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Document Title:	**The Relationship between Prison Length of Stay and Recidivism: A Study using Regression Discontinuity with Multiple Break Points**
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This BJS grant-funded report evaluates the relationship between prison length of stay and recidivism post-release. It examines Federal sentencing structure to determine if increasing the length of prison terms increases or decreases recidivism, and seeks to measure if the effect of increasing the length of prison differs across individuals.

The Relationship between Prison Length of Stay and Recidivism: A Study using Regression Discontinuity with Multiple Break Points

Imprisonment is an expensive sanction. Justifying its use often rests partly on its presumed utility to reduce post-release reoffending. Most scholarship separates the research on imprisonment effects into two subsets: imprisonment in contrast to an alternative sanction and prison length of stay (Nagin, Cullen, Jonson, 2009; Smith, Goggin, and Gendreau, 2002). If prison is expected to deter offenders from future reoffending, then how does it compare to a sentence of probation, home confinement or other alternative sanction? Likewise, if prison is chosen as a preferred sanction, can a deterrent effect of imprisonment be achieved with a shorter sentence?

This paper capitalizes on federal sentencing structure to evaluate this latter question: Does increasing the length of prison increase or decrease recidivism. Few studies of how prison length affects recidivism meet rigorous experimental or quasi-experimental requirements (Nagin, Cullen and Jonson, 2009). As of 2009, Nagin et al. had identified two experiments and three matching studies. There have only been a few studies of length of stay using strong quasi-experimental designs since the Nagin et al., 2009 review. We cover these in our literature review.

A second goal of this study is to measure length of stay treatment heterogeneity: Does the effect of increasing the length of prison differ across individuals? The federal sentencing structure once again provides this opportunity. Scholars have argued the effect of prison may depend on a host of factors (Mears, Cochran, and Cullen, 2014; Nagin, Cullen and Jonson, 2009; National Research Council, 2014). Nagin, Cullen, and Jonson (2009) propose that imprisonment effects may depend on characteristics of the offender, institution, and sentence. The National Academy of Sciences, reporting on the causes and consequences of mass incarceration (NRC, 2014), discusses potential variations in punishment effects that depend on characteristics of individuals, social context, and units of analysis. As an example, the NAS report cites the research on the stigma of a criminal record on job seeking (Pager, 2007; Pager and Quillian, 2005) affecting black more than white applicants. Incarceration treatment heterogeneity is important because as the NRC report indicates, unpacking treatment effect dependence may lead to an explanation of why we observe outcome variations across imprisonment studies found in all the systematic reviews (Nagin, Cullen, Jonson, 2009; Smith, Goggin, and Gendreau, 2002; Villettaz, Killias, and Zoder, 2006).

Identifying the Causal Relationship between Prison Length of Stay and Reoffending

As researchers have pointed out (Berube and Green, 2007; Green and Winik, 2010; Loeffler, 2013; Loughran et al., 2009; Nagin, Cullen and Jonson, 2009), estimating the causal relationship between length of stay and reoffending raises validity challenges. It seems plausible if not probable that offenders receive long prison terms in part because they are likely to reoffend, potentially inducing a spurious positive correlation between prison length of stay and recidivism. For example, prior criminal record is a good predictor of future offending, and sentencing guidelines (when they are used). When guidelines are absent, judges typically take criminal records into account, sentencing offenders with criminal histories to serve longer terms than those with no or minimal prior criminal justice contact. Consequently, even if estimated in a regression framework attempting to control for confounders, a partial correlation of time-served and recidivism may be uninformative. The research provided in this paper addresses this methodological concern about identification by employing the logic of a regression discontinuity design (hereafter RDD) to rigorously identify the causal relationship between time-served and recidivism. The identification strategy rests on the structure of guideline sentencing. In the Nagin et al. review these scholars anticipated the utility of this approach, “Determinant sentencing grids,…, may provide a quasi-experiment for constructing the dose-response relationship between sentence length and reoffending (Nagin et al., 2009: 184).” Using a regression discontinuity design, we find that lengthening a prison term does not increase recidivism. In fact, increasing the length of a prison term may reduce recidivism, but if so, the effect is small.

As we explain, the U.S. Sentencing Guidelines provide a structure for an RDD, because holding constant factors believed to affect recidivism, the guidelines recommend longer or shorter sentences for similar offenders based on offense factors that likely have nothing to do with an offender's risk of recidivism. Many methodologists consider RDD to be a close second-best alternative to random assignment (Cook, 2008; DiNardo & Lee, 2010; U.S. Department of Education), so this study provides a rigorous approach to answering the research question. However, while we study whether longer prison terms affect recidivism more than do shorter terms, except for criminal history, our data and design provide limited information about heterogeneity.

THE PAROLE EFFECT

Roodman (2016) identifies a potential source of bias of imprisonment studies because of the inextricable dependency of time served and parole supervision. Parole agencies "carve up" a sentence of imprisonment into that portion to be served in prison and the remainder to be served under community supervision. Even though parole terms can be shortened because of good behavior, the length of community supervision will depend on the prison term. To the extent the prison release date is delayed, the period of supervision will be curtailed. The opposite is also true, shorter prison terms lead to longer parole supervision terms. Roodman argues that this means there are two simultaneous treatments that cannot be disentangled. Many studies find supervision increases returns because of the closer surveillance and the added conditions of supervision (see for example Gaes, Bales, and Skaggs 2016 comparing close community supervision monitoring to regular probation). People with longer terms of community supervision have more time and therefore more opportunity to be revoked, and irrespective of any other imprisonment impact, this artifact would bias shorter prison terms toward higher levels of recidivism, and longer terms toward lower recidivism. This potential bias does not affect our study because everyone in the federal system is given a post-release term of supervision separate from the prison term. One term is independent of the other. Furthermore, we censor observations when a term of supervision ends. This removes the potential artifact, but it also means the recidivism results might be different if federal offenders were released without supervision as is often the case in many state jurisdictions.

THEORIES ON THE EFFECT OF IMPRISONMENT

Scholars have proposed alternative theories explaining the impact of imprisonment as having either a criminogenic or a preventative/deterrent effect. The criminogenic theories include: a variant of life course research highlighting the interruption of the normal stages of adult development; a prison induced strain theory; a recognition that prison is a total institution and may socialize offenders into an anti-normative lifestyle; the possibility that prison are schools for crime; and the stigma attached to incarceration once the prisoner is released. Preventative theories of imprisonment cite mechanisms involving rehabilitation and specific deterrence. We discuss both below.

CRIMINOGENIC THEORIES

Loeffler (2013) cites the life course literature (Horney, Osgood, Marshall, 1995; Sampson and Laub, 1993; Western, 2002) and discusses how prison delays the normal development of adults and interrupts many of the stabilizing events such as marriage, employment, and education that might otherwise promote prosocial development. Prison strain has been studied by Listwan, Sullivan, Agnew, Cullen and Colvin (2013). Using survey methods, they examined prison deprivations and victimization aspects of prison strain on recidivism finding evidence of a relationship between strain variables and recidivism in the predicted direction. Early ethnographers emphasize strain by recounting the pains of imprisonment. However, these same ethnographers also emphasize prison socialization. This scholarship includes classic ethnographies of prison life such as Sykes' *Society of Captives: A Study of a Maximum Security Prison* (1958) and Clemmer's *The Prison Community* (1940). Clemmer and Sykes provided a framework for studying inmate society and culture emphasizing the nature of prison as a total institution in which separation from the community and the relationship between inmates and guards redefine roles, rules, and personal identities. Later ethnographies by Carroll (1988), Jacobs (1977), Goffman (1961), Irwin (2009), Owen (1998), and Fleisher (1989) all emphasize the strains deriving from both prison conditions and the antagonism pitting inmates against guards. None of these ethnographers provide strong empirical support for the relationship between acculturation, strain and recidivism.

A line of research employing experimental and regression discontinuity designs (Chen and Shapiro, 2007; Gaes and Camp, 2009; Lerman 2009 a, b) evaluate the impact of security level placement on in-prison and post-release outcomes. Under the assumption that higher security levels produce more strain, Chen and Shapiro using an RDD and Gaes and Camp using an experiment found higher security level placement led to higher levels of post-release recidivism. Lerman, employing an RDD, found that inmates placed in higher security levels had friends who were more likely to be gang-involved, and who were more likely to have had prior arrests and commitments. Using other data, Lerman shows how these personal networks of friends are likely to be acquired in prison. Although these studies identify security level placement as a cause of in-prison socialization and post-prison increases in recidivism, supporting strain and socialization hypotheses, they cannot rule out other mediating mechanisms such as labeling though self-identification.

Other criminologists argue that prisons are schools for crime where prisoners learn from other inmates how to become more knowledgeable offenders (Cullen, Jonson, and Nagin, 2011;

Letkemann, 1973; Nguyen, Loughran, Paternoster, Fagan, & Piquero, 2017). Other research suggests that prisons reinforce cultural values conforming to criminal activity; that is, offenders import these values from their community and deviant values are reinforced by the structure and organization of prison (Carroll, 1988; Irwin, 1980, 2005; Jacobs, 1977; Wacquant, 2001).

Even if an offender's orientation toward reoffending is unaltered by prison, labeling (Braithwaite, 1989) may affect recidivism. Ex-offenders may be denied employment or prohibited from accessing social benefits programs after release. Whether these events are internalized, limiting the offender's subjective expectations, or simply reduce legitimate avenues for community integration, they may increase the propensity to reoffend. By manipulating job applications (Boshier & Johnson, 1974) or by using actors posing as prior offenders (Pager et al., 2009), research has demonstrated a stigmatizing effect of a criminal record on employment reducing legitimate opportunities for ex-offenders, especially in a low wage job market. Of course, the stigma may come from any term of imprisonment, and long terms may be no more disadvantageous than short terms.

PREVENTATIVE THEORIES

Preventative theories of imprisonment rest on a prisoner's rational expectation of future punishment or on theories of rehabilitation that envision prison as an opportunity to reform and retrain. Specific deterrence theorists (Becker, 1968) assert that offenders are rational actors weighing the costs and benefits of crime. Experiencing prison reinforces its noxiousness and provides a disincentive to future criminal behavior. Reentry programming and other rehabilitative training adopting evidence-based practices may reduce recidivism (Andrews & Bonta, 2006; Andrews et al., 1990; Gaes et al., 1999; Gendreau et al., 1999; Gendreau et al., 2008; MacKenzie, 2006; Smith, 2006), so that prison can be used to change the trajectory of criminality. Longer prison terms might allow for more intensive reentry training and longer prison terms should reinforce the expectation of future punishment. However, because of the strong association between age and crime, longer prison terms produce inmates who are older at the time of release so that merely incapacitating offenders for longer periods will reduce the likelihood of crime absent any other intervention or causal mechanism. As noted in Snodgras et al. (2015), length of stay and aging are inseparable causal factors even if one controls for age at release or age at sentence.

Prison experiences may unleash forces that both push offenders away from crime and pull them toward reoffending. However, as Loefler (2013) argues, prison may simply collect people who are predisposed toward criminality because of pre-existing life experiences and deficits. He calls this social selection crediting Manski and Nagin (1998), Nagin, Cullen and Jonson (2009) and Smith and Paternoster (1990). Loeffler argues that imprisonment itself is a highly selective process. Only a small proportion of people are imprisoned, even people who are committed for the first time usually have an extensive history of criminal justice contacts. From this point of view, prison has no causal impact; it is where offenders are housed much as hospitals are places where people predisposed to illness are treated. Loeffler’s argument is more relevant to an analysis of imprisonment than to length of stay. The relevant question we address is: Even if there is a selection process, does length of stay have an impact on future reoffending?

PRIOR STUDIES OF PRISON LENGTH OF STAY

Relying on criminological theory, then, provides no clear expectation of whether incarceration will decrease or increase reoffending. Nagin et al. (2009) review empirical studies addressing the question of whether, on balance, imprisonment pushes ex-offenders away or pulls ex-offenders towards new crime. They conclude that imprisonment per se seems to have a criminogenic effect, although they recognize studies that contradict that conclusion. Nagin et al. also state there is little convincing evidence that length of stay – in their parlance prison dosage –

increases recidivism. They are skeptical of observational studies that use regression procedures to uncover the causal impact of prison length of stay because identification requires an exacting standard -- selection on observables (Rosenbaum, 2002) -- in which covariates completely adjust for any spurious relationship between time-served and recidivism.

Nagin, Cullen, and Jonson (2009) identify 14 regression studies, two experiments and three matching studies evaluating the effect of length of stay in their 2009 review. Because we are also skeptical of the regression studies we do not review them[1]. The experimental studies of length of stay by Berecochea and Jaman (1981) and Deschenes, Turner, and Petersilia (1995) manipulated sentence reductions for offenders who had already served time in prison. Part of the study design for the Deschenes et al. experiment diverted offenders from prison and would qualify as an imprisonment but not a length of stay study. However, a subset of offenders was randomly assigned to early release to be placed on intensive community supervision. Analysis of the 17 dependent variables showed only one that was significantly affected by prison length. The Berecochea and Jaman experiment randomly assigned offenders to a six-month reduction which was, on average, a 16 percent sentence reduction. Inmates receiving a sentence reduction had higher parole failure rates at one and two years after the release from prison, but these differences were not statistically significant. Thus, rigorous random design studies do not find that increasing prison length either increases or decreases future criminality.

Of the three matching studies identified by Nagin, Cullen, and Jonson (2009), two evaluate length of stay on juveniles (Kraus, 1981 and Loughran et al, 2009). The third study by Jaman, Dickover, and Bennett (1972), found recidivism to be higher for adult male burglars receiving 25 months or longer versus those serving 24 or fewer months, but the difference was only significant at

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[1] The 14 regression studies mostly find null effects with specific deterrent and criminogenic effects about equal.

the 24-month horizon. Kraus (1981) evaluated length of stay contrasts for juveniles that depended on the juvenile's age group. For example, Kraus contrasted a term of 8 to 20 months versus a term up to 7 months for 14 and 15-year old's; however, for juveniles who were 13, the contrast was between 9 to 16 months and 8 months or less. None of the contrasts were statistically significant. Loughran et al (2009) employ propensity score methods. The authors categorized the continuous length of stay into discrete ordinal categories and used ordinal logit to estimate propensity scores. The variation in length of stay extended from 0-3 months up to greater than 12 months. Whether using arrest or self-reported reoffending, Loughran et al. found no marginal effect of length of stay on either outcome.

Meade Steiner, Makarios, and Travis (2012) also used ordinal logit to estimate the propensity score creating ordered subsamples stratified by the estimated log odds of being in a category of length of stay. They stratified their sample into dosage categories and compared people within a "dosage" stratum to those with "similar" propensity scores who had longer or shorter sentences. Meade et al. found that beyond 16 months, higher doses of imprisonment lowered recidivism (felony arrest); however, only the highest dose (>= 78 months) was statistically significant.

Snodgrass, Blokland, Haviland, Nieuwbeerta and Nagin (2011) also conducted a dosage study of time served among prisoners in the Netherlands and restricted their sample to first time commitments convicted of violent, property or drug offenses, who were between age 12-40 and who had sufficient time post-release to assess recidivism. The discrete categories of time served were 1 month or less, 1-2 months, 2-3 months, 3-6 months, 6-12 months and 12+ months. Consistent with the other propensity score studies, they estimated the propensity score using ordinal logit with many covariates to predict the ordinal level of prison length of stay. Rather than use only the propensity scores to match offenders, they used exact matches on sex, age (+- two years), and one of four trajectory group memberships as well as a propensity score matching caliper of .15. The matched pairings produced sets of offenders who served different sentence lengths. The matched pairs allowed Snodgrass et al. to examine measures of recidivism comparing any given ordinal category of length of stay to the other categories where a matched pair could be found. The results showed no relationship between length of stay and recidivism. The Snodgrass et al. study takes great care in constructing counterfactuals for the offenders within a given length of stay class. But as they note, the average levels of time served are much shorter than one would observe in most United States adult prison samples.

Roach and Schanzenbach (2015) use randomization of judges assigned to sentencing convicted offenders to map the relationship between length of stay (median of 4 months) and one, two and three-year rates of recidivism. Using reduced form and two-stage regression, they find that each additional month of sentence length decreases recidivism by about 1 percent. When they stratify on criminal history, Roach and Schanzenbach find a strong effect of sentence length for offenders with minimal criminal history and a weak or insignificant effect for offenders with more extensive criminal histories. They also found that relative to the base hazard rate, the impact occurred almost entirely within the first year of release. Roach and Schanzenbach argue the impact of length of stay is a combination of severity of punishment and rehabilitation. The researchers argue that offenders with longer sentences can participate in more rehabilitative prison programs. Roach and Schanzenbach's Table 2 (page 19) does indicate possible sources of bias in the relationship between length of stay and recidivism. The judge dummy variables not only demonstrate a strong relationship to sentence length essential to the study design, but they also predict the offender's offense severity level and number of prior non-violent convictions. These offender characteristics could be associated with the sentence length and the level of recidivism qualifying as backdoor variables (Morgan and Winship, 2015). Roodman's (2016) review of this study offers the possibility the longer sentences in this study would lead to longer parole supervision elevating recidivism because of technical parole violations.

Kuziemko (2012) uses the Georgia parole guideline grid as a means of identifying the causal effect of length of stay. She restricts her analysis to offenders with less severe offenses and whose criminal history category is in the average to high range. She uses the discontinuity in the grid recommendation (separating high from average risk) as an instrument for time served, with the instrumented time served in stage one, and recidivism regressed on the instrument in stage two along with other covariates. One of her estimates restricts the sample to people whose points are just above or just below the discontinuity threshold (8 versus 9 points) which should yield the best approximation to an exogenous variation. She finds each additional month of time served lowers the three-year recidivism rate by 1.5 percent. It is not clear why Kuziemko restricted her analysis to this threshold, limiting the analysis to a narrow range of the guideline grid. Most offenders have lower criminal history scores and restricting the analysis to the higher thresholds limits the generalizability of the study. In fact, Roodman's (2016) reanalysis of these data evaluates other discontinuity cut points and finds either insignificant length of stay coefficients or coefficients with the opposite sign indicating increased length of stay increases the probability of recidivism.

Mears et al., (2016) used the generalized propensity score to balance covariates across levels of length of stay. They balanced within four subgroups of time served categories using demographics, categories of commitment offense, offense severity scores, prior record scores, dummy coded year of release, and dummy coded judicial district. While they achieved balance on this set of covariates, they did not conduct a sensitivity test to evaluate the potential impact of an omitted variable (Rosenbaum, 2002). Mears et al (2016) found the effect of time served on recidivism depended on the level of length of stay. Short terms produced increased recidivism up to one year. From one to two years, time served decreases recidivism, and thereafter has no effect. Long terms (more than 6 years) could not be evaluated due to large error terms.

An RDD design, employed by Hjalmarsson (2009), was also based on a guideline system; however, the study applied to a juvenile population in the state of Washington, and the discontinuity was based on a sanction to a state run juvenile facility versus a local sanction. This latter punishment included sanctions involving much shorter sentences served in a local facility, a term of community supervision, fines, or community service. Hjalmarsson found a deterrent effect of state-run confinement for juveniles compared to the local sanctions. This study was not a test of length of stay, but a test of state-run juvenile confinement as opposed to a bevy of alternate juvenile punishments. To control for the fact the running variable was correlated with recidivism across levels of criminal history, the study used a quadratic regression of the running variable. Furthermore, the author used all the data without regard for bandwidth. This puts tremendous pressure on his quadratic expression being correct. Secondly, rather than using an intent-to-treat estimator, and perhaps converting that into LATE, the author drops cross-overs from the analysis. There is no justification for doing so, and we are unaware of other RDD's that takes such a step. Finally, the author made no attempt to justify an implicit assumption that right-hand-censoring is independent. In fact, given data construction, and what we know about juvenile justice, right-hand-censoring is unlikely to be independent. Specifically, offenders with longer terms are more likely to be censored. This relationship alone may explain the findings that incarceration reduced recidivism.

One last study of note was conducted by Drago, Galbiati, and Vertova (2009) in which the authors examined the effect of a Clemency Bill in Italy that shortened the sentence of offenders anywhere from 1 to 36 months. This could be described as an exogenous treatment since the application of clemency was applied irrespective of criminal history, offense, age, and other factors affecting the outcome. However, the early release came with a caveat. The remaining time to serve would be reinstated if the offender was convicted of another crime within five years. So, this was a reduction in length of stay with an added disincentive to commit a new offense. People who had spent more time in prison were less likely to reoffend, and each month of remaining time to serve was associated with a 1.24 percent reduction to commit a new crime[2].

Even with these additional studies, the literature on length of stay using strong counterfactual designs is still sparse and presents a mixed picture of effects. Most studies show no effect, or that increased prison length reduces recidivism slightly. Several are imbalanced in ways that could bias the length of stay effects. The studies also span different populations: juveniles, adults, and cover different international jurisdictions (Italy, the Netherlands, and United Sates). The present study uses a very large sample of federal offenders sentenced to prison with varying lengths of stay.

INCARCERATION TREATMENT HETEROGENEITY

Scholars have argued that prison dosage (length of stay) may have different effects conditional on factors associated with the individual, with the social context, with sentencing structure and with potentially many other factors. The National Academy of Sciences study on mass incarceration finds evidence for the heterogeneity of incarceration effects (NRC, 2014: 427). But we need a template to systematically examine incarceration heterogeneity.

Mears, Cochran and Cullen (2014) provide one such template. They propose three types of sources of heterogeneity: prior sanctioning experience, in-prison experiences, and the level of post-

[2] Kuziemko (2012) also capitalizes on early release in Georgia because jail overcrowding allowed her to compare offenders with different amounts of time served having the same prior risk levels. She finds people with longer time served have lower recidivism rates. These results are challenged by Roodman as possible evidence of parole bias in Kuziemko's analysis.

release supervision – a pre-, within-, post-custody framework. The National Academy of Sciences report takes an even broader perspective by suggesting that there may be incarceration effects for different units – the effect on people may be substantially different than the effect on families or neighborhoods. Based on the Mears et al. hypothesized framework, it seems sentencing jurisdictions will vary on many of the important pre, within, and post dimensions. We may be reluctant to accept that results from Florida apply to Massachusetts unless we have a framework that tells us how to compare jurisdictions within this pre, within, and post sentencing framework.

For the current application, we use the structure of the U. S. Sentencing Guidelines sanctioning grid in our RDD design to test whether criminal history or offense seriousness moderates the effect of length of stay increments. We find that increments in length of stay have about the same effect regardless of either of these dimensions. We also separately evaluate criminal history, sex, race, education and marital status all showing homogeneous effects of length of stay.

PRESENT METHODOLOGY: DATA, STATISTICS AND INFERENCE

This methodology section has three parts. The first part identifies the source of the data – the Federal Justice Statistics Program. To provide necessary background before discussing the regression discontinuity design, the second part summarizes the U.S. Sentencing Guidelines. The third part discusses statistical methods and the RDD identification strategy.[3] This application of RDD is unconventional: There are multiple break-points and hence multiple effects; the effects are summarized using a meta-regression borrowed from meta-analysis. Furthermore, for each effect, the bandwidth is exactly two units, so search for an optimal bandwidth is not a concern. Several

[3] Some readers may prefer to characterize our approach as using instrumental variables. The U.S. Sentencing Guidelines are the instrument that moves offenders who otherwise have the same propensity to recidivate between two guidelines cells, the first of which prescribes a shorter prison term on average than does the second. Because there are many guideline cells, there are many estimates based on contiguous cells. The multiple estimates are averaged using inverse variance weights.

conventional tests used in RDD are inapplicable in this setting. For example, we need not test how expanding the bandwidth affects estimates.

Although the parallel is disputed (Imbens & Lemieux, 2007), some see RDD as an instrumental variable estimator, and it is practical to employ IV as the estimator. This alternative estimator is also used in this study. Results show the RDD estimator and the IV estimator yield essentially the same results.

Data Source

Data for this study come from the Federal Justice Statistics Program (FJSP), a Bureau of Justice Statistics (BJS) sponsored project to assemble federal justice records. To be included in the analysis, an offender must have been sentenced under the guidelines between 1999 and 2014. Practically the period studied is shorter because to be included in the analysis, an offender must have entered community supervision, but many offenders sentenced between 1999 and 2014 were still incarcerated at the end of 2014.

The FJSP has a thirty-five-year history; however, recent refinements link federal offenders from investigation through corrections. This paper uses a subset of the linked data running from sentencing through a single cycle of release to community supervision and possible recidivism. The U.S. Sentencing Commission (USSC) provides data on offenses and offenders at the time of sentencing; the Administrative Office of the U.S. Courts (AOUSC) provides data on offenders under community supervision; and the Bureau of Prisons (BOP) provides data on offenders during periods of incarceration. Details regarding construction of the FJSP data appear elsewhere (Kling, et al., 2016); this paper summarizes.

Since 1987, federal offenders have been sentenced using sentencing guidelines and data about guideline application have been assembled (and contributed to the FJSP) by the USSC. Guidelines data provide detailed accounts of the offender's criminal history, the offense of conviction, the sentence imposed and expected prison time. Expected prison time differs from actual prison time but the two are close because sentencing in the federal system uses determinate or real-time sentencing and, as explained below, actual time-served is observed with systematic error.

When an offender is sentenced to prison, he or she serves time in a facility run by the Federal Bureau of Prisons (BOP), which reports data from its inmate tracking system (SENTRY) to the FJSP. Most offender prison terms are recorded in SENTRY, but some are not because (1) some offenders are sentenced to time-served prior to trial, (2) some offenders spend part of their terms in state detention, and (3) terms of thirty days or fewer are inconsistently recorded in SENTRY. Because of these limitations, we use expected time-served reported by the USSC as a measure of prison time. However, we use admission into a BOP facility during the period of community corrections as one measure of recidivism.

When an offender is sentenced to probation, or when an offender leaves prison to serve a statutorily required term of supervised release (a judge-imposed sentence to post-incarceration supervision), his or her term of community supervision is recorded in Probation and Pretrial Services Automated Case Tracking System (PACTS), an information system maintained by the Administrative Office of the U. S. Courts, specifically by the Probation and Pretrial Services Office (PPSO). After some manipulation[4], the PACTS data record periods of community supervision and outcomes. Being

[4] Federal probation officers frequently open terms of probation before the offender is sentenced and terms of supervised release before the offender is released from prison. For probation, the probation officer may be responsible for pretrial supervision continued into post-conviction supervision, and this appears as a continuous period of community supervision

revoked from community supervision is another measure of recidivism. Every federal offender in our sample received a term of community supervision eliminating one source of variability and heterogeneity.

For the recidivism analysis, time-at-risk starts with the beginning of a term of supervised release for offenders sentenced to prison. The time-at-risk ends with recidivism, or with the first occurrence of one of the following censoring events: completion of the community supervision term, three years, or with the end of data collection. Using the measures described above, recidivism is defined by the occurrence of a recognized failure event. If the offender reappears in the federal BOP data, he or she is deemed to have recidivated. If the offender does not reappear in the BOP data but we observe the offender is revoked from community supervision, he or she is deemed to have recidivated. Although there are several explanations for being revoked from supervision but not entering federal prison, one common reason is the offender is arrested for a state-level crime and detained in state custody pending trial. The date when an offender reenters prison is the date of recidivism; if the offender does not reenter prison but is revoked from supervision, the date of the revocation is the date of recidivism. Based on years of working with the PPSO, checking algorithms with PPSO administrative staff and field staff, we have confirmed that this approach – providing primacy to returning to prison according to the BOP – is the best way to identify recidivism.[5]

Many federal offenders are arrested for new crimes without returning to prison or having their community supervision status revoked. Using the FJSP data, we estimate three-year recidivism

in the FJSP. Also for probation, an officer may open a supervision record when preparing a presentence report. The correction is to change the beginning of supervision to the date of sentencing. For a term of supervised release, the probation officer might open the term in preparation for post-release planning. To correct this, we changed the beginning of supervision to coincide with the end of prison.

[5] An offender's community supervision must be revoked before he or she is committed to prison, so the date of revocation would seem to be the better date for the failure event. In fact, probation officers often fail or are unable to record the date of revocation until weeks or months after it occurs. Consequently, the date of entering prison (when it occurs) is a more accurate indicator for a study of recidivism.

rates of about 20 percent for federal offenders placed on community supervision. If new arrests were included in the analysis, the recidivism rate would be closer to 30 percent.[6] The FJSP does not include arrest data, so we could not define recidivism as being arrested, but an arrest that does not result in revocation is typically for a minor or unfounded crime, so the outcome measures used in this study are for events that are sufficiently serious to provoke a return to custody.

To be included in the analysis, the offender must have been convicted and sentenced under the U.S. Sentencing Guidelines since 1999. This is the period covered by the data linkage. Only offenders convicted of felonies and serious misdemeanors are sentenced routinely under the guidelines, so this study is limited to those serious offenders. Others rarely serve prison terms, so the omission is inconsequential. To be included in the study, the offender must have entered community supervision, else we could not identify a recidivism follow-up period. Because of this requirement, some offenders sentenced under the guidelines are excluded from the analysis. Several reasons explain why offenders do not enter community supervision. First, before entering community supervision, offenders sentenced to prison must have been released from prison. Second, many federal offenders are foreign nationals in the country legally or illegally; they are often deported upon prison release, so they are not candidates for a recidivism study, and consequently this study is based on U.S. citizens.[7] Some other offenders are released from prison to detainers exercised by state authorities. Because offenders who never enter community supervision provide no information for analysis, they are excluded from the analysis file.

[6] The authors have worked with PPSO for six years to match community supervision records with criminal history records (e.g. criminal record histories held by state authorities and linked using an FBI index). Data use agreements preclude including criminal history records in the FJSP, so they are unavailable for this analysis. Based on our work with PPSO, we know that recidivism rates based on arrests are near 30 percent.

[7] Non-citizens are sentenced to community supervision, but they are deported and never enter *active* community supervision. If they reenter the county illegally, their supervision status can be revoked. However, this form of recidivism is very different from the form of recidivism studied in this paper, so we chose to exclude non-citizens. We might have retained non-citizens who entered active community supervision, but they are difficult to distinguish reliably in the OBTS, so we chose to exclude all non-citizens.

Data limitations discussed in this section seem to raise concerns about bias when measuring recidivism, but these concerns are not serious for this study. Essentially a RDD study compares offenders from group A with offenders from group B, where group A and group B are closely matched. Due to matching, whatever biases arise regarding the measurement of recidivism are likely the same in groups A and B. Therefore, a comparison of recidivism for groups A and B is valid even if estimates of recidivism are systematically biased.

SENTENCING GUIDELINES

Explaining the RDD requires a short description of federal sentencing guidelines. Under the guidelines, offense seriousness is determined by a complex set of rules that take the offense of conviction and real offense behavior into account to classify sentencing factors into one of 43 *offense seriousness levels* with j=1 being the least serious and j=43 being the most serious and six *criminal history categories* with k=1 being the least serious (least likely to recidivate) and k=6 being the most serious (most likely to recidivate). By design, the offense seriousness levels reflect the USSC's assessment of offense seriousness and do not consider an offender's threat to recidivate. Likewise, by design, the offender's criminal history category reflects the USSC's assessment of the likelihood that an offender will recidivate. Important to note, subtle differences in offense conduct cause an offender's crime to be associated with offense level j rather than offense level j-1 or j+1. For example, unarmed bank robbers, armed bank robbers who do not display their weapons, armed bank robbers who display but do not threaten with their weapons, and bank robbers who explicitly threaten with their weapons, all appear in different seriousness levels. Furthermore, offenders convicted of violent crimes, drug-law violations and property crimes can all appear in the same offense seriousness level. Based on this subtlety, we expect that offenders in offense seriousness level j will not be inherently more likely to recidivate than will offenders in seriousness levels j-1 and j+1 *provided criminal history is held constant*.[8] In fact, holding criminal history constant, it seems likely that offenders in offense seriousness category j are not inherently more or less likely to recidivate than are offenders in offense seriousness categories j-k and j+k where k is a small number. This assumption provides the identification logic for the RDD and for the instrumental variable alternative.

Under the guidelines, another complex set of rules determine *criminal history points*, which are collapsed into six *criminal history categories* for the guidelines application, with category 1 being the least serious criminal record and category 6 being the most serious record. Based on the offense seriousness level and the criminal history category, the federal sentencing guidelines are a 43x6 grid comprising 43 offense seriousness rows and 6 criminal history columns. Holding the criminal history category constant, moving down the grid from least serious offense level to most serious offense level, the recommended prison term increases. Holding the offense seriousness level constant, moving from left to right across the grid from least serious criminal history category to most serious criminal history category, the recommended prison term increases. For the least serious offenses and for the least dangerous offenders, the guidelines recommend probation or alternative sanctions, although prison terms are allowed and sometimes imposed.

Every year, the Commission can recommend changes to the guidelines for Congressional approval, and in fact the guidelines have changed over time. These changes have little consequence for the analysis reported in this paper. Even if the composition of offenders within specific guideline cells has changed over time, recommended sentence terms have always increased with offense

[8] Possibly some factors entering the determination of the offense seriousness level might predict recidivism. However, crimes committed in the federal system are so diverse, and factors entering the guidelines are so detailed, it seems unlikely that increases in the offense seriousness levels would be systematically associated with recidivism holding criminal history constant. Possibly, for some cell j/j+1 comparisons, factors that enter the offense seriousness determination may cause recidivism to increase or decrease, but provided these factors are not systematic – meaning that they do not always increase recidivism as the comparison moves from cell j to j+1 – the treatment effect is still identified.

seriousness level. As explained in this next section, that regularity is sufficient to support the RDD estimation procedure.

THE RDD AND STATISTICAL METHODOLOGY

During the last decade, methods for inferring causality have gone well beyond naive regression models criticized by Nagin and his colleagues (Morgan & Winship, 2015; Imbens & Rubin, 2015). A regression discontinuity design (Hahn, Todd, & van der Klauuw, 2001; van der Klaauw, 2002; Imbens & Lemieux, 2007; Lee & Lemieux, 2010; Bloom, 2012) is a rigorous approach often applied in non-criminal justice research and sometimes applied in criminal justice studies (Berk & Rauma, 1983; Berk & DeLeeuw, 1999; Berk, 2010; Rhodes & Jalbert, Regression Discontiuity Design in Criminal Justice Evaluation: An Introduction and Illustration, 2013). Commonly the RDD is applied in a setting where a decision rule based on factors (such as offense seriousness level holding criminal history constant) can be used to define a “cut-point” that places study subjects (such as offenders) into one group that does and another group that does not receive treatment. Very near that rule-imposed cut-point, study subjects who do and do not receive treatment are so similar that a comparison of outcomes for the two groups is deemed valid. This section explains that our application of the RDD identification strategy differs from the conventional application.

One difference between our application and the standard application is that for us treatment is a dose (e.g. the length of a prison term), while in standard applications treatment is a binary condition. On average, the dose received by offenders in cell j+1 is higher than the dose received by offenders in cell j. Because offenders in cells j and j+1 are otherwise the same regarding the threat to recidivate, we can ask whether offenders in cell j+1 have higher or lower recidivism rates than offenders in cell j, and if so, we have justification for attributing causation. As a stylized example, suppose first-time offenders who steal $100,000 are sentenced according to offense seriousness level j while first-time offenders who steal $125,000 are sentenced according to offense seriousness level j+1. In deference to the more serious crime, the guidelines call for a term between 20 and 23 months for first-time offenders sentenced within guideline cell j and between 22 and 26 months for first-time offenders sentenced within guidelines cell j+1. Given judicial discretion, sentences imposed on offenders within guideline cell j overlap with sentences imposed on offenders within guideline cell j+1, but on average prison sentences within guideline cell j+1 are longer than those imposed within guideline cell j. This average difference drives the identification strategy. Specifically, holding the criminal history category constant, offenders sentenced within guideline cell j+1 are not inherently more recidivistic than are offenders sentenced within guideline cell j, but offenders sentenced within guideline cell j+1 receive longer prison terms on average, and we can test for whether those longer prison terms account for higher or lower recidivism.

Usually when applying RDD, researchers consider a variable X (called a *running variable* in the RDD context) and a critical value X^o that assigns study subjects to a comparison group when $X < X^o$ and to a treatment group otherwise. A *sharp* design occurs when the assignment is exact; a *fuzzy* design occurs when the assignment is inexact. Although both sharp and fuzzy designs both estimate the treatment effect, sharp designs are preferred because (from a statistical estimation standpoint) they are more efficient and interpreting the effect is clear.

When treatment is binary, as it is in standard applications, the distinction between sharp and fuzzy is straightforward. Let P_L be the probability of treatment when $X < X^o$ and let P_R be the probability of treatment otherwise. If $P_L = 0$ and $P_R = 1$, then the design is sharp. Let Y_L be the mean outcome when $X < X^o$ and let Y_R be the mean outcome otherwise. An estimator for the treatment effect is Y_R-Y_L, or just the difference in the mean outcomes. But suppose that $P_L \geq 0$ or $P_R \leq 1$ or both;

then the design is fuzzy. Given a fuzzy RDD, an often-used estimator is $(Y_R-Y_L)/(P_R-P_L)$. This alternative estimator is sometimes called a local average treatment effect (LATE). Note the numerator is an intent-to-treat estimator, and in fact, statistical testing is based on the numerator because dividing by a constant will not change the properties of the basic test of whether treatment is effective.

For our application, treatment is a dose, and doses are distributed about a mean that depends on whether $X < X^o$ or $X \geq X^o$. A necessary condition is that a shift in the offender from guideline cell j to guideline cell j+1 will always (a sharp design) or sometime (a fuzzy design) cause an offender to receive a longer prison term. The *monotonicity assumption* is that this shift will never cause an offender to receive a shorter prison term. We presume the monotonicity assumption holds and see no reason to assume otherwise.

A test of the null hypothesis of no treatment effect is based on Y_R-Y_L. Let T_L be the average prison term for $X < X^o$; let T_R be the average prison term for $X \geq X^o$. Then the counterpart to the LATE estimator for the binary treatment effect is $(Y_R-Y_L)/(T_R-T_L)$, or the change in recidivism per unit of additional prison. This, too, is a local average treatment effect. Its interpretation is the average increase/decrease in recidivism resulting from an average increase in prison length.

In both these illustrations, the estimator is LATE. LATE is not necessarily the average treatment effect, or in this case, the average increase/decrease in recidivism from increasing time-served by one unit. However, LATE is almost always used as a substitute for that average, which is unidentified (Imbens, 2009).

Returning to the standard application, the ideal identification strategy requires that estimation be limited to study subjects whose X values are just slightly less than X^o or equal to or slightly more than X^o.[9] Because very few observations meet these demanding conditions, evaluators usually expand the *bandwidth* to be broader than "slightly less" and "slightly more". Because this expansion of the bandwidth violates the asymptotic identification assumptions, evaluators typically assume that a *local linear regression* of the outcome variable Y on X will capture the relationship between Y and X absent any intervention. Given the intervention, any departure from the local linear regression around X^o is deemed the *treatment effect*. A standard application would test the credibility of this local linear regression and a standard application would test the sensitivity of findings to the size of the bandwidth.

Our application differs from the standard application in important ways. Typically, when researchers apply the RDD strategy, there is a single cut-point, so there is a single estimate of the treatment effect. Less commonly, there are *multiple* cut-points, and a researcher can estimate multiple treatment effects (Cattaneo, Keele, Tituinik, & Vazquez-Bare, 2016). Given the structure of the sentencing guidelines, our study is based on a multiple cut-point RDD. That is, we estimate multiple effects then combine them to generalize conclusions.

Rather than increasing efficacy by expanding the bandwidth, we always make comparison across adjacent cells so that cell j is deemed "slightly less" than the offense seriousness level that determines cell j+1. Employing multiple cut-points achieves small standard errors by combining the multiple estimates that arise from comparing outcomes for cells j/j+1, j+1/j+2, j+2/j+3 and so on. It is unnecessary to expand the bandwidth beyond two units.

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[9] "Slightly less" and "slightly more" mean than X falls within a region X+δ and X-δ as δ approaches zero. Expanding the bandwidth means that δ is allowed to get bigger.

In the conventional application, a researcher would assure the relationship between the outcome variable and the running variable is linear to the left and right of the breakpoint.[10] Because our approach employs just two points, j and j+1, this test is unavailable to us. Arguably it is also inapplicable because, when estimating an effect, we do not expand the bandwidth beyond j and j+1. In the conventional application, the researcher will assure there is a discontinuity in treatment, so that treatment is more likely for those to the right of the cut-point than it is for those to the left. We do apply this conventional test showing that it holds.

In conventional RDD analysis, researchers worry about "gaming". In the present context, defense attorneys have incentives to move their clients to lower guideline cells, and prosecuting attorneys have countervailing incentives to move offenders to higher guideline cells. Gaming is possible by manipulating guideline elements, such as the amount of money stolen. Defense attorneys would argue the amount is lower than alleged; prosecutors would argue the opposite. In conventional RDD analysis, researchers use imperfect devices to test for gaming, but those tests are not necessary in the present context.

To explain, suppose a variable Z is associated with recidivism and is subject to gaming such that defense attorneys are generally able to shift offenders from cell j+1 to cell j based on the value of Z. In our study, we would expect a general shift in Z from j+2 to j+1, from j+1 to j, from j to j-1, from j-1 to j-2, and so on. In general, this shift would have no effect on the contrasts, because it occurs across all adjacent cells. Thus, gaming is less relevant for a multiple-effect RDD than for a conventional single-effect RDD. Furthermore, we are only concerned with gaming about variables

[10] Postulating a linear relationship is frequent, but some researchers expand their models to allow power functions of the running variable. Hjalmarsson (2009), in a study of guidelines and juvenile justice, discussed earlier, used a quadratic. This is a dangerous practice because the validity of the quadratic is difficult to assess, and findings based on a RDD are sensitive to getting the model specification correct. There are other elaborate alternatives to using local linear regressions, but they are not applicable to our study.

that affect recidivism. Because criminal history is held constant by construction of the guideline cells, the avenue for gaming is restricted. Nevertheless, we introduce some test for whether variable associated with recidivism vary systematically across cells so that, for example, we ask whether offenders in cell j+1 tend to be older than offenders in cell j. We introduce these potential confounders into the local linear regression. Although we do not need a local linear regression for identification, we increase the efficiency of estimates by using a regression to include covariates in the analysis. Given the nature of the outcome variable, when comparing outcomes in cells j and j+1, our study uses a Cox survival model, sometimes called a Cox proportional hazard model, but it is "local" only in the sense that (absent the intervention) the baseline survival model is invariant around the cut-point, and we estimate the treatment effect from a unit shift. There exists an extensive methodological literature on survival models (Lancaster, 1990; Kalbfleisch & Prentice, 2002; Hosmer, Lemeshow, & May, 2008), which are frequently applied in studies of criminal recidivism, where time-at-risk is often censored. A separate study justifies using the proportional hazard model (rather than a parametric alternative) to study recidivism in the federal system (Rhodes et al., 2012). Although the approach seems uncommon, others have used survival models as the estimator in a RDD context (Rhodes & Jalbert, 2013; Bor, Moscoe, Mutevedzi, Newell, & Barnighausem, 2014). We use the RDD identification strategy and the survival model estimation strategy to identify and estimate the change in the hazard of recidivism across paired guideline cells.

The most defensible approach consistent with the intuition of the RDD would seem to be to compare the relative hazards for cell j and j+1 (holding criminal history category constant) because offenders in cells j and j+1 are more alike than in any other comparison. Both the previous discussion and descriptive statistics, discussed subsequently, points toward a problem: Cells j and j+1 are so alike the average time-served is not guaranteed to increase when comparing cell j to cell j+1.[11] Even when it increases, the size of the increase may be so small that it is not informative. As an extreme example, there is not much use in comparing recidivism for offenders who serve two years on average when sentenced within cell j and two years plus one day on average when sentenced within cell j+1. From experimentation, to assure that average time served will increase between contrasted cells, we have found it better to compare the outcome from cell j and cell j+2.[12] Notwithstanding what was written above, most of the analysis is based on that j/j+2 comparison, but we do offer sensitivity tests that employ the original j/j+1 comparison. An alternative approach is to combine cells j and j-1 and compare that with the combined cells j+1 and j+2. (This is equivalent to expanding the bandwidth.) However, this alternative approach seems to offer no advantages and introduces some difficult estimation problems (serial correlation and general model specification issues) when combining multiple effects so we avoid its application.

Because this identification/estimation strategy results in many hazard estimates (potentially 41x6), we use techniques familiar from meta-analysis to combine estimates (effects) into summary statistics. A useful explanation of a fixed-effect meta-regression is Rhodes (2012). For example, for the first criminal history category, there are potentially 41 effects. We estimate a weighted least-squares linear regression to estimate the average of the 41 effects. Because there are 6 criminal history categories, we repeat the meta-regression across the 6 criminal history categories and then average the 6 meta-regression averages to derive a single inverse variance weighted estimate and

[11] Especially for drug-law violations and weapons offenses, federal statutes require application of mandatory minimum sentences. Regardless of guideline recommendations, judges cannot sentence below the mandatory minimums. Because mandatory minimums are not evenly distributed across the offense seriousness levels, it is possible for the average terms in cell j to be larger than the average terms in cell j+1.

[12] Across the six criminal history categories, the average percent change in time-served between cells j and j+1 ranges from 9% to 18%, with the change being largest for criminal history category 1. In comparison, across the six criminal history categories, the average percent change in time-served between cells j and j+1 ranges from 20% to 38%, with the change being largest for criminal history category 1.

test statistic. Thus, although there are many estimates, there is only one test statistics and hence no multiple comparison problem.

For reasons explained below, when applying the meta-regression, we must deal with error terms that may be auto-correlated and heteroscedastic. We will show that heteroscedasticity is a major problem, and to deal with it we used weighted least squares. Because we compare cells j with j+2, j+1 with j+3, j+2 with j+4, and so on, we potentially introduce autocorrelation across the estimates. The j/j+2 and j+2/j+4 comparisons are not independent because they have j+2 in common. This problem has corrections (Prais-Winston estimators, for example) but they are suspect in small samples where ignoring the problem is likely better than attempting to introduce a feasible generalized least squares solution (Greene, 2008, p. 648). This follows because small samples provide inaccurate estimates for the autocorrelation coefficient. We concentrate on the heteroscedasticity problem; however, we provide a sensitivity test suggesting that autocorrelation can safely be ignored.

The meta-regression provides the average effect of moving from guidelines cell j to cell j+2. This is an intent-to-treat estimator. If this movement were not statistically significant, we would conclude that increasing prison terms has no effect on criminal recidivism. If the average effect is statistically significant, by itself it does not provide LATE, the size of the effect resulting from a unit increase in prison time. Our approach is to report the size of the average effect when moving from cell j to j+2, the average increase in the length of the prison term when moving from cell j to j+2, and the ratio of the two. The ratio is essentially the LATE.

A more direct approach, which requires the same assumptions as were applied above, is to apply an instrumental variable (IV) estimator where the shift from guideline cell j to j+2 is the instrument. That is, offenders in cell j+2 are not inherently more or less recidivistic than are offenders in cell j, but the longer average sentences served by offenders in cell j+2 cause those offenders to recidivate at a greater or lesser rate than offenders in cell j. Instead of a dummy variable representing membership in cell j or j+2, the important right-hand-side variables is the average time-served by offenders in cell j and j+2.[13] That is, the treatment variable takes one of two values: the average time-served by offenders in cell j (when the observation belongs to cell j) or the average time-served by offenders in cell j+2 (when the observation belongs to cell j+2). The advantage of this approach is that parameter estimates have the interpretation of "change in the relative hazard per additional month of time served." The disadvantage is that IV estimators lack desirable asymptotic properties (that is, they are biased even in large samples) when applied to a non-linear model, and the Cox regression is a non-linear model (Rhodes, 2010). Nevertheless, we expect estimates to be approximate and worth comparing with the RDD estimates.

RESULTS

This section has two parts. The first part provides descriptive statistics and justification for deleting cases from the analyses. The second section provides estimates of the effect of increasing time-served on recidivism using the RDD and IV approaches; it also provides sensitivity tests for case deletion and other analytic decisions.

DESCRIPTIVE STATISTICS AND RULES FOR DELETING CASES

Table 1 reports the number of offenders available for the analysis before we add restrictions. To be included in this table, an offender must meet three criteria. First, he or she must have been sentenced under the guidelines, because guidelines calculations are used in the analysis. Second, he

[13] This is a two-stage estimation model. The first-stage involves a regression of time-served on dummy variables representing the adjacent guideline cells. But this regression will simply provide the mean time-served in each cell, so we do not actually perform the regression.

or she must be a U.S. citizen. We cannot follow recidivism by deported offenders. And third, he or she must have entered active community supervision following sentencing – either probation or a term of supervised release – because active supervision initiates the time at risk of recidivism. This third criterion has two implications. If an offender never entered active community supervision, that offender would not appear in the analysis. Specifically, if an offender was sentenced to prison and remained in prison as of September 30, 2014 that offender would not appear in the analysis. Thus, offenders sentenced to long prison terms, and especially offenders sentenced toward the end of the data collection window, are more likely to be excluded than are other offenders. This systematic selection will not bias the RDD estimates because any selection bias will be equally applicable to cell j and cell j+2. That is, the RDD implicitly adjusts for the selection bias.

Insert Table 1 about here

Shading identifies cells with fewer than 100 observations. Although data for the most and least serious offense levels are sparse, the sample sizes for the other cells are ample, an important point because the RDD approach holds criminal history category constant and compares outcomes for offense serious levels j and j+2. Notice that about half the offenders fall into criminal history category 1, and given that these are relatively inexperienced offenders, a reader might put more subjective weight on the analysis of offenders in this criminal history category. To be included in the principal analysis, a cell must have 100 or more offenders. Stripped of some other statistical issues, basically the RDD approach requires a comparison of the mean recidivism rates for cells j and j+2. Because the sampling variance for estimated difference in outcomes between offense seriousness levels j and j+2 is inversely proportional to the number of offenders in cells j and j+2, setting a minimum number of observations increases the average precision of estimated effects for those cells retained in the analysis, but we will present sensitivity analyses of the 100-offender criterion, showing the analysis appears insensitive to this criterion.

Using the same data applied to produce Table 1, Table 2 reports the percentage of offenders who receive prison terms by guidelines cell. Shading denotes that fewer than 50% of offenders within a cell are sentenced to prison. Prison is often imposed for federal crimes but sentencing guidelines (and hence this table) are only mandatory for felonies and serious misdemeanors. Additionally, many federal crimes are dual jurisdiction offenses, and generally the most serious crimes are prosecuted in federal court while the least serious crimes are prosecuted in state courts. These factors explain why prison terms are so frequent in these data. Still, prison is infrequent for offenders convicted of the least serious crimes provided they lack serious criminal records. For a cell to be included in the principal analysis, at least 50% of offenders in that cell must have received prison terms. Justification is that for estimating effects, the effective sample size is proportional to the number in the cell multiplied by the percentage sentenced to prison because probation sentences count as zero for these calculations. (Offenders sentenced to straight probation are assigned prison terms of length zero.) However, we will provide sensitivity testing of this selection criterion showing that findings are insensitive to the criterion.

Insert Table 2 about here

Inspection of Table 2 shows some irregularities in the progression of sentence severity, especially for the least serious offense levels, but the general pattern is that prison terms become increasingly certain as we look down the table (from least to most serious crimes) and across the table (from least to most serious offenders). Inspection of Table 3, which reports average time-served (counting probation as zero), also shows some irregularities, but the general pattern is that average time-served in prison increases as we look down the table (from least to most serious crimes) and across the table (from least to most serious offenders).

The irregularities cause a problem for the RDD. If the RDD compared cells j with j+1, for a RDD strategy to work, we must observe a substantial increase in the average time in prison as we compare cell j with cell j+1. Logically, small changes fail to create a credible contrast. Looking especially at Table 3, several cell j and j+1 contrasts fail to meet this criterion. The problem is that moving from cell j to cell j+1 is a subtle shift, and we find a better comparison to be between cell j and j+2. This is the reason for using a j/j+2 comparison instead of a j/j+1 comparison. Although comparing cells j and j+2 may raise validity issues, whatever bias is introduced is likely to be small, and from a mean-squared-error perspective, comparing j/j+2 instead of j/j+1 seems justified. However, as a sensitivity test, we also report estimates based on a comparison of cells j and j+1, and qualitative results prove insensitive to whether the comparison is between j/j+1 or j/j+2.

Insert Table 3 about here

One more table is useful for summarizing the recidivism measure. Table 4 reports the percentage of offenders who recidivate during their periods of community supervision. These statistics are right-hand-censored by the length of the time-at-risk and capped at three years. They do not account for censoring due to supervision terms less than three years. Although most offenders avoid returning to prison, returning to prison is not an infrequent occurrence. At this descriptive stage of the analysis, patterns may seem peculiar. Looking down each column, recidivism rates are lower for offenders incarcerated for the most serious crimes and higher for offenders incarcerated for the least serious crimes. Although as description this pattern is correct, the effect is probably not causal. It seems likely that offenders who serve the longest terms are older when released from prison than are offenders who serve the shortest terms, and given that age is predictive of recidivism, the patterns may be partly a result of age. Reading across the table, recidivism tends to increase with criminal history, but small sample sizes for the most serious criminal history scores may be responsible for imprecise estimates.

Insert Table 4 about here

Intended as a summary of the patterns shown by Table 4, Figure 1 presents survival curves based on Kaplan-Meier estimates stratified by criminal history categories. To be included in this analysis, an offender had to be sentenced within a guideline cell with 100 or more offenders and at least 50 percent of offenders within that cell must have received prison terms. Those restrictions are consistent with the restrictions imposed for the principal analysis presented later. However, the figure changes very little when the restrictions are removed. In general, as the criminal history category increases, so too does the rate of recidivism, although the difference between criminal history categories 5 and 6 is barely perceptible.

Insert Figure 1 about here

Table 5 extends the descriptive analysis appearing in Figure 1 to include covariates. (The table reports parameters from a Cox model, not relative hazards.) The regression is naïve in the sense that it does not account for potential confounding variables that might – if known – explain why recidivism falls as the offense severity level (and hence time-served) increases. This regression uses variables that will reappear in the local linear regressions that we use for the RDD analyses. Separate Cox regressions are reported for each criminal history category.

Scaled time-served — This is estimated time-served rescaled by dividing the original time-served by the maximum time-served. Time-served is capped at 180 months. The cap is inclusive of more than 99 percent of terms.

Time-served squared	This is the square of scaled time-served.
Scaled age	This is the offender age at the time that he or she entered community supervision rescaled to run from 0 to 1.
Age squared	This is scaled age squared.
Female	This is a dummy variable for female.
Black	This is a dummy variable for Black.
Other race	This is a dummy variable for race other than White or Black
Hispanic	This is a dummy variable for Hispanic ethnicity. It is not collinear with the race variables.
High school	This is a dummy variable indicating the offender has a high school degree or equivalent.
Advanced degree	This is a dummy variable indicating the offender has a degree beyond high school.
Lives alone	This is a dummy variable indicating the offender is neither married nor living in a common law relationship at the time of sentencing.
Centered CH points	The guidelines assign criminal history points, based on criminal records, that are collapsed into the six criminal history categories. The criminal history points are centered to have a mean of zero for each guideline cell.
District	This is the judicial district where the sentence was imposed. There are 94 federal districts including those in territories. District enters the analysis

when estimating the frailty parameter, that is, as a random effect associated with the Cox model.

Because they run from zero to one, scaled variables are useful for interpreting quadratics. The centered criminal history points provide a more refined measure of criminal record beyond the broader criminal history category. Centering this score is unimportant for the results presented in Table 5 but centering is useful for later analyses.

Insert Table 5 about here

As description, the estimates appearing in Table 5 tell a simple story: As best as a quadratic can capture the pattern, recidivism falls as time-served increases after accounting for the criminal history category. The declines are substantial and tend to be greater (but not consistently) for those with minor criminal records (category 1) than for those with serious criminal records (categories 2-6 exclusive of 5). As an illustration, when time-served changes from 30 months to 60 months, the relative hazards from criminal history 1 to criminal history 6 are: 0.81, 0.74, 0.72. 0.75, 0.81 and 0.77 respectively.[14]

Except regarding age, other parameters are less interesting for this current study because they tell us nothing about how recidivism changes with time-served. (Women are less recidivistic than men; Blacks and other races are more recidivistic than Whites; Hispanics tend to be less recidivistic than non-Hispanics. The educated are less recidivistic than the under-educated. Marital association appears to have no predictive effect. The relative importance of these covariates appears to decrease as criminal history increases.) The inverse relationship between recidivism and

[14] For example, the relative hazard for criminal history category 1 equals $\exp\left(-1.707\left(\frac{60}{180}-\frac{30}{180}\right)+0.941\left(\left(\frac{60}{180}\right)^2-\left(\frac{30}{180}\right)^2\right)\right)$.

time-served cannot be explained by age. If we were to accept results from this naïve regression, we would conclude that longer prison terms cause recidivism to fall dramatically (up to 28 percent for a 30-month increase), but we are reluctant to attach causality to these regression results, because we cannot be sure that all confounding factors have been considered. Hence, we turn to the RDD.

The results reported in Table 5 suggest that marital status does not predict recidivism. Because this variable is frequently missing, we drop it from the following analysis. The ethnic status Hispanic sometimes predicts recidivism, but the effect is not large, and this variable is often missing. Consequently, we also drop the ethnic category Hispanic from the following analysis.

ESTIMATES OF THE EFFECT THAT INCREASING TIME-SERVED HAS ON RECIDIVISM

The previous section described data sources and gross patterns in those data. This section presents evidence based on a regression discontinuity design. For every j/j+2 contrast, we compute a unique effect as a log-relative hazard. Potentially this leads to 6x41 effects because there are 41 possible contrasts within each criminal history category and there are 6 criminal history categories. In fact, we produce fewer estimates because data selection rules exclude some of the contrasts. The number entering the analysis depends on the sensitivity tests, but 175 contrasts appear in the principal analysis.

For each contrast between cells j and j+2, we estimate a Cox survival model.[15] The effect size is the parameter associated with a dummy variable coded 1 for cell j+2 and coded zero for cell j. The

[15] By default, we estimated a Cox model with covariates and shared frailty; the covariates and criterion for the shared frailty are described in the text. Sometimes to estimate the model we removed the shared frailty parameter, in which case we estimated the relative hazard using the covariates but not the shared frailty; given the RDD, the relative hazard is still

outcome measure is returning to prison following placement on community supervision or being revoked from supervision. (When an offender enters community supervision multiple times, we only include the first period of supervision in the analysis.) Time at risk is censored by the end of the study period, or by the end of community supervision or at three years if supervision is ongoing after three years. Censoring by the end of community supervision is important because thereafter an offender cannot return to federal prison unless he or she is convicted of a new federal crime. Fundamentally, once an offender successfully completes his term of community supervision, the FJSP no longer tracks his criminality, so the study cannot extend beyond the end of community supervision. Occasionally an offender may serve his entire sentence and then commit a new federal crime. Although he or she would reappear in the FJSP, we only consider the first conviction.

When applying a RDD identification strategy, researchers are careful to justify that two conditions hold. Translated into the current setting, where we compare outcomes in cell j and j+2, the two conditions are: (1) on average, time-served in cell j+2 must be greater than time-served in cell j. (2) There must be no other differences between offenders in cells j and j+2 that could account for differences in recidivism. In practice, this second condition is difficult to establish. By construction of the guidelines, and given the fact that we compare cells j and j+2 along the offense seriousness dimension and never along the criminal history dimension, we are confident this condition holds. Nevertheless, an indirect empirical test is whether the guidelines cause offenders in cell j+2 to look systematically different than offenders in cell j. This is testable.

By the design of the guidelines, we anticipate that variables sometimes associated with recidivism will have about the same mean values in cell j as in cell j+2. Of course, we expect time-

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consistent. Sometimes to estimate the model we removed both shared frailty and the covariates, in which case we estimated the relative hazard with just an indication the data came from cell j rather than j+2. Again, given the RDD, the estimate of relative hazard is consistent but not efficient. If we could not estimate the relative hazard, the estimate is reported as missing.

served to differ between cell j and cell j+2, and it does by 5.4 months on average (P<0.01). If this difference were not large, there would be little point to the analysis.

On average, other variables do not change significantly between cell j and j+2. [16] We tested the following variables: date of birth, educational attainment, sex, race, Hispanic status, marital status, date when sentenced, and criminal history point score. The differences were small, and none approached statistical significance. The results for criminal history point score seem especially important because we consider this variable to be highly correlated with recidivism. The fact that the criminal history point score does not change systematically from cell j to j+2 suggests that offenders in cell j+2 are not inherently more or less recidivistic than are offenders in cell j. Therefore, the data pass this diagnostic test.

While the inclusion of covariates may be unnecessary for identification, including covariates in the Cox regression is expected to reduce residual variance and improve the precision of the estimates. As noted, the principal variable is a dummy variable coded one if the observation is from cell j+2 and coded zero if from cell j. Other covariates with the noted exceptions are the same as were used in the naïve regression. Obviously, we have also excluded the scaled time-served variable and its square. Because there are 41x6 possible regressions (175 in this main application), we do not show the regression results from this stage of the analysis. Instead we report the single parameter of interest (the average effect) for each regression in graphical form.

Within each criminal history category, the Cox model estimates the hazard of recidivism for offenders in category j+2 relative to offenders in category j holding covariates constant. Using the

[16] The change is computed for every j/j+2 pair. The statistics reported in the texts are a weighted average of those pair comparisons. Weights equal the inverse of the sampling variances. Because of the distribution of offenses within the guidelines, the least serious criminal history categories and the middle of the offense seriousness range dominate the calculations. Arguably, the estimates are not independent, so standard errors are suspect. Nevertheless, the estimates of differences between cells j and j+2 are not biased and these average differences are small.

log-relative hazard (e.g. the parameter that is exponentiated to get the relative hazard), the next step is to estimate a regression where the estimated log-relative hazard is the dependent variable and the independent variable is the centered offense seriousness level. This is the meta-regression. It is specific to a criminal history category, so there are six meta-regressions. This specification allows us to examine how the log-relative hazard varies over the offense seriousness levels. That is, the regressions allow us to combine effects (our main motivation) and test for heterogeneity (a secondary motivation).

Effects may be heterogeneous across offense seriousness levels for two reasons. For one, offenders convicted of the most serious crimes, who serve the longest prison terms, may respond differently to incremental increases in the lengths of their terms than do offenders convicted of the least serious crimes, who serve the shortest prison terms. For another, for offenders convicted of the most serious crimes, the incremental change in the length of a prison term from cell j to j+2 is much larger than the incremental change for offenders convicted of the least serious crimes. (See Table 3.) Heterogeneity in treatment effects seems likely, but how that heterogeneity varies with offense severity level is uncertain. Examining Tables 1-3, recalling the selection rules, and recalling that we are using weighted least squares regression to estimate the heterogeneity, information used by the regression comes mostly from the middle offense seriousness levels, so incremental changes to time-served are fairly constant. Quite possibly there is heterogeneity, but we will have insufficient power to detect it.

As before, the analysis is stratified on the criminal history category, so for each regression, there are potentially 41 data points, but many fewer (29 to 37 in the main analysis) enter the meta-regression because of selection criteria identified earlier. Referring to Tables 1 and 2, almost all the lost contrasts come from having too few cases per cell or from having a low proportion of prison terms per cell; referring to Table 3, few cells (and hence contrasts) are lost because average prison terms do not increase from cell j to j+2. Because the RDD identifies the effect, the right-hand-side variables are not control variables; rather, the meta-regression is a descriptive device (with statistical implications) helping us to detect possible patterns.

Estimation has multiple steps. We summarize with an algorithm:

1. Select all offenders from criminal history category k = 1.
    a. Select all offenders with offense seriousness levels j = 1 and j = 3. If the j/j+2 contrast does not meet selection criteria, increase j by 1 and try again.
    b. It the j/j+2 contrast meets the selection criteria, estimate a Cox regression where a dummy variable is coded 1 for offenders in offense seriousness level j+2 and coded 0 for offenders in offense seriousness level j. The parameter associated with this dummy variable is the log-relative hazard for the j/j+2 contrast. Save this estimate.
    c. Increase j by 1 and repeat steps a-c until j = 41.
    d. Estimate a meta-regression where the dependent variable is the log-relative hazards from steps a-c. The independent variables are a constant and the centered offense seriousness level equal to j+2 for the j/j+2 contrast. Save the estimated constant and its standard error for use in step 3.
2. Repeat steps 1a-d for criminal history categories 1-6. This leads to six estimated constants and six estimated standard errors for step 3.
3. For step 3, create weights proportional to the inverse of the squares of the six standard errors. The final estimate is the weighted average of the six estimated constants. This is interpreted as the average log-relative hazard. Results are discussed below.

MAIN RESULTS

The main model adopts the following selection criteria:

- For a guideline cell to be included in the analysis, 100 or more offenders must appear within that cell.
- For a guideline cell to be included in the analysis, 50% or more of offenders must have received prison terms.

Comparisons are of outcomes in cell j+2 and cell j.

The main results are shown in Figure 2. The figure shows patterns in the relative hazards and associated statistics. The figure has six panels, one for each of the criminal history categories. Within each panel, the vertical axis is the scale for the relative hazard as estimated by the Cox model. A hazard equal to 1 implies that increasing prison time has a neutral effect on recidivism. A hazard greater than 1 implies that increasing prison time increases recidivism; a hazard less than 1 implies that increasing prison time decreases recidivism. The horizontal axis corresponds to the contrast: The offense seriousness level of 3 is the contrast between offense seriousness levels 1 and 3, the offense seriousness level of 4 is the contrast between levels 2 and 4, and so on until level 43. Within each panel, the dots report the relative hazards as estimated using the Cox model. When we could not estimate the relative hazard, because of the cell exclusion criteria, the point is missing. The vertical lines surrounding the dots are 95% confidence intervals for the relative hazards. They are non-symmetric and when the confidence interval extends above 2.5, it is truncated at 2.5 for improved resolution of the figures. The three lines running left to right come from the regression; the straight line shows the prediction and the curved lines represent a 95% confidence interval for the regression line – not to be confused with a 95% confidence interval for a predicted point, explaining why many points fall outside the confidence interval for the regression.

Insert Figure 2 about here

The table below the figure shows associated statistics from the meta-regression. Recall the dependent variable is the parameter associated with the log-relative hazard – that is, the natural logarithm of the relative hazard. The row specified as "linear" identifies the centered offense severity level variable entering the weighted least squares meta-regression. The simple regression specification has a constant and a term that is linear in the centered offense severity level. Because the estimator is weighted least squares, and given the offense severity level is centered, the constant is the weighted average effect across the contrasts. It is our principal interest. R^2 has its usual interpretation but note that any "explained variance" is attributable to the linear trend; because we are most interested in the constant, R^2 is not an especially important statistic and certainly has no interpretation as the "goodness of fit" for our purposes because the constant does not contribute to R^2. At a maximum, the regression would be based on 41 observations (N in the table), but fewer data points enter the analysis because (as already discussed) we discarded contrasts that failed to meet predetermined criteria.

The table below the figure requires some additional explanation. Recall the right-hand-side variable was centered, so the constant can be interpreted as an average. A constant of zero is neutral, implying that on average additional prison time neither increases nor decreases recidivism. A positive constant implies that additional prison time increases recidivism; a negative constant implies that additional prison time decreases recidivism. Converting these constants into a hazard metric just requires exponentiation: Letting α be the constant, $\exp(\alpha)$ is the relative hazard. Letting σ^2 represent the constant's estimated sampling variance, by the delta rule, the estimated sampling variance for the hazard is $\exp(\alpha)^2 \sigma^2$. These transformations are employed when drawing Figure 2.

Examining the table below Figure 2, the constant is negative and statistically significant in all six regressions. On average the estimated constant equals -0.073 and is statistically significant with a standard error of 0.007. The implied hazard is 0.930. These are the inverse-variance weighted estimates across the six criminal history categories. The linear slopes have mixed signs and are never statistically significant, so we detect no heterogeneity.

We must be caution about this interpretation. Suppose, as suggested by the evidence, the relative hazards are the same for all criminal history categories. Looking at table 3, the average increase in time-served for a move from cell j to j+2 is about 7 months for criminal history category 1 and about 10 months for criminal history category 6. Thus, a somewhat larger increase in time-served for more serious criminal history categories achieves about the same effect as a somewhat smaller increase in time-served for less serious criminal history categories. This might be taken as some evidence of criminal history heterogeneity, but if so, the evidence is not especially compelling.

Outliers are a potential concern in Figure 2. However, when an outlier appears, it is associated with a high standard error and plays a small role in the weighted least squares regression. Therefore, we conclude that outliers are not a practical concern.

On average, the estimated relative hazard that results from moving from cell j to cell j+2 is 0.930. Qualitatively, a relative hazard less than 1 implies that a longer prison term decreases recidivism, but translating this relative hazard into a quantitative decrease in the probability of recidivism is complicated because the decrease in the probability of recidivism varies across the j/j+2 comparisons. A descriptive summary provides some intuition.

First, continue to consider the relative hazard as a metric. On average, across the severity levels and the criminal history categories, moving from cell j to cell j+2 increases time-served by about 27%. (On average, this 27% change equals about 5.4 months.) This is to say that a 27% increase in the length of incarceration appears to reduce the instantaneous probability of recidivism (e.g. the hazard) by less than 10 percent. For readers who are familiar with interpreting hazards, a sizable increase in prison time (namely a 27% increase on average), has a fairly small decrease in the relative hazard (by a multiple of 0.930). Admittedly, the descriptive term *fairly small* is subjective and a reader can make his or her own subjective assessment.

A change in the instantaneous probability of recidivism – the relative hazard – is not the same as a change in the probability of recidivism. Second, then, consider the probability of recidivism as a metric. We can get a sense of how a change in the relative hazard affects the probability of recidivism. Because we are using the Cox model the probability of recidivism equals one minus the baseline survival rate to the power of the relative hazard (Hosmer, Lemeshow, & May, 2008, p. 72). For our data, the probability of recidivism is about 0.2 within three years. (See figure 1.) The relative hazard is about 0.930. So, a 27% increase in the length of incarceration would decrease the probability of recidivism by about 0.2 – ($1\text{-}0.8^{0.930}$). That is, from a base rate of recidivism of about 20%, a 27% increase in prison time would cause recidivism to fall to about 18.7%. Readers can draw their own conclusions, but our sense is the effect is not large, and that prison length of stay has a largely neutral effect on recidivism.

Once again, some caution is merited. If we again accept the relative hazards are about the same across the six criminal history categories, figure 1 shows the baseline rates of recidivism are progressively higher as the criminal history category increases from k=1 to k=6, so the percentage change in recidivism is larger for more serious criminal history categories than for the lesser criminal history categories. This might be deemed heterogeneity – that increasing time-served has a larger effect on the most serious offenders. We are reluctant to accept such a conclusion because it comes from accepting the null hypothesis of constant relative hazards across the criminal history categories. More likely, if such heterogeneity exists, we have too little power to detect its effect.

It is possible to stratify the analysis by sex, by race, and by education – variables described earlier. Cell sizes get small, so we restrict the analysis to offenders in criminal history category 1, and we require a minimum of 50 offenders per cell rather than 100. For men, the relative hazard is 0.935, and for women, it is 0.931, but these differences are not statistically significant, so we fail to reject the null that prison length has the same effect on men and on women. For white offenders, the relative hazard is 0.936 and for black offenders the relative hazard is 0.932, and the difference is not statistically significant, so we fail to reject the null that prison length has the same effect on white and black offenders. For offenders who lack high school degrees, the relative hazard is 0.930, and for offenders who have high school degrees, the relative hazard is 0.933. The difference is not statistically significance. This test, based on education, is not as sensitive as desired because most offenders have high school degrees or the equivalent. Overall, we do not find strong evidence the effect of time-served on recidivism is sensitive to sex, race, or education.

We observed that an IV estimator should lead to estimates that are like the RDD estimates. We demonstrate that here. The only difference between the IV and RDD approaches is that for the IV approach, the treatment variable is the mean time-served in cell j+2 or j, and for the RDD approach, the treatment variable is a dummy variable denoting sentencing in cell j+2 rather than cell j.[17] Interpretations differ. For the IV approach, the treatment effect is the change in recidivism per month of additional time-served; for the RDD approach, the treatment effect is the change in recidivism from being sentenced in cell j+2 rather than cell j. These differences are scale effects; we do not expect then to change substantive interpretations.

[17] Instrumental variable estimation often uses a two-step approach. In the first step, the treatment variable (here the time served) is regressed on the instrument (and perhaps some additional variables) to predict the treatment (or, in our case, the average time-served). In the second step, the average time-served replaces the observed time-served. We compute average time-served without using the first-step regression.

Figure 3 reports results from the IV estimation. Across the six criminal history categories, the weighted average effect size is -0.0122, which is statistically significant at P < 0.01. While this effect seems much smaller than the effect based on the RDD, when we multiple by the average increase in time-served between cell j and j+2, the rescaled effect is -0.066 and relative hazard of a 5.4 month increase in the sentence is 0.936. There is no material difference between the RDD estimator and the RDD estimator.

Insert Figure 3 about here

SENSITIVITY TESTING

We conducted sensitivity tests evaluating whether the effect of length of stay on recidivism depends on constraints we applied to produce the results shown in Figure 2 and its associated table.

Minimum Cell Size of 50 instead of 100 - Figure 4. A first step in sensitivity testing is to learn whether requiring a minimum of 50 cases instead of 100 cases per cell changes findings. The advantage to relaxing this assumption is that we gain a few data points. The disadvantage is that some of those new data points have extreme values. As before, the constant is always negative and statistically significant. The weighted average is -0.073 with a standard error of 0.007 and the implied relative hazard is 0.930. Because we use weighted least squares, results do not change much from those reported for the main model; despite the expansion of sample size, estimates with large standard errors do not contribute much to the regression.

Insert Figure 4 about here

Minimum cell size of 150 instead of 100 – Figure 5. If we set a criterion of 150 cases per cell instead of 100, we lose a few data points. The constant remains negative and statistically significant across the six criminal history categories. Overall, the average constant is -0.071 (with a standard error

0.008) and the corresponding relative hazard is 0.931. Apparently, the findings are insensitive to the criterion for cases per cell because the weighted least squares analysis is dominated by calculations that are based on large cell sizes. For the remaining sensitivity tests, we set the minimum cell size to 100.

Insert Figure 5 about here

Minimum percentage of prison terms 75% instead of 50% - Figure 6. The next sensitivity test changes the criterion for minimum percentage of prison terms to be 75% within a cell rather than 50% within a cell. As expected, some cells are excluded from the analysis, but as before the substantive findings do not much change. The average affect is -0.077 with standard error of 0.009. The increased standard error is understandable. Given the RDD basically calls for a difference in mean estimates between cells j and j+2 (conditional on covariates), the effective sample size for computing variances is the number of offenders within a cell multiplied by the proportion who are sentenced to prison. Cells with high proportions of offenders serving probation terms have high sampling variances for two reasons: (1) The effective sample size is smaller than the apparent sample size based on the number of offenders within a cell. (2) The average difference in time-served between cells j and j+2 tends to be small (so the effect is small and has high sampling variance) for those cells where probation is especially likely. Consequently, given the regressions use weights that are inversely proportional to the sampling variance, the estimates are insensitive to the criterion establishing a minimum proportion of offenders sentenced to prison.

Insert Figure 6 about here

Contrasts that are j and j+1 instead of j and j+2 – Figure 7. We believe that contrasts observed by comparing cells j and j+2 are more appropriate for this analysis than contrasts observed by comparing cells j and j+1 because the contrast between j and j+2 provides assurance that average prison terms increase by a substantial amount. This assurance is not forthcoming when comparing cells j and j+1. Nevertheless, increasing the distance between compared cells raises validity challenges because the RDD rests on an assumption that offenders within both compared cells are alike in ways that affect recidivism. The closer the cells, the more justified the assumption. For this sensitivity test, we maintain the original criteria for selecting cells but contrast cell j with cell j+1. (We discard contrasts where the average sentence in cell j+1 is less than the average sentence in cell j.) Averaged across the six estimates, the estimated constant is -0.030 with a standard error of 0.013. This is statistically significant at $P < 0.05$. The relative hazard is 0.970. While results from the j/j+1 contrasts are smaller than those from the j/j+2 contrasts, this is expected because the j/j+2 contrast results in a larger increase in time-served and consequently a larger effect.

Insert Figure 7 about here

Contrasts testing for auto-correlation using the Prais-Winston estimator – Figure 8. The analysis immediately above, which changed the comparison from cells j/j+2 to cells j/j+1, suggests another sensitivity test. Earlier we commented that by construction, we may have induced autocorrelation in the data used for the final step regression. For example, consider the estimated hazard for cells j and j+1 and the estimated hazard for cells j+1 and j+2. The estimates have j+1 in common and this may introduce first-order autocorrelation into the analysis. Following recommendations from Greene, we have elected to ignore this complication because commonly used corrections have uncertain small sample properties. However, we can test how much the autocorrelation affects the results using a Prais-Winston estimator, a feasible generalized least squares estimator that corrects for first-order autocorrelation.

Insert Figure 8 about here

A problem when using the Prais-Winston estimator is that we cannot simultaneously adjust for autocorrelation and heteroscedasticity. To correct estimated standard errors, we can employ a robust covariance estimator, but this is problematic in two ways. First, without adjusting for heteroscedasticity during parameter estimation, the solution is inefficient because the data points with large standard errors receive the same weight as the data points with low standard errors. Figure 2 suggests the inefficiency can be large. Second, the Prais-Winston estimator assumes homoscedasticity, but we know that our data are heteroscedastic. To deal with this problem, we adopt restrictions: (1) There must be 150 observations per cell and (2) at least 75% of offenders within any cell must receive prison terms. The idea is to minimize heteroscedasticity and we have already seen that imposing these restrictions has little effect on results. Allowing the contrast to be j/j+1, which may cause a one-lag autocorrelation, we use a Prais-Winston estimator to estimate the effects.

The sensitivity test is encouraging. The estimates for ρ, the autocorrelation efficient have mixed signs. We take this wide variation in the estimates of ρ to mean the estimates are imprecise. Apparently, provided we restrict estimates to cells with enough offenders, we induce modest autocorrelation, and our decision to ignore autocorrelation seems justified. Second, basing the results on the Prais-Winston estimator does not make a big difference in the findings. The average value for the constant is -0.042 with a standard error of 0.008.

Separating contrasts into independent sets – Tables 6 and 7. Another approach to dealing with autocorrelation is straightforward and can be performed with the j/j+2 contrasts and both the 100 observations per cell criterion and the 0.50 prison sentence criterion. To explain, consider the contrasts based on cells 1/3, 2/4, 3/5, 4/6, 5/7 and so on. The contrasts 1/3 and 2/4 are independent, and the contrast 3/5 and 4/6 are independent, but the contrasts 1/3 and 3/5 are not independent because they both rely on cell 3. A solution is to break the analysis into two parts. In the first part, we use the contrasts 1/3, 2/4, 5/7, 6/8 and so on; in the second part, we use the contrasts 3/5, 4/6, 7/9, 8/10 and so on. For each analysis, the effects can be averaged across the criminal history categories and then across the two analyses. For the first analysis, the effect is -0.034 with a standard error of 0.018. For the second analysis, the effect is -0.106 with a standard error of 0.030, significant at $p < 0.01$. The average effect is -0.067 with a standard error of 0.018. Recalling the effect from the main analysis was -0.073 with a standard error of 0.007, these new results are very close even though they are based on a different weighting scheme (a result of running two regressions instead of one). As with the previous test, we take this sensitivity test as evidence that, provided we restrict the effective number of observations per cell, results are insensitive to autocorrelation.

Insert Table 6 and Table 7 about here.

Entering prison as recidivism – Figure 9. An additional sensitivity test returns to the original model specification, whose results were presented in Figure 2, and changes the definition of recidivism from "entering prison or being revoked from community supervision" to "entering prison". We are unsure of the best definition of recidivism, but it appears the definition does not much matter. The estimated effects are about the same. The log-relative hazard is -0.076 with a standard error of 0.007; the relative hazard is 0.927. The rate of recidivism is about 20 percent using the comprehensive definition of returning to prison or being revoked and slightly less than 19 percent when using the more restrictive definition of returning to prison, so consistency between the main results and results from this sensitivity test is not surprising.

Insert Figure 9 about here

Eliminating drug law offenders from the analysis – Figure 10. A final analysis recognizes that drug law violations are somewhat peculiar to the federal system. High-level drug dealing is often dealt with in the federal system because cross-border drug dealing is a federal law enforcement responsibility, the federal government has more resources for dealing with high-level drug dealing than do state governments, and even when state and federal authorities collaborate, federal prosecution is typical because federal sentences are more severe.[18] Judges frequently sentence high-level drug dealers to Congressionally-mandated mandatory minimums, so for high-level drug dealers, the guidelines operate differently than described earlier. Another sensitivity test results from removing drug-law violators from the analysis.

Insert Figure 10 about here

With the removal of drug-law violations, fewer cells enter the analysis, but the parameter estimates do not change much so qualitative conclusions do not change. The average effect is -0.060 with a standard error of 0.008. This is statistically significant at $p<0.01$. Dropping drug law violators does not alter conclusions.

Eliminating covariates from the analysis – Figure 11. Reviewers of an earlier version of this study wondered if findings based on Cox regressions with no covariates would be the same as findings based on Cox regressions with covariates. After eliminating covariates, the weighted average effect is -0.073 with a standard error of 0.008. Removing covariates has caused the standard error to increase by about 10%, but has not altered substantive conclusions. This is sensible. The covariates were unnecessary for identification, but they have the potential to reduce standard errors.

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[18] This assertion is based on the authors' extensive experience performing program reviews for High Intensity Drug Trafficking Areas, multijurisdictional drug task forces with federal, state and local participation. Our team has visited all 28 HIDTAs over a five-year period. The assertion made in the text is based on those visits.

Figure 11 about here.

Using the Earlier Guidelines – Figure 12. Reviewers of an earlier version of this study also observed that guidelines have changed over the period of this study and expressed concern that these changes may affect conclusions. We doubt these changes matter, because cell j+2 has always had longer terms than cell j. Nevertheless, in response to reviewers' concerns, we identify the mean date for guidelines entering the analysis, and we perform the analysis on just those offenders sentenced under the guidelines prior to that mean date. The effect is – 0.088 with a standard error of 0.009. The relative hazard is 0.916. It seems unlikely that changes to the guidelines account for findings.

Figure 12 about here.

Examining offenders sentenced during the earlier years provides another implicit diagnostic test. Because the analysis is based on offenders who completed their prison terms and were released to community supervision, offenders with high offense seriousness scores or high criminal history scores and therefore long prison terms, are unlikely to enter they analysis when sentenced close to the data collection period. Any resulting bias is lessened by restricting the analysis to offenders sentenced during the early part of the data collection period. If there is a selection bias, it does not appear to be serious.

Summarizing the sensitivity testing: From the principal analysis, the estimated average effect across the six criminal history categories was -0.073 with a standard error of 0.007. Provided we contrast outcomes in cells j and j+2, results do not much change over sensitivity tests, and even when we contrast outcomes in cells j and j+1, results conform with expectations.

DISCUSSION

The effect of prison length of stay on recidivism has implications for both theory and policy. From a theoretical perspective, incarceration is a potentially transformative event experienced during an offender's life course. This study provides no evidence that an offender's criminal trajectory is negatively affected – that is, that criminal behavior is accelerated – by the length of an offender's prison term. If anything, longer prison terms modestly reduce rates of recidivism beyond what is attributable to incapacitation. This "treatment effect" of a longer period of incarceration is small. The three-year base rate of 20% recidivism is reduced to 18.7% when prison length of stay increases by an average of 5.4 months. We are inclined to characterize this as a benign, close to neutral effect on recidivism. From a policy perspective, prison length of stay can be reduced without incurring a large increase in recidivism.

All observational studies come with validity challenges. Still, a regression discontinuity design is a strong quasi-experimental design, and we have argued the guidelines' structure satisfies the assumptions of an RDD. We conducted many sensitivity tests to assess the robustness of the results. The findings require qualification. Because of limitations in the bandwidth of an RDD design, we are unable to examine how long prison terms (say five years) affect recidivism relative to relatively short terms (say two years). The logic of the RDD forces us to compare prison terms of length L relative to prison terms that are about 27% longer. While this is a limitation in the application of the RDD design, it seems unlikely that Congress or the U.S. Sentencing Commission would reduce terms by a large percentage, but not inconceivable that they might reduce prison terms by a small percentage. Thus, these findings – limited to small changes in prison terms – have implications for sentencing reform.

A secondary goal of this research was to examine potential heterogeneity in the effects of prison length of stay. To evaluate heterogeneity, we examine whether exogenous covariates moderate the effect of length of stay. Neither offense seriousness nor criminal history moderate the effect of prison length of stay. In the context of federal sentencing, these would be two of the most important potential moderators of prison length of stay. The Sentencing Reform Act of 1984 precludes federal judges from using demographic factors in assigning sentences even if these are empirical moderators of the effect of prison length of stay. We also found the effect of time-served on recidivism was invariant with respect to sex, race, and education.

One class of moderators suggested by Mears, Cochran, and Cullen (2014) is the level of post-release supervision. In many state jurisdictions a sizable number of offenders are released without supervision. For those released to supervision, the level of community control and surveillance is usually commensurate to risk. This source of heterogeneity is limited in the federal context because everyone receives a term of post-release supervision. We censored anyone whose supervision ended prior to the end of the three-year follow-up period. Post-release supervision is also a potential source of bias as suggested by Roodman, but we could eliminate this potential artifact because everyone in our sample received a term of post-release supervision and they were censored when that term ended.

Some additional caveats are applicable. First, the results apply to adults. Rarely appearing in federal court, juveniles are excluded from the analysis. Second, we offer no comparison between adult offenders sentenced to prison rather than probation. Third, although there is considerable overlap between federal and state crimes, jurisdictional differences cause federal crimes to differ from state crime counterparts, so the relationship between prison terms and recidivism at the state level may differ from the relationship at the federal level. These are important inter-jurisdictional differences, but by removing offenders who were deported or transferred to other authorities, we have made the federal offenders look more like state offenders, and when we exclude high-level drug traffickers, results do not much change. Furthermore, federal jurisdiction is national, while state jurisdictions are local, so a study based on national data has a stronger claim to generalizability. It is difficult to see how federal and state crimes are so different that criminologist can disregard these findings. A further limitation of our findings is that they apply only to offenders who receive a term of community supervision after their release. May states have offenders who receive no such post-release supervision.

While some scholars might consider the federal system unique and our results limited to federal offenders, there are many structural differences among states that often preclude generalizations beyond a specific state as well. Even if these findings are not generalizable to state jurisdictions, the federal prison population is about 12 percent of all incarcerated offenders, and represents a significant public policy concern.

As we mentioned, the RDD framework only allows us to examine relatively small increments in time-served – about 5.4 months on average. Although these are small relative changes, these are not small absolute changes. Five additional months in prison is a serious disruption of an offender's life and the associated cost savings of a five-month reduction in federal prison length of stay would save substantial capital and operational costs. We make a back-of-the-envelope estimate of potential savings in prison beds. The current average federal prison length of stay for federally sentenced prisoners is about 35 months. A reduction of 5 months in a steady state system would reduce the prison population over time by 15 percent. Given the current Bureau of Prisons inmate population of 195,000, a 15 percent reduction would save 29,000 beds. Under this scenario, the BOP could close prisons rather than simply reduce the prison population in each of its facilities saving both operational and maintenance costs. This back-of-the-envelope calculation shows that small decrements in length of stay can have large impacts when they are not confined to select subgroups of offenders.

Amplifying this latter point, one of the advantages to evaluating prison length of stay is that any change will have a broad impact on the entire incarcerated population. Most contemporary reforms, such as those found in states which implemented the Justice Reinvestment Initiative (JRI), seek to move the least serious offenders convicted of the least serious crimes from prison to community corrections (La Vigne, et al., 2014). Low-level offenders are a very small proportion of federal prison stocks, and based on tabulations from the National Corrections Reporting Program, they are also a small proportion of state prison stocks. A policy shift that reduces the length of *all* federal prison terms by 27 percent would reduce the federal prison stock by about 29,000 inmates without unduly harming public safety. This policy change would have a bigger impact on the federal prison population than diverting offenders with low offense seriousness and minor or no criminal history.

Prisons are expensive and reducing prison stays has the potential to save public expenditures (La Vigne, et al., 2014). Nevertheless, advocates of general deterrence and incapacitation might argue that maintaining or even increasing the length of prison stays is worth the additional expense and, at least, increasing prison stays will not make offenders any more criminogenic. Nagin et al. (2009) propose a theoretical model that frames these questions distinguishing between high and low sanction regimes. Higher sanction regimes have a higher probability of imprisonment and longer length of stay than low sanction regimes. This model specifies relationships between these relative regimes, the size of the prison populations, sizes of the populations with and without a prison record, and the underlying crime rate. The model provides a framework for addressing the interrelationship among these factors without precisely specifying how the outputs change under the low and high sanction regimes. It is straightforward to postulate that high sanction regimes will produce more people with criminal records; however, the impact of a high sanction regime on the crime rate is uncertain. High sanction regimes may preclude more crime through general deterrence, but may increase crime if imprisonment is criminogenic not just for the individual, but for the prisoner's family and the community (NRC, 2014). The marginal effects and the sizes of the subpopulations affected will determine the impact on crime rates. If we could analyze all the system dynamics of this model simultaneously, we could optimize the level of sanctions to minimize the crime rate. In the absence of such a comprehensive model, policy goals should be circumscribed. If length of stays were reduced incrementally, it is hard to see how this would have more than a minor effect on crime through any general deterrent, mediated impact, and from our results, through any criminogenic impact on the offender.

Savings from reduced prison usage can be diverted into evidence-based programs that reduce offending, but such diversion is unlikely as most JRI states have put savings into other non-justice programs (La Vigne, et al., 2014). Even if these funds are diverted to other social welfare programs, higher education, or other state and local needs, we would expect a net social welfare benefit.

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Tables and Figures

Table 1 - Number of Offenders Meeting Study Requirements per Guidelines Cell

Offense Seriousness Level (Least Serious (1) to Most Serious (43))	Criminal History Categories (1 least serious; 6 most serious)						
	CH 1	CH 2	CH 3	CH 4	CH 5	CH 6	Total
1	38	6	1	1	1	1	48
2	909	176	178	90	42	61	1,456
3	143	26	33	20	9	25	256
4	3,244	818	981	523	371	613	6,550
5	442	119	132	84	64	119	960
6	3,005	641	752	429	300	505	5,632
7	1,511	381	653	576	440	680	4,241
8	4,497	882	1,023	543	361	552	7,858
9	1,969	465	566	321	229	363	3,913
10	8,511	1,873	2,342	1,119	765	1,109	15,719
11	2,824	558	728	472	304	521	5,407
12	9,748	2,368	3,330	1,968	1,204	1,667	20,285
13	13,159	2,961	3,668	2,009	1,226	1,777	24,800
14	4,219	746	854	430	275	788	7,312
15	10,534	2,680	3,131	1,599	951	1,257	20,152
16	4,394	760	831	396	227	318	6,926
17	8,743	2,598	4,189	2,919	1,690	1,981	22,120
18	4,876	745	758	388	212	306	7,285
19	9,053	2,116	2,902	1,720	1,095	1,301	18,187
20	3,761	779	930	532	262	319	6,583
21	13,674	2,293	3,341	2,511	1,706	2,421	25,946
22	3,674	654	850	480	289	392	6,339
23	10,627	4,101	5,642	2,996	1,742	1,904	27,012
24	3,706	719	788	487	282	445	6,427
25	7,748	2,816	3,460	1,835	1,036	1,137	18,032
26	2,946	728	856	420	228	355	5,533
27	9,418	1,797	2,221	1,126	630	647	15,839
28	2,509	506	554	306	144	178	4,197
29	6,180	2,614	3,442	1,736	870	3,174	18,016
30	1,819	344	367	384	218	654	3,786
31	3,868	1,800	2,308	1,157	554	2,945	12,632
32	1,144	307	377	161	86	213	2,288
33	2,570	873	1,155	566	259	365	5,788
34	776	244	260	111	79	1,831	3,301
35	1,145	645	820	368	197	446	3,621
36	431	128	153	66	35	47	860

	Criminal History Categories (1 least serious; 6 most serious)						
Offense Seriousness Level	CH 1	CH 2	CH 3	CH 4	CH 5	CH 6	Total
37	543	210	305	148	54	221	1,481
38	290	83	122	59	30	64	648
39	200	62	91	36	21	58	468
40	174	37	58	22	22	35	348
41	94	31	43	18	6	23	215
42	64	23	28	9	5	8	137
43	86	24	25	12	4	19	170
All	169,266	42,737	55,248	31,153	18,525	31,845	348,774

Note: Cells with fewer than 100 observations are highlighted.

Table 2 -- Percentage of Offenders Sentenced to Prison per Guideline Cell

	Criminal History Categories (1 least serious; 6 most serious)						
Offense Seriousness Level	CH 1	CH 2	CH 3	CH 4	CH 5	CH 6	Total
1	11%	0%	0%	0%	100%	100%	13%
2	15%	20%	42%	52%	69%	72%	25%
3	11%	31%	42%	50%	78%	76%	29%
4	14%	27%	45%	68%	74%	82%	34%
5	14%	34%	54%	83%	84%	93%	42%
6	17%	40%	59%	73%	89%	94%	40%
7	16%	43%	69%	91%	95%	96%	58%
8	17%	43%	58%	86%	91%	96%	39%
9	27%	53%	79%	90%	90%	97%	53%
10	33%	75%	86%	94%	96%	96%	58%
11	63%	85%	91%	95%	99%	98%	77%
12	69%	84%	92%	96%	97%	98%	81%
13	74%	89%	94%	97%	98%	98%	83%
14	79%	90%	94%	97%	99%	98%	86%
15	82%	93%	96%	97%	99%	99%	89%
16	84%	92%	95%	98%	98%	98%	88%
17	85%	93%	97%	98%	99%	99%	92%
18	90%	95%	97%	99%	98%	99%	92%
19	89%	93%	97%	99%	99%	99%	93%
20	92%	96%	99%	98%	99%	99%	94%
21	92%	96%	98%	99%	99%	99%	95%
22	93%	96%	98%	99%	99%	99%	95%
23	93%	97%	98%	99%	99%	99%	96%
24	95%	98%	99%	100%	99%	100%	96%
25	95%	98%	98%	99%	99%	99%	97%
26	97%	98%	98%	99%	100%	100%	98%
27	96%	99%	99%	100%	99%	100%	97%
28	96%	98%	99%	99%	100%	99%	97%
29	96%	98%	99%	99%	99%	99%	98%
30	97%	100%	100%	98%	99%	100%	98%
31	97%	98%	99%	99%	99%	99%	98%
32	98%	98%	99%	99%	100%	100%	99%
33	96%	98%	99%	99%	100%	99%	97%
34	97%	100%	99%	100%	100%	99%	99%
35	97%	98%	99%	98%	99%	99%	98%
36	98%	99%	99%	97%	100%	100%	98%
37	97%	98%	99%	100%	96%	98%	98%
38	98%	99%	98%	100%	100%	98%	98%

	Criminal History Categories (1 least serious; 6 most serious)						
Offense Seriousness Level	CH 1	CH 2	CH 3	CH 4	CH 5	CH 6	Total
39	98%	98%	99%	100%	100%	98%	99%
40	98%	97%	100%	100%	100%	100%	99%
41	98%	100%	100%	100%	100%	100%	99%
42	97%	100%	100%	100%	100%	100%	99%
43	98%	100%	96%	100%	100%	95%	98%
All	78%	89%	93%	97%	98%	98%	87%

Note: Cells with less than 50% prison terms are highlighted.

Table 3 -- Average Time Served (Months) per Guideline Cell

Criminal History Categories (1 least serious; 6 most serious)							
	Offense Seriousness Level	CH 1	CH 2	CH 3	CH 4	CH 5	CH 6
Least Serious (1) to Most Serious (43)	1	0.4	0.0	0.0	0.0	20.9	6.5
	2	1.0	1.3	2.5	3.6	4.1	7.3
	3	0.8	1.7	2.7	4.7	4.7	7.3
	4	1.3	2.2	3.5	5.7	6.7	8.5
	5	1.3	2.1	4.2	8.7	9.0	13.8
	6	1.3	4.1	4.9	8.4	11.5	14.0
	7	1.1	2.9	5.0	8.1	11.7	14.6
	8	1.4	3.8	5.9	9.6	13.7	17.5
	9	2.1	5.0	7.1	11.9	16.8	20.8
	10	3.0	7.2	9.4	15.0	19.1	23.0
	11	4.3	8.2	11.9	16.1	21.7	25.1
	12	6.0	10.3	13.7	18.9	23.7	27.9
	13	8.1	12.4	15.7	21.2	26.0	30.0
	14	10.6	16.0	18.7	25.6	29.8	32.4
	15	12.0	17.0	20.4	25.6	32.0	35.5
	16	14.7	19.8	23.3	30.3	35.9	40.8
	17	16.4	22.2	26.0	32.6	39.2	44.6
	18	19.6	25.7	29.7	36.2	45.4	51.5
	19	21.6	28.2	32.6	40.7	49.8	54.7
	20	26.2	34.3	40.1	48.1	57.3	63.7
	21	25.5	34.2	39.4	48.1	57.0	63.6
	22	31.3	40.9	48.3	58.2	68.9	72.1
	23	33.6	42.9	48.4	57.7	67.0	72.8
	24	36.9	49.3	54.3	68.5	78.6	85.3
	25	41.8	49.5	56.2	67.1	78.5	84.2
	26	48.0	57.1	67.6	78.3	90.2	96.4
	27	47.4	59.7	65.9	78.3	86.8	90.9
	28	55.2	70.6	78.4	90.2	96.1	104.8
	29	58.4	72.2	81.3	87.8	97.6	98.1
	30	68.5	84.5	93.8	106.3	112.0	121.3
	31	69.5	82.3	91.6	103.9	112.9	111.7
	32	85.0	99.1	111.6	124.1	141.7	131.2
	33	77.9	93.5	105.6	120.3	133.8	140.1
	34	96.9	116.6	122.0	136.5	139.6	132.6
	35	90.0	103.3	118.2	133.5	138.5	140.3
	36	117.6	126.5	145.6	151.0	155.1	146.1

Criminal History Categories (1 least serious; 6 most serious)							
	Offense Seriousness Level	CH 1	CH 2	CH 3	CH 4	CH 5	CH 6
	37	102.8	128.9	135.6	160.4	138.2	184.9
	38	127.9	143.4	162.5	172.6	139.9	176.9
	39	107.2	146.8	154.0	156.7	154.4	165.6
	40	141.4	152.0	164.8	148.2	149.0	165.8
	41	131.5	156.2	178.8	204.5	212.4	189.9
	42	126.3	187.8	146.8	197.4	142.2	197.4
	43	149.7	168.5	207.8	290.9	128.0	224.3

Table 4 -- Percentage of Offenders Recidivating within Three Years (Not Accounting for Censoring) per Guideline Cell

		Criminal History Categories (1 least serious; 6 most serious)					
	Offense Seriousness Level	CH 1	CH 2	CH 3	CH 4	CH 5	CH 6
Least Serious (1) to Most Serious (43)	1	39.5%	33.3%	100.0%	0.0%	0.0%	100.0%
	2	23.2%	32.4%	38.2%	43.3%	52.4%	32.8%
	3	23.8%	34.6%	42.4%	45.0%	44.4%	44.0%
	4	20.1%	33.4%	39.9%	40.5%	41.5%	51.2%
	5	21.5%	37.8%	37.9%	44.0%	42.2%	45.4%
	6	19.8%	34.8%	43.0%	46.2%	40.7%	47.5%
	7	21.2%	33.6%	44.9%	50.7%	50.2%	56.8%
	8	17.7%	33.0%	32.4%	38.5%	42.7%	43.8%
	9	18.2%	28.6%	33.7%	34.0%	39.7%	47.7%
	10	16.7%	26.2%	35.0%	36.7%	38.3%	45.9%
	11	11.6%	22.8%	29.9%	32.6%	36.5%	40.7%
	12	13.0%	26.6%	33.5%	38.8%	44.2%	47.5%
	13	13.2%	24.8%	29.7%	36.8%	39.6%	44.4%
	14	9.4%	18.0%	29.7%	34.7%	36.0%	39.1%
	15	12.9%	24.1%	31.5%	38.6%	42.0%	42.3%
	16	9.5%	18.6%	27.6%	32.3%	30.0%	37.7%
	17	9.9%	20.7%	28.0%	34.2%	38.2%	42.5%
	18	9.5%	17.9%	24.0%	31.2%	27.4%	35.9%
	19	10.6%	19.7%	27.4%	32.0%	35.9%	41.4%
	20	9.4%	18.1%	24.7%	30.1%	33.6%	37.6%
	21	10.3%	17.2%	23.5%	28.3%	33.6%	35.2%
	22	8.1%	19.1%	25.9%	31.3%	35.6%	37.5%
	23	8.4%	14.2%	19.2%	24.7%	29.3%	33.0%
	24	8.6%	17.2%	22.1%	27.7%	33.7%	34.8%
	25	7.7%	12.2%	16.7%	21.5%	25.1%	27.7%
	26	7.0%	12.6%	17.3%	19.3%	24.1%	31.8%
	27	7.1%	12.4%	15.7%	18.6%	21.6%	25.3%
	28	7.2%	14.4%	14.8%	18.0%	20.1%	29.8%
	29	6.3%	9.3%	13.1%	16.9%	20.8%	22.1%
	30	7.3%	9.6%	16.6%	18.8%	23.9%	25.2%
	31	5.2%	9.9%	12.9%	13.7%	18.1%	16.6%
	32	6.8%	9.4%	13.5%	16.1%	10.5%	17.4%
	33	5.1%	8.6%	12.7%	13.3%	20.1%	18.6%
	34	4.4%	9.0%	8.8%	15.3%	21.5%	11.8%
	35	5.3%	6.2%	8.7%	12.2%	17.8%	14.3%
	36	4.2%	5.5%	8.5%	13.6%	22.9%	12.8%
	37	5.2%	9.0%	9.5%	13.5%	9.3%	16.3%

		Criminal History Categories (1 least serious; 6 most serious)					
	Offense Seriousness Level	CH 1	CH 2	CH 3	CH 4	CH 5	CH 6
	38	2.4%	2.4%	5.7%	6.8%	23.3%	15.6%
	39	2.0%	4.8%	9.9%	13.9%	19.0%	5.2%
	40	4.0%	10.8%	10.3%	18.2%	13.6%	11.4%
	41	3.2%	3.2%	4.7%	11.1%	33.3%	13.0%
	42	6.3%	0.0%	14.3%	22.2%	20.0%	12.5%
	43	8.1%	0.0%	20.0%	16.7%	25.0%	15.8%

Table 5 -- Results from a Naive Regressions (Cox Proportional Hazard)

	CH 1	CH 2	CH 3	CH 4	CH 5	CH 6
scaled time-served	-1.707	-2.381	-2.536	-1.941	-1.079	-1.588
	(0.155)**	(0.212)**	(0.155)**	(0.187)**	(0.235)**	(0.161)**
time-served squared	0.941	1.166	1.154	0.487	-0.336	0.016
	(0.263)**	(0.328)**	(0.234)**	(0.263)	(0.323)	(0.192)
scaled age	-7.718	-6.272	-6.200	-6.174	-6.028	-2.779
	(0.257)**	(0.385)**	(0.295)**	(0.388)**	(0.496)**	(0.468)**
age squared	6.798	5.207	5.927	6.619	6.433	1.160
	(0.503)**	(0.775)**	(0.608)**	(0.791)**	(0.983)**	(0.881)
female	-0.348	-0.343	-0.281	-0.219	-0.280	-0.331
	(0.021)**	(0.035)**	(0.028)**	(0.040)**	(0.052)**	(0.041)**
Black	0.194	0.115	0.120	0.088	-0.016	-0.054
	(0.023)**	(0.030)**	(0.022)**	(0.026)**	(0.031)	(0.023)*
other race	0.567	0.426	0.348	0.301	0.371	0.267
	(0.030)**	(0.049)**	(0.039)**	(0.049)**	(0.062)**	(0.051)**
Hispanic	-0.085	-0.095	-0.072	-0.018	0.008	-0.053
	(0.024)**	(0.037)**	(0.028)**	(0.034)	(0.041)	(0.034)
high_school	-0.124	-0.142	-0.127	-0.106	-0.088	-0.036
	(0.039)**	(0.049)**	(0.036)**	(0.042)*	(0.051)	(0.038)
advanced_degree	-0.611	-0.441	-0.423	-0.287	-0.169	-0.051
	(0.053)**	(0.084)**	(0.070)**	(0.088)**	(0.112)	(0.078)
lives_alone	-0.023	0.006	0.011	-0.020	-0.033	-0.036
	(0.021)	(0.029)	(0.021)	(0.025)	(0.030)	(0.022)
centered points	0.311	-0.026	0.100	0.057	0.051	0.041
	(0.015)**	(0.022)	(0.009)**	(0.012)**	(0.013)**	(0.002)**
N	143,058	38,733	52,654	30,049	17,697	30,247

Table 6 – Results when Contrasts are Cells 1/3, 2/4, 5/7, 6/8, 9/11, 10/12 and so on

	Criminal History 1	Criminal History 2	Criminal History 3	Criminal History 4	Criminal History 5	Criminal History 6
linear	0.002	-0.002	-0.002	-0.003	-0.008	-0.005
	0.004	0.003	0.004	0.003	0.005	0.003
constant	-0.074	-0.034	-0.050	-0.012	0.054	-0.054
	0.048	0.041	0.041	0.037	0.062	0.041
R^2	0.010	0.040	0.020	0.060	0.150	0.110
N	15	15	18	19	17	19

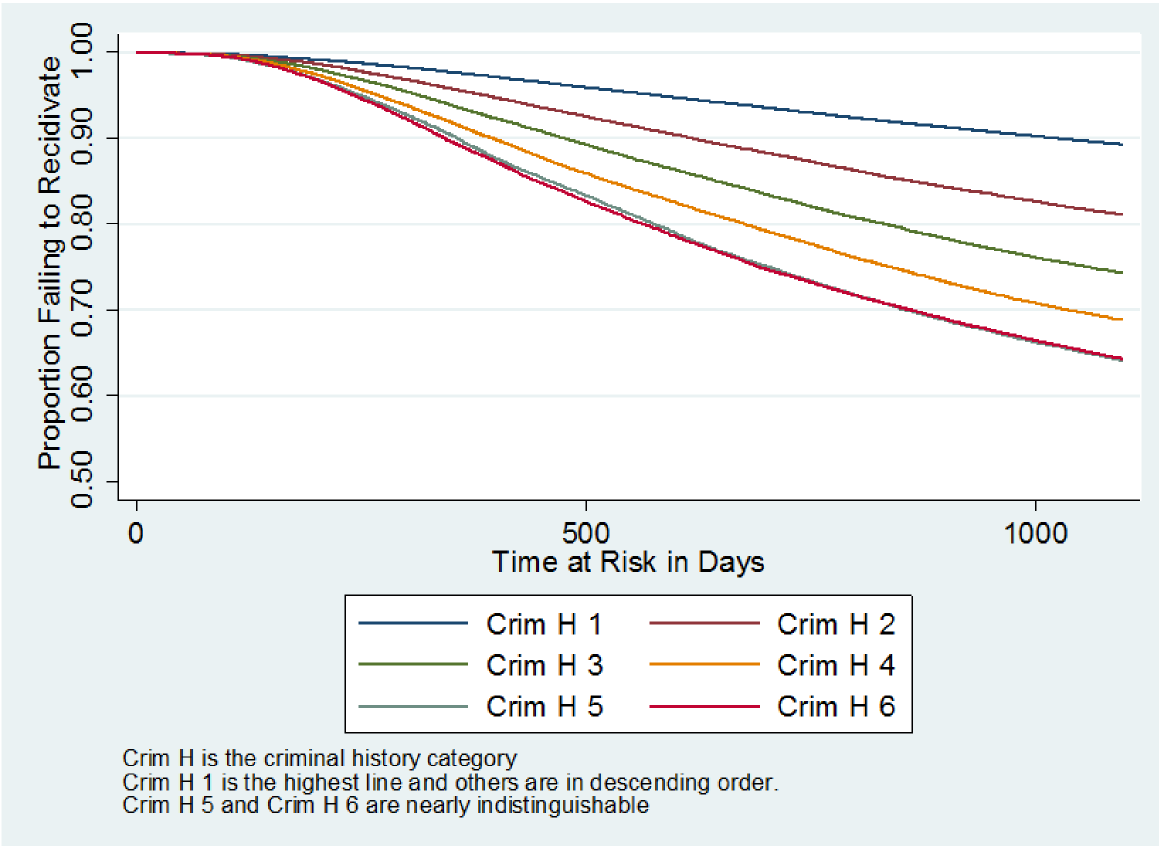
* p<0.05; ** p<0.01
The constant is the average parameter for the estimated hazard (zero is neutral)
Six criminal history scores labeled Criminal History.
The contrasts are cells 1/3, 2/4, 5/7, 6/8, 9/11, 10/12 and so on. All the contrasts are independent.

Table 7 – Results when Contrasts are Cells 2/4, 3/5, 6/8, 7/9, 10/12, 11/13 and so on

	Criminal History 1	Criminal History 2	Criminal History 3	Criminal History 4	Criminal History 5	Criminal History 6
linear	0.003	0.004	0.003	-0.002	0.007	0.000
	-0.003	-0.005	-0.003	-0.004	-0.005	-0.004
constant	-0.103	-0.153	-0.109	-0.097	-0.188	-0.049
	(0.032)**	-0.071	(0.033)**	-0.050	(0.064)*	-0.048
R^2	0.060	0.070	0.060	0.010	0.120	0.000
N	16	14	18	17	17	18

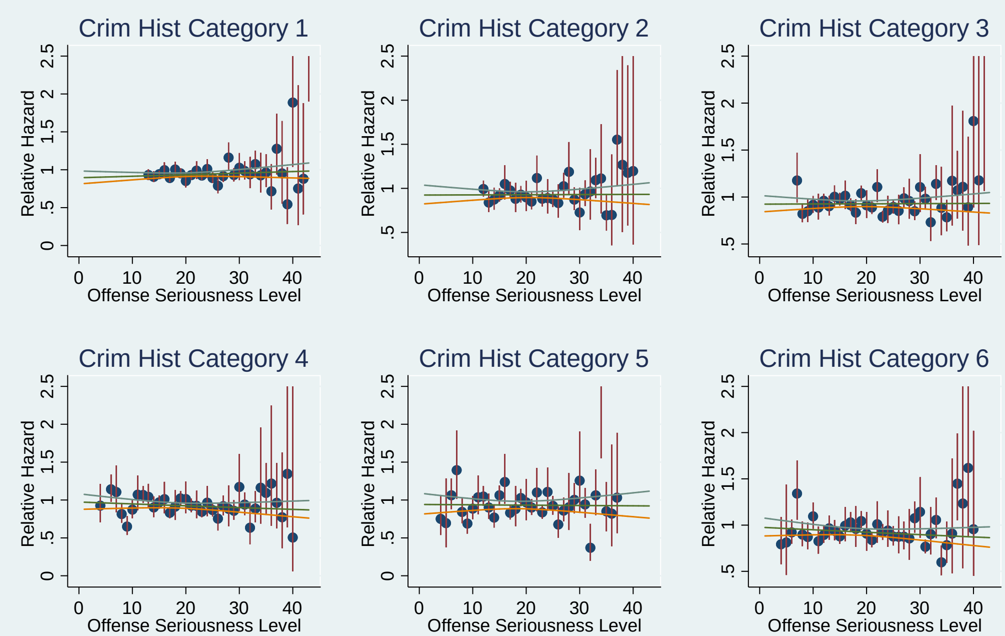
* p<0.05; ** p<0.01
The constant is the average parameter for the estimated hazard (zero is neutral)
Six criminal history scores labeled Criminal History.
The contrasts are cells 2/4, 3/5, 6/9, 7/10, 10/12, 11/13, etc.

Figure 1 – Kaplan-Meier Estimates of Time until Recidivism by Criminal History Category



* *p*<0.05; ** *p*<0.01
Six criminal history scores labeled Criminal History.
Scales variables run from 0 to 1
Centered points: Criminal history points centered at zero.

Figure 2 – Relationship between Time-Served and Recidivism Principal Specification (Return and Revocation)

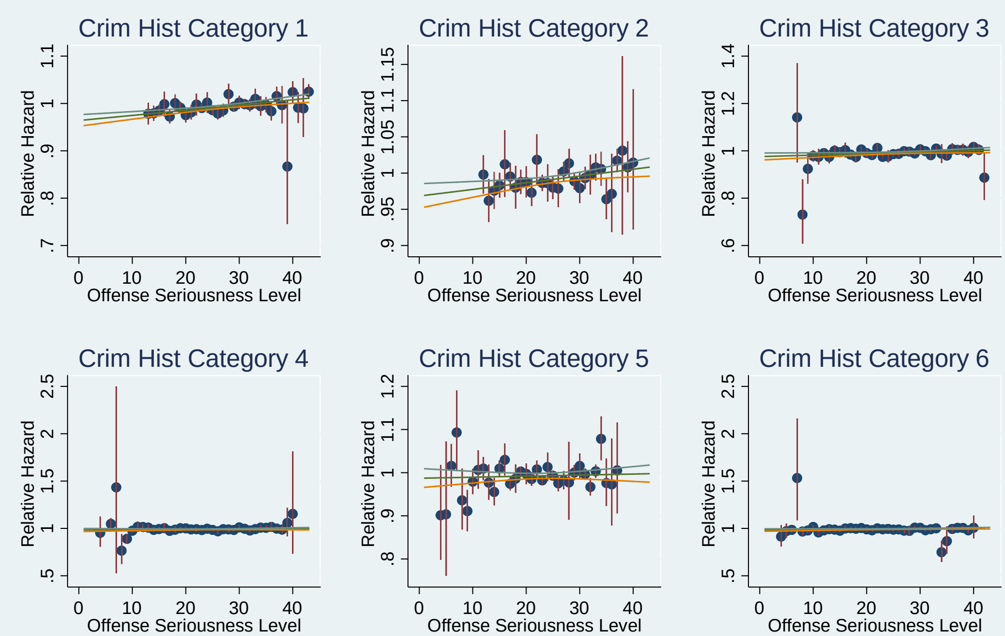


Dots are estimated relative hazards (shown if between -2.5 and 2.5)
Vertical lines are 95% confidence interval for the estimated relative hazards (truncated at 2.5)
Smooth lines are linear predictions and 95% confidence intervals for the regression line.

	Criminal History 1	Criminal History 2	Criminal History 3	Criminal History 4	Criminal History 5	Criminal History 6
linear	0.002	0.000	0.000	-0.003	0.000	-0.003
	(0.002)	(0.003)	(0.002)	(0.003)	(0.004)	(0.003)
constant	-0.068	-0.075	-0.075	-0.076	-0.068	-0.077
	(0.013)**	(0.019)**	(0.017)**	(0.019)**	(0.028)*	(0.020)**
R^2	0.03	0.00	0.00	0.03	0.00	0.03
N	31	29	36	36	34	37

* *p*<0.05; ** *p*<0.01
The constant is the average parameter for the estimated hazard (zero is neutral)
Six criminal history scores labeled Criminal History.

Figure 3 – Relationship between Time-Served and Recidivism Principal Specification (Return and Revocation) using an Instrumental Variable Estimator

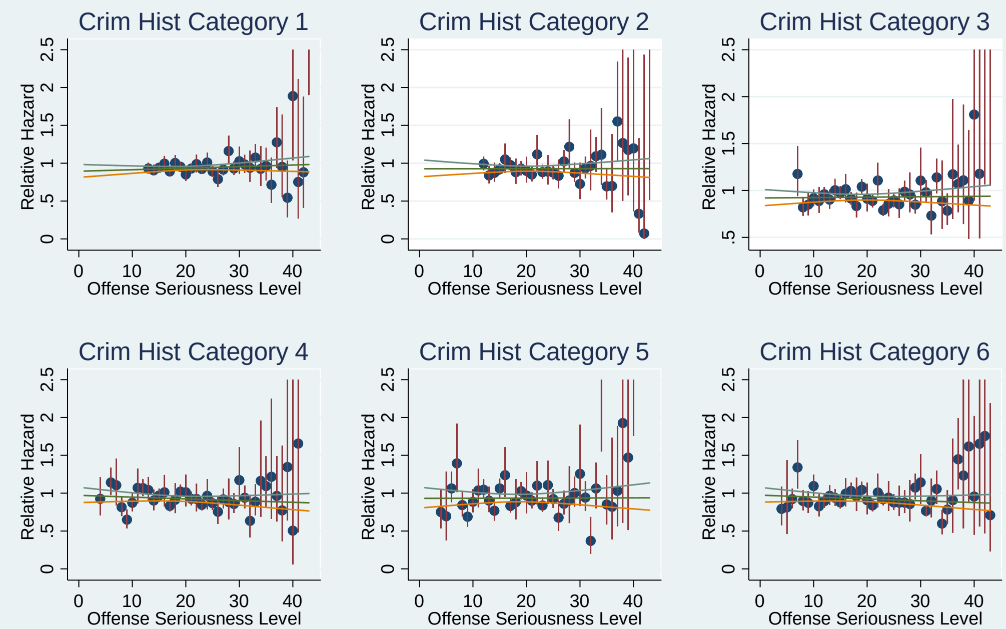


Dots are estimated relative hazards (shown if between -2.5 and 2.5)
Vertical lines are 95% confidence interval for the estimated relative hazards (truncated at 2.5)
Smooth lines are linear predictions and 95% confidence intervals for the regression line.

	Criminal History 1	Criminal History 2	Criminal History 3	Criminal History 4	Criminal History 5	Criminal History 6
linear	0.001	0.001	0.001	0.000	0.000	0.000
	(0.000)**	(0.000)**	(0.000)*	0.000	0.000	0.000
constant	-0.014	-0.015	-0.011	-0.011	-0.009	-0.009
	(0.002)**	(0.003)**	(0.003)**	(0.003)**	0.005	(0.003)**
R^2	0.43	0.22	0.12	0.03	0.01	0.10
N	31	29	36	36	34	37

* $p<0.05$; ** $p<0.01$
The constant is the average parameter for the estimated hazard (zero is neutral)
Six criminal history scores labeled Criminal History.

Figure 4 – Sensitivity Test: Minimum Cell Size 50 instead of 100

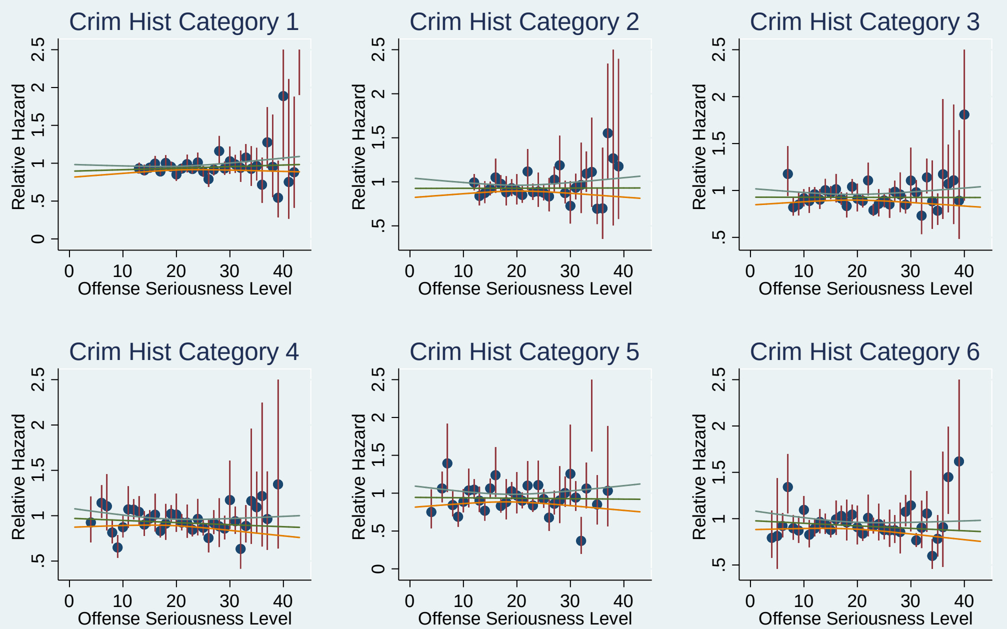


Dots are estimated relative hazards (shown if between -2.5 and 2.5)
Vertical lines are 95% confidence interval for the estimated relative hazards (truncated at 2.5)
Smooth lines are linear predictions and 95% confidence intervals for the regression line.

	Criminal History 1	Criminal History 2	Criminal History 3	Criminal History 4	Criminal History 5	Criminal History 6
linear	0.002	0.000	0.000	-0.002	0.000	-0.003
	(0.002)	(0.003)	(0.002)	(0.003)	(0.004)	(0.002)
constant	-0.068	-0.075	-0.073	-0.078	-0.068	-0.084
	(0.013)**	(0.018)**	(0.018)**	(0.019)**	(0.027)*	(0.021)**
R^2	0.03	0.00	0.00	0.02	0.00	0.03
N	31	32	37	37	37	40

Six criminal history scores labeled Criminal History.
* $p<0.05$; ** $p<0.01$
The constant is the average parameter for the estimated hazard (zero is neutral)

Figure 5 – Sensitivity Test: Minimum Cell Size 150 instead of 100

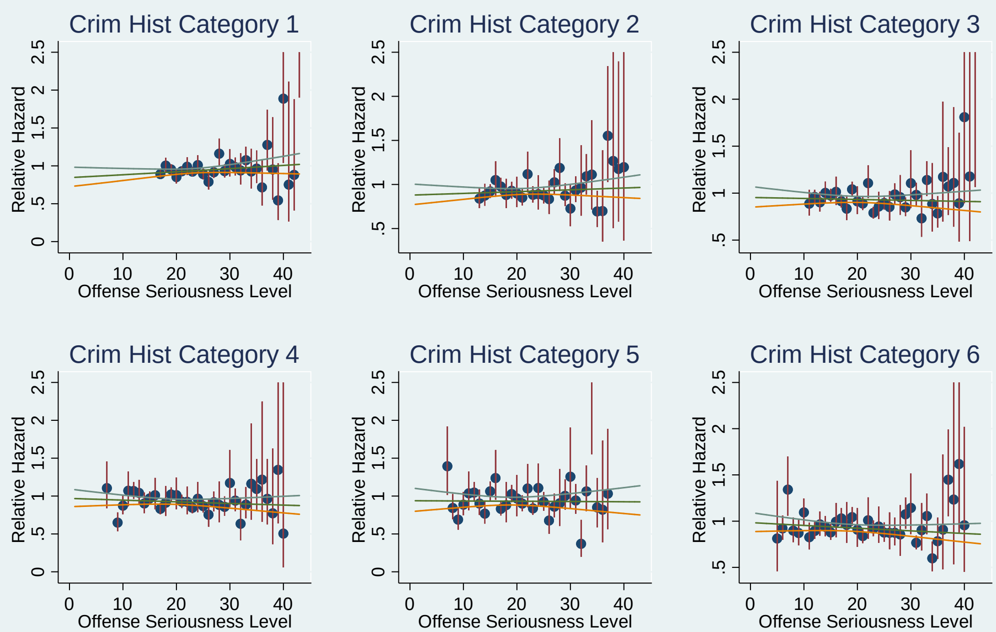


Dots are estimated relative hazards (shown if between -2.5 and 2.5)
Vertical lines are 95% confidence interval for the estimated relative hazards (truncated at 2.5)
Smooth lines are linear predictions and 95% confidence intervals for the regression line.

	Criminal History 1	Criminal History 2	Criminal History 3	Criminal History 4	Criminal History 5	Criminal History 6
linear	0.002	0.000	0.000	-0.003	-0.001	-0.003
	(0.002)	(0.003)	(0.002)	(0.003)	(0.004)	(0.003)
constant	-0.068	-0.075	-0.076	-0.071	-0.066	-0.071
	(0.013)**	(0.021)**	(0.017)**	(0.020)**	(0.031)*	(0.021)**
R^2	0.03	0.00	0.00	0.03	0.00	0.04
N	31	28	34	34	32	35

* p<0.05; ** p<0.01
The constant is the average parameter for the estimated hazard (zero is neutral)
Six criminal history scores labeled Criminal History.

Figure 6 -- Sensitivity Test: Minimum Percentage of Prison Terms 75% instead of 50%

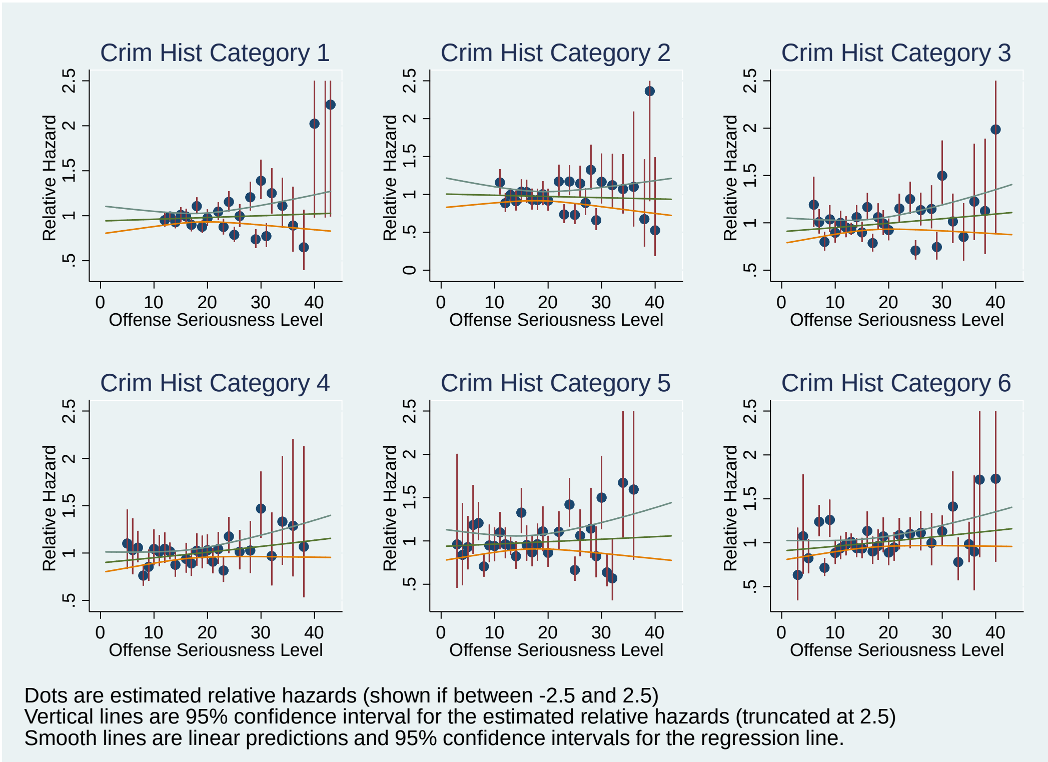


Dots are estimated relative hazards (shown if between -2.5 and 2.5)
Vertical lines are 95% confidence interval for the estimated relative hazards (truncated at 2.5)
Smooth lines are linear predictions and 95% confidence intervals for the regression line.

	Criminal History 1	Criminal History 2	Criminal History 3	Criminal History 4	Criminal History 5	Criminal History 6
linear	0.004	0.002	-0.001	-0.002	0.000	-0.003
	(0.003)	(0.003)	(0.003)	(0.003)	(0.004)	(0.003)
constant	-0.086	-0.091	-0.070	-0.074	-0.069	-0.076
	(0.021)**	(0.023)**	(0.017)**	(0.020)**	(0.031)*	(0.020)**
R^2	0.07	0.02	0.01	0.02	0.00	0.04
N	27	28	32	33	31	36

* p<0.05; ** p<0.01
The constant is the average parameter for the estimated hazard (zero is neutral)
Six criminal history scores labeled Criminal History.

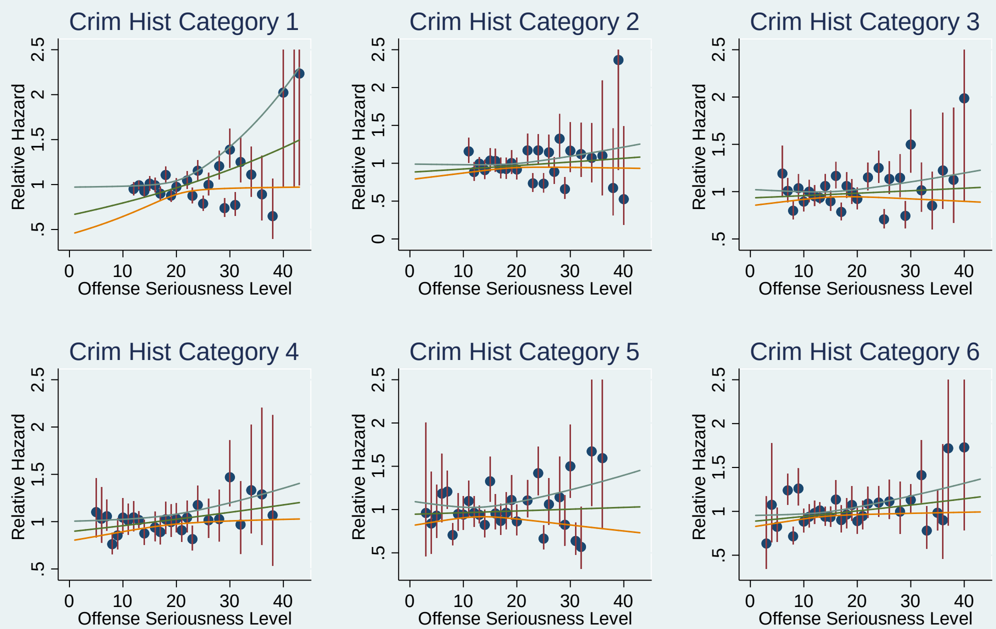
Figure 7 – Sensitivity Test: Contrasts that are J and J+1 instead of J and J+2



	Criminal History 1	Criminal History 2	Criminal History 3	Criminal History 4	Criminal History 5	Criminal History 6
linear	0.002	-0.002	0.005	0.006	0.003	0.006
	(0.004)	(0.005)	(0.004)	(0.004)	(0.006)	(0.004)
constant	-0.030	-0.017	-0.037	-0.039	-0.031	-0.023
	(0.029)	(0.040)	(0.032)	(0.028)	(0.046)	(0.028)
R^2	0.010	0.00	0.04	0.10	0.01	0.09
N	25	25	27	26	29	29

* p<0.05; ** p<0.01
The constant is the average parameter for the estimated hazard (zero is neutral)
Six criminal history scores labeled Criminal History.

Figure 8 -- Sensitivity Test: Using the Prais-Winston Estimator

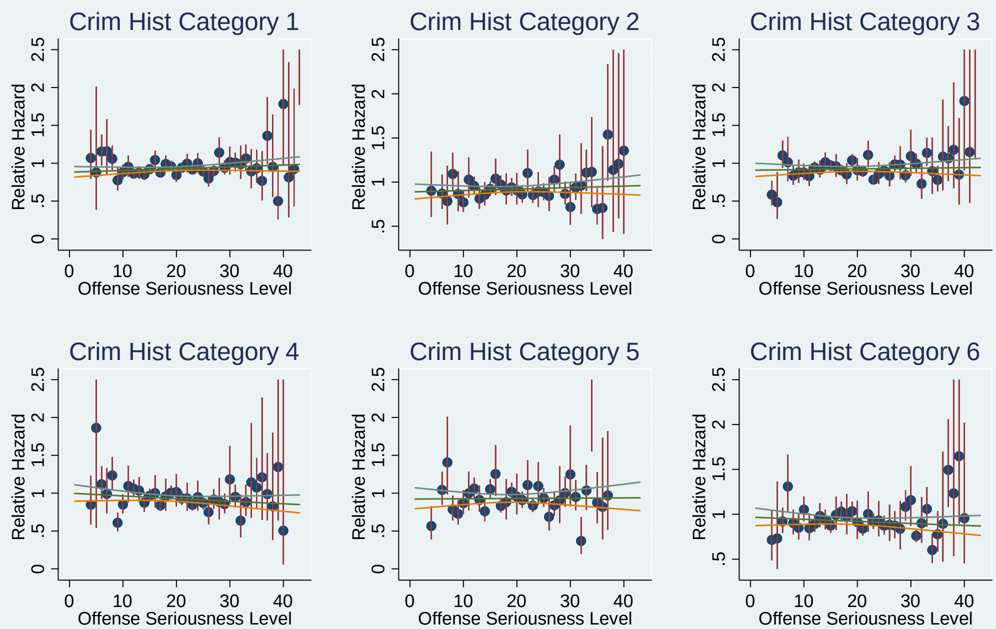


Dots are estimated relative hazards (shown if between -2.5 and 2.5)
Vertical lines are 95% confidence interval for the estimated relative hazards (truncated at 2.5)
Smooth lines are linear predictions and 95% confidence intervals for the regression line.

	Criminal History 1	Criminal History 2	Criminal History 3	Criminal History 4	Criminal History 5	Criminal History 6
linear	0.019	0.005	0.003	0.007	0.002	0.006
	-0.010	-0.003	-0.003	(0.003)*	-0.006	(0.003)*
constant	-0.133	-0.060	-0.035	-0.030	-0.032	-0.040
	0.065	0.020	0.016	0.031	0.029	0.013
Rho	0.100	-0.940	-0.790	0.010	-0.260	-0.730
R^2	0.240	0.210	0.110	0.210	0.020	0.330
N	25	25	27	26	29	29

* p<0.05; ** p<0.01
The constant is the average parameter for the estimated hazard
Six criminal history scores labeled Criminal History.

Figure 9 -- Sensitivity Test: Entering Prison as Recidivism

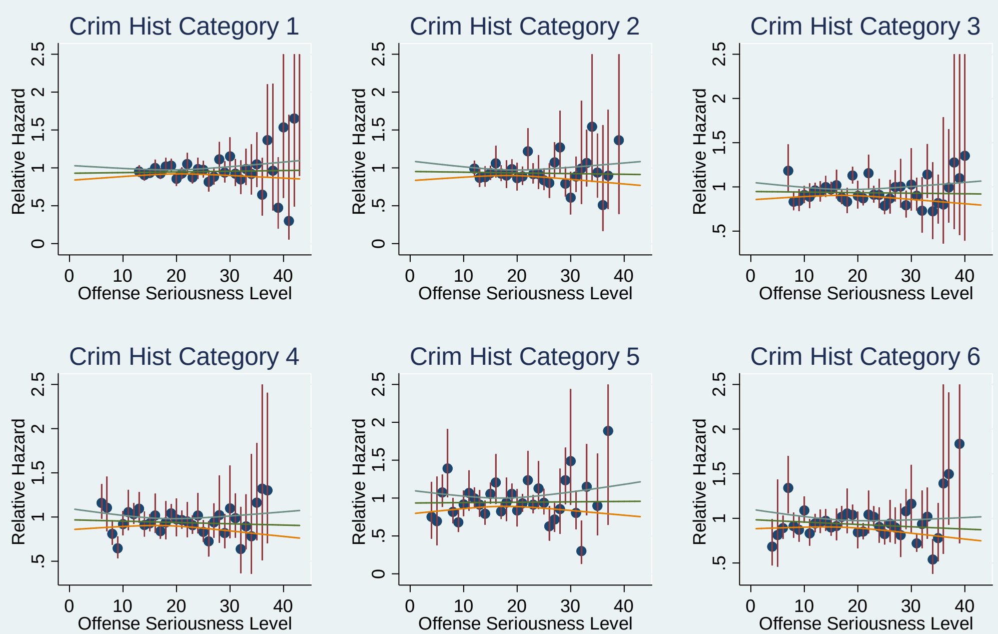


Dots are estimated relative hazards (shown if between -2.5 and 2.5)
Vertical lines are 95% confidence interval for the estimated relative hazards (truncated at 2.5)
Smooth lines are linear predictions and 95% confidence intervals for the regression line.

	Criminal History 1	Criminal History 2	Criminal History 3	Criminal History 4	Criminal History 5	Criminal History 6
linear	0.003	0.002	0.001	-0.004	0.000	-0.003
	(0.002)	(0.002)	(0.002)	(0.003)	(0.004)	(0.003)
constant	-0.070	-0.084	-0.077	-0.071	-0.074	-0.080
	(0.014)**	(0.017)**	(0.018)**	(0.020)**	(0.030)*	(0.020)**
R^2	0.04	0.02	0.00	0.05	0.00	0.03
N	40	36	39	37	33	37

* $p<0.05$; ** $p<0.01$
The constant is the average parameter for the estimated hazard (zero is neutral)
Six criminal history scores labeled Criminal History.

Figure 10 -- Sensitivity Testing: Eliminating Drug-Law Offenders from the Analysis

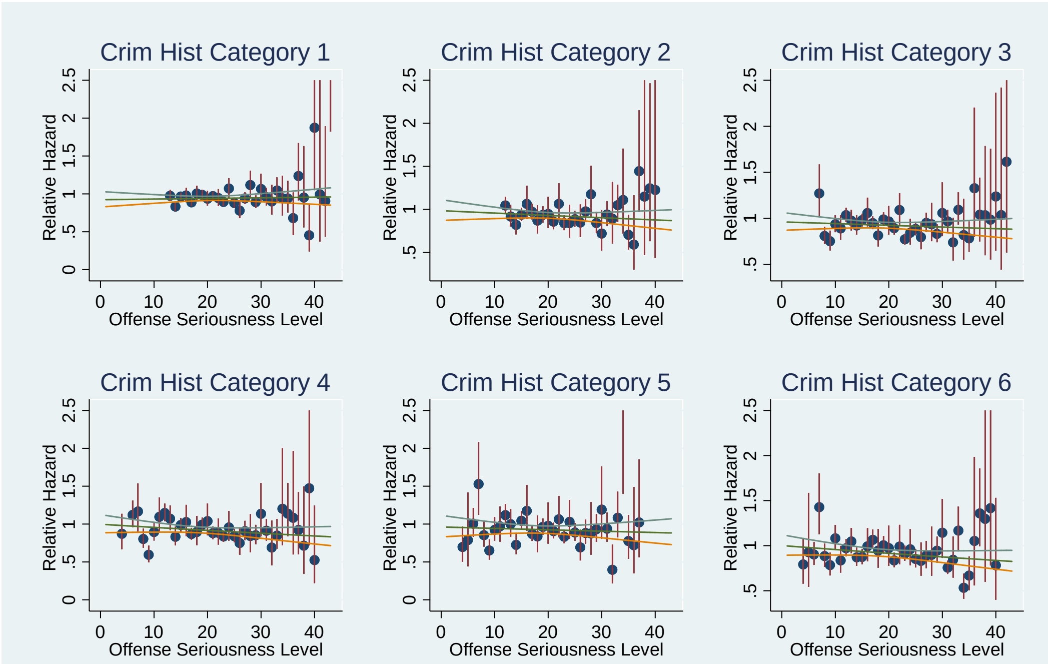


Dots are estimated relative hazards (shown if between -2.5 and 2.5)
Vertical lines are 95% confidence interval for the estimated relative hazards (truncated at 2.5)
Smooth lines are linear predictions and 95% confidence intervals for the regression line.

	Criminal History 1	Criminal History 2	Criminal History 3	Criminal History 4	Criminal History 5	Criminal History 6
linear	0.001	-0.001	-0.001	-0.002	0.001	-0.003
	(0.003)	(0.004)	(0.003)	(0.003)	(0.005)	(0.003)
constant	-0.055	-0.065	-0.066	-0.057	-0.059	-0.062
	(0.014)**	(0.022)**	(0.019)**	(0.022)*	-0.031	(0.022)**
R^2	0.00	0.00	0.00	0.01	0.00	0.03
N	31	27	34	32	32	35

* p<0.05; ** p<0.01
The constant is the average parameter for the estimated hazard (zero is neutral)
Six criminal history scores labeled Criminal History.

Figure 11 -- Sensitivity Testing: No Covariates

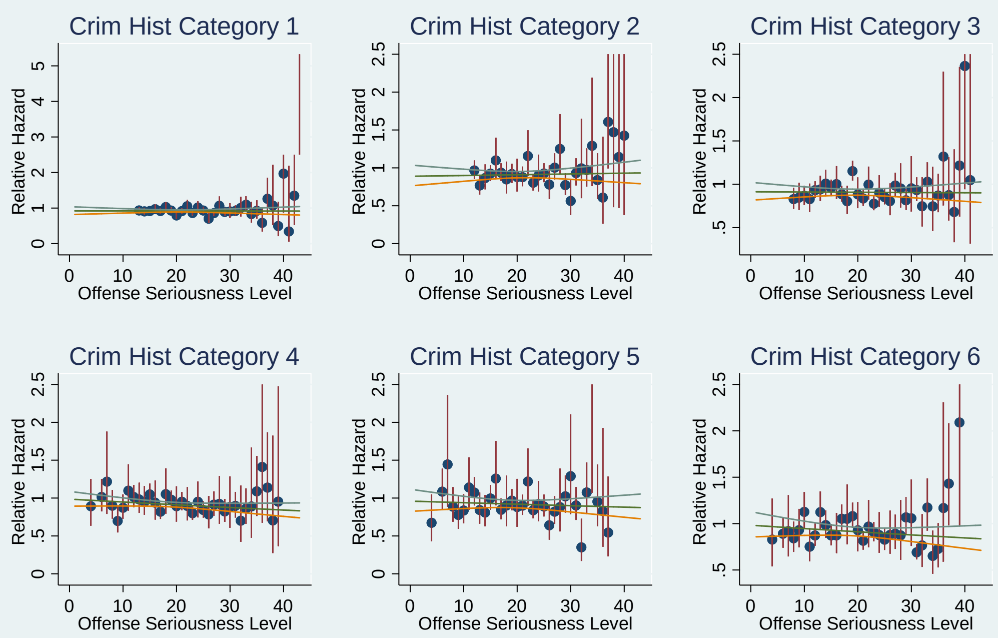


Dots are estimated relative hazards (shown if between -2.5 and 2.5)
Vertical lines are 95% confidence interval for the estimated relative hazards (truncated at 2.5)
Smooth lines are linear predictions and 95% confidence intervals for the regression line.

	Criminal History 1	Criminal History 2	Criminal History 3	Criminal History 4	Criminal History 5	Criminal History 6
linear	0.001	-0.003	-0.002	-0.004	-0.002	-0.004
	(0.003)	(0.003)	(0.003)	(0.003)	(0.004)	(0.003)
constant	-0.062	-0.067	-0.081	-0.082	-0.071	-0.084
	(0.015)**	(0.020)**	(0.018)**	(0.022)**	(0.028)*	(0.022)**
R^2	0.00	0.03	0.02	0.05	0.01	0.07
N	31	29	36	36	34	37

* p<0.05; ** p<0.01
The constant is the average parameter for the estimated hazard (zero is neutral)
Six criminal history scores labeled Criminal History.

Figure 12 -- Sensitivity Testing: Using the Earlier Guidelines



Dots are estimated relative hazards (shown if between -2.5 and 2.5)
Vertical lines are 95% confidence interval for the estimated relative hazards (truncated at 2.5)
Smooth lines are linear predictions and 95% confidence intervals for the regression line.

	Criminal History 1	Criminal History 2	Criminal History 3	Criminal History 4	Criminal History 5	Criminal History 6
linear	0.000	0.001	0.000	-0.004	-0.002	-0.004
	(0.003)	(0.004)	(0.003)	(0.002)	(0.004)	(0.003)
constant	-0.085	-0.097	-0.096	-0.085	-0.076	-0.081
	(0.017)**	(0.025)**	(0.019)**	(0.018)**	(0.030)*	(0.028)**
R^2	0.00	0.00	0.00	0.07	0.01	0.04
N	31	29	34	35	33	34

* p<0.05; ** p<0.01
The constant is the average parameter for the estimated hazard (zero is neutral)
Six criminal history scores labeled Criminal History.