Journal of Safety Research 41 (2010) 65–67

Contents lists available at ScienceDirect

Journal of Safety Research

journal homepage: www.elsevier.com/locate/jsr

Letter to the Editor

Safety effects of mobile speed cameras in Norfolk: No more than regression to the mean?

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This letter is a comment on the paper by Jones, Sauerzapf and Haynes, "The effects of mobile speed camera introduction on road traffic crashes and casualties in a rural county of England," published in this journal (vol. 39, issue 1, pp. 101-110, 2008). These authors analyze the effects of a camera enforcement program on injury crashes in the county of Norfolk. The investigation reported is a sound observational before-after accident study. The paper is clear and well documented. It seems, however, that a slight error was made in the calculation of the regression-to-the-mean (RTM) effect, leading to an underestimation of RTM bias. A correction of this error would partly change the conclusions of the paper: based on the corrected results, there is no clear evidence of an overall safety benefit due to the mobile speed camera introduction at the treated sites.

For quantifying the RTM effect R, *i.e.* the relative accident reduction attributable to RTM, Jones et al. (2008) use the following formula (p. 104, Eqs. (1) to (3)):

$$R=\left(\frac{(A_1+A)n}{(n_1+n)A}-1\right).100 \tag{1}$$

where R is the RTM effect (in percent), A is the number of crashes at the treated site over a period of n years, and where

$$A_1=\frac{\overline{a}^2}{(\mathrm{var}(a)-\overline{a})} \tag{2}$$

and

$$n_1=\frac{\overline{a}}{(\mathrm{var}(a)-\overline{a})} \tag{3}$$

In these expressions (taken from DfT, 2003, pp. B13-B14, and based on Abbess, Jarrett, & Wright, 1981), $\bar{a}$ and $\mathrm{var}(a)$ are the mean and variance of the number of crashes *over a one-year period* for a reference sample of sites similar to the treated site. The main assumptions behind these calculations are: (a) At a given site the number of crashes per year is Poisson distributed; (b) among the population of sites similar to the treated site, the underlying Poisson means — *i.e.* the expected values of the number of crashes over a one-year period at these sites — are Gamma distributed (A_1 and n_1 are estimates of the shape parameter and scale parameter of this distribution); (c) there is no noticeable change in these Poisson means from year to year during the 'before treatment' period (Abbess et al., 1981).

The text of the paper by Jones et al. (2008, p. 106) and Tables 3 and 4 (pp. 107 and 108) clearly show that these authors calculated $\bar{a}$ and $\mathrm{var}(a)$ as the mean and variance of the number of crashes *over the whole two-year 'before' period* for the reference sample of sites, which is not compatible with the formula applied (equation 1, above).

If $\bar{a}$ and $\mathrm{var}(a)$ are calculated over the whole 'before' period, an appropriate estimation of the RTM effect is however possible. But, in this case, $A_1=\bar{a}^2/(\mathrm{var}(a)-\bar{a})$, and $n_1=\bar{a}/(\mathrm{var}(a)-\bar{a})$ are estimates of the shape parameter and scale parameter of a Gamma distribution of the expected values *over the whole 'before' period*. An empirical Bayes (EB) estimate for the expected value of the number of crashes at the treated site during the 'before' period is then $m=(A_1+A)/(n_1+1)$, as shown by several authors (see, for example, Mountain & Fawaz, 1991, or Hauer, 1997, p. 192). Then the RTM effect (in percent) can be expressed as follows

$$R=\left(\frac{m-A}{A}\right).100=\left(\frac{(A_1+A)}{(n_1+1)A}-1\right).100 \tag{4}$$

Using this last equation, the results presented by Jones et al. (2008) in Tables 3 and 4 can easily be corrected.

If the trend effect is considered as negligible (on this point, see below), the expected value of the number of crashes during the two-year 'before' period, m, also represents what would be expected during the two-year 'after' period, had the treatment not been implemented. It can be compared to the number of crashes observed during this 'after' period, which reflects what happened in the presence of treatment. The results concerning the RTM effect for the injury crashes are given in Table 1 below (corresponding to Table 3 in the paper by Jones et al., 2008).

Similar results concerning the KSI crashes (crashes in which people were killed or severely injured) are given in Table 2 (corresponding to Table 4 in the paper by Jones et al., 2008). Note that, for these KSI data, in some cases $\mathrm{var}(a)$ is slightly smaller than (and close to) $\bar{a}$, leading to negative values for A_1 and n_1. In this

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doi:10.1016/j.jsr.2009.12.001

Table 1
Estimated effect of RTM for injury crashes (based on Jones et al., 2008, Table 3, corrected).

Site identity	Number of crashes in pre-camera period (Oct 1999 – Sept 2001)	Number of reference sites	Mean number of crashes for reference sites (Oct 1999 – Sept 2001)	Variance of the number of crashes for reference sites	Estimated effect of RTM (%)	Number of crashes expected without treatment (RTM effect alone) (Oct 2001 – Sept 2003)	Observed crashes in camera enforcement period (Oct 2001 – Sept 2003)
1	6	23	7.30	21.04	7.52	6.45	4
3	20	16	6.00	19.47	−21.57	15.69	15
5	6	32	2.75	3.68	−40.48	3.57	10
10	4	37	1.73	7.70	−12.75	3.49	2
11	5	38	2.39	5.06	−24.66	3.77	5
13	7	20	5.45	12.37	−9.76	6.32	1
16	13	27	2.85	5.36	−41.51	7.60	10
17	6	12	3.42	13.17	−11.17	5.33	8
18	8	33	4.88	8.42	−22.60	6.19	7
22	8	21	7.10	14.89	−5.36	7.57	6
26	8	24	3.04	3.78	−49.86	4.01	8
29	11	17	5.71	17.60	−15.60	9.28	13
30	7	22	5.36	13.00	−9.66	6.32	6
31	5	44	1.91	3.76	−31.39	3.43	2
32	13	38	1.63	3.59	−39.71	7.84	6
39	5	27	2.81	7.31	−16.84	4.16	6
4	5	20	7.30	14.12	23.78	6.19	6
9	3	31	6.65	20.70	39.09	4.17	5
19	4	24	6.63	12.68	34.38	5.38	6
20	1	23	1.74	3.84	33.53	1.34	3
All sites	145					118.09	129

situation, the calculation $m=(A_1+A)/(n_1+1)$ is not appropriate. Considering that $\mathrm{var}(a)\approx\bar{a}$, the relevant EB estimate for the expected value of the number of crashes is then $m=\bar{a}$ (see for example Hauer, 1997).

Table 1 shows that, among 20 camera sites, only 9 have fewer post-camera crashes than expected from RTM effect alone. Based on Table 2, it appears that, among the 19 camera sites taken into account for the analysis of KSI data, only 7 have fewer post-camera KSI crashes than expected from RTM effect. When considering the treated sites as a whole, one can see that for injury crashes (Table 1), due to RTM effect alone, about 118 crashes would be expected without treatment, while with camera enforcement 129 crashes are observed. For KSI data (Table 2), about 32 KSI crashes would be expected without treatment, while with camera enforcement 39 KSI crashes are observed. These results do not suggest an overall beneficial effect of this camera enforcement program in terms of safety, beyond the artefact of RTM.

As in Tables 3 and 4 in the paper by Jones et al. (2008), the trend effect is not taken into account in these results. Based on data reported by these authors (p. 106), it seems acceptable to neglect this effect for injury crashes, since there was only a non-significant 1.4% decrease of injury crashes at non-camera sites in Norfolk, from 'before' to 'after' period. The decrease is more noticeable for KSI crashes (significant decrease of 9% at non-camera sites). Taking this effect into account in Table 2 would lead to lower the number of KSI crashes expected without treatment, which would not substantially change the findings.

In conclusion, RTM appears to play a major role in this evaluation study. That is not surprising, since the selection of sites to be treated

Table 2
Estimated effect of RTM for KSI crashes (based on Jones et al., 2008, Table 4, corrected).

Site identity	Number of KSI crashes in pre-camera period (Oct 1999 – Sept 2001)	Number of reference sites	Mean number of KSI crashes for reference sites (Oct 1999 – Sept 2001)	Variance of the number of KSI crashes for reference sites	Estimated effect of RTM (%)	Number of KSI crashes expected without treatment (RTM effect alone) (Oct 2001 – Sept 2003)	Observed KSI crashes in camera enforcement period (Oct 2001 – Sept 2003)
1	4	23	2.26	3.75	−26.22	2.95	4
3	7	16	1.94	3.13	−44.80	3.86	6
5	3	32	0.91	0.99	−64.04	1.08	1
10	4	37	0.35	0.79	−40.43	2.38	0
11	3	38	0.79	1.14	−51.05	1.47	0
13	3	20	0.85	1.29	−47.22	1.58	0
16	4	27	0.85	0.75	−78.75	0.85	3
17	4	12	1.00	0.91	−75.00	1.00	2
18	5	33	1.42	1.38	−71.60	1.42	3
19	3	24	2.05	1.87	−31.67	2.05	1
22	3	21	1.90	2.29	−30.42	2.09	3
26	5	24	0.92	0.78	−81.60	0.92	2
29	5	17	1.41	1.26	−71.80	1.41	3
30	4	22	1.55	1.36	−61.25	1.55	2
31	3	44	0.70	0.91	−58.97	1.23	2
32	5	38	0.53	0.74	−64.03	1.80	3
39	2	27	0.96	1.65	−30.25	1.39	2
4	1	20	1.95	2.26	81.97	1.82	1
9	1	31	1.87	2.65	61.39	1.61	1
All sites	69					32.47	39

was based "primarily on the injury crash record," as stated by Jones et al. (2008, p. 103). Regarding the effect of the program, based on the corrected results we have presented above, the study does not seem to provide evidence of an overall safety benefit due to the introduction of mobile speed cameras for this set of treated sites.

Response from Author

We thank the author of the letter for picking up the error in our calculations. We agree that the error led to an underestimation of regression to the mean bias, and that our conclusion needs to be amended accordingly.

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