The Speed Camera Delusion*

exposed by

Idris Francis B.Sc.

* *A belief that is held with strong conviction despite superior evidence to the contrary*

This analysis is dedicated to

Winifred Evans

for

making life fun

inspirational teachers
who influenced this analysis:

Thomas Davies

for

When you have worked out the answer
always ask yourself, 'Does it make sense?'

Timothy Jones

for

Remember - your results can never be
more accurate than your measurements

Edward de Bono

for

lateral thinking

The Speed Camera Delusion

Contents

Appendices

The Law of Unintended Consequences strikes again – will they never learn?
Photo- Eric Bridgstock

a. About this Engineer/Analyst

Idris Francis won a State Scholarship in Pure and Applied Mathematics and Physics in 1957, a First Class Honours B.Sc. in electrical engineering in 1960 but in 1962 abandoned a Cambridge Ph.D. in a boring subject to become a circuit designer engineer for Leo Computers in NW London.

In 1964 he founded his own electronics manufacturing company which he sold in 1994 after winning a Queens Award for Export Achievement with an 85% export rate Many of his electronic joystick control designs, including the world's first contactless units, remain in volume production.

From 1983 he wrote relational database programs to control every aspects of his company and from 2002 to date has used the same software to analyse more camera site data than any other analyst appears to have done.

In 2002 he filed an **Application to the ECHR* over the breach of the centuries-old *Right to Silence*** inherent in the Road Traffic Act 1988, losing in 2007 on the absurd argument that reducing collisions trumps that fundamental right, still available to anyone suspected of any other criminal offence including treason, murder, rape, right down to petty theft!

It was that seriously flawed verdict that prompted this former engineer to assess other analysts' methods and the data they use.

The results provided here are surely sufficient grounds to have that perverse verdict reversed.

* Full details on web, search "Idris Francis", "ECHR", "speed", "camera", "right to silence".

b. Preface

Newcomers to this subject might reasonably assume that identifying and quantifying the effects of many thousands of speed and red-light cameras on millions of vehicle collisions **must be difficult and complicated.** In reality and as this assessment demonstrates, the opposite is true – **the larger the volume of data, the easier the task becomes and the more accurate the results.**

As a former **engineer, life-long contrarian and enthusiastic driver** for 60 years, this analyst realised some years ago that the claims of collision reductions achieved by cameras far exceeded what could ever be possible and that **those analysts' methods and/or data must therefore be seriously flawed.**

Influenced once again by Edward de Bono's ***lateral thinking*** ideas and to avoid repeating others' errors, he tackled these problem from basic engineering principles and found that far from being complex as statisticians seem to believe, **analysis can be both simple and accurate.**

The results that follow are consistent, credible and compelling. Based on much larger volumes of better data than other analyses have used and achieved by simple and transparent methods, **they are also beyond rational dispute.** That is not to say, of course, that **there will be no irrational dispute,** given the scale of money, jobs, reputations, ego and other vested interests now (hopefully) at risk in the camera "industry". Instead it is more than likely that the usual suspects **will challenge these results, and when that fails, try instead to ignore them.**

However, the simple and appalling truth is that spending several billion pounds of public money and penalising millions of drivers, far from improving road safety has led directly and indirectly to significantly more collisions than would otherwise have occurred – and that's before factoring in the collisions that would have been prevented **had similar resources been spent on effective measures.**

Clearly, this nonsense must be stopped but the most difficult obstacle will surely be the human frailties understood by Tolstoy more than 100 years ago, and observed innumerable times by this analyst over more than 10 years:

"I know that most men - not only those considered clever, but even those who are very clever, and capable of understanding most difficult scientific, mathematical, or philosophic problem - can very seldom discern even the simplest and most obvious truth if it be such as to oblige them to admit the falsity of conclusions they have formed, perhaps with much difficulty - conclusions of which they are proud, which they have taught to others, and on which they have built their lives."

Leo Tolstoy quoted at https://enwikipediaorg/wiki/Confirmation_bias See also (App. A)

1. Objectives

Most critics of speed cameras realised long ago that no Minister, government department, police officer, camera partnership, statistician or road safety organisation **is prepared even to consider, let alone accept, increasingly compelling evidence that speed and red-light cameras cause more collisions and injuries than they prevent** (App. A)

This damning analysis of the effects of cameras is therefore **intended primarily for the public and the media in order to trigger so much public anger about 25 years of incompetent analysis, blatantly false claims of camera benefit, outright lies, cynical misrepresentation of the data and utter determination to avoid admitting error, that the authorities will have no alternative but to put an end to this expensive, immoral and disastrous policy.** And of course in order that the funds and other resources currently wasted on cameras may be diverted to effective safety measures on our roads or indeed elsewhere.

In order for this assessment to be fully effective it must not only explain how to accurately analyse the effects of cameras but also **the seriously flawed methods and unforgiveable errors that led others to claim of camera benefit far greater than could ever have been achieved.**

For that reason, because no policy as flawed as this should be immune to criticism and because those who have now been proved so badly wrong have only themselves to blame, no apology is needed or offered for the plain words that follow:

*** See for example www.fightbackwithfacts.com/cameras-versus-activated-signs/**

2. Words, phrases, jargon and acronyms

Cameras	read as **fixed or mobile** speed cameras and/or **red-light** cameras
Cognitive dissonance*	**mental conflict between existing opinions and contradictory evidence**
Collisions	read as **collisions and/or injuries** unless context determines otherwise
Confidence intervals	**percentage figures** specifying margins of error in the results
Confirmation bias*	**preferring information that confirms existing opinions**
Confounding factors	are factors which affect collision numbers but are unrelated **to cameras**
Group think*	**preferring consensus to deba**te and being overconfident in the group's abilities and dismissive of others'
Installation	read as **camera installation**
Partnership	read as **camera partnership**
Poisson distribution	statistical term for the **normal distribution curve** of near-random data
Sites	read as **camera sites**
Stats19 data	**road collision data** collected by the police and published by the DfT
Synchronous detection	a method of separating wanted from unwanted data, based on timing

* For greater detail on these three increasingly serious problems see App. A and Wikipedia.

Acronyms

FC	Fatal Collisions
K	Killed
FSC	Fatal and Serious Collisions
KSI	Killed and Seriously Injured
SLC	Slight Injury Collisions
SI	Site selection bias due installed where collision numbers were recently high
RTM	Regression to Mean i.e. return to normal following biased site selection
DfT	Department for Transport
TfL	Transport for London

Duplication

Some text appears in more than one section in order that each be complete on its own.

Data Availability

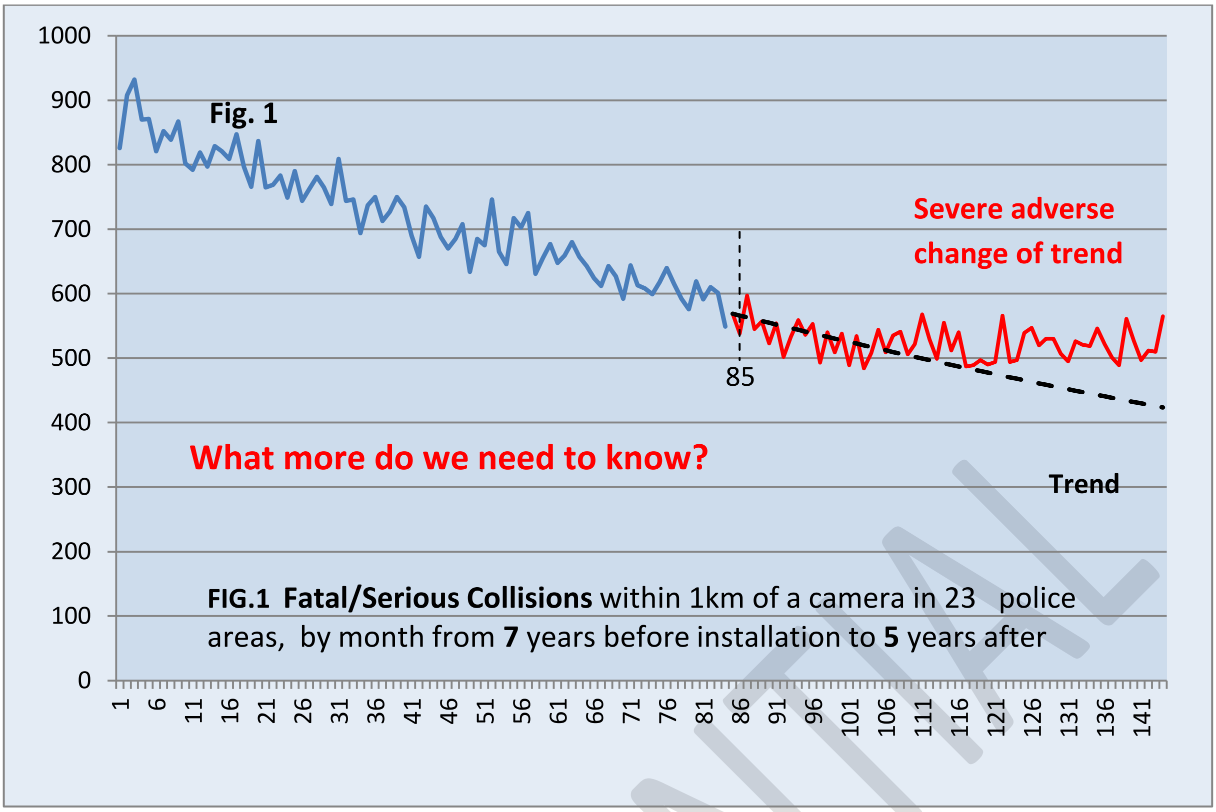
All raw data and results are available in Excel format via idris.francis@btinternet.com or on-line at http://www.fightbackwithfacts.com/the-speed-camera-delusion/ allowing **independent validation of data, methods and results**. Many more similar graphs may be drawn quickly and easily from that data.

3. Executive Summary

Other analysts use relatively **complex statistical methods to analyse low volumes of unreliable camera partnership data for narrowly-defined sites**, accepting from the outset that their estimates of camera effects will be subject to wide margins of error. The methods and results described here demonstrate that **there was never any need to use such methods or to accept such wide margins of error. *Group Think, Confirmation Bias*** and similar problems help explain why so many supposed experts **were so badly wrong and made the same mistakes.** (App. A)

Using only simple arithmetic, this analysis avoids all those problems by analysing much more and better data and by assessing camera effects **only after installation** when selection bias and regression to mean have, by definition, ended. **Yes, accurate analysis really can be this simple:**

- **Obtain a great deal of Stats19 collision, camera location and installation data.**
- Record the **distance between each collision and the nearest camera,** if within 1km.
- Record the number of **months between collision and installation,** if > -85 and < 61
- **Sum collisions within 1km** according to those time intervals and draw graphs of the totals as in Fig.2



- Summing data relative to installation **averages out the underlying trend.** (App. G)
- It also **averages out confounding effects,** inherently not positioned in time relative to installation) so **they cannot significantly affect the blue or red parts of the graph. (App. G)**
- **Site selection bias and Regression to Mean,** by definition ending before installation, **cannot affect the post-installation red graph.** (App. G)
- **Camera effects are not averaged out, so only they** can affect the **red** part of the graph. (App. G)

This and every other graph of this kind, for fixed, mobile and red-light cameras confirms:

(a) **no reductions in collisions** following installation

(b) **significant adverse changes of trend** as drivers become used to the cameras.

Photo by this analyst

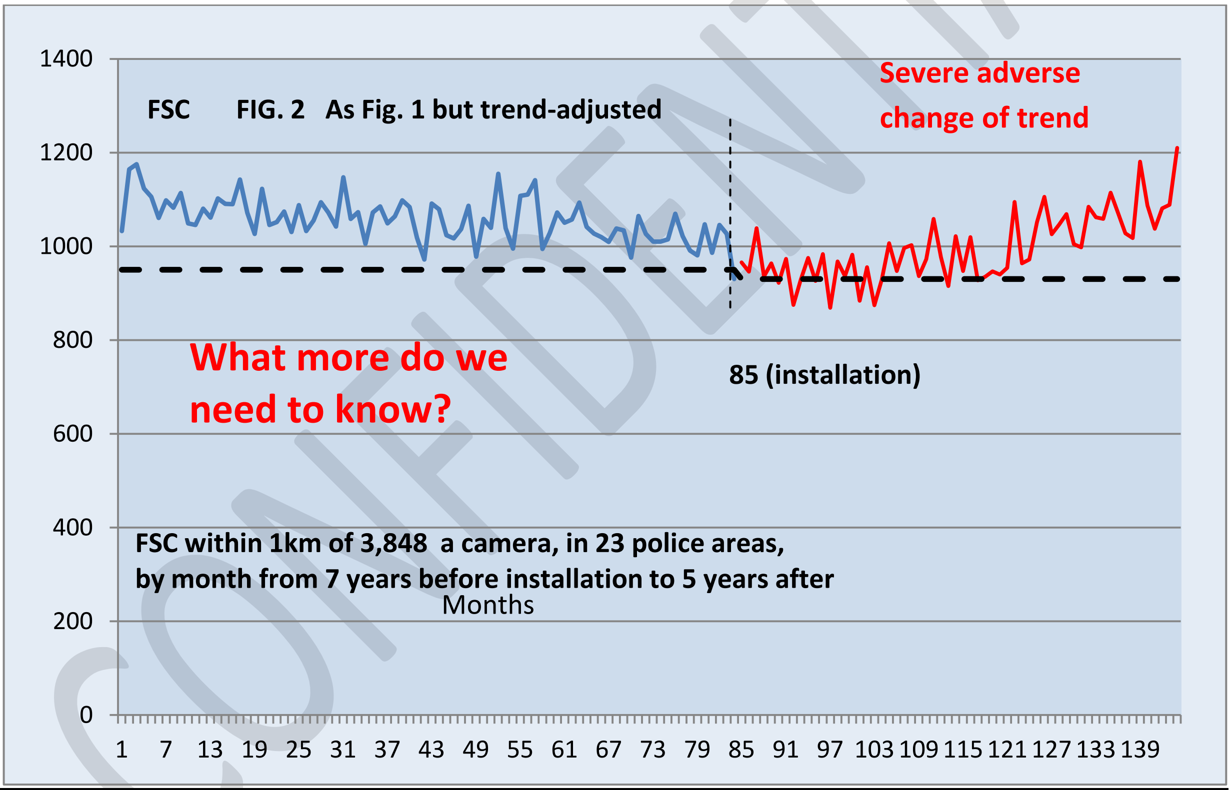
4. <u>Analysis Overview and trend-adjusted graph of camera effects</u>

Using **far better methods and much more data than others,** this analysis establishes beyond rational dispute that speed and other cameras **cause more collisions than they prevent** (if indeed they prevent any.*)

Almost all other analyses use the limited volume of flawed and often incomplete data provided by partnerships. One of many problems with that data is that the **official boundaries of most sites are narrowly defined on the (clearly false) assumption that the effects of cameras are somehow restricted to 500m either side of the camera and only on that road.** (App. E)

Because **40 or so adverse effects of cameras,** many of which clearly extend beyond such narrow boundaries were identified in 2007** this assessment **covers all collisions within 1km of each camera, to include the wider-area effects that others ignore**. All the graphs provided here confirm both that **the wider-area effects do exist and that they are significantly adverse**.

Fig. 2 is just one of many similar graphs showing not benefit but significant adverse effects:



<u>All such graph for different combinations of police area, camera type and site radius show:</u>

- **no sensibly identifiable collision reductions soon after installation when they should occur.**
- **significant adverse effects that can only be due to the cameras,** mainly in areas near to but outside official boundaries and mainly beyond others' usual 3-year monitoring periods.

Although only one graph for 1km radius sites is shown here, graphs for **250m and 500m radius** may be added to indicate how camera effects vary with distance.

* **Eric Bridgstock** www.dorsetspeed.org.uk/eric/the%20Bridgstock%20Theory%20v1%20Poole.doc

** **Paul Smith of** www.safespeed.org.uk

7. <u>In more detail</u>

After identifying the problems involved in analysing the effects of cameras on nearby collision rates and how others fail to resolve them (App. C) this analysis uses **only simple arithmetic to analyse Stats19 and camera location data to arrive at reliable and damning results which demonstrate that:**

- **there was never any need for other analysts' complex mathematics**, questionable assumptions, subjective estimates, selective analysis, probability theories, computer models or the wide confidence intervals to which they lead.
- other analysts used their seriously flawed methods and partnership data **only because <u>they failed to realise</u> that far more, and better, collision data was available from Stats19 records** and that (regardless of the quality of their other data) partnerships **must at least know where and when their cameras operate.**
- they also **failed to realise that effective cameras would cause discontinuities in the graphs from the month of installation**, as the proportion of drivers aware of the cameras rises, initially rapidly and then asymptotically towards a maximum of perhaps 90% in the same way that the effects of cameras would gradually approach maximum.
- they also **failed to realise that *synchronous detection*** (familiar to generations of electronic engineers, if not to statisticians) **allows the effects of cameras to be easily and accurately differentiated from all other *confounding effects.*** (App. G)
- they also **failed to realise that identifying and quantifying the discontinuity**, if it exists, makes it unnecessary to compare changes in collision rates at sites with those elsewhere, risking the introduction of further errors due to varying trends.

Because this analysis is simple, logical and transparent, no prior knowledge of statistics is necessary to understand it - except where it explains others' unnecessarily complex methods and errors. Appendices A to P provide clarification of the details. **The analysis therefore differs from all others in that it:**

- **Uses Stats19 data in preference to, and in much greater volume than, partnership data.**
- **Uses monthly collision totals for more accurate monitoring of deviations from trend,** especially in the significant months following installation.
- **Provides results in month-by-month graphical form** rather than as **simplistic and unhelpful % reduction numbers** that indicate nothing about how camera effects change over time.
- **Uses circular site boundaries up to 1km radius** to include collisions near to, but outside the narrowly-defined official boundaries.
- **Provides graphs of collisions within 250m, 500m and 1,000m of cameras and for 5 years after installation, for fixed, mobile and red-light cameras.**
- **Eliminates SSB and RTM effects by assessing only post-installation data.**
- Sums collisions **relative to installation month** instead of to calendar year to achieve **greater accuracy**, **average out long-term trends**, **reduce the effects of non-camera-related *confounding*) factors by more than 90% (i.e. to insignificance)** and ensure that only camera effects can cause post-installation deviations from the trend of those sites. (App. G)

6. Preposterous claims of camera benefit

Camera use since 2001 has been supported by absurd claims of FSC/KSI reductions such as:

Table 1

2002 Eight Area Trial fixed cameras.........**65**%	2013 RAC Foundation (June)....................**27**%
2002 Eight Area Trial mobile cameras.....**29**%	2013 RAC Foundation (Nov)......................**22**%
2004 Third National Report.......................**40**%	2014 Dublin University MSc Thesis..........**65**%
2005 Fourth National Report.....................**42**%	2014 Data Unit Wales Report **17**%
2008 Norfolk Report..................................**44**%	2014 Transport for London.......................**58**%
2010 RAC Foundation **27**%	2017 SERC section of LSE...............up to **90**%

These claims are in stark contrast to Stats19 causation analysis from 2005 to date:

Table 2: Contributory factors: Accidents[1] by severity: GB 2005

Percentage of accidents[1,2]

Contributory factor reported in accident	Fatal	Serious	Slight	All accidents
Injudicious Action	32	27	28	28
Disobeyed traffic signal	1	1	2	2
Disobeyed Give Way or Stop sign	2	3	4	4
Disobeyed double white lines	1	1	0	0
Disobeyed pedestrian crossing	1	1	0	1
Illegal turn/direction	1	1	1	1
Exceeding speed limit **Note – FSC about 8%**	12	7	4	5
Going too fast for conditions	17	13	11	12
Following too close	1	3	8	7
Vehicle travelling along pavement	0	0	0	0
Cyclist entering road from pavement	1	1	1	1

Important Note

Stats20 instructions for completing Stats19 forms show that these numbers include "***possible***" as well as "***very likely***" contributions and **collisions in which speeding was not the main or *precipitating* factor**.

Accordingly, even that low **8% figure is overstated** and should arguably be more like **5%**.

So which analysis' claim is right? Not one – all are wildly wrong!

As critics have long pointed out, **the modest speed reductions achieved by cameras could never have brought about much larger reductions in collision rates than involved speeding** in the first place, even if they eliminated all speeding, which as a matter of record, they do not.

His scepticism reinforced by the above analysis and realising that the only possible explanation of such discrepancies was **that other analysts had failed accurately to account for the *confounding* factors** that affect collision rates, this analyst set out to identify and eliminate their errors.

Which, as we will see, turned out to be surprisingly easy?

7. Understanding the problems and avoiding others' errors

The basis of most flawed methods

In a letter to *Local Transport Today* (7/3/16) Professor Richard Allsop, author of camera analyses for the RAC Foundation, explained that he and other analysts use:

"….***probability theory"*** and "***an assumption of Poisson distribution of numbers of collisions"*** due to ***"the smallness of the numbers and limited extent of the data***……[the results]…..***are therefore <u>far from definitive</u>, and by no means rule out the possibility of the effect being much smaller in other partnership areas……..The data <u>do not enable rigorous elimination of the effect of RTM</u>***…"
[End quote, this analyst's underscoring]

However, this analyst's detailed study of SSB and RTM established some time ago that **no data available or likely to be <u>could ever "*enable rigorous elimination of the effects of RTM*</u>"!** (App. F)

It has now been widely, belatedly and clearly reluctantly acknowledged that **the early claims of camera effectiveness which led to the large increase in camera numbers were much exaggerated by, if not entirely the result of RTM effects being ignored or downplayed**. (App. F)

However, as this analysis demonstrates, **even the later claims** (Table 1) are still **wildly wrong,** in major part due to **utterly misplaced confidence that, by using various peculiar methods, analysts had succeeded in accurately quantifying and adjusting for RTM.** <u>**Nonsense – it cannot be done**</u>! Not only must the wide variation between the claims of Table 1 be due in large part to the different Further, the different errors introduced by those different methods are the root cause of the wide variations in claims. **Any analyst who claims to have calculated SSB/RTM effects accurately and then accounted for them demonstrates only that he does not understand them!**

<u>Other problems</u>

Perhaps for reasons of space Professor Allsop's letter **did not explicitly identify other problems** that, if not already allowed for, would justify even wider confidence intervals:

- **<u>Calendar year</u>** collision totals and camera installation dates provided by partnerships (though they clearly must have monthly data) **make it impossible to determine which collisions in the installation year occurred <u>before</u> installation and which <u>after</u>**. This is particularly important **because any reductions achieved by effective cameras would logically occur in the first few months after installation.**
- **The SERC/LSE 2017** analysis (Table 1) implies, without giving details, that **collision numbers do not necessarily follow the assumed *Poisson distribution.***
- **Only collisions on the particular roads on which the cameras are located were analysed, thus ignoring adverse effects** which can and clearly do extend well beyond narrowly-defined site boundaries. (App. E)
- **Trend adjustment of their low volumes of data, by** comparing two small and hence volatile sets of numbers **is unreliable.** (App. H).
- **Delays** of a few days to a few years **between site selection and installation cause errors due to trends that differ** not only year-on-year and also area-by-area.

Professor Allsop' 2013 analyses for the RAC Foundation (Table. 1) **failed to find the correlation between the reductions in speed and collisions that we would expect to see** if as he claimed the former caused the latter. Engineers see the absence of the correlation as proof they do not.

All the above problems lead directly to errors in calculated camera effectiveness subsequently made worse by the wide margins of error being forgotten or deliberately ignored by commentators and vested interests, as those approximations morph into DfT mantras to be used to brush off criticism.

Synchronous detection/analysis

Other analyst failed to realise that as the **effects of each camera are inherently positioned in time relative to its installation date, the total effects of many cameras installed over many years may easily be differentiated from all other effects on the basis of timing alone. By applying this principle,** known to generations of electronic engineers as ***synchronous detection,*** this analysis **sums post-installation camera effects accurately, while averaging-out the effects of *confounding factors* so that they fall by more than 90% i.e. to trivial levels.** (App. G)

Statisticians estimate, engineers measure

Professor Allsop explained why statisticians resort to their standard methods when confronted by **low volumes of poor quality data**, knowing from the outset that their results will be subject to wide confidence intervals.

Such methods and results may be par **for the course for statisticians** in those circumstances but are **no use whatever to design engineers or others** who, lacking sufficient data, have **no alternative but to find more.** As a born contrarian and former engineer preferring ***lateral thinking*** to ***group think*** (App. A), this analyst realised that:

- more than enough accurate collision data **must already exist in Stats19 files.**
- using more data **reduces volatility and errors while using monthly totals shows much more clearly** the relatively rapid changes that would follow installation of effective cameras.
- Partnerships must surely know **where and when their cameras operate**.
- distances between each collision and its nearest camera may be **calculated from the ordnance survey grid references of both.**
- **Collisions in groups of distance may easily to be summed for the circular sites we** need in any case, to **capture the wider-area effects ignored by others.** (App. E)
- **the time** interval between each collision and its nearest camera **may be calculated easily from the dates of both and used to sum collisions correctly** i.e. relative to installation of cameras rather than by the calendar. (App. G)
- for the same reason, **summing collisions relative to installation averages out all other data <u>not</u> related to installation dates. This makes it impossible for such *confounding* effects to cause significant deviations in the graphs.** (App. G)
- timings of SSB and RTM effects, notionally tied to installation dates, vary significantly due to installation delays. Accordingly they **can affect pre-installation data** but these effects are irrelevant here as **only post-installation data are analysed.**

8. Obtaining accurate results from the data

As we have seen, other analysts use relatively complex statistical methods including probability theory to analyse insufficient and flawed data for narrowly defined sites and to claim collision reductions far beyond those that could ever be achieved - but which they appear to believe.

This analysis in contrast applies only simple arithmetic to much more and more accurate data to show month-by-month changes in collision rates near cameras, from 7 years before installation to 5 years after.

As SSB and RTM cannot affect collision rates after installation (App. G) and all non-camera effects are averaged-out to trivial levels, the test of camera effectiveness is now a simple one – are there **any relatively sudden deviations from the trend of those same sites?** Note in particular that this test does not rely on comparisons with trends where there are no cameras, and is therefore not subject to the errors that arise. (App. H)

Understanding others' errors and how to avoid them, this method ensures accuracy by:

- **calculating the distance** from each collision to each camera (see note below)
- **if the distance if 1km or less and the time interval between collision and installation is from -84 to +59 months** (-7 to +5 years), **recording the police area code, distance apart, time interval, speed limit (optional, see note below), camera type and reference number.**
- **if more than 1 match is found, using only the nearest camera.**
- **summing qualifying data into groups** sharing the same **police area**, **camera type**, **distance apart** and (optionally) **speed limit**, and then entering each total into the appropriate **cell in a Row in an Excel spreadsheet** in the format shown in Table 4 and the data available on-line:

Table 4 Format of Excel sheets of trend-adjusted collision data

P F	Type	Radius	1	2	3	4	5	6	7	8	9	10	11	12	to 144
1	Fixed	000-250m	41	62	36	53	55	48	60	53	49	54	54	71	
1	Red-light	000-250m	57	67	78	74	89	59	54	47	83	64	64	57	
1	Fixed	250-500m	72	82	85	67	88	85	82	70	71	75	73	68	
1	Red-light	250-500m	60	71	81	80	74	71	64	70	55	62	67	53	
6	Fixed	000-250m	5	4	8	7	3	8		8	6	4	6	1	
6	Red-light	000-250m	4	5	12	3	3	4	8	6	8	7	2	9	
6	Fixed	250-500m	9	9	14	9	9	11	13	5	11	8	9	8	
6	Red-light	250-500m	9	6	6	10	10	5	7	10	10	7	9	7	
10	Fixed	000-250m	4	1	1	1	1	5	3	2	4	1	4	3	

etc.

Graphs of this data may be drawn quickly and easily by clearing unwanted data, summing what remains and then using the **Copy/Insert/Line/2D-line** commands.

The Excel sheets available on-line are pre-formatted to show month-by-month totals of all data that has not been cleared and also **3, 6 and 12-month averages** to reduce volatility to acceptable levels when the volume of data remaining after selection is low.

Maximise accuracy by:

- **using monthly totals not annual,** to increases overall accuracy but in particular to be able to identify any the relatively rapid changes that would follow installation of effective cameras
- **summing data relative to camera installation** so that the effects of cameras sum correctly and that the effects of all non-camera-related long-term trends are averaged-out into a smooth trend and all other non-camera-related factors such as seasonal, local and random variations are averaged out reduced by more than 90%. (App. G)
- **analysing only post-installation data,** by definition free of the effects of SSB and RTM.
- **assessing camera effects by monitoring deviations from the smoothed-out trend of those same sites, not by unreliable comparison with trends where there are no cameras.** (App. K)
- **including data up to 1,000m away from cameras** to include most of the wider-area adverse effects other analysts ignore
- **showing graphs of 0-250m and 0-500m data** to monitor how camera effects vary with distance from cameras.

Notes

- **As 85% of collisions at sites occur in 30mph areas, the volume of data for other speed limits is low and hence volatile.** In addition, many speed limits have been changed over the years. Accordingly **this analysis does not differentiate results by speed limit,** which has the benefit of reducing the number of groups/Rows by a factor of 5.
- **All the raw Stats19 data, including distances apart, time intervals** relative to nearby cameras, and **partnerships' camera locations and installation dates data** are available on request, preferably on DVD due to the large file sizes.
- **Calculating distances from nearly 4m collisions to nearly 4,000 cameras** using this analyst's preferred but old ***Silicon Office*** database software on the 1997 machine on which it runs took several weeks. However, the results have since been **replicated and validated** using modern software and computers which take only a matter of minutes.
- **Graphs are shown both in non-trend-adjusted form** ("normal" being a falling line) and in **trend-adjusted form** ("normal" being a horizontal reference line). Excel sheets holding both types of data are included.
- **Trend-adjusted data should be calculated to at least four decimal places before summing,** to minimise rounding errors. For (visual) convenience, Excel cells may be formatted to hide, but not ignore, the decimal places.
- More detailed information on using the data is provided in the Excel file.

9. Drawing graphs of the Excel data

- **Six Excel spreadsheets** in one file hold **FC, FSC and SLC data** in non-trend-adjusted (Fig. 1) and trend-adjusted (Fig. 2) formats. Another 3 sheets provide the **annual trends** of each severity of collision from 1989 to 2011.
- Graphs may be drawn from **any one of the above 6 sheets,** for any combination of police area, camera and site radius **by Sorting** on selected columns, **Clearing** selected **Rows** and **Summing** what columns remain.
- For convenience, data is provided for **0-250m, 251-500m** and **501-1,000m** radius sites, allowing graphs to be drawn for **0-250m, 251-500m, 0-500m, 501-1,000m and 0-1,000m**.
- **3-month, 6-month and 12-month averages** are automatically generated at the bottom of the sheets to **reduce volatility when totals are low due to selection.**
- Graphs are shown for each of **the 3 types of cameras to compare their effects. They show that while 3 types cause more collisions than they prevent, mobiles cameras are** (pro-rata) **the worst.**
- To draw graphs**, highlight the Totals Row** then **Select/Copy/Insert/Line/2D-line**.

<u>To include a graph of area trend</u>

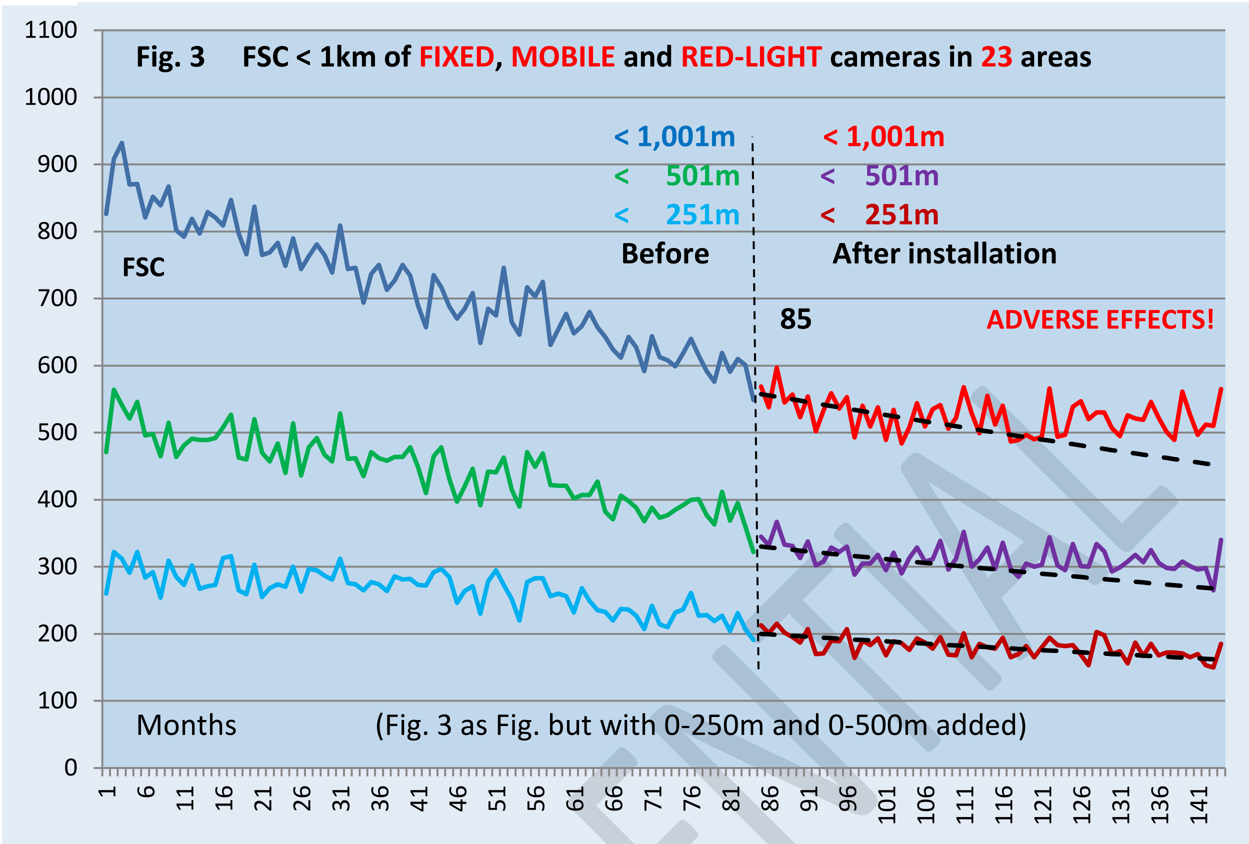
Estimate from the relevant Excel Trend sheet the average % per month fall for the same severity and combination of police areas:

- Enter beneath the **Totals Row** (as above) a **Row which starts at month 85 with the same value as the Totals Row** but falls by that constant percentage every month until month 144.
- **Fine-tune the start value (month 85) so that the trend graph matches the site data.**
- The reference trend line may be added to help identify any significant discrepancies but **should not be used to quantify camera effects,** more accurately done by observing any significant post-installation deviations from the smoothed-out trend of those sites.

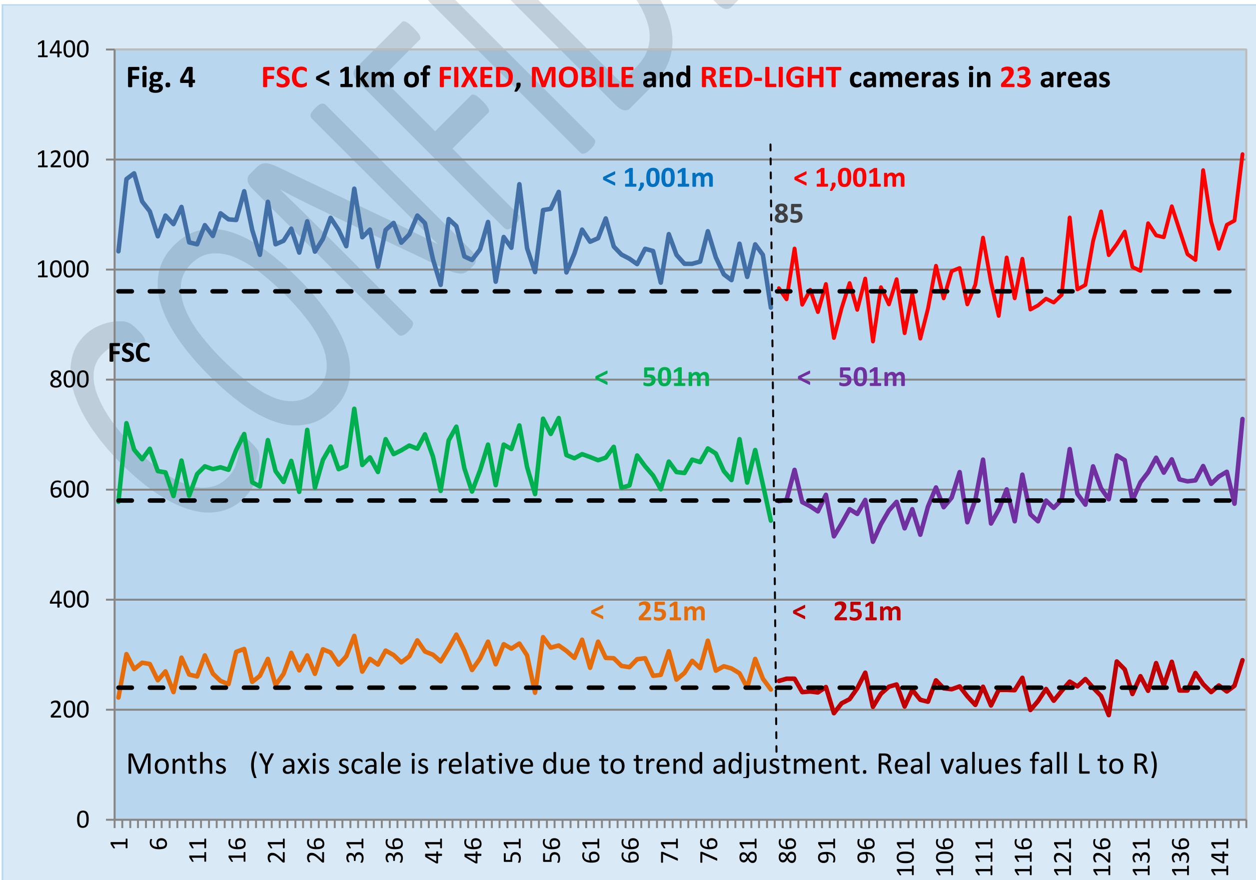
<u>Alternatively, trend-adjust the data so that normality is represented by a horizontal graph</u>

- **Data should be trend-adjusted at least to 4 decimal places** to minimise rounding errors, **before being summed according to position in time** relative to installation and being entered into the Excel sheet.
- **To show graphs for more than one radius on the same sheet,** generate each **Row of totals** in turn and enter them into successive blank Rows, using ***Paste Special / Values,*** then select all relevant rows, including trend graphs where appropriate, and draw the graphs as before.
- More detailed instructions are provided in the Excel data file.

10. The two most important graphs of the effects cameras



Trend-adjusted version



11. Comments on the graphs of results

Sixteen such three-line graphs are shown in Appendix P for these combinations of parameters:

- Trend-adjusted or not
- Fatal and Serious Collisions (FSC) and Slight Injury Collisions (SLC)
- Fixed, mobile and Red-light cameras, plus all 3 combined.

Each plot shows graphs for **0-250, 0-500m and 0-1,000m** radius from cameras, but the red-light results also show **0-50m** because they are aimed at reducing collisions at or near traffic lights. Optionally, graphs could also be drawn for 251-500m, 251-1,000m and 501-1,000m to see more clearly what happens further away from the cameras.

While it is useful to compare the results of the 3 types of cameras – **there is no particular point in differentiating between police areas because:**

- there is no logical reason for camera effectiveness to be significantly different in one police area from another
- any apparent differences might well be due to chance and therefore different a few years later
- **it is the overall results that matter**.
- **it is clear that there is no possibility whatever of these types of cameras being effective, let alone cost-effective.**
- f**or all these reasons, it is highly unlikely that other types,** such as average-speed cameras, would be effective or cost effective.

Every one of these graphs confirm the adverse effects of cameras

Every one of the 16 above graphs, trend-adjusted and not, FSC and SLC, fixed, mobile and red-light and all 3 combined, confirms that:

- **The relatively rapid falls** in collision numbers that would occur over the first few months to a year or so after installation of effective cameras **do not occur.**
- Within **250m** of the cameras there is **no sensibly quantifiable effect**, except in some cases a slight increase towards the end of the 5 year "after" period.
- Within **500m the adverse effects are more significant.**
- Within **1,000m the adverse effects are severe, especially for mobile cameras.**

That cameras provide no net benefit is more than enough reason for scrapping them, especially as the **£200/300m pa wasted on them** could be used to improve road safety using effective methods. **The significant increases in collisions beyond most official site boundaries can only be due to the presence of the cameras** (App. G) and can only be due to the adverse effects on driver behaviour continuing well beyond those boundaries. **However, it is not essential to <u>prove</u> that cameras increase collisions, failing to reduce them is good enough reason to scrap them.**

<u>As die-hard camera enthusiasts are likely to dispute results based on circular site areas:</u>

The simple answer to any such objection is that **it does not matter in the slightest if significant reductions <u>are</u> achieved within narrowly-defined official boundaries <u>if at the same time increases beyond those boundaries</u>,** that can be only due to the same cameras, **more than negate those reductions.** (App. E)

As these graphs used very large volumes of the best available data, and all the confounding factors that defeated other analysts are eliminated or reduced to trivial levels, these results really are beyond rational dispute. Which, as before, is not to say that there will be no <u>irrational</u> disputes. (App. A)

12. The simple long-term method and TfL's bogus "58%" claims

Fig. 4 (App. B) shows how **SSB, RTM**, varying timings and trend make it impossible to analyse what happens in the middle years of that theoretical graph or indeed of Fig. 1 below, the equivalent **KSI graph of Transport for London's official site data.** The higher totals in the middle years show that **SSB at TfL sites was higher** than the 23.3% assumed for the example.

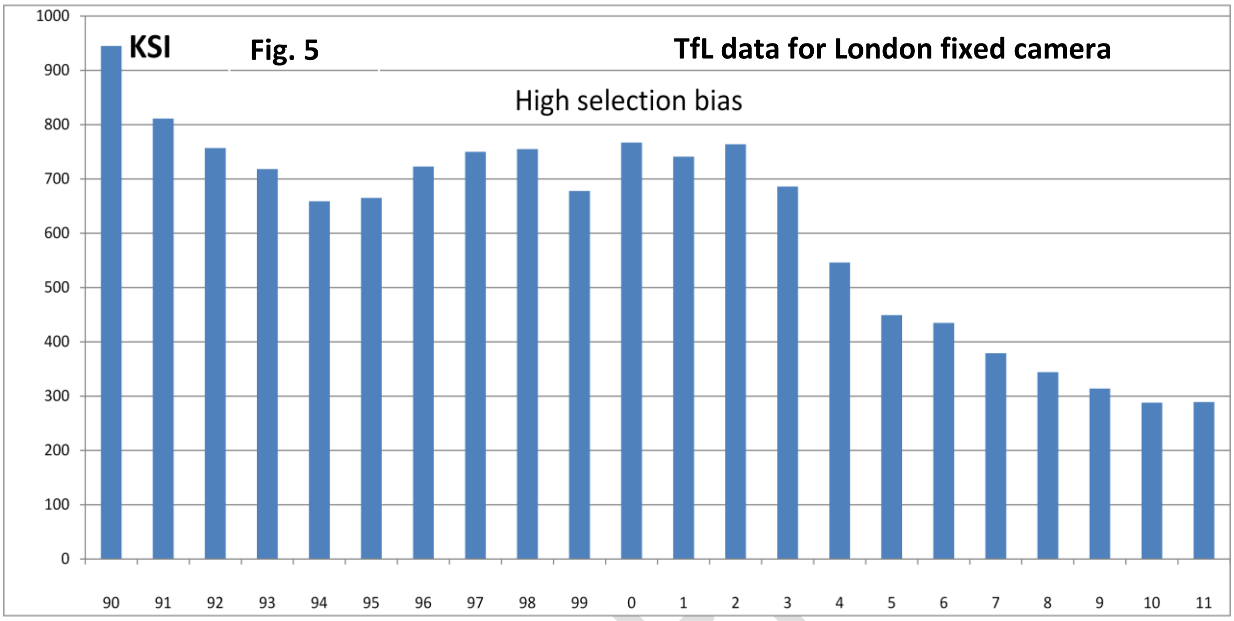


Fig. 5 Transport for London KSI data, fixed camera sites installed 1994-2008

It clear however that there was a substantial **reduction in KSI from 1990** (when SSB had little effect and RTM had none) **to 2011** (when neither had any effect).

But what matters is of course is how that fall compares to the fall where there were no cameras:

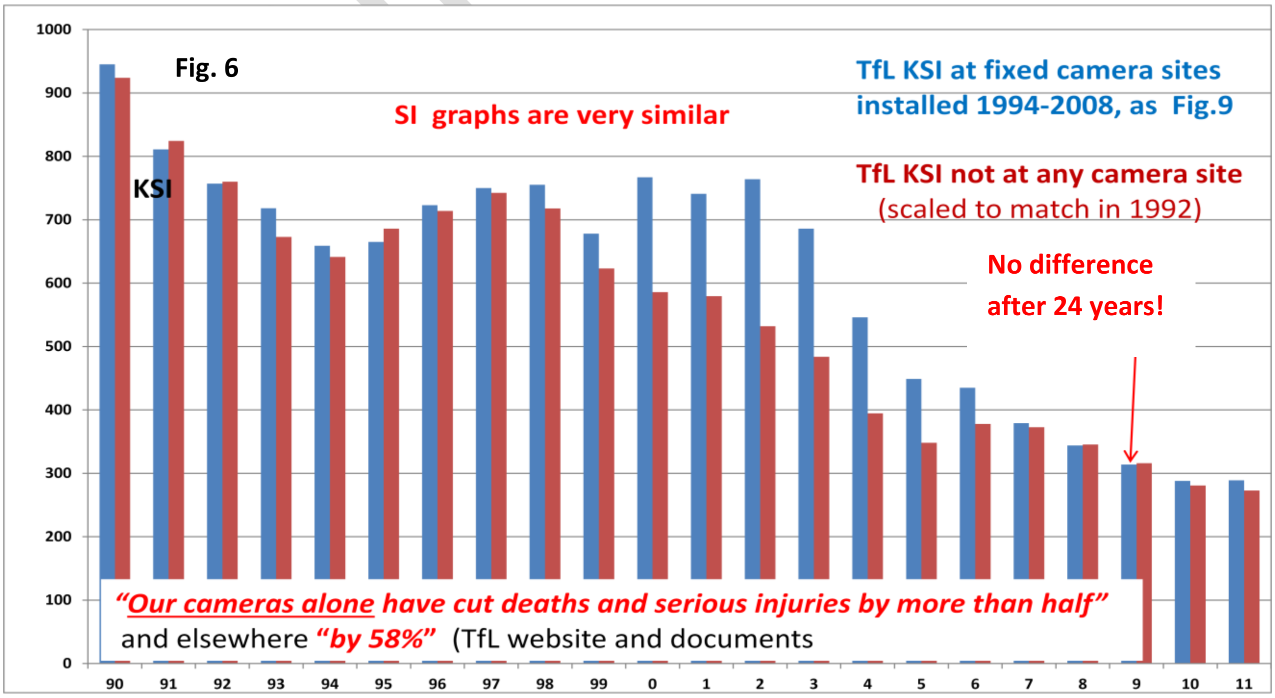


Fig. 6 KSI Comparison at London camera sites and elsewhere

Fortunately, that comparison is easy to do (Fig. 6) and it is clear that there was **no sensibly identifiable difference between falls with or without cameras – i.e. <u>the cameras had no effect.</u>**

TfL has been aware of these figures since July 2014!

Within 24 hours of receiving their data in July 2014 this analyst **repeatedly e-mailed Transport for London to point out that their own data showed that their cameras had, at best, no effect on collision or injury rates**. His several offers to visit TfL to discuss the analysis were rejected **as were multiple written complaints** to the Managing Director, Leon Daniels and the Surface Transport Panel.

From September 2014 to mid 2015 an Emeritus Professor of Statistics long experienced in camera analysis **visited TfL four times to present his assessment showing net adverse effects.** He left each time under the impression that his figures had been agreed **but nothing changed except that a few weeks after the first visit TfL stated that installation of 600 more cameras was under way! (App. A and K)**

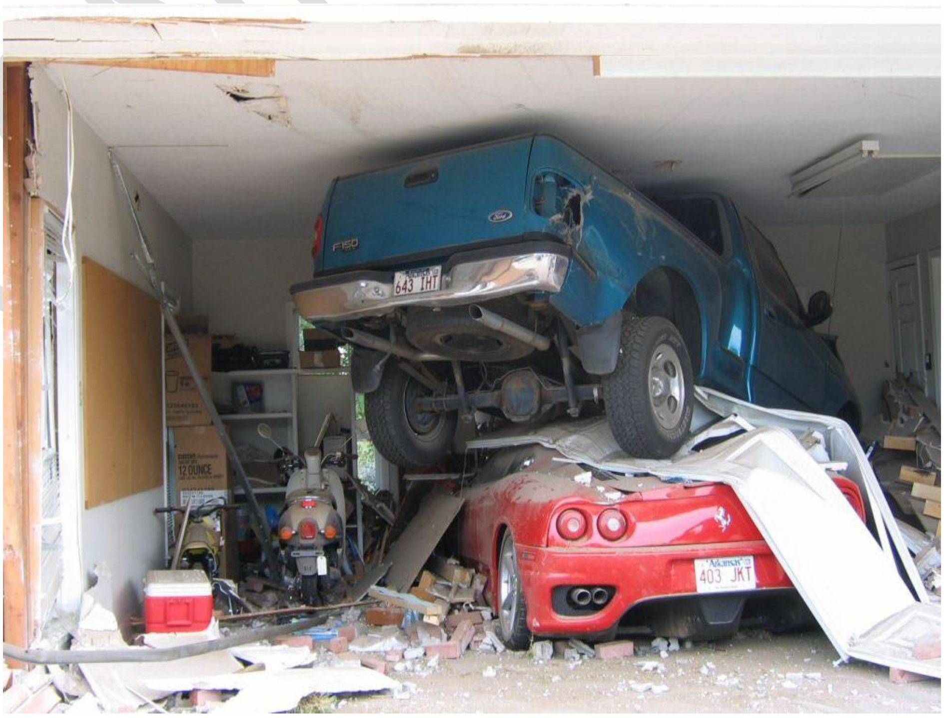
All the correspondence and media comment are at www.fightbackwithfacts.com/tfls-bogus-claim/

Despite their accuracy limitations these graphs can still be useful

Although the limitations of these long-term comparisons make it impossible for the results to be as accurate as those of the synchronous detection method, they can nevertheless provide indicative results quite quickly for police areas for the 19 of the 43 police areas which have failed to provide camera site data.

The simple test it provides is whether falls in collision numbers are any greater at camera sites than elsewhere. None are.

See App. J for more detail.



13. <u>Conclusions</u>

Given that claims of camera benefit over the last 16 years were literally incredible it is **not at all surprising to find no camera benefit whatever.** Nor should we be surprised that, as critics predicted years ago, **adverse effects on nearby roads more than overcome any marginal benefit at narrowly-defined official sites.**

In any case, any such marginal benefit that might appear in any particular graphs **is no more than could be accounted for by a small proportion of drivers diverting to avoid cameras, taking their share of collisions with them.**

Graphs may be drawn for different combinations of police areas but **there is no logical reason, other than chance or different mixes of roads and traffic** for any significant variations in apparent effectiveness from one area to another.

Separate graphs are provided for each of the 3 types of camera to compare their effectiveness. All three are clearly worse than useless, mobile cameras are the worst of all.

It is important to recognise that the results provided here <u>are derived by applying only simple arithmetic to large volume of the best data available</u> <u>and do not rely theories, assumptions or complex mathematics</u>. The analysis is also fully transparent as full details of the methods used and all the raw data are available on-line for others to validate if they wish.

There is not the remotest possibility of cameras being cost effective, even on the basis of the Department for Transport's preposterous collision valuations (App. M) let alone on the very much smaller real figures.

In view not just of the £3bn or so wasted to date but also of the increases in collisions and injuries caused directly and indirectly by cameras, it is impossible to justify the continued use of these cameras. Indeed, continuing to use them once aware of this compelling and damning evidence should amount to Breach of Duty of Care, Misfeasance in a Public Office and arguably, given how many have already died as a result of these cameras, Corporate Manslaughter.

Enough is more than enough – stop this madness now.

Idris Francis
April 2018
END