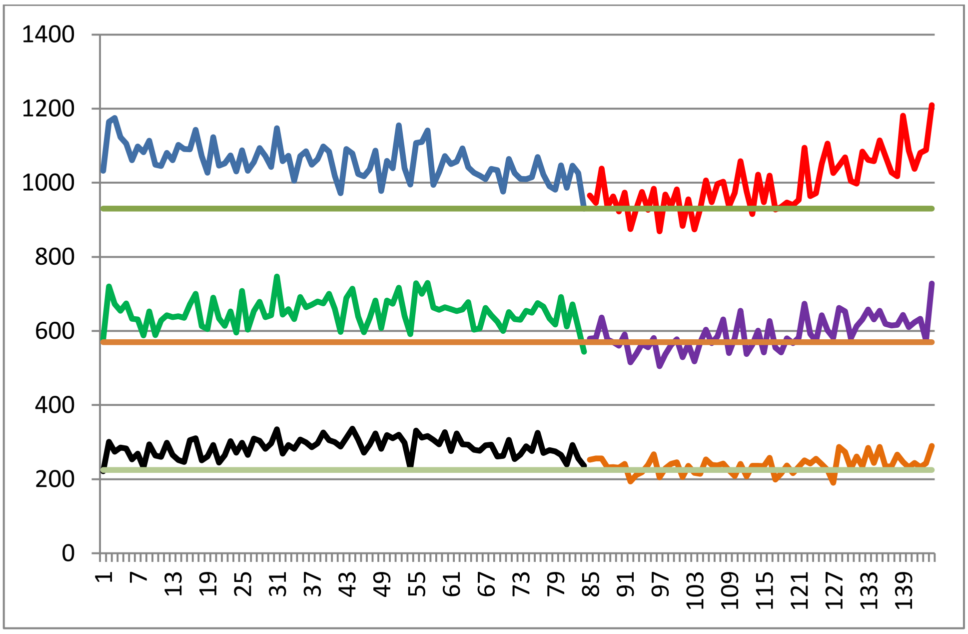
SERC Discussion Paper 221 "Do Speed Cameras Save Lives by Cheng Keat Tang (SERC)
http://www.spatialeconomics.ac.uk/textonly/SERC/publications/download/sercdp0221.pdf

Dear Professor Overman,

Your dismissive response to my 5th December criticisms of the SERC Discussion Paper 221, failing even to mention, let alone challenge, the points I raised, reminded me of the "***My mind is made up, please don't confuse me with the facts***" syndrome with which I have become wearily familiar over more than ten years of bringing speed camera errors to the attention of the authorities. Accordingly I summarise in Appendix A how that blinkered approach, often known as "***being in denial"*** but by psychologists as "***cognitive dissonance***", can "*sustain theories or research programs* ***in the face of inadequate or even contradictory evidence***".

Having read Paper 221 again, I find that it is even worse than I first thought, with many gross errors of fact and method. I have therefore revised and enlarged my original critique to include more detail on more issues and to provide supporting evidence.

I also enclose in Appendix D a brief summary of my now complete large-scale analysis, also based on Stats19 and camera data, that confirms beyond rational dispute not only that there is no **relatively sudden decrease in collision rates following installation of cameras but also that there are significant increases in nearby areas:**



Trend-adjusted **Fatal and Serious Collisions, by month,** within 1km, 500m and 250m radius of **fixed, mobile and red light cameras** from 7 years before installation to 5 years later. **Selection bias** is clear as is the last **RTM** fall immediately before installation (i.e. when the graphs change colour).

I believe that anyone who publishes an analysis of important subjects, especially those with implications for injury and death, is **morally obliged to take seriously any criticism received,** rather than dismiss it out of hand.

I challenge you to prove me wrong on any of the points I raise and I confirm that I am totally confident that my own analysis is correct and worthy of your close attention.

Idris Francis

SERC Discussion Paper 221 "Do Speed Cameras Save Lives by Cheng Keat Tang (SERC)
http://www.spatialeconomics.ac.uk/textonly/SERC/publications/download/sercdp0221.pdf
Errors, Omissions and False or Misleading Assertions

1/ Introduction

2/ Wildly exaggerated costs of collisions

3/ Wildly exaggerated contribution of speeding to accident causation

4/ Failing to recognise the degree of under-reporting of non-fatal collisions

5/ Incorrect and misleading definition of serious injuries

6/ Ignoring nearly 40 adverse effects and serious flaws in official data.

7/ Using calendar year installation dates

8/ Restricting analysis to roads selected for cameras when there was no need to do so

9/ Claiming to deal with selection bias but failing lamentable to do so

10/ Misrepresenting the rules for 3-year selection periods

11/ Claiming wrongly to have dealt with selection bias

12/ Nonsense about regional specific shocks that could correlate with the camera installations

13/ Nonsense about bad weather shocks correlated with camera installations

14/ Failing to realise that 60mph collisions fell because so many 60mph roads were changed to 50mph

15/ Claims to analyse how effects vary across space but only on the specific road

16/ Absurd claims of camera effectiveness

15/ Failure to recognise that higher proportions of fatalities at sites are due to selection bias

16/ The "rigorous" assessment of camera benefits is self-evident nonsense

17/ Absurd claims of camera benefit, far beyond anything possible

18/ Speculation on why fatality reductions are high when the real reason is failure to account for bias

19/ Failure to realise that camera warning signs outside official site boundaries can trigger collisions which are not recorded in site data

20/ Quoting widely discredited earlier analyses

21/ More failure to understand trends

22/ Again the fundamental error of restricting analysis only to the roads with cameras

23/ More claims for far greater reductions than ever involve speeding in the first place

24/ Incredible claims that camera effectiveness continues to increase for many years

25/ More claims that ignore adverse effects in surrounding areas

26/ Cost/benefit analysis - the economics of the madhouse, from beginning to end!

Appendix A Refusing to Face Facts

Appendix B Selection bias, Regression to Mean, Trends and random variations

Appendix C Official data is fundamentally flawed

Appendix D A far better method that eliminates all the problems

1/ Introduction

Every analysis of the effects of speed cameras from the Eight Area Trial of 2000/2 to date has been littered with obvious errors and omissions, false assumptions, inappropriate or skewed analysis, low volumes of data, wide confidence intervals and/or absurd claims that reductions of a few mph in average speeds reduce collisions by far more than the proportion that involve speeding in the first place. In particular, the effects of selection bias and regression to mean have been widely misunderstood and misrepresented as camera benefit. These serious errors have then been compounded by wildly exaggerated figures for the cost of collisions and the savings achieved by preventing them.

Paper 221 not only repeats many of these errors but also introduces new ones, all are highlighted below in ***red italics***:

2/ *"Collisions cost UK a total of £10.3 billion just in 2015"*

Comment

Simply untrue. The statement seriously misrepresents the (2012) DfT figures at https://www.gov.uk/government/uploads/system/uploads/attachment_data/file/254720/rrcgb-valuation-methodology.pdf

Table 2: Average value of prevention of road accidents by severity and element of cost: £GB 2012

	Casualty related costs		
Accident Severity	*Lost output*	*Medical and Ambulance*	*Human costs*
Fatal	***635,233***	*5,529*	***1,247,433***
Serious	*25,157*	*15,095*	*171,356*
Slight	*3,163*	*1,342*	*15,073*
All injury accidents	*13,429*	*3,364*	*51,370*

Human costs reflect the non-resource element of the costs associated with human life of the effects of injury, such as the pain and distress felt by the accident victims or their relatives, as well as the intrinsic loss of enjoyment of life in the case of fatalities. Costs are based on estimates of people's WTP [Willingness to Pay] *for small reductions in the risk of exposure to such effects.*

*Estimates of the total value of prevention of road casualties and road accidents in Great Britain during 2012 are provided below. The estimates were derived using the values for prevention of casualties and accidents listed above, and are cost benefit values that represent the **benefits** which would be obtained by prevention of road accidents. **The estimates do not represent actual costs incurred as the result of road accidents.***

Further, any economist should realise that the **lost output of anyone who dies on the road** (or anywhere else for that matter) **is offset by what he no longer consumes**. In simple terms, 30m workers support a population of 60m so average output is double average consumption. The average age of a road fatality being 46, some 20 years output lost (assuming no one else takes his place) would be more or less cancelled out by what he no longer consumes. Several years after I first pointed this out Professor Richard Allsop, author of camera reports for the RAC Foundation, confirmed to me not only that I was right but also that the NERA Report of 18th March 2011 advised the DfT that "***the net lost output of road fatalities is on average negligible"*** and that,

> For *fatalities*, the current convention for estimating lost net output is incomplete. It omits the "negative output" of individuals over some periods of their lives, in particular in later life in the form of consumption funded by, especially, pensions and state funded health and personal social services.

When I checked this with the DfT recently they copied me 4 successive updates of a

VPF/ VPI – Post Phase 1 work plan including

Lost net output for road fatalities: This should be assumed to be negligible.	On full update of VPF	Agreed	No immediate action	n/a

Also, as **output is largely determined by demand not by labour supply, it is at least arguable that lost output for non-fatal injuries should also be assumed to be negligible** because casualties will be routinely be replaced by others to ensure that output meets demand, in the same way that employees who retire, change jobs, become unable to work or die for other reasons are replaced.

Table 3 of the same DfT document shows totals of casualty and accident related values:

Totals……………………………………………………………………………………………………… £**10,589**bn

Remove the non-cash Human Costs of…………………………….£**7,478**bn………….£**3,111**bn

Remove the spurious Lost Output of fatalities……………….. £**1,040**bn………….£**2,071**bn

Remove arguably spurious Serious and Slight Lost Output,….£**915**bn………….£**1,156bn**

Mr. Tang's figure of £10.3bn therefore overstates by a factor of 5 the real cash costs of injury accidents, rising to a factor of 10 if, as seems reasonable, the lost output of non-fatal casualties is also negligible.

That is not of course to say that factors such as pain and suffering are not important, only that the values assigned to them are **not cash costs that could be avoided** but subjective values for use in Willingness to Pay calculations.

That speed cameras do not in any case reduce collisions (see below) means of course that the above figures are not relevant to but they are worth mentioning because they (a) highlight gross errors in the Paper and (b) are widely used by other Government departments.

3/ *According to Department for Transport (DfT), speeding accounts for more than 60% of the fatal accidents in UK in 2015 (e.g. exceeding speed limit, travelling too fast for conditions, loss control of vehicle, swerved vehicle).*

While speeding might be considered a menial offence to many, it is evident that it is immense in determining both the probability and gravity of crashes.

Comment

Again, simply untrue

Table RAS5008 Police Stats19 causation analysis for **2015** recorded these percentage contributions to accidents,

	Fatal	Serious	Slight	All
Exceeding speed limit	***15***	***7***	***4***	***5***
Going too fast for conditions	***8***	***7***	***6***	***6***

These numbers have not changed significantly since the current system was introduced in 2005

Even when either "speeding" box has been ticked, it can be the ***precipitating factor*** or one of 5 other degrees of lesser significance. Accordingly many of those collisions would still have happened even if speeding had not been involved.

As the figures include "***possible***" as well as "***very likely***" involvement they are inherently overstated.

As cameras **far from eliminate speeding** and not infrequently **cause collisions** it is unlikely that maximum benefit could exceed **8, 4, 2 and 3%**.

It is clearly misleading and unacceptable to include in the "*speeding*" numbers other factors such as "***travelling too fast for conditions, loss control of vehicle, swerved vehicle"*** when the investigating officer decided not to tick even "*possible*" speeding. The effect of this exaggeration, deliberate or otherwise, is of course to make those wild claims seem less absurd than they really are.

4/ *It is possible that there could be under-reporting of non fatal accidents to the police. This should not be an issue for more serious crashes that are usually reported to the Police. As long as the under-reporting of accidents is random across time and is not correlated with camera installation, it should not bias my estimates*

Comment

Wildly wrong again!

In fact the DfT have known for decades that even serious collisions are significantly under-reported and **analyses** have been carried out at least since 2006 and estimates have been included in the annual RRCGB reports after the Transport Select Committee pointed out that it made no sense at all that from 1996 to 2007 serious injuries had continued their long-term downward trend while fatalities almost stopped falling. From memory, one DfT report estimated that the reporting level of serious injuries fell over that period from 37% to 28% i.e. by 33%. Extensive comparisons of hospital records and Stats19 data have shown large differences.

In one extreme example, Thames Valley Police confirmed that their 60% increase in SI in 1999, reversing a 40% reduction in 1991, had been due to re-training in differentiating between slight and serious injuries after the authorities noticed a large discrepancy in their area. So it's not just a question of reporting levels, it's also a question of how capable police officers are of determining the degree of severity of an injury.

It is of course impossible to determine in retrospect what the precise figures were or how they have changed and we have no alternative but to use Stats19 as being the best data available despite reservations about its accuracy. The importance of this point is that it **is yet another example of an incorrect and misleading assertion that Mr. Tang could and should have checked.**

5/ *According to the definition provided by the Department for Transport.Serious Injury is when the injury causes the person to be detained in the hospital for medical treatment and that the injury causes death more than 30 days after the collision.*

Comment

Simply wrong! The real definition is:

Serious injury:

An injury for which a person is detained in hospital as an in-patient, <u>or any of the following injuries whether or not they are detained in hospital: fractures, concussion, internal injuries, crushings, burns (excluding friction burns), severe cuts severe general shock requiring medical treatment and injuries causing death 30 or more days after the accident</u>

6/ *Finally, questions are also raised whether these devices induce more collisions due to "kangaroo" effects (Elvik, 1997). That is when drivers abruptly slow down in proximity to the camera or immediately speed up beyond surveillance, causing more accidents further away from the camera. Thus, the objective of this Paper is to address these questions through rigorous empirical analyses.*

Comment

There some 40 such adverse effects, not just 1, including sudden braking, distraction, tailgating, bunching, overtaking etc, many of which clearly extend well beyond 500m, and not just on the particular roads that have cameras but also on nearby roads. see https://www.google.co.uk/url?sa=t&rct=j&q=&esrc=s&source=web&cd=1&ved=0ahUKEwjOkrWR_ObXAhViCcAKHYHyBIYQFggnMAA&url=http%3A%2F%2Fwww.fightbackwithfacts.com%2Fwp-content%2Fuploads%2F2011%2 F07%2FE.22-DfT-cancels-camera-side-effect-research.doc&usg=AOvVaw1Edna6jIvNGQsgZgYkGIVt

By using data only from roads on which cameras were installed Paper 221 completely ignores these adverse effects of cameras in nearby areas. The much better method summarised in Appendix D confirms not only that there is no significant reduction in collisions following camera installation but also that there are **significant increases in the wider area due to these adverse effects.**

7/ *information on the location of camera housing, <u>year of installation</u>, speed limits and camera type are provided…………………… This is possible as I can <u>accurately</u> determine the installation dates of fixed speed cameras.*

Comment

Mr. Tang's sensible use of Stats19 collision data for greater precision in terms of location and date was severely compromised by his use of **calendar year installation dates that make it impossible to monitor the short-term changes** that would occur after installation of effective cameras. And equally **impossible to determine which collisions in the year of installation happened before installation and which aft**er – a rather important distinction! It was for these reasons that this analyst's much better method (Appendix D) uses **month-by-month** installation dates of some 3,800 cameras.

8/ *Roads with enforcement cameras are peculiar accident-prone roads with many drivers exceeding speed limits. Those without cameras are likely to very different from those with. Hence, the strategy adopted in this paper is to <u>restrict the sample to only sites with cameras</u> and exploit the variation in the timing of installation.*

Identification stems from comparing changes in accident outcomes with changes on roads that will have cameras installed in the near future. The assumption is that sites that have enforcement cameras in the future are quite similar for sites that have cameras installations now.

Comment

The first assertion is **a matter of opinion not fact** because (a) many cameras are installed at public concern sites that have no significant record of collisions (b) many sites that do meet selection thresholds over a particular 3-year period never do so again (c) many sites that meet selection criteria do not have cameras installed and (d) most drivers believe that cameras are located to maximise revenue not safety.

If as it seems Mr. Tang differentiates between sites that have cameras at some stage and those that do not because he assumes that those differences lead to different trends, he could not be more wrong as Fig 1 from Appendix D and below confirms:

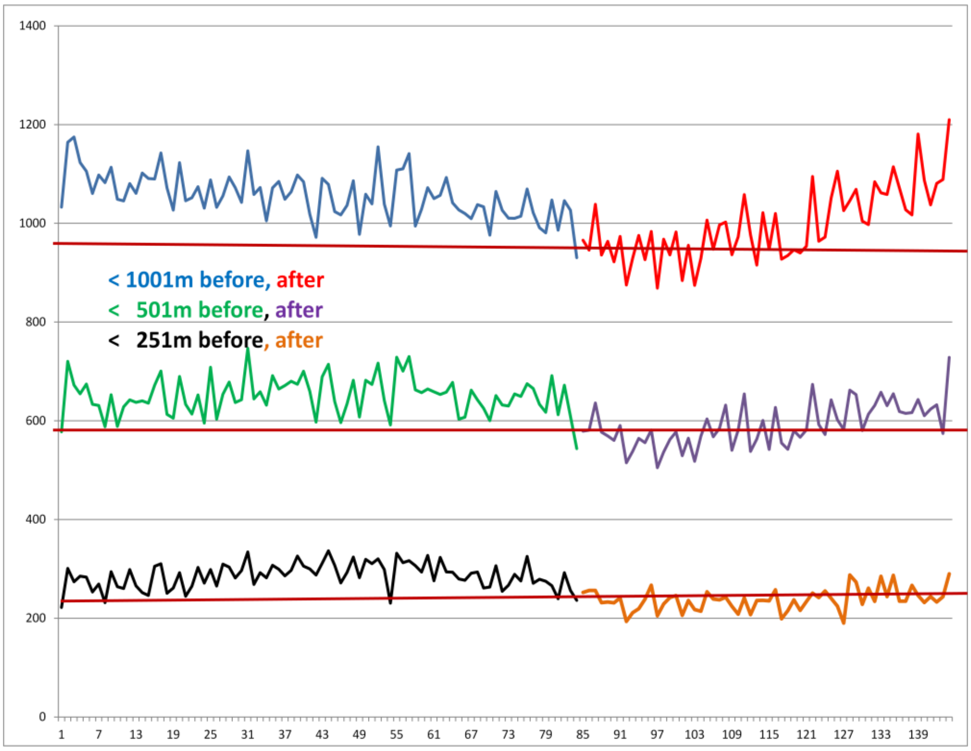


Fig.1 FSC within 250m, 500m and 1,000m of fixed, mobile or red-light cameras, from 7 years before installation to 5 years later.

FSC adjusted for the overall trend more than 1,000m from any camera, in the same 23 police areas.

It is clear that **collisions near cameras follow essentially the same trend as collisions further away**, apart from selection bias prior to installation and later marked increases due to adverse effects.

Equally importantly, even if sites that received cameras were ***very different*** from those that did not, **that does not mean that they are *similar* enough** to justify the Mr. Tang's comparisons of pairs of sites. Accordingly the **entire analysis is invalidated by the false assumption that (in the absence of cameras) outcomes would me similar at each site of a pair.**

In addition Mr. Tang's **abject failure to deal with selection bias** (see section 9 below) confirms only that he does not understand it and that in consequence **his claims of camera benefit are as wildly inflated by RTM falls in the same way as most other analyses.**

9/ Pre-treatment accident outcomes are computed by averaging the number of collisions within 2 kilometres from the housing five years before the camera is installed. For instance, if a camera is installed in 2000, I will take the mean of annual crashes from 1995 to 1999.

Comment

Earlier literature addressed selection bias in many ways ranging from ignoring it completely to recognising but trivialising it, to the Empirical Bayes method, to averaging pre-installation data over 6 years instead of 3 and even to attempting to determine the rate at which RTM falls occur despite it being clear from basic principles that they occur the moment selection periods end!

Common to all such attempts to quantify bias and RTM is the failure to recognise that it is impossible to do so accurately from the data which is or is ever likely to be available – see Appendix B.

10/ ***At least 4 killed and serious collisions (KSI) & 8 personal injury collisions (PIC) per kilometre in the 3 years before installation***

The statement **misrepresents** the national site selection guidelines which in fact allow **any** period of 3 consecutive years before installation (as indeed they must, because data acquisition, site selection, planning and logistic delays can result in **cameras being installed up to 4 years after the end of the site selection periods)**. These extended delays are occasionally visible in partnership data and are clearly visible in the graphs in Appendix D and Fig, 2 below.

11/ ***In contrast to earlier literature, I address the selection bias by analyzing only sites that will ever have a difference-in-difference framework that relies speed camera installed.***

Thus, in column (7), I restrict the reference groups to just recently treated cameras by excluding any observations more than 3 years before and after the camera is installed.

As his 5-year averaging periods are bound to include 3 years of selection bias at most sites**,** Mr. Tang has **clearly not addressed the selection bias problem**! The effect of his method is to exclude any selection bias more than 3 years before installation of control sites but not of sites with cameras. This too invalidates any comparison of the two sites.

The degree of bias at each site will vary depending on (a) the extent to which collisions in the selection period **exceed,** rather than just meet, the selection threshold (b) **what selection threshold, if any was actually applied** and (c) whether the **installation delay was long enough** for one or more years of the selection period to have fallen prior to the 5 year period (or the 3-year period of some control sites).

Further, even if Mr. Tang's figures for the ***pre-treatment accident outcomes*** of a matched pair of sites were the same, **one could be the average of low normal levels and high selection bias and the other the average of high normal levels and low bias, again invalidating any comparison.** In addition of course collision numbers at individual sites, are much affected by random chance as well as different local trends making it even less likely that changes in collision rates at the 2 sites would be the same if cameras were not installed.

For all these reasons the basic premise of Paper 221, that the method accounts for selection bias is simply not credible.

Note also that ***robustness tests prove*** **nothing if the input data is flawed –** the age-old warning "*Garbage in, garbage out*" always applies.

12/ ***Next, I include a vector of time-variant local authority (LA) characteristics to partial out regional specific shocks that could correlate with the camera installations and affect outcomes. This include demographic (population size and % of population between 18 to 25) and labour characteristics (gross annual salary and working hours). Controlling for these differences has an inconsequential effect on the estimates.***

Comment

An absurd waste of time and effort – there was never any possibility of meaningful correlation of that kind, especially over the relatively short comparison period.

13/ ***I further include a number of weather controls including temperature and wind speed. The concern is whether bad weather shocks, which could induce more accidents, are correlated with camera installations.***

This significantly reduce the sample by more than two-third due to missing data but again did not change the estimates much.

Comment

Again an absurd waste of time and effort – there was never any possibility that there would be any meaningful correlation of that kind, especially as camera installation over many years would tend to average out any such shocks.

14/ ***From Panel A, although I find significant improvement in road safety across different speed limits, more pronounced enforcement effects are documented along roads with higher speed limits. Specifically, the number of collisions fell by 50 to 57% along 60mph roads compared to 22 to 25% along 20mph roads.***

Comment

Mr. Tang may be unaware that **so many 60mph roads were changed to 50mph** over those years that 50mph collisions rose steeply as 60mph collisions fell! In any case it is simply not possible for modest reductions in speed to reduce collisions by 50 to 57%, some 10 times the proportion that involve speeding in the first place.

15/ ***Third, with fine spatial temporal information on accidents and speed cameras, I can accurately capture how enforcement effects vary across space, allowing us to understand whether cameras exacerbate collisions away from the site.***

Comment

Temporal information is not ***fine*** because installation dates are provided only by calendar year.
The ***space*** referred to is restricted to the roads which have cameras at some stage, **with the result that adverse effects on nearby roads are ignored**. This author' analysis using circular sites (Appendix D and Fig.1 above) shows that **collision totals in the wider area (a) do not fall promptly after installation and (b) increase from the 2nd year at least to the 5th year after installation.**

Clearly, if collisions fall within a small area but rise within a larger area that includes it, it is **the results in the larger area that matter.** This is a fundamental error in the SERC analysis and all others that **rely on official site data for narrowly-defined sites** (see Appendix C)

16/ ***Finally, utilizing estimates from my analysis, I compute the welfare effects associated with speed cameras to provide rigorous assessment whether these devices should be deployed.***

Comment

Given that the assessment of cash benefits and collision reductions are both nonsense**, the word *rigorous*** is **not remotely appropriate**

17/ ***The headline finding is that speed cameras unambiguously reduce both the counts and severity of collisions. After installing a camera, the number of accidents and minor injuries fell by 17%-39% and 17%-38%, which amounts to 0.89-2.36 and 1.19-2.87 per kilometre. As for seriousness of the crashes, the number of fatalities and serious injuries decrease by 0.08-0.19 and 0.25-0.58 per kilometre compared to pre-installation levels, which represents a drop of 58%-68% and 28%-55% respectively.***

Putting these estimates into perspective, installing another 1,000 speed cameras reduce around 1130 collisions5, mitigate 330 serious injuries, and save 190 lives annually[6], generating benefits of around 21 million[7]. These findings are robust across a range of specifications that relaxes identification assumptions

Comment

That only 4% of slight, 8% of serious and 15% of fatal collisions involve (or might involve) speed- above limits - and then not necessarily as the main or precipitating factor, that speeding is far from eliminated and cameras cause collisions, **those claims are clearly nonsense.**

18/ ***The ratio of lives save in my study is much higher than the average national accidents death ratio over the last 10 years from 1995 to 2015 (1.02%). There are several explanations to this. First, speed cameras are often found along roads with a much larger proportion of death related accidents. The pre-treatment percentage of deaths from collisions around speed camera sites is 2.50% (see Table 1) more than twice the national ratio.***

Second, by reducing speed through deterrence, cameras could have disproportionately mitigated more severe accidents. Another explanation is that speed cameras are less effective in preventing collisions compared to deaths. Possible kangaroo effects, such as sudden braking in front of camera, or speeding up beyond surveillance, could have attributed to more collisions.

Comment

Not surprising given that most camera sites are selected for 3 or 4 fatal or serious collisions in recent years, that (see section 9) Mr. Tang fails lamentably to deal with selection bias and that the high figures in selection periods are mostly random variations, not long-term high levels.

It is also likely that reductions near cameras are accompanied by increases in surrounding areas.

19/ ***Once installed, several clear signages must be placed less than 1 kilometre away from the camera. This is to warn drivers about the presence of camera and to inform them about the speed limit.***

Comment

All such warning signs, including many that are nowhere near cameras, risk triggering adverse effects in the wider area outside official site boundaries, adverse effects which the analysis ignores but which this analyst identifies (Fig.1)

20/ ***Previous literature, largely from transport engineering , show that speed cameras reduce travel speed, accidents, injuries and fatalities near the camera (Gains et al., 2004, 2005; Chen et al., 2002; Shin et al., 2009).***

Comment

The four analyses by Gains *et al* are fatally flawed by their **failure to correct for selection bias/RTM and for many other reasons including narrowly-defined sites which ignore nearby adverse effects and failure to realise that the DfT's Hen1 numbers are Willingness to Pay estimates not cash.** Even then Gains claim a benefit/cost ratio of only 2.7 to 1 when the true figure would be well below 0.1 even if the reductions claimed were valid, which even Gains admitted in 2005 they are not.

21/ ***Without controlling for the general downward trends of accidents due to technological advancements over time, such as better brake system, more robust car frame and improved road built, it is likely that the documented enforcement effects are biased. For studies with control groups, they do not account for the fact that camera location choices are endogenous.***

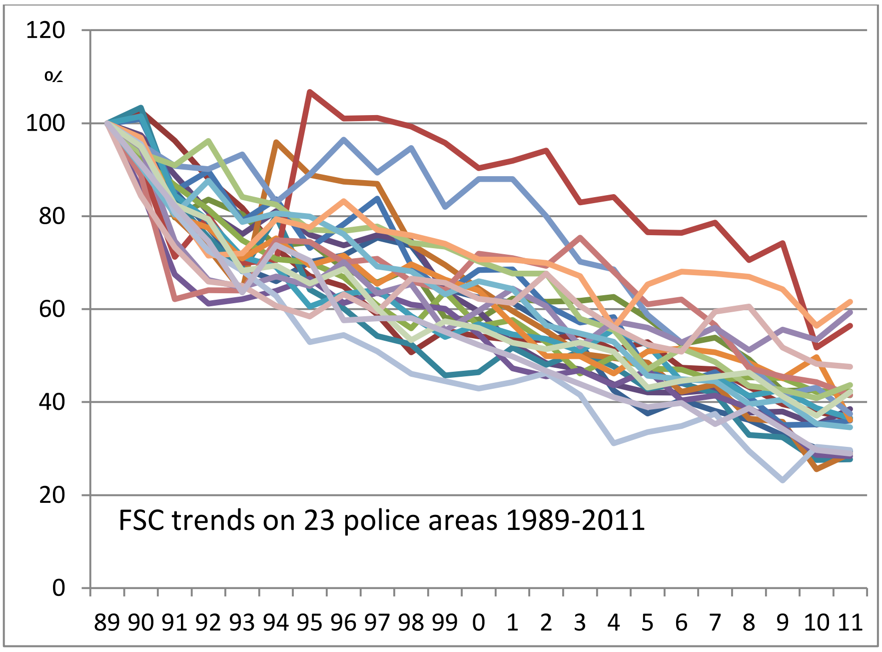
As mentioned before, selected sites are peculiar accident "black" spots with many drivers exceeding speed limits. Hence, sites with no camera installation are unlikely to be comparable. These differences, if unobserved or imprecisely measured, will enter the specification and bias the estimates.

Comment

See also section 8

It is of course true that failing to allow for general downward trends will bias estimates of camera effects but it is unclear how Mr. Tang has done this, if indeed he has.

The fundamental trend problem is that the smaller the number of collisions the less likely they are to follow national or local trends. Indeed, even relatively large numbers of collisions in different police areas follow quite different trends and it is logical to assume that they vary even more within any each police area. To put that another way, small volumes of data are too volatile to reveal underlying trends.



Fig, 2

It is therefore impossible accurately to adjust data for individual sites for trend or to adjust data for matched pairs of site to take account of trend changes over the period covered.

That is why this analyst's better method (Appendix D) adjusts **only totals of large volumes of data, for the trend of the same overall area. Fig 1 in Section 8 confirms that trends can be accurately taken into account as long as the numbers are very large and for the same overall area.**

22/ *Finally, there is also a lack of analysis on how these enforcement cameras fare over time and over different speed limits. Combining the various sources of information using Geographic Information System (GIS), I am able to match the location of speed cameras and accidents to the road network. To visualize, refer to Figure 3 and imagine the line as a particular stretch of road with a camera installed. With the exact location of each accident, I could sum accident outcomes along the road that the speed camera installed. With the exact location of each accident, I could sum accident outcomes along the road that the speed camera is installed annually between k and k - 100 metres interval. For example, within 100 metres from the camera, all accidents that take place in area "A" in a particular year are accounted for. For my baseline estimates, which examine the effects 500 metres left and right of the housing, I will aggregate all the accidents that took place in "A", "B", "C", "D" and "E".*

Comment

By limiting itself to specific roads with cameras the analysis ignores the many adverse effects which can and do occur nearby. **This invalidates the results.**

The great majority of cameras are on 30mph roads and speed limits have been reduced on many others (especially 60mph to 50mph) making differentiation by speed limit problematical.

23/ ***I observe collisions fell by 17% to 39%, representing an absolute reduction of 0.89 to 2.36 per kilometre per annum. Slight injuries fell by between 1.19 and 2.87 per kilometre per annum, which amounts to a 17% to 38% decrease. There are between 0.25 and 0.58 less serious injuries surrounding the camera, equivalent to a 28% to 55% fall from pre-treatment levels. Largest reductions in relative levels are documented for deaths. On average, there are approximately 0.08 to 0.19 less fatalities per kilometre, which represents a massive 58% to 68% decline[19].***

Specifically, the number of collisions fell by 50 to 57% along 60mph roads compared to 22 to 25% along 20mph roads. Similar larger decreases are observed for serious injuries, at around 87-88%, and deaths, at around 94-95%, on 60mph roads. In contrast, serious injuries fell by 36-41% along 20mph roads and no significant reductions in deaths are reported. There are several explanations to this. First, it could be that drivers along the lower speed limit roads are already commuting slowly and insignificant reductions in speed achieve by cameras do not matter much in reducing the gravity of collisions. Second, attenuated enforcement effects for more binding speed limits suggest that drivers may be forced to hastily drop speed so as not to be fined, inducing more collisions in some instances that could reduce enforcement effects.

Comment

The reductions claimed **could never be achieved by cameras**. Further, the adverse effects over wider areas exceed the benefits near cameras (Fig.1 above)

24/ ***Next, I examine how the effectiveness of speed cameras vary over time. Results are summarized in Figure 10. This is to understand whether the enforcement effects diminish over time to justify the decision to switch off the cameras. Results reveal that cameras remain effective and in fact become more potent in reducing collisions and fatalities over time. Weaker effects in the beginning suggest that some drivers could be unfamiliar with the locations of camera. They could be forced to abruptly drop speed to avoid fines, inducing more crashes. Over time, drivers learn about these locations and are less prone to reckless braking, explaining stronger enforcement effects.***

Comment

It is simply **not credible that camera benefits continue to increase for many years after installation**! Most drivers have been familiar with cameras for years so any benefit of a particular camera from installation onwards will increase as the proportion of passing drivers aware of the camera increases – i.e. rapidly at first, from zero and then asymptotically towards a likely maximum of 90% or so, within a year or so. Nor is there any reason to believe that drivers long familiar with other cameras would take years to "learn about these locations".

In any case this analyst's better method (Appendix D) shows that those benefits do not exist.

25/ ***This result is fairly consistent across the different accident outcomes and correspond to earlier literature (Liet al., 2013). Beyond 700 metres from the device, fixed speed cameras are no longer able to enhance road safety. Moving further away, beyond 1500 metres from the camera, there are suggestive evidence of kangaroo effects as I report small rebound in the number of collisions, serious injuries and deaths. A small proportion of drivers could have speed up beyond the surveillance of cameras, inducing more collisions post implementation. However, these effects are quite small compared to the enforcement effects from cameras.***

Comment

As before, there are close to 40 such effects including tailgating, bunching, loss of concentration, irritation, the perceived need to make up lost time and many others. This author's analysis shows that these adverse effects far exceed any benefits near cameras.

26/ ***Cost-Benefit Analysis***

This section reports a cost benefit analysis on speed cameras. The costs include the fixed and operating costs of camera and the time delays incurred by passing drivers, while the benefits include the savings from less collision, injuries and fatalities.

Initial comment

For reasons already given and others below, **this analysis is nonsense from beginning to end and would still be nonsense even if the absurd claims of collision reductions were valid**.

For the benefits, I rely on the savings per traffic accidents, injuries and deaths computed by DfT. These values account for both (1) casualty-related costs (loss output, medical and ambulance, human costs) and (2) accident-related costs (property damage, insurance and administrative and police costs). Total savings are computed by multiplying earlier estimates on reductions with the savings on per capita or accident basis.

For more information, refer to https://www.gov.uk/government/uploads/system/uploads/

attachment_data/file/254720/rrcgb-valuation-methodology.pdf

the annual net loss in income from delays per camera per annum amounts to £346,749. The cost of installing a fixed speed camera is approximately £59,000 and the operating and maintenance cost is around £12,441.

Even without considering these perks, net benefits generated per camera amount to around £21,119 per annum, justifying the implementation of cameras.

Comment

I learned long ago to be wary of those who enter estimates and approximations into equations and then quote the result to 5 or more significant figures! **Do they not realise that their results can never be more accurate than the data they use,** or that while their calculators or computers provide results to many significant figures, all but the first few are meaningless? Again, *Garbage in, Garbage out.*

Assuming a linear relationship between cameras and collisions, putting another 1,000 speed cameras on roads could reduce approximately 1130 crashes, preventing around 330 serious injuries and in turn, saving 190 lives every year and generating benefits of up £21 million. These results remain robust across a range of specifications that relaxes the identification strategies. Dwelling further, however, reveal that these effects are largely localised within 0 to 500 metres from the camera and there are suggestive evidence of a rebound in collisions further away from the camera.

Comment

The first 4 reports by Gains et al claimed that some 4,000 cameras had saved around 100 lives – though in their 4th report (2005) they admitted that the number had been exaggerated by their failure to adjust for selection bias/RTM. Appendix H of the same report estimated that 75% of the observed reductions in KSI, after trend, were due to RTM and only 25% due to cameras. Yet Paper 221 claims that just 1,000 more cameras would save 190 lives, **some 30 times more effective!** Clearly nonsense!

This illustrates the possibility of drivers speeding up beyond the surveillance of cameras and inducing more accidents. Nevertheless, simple cost benefit analysis reveals that the perks from installing a camera are marginally larger than the cost of cameras.

Comment

Marginal indeed – and far less than the obvious margins of error in the raw data – see below.

But with technology advancement, newer prototypes, such as mobile and variable speed cameras, should be considered to circumvent the weaknesses associated with fixed speed cameras and more effectively deter speeding.

Comment

It is inconceivable that any improvements in speed cameras could bring about meaningful reductions in collisions, let alone do so cost effectively. Some £2bn to £3bn has been sent over the last 25 years but the result has been more collisions and injuries than would otherwise have occurred.

Cost of Slowing Down Traffic at Camera Sites

For the costs, to compute the time delays from speed camera, I rely on estimates from Gains et al. (2005). Speed is approximately 10kmh slower after the camera is implemented. Taking the average speed limit of 58kmh22 (30mph) and a distance of 1km around the camera, drivers incur a delay of 1.24 minutes whenever they pass a speed camera. Based on the Annual Average Daily Traffic (AADT) flow along roads with speed cameras from DfT, I assume that there are approximately 18,500 vehicles bypassing each camera every day, corresponding to around 6.75 million annually. In total, assuming an average occupancy of 1.56 per car, time delays incurred by all bypassing vehicles amount to 217,700 hours every year.

To compute loss of income from time delays, I assume that 63.3% of the commuters are between 16 to 64 and are working according to population estimates. As of 2015, employment rate in U.K is around 74.5%. After excluding holidays and weekends, there are about 261 working days annually and if individuals work around 8 hours every day, and taking median hourly wage as £14.17/hour, the annual net loss in income from delays per camera per annum amounts to £346,749.

Comment

I am not aware that any other analysis of these costs but it beggars belief that the figure of £**346,749**, based on **8 different estimates,** each of which is subject to significant margins of error and variation by area and time, **is shown to 6 significant figures that imply a wholly spurious degree of accuracy.**

In any case, many of the assumptions made are questionable - why use the annual net loss of **workers' income** instead **their output** – typically twice as much? Also, many workers who arrive late for work due to delays **stay on to make up for it** while delays driving home would normally results in lost leisure time, not lost working time. In other words, **all of the numbers used in these calculations are highly subjective and it would be very easy to obtain any desired result simply by changing them.**

Table 5 on pg. 33 of the analysis provides the detailed figures for the cost/benefit comparison:

(A) Total Benefits		**£439,279**
(B) Loss of Income per camera	**£346,749**	
(C) Total Camera Costs per year	**£ 71,411**	
	£418,160	**£418,160**
Net Costs/Benefit		**£ 21,119**

Comment

The economics of the madhouse! No business in its right mind would embark on, let alone later seek to justify, a project involving costs in excess of £400,000 a year **in the hope of making a 5% profit, a figure significantly less than the margin of error of each of the estimates involved!**

Given that most the supposed **cash benefits do not exist (section 2) and the cash cost of slowing traffic is so very dependent on the assumptions made, the entire analysis is preposterous and were the subject not so serious, would be laughable.**

Idris Francis
Idris.francis@btinternet.com
01 Jan 2018