ANNUAL REVIEWS

Annual Review of Criminology

The Impact of Incarceration on Recidivism

Charles E. Loeffler[1] and Daniel S. Nagin[2]

[1]Department of Criminology, University of Pennsylvania, Philadelphia, Pennsylvania 19104; email: cloef@upenn.edu

[2]Heinz College, Carnegie Mellon University, Pittsburgh, Pennsylvania 15213

Annu. Rev. Criminol. 2022. 5:133–52

First published as a Review in Advance on September 29, 2021

The *Annual Review of Criminology* is online at criminol.annualreviews.org

https://doi.org/10.1146/annurev-criminol-030920-112506

Keywords

incarceration, recidivism, offending, imprisonment effects

Abstract

The US prison population stands at 1.43 million persons, with an additional 740,000 persons in local jails. Nearly all will eventually return to society. This review examines the available evidence on how the experience of incarceration is likely to impact the probability that formerly incarcerated individuals will reoffend. Our focus is on two types of studies, those based on the random assignments of cases to judges, called judge instrumental-variable studies, and those based on discontinuities in sentence severity in sentencing grids, called regression discontinuity studies. Both types of studies are designed to account for selection bias in nonexperimental estimates of the impact of incarceration on reoffending. Most such studies find that the experience of postconviction imprisonment has little impact on the probability of recidivism. A smaller number of studies do, however, find significant effects, both positive and negative. The negative, recidivism-reducing effects are mostly in settings in which rehabilitative programming is emphasized and the positive, criminogenic effects are found in settings in which such programming is not emphasized. The findings of studies of pretrial incarceration are more consistent—most find a deleterious effect on postrelease reoffending. We also conclude that additional work is needed to better understand the heterogeneous effects of incarceration as well as the mechanisms through which incarceration effects, when observed, are generated. For policy, our conclusion of the generally deleterious effect of pretrial detention adds to a larger body of evidence pointing to the social value of limiting its use.

INTRODUCTION

The population of individuals incarcerated in US prisons, whether federal or state, has declined every year since 2009, which was the population peak following a three-decade-long stretch of annual increases. By the close of 2019, the prison population stood at 1.43 million, a decline of 17% in the per capita imprisonment rate from 2009 (Carson 2020b). Still, the current imprisonment rate remains more than four times higher than the historical level of approximately 110 per 100,000 that prevailed prior to the steady rise from the early 1970s to 2009 (Blumstein & Cohen 1973). Adding populations in local jails to the prison population, nearly 1% of the US adult population is incarcerated (Maruschak & Minton 2020, Zeng & Minton 2021).

Although incarceration rates in other countries are not nearly as high as in the United States (Natl. Res. Counc. 2014), in all countries the human cost of incarceration to taxpayers, prisoners, and their families raises the question of whether the return in terms of public safety is justified. The threat of the prison sanction may prevent crime by what criminologists refer to as general deterrence, whereby individuals refrain from committing crimes out of fear of the sanctions that might be incurred were they to commit a crime. Crime prevented by general deterrence has the great advantage of avoiding both the costs of crime and the costs of punishment, as Cesare Beccaria [1764 (1819)] recognized with his observation that "it is better to prevent crimes than punish them."

Still, deterrence fails too often, thereby triggering punishment. Punishments involving incarceration may reduce crime by incapacitation—the physical separation of the incarcerated individual from free society. It may also reduce the incarcerated individual's subsequent criminality following release by what criminologists call special or specific deterrence. Unlike the concept of general deterrence, first described two-and-a-half centuries ago by Enlightenment philosophers Jeremy Bentham and Cesare Beccaria and formalized by Becker (1968), the concept of specific deterrence is not well specified. It implies that the actual experience of punishment, in this case, incarceration, has a chastening impact that was not anticipated when deterrence failed and the crime was committed. Beyond deterrence, the experience of punishment may affect future criminality by many other mechanisms, some of which may also reduce future criminal involvement, e.g., by rehabilitation, whereas others may increase criminal involvement, e.g., due to social stigma and labeling or a deterioration of the human capital necessary for success in legal labor markets.

This review focuses on the evidence of how the experience of incarceration, whether in a jail or prison, affects recidivism. We label this effect with a term—the effect of incarceration on recidivism—that is deliberately agnostic about the direction of effect. In the jargon of causal inference, our aim is to review the evidence on the treatment effect of incarceration. The term treatment effect is inherently comparative to some alternative form of treatment. For the purposes of this review, that alternative is either a nonincarceration sanction, such as probation for individuals convicted of a crime, or, for individuals held in jail pretrial, not being so held.

This review builds from a prior review, "Imprisonment and Reoffending" (Nagin et al. 2009), by one of the authors. That review concluded that "compared with noncustodial sanctions, incarceration appears to have a null or mildly criminogenic effect on future offending" (Nagin et al. 2009, p. 115). However, the review also observed that "the existing research [was] limited in size, in quality, in its insights into why a prison term might be criminogenic or preventative, and in its capacity to explain why imprisonment might have differential effects depending on offenders' personal and social characteristics" (Nagin et al. 2009, p. 115). Subsequent reviews by Villettaz et al. (2006, 2015) and Roodman (2017) reached the similar conclusion that there was little evidence that the experience of incarceration reduced reoffending.

The focus of this review is on two types of analyses that began appearing after Nagin et al. (2009) that have materially improved our capacity to draw more confident conclusions about the effect of incarceration on reoffending—judge instrumental-variable (IV) and regression discontinuity (RD) studies. These studies were not a central focus of the reviews by Villettaz et al. (2006, 2015) and were only partially covered by Roodman (2017). A major challenge to making confident inferences about the effect of incarceration on reoffending is selection—judges may be more likely to incarcerate pretrial and sentence to incarceration postconviction those individuals who are perceived to be more prone to reoffend. Older studies attempted to account for selection with the use of matching and regression techniques to control for measured covariates such as age, sex, charge type, and prior record. These studies, however, remained vulnerable to biases from unmeasured covariates affecting both selection and recidivism. The more recent studies we review here are less likely to be affected by this form of selection bias.

The judge IV and RD studies we review examine the effect of incarceration on recidivism in a range of settings, including juvenile and adult correctional institutions, pretrial and postconviction facilities, and US and non-US localities. For US-based studies of adults, both types of studies reach strikingly similar conclusions to those in Nagin et al. (2009). The studies find, with only two exceptions, that postconviction imprisonment either has no effect on reoffending or exacerbates it. Studies of the effect of pretrial incarceration on reoffending find that pretrial detention results in higher rates of conviction and often higher rates of recidivism.

JUDGE INSTRUMENTAL-VARIABLE STUDIES

IV studies, once a niche way of estimating systems of simultaneous equations (e.g., supply and demand equations in economics), have now become a mainstay of estimating the causal impact of incarceration on reoffending. Unlike standard general linear regression models, IV regressions do more than condition on observable characteristics of cases to avoid selection bias contaminating the estimated impact of assignment to prison on reoffending. Instead, IV regressions exploit plausibly exogenous variation in the assignment-to-incarceration process to approximate the benefits normally associated with randomized control trials (Angrist et al. 1996). This exogenous variation frequently comes in the form of naturally occurring variability in the use of incarceration that is likely independent of the outcome of incarceration. For this reason, IVs are often described as the product of a natural experiment. In the literature dealing with imprisonment effects, the most common form of this natural experiment is the random assignment of cases to judges—a practice that historically has been implemented to curtail judge shopping. Randomization ensures that caseload characteristics for both measured and unmeasured case features (e.g., criminal history, offense seriousness, risk of recidivism) are the same across judges. Such balance in case features, coupled with stable and sizable differences across judges in their propensity to use imprisonment, provides the basis for using the judicial assignment as the IV for obtaining an unbiased estimate of the impact of imprisonment compared to noncustodial sanctions. Judges with identical caseloads but higher use of incarceration can be compared to judges with identical caseloads and lower use of incarceration. If recidivism rates for their caseloads are also identical, then it is unlikely that the differential use of incarceration has a causal impact on recidivism. However, if the outcomes for the same two or more judges are seen to noticeably differ, these recidivism differences are likely caused by their differential use of incarceration. Beyond the assumption of random assignment, the unbiasedness of IV-based causal estimates depends on other assumptions. For our purposes here, the most important assumption is the so-called monotonicity assumption, which we discuss below. The ubiquity of random judge assignment for felony criminal cases, along with the increasing availability of large-scale administrative data sets, figures significantly in the recent expansion in IV literature that estimates the effects of postconviction imprisonment and also in the emerging literature that examines the effects of pretrial incarceration.

Table 1 summarizes the thirteen studies that utilize a version of the IV research design to estimate the impact of incarceration on recidivism. Summarized study features include the population examined, the counterfactual comparison made, the measure of recidivism, the main non-IV result, and the main IV result.

Postconviction Imprisonment

Green & Winik (2010) is the first published study to use judge IVs to infer the impact of incarceration on reoffending. Their study is based on a sample of just over 1,000 drug case defendants in Superior Court in Washington, DC, in which cases were randomly assigned to nine judges who also systematically differed in their use of imprisonment. After confirming that randomization had produced observably similar presentencing caseloads across judges and variation in the use of imprisonment of up to 40%, they found no discernable effect of imprisonment on rearrest within four years of conviction. Encountering the common problem that reoffending during the follow-up period is affected both by the reduction in time-at-risk generated by incapacitation and any changes in postrelease reoffending patterns, the authors simulated how incarceration impacted cases least likely to be affected by incapacitation. The results of this simulation suggest that there may be some weak evidence for criminogenic effects after accounting for the short-term benefits of incapacitation. However, the central finding remained that incarceration had no discernable effect on recidivism after four years.

Loeffler (2013) applied the judge IV research design to a sample of more than 20,000 felony defendants from Cook County, Illinois. Significant differences in judge propensity to imprisonment were identified, which is a necessary prerequisite for successful inference in the IV setup. He found no significant effects of imprisonment on recidivism or employment over a five-year observation period after indictment. The observed null effect is the same as Green & Winik's (2010) finding for Washington, DC, drug offenders, suggesting that these results potentially generalize onto the broader population of nondrug felony defendants, at least in northern Illinois. However, because this study made no attempt to adjust estimates for any incapacitation effects, the estimated imprisonment null effect represents the bundled treatment of any behavioral effect of incarceration and any incapacitation effect. For this reason, the analysis may not have detected any criminogenic behavioral impact.

Nagin & Snodgrass (2013) is another early judge IV effects analysis. It is based on data for individuals convicted of felonies in Pennsylvania. They examined all counties within Pennsylvania and identified six counties that met the three necessary inclusion criteria—confirmed randomization of cases, stable sentencing practices, and significant interjudge sentencing differences in their use of incarceration. They examined an annual rearrest rate, recalculated at 1, 3, 5, and 10 years from sentencing, using statewide criminal history data for their sample of 6,515 cases. The impact analyses for the 5- and 10-year observation periods make this study less prone to measuring incapacitation than the previous two studies, resulting in a clearer view of the enduring behavioral effects of imprisonment. Also, the availability of rearrest data for the entire state means that these results are less likely to underestimate recidivism as compared to the previous two studies. Even with these important methodological differences, similar to Green & Winik (2010) and Loeffler (2013), no significant effects of imprisonment on recidivism were observed for either statewide pooled results or individual counties.

Although these three early IV papers suggest no effect of imprisonment on recidivism, at least on average, several newer papers do find evidence of an impact. Harding and colleagues

Table 1 Instrumental-variable studies of incarceration versus nonincarceration

Authors	Population	Counterfactual		Outcome		
					Estimated effect of incarceration on recidivism	
		Incarceration	Nonincarceration	Follow-up	OLS Result	IV Result
Green & Winik (2010)	Felony drug cases, Washington, DC ($n = 1{,}001$)	Prison	Probation	Rearrest within 4 years	Preventative	Null
Loeffler (2013)	Felony cases, Cook County, IL ($n = 20{,}297$)	Prison	Probation	Rearrest within 5 years	Criminogenic	Null
Nagin & Snodgrass (2013)	Felony cases, Pennsylvania ($n = 6{,}515$)	Prison	Probation	Rearrest within 1, 3, 5, and 10 years	NA	Null
Mueller-Smith (2015)	Felony cases, Harris County, TX ($n = 462{,}377$)	Jail/prison	Release/probation	Rearrest, reconviction after release	Criminogenic	Criminogenic
Harding et al. (2017)	Felony cases, Michigan ($n = 111{,}110$)	Prison	Probation	Reconviction within 5 years	Criminogenic	Null
				Reincarceration within 5 years	Criminogenic	Criminogenic
Aizer & Doyle (2015)	Juvenile cases, Chicago, IL ($n = 37{,}692$)	Juvenile detention	Juvenile probation	Reincarceration as an adult	Criminogenic	Criminogenic
Bhuller et al. (2019)	Criminal cases, Norway ($n = 33{,}548$)	Prison	Probation	New charges within 5 years	Criminogenic	Preventative
Williams & Weatherburn (2020)	Criminal cases, New South Wales, Australia ($n = 8{,}826$)	Prison	Electronic monitoring	New charges within 10 years	Criminogenic	Criminogenic
Dobbie et al. (2018)	Criminal cases, Philadelphia, PA ($n = 302{,}862$)	Pretrial detention	Release	Rearrest within 2 years	Null	Null
Gupta et al. (2016)	Criminal cases, Philadelphia and Pittsburgh, PA ($n = 862{,}163$)	Pretrial detention	Release	New charges in year after release	NA	Criminogenic
Leslie & Pope (2017)	Felony cases, New York, NY ($n = 111{,}754$)	Pretrial detention	Release	Rearrest within 2 years	Criminogenic	Criminogenic
Heaton et al. (2017)	Misdemeanor cases, Harris County, TX ($n = 380{,}689$)	Pretrial detention	Release	New charges within 18 months	NA	Criminogenic
Di Tella & Schargrodsky (2013)	Criminal cases, Province of Buenos Aires, Argentina ($n = 24{,}362$)	Pretrial detention	Electronic monitoring	Reincarceration within 2–3 years	Criminogenic	Criminogenic

Abbreviations: IV, independent variable; NA, not available; OLS, ordinary least squares.

(Harding et al. 2017) examine the effects of imprisonment in Michigan using a judge IV setup and statewide data comparing ex-prisoners to probationers.[1] Consistent with earlier studies, they report no discernable effect of imprisonment on recidivism as measured by reconviction for a new offense. However, they do report that imprisonment leads to higher levels of reimprisonment among ex-prisoners. This unusual finding—the absence of a difference in the probability of reconviction and the presence of large increases in reimprisonment, the authors conclude, is driven by readmissions for technical violations, which in turn are driven by differences in the supervision of parolees and probationers. This finding illustrates how the commonly used measures of recidivism (rearrest, reconviction, and reimprisonment) are not always interchangeable for the purposes of estimating the effects of correctional interventions (Andersen & Skardhamar 2017, Maltz 1984). It also suggests that at least some measures of recidivism may be more usefully thought of as primarily measuring criminal justice contact rather than criminal behavior itself. In those situations in which the use of more inclusive measures of criminal justice contact (e.g., rearrest, reconviction) fails to generate significant effects but more restrictive measures (e.g., reincarceration) does, it is likely that incarceration is criminalizing existing behaviors such as failure to comply with parole restrictions rather than generating novel criminal behaviors.

Another recent judge IV paper, using a rich and large data set from Harris County, TX, reports an increase in recidivism after release from imprisonment (Mueller-Smith 2015). The baseline ordinary least squares (OLS) regression estimates in this paper show a 2% increase in the probability of rearrest after release. After instrumenting the probability of incarceration using a judge and covariate interacted instrument that allows for a relaxing of one of the key assumptions of the judge IV estimator, i.e., that all judges are uniformly punitive or lenient across crime types (which defines the monotonicity assumption), the author reports a larger and significant effect of 6% per quarter. Although this increase is sizable, it is notable that the sign of the effect remains the same from the reported OLS estimates. The effects observed also appear to be quite similar to those reported for another Harris County paper focusing on misdemeanor cases discussed below (see Heaton et al. 2017), potentially indicating commonalities between the impact of pretrial and postconviction incarceration in this locality.

Roach & Schanzenbach (2015) examined sentencing in Washington State for a sample of just under 9,000 criminal defendants who pleaded guilty and had their cases assigned to a quasi-random assortment of judges hearing such cases. Unlike previous studies, the exogenous variation used in this study is the length of prison sentences rather than the in/out imprisonment decision. However, an imbalance in a key covariate, offense seriousness, makes this paper's substantive result—a reduction in one-year recidivism rates of 1% on an instrumented one-month increase in imprisonment—difficult to interpret as an unbiased estimate of a causal effect. We, therefore, do not include this paper in our overall assessment of the evidentiary implications of the judge IV studies.

Compared to the voluminous literature on the effects of adult incarceration, less work has been done on the effects of juvenile incarceration. This may be because the per capita level of juvenile incarceration is much lower than the adult level and also juvenile incarceration rates have been dropping rapidly over the past decade (Puzzanchera 2018). As with the adult incarceration literature, regression-based studies of the impact of juvenile incarceration report a positive correlation between incarceration and recidivism (Walker & Herting 2020). Also, as with the adult studies, the

[1]Harding and colleagues report similar results examining individuals given split sentences instead of probation (Menefee et al. 2020).

challenge in interpreting this correlation is distinguishing selection effects from real incarceration effects. Juvenile courts do not routinely randomly assign cases to judges. As an alternative, Aizer & Doyle (2015) use juvenile court judicial calendars to create judge IVs. Specifically, in Cook County, Illinois, the source of the data used in this study, judges work on an assigned work schedule that is not randomly allocated. Despite this nonrandomness, the similarity of their caseloads and the inability of defendants to self-sort to different judicial calendars preserves many of the benefits of the randomized judge IV. After confirming that the setup did produce adequate covariate balance on measured case and individual characteristics and that there were significant differences across judges in their use of pre- and postadjudication detention, the authors report large adverse effects of juvenile incarceration on adult incarceration. However, they did not estimate the impacts on rearrest and reconviction measures.

In this regard, the results are comparable to those reported in Harding et al. (2017), which examined the impact of adult imprisonment on future adult imprisonment. Both papers find a strong unadjusted relationship between prior imprisonment and future imprisonment. However, the results of OLS and IV specification reported in Aizer & Doyle (2015) are also quite consistent with those reported in other non-IV-based studies of pretrial detention's effects on recidivism measured by new court filings (see Walker & Herting 2020). Thus, the Aizer & Doyle finding may reflect a behavioral effect, as was the case in Walker & Herting (2020), or a supervision effect, as was the case in Harding et al. (2017).

Several judge IV studies have also examined non-US samples of cases. Each of these serves as an important contrast to the US postconviction papers. Bhuller et al. (2019) examine the impact of incarceration on recidivism and employment among a sample of Norwegian criminal cases randomly assigned to judges across the country. This study is notable for several reasons. The first is that it used national-level register crime data, thereby avoiding many of the coverage issues that complicate US-based imprisonment studies. The second is that the study's central findings showed that imprisonment reduced, not increased, recidivism. They also found that incarceration increased employment, especially for individuals without prior attachment to the labor market. The authors interpret this result as reflecting the Norwegian prison system's focus on rehabilitative services, including employment training services.

With such a strong finding and novel results compared to US-based studies, it is worth considering the differences between Norwegian and US prisons. One is that Norway makes far more sparing use of imprisonment than the United States. Indeed, prison beds are scarce—so scarce that there is a waiting list for convicted individuals to serve their court-ordered prison sentences (Andersen et al. 2020). Another important difference is that Norway's prison system is far more oriented toward rehabilitation than the US system. Combined, these two differences may explain both the salutary impact of the prison experience in Norway and why the noted salutary effect grows with time. Initially, there is no difference in the rates of recidivism for the incarcerated and the nonincarcerated. Given the queuing to serve sentences, the initial null effect may be explained by the absence of any applied treatment immediately following sentencing. As individuals assigned to incarceration move up in the queue and begin serving their sentences, this may produce the observed growth in crime reductions. Although the reported results, especially for employment, are impressive, the lingering and unobserved time-to-treatment issue suggests that the follow-up period of several years may need to be extended to allow for a clearer picture of the behavioral effect of imprisonment on recidivism. This study also reinforces the importance of coupling strong identification with detailed follow-up to ascertain the timing of treatment, treatment compliance, and relevant competing risks, including mortality. Perhaps most importantly, it also makes clear the importance of prison conditions and the prison experience in the determination of both the direction and magnitude of the treatment effect of incarceration.

Another non-US judge IV study also examined the effects of imprisonment on recidivism, but instead of comparing imprisonment to probation or a similarly conventional community supervision regime, it compared imprisonment to community-supervised electronic monitoring (EM) (Williams & Weatherburn 2020). Like the juvenile detention study, this one bases its judge IV analysis on plausibly exogenous variation in case assignment to judges within the study location (New South Wales, Australia) rather than on full random assignment. Nonetheless, the authors observe good covariate balance between judges who significantly differ in their propensities to use EM rather than imprisonment. At the low-end of EM propensity, some judges assign just 2% of their caseload to EM, whereas at the high-end judges assign 25% of their caseload to it. Exploiting this variability in sentencing patterns, the study's authors find that assignment to EM compared to incarceration reduces the probability of reoffending by 22% within five years and 11% within ten years.

What do the judge IV studies tell us about the effect of postconviction imprisonment on recidivism for adults? For studies based on US data, a majority concludes that the effect is indistinguishable from zero (Green & Winik 2010, Loeffler 2013, Nagin & Snodgrass 2013). One study by Harding et al. (2017) finds evidence of a ratchet effect whereby past imprisonment increases future imprisonment resulting from criminal justice system processes such as parole revocation; another study by Mueller-Smith (2015) finds evidence of a criminogenic effect. None find evidence of a crime-reduction effect.

When the field of view is expanded to include juvenile imprisonment and non-US imprisonment, the results are more varied. Consistent with the non-IV-based literature on the impact of juvenile detention on recidivism, Aizer & Doyle (2015) find a pronounced deleterious effect. Williams & Weatherburn's (2020) analysis of incarceration compared to EM based on Australian data finds that persons receiving an EM sentence had markedly lower rates of recidivism. The findings of these three studies are consistent with the findings of the adult US-based studies that incarceration, at best, has no effect and sometimes has a criminogenic effect but never a preventive effect. The one study finding a preventive effect, Bhuller et al. (2019), is based on data from Norway, a country with a commitment to providing intensive rehabilitative services to incarcerated individuals. It is an important reminder that the treatment effect of imprisonment depends on what happens within the prison walls.

Our review thus far also leads us to several additional conclusions. First, most analyses reporting both IV and OLS estimates find that these different methodologies produce estimates having the same sign and comparable magnitudes. This suggests that concerns about biases arising from selection on unobserved case or person characteristics that spawned the IV literature ex-post may have been misplaced. Second, the work by Harding et al. (2017) suggests that even in the absence of a large behavioral effect, imprisonment can trigger a one-way ratchet of increased future use of imprisonment as a response to typical (and likely unaffected) levels of continued reoffending. Third, Mueller-Smith (2015) and subsequent work by Frandsen et al. (2020) have called attention to certain subtleties of key assumptions of the IV setup when it is applied to judges. One of these assumptions is known as the monotonicity assumption, which requires that judges differ in the level of their use of imprisonment and also that that level difference is the same in proportionate terms across crime type (e.g., judge A is twice as likely to impose a prison sentence as judge B for all crime types).[2] The monotonicity assumption has been historically understudied. Judges may differ in their leniency depending on type of case and/or defendant they are sentencing. If unaddressed,

[2]Mueller-Smith (2015) also discusses potential violations of the exclusion restriction, which requires that the judge influences reoffending only through the choice of a prison sentence. In the case of judge IVs, the

these differences in judicial behavior could induce a biased estimator of the effects of imprisonment. Usefully, there is a simple step that can be taken to check for violations of the monotonicity assumption. The judge IV terms and observable covariate terms can interact. If results are the same with or without this set of interactions, this suggests that results are unlikely to be highly biased by violation of the monotonicity assumption. Reassuringly, although the use of these checks produces estimates that are nonidentical to those that do not attend to this problem, the practical significance might be somewhat limited. In the several papers that report results with and without adjusting for violations of the monotonicity assumption, reported results do not change in sign, order of magnitude, or significance (Frandsen et al. 2020, Mueller-Smith 2015, Stevenson 2018). This suggests that an estimator, with or without judge/covariate interaction terms, provides a substantively similar estimate of imprisonment's effect.

Pretrial Incarceration

Although imprisonment accounts for the vast majority of postconviction incarceration experiences, totaling more than 1.7 million individuals in the United States in 2018, pretrial incarceration represents an important and historically understudied aspect of the incarceration–reoffending relationship (Carson 2020a, Zeng 2020). On any given day, approximately half a million individuals are incarcerated in US jails awaiting the disposition of their criminal cases, another 250,000 individuals are held postconviction, and a total of nearly 11 million inmates are admitted to US jails in a typical year (Zeng 2020). Because of the disruption it represents in the life course (Loeffler 2018, Wakefield & Andersen 2020), pretrial incarceration is notable for its potential impact on individuals subsequently imprisoned but also on individuals subsequently sentenced to probation or released without a conviction. Yet despite having many similarities to postconviction imprisonment, including deprivation of liberty, loss of social and human capital, and loss of access to social, political, and economic roles (Goffman 1961, Jacobs 1977, Sykes 1958), pretrial detention as a form of incarceration is also notable for how it differs from postconviction imprisonment. Generally, incarcerated defendants awaiting trial have less access to programming compared to prisoners and less access to substance abuse and mental health treatment resources; they serve shorter terms of incarceration, and they experience an environment typified by higher levels of population turnover and higher levels of stress and mental illness (May et al. 2014, Schnittker et al. 2012, Sugie & Turney 2017, Turney & Conner 2019). Each of these factors separately and collectively could have different effects when compared to the experience of postconviction imprisonment.

Theories of the effect of imprisonment on postrelease reoffending are not generally differentiated by their relevance to pretrial and postconviction populations let alone to local jail-based incarceration versus state-wide correctional facilities, including traditional prisons. The classic theories of prisonization, social stigma, and deskilling, as well as the aforementioned loss of social capital, have all been offered as theoretical explanations for the poor postrelease performance of many ex-inmates in the United States. These are often contrasted with the explanations for success—deterrence, rehabilitative programming, and substance abuse and mental health treatment as well as educational and vocational training. As noted by Turney & Conner (2019), the relative applicability of these different explanations to jail incarceration is currently an open question.

---

concern is that judges may differ in their use of sentencing alternatives such as electronic monitoring or other conditions of community supervision.

One of the first IV studies on pretrial detention was by Gupta et al. (2016), who examined pretrial incarceration in Philadelphia and Pittsburgh, Pennsylvania's two largest cities. Philadelphia uses a quasi-random case assignment procedure based on bail commissioner scheduling. Bail commissioners are responsible for setting cash bail amounts, if any, for defendants awaiting trial in criminal cases. The Philadelphia sample has a base rate of recidivism of 12% per year as measured by the filing of new charges, and its IV-based estimate is that pretrial detention adds 0.7% to the probability of recidivism. The Pittsburgh sample produces a similar result. Together, the Pittsburgh and Philadelphia results show that pretrial detention leads to a 9% increase in future reoffending, an increase, however, that is only statistically significant in the pooled sample. These results are primarily driven by misdemeanor defendants rather than felony defendants.

Another paper examining the impact of pretrial detention on recidivism is also based on Philadelphia data (Dobbie et al. 2018).[3] Using a sample composed of two-thirds felony cases and one-third misdemeanor cases, they estimate the impact of pretrial detention on a range of case and life-course outcomes. Like Gupta et al. (2016), they find evidence of a long-term criminogenic effect of pretrial detention on recidivism as measured by rearrest, but their estimate is on the margin of significance. By contrast, their results for the impact of pretrial detention on postrelease employment are both negative and highly significant.

Although both of these studies hint at adverse effects of pretrial incarceration on recidivism, they both suffer from imprecise estimates. However, two other studies based on far larger samples report deleterious impacts on multiple dimensions. One examines nearly one million arraignments in New York City involving both felony and misdemeanor cases (Leslie & Pope 2017). Like the other pretrial incarceration studies, the authors exploit the rotation of arraignment judges to produce quasi-random variation in the assignment of cases to judges. They find that although the assignment appears observably random for felony cases, it is not so for misdemeanor cases. For felony cases, they find that rearrest probabilities initially drop for detained sample members by up to 12%, likely due to an incapacitation effect of pretrial detention. This result is also seen in other pretrial detention studies. However, within two years after the disposition of their case, the recidivism rates reverse and switch signs. The probability of rearrest increases by 12% for the detained individuals. Interestingly, as was noted with the postconviction studies, OLS and IV are comparable, especially for the two-year postdisposition results.

A second large sample study, conducted by Heaton et al. (2017), examines the impact of pretrial detention in a sample of nearly 400,000 misdemeanor criminal cases in Harris County, Texas. As with most other pretrial release decisions, in Harris County, bail judges are instructed to consider public safety and the likelihood of appearance at trial when deciding to either fix a financial amount of cash bail that the defendant must pay to be released or deny bail entirely. As in many jurisdictions, the nature of the instant charges and prior criminal history feature prominently in this decision process, with a lesser role for other case and defendant characteristics. The plausibly exogenous variation in this study arises from the timing of hearings within the week. Although individuals with bail hearings on Tuesdays and Wednesdays look nearly indistinguishable from individuals with bail hearings on Thursdays, they are slightly more likely to be released. Consistent with the findings in Leslie & Pope (2017), Heaton et al. (2017) find that pretrial detention initially reduces reoffending, as measured by the filing of new charges, among detained defendants due to incapacitation. However, by 30 days after the bail hearing, this pattern is reversed and there is a 10% increase in recidivism for detained misdemeanor defendants; that increase persists until the

[3]The paper includes analysis of a second sample of cases from Miami. This second sample does not include recidivism results.

end of the study's follow-up period. A parallel analysis for felony defendants reveals a similar pattern, with felony recidivism for the detained eventually exceeding released defendants' recidivism by 32% after 18 months. These larger pretrial incarceration studies strongly suggest that pretrial detention, when compared to pretrial release, exacerbates recidivism.

One final analysis examines the effect of pretrial detention using non-US data. The Di Tella & Schargrodsky (2013) analysis is based on Argentinian data and instead of evaluating the counterfactual of detention versus nondetention, this study examines detention versus EM. Once again exploiting the random assignment of cases to judges who vary considerably in their use of EM and pretrial incarceration, they report an 11–15% reduction in the probability of reincarceration within three years after assignment to EM. Although larger than the OLS estimates of a 9–10% reduction in reincarceration, these estimates are again relatively close. These results therefore reinforce the detrimental effects of pretrial detention and the relative efficacy of alternatives to detention, especially EM.

Like the postconviction imprisonment effects literature, the pretrial detention literature reports a mixture of null and deleterious effects, with one study reporting a null effect with hints of an adverse effect and four suggesting adverse effects. In our judgment, the balance of the evidence in the pretrial literature is that pretrial detention exacerbates postrelease recidivism. With the combination of disruption from temporary detention and the absence of programming or reentry resources, pretrial detention appears unfavorable compared to less restrictive pretrial monitoring alternatives. Still, there is reason for caution—the judge IV in the postconviction literature is based on a form of randomization known to create balance in all case and individual features whether measured or not—random case assignment—whereas the judge IV in the pretrial detention literature is predominantly based on judge scheduling, which does not provide a guarantee of balance; balance can be demonstrated only for measured case and individual characteristics. Thus, the cloud of hidden bias from unmeasured characteristics remains, as does the issue of imprecise estimates for studies using Philadelphia as their research location.

REGRESSION DISCONTINUITY PAPERS

Although judge IV studies are an important new addition to the literature on imprisonment effects, they represent just one of the strategies that have been employed to address selection bias in estimates of incarceration's effects on reoffending. Another method is the use of RD research designs. First developed and commonly deployed in education research (Thistlewaite & Campbell 1960), these methods have been intermittently used within criminology to estimate the causal effect of a treatment (Owens & Ludwig 2013, Rhodes & Jalbert 2013). Examples include the examination of the effects of custodial security classification levels on misconduct and reoffending (Berk & de Leeuw 1999, Chen & Shapiro 2007), the impact of trying juveniles as adults (Lee & McCrary 2009, Loeffler & Grunwald 2015), and reentry programming (Berk & Rauma 1983).[4] Up until recently, however, they had not been extensively used to examine the effects of incarceration.

All RD research designs rely on the assignment of units to treatments based on a score-based system. Examples of criminal justice–related assignment scoring systems include youth detention screening tools, prison inmate security-level classification scores, and sentencing grid offense or criminal history scores. In the context of these scoring systems, when the assignment is discontinuous and deterministic at some threshold value along the score, any corresponding changes in the outcome of interest can be causally attributed to the effects of treatment. In the case of

[4]For a general overview of this method's use in criminology, see Rhodes & Jalbert (2013).

Table 2 Regression discontinuity design studies of effect of incarceration on recidivism

Authors	Population	Counterfactual		Outcome	
		Treatment	Control	Follow-up	Estimated effect of incarceration on recidivism
Hjalmarsson (2009)	Juvenile cases, Washington State (n = 20,542)	Statewide detention	Local sanctions	Readjudication probability and timing	Preventative
Mitchell et al. (2017a)	Felony cases, Florida (n = 22,094)	Prison	Community-based sanctions	Reconviction within 3 years	Null
Franco et al. (2020)	Felony cases, Michigan (n = 23,000)	Prison	Probation	Reconviction within 5 years	Null
				Reincarceration within 5 years	Criminogenic
Rose & Shem-Tov (2020)	Felony cases, North Carolina (n = 495,824)	Prison	Probation	Rearrest or reconviction within 3 years	Preventative
				Reincarceration within 3 years	Preventative
Rhodes et al. (2018)	Federal cases, United States (n = 2,069)	30% longer prison sentence	Prison sentence	Reincarceration within 3 years	Preventative

a hypothetical sentencing grid, individuals with three or more criminal history points receive mandatory sentences of incarceration and individuals with scores of two or less receive noncustodial sentences. As this cursory description implies, there are necessary assumptions that must be met. First, the assignment variable is ideally continuous or nearly so and thereby composed of more than a few values. A scoring system with only two values (e.g., no prior criminal history and prior criminal history) does not allow for counterfactual estimation. Second, the discontinuity or discontinuities along the running variable should be deterministic to prevent affected persons or units from sorting to a favorable side of the threshold. For this reason, mandatory scoring systems will more readily lend themselves to RD studies than presumptive or voluntary scoring systems. Third, the threshold discontinuity should ideally be nonfuzzy, which simply means that everyone above the threshold should get assigned to the treatment and everyone below should not. The condition would apply to mandatory sentencing systems lacking in discretionary overrides. If these conditions are met, then cases just above and below the threshold should be balanced except for treatment. Any differences generated by that treatment are then attributable to the differences in treatment generated by the threshold, thereby approximating the benefits of exogenous variation typically associated with randomized control trials (Berk et al. 2010).

In practice, these conditions are rarely perfectly met. Assignment variables are not plentiful, sorting happens, and assignment at the threshold is partial. Still, this method offers plausibly exogenous variation. When enough of these assumptions are adequately achieved, a local average treatment effect estimate is generated (Imbens & Angrist 1994). To date, five RD studies have been used to examine the effects of imprisonment. All five exploit the structured sentencing guideline grids, composed of offense severity and criminal history points, to estimate the effects of imprisonment or imprisonment length on recidivism.

Table 2 summarizes the five studies that utilize a version of the RD research design to estimate the impact of incarceration on recidivism. Summarized study features include the population examined, the counterfactual comparison, the measure of recidivism, and the main result.

The first study to apply the RD design to a structured sentencing grid did so using the Washington State sentencing grid for juvenile offenders, which was originally created in 1977 and amended in 1998 (Hjalmarsson 2009). For juveniles adjudicated for less serious crimes or juveniles with fewer prior adjudications, local sanctions are mandated under the guidelines. However, for more serious crimes or crimes involving juveniles with more extensive prior adjudications, incarceration in a statewide detention facility was required. Across the sentencing grid, adjacent cells transition from a presumptive sentence of local sanctions to a presumptive sentence of statewide sanctions of 15–36 months. Examining this cell transition reveals large jumps in the probability of statewide incarceration ranging from 40% to 70%. After confirming minimal evidence of sorting at these transitions, Hjalmarsson (2009) finds much lower levels of recidivism (as measured by rearrest) for juveniles just above these thresholds compared to those just below. Incarcerated juveniles were 37% less likely to recidivate on any given day compared to their nonincarcerated peers even after adjusting for time served. Although Hjalmarsson (2009) attributes the reduction to deterrence, these crime reductions could also be interpreted as evidence of rehabilitation. Subsequent reports from the Washington State Institute of Public Policy document the provision of extensive psychological and educational services in the state's juvenile correction facilities (Fumia et al. 2015).[5] Whichever mechanism is at work, distinguishing between deterrence and rehabilitation when crime reductions are observed remains an ongoing practical and conceptual challenge in the literature.

Since that early study, no other analysis has used the RD design to explore juvenile incarceration, but several have taken a similar approach to examine the impact of incarceration on adult reoffending. Mitchell et al. (2017a) use data on a sample of 330,000 mostly drug and property felony offenders with limited felony convictions in Florida between 1999 and 2002. The Florida sentencing guidelines use a point system based on offense seriousness, criminal history, and other factors. It recommends that any case with 44 or more points be assigned to prison. Cases just below this threshold have a less than 20% chance of going to prison, whereas cases just above this threshold have a 40% or higher chance of going to prison. This difference is driven by the design of the sentencing grid rather than the idiosyncratic decisions of judges. Still, this design rests heavily on the comparability of cases just above and below the relevant guideline threshold. If individuals are able to self-sort or be sorted to an advantageous side of the threshold (e.g., probation), then the comparability is threatened. In practice, the authors observe good comparability on observables but some evidence of fewer cases, in absolute terms, just above the threshold. This sort of pattern can be indicative of sorting, but additional analyses suggest that this manipulation may not completely threaten the internal validity of the comparison. Unlike the results reported in Hjalmarsson (2009), they report no observable impact of imprisonment on recidivism as measured by reconviction within three years. Subsequent work examining a subset of cases composed only of drug offenders also found no evidence of an effect and no evidence of treatment-effect heterogeneity (Mitchell et al. 2017b).

Franco et al. (2020) took a similar approach in an analysis based on the Michigan sentencing guidelines. Franco and coauthors examined the effects of imprisonment for low-level offenders by taking advantage of discontinuities in the probability of imprisonment across the state's

[5]Indeed, in their recent work, Hjalmarsson & Lindquist (2020) examine the impact of a policy change in Sweden that increased the minimum stay in prison before release on parole. They find that the increase reduced both mortality and recidivism. They attribute the decrease to the more extended exposure to the extensive rehabilitative programming provided in Swedish prisons. Their finding is reminiscent of the Bhuller et al. (2019) study of Sweden's Scandinavian neighbor Norway, which has a similar orientation toward rehabilitation.

sentencing guidelines grid. They observe short-term reductions in recidivism during the first year, which they link to incapacitation. Five years after sentencing, there are no statistically significant differences in the probability of recidivism as measured by the filing of a new criminal case. However, there are persistent differences in the probability of imprisonment, with prior imprisonment increasing the chances of future imprisonment, a result that echoes results reported in their previous work (Harding et al. 2017). Beyond the substantive results, this study, combined with the previous Michigan paper (Harding et al. 2017), represents one of the rare situations in which investigators use two different quasi-experimental estimators on the same population. The similarity of the results from these two estimators suggests, at least for Michigan, that individual local average treatment-effect estimators can produce consistent results.

A final state-level RD study examines the impact of discontinuities within the North Carolina sentencing guidelines (Rose & Shem-Tov 2020). As in other RDs using sentencing guidelines, they observe large discontinuities in the probability of imprisonment as well as the length of imprisonment corresponding to thresholds built into the guidelines themselves. One such discontinuity corresponds to a 30% increase in the probability of imprisonment. They also find that the cumulative probability of reoffending is reduced by imprisonment on the order of 5% with little evidence of treatment-effect heterogeneity. Like the effects observed for juvenile offenders in Washington State (Hjalmarsson 2009), the impacts on reoffending are sizable. However, over time the size of the reoffending gap shrinks, leaving as an open question whether the nonincarcerated might eventually catch up with the reduced offending level of the incarcerated.

Although the vast majority of incarceration in the United States occurs at the state and local level, the federal government is still responsible for a considerable minority of US prisoners. As such, estimates of the effects of federal imprisonment are vital. Past studies of federal imprisonment have explored conditions of confinement (Chen & Shapiro 2007), but studies examining the effects of imprisonment itself are few (Glaser 1964, Weisburd et al. 1995). Rhodes and colleagues (Rhodes et al. 2018) used the basic structure of the federal sentencing guidelines, defined by offense seriousness and criminal history, to estimate the impact of changes in the amount of prison time served. This approach complements the more common approach of studying the in/out decision, also known as the extensive margin of incarceration. In practice, Rhodes et al. (2018) observed that probability of imprisonment is not monotonically increasing as offense levels increase. It sometimes goes up but often stays roughly the same. This leads them to compare the nth cell to the nth + 2 cell. Although this has the potential to introduce bias from the greater probability of noncomparability across a larger span of cells, this is similar to widening the comparison window near the threshold, which most RD studies do at some point as part of a sensitivity check. Rhodes et al. (2018) report evidence of a deterrent effect for prisoners serving 7.5 months longer in prison due to discontinuities in the federal sentencing guidelines. If the average sentence were raised by 7.5 months, the average recidivism level within 3 years is estimated to shrink from 20% to 19%.

Taken together, these results demonstrate that RD studies offer another tool to provide unbiased estimates of the impact of incarceration. When implemented, they also generate results that are broadly consistent with the adult judge IV literature. Two studies (Mitchell et al. 2017a, Franco et al. 2020) examining adult state sentencing systems (Florida and Michigan) find a null effect of imprisonment on recidivism. And the one other study (Rose & Shem-Tov 2020) of adult state sentencing systems (North Carolina) finds a notably large crime-reducing effect. Putting aside the nonadult and nonstate estimates, these results suggest that imprisonment may reduce recidivism but more likely has minimal impact on it. The completion of additional studies in other jurisdictions will likely reduce this remaining ambiguity or confirm that heterogeneous implementations of incarceration are likely to produce a diversity of causal estimates of incarceration's effects on recidivism. Several other features of the RD studies also bear mentioning. Like judge IV estimators, RD designs can be implemented with both great simplicity and surprising complexity. The literature is evolving in the direction of more complexity to allow for the pooling of multiple separate estimators into global estimators. This has tended to produce greater precision than otherwise would be possible with isolated estimators from single courthouses or sentencing thresholds. However, this improvement has come at the expense of transparency regarding the population treated. Reporting individual estimates alongside pooled estimates whenever possible is one solution to this emerging issue. Finally, the diversity of sentencing systems with their corresponding differences in the use of rehabilitative programming, severity of prison conditions, and quality of noncustodial community supervision suggests that a range of estimates will continue to appear as long as imprisonment remains a multitude of implementations compared to a similar multitude of alternatives.

DISCUSSION AND CONCLUSIONS

The focus of this review has been IV and RD studies that began appearing about a decade ago and were not fully examined in prior reviews linking incarceration and reoffending. IV and RD studies emerged in response to concerns that impact estimates of prior studies were contaminated by selection biases through which unaccounted factors were affecting both sentencing and recidivism and biasing estimates of the treatment effect of incarceration on reoffending. More specifically, the concern was that the estimates were biased toward showing a criminogenic effect of incarceration on reoffending because individuals who were more crime prone in unmeasured ways were more likely to receive sentences involving incarceration (Berecochea & Jaman 1981, Manski & Nagin 1998, Nagin et al. 2009, Smith & Paternoster 1990).

After reviewing over a dozen new and well-identified imprisonment effects studies, the conclusion of our IV- and RD-focused review is strikingly similar to prior reviews of the effect of postconviction incarceration, only with more confidence that measured effects are not contaminated by biases stemming from selection. Most studies we review, in fact, find that the experience of postconviction imprisonment has little impact on the probability of recidivism. A smaller number of studies do, however, find significant effects, both positive and negative. The negative, recidivism-reducing effects are most often observed in settings in which rehabilitative programming is emphasized; the positive, criminogenic effects, are generally found in settings where such programming is not emphasized. This finding of a predominantly minimal effect of postconviction imprisonment on criminal recidivism raises three interconnected questions: What have we learned from recent efforts to better identify the causal effect of incarceration? Why are the effects of imprisonment on recidivism in many places not greater given the magnitude of the intervention involved? Why do some places seem to generate much better results and other places generate much worse results?

Beyond the greater confidence in the accuracy of existing estimates due to the extensive effort in producing IV and RD studies, the similarity of our conclusion with prior reviews implies that concerns about omitted variable bias may have been overestimated. This observation is reinforced by our finding that the sign and significance of the IV and RD estimates were generally consistent with companion regression estimates using the same data that are based on regression analyses using observed case characteristics only. We note that the seeming absence of a sizable omitted variable bias is consistent with a substantial literature showing that predictions of dangerousness based on simple regression models often outperform human predictions (Ægisdóttir et al. 2006). We hasten to emphasize, however, the value and importance of these studies, as they illuminate the potential extent of these biases and facilitate more confident conclusions about the effect of incarceration on reoffending.[6]

Our conclusion concerning omitted variable bias suggests that future research does not need to use IV or RD designs or other approaches such as partial identification (Manski 1995) to account for unmeasured case features to be valid. It would be preferable, of course, if a research setting could be identified where methods designed to account for unmeasured case features could be used, but our review suggests that this is not a necessity for producing credible estimates of the causal effect of incarceration. Estimates based on designs, assuming controls for observables are sufficient to avert material biases due to selection, must, however, include a rich set of such controls. Nagin et al. (2009) recommend that at the minimum two case-characteristic variables—prior record and conviction offense type—and three demographic variables—age, race, and sex—must be included. We concur but reiterate that this is the minimum. Other data-measuring factors such as drug dependency, other problem behaviors, and personal and social stability are also highly desirable in the absence of exogenous variation.

The absence of a large positive or negative effect of imprisonment on postrelease recidivism also suggests that theoretical explanations for the effects of incarceration may best be understood as imperfectly measured explanatory competitors. This conclusion derives from the consistent shape and location of the distribution of imprisonment effects. Since the distribution is centered near zero, it is unlikely that a single large effect estimate consistent with only one theoretical paradigm is likely to be uncovered in future studies. Given the wave of well-identified and high-precision studies now available in the literature, the distribution of imprisonment effects on recidivism that we currently see is likely the distribution we will continue to see in the absence of large-scale changes in the practice of imprisonment. And yet, the existing distribution of treatment effects is not without variance. Pretrial incarceration appears more likely to generate adverse recidivism impacts than postconviction incarceration. And programmatic incarceration appears more likely to generate positive recidivism impacts than punitive incarceration.

We therefore believe that future empirical research should refocus on measuring the treatment differential through which each theoretical aspect of incarceration is hypothesized to impact recidivism and establishing to what extent a theoretically meaningful differential exists. We favor this approach because the effect of incarceration on recidivism depends on not only what goes on within the prison walls but also the treatment of former prisoners in the larger society and the alternatives to which incarceration is being compared. The most salient example of such effect heterogeneity is the differences in findings between studies of Scandinavian-based prisons, with their orientation toward rehabilitation, and studies of US-based prisons, with their orientation toward punishment. But research on prisoner reentry provides further evidence of how the impact of incarceration on recidivism also depends on the state of the economy (Sabol 2007, Yang 2017), neighborhood characteristics (Kirk 2015, Morenoff & Harding 2014), and supervision protocols (Harding et al. 2017, Petersilia 2003, Turner et al. 1986). Studies that explore these interdependencies also suggest the need for additional work on how recidivism fits into other dimensions of the reentry process, including employment, housing, and social reintegration (Harding & Harris 2020, Kirk 2020, Western 2018). More generally, we anticipate that closer attention to effect

[6]The use or not of methods designed to account for potential selection biases can also be considered from the lenses of the variance–bias trade-off. Effect estimates based on IV models have larger variance than the counterpart regression specification without an IV correction. Our conclusion that selection biases do not appear to be as large as feared suggests that the variance–bias trade-off may be tipped in favor of the non-IV estimate.

heterogeneity will yield, perhaps, less dramatic differences in effect size that are still worthy of policy attention.

Even with the insufficiency of evidence on underlying mechanisms, the findings of our review have two important policy implications. First, the findings of studies of pretrial incarceration are consistent—most find a deleterious effect on postrelease reoffending. This finding adds to the growing literature on the disruptions that pretrial detentions have on the lives of those detained even as many are not convicted or are convicted of minor offenses (Heaton et al. 2017, Kohler-Hausmann 2013, Natapoff 2015, Uggen et al. 2014). By implication, it supports policies intended to make more parsimonious use of pretrial detention. Second, we find that most US-based studies find no evidence of a postconviction-specific deterrent effect of imprisonment. This finding suggests that on the margin, more sparing use of imprisonment as the sanction imposed on the convicted will not increase crime. We emphasize "on the margin" because the IV and RD effect estimates are based on individuals who have been convicted of less serious crimes and have less extensive prior records. Returning to the theme of effect heterogeneity, we note that impact estimates for this marginal group may not apply to other groups of prisoners.

Additional research is needed on the effects of incarceration for subpopulations that are almost always subject to incarceration. These often include individuals sentenced for violent and other serious offenses even for individuals who do not have extensive criminal histories.

DISCLOSURE STATEMENT

The authors are not aware of any affiliations, memberships, funding, or financial holdings that might be perceived as affecting the objectivity of this review.

ACKNOWLEDGMENTS

We thank Frank Cullen, John Laub, and Bruce Western for their helpful comments. All errors remain our own.

LITERATURE CITED

Ægisdóttir S, White MJ, Spengler PM, Maugherman AS, Anderson LA, et al. 2006. The meta-analysis of clinical judgment project: fifty-six years of accumulated research on clinical versus statistical prediction. *Couns. Psychol.* 34(3):341–82

Aizer A, Doyle J. 2015. Juvenile incarceration, human capital, and future crime: evidence from randomly assigned judges. *Q. J. Econ.* 130(2):759–804

Andersen SN, Hyatt JM, Telle K. 2020. Exploring the unintended consequences of implementing electronic monitoring on sentencing in Norway. *Nordic J. Criminol.* 21(2):129–51

Andersen SN, Skardhamar T. 2017. Pick a number: mapping recidivism measures and their consequences. *Crime Delinquency* 63(5):613–35

Angrist J, Imbens G, Rubin D. 1996. Identification of causal effects using instrumental variables. *J. Am. Stat. Assoc.* 91:444–55

Beccaria C. 1764 (1819). *An Essay on Crimes and Punishments*. Philadelphia: Philip H. Nicklin

Becker GS. 1968. Crime and punishment: an economic approach. *J. Political Econ.* 76(2):169–217

Berecochea J, Jaman D. 1981. *Time served in prison and parole outcome: an experimental study—report number 2*. Rep. NCJ 82800, Calif. Dep. Correct., Sacramento. **https://www.ojp.gov/pdffiles1/Digitization/82800NCJRS.pdf**

Berk R, Barnes G, Ahlman L, Kurtz E. 2010. When second best is good enough: a comparison between a true experiment and a regression discontinuity quasi-experiment. *J. Exp. Criminol.* 6(2):191–208

Berk R, de Leeuw J. 1999. An evaluation of California's inmate classification system using a generalized regression discontinuity design. *J. Am. Stat. Assoc.* 94(448):1045–52

Berk R, Rauma D. 1983. Capitalizing on nonrandom assignment to treatments: a regression-discontinuity evaluation of a crime-control program. *J. Am. Stat. Assoc.* 78(381):21–27

Bhuller M, Dahl GB, Løken KV, Mogstad M. 2019. Incarceration, recidivism, and employment. *J. Political Econ.* 128(4):1269–324

Blumstein A, Cohen J. 1973. A theory of the stability of punishment. *J. Crim. Law Criminol.* 64(2):198–207

Carson EA. 2020a. *Prisoners in 2018.* Bur. Justice Stat. Rep. NCJ 253516, US Dep. Justice, Washington, DC. **https://bjs.ojp.gov/content/pub/pdf/p18.pdf**

Carson EA. 2020b. *Prisoners in 2019.* Bur. Justice Stat. Rep. NCJ 255115, US Dep. Justice, Washington, DC. **https://bjs.ojp.gov/content/pub/pdf/p19.pdf**

Chen MK, Shapiro JM. 2007. Do harsher prison conditions reduce recidivism? A discontinuity-based approach. *Am. Law Econ. Rev.* 9(1):1–29

Di Tella R, Schargrodsky E. 2013. Criminal recidivism after prison and electronic monitoring. *J. Political Econ.* 121(1):28–73

Dobbie W, Goldin J, Yang CS. 2018. The effects of pretrial detention on conviction, future crime, and employment: evidence from randomly assigned judges. *Am. Econ. Rev.* 108(2):201–40

Franco C, Harding DJ, Bushway SD, Morenoff JD. 2020. *Failing to follow the rules: Can imprisonment lead to more imprisonment without more actual crime?* Work. Pap., Univ. Calif., Berkeley, CA

Frandsen BR, Lefgren LJ, Leslie E. 2020. *Judging judge fixed effects*. Work. Pap., Brigham Young Univ., Provo, UT

Fumia D, Drake E, He L. 2015. *Washington's coordination of services program for juvenile offenders: outcome evaluation and benefit-cost analysis*. Rep., Wash. State Inst. Public Policy, Olympia, WA

Glaser D. 1964. *The Effectiveness of a Prison and Parole System*. Indianapolis, IN: Bobbs-Merrill

Goffman E. 1961. *Asylums: Essays on the Social Situation of Mental Patients and Other Inmates.* Garden City, NY: Anchor Books. 1st ed.

Green DP, Winik D. 2010. Using random judge assignments to estimate the effects of incarceration and probation on recidivism among drug offenders. *Criminology* 48(2):357–87

Gupta A, Hansman C, Frenchman E. 2016. The heavy costs of high bail: evidence from judge randomization. *J. Leg. Stud.* 45(2):471–505

Harding DJ, Harris HM. 2020. *After Prison: Navigating Adulthood in the Shadow of the Justice System*. New York: Russell Sage Found.

Harding DJ, Morenoff JD, Nguyen AP, Bushway SD. 2017. Short- and long-term effects of imprisonment on future felony convictions and prison admissions. *PNAS* 114(42):11103–8

Heaton P, Mayson S, Stevenson M. 2017. The downstream consequences of misdemeanor pretrial detention. *Stanf. Law Rev.* 69:711–94

Hjalmarsson R. 2009. Juvenile jails: a path to the straight and narrow or to hardened criminality? *J. Law Econ.* 52(4):779–809

Hjalmarsson R, Lindquist MJ. 2020. *The health effects of prison*. Work. Pap., Univ. Gothenbg., Gothenburg, Swed.

Imbens G, Angrist J. 1994. Identification and estimation of local average treatment effects. *Econometrica* 62(2):467–75

Jacobs JB. 1977. *Stateville: The Penitentiary in Mass Society*. Chicago: Univ. Chicago Press

Kirk DS. 2015. A natural experiment of the consequences of concentrating former prisoners in the same neighborhoods. *PNAS* 112(22):6943–48

Kirk DS. 2020. *Home Free: Prisoner Reentry and Residential Change After Hurricane Katrina*. New York: Oxford Univ. Press

Kohler-Hausmann I. 2013. Misdemeanor justice: control without conviction. *Am. J. Sociol.* 119(2):351–93

Lee DS, McCrary J. 2009. *The deterrence effect of prison: dynamic theory and evidence*. Work. Pap., Univ. Calif., Berkeley, CA

Leslie E, Pope NG. 2017. The unintended impact of pretrial detention on case outcomes: evidence from New York City arraignments. *J. Law Econ.* 60(3):529–57

Loeffler CE. 2013. Does imprisonment alter the life course? Evidence on crime and employment from a natural experiment. *Criminology* 51(1):137–66

Loeffler CE. 2018. Pre-imprisonment employment drops: another instance of the Ashenfelter Dip. *J. Crim. Law Criminol.* 108(4):815–38

Loeffler CE, Grunwald B. 2015. Processed as an adult: a regression discontinuity estimate of the crime effects of charging nontransfer juveniles as adults. *J. Res. Crime Delinquency* 52(6):890–922

Maltz MD. 1984. *Recidivism*. Orlando, FL: Academic

Manski CF. 1995. *Identification Problems in the Social Sciences*. Cambridge, MA: Harv. Univ. Press

Manski CF, Nagin DS. 1998. Bounding disagreements about treatment effects: a case study of sentencing and recidivism. *Sociol. Methodol.* 28:99–137

Maruschak LM, Minton TD. 2020. *Correctional populations in the United States, 2017–2018*. Bur. Justice Stat. Rep. NCJ 252157, US Dep. Justice, Washington, DC. **https://bjs.ojp.gov/content/pub/pdf/cpus1718.pdf**

May DC, Applegate BK, Ruddell R, Wood PB. 2014. Going to jail sucks (and it really doesn't matter who you ask). *Am. J. Crim. Justice* 39:250–66

Menefee MR, Harding DJ, Nguyen AP, Morenoff JD, Bushway SD. 2020. *The effect of split sentences on employment and future criminal justice involvement: evidence from a natural experiment*. Work. Pap., Univ. Calif., Berkeley, CA

Mitchell O, Cochran JC, Mears DP, Bales WD. 2017a. Examining prison effects on recidivism: a regression discontinuity approach. *Justice Q.* 34(4):571–96

Mitchell O, Cochran JC, Mears DP, Bales WD. 2017b. The effectiveness of prison for reducing drug offender recidivism: a regression discontinuity analysis. *J. Exp. Criminol.* 13:1–27

Morenoff JD, Harding DJ. 2014. Incarceration, prisoner reentry, and communities. *Annu. Rev. Sociol.* 40:411–29

Mueller-Smith M. 2015. *The criminal and labor market impacts of incarceration*. Work. Pap., Univ. Mich., Ann Arbor, MI

Nagin DS, Cullen FT, Jonson CL. 2009. Imprisonment and reoffending. *Crime Justice* 38(1):115–200

Nagin DS, Snodgrass GM. 2013. The effect of incarceration on re-offending: evidence from a natural experiment in Pennsylvania. *J. Quant. Criminol.* 29(4):601–42

Natapoff A. 2015. Misdemeanors. *Annu. Rev. Law Soc. Sci.* 11:255–67

Natl. Res. Counc. 2014. *The Growth of Incarceration in the United States: Exploring Causes and Consequences*. Washington, DC: Natl. Acad. Press

Owens EG, Ludwig J. 2013. Using regression discontinuity designs in crime research. In *Experimental Criminology: Prospects for Advancing Science and Public Policy*, ed. BC Welsh, AA Braga, GJN Bruinsma, pp. 194–222. New York: Cambridge Univ. Press

Petersilia J. 2003. *When Prisoners Come Home: Parole and Prisoner Reentry*. Oxford, UK: Oxford Univ. Press

Puzzanchera C. 2018. *Juvenile arrests, 2016*. Off. Juv. Justice Delinquency Prev. Rep., US Dep. Justice, Washington, DC. **https://ojjdp.ojp.gov/sites/g/files/xyckuh176/files/pubs/251861.pdf**

Roach M, Schanzenbach M. 2015. *The effect of prison sentence length on recidivism: evidence from random judicial assignment.* SSRN Res. Pap. 2701549. **http://dx.doi.org/10.2139/ssrn.2701549**

Rhodes W, Gaes GG, Kling R, Cutler C. 2018. Relationship between prison length of stay and recidivism: a study using regression discontinuity and instrumental variables with multiple break points. *Criminol. Public Policy* 17(3):731–69

Rhodes W, Jalbert SK. 2013. Regression discontinuity design in criminal justice evaluation: an introduction and illustration. *Eval. Rev.* 37(3–4):239–73

Roodman D. 2017. *The impacts of incarceration on crime*. Rep., Open Philanthr., San Francisco, CA

Rose EK, Shem-Tov Y. 2020. *How does incarceration affect crime? Estimating the dose-response function*. Work. Pap., Univ. Calif., Berkeley, CA

Sabol W. 2007. Local labor-market conditions and post-prison employment experiences of offenders released from Ohio state prison. In *Barriers to Reentry? The Labor Market for Released Prisoners in Post-Industrial America*, ed. S Bushway, MA Stoll, DF Weiman, pp. 257–303. New York: Russell Sage Found.

Schnittker J, Massoglia M, Uggen C. 2012. Out and down incarceration and psychiatric disorders. *J. Health Soc. Behav.* 53(4):448–64

Smith DA, Paternoster R. 1990. Formal processing and future delinquency: deviance amplification as selection artifact. *Law Soc. Rev.* 24(5):1109–31

Stevenson MT. 2018. Distortion of justice: how the inability to pay bail affects case outcomes. *J. Law Econ. Organ.* 34(4):511–42

Sugie NF, Turney K. 2017. Beyond incarceration: criminal justice contact and mental health. *Am. Sociol. Rev.* 82(4):719–43

Sykes GM. 1958. *The Society of Captives*. Princeton, NJ: Princeton Univ. Press

Thistlewaite DL, Campbell DT. 1960. Regression-discontinuity analysis: an alternative to the ex post facto experiment. *J. Educ. Psychol.* 51(6):309–17

Turner S, Peterson JE, Petersilia J. 1986. *Prison versus probation in California: implications for crime and offender recidivism*. Rep., RAND Corp., Santa Monica, CA

Turney K, Conner E. 2019. Jail incarceration: a common and consequential form of criminal justice contact. *Annu. Rev. Criminol.* 2:265–90

Uggen C, Vuolo M, Lageson S, Ruhland E, Whitham HK. 2014. The edge of stigma: an experimental audit of the effects of low-level criminal records on employment. *Criminology* 52(4):627–54

Villettaz P, Gillieron G, Killias M. 2015. The effects on re-offending of custodial vs. non-custodial sanctions: an updated systematic review of the state of knowledge. *Campbell Syst. Rev.* 11(1):1–92

Villettaz P, Killias M, Zoder I. 2006. The effects of custodial vs. non-custodial sentences on re-offending: a systematic review of the state of knowledge. *Campbell Syst. Rev.* 2(1):1–69

Wakefield S, Andersen LH. 2020. Pretrial detention and the costs of system overreach for employment and family life. *Sociol. Sci.* 7:342–66

Walker SC, Herting JR. 2020. The impact of pretrial juvenile detention on 12-month recidivism: a matched comparison study. *Crime Delinquency* 66(13–14):1865–87

Weisburd D, Waring E, Chayet E. 1995. Specific deterrence in a sample of offenders convicted of white-collar crimes. *Criminology* 33:587–607

Western B. 2018. *Homeward: Life in the Year After Prison*. New York: Russell Sage Found.

Williams J, Weatherburn D. 2020. Can electronic monitoring reduce reoffending? *Rev. Econ. Stat.* In press

Yang CS. 2017. Local labor markets and criminal recidivism. *J. Public Econ.* 147:16–29

Zeng Z. 2020. *Jail inmates in 2018*. Bur. Justice Stat. Rep. NCJ 253044, US Dep. Justice, Washington, DC. **https://bjs.ojp.gov/content/pub/pdf/ji18.pdf**

Zeng Z, Minton TD. 2021. *Jail inmates in 2019*. Bur. Justice Stat. Rep. NCJ 255608, US Dep. Justice, Washington, DC. **https://bjs.ojp.gov/content/pub/pdf/ji19.pdf**

Annual Review of Criminology
Volume 5, 2022

Contents

A Focus on Youth

Errata

An online log of corrections to *Annual Review of Criminology* articles may be found at http://www.annualreviews.org/errata/criminol