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ABSTRACT

This paper provides causal evidence of the effect of private prisons on court sentencing, using novel data on private prisons and state trial courts. Our identification strategy uses state-level changes in private-prison capacity and compares changes in sentencing only across court pairs that straddle state borders. We find that the opening of a private prison increases the length of sentences relative to what the crime's and defendant's characteristics predict. Effects are concentrated at the margin of sentence length, not of being sent to prison. The effect does not appear to be driven by 'judicial capture'; instead the evidence is most consistent with the cost savings from private prisons leading judges to pass longer sentences. Private prisons do not appear to accentuate existing racial biases in sentencing decisions.

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1 Introduction

A major difference between common law and civil law countries is the degree of discretion that judges have under the two systems. Under the formalistic or legalistic arrangements found in civil law countries, sentences are determined, at least in theory, purely based on the law and the facts of the case. By contrast, judges under case law exercise a lot of discretion. At the level of appellate and supreme courts, this can turn judges into "occasional legislators;" at the level of trial courts it means that judges preferences' and ideology play a large role in sentencing, and that judges may make extra-legal considerations in their sentencing choices (Posner, 2008, p8-11).[1] This, in turn, matters to economists because sentencing decisions have many consequences for defendants, their families, and overall society that economists care about: labor market outcomes, recidivism, child-rearing, and (racial) inequality to name a few.

In this paper we study the effect of one such extra-legal factor on judicial sentencing. Namely, we investigate the effect of the prison system, and in particular the *private* prison system on judicial sentencing in the United States. Researching this question in the U.S. makes sense not only because of the judicial discretion of U.S. trial court judges, but also because the U.S. 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. An additional 4.7 million were either on probation or parole, bringing the total number of adults under some form of correctional supervision in the U.S. to 6.85 million in 2014, close to 3 percent of the population. Furthermore, the share of incarcerated people out of the total population has seen a secular increase over the last three decades. While the U.S. population has increased by 36 percent since 1985, the imprisoned population has increased by 194 percent[2]

Policy observers frequently attribute the disproportionate growth in the number of prisoners in the U.S. in part to the emergence of private prisons starting in 1984 (Mattera, Khan, LeRoy, and Davis, 2001; Hartney and Glesmann, 2012).[3] This connection is not far-fetched, since private

[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

correctional companies clearly have a profit motive in harsher sentencing and a growing prison population, as is apparent in the following quote: "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 or through the decriminalization of certain activities that are currently proscribed by our criminal laws" (*Corrections Corporation of America*, 2014 Annual Report). However, there is no causally identified evidence of this connection.

To provide rigorous quantitative evidence on the effect of private prisons on sentencing, we have constructed a new panel dataset that geo-locates all private and public prisons from 1980 to today, including many openings and closings of both types of facilities. We combine this with newly collected data on criminal sentencing 13 states' state trial courts.[4] Trial courts are the states' courts of general jurisdiction. 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 appelate courts. These courts handle the vast majority of criminal cases in the U.S. (Berdejó and Yuchtman, 2013). Importantly, unlike federal courts, a state's trial court sends it convicts to its state prisons, thus establishing a spatial connection between sentencing and the location of private prisons that does not exist at the federal court level.[5]

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). We always control for changes in overall prisons capacity, collected from the *Bureau of Justice Statistics*. We then compare changes in sentencing only within contiguous trial court pairs that straddle state borders. Our data from thirteen states straddle sixteen state borders. By focusing only on such county-pairs, we are able to account for unobserved heterogeneity and local trends in crime rates through border-pair-specific trends or year fixed effects.[6] With year fixed effects, we identify

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.

[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] Trial courts cover either a single county or a small number of counties. They send all their convicts to prisons in the same state.

[6] 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). We also considered relating a court's 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.

within-year variation in state-specific private prison capacities. Our identification framework cannot plausibly answer the question of whether private prisons change state legislation. With this being a real possibility, the inclusion of year fixed effects is thus primarily motivated by a desire to absorb changes in state-laws that may induce a spurious correlation between private prisons and sentencing.[7] Stated differently, our identification framework absorbs any potential 'legislative capture' through fixed effects, in order to answer the question of whether private prisons impact sentencing through the judicial process, be that through the 'judicial capture' or the 'fiscal constraints' channel.

Our core finding is that a doubling of private prison capacities increases sentence lengths by 1.3 percent, corresponding to an increase of 23 days. We find no evidence that private prisons change the likelihood of being sent to prison. The effects we find are thus economically relatively small (without wanting to minimize any length of prison time). They are, however, statistically significant and robust, and thus worth explaining further.

There are three plausible channels through which private prisons may influence sentencing. 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). This 'legislative capture' argument falls outside of what can be labeled judicial discretion. A second channel is 'judicial capture', i.e., that private prison companies directly influence judges through campaign contributions or revolving door promises.[8] Third is the 'fiscal constraints' channel: There is evidence that judges take such constraints into considerations and that lower incarceration costs induce judges to pass harsher sentencing (Ouss, 2015). While studies have shown no clear evidence that private prisons actually lead to lower *average* incarceration costs, there is clear evidence that they lead to lower *marginal* incarceration cost to the state. This is because in most states private contractors are required by law to be at cheaper on a per-prisoner, per-day basis than in their corresponding state facilities (Mukherjee, 2015). For example, in Mississippi private prisons have to be at least 10 percent cheaper than public prisons.[9] Therefore, the

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

[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] The Mississippi Senate Bill #2005 states: “No contract for private incarceration shall be entered into unless the cost

marginal cost of an additional inmate at a private prison is smaller.

Additional data allow us to shed further light on the 'judicial capture' and the 'fiscal constraints' channels. 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 reelection 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 test for the 'fiscal constraints' channels, we obtained data on the states' minimum required savings from usage of private prisons or estimated it for the states without those requirements. While these savings are of course endogenously determined in negotiations between the state and private prisons, they should not be econometrically endogenous to over-time variation in judges' sentencing decisions. When we split our explanatory variable into prison capacities with high and low savings, we find that the effect is twice as large for states with high required savings. When we alternatively interact private prison capacities with the cost-saving rate, we find that this interaction is more significant than the baseline private prison coefficient.

As a last exercise, we test whether the presence of private prisons creates or exacerbates racial biases in the courts. The inmate population of private prisons has a disproportionate share of Blacks and Hispanics (Austin and Coventry, 2001), and there is compelling evidence of racial biases in sentencing (in addition to any biases in policing and legislation); see Abrams, Bertrand, and Mullainathan (2012) and references therein. Critics of the private-prison system have advanced that private prisons may exacerbate these biases because they prefer minority prisoners because who are allegedly viewed as less likely to litigate against prison mistreatment (Petrella and Begley, 2013). Another defendant characteristics along which some have suggested the effect could be heterogeneous is age, since younger defendants are viewed as cheaper because they require less health care (Austin and Coventry, 2001). 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 and

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."

age.

If we are willing to assume that private prisons really do prefer younger inmates and minority inmates, then this non-finding may be viewed as further evidence against the 'judicial capture' channel and in favor of the 'fiscal constraints' channel.

Overall, our findings do not suggest a large role for private prisons in explaining the disproportionate growth in the U.S. prison population since the mid-80s. This growth is likely due mostly to policy changes such as the war on drugs, and the emergence of private prisons should probably be viewed primarily as a market response to such policy changes. Although that is not to say private prisons could not influence sentencing in some cases, as in the "kids for cash" scandal, the evidence does not suggest this is a statistical regularity.

Our findings also 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 has considerable data limitations: It uses a sample of *federal* criminal trials "off the shelf" from the federal U.S. Sentencing Commission (USSC) combined with ICPSR's National Archive of Criminal Justice Data (NACJD). With the former they relate the trial outcome to the presence of private prisons in the same state that the federal court is located in. This is problematic because federal court districts are spatially too large to control for local trends in crimes and sentencing, and because the USSC has no individual-level data, so one cannot control for the crime's severity and the defendant's criminal history. More problematic is that 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. By contrast, the state courts in our data always send convicts to prisons in the same state. With the latter, NACJD data is not representative in terms of severity of crimes and if state reports more severe crimes in states with more private prisons this would create a non-classical measurement error. More importantly, there very few observations per state-year: In 25th percentile, NACJD data has only 18 cases and 335 cases for the 50th percentile. By contrast, we obtain the universe of all criminal cases.

The remainder of the paper is organized as follows. Section 2 introduces the history of America's private prison system. Section 3 describes data sources and data construction. Section 4 presents our identification strategy and empirical specifications. Section 5 contains the results and a discussion of likely mechanisms. Section 6 concludes.

2 Background

In private prisons individuals are confined or incarcerated by a third party that is contracted by a government agency. Private prison companies typically enter into contractual agreements with governments and are usually paid for each prisoner admitted in the facility. 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. In the federal court system, which of 94 judicial districts a defendant is sentenced in is unrelated to which federal prison they are sent, making it nigh impossible to link individual prisons to individual judicial districts in any form. This is the reason we focus on states' courts and state prisons.

2.1 Brief History of Private Prisons in the United States

The contemporary private prison industry emerged in the mid-1980s as a way of dealing with a rapidly increasing prison population.[10] The increasing prison population was in turn a result of the War on Drugs, which Richard Nixon had 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 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

[10] 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).

for specific services, but soon moved into the overall management and operation of entire prisons.[11] In 1984 the *Corrections Corporation of America* (hereafter CCA), was awarded its first contract to fully manage a facility in Hamilton County, Tennessee.[12] The late 1980s and early 1990s then saw rapid growth in the private prison industry that resulted and several private prison operators became stock-listed. While growth has stalled in recent years, 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.[13]

2.2 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

[11] theguardian.com/society/2015/may/20/misconduct-youth-jail-rainsbrook-ofsted-g4s

[12] The following year CCA made a proposal to take over the entire prison system of Tennessee, which was seen as audacious at the time. However, 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.

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

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.[14] For-profit prisons are alleged to favor minority inmates, particularly blacks, because they are seen as less likely to litigate over poor prison management.[15] 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 Enforcement detention centers are now privately owned.[16] 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.

[14] 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.

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

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

2.3 Private Prisons' Influence on Government

Private prisons corporation are seen to engage in lobbying, direct campaign contributions and building relationships through the 'revolving door' (Ashton and Petteruti, 2011), i.e., 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.[17] The *Washington Post* reports that in combination GEO and CCA "have funneled more than $10 million to candidates since 1989."[18] 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."[19]

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

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

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

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

3 Data Sources and Construction of Samples

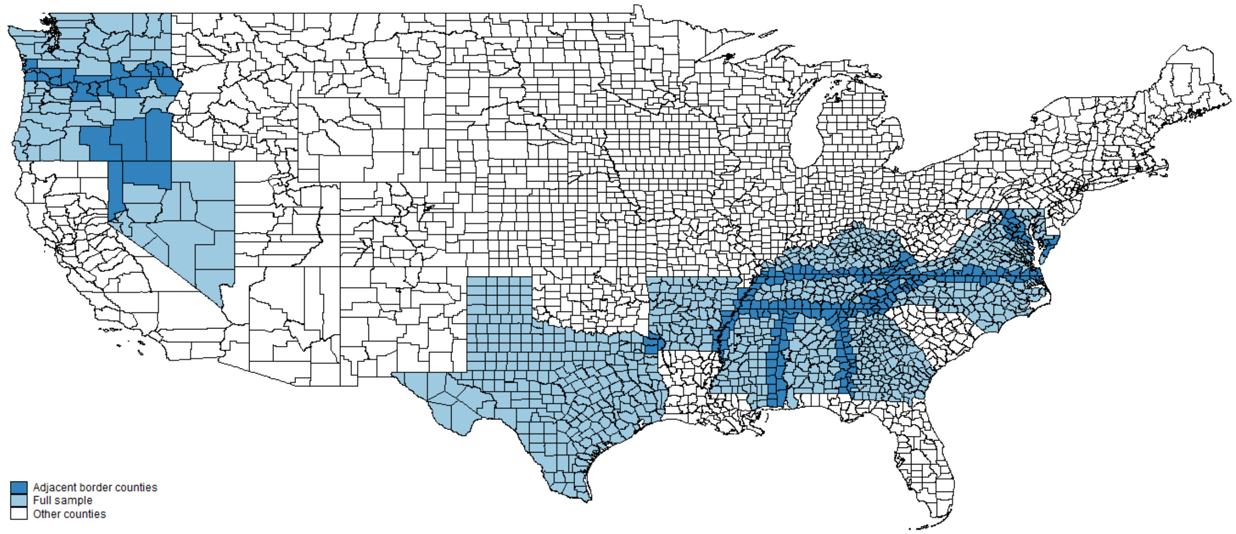
3.1 Sentencing Data

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 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 varied by state and range from 1980 to 2017. We use only trial court sentencing decisions in felony offenses. Our main dependent variable is the length of a sentence. We assign zero value for all cases that the defendant was found not guilty, or paroled.[20]

A crime's severity is one of the two major characteristics that determine any case's court sentence: Classification of crimes varies across the different states' Sentencing Commissions so we had to create state-specific variables for the severity of a crime and for recidivism. Some states have ordinal scales in their classification of crime severity and recidivism, and some use cardinal measures. We combine these different classification schemes into a single regression, we turn them into state-specific sets of fixed effects for values of crime severity. A defendant's past criminal history ('recidivism') is the other major characteristic that determines any case's court sentence. Our sentencing data also included basic characteristics of the defendant, including age at sentencing, gender, and race (Asian, Black, Hispanic, Native American, White, and Other).

[20] In case of consecutive sentences we summed all sentencing within each case and took the maximum for the concurrent sentencing. Consecutive sentences assumed to run one after another, while concurrent sentences can run at the same time. Thus assuming defendant got two sentences, of one and three years, under consecutive sentencing the total sentence length will be four (1+3), and under consecutive — three (max(1;3)).

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



Further details on the sentencing data requests can be found in Online Appendix A.1.

3.2 Sample Construction

Our main sample consists of all the continuous county-pairs that straddle the state border and have available continuous sentencing data. Among the 3,081 counties in the mainland United States, 1,139 lie along state borders. 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 sample covers 252 border counties, or 236 distinct county-pairs, in 13 states. See Table 1. Figure 1 shows the 252 counties on a map (in dark blue).

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 (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. We also added the months of opening and closure of prison to improve the precision of our treatment.

Then we created a year-month-prison panel dataset spanning from 1880 to 2017. Figures 2 and 3 demonstrate the resulting variation in private (solid) and public (dashed lines) prison capacities. Further details on the prison data construction can be found in Online Appendix A.2.

3.4 The Location of Prisons

The location of a new prison, whether private or public, is determined by the state legislature. There is clear evidence that prisons tend to be located in structurally weak areas, with a view towards providing local employment opportunities (Mattera et al., 2001; Chirakijja, 2018). It turns out that this selective nature of where prisons are located does not impact our identification strategy because proximity plays no role in which prison a convict from a given trial court is sent to. Which prison a convict is sent to is, instead, largely dictated by prisons' occupancy and by the severity of the crime, since different prisons host convicts of varying security levels.

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	WI	MN	12	12	23	1991	2014	23
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	KT	VA	4	4	7	2007	2016	9
15	AL	TN	4	7	10	2002	2016	14
16	TN	KT	14	17	30	2002	2017	15
Total	13		122	145	252	2010	2014	236

Figure 2: Variation in opening/closing of private and public prisons

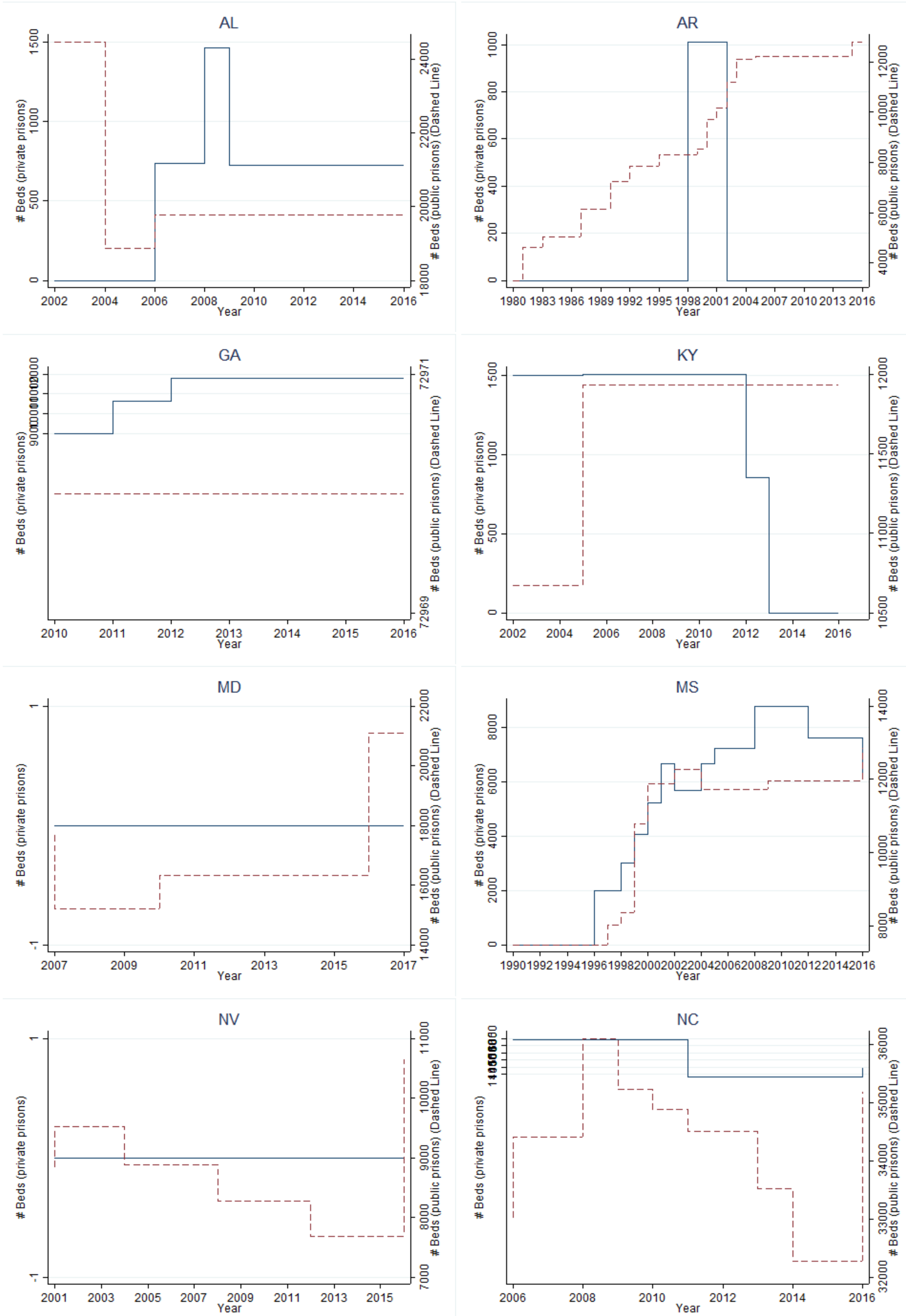
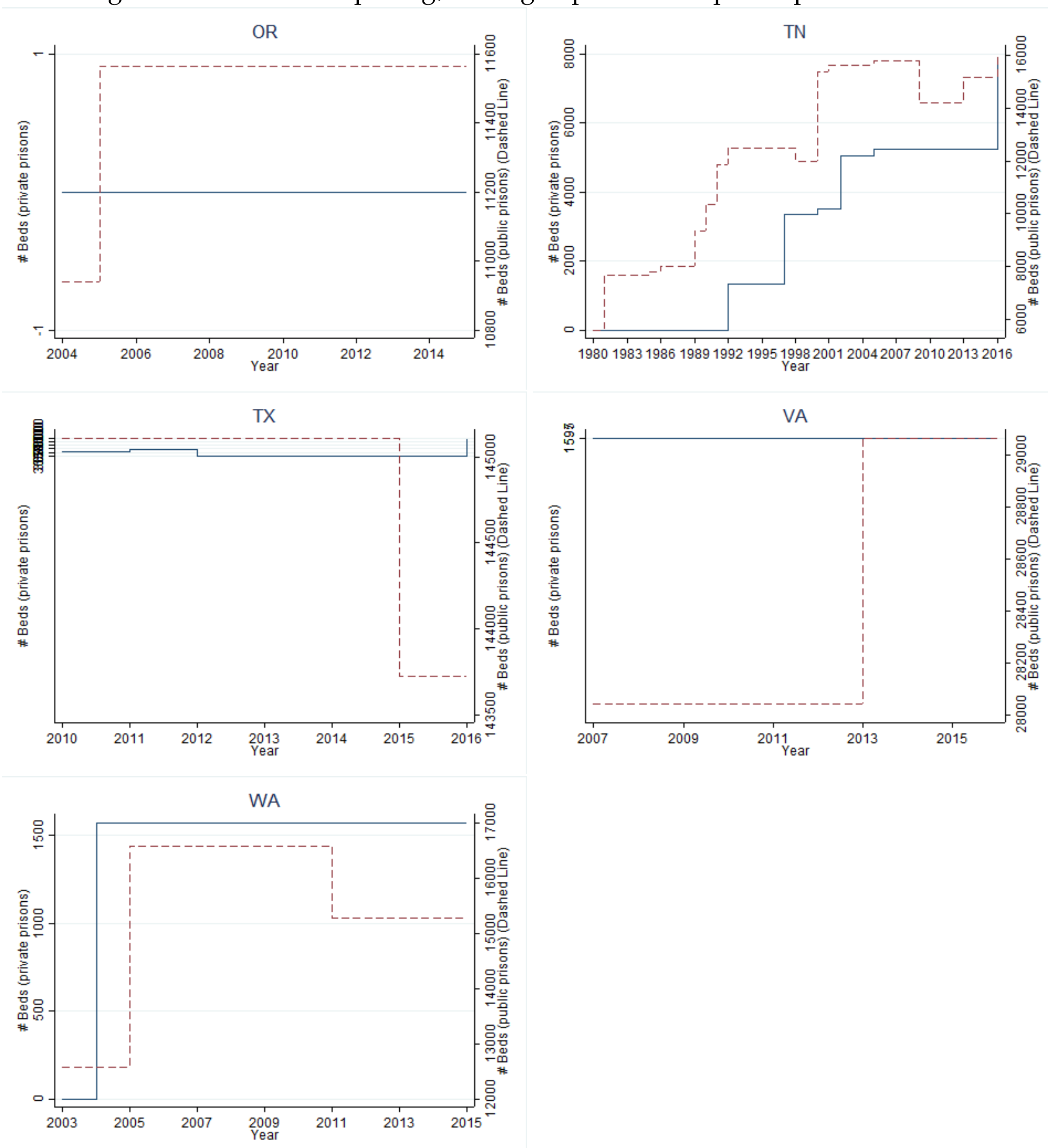


Figure 3: Variation in opening/closing of private and public prisons – continuation



Note: 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).

3.5 Judicial Electoral Cycle Data

Six of the states in this paper include judge names or identifers in the sentencing data: Alabama, Georgia, Kentucky, North Carolina, Tennessee, and Washington. We the code up judge biographies, including when they are up for re-election from `www.ballotpedia.org`. For details, see Online Appendix A.3.[21]

4 Empirical Model and Identification

There are compelling reasons for focusing on county pairs that bordering states when identifying the effect of state-level policy changes. The advantages of using border discontinuities to identify the effects of a state-level treatment are well understood, and border-pair comparisons are commonly used for research questions such as minimum wages (Dube et al., 2016), manufacturing policies (Holmes, 1998), or banking policies (Huang, 2008).[22] Primarily, what this sample selection achieves is to better control for localized trends, which are in our setting trends in criminal activity and sentencing. Contiguous counties form better controls in his respect because they are more comparable in local conditions that can affect sentencing decisions. Importantly, by making time-trends specific to the county-pair with one county in the treated state and the other county in the control state, researchers can allow for flexible time-trends that closely mirror or exactly replicate the time-variation of the treatment itself. The latter is possible because our treatment — opening/closure of private and public prison — varies on month-year level.[23]

In the border-county sample, the regression specification is

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

where case i is heard in court c (belonging to state s), and i's sentence is passed in month or year

[21] Where judges are identified by name, merging the judge biographies is straightforward. Where only judge identifiers are given, these identifiers still almost always include a variant of the judges' initials. As well we observe entry and exit dates and which circuit a judge id is identified with. Based on these, we created a crosswalk from judge id to judge name. For details on merging judge biographies to the data see Dippel and Poyker (2019).

[22] 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.

[23] For example, Dube et al. (2016) use quarter-year variation. Thus, using year-county-pair fixed effects we identify within year changes in prison capacities.

t.[24] X_i are characteristics of the crime and of the defendant. The two most important explanatory variables in any sentence are a crime's *severity* and a defendant's degree of *recidivism*, i.e., past criminal history. Depending on state these two variables together usually explain around 60 percent of a sentence's length). X_i can also include age, age squared, and race of defendant as controls. Our regressor of interest $PrivateC_{ct}$ is log of beds in private prisons – a prison treatment that varies at the level of the state s, as well as over time with the opening and closing of public and private prisons.[25] Public prison capacity $PublicC_{ct}$ is a log of number of beds in public prisons that we add to account for changes in the total capacity of all prisons.

Table 2: Balance Table

	I		II	
	All-County Sample		Contiguous Border County-Pair Sample	
	Mean	s.d.	Mean	s.d.
Population, 2000	180,982	423,425	167,956	297,750
Population density, 2000	465	2,533	556	3,335
Land area (square miles)	1,107	1,761	1,380	2,470
Manufacturing employment	6,608	20,323	6,312	14,100
Maufacturing average weekly earnings ($)	573	202	576	204
Retail employment	4,703	14,642	4,543	11,545
Retail average weekly earnings ($)	306	77	304	77
Average sentence length	30.5	65.4	33.5	63.5
Share of Black defendants	0.30	0.46	0.34	0.47
Share of Hispanic defendants	0.03	0.17	0.03	0.16
Average number of beds in private prisons	1,347	1,438	1,563	1,614
Average number of beds in public prisons	15,847	8,021	16,021	6,958

Expression (1) includes state-specific time controls μ_{st}, as well as border-county pair fixed effects $\Psi_{p(c)}$ (where $p(c)$ denotes the county-pairs in which county c is contained). The biggest difference relative to a specification that includes all counties is that expression (1) allows time-trends $\Psi_{p(c)t}$ to closely mirror or exactly replicate the temporal variation in the treatment variable, because $\Psi_{p(c)t}$ applies to a pair of trial courts that are in separate states. (In a full sample with all counties and no border pairs, this would make treatment co-linear to the time fixed effects.)

[24] 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.

[25] In fact, for prison capacities and sentencing, we use inverse the hyperbolic sin ($\log(y_i + (y_i^2 + 1)^{1/2})$), which is approximately equal to $\log(2) + \log(y_i)$, and can be interpreted in exactly the same way as a standard logarithmic variable but without needing to fill in zero values (Burbidge, Magee, and Robb, 1988).

Having highlighted the advantages of the border sample for identification, it is important to check that the resulting estimations will generalize to the full sample, by verifying that border counties look similar to all counties on observable characteristics: comparing the full set of counties (Column I) to the contiguous-border sample (Column II) in Table 2, we find that they are similar in economic outcomes and sentencing behavior. Results based the border-county specification are therefore likely to be externally valid.

5 Results

This section is structured as follows. In section 5.1 we present the core results of estimating equation (1), and check their robustness. 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

We present our main results in Table 3. This table reports on results using only courts (which are $1:1$-mapped to counties) that straddle state-boundaries, allowing us to effectively control for local trends in both crime and sentencing. Specifications get incrementally more demanding across columns: Column I reports results for the specification with (time-invariant) border-county-pair as well as state-year fixed effects. The only other control is the log of public prison capacities.[26] The resulting coefficient on private prisons is significant and positive. 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).[27] These are all viewed as important in the literature. Column III adds controls for case characteristics, i.e., the severity of the crime. Column IV adds a linear trends that is 'calendar-month varying, thus controlling for within-year trends. Finally, Column V replaces state-year fixed effects with county-pair specific year fixed effects. Following the rest of the literature, standard errors are always clustered on state and border segment.

In Panel A, the coefficient for private prison capacities is positive and significant across all columns, while the coefficient on public prison capacity (or alternatively, total capacity) is consistently insignificant. Both dependent and explanatory variables are in logs so that coefficients are

[26] Our results hold if instead we control for the log of total prison capacities.
[27] Our results also hold if we control for the state-specific recidivism dummies.

Table 3: The Effect of Private Prisons on Sentencing

Panel A: log Sentence-Months, the 'Intensive Margin'

	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	767,249

Panel B: I(Incarceration), the 'Extensive Margin'

	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	767,241

Notes: (*a*) This table reports on results using only courts (which are 1 : 1-mapped to counties) that straddle state-boundaries. (*b*) Panel A reports on the effect on the length of a sentence (in log months), Panel B reports on the effect on the 'extensive-margin' decision of sending a defendant to prison. (*c*) 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: dummy for recidivism, age, age squared, and race (Asian, Black, Hispanic, and Native American), a 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 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

elasticities: a doubling of private prison capacities increases length of sentencing by 1.3 percent. In our data, this corresponds to an increase in sentence length of just under one month.

In Panel B of Table 3, we check whether the private-prison effect is also present for the 'extensive-margin' decision of sending a defendant to prison at all. This turns out not to be the case: While the private-prison effect is positive, it is never significant, and in the more demanding specifications, it is very far from conventional significance levels. In combination, Panels A and B thus suggests that the effect of private prisons on incarceration is small and concentrated at the intensive margin.

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 private prison capacity					
t+12					
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: (*a*) In all columns, we take most demanding specification from the baseline results, i.e., Column V in Table 3. (*b*) In Column II we estimate the specification on the subsample of female defendants. (*c*) 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 $t+9$. (*d*) 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 4 we provide several placebo tests that demonstrate that our results are not driven by within-state-within-year unobserved factors. In Column II, we estimate the baseline effect of (male) private prison capacities on the subsample of female defendants; we find no effect of expanding male private prisons on female sentencing length. In Columns III–V, instead of treatment at month t we use treatment (both private and public prison capacities) at month $t+3$, $t+6$, and $t+9$, always evaluated relative to a state-specific year fixed effect. None of columns II-V has a significant coefficient, making it highly unlikely that unobservable confounders could drive the main results.

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.[28]

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

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

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 $\tau(j)$ the number of days since the begging of judge j cycle, i.e., in Washington $\tau(j) = 0$ in the first day after previous election of a judge's term and $\tau(j) = 1461$ in the day of elections.[29] Let $\mu_{\tau(j)}$ be the "proximity" to the next election of judge j. We can code $\mu_{\tau(j)} = 0$ in the first day after an election, and consecutively increase it before it tops out at $\mu_{\tau(j)} = 1$ right before an election.[30] We set $\mu_{\tau(j)} = 0$ for all judges that do not face reelection (e.g., those that face re-appointment) or for observations with missing judges. A natural extension of specification (1) is to regress

$$\text{Sentence}_{i(ct)} = \beta^{\text{T}}_{\mu_{\tau(j)}} \cdot \mathit{PrivateC}_{st} \cdot \mu_{\tau(j)} + \mu_{\tau(j)} + \beta^{\text{T}} \cdot \mathit{PrivateC}_{st} + \beta^{\text{X}} \cdot X_i + \Psi_{p(c)} + \Psi_{p(c)t} + \mu_j + \epsilon_{icts}, \quad (2)$$

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

We present our results in Table 5. 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 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 fact, Dippel and Poyker (2019) show that among ten states where judges are included in sentencing commission data, electoral cycle exists only in Washington and North Carolina and is weak or non-existent in other states.[31] 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 $\beta^{\text{T}}_{\mu_{\tau(j)}}$. 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-

[29] 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.

[30] For example, in Washington we divide number of days since the previous elections by $4 \times 365 + 1 = 1,461$ and in North Carolina (with 8 year cycle) by $2,922$.

[31] These ten include some states not included in the analysis here because they do not border with any other state for which w have sentencing data.

Table 5: 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		0.252***	0.550	0.551	0.873
		[0.0000]	[0.9998]	[0.9998]	[0.9997]
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	765,596	765,596	765,596	765,596	765,596

Notes: (*a*) This table reports on results of estimating equation (2). (*b*) 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. (*c*) 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

specific coefficients are significant. We re-run the same specification for the extensive margin effect in Online Appendix Table 1, and also do not find evidence to support 'judicial capture' channel. In summary, while we do find (relatively weak) evidence that judges display harsher sentencing before reelection, private prisons do not appear to affect this electoral cycle. In principle, this non-finding does not rule out other variants of the 'judicial capture' channel. However, given the importance of elections for judges it seems to us that this non-finding makes judicial capture overall unlikely to be a statistical regularity.

Table 6: Evidence on the 'Fiscal Constraints' Mechanism

	I	II	III	IV	V
	Dependent variable: Sentence (log months)				
Log private prison capacity	0.013**			0.007	0.006
	[0.020]			[0.1852]	[0.3175]
Log private prison capacity in low saving states		0.008***	0.008***		
		[0.000]	[0.0002]		
Log private prison capacity in high saving states		0.014**	0.013*		
		[0.035]	[0.0752]		
Log private prison capacity x share saved				0.030*	0.033*
				[0.0626]	[0.0711]
Log public prison capacity	-0.315	-0.251	-0.208	-0.271	-0.217
	[0.423]	[0.4898]	[0.6066]	[0.4852]	[0.5982]
Judge FEs			X		X
R-squared	0.469	0.469	0.473	0.469	0.473
Observations	767,249	767,249	765,596	767,249	765,596

Notes: (*a*) 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. (*b*) We use 10% threshold to distinguish states with high and low savings. States with high savings are Alabama, Arkansas, Georgia, Kentucky, Mississippi, Nevada, and Texas. States with low savings are Maryland, North Carolina, Oregon, Tennessee, Virginia, and Washington. (*c*) Data sources for saving rates are described in Online Appendix A. (*d*) 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

Fiscal constraints: Another possible explanation 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 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 the same as public prison costs, then $saving_s = 0$. (See details in Online Appendix A.4.)

We test the 'fiscal constraints' hypothesis by estimating two specifications. First, we split the explanatory variable $PrivateC_{st}$ in $PrivateC_{st}^{High}$ and $PrivateC_{st}^{Low}$ where the former measures the effect of prison capacities where saving rate is equal or larger than 10% and the later is equal to prison capacities where states save less than 10% per-prisoner per-day. Table 6 columns II and III report the results separately for specification with and without judge fixed effects: both coefficients appear to be significant and the magnitude of the estimate for high-saving states is larger than the one for the low-saving states. Second, we add to the baseline explanatory variable its interaction with the time-invariant state-specific saving rate. Table 6 columns IV and V report the results. The baseline coefficient becomes marginally, with the effect loading on the positive and significant interaction.

In summary, the results reported in section 5.2 provide no evidence suggesting 'judicial capture', and are instead much more consistent with the 'fiscal constraints' mechanism, whereby judges appear to be sensitive to the cost of incarceration in their sentencing decisions.

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); see Abrams et al. (2012) and references therein. It is also true that the inmate population of private prisons has a disproportionate share of Blacks and Hispanics (Austin and Coventry, 2001). Perhaps because of the combination of these two facts, critics of the private-prison system have advanced that private prisons may exacerbate racial biases because they prefer minority prisoners because who are allegedly viewed as less likely to litigate against prison mistreatment (Petrella and Begley, 2013). 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).

Table 7: Heterogeneous Effects of Private Prisons on Sentencing

Panel A: log Sentence-Months (the 'Intensive Margin')

	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

Panel B: I(Incarceration), the 'Extensive Margin'

	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: (*a*) This table reports on results of estimating equation (3). (*b*) In all columns, we extend the most demanding specification from the baseline results, i.e., Column V in Table 3. (*c*) 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

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

$$\text{Sentence}_{i(ct)} = \beta^{\text{T}} \cdot \textit{PrivateC}_{st} + \beta^{\text{T}}_{\mu_i} \cdot \textit{PrivateC}_{st} \cdot \mu_i + \beta^{\text{X}} \cdot X_i + \Psi_{p(c)} + \Psi_{p(c)t} + \mu_i + \epsilon_{icts}, \tag{3}$$

where μ_i are is a defendant's race (which in specification (1) and specification (2) was also included, but subsumed in X_i). If being a minority makes i indeed a more attractive prisoner to private prisons, then we may see a statistically significant coefficient $\beta^{\text{T}}_{\mu_i}$ on the interaction.

Table 7 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. Panel A reports result when the outcome is the length of a sentence (in log months), Panel B reports result when the outcome is an indicator variable for if person i is incarcerated.

We find evidence that is suggestive of racial biases in the system, and that confirms existing results: The coefficient on Hispanic, Black, and 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. In Column V, we also test whether private prisons disproportionately affect the incarceration of younger people and find no evidence that they do. Panel B of Table 7 provides results for the same specification but for the probability of being incarcerated, with the same non-results. 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.

If we are willing to assume that private prisons really do prefer younger inmates and minority inmates, then the lack of a statistical interaction between $\textit{PrivateC}_{st}$ and either defendant characteristic may also be viewed as further evidence against the 'judicial capture' channel and in favor of the 'fiscal constraints' channel, since cost-saving considerations would be unaffected by either of the defendant characteristics.

6 Conclusion

In this paper we provided first causal evidence of the effect of private prisons on incarceration and sentencing and tested for the possible channels of these effects.

Using sentencing data from thirteen states and comparing county-pairs that straddle (sixteen) state borders, we found that a doubling of private prisons' capacities causes a moderate increase in the sentencing length of 23 days, but has no effect on the probability of getting a prison term. We find no effect of public prison capacities on incarceration. We find no evidence that this effect is heterogeneous in race or age of the defendant.

Our research design rules out changes in state-legislation as the driver. This implies that our baseline effect likely comes out of the judicial process. We test for two broad mechanisms: A 'judicial capture' mechanism, whereby judges are influenced by private prisons directly. And a 'fiscal constraints' mechanism, whereby judges that internalize fiscal considerations may pass harsher sentences because they private prisons reduce the marginal costs of incarceration. The evidence leads us to reject the former explanation in favor of the latter.

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Online Appendix

to

"Do Private Prisons Affect Criminal Sentencing?"

Online Appendix A Data Description

Online Appendix A.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 A.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.[32] From these cross-section of the universe of US correctional facilities we

[32]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.[33]

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.[34] 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 A.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.[35] 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.[36]

[33] 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.

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

[35] Or courts of the similar level.

[36] 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 A.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.[37]

For private prisons we use state legislation in cast there is a mandatory requirements on the savings: see KY. REV. STAT. ANN. 197.510(13) (West 2007); MISS. CODE ANN. 47-5-1211(3)(a) (West 2012); TENN. CODE ANN. 41-24-104(c)(2)(B), 41-24-105(c) (West 2014); and TEX. GOVT CODE 495.003(c)(4) (West 2013). We also use data from Hakim and Blackstone (2013) and state reports and news articles to find the rest of the information.[38]

Thus to compute the saving we estimate $saving_s = 1 - \frac{\text{Cost in private prison}}{\text{Cost in public prison}}$. 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 B Additional Results

[37] 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

[38] 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 Virginia see www.tkevinwilsonlawyer.com/library/virginia-private-prisons.cfm, and for Washington see www.thenewstribune.com/news/special-reports/article25860412.html.

Table Online Appendix Table 1: Private Prisons and Judges' Electoral Cycles (the 'Extensive Margin')

	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	765,588	765,588	765,588	765,588	765,588

Notes: (*a*) This table reports on results of estimating equation (2). (*b*) 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. (*c*) 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