RADIO 4 TODAY PROGRAMME SPEED TRIBUNAL

Report of M. Stone, emeritus professor of statistics at University College London.

Though all men are in error, they are not all in the same error, nor at the same time...each therefore may possibly heal the other...even as two or more physicians, all diseased in their general health, yet under the immediate action of the disease on different days, may remove or alleviate the complaints of each other. (Coleridge)

PREFACE

This report weighs the evidence and arguments that Mr Paul Smith and Mr Robert Gifford have presented for and against the propositions that were put to them—that speed cameras should be scrapped and that road humps should be flattened. I give my reasons for downweighting some of the evidence (from both sides) and for lending weight (to both sides) when the evidence merits support. Beyond that, my conclusion is that there can be no formal adjudication of such complex issues. There is a continuum between complete removal of cameras or humps and an increase in their number to a level that would satisfy the most risk-averse person in the land. One's position on that continuum has to be a matter of personal judgement (preferably well-informed). How, for example, should one weigh in the balance the death of a child in a road accident against the statistically determined fraction of the motorists' "freedom of the road" that might be held responsible for that death?

The Today tribunal was intended to bring into public focus and to throw some light on the conflict on our roads between responsible and irresponsible speed—and on the role that the three Es (Engineering, Education and Enforcement) can play in Society's determination of what is responsible and what is not. The main aim of the tribunal, however, was to generate interest in two more Es—those for the Evaluation of Evidence that is often either completely missing in public debate or treated with insufficient attention to important detail.

Not all of the items and ideas associated with the issue of speed could be touched on or adequately explored in one day's discussion or in the position statements that Messrs Gifford & Mr Smith will lodge on the Today website. So I have taken the opportunity here to cover some of the omitted ones and develop others, in order to extend the evidence base on which others will then be free to determine their own position.

A DAY IN CONVERSATION AND CONTROVERSY

"There's been an accident!" they said,
"Your servant's cut in half; he's dead!"
"Indeed!" said Mr Jones, "and please"
"Send me the half that's got my keys." (Harry Graham)

Our tribunal was held at Broadcasting House on June 16th. With producer Daniel Clarke at the controls and Roger Harrabin in the chair, it was in effect a conversation that filled most of the day. A "tribunal" is supposed to be a device for delivering justice. Here, the justice was to be to the arguments set out in the two-by-two written statements from Messrs Gifford & Smith that now accompany this report. The conversation ranged widely. It tested each party's claims in a friendly but searching way. All of us tried to bear in mind the words of Coleridge that I have quoted already and with which I started the proceedings.

At the end of all the challenges and rebuttals, the two parties stuck to their guns. Neither wanted to withdraw any significant part of their written statements. Although inevitably influenced by what Messrs Gifford & Smith said with entertaining and undiminishing vigour throughout the whole day, the judgements in this section of my report will be largely based on what we can all read either in those statements or in the background papers and website files to which they refer. They are judgements that I would not like to be associated in any way with the legal profession. Rather they should be treated as no more than contributions to what will be, and what should be, an on-going, preferably informed and scientifically-inclined debate. The rest of the report will go more deeply into some of the background papers. It will also take some of the issues into areas that were either only touched on in our conversation or that bear on it either directly—or indirectly (at the risk of treading on some political toes).

MY JUDGMENT ABOUT HUMPS

Mr Smith's statement is entitled "The Question of ..." rather than "The Case for ...". The first paragraph indicates that Mr Smith was not quite ready to deploy his technical expertise and forensic skill and come down on one side or the other. Thinking laterally, he does raise the possibility of "distantly related" consequences—a "monster 4×4" bought to ride easily over humps and then doing great damage elsewhere, the "dark and wet night" tyre damage that waits to reveal itself so catastrophically on a motorway. But novella do not make any case! The only reference to hard evidence is when he repeats the widely-held view that the London Ambulance Service (LAS) has "estimated that delays caused by speed humps were responsible directly for the loss of 500 lives each year" and even that was withdrawn in the course of the day's conversation. I will return to the LAS-related evidence in my Appendix B.

Mr Gifford does make a case against road humps being dug up, for which he gives both chapter and verse. The interesting information about how the variety of humps came into being and about the principles that guide their installation can be simply noted. I will look at some of the supporting references in my Appendix B, but here I will simply outline the factors that have to be weighed in any overall judgement.

The benefit of humps that has quite naturally received most attention is the dramatic reduction in the overall number of recorded accidents on humped roads—with fewer deaths and personal injuries, especially among children. Mr Gifford makes reference to the experience of the cities of Hull and York as extensive examples of such records. To that can be added the study by Transport Research Laboratory Ltd commissioned by Transport for London for the GLA hump "scrutiny"[1], which I have not yet been able to get a sight of. It should be noted that data recorded in such 'before/after' studies may be influenced strongly by the statistical artefact of *selection bias* (Appendix A).

A secondary benefit of humps (some may place it first) is their "traffic-calming" effect. Humps undoubtedly create a more pleasant environment for pedestrians, and one that should encourage outdoor physical activity (children's play, cycling etc) with further long-term health benefits. There is a striking photograph on the first page of the Webster & Mackie paper[2]. It shows a narrow humped street in a 20mph zone with parked vehicles on both sides and the front doors of the terraced houses opening onto the pavements—manifestly the kind of road where "traffic-calming" gets the votes of residents and even of sympathetic outsiders. One element on the benefit side of things is the reduction in tyre and engine noise of vehicles travelling at high speed through the residential roads where humps are usually located.

The disbenefits of humps include noise from the gear-changing, acceleration and braking that humps induce some drivers to make—as well as vehicle fumes that such driving will generate. Any damage to vehicles is an optional extra under the control of the driver, except where an emergency vehicle needs to keep its speed up. Damage to individuals sensitive to sharp movements has been widely reported.

However there is a potentially serious disbenefit that might follow any widespread "roll out" of humping and it is one that Mr Gifford does not mention—a possible loss of lives as a direct consequence of longer response times by ambulances and fire-engines. It is possible that we are facing what Bunte's thesis[3] calls "A competition of two public goods". There is the "good" of lives saved by traffic-calming measures, and there is the competing "good" of lives saved by promptly responding emergency vehicles unhampered by traffic-calming humps. This is a matter in which we could have shroud-waving on both sides! I am not aware of any attempt in this country to make a scientific analysis of this particular question. There is probably no hard evidence to influence either way the strength of Mr Gifford's case.

There are also the value judgements that will weigh (on a scale on which life also has to be weighed) the benefits of a pleasant environment for residents against those of a comfortable ride for motorists using residential roads to bypass the traffic holdups on major roads.

Any reader of this will not need me to point out that there can be no judgement about humps that would resolve the matter to the satisfaction of all interested parties. Value judgements are at the core of this question, which takes us into the small-p political arena. Judgment would be assisted by better organization of the fragmentary and not easily available evidence. With a public well-informed rather than politically canvassed, perhaps only city-wide referenda will be able to deal with the question —To Hump or Not To Hump—in a sufficiently democratic way.

Nowadays, statisticians are urged by philosophers of their subject to use prior probabilities. If I were obliged to make a judgement at this stage, it might be that I would accept an even bet that humps would win over emergency vehicle

response times. The bet would be a 'cert' in favour of humps if, as a society, we could get our act together and make good use of paramedics on motorbikes. On the issue as a whole, I have seen no case for wholesale removal of humps. However there is a strong case, before any extensive "roll out" of humps on residential roads, for (a) safeguarding key routes for emergency vehicles, (b) collating and evaluating data about their response times and (c) getting to grips with the problem of selection bias.

MY JUDGEMENT ON SPEED CAMERAS

There is a clarity about the road hump question that is in short supply when we turn to the issues that surround the use of speed cameras. Although there are many sorts of humps and the like, it is clear what they are all intended to do—force traffic to go slowly on residential roads. Humps are very successful in doing this compared with all other methods.

The DfT report[7] just published sets out clearly the "site selection guidlines" that have to be followed by police forces before a camera can be used at any location where a need is felt to do so. For fixed cameras one of the criteria is that there should have been "at least 4 KSI [killed or seriously injured] per km (not per annum) in the last three years". For mobile cameras, only 2 KSI are required instead of 4. All cameras have to be clearly visible to a motorist with eyes on the road. Currently speed cameras have only two forms. They can remain in fixed positions for long periods or move up and down long stretches of road. But whereas, in the case of humps, this variety is concerted towards a common measurable outcome (such as a reduction in average speed to below 20mph), there is no common measurable outcome in the case of cameras. The public is therefore uncertain about what exactly highly visible speed cameras are intended to achieve—beyond getting people to observe the speed limit for a while. The government may claim that fixed cameras, at least, are there to deal with recognised accident "hot spots" whose risk cannot be engineered away. At these locations, they play a role much like that of red-light cameras at dangerous intersections. For cameras in general there is an implicit claim to some higher level purpose that would make sense of Mr Spellar's remark, when he painted cameras yellow in 2001, that he hoped "motorists will realise that road safety is our main concern". But everyone can see that the measurable outcome of cameras is a very localised reduction in speeds. Any technically-minded reader interested in what the outcome may be for accidents can go to a well-designed study[4] from South Wales of which I give a brief account in Appendix B. It appears that the measurable effect on accidents may extend no more than a few hundred yards down the road. In effect, the passing of speed cameras has become an exercise in "gaming" for those eager to exceed the speed limit—a sport that would be frustrated by deployment of hidden cameras at randomly changing locations in the whole road network.

Thus far, I have been giving my own view of the issue, not those of Messrs Gifford & Smith. That is because the multifaceted incoherence of the issue led to a fascinating and wide-ranging discussion of the whole area which is not easily brought into focus. So I will now try to extract some of the arguments in their *written* statements to see where the declared division of opinion really lies.

Start with speed cameras as we now have them—all clearly visible whether fixed or mobile—or as they have been in the last decade. One division between Mr Gifford and Mr Smith is in how willing they are to question the published

evidence. Mr Gifford takes the robust view, shared by most of us, that researchers whose job is to analyse and evaluate data can be reasonably assumed to have done so to the highest available scientific standards, and that those in government whose job is to inform (by condensation of complex documents into executive summaries or press releases) will observe high standards too. If good people cannot assume that, where are we as a society? Mr Smith in contrast will not accept anything that he has not subjected to his own intense scrutiny. Since trust and appeal to authority are anti-scientific virtues, my judgement will have to be made from a stance similar to that of Mr Smith—which is not to suggest that on any particular point one party is more likely to be correct than the other. For evidence about the effect of speed cameras on accidents, Mr Gifford refers to the 1996 Home Office study[5], the 1997 West London study[6], surveys from OECD and the European Commission, and to the widely publicised DfT study of which the report[7] on the first three years of the associated trial was released from its embargo just one day before our tribunal. Mr Gifford expressed his trust in the latter, as Executive Director of the Parliamentary Advisory Council for Transport Safety, within hours of its publication:

> "This report shows clearly that cameras work and are a positive contribution to cutting deaths and injuries on our roads. They have also maintained a high level of support despite the nit-picking of opponents....There are over 100 people alive in these [safety camera] partnerships because of cameras."

Mr Gifford does not refer to the South Wales study[4] to which I have already referred or to the paper[8] by Rune Elvik (see Appendix C). Each of these does cope, in different ways, with the tricky problem of regression to the mean otherwise known as selection bias (explained in my Appendix A). My reading of these two papers (not the others) is that safety cameras do have a real effect on accident numbers. I give some details of the South Wales study in my Appendix C: the estimated effect (a 51% reduction in PIAs) was confined to within a few hundred yards of the cameras. The paper puts this finding nicely into context:

> "In 2000 only 2.9% of all crashes [PIAs]in South Wales occurred within a 500 metre route of [101] mobile speed camera sites included in the present analysis. Thus the effect across the entire area would be expected to be a 1.5% overall reduction, a figure that is well within year-to-year variability. To have a much greater effect, cameras would need to be employed much more widely, and perhaps randomly."

In Elvik's Norwegian study, a 20% reduction was found on a total road length of 336 kilometres. Both studies accord with the message that Mr Gifford has managed to extract from the massive European Commission MASTER study—that the impact of cameras on speeding is a reduction that is "limited in time and space" but that "the effect can be improved by installing a number of camera boxes along the road".

Turning now to the written statement of Mr Smith, the reader should know that I have downloaded most of the files, acquired most of the papers to which he referred, and gone through them with as much care and attention as I could summon. In itself, an achievement of sorts—but paling into insignificance compared with that of Mr Smith himself. He has single-handedly taken on the road safety establishment. He has brought to the fore hitherto neglected

questions with admirable forensic skill and logic. He is a gadfly *par excellence* whose bite must have already irritated many in the road safety world who prefer a quieter way of dealing with issues.

His piece is a powerful polemic attacking the interpretation that others have placed on the body of evidence about the relationship between speed cameras and accidents. I need to discover, here and now, how much I accept of what Mr Smith is claiming. To do this in a reasonably coherent way, I will set out a *hypothetical stance* about the extent and nature of the speed camera nexus—as it is now and as it might be with different management. I will then use that viewpoint (which might be my own) as a baseline for argument, but will be more than ready to change it when I go through Mr Smith's piece looking for anything that could change it. I hope Mr Smith will accept that this way of getting to a final judgement is simply a device for the preservation of mental hygiene.

The assumed stance is a very straightforward one. It is that the present growing multiplicity of highly visible speed cameras, whether fixed or mobile, may be more of an irritant than a serious contribution to road safety. It is possible that the Transport Secretary is right in his claim, on the back of the just-released DfT Report[7] that *at camera sites* there has been a 40% reduction in the number of those killed and seriously injured—over 100 lives saved by cameras! (It should be noted that the DfT estimates contain an unknown element of selection bias, dependent on how the sites were selected. See the end of Appendix A). But no-one can say that these localised savings may not be outweighed by an irritation-induced increase on the 99% plus of the road network that is well away from any safety camera. As the South Wales researchers have pointed out in relation to the savings themselves, such small numbers could be hidden in the statistical noise of year-to-year variability. On the positive side, there is a sizeable majority of the population, even of drivers, who are well-disposed to the use of such things as safety cameras—even when allowance is made for the disgraceful positive bias in the questionnaire by which DfT ascertains that opinion (see Appendix D). This reservoir of public favour is there to be drawn on for law enforcement by hidden technologies of one sort or another—fixed or mobile, random or discriminating. Such enforcement should, as all participants on the tribunal day agreed, be monitored by a reconstituted traffic-police force that could exercise some discretion in how it is applied. (I would recommend not too much discretion, which might introduce too much variation in drivers' expectations!)

Turning now to Mr Smith's multifaceted written statement and to my privileged recollections of our one-day conversation, I extract the over-riding theme of his approach to the problem of speed. It is that speed in itself does not cause accidents. What does is misjudgment by the average driver. Misjudgement stems from lack of training in the skills that will allow the trained motorist his/her own choice of appropriate speed without fear of consequent accident. That Mr Smith is serious about such retraining is revealed by his approval of the old rule that a driver "should always be able to stop within the distance that he knows to be clear". Mr Smith does recognise that there is a category of tear-away criminal driving that would have to be dealt with by a new tough-minded traffic-police force. But he sees only arguments against speed cameras. His program requires that effort should rather be put into ensuring that the bulk of our driving community has driving skills on a level with those of an Advanced Motorist or a product of the Hendon Police Driving School—perhaps high enough to earn a "Licence to Speed". Are we persuaded by this vision to change our hypothetical stance? My own problem with doing that would be that I find I am living in a different world from the one that might render Mr Smith's proposal feasible. Would not the cost of the

necessary remoulding of the national character—now seemingly approaching cut-throat competition—be too onerous for Gordon Brown to consider? The feasibility of government intervention (who else would do it?) has to be taken into account. In the end, perhaps sophisticated speed limit enforcement—which is what the hypothetical stance envisages as a replacement for the current practice—is the price that the "normal motorist" has to pay for peace of mind on the roads. This trade-off would be another example of Bunte's "competition of two public goods"—where peace of mind about road safety is one good and the "freedom of the road" is the other.

There is much in Mr Smith's piece that is offered in support of his claim that speed in itself is not the cause of accidents. One way of testing the sense of that claim is to ask whether we can imagine any scenario in which "speed" could ever be said to be the sole cause of an accident. Perhaps my imagination or my powers of lateral thinking are weakening with the years, but I have been unable to do that with any degree of realism. I find that my "speed" has to be combined with a precipitating factor for anything to materialise. I have to accept then that "speed" must always work in combination with some other factor. Speed can then be truly said to be a causal element if we can maintain the following: that the accident would probably not have happened if the speed had been so-and-so many miles per hour less, other factors remaining constant. If I accept this line of reasoning, I cannot accept the relevance of Mr Smith's treatment of the matter as grounds for rejecting the hypothetical stance.

A second strand of Mr Smith's case rests on his questioning, verging on the censorious, of the propriety of attributing causality to the role of speed in empirically established relationships between speed-based measures (such as its mean and coefficient of variation) and accident numbers. In this, I think he has a valid point. But it is one whose proper place is in the scientific discourse that organisations such as TRL Ltd should now be willing to entertain. Not one, I suggest, that can overturn our hypothetical stance in favour of Mr Smith's case.

A third strand allows us to end this examination of that case on a lighter note. It is something that is given great weight by some of the parties to this dispute. Mr Smith's document (www.safespeed.org.uk/fatality.html) is a beautiful exposition of the fact that between the mid-Seventies and the mid-Nineties the number of road deaths in Great Britain per so-and-so miles showed a fairly constant percentage decrease from year to year, but that from 1995 until today there has been an increasing departure from that trend line—the so called "fatality gap". Karl Pearson (of chi-square fame) can be brought in again, as Mr Smith points out, in the shape of an impressive correlation coefficient between this gap and the number of fines "issued by speed cameras". Mr Smith argues that this must be a causal relationship (at least in part) because he can find arguments against all the other possible explanations he can think of. I cannot follow him in this because I do not accept that his list is exhaustive. There have been many other progressive changes in the last decade (in our social structure, demography, behaviour etc) that I will not enumerate here, but rather invite the reader to do the same. We must always bear in mind the classic year-by-year relationship between the number of breeding storks in a certain Prussian city and the number of babies born in the months that followed the breeding season. Not far, as the crow flies, from the inference problem we have here. It is, of course, *possible* that Mr Smith is right and that the frightful accumulation of deaths from the "fatality gap" ought to be laid at the door of irritation with visible speed cameras. Nothing short of a widespread psychoanalysis of drivers could provide evidence on the matter, but right now there is no evidence.

I am therefore left with an acceptance of the hypothetical stance, which I will complement with the following comment on the DfT program. The "roll out" of safety cameras by separate Safety Partnerships was initiated by DoT. Its management was placed in the hands of the private sector company PA Consulting Group. This "cost recovery" program has failed except for the HMT requirement that it should be self-financing. There has been a failure to design the program so that it would provide the information needed to evaluate alternative ways of getting the benefits of speed camera enforcement. The emphasis on political acceptability has led the program down a *cul de sac* in which essential public trust has been lost. The mistakes already made should be openly recognised, and the program should be subjected to a root-and-branch rethink.

THE WIDER FRAMEWORK

> *18 September 1939: I set off slowly on the Great North Road. At Ferrybridge, a cyclist shot out of a side road straight across the major road. I swerved and just managed to miss him by inches. He pedalled on furiously and went headfirst into iron railings on the far side verge...*
> *22 September: The old cyclist from Ferrybridge is going to survive...His son had been killed on that crossroads quite recently and the old man had sworn never to stop for a car again in his life.* (Countess of Ranfurly)

The present cacophony and confusion about cameras and humps was entirely predictable, given the record of government activity concerning road safety over the last twenty years. With no coherent longterm policy free from day-to-day political interference, we now run the risk that the issues will be decided by soundbite and snappy editorial. The pity is that, underneath the obvious discord, there are many good people doing good work that could be put together to reach a reasonable compromise between the two Ss of Safety and Speed. Even good people may find themselves disagreeing strongly, as do Mr Gifford and Mr Smith, but for sixty million people unevenly distributed on one small island compromise is surely better than conflict.

There are two ways of writing the recent history of government activity as it affects England and Wales. The first would be a bland list of legislative steps and their Green or White Papers. The second would be an "inside story"—of how the justifications or excuses for these steps have been made or obtained from a variety of sources. Both ways of writing the history are necessary, but the latter is the more essential if we are to understand the context and shape of the current debate.

For most of the 20th Century government departments relied, for the research they needed for decision-making, on a variety of "official" scientific workers, from in-house civil servants to others working in universities, research council groups such as the MRC Applied Psychology Unit (APU) and government-funded organizations such as the Road Research Laboratory (RRL). The work of these scientists was routinely subject to wide, often public, peer review. For example, the influential study in the 1950s on the effect of alcohol on driving skill was the work of individuals from APU, RRL and UCL.

By the 1990s the emphasis had shifted significantly towards a wider range of research inputs to the processes of government decision-making. Greater reliance was placed on the "wisdom of the private sector" and on the contractual arrangements that dealing with the private sector required. By the 1990s universities had embraced, either willingly or under the financial pressures of their dictated expansion, much of the ethos of the private sector and saw themselves as competing in the market place for what might be described as "market research" contracts with government departments. All of which was accompanied by a steady decline in the number of (uncompetitive) "official" scientific workers. In the following, "DoT" embraces all predecessors of the present Department for Transport (DfT).

Legislative growth:

- *1981*: Transport Act authorises the road hump concept.
- *1990*: Local authorities allowed permanent 20mph limits if they can get average speeds below 20mph by physical "traffic calming" measures e.g. humps.
- *1991*: Road Traffic Act allows prosecution based on automatic recording of vehicle speed.
- *1992*: The first speed cameras at fixed sites. DoT, Home Office and Welsh Office say that sites should be chosen to maximize road safety benefits (by taking accident statistics into account), but that cameras should be used only as a last resort.
- *1993*: A plan to use more safety (speed and 'red-light') cameras.
- *1994*: Over half the 43 police forces are using cameras.
- *1996*: Over 700 cameras in place.
- *1997*: Removal of traffic-policing from the core duties of police forces.
- *1998*: Treasury allows fines from cameras to be used to pay for more cameras or better enforcement.
- *1999*: Local authorities get a free hand to put humps on up-to-30mph roads.
- *2001*: The Vehicles (Crime) Act 28 leads to a national "roll-out" of cameras.
- *2002*: Cameras are painted bright yellow in response to pressure from motoring lobbies.
- *2004*: Penalty points are to discriminate between moderate and excessive violation of the speed limit. The current camera number of roughly 5500 to be increased by 1500.

Assembling justifications:

- *1992:* The Highways Agency funds the West London Speed Camera Demonstration Project.
- *1993:* DoT funds a Brunel University study of how drivers describe their own response to safety cameras.

- *1995:* The Home Office engages Price Waterhouse to make a cost benefit analysis of safety cameras.
- *1996:* The Transport Research Laboratory (TRL) reports on a study of physical traffic-calming measures—humps and the like. (TRL is here still an executive agency of DoT, but by 1997 it will be a private company.)
- *1997:* DoT pays TRL Ltd for its study of the effect of traffic-calming on vehicle emissions.
- *2000:* TRL Ltd reports for DoT on the effect on accidents of traffic-calming measures in 56 villages throughout Great Britain.
- *2000:* DoT publishes its in-house exhortation *Tomorrow's roads—safer for everyone.*
- *2001:* DoT contracts PA Consulting to make the first-year report on a pilot study of the costs and benefits of safety cameras in eight police force areas.
- *2002:* DfT engages UCL to help PA Consulting to produce the two-year report on the pilot study.
- *2004:* DfT publishes its first review of progress to the 2010 targets set in the DoT's millenial exhortation.
- *2004:* After the June elections, DfT releases the PA Consulting Group & UCL's three-year report on the pilot study for 24 police force areas.

THE VARIOUS SORTS OF EVIDENCE

It is good to be out on the road
And going one knows not where. (John Masefield)

There are four broad categories of evidence relevant to the growing problem of moving around the roads of this country:

(i) traffic flow measurements,

(ii) vehicle speed measurements,

(iii) police records (STATS19) of personal injury accidents (PIAs) and Home Office records of traffic offences and criminal convictions,

(iv) surveys of public opinion and self-reported behaviour.

These categories differ greatly in both quantity and quality. Category (iii) is effectively comprehensive since it is legally mandated. The others are costly and, as may be expected, not comprehensive enough to answer some of the important questions that need to be answered. I discovered this for a study[9] of cycling accidents: we have little idea of how much cycling is done by different categories of cyclist and so cannot generally compare the different risks.

The Today tribunal concentrated on PIAs and on speed as a *necessary* causal factor—no-one is injured unless there is movement in one vehicle or another. However one element in the more general debate is the "injury" to lifestyle that moving vehicles can collectively generate for pedestrians and cyclists in cities and country lanes. "Measuring" that injury in category (iv) is straightaway to enter the political debate about "traffic-calming' and cameras'.

Any police officers who filled in the STATS19 record for an accident, during the decade following its inception in 1949, was required to look for its causes—specifically to identify what were called "contributory factors" from a prescribed list. When the requirement was dropped in 1959, many police forces continued to do this seeing it to be in the public interest. At this time, when a causal analysis of road accidents appears even more necessary than it was in the Fifties, we are still without a coherent system of national records of "contributory factors" that would throw light on the relationship between "excessive" or "inappropriate" speed and accident rates.

There is another evidential matter that bears heavily on some of the controversies in our debate. When they "attend" an accident, police have some discretion about whether or not to treat it as an accident involving personal injury (PIA) thereby initiating the time-consuming STATS19 recording prcedure. Even greater discretion can be exercised as to whether the injury is classified as "slight" or "serious". "Fatality" (death within 30 days) is less ambiguous except in very rare circumstances!

PUBLIC MOOD AND OPINION

What academic papers say is one thing—what "the papers" say is another, and what you find on the Internet is yet another. At the end of May, I got four numbers out of Google. They were the numbers of "hits" for the two entries "speed cameras" and "road humps" (entered with the quotes)—first for the world and then for the UK. Here they are:

	World	UK	UK percentage
"speed cameras"	197,000	134,000	68%
"road humps"	8,980	7,160	80%

Is there a British obsession in these matters? It would be no more than a life sentence for me to click on the 63,000 and 1820 non-UK English-usage hits for cameras and humps respectively. But I would have to enrol my grandchildren to do the same for the much larger number of hits from the home country. Why the inequality?

That question may already have been implicitly answered in the June issue of the house magazine of the Royal Statistical Society—where an editorial[10] concludes:

> "It is surely in the government's interest that its own statistics are perceived to be trustworthy and above political manipulation. But carefully selecting or spinning statistics can offer some short-term political benefit, even if history suggests that the term may turn out to be a good deal shorter than the spinner anticipated [expected].... Is the government sufficiently far-sighted to be self-denying, and to legislate to put the weapon of statistical spin out of its own reach?"

The editor was writing about the handling of the "official statistics" that (unthinking) statisticians have to collect as carefully as possible so that others—mandarin economists and the like—can think about them at the level of public policy. But I think her comments apply to the matter in hand too. Many of us appear to have lost confidence that our political masters can reliably sort out complex issues without resorting to some level of subterfuge and concealment destructive of trust. If Lord Hutton had written this he might have interpolated an "unconscious" before the "subterfuge".

How DfT ascertains how favourable the public is towards speed cameras, and what percentage are in favour, are matters that may be as important as technical studies of their effects. Many of the judgements of individuals claiming a louder voice on the basis of their technical expertise in road safety matters are simply value judgements. The crude expression of public opinion is also a value judgement made up of a large number of less weighty contributions which in aggregate do have a significant combined weight.

The design of the DfT questionnaire that elicits the targeted opinion "South of the Border" is a sorry tale best half-concealed. Those willing to read it should go now to Appendix D. "North of the Border", the 2003 study[12] by Stradling *et al* commissioned by the Scottish Executive has a more straightforward definition of favourability than the one used by DfT. (Does the wind on the grouse moors clear away academic cobwebs and the spinning that goes with them?) The Scottish question was (I trust) something non-prejudicial like "Are you in favour of or are you against speed cameras?". Those either "Strongly in favour" or "In favour" were counted as favourable. Of over a thousand Scottish car drivers, 68% of males were favourable, 82 % of females. Only for 17-24 year-old males did the percentage fall below 50.

There are very few studies that pay proper attention to the opinions of other road-users such as cyclists, pedestrians, and those dependent on or making use of public transport. The evidence of London boroughs to the GLA hump scrutiny reveals both the superficiality and the difficulty of ascertainments of public opinion in such matters. For example, drivers will want humps in their own residential road but not elsewhere.

APPENDIX A

Selection bias: Regression to the mean

This is a statistical phenomenon that has for many years haunted researchers in before/after studies of accidents. It is a statistical artefact pointed out by Francis Galton over 100 years ago, which he illustrated by thinking about the heights of fathers and sons:

> *Tall fathers and short fathers tend to have sons closer in height to the average than they were.*

It looks, superficially, as if the heights of a population should be getting closer to the average with time—clearly an absurdity that conflicts with experience! The resolution of the paradox lies with the counter-tendency of fathers of medium height to have sons that are further away from the average than they are.

The regression to the mean (rttm) artefact has to be taken into account when streets (corresponding to fathers) are selected for humping because they have an accident record (corresponding to a father's height) that is well above average—well enough to generate the political will to put humps in place. The problem of inference about the success of the humping is that we may then be unable to estimate how much of any decrease in the subsequent accident record (corresponding to the son's height) is due to the slowing down of the traffic and the reduction in its volume, and how much is due to Galton's rttm. The same problem is faced by researchers wanting to make inferences from the accident data "at" camera sites before and after the installation of the cameras—or by others with an eye to safety campaigning or political propaganda. The seriousness of the rttm effect depends on two features of any study:
(i) The *degree of selection* (Are we picking out only a small fraction of streets or sites from a larger number with very similar accident risk levels?)
(ii) The *length* of the 'before' period of observations on which the selection is based. (If long enough, the accident levels of the selected streets or sites will be measures in which chance variation plays little part, and which establish the base levels from which any change may be properly attributable to the humps or the cameras.)

APPENDIX B

Webster & Mackie's (W&M's) 1996 review[2] of traffic calming schemes

TRL researchers may have given crucial advice on physical design but it seems they were never in charge of the experimental design —involving the decision processes by which local authorities selected the roads that would make up their 20mph traffic calming zones. (These were political decisions.) W&M[2] state that "the most quoted reason for applying for an authorisation for a 20mph zone was accident reduction". They knew this raised the spectre of Galton's regression to the mean (rttm) phenomenon already noted in connection with the Price Waterhouse camera study[5]. W&M offers reassurances that rttm is "unlikely to be a major problem with 20mph zones as they have generally not been selected because of a high accident rate" and that "an additional safeguard is the long 'before' period (up to 5 years)".
Of the 200 or so local authority schemes then in operation, W&M looked carefully at the data on accidents for the 72 schemes that had been in place for at least a year, and at the data on speeds for the 32 that had data on both 'before' and 'after' speeds. (The professionalism of this TRL report allows the reader to see these data too.)
It is no surprise to see that the average speed went down in all of the 32 schemes by an average of 9% (the 'after' average was the average of the mean of the between and going-over hump speeds). So there was undoubtedly "traffic calming"—although we are not told about the few motorists who found they could feel no bump if they went over the humps at 60mph, because their shock absorbers took the bump instead.
There were 857 injury accidents (PIAs) in the months 'before' the schemes were started up at different times over the period 1990-1994. Their number over the months 'after' was 231. Dividing these numbers by the respective aggregate numbers of months over the schemes, 3047 and 2098, and multiplying by 12 to get years, the "annual rate" went down from 3.38 to 1.32—an impressive reduction of 61%. The paper pays homage to the Chi-square test to

declare this reduction statistically significant at a miserly 5% level—another misuse of Karl Pearson's legacy, I fear. All that was needed was the observation that, when we compare the more justifiable 'before' and 'after' annual rates for the 72 individual schemes, there were 63 decreases, 7 increases and 2 no changes. Did you ever toss a fair coin 70 times and get 63 heads?

An alternative to accepting W&M's reassurance about rttm might be to look at the accident number in the 'before' months between the time when the local authority decided on the scheme and the time of its implementation. My experience of local authority decision-making is that this interval could be a year or longer. The rttm phenomenon would not affect a before/after comparison based on such numbers. However even if we could get an estimate of the reduction that we could be sure was unaffected by rttm, further adjustments would be needed to take account of the general downward trend in PIAs and of the reductions in traffic flow that averaged 27% for the 19 schemes where 'before and after' flows were measured.

Finally, a comment on the striking conclusion in the Executive Summary that "there was a 6.2% reduction in accidents for each 1mph reduction in vehicle speed". Such summaries of otherwise credit-worthy reports are the bane of government science just as executive houses are an architectural blight in England: the frequent misrepresentation of the content of official technical reports leads one to wonder whether authors are free to control what their Executive Summaries tell us and how the say it. All of us, not just executive summary writers, are prone to accept single statistics divorced from the serious or playful work that generated them—statistics that require effort in order to be properly appreciated. (Questioning them is such a chore!)

The 6.2% reduction was the ratio of 58% (the average of 32 percentage reductions in accident rates) to 9.3mph (the average reduction in speed for the 32 schemes). The Figure in the text that playfully supports the 6.2% (as the slope 6.1 of the fitted regression line) shows that *there is no progressive relationship between speed and reduction in accident frequency* (as the wording of the Executive Summary suggests) if Brighton and Preston are excluded. These two schemes were anomalous in roughly doubling their accident rates but with smaller speed reductions. They might well have been treated as outliers from an otherwise zero-slope relationship or as unreliable "due to random fluctuations as the numbers are small" (the phrase used by W&M to enhance the positive findings for the reductions in child pedestrian & cyclist accidents). All this matters because of the wording of the Executive Summary. Its "conclusion" that there is a progressive relationship may influence local authorities to make further speed reductions in their existing traffic calming zones in the interests of accident reduction—when the W&M study provides no statistically acceptable evidence for a progressive relationship. It remains a possibility, of course, that any speed reduction could ameliorate accident *severity* even if it did not reduce accident *frequency*.

Mackie's 1998 review[13] of urban speed management

Data from America, Austria, England, Germany and Scotland are nicely set out by Mackie, and put together for the conclusions that Robert Gifford has reproduced. For speed, the overall conclusion is hardly questionable—that obstruction of the highway by humps, bumps or other physical devices is the most effective way of cutting average speed. For accidents, if lives directly saved by traffic calming have to be weighed against those lost elsewhere, the

size of the reduction will have to be estimated—and for this the picture is murkier. Covering many separate sources of evidence, Mackie could give few details, but he did note that German researchers may have been influenced by Galton's rttm artefact! This may have infected a simple table of annual rates of personal injury accidents (PIAs) or fatality and serious injury (KSI) accidents—the 'before' and 'after' data for the installation of *either* only 30kph signs in 36 zones *or* signs plus "physical calming" in 24 zones.

	Signs only in 36 zones			Signs + calming in 24 zones		
	Before	After	% change	Before	After	% change
PIA accidents	45.6	41.5	-9	115.9	80.2	-31
KSI accidents	9.5	12.8	+35	36.8	23.3	-37

Did Herr Pfundt claim that these and similar data showed that signs are ineffective in reducing accidents whereas physical calming is? It is a pity that the study did not somehow let signs show what they could have done with the high 'before' levels of accidents and physical calming what it would have done with the low 'before' levels. The "somehow" would have been achievable by allocating zones to signs or calming at random. This would at the same time have resolved the rttm problem, which is perhaps manifesting itself in the 35% *increase* for the KSI signs-only accidents. If, as is likely, the 24 traffic-calmed zones were selected for their high KSI rates then the complementary selection will make the signs-only zones an analogue of Galton's short fathers.

Bowman's 1997 mathematical analysis[14] of emergency response delay

The air, especially the air of London, is full of unresolved claims about how humps affect the response times of emergency vehicles. These concentrate on the question of whether more lives are lost, through delay in getting paramedics to those within minutes of dying after the 999 call, than are saved by physical calming measures.
DoT implicitly recognised the problem for fire and ambulance services in their 1994 code of practice [15]:

> "There is concern that the cumulative effect of the growing number of traffic calming schemes could compromise the ability of fire and ambulance services operations to meet required response times. They should have the opportunity to make an input to decisions on the introduction and design of traffic calming schemes."

Webster & Mackie[2] gave some (not particularly helpful) advice:

> "Emergency services vehicles may have particular needs and schemes should take care to avoid the need for them to travel over long stretches of traffic-calmed streets."

In his evidence to the GLA "scrutiny"[1] the chief executive of the London Ambulance Service (LAS), Peter Bradley, wrote that :

> "...LAS is not aware of any significant UK-based report that has made any scientic study into the overall effects of speed humps, including their impact on ambulance response times, the potential consequences of delayed responses by emergency vehicles to 'life threatening' situations, and their effect on the comfort of patients being conveyed to hospital. We would welcome the opportunity to be involved in any such research if it were to be commissioned."

and then

> "This Service believes that it could probably save more lives if the overall traffic flow were to be improved. Just among the 5000 cardiac care victims that we try to resuscitate this could possibly save about 500 lives. In addition a minute gained in reaching other life threatening cases could potentially save hundreds of lives."

Bowman's paper is a prototype of the scientific analysis that Peter Bradley called for. It was devised in response to a Boulder City Councillor's request for data that would "allow a comparison of the impacts of delayed emergency response time and the putative increases in neighbourhood safety":

> "His question can be answered well enough by considering cardiac arrest emergencies, for which reasonably good data exists, and then making a crude estimate to allow for the other critical emergencies. Data on the following are required: traffic accident deaths, cardiac arrest emergencies and survivals, emergency response times, and delays from traffic mitigation devices. Fairly accurate numbers exist for all of these items. They are certainly accurate enough for reasonable 'risk assessment', which often involves far worse vagaries and rough estimates than in the present case."[14]

In this country, getting reliable enough data may be more of a problem than it seemingly was for Mr Bowman. However, the mathematics needed to elicit what the data could tell us is hardly intimidating. The probability of surviving a cardiac arrest is the expectation (over the distribution of response times) of the probability of survival when resuscitation gets under way at those times. Bowman used a mixture of local and medical textbook data to estimate how survival probability would go down for a 14% increase in all response times. The answer was—enough to put any traffic-calming life-saving in the shade. Bowman's work has been around for seven years. In 2000, Bunte's US study[3] (a 257-page Master's Thesis) gave strong support to his findings. The method could and should be refined if it were transferred from Boulder to London or Hull. For instance, there would be a case for replacing *numbers* of lives saved by the *expectation of years of life* saved: ambulances & fire-engines save mostly the elderly but humps save lives too, particularly of children. Whatever is done, should we not be curious (rather than prejudgemental) about whether carefully collected data here would give humps the green light? If it did, that would suggest that the US findings are reflecting some cultural difference, such as the observable discipline of drivers and road-crossing pedestrians on residential roads in the US.

The GLA "scrutiny"[1]

The Foreword to the London Assembly Transport Committee's "scrutiny"[1] gives short shrift to the LAS's appeal for overall scientific analysis:

> "The clamour has grown to fever pitch as the Borough of Barnet has begun to remove humps from their roads and the London Ambulance Service has claimed that they could probably save more lives if overall traffic flow were to be improved. *The purpose of the London Assembly's investigation is to examine the available evidence...The evidence is overwhelming* in terms of the success of humps in reducing death and serious injury. The challenge for this scrutiny has been to make recommendations that will help improve the design and implementation of traffic calming schemes in future years....The *Boroughs* and the emergency services must work together...we need accurate monitoring of the effectiveness of *each scheme*...any borough removing humps must replace them with an equal or better alternative..." [My italics]

None of the committee's recommendations call for a London-wide scientific analysis involving response times of emergency services. Rather they leave things in the hands of individual boroughs and their local emergency services. The body, Transport for London (TfL), that could already have commissioned such a study clearly thinks that further research is unnecessary:

> "The London Ambulance Service has made claims that 500 lives are lost each year due to increased response times, which they attribute entirely to road humps [??]. This claim is unsubstantiated. The main factors affecting emergency vehicle response times are general vehicle speeds in the network, which result from overall levels of flow and congestion." (Para. 2.2 of TfL's evidence to the GLA scrutiny.)

APPENDIX C

About cameras

The Price Waterhouse study[5] *for the Home Office*

This 1996 cost/benefit study suggested ("demonstrated" was its claim) that cameras can generate substantial benefits. For speed cameras, the calculation of net benefit was based on changes in the annual rate of personal injury accidents (PIAs) "at" 174 camera sites in nine police force areas, between a period ("usually 3 years") and "the period" (unspecified) after their installation. The overall reduction was 28%—from annual PIA rates per camera site that ranged from 2.6 to 15.2 for the nine areas.

Unfortunately this study does not and cannot tell us how much of the 28% if any is left over once allowance has been made for the statistical artefact pointed out by Francis Galton over 100 years ago, which he illustrated by thinking

about the heights of fathers and sons. Tall fathers and short fathers tend to have sons closer in height to the average than they were. The standard criteria for a speed camera installation are that there must be at least 4 accidents per km involving fatality or serious injury and at least 8 PIAs per km, over a site length of 400-1500 metres (that is what the "at" means). In other words, cameras are put where they appear to be needed in terms of a highly variable number of accidents from year to year—which is the precise analogue of looking only at tall fathers! We have to expect some reduction, on the average, when we go from the period before the installation of a camera to the period after—and no cost/benefit analysis is sensible unless we can make a reasonable estimate of the size of that reduction. The authors of the report ignored the possible influence of Galton's regression to the mean (Appendix A) and accepted the numbers they were given with the comment:

> "It should be noted that the sites covered within Table 10 were not randomly selected, but were chosen because the local authority responsible had already collated some 'before and after' data in relation to them."

The neglect of Galton was then compounded:

> "Time restrictions also meant that it was not feasible to undertake precise matching exercises to determine whether any reductions identified formed part of a more general underlying trend, rather than reflecting the impact of traffic cameras. Careful consideration was given to whether other factors could have accounted for the identified reductions. No evidence was found to suggest that this was the case or that the conclusions reached were unduly optimistic."

The West London Speed Camera Project [6]

By October 1992, 21 speed cameras and 12 red-light cameras were in place on trunk roads in West London, installed in accordance with the DoT criteria already mentioned i.e. in accident-prone locations. The "Project" selected 10 stretches of road, 53 miles in all, to include all 33 camera sites, and used the police records (STATS19) to look at changes in the numbers of accidents and casualties between the three years before and the three years after the camera sites became operational. There were some big changes. The most dramatic was that the number of accidents involving fatality went down by 69% (from 62 to 19).

The study used what it called "rigorous statistical analysis" to suggest that this reduction was a real effect of the cameras. The report misused the widely-taught chi-square test of statistical significance in its attempt to make a "rigorous statistical analysis" of the big differences between the percentage changes (in accident numbers) for the 10 different stretches of road. These indicate that there are other factors at work between stretches that pull the rug from the application of chi-square to the totalled numbers! Chi-square needs integer data but integer data does not automatically justify or require chi-square.

In pursuit of rigour, the analysis did not do what it should have done—use a broader statistical brush not dominated

by over-computerized significance testing. The latter produced a particularly memorable line in the table for fatality accidents:

Old Western Avenue	0	0.0	0	0.0	*0.0*	*0.0*	Highly significant (1% level)

where the first and third zeros are the 'before' and 'after' accident numbers and the others are derived from them. The high significance is probably the result of the computer trying to deal with an illegal division by zero.
Homage to chisquare also had the debilitating consequence that the project felt unable to compare casualty numbers from the camera roads with the comparison ("control") casualty numbers. The grounds for this were that

> "Only accident categories are subject to rigorous statistical testing. Casualty categories are not appropriate for such testing because they are the outcome of random events and not the events themselves."

The quotation shows an admirable awareness that chi-square cannot be used for everything. In fact, the conditions for its valid use are so restricted that misuse is much more common than proper application. That was not what its inventor Karl Pearson (the first professor in my department at UCL) intended over 100 years ago. But we all err from time to time. Even Homer nods, and Pearson himself started by getting the "degrees of freedom" for his invention wrong and had to be corrected by another UCL man, the illustrious R.A.Fisher. Incidentally, Pearson made something else that bears heavily on the present debate—the observation, well before government became a significant contractor in scientific research, that

> "To endow research is merely to encourage the research of endowment."

If we discount the misuse of chi-square, it is still difficult not to be strongly convinced, by that dramatic 69% reduction in the number of fatalities, that the cameras did what they were intended to do. But that may be because we are not told enough about the design of the project to know that Galton's rttm has struck again. We are once again left with uncertainty about how much of the 69% is real camera effect and how much is statistical artefact. Did the Highways Agency or some other body point the project to West London and those particular stretches of trunk road because it was observed, during the three years before the trial started, that they were suffering exceptionally high levels of fatality accidents? Were the lengths of the trial stretches just long enough to include some known accident "hot spots" for camera locations? How does their total length compare with that of all A-class trunk roads in "West London"? The report might have given readers some reassurance or information on these questions. It was also remiss in other matters. We are not given the accident numbers on the so-called "control" roads with which the trial stretch numbers are compared. The same numbers are used in each of the applications of Chi-square to individual stretches of road: their reliability (determined in part by whether the numbers are large or small) bears heavily on the extent to which these separate significance tests can be treated as independently informative.
From 1992 onwards, cameras were introduced all over London, and the project was therefore without any scientifically

respectable control data. The project researchers had to be content with comparing the 69% reduction with a smaller decrease of 14% on non-trunk A-roads in other Boroughs (I was able to deduce the 14% by a reverse calculation that I should have not had to make). Logic excluded using the non-trunk A-roads in the *same* Borough for such comparison, since any difference favourable to cameras could have been attributed to a displacement of accidents to those roads from the roads with cameras.

On the negative side, the study found one worrying feature of the data—an overall 28% increase in rear-end collisions on the camera stretches over and above their percentage increase on the comparison roads. In this case, where the data were saying something against camera advocacy, we *are* given some information about accident numbers on the "control" roads!

It is a pity that the informativeness of this study is less than it would have been if the "roll out" of safety cameras over London had had at least an element of experimental randomization—akin to that routinely used in medical research to find out whether a new drug really works. (A staged introduction of cameras could have incorporated this, and a full "roll-out" could have been made as soon as the evidence showed that the cameras were saving lives.)

Evading the rttm artefact.....with Hauer and Elvik

Thinking of Galton's tall fathers, there would be no rttm in the heights of their sons if we could estimate the parts of the fathers' heights due to chancy environmental non-genetic factors. These parts could then be subtracted leaving adjusted heights that (discounting any tendency for heights to increase with the elimination of poverty) would be unbiased expectations of the sons' heights. We can replace the heights in this argument by accident rates, and the fathers and sons by the 'befores' and 'afters' of speed camera installations.

Working in the Safety Studies Group of the University of Toronto, E.Hauer[16] has shown how the chancy potions of the 'before' rates can be estimated and removed, provided there is enough reliable information about the accident rates in the wider area from which the camera sites are selected (e.g. as passing the DfT's high accident rate criteria for camera installation). Working in the Norwegian Institute of Transport Economics, R.Elvik[8] has been able to apply Hauer's method to data from 64 sections of road between 1988 and 1995. When the adjustments had been made (and the rttm element hopefully removed) the average decrease in accident rate was 17%. But for a quarter of the 64 road sections there was an *increase*. Adjustment works both ways—short fathers have taller sons: the accident rate increases were predominantly among the road sections where cameras had been installed before the requirement from 1992 that the section had to have above average speeds and accident levels to qualify for cameras. The Hauer method is in its infancy and merits further development and wider application. It will always depend on the validity of the necessary statistical modelling. So it is unlikely ever to be as convincing as a experimental randomised trial—the "gold standard" that is called for in our next study by a group of clinical science workers in Wales.

Evading the rttm artefact.....with Christie et al [4]

In 1999, DfT initiated a pilot trial (from April 2000) of a "cost recovery system" that would make safety cameras

self-financing. I will comment later on the commissioned reports on this trial that have been made every year since then—the latest was released from its embargo just one day before the Today tribunal. The Christie *et al* paper[4] was not commissioned by DfT and is therefore a strictly scientific report without any political colouring.
The authors regret that they were obliged to adopt the widely-used matched-pairs clinical trial as a model for their observational (non-experimental) study:

> "The gold standard for the evaluation of interventions is the randomised controlled trial. Unfortunately, randomised trials in this field are difficult to implement, probably because of differences in belief between academics and some injury control practitioners and politicians that the existing evidence is sufficiently persuasive."

Christie *et al* had to take as given the 101 sites for mobile cameras, which operated from parked marked vehicles and had been gradually introduced throughout the South Wales police force area (PFA) between 1996 and April 2000. Since these sites were selected by others for their high accident levels, Galton's rttm had to be taken into account. Matching locations, where cameras might have been but were not put, were found in the adjacent PFA of Gwent. Matching was made on several criteria including equality (to within 20%) of the numbers of PIAs within 500m: the totals were within 1%. The analysis rests on the assumption that the rttms will also match, at least statistically. A *rate ratio* R was calculated for each site for accidents in any category of interest, calculated as the ratio of the number of observed PIAs for the camera site after installation of the box to the expectation of that number—calculated as the product of the number of observed PIAs before installation and the ratio of the 'after' to the 'before' number for the matching site (as recorded for the same periods as at the camera sites). An R-value of unity means that the percentage changes are the same at both camera and matching site. If R were as small as 0.30, the estimation of the camera effect would be that there has been an underlying 70% (i.e. (1-0.30)×100) reduction in the accident rate. The results for accidents within specified distances from the camera site on the same road were:

Distance (m)	No. of sites	Percentage reduction
0-100	101	70%
100-300	90	45%
300-500	75	41%
500-1000	58	10%

(the numbers of sites going down with increasing distance to avoid getting closer than 60m to any junction). The percentage reductions speak for themselves: the paper gives confidence intervals that give reassurance that there is a real camera effect (on top of Galton's rttm) at least to a distance of 500m from the camera site.

The New Zealand hidden camera trial [17,18]:

When the Minister for Transport John Spellar painted this country's cameras yellow in 2001, he gave a triply psychological reason in the absence of anything based on research about the influence of camera visibility:

> "I *hope* that by instructing local authorities to make them more visible, motorists will *realise* that road safety is our main *concern*. ...These instructions emphasize the Government's commitment to using safety cameras as a deterrent against excessive speeding, and not as a means of raising money." [my italics]

Had the Minister's advisors told him about the 1999 New Zealand study that purports to show that hidden cameras are a better deterrent against excessive speeding than overt and visible ones? Mr Gifford has elsewhere adduced the study as evidence in favour of cameras in general as agents for accident reduction. However, what the New Zealand study really shows is that a hidden camera has a greater potentiality for speed and accident reduction than a camera that is clearly visible to the motorist.

The study set out to be a "scientifically designed trial" of the effectiveness of hidden, as opposed to visible, cameras. But, no doubt for good reasons, it ended up with no element of experimental randomization. As far as injury accidents (PIAs) are concerned, its informativeness is essentially based on a change in the slope of a single time-series. The change of slope is in the graph against time of the seasonally-adjusted logarithm of the ratio of monthly numbers observed on open roads of two areas covering New Zealand: the Midland Region police force area of North Island (the *trial area* where hidden cameras were introduced and exclusively deployed from July 1997) and the rest of New Zealand (the *control area* using visible cameras throughout). There are six accident or casualty variables and six associated "percentage reductions" in this study[18]:

Accident/casualty category	"Percentage reduction"
All open road PIAs	11%
All open road casualties	19%
All open road casualties per PIA	9%
Camera area PIAs	17%
Camera area casualties	31%
Camera area casualties per PIA	11%

These "percentage reductions" are in quotes because they can be easily misinterpreted. They are percentage changes in the ratio (no. per site in trial area)/(no. per site in control area), where that ratio has been estimated by a sophisticated mathematical model of the apparent effect of the hidden camera programme on seasonally adjusted numbers over and above a steady straight-line trend in the ratio of numbers for the two areas. Any single time-series has to be regarded as a fragile thing influenceable by factors that come and go. There was a revealing graph in the earlier 1999 report[17] for the New Zealand National Road Safety Committee. It showed how the monthly number of casualties in the *control* area varied over the years—a downward trend until two months after the introduction of hidden cameras in the trial area, followed by a much larger increase above that trend than in the trial area. How did things go in the next two years? The 2002 report[18] does not give us the extended graph, but gives one that removes the trend from the pre-trial logarithmic series making it reassuringly flat before a downward trend is revealed when the trial gets under way. If you get to see it, tilt your head to the right to see that the series can also be described

as a random excursion with a peak a bit before the trial begins.
This interesting study was carefully conducted and nicely reported. But the estimated effects should be treated only as strong indications that will usefully contribute to any meta-analysis of similar studies—not studies of the value of cameras themselves but of the *relative* value of keeping them hidden. Accident numbers may have gone up in both trial and control areas but that would not be revealed by the "percentage reduction".

The rolling 2004 DfT study

The three-year DfT report[7] was released from a politically-dictated embargo on June 15th. As I implied in my speed camera judgement, its analysis of the data from the 24 police force areas makes no quantitative allowance for rttm. Section G3 of the report gives reasons for thinking that "the established statistical phenomenon of regression to the mean [rttm] will not apply in full measure". I interpret this to mean that the authors acknowledge that the estimates of savings of casualties and accidents in the Executive Summary should be taken as upper bound estimates. Will the public be made sufficiently aware of this qualification? Was the minister?

APPENDIX D

That government has long been interested in public opinion about the camera roll-out is shown by the DoT investment in a series of 12 surveys of drivers, 6879 in total, between 1993 and 1996. The eleventh report[11] on the findings can be found on the National Safety Camera Liaison website. Mr Gifford has already noted that the study classified drivers as "conformers", "deterred", "manipulators" and "defiers" in their response to speed cameras. The researchers got "the impression" from all their data that "everyone has a price" and that "provided the threat of cameras remains a potent one, the proportion who ignore them will reduce sooner rather than later [?? sooner or later]". They found that "most drivers in each survey were *favourable* to cameras, although those who had been caught approved less" [my italics]. They devised 10 ways of using cameras to get drivers to comply with speed limits. I think that "sooner or later" may have been intended because the Executive Summary (in this case surely not DfT-speak) ended rather pessimistically (menacingly, some might think):

> "...although we conclude that the proliferation of speed cameras is an important means by which to raise drivers' awareness of the dangers of speed and of inadvertent speeding, ultimately more than this and other forms of speed limit enforcement will be required in order to modify drivers' *views* on speed [my italics]."

How did the researchers determine whether or not a driver was "favourable" to speed cameras? Drivers were asked to respond to eight statements about speed cameras, each answer scorable -1, 0 or +1 on a favourable-to-cameras scale. Guessing a little, I think a driver would have been classified as "favourable" if his/her total score was positive. For four of the eight statements, agreement was interpreted as favourable to cameras, disgreement for the other four—a sort of neutrality. Currently, only four of Corbett & Simon's statements appear in the seven that are now in use by

Safety Camera Partnerships at the dictation of DfT. The four that were dropped were

> "Cameras mean that the government is gaining too much power over drivers."
> "Cameras are a new way to harass drivers."
> "On roads with cameras the enjoyment is taken out of driving."
> "Cameras are an accurate way of detecting drivers' speeds."

replaced by

> "The primary aim of safety cameras is to save lives."
> "The use of safety cameras should be supported as a method of reducing casualties."
> "There are too many cameras in our local areas."

With a little effort, you can see that, in the new seven there are now five statements favouring cameras, and only two against. If, as I believe to be the case, there is a sizeable majority of the population in favour of speed cameras, why use such a loaded instrument to determine or inflate it? More significantly, why do something that could be destructive of public trust?

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