Short- and long-term effects of imprisonment on future felony convictions and prison admissions

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A substantial contributor to prison admissions is the return of individuals recently released from prison, which has come to be known as prison's "revolving door." However, it is unclear whether being sentenced to prison itself has a causal effect on the probability of a subsequent return to prison or on criminal behavior. To examine the causal effect of being sentenced to prison on subsequent offending and reimprisonment, we leverage a natural experiment using the random assignment of judges with different propensities for sentencing offenders to prison. Drawing on data on all individuals sentenced for a felony in Michigan between 2003 and 2006, we compare individuals sentenced to prison to those sentenced to probation, taking into account sentence lengths and stratifying our analysis by race. Results show that being sentenced to prison rather than probation increases the probability of imprisonment in the first 3 years after release from prison by 18 percentage points among nonwhites and 19 percentage points among whites. Further results show that such effects are driven primarily by imprisonment for technical violations of community supervision rather than new felony convictions. This suggests that more stringent postprison parole supervision (relative to probation supervision) increases imprisonment through the detection and punishment of low-level offending or violation behavior. Such behavior would not otherwise result in imprisonment for someone who had not already been to prison or who was not on parole. These results demonstrate that the revolving door of prison is in part an effect of the nature of postprison supervision.

incarceration | recidivism | crime | parole | probation

Between the 1970s and the late 2000s, the United States experienced an enormous rise in incarceration (1, 2). A substantial contributor to prison admissions is the return to prison of individuals recently released from prison (3, 4), which has come to be known as prison's "revolving door" (5). Such prison returns are due to a mix of new crimes and technical violations of the conditions of postprison community supervision (commonly called parole). However, it is unclear whether being sentenced to prison itself has a causal effect on the probability of a entering prison at a later time, and if so, the degree to which such effects are a result of increased offending or of postprison community supervision. Prior studies on the relationship between incarceration and subsequent offending have found either no association or that going to jail or prison is associated with a higher likelihood of subsequent offending, but whether such associations are causal is unclear (6, 7). Individuals who are sentenced to prison may be systematically different from others in ways that also affect future offending but which are unobserved by researchers. Moreover, the challenges of estimating causal effects of imprisonment mean that, although much theorized, the processes by which imprisonment affects subsequent offending and reimprisonment—either positively or negatively—also remain unclear empirically (6, 7).

Our examination of the effects of being sentenced to prison on future offending and future imprisonment is informed by existing theorizing on the consequences of incarceration. Some theories suggest that prison is crime-suppressive, while others suggest it is

criminogenic. It has been hypothesized that prison reduces crime through incapacitation, rehabilitation, and specific deterrence (6–8). The magnitude of any incapacitation effect depends on the offending of a comparison group of individuals who have not been imprisoned, and incapacitation effects occur only while the individual remains incarcerated. In contrast, rehabilitation and specific deterrence will exert their effects after release. It has also been hypothesized that prison increases criminal offending through stigmatization and labeling effects, through social learning of procriminal attitudes, values, skills, and roles (prisons as "schools of crime"), and through prison's effects on employment prospects (1, 7, 9, 10).

Being sentenced to prison also carries important postrelease consequences that may influence the likelihood of both committing offenses and being imprisoned again. Over 80% of prisoners are released onto parole or some other form of community supervision (11), and postprison community supervision is typically more intensive than probation supervision (12). Such supervision is intended to serve a monitoring function and facilitate access to social services after release, which could reduce a person's opportunity and proclivity to offend, but it also may increase the detection and sanctioning of minor crimes and violation behavior, thus increasing the chances of reimprisonment (12–15). Technical violations of parole account for almost 30% of all prison admissions nationwide (3). Due to the stigma of prior imprisonment, the risk of being sentenced to prison for a new crime may also be greater if the individual was on community supervision at the time of the offense or has been to prison before.

To examine the causal effect of being sentenced to prison on subsequent offending and reimprisonment, we leverage a natural experiment using the random assignment of judges with different

propensities for sentencing offenders to prison. This strategy isolates only the variation in sentencing that is related to the randomly assigned judge, using that variation to produce estimates of the effects of a prison sentence that are uncontaminated by unobserved differences between prisoners and the comparison group, those sentenced to probation. Although some prior studies have used a similar identification strategy (16–19), this study is distinctive in that it examines the effects of imprisonment on future prison admissions in conjunction with effects on new felony convictions. It also directly addresses incapacitation effects and distinguishes between prison admissions for new crimes and prison admissions for technical violations of probation or parole. These features allow us to shed light on the mechanics of prison's revolving door.

Drawing on data on all individuals sentenced for a felony in Michigan between 2003 and 2006, we compare individuals sentenced to prison to those sentenced to probation, taking into account sentence lengths. Because rates of incarceration, returns to prison, and recidivism differ by race, we stratify all analyses by race, analyses made possible by our multiyear, statewide data (1). (Although 97% of those who are nonwhite are in fact African-American in our sample, we use the terms nonwhite and white to be precise about the groups we are analyzing.) Our analysis measures outcomes at 1, 3, and 5 y after sentencing and after prisoners are released from prison (*Materials and Methods*). The "after-release" results remove any incapacitation effect of imprisonment, allowing us to examine effects of imprisonment after a prisoner has returned to the community.

Results

Fig. 1 shows the probability of ever having been admitted to prison over time since release by race for three related measures of future imprisonment: admission for any reason, admission for a technical violation, and admission for a new sentence (arising from conviction for a new felony). It compares outcomes among those sentenced to prison with our comparison group, those sentenced to probation, showing much higher rates of all three imprisonment outcomes among prisoners compared with probationers for both racial groups at all time periods. Imprisonment for technical violations among prisoners is concentrated in the first 2 y after release, whereas imprisonment for new sentences increases gradually over the 5-y period for both prisoners and probationers. In addition, comparing the rates of prison admissions for technical violations to admissions for new sentences shows that technical violations are a larger component of prison admissions for former prisoners than for probationers at almost all time points. Among probationers, prison admissions for new sentences are more common than prison admissions for technical violations.
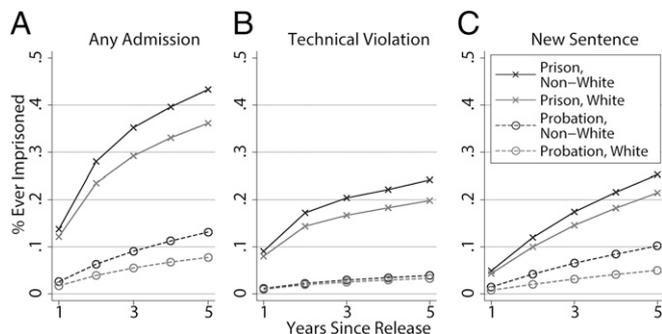

Fig. 1. Future prison admission for any reason (*A*), for technical violations (*B*), and for a new sentence (*C*) by prison vs. probation sentence, time since release, and race.

We leverage the natural experiment based on the random assignment of judges to examine the degree to which such prisoner–probationer differences in recidivism are causal effects of a prison sentence rather than preexisting differences between those sentenced to prison vs. probation. Table 1 shows instrumental-variables (IV) estimates of the effects of prison compared with probation on future admissions to prison and new felony convictions, measured at 1, 3, and 5 y after release (comparable ordinary least-squares estimates are provided in *SI Appendix*, Table S5; full model results are provided in *SI Appendix*, Table S6 *A–D*). Being sentenced to prison rather than probation increases the probability of future imprisonment quite dramatically for both racial groups at all follow-up times. In the first year since release, the effect is 8 percentage points among nonwhites and 10 percentage points among whites. These effects grow considerably at 3 y since release, to 18 percentage points among nonwhites and 19 percentage points among whites, and eventually reach 20 percentage points for both racial groups at 5 y since release.

Table 1 also shows that these effects are driven primarily by imprisonment for technical violations of community supervision rather than sentences for new felony crimes. (At longer follow-up times, it is possible for an individual to experience both types of prison admissions, which explains why the any admission effect is not simply the sum of the effects for the two admission types.) Whereas the effect of a prior prison sentence on being sentenced to prison in the future for a technical violation is significant at all time periods for both racial groups, reaching 18 percentage points for nonwhites and 14 percentage points for whites after 5 y, the effect on future imprisonment for a new felony is smaller (and not always significant), 4 percentage points for nonwhites and 9 percentage points for whites after 5 y. None of the differences by race in the estimates in Table 1 is statistically significant. Collectively, these results present strong evidence that prison's revolving door is a causal effect of imprisonment rather than an artifact of preexisting differences between prisoners and probationers and that imprisonment for technical violations is a key mechanism driving this effect.

One explanation for why technical parole violations are the main pathway through which people sentenced to prison are readmitted after their release is that parole supervision is more intensive than probation supervision and it increases the detection and punishment of low-level offending or violation behavior. An alternative explanation is that some prosecutors may decide not to charge individuals on parole for low-level felony crimes because such individuals can be reimprisoned more easily for a technical violation. If we can assume that prosecutors would almost always prosecute medium- or high-severity felonies but that they might not prosecute low-level felonies when the suspect is already being sent back to prison for a technical violation, we can test this hypothesis by analyzing the effects of imprisonment on new felony convictions by the severity of the new crime. Under this hypothesis, we would expect prison sentences to have negative effects on less severe felonies (if prosecutors refrain from charging low-level felonies when the person could be returned to prison for a technical violation) and null or positive prison effects on more severe felonies.

We created categories of felony severity based on the maximum prison sentence for the specific crime committed: low-severity felonies are those with maximum sentences of 0–48 mo, medium are 49–72 mo, and high severity are 73 or more months. (Because the maximum sentence is set by law by the Michigan state legislature, this measure of felony severity reflects the assessment of the state legislature. We have explored whether results vary with the definitions of the severity categories. Results with categories defined at 0–36, 37–60, and 61+ are very similar.) We examine convictions for any felony, medium- or high-severity felonies, or high-severity felonies only. (The medium–high-severity felony variable is coded 1 for medium- or high-severity felonies and zero

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Table 1. 2SLS estimates of effects of prison vs. probation sentence on future prison admissions and new felony convictions by time since release and race

Outcome	Nonwhites			Whites		
	1 y	3 y	5 y	1 y	3 y	5 y
Future imprisonment						
Any prison admission	0.080***	0.176***	0.195***	0.099***	0.188***	0.197***
	(0.012)	(0.018)	(0.020)	(0.013)	(0.018)	(0.020)
Prison admission for a technical violation	0.074***	0.166***	0.184***	0.070***	0.127***	0.143***
	(0.010)	(0.014)	(0.016)	(0.010)	(0.014)	(0.016)
Prison admission for a new sentence	0.007	0.026	0.043*	0.028***	0.077***	0.092***
	(0.008)	(0.014)	(0.018)	(0.008)	(0.014)	(0.017)
New felony convictions						
Any new felony	−0.020	−0.028	−0.027	0.009	0.036	0.034
	(0.013)	(0.021)	(0.024)	(0.013)	(0.020)	(0.024)
Medium- or high-severity felony	−0.017	−0.037*	−0.048*	−0.004	0.017	0.015
	(0.011)	(0.017)	(0.021)	(0.010)	(0.017)	(0.021)
High-severity felony	−0.009	−0.026*	−0.036*	−0.003	0.014	0.014
	(0.008)	(0.013)	(0.016)	(0.007)	(0.012)	(0.015)

Notes: Effect of prison vs. probation at 24-mo sentences; robust SEs in parentheses; 2SLS, two-stage least squares; ***$P < 0.001$, **$P < 0.01$, *$P < 0.05$; full model results in *SI Appendix*, Table S6 A–D.

for low-severity felonies or those that were not convicted of any felony. The high-severity felony variable is coded 1 for high-severity felonies and coded zero for medium- and low-severity felonies and those with no felony convictions.) Fig. 2 shows descriptive statistics on new felony outcomes by race and time since release. The differences between prisoners and probationers on felony convictions are smaller than the differences for imprisonment but still sizeable for both racial groups.

Results of our IV analysis of prison sentences' effects on future felony convictions appear in the bottom panel of Table 1. Being sentenced to prison rather than probation had no effect on conviction for any new felony (the general measure that does not specify severity) for either racial group at any of the time periods after release. Our severity-specific felony conviction analysis shows that prison reduces the probability of having a medium–high- or high-severity felony, but only for nonwhites and at 3 or 5 y after release from prison. Because there is a slight negative effect on medium–high-severity felonies and a smaller negative effect on felonies of any severity (which is not statistically different from zero) at both 3 and 5 y, we can also infer by differencing these two estimates that there is a slight positive effect of prison on low-severity felonies after release. In sum, the results of our postrelease felony conviction analysis lead us to conclude that prison has no effect on serious criminal offending among whites and small effects among nonwhites, decreasing slightly both medium- and high-severity felonies (while increasing slightly low-severity felonies).

Considering together the results of our analysis of both postrelease prison admissions and felony convictions, we found that the strongest relationship between prison sentences and recidivism is their positive effects on future prison admissions due to technical violations. Moreover, since we see null and negative effects of prison on medium- and high-severity felonies and a slight positive effect of prison on low-severity felonies, our findings do not support the hypothesis that the positive effects on technical violations are a by-product of decisions not to prosecute low-level felonies among former prisoners on parole. Finally, it is also striking in Table 1 that being sentenced to prison increases one's probability of a prison admission for a new felony while simultaneously decreasing or having no effect on the probability of a conviction for a new felony. We suspect this reflects the role of previous imprisonment in sentencing decisions, an additional aspect of prison's revolving door.

Thus far, we have presented results for outcomes measured postrelease. Such outcomes remove any effect of a prison sentence

on future crime and imprisonment due to incapacitation during the initial prison sentence, one of the primary ways prison is theorized to affect recidivism. To examine incapacitation effects, we consider outcomes measured since the sentence date, thus building in any incapacitation effect of imprisonment. To gauge the degree to which postsentence outcome measures are the product of incapacitation, we must consider the time spent in prison among those sentenced to prison. The median time in prison among prisoners in this sample is 28 mo for both nonwhites and whites (the mean is 38 mo for both groups). At 1 y after sentence, 86% of nonwhite with a prison sentence and 87% of whites are still incarcerated on the initial prison sentence. At 3 y after sentence, these figures are 48.9% (nonwhites) and 45.6% (whites). At 5 y, these figures are 32.5% (nonwhites) and 27.5% (whites).

Table 2 shows IV effect estimates by race and time since the original sentence (for full model results, see *SI Appendix*, Tables S7 A–D; descriptive statistics on outcomes are in *SI Appendix*, Table S1 and Figs. S2 and S3). This analysis counts time that people sentenced to prison spent incarcerated, so it includes incapacitation effects, especially in the earlier time periods, before most individuals were released from prison. The estimated effects of imprisonment reported in Table 2 are more negative compared with the results in Table 1. In the first year after sentence, there are no effects among whites and only small effects among non-whites, for whom a prison sentence reduces the probability of a
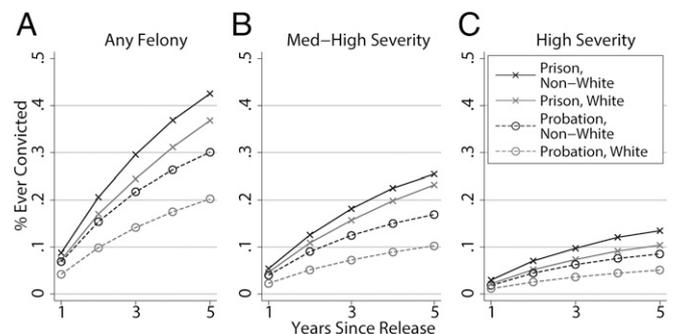

Fig. 2. Future felony conviction for any felony (A), for medium or high severity felony (B), and for high severity felony only (C) by prison vs. probation sentence, time since release, and race.

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Table 2. 2SLS estimates of effects of prison vs. probation sentence on future prison admissions and new felony convictions by time since sentence and race

Outcome	Nonwhites			Whites		
	1 y	3 y	5 y	1 y	3 y	5 y
Future imprisonment						
Any prison admission	−0.015*	0.021	0.098***	−0.006	0.068***	0.140***
	(0.008)	(0.016)	(0.018)	(0.008)	(0.015)	(0.018)
Prison admission for a technical violation	0.005	0.063***	0.125***	−0.000	0.065***	0.117***
	(0.006)	(0.011)	(0.013)	(0.006)	(0.012)	(0.014)
Prison admission for a new sentence	−0.019***	−0.037**	−0.012	−0.007	0.004	0.037**
	(0.005)	(0.012)	(0.015)	(0.005)	(0.011)	(0.014)
New felony convictions						
Any new felony	−0.078***	−0.190***	−0.146***	−0.052***	−0.104***	−0.088***
	(0.010)	(0.020)	(0.021)	(0.010)	(0.017)	(0.020)
Medium- or high-severity felony	−0.049***	−0.124***	−0.106***	−0.037***	−0.063***	−0.054**
	(0.008)	(0.016)	(0.018)	(0.007)	(0.015)	(0.017)
High-severity felony	−0.018**	−0.055***	−0.051***	−0.017**	−0.023*	−0.017
	(0.006)	(0.011)	(0.014)	(0.005)	(0.010)	(0.012)

Notes: Effect of prison vs. probation at 24-mo sentences; robust SEs in parentheses; 2SLS, two-stage least squares; ***$P < 0.001$, **$P < 0.01$, *$P < 0.05$; full model results in *SI Appendix*, Table S7 A–D.

prison admission for a new sentence by about 2 percentage points. These estimates reflect the incapacitation of prisoners and the low rate of prison admission among probationers (3%). Three years after sentence, the effect of a prison sentence on future admission to prison is not statistically different from zero among nonwhites, but this reflects counterbalancing positive effects on new admissions for technical violations (an approximately 6-percentage point increase) and negative effects on admissions for new sentences (4-percentage point decrease). Among whites, there is an approximately 7-point effect on prison admissions, which is entirely due to an effect on admissions for technical violations. At 5 y after the sentence, any incapacitation effects of the initial prison sentence have been swamped by greater prison admissions among those initially sentenced to prison. Both nonwhites and whites show positive effects of prison vs. probation on future prison admissions (10 percentage points among nonwhites and 14 percentage points among whites). These effects appear to be entirely due to admissions for a technical violation among nonwhites, and primarily due to technical violations among whites, with a 12-percentage point effect on admissions for technical violations and 4-percentage point effect on admissions for new sentences. Only the 3- and 5-y estimates for prison admissions for new sentences are statistically different by race. These results provide further evidence of the power of prison's revolving door: sentencing an individual to prison rather than probation increases his or her probability of entering prison again at some point within 5 y of the sentence by 10–14 percentage points, swamping the short-term negative effects of incapacitation.

The bottom panel of Table 2 shows after-sentence results for new felony convictions. Effect estimates for 1 y after sentence reflect entirely the incapacitation effect of imprisonment because the probability of a prisoner from either racial group being convicted of a new felony in the first year after sentence is effectively zero (*SI Appendix*, Table S1). In other words, these effects are driven by the new felony behavior of probationers. These estimates show an 8-percentage point reduction in the probability of being convicted of any new felony among nonwhites and a 5-percentage point reduction among whites. This includes a 5-percentage point reduction in medium- and high-severity felonies and 2-percentage point reduction in high-severity felonies among nonwhites, with 4- and 2-percentage point reductions, respectively, among whites. The prevention of new felonies from a prison sentence is largest at 3 y at 19 percentage points among nonwhites (12 for medium–high and 6 for high severity) and 10 percentage

points among whites (6 for medium–high and 2 for high severity). By 5 y after sentence, the preventative effect of prison falls again, to 15 percentage points for nonwhites (11 for medium–high and 5 for high severity) and 9 percentage points for whites (5 for medium–high and zero for high severity). In sum, our comparison of convictions for new felonies between prisoners and probationers in the years after their sentences leads us to conclude that there is an important incapacitation effect of prison on felony offending, but such effects are modest in size. It is certainly not the case that each individual imprisoned would have committed a felony had he or she remained in the community.

Discussion

The increase in incarceration in the late 20th and early 21st centuries in the United States raises important questions about both the causes of the rise and its consequences for the prevention or generation of serious criminal behavior. This study has leveraged a natural experiment using the random assignment of judges to criminal cases in Michigan from 2003 to 2006, with individuals followed through September 2013. Because judges vary in their penchant for sentencing offenders to prison rather than probation, our research design allows us to isolate variation in prison vs. probation sentences that is independent of offender characteristics, both observed and unobserved, and thereby generate causal estimates of the effect of being sentenced to prison on future prison admissions and felony convictions.

With regard to the rise in incarceration, our results demonstrate that imprisonment leads to future imprisonment. In other words, prison's figurative revolving door has real causal force, rather than being the simple consequence of imprisonment of individuals at higher risk for future offending. For example, being sentenced to prison rather than probation increases the probability of a future prison admission within 3 y after release by 18–19 percentage points. These results imply that the rise in incarceration was to some degree self-generating, as imprisonment creates more imprisonment. Although the estimated effect sizes are not different by race, substantially higher rates of imprisonment among nonwhites mean that the consequences of these effects in the aggregate are much more severe for racial minorities, particularly African-Americans, who are imprisoned at over five times the rate as whites nationwide (20).

Our results also demonstrate that the majority of this reimprisonment effect is generated by parole violations rather than prison admissions for new felony convictions. Postprison parole

supervision surveilles and punishes, and in so doing increases incarceration. In other words, the rise in incarceration was in part a self-perpetuating process resulting from the workings of the criminal justice system itself. The most common reasons for parole violations in Michigan include failure to report (included in 73% of violation reports), failure to complete required programming (61%), substance abuse (50%), moving residence without permission including absconding (50%), curfew violations or entering restricted areas (49%), associating with felons (29%), and possession of weapons other than firearms (29%). However, those most associated with revocation to prison are moving residences/absconding, nonfirearms weapons possession, failure to maintain registration as a sex offender, substance abuse, and driving without permission (21).

With regard to the crime preventive or criminogenic effects of imprisonment, our results demonstrate that there is a moderate incapacitation effect of imprisonment while an offender is in prison. For instance, a prison sentence reduces the probability of being convicted of a new felony by 5–8 percentage points in the first year after sentence. The reduction for medium–high-severity felonies is 4–5 percentage points and for high-severity felonies, the reduction is about 2 percentage points. These effects rise at 3 y after release and then fall slightly at 5 y after release. However, our results find only small effects of imprisonment on new felony convictions after the prisoner has returned to the community (at most a reduction of 3–5 percentage points, although such effects are concentrated among high-severity felonies). We find no evidence that imprisonment increases overall criminal behavior after release, at least when criminal behavior is measured as convictions for any new felony. Finally, it is possible that some of the crime-preventative effects of the initial prison sentence on more severe felonies could be due to incapacitation effects of the type documented in Table 1 occurring during subsequent imprisonment. However, this cannot be the entire explanation. Note that if future prison admissions were creating a large incapacitation effect on new felonies, we would expect the new felony effect sizes to increase rather than decrease between years 3 and 5 as more and more prisoners were returned to prison.

Our results have important policy implications. First, probation sentences might be employed more frequently as an alternative to incarceration. The cost savings associated with probation is large relative to the incapacitation effect of imprisonment. In addition, a prison sentence does little to reduce criminal offending after release relative to offending by probationers. Second, because parole violations play a substantial role in the growth of the prison system through "back-end" sentencing, diverting technical parole violators from reimprisonment to alternative sanctions may be an important mechanism for shrinking the size of the prison system. However, more research is needed on which alternative sanctions are likely to be most effective at reducing offending and assisting ex-offenders in escaping involvement in the criminal justice system. Such a policy recommendation also depends critically on whether individuals returned to prison on parole violation would have engaged in serious offending had they remained in the community, a hypothesis that warrants further scrutiny.

We alert the reader to the limitations of this study. First, our results come from a single state and may not be generalizable to other states with different criminal justice policies and practices. The importance of returns to prison for parole violations is likely a direct consequence of the relative rates of parole and probation revocations in Michigan. Compared with state corrections practices across the nation in the same time period, Michigan's rates of probation revocations to incarceration were slightly lower than national averages while its rates of parole revocations to prison were much lower than national averages. In 2007, Michigan's probation revocation rate was 0.5% while the national average was 2%, and Michigan's parole revocation rate was 6.6% while the national average was 11.4%. [These values were calculated from

Bureau of Justice Statistics counts for Michigan and "All States" (23). Denominators are total number of individuals on parole or probation on January 1, 2007, plus all individuals entering that year (tables 2 and 3 in ref. 23). Parole numerator is revocations to prison without new sentence (table 7 in ref. 23). Probation numerator is incarcerations under current sentence (table S3 in ref. 23).] Thus, our findings regarding the sizeable impact of technical revocations on reimprisonment were found in a state with parole revocations rates that were lower than the national average. The estimated revolving-door effect estimated here might well be small relative to what would be found nationwide. Furthermore, our results come from a particular historical period when rates of reimprisonment for technical violations of parole were high, especially among African-Americans (22).

Second, we are unable to examine racial groups other than whites and blacks due to low numbers of Latinos and Asians in Michigan. Third, we can only examine criminal offending that is both known to law enforcement and results in prosecution and conviction. In addition, we focus on criminal offending that occurs in the community, the explicit focus of most studies of incapacitation. Our data do not allow for reliable measurement of crimes committed in prison, which typically only result in arrest or prosecution for the most serious crimes. Most criminal activity in prison is handled through internal disciplinary procedures. Fourth, our data do not currently allow for long-term follow-up.

Fifth, causal effect estimates from an IV analysis are "local average treatment effects." This means we are estimating the effect of incarceration in prison compared with probation among individuals for whom the randomly assigned judge made the difference between prison and probation (*SI Appendix, Methods*). Those are individuals who are on the margin between prison and probation. Our estimates do not provide average treatment effects for all individuals sentenced to prison in Michigan. As a result, they should not be interpreted as informative regarding radical policy changes such as decarceration on a massive scale, which would surely involve individuals who are far from the margin at which the effects in this paper are estimated.

Materials and Methods

Data. We collected, cleaned, and coded data on all individuals sentenced for felonies in Michigan between 2003 and 2006 based on administrative databases at the Michigan Department of Corrections (MDOC). A primary source of data are the presentence investigation reports prepared for judges before sentencing, which provide our presentencing covariates as well as judge identifiers. In Michigan, these reports are prepared by an employee of MDOC for all felony cases, even individuals not sentenced to prison. We follow our subjects in MDOC records (felony probationers are supervised by MDOC) between the date of their sentencing and the end of September 2013 to see when prisoners are first released from prison and to track all subsequent entries into prison, both for new felony sentences and for technical violations of probation and parole. Missing covariate data are imputed using a hotdeck procedure based on race and gender. The only variable with substantial missing data is education (14% of the sample). Race is missing for 0.2% of the sample, and marital status is missing for 0.3% of the sample. We stratify our analyses by race (white or nonwhite). Unfortunately, there are too few Latinos or members of other racial/ethnic groups in Michigan to examine effects separately for those groups.

Our analytical sample excludes individuals for whom judges have no discretion in sentencing. This excludes individuals sentenced for first-degree murder or for "flat" sentences, in which the minimum sentence is the same as the maximum sentence and is set by statute (mostly felony firearm crimes). We also exclude individuals whose cases are handled by specialty courts, as judges are not randomly assigned in such courts, and individuals who were on probation and were resentenced for a technical probation violation. This leads to a final analytic sample of 111,110 individuals sentenced for a felony between 2003 and 2006, of whom 9,922 were nonwhite and sentenced to prison, 21,279 were nonwhite and sentenced to probation, 10,067 were white and sentenced to prison, and 22,327 were white and sentenced to probation (the remainder were sentenced to jail or jail followed by probation, sentences included in the models but for which results

are not reported in this paper). Sample descriptive statistics are provided in *SI Appendix*, Tables S1 and S2.

Analytic Strategy. We estimate the causal effect of being sentenced to prison rather than to probation taking into account the effects of prison sentence length and probation sentence length. Due to the threat of unobserved differences between individuals sentenced to prison and probation, we rely on a natural experiment based on the random assignment of judges to criminal cases. In other words, judge identifiers serve as instruments for sentence type and length. Because judges are randomly assigned to offenders, they provide a source of exogenous variation in sentence type, or variation that is uncorrelated with offender and offense characteristics that might be predictive of recidivism. The intuition behind an IV design is to estimate the causal effect of interest (e.g., prison vs. probation sentence) using only the variation in the "treatment" produced by the IVs. This approach also assumes that the variation in treatment assignment provided by the instrument is independent of both observed and unobserved predictors of the outcome (see *SI Appendix, Methods*, on IV assumptions in this study). Counties may differ on a number of institutional and contextual factors (e.g., prosecutor practices, including use of plea bargaining, unemployment rates), and outcomes and offenders may vary systematically across counties, so we remove all between-county variation by including county fixed effects in our models. This also ensures that only variation in sentencing practices between judges in the same county will be used in the identification of causal effects.

To implement our IV estimator of the effect of incarceration in prison vs. probation, the effect of prison sentence length, and the effect of probation sentence length we use two-stage least-squares (2SLS) estimation. Our instruments are a set of dummy variables for the assigned judge plus interactions between judge dummies and presentence characteristics. Our treatments are dummies for prison vs. probation as well as the other two possible sentences one can receive for a felony (prison vs. jail and prison vs. jail with probation). Although we include the other treatments in the model to avoid introducing sample selection bias and also instrument for those sentences, we focus on the prison vs. probation comparison because it is the cleanest comparison of incarceration vs. no incarceration. We also include as treatments the interaction between the prison vs. probation dummy and sentence length variables, measured in months. Sentence length variables are centered at 24 mo (the modal sentence length for both types of sentences), so coefficients on the main effect of prison vs. probation are interpretable as the effect of a 24-mo prison sentence compared with a 24-mo probation sentence.

Risk Periods. A key issue for our analysis is the appropriate "risk" period for measuring outcomes. Probationers will be "at risk" for recidivism outcomes immediately following sentencing, but those sentenced to prison will not be at risk until their first release (or parole) due to incapacitation in prison. One policy effect of interest is the total effect of imprisonment, which includes the effect of incapacitation while imprisoned. This effect is captured by starting the risk period at the sentence date for all cases (Table 2). Any reform in sentencing for the marginal prisoner would correspond to effects estimated by starting the

risk period at sentencing. This also provides the cleanest counterfactual comparison between those who receive different types of sentences.

The other risk period starts the risk for prisoners at release (Table 1). Individuals in the treatment and control groups who were sentenced in the same year will start their risk periods in different years, and as a result, differences in sanction type could be conflated with the passage of time, which itself can affect recidivism or employment through either period effects (e.g., changes in the labor market or secular influences on crime) or age effects. When the risk period is started at release for prisoners, treatment group members are likely to be older, on average, at the start of the risk period (due to the passage of time), and as a result treatment effects could be biased. Another potential problem with starting the risk period at release is that release dates are endogenous because they are determined in part by postsentencing behavior in prison. Finally, starting the risk period for those sentenced to prison at their prison release date will shrink their follow-up period, meaning those with longer prison sentences will not have postrelease outcomes to measure, potentially introducing some sample selection bias into the estimates.

Because we want to identify the incapacitation mechanism and its contribution to the overall effect, we conduct our analysis both ways. This also facilitates comparison with other published studies, which typically measure outcomes from the release date. To deal with the confounding of age and period discussed above, we residualize all outcomes on the entire sample by age and year. (We estimate an ordinary least-squares regression model for each outcome that includes only dummies for age and year as predictors and then take the residuals from these models as our outcomes in the main analyses. Such residualized outcomes are therefore independent of age and year.) We have no solution to the endogeneity of release or the sample selection problems, so these "from release" estimates should be interpreted more cautiously. (Overall, 15.5% of prisoners were not released in time to measure outcomes 1 y following release, 21.3% of prisoners were not released in time to measure outcomes 3 y following release, and 33.1% of prisoners were not released in time to measure outcomes 5 y following release.) This problem is shared by most studies that use prison release as the start point for the risk period.

This study was reviewed and approved by the Health Sciences and Behavioral Sciences Institutional Review Board at the University of Michigan (FWA00004969) under Study ID HUM00046081. A waiver of informed consent was granted to this study due to the anonymized nature of the data and the number of subjects, which precluded individual consent. Permission to access the data from this paper can be requested from the MDOC (Office of Research and Planning, Michigan Department of Corrections, 206 East Michigan Avenue, Grandview Plaza, P.O. Box 30003, Lansing, MI 48909).

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