Do Private Prisons Affect Court Sentencing?*

Christian Dippel† Michael Poyker‡

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Abstract

This paper is the first to provide systematic evidence on the causal effect of private prisons on judges' sentencing behavior. We link novel state-level panel data on the opening and closing of private (and public) prisons with state-circuit court data on sentencing. Our identification strategy relies on state-specific changes in private- and public-prison capacity and compares changes in sentencing only across circuit-court pairs that straddle state borders. We find that added private-prison capacity increases the length of sentences relative to what the crime's fundamentals would predict. While the mechanism is likely some form of political influence, we can rule out two specific variants of this channel: one, changes in state-legislation cannot drive the results because we can absorb them with state-year fixed effects; two, we find no evidence that judges' electoral cycles (harsher sentencing before judges run for re-election) become more pronounced with the presence of private prisons.

Keywords: Private Prisons, Judge Elections, Incarceration, Racial Discrimination
JEL Codes: D72, H76, K41, L82

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†University of California, Los Angeles, and NBER.
‡Columbia University.

1 Introduction

The United State 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. Going back three decades, there were only 757,000 adults in U.S. prisons in 1985, with an additional 2.15 million either on probation or parole. Thus, today's situation marks a sharp increase relative to 30 years before. 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). 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). However, these accusations, while they may well be valid, are primarily based on the mere fact that private correctional companies do lobby and that they do have profit motives that are quite apparent in their annual reports. For example, *Corrections Corporation of America* wrote in its 2014 annual report:

> The demand for our facilities and services could be adversely affected by the relaxation of enforcement efforts, leniency in conviction or parole standards and sentencing practices or through the decriminalization of certain activities that are currently proscribed by our criminal laws.

While such language confirms that private prisons have an interest in harsher sentencing, this in itself is not evidence that they actually impact sentencing practices. That is not to say that there is no such evidence at all: For example, in 2011, two judges in Pennsylvania were convicted of taking

[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.

bribes from private detention facilities in exchange for harsher juvenile offender sentences, in what the media labeled the "kids for cash" scandal. Beyond the anecdotal, however, there is little to no rigorous quantitative research showing a causal effect of private prisons on incarceration rates or sentencing.[2]

This paper fills that gap. We have constructed a new panel dataset that geo-locates all private and public prisons from 1985 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 where we were able to obtain sentencing data.[3] 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.[4]

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 circuit-court-pairs that straddle state borders, we are able to account for unobserved heterogeneity and local trends in crime rates through border-pair-specific trends or year fixed effects.[5] We find that a doubling of the private prison capacity increases the average sentence length by 1.5 percent, which translates into one month in our data

There are a number of distinct channels through which lobbying by private prisons could influence sentencing. The most obvious channels are that private prisons could influence judges to increase guilty verdicts or lengthen sentences, and that they could influence legislators to implement harsher laws. Call these the 'influence-on-judiciary' and the 'influence-on-legislature'

[2] The study by Galinato and Rohla (2018) is close in motivation to ours, but differs in a number of important dimensions: It uses a sample of *federal* criminal trial outcomes 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 the 94 federal judicial districts are spatially too coarse to control for local trends in crimes and sentencing. More problematic still is that there is no spatial relation between the location of a federal court and the location of the federal prison that a convict from that court is sent to. By contrast, the state courts in our data send convicts to prisons in the same state. In addition, the NACJD has no extensive margin of conviction because it includes only convicts, and the USSC has no individual level data, meaning one cannot control for the two main individual driver of sentencing which is recidivism.

[3] 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. What's the matter with Kansas?

[4] Most circuit courts cover either a single county or or a small number of counties. There are small number of states, namely Texas, Delaware, and Alaska, where one county can be divided into more than one circuit-court district.

[5] 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).

channels. There is at least anecdotal evidence for both. See the "kids for cash" mentioned previously, 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 (Brickner and Diaz, 2011).

Our identification strategy specifically conditions out the 'influence-on-legislature' channel: The vast majority of state-laws come into effect on January 1st every year. As such, they are absorbed by our inclusion of state-year fixed effects. By contrast, we track changes in prison capacity by month so that we identify from within-state-within-year changes in them. Therefore our results cannot be explained by changes in legislation (which does not imply that this channel is not present).

We can also rule out one particular variant of the 'influence-on-judiciary' channel: Judges in most U.S. states are elected and there is considerable evidence that this introduces electoral cycles into their sentencing, which tends to become harsher 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; Lim, 2013; Berdejó and Yuchtman, 2013). Indeed, we find fairly strong electoral cycles in most states where we have judge identifers. We then ask whether such electoral-cycles in sentencing are more pronounced in the presence of private prisons. This might be the case if private prisons are either significant contributors to judges' re-election campaigns or alternatively seek to directly mobilize the electorate, e.g. through their own advertisements. Interestingly, we find no evidence whatsoever that electoral cycles respond to state-level changes in private prisons. This is of course not to say that private prisons do not potentially influence judges, but merely that this influence does not follow judges' electoral cycles.

As a last exercise, we study 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). There are several possible reasons for this, but critics of the system have advanced in particular that private prisons prefer minority prisoners because they are less likely to litigate against prison mistreatment (Petrella and Begley, 2013). At the same time, there is pretty compelling evidence of racial biases in sentencing, over and above biases introduced by policing and legislation; see Abrams, Bertrand, and Mullainathan (2012) and references therein. While it may well be true that private prisons prefer and disproportionately house inmates of a specific demographic, we find no evidence that the presence of private prisons changes racial biases in sentencing (but we do indeed find evidence that such biases exist).

In summary, we find robust evidence that the opening (closing) of private prisons in a state makes sentencing harsher (less harsh) in that state. The intensive-margin effect on sentence length is statistically much more significant than the extensive-margin effect on being sentenced. While the effect is likely to operate through private prisons exerting political influence, we specifically rule out that legislative changes as a driver, and we rule out that this effect ebbs and flows with judges' electoral cycles. Lastly, we find no evidence that such biases transmit into more pronounced racial biases in sentencing.

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; 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 future recidivism, while Thomas (2005) finds the opposite results in the same data. More recently, Mukherjee (2015) shows no statistical effect of private prisons on recidivism.

The remainder of the paper is organized as follows. Section 2 introduces the history of U.S. private prison system. Section 3 describe data data sources and data construction. Section 4 presents our identification strategy and empirical specifications. Section 5 contains our results and investigates judicial electoral cycle as a mechanism. 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. Our empirical focus on states' circuit-courts implies that we are focused on 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.[6] 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.[7] In 1984 the *Corrections Corporation of America* (hereafter CCA), was awarded its first contract to fully manage a facility in Hamilton County, Tennessee.[8] 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.[9]

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

[6] 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).

[7] See `www.theguardian.com/society/2015/may/20/misconduct-youth-jail-rainsbrook-ofsted-g4s`.

[8] 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.

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

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

Ethnic and Race-Based 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.[10] For-profit prisons are alleged to favor minority inmates, particularly blacks, because they are seen as less likely to litigate over poor prison management.[11] 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.[12] 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.

Reports from the *Council on Hemispheric Affairs* indicate some of the ways in which private prisons corporations have tried to influence incarceration rates and immigration policies.[13] There are anecdotes that private prisons do try to influence judges' decisions: in 2008 *Mid-Atlantic Youth Services Corp*, a private prison company which runs juvenile facilities, was found guilty of paying two judges, Mark Ciavarella and Michael Conahan, $2.8 million to send 2000 children to their prisons for such crimes as trespassing in vacant buildings and stealing DVDs from Wal-Mart.[14]

2.3 Private Prisons' Influence on Government

According to the Justice Policy Institute, private prisons use three strategies to influence policy: lobbying, direct campaign contributions and building relationships through the 'revolving door' (Ashton and Petteruti, 2011).

[10] 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.

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

[12] http://grassrootsleadership.org/reports/payoff-how-congress-ensures-private-prison-profit-immigra

[13] www.truth-out.org/news/item/30804-how-us-private-prisons-profit-from-immigrant-detention

[14] www.nytimes.com/2009/03/28/us/28judges.html.

Lobbying: The CCA and GEO Group have lobbied Congress as well as state legislatures on issues related to management and construction of privately operated prisons and detention facilities, and appropriations for both BOP and 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) highlight the many former legislators among CCA's board of directors that have have no experience in corrections industry expertise but instead provide access: Former United States Senator Dennis DeConcini (D-AZ); former Reagan administration official Donna M. Alvarado; former Clinton administration official, son of Supreme Court Justice, and civil rights icon Thurgood Marshall Jr.; and the President of the Freedom Forum, Charles L. Overby, provide bipartisan political access cover. For instance the relationship between

[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-bigges

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

government officials and private prison companies has been part of the fabric of the industry from the start; Tom Beasley, one of the founders of CCA, was a former government official in Tennessee. Other examples include relationships between. Stacia Hylton (Director of the U.S. Marshals Service and the GEO Group), John Kasich (Lehman Brothers and CCA) and the former CCA Warden Joe Ponte and Maine Department of Corrections[18].

3 Data Sources and Construction of Samples

In this Section, we provide brief description of the data used throughout the paper. Details regarding variable construction and the data cleaning process are provided in Appendix A.

3.1 Sentencing Data

To gather sentencing data we requested most of the U.S. states' Sentencing Commissions and Departments of Corrections. The length of the sentencing data varied by state and ranges from 1880 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.[19]

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 turn these information in fixed effects and create a state-specific set of dummies for each value of crime severity and recidivism.

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] www.allgov.com/officials/hylton-stacia?officialid=28491, www.cleveland.com/open/index.ssf/2011/01/gov-elect_kasich_picks_private.html, and www.centralmaine.com/2011/04/04/private-prison-debate-continues_2011-04-03/.

[19] 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: Variation in opening/closing of private and public prisons

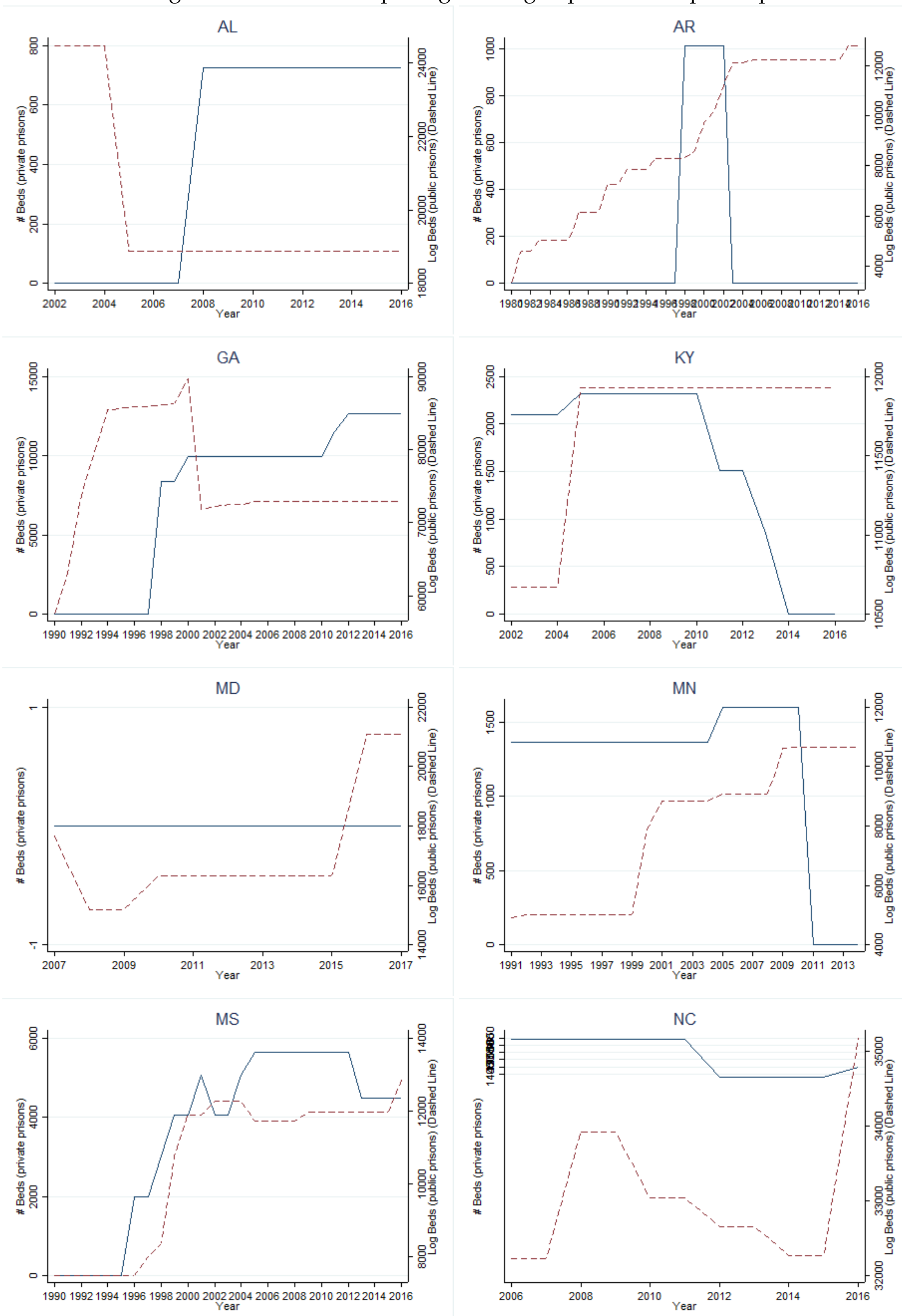
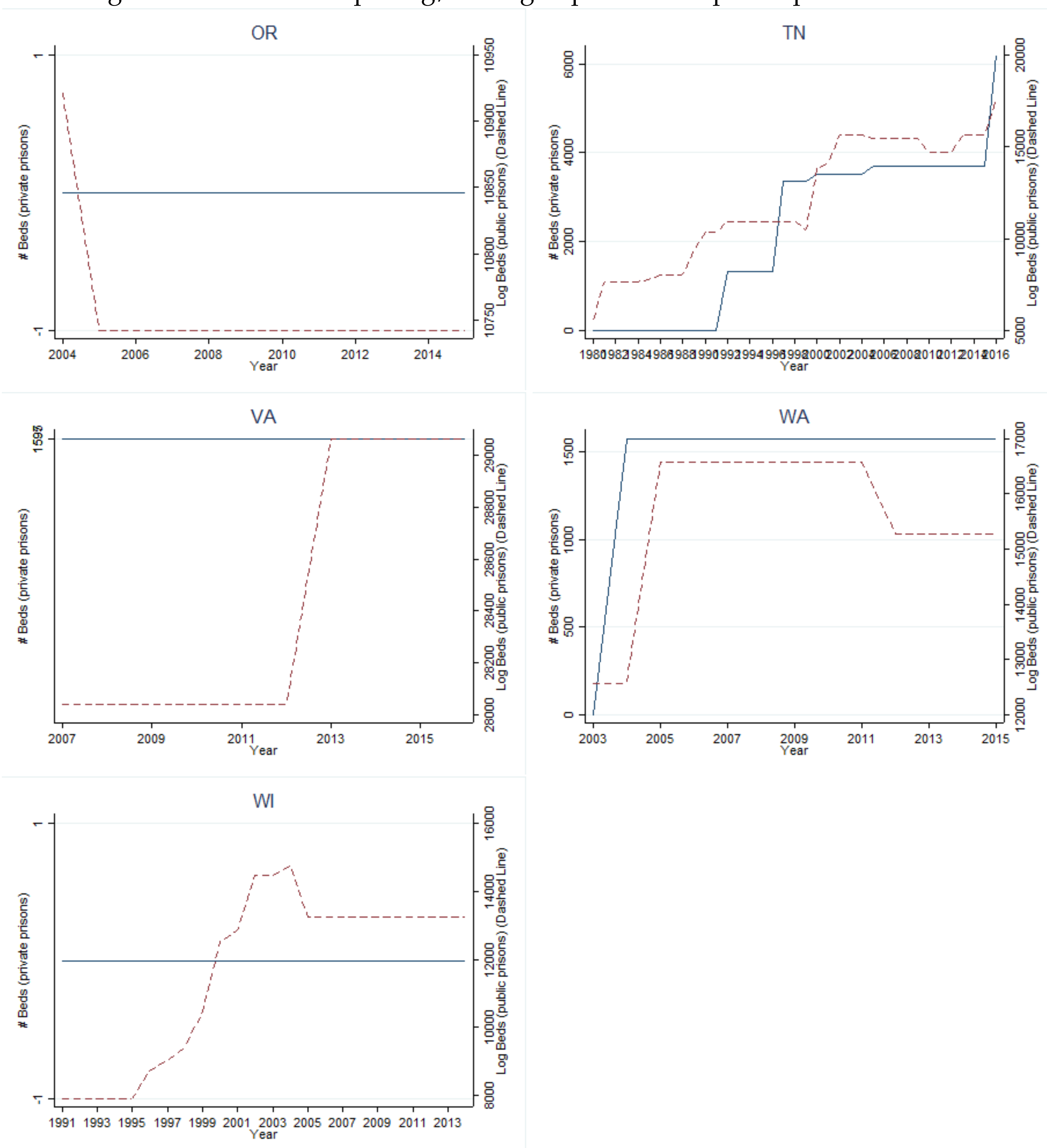


Figure 2: Variation in opening/closing of private and public prisons – continuation



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).

3.2 Prison Data

Prison data is constructed from several sources. First, we use the 2005, 2000, and 1995 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 addded 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 1 and 2 demonstrate the resulting variation in private (solid) and public (dashed lines) prison capacities.

3.3 Location Selection

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.

3.4 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.[20]

[20] 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.

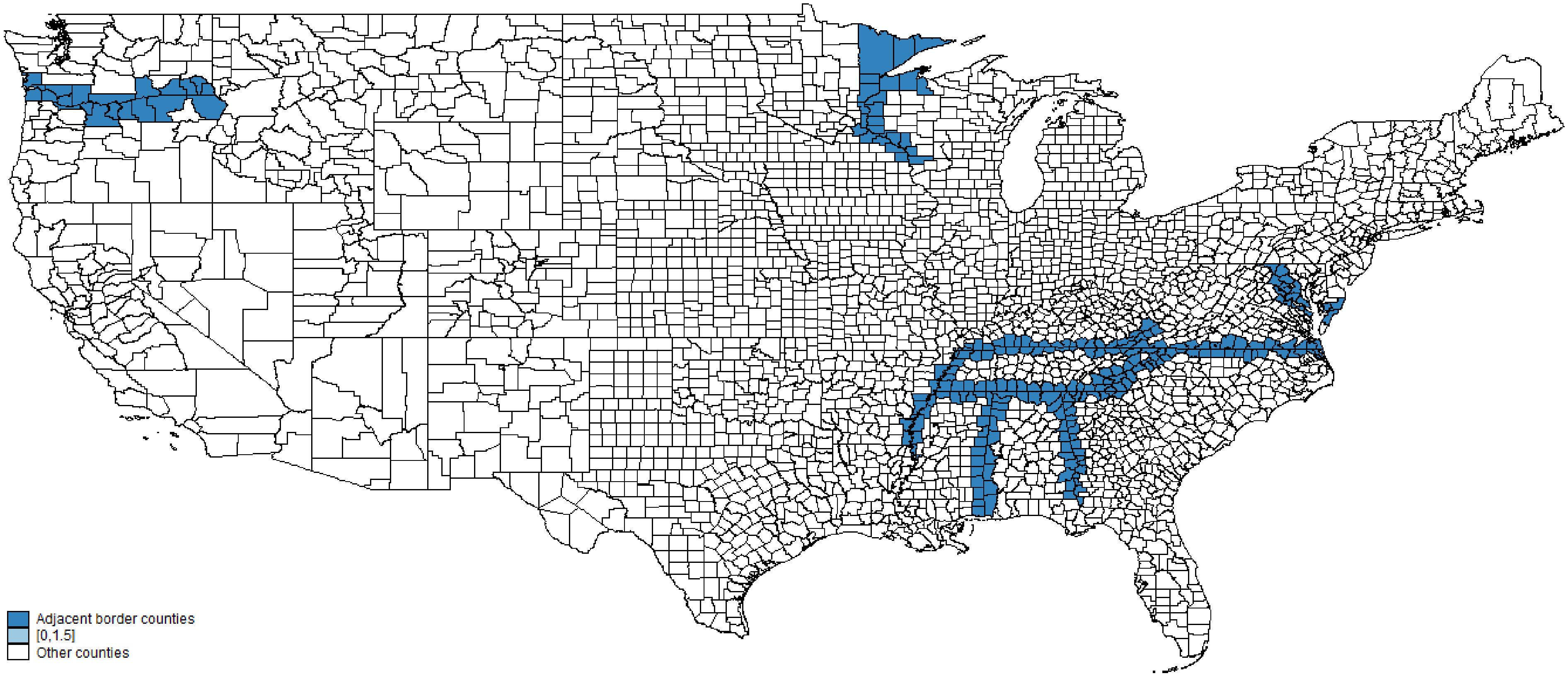
3.5 Sample Construction

We obtained sentencing data from the following states in our analysis:

1. Alabama
2. Arkansas
3. Georgia
4. Kentucky
5. Maryland
6. Minnesota
7. Mississippi
8. North Carolina
9. Oregon
10. Tennessee
11. Virginia
12. Washington
13. Wisconsin

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. Our sample covers 454 border counties or 252 distinct county-pairs. See Table 1. Figure 3 shows the 454 counties on a map.

Figure 3: Contiguous-Border County-Pairs in our Sample



Note:

Table 1: Contiguous-border county-pairs

Segment	pairs		#counties		#pairs	data overlap		#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			
8	NC	GA	4	4	7			
9	AL	GA	11	17	27			
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:	14		122	145	252			

4 Empirical Model and Identification

4.1 Empirical Specification

There are compelling reasons for using county-pairs bordering states when identifying the effect of state-level policy changes. See Dube et al. (2016) for a very nice 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. The border-county sample is therefore the basis of our empirical investigation. Nonetheless, as is standard, we present results for the border-county sample as well as for the full sample. The combination of possible fixed effects that can be included in any specification varies across these two samples so that we present specifications for the two samples separately in equations (1) and (2). In the full sample, the baseline regression specification is

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

in the border-county sample, the baseline regression specification is

$$\text{Sentence}_{i(ct)} = \beta^{\text{T}} \cdot \textit{PrivateC}_{st} + \beta^{\text{T}'} \cdot \textit{PublicC}_{st} + \beta^{\text{X}} \cdot X_i + \mu_{st} + \Psi_{p(c)} + \Psi_{p(c)t} + \epsilon_{icts}. \tag{2}$$

Case i is heard in circuit-court c (belonging to state s), and i's sentence is passed in month m in year t. Our main outcome–Sentence$_{i(ct)}$–is the length of a sentence (in log months). Each case i is always uniquely mapped to a circuit-court in a year-month, a circuit-court is almost always a county. Our second main outcome $Incarceration_{i(ct)}$) is an indicator variable equal to unity if person i is incarcerated and zero otherwise.

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 up to 60% of a sentence's length). X_i can also include age, age squared, and race of defendant as controls.

μ_{st} can stand for a number of broad time controls. In the coarsest case, it can be a set of common year fixed effects, which reflect broad changes in sentencing behavior nationwide, such as those brought about by the War on Drugs, or changes in administration and the Attorney General's office. To control for state-specific time trends, μ_{st} can also be a set of state-specific linear trends, reflecting state-specific changes in legislation as well as sentencing behavior.

In the full-sample specification in (1), μ_c are circuit-court fixed effects, and μ_{ct} are circuit-court time-trends. In the border-pair sample used for specification (2), $\Psi_{p(c)}$ are fixed effects for border-county-pairs (where $p(c)$ denotes the county-pairs in which county c is contained), and $\Psi_{p(c)t}$ are border-county-pair time-trends.

Our main variable of interest $\textit{PrivateCapacity}_{ct}$ 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.[21] Public prison capacity $\textit{PublicCapacity}_{ct}$ is a log of number of beds in public prisons that we add to account for the baseline level of prison capacities as it will absorb direct effects that prisons can create on local-labor markets.

[21] In fact we use inverse hyperbolic sin ($\log(y_i + (y_i^2 + 1)^{1/2})$) instead of log transformation. It is approximately equal to $\log(2) + \log(y_i)$, so it can be interpreted in exactly the same way as a standard logarithmic variable but without doing $\log(1 + y_i)$ (Burbidge, Magee, and Robb, 1988).

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

4.2 Identification Using Only Contiguous Border Counties

Our core identification strategy in (2) relies on comparing changes within circuit-court-pairs that straddle a state border. The core of what this sample selection achieves is to properly control for localized trends in criminal activity as well as in sentencing. Contiguous counties are relatively similar to each other, are comparable in local conditions that can affect sentencing decisions, and hence form better controls with respect to underlying crime and sentencing trends. To identify state-level treatments such as ours, one then needs to focus on contiguous county pairs that straddle state boundaries. Border-pair comparisons have been used among others in Holmes (1998), Huang (2008), and Dube et al. (2016).

Table 2 provides descriptive statistics for our sample. Comparing the full set of counties (column I) to the contiguous-border sample (column II) we find that they are similar in economic outcomes and sentencing behavior. Thus our results on the contiguous-border are likely to be externally valid.

5 Results

This section is structured as follows. In section 5.1 we present the core results of estimating equations (1) and (2). In section 5.2 we investigate the effect of private prisons on electoral cycles in sentencing. 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. Columns I–III contain results for the full-sample specification in equation (1), and Columns IV–VIII contain results for the contiguous border counties' sample specification in equation (2).

Column I reports results for the specification with county and state-year fixed effects. Our only control is log of public prison capacities. The resulting coefficients on private prisons is insignificant and negative. Column II includes demographic and case controls. In particular, we control for age, age squared, and race (Asian, Black, Hispanic, Native American, and Other), dummy for recidivists and state-specific set of crime severity dummies. These are viewed as important in the literature but do not add very much explanatory power in our case. Column III includes county-year fixed effects, which in principle move this specification closer to the spirit of the border sample, which is to control for local trends in crime and punishment. However, in this data results do not change much in column III. In summary, a conventional full-sample specification with fixed effects suggests no effect of private or public prisons on sentencing length.

In Column IV, we show results for the specification with county-pair and state-year fixed effects on the sample of contiguous-border counties. The coefficient for private prison capacities become positive, and significant. Both dependent and explanatory variables are in logs so that coefficients are elasticities: one one-percent increase in private prison capacities increases length of sentencing by 1.7 percent. In column V, we add case and demographic controls. These add explanatory power without affecting our coefficient of interest. column VI–VII are our preferred specifications, using only border-counties, and allowing for local (i.e. border-county-pair) time-trends. In column VI, we add pair-specific linear trends, and in column VII we include pair-specific year fixed effects.

Table 3: Private Prisons and Sentence Length (the 'Intensive Margin')

	I	II	III	IV	V	VI	VII
	Dependent variable: Sentence (log months)						
	Full Sample			Sample of Adjacent Border Counties			
Log private prison capacity	-0.002	-0.002	-0.002	0.017**	0.015**	0.015*	0.017**
	[0.2823]	[0.1658]	[0.2711]	[0.0195]	[0.0458]	[0.0563]	[0.0220]
Log public prison capacity	-0.169	-0.156	-0.185**	0.003	0.012	-0.053	-0.092
	[0.1103]	[0.1036]	[0.0347]	[0.9919]	[0.9668]	[0.8522]	[0.7378]
state-specific year-fixed effects	X	X		X	X	X	
Demog. & case controls		X	X		X	X	X
county(-pair)-year f.e.			X				X
pair-specific year-trend						X	
county(-pair) f.e.	X	X		X	X	X	
R-squared	0.335	0.338	0.351	0.335	0.394	0.397	0.404
Observations	3,301,011	3,301,011	3,300,047	683,248	683,248	683,248	683,188

Notes: Columns I–III use the full sample of all circuit-courts in each state. Columns IV–VII is our preferred border county sample, which contains only circuit-courts that straddle state-boundaries, allowing us to effectively control for local trends in both crime and sentencing. In the table notation, "county(-pair)" implies county in columns I–III, and county-pair in column IV-VII. Columns II–III and V-VIII include controls for case characteristics (severity) and defendant characteristics (recidivism and age) Our preferred specifications in columns VI–VII use only border-counties, include case and defendant characteristics, and allow for local (i.e. border-county-pair) time-trends In square brackets we report p-values for standard errors are clustered on state (10 clusters) and border segment (19 clusters). *** $p<0.01$, ** $p<0.05$, * $p<0.1$

Table 4 contain results for the same specifications but with indicator variable for the defendant being incarcerated as a dependent variable. The results follow the same pattern as Table 3, at overall lower levels of statistical significance: The coefficient on private prisons flips from column I–III to column IV-VII. Columns IV-VII suggest that a one standard-deviation increase in prison-capacity (log beds=4.5) increase the likelihood of being sent to prison by a little over one percent (4.5×0.002). This effect is consistently borderline insignificant, suggesting (perhaps reassuringly) that the influence of private prisons on incarceration operates at the intensive margin more than at the extensive margin.

Since our estimation strategy controls for local trends in crime and sentencing as well as controls in overall 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 (the 'influence-on-legislature' channel). Our identification strategy specifically conditions out the 'influence-on-legislature' channel: This is because the state-laws overwhelmingly come into effect on January

Table 4: Private Prisons and the Probability of Incarceration (the 'Extensive Margin')

	I	II	III	IV	V	VI	VII
	Dependent variable: 1(Incarceration)						
	Full Sample			Sample of Adjacent Border Counties			
Log private prison capacity	-0.000	-0.000	-0.000	0.002	0.002	0.002	0.002
	[0.4763]	[0.4763]	[0.6261]	[0.1226]	[0.1432]	[0.1835]	[0.1708]
Log public prison capacity	0.010	0.010	0.010	0.081	0.081	0.078	0.055
	[0.5809]	[0.5809]	[0.6522]	[0.2087]	[0.2295]	[0.2472]	[0.4277]
state-specific year-fixed effects	X	X		X	X	X	
Demog. & case controls		X	X		X	X	X
county(-pair)-year f.e.			X				X
pair-specific year-trend						X	
county(-pair) f.e.	X	X		X	X	X	
R-squared	0.262	0.265		0.251	0.253	0.274	0.286
Observations	3,301,011	3,300,144	3,300,047	683,248	683,248	683,248	683,188

Notes: Columns I–III use the full sample of all circuit-courts in each state. Columns IV–VII is our preferred border county sample, which contains only circuit-courts that straddle state-boundaries, allowing us to effectively control for local trends in both crime and sentencing. In the table notation, "county(-pair)" implies county in columns I–III, and county-pair in column IV-VII. Columns II–III and V-VIII include controls for case characteristics (severity) and defendant characteristics (recidivism and age) Our preferred specifications in columns VI–VII use only border-counties, include case and defendant characteristics, and allow for local (i.e. border-county-pair) time-trends In square brackets we report p-values for standard errors are clustered on state (10 clusters) and border segment (19 clusters). *** p<0.01, ** p<0.05, * p<0.1

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.

5.2 Using Judges' Electoral Cycles to Elicit Mechanism

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; Lim, 2013; Berdejó and Yuchtman, 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 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

Table 5: Private Prisons and Judges' Electoral Cycles

	I	II	III	IV	V
	Dependent variable: Sentence (log months)				
Log private prison capacity	0.015*	0.014*	0.014*	0.017**	0.014**
	[0.0523]	[0.0501]	[0.0502]	[0.0183]	[0.0391]
Log public prison capacity	-0.264	-0.158	-0.168	-0.105	-0.183
	[0.4095]	[0.6206]	[0.5959]	[0.7387]	[0.5652]
Tenure			0.009***	0.009***	0.009***
			[0.0000]	[0.0000]	[0.0000]
Distance to election				0.005**	0.029***
				[0.0500]	[0.0111]
Log private prison capacity					-0.001
x distance					[0.3433]
Judge FE		X	X	X	X
R-squared	0.422	0.424	0.424	0.424	0.424
Observations	683,188	683,188	683,188	683,188	683,188

Notes: All columns contain constant, adjacent-county pair dummies (155), pair-specific linear trends, year-state fixed effects, month fixed effects. The following variables are used as controls: age, and race dummies (Black, Hispanic, Asian, Naive American, and other). In square brackets we report p-values for standard errors are clustered on state (10 clusters) and border segment (19 clusters). *** p<0.01, ** p<0.05, * p<0.1

sentencing electoral cycle to be more pronounced in the presence of private prisons. Define as $\tau(j)$ the quarter left to the end of judge j cycle, i.e. $\tau(j) = 0$ in the last year of a judge's term. A natural extension of specification (2) is to regress

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

where $\mu_{\tau(j)}$ is the "distance" in the remaining quarters to the re-election of judge j's term. Judges get elected every four years. 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.[22] 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 $\beta^{\text{T}}_{\mu_{\tau(j)}} > 0$.

We present our results in Table 5. In Column I, we re-report the baseline result of column V in Table 3 for comparison. In Column II, we add judge fixed effects. In Column III, we add

[22] Alternatively, 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. This generates very similar results after re-scaling.)

judge's tenure length. More senior judges appear significantly less lenient in this date, although this adds little explanatory power overall. Column IV is the first specification that checks for an electoral cycle in sentencing and indeed we find evidence for this in our data, although again this adds little explanatory power overall and does not affect the magnitude or significance of the private-prison's coefficient it is positive and significant[23] In Column V, we add the interaction of the private prison capacities and distance-to-election. While the separate private-prison and electoral-cycle coefficients remains positive and significant, their interaction is insignificant and negative.

In summary, while judges indeed demonstrate increased harshness on crimes before reelection, private prisons do not 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.

5.3 Heterogeneous Effects of Private Prisons on Minorities

We lastly test the often voiced hypothesis that private prisons tilt sentencing more for defendants of particular demographics, particular Black and Hispanic defendants who are viewed as less litigious against bad prison conditions, and younger defendants who are viewed as cheaper as they require less healthcare (Austin and Coventry, 2001; Petrella and Begley, 2013). We operationalize this idea by defining a set of indicators μ_i for demographic characteristics that may make i a more attractive prisoner. A natural extension of specification (2) is to regress:

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

where μ_i is an "attractive" prisoner's characteristics. Specification (4) can be thought of as generalized triple-differences strategies which study the generalized difference-in-differences effect of T_{ct}, conditional on i's characteristics.

Table 6 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-

[23] Berdejó and Yuchtman (2013) was based on Washington State only. Washington State is in our data and electoral cycles indeed appear to be by far the strongest in that state.

Table 6: Private Prisons and Racial Biases in Sentencing

	I	II	III	IV	V	VI	VII	VIII
	Dependent variable: Sentence (log months)							
Characteristic:	White	Black	Asian	Hispanic	Native Am.	Age	Age(>50)	Age(<30)
Log private prison capacity	0.009	0.018*	0.015*	0.015*	0.015*	0.014*	0.014*	0.014*
	[0.3549]	[0.0821]	[0.0737]	[0.0689]	[0.0693]	[0.0871]	[0.0862]	[0.0977]
Log private prison capacity x characteristic	0.010	-0.011	0.007	-0.001	-0.017	0.000	0.022**	0.003
	[0.2573]	[0.3253]	[0.6543]	[0.9704]	[0.5923]	[0.7509]	[0.0373]	[0.5136]
Characteristic	0.026	0.251***	-0.049***	0.042***	0.229***	0.004***	-0.441***	-0.197***
	[0.060]	[0.0001]	[0.0003]	[0.0036]	[0.0000]	[0.0019]	[0.0015]	[0.0011]
Log public prison capacity	-0.117	-0.118	-0.108	-0.108	-0.108	-0.111	-0.112	-0.111
	[0.7004]	[0.6984]	[0.7187]	[0.7187]	[0.7197]	[0.7107]	[0.7111]	[0.7113]
R-squared	0.414	0.414	0.414	0.414	0.414	0.414	0.414	0.414
Observations	692,714	692,714	692,714	692,714	692,714	692,714	692,714	692,714

Notes: All columns contain constant, year-pair dummies. The following variables are used as controls: age, and race dummies (Black, Hispanic, Asian, Native American, and other), dummy for recidivists and state-specific set of crime severity dummies. In square brackets we report p-values for standard errors are clustered on state (10 clusters) and border segment (19 clusters). *** p<0.01, ** p<0.05, * p<0.1

istics. We do find evidence that is suggestive of racial biases in the system: The coefficient on Asian is negative and that on Hispanic, Black, and Native American are positive, relative to the white baseline. However, we find no evidence that these racial biases respond to the presence of private prisons: The interaction of private prisons with demographics is always insignificant. Throughout, the coefficient for the effect of private prisons remain significant and similar in magnitude to one in the baseline specification (Column VII of Table 3).

Thus, while it may well be true that private prisons prefer and disproportionately house inmates of a specific demographic, we find no evidence that the presence of private prisons changes racial biases in sentencing.

6 Conclusion

TBA

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Appendix A Data Appendix

Appendix A.1 Sentencing Data

TBA: Details of Severity and Recidivism Coding.

Online Appendix

to

"Do Private Prisons Affect Court Sentencing?"