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Barbara Langeland-Orban

Etienne E. Pracht

John T. Large

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# Counterpoint: The Insurance Institute for Highway Safety Study Actually Found Cities Using Red Light Cameras Had Higher Red Light Running Fatality Rates

Barbara Langland-Orban, PhD, Etienne E. Pracht, PhD, John T. Large, PhD

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## ABSTRACT

*In February 2011, the Insurance Institute for Highway Safety (IIHS) disseminated their research study that compared red light running traffic fatality rates between cities that implemented red light camera (RLC) programs with cities that did not. The IIHS researchers concluded cities that used RLCs had a significantly larger percentage reduction in both red light running (RLR) fatality rates and total fatality rates at signalized intersections. Because a previous IIHS study on RLCs was found to use flawed research methods, as well as to incorrectly report findings, the current IIHS RLC analysis is reviewed for adherence to scientific methods. Our review reveals the 2011 IIHS study is logically flawed and violates basic scientific research methods that are required for a study's findings to be valid. It has neither internal nor external validity. More importantly, the IIHS did not fully explain the results of its analysis. Correctly interpreting its model's results actually shows that cities using RLCs had an estimated higher rate of red light running fatalities, specifically 25%, than cities that did not use RLCs in the period "after" cameras were used. Further, the IIHS study was only able to make statements suggesting favorable results from the use of RLCs due to the biased selection of sampled cities. The red light running fatality rate as well as the total fatality rate at all signalized intersections in cities that used cameras was higher in both the "before" and "after" time periods, which affirms that superior interventions exist. Also, we explain the IIHS' financial conflict of interest regarding photo enforcement.*

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## Background

In February 2011, the Insurance Institute for Highway Safety (IIHS) disseminated their study that concluded cities with red light camera (RLC) programs experienced a greater percent reduction in their red light running (RLR) fatality rate and, to a lesser extent, in their total fatality rate at signalized intersections, relative to cities that did not implement RLC programs (Hu, McCartt, & Teoh, 2011). In contrast to this IIHS conclusion, Langland-Orban, Large and Pracht (2011) published an analysis that summarized studies identified as the best designed RLC research in a National Highway Traffic Safety Administration (NHTSA) compendium (Decina et al, 2007). Most of these studies found that fatalities at RLC sites occurred in *larger* number than at comparison sites, thereby yielding conclusions directly contrary to the 2011 IIHS study. A major difference is that the studies, classified as best designed, had evaluated actual RLC sites and adjusted for traffic volume, whereas the IIHS study analyzed city-wide data, not specific to camera sites.

The controversy surrounding RLCs was disclosed in the Office of the Majority Leader's

Report (2001) entitled *The Red Light Running Crisis: Is It Intentional?* The report explained that when yellow light timings are correctly set at intersections, red light running is a relatively infrequent occurrence. However, for RLC programs to be profitable, it is necessary to shorten yellow light timings to create a larger "dilemma zone" where drivers cannot stop in time and hence receive a ticket for entering the intersection on a red light. Further, RLCs can encourage drivers to stop abruptly in attempts to avoid a ticket, which is a hazardous driving action that is known to increase rear end crashes. The Majority Leader's Report also explained that, in 1994, the Institute of Transportation Engineers (ITE) recommended that when red light running is a problem at an intersection, the yellow light timing can be lengthened (the prevailing standard), or alternatively, enforcement (tickets) can be used (a new provision). This change to permit enforcement was endorsed by the Federal Highway Administration in 2000, which allows for creating "dilemma zones" at signalized intersections that are associated with red light running and thus increase RLC tickets.

Because the IIHS findings on the association between RLCs and fatalities is contrary to the RLC

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studies classified as best designed in the NHTSA compendium, the 2011 IIHS study is reviewed here for adherence to basic research methods, which are required for valid conclusions to be drawn. It should be noted that a previous IIHS study on RLCs (Retting & Kyrychenko, 2002) was found to have used flawed research methods, as well as to incorrectly report findings, rendering the findings invalid (Burkey & Obeng, 2004; Large, Orban, & Pracht, 2008). In addition, the IIHS financial conflict of interest regarding photo enforcement is explained.

### Critique of the IIHS Methods

The IIHS analysis included 14 cities that used camera programs and compared them with 48 cities that did not. The “before period” was defined as the combined years of 1992–1996 when none of the 62 cities had cameras. The “after period” was defined as the years 2004–2008. The 14 “camera” cities were reported to have used RLCs at some sites throughout this five-year period, whereas the 48 comparison cities never used RLCs.

The IIHS study developed two Poisson regression models. The first model, reportedly, used red light running (RLR) fatalities per 100,000-population as the outcome measure (dependent and continuous variable). The second model reportedly used fatalities per 100,000-population at signalized intersections (also a continuous variable). The use of the word “reportedly” is explained below in facts 4 and 5. The determinants (independent variables) used to estimate each outcome were as follows:

- Land area in square miles
- Thousands of persons (population) per square mile
- “After” camera period (0 = 1992-1996; 1 = 2004-2008)
- Camera cities (0 = never used cameras; 1 = cameras in 2004-2008)
- Interaction between “after” camera period and camera cities

Five facts are immediately apparent about the research design, which jeopardize the internal and external validity of their findings.

- (1) The Poisson regression models excluded variables (determinants) known to be associated with traffic fatalities, such as changes in public policies or engineering improvements made during or between the study periods. For example, some states, e.g., Florida, repealed their motorcycle helmet law between the two time periods, which was associated with increased fatalities. It is noteworthy that the Federal Highway Administration’s RLC study (Council et al., 2005) suggested that

fatalities should be ignored in RLC analyses because they are an infrequent outcome and result from issues associated with “*occupant age, restraint use, and the type and size of vehicles involved.*” The IIHS study likewise did not consider these FHWA-cited factors, which are associated with fatalities. By excluding factors known to be associated with fatalities, the IIHS study likely suffers from omitted variables bias (i.e., under-specification). Unless the excluded variables were statistically independent from those that were included, the influence of the former will be, incorrectly, attributed to the latter.

- (2) Two of their explanatory variables (“land area” and “persons per square mile”) are not established factors associated with motor vehicle crashes or fatalities and have not been used in other RLC research. Whereas variable selection is to some extent subjective, a clear theoretical explanation for a variable’s inclusion must be provided. If no theoretical basis exists for including variables, they should be left out of the model since their inclusion can alter (distort) the findings. This is especially true because the authors found no statistical significance for these factors.
- (3) Assuming that the dependent variable for each model is defined as reported, some variables are included more than one time, making them redundant. For example, population is the denominator in both outcome measures reported (e.g., fatalities per 100,000-population), as well as a numerator in the variable “population per square mile.” Further, land density is an independent variable and is also used in a second independent variable “population per square mile.” This introduces the problem of multi-collinearity, meaning variables used in the model are highly correlated and coefficient estimates, such as the association between cameras and fatalities, can be inaccurate as a consequence.
- (4) The authors report their dependent variables as rates (e.g., fatalities per 100,000-population), which are defined as continuous. They then proceed with Poisson regression which is designed for count data (not rates). If these first four points appear confusing, it stems from the authors’ erroneous use of variables and descriptions. The review will proceed assuming that the dependent variables were actual counts.

- (5) If the use of a Poisson regression is appropriate and the dependent variable is indeed a discrete number (i.e., count data) then the correct interpretation of the estimated coefficients is “a change in the number of fatalities, holding the population density and land area constant.” This is, of course, qualitatively and quantitatively different from a *percentage change in the rate* of such fatalities. Examination of the “before” camera period data illustrates the importance of this distinction. About 93% of all camera cities had at least seven fatal RLR accidents in the 1992-1996 period, with most having substantially higher numbers. In contrast, 56% of the non-camera cities had six or fewer RLR fatalities. The authors of the IIHS study ignored the fact that the non-camera cities had substantially fewer RLR related fatalities in the “before” period, when cameras were not used in any of the 62 studied cities. Of even greater impact, 23% of the non-camera cities had two or fewer (including zero) such accidents. Because no city can improve its fatality rate if it is already zero, the simple fact is that the “number” in particular, and by extension the “percentage change in the rate,” of fatalities had much less room for improvement in the non-camera cities.

### Review of the IIHS Findings

Ignoring the obvious problem relating to the use of “percentage change in rate” as discussed above, the following provides a more detailed review of the results and interpretations. Table 1 provides a portion of Table 1 from the IIHS results. The IIHS reported that cities using RLCs had a larger percent reduction in red light running fatality rates. This misrepresents their findings. Cities that used RLCs had a substantially higher rate of red light running fatalities in both time periods, “before” and “after” camera use, relative to cities that did not use RLCs (point 5). Similarly, cities that used RLCs had a higher fatality rate at signalized intersections than cities that did not use them, in both time periods. Cities starting from a higher absolute base can show greater relative improvement than those already performing well, even in the absence of an intervention, hence the larger percentage rate change in the “camera cities.”

Table 1 also reveals that the IIHS method for selecting comparison cities violated research methods since the comparison cities (no RLC use) averaged much lower fatality rates in the “before” period, relative to camera cities. Scientific research methods require that the comparison group is

selected to be similar to the treated group, in this case “camera cities.” The fact that the two groups have a large difference in fatality rates in the “before” period reflects bias in the selection of the comparison group, which jeopardizes the validity of the findings (Campbell & Stanley, 1963).

Further, the IIHS included cities that had extreme fatality rates. Extreme rates, whether high or low, may regress toward the mean (the average) absent any intervention, meaning high rates may fall over time while low rates may rise. Extreme rates may also indicate the presence of factors that are unique to the particular observations. For example, within the context of the IIHS study, an extremely high rate in a particular city may be explained by yellow light intervals that are systematically shorter on average compared to the remaining cities in the analysis.

Scientific research methods allow for including sites with extreme rates; however, the comparison sites must be selected to be similarly extreme, whether high or low (Campbell & Stanley, 1963). Instead, the extremes are dissimilar because 26 (54%) of the 48 comparison (no-camera) cities have RLR fatality rates per 100,000 population that were less than 0.4 in the “before” period; two of which had a rate of zero, making a reduction impossible. In contrast, only one of the 14 cities using cameras (7%) had a rate less than 0.4.

A specific case in point of an extreme was Phoenix, a camera city. Its status as an outlier is illustrated by the fact that it had an RLR fatality rate of 1.82 per 100,000 population in the “before” period, which is almost four standard deviations above the average of the sample. The Phoenix rate declined to 1.01 in the “after” period. The inclusion of Phoenix reflects further selection bias due to its extremely high fatality rate, which is then compared with cities that already have low fatality rates and then ultimately reporting the findings as percent changes only. According to the IIHS study results, Phoenix had a 45% decrease in its red light running fatality rate. It is noteworthy that the average “before” rate in the non-camera cities was 0.4, or, stated differently, the Phoenix “before” period rate was over 300% higher compared to the non-camera city average. Given its starting position, interpreting a 45% decrease as evidence of RLC effectiveness is faulty because the fatality rate of 1.01 in the “after” period is still extraordinarily high and 2.7 standard deviations higher than the sample average.

The dynamic of bias described above permeates the interpretation of the regression results as revealed in Table 2, which replicates Table 2 from the IIHS study, reporting the results from their statistical analysis of RLR fatalities per 100,000 population.

The IIHS researchers wrote the following about the cities: “*The rate of fatal red light running crashes between 1992-96 and 2004-08 was reduced by an estimated 16 percent ( $[\exp(-0.1709)-1]\times 100$ ) for cities without camera programs and by an estimated 36 percent ( $[\exp(-0.1709-0.2809)-1]\times 100$ ) for cities with cameras.*” This is a favorable presentation of the findings, but distorts actual results because the authors excluded the estimate for the “camera cities” variable, which is both large and positive.

Table 3 presents our interpretation of their results and summarizes the percent differences, estimated relative to the base case. The base case is the “before” period in cities not using cameras. Thus, cities using cameras had an estimated 65% higher rate of red light running fatalities in the “before” period. This extreme starting position undoubtedly impacts the ending position. Cities not using cameras had an estimated 16% decrease in the RLR fatality rate in the “after” period, despite the fact that some started with a rate of absolute zero. Both of these findings are, nonetheless, correctly reported in the IIHS report. However, cities that used cameras had an estimated 5% higher fatality rate in the “after” period relative to the base case, a finding not reported by the IIHS researchers, albeit this difference is unlikely to be statistically significant.

The important question is what was the difference between cities using vs. not using cameras in the “after” camera period? The “after” period estimate (-0.17) is the same for both groups. Thus, the difference between the two groups is the “cities with cameras” estimate (0.4998) plus the “interaction” estimate (-0.28). Thus, cities using cameras are estimated to have a 25% higher red light running fatality rate  $[(\text{EXP}(0.4998-0.28))-1]$  in the “after” period relative to cities not using cameras, despite the greater reported percent reduction in the former. The authors’ incorrect conclusions were based on the interaction effect only and not the result from both the main effect (cities with cameras) and the interaction effect.

### Understanding the IIHS Conflict of Interest

The IIHS is supported and funded by automobile insurance companies and associations, and their financial interest in traffic tickets and ambivalence toward lowering crash costs were explained nearly 50 years ago by Ralph Nader, an expert on traffic safety. In Nader’s landmark book, *Unsafe at Any Speed: The Designed in Dangers of the American Automobile*, a chapter titled “*The traffic safety establishment: Damn the driver and spare the car,*” explained the IIHS interests. Nader (1965) described the IIHS as part of a private “traffic safety

establishment,” which was focused on defending business interests and profits, while subjugating evidenced-based interventions that reduce injuries and fatalities. He wrote, “*Under existing business values, potential safety advances are subordinated to other investments, priorities, preferences, and themes designed to maximize profit.*”

Nader (1965) described the private “traffic safety establishment” as focusing exclusively on driver behavior, instead of engineering improvements that are associated with the prevention of crashes and injuries. Nader explained that crashes resulting from engineering defects of automobiles can be imputed to drivers, which is also true of roadway engineering defects, such as yellow light timings that are set too short, thereby forcing red light running. Nader explained the myopic focus of the private “traffic safety establishment” as follows:

*Today almost every program is aimed at the driver – at educating him, exhorting him, watching him, judging him, punishing him, compiling records about his driving violations . . .*

Although published in 1965, this is an accurate description of photo enforcement programs.

Nader also explained why automobile insurance companies are ambivalent about reducing crash costs. First, insurance companies are able to gain approval from state regulators to raise insurance premiums to cover higher losses, making them indifferent about loss prevention, since increased losses justify increased premiums, passing higher crash costs on to drivers. Second, automobile insurance companies earn more profit from investment income (investing premiums collected from drivers) than from underwriting activities. Thus, higher premiums produce more money to invest and hence more profit for insurance companies.

The importance of these principles is evidenced in the average automobile insurance rate change that occurred nationwide in 2009. From 2004 to 2008, the annualized rate of premium increase was about 3.2%, consistent with inflation (U.S. Department of Labor, Bureau of Labor Statistics, 2008). In 2008, the real estate bubble burst and the U.S. stock market crashed, with average stock market returns being down over 37% (Anspach, 2011). This loss may explain the large increase in automobile insurance premiums in 2009, which jumped by double digits across all states. For example, average automobile insurance rates in Florida increased by 58%, averaging \$1,055 in 2008 and \$1,668 in 2009 according to [www.insurancelevel.com](http://www.insurancelevel.com) (2010) and

**Table 1. Table 1 from the IIHS Study - Average Annual per capita Rates of Fatal Red Light Running Crashes and All Fatal Crashes at Signalized Intersections for Cities with and without Red Light Camera Enforcement Programs, 1992-96 and 2004-08**

	14 cities with camera programs			48 cities without camera programs		
	1992-96	2004-08	Percent change	1992-96	2004-08	Percent change
Average annual population (million)	9.02	10.08	11.7	17.07	19.08	11.7
Average annual rate of fatal red light running crashes per million population	7.16	4.66	-34.9	4.79	4.10	-14.4
Average annual rate of all fatal crashes at signalized intersections per million population	16.38	14.02	-14.4	13.02	13.27	1.9

**Table 2: Table 2 from the IIHS Study - Poisson Model of the Effects of Red Light Camera Enforcement on Average Annual per capita Rate of Fatal Red Light Running Crashes**

Parameter	Estimate	Standard error	p value
Intercept	1.7050	0.1547	<0.0001*
Land area in square miles	0.0001	0.0003	0.6391
Population density (thousands of persons per square mile)	-0.0371	0.0191	0.0527
After period (2004-08) vs. before period (1992-96)	-0.1709	0.0678	0.0117*
Cities that implemented red light cameras vs. cities that did not	0.4998	0.1436	0.0005*
Interaction of study period and city group	-0.2809	0.1079	0.0092*

**Table 3: Summary of Percent Differences**

Estimate	-0.17	0.4998	-0.28		
	Before = 0	No Camera = 0	After & camera = 1		
Period	Cameras	After = 1	Camera = 1	All other cases = 0	% Difference
Before	No	0	0	0	
Before	Yes	0	0.4998	0	0.65
After	No	-0.17	0	0	-0.16
After	Yes	-0.17	0.4998	-0.28	0.05

www.CarInsurance.com (2011). Thus, auto insurance rates jumped subsequent to insurance company investment losses, suggesting the increase may have occurred to achieve return on investment expectations, and not due to a large increase in individual risk relative to crashes and injuries.

This supports Nader's assertion that higher losses are simply passed on to drivers in the form of higher premiums, as there was no large increase in individual risk. Instead, traffic fatalities had declined. The National Highway Traffic Safety Administration (2010) reported that fatal crashes declined between 2007 and 2009. For example, the number of fatal motor vehicle traffic crashes declined from 34,172 in 2008 to 30,797 in 2009, representing an almost 10% drop. In addition, the rate of fatalities per 100,000 population declined from 1.26 to 1.13. The drop in fatalities is in accordance with the reduction in miles traveled that followed the recession: billions of miles traveled declined from 3,032 to 2,979 in respectively, 2007 and 2009. These data cast serious doubt on the notion that premiums increased in response to increased risk.

Further, automobile insurance profitability had increased prior to 2004 after the insurance industry developed new pricing tools in 2000. Instead of categorizing drivers into four or five tiers for underwriting purposes, insurance companies began using thousands of factors to determine a driver's rate (Oster, 2004). A proliferation of traffic tickets, via photo enforcement, creates a proliferation of factors (tickets) to use in underwriting that justify premium increases. Hence, the automobile insurance industry has a financial interest in advocating for photo enforcement.

Tickets can result in multi-year automobile insurance increases from surcharges due to points on a driver's license *and/or* from underwriting penalties. In Florida, a state insurance specialist explained that RLC tickets can be used in underwriting, similar to other tickets, to increase a driver's automobile insurance rate, even though the tickets do not add points to a driver's license (Rick Lunsford, personal communication, July 29, 2011). It was noted that practices vary among insurance companies, such that drivers would need to contact their own company to ascertain the percent increase and duration (years) of any penalty from a camera ticket.

It is not surprising that public officials can be misinformed about the relative effectiveness of traffic tickets and photo enforcement in reducing crashes. Ralph Nader (1965) had also explained that the private "traffic safety establishment" has inserted themselves as educators to public officials and law enforcement regarding traffic safety. He explained that the merging of public and private funds is a

*"recurrent practice in the traffic safety establishment and assures the participation of industry people directly in official programs."* This continues today, as evidenced by agendas from the Governors Highway Safety Administration (GHSA) annual meetings. The GHSA membership includes highway safety representatives from each state. Insurance companies and camera vendors participate as associate members (Governors Highway Safety Administration [GHSA], 2011), and IIHS representatives and other special interests participate as speakers. For example, in 2010, a representative from the *Partnership for Advancing Road Safety* presented "Automated Enforcement: We've Got Your Number" (GHSA, 2010), which is an organization funded by traffic camera vendors (thenewspaper.com, 2010). Whereas free speech laws permit such presentations, it illustrates how the process allows proprietary interests to influence public officials, apparently absent disclosures of financial conflicts of interest.

### Conclusions

The 2011 IIHS study actually found that cities that used cameras had noticeably higher red light running fatality rates than cities that did not use cameras in both "before" and "after" time periods. This finding was also true regarding the total fatality rate at signalized intersections. This suggests other interventions were more effective in lowering fatality rates at signalized intersections. However, the authors of the IIHS study did not cite these findings. Further, the extremely high rates of red light running fatalities in the "after" period in both Phoenix (1.01 per 100,000 population) and Bakersfield (1.06 per 100,000 population), which used cameras, are evidence that other interventions may prove particularly effective in these cities if ever implemented, as comparison cities (no-camera) averaged 0.41 per 100,000-population in the "after" period.

Further, the impropriety of the IIHS research approach, which uses cities as the unit of analysis instead of RLC sites, is evidenced by the Washington and Shin (2005) analysis of the 10 RLC sites in Phoenix. Washington and Shin (2005) analyzed crashes and injuries at RLC and comparison sites in Phoenix, and adjusted for traffic volume. They concluded: (1) total crashes did not change at RLC sites; (2) the net safety benefit was negligible since RLCs were *not* associated with reducing injuries or fatalities; (3) spillover effects were not found; they wrote: *"the findings may suggest motorists are aware of which approaches have cameras and which do not;"* (4) the RLC sites had a *higher* percent of fatal angle crashes, relative to comparison sites, in the "after" period; and (5) the cost of

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fatalities was excluded from the economic analysis (meaning the negligible safety benefit that was reported is incorrect because the higher fatal crash costs at RLC sites were excluded from the economic analysis). As the Washington and Shin (2005) analysis has revealed, analyzing only RLC and comparison sites within a community produces contrasting results than analyzing aggregated data from all signalized intersections within a community (as done by the IIHS).

Meanwhile, the U.S. PIRG (2011) has published recommendations regarding RLCs that are designed to advance the public's interests when government entities consider camera programs. These recommendations can also be used to evaluate existing RLC programs to assess adherence and, thereby, determine if cameras are used for public safety or for advancing private business interests.

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Barbara Langland-Orban ([borban@health.usf.edu](mailto:borban@health.usf.edu)) is Associate Professor, Etienne E. ([epracht@health.usf.edu](mailto:epracht@health.usf.edu)) is Associate Professor, and John T. Large ([jlarge@health.usf.edu](mailto:jlarge@health.usf.edu)) is Assistant Professor in the Department of Health Policy and Management, University of South Florida College of Public Health, Tampa, FL. This paper was submitted to the *FPHR* on November 8, 2011, and accepted for publication on January 12, 2012. Copyright 2012 by the *Florida Public Health Review*.