A Silent Corrupting Force?
Criminal Sentencing and the Threat of Recall

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Abstract

39 U.S. states authorize recall elections, but the incentives they create are not well understood. We examine how changes in the perceived threat of recall alter the behavior of one set of officials: judges. In 2016, outrage over the sentence imposed on a Stanford athlete following his sexual assault conviction sparked a drive to recall the presiding judge. Using disposition data from six California counties and arrest records for a subset of defendants, we find a large, discontinuous increase in sentencing severity associated with the recall campaign's announcement. Additional tests suggest that the observed shift may be attributed to changes in judicial preferences over sentencing and not strategic adjustment by prosecutors. We also demonstrate that the heterogeneous effects of the announcement did not mitigate preexisting racial disparities. Our findings are the first to document the incentive effects of recall and suggest that targeted political campaigns may have far-reaching, unintended consequences.

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1 Introduction

When evaluating the performance of incumbent officials, voters may be only dimly aware of those officials' choices and their consequences, and the availability to voters of relevant information may itself be contingent on incumbent behavior. Either of these considerations may distort the incentives of elected officials. Accordingly, assessing the extent to which electoral institutions mitigate or exacerbate such distortions is a critical task for empirical political science.

One electoral institution that enjoys widespread use in the United States is the recall election, in which voters may remove an incumbent from office before the expiration of his or her ordinary term. 39 states authorize recall elections for at least some offices. In the 19 states that allow for gubernatorial recall, eleven governors have faced recall since 2018 (Greenblatt, September 17, 2020). And of the 45 state-level recall elections in U.S. history, nearly half have occurred in the past ten years.[1]

Despite the availability and increasing prominence of the recall option, there exists, to our knowledge, no study systematically assessing how the threat of recall affects incumbent behavior. This lacuna may stem in part from a host of methodological challenges. Most obviously, recall campaigns are not randomly assigned, and so comparing officials who do and do not experience them is likely to suffer from substantial omitted variables bias. Relatedly, if public officials rationally anticipate the consequences of a recall threat, they may take pains to avoid it. And finally, the behavior of officials in jurisdictions with and without the recall institution may differ in innumerable ways besides the availability of that specific institution, and the durability of the institution makes within-jurisdiction comparisons infeasible.

To circumvent these difficulties, we examine the effect of a shock to the salience of the recall threat brought by a widely publicized and ultimately successful recall campaign. In

[1]Source: `https://www.ncsl.org/research/elections-and-campaigns/recall-of-state-officials.aspx`.

June 2016, Santa Clara Superior Court Judge Aaron Persky achieved notoriety for imposing an unusually lenient sentence on Brock Turner, an affluent, white Stanford student athlete convicted of two counts of sexual assault and one count of attempted rape. Two years later, 61.5% of Santa Clara voters elected to remove Persky from office.

Several weeks before the 2018 vote, Persky delivered a speech with the following warning:

> We promise as judges to rule on the facts and on the law, not on public opinion... When public opinion influences a juror's decision or a judge's decision, it corrupts the rule of law. This recall, if successful, will make it harder for judges to keep that promise ... The judicial recall, if successful, will be a silent force, a silent corrupting force. A force that will enter the minds of judges as they contemplate difficult decisions.

A host of elected officials, political activists, and legal academics echoed Persky's warning about the incentive effects of the recall effort, arguing that it might push judges to become more punitive in their sentencing decisions, even while condemning the leniency of the specific sentence that instigated the campaign.[2] Several of these observers argued that the burden of any change in electoral incentives would be borne disproportionately by minority defendants rather than white ones – a cruelly ironic prediction given that the behavior the recall aimed to sanction was leniency toward a white defendant.

Using data on almost 20,000 sentences handed down by over 158 Superior Court Judges in six California counties from 2015 to 2018, we examine whether critical events in the recall campaign were accompanied by corresponding changes in other judges' sentences. Specifically, using a regression discontinuity in time (RDit) approach (Hausman and Rapson, 2018), we examine the effects of two specific events: the initial announcement of the recall

[2]In a similar spirit, we wish to strongly caution against interpreting any of the findings presented in this paper as speaking to the merits of the specific sentence that motivated Judge Persky's recall.

petition; and the recall election itself.

Our main results point to an instantaneous increase in average sentence length of over 30% in the immediate aftermath of the recall petition announcement. This result is robust to the inclusion of judge- and charge-level fixed effects, and a battery of placebo and specification tests. We demonstrate that the effect is driven by increases in sentencing on non-sexual violent crimes, and is unlikely to be an artifact of strategic adjustment by prosecutors. In contrast to the results for the petition announcement, we find no evidence that the recall election itself induced changes in sentencing. The conjunction of these findings suggests that the announcement of a well-organized, well-funded recall campaign against a Superior Court Judge signaled a new political reality for judges that was "priced in" by judges by the time the election took place.

Next, we consider whether the effects of the observed shift were borne disproportionately by minority defendants. Drawing on recent research decomposing the sources of racial disparities in sentencing (e.g., Rehavi and Starr, 2014), we hypothesize direct and indirect channels through which disproportionate burdens might manifest themselves. We find little evidence for either channel: sentencing increases were larger for white than minority defendants, but this does not appear to have mitigated preexisting, longer-term racial disparities.

In the last part of the paper, we estimate the aggregate effect of the change in judicial precipitated by the petition announcement over a narrow (45 day) time frame. Our most conservative estimates suggest that the petition announcement led to approximately 88 years of additional prison time in the six counties for which we have data.

In the most immediate sense, our findings corroborate the concern that the campaign to remove a sitting a judge would affect the behavior of other judges, and amplify preexisting disparities in the criminal justice system. More generally, they contribute to our understanding of the role recall elections may play in contemporary political life. Arguments in favor of recalling an elected official invariably focus on a selection function: the recall gives voters the opportunity to remove a specific malfeasant public official. The findings presented here suggest broader incentive effects that may extend beyond the official in question, and that may operate counter to the objectives of the recall's proponents.

2 Background

2.1 Institutional Setting

Recall elections in California and elsewhere. 39 states allow recall elections – those in which voters have the opportunity to remove a public official prior to the expiration of his or her term – in some form. Considerable variation exists, however, with respect to the particulars: whether state or local officials are eligible; the eligibility of appointed officials; signature requirements; permissible grounds for recall; and procedures for filling vacancies from successful recalls. Of the 39 states that permit recall, seven (AZ, CA, CO, MN, ND, OR, and WI) specifically permit recall of judges.

California, the setting of the empirical analysis that follows, adopted recall elections by a constitutional amendment in 1911. Since the amendment went into effect in 1913, there have been 165 attempts to recall statewide officials, of which ten qualified for the ballot, and six were successful – the most well-known being the recall of Governor Gray Davis in 2003.[3] Far more ubiquitous in the state are recall efforts for local officials. Elected state legislators have been removed by voters in safe as well as very competitive districts.[4] Since 1995, the earliest

[3]Source: Complete List of Recall Attempts, California Secretary of State. Available at `https://www.sos.ca.gov/elections/recalls/complete-list-recall-attempts/`.

[4]Two of the five recalled state legislators were Republican state Assembly members removed by Republicans in heavily Republican districts, ostensibly for compromising with Democrats (Morton 2006). The most recent legislator, Democratic State Senator Josh Newman, lost a 2018 recall after winning his seat by 0.8% in 2016. The Republican-led recall protested his support for a bill that raised California's gas tax. Source: `https://www.sfchronicle.com/politics/article/Recall-of-state-Sen-Newman-costs-Democrats-12971819.php`.

year for which we have systematic data, recall attempts for 333 local officials have qualified for the ballot (reflecting a fraction of the full set of recall attempts); of these 244 have been successful.[5] The anti-Persky campaign, discussed in greater detail below, represents the only attempt to recall a superior court judge to qualify for the ballot anywhere in the U.S. since 1982 (Spivak, 2020).[6]

Judges and judicial discretion in California. California has the largest judicial system in the nation, with 1,743 authorized superior court judges sitting in 58 county courts. During 2016– 2017, approximately 6 million cases were filed in these courts. Superior courts in California have jurisdiction over civil and criminal cases. These courts also hear appellate cases arising from certain civil cases worth under $25,000 as well as some misdemeanor cases.[7] Since 1998, superior courts are the only consolidated general jurisdiction trial courts. Superior court judges run in non-partisan competitive elections for six-year terms. In the event of a vacancy, judges are appointed by the Governor.

Judicial discretion over sentencing in California is constrained by a complex array of considerations. Since 1977, sentencing for most crimes operates according to a triad system, in which the judge is given the choice between upper, middle, and lower "base" terms. For example, Assault with a Deadly Weapon (§245(a)(1) of the California Penal Code) carries a base term of 2, 3, or 4 years in prison. Although there is a a presumption in favor of the middle term in the absence of aggravating or mitigating factors, few sentences precisely match the three prescribed base terms, for three reasons. First, judges have discretion over whether the sentences for convictions on multiple counts run consecutively or concurrently.

[5]Source: California Election Data Archive, available at `http://hdl.handle.net/10211.3/210187`.

[6]It turns out that Persky was not the first lower court judge recalled in California history. Three Los Angeles County judges were recalled in 1932 following a campaign against them by the California Bar Association (Smith, 1951). To our knowledge, this precedent was never cited in contemporary coverage of the Persky recall.

[7]`https://www.courts.ca.gov/documents/California_Judicial_Branch.pdf`.

Second, judges can issue sentencing enhancements for aggravating factors such as gang or hate crimes, or prior convictions. Third, since 2011, judges have been granted discretion to issue suspended or split sentences for certain felonies.[8]

As is generally the case in the United States, the vast majority of cases are resolved via plea bargain. Plea agreements consist of a guilty plea and a sentencing recommendation to the judge, who has ultimate discretion on whether to accept or reject it.[9] Even still, a threat to inference that must be addressed is the possibility that the Persky recall induced changes in the behavior of *prosecutors* rather than in the behavior of judges. We discuss this issue, and our strategy for circumventing it, in detail below.

2.2 The Persky Recall

Our empirical analysis focuses on a shock to the salience of the recall threat to judges in California brought about by the campaign to recall Judge Aaron Persky from 2016 to 2018. The campaign was initiated in response to Judge Persky's sentencing decision in a widely publicized sexual assault case. On January 28, 2015, Brock Turner, a white Stanford student athlete, sexually assaulted Chanel Miller,[10] a visiting student, and was arrested. Five days later, Turner was indicted on two rape counts, two felony sexual assault counts, and one attempted rape count. The rape charges were later dropped, and in March 2016, Turner was convicted on the sexual assault and attempted rape charges.

[8]Effective since 2015, many crimes that are neither sexual crimes, violent crimes nor serious crimes are also eligible for county jail sentencing (for terms of 16 months, 2 years, or 3 years).

[9]See, e.g., *People vs. Orin*, 13 Cal. 3d 937 (1975: "Judicial approval is an essential condition precedent to the effectiveness of the 'bargain' worked out by the defense and prosecution ..."), more recently reiterated in *People vs. Clancy*, 56 Cal. 4th 562 (2013). Some counties in California authorize limited participation of judges in plea negotiations.

[10]While ordinarily we would strictly adhere to the norm of respecting the anonymity of sexual assault victims, Miller has specifically expressed a preference *not* to remain anonymous, both in public appearances and her memoir (aptly titled *Know My Name*).

Turner faced a maximum sentence of 14 years for these convictions, but on June 2, 2016, Judge Persky sentenced Turner to six months in prison and three months probation. The lenient sentence and Miller's impact statement, published by Buzzfeed, sparked widespread national attention.[11] On June 6, 2016, Stanford Law School Professor Michele Dauber announced the formation of a committee and began the process of collecting signatures to recall Judge Persky. With 94,000 verified signatures collected, the Santa Clara Registrar certified the signature threshold had been met on January 24, 2018. Finally, Judge Persky was recalled (with 61.5% supporting removal) on June 5, 2018. According to the *Palo Alto Daily Post*, the campaign to remove Persky raised more than one million dollars.[12]

Criticisms of the recall campaign were immediate and widespread. 95 Californian law professors signed an open letter in August 2017 opposing the recall petition. Californian mayors, state legislators, former Supreme Court justices, and hundreds of Superior Court judges supported the Retain Judge Persky Campaign.[13] Critics were primarily concerned with judicial independence and impartiality (Santa Clara County Association, June 14, 2016; Law Professors Statement, August 15, 2017). Some critics also predicted an increase in judicial punitiveness, with disproportionate effects on minority defendants (Butler, July 11, 2016; Gersen, June 17, 2016; Woolf, June 24, 2016). These predictions were bolstered by the empirical literature, cited below, documenting how concerns with reelection induce trial judges to impose longer sentences; as well as the significant literature, also discussed below, documenting the disproportionate burden imposed by the criminal justice system on minority

[11] https://www.buzzfeednews.com/article/katiejmbaker/heres-the-powerful-letter-the-stanford-victim-read-to-her-ra.

[12] https://padailypost.com/2018/05/27/recall-persky-campaign-raises-more-than-1-million/. By contrast, an attempt to recall an Orange County judge, Marc Kelly, in 2015, raised less than $25,000 and did not achieve the required number of signatures (Source: https://www.nbcnews.com/news/us-news/group-pushing-recall-effort-stanford-rape-case-judge-it-long-n590431).

[13] The associated website, Voices Against Recall, has since been removed. An archived version is available here: https://web.archive.org/web/20180423164925/http://www.voicesagainstrecall.org/.

defendants.

2.3 Related Research

Electoral incentives. The current research contributes to a rich literature on the incentive effects of electoral institutions on the behavior of incumbents generally (see, especially, Besley and Case, 1995; Alt, Bueno de Mesquita, and Rose, 2011; Ferraz and Finan, 2011) and judges specifically (Besley and Abigail Payne, 2013; Brace and Hall, 1995; Huber and Gordon, 2004; Gordon and Huber, 2007; Lim, 2013; Berdejo and Yuchtman, 2013; Matsusaka et al., 2010). One feature of judicial elections that makes them particularly noteworthy in the empirical analysis of electoral incentives is the nature of the informational environment in which they occur. Voters often lack verifiable information to evaluate judicial performance, a problem further complicated by the fact that judges often face voters in retention elections (in which there are no challengers) and nonpartisan elections (in which voters lack clear cues such as party labels). As a result, voters may be highly responsive to well-publicized examples of apparent judicial "error," as revealed by the media, organized interest groups, or challengers. If being perceived by voters as overly lenient is either more likely or more politically costly than being perceived as overly punitive, judges will face electoral pressures to become more punitive than they would be otherwise, *even if* under ordinary circumstances voters know little or nothing about judicial behavior.

Recall Elections. To our knowledge, there exists no extant empirical research on the incentive effects of recall elections. Political science research on recall elections has instead focused on voter behavior in recall elections – see, e.g., Ho and Imai (2006); Segura and Fraga (2008); Masket (2011); Shaw, McKenzie, and Underwood (2005). One explanation for this lacuna might be that the most straightforward research designs available to researchers do not translate well to the recall setting. Because the institution of recall is not randomly assigned, comparing the behavior of officials in states with and without recall is likely to be confounded by numerous other interstate differences. There are also issues characterizing variation in the "treatment" of officials within the same state because the timing and occurrence of recall attempts are random and idiosyncratic. Finally, studying changes in the behavior of an individual official subject to a recall effort will afford essentially no statistical power. More generally, a challenge to studying the effects of recall elections on official behavior is that the threat of recall will be "priced into" the behavior of the officials. Unanticipated shocks, should they occur, are likely to be exceptionally rare and highly localized.[14]

Judicial bias and asymmetric burdens of criminal justice system African Americans face a six-fold greater rate of imprisonment than whites in the United States (Bronson and Carson, 2019). While noting potential racial differences in criminal behavior, a number of recent papers have highlighted the influence of disparities induced by judicial and prosecutorial discretion, even among defendants facing similar charges and of similar criminal backgrounds. Evidence from randomly assigned cases indicates that judges differ in the degree to which race influences their likelihood of incarceration (Abrams, Bertrand, and Mullainathan, 2012). In Kansas, retention pressures, discussed above, induce increased judicial punitiveness but only in cases involving black felons (Park, 2017). Capital punishment sentences involving white victims are significantly more likely to be overturned by appellate courts when the defendant is African American, providing evidence that lower courts discount the wrongful convictions of black defendants (Alesina and La Ferrara, 2014). Racially disparate judicial decision-making is in turn compounded by racial disparities in charging and plea bargaining (Rehavi and Starr, 2014), jury decision-making (Bayer, Hjalmarsson, and Anwar, 2012) and policing (Grogger and Ridgeway, 2006).

[14] In this vein, the unusual nature of the Persky recall should be viewed as an essential feature of our research design rather than a flaw.

3 Data and Approach

3.1 Data on Sentencing in California

Unlike in other states, at the time of writing there is no publicly accessible, centralized repository for sentencing data. To overcome this limitation, we scraped criminal cases with hearing dates between January 2015 and December 2018 from the websites of six superior courts that make these data available electronically: Fresno, Napa, Sacramento, Santa Barbara, San Bernardino, and Santa Cruz.[15] Our search produced a total of 19,744 cases encompassing 21,939 felony charges with initial sentencing dispositions in the remaining six courts.[16] The sample counties represent 19% of California's total incarcerated population. While we make no claims concerning how representative these counties are of the broader state, we have no a priori reason to believe that the effect of the recall should be larger or smaller in these counties than in counties for which data were not readily available.

Each count on which the defendant is convicted is associated with a sentence length in days. 92% of cases have only one count in the conviction. For the remaining 8% of cases with multiple counts, we encountered a number of inconsistencies in the data: generally, whether sentences run concurrently or consecutively is not evident – in some cases, the total sentence is entered for a top count (in clear excess of the legal maximum for that count), while in others the same sentence is entered for counts with dramatically different sentencing ranges. To reduce the effect of this issue, in our main analysis we restrict attention to the sentence entered for the top count in the conviction, and for specifications in which we adjust for covariates, we condition on the total number of counts. We also conduct a robustness test in which we restrict attention to convictions with only one count.

For each offense code, we acquired base terms from the State of California Attorney

[15] Alameda also has some online data, but it is apparently radically incomplete: fewer than 170 felony convictions are detailed for the relevant period.

[16] Sentences may be amended – for example, in cases of probation violations.

General's office operative for the period of our sample.[17] Additional case information in our final dataset include the charge (410 unique offenses) and sentencing judge (157 unique judges). We categorized crimes as nonviolent or violent based on offense codes from the California Attorney General: 82.7% of cases in the sample are classified as nonviolent crimes; 4.3% as (violent) sex crimes, and the remaining 13% as other violent crimes.

To explore heterogeneity by race, we linked defendants in our data to publicly available arrest records sourced from county and municipal law enforcement agencies in California.[18] We crawled 201,066 arrest records. Defendants were matched based on first name, middle name, last name, county of arrest and arrest date. Across the six counties, 12,844 defendants could be matched to arrest records, of which 11,184 defendants have race identified.

3.2 Empirical Approach

In the main part of our analysis, we look for sharp increases in judicial punitiveness immediately following key moments during the recall campaign. In particular, we consider two critical events: the announcement of the campaign itself, on June 6, 2016; and the recall election itself, on June 5, 2018.[19] Our main specification is the following local linear estimator of a regression discontinuity in time (RDit; see Hausman and Rapson (2018)):

$$y_{ijt} = \beta_0 + \beta_1 \mathbb{I}(t > t_k) + \beta_2 f(t - t_k) + \varepsilon_{ijt} \tag{1}$$

Where t_k is the calendar date of a critical event k; $y_{ijt} \equiv \min\{s/\overline{s}, 1\}$ is the normalized sentence of conviction i by judge j at time t (cf., Lim, 2013); and $f(\cdot)$ is smooth function of time. The normalization divides the sentence length in days s by the upper base term

[17] https://oag.ca.gov/law/code-tables.

[18] Source: https://www.localcrimenews.com/.

[19] Another candidate is the date on which petition signatures were certified by the Santa Clara Registrar: January 24, 2018. Results from this event may be found in Appendix Figure B.I.

$\overline{s}$, creating a fractional measure of judicial discretion expressable in percentage terms and comparable across different offenses. So, for example, a sentence of six months on an assault with a deadly weapon charge with an upper base term of four years would be coded as 0.125. The measure is censored at one so as not to be skewed by cases with unusual aggravating factors (or multiple charges) that increase the sentence above the upper base term for the top count. In point of fact, more than 95% of cases fall at or below the upper base term. In robustness tests we use the uncensored measure as well as the raw sentence (in days) as outcome measures. We report bias-corrected RD estimates with MSE-optimal bandwidths (Calonico, Cattaneo, and Titiunik, 2014).

In addition to this unadjusted specification, we also present results throughout that adjust for a vector of judge- and offense-specific fixed effects, as well as the number of counts in the conviction. The adjusted estimates discard sentencing data from Sacramento County, whose data do not include judge identifiers. For both sets of estimates, we weight observations using a triangular kernel. Standard errors for the unadjusted specifications are clustered at the county-statute level, and adjusted specifications at the judge-statute level.

As noted above, we wish to rule out the possibility that observed changes in sentencing associated with the Persky recall are driven by prosecutorial, rather than judicial behavior. Note that from the perspective of the analyst, a more stringent plea offer made by a prosecutor in expectation of increased judicial stringency is observationally equivalent to a relatively lenient plea offer that is rejected by a judge, necessitating a second round of negotiation between the prosecutor and defendant. Both, however, are consistent with the account of stronger electoral incentives for judges brought about by the recall campaign. Another possibility, however, is that the recall campaign induced an increase in prosecutorial rather than judicial stringency. To adjudicate between these two accounts, we first assess whether the recall campaign led to a reduced willingness of prosecutors in a domain over which they exert greater autonomy: charge reduction, that is, the decision to drop higher counts in an indictment as a condition of a guilty plea. Our measure of charge reduction is equal to one minus the ratio of the maximum sentence at conviction to the maximum sentence at arraignment (restricted to the set of cases that did not go to trial and for which arraignment occurred prior to the critical date). To be sure, a prosecutor's inflexibility in this area will reflect expectations about the likelihood of conviction on more severe charges. To the extent that the recall campaign influenced *jury* decisionmaking at trial, it would bias any finding of the effect of the recall on charging away from zero. Accordingly, a null finding for this test would strongly argue against the importance of the prosecutorial channel. We also conduct supplemental tests to see whether critical dates in the recall process were followed by compositional changes in the set of top charges reached at conviction.

The identifying assumption of regression discontinuity designs is that treatment assignment is ignorable (conditional on covariates) sufficiently close to the cutoff (the critical event in the RDit setting). We examine threats to inference arising from shocks that vary discontinuously within the treatment windows. A sequence of placebo regressions for all dates in each calendar year alleviates the concern that the findings result from some confounding structural break (for instance, the ratification of two laws in September 2016 requiring mandatory sentences for sexual assault). To bolster further our claim that the recall events do not coincide with unrelated shocks to judicial decision-making, we examine contemporaneous sentencing patterns in the nearby state of Washington. Finally, we assess robustness to various specifications of the outcome and bandwidth.

Next, we examine whether any observed effects of key events on punitiveness are driven by sentencing for sexual, non-sexual violent or nonviolent crimes. As the recall campaign centered around Judge Persky's sentencing in a sex crime case, judges might have anticipated that voters would pay greater attention to perceived leniency on similar cases.

Third, we assess whether any increase in judicial punitiveness induced by the recall campaign placed a disproportionate burden on minority defendants, as anticipated by some of the campaign's critics. It is important to note that there are (at least) two channels through which this might operate. One involves judges apprehensive that a racially biased electorate might react especially negatively to perceived leniency toward minority defendants. In that case, we would anticipate that the instantaneous effect of the recall on sentencing would be larger for them than than for white defendants. Call this the *direct racial burden* hypothesis.

Testing the direct burden hypothesis is subtle, as the following example demonstrates: suppose that racial disparities in discretionary sentencing were *already present* prior the electoral shock, so that white defendants were sentenced at the lower, and minority defendants at the upper, ends of judges' discretionary sentencing range. In that case, and even in the presence of the direct channel, we might observe a (weakly) larger effect of the electoral shock for white than minority defendants. To assess this possibility, we examine whether race-based disparities in discretionary sentencing were in evidence immediately prior to the electoral shocks. Note that this is a purely descriptive exercise aimed at clarifying the mechanism – in the current context we lack a strong identification strategy for assessing the causal effect of race on sentencing discretion directly (clearly a critical task, but one beyond the scope of the current paper).

The absence of evidence for the direct burden hypothesis does eliminate the possibility that minority defendants are hit harder by the consequences of an electoral shock brought about by the recall drive. This is because it could be that white and minority defendants are charged with crimes that vary in their severity. Suppose judges increase their discretionary sentencing in an apparently race-neutral way – e.g., from 0.5 to 0.6 on the normalized sentencing scale – but that minority defendants tend to be convicted of crimes with higher statutory maximum penalties. Then the electoral shock will mechanically lead to a higher cumulative sentencing load for minorities. Call this the *indirect racial burden* hypothesis. To assess the indirect burden hypothesis, we examine whether minority defendants in our sample are convicted of crimes with higher maximum penalties, and test for heterogeneous effects in total sentencing.

The RDit approach identifies a local average treatment effect (LATE) at the precise moment of the critical event in question. In the final part of our analysis, we compute aggregate effects, which require estimating longer-term consequences of electorally-induced shifts in judicial behavior. Doing so requires more stringent identifying assumptions than those necessary to identify the LATE. Accordingly, rather than committing ourselves to one set of assumptions, we adopt four separate approaches: (1) assuming the estimated LATE persists as an average treatment effect in a window of time after the announcement; (2) a fully parametric approach that attributes any post-announcement time trends to decay or growth in the effect of the announcement itself; (3) a linear reweighting estimator; and (4) a propensity score estimator (the latter two approaches suggested by Angrist and Rokkanen (2015)).

4 Empirical Results

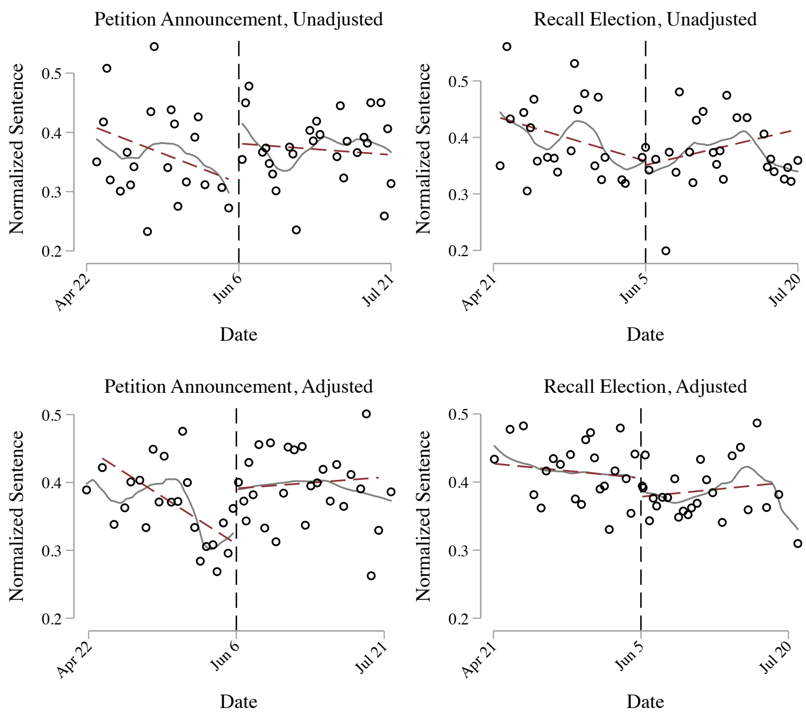
4.1 Main Results: Instantaneous Effects of Critical Events

Graphical Evidence. Before proceeding to our local linear estimation, our first step in assessing the effect of the events described above is graphical. Figure 1 illustrates the main effects at the core of the paper, presenting binned averages of normalized sentence length (sentence length as a fraction of maximum sentence, top-coded at one), within the 90 day window surrounding each event.[20] Linear predictions and local polynomial smoothers are fit separately on either side of the event date.

The two panels in the top row present plots of data unadjusted for covariates. We observe a large, discontinuous increase in average normalized sentence transitioning from

[20]The ± 45 day window approximates the MSE-optimal bandwidth; see below.

Figure 1
Effect on Sentencing of Critical Events in Persky Recall: Graphical Analysis



The left panels depict average normalized sentence lengths (as tokens) in equally-sized bins. The panels to the right depict binned means residualized using judge- and offense-specific fixed effects and the number of counts at conviction. Linear fit (maroon) and local polynomial smoothers (gray) are fit separately on each side of event under consideration.

immediately prior to, to immediately following the June 6, 2016 petition announcement. This increase corresponds to an increase of approximately 10 percentage points on the normalized sentencing scale, reflecting a proportionate increase of approximately 29 percent over pre-announcement levels. Turning to June 5, 2018, the date of the recall election itself, we observe no change in average sentence length from before to after that date.

Plots in the bottom row depict binned means residualized using judge- and offense-specific fixed effects and the number of conviction counts. The graphical analysis of the petition announcement adjusting for the fixed effects reveals a similar pattern to that in the unadjusted panel: an increase of around eight percentage points. Using the adjusted estimates, we again no apparent difference before and after the election itself.

Local Linear Regression Results To interrogate the preliminary inferences suggested by the graphical analysis in a more rigorous fashion, we next present local linear regression estimates of the local average treatment effect (LATE), β_1 from equation (1). The LATE estimates appear in Table 1. Following the recommendation of Gelman and Imbens (2019), we report results from a local linear specification rather than estimating higher-order polynomials (which are susceptible to over-fitting).

Estimates in the table corroborate the results from the graphical analysis. We estimate a large, statistically significant effect of the June 6 petition announcement: unadjusted (first column), the estimated effect is 9 percentage points on the normalized sentencing scale; adjusted for judge- and offense-specific fixed effects (second column), the estimate increases to 10.3 points. To give a sense of the substantive significance of these estimates, immediately prior to the announcement, the estimated average normalized sentence length (the left-side intercept in the Table) was around 0.3; hence, these effects correspond to an immediate proportionate increase of 29.8 percent. (Using the same baseline, the adjusted estimate implies a 33.1 percent increase.) Both RD estimates easily surpass conventional thresholds for statistical significance.

Table 1 Effect on Sentencing of Critical Events in Persky Recall: RD Estimates

	Petition Announced		Recall Election	
RD estimate	0.09	0.103	0.014	0.019
	(0.044)	(0.032)	(0.058)	(0.047)
Left-side intercept	0.303	0.311	0.341	0.357
	(0.03)	(0.023)	(0.049)	(0.039)
Bandwidth	45.1	47.3	40.5	36.8
Adjusted	N	Y	N	Y
Effective observations	1,476	1,289	1,209	954

Dependent variable in each column is the truncated normalized sentence length (see text for description). Estimates in the second and fourth columns adjust for the number of counts and judge- and statute-specific fixed effects, and exclude Sacramento County (which does not report judge identifiers).

The third and fourth columns of the table display the analogous estimates for the recall election date. In contrast to the announcement estimates, the estimated effect, whether adjusted or unadjusted, is small and statistically indistinguishable from zero.

4.2 Prosecutors or Judges?

The next step in our analysis is to assess whether the main finding reflects adjustment by prosecutors rather than judges. We proceed in three steps. First, to minimize the role of idiosyncratic variation in prosecutorial discretion in our central results, we re-ran the main analysis, substituting the statutory maximum sentence for the top count at *arraignment* for its analog at *conviction* as the denominator of the outcome variable, and restricting the sample to cases arraigned before the critical events under study. Results appear in Panel A of Table 2. Given the fact that the maximum sentence at arraignment is weakly larger than the maximum sentence at conviction, it is unsurprising that the left-side intercepts are smaller than the analogous estimate in Table 1. More notable is that the RD estimates associated with the petition announcement are slightly larger. The combination of these effects implies

Table 2 Assessing the Prosecutorial Adjustment Hypothesis: RD Estimates

	Petition Announced		Recall Election	
A. Sentence Normed to Top Arraignment Count				
RD estimate	0.107	0.117	0.04	0.041
	(0.044)	(0.031)	(0.052)	(0.04)
Left-side intercept	0.259	0.268	0.306	0.32
	(0.028)	(0.022)	(0.042)	(0.032)
Bandwidth	39.4	40	38.5	37.2
Adjusted	N	Y	N	Y
Effective observations	1285	1079	1181	966

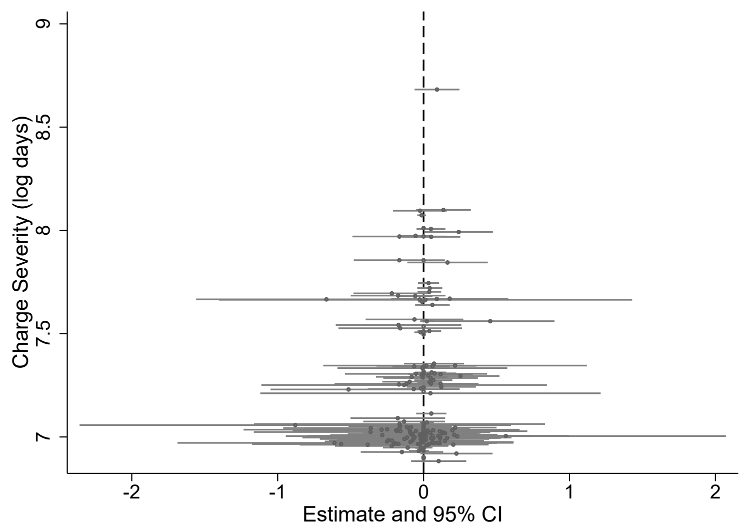
B. Charge Reduction				
RD estimate	-0.022	-0.031	-0.017	-0.023
	(0.027)	(0.019)	(0.023)	(0.022)
Left-side intercept	0.104	0.097	0.064	0.072
	(0.022)	(0.014)	(0.018)	(0.018)
Bandwidth	45.6	45.5	45	38.2
Adjusted	N	Y	N	Y
Effective observations	1268	1088	1209	915

that normalized to the arraignment maximum, the estimates imply a proportionate increase (relative to baseline) on the order of 41.4 to 43.7 percentage points.

Second, we assess the effects of the petition announcement and recall on charge reduction by prosecutors, which is outside of direct control of sitting judges. Panel B of Table 2 reports RD estimates using the measure of charge reduction described above as the outcome. A statistically significant, negative RD estimate would reflect a reduced willingness of prosecutors to drop higher counts as a condition of plea bargaining following the event in question. While the estimates are negative, they are both small and imprecisely estimated. Hence, we cannot reject the null hypothesis that charge reduction practices were unaffected by the petition announcement.

Finally, we look for evidence of any change in the composition of the set of cases around the petition announcement. If, for example, prosecutors expedited convictions for more severe offenses in response to the petition announcement, we might observe a mechanical, positive effect on sentencing severity. To assess balance on the charges' distributions, we present RD estimates for the daily count of each crime against its severity (logged maximum possible sentence in days) in Figure 2. We find no evidence of an imbalance in charge severity that might induce the purported effect.

Figure 2
Charge-FE RD Estimates and 95% Confidence Intervals



Each grey circle (and grey line) represents the RD estimate (and 95% confidence interval) associated with a unique crime's daily count.

4.3 Additional Robustness Checks

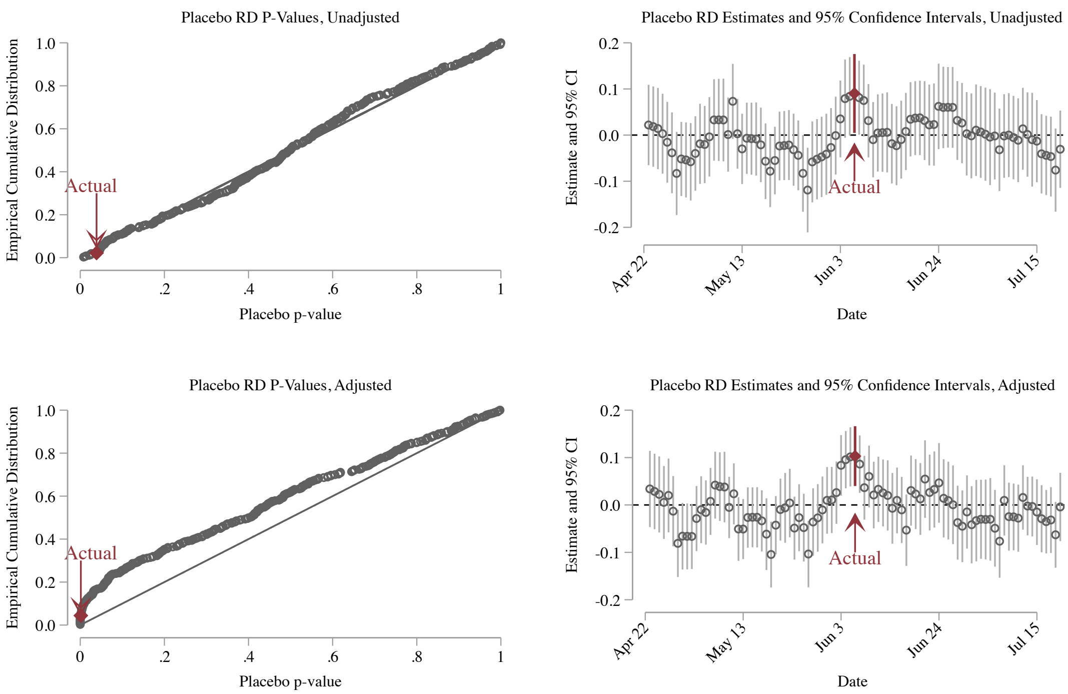
Placebo tests for temporal confounding. Our main analysis implies that the announcement of the recall petition caused a substantial and immediate increase in the length of felony sentences in California. One threat to inference is the possibility that other events may have been taking place around the time of the announcement. An event that is particularly relevant for our analysis is the 2016 California primary, which took place on June 7. A second is Persky's actual sentence of Brock Turner on June 2.

With respect to the primary, there are two immediate responses. First, a Superior Court judge who faced a challenger in 2016 did so initially in a top-two primary, and would only need to face the voters in the general election upon placing in the top two but receiving less than 50% of the vote. Owing to California's unusual electoral rules, the vast majority of judges would thus see the sway of electoral incentives *diminish* following a contested primary or remain roughly constant following an uncontested one. The anticipated behavioral response (given the prior research cited above) would be a reduction in average sentence length; hence, the overall effect would be to bias the above results *downward*. In point of fact, only one incumbent judge in our sample (in San Bernardino County) faced a primary challenge, and she did not hand down a sentence in the sample period.

With respect to the Brock Turner sentence, it is less clear what the direction of the bias might be. It is conceivable that judges, anticipating the electoral backlash from outrage over the sentence, might ratchet up sentencing in their courtrooms in response, and that this anticipation is what our main results are capturing. This would confirm the power of anticipated electoral threat, but complicate our efforts to make inferences about the specific effect of the petition announcement. On the other hand, perhaps the Turner sentence signaled the acceptability of unusual sentences. In the first account, our main estimates are biased upward; in the second; biased downward.

Another threat to inference with which to contend is that there may be numerous structural breaks throughout the calendar year that affect sentencing, some associated with the explicitly political stimuli discussed above and others associated with, inter alia, changes in sentencing guidelines, news accounts of prison overcrowding (or litigation on that issue), or shifts in prosecutorial behavior. The relevant question then becomes whether the shift associated with the June 6, 2016 cutoff was particularly unusual relative to other candidate

Figure 3
Placebo Tests for Main Effect of Petition Announcement



The left panels displays empirical cumulative distributions of estimated placebo p-values (in gray), with the actual petition announcement p-value overlaid in maroon. The right panels displays placebo RD estimates and associated 95% confidence intervals (in gray), with the actual estimate and confidence interval in maroon, for the 90 day period surrounding June 6, 2016.

breakpoints (including the Turner sentence).

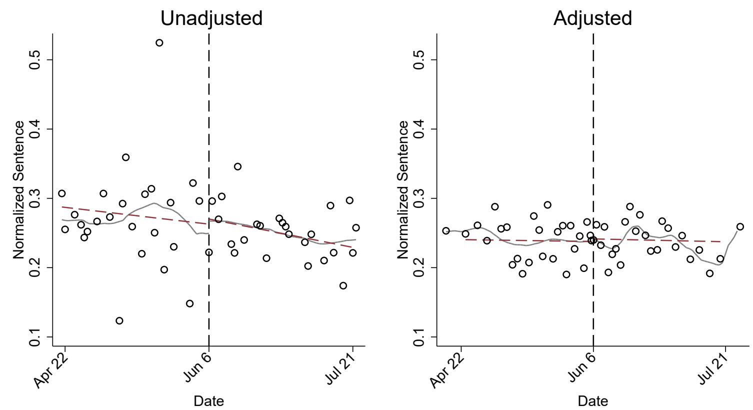
We therefore conducted a sequence of placebo tests, using every day of calendar year 2016 as a breakpoint in (MSE-optimal bandwidth) regression discontinuity analyses. Figure 3 displays the results. The left panels display the empirical cumulative distribution of p-values for the placebo tests, along with the June 6 p-value (labeled “Actual”). For the unadjusted (adjusted) estimates, the figure demonstrates that the p-value for the RDit using June 6 as the breakpoint is smaller than 98% (96%) of its placebo analogs.

The right panels zoom in, for clarity, to the 90 day period surrounding the June 6, and plots 90 placebo estimates plus their associated 95% confidence intervals, along with the June

6 estimate and its confidence interval (again labeled "Actual"). Because they are estimated using nearly identical data, any adjacent RD estimates are very unlikely to be statistically significantly different from each other. That being said, it is instructive that the actual June 6 estimate is larger than any of the surrounding placebo estimates – including June 7 (the primary) and June 2 (the sentence). In fact, the placebo estimates for the date of the Brock Turner sentence are statistically indistinguishable from zero.

Washington as a placebo state. To further assuage concerns that the petition announcement coincided with an unrelated shock to judicial decision-making, we assess shifts in judicial punitiveness in nearby Washington state. Like judges in California, judges in Washington face nonpartisan elections (four year terms) and have broad discretion to issue sentences within the appropriate sentencing guidelines. However, unlike California, the Washington state constitution does not allow for the recall of judges. Using data sourced from the Washington State Department of Corrections, we extracted the sentencing judge, charge and sentence length associated with 67,441 charges. In Figure 4, we present binned averages of the normalized sentence within 45 days of the petition announcement date. Neither the unadjusted averages nor the averages adjusted on judge and charge fixed effects significantly change after the petition announcement date. Local linear regression results confirm the null finding.

Figure 4
Effect on Sentencing of Petition Announcement in Washington State: Placebo Test

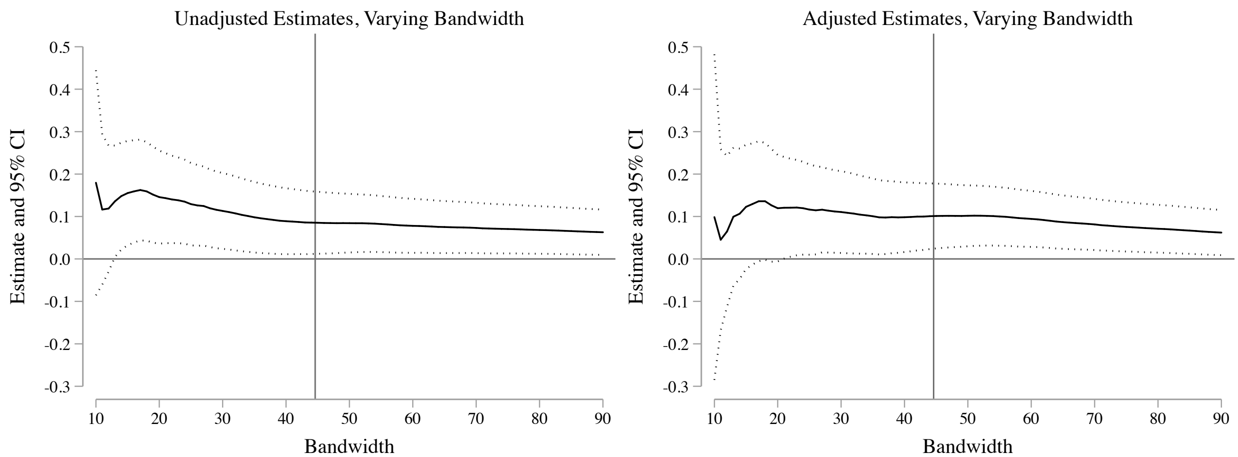


See notes in Figure 1.

Tests for bandwidth artifacts. While the estimates above employ a principled means of selecting the optimal bandwidth for the RD estimates, we wish to make sure that the significance of our results is not overly dependent on the breadth of the interval employed in the analysis. Accordingly, we re-ran our analysis of the effect of the petition announcement for different bandwidths, ranging from one week to 90 days. Results of this exercise appear in Figure 5. For very short bandwidths, of course, the sample size declines dramatically, substantially diminishing the precision of the estimates. However, past around a two-week bandwidth for both adjusted and unadjusted specifications, our main results are robust to a wide range of different bandwidths.

Alternative measures of the outcome. Finally, we consider whether our estimates are influenced by the choice of outcome variable. Table B.I in the Appendix replicates the main analysis in Table 1 using the same normalization but without top-coding at one. This operationalization will pick up increases in judicial punitiveness that result from, e.g., decisions to let sentences for multiple charges run consecutively instead of concurrently. Using this alternative coding leads to slight changes in coefficient magnitudes, but reproduces

Figure 5
RD Estimates Varying Bandwidth



As in the main analysis, estimates employ triangular kernel, with standard errors clustered at the judge-charge level. The solid line denotes the MSE-optimal bandwidth.

the main results: substantial, statistically significant increases in sentencing associated with the petition announcement, and no significant change associated with the recall election.

Table B.II in the Appendix uses the raw sentence (in days) rather than the normalized measure. Here, the offense-specific fixed effects are particularly important, as they pick up mean sentence length for specific charges. Using the non-normalized time scale as the outcome, the substantive import of the findings remains unchanged, with our fixed effects estimates suggesting that the petition announcement had an average (within-charge, within-judge) effect of about 125 days additional incarceration (relative to a baseline of slightly more than a year).

4.4 Effects by Type of Crime

A natural question to consider in assessing the above result concerning the petition announcement is the extent to which it is driven by increases in sentencing for different crimes. To the extent that the precipitating event for the Persky recall was a lenient sentence for

Table 3 Heterogeneous Effects of the Petition Announcement: RD Estimates by Crime Type

	Sex Crimes		Other Violent Crimes		Nonviolent Crimes	
RD estimate	-0.033	-0.003	0.248	0.188	0.077	0.106
	(0.17)	(0.048)	(0.098)	(0.056)	(0.048)	(0.037)
Left-side intercept	0.29	0.448	0.211	0.2	0.318	0.319
	(0.133)	(1.8e-09)	(0.046)	(0.031)	(0.028)	(0.023)
Bandwidth	65.5	33.9	43.8	38.1	46.5	46
Adjusted	N	Y	N	Y	N	Y
Effective observations	85	29	207	151	1,232	1,041

See notes in Table 1 for estimation details.

a violent sex crime, we wish to consider whether the incentive effect of the petition was confined to sex crimes. Accordingly, we partition the sample of felony cases into sex crimes, non-sexual violent crimes, and nonviolent crimes,[21] and run the local linear regression estimator (unadjusted and adjusted for judge and offense fixed effects) separately for each of the three categories. Results appear in Table 3.

Turning first to the analysis of sex crimes (the first and second columns), one immediately notes the very small sample size. This contributes to marked imprecision in the estimated coefficients, especially for the unadjusted estimates. Both estimates are negative and nowhere close to statistical significance. By contrast, we observe highly significant estimates nearly twice the magnitude of the pooled estimates for non-sexual violent crimes (third and fourth columns). For non-violent crimes, RD estimates are in the vicinity of the pooled estimates, but only reach statistical significance in the covariate-adjusted specification (despite the far larger sample of non-violent felony convictions). Taken together, the effects described in the main analysis appear to be driven largely by increases in sentences for non-sexual violent crimes, and possibly for nonviolent crimes. This is consistent with the prediction by critics of the recall effort that any resulting increases in sentencing stringency would not be confined to sex crimes.

[21] We rely on California Penal Code §667.5 to categorize crimes as violent or non-violent.

4.5 The Recall Petition and Disproportionate Burden by Race

We next assess the argument made by critics of the recall effort that notwithstanding the aim of sanctioning a judge for imposing a lenient sentence for a White defendant, any increase in judicial punitiveness driven by the recall itself would likely be disproportionately borne by Black or Hispanic defendants. As discussed above, doing so requires adjudicating between the direct and indirect racial burden hypotheses.

The direct racial burden hypothesis. Two patterns in the data would be consistent with the direct burden mechanism: (1) a strictly more severe instantaneous effect of the petition announcement on normalized sentences for minority defendants than White defendants; or (2) a weakly more severe instantaneous effect for White than minority defendants, and a higher average normalized sentences for minority defendants prior to the announcement. Critically, the second pattern, while consistent with the direct burden hypothesis, would not definitively confirm it. This is because the same pattern would also be expected if there were preexisting racial disparities, and either no racial differences in the effect of the petition announcement, or a larger effect for White defendants (for example, if outrage at the Turner sentence pushed judges to mitigate underlying racial biases in sentencing).

Panel A of Table 4 displays local linear RD estimates of the petition announcement reported separately for Black, Hispanic, and White defendants. The first thing to note is that relative to our main analysis, the effective sample size is considerably smaller, owing to the difficulty matching arrest and sentencing records. Second, coefficient estimates are highest for White defendants, followed by Black and then Hispanic defendants. (In neither specification can we reject the null hypothesis of no effect for Hispanic defendants.) Panel B of the table displays results from a sequence of hypothesis tests comparing the race-specific RD estimates. These tests permit us to reject null hypotheses of no racial differences in each of the covariate-adjusted tests, but none of the unadjusted tests. In other words, the effect of the announcement for white defendants is significantly higher than for Black or Hispanic defendants in the adjusted specification.

That the effects of the petition announcement are apparently largest for White defendants essentially rules out the first pattern consistent with the direct burden hypothesis. To assess the second, we consider the left-side intercepts associated with each RD estimation – these correspond to the expected sentence immediately prior to the announcement for defendants of different races. Hypothesis tests comparing left-side intercepts appear in Panel C of Table 4. Consistent with expectations, the intercept is lower for White than either Black or Hispanic defendants. However, as the test statistics indicate, in no case can we reject the null hypothesis that they are equal across defendant race. Taken together, these results suggest scant evidence for the direct burden hypothesis.

The indirect racial burden hypothesis. The indirect burden hypothesis suggests that comparable effects of the petition announcement across defendant racial categories could obscure a disparate impact that would emerge if minority defendants tend to be charged with more severe crimes (and are thus eligible for higher sentences generally). To assess this possibility, we proceed in two steps. First, we assess whether minority defendants tend to be charged with more severe crimes. Table 5 displays estimates from a regression of a case's *statutory maximum sentence* (in days) – a measure of crime severity – on indicator variables for race (Black and Hispanic – the omitted category is White)[22], adjusting in some specifications for judge-specific fixed effects. (As the primary instrument for manipulating charging severity is the choice of offense itself, we omit statute-specific fixed effects for this portion of our analysis.)

In the full sample, we find descriptive evidence that African American defendants indeed tend to be sentenced to more severe crimes than their white counterparts, with sentences for Hispanic defendants occupying a position intermediate to those of their Black

[22]Only a tiny fraction of defendants in the sample are identified as Asian or Native American in the arrest data.

Table 4 Assessing the Direct Racial Burden Hypothesis: RD Estimates by Defendant Race

A. RD Estimates by Race of Defendant						
	Black		Hispanic		White	
RD estimate	0.136	0.24	0.098	0.062	0.206	0.545
	(0.12)	(0.074)	(0.059)	(0.045)	(0.096)	(0.072)
Left-side intercept	0.355	0.37	0.302	0.31	0.247	0.225
	(0.066)	(0.055)	(0.036)	(0.026)	(0.064)	(0.05)
Bandwidth	64.2	41.6	65.9	54.1	56	26.6
Adjusted	N	Y	N	Y	N	Y
Effective observations	304	136	689	516	326	126
B. Hypothesis Tests of Equality of RD Estimates						
$H_0 : RD_{Black} = RD_{White}$	0.07	0.305				
	(0.153)	(0.103)				
$H_0 : RD_{Black} = RD_{Hispanic}$	0.039	0.178				
	(0.134)	(0.086)				
$H_0 : RD_{Hispanic} = RD_{White}$	0.108	0.483				
	(0.112)	(0.084)				
C. Hypothesis Tests of Equality of Intercepts						
$H_0 : LSI_{Black} = LSI_{White}$	0.108	0.146				
	(0.109)	(0.094)				
$H_0 : LSI_{Black} = LSI_{Hispanic}$	0.052	0.061				
	(0.091)	(0.075)				
$H_0 : LSI_{Hispanic} = LSI_{White}$	0.056	0.085				
	(0.085)	(0.073)				

See notes in Table 1 for estimation details. LSI is the left-side intercept, i.e., the value of the regression function estimated using data prior to the petition announcement at the date of the announcement.

Table 5 Assessing Indirect Racial Burden: Racial Disparities in Crime Severity, as Measured by Statutory Maximum Penalties

	Pre-Announcement		Post-Announcement		Full Sample	
Black	73.0	60.5	79.4	80.0	78.1	76.2
	(14.3)	(56.6)	(21.1)	(16.0)	(19.0)	(15.7)
Hispanic	48.5	5.2	44.4	21.6	44.9	19.7
	(38.6)	(21.1)	(15.7)	(12.8)	(18.4)	(12.6)
Intercept	1300.0	1338.8	1283.4	1302.0	1286.2	1307.1
	(29.6)	(15.3)	(12.9)	(7.8)	(15.2)	(7.6)
Judge fixed effects	N	Y	N	Y	N	Y
N	1,805	1,155	9,343	7,304	11,148	8,459

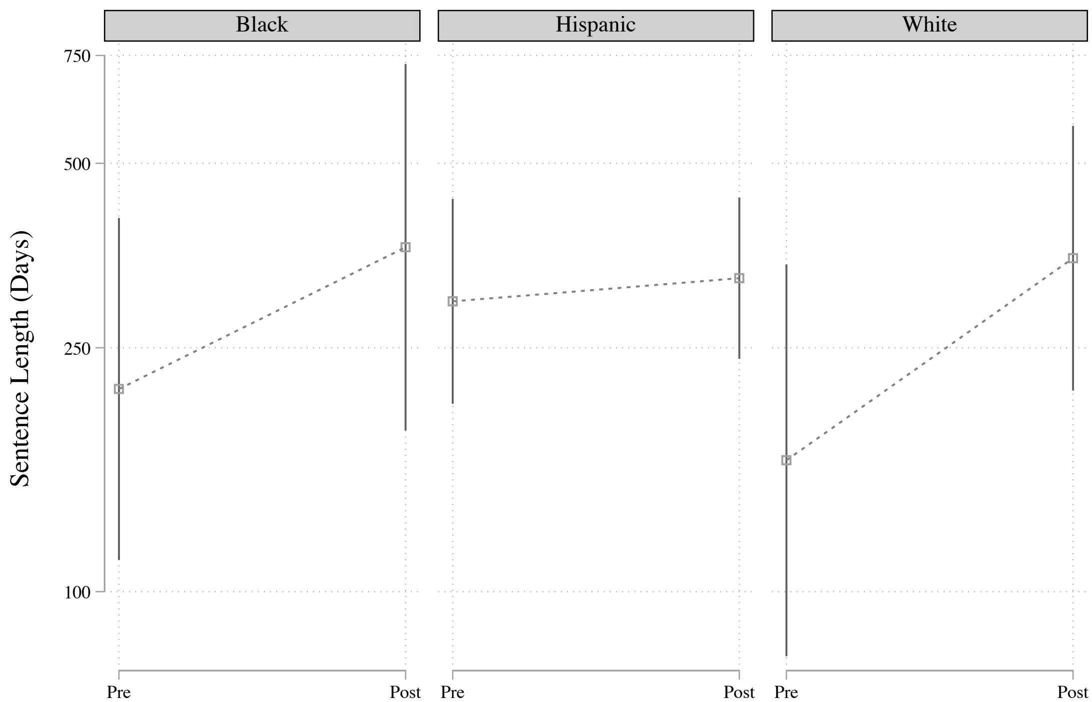
The dependent variable in each column is the statutory maximum sentence (in days) associated with each charge. The excluded category is white defendants. Standard errors are clustered at the county (Columns 1, 3, and 5) or judge level (Columns 2, 4, and 6).

and White counterparts. This pattern holds when we partition the data into pre- and post-announcement periods, although the statistical significance of the results is attenuated for the pre-announcement period.

Next, recall that our earlier analysis suggested that the effect of the petition announcement on sentencing was larger for White than Black or Hispanic defendants (although the statistical significance of that disparity differed by specification). Thus, in evaluating the indirect burden hypothesis we are interested in assessing whether the apparently larger effect for Whites attenuated or reversed the underlying disproportionate burden for minority defendants. To assess this, we ran our unadjusted RD estimator, disaggregated by racial category, with the (logged) sentence length as the outcome variable. This will capture the total effect, by race, of the petition announcement on average sentencing.

Figure 6 displays the left- and right-side intercepts (and associated 95% confidence intervals) from this analysis for Black, Hispanic, and White defendants. The figure implies that we cannot reject the null hypothesis of no differences in sentence length *immediately* before, or after, the petition announcement. Coupled with the descriptive results in Table 5, which suggest that the disparities do exist in the time periods both before and after the announcement, the results imply that the apparently larger effect of the petition announcement on judicial sentencing for White defendants documented in Table 4 neither mitigated, nor exacerbated, longer-term racial disparities in sentencing.

Figure 6
Assessing Indirect Racial Burden:
Effect of Petition Announcement on Sentence Length, by Race



Each panel displays left- and right-side intercepts (and associated 95% confidence intervals) from a MSE-optimal bandwidth regression discontinuity in time around the petition announcement.

4.6 Aggregate Effects

An advantage of the regression discontinuity in time approach is that it precisely identifies a local average treatment effect at the time of the critical event under consideration under relatively weak assumptions. However, insofar as effects are only identified at the boundary, interpreting their broader substantive implications requires additional assumptions. In the current application, the most relevant consideration – both in terms of cost to defendants and cost to the state of California – is a counterfactual one: how does the shift in judicial behavior following the petition announcement translate into additional days, months, or years of additional prison time? Rather than commit ourselves to one set of assumptions, in this section we adopt four alternative approaches. Likewise, rather than extrapolate over a prolonged period of time (in which, per our placebo tests above, a sequence of additional factors not pertaining to the Persky recall may have affected judicial punitiveness), we restrict ourselves to the 45 day window following the petition announcement (with the 45 day length approximating the optimal bandwidth from the RD estimates above). The first approach is to assume that the identified local average treatment effect is the average treatment effect over the 45 day window. This approach assumes no decay or growth in the effect of the announcement on sentencing considerations. We proceed by multiplying the LATE estimate for the increase in raw sentence days, expressed as a percentages of a case's statutory maximum by the total number of cases in the 45 day window. We report results using the unadjusted LATE estimate and the estimate adjusted for judge- and offense-specific fixed effects.

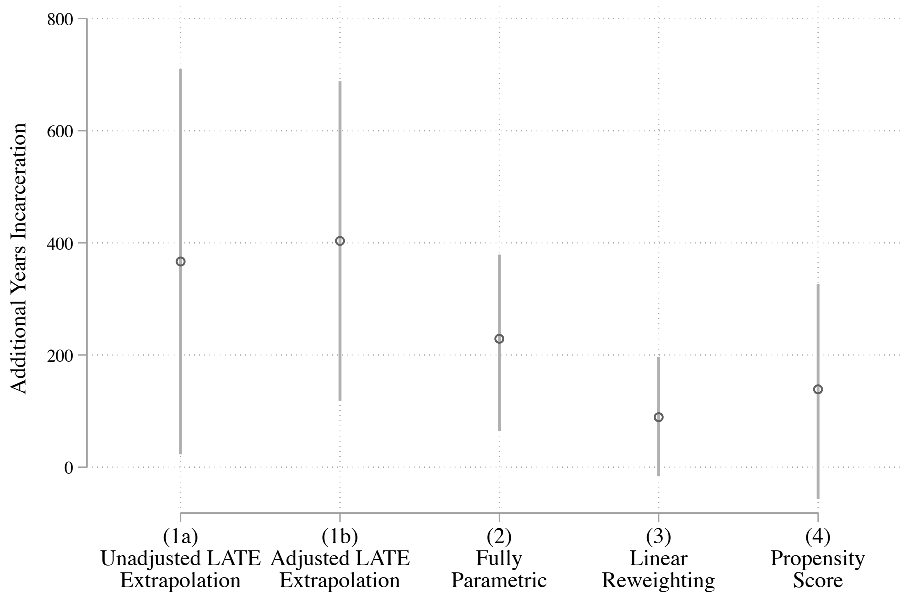
The second approach is to estimate a fully parametric regression model that adjusts for time trends before and after the announcement, and use the predicted values from that model to estimate the aggregate effect over the 45 day window. This approach may capture growth or decay in the effect over the interval following the announcement; however, it may also erroneously attribute factors unrelated to the announcement to the announcement itself. In order to protect against the possibility that downward pre-announcement trends might artificially inflate anticipated sentencing differences, we constrain the trend to zero when predicting counterfactual sentences.

The third and fourth approaches employ estimators recommended by Angrist and Rokkanen (2015), which rely on a critical feature of the regression discontinuity design: that failure to control for the running variable (time) is the only source of omitted variables bias. Their approach to estimating average treatment effects away from the boundary is to test whether, conditioning on covariates, a relationship exists between the outcome and running variables; and if not, to estimate average treatment effects for an interval using either a linear reweighting or propensity score estimator (see Angrist and Rokkanen (2015) for details). We report results using both approaches.

Figure 7 displays the estimated additional incarceration (in years) for the counties in our sample using the approaches described above. Depending on the approach, point estimates suggest total effects of between 88 and 403 years additional incarceration associated with the announcement. The larger figure comes from the adjusted LATE specification, and the smallest from the linear reweighting estimator. Note that the smaller estimates discard observations for which we lack covariate overlap pre- and post-treatment (e.g., sentences from the same judge both before and after the announcement), and are likely to be biased downward.

While the human cost of this estimate on defendants is difficult to calculate without very strong assumptions, a far easier calculation is the total cost to the state: In 2016-17, the average annual cost of incarceration in the California Department of Corrections was $71 thousand per inmate. Using the most conservative 88 year estimate, our analysis suggests a total cost to the six counties in our sample of $6.25 million. Note also that defendants from the counties in our sample make up just 12% of the incarcerated population in the state. Under fairly restrictive assumptions (most importantly, that the distribution of charges and the effect of the petition announcement are both uniform across the state) a back-of-the-envelope calculation using the most conservative estimate suggests that the total effect statewide is 733 years, reflecting a total cost to the state of $52.1 million. If the effect of the petition announcement persisted longer than the 45 day window under consideration,

Figure 7
Estimating Aggregate Effects of the Petition Announcement: 45 Day Window



Aggregate effect estimates in (1a) and (1b) assume the LATE is the ATE. (1a) reports the unadjusted ATE and (1b) reports the ATE adjusted for judge- and offense-specific fixed effects. Estimate (2) is the fully parametric aggregate effect, allowing for time trends before and after the announcement date and adjusting for judge- and offense-specific fixed effects. Estimates (3) and (4) employ two conditional independence assumption based estimators - a linear reweighting and a propensity score estimator respectively. Confidence intervals for (2), (3), and (4) are derived using the nonparametric bootstrap.

actual costs could be considerably higher.

5 Discussion

In 2016, a Superior Court judge in California had not been recalled from office by voters in 84 years. But in the summer of that year, days after a Santa Clara County judge handed down a widely-publicized lenient sentence to an affluent, white defendant for a sexual assault and attempted rape conviction, an unanticipated recall campaign against that judge raised the threat of potential electoral sanctions for other judges.

The research presented in this paper documents the far-reaching consequences of that threat for the criminal justice system. Using data from six California counties, we observe large, instantaneous increases in judicial punitiveness immediately following the announcement of the recall campaign, which are most readily apparent in sentencing for non-sexual violent crime. While we uncover no evidence that these instantaneous effects were larger for minority than White defendants, we demonstrate that the petition announcement neither mitigated nor exacerbated observed longer-term racial disparities in sentencing.

The broader import of these findings – for our understanding of the criminal justice system in the United States and our understanding of electoral accountability – is twofold. First, they underscore the fact that even political campaigns targeting individual officeholders may have broad, unintended consequences. This is because such campaigns do not operate in a vacuum, and thus may alter the expectations of other officeholders that they themselves might be subject to such campaigns. The fact that we document no observable effects of the eventual recall election itself is consistent with this shift in beliefs to a "new normal" in the political environment of sitting trial judges, about which the ultimate (and widely-anticipated) electoral outcome conveyed no additional information. And critically, although the defendant in the precipitating case was White, and the crimes for which he was convicted were sex crimes, the use of the recall tool cannot be restricted to similar cases. And as such, neither can any anticipatory responses to that threat by judges in their courtrooms.

Second, the research presented contributes to our understanding of the electoral incentives of public officials. We provide the first empirical evidence that the threat of recall affects the behavior of incumbent officials. In the current context, we provide evidence that an exogenous shock to judges' beliefs in the risk of recall affected their sentencing decisions. We document a substantial and immediate increase in sentencing severity following the highly-publicized announcement of a recall campaign, and calculate aggregate effects of that increase on the order of 88 years of additional incarceration for around 600 defendants in the 45 day period following the announcement. Insofar as we restrict our attention to a narrow window of time and only six counties, these estimates likely substantially underestimate the broader effects of this change in the behavior of these officials.

Finally, our analysis provides a roadmap for studying non-standard electoral institutions whose structure does not lend itself to standard research designs that exploit, inter alia, proximity to the next election, cross-sectional institutional variation, or term limits. This is particularly valuable for an institution such as the recall, which, although widespread, is poorly understood. In the same vein, understanding the scope of incentive effects of recall efforts that vary in their intensity, and the political responses of incumbent officials,[23] is an important topic for future research.

[23]Consider for example, a group of Californian judges who launched the Judicial Fairness Coalition shortly after the Persky recall (`https://www.caljudges.org/CommFairness.asp`), in part to provide resources for judicial officers facing potential recall threats.

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A Descriptive Statistics

Table A.I Descriptive Statistics

	All		Pre-&Post-Petition Announcement		Pre-&Post-Recall Election	
	Mean	SD	Mean	SD	Mean	SD
	Sentencing Characteristics					
Sentence Length (days)	559.165	1091.419	496.503	544.095	587.403	1039.722
Uncensored Normalized Sentence	0.417	0.764	0.385	0.383	0.415	0.423
Normalized Sentence	0.377	0.332	0.367	0.322	0.384	0.334
	Charge Characteristics					
Sex Crime	0.043	0.202	0.044	0.206	0.045	0.207
Violent Non-sex Crime	0.142	0.349	0.146	0.353	0.148	0.355
Non-violent Crime	0.827	0.378	0.817	0.387	0.822	0.383
	Defendant Characteristics					
Black	0.214	0.410	0.198	0.398	0.229	0.420
Hispanic	0.418	0.493	0.493	0.500	0.408	0.492
White	0.331	0.470	0.273	0.445	0.321	0.467
Male	0.831	0.375	0.861	0.346	0.837	0.369
Age	36.602	10.976	36.306	10.625	35.389	11.140
Num. Cases	19,832	19,832	1,476	1,476	1,383	1,383
Num. Defendants	18,293	18,293	1,434	1,434	1,342	1,342

This table presents summary statistics of our disposition data overall and within bandwidths relevant for the judicial recall campaign. Columns 3-4 report statistics for the sample restricted to within 45 days of the petition announcement date; Columns 5-6 report the corresponding values with respect to the recall election date.

B Additional Results

B.1 Uncensored Normalized Sentences

Table B.I Replication of Main Analysis Using Uncensored Normalized Sentences as Outcome

	Petition Announced		Recall Election	
RD estimate	0.137	0.162	-0.006	-0.011
	(0.058)	(0.052)	(0.068)	(0.051)
Left-side intercept	0.326	0.312	0.372	0.37
	(0.029)	(0.027)	(0.058)	(0.044)
Bandwidth	54.5	43.8	41.8	36.4
Judge fixed effects	N	Y	N	Y
Statute fixed effects	N	Y	N	Y
Effective observations	1533	1078	1176	919

The dependent variable in each column is the uncensored normalized sentence length (sentence length as a fraction of statutory maximum sentence). See notes in Table 1 for estimation details.

B.2 Non-Normalized Sentence Length

Table B.II Replication of Main Analysis Using Non-Normalized Sentence Length as Outcome

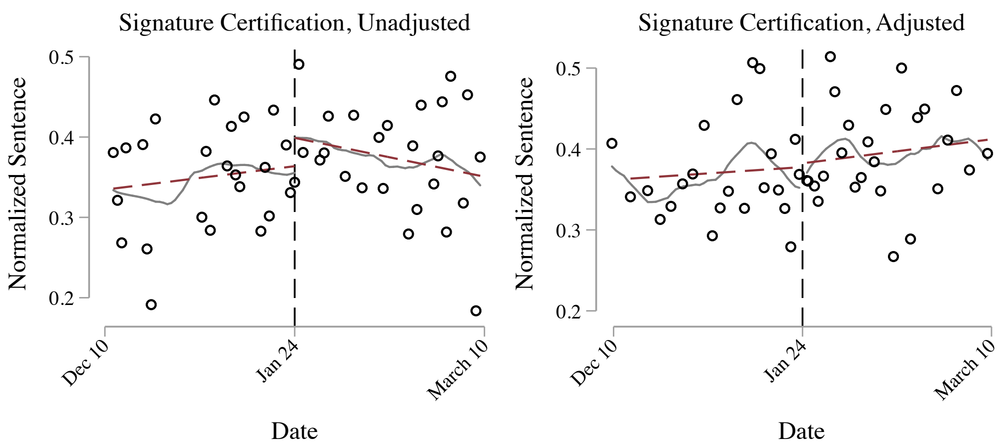
	Petition Announced		Recall Election	
RD estimate	116.704	124.853	-60.407	-29.494
	(58.497)	(45.845)	(155.625)	(66.836)
Left-side intercept	387.113	374.297	589.261	583.316
	(39.902)	(31.134)	(135.467)	(57.366)
Bandwidth	62.3	50.2	51.1	38.9
Judge fixed effects	N	Y	N	Y
Statute fixed effects	N	Y	N	Y
Effective observations	1887	1244	1441	900

The dependent variable in each column is the sentence length (in days). See notes in Table 1 for estimation details.

B.3 Signature Certification Date

Figure B.I

Effect on Sentencing of Signature Certification Date in Persky Recall: Graphical Analysis



The left panel depicts average normalized sentence lengths (as tokens) in equally-sized bins. The panel to the right depicts binned means residualized using judge- and offense-specific fixed effects. Quadratic curve (maroon) and local polynomial smoothers (gray) are fit separately on each side of the signature certification date.