

A methodology for reconviction studies using Police National Computer [PNC] data*

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Abstract

This paper gives an overview of measures commonly used in reconviction studies and outlines how to use Police National Computer [PNC] data to conduct a reconviction study. It highlights the need for standardisation in how reconviction studies are conducted and openness about limitations of working with official sources of data. The paper is written for researchers, practitioners and students alike who are interested in conducting reconviction studies and/or the use of PNC data in research more widely.

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1 Introduction

Reconviction studies are a standard part of criminological work arising primarily from evaluations and longitudinal studies of individuals. As the name suggests, the Police National Computer (herein PNC) is the national database for offence and conviction recording and is often the backbone of any study which seeks to establish whether an individual has been reconvicted in the UK. Despite this, and in spite of others pointing out the limitations of reconviction studies more generally (e.g. Friendship *et al.* [16]; Maltz [26]) no clear guidance on *how* to use PNC data to conduct such a study exists either in the academic literature or government documents. It seems that this crucial element of ‘real world’ criminological work is being constantly (and needlessly) reinvented by research teams and students across the country. Hence there is a need to detail a methodology and make it publicly available. This allows the methodology to be subject to scrutiny rather than being passed ‘secretly’ among researchers and brings the possibility – but not a guarantee – of consistency.

This paper is designed as a practical ‘pen and paper’ version of a process which is now routinely automated by researchers and government departments alike. The main reason for doing this is because whilst many studies, most obviously evaluations for government agencies, include reconviction as an outcome, very few of these papers detail precisely how they arrive at their ‘headline’ figures for reconviction (for an exception see Francis *et al.* [14]). The benefit of a ‘pen and paper’ exercise is that it forces researchers to think hard about what they are collecting, that is, how and why is something collected in one way and not another? The paper proceeds as follows: (1) a discussion about what reconviction data can be used for and limitations therein; (2) a discussion of why standard follow up periods matter; (3) a brief overview of how PNC data are organised; (4) a worked example of counting up offences using PNC; and (5) an overview of research designs commonly used in such studies (and their limitations).

Definitions

It is useful to first define what the key terms in the paper are. First, we are focused on so-called ‘*proven reoffending*’ - that is *offences* which have convictions associated with them. This is distinct from studies which focus on the date of reconviction as this can relate to offences committed during an intervention or before it. A *conviction* relates to any criminal justice sanction, including discharges.

2 What can be calculated in a reconviction study

Before dealing with the details of PNC, a brief overview of what outcomes can be assessed in a reconviction study is offered. Here, we try to cover the most commonly used, but what is ‘most common’ varies according to what one is trying to demonstrate. For the purpose of exposition, it is assumed we are working within an outcome

evaluation framework, where we want to assess whether or not an intervention manages to affect the ‘proven reoffending’ of a given group versus some type of control group. A key point is establishing early on when to count reconviction from i.e. when the intervention starts. In the case of a court ordered community sanction, we might (ordinarily) use the start date of the sentence. For a prison-based programme it would be the point where the intervention takes place in prison (e.g. not the actual date of sentence). With something based in the community which is not linked to a specific sentence (e.g. a person can be released from prison but not start an intervention until they engage with a third-sector agency), we would use the date we can be certain an intervention begins (e.g. the first physical meeting which takes place). Obviously with interventions which are vague in what they aim to do (or about when work begins) this task becomes increasingly difficult and questions should be asked about whether such a programme can ever meaningfully be assessed for ‘effectiveness’. My own experience of evaluating a programme with these problems is presented in Cooper *et al.* [7].

2.1 Reconviction or recidivism rate

The most basic measure which can be calculated in a reconviction study is the simple reconviction rate (also known as the recidivism rate). If one’s sole interest is in establishing whether an individual had a proven offence within a given time period after an intervention began, then a ‘yes’ or ‘no’ will suffice. This will tell us what proportion of the intervention (and comparison) group were reconvicted, from there we might try and draw conclusions about effectiveness. However, this is problematic for two reasons. First, binary outcomes are regarded as ‘all or nothing’ measures (Friendship *et al.* [16]) and thus studies often expand beyond a simple yes or no. Second, the utility of the reconviction rate as a measure of effectiveness is almost entirely determined by the research questions being asked and the design used. In essence, without an adequate comparison group it is not possible to draw firm conclusions about effectiveness and this is even a difficulty with a comparison group (see Morgan and Winship [30]; Merrington [27]).

2.2 (Re)offending frequency

A step beyond simple reconviction rates is to compare the frequency of offending pre- and post-intervention. This involves counting up the number of offences in fixed time periods before and after the intervention for both intervention and comparison groups. Subject to caveats about research design, one is then able to test whether the two groups statistically differ in their frequency of proven reoffending in the period after the intervention starts. This design requires the researcher to use fixed followups, meaning both intervention and comparison group have been “at risk” for equal periods of time (for more on using count data, see MacDonald and Lattimore [25]).

2.3 Serious reoffending

Even if offence *frequency* remains the same, it matters that some people are committing more or less *serious* offences over time and this is something which can be assessed. That said, determining offence seriousness is a complex issue and there are different ways to measure it with (as yet) no firm agreement on a methodology. In developing their own methodology, Francis *et al.* [15] review current approaches which are summarised below.

Public opinion research. This is asking the general public (or sub-sets thereof) to rank offences in order of seriousness (see e.g. Roberts and Hough [35]). Issues with this approach are that (i) researchers can only present a limited range of offences to the public to rate; (ii) public opinion is ‘volatile’, influenced as it is by media reporting amongst other things and (iii) the general public are often poorly informed about criminal justice.

Expert panels rank ordering offences. This has the limitation that expert panels will vary according to their constituent members and will thus result in a good deal of variability between different panels, leaving aside the issue of who is an expert and how they are chosen. Again, such panels cannot consider the whole range of the several thousand offences it is possible to commit in England and Wales. There is also some disagreement, relating to both expert panels and public opinion, about whether a consensus of opinion on anything but the most serious crimes could be reached (Francis *et al.* [15]).

Maximum penalty in legislation. Akin to calculating the range in basic statistics, this approach is very limited in that it only uses one point on a scale, the maximum. As such the maximum might be wholly unrepresentative of what actually happens in the courts. Similarly, what constitutes the maximum is a ‘moveable feast’, subject to legislative changes (think of how often maxima for gun or knife crime have changed in recent years, or the squabbles over drug classification).

Average sentence length. The underlying idea is that no matter the complexity of individual cases, ultimately the average length of sentence reflects how serious a crime is believed to be by the courts at a given time.¹ This approach has the benefit of utilising information available on all offences, which in itself is quite important, but is complicated by multiple offences/sentences on a given date and the difficulty in comparing custodial with non-custodial sentences.

¹ For a discussion of which elements the courts consider under seriousness, see the Sentencing Guidelines Council [40].

The paper by Francis *et al.* [15] argues for the development of a new approach based on correspondance analysis which utilises as much of the available information in court records as possible and goes beyond the ‘average sentence’ approach because it utilises both the length of sentence *and* the type of sentence (i.e. both custodial and non-custodial).² Whilst this is an improvement over using average length of sentence, it does give rise to some apparent ‘oddities’ in how offences are ranked (e.g. rape of a female under 16 is considered to be ‘more serious’ than rape of a male under 16, but this might reflect less sentencing information for the latter offence). Furthermore, any offence seriousness scale raises the possibility of frequency being equated with seriousness, that is, the idea that multiple shop thefts might somehow equate in seriousness to a rape. To overcome this, Francis *et al.* [15] suggest keeping seriousness and frequency as separate measures and determining which individuals are ‘serious but not frequent’, ‘frequent but not serious’ or ‘frequent *and* serious’. What should be clear from even this brief discussion is that judging offence seriousness is a vexed issue and is no trivial undertaking.

2.4 Time to next offence

Another facet of reconviction one might be interested in, particularly with a view to conducting event history analyses (EHA) where ‘time-to-failure’ is the outcome, is recording the date of the next (convicted) offence. So if person A was released on 1st January 2012 and next convicted offence occurs on the 28th August 2012 then this would be 237 days (e.g. using the DAYS360 function in Excel). Event history models focus on the number of days between start/release and the next proven offence, or in trying to explain variation in the number of days as a function of time-constant or time-varying covariates (for detail on the technique, consult Allison [1]. For examples of EHA see e.g. Bowles and Florackis [4]; Sutherland [45]; Sadlier [39].³

2.5 Offence specialisation

An interesting aspect of offence analysis more broadly is the debate regarding so-called *offence specialisation*. That is, to what extent can ‘types’ of offenders be identified, or rather, are there offences for which some individuals show a preference for committing

² Correspondance analysis is in essence a special type of factor analysis for categorical data which uses cell counts. Francis *et al.* [15] categorise offences, disposals/disposal lengths (p.20-1 and Appendix E1). This allows a count of all those who committed, say, robbery and received a custodial sentence of four years or more but less than six years. This was complex in 2005 but should, in theory, be more straightforward with the homogenisation of community disposals via the generic adult and juvenile community sentences.

³ As is discussed below, in an outcome evaluation framework we still require a comparison group when using this technique. Similarly, EHA does not overcome the issue with measuring time ‘at liberty’ which is discussed below.

over time?⁴ Another way of thinking about this is asking the question ‘if someone commits x offence, how likely were/are they to commit y offence before or after?’. Using techniques such as latent class analysis, latent transition analysis, latent trajectory modelling, measures such as diversity indices (D) and so on, researchers are able to group together offences/offenders to draw out patterns of offence specialisation (for an overview and more recent innovations, see Nieuwebeerta *et al.* [31]). Debates about whether it makes sense to classify offences and/or offenders in this way continue, particularly in the context of ‘criminal careers’ research, but there is some agreement (at least according to Soothill *et al.* [43]) regarding “a shift in view towards the existence of short-term specialization, and that specialization exists for sex offenders and violent offenders” (p.113) (see also Stander *et al.* [44]).

2.6 Lambda

Not to be confused with measures of statistical association such as Wilks’s lambda, in the context of reoffending lambda (λ) is the offending rate for each offender during a given time period (Blumstein and Cohen [3]). That is, if an offender has 20 proven offences within a 24-month period then the calculation would be $20/2$, giving a λ of 10 (offences per year). This then allows the relative frequency of offending to be assessed on a single scale.⁵ This is not to say that λ is unproblematic (see e.g. Horney and Marshall [22]). The key for ensuring the comparability of λ ’s is that each individual is ‘at risk’ of offending for the same period. That is, if we have a 24-month window, this must be 24-months ‘at liberty’ rather than 24 calendar months, otherwise λ ’s will vary not only as a function of actual offending, but of periods spent in custody as well. The use (and abuse) of ‘at risk’ periods, and their utility for reconviction, is to what we now turn.

3 Time ‘at liberty’

The aim of this paper is to detail a methodology which provides some consistency between reconviction studies. Part of the basis for this consistency is the time-frame used to count offences. Here we use twelve months ‘at liberty’ (AKA “street” time, Horney and Marshall [22]), meaning twelve months where an individual was/is free to commit offences before/after an intervention (we can think of this as ‘the period at risk for offending’). Using twelve months ‘at liberty’ is an important benchmark because it is a standardised ‘follow-up’ period wherein the ‘at risk’ window is the same for each case. This means that rather than being an arbitrary cut point (such as the date that the funding for an intervention finishes), every individual has had the same length of time to commit further offences after an intervention has finished and/or the period

⁴ This should really be ‘are there offences which certain offenders are repeatedly convicted of over time?’.

⁵ We could obviously examine this for smaller time-scales such as months, weeks or days.

prior to the intervention beginning. This means that for some people who have spent many periods in prison, we are counting back (or forward) a number of years to capture 12 months ‘at liberty’.

This can be contrasted with a recent consultation on criminal justice reforms in England and Wales which states that reconviction is currently measured by assessing “whether an offender has been convicted of a further offence or offences committed within a year of being released from prison or starting a community sentence” (Ministry of Justice [28:44]). The issue with using a calendar year from the start of intervention is that the results could be unsound because unfair comparisons are being made. For example, for inclusion in one study respondents “must have been released for a minimum of one and a half years...to be included in the matched sample” (Sadler [39:10]), where the outcome consisted of whether an individual had been “convicted of a recordable offence . . . within the 12-month period after release from [custody]” [39:11]. The obvious issue is that some individuals will be ‘at liberty’ for longer than others within a given 12-month period. Further, we are leaving aside the much trickier issue of whether the probability of being ‘at liberty’ is in some way related to membership of the groups being compared (which it likely will be unless using randomisation).

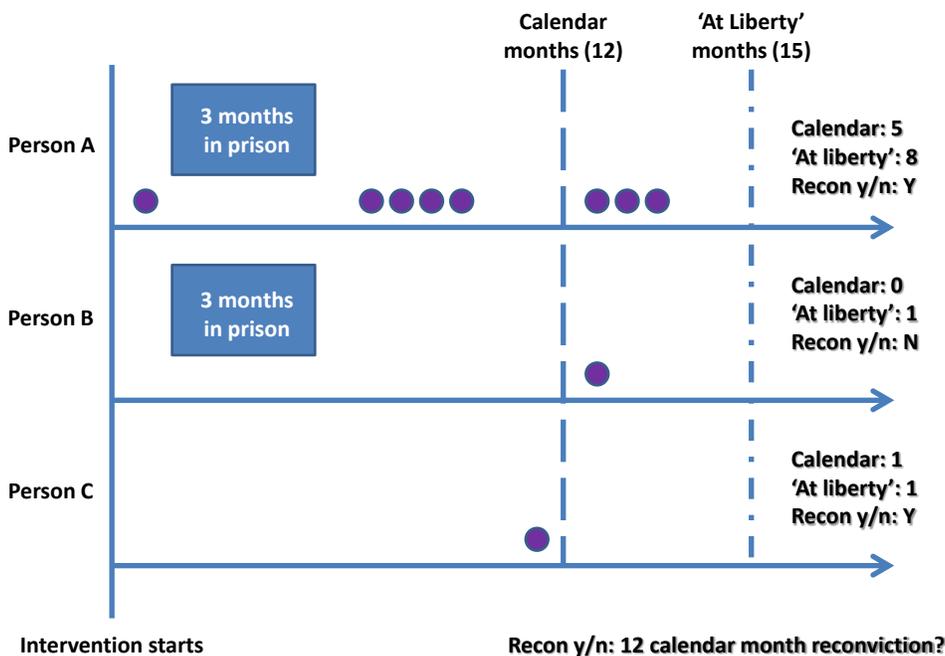
Figure 1 highlights the impact that these two approaches can have on reconviction studies. With ‘Person A’, we see that using calendar months results in five offences being counted, but if 12 months ‘at liberty’ is used, then the total is eight offences (38% more), meaning that we would be systematically under-counting the number of offences and perhaps making an intervention look better than it is (e.g. if we were to calculate λ for ‘A’ in both scenarios (offences per month) then for calendar months it would be .42 and ‘at liberty’ would be .66). Yet the greater problem lies in the comparison of persons ‘B’ and ‘C’. With the calendar month approach ‘Person B’ appears to be a ‘success’, in that s/he has not been reconvicted within 12 calendar months of the intervention starting, whereas ‘Person C’ has been. However, ‘B’ spent three months in prison so his/her ‘at risk’ period is shorter than ‘C’'s. If we were to use the ‘at liberty’ approach then both ‘B’ and ‘C’ would be recorded as having reoffended.

On the face of it, one might think that problems with the calendar versus ‘at liberty’ approach means the former should not be used. This is an empirical question which has been addressed in only a handful of studies (itself a surprise). In a recent paper, Ferrante *et al.* [13] summarise the situation:

‘Failure to adjust for time spent in custody (TSIC; sometimes referred to as ‘non-street time’) will likely lead to an underestimation of re-offending, and a corresponding over-estimation of the rate of desistance, by virtue of having over-estimated exposure time’ (p.274).

Yet Ferrante and colleagues found that the exclusion of TSIC made little difference (2-3 percentage points) to the probability of re-arrest when they examined their population of offenders in Western Australia (WA). But they noted that differences were found for specific sub-groups (such as those from the Aboriginal community), for shorter time-frames and were likely to be found for samples rather than population-level data. We

Figure 1: Impact of calendar months vs. ‘at liberty’ on reconviction studies



should not lose sight of the fact that Ferrante and colleagues examined *rearrest* not reconviction - the two are quite different (and vary between jurisdictions). We should also bear in mind that the offender population in Western Australia might be very different to that of the UK (or Europe). The authors themselves note that WA has a higher arrest and sanction rate than other Australian states and thus may not be comparable even to the rest of the country (Ferante *et al.* [13:285]).⁶

To summarise, there are many possible measures which can be developed for reconviction, what was given above is by no means exhaustive. However, as recently as 2006 Israel and Chui [23] noted that in the UK ‘Most evaluation studies of offender programmes in the community and in prison have been based on single measures, mostly the recidivism rate’ (p.181). This suggests that whilst problems with using basic measures are by no means new (see England [10]), there is still not yet a great deal of diversity in how reconviction studies assess central outcomes. We should also remember that there are vagaries and complications arising even just using the ‘simple’ recidivism rate (Maltz [26]; Israel and Chui [23]; Merrington [27]).

⁶ Ferante *et al.* [13] also found that mortality does not appear to affect the same population level probability of re-arrest, but it is worth remembering that differential mortality rates within an offender population could affect such estimates, particularly if one is doing a long-term follow-up study.

4 Using PNC for reconviction analyses

In this section we detail the steps in conducting a reconviction study which has at its heart the idea that we include offences committed within periods spent ‘at liberty’ (i.e. ‘not in prison’). The ‘industry standard’ for reconviction studies according to, e.g. Friendship *et al.* [16], is a follow-up period of 24 months, but we discuss this in terms of 12 months for ease of exposition as the issues are the same. It is worth pointing out that although 24 month follow-up periods are ‘standard’, these often mean 24 months after release rather than ‘at liberty’. It is also worth noting that length of follow-up period is *entirely arbitrary* (see Maltz [26]).

4.1 Structure of PNC data

Assuming your application for PNC data is successful (for an overview of how to apply, see Appendix A) you will normally receive PNC data in Excel format, with one line relating to each offence for each offender. For example:

Table 1: PNC data structure

Person ID	Offence ID	STARTDAT	Offence type	CTCAUDAT
1	1	21/09/11	Theft	01/Jan/12
1	2	18/06/11	Theft	01/Jan/12
1	3	17/06/11	Violence against the person	15/Jan/12

This means that even with relatively few offenders it is possible to have many times that number of rows of data (e.g. in a recent study the author requested data on less than 200 individuals but got back 6,000 rows of offence data). PNC files also contain the long-form offence code and the Home Office offence code. It is therefore possible to create simple distribution tables for more detailed types of offences if desired (see e.g. table 2).

4.2 Before counting offences

There are a lot of columns included in the file (see appendix B for details), not all of which will be relevant. Moving the columns around so that they look something like the example in table 1 above - with your unique ID first, followed by the offence ‘start date’, long offence description, then sentence date/type is one solution. Once you have done that, sort the data by ADJUC code (adjudication code), userref (your unique ID) and caseId. We might primarily be interested in cases with a “G” in ADJUC code as this is ‘Guilty’ (admitted or found guilty), so we are dealing with so-called ‘proven reoffending’. Most other ADJUC codes are not convictions so we would not ordinarily include them, however using only “G” cases is a restrictive view of what reconviction relates to, others (e.g. Francis *et al.* [14]) take the view that one would also include

Table 2: Offence categories summary table

Offence	Freq.	Percent	Cum. %
Violence against the person	291	5.52	5.52
Sexual offences	15	0.28	5.81
Burglary	99	1.88	7.69
Robbery	84	1.59	9.28
Theft and handling stolen goods	2,105	39.96	49.24
Fraud and forgery	229	4.35	53.59
Criminal damage	59	1.12	54.71
Drugs offences	236	4.48	59.19
Other indictable offences	757	14.37	73.56
Indictable motoring offences	1	0.02	73.58
Summary offence excl. motoring	883	16.76	90.34
Summary motoring offences	146	2.77	93.11
Unknown	2	0.04	93.15
Offences outside England and Wales	7	0.13	93.28
Breach offences	354	6.72	100.00

reprimands/cautions, etc. To do this, we would also include the ADJUC code “J”. We sort by userref and caseId for convenience. We are then focused on two pieces of information, the court date (CTCAUDAT) and the date of the offence (STARTDAT).

It is worth noting here that PNC data *on its own* may not be sufficient to conduct a reconviction study - researchers will often need to combine it with other administrative data from intervention providers. The knock-on effect is that unless those providers capture the ‘right’ data, such as when an intervention starts, the task of conducting a reconviction analysis of any kind is seriously hindered. Here is an opportunity for *quid pro quo* with intervention providers - researchers improve basic data collection and in return get better data.

Working assumptions

As with any research, there are several working assumptions when using PNC data. Some of these arise from the nature of the data itself, others from needing other sources of data in order to undertake the analysis.

First, where multiple sentences exist on the same court date (CTCAUDAT), *we assume concurrent sentences*. So if one sentence is 41 days and another 20 and the sentencing date is the same, the sentence length is assumed to be 41 not 61 days. The exception to this is where a person is released then imprisoned on the same date, e.g. released on 28/07/12 but with a “14-ImdCust” as a court outcome on that date. In this case, *we have to assume that this (unlucky?) person had a consecutive sentence*. E.g. 60 days custody on 28/06/12, served until 28/07/12 in prison, then received

another sentence of 30 days on 28/07/12. Francis *et al.* [15] state that there is a flag for this information included in the PNC so if this is requested and provided then we can leave this assumption to one-side. Second, *breaches of orders pre/post should be counted separately* (e.g. in separate columns) as these usually do not constitute a fresh criminal offence.⁷

Third, ‘*bundled offences*’, where a person commits many offences at the same time but there is only a single ‘offending episode’, could be treated as ‘one offence’. For example ‘No licence’, ‘no insurance’, and ‘taking without owners consent (TWOC)’ would all be listed on the same date as separate offences, but arise from the single action of driving away a stolen car. However, multiple thefts on the same date would always be counted as separate offences. This obviously depends on what one intends to measure. Purely concerning oneself with the number of actual offences would lead to counting each offence arising from a TWOC separately. The clear implication of treating such events as a single ‘offending episode’ is that one will systematically under-represent the number of convicted offences. Given that official records under-represent actual offending (discussed in section 6.1 below) then counting all offences might make more sense, but this is down to individual researchers to make clear.

Finally, unless we have the actual release date *we need to assume all determinate sentence prisoners only serve half their sentence in prison* unless there is evidence to the contrary. If there is a SENTENCEDAT given as well in the PNC data this may indicate that an appeal was lodged or there was some other court follow up which might have meant an early release.

4.3 Walk-through calculating 12 months ‘at liberty’

In essence we begin with a naïve calendar month count back (or forward). So if an intervention started for an individual on the 1st of January 2012, we take 01/01/2011-31/12/2011 as our calendar year prior to the intervention beginning. From there we use sentencing information (CTCAUDAT)⁸ and the disposal type (DISP1) to work out whether or not that person was in prison during the period we are interested in (the outcome in DISP1 for prison is “14-ImdCust”). If they were, we again use the sentence data to work out how long these prison sentences were (DISPDUR1 and release date) then add the actual ‘time served’ to the count back, meaning the time for offences to occur in is greater than 12 calendar months.⁹ This means for people

⁷ Other reconviction studies have used this approach (e.g. Moore and et. al [29]; Gray *et al.* [18]) and current MoJ guidance on how to score OGRS3 (NOMS [32]) also highlights that non-criminal breaches should not ‘count’. However, that same guidance also gives a list of breaches which do result in a criminal conviction, and these are repeated in Appendix C.

⁸ CourT or CAUTION DATE

⁹ In the event that we do not have actual release dates we add *half* the sentence length (for adults) as per current guidance (gov.uk [17]). With those under 18 years old, it is worth checking the specifics of a given sentence (e.g. DTOs typically have a half custody-half community structure), but the half-way release point is reasonable for many such cases. However, the need for such assumptions is negated if actual release dates are given.

who have multiple periods of incarceration, we might be counting back several years. Having worked out our time window for offences to take place in, we count up the number of offences using the offence start date (STARTDAT) variable. *Be sure to check a few offences beyond the calculated time window to ensure that there are none which resulted in a period of imprisonment you might have missed.* You will also see a column called, SENTENCEDATE. This applies to cases where the offender has made an appeal, meaning that he or she would have a subsequent appearance date at court. CTCAUTDAT will be different from SENTENCEDAT only if the offender has a subsequent appearance *and* their sentence was revised on the subsequent appearance date.

Steps for counting proven offences

1. Establish the start date of intervention for each case.
2. Count back/forward 12 calendar months.
3. Are there any periods of incarceration in that 12m period? (If no, then just use the 12 calendar months as those ‘at liberty’.)
 - (i) If yes, how long is each prison term in days?
 - (ii) Sum these periods.
 - (iii) Add the number of days in prison onto 12 calendar months to give 12 months ‘at liberty’ either side of the intervention.
4. Use the dates for 12m ‘at liberty’, move to looking at the offence date (STARTDAT) to see which offences fall within the period of interest.
5. Outcomes which are “15-Other” *should be included*. Ministry of Justice data (personal communication) show that practically all outcomes with this code relate to some kind of valid court disposal (i.e. they do not relate to No Further Action cases).¹⁰
6. Check whether any outcomes for the offences just prior to the ‘at liberty’ time-window resulted in custody. If so, then repeat the process above and add that time again to the count-back.
7. ‘Odd’ timings of release dates and offences (i.e. offences taking place when the person should have been in prison) might arise if an individual has spent time on remand for that offence prior to sentence (discussed below).¹¹ In such cases, take *the day before the next offence occurs* as the release date and count back accordingly.

¹⁰ Of 5,833,223 cases with ADJUC code “G” and disposal code “15-Other”, only one was No Further Action (NFA).

¹¹ This is usually included as part of a sentence because it involves a deprivation of liberty. If we have the actual release date this should not be a problem.

Where a case has less than 12 months ‘at liberty’ pre-/post-intervention they *cannot be included in any reconviction analysis*. You can still use these cases to calculate average pre-intervention offences, but you need to flag these cases in your database to ignore them in later analyses. We might still calculate λ for such cases but they would need to be flagged and λ given both with and without those cases included. Where we have actual release dates from prison, we can use programmes such as Excel or the inbuilt date/time functions in Stata, R, SAS or SPSS to calculate length of time served and turn this into another variable. One reason for doing this is because if an individual has served *longer* than the minimum in prison it might be indicative of disciplinary problems in prison (see gov.uk [17]). A worked example of the countback process is included in appendix D.

4.4 Offence seriousness in PNC

Notwithstanding the earlier points raised about the problems of measuring offence seriousness, there is some data routinely included with PNC data on ‘offence seriousness’. The Ministry of Justice (MoJ) divides offences into four tiers¹² where tiers 1a and 1b are flagged as “serious offences”. This data could be used to create additional variables, e.g. (1) was a serious offence committed in the 12 months pre- and (2) post-intervention start. If we use a ‘gravity score’ approach based upon length of custodial sentence, then we could calculate the average offence gravity before and after the intervention began (e.g. Gray *et al.* [18]). We could, of course, just count up the number of ‘serious’ offences committed pre- and post-intervention.¹³

The main issue with the MoJ approach is that it may depend on arbitrary delineations of what constitutes a serious offence. Whilst ‘on the whole’ the judiciary, public and policy-makers agree on which crimes are ‘most serious’ (such as murder or rape), the waters are muddied somewhat in the middle-range. For instance, ‘burglary of a dwelling’ is regarded as serious as robbery by one offence ‘gravity’ scale (Youth Justice Board [49]), but is an ‘Acquisitive Crime’ under the MoJ classification and is therefore not as serious as robbery. Similarly under the MoJ scheme a minor sexual offence would be classified as ‘serious’ whereas a multi-million pound fraud would not, despite both causing harm to the community (see Sentencing Guidelines Council [40]).

Automating the process

Presenting this as a pen and paper exercise is a pedagogical device which forces the researcher to think about the methodology and why things ‘are as they are’. The

¹² Tier 1a “Violent offences”; Tier 1b “Sexual offences”, Tier 2 “Acquisitive Crimes”; and Tier 4 “Other offences”.

¹³ The MoJ has an ‘offence map’ which indicates whether an offence is classified as ‘serious’ or not using the Home Office offence code (HOCODE) (available on request). Using the MoJ tiers, it would be possible to report whether an individual had committed a ‘serious offence’ (and how many) prior to the intervention (or afterwards).

reality, in a world where ‘big data’ is becoming more prevalent and the transparency agenda is opening up government data to researchers, is that undertaking such tasks by hand is unnecessary, error prone and (quickly becoming if not already) impossible. The data manipulation required and calculation of outcomes of interest set out above can all be automated as they are simply a set of logical steps. Accompanying this paper is an example dataset along with a ‘script’ for running a reconviction analysis for 12 months ‘at liberty’ using the (freely available) statistical software *R* (<http://www.r-project.org/>).

5 Analysing reconviction data

So far we have not discussed *analysing* PNC reconviction data and there is a good reason for this: analytical approaches depend almost entirely on the research design used. As such, reconviction studies can use an array of methods, ranging from simple *t*-tests to multilevel regression models. One thing to be clear on is that unless your research design is adequate and you have taken steps to generate an appropriate counterfactual (comparison group), no amount of statistical sophistication will help and robust conclusions about ‘effectiveness’ are not possible.

5.1 Comparison group designs

Papers and books on appropriate designs for comparison group studies are legion so we will not attempt a summary of (or even reference) them all here. For the interested reader, you should first understand the role that comparison groups play in answering ‘what if?’ questions (i.e. “what if person x had not had the intervention? What would have happened to him/her?”). Because we cannot simultaneously observe the same person ‘having’ and ‘not having’ an intervention, one way of answering such questions is through random assignment (i.e. a person is assigned the intervention ‘by chance’, such as through flipping a fair coin).¹⁴ Random assignment, if carried out with fidelity, ensures that any pre-existing differences between treatment and control groups are ‘balanced’ (i.e. can be ignored), meaning that the only difference between is whether or not they received the treatment (this is not to say that there is harmony about such an approach, see e.g. Hollin [21], but also see Killias [24]).

Short of random assignment are so-called ‘quasi-experimental’ techniques which might be employed where randomisation is not feasible (see e.g. Cave *et al.* [5]). One example of this group of techniques is propensity score matching (PSM).¹⁵ The essence of PSM is this: if we can match the intervention and treatment group on a set of

¹⁴ For a wonderfully brief and engaging introduction to randomised control trials for policy evaluation, see Haynes *et al.* [19].

¹⁵ Quasi-experimental research designs use “nonequivalent groups that differ from each other in many ways other than the presence of a treatment whose effects are being tested. The task confronting persons who try to interpret the results from quasi-experiments is basically one of separating the effects of a treatment from those due to the initial noncomparability between the average units in

factors related to the outcome of interest, then whatever differences we observe *should* be due to the ‘treatment’. That is, conditional on a set of observables, we assume that assignment to treatment is ‘as if’ random (Rosenbaum and Rubin [36]; Dehejia and Wahba [8]). The issue for quasi-experimental approaches is that once we move away from random assignment the requirements on data quality, the amount of data needed (e.g. to provide a good match), *and* the technical sophistication for analysis quickly increase Cook and Campbell [6]; Morgan and Winship [30]).

Randomisation and time ‘at liberty’ Even with random assignment it would be necessary to use an approach similar to the one presented here to ensure a fair comparison between treated and untreated individuals before and after the intervention begins. You might ask ‘why is this necessary? Doesn’t the random assignment take care of such differences, allowing us just to use a naïve calendar follow up?’. Random assignment, if done well, takes care of all *pre-existing* differences between the two groups, but has no bearing on what happens in terms of conducting a reconviction study for *post*-intervention offences. In an extreme scenario we can see what impact arbitrary cut points might have. Let us say we have a new intervention called ‘Project X’ which is randomly assigned to individuals resulting a treatment and a control group. If we imagine for a moment that ‘Project X’ is actually extremely harmful and results in those treated becoming much more crime prone but not very good at being criminals, then those in the treated group would quickly be reconvicted.¹⁶ For a simple measure of the proportion reconvicted, then a 12-calendar month follow up might suffice, but as highlighted earlier, this can be misleading and is an extremely blunt measure of ‘effectiveness’. Furthermore, we can quickly see why this would be problematic if we think about other outcome measures. For example, if we wanted to examine changes in offence frequency then ‘Project X’ might be seen as a stunning success because most of the treatment group would be in custody immediately following the start of treatment, reducing their opportunities to commit more offences and thus making the comparison with the un-treated group nonsensical.

5.2 What if no comparison group is available?

There may be some scenarios where, for whatever reason, it is not possible to include a comparison group. Whilst far from ideal in terms of establishing effectiveness (well, it makes this *impossible*), if the same methodology is followed by all studies for calculating reoffending data, then it *is* possible to establish which interventions may “show promise” or at least are of interest for further investigation. For example, if we are interested in whether a new post-custody programme for offenders from a specific

each treatment group; only the effects of the treatment are of research interest” (Cook and Campbell [6:6]).

¹⁶ A (not wholly) fatuous example might be the likely effect if a programme was set up that involved the treatment group of habitual shoplifters coming to a city-centre shopping arcade for intervention appointments.

prison ‘works’, but we do not have the option of random assignment or propensity-score matching, then we might as a very basic step compare the new programme with the overall reconviction rate for that prison, being as careful as possible to make any comparison ‘fair’,¹⁷ and being sure to highlight caveats where required. For instance, we might compare the reconviction rate from a sub-sample of prisoners who all participated in a post-release intervention programme with the overall reconviction rate for all prisoners with similar characteristics from the same prison. Whilst a simple t -test or χ^2 test might suffice to tell us about whether the two groups differ, without random or quasi-random assignment, the results tell us *nothing* about whether any observed difference(s) are attributable to the programme. This is why comparison group designs are so vital in understanding effectiveness.

In terms of the *quality* of evidence a pre- post-intervention study without a comparison group provides, the most we could say is that it would be at number two (of five, with five the highest) on the Maryland Scientific Methods Scale (Sherman *et al.* [41]), wherein the timing “between the program and the crime or risk outcome [is] clearly observed” (p.4). So even with many studies replicating such a design, the lack of a comparison group means there would be no evidence of effectiveness either way (see also Farrington [12]). (For more details on the important role of comparison groups, see e.g. Sherman *et al.* [41] or the (free) book by Evans *et al.* [11].)

5.3 Predicted versus actual reconviction

In the event that a comparison group design is not possible, an alternative approach (albeit one fraught with problems) would be to compare the predicted versus actual reconviction rate for a given group of individuals. In order to do this, one would require additional information such as Offender ASsessment System (OASys) data from the National Offender Management Service (NOMS). OASys contains within it a validated risk of reconviction tool OGRS3 (see NOMS [32]). Individuals are scored according to a list of static and dynamic risk factors for reconviction and placed into OGRS3 score bands (low, medium, high, very high). Each group has an associated risk of reconviction within a one or two year period - using PNC data it would be possible to assess how ‘well’ a given group do against their predicted reconviction likelihood, as long as we also use a one/two year follow up period. Table 3 below gives an example of how this might look - we can see that for those at ‘High’ risk of reconviction our imagined intervention appears to have some effect. But, and this should always be reiterated, *conclusions about effectiveness are not possible without a comparison group.*¹⁸

A problem with predictive instruments such as OGRS3 is that the score bands and likelihood of reconviction relate to *group* averages. So for a medium score, the proportion reconvicted ranges from 50-74% on average for that group. Any *individual*

¹⁷ Data for doing this are becoming more readily available, one example can be found here: <http://www.guardian.co.uk/news/datablog/2010/nov/05/reoffending-rates-prison>

¹⁸ Here, as with other examples, we ignore any problems arising from selective attrition, sampling bias, and so on.

Table 3: Percent reconvicted versus OGRS3 score bands

Score band	Predicted recon. (2yrs)	Actual recon. (2yrs)	N
Low	1-49%	45%	250
Medium	50-74%	48%	294
High	75-89%	65%	191
Very high	90-99%	87%	135
Missing	N/A	9%	116
Total		51%	986

within that group could have a realised probability of reconviction above or below that range. This is the so-called ‘ecological fallacy’, where one draws inferences about individuals on the basis of aggregate data (for early discussions of the ‘ecological fallacy’ see Dokan and Rokkan [9], for discussions of different types of validity see Rust and Golombok [37]). Even with a risk of reconviction tool that had high predictive validity, say 70% correct, this prediction is correct in only 7-in-10 cases and we do not know to which group a given individual belongs until after the fact.

6 Limitations to working with PNC data

Official data on offending have their limitations (Rutter *et al.* [38]) and PNC is no exception. Below are some very general points about the use of such data, many of which will be familiar to those who have ever taken a course on criminal justice statistics or had to use official convictions data. It is important to recognise that PNC is just one source of data and other studies have shown that it carries/ed some problems (Friendship *et al.* [16]; Gray *et al.* [18]; Francis *et al.* [14]), least of all that official crime data are always affected by police recording practices. It is perhaps surprising to note that very little work seems to go on, publicly at least, regarding the reliability of PNC data, but see Wilcox [48]; Francis *et al.* [14] for exceptions to this.

6.1 Under-reporting and the ‘dark figure’

Common-sense tells us that whatever the overall volume of crimes that result in a conviction, these are only a fraction of all crimes committed. Andrew Ashworth and Mike Redmayne [2] elegantly summarise this point:

... [The British Crime Survey] suggests that only some 43 per cent of crimes are reported to the police, and of those, only two-thirds are recorded. This means ... that figures of crimes recorded by the police may significantly under-estimate the amount of crime in society, [and] that the number of offenders detected and prosecuted starts from a low base ... [Furthermore]

the ‘attrition rate’, i.e. the percentage of alleged crimes that do not result in a conviction, is considerable . . . only two-thirds of 43 per cent of crimes are recorded by the police [28%]. Of these, fewer than a quarter are ‘cleared up’ by the police, which means that only six per cent of all crimes are traced to a suspected offender. About half of those result in a conviction or a caution. In other words, the percentage of alledged crimes that end up in a conviction is between two and three per cent. (p.2-3)

Aside from the British Crime Survey, robust estimates of the so-called ‘justice gap’ (Ashworth and Redmayne [2]) come from self-report data in a handful of longitudinal studies within criminology. One such study, the Peterborough Adolescent and young adult Development Study (www.pads.ac.uk) gives perhaps the starkest illustration of this. Wikström *et al.* [47] found that 11% of those self-reporting a crime between ages 12-16 years old had been sanctioned, but these sanctions related to only 0.7% of their crimes. This meant that ‘on average, young people received police sanctions for one crime in every 140 crimes they self-reported’ (p.118).¹⁹ Accordingly, and perhaps this is blindly obvious by now, one should consider any official reconviction data a lower bound on the likely (re)offending of a given individual, particularly if that individual is a prolific offender with an extensive criminal record.

6.2 Spent offences

An important limitation of PNC data is that, as it is an operational database it is ‘weeded’ for old offence data (Francis *et al.* [15]). This is because under the Rehabilitation of Offenders Act (1974), old convictions are ‘spent’ after a certain time (seven years for most offences) and if a person has not offended within a given period (10 years at present) their *entire record* is removed from the active PNC file (for further see Francis *et al.* [15, 14]).²⁰ Whilst this is an obvious problem for research which attempts to detail long-term offending, a ‘typical’ reconviction study might only include a one or two-year follow up period (bearing in mind the point above about being ‘at liberty’). Francis *et al.* [14] also discuss the issue of allowing time for offences to be proven and for that outcome to be logged on the PNC file. That is, an offence might be committed at the very end of the period ‘at liberty’ and unless sufficient time is left for that crime to be brought to court, might remain unproven. The authors recommend that an additional three month ‘confirmation period’ be added to any follow up to allow for offences to appear on PNC. The implication of this is that if we have an intervention which begins on 01/01/2010 and runs for one year to 31/12/10, collecting PNC data exactly one-year after the intervention ends (31/12/11) will mean *a priori* that some individuals will not have had enough time ‘at liberty’ to reoffend. In essence

¹⁹ We might be reassured that those who *had* been sanctioned were the more prolific and versatile offenders in the Peterborough cohort.

²⁰ The file is still available if requested.

what is meant is that, as a minimum, we should collect PNC data for this group 15 months after the intervention ends.²¹

6.3 Pseudo-reconvictions

One issue when working with official data is that the wheels of justice can often revolve slowly due to delays in court proceedings or the ‘clever’ criminal might evade sanction for many years. This can result in what are called ‘pseudo-reconvictions’ - convictions which occur *after* an intervention starts for offences which were committed *prior* to the intervention. In this scenario, check whether the conviction relates to an offence which falls within the ‘count-back’ period and add it to the tally of offences if it does. If not, then such reconvictions should be either counted separately or ignored.

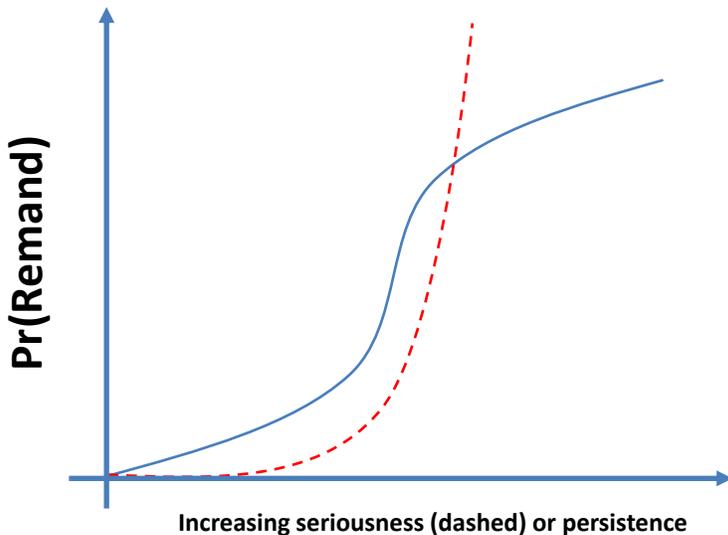
6.4 Remands to custody

Another issue with PNC data which we have not addressed here is periods ‘on remand’ (where a person is held in custody prior to trial or sentence). According to Her Majesty’s Inspectorate of Prisons [20], remand prisoners account for roughly 15% of the prison population (12,000-13,000 inmates). Remand is a problem with regards to reconviction studies which use PNC data for two reasons. First, periods in custody on remand constitute time ‘not at liberty’ and should therefore be added to the count back period. However, unless one is working with Youth Offending Team (YOT) data (which often does contain remand information) or local police data, then it is unlikely that such data will be available as PNC does not contain this information directly. Though it might be deduced from the actual release date for a given offence (e.g. someone serves significantly less than their sentence and there is no evidence of an appeal), for remand periods related to other offences this information is not routinely available. This means that periods in custody may be missing from calculations, meaning that time ‘at liberty’ is under-represented. The second related issue is that those most likely to be remanded to custody prior to a court case may well be persistent and/or serious offenders. For example, there are restrictions on the right to bail placed on those accused or convicted of the most serious crimes,²² and consideration is given to factors such as the likelihood of being a flight risk, offences on bail, the protection/welfare of the defendant (depending on age) (Player *et al.* [34]). A hypothetical representation of the relationship between the likelihood of being remanded to custody and frequency/seriousness is shown in figure 2.

²¹ To an extent, this can be mitigated by specifying in your application for PNC data suitable time-frames to allow offences to be confirmed and by dividing up requests depending on what data are needed at what point in the study. E.g. if there is a need to establish early on whether those selected/not-selected for an intervention differ in some way, then requesting PNC data for the two groups early on in the project would allow this to be established. The follow up data could then be requested at a later date.

²² Murder, manslaughter, rape or attempted rape.

Figure 2: Probability of remand as a function of seriousness/persistence



The courts also consider the following during deliberations about bail: ‘the nature and the seriousness of the offence in question; the defendant’s social background, paying regard to the defendant’s character, antecedents, associations and community ties; the defendant’s previous history of compliance with bail conditions, and the strength of the evidence against the defendant’ (Player *et al.* [34:233]). Yet there is considerable discretion afforded to the courts in such scenarios and it seems unlikely that persistent and/or serious offenders would frequently ‘make bail’.

The proposition that those with previous convictions/arrests are more likely to be remanded is supported by a recent ‘thematic review’ on remand prisoners by Her Majesty’s Inspectorate of Prisons [20]. This found that ‘Half of all remand prisoners reported they had been in prison on two or more previous occasions’ (p.11). The Inspectorate’s report also highlights the increased probability of being remanded depending on the seriousness of offence, for example, 62% of Crown Court defendants were remanded to custody.

The Inspectorate go on to report that of those who responded to the HMIP survey ‘34% reported that this was their first time in prison’ (*ibid.*). This means we need to gain a handle on the largely unanswered empirical question as to how long those with different offending histories typically spend remanded to custody. For those awaiting trial or found guilty awaiting sentence, over half spend between 1-6 months in custody on remand (Her Majesty’s Inspectorate of Prisons [20]). This reinforces the view that

periods spent on remand require attention in reconviction studies as this appears to affect both first-timers and ‘old lags’.

6.5 Statistical issues

The statistically literate reader will also be aware of issues relating to ‘regression to the mean’, wherein extreme values at Time 1 are followed by a ‘natural’ decline (or increase) towards some person or group-specific mean value at Time 2. The best example of this is assessing the reoffending of persistent offenders. If someone whose offending behaviour is reaching a peak (as has been observed in the famous age-crime curve) is placed on an intervention, then we would expect their offending to decrease *anyway* owing to any number of possible reasons (e.g. an improvement in self-control, random variation, and so on). When assessing the effectiveness of a programme, particularly if no comparison group is used, then this ‘natural’ drop might be mistakenly attributed to the intervention. With a comparison group, what we would want to establish is whether the intervention group declined in their offending frequency faster than the comparison group. Gray *et al.* [18] do just this type of analysis, finding that a flagship intervention for persistent young offenders costing upwards of forty-five million pounds was no better than ‘treatment as usual’. The figures on p.55 of that report (reproduced below in figure 3) are an almost perfect example of regression to the mean.²³

We should not forget that there are more general statistical issues relating to sample size, statistical power, effect size, temporal or spatial clustering of data, amongst others, which should also be considered when undertaking reconviction studies.²⁴ For overviews of some of these issues, see the chapters in Piquero, A. R. and Weisburd, D. (Eds.) [33]; Weisburd *et al.* [46].

7 Conclusion

The author was first involved in collecting reconviction study data in 2001 for an evaluation of persistent young offenders (Moore and *et. al* [29]) and returned to this task only very recently. In that time, with some exceptions (e.g. Francis *et al.* [14]; Wilcox [48]; Friendship *et al.* [16]; Maltz [26]) it seems as though only limited endeavour has gone on in relation to publicising this core area of ‘real world’ criminological work, and very little specifically on PNC. As such, this paper was motivated by a desire to ‘expose’ a methodology for reconviction studies to critical review in the hopes that some form of standardisation might follow and that colleagues working in this area would not need to carrying on reinventing the wheel. This is not to say that this is the last word on the subject, but is an attempt to open up this ‘black box’ a little and demystify what is ostensibly a fairly straightforward undertaking.

²³ Note that in spite of this clear evidence, the programme continues to this day.

²⁴ Clustering is a particularly important but often neglected point - ignoring clustering results in biased standard errors and incorrect *p*-values, see Snijders and Bosker [42].

Figure 3: ISSP and regression to the mean

Figure 4.2 Trend in offending frequencies over 12 and 24 months (community groups)

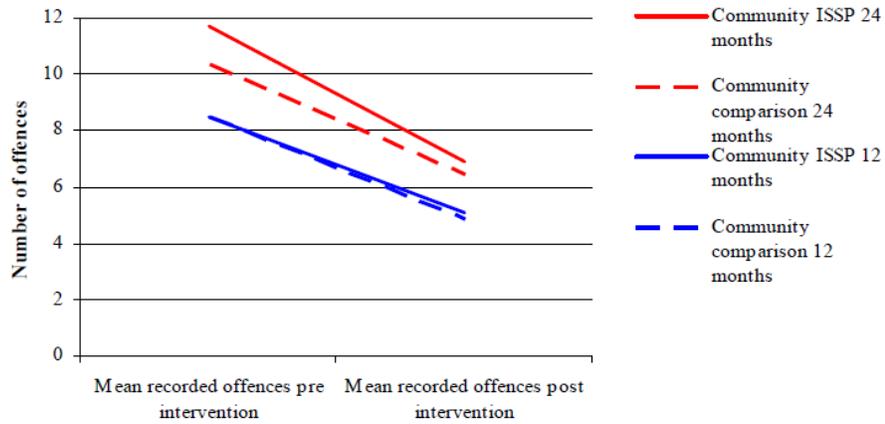
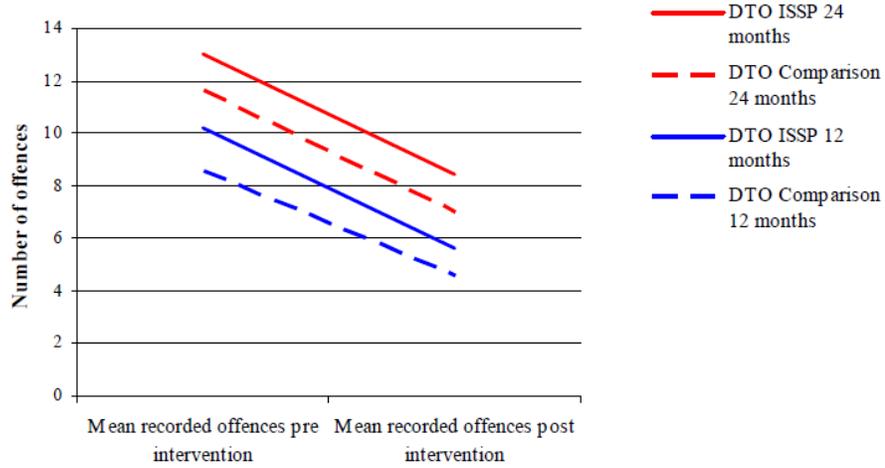


Figure 4.3 Trend in offending frequencies over 12 and 24 months (custodial groups)



To summarise, we have briefly covered some of the issues relating to outcome measures, underlying assumptions and approaches to reconviction studies. We have also ‘walked through’ an example of conducting a reconviction study using PNC data in order to highlight this data source, a methodology and also where possible, to bring working assumptions to the fore and flag potential areas of interest/concern. As the landscape shifts around us towards ‘Payment By Results’, the need for individuals who are able to conduct reconviction studies in academia, government and the third sector will increase. This is not to say that universities should simply ‘tool up’ graduates (or staff) to do governmental bidding, but an awareness of how such studies are conducted, their limitations and how they might be improved benefits everyone.

Minimum data checklist for reconviction studies In reviewing this area, and in addition to any measures individual researchers might use for a primary research study, it seems there are some basic requirements for conducting a reconviction analysis using routinely collected criminal justice data. These data are given below and where they might be sourced from in parentheses:

1. PNC data (Ministry of Justice [MoJ]).
2. Actual date(s) of release from prison for each custodial sentence (MoJ/National Offender Management Service [NOMS]).
3. Whether sentences were concurrent or consecutive. Francis *et al.* [15] state that this is included in PNC but may have to be specifically requested.
4. Intervention start date (depending on what the intervention is: MoJ / intervention provider).
5. OGRS3 / OASys / Asset data (MoJ / Youth Offending Teams [YOTs]). The Youth Justice Board does not collect Asset risk/need assessment data centrally so individual YOTs would need to be contacted for this.

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Appendix A: Applying for PNC data

Before you apply for PNC data first clarify that you can actually do a reconviction study (or rather, why you think you need PNC data). You might want to establish the offence patterns for the group of people you are working with/have data on. Equally you might be doing a full-blown randomised control trial of an intervention. Either way, if you want to look at *reconviction*, PNC data is the most reliable way of doing this.²⁵ Second, do you have consent forms for PNC / data sharing for those involved in the study? If no then it is very unlikely that you will be able to gain access to PNC data unless there is some special arrangement or circumstance which allows a third party to agree to it.²⁶ Once you are content that you need to use PNC data, applications should be directed to the PIAP review panel. In the application you will need to detail (amongst other things) what the study aims to do, why PNC data are needed, how you will protect the data when you receive it and a timeline for the project.²⁷

Perhaps the most important requirement is that there is someone from inside the Ministry of Justice sponsoring your application. If your application is approved, it will then be passed to the (MoJ) for the PNC data to be collated. The PNC section have quite limited resources and their work priorities are generally focused on internal requests from government departments (hopefully both situations will change as the ‘payment by results’ approach is adopted and the push for more data availability within government). But in short, these limitations mean that if you have a tight time-scale then applying for PNC should be one of the first tasks on your list. If you are interested in reconviction data you will also (likely) be interested in obtaining data from prisons and/or probation trusts. If this is the case (and one has a hard time imagining when it would not be true) then you would also be well-advised to, excuse the consultancy speak, ‘dovetail’ your application for PNC data with an application to the NOMS National Research Committee (NRC).²⁸

²⁵ If you are interested in *reoffending* then self-report would do just as well. Ideally, include both sources.

²⁶ E.g. if everyone involved in the study you are doing died many years ago then obviously consent would have to be sought from a third party. There are also exemptions for consent if a government department has commissioned the research and under s.251 of the NHS Act (2006) for health research (nigb.nhs.uk/s251).

²⁷ Requests for PNC data should be directed to acpo.pnc.piap.portfolio@hampshire.pnn.police.uk.

²⁸ Details on applications should be sent via the NOMS/MoJ National Research Committee website: <http://www.justice.gov.uk/publications/research-and-analysis/noms>.

Appendix B: PNC column headings and description

Those variables in **bold** are those used in counting up offences. More detail on some of these variables is included as supplementary information online.

- **Userref1 Unique reference No**
- **CASEID Court Case ID several offences could be dealt with on the same court occasion**
- **OFFENCEID Offence ID**
- CourtCautionAge Age at sentence
- CASETYPE Case Type
- **CTCAUDAT Court / Caution Date**
- **SENTENCEDAT Sentence Date**
- ACPO ACPO offence Code which used by police force
- CCCJS CJS Offence Code which use by court
- **STARTDAT Offence Start Date**
- ENDDAT Offence End Date
- FORCE Police force code which processes the case
- HOCODE Home Office Offence Code
- OFFCAT Home Office Offence Class
- PLEA Flag of Plea
- **DISP1 First Disposal Category**
- **DISPDUR1 First Disposal Duration** where sentence is community sentence or custodial sentence
- DISPAMT1 First Disposal Amount where sentence is fine
- HODIS1 First Home Office Disposal Code
- DISP2 Second Disposal Category
- DISPDUR2 Second Disposal Duration
- DISPAMT2 Second Disposal Amount

- HODIS2 Second Home Office Disposal Code
- DISP3 Third Disposal Category
- DISPDUR3 Third Disposal Duration
- DISPAMT3 Third Disposal Amount
- HODIS3 Third Home Office Disposal Code
- DISP4 Fourth Disposal Category
- DISPDUR4 Fourth Disposal Duration
- DISPAMT4 Fourth Disposal Amount
- HODIS4 Fourth Home Office Disposal Code
- **AdjudicationCode Adjudication Code**
- **PRIMARY Flag of primary offence** which indicates the main offence where multiple offences are dealt with on the same court occasion
- **LONGOFF full Offence description**

Appendix C: Breaches of civil orders resulting in criminal convictions

Current guidance on scoring OGRS3 within OASys NOMS [32] sets out the following breaches (of civil orders) which result in a criminal conviction, meaning that they should be counted. “Only count breaches when the breach itself constitutes a criminal offence: as of February 2008, these include breaches of conditions of injunctions against harassment (offence code 00829), Restraining Order (codes 00831 and 00839), Anti-Social Behaviour Order (code 00832), Sex Offender Order (code 06608), other orders for sex offenders (codes 066-17 to 20), Non-Molestation Order (code 06639), Control Order (code 06640), Domestic Football Banning Order (code 12530) or Individual Support Order (code 19613). Failures to comply with Reparation Order (code 19555), Action Plan Order (code 19556), Detention and Training Order (code 19560), early release (code 19566), supervision (code 19567) or Sex Offenders Act 1997 (codes 195-95 to 98) requirements are also criminal offences. In general, fully numeric offence codes indicate criminal offences, while those starting with a *B* are non-criminal.”

Appendix D: Worked example of counting back ‘at liberty’ using PNC data

An example of real data from the PNC is used here to highlight the ‘how to’ of working with this dataset. Below is the complete offence history for one individual. In this scenario, we are evaluating the impact of a post-release community programme which begins on the day of release from prison. This means that (separately from the PNC data) we have collected administrative records from the intervention provider about when they began working with each individual. For the purpose of exposition we initially assume that only half of a prison term is served ‘inside’, adjusting this assumption if necessary (i.e. if the administrative record or other PNC data suggests otherwise).

Counting back 12 months ‘at liberty’. As above, we are assessing the impact of a community-based intervention which begins on the day of release from prison. We know from administrative records collected from the intervention provider that this was the 11/03/11 (indicated by the dashed horizontal line).

- A naïve 12-month countback from 11/03/11 takes us to the 10/03/10, but we know that he was imprisoned at this point. So we have to look back to when this period of imprisonment started, that is, 21/12/09.
- We can see that he received a sentence of 630 days on 21/12/09, along with several other sentences for 540 days. As above, we assume these sentences are served *concurrently* rather than consecutively, meaning the sentence length was 630 days.
- With a 630 day sentence, the half way point would have been 01/11/10 as we initially assume only half the sentence is served in custody. *But*, we know from the intervention provider that he was actually released on 11/03/11 (and we are using that as the start date of intervention), meaning he served 440 (of 630) days in custody. That is, he was *continuously in custody* from 21/12/09 to 11/03/11.
- This means that the countback begins from the date of release (11/03/11) and actually goes back until the 20/12/08, which is 811 days, or roughly 2.25 years.
- We can see that he was in prison for some periods prior to 20/12/08, so we check whether any of these overlap with the countback dates. They do not, meaning that we count all offences (using the STARTDAT column) between 11/03/11 and 20/12/08. This is our 12 months ‘at liberty’ count-back period.
- Doing so yields four offences (all robbery) and no breaches. If we were interested in a simple yes/no measure of reconviction, this case would be a ‘yes’, but the details are lost. Lambda (λ) for this case is calculated by dividing the four proven offences by 12 months ($4/12 = 0.33$ offences per month).

Table 4: Offence history example

STARTDAT	LONGOFF	CTCAUDAT	DISP1	DISPDUR1	AdjudCode
21/07/2011	Theft Act 1968 Sec.8 Robbery.	16/03/2012	14-ImdCust	360	G
21/07/2011	Theft Act 1968 Sec.8 Robbery.	16/03/2012	14-ImdCust	360	G
03/10/2011	Theft Act 1968 Sec.8 Robbery.	16/03/2012	14-ImdCust	360	G
17/10/2011	Theft Act 1968 Sec.8 Robbery.	16/03/2012	14-ImdCust	1095	G
16/10/2011	Theft Act 1968 Sec.8 Robbery.	16/03/2012	14-ImdCust	1095	G
21/07/2011	Theft Act 1968 Sec.8 Robbery.	16/03/2012	14-ImdCust	360	G
21/07/2011	Theft Act 1968 Sec.8 Robbery.	16/03/2012	14-ImdCust	360	G
29/09/2011	Theft Act 1968 Sec.8 Robbery.	16/03/2012	14-ImdCust	630	G
17/08/2011	Theft Act 1968 Sec.8 Robbery.	16/03/2012	14-ImdCust	540	G
15/04/2009	Theft Act 1968 Sec.8 Robbery.	21/12/2009	14-ImdCust	540	G
15/04/2009	Theft Act 1968 Sec.8 Robbery.	21/12/2009	14-ImdCust	540	G
15/05/2009	Theft Act 1968 Sec.8 Robbery.	21/12/2009	14-ImdCust	540	G
13/06/2009	Theft Act 1968 Sec.8 Robbery.	21/12/2009	14-ImdCust	630	G
24/10/2008	Having possession of a controlled drug:-	27/02/2009	10-CondDis	360	G
02/08/2006	Theft Act 1968 Sec.8 Robbery.	15/11/2006	14-ImdCust	180	G
04/11/2004	Theft Act 1968 Sec.8 Robbery.	19/10/2005	14-ImdCust	540	G
28/10/2004	Theft Act 1968 Sec.8 Robbery.	16/09/2005	14-ImdCust	730	G
28/10/2004	Firearms Act 1968 Sec. 5	16/09/2005	15-Other		G
24/01/2005	Criminal Damage; £5000 or less;	08/02/2005	12-CommPen	180	G
25/01/2005	Criminal Damage; £5000 or less;	08/02/2005	12-CommPen	180	G