ORIGINAL ARTICLE

Are mobile speed cameras effective? A controlled before and after study

S M Christie, R A Lyons, F D Dunstan, S J Jones

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See end of article for authors' affiliations

Correspondence to: Professor Ronan Lyons, Clinical School, University of Wales Swansea, Grove Building, Singleton Park, Swansea SA2 8PP, UK; r.a.lyons@swansea.ac.uk

Objective: To identify the most appropriate metric to determine the effectiveness of mobile speed cameras in reducing road traffic related injuries.
Design: Controlled before and after study which compares two methods for examining the local effectiveness of mobile speed cameras—a circular zone around the camera and a route based method to define exposure at various distances from sites.
Setting: South Wales, UK.
Subjects: Persons injured by road traffic before and after intervention.
Intervention: Use of mobile speed cameras at 101 sites.
Main outcome measures: Rate ratio of injurious crashes at intervention and control sites.
Results: Camera sites had lower than expected numbers of injurious crashes up to 300 metres using circles and up to 500 metres using routes. Routes methods indicated a larger effect than the circles method except in the 100 metres nearest sites. A 500 metre route method was used to investigate the effect within strata of time after intervention, time of day, speed limit, and type of road user injured. The number of injurious crashes after intervention was substantially reduced (rate ratio 0.49, 95% confidence interval 0.42 to 0.57) and sustained throughout two years after intervention. Significant decreases occurred in daytime and night time, on roads with speed limits of 30 and 60–70 miles/hour and for crashes that injured pedestrians, motorcycle users, and car occupants.
Conclusions: The route based method is the better method of measure effectiveness at distances up to 500 metres. This method demonstrates a 51% reduction in injurious crashes.

Although road traffic crashes and collisions are a leading cause of death and disability among people aged under 35 years,[1] there is a paucity of research evaluating the effectiveness of road safety interventions published in academic journals.[2] In the United Kingdom, one controversial approach is the use of mobile speed cameras. These capture the license plate of passing vehicles exceeding a speed above the legal limit and mail a fine and penalty points to the driver of the vehicle. Unlike static speed cameras, which are operated remotely from permanent camera housings at the roadside, mobile cameras operate in this situation from parked marked vehicles and can be quickly moved from one site to another. Mobile cameras tend to be rotated around a much larger number of sites than static cameras and are almost exclusively used during daylight hours, whereas static cameras typically operate around the clock all year. These differences result in fewer hours of operation at each mobile camera site compared with static camera sites. Mobile cameras might be expected to be less effective than static cameras, when measured as reductions in injurious crashes per annum because of the differences in enforcement intensity.

Evaluations of static speed cameras in the United Kingdom have shown significant decreases in numbers of injurious crashes near camera sites.[3 4] A meta-analysis merging the results of 10 local effects of camera studies, which included static and mobile sites and a variety of analytical methods, in the United Kingdom and six other countries found a decrease of 19% in all injury crashes.[5]

Hypothecation or netting off is a government scheme that allows for operating and associated costs of speed cameras to be met, or netted off, from speeding offence fines. It is a stand-alone system which does not compete for funds with other injury prevention programmes. Site selection for evaluation of this scheme was initially according to numbers of injurious crashes within a 500 metres radius circle or along a route of between 400 and 3000 metres.[6] In 2002 these guidelines were modified and the circles approach abandoned.[7] Existing evaluations, including the most recent Department for Transport evaluation,[8] have used a mix of circle and route based approaches, but do not give sufficient methodological detail in the publication to be certain that all biases are excluded. The latter study reported very variable changes in injurious crashes at camera sites across eight regions, ranging from a 64% reduction to 14% excess.

Of the two approaches, circles are methodologically simpler—the road network geometry does not need consideration and injurious crashes within an arbitrary radius can be selected using mathematical functions of grid coordinates, making geographical information systems unnecessary. Evaluations in some countries have used radiuses ranging from 0.5 kilometres to as much as 15 kilometres.[9] The drawbacks of this approach are numerous. Where circles overlap, injurious crashes may be double counted and contribute to the evaluation of more than one site. This is compounded for injurious crashes occurring between camera deployment dates for nearby sites—before-after status becomes ambiguous as an injurious crash can be classified as before intervention at one site and after at another. The use of circles also leads to the inclusion of injurious crashes occurring on roads some distance from the target route. Compared with the circles approach, routes seem more rational and are now regarded as the preferred method for identifying speed camera sites and then monitoring site

Abbreviations: CI, confidence interval; mph, miles per hour effectiveness.[7] However, the effect of using routes versus circles has not been investigated.

South Wales was one of eight regions in the United Kingdom that participated in a pilot scheme for speed camera fine hypothecation from 1 April 2000. This led to a substantial increase in the use of speed cameras in the region, with 10 cameras in operation by the end of 2000. However, use of speed cameras in the region had been growing steadily since 1995, making a within region before-after comparison impractical.

This study aimed to compare the various methods (circles and routes of various sizes) for assessing local effectiveness of mobile speed cameras, and then to use the most appropriate of those methods to investigate the effectiveness of speed cameras by time after intervention, time of day, speed limit, and type of road user injured, using a controlled before-after method of investigation.

METHODS

The South Wales region encompasses an area of 2121 square kilometres and includes about 6700 kilometres of public roads, of which 1% is motorway, 12% A-roads, 11% B-roads, and the remainder mostly unclassified roads (see footnote table 1 for descriptions). The majority (94%) of the total resident population of 1.3 million live in urban areas, the largest of which are Cardiff and Swansea.

In the United Kingdom, data on road traffic crashes or collisions in which at least one person is injured are collated by the police using the "STATS 19" form and these data were available for the South Wales region for the period 1996–2000 from the United Kingdom Data Archive.[10] STATS 19 consist of all personal injury road traffic collision records and data fields include date and time of crash, posted speed limit, type of road users injured, and the grid reference for the location. We identified whether each crash occurred during daylight by comparing crash dates and times with daily sunrise and sunset times for Cardiff for the five year study period.

South Wales Safety Cameras Partnership provided information on location and approximate date of first camera deployment at 101 mobile speed camera sites. Gwent, a neighbouring police force area operating only one mobile and no static cameras, was chosen as the source of sites as matching sites could not be found in the South Wales area due to the high use of cameras. In 2001, the road casualty rate per 100 000 was 424 in South Wales and 421 in Gwent.[11] In this analysis the two areas are not being compared but matched sites where cameras have, or could be located, are compared. Camera and matched sites and injurious crashes in both areas were mapped using MapInfo Professional Version 6.5.[12] Since each operational site had different deployment dates, the before intervention maps were modified for each control site selection and were used without after intervention data to ensure that selection of controls was blind. Control sites were chosen to be at least 500 metres from any Gwent speed camera site and were matched for posted speed limit, road class (motorway, A-road, B-road, or other road), and injurious crashes history (±20% number of injurious crashes within 500 metres radius in the before intervention period).

Using the geographical information system circles and routes (polygons around the roads) were then drawn around all intervention and control sites, with various sizes used to represent potential zones of protection. Circles of radius 100, 300, 500, and 1000 metres were drawn around each site, along with similar route lengths. Routes were extended in both directions to the set distance (100, 300, 500, or 1000 metres), but terminated 60 metres short of any roundabout, T-junction, or other major junction that would cause traffic to slow or stop. Route polygons included an area of 30 metres either side of the centre of the road to allow for imprecision in grid referencing of crash location. Any portions of either the circles or routes polygons that overlapped any other polygon with an earlier camera deployment date were excluded to avoid double counting or misclassification of before-after status of crashes.

Because of a finite number of control sites, each intervention-control site pair could not be perfectly matched for injurious crashes history due to the ±20% variability mentioned above. For each pair we calculated a matching ratio as the total number of injurious crashes within 500 metre radius of the control site divided by the number at the intervention site in the pre-intervention period. We calculated the expected number of injurious crashes at each site as the observed number at the control site divided by the matching ratio. For example, an intervention-control pair with six injurious crashes in the control-before and seven injurious crashes in the intervention-before period would have a matching ratio of 0.86 and if there were four injurious crashes in the control-after period the expected number in the intervention-after period would be 4/0.86 = 4.7 (rounded). This method adjusts for unequal before and after periods and regional trends in injurious crash rates. Rate ratios and 95% confidence intervals (CI) were calculated using the "standardised ratio" module of confidence interval analysis statistical software.[13] The dates of first use of mobile speed cameras at each site were often known only approximately; therefore, to avoid possible exposure misclassification, all injurious crashes in the three months immediately before and after intervention were excluded from the analysis.

The effect of varying catchment areas was tested for circles and routes of radius or length 0–100, 100–300, 300–500, and 500–1000 metres. Having calculated rate ratios for the eight combinations of shape (circle *v* route) and distance, we investigated effectiveness within strata of time after intervention, time of day, speed limit, and type of road user injured using one of the eight methods. For these stratum specific analyses we used the routes rather than circles method because it is less prone to exposure misclassification. We used the largest route length that did not extend beyond

Table 1 Camera sites, number by year of intervention, speed limit, and road class

	No of sites
Year of intervention	
1996	8
1997	7
1998	19
1999	26
2000	41
Speed limit (mph)	
30	76
40–50	5
60–70	20
Road class	
Motorway	8
A-road	37
B-road	25
Other road	31
Total	101

mph, miles per hour.
Motorway, road with multiple lanes with traffic in either direction physically separated and with entrance and exits only from the slow lane.
A-road, trunk and principal roads (excluding motorways), which are part of the strategic highway network linking major towns and cities, frequently, but not always, dual carriageways.
B-road, non-A class or motorway roads which connect villages or towns.

the apparent footprint of local camera effectiveness revealed in the unstratified analysis and that could be implemented for the majority of intervention sites.

RESULTS

Numbers of camera sites in use increased throughout the study period such that 66% (n = 67) were deployed during 1999 and 2000 (table 1).

Excluding the three months immediately before and after intervention, the average length of time before intervention was 38 months and the average follow up was 17 months. In the before period a total of 1456 injury related crashes occurred at the 101 intervention sites and 1469 at the matched control sites (ratio 0.99).

The apparent effect of mobile speed cameras varied greatly with the shape (circle or route) and size of the polygon used to capture injurious crashes near the site (table 2). Within 100 metres radius of sites, the number of injurious crashes was 73% lower than expected (rate ratio 0.27, 95% CI 0.19 to 0.39). In the 100–300 metre radius band, the decrease was 24% (0.76, 0.66 to 0.88). Decreases were not statistically significant in the 300–500 and 500–1000 metre radius bands. Using the routes method, we found a large decrease in injurious crashes within 100 metres of sites (0.30, 0.20 to 0.42), and smaller decreases at distances of 100–300 metres (0.55, 0.43 to 0.68) and 300–500 metres (0.59, 0.46 to 0.76). As with the circles method, the routes method revealed no statistically significant change in injurious crashes beyond 500 metres. The number of injurious crashes prevented was greatest near sites: 0.8 fewer than expected per site during the period of follow up occurred within 100 metres radius (circles method) and 0.7 for the 100 metres routes method.

As a defined route is extended along a target road, it eventually meets either a major road junction or a route for another camera site, leading to a drop-off in the proportion of routes that can be evaluated with increasing route length. This is a particular issue in urban areas, which have more mobile speed cameras and more road junctions. A route analysis of 500 metres was possible only for 57% (n = 58) of sites, decreasing to 38% for 1000 metre routes. For this reason, no analysis was carried out beyond 1000 metres.

The 500 metre routes, with significant decreases in injurious crashes and 57% of sites available, were then used to investigate local effects. Along these routes the rate of all crashes decreased 51% (0.49, 0.42 to 0.57), and an average of 1.8 injurious crashes were prevented per site.

Analysis of several factors showed that the effectiveness of cameras did not significantly change with time and was similar on 30 miles per hour (mph) and 60–70 mph roads but greater for pedestrians (0.22, 0.14 to 0.32) than car occupants (0.48, 0.40 to 0.59) (table 3). For the time period 21–24 months, speed limits of 40–50 mph, and pedal cyclists the number of events are too small to draw statistically valid conclusions.

DISCUSSION

The results of this study show that the route method is the better method for evaluation of mobile speed cameras. Camera use is associated with a reduction in injurious crashes and the effect size varies by the type of road user. Before accepting these results it is important to consider and attempt to quantify potential biases.

Inclusion of traffic volume and distances driven in studies such as these is well established.[14] However, no such data were available at this small level of analysis and it is necessary to assume no change in these factors occurred at speed camera sites. It is also necessary to assume that speed cameras did not cause diversions to alternative routes, again because of a lack of data. Had the research team been involved in designing the intervention, rather than the evaluation, traffic flows on roads with cameras and alternative routes would have been included. Previous evaluations of static speed cameras in the United Kingdom have, however, found that crash risk either remained unchanged or decreased to some extent in neighbouring areas or routes.[15] The availability of such data would strengthen the conclusions that camera deployment led to decreases in crashes.

Precision of crash location measurement varied between area divisions of the police force, some locations recorded to the nearest 10 metres, others to the nearest 100 metres. Such imprecision would not have substantially affected the circles analyses, since most crashes did not occur near circle perimeters, but will have resulted in crashes being wrongly excluded from routes analyses and a smaller number being wrongly included. Since the proportion of imprecisely located crashes decreased over the study period, an association with before-after status exists and could have biased results. Along routes, this may have led to underestimations of the protective effect of cameras by as much as 3%–4%.

Speed camera site selection guidelines require that cameras only be used at sites where other road safety interventions are considered unsuitable. Thus it is unlikely that any road safety engineering, such as ''traffic calming'' devices, or other local safety interventions would have biased results. However, locations of such interventions are not readily available to confirm this.

The gold standard for the evaluation of interventions is the randomised controlled trial. Unfortunately, randomised trials in this field are difficult to implement, probably because of differences in belief between academics and some injury control practitioners and politicians that the existing evidence is sufficiently persuasive. A recent systematic review of traffic calming by the Cochrane Collaboration included 16

Table 2 Rate ratio and number of injurious crashes prevented by distance, circle, and route methods

Shape	Distance band (m)	No of sites*	Expected	Observed	Rate ratio (95% CI)	NP† (95% CI)
Circle	0–100	101	117	32	0.27 (0.19 to 0.39)	0.8 (0.7 to 0.9)
	100–300	101	257	196	0.76 (0.66 to 0.88)	0.6 (0.3 to 0.9)
	300–500	101	360	358	1.00 (0.90 to 1.12)	0.0 (−0.4 to 0.3)
	500–1000	101	944	914	0.97 (0.91 to 1.03)	0.3 (−0.3 to 0.9)
Route	0–100	101	104	31	0.30 (0.20 to 0.42)	0.7 (0.6 to 0.8)
	100–300	90	146	80	0.55 (0.43 to 0.68)	0.7 (0.5 to 0.9)
	300–500	75	106	63	0.59 (0.46 to 0.76)	0.6 (0.3 to 0.8)
	500–1000	58	83	75	0.90 (0.71 to 1.13)	0.2 (−0.2 to 0.4)

*Number of sites with routes extending in both directions into this distance band.
†Number of injurious crashes prevented per site during follow up.
m, metre.

Table 3 Rate ratio and number of injurious crashes prevented, by time after intervention, time of day, speed limit, and type of road user injured, 0–500 metre routes method

	No of sites*	Expected	Observed	Rate ratio (95% CI)	NP† (95% CI)
Total all injurious crashes, all sites‡	101	357	174	0.49 (0.42 to 0.57)	1.8 (1.5 to 2.1)
Time after intervention (months)					
3–6	88	58	27	0.47 (0.31 to 0.68)	0.3 (0.2 to 0.5)
6–9	66	48	29	0.61 (0.41 to 0.87)	0.3 (0.1 to 0.4)
9–12	60	40	25	0.63 (0.41 to 0.93)	0.2 (0.0 to 0.4)
12–15	54	41	22	0.54 (0.34 to 0.82)	0.3 (0.1 to 0.5)
15–18	41	25	16	0.64 (0.36 to 1.04)	0.2 (0.0 to 0.4)
18–21	38	28	5	0.18 (0.06 to 0.42)	0.6 (0.4 to 0.7)
21–24	34	10	11	1.12 (0.56 to 2.01)	0.0 (−0.3 to 0.1)
Time of day					
Day	101	255	118	0.46 (0.38 to 0.56)	1.4 (1.1 to 1.6)
Night	101	102	56	0.55 (0.42 to 0.71)	0.5 (0.3 to 0.6)
Speed limit (mph)					
30	76	291	141	0.49 (0.42 to 0.58)	1.9 (1.6 to 2.2)
40–50	5	7	6	0.80 (0.30 to 1.75)	0.3 (−1.1 to 1.1)
60–70	20	58	24	0.41 (0.27 to 0.62)	1.7 (1.1 to 2.1)
Type of road user					
Pedestrian	101	118	26	0.22 (0.14 to 0.32)	0.9 (0.8 to 1.0)
Pedal cyclist	101	14	12	0.83 (0.43 to 1.46)	0.0 (−0.1 to 0.1)
TWMV user§	101	16	6	0.37 (0.13 to 0.80)	0.1 (0.0 to 0.1)
Car occupant	101	219	106	0.48 (0.40 to 0.59)	1.1 (0.9 to 1.3)

*Number of sites contributing to this analysis.
†Number of injurious crashes prevented per site during follow up.
‡Expected numbers may not sum to exactly the stated total because of rounding.
§TWMV, two wheeled motor vehicle.

controlled before and after studies but no randomised trials.[16] Properly designed controlled before and after studies are superior to before and after studies as they can avoid regression towards the mean biases. In this study the control and intervention sites were so well matched for all injurious crashes (matching ratio 0.99) that if any regression to mean occurred it should have had an equal effect at camera and control sites. Small numbers within some strata of analysis limited the study's power to detect changes in crashes, particularly on 40–50 mph roads, and for crashes that injured pedal cyclists.

This study is based on road traffic crashes in which at least one person was injured, as judged by the police. The police also judge the severity of each injury to be fatal, serious, or slight with the respective proportions being 1%, 12%, and 87%.[11] However, it has been shown that the police categorisation of serious and slight is unreliable with a marked tendency to underestimate severity compared with hospital records.[17] Also, during the period of this evaluation there is evidence that recording practices for severe injury changed in South Wales.[7] Consequently, we have used all crashes that resulted in any injury rather than by severity category.

Very near sites (within 100 metres) we found a similar decrease in crashes using both circles and routes methods. Beyond 100 metres, the routes method revealed a larger effect than did the circles method and the difference increased with distance. This is likely to be due mainly to over-inclusion of roads well away from the target road when using circles. Such crashes might be influenced by a generalised effect of speed cameras across an entire police force area but are unlikely to be influenced by any local effect of a particular camera site. Local road design limited the study as a route analysis could only be extended to 500 metres for 57% of sites and we had very little power to determine effectiveness over larger distances.

It may be that many drivers slow only when encountering speed camera warning signs, which are typically 100–200 metres from sites on 30 mph roads and approximately 1 kilometre from sites on high speed roads. The pattern is consistent with results of driver attitude surveys in England[18] and Scotland,[19] in which many drivers reported slowing at speed camera sites but not elsewhere. The government's policy, as expressed by the former Department for Transport, Local Government and the Regions,[20] of increasing local visibility of speed cameras and removing camera signs from sites that are not used for enforcement facilitates this pattern of driving and may be counterproductive to the aim of reducing road casualties.[21] In 2000 only 2.9% of all crashes in South Wales occurred within a 500 metre route of mobile speed camera sites included in the present analysis. Thus, the effect across the entire area would be expected to be a 1.5% overall reduction, a figure that is well within year-to-year variability. To have a much greater effect, cameras would need to be employed much more widely, and perhaps randomly.

CONCLUSIONS

This study has compared methods for investigating the local effectiveness of mobile speed cameras. It found that deployment of mobile speed cameras was associated with a sustained decrease in the risk of injurious crashes near camera sites. Injurious crashes decreased more very near to sites than further away and the decrease was revealed as larger when a routes rather than circles method for capturing

Key points

- Evaluation of the effectiveness of mobile speed cameras is sensitive to the choice of metric used.
- A 500 metre linear route was found to be the most suitable metric.
- Mobile speed cameras are associated with a 50% reduction in personal injury accidents within 500 metres.
- The largest reduction in injuries occurred in pedestrians, where 78% were prevented.

nearby injurious crashes was used. The effect of mobile speed cameras varies by geography and by type of road user.

Authors' affiliations

S M Christie, National Public Health Service for Wales, Pontypool, Wales, UK

R A Lyons, F D Dunstan, S J Jones, Department of Epidemiology, Statistics and Public Health, University of Wales College of Medicine, Cardiff, Wales, UK

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