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Christian Dippel
Michael Poyker

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ABSTRACT

This paper provides causal evidence of the effect of private prisons on criminal sentencing. Our identification strategy uses state-level changes in private-prison capacity and compares changes in sentencing across trial court pairs that straddle state borders. We find that a doubling of private prison capacity raises sentence lengths by 1.3 percent, but not the likelihood of conviction. The effect is not driven by changes in state legislation, and we find no evidence for 'judicial capture'. We do find some evidence that judges may internalize the lower cost of imprisonment in private prisons. Lastly, private prisons do not appear to accentuate existing racial biases in sentencing decisions.

Christian Dippel
UCLA Anderson School of Management
110 Westwood Plaza, C-521
Los Angeles, CA 90095
and NBER
christian.dippel@anderson.ucla.edu

Michael Poyker
Columbia Business School
Uris Hall 126
3022 Broadway
New York, NY 10027
mp3780@columbia.edu

1 Introduction

In common law countries like the United States, judges exercise a lot of discretion, and may incorporate "extra-legal considerations" in their sentencing decisions (Posner, 2008, p8-11).[1] Understanding these extra-legal considerations is of importance for economists because of the many economic consequences that sentencing decisions have for defendants and their families, as well as for society; e.g. labor market outcomes, recidivism, child-rearing, and (racial) inequality to name a few. In this paper, we study whether a *private* prison system for convicts is one of the extra-legal considerations that influence judges' sentencing decisions.

Our study focuses on the U.S. not only because of the judicial discretion of its trial court judges, but also because it has the highest per capita incarceration rate in the world. According to the *Bureau of Justice Statistics*, more than 2.2 million people were incarcerated in federal, state, and county prisons in 2014. Furthermore, 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, while the overall population has only increased by 36 percent.[2] Policy observers have argued that this disproportionate increase in the prison population is partly due to the influence of private prison companies on the judiciary (Mattera, Khan, LeRoy, and Davis, 2001; Hartney and Glesmann, 2012).[3] Private prison companies' economic interest in harsher sentencing is apparent in the following quote from *Corrections Corporation of America*'s 2014 Annual Report: "The demand for our facilities and services could be adversely affected by the relaxation of enforcement efforts, leniency in conviction or parole standards and sentencing practices." While this quote highlights the unsurprising fact that private prisons have an interest in seeing more defendants convicted in court, there is no causally identified evidence that private prisons actually influence judges' sentencing decisions.

Our study provides such evidence, based on a newly collected panel dataset of the universe of all private and public state prisons from 1980 to today, including their capacity (in beds) and geo-

[1] This fact has given rise to large number of empirical studies using judge fixed effects as exogenous determinants of defendant outcomes. See, e.g., Kling (2006), Aizer and Doyle Jr (2015), and Dobbie, Goldin, and Yang (2018). An interesting case of judicial discretion in a civil law country is Di Tella and Schargrodsky's study of Buenos Aires trial court judges and their choice of prison vs GPS monitoring in criminal sentencing.

[2] See the 1985 and 2015 *Correctional Populations in the U.S. Series* reports on the BJS website.

[3] Think tanks like the American Civil Liberties Union, the Sentencing Project, and the Justice Policy Institute have all written reports on the detrimental effect of private prison lobbying on judicial institutions and judicial integrity (Ashton and Petteruti, 2011; Shapiro, 2011; Mason, 2012). Partly as a result of such reports, the Department of Justice to announce its discontinuation of the use of private prisons in the federal system in August 2016, although this stance has been reverted under the current administration.

location. We combine this with newly collected data on criminal sentencing in state trial courts.[4] We focus on state trial courts because, unlike federal courts, convicts from state courts are sent to the same state's prisons, thus establishing a connection between the location of a court and the location of private prisons.[5] Furthermore, trial courts are the states' courts of general jurisdiction, and as such handle the vast majority of all criminal cases in the U.S. (Berdejó and Yuchtman, 2013).[6]

Our identification strategy relies on within-state changes in private-prison capacity (which can be driven by the opening or closing of a private prison or the privatization of a public one), and on comparing changes in sentencing only within neighboring trial court pairs that straddle a state border. By focusing only on such pairs, we are able to account for local heterogeneity and local trends in unobservables; in our case unobservables include criminal activity, policing, and the local demand for sentencing.[7]

Our core finding is that a doubling of private prison capacities in a state increases sentence lengths in that state's courts by 1.3 percent, corresponding to 18 days, compared to adjacent courts in other states. We find no evidence that private prisons change the likelihood of being sent to prison. This baseline effect is robust in magnitude and statistical significance to the inclusion of meaningful control variables, and to different variations of spatial, time, and judge fixed effects.

We explore possible mechanisms underlying this baseline finding. We argue that private prisons may influence court sentencing through three plausible channels: The first channel is that private prison companies may lobby legislators for harsher sentencing laws and guidelines. A number of politicians have recently come under public scrutiny in this regard for accepting large campaign contributions from private prisons corporations (Brickner and Diaz, 2011). A second channel is 'judicial capture', i.e., that private prison companies directly influence judges through

[4] Data on state-court sentencing is handled by states individually and many do not share the data. The only state that was willing to share its sentencing data and is not included in our analysis is Kansas, which would have charged five times more than other states for our data processing request leading us to echo Frank's 2007 question.

[5] We considered relating a court's geographic proximity to private prisons within states, but conversations with several states' sentencing commission and DOJ employees revealed that there is no within-state spatial connection between a court's location and which prison its defendant are sent to.

[6] Their label varies; in some states they are labeled circuit courts, district courts, or superior courts, but they are always identified as being above the courts of limited jurisdiction and below the state appellate courts.

[7] The advantages of state border discontinuities for identification are well understood. They have also been used in other contexts, e.g., minimum wages (Dube, Lester, and Reich, 2016), manufacturing (Holmes, 1998), or banking (Huang, 2008).

campaign contributions or revolving door promises.[8] Third is the 'fiscal constraints' channel: in most states private prisons are required by law to be a fixed percentage cheaper than state facilities (Mukherjee, 2015),[9] judges may take the lowered fiscal burden of incarceration into consideration when making sentencing decisions. The idea that lower incarceration costs induce judges to pass harsher sentencing may be surprising, but evidence exists that this is the case, at least in the U.S. (Ouss, 2015).

Because we cannot causally identify the potential effect of private prisons on state legislative changes, we instead condition out the 'legislative capture' channel in our identification framework through state-year fixed effects.[10] Our identification thus comes from within-year variation in state-specific private prison capacities. This does not rule out the existence of 'legislative capture', but it means it cannot drive the results we identify. To glean evidence for potential 'judicial capture', we bring to bear evidence on judicial elections: judges in most U.S. states are elected and existing evidence suggests that this introduces electoral cycles into their sentencing, with sentencing becoming harsher closer to re-election dates, a fact that is commonly attributed to a demand for harsher sentences by the electorate (Huber and Gordon 2004; Gordon and Huber 2007; Berdejó and Yuchtman 2013, and Lim 2013). In the presence of 'judicial capture', we expect private prisons to have the most influence over judicial decisions at the peak of the electoral cycle, i.e., close to the election. We find no evidence whatsoever that electoral cycles respond to state-level changes in private prisons. To glean evidence for the 'fiscal constraints' channel, we peruse states' legislated minimum cost savings (per bed) required of private prisons. While these legislated savings may be determined in negotiations between states and private prisons, we view them as econometrically exogenous to over-time variation in judges' sentencing decisions. We find that sentencing responds more to an expansion of private prison capacity when the legislated cost savings to the state are higher.

We recognize that our evidence on mechanisms needs to be interpreted with caution: condi-

[8] In its bluntest form, this can also take the form of bribes: in 2011, two judges in Pennsylvania were convicted of taking bribes from private detention facilities in exchange for harsher juvenile offender sentences, in what the media labeled the "kids for cash" scandal.

[9] For example, in Mississippi private prisons have to be at least 10 percent cheaper than public prisons. The Mississippi Senate Bill #2005 states: "No contract for private incarceration shall be entered into unless the cost of the private operation, including the states' cost for monitoring the private operators, offers a cost savings of at least 10 percent to the Department of Corrections for at least the same level and quality of service offered by the Department of Corrections."

[10] State-laws come into effect on January 1st every year.

tioning out the 'legislative capture' channel does not mean it does not exist; finding no effect on judges' electoral cycles does not conclusively rule out other potential forms of judicial capture; and the evidence on cost-savings is very coarse-grained. Despite these caveats, the combined evidence on 'extra-legal considerations' in criminal sentencing is nonetheless valuable in that it offers more support for one channel than for another.

As a last exercise, we test whether the presence of private prisons exacerbates previously documented racial biases in sentencing; see Abrams, Bertrand, and Mullainathan (2012) and references therein. This may be the case if private prisons 'seek' minority prisoners whom they allegedly prefer because they are less likely to litigate against bad prison conditions (Petrella and Begley, 2013). We confirm existing evidence of racial biases in our data, but find no evidence of a heterogeneous effect of private prisons along the dimensions of race. On the premise that private prisons prefer minority inmates and younger inmates, our non-finding may be viewed as further evidence against the 'judicial capture' channel and in favor of the 'fiscal constraints' channel, since cost-savings to the state are unaffected by defendant characteristics.

Our findings speak to a large literature that studies the sentencing behavior of judges (Steffensmeier and Demuth (2000); Lim and Snyder (2015); Lim, Snyder, and Strömberg (2015); Lim, Silveira, and Snyder (2016); Park (2014a,b), and Eren and Mocan (2016)). There is also a smaller literature on the effects of private prisons, mostly focused on effects on prisoners: Lanza-Kaduce, Parker, and Thomas (1999) and Bales, Bedard, Quinn, Ensley, and Holley (2005) use matching techniques for inmates released from two private prisons in Florida to find negative effect of exposure to private prison on recidivism (the likelihood of committing a crime again), while Thomas (2005) finds the opposite results in the same data. More recently, Mukherjee (2015) shows no statistical effect of private prisons on the likelihood of recidivism. One study by Galinato and Rohla (2018) investigates a similar question as ours, but is more limited in the data at its disposal.[11]

In the following, Section 2 provides a short background, Section 3 describes the data, Section 4 discusses our identification strategy, Section 5 presents results, and Section 6 concludes.

[11] It uses a sample of *federal* criminal trials "off the shelf" from the federal U.S. Sentencing Commission (USSC). In this data, federal court trial outcomes are related to the presence of private prisons in the same state that the federal court is located in. This is problematic because the USSC includes no data on a crime's severity and the defendant's criminal history, because federal court districts are spatially large, and because there should be no spatial relation between the location of a federal court and where in the federal prison system a convict is sent to.

2 A Brief History of Private Prisons in the United States

The U.S. prison population began its disproportionate rise with the 'War on Drugs', which Richard Nixon declared in 1971, and which dramatically increased mandatory sentencing guidelines for drug offenses. New York governor Nelson Rockefeller followed in his footsteps by declaring "for drug pushing, life sentence, no parole, no probation." His policies promised 15 years of imprisonment for drug users and drug dealers. By the early 1980s, prison overcrowding and rising costs of state-run prisons became problematic for local, state and federal governments. Private business enterprises initially stepped in as more cost-effective contractors for specific services, but soon moved into the overall management and operation of entire prisons.[12] In 1984 the *Corrections Corporation of America* (hereafter CCA), was awarded its first contract to fully manage a facility in Hamilton County, Tennessee (visible in the top-panel of our Figure 2).[13] The late 1980s and early 1990s then saw rapid growth in the private prison industry. Today, private prisons in the United States are responsible for approximately 6% of state prisoners, 16% of federal prisoners as well as inmates in local jails in states like Texas or Louisiana. While the share of private prisoners is higher in the federal than in the state system, we focus on state prisons because we require a spatial connection between the location of a court where defendants are sentenced and the prison where they are sent.

3 Data Sources and Construction of Samples

3.1 The Sentencing Data

Our focus is on trial court sentencing decisions in felony offenses of male defendants.[14] We requested sentencing from almost all states' Sentencing Commissions and Departments of Corrections. Many states do not maintain an organized electronic repository of their court cases, or are

[12] There was an earlier history of private prisons in the United States dating back to 1852, when the first private prison was established at in San Quentin. More about the history of the private prisons in the U.S. can be found in (McKelvey, 1936, ch.1-2).

[13] The following year CCA made a proposal to take over the entire prison system of Tennessee, which was seen as audacious at the time. The state legislature, faced with strong opposition from public employee groups and others, declined to act on the offer. CCA did, however, succeed in its effort to win a contract to operate a 400-bed jail in Bay County, Florida.

[14] This excludes courts of limited jurisdiction, such as family courts and traffic courts, and, amongst the courts of general jurisdiction, excludes crimes of minor severity. We focus on male defendants because almost all private prisons in in our data are for male prisoners. We present "placebo results" for female defendants.

otherwise not willing to share their data. However, 14 states were willing to share their data with us at a reasonable cost: Alabama, Arkansas, Georgia, Kentucky, Maryland, Minnesota, Mississippi, Nevada, North Carolina, Oregon, Tennessee, Texas, Virginia, and Washington. The years covered in the sentencing data vary by state, and range from 1980 to 2017.[15]

3.2 Sample Construction

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			

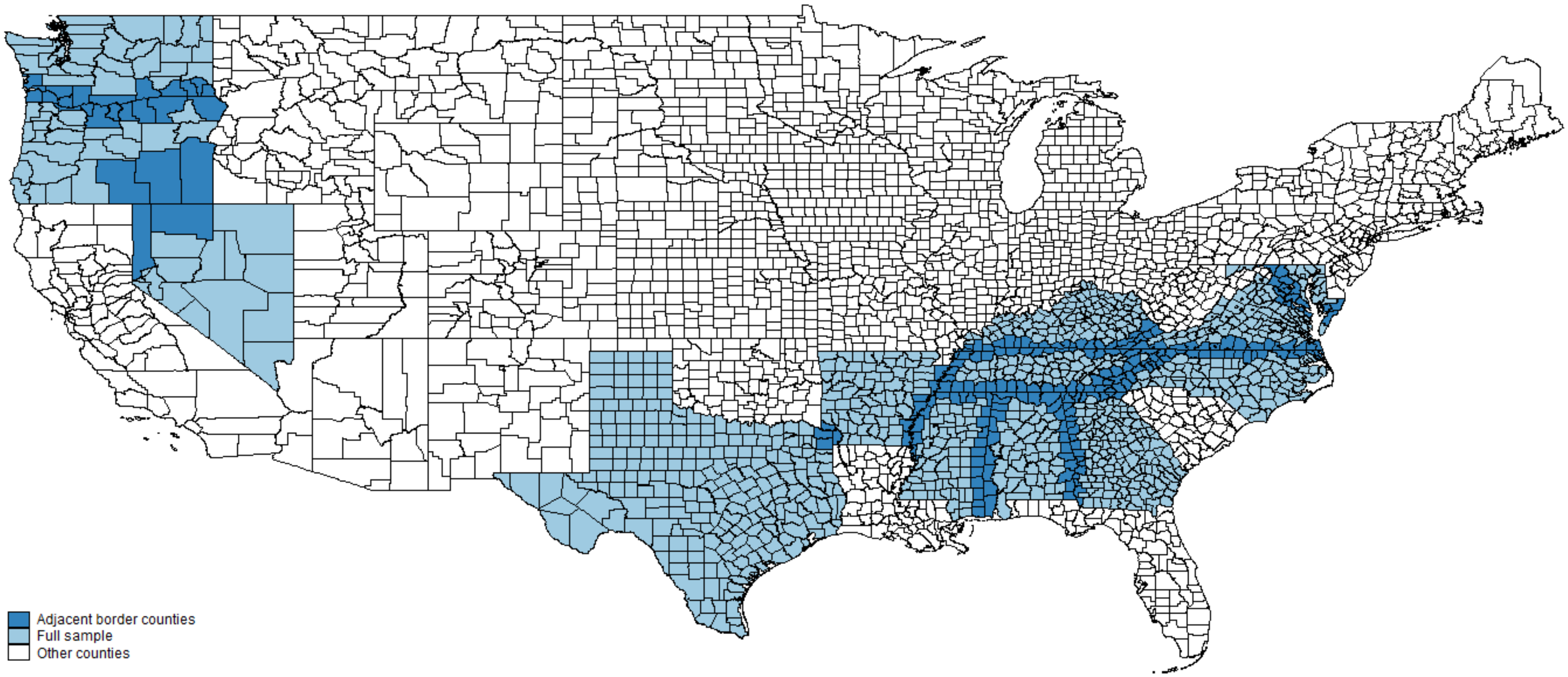
Notes: This table decomposes the sample of 237 border-counties into 17 state-border segments. The table clarifies how many border-segments are linked to each state, and which segments are dropped when a state is dropped from the analysis, as the robustness check reported in Figure 4 will do. The table 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.

Our main sample consists of all the continuous county-pairs that straddle the state border and have available continuous sentencing data.[16] Of the 14 states that shared their data, we cannot use Minnesota, because we don't have a neighboring state for it, and we use a border-sample identification strategy. As a result, our effective sample covers border counties in 13 states, more

[15] Details on how we obtained the sentencing data can be found in Online Appendix B.1.

[16] Among the 3,081 counties in the mainland United States, 1,139 lie along state borders.

Figure 1: Contiguous-Border County-Pairs in our Sample



precisely it covers 252 border counties in 237 distinct county-pairs. Figure 1 shows a map of the 13 states (in light blue) and the 252 counties (in dark blue). Table 1 provides a more detailed breakdown of the number of pairs on each state-border segment. This table clarifies how many border-segments are linked to each state. It also clarifies how many years of sentencing data are used for each border segment, where the constraint on each segment is the state with less available sentencing data. 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). It also has the longest time-coverage in its sentencing data, going back to the 1980s. Tennessee shares segment #5 with Mississippi, which also has a long time coverage of sentencing data, going back to 1990. By contrast, segment #7 is shared with Georgia, whose sentencing data only goes back to 2010, so that county-pairs on that segment can only use six years of overlapping sentencing data.

While the advantages of border-discontinuities in terms of statistical identification (which are discussed in more detail in Section 4) 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. To confirm this is the case, Table 2 provides summary statistics on the socio-economic characteristics of the border counties in column-set I. 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 defendant race and our main dependent variable, the length of a sentence.[17] 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.

3.3 Prison Data

Prison data is constructed from several sources. First, we use the 2005, 2000, 1995, 1990, and 1985 Census of State and Federal Adult Correctional Facilities. Those censuses contain cross-sectional information regarding all U.S. prisons, such as: year of opening a prison, ownership of prison

[17] 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.

Table 2: Border-County Balance Table

	I		II		III	
	All-County Sample		Contiguous Border County-Pair Sample		Differences	
	Mean	s.d.	Mean	s.d.	Mean	P-value
County Controls:						
Population, 2000	67,056.242	(178549)	65,218.324	(131987)	-1,837.916	(0.870)
Population density, 2000	198.911	(604)	208.617	(756)	9.706	(0.792)
Land area (square miles)	774.831	(1,186)	749.675	(1,292)	-25.156	(0.832)
Manufacturing employment	4,968.563	(11,852)	4,327.330	(6,354)	-641.232	(0.362)
Maufacturing average weekly earnings ($)	592.043	(235)	577.130	(166)	-14.912	(0.410)
Restaurant employment	3,363.323	(7,963)	2,795.611	(5,005)	-567.712	(0.347)
Restaurant average weekly earnings ($)	187.561	(33)	186.937	(40)	-0.624	(0.874)
Sentence and Defendant Data:						
Average sentence length in months, men	47.470	(135.4)	44.826	(116.9)	-2.644	(0.671)
---, men conditional on incarceration	60.061	(131.9)	62.329	(152.2)	2.268	(0.698)
Share of sentences for men	0.831	(0.375)	0.844	(0.363)	0.013	(0.130)
Share of Black defendants	0.336	(0.472)	0.342	(0.474)	0.005	(0.914)
Share of Hispanic defendants	0.041	(0.197)	0.031	(0.173)	-0.010	(0.464)

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.

(private or public), if the prison is for male, female, or for both genders, and the security level. We only use state prisons. We then used each state's Departments of Correction websites to augment the base data to include prisons that opened, expanded, or closed after 2005.[18] Each change in private and public prisons capacity is expressed as a change in the number of available beds, as could be seen in Figure 2. The frequency of these changes is monthly, which is important as we will have a strong preference for having state-year fixed effects in our empirical specifications.

In our data, almost all private prisons are for male prisoners so that we will focus on the effect of prisons for men on male defendants.

Figure 2 displays the evolution of the private, as well as public, prisons capacity in Tennessee and Mississippi, including the opening of the very first privately operated state-run prison in Hamilton County, Tennessee, in 1984.

There is evidence that state legislatures select the location of prisons partly with economic considerations in mind; they tend to be located in structurally weak areas, with a view towards providing local employment opportunities (Mattera et al., 2001; Chirakijja, 2018). While we geo-located all prisons in our data, detailed conversations with state department of corrections representatives revealed that the location of prisons (and their selective nature) plays no role in where

[18] Further details on the prison data construction can be found in Online Appendix B.2.

convicts from any given court in a state are sent. While it is true that defendants who await trial (and those who serve short sentences) are usually housed in county jails in the same county as the court, the same is not true of convicts once they enter the state prison system. Which state prisons a convict is sent to depends on capacity at the time of the sentence and on the severity of the crime.

3.4 Judicial Electoral Cycle Data

Ideally we would like to always include judge fixed effects in our data. Unfortunately, this is not always possible because only seven of the states we peruse in this paper include judge identifiers in their data.[19] In order to make use of the judge identifiers where we do have them, we collected individual judges' re-election dates in order to be able to investigate whether any observed electoral cycles in sentencing responded to the presence of private prisons.[20]

4 Identification in the Border-County-Pair Setup

The empirical specification we will estimate is

$$\text{Sentence}_{i(ct)} = \beta^{\text{T}} \cdot \textit{PrivateC}_{st} + \beta^{\text{T}'} \cdot \textit{PublicC}_{st} + \beta^{\text{X}} \cdot X_i + \mu_{st} + \Psi_{p(c)} + \Psi_{p(c)t} + \epsilon_{icts}, \qquad (1)$$

where case i is heard in court c (belonging to state s), and i's sentence is passed in month or year t.[21] Our regressor of interest $\textit{PrivateC}_{st}$ is (the inverse hyperbolic sin of) the capacity of private prisons, measured in beds. Public prison capacity $\textit{PublicC}_{st}$ is measured the same way.[22] This treatment varies at the level of the state s (to which county c is linked), as well as over time with changes in the capacity of private and public prisons. X_i are characteristics of the crime and of the defendant, and μ_{st} are state-specific time controls. Because almost all private prisons are for male prisoners, we focus on the effect of prisons for men on male defendants.

There are compelling reasons for focusing on county pairs across bordering states when iden-

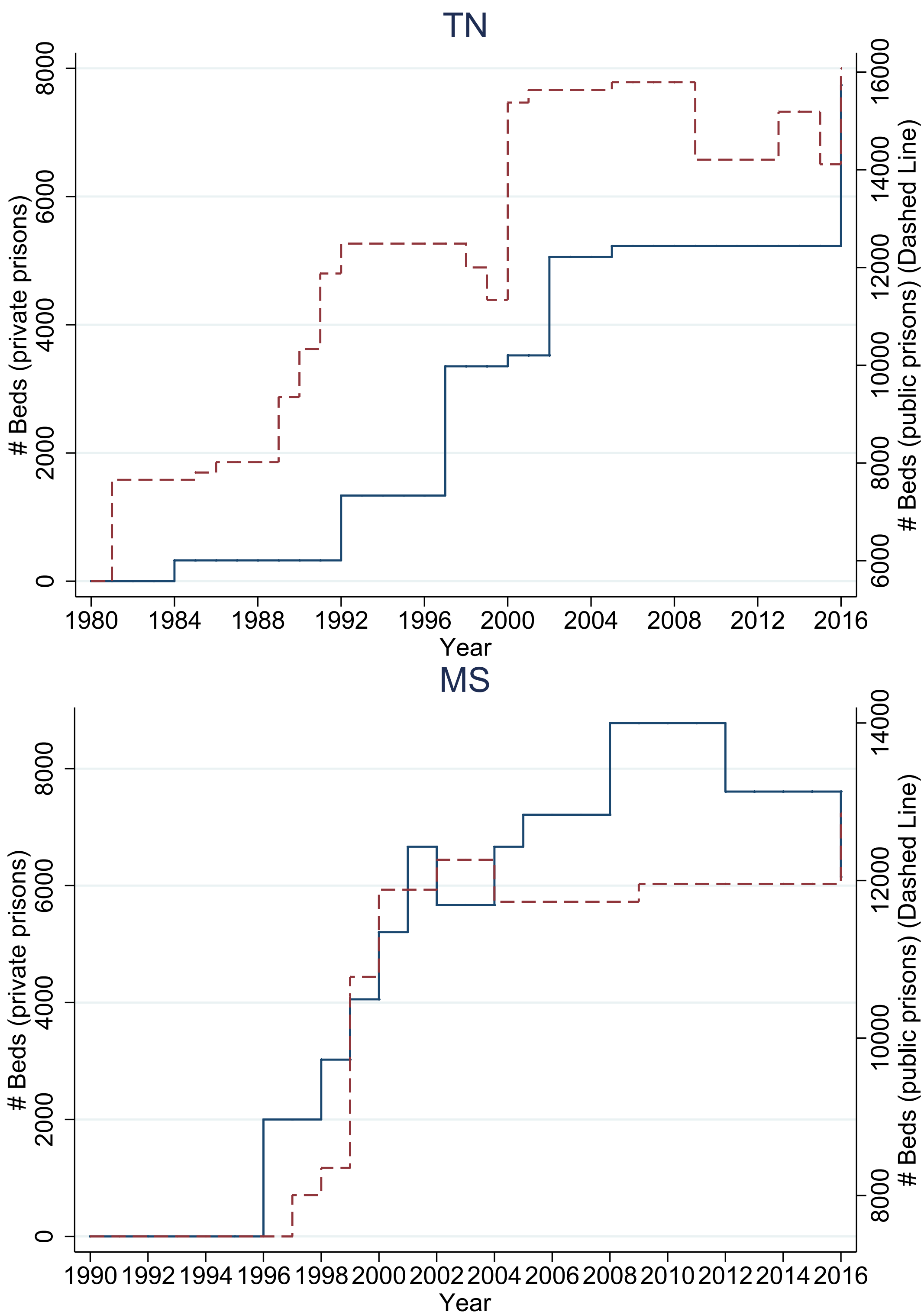
[19] These are Alabama, Georgia, Kentucky, North Carolina, Tennessee, Virginia, and Washington.

[20] Details are provided in Online Appendix B.3.

[21] Each case i is always uniquely mapped to a court in a year-month, a court is almost always a county. Our main outcome is the length of a sentence (in log months), the second outcome is an indicator variable for whether person i is incarcerated and zero otherwise.

[22] Whenever we refer to the logarithm in this paper, it is shorthand for using the inverse hyperbolic sin ($\log(y_i + (y_i^2+1)^{1/2})$), which can be interpreted in exactly the same way as a standard logarithmic variable but without needing to change zero values (Burbidge, Magee, and Robb, 1988; Card and DellaVigna, 2017).

Figure 2: Private and Public Prison Capacity in Tennessee and Mississippi



Notes: 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.

tifying the effect of state-level policy changes (Dube et al., 2016; Holmes, 1998; Huang, 2008). Primarily, what this sample selection achieves is to better control for local trends. In our setting, this means trends in criminal activity, policing and in the local electorate's demand for sentencing. Legislated trends, which are not local, will be absorbed by μ_{st} taking the form of state-year fixed effects in our preferred specification.[23] Contiguous county-pairs form the best treatment-control comparison in this respect because they are the most comparable in local conditions that can affect sentencing decisions.[24] Because the treatment variable $PrivateC_{ct}$ varies at the state-level (as does $PublicC_{ct}$), only contiguous county-pairs that straddle state-border can contribute to statistical identification. In expression (1), this is reflected in a time-invariant county-pair fixed effect $\Psi_{p(c)}$, as well as in pair-specific time-trends $\Psi_{p(c)t}$.

Figure 3 illustrates the identifying variation across the border-segment that connects Georgia to Tennessee. This is segment #7 in Table 1. Tennessee has considerable variation in $PrivateC_{ct}$, as can be seen in Figure 2. 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.

5 Results

This section is structured as follows. In section 5.1 we present the core results. In section 5.2, we investigate mechanisms. In section 5.3 we investigate the effect of of private prisons on racial biases in sentencing.

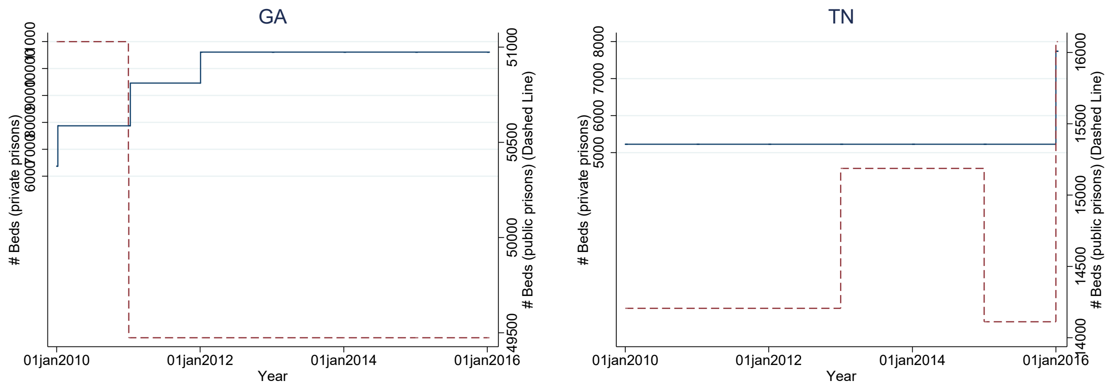
5.1 Core Results

Table 3 presents the results of estimating equation (1), using only courts/counties that straddle state-boundaries. Specifications get incrementally more demanding across columns: Column I reports results for the specification with (time-invariant) border-county-pair fixed effects $\Psi_{p(c)}$, as

[23] This means identification comes from within-year monthly-frequency variation in $PrivateC_{ct}$ and $PublicC_{ct}$.

[24] See Dube et al. (2016) for a taxonomy of the differences between identifying the effect of state-level policy changes in a "full sample" of all counties (or states) vs identifying the same changes in a border-county sample.

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 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).

well as state-year fixed effects μ_{st}. As previously noted, the inclusion of state-year fixed effects absorbs any changes in legislation that may affect sentencing. We always include $PublicC_{st}$, the log of public prison capacities. Column II adds defendant characteristics. 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.[25] Column IV adds a linear trends that is defined in months, thus controlling for within-year linear trends. Finally, Column V replaces state-year fixed effects with county-pair specific year fixed effects. Standard errors are always two-way clustered on state and border segment.

Across specification, the estimated coefficient on $PrivateC_{st}$ in Panel A is always significant, while that on $PublicC_{st}$ is consistently insignificant. The coefficient is reduced by about one-quarter from including defendant and crime characteristics ($0.012/0.016$) but remains unchanged when further introducing more restrictive local heterogeneity fixed effects and local time trends in columns IV and V. The coefficients are elasticities, implying that a doubling of private prison capacities increases length of sentencing by 1.3 percent.[26] In our data, this corresponds to an increase in sentence length of 18 days.

Figure 4 reports on the robustness of our preferred estimate in column V to dropping one state

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

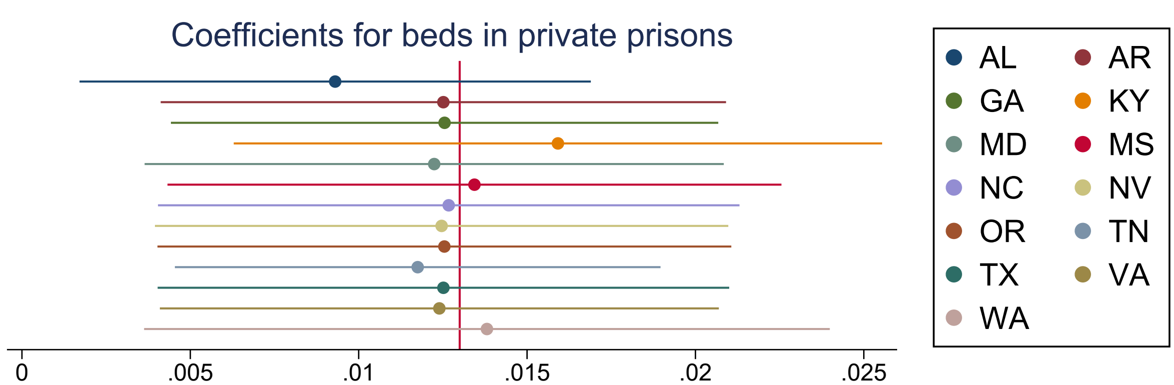
[26] We use inverse hyperbolic sin function in place of the log function, as noted in footnote 22.

Table 3: The Effect of Private Prisons on Sentence Length

	I	II	III	IV	V
	Dependent variable: Sentence (log months)				
Log private prison capacity	0.016***	0.014**	0.012**	0.013**	0.013**
	[0.0046]	[0.0128]	[0.0452]	[0.0265]	[0.0196]
Log public prison capacity	-0.135	-0.151	-0.176	-0.230	-0.315
	[0.6730]	[0.6264]	[0.5698]	[0.4611]	[0.4230]
Demographic controls		X	X	X	X
Case controls			X	X	X
State-year f.e.	X	X	X	X	
County-pair f.e.	X	X	X	X	
State linear calendar-month trends				X	X
County-pair year f.e.					X
R-squared	0.380	0.391	0.456	0.456	0.469
Observations	767,410	767,410	767,410	767,410	767,410

Notes: This table reports on the baseline results from estimating equation (1). Column I reports includes only (time-invariant) border-county-pair and state-year fixed effects, and the log of public prison capacities. Column II adds defendant characteristics: a dummy for recidivism, age, age squared, and race (Asian, Black, Hispanic, and Native American). Column III adds controls for case characteristics, i.e., the severity of the crime. Column IV adds a calendar-month linear trend that controls for within-year trends. Column V replaces state-year fixed effects with court/county-pair specific year fixed effects. 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

Figure 4: Robustness of the Results in Table 3



Notes: This figure reports on the point-estimate and 95th-percent confidence band that results when re-estimating the core specification in Column V of Table 3, 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.

at a time. The estimated coefficient always remains significantly different from zero. Dropping Alabama, which shares border-segments with three states (segments # 4, 9, 15 in Table 1), reduces the coefficient the most, from 0.013 to 0.009. Dropping Kentucky, which shares border-segments with two states (segments # 14, 16 in Table 1), increases the coefficient the most, from 0.013 to 0.016.

In Table 4 we provide several placebo tests that demonstrate that our results are not driven by confounding factors. The baseline estimate in Column I is estimated for male defendants, because almost all private prisons in our data are for male prisoners. In Column II, we re-estimate this baseline effect for female defendants.[27] 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. In Columns III–V, we shift the time-period of the treatment. With state-year fixed effects μ_{st} included, identification of our baseline estimates in Table 3 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 $t+9$, always evaluated relative to a state-specific year fixed effect. None of the resulting estimates in columns III-V has a significant coefficient, making it unlikely that unobservable confounders could drive our baseline results.

We now report on a range of additional checks. First, we compare results from the state-border discontinuity sample to the equivalent results when the specification is run on the full sample of all courts/counties in a state. The identification concern with estimating the effect of state-wide policy changes is that unobserved trends of more crime or of harsher sentencing would subsequently lead to necessary increases in state-wide prison capacity, making it difficult to estimate the effect of prison capacity changes on sentencing. In the state-border discontinuity sample, this concern is mitigated because unobserved trends are more likely to be shared within two bordering counties even if they straddle a state border.[28] In the full sample of all counties, the endogeneity of changes in the state-wide prison capacity to trends in criminal sentencing in the aggregate makes it less likely to detect any statistical effect of prison capacity on sentencing. Indeed, Online Appendix Table 1 confirms that we find no effect of $PrivateC_{st}$ on criminal sentencing in the full sample.

[27] Female defendants are not included in the baseline estimation in Column I.

[28] Recalling that changes in state legislation are absorbed by state-year fixed effects.

Table 4: Placebo Specifications

	I	II	III	IV	V
	Dependent variable: Sentence (log months)				
	Baseline	Female defendants	Lead, t+3	Lead, t+6	Lead, t+9
Log private prison capacity	0.013**	0.003			
t	[0.0196]	[0.8025]			
Log private prison capacity			0.002		
t+3			[0.8109]		
Log private prison capacity				-0.006	
t+6				[0.4903]	
Log private prison capacity					-0.001
t+9					[0.8141]
Log public prison capacity	-0.315	0.169	0.029	0.042	-0.012
[t-specific]	[0.4230]	[0.7823]	[0.5743]	[0.4500]	[0.7937]
R-squared	0.469	0.564	0.469	0.469	0.469
Observations	767,249	141,309	767,249	767,249	767,249

Notes: In all columns, we take the most demanding specification from the baseline results, i.e., Column V in Table 3. In Column II, we estimate the specification for female defendants. In Columns III–V, 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 5: The Effect of Private Prisons on I(Incarceration)

	I	II	III	IV	V
	Dependent variable: 1(Incarceration)				
Log private prison capacity	0.002	0.002	0.002	0.002	0.002
	[0.1164]	[0.2662]	[0.3727]	[0.3566]	[0.6102]
Log public prison capacity	0.039	0.036	0.033	0.012	-0.015
	[0.4945]	[0.5376]	[0.5914]	[0.8533]	[0.8511]
Demographic controls		X	X	X	X
Case controls			X	X	X
State-year f.e.	X	X	X	X	
County-pair f.e.	X	X	X	X	
State linear calendar-month trends				X	X
County-pair year f.e.					X
R-squared	0.258	0.262	0.293	0.293	0.305
Observations	767,402	767,402	767,402	767,402	767,402

Notes: This table reports on the results of estimating equation (1), replacing the length of sentence with the likelihood of being convicted. The column-structure is the same as in Table 3. 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

In Table 5, we check whether the private-prison effect is also present for the 'extensive-margin' decision of convicting a defendant to be sent to prison at all. We do not observe whether sentencing guidelines in a case in our data include or exclude the zero sentence, although it is clear that for very severe crimes they would not. If sentencing guidelines in a case exclude the zero sentence, then an observed zero sentence implies that a defendant was judged to have been innocent (unless they went free for procedural reasons). We find no evidence that $PrivateC_{st}$ influences the likelihood of conviction. While the coefficient is always positive, it is never significant, and in the more demanding specifications, it is very far from conventional significance levels. If there is any effect on the likelihood of incarceration, Table 5 suggests it is very noisy. Nonetheless, the estimated magnitude is robust enough across columns that we also re-report the baseline results when the roughly 15 percent of cases with zero-sentences are set to missing. This is done in Online Appendix Table 2. The resulting estimate on the effect of $PrivateC_{st}$ is about forty percent smaller $(0.008/0.013)$. This is unsurprising because the average sentence length conditional on a positive sentence is also forty percent longer than the unconditional average in Table 2. The implication that a doubling of private prison capacity increases the average sentence by 18 days is therefore unchanged.

5.2 Mechanisms

Given the qualitative evidence and background, there are three plausible channels through which private prisons may influence sentencing: Private prison companies may influence legislators to pass harsher sentencing laws and guidelines. This is the 'legislative capture' mechanism. Private prison companies may influence judges to pass harsher sentences within the parameters set by laws and guidelines. This is the 'judicial capture' mechanism. And judges that internalize fiscal considerations may pass harsher sentences because they internalize that private prisons reduce the marginal costs of incarceration. This is the 'fiscal constraints' mechanism. A fourth possibility is that it may be state prosecutors and not judges that seek harsher sentences when private prisons open. This seems plausible since prosecutors, like judges, are elected in some states, and have some discretion in what sentence length they pursue (Kessler and Piehl, 1998). What rules out this possibility, however, is that this discretion is applied on the dimension of what crimes to charge defendants with, which is a characteristic we always control for.[29]

Legislative Capture: Our identification strategy specifically conditions out this channel because state-laws come into effect on January 1st of a year, and are as such absorbed by our inclusion of state-year fixed effects. We emphasize that this is not done because we want to rule out the 'legislative capture' channel, but rather because the most credible empirical specification happens to rule it out. As a result, our core results have to be driven by something in the judicial process.

Judicial Capture: There is evidence that judges tend to pass harsher sentences in the run-up to re-election dates, a fact that is commonly attributed to a demand for harsher sentences by the electorate (Huber and Gordon, 2004; Gordon and Huber, 2007; Berdejó and Yuchtman, 2013; Lim, 2013). If 'judicial capture' was one mechanism underlying the baseline effect, then we would expect this to show up more strongly when judges come up for re-election since private prisons may exert disproportionate influence over sentencing when judges are in the run-up to re-election. This could be because the need for campaign finances gives any lobby more leverage, or because private prisons actually focus attention on making harsher sentencing a more salient issue for voters. Let j be a judge. All judges are uniquely mapped to one court at any given time, and as a result case i can be uniquely linked to judge j. Define as τ_j the time passed between the beginning of judge j's cycle and the end, scaled as $\tau_j \in [0, 1]$ so that $\tau_j = 0$ on the first day of a judge's term, and $\tau_j = 1$ on the last day.[30] Online Appendix Figure 2 provides a visualization of what an electoral cycle looks like in the data. A natural extension of specification (1) is

$$\text{Sentence}_{i(ct)} = \beta^{\text{T}}_{\tau_j} \cdot \textit{PrivateC}_{st} \cdot \tau_j \beta^{\text{T}} \cdot \textit{PrivateC}_{st} + \beta^{\text{X}} \cdot X_i + \Psi_{p(c)} + \Psi_{p(c)t} + \mu_j + \epsilon_{icts}, \qquad (2)$$

where the first two terms on the right had side are added to specification (1).[31] The hypothesis of a differential electoral cycle is that $\beta^{\text{T}}_{\tau_j} > 0$.

A practical challenge that arises with estimating equation (2) is that judge names are only included in the sentencing data of seven of the 13 states, so that the inclusion of judge fixed effects substantially reduces the sample. Table 6 reports on the results of re-estimating equation (1) with

[29] Prosecutors are not identified in the sentencing data other than in North Carolina, where, in turn, electoral data was not obtainable for them.

[30] The length of the judge cycles is state-specific. In Georgia and Washington judges get elected every four years. In Alabama judges get elected every six years. Kentucky, North Carolina, and Tennessee have eight-year cycles.

[31] $\beta^{\text{T}'} \cdot \textit{PublicC}_{st}$ is omitted for notational simplicity only.

Table 6: Judge Fixed Effects

	I	II	III	IV
	Dependent variable: Sentence (log months)			
	Baseline	Baseline	Sample w judge identifers	
Log private prison capacity	0.013**	0.012**	0.015	0.015
	[0.0196]	[0.0365]	[0.5006]	[0.4729]
Log public prison capacity	-0.315	-0.266	0.198	0.083
	[0.4230]	[0.5220]	[0.4312]	[0.7256]
Judge FEs		X	X	
R-squared	0.380	0.473	0.379	0.369
Observations	767,410	767,410	380,453	380,453

Notes: This table re-estimates the main specification from Column V in Table 3, (reported again in here in Column I) and adds judge fixed effects. Column II adds judge fixed effects but keeps *all* observations, setting missing judge-identifiers to a constant term. Column III keeps only observations from the seven states whose data includes judge-identifiers. Column IV keeps those same observations, and omit the judge fixed effects. 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

judge fixed effects. Column I re-reports Column V in Table 3. Column II adds judge fixed effects but keeps *all* observations, setting missing judge-identifiers to a constant term. Column III keeps only observations from the seven states whose data includes judge-identifiers. Column IV keeps those same observations, but omits the judge fixed effects. More importantly the direct comparison of whether judge fixed effects are included or not (comparing Column I to II or III to IV) shows that judge fixed effects do not by themselves impact the coefficient of interest at all. It is only the sample selection of disregarding six of the thirteen states that affects the estimates, and it primarily affects their precision, whereas the point estimates remain very stable.[32] Given these tradeoffs, our baseline approach is to keep all observations when we add judge fixed effects, and to set missing judge-identifiers to a constant term, setting $\tau_j = 0$ for observations with missing judges. We recognize that the downside of this choice is to introduce measurement error, and in Online Appendix Table 3 we report the results that follow, using only the data where judge names are observed.

We present our results in Table 7. In Column I, we report the baseline result of Column V in Table 3, adding only judge fixed effects. In Column II, we add judge's tenure length. More

[32]In fact, because Oregon is omitted, we also have to omit its border state Washington. Thus, effectively, seven states are omitted.

Table 7: Evidence on the 'Judicial Capture' Mechanism

	I	II	III	IV	V
	Dependent variable: Sentence (log months)				
	+Judge FE	+Tenure	+Proximity	+Proximity & interaction	+Proximity & interaction, by state
Log private prison capacity	0.012**	0.012**	0.012**	0.013**	0.013**
	[0.0365]	[0.0339]	[0.0427]	[0.0355]	[0.0295]
Log public prison capacity	-0.266	-0.266	-0.268	-0.263	-0.257
	[0.5220]	[0.5265]	[0.5262]	[0.5330]	[0.5418]
Tenure		0.004*	0.002	0.002	-0.011**
		[0.0701]	[0.3412]	[0.3369]	[0.0111]
Proximity to election			-0.009	0.022	0.022
			[0.7713]	[0.5638]	[0.5188]
Log private prison capacity				-0.004	
x proximity				[0.4513]	
x Alabama					-0.003
					[0.7659]
x Georgia					-0.003
					[0.3863]
x Kentucky					-0.006
					[0.3151]
x North Carolina					0.001
					[0.8275]
x Tennessee					-0.003
					[0.6890]
x Washington					-0.006
					[0.1018]
Judge FE	X	X	X	X	X
R-squared	0.473	0.473	0.473	0.473	0.473
Observations	767,410	767,410	767,410	767,410	767,410

Notes: This table reports on results of estimating equation (2). In all columns, we take most demanding specification from the baseline results, i.e., Column V in Table 3, and extend it by adding further interactions. Judges in Virginia are appointed, thus they don't have proximity to election. 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

senior judges appear less lenient in this date, although this adds little explanatory power overall. Column III is the first specification that checks for an electoral cycle in sentencing. The evidence for an electoral cycle in sentencing in our data is weak. This turns out to mask a lot of heterogeneity. The effect is strong in Washington State, which is the state that Berdejó and Yuchtman (2013) used in their study. In Column IV, we add the interaction of the private prison capacities and proximity-to-election. The baseline private-prison effect gets slightly stronger from this; and the separate electoral-cycle coefficient becomes positive although insignificant. The interaction coefficient $\beta^{\mathrm{T}}_{\tau_j}$. is insignificant and negative. In Column V, we make this interaction state-specific because electoral cycles can vary across states. The absence of an interaction does not appear to mask any interesting heterogeneity: not a single one of the state-specific coefficients are significant.[33]

For argument's sake we also re-estimate equation (2), for *all* observations, i.e. including the roughly half of all data-points where we have no judge-identifier. For these, we set the to a constant term. The results when omitting observations with missing judge-identifier are reported in Online Appendix Table 3, and the qualitative insights are the same as in Table 7: the evidence for the presence of electoral cycles in sentencing is weak to begin with, and there is never a positive interaction with the presence of private prisons. In principle, this non-finding does not rule out other variants of the 'judicial capture' channel, such as the bribe-paying in the "kids for cash" scandal discussed in footnote 8. However, our view is that the non-finding in Table 7 makes judicial capture overall unlikely to be a statistical regularity.

Fiscal Constraints: A third explanation for the baseline effect is that judges respond to pressure that is internal to state governmental institutions and driven by fiscal considerations rather than lobbying. This explanation finds support in existing evidence: Ouss (2015) provides compelling evidence that sentencing responds to the cost of incarceration, and that lower costs increase sentencing. In case of private prisons, they are mandated to be cheaper on a per-prisoner, per-day basis, and states mandate that they are filled first. Thus, the marginal costs of sending inmates to private prison are smaller than for public ones. In our data, four states have legal requirements for private prisons to be cheaper than public (10% for Kentucky, Mississippi, and Texas, and 5% for Tennessee). In the other states, the per-bed cost of private prisons is negotiated but is

[33] We also re-run the same specification for the extensive margin effect in Online Appendix Table 4, and also do not find evidence to support 'judicial capture' channel.

still public information, so that we can compute the saving rate for all states. The saving rate is $Saving_s = 1 - \frac{\text{Cost in private prison}}{\text{Cost in public prison}}$. If private prison costs are not required to be below public prison costs, then $Saving_s = 0$.[34] We estimate the following extension of expression (1)

$$\text{Sentence}_{i(ct)} = \beta^{\text{T}}_{\text{sav}} \cdot PrivateC_{st} \cdot Saving_s + \beta^{\text{T}} \cdot PrivateC_{st} + \beta^{\text{X}} \cdot X_i + \Psi_{p(c)} + \Psi_{p(c)t} + \mu_j + \epsilon_{icts}, \quad (3)$$

where the first interaction tests the 'fiscal constraints' hypothesis.[35]

Table 8: Evidence on the 'Fiscal Constraints' Mechanism

		I	II
		Sentence (log months)	
Log private prison capacity		0.007	0.006
		[0.1852]	[0.3175]
Log private prison capacity		0.030*	0.033*
	x share saved	[0.0626]	[0.0711]
Log public prison capacity		-0.271	-0.217
		[0.4852]	[0.5982]
Judge FEs			X
R-squared		0.469	0.473
Observations		767,410	767,410

Notes: In all columns, we take most demanding specification from the baseline results, i.e., Column V in Table 3, and extend it by adding further interactions. 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

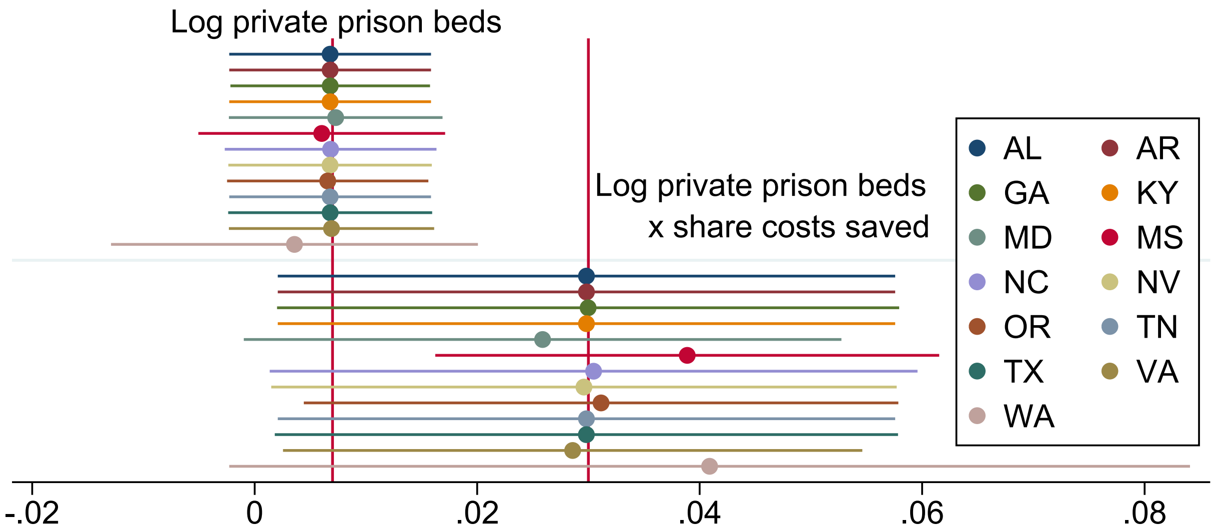
Table 8 reports on the results. Columns I takes most demanding specification from the baseline results, i.e., Column V in Table 3, and adds the interaction with $Saving_s$. Column II adds judge fixed effects to this. The baseline coefficient on $PrivateC_{st}$ is insignificant in Table 8, with the effect loading on the positive interaction $PrivateC_{st} \cdot Saving_s$. Given the coarse-grained nature of $Saving_s$, which only varies across state, this interaction is unsurprisingly not very precisely estimated. In Figure 5, we therefore provide an added check on its robustness to dropping states. The interaction only becomes insignificant when we drop one of Washington or Maryland, which are the only two states with $Saving_s = 0$.

We recognize that the additional evidence presented in section 5.2 needs to be taken with a grain of salt, given data limitations: the judge fixed effects in the estimation of expression (2) are only available for half the observations, and the cost savings estimates in the estimation of

[34] Details are provided in Online Appendix B.4.

[35] $\beta^{\text{T}'} \cdot PublicC_{st}$ is omitted to avoid clutter.

Figure 5: Robustness of the Results in Table 8



Notes: This figure reports on the point-estimate and 90th-percent confidence band that results when re-estimating the core specification in Column I of Table 8, dropping one state at a time. The left vertical line is the point estimate of the level effect in Table 8. The right vertical line is the point estimate on the interaction $PrivateC_{st} \cdot Saving_s$. The results are sorted top-to-bottom in alphabetical order, i.e., omit AL, then AR, then GA, etc.

expression (3) are very coarse. Furthermore, conditioning out the 'legislative capture' channel only means it cannot drive our baseline results, not that such a channel does not exist. Our view is that the results presented in section 5.2 are nonetheless useful in that they reveal no evidence of judicial capture, but some evidence consistent with the fiscal constraints mechanism.

5.3 Heterogeneous Effects of Private Prisons on Minorities

There is compelling evidence of racial biases in sentencing (in addition to any biases in policing and legislation) (Abrams et al., 2012). Critics have argued that private prisons may exacerbate such racial biases, because they prefer minority prisoners who are allegedly seen as less likely to litigate against bad prison conditions (Petrella and Begley, 2013).

A natural extension of specification (1) is to regress:

$$\text{Sentence}_{i(ct)} = \beta^{\text{T}} \cdot PrivateC_{st} + \beta^{\text{T}}_{\mu_i} \cdot PrivateC_{st} \cdot \mu_i + \beta^{\text{X}} \cdot X_i + \Psi_{p(c)} + \Psi_{p(c)t} + \mu_i + \epsilon_{icts}, \tag{4}$$

where μ_i are is a defendant's race (which in specification (1) and specification (2) was also in-

Table 9: Heterogeneous Effects of Private Prisons on Sentencing

	I	II	III	IV	V
	Dependent variable: Sentence (log months)				
Defendant-characeristic:	Black	Hispanic	Native American	Asian	Age
Log private prison capacity	0.016**	0.013**	0.013**	0.013**	0.013**
	[0.0449]	[0.0183]	[0.0195]	[0.0176]	[0.0147]
Log private prison capacity	-0.012	-0.003	-0.013	0.005	-0.000
x defendant-charac.	[0.2118]	[0.8362]	[0.6788]	[0.7384]	[0.7718]
Defendant-characteristic	0.253***	0.077**	0.219***	-0.081	0.032
	[0.0001]	[0.0361]	[0.0000]	[0.1475]	[0.1925]
Log public prison capacity	-0.324	-0.315	-0.314	-0.315	-0.312
	[0.4173]	[0.4242]	[0.4236]	[0.4233]	[0.4295]
R-squared	0.470	0.469	0.469	0.469	0.469
Observations	767,249	767,249	767,249	767,249	767,249

Notes: This table reports on results of estimating equation (4). In all columns, we extend the most demanding specification from the baseline results, i.e., Column V in Table 3. Across columns, we add interactions between the effect of private prisons and one defendant characteristic at a time. The separate effect of the defendant characteristic that is reported below the interaction was already included in Table 3 but not reported. 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

cluded, but subsumed in X_i).[36] If being a minority makes i indeed a more attractive prisoner to private prisons, then we may see a statistically significant coefficient $\beta^{\mathrm{T}}_{\mu_i}$ on the interaction.

Table 9 presents the results. The coefficient on 'characteristic' tests whether the defendant's demographics have any explanatory power over and above recidivism and the crime's characteristics. We find evidence that is suggestive of racial biases in the system, and that confirms existing results: The coefficient on a defendant being Hispanic, Black, or Native American are all positive relative to the white baseline. However, we find no evidence that these racial biases interact with the presence of private prisons. Across columns, the interaction is completely insignificant. Another defendant characteristics along which some have suggested the effect could be heterogenous is age, since younger defendants are viewed as cheaper because they require less health care (Austin and Coventry, 2001). In Column V, we test whether private prisons disproportionately affect the incarceration of younger people and find no evidence that they do.[37]

[36] $\beta^{\mathrm{T}'} \cdot PublicC_{st}$ is omitted to avoid clutter.

[37] Online Appendix Table 5 repeats the estimation, replacing the outcome with an indicator variable for incarceration. Again, there is no evidence for a significant interaction with the defendant's demographics.

In summary, the evidence in section 5.3 is suggestive of racial biases in the judicial system, but we find no evidence that the presence of private prisons interacts with these biases. Taking as given the premise that private prisons prefer minority inmates and younger inmates, the lack of a statistical interaction between $PrivateC_{st}$ and either defendant characteristic may also be viewed as further evidence against the 'judicial capture' channel, and thus in favor of the 'fiscal constraints' channel, since cost-saving considerations from the judge's point of view should be unaffected by either of the defendant characteristics.

6 Conclusion

In common law countries like the U.S., judges may incorporate "extra-legal considerations" into their sentencing decisions. This paper studies the effect of one potentially important such extra-legal consideration, namely the effect of a private prison system on criminal sentencing.

Identifying variation comes only from within-state, over-time variation in private prison capacity, and a state-border-discontinuity neighboring county comparison. We find that a doubling of private prison capacity increase sentencing lengths by 18 days. This effect is robust to a range of different specifications, and robustness checks, including a number of placebo exercises. We find only very weak evidence for an effect on the likelihood of being convicted.

Turning to the mechanisms underlying our baseline result, we consider three plausible mechanisms by which the presence of private prisons may influence judicial decision making. We label these 'legislative capture', 'judicial capture', and 'fiscal constraints.' Our empirical design has to condition out the 'legislative capture' mechanism by necessity, which means our baseline results cannot be driven by it. On the 'judicial capture' mechanism, we test whether the temporal variation in judge re-election cycles impacts sentencing more in the presence of private prisons, and find this not to be the case. On the 'fiscal constraints' mechanism, we find that sentencing responds more to an expansion of private prison capacity when the legislated cost savings (per bed) to the state from usage of private prisons are higher.

We recognize that conditioning out the 'legislative capture' channel does not mean it does not exist, that finding no effect on judges' electoral cycles does not conclusively rule out other potential forms of judicial capture, and that the evidence on cost-savings is very coarse-grained.

With these caveats, the combined evidence on mechanisms can nonetheless be summarized as being more supportive of the view that private prisons impact criminal sentencing because they reduce the cost of imprisonment to the state and judges take account of this, than it is of the more pernicious view whereby judges are directly influenced by the private prisons system though lobbying, campaign contributions or bribes.

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Online Appendix

to

"Do Private Prisons Affect Criminal Sentencing?"

Online Appendix A Additional Background on Private Prisons

Online Appendix A.1 Controversy Associated With Private Prisons

Mis-Management: Brickner and Diaz (2011) provide a useful taxonomy of the purpose of imprisoning a person. It is threefold: protection for the public, rehabilitation of the offender, and punishment for the criminal. While it is difficult to objectively measure the last one, there is abounding evidence that private prisons fall short on the first two dimensions (Brickner and Diaz, 2011, p.15). In the year 2010 alone, there were 4 major scandals associated with private prisons.

1. In Arizona, a prison operated by the Management and Training Corporation let three inmates – two convicted of murder and one convicted of attempted murder – to escape.

2. Later in 2010, at a private Correctional Center in Idaho, a video was released showing an inmate violently beaten and kicked, while the prison guards made no attempt to intervene.

3. In Kentucky, a sex scandal involving female prisoners and guards forced a CCA prison to relocate several hundred women 377 miles away to a state-run prison.

4. GEO group was forced into a $2.9 million settlement to provide up to $400 to inmates at six facilities for illegal and unnecessary strip searches.

Critics of private prisons argue that events like these show the hidden costs of private prisons' efforts to maximize profits by fulfilling only the absolute minimum requirements that contracts allow. Private prisons, like any organization, are subject to moral hazard, and outsourcing incarceration to private corporations comes with the same trade-offs as any other outsourcing of government functions to the private sector. Hart, Shleifer, and Vishny (1997) explored this trade-off theoretically, with an explication to private prisons, concluding that "the private contractor's incentive to engage in cost reduction [relative to the government employee] is typically too strong because he ignores the adverse effect on noncontractible quality." The main difference is likely that the hidden costs and resulting negative externalities from cost-slashing might be more severe in this case that in other areas where government services can be outsourced, although empirical research finds no robust differences in recidivism between former private and public prison inmates (Lanza-Kaduce et al., 1999; Bales et al., 2005; Thomas, 2005; Mukherjee, 2015). Hart et al. (1997) show that competition can alleviate the problem of "noncontractible quality" but the prisons industry today is more monopolized than at any prior point, a concern frequently raised in the criminology literature (Harding, 1997, 2001; Fathi, 2010; Petersilia and Cullen, 2014).

Racial Biases in the Justice System: For-profit prisons are frequently accused of contributing to racial disparities in incarceration, a hot-button issue because of the startling racial disparities in incarceration in the U.S.[38] For-profit prisons are alleged to favor minority inmates, particularly blacks, because they are seen as less likely to litigate over poor prison management.[39] Similarly, private prisons have in recent years particularly expanded into managing detention centers for illegal immigrants, again allegedly because this population has less legal recourse when it comes to mismanagement. One think tank report suggests that 62% of the Immigration and Customs

[38] Statistics suggested that African Americans are almost two times more likely to be arrested and six times more likely to be imprisoned when compared to whites. If current trends persist, one in four black males born today could be imprisoned during their lifetime. Recent statistics suggest that 70% of black males that drop out of high school end up in jail during their lifetime.

[39] See www.huffingtonpost.com/bernie-sanders/we-must-end-for-profit-pr_b_8180124.html.

Enforcement detention centers are now privately owned.[40] Reports from the *American Civil Liberties Union* (ACLU) suggest that "The criminalization of immigration ... enriches the private prison industry" by segregating most of the resulting inmates into one of thirteen privately run *Criminal Alien Requirement* (CAR) prisons.

Online Appendix A.2 Private Prisons' Influence on Government

Private correctional facilities were a $4.8 billion industry in 2016 with profits accumulating to $629 million according to industry market research firm *IBISWorld*. In 2016, CCA and the GEO Group hold roughly 37% and 28% of the industry's market share.[41] Perhaps unsurprisingly, given this market concentration, private prisons corporation are seen as active players on "K Street" (Ashton and Petteruti, 2011). The following lists anecdotal evidence related to these activities, organized by the three strategies commonly associated with any special interest group's efforts to influence policy (Grossman and Helpman, 2001).

Lobbying: The CCA and GEO Group have lobbied Congress as well as state legislatures on issues related to the management and construction of privately operated prisons and detention facilities, and appropriations for both the Bureau of Prisons (BOP) and Immigration and Customs Enforcement (ICE). Both companies lobbied on issues related to the funding of ICE detention facilities. Specifically, GEO Group lobbyists reported lobbying on "issues related to alternatives to detention within ICE" in connection with the administration's 2013-2014 budget requests and CCA's lobbyists reported lobbying on "funding related to the ICE in FY 2013 budget requests." Additionally, both CCA and GEO group have lobbied aggressively against a bill that would have subjected private prisons to the *Freedom Of Information Act* (FOIA). CCA has encouraged shareholders to vote against a resolution that would have brought more transparency to the company.

Campaign Contributions: According to the *National Institute on Money in Politics*, GEO Group alone has given over $6 million to Republican, Democratic and independent candidates over the years.[42] The *Washington Post* reports that in combination GEO and CCA "have funneled more than $10 million to candidates since 1989."[43] CCA's *Political Action Committee* (PAC) contributed over $130,000 and GEO's PAC contributed over $60,000 to congressional candidates in the 2012 election cycle. "In the 2012 cycle, CCA itself, its PAC, its employees and their families contributed more than $1.1 million to candidates, leadership PACs, parties, and committees organized under provision 527 of the Tax Code. GEO Group, its PAC, its employees and their families contributed over $400,000 to candidates, leadership PACs, parties and provision 527 committees in the 2012 cycle. According to political contribution reports released by CCA, "the company gave over $680,000 to state candidates, parties, and committees in the 2012 cycle."[44]

Revolving Door: Ashton and Petteruti (2011) discuss the disproportionate number of former legislators with no corrections industry experience expertise among CCA's board of directors: Former U.S. senator Dennis DeConcini (D-AZ); former Reagan administration official Donna M. Alvarado; former Clinton administration official, and civil rights icon Thurgood Marshall Jr.; and

[40] http://grassrootsleadership.org/reports/payoff-how-congress-ensures-private-prison-profit-immigrant-detention-quota

[41] Subscribers to the reports can access them at www.ibisworld.com/industry/home.aspx.

[42] http://beta.followthemoney.org/entity-details?eid=1096

[43] www.washingtonpost.com/posteverything/wp/2015/04/28/\how-for-profit-prisons-have-become-the-biggest-lobby-no-one-is-talking-about/

[44] http://ir.correctionscorp.com/phoenix.zhtml?c=117983&p=irol-politicalcontributions.

the President of the Freedom Forum, Charles L. Overby, provide bipartisan political access to Washington for CCA.

Online Appendix B Data Description

Online Appendix B.1 Sentencing Data

Sentencing data was collected separately from each state. 14 states were willing to share their data with us for free or at reasonable cost: Alabama, Arkansas, Georgia, Kentucky, Maryland, Minnesota, Mississippi, Nevada, North Carolina, Oregon, Tennessee, Texas, Virginia, and Washington.

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),
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 (2015)

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 B.2 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.[45] From these cross-section of the universe of US correctional facilities we

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

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.[46]

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.[47] 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 censi.

Finally, we assign latitude and longitude data for each prison location from the Google Maps.

Online Appendix B.3 Data on Judges and Judge Elections

All data about judge electoral cycles was taken from the ballotpedia.org. The site contain information about the judges of each circuit court for each state.[48] The individual page of each judge contain data for age and gender of a judge, the dates when she was appointed/elected, date of retirement (if already retired), name of a governor by whom she was appointed (if appointed), and whom the judge replaced.

To collect the data research assistants started with the contemporary judges, collected their data and proceeded with their predecessor judges. This procedure resulted in collecting information for approximately 80% of the judges mentioned in the sentencing data. For the states where the name of a judge was known we searched those judges individually on the sites of their courts and added them to the dataset.

Similarly, ballotpedia.org provides information if judge was unopposed during the election and her winning margin. These information was collected from the pages with state specific yearly results of judge elections.[49]

[46] 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.

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

[48] Or courts of the similar level.

[49] For example, see https://ballotpedia.org/Washington_local_trial_court_judicial_elections,_2016.

As election dates are fixed countrywide, we assume that it is always November 8th for the elections and August 8th for the primaries.

Online Appendix B.4 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.[50]

For private prisons we use state legislation in cast there is a mandatory requirements on the savings: Kentucky (10%),[51] Mississippi (10%),[52] Tennessee (5%),[53] and Texas (10%).[54] 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%).[55] The remaining states have no state private prisons, and thus no legislated cost savings.

Thus we compute $Saving_s = 1 - \frac{\text{Cost in private prison}}{\text{Cost in public prison}}$. 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 1 shows the data.

[50] 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

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

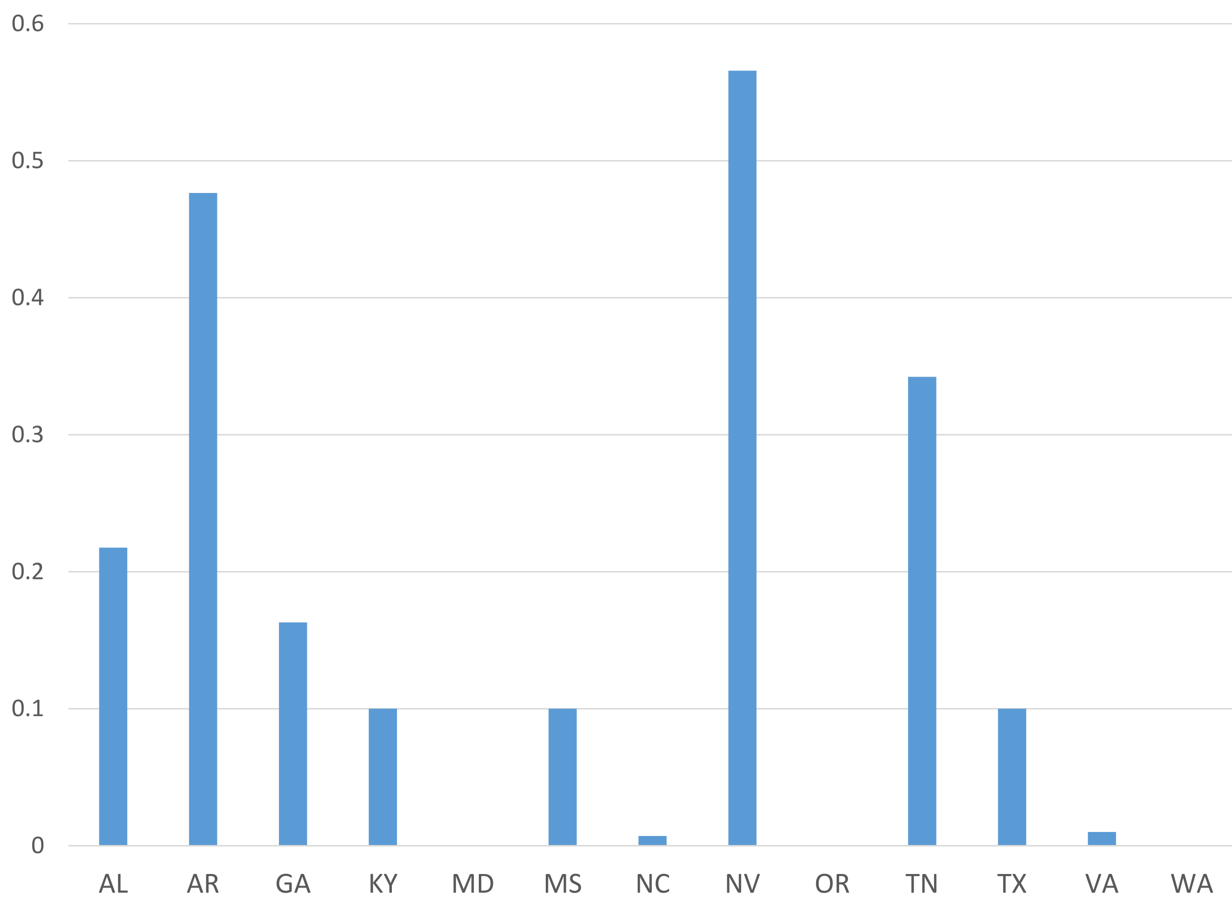
[52] MISS. CODE ANN. § 47-5-1211(3)(a) (West 2012)

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

[54] TEX. GOV'T CODE § 495.003(c)(4) (West 2013)

[55] 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 1: A histogram of $Saving_s$



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

Online Appendix C Additional Results

Table Online Appendix Table 1: Using the Full Sample

	I	II	III	IV	V
	Dependent variable: Sentence (log months)				
Log private prison capacity	-0.001	-0.001	-0.003	-0.003	-0.004
	[0.4936]	[0.5785]	[0.1026]	[0.1516]	[0.1249]
Log public prison capacity	-0.114*	-0.132**	-0.159**	-0.158	-0.215**
	[0.0692]	[0.0361]	[0.0461]	[0.1409]	[0.0351]
Demographic controls		X	X	X	X
Case controls			X	X	X
state-year f.e.	X	X	X	X	
county f.e.	X	X	X	X	
State linear calendar-month trends				X	X
county-year f.e.					X
R-squared	0.368	0.382	0.437	0.418	0.447
Observations	3,544,144	3,544,144	3,544,144	3,544,144	3,544,144

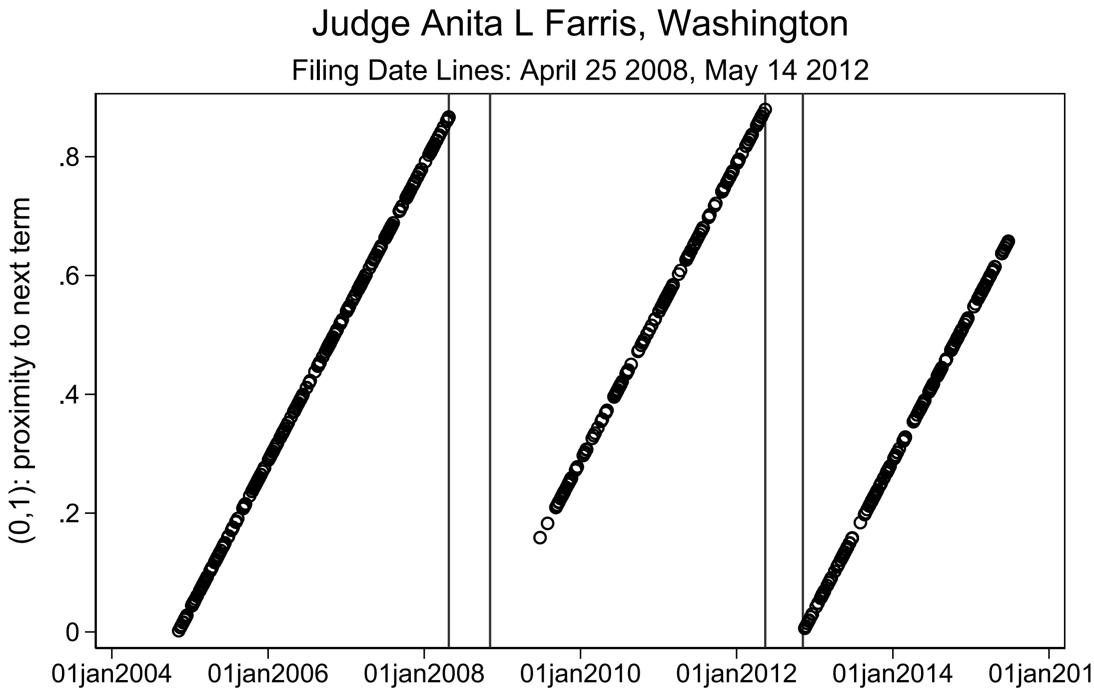
Notes: This table reports on the results of estimating equation (1), using the full sample instead of the border-county sample. The column-structure is the same as in Table 3. In square brackets we report p-values for standard errors are clustered on state and border segment; *** $p<0.01$, ** $p<0.05$, * $p<0.1$

Table Online Appendix Table 2: Treating Zero-Sentences as Missing

	I	II	III	IV	V
	Dependent variable: Sentence (log months)				
Log private prison capacity	0.008***	0.007***	0.006*	0.007*	0.008*
	[0.0030]	[0.0078]	[0.0989]	[0.0690]	[0.0831]
Log public prison capacity	-0.345**	-0.351**	-0.374**	-0.340**	-0.346
	[0.0435]	[0.0283]	[0.0365]	[0.0412]	[0.1428]
Demographic controls		X	X	X	X
Case controls			X	X	X
state-year f.e.	X	X	X	X	
county-pair f.e.	X	X	X	X	
State linear calendar-month trends				X	X
county-pair year f.e.					X
R-squared	0.360	0.369	0.491	0.491	0.516
Observations	556,885	556,885	556,885	556,885	556,885

Notes: This table reports on the results of estimating equation (1), setting zero-sentences to missing. The column-structure is the same as in Table 3. 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

Figure Online Appendix Figure 2: Judicial Electoral Cycles Example



Notes: (*a*) This figure shows an example electoral cycles in our data. The example is from Washington, where judges are elected for four-year cycles. This data is from Dippel and Poyker (2019) and was originally collected from `ballotpedia.org`. Proximity on the vertical axis is defined on a $0, 1$ scale, where proximity equals 1 on the day of the general elections in early November. We trim the electoral cycles at the state-wide filing date, after which the electoral cycle effectively ends for the large majority of judges who have no challenger for their seat. The time between filing date and general election date is sandwiched between two vertical lines. The electoral cycle restarts with the general election date. An observation is a day in which a judge passed a sentence.

Table Online Appendix Table 3: Table 7, Using Only Six States

	I	II	III	IV	V
	Dependent variable: Sentence (log months)				
w/o states/border segments w missing judge IDs	+Judge FE	+Tenure	+Proximity	+Proximity & interaction	+Proximity & interaction, by state
Log private prison capacity	0.015	0.015	0.015	0.016	0.017
	[0.5006]	[0.5151]	[0.5162]	[0.4922]	[0.4663]
Log public prison capacity	0.198	0.198	0.198	0.197	0.209
	[0.4312]	[0.4233]	[0.4288]	[0.4313]	[0.3729]
Tenure		0.001	-0.002	-0.002	-0.035***
		[0.9359]	[0.8899]	[0.8949]	[0.0010]
Proximity to election			-0.005	0.019	0.035
			[0.8292]	[0.7247]	[0.5450]
Log private prison capacity				-0.003	
x proximity				[0.5727]	
x Alabama					-0.008
					[0.6317]
x Georgia					-0.003
					[0.6500]
x Kentucky					-0.009
					[0.3350]
x North Carolina					-0.001
					[0.9269]
x Tennessee					-0.003
					[0.6763]
x Washington					-
					-
Judge FE	X	X	X	X	X
R-squared	0.379	0.379	0.379	0.379	0.379
Observations	378,801	378,801	378,801	378,801	378,801

Notes: This table reports on results of estimating equation (2). It replicates Table 7, but only includes observations from the seven states where we observe judge-identifiers. Washington is omitted because its border state Oregon does not have judge-identifiers. Virginia's judges are appointed and don't have proximity to elections. In all columns, we take most demanding specification from the baseline results, i.e., Column V in Table 3, and extend it by adding further interactions. 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: Table 7 for I(Incarceration)

	I	II	III	IV	V
	Dependent variable: 1(Incarceration)				
	+Judge FE	+Tenure	+Proximity	+Proximity & interaction	+Proximity & interaction, by state
Log private prison capacity	0.002	0.002	0.001	0.002	0.002
	[0.5586]	[0.5077]	[0.5761]	[0.4968]	[0.4810]
Log public prison capacity	-0.005	-0.005	-0.007	-0.007	-0.005
	[0.9552]	[0.9515]	[0.9720]	[0.9744]	[0.9801]
Tenure		0.069	-0.585	-0.581	-0.597
		[0.9991]	[0.9949]	[0.9969]	[0.9872]
Proximity to election			-0.014	-0.009	-0.008
			[0.1557]	[0.5335]	[0.5619]
Log private prison capacity				-0.001	
x proximity				[0.6229]	
x Alabama					-0.002
					[0.4468]
x Georgia					-0.002*
					[0.0840]
x Kentucky					-0.000
					[0.6764]
x North Carolina					0.000
					[0.9776]
x Tennessee					-0.000
					[0.9478]
x Washington					-0.000
					[0.8217]
Judge FE	X	X	X	X	X
R-squared	0.312	0.312	0.312	0.312	0.313
Observations	767,410	767,410	767,410	767,410	767,410

Notes: This table replicates Table 7, with a binary incarceration dummy as the outcome. In all columns, we take most demanding specification from the baseline results, i.e., Column V in Table 3, and extend it by adding further interactions. 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 5: Table 9 for I(Incarceration)

	I	II	III	IV	V
	Dependent variable: 1(Incarceration)				
Defendant-characeristic:	Black	Hispanic	Native American	Asian	Age
Log private prison capacity	0.003	0.002	0.002	0.002	0.001
	[0.2906]	[0.5018]	[0.4084]	[0.3921]	[0.5990]
Log private prison capacity	-0.003	0.001	-0.005	0.005	0.000
x defendant-charac.	[0.1534]	[0.7998]	[0.3703]	[0.4832]	[0.6435]
Defendant-characteristic	0.056***	0.019***	0.074***	-0.057***	0.002
	[0.0026]	[0.0001]	[0.0003]	[0.0088]	[0.6095]
Log public prison capacity	-0.015	-0.013	-0.012	-0.013	-0.014
	[0.8532]	[0.8724]	[0.8772]	[0.8752]	[0.8588]
R-squared	0.308	0.308	0.308	0.308	0.308
Observations	767,241	767,241	767,241	767,241	767,241

Notes: This table reports on the results of re-estimating equation (4) (reported in Table 9), with the outcome replaced by a dummy for incarceration. In all columns, we extend the most demanding specification from the baseline results, i.e., Column V in Table 3. Across columns, we add interactions between the effect of private prisons and one defendant characteristic at a time. The separate effect of the defendant characteristic that is reported below the interaction was already included in Table 3 but not reported. 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