A Response to Unfounded Criticisms of Burkey and Obeng (2004) Made by the IIHS

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The first two pages serve as a brief response to the false allegations by the IIHS (via Richard Retting and Sergey Kyrychenko). Details and documentation for each statement follow this brief introduction.

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Brief Response

It is surprising that an institution such as the IIHS resorts to name-calling when a project reports data that go against their deeply-held beliefs. If there are valid limitations of our work, we made every effort to point them out in the report. We welcome any additional valid criticism and suggestions. However, Retting and Kyrychenko (R&K)'s comments admit to one of only two explanations: 1) They are purposefully distorting our methods, or 2) They did not read and understand the methods and careful robustness checks that were done. They bring up three main objections to our work:

1) *"…Burkey and Obeng treated data from intersections with and without cameras as if the cameras had been randomly assigned to their locations. In fact Greensboro officials installed cameras at intersections with higher crash rates…"* Status Report 2005

FALSE. It is impossible for our report to give this impression. We dealt with this issue at length, and modeled it in two specific ways. First, we included many variables that account for these differences in crash rates. Second, in order to make sure that we had accounted for any additional unobserved heterogeneity, we ran a Fixed Effects model. This method tracks each intersection individually, allowing the Red Light Camera (RLC) variable to pick up only the effect of the RLC placement relative to the accident rate at these *same intersections* (see last page of the report). The model R&K use with contrived data is in no way related to our methods, and the data does not reflect the data in the tables in our report. Their suggestion of a RLCGROUP variable is unacceptable, and is demonstrated to give incorrect results in this document. Additionally, Greensboro officials installed cameras at both high and low accident locations. Many high accident locations did not receive RLCs. We discuss the non-random, non-experimental nature of our data at length in the report, and discuss its shortcomings.

2) *"Publicity and media coverage generally make drivers aware that a city is using red light cameras, not specifically which intersections have cameras. … By ignoring the spillover effect, the authors could obtain only a biased (low) estimate of red light camera effectiveness."* R&K 2004

FALSE. *NCGS § 160A-300.1b: Any traffic control photographic system installed on a street or highway must be identified by appropriate advance warning signs <u>conspicuously</u> posted not more than 300 feet from the location of the traffic control photographic system.* In other words, drivers in North Carolina know exactly where the cameras are located. Additionally, we did not ignore the possibility of spillover effects, and discussed this in the report. The spillover effect is well-known, but far from well-documented. Indeed, the *primary effects* of RLCs are still not well-understood. The IIHS often cite two of Retting's studies as evidence of a spillover effect, which looked at 5 intersections for 24 hours to look for spillovers. We geocoded Greensboro's intersections preparing to explicitly test for spillover effects using spatial correlation over time. If spillover effects exist, they should be stronger at intersections closest to the RLC sites since they are clearly marked. However, when many different (and appropriate) modeling techniques failed to show a benefit, measuring the spillover effects of the nonexistent benefits appeared to be a moot exercise. However, we tested for spillover effects several ways, and found nothing.

3) Additionally, they state that our **conclusions** were not reviewed by peers. They say that the purpose of peer review is to provide a "seal of competence" and that it tells us "These findings

are worth paying attention to." The purpose of peer review is **not** to review conclusions, but methods and clarity. Science is not about whether or not one is happy with the answer; rather, it is about trying to discern whether or not the investigator is honestly and intelligently searching for the truth. Of course, peer review is not a guarantee of this, but should be encouraged.

Our work has not been peer reviewed simply because enough time has not elapsed since finishing the initial report. In my profession we generally issue reports, get important feedback and comments, present our work at conferences, and then submit our work for formal "peer review" and publication. The process of peer review through publication can take many years, and lack of peer review is not a valid criticism of anyone's recent work. Our methods are now being peer reviewed. Attempts by the IIHS staff to subvert this process are unprofessional.

In our study, the accident rates at intersections without RLCs went down much more than those with RLCs, continuing a long-term decreasing trend in accidents in Greensboro, NC. We address how this cannot reasonably be attributed to spillover effects in our report. We believe that we reasonably concluded, "At a minimum, we can say that *there is no evidence that the RLC program is decreasing accidents*. Additionally, the data shows that the sites with RLCs are not benefiting from the overall *[long-term]* decreasing trend in accidents in Greensboro." (p. 48)

From looking at the data, the question should not simply be *whether* RLCs work, but when they may work and when they may not. From Table 4.1 in the report, simple before/after comparisons of accident rates at the 18 RLC sites in Greensboro show anywhere from a 36% decrease in accidents at one intersection, to a 57% increase in accidents at another. We need to design careful studies to examine whether these differences are random, or if some intersections can really benefit from RLCs. The IIHS wants you to believe that RLCs are always appropriate and will always reduce accidents at all intersections, including those without RLCs. If you question this conclusion the IIHS will label your work "incompetent junk science". Real scientists who are objectively looking for the truth do not behave in this way.

There are flaws in all studies on the efficacy of RLCs, including ours. But the ones invented by Retting and Kyrychenko are simply not true. We continue to call for more careful studies of RLCs, because most scientists are simply not convinced either way. If we truly care about reducing accidents, we will continue to plan and execute more careful studies. Until we truly understand what will happen when a RLC is placed, we should be cautious about using them.

Please read the details that follow if you want additional documentation of each of the statements above. Consult researchers who do not have an agenda. Read any of the good comprehensive reviews of RLC literature by McFadden and McGee (1999), Maccubbin, Staples, and Salwin (2001), McGee and Eccles (2003), or Milazzo, Hummer, and Prothe (2001). Come to your own conclusions. Demand convincing evidence from people who demean those who disagree with them, and from anyone who wants to convince you that there is a clear and simple answer.

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Supporting Details[1]

Let me first apologize for the length of what follows, but the response to these unsubstantiated claims must be as clear as possible. I will try to make this response as clear as possible for both the scientific community and the body of public officials and engineers. I will try to avoid as much jargon as possible, but technical details are necessary. I will refer the reader to additional reading material in such cases. My goal here is neither to convince you that my study is perfect nor to demonize Retting and Kyrychenko (R&K). However, it is important to realize the lengths they have gone to in an attempt to unfairly censure the hard, competent work we did.

I will now rebut the points made by Retting and Kyrychenko about the report, and then delve into some other details. Please take Burkey and Obeng (2004) for what it is—a competent, honest look at the data, albeit with many real-world limitations on the data that are fully disclosed in the report.

I. Just Look at the Data!

First, let us look at the data. Even if one disagrees over methods, the numbers should convince you that the impact of RLCs is not clear. Making simplistic tables is difficult for this data because the RLCs in Greensboro were phased in between the 27th and 36th month of our 57-month study Jan. 1999-Sept. 2003. First, let me reproduce Table 4.1 from our study, which focuses only on the 18 intersections where RLCs were placed.

The crash rates are "normalized" to a rate per 10 months to make comparisons a little easier. For example, at site #01, the camera was placed toward the end of the 26th month of the study (February, 2001). So we observe 26 months of data before the installation, and then 31 months after the installation. For this one intersection, there were 61 accidents reported in the first 26 months and 72 during the remaining 31 months. This is a monthly rate of 2.346 accidents per month before and 2.323 after installation. We then multiply these numbers by 10 to get an accident rate per 10 months. In total for these 18 intersections there were 841 accidents observed before RLC placement and 778 after; 527 intersection-months before and 499 after gives the overall 2.5% decrease in the rate we saw at these intersections.[2] If Red Light Cameras are having a beneficial effect on accident rates, it is very small. Additionally, if more of the RLCs were placed at high accident locations, then the regression to the mean effect should cause some of these intersections to experience a reduction in accident rates naturally. However, this is not generally borne out by the data.

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[1] In this document I use the words "I", and "my study" on occasion. The purpose is not to belittle the valuable contributions of Dr. Obeng and the student assistants on this project. Rather, as the principal investigator "I" am giving my personal response to the claims of the IIHS.

[2] Let me alert everyone that in double checking my numbers for this table, I discovered that I incorrectly divided the after RLC numbers by the (number of months observed - 2) due to misplaced parentheses in a formula. This unfairly inflated the "after" figures in this table. However, the "total" figures were correct, and the wide variation in results is still apparent. The corrected table appears in this document.

Table 4.1 : Before/After Statistics for 18 RLC Sites													
	RLC Sites: No RLC Normalized/10 months						RLC Sites: With RLC Normalized/10 months						
ID #	**FTL**	**AINJ**	**BINJ**	**CINJ**	**PDO**	**Total**	**FTL**	**AINJ**	**BINJ**	**CINJ**	**PDO**	**Total**	**%Chg**
01	--	--	2.31	10.00	11.15	23.46	--	0.32	1.61	8.71	12.58	23.23	-1.0%
02	--	0.38	1.92	9.62	6.54	18.46	--	--	0.32	8.71	8.39	17.42	-5.6%
03	--	--	2.69	1.92	7.31	11.92	0.32	--	1.61	2.58	6.77	11.29	-5.3%
04	--	0.38	1.15	4.23	10.00	15.77	--	0.32	1.29	8.71	9.35	19.68	24.8%
05	--	--	3.21	5.36	6.79	15.36	--	0.34	0.34	7.93	15.52	24.14	57.2%
06	--	--	1.85	7.41	7.41	17.04	--	0.33	0.33	7.67	8.67	17.00	-0.2%
07	--	0.37	1.11	6.30	1.85	9.63	--	--	1.00	4.67	2.33	8.00	-16.9%
08	--	--	0.36	6.79	10.36	17.50	--	--	1.72	3.79	6.90	12.41	-29.1%
09	--	--	0.33	4.33	9.33	14.00	--	--	--	1.48	9.26	10.74	-23.3%
10	--	--	--	1.03	1.03	2.07	--	--	--	0.36	1.07	1.43	-31.0%
11	--	--	2.07	6.55	12.07	20.69	--	--	2.14	5.36	6.43	14.29	-31.0%
12	--	--	0.67	5.33	6.33	12.33	--	--	1.11	3.70	7.04	11.85	-3.9%
13	--	0.32	0.65	4.52	6.13	11.61	--	--	0.77	4.62	9.23	14.62	25.9%
14	--	--	0.86	9.71	20.29	30.86	--	--	--	5.45	17.27	22.73	-26.3%
15	--	0.30	1.52	4.24	5.76	11.82	--	--	0.42	2.92	4.17	7.50	-36.5%
16	--	--	1.52	3.64	5.76	10.91	--	0.42	0.42	3.33	6.25	10.42	-4.5%
17	--	--	2.50	5.31	10.63	18.44	--	--	0.40	12.80	14.40	27.60	49.7%
18	--	0.32	1.61	7.74	13.87	23.55	--	--	1.15	13.85	11.92	26.92	14.3%
Total	--	0.11	1.44	5.78	8.63	15.99	0.02	0.10	0.84	5.95	8.66	15.59	-2.5%

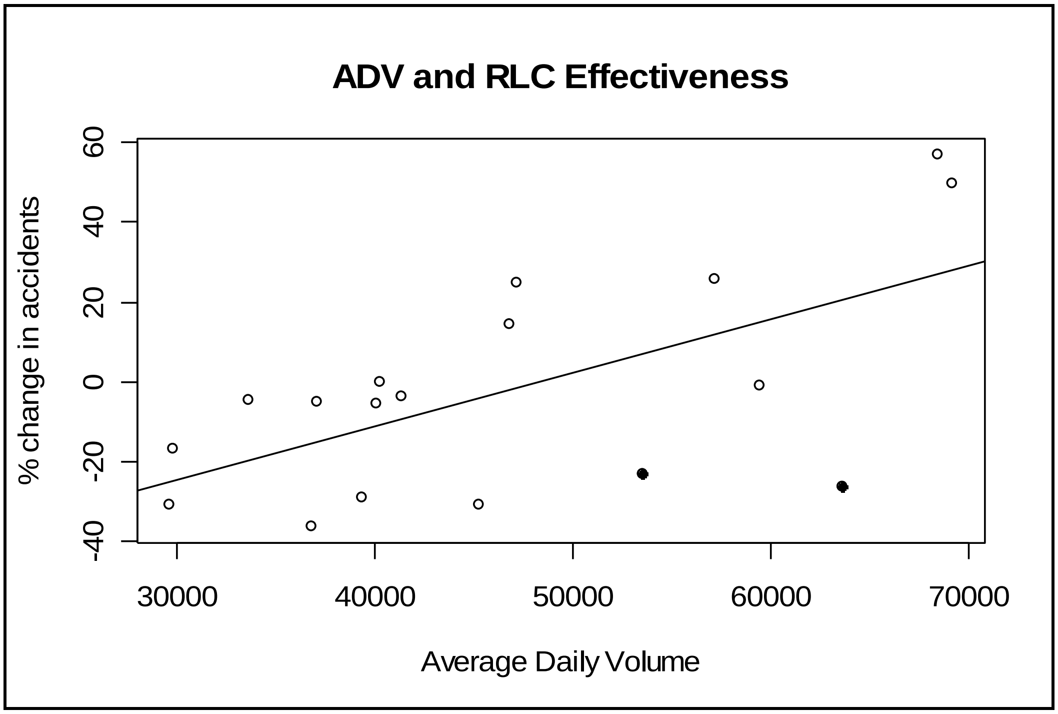
It is not possible to simply compare the experience at these 18 intersections with the overall trends experienced in the rest of the city because the cameras were installed over the course of 9 months (27^{th}-35^{th} month of the data). However, to get a sense of what was happening at intersections without RLCs, we can divide the data into two roughly equal periods:

Table 4.2: 18 RLC Sites Before and After the RLC Program: Common Types of Accidents												
	First 29 months of Data						Last 28 months of data					
CRASH_TYPE	FTL	AINJ	BINJ	CINJ	PDO	Total	FTL	AINJ	BINJ	CINJ	PDO	Total
REAR END, SLOW OR STOP	--	1	17	152	188	358	--	1	6	172	201	380
ANGLE	--	2	34	78	115	229	--	2	15	82	124	223
LEFT TURN, SAME ROADWAY	--	1	7	29	44	81	--	--	5	16	26	47
SIDESWIPE, SAME DIRECTION	--	--	2	4	37	43	--	--	--	10	37	47
LEFT TURN, DIFFERENT ROADWAYS	--	--	6	13	15	34	--	--	2	1	3	6
REAR END, TURN	--	--	--	5	10	15	--	--	1	1	11	13
TOTAL (including omitted categories)	--	6	78	302	454	840	1	5	40	299	432	777

Table 4.3: 285 Control Sites Not Chosen for RLC Installation												
	First 29 months of Data						Last 28 months of data					
CRASH TYPE	FTL	AINJ	BINJ	CINJ	PDO	Total	FTL	AINJ	BINJ	CINJ	PDO	Total
REAR END, SLOW OR STOP	--	2	41	600	728	1371	--	2	42	570	663	1277
ANGLE	4	22	178	581	857	1642	4	12	168	609	832	1625
LEFT TURN, SAME ROADWAY	2	9	69	231	338	649	--	5	37	117	175	334
SIDESWIPE, SAME DIRECTION	--	--	8	33	211	252	--	--	4	30	235	269
LEFT TURN, DIFFERENT ROADWAYS	1	2	19	76	131	229	--	--	9	38	67	114
REAR END, TURN	--	--	1	22	35	58	--	--	5	18	23	46
TOTAL (including omitted categories)	10	46	406	1705	2660	4827	8	27	339	1538	2299	4211

Looking at the RLC intersections in table 4.1 we see a 2.5% decrease overall. For comparison, looking at the totals in tables 4.2 and 4.3 we see roughly a 7.5% decrease at the RLC sites and a 12.8% decrease at the non-RLC sites during the same time period. Could the 12.8% decrease be caused by spillover effects? Not likely, but this was discussed at length and tested for in many different ways in the original report.

Looking at tables 4.2 and 4.3 we see no real decrease in angle accidents (229 to 223), but see a moderate increase in rear-end accidents (358 to 380). During the same time period, the non-RLC intersections saw a decrease in rear-end accidents, and a much smaller decrease in angle accidents. Looking again at Table 4.1, we see some intersections that had large improvements in accident rates, and some with large increases. Apparently, Red Light Cameras have the potential to cause great harm as well as have great benefits as recently discussed in Persaud et al. (2004). My next (and last) project regarding Red Light Cameras will be to explore any systematic differences between the intersections that see a benefit, and those that experienced an increase in accidents. Let me give you a little insight into this now. The main variable that seems to account for these differences is traffic volume. The following graph illustrates this:



One can see a fairly clear pattern here, with a trend line displayed for reference. The more traffic volume on the road, the more likely RLC placement will be associated with an increase in accidents. Two clear outliers to the trend, labeled with black dots, appear to have high volumes but saw a decrease in accidents after RLC placement. What could account for this? It turns out that these two intersections are “T” intersections.[3]

This is the type of information that the die-hard proponents of RLCs should be providing. Of course, the information presented above doesn’t constitute “proof” of anything, but represents good evidence suggesting that we need more serious study on this issue. This is particularly true given the tendency to locate RLCs at high volume intersections.

[3] Although one of them also involves a driveway into a hardware store.

II. Flaw #1, Ignoring that intersections are neither created nor chosen equally

"…Burkey and Obeng treated data from intersections with and without cameras as if the cameras had been randomly assigned to their locations. In fact Greensboro officials installed cameras at intersections with higher crash rates…" Status Report 2005

R&K *do not* mention that this accusation should actually cause us to find a much larger improvement at the RLC sites due to "regression to the mean bias"[4]. However, many high and low accident locations were chosen for RLC placement. Many high accident locations were *not* chosen for placement. The table below illustrates this. Out of the 35 intersections with the highest number of crashes in the first 24 months or our data (the RLC program began in 27th month), 11 of the 18 RLCs were placed at them. Out of the top 15 high accident locations, five were eventually chosen for RLC placement. Still, we did not treat the RLC locations as randomly selected, as this would obviously be improper.

Intersection Rank, First 24 Months of Data

Rank Order	Accident Count	RLC SITE?	Rank Order	Accident Count	RLC SITE?
1	78	Y	19	40	Y
2	62	N	20	39	Y
3	59	Y	21	38	Y
4	54	N	22	37	N
5	54	Y	23	35	N
6	51	Y	24	35	N
7	51	N	25	35	N
8	49	N	26	34	N
9	47	N	27	33	N
10	47	N	28	32	N
11	46	N	29	32	N
12	46	N	30	32	N
13	45	N	31	31	N
14	44	N	32	31	N
15	43	Y	33	30	N
16	43	Y	34	30	N
17	42	Y	35	30	N
18	41	Y			

We took several measures to control for differences between the 303 intersections in the study, including controlling for any differences that we might not be able to measure, called "unobserved heterogeneity". As an economist, the type of statistical techniques I use, called "econometrics", are designed for controlling for differences in individual subjects, even unobserved differences. We realize that real world data is non-experimental. These issues are discussed at length in the report. We repeatedly cautioned readers not to literally interpret the results because of the non-experimental nature of the data, and because no one knows the "perfect" model to use to explain accidents.

[4] For example, see Persaud et al. (2004) for a discussion.

We worked with local officials to carefully measure the characteristics of the 303 intersections in our report. We focused on collecting data on those characteristics that would help explain accident rates regardless of whether a RLC is present. We used amber timing, all red clearance intervals, speed limits, traffic volume, number of traffic lanes, signage, and many others. Properly including these factors in the analysis do not allow the intersections to be treated as identical. The result is that, unless an unknown, important variable was omitted:

***Only* the part of the relationship between RLCs and accident rates that cannot be accounted for by all of the other explanatory variables will be attributed to the RLCs.**

We then took the most conservative approach to ensure that our results were not due to omitted variables. We ran many other models that would pick up any problems due to differences between intersections that our explanatory data could not account for (i.e. unobserved heterogeneity). The most complete way do this with panel data is to run a *fixed effects model*.[5] Basically, this model considers each intersection as an individual, marking each with a dummy (categorical) variable.

R&K showed ingenuity by making up some data and an oversimplified linear model to "prove" how incompetent our analysis was. A lesson I carefully teach in my statistics classes is that one can always find a model that will seemingly "prove" anything you want. However, such a model will not stand up to a "smell test" by a competent econometrician. Let us analyze the imaginary data that R&K created with proper techniques (like those used in our report), which are NOT the techniques they suggest.

Table 2 from R&K(2004) from Imaginary City

Month	Acc. Int. A	Camera A	Acc. Int. B	Camera B	Acc. Int. C	Acc. Int. D
1	50	N	48	N	20	22
2	56	N	53	N	18	24
3	46	N	44	N	19	21
4	50	N	44	N	20	20
5	43	N	47	N	19	17
6	42	N	46	N	18	17
7	44	N	30	Y	18	16
8	43	N	29	Y	20	17
9	28	Y	27	Y	17	20
10	26	Y	27	Y	18	16
11	28	Y	25	Y	19	15
12	27	Y	26	Y	18	19

[5] We also ran the closely related "random effects model". See any good econometrics book, e.g. Econometric Analysis of Cross Section and Panel Data, Wooldridge, 2002. See the last page of Burkey and Obeng (2004) where we discuss some of the additional models we ran to verify our results.

I rearranged the table a bit to make it clearer for discussion. There are four intersections, A through D. RLCs are placed at A in month 9 and B in month 7. R&K correctly show that if they run an incorrect model, they get meaningless results. If you try to "explain" the accident rate using only the month and whether a RLC is present, the fact that RLCs are placed at high accident locations will make it appear that RLCs are *causing* the increased accidents. This is because of a high simple correlation between accident rates and the RLC variable.

Would anyone seriously attempt to use such a model? Certainly not anyone trained in linear models. It is hard to believe that R&K actually think that this is the model we presented. If we run the incorrect model they claim that we used (on the REAL data from Greensboro, NC-- however running a Poisson model rather than linear) we get the following coefficients:

Model R&K Claim We Used, Greensboro Data

Variable	Coefficient	Std.Err.	b/St.Er.	p value
Constant	-0.37265853	2.30E-02	-16.211	0
MONTH	-5.39E-03	7.15E-04	-7.541	0
RLC	**1.05048833**	4.51E-02	23.287	0

Indeed, this model does give incorrect results, picking up the fact that the RLC intersections have higher accident counts, "collapsing two effects" as R&K suggest. This makes the coefficient on RLC 1.05 rather than the 0.349 estimated in Burkey and Obeng (2004) for total crashes. The coefficient of 1.05 in a Poisson model would indicate an enormous difference between the number of accidents at RLC intersections and non-RLC intersections.[6]

In Burkey and Obeng (2004) we felt that there was a chance that we were not truly capturing all of the reasons why some intersections have higher accident rates than others by including our explanatory variables. In order to make sure, we ran the (*Poisson or negative binomial*) *fixed effects* model described earlier. In its simplest form, such a regression model includes a variable that keeps track of each intersection individually. In Imaginary City, for these four intersections we need three such variables, which will represent the difference in accident counts between the four. One of the intersections will serve as the reference category (say, intersection D), and the three variables will represent the difference in the average rates between (A and D), (B and D), and (C and D). These variables are known as dummy variables because mathematically they will be entered into the data set as either zero or one. Note: these results use the more proper Poisson model:

Output from Imaginary City: Fixed Effects Model

Variable	Coefficient	Std.Err.	b/St.Er.	p value
Constant	3.0620	0.0946	32.3850	0
Month	-0.0212	0.0107	-1.9830	0.0473
RLC	**-0.4132**	0.0975	-4.2390	2.24E-05
A	0.8775	0.0848	10.3440	0
B	0.8612	0.0913	9.4300	0
C	0.0000	0.0945	0.0000	0.999

Here we see what the data tell us: a small decreasing time trend, RLCs reduce accidents (from the -0.4132 coefficient on RLC), the accident rate at B is roughly the same as A, but both are

[6] More technically, it would represent the difference between RLC intersections after RLC installation and all other intersections, including RLC intersections before RLCs were installed.

larger than C and D. The coefficient on C (0.000) picks up the fact that C and D's accident rates are equal (for a zero difference). Intersection D does not have a coefficient estimate, because D is picked up in the "Constant" term. The RLC variable has the proper sign and magnitude, because the "dummy" variables A, B, and C pick up any differences in accident rates between the intersections. The fixed effects estimation told the same basic story as reported in Burkey & Obeng (2004): No reduction in angle accidents and a large increase in rear-end accidents.

Retting and Kyrychenko's Suggested RLCGROUP Methodology WILL NOT WORK (Except on made-up data)

Let me again note that the Greensboro DOT did not place RLCs only at high accident locations, and many high accident locations did not receive RLCs.[7] R&K suggest that the proper model would be to include a dummy variable for all intersections that were selected for a RLC placement, as a group. This suggestion is improper because it assumes that all of these intersections are the same in terms of average accidents. If you look at Table 4.1, you see that there is a great deal of variation in the accident rates among RLC sites. Let me demonstrate the superiority of the Fixed Effects method over R&K's "RLCGROUP" methodology.

It makes no difference in R&K's Imaginary City, because they create data in such a way that the RLC sites are identical. Data analysis would be easy in such a world. Suppose that next door to Imaginary City, Fakesborough also has four intersections and places RLCs at two of them, in exactly the same manner as Imaginary City. However, in Fakesborough, Intersection A has a lower accident rate than Intersection B, but the RLC causes the same drop in the number of accidents. Here, we take 20 accidents from the previous numbers for Intersection A and add them to Intersection B to introduce some "real-world" heterogeneity.

Accidents in Fakesborough

Month	Acc. Int. A	Camera A	Acc. Int. B	Camera B	Acc. Int. C	Acc. Int. D
1	30	N	68	N	20	22
2	36	N	73	N	18	24
3	26	N	64	N	19	21
4	30	N	64	N	20	20
5	23	N	67	N	19	17
6	22	N	66	N	18	17
7	24	N	50	Y	18	16
8	23	N	49	Y	20	17
9	8	Y	47	Y	17	20
10	6	Y	47	Y	18	16
11	8	Y	45	Y	19	15
12	7	Y	46	Y	18	19

A properly done Poisson model will estimate a coefficient that reflects the proportional decrease in accident rates caused by RLC placement. These decreases are around 29% for Intersection B,

[7] See for example Reese, Phillip. "Rate of Red-Light Crashes Steady," The News and Record, March 3, 2002, B1.

and around 73% for Intersection A. Our estimate should reflect a proportion between these two. First, let's run the RLCGROUP methodology that R&K suggest:

R&K's RLCGROUP Method: Fakesborough

Variable	Coefficient	Std.Err.	b/St.Er.	p value
Constant	3.22206	0.07912	40.726	0.00000
MONTH	-0.0475	0.01069	-4.443	0.00001
RLC	**-0.06153**	0.09393	-0.655	**0.51200**
RLCGROUP	0.75047	0.06594	11.381	0.00000

The coefficient on RLC is much too low at -0.06153, and is not statistically significant. The Fixed Effects estimation:

Fixed Effects Method: Fakesborough

Variable	Coefficient	Std.Err.	b/St.Er.	p value
Constant	3.0680	0.0955	32.1370	0.00000
Month	-0.0221	0.0109	-2.0240	0.04300
RLC	**-0.3795**	0.0961	-3.9490	**0.00008**
A	0.1824	0.0961	1.8980	0.0577
B	1.2790	0.0869	14.7130	0.00000
C	0.0000	0.0945	0.0000	0.999

Here the estimate is much more reasonable (-0.3795), predicting a much larger drop in accidents due to RLCs. As previously mentioned, it is always possible to use a poor model with created data to get nonsensical results. Fixed effects models are not poor models, but the RLCGROUP suggestion obviously is.

R&K accuse our report of being "incompetent" "junk science", yet they should understand that we properly accounted for intersection heterogeneity as described in the report. They ought to know what a Fixed Effects model is,[8] and should be able to read in the study where we stated:

> **"***We ran fixed effects models dropping the intersection characteristics, since there was so little within site variation. The overall results remained unchanged." (P. 55)*

Please believe that we went to extraordinary lengths to try many different techniques as appropriately as possible. Every single technique we used gave the same indications about RLCs. The Fixed Effects model version of the paper is now under peer review.[9]

True, we did not use this method for presenting the results in the main body of the paper, because we found that the seventeen explanatory variables accounted for the differences between accident rates at intersections well enough, and we were also interested in examining those variables for the report.

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[8] I recommend Karlaftis and Tarko (1998) from *Accident Analysis and Prevention* as a good introduction and example if you are unfamiliar with these models.

[9] The fixed effects model was chosen for peer review because it is easier to describe and present results succinctly.

III. Flaw #2: Ignoring "Well-Known" Spillover Effects

R&K often assert, rather than prove that spillover effects <u>of RLCs on safety</u> are well-documented. The very design of the Oxnard study makes a powerful assertion: That installing an RLC in a town will improve safety at all signalized intersections, but will not affect non-signalized intersections. This should not be a maintained assumption, but is a testable hypothesis. This principle of "spillover effects" has not been adequately demonstrated with accident rates for red light cameras. Indeed, as discussed in the next section, the overall safety effect of red light cameras at intersections *with* red light cameras has not been convincingly demonstrated.

The IIHS regularly cites two studies with Retting as the lead author as evidence of a spillover effect on safety (Retting, Williams, Farmer and Feldman (1999(1)), Retting, Williams, Farmer and Feldman (1999(2)). Together, these two studies describe examining a total of 5 intersections without red light cameras for an average of 24 hours each (both before and after). They do not measure the effect on *safety*, but measure changes in the number of *red light running violations* at each intersection. The extent to which the data on 5 intersections can be considered proof of a general concept should be considered limited. In North Carolina all four legs of an intersection are clearly marked with large signs stating that a RLC is present. It is not clear *a priori* that a spillover effect will exist in such a situation.

We do not ignore spillover effects in the study. However, if we do not measure a beneficial effect, why focus on measuring the spillover of these (non)effects? In the raw data we observe that accident rates are decreasing much more at untreated intersections than at the RLC intersections. In regional science and geography there is a principle called "Tobler's Law". Tobler's Law simply states that everything is dependent on everything else, but the effect is stronger as things are closer. Unless one believes that the *spillover effects* are stronger than the *effect* of an RLC at its location, the data do not support the idea of spillover effects. However, we looked for a spillover effect of the RLCs on the overall accident trend in Greensboro, and found none. Lastly, if there really are spillover effects, then the increases in rear-end accidents should spill over as well as any benefits, perhaps resulting in a "wash" depending on the relative frequencies of the two types of accidents.

While data collection was in progress, we constructed a sophisticated model of spatial spillovers used in regional science and epidemiology, and geocoded the 303 intersections in our study for use with this model. The model allows the effects of cameras to spread to nearby intersections, and allows the strength of the effect to decrease with distance. I was somewhat surprised when we did not find any beneficial effects, because it made the idea of measuring the spillover of these effects moot. See Anselin (1998) for more information on these models. We are now investigating whether any useful results can be gleaned from this model.

IV. The effects of RLCs are still uncertain

R&K assert that our conclusions are obviously false because they contradict all other competent studies. Many good reports estimate very different effects from RLCs. How the program is implemented, what types of intersections are selected for RLCs, accompanying changes in signage, and education campaigns will likely be important factors that determine the outcome of an RLC program. In Greensboro (as in most places) the most common crashes at signalized intersections by far are angle accidents and rear-end accidents. If the results in Persaud et al. (2004) are correct, then on average we should expect to find an average 23.3% decrease in angle accidents and a 17.5% increase in rear end accidents.[10] Depending on the relative frequency of these accidents in a jurisdiction, the overall effects of the cameras at RLC sites could go either way. At the 18 RLC intersections in Greensboro there were 63% more rear end accidents than angle accidents during the study—not the ideal type of intersection for RLC placement. Note that in Persaud et al., one jurisdiction saw a small increase in angle accidents **and** an increase in rear end accidents, similar to our findings.

In the evaluation of Red Light Cameras (RLCs), many comprehensive literature reviews (e.g. McFadden and McGee (1999), Maccubbin, Staples, and Salwin (2001), McGee and Eccles (2003), Milazzo, Hummer, and Prothe (2001), and Persaud et al.(2004) have concluded that poor data and poor statistical analyses in almost all studies on the safety impact of RLCs leave us unsure whether these devices improve safety or not. It is simply impossible to look at all of the available data and be certain of the direction and magnitude of the effects. Thus, the claim that our study is flawed because it contradicts previous studies is simply meaningless.

The IIHS does its best to promote the studies of RLCs that support the efficacy of RLCs, and they have already spent a lot of effort denouncing our report and Andreassen(1995). Another recent report is a Master's Thesis from Nattaporn Yaungyai[11] at Virginia Tech, supervised by Drs. Hobeika, Collura, and Trani (April 30, 2004). It also finds no statistically significant reduction in crashes due to RLCs. Although I have not had the opportunity to examine the methods used in this report, I am certain that like all studies it is not perfect, nor does it deliver the final answer. I hope that the IIHS will refrain from issuing a report finding that Virginia Tech is full of incompetent scientists as well.

V. Other Issues

Let me quickly respond to a few additional issues and claims of R&K:

On page 6 of the 2004 report on our work, R&K state that we provide misleading information on population growth rates that should have been used in their report. I apologize if this is true. The Census Bureau's data are not adequate for R&K, and they prefer "official California estimates" from the California Department of Finance. However, they twist these numbers unjustly: "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*** slower pace**." (R&K (2004))** *(Emphasis not in original)*

[10] These percentages are a simple average of the results presented in Table 5, page 11, and may not accurately reflect a proper weighted average of their data.
[11] http://scholar.lib.vt.edu/theses/available/etd-06242004-230619/unrestricted/Thesis_3.pdf

When the estimate of accident reduction is 7%, the differences in these growth rates are not *slight*. Twice and half as much cannot honestly be considered a slight difference. Here is the data from the Census Bureau for the 1990-2000 censuses that we provided in the report:

Population Growth in California Cities

City	1990 Population	2000 Population	% Change
Bakersfield	174,820	247,057	41.32%
San Bernardino	164,164	185,401	12.94%
Santa Barbara	85,571	92,325	7.89%
Oxnard	142,216	170,358	19.79%

I prefer to use census data because it has historically been much more accurate than state estimates.[12] With Bakersfield growing at more than 5 times the rate of Santa Barbara, and more than twice as fast as Oxnard, the failure to factor this into the controls will confuse an increase in accident *rates* with an increase in population. If we make the simplistic assumption that the population grew linearly during the decade in each city, and calculate estimates of the population roughly in the middle of the before and after periods, we get the population figures in Table 4. Adjusting the data to accidents per 1,000 people we see accident rates falling in all cities but San Bernardino.

Accidents Rates Adjusted for Population

City	Type of Intersection	Pop Est. Before	Pop. Est. After	Rates per 1000 people		% Change
				Before	After	
Bakersfield	Nonsignalized	209,628	229,220	3.63	3.29	-9.39%
Bakersfield	Signalized	209,628	229,220	3.68	3.22	-12.34%
San Bernardino	Nonsignalized	174,991	180,578	6.97	7.10	1.91%
San Bernardino	Signalized	174,991	180,578	7.57	7.75	2.47%
Santa Barbara	Nonsignalized	89,053	90,818	8.00	6.85	-14.34%
Santa Barbara	Signalized	89,053	90,818	5.48	4.82	-11.99%
Oxnard	Nonsignalized	156,356	163,822	6.36	6.17	-2.92%
Oxnard	Signalized	156,356	163,822	8.46	7.63	-9.76%

The accident rates at signalized intersections in Oxnard did not fall by quite as much as those in either Bakersfield or Santa Barbara. Could this be due to the fact that Oxnard installed Red Light Cameras? It is hard to interpret this data set as evidence of RLC safety improvement.

From this highly aggregated data set and several strong possible confounding variables one cannot hope to find the true answer. One should take note of the fact that the accident rate at the signalized intersections in Oxnard was the highest in the group. A commonly found pattern in accident studies is that intersections or areas with extremely high accident rates in one time period will normally become lower naturally. Termed the “regression to the mean effect”, this pattern should be considered when interpreting the results of any study of safety.

[12] If one uses R&K’s preferred growth rates, the story remains much the same.

On page 6 of R&K (2004), they state that they cannot understand the confusion over their methods in the Oxnard study. First, they do not accurately describe their model because they do not reveal that they dropped a variable. Second, I contacted R&K several times asking them what they did. They responded that they did not remember what they had done. This response was very perplexing. Kyrychenko then told me that they might have dropped one of the interaction terms. In an attempt to verify their results, I dropped each of the interaction terms, and could not duplicate their results.[13] Years later, they reveal in their critique of my study that they dropped the sig/nonsig variable (NOT an interaction term) because it wasn't a good predictor. Dropping a variable only because it lacks statistical significance is an improper technique, and it is improper not to disclose this in their "peer reviewed" study. One should never drop a variable simply because it is not statistically significant. Kennedy (1998, p. 94) states:

> *Omission of a relevant independent variable:*
>
> *(a) In general, the OLS estimator for the coefficients of the remaining variables is biased. […]*
>
> *(b) The variance-covariance matrix of estimated OLS coefficients becomes smaller (unless the omitted variable is orthogonal to the included variables, in which case it remains unchanged). […]*
>
> *(c) The estimator of the (now smaller) variance-covariance matrix of the OLS coefficients is biased upward, because the estimator of σ^2 (the variance of the error term) is biased upward. This causes inferences regarding these parameters to be inaccurate. This is the case even if the omitted variable is orthogonal to the others.*

Unless the authors believe that there is no fundamental difference between signalized and nonsignalized intersections, you must account for this in a model. To drop a variable simply to increase the degrees of freedom and get statistical significance is statistically dishonest, and is not how one should model a data generating process. Dropping variables for any reason other than "we know theoretically that they are irrelevant" causes the other coefficients in a well-developed model to become biased, and inferences to be wrong.

The fact that their model is not properly developed is another matter that is adequately discussed in Burkey and Obeng (2004). They respond to these criticisms as well, but the last two responses in their document to our criticisms are self-contradictory. They misuse the terms "control" and "statistical significance". They admit in one paragraph that the calculation of the estimate of crash reduction for RLCs is independent of the data on other cities (p. 6). They then claim, "Therefore, the effect of the fine increase (if any) on crashes is captured by the comparison cities." (p. 7) Obviously, this is untrue.

On average the accident rate in the 3 "control cities" fell by 7.3% at signalized intersections. If this decrease was caused by the increase in the fine for red light running in California from $104 to $270, an approximate 7.3% decrease in accidents should be seen in Oxnard as well at signalized intersections **due to the fine increase**, and this must be subtracted from the decrease that R&K claim is caused by RLCs. We understand that they used the nonsignalized

[13] I ran all 12 possible models, but must not have checked the one missing the sig/nonsig variable dropped closely enough. I was in error, but would have appreciated accurate responses from the authors.

intersections as controls. However, they never adjusted the estimate of crash reductions in any way for the effect of the fine increase. Perhaps it is believed that the fine increase had no effect; if so, they should say so. However, for some reason the accident rate per 1,000 people fell much more at signalized intersections in Bakersfield and Santa Barbara than in Oxnard. Perhaps these larger decreases were related to the fine increase and/or RLC program, perhaps not. Simply differencing the effect of signalized and nonsignalized intersections *in Oxnard* in no way corrects for this.

VI. Final thoughts on the nature of science

For many years parties have chosen sides over whether or not Red Light Cameras are "good" or "bad". They have done so not by looking carefully at the costs and benefits; rather, they do so because of preconceived notions of whether they *should* reduce accidents, or whether they are *fair*, or whether they bring revenue either to a municipality or a corporation. The two factions have entered into a prolonged period of cognitive dissonance:

> **Cognitive Dissonance: "Inconsistency among related beliefs . . .produces motivation to do whatever is easiest in order to regain cognitive consistency or consonance among beliefs."** *Jones and Gerard(1967), p. 42.*

That is, those opposed to Red Light Cameras scramble to find data that support their beliefs, and demonize evidence to the contrary. Similarly from the other side, the Insurance Institute for Highway Safety has demonized our recent work in the area, calling it "JUNK SCIENCE" [14], implying that we are incompetent, and making the claim that our findings are "so different from previous studies" solely because of this incompetence. Retting and Kyrychenko have also issued a report where they "scientifically" show how ignorant we are. In fact, several studies agree with our conclusions, and to dismiss them simply because they are not peer reviewed is illogical.

If people in this arena cannot try to honestly look at data and make a reasonable decision, they should step aside. Engaging in a battle over a preconceived opinion does not show evidence of understanding what *science* is. Our study was very open and honest about the methods and data, and the limitations of these. I encourage everyone to read the report that R&K criticize, and count how many times the limitations of the estimates are discussed, and caution the reader to not accept the study (or any study) blindly. We implore the reader:

> *While this study incorporated many advances in methodology over previous studies, additional work remains to be done. Because accident studies rarely use a true experimental design and data are not perfectly observable, additional careful study of RLCs is warranted to verify our results.* (Executive Summary, Burkey and Obeng (2004))

Why do they label our work "Junk Science"? Because the data gave us an answer that they didn't like. *We did not create the answer—the data gave it to us*. We did not spend time trying to figure out a way to get a right or wrong answer. We tried to discover the truth to the best of our abilities. We used many, many methods to analyze the data, and every one of them told the same story given

[14] In Status Report Vol. 40, No. 1, January 3, 2005.

in our conclusion. While “Junk Scientists” do shoddy work with shoddy data, twisting it to fit a particular position, hiding important details of the work, and overstating the true meaning of their results, we were very careful to give as honest an appraisal of our work as possible.

In my research, I use Richard Feynman as my guide in how to approach the problem:

> *"But I would like not to underestimate the value of the world view which is the result of scientific effort. The same thrill, the same awe and mystery, comes again and again when we look at any question deeply enough. With more knowledge comes a deeper, more wonderful mystery, luring one on to penetrate deeper still. Never concerned that the answers may prove disappointing, with pleasure and confidence we turn over each new stone to find unimagined strangeness leading on to more wonderful questions and mysteries - certainly a grand adventure." Richard Feynman, 1965 Nobel Physicist*

A real scientist is never disappointed with the answer. Honestly looking for the answer is the process of science. Retting and Kyrychenko use contrived data and contrived, simplistic models to justify name-calling. Had our report supported their position, they would have been likely to praise us as geniuses.

I leave it to the reader to decide who the junk scientists are.

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