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DO PRIVATE PRISONS AFFECT CRIMINAL SENTENCING?

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ABSTRACT

Using a newly constructed complete monthly panel of private and public state prisons, we ask whether private prisons impact judges' sentencing decisions in their state. We employ two identification strategies, a difference-in-difference strategy comparing court-pairs that straddle state-borders, and an event study. We find that the opening of private prisons has a large effect on sentence lengths shortly after opening but this effect dissipates once the prison is at capacity. Public prison openings have no such effects, suggesting that private prisons have an impact on criminal sentencing that public ones do not.

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1 Introduction

Of the fourteen candidates for the Democratic Party presidential nomination in December 2019, twelve had explicit platforms to eliminate all private prisons, a position also shared by the Clinton campaign in the 2016 race.[1] This unified position is explained by two primary concerns that override the appeal of private prisons' lower costs of incarceration. The first concern is that private prisons shirk on non-contractible dimensions of quality, e.g. lower-quality rehabilitation programs which lead to higher rates of recidivism (Inspector General, 2016; Eisen, 2017). The second concern is that private prisons may have a negative impact on the application of justice or on judicial institutions (Mattera, Khan, LeRoy, and Davis, 2001; Ashton and Petteruti, 2011; Shapiro, 2011; Hartney and Glesmann, 2012; Mason, 2012). In its most deleterious form, this concern was manifested in the 2011 "kids for cash" scandal, in which two Pennsylvania judges took bribes from private detention facilities in exchange for harsher juvenile sentences.[2]

While the first concern has been analyzed both theoretically (Hart, Shleifer, and Vishny, 1997) and empirically (Bayer and Pozen, 2005; Thomas, 2005; Spivak and Sharp, 2008; Mukherjee, 2019), the second concern has not yet been the subject of any causally identified empirical analysis. Our study provides the first rigorous analysis of this question — whether private prisons impact sentencing decisions — based on a newly constructed monthly panel dataset of the geo-location and capacity of the universe of all private and public state prisons from 1980 to today. We combine these data with newly collected data on criminal sentencing in thirteen states' trial courts.[3] We focus on state trial courts because, unlike federal courts, convicts from state courts are sent to the same state's prisons. Furthermore, trial courts are the states' primary courts of general jurisdiction, and as such handle the vast majority of all criminal cases in the U.S.

We pursue two identification strategies: The first emphasizes unobserved spatial heterogeneity that may correlate with both criminal sentencing and capacity changes of private prisons. In a generalized difference-in-difference strategy, we rely on within-state changes in private-prison

[1] As of December 2nd, see `www.politico.com/2020-election/candidates-views-on-the-issues/criminal-justice-reform/private-prisons/`.

[2] The growth of the private prison system in the last three decades has coincided with a huge increase in the U.S. prison population: in the roughly three decades since the first private prison in the U.S. opened in 1984, the imprisoned population has increased by 194 percent relative to a general population increase of 36 percent (Bureau of Justice Statistics, 1984, 2015).

[3] Data on state-court sentencing is handled by each state separately, many of whom have not digitized their data yet. Obtaining data for 13 states was the result of a three year process of requesting data from every single state.

capacity (driven by openings, closings, expansions, contractions or occasionally privatizations of public prisons), and on comparing changes in sentencing only within neighboring trial court pairs on either side of a state border. By focusing only on such *contiguous-border court-pairs* (CBCP), we are able to account for local heterogeneity and local unobserved trends in criminal activity, policing, and the local demand for sentencing.[4] The first strategy identifies the opening and closing of private and public prisons, as well as (more continuous as well as more frequent) capacity expansions and contractions of existing facilities. Our second strategy is an event study analysis that focuses on the monthly time path of sentencing effects around only openings and closings of prisons, because these are the largest and most salient shocks to prison bed capacity. Our data contain 40 private prison openings including three privatizations of public prisons, 13 private prison closings, 42 public prison openings, and 27 public prison closings (exclusive of three privatizations).

In the CBCP strategy, a 0.40 percent increase in private prison capacity (the average associated with a newly opened prison in the data) increases sentence lengths in that state's courts by roughly one percent, or 14 days, compared to adjacent courts that are in neighboring states and not affected by the prison capacity change. The effect is robust to many variations of spatial, time, and judge fixed effects. The baseline effect includes probations (zero-sentences) and thus combines the intensive margin of sentence length with the extensive margin of going to prison. When we drop probation observations, the intensive margin effect of a 0.40 percent increase in private prison capacity is an increase in sentence length of 10 days. We find no such effect of capacity changes in *public* prisons on sentencing.

While the effects estimated in the CBCP strategy are averaged over the full post-period of all prison capacity changes, the event study investigates the more granular time-path of sharp events of prison openings and closings. The event study setup shows that the opening of a private prison leads to an increase in sentence length of over three months in the first two months after the opening, but a precisely estimated zero-effect thereafter.

In other words, defendants who happen to stand trial shortly after a private prison opens can expect to be sentenced to three additional months relative to otherwise identical defendants who stand trial for the same crime a few weeks earlier or later. A back-of-the-envelope calculation

[4] The identification-advantages of state border discontinuities have been explored, among others, in the context of labor market regulation (Dube, Lester, and Reich, 2010), manufacturing (Holmes, 1998), and banking (Huang, 2008).

shows that it takes two-and-a-half months for new convictions in a states' courts to fill the capacity of the average newly opened private prison. In short, private prisons appear to have an effect on sentencing precisely as long as they have vacant capacity.

There are no pre-trends in sentencing before a private (or public) prison is opened. Interestingly, however, private prison closings are preceded by a strong negative pre-trend (sentences getting shorter), which may be anticipatory: defendants are not sent to facilities that are closing soon. There are no such pre-trends before public prison closings. Finally, public prison openings and closings have precisely estimated zero-effects on sentencing, despite being very similar in magnitude (i.e., average capacity change) to private prison ones.

How, then, do private prisons impact judges' sentencing decisions? There are two potential explanations. The first—more deleterious— explanation is that private prison companies may directly influence judges, for example through campaign contributions or even outright bribes as in the "kids for cash" scandal. A second possibility is that judges internalize the cost-savings associated with private prisons, consistent with other evidence that judges trade off benefits of longer sentences (primarily lowered recidivism) against their fiscal burden (Ouss, 2018; Mueller-Smith and Schnepel, 2019).[5] The cost-savings motive resonates particularly with the core effect being concentrated in the two months after a prison's opening because most states guarantee private prisons to pay for an occupancy of between three-quarters to ninety percent of their bed capacity (In the Public Interest, 2013), effectively reducing the marginal fiscal burden of an inmate to zero up to this guaranteed occupancy.

We can measure cost savings from state laws governing private prison contracts. We find that private prisons indeed have more pronounced effects on sentencing when their cost-savings are larger (which should also reduce rents from direct influence), both in the CBCP difference-in-difference identification strategy and in the event study.

A 'direct influence' channel of private prisons to judges is inherently more difficult to measure. However, the literature on 'electoral sentencing cycles' suggests an indirect test for such influence. It shows that electoral competition leads judges to levy harsher sentences in the lead-up to elections (Huber and Gordon, 2004; Berdejó and Yuchtman, 2013; Dippel and Poyker, 2019; Abrams, Galbiati, Henry, and Philippe, 2019). The presence of such cycles implies that 'direct influence' by

[5] Of course, judges may also internalize the costs to defendants.

private prisons should be most important when judges are seeking re-election (by way of campaign contributions). This suggests the testable hypothesis that under the 'direct-influence' story, private prisons should have more pronounced effects on sentencing when judges are coming up for re-election. We find considerable evidence for the presence of 'electoral sentencing cycles' in our data, but we find no evidence that the effect of private prisons on sentencing varies with these cycles. Overall, the evidence is thus more consistent with the cost-savings explanation than with the direct-influence explanation fore the main effect we document.

Our paper contributes to a literature on judicial decision making, and in particular the "extra-legal considerations" of judges (Posner 2008, p8-11). Amongst these are the fiscal costs of incarceration (Ouss, 2018), prisons' capacity constraints (Mueller-Smith and Schnepel, 2019), defendants' race (Steffensmeier and Demuth, 2000; Abrams, Bertrand, and Mullainathan, 2012; Park, 2014), media scrutiny (Lim, Silveira, and Snyder, 2016), judges' own characteristics like gender, ethnicity and party affiliation (Lim and Snyder, 2015; Lim, Snyder, and Strömberg, 2015), their re-election concerns (Huber and Gordon, 2004; Berdejó and Yuchtman, 2013; Abrams et al., 2019), and even their emotions on the day (Eren and Mocan, 2018). We show that the presence of private prisons needs be added to the list of extra-legal considerations, and that their impact is most likely explained by judges' considering the fiscal costs of incarceration.[6] Although a 'direct influence' effect would be yet more deleterious, the 'costs savings motive' on its own also raises serious equity concerns about the effect of private prisons on the application of justice in U.S. courts.

There is a closely related literature on the consequences of judicial decision making for defendant outcomes (Kling, 2006; Di Tella and Schargrodsky, 2013; Galasso and Schankerman, 2014; Aizer and Doyle Jr, 2015; Dobbie, Goldin, and Yang, 2018; Mueller-Smith and Schnepel, 2019; Norris, Pecenco, and Weaver, 2019; Bhuller, Dahl, Løken, and Mogstad, 2020).[7] In this literature, we relate closely to Mukherjee (2019). Focusing on Mississippi, and using changes in private prison

[6] Galinato and Rohla (2018) investigate the same question as us through the lens of a game-theoretic model, in which judges care about prison capacity constraints as well as bribes from private prisons. They provide evidence in favor of the direct influence mechanism but they lack a plausible identification strategy and the right data, because they use an "off the shelf" sample of *federal* criminal trials from the federal U.S. Sentencing Commission (USSC). Like us, they relate sentence length to the presence of private prisons in the same state, but this approach is not valid in the USSC data because (*i*) there is no spatial relation between the location of a federal court and where in the federal prison system a convict is sent to, and because (*ii*) the USSC includes no data on a crime's severity and the defendant's criminal history.

[7] For identification, most of these papers use judge fixed effects coupled with random assignment of judges to cases. Because of the need to link sentencing data to other defendant outcome data, these papers typically use data on just a single county or at most one state. In contrast, we use data for 13 states.

capacity over time as an instrument for whether an individual serves time in a private prison, Mukherjee finds that this increases a convict's total time incarcerated (holding fixed the sentence) because private prison inmates receive more infractions, thus delaying parole. This pro-longed incarceration wipes out half the savings from the lower cost of private prisons. Concerningly, Mukherjee finds that longer incarceration in private prisons does not reduce recidivism upon release, which runs counter to existing evidence that longer incarceration does in general reduce recidivism (Owens, 2009; Kuziemko, 2013; Hansen, 2015; Bhuller et al., 2020). This is explained by the lower quality of rehabilitation programs in private prisons, providing prima facie evidence for the concern originally formalized in Hart et al. (1997). Mukherjee's and our findings amplify one another with regards to equity concerns because they suggest that defendants who happen to stand trial shortly after a private prison opens can expect to not only receive longer sentences but also serve a larger portion of these sentences, while potentially receiving lower-quality rehabilitative programs.

2 Data Sources and Construction of Samples

2.1 Prison Data

The U.S. private prison industry began in the early 1980s, when the rising cost of state-run prisons started becoming a fiscal problem for state governments.[8] The first privately run corrections facility, Hamilton County Jail in Tennessee, was opened in 1984 (visible in the top-panel of Figure 1). It was run by *Corrections Corporation of America*'s (CCA). Only a year later, CCA proposed to take over the entire prison system of Tennessee. This did not happen, but 1984 nonetheless marked the start of a rapid expansion of the private prison industry.

The monthly panel dataset of private and public state prisons we use in this paper was constructed from several sources. First, we use the 2005, 2000, 1995, 1990, and 1985 Census of State and Federal Adult Correctional Facilities. Those censuses record for all U.S. prisons when they opened, whether they are private or public, their capacity (measured in beds) and when it was changed, and if they house male, female, or both convicts. We focus only on state prisons. We

[8] There was a 19th century history of private prisons in the United States dating back to the first private prison's establishment in San Quentin in 1852. See (McKelvey, 1936, ch.1-2).

then used each state's Departments of Correction website to update the base data to include prisons after 2005.[9] The top-panel of Figure 1 displays the evolution of $PrivateC_{ct}$ and $PublicC_{ct}$, i.e., the capacity (in beds) of private and public prisons, in Tennessee. Capacity can change because of the expansion, contraction or closing of existing facilities, or the opening of new ones. Bigger capacity changes (above 200 beds) are almost always the result of openings or closings. The time range is determined by the availability of the sentencing data, which we discuss next.

2.2 The Sentencing Data

We focus on state trial courts' sentencing decisions in felony offenses.[10] Private prisons in our data are for male prisoners only, and we therefore focus our analysis on the effect of male prisons on the sentencing of male defendants (which generates a natural placebo exercise of looking at sentences of female defendants). Each state maintains their own sentencing data, and we separately requested these data from every states' Sentencing Commissions and Departments of Corrections. Many states do not maintain an organized electronic repository of their court cases, or do not share it. Nonetheless, we were able to obtain sentencing data from 15 states: Alabama, Arkansas, Colorado, Georgia, Kentucky, Maryland, Minnesota, Mississippi, Nevada, North Carolina, Oregon, Tennessee, Texas, Virginia, and Washington. Obtaining these data from each state separately took several years and the process was quite idiosyncratic. Online Appendix A.2 provides details on how we obtained the sentencing data in each state. Colorado and Minnesota are the only states in our data that do not have a neighboring state within the sample, which reduces the effective number of states to 13 in our *contiguous-border court-pairs* (CBCP) identification strategy. The bottom-panel of Figure 1 shows these 13 states (in light blue) as well as the counties on their borders (in dark blue), which are discussed in Section 2.3.

2.3 Border-Pair Sample Construction

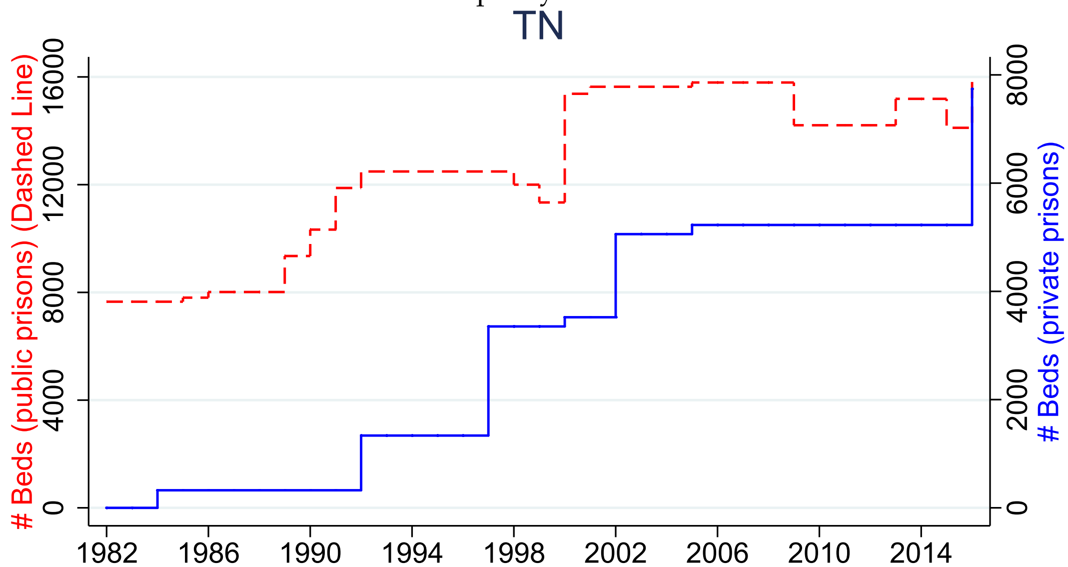
Why prison-capacity changes are state-wide changes: The location of prisons, both public and private is determined in state legislatures. Locations are selected with economic considerations in mind, typically in structurally weak areas and with a view towards providing local employment

[9] Details in Online Appendix A.1.

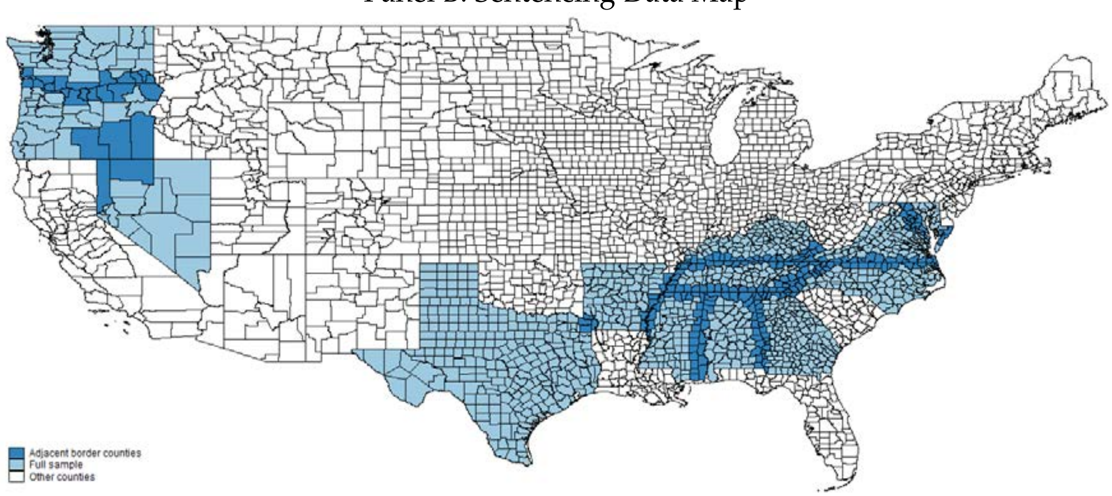
[10] This excludes courts of limited jurisdiction, such as family courts and traffic courts, and, excludes crimes of minor severity amongst the courts of general jurisdiction.

Figure 1: The Data

Panel A: Prison Capacity Variation in Tennessee



Panel B: Sentencing Data Map



Notes: The top-panel shows the evolution of the capacity of private and public prisons in Tennessee. The dashed (red) line is the state-specific time-series of public prison capacity (number of beds). The solid (blue) line is the state-specific time-series of private prison capacity (number of beds). The time range in each state is determined by sentencing data availability. In Figure 1, Tennessee has openings in 1984, 1992, 1997, 2002, and 2016. The figure includes the opening of the very first privately operated state-run prison in Tennessee's Hamilton County in 1984. As an added point of comparison, Online Appendix Figure 1 shows the equivalent data for Mississippi, where the data displays essentially the same variation Figure 1 in Mukherjee (2019). The bottom-panel shows the 13 states (in light blue) in our sample, and the 252 border-counties thin this sample (in dark blue).

opportunities (Mattera et al. 2001, p.7, Chirakijja 2018, p.6). Often such structurally weak areas actively compete in lobbying the state legislature for the construction of a prison (private or public) in their county. For example, Mattera et al. (2001, p.22) recounts how the collapse of the oil boom in the late 90s, and the resulting dearth of local jobs, was a driving factor in allocating the construction of an Oklahoma private prison to the town of Hinton. Similar examples abound in the same and in other sources. The location of a newly opened prison is therefore endogenous to local factors that could also impact local crime and therefore sentencing. This implies that using a court's distance to a private prison as a source of variation is endogenous, despite its intuitive.[11]

Once we think of $PrivateC_{ct}$ and $PublicC_{ct}$ as state-level changes, the *contiguous-border court-pair* (CBCP) sample offers the cleanest spatial comparison because it is amenable to non-parametrically controlling for unobserved local trends (Dube et al., 2010; Holmes, 1998; Huang, 2008).[12] In our setting, this means trends in criminal activity, policing and in the local electorate's demand for sentencing. Trial court districts in our data are always the same as counties. Our CBCP identification sample therefore consists of all the county-pairs in our data that straddle a state border. This amounts to 252 border counties out of of total of 417 counties in our 13 states. These border counties are mapped to 237 distinct county-pairs.

We provide a detailed breakdown of the number of counties and pairs on each state-border segment in Online Appendix Table 1. This table clarifies how many border-segments are linked to each state, and also reports on the years covered by each state's sentencing data, with varying coverage of the years 1980 to 2017. While the advantages of border-discontinuities in terms of statistical identification do not really depend on this, the generalizability of the results will be higher if the border counties are representative of all counties in a state on observable characteristics. Online Appendix Table 2 shows that the socio-economic characteristics of border-court counties are not statistically different from all other counties in the same states. Further, Online Appendix Figure 3 provides an illustration of the identifying variation in a border-segment, using as an example the border-segment shared by Georgia and Tennessee.

[11] For completeness, we do report regressions where the treatment is a distance-weighted sum of private prison capacities in a state in Online Appendix Table 4.

[12] See Dube et al. (2010) for a taxonomy of the differences between identifying the effect of state-level policy changes in a "full sample" of all counties vs identifying the same changes in a border-county sample.

3 The Effect of Private Prisons on Criminal Sentencing

Section 3.1 presents the results of the generalized difference-in-difference estimation strategy in the CBCP sample described in Section 2.3, where we regress sentence length on changes in private and public prison capacity, as well as defendant and crime characteristics. In Section 3.2, we focus only on sharp openings and closings of private and public prisons in an event study framework. Section 3.3 provides suggestive evidence on the mechanisms.

3.1 The Effect of Private Prisons on Sentencing

The empirical specification we will estimate is

$$\log(\text{sentence})_{i(ct)} = \beta^{\text{T}} \cdot \log(\text{PrivateC})_{st} + \beta^{\text{T}'} \cdot \log(\text{PublicC})_{st} + \beta^{\text{X}} \cdot X_i + \mu_s + \mu_{p(c)} + \mu_{st} + \mu_{p(c)t} + \epsilon_{icts}, \tag{1}$$

where case i is heard in court c at time t. We use $\log(\cdot)$ as shorthand for the inverse hyperbolic sin which can be interpreted in the same way as the log function but allows us to keep zero values in private prison capacity.[13] X_i are characteristics of the crime and of the defendant.

The CBCP design is reflected in the combination of a state fixed effect μ_s that absorbs fixed state-differences in the police, legislation and judicial system, and a court-pair fixed effect $\mu_{p(c)}$ that absorbs local social, political and economic conditions. Separate time trends or period fixed effects μ_{st} and $\mu_{p(c)t}$ are specific to these separate spatial aggregates.

Panel A in Table 1 presents the baseline results of estimating equation (1). Specifications get incrementally more demanding across columns: Column I reports results for the specification with (time-invariant) court-pair fixed effects $\mu_{p(c)}$ and state-specific year- and fiscal-year fixed effects μ_{st}.[14] Column II adds defendant characteristics subsumed in X_i. In particular, we include for a dummy for recidivism, age, age squared, and race (Asian, Black, Hispanic, and Native American). Column III adds controls the severity of the crime, also subsumed in X_i.[15] Column IV uses pair-specific year fixed effects as a more flexible version of $\mu_{p(c)t}$. Finally, column V adds pair-specific

[13]See Burbidge, Magee, and Robb (1988); Card and DellaVigna (2017).

[14] Calendar years are important because legislation (which can affect sentencing) usually changes on the 1st of January. Fiscal years are important because many transfer programs that can affect crime (like *Supplemental Nutrition Assistance Programs*) change with fiscal years. Fiscal years in our data end on March 31st, June 30th or September 30th.

[15] States report crime severity in varying ways, using ordinal scales or cardinal scales. To combine these different classification schemes into a single regression we turn them into state-specific sets of fixed effects.

Table 1: The Effect of Private Prisons on Sentence Length

	I	II	III	IV	V	VI
Panel A: baseline	Dependent variable: Sentence (log months)					
Log private prison capacity	0.022**	0.021**	0.020**	0.018**	0.020**	0.021*
	[0.0208]	[0.0336]	[0.0311]	[0.0322]	[0.0432]	[0.0583]
Log public prison capacity	-0.166	-0.133	-0.094	-0.113	-0.159	-0.142
	[0.5986]	[0.6353]	[0.7507]	[0.7408]	[0.6320]	[0.6955]
R-squared	0.379	0.390	0.455	0.468	0.468	0.474
Observations	765,338	765,338	765,338	765,338	765,338	765,338
Panel B: exclude probation						
Log private prison capacity	0.014***	0.013***	0.012***	0.012***	0.014***	0.013***
	[0.0000]	[0.0000]	[0.0000]	[0.0000]	[0.0000]	[0.0000]
Log public prison capacity	-0.326	-0.309	-0.297	-0.361	-0.343	-0.346
	[0.1410]	[0.1097]	[0.1017]	[0.1567]	[0.1549]	[0.1570]
R-squared	0.380	0.392	0.515	0.530	0.530	0.535
Observations	570,674	570,674	570,674	570,674	570,674	570,674
Panel C:	Dependent variable: D(Incarceration / No Probation)					
Log private prison capacity	0.002	0.002	0.002	0.002	0.002	0.002
	[0.3201]	[0.3679]	[0.3866]	[0.4486]	[0.4285]	[0.4714]
Log public prison capacity	0.028	0.032	0.035	0.040	0.022	-0.142
	[0.5426]	[0.4860]	[0.5000]	[0.5027]	[0.6802]	[0.6955]
R-squared	0.258	0.263	0.293	0.309	0.309	0.313
Observations	765,338	765,338	765,338	765,338	765,338	765,338
Panel D: full sample						
Log private prison capacity	0.002	0.002	0.000	0.002	0.002	0.003
	[0.7237]	[0.6456]	[0.9275]	[0.7146]	[0.7105]	[0.6044]
R-squared	0.368	0.382	0.437	0.448	0.448	0.452
Observations	3,544,144	3,544,144	3,544,144	3,544,144	3,544,144	3,544,144
Panel E: full sample, exclude probation						
Log private prison capacity	0.003**	0.003**	0.000	0.001	0.001	0.002
	[0.0182]	[0.0297]	[0.9008]	[0.6325]	[0.6220]	[0.1631]
R-squared	0.382	0.389	0.501	0.514	0.514	0.521
Observations	2,699,237	2,699,237	2,688,027	2,688,033	2,687,664	2,684,438
Log public prison capacity	✓	✓	✓	✓	✓	✓
FEs: state x fiscal-year	✓	✓	✓	✓	✓	✓
Demographic controls		✓	✓	✓	✓	✓
Case controls			✓	✓	✓	✓
FEs: state x year	✓	✓	✓			
FEs: court-borderpair	✓	✓	✓			
FEs: court-borderpair x year				✓	✓	✓
Linear trend: state x calendar-month					✓	✓
FEs: judge						✓

Notes: (*a*) The fixed effects taxonomy pertains to the CBCP sample in panels A–C. In the full sample in panels D–E, court-borderpair fixed effects are replaced with court (=county) fixed effects. (*b*) We report p-values in square brackets. In Panels A–C, standard errors are two-way clustered on state and border segment. In Panels D–E, standard errors are two-way clustered on state and county level. *** p<0.01, ** p<0.05, * p<0.1

time controls $\mu_{p(c)t}$ in the form of a pair-specific linear trend that increments in months. Finally, column VI adds a specification with judge fixed effects. Unfortunately, we only have judge identifiers in nine of the 13 states' sentencing data.[16] Nonetheless, judge fixed effects are an important robustness check given the large "judge fixed effect" literature. It is reassuring that judge fixed effects add considerably to the regressions' *R-squared* but do not move our coefficient of interest. A 0.42 percent increase in private prison capacity (equal the average state-wide capacity increase from a newly opened prison in our data) increases sentence lengths by roughly 14 days (a roughly one-percent increase, 0.42×0.023, multiplied by a mean sentence length of 45 months) compared to adjacent courts that are in neighboring states and not affected by the prison capacity change. This point estimate is precisely estimated and also practically unchanged in magnitude across columns. In contrast, the point estimate on *PublicC*$_{st}$ is never close to conventional statistical significance.

Panel A combines potential effects of private prisons at the intensive margin (longer sentences) and at the extensive-margin (being sent to prison). In Panel B, we isolate the intensive margin by dropping cases that received probation.[17] As expected, the point estimate is smaller for only the intensive margin, but it remains equally stable across columns and is even more precisely estimated than in Panel A. On the intensive margin alone, a 0.42 percent increase in private prison capacity increases sentence lengths by roughly 10 days (a 0.42×0.013 increase, multiplied by a mean sentence length of 60 months). Panel C isolates the extensive margin. The point estimate follows from the composition of the total effect (0.023) into its intensive- and extensive margins $(0.013 + 0.002 \times log(60))$. It is, however, very imprecisely estimated.

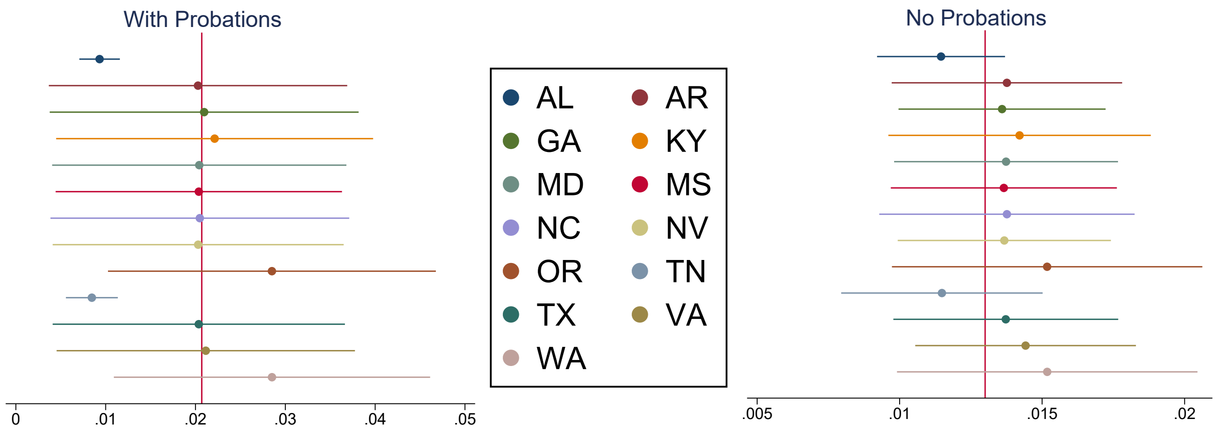
Figure 2 verifies that our results are not driven by just one state or a small set of states. Dropping one state at a time, the estimated coefficient always stays close to the baseline estimate and always remains statistically significant. Dropping either of Alabama or Tennessee cuts the coefficient in half, but also increases its precision considerably.[18] The reason for this is that the border-segments of Alabama and Tennessee come closest to generating an *extensive* margin effect on the probability of going to prison, which is part of the total effect estimated in the data with probations

[16] In states without judge identifiers we use the court fixed effect as a placeholder.

[17] Online Appendix Figure 4 displays the distribution of sentence lengths with and without probations.

[18] Dropping Alabama drops three border-segments from the analysis (with Mississippi, Georgia, and Tennessee). Dropping Tennessee drops seven of the 17 border-segments in Online Appendix Table 1.

Figure 2: Robustness of the Results in Table 1



Notes: This figure reports on the point-estimate and 95th-percent confidence band that results when re-estimating the specification in Column VI of Table 1, dropping one state at a time. The (red) vertical line is the baseline point estimate. The results are sorted top-to-bottom in alphabetical order, i.e., omit AL, then AR, then GA, etc.

(see Online Appendix Figure 5). When we focus on the *intensive margin only* in the right panel, the effect of dropping Alabama or Tennessee is much less pronounced.

Online Appendix Table 3 reports on a number of additional validity checks that ensure the results in Table 1 are not driven by confounding factors: We show no effect on female defendents (almost all private prisons in our data are for male prisoners only), and we show results insignificant when we counter-factually shift the time of all changes in private prison capacity by 9, 6, or 3 months. For completeness, we also report the full sample results in panels D–E. The estimated coefficients are sign consistent but much smaller and usually not near conventional statistical significance levels. Finally, Online Appendix Table 4 reports on regressions where treatment is a state's distance-weighted sum of private prison capacities. Results fall in between CBCP and full sample in magnitude and significance, but are subject to serious endogeneity concerns; see Section 2.3.

3.2 Event Study Evidence

The results in Section 3.1 identify the smaller (and more frequent) capacity changes of existing facilities in addition to the sharper capacity changes associated with the opening and closing of prisons. In this section, we focus only on prison openings and closings because these are the largest shocks to prison bed capacity (and should be the most salient to judges).[19] Our data contain 40 private prison openings including three privatizations of public prisons, 13 private prison closings, 42 public prison openings, and 27 public prison closings (exclusive of the three privatizations). The regression we run is

$$\mathrm{y}_{i(ct)k} = \underbrace{\sum_{l=\underline{l}}^{-1} \gamma_l \cdot \mathrm{D}(t - E_i^k = l)_{it}}_{\text{pre-event period}} + \underbrace{\sum_{l=1}^{\bar{l}} \gamma_l \cdot \mathrm{D}(t - E_i^k = l)_{it}}_{\text{post-event period}} + \beta^{\mathrm{X}} \cdot X_i + \lambda_k + \lambda_{st} + \lambda_c + \varepsilon_{icts}, \quad (2)$$

where $\mathrm{y}_{i(ct)k}$ is sentence-length of case i inside event window k, and λ_k is a fixed effect for event k. We slice the sentencing data into 12-month *observation-windows* (6 months before and after each event). All observation windows are then pooled. Following best practice, we bin the end-points using an *effect window* of 8 month, so that the fourth to sixth months before and after events each share a coefficient (Borusyak and Jaravel, 2016; Schmidheiny and Siegloch, 2019).[20]

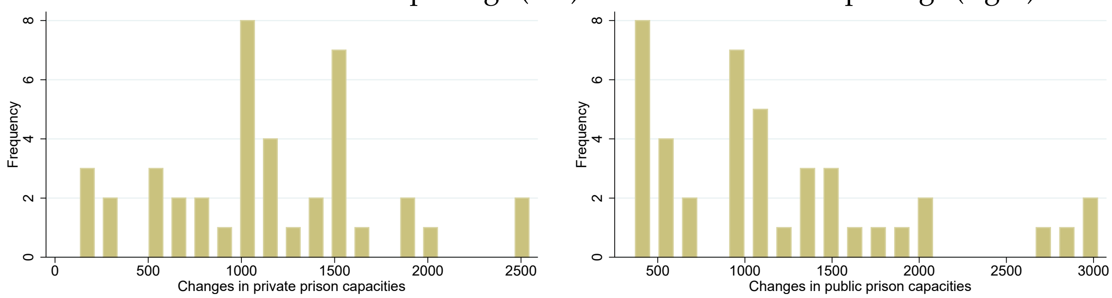
Figure 3 in its top-panel reports histograms of the 40 private prison and 42 public prison openings. Capacity changes associated with these two event-types are very similar in magnitude. The means are respectively 1,150 and 1,200. Any differences in estimated effects on sentencing across the two event types is therefore not likely to be driven by the events' magnitudes. The medium-panel is the core-result, comparing the effect of private prison openings on the left and public prison openings on the right. The first note-worthy feature is that neither type of opening exhibits any pre-trends. This suggests the exact timing of the openings is not related to trends in crime or in sentencing. It is also consistent with the fact that although a prison opening is known long in advance, there is no reason for sentencing to respond before it actually does open. The second noteworthy feature is that there no effect at all of public prison openings, but a notable effect of private prison openings, and it is concentrated in the first two months after the opening. Sentence lengths are increased by 3.7 months in the first months after an opening, and by 3 months in the second month after an opening. These point estimates are much more pronounced than the 14 day estimate implied in Panel A in Table 1. This is because the generalized difference-in-difference specification (1) averages the effect over an openings' full post-period and therefore

[19] The average change associated with prison openings or closings is 1,150 beds.

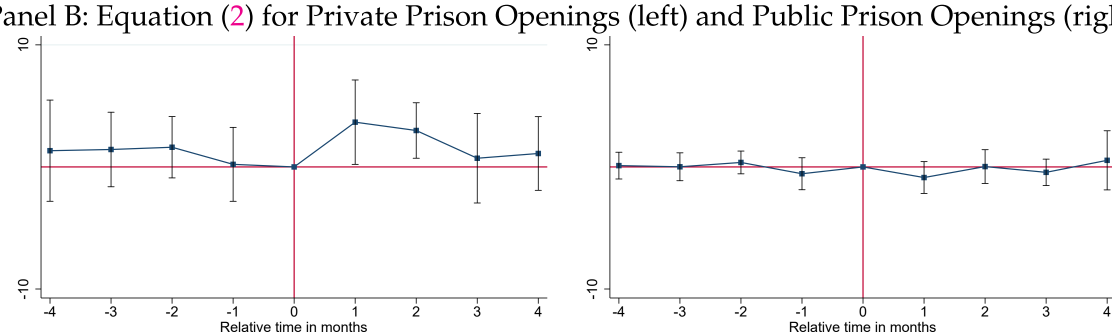
[20] An *observation-window* of 12 months and *effect window* of 8 months imply that $\{\underline{l}, \bar{l}\} = \{-4, +4\}$, and that γ_{-4} is identified using sentences in $t = \{-6, -5, -4\}$, and γ_4 using sentences in $t = \{4, 5, 6\}$, away from event k's date E_i^k.

Figure 3: Event Study Analysis

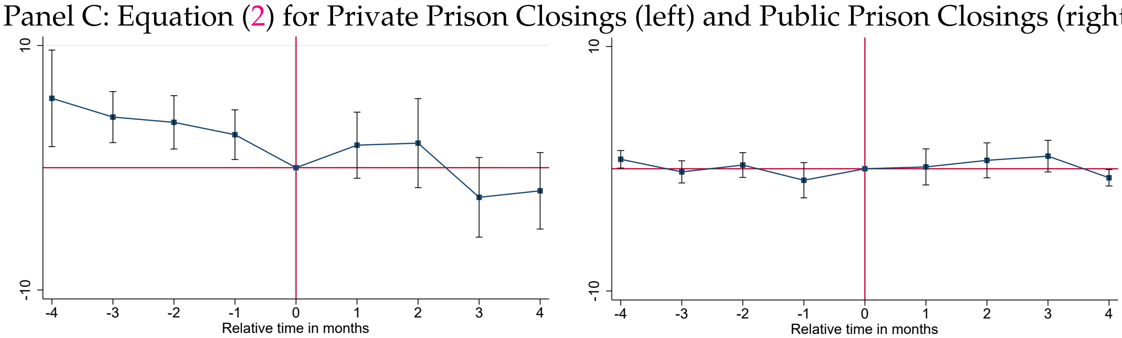
Panel A: Private Prison Openings (left) and Public Prison Openings (right)



Panel B: Equation (2) for Private Prison Openings (left) and Public Prison Openings (right)



Panel C: Equation (2) for Private Prison Closings (left) and Public Prison Closings (right)



Notes: Panels B and C graph the results of estimating equation (2) for private and public prison openings and closings. Point estimates are reported in Online Appendix Table 5. Online Appendix Figure 6 and Online Appendix Table 6 show results when we exclude probations.

does not capture the time path of the effect.

The narrowly concentrated effect we find makes sense from the perspective of average event's magnitude: a back-of-the-envelope calculation shows that it would take two-and-a-half months worth of convictions to fill the capacity of the average newly opened private prison if every convict in a state was sent there after its opening. In short, private prisons appear to have an effect on sentencing precisely as long as they have vacant capacity.

The bottom panel reports results from the same setup, but comparing the results of closings. There is again neither a pre-trend or effect for public prisons. There is, however, a notable pre-trend for private prisons. This is consistent with the fact that closings are anticipated. Unlike with openings, which are of course also anticipated, sentencing may respond in anticipation *to closings* because new inmates are unlikely to be sent to prisons that are about to close.

3.3 Evidence on Mechanisms

One potential explanation for an effect of prison expansions on sentence lengths could be that judges consider prison capacity per se. For example, Mueller-Smith and Schnepel (2019) argue that over-crowding drives prosecutors and judges to "divert" people from criminal prosecution. However, a capacity explanation for the effects we find is inconsistent with the effect being concentrated in only private prisons because there are as many new public prison openings in our data as private ones, and they are of similar magnitude.[21]

This leaves two primary explanations. One is the sort of 'judicial capture' suggested by the "kids for cash" scandal, i.e., private prison companies may directly influence judges to levy longer sentences. A second is that judges internalize the cost-savings associated with incarceration in private prisons. To make progress on this question, we need direct measures for the two distinct mechanisms. We can measure the cost of private relative to public prisons because in most states these are written into the laws governing the contracting with private prisons.[22] We define the (per bed) savings rate from private prisons as

$$Saving_s = 1 - \frac{\text{Cost in private prison}_s}{\text{Cost in public prison}_s}. \tag{3}$$

[21] Online Appendix Table 9 shows that our results are not driven by over-crowding.

[22] When they are not legislated, we take the measures from reported savings. We check the robustness of results to capping reported savings at 20 percent. Details in Online Appendix A.3.

Table 2: Cost Savings Motives vs Judicial Capture

	I	II	III	IV	V	VI	VII	VIII	IX	X
	Dependent variable: Sentence (months)									
Sample:	CBCP			Full		Judge-Sample: AL, GA, NC, TN, WA				
Log private prison capacity	0.659*	0.245		0.011		0.206*	0.254**	0.343**	0.195	
	[0.0608]	[0.3014]		[0.9144]		[0.0761]	[0.0134]	[0.0442]	[0.4246]	
Log private prison capacity		3.866***		1.369**					1.074*	
x share saved		[0.0063]		[0.0158]					[0.0977]	
Log private prison capacity			0.084		-0.003					0.195
in low saving states			[0.3461]		[0.9774]					[0.4238]
Log private prison capacity			1.098***		0.270***					0.428**
in high saving states			[0.0010]		[0.0000]					[0.0133]
Proximity to election							0.925***	1.862	1.899	1.899
							[0.0055]	[0.3018]	[0.3650]	[0.3649]
Proximity to election x								-0.115	-0.134	-0.134
Log private prison capacity								[0.5667]	[0.5713]	[0.5713]
Δ High and Low savings, p-value			[0.0040]***		[0.0214]**					[0.0982]*
R-squared	0.310	0.310	0.310	0.341	0.341	0.469	0.469	0.469	0.464	0.464
Observations	765,338	765,338	765,338	3,544,144	3,544,144	804,752	804,752	804,752	804,752	804,752
Log public prison capacity	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
FEs: state x fiscal-year	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
FEs: quarter FE	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
FEs: court-borderpair	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
FEs: state x year	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
FEs: court-borderpair x year	✓	✓	✓	✓	✓	✓	✓	✓		
Demographic controls	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Case controls	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
FEs: judge	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓

Notes: In columns IV-X, the court-border-pair fixed effects become simple court(=county) fixed effects. Similarly border-pair year fixed effects become court year fixed effects in columns IV–VIII. In square brackets we report p-values. Standard errors are two-way clustered on state and border segment in columns I–III, two-way clustered on state and county in columns IV–V, and two-way clustered on on calendar-year and quarter-of-year in columns VI-X. *** p<0.01, ** p<0.05, * p<0.1

In the states that include judge identifiers we follow the electoral cycle literature (Huber and Gordon, 2004; Berdejó and Yuchtman, 2013) in defining judge j's election cycles as a linear running variable 'proximity to election'

$$PtE_{j(s)t} = t/T_s, \tag{4}$$

that starts at 0 on the day after a general election, is scaled from 0 to 1, and increases by $1/T_s$ each day until it equals 1 on the day of the next general election.[23] With these measure in hand, we estimate the following extension of expression (1)

$$\begin{aligned} \text{Sentence}_{i(jct)} = & \quad \beta^{\mathrm{T}} \cdot \mathit{PrivateC}_{st} + \beta_{\mathrm{CS}} \cdot \mathit{PrivateC}_{st} \cdot \mathit{Saving}_s \\ & + \quad \beta_{PtE} \cdot \mathit{PtE}_{jt} + \beta_{DI} \cdot \mathit{PtE}_{jt} \cdot \mathit{PrivateC}_{st} \\ & + \quad \beta^{\mathrm{T}''} \cdot \mathit{PublicC}_{st} + \beta^{\mathrm{X}} \cdot X_i + \mu_s + \mu_{p(c)} + \mu_{st} + \mu_j + \mu_{p(c)t} + \epsilon_{icts}, \end{aligned} \tag{5}$$

where β^{T} and $\beta^{\mathrm{T}''}$ are the same as before, $\widehat{\beta_{\mathrm{CS}}} > 0$ would be evidence for the *cost-savings* hypothesis, $\widehat{\beta_{\mathrm{PtE}}} > 0$ indicates the presence of electoral sentencing cycles, and $\widehat{\beta_{\mathrm{DI}}} > 0$ would be evidence for the *direct-influence* hypothesis.

Expression (5) nests specifications where we check the two mechanisms. In columns I–V of Table 2 we test for the cost-savings mechanisms by omitting $\beta_{PtE} \cdot \mathit{PtE}_{jt} + \beta_{DI} \cdot \mathit{PtE}_{jt} \cdot \mathit{PrivateC}_{st}$. In columns I–III we use the CBCP sample. Column I is the same as column XI in Panel A of Table 1, except the outcome is in levels rather than logs.[24] Columns II interacts private prisons with the continuous cost-savings measure in expression (3), Column III instead does a binary split that defines states with greater than five percent savings as 'high-savings' and those with less as low-savings states. Both specifications suggest that the effect of private prison capacity changes is most pronounced in states where the cost savings from private prisons are high. (We also confirm this when we break the event-study analysis into high-savings and low-savings states in Online Appendix Figure 8.) Columns IV–V re-estimate II–III for the full sample.

The second half of the table compares this to the evidence for judicial capture. Only the nine states with judge identifiers in the data can be included. Further, only states where there is at

[23] T_s is the length of state s's electoral cycle, i.e., $T_{WA} = 4 \times 365$ in Washington where elections are every 4 years, and $T_{NC} = 8 \times 365$ in North Carolina where elections are every 8 years.

[24] The outcomes are in levels here because the interactions that are the regressors of interest are in levels so that the objects of interest are not elasticities.

least some evidence for the electoral sentencing cycles can be included.[25] This omits Colorado, Kentucky, Minnesota, and Virginia. Column VI confirms our baseline effect in this new full sample of five states. Column VII confirms the presence of electoral sentencing cycles, i.e., $\widehat{\beta_{\text{PtE}}} > 0$, in this sample. Column VIII tests for judicial capture: there is no evidence at all that sentencing cycles are more pronounced in the presence of private prisons. In combination, the evidence in columns II-III and VIII supports the cost savings story but not the judicial capture story. Because these are estimated in different samples, we introduce cost savings into the electoral cycle sample in columns IX–X. The cost-savings coefficients continue to show up significantly, while the judicial capture coefficient continues not to.[26] While this evidence on mechanisms is coarse, the observed patterns are surprisingly robust in their support of the cost savings explanation for the effect of private prisons on sentencing.

4 Conclusion

We study whether private prisons impact judges' criminal sentencing decisions. We find that the opening of a private prison has large effect on sentence lengths in the prison's state, but only during the first two months after opening. Public prison openings have no such effects. This suggests that private prisons have short-run effects on the application of justice that public ones do not. This finding has considerable implications for judicial equity: defendants who happen to stand trial shortly after a private prison opens can expect to be sentenced to three additional months relative to otherwise identical defendants who stand trial for the same crime a few weeks earlier or later. The equity implications of this effect are further accentuated by other research showing that these defendants may also serve a larger portion of their sentences, with lower-quality rehabilitative care. The evidence on mechanisms is overall most consistent with the hypothesis that judges respond to private prisons by levying longer sentences because they consider the lower per-day per-bed fiscal cost of incarceration in their sentencing decision.

[25] The presence of the electoral sentencing cycles varies considerably across states (Dippel and Poyker, 2019).

[26] Online Appendix Table 10 shows that these insights are unchanged when we omit probations from the data. Online Appendix Table 11 shows additional robustness checks to variations in the fixed effects reported at the bottom of the tables.

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Online Appendix

to

"Do Private Prisons Affect Criminal Sentencing?"

Online Appendix A Data Description

Online Appendix A.1 Prison Data

Prison-year panel dataset was constructed combining several sources. Below we provide the description of the process of its creation.

First, we access Census of State and Federal Adult Correctional Facilities for the years of 2005, 2000, 1995, and 1990.[27] From these cross-section of the universe of US correctional facilities we construct a panel with three years only. In this panel we observe the capacity of each prison and the year when each penitentiary is founded, and if the prison is publicly or privately managed. As we study state prison system we omit all federal prisons from the dataset.

Second, we create observations for each prison for each year between 1990 and 1995, between 1995 and 2000, and between 2000 and 2005. By doing this we assign prison capacity values of 1990 for all years 1991 to 1994, assign prison capacity values of 1995 for years 1996 to 1999 and so on. Then we prolong our panel to December 2016, and assign prison capacity of the 2005 for all years starting with 2006.

Such approach has its drawbacks, and one of the most important is that prisons can disappear or appear between the years when censi data was collected. However, if a new prison appears e.g. in 1995 census but is not present in 1990 census we can see the year when it was opened and correct the dataset. But if the prison was in 1990 census but disappear in 1995 census we do not know exactly when it was closed. There are few cases when prison was closed and we manually checked the dates when they were closed and augmented the dataset.[28]

Third, as we can not observe if new prisons were opened after 2005, we use states' Department of Correction sites to add new prisons in the dataset.

Forth, as our main treatment comes from the private prisons, we treated the subsample of private prisons specially. In particular, we studied sites of all the private prison companies and collected yearly prison capacity data for 1990-2016.[29] In addition, if prison was privatized it may appear as public e.g. in 2000 and remain public until 2005 in our data even if it was privatized at 2001. Thus by walking through all US private prisons one-by-one we adjust the dummies for being private prison in our dataset. Similarly we check if private prison switched from hosting federal (state) to hosting state (federal) prisoners in between the prison censuses.

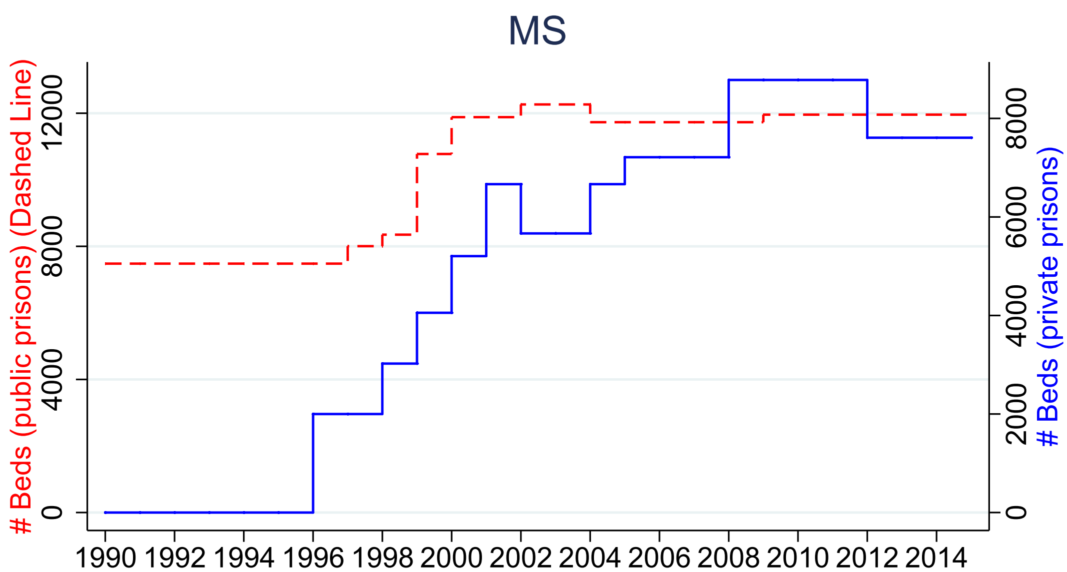
Finally, we geo-locate latitude and longitude data of each prison location using the Google Maps API.

[27] These datasets are publicly available at ICPSR. Their codes are 24642.

[28] In particular we used dataset of prisons available at ENIGMA (https://app.enigma.io/table/enigma.prisons.all-facilities?row=0&col=0&page=1). That cross-sectional dataset contains data about all ever existing correctional facilities in the US. While it does not contain the year when the prison was founded it contains the year when it was closed and we used it to find closed prisons.

[29] In comparison with public prison that have prison capacity variable change only at 1990, 1995, 2000, 2005 or later (if opened after 2005).

Figure Online Appendix Figure 1: Private and Public Prison Capacity in Tennessee and Mississippi



Notes: This figure is equivalent to Figure 1's visual for Tennessee. The dashed (red) line is the state-specific time-series of public prison capacity (number of beds). The solid (blue) line is the state-specific time-series of private prison capacity (number of beds). The time range in each state is determined by sentencing data availability. Mississippi has private prison openings in 1996, 1998, 1999, 2000, 2001, 2004, and 2008, and closings in 2002 and 2013. Our figure for Mississippi is very similar to Figure 1 in Mukherjee (2019).

Online Appendix A.2 Sentencing Data

Sentencing data was collected separately from each state. 15 states were willing to share their data with us for free or at reasonable cost: Alabama, Arkansas, Colorado, Georgia, Kentucky, Maryland, Minnesota, Mississippi, Nevada, North Carolina, Oregon, Tennessee, Texas, Virginia, and Washington. Obtaining these data from each state separately took several years and the process was quite idiosyncratic.[30]

We contacted each state with the following initial data request:
The data we are looking for has a court case (or 'sentencing event') as the unit of observation. In some states the data is organized by charge (with several charges making up the case or sentencing event) and that is equally fine. The key data that we need are:
1. date, month and year of sentencing for
2. type of crime,
3. length of sentencing,
4. type of sentencing (low-security, high security, etc),
5. defendant's sex,
6. defendant's race,
7. court identifier
8. name of judge or judge identifier number,
9. type of court that convicted (trial, appeal, etc),

[30] The only state with digitized sentencing data not included in our analysis is Kansas, which charged five times more than other states for our data processing request leading us to echo Frank's 2007 question.

10. in what prison the person was sent
We do not seek any information that identifies defendants.
Sincerely, XXX

The following reports for each state the office responsible for storing the data, as well as relevant contact emails and numbers at the time we requested the data between late 2016 and mid 2018. Longer processing times were typically do either to backlogs of data-technicians or to having to go get our request vetted and signed off on in the institutions that manage the data.

1. Alabama
   - Initial contact with the Sentencing Commission at http://sentencingcommission.alacourt.gov/
   - After emailing sentencing.commission@alacourt.gov, Bennet Wright processed our request.
   - Time between data application and delivery: 16 months.
2. Arkansas
   - Initial contact with the Sentencing Commission at https://www.arsentencing.com/
   - Were referred the Administrative Offices of the Courts. Their email was ORJShelp@arcourts.gov and Joe Beard processed our data request.
   - Time between data application and delivery: 4 months.
3. Georgia
   - Initial contact with Department of Corrections at http://www.dcor.state.ga.us/Divisions/ExecutiveOperations/OPS/OpenRecords.
   - After emailing open.records@gdc.ga.gov it was recommended we go through their 'Media Inquiries' under +1-478-992-5247, where Jamila Coleman coordinated our request with their data technicians.
   - Time between data application and delivery: 3 months.
4. Kentucky
   - We spoke on the phone to Cathy Schiflett at the Kentucky Courts Research and Statistics Department.
   - She guided us to https://courts.ky.gov/Pages/default.aspx, where we had to select 'Statistical Reports' and then submit our data request.
   - Daniel Sturtevant handled our request.
   - Time between data application and delivery: 9 months.
5. Maryland
   - After initial contact though http://www.courts.state.md.us/reference/piarequests.html, we submitted our request to the Maryland State Commission on Criminal Sentencing Policy, at http://www.msccsp.org/Default.aspx

- Our request was processed by Lou Gieszl, Assistant Administrator for Programs at the Administrative Office of the Courts
- Time between data application and delivery: 1 month Unlike most states, Maryland's data was 'off-the-shelf' available as the MSCCSP (Maryland State Commission on Criminal Sentencing Policy) dataset

6. Minnesota
   - Initial contact with the Minnesota Sentencing Guidelines Commission at http://mn.gov/sentencing-guidelines/contact/contact-us.jsp
     Email address: sentencing.guidelines@state.mn.us
   - Kathleen Madland was the Research Analyst who processed our request
   - Time between data application and delivery: 2 months

7. Mississippi
   - Initial contact with the Mississippi Department of Corrections at https://www.ms.gov/mdoc/inmate
   - Audrey MacAfee and Lynn Mullen processed our request
   - Time between data application and delivery: 2 months We use essentially the same data as Mukherjee (2019)

8. Nevada
   - After initial contact with the Nevada Department of Corrections at http://doc.nv.gov/Inmates/Records_and_Information/Public_Record_Fees/, with email pio@doc.nv.gov, our request was handled by Brooke Keast, Public Information Officer
   - We were provided with the codebook and scraped the raw data from the Nevada's DOC site on 7th of July 2016: http://167.154.2.76/inmatesearch/form.php

9. North Carolina
   - Initial contact though http://www.ncdoj.gov/Top-Issues/Public-Integrity/Open-Government/Understanding-Public-Records.aspx
   - Then we were put in touch with the North Carolina Administrative Office of the Courts, where our data request was processed by the 'Remote Public Access' data technicians
   - Time between data application and delivery: 3 months

10. Oregon
    - In Oregon, sentencing data is handled by the Criminal Justice Commission's Statistical Analysis Center at https://www.oregon.gov/cjc/SAC/Pages/CurrentProjects.aspx
    - Kelly Officer processed our request
    - Time between data application and delivery: 1 month

11. Tennessee

- Initial contact with Tennessee's Department of Corrections at https://www.tn.gov/correction/article/tdoc-prison-directory
- Tanya Washington, the DOC's Director of Decision Support: Research & Planning, processed our request
- Time between data application and delivery: 6 months

12. Texas

- Downloaded data online on 4th of November 2016 : https://www.tdcj.state.tx.us/kss_inside.html

13. Virginia

- Initial contact was through a web-form of the Virginia Criminal Sentencing Commission at http://www.vcsc.virginia.gov/
- After being initially denied on the grounds that FOIA requests could only be processed for Virginia residents, we called +1-804-225-4398, and were eventually approved after speaking to the director Meredith Farrar-Owens.
- Time between data application and delivery: 3 months

14. Washington

- Initial contact with the Department of Corrections at http://www.doc.wa.gov/aboutdoc/publicdisclosure.asp, where Duc Luu processed our request
- We use essentially the same data as Berdejó and Yuchtman (2013)
- Time between data application and delivery: 2 weeks

Online Appendix A.3 Prison Costs Data

We collect information on savings from using convict labor from the multiple sources. First, the costs of state public prisons we use data from Vera Institute of Justice.[31]

For private prisons we use state legislation in cast there is a mandatory requirements on the savings: Kentucky (10%),[32] Mississippi (10%),[33] Tennessee (5%),[34] and Texas (10%).[35] We also use data from Hakim and Blackstone (2013) and state reports and news articles to find the rest of the information: Alabama (22%), Arkansas (48%), Georgia (16%), Nevada (55%), Virginia (1%), and Washington (0%).[36] The remaining states have no state private prisons, and thus no legislated cost savings.

[31] www.vera.org/publications/price-of-prisons-2015-state-spending-trends/price-of-prisons-2015-state-spending-trends/price-of-prisons-2015-state-spending-trends-prison-spending

[32] See KY. REV. STAT. ANN. 197.510(13) (West 2007)

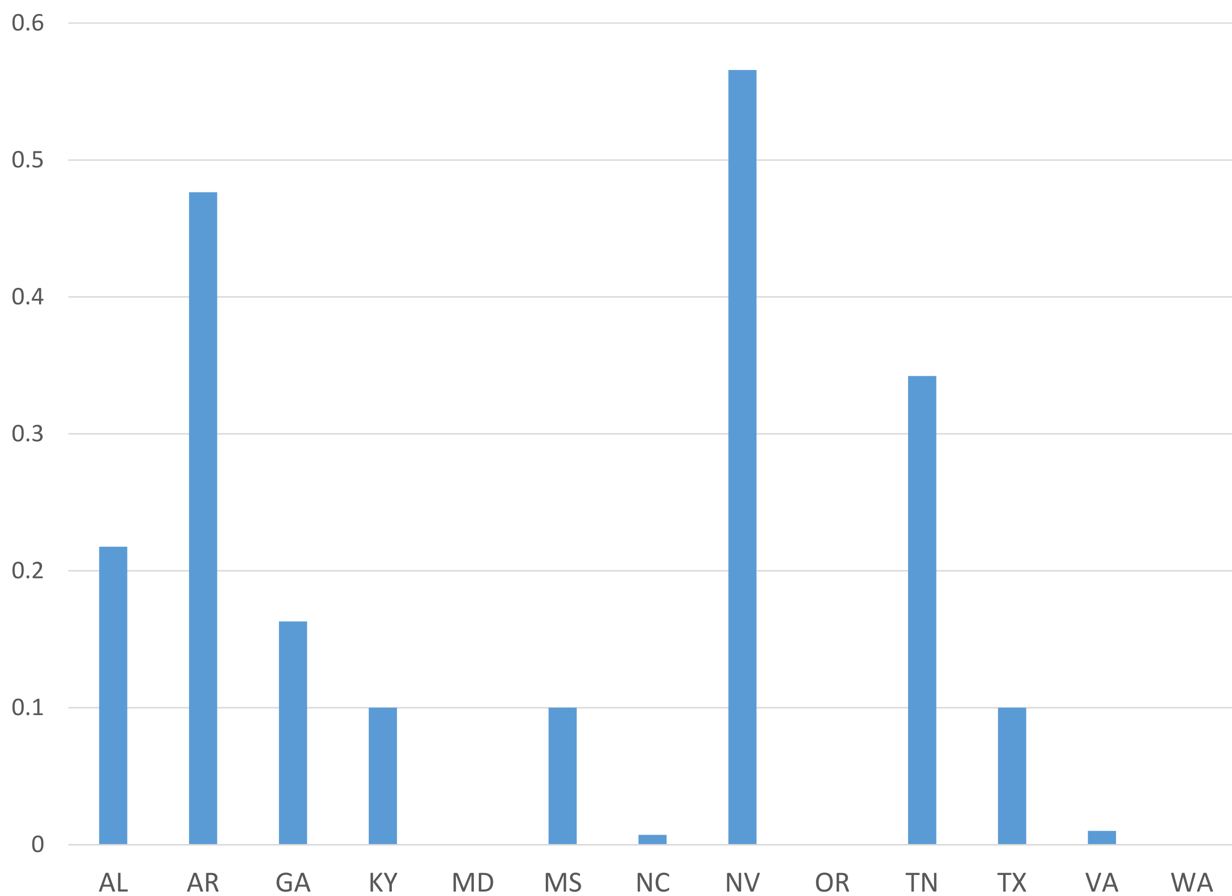
[33] MISS. CODE ANN. 47-5-1211(3)(a) (West 2012)

[34] TENN. CODE ANN. 41-24-104(c)(2)(B), 41-24-105(c) (West 2014)

[35] TEX. GOVT CODE 495.003(c)(4) (West 2013)

[36] For Alabama see http://www.doc.state.al.us/docs/AnnualRpts/2016AnnualReport.pdf, for Arkansas see www.arktimes.com/arkansas/the-private-prison-swamp/Content?oid=23890398, for Georgia, see www.savannahnow.com/column/opinion/2017-11-23/robert-pawlicki-private-prisons-are-bad-deal-georgians, for Nevada see https://thenevadaindependent.com/article/lawmakers-try-again-to-ban-nevadas-use-of-private-prisons-say-companies-focused-on-profit-not-rehabilitation for Virginia see www.tkevinwilsonlawyer.com/library/virginia-private-prisons.cfm, and for Washington see www.thenewstribune.com/news/special-reports/article25860412.html.

Figure Online Appendix Figure 2: A histogram of $Saving_s$



Notes: This figure displays a histogram of $Saving_s$.

Thus we compute $Saving_s = 1 - \frac{\text{Cost in private prison}_s}{\text{Cost in public prison}_s}$. If private prison costs are the same as public prison costs, then $Saving_s = 0$. We assign the value of zero for the states where there is no private prisons. Online Appendix Figure 2 shows the data.

Online Appendix B Additional Results

Online Appendix Table 1 provides a detailed breakdown of the number of pairs on each state-border segment. This table clarifies how many border-segments are linked to each state. Tennessee, for example, is the most 'connected' state in our data, sharing border-segments with seven states (segments # 5, 6, 7, 10, 11, 15, 16). Online Appendix Table 1 also clarifies how many years of sentencing data are used for each border segment: The years covered by each state's sentencing data vary somewhat, ranging from 1980 to 2017, and the years included in a border-pair are determined by the state with less sentencing data coverage. For example, Tennessee's data sentencing data goes back to the 1980s. There are 26 years of data in its border-pairs with Mississippi (segment #5), whose data also go back to 1990. By contrast, there are only 6 years of data in its border-pairs with Georgia (segment #7), whose sentencing data only goes back to 2010.

The years included in a county-pair are determined by the state with less sentencing data coverage. For example, there are 26 years of data in Tennessee's border-segment with Mississippi, because Mississippi's data cover 1990–2016 and Tennessee's cover 1980–2016.

Table Online Appendix Table 1: Contiguous-Border County-Pairs

	Pairs		#counties			Sentencing overlap		
Segment	1	2	1	2	#pairs	y-start	y-end	#years
1	OR	WA	10	11	20	2004	2015	11
2	OR	NV	3	2	4	2004	2015	11
3	AR	MS	5	6	10	1990	2016	26
4	AL	MS	10	12	21	2002	2016	14
5	TN	MS	5	6	10	1990	2016	26
6	AR	TN	2	4	6	1974	2017	43
7	TN	GA	4	6	9	2010	2016	6
8	NC	GA	4	4	7	2010	2016	6
9	AL	GA	11	17	27	2010	2016	6
10	TN	NC	9	10	18	2006	2016	10
11	TN	VA	5	5	9	2007	2016	9
12	MD	VA	8	10	17	2007	2016	9
13	NC	VA	15	14	28	2007	2016	9
14	KY	VA	4	4	7	2007	2016	9
15	AL	TN	4	7	10	2002	2016	14
16	TN	KY	14	17	30	2002	2017	15
17	AR	TX	2	2	4	2010	2016	6
Total	13				237			

Notes: This table decomposes the sample of 237 border-counties into 17 state-border segments. The table clarifies how many border-segments are linked to each state, and which segments are dropped when a state is dropped from the analysis, as the robustness check reported in Figure 2 will do. The table also clarifies what years of sentencing data are used in each border segment, where the constraint on each segment is the state with less available sentencing data.

Table Online Appendix Table 2: Border-County Balance Table

	I		II		III		IV	
	All-County Sample		Contiguous Border County-Pair Sample		Differences (Between Full and CBCP Sample)		Differences (Between County-Pairs)	
	Mean	s.d.	Mean	s.d.	Mean	P-value	Mean	P-value
County Controls:								
Population, 2000	67,056.2	(178549)	65,218.3	(131987)	-1,837.9	[0.870]	-26,322.1	[0.315]
Population density, 2000	198.91	(604)	208.62	(756)	9.71	[0.792]	-42.86	[0.335]
Land area (square miles)	774.83	(1,186)	749.68	(1,292)	-25.156	[0.832]	192.246	[0.374]
Manufacturing employment	4,968.6	(11,852)	4,327.3	(6,354)	-641.2	[0.362]	-629.2	[0.628]
Maufacturing average weekly earnings ($)	592.04	(235)	577.13	(166)	-14.91	[0.410]	-17.74	[0.508]
Restaurant employment	3,363.3	(7,963)	2,795.6	(5,005)	-567.7	[0.347]	-1,636.7	[0.247]
Restaurant average weekly earnings ($)	187.56	(33)	186.94	(40)	-0.624	[0.874]	-8.589	[0.358]
Sentence and Defendant Data:								
Average sentence length in months, men	47.47	(135.4)	44.83	(116.9)	-2.644	[0.671]	2.398	[0.475]
---, men conditional on incarceration	60.06	(131.9)	62.33	(152.2)	2.268	[0.698]	0.772	[0.816]
Share of sentences for men	0.831	(0.375)	0.844	(0.363)	0.013	[0.130]	0.001	[0.909]
Share of Black defendants	0.336	(0.472)	0.342	(0.474)	0.005	[0.914]	-0.002	[0.938]
Share of Hispanic defendants	0.041	(0.197)	0.031	(0.173)	-0.010	[0.464]	0.002	[0.396]

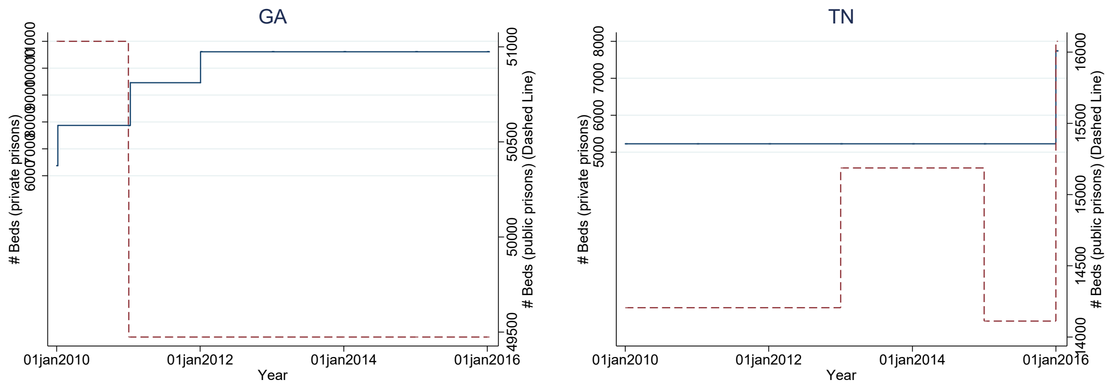
Notes: This table shows that the border-county sample is representative of the full sample of counties in the 13 states we peruse. The top-panel reports on county-characteristics. The bottom-panel reports on sentencing data.

Online Appendix Table 2 shows that the socio-economic characteristics of border-court counties are not statistically different from all other counties in the same states. Column-set I reports on border-court counties Column-set II reports the same for all other counties in the 13 states. Reassuringly, column-set III confirms that border counties are representative of counties in their states more broadly, the difference between the two samples is never near conventional significant levels. In the bottom panel, we verify the same holds true for sentencing and defendant characteristics, including defendants' race and our main dependent variable, the length of a sentence.[37] The two most important characteristics of a court case are the crime' severity and the defendant's recidivism. Because each state classifies these two variables into unique discrete scales, we cannot report descriptive statistics on these.

Column-set IV reports differences between cross-border contiguous counties. It shows that *within* such pairs, socio-economic characteristics of border-court do not significantly vary between counties.

[37] Sentences of length zero represent cases that the defendant was found not guilty, or sentenced to non-prison conditions (e.g., fines, probation, or community services). In the case of consecutive sentences, we summed all sentencing within each case. In the case of concurrent sentencing, we took the maximum.

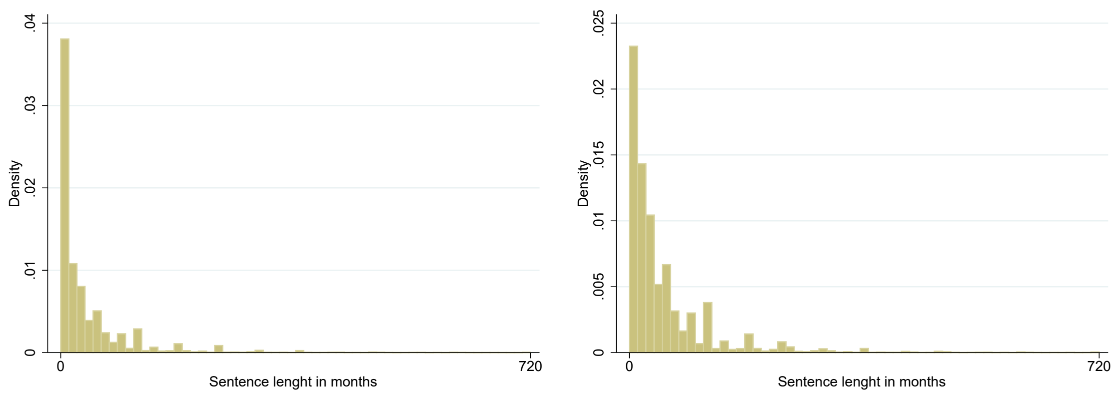
Figure Online Appendix Figure 3: Illustrating the Border-Sample Identifying Variation



Notes: This figure shows the time variation in $PrivateC_{ct}$ and $PublicC_{ct}$ for two neighboring states (Georgia and Tennessee, which together form segment #7 in Table Online Appendix Table 2) over the same time horizon. The dashed (red) line is the state-specific time-series of public prison capacity (number of beds). The solid (blue) line is the state-specific time-series of private prison capacity (number of beds).

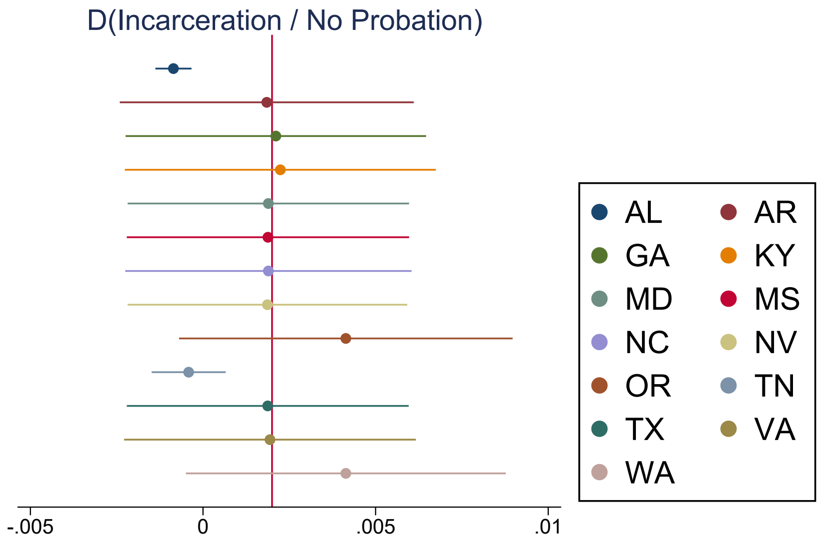
Online Appendix Figure 3 illustrates the identifying variation across the border-segment that connects Georgia to Tennessee. This is segment #7 in Online Appendix Table 1. Tennessee has considerable variation in $PrivateC_{ct}$, as can be seen in Figure 1. However, sentencing data from Georgia only goes back to 2010, and within the 2010 to 2016 time frame, Tennessee displays no variation in $PrivateC_{ct}$. The effect of $PrivateC_{ct}$ on courts/counties along border segment #7 is therefore estimated by comparing the expansion in Georgia's private prison capacity in 2011 and again in 2012 relative to a constant capacity in Tennessee.

Figure Online Appendix Figure 4: Distribution of Sentences, with and without Probation)



Notes: The left panel shows the distribution of sentence length with probation, the right panel without. We truncate the sentence length at 720 months.

Figure Online Appendix Figure 5: Robustness of the Results in Panel C of Table 1



Notes: This figure reports on the point-estimate and 90th-percent confidence band that results when re-estimating the specification in Column VI of Table 1, dropping one state at a time. The (red) vertical line is the baseline point estimate. The results are sorted top-to-bottom in alphabetical order, i.e., omit AL, then AR, then GA, etc.

Online Appendix Figure 5 is the equivalent of Figure 2. but for only the *extensive* margin effect on the probability of going to prison. Online Appendix Figure 5 explains the pattern observed in the left panel of Figure 2.

Panel A of Online Appendix Table 3 reports on a placebo test that ensures our results are not driven by confounding factors. Namely, we re-estimate this baseline effect for female defendants because private prisons in our data are almost entirely male only. Indeed, we find no effect of expanding male private prisons on female sentencing length, making it unlikely that there is an unobservable confounding trend that correlates with *PrivateC*$_{st}$ and that makes sentences harsher across the board.

Panel B of Online Appendix Table 3 shifts the time-period of the treatment. With state-year fixed effects μ_{st} included, identification of our baseline estimates in Table 1 comes from within-state within-year variation. To check that this variation truly estimates the effect of changes in *PrivateC*$_{st}$, rather than within-year trends, we shift *PrivateC*$_{st}$ to month $t + 3$, $t + 6$, and $t + 9$, and to month $t - 3$, $t - 6$, and $t - 9$, always evaluated relative to a state-specific year fixed effect.

None of the resulting estimates has a significant coefficient, which indicates that the effects are quite concentrated around the capacity changes. This is what we further investigate in the event study analysis.

Table Online Appendix Table 3: Placebo Specifications

	I	II	III	IV	V	VI
Panel A: female defendants	Dependent variable: Sentence (log months)					
Log private prison capacity	0.004	0.003	0.004	0.009	0.012	0.011
	[0.7706]	[0.8111]	[0.7898]	[0.4127]	[0.1947]	[0.1621]
Log public prison capacity	1.135	1.051	0.837	1.032	1.120	0.930
	[0.3023]	[0.3330]	[0.3095]	[0.1824]	[0.1815]	[0.3463]
R-squared	0.496	0.507	0.534	0.566	0.566	0.574
Observations	141,980	141,980	141,980	141,980	141,980	141,980
Log public prison capacity	✓	✓	✓	✓	✓	✓
FEs: state x fiscal-year	✓	✓	✓	✓	✓	✓
Demographic controls		✓	✓	✓	✓	✓
Case controls			✓	✓	✓	✓
FEs: state x year	✓	✓	✓			
FEs: court-borderpair	✓	✓	✓			
FEs: court-borderpair x year				✓	✓	✓
Linear trend: state x calend.-month					✓	✓
FEs: judge						✓
	Lag, t+3	Lag, t+6	Lag, t+9	Lead, t+3	Lead, t+6	Lead, t+9
Panel B: lags & leads						
Log private prison capacity	0.004	-0.008	0.003	0.005	-0.013	0.005
[t-specific]	[0.5584]	[0.2632]	[0.7237]	[0.1651]	[0.3915]	[0.5277]
Log public prison capacity	0.011	-0.070	-0.168	0.017	0.274	-0.051
[t-specific]	[0.9308]	[0.8257]	[0.1599]	[0.4003]	[0.4630]	[0.2308]
R-squared	0.476	0.476	0.476	0.476	0.476	0.476
Observations	765,338	765,338	765,338	765,338	765,338	765,338

Notes: Panel A of this Table replicates Panel A of Table 1 but uses only the sample of female defendants. In Panel B all columns, we take the most demanding specification from the baseline results, i.e., Column VI of Panel A in Table 1. In Columns I–III of Panel B, instead of private and public prison capacities at year-month t we use corresponding variables at year-month $t-3$, $t-6$, and $t-9$. In Columns IV–VI of Panel B, instead of private and public prison capacities at year-month t we use corresponding variables at year-month $t+3$, $t+6$, and $t+9$. In square brackets we report p-values for standard errors are clustered on state and border segment; *** p<0.01, ** p<0.05, * p<0.1

Table Online Appendix Table 4: Prison Capacities Weighted by Distance-to-prison

	I	II	III	IV	V	VI
Panel A: baseline	Dependent variable: Sentence (log months)					
Log Σ_p(private prison capacity$_p$ / distance$_{cp}$)	0.008	0.009*	0.010**	0.006*	0.006*	0.005
	[0.1295]	[0.0827]	[0.0453]	[0.0857]	[0.0887]	[0.1355]
R-squared	0.366	0.380	0.435	0.446	0.446	0.450
Observations	3,544,144	3,544,144	3,544,144	3,544,144	3,544,144	3,544,144
Panel B: exclude probation						
Log Σ_p(private prison capacity$_p$ / distance$_{cp}$)	0.002	0.002	0.003**	0.002*	0.002*	0.002
	[0.4499]	[0.3917]	[0.0261]	[0.0855]	[0.0768]	[0.1347]
R-squared	0.382	0.389	0.502	0.515	0.515	0.522
Observations	2,699,237	2,699,237	2,688,027	2,688,033	2,687,664	2,684,438
Log public prison capacity	✓	✓	✓	✓	✓	✓
FEs: state x fiscal-year	✓	✓	✓	✓	✓	✓
Demographic controls		✓	✓	✓	✓	✓
Case controls			✓	✓	✓	✓
FEs: state x year	✓	✓	✓			
FEs: court-borderpair	✓	✓	✓			
FEs: court-borderpair x year				✓	✓	✓
Linear trend: state x calendar-month					✓	✓
FEs: judge						✓

Notes: This table replicates Panels D and E of Table 1, but uses prison capacities weighted by the distance from a defendant's court to each prison. In square brackets we report p-values for standard errors are clustered on state and county level.*** p<0.01, ** p<0.05, * p<0.1

In Section 2.3 we argue that the *location* of prisons is clearly endogenous to local factors that could also impact local crime and therefore sentencing. This implies that using a court's distance to a private prison as a source of variation is endogenous, despite the intuitive appeal of this margin of variation. For completeness, Online Appendix Table 4 reports on regressions using this endogenous distance-based measure.

Endogeneity concerns aside, conversations with officials in several states revealed that proximity seems to be anyway largely irrelevant in determining which prison a defendant is sent to. Prisons are different from jails in this respect: jails are run by the county, and defendants awaiting trial do so in the jail of the trial court's county.

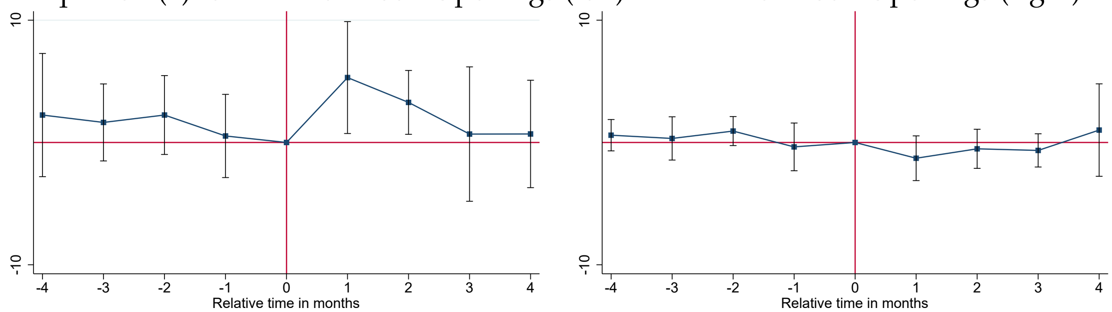
Table Online Appendix Table 5: Event-Study Coefficients for Figure 3

	I	II	III	IV
	Dependent variable: Sentence (months)			
	Openning of private prison	Closing of private prison	Openning of public prison	Closing of public prison
4 months before event	1.330	5.676**	0.330	0.511
	[0.5709]	[0.0249]	[0.5228]	[0.3242]
3 months before event	1.428	4.142***	0.184	-0.102
	[0.4135]	[0.0041]	[0.7855]	[0.8509]
2 months before event	1.614	3.711**	0.525	0.138
	[0.2692]	[0.0104]	[0.2452]	[0.8908]
1 month before event	0.212	2.700**	-0.403	-1.273
	[0.9007]	[0.0354]	[0.5756]	[0.2398]
1 month after event	3.666*	1.844	-0.788	-0.180
	[0.0837]	[0.2471]	[0.1956]	[0.7494]
2 months after event	2.985**	2.009	0.110	-0.307
	[0.0394]	[0.3446]	[0.8537]	[0.6375]
3 months after event	0.712	-2.422	-0.363	-0.138
	[0.7299]	[0.2097]	[0.4757]	[0.8798]
4 months after event	1.101	-1.889	0.612	-1.000
	[0.5206]	[0.3032]	[0.6400]	[0.5287]
R-squared	0.107	0.107	0.029	0.164
Observations	163,372	155,354	434,725	353,513

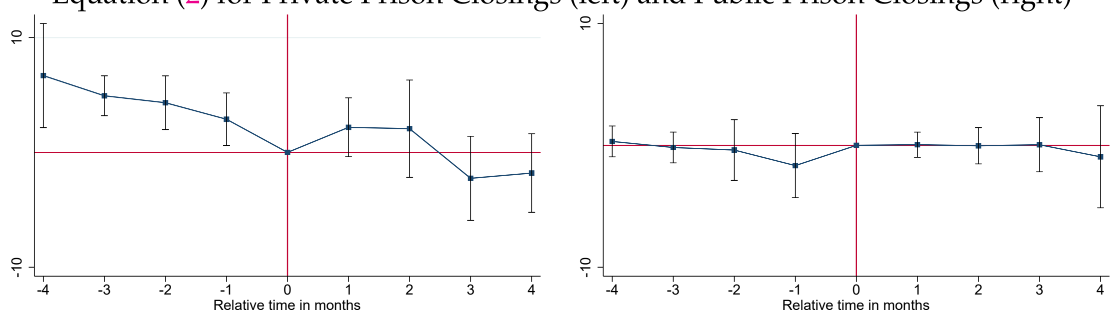
Notes: This Table estimates event-study specification 2. Column I reports results for the opening-of-private-prison events. In Column II, the events are closing of private prisons. Columns III and IV look at opening and closing of public prisons, respectively. In square brackets we report p-values for standard errors are clustered on state and event level; *** p<0.01, ** p<0.05, * p<0.1

Figure Online Appendix Figure 6: Re-Estimating Figure 3 with Probations Excluded

Equation (2) for Private Prison Openings (left) and Public Prison Openings (right)



Equation (2) for Private Prison Closings (left) and Public Prison Closings (right)



Notes: Panels A and B graph the results of estimating equation (2) for private and public prison openings and closings without cases that received probation. The results are reported in Online Appendix Table 6.

Table Online Appendix Table 6: Event-Study Coefficients for Online Appendix Figure 6 with Probations Excluded

	I	II	III	IV
	Dependent variable: Sentence (months)			
	Openning of private prison	Closing of private prison	Openning of public prison	Closing of public prison
4 months before event	2.244	6.688**	0.596	0.321
	[0.4354]	[0.0221]	[0.3536]	[0.6024]
3 months before event	1.640	4.929***	0.331	-0.174
	[0.3648]	[0.0003]	[0.7075]	[0.7761]
2 months before event	2.245	4.326***	0.934	-0.385
	[0.2340]	[0.0063]	[0.1193]	[0.7497]
1 month before event	0.532	2.887**	-0.360	-1.660
	[0.7807]	[0.0439]	[0.7113]	[0.2031]
1 month after event	5.310*	2.182	-1.289	0.057
	[0.0623]	[0.1552]	[0.1620]	[0.9092]
2 months after event	3.278**	2.067	-0.519	-0.035
	[0.0470]	[0.4013]	[0.5157]	[0.9616]
3 months after event	0.689	-2.256	-0.649	0.050
	[0.8235]	[0.2944]	[0.3399]	[0.9626]
4 months after event	0.697	-1.800	1.014	-0.940
	[0.7777]	[0.3662]	[0.5906]	[0.6438]
R-squared	0.113	0.094	0.028	0.181
Observations	103,572	130,337	299,338	274,357

Notes: This Table estimates event-study specification 2 without cases that received probation. Column I reports results for the opening-of-private-prison events. In Column II, the events are closing of private prisons. Columns III and IV look at opening and closing of public prisons, respectively. In square brackets we report p-values for standard errors are clustered on state and event level; *** $p<0.01$, ** $p<0.05$, * $p<0.1$

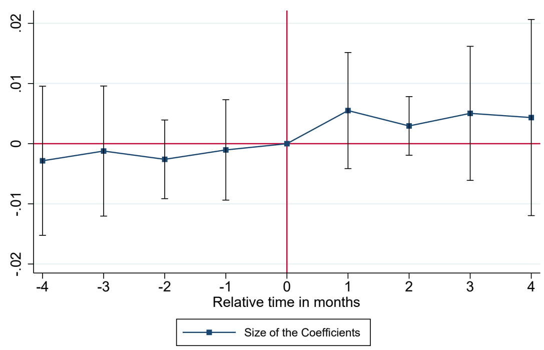
Table Online Appendix Table 7: Full Sample 2-months Dummy

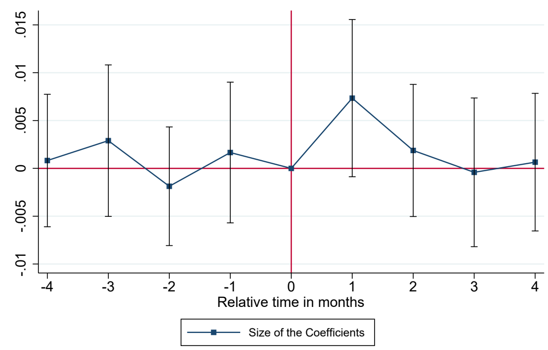
	I	II	III	IV	V	VI
	Dependent variable: Sentence (log months)					
D(Private prison opens within 2 months)	0.026**	0.028**	0.018***	0.022**	0.020***	0.020**
	[0.0252]	[0.0188]	[0.0097]	[0.0107]	[0.0085]	[0.0130]
R-squared	0.368	0.382	0.437	0.418	0.448	0.452
Observations	3,544,144	3,544,144	3,544,144	3,544,144	3,544,144	3,544,144
Log public prison capacity	✓	✓	✓	✓	✓	✓
FEs: state x fiscal-year	✓	✓	✓	✓	✓	✓
Demographic controls		✓	✓	✓	✓	✓
Case controls			✓	✓	✓	✓
FEs: court	✓	✓	✓			
FEs: state x year	✓	✓	✓			
FEs: court x year				✓	✓	✓
Linear trend: state x calendar-month					✓	✓
FEs: judge						✓

Notes: This Table replicates Panel D of Table 1 but instead of the log of private prison capacities as the main explanatory variable uses dummy equal to one if private prison was open within a 2-months time interval. In square brackets we report p-values for standard errors are clustered on state and county; *** p<0.01, ** p<0.05, * p<0.1

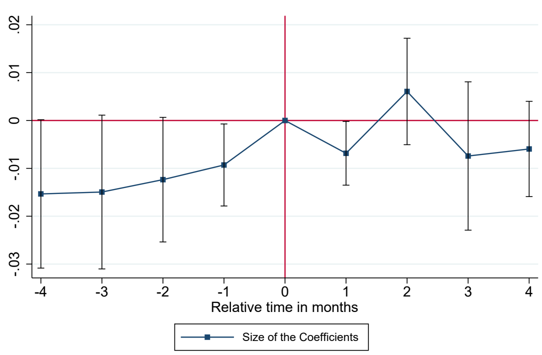
Figure Online Appendix Figure 7: Re-Estimating Figure 3 with Probability of Incarceration

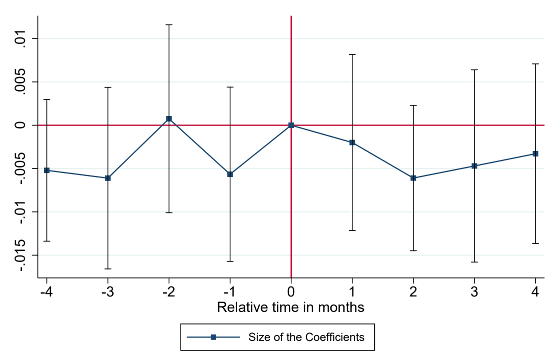
Equation (2) for Private Prison Openings (left) and Public Prison Openings (right)





Equation (2) for Private Prison Closings (left) and Public Prison Closings (right)





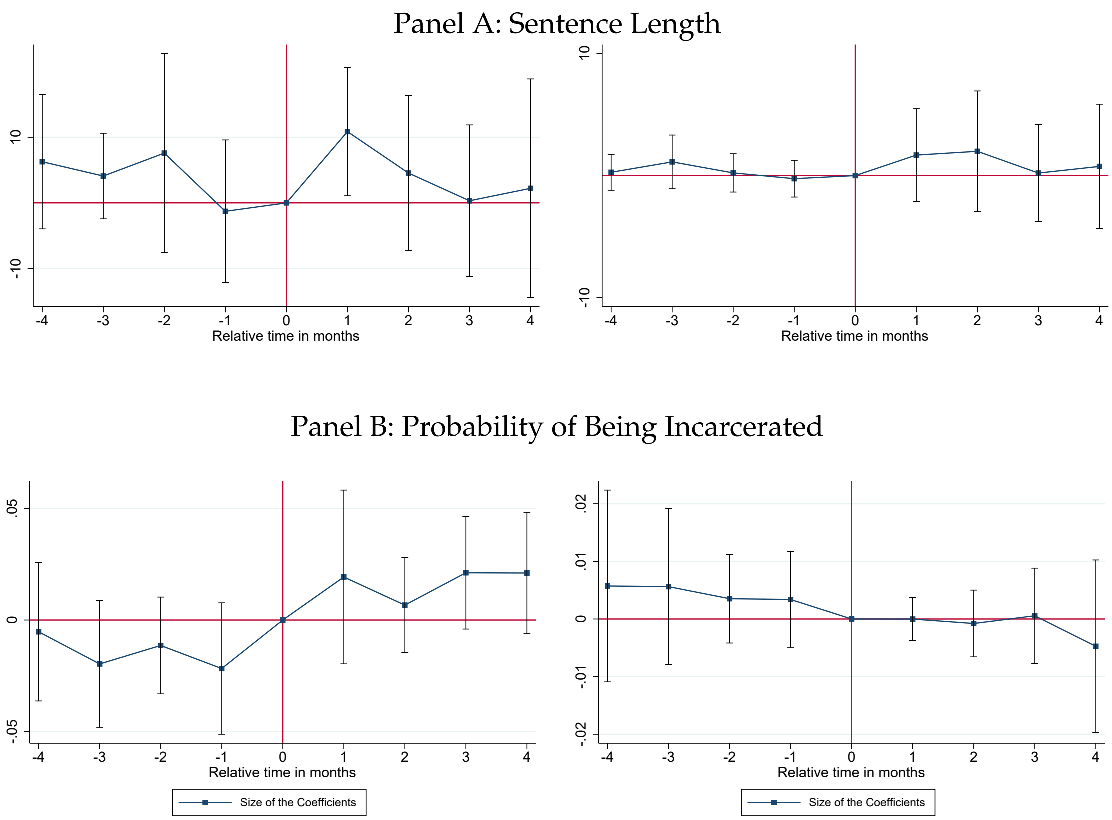
Notes: Panels A and B graph the results of estimating equation (2) for private and public prison openings and closings. The results are reported in Online Appendix Table 8.

Table Online Appendix Table 8: Event-Study Coefficients for Online Appendix Figure 6 with Probability of Incarceration

	I	II	III	IV
	Dependent variable: D(Incarceration / No Probation)			
	Opening of private prison	Closing of private prison	Opening of public prison	Closing of public prison
4 months before event	-0.003	-0.015	0.001	-0.005
	[0.6841]	[0.1032]	[0.8431]	[0.2857]
3 months before event	-0.001	-0.015	0.003	-0.006
	[0.8400]	[0.1231]	[0.5415]	[0.3276]
2 months before event	-0.003	-0.012	-0.002	0.001
	[0.4846]	[0.1163]	[0.6142]	[0.9066]
1 month before event	-0.001	-0.009*	0.002	-0.006
	[0.8246]	[0.0772]	[0.7066]	[0.3442]
1 month after event	0.006	-0.007*	0.007	-0.002
	[0.3225]	[0.0914]	[0.1404]	[0.7391]
2 months after event	0.003	0.006	0.002	-0.006
	[0.2956]	[0.3504]	[0.6505]	[0.2251]
3 months after event	0.005	-0.007	-0.000	-0.005
	[0.4283]	[0.4103]	[0.9291]	[0.4743]
4 months after event	0.004	-0.006	0.001	-0.003
	[0.6370]	[0.3077]	[0.8802]	[0.5909]
R-squared	0.285	0.482	0.282	0.338
Observations	163,372	155,354	434,725	353,513

Notes: This Table estimates event-study specification 2. Column I reports results for the opening-of-private-prison events. In Column II, the events are closing of private prisons. Columns III and IV look at opening and closing of public prisons, respectively. In square brackets we report p-values for standard errors are clustered on state and event level; *** p<0.01, ** p<0.05, * p<0.1

Figure Online Appendix Figure 8: Event-Study of Openings in High (left) vs Low (Right) Cost-Savings Settings



Notes: This graph shows the results of estimating equation (2) for private prison openings for states with high ($> 5\%$) and low ($\leq 5\%$) cost of private relative to public prisons.

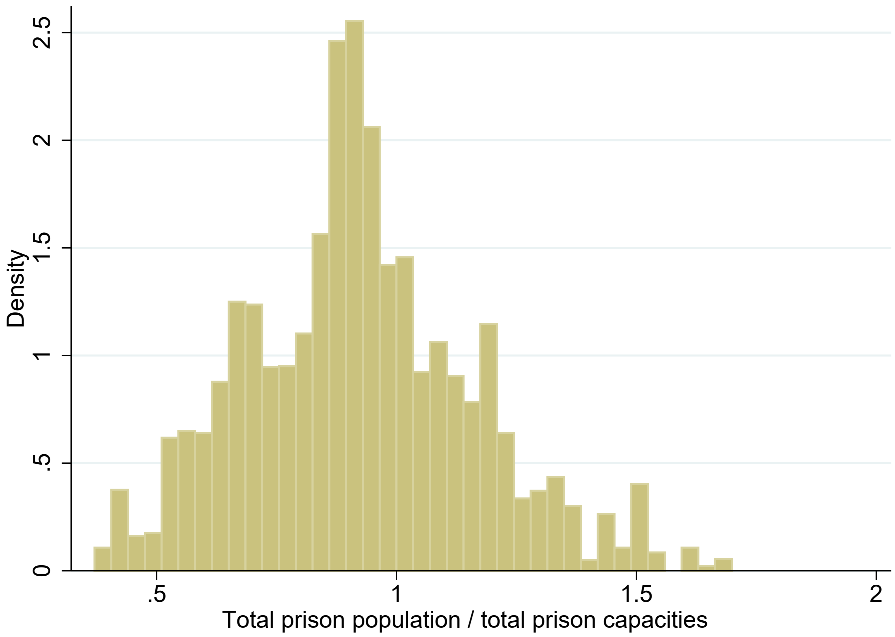
Table Online Appendix Table 9: Interactions with Prisons' Overcrowding

	I	II	III	IV	V	VI
	Dependent variable:					
	Sentence (log months)		Sentence (log months), no probations		D(Incarceration / No Probation)	
Log private prison capacity	0.036**	0.022**	0.007*	0.014***	0.006**	0.002
	[0.0125]	[0.0448]	[0.091]	[0.0001]	[0.0409]	[0.4059]
Log private prison capacity	-0.016*		0.006*		-0.005**	
x D(total overcrowding)	[0.0812]		[0.072]		[0.0106]	
D(total overcrowding)	-0.467	-0.171	0.061	-0.059	-0.172**	-0.084
	[0.1443]	[0.8006]	[0.579]	[0.6266]	[0.0149]	[0.5829]
Log private prison capacity		-0.012		0.004		-0.004
x D(overcrowding in private prisons)		[0.2197]		[0.3898]		[0.1410]
D(overcrowding in private prisons)		0.098		0.020		0.021
		[0.2428]		[0.5751]		[0.2997]
Log public prison capacity	-0.088	-0.207	-0.389	-0.331	0.036	0.000
	[0.8709]	[0.6720]	[0.120]	[0.1662]	[0.6988]	[0.9987]
Log private prison capacity	0.054*	0.015	0.010	0.006	0.019***	0.008
x D(total overcrowding)	[0.0725]	[0.7959]	[0.323]	[0.5449]	[0.0053]	[0.5772]
R-squared	0.468	0.468	0.530	0.530	0.309	0.309
Observations	765,168	765,338	570,674	570,674	765,338	765,338

Notes: This replicates Panel A of Table 1 This Table replicates Panel A of Table 1 but adds the interaction of log private prison capacities with a dummy for overcrowding in a state's prisons. Crowding is defined as a state's annual prison occupancy relative to its capacity. The distribution of this ratio is depicted in Online Appendix Figure 9. We use an indicator for over-crowing defined as D(total overcrowding) ≥ 1. Similarly, we compute overcrowding of the private prisons (D(overcrowding in private prisons)). The capacity is the same used everywhere in the data. The occupancy data is annual data from the Bureau of Justice Statistics (Carson and Mulako-Wangota 2020). In square brackets we report p-values for standard errors are clustered on state and border segment; *** p<0.01, ** p<0.05, * p<0.1

We argue in our paper that our results are not likely to be driven by capacity or over-crowding considerations. A capacity- or crowding-based explanation for the effects we find is inconsistent with the effect being concentrated in only private prisons, because there are as many new public prison openings in our data as private ones, and they are of similar magnitude. Online Appendix Table 9 makes this point clearer, by showing that prison (over-)crowding interacts weakly and (across columns) inconsistently with our baseline effect, and does not nullify the baseline effect of prison capacity.

Figure Online Appendix Figure 9: Overcrowding hist



Notes: Online Appendix Figure 9 depicts the ratio of a state's annual prison occupancy over its capacity. This ratio is used (as an indicator) in Online Appendix Table 9.

Table Online Appendix Table 10: *Cost-Savings* vs *Direct-Influence* Channels: No Probation

	I	II	III	IV	V	VI	VII	VIII	IX	X
NO Probation	Dependent variable: Sentence (months)									
Sample:	CBCP			Full		Judge sample w/o VA, CO, MN, KY				
Log private prison capacity	0.591	0.244		0.034		1.377***	1.490***	4.908**	.5081382**	
	[0.1198]	[0.3374]		[0.7069]		[0.0022]	[0.0013]	[0.0156]	[0.045]	
Log private prison capacity		3.880**		0.523					0.5073032	
x share saved		[0.0210]		[0.3156]					[0.395]	
Log private prison capacity			0.071		0.099***					0.508**
in low saving states			[0.5192]		[0.0000]					[0.0452]
Log private prison capacity			1.115***		0.154***					0.618***
in high saving states			[0.0078]		[0.0058]					[0.0001]
Proximity to election							0.219***	0.563***	4.778708**	4.779**
							[0.0018]	[0.0037]	[0.028]	[0.0281]
Proximity to election x								-0.444*	-0.4294*	-0.429*
Log private prison capacity								[0.0596]	[0.088]	[0.0875]
Δ High and Low savings, p-value			[0.0170]**		[0.2933]					[0.3963]
R-squared	0.294	0.294	0.294	0.325	0.325	0.524	0.524	0.524	0.524	0.524
Observations	570,674	570,674	570,674	2,699,237	2,699,237	671,426	671,426	671,426	671,426	671,426
Log public prison capacity	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
FEs: state x fiscal-year	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
FEs: quarter FE	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
FEs: court-borderpair	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
FEs: state x year	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
FEs: court-borderpair x year	✓	✓	✓	✓	✓	✓	✓	✓		
Demographic controls	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Case controls	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
FEs: judge	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓

Notes: This table re-estimates Table 2 without probations. In square brackets we report p-values. Standard errors are two-way clustered on state and border segment in columns I–III, two-way clustered on state and county in columns IV–V, and three-way clustered on state, calendar-year, and quarter-of-year in columns VI-X. *** p<0.01, ** p<0.05, * p<0.1

Table Online Appendix Table 11: *Cost-Savings* vs *Direct-Influence* Channels: Fixed Effect Variations

	I	II	III	IV	V	VI	VII	VIII	IX	X
	Dependent variable: Sentence (months)									
Sample:	with Probation					no Probation				
Log private prison capacity	0.798**	0.878**	2.130	0.154		0.178	0.265***	0.511**	0.356	
	[0.0433]	[0.0287]	[0.2773]	[0.5084]		[0.1147]	[0.0023]	[0.0309]	[0.2723]	
Log private prison capacity				1.399					1.291	
x share saved				[0.1542]					[0.1777]	
Log private prison capacity					0.154					0.356
in low saving states					[0.5066]					[0.2712]
Log private prison capacity					0.458**					0.636***
in high saving states					[0.0209]					[0.0021]
Proximity to election		0.209***	0.329*	1.440	1.441		1.375***	3.852	3.389	3.390
		[0.0088]	[0.0571]	[0.4708]	[0.4703]		[0.0009]	[0.1190]	[0.2204]	[0.2201]
Proximity to election x			-0.158	-0.070	-0.070			-0.314	-0.262	-0.262
Log private prison capacity			[0.4765]	[0.7513]	[0.7510]			[0.2738]	[0.4108]	[0.4104]
Δ High and Low savings, p-value					[0.6195]					[0.3343]
R-squared	0.464	0.464	0.464	0.469	0.469	0.530	0.530	0.530	0.530	0.530
Observations	804,752	804,752	804,752	804,752	804,752	671,426	671,426	671,426	671,426	671,426
Log public prison capacity	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
FEs: state x fiscal-year	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
FEs: quarter FE	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
FEs: court-borderpair	court	court	court	court	court	court	court	court	court	court
FEs: state x year	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
FEs: court-borderpair x year				✓	✓				✓	✓
Demographic controls	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Case controls	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
FEs: judge	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓

Notes: Columns I–V re-estimate columns VI–X of Table 2 with alternate fixed effects. Columns VI–X re-estimate columns VI–X of Online Appendix Table 10 with alternate fixed effects. In square brackets we report p-values. Standard errors are two-way clustered on state and border segment in columns I–III, two-way clustered on state and county in columns IV–V, and three-way clustered on state, calendar-year, and quarter-of-year in columns VI-X. *** p<0.01, ** p<0.05, * p<0.1

In Online Appendix Table 12, we check the robustness of the results to capping the reported cost savings. In the case of Nevada and Arkansas, these cost savings surprisingly large (55%, and 48%), and these are reported from local news reports (see Online Appendix A.3). In Online Appendix Table 12, we cap these cost savings at 15% (the maximum number obtained from actual data/estimates) and re-estimate the model. Reassuringly, the key patterns in the table are not affected by capping the cost-savings data in this way.

Table Online Appendix Table 12: *Cost-Savings* vs *Direct-Influence* Channels: Capped Savings

	I	II	III	IV	V	VI	VII	VIII	IX	X
Capped Savings (20%)	Dependent variable: Sentence (months)									
Sample	CBCP			Full		Judge sample w/o VA, CO, MN, KY				
Log private prison capacity	n/a	0.230		0.009		n/a	n/a	n/a	0.195	
		[0.3003]		[0.9316]					[0.4244]	
Log private prison capacity x share saved		4.312***		1.492**					1.167*	
		[0.0050]		[0.0166]					[0.0980]	
Log private prison capacity in low saving states			n/a		n/a					n/a
Log private prison capacity in high saving states			n/a		n/a					n/a
Proximity to election							n/a	n/a	1.899	
									[0.3650]	
Proximity to election x Log private prison capacity								n/a	-0.134	
									[0.5713]	
Δ High and Low savings, p-value										
R-squared		0.310		0.341					0.464	
Observations		765,338		3,544,144					804,752	
Log public prison capacity	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
FEs: state x fiscal-year	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
FEs: quarter FE	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
FEs: court-borderpair	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
FEs: state x year	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
FEs: court-borderpair x year	✓	✓	✓	✓	✓	✓	✓	✓		
Demographic controls	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Case controls	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
FEs: judge	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓

Notes: This table re-estimates columns II–V, and IX–X of Table 2 with $Saving_s$ in expression (3) capped at 20 percent. In square brackets we report p-values. Standard errors are two-way clustered on state and border segment in columns I–III, two-way clustered on state and county in columns IV–V, and three-way clustered on state, calendar-year, and quarter-of-year in columns VI-X. *** p<0.01, ** p<0.05, * p<0.1