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Three Essays in Law and Economics: Consequences and Formation of Legal Rules in Other Markets

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Approval of the Dissertation Committee

This proposal has been duly read, reviewed, and critiqued by the Committee listed below, which hereby approves the manuscript of Minjae Yun as fulfilling the scope and quality requirements for meriting the degree of Doctor of Philosophy in Economics.

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Abstract

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By
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In this thesis, I study the consequences and formation of legal rules in the "non-traditional" markets to show that non-traditional markets are fundamental to a society's functioning. The first chapter shows how legal participants strategically behave based on judicial disqualification. The second chapter examines the unintended consequence of a decarceration policy in the criminal-justice system by linking the ex-criminal re-entry issue and homelessness. Lastly, the third chapter concentrates on how the tenure of elected law enforcement agents contributes to the jail-crowding environment, leveraging term limits imposed on local county elected officials.

The first chapter examines the use and impact of peremptory challenges against judges in the California courts. Under California law, a judge cannot preside in a case when the judge is prejudiced against a party or attorneys who are involved in the case. A "peremptory" challenge does not require an attorney to show cause, such as a judge who has a personal interest in the case, nor is it necessary for the party requesting the peremptory challenge to prove that the judge is biased. Moreover, no factual basis for the claim of bias is even required. The party simply has to state that he or she believes the judge is prejudiced against him or her and that they do not believe they can receive a fair trial. Finally, judges cannot oppose challenges made in a timely manner. Upon the filing of a peremptory-challenge motion, the judge loses control of the case, and any rulings are voided. The major difference with peremptory challenges in juries is that in the case of judges, each side of the case is allowed only one peremptory challenge. This work bases the decision to challenge on the likelihood a potential replacement judge who could be randomly assigned the case if the attorney challenges the judge is more likely to be favorable to the challenger side of the case. The results show that peremptory challenges are used strategically to remove judges when the replacement is more likely to be against the opponent's preferences in terms of ideology or past

rulings on motions.

The second chapter examines the effect of re-sentencing policies as a means of decarceration on community well-being. In 2011 and 2014, California passed jail decarceration policies, *AB 109* and *Prop 47*, respectively. *AB 109* reallocated state prison inmates into local county jails. On the other hand, *Prop 47* reduced penalties for non-serious property crimes, thereby providing a second chance for offenders who committed specific non-violent crimes, while lowering the burden on county jails by shifting offenders into local communities. My results indicate that *Prop 47* increased the homeless population and health-related governmental spending but did not reduce governmental spending on corrections. Furthermore, California jail-disposition data show heterogeneous effects on recidivism. For example, *Prop 47* decreased recidivism rates for *Prop 47* charges (non-serious and non-violent charges) after *AB 109* increased the rates in county jails. However, *Prop 47* failed to lower recidivism rates for control-group charges (more severe than *Prop 47* charges) after *AB 109* raised these rates in county jails. Finally, I find that *Prop 47* raised non-violent crime rates, utilizing Los Angeles crime data, especially among non-homeless offenders.

The third chapter strives to find the cause of jail and prison overcrowding. Calls for criminal justice reform have become commonplace, as issues ranging from ethnic and racial bias in policing to prison overcrowding have taken center stage in many policy discussions. The demand for change in the criminal justice system largely falls on the shoulders of leadership within the criminal justice system who might have their own preferences toward criminal justice reform. This work examines the issue of sheriff tenure on jail occupancy rates, as jails are directly managed by the sheriff and capacity issues in jails have resulted not only in safety and security problems for inmates and staff but also in fiscal stress. Utilizing a unique institutional feature - that 192 county sheriffs are limited to no more than two terms in the position - the work employs an instrumental variable identification strategy to examine the impact of sheriff tenure on jail occupancy rates.

Acknowledgments

Gregory DeAngelo is the first person who believed in my ability to research in the United States when I had nothing to prove, and he gave me various opportunities. I am grateful to have the experiences that I would not have had but for him. Greg then introduced me to Eric Helland, and I was given more opportunities to be involved in active research. It has been inspiring to discuss with him. It was sometimes hard to follow his sharp thinking processes, and I had to stop and think twice to understand them. And I know these interesting discussions will continue. Next, I thank Melissa Rogers for her thoughtful explanations and knowledge about how the political system works. I also thank CarlyWill Sloan for her intelligent comments on research details and her need-based student support.

Greg created the Computational Justice Lab, and Maryah Garner became the horsepower of the lab. She had been a definitive leader in fostering a friendly and productive research environment, and I was encouraged by it. I got to know Uyen (Yunie) Le, Jiusi (Josie) Xiao, and Rainita Narrendar. It was nice to have such kind people in the lab. We also had finance people in the lab, one of which was my first friend in the United States, Rebecca Bommarito (now Ferguson). It was pretty fun to study with her every day and night, and it became a priceless memory to be insane together.

Fellows from my undergraduate university have been a support group. Every one of the four members studies social sciences across the United States as a foreigner. Our locations are geographically balanced in that two members live in the west, and the others live in the east. Thus, exchanging ideas was helpful to understand the general characteristics of life in the United States. Followingly, Jieun Hurt, Dasom Jang, and Yoonyoung Choi became the ones I could talk to and ask for an opinion without hesitation.

Claremont Korean Student Association was a platform to form many groups for Koreans, and I enjoyed the K-pop dance time. Also, it would have been much more challenging to get accustomed to life in the United States without Sunku Kang, Jennifer Choi, and Hae Won Byun. Five people became an occasional hangout group during the pandemic, including Minji Cho, Ayoung Seok, Sekwen Kim, and Seong-gyu Park. I appreciate all the unnecessary and unimportant discussions with boozes.

Another group of people formed a pandemic group. Housemates and neighbors started a Covid pod, including the landlord Alan Medak, Collin Ring, Anne Bai, Riki Wolf, Ken Wolf, and Dan DeChaine. I learned the chef's spirit from Alan. I am blessed to have the fresh vegetables and dishes from Wolf's garden. Dan, the great fencer and engineer, made me wonder about the world with all the lively stories across European and American countries.

Lastly, I appreciate my mother, father, and sister supporting me. I inherited the family heritage enabling me to reason and exert massive energy whenever necessary.

Contents

1 Chapter 1: Nixing the Judge: Peremptory Challenges of Judges and Strategic Behavior

Coauthored with Eric Helland

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1.1 Introduction

One of the bedrock principles of judicial independence is that a litigant does not get to choose his or her own judge. Concerns with form shopping and judicial elections are long-standing features of the literature on civil justice cases (see Atkinson et al. (2009) or White (2006)). The independence of the judiciary is balanced with a concern that judges not be biased for or against one party. For this reason, judges can be disqualified for having a financial interest in a case or a relationship with one or more of the parties.[1] Institutions for assigning judges to cases attempt to balance the competing tensions of independence and bias. The balance is often difficult to attain. Making it easier to disqualify a judge makes it easier for litigants to influence who will hear their case while making it harder to disqualify a judge makes it more likely a litigant will be stuck with a biased judge. In this paper we examine California's use of peremptory challenges against judges. California's institutional solution to selection and challenge has made it easier to remove judges who are perceived by one or the other party to be biased. The question is whether that shift has reduced judicial independence by allowing litigants to strategically remove judges who, while not being biased in the traditional sense, may be less favorable to one side.

The role of peremptory challenges in assuring a fair and impartial jury and the potential problems that the peremptory-challenge system produces in terms of the composition of jury has been widely studied.[2] While the impartiality of judges has a similarly vast literature (see Epstein et al.

[1] In California the code of civil procedure, section 170.1, states that a judge can be removed for cause if she has personal knowledge of the case, has served as a lawyer or advised either party in the case, has a financial interest in the case, has a spouse who is a party to the case, or has a relationship with the lawyers in the case. The statute also allows a judge to remove himself if his recusal would "further the interests of justice" or there is substantial doubt about his ability to be impartial.

[2] See, for example, Flanagan (2015) or Anwar et al. (2012).

(2013) and Helland (2019)) there is far less literature of the role of peremptory challenges of judges.[3] Seventeen states allow litigants to peremptorily challenge a judge. Like peremptory challenges in jury selection, these challenges do not have to be for cause and can merely assert that a judge is biased. The motion automatically results in the removal of the judge if the challenge is timely.

While all states use some form of random assignment to ensure judicial independence, states are largely free to adopt whatever system they choose to prevent bias (Menendez and Samuels (2016)). The most common system is that lawyers or litigants must challenge the judge with a specific allegation of bias.[4] All states and the federal system include several common factors such as requiring mandatory recusal if the judge has a financial interest in the case or if the judge has a relative with an interest in the case.[5] These rules arguably leave considerable gaps between what litigants perceive as bias on the part of judges and the factors that would disqualify a judge from hearing a case. For example, in 2016, the Supreme Court ruled in *Williams v. Pennsylvania* [6] that the Due-Process Clause of the Fourteenth Amendment requires recusal if there is a "serious" risk of judicial bias. Williams is the second case in which the court has made such a ruling. In the 2009 case *Caperton v. Massey*, the court issued a similar ruling. In *Williams*, the issue was that the judge had overseen the case as the district attorney before joining the bench. In the *Caperton* case one of the litigants was a major campaign contributor to the judge's reelection campaign.[7] Although the Supreme Court has not addressed what administrative mechanism is constitutionally required to insure an impartial judiciary, both cases suggest that the state courts, at least in some instances, have come up short in their efforts to insure that an impartial judge hears a specific case.

There are reasons to be concerned that the challenge system, even if a different party reviews the recusal motion, might be inadequate to assure an impartial judiciary. Even in states that do not have the challenged judge ruling on disqualification motions, lawyers and judges are often repeat players. Lawyers are forced to provide a reason why the judge is not impartial, which

[3]See Anderson et al. (2014) for a discussion for discussion of the impact of recusal rules on judges' financial holdings on the composition of the appellate judges available to hear appeals.

[4]Even within these challenge systems, important differences persist. For example, in 29 states the trial judge himself rules on the recusal motion with no opportunity for review.

[5]See Anderson et al. (2014) and Helland (2019).

[6]*Williams v. Pennsylvania*, 136 S. Ct. 1899 (2016).

[7]West Virginia elects its judges in partisan elections.

is an insult too many judges. These concerns have led to various proposals, dating back to the 19th century in order to provide a peremptory challenge system in the federal courts and similar proposals to implement such a system in states that do not already have a peremptory challenge system (Menendez and Samuels (2016)).

In the states that utilize them, peremptory-challenge systems exist in addition to the usual disqualification system for bias. A typical proposal is that each side in litigation, or each party if the litigation involves multiple parties, be allowed a peremptory strike of an assigned judge. The process is somewhat different from motions for disqualification. In the 17 states that have the peremptory challenge system, a litigant who files within a prescribed amount of time can ask for a judge to be removed without having to prove or even provide evidence that the judge is biased against them. In these states, litigants retain the option of challenging the judge through the disqualification process, meaning that litigants have essentially two methods of removing a judge: one certain and one subject to review. Proponents of peremptory challenges argue that they offer litigants a lower-cost method of dealing with bias. Further, because peremptory challenges do not require a documented cause for the disqualification, and hence do not have to make specific allegations about the judge's impartiality, they may make disqualification less threatening to judges.

Yet there is a fundamental tension between avoiding judicial bias and judge shopping.[8] The random assignment of judges is a bedrock of judicial impartiality since it assures that a litigant cannot pick the judge most favorable to his or her claim. Critics of peremptory challenges of judges raise many of the same issues which are raised against peremptory challenges when juries are involved.[9] The largest concern is that litigants may use peremptory challenges for strategic reasons rather than to avoid narrowly defined bias (Bassett (2015)). In some ways, the difference between strategically challenging a judge who is less favorable to a litigant's case versus challenging a judge who the litigant or her lawyer thinks will be biased is definitional. However, there are reasons to be

[8]The debate is long-standing in common law. See the famous quote attributed to Sir Edward Coke in the 17th century, "no man shall be a judge in his own case."

[9]The first, and possibly most benign, is that peremptory challenges impose an administrative burden (Flamm (2007)). Judges may have invested time and energy into a case and the forcing new judge potentially to duplicate this effort. Assigning a new judge is also not costless, as judicial assignment mechanism must balance random assignment with the inevitable variation in caseload that results from cases resolving at different times or being of different complexity.

concerned that peremptory challenges might allow litigants to alter the pool of potential judges.[10]

In this paper, we explore the impact of peremptory judicial challenges on the bench. We examine data on over 215 thousand civil cases in Los Angeles County, California, from 2012 to 2018. California has a peremptory-challenge system that has been in effect for decades. We draw our sample from civil litigation filed in the Stanley Mosk Courthouse in Los Angeles. Stanley Mosk is the main courthouse of the Los Angeles County court system and handles the vast majority, almost 80%, of the civil litigation in the county. Since replacement judges are drawn from judges assigned to this courthouse, we know the pool of potential replacement judges and we can examine the cognitive distance between assigned judge and the pool of potential replacement judges.

To capture the role of strategic behavior, we examine two factors. The first is the success of motions before the judge by plaintiffs or defendants in previous cases. The measure, the rate of successful rulings in the past by each side gives us a probability measure of drawing a judge who is less favorable to the opponent by challenging the current judge. We find that outlier judges, judges with higher pro-plaintiff or pro-defense scores relative to the distribution of potential replacements, are more likely to be challenged by the opposing side.

Our alternative measure of the judge's disposition toward the plaintiff or defendant is the judge's ideology score. Our hypothesis is that the lawyers peremptorily challenge judges whose ideology makes them more likely to favor one side or the other. We assume that conservative judges are more pro-defendant and liberal judges more pro-plaintiff. To capture this effect, we use the Database on Ideology, Money in Politics, and Elections (DIME) to identify ideology of the judge assigned to the case (see Bonica and Woodruff (2014)).

In our preliminary data analysis, we find evidence that judges are more likely to face peremptory challenges from both sides, consistent with the theory that peremptory challenges are used against "bad" judges. These challenges, often for reasons of incompetence or simply being unpleasant are more likely to be made by both sides. However, we also find that litigants are more likely to challenge judges when the probability of drawing a more favorable judge is higher, a finding that is consistent with the strategic use of peremptory challenges by litigants. Thus, the results are

[10]See Flanagan (2015), and the discussion below, of peremptory challenges in the context of juries.

consistent across both the motion and ideology measures. The results are consistent with a simple strategic model in which litigants challenge judges whom they reasonably consider unfavorable to their side of the litigation.

The rest of our analysis proceeds as follows. The next section provides background on the peremptory challenge system in California, the Los Angeles County court system, and the DIME database. Section 3 presents the data and summary statistics. Section 4 presents the empirical models and results, and Section 5 concludes.

1.2 Background

1.2.1 Peremptory Challenges and Disqualification

Since it was put forward in 1972, all 50 state and the federal courts have adopted the American Bar Association (ABA) Model Code of Judicial Conduct. Under the ABA model code, a judge should disqualify himself if his impartiality might be reasonably questioned Flamm (2007).[11] The factors that require recusal include personal knowledge of the case, a financial interest in the case, a close relationship with parties to the case or bias concerning one or another of the involved parties.[12] Recusals have both substantive and procedural components. Substantive rules are those in which the judges must disqualify themselves, such as the requirement that a judge recuse themselves whenever he or she might have a financial interest in the case Anderson et al. (2014). By contrast, procedural rules deal with who reviews and the standards for review of motions from the parties asking that a judge disqualify themselves. In 29 states and the federal system, the trial judge decides on recusal motions (see Menendez and Samuels (2016)).

The main concern with a disqualification process based on motions, which must cite a cause for the judge's bias, is that parties may be reluctant to challenge a biased judge due to fear of reprisal if the trial judge or another judge, in the case of the 21 states in which another judge evaluates the motion, finds against disqualification. Moreover, at the trial court level many lawyers

[11] In common parlance the words "disqualification" and "recusal" are synonymous. Some state statues refer to disqualification and the result of a motion by one of the parties while recusal is when a judge withdraws from a case independently. Federal statutes, by contrast, use only disqualification (see Geyh (2010)).

[12] All the ABA Model Code and the codes adopted by state and federal courts codified these criteria these standards were present in common law as well (see Burg (1981))

are repeat players and may be reluctant to challenge a possibly biasd judge who may hear one of their cases in the future. One alternative is to allow both parties a peremptory challenge of the judge, much as is the case with juries. These challenges would not require cause and, it is argued, would therefore be less likely to invite reprisals by the judge since they make no specific allegation of bias (Menendez and Samuels (2016)).[13] The Federal process, like the ABA model code, does not include a peremptory challenge, although periodically there have been proposals to allow litigants in Federal Court peremptorily to challenge the trial judge. Proponents of peremptory challenges typically argue that such challenges would increase the public's confidence in the impartiality of the judiciary.[14] Opponents of a peremptory-challenge system worry about "judge shopping" (see Burg (1981)) discussed further below.

The state processes concerning disqualification motions are substantively similar to the federal process with the notable exception, mentioned above, as to who decides on disqualification motions: the trial judge or another judge. The other notable difference in the disqualification process is that in 19 states a peremptory challenge process exists in addition to motions to disqualify a judge for specific causes. These processes typically allow each party one peremptory challenge and does not require the courts to evaluate the accuracy of a bias claim. Thus, the appeal of the peremptory challenge system is its simplicity.

The problem of biased judges who are not disqualified is potentially substantial and has been the subject of two recent Supreme Court cases. In 2016 the court ruled in *Williams v. Pennsylvania*[15] that the Due Process Clause of the Fourteenth Amendment required judges to be disqualified if there is serious risk of bias (Menendez and Samuels (2016)). In *Williams* the issue was that the Chief Justice of the state court had been the District Attorney at Williams' trial. In 2009 in *Caperton v. Massey* the Court issued a similar ruling in a case involving campaign contributions to a West Virginia Supreme Court Justice.[16]

[13] There is some concern that a peremptory challenge system has the opposite to the intended effect. If judges do not view peremptory challenges as pro forma, their existence may create ill feelings. It is unclear how we should weigh the feeling of judges but it is potentially a moral issue. Moreover, if judges take peremptory challenges as a personal affront and potentially retaliate against lawyers or other lawyers from the same firms who make them.

[14] Proposals were also introduced in the 1980s, 1970s (the so-called Bayh Bill) and even the 1880s (see Burg (1981)).

[15] 579 U.S. (more)136 S. Ct. 1899; 195 L. Ed. 2d 132

[16] *Caperton v. A. T. Massey Coal Co.*, 556 U.S. 868 (2009)

Others have argued that the problem is not merely an unwillingness of parties to challenge potentially biased judges but that the criteria for bias are in fact too narrow and parties need a broader ability to remove "bad judges" Miller (2004). The costs of "bad judges" should not be underestimated. Beyond bias, Miller lays out a number of criteria for what he calls "bad judges." These range from corruption, to incompetence, to abuse of authority, to personal misconduct. Miller points out that not all of these would be grounds for disqualification even if they might be grounds for disciplinary proceedings. Litigants do not have the ability, absent a peremptory challenge to avoid a bad judge who does not fall into one of the four categories necessary for recusal.[17] It is worth noting that removing a bad judge by Miller's definition, would not be strategic. There is no reason to suppose that the replacement judge would be predisposed in the parties favor, only that he or she would be less likely to be incompetent or abusive.

California, like 19 other states has an additional mechanism for removing a judge, the peremptory challenge system, also known as a 170.6 challenge for the section of the California code of civil procedure that outlines the mechanism. Under the system, the party files a motion that the judge is biased in that the judge is prejudiced against him and he cannot receive a fair trial.[18] The party does not have to assert any factual basis for this claim and the judge cannot oppose the motion and a new judge is immediately assigned to the case with all of the previous judge's decisions being voided. California allows each party one challenge and the challenged judge can deny the challenge only if it violates the timeliness requirement.[19]

There are serious concerns with the peremptory-challenge process and these concerns largely focus on abuses in the use of challenge system that result in judges that one party views as less favorable to their case being excluded. The exact name for this abuse varies widely across authors; Burg refers to it as "judge shopping," while others refer to a strategic use of peremptory challenges. The idea is that judges are challenged because their judicial ideology, prior rulings or reputation

[17]It is true that all 50 states, the District of Columbia have agencies tasked with evaluating improper judicial conduct. It is also the case that these commissions are often drawn from the judiciary or lack the power to administer formal sanctions.

[18]Nicely for our ability to identify the motions in the docket and documents filed information we scrapped from the LA Courts webpage, the motion must include the specific language of the 170.6 statute making it easier to identify than most other motion types.

[19]See Cal. Civ. Proc. Code §170. A peremptory challenge is timely in California if it is made with 10 days of learning which judge is assigned to the case unless new information comes to light.

makes a lawyer or litigant think that another randomly drawn judge would be more favorable to the challenging litigant's interests. For example, Burg (1981) notes the *Solberg v. Superior Court* case in which a judge dismissed prosecution cases due to her view that defendants faced discriminatory law enforcement. A district attorney peremptory challenged the judge who refused to step down on the grounds that the challenge was solely motivated by her earlier rulings. The California Supreme Court ultimately ruled against the judge and she was disqualified from the case, thus upholding California's peremptory-challenge rule.

There are reasons to be concerned about "judge shopping." In fact, almost all of the concerns mentioned in the vast literature on forum-shopping potentially apply to the peremptory challenge system. Outcomes in litigation clearly vary across judges and this is why all states and the federal courts utilize some form of random assignment of judges. If the system results in the toughest or most lenient judges, or those at either ends of any distribution, being more regularly challenged and removed one could argue that this is a benefit since it reduces the tails of the distribution and makes judicial rules more consistent and less dependent on judicial assignment. For example, Schwartz and Schwartz (1996) make this argument with respect to peremptory challenges of jurors. By contrast, modeling the impact on juries, Flanagan (2015) argues that peremptory challenges actually have the opposite effect. They push juries to be more extreme by introducing correlation among the jurors who are chosen. While the peremptory challenge process reduces likelihood of individual jurors with extreme biases, the process increases the likelihood of juries composed of jurors with similar characteristics. The idea is that because each side will challenge jurors with similar characteristics, the resulting jurors who eventually replace the excluded jurors are more, not less, likely to have those characteristics. Of course the strength of that correlation is determined by the number of peremptory challenges so it is possible that California's system of one challenge for each side does not introduce a strong correlation.[20] Peremptory challenges against judges, like those in juries, could be used to exclude judges based on race or gender if it is thought by attorneys that those characteristics are likely to influence a judge's ruling in the cases.

The issue of the administrative costs of peremptory challenges of judges is more subtle. In

[20]De Clippel et al. (2014) model the arbitrator problem in which only one arbitrator is chosen. The key difference from our case is that the parties interest in arbitration may be aligned, which would not be the case in civil litigation.

smaller counties it is possible that trials must be delayed until an out of town judge is found or the case can be transferred (Burg (1981)). It is also possible that even in larger counties or court systems, a particular courthouse may have too many cases to assign quickly a new judge. Given these administrative costs, there is the possibility, noted by Burg 1981 that peremptory challenges can be used for strategic delay.[21]

It should be noted that one factor limiting the concerns about the peremptory challenge system is that there is only one peremptory challenge per party and the replacement judge is generally not known in advance of the challenge. Thus a key strategic concern, to the extent that litigants are using the peremptory challenge system strategically, is the pool of judges from which the replacement will be drawn. When multiple parties are involved, concern about strategic action becomes more acute since multiple parties with similar interests could effectively challenge multiple judges. Most states that allow peremptory challenges allow only one challenge when the parties have aligned interests.

1.2.2 Strategic Challenges and Judge Shopping

One challenge for our approach is that we need to differentiate between bias and strategic challenges. We define strategic behavior as lawyers using peremptory strikes to remove judges in the hope that the replacement judge is less favorable to their opponent. This is different from bias in that the judge has no particular bias specific to this case such as having a financial interest in the case (i.e. if the plaintiff wins the defendant corporation in which the judge owns stock would suffer a financial loss hence reducing the value of the judge's stock.)

Thus our test of the strategic use of peremptory challenges is that a key factor in a lawyer's decision to execute a peremptory challenge is the likelihood that the judge replacing the challenged judge will be less favorable to the opponent of the party making the challenge. Obviously the same could apply to motions for disqualification but there is a key difference. The party making the challenge requires cause. In our estimation we will examine this question directly by estimating

[21] In most states that use them, California included, peremptory challenges cannot be made for purposes of delay or for bad faith. However, as Burg 1981 notes that the bad faith is likely to be an ineffective standard since almost by definition a client challenging a judge he feels is unlikely to be sympathetic to his client is almost by definition acting in good faith.

the impact of our strategic challenge measures on likelihood a party files a disqualification motion.

We have two ways of measuring how favorable a particular judge is for a given side. The first is fraction of motions granted by the judge for each side in all other cases the judge has heard in the sample. That is, we examine the fraction of motions put forward by plaintiffs that the judge rules in favor of the defendant in all cases before the judge, excluding this case, prior to the filing of the current case. The higher this fraction, the more pro-plaintiff is a judge and vice versa for defense motions. We then use the fraction of motions in which the judge ruled for the plaintiff (defense) as measure of the defense (plaintiff) side's decision. In other words, each side observes how much the particular judge is biased towards their opponent. We utilize data from 2011 to the filing date of the case so that even the earliest cases in our sample have at least a full year of rulings data. The motion data is rolling so that as judge's measure can change over time as they rule on additional motions. For the small number of judges who join the bench during our sample period we treat the motion success rate for both sides as zero until the judge has ruled on a motion. The results are robust to excluding new judges for the first year of their tenure.

$$\text{favorable bias} = \begin{cases} \text{proportion successful ruling for plaintiff side,} & \text{if defendant} \\ \text{proportion successful ruling for defense side,} & \text{if plaintiff} \end{cases}$$

The measure is bounded by 1, the most biased judge towards the opponent, and 0, the least biased judge. One concern is that litigants may not know the outcome of recent ruling in other cases. For this reason we also use the judge's motion from 2011 to one year before the filing date.

As an indication of the value of a judge's past rulings on motions in predicting future judicial rulings, a market for motion success data already exists. A startup company known as Trellis: Legal Intelligence provides information on judges' motion rulings (Figure 1). The firm argues on its webpage that it can help lawyers, "Stop making 170.6 decisions blindly" by providing the insight needed in making such decisions, Trellis provides clients a report on the judge's rulings on pretrial motions.[22] In conversations with founders of Trellis they indicated that large law firms already provided this information to their associates and that Trellis was, in effect, leveling the playing

[22]See https://trellis.law/.

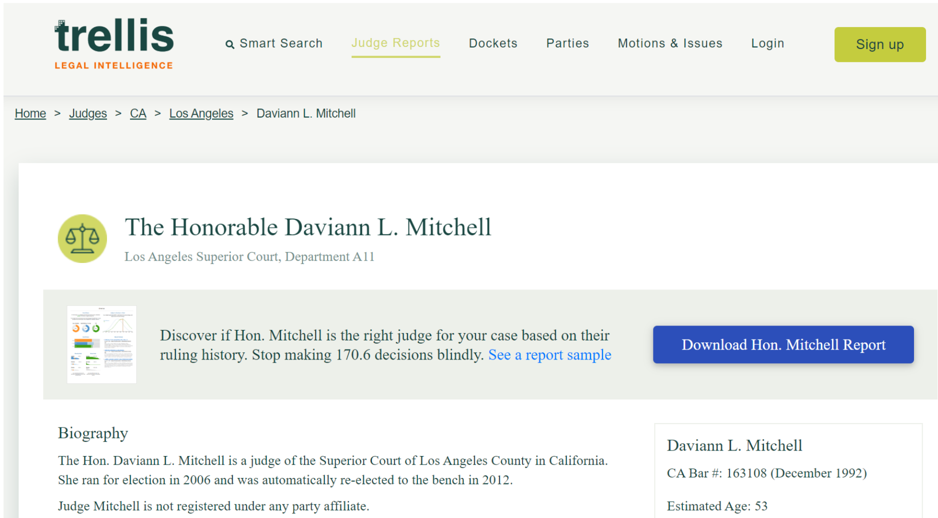


Figure 1: An example webpage of Trellis

field for attorneys.

The value of motion-success information notwithstanding, there are a number of reasons why a judge might rule in favor of one side or the other more frequently. Anecdotally, medical malpractice cases have a low plaintiff motion-success rates relative to other case types. As an alternative we also make use of a measure of judicial ideology. The ideology measure is motivated by the larger literature that a judge's personal ideology is predictive of how a judge rules in specific cases.[23] Under the assumption that more liberal justices are more pro-plaintiff and conservative justices more pro-defendant, we are able to calculate the likelihood of a more favorable justice being drawn after a peremptory challenge using this broader measure. The ideology measure, discussed further (below), does not vary across the sample period and is based on the judge's contributions to political campaigns. As such endogeneity may be less of a concern.

To determine the probability of drawing a judge less favorable to the opponent if a peremptory challenge is used, we create a 'z-score' for each judge based on his or her position in the distribution of motion success or judge ideology. The z score is generated based on the distribution of motion success scores excluding the current judge, i, in other words,

[23] See for example Segal and Spaeth (2002), who find that ideology is strongly predictive of voting on the US Supreme Court, Epstein et al. (2013), the US courts of appeals and US district courts, Brace et al. (2000), state courts, and Helland (2019) Daubert rulings.

$$zscore_i = (score_i - mean(score_{-i}))/sd(score_{-i})$$

Based on this, we can compute the probability that a replacement judge is closer to the ideal point of the defense or the plaintiff. We assume that the distribution of judges is normal but the results are robust to using non-parametric methods.

1.3 Data

Our analysis focuses on the determinants of peremptory challenges of judges assigned to civil cases in the Stanley Mosk Courthouse in Los Angeles. We focus on the Stanley Mosk Courthouse, the central courthouse of the Los Angeles County Court System, because it allows us a more precise measure of the previous decisions of the judges within this pool of potential replacement judges in the event that one of the parties peremptorily challenges the assigned judge. We utilize three main sources of data: a data collection of the LA Courts online case information system, the Database on Ideology, Money in Politics, and Elections (Public version 2.0) and Martindale-Hubble Lawyer's Directory.

1.3.1 The Los Angeles County Court System

The Superior Court of California, County of Los Angeles, has jurisdiction over all cases in Los Angeles County. It is the largest single unified court system in the United States with 47 court houses with 600 courtrooms and 481 judges. In 2019 it handled over 150,000 civil cases. We narrow this to only civil cases with over $25,000 in dispute which reduces the number of cases to approximately 50,000 per year in LA county. Over 80% of these cases are filed in central district. Among 240,000 central district civil cases filed in the Los Angeles Superior court during 2012-2018, judges are identified for 215,000 cases and about 3% of the cases have a peremptory challenge against a judge.

The Stanley Mosk Courthouse, which handles the vast majority of of central district civil cases, is located in downtown Los Angeles. It has over 100 courtrooms and is the largest courthouse in the United States. Judges in the LA Courts do not, generally, move from courthouse to courthouse and this allows us to know the pool of judges from which any replacement judge resulting from a

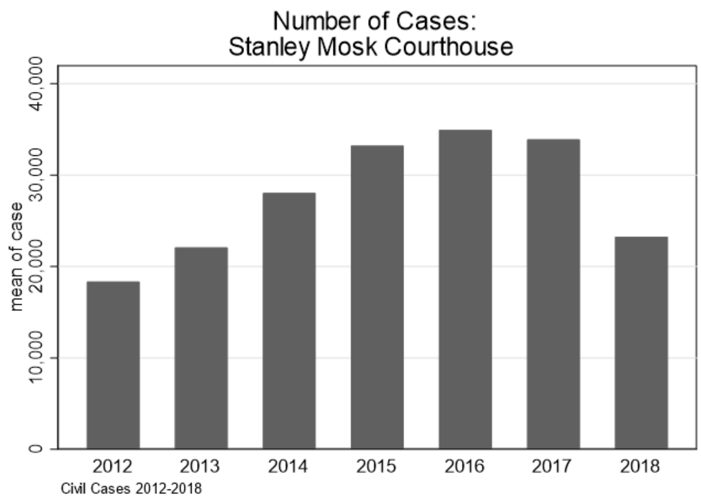


Figure 2: Number of cases each year

peremptory challenge will be drawn. In 2019 there were 104 judges assigned to the courthouse and if a judge is subject to a peremptory challenge or disqualified, the replacement judge is drawn from the same courthouse.[24] Civil cases can take any one of several paths to Stanley Mosk. Civil cases can be filed in local courts but most local courts do not normally handle civil cases after 2011. Due to budgetary pressures, the LA county courts underwent a consolidation of courts, with civil cases being far more centralized at Stanley Mosk.

The Los Angeles court system uses the California Court Case Management System, which is in many ways similar to the better known federal courts PACER system. The LA Court system provides information on all cases including the case number, filing date, courthouse, and the type of case. Figure 2 shows the total number of cases by year and Figure 3 shows the breakdown by case type. The system also includes information on the status of the case (dismissed, trial, etc.), the parties to the case and the attorneys for each side. The docket information includes a summary of all proceedings in the court, when the proceeding was held or the motion was filed, and a brief summary of each event's significance.

Judges are assigned randomly when a case is filed. There are, however, several exceptions to

[24]Prior to 2012 judges who where challenged where replaced by judges who had been challenged in another case. The court discontinued this system early in 2012 and now draws replacement judges using the same method of random selection as new cases.

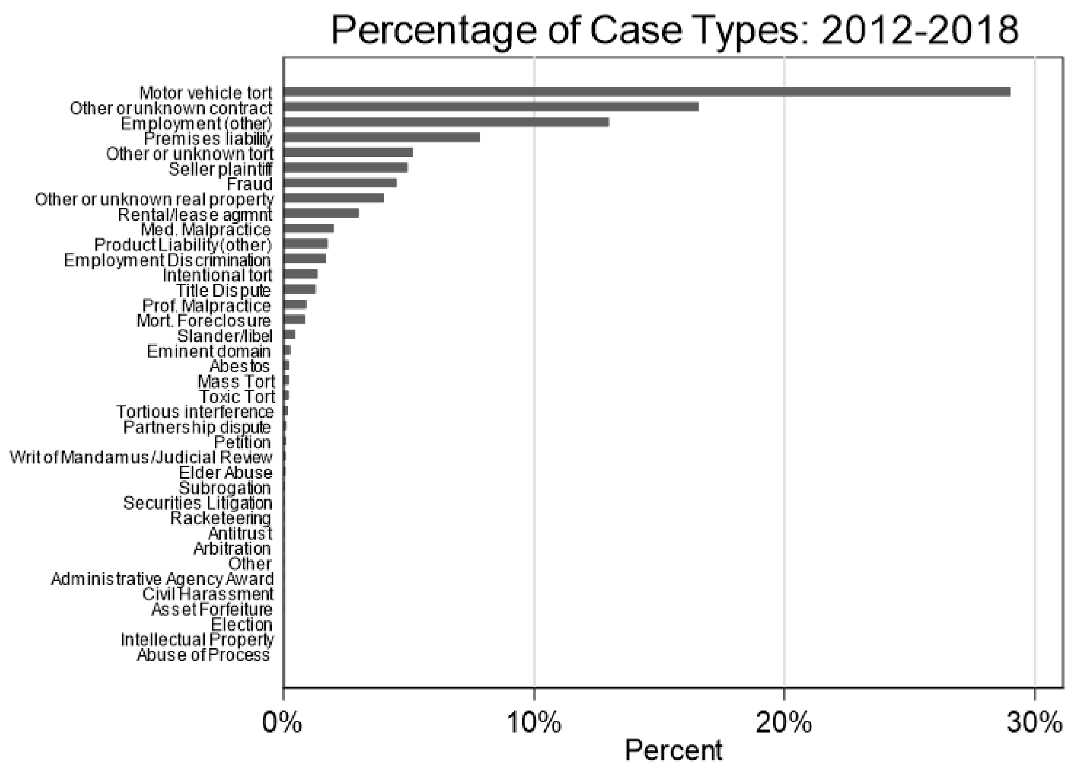


Figure 3: Number of cases per case type

general random assignment. The most important is the creation in 2012 of the personal injury hub.[25] The hub was designed to deal with budget cut in the mid-2000s that had resulted in delays in processing trials. The hub handles routine cases, mostly auto accidents, and although several judges are assigned to specific personal injury hub cases, on any given day a motion filed in a personal injury hub case will be ruled on by an available judge who is essentially randomly assigned to the case for that day.

The second deviation from pure random assignment is the complex-case court.[26] This is LA County's analog to the multi-district litigation system in the federal courts. Once a case has been ruled complex, the case and all related cases, for example all cases involving litigation against a pharmaceutical company for injuries resulting from the use of the same drug, would then be consolidated for the purposes of pre-trial motions and discovery. A new judge, in the complex court, would be assigned and the case would be moved from the Stanley Mosk courthouse to nearby Central Civil West.

One final issue that is not related to random assignment is that all employment cases can be filed either in Stanley Mosk or a local court house, if the local courthouse handles civil cases. In fact, most employment cases are handled at Stanley Mosk.

We are able to construct most of our variables from data collected from the LA Courts. Beyond case numbers and the names of the parties we also know the type of case, i.e. motor vehicle accident, the status of the case, and the attorneys for both sides. Most importantly from the docket sheets and list of filed documents, we are able to construct whether and which side used its 170.6 peremptory challenge against the judge assigned to their case. We also construct an indicator for challenges for cause. We determine which judge was assigned to the case, allowing us to link the judge to the ideology scores discussed below as well as the judges' tenure on the bench. We label any litigant without a lawyer listed as a pro se litigant and we construct the number of parties in the case and the number of lawyers.

We are also able to determine the number of motions filed and if the motion was ruled on, the eventual ruling on those motions from the docket, as well as list of documents filed and the register

[25]see http://www.lacourt.org/division/civil/pdf/FAQsPICourts.pdf
[26]see http://www.lacourt.org/division/civil/CI0033.aspx

of actions. Finally, we are able to determine if the case was moved to the complex-case system, whether the case as assigned to the personal injury hub and whether the parties to the case were individuals or businesses.

1.3.2 DIME Database

Although our motion-success measure comes directly from the LA Courts' data in order to create an ideology measure, we use the Database on Ideology, Money in Politics and Elections (DIME). The database provides a map from individual/organizational contributions to common-space-scaled ideology scores from 1979 to 2014. This database includes all individuals making campaign contributions from 1979 to 2014 (Bonica and Woodruff, 2014) .[27]

Bonica addresses several potential concerns with DIME data as a measure of ideology including strategic giving and sample selection based on which judges actually make contributions. In the latter case, we have 4 judges who do not give any recorded contributions. For these judges we use the Common-Score measure of the ideology of the governor who appointed them. In the case of strategic giving we are less concerned about mis-measuring ideology, given our alternative measure of motion success.

The common-space-ideology score ranges from -2 (the most liberal) to 2 (the most conservative). As noted above our hypothesis is that plaintiffs find more liberal judges more sympathetic and defendants the reverse.

1.3.3 Martindale-Hubble Lawyers' Directory

To determine the characteristics of the lawyers involved in the case, we use the Martindale-Hubble (MH) directory. We have 68,674 unique individual lawyers in the sample (97,235 including lawyers who used corporate names). We match each of these lawyers to the Martindale-Hubble directory. We also match judges in the sample in order to get judicial tenure. We are able to match

[27]As Bonica and Woodruff (2014) note its is very challenging to measure ideology using the conventional measures such as Martin-Quinn scores (see Martin and Quinn (2002)), Judicial Common Scores (see Epstein et al. (2007)), or Boyd (2010)) as judges in lower courts rarely sit together, making relative comparisons difficult. See Bonica and Woodruff (2014) for an examination of the relationships between ideology scores produced by relative measures and DIME for state supreme court justices.

25,851 individual lawyers.[28]

Martindale-Hubble draws its data from California state bar directory and other publicly available sources. Although the amount of information varies considerably, an entry always contains the lawyers office address, law school attended, and date they were admitted to the California bar. It also typically tells if the lawyer's practice is primarily plaintiff or defense related and the types of cases the lawyer typically handles. Finally, it also typically includes information about whether the lawyer is a solo practitioner, part of a smaller firm or of a large firm. We use this to get our lawyer experience measure and whether the lawyer works for a large multi-lawyer firm, (defined as a firm with more than one lawyer) in our data.

1.3.4 Summary Statistics

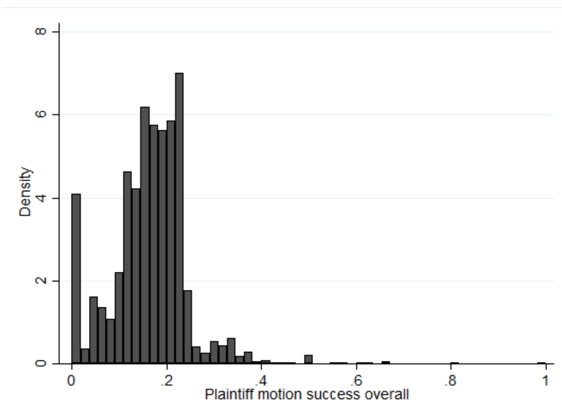
Table 1 presents summary statistics. We put the variables into four categories: the frequency of peremptory challenges and motions to disqualify a judge, the measures of ideology and plaintiff and defendant success on motions, the probability that a new judge will be more favorable to the plaintiff or defendant, information on case type, and judge and lawyer characteristics. Since each side can issue one peremptory challenge, we organize our data by plaintiff and defendant. We drop the small number of cases with multiple defendants or plaintiffs who do not have common representation and have been deemed not to have a common interest, since these cases might have more than one challenge per side available. Thus each case has two observations: one for the plaintiff and one for the defendant. Thus, the probability that a challenge resulting in a more favorable judge, as measured by either motion success or ideology, would be the p value of the above z-score, p for the plaintiff and $1-p$ for the defendant.

The dependent variable is whether the judge is challenged by the respective party. In Table 1 we break the sample into challenged and non-challenged observations. Although it is possible for both sides to challenge the same judge, we have only a handful of examples of this in our data. The histogram of our motion measure is presented in Figure 4. Figure 5 shows the histogram and

[28] 21,105 unique individuals have been identified based on MH name list – meaning we have "duplicates" in our court data – for example, "ZEIF AARIN APRIL" and "ZEIF AARIN A" in court data are matched with "Aarin April Zeif" in MH. This is mostly due to the writing scheme being dependent on each clerk who files out the case information on the LA court webage.

Table 1: Summary statistics

	Full Sample		Challenges		Non-Challenge	
	Mean	S.D.	Mean	S.D.	Mean	S.D.
A party filed the challenge to judge	0.038	0.190	1.000	0.000	0.000	0.000
Challenges for cause	0.001	0.029	0.005	0.073	0.001	0.026
Judge's common-space CFscore	-0.703	0.474	-0.678	0.460	-0.704	0.474
Judge Tenure	11.378	8.933	16.418	10.384	11.181	8.814
Pro se litigant	0.166	0.372	0.043	0.203	0.171	0.377
Number of Parties to the Case	2.239	11.894	2.474	8.114	2.230	12.017
Number of Lawyers	1.071	0.941	1.451	1.343	1.056	0.918
Case is assigned the the Personal Injury Hub	0.258	0.437	0.079	0.269	0.265	0.441
Lawyer is in a multi-attorney firm	0.318	0.466	0.382	0.486	0.316	0.465
Complex case designation	0.003	0.053	0.010	0.098	0.003	0.050
Ratio of corporate to total parties	0.113	0.269	0.110	0.258	0.114	0.270
Prob(bias)	0.509	0.315	0.532	0.281	0.508	0.317
Prob(bias) Past Year	0.434	0.326	0.513	0.293	0.431	0.327
Prob(bias) Ideology	0.500	0.250	0.500	0.249	0.500	0.250
Observations	369008		13853		355155	



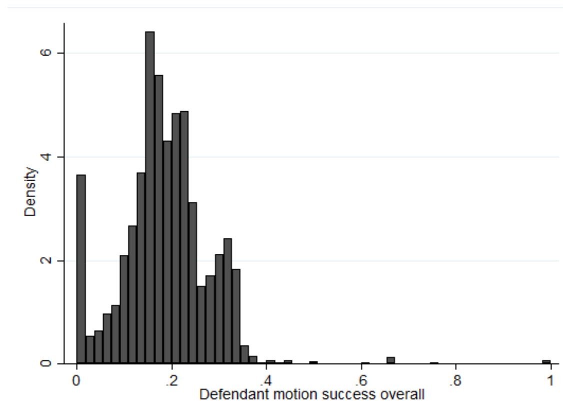


Figure 4: Distribution of successful ruling by plaintiff and defense side

kernel density of our ideology measure.

1.3.5 Preliminary Data Analysis: Challenges with Judge-Fixed Effects

Figure 6 lays out the frequency with which the almost 100 judges in our sample are subject to peremptory challenges. Although the average challenge rate is 3% certain judges are challenged far more frequently. For example, the judge assigned a randomized ID numbered 11 is challenged in over 16% of his cases while judge 175, who serves throughout our sample, is never challenged. In Figure 7 we break out challenges by case type, showing that challenges appear to be far more likely in employment cases than motor-vehicle-accident cases. This may simply be due to the fact

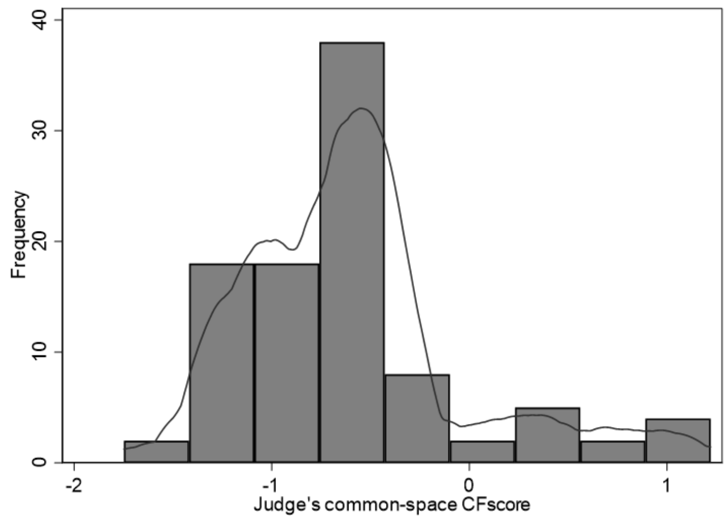


Figure 5: Distribution of judge ideology

that most auto accident cases are assigned to the personal injury hub and given the mechanics of assignment in the hub challenging the assigned judge, who is highly unlikely to be the judge ruling on any motions in the case, makes very little sense strategically.

To determine the impact of case type and year we estimate the following model

$$y_{ij} = \mu_j + \gamma_c + \tau_t + e_{ij} \tag{1}$$

where y_{it} is an indicator variable equal to one if party, plaintiff or defense, challenges judge j. We also include year fixed effects τ_t and case type fixed effects γ_c. e_{ij} is the error term clustered on case.

Figure 8 plots the judge-fixed effects by their likelihood of being challenged holding case type and year constant. Consistent with Figure 6, there is wide variation in the likelihood that a judge is challenged. By itself this might suggest that challenges are not being used strategically and are, in fact, used to challenge judges whom litigants on both sides perceive as "bad." Perhaps certain judges, like unpopular judge 11 for example, are simply disliked by both sides due to their demeanor on the bench or concerns about their competence.

In Figure 9 we re-estimate the above model but interact the judge fixed effects with litigant

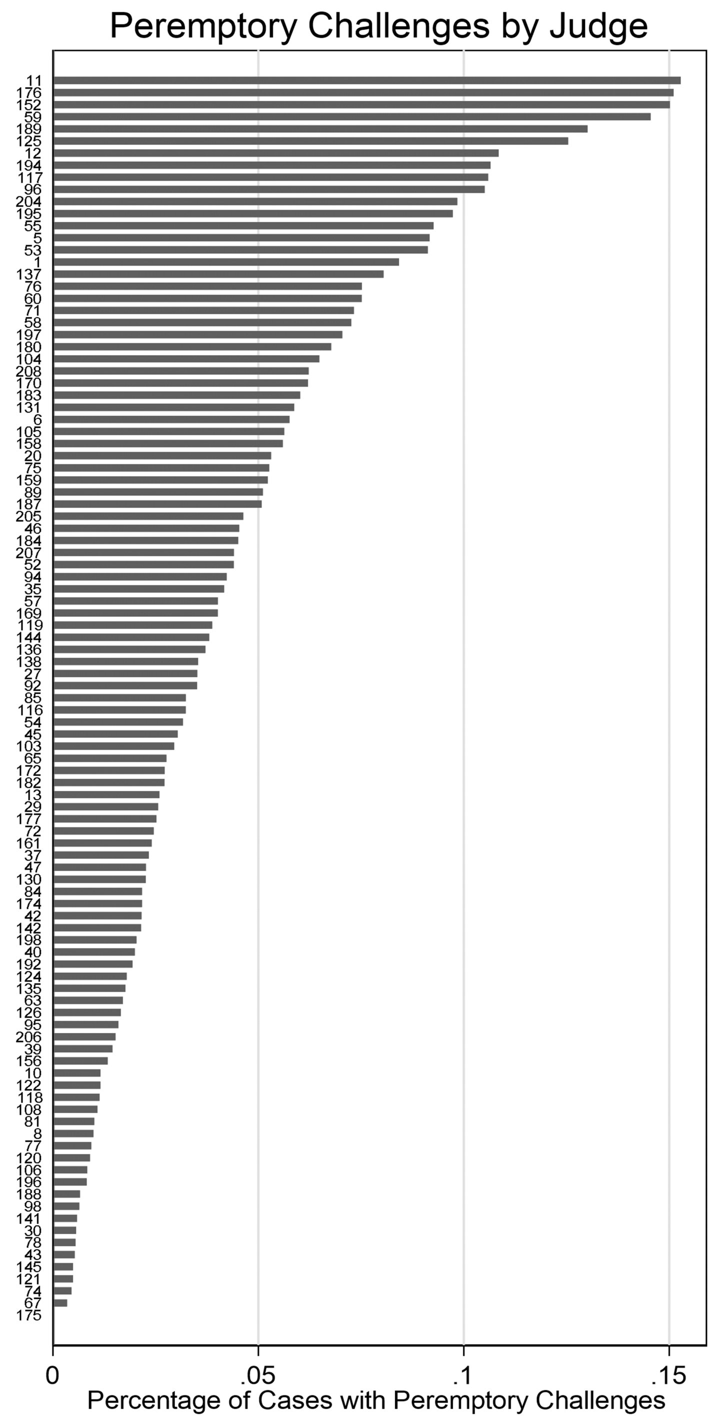


Figure 6: Peremptory challenge by judge

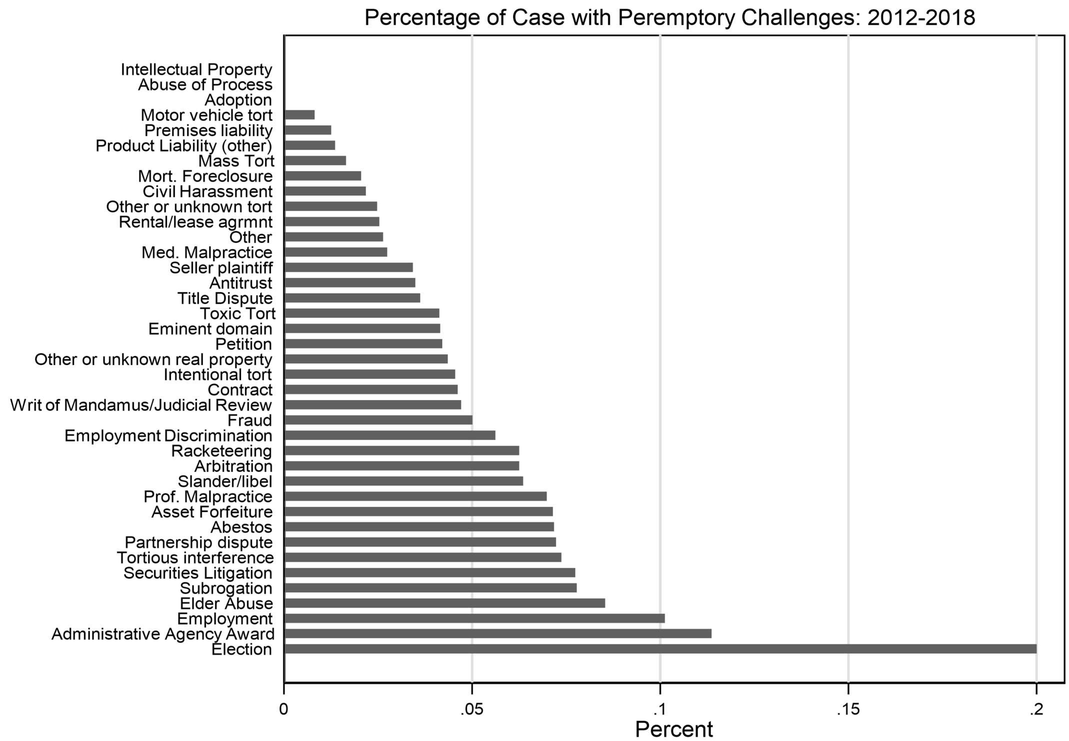


Figure 7: Peremptory challenge by case type

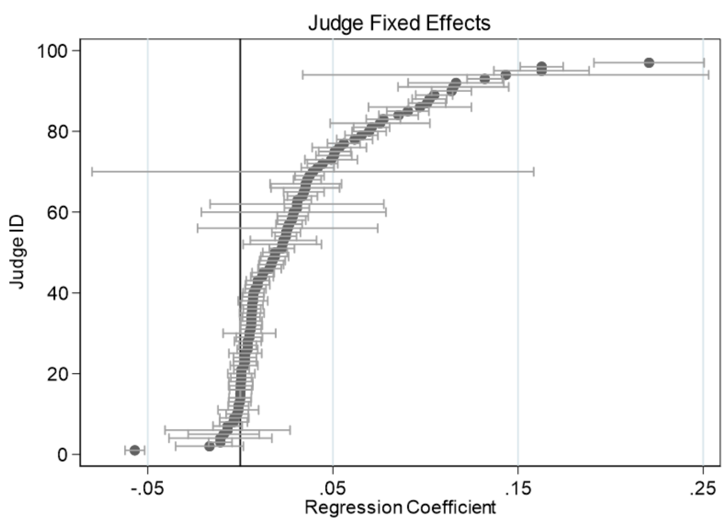


Figure 8: Judge-fixed effects

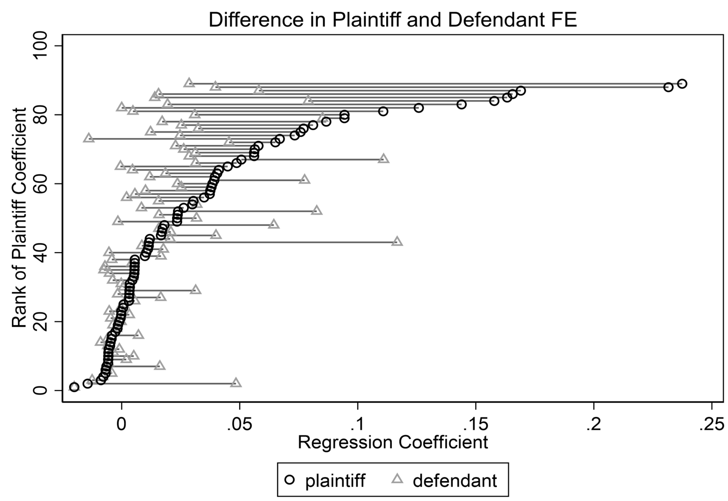


Figure 9: Difference in plaintiff- and defendant-fixed effects

type,

$$y_{ij} = \mu_j * \text{plaintiff} + \mu_j * \text{defendant} + \gamma_c + \tau_t + e_{ij}. \tag{2}$$

The results suggest that a number of judges who are challenged frequently by plaintiffs are far more or less likely to be challenged by defendants. While this is not proof of strategic behavior, it does suggest that defendants and plaintiffs view at least certain judges differently.

While the fixed-effects are suggestive that both "bad judges" and strategic components exist to peremptory challenges, it is possible that defendants and plaintiffs simply view "bad" characteristics differently. For example, if incompetence on the part of judges is more vexing to plaintiffs than to defendants, we might observe the pattern in Figure 9 without strategic behavior. In the next section we turn to a more direct test of strategic behavior, such as whether the judge in a case is more likely to be challenged when the probability that his or her replacement is more favorable to the challenging side.

1.4 Results

We now turn to our estimates of strategic behavior on the part of litigants who are challenging the judge. Our estimating equation is similar to the equation in the last section but we added several control variables and our measure of the probability that if the judge is challenged and (automatically) removed, the replacement judge will be against the opponent party's ideal position and/more likely to rule in favor of the parties motion. We estimate the model,

$$y_{ij} = \alpha_0 + \alpha_{\text{plaintiff}} + \alpha_1 prob(bias)_{ij} + \beta X_{ij} + \gamma_c + \tau_t + e_{ij}. \quad (3)$$

As defined above, $prob(bias)$ is a probability of drawing a judge biased against the opponent's ideal point based on the current judge's ideology or more likely to be favorable to the parties motions depending on the specification. We also include several control variables: which party is the plaintiff, whether the case is assigned to the personal injury hub, whether the case will be designated complex but has not yet been moved to the complex court, lawyer's tenure, whether the lawyer is part of a multi-lawyer firm, the number of parties in the case, number of lawyers of the case, whether the relevant party represents itself and the proportion of the litigants on the respective side who are businesses or corporations.

We estimate the model using both the linear probability model (LPM) and the logit model. The concern is that whether or not, with a large number of fixed effects, such as our fixed effects on case type, this will produce the incidental-parameters problem. In other words, with a large number of fixed effects, nonlinear models, but not the LPM, are inconsistent. The concern with the LPM is that only 3% of our sample has a challenge and LPM often produce very different answers than nonlinear models when the probability of zero or one is far removed from .5. For this reason we estimate the model using both techniques.

1.4.1 Peremptory Challenge Results

The results of our peremptory challenge regressions are presented in Table 2. Panel A of Table 2 shows the impact of the motion z score. Column (1) shows the results for the LPM. The coefficient is

.006, suggesting that a one-standard deviation change in the likelihood of drawing a more favorable judge increases the probability of a challenge by about one percent. When estimated using a logit we find a slightly larger effect with a one-standard deviation change causing a 1% point increase in the likelihood of a challenge. The results in both specifications are statistically significant.

In Panel B of Table 2 we estimate the results using only motions filed a year before the filing data of the case. The results are similarly significant although now the linear probability model and the marginal effect of logit model are similar in magnitude and are consistent with the logit results in Panel A.

In Panel C we use the DIME database common score measure of ideology. The ideology measure is significant in all specifications and the impact is somewhat larger than when we use the motions-success measure. For the LPM a one standard deviation increase in the likelihood of drawing a more favorable judge ideologically increases the probability of a challenge by 1.5% point. In the logit the impact is slightly smaller, 1% point.

The results thus far suggest that lawyers are using their challenges strategically, although the effect is relatively small. A judge who is a full-standard deviation away from mean of the distribution of replacement judges, which increase the likelihood of drawing a more favorable judge from a coin toss to 75% increases the probability of a challenge by only 2-3% depending on the specification. This estimate, however, is based on pooling all case types, and as is clear from Figure 7, the likelihood of a peremptory challenge varies greatly across case types. A second issue is that, lawyers may vary greatly in their knowledge of the peremptory challenge process, its implications and the even the likelihood of drawing a more favorable judge. In the next section we decompose the results by case type and lawyer type.

In columns 4-6 of Table 2 we estimate the probability of a second-stage challenge. This means that we examine the subset of cases in which the initially assigned judge is challenged, a new judge is randomly chosen and the party who has not yet used its challenge must decide whether or not to challenge the new judge. While challenges are quite rare in the data, about 3% of the sample, second challenges are even less frequent. Of the 7,276 cases with a first challenge, only 528 cases have a second challenge. Nonetheless, second challenges potentially tell us something important about

Table 2: First vs. second peremptory challenge with motion measure

	Regression on First Challenge			Regression on Second Challenge		
	LPM	Logit	ME	LPM	Logit	ME
Panel A. Motion z score						
Prob(bias)	0.006***	0.127***	0.010***			
	(0.001)	(0.015)	(0.001)			
Second Prob(bias)				0.042***	0.396***	0.042***
				(0.010)	(0.093)	(0.010)
Plaintiff	0.017***	0.223***		0.126***	1.128***	
	(0.001)	(0.010)		(0.007)	(0.057)	
Case is assigned to the Personal Injury Hub	-0.004***	-0.151***		-0.017	-0.178	
	(0.001)	(0.020)		(0.014)	(0.126)	
Lawyer is in a multi-attorney firm	-0.001	0.016		0.001	-0.017	
	(0.001)	(0.010)		(0.007)	(0.056)	
Complex case designation	0.038***	0.181***		-0.073***	-0.899**	
	(0.010)	(0.052)		(0.022)	(0.427)	
Pro se litigant	-0.009***	-0.431***		-0.022***	-0.378***	
	(0.001)	(0.019)		(0.006)	(0.108)	
Number of Parties to the Case	-0.000	-0.000		0.000	0.000	
	(0.000)	(0.000)		(0.000)	(0.001)	
Number of Lawyers	0.014***	0.104***		0.003*	0.040**	
	(0.001)	(0.006)		(0.001)	(0.016)	
Ratio of corporate to total parties	-0.011***	-0.106***		0.002	0.069	
	(0.001)	(0.015)		(0.007)	(0.091)	
Observations	355783	355731	355731	8122	8065	8065
Panel B. Motion 1yr z score						
Prob(bias)	0.013***	0.209***	0.016***			
	(0.001)	(0.015)	(0.001)			
Second Prob(bias)				0.036***	0.318***	0.033***
				(0.010)	(0.094)	(0.010)
Plaintiff	0.018***	0.218***		0.133***	1.192***	
	(0.001)	(0.010)		(0.007)	(0.061)	
Case is assigned to the Personal Injury Hub	-0.009***	-0.260***		-0.015	-0.150	
	(0.001)	(0.022)		(0.015)	(0.136)	
Lawyer is in a multi-attorney firm	-0.001	0.012		-0.003	-0.058	
	(0.001)	(0.010)		(0.007)	(0.058)	
Complex case designation	0.036***	0.165***		-0.080***	0.000	
	(0.010)	(0.054)		(0.016)	(.)	
Pro se litigant	-0.009***	-0.436***		-0.021***	-0.383***	
	(0.001)	(0.020)		(0.006)	(0.115)	
Number of Parties to the Case	-0.000	-0.000		0.000	0.000	
	(0.000)	(0.000)		(0.000)	(0.001)	
Number of Lawyers	0.014***	0.105***		0.004**	0.047***	
	(0.001)	(0.007)		(0.002)	(0.018)	
Ratio of corporate to total parties	-0.012***	-0.113***		0.002	0.053	
	(0.001)	(0.016)		(0.007)	(0.097)	
Observations	322580	322518	322518	7516	7427	7427
Panel C. Ideology score						
Prob(Closer)	0.015***	0.128***	0.010***			
	(0.001)	(0.015)	(0.001)			
Second Prob(Closer)				-0.004	-0.034	-0.003
				(0.009)	(0.088)	(0.009)
Plaintiff	0.019***	0.227***		0.086***	0.792***	
	(0.001)	(0.010)		(0.007)	(0.055)	
Case is assigned to the Personal Injury Hub	-0.006***	-0.221***		0.016	0.123	
	(0.001)	(0.020)		(0.018)	(0.138)	
Lawyer is in a multi-attorney firm	-0.001*	0.007		-0.001	-0.022	
	(0.001)	(0.010)		(0.007)	(0.059)	
Complex case designation	0.035*	0.144		-0.057	-0.651	
	(0.018)	(0.114)		(0.043)	(0.541)	
Pro se litigant	-0.010***	-0.432***		-0.012*	-0.253**	
	(0.001)	(0.020)		(0.007)	(0.099)	
Number of Parties to the Case	-0.000*	-0.000		-0.000	-0.000	
	(0.000)	(0.000)		(0.000)	(0.001)	
Number of Lawyers	0.013***	0.107***		0.005**	0.049***	
	(0.001)	(0.007)		(0.003)	(0.018)	
Ratio of corporate to total parties	-0.011***	-0.110***		0.009	0.141	
	(0.001)	(0.016)		(0.007)	(0.089)	
Observations	325074	325022	325022	7276	7242	7242
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Claim FE	Yes	Yes	Yes	Yes	Yes	Yes

Robust standard errors clustered at the case level in parentheses
*** $p<0.01$, ** $p<0.05$, * $p<0.1$

the strategic use of challenges. First, Flanagan (2015) predicts that the probability of drawing a more favorable judge should have a larger effect in the second stage, since the opposing party has already used its challenge and if the challenging party draws a more favorable judge, the outcome cannot be overturned by the other party. However, the distribution of judges that can be drawn as replacements is now different since original judge cannot be reassigned to the case. But differently, in Flanagan's model we should observe a higher propensity to challenge but less frequent challenges.

This is what we find in Table 2 for our two motion measures. In both the LPM and logit model the coefficient is .042, almost 4 times as large as the first stage. The impact for motion rulings in the last year is slightly smaller, only 3 times as large as the first stage. Our ideology measure is not significant for second challenges.

1.4.2 Robustness Checks

Table 3 presents the results for nine most common claim types. Each cell of Table 3 represents a separate regression. Since the results from measuring motion success either with a one-year lag or truncated at the start of the current case are largely identical, we omit the results using the one-year lag. As shown in Figure 3, motor vehicle accidents are the most common types of cases with premises-liability and employment cases being second and third most common case type. We find that employment cases have the largest impact of motion success on the probability of challenges while the impact for motor vehicle accidents and premises liability are not significant.

The results from Table 3 suggest that challenges are most common in employment, fraud, real property and rental-agreement cases. In the next section we examine whether other challenges for cause follow a similar pattern of challenging a judge strategically when they are more likely to be replaced with a judge who is preferred by the relevant party.

Table 3: Results by nine most common claim types

	Motor vehicle tort	Contract	Employment	PL	Other	Seller plainitff	Fraud	Real property	Lease
Prob(bias) Motion	0.000	0.024***	0.043***	0.002	-0.001	0.007	0.026***	0.027***	0.012*
	(0.001)	(0.003)	(0.005)	(0.002)	(0.003)	(0.006)	(0.007)	(0.007)	(0.007)
Observations	98118	59390	52068	27742	17998	17340	16154	14181	9689
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Claim FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Robust standard errors clustered at the case level in parentheses
*** p<0.01, ** p<0.05, * p<0.1
PL indicates premises liability, Real property means other or unknown real property, and Lease is rental/lease agreement.

1.4.3 Disqualification-Motion Results

As noted above, there are 17 states which use the peremptory-challenge system. But all states allow for judges to be disqualified for cause. In Table 4 we estimate the model using challenges for cause rather than peremptory challenges. Challenges for cause are almost 30 times less likely than 170.6 challenges. The reason, likely, lies in the fact that peremptory challenges are automatically successful while challenges for cause at least theoretically could be rejected. We find that for our motion-success measure, an increasing probability of replacing the judge with a more favorable judge has a negative impact on the likelihood of a challenge for cause. One explanation is that litigants are more likely to use a peremptory challenge with an outlier judge rather than risk having a challenge for cause refused. However, one issue with this interpretation is that challenges for cause and peremptory challenges are not mutually exclusive. One could try a challenge for cause and in the rare instance it is not successful follow it up with a peremptory challenge.

Table 4: Disqualification for cause

	LPM	Logit	ME	LPM	Logit	ME
Prob(bias) Motion	-0.000**	-0.109*	-0.000*			
	(0.000)	(0.064)	(0.000)			
Prob(bias) Ideology				0.000	0.050	0.000
				(0.000)	(0.070)	(0.000)
Plaintiff	0.001***	0.192***		0.001***	0.193***	
	(0.000)	(0.040)		(0.000)	(0.041)	
Case is assigned the the Personal Injury Hub	-0.000	-0.234**		-0.000	-0.293***	
	(0.000)	(0.110)		(0.000)	(0.112)	
Lawyer is in a multi-attorney firm	-0.000*	-0.063		-0.000**	-0.074	
	(0.000)	(0.044)		(0.000)	(0.045)	
Complex case designation	0.000	0.078		0.002	0.245	
	(0.001)	(0.245)		(0.004)	(0.391)	
Pro se litigant	0.000	0.018		0.000	-0.000	
	(0.000)	(0.054)		(0.000)	(0.056)	
Number of Parties to the Case	-0.000**	-0.004		-0.000*	-0.003	
	(0.000)	(0.004)		(0.000)	(0.004)	
Number of Lawyers	0.000***	0.041***		0.000***	0.043***	
	(0.000)	(0.009)		(0.000)	(0.010)	
Ratio of corporate to total parties	0.000	0.048		0.000	0.049	
	(0.000)	(0.056)		(0.000)	(0.057)	
Observations	355783	352148	352148	325057	321902	321902
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Claim FE	Yes	Yes	Yes	Yes	Yes	Yes

Robust standard errors clustered at the case level in parentheses
*** p<0.01, ** p<0.05, * p<0.1

When we measure the favorability of a judge using ideology, however, the impact is small, positive, and statistically insignificant. Overall, it appears that challenges for cause are governed by a different process than the strategic challenging we see in peremptory challenges.

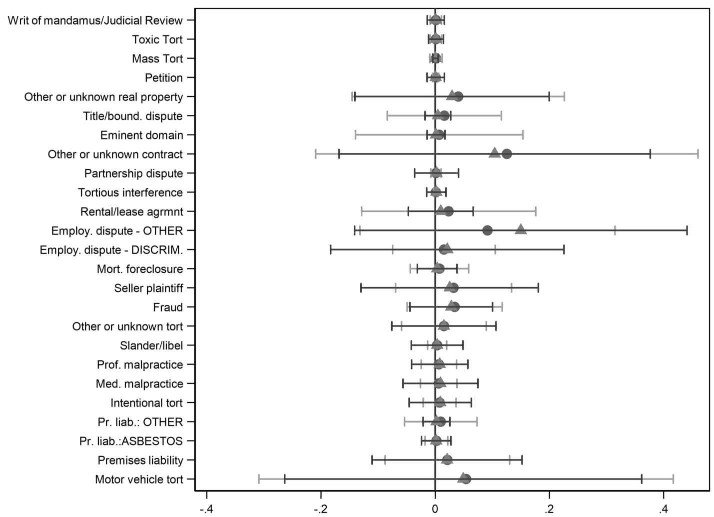


Figure 10: Balance plot

1.4.4 Random Assignment Pre and Post Challenge

Thus far we have found evidence that peremptory challenges are used strategically by lawyers in exactly the fashion predicted by a simple game-theoretic model. The question remains: what are the consequences of these challenges? Some evidence of the impact of peremptory challenges is found in Figure 10. Figure 10 is a graphical balance plot of the 98 judges by case type.

$$type_{ic} = \mu_j judge_j + \alpha caseload_{ij} + \tau_t + e_{ij} \tag{4}$$

That is, figure plots the distribution of probability that a judge was assigned to a particular case type, μ_j. If judges are randomly assigned, all of these coefficients would be zero but in fact we might expect to see variation in assignment as judges work in the personal injury hub or on complex cases. We do see large error bars on certain case types. Contrast employment cases, in which the 95% confidence interval on the distribution of coefficients includes judges who are 20% less likely and almost 40% more likely to be assigned to an employment case than some other case type. Interestingly, peremptory challenges actually tighten the confidence intervals (the light grey bars) relative to the distribution before the challenge for some case types.

The results in figure 9, however, do not suggest a pattern. Whatever the baseline level of randomness in the assignment of judge at Stanley Mosk, the peremptory challenge system does not seem to be altering this baseline level.

1.4.5 Weighting with Expected Second-Stage Behavior

A model misspecification concern arises when the peremptory challenge of the first actor is modeled based on the expectation about the second actor's challenge. Bas et al. (2008) provides a statistical way of incorporating the second stage probability in a strategic sequential game with agents' errors and private information. The common practice of this Statistical Backward Induction ("SBI") method (Bas et al., 2008) is to weight the data using the predicted probability of the second stage action. However, in our setting, the second actor faces a different incoming judge after the first challenge, which leads to a different probability distribution, making it hard to apply the SBI method. Thus, to obtain the second-stage probability of the peremptory challenge, we focus on the probability of a less favorable judge than the current judge to the second mover. In other words, we take the expected values from the truncated distribution as a basis for the incoming judges. Finally, we calculate the probability of finding judges that are more favorable to the second mover than the expected incoming judges.

The SBI method then uses the probability of the second challenge to weigh the first mover's utility with and without the second challenge, respectively. This is depicted in Equation 5, where β_1 is the utility from challenging when the second challenge is present, and β_2 is the utility from challenging when the second challenge is absent.

$$y_{it} = prob(second\ \ challenge) * X_{it}\beta_1 + (1 - prob(second\ \ challenge)) * X_{it}\beta_2 + e_{it} \quad (5)$$

The other difference between the common setting presented by the SBI method and our research setting is that players face simultaneous and sequential games at the same time, resulting in a situation where player 1 not challenging does not necessarily lead to a resolution of a conflict.

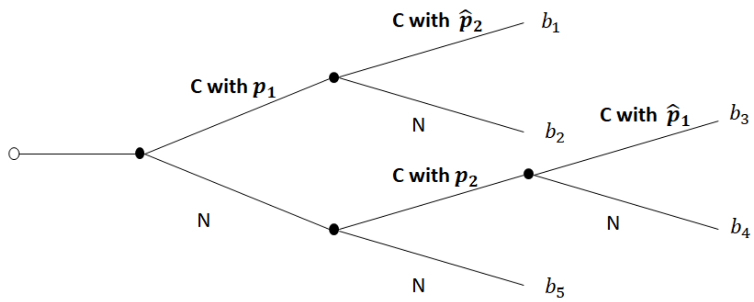


Figure 11: Statistical Decision Tree

Figure 11 depicts the decision sequence.[29] To incorporate every possible option, we add one term into the Equation 5. In Equation 6, β_3 measures the utility of player 1 when player 2 challenges the current judge first, and player 1 then decides to challenge the incoming judge. There are two additional outcomes, normalized as zero. The first one measures player 1's utility when player 2 challenges the current judge first, and player 1 does not challenge the incoming judge, and the second one is the case where no one challenges.[30]

$$y_{it} = p_1 * \widehat{p_2} * X_{it}\beta_1 + p_1 * (1 - \widehat{p_2}) * X_{it}\beta_2 + (1 - p_1) * p_2 * \widehat{p_1} * X_{it}\beta_3 + e_{it} \quad (6)$$

In Table 5, Column (1) displays the baseline coefficient from the main Table 2, but excluding the covariates to simplify the statistical backward induction procedure. Column (2) reports the results from Equation 6. To interpret the results, Compared to the baseline result in Column (1), the results stay similar in Column (2) when conducting the linear combination test of three coefficients ($\widehat{\beta}_1$ + $\widehat{\beta}_2$ - $\widehat{\beta}_3$). Finally, we bootstrap the standard errors, repeating 1,000 times; since we directly calculate and use the weighted variables, the final standard errors do not account for the errors from the second stage behavior.

[29] Note that the figure is not the formal game theoretical decision tree. It helps to picture how the SBI method works.

[30] Bas et al. (2008) also normalized outcomes as zero where the first player is inactive.

Table 5: Comparing Linear Probability Model Results

	(1) Baseline	(2) SBI with Bootstrap
Prob(bias)	0.00981***	
	(0.000927)	
U(C,C)		-0.00450
		(0.00348)
U(C,N)		0.0287***
		(0.00464)
U(N,C,C)		0.00857**
		(0.00299)
Observations	355783	354886
R^2	0.025	0.025

*** p<0.01, ** p<0.05, * p<0.1

1.5 Conclusion

In this paper we have examined the role of strategic behavior in California's peremptory challenge system for judges. A basic model of the strategic behavior within the peremptory challenge system would predict that litigants are more likely to challenge judges who are less favorable from the litigants position if their judge is more likely to be replaced with a judge who is more favorable. This is what we find. However, the effects are far from uniform. Challenges are far more likely in employment cases and cases involving inexperienced lawyers and those without legal representation.

One question is why certain case types might be more amenable to a peremptory challenge. One theory is that the stakes in these cases are simply higher. Employment cases involve more money than traffic accidents but they also set precedents for future employment cases against the defendant. A second reason may have to do with our motion measure. Some case types not only have more motions but motions tend to be more important, giving more weight to judges' predispositions in motion rulings. There are very few motions on evidence, for example, in auto cases but motions on evidence are determinative of the whole case's viability in toxic torts where establishing causation is critical to the success of a toxic tort case. In fact a toxic tort case will often collapse in the face of a negative ruling on the admissibility of expert evidence.

2 Chapter 2: Criminal Decarceration Policies and the Effect on Community Safety

2.1 Introduction

Decarceration policies have received bipartisan support in recent years as progressives have raised concerns regarding racial disparities and worsening welfare outcomes for inmates. At the same time, conservatives aim to reduce the financial burden of the criminal justice system on the government budget (Takei, 2017). The impact of reductions in the size of incarceration on community safety remains an open empirical research question. Given the political agreement, it is perhaps unsurprising that decarceration policies have been established. Specifically, recent calls for reform in the criminal justice system have targeted reducing the incarcerated population and converting low-level, non-violent felony offenses into misdemeanors.

The question then arises as to whether mere re-sentencing is the appropriate means for decarceration when re-sentencing is not accompanied by resources to mitigate the cycle of individuals with a high risk for re-offending. For example, individuals released from jail could struggle with employment, finding housing, or addressing mental health issues. In this work, I examine Proposition 47 (Prop 47), which mirrors the calls for reform discussed nationwide in the United States. I will specifically investigate the effect of criminal re-sentencing policies on the homeless population, government spending, and criminal deterrence in California communities.

Historically, radical changes followed the United States Supreme Court ruling that the California prison system must reduce its population in 2011, *Brown v. Plata, 563 US 493 (2011)*. Namely, California was required to reduce the prison population by 34,000 inmates by June 2013, approximately 20% of the prison population. To meet this goal, California enacted Assembly Bill 109 (AB 109) in 2011, which realigned the prison population, shifting many inmates to county jails, especially non-serious criminal offenders (Lofstrom et al., 2016). A shock to governmental fiscal stress was experienced during this event while not receiving attention from the public. As noted in Boylan and Mocan (2014), the court order to reduce the prison population led to a substitution effect in public spending that raised expenditures for prison reform, which was associated with a reduction in welfare expenditures. Three years later, the people of California passed Prop 47 in 2014 as another major reform to the criminal justice system.[1] Prop 47 aimed to remove racial disparities in the criminal justice system and decrease the burden of county jails, as driven by the realignment in 2011, by reducing the penalty for non-serious property crimes so that those offenders did not have to be incarcerated in county jails. The jail population dropped by 9,000 inmates within two years after Prop 47, achieving pre-realignment levels for county jail populations (Lofstrom et al., 2016).

The major difference between these two pieces of legislation is that Prop 47 shifted offenders from incarceration to local communities. In contrast, AB 109 contained offenders within the carceral system, only moving the state prison population into county jails. Specifically, offenders with non-serious property crimes classified as misdemeanors under Prop 47 were likely to remain in local communities. County jails rarely admitted offenders found guilty of one of the charges outlined in Prop 47, which included drug possession, receiving stolen property, theft, shoplifting, writing bad checks, and check forgery.[2] Additionally, inmates who entered jails before Prop 47 for one of the Prop 47 charges could petition to be released early.

However, people with a high risk for re-offending are likely to become homeless without successful re-entry. It is not common for offenders to naturally return to society even after they are released; evidence shows that 70% of the unsheltered homeless in San Diego reported a history of incarceration in a 2018 survey conducted by the US Department of Housing and Urban Development.[3] Additionally, 15% of inmates reported a history of homelessness in a 2002 national survey conducted by the US Department of Justice (Greenberg and Rosenheck, 2008). Furthermore, not admitting offenders to county jails might stress alternative institutions to house the offender pop-

[1] Prop 47 is not only supported by both political parties but also supported by the public. The people of California widely supported Prop 47. 59.6% of voters (4,238,156 in level) supported Prop 47, while 40.4% (2,871,943 in level) opposed it. The number of votes amounts to 40% among the total 17,803,823 registered voters (Debra Bowen, 2014, Statement of Vote, November 4, 2014 General Election, California State of State).

[2] The list of Penal Code sections for Prop 47 charges that qualify for re-sentencing is 459.5, 484, 487, 496, 470, 471, 472, 473, 474, 475, 476, and 476(a). The list of Health and Safety Code sections for Prop 47 charges that qualify for re-sentencing is 11350, 11357(a), and 11377. These charge-code sections contain further subsections. Also, multiple charges occasionally get bundled; thus, the total number of charges containing any Prop 47 charges is 260 among the complete charge list provided by California county jails. I treat all the subsections and bundled multiple charges eligible for re-sentencing. Table 3 displays examples of charge descriptions.

[3] *Criminal Justice System Involvement and Mental Illness among Unsheltered Homeless in California* (Policy Brief, November 2018).

ulation since one of the stated functions of jails is to handle individuals with mental illness that and/or who have a high risk of committing crimes (Takei, 2017).

The homelessness/social issues are not mutually exclusive when considering offenders' re-entry into communities and mental illness. In addition, evidence has accumulated regarding the difficulty in handling homelessness because the homeless population is more likely to be involved in substance abuse and criminal activities (Coldwell and Bender, 2007; Munthe-Kaas et al., 2018). Moreover, Prop 47, in and of itself reduces the expected cost of committing Prop 47 crimes. Thus, in the current research, I empirically examine the effect that Prop 47 had on the size of the homeless population and changes in government welfare spending and criminal deterrence of non-serious property crimes more generally.

This research builds on the literature on the cost-benefit analysis of incarceration, incapacitation and deterrence by explicitly discussing the effect of decarceration policies. Current evidence finds that the benefits of incarceration are minimal; the deterrence effect resulting from lengthy incarceration is relatively small (Abrams, 2012; Buonanno and Raphael, 2013; Lofstrom and Raphael, 2016). On the other hand, incarceration costs are sizable; a short period of pre-trial detention significantly and negatively impacts the welfare of detained potential offenders (Aizer and Doyle, 2015; Dobbie et al., 2018). Some evidence indicates mixed results; health risk increases among children who experienced family-member imprisonment (Provencher and Conway, 2019), whereas Norris et al. (2021) found that children benefited from parental/sibling imprisonment, lowering their likelihood of incarceration. Additionally, Barbarino and Mastrobuoni (2014) show that incapacitation causally decreased crime, presenting an Italian case where the social cost of releasing prisoners exceeds the cost of keeping them in prison. However, this evidence is less informative regarding US prison reform since Italian prison crowding is much lower than US prison crowding.

Regarding the cost-benefit analysis of decarceration, research on the benefits of decarceration centered on diversion-programs research. However, research indicates that offenders remained risky in terms of recidivism rates under traditional diversion programs (Steadman et al., 1995; Sung, 2011; DeMatteo et al., 2013; Tartaro, 2015; Wong et al., 2016). Thus, according to previous work, there is a call for further research on practical and responsible diversion programs. At the same time, specialized courts such as drug courts and mental-health courts can help to mitigate recidivism (Pettus-Davis and Epperson, 2015). My research builds on this work by examining another decarceration cost, namely, homelessness.

I start by analyzing the effect on the homeless population of re-sentencing, thereby not admitting offenders to county jails, using Continuum of Care (CoC) level data, constructed and defined by the Department of Housing and Urban Development, from 2009 to 2019.[4] US Census State and Local-Government Finance data provide welfare-related spending information (e.g., health and hospital expenditures) so as to examine whether or not Prop 47 stresses welfare-related county government spending. I then use county-jail inmate disposition, admission, and discharge data provided by the California Department of Justice (CA DOJ) to test whether Prop 47 raised recidivism rates for offenders directly influenced by Prop 47. Finally, I refine my analysis to examine the effect of criminal behavior by homelessness status using Los Angeles crime data to see how non-serious property crimes changed after the passage of Prop 47 by offender homelessness status.

My results find a 26-percentage-point increase in the homeless population relative to a set of propensity-score-matched CoC units outside California. Also, health spending per capita increases by 11 percentage points without reducing correctional or judicial costs. County jail disposition data provided by CA DOJ show that Prop 47 decreases county jail crowding, which resulted from shifting the state prison population into county jails. However, recidivism rates for control-group charges remain high after both AB 109 and Prop 47, where the control group is a complementary set of Prop 47 charges sharing the same classification. In other words, Prop 47 offenders commit the control-group charges that are still nonviolent but more severe than Prop 47 charges, considering that Prop 47 charges are regarded as nonviolent and also non-serious charges. Within LA, both the Prop 47 charges and the control-group charges increase among homeless offenders, while non-homeless offenders are the source of a large increase in control-group charges.

Collectively, these findings indicate the failure of re-sentencing policies in solving racial disparities and reducing incarceration spending. While difficult to measure, the current research hints at the re-entry failure of re-sentencing that policymakers overlooked. Specifically, Prop 47 aimed to

[4] 51% of CoC units are comprised of several counties.

reduce the incarcerated population in county jails and decriminalize low-level, non-violent offenses. Ultimately, it appears to lead to increases in the unsheltered homeless population and health-related governmental spending without yielding savings from reduced correctional expenditures.

The remainder of this paper consists of five parts. The following section details the history of institutionalization and re-entry programs of offenders and the mentally ill. Section 3 provides a complete description of the data used in the analysis and the matching process employed to ensure that I find comparable locations. Section 4 describes the estimation strategy for the empirical analysis. Section 5 presents the regression results, and robustness specifications are presented in section 6. Finally, I conclude with a discussion of the empirical results and their implications for criminal justice reform.

2.2 History of Institutionalization of Offenders and the Mentally Ill, and Re-entry

Re-entry of offenders and the mentally ill is the key to successful decarceration policies if the fundamental objective of decarceration is to solve racial disparities and maximize public safety and well-being (Pettus-Davis and Epperson, 2015). Moreover, the ideal re-entry is offenders returning to the community rather than moving between different institutions (mental health facilities, prison, jail, etc.). In this regard, this section discusses the history of (de)institutionalization of offenders and the mentally ill, outlining research efforts on investigating and solving re-entry issues.

In the 1970s, the mentally ill were deinstitutionalized from mental health facilities by shortening the length of their stay in mental health facilities. Empirically, Raphael and Stoll (2013) found the "transinstitutionalziation" effect from mental health hospitals to jails or prisons from the 1980s to 2000s. In contrast, they did not find evidence of the deinstitutionalization effect of mental health facilities on the prison population from the 1950s to the 1980s. In this research, deinstitutionalization started with discharging the elderly and women and then moved towards discharging males. Considering that the elderly and women are not representative prison inmates, this can explain why evidence identified a null effect of early deinstitutionalization from the 1950s to the 1980s of mental health facilities on the prison population, as young white and Black males primarily represent the prison population. Over the years, the mentally ill prevailed in today's jails and prisons, which requires a thorough investigation of proper treatment (Steadman et al., 2009; Torrey et al., 2010; Steadman, 2016).

Recent research examines re-entry programs with a magnified emphasis on targeted treatments for inmates after the prevalence of the mentally ill among prisoners (Vogel et al., 2007).[5] There are numerous studies on re-entry programs, according to the National Institute of Justice on their website.[6] Among about 600 studies, they evaluated 400 studies, including 280 randomized controlled trials. The programs included cognitive-behavioral treatment, education, employment, family-based programs, housing, mental and physical health, sex-offender treatment, substance abuse, supervision and sanctions, and youth re-entry and aftercare programs. Overall results indicate both some partial successes and general failures of treatments.[7] In economics, there are two causally effective studies related to reintegration that concentrate on employment's effect on recidivism. Consistent with previous literature on the value of stable jobs, Schnepel (2018) found that areas with job opportunities for manufacturing and construction had lower recidivism rates than areas with fewer job openings in the manufacturing and construction areas. Also, Bhuller et al. (2020) found rehabilitative training programs enhanced employment and reduced recidivism among inmates. These re-entry studies focus on the success rate of specific re-entry programs. My paper addresses the process of offenders failing to be back to legal society by examining homelessness.

[5]Past research primarily focused on offenders who participated in restorative justice programs rather than directly handled re-entry issues (Visher et al., 2005; Latimer et al., 2005; Ndrecka, 2014; Mitchell et al., 2016; Berghuis, 2018; Lipsey, 2019). Visher et al. (2005) examined eight studies from the 1970s and the 1980s and they found the community-based employment interventions did not statistically lower the rate of recidivism. And this is likely to be a failure of intervention instruments as stable employment itself is the crucial factor in reducing recidivism. On the other hand, twenty-two studies did show a statistically significant effect of employment interventions on recidivism, and studies drew a large proportion of effect sizes from journals without a peer review (less suffering from publication bias). This body of works did not rule out self-selection bias due to the voluntary nature of the restorative justice program (Latimer et al., 2005). On the contrary, a more recent meta-analysis found no significant difference between voluntary and mandatory attendance (Ndrecka, 2014). The most recent meta-analysis hints at the importance of quality treatments; targeted intervention design is effective with the involvement of evaluators and with a focus on cognitive skill, group work, mentoring, and mental health treatment (Lipsey, 2019).

[6]https://crimesolutions.ojp.gov

[7]Nevertheless, two programs are evaluated to substantially affect - cognitive-behavioral treatment and targeted treatment for the mentally ill. In England, several cognitive -behavioral therapies effectively enhanced thinking patterns and cognitive skills of offenders during twenty sessions for two hours each. There is one effective Washington-based study in the US, targeting mentally ill offenders and deploying random assignment designs.

2.3 Data

In this section, I outline the data sources that I use to analyze the effects of AB 109 and Prop 47 on community safety and well-being. I start with discussing the US Department of Housing and Urban Development homelessness data, which collected information from the Continuum of Care units across the entire US. Governmental expenditure information by categories which comes from State and Local Government Finances data by the US Census Bureau. In addition, I discuss control variables sourced from the National Cancer Institute, Bureau of Economic Analysis, and US Census Bureau, utilized to find a suitable comparison group. I also discuss charge disposition and jail admission/discharge data with codified inmate identifiers from all of the county jails within California. I then provide a discussion of charge data that are specific to Los Angeles, including offender information by homelessness status.

2.3.1 Nation-Wide Data Sources

US Department of Housing and Urban Development Homeless Data

US Department of Housing and Urban Development (HUD) data are utilized, which provide measures of homeless populations from Point-in-Time (PIT) surveys, and measures of homelessness resources from the Housing Inventory Count (HIC) survey. These measures are relatively consistent from 2007 to 2019, collected in the middle of January each year. The unit of measures is the Continuum of Care level (CoC), which comprises 386 entities across the United States, geographically smaller than states but more aggregated than counties. Figure 1 shows that the average number of the unsheltered homeless population is trending upward in the CoC units in California. On the other hand, other CoC units outside of California do not on average exhibit an upward trend. In the analysis, the treatment group includes every CoC in California, and the pool of control locations has every CoC outside of California.

State and Local Government Finances

Government spending data, which will be incorporated as an outcome variable, are sourced from State and Local Government Finances published annually by the US Census Bureau. I use a database compiled by Pierson et al. (2015), which generates a measure of coherence for

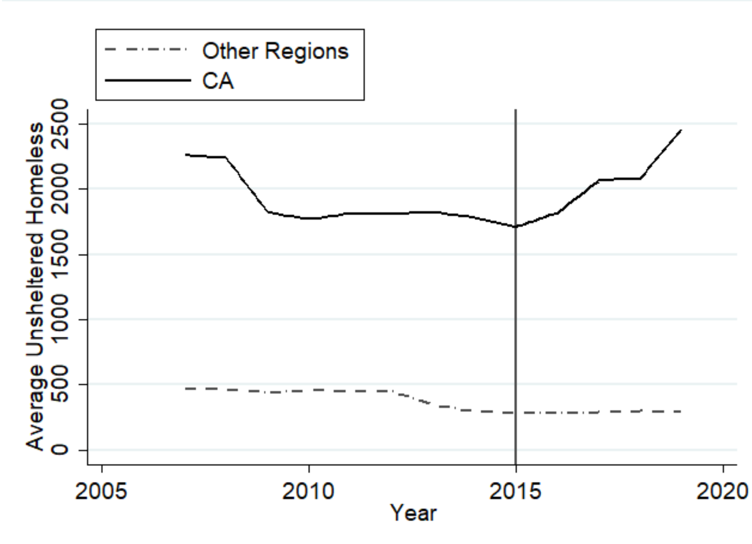


Figure 1: Unsheltered Homeless Population

detailed expenditure categories over time. I select seven variables in the analysis - health, policing, correctional, judicial, hospital, welfare, and community development expenditures per capita (in current dollars). One shortcoming of the data is it is only available for a set of representative counties. Nevertheless, US Census collects information from more than half the total counties in the US.[8] After combining data sets based on a crosswalk between CoC units and counties (Byrne et al., 2013), more than 80 percent of available data points among counties were mapped into CoC units (Figure 2).[9]

Covariates

Covariates for matching are obtained from the Surveillance, Epidemiology, and End-Results Program (SEER) data provided by National Cancer Institute, Personal Income and Employment by Major Component (CAINC4) data from the Bureau of Economic Analysis, and the American Community Survey 5-Year Estimates (ACS5) from the US Census Bureau. Specifically, SEER data contains population demographics such as the percent of Female, White, and Black population by

[8]US Census collects information from the entire 3,006 counties in a 5-year cycle. For example, the data contain the universe of the entire counties in 2012 and 2018. In 2009, 2010, 2011, 2013, 2014, 2015, 2016, and 2017 (except for 2012 and 2018), the data have information for around 1,600 counties.

[9]Some large counties (e.g., Los Angeles County) were distributed into several CoC units. In these instances, I weight counties' governmental spending and socioeconomic values by the number of CoC units. The implicit assumption is that several CoC units evenly share the burden of homelessness.

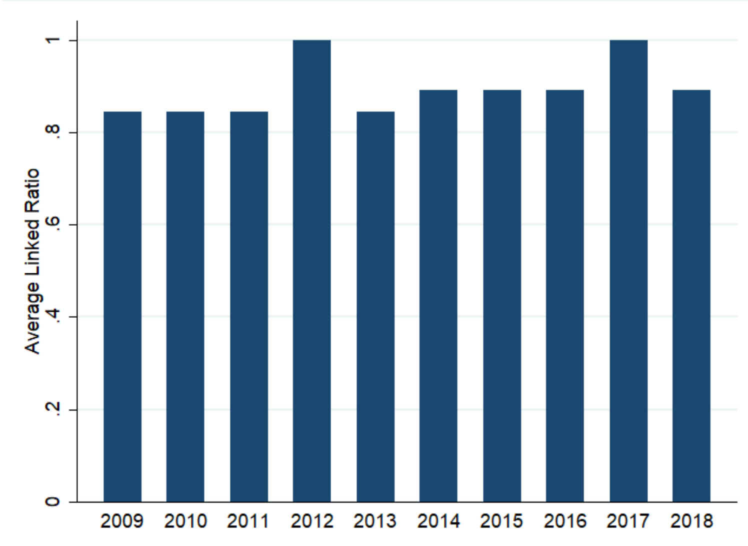


Figure 2: Linked Ratio of Counties to CoC Units

age groups (5-year breaks from 10 to 64). CAINC4 data includes averages of farm/non-farm income, proprietors' income, total income, wage, employer/employee contributions for pension and insurance funds, full-time and part-time employment, and proprietors' employment in each county. ACS5 contains geographic mobility by race (White, Black, and Asian), poverty status by race, population by occupation, and sex by race. Additionally, I use information from the unsheltered homeless population and the number of temporary shelter beds from HUD.

Generating an Appropriate Control Group

I conduct a matching procedure using a random-forest method and optimal matching, using data from before the treatment period (2009 to 2014) so as to avoid contamination from covariates after treatment has occurred. Note that the homeless population should not be influenced by AB 109 because inmates are still housed within carceral facilities. AB 109 transfers them from the state prison to county jails.[10] The random forest method utilizes a non-parametric procedure, which reduces the collinearity problem of including too many variables for matching. 111 CoC units are selected as a control group for the 40 treated CoC units.[11] After matching, several variables

[10] At best, county correctional expenditures could be impacted by AB 109. However, there is no evidence county jail staffing drastically increased, which is consistent with the trend of average correctional costs presented in Figure 7 in Section 2.4.

[11] Table 10 displays the list of CoC units utilized in the analysis.

are aggregated and included in the regression analysis as a set of covariates, including the share of populations by race and sex, employment per capita, and personal income per capita (Table 1). The differences between groups are not economically meaningful but statistically significant;[12] thus, I can control for some aggregated covariates (below) in the final regressions.

Table 1: Descriptive Statistics Comparing Groups

	Total	Treat	Control
Unsheltered Homeless	530.17***	1915.10	646.41
	(1752.82)	(4219.96)	(1319.54)
Percent White Male 10-64	0.31***	0.31	0.33
	(0.05)	(0.04)	(0.05)
Percent White Female 10-64	0.31***	0.31	0.33
	(0.05)	(0.04)	(0.05)
Percent Black Male 10-64	0.05***	0.02	0.03
	(0.04)	(0.01)	(0.03)
Percent Black Female 10-64	0.05***	0.02	0.03
	(0.05)	(0.02)	(0.04)
Percent Asian Male 10-64	0.02***	0.05	0.02
	(0.03)	(0.03)	(0.04)
Percent Asian Female 10-64	0.02***	0.05	0.02
	(0.03)	(0.04)	(0.04)
Employment per capita	0.58***	0.54	0.59
	(0.12)	(0.12)	(0.11)
Personal Income per Capita	47.06***	50.68	47.58
	(13.79)	(18.94)	(12.84)
Observations	4101	400	1060

*** p<0.01, ** p<0.05, * p<0.10 for indicating statistical difference between groups. Standard deviations are in parenthesis.

2.3.2 California State-Wide Data Source

Two more data sources are included to further delve into the detailed impact of Prop 47 on local communities. First, the California Department of Justice (CA DOJ) data provide daily charge disposition information with codified individual identifiers. The data enable me to examine the effect of decarceration policies on the jail populations and the number of offenders in jails. Importantly, this data set allows me to calculate the recidivism of discharged offenders.[13] I utilize data for the period January 2005 - December 2018. The analysis then compares charges eligible

[12] A group of crucial variables likely determine the propensity score among 185 variables deployed in the propensity score matching, thus leaving space for several socioeconomic control variables to remain different at an aggregate level.

[13] I focus on recidivism within one year to avoid data censoring in later years.

for Prop 47 with similar charges but which are not impacted by Prop 47. Specifically, nine charge groups out of forty-two (defined by CA DOJ) contain at least one charge eligible for Prop 47 (bold text in Table 2). The control group is a complementary set of Prop 47 charges within each charge group.[14]

Table 2: CA DOJ Classification of 42 Charge Groups

Group Code	Description
01	SOVEREIGNTY
02	MILITARY
03	IMMIGRATION
04	FEDERAL OFFENSE
08	JUVENILE OFFENSE
09	HOMICIDE/MANSLAUGHTER
10	KIDNAPPING
11	SEXUAL ASSAULT
12	ROBBERY
13	ASSAULT
14	ABORTION
16	TERRORIST THREATS
20	ARSON
21	EXTORTION
22	BURGLARY
23	**LARCENY**
24	STOLEN VEHICLE
25	**FORGERY**
26	**FRAUD**
27	**EMBEZZLEMENT**
28	**STOLEN PROPERTY**
23	PROPERTY DAMAGE
30	**CREDIT CARD/ACCESS CARD OFFENSE**
31	**IMPROPER BUSINESS PRACTICE**
32	TRESPASS
33	SCHOOL DISTURBANCE/TRESPASS
34	INHUMANE TREATMENT OF ANIMALS
35	**DANGEROUS DRUGS/NARCOTICS**
36	SEX OFFENSE
37	OBSCENE MATTER
38	FAMILY OFFENSE
39	GAMBLING
40	COMMERCIAL SEX
41	LIQUOR OFFENSE
42	DRIVING UNDER INFLUENCE
43	CONTRIBUTE DELINQUENCY OF MINOR
44	IMPERSONATION
45	LIBEL/SLANDER
46	ACCESSORY/CONSPIRACY
47	PUBLIC JUSTICE
48	OBSTRUCT PUBLIC OFFICER
49	FLIGHT/ESCAPE

Table 4 presents a summary of offender information with treatment and control groups. The

[14]Two hundred sixty charges from the treatment group and 1,600 charges from the control group account for approximately 25% of the total charges. Table 3 presents the example charges.

Table 3: CA DOJ Example List of Charges Classified into Prop 47 and Control 47 Charges

Charge Description	Charge Group	Type	Disqualification
LARCENY	23		
602 WI-JUVENILE/LARCENY	23	Control47	0
182 PC-CONSPIRACY/LARCENY	23	Control47	0
664 PC-ATTEMPT CRIME/LARCENY	23	Control47	0
220 PC-ASSAULT TO COMMIT GRAND LARCENY	23	Control47	1
337.4 PC-OBTAIN OVER $200 BY TOUTING	23	Control47	0
355 PC-DESTROY EVIDENCE OF OWNERSHIP	23	Control47	0
356 PC-DESTROY OWNER'S ID MARK ON LUMBER	23	Control47	0
484 PC-THEFT	23	Prop47	0
484(A) PC-THEFT OF PERSONAL PROPERTY	23	Prop47	0
484(B) PC-THEFT:NONRETURN OF RENTAL PROPERTY	23	Prop47	0
484H(B) PC-FAIL TO GIVE GOODS AS STATED	23	Prop47	0
485 PC-APPROPRIATE LOST PROPERTY	23	Control47	0
487 PC-GRAND THEFT	23	Prop47	0
487.1 PC-GRAND THEFT:PROPERTY	23	Prop47	0
487.2 PC-GRAND THEFT FROM PERSON	23	Prop47	0
487.3 PC-GRAND THEFT:MISCELLANEOUS	23	Prop47	0
487A PC-GRAND THEFT:ANIMAL CARCASS	23	Prop47	0
487B PC-GRAND THEFT:CONVERT REAL PROPERTY	23	Prop47	0
STOLEN VEHICLE	24		
602 WI-JUVENILE/STOLEN VEHICLE	24	Control47	0
182 PC-CONSPIRACY/STOLEN VEHICLE	24	Control47	0
664 PC-ATTEMPT CRIME/STOLEN VEHICLE	24	Control47	0
487.3 PC-GRAND THEFT:AUTO	24	Prop47	0
499B PC-TAKE VEHICLE FOR TEMPORARY USE	24	Control47	0
499D PC-TAKE AIRCRAFT W/O OWNER'S CONSENT	24	Control47	0
18 2312 US-INTERSTATE TRANSPORT STOLEN VEHICLE	24	Control47	0
AUTO THEFT	24	Control47	0
503 VC-TAKE CAR W/OUT OWNERS CONSENT	24	Control47	0
FORGERY	25		
602 WI-JUVENILE/FORGERY	25	Control47	0
182 PC-CONSPIRACY/FORGERY	25	Control47	0
664 PC-ATTEMPT CRIME/FORGERY	25	Control47	0
115 PC-OFFER/ETC FALSE/FORGED INSTRMNT TO FILE	25	Control47	0
366 PC-COUNTERFEIT QUICKSILVER STAMPS	25	Control47	0
470 PC-FORGERY	25	Prop47	0
471 PC-MAKE FALSE ENTRIES IN RECORDS	25	Prop47	0
472 PC-FORGE OFFICIAL SEAL	25	Prop47	0
473 PC-FORGERY	25	Prop47	0
474 PC-SEND FORGED TEL/TEL MESSAGE TO DEFRAUD	25	Prop47	0
475 PC-POSS/ETC FORGED NOTES/ETC	25	Prop47	0
475A PC-POSSESS BAD CHECK/MONEY ORDER	25	Prop47	0
476 PC-MAKE/PASS FICTITIOUS CHECK	25	Prop47	0
477 PC-COUNTERFEITING	25	Control47	0
478 PC-COUNTERFEITING	25	Control47	0
479 PC-POSSESS/RECEIVE COUNTERFEIT COIN/ETC	25	Control47	0

Note: disqualification column indicates a previous record of the marked charge that disqualifies offenders from being qualified for Prop 47 classification.

Prop 47 charge descriptions include drug possession, receiving stolen property, theft, shoplifting, writing bad checks, and check forgery. The control charges are a complementary set of Prop 47 charges sharing the same charge-group classification defined by the CA DOJ. Table 3 details how different charges are classified into each charge group and which specific charges are part of the Prop 47 group and which are in the control group. Additionally, the "Violent" charge group includes burglary, robbery, and assault (charge group code 22, 12, and 13 respectively in Table 2). "Other" charge group contains all other charges excluding the charges described above.

2.3.3 Los Angeles City-Wide Data Source

Next, I linked Los Angeles Police Department daily charge data to the homelessness status of offenders, provided by the New York Times. The data include race/sex information for individual offenders and misdemeanor complaints about charges from 2012 to 2016. These data make it possible to test which population between homeless and non-homeless offenders drives potential increases in non-serious property crimes. Finally, I aggregated the data to the monthly level.

I further subset the data to focus on relevant observations for the analysis. With a similar logic in the California DOJ data, I use Prop 47 charges as a treatment group and focus on the charge groups that contain at least one Prop 47 charge. Six charge groups include Prop 47 charges (bold text in Table 5), "Forgery/Counterfeit," "Fraud/Embezzlement," "Larceny," "Narcotic Drug Laws," "Receive Stolen Property," and "Vehicle Theft" charge groups. The control group is a complementary set of Prop 47 charges within each charge group.

To determine the plausibility of the Prop 47 treatment and control groupings, I examine the share of misdemeanor complaints per charge across time. Prop 47 converted some felony charges into misdemeanors; thus, the treatment group is associated with the increased percentage of misdemeanor complaints. The treatment group (Prop 47 charges) displays an increase in the ratio of misdemeanor complaints from 0.3 to 0.6 in the left panel of Figure 3. On the contrary, the control group did not show a similar increase (right panel of Figure 3).

Table 4: Comparison of Before and After Prop 47 among Different Charge Groups

	Total	Before AB109	AB109	Prop47
Control 47 Offender	379.29	429.51	330.18	333.57
	(663.91)	(752.28)	(534.40)	(586.08)
Control 47 Charge	994.90	1123.99	874.20	873.23
	(1597.48)	(1808.54)	(1294.29)	(1405.45)
Control 47 Discharge	66.37	79.21	60.56	48.78
	(128.13)	(150.32)	(101.93)	(99.23)
Control 47 Repeat Any	55.81	67.24	50.75	40.07
	(108.13)	(127.79)	(84.49)	(82.18)
Control 47 Repeat Prop 47	13.06	14.43	12.78	10.93
	(25.62)	(28.25)	(21.94)	(23.25)
Control 47 Repeat Control 47	29.17	34.97	26.22	21.48
	(53.89)	(63.31)	(41.89)	(42.11)
Control 47 Repeat Violent	7.58	8.87	7.57	5.38
	(14.19)	(16.32)	(12.21)	(11.07)
Control 47 Release	7.14	8.37	6.73	5.33
	(22.74)	(27.36)	(17.47)	(16.63)
Prop 47 Offender	492.42	525.05	504.82	429.91
	(960.99)	(1057.66)	(974.99)	(761.31)
Prop 47 Charge	1680.05	1744.63	1738.03	1531.46
	(2762.47)	(2975.92)	(2790.55)	(2342.87)
Prop 47 Discharge	123.44	131.24	142.28	95.70
	(247.50)	(268.40)	(276.57)	(173.69)
Prop 47 Repeat Any	104.82	110.42	122.59	81.61
	(207.61)	(223.46)	(232.64)	(149.09)
Prop 47 Repeat Prop 47	53.62	53.89	66.13	43.47
	(104.59)	(109.30)	(122.21)	(77.01)
Prop 47 Repeat Control 47	38.11	38.54	43.12	33.50
	(64.96)	(67.39)	(68.67)	(57.04)
Prop 47 Repeat Violent	17.67	18.31	21.84	13.35
	(33.35)	(35.19)	(38.99)	(23.48)
Prop 47 Release	9.08	9.66	11.08	6.55
	(30.94)	(35.16)	(34.93)	(16.53)
Observations	9621	4658	2116	2847

Standard deviations are in parenthesis.

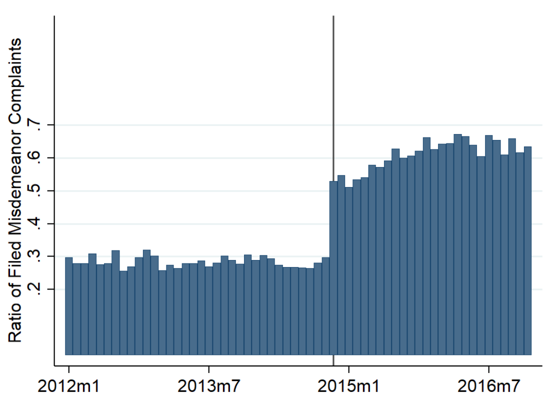
Notes: "Before AB109" columns shows the averages of observations from January, 2005 to September, 2011. "AB109" column shows the averages of observations from October, 2011 to October, 2014. "Prop47" column shows the averages of observations from November, 2014 to December, 2018.

Notes: "Prop 47" charges include drug possession, receiving stolen property, theft, shoplifting, writing bad checks, and check forgery, which were impacted by Proposition 47. "Control 47" charges share the same upper categories with "Prop 47". "Violent" charges include burglary, robbery, and assault. "Other" charges are all other charges excluding "Prop 47", "Control 47", and "Violent".

Table 5: List of Charge Groups and Total Number of Incidents

Charge Group	Count	Percent
Against Family/Child	4,576	0.77
Aggravated Assault	50,423	8.53
Burglary	14,564	2.46
Disorderly Conduct	2,174	0.37
Disturbing the Peace	466	0.08
Driving Under Influence	73,560	12.44
Drunkeness	2,776	0.47
Federal Offenses	119	0.02
Forgery/Counterfeit	4,786	0.81
Fraud/Embezzlement	5,860	0.99
Gambling	212	0.04
Homicide	1,350	0.23
Larceny	39,322	6.65
Liquor Laws	792	0.13
Miscellaneous Other Violations	116,707	19.74
Moving Traffic Violations	45,533	7.7
Narcotic Drug Laws	112,991	19.11
Non-Criminal Detention	9	0
Other Assaults	34,310	5.8
Pre-Delinquency	9	0
Prostitution/Allied	21,794	3.69
Rape	1,595	0.27
Receive Stolen Property	4,730	0.8
Robbery	13,338	2.26
Sex (except rape/prst)	6,142	1.04
Vehicle Theft	15,160	2.56
Weapon (carry/poss)	17,918	3.03

Note: charge groups in bold text qualify for Proposition 47.



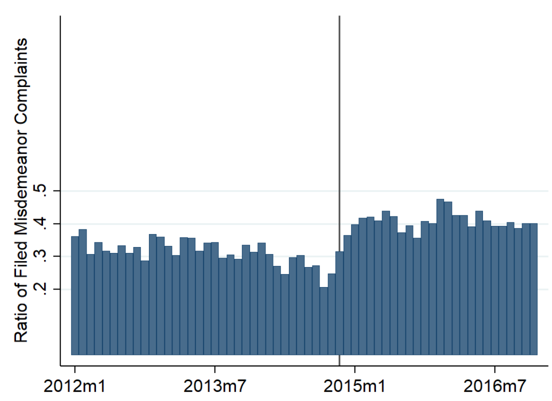


Figure 3: Share of Misdemeanor Complaints per Charge - Treatment Group Vs. Control Group

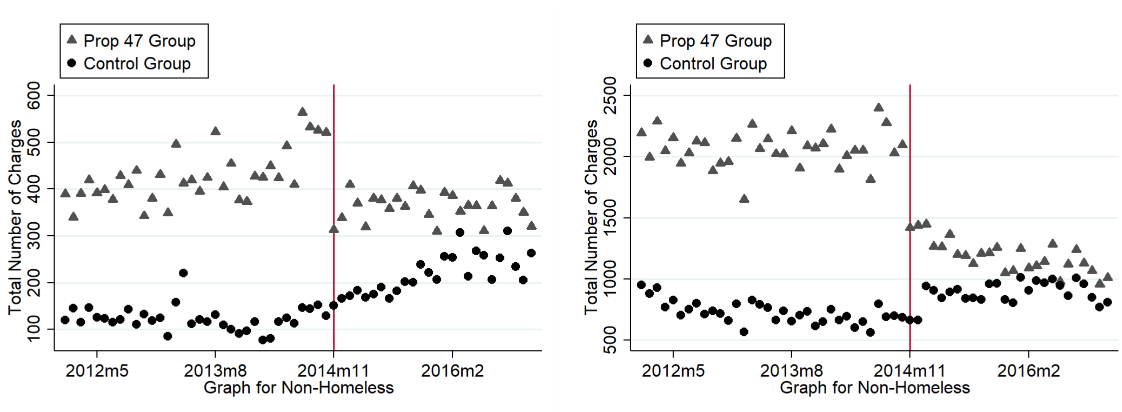


Figure 4: Comparing Total LA Crime Trends between Groups by Offender-Homelessness Status Based on Raw Data

Location information contains an address unit obscuring to the "nearest hundred block" level[15] and 21 policing-area units. I use the 21 policing areas as the unit of analysis. The graphical representation of the average number of charges by the policing areas in Figure 5 mirrors the total number of charges from the raw data in Figure 4.[16] The homeless offenders are predominantly older, Black and white citizens, and more prevalent in the Central and Hollywood policing areas (Table 6).

The left panel of Figure 5 shows crime trends among homeless offenders. Prop 47 impacted the number of Prop 47 charges and the number of control charges, a potential violation of $SUTVA$.[17] Graphically, the number of Prop 47 charges decreased, and the control charges exhibited upward trends. The right panel of Figure 5 shows charge trends among non-homeless offenders. The effect is more drastic because Prop 47 charges abruptly decreased, whereas the control charges show a smooth upward trend.

[15] For example, if the true address was 216 Main Street, my data would simply state 200 Main Street.

[16] The trends by the "nearest hundred block" address unit in Figure 6 do not mirror the raw data, likely due to the widely unbalanced data structure. Specifically, among the entire 182,000 daily observations from 2012 to 2016, there are 23,000 unique "nearest hundred block" address units. On average, $23{,}000/182{,}000 = 8$ incidents per address unit occurred across the entire time. The variance is substantial, as the most frequent address unit appeared 1,300 times. Since the OLS regression is central to the Difference-in-Differences strategy, the unbalanced data structure is likely to result in biased and inconsistent estimates. Although not reported, checking the regression results with the "nearest hundred block" address unit reveals the direction of coefficients remains the same regardless of the unit of analysis. Also, the treatment effect remains statistically significant among non-homeless offenders with a smaller coefficient magnitude compared to the regression result reported in Section 2.5.

[17] This means we need to exercise caution in interpreting the regression coefficients due to spillover effects.

Table 6: Summary between Homeless Offenders and Non-homeless Offenders

	Total	Homeless	Non-Homeless
Male	0.79***	0.82	0.79
Age	34.48***	39.33	33.61
Felony	0.45***	0.43	0.45
Misdemeanor	0.52***	0.51	0.52
Other	0.03***	0.06	0.03
Black	0.32***	0.39	0.31
Hispanic	0.44***	0.28	0.47
Other	0.05***	0.03	0.06
White	0.18***	0.30	0.16
77th Street	0.07***	0.03	0.08
Central	0.09***	0.22	0.07
Devonshire	0.03***	0.02	0.03
Foothill	0.04***	0.03	0.05
Harbor	0.04***	0.03	0.04
Hollenbeck	0.03***	0.02	0.03
Hollywood	0.08***	0.13	0.07
Mission	0.05***	0.04	0.06
N Hollywood	0.05	0.05	0.05
Newton	0.05***	0.04	0.05
Northeast	0.03	0.04	0.03
Olympic	0.04***	0.03	0.04
Pacific	0.05***	0.08	0.05
Rampart	0.04***	0.05	0.04
Southeast	0.05***	0.02	0.05
Southwest	0.06***	0.03	0.07
Topanga	0.04***	0.03	0.04
Van Nuys	0.06***	0.05	0.06
West LA	0.02***	0.03	0.02
West Valley	0.04***	0.03	0.04
Wilshire	0.03**	0.03	0.03
Observations	591216	90743	500473

*** $p<0.01$, ** $p<0.05$, * $p<0.1$ for indicating statistical difference between groups

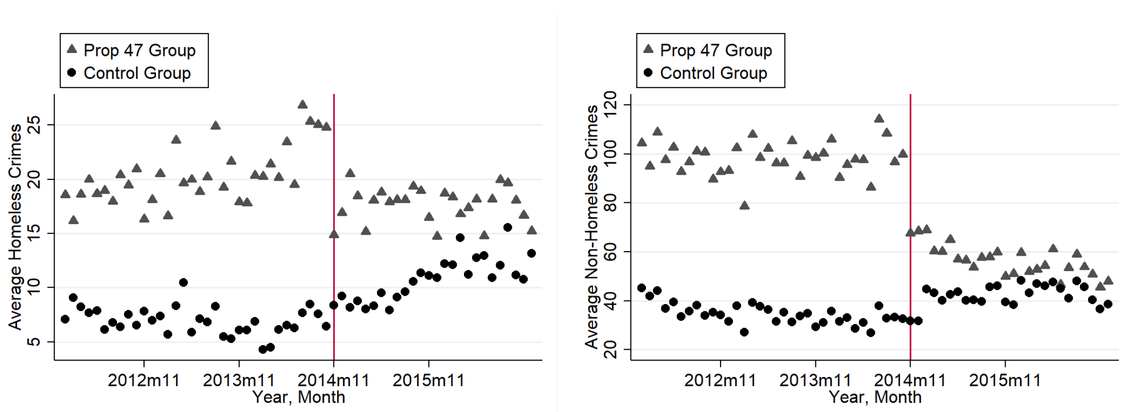


Figure 5: Comparing Average LA Crime Trends between Groups by Offender Homelessness Status Based on 21 Policing Area Unit

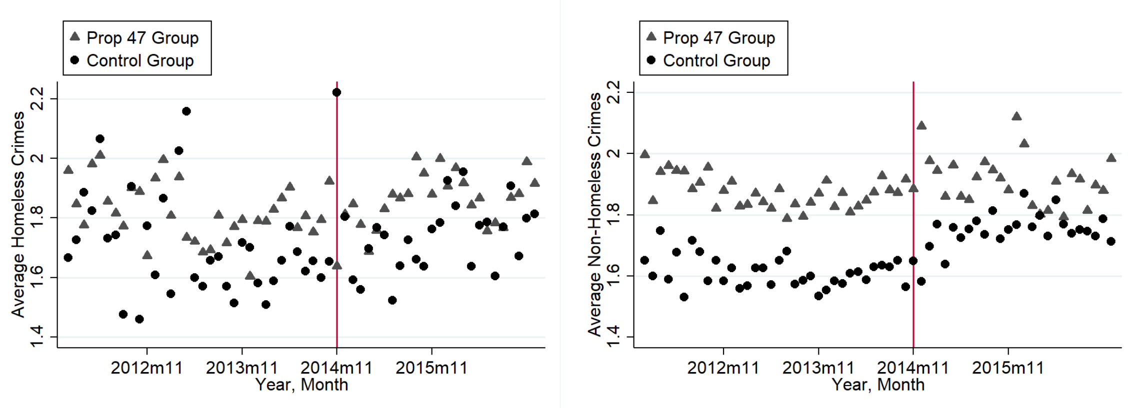


Figure 6: Comparing Average LA Crime Trends between Groups by Offender Homelessness Status Based on Address Unit

2.4 Empirical Strategy

2.4.1 Nation-Wide Data

I examine the effect of Prop 47 on the homeless population and government spending using a differences-in-differences (DiD) strategy (Equation 1):

$$log(y_{it}) = \alpha_0 + \alpha_1 treat_i \times post_t + X'r + c_i + t_t + e_{it}, \tag{1}$$

where y_{it} is the outcome of interest to measure homelessness and governmental spending. $treat_i$ is indicating CoC units impacted by Prop 47. $post_t$ is an indicator variable for the period after Prop 47 was enacted on November 5^{th}, 2014. c_i and t_t are individual CoC and year-fixed effects, respectively. X is a set of socioeconomic control variables including the percentage of the population with aged 10 to 64 by Female, White, Black, and Asian populations, the average income per capita, and employment per capita to absorb variation at the CoC level over time. Robust standard errors are clustered in CoC unit.

The crucial identifying assumption to apply Difference-in-Differences identification strategy is that in the absence of Prop 47, adopting counties would have experienced changes in outcome variables similar to non-adopting counties in comparable locations. The homeless and government spending data make it possible for me to support the parallel trend assumption by presenting visual

Figure 7: Comparison of Homeless Population and Governmental Expenditure between Treatment and Control Groups after Matching including Unsheltered Homeless, and Health, Policing, and Correctional Expenditures

evidence that the outcomes of the two groups are parallel in the years prior to adoption (Figure 1). Regarding governmental expenditures, Figure 7 reveals that parallel pre-trends hold in the unsheltered homeless population, health, and correctional spending.

In addition, the history of homeless policies in Los Angeles supports the assumption of no anticipation effects corresponding to Prop 47 on the homeless population.[18] Sheeley et al. (2021) organized a detailed history of homelessness within Los Angeles, where both homeless and resources for the homeless are prevalent. They note that the most relevant policy, before Prop 47, was *not* to pursue a plan of replacing part of a Men's Central Jail facility into an integrated mental illness

[18]This does not mean California did not put any effort into handling the homelessness issue by itself. Historically, California counties' awareness about homelessness goes back to the 1980s, and some stylized facts emphasize education, employment, and affordable housing (Quigley et al., 2001; United States Interagency Council on Homelessness, 2015).

treatment center in 2013. The second most recent policy occurred in 2007, a permanent supportive housing program targeting the most vulnerable population, launched by the Board of Supervisors in Los Angeles. After that, the Los Angeles County Board of Supervisors established a grand plan to solve the homelessness issue collaboratively with other counties, cities, and community partners in 2015 (Los Angeles County Chief Executive Office, 2016), which indicates LA county initiated homeless policies and programs after Prop 47 in 2014.

2.4.2 California State-Wide Data

Second, I examine the the effect of AB 109 and Prop 47 on county jail population within California using a differences-in-differences (DiD) strategy (Equation 2):

$$\begin{aligned} log(y_{it}) = \alpha_0 + \alpha_1 treat_i \times ab109_t + \alpha_2 treat_i \times prop47_t \\ +X'r + c_i \times treat_i + t_t + e_{it}, \end{aligned} \tag{2}$$

where y_{it} includes the number of offenders and recidivism. $treat_i$ is an indicator variable whether Prop 47 charge group or control charge group. $ab109_t$ is an indicator for the period October, 2011 to October (AB 109 period), 2014. $prop47_t$ is an indicator for the period November, 2014 to December, 2018 (Prop 47 period). c_i are county-fixed effects. t_t is month-by-year fixed effects. Different from Equation 1, the current equation includes county by Prop 47 status fixed effects because geographical location is not the main variable to separate treatment and control groups (the treatment group is classified by specific charges). Thus, county-by-month-by-year fixed effects are included to absorb geographical variations across time regardless of treatment status. Robust standard errors are clustered according to county by Prop 47 status.

The identifying assumption is that offenders with offenses impacted by AB 109 and Prop 47 would have behaved similarly to those with comparable offense charges which were not affected by Prop 47. CA DOJ data make it possible for me to support the parallel trend assumption by presenting visual evidence that the jail population trends between two groups are parallel in the

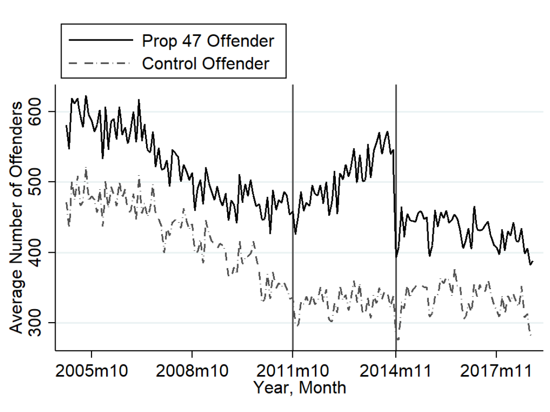


Figure 8: Number of Prop 47 Charges and Control Charges

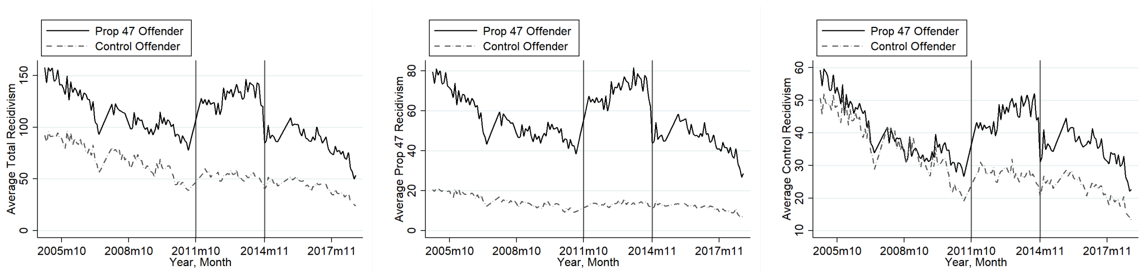


Figure 9: Recidivism for All Charges Vs. Prop 47 Charges Vs. Control Charges

years prior to the adoption of AB 109 (Figure 8 and Figure 9).[19] Thus I set the periods before AB 109 as pre-treatment periods for the Difference-in-Differences strategy.

2.4.3 Los Angeles City-Wide Data

Lastly, I examine the the effect of Prop 47 on homeless and non-homeless crime rates within Los Angeles using a differences-in-differences (DiD) strategy (Equation 3):

$$\begin{aligned} log(y_{it}) &= \alpha_0 + \alpha_1 treat_i \times post_t + a_i \times t_t \\ &\quad + a_i \times c_i \times treat_i + e_{it}, \end{aligned} \tag{3}$$

[19]For graphical representation, I dropped observations six months before December 2007, 2011, and 2015 due to massive reductions in the numbers, consistent between groups and charges. Since a Difference-in-Differences strategy is used, the parallel trend between groups and charges cancels out in the final regression outcomes; although not reported, the regression results remain the same before and after dropping these observations.

where $treat_i$ is 1 if a charge is a Prop 47 charge and 0 if a charge is the complementary set of Prop 47 charges. a_i, c_i, and t_t are indicator variables for the 21 policing area units, charge-group, and month-by-year fixed effects, respectively. Different from Equation 1, the current equation includes policing area by charge group by Prop 47 status-fixed effects because geographical location and charge group are not the main variables to separate treatment and control groups (the treatment group is classified by specific charges). Thus, policing area-by-month-by-year fixed effects are controlled for absorbing geographical variations across time regardless of treatment status. Robust standard errors are clustered in policing area by charge group by Prop 47 status.

The identifying assumption is that offenders with offenses impacted by Prop 47 would have behaved similarly to those with comparable offense charges which were not affected by Prop 47. LA crime data make it possible for me to support the parallel trend assumption by presenting visual evidence that the outcomes of the two groups are parallel in the years prior to the adoption of Prop 47 (Figure 5).

2.5 Results

2.5.1 Nation-Wide Data

Table 7 displays the regression results from Equation 1. Columns (1), (3), and (5) show the estimates from the DiD specification on the unsheltered homeless population, health expenditures, and policing expenditures. Columns (2), (4), and (6) include DiD estimates for each year, which are also plotted in Figure 10. The coefficient in Column (1) means Prop 47 leads to a 26.7 percentage-point increase in the unsheltered homeless population. Similarly, health and policing expenditures increased (however, policing expenditures are accompanied by changes in pre-trend). The 11 percentage-point increase in health spending in Column (3) indicates governmental financial stress on health expenditures. Since the outcome variable is health spending per capita, the results imply individual California residents bear an 11 percentage point larger burden than residents in the control group, CoC units outside California. The even-numbered Columns indicate the treatment effects each year. Column (2) reveals that the effects come from 2015 and 2016, and homelessness' most considerable impact from 2019. Column (4) shows a more consistent effect across time

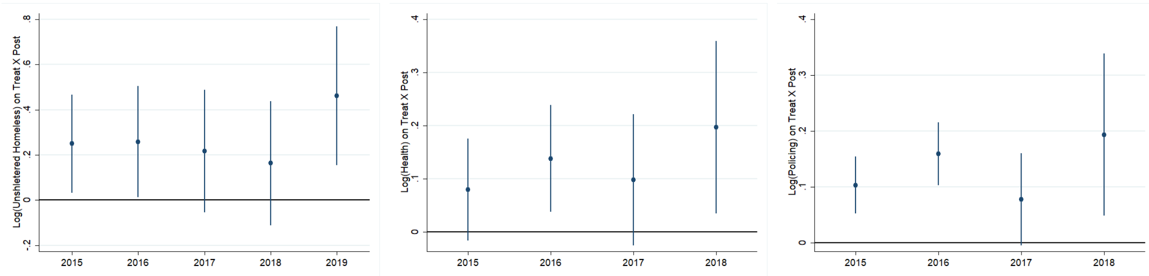


Figure 10: Coefficients Plot for DiD Estimates Each Year - Unsheltered Homeless, Health, Policing Expenditures

in health spending. Still, the effect sizes are more prominent in later years. This phenomenon could be due to the accumulation of homeless and ex-convict populations. In other words, without successful reentry, those populations could keep rising, stressing government spending more over time.

Table 7: The Effects of Prop 47 on Homelessness and Government Finance

	Log(Homeless)		Log(Health)		Log(Police)	
	(1)	(2)	(3)	(4)	(5)	(6)
	DiD	Annual	DiD	Annual	DiD	Annual
Prop47 × Post	0.267**		0.117**		0.135***	
	(0.118)		(0.053)		(0.035)	
Prop47 × 2015		0.251**		0.073		0.106***
		(0.109)		(0.049)		(0.026)
Prop47 × 2016		0.255**		0.132**		0.160***
		(0.123)		(0.051)		(0.028)
Prop47 × 2017		0.225		0.089		0.087**
		(0.137)		(0.062)		(0.042)
Prop47 × 2018		0.156		0.182**		0.197***
		(0.140)		(0.082)		(0.073)
Prop47 × 2019		0.466***				
		(0.155)				
Observations	1620	1620	1368	1368	1392	1392
Mean of Dept. Var. (Level)	1516		146		118	
F stat	5.03					
R^2	0.935	0.935	0.933	0.933	0.944	0.944
CoC and Year FEs	X	X	X	X	X	X
Covariates	X	X	X	X	X	X

Robust standard errors clustered at the CoC level in parentheses
*** p<0.01, ** p<0.05, * p<0.10

2.5.2 California State-Wide Data

Table 8 presents the regression results for Equation 2. The jail population (Column 1), discharged population (Column 2), and recidivism within one year (Column 3) showed statistically significant increases during the AB109 period and also increased during Prop 47 periods compared to periods before AB109. However, the Prop 47 effects are statistically smaller than the AB 109 effect. Combined with the graphical trends in Figure 8, this implies Prop 47 is likely to absorb the jail crowding shock from the 2011 realignment, consistent with the purpose of Prop 47. The difference between AB 109 and Prop 47 effects is approximately 0.10 across outcomes in Columns (1), (2), (3), and (6). In other words, Prop 47 decreases the amount of each outcome increased from AB 109 by ten percentage points, which translates into around 43 offenders based on the average number of offenders in Column (1). Similarly, the results translate into about nine discharges based on the average number of discharges in Column (2), around eight recidivism based on the average number of recidivism in Column (3), and approximately 13 recidivism of violent crimes in Column (6).

I observe heterogeneous effects among different types of recidivism behaviors. Column (4) in Table 8 estimates the Prop 47 effect on recidivism within one year, specifically Prop 47 charges. The difference between the AB109 and Prop 47 effect is 0.16 (0.315 - 0.156) for Prop 47 charge recidivism in Column (4), which means that Prop 47 decreases the amount of each outcome increased from AB 109 by 16 percentage points (greater than ten percentage points in Columns (1), (2), (3), and (6)). However, in Column (5), Prop 47 fails to absorb the shock from AB 109 for recidivism behavior for control-group charges (a complementary set of Prop 47 charges). The coefficient in Column (5) indicates that the Prop 47-charge offenders re-offend control-group charges 39 percentage points more, on average than control-group charge offenders re-offending control-group charges. Considering control charges are a complementary set of Prop 47 charges (which are non-serious and non-violent charges according to policymakers), this indicates a spillover effect from non-serious charges to charges that are still non-violent but more serious than Prop 47 charges.

2.5.3 Los Angeles City-Wide Data

The violation of SUTVA makes it hard to interpret the regression results. Additionally, the narcotic drug-law charges can be the driving charge group of the results considering they are 62% of the data used in the analysis (113,000 out of 183,000 daily charges used for the analysis from 2012 to 2016 shown in Table 5). Thus, I present the graphs of average trends (Figure 11) excluding

Table 8: DiD Estimates of the Effects of AB 109 and Prop 47 on Number of Offenders, Discharges, and Recidivism within California

	(1) Log(Offender)	(2) Log(Discharge)	(3) Log(Total Repeat)	(4) Log(Prop47 Repeat)	(5) Log(Control Repeat)	(6) Log(Violent Repeat)
Treat × Post1 (AB 109)	0.185***	0.336***	0.375***	0.315***	0.362***	0.328***
	(0.023)	(0.030)	(0.033)	(0.032)	(0.037)	(0.038)
Treat × Post2 (Prop 47)	0.097***	0.226***	0.278***	0.156***	0.390***	0.249***
	(0.035)	(0.043)	(0.045)	(0.053)	(0.042)	(0.050)
Observations	18932	17444	17048	14052	15100	12440
Mean of Dept. Var (Level)	436	95	81	34	34	13
Linear Combination	p<0.01	p<0.01	p<0.01	p<0.01	p>0.1	p<0.10
R^2	0.992	0.980	0.979	0.967	0.968	0.945
County by Year-Month FEs	X	X	X	X	X	X
County by Prop 47 FEs	X	X	X	X	X	X

Robust standard errors clustered at the CoC by Prop 47 status level in parentheses
*** p<0.01, ** p<0.05, * p<0.10
Control group is the offenders with a complementary set of Proposition 47 charges within the same category.
"Linear Combination" row indicates the p-values from testing whether the AB 109 effect is statistically different from the Prop 47 effect.

the narcotic drug law charges. Figure 11 reveals the spillover effect which is driving the results (the control group contributes to the statistically significant treatment effect) and is especially visible among charges involved with non-homeless offenders (the right panel of Figure 11). The trends of Prop 47 charges are stable regardless of offender homelessness status before and after the passage of Prop 47. The breakdowns of crime trends in Figure 12 shows that the spillover effects are evenly distributed across felony and misdemeanor complaints in the control group.

Combined, the number of drug offenses eligible for Prop 47 decreased. A possible explanation is that the policing behaviors changed due to Prop 47, resulting in fewer arrests being made due to non prosecution, especially for drug offenders (Garner, 2020). On the contrary, other property offenses such as larceny and vehicle theft ineligible for Prop 47 increased and the effect is more significant among the non-homeless offenders. This phenomenon can be traced to two possible explanations. First, since Prop 47 reduced the penalty for non-violent crimes, offenders began committing more non-violent crimes, spilling over more severe non-violent crimes. Second, prosecutors and police officers changed their behaviors, focusing on non-Prop 47 charges.

2.5.4 Robustness

The core of DiD estimates is to find the right control group. Instead of deploying the matching method to find the control group, we can also see the distribution of DiD estimates with different control groups by randomly matching the control group. By doing so, we can see if the main results are plausible compared to the distribution of possible coefficients. To perform this analysis, I use a strongly balanced panel of 40 consistent (across years) CoC units in California as a treatment

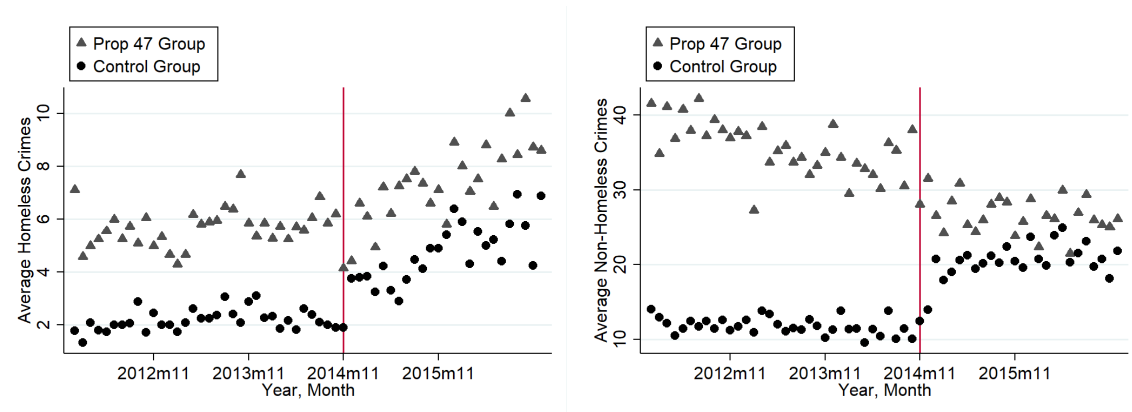


Figure 11: Comparing LA Crime Trends between Homeless and Non-Homeless Excluding Narcotic Drug Laws Charges

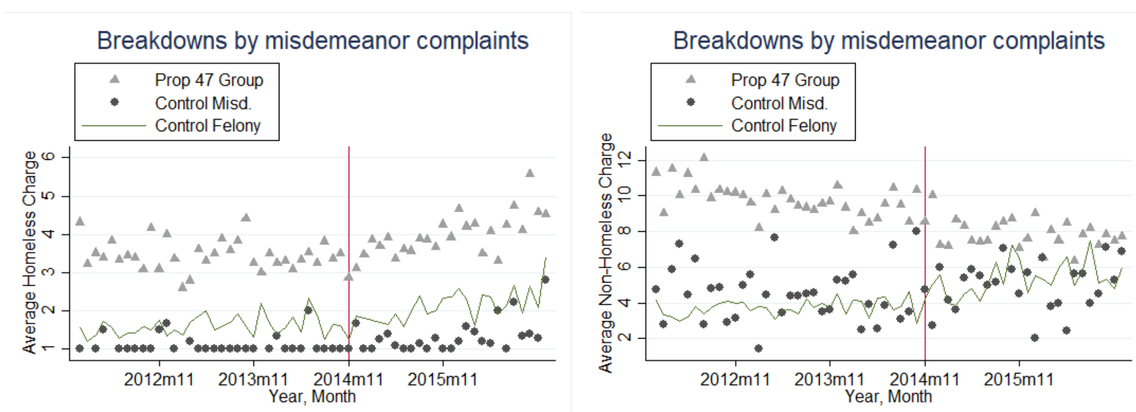


Figure 12: Breakdowns of LA Crime Trends between Homeless and Non-Homeless

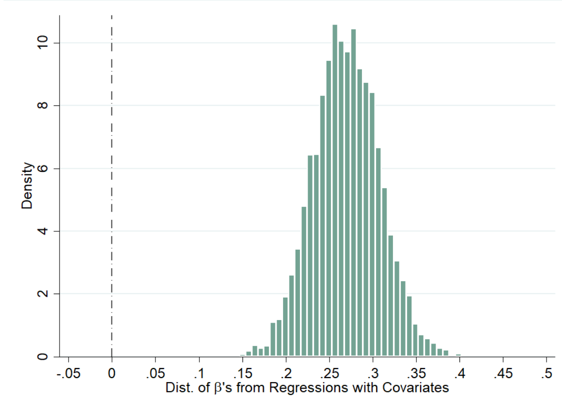


Figure 13: Coefficients from Randomly Matched Control Groups

group and then randomly find 160 CoC units from other states as a control group. Figure 13 shows the distribution of coefficients from 5,000 repetitions. The coefficient from the main regression after propensity-score matching (0.27) is located in the middle of the distribution; the main regression results present a viable DiD estimate compared to the distribution of possible DiD estimates.

2.6 Mechanism

2.6.1 Sheltered Homeless Vs. Unsheltered Homeless

The sheltered homeless population is less likely to be the representative prison population since the male and the Black dominate in the state prison and county jails. On the other hand, CoC units prioritize the housing of the most vulnerable, women and children, thus, they are out of the scope of the current analysis. Nonetheless, one might point out that sheltered homeless population exhibits a continuously decreasing trend regardless of treatment status (left panel of Figure 14). However, this seems to reflect a homeless-housing regime change from temporary housing to permanent housing. According to the HUD's definition, the permanently housed population is not classified as the homeless. In other words, a portion of the sheltered-homeless population is constantly being re-defined as the permanently housed population, thus lowering the count of the sheltered-homeless population. The accompanying Housing Inventory Count (HIC) data supports this argument (Figure 14). The middle panel in Figure 14 indicates the temporary housing stocks, and the right panel displays the total housing stocks, including permanent housing. The center

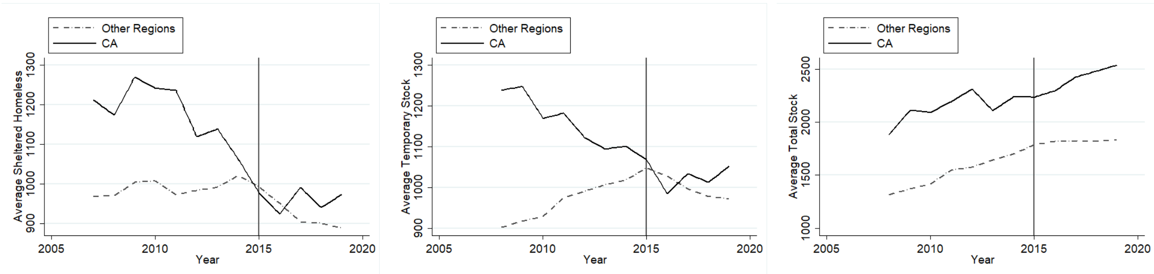


Figure 14: Sheltered Homeless Vs. Temporary Shelter Beds Vs. Total Shelter Beds

panel exhibits precisely the same trend as the sheltered homeless population. The right panel shows that the total amount of housing stock is irrelevant to the policy change.

2.6.2 Total Effect Vs. Indirect Effect

The absence of statistical significance in total effect does not necessarily mean the variable of interest does not have meanings, as total effects could overshadow the mechanism. As a trial for unpacking the mechanism, we might separate the total impact and the indirect impact of Prop 47 through the homeless population. In this case, I can utilize a mediation analysis, a method designed to examine the mechanism by separating indirect effect from total effect through the linear projection of treatment effect on the indirect effect of interest. I conduct a mediation analysis of the impact of Prop 47 on the outcome of interest in Equation 4.

$$log(y_{it}) = \gamma_0 + \gamma_1 treat_i \times post_t + \gamma_2 \widehat{log(h_{it})} + X'r + c_i + t_t + e_{it}, \tag{4}$$

where $log(\widehat{h_{it}})$ is the indirect of Prop 47 through the homelessness population and obtained from Equation 1. γ_2 is the coefficient measuring the indirect effect of the unsheltered homeless population and γ_1 is total effect of Prop 47 excluding the indirect effect.

Lastly, we can investigate fiscal stress largely involved with the homeless population (Equation 5). According to State and Local Government Finance data documentation, community development, and housing expenditures include activities on "urban renewal and slum clearance; redevelopment and rehabilitation of substandard or deteriorated facilities and areas; rural redevelopment; and revitalization of commercial." Thus, community housing and development expenditures are relevant in dealing with chronic homeless populations, not offender-related expenses. Therefore, I implement an instrumental variable strategy, see Equation 5, with Prop 47 being instrumented for the unsheltered homeless.

$$log(y)_{it} = \delta_0 + \delta_1 \widehat{log(h)}_{it} + X'r + CoC_i + Year_t + e_{it} \tag{5}$$

where $log(\widehat{h_{it}})$ is the predicted effect of the unsheltered homeless population on the governmental spending.

Table 9 shows the regression results from Equation 4 and Equation 5. One of the aims of Prop 47 was to reduce correctional expenditure by decreasing the jail population. As seen in column (1) of Table 9, there is no statistically significant reduction in correctional spending due to Prop 47. However, the signs of coefficients imply a potential mechanism. While the coefficient from $Treat \times Post$ is the total effect of Prop 47, the coefficient from $Log(\widehat{Homeless})$ is the indirect effect of Prop 47 through the homeless population. The negative sign of the indirect effect coefficient means that the increased homelessness from Prop 47 is pushing the correctional expenditures to decrease. The indirect channel of homelessness indicates an increase of the hospital expenditures in Column (2). Since the homeless population grew by 26.6 percentage points, the coefficient 0.847 implies a 26.6 × 0.85 = 22.5 percentage-point increase in hospital expenditure. Community development expenditures in Column (3) did not show a statistically significant increase, but the sign indicates the predicted direction. Ultimately, Prop 47 was planned to save correctional funds but does not save funds stressing other governmental expenditures.

2.7 Conclusion

In this analysis, I examine the impact of decarceration policies on the homeless population, county governmental spending, crimes, and recidivism. Specifically, the study implements a nation-wide data analysis on the homeless population and governmental spending, a California state-wide data analysis on county jail population and recidivism, and a Los Angeles city-wide data analysis

Table 9: Disentangling the Effect of Prop 47 on Homelessness and Government Finance

	Log(Correct)	Log(Hospital)	Log(Develop)
$\widehat{log(h)}$	0.050	0.847	0.595
	(0.197)	(0.523)	(0.595)
treat × post	-0.009	-0.057	
	(0.073)	(0.147)	
Observations	1333	533	1002
R^2	0.027	-0.731	-0.061
CoC and Year FEs	X	X	X
Total Revenue	X	X	X

Robust standard error clustered at the CoC level
Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.10
Column (1) and (2) represent regression results from mediation analysis. The coefficients from $\widehat{log(h)}$ can be interpreted as the indirect effects of treatment.

on the number of charges by offender homelessness status.

This investigation is vital because re-sentencing is one of the significant decarceration policies and the recent Criminal Justice Reform, and receives attention from national policymakers. Also, the current analysis complements the cost/benefit analysis of incarceration and decarceration policies.

I find that Prop 47 raised the unsheltered homeless population by 26 percentage points and health spending by 11 percentage points in nationwide data analysis. In California state-wide data analysis, AB 109 raised the county jail population by 18.5 percentage points and recidivism by 37.5 percentage points. The Prop 47 effect mitigates the AB 109 effect by ten percentage points, implying that Prop 47 absorbed the county jail-crowding pressure stemming from AB 109. However, Prop 47 fails to decrease the recidivism rate among Prop 47 offenders who commit control-group charges. Los Angeles city-wide data analysis observes that Prop 47 raises overall non-violent crime rates.

Several important questions remain for future research. Fundamentally, the role of decarceration is more concerned with distributing central government function as a social control into local communities (Chan and Erickson, 1981). However, re-sentencing and reduced penalties imply the offenders essentially come under supervision such as probation and parole. Then communities with a lack of resources are likely to rely on private companies, reallocating the inmate population formally managed by governmental correctional facilities into private-supervision companies. For instance, private prisons multiplied, housing around 7,000 prisoners in 1990, to more than 126,000 in 2015 across the United States when state prisons were under crowding pressures (Takei, 2017). Furthermore, private prisons do not yield a cost-efficient outcome (Pratt and Maahs, 1999). Additional research ought to tackle these concerns so as to aid policymakers.

Table 10: List of CoC Units in Analysis

CoC Name	State	CoC Name	State
Amador, Calaveras, Mariposa, Tuolumne Counties CoC	CA	Grand Traverse, Antrim, Leelanau Counties CoC	MI
Bakersfield/Kern County CoC	CA	Holland/Ottawa County CoC	MI
Chico, Paradise/Butte County CoC	CA	Lenawee County CoC	MI
Colusa, Glenn, Trinity Counties CoC	CA	Livingston County CoC	MI
Daly/San Mateo County CoC	CA	Marquette, Alger Counties CoC	MI
Davis, Woodland/Yolo County CoC	CA	Monroe City & County CoC	MI
El Dorado County CoC	CA	Duluth/St.Louis County CoC	MN
Fresno City & County/Madera County CoC	CA	Minneapolis/Hennepin County CoC	MN
Glendale CoC	CA	Moorhead/West Central Minnesota CoC	MN
Humboldt County CoC	CA	Northeast Minnesota CoC	MN
Imperial County CoC	CA	Northwest Minnesota CoC	MN
Long Beach CoC	CA	Rochester/Southeast Minnesota CoC	MN
Los Angeles City & County CoC	CA	Southwest Minnesota CoC	MN
Marin County CoC	CA	Joplin/Jasper, Newton Counties CoC	MO
Mendocino County CoC	CA	Montana Statewide CoC	MT
Merced City & County CoC	CA	Chapel Hill/Orange County CoC	NC
Napa City & County CoC	CA	Northwest North Carolina CoC	NC
Oakland, Berkeley/Alameda County CoC	CA	Lincoln CoC	NE
Oxnard, San Buenaventura/Ventura County CoC	CA	Nebraska Balance of State CoC	NE
Pasadena CoC	CA	New Hampshire Balance of State CoC	NH
Redding/Shasta, Siskiyou, Lassen, Plumas, Del Norte, Modoc, Sierra Counties CoC	CA	Bergen County CoC	NJ
Richmond/Contra Costa County CoC	CA	Jersey City, Bayonne/Hudson County CoC	NJ
Riverside City & County CoC	CA	Morris County CoC	NJ
Roseville, Rocklin/Placer County CoC	CA	New Brunswick/Middlesex County CoC	NJ
Sacramento City & County CoC	CA	Warren, Sussex, Hunterdon Counties CoC	NJ
Salinas/Monterey, San Benito Counties CoC	CA	Albuquerque CoC	NM
San Bernardino City & County CoC	CA	Las Vegas/Clark County CoC	NV
San Diego City and County CoC	CA	Nevada Balance of State CoC	NV
San Francisco CoC	CA	Reno, Sparks/Washoe County CoC	NV
San Jose/Santa Clara City & County CoC	CA	Cattaragus County CoC	NY
San Luis Obispo County CoC	CA	Clinton County CoC	NY
Santa Ana, Anaheim/Orange County CoC	CA	Columbia, Greene Counties CoC	NY
Santa Maria/Santa Barbara County CoC	CA	Elmira/Steuben, Allegany, Livingston, Chemung, Schuyler Counties CoC	NY
Santa Rosa, Petaluma/Sonoma County CoC	CA	Franklin, Essex Counties CoC	NY
Stockton/San Joaquin County CoC	CA	Glens Falls, Saratoga Springs/Saratoga, Washington, Warren, Hamilton Counties Co	NY
Turlock, Modesto/Stanislaus County CoC	CA	Ithaca/Tompkins County CoC	NY
Vallejo/Solano County CoC	CA	Jamestown, Dunkirk/Chautauqua County CoC	NY
Visalia/Kings, Tulare Counties CoC	CA	Jefferson, Lewis, St. Lawrence Counties CoC	NY
Watsonville/Santa Cruz City & County CoC	CA	Nassau, Suffolk Counties CoC	NY
Yuba City & County/Sutter County CoC	CA	New York City CoC	NY
Alaska Balance of State CoC	AK	Sullivan County CoC	NY
Anchorage CoC	AK	Norman/Cleveland County CoC	OK
Fayetteville/Northwest Arkansas CoC	AR	Central Oregon CoC	OR
Phoenix, Mesa/Maricopa County CoC	AZ	Clackamas County CoC	OR
Colorado Balance of State CoC	CO	Eugene, Springfield/Lane County CoC	OR
Metropolitan Denver CoC	CO	Hillsboro, Beaverton/Washington County CoC	OR
Columbia, Hamilton, Lafayette, Suwannee Counties CoC	FL	Medford, Ashland/Jackson County CoC	OR
Hendry, Hardee, Highlands Counties CoC	FL	Oregon Balance of State CoC	OR
Miami-Dade County CoC	FL	Portland, Gresham/Multnomah County CoC	OR
Monroe County CoC	FL	Philadelphia CoC	PA
Naples/Collier County CoC	FL	South Dakota Statewide CoC	SD
Orlando/Orange, Osceola, Seminole Counties CoC	FL	Austin/Travis County CoC	TX
Punta Gorda/Charlotte County CoC	FL	Dallas City & County, Irving CoC	TX
St. Johns County CoC	FL	El Paso City & County CoC	TX
Tampa/Hillsborough County CoC	FL	Houston, Pasadena, Conroe/Harris, Ft. Bend, Montgomery, Counties CoC	TX
West Palm Beach/Palm Beach County CoC	FL	San Antonio/Bexar County CoC	TX
Georgia Balance of State CoC	GA	Texas Balance of State CoC	TX
Hawaii Balance of State CoC	HI	Provo/Mountainland CoC	UT
Honolulu City and County CoC	HI	Salt Lake City & County CoC	UT
Iowa Balance of State CoC	IA	Arlington County CoC	VA
Sioux City/Dakota, Woodbury Counties CoC	IA	Harrisburg, Winchester/Western Virginia CoC	VA
Boise/Ada County CoC	ID	Loudoun County CoC	VA
Idaho Balance of State CoC	ID	Burlington/Chittenden County CoC	VT
Aurora, Elgin/Kane County CoC	IL	Vermont Balance of State CoC	VT
Chicago CoC	IL	Everett/Snohomish County CoC	WA
McHenry County CoC	IL	Seattle/King County CoC	WA
South Central Illinois CoC	IL	Spokane City & County CoC	WA
Waukegan, North Chicago/Lake County CoC	IL	Vancouver/Clark County CoC	WA
West Central Illinois CoC	IL	Washington Balance of State CoC	WA
Cape Cod Islands CoC	MA	Wisconsin Balance of State CoC	WI
Massachusetts Balance of State CoC	MA	Huntington/Cabell, Wayne Counties CoC	WV
Pittsfield/Berkshire, Franklin, Hampshire Counties CoC	MA	Wheeling, Weirton Area CoC	WV
Carroll County CoC	MD	Wyoming Statewide CoC	WY
Cumberland/Allegany County CoC	MD		
Garrett County CoC	MD		
Howard County CoC	MD		
Montgomery County CoC	MD		
Prince George's County CoC	MD		

3 Chapter 3: Law Enforcement Leadership, Tenure & Jail Overcrowding

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3.1 Introduction

Rallying cries for criminal-justice reform are now commonplace in policy circles,[1] among academic researchers (Beckett et al., 2016; Levin, 2018), in the media and within criminal-justice agencies.[2] Some of the most typical reforms include reducing or eliminating jail or prison sentences for low-level offenses (Helland and Tabarrok, 2007; Mayson and Stevenson, 2020; Zimring, 2006; Green and Winik, 2009; Garner and DeAngelo, 2021), training and personnel changes to criminal justice actors (Banerjee et al., 2012; DeAngelo and Owens, 2017; West, 2018; Johnson et al., 2021), examination of ethnic and racial disparities throughout the criminal justice system (Yang, 2015; DeAngelo et al., 2018; Fryer, 2019; Hoekstra and Sloan, 2020), and reducing the number of incarcerated individuals (Raphael and Stoll, 2013).

One of the most prominent discussions about criminal-justice reform involves reducing the incarcerated population. Indeed, this is often touted as a reform that can be agreed upon between individuals identifying as either liberal or conservative, as both desire fewer incarcerated individuals. From a fiscally conservative perspective, the impetus for reducing incarceration is often grounded in reducing funds spent on the criminal justice system (Wagner and Rabuy (2017) estimating these costs at $180 billion annually,) reducing the limits of government (almost 1 in 33 adults is under some type of control by the criminal justice system), and government accountability.

[1]Examples of policy groups include the Vera Institute of Justice, Brennan Center for Justice, and Measures for Justice.

[2]For example, Seattle Police Department is reallocating funds from police services toward victim services. Los Angeles Police Department's budget was reduced by $133 million and a Community Safety Partnership Bureau was started in the aftermath of the Black Lives Matter movement. Also in Los Angeles, newly elected District Attorney George Gascón vowed to end the death penalty, stop most uses of cash bail for misdemeanor non-serious or nonviolent felony offenses and prioritize cases for re-sentencing inmates whose prison terms are deemed excessive. This reform is the latest of a number of district attorneys offices that have made similar claims, including Suffolk County (MA), Dallas County (TX), and Santa Clara (CA).

Indeed, there are safety, aging/medical and financial reasons to want to reduce the incarcerated population. Additionally, overcrowded jails and prisons contribute to increased inmate misconduct, which negatively affects the safety and security of inmates and staff (Cummings and Scott, 2012; Enns, 2016). Nevertheless, there are also community safety concerns related to incarcerating dangerous individuals (Barbarino and Mastrobuoni, 2014; Green and Winik, 2009; Friedman et al., 2011; Cunningham and Kang, 2018)

The question then arises as to what contributed to the increases in incarcerated populations. There is theoretical support that more experience in government leadership leads to greater expertise in both government performance and rent seeking behaviors (Tollison, 2012). On one hand, experienced political leadership can lead to increased ability to bring more resources into their district, making term limits less ideal (Buchanan and Congleton, 1994). On the other hand, López (2003) analyzed a comprehensive body of research on the impact of term limits and tenure of government officials and concluded that the existing evidence indicates mixed results and there is no clear evidence that term limits are ideal. Smart and Sturm (2013) developed a theoretical model suggesting that term limits can reduce inefficiencies by limiting voters' discretion to not replace the incumbent. Specifically in the policing area, Crank and Langworthy (1992) discuss policing institutions and quality in terms of police chief control by delving into the removal and replacement of a disgraced police chief by a new chief with a "legitimate" mandate. Bulman (2019) produces one of the only pieces of causal research showing a positive impact of sheriff leadership by investigating the race of the sheriff and its impact on disparities in public safety. His results find evidence that the race of the sheriff reduced racial bias in policing, however, this work did not examine the impact of experience.

The relationship between a sheriff's preferences toward criminal behavior, re-election incentives, and jail overcrowding might not be immediately transparent. However, as noted in Enns (2014), the public's desire for punitiveness has lead to steadily increasing incarceration rates, even after controlling for other important features, such as crime rates, drug use, inequality, and the political party in power. In addition to the community's appetite for punishing criminal behavior, the community displays a negative sentiment toward elected officials in the criminal justice system (e.g. district attorneys, sheriffs, judges) when individuals that are released from incarceration and re-offend (Tonry, 1998; D'Elia, 2010; Campbell, 2011). To deter recidivism, sheriffs are reluctant to grant early release to inmates, especially those potentially displaying violent tendencies, which could result in more congested jails.

We do not, however, expect that all elected sheriffs would behave similarly in terms of the way that they handle jail populations. As noted in Shepherd (2009), agents of the criminal justice system are responsive to election pressure. Politicians that are more conservative tend to be viewed as being "tougher on crime" (Braithwaite and Pettit, 1992), whereas politicians that are more progressive tend to favor criminal justice policies that are less punitive and more restorative. We leverage the competitiveness of sheriff elections to examine how politically competitive counties compare to non-competitive counties. We posit that Democrat (Republican) dominant, but politically competitive counties will pursue harsher (lenient) criminal justice policies to gain the support of more (less) conservative voters.

We contribute to the existing literature by focusing on the effect of a sheriff's tenure on their county's jail-occupancy rate.[3] We focus on sheriffs because they are the sole criminal justice actor in charge of operating jails in most jurisdictions.[4] Our emphasis on jail occupancy stems from negative outcomes that have been associated with jail crowding. Specifically, jail crowding can impact bail decisions (Williams, 2016), the mental or physical well-being of inmates, and correctional officers beyond the impact of being incarcerated/employed in a facility that is not above its rated occupancy (Castle and Martin, 2006).[5] However, some studies have found that increases in jail populations can reduce crime rates (Levitt, 1996), while others find that they do not (Sundt et al., 2016). Nevertheless, the US Department of Justice has identified jail and prison occupancy as an important enough issue that they have placed many jurisdictions under a federal

[3]Previous work has examined the effect of police experience on various outcomes. DeAngelo and Owens (2017) examine the effect of police experience on issuing citations for recently changed laws, while West (2018) examines the relationship between law enforcement experience and the likelihood that an investigation yields contraband.

[4]Our analysis focuses on 35 states because our identification strategy leverages county sheriffs that are term-limited, which we discuss in Section 3.3. As such, we omit 15 states from our analysis because they are not comparable to term-limited locations along observable variables. Out of 514 counties in the 35 states included in our analysis, we have identified 502 counties where the sheriff is in charge of operating the jail.

[5]Kinkade et al. (1995) conduct a nationally representative survey of sheriffs to examine the effect of jail occupancy on a number of secondary issues associated with jail occupancy, noting that occupancy in larger jail facilities is having a profound impact on jail safety, and that inmate safety suffers the most from jail occupancy.

consent decree.[6]

The difficulty in examining the decisions of actors or enacted policies within the criminal justice system is that many of these policies are adopted and enacted by individuals that have either been appointed or elected into positions, which are often the product of forces that are not observable by a researcher (Lee et al., 2004). Recent empirical work in public choice points to the important influences that appointments and elections could be having on the criminal justice system. For example, Ouziel (2020) outlines the conditions under which attempts to reform the criminal justice system will be (un)successful. Bandyopadhyay and McCannon (2014) examine the impact of elections on the willingness of prosecutors to take cases to trial, as well as the sanctions that result from those trials. DeAngelo and McCannon (2020) find that judges that face electoral pressures and who handle criminal and civil cases have increased error rates during their election campaigns. The error rate is also rising with financial contributions received by judges toward their re-election campaign. Additionally, appointed judges could be altering their decisions in response to the preferences of the appointing agent (Shepherd, 2009). Criminal justice unions (police, prosecuting attorneys, public defenders, etc.) could be engaging in strategic behavior to ensure the protections of their members (Dharmapala et al., 2020). Fines and fees as well as seized civil assets that are part of the criminal justice system could contribute non-negligibly to the operating budget of a county, acting as a force against reducing law enforcement activities (Makowsky and Stratmann, 2009; Makowsky et al., 2019). While these are just a few examples, the main upshot is that the preferences of individuals in positions of authority (e.g. county commissioners, chief budget officers, etc.) could be impacting the decisions of appointed or elected members of the criminal justice community.

Potential endogeneity between a sheriff's tenure and jail occupancy presents another empirical issue. The decision of a sheriff to allow a jail to be crowded could be a function of the sheriff's confidence that they will be re-elected, the electorates preferences toward criminals (e.g. "tough on crime") or the sheriff's personal preferences. To overcome this issue, we leverage a situation where 192 counties across 4 states have legislatively limited sheriffs to a maximum of two 4 year terms.

[6] Lawrence (2014) discusses jails under federal court order in California.

Using sheriff term limits as an instrument for sheriff tenure, we examine the effect of sheriff tenure on jail occupancy. The identifying assumption is that sheriff term limits do not have a systematic or direct relationship with jail occupancy other than through its impact on a sheriff's decisions. Importantly, though, term limits exogenously impact the tenure of a sheriff, which enables us to examine the impact of sheriff tenure on jail capacity.

Our main results find that an additional year as a sheriff raises the jail-occupancy rate by 3 percentage points, where the average jail-occupancy rate is 85% of jail capacity. To further examine the mechanism through which longer sheriff tenure leads to higher jail-occupancy rates, we separate our data along two margins: the competitiveness of elections and political party dominance. We then explore heterogeneous treatment effects by (non-)competitive counties with a majority of democratic versus republican voters. Our results indicate that counties that typically vote for republican candidates and are not politically competitive display more jail crowding. We also find that counties that typically vote for democratic candidates and are politically competitive display more jail crowding. This work contributes novel data about the role of local politics on an important criminal justice outcome, as data availability issues have been blocking causal research on policing leadership despite the emphasis on its importance (Sumner et al., 2018).

The remainder of this paper consists of six parts. In the next section we provide background on term limits for sheriffs. In section 3 we provide a description of the data used in our analysis as well as the matching process that is employed to ensure that we are conducting our analysis on comparable locations. In section 4 we describe our research design and the specific estimation strategy that we employ in our empirical analysis. Our main results are presented in section 5 and robustness specifications are presented in section 6. Finally, in section 7, we conclude with a discussion of our results and their implications for criminal-justice reform.

3.2 History of Sheriff Election and Term Limits for Sheriffs

The county sheriff position is one of the few criminal-justice leadership positions that is determined by election. While sheriff elections are quite common in the US (3,076 counties elect sheriffs among 3,143 counties as of 2020), term-limited sheriffs are not a common phenomenon. Indeed, only 4 states (Colorado, Indiana, New Mexico and West Virginia) have two-term limits for the sheriff position, corresponding to 192 counties. In Colorado, term limits date back to January 1, 1995. Although the state policy requires term limits on elected officials, only 12 counties abide by this policy.[7] Indiana has had term limit laws as far back as 1952. An amendment was proposed in the 1980s that tried to remove the constitutionally mandated two-term limits (Constitution Article 6, Section 2), but was defeated (Bell and Byers, 2011). The constitution of the state of New Mexico, written in 1914, dictated "all county officers, after having served two consecutive four-year terms, shall be ineligible to hold any county office for two years thereafter" (Article 10 Section D). In our data, there are only two sheriffs out of 88 who returned to the sheriff's office after another sheriff served in the office. West Virginia has had a two-term limit law imposed on sheriffs since 1974 (Bell and Byers, 2011). According to the National Sheriff's Association, there are no other locations that have term limits imposed on sheriffs. However, Ballotpedia notes that Missouri proposed a constitutional amendment of two-term limits to be imposed on sheriffs, but this does not appear to be the practice within the state as at least 27% of sheriffs served more than two terms.[8]

3.3 Data

In this section we describe our data sources and procedures for constructing our main explanatory variable: sheriff tenure. We also describe the process by which we match term-limited locations to non-term-limited locations to determine a comparable group for our analysis.

3.3.1 Sheriff Tenure

To obtain the tenure of each sheriff we identify information on sheriff names by year and county and calculate the length of time that they held the sheriff position. Sheriff elections appear in the vast majority of states, except for Alaska and Connecticut, which are omitted from the analysis.

[7] There was a nationwide debate to impose term limits on state legislatures for "being corrupt, indebted to special-interest groups, and more interested in getting re-elected than working for the people" (Bill of Right in Action Spring 1995 (11:3) c). Colorado set two-term limits on US Senate, Colorado Senate, and officials in the State Executive Department, State Education Boards, and local governments.

[8] Some Missourri counties are not represented in the data, so 27% is likely a lower bound.

There are also a few county- and city-level exceptions that are omitted from the analysis.[9] Hawaii and Rhode Island have appointed sheriffs, as well as two Colorado counties and Dade County (FL) according to the executive summary by the National Sheriff's Association. The District of Columbia is also excluded from our analysis because it is a federal district and the duties of the sheriff department are handled by several other agencies, including the United States Marshal Service and District of Columbia Protective Services Police Department.

The main source of tenure information is the Municipal Year Book series from the International City/County Management Association, which contains an extensive list of county officials across the United States. Unfortunately, the data are occasionally missing because it is not a requirement for sheriff's departments to report to this source. To supplement the missing data, the information is obtained from various sources including state-wide repositories, official county web sites, and searching local newspaper articles.

3.3.2 Jail Operations and Socioeconomic Indicators

Our main outcome variable, the occupancy of the jail, relies on the sheriff's discretion in operating the jail. To obtain this information we rely on the Census of Jails periodical surveys, which were conducted in 1983, 1988, 1993, 1999, 2005, 2006, and 2013. Among a variety of survey questions, we selected items including average daily jail population, rated capacity, and total new admissions to and final discharges from jail facilities.

We also include a series of socioeconomic indicators as control variables in our main analysis and propensity score matching process. These variables are sourced from two locations: Intercensal State and County Characteristics Population Estimates (US Census Bureau, 2018) and Personal Income and Employment by Major Component (Bureau of Economic Analysis, 2018). The variables that we utilize in our analysis include the percent of the population that is female, White, Black, the average income per capita, and employment per capita. Additionally, population coverage per agency is obtained from the FBI's Uniform Crime Reporting (UCR) data. The population coverage, which is defined by the UCR program, indicates the population that the law-enforcement agency

[9] Five populated territories (American Samoa, Guam, the Northern Mariana Islands, Puerto Rico and the US Virgin Islands) do not have county governments and, accordingly, do not have sheriffs departments.

oversees.

3.3.3 Matching term and non-term-limited locations

After excluding locations where sheriffs are appointed or do not exist, term-limited locations make up 6% of all locations (192 out of 3,076 locations). With a few exceptions, the majority of our term-limited locations are rural, which distinguishes term-limited counties from many non-term-limited counties. To ensure that our analysis is conducted on comparable locations, we conduct a propensity-score-matching (PSM) exercise to restrict our analysis to term-limited and non-term-limited locations that are comparable, at least based on observables. We utilize the following procedure to match locations for our analysis. To start, 59 variables are used in the matching analysis, which include population information (percent of female, white, and Black population by 18 age groups[10]), farm/non-farm income, total income, employment-to-population ratio and total population in each county. We conducted PSM for each year (38 years from 1978 to 2015) using three conditions: (1) all control counties with replacement, (2) the nearest neighbor matching method, and (3) allowing up to 10 control locations.[11]

The actual number of matched control locations varies from 760 to 1460 each year. To identify our final set of control locations, we keep only those locations that matched to our term-limited locations in at least ten of the 38 years of data in our sample, which is 680 locations.[12] Finally, tenure information has been obtained for 287 non-term-limited locations, covering 32 states. After merging with the Census of Jail data, the final analysis includes 274 non-term-limited locations and 155 term-limited locations.

3.3.4 Descriptive Statistics

Table 1 presents descriptive statistics comparing locations with and without term limits after the the PSM exercise was completed. The unit of analysis is county by year observations. Column (1) includes all observations, while column (2) includes only term-limited locations, and column

[10]The 18 age group data includes 1-4 years, 5-9 years, 10-14 years, 15-19 years, ... , 80-84 years, and total years.

[11]We require a minimum of 10 control locations to ensure a large enough pool of candidate locations for matching.

[12]Our results are not sensitive to this threshold. In results, which are available upon request, we find similar results using a threshold of 15 or 20 years.

(3) includes only non-term-limited locations.[13] The variables from rows 1 - 4 are used to construct the main outcome variables in our analysis, obtained from the jail operation data. *Avg Daily Jail Population* is the average inmate population within a jail in the year of data collection. There were 145 inmates, on average, across all jails with 116 inmates in term-limited locations and 175 inmates in non-term-limited locations. *Rated capacity* is an officially calculated measure of the official inmate capacity that the facility can accommodate in the year of data collection. *Total new admissions* and *Total final discharges* are a snapshot of the total inmates who were admitted and discharged from a facility during a target period.[14] Overall, the raw data show that non-term-limited locations, on average, have statistically larger numbers of inmates: non term-limited locations have, on average, 44 more daily inmates. Similarly, non-term-limited locations have, on average, higher rated capacity, new admissions, and final discharges than term-limited locations.

Rows 5 - 8 are the main outcome variables used in our analysis, which are constructed from rows 1 - 4. First, *Jail-occupancy rate* is calculated as the average daily population divided by the rated capacity. In this way we have normalized each jail to the rated capacity to make our analysis comparable across facilities of different sizes. Similarly, the *Discharge/admission ratio* is calculated as the *Total final discharges* divided by the *Total new admissions*. Then we converted these two variables into binary-outcome variables for better understanding of interpretation of our coefficients. *1(Jail-occupancy rate>0.95)* is coded as 1 if the jail-occupancy rate exceeds 95%. *1(Admission>Discharge)* is coded as 1 if the number of admitted inmates is greater than the number of discharged inmates. Among the four constructed variables, the jail-occupancy rate shows a 5 percentage point average difference between groups, indicating that non-term-limited locations are more likely to have the higher jail-occupancy rate.

The tenure variable is constructed by the name of sheriffs in an office by year and county, as noted in Section 3.3.1. Specifically, we determine the first year that a sheriff's name appears within a county and begin counting that sheriff's tenure in that year. On average, sheriffs in the term-limited locations have 2.5 fewer years of experience in the office than sheriffs in non-term-limited

[13] The asterisks in the first column correspond to statistically significant differences in means between columns (2) and (3).

[14] The target period indicates either the previous month (usually the month of June) or the week prior to data collection. All variables have been normalized such that we are examining comparable measures.

Table 1: Descriptive Statistics Comparing Locations with and without Term Limits

	Total	Term limit	No term limit
Avg. daily jail population	146.51*	116.03	160.41
	(426.30)	(180.16)	(499.20)
Rated capacity	141.83+	122.54	150.84
	(307.87)	(186.55)	(350.16)
Total new admissions	1381.99**	1104.39	1511.08
	(2510.62)	(1857.78)	(2753.46)
Total final discharges	1342.94***	1012.57	1489.96
	(2500.04)	(1827.17)	(2735.00)
Jail occupancy rate	0.84**	0.81	0.86
	(0.34)	(0.33)	(0.34)
Discharge/admission ratio	1.00	1.01	1.00
	(0.55)	(0.68)	(0.49)
1(Jail occupancy rate>0.95)	0.31	0.31	0.31
	(0.46)	(0.46)	(0.46)
1(Admission>Discharge)	0.58	0.56	0.59
	(0.49)	(0.50)	(0.49)
Tenure	6.19***	4.63	7.16
	(5.27)	(3.16)	(6.04)
Percent female	50.58*	50.61	50.56
	(1.15)	(1.26)	(1.08)
Percent white	95.74***	96.28	95.41
	(6.48)	(6.35)	(6.53)
Percent black	2.03***	2.15	1.96
	(2.88)	(3.64)	(2.27)
Income per capita (in thousands)	32.63***	34.06	31.73
	(14.29)	(14.77)	(13.91)
Employment per capita	47.26***	45.78	48.19
	(12.73)	(12.27)	(12.92)
Population coverage per agency	31240.44***	25532.53	34811.80
	(35107.86)	(27608.98)	(38649.71)
Observations	14519	5588	8931

*** $p<0.001$, ** $p<0.01$, * $p<0.05$, + $p<0.1$ for indicating statistical difference between groups.

Notes: standard deviations are in parenthesis. Avg. daily jail population indicates the average inmates population across the year. Jail occupancy rate is defined as the average daily population divided by the rated capacity. 1(Jail occupancy rate>0.95) is coded as 1 if the jail occupancy rate is greater than 95%. Discharge per admission ratio is defined as the total new admissions divided by the total final discharges. 1(Admission>Discharge) is coded as 1 when the discharge per admission ratio is less than 1.

locations. The statistically significant mean difference between term-limited locations and non-term-limited locations implies we are likely to have a strong first stage. The remaining variables (rows 10 - 16) are covariates, which include the percent of the population that is female, White, and Black, as well as the average income per capita, total employment per capita, and population coverage per agency.

3.4 Empirical Strategy

To examine the effect of police tenure on jail occupancy we utilize a model based on the law enforcement learning literature (West, 2018; DeAngelo and Owens, 2017). The baseline specification examines two separate measures of jail occupancy as a function of the tenure of sheriffs, controlling for fixed effects and time-carying covariates. Equation 1 presents the relationship between sheriff tenure and jail occupancy, which ideally we want to estimate:

$$y_{it} = \beta_0 + \beta_1 T_{it} + X'r + t_t + \epsilon_{it} \tag{1}$$

where y_{it} is the level of jail occupancy based on the relative number of inmates, as discussed in Section 3.3. T_{it} is the number of working years of the sheriff in office[15], and t_t are year-fixed effects that account for unobserved variation within a year. X is the set of time-varying socioeconomic indicators including percentages of Female, White and Black populations, the average income per capita, employment per capita, and population coverage by each agency to absorb variation at the county level over time. Robust standard errors are clustered at the county level.

However, this specification will suffer from several sources of endogeneity, likely due to political motivations. Theoretical research suggests that the electoral process encourages government officials to be more conscious of their performance (Besley and Case, 2003), implying possible reverse causality. Specifically, higher-performing sheriffs in their early career are more likely to be re-elected, thus longer tenure is associated with high-performing sheriffs. Alternatively, idiosyncratic preferences of communities toward criminal actors can lead to elected officials that engage

[15] Our baseline specification does not include sheriff fixed effects, but in unreported specifications the OLS regression results with the sheriff fixed effects result in a flipping of the sign of the point estimate.

in practices that might not be tolerated in most locations (e.g. Sheriff Arpaio in Maricopa County (AZ)). Thus, we aim to use an instrumental variable estimation to examine the causal effect of tenure on jail occupancy.

3.4.1 Specification

To overcome concerns of endogeneity and obtain the causal effect of police leadership tenure on jail occupancy we implement an instrumental variable approach using the two-term limits imposed on a subset of sheriffs to influence the length of their tenure. Term limits are mechanically linked to the length of working experience, as term-limited sheriffs cannot serve more than two terms. Our identifying assumption is that term limits only impact the jail operation through the length of the sheriff's tenure. Since we balance our data along relevant and observable socioeconomic characteristics, we do not believe that jail operations are being directly impacted by term limits other than their effect on the sheriff's tenure. In addition, it is unlikely that jail operations would impact term limits since term limits are codified into law and would require a considerable amount of time and effort to alter. Finally, it is unlikely that jail operations, or even sheriff behavior, would motivate bureaucrats to enact policies to change the term limits of the sheriff, as these term limits are typically applied to all elected county officials.

Equation 2 represents the first stage of the IV strategy:

$$T_{it} = \beta_0 + \beta_1 l_{it} + t_t + X'r + e_{it}. \tag{2}$$

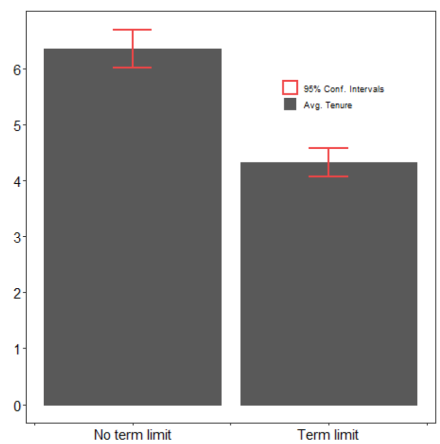
where T_{it} is the number of working years of the sheriff in office, and l_{it} is a binary variable for whether a county imposed two-term limits on the county sheriff. We include t_t fixed effects to account for unobserved variation within a specific year. X is the same set of socioeconomic indicators presented in Equation 1. We do not include the sheriff (or county) fixed effects because the instrumental variable, two-term limits imposed on sheriffs, are county level observations that do not vary over time. Robust standard errors are clustered at the county level.

The predicted value of tenure ($\widehat{T_{it}}$) is obtained from equation 2 and included in the second stage of our analysis:

$$y_{it} = \alpha_0 + \alpha_1 \widehat{T_{it}} + t_t + X'r + \epsilon_{it}. \tag{3}$$

Figure 1 displays the mean difference between locations that are and are not term-limited. The left (right) bar indicates the average tenure of the sheriff in counties without (with) term limits. The red whiskers represent 95% confidence intervals. Term-limited locations have statistically shorter average tenure length for sheriffs, providing evidence for the strength of our first stage. Figure 2 displays the average jail-occupancy rate by locations that are and are not term-limited. This figure visually displays the reduced form of our 2SLS model and provides support for the notion that term-limited locations have lower average jail-occupancy rates.

Figure 1: Tenure Length by Term Limits

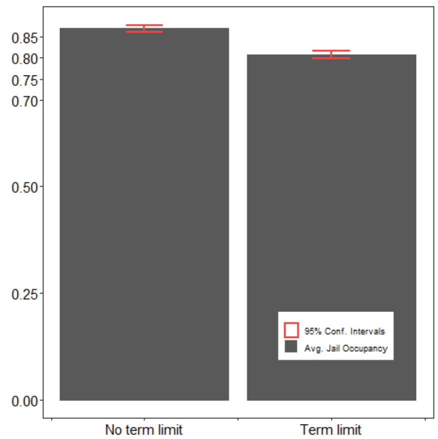


Notes: the graph shows the difference of tenure length between locations with and without term limits, which mirrors the first-stage regression results.

3.5 Results

In this section we examine the effects of sheriff tenure on jail operations using the IV strategy described in Section 3.4. Specifically, we have two jail-crowding measures derived from the average

Figure 2: Jail-Occupancy Rates by Term Limits



Notes: the graph shows the difference of jail-occupancy rates between locations with and without term limits, which mirrors the reduced form regression results.

daily jail population and rated capacity variables: 1) the jail-occupancy rate, which is defined as the average daily jail population divided by the rated capacity and 2) whether the jail occupancy reaches a critical threshold level. The second measure is a binary indicator variable, *1(jail-occupancy rate>0.95)*, coded as 1 if the jail-occupancy rate is greater than 95% capacity. The regression results from the first measure are shown in Table 2. The first column in Table 2 contains the estimates of the first-stage analysis. We find that term-limited sheriffs, on average, are in the position for approximately 2.2 fewer years than non-term-limited sheriffs, which is approximately 40% of the average length of time that a sheriff serves in this position. Additionally, our instrument is sufficiently strong to ensure that we are not suffering from a weak instrument issue (Stock and Yogo, 2005).

Columns 2 - 4 of Table 2 produce the OLS, reduced form, and two-stage least squares estimates of the effect of sheriff tenure on jail-occupancy rates. The OLS estimates report no statistically significant relationship between sheriff tenure and the jail-occupancy rate. However, the two-stage least squares estimates establishes a statistically significant, positive relationship between sheriff tenure and jail-occupancy rates. We specifically find that a one year increase in sheriff tenure leads to a 3.0 percentage point increase in the jail-occupancy rate, which is approximately 3.5% of the average jail-occupancy rate.

Table 2: First Stage, OLS, Reduced Form, and 2SLS Estimates of the Effect of Sheriff Tenure on Jail-Occupancy Rate and 1(Jail-Occupancy Rate>0.95)

		Jail Occupancy Rate			1(Jail Occupancy Rate> .95)		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	First Stage	OLS	Reduced	2SLS	OLS	Reduced	2SLS
Term limit	-2.237***		-0.066**			-0.013	
	(0.270)		(0.022)			(0.026)	
Tenure		-0.002		0.030**	-0.005+		0.006
		(0.002)		(0.011)	(0.003)		(0.012)
Percent female	0.135	0.013	0.015	0.011	0.014	0.014	0.013
	(0.125)	(0.010)	(0.010)	(0.011)	(0.012)	(0.013)	(0.012)
Percent white	0.027	0.001	0.002	0.001	0.001	0.001	0.001
	(0.023)	(0.001)	(0.001)	(0.002)	(0.003)	(0.003)	(0.003)
Percent black	-0.001	0.012*	0.013**	0.013*	0.022**	0.023**	0.023**
	(0.054)	(0.005)	(0.005)	(0.005)	(0.007)	(0.007)	(0.007)
Income per capita (in thousands)	-0.002	-0.001	-0.001	-0.001	-0.001	-0.001	-0.001
	(0.026)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)
Employment per capita	0.027*	-0.001	-0.001	-0.002+	-0.001	-0.001	-0.001
	(0.013)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Population coverage per agency	0.000	0.000**	0.000*	0.000	0.000+	0.000	0.000
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Observations	1651	1651	1651	1651	1651	1651	1651
Mean. of Dept. Var.	5.67	0.848	0.848	0.848	0.318	0.318	0.318
R^2	0.20	0.14	0.14	-0.18	0.08	0.08	0.02
F stat	13.66	4.16	5.57	4.29	4.30	3.55	3.47
Year FE	X	X	X	X	X	X	X
Covariates	X	X	X	X	X	X	X

Robust standard errors clustered at the county level
*** p<0.001, ** p<0.01, * p<0.05, + p<0.10
Notes: Column (1) presents the first stage estimate, the effect of term limit on tenure. Column (2), Column (3) and Column (4) show OLS, reduced form, and 2SLS regression results for Jail Occupancy Rate. Column (5), Column (6), and Column (7) display OLS, reduced form, and 2SLS regression results for 1(Jail Occupancy Rate > 0.95. The unit of observations is the county-year level.

Columns 5 - 7 of Table 2 produce the OLS, reduced form, and two-stage least squares estimates of the effect of jail occupancy on a binary indicator for jail crowding. The OLS estimates report a weakly statistically-significant negative relationship between sheriff tenure and jail overcrowding. However, the two-stage least squares estimates fails to establish a statistically significant relationship between sheriff tenure and overcrowding.

We also derive jail crowding measures from the admitted and discharged inmates variables. Specifically, the jail crowding measures are 1) the discharge-to -admission ratio, which is defined as the total final discharges divided by the total new admissions and 2) the likelihood of jail occupancy net growth. The second measure is a binary indicator variable, *1(Admission>Discharge)*, coded as 1 if the number of admitted inmates is greater than the number of discharged inmates.[16]

In column 1 of Table 3 we once again establish a statistically significant first-stage estimate. However, the F-statistic of approximately 8 indicates that we are not establishing a strong instru-

[16] The number of observations differs between outcome variables due to the missing responses from the Census of Jails.

ment using this smaller data sample. Columns 2-4 examine the OLS, reduced form and two-stage least squares estimates when examining the discharge to admission to prison ratio as our dependent variable. Columns 5-7 examine the OLS, reduced form and two-stage least squares estimates when examining a binary variable for whether the admission to discharge ratio is greater than one. In both of these specifications, we find no statistically significant relationship in the OLS or two-stage least squares estimates. Thus, unlike the results in Table 2, when we examine information about the discharge and admission of inmates, we do not find evidence of overcrowding.

Table 3: First Stage, OLS, Reduced Form, and 2SLS Estimates of the Effect of Sheriff Tenure on Jail Discharge/admission Ratio and 1(Admission>Discharge)

		Discharge/Admission Ratio			1(Admission>Discharge)		
	(1) First Stage	(2) OLS	(3) Reduced	(4) 2SLS	(5) OLS	(6) Reduced	(7) 2SLS
Term limit	-1.625***		0.018			-0.010	
	(0.262)		(0.042)			(0.030)	
Tenure		-0.001		-0.011	-0.000		0.006
		(0.004)		(0.026)	(0.003)		(0.018)
Percent female	0.152	-0.072	-0.073	-0.071	-0.005	-0.005	-0.006
	(0.133)	(0.051)	(0.051)	(0.049)	(0.016)	(0.016)	(0.016)
Percent white	0.030	0.001	0.001	0.002	-0.002+	-0.002+	-0.003+
	(0.020)	(0.002)	(0.002)	(0.002)	(0.001)	(0.001)	(0.001)
Percent black	-0.014	0.007	0.007	0.007	-0.000	-0.000	-0.000
	(0.062)	(0.006)	(0.006)	(0.006)	(0.006)	(0.006)	(0.006)
Income per capita (in thousands)	0.002	-0.005	-0.005	-0.005	-0.001	-0.001	-0.001
	(0.028)	(0.004)	(0.004)	(0.004)	(0.002)	(0.002)	(0.002)
Employment per capita	0.033*	0.001	0.001	0.002	-0.000	-0.000	-0.001
	(0.014)	(0.002)	(0.002)	(0.002)	(0.001)	(0.001)	(0.001)
Population coverage per agency	0.000	0.000	0.000	0.000	0.000*	0.000+	0.000+
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Observations	1267	1267	1267	1267	1267	1267	1267
Mean. of Dept. Var.	5.31	1.004	1.004	1.004	0.575	0.575	0.575
R^2	0.22	0.02	0.02	0.01	0.01	0.01	0.00
F stat	7.70	0.85	0.84	0.84	1.51	1.51	1.51
Year FE	X	X	X	X	X	X	X
Covariates	X	X	X	X	X	X	X

Robust standard errors clustered at the county level
*** p<0.001, ** p<0.01, * p<0.05, + p<0.10
Notes: Column (1) presents the first stage estimate, the effect of term limit on tenure. Column (2), Column (3) and Column (4) show OLS, reduced form, and 2SLS regression results for Discharge to Admission Ratio. Column (5), Column (6), and Column (7) display OLS, reduced form, and 2SLS regression results for 1(Admission > Discharge). The unit of observations is the county-year level.

3.6 Mechanism and Robustness

We examine the mechanism and robustness of our results in this section. To examine the mechanism through which increases in sheriff tenure are leading to higher jail-occupancy rates we explore heterogeneous results by dividing our data along political party lines (Democratic versus Republican dominant counties) as well as counties that are politically competitive versus counties that are not. We also conduct a falsification exercise to determine if our results would have been obtained randomly.

3.6.1 Political Competition

Political competition could drive sheriff behavior in our data, as sheriffs might have more leeway in operating jails in the absence of political competition. Besley et al. (2010) constructed a party neutral variable measuring political competition by looking at vote margins. It suggests that we can measure (the lack of) political competition by the dominance of either the Democratic or Republican party in elections.

While we would like to utilize information on sheriff elections to determine whether elections were contested, this is not possible due to a lack of detailed information on sheriff election results. To deal with this issue, we imported county vote shares for state legislature elections as a proxy. County vote shares include those for governors, senators, US representatives, state offices, and US presidents, which provides election data every other year. To conduct this analysis, we obtained county-level state legislature election voting results from ICPSR 00013 for 1970–1990 and CQ press[17] for data after 1990. We filled in missing years with the average of the most recent elections.

We then calculate the ratio of Democratic votes to the sum of Democratic and Republican votes and subtract 0.5 to obtain the vote margin variable[18]:

$$Vote\ margin_{it} = Dem_{it}/(Dem_{it} + Rep_{it}) - 0.5 \tag{4}$$

where Dem_{it} is the total Democratic votes and Rep_{it} is the total Republican votes.

We convert the continuous vote margin measure into a binary variable based on a 5-year moving average of the vote margin. A binary variable is coded as 1 if vote margin is *close*. Vote margin is considered to be *close* when it is between the 5-year moving average +/- the standard deviation divided by 3[19]:

[17] "Voting and Elections Collection" data retrieved from CQ Press. See `https://library.cqpress.com/elections/static.php?page=sources-and-definitions&type=public`.

[18] We note that a similar measure of political "closeness" is used in Lee et al. (2004).

[19] In results available upon request we produce results that use a more and less stringent threshold to measure

$$Competitiveness = 1\ if\ Vote\ margin \in (\mu_{it} - sd_{it}/3,\ \mu_{it} + sd_{it}/3) \qquad (5)$$

where μ_{it} is the 5-year moving average of vote margin and sd_{it} is the standard deviation of the vote margin.

If the vote margin is *close*, then the incumbent is likely concerned about maintaining control of their elected position and will engage in practices that will enhance their likelihood of re-election. Alternatively, if the vote margin is not *close*, then the incumbent would be less motivated by election concerns. The 5-year window is intended to incorporate the dynamics of vote margins across locations changing over time. We ensure approximately 25% of observations would be coded as locations with political competition by using the bounds of standard deviation divided by 3. Outside of these bounds approximately 75% of our observations will be coded as locations without political competition.

Panel A and Panel B in Table 4 report the 2SLS results for the Democratic-party-dominant locations without and with political competition, respectively, with the jail-occupancy rate as the outcome variable. We observe that jail-occupancy rates are rising with sheriff tenure in Democratic-party-dominant locations with political competition, suggesting that sheriffs increase jail-occupancy rates when they are likely to face backlash in the form of political competition.[20] Thus, it appears that sheriffs in politically competitive, but Democrat majority counties move toward being "tougher on crime", perhaps to attract the votes of more conservative voters.

In Panel C and Panel D of Table 4 we report the 2SLS results for the Republican-party-dominant locations without and with political competition, respectively. We observe the biggest effect of sheriff tenure on jail-occupancy rates in politically non-competitive locations, whereas we do not observe any effect in politically competitive locations. Thus, the result of longer tenure leading to higher jail-occupancy rates appears to be primarily attributable to Republican-dominant locations without political competition. We interpret these results as being consistent with existing theories about the role of sheriffs being "tough on crime" in Republican dominant locations with a lack of

"closeness" and find similar results.

[20]We note, though, that by subsetting the data we do encounter issues of instrument strength.

Table 4: Examining 2SLS Estimates by Political Party and Political Competition

	(1) First	(2) OLS	(3) Reduced	(4) 2SLS
Panel A: Democratic party without political competition				
Term limit	-2.902***		-0.013	
	(0.548)		(0.045)	
Tenure		-0.006+		0.005
		(0.004)		(0.015)
Observations	493	493	493	493
Mean. of Dept. Var.	5.20	0.859	0.859	0.859
R^2	0.23	0.15	0.14	0.02
F stat	5.70	2.93	2.53	2.51
Year FE	X	X	X	X
Covariates	X	X	X	X
Panel B: Democratic party with political competition				
Term limit	-3.110***		-0.105*	
	(0.890)		(0.047)	
Tenure		0.005		0.034+
		(0.004)		(0.018)
Observations	189	189	189	189
Mean. of Dept. Var.	5.50	0.871	0.871	0.871
R^2	0.34	0.27	0.29	-0.01
F stat	3.01	2.97	3.26	2.56
Year FE	X	X	X	X
Covariates	X	X	X	X
Panel C: Republican party without political competition				
Term limit	-1.737***		-0.088**	
	(0.412)		(0.030)	
Tenure		-0.002		0.051*
		(0.003)		(0.021)
Observations	685	685	685	685
Mean. of Dept. Var.	5.94	0.838	0.838	0.838
R^2	0.17	0.16	0.17	-0.63
F stat	5.64	2.87	3.53	2.08
Year FE	X	X	X	X
Covariates	X	X	X	X
Panel D: Republican party with political competition				
Term limit	-1.525**		-0.060	
	(0.570)		(0.059)	
Tenure		-0.004		0.039
		(0.005)		(0.043)
Observations	284	284	284	284
Mean. of Dept. Var.	5.93	0.839	0.839	0.839
R^2	0.27	0.13	0.13	-0.30
F stat	3.12	2.70	1.64	0.87
Year FE	X	X	X	X
Covariates	X	X	X	X

Robust standard errors clustered at the county level
*** p<0.001, ** p<0.01, * p<0.05, + p<0.10
Notes: we calculated the ratio of Democratic votes to the sum of Democratic and Republican vote shares and subtracted 0.5 to obtain the vote margin variable. The information for the relative vote share comes from county level votes for state legislature elections including those for governors, senators, U.S. representatives, state offices, and presidents every other year, after filling in information of the missing years. The political competition measure is then calculated using the vote margins generated above. A binary variable is generated measuring whether the continuous vote margins are tight based on five year moving average of vote margins. Vote margins are considered to be tight when they are between the moving average +/- the standard deviation divided by three.

political competition.

3.6.2 Falsification

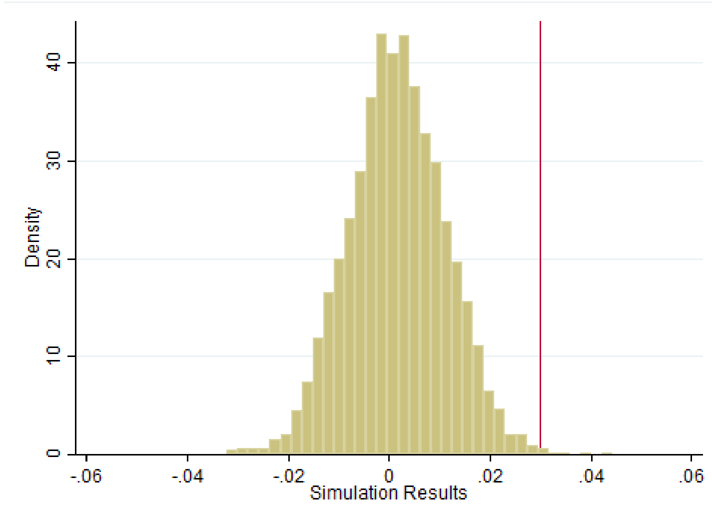
We conduct two-step randomization inference to detect whether a placebo effect exists by structurally randomizing the tenure variable after randomly assigning term limits. The following steps are taken to conduct the two-step randomization test. First, we preserve the ratio of term-limited observations to total observations in each year and randomly assign false term limits to observations. Tenure length is then randomly matched with integers from a uniform distribution in the interval [1, 8], within the falsely assigned term-limited locations. We obtain 2SLS estimates based on the assigned term limit and tenure variables, and then calculate the distribution of 5,000 estimates from two-step randomization.[21] As expected, the average placebo effect obtained from randomization is centered about zero. However, as seen in Figure 3, our main 2SLS estimate (the vertical red line) of sheriff tenure on jail-occupancy rates is statistically different from the randomized distribution of placebo effects with 5,000 iterations, indicating that that true effect would not have been obtained at random.

3.7 Conclusion

Calls for criminal-justice reform have become commonplace and vary widely from de-funding law enforcement to not arresting or prosecuting low-level offenses. In this work we examine another criminal justice issue that has attracted considerable attention - occupancy rates in jails. We specifically examine the impact of a sheriff's tenure on the occupancy of the jail that they manage. The rationale for our focus on sheriffs and jails is two-fold. First, sheriffs are one of the few elected actors in the criminal justice system. In most counties in the United States they also manage the operations of the jail. Second, a unique feature of sheriffs is that a subset of sheriffs in the United States are limited to a maximum of two terms as the sheriff.

[21]We do not obtain the randomized distribution from the total set of combinations due to computational limitation. Suppose we are to randomize the tenure length with 8 values (from 1 to 8), then even with just 10 locations, there would be more than 1 billion (8^{10}) permutations. The actual set of tenure length values in our data range from 1 to 33. Imbens and Rubin (2015) show in a simulation study that the randomization inference p-values become stable after 1,000 iterations in the case of a binary treatment variable.

Figure 3: Distribution of Simulated Estimates



Notes: the graph shows the distribution of 2SLS estimates from randomly assigning tenure values to term limited locations. The vertical red line is the 2SLS estimate from the main results regarding the jail-occupancy rate (0.030). Considering 0.0176 is the value at the 95th percentile of the distribution, we can conclude that the main 2SLS estimate is plausibly different from the randomly simulated distribution.

We exploit the two-term limit on a subset of sheriffs to explore the relationship between the tenure of a sheriff and the occupancy rate of the jail that they manage. The two-term limit on sheriffs enable us to explore the causal effect of tenure on jail occupancy. We find that increases in tenure lead to higher occupancy rates in county jails. This result is robust to alternative specifications and also holds up to randomization inference. To explore the mechanism leading to higher jail-occupancy rates with increased sheriff tenure we present heterogeneous effects by splitting locations along two margins: (1) whether the county is Democrat-or-Republican dominant and (2) whether or not elections are competitive. Our heterogeneous results indicate that our main results are being driven by counties that are Republican dominant and that do not face political competition in elections.

It is important to note that this research produces avenues for future research. First, given the granularity of our data, we are not able to include geographic (or geography-by-time) fixed effects, which would absorb unobserved variation in the data that could simultaneously be explaining election outcomes and jail-occupancy rates. Second, while the focus of this research is on jail-occupancy rates, there is considerable interest in the effect of criminal-justice leadership on other measures of community safety.

Our research contributes to the growing literature on the relationship between elections and the criminal justice system, especially as calls for criminal-justice reform continue to grow. We also further the literature on criminal-justice leadership and the impact that leadership tenure has on the safety of the community. While our analysis enables us to examine one important measure of the criminal justice system, jail-occupancy rates, the impact of sheriff tenure on criminal behavior deserves further attention, which could be a fruitful area of future research.

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