 17,271 monthly crash counts. Many of those counts were zeros. The data set included 10,721 crashes.

The crash counts were used as a dependent variable in a regression model. The key independent variable was the presence or absence of a red light camera. Signalized intersections without cameras in the same community were used as controls. Average Daily Traffic Volumes (ADVs) were used to attempt to adjust for exposure. Some auxiliary independent variables, based on the descriptive features of the 303 signalized intersections, were used in the model; these include yellow timing, all-red timing, speed limits, number of lanes, number of left turn lanes, and weather conditions.

The authors claimed that red light cameras were associated with 42 percent more crashes, 78 percent more rear-end crashes, and 12 percent (nonsignificant) more angle crashes. These findings are contrary to those of previous studies, which have been peer reviewed and appear in scientific journals (McGee and Eccles, 2003; Ng et al., 1997; Retting et al., 2003; Retting and Kyrychenko, 2002).

Major Flaws in the Study

The methods used by Burkey and Obeng (2004) contain major flaws that account for the contrary findings and that invalidate the study's conclusions. A principal flaw is the authors' selection of controls — signalized intersections without red light cameras in the same community. The goal of photo enforcement is to reduce violations and crashes on a citywide basis. This is accomplished by locating cameras throughout the community. Publicity and media coverage generally make drivers aware that a city is using red light cameras, not specifically which intersections have cameras. Also, many other North

Carolina cities implemented red light cameras at about the same time as Greensboro, creating further general awareness of this type of enforcement at signalized intersections. Numerous published studies have reported spillover effects of red light cameras to intersections where cameras are not located. Because of these generalized changes in driver behavior, assigning signalized intersections in the same community as controls is likely to produce inaccurate estimates of crash effects that underestimate the benefits associated with red light camera enforcement.

However, the authors do not just estimate a small benefit of red light cameras; they concluded that cameras are increasing crashes. How can this occur? It occurs because in addition to their logical error of ignoring the spillover effect, the authors also misanalyzed the data. They constructed an erroneous statistical model that failed to account for the fact that red light cameras are normally located at high-crash locations, not random intersections. A simple analysis of crash data provided in Tables 4.2 and 4.3 of the report for the 29-month period prior to camera enforcement shows that more than twice as many crashes per intersection occurred at the intersections where Greensboro officials later installed red light cameras.

Table 1
Number of Crashes per Intersection during 29 Months Prior to Camera Enforcement

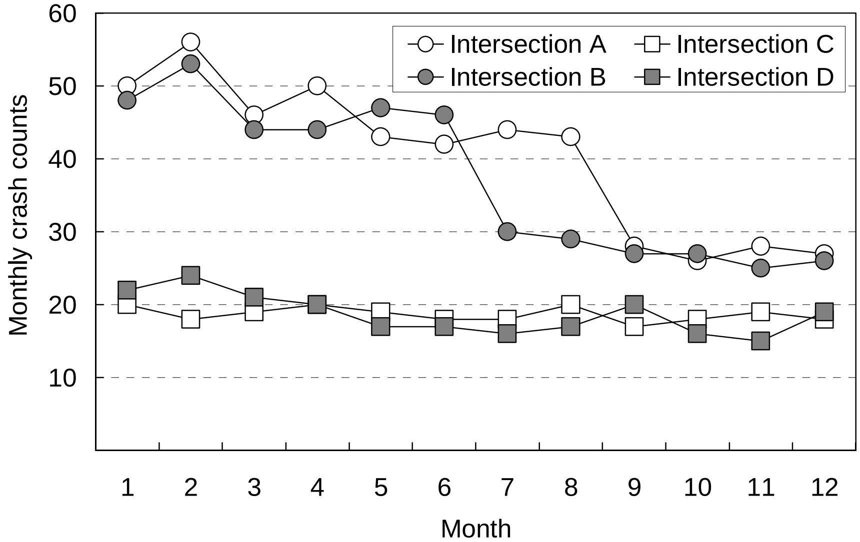
	Number of Intersections	Crashes	Crashes per Intersection
Camera sites	18	840	47
Noncamera sites	285	4,827	17

A hypothetical example illustrates how Burkey and Obeng’s (2004) methodology produces biased effectiveness estimates in this context, where the treated intersections have many more crashes to begin with than the untreated ones. Table 2 and its accompanying graph present monthly crash counts for an imaginary city with four signalized intersections. Prior to installing red light cameras, about 47 crashes

Table 2
Monthly Crash Counts in an Imaginary City with Four Intersections

Month	Intersection A	Intersection B	Intersection C	Intersection D
Before camera installation				
1	50	48	20	22
2	56	53	18	24
3	46	44	19	21
4	50	44	20	20
5	43	47	19	17
6	42	46	18	17
After camera installation				
7	44	30	18	16
8	43	29	20	17
9	28	27	17	20
10	26	27	18	16
11	28	25	19	15
12	27	26	18	19

Figure 1
Monthly Crash Counts in an Imaginary City with Four Intersections



occurred each month at intersections A and B, whereas at intersections C and D the monthly crash counts were about 20. As in Greensboro, officials in our hypothetical city located red light cameras at intersections with higher numbers of crashes — A and B.

Suppose cameras were installed in July at intersection B and in September at intersection A, and crashes subsequently dropped to about 30 per month at these camera-equipped intersections. If no spillover effect were assumed (therefore monthly crash counts for intersections C and D remained about the same after cameras were deployed at intersections A and B) and the data were fed into a Burkey and Obeng-type model, the finding would be 30 percent more crashes associated with cameras (Table 3). (Note: If spillover effects were assumed in this hypothetical example, the spurious estimated increase caused by red light cameras would be even greater.) This result occurs because the model does not recognize differences between the intersections other than the presence or absence of cameras. The variable *RLCPRES* (short for red light camera presence) identifies intersections with cameras (months 9-12 for intersection A and months 7-12 for intersection B). After cameras were installed at intersections A and B, crash counts did go down sharply. They did not, however, decline to the levels at intersections C and D (without cameras) because the actual crash counts at intersections A and B were much higher to begin with.

Table 3
Estimates from the Burkey and Obeng-Type Model

	Parameter Estimate	Estimated Percentage Change
Intercept	3.73	
Month	-0.07	-7
RLCPRES	0.26	+30

Given the fact that red light cameras tend to be installed not at random, but rather at intersections with large numbers of crashes, it is important to consider two separate phenomena: (1) the effect of cameras on the number of crashes and (2) the effect of number of crashes on selection of camera locations. The Burkey and Obeng (2004) model contained only one variable, *RLCPRES,* related to cameras. The appropriate analysis, if one assumes no spillover effect, would include another variable, *RLCGROUP*, to indicate whether or not an intersection has a camera, regardless of the month (*RLCGROUP* equals 1 for intersections A and B and 0 for intersections C and D, regardless of the month). This variable is necessary because intersections that eventually are equipped with cameras differ from those that are not. Omitting this term is acceptable only if cameras are placed at random.

Adding *RLCGROUP* changes the results for this hypothetical example dramatically (Table 4). Now a 33 percent decrease in crashes is associated with the introduction of red light cameras. The estimate for the new variable (*RLCGROUP*) is positive and large, indicating that crash rates at intersections A and B, selected for the cameras, were more than twice as high as rates at the other two intersections. This also is consistent with the raw data.

Table 4
Estimates from the Correct Model

	Parameter Estimate	Estimated Percentage Change
Intercept	3.08	
Month	-0.02	-2
RLCGROUP	0.87	+138
RLCPRES	-0.40	-33

Therefore, it is not camera placement that causes higher numbers of crashes at intersections A and B. Rather, it is higher numbers of crashes that caused red light cameras to be placed at these intersections. Including the *RLCGROUP* variable reveals this; omitting it leads to erroneous estimates. Burkey and Obeng's (2004) use of a single camera-related variable (*RLCPRES*) collapsed two effects: the effect of cameras on the number of crashes and the effect of number of crashes on selection of camera locations. When the second variable (*RLCGROUP*) is added, the two effects are separated out, and estimates of the true camera effect can be obtained.

In summary, the methods used by Burkey and Obeng (2004) contain major flaws that invalidate the study. By ignoring the spillover effect, the authors could obtain only a biased (low) estimate of red light camera effectiveness. And by failing to account for the fact that treated intersections would have higher crash rates to begin with, the authors forced their model to estimate a negative effect of red light cameras. Not surprisingly, these findings are contrary to those published in peer-reviewed scientific journals, which generally indicate that red light camera enforcement can significantly reduce red light violations and injury crashes.

Criticisms by Burkey and Obeng of Institute Research and Our Response

Retting and Kyrychenko (2002) published a study evaluating changes in crashes associated with red light camera enforcement in Oxnard, California. The study was published in the *American Journal of Public Health*. Burkey and Obeng (2004) claim there are problems with the Institute's analysis of crash data in the Oxnard study. But unlike Burkey and Obeng's paper, the Institute study underwent rigorous peer review by experts in the fields of highway safety and statistics. After a brief summary of the Institute's study, we will address each of Burkey and Obeng's criticisms in detail.

The objective of the Oxnard study was to estimate the impact of red light camera enforcement on crashes in one of the first U.S. communities to employ such cameras. Prior research in Oxnard (as well as in Fairfax, Virginia) found that red light cameras reduced red light violations by about 40 percent at both intersections equipped with cameras and signalized intersections in the same communities not equipped with cameras. Because of this generalized effect of photo enforcement on driver behavior, it is inappropriate to use signalized intersections in the same community not equipped with cameras as controls for camera-equipped sites. Doing so would produce biased estimates of crash effects associated with red light camera enforcement. Instead the Institute chose to control for a wide range of external influences affecting crash counts at signalized intersections (e.g., traffic volume, economic conditions, population growth, weather conditions, and driver licensing laws) by monitoring crashes at intersections without traffic signals in the same community. This allows researchers to separate changes in crash counts at signalized intersections, which are targeted by red light cameras, from general crash trends in the same community. In addition, the methods of the Oxnard study incorporated crash data for the same time periods for signalized and nonsignalized intersections in three comparison cities in California without red light cameras. Incorporating these data confirmed similarities in the relationship between crashes at signalized and nonsignalized intersections outside Oxnard and thus strengthened the statistical significance of the method.

Criticism: "[T]he fact that only aggregate data are used for four towns, ignoring such important variables as traffic counts and the numbers of the various types of intersections involved, is troubling" (Burkey and Obeng, 2004, p.13).

Response: In the Oxnard study, crash data were analyzed for hundreds of intersections in Oxnard and three comparison cities. Systematic traffic counts are simply not available for all intersections in these (or most other) cities. Burkey and Obeng encountered the same lack of systematic traffic volume data in Greensboro. To "solve" this problem they decided to average any available observations for each intersection, thus holding traffic volume constant over the entire study period. Such a method renders traffic volume data essentially useless, since the main purpose of these data is to see whether month-to-month changes in crashes are explained by changes in traffic volume. Their method did not address the issue at hand because they did not look at changes in traffic volumes over time. A principal reason for incorporating crashes at nonsignalized intersections in the Oxnard analyses was to control for overall trends in traffic volume on a citywide basis, which Burkey and Obeng's method of using constant traffic volume data throughout the study fails to do.

Criticism: "The study period was from January 1995 through December 1999. During the 1990s the four towns in the study grew at very different rates, seeing population changes from 7.89 percent (Santa Barbara) to 41.32 percent (Bakersfield). At a minimum, adjustments to the crude accident counts should have been made for these large variations in population growth" (Burkey and Obeng, 2004, p.13).

Response: Burkey and Obeng provide misleading information regarding population growth rates. According to official California estimates (California Department of Finance, 2002), population growth rates in Oxnard, Bakersfield, San Bernardino, and Santa Barbara between 1995 and 1999 were 6.7, 12, 2.5, and 3 percent, respectively. So compared with Oxnard, one city grew at a slightly faster pace while two grew at a slightly lower pace. The combined populations of the three comparison cities grew at 6.8 percent, essentially equivalent to Oxnard.

Criticism: "[I]f the analysis is performed as... described... 16 observations and 12 dummy variables leave 3 error degrees of freedom. Replicating the analysis reportedly done, one should end up with the following... It is striking how the Estimate and Mean Square are identical to those reported... however, the degrees of freedom and p-value have changed" (Burkey and Obeng, 2004, p.13).

Response: The initial model can indeed be expressed using 12 variables (3 for the cities, 1 for the type of intersection, 1 for the time period, 6 for the interactions of city with type and period, and 1 for the presence of cameras). However, the variable for type of intersection was found to be statistically insignificant in the initial model ($p = 0.87$). This implied that the type of intersection alone (signalized/ nonsignalized) did not do a good job in explaining variability in this particular dataset. The corresponding variable was therefore dropped from the model leaving 4 degrees of freedom to the error. We do not understand why the authors could not replicate our results, given that all necessary data to do so are provided in the paper. This is simply a mathematical exercise. In contrast, Burkey and Obeng's paper does not provide data necessary to replicate their results.

Criticism: "[T]he analysis performed does not do what the authors claim. The authors believed that they were using the three cities in California other than Oxnard as controls in an analysis of variance" (Burkey and Obeng, 2004, p.14).

Response: To estimate the effect of cameras, we examined changes in crash counts at signalized versus nonsignalized intersections. That is, we were using the nonsignalized intersections in Oxnard as controls. To rule out the possibility that differences between signalized and nonsignalized intersections could be due to some factors other than cameras, or because of chance alone, we looked at such changes in other cities that did not have cameras. Thus, the estimate of camera effectiveness in Oxnard was not based solely on the number of crashes in comparison cities. However, it is a misstatement that crash counts in comparison cities were not used in the analysis. Mathematically, these counts contributed to an important part of the results: the statistical significance. If differences between changes at signalized and nonsignalized intersections of, say, San Bernardino were of similar (or larger) magnitude as in Oxnard, the model would render the Oxnard estimates statistically insignificant.

Criticism: "The overall implication is that the effect attributed to the red light cameras... is only a comparison of the accident growth rate between signalized and nonsignalized intersections in Oxnard, CA. The other data does not act as a control, nor does it add any information to this model. This lack of control is especially critical for this study done in California because several important policy changes were implemented in the state during the period of the study. Most importantly, the fine for red light violations was increased from $104 to $270. In addition, the graduated licensing program for minors was expanded... Because of the way this model was constructed, the p-value calculated has no statistical meaning, and the estimate cannot be described as an effect of red light cameras" (Burkey and Obeng, 2004, p.14).

Response: The fine increase for red light running (whether enforced by cameras or conventional police traffic stops) applied to all California cities, including Bakersfield, San Bernardino, and Santa Barbara. Therefore, the effect of the fine increase (if any) on crashes is captured by the comparison cities. Effects (if any) of graduated licensing provisions also are controlled for in our study since teen drivers are subject to the same licensing provisions at nonsignalized intersections in Oxnard and in comparison cities.

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Response to Comments by Mark Burkey Concerning Institute Criticism of Burkey and Obeng Study of Red Light Cameras

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These comments concern a response by Mark Burkey (2005) of the Economics Department of North Carolina A&T State University to recent critical reviews by the Institute (Insurance Institute for Highway Safety, 2005; Kyrychenko and Retting, 2004) that discuss flaws in Burkey and Obeng's (2004) evaluation of red light cameras in Greensboro, North Carolina.

Nothing in Burkey's (2005) response changes the fact that the publicly available Burkey and Obeng (2004) study contains significant methodological flaws, including a model that fails to account for installation of red light cameras at high-crash locations, and use of noncamera sites in the same city as controls. Burkey's response reviews his data and analysis in some detail in an effort to refute the Institute's criticism that his method does not properly account for the fact that red light cameras generally were installed at high-crash locations. The fact remains that no term in his model fully controls for differences between intersections selected for red light cameras and those not selected. Instead, his method relies on covariates to capture those differences. However, Burkey did not test to see if these covariates accomplished this critical function. While Burkey and Obeng take exception to the Institute's research and believe they have discovered the "true" effect of red light cameras, it should be noted that many researchers unaffiliated with the Institute have come to the conclusion that red light cameras reduce — not increase — crashes. The latest scientific evidence is provided in the form of a systematic review of red light camera studies conducted by the Cochrane Partnership in the United Kingdom (Aeron-Thomas and Hess, 2005). The authors concluded that red light cameras are effective in reducing injury crashes, but that the evidence is less conclusive regarding total collisions. The following comments respond to Burkey's rebuttal of the Institute's criticism.

Flawed Regression Model

One of the Institute's principal criticisms of the Burkey and Obeng (2004) study is its use of a model that ignores the fact that red light cameras generally were placed at high-crash locations. This error is important because the Burkey and Obeng study is not a traditional before-after analysis, but rather a correlation study, in which the authors attempted to correlate crash counts (before and after red light cameras were installed) with numerous intersection variables, including the presence or absence of a red light camera. This correlation analysis would be appropriate if red light cameras were randomly assigned, to avoid the bias resulting from placement of cameras at high-crash locations. Otherwise, it would come as no surprise if intersections selected for red light cameras were correlated with higher numbers of crashes than those where cameras were not installed.

In his rebuttal, Burkey (2005) dismissed this criticism and claims that "many high and low crash locations were chosen for RLC placement, and that many high crash locations were not chosen." However, data provided by Burkey support the Institute's criticism and not his argument. In the Burkey and Obeng (2004) study, it is revealed that intersections selected for red light cameras had almost 3 times as many crashes before cameras were installed as intersections not selected for cameras. This is a significant bias. And according to data included in Burkey's rebuttal, 44 percent of the highest (top 25) crash locations in Greensboro received cameras whereas only 2.5 percent of the remaining intersections received cameras.

Burkey (2005) says that he has now conducted a new analysis using a Fixed Effect model, which could address problems associated with placement of cameras at high-crash locations. However, Burkey has not yet revealed the methodological details or results of the new analysis. The Institute is eager to review Burkey's revised analysis.

One should keep in mind that because Burkey and Obeng's (2004) analyses are confined to data for signalized intersections in Greensboro, it would be difficult — if not impossible — to develop a model that would produce a true estimate of the crash effects. This is due to the spillover effect of red light cameras to noncamera sites, which was another central point of the Institute's criticism.

Spillover Effect

In his rebuttal, Burkey (2005) dismisses the possibility of a spillover effect and claims the authors tested for spillover effects in Greensboro using different methods but did not find one. However, the range of methods is limited when one's data are confined to signalized intersections in the same community.

Burkey (2005) also questions the Institute studies demonstrating spillover effects, and he attempts to trivialize them by claiming the Institute examined "a total of 5 intersections without red light cameras for an average of 24 hours each." Here, Burkey is again inaccurate. In terms of the Institute's studies conducted in Oxnard, California (Retting et al., 1999a) and Fairfax, Virginia (Retting et al., 1999b), these employed designs that allowed for estimation of spillover effects of camera enforcement to intersections in the same city that were not equipped with red light cameras. In both cities, reductions in violations were observed at intersections not equipped with red light cameras, and the reductions were comparable in magnitude to those at camera-equipped sites. The studies clearly state that violation data at noncamera sites were collected for a total of 146 hours in both Fairfax and Oxnard.

Numerous researchers unaffiliated with the Institute have documented spillover effects of red light camera enforcement. These include studies in Brisbane, Australia (Arup Transportation Planning, 1992), Melbourne, Australia (Kent et al., 1995), and British Columbia, Canada (Chen et al., 2001). And in a recent international review of red light camera studies, Aeron-Thomas and Hess (2005) state the following: "As red light camera programs involve publicity campaigns and warning signs, behavior in general may be influenced, with drivers inclined to obey red lights at all signalized junctions thus reducing the risk of collisions at noncamera sites."

Other Studies of Red Light Cameras

Finally, Burkey (2005) suggests that readers “Consult researchers who do not have an agenda. Read any of the good comprehensive reviews of the red light camera literature by McFadden and McGee (1999), Maccubbin et al. (2001), McGee and Eccles (2003), and Milazzo et al. (2001).” These reviews (not all of which are “good and comprehensive”) do not support (and in some cases, even contradict) Burkey’s contention that red light cameras increase crashes. The following is a brief synopsis of the reviews and their findings.

- McFadden and McGee (1999) synthesized results from Federal Highway Administration funded demonstrations of red light camera technology. Their major conclusion was that implementation of a red light camera program “should translate into at least 20 and as much as a 60 percent reduction in violations.” They reported reductions in crashes in Howard County (Maryland) and Polk County (Florida) when comparing crash data one year before and one year after red light running campaigns were implemented, but they also noted these simple comparisons were not statistically meaningful.
- Maccubbin et al. (2001) of Mitretek systems conducted a literature review of red light camera studies. For the most part, they simply summarized what others have said and did not provide a critical review of the literature. An exception involves some analysis by Mitretek of crash data provided by Howard County (Maryland). Their analysis indicated “statistically significant reductions in the total number of both right-angle and rear-end crashes at camera enforced intersections … The measured reductions were a 42.5 percent decline in right-angle collisions and a 29.5 percent reduction in rear-end crashes at the enforcement sites.”
- McGee and Eccles (2003) summarized results from a project conducted for the Transportation Research Board in which they evaluated crash effects of red light camera enforcement. They concluded that “Based on the information that has been acquired and reviewed, it appears that (red light cameras) can be an effective safety countermeasure. The findings of several studies support that, in general, red light cameras can bring about a reduction in more severe angle crashes with, at worst, a slight increase in less severe rear-end crashes.” They did state there was not enough evidence at the time to state this conclusively.
- Milazzo et al. (2001) developed a proposed framework for North Carolina officials to consider for addressing red light running problems, including engineering and enforcement countermeasures. The report concludes that “Automated enforcement cameras provide a non-intrusive, 24-hour surveillance method — not perfect, but highly reliable and very effective at observing violations. If implemented at appropriate locations under a framework that contains economic incentives that maximize safety, automated enforcement can continue to play a positive role.”

- In addition to these reviews cited by Burkey, a systematic international review of red light camera studies sponsored by the Cochrane Partnership was recently published (Aeron-Thomas and Hess, 2005). For this project, two reviewers independently extracted data from 10 studies that met inclusion criteria and calculated a weighted intervention effect across trials. The authors concluded that red light cameras are effective in reducing injury crashes (a 16 percent effect was estimated), but that the evidence is less conclusive regarding total collisions.

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