Road traffic crash injuries near mobile speed cameras: a controlled before and after study

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ABSTRACT

Speed cameras have been used in many countries as a traffic enforcement intervention to deter speeding and prevent road traffic crashes. Most of the research on effectiveness has been carried out at static speed camera sites and there is relatively little evidence about mobile cameras. Various methods have been used to evaluate local effectiveness of cameras. The appropriateness of those methods has not been assessed. Our study compares methods for investigating the local effectiveness of mobile speed cameras, using circles and lines methods for defining the local area of exposure, at various distances or radiuses from camera sites, and within strata of time after camera deployment, time of day, posted speed limit, and type of road user injured. It compares observed and expected numbers of personal injury crashes in the period 1996–2000, adjusted for seasonal and annual variation, after cameras were first deployed at 101 sites in South Wales and at 101 control sites matched for crash history, posted speed limit, and road class. We repeated the analysis using circle radiuses and line lengths of 0–100, 100–300, 300–500 and 500–1000 metres.

A sustained decrease in crashes until at least two years occurred at camera sites. Crashes decreased 61 percent (95 percent confidence interval 43–75) within 100 metres radius of sites; the decrease was not statistically significant beyond 500 metres radius. The lines method revealed greater decreases than did the circles method, particularly at large radiuses. Significant decreases in crashes occurred during day-time, on 30 miles per hour roads, and for crashes involving pedestrian and car occupant casualties. The decreases in crashes cannot be attributed to regression-to-mean bias. We conclude that mobile speed cameras substantially reduce crashes. Their effectiveness varies by geography and by type of crash; these dimensions need to be considered in any evaluation of local effectiveness.

***Keywords and phrases*:** speed camera, road safety, road traffic, motor vehicle crash, injury prevention

1.0 INTRODUCTION

At the global scale road traffic crashes (RTCs) are the leading cause of death and disability among people aged under 35 years (Murray and Lopez, 1996). The epidemic of RTC injuries is worsening such that by the year 2020, RTCs are forecast to be the third leading cause of disability-adjusted years of life lost for all ages (Murray and Lopez, 1997). In ‘highly motorised’ countries, where RTC death rates have decreased substantially over the past two decades, the cost of RTCs is still large; one estimate puts it at 2 percent of gross national product (Jacobs and Aeron-Thomas, 2000). Within the UK, the burden of RTCs is inequitably distributed across the population. For example, among children, for whom RTCs are the leading cause of death, the RTC death rate has been estimated to be between five and eight times greater in the lowest social class than in the highest (Roberts and Power, 1996). Considering the magnitude, the social inequality, and the avoidable nature of the impact that RTCs make on population health, road safety research has received little attention compared with other public health issues (WHO, 2001).The result is a paucity of evaluation of road safety interventions (Roberts et al, 2001). One such intervention that has frequently been the subject of contention in the UK media is mobile speed cameras.

At the regional level, speed cameras have been shown to cause a significant decrease in crashes (Keall et al, 2001). Evaluations of static speed camera programmes in the UK have found significant decreases in crash rates near camera sites (Hooke et al, 1996; Highways Agency, 1997). A meta-analysis merging the results of 10 studies of the local effect of speed cameras in the UK and six other countries found an average decrease of 19 percent in all injury crashes (Elvik et al, 1997). The studies included both static and mobile sites and a variety of methods were used to define which crashes were local to camera sites.

Unlike static speed cameras, mobile cameras require no permanent installation facilities and can be quickly moved from one site to another. Mobile cameras tend to be rotated around a much larger number of sites than are static cameras, and are almost exclusively used during daylight hours, whereas static cameras typically operate around the clock. These differences result in fewer hours of operation at each mobile camera site compared to static camera sites. Mobile cameras might be expected to be less effective than static cameras because of the differences in enforcement intensity and the circumstances of operation.

South Wales was one of eight UK regions that participated in a pilot scheme for speed camera fine hypothecation from 1 April 2000. The scheme allowed for operating and associated costs of speed cameras to be met from speeding offence fines. Consequently, there was a substantial increase in the use of speed cameras from that date onwards, with 10 mobile cameras in operation by the end of year 2000. However, some speed cameras were used at sites over the preceding four or five years, thus a regional scale before-after study would be hampered by difficulty in accurately classifying before and after periods. The situation does present an opportunity, which is taken up in the present study, to conduct a small-area before-after comparison study around camera sites.

Most evaluations of local speed camera effectiveness in the UK have used a circles method for defining the geographical extent of local exposure. The circles method is methodologically simple in that road network geometry need not be considered; crashes within an arbitrary radius can be selected on the basis of a mathematical function of grid coordinates, thus a geographic information system (GIS) is not required. Circles methods with various radiuses from 500 m up to 15 km (in rural areas) have been used in speed camera evaluations in some countries (Newstead et al, 1995). However, the circles method most commonly used in the UK, as described by the Department of Transport, Local Government and the Regions (DTLR, 2001a), has four drawbacks that may bias the results of speed camera evaluations. Firstly, wherever crashes occur in the overlapping portion of two circles, they may be double counted (each crash contributes to the evaluation of more than one site). Secondly, if such crashes occur between the camera deployment dates of the nearby sites, the before-after status of crashes is ambiguously classified (counted as before camera deployment at one site and after deployment at another site).

Thirdly, the circles method results in crashes that are located away from the target road being classed as exposed, when in fact they may be a large driving distance from the camera site, and therefore unlikely to be influenced by any local speed camera effect. This particularly applies where road network geometry consists of parallel roads with relatively few connections, as is common in the valleys and some urban areas of South Wales. The lines (or *routes*) method, in which only crashes on the target road are included, therefore seems more rational, and is regarded as a suitable alternative to the circles method for monitoring speed camera effectiveness and for selecting appropriate speed camera sites (DTLR, 2001a). The two methods are graphically displayed in Figure 1. The effect of the alternative methods has not been investigated. Fourthly, before-after comparisons of crashes at speed camera sites may be subject to regression-to-mean bias. We sought to address each of these methodological issues in the present study.

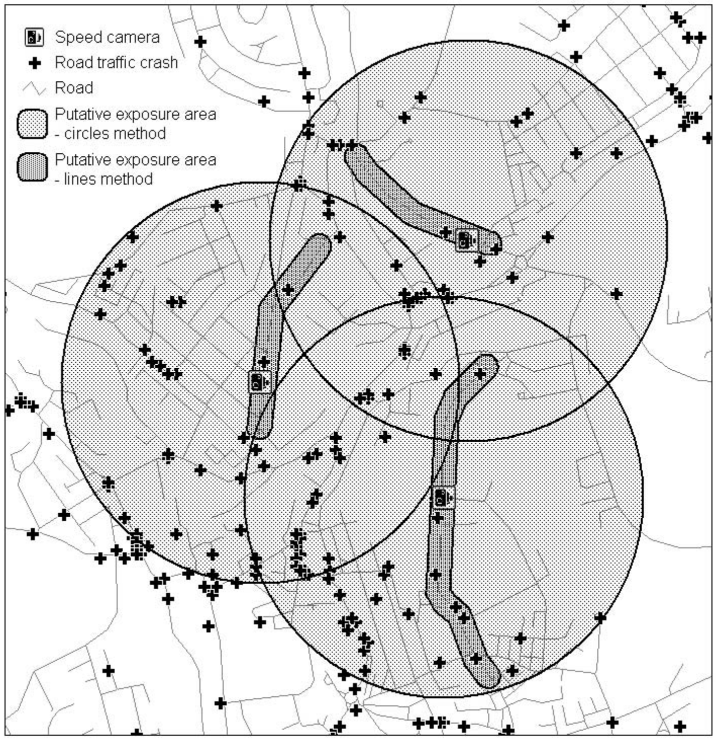

Figure 1. Circles and lines methods for defining local exposure area.

2.0 METHODS

For every road traffic crash that is notified to the police and that involves personal injury, the police complete a form (Stats19) containing details about the vehicles, casualties and circumstances of the crash. We obtained Stats19 data for the South Wales Police region for the period 1996–2000 from the UK Data Archive (UKDA, 2001). The data include information on location, date, and time of crash, posted speed limit, and type of road user injured. We determined whether each crash occurred during daylight hours by comparing crash dates and times with a table of daily sunrise and sunset times for Cardiff for the five-year study period.

South Wales Police provided information on location and approximate date of first camera deployment at 101 mobile speed camera sites that were first used in the period 1996–2000. For each camera site we selected a control site in the neighbouring region of Gwent, in which no static speed cameras and only one mobile speed camera were operated during the study period. Control sites were chosen to be at least 500 m from any site where Gwent's mobile camera was known to have been used, and were matched for posted speed limit, road class (motorway, A-road, B-road, or other road), and RTC history (+/– 20 percent number of crashes within 500 m radius in the before-exposure period). Selection of controls, and all geographic analyses were performed using Mapinfo Professional Version 6.5 software (Mapinfo, 2000). In order for the selection of controls to be blind with respect to crash locations in the after-exposure period, we selected controls while examining on-screen maps on which only crashes in the before-exposure period were shown. Since sites had different camera deployment dates, this required modifying the map for each control selection.

For each site we defined circular and linear geographical polygons of various sizes representing the putative extent of local exposure. For the lines method we constructed a polyline representing the centreline of the target road for each camera site and extended the line to a set distance both directions but terminating 60 m short of any roundabout intersection, T-intersection or other major junction that would likely cause traffic to slow or stop. We then constructed a 30 m buffer around the polyline to allow for a small amount of imprecision in crash location. We excluded from the circles and lines polygons any portion overlapped by the polygon for a nearby pre-existing mobile or static camera site, in order to avoid any double-counting or misclassification of before-after status of crashes.

We multiplied each site's before-exposure crash rate (crashes per unit time) by the period of follow-up after deployment to generate an expected number of crashes. We multiplied the expected number at each site by the ratio of before and after crashes that occurred in non-hypothecation regions of Wales (Dyfed Powys, Gwent, and North Wales), where speed cameras were used to a minor extent compared with South Wales. This secular trend adjustment altered expected numbers by up to 2 percent. We calculated ratios of rate ratios (RR) and 95 percent confidence intervals (CI) using Confidence Interval Analysis statistical software (Gardener et al, 1992). Using the 500 m radius circles method we examined the RR for each three-month period from 0–3 months to 21–24 months, and 24+ months after camera deployment. Dates of first use of mobile speed cameras at each site were often known only approximately. To avoid possible exposure misclassification, we excluded from all subsequent analyses a 'window of uncertainty' three months before to three months after the estimated date of deployment.

We calculated RR within strata of time of day (day-time versus night-time), posted speed limit (30, 40–50, 60–70 mph), and type of road user injured (pedestrian, pedal cyclist, two-wheeled motor vehicle (TWMV) user, car occupant). We investigated the effect of the geometry and size of exposure area by repeating the main analysis, but without strata, at radiuses and distances of 0–100, 100–300, 300–500, and 500–1000 metres.

3.0 RESULTS

Excluding the three months either side of camera deployment dates, the average length of time of observation before deployment was 3.1 years, and the average period of follow-up after was 1.4 years. Most sites were first used in the last two years of the study period, and most were located on roads with a 30 mph speed limit (Table 1).

Table 1. Mobile speed camera sites, number by year of camera deployment, speed limit, and road class.

		Number of sites
Year of deployment	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

Based on the 500 m circles method, the overall observed number of crashes was 20 percent lower than expected after camera deployment (RR 0.80, 95% CI 0.72–0.89). Observed numbers were lower than expected in every three-month period after camera deployment, though most of the decreases were not statistically significant (Table 2). The decrease in crashes varied by type of crash (Table 3). Significant decreases occurred during day-time, on 30 mph roads, and for crashes that injured pedestrians and car occupants. Changes were not statistically significant for crashes during night-time, on roads faster than 30 mph, or that injured pedal cyclists or TWMV users.

Table 2. RTC results by time after camera deployment, 500 m radius circle method.

Time (months)	Camera sites Expected unadjusted	Expected adjusted	Observed	Control sites Expected unadjusted	Expected adjusted	Observed	RR (95% CI)
0–3	105.6	104.6	85	120.1	116.5	111	0.85 (0.64–1.14)
3–6	97.5	100.9	84	112.2	112.9	130	0.72 (0.54–0.96)
6–9	85.0	88.1	73	99.1	99.9	105	0.79 (0.58–1.07)
9–12	74.2	76.8	81	85.5	86.4	102	0.89 (0.66–1.21)
12–15	69.8	72.2	67	78.7	79.7	84	0.88 (0.63–1.23)
15–18	58.3	60.6	51	66.8	67.8	58	0.98 (0.66–1.46)
18–21	50.8	53.1	33	57.7	58.8	58	0.63 (0.40–0.98)
21–24	48.5	50.7	33	55.3	56.3	44	0.83 (0.51–1.34)
24+	172.6	185.3	161	206.7	212.6	247	0.75 (0.61–0.92)

Table 3. RTC results, 500 m radius circle method, by time of day, speed limit, and type of road user injured.

	Camera sites Before	Expected unadjusted	Expected adjusted	Observed	Control sites Before	Expected unadjusted	Expected adjusted	Observed	RR (95% CI)
Total	1249	656.8	687.7	586	1403	762.0	774.3	829	0.80 (0.72–0.89)
Day time	867	469.5	492.5	394	1029	548.3	557.0	598	0.75 (0.66–0.85)
Night time	382	187.3	195.2	192	374	213.7	217.3	231	0.93 (0.76–1.13)
30 mph	1045	591.0	616.3	479	1218	669.7	680.0	693	0.76 (0.68–0.86)
40–50 mph	55	21.6	22.6	29	33	22.3	22.8	29	1.01 (0.58–1.74)
60–70 mph	149	44.2	48.8	78	152	70	71.5	107	1.07 (0.79–1.44)
Pedestrian	264	145.9	150.9	111	423	263.1	267.6	250	0.79 (0.62–0.99)
Pedal cyclist	68	43.1	47.1	37	124	82.1	83.6	52	1.26 (0.81–1.96)
TWMV user	77	20.3	20.8	30	66	31.7	32.1	39	1.19 (0.71–1.96)
Car occupant	790	413.8	432.4	366	760	369.4	375.1	504	0.63 (0.55–0.72)

The local effectiveness of mobile speed cameras appeared much greater when a small radius of circle was used to define the extent of local exposure. Evaluation using a radius of 100 m revealed a 61 percent decrease in crashes (Table 4). The RR increased with radius and was 0.94 at 100–300 m, 0.80 at 300–500 m, and 0.98 at 500–1000 m. We found a similar pattern of increasing RR with distance using the lines methods, though with different magnitudes (Table 5). Very near camera sites (within 100 m), both lines and circles methods revealed approximately the same magnitude of decrease in crashes. With increasing distance, the difference between circles and lines methods became greater. At 500–1000 m, the lines method indicated a 21 percent decrease in crashes, compared to a 2 percent decrease revealed by the equivalent circles method.

Table 4. Circles method RTC results, by radial distance from camera sites.

Radius (m)	Camera sites Before	Expected unadjusted	Expected adjusted	Observed	Control sites Before	Expected unadjusted	Expected adjusted	Observed	RR (95% CI)
0–100	86	55.6	60.3	32	116	77.6	82.6	114	0.39 (0.25–0.57)
100–300	436	263	263.6	231	528	312.1	313.7	293	0.94 (0.79–1.12)
300–500	727	338.2	363.8	323	759	372.3	378	422	0.80 (0.69–0.92)
500–1000	1636	1118.3	1116.8	996	1282	1327.9	1357.9	1231	0.98 (0.90–1.07)

Table 5. Lines method RTC results, by distance from camera sites.

Route length (m)	Camera sites				Control sites				RR (95% CI)
	Before	Expected unadjusted	Expected adjusted	Observed	Before	Expected unadjusted	Expected adjusted	Observed	
0–100	73	47.6	52	31	114	61.8	65.3	103	0.38 (0.24–0.57)
100–300	153	87.1	92.7	80	286	165.7	176.4	169	0.90 (0.68–1.18)
300–500	149	70.3	73.6	60	181	91.7	98.9	110	0.73 (0.53–1.01)
500–1000	234	78.1	81.5	75	199	75	81.4	95	0.79 (0.58–1.08)

4.0 DISCUSSION

Very near sites (within 100 m) we found a similar decrease in crash rates using both lines and circles methods. Beyond 100 m, the lines method revealed a larger effect than did the circles method and the difference increased with distance. Significant decreases in crashes occurred during day-time, which is when mobile speed cameras are almost exclusively used, on 30 mph roads, and for crashes that injured pedestrians or car occupants.

The desirability of including number of vehicles or vehicle-distance driven in the denominator for crash rate analyses is well established (Swadling and McInerney, 1999). For the present study no such data were available at the small area level of analysis, so it is necessary to assume no change in these factors occurred at speed camera sites.

Establishing that presence of mobile speed cameras did not cause traffic to divert onto alternative routes and therefore that crash risk did not transfer to other routes would strengthen the conclusion that deployment of cameras caused a decrease in crashes. However, there are insufficient data for camera sites included in the analysis to investigate that issue. Evaluations of static speed cameras have found that crash risk either remained unchanged or decreased to some extent in neighbouring areas or routes (Winnett et al, 1997).

The location of the majority of crashes in Stats19 was recorded with a precision of 10 m, but 42 percent were precise only to the nearest 100 m. Precision of measurement varied between area divisions of the police force. Such imprecision would not have substantially affected the results of circles analyses, since most crashes did not occur near circle perimeters, but will have resulted in some crashes being wrongly excluded from lines analyses and a smaller number being wrongly included. Since the proportion of imprecisely located crashes decreased over the study period, from 48 percent in 1996 to 30 percent in 2000, an association with before-after status exists and could have biased results. The overall effect of this imprecision bias for rate ratios calculated using lines methods is that we may have underestimated the protective effect of mobile speed cameras by as much as a few percent; cameras may be slightly more effective at preventing crashes than we have estimated.

The guidelines for selection of speed camera sites require that cameras only be used at sites where other road safety interventions are considered unsuitable. Thus it is unlikely that any traffic calming or other local road safety interventions would have biased results. However, information on locations of such interventions is not readily available to confirm that inference.

Small numbers within some strata of analysis limited the study’s power to detect changes in crash rates, particularly on 40–50 or 60–70 mph roads, and for crashes that injured pedal cyclists or TWMV users.

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 percent, 12 percent, and 87 percent (NAW, 2001). However, the police categorisation of serious and slight is unreliable with a marked tendency to underestimate severity, compared with severity assessments made at accident and emergency departments (Simpson, 1996). Consequently, we have included all crashes that resulted in any injury rather than only the most severe injuries. We were unable to measure the severity of the injuries prevented as the guidance and rules on data confidentiality do not allow linkage of police and hospital data without the informed consent of individuals, which it is not feasible to collect.

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 an injurious RTC near camera sites. That decrease cannot be explained by confounding by seasonal or annual trends in RTC risk. If any regression-to-mean occurred equally at camera and control sites, which were matched for RTC history, then the apparent decrease in rates cannot be attributed to regression-to-mean bias. RTC rates decreased more very near sites (within 100 m) than further away.

It may be that many drivers slow only when encountering speed camera warning signs, which are typically 100–200 m from sites on 30 mph roads and in the order of 1 km from sites on motorways. The pattern is consistent with results of driver attitude surveys in England and Scotland, in which many drivers reported slowing at speed camera sites but not elsewhere (The Scottish Office, 1997; Corbett and Simon, 1999). The UK government's policy (DTLR, 2001b) 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 (Pilkington, 2002). In year 2000 less than 1 percent of all crashes in South Wales occurred within 100 m radius of mobile speed camera sites included in the present analysis; thus the population level effect of preventing crashes is likely to be small unless authorities adopt strategies to regionalise the effect.

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