Submission No 571

MOBILE SPEED CAMERA ENFORCEMENT PROGRAMS IN NSW

Name: Name suppressed

Date Received: 9 July 2021

Ideological bias in research makes appraising NSW's mobile speed camera regime impossible – let alone making honest decisions on it

Plus ça change, plus c'est la même chose: *Introduction*

The problem with examining New South Wales' mobile speed camera program is that it is very difficult due to the political and academic bias that surrounds it. This influences the interpretation of the often sparse, poor quality and difficult to elucidate data. These resultant biased interpretations are then fed into a loop between government and academia. The lack of the diversity of thought in academia and the low availability of the detailed data to the general public means that finding dissenting views is very difficult, and as sceptics of the 'speed limits, speed cameras' ideology tend not to be paid to do their research, it is very difficult to mount any kind of opposition to those who are paid to do so and whose ideology says that mobile speed cameras are very effective.

Over the past year the NSW Government has taken two steps without consultation, debate, or an independent appraisal of the evidence – removing the warning signs from mobile speed cameras, and, as this inquiry launched, tripling the deployment hours. These decisions have been made at the behest of a well-heeled academic elite in a financial cycle with the government.

I have no doubt that the Monash University Accident Research Centre (MUARC) will, in this inquiry produce a much better made, better researched document. This does not stop it from suffering from the issues I am about to describe here. These issues, in fact, run to the heart of road safety research and are why it is so hard to have a proper debate around the various issues. Anyone reading research by MUARC, CARRS-Q or any other academic research institute (or more accurately, think tank) would do well to read them with these points in mind.

The root of the problem: *Myside Bias and Ideology*

'Myside Bias' is a form of cognitive bias. As described by the American psychologist Keith Stanovich, it is the tendency of people to interpret facts in a way that fits their prior beliefs. This bias is unusual in that it is not affected by intelligence. While many other cognitive biases are reduced as measures of cognitive intelligence increase, myside bias does not. Hence academics are especially prone to it, is they often believe they are, and genuinely are, less biased - in all other biases except the myside bias. Hence, they are even less likely to question whether their conclusions are simply reflecting their prior beliefs due to their faith in their own expertise.
The kind of research that has justified the massive expansion of mobile speed camera usage in NSW is ultimately based in this bias. Road safety academics approach roads first and foremost by thinking about risk of death rather than transport. A telling example of this was a recent letter by the president of the Australian College of Road Safety, who

described New Zealand's 100 km/h roads as 'killing fields'. Aside from being grossly insulting to those who have experienced genuine war and genocide, it betrays an attitude widespread in the road safety community. Road safety academics are not neutral researchers who look at the facts dispassionately. They are predominantly very biased activists who imbue their own research with an extremist view where preventing the possibility of death is the only important factor, despite how unachievable it is and how many much more common ways there are to die. It is all and well being ambitious, but 'Vision Zero' has a religious extremism in its strongest adherents.

Behind the Numbers: Data Interpretation and Scientific Methodology

It is a knee-jerk reaction on social media whenever anyone questions a scientific assertion to say 'look at the data'. The problem is that data are abstracted, often largely shorn of their context in the phenomena they are supposed to describe. Data *always* require interpretation. It is this stage where the biases creep in, especially when the data are ambiguous, noisy or too small in sample size.

Some examples of both confounding issues and slanted data interpretation can be seen in NSW's evaluations. For example, the period of comparison used is always the five years before the camera is installed, and then the most recent five years – even if there is a considerable gap in between. This is a form of cherry picking where other phenomena may be responsible for changes in the meantime, and the differing choice of time period may see completely different results.

Furthermore, the fact that cameras are installed at sites considered to have an unusually high crash risk means they can suffer from reversion to the mean. This is the natural retreat from extremes (for example, a high number of crashes) to more moderate values that is common in non-uniformly distributed data (as crashes have a single-tailed distribution the reversion will almost always be down).
Secular trends are another confounding factor. The standard method of measuring camera effectiveness is through summing the number of crashes, or comparing average rates over time, for the before and after period. If there is a large-scale secular trend, as has generally been the case in road safety due to improving vehicles (and infrastructure to a lesser extent), then the 'after' period will always be lower than the before period, regardless of the real effect of the cameras. This is a bias that will actually grow larger with the dataset as more cameras of marginal effectiveness are included.
This is why it is important to compare changes in trends and account for the mean reversion of any random spikes – rather than simple sums. If there is no change in the trend then the camera may have had no effect, even if the after period has fewer crashes. One necessity to reduce this error is to use a longer period than the standard five years, which is too short to determine underlying trends with any certainty. When reports justify targeting a road with speed cameras, speed limit reductions or both as crashes increased over the past five years this may simply be random variation imposing itself on a general flat or downwards trend. This explains why the high reported effectiveness of these measures has not carried over into overall

improvements in road safety in Australia or Europe. Camera use increased massively over the 2010s, yet safety stagnated.

Another good example of slanted data interpretation is the paper recently published in the Journal of the Australia College of Road Safety by Max Cameron and Stewart Newstead, *Increasing the effectiveness of mobile speed cameras on rural roads in Victoria based on crash reductions from operations in Queensland* (2021). This purports to show a 41% decrease in fatal crash reductions from mobile speed camera operations. The problem is that camera deployments are non-uniform, in this case specifically being placed in known recent crash hotspots. This means that mean reversion already automatically overstates the effectiveness of the cameras. There are no control sites included which means an examination of secular trends in the area is impossible, and the data presented are not sufficiently granular to discern whether crashes were speed related – if they were mainly not, then other factors, not the camera, were responsible. These kinds of studies are akin to observational studies in medicine. Although common, these studies are considered one of the weakest methodological types as all confounding factors have to be pinned down to draw a robust conclusion. This paper makes no attempt to do so and does not even acknowledge them. This is very common with speed camera and speed limit studies in general, confounding factors and control groups are often swept under the carpet. Not even traffic volumes are measured.
The gold standard of medical study type is the double-blind, randomized trial. Difficulties in applying this kind of study aside, conducting this kind of study would be impossible as the road safety community baulks at anything that deviates from its prior ideology, so testing a road with higher speed limits and no speed cameras is moral anathema. Yet these kinds of studies are conducted regularly on medical phenomena which cause far more deaths and have a far higher societal cost than road crashes.
The Cameron and Newstead study is almost certainly cited in MUARC's submission and was primarily responsible for the recent policy changes in NSW and increases in concealed mobile camera enforcement in Victoria. Yet it is too methodologically weak to prove what it asserts to show and it and the models derived from it are nearly valueless due to this.

Examples of these issues are shown at the end of this report.

A side note on this issue is also how the data are used. For example, in the 2019 NSW Speed Camera evaluation 'retain' is the default with 'retain and monitor' used where crashes have increased. This leads to an automatic bias where there is no metric for removing an ineffective camera. Furthermore, it is consistently noted that trends where crashes have increased are disclaimed as not statistically significant, yet there is no equivalent note where the decreases are also not significant (which most are not). Therefore, increases are framed as being less relevant, important and robust than decreases even though there is no genuine reason to do so. This is a clear example of bias where data in the ideologically correct direction is treated differently to data that are not. If they were to be treated fairly, then increases similar in magnitude to decreases should see a recommendation for a camera being removed, as should no trend (as in a

liberal society controls should not be imposed unless needed). Instead the opposite is done.

This also extends to framing – the removal of warning signs from mobile speed cameras was passed in a bill spurred by the death of a family from a drunk driver who mounted the footpath. It is not even clear if he was speeding, and removing signs from mobile speed cameras would not have prevented the accidents. Yet this tragedy was used as a framing device to pass a coercive measure that the community did not want.

A Bug in the Bear: *Models, Assumptions and Bias*

A well-known saying about models (computer or otherwise) is “garbage in, garbage out”, referring to the fact that when questionable data is used in a model, the results are just as questionable. But there is another problem, which is the assumptions used in actual constructing the model. When a model is made with biased assumptions, the results will be biased. The ability to tune the parameters of a model to get the results you want was expressed by Enrico Fermi, “with four parameters I can fit an elephant, with five I can wiggle his trunk”. In this case the users of a model have an ideological bias towards mobile speed cameras being effective. Observational data, as previously explained, is interpreted to fit their beliefs. These interpretations are then used to construct models which are biased towards showing speed enforcement, lower speed limits or any similar ideological vehicle as being effective.
Therefore, it is small wonder that MUARC and the like will always show such measures as being effective and will recommend them. Because their entire research process is dedicated not to a neutral examination of the facts, but specifically to proving that low limits and cameras are the best road safety strategy in line with their ideological precepts. This is no different to cigarette companies producing studies that show there is little risk from smoking. In both cases ideology drives the science, not the other way around.

Rush ‘em in: *Mobile Average Speed Cameras*

A perfect example of the ideology getting ahead of the data is the constant advocacy by MUARC for mobile average speed cameras. I have no doubt that their submission to this inquiry includes a strong plug for them. They claim that they will reduce accidents by about 30% or so, apparently no matter where they are deployed. The problem with this assertion is that it is fantasy.
Why is it fantasy? Because they haven’t been deployed anywhere, so there are no data on their effectiveness. These are instead based through a conflation of data on fixed average and mobile single-point cameras, all of which is subject to the problems described earlier, and hence only somewhat useful as evaluation sources – mainly if you are ideologically inclined that way.

There is no country in Europe that uses these cameras. MUARC also advocates for them to be concealed. This is two bluffs in one. No Western country is as reliant on

concealed mobile enforcement as Australia. In the Netherlands, mobile cameras are mainly deployed to problem areas (thus suffering from reversion-to-mean overstating their effectiveness) for a limited time and are usually fairly well signposted. Permanent average speed cameras are often used on rural roads in the United Kingdom, but are always very clearly signposted. These are often employed on popular motorcycling roads where speed limits have already been reduced, and have had a devastating impact on the recreational motorcycling community. Yet despite widely reported improvements in speed compliance and great reductions in overall motorcycle traffic on these roads the UK's overall fatality and injury record has stagnated since 2014-15, the years that these establishments became common place. The big picture is sometimes the most important – and it shows they are less effective at stopping people being killed than is commonly supposed from myopic research. There is no reason to think this would be any different in Australia.

Keeping out the heretics: *The Information and Knowledge Cartel*

Road safety research exists in what might be described as an information cartel. Despite the importance of the topic the vital accident and traffic data are only available to credentialed researchers or occasionally though expensive freedom of information requests, which are priced far out of proportion (more akin to when the data was kept on paper), the meaning that the data are effectively kept from scrutiny by the general public. While progress has been made recently, for example with the speed camera evaluations being made public, this can only reveal so much (a majority of accidents have causes other than speed) and these data are not granular enough. Furthermore, they are presented in a stylized format that is designed to convey a message.

The most granular data is generally used to produce studies which is usually published in paywalled journals which prevents critical examination by people outside the academic circle. Since the academic road safety community is heavily biased towards speed cameras, this means that research proclaiming their effectiveness will go unscrutinised. And since the academics are considered experts, these conclusions will be accepted uncritically by an already receptive government even though they may be (and in this case are) heavily biased.
The most insulting thing about this knowledge cartel is that it is almost entirely taxpayer funded. The general public is excluded from examining what they have paid for. There are a large number of people with scientific, technical or mathematical backgrounds or knowledge who could easily scrutinise the methodology or statistical methods of road safety research. This is because these things can be separated from the field-specific knowledge (numbers in mathematical expressions stand abstracted on their own and are not teleologically determined by the phenomena to which they relate, and methodology is about how the study is conducted, not what the study contains).

The financial incentive of speed cameras further complicates matters. Road safety research is funded almost entirely by the Government, who in turn receives money from certain measures proposed. Thus, speed cameras exist in a self-perpetuating finical

cycle, where they effectively help fund the research that writes the policy for them. As it currently stands it is impossible to get a dispassionate and unbiased view of their effectiveness as their use begets the research used to justify them and so on.

This is further worsened by the difference in information exchange between the government and public compared to the government and academics. As ‘experts’ the academics are able to present to high level audiences within the public service and have they own views communicated comprehensively to the minister and their office. In contrast even recognised advocacy groups struggle to get ministerial audiences, and individuals get copy-paste replies from mid-level public servants in the relevant department, probably without the minister or policy makers seeing the replies, let alone having them considered. This leads to a previously skewed situation where a few activist academics can effectively write policy and have it implemented without debate (this also means that party differences have largely disappeared, as seen with South Australia’s recent draconian reforms)
The removal of warning signs from mobile speed cameras and the expansion of their operation are perfect examples of this. Both were announced out of the blue without a debate and both had the clear and explicit backing of a well-known speed extremist research group, MUARC. This is not how representative democracy is supposed to work. It is more reminiscent of an oligarchy, even a dictatorship, where a very small number rule over the majority without their consent. A complete governance overhaul is needed.

Honesty and Openness: *The Need for Change*

1: All research used in road safety policy should be published open access with full raw data, modelling code and anything else required to replicate the paper available. If researchers are not willing to conform to this then the research should be excluded from consideration.

2: All collected crash data such as times, causes, location, vehicle type should be freely available, perhaps from an online portal. This should be anonymised to prevent negative coverage beyond that which normally occurs, but otherwise should be as accessible to anyone as to credentialed researchers.

3: Examinations of speed camera effectiveness must account for all confounding factors and potential alternative explanations for data trends. Data must never be presented in isolation but always shown in comparison to control sites. Comprehensive traffic volume data, with breakdown by vehicle type, should be required. Raw data must always be accompanying in an easy format, for example as an Excel file or a format that is easy to place in Excel, Python, R or any other statistical package such as comma-separated values.

4: The longest possible periods should be used for evaluation. The use of five-year periods likely captures peaks of accidents and obscures the overall trend. A minimum 7-8 year tend should be used. Outlier years should be treated as such.

5: Full accounting for of traffic volumes and traffic composition is necessary.

6: Automatic biases in assessments should be removed. If a camera is not proving effective it should be removed. Trends in both directions should have their significance stated in evaluation.

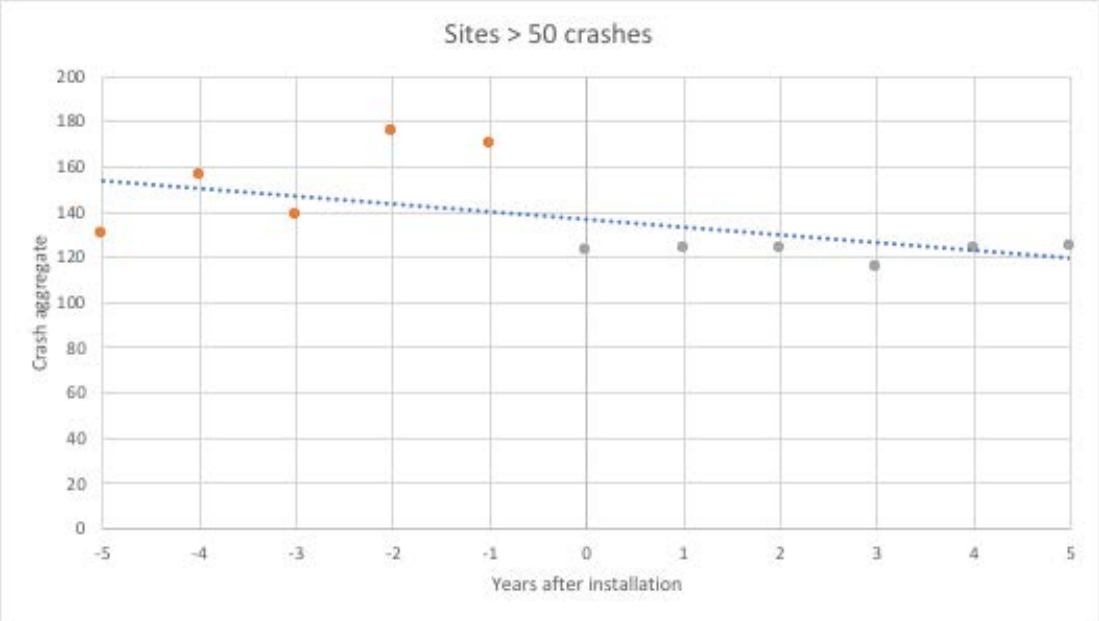
7: Access for road user advocacy groups and the general public should be increased and the influence of the academic community decreased. Decisions should be debated in balanced councils with open reporting of discussions, and decisions as to what measures are used where and how should be arrived at by community consensus, not through academic fiat.

8: Framing of policy must be addressed. Sensationalism, association with accidents prominent in the media, the well-timed releases of the footage of ‘hoons’ and so on should all be done away with. Emotion should not be used to make policy.

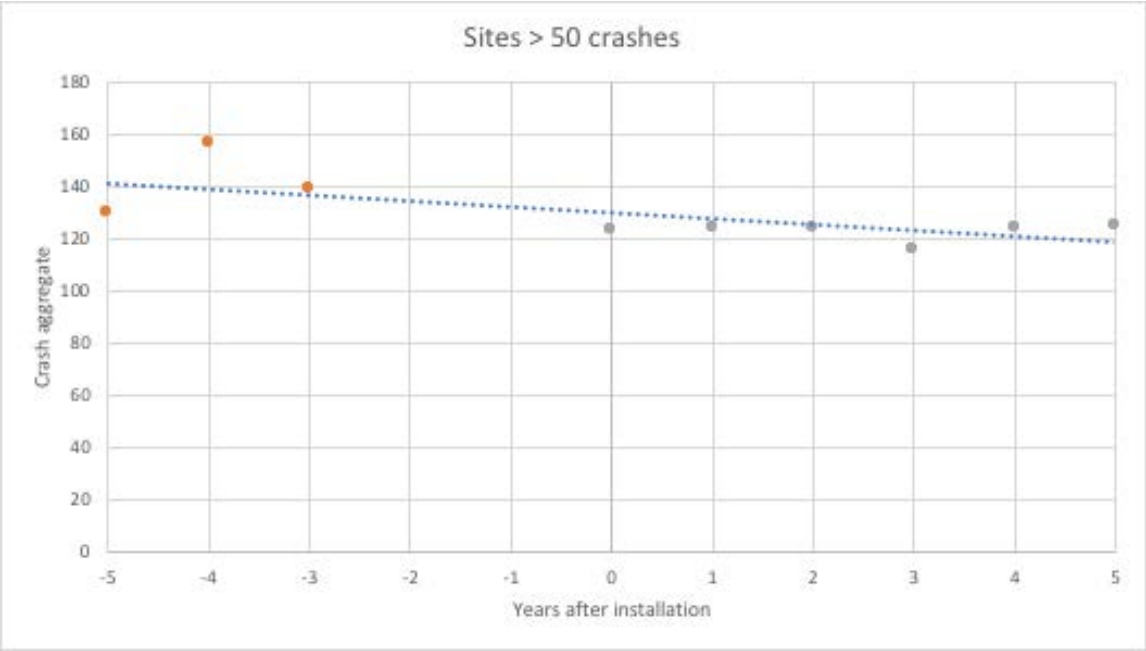
9: Assessments to be conducted by groups independent of MUARC, CARRS-Q, Adelaide University and TARS. All policy papers from these groups should be passed off to several independent statisticians for scrutiny, and examination by at least one skeptical group should be mandatory to counteract the inherent bias in these organisations.

10: Framing of assessments: reports from MUARC etc are to not be referred to as ‘independent’. They should be referred to as ‘government-sponsored/commissioned’ which will make clear that they are not, in fact, truly independent.

Examples of reversion to the mean, spikes imposed on trends and cherry picking:

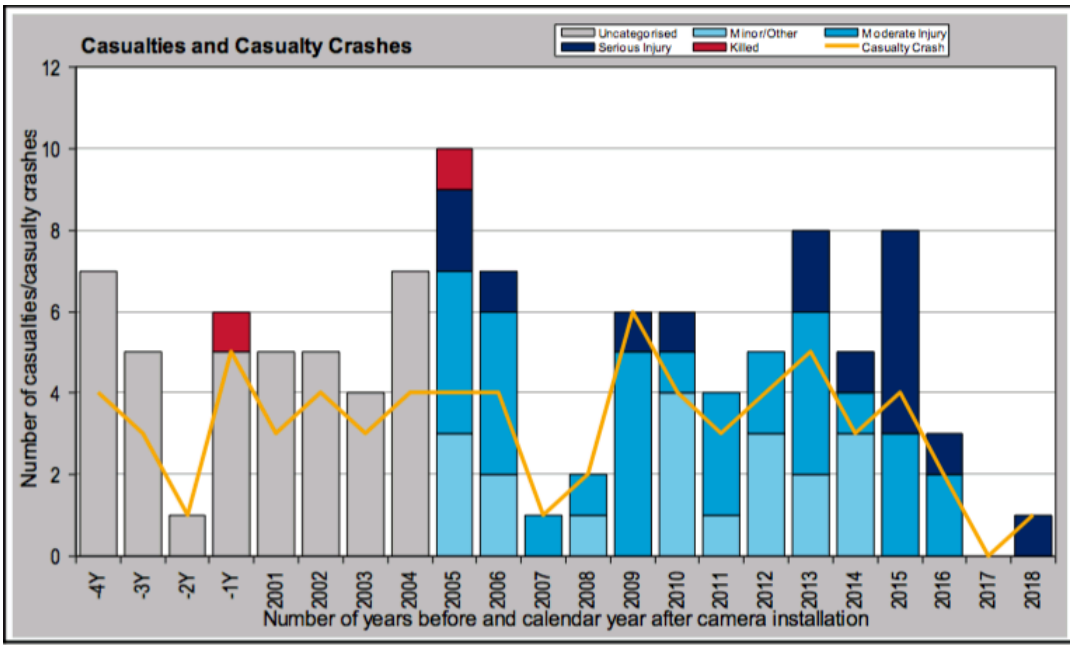


The above chart shows the number of crashes at fixed speed camera locations with more than 50 crashes in the combined reference period, with a minimum before period of five years. A total of seventeen sites are included. It can be seen that there is a fairly distinct break in trend, with the after period having 25.7% fewer crashes per year. However, it can be seen that the trend before is highly variable, with the two prior years having an especially large number of crashes. It is such a spike that may lead to a speed camera being installed in a location.

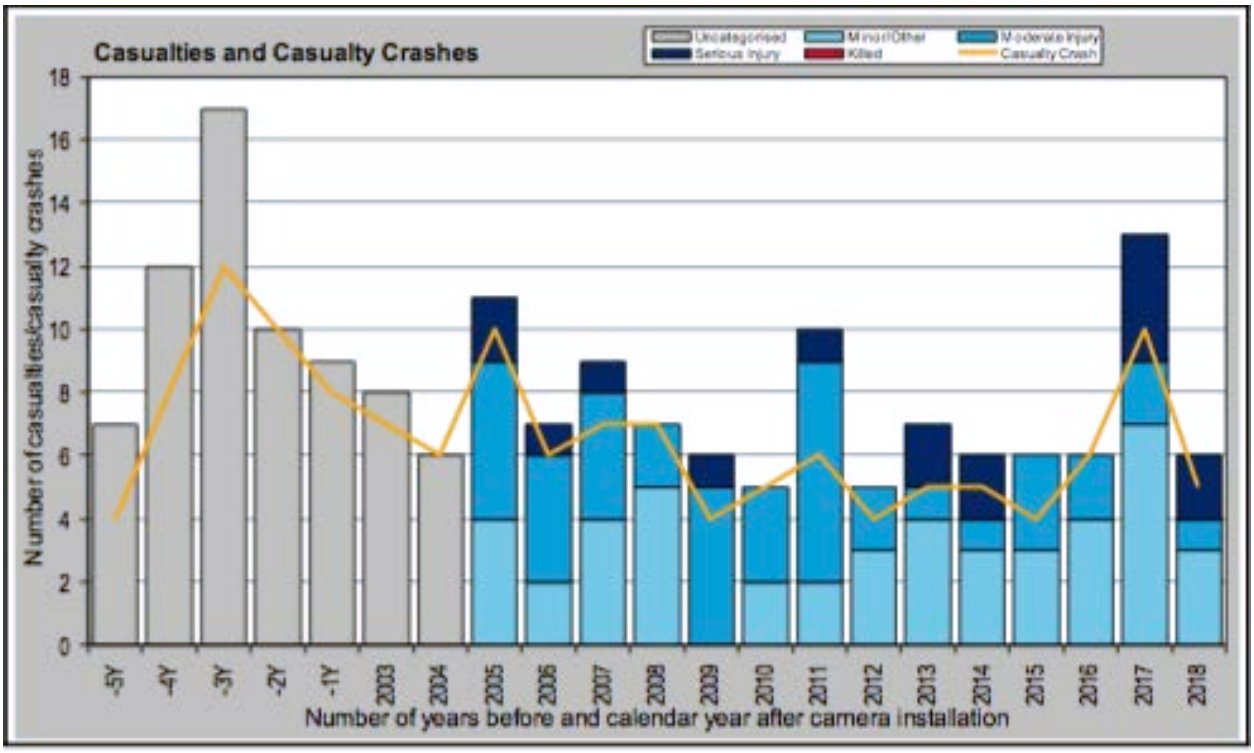


If these two ‘spike years’ are removed, a much flatter trend appears. The reduction falls to 15.5%. The short ‘before’ period can be seen to be too short to draw trends from, a longer

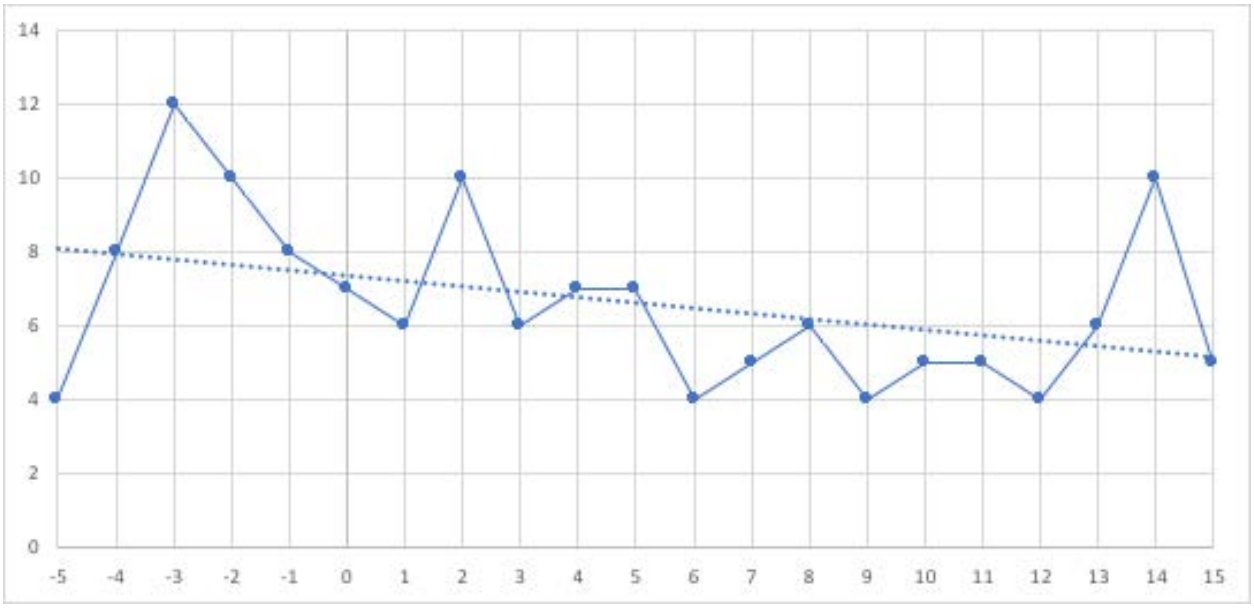
period (say, seven years) would help this. The apparent reduction when the two ‘spike years’ are included can be seen to be part of regression to the mean, thus the cameras are less effective than supposed. The overall high irregularity of the pre-camera trends makes any comparison to this data less robust.



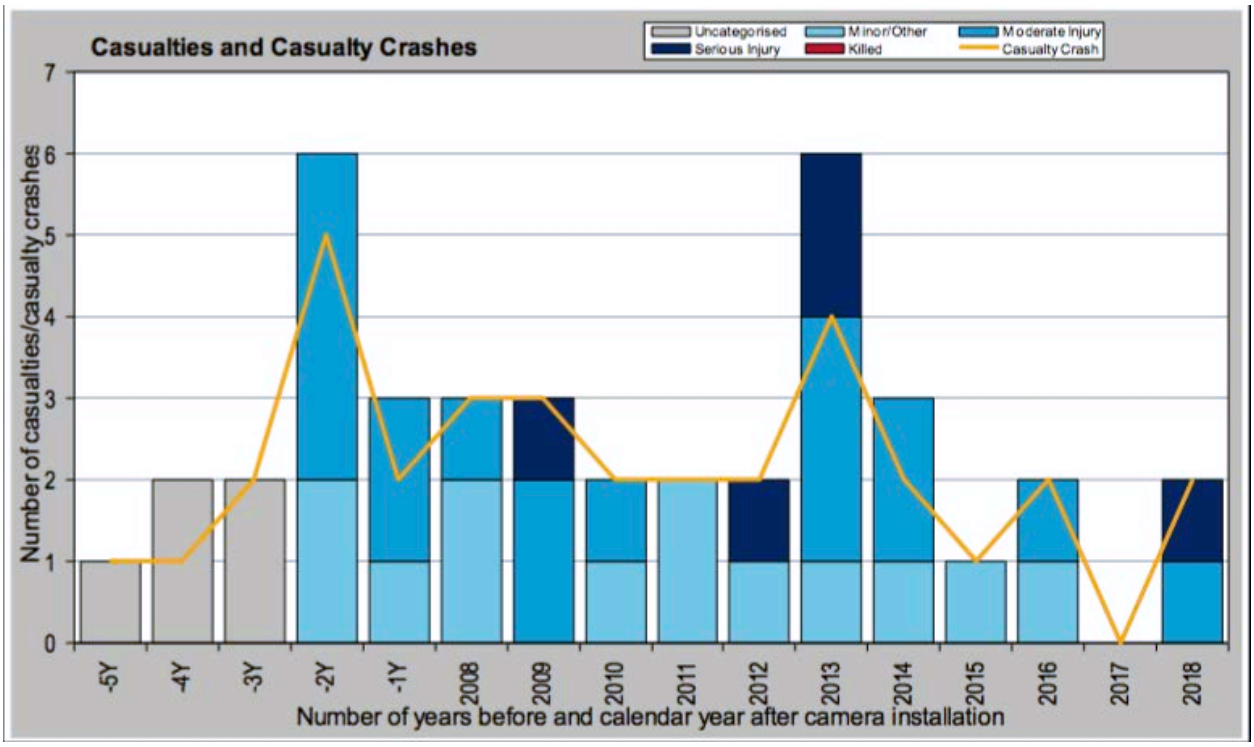
The above chart, from site 9643, shows a cherry-picked reference period. Site 9625 is nearly identical in pattern and 9540, 9557 and 9557 similar. By choosing the last five years of the chart as the ‘after’ period, this can be used to misleadingly attribute a decrease as an effect of the camera when the average post-camera years show a slight average increase in crashes. None of those sites can be used to show the effectiveness purported of them as there was no immediate change in crashes. The later decreases have different, unknown causes.



The above chart, from site 9533, shows an example where a secular trend has been misinterpreted. There is a steady downwards trend beginning from year -3 (a spike year). Yet by averaging the before periods a misleading picture is obtained as the before period will always be higher if there is a steady trend.



No trend break is visible therefore this cannot be used to prove that the camera is effective.



The above chart form site 9813/9814 shows classic mean reversion, where a single spike year misleadingly inflates the before average. A similar year after installation is not included through cherry-picking the reference period. The purported decrease in crashes is 36%, however using the average of the entire 'after' period changes this to just 5%.