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Incarceration and Desistance: Evidence from a Natural Policy Experiment

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FINAL REPORT

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PROJECT SUMMARY

There is widespread recognition that incarceration rates and criminal justice supervision more broadly are exceptionally high in the U.S. (National Research Council, 2014). As a result, a growing number of states are attempting to slow the growth in incarceration and decrease the size of the incarcerated population. Some efforts seek to divert individuals convicted for non-violent offenses from incarceration entirely while others seek to reduce time sentenced or time served among those sentenced to incarceration. More than 30 states have adopted sentencing reforms since 2007 (PEW Charitable Trusts, 2016). As of 2020, the nation has experienced a 25% decline in the prison population since its peak in 2009 (Carson 2021). During the same period, the nation has witnessed a 32% decline in crime rates (FBI 2022), raising the possibility that the decreased use of prison does not decrease public safety (Gelb & Denney, 2018). While the trends are provocative, more evidence is needed at the individual-level.

At the heart of the debate is whether incarceration is simply a form of incapacitation during time served, whether incarceration deters individuals from future offending, or conversely, whether there is a criminogenic effect of time spent behind bars (Bushway and Paternoster, 2009; National Research Council, 2014). While competing theories relating to incapacitation, deterrence, desistance, and the criminogenic effects of prison abound, strong conclusions based on rigorous empirical evidence are rare (Miles and Ludwig 2007; Loeffler and Nagin 2021). This is particularly the case for the dose-response effect of incarceration among adults. This is a notable gap for two reasons. First, changes to sentence length may be a more politically feasible policy recommendation for many offenses relative to the recommendation to eliminate incarceration. And secondly, a reduction in sentence length for adults may, in theory, be applicable to a larger share of offenders than diversion simply because the lack of public

support for diversion could result in relatively fewer offenses (and hence fewer people) eligible for such reforms. The fundamental issue is that diversion may be considered insufficiently punitive and so such policies may be more limited in scope versus shorter sentence lengths (Petersilia, 1999; 2011). As a result, sentence length reductions might have a larger impact with respect to reducing the incarcerated population and related costs.

Among the few rigorous studies that consider the dose-response effect of incarceration, some studies find a null effect while others find a negative association between length of incarceration and recidivism – that is, longer sentences are associated with somewhat lower recidivism. Some researchers have argued that heterogeneity across offenders, regimes and sentence length may play a role in the mixed findings (e.g. Estelle & Phillips, 2018; Mears et al. 2016). But others find little evidence for heterogeneity and still others have produced divergent findings even within the same data and regime, suggesting a role for methodological differences as well (Rhodes, Gaes, Kling, & Cutler, 2018; Roodman, 2017). In this grant, we approached this issue with two distinct studies.

STUDY 1

Major Goals and Objectives

In this study, we used a rigorous quasi-experimental approach to improve our understanding of how sentence length influences time to next arrest. This focus on time to next arrest with a focus on desistance, rather than recidivism. The goal is to use a model that allows us to identify whether the trajectory of offending changed when people received shorter lengths of incarceration.

Research Design

Specifically, we estimate an instrumental variable hazard model using exogenous variation resulting from South Dakota's Public Safety Improvement Act of 2013 (hereafter, SB70). SB70 was a major sentencing reform that reduced the likelihood of incarceration and the length of sentence for certain non-violent offenses. The instrumental variable approach exploits this plausibly exogenous variation in sentence length associated with SB70 to estimate the relationship with time to re-arrest. For grand theft below $5,000 and third-degree burglary, the reform reduced the severity of the felony class and the maximum allowed sentence length by 50%-80%. For these offenses, the reform provides an opportunity to address the important question of how sentence length influences time to rearrest by utilizing this exogenous variation. In addition, only certain offenses were subject to sentencing reductions such that unaffected offense types can also be included in the analysis.

Our modeling approach is a novel application of an instrumental variable hazard model and provides among the most rigorous evidence of the relationship between sentence length and recidivism to date for several reasons. First, this study utilizes the time to next arrest instead of a dichotomous indicator or count of re-arrest during a pre-specified follow-up period. This takes full advantage of the timing of events, addresses the fact that those exposed to SB70 will necessarily have a shorter follow-up period due to the timing of SB70, and avoids the choice of an arbitrary follow-up period. Second, the model leverages exogeneous variation in sentence lengths resulting from SB70. This allows for identification of the relationship between sentence length and time to next arrest by using SB70 as an instrumental variable, and only requires a set of assumptions that are likely to hold in the data. Finally, the inclusion of offenses that were unaffected by SB70 allows for robust control of secular trends unrelated to SB70 in sentence lengths and/or desistance.

Data

South Dakota's Computerized Criminal History System contains individuals' complete arrest, disposition, and sentencing history. Our analysis focuses on arrests between 2009 and 2015; we refer to the arrest that got them into the sample as the "index" arrest. For individuals arrested from 2000 onward, the dataset contains all criminal justice interactions going back to the 1980s (sometimes earlier) through May 31, 2020. For each index arrest, we constructed a comprehensive set of characteristics such as the number and type of offenses on the arrest (e.g., violent, drug, property, other), felony status and felony conviction, and various indicators of time sentenced over all offenses on the arrest. Other measures capture the criminal history as indicators and running counts of events prior to the current arrest: number of prior arrests, prior conviction, and prior prison. The data also capture the arrest date and county, which allows us to generate time-to-event measures and jurisdiction indicators. Demographic characteristics include race/ethnicity, sex, and birthdate.

The outcome for our analysis is time to next arrest in days (i.e., days between the index arrest and the subsequent arrest for those who were arrested again). Outcomes were censored on May 31, 2020, the last arrest date present in our dataset. The treatment in our analysis, sentence length, was defined as the sum of the sentence lengths for all convicted charges, in units of years. Because individuals may be charged and convicted for multiple offenses, the sum of sentence lengths for the treated group may include non-SB70 eligible offenses in addition to at least one SB70-eligible offense. The instrument, exposure to the SB70 policy, was defined as (a) arrest occurred after the introduction of the SB70 policy on July 1, 2013, and (b) arrest resulted in a conviction for at least one treated property offense impacted by SB70 (i.e., offense with relevant SDCL code 60 or 62). We adjusted for variables related to time of index arrest, demographic characteristics of the arrestee, details of charges related to the index arrest, and criminal history of the arrestee.

Our analysis sample included arrests in South Dakota between January 1, 2009, and December 30, 2015, that resulted in a conviction for (A) at least one SB70-eligible grand theft or third-degree burglary offense without a homicide offense (SDCL codes 60 or 62, as the treated group) or (B) at least one other burglary offense not impacted by SB70 (i.e., non-third degree burglary as the comparison group). Thus, our sample included arrests with sentences that were impacted by SB70 and a comparison group of arrests resulting in a conviction for similar offenses with sentences that were not impacted by SB70. Some individuals contributed more than one unit to our analysis; however, most individuals (88%) had only one relevant arrest during the analysis period.

Methods

To estimate the effect of sentence length on time to next arrest, we used instrumental variable survival analysis with exposure to the SB70 policy as the instrument (Martinussen, Nørbo Sørensen, & Vansteelandt, 2019). Unbiased estimation of a hazard ratio using an instrumental variable is feasible with G-estimation techniques (Martinussen et al., 2019), and we are the first study of criminal recidivism to utilize the approach. Beyond the advantages described in a previous section, this approach does not require the monotonicity requirement – an important feature for our analysis, given that a few cases of grand theft above $100,000 transition to a more severe felony class (Hernán & Robins, 2006). Three assumptions are required for a variable to be an appropriate instrument. These are: (1) the instrument is associated with the treatment; (2) the instrument causally impacts the outcome only through treatment; and (3) there are no unmeasured confounders of the relationship between the instrument and outcome (Hernán & Robins, 2006). The SB70 policy is an appropriate instrument because (1) the policy impacted sentence length; (2) the policy is unlikely to causally affect time to rearrest except through sentence length as the policy impacted only sentence lengths and no other features of the justice process for the offense types included in this analysis, and; (3) unmeasured confounders of the relationship between SB70 and time to next arrest are unlikely, since implementation of the policy was determined only by timing of arrest and type of charge, both of which are measured.

Our analysis sample included arrests in South Dakota between January 1, 2009, and December 30, 2015, that resulted in a conviction for (A) at least one SB70-eligible grand theft or third-degree burglary offense without a homicide offense (SDCL codes 60 or 62, as the treated group) or (B) at least one other burglary offense not impacted by SB70 (i.e., non-third degree burglary as the comparison group). Thus, our sample included arrests with sentences that were impacted by SB70 and a comparison group of arrests resulting in a conviction for similar offenses with sentences that were not impacted by SB70. Some individuals contributed more than one unit to our analysis; however, most individuals (88%) had only one relevant arrest during the analysis period.

The main analysis sample consists of 2,616 arrests from January 1, 2009, to December 31, 2015, with a conviction for grand theft or burglary (Table 2). Among those arrests, 1,727 were charged with a treated offense (i.e., grand theft or third-degree burglary), while the remainder were charged with other burglary. The time to failure ranged from one to 4,168 days with a median of 574 (interquartile range: 114 to 1,992). The share of arrests with a subsequent arrest during follow-up was 75.0% (N = 1963) with a median time to re-arrest of 274 days (interquartile range: 76 to 877). The shortest follow-up time post SB70 was 4.5 years. The longest was almost 11 years.

Main Results

The analysis focuses on time to next arrest, which is better suited to our data and research question, as it avoids specification of an arbitrary follow-up period to assess re-arrest. It also allows for direct handling of censoring since those exposed to SB70 will necessarily have a shorter follow-up period since the sentencing reforms took effect in the middle of the study period. The focus on time to next arrest differentiates this analysis from more common approaches in this literature, which focus on dichotomous or count outcomes such as whether an individual is arrested or the number of times arrested during a follow-up period (e.g. 5-years).[1] The latter is complicated by our inability to measure time served in prison or jail limits the utility of that approach (Eggleston, Laub and Sampson 2004).

The main analysis for the full sample of convicted individuals produces a hazard ratio of 0.99 (CI: 0.95-1.03). This would represent an estimated 0.9% decrease in the hazard of re-arrest for every one year increase in sentence length, but is insignificant. Covariates that show a significant relationship to the hazard of re-arrest include age at arrest, age at first arrest, number of prior arrests, number of prior drug felony arrests, current drug charges, and prior prison time.

A critical part of the instrumental variable analysis is the strength of the instrument. As described above, SB70 mandated reductions in maximum sentence length for these particular

[1] Counts of arrests during follow-up suffer from an additional concern as subsequent arrests are not subject to the natural policy experiment.

offenses, which would support a valid and strong instrument, in theory. Empirically there is indeed a substantial decline in sentenced time coincident with SB70's implementation in July 2013 for the treated offenses. Further, there is a strong negative relationship between time sentenced and the policy instrument consistent with the SB70's mandate to reduce sentenced time for individuals convicted of these particular offenses.

The analysis has several important features: 1) the outcome is time to next arrest, which does not require us to choose an arbitrary follow-up period to assess re-arrest but rather takes full advantage of the data by considering the timing of re-arrest, if any, for all individuals through May 31, 2020; 2) the follow-up period is long – almost 7 years; 3) the instrument leverages the exogenous shift in sentencing for felony grand theft and felony third degree burglary as a result of SB70; and 4) the model includes arrests not subject to the sentencing reforms of SB70, which allows for better identification of secular trends in the patterns of rearrest after the sentencing reforms of SB70.

On net, we do not find support for either a criminogenic or deterrence/incapacitation effects of longer sentences. The models indicate that longer sentences are not associated with any changes in the overall rates of survival. The hazard ratios themselves are very close to 1, and all are statistically insignificant. The confidence intervals are tight enough to rule out moderate changes in timing to next arrest: The confidence interval from our main analysis excludes changes in the hazard of re-arrest of larger than 5% for every one-year increase in sentence length. The instrument used in in this analysis was strong, providing assurance that this confidence interval is trustworthy. The results are robust to multiple sensitivity analyses that modify the sample as well as model specification.

Our null finding could mean that longer sentences do not affect the timing of future offending because there is no long-term criminogenic effect of prison. Alternatively, it is possible that the criminogenic effects of prison are offset by the deterrence/incapacitation effects. The results are robust to a number of sensitivity analyses that modify the sample as well as model specification. Overall, our result supports efforts like SB70 that seek to shorten sentence lengths among those who are incarcerated – these shorter sentences do not change the rate at which people desist in the long run. Our results are most relevant for relatively short sentences among serious property offenses. The follow-up period of close to almost seven years post-SB70 produces a great deal of confidence in the answer.

Our results are similar to the rearrest results reported most recently by Rose and Shem-tov (2021). They find that small increases in incarceration, particularly for those sentenced to relatively short sentences, can have an incapacitative impact for rearrest. However, they find that over time the cumulative difference in rearrest decreases for those with different sentence lengths, such that it is close to zero by the 8th year after sentencing.

Limitations

First, we caution that the results may not be generalizable to other offense types and other settings in the presence of heterogeneity such as that documented in prior studies (e.g. Estelle & Phillips 2018, Orsagh and Chen 1988, Rose & Shem-Tov 2021). Our findings are based entirely on individuals convicted for grand theft and third degree burglary and who serve relatively short sentences. According to South Dakota administrative reports for FY2015, SB70-eligible offenders serve only a small share of their sentences: seven of 53 months for third degree burglary and five of 29 months for Class 5/6 grand theft. It seems reasonable for future studies to consider whether incapacitation, deterrence, and criminogenic effects differ with length of stay. A relatively short stay may not be as damaging to family, employment, and other ties in the community and may not be sufficient to produce an incapacitation effect. On the other hand, if simply the experience of incarceration is a deterrent, then even five months may be sufficient.

Second, we observe time sentenced but not actual time served. The main advantage of a focus on time sentenced is that this is an observable policy lever. However, we are particularly interested in the impact created by time served in prison (Bushway and Owens 2013). We have investigated a variety of sources that examine SB70 which providing supporting evidence that time served was also reduced as part of SB70 (Elderbroom et al., 2016; Public Safety Improvement Act Oversight Council, 2018). The short-term drop in survival rates post-SB70 shown in Figure 3A is also consistent with a decrease in time served, reflecting an incapacitative effect in the short run (Elderbroom et al., 2016; Public Safety Improvement Act Oversight Council, 2018).

Third, we are unable to completely isolate grand theft offenses with a reduction in the severity of their classification. This would attenuate the relationship between SB70 and sentenced time, yet we nevertheless document a large and significant reduction in sentenced time relative to the comparison group. Sensitivity analyses to remove arrests for offenses not subject to the sentence reduction yield similar results.

Fourth, our study suffers from a limitation typical in this literature in that we do not observe criminal offending. Instead we observe arrest, which is a useful proxy for offending (Junger-Tas & Marshall, 1999). Involvement with the criminal justice system, which carries with it a host of additional consequences, is also an important outcome in its own right.

Fifth, this is not a randomized control trial. Given the infeasibility of randomly assigning individuals to different sentences, we rely on quasi-experimental methods to address limitations common in the literature. Such interventions have been used previously and SB70's effect on time sentenced is similarly sharp and substantive as to limit alternative explanations. The combination of a natural policy experiment combined with comprehensive criminal justice data for a statewide census of arrestees permits advanced econometric techniques to reliably estimate the effects of incarceration.

Finally, results from South Dakota may not generalize to other settings. We are reassured somewhat by the fact that South Dakota experienced similar increases in incarceration and criminal justice populations as the rest of the country. We are also reassured by the fact that the implications of our findings are similar to other studies that focus on purposeful policies such as Rose and Shem-Tov (2021) in North Carolina and Rhodes et al. (2018) in the federal system.

Participants

Shawn Bushway, Marika Booth, Matthew Cefalu, Beau Kilmer, Nancy Nicosia, and Emma Thomas. All were researchers at RAND when they worked on the project.

Products

In July 2022, we resubmitted our paper entitled "Do Shorter Prison Sentences Influence Time To Re-Arrest? to the *Journal of Policy Analysis and Management (JPAM)* after conducting extensive revisions in response to review comments received in February 2022. We expect to hear a decision from the editors soon. *JPAM* is a leading journal in the field of public policy and it would represent a major accomplishment to publish the paper in that journal. We could receive a second-round revision, which we would address immediately.

STUDY 2

Major Goals and Objectives

The second study explores methods to extend the research in the first study beyond the hazard spell that immediately follows the incarceration spell. The study of desistance, which is the study of individual variation in behavior over time, has been plagued by definitional and methodological inconsistencies that reduce the study of desistance to the study of between-individual, rather than within individual-individual changes in (Bersani and Doherty, 2018; Bushway, 2021; National Research Council, 2007; Rocque, 2017, 2021).

This study attempts to move the field move forward with a more explicit conceptualization of individual behavior to enhance the understanding of within-individual variation in long term patterns of individual behavior (Sampson and Laub, 2016). Lambda, the rate of offending, was isolated by criminal career researchers to capture the distinction between prevalence and frequency of offending, two key parameters in the description of the criminal career. In retrospect, lambda was not intended to describe within individual differences in offending over time. Instead, it was explicitly created to explain between-individual differences in offending. In fact, in the most famous conceptualizations of the criminal career paradigm (Barnett, Blumstein and Farrington, 1987), lambda does not change over time. The goal instead is to show how the aggregate age crime curve can be explained by different distributions of constant lambdas and participation rates. Even the exit parameter doesn’t change over time for any given individual.

The problem isn't only in the types of questions asked by criminal career researchers. Lambda itself as a variable is not constructed to describe change in the underlying probabilities of sequences of criminal activities (Hagan and Palloni, 1988). It is at best an ambiguous summary measure of dynamic processes that generate criminal events. In the words of Hagan and Palloni (1988), "examinations of age-crime curves across periods and cohorts have provided only crude proxies for the explanatory test that are ultimately required (p.98)." It is more than a little eerie to note the correspondence between the critique offered of studies that focus on lambda in 1988 and then again in 2016 (Sullivan and Piquero 2016).

Research Design

In this study, we argue that the solution has been there all along, hidden in plain sight in the Hagan and Palloni (1988) comment on the debate between Blumstein and Gottfredson and Hirschi. Hagan and Palloni conclude with the following statement:

> We have argued that a renewed research agenda is best developed through a conceptualization of crimes as social events that are embedded in the life course and best analyzed *through the methods of event history analysis* (emphasis added) … These issues of conceptualization and operationalization are in our judgment more important than some of the more specific questions emphasized thus far in this debate (pg. 96).

It might be an interesting exercise in its own right to explore why this fundamental insight has been lost in the ensuing years. However, we suspect that the answer is itself a bit mundane – the types of recurrent event hazard models that Hagan and Palloni (1988) are suggesting are computationally intensive and require data which allows the research to calculate the timing between events. The prospective data created in response to the debates spawned by the criminal career paradigm did not have dates and computational power was quite limited in 1988. Neither of these things is true today. Growing tranches of administrative data of criminal justice involvement do have dates, and multievent hazard modeling has developed extensively both computationally and theoretically. Indeed, there are now a wide array of models available for estimation using any number of statistical packages.

We avoid a narrow focus on one particular statistical method. Instead, we focus on how the reconceptualization of the problem of lifecourse criminology as the study of order-specific hazards that characterize the entire process of interest, which we describe using convictions in the criminal justice system (other possibilities, like arrests or even discrete crimes, are clearly possible). The use of event history methods to model the probabilities of sequences of criminal justice involvement—instead of the rate of offending, lambda— will bolster the power of hypothesis testing focused on the covariates that drive within-individual variation in offending over time. We have already seen some hint at the conceptual power of hazards. The assumed inevitability of reoffending motivated expanded use of background checks, but hazard mode estimates show that some previously justice-involved people do eventually have very low rates of offending that resemble the general population (Kurlychek, Brame and Bushway, 2006; Blumstein and Nakamura, 2009; Bushway, Nieuwbeerta and Blokland, 2011). However, these single event hazards do not speak to the underlying interest in the within-individual variation of behavior over the life course.

In this study we apply the logic of recurring event hazard models that predict the probability of events over time with two distinct objectives: identify desisters and estimate the impact of life events on the longitudinal pattern of offending using administrative data from North Carolina. We connect commonly used trajectory models to recurrent event survival models.

Data

We use a dataset on criminal histories in North Carolina. Public data on convictions, sentences, and periods of incarceration in prison for all North Carolina Department of Public Safety (NCDPS) Division of Adult Correction offenders were obtained from the NCDPS Offender Public Information Search/Inmate Locator website.[2] Three files were downloaded: (1) the Offender Profile file, which contains demographic information on offenders; (2) the Sentence Component file, which lists individual sentences and associated information for each conviction, and; (3) the Sentence Computation file, which lists dates of incarceration. These files included records associated with convictions from 1972, the earliest available in the database, up until the download date, April 8th, 2021.

In our analytic sample, we included only times at risk of new conviction following convictions or release from institutional corrections that occurred and limited the analysis to risk periods following convictions from January 1995 to November 2019. After November 2019, the law surrounding age of eligibility for conviction in the adult criminal justice system changed and more recent data were affected by changes related to the outbreak of COVID-19. We also excluded convictions that happened while the offender was incarcerated because the mechanisms leading to convictions in prison are likely to be different from other convictions.

For illustrative purposes, we restricted our sample to individuals born in 1979. Focusing on a particular birth cohort means offender age and calendar time are almost perfectly correlated. This is an advantage for some analyses— e.g., when studying treatment effects as it allows us to control for age and secular trends simultaneously— but a disadvantage for other analyses, e.g., those interested in the causal effect of age, since this is completely confounded with calendar

[2] See https://webapps.doc.state.nc.us/opi/offendersearch.do

time. The 1979 birth cohort turned 16 and hence became eligible for adult conviction in 1995 and would be 40 years old at the end of the study period in 2019. Because we did not have access to death data we could not determine exactly when an individual was no longer at risk for conviction due to death. However, deaths are likely to have less impact on our analysis since approximately 95% of men and 97% of women live beyond 40 years in North Carolina (Arias et al., 2018).

The unit of analysis in this study is the risk periods, observed potentially many times per person, and the outcome variable is the length of the risk period, i.e., the time between prison release (or previous conviction, if no incarceration occurred) and the next conviction, in years. Risk periods are censored at November 2019, the end of the study period. The independent variables included in the analyses include individual demographic information, measures of the frequency and variety of prior convictions, and measures of prior incarceration spells.

Methods

We use a recurring hazards model calculating individual-level frailties to predict the probability of desistance, which we define to be 1–P(Conviction). This "on/off switch" definition of desistance is parsimonious and consistent with prior work (e.g., Barnett, Blumstein & Farrington, 1989; MacLeod, Grove & Farrington, 2012; Schmidt & Witte, 1988) and compatible with studies of desistance using quasi-experimental methods (e.g., Doleac, 2022; Estelle and Phillips, 2018; Harding et al., 2018) that arose from the credibility revolution in applied microeconomics (see Angrist & Pischke, 2010). The definition is less aligned with the concept of desistance as applied in life course crimonology. Life course studies typically use trajectory models to graph changes in offending rates over age, most often as rates for aggregates of individuals for whom offending patterns are similar over time (group-based trajectory models) or by assuming a continuous distribution of parameters that are fixed within groups (growth curve models). In this approach, desistance is not a parameter of the model – it is the description of a subset of trajectories that approach zero before the person dies (Sampson and Laub, 2003; Bushway, 2013).

The repeat hazard model with frailty that we use estimates individual-level predictions based on the relationship between time to re-conviction and observed covariates, conditional on time at risk. In all hazard models, individuals are observed over many points in time, and the frailty model considers these time units to be nested within individuals, which is analogous to a hierarchical or nested models (Balan & Putter 2020). The frailty parameter estimate calculated in the model for each individual is a multiplicative factor by which the predicted hazard is adjusted to account for individual-specific latent information.

We fit a Cox proportional hazards frailty model using a Breslow estimator to predict recidivism probability and its complement, desistance probability, as a function of the independent variables, including age at conviction, gender, criminal history, and frailty. The association of time to reconviction with each of the independent variables is represented as the hazard ratio associated with each of the covariates and is interpreted just as a conventional Cox model's estimates are. The distribution of individual frailties represents the variation in unobserved factors affecting reconviction probability.

Based on the fitted model, we define an individual as a "likely desister" if their probability of desistance exceeds 90 percent over a five-year period, though the choices of time period and the desistance threshold are left up to the analyst. These might depend on factors such as the severity of the crime, with more severe crimes calling for larger time periods or higher desistance probabilities.

Main Results

Table 1 displays descriptive statistics on unique offenders in the NCDPS data. We present both the full sample and the analytic sample of individuals who were born in 1979.

The sample includes 27,300 unique individuals with at least one conviction who were 25% female, 40% Black, 47% white and 13% other race. These individuals comprised 68,623 risk periods, defined as spells of time between release and next conviction or the censor date. The median age at first conviction was 22 years. 50% of offenders contributed one conviction event to the dataset and 65% were never imprisoned during the study period. During the study period, offenders were at risk for a conviction for a median of 16.2 years (IQR 11.1 to 19.9); this includes all time in community following any convictions.

Sixty-two percent of the risk periods ended in reconviction. For the most recent conviction date prior to each risk period, 14% included a violent conviction, 25% included a property conviction, and 35% included a drug conviction. Additionally, 36% included a felony conviction and 52% included a misdemeanor. For the conviction events that resulted in a prison sentence, the median sentence length was 0.6 years (IQR 0.3 to 1.2). At the start of each risk period, the median age was 26.5 years. Note that we distinguish convictions (unique charges resulting in conviction) from conviction events (a date on which the individual was convicted of at least one, and possibly more than one, crime).

Table 1. Risk period-level summary statistics (n=68,623 based on 27,300 individuals)

Variable[a]	**1979 Cohort**	
Risk period ended with reconviction	42,201	(61.5%)
Any violent convictions	9,644	(14.1%)
Any property convictions	17,185	(25.0%)
Any drug convictions	24,195	(35.3%)
Any incarceration	18,097	(26.4%)
Number of prior incarcerations		
0	50,033	(72.9%)
1	10,259	(14.9%)
2+	8,331	(12.1%)
Number of prior conviction events		
1	27,300	(39.8%)
2	13,736	(20.0%)
3	8,860	(12.9%)
4	6,023	(8.8%)
5+	12,704	(18.5%)
Number of felonies		
0	44,093	(64.3%)
1	14,023	(20.4%)
2+	10,507	(15.3%)
Number of prior felonies		
0	45,547	(66.4%)
1	4,861	(7.1%)
2+	18,215	(26.5%)
Number of misdemeanors		
0	23,571	(34.3%)
1	34,716	(50.6%)
2+	10,336	(15.1%)
Number of prior misdemeanors		
0	33,207	(48.4%)
1	11,397	(16.6%)
2+	24,019	(35.0%)
Time incarcerated (if any), median (IQR)	0.6	(0.3, 1.2)
Years at risk, median (IQR)	3.1	(0.9, 9.7)
Age at start of risk period, median (IQR)	26.5	(21.8, 32.1)

Table 2 provides the preliminary results of the Cox frailty model. We estimate that any incarceration from the prior conviction is associated with a decrease in the risk of subsequent reconviction (HR=0.782; 95% CI = (0.804,0.760)). This result must be interpreted with care. The model used here is meant to be illustrative, and it does not carefully and comprehensively control for potential confounding. We further find evidence that the frailty variance parameter,

$\ln(\hat{v})$, is statistically different than zero. Under a strong independence assumption, this indicates that a potentially large portion of variation in desistance probability is due to individual-specific risk factors.

Table 2. Cox proportional hazards frailty model estimates (n=68,623)

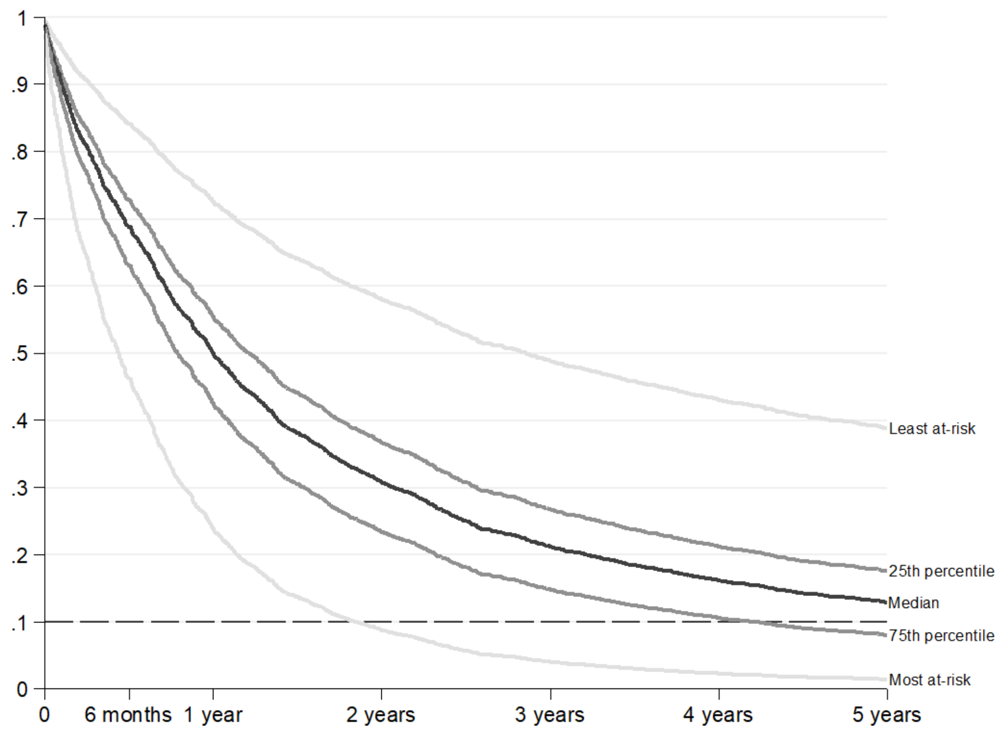
	Hazard Ratio	Standard Error
Any incarceration	.782**	(.011)
Age at release	.974**	(.002)
Total prior time incarcerated	.980**	(.006)
Number of convictions		
1	1.000	(.000)
2	.922**	(.018)
3	.845**	(.021)
4	.766**	(.022)
5+	.648**	(.023)
Age at first conviction	.967**	(.003)
Gender		
Female	1.000	(.000)
Male	1.414**	(.032)
Race		
Black	1.000	(.000)
Other	.384**	(.013)
White	.853**	(.016)
Number of prior felonies	1.030**	(.004)
Number of felonies	1.134**	(.006)
Number of prior misdemeanors	1.016**	(.003)
Number of misdemeanors	1.194**	(.009)
Any violent convictions	.875**	(.017)
Any property convictions	1.015	(.016)
Any drug convictions	.900**	(.013)
$\ln(\hat{v})$	.265**	(.024)

** p<.01 * p<.05

To demonstrate the heterogeneity present in the North Carolina dataset *after* explicitly controlling for factors associated with recidivism, Figure 1 shows the empirical distribution of reconviction probability over five values of the estimated frailty parameter, holding all else constant. These curves represent a theoretical individual that is the most at-risk of reconviction (i.e., maximum frailty value), the 75th percentile, median, 25th percentile, and least at-risk (i.e., minimum frailty value). The dotted line at .1 marks the threshold probability to indicate a likely desister.

The frailty estimates show that, even after controlling for important predictors of reconviction, there is nearly a 40-percentage point difference in reconviction probability between the most and least at-risk in the data. It further illustrates the changing risk within individuals over time, where the most at-risk exhibits a rapidly shrinking probability of desistance as the curve approaches zero while the least at-risk exhibits a decline that is closer to linear.

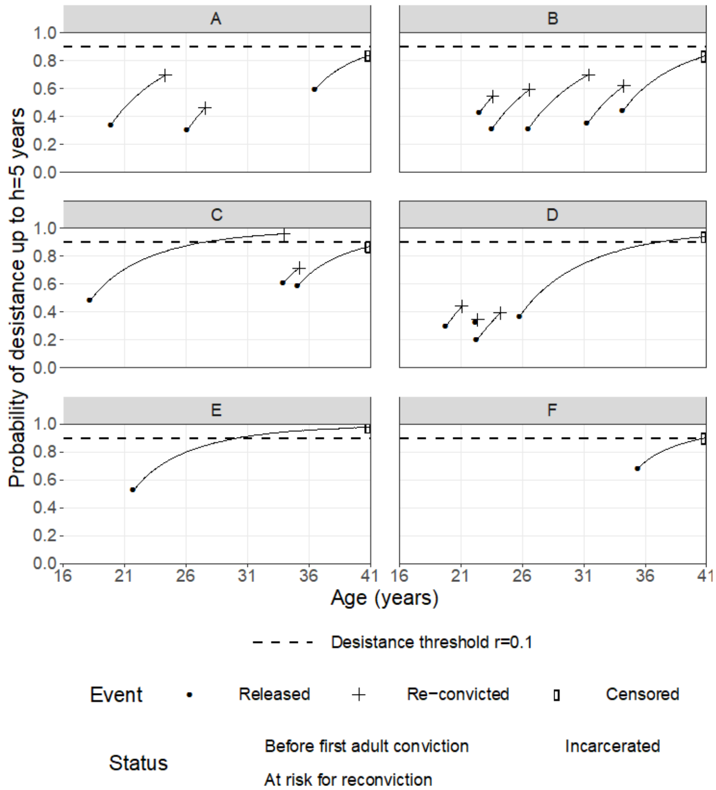
Figure 1: Distribution of survival probability over values of estimated frailty parameter



We can combine the joint effect of observed individual characteristics with individual-level frailty to predict desistance in the sample based on the probabilities estimated by Equation 2. Figure 2 shows the probability of desistance (1–Pr(conviction)) up to 5 years into the future, as a function of age for six individuals in the data (labelled A through F). As shown in the figure, for all individuals, desistance probability increases with t, the time since becoming at risk for a new conviction. These patterns indicate that individuals' probability of desistance is not static; risk varies by current circumstances and temporal distance from a prior offense, and latent risk (measured as frailty through observed prior patterns of criminal involvement).

Individuals A and B are examples of offenders who never showed evidence of desistance during the study period. Individual A has 3 convictions between ages 16 and 41, and is incarcerated twice. They are never in the community for long enough to be considered a desister; that is, the probability of avoiding re-conviction over the next 5 years never goes above 0.9. Individual B had 5 convictions between ages 16 and 41 but was never incarcerated. For this person, frequent convictions meant they were never classified as a potential desister. Individuals C through F all showed evidence of desistance at some point between ages 16 and 41. Individual C had one conviction around age 16, and then were not re-convicted for around 20 years meaning they were temporarily classified as a likely desister, until they committed two new offenses in their mid-thirties. Individual D, by contrast, had four convictions in their teens and early twenties, and then had no further convictions, meaning they were eventually classified as a desister. Individual E committed one crime around age 19, was in prison for a few years, but had no convictions after release, meaning they were classified as a desister around age 30. Finally, F had their first conviction relatively late, around age 35, and committed no further crimes up to age 41, at which point they were relatively quickly classified as a desister. The latter likely happened because older age is associated with a lower rate of re-conviction, which is accounted for by the model.

Figure 2: Method to identify likely desisters from criminal histories of six individuals



The final steps of this analysis are to display changes over time in the impact of life events that are associated with desistance and to compare and contrast the information generated by this model with more common group-based trajectory models.

Limitations

This study is intended to illustrate the analytic potential of an event history model that estimates within-individual changes in desistance probability over time. We do not purport to generate causal estimates of the impacts of incarceration or conviction on desistance; other papers on causal inference in survival models using potentially more robust estimation strategies exist but are focused narrowly on epidemiological outcomes.

The limitations discussed for Study 1 are applicable here as well. Additionally, we use absence of a conviction as an indicator of desistance over time. Valid arguments against the use of conviction data in this way—including the introduction of racial bias in risk of arrest, charge, and plea bargaining, may confound conviction as a measure of delinquency or criminal activity. However, arguments against alternate measures also exist, and replication of the method we describe here using alternative measures is important.

We also choose a threshold probability of reconviction of 0.10 to indicate likely desisters to illustrate the model and replicate the logic that plays out among justice system actors deciding on sanctions after conviction. This choice is arbitrary. The desistance probability threshold is external to the model, so can be varied easily.

Products

A manuscript is in preparation for submission to *Criminology* in January 2023.

Participants and other collaborating organizations

Our original statisticians for the project, Emma Thomas and Matthew Cefalu, both left RAND in the Spring of 2022. We asked Greg Midgette, a former RAND colleague who is now a professor at the University of Maryland, to join the project.

Changes in approach from original design and reason for change, if applicable

We originally thought we would use the South Dakota data for Study 2 but realized that was not possible because it was missing data on incarceration spells. We then pivoted to using the North Carolina data to which we already had access for other projects.