

Differences between Groups of Drivers: Offences Contrasted with Crashes

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If an intervention can be shown to affect the number of driving offences, is this also evidence that it has an effect on road crashes? We summarise two recent studies in which we found a difference between groups in respect of driving offences but not in respect of crashes. One study focused on the method of obtaining a driving licence, the other concerned participation in a brief intervention program for young offending drivers. Further, the literature reveals other examples of different effects on offences and crashes. One possible explanation is that there is a closer link between the behaviours targeted by the intervention and being caught offending than between those same behaviours and being involved in a crash. Unfortunately, the question remains open as to whether there is an effect on crashes that is in the same direction as the effect on offences but smaller, or whether there is no effect on crashes because the behaviours that differ between the groups are not relevant to crashes.

Introduction

As in other fields, there is a need to conduct research into road safety as cheaply as practicable. Consequently, it sometimes happens that the number of crashes available for analysis is too small to draw any definite conclusions. It would therefore be useful if driving offence information could be added to the totality of evidence - specifically, with regard to the effects (if any) of intervening with the driver to try to reduce their likelihood of crashing.

An extreme statement of the idea is as follows: 'Accidents are a function of violations [offences]; the accident expectancy of any group of violators is in proportion to the violation frequency of that group. Since accidents occur with relative infrequency, therefore, violations are a more accurate measure of accident expectancy than the number of accidents themselves' (Wichita Police Department 1939, p. 86).

If it could be established that an effect on crashes is often a diluted version of the effect on driving offences (a possibility to be examined below), it could be argued that a statistically significant effect on driving offences would be evidence for a real effect on crashes, even if the latter were not statistically significant. Raub et al. (1999)

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found that attendance at traffic safety school had a statistically significant effect on driving offences but the effect on crashes was not statistically significant. They attributed this lack of effect to the sample size being too small. There is a degree of plausibility to using effect on driving offences as a proxy for effect on crashes, as some driving offences are behaviours that are clearly dangerous and increase the risk of a crash. Yet driving offences are different from crashes and what affects one might not affect the other. (The present paper does not employ an exact single definition of driving offence or crash. The empirical results come from many previous studies that used many different definitions. As will be discussed in the concluding paragraphs, it is possible that an appropriate way forward is to disaggregate data into different types of driving offence and different types of crash.)

As we will see below, there is reason for caution in attempting to translate a conclusion about driving offences into a conclusion about crashes. In case there is any doubt, though, we wish to emphasise that this issue is distinct from the question of whether enforcement of traffic laws is itself an effective means of promoting road safety. Many enforcement measures (e.g. of speed limits, drink driving laws and seat belt wearing) do have very positive effects on road safety (Elvik and Vaa 2004, p. 965). And although many authors note how frequently drivers infringe traffic laws by exceeding the speed limit and in other ways (see for example, Corbett 2003), it seems to us that it is not a hopeless task to enforce the traffic laws and get the vast majority of drivers to obey the speed limit. It is common on the straight dual carriageway arterial roads of suburban Adelaide for traffic to travel at the speed limit (usually, 60 km/h), not 15 or 10 or even 5 km/h above it. It may be no coincidence that the daily newspaper, *The Advertiser*, gives a great deal of prominence to road tragedies and to road safety initiatives.

Much of the research in the road safety literature on driving offences has been concerned with demonstrating that the numbers of crashes and driving offences a driver experiences in one year are predictive of the numbers of crashes and driving offences the driver has in the following year. The literature seems to show that there is some persisting characteristic responsible for the correlation between driving behaviour in one year and the next, but this literature is not very useful for present purposes, as the predictive success could be due to a persisting characteristic of poor driving behaviour, or it could be due to a persisting characteristic of the distance driven. (Even though one of the most important determinants of the number of crashes to a group of drivers is the distance they drive - their 'exposure to danger', as it is termed - most road safety research is conducted substantially in ignorance of

this, and thus a reduction in crashes might be due to a reduction in the per kilometre rate or in the kilometres driven.)

The distinctive aspect of the present paper is the attention drawn to the need for theory about interventions, behaviours, offending and crashes. The paper is structured as follows:

- The next two sections will summarise recent studies of ours in which a contrast between driving offences and crashes was noticed. One of these studies focused on differences in the methods of obtaining a driving licence. The second study investigated the impact of an intervention aimed at disqualified young drivers.
- Then we describe a review by Struckman-Johnson et al. (1989) of driver interventions that employed randomised trials, which noted a similar phenomenon. (We do not wish to lay too much stress on our own studies, as there are methodological reasons why a critic could discount their results.) We summarise the explanation suggested by Struckman-Johnson et al. (1989) and by Peck (1976).
- The final two sections consist of discussion. First, we go into greater detail about possible reasons why the contrast occurs, starting from the ideas discussed by Struckman-Johnson et al. (1989). Second, we suggest that looking at data for different types of crash and different types of offence might throw light on how important the different reasons are.

Study 1: Methods of obtaining a driving licence

Background

There are two methods of obtaining a driving licence in South Australia. The driver with a learner's permit (L) either takes a vehicle on-road test (VORT) conducted by an authorised examiner or, over a period of time, completes a log-book of driving experiences with an accredited instructor (competency-based training and assessment, CBTA). Once the on-road test is passed or the driving experiences completed, the driver gets a Provisional (P) licence. It is of interest whether drivers who recently obtained their licence by one of these methods have similar subsequent

driving records to those who obtained their licence by the other method. For more information, see Anderson et al. (2005)¹.

Method

By matching driver licence numbers in the traffic crash (reported to the police) and driver licensing datasets, Anderson et al. (2005) obtained the subsequent driving records of young, newly-licensed drivers who had taken either a VORT or CBTA. The dataset included all drivers in the age range 18-25 years who gained their P licence in the period 1998-2002. There were 91,903 drivers in the sample. The results below will refer to the first 180 days after getting their P licence. The three sets of results summarised below are for all crashes, for crashes in which someone was injured or killed, and for all recorded driving offences (except that speed camera offences were not in this dataset).

No randomised experiment was conducted; the drivers themselves decided which method of licensing to utilise. Hence, there may be differences between the two groups in factors that affected subsequent crashes and offences.

No information was available about distances driven. Any differences found between the groups in respect to crashes or driving offences could be due to differences in rates of crashing or offending per kilometre driven, or to differences in distances driven, or to a combination of both.

Anderson et al. (2005) did not attempt to infer responsibility for each crash from the details recorded. Young drivers tend to be largely responsible for the crashes they are involved in. Results and conclusions concerning the totality of their crashes may not apply to the minority of their crashes in which they are blameless.

A logistic regression was carried out, with crash occurrence as the dependent variable and 'group' (VORT or CBTA) together with some other variables (notably age, sex, postcode group of residence, and period spent on an L-plate) as predictors. That is, the question is asked whether 'group' is a predictor of subsequent road crashes when controlling for the effects of these other variables. A similar method was used for driving offence occurrences.

¹ However, the results for offences were not included there, and are reported in this paper for the first time.

Results

In the first 180 days after getting their P licence, the odds of having a crash were 6 per cent higher for drivers who had obtained their licence via VORT than for the CBTA drivers. This difference was statistically significant. The background variables were also associated with crashes: drivers aged 16-17 years had a higher crash rate than those aged 18-25; males recorded a higher rate than females; residents of metropolitan Adelaide had a higher rate than country residents; and those drivers who spent one to five months on an L-plate permit had a higher rate than did those drivers who spent six or more months on an L-plate. The sizes of the effects of the background variables were bigger than that of the method by which the drivers obtained their licence.

In the case of casualty crashes, the difference between VORT and CBTA was 8 per cent, similar to the 6 per cent estimated for all crashes, though in this case it was not statistically significant.

In respect of driving offences that occurred within the first 180 days after obtaining their P licence, the odds for the VORT group were 37 per cent higher than for the CBTA group. Some background variables were also associated with the likelihood of recording a driving offence: drivers aged 18-19 years had a higher rate of offending than those aged 16-17 or 20-25; males had a much higher rate than females; and those who had spent 1-5 months on L-plate had a higher rate than those with six or more months as a learner-driver.

It may also be noted that the odds of a crash before getting their P licence were about 25 per cent higher for the VORT group than for the CBTA group, and the odds of an offence were more than doubled. Whatever the reasons - for example, different distances driven or driving habits or attitudes - these results suggest that the groups were different and hence, too much should not be read into any small differences in crash rates after getting a P licence.

In summary then, a comparison between drivers who obtained their licences via different methods indicated some variations between the two groups in both the number of subsequent offences and the number of subsequent crashes, although the crash differences were much smaller.

Study 2: An intervention aimed at disqualified young drivers

Background

In South Australia, many young L- and P-plate drivers who have been disqualified from driving attend the Driver Intervention Program (DIP). This workshop lasts some 90 minutes and confronts the driver with the risks and consequences of road crashes. Naturally, it is hoped that those who attend have a lower subsequent risk of crash involvement than they otherwise would have.

Although the relevant drivers are 'required' to attend the DIP, many do not but pay an expiation fee instead. Thus, it is possible to compare the subsequent crash records of those who did or did not attend DIP (see Kloeden and Hutchinson 2006; 2007 for further details).

All the drivers referred to DIP are disqualified from driving. However, many successfully appeal against disqualification. But even if they do successfully appeal, they are still required to attend DIP. Attendance at DIP typically takes place about six months after the offence.

For more information about who is required to attend DIP and what the workshop consists of, together with a review of literature on driver improvement and a description of personality characteristics and attitudes of DIP participants, see Wundersitz and Hutchinson (2006).

Method

By matching driver licence numbers in the traffic crash (reported to the police) and driver licensing datasets, Kloeden and Hutchinson (2006, 2007) obtained the subsequent crash and offence experiences of drivers who attended DIP or should have attended but paid an expiation fee instead. The drivers were those who were sent their first Notice to Attend DIP in 2001 or 2002. Of the approximately 5,500 drivers in our sample, 65 per cent were aged 17 or 18 years, and 84 per cent were male. Results below refer to the 18 months after the Notice to Attend. The main sets of results summarised below are for all crashes and for all recorded offences (except that speed camera offences were not in this dataset), and there is a note about moving offences differing from administrative offences.

No randomised experiment was conducted; the drivers themselves decided whether to attend DIP or to pay the expiation fee. Thus, there may be differences between the groups in respect of factors that affect subsequent crashes and offences.

No information was available about distances driven. Any differences found between the groups in respect to crashes or offences could be due to differences in rates per kilometre driven or to differences in distances driven or to a combination of both.

A difficulty arises because for the drivers who attended DIP, there is a particular date that separates the pre-DIP from the post-DIP period, but there is no comparable point in time for the drivers who did not attend. Instead, for all drivers, the 'index' date used was the date on which the Notice to Attend the DIP was sent out. This could possibly result in a short-lived effect of the DIP being overlooked, as it would be diffused over two or three six-month periods after the sending of the Notice to Attend, instead of being concentrated in one six-month period after the DIP.

A logistic regression was carried out with crash occurrence as the dependent variable and 'group' (DIP or Expiation) and sex-age combination as predictors. That is, the question asked is whether 'group' is a predictor, when sex and age differences have been taken into account. A similar method was used for driving offence occurrence.

Results

Whether or not the driver had attended DIP had no statistically significant effect on subsequent crashes. We considered the percentages involved in a crash in a six-month period and calculated the crash ratio for the two groups (i.e. the Expiation and DIP groups), after allowing for any age-sex differences. For three six-month periods after the Notice to Attend, the ratios were 1.1, 0.8 and 1.0.

Drivers in the DIP group tended to commit fewer subsequent driving offences. Again, we considered the percentages committing any driving offence in a six-month period after the Notice to Attend was sent and then calculated the ratio for the two groups, after allowing for age-sex differences. For three six-month periods after the Notice to Attend, the ratios were 1.7, 1.4 and 1.4. That is, drivers in the Expiation group were committing about 50 per cent more offences than those in the DIP group. (The difference was highly statistically significant.)

Kloeden and Hutchinson (2006, 2007) made a distinction between what they called 'moving' offences (e.g. speeding, drink driving, performing an illegal manoeuvre or disobeying signs or traffic signals) and 'administrative' offences (e.g. not using a seat belt, unlicensed driving, failing to carry a licence or driving an unregistered vehicle). The difference between DIP and Expiation groups was larger for 'administrative' offences than for 'moving' offences.

There were some differences between the DIP and Expiation groups in respect of crashes and offences in the 12 months before the Notice to Attend was sent. As for Study 1, this is a warning that the groups were different to start with, and hence too much should not be read into any small differences found in their post-intervention behaviour.

In summary, there appears to be a substantial difference between the DIP and Expiation groups in terms of the number of subsequent offences committed but not for the number of crashes.

One possibility is that there really is a difference in crash-related driving behaviour, with driving offences being a more sensitive indicator of this than crashes. (Statistical testing establishes that the data are consistent with zero difference in respect of crashes, but this does not prove that the difference is exactly zero.) The other possibility is that there is a difference in offence-related driving behaviour, with no difference in crash-related behaviour. And when we remember that we are referring to crashes and offences per unit time, further possibilities as to crashes and offences per kilometre driven are evident.

Contrast between offences and crashes in randomised experiments

Empirical findings

A possible reaction to the above two studies is that we are trying to draw strong conclusions from research that used a weak methodology. Randomised experiments were not conducted, which means that any differences found could be due to self-selection bias. (For the relevance of randomised experimentation to road safety, see Hutchinson and Meier 2004 and Hutchinson 2007.)

However, an effect on offences but not on crashes is quite a common finding in the literature, even in driver improvement studies using randomised trials. This was the general pattern found in the review by Struckman-Johnson et al. (1989), which covered 59 sub-studies in 19 publications that reported on both crashes and offences and also employed a good methodology, including randomised allocation. The types of treatment in those studies included behaviour analysis interviews, warning letters, driver improvement meetings, individual counselling, mailed pamphlets and self-test, and defensive driving courses.

Overview of explanation

So what is the explanation of the contrast between offences and crashes? One possibility seems to be that behaviours targeted by different interventions/programs/treatments are more tightly focused on offending than on crashing (Peck 1976; Struckman-Johnson et al. 1989). For one thing, crashes may be the fault of the other driver.

The following quotations from Struckman-Johnson et al. (1989, p. 204) distinguish between two versions of this explanation.

'[There is a] relatively large stochastic component in crash involvement. The behaviour cited in violation [i.e., offence] statistics is, for the most part, under the volition of a driver despite the probabilistic nature of detection. In comparison, crash involvement is less dependent on the behaviour of a particular driver and more dependent on the environmental circumstances and the behaviour of other drivers. As a result, modification of behaviour is reflected more clearly in violations because they are more closely associated with the behaviour of a driver behaviour program participant.'

'Driver improvement activities are ineffective in changing driving behaviour. Reductions in violations could actually be the result of skilful manipulation of the traffic safety system by driver improvement participants rather than any real change in driving behaviour'.

Our interpretation of these words is as follows:

The first possibility is that interventions typically influence attitudes and behaviours that affect the risk of both crashes and offences, with the effect on crashes being hidden by the greater random component.

The second is that interventions typically affect attitudes and behaviours that are unrelated to crashes.

An important but difficult task for the future is to refine these theoretical ideas about the relationship between crashes and offences and thereby develop a better understanding of the plausibility (or implausibility) of driver improvement programs exerting an effect on crashes. In the first case, it might even be possible to develop a theory that will lead to a formula for estimating the effect on crashes from the effect on offences.

Suppose there is only one process operating, but behaviour is more tightly linked to offences than to crashes. It could be that a greater 'stochastic' or 'random' component in crashes is only part of the reason. Instead, more generally, a behaviour may be typically relevant to a smaller fraction of crashes than offences. This would suggest that if data on different types of crash and different types of offence were available, the effect of an intervention on the relevant types of crash would be seen to be quite similar to the effect on the relevant types of offence. A specific form of this may be illustrated with the example of speeding. The probability of appearing in the dataset of offences jumps suddenly when we contrast a speed just below the speed limit with one just above the limit. In contrast, the probability of appearing in the dataset of crashes increases with speed more smoothly, without a sudden jump. More generally, the main factor may be how steeply or strongly the relevant probability (of being recorded as having a crash or an offence) depends upon a measure of behaviour.

On the other hand, suppose the attitudes and behaviours that are influenced by an intervention (or, in the case of studies not employing randomisation, that are associated with its choice) are unrelated to crashes. What might these attitudes and behaviours be? Among the possibilities are:

- Being careful not to get caught;
- In some jurisdictions, drivers are able to prevent offences appearing in their driving records by enrolling in driver improvement courses (Struckman-Johnson et al. 1989, p. 204);
- Learning to be polite to police (in which case, there should be no effect on offences recorded by automatic equipment).

Three comments

The following three points should not be overlooked, but they do not eliminate the need to refine the concept of a tighter link of behaviour to offences than to crashes.

Another possible reaction to our studies and those reviewed by Struckman-Johnson et al. (1989) is that they all refer to different interventions conducted with different populations of drivers, and there is no reason to expect them to share a common feature, such as an effect on offences but not on crashes, or a common explanation if a common feature is identified. It is worthwhile bearing this possibility in mind, but that should not stop us exploring the possibility of a common feature with a common explanation.

As mentioned earlier, the distance driven per year undoubtedly is important in determining how many crashes a group of drivers has per year, as is the rate per kilometre of crashing. In our studies, as in many others, no information was available on distance driven. When an intervention is found not to be associated with crashes, the simplest explanation is that both the rate of crashing per kilometre and the kilometres driven are probably unaffected. It could be that they are affected in opposite directions, but that seems less likely. If distance driven is unaffected, then the observed effect on offences must be occurring via the rate of offending per kilometre.

The rejection or non-rejection of a null hypothesis is often used as a guide, or even a criterion, for deciding whether or not to believe in the reality of an observed difference. In many datasets, offences are more frequent than crashes. This will lead to a statistical test being more powerful for offences than for crashes. Thus an intervention might have a statistically significant effect on offences but not on crashes, even when the percentage reduction is the same. We regard such a phenomenon as merely a consequence of focussing on the result of a hypothesis test rather than on the estimated size of an effect (in terms of an estimated reduction in offences or crashes). Struckman-Johnson et al. (1989), after considering many different studies and after looking at subsets of studies with large sample sizes, felt that low statistical power could not be the explanation for the pattern of results they found.

Varieties of explanation

Continuing our discussion of the tendency to find an effect on offences but not on crashes, we offer the following way of structuring the issues.

1. Does the intervention itself cause the observed differences between the groups receiving and not receiving the intervention? When the data are not from a randomised experiment, the possibility that differences result from different types of people choosing different strategies or interventions is quite a plausible one.
2. Does the same basic process operate for crashes as for offences, or is there something unique to offences?
3. If the same process affects both crashes and offences, does it operate via the rate (per kilometre driven) of occurrence of the relevant outcome (crashes or offences), or via the number of kilometres driven?
4. If there is something unique to offences, does it operate via some form of driving behaviour, or via the interaction of the driver with the system for detecting and recording offences (e.g. noticing a police vehicle, or being polite to the police).

The distinction between one process and different processes is clear when the examples used are choosing a lower driving speed (fewer offences committed and fewer crashes) and learning to be polite to police (fewer offences recorded but not necessarily fewer offences committed and hence, no effect on crashes). But the distinction is not always clear cut, as when there is a single process of improved behaviour, but one that affects different people to different degrees, with the people being affected the most being those who are already the safest. To take an extreme example, suppose no-one had crashes except when speeding, and that the crash rate among young speeding drivers was much higher than among middle aged speeding drivers. Suppose further that driver education reduced speeding to a much greater degree in the middle aged than in the young. The consequence would be a considerable effect on speeding, and hence on speeding offences, but the effect on crashes would be less.

In the case of our own studies summarised earlier, these did not employ randomisation, and the position with greatest immediate appeal is that the differences resulted from self-selection, and that there is dilution when considering crashes rather than offences. However, there are three reasons why we are not fully satisfied with this.

- The first is the degree of contrast between crashes and offences. We carried out a variety of analyses of our datasets, variations on those reported above. Whatever variation we tried, it was difficult to identify any difference in crashes but it was easy to see a difference in offences. It is not impossible that dilution is all that is happening, but it seems a little unlikely.
- The second regards the nature of the effect. Suppose it concerns the rate of dangerous behaviours: in many cases, there is a range of dangerous behaviours, and it would be odd for the effect to be limited to a range that led to dilution being more than quite modest. Suppose it concerns the distance driven: we would expect this to affect crashes as well as offences. (A complicating factor is that distance driven may itself have an effect on the rate of crashes per unit distance.) Suppose it concerns the interaction between the offender and the police officer when an offence is detected: this is a possibility, and it introduces a distinctly different process not applicable to crashes.
- The third is that self-selection does not apply to the studies in Struckman-Johnson et al. (1989). Thus the 'same process or different' question is still open.

Thus we are currently open-minded about exactly which explanation is the most likely.

Different explanations might apply to different studies. It could be, for example, that there have been some methodologically satisfactory interventions that have had genuine effects, but these effects have usually consisted of imparting the motivation and/or the skills to notice and avoid police (without improving safety very much), and that studies not employing randomisation have often had effects via self-selection of different types of driver.

Disaggregation of data

It has already been noted that we found a greater difference between DIP and Expiation groups for administrative offences than for moving offences (Kloeden and Hutchinson, 2006, 2007). This is reminiscent of results from Tasmania reported by Boyce and colleagues in the 1970's. Boyce and Dax (1975) compared a high school that included driver education in the curriculum with four high schools that did not. They found that at the former, there were reductions in both non-moving and

moving offences, but that the reduction in non-moving offences was greater. And Boyce, Dax, and Hagger (1979) found that non-moving offences, but not moving offences, were more frequent in a sample of young people of low intelligence than in young people of higher intelligence. They suggested this might be a literacy or verbal intelligence problem. The ratio of non-moving to moving offences was also relatively high in young people from 'multi-problem families'. The exact relationship of this to our results is not clear, but at least it brings home the point that recorded vehicle offences may be related to behaviours that are not usually considered to cause crashes. For some references to influences of driver's age, sex, demeanour, and circumstances on what action the police take when they stop a vehicle, see Lichtenberg (2002-2003, pp. 439-441, 446-450). There is earlier literature on the exercise of police discretion in encounters with motorists (for example, the studies of Dix and Layzell (1983) in Oxfordshire and Gardiner (1969) in Massachusetts).

According to some researchers, risk factors for (non-traffic) criminal activities are similar to those for road crashes. Low self-control may be particularly important - see, for example, Corbett (2003, pp. 3, 9-10, 138-140) and papers in the collection edited by Farrington and Junger (1995). It may be that this will be found to hold implications for the interpretation of crash data. And recently there has been renewed interest in the hypothesis that among those committing driving or vehicle offences (or, perhaps, among those doing so repeatedly), there are appreciably more serious or prolific or active criminals than would be expected by chance --- see, for example, Roach (2007), and references cited there.

It seems at present that two alternatives are plausible: an intervention has an effect on crashes that is in the same direction as the effect on offences but is quantitatively smaller because it is diluted, or it has no effect on crashes because the aspects of behaviour that are being affected are not relevant to crashes. Thus, unfortunately, at present it is uncertain whether studying driving offences is relevant to road safety, or is irrelevant to that. A possible way forward is disaggregation of crashes and offences, including disaggregation according to how they come to police attention. It might be that features of some types of crashes are similar to those of some types of offences, and different from others. Among the most important variables in the driver improvement context are age, sex, and driving experience of the drivers. It might be asked whether these have qualitatively similar effects on some types of offences as they have on some types of crashes.

Our opinion at present is that it would be unsafe to take a reduction in offences as evidence for a reduction in crashes. When disaggregated offence data is available,

it might be possible to argue that some particular type of offence is tightly linked (for example, without being subject to police discretion) to some particular type of behaviour that in turn is tightly linked to some particular type of crash. However, any such argument would need to be made cautiously and carefully.

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