

Do Private Prisons Affect Court Sentencing?*

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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 circuit courts. Our identification strategy uses state-level changes in private-prison capacity and compares changes in sentencing only across circuit 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. We rule out a number of possible mechanisms, and discuss which plausible mechanisms remain.

Keywords: Private Prisons, Judge Elections, Incarceration, Racial Bias

JEL Codes: D72, H76, K41, K42

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1 Introduction

The United States have the largest number of prisoners of any country 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. In 1985, there were only 757,000 adults in U.S. prisons, with an additional 2.15 million either on probation or parole. Thus, while the overall U.S. population has increased by 36 percent over these thirty years, the imprisoned population has increased by 194 percent, and the population under some form of correctional supervision has increased by 137 percent.¹

This disproportionate growth is often attributed to the commercial private prisons system, which began emerging in 1984 and which is often accused of lobbying for more punitive legislation, stricter policing and harsher sentencing, all in order to produce more prisoners ([Mattera, Khan, LeRoy, and Davis, 2001](#); [Hartney and Glesmann, 2012](#)).² Reports like these motivated 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. If private prisons had a sizeable and clear-cut effect on sentencing, it would indeed be deserving of front-line news, since it would imply that business interests are undermining one of the three pillars of America's democracy.

However, as they currently stand, arguments against private prisons are primarily based on the fact that private correctional companies do lobby and that they do have profit motives, which is quite apparent from their annual reports: "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*). Such language confirms that private prisons have an interest in harsher sentencing, and while one may well object to them on this ground alone, it is neither surprising nor is it evidence

¹ $2.37 = 6.85 / (0.757 + 2.15)$, see the 1985 and 2015 *Correctional Populations in the U.S. Series* reports on the BJS website.

² 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 integrity ([Ashton and Petteruti, 2011](#); [Shapiro, 2011](#); [Mason, 2012](#)).

that they actually impact sentencing practices. That is not to say that there is no such evidence at all, albeit anecdotal: For example, 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.³

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 sentencing at the state-circuit-court level for 13 states that shared their sentencing data with us.⁴ We focus on state-level circuit-courts because they send convicts to their state’s prisons, thus establishing a link between sentencing and the location of private prisons that does not exist at the federal 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 then compare changes in sentencing only within contiguous circuit-court pairs that straddle state borders. By focusing only on such 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 Our core finding is that a doubling of private prison capacities increases sentence lengths by 1.5 percent, corresponding to an increase of 23 days. We find no evidence that private prisons change the likelihood of being sent to prison.

We also ask 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 sentenc-

³ 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), and relates 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.

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 Most circuit courts cover either a single county or a small number of counties.

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](#)).

ing (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 age. While private prisons may well prefer minority inmates and younger inmates, they do not appear to disproportionately affect their sentence length or likelihood of being sent to prison.

There could be a number of distinct channels by which our finding could conceivably be driven by private-prison lobbying, as alleged by some ([Ashton and Petteruti, 2011](#); [Shapiro, 2011](#); [Mason, 2012](#)). Private prisons could influence judges to increase guilty verdicts or lengthen sentences, that they could influence legislators to implement harsher laws, or that they could influence police enforcement. We call these the ‘influence-on-judiciary’, ‘influence-on-legislature’, and ‘influence-on-enforcement’ channels. The “kids for cash” provides at least anecdotal evidence for the former, and the fact that several prominent politicians like former Arizona governor Jan Brewer and Florida senator Marco Rubio have come under scrutiny for accepting large campaign contributions from private prisons corporations is at least suggestive of the latter ([Brickner and Diaz, 2011](#)). Variation in enforcement may lead to a higher likelihood of being prosecuted for a given crime, but should play no role in our empirical setup which investigates sentences conditional on the crime’s and the defendant’s characteristics. As well, our identification strategy specifically conditions out the ‘influence-on-legislature’ channel because changing state-laws are absorbed by our inclusion of state-year fixed effects.⁷ Therefore, while we do not rule out the ‘influence-on-legislature’ channel, we know it does not explain our results.

We can also rule out one variant of the ‘influence-on-judiciary’ channel that has received attention in the literature: judges in most U.S. states are elected and there is evidence that this introduces electoral cycles into their sentencing, which tends to become harsher in the run-up to

⁷State-laws come into effect on January 1st every year. By contrast, we track changes in prison capacity by month so that we identify from within-state-within-year changes in them.

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)). We ask whether electoral-cycles in sentencing become more pronounced in the presence of private prisons. This could be the case if private prisons are either significant contributors to judges' re-election campaigns or alternatively seek to directly mobilize the electorate to demand harsher sentencing. We find no evidence whatsoever that electoral cycles respond to state-level changes in private prisons. Therefore, while we do not rule out the 'influence-on-judiciary' channel in general, it does not appear to work through judges' electoral cycles.

In summary, we find robust evidence that the opening of private prisons in a state makes sentencing harsher in that state. Without wanting to minimize any length of prison time, the effect is relatively small, and there is no effect on the likelihood of being sent to prison. We also find no evidence of heterogenous effects in defendants' race or age. Lastly, while the effect could operate through private prisons exerting political influence, we specifically rule out legislative changes as a driver, and we rule out that this effect ebbs and flows with judges' electoral cycles. We conclude with a qualitative discussion of likely channels, and argue that our results may well be driven primarily or entirely by a relaxing of government-internal fiscal pressures, because private-prison contracts are often written to push the fiscal burden from an additional prisoner to zero.

These 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.

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' circuit 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.⁸ 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 for specific services, but soon moved into the overall management and operation of entire prisons.⁹ In 1984 the *Corrections Corporation of America* (hereafter CCA), was awarded its first contract to fully manage a facility in Hamilton County, Tennessee.¹⁰ 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 mar-

⁸There 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).

⁹See www.theguardian.com/society/2015/may/20/misconduct-youth-jail-rainsbrook-ofsted-g4s.

¹⁰ 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.

ket research firm *IBISWorld*. In 2016, CCA and the GEO Group hold roughly 37% and 28% of the industry's market share.¹¹

2.2 Controversy Associated With Private Prisons

Cost Savings: Much of the rationale for private prisons hinged on cost efficiency, although evidence that private prisons actually decrease costs is mixed (Kish and Lipton, 2013; Lukemeyer and McCorkle, 2006). Advocacy groups frequently accuse government officials of skewing cost statistics to make private prisons more appealing to taxpayers. Overcrowding in particular is seen as way to drive the cost of the price-per-inmate figure of a facility downwards, but comes at the cost of safety, and convict rehabilitation and training.

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

¹¹Subscribers to the reports can access them at www.ibisworld.com/industry/home.aspx.

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 than 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.¹² For-profit prisons are alleged to favor minority inmates, particularly blacks, because they are seen as less likely to litigate over poor prison management.¹³ 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.¹⁴ 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.

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

¹² 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.

¹³ See www.huffingtonpost.com/bernie-sanders/we-must-end-for-profit-pr_b_8180124.html.

¹⁴ <http://grassrootsleadership.org/reports/payoff-how-congress-ensures-private-prison-profit-immigra>

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.¹⁵ The *Washington Post* reports that in combination GEO and CCA "have funneled more than \$10 million to candidates since 1989."¹⁶ 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."¹⁷

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.

¹⁵<http://beta.followthemoney.org/entity-details?eid=1096>

¹⁶ www.washingtonpost.com/posteverything/wp/2015/04/28/how-for-profit-prisons-have-become-the-biggest/

¹⁷ <http://ir.correctionscorp.com/phoenix.zhtml?c=117983&p=irol-politicalcontributions>.

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

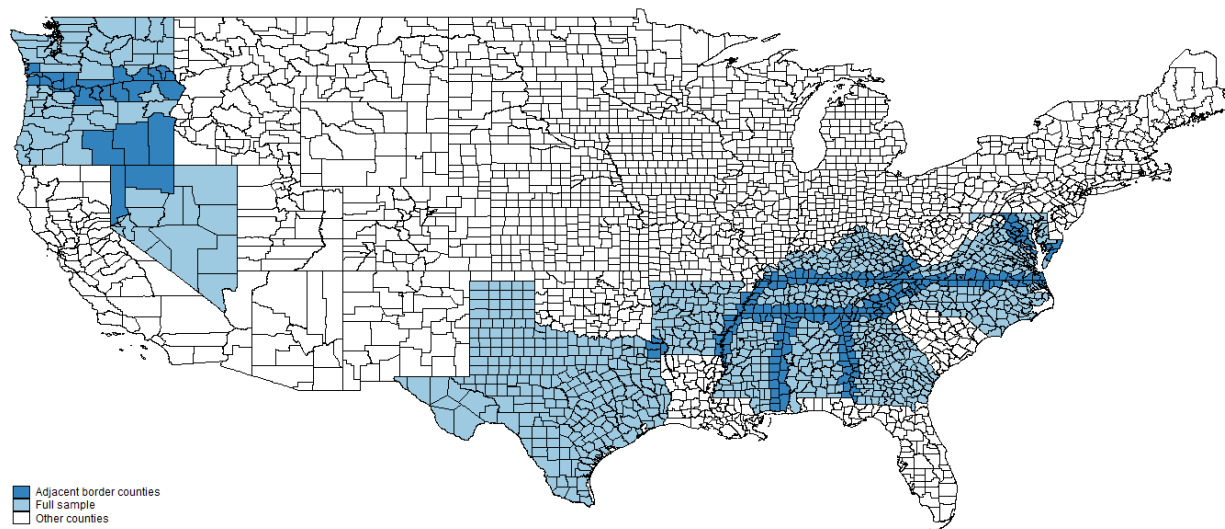
3.1 Sentencing Data

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 circuit court-level 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.¹⁸

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).

¹⁸ 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



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.

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 circuit 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

Segment	Pairs		#counties		#pairs	Sentencing overlap		
	1	2	1	2		y-start	y-end	#years
1	AL	TN	4	7	10	2002	2016	14
2	TX	AR	2	2	3	2000	2016	16
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	OR	WA	10	11	20	2004	2015	11
16	NV	OR	2	3	4	2004	2015	11
17	TN	KT	14	17	30	2002	2017	15
Total	13		114	138	236	2010	2014	240

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

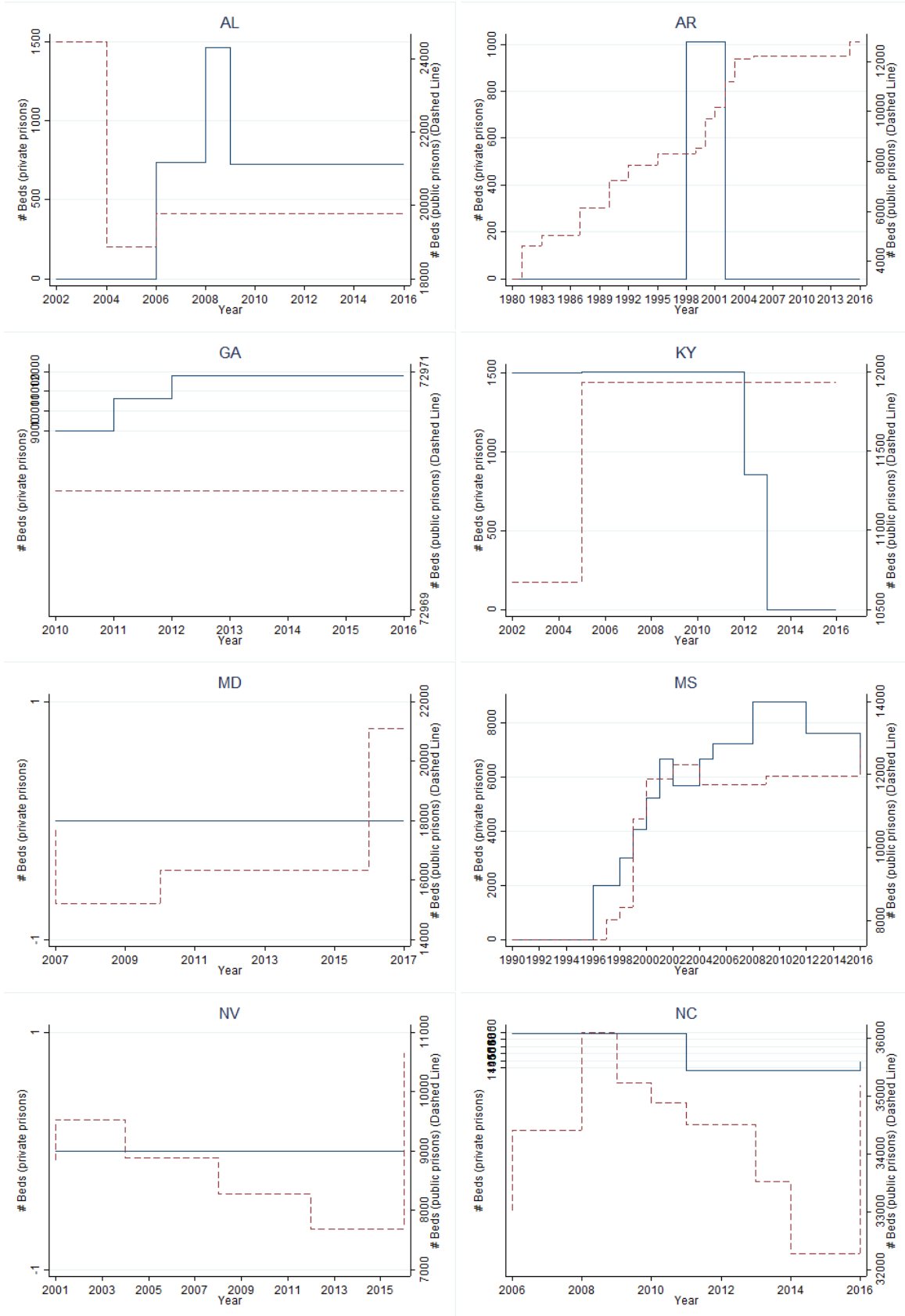
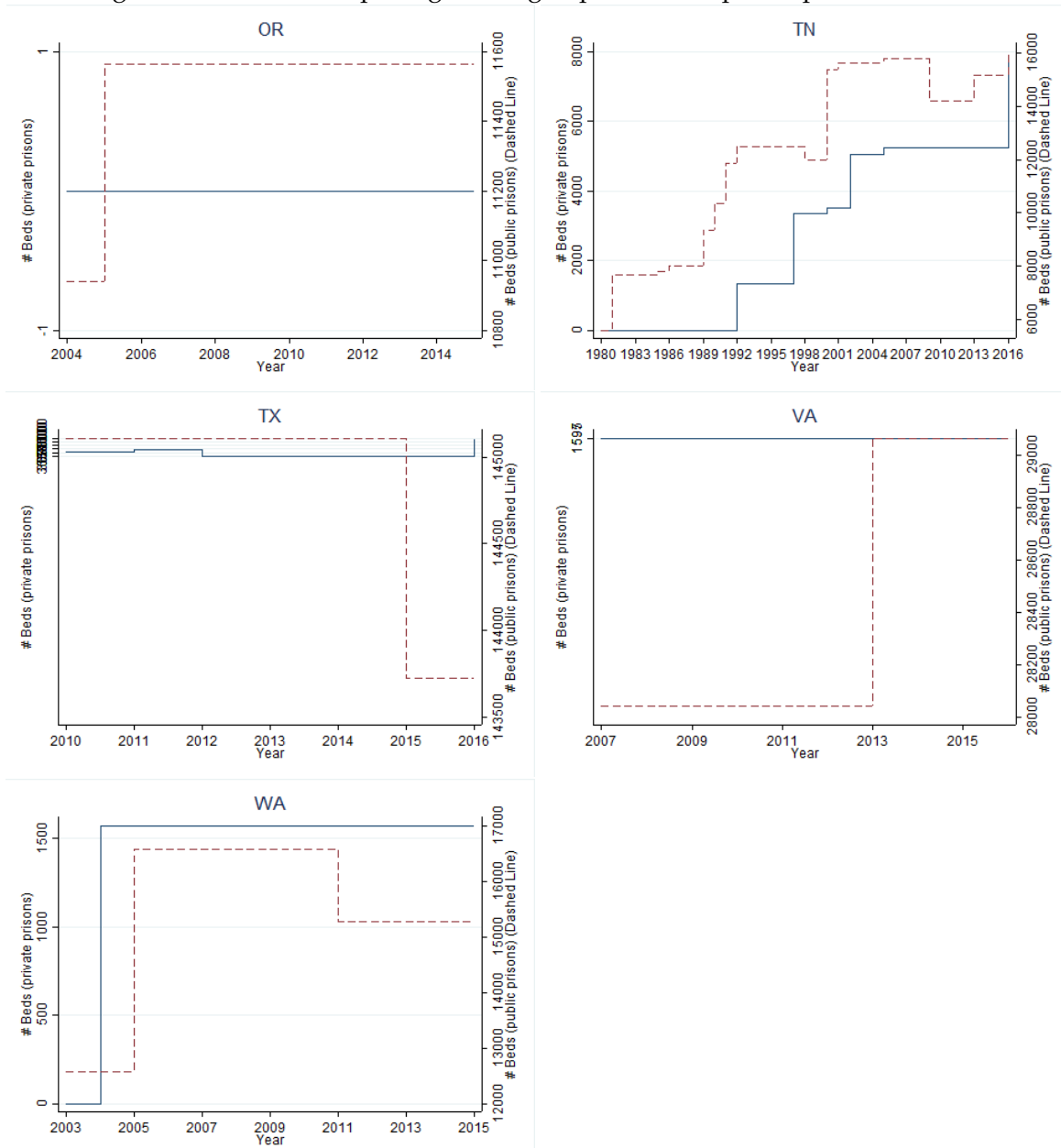


Figure 3: Variation in opening/closing of private and public prisons – continuation



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

We used the site Ballotpedia (www.ballotpedia.org) to find information on circuit-court judges. Then we match judges by name to the state sentencing data where that contains judge names. In some states judges are identified only by a generic identifying number. In those cases we match the judge biographies on Ballotpedia to the years and circuit court in which an identifying number is observed to determine a judge’s identity.¹⁹

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).²⁰ 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.²¹

In the border-county sample, the regression specification is

$$\text{Sentence}_{i(ct)} = \beta^T \cdot \text{Private}C_{st} + \beta^T \cdot \text{Public}C_{st} + \beta^X \cdot X_i + \mu_{st} + \Psi_{p(c)} + \Psi_{p(c)t} + \epsilon_{icts}, \quad (1)$$

where case i is heard in circuit-court c (belonging to state s), and i ’s sentence is passed in month

¹⁹ We were able to match more than 90% of judges this way. Matching is worse for judges whose term starts or ends beyond the range of the sentencing data, but we still able to match more then 70% of these judges.

²⁰ 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.

²¹For example, Dube et al. (2016) use quarter-year variation. Thus, using year-county-pair fixed effects we identify within year changes in prison capacities.

or year t .²² 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.²³ 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	169,660	298,038	150,834	207,749
Population density, 2000	592	236	474	1,232
Land area (square miles)	1,000	1,643	853	1,331
Manufacturing employment	4,969	11,853	4,327	6,354
Manufacturing average weekly earnings (\$)	542	208	577	167
Retail employment	711	2,719	595	1,703
Retail average weekly earnings (\$)	376	129	357	119
Average sentence length	37.9	59.0	37.6	68.7
Share of Black defendants	0.33	0.47	0.35	0.48
Share of Hispanic defendants	0.02	0.14	0.02	0.13
Average number of beds in private prisons	2,729	3,871	2,728	2,947
Average number of beds in public prisons	24,050	20,504	18,768	10,688

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 circuit 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.)

²² Each case i is always uniquely mapped to a circuit-court in a year-month, a circuit-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.

²³ 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). In section 5.2 we investigate the effect of private prisons on racial biases in sentencing. In section 5.3 we investigate the effect of private prisons on electoral cycles in sentencing. Having ruled out that private prisons influence electoral cycles in sentencing, we provide a qualitative discussion of likely mechanisms in section 5.4.

5.1 Core Results

We present our main results in Table 3. This table reports on results using only circuit-courts (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. Our only control is the log of public prison capacities.²⁴ The resulting coefficient on private prisons is insignificant and negative. 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). 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. 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

²⁴ Our results hold if instead we control for the log of total prison capacities.

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 elasticities: a doubling of private prison capacities increases length of sentencing by 1.5 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.

5.2 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](#)). To test these hypotheses, we define a set of indicators μ_i for demographic characteristics that may make i a more attractive prisoner to private prisons. A natural extension of specification (1) is to regress:

$$\text{Sentence}_{i(ct)} = \beta^T \cdot \text{Private}C_{st} + \beta_{\mu_i}^T \cdot \text{Private}C_{st} \cdot \mu_i + \beta^X \cdot X_i + \Psi_{p(c)} + \Psi_{p(c)t} + \mu_i + \epsilon_{icts}, \quad (2)$$

where μ_i is a prisoner’s characteristics, and from now we subsume $\beta^T \cdot \text{Public}C_{st}$ inside $\beta^X \cdot X_i$ for brevity. Specification (2) can be thought of as the generalized difference-in-differences effect of $\text{Private}C_{st}$, conditional on i ’s characteristics.

Table 4 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 character-

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	0.015** [0.0183]	0.014** [0.0312]	0.012* [0.0640]	0.014** [0.0419]	0.015** [0.0248]
Log public prison capacity	-0.040 [0.9090]	-0.053 [0.8742]	-0.085 [0.8074]	-0.139 [0.6868]	-0.303 [0.4154]
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	0.327	0.339	0.404	0.404	0.408
Observations	765,772	765,772	765,772	765,772	765,612

Panel B: Dummy(Sentence) (the 'Extensive Margin')

	I	II	III	IV	V
	Dependent variable: 1(Incarceration)				
Log private prison capacity	0.003 [0.1129]	0.002 [0.2459]	0.002 [0.4497]	0.002 [0.4293]	0.002 [0.5846]
Log public prison capacity	0.048 [0.4528]	0.045 [0.4795]	0.043 [0.5623]	0.021 [0.7733]	0.002 [0.9820]
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	0.257	0.261	0.292	0.292	0.305
Observations	765,772	765,772	765,772	765,772	765,612

Notes: (a) This table reports on results using only circuit-courts (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; *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

istics. 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 4 provides results for the same specification but for the probability of being incarcerated, with the same non-results. In summary, we find evidence suggestive of racial biases in the judicial system, but we find no evidence that the presence of private prisons interacts with these biases.

5.3 Using Judges' Electoral Cycles to Elicit Mechanism

Since our estimation strategy controls for local trends in crime and sentencing as well as controls in public prison capacity, the most likely explanation for our results is that private prisons actually influence the process of sentencing. The most salient potential margins are that private prisons could influence judges to increase guilty verdicts or lengthen sentences (the 'influence-on-judiciary' channel), and that they could influence legislators to implement harsher laws (not to relax existing harsh laws) (the 'influence-on-legislature' channel). Our identification strategy specifically conditions out the 'influence-on-legislature' 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. Therefore our results cannot be explained by changes in legislation (which does not imply that this channel is not present).

We now focus on one particular variant of the 'influence-on-judiciary' channel, for which we have identification in the data. There is an established body of empirical research showing the presence of electoral cycle for judges. Namely, judges tend to levy 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). Given this, it is natural to hypothesize that private prisons may exert disproportionate influence over sentencing when judges are in the run-up to re-election. This could be true because

Table 4: 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-characteristic:	Black	Hispanic	Native American	Asian	Age
Log private prison capacity	0.016* [0.0526]	0.014** [0.0281]	0.014** [0.0285]	0.014** [0.0256]	0.015** [0.0169]
Log private prison capacity x defendant-charac.	-0.007 [0.4684]	0.005 [0.7797]	-0.012 [0.7545]	-0.002 [0.9099]	-0.000 [0.7216]
defendant-characteristic	0.250*** [0.0003]	0.077** [0.0414]	0.230*** [0.0000]	-0.071 [0.2006]	0.033 [0.1926]
Log public prison capacity	-0.276 [0.4722]	-0.274 [0.4736]	-0.272 [0.4749]	-0.273 [0.4744]	-0.269 [0.4814]
R-squared	0.418	0.418	0.418	0.418	0.418
Observations	765,612	765,612	765,612	765,612	765,612

Panel B: Dummy(Sentence) (the 'Extensive Margin')					
	I	II	III	IV	V
	Dependent variable: 1(Incarceration)				
defendant-characteristic:	Black	Hispanic	Native American	Asian	Age
Log private prison capacity	0.003 [0.2995]	0.002 [0.5074]	0.002 [0.3836]	0.002 [0.3676]	0.002 [0.5847]
Log private prison capacity x characteristic	-0.004 [0.1509]	0.001 [0.8127]	-0.005 [0.3978]	0.005 [0.4964]	0.000 [0.6679]
defendant-characteristic	0.056*** [0.0027]	0.019*** [0.0001]	0.075*** [0.0004]	-0.057*** [0.0092]	0.002 [0.6221]
Log public prison capacity	0.002 [0.9768]	0.004 [0.9618]	0.004 [0.9572]	0.004 [0.9592]	0.003 [0.9704]
R-squared	0.308	0.308	0.308	0.308	0.308
Observations	765,612	765,612	765,612	765,612	765,612

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

the need for campaign finances gives any lobby or more leverage, or because private prisons actually focus attention on making harsher sentencing a more salient issue for voters. If either was true, we might expect the sentencing electoral cycle to be more pronounced in the presence of

private prisons. Let j be a judge. All judges are uniquely mapped to one circuit court at any given time, and as a result case i can be uniquely linked to judge j . Define as $\tau(j)$ the quarter since the begging of judge j cycle, i.e., $\tau(j) = 1$ in the first quarter of a judge’s term. A natural extension of specification (1) is to regress

$$\text{Sentence}_{i(ct)} = \beta^T \cdot \text{PrivateC}_{st} + \beta_{\mu_{\tau(j)}}^T \cdot \text{PrivateC}_{st} \cdot \mu_{\tau(j)} + \beta^X \cdot X_i + \Psi_{p(c)} + \Psi_{p(c)t} + \mu_{\tau(j)} + \mu_j + \epsilon_{icts}, \quad (3)$$

where μ_j is a judge fixed effect, and $\mu_{\tau(j)}$ is the “proximity” in the remaining quarters to the reelection of judge j ’s term. (Judges get elected every four years.) We can code $\mu_{\tau(j)} = 1$ in the quarter after an election, and consecutively increase it by one each quarter before it tops out at $\mu_{\tau(j)} = 16$ right before an election.²⁵ 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. The hypothesis of a differential electoral cycle is that $\beta_{\mu_{\tau(j)}}^T > 0$.

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. But it is weak or non-existent in other states. In Column IV, we add the interaction of the private prison capacities and distance-to-election. The baseline private-prison effect gets slightly stronger from this; and the separate electoral-cycle coefficient also becomes significant. However, their interaction 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.

We re-run the same specification for the extensive margin effect in Table 6, and also do not find evidence to support ‘influence-on-judiciary’ channel. In summary, while we do find (relatively weak) evidence that judges display harsher sentencing before reelection, private prisons do not

²⁵ Alternatively, as in Berdejó and Yuchtman (2013), we code $\mu_{\tau(j)} = 0$ for the first 3 years in a cycle and consecutively equal to 1, 2, 3, 4 in the four quarters leading up to an election. This generates very similar results after re-scaling.

Table 5: Private Prisons and Judges' Electoral Cycles (the 'Intensive Margin')

	I	II	III	IV	V
	Dependent variable: Sentence (log months)				
	+Judge FE	+Tenure	+Distance	+Distance & interaction	+Distance & interaction, by state
Log private prison capacity	0.014** [0.0381]	0.014** [0.0389]	0.014** [0.0386]	0.016** [0.0125]	0.017*** [0.0088]
Log public prison capacity	-0.209 [0.5913]	-0.218 [0.5774]	-0.222 [0.5705]	-0.220 [0.5702]	-0.222 [0.5668]
Tenure		0.007 [0.6853]	0.007 [0.5364]	0.007 [0.6337]	0.008 [0.5299]
proximity to election			0.002 [0.3292]	0.005** [0.0273]	0.005* [0.0998]
Log private prison capacity x proximity				-0.000 [0.3665]	
x Alabama					-0.000 [0.7295]
x Georgia					0.000 [0.9782]
x Kentucky					-0.001 [0.1466]
x North Carolina					-0.001 [0.1491]
x Tennessee					-0.000 [0.4088]
x Washington					-0.000 [0.3857]
Judge FE	X	X	X	X	X
R-squared	0.422	0.422	0.422	0.422	0.422
Observations	765,611	765,028	765,028	765,028	765,028

Notes: (a) This table reports on results of estimating equation (3). (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

Table 6: Private Prisons and Judges' Electoral Cycles (the 'Extensive Margin')

	I	II	III	IV	V
	Dependent variable: 1(Incarceration)				
	+Judge FE	+Tenure	+Distance	+Distance & interaction	+Distance & interaction, by state
Log private prison capacity	0.002 [0.4305]	0.002 [0.4359]	0.002 [0.4294]	0.002 [0.3966]	0.002 [0.3690]
Log public prison capacity	0.015 [0.8456]	0.013 [0.8691]	0.012 [0.8709]	0.013 [0.8775]	0.013 [0.8655]
Tenure		0.000 [0.8723]	0.000 [0.8470]	0.000 [0.8654]	0.000 [0.8214]
proximity to election			0.000 [0.3500]	0.001 [0.1458]	0.001 [0.2552]
Log private prison capacity x proximity				-0.000 [0.7019]	
x Alabama					-0.000 [0.9490]
x Georgia					0.000 [0.4086]
x Kentucky					-0.000 [0.3385]
x North Carolina					-0.000 [0.5409]
x Tennessee					-0.000 [0.5932]
x Washington					-0.000 [0.3561]
Judge FE	X	X	X	X	X
R-squared	0.311	0.311	0.311	0.311	0.311
Observations	765,611	765,028	765,028	765,028	765,028

Notes: (a) This table reports on results of estimating equation (3). (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

appear to affect this electoral cycle. Thus, the effect of private prisons on sentencing decisions must either be of a more permanent nature or otherwise fluctuates with factors that are orthogonal to judges electoral cycles.

5.4 Discussion of Possible Mechanisms

Our research cannot speak to whether legislation or policing practice might respond to the expansion of private prisons in our data. By design, our core results have to be driven by something in the judiciary process. We can rule out one particular variant of the ‘influence-on-judiciary’ channel, namely that private prisons impact electoral cycles in judges’ sentencing.

In principle, this leaves other variants of the ‘influence-on-judiciary’ channel as possible channels. One possibility is that judges are influenced by private prisons but that this influence is temporally uncorrelated with judges’ electoral cycles. Another possibility is that it may be state prosecutors and not judges that are being influenced to more aggressively seek harsher sentences.²⁶

Another possibility explanation is that judges respond to pressure that is internal to state governmental institutions and driven by fiscal considerations rather than lobbying. In our view, this is the most likely explanation because it finds support in existing evidence: [Ouss \(2015\)](#) provides compelling evidence that sentencing responds to the cost of incarceration, and that lower costs increase sentencing. This matters in our context because each defendant sent to a state prison creates a roughly constant marginal cost, at least up to capacity constraints. By contrast, the marginal cost of sending a defendant to a private prison is often effectively zero because private-prison operators sign lump-sum contracts with states in which states pay for the capacity regardless of whether beds are filled or not. One advocacy group analyzed 62 contracts between states and private prisons and found that the majority of contracts had clauses that promised to pay for empty beds. The resulting report argues that for-profit prisons “rely on occupancy guarantee clauses in government contracts to guarantee profits and reduce their financial risk, since the ability of private prison companies to ensure prison beds are filled generates steady revenues” ([In the Public Interest, 2013](#)).

²⁶This seems plausible since prosecutors, like judges, are elected in some states. Unfortunately, we were only able to acquire prosecutors’ data for a single state (North Carolina) and therefore cannot credibly test this hypothesis.

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 cause moderate (5.6 months) increase in the sentencing length 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 or police-enforcement as the driver, indicating that our baseline effect comes out of the judiciary process. We rule out differential electoral cycles in judges' sentencing as a explanation, and conclude with a discussion of possible and probable alternative channels.

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