# Freedom for Drivers Foundation

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## A Review of the Effectiveness of Speed Cameras

### Preface

A careful examination of the RAC Foundation report entitled "The Effectiveness of Speed Cameras", authored by Professor Richard Allsop has identified some questionable methodological procedures, defective analyses and significant omissions. In my view these severely compromise the validity of its conclusions and recommendations.

The excessive reliance on speed cameras in the UK over the last two decades has affected road safety adversely. A massive diversion of resources towards speed enforcement and away from more cost-effective alternatives has regrettably resulted. This process has primarily been driven - and supported - by the financial interests of the equipment manufacturers and of the safety camera partnerships, rather than by concerns for improvement in UK road safety.

Speed cameras are far less effective than Prof. Allsop's paper claims. They are also far less cost effective than other available devices. Resources misdirected on speed camera enforcement would, in our view, have resulted in far greater road casualty reductions - and particularly in the KSI (Killed and Seriously Injured) figures - had they been employed on alternative road safety measures.

Independent evidence shows that Vehicle Activated Signs are 50 times more cost effective than speed cameras; yet the proponents of speed cameras continue to argue for their use. It is in the public interest to correct any misapprehensions which may have been created by the widely distributed, but potentially misleading, Allsop paper.

We therefore produced the attached report. We believe that it addresses all the key points.

Despite their complexities, we hope that you will read both studies and so identify for yourself the shortcomings in the Allsop paper. These are sufficient in number to seriously compromise many of its conclusions regarding the effectiveness of speed cameras as a road safety tool.

Roger Lawson

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# A Review of "The Effectiveness of Speed Cameras" by Prof Richard Allsop

## The Report's Claims

The document "The Effectiveness of Speed Cameras" was published in late November 2010 by the RAC Foundation (and presumably was commissioned by them). See www.racfoundation.org/research/safety/effectiveness-of-speed-cameras for a copy of the report. It was widely promoted by the RAC under the headline "Cameras Save Lives" and has been circulated to many public bodies. The document claims to be a "Review of evidence" in relation to the effectiveness of speed cameras. The main claims in this document are as follows:

1. That speed camera deployment results in significant reduction in traffic speeds (see page vi).

2. That speed cameras installed at 4,000 sites resulted in 1,000 fewer people being killed or seriously injured in the year ending March 2004 (page vi).

3. That the widespread use of speed cameras has resulted in "sustained falls in the average speeds of cars on 30 mph roads, and in the proportion of cars exceeding the speed limit" (see pages v/vi where there is a claim that this reduction is linked to the rollout of camera enforcement).

4. That speed cameras lead to a reduction in casualties across a wide area (see page vii), not just at camera sites.

5. That public acceptance of speed cameras is high (see page vii).

6. That national decommissioning of speed cameras would result in 800 extra people being killed or seriously injured across Great Britain (see page vii, and emphasized in the Foreword by Professor Glaister*).*

I will deal with each of these claims later in this note in more detail.

## Introduction

This document is primarily a review of prior studies on the effectiveness of speed cameras. It does not actually contain any new evidence. But the review only covers a very limited number of the available studies from which it attempts to draw some conclusions. Professor Allsop takes a very unscientific approach in formulating the conclusions in the report, even though he appears well qualified to do so. He reports data without any statistical "confidence" limits and the extrapolations he includes likewise contain no information on the level of confidence that the reader might expect to see. Indeed none of the underlying data on which the conclusions are based are contained in the report although some references are provided.

The following sections of this note relate to the Allsop report headings and paragraph numbering:

## 1. Background

***1.1 The need for speed management.*** Prof Allsop claims *"...it is inherent in the road traffic system that many of us tend to go somewhat faster than is good for ourselves or society*", without substantiating this claim at all. Indeed his general attitude and prejudice against higher speeds or the choices that individual drivers might make is clear from this statement alone.

He then goes on to say "*It would thus be wrong for each of us to be free to choose how fast to drive*". We believe otherwise. Only by encouraging drivers to adjust their speed to prevailing road and traffic conditions can safe driving be encouraged, although we accept that speed limits can assist drivers who are unfamiliar with the road or otherwise need reminding of what might be a safe speed (if speed limits are set correctly).

This whole section of the report shows a bias which is inappropriate in a publication that is claiming to be an authoritative and scientifically founded report. Indeed it immediately suggests that this report is more a polemic than the presentation of an argument based on facts. The views of Prof Allsop in this part of the report are simply opinions with weak substantiation by the evidence.

***1.2 Speed limits and appropriate speed.*** Prof Allsop states "*If speed limits are to be effective, and respect for them as traffic law is to be maintained, they need to be understood and either to be largely self-enforcing, or perceived by the majority of drivers to be reasonable...*". We would not argue with that statement. But he concludes that sentence with the phrase "*...and to be enforced, so that they cannot be widely exceeded with impunity*". How the first part leads on to the last part is not clear. One of the reasons why many drivers are opposed to speed cameras is that many of the locations chosen for them seem to have inappropriate speed limits. They may simply be locations where a random group of accidents happened to occur in close proximity or time. Indeed some locations seem to have been deliberately chosen to catch drivers unawares with the suspicion of the objective of raising revenue from fines to fund the safety camera partnerships (irrespective of whether such an allegation can be substantiated).

***1.4 The Introduction of Speed Cameras.*** The claim is made that the West London Speed Camera Demonstration Project resulted in "substantial reductions in the numbers and severity of accidents and casualties. But this is not true – the claimed reductions were very small. See later for more on this, but it is a good example of the over-inflated and unsubstantiated claims in this report.

***1.5 Partnerships.*** The safety camera partnerships were created on the basis that as cameras seemed to be effective (but based on defective analysis of the pilot schemes), that they should be rolled out more widely. As the funds to do this were otherwise limited, the idea was that fines from motorists would be used to finance more and more of them. There was no commitment to use any surplus funds on other road safety measures, and indeed they could not be based on the rules set by the DfT. As a result the partnerships used the funds so obtained to simply finance their own operations and the expansion of those operations with more cameras (see later for more on the cost structure).

The natural result was that the camera partnerships became more interested in empire building than in accident reduction. The fact that these "partnerships" have no clear legal identity, are not subject to public scrutiny by elected bodies, often produce no reports on their activities, and have in some cases claimed to be outside the provisions of the Freedom of Information Act has also undermined all public confidence in them. The Government has now wisely chosen to change the basis of finance so that revenue from fines goes to the Treasury who return it to the local authorities for funding of road safety measures (including camera partnerships).

Prof Allsop says "*Contrary to a reportedly widespread misapprehension, it is not and has never been to the financial advantage of partnerships or any of their member organisations to increase the number of fixed penalties imposed on the basis of camera detection*". This is misleading in several ways. Firstly it was certainly the case that revenues from fines financed jobs, equipment and facilities within the safety camera partnerships, and still do indirectly. It was therefore in the interest of the managers of such organisations to ensure that fines were maintained at a high level. Indeed the more fines that could be raised, at the least cost, the better so far as they were concerned and this has had a distorting influence on the activities of these "partnerships". Secondly, there were no financial incentives put in place to counterbalance this such as related to the accident reduction rate, or "public satisfaction" with their activities.

## 2. The Four-Year Evaluation Report

A major part of Prof Allsop's report is concerned with the study of the operations of 38 camera partnerships in the UK that was reported on in 2004 by PA Consulting/UCL from which he draws most of his conclusions. This is very selective data when there are many other reports available which he totally ignores (and although he covers the West London Camera Demonstration Project he puts much less emphasis on it probably because the results from it are not as positive).

His introduction says "*in the case of changes in the numbers of collisions and casualties, a combination of statistical analysis and judgement is required to assess how much of the recorded changes should be attributed to the operation of the cameras*". But no statistical analysis is provided and instead we get lots of judgements about underlying data that are based on Prof Allsop's opinions, if not downright prejudices.

It must be pointed out that it appears that the data on which the 2004 analysis was based was reported by the safety camera partnerships themselves (although some of it came from other original sources such as the police). Clearly this is far from ideal. Such partnerships had a strong incentive to justify their existence, so selection of data could have happened both consciously or unconsciously. This was not a properly "controlled" trial in the scientific sense – the people reporting the data had an interest in the result of the experiment. In addition Prof Allsop is commenting on a report partly authored by his own organisation (he is a Professor of UCL).

***2.1 Changes in speed.*** Prof Allsop reports that average speeds at mobile urban and rural sites were reduced by 1.4 mph and 1.0 mph respectively. Now this is the kind of reduction in speed that can be achieved simply by signage (ordinary signs, or even higher in the case of speed display devices).

This is perhaps not unexpected as with mobile sites there may well be a sign installed, but for most drivers at most times, they won't actually see a speed camera. But the conclusion must be therefore that the effect is probably due to the sign and not the presence of an actual camera.

So the questions arise: would the speed reduction wear off if a camera was never actually present; and would any warning about excessive speed rather than a camera sign have the same effect? No answers can be attempted to these questions without more data, but it does suggest that the recent trend to the use of mobile cameras rather than fixed ones is not necessarily driven by the greater effectiveness of mobile cameras in reducing speeds. In any case, it is probable that a simple warning sign, which has minimal cost in comparison with operating a mobile camera, would achieve the same outcome in terms of speed reduction. A speed display device, that can particularly warn drivers who are exceeding a reasonable speed, could be even more effective.

Fixed cameras are obviously more effective at reducing traffic speeds. Prof Allsop says that it is understandable that there were smaller changes at mobile sites and tries to explain this on the basis that mobile sites had a higher "conspicuity requirement" and hence presumably that drivers could expect to see the camera from further in advance than at fixed sites and hence might not slow down if they did not see one. At least that is what he seems to be saying. But it is wrong to suggest that mobile cameras are more conspicuous than fixed ones – indeed they are usually less so. Mobile cameras are forward facing (over a very long distance in some cases), whereas fixed cameras are generally rear facing. Drivers have much more warning of fixed cameras than of mobile ones and time to brake in advance for fixed ones when they often do not for mobile ones so the logic here simply seems confused. He seems to be trying hard to excuse the poor efficacy of mobile cameras at reducing traffic speeds as against simple signage on the basis that they are too conspicuous!

Prof Allsop then covers the issue of the correlation between the speed of traffic and injury collisions, and resulting KSIs. It is beyond the scope of this note to do a full analysis of this subject. But the reported studies generally do not enable one to draw conclusions about what might happen at a particular location on a particular road if the speed of traffic is artificially reduced by the use of speed cameras. Clearly if traffic speed was reduced to zero there would be no accidents, so in extremis it must be true, but obviously that is not a very practical or realistic assumption. It is also well known that the safest roads are typically motorways, where speeds are highest, and the least safe those in urban areas, where speeds are lowest.

Prof Allsop misapplies the correlation between the speed of traffic on particular types of roads and the KSIs, to hypothesize that the same relationship might apply if the speed was varied at a particular location on a specific road. He then uses the highly dubious correlation thus deduced to forecast the likely impact on accidents (see Table 1 on page 13).

Note that a statistical correlation does not mean that there is a causative factor. It is misapplying basic statistics and scientific principles to suggest that speed is the cause of accidents just because there is a correlation when the data is viewed in certain groups. It is not a provable premise simply from the fact that there is a correlation. Likewise forcing a change of speed may not have any impact on the number of accidents if there is no direct causation.

***2.2 Changes in numbers of injury collisions and KSIs.*** There is extensive discussion of the results in terms of the impact on injury accidents of the introduction of speed cameras in the 2004 report. As Prof Allsop points out, there has been widespread criticism of that report because it did not take account of "regression to the mean" (or RTM). Because cameras might be sited where there had been a random but unexpectedly large number of accidents than normal at a particular location in a recent period of time, the accidents might simply revert to their previous level. Prof Allsop attempts to correct for the RTM problem, and his result is Table 3 on page 17. It is of course unfortunate that the underlying data and the details of his calculations used to create that Table are not in the report. Why not?

One immediate peculiarity one can see in Table 3 is that the percentage reduction in PIC (personal injury collisions) is 19.4% for Mobile Urban (after adjusted for Trend and RTM) whereas it is only 16.6% for Fixed Urban. In other words, this suggests that fixed camera sites are less effective than mobile ones which seems to be rather at odds with his other findings, and what one might expect. For example, he has already shown that fixed camera sites are more effective at reducing traffic speeds and he has argued that traffic speeds directly correlate with resulting injury accidents. The difference between fixed and mobile KSIs is also quite small, although at least it is a positive difference rather than negative one.

Of course he also does not give any indication of the "confidence limits" (i.e. the statistical confidence limits in a technical sense) that might apply to the figures in this table as one might expect in a scientific study. The omission of such information undermines the reliance one can place on the data and its analysis, but also do not enable us to see if the data could be construed to be consistent with a contrary assumption either.

He does not even bother to report the figures for non-Urban sites so again he is selectively reporting the results of his analysis. His excuse is that "*the data for rural sites indicated that the available predictive models would not be suitable for application to these sites*", without further explanation. Perhaps they simply gave the wrong results?

In the last paragraph on page 17, he discusses specific camera site data and says "*Another six sites were from South Wales, which showed a reduction of only 1.8% in PIC and whose sites were excluded from the main analysis of numbers of KSI because of possible effects upon recording of KSI casualties of a change in definition*". De-selecting data that does not fit one's hypothesis is a well known error in scientific reporting, and tends to arouse great suspicion when it is done.

There is always some reason that can be found for excluding "outlying" data that just happens to appear unreasonable or exceptional. It is not a technique to be recommended and again undermines confidence in the data and the way it is analysed in the report.

At this point Prof Allsop seems unsurprisingly to have become unhappy with the results of the analysis and he says "*In the writer's opinion, therefore, the estimates of the effect of RTM on numbers of PIC and FSC that are quoted in Table 3 should be viewed with considerable caution…..*". So he then goes on to make some further "adjustments" which are very difficult to follow. The reasons for these changes are unclear but Appendix 1 to his note attempts to provide the calculations. I suggest readers of this note see for themselves whether they understand the reasons for the adjustments and the basis for doing so. But it is questionable whether other contrary "adjustments" could not be argued for with some merit.

The end result is given in Table 4 on page 20. The reduction in PICs between Fixed and Urban sites is now more like what might be expected (although still a relatively small difference). However the KSI figures are also now "improved" in that they not only show a bigger reduction at fixed sites than in Table 2, but the fixed sites are much better than the mobile ones. This seems somewhat fortuitous. How many different "adjustments" did Prof Allsop study before he decided that this gave the correct outcome one wonders?

Table 6 on page 21 is simply the data from Table 2 (the "unadjusted" figures from the 2004 report) but using a different method of adjusting for RTM. It again shows the peculiarity that Mobile Urban cameras have the same apparent effectiveness in reducing PICs as Fixed Urban.

At the top of page 22, Prof Allsop says "*The evidence is by its nature imprecise and incomplete, and its interpretation requires subjective judgement as well as objective calculation*" (underlining added). The evidence is certainly unclear and yet Prof Allsop then uses his subjective judgement in a totally unscientific way to formulate some estimates of what he believes are the benefits of speed cameras. The resulting estimates are given in Table 7 on page 23. His arguments preceding that table for the basis of his estimates are contestable but it would be tedious to go through all the individual points he makes, when in essence the underlying data is so poor and is clearly inconsistent in some regards. How much confidence can we have in the judgement of Prof Allsop when he bases it on such weak evidence?

He then extrapolates the apparent reduction in accidents across the 4,000 sites covered by the 2004 study to give a total of between 800 and 1,300 KSI saved (Table 8 on page 24).

But he then says "*It should be noted that these estimates take no account of the possibility of the reduction in PIC and KSI at camera sites may have resulted from diversion of traffic to alternative routes…*". In other words, one of the basic controls that might have been introduced into the study to improve its rigour has not been used – namely the recording of some traffic counts.

***2.3 Public acceptance of cameras.*** The surveys reported do not ask a simple question such as "do you support the use of speed cameras?" but more complex ones. Why? In reality the questions posed clearly are structured in such a way that a response that supports speed cameras is more likely. The preamble to putting the question is also usually biased.

Some of the questions ask the obvious and get the obvious response. For example, the question *"…the primary aim of cameras is to save lives?"* will be bound to get a response of Yes from most people because few people believe that those who install speed cameras do not have that objective in mind. The vast majority of the population generally believe that most people have good intentions and are not deliberately attempting to deceive. But that does not mean that those people who reply Yes actually believe that cameras have any efficacy in reducing accidents.

Of course the use of surveys that are commissioned by those with a vested interest in the answers usually results in the answers they want.

An AA survey is mentioned, but there was another survey of 17,500 AA members that showed that 72% thought that road and junction improvements would make the UK's roads safer, whereas only a quarter (25%) thought more traffic police would make a difference, with just 4% saying more speed cameras would do the trick. In other words, the merit of speed cameras in the public's eye depends on the question you ask. In any case, the AA surveys are not a random sample of all drivers, but are based on the responses of volunteers so may be biased by self-selection or the activities of pressure groups.

Other surveys run by motorists groups or by national newspapers have shown contrary results. ICM Research, commissioned by an insurance company, undertook a survey of motorists' attitudes to speeding and cameras and effectively got results that were not supportive of speed cameras.

There has been no proper independent study, using a controlled sample of the population and proper scientific techniques, to determine the support or otherwise for speed cameras so again we have Prof Allsop promoting information of dubious merit. In any case, as many people argue that speed cameras have been promoted to the general public on doubtful grounds and using spurious evidence, it is quite possible that the public have a distorted view of the merits of cameras.

## 3. Other evidence

***3.1 The West London Demonstration Project.*** This project reported in 1997 and is covered in the report in some detail. The reduction claimed by Prof Allsop (after various adjustments) is about 6.4% in KSIs based on a “wide-area” view of the accident figures. By looking at the overall data over a wide area, the effect of RTM is avoided. However, Prof Allsop suggests that because the impact of particular camera sites (only 21 were installed) is higher than one might expect from other data, his view is that cameras have a more widespread effect than simply at the camera sites. Why would that be so? My suggestion is that this is simply another anomaly which cannot be accounted for and which suggests the basic data is misleading.

In any case, a 6.4% reduction is quite a small amount. Is it significant? What confidence limits can be applied to it? No such data is given although there were confidence figures on the “unadjusted” data contained in the original report. The Project data also seems to contain no adjustments for the general trends in accident figures. As this is a “3 years before” versus “3 years after” kind of study, the general trends in accidents across the whole road network need to be taken into account (improved in-car safety, better medical treatment, etc, has resulted in a general reduction in injuries). Likewise no controls on traffic volumes or changes to road layouts seem to have been considered. In summary it is very difficult to have much confidence that the claimed reduction is a real reduction that would be repeatable elsewhere.

***3.2 Effects at camera sites across London.*** This is covered only briefly in the report, probably because again the reported results are quite low (12% reduction in PICs for example), and are significantly less than those estimated for urban sites as given above. The data again seems to be anomalous.

***3.3 Wider changes in speed and in number of collisions/casualties***.
Figure 1 in the report shows that the average speed of cars fell between 1995 and 2010, and speeds in excess of 30 mph in urban areas fell even more. Prof Allsop links this partly to the introduction of the national safety camera programme. However this is clearly an unproveable supposition, as there were many other factors that might have impacted on average speeds. More traffic congestion due to higher numbers of vehicles will reduce average speeds as was certainly the case in that period. Likewise road safety publicity campaigns focused on speed may have had an impact, together with various traffic calming measures.

There have been minimal changes in speeds on non-urban roads, but Prof Allsop assigns the reduction in casualties on those roads to improved car occupant protection. Is he saying that there was no benefit from using speed cameras on non-urban roads when there are obviously quite a number installed on such roads? He claims there is a benefit from his analysis earlier in the report.

He also ignores the potential impact of increases in the under-reporting of accidents, particularly in non-fatal ones, which is a problem now well known to the DfT (see: www.bmj.com/content/early/2005/12/31/bmj.38883.593831.4F.full.pdf - a BMJ report). Many studies in this field use before/after trends without taking any account of the trends in under-reporting, and other positive trends such as improved medical treatment of casualties, improved in-car protection, improved braking systems, improved external design to reduce pedestrian casualties and many other factors.

Prof Allsop mentions the reporting by police who attend accidents of excessive speed as a contributory factor in those accidents. But his presentation is misleading in that he fails to mention that these are only reports of contributory factors and not necessarily the primary factors. In reality it is known that speed is not the commonest contributory factor - it is "inattention". Indeed speed cameras will not deal with "driving too fast for the conditions" which contributes to 14 per cent of fatal accidents, and should be deducted from the quoted figures.

***3.4 Persistence of reductions in fatal/serious casualties at camera sites.*** Table 11 on page 33 shows the apparent reductions (ignoring RTM) in 12 partnership areas. But as Prof Allsop says on page 34 "*the partnerships for which this information is available cannot be regarded as representative...*". In other words, they could be self-selected. Those partnerships that did not report figures may simply have wanted to conceal the facts.

Later in this section of the report, it is explained that the effectiveness of speed camera enforcement seems to have declined over the years, as have accident figures generally, and hence Prof Allsop has revised downwards his estimate of the KSIs saved nationally to 800 (in comparison with the 1,000 reported by the 2004 study from 50% fewer sites installed at that time).

***3.5 Changes in speeds when cameras are known to be out of action***. Much of the evidence reported here is by camera partnerships, and hence may well be selective. As it says at the end of this section, "*At a few of these sites there was little change in speeds, but there were other sites at which the changes in speed were in the teens of miles per hour*". Perhaps these were simply sites where cameras were installed and speed limits set that most drivers saw as inappropriate and hence the cameras were removed?

***3.6 The finances of camera enforcement.*** The figures given in this section of Prof Allsop's report show clearly that the vast majority of revenue from camera fines up to the end of March 2007 went in maintaining the safety camera partnership's operations (£97.5m out of £104.6m of revenue in that year). This shows how the "industry" of safety camera operations has developed into a self-financed and self-perpetuating "business opportunity" for the people employed in it and for the equipment manufacturers who supply it. Indeed, the surplus available to the Government actually declined over the figures for 2004, despite the rise in the number of cameras of 50%.

Prof Allsop then calculates the cost/benefit ratio of camera enforcement based on his previous estimates of personal injury accidents (PICs) saved, and the total expenditure on cameras. He uses the DfT's figures for the average cost of a PIC in the UK (at least this is the assumption as he does not supply his detail working). This may be the conventional measure to use, but it can be quite misleading if you do not understand how it is formulated.

Most of the "cost" calculated by the DfT is not the real costs such as the cost of hospital treatment, emergency services attendance, loss of earnings, etc, but is deduced from asking people what they might be willing to pay to avoid such an accident. As a result, the figures tend to be on the high side – a very minor injury can hence be valued at £15,000 or more.

Ignoring that point, Prof Allsop comes up with a cost/benefit ratio of 2.3 (the total value per annum of accidents saved divided by the cost of camera operations). Of course there are many ways to spend money on road safety to reduce accidents. Road improvements for example are one. Prof Allsop does not attempt to compare this figure with other possible road safety measures – more on this later.

He also does not attempt to quantify and cost other associated costs that might be related to camera enforcement. For example, speeding offences are handled through the normal criminal court system unless a fixed penalty is accepted, and hence they have enormous costs in processing those cases, particularly where defendants plead not guilty and are acquitted (where they can recover their own legal costs). Prof Allsop provides no data on these costs or the number of cases involved which is a major omission – were they included? He also ignores the time costs of the people prosecuted. In addition, there are about 200,000 drivers now banned from driving annually because of speeding offences, or "totting up" of their points. Many of these will lose their jobs, or incur significant other costs on top of the fines they pay. Those additional costs should have been added in.

***4. Conclusions.*** Prof Allsop's conclusions are based on the evidence he has presented in the report. However, as I have pointed out above, the evidence he presents is selective, is not conclusive in any way and lacks statistical and scientific rigour. As a result the conclusions tend to be his personal opinions which many might dispute. Taking his specific claims (as first listed at the front of this note), my comments are:

1. *That speed camera deployment results in significant reduction in traffic speeds (see page vi).*

   All that is shown by the evidence is that speed cameras at fixed sites may have some impact on traffic speeds, particularly on those exceeding the speed limit (not unexpected, even if the site chosen has an artificially set speed limit which is lower than most drivers might consider reasonable). At mobile sites, the reduction seems to be only that which might be achieved by a simple sign.

2. *That speed cameras installed at 4,000 sites resulted in 1,000 fewer people being killed or seriously injured in the year ending March 2004 (page vi).*

   Despite the fact that cameras have increased in number since 2004 by 50%, the latest estimate is only 800. Unfortunately even this is based on calculations that are subject to grave doubt as is shown above. There is no certainty that this figure is anywhere near correct, and no confidence limits can be applied to it.

3. *That the widespread use of speed cameras has resulted in "sustained falls in the average speeds of cars on 30 mph roads, and in the proportion of cars exceeding the speed limit" (see pages v/vi where there is a claim that this reduction is linked to the rollout of camera enforcement).*

   There is simply no conclusive evidence in the report to back up this statement. The average speed of traffic could have fallen for many other reasons. This is a blatantly unsubstantiated claim which should not have been included in a report that is claiming to be an authoritative commentary on the issue of traffic speed and speed cameras.

4. *That speed cameras lead to a reduction in casualties across a wide area (see page vii), not just at camera sites.*

   Again there is simply no conclusive evidence in the report to enable such a claim to be made. No properly controlled studies or experiments have been undertaken that could confirm this.

5. *That public acceptance of speed cameras is high (see page vii).*

   A very questionable assertion. There are no well designed surveys, undertaken by independent bodies with no interest in the results, to support this assertion.

6. *That national decommissioning of speed cameras would result in 800 extra people being killed or seriously injured across Great Britain (see page vii, and emphasized in the Foreword by Professor Glaister).*

   I hope I have shown that this claim is based on very flimsy evidence. Even if it was true (i.e. is a hypothesis that should be considered), the real question that should have been answered is whether speed cameras are the best and most cost effective way of spending £100m per annum to reduce road accidents. Do they justify the effort involved in installing and running them, and the inconvenience that is imposed on about 1.5 million people every year from having to pay a fine or defend against a prosecution? I have attempted a review of this in a later section.

## The Cochrane Review

Prof Allsop quotes from the Cochrane Review of speed camera studies in his Conclusions, and elsewhere, and it is also quoted from by Prof Glaister in his foreword. It is well worth reading that Review. The "Discussion" section on pages 35 onwards is particularly worthy of study.

It is clear that there are no randomised controlled trials in the studies they review – it seems there have never been any of speed cameras. They say "*assessment of the quality of non-randomised controlled trials is problematic",* which is a truism indeed. They excluded a lot of studies but even so included some with very short durations (ie. one year or two of before/after data) which any traffic engineer will tell you is an exceedingly short period – three years is the usual norm. Only a minority of the studies attempted to adjust for RTM. Similarly only a "*small number of studies controlled for other long term trends in crash rates and changes in traffic volumes*".

They suggest more traffic volume data needs to be collected, and say "*<u>This is particularly important in light of the fact that studies available in this field have a quasi-experimental design, where the adequacy and appropriateness of comparison/control areas is often questionable</u>*". To put this in plain English, the studies are often poorly constructed, ignore basic scientific principles on how to avoid bias in the collection of data, and fall into many of the common errors involved in the design of experimental studies or the analysis of social data.

The Cochrane report goes on at some length on these issues. Another telling section is where it says "*The determination of speed camera effect on speed, speeding and crashes is made difficult because traffic volume and speed are not stable but fluctuating, resulting in different degrees of signal to noise on any given road as well as from one road to another."* They point out that the magnitude of the effect as reported in the studies reviewed is variable and go on to say in respect of the results that *"These are therefore insufficient alone to discriminate signal from noise in the effect.".* They go on to suggest that more studies need to be undertaken with more rigourous scientific design.

So the conclusions that Prof Allsop quotes needs to be read with the above warnings in mind (which of course he omits to cover in detail). Indeed although he quotes from them as saying "*However, whilst the evidence base clearly demonstrates a positive direction in the effect, an overall magnitude of this effect is currently not deducible due to heterogeneity and lack of methodological rigour.",* this is a major caveat which Prof Glaister omits when he quotes from the preceding sentence.

So we are left with the conclusion from the Cochrane report, that even after accepting all that they say about the defects in the design of the studies, they do believe there is some positive effect from the use of speed cameras. But they don't even attempt to indicate the level of magnitude of the effect or whether it is cost effective to use speed cameras to obtain it.

So Professors Allsop and Glaister seem to be presumptuous in quoting from the Cochrane report in support of their adopted stance without reservations.

**The problems of experimenting on people.**

As the Cochrane review rightly pointed out, there are many defects in the studies of the impact of speed cameras. But they overlooked another issue which was well known in industrial psychology circles many years ago, but has latterly been ignored by many practitioners in the social sciences.

In recent years we have seen a major emphasis on traffic calming schemes and speed reduction measures (e.g. hundreds of extra speed cameras, lots more speed bumps) but they seem to have negligible impact on overall accident statistics, which stubbornly refuse to come down (any reduction that is present is very questionable due to increased under-reporting and the fact that in-car safety has improved enormously and medical treatment also improved). The trend in accident reductions does not seem to respond to more expenditure. This is despite the fact that there are studies that clearly appear to demonstrate the effectiveness of these measures based on before/after studies of accident statistics in particular locations.

There are three major reasons why these statistics are misleading:

Firstly because they often ignore the effect of diverting traffic. To be accurate you need to take account of the changed volume and mix of traffic which is rarely done.

Secondly they often fall into the common traps of using selective statistics (i.e. the bad comparables are ignored and the good ones published), or they don't allow for extraneous factors such as weather conditions, or they ignore random statistical variation. Rarely are "confidence levels" attached to the numbers as they then make poor political headlines (in fact the statistics are usually based on such poor experimental design that it would be folly to do so anyway).

Thirdly though they totally ignore the major problem when experimenting on human beings, that predictions tend to be self fulfilling. This was clearly demonstrated back in about 1930 in a series of research projects in industrial psychology undertaken by Elton Mayo and known as the Hawthorne Experiments (there are several references on the Internet to this work if you want more details as it is a classical study in this field). One of the things he did was to test the effect of increasing or decreasing lighting conditions in the workplace. With an increase, he expected an improvement in output, and got it. With a decrease, he was expecting a reduction, but got an increase. In other words, any change improved performance. Why was this? Because the subjects expected the change to improve performance because they knew that was what the experiments were about, and hence it did. Behaviour changed to match people's expectations.

So let's take up the analogy with the introduction of speed cameras. People expect the installation of speed cameras will reduce the number of accidents (after all we are told they are only sited at accident black spots), so in fact they might well react accordingly, i.e. they will act to match their expectations.

How long will this effect last: well quite a long time according to Mayo, but clearly it could not last for ever because otherwise you could simply keep changing the environment and endlessly improve performance.

One of the clear conclusions is that when experimenting on people you have to be very careful when interpreting the results. This is why medical experiments typically use a double-blind technique where neither the subject not the collector of the statistics knows who is getting the real medicine or who is getting the dummy.

In addition the introduction of speed cameras results in a changed "view of the road" by the drivers of vehicles. It is known that any change in the appearance of a road tends to make drivers more cautious, at least for a while. So almost any change to a road will reduce accidents for a time. This is why the experimental design of studies for road safety measures is very difficult.

To really produce proper before/after studies to measure the effectiveness of accident prevention measures, you therefore have to be exceedingly careful. Certainly it must be extended over a long period of time so the Hawthorne effect wears off. Secondly, you should also try removing the change to see what effect that has, or introduce other similar but different measures to see whether any change in the environment stimulates the same change.

For example, compare the effect of a real speed camera, with a sign warning of hazards ahead.  Also you need to separate the collectors of the statistics from the interpreters (in practice they are often the same police at present). Unfortunately it is so difficult to do this kind of study in an unbiased and effective manner that in practice it is unlikely ever to be done properly.

But the message is clear - take any claims for breakthroughs in traffic accident reduction with a pinch of salt.

**The Cost Effectiveness of Speed Cameras versus the alternatives.**

There are of course many other practical alternatives to reducing speeds and accidents at particular sites, or other things that available funds could be spent on to reduce accidents in other locations or over a wider area. For example, it is well known that the construction of the M25 around London had a major effect on road accidents in the London area, mainly apparently because heavier vehicles diverted onto the new road with reductions in KSIs resulting. Even such low cost changes as anti-skid surface treatment of roads on bends can have major impacts (J.J.Leeming gives some figures in his book "Road Accidents: Prevent or Punish" on page 46 for a range of interventions). His data ranged from a ratio of after/before accident reductions of from 0.13 for surface treatment, 0.34 for junction improvements, 0.42 for roundabout construction, 0.68 for dual carriageway construction to 0.81 for motorways. Almost all of these give higher benefits than those claimed for speed cameras.

Another option is the use of speed display devices (or "vehicle activated signs" – VAS) which are much more cost effective in terms of accidents or injuries prevented. The following is a brief summary of the information present on the Safespeed web site (see www.safespeed.org.uk/vas.html which was produced by Idris Francis and others), based on the original TRL report on the subject and scientific analysis of the relative costs and benefits:

The original TRL548 report on such devices said that they reduced accidents by one-third in their study and that they were very effective at reducing speeds. Indeed they are more effective than speed cameras are at reducing accidents and casualties.

Vehicle activated signs cost about £5,000 (or less) to install, with very low maintenance costs leading to average costs of less than £1,000 a year whereas speed cameras cost about £50,000 to install with average annual costs of the same order. In addition VAS can be used for more than just speed limit reminders – they can actually warn of junctions or the nature of other hazards ahead.

The relative cost-effectiveness of display devices versus cameras is therefore about 50 to one. This is an enormous difference and yet even after this figure was well known, speed cameras are still being advocated by central Government and some politicians who seem more interested in "punishing" motorists than in cutting casualties.

The key point is that for the same amount of money (and budgets are always limited), you can save many more lives and injuries by spending the limited resources that are available on speed display devices or VAS, and not on cameras.

In addition you avoid the criminalisation of large swathes of the population (over 200,000 people banned from driving now annually due to getting too many points on their license, thus threatening their livelihoods). In addition, thousands of people are involved in the totally unproductive activity of issuing speeding tickets, and collecting the fines, including of course the police and courts staff who would be better occupied on real crime.

## The Contrary Evidence Ignored

Prof Allsop has been very selective in the material used to produce his report. He only covers two reports in any detail (the PA Consulting/UCL 2004 Evaluation, and the even older West London Demonstration Project which reported in 1997). No more recent or foreign investigations are reported and he only mentions in a misleading manner the literature review contained in the Cochrane Review of speed camera studies (see above).

But there is considerable contrary evidence which is widely available and which he does not mention. A summary of some of that is given in Appendix A.

**Summary and Conclusions**

I hope I have shown in this note that the report from Prof Allsop is defective in many areas and potentially biased. It appears to promote a specific agenda, ignores the contrary evidence and does not fairly summarise the weakness of the evidence presented.

There is no certainty concerning the impact of speed cameras, other than we know how many people are fined as a result and what is spent on the safety camera partnerships.

In addition the Allsop report totally ignores the clear evidence that there are better ways of spending money available for road safety measures than on speed cameras. The bottom line is that wasting money on the least cost effective methods inevitably leads to more deaths and injuries than would otherwise occur.

Roger Lawson, B.Sc., M.B.A., M.B.C.S.
Freedom for Drivers Foundation
23/12/2010

## Appendix A – The Contrary Evidence

This Appendix is simply a summary of some of the evidence of which we are aware which presents a different picture of the effectiveness of the UK speed camera programme. No detailed investigation of the basis of these claims has been made. You should use your own judgement if you place reliance on this information. For more details please refer to the original sources.

1. Idris Francis realised some time ago that although the sort of double-blind trial of treatment/placebo that would be routine in any analysis of medical benefits could not sensibly have been carried out with cameras, both because of the problems inherent in identifying accident sites having similar characteristics and the political difficulty of deliberately not installing cameras at sites deemed dangerous enough to need them, one enormous double blind trial had happened, by default, simply because not enough camera could conceivably have been installed to cover all qualifying sites.

He therefore analysed all 4.2m injury accidents in Britain between 1991 and 2007 and indeed found between 1991 and 2004 that there had been 131,303 one square km. areas ( approximating to the area of camera influence) which had suffered at least 4 KSI accidents in 3 years (the usual installation threshold). This number is so vastly greater than the 6,000 or so sites which ever had cameras installed - and those for the most part only since 2000 - that the overall patterns of accident and casualty changes in the three year periods following the qualifying periods were almost entirely due to regression to the mean, long term trend and (other than for fatalities) changes in reporting levels, with hardly any camera effect.

In other words this analysis offers a way of differentiating between camera effects (including traffic diversion) and the sum of the other effects. Furthermore, this analysis relies on no assumed relationships between speed and accidents, no sophisticated statistical analysis reliant on assumptions or theories or higher mathematics, but is simply a record of what did actually happen (at least in terms of those accidents that became known to the authorities).

Using official Stats19 data obtained from the Essex Data Archive and extracting only the relevant data it was an easy - though tedious - task to generate a database with a separate record for every 1 sq km area, with location and police area code, and to enter up in each record the total numbers of each grade of severity of injury for each year from 1991 to 2007. By trawling through this database it was then possible to enter into an Excel spread sheet a single row showing before and after totals of K, SI, KSI, Slight and All casualties, and the average percentage falls of each. Selection by police area code allowed a new spreadsheet row for each police area to give a total of about 45 rows.

Although the single most significant spreadsheet is that for the usual 4 KSI threshold, over the whole period, sheets were produced for all combinations of 4, 6, 8 and 10 KSI thresholds, for the whole period, the first half and the second half - and then the same again for accidents by severity, 24 sheets in all.

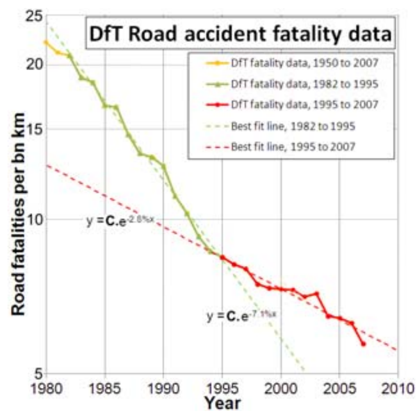
The results throughout are consistent, and the 4 KSI, whole period casualty sheet shows the following national percentage falls: K = 34%, SI = 24%, KSI = 25%, Slight = 4%, All = 5%. However these are quite heavily affected by data for London, a special case due to size, congestion and low speeds. Excluding London the figures are 38%, 28%, 29%, 3% and 7%.

It is clear that, especially for the more serious casualties, these results (arising from RTM, trend and reporting levels only) are of the same order of magnitude as the falls routinely claimed as being speed camera benefit. This implies that camera proponents are actually claiming credit for casualty reductions that have always happened, and would have continued to happen, with no camera present, or as Paul Smith used to say, if a garden gnome had been placed there instead. That this is likely to be the case is confirmed by the practical point that claims made for camera benefit - achievable only by reducing speeds above limits, consistently exceed the proportions of casualties that ever involve speeding in the first place (see below).

More detailed comparisons of these results with camera claims need to be carried out and to make that easier Mr Francis will soon be recalculating the spreadsheets again with all percentage falls adjusted for national trends.

Mr Francis has also questioned how cameras can bring about a 35% to 40% reduction in KSI accidents (or anything over 10%) when only 9% of all accidents involve speeds above the speed limit in the first place, even as a minor causal factor, and that when the "speeding" box is ticked and at least one vehicle was speeding - or might have been? This is the percentage contributory factor from the Stats20/Stats19 police reporting forms. This is particularly so when cameras far from eliminate speeding.

2. Dr Geoff Luxford has shown that the introduction of speed cameras seemed to have a negative impact on the trends in road casualty reduction in the UK. See this web page for the full analysis:
[www.transport-watch.co.uk/transport-speed-cameras.htm](www.transport-watch.co.uk/transport-speed-cameras.htm)
He has also shown that the “extra road deaths” relative to the pre-existing trend closely correlates with the rise in speeding prosecutions. A graph taken from his report is shown below.



3. TRL Report 595 contained some data on the impact of the use of speed cameras (both conventional “spot” cameras, and average speed cameras) in motorway road works. There is no evidence in the report that cameras had any effect on accident rates or accident severities at the sites. To quote from the report (page 1): “*no significant difference was observed in the PIA rate for sites with or without speed cameras*”. See the Safespeed site at www.safespeed.org.uk/trl595.html for a fuller discussion, or refer to the original report available from TRL.

4. The effectiveness of average speed cameras (which one would expect to be more effective than single cameras as they typically cover a long stretch of road rather than a single point) is also questioned by a review of the accident figures for Lower Thames Street in London (see do “Average Speed Cameras Work” on this page: www.freedomfordrivers.org/Cutting_Excessive_Speed.htm).

There are other commentators such as Safespeed (founded by the late Paul Smith, who originally raised the issue of RTM and the lack of adjustment for it in many speed camera studies) and Eric Brigstock who have put a lot of effort into analyzing speed cameras and the negative effects they have had on road safety (see www.safespeed.org.uk/sideeffects.pdf ). Prof Allsop seems to have ignored almost all of this material.

It is unfortunately beyond the scope of this note, and the resources of the author, to undertake a full analysis of all the evidence in relation to speed cameras. Regrettably there are few, if any, soundly based studies which can be relied upon to formulate such an analysis.