



# University of HUDDERSFIELD

## University of Huddersfield Repository

Wilcox, Aidan

Any number you want: the impact of data cleaning on internal validity

### Original Citation

Wilcox, Aidan (2007) Any number you want: the impact of data cleaning on internal validity. *Internet Journal of Criminology*.

This version is available at <http://eprints.hud.ac.uk/5840/>

The University Repository is a digital collection of the research output of the University, available on Open Access. Copyright and Moral Rights for the items on this site are retained by the individual author and/or other copyright owners. Users may access full items free of charge; copies of full text items generally can be reproduced, displayed or performed and given to third parties in any format or medium for personal research or study, educational or not-for-profit purposes without prior permission or charge, provided:

- The authors, title and full bibliographic details is credited in any copy;
- A hyperlink and/or URL is included for the original metadata page; and
- The content is not changed in any way.

For more information, including our policy and submission procedure, please contact the Repository Team at: [E.mailbox@hud.ac.uk](mailto:E.mailbox@hud.ac.uk).

<http://eprints.hud.ac.uk/>

# ANY NUMBER YOU WANT? THE IMPACT OF DATA CLEANING ON INTERNAL VALIDITY

By Aidan Wilcox\*

## *Abstract*

*Concerns about the internal validity of reconviction studies tend to focus on factors such as initial comparability of groups. Often overlooked is the impact that data preparation can have. Data preparation refers to the decisions taken by researchers regarding which offenders to retain in the sample for analysis. Using data relating to a sample of offenders in two police forces, it is shown that these decisions, even when applied equally to both groups, can impact differentially on reconviction rates, weakening a study's internal validity. Implications of the findings are considered and recommendations made to improve the transparency of the process.*

---

\* Dr Aiden Wilcox is Senior Research Fellow at the Applied Criminology Centre, University of Huddersfield

## Introduction

In recent years the government has expressed a commitment to be 'guided by the evidence' in criminal justice policy-making (e.g. Blunkett 2000), and this has come to be known as the 'what works' approach (in contrast to the 'nothing works' pessimism of the 1970s and 1980s). What the government means by what works is whether an intervention or policy successfully reduces crime, whether at the level of society, community or the individual. For interventions that seek to reduce crime by targeting individuals (rather than places, as in situational crime prevention), the reconviction study is the methodology of choice when it comes to assessing the question 'has it worked?'<sup>i</sup> The present what works era, which arguably began in 1998 (Wilcox 2005), has been characterised by a significant increase in both funding for programmes aimed at changing offending behaviour,<sup>ii</sup> and reconviction studies commissioned to test their effectiveness.<sup>iii</sup>

As in any type of evaluation, a key concern for those commissioning and using research has been the strength of the research design. There are several criteria against which a research study could be judged; for example in the quality of the reporting of its data (descriptive validity), the generalisability of its findings (external validity) or the extent to which operational definitions of variables adequately reflect their theoretical constructs (construct validity) (Farrington 2002). However, for research which aims to ascertain whether an intervention (e.g. restorative justice) has reduced reconviction rates, *internal* validity is the prime concern. Internal validity addresses the truth of the question as to whether the intervention caused the outcome (Cook and Campbell 1979). In other words an internally valid reconviction study is one which is able to demonstrate a cause-effect relationship. In order to establish such a relationship, the researcher must be able to rule out plausible alternative explanations for the outcome, which are known as threats to validity.

It is worth briefly explicating the different threats to internal validity before considering how data cleaning might pose such a threat. Let us assume that a reconviction study involves, as a minimum, a treatment or intervention (for example, restorative cautioning), an outcome measure (in this case reconviction), and some form of comparison between different treatment conditions in order to allow an inference about the treatment to be drawn. Bias can be introduced to the results due to initial non-comparability of the groups, methods of analysis, or through attrition – which is explained in-depth below. Specifically, the main threats to validity are as follows:<sup>iv</sup>

*History.* One way in which groups may differ is in their exposure to events *other than the intervention*, in which case, it may be that this 'history' event may be responsible for any observed differences in outcome, rather than the intervention itself (Cook and Campbell 1979). For example, if one were evaluating the impact of an anger management programme by comparing reconviction rates of prisoners from one prison where the programme was available, and a second prison where it was not, it may be that other characteristics of the two prisons (e.g. ethos, staffing levels) were responsible for any differences in reconviction rates.

*Maturation.* Whereas history refers to a particular event which groups may encounter, maturation concerns normal, developmental processes occurring within them (Trochim 2000). We know from criminal career research (e.g. Farrington 1997), that offending is not constant throughout the life course, and tends to increase in late adolescence and decline through the twenties. It would be a maturation threat to validity, therefore, if individuals in the comparison and intervention groups were at different stages of their criminal careers.

*Regression.* Regression simply means that left to themselves things tend to return to normal. An individual's involvement in offending tends to oscillate, such that periods of high offending are generally followed by lower rates of offending, regardless of any outside intervention (Maltz 1984). Treatment and comparison groups may experience different rates of regression to the mean simply because they were at different stages of this cycle to begin with, rather than because the intervention worked.

*Instrumentation.* There may be more than one way to measure the outcome of interest. If different measures (instruments) are used for the intervention and comparison groups, then differences in outcome may be due to differences in the test rather than to the intervention itself. The two main sources of reconviction data are the Offenders Index (OI) and the Police National Computer (PNC) and they are known to vary in their completeness and accuracy (Francis, Crosland and Harman 2002). In the (admittedly unlikely event) that the OI were used to calculate a reconviction rate for the intervention group, while the PNC were used for the comparison group, this could threaten the internal validity of the study.

*Attrition.* Intervention attrition<sup>v</sup> generally refers to loss of cases in a study due to participants dropping out of the intervention, the danger being that those who drop out differ in some significant way from those who do not. For example, those who drop out of an anger management programme may be less motivated to desist from crime than those who attend. If rates of intervention attrition differ between intervention and comparison groups, it would be misleading to compare reconviction rates only of those who completed (Farrington 2002). One can protect against this attrition threat by including in the analysis *all* those originally assigned to the intervention and the comparison groups when comparing reconviction rates.<sup>vi</sup> However, the higher the rate of intervention attrition, the harder it becomes to detect a statistically significant effect of the intervention (if one exists).

There is a second type of attrition, which I term 'data attrition' (Wilcox 2005), in which individuals are excluded from the study *by the researchers*, due to concerns about the accuracy of the data held on them. As in intervention attrition, it is possible that those excluded have different characteristics from those included. However, because they are excluded from the analysis, one cannot calculate their reconviction rate. Thus there is no way to guard against the threat to validity that data attrition poses (except to try to minimise its extent in the first place). It is this second type of attrition which is the concern of this article, and it is explained further below.

*Social interaction threats.* Due to the fact that social research involves reflexive human beings, interactions between them (e.g. between offender and probation officer) may lead to differences in outcome not directly related to the intervention

itself (Cook and Campbell 1979). Resentful demoralisation or compensatory rivalry may result if one group becomes aware that it is receiving a less favourable treatment than another. Alternatively those involved with providing the less favourable treatment may try to compensate the offenders in some way. Such social interaction threats have been observed in a number of criminological experiments (e.g. Clarke and Cornish 1972).

The threats to internal validity are well known, and the seminal work of Cook and Campbell (1979) regarding social science research has been updated for criminological research (e.g. Farrington 2002). This has been accompanied by encouragement from the Home Office and other funding bodies to evaluators to adopt a more experimental approach. In their research standards for reconviction studies, the Home Office emphasises the importance of ensuring comparability between intervention and comparison groups (Home Office 2004). However, what is notable in both the academic literature and official documentation is that the possibility that data attrition may affect internal validity is not mentioned. This is perhaps because it is perceived as unlikely; in order to threaten internal validity the procedures for cleaning data would have to result in different rates of attrition (*and* reconviction) in the two groups. The assumption has been that as long as the same procedures are adopted for both groups this will not happen. The reasonableness of this assumption is tested in this article.

One of the reasons for conducting this research was that the possibility that data cleaning could affect internal validity had been raised by an evaluator I interviewed as part of my doctoral research (Wilcox 2005). In the interview she noted that the decisions she had taken regarding data cleaning affected the overall reconviction rate:

In the [...] study I had six categories of cases, according to how sure I was about the accuracy of the match. I could have had any number in the final sample depending on which matching criteria were applied and I found they all gave different [reconviction] rates.<sup>vii</sup>

Given that it is possible for data cleaning to affect reconviction rates, the question is whether it can do so differentially between groups, thus undermining internal validity. Before explaining how this question was addressed, further explanation is needed about data cleaning.

### **What is meant by data cleaning?**

Data cleaning is the process by which the researcher decides which cases to keep in the final sample. This is based on the degree to which the cases appear to be accurate. For those unfamiliar with reconviction data, it might seem strange that the data need any 'cleaning' or preparation before the statistical analysis can be carried out. It is worth, therefore, explaining briefly where reconviction data come from, and the potential sources of inaccuracy in them.

As mentioned above, the two main sources of reconviction data are the Offenders Index and the PNC. The process of obtaining the data are similar in both cases, in that researchers need to provide certain identifying variables, including the offender's name, gender and date of birth so that they can be traced. The reconviction data which are returned are messy and complex, reflecting the reality of offenders' criminal careers. In the 'raw' output from the OI or PNC, each offence for which someone is convicted or cautioned results in one row on a spreadsheet, containing details of the offence and disposal.<sup>viii</sup> Depending on how many offences they have been sanctioned for, an offender may have anything from one to several hundred rows of data. One of the main tasks of the researcher is to identify from this reconviction history, the offence (known as the target offence) which led to the intervention of interest. The researcher will already have from their own records (perhaps from a probation file) the date of the target conviction or caution, and what type of offence it was for. The question is whether this information matches up with the details of convictions as recorded in the OI or PNC. Data cleaning, therefore, relates to this process of deciding whether the two sets of data match up; if they do the offender is kept in the sample, if not then a decision has to be made as to whether to exclude them. In an ideal world, of course, PNC and OI records would coincide exactly with the data held by researchers. In practice, there are often discrepancies (as there are between the OI and PNC). In the section which follows, the ways in which data could be cleaned are explained.

## Methodology

In order to test the effect of data preparation on reconviction rates a random sample of 1000 intervention and 1000 comparison group cases from an evaluation of restorative cautions conducted by the author and colleagues (Wilcox, Young and Hoyle 2004) was selected.<sup>ix</sup> The cases were taken from the original reconviction data file sent by the Home Office, i.e. they had not yet been cleaned or prepared in any way. The file contained only those cases which Home Office staff had been able to trace on the PNC on the basis of the identifiers sent to them.<sup>x</sup> It was decided to select 1000 cases for each group as this would provide sufficient numbers for the different methods of data preparation to be carried out, and yet would be small enough so that the data could be prepared manually where needed (i.e. without the use of syntax).<sup>xi</sup> The dataset was anonymised by removing the names of the offenders. As mentioned in footnote 9, PNC data contain over 30 variables, most of which were irrelevant for the purposes of the current study (e.g. court at which offender was sentenced), and these were removed.

When deciding which cases to retain for analysis, researchers generally compare the raw reconviction data provided by the Home Office to information about the offender that they have collected independently from other sources. In the case of the restorative cautioning study, the police forces had provided us, from their local files, with the date of caution for each offender. Thus in deciding which cases to retain for analysis, these dates were compared to the full criminal history as contained in the PNC. In the analysis that follows, reconviction rates refer to the proportion of offenders receiving a conviction for an offence occurring within the 12 months following the date of the target caution (i.e. the date of the offence is used in the calculation).<sup>xii</sup>

## Results

The first calculation of reconviction rates is for offenders for whom we are most certain that the data are accurate. This is therefore based on offenders for whom the date of caution as recorded in the local police database exactly matches one of the dates of caution in their criminal history as recorded in the PNC. Adopting this criterion leaves us with a sample of 829 and 914 cases in the intervention and comparison groups respectively; the reconviction rates of these groups are 18.9 and 26.0 per cent. This first comparison shows that there is a far higher rate of attrition in the intervention group (17.1%) than in the comparison group (8.6%) (reasons for this are discussed later).

The question arises as to whether cases which do not match exactly should be included in the analysis. There could be two reasons why cases do not match. The first is that the reconviction data contained on the PNC does not in fact relate to the individual of interest. Although they have already been matched on name and date of birth, it is possible that they are not the same individual, and should therefore be excluded. The second possibility is that the reconviction data held on them are incomplete. It may have been that details of the target caution were incorrectly recorded on one or both of the databases,<sup>xiii</sup> or even not recorded at all. This means it would not be possible accurately to determine whether they have been reconvicted, and again such cases arguably should be excluded. On the other hand, it could be argued that insisting on an exact match between the two data sources (PNC and police records of cautions) is too stringent, and that the dates of some of the cautions may have been entered onto the PNC a day or so out. There is no right answer as to whether one should include only exact matches or close matches, and it is precisely this grey area which is the subject of the article. When researchers make decisions as to the acceptable parameters, they are also implicitly accepting that the rates of data attrition, and, as we shall see, of reconviction may differ as a result.

If we widen the criteria so that any caution recorded on the PNC within a day of the intervention date is included, a further 27 individuals are located (interestingly, 26 of these are from the intervention group), and the reconviction rate of the intervention group increases by 0.3 percentage points.

Obviously one can widen the criteria even further in an attempt to reduce the level of data attrition. Thus, if one decides that any caution recorded on the PNC within one month of the date of intervention is a 'match', then the sample size increases to 876 and 925 cases in the intervention and comparison groups, while the reconviction rates increase to 19.9 and 26.2 per cent respectively. One can further increase the sample size by substituting for those offences where the date of offence was missing, the date of conviction – this adds only nine more offenders (eight of whom were in the intervention group).

The criteria could be relaxed indefinitely; however, as one does this, the risk increases of wrongly identifying a sanction as a target conviction, when it may in fact be a previous conviction or reconviction. The parameters described above rely on writing a syntax (computer command) which is applied automatically to the data. One problem with syntax is that it can be difficult to cover every conceivable possibility. For

example, a caution date may have been incorrectly entered on one database as 03/01/1999 instead of 03/11/1999, and this would not be identified as matching the intervention caution date if one used syntax which simply looked for cautions within a month either side of the target date. For this reason, a visual comparison was made of the remaining cases where there was no match within one month of the target date, and a judgment was made as to whether the case should be included (the example above was included). Such visual comparisons resulted in the addition of a further eight cases which had not been picked up by other methods, resulting in samples of 891 and 927 and reconviction rates of 20.8% and 26.3% in the intervention and comparison groups respectively.

Table 1 summarises the results of the analyses previously described, and gives the sample size and reconviction rates for both groups.

T ABLE 1

*Summary of results: sample size and reconviction rates by type of analysis*

Criterion	Intervention group		Comparison group		% point difference
	N. in sample	% reconvicted	N. in sample	% reconvicted	
Court date exact	829	18.9	914	26.0	7.1
+/- one day	855	19.2	915	26.0	6.8
+/- seven days	868	19.4	921	26.1	6.7
+/- 30 days	876	19.9	925	26.2	6.3
Based on date of conviction	884	20.4	926	26.2	5.8
Based on visual comparison	891	20.8	927	26.3	5.5

The first point to note is that even though identical procedures were applied to both groups, the rate of 'data attrition' varied significantly. For example, 17.1% of cases in the intervention group could not be matched exactly, compared to just 8.6% in the comparison group. Thus even before any variations of approach to data cleaning were made, a differential rate of attrition existed.

It is important to know, when assessing internal validity, whether those 'lost to analysis' differed from those who were matched, in terms of variables predictive of reconviction. Of those whose court date could not be matched exactly, just 4.7% were female, compared to 18.7% of those retained. In respect of age, missing offenders were significantly younger (by 3 ½ years) than those retained. Although it is not possible to calculate an accurate reconviction rate for those missing,<sup>xiv</sup> one can look at how many times they had been cautioned or convicted in total, and this reveals that they had received almost twice as many sanctions as retained offenders (5.2 compared to 2.7). What this suggests is that the differential rate of data attrition is likely to affect the validity of the results. This is because in the intervention group, there were more than twice as many missing cases, and as these were more likely to be reconvicted (as being young, male and having multiple convictions are all associated with a higher probability of reconviction), the reconviction rate based on exact matches is likely to be an underestimate of that group as a whole.

The second finding from the table is that the *difference* in reconviction rates between the two groups varies according to which data cleaning criteria have been applied. For example, in the intervention group, the reconviction rate increased from 18.9% for exact matches to 20.8% for manual matches (an increase of 1.9 percentage points). By contrast, in the comparison group the respective rates changed from 26.0% to 26.3% (an increase of just 0.3 percentage points). This is because most of the additional cases included as a result of relaxing the criteria were from the intervention group, and most of these were also reconvicted. As can be seen from the final column, this meant that the difference in reconviction rates between the two groups declined from 7.1 to 5.5 percentage points, a decrease of 1.6 percentage points. While this may not seem a particularly large difference, the fact there is any difference at all provides evidence that data cleaning can reduce internal validity. Secondly, if one considers that interventions may reduce reconviction rates by just a few percentage points, it is clear that the spurious difference caused by data cleaning could result in a failure to detect a real intervention effect, or alternatively, result in the detection of an effect where one does not exist. The smaller the sample size of a study, the more likely this is to be a problem.

We have seen that changing the criteria for data cleaning can result in differential changes to reconviction rates. How might this occur? Although it was not possible to explore this empirically (for example through interviews with those in police forces responsible for entering data onto the PNC, or by examination of data entry procedures), it seems plausible that it arises due to a combination of, firstly, the differences in the accuracy of data entry between forces and, secondly, the systematic differences between those lost to analysis and those retained (described above). It is well known that PNC data suffer from problems of accuracy and completeness (Russell, 1998). A recent Home Office report found differences in working practices between police forces and variable levels of compliance with national standards on PNC data entry which adversely affected data quality. It went on to argue that 'the current inconsistent approach across police forces impacts adversely upon any organisation dependent upon the data'. (Home Office 2005, p. 5). The consequence for researchers is that the accuracy of the data is likely to vary by police force area, resulting in differing levels of data attrition. This would not necessarily be a problem were the cases for which data were missing distributed at random. However, as we have seen above, cases lost to analysis tend to have characteristics predictive of a higher rate of offending than those retained.

## Qualifications

Before considering the implications of the results, it should be noted that there are a number of potential limitations to the analysis. Firstly, the results are derived from the data set of one reconviction study relating to just two police forces, and it is possible that these results are unique to these two forces. However, the findings of the Home Office (2005) report referred to in the previous section suggest that this is unlikely to be an isolated example.

Secondly, the analysis was based on PNC data, and may not be generalisable to the other main source of reconviction data – the Offenders Index. Again, there is no reason to believe that this should be the case. Following a comparison of the two data sources, Friendship et al found that ‘neither source proved more reliable’ (Friendship et al 2001, p. 21). What we do not know, of course, is whether a greater or lesser effect would have been found had OI data been used instead.

Thirdly, the analysis presented above represents a considerably simplified version of the reality of conducting reconviction studies and of the impact that data attrition can have. I have concentrated solely on the effect that data cleaning by *researchers* can have on data attrition. I have not explored the attrition which can occur *before* researchers obtain their data. In the case of the reconviction study on which this analysis is based (Wilcox et al 2004), a substantial proportion of cases (between 12.7 and 17.7 per cent, depending on the police force) were lost to analysis because the Home Office PNC section were unable to trace some offenders on the basis of the identifiers (name, date or birth and PNC number) we had supplied. Given what we know about the characteristics of those lost to analysis (above), it is a distinct possibility that the attrition occurring at this early stage further reduces internal validity.

Finally, it should be recognised that the generalisability of these findings is somewhat limited, as this type of threat to validity is not inevitable. It is only likely to occur in studies where the intervention and comparison groups originate from different administrative areas; in other words where data relating to the two groups are collected by different organisations or branches of organisations. In the example used here they were different police forces. Similarly if the groups were from different probation service areas or local authorities one might expect to see a similar effect. However, if both groups were from the same area, as might be the case in a randomised trial, for example, data attrition would be a less plausible threat to internal validity. Although the analysis here related to reconviction studies, the threats to internal validity demonstrated here could apply to other types of evaluation which depend on the accuracy of different datasets. For example, many situational crime reduction initiatives are evaluated using geographic information systems (GIS) in which the geographic locations of crimes are compared to the locations of crime prevention initiatives such as CCTV (Hirschfield, 2005). Errors in either dataset could threaten internal validity in much the same way as in reconviction studies.

In brief, the magnitude of the effect of data cleaning on reconviction rates as revealed in this analysis may be unrepresentative of the population of reconviction studies. However, as the aim was to test the hypothesis that data cleaning made *no* difference to reconviction rates, the size of effect is immaterial. An effect has been found, and

the hypothesis can be rejected. The results of this small study show that bias can unwittingly be introduced as a result of the routine cleaning of data which is an essential part of the conduct of a reconviction study. In the concluding section, the implications of the results are discussed and some solutions proposed for future practice.

## Conclusions, Implications for research, and the way forward

Maltz noted over 20 years ago that: ‘There are so many possible variations in the method of computing recidivism that one doubts if more than a handful of the hundreds of correctional evaluations are truly comparable’ (1984, p. 22). Seemingly, this finding holds true today: data preparation can make a difference to the reconviction rate. This may not necessarily be a large one, but the fact remains that even if identical criteria are applied to both groups, the rates of attrition and thus reconviction of one may be more affected than the other. This poses a threat to internal validity. The threat is likely to be significant if the overall differences in reconviction rates between groups are not themselves very large. In such cases data cleaning could result in a spurious finding of a treatment effect (or an obscuring of a real treatment effect).

The findings of this study have a number of implications for the conduct and interpretation of reconviction studies. Firstly, researchers should be more cautious when making comparisons between the reconviction rates of different studies. We have seen that the assumption that the reconviction rate is unaffected by data cleaning is not a valid one. In a reconviction study of final warnings, for example, the reconviction rates of offenders in four youth offending teams were compared to the reconviction rates of a *national* sample of cautions, and the authors argued ‘it is our view that this national sample of cautions is a reasonable comparison group and will offer appropriate indications of the success of otherwise of final warnings in preventing offending’ (Hine and Celnick 2001, p. 7). Given that the reconviction rates of the final warnings and cautions were calculated by two different sets of researchers, and that the groups were from different geographical areas, the validity of such an assumption is questionable.

Secondly, if readers of a reconviction study are to be able to assess whether data attrition is a likely threat to validity, then the researchers conducting it need to be more explicit about the decisions they have made regarding data cleaning. In essence, this is a call to improve the descriptive validity (quality of reporting) of reconviction studies. When writing up the methodology, it should be noted, for example, whether cases that did not match exactly were excluded; whether cases were checked visually or only through syntax, and so on. Indeed, it would be a good idea for researchers to provide a range of reconviction rates (and associated rates of attrition) based on different methods of data cleaning. If a statistically significant treatment effect was found for each, then one could have more confidence in the results.

Finally, it would be useful if further investigation could be conducted into the sources of error in PNC and OI data. Why, for example, are those with longer criminal histories more likely also to be lost to analysis, and why does data attrition tend to vary by gender? Consideration also needs to be given to what can be done to reduce the variability in (and improve overall levels of) data quality both between different criminal justice agencies and within those agencies. This would do much to negate the threat to internal validity that data cleaning currently poses. The practical difficulties in achieving universally accurate reconviction data should not be underestimated (Francis, Crosland and Harman 2002), and in the meantime, those conducting reconviction studies need to demonstrate transparency in the data cleaning process,

and greater rigour in the reporting of methodology. Otherwise reconviction rates will remain 'any number you want'.

## End Notes

- 
- <sup>i</sup> Reconviction data, on which reconviction studies are necessarily reliant, have well known limitations (Maltz 1984), not least of which is the fact that *reconvictions* represent an unknown, but smaller proportion of the amount of *re-offending*. However, alternative sources of data, such as offender self-report, also have weaknesses (Junger-Tas and Marshall 1999).
- <sup>ii</sup> The £400m Crime Reduction Programme, which ran from 1999-2002, is one example.
- <sup>iii</sup> In the six years 1993-1998, the Home Office published six reconviction studies, compared to 32 in the six years 1999-2004 (Wilcox 2005).
- <sup>iv</sup> In addition to the threats discussed in the main text, there is also a ‘testing’ threat, in which the fact that a group took a pre-test causes an observed difference on the post test. In other words individuals may perform better on a post-test not because of the effect of the intervention, but because the pre-test primed them to perform better at the second time of testing (Cook and Campbell 1979). IQ tests are a good example of this. However, in reconviction studies, testing is not a plausible threat, because the measurement of the outcome (reconviction) is carried out without the involvement of the offender (i.e. through official records). In other words, the fact of measuring someone’s previous convictions is unlikely to affect their rate of subsequent convictions.
- <sup>v</sup> The term intervention attrition is used in preference to attrition, to distinguish from data attrition (see below).
- <sup>vi</sup> In medicine, this is known as ‘intention to treat’ analysis.
- <sup>vii</sup> Interviews were conducted under condition of anonymity.
- <sup>viii</sup> With PNC data, each offence (row) generates over 30 variables, including type, length and size of disposal, name and offence group of the offence and date of offence, charge and conviction. A similar number and type of variables are produced in OI output.
- <sup>ix</sup> Using the random sample function in SPSS.
- <sup>x</sup> In fact, the Home Office had not been able to trace around one fifth of the cases. Non-tracing is another source of data attrition. The importance of this point is discussed in the conclusion.
- <sup>xi</sup> Syntax is computer code used to manipulate or analyse data. An example might be to recode age recorded in years to a variable containing five age bands.
- <sup>xii</sup> Similar results obtain if the period selected is 24 months, although evidently the reconviction rates are higher.
- <sup>xiii</sup> There is no reason to believe that one database is more accurate than the other.
- <sup>xiv</sup> By definition, those missing from analysis do not have a matching ‘target date’ and so one does not know where to start counting reconvictions from. This is because we do not know which of the databases (PNC or local police record of cautions) is inaccurate in any particular case.

---

## REFERENCES

Blunkett, D. (2000) *Influence or Irrelevance? Can Social Science Improve Government?* Secretary of State's ESRC Lecture 2 February 2000 London: DfEE.

Cook, T.D. and Campbell, D.T. (1979) *Quasi-experimentation: Design and Analysis Issues for Field Settings*, Chicago: Rand McNally College.

Clarke, R.V.G. and Cornish, D.B. (1972) *The Controlled Trial in Institutional Research – Paradigm or Pitfall for Penal Evaluators?* (Home Office Research Study 15), London: HMSO.

Farrington, D.P. (2002) *Methodological Quality Standards for Evaluation Research* Paper Represented at the Third Annual Jerry Lee Crime Prevention Symposium, University of Maryland.

Farrington, D.P. (1997) 'Human Development and Criminal Careers' in: M. Maguire, R. Morgan and R. Reiner (Eds.), *The Oxford Handbook of Criminology*, Oxford: Oxford University Press.

Francis, B., Crosland, P. and Harman, J., (2002) *The Police National Computer and the Offenders Index - Can They be Combined for Research Purposes?* Home Office: London.

Friendship, C. et al, (2001) 'Reconviction: A Critique and Comparison of Two Main Data Sources in England and Wales', *Journal of Legal and Criminological Psychology* 6, 121-129.

Hine, J., and Celnick, A. (2001) *A One Year Reconviction Study of Final Warnings*. London: Home Office.

Hirschfield, A. (2005) 'Analysis for Intervention' in: N. Tilley (Ed.) *Handbook of Crime Prevention and Community Safety*, Cullompton: Willan Publishing.

Home Office (2004) *Home Office RDS and YJB Standards for Reconviction Studies* London: Home Office.

Home Office (2005) *Code of Practice: The Police National Computer* London: Home Office. Available online at: [http://police.homeoffice.gov.uk/news-and-publications/publication/operational-policing/Police\\_nat\\_comp.pdf?view=Binary](http://police.homeoffice.gov.uk/news-and-publications/publication/operational-policing/Police_nat_comp.pdf?view=Binary)

Junger-Tas, J. and Marshall, I. (1999) 'The Self-report Methodology in Crime Research' in: M. Tonry (Ed.) *Crime and Justice: A Review of Research* (vol. 25) London: University of Chicago Press.

Maltz, M. (1984) *Recidivism*, Orlando: Academic Press.

---

Russell, J. (1998) *Phoenix Data Quality* (Police Research Group Special Interest Series 11), London: Home Office.

Trochim, W.M. (2000), *The Research Methods Knowledge Base*, 2nd ed., available online at: [www.socialresearchmethods.net/kb/](http://www.socialresearchmethods.net/kb/)

Wilcox, A., Young, R. and Hoyle, C. (2004) *Two-year Resanctioning Study: A Comparison of Restorative and Traditional Cautions* (Home Office Online Report 57/04) Available online at: <http://www.homeoffice.gov.uk/rds/pdfs04/rdsolr5704.pdf>

Wilcox, A. (2005) *The Validity of Reconviction Studies* (Unpublished PhD Thesis) Oxford, University of Oxford.