How Flawed Analysis and Wishful Thinking Wildly Exaggerated Speed Camera Benefits

Writer's Note: Words in italics are quoted from these reports or others referred to .

Part 1 - Department for Transport "A cost recovery system for speed and red-light cameras - two year pilot evaluation" Research paper 11 February 2003

This was the pilot study which was then rolled out across the country as the Hypothecation (aka Netting Off) Scheme including more than 4,000 cameras - despite these following blatant anomalies:

1/ Claims of ***65% KSI reduction*** at fixed camera sites, ***29%*** at mobile camera sites, after trend - despite speeding being far from eliminated and despite being involved in no more than **9% of all KSI accidents**. It would have been absurd for Report to claim that reductions of that scale could be achieved by elimination of all speeding, let alone to the often modest reductions actually achieved. (See Introduction pg.2).

2/ "***Public reaction has been positive*"** It is axiomatic in professional safety circles that the opinions of the uninformed public should play no part in policy (unlike **in politics of course, where public opinion is often paramount).** That the general public, in this context, know little more than the official propaganda, often written by those with little or no understanding of the subject, serves only to confirm the irrelevance of those reflected opinion. In any case, as is well known, poll results can easily be skewed by the questions asked. .

3/ " ***these constraints could be removed by allowing local road safety groups to recover enforcement costs from fine income***". What the planners failed to realise (though they could and should have done) was that those whose jobs depend on income from fines will be interested only in more cameras and more fines. Indeed, that was **why the scheme was closed down in April 2007** as para. 116 of Transcom's 2006 Report reported:

"The Department and the Home Office were reticent when it came to recommending more cameras. The Transport Minister suggested that partnerships should be encouraged to look at solutions to speeding other than cameras. He told us: "It was clear to us that, in certain areas, partnerships had formed which might be minded to look first for a road camera based solution rather than a better and perhaps more cost effective solution." Later the report states that "*This was necessary to ensure that the system did not distort operational priorities, for example using the system to generate revenue* [and protect jobs] *rather than address a specific problem*."

Only a year or two later however when Local Authorities cut camera funding in the recession, Acpo stepped in with a replacement scheme suffering same perverse incentive to maximise cash flow, not road safety. (That Acpo e-mailed to most local authorities blatantly misleading claims of camera benefit in an attempt to to help secure local authority funding surely amounted to the a breach of their a-political remit).

4/ "***The pilots were launched in April 2000 and were originally envisaged to run for two years. However, results from the first year were so encouraging that the Government took the decision to extend the system nationally***". Absolute nonsense! Any novice analyst knows that, even on a national scale, at least three years' data is needed to assess and identify changes of trend or the effects of any safety intervention. For that reason, the data for a relatively small number of sites over only one year was all-but statistically meaningless - yet was used to launch a scheme that scheme as now cost taxpayers the best part of £2bn, and many millions of drivers fines, penalty points and many their licenses and jobs and business. And to achieve what? Little or nothing, as we shall see:

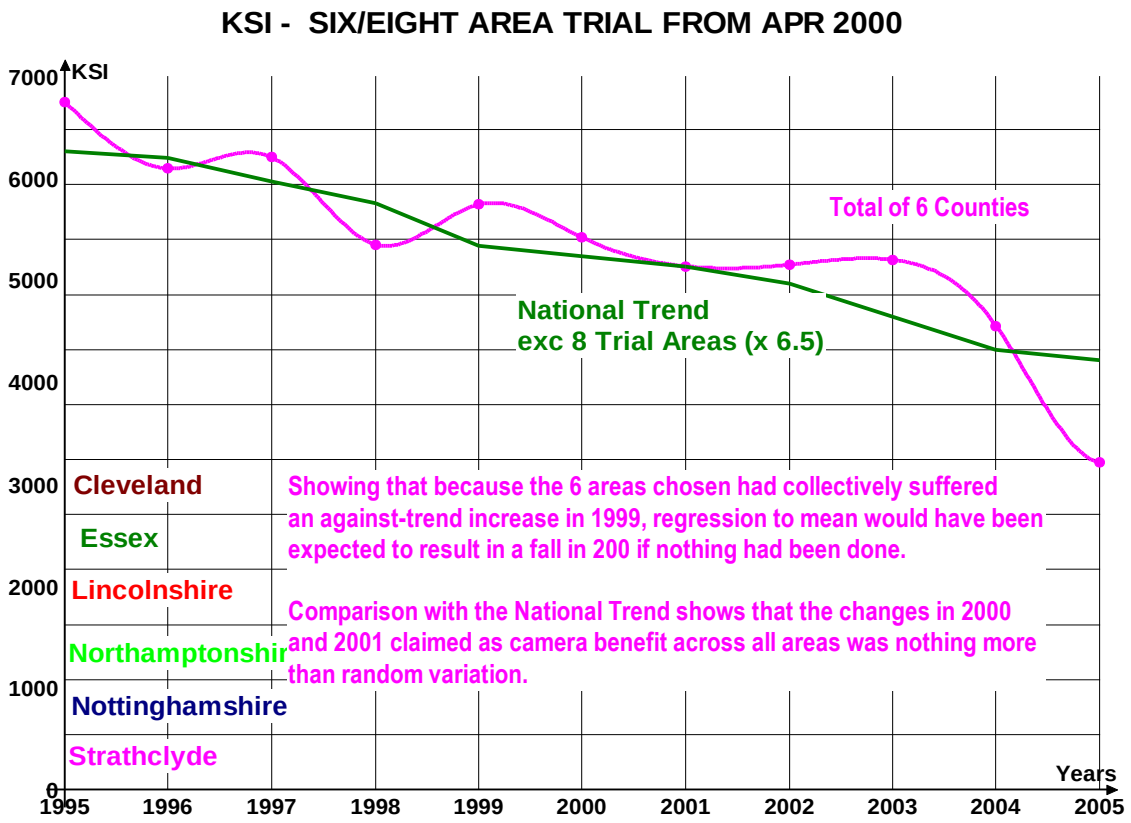
5/ ***During the study period there was a change in the recording of serious casualties in South Wales and Thames Valley and so their casualty results have been analysed separately to the other six pilots.***

What actually happened was that during the "before" baseline Thames Valley and South Wales police re-trained their officers in the subjective and difficult differences between "Serious" and "Slight" casualties because their ratios between the two differed substantially from those in other areas. The result was a 66% increase in SI in 1999 in Thames Valley and 43% in 1999/2000 in South Wales. That was why their data had to be excluded from the KSI analysis. However as the distinction did not affect accident and injury totals their exclusion from the PIC analysis was unnecessary. The crucial point here however, ignored by the Report was the extent to which SI (and therefore KSI) data is so very subjective and hence at risk to conscious or unconscious bias, as for instance when trying to meet targets. In that context it should be noted that there being 5 slight injuries for each SI, a 2% (say) reduction in SI results in only a 0.4% increase in SI.

Anyone who thinks skewing of such necessarily subjective judgements unlikely might reflect on what is now known to happen in hospitals and what has just been admitted (Dec 2013) by serving police officers about crime figures being manipulated, in both cases to meet targets. As always, the first rule of targets is that they will be met, whatever else suffers as a result.

6/ ***"In the six comparable pilot areas (the whole partnership areas not just at camera sites) the annual number of killed and serious injuries has fallen to 4% below the long-term trend. In this respect, the six areas have outperformed the rest of Great Britain***."

The significance of this statement is the false impression given that speed camera effect extends beyond the sites themselves into their wider areas. However as Stats19 data shows (and as the authors of the report could and should have known) the six selected areas had collectively suffered a counter-trend increase in 1999 and it was therefore always likely that numbers would fall in 2000/01 due to RTM. As the following graph makes clear those falls were no more than the random variation of relatively small numbers superimposed on trend. It is important to note also that it is a long time since anyone seriously suggested camera benefits apply over any area wider than the +/- 0.5km dimensions of typical sites, representing no more than 2% of road length.



(Note typing error 200 instead of 2000 above)

7/ "***Average speed across all sites dropped by around 10% or 3.7mph***"

The significance of that claim is of course that the reader is expected to believe that lower average speeds automatically result in fewer accidents - not necessarily true. Hence TRL 421 with its false claim that risk increases by 5% for every 1mph increase in speed (originally only in specific circumstances but since often extrapolated without caveat) was rightly savaged by Paul Smith of Safe Speed when it was first published - see www.safespeed.org.uk/trl421.html

As any engineer knows, average figures can mask wide variations and so must be treated with caution. For example, the average speed of 50 cars at 50mph is the same as of 49 cars at 49mph plus one car at 99mph (or for that matter of 25 cars at 40mph plus 25 cars at 60mph). Does anyone seriously imagine that risk is the same in all three cases?

TRL 421 compared accident rates on roads with different average speed but tells us nothing about the possible adverse effects of whatever measures are used to force down speeds nor do average speeds tells us anything about speed differentials that are at least as important in crash causation, especially on single carriageways. Are TRL not aware of the Law of Unintended Consequences? Close to forty adverse effects of speed camera policy, that DfT flatly refuses to discuss, let alone quantify (www.fightbackwithfacts.com/camera-overview/, E21 to E24) include sudden braking, greater speed differentials, drivers inhibited from overtaking as rapidly and therefore as safely as possible, and many others. In any case the claims of 67% reductions at fixed camera sites are 3 times greater than TRL 421 would predict!

8/ *The* ***reduction in speed is more noticeable at fixed camera sites. At these sites the number of vehicles exceeding the speed limit dropped by 67%, compared to 37% at mobile sites***". Of course! Did they not realise that mobile cameras are rarely used in the dark, and that drivers know it?

9/ ***This demonstrates that speed cameras, of all types, reduce vehicle speed. There is strong evidence that these reductions have been sustained over time****".* Perhaps they do, but that does not necessarily mean that they reduce casualties to the extent the report claims, if at all. It is of course true because casualty numbers at camera sites are relatively small and slow to mount up, speed reductions are often taken as a proxy for camera benefit because they can be measured in the short term - but that is no excuse for linking the two.

10/ ***There have been significant reductions in casualties".*** Indeed there were, but the real question is the extent, if any, to which cameras were responsible for those reductions.

11/ ***There were reductions in casualties at both fixed and mobile camera sites. The former appeared to be the most effective – on average, killed and serious casualties fell by 65% at fixed and 28% at mobile sites. This was consistent with results from the speed surveys.*** Nonsense and wishful thinking. As above, there is no way that those modest reductions in speed could have brought about such large reductions in casualties. Hence something else must explain them and, as the Finney Report and the present writer can now prove, those reductions were largely, if not entirely, due to RTM.

12/ ***The benefits to society, in terms of the value of casualties saved, are estimated to be in the region of £112m in the first two years".*** Arrant nonsense! Not only are casualty reductions due to cameras greatly overstated so too are the DfT's estimates of accident values. Pain and suffering supposedly avoided, a major element of the total do not amount to cash savings however desirable they may be, and as the National Audit Office has confirmed should not be presented as if they do. Equally, the substantial "lost output" element - now about £700,000 - in a fatal accident - is fantasy in that the output any casualty is no longer able to produce continues unchanged as others step forward to meet demand - one of the most basic principles of any market economy and one well understood by anyone running a real world business. See http://www.fightbackwithfacts.com/bogus-dft-values/ The Fourth Report by the same authors claims a benefit/cost ratio of 2.7 to 1, the real figure is way, way below cost.

13/ ***The total value of prevention of all road accidents in 2000 was therefore estimated to have been £16,959m."*** It beggars belief that anyone writing a supposedly expert report quotes to five significant figures a result based on multiplying unreliable data by guesses, estimates and subjective assumptions!

14/ ***The funding system, referred to in the strategy, was introduced in eight pilot areas in April 2000 and is now being introduced nationally."*** And (as above) was closed down in April 2007 when the DfT realised that, in true Parkinson's Law fashion, those whose jobs depended on fine income would not be interested in what the then Minister called "***perhaps more cost effective methods***". Who would not have predicted that? Not, it seems, the authors of this report on the scheme they themselves devised.

15/ ***This would be the first self-financing road safety system in Great Britain and would, in turn, free up resources to be spent on other local priorities, such as engineering and education".*** "Self-financing"? Only from the point of view of the authorities, certainly not from the point of view of drivers or GB as a whole! There is **no difference in principle** between funding cameras by **increasing existing taxes** or by what is in effect an **inverse lottery applied only to drivers**. Except of course that when they extract it from drivers in the name of "safety" they care rather less about whether it is wasted. And "resources", a weasel word for "money" are not freed up in the pain-free way implied, but amount to taxpayer money that is transferred to other uses.

16/ ***There is a large number of factors that can affect the speed and casualty rate in an area, and there can be a number of explanations why casualty rates could increase or decrease. Care must be taken in interpreting the results that are presented in the following sections .....The following two chapters summarise the effects that speed cameras have had on both vehicle speed and casualties in the first two years of the pilot."*** Having made the vitally important point that other factors can affect speed and casualty rates, the Report then fail to provide any information on how "***care must be taken***" and instead go on to portray, without further caveat the data they provide as "***the effects that speed cameras had***". For all the above reasons, this is simply untrue, camera effect caused only a small part, if that, in the observed reductions.

17/ ***A generally accepted relationship is that each 1mph reduction in speed should result in around a 5% reduction in accidents***" . As before, this TRL 421 assessment is palpable nonsense and has long been exposed as such. As there is no such meaningful relationship and as it is in any case impossible for the modest changes in speeds to achieve far greater reductions than involve speeding in the first place.

18/ ***Pilot camera sites have performed well compared to the rest of the GB – even taking into account long-term trend".*** True enough - but that the real question is why they performed better and the real answer to that is as they were selected for higher than normal accident levels they were always likely to do better, due to RTM.

19/ As above, the "***encouraging signs***" of camera benefit "***in the wider area***" were also illusory due to the against-trend rise in 1999, and **could and should have been identified as such at the time**. It is surely **significant that all reference to benefit in wider areas was dropped from later reports** - and indeed the standard response of Partnerships to complaints of worsening trends elsewhere rapidly became "*Nothing to do with us, not our responsibility."*

20/

Chart 3 *Change in KSI casualties at camera sites in six pilot areas*

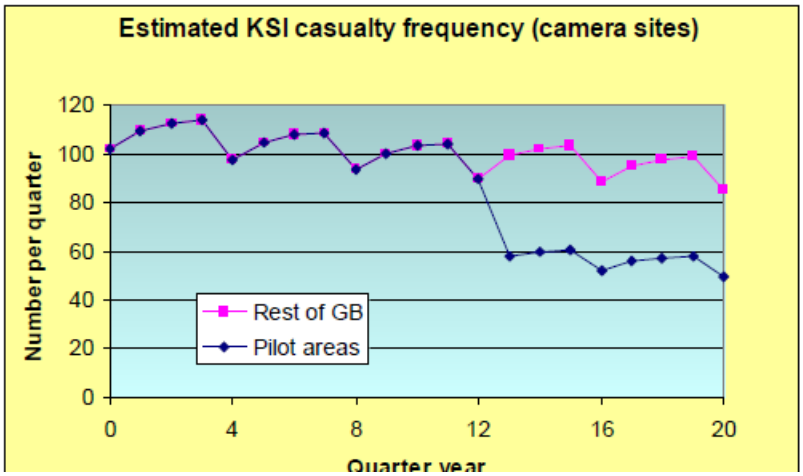


Chart 3 compares KSI frequency at camera sites in the pilot areas in the thirteen quarters prior to the pilot and in the eight quarters afterwards. The chart shows that, since the increase in enforcement funded under cost recovery (start of quarter 13) there has been a substantial reduction (35%) in the frequency of KSI casualties at camera sites compared to the long-term trend.

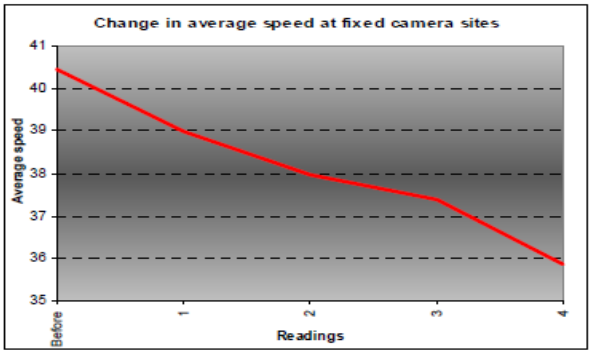
This is a very significant graph, albeit not in the way the authors seem to believe. The present author's detailed analysis of where and when several million accidents happened includes month-by-month graphs of KSI at sites that would have qualified for speed cameras. From the 220,000 such examples readers will be able to produce graphs such as that below for the 6 areas in question, for every police area and the whole country, for any combination of speed limits, showing precisely how KSI changes at such sites.

THIS SECTION TO BE REWRITTEN

Continuing:

However a graph in Appendix D shows **changes in average speeds as being relatively gradual (**as would be expected, given that the proportion of drivers at any given site already aware of the presence of the camera can only rise gradually, over weeks or months after camera installation.)

Chart 17 *Changes in average speed at fixed camera sites*



While no time scale is provided for the X axis it is **not credible that a** near instantaneous fall in KSI could be due to a gradual change in average speeds. The same applies to changes in speeds shown in Charts 19, 20 and 21.

However standard RTM statistical theory, the Finney Report (ref ?????) and the author's own analysis (Ref ??? of many millions of accidents all confirm that **RTM effect at sites chosen for their recent history of high KSI numbers is virtually instantaneous**, the moment the selection period ends and would therefore explain the sudden falls at these sites..

~~These two graphs therefore provide prima facie evidence that the observed falls in KSI **were due at least primarily to RTM and not to the presence of the cameras.**~~ **This question of precisely when casualty rates change, relative to when cameras are installed, more or less ignored in most if not all reports to date, will be reviewed in more detail in this and other papers.**

21/ ***E.1 EFFECT ON KILLED AND SERIOUS CASUALTIES (KSI)***

As above, it is important to recognise that the KSI reductions claimed ignore both RTM and possible traffic diversion.

22/ ***E.1.2 Effect on KSIs in wider partnership area***

As above, it is important to recognise that the KSI reductions claimed ignore what is clearly a significant RTM reduction following an against-trend increase in the Before period.

23/ ***E.1.3 Effect on KSIs by camera type***

As above it is not possible for the speed reductions identified to result in 65% or 29% reductions in KSI when only 8% or so of KSI accidents involve speeds above limits and even then not necessarily as the primary cause. It follows that the explanation of the differing results must lie elsewhere.

24/ ***F.1 CONCLUSIONS***

Costs and income increased in year two and this may in part be due to the fact that many pilots were not fully operational until then. In the second half of year two the numbers of fixed penalties paid started to plateau and this may be due to greater compliance with speed limits. That "*many pilots were not fully operational*" until year two begs the question of precisely when each did become operational. This is important because (as above) RTM falls happen immediately the site selection period ends. The longer camera installation is delayed the greater the possibility that the observed reduction was due to RTM not to camera effect. Indeed, delays of a year or more were normal for all areas joining the scheme later. Given the data for those sites it should, even now, be possible to determine the extent to which RTM before installation contributed to those falls.

Figures are given in Fig 2a and 2b for "site months" of camera use, but that data is not helpful. For instance, an area that had (say) 20 cameras for 24 months from the start would show the same site months as an area that had 60 cameras for the last 8 months - but in the latter case there would have been no camera effect for the first 16 months, when RTM might well have made significant contributions. Further, that the number of fixed penalties started to plateau only in the second half of year two strongly implies that they could played little part in the instant falls in Quarter 13, the start of Year 1 (see above) Smaller effects on speeding in the early part of the trial makes the claims even more unlikely to be valid.

25/ G.2 VALIDATION OF THE SELECTION CRITERIA

The chart [for London] *shows that, in general, sites with the largest number of KSI accidents prior to enforcement, recorded the greatest reduction in killed or serious accidents after enforcement took place.*

For example, at fixed sites that recorded 1 ***KSI in the 'before*** *' period there was on average a* ***2% increase*** *in killed and serious accidents after enforcement. This compares to sites where* ***8 or more KSIs*** *had been previously been recorded and which on average showed* ***a -52% reduction*** *in killed and serious accidents after enforcement.*

This means that at sites where there were fewer than 2 killed and serious accidents in the before period, there were 55 more killed and serious accidents recorded in the most recent three years after enforcement took place.

This is precisely what the author's analysis of RTM finds for London sites with few if any cameras. London roads have so many accidents that 1 KSI in 3 years is below normal will show increases later as numbers return to their higher normal level, but 8 KSI being above normal tend to show large falls towards the much lower normal level. There is nothing in these figures to confirm any camera effect.

26/ *G.3 CONCLUSIONS*

The results of this analysis show that a strategy of identifying and targeting accident hotspots is likely to be the most effective use of camera resources and is likely to bring about the greatest reduction in fatal and serious casualties over time. In fact the author's an analysis of millions of sites shows clearly that no group of sites selected for high levels of KSI in any three year period suffers the same high levels again, before or after - because next year accidents happen somewhere else. In fact targeting sites where accident levels are abnormally high will ensure that, collectively, they will return to normal levels immediately, without cameras due to RTM.

27/ ***In some cases cameras were already installed in the 1997-1999 period and levels of enforcement increased during the trial period. It was not clear from all the datasets which cameras in the dataset were previously installed. All comparisons will be to the 1997-1999 baseline period to avoid potentially misleading trend effects.*** Another serious flaw in the analysis - reductions claimed for cameras which had been installed during

the selection period or even before. Logic suggests that any effect those cameras might have had on KSI would already have happened before the April 2000 to March 2002 trial period and that while those effects might have been sustained they would not have resulted in further falls during the trial**.** Without the data it is not possible to quantify the effect but it does highlight what too many Partnerships continue to do to this day, including in claims of benefit the supposed further effects of cameras installed many years before. This is simply not acceptable.

28/ APPENDIX H: TECHNICAL DETAILS OF CASUALTY ANALYSIS
H.1 BACKGROUND

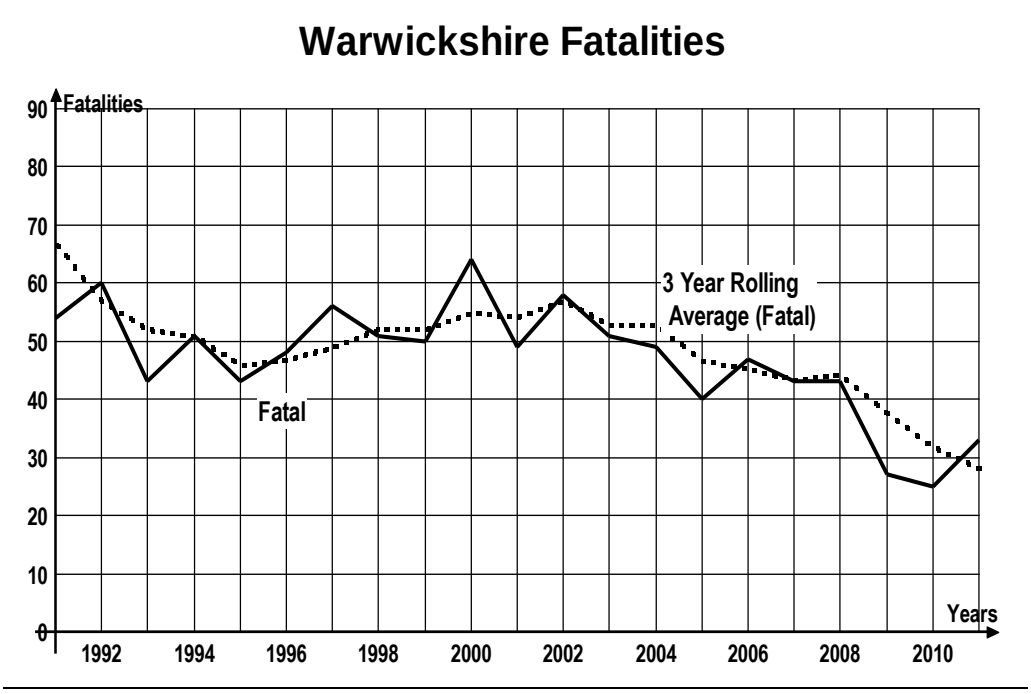
2. Is there a regression to mean, migration or other effect that will counteract this apparent effectiveness, i.e. are the changes that have occurred in the pilot area camera sites a fair reflection of the consequences of introducing this kind of road safety measure?

Data outside the treated areas were not available for individual sites, but only as area wide totals. It was therefore not possible to identify a regression to mean effect at the site level because the mean could not be estimated for individual sites. (Writer's emphasis)

and also in the summary

We could not obtain data for the before period for individual sites other than at camera sites. It was therefore not possible to check fully for regression to the mean at the site level. The results for areas that bid unsuccessfully for participation in the pilot could be used as a comparison for what might have occurred in participating areas if they had not been treated. The PIA and KSI frequencies for these areas do not differ significantly from other similar areas that did not bid for pilot status at all. On this basis, there is no evidence in the present data for any substantial illusory benefit due to the regression to the mean effect.

This is all abject nonsense! Not only is regression to the mean a normal aspect of innumerable statistical observations and well understood, but is perfectly obvious in almost any graphs of road casualty data such as the following for Warwickshire:



As is obvious from almost any of such graphs, each diversion above longer term trend is automatically followed soon afterwards by a fall and it is impossible to understand how the Report fails to recognise it.

It should also have been perfectly obvious that selecting sites with a recent history of higher than normal numbers would be bound to lead to significantly greater regression to mean falls shortly afterwards without intervention, as the writer's analysis of 220,000 qualifying sites has recently confirmed.

Equally inexplicable is the statement that "*Data outside the treated areas were not available for individual sites, but only as area wide totals. Data outside the treated areas were not available for individual sites, but only as area wide totals. It was therefore not possible to identify a regression to mean effect at the site level because the mean could not be estimated for individual sites".*

This is simply not true - Stats19 data was available for all recorded accidents and it would have been a simple matter to select similar sites that did not have cameras and establish what happened there. That same Stats19 data could and should have been used to establish what was "normal" before the selection period for the sites chosen for cameras, so that the "after" numbers could be compared with "normal" not with the abnormal levels of those sites. This section summarises the fundamental flaw in the whole of the Trial - that results were compared (after trend) with the abnormally high levels of the selection period, with no effort made to establish what had previously been normal.

Indeed what the Finney Report did in 2010 (?), comparing results of 75 Thames Valley mobile cameras with earlier normal levels. And what it found was that KSI numbers fell back to the prior normal levels in the year or more delay between site selection and camera installation, with no subsequent camera benefit whatever.

Readers might ask themselves these questions:

i/ Why did the authors bother to ask the rhetorical question of whether RTM should be allowed for when it was always obvious that it is a significant factor - and indeed was in the totals for the six areas (see graph on pg. 2)

ii/ Why did they claim that data for other sites was not available, when (a) it was and (b) it wasn't needed anyway?

iii/ Why, having wrongly claimed that no evidence either way was available, did they assume that RTM was not a factor?

iv/ The authors of the Report also designed the scheme and therefore had vested interests in it seeming to succeed.

ac/ ***Data provided by the DfT***

The DfT provided data from Quarter 1 (Q1) 1997 to Q4 2001 for each of the area types: shire counties that did not bid, metropolitan counties, unsuccessful bidders, and entire pilot areas. As far as is known these data are complete. This is nonsense - the DfT has long warned that only 37**% of so of SI accidents are made known** to the authorities, the proportion for slight injuries is logically likely to be lower still.

ad/ The smaller size of the general reduction in frequency of PIAs than that in KSI's suggests that ***operating cameras under this regime is especially effective in reducing the frequency of occurrence of more serious accidents****, and hence in reducing the severity of accidents in general. This is consistent with the mechanism by which they act in that they discourage high speed, which is associated with both frequency of PIAs and the severity of those PIAs that do occur.* More nonsense! There are 5 times as many slight accidents and injuries as fatal plus serious and hence the mores serious classifications are much more volatile than the slight, especially in the relatively small numbers involved at camera sites. For that reason and as the writer's own analysis shows, the greater falls in KSI than in Slight injuries s are entirely consistent with RTM, far more so than with the supposed "mechanism" of camera effect.

The crucial difference between the two effects, one that the Report could and should have studied, was the timing of these changes. As the writer's analysis of 220,000 sites shows beyond rational dispute, RTM happens the moment the selection period ends, camera effect if any happens (obviously) only after the cameras have been installed, normally at least a year later.

ad/ ***Comparison of the pilot areas in their entirety between before and during the pilot indicates that although there was no detectible reduction in PIAs as a whole, there was a greater reduction than the national one in the frequency of KSI casualties in the entirety of these areas. Because of this, the frequency of KSI casualties in the whole of the pilot areas fell faster than the national average at the time of implementation. This suggests that there was no general increase in frequency of either PIAs or KSI casualties in the pilot areas away from the camera sites. This shows that there is a benefit in the entire pilot area associated with participation in the pilot, and that there is no gross accident migration effect in this case In all but one of the pilot.*** This is nonsense because (see pg. 2) the KSI reduction in the whole area was clearly due to RTM and was not sustained. In terms of migration, the Report seems not to understand that migration of a small number of KSI from the 2% of road length covered by cameras would become a much smaller percentage increase in the larger numbers of KSI on 98% of road length.

ae/ ***In all but one of the pilot areas there were reductions in KSI casualties and PIAs. However, we note that there were increases in PIAs in the Thames Valley area. We conclude that in the majority of pilot areas the reduction in KSI casualties and PIA are substantial and real effects over and above the general national reduction in casualties that has been achieved during the study period. This reduction is particularly notable in the reduction in the frequency of KSI casualties at pilot sites and in participating areas as a whole.*** *As before, the reference to whole area benefit are not credible, while site only benefit would have been much influenced by RTM.*

af/ ***In total, the system has released around £20m of additional funds for local partnerships to spend on speed and traffic signal enforcement and raising public awareness of the dangers of speeding. This money would have normally been returned to the Treasury.*** Weasel words to disguise what really happened. This did not "*release around £20m of additional funds*", it transferred those funds from the Treasury to the Partnerships - no new money was involved - the equivalent of swapping money from one pocket or pigeon-hole to another, a sleight of hand. What we have learned since is that police forces routinely reduce road patrols as they install cameras - the near invisibility of police patrol cars is now widely recognised - but no one knows what adverse effect this has had on road safety.

ag/ ***The benefits to society, in terms of the value of casualties saved, are estimated to be in the region of £112m in the first two years.*** As above, the estimates are sheer fantasy, estimated by multiplying wholly fanciful reductions achieved by equally fanciful values of those reductions.

END OF COMMENT ON THAT 8 YEAR REPORT, NOW ON TO OTHERS