

**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.

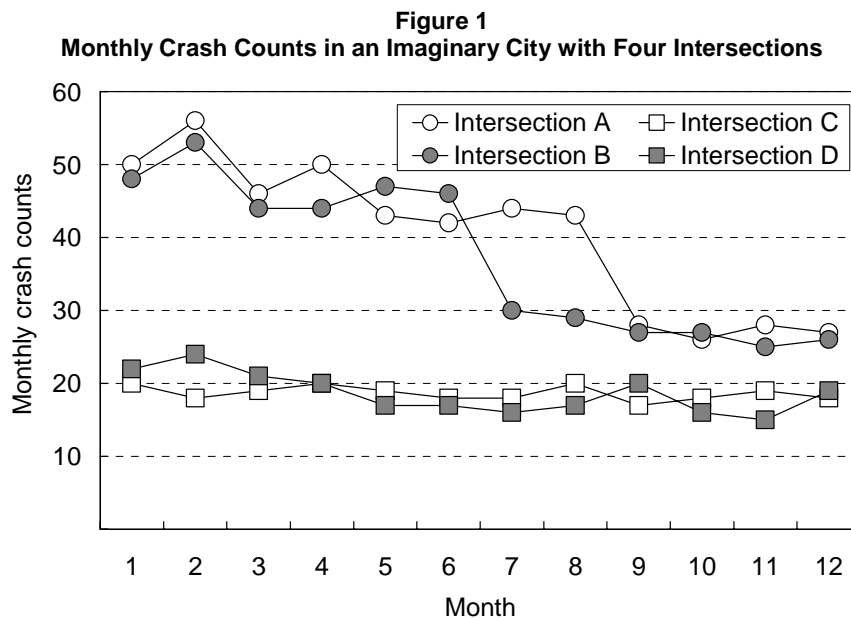
Table 1
Number of Crashes per Intersection during 29 Months Prior to Camera Enforcement

	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



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
<i>RLCPRES</i>	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
<i>RLCGROUP</i>	0.87	+138
<i>RLCPRES</i>	-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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