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Criminal Justice Reform: Voting, Policing, and Public Attitudes

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Criminal Justice Reform: Voting, Policing, and Public Attitudes

by

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University of California, Berkeley

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Professor Gabriel Lenz

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Abstract

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This dissertation examines the political dynamics of criminal justice reform in the United States, focusing on the interplay between voters, elected prosecutors, and police departments. It introduces a novel framework for understanding voter preferences along extensive (scope) and intensive (severity) margins of criminal justice policy. Through a case study of a reform prosecutor recall election, the research demonstrates how electoral accountability hinges on accurately gauging public attitudes along these margins. The study also reveals police behavior shifts in response to prosecutor elections, evidencing a slowdown during reform efforts and increased activity post-recall. Additionally, it explores the role of racial and political group dispositions in shaping voters' criminal justice policy preferences, highlighting the influence of Black voter cues and racial attitudes. By illuminating the complex incentives and motivations of key stakeholders, this dissertation offers critical insights for aligning institutional behavior with criminal justice reform goals, contributing to both theoretical understanding and practical policy implementation.

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Completing a PhD happens only once (usually), which puts it in the special category of events that deserve intentional reflection. I would never have the courage to aim this high if it were not for my forever partner, Michal. Thank you for teaching and showing me that everything is possible; everything I do is thanks to your love. This also would not have been possible without my parents, who made me who I am. It is impossible to list all the colleagues and mentors who shaped the way I work. Every single person I met (in real life, via Zoom, or through readings) while working to get the PhD was transformative in shaping how I think. Finally, while working toward the PhD I had the privilege of achieving something more significant than an academic degree. It has been a period of growth not just intellectually but emotionally. Moving to the other side of the world and finding my tiny place in Academia allowed me, surprisingly, to simultaneously open my heart to friendships that are part of this once-in-a-lifetime milestone. My friend Kaitlin, Jedi-Master Nate, big-brain-big-heart Ashley, and heroic Daniel - thank you.

Contents

List of Figures

List of Tables

Chapter 1

Introduction

The institutions that govern criminal justice policy in the U.S. are political. This dissertation argues that criminal justice reform depends on understanding these institutions' incentives and aligning them with criminal justice reform goals. Specifically, the interactions between voters, elected prosecutors, and police departments' motivations reveal obstacles to reform. Each has different motivations and incentives shaping their political behavior. Elected prosecutors must win voters' trust by remaining responsive to their constituents' preferences. Police departments are incentivized to resist increased scrutiny, a core aspect of reform efforts. Voters are constructing attitudes along the extensive and intensive margins of criminal justice policy while considering matters of safety (through retribution and deterrence concerns), together with racial and social justice.

The second chapter introduces the theory of extensive and intensive margin of criminal justice and argues that voters' preferences are best understood along this axis. The extensive margin explains how many behaviors should be governed through the criminal legal system; the scope of criminal justice. The intensive margin explains how should the criminal legal system treat convicted individuals; the severity of of the criminal justice. Using a case study of a reform prosecutor recall election, the chapter demonstrates that achieving electoral accountability depends on correctly understanding public attitudes. I show that the correct response to voters requires attention to legal reform's intensive and extensive margins. Despite the media narratives, I argue that voters favor reforming the intensity of the criminal legal system; voters support reducing outcomes' harshness but not limiting the scope of prosecuted behavior. This research also indicates that moral concerns drive support for decreasing the intensive margin, while opposition to changing the extensive margin is rooted in the desire to maintain deterrence. Politicians who intend to end Mass Incarceration should focus on reducing the criminal legal system's intensive margin to gain political approval.

The third chapter examines another aspect of this reform prosecutor recall election - police behavior. It uses administrative data on police behavior to investigate how policing is affected by prosecutor elections. It explores the effect of a District Attorney recall election on policing patterns following a contentious relationship between the prosecutor's office and the corresponding police department. With policing, prosecution, and jail data from San

Francisco, we use an Interrupted Time Series (ITS) estimator with a kink at the recall election date to study how local prosecutor politics affect police behavior. We find that before the recall election of an "unfriendly DA," the police decreased its activity. Immediately following the successful recall, the police ramped up their efforts, increasing the local jail population. Our findings illustrate the importance of accounting for police departments' responses to prosecutorial politics when considering the effects of prosecutor policies on crime.

Finally, the fourth chapter examines the political psychology of voters' criminal justice policy preferences. It argues that voters' dispositions toward racial and political groups affect their criminal justice policy preferences. It develops a theory of racial and criminal justice cognitive relatedness to explain why people support different policy responses to crime. This study investigates the factors shaping public attitudes toward criminal justice policy reform, focusing on dispositional racial attitudes and political and racial group cues. Employing a conjoint design and a follow-up survey experiment, I demonstrate that people's dispositions toward racial and political groups affect their criminal justice policy preferences. Both people of color and white respondents follow cues from Black voters, with racial attitudes playing an important moderating role. Furthermore, I find little evidence for partisan cues' influence on support for reform. These findings have fundamental implications for political activists and their efforts to support criminal justice reform campaigns.

Taken together, this dissertation contributes to theories of criminal justice politics; achieving accountability relies on properly characterizing public opinion, which is both affected by policing and prosecutor practices and affects criminal justice institutions.

Chapter 2

Extensive and Intensive Margins of Criminal Justice Policy

1 Introduction[1]

The political landscape of the U.S. criminal legal system is shaped by a complex array of local institutions, highlighting the potential for electoral accountability. Elected officials, including mayors, sheriffs, and prosecutors, govern key aspects of law enforcement and state prosecution along county lines. Citizens' ability to hold these officials accountable can enhance policy congruence. Generally, policy responsiveness to public attitudes was shown to track changes in punitive sentiment, not nuanced policy preferences (Enns 2016); sentiment impacts policy through lawmakers anticipating the types of policies—rather than specific policies the public prefers (Bartels and Stimson 1992; Stimson 2004; Stimson, Mackuen, and Erikson 1995). Politicians adopted a tough-on-crime stance through rational anticipation of certain electorate sentiments (Beckett 1999) and as the outcome of pressure at the national and state level from law-and-order interest groups and victims' movements (Lisa L Miller 2008; Gottschalk 2006). Arguably, policymakers were over-responsive to the least affected and under-responsive to the most affected groups (Duxbury 2020; Duxbury 2021; Lerman and V. M. Weaver 2020; Soss and V. Weaver 2017).

This article expands the traditional punitive-progressive divide using a theoretical framework explaining voter preferences. The theory posits two aspects of criminal justice policy: extensive and intensive margins. The extensive margin refers to the scope and reach of government intervention, encompassing the range of behaviors criminalized and the number of individuals subject to legal prosecution and penal control. The intensive margin relates to the depth or severity of government intervention, measuring the harshness of penalties, the length of sentences, and the overall rigor of punishment imposed. Using this framework, we understand punitive sentiment as heightened margins and progressive alternatives as efforts to lower the extensive and intensive margins of the criminal legal system. Further, per the extant literature, this article argues that voters affect policy by sending a message on their policy sentiment (understood here through a position on the extensive and intensive margins), not specific policy preferences.

This article applies the theoretical framework to prosecutor elections, using the recall of the San Francisco District Attorney (DA) as a case study. This recall highlights how voters' preferences on the scope and severity of criminal justice interventions influence their voting behavior. I argue that this novel theoretical framework explains how voters signaled their support for extensive prosecution of wrongdoing while preferring a reduction in the intensity of punitive responses. I demonstrate that the prevailing interpretation of the recall election result as a 'reversion to tough-on-crime' is erroneous. Instead, it exposes an accountability failure, revealing the recall's inability to address citizens' concerns due to a misperception of 'public support for a public policy or person' (Berinsky and Lenz 2014).

This article utilizes multiple data sources to substantiate the theory and explain the highly publicized prosecutor recall election. Beyond common survey methods, it presents novel 'digital exit poll' data on verified recall voters' motivations during San Francisco's DA

[1] Thanks to Rebekah Jones and participants of Lenz-Broockman American Politics Lab for help in developing and executing this project.

recall election (June 2022). I show that about one in three 'progressive' voters voted to recall the progressive DA.[1] A survey experiment demonstrated that these 'conflicted progressives' prefer reducing the intensity but not the extent of prosecuted behavior. Additional surveys with national and California samples show that voters distinguish between the two theoretical constructs regardless of the recall setting, suggesting the results can be generalized. The article concludes that a progressive candidate losing an election does not indicate voters turning their backs on criminal justice reform. This theoretical contribution advances the field's understanding of crime and justice politics by unpacking the broad 'progressive' and 'punitive' political labels into two precisely defined political attitudes: intensity and extent in the politics of crime.

2 Electoral Accountability in the Criminal Legal System

American criminal justice is fragmented and extremely decentralized. Stuntz describes it as a "vertical allocation of power" in which "local governments do most criminal law enforcement" (Stuntz 2006, p. 786). There are 3,000 counties in the United States; hence around 3,000 jails, 3,000 juvenile facilities, about 3,000 county court systems, 3,000 adult probation agencies, and 3,000 juvenile probation agencies. Moreover, there are 17,985 police agencies in the United States, including city police departments, county sheriff's offices, state police/highway patrol, and federal law enforcement agencies. Many of these institutions are governed by elected officials. DAs (the chief law enforcement officer of the community) are elected per county in most jurisdictions, as are sheriffs and judges (Brace and Boyea 2008; Sklansky 2018; R. F. Wright 2008). Mayors control the budgets of police and social-response units. This is a unique phenomenon, as most law enforcement outside the US is linked to the electorate through delegation, not election.

Electoral accountability should flourish when many officials are directly elected. Yet, until recently, responsiveness to marginalized communities has been arguably distorted (Forman Jr 2017). Police responsiveness to the needs of community members was distorted, particularly in communities that are highly policed (Prowse, V. M. Weaver, and Meares 2020; Soss and V. Weaver 2017; V. M. Weaver 2007). County sheriffs have been found to manipulate policy during election years (Su and Buerger 2024). Trial courts' response to judicial elections is associated with more punitive judicial behavior (Taylor 2021; Gordon and G. Huber 2007). Further, elections for state supreme courts are associated with an increased effect of public opinion death penalty preferences on judges' decision-making (Brace and Boyea 2008; Canes-Wrone, Clark, and Kelly 2014). On the other hand, Nelson (2014) argued that prosecutors and judges respond correctly to votes on a marijuana legalization initiative. In the wake of a new wave of competitive prosecutor elections, the politics of crime and justice has

[1] Voters opposing the progressive agenda were vastly more uniform in their voting decisions, about 90% supporting the recall.

an opportunity to repair its responsiveness to citizens (Davis 2019). This requires a new understanding of how the public forms criminal justice attitudes.

This article contends that citizens' preferences are complex; to understand them, broadening the theoretical approach to the punitive-progressive divide is beneficial. Importantly, people distinguish between enforcement and prosecution (the extensive margin) and the severity of punishment (the intensive margin). This observation adds theoretical content to the useful yet barely understood constructs of "punitive" and "progressive:" The attitudes along the margins map into different forms of punitive-progressive sentiment. Next, I explain the theory of intensive and extensive margins in the criminal legal system and argue that it is necessary for achieving electoral accountability.

Extensive and Intensive Margins

Policymaking entails deciding whether the government should intervene, known as the extensive margin, which covers the scope and reach of policies, and determining the degree of intervention, referred to as the intensive margin, which focuses on the depth or severity of these policies. The public has mixed attitudes toward the preferred extent of government intervention (Pew Research Center 2019). For example, the history of Medicare and Medicaid demonstrates the tension in balancing the extent of government intervention (Ruggie 1992). Differentiation along the extensive and intensive margins is essential in understanding the nuanced impacts of policymaking, as changes in either margin can lead to markedly different outcomes (Hetherington 2005; Peyton 2020). Yet, theories of crime control politics focus mostly on the punitive dimension, primarily from measurement and historical perspectives (Enns 2016; King and Maruna 2009; V. M. Weaver 2007; Adriaenssen and Aertsen 2015); understanding attitudes along the extensive and intensive margins can provide a better understanding of both punitive attitudes and progressive reform preferences.

The American criminal justice system is both extensive in scope and intense in severity. In recent decades, the United States has been defined by a growing percentage of the population interacting with the government through penal policies and by a government that uses disciplinary policies to achieve political goals (Simon 2007; Lerman and V. M. Weaver 2020). In terms of extent, the United States responded to crime by extending penal policies without corresponding social policies (Lisa Lynn Miller 2016). The carceral state emerges when, for a non-negligible segment of the population, repressive policies become extensive such that they shape political identity, action, and thought. Increased contact with the criminal system has decreased political participation and civic engagement, undermining citizenship (Burch 2011; Burch 2013; Lee, L. C. Porter, and Comfort 2014; White 2019; Owens and Walker 2018; Prowse, V. M. Weaver, and Meares 2020).

The American criminal justice system's extensive scope is accompanied by excessive intensity, leading the United States to become an outlier in global terms of incarceration rates. Increasing the intensity of criminal justice outcomes shifted policies to impose harsher sentences more frequently and for lesser offenses, which has dramatically increased the prison population. Such extensive and intensive approaches have significant implications for public perception of justice and the state's role in individual lives (Lerman and V. M. Weaver 2020;

Walker 2020). Research has consistently found that "changes in policy and practice (rather than rising crime rates) are the proximate drivers of the prison boom." (Beckett and Francis (2020); see also Western, Lopoo, and Pettit (2006) and Murakawa (2014)). Raphael and Stoll (2009) and Raphael and Stoll (2013) similarly estimate that 80% to 85% of the growth in US prisons can be attributed to sentencing law.

In response, politicians and policy activists work to shift policy in the other direction (Goodman, Page, and Phelps 2017). In recent years, most states have successfully enacted reforms to reduce the *intensive* margin. Some examples include expanding release from prison during the COVID-19 pandemic, restricting the length of probation and parole supervision, and repealing the death sentence (N. D. Porter 2021). In the context of police reform, Vaughn, Kyle Peyton, and G. A. Huber (2022) finds that the public generally supports reforming police action but resists reducing the extensive margin - *minimizing* police action. Support declines substantially when the slogans "defund" or "abolish" are presented; public support for police reform depends on perceptions of reform's effect on the level of police intervention.

To support this article's main contribution - that we can better understand the concepts of "punitive" and "progressive" sentiments using the extensive and intensive margins framework - I collected data from a representative sample of California voters to provide initial evidence for the theoretical framework. This survey explored whether voters' perceptions align with the distinction between criminal justice policy's intensive and extensive margins, as outlined above. Specifically, the survey assessed voters' attitudes towards different approaches to public safety, which reflect varying levels of government intervention along these two margins.[2] I asked voters to choose "Which of the following four statements about how the government should approach public safety comes closest to your views?" and provided the following options:

Option 1 ('Get Tough') reflects a preference for increasing both the extensive and intensive margins, while Option 4 ('Less extensive and less intensive') suggests a preference for reducing both. According to the theory, the distribution of attitudes should include significant differences between voters who choose options 2, 3, and 4, which represent different attitudes toward the combination of reforming the intensive and extensive margins. The data collected from California voters illustrates the practical application of the extensive and intensive margins framework and offers a deeper understanding of public attitudes toward criminal justice reform. Indeed, the distribution of responses across these categories is not random ($\chi^2(2) = 122.29, p < .001$). Further, when testing for whether the distribution between the two marginal options 2 and 3 (different reform for different types of margin) was random, we discovered it was not ($\chi^2(2) = 101.58, p < .001$). Voters showed a significant preference for Option 2 (18%) over Option 3 (11%) ($t = -10.995, p < .001$), indicating

[2] The poll was administered online in English and Spanish, between October 25 - October 31, 2023, among 6,342 California registered voters. Email invitations were distributed to stratified random samples of the state's registered voters. Samples of registered voters with email addresses derived from information on the official voter registration rolls. Before the distribution of emails, the overall sample was stratified by age and gender. To protect the anonymity of respondents, voters' email addresses and all other personally identifiable information were purged from the data file.

1 "Get Tough"	2 "More extensive, less intensive"	3 "Less extensive, more intensive"	4 "Less extensive and less intensive"
The government should prosecute MORE people it thinks committed crimes and give those convicted LONGER prison sentences than it does today.	The government should prosecute MORE people it thinks committed crimes and give those convicted SHORTER prison sentences than it does today.	The government should prosecute FEWER people it thinks committed crimes, but give those convicted LONGER prison sentences than it does today	The government should prosecute FEWER people it thinks committed crimes, but give those convicted SHORTER prison sentences than it does today

Table 1: Options for Government Policy

a subtle perception of the criminal justice system, where increasing the extensive margin (more prosecutions) does not necessarily equate to increasing the intensive margin (length of sentences).[3] This finding aligns with the theoretical distinction between these two margins.

3 Background - District Attorney Politics

This article applies the theoretical framework of extensive and intensive margins in the criminal legal system to the politics of DA elections as a first step in defining this expansion of the traditional punitive-progressive divide. American prosecutors represent local jurisdictions and enjoy independence and discretionary power unmatched worldwide (Sklansky 2018; Tonry 2012; J. Pfaff 2017). Yet, DAs can be held accountable through election; voters are expected to support a DA in an election based on their attitudes toward crime and punishment and success at trials (Sung 2022; Gordon and G. A. Huber 2002). J. Pfaff (2017) argued that prosecutors' effectiveness is perceived as a product of their ability to secure charges; they benefit politically from a high conviction rate (a high extensive margin).

During the rise of Mass Incarceration, the prosecutor's power has expanded at the expense of judges and defense attorneys (Simon 2007). The institutional structure and incentives faced by prosecutors contributed significantly to their harsh approach and, accordingly, to Mass Incarceration (J. F. Pfaff 2012; J. Pfaff 2017). Prosecutors' immense discretionary power made it possible to pursue harsh sentences partly for political benefit. Sances (2021) found that in California, between 2012 and 2016, DAs adopted a traditionally "get-tough" approach regardless of their constituents' revealed preferences. A conjoint experiment found an effect of voters' policy positions on their prosecutor preferences (Sung 2022). Other studies

[3] See full proportions in the Supplementary Information, Section S6.

found that DAs are more punitive in an election year (Bandyopadhyay and Mccannon 2014; Dyke 2007; Nadel, Scaggs, and Bales 2017; Okafor 2021).

It used to be common wisdom that prosecutor elections are apolitical: rarely contested (J. Pfaff 2017; R. F. Wright 2014; Bibas 2016), and incumbents "win until they quit" (Bazelon 2020, p. 80). However, in a recent study of prosecutor elections in 200 high-population districts in the US between 2012 and 2020, Wright, Yates, and Hessick (2021) find that the likelihood that an incumbent would run unopposed "fell by roughly eight percent for each passing year." (Wright, Yates, and Hessick 2021). They show that the disappearance of uncontested elections is prevalent but "applied most strongly to non-white incumbents, who were most likely to attract opponents in primary elections" and win fewer elections (Wright, Yates, and Hessick 2021, p. 127). Similarly, C. B. Hessick and Morse (2019) collected election results for 2,315 districts across 45 states and found that in urban jurisdictions, elections were more likely to be contested and competitive. Given the concentrated consequences of criminal justice in urban areas, these prosecutor elections becoming competitive have a significant impact.

A prominent political reform movement is the emergence of competitive elections for DAs by reform-minded challengers ("progressive DAs"). Indeed, America's five biggest cities by population all have elected progressive DAs (including Los Angeles, Philadelphia, Boston, NY, Chicago, and Houston) (C. B. Hessick and Morse 2019). The election of DAs vocally dedicated to internal reforms of the criminal legal system, a movement gaining traction (Bazelon and Krinsky 2018), is a critical trend in contemporary politics. These 'anti-prosecutorial' DAs symbolize a shift in public attitudes and policy debates. Central to understanding these reform-oriented DAs' political success (and failures) is voters' perception of their stance on criminal justice policymaking's intensive and extensive margins. The recall of San Francisco's progressive DA may reflect deeper voter sentiments about balancing criminal justice policy's extensive and intensive margins.

In 2022, San Francisco voters exercised their democratic power by initiating a recall election targeting the local DA, a self-proclaimed progressive reformist. Voters removed the DA from office,[4] and numerous news outlets rushed to offer their interpretations.[5] The impulsive reactions and hasty judgments surrounding the high-profile recall, distort the political system's ability to respond to voters. A functioning democratic responsiveness requires an accurate interpretation of elections' outcomes, often defined by politicians and the media (Hershey 1992; M. Shamir and J. Shamir 2008). Applying the extensive and intensive margins theory shows that voters did not revert to a "get tough" agenda, contrary to popular belief.

[4]In June 2022, the San Francisco DA was recalled by a 55% popular vote. At that time, 62.8% of San Francisco's registered voters were Democrats, while only 6.7% identified as Republicans.

[5]For example, the New York Times declared that "California Sends Democrats and the Nation a Message on Crime" (Goldmacher 2022). See also: NPR, NYT, SF Chronicle, The Washington Post, Slate.

4 Study 1: The Curious Case of the San Francisco Recall Election

Sample

The present study is based on a novel survey mode - digital exit polls. Exit polls have the advantage of providing behavioral measures. Verified voters' opinions reflect more on political behavior, especially when studying the factors influencing vote choice. However, traditional exit polls rely on physical voting, which currently is a small percentage of ballots cast in San Francisco. In the June 7th primary election, when the recall was on the ballot, only 9.6% of cast votes were on election day (4.48% of registered voters), while 90.3% of cast votes were early votes by mail (41.79% of registered voters). To solve this issue and receive the benefits of surveying voters, I partnered with Political Data Inc. to email voters immediately after they cast their early ballot and the city receives it.

Using Political Data Inc.'s (PDI) platform for political campaigns, I contacted, directly by email, voters who returned their votes before the election date. This resulted in a sample of 545 verified voters who completed the study. I could also directly contact all registered voters in San Francisco after the polls closed who did not vote by mail, resulting in another sample of 343 potential voters. I used the San Francisco voter file to construct weights based on age, party ID, and zip code. After constructing the weights, combining the samples resulted in 791 respondents who either verified voters or indicated they voted, 33 who indicated they intended to vote, and 15 who indicated they did not intend to vote (Full information in the Supplementary Information section S1).

Methods and Procedures

The dependent variable is vote choice.[6] After providing information about their voting status, each respondent reported whether they voted in favor or against the recall. I then collected information regarding why the respondents voted for or against the recall, how they would rate the performance of Boudin (as a validity check to the voting preference question), whether they voted in previous DA elections, and whether they knew who was the previous DA. Respondents also reported information on gender, race, political party support, home ownership, income, education, and political ideology (on a liberal-conservative scale). The following parts of the questionnaire were devoted to testing the predictors of voting behavior.[7]

The first set of independent variables encompasses punitive and progressive sentiments. Limited research investigates the link between crime control attitudes and vote choice (Wozniak, Calfano, and Drakulich 2019; Mears and J T Pickett 2019; Schutten et al. 2022). Researchers identified different components of crime control attitudes: punitive sentiment (Enns 2016; Ramirez 2013), racial attitudes (Pager 2008; Tonry 2011; Cullen, Butler, and

[6]The question is why voters voted the way they did. As such, I do not expect to explain the election result as this would require information on the decision to turn out to vote.

[7]See Section S2 in the Supplementary Information for additional information on wording.

Graham 2021), and the salience of crime (Adriaenssen and Aertsen 2015). I used the three items capturing most of the variance in punitive sentiment studies: Spending on halting crime, death penalty preference, and harshness of the current punishments (edited to discuss San Francisco) (Enns 2016; Duxbury 2021). The three items were combined into a scale with equal weights (Cronbach's alpha is 0.627). Progressive sentiment was measured using two items: attitudes toward reducing prison and jail population and reducing police budget (Cronbach's alpha is 0.751).[8] Belief in the redeemability of offenders was also examined (Maruna and King 2009; Burton et al. 2020) (Cronbach's alpha is relatively low, 0.548).

The second set of independent variables were crime salience and victimization. In addition, I used the log of reported crime rates in each respondent's zip code. Data on reported crimes was gathered from the San Francisco Police Department Incident Reports: 2018 to Present.[9] The reported incidents are aggregated by zip code, and the rate is calculated based on the data from the 2020 census per zip-code.[10] I use a measure of crime rate increase per respondent zip code by comparing the log of crime rate from 2020-2022 to 2018-2020. Lastly, the third independent variables set measured racial attitudes: Racial Resentment (Kinder and Sanders 1996) (Cronbach's alpha 0.884) and an abbreviated version of the Racial Sympathy battery (Chudy 2021) (Cronbach's alpha 0.751).

Analytical Strategy and Results

Table 2 illustrates the relationship between theoretical predictors and vote choice, with a vote for recall coded as 1. The table progresses from parsimonious to comprehensive specifications, identifying stable coefficients. Column 1 displays the predictive power of each theory in isolation using 10 bi-variate models. Column 2 presents the same models, controlling for demographic factors: age, gender, household income, political ideology, partisanship, race, education level, and homeowner status. Lastly, column 3 combines all predictors into a single model, reporting each coefficient.

Columns 1 and 2 indicate that all variables except crime rate predict vote choice in line with existing theories. In column 3, examining the combined predictive power of all variables, punitive sentiment and crime salience emerge as strong predictors of recall support, while actual crime rates in voters' zip codes do not. Additionally, the subjective crime salience is uncorrelated with actual crime report rates, $r(858) = 0.029$, $p = 0.38$.[11] Progressive sentiment predicts recall opposition, holding other factors constant.

[8] The scale's validity is discussed in the Supplementary Information, section S3.

[9] Available here.

[10] Using crime reports since January 2020, the year Boudin assumed office. The results are the same when using crime reports for the 2018-2022 period.

[11] Log-transformed crime rates in respondents' zip codes and crime salience are not correlated, $r(858) =$ -0.004, $p = 0.89$.

Table 2: Predicting Recall Support using Theories of Crime Control Attitudes

	A Vote in Favor of the Recall		
	Bivariate Models	Bivariate w/controls	Multivariate w/controls
Redeemability belief	-0.733	-0.187	0.006
	[0.089]	[0.091]	[0.072]
	(<.001)	(0.042)	(0.929)
Racial Sympathy	-0.881	-0.426	-0.051
	[0.053]	[0.080]	[0.083]
	(<.001)	(<.001)	(0.535)
Racial Resentment	0.994	0.616	0.087
	[0.045]	[0.084]	[0.100]
	(<.001)	(<.001)	(0.390)
Punitive Sentiment	0.938	0.633	0.350
	[0.036]	[0.063]	[0.073]
	(<.001)	(<.001)	(<.001)
Salience of Crime	1.160	0.768	0.388
	[0.054]	[0.073]	[0.087]
	(<.001)	(<.001)	(<.001)
Crime Victim?	0.121	0.051	0.026
	[0.049]	[0.040]	[0.035]
	(0.014)	(0.210)	(0.451)
Crime Rate (log)	0.031	0.044	0.031
	[0.036]	[0.031]	[0.028]
	(0.393)	(0.157)	(0.274)
Increase in Crime Rate	-0.029	-0.006	0.107
	[0.189]	[0.150]	[0.136]
	(0.877)	(0.964)	(0.437)
Criminal Justice Progressive Sentiment	-0.845	-0.573	-0.246
	[0.035]	[0.060]	[0.077]
	(<.001)	(<.001)	(0.001)

Continued on next page

Table 2 – continued from previous page

	A Vote in Favor of the Recall		
	Bivariate Models	Bivariate w/controls	Multivariate w/controls
Crime Politics Knowledge	-0.394	-0.166	-0.068
	[0.131]	[0.117]	[0.112]
	(0.003)	(0.162)	(0.546)
FE	No	Yes	Yes
Num.Obs.			749
R2			0.673
R2 Adj.			0.654
RMSE			0.34
Std.Errors	response_id	response_id	response_id

Note: Standard errors in brackets and two-tailed p-values in parentheses. The dependent variable is recall voting (binary, 1 = Yes) with an 839-weighted observations sample size (using only observations with complete data). The first column shows bivariate results for the 10 variables (from 10 separate models). The second column adds the demographic covariates age, reported gender, household income, political ideology, partisanship, reported race, level of education, and homeowner status; the third column is the results for all 10 variables and the demographic covariates (all in one model). Standard errors are clustered at the respondent's level as a conservative approach to strengthen the within-respondent independence assumption. Weights were calculated using the San Francisco voter file with age, zip code, and party ID as targets.

Compared to voters identifying as White, Asian voters were statistically significantly more likely to support the recall.[12] I find no statistically significant effect for reported gender, age, partisanship, or home ownership (see Figure S5.1 in the Supplementary Information).

The traditional punitive-progressive constructs have strong predictive power, and their focus on measurement remains valuable with no clear alternative; yet, they fall short of providing a theoretical explanation for why voters opposed or supported the recall.[13] Notably, measuring punitive sentiment is challenging (Adriaenssen and Aertsen 2015). The punitive scale, used by Enns (2016) as an ad-hoc measure, was not designed to elucidate the underlying meaning of a punitive attitude. It comprises separate and distinct survey questions not intended for this purpose. There is 'no adequate measure of the public's preferences for being tough on crime' (Enns 2016). This underscores the necessity of developing a theoretical framework to interpret what it means to be 'high on the punitive scale.' Furthermore,

[12]In line with expectations, see Two-thirds of registered Asian American voters favored the recall.

[13]Importantly, the distinction between a measurement and the underlying theory has been debated in other contexts, most notably around the Racial Resentment scale (Kinder and Sanders 1996; D. W. Davis and Wilson 2021). The scale has been shown to hold consistent and strong predictive power, yet debates on the underlying theory and extensions of it are far from settled Wilson and D. W. Davis (2011) and Agadjanian et al. (2023).

while the progressive sentiment scale might provide similar predictive power[14], the behavior of 'progressive' respondents is notably less homogeneous. About 90% of voters opposing progressive reform voted against the progressive DA, yet only approximately two-thirds of 'progressive' voters supported the progressive DA. Why did one out of three progressive voters oppose the progressive DA? This raises important questions about voters' motives, highlighting an area ripe for further theoretical exploration of the punitive-progressive divide.

Conflicted Progressives

Voting behavior data revealed "conflicted progressives": voters endorsing reform yet recalling a progressive DA. Figure 1 highlights these voters in purple, sharing similar progressive scale scores with recall opponents (bottom row, in blue). Progressive sentiment encompasses support for police defunding, reduced incarceration, or both (see section S3 in Supplementary Information). In the sample, conflicted progressives constitute 42% of recall supporters and 29% of progressive voters. Why didn't these progressive voters support the progressive DA? Tables 3 and 4 juxtapose conflicted progressives with recall-supporting and recall-opposing voters. Conflicted progressives exhibit significant differences from both groups.

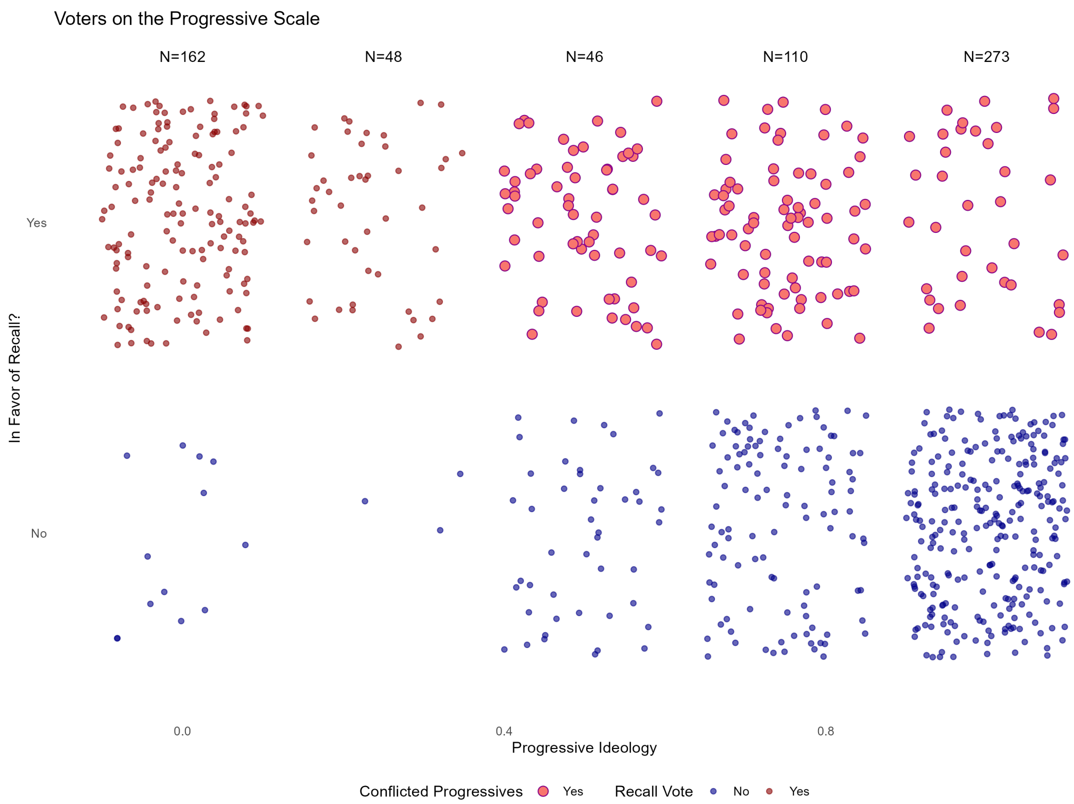
Table 3: Voters Supporting the Recall

	Not Progressive (N=245)	Conflicted Progressives (N=177)	P-value
Salience of Crime	0.724 (0.201)	0.586 (0.219)	<.001
Redeemability Belief	0.672 (0.233)	0.784 (0.159)	<.001
Punitive Sentiment	0.831 (0.223)	0.645 (0.278)	<.001
Crime Victim	0.595 (0.492)	0.535 (0.500)	0.234
Racial Sympathy	0.463 (0.293)	0.645 (0.241)	<.001
Racial Resentment	0.548 (0.275)	0.272 (0.233)	<.001

Note: The values are means and standard deviations.

[14]See more details in Supplementary Information Section S3.

Figure 1: Distribution of voters across the progressive sentiment scale



Note: This figure shows the heterogeneity in the distribution of voters who opposed and supported the recall across the progressive sentiment scale. Points are jittered.

Table 4: All Progressive Voters

	Oppose the Recall (N=428)	Conflicted Progressives (N=177)	P-value
Salience of Crime	0.347 (0.182)	0.586 (0.219)	<.001
Redeemability Belief	0.822 (0.156)	0.784 (0.159)	0.00755
Punitive Sentiment	0.291 (0.274)	0.645 (0.278)	<.001
Crime Victim	0.378 (0.485)	0.535 (0.500)	<.001
Racial Sympathy	0.782 (0.199)	0.645 (0.241)	<.001
Racial Resentment	0.110 (0.139)	0.272 (0.233)	<.001

Note: The values are means and standard deviations.

Did conflicted progressives oppose the progressive DA because of specific policy opposition? I presented Chesa Boudin's major policy reforms and asked the voters to indicate their support for each. Respondents were randomized to either receive information that the policies were Boudin's or receive the policies without information about Boudin's affiliation.

Table 5: Policies for "Chesa" context Experiment

Police Accountability	Do not prosecute a defendant if the officer pressing charges has a record of misconduct.
Reverse "Three Strikes"	Roll back sentencing enhancements from the "Three-Strikes and You're Out" era.
Parents Alternative Sanctions	Providing alternatives to jail and prison for parents in the justice system.
Eliminate Cash Bail	Eliminating the use of cash (money) bail for release before trial.

Compared to other voters who supported the recall, conflicted progressives supported the progressive policies about twice as much on average, regardless of treatment condition. (Figure 2).[15] On average, conflicted progressives supported 65% of the policies without information about Boudin. What explains the adverse effect of information about Boudin? The next part argues that it is an aversion toward minimizing government intervention in the criminal legal system.

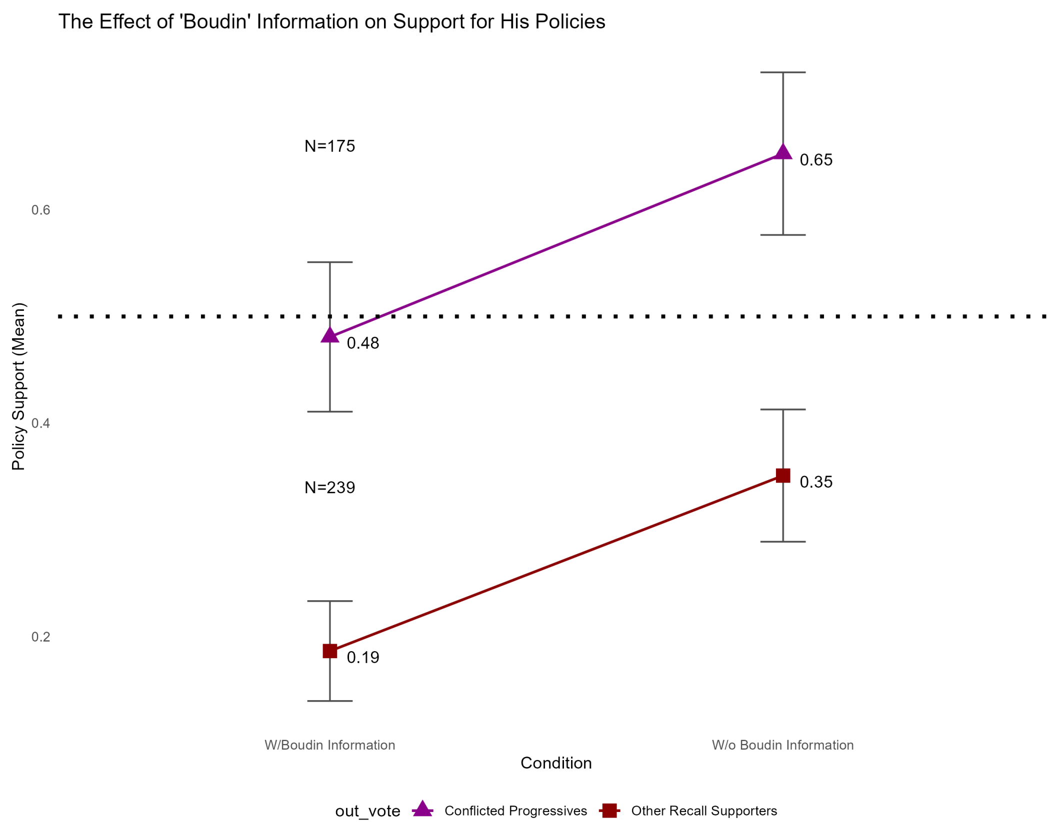
To conclude Study 1, voters who supported the recall were a mixed bag of punitive and progressive voters who resisted Boudin despite supporting his policies. Thus, voters affected the criminal legal system not through specific policy preferences but through the message they sent and the general package of policies policymakers might pursue next. Why do progressive voters vote against a progressive candidate? Study 2 offers and tests a theory.

5 Study 2 - Disentangling the "Progressive" Agenda

Because the American criminal legal system is harsh in outcomes and utilized to respond to a wide range of social issues, progressive reform can mean reducing the intensity (harshness of punishment), reducing the extent (scope of prosecuted behavior), or both. This study documents the effect of deconstructing the progressive agenda on support for reform in the context of prosecutor elections.

[15]Voters who opposed the recall supported the policies on average about 90% of the time, regardless of treatment condition (Figure S7.1 in the Supplementary Information).

Figure 2: The effect of Boudin's name on support for Boudin's policies



Note: This figure shows the effect of the experimental condition on the voters who supported the recall separately by their level of support for progressive ideology. A complete analysis is in Section S7 in the Supplementary Information.

Materials

Within the same survey of 888 registered voters in San Francisco, respondents were placed into one of three conditions, as detailed in Table 6.[16] The "lower extensive margin" was adapted from the platforms of progressive DA candidates nationwide (the Supplementary Information includes examples in section S4) to emphasize shrinking the jurisdiction of the criminal justice system. In contrast, the "lower intensive margin" condition is focused on reducing the intensity of severe punishments. To address ecological validity concerns, section S4 in the Supplementary Information includes a table showing how DA candidates express

[16]The study also included a control condition to illicit baseline attitudes toward DAs. It is identical to the other conditions in structure, but the hypothetical candidate "wants to make sure public safety is the top priority" and declares that "some offenders require attention." For interpretability, the coefficients on the control are not reported. S8 includes results with the control condition in Table S8.1.

their agenda and how it fits with the three theoretical constructs.

Table 6: Government Intervention - Constructs and Experimental Conditions

Construct	**Treatment Condition**
"get-tough": Reforming the criminal legal system in a punitive direction.	*"A new possible candidate promised to keep criminals accountable. The candidate wants to* ***replace short sentences with longer prison sentences*** *for first-time, nonviolent low-level criminal defendants. According to the candidate: '****These offenders do not belong in our city,*** *my office will deter them by lengthening sentences and removing them from our streets!'"*
"Lower extensive margin": Reforming the criminal legal system to minimize its scope - to make it **less extensive** by reducing how many behaviors are acted on by the criminal legal system.	*"A new possible candidate promised to keep criminals accountable. The candidate wants to* ***reduce prosecution*** *of first-time, nonviolent low-level criminal defendants. According to the candidate: '****Some offenders do not belong in the criminal system,*** *my office will not concern itself with taking such low-level offenses to court!'"*
"Lower intensive margin": Reforming the criminal legal system to reduce the severity of outcome - to make it **less intensive** by replacing traditional imprisonment solutions with different initiatives.	*"A new possible candidate promised to keep criminals accountable. The candidate wants to* ***replace short sentences with intense rehabilitation*** *'boot camps' for first-time, nonviolent low-level criminal defendants. According to the candidate: '****Some offenders do not belong in prison,*** *my office will supervise them under new rehabilitative paths!'"*

Analytical strategy

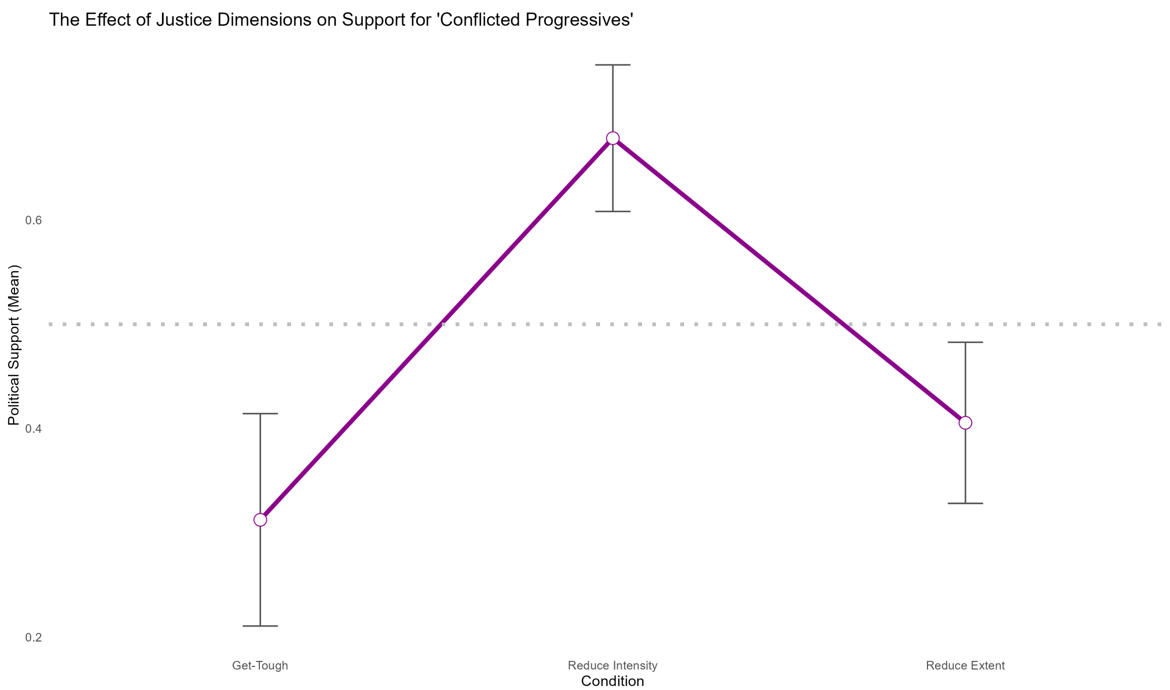
Models 1-3 estimate the effect of the treatment conditions on the voters who supported the recall, opposed it, and the conflicted progressives, against the "get tough" reference category.

$$\text{(1-3) Candidate Support}_i = \beta_0 + \beta_1\text{Lower intensive margin} + \beta_2\text{Lower extensive margin} + \beta_3\text{Get tough} + \epsilon_i$$

Results

Progressive voters who supported the recall resist a Get-Tough and a Reduce Extent candidates similarly (Figure 3). Yet, they support the Reduce Intensity candidate.

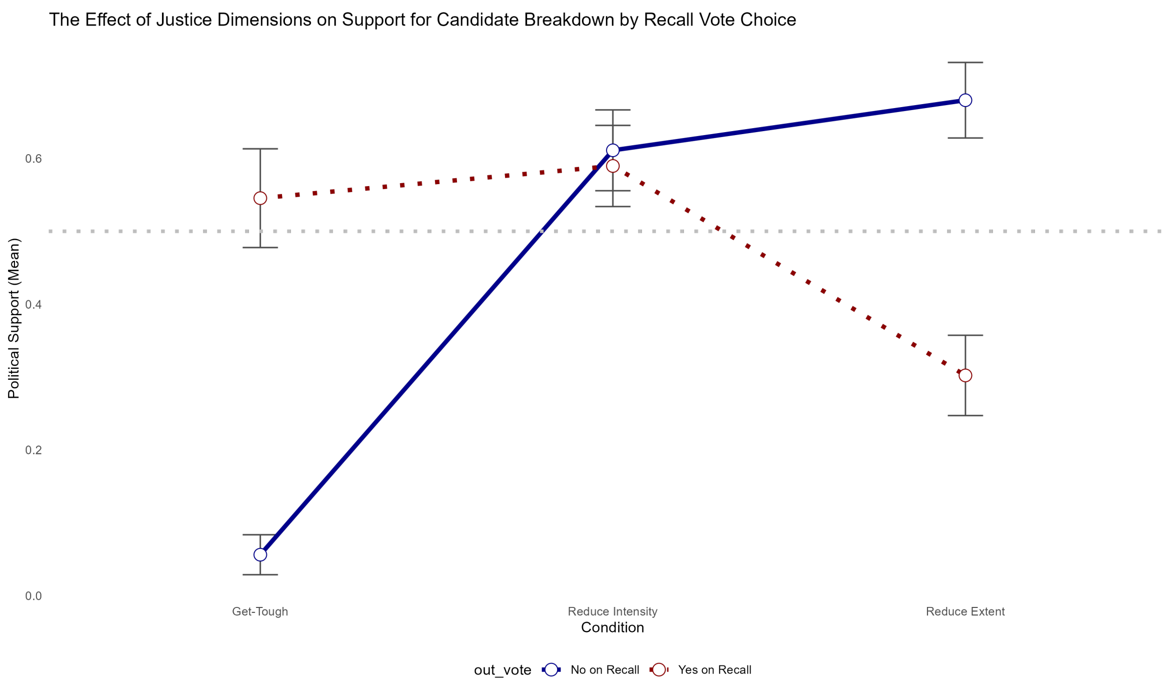
Figure 3: The effect of treatment conditions on conflicted progressives



Note: This figure shows the effect of the experimental condition on the voters who supported the recall separately by their level of support for progressive ideology. Table 8 presents the statistical analysis.

Figure 4 visually presents the results by vote choice for the entire sample. These findings reveal a comparative indifference between the Get-Tough and Reduce Intensity conditions for recall supporters and between the Reduce Extent and Reduce Intensity conditions for recall opponents.

Figure 4: The effect of treatment conditions on the entire sample



Note: This figure shows the effect of the experimental conditions separately for the voters who supported the recall and opposed the recall. A complete analysis is in the Supplementary Information Section S8. The black dashed line represents the 50% mark.

Table 7 presents the result of the main analysis, showing respondents in the same experimental category but who voted differently in the recall election (dotted and straight lines in Figure 4) had significantly different outcomes.

Table 8 presents the difference in experimental conditions within the voter groups.

Table 7: Difference in Differences - Effect of Experimental Conditions Across Groups

Group	t-Statistic	p-Value	LowerCI	UpperCI	N (Recall)	N (No Recall)
Get Tough	13.29	$<.001$	0.42	0.56	100	112
Reduce Extent	-9.89	$<.001$	-0.45	-0.30	111	115
Reduce Intensity	-0.54	0.59	-0.10	0.06	107	109

Note: This table reports the results of a difference-in-differences analysis comparing two groups of respondents across experimental conditions. Confidence intervals are calculated at the 95% confidence level.

Table 8: Effects of experimental conditions on supporting a DA candidate

	(1) Recall proponents	(2) Recall opponents	(3) Conflicted progressives
Lower extensive	**-0.240**	**0.626**	0.104
margin	[-0.325, -0.154]	[0.568, 0.683]	[-0.017, 0.225]
	t = -5.512	t = 21.274	t = 1.700
	p < .001	p < .001	p = 0.091
Lower intensive	0.053	**0.553**	**0.379**
margin	[-0.032, 0.139]	[0.493, 0.614]	[0.262, 0.495]
	t = 1.225	t = 18.057	t = 6.431
	p = 0.221	p < .001	p < .001
Num.Obs.	421	464	177
R2	0.195	0.483	0.276

Note: All models report the result of the effect of the experimental conditions on supporting a hypothetical DA, with ”get-tough” as the reference category. Standard errors are clustered at the respondent’s level.

Study 2 identifies three distinct voter groups. It reveals no common ground concerning ”getting tough” or reducing the extensive margin, but all three groups supported lowering the intensive margin.

6 Study 3: Construct Validation with a National Sample

In a national survey experiment, I assessed the generalizability of Reduce Intensity, Reduce Extent, and Get-Tough constructs beyond the San Francisco recall election context. This experiment aimed to (1) validate the constructs via a placebo test and (2) obtain qualitative insights through an open-text question.

Sample and Materials

The survey recruited a broadly representative sample of 1,030 adult Americans through the online marketplace Lucid Theorem on September 28, 2022. After removing inattentive respondents, I am left with a sample of 983 (2.1).[17] Lucid Theorem employs quota sampling to produce samples matched to the US population on age, gender, ethnicity, and geographic region; recent research demonstrates the suitability of the Lucid platform for evaluating social scientific theories; it was also validated to return similar answers to experiments conducted on nationally representative samples (Coppock and McClellan 2019; Coppock 2023). In this study, no weights were used in the survey experiment analysis; using weights in survey experiments analysis depends on the type of generalization (external validity) the researcher seeks to achieve (Egami and Hartman 2022) and on whether we can identify covariates that predict both treatment heterogeneity and selection into the sample (Miratrix et al. 2018). The difference in the composition of units in the experimental sample and the target population (voting-age Americans) does not raise specific treatment-generalization issues because selection into the experiment and treatment effect heterogeneity are unrelated to each other (Egami and Hartman 2022).

To validate the three constructs, I employed a placebo test with varied language across three treatment conditions while maintaining the substantive construct. This assessed whether outcome differences were tied to specific treatment versions or the underlying construct. The placebo realizations involved different subjects, alternatives to traditional punishment, and wording variations (see Supplementary Information section S9). By randomizing respondents within conditions to different versions, outcomes could be attributed to the construct if no differences were found between the versions. Lastly, an open-text question was included to gather insights into participants' perceptions of the constructs by asking them to explain their support for the hypothetical candidate.

Analytical Strategy - Treatment Effect Heterogeneity

Previously, I assessed support for different DA approaches and found both voters who opposed the recall and supported it disliked each other's "classic" candidate but coalesced

[17] I removed respondents who failed an attention check and whose survey completion time was less than three minutes. See Section S1 for additional information.

around the "Reduce Intensity" candidate. The national sample, however, lacks voting behavior data. So, to replicate the original finding with a national sample, I divided it by estimated "vote choice," using recall opposers and supporters as reference groups. Both propensity score matching and random forest methods were employed. Random Forest, the algorithmic approach, is considered to be significantly more accurate (Muchlinski et al. 2016); thus, the propensity score matching method is reported in the Supplementary Information (Figure S9.1).

I predicted vote choice using the Random Forest algorithm, a classification method without distributional assumptions that assess variable importance based on prediction accuracy (Breiman 2001; Jones and Linder 2015). This algorithm partitions data repeatedly to estimate the conditional distribution of a response given a set of explanatory variables, ultimately finding homogeneous partitions of the outcome (vote choice) given the predictors.[18]

Utilizing a parsimonious model, I employed significant predictors of vote choice: punitive sentiment, crime salience, progressive sentiment, racial attitudes, and average support for Boudin's policies (Table 2, Figure S7.1). The randomForest package in *R* (version 4.7-1.1) was used, with 2,000 trees and one variable randomly sampled as candidates at each split. National sample respondents were then assigned a predicted value for "vote choice."

To account for potential stochasticity in random forest model selection, I repeated the process 100 times. Treatment effects from these predictions demonstrate result independence from any single prediction model. A t-test was employed to estimate differences in treatment effects.

Results

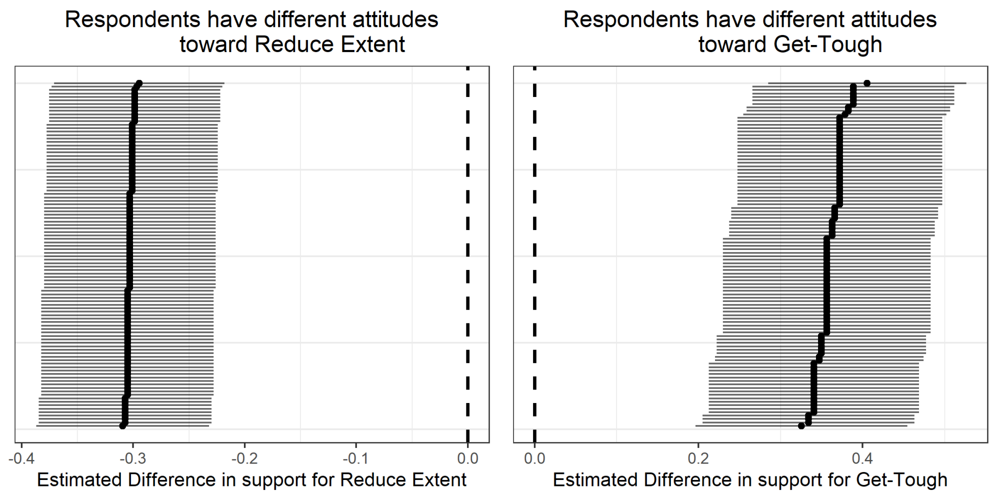
The placebo tests were passed successfully. There are no differences between the effects of the different treatment realizations, $\chi^2(2) = 2.066, p = 0.3559$.

The following figures verify whether the differences in voters' attitudes presented in Study 2 generalize to the national sample. All figures plot differences in predicted treatment effects as a function of individuals' covariate profiles, along with 95% CIs. Every observation is an estimated difference between respondents similar to recall proponents and respondents similar to recall opponents according to a single random forest model.

As anticipated, proxy recall proponents and opponents exhibit divergent preferences toward reducing the extensive margin and attitudes toward "get-tough" approaches. Figure 5 validates these expectations across all 100 random forest iterations, revealing a 30 percentage point gap in support for the Reduce Extent candidate and a nearly 40 percentage point gap for the Get-Tough candidate.

[18]More information in Supplementary Information section S10.

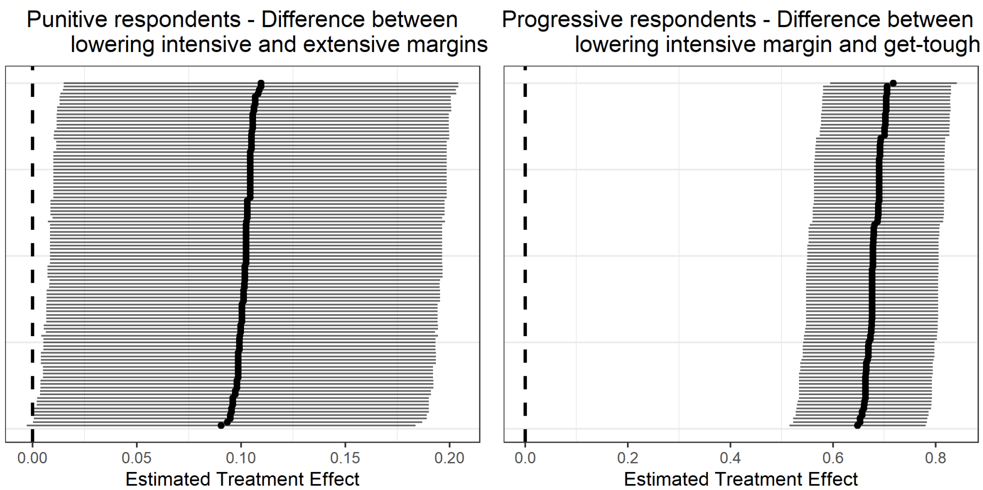
Figure 5: Estimated differences in treatment effects between proxy recall opponents and proponents



Note: Sub-group differences in treatment effects, along with 95% CIs. The left figure is for the Reduce Extent condition, and the right is for the Get-Tough condition. Every observation is an estimated difference regarding the favorability of a candidate between proxy recall proponents and proxy recall opponents according to a single random forest model.

Study 2 predicts proxy recall proponents to favor lowering the intensive margin over the extensive margin, while proxy recall opponents should prefer reducing the intensive margin to "getting tough." Figure 6 corroborates these expectations: proponents support reducing the intensive margin by approximately 10 percentage points more than the extensive margin, while opponents favor reducing the intensive margin over the Get-Tough candidate by nearly 70 percentage points.

Figure 6: Estimated treatment effects differences within proxy recall opponents and proponents



Note: Differences in predicted treatment effects within sub-groups, along with 95% CIs. The left figure is for proxy recall proponents, and the right is for proxy recall opponents. Every observation is an estimated difference between treatment conditions, according to a single random forest model.

Lastly, despite the previously observed differences, we anticipate both respondent groups to exhibit comparable attitudes toward reducing the intensity of the criminal legal system. Figure 7 demonstrates that the difference in average support for the Reduce Intensity candidate between the two groups is rarely statistically significant.

Study 3 reveals that potential voters with divergent attitudes towards crime control disagree on the ideal candidate yet display strikingly similar support for a Reduce Intensity candidate. This indicates that while attitudes towards reforming the extent of the criminal legal system vary, those regarding reforming intensity are consistent across the spectrum.

Analyzing open-text responses

I have posited that support for reducing the intensive margin is widespread, unlike attitudes toward reducing the extensive margin. Why do voters support reducing the intensive margin but not the extensive margin of the criminal legal system? Are instrumental concerns, such as deterrence, or expressive concerns, like retribution, driving these preferences (Ramirez 2013; Tyler and Boeckmann 1997)? I included an open-text question following their decision-making process to explore respondents' motivations.

I employed a Natural Language Processing (NLP) technique for text summarization to analyze open-text responses. I utilized the widely-used BART[19] model (Lewis et al. 2019),

[19] BART's seq2seq/machine translation architecture combines a bidirectional encoder (like BERT) and a

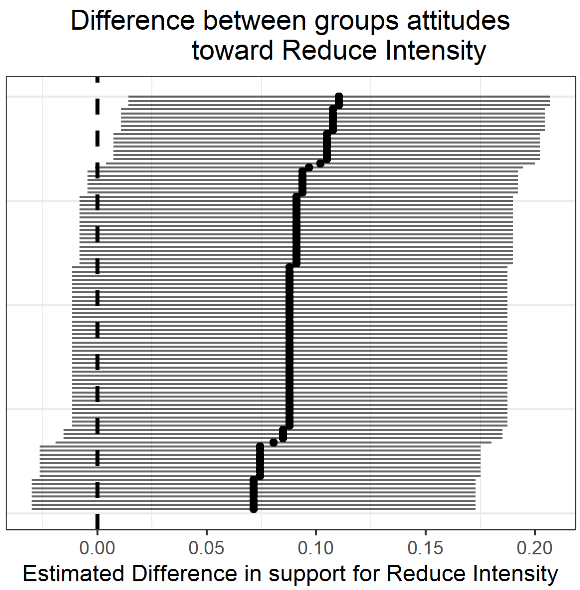


Figure 7: Estimated differences in treatment effect between proxy recall opponents and proponents

Note: This figure shows sub-group differences in predicted treatment effects, along with 95% CIs. Every observation is an estimated difference in the ”reduce intensity” treatment effect between proxy recall opponents and proponents, according to a single random forest model.

from the Hugging Face repository.[20] The analysis was conducted using R's ”text” package (Kjell and Schwartz In progress).

Why did respondents oppose a Reduce Extent candidate: Table 9 shows that respondents, by and large, opposed the Reduce Extent candidate based on instrumental concerns. Respondents worried that the result of ”outsourcing” crime control would incentivize crime.

left-to-right decoder (like GPT), excelling in comprehension tasks and text generation, including summary, translation, classification, and question answering. More information in Supplementary Information section S10.

[20] Available here.

Table 9: NLP Summarization - Opposing Reduce Extent

Summary of all the responses to "why did you oppose the DA?"
A crime is a crime, no prosecution encourages more crime, regardless of how small. All crimes should be prosecuted, low-level non-injury convictions should be adjudicated with restitution and effort. Most criminals caught for the first time have been getting away with crime for a long time. Some low-level crimes should still be punishable if they are repeat offenders, we should worry about all criminal offenses, not ignore the small ones.

Note: Only summarizes the answers for respondents assigned the "Reduce extent" condition and decided to oppose it.

Why did respondents oppose a Reduce Intensity candidate: Table 10 shows that respondents who opposed the Reduce Intensity candidate were explicitly punitive. Unlike the opposition to Reduce Extent, here, the opposition is not driven by a concern for lack of deterrence but a retributive concern based on moral reasons.

Table 10: NLP Summarization - Opposing Reduce Intensity

Summary of all the responses to "why did you oppose the DA?"
Candidates should be more strict about punishment for crimes. Criminals should do their time, not community service. Offenders belong behind bars. They should think about how it's gonna affect the criminal community. It's not possible to monitor someone at all times.

Note: Only summarizes the answers for respondents assigned the "Reduce intensity" condition and decided to oppose it.

Why did respondents oppose a Get-Tough candidate: Table 11 shows that respondents opposed the Get-Tough candidate based on traditional progressive values: the distinction between violent and non-violent crime, fairness.

Table 11: NLP Summarization - Opposing Get-Tough

Summary of all the responses to "why did you oppose the DA?"
The DA's policy on non-violent crime is excessive. It is unfair to penalize low-level criminals. It would be better for them to be rehabilitated and show them that crime is not the answer.

Note: Only summarizes the answers for respondents assigned the "Get tough" condition and decided to oppose it. The length of the aggregated text responses for the "Get-tough" required using the t5 model instead of BART. T5 can be trained for various tasks, while BART is specifically designed for text summarization.

Next, I employed keyness statistics[21] to compare word frequency differences between respondents who opposed and supported the same candidate (Zollinger 2022); the diverse justifications for their decisions provide face validity to the survey instrument. Comparing justifications for supporting a lower extensive margin versus opposing it, Figure 8 reveals that supporters mention terms like "help," "chance," and "second," while opposers predominantly focus on "crime." These findings align with the results of the text summarization technique. Figure 9 indicates that support for reducing intensity is associated with a combination of the terms "rehabilitation" and "prison." Conversely, opposition centers on the prominence of "punishment."

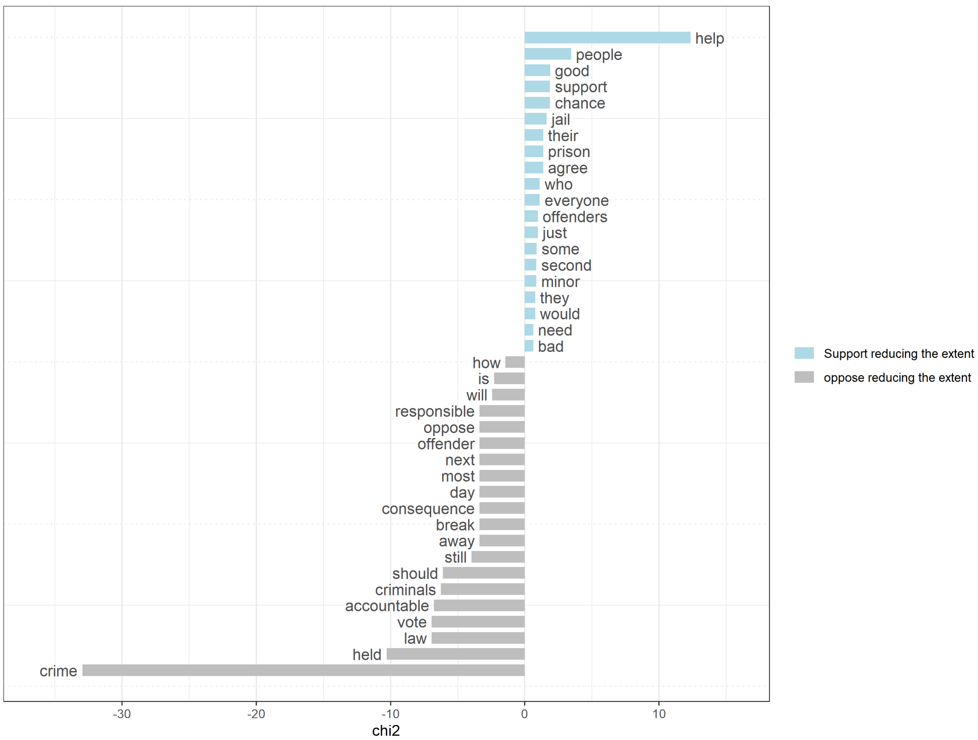
In summary, the difference in attitudes is based on the assumption that the extensive margin serves instrumental purposes while the intensive margin addresses expressive purposes. This implies that most individuals seek to reduce excessive retribution without compromising deterrence in reform considerations. Moral concerns underpin the common support for decreasing the intensive margin, while any opposition to altering the extensive margin is grounded in preserving deterrence.

7 Discussion and Implications

I provide evidence of polarized attitudes toward crime and justice. I identify a significant negative correlation between punitive and progressive sentiment, revealing a sharp divergence between progressive and punitive voters (Justin T Pickett and Baker 2014; Unnever et al. 2010). However, I highlight a crucial variation within the progressive group. Some voters display seemingly conflicting attitudes: supporting crime-control policy reforms but opposing

[21] More information in Supplementary Information section S10

Figure 8: Keyness statistics by the decision to support or oppose a candidate



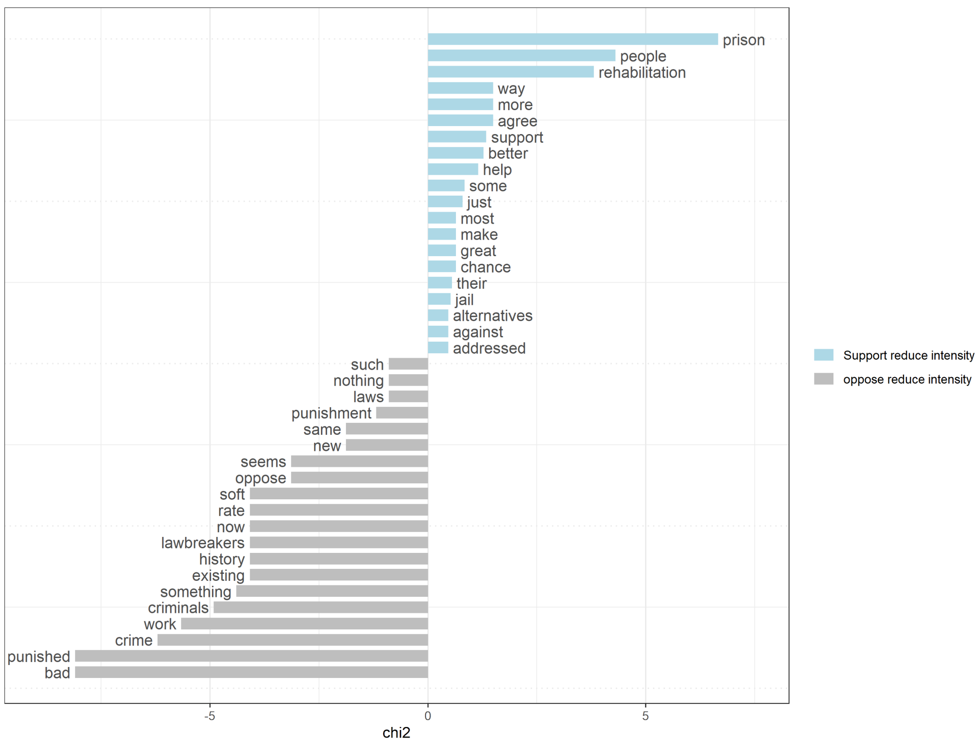
Note: Shows terms mentioned with greatest relative frequency by respondents who supported a candidate, relative to respondents who opposed it.

progressive politicians. Three survey experiments (local and national samples) elucidate this phenomenon, pointing to the politics of crime and justice hinging on two parameters: intensity and extent. Intensity concerns penal outcome harshness, while extent pertains to the desired scope of behaviors subjected to criminal justice intervention.

Reducing the intensive margin is popular as a prosecutorial agenda. "Get-tough" and "radical progressives" alike favored reducing the intensive margin, suggesting that punitive Americans can be "pragmatic" (Cullen, Fisher, and Applegate 2000). Open-text analysis revealed that only the most zealous tough-on-crime people oppose reducing the intensive margin, driven by expressive concerns, while the majority prioritizes instrumental concerns.

Conversely, reducing the extensive margin for prosecutors running for election is unpopular. Respondents express concern about a government vacuum, potentially incentivizing crime. This finding may be specifically relevant to the politics of prosecutor elections: the institutional structure that tied prosecutor effectiveness with their ability to achieve high conviction rate (J. Pfaff 2017) might explain why punitive and progressive voters alike wish their DA to uphold a high extensive margin. These findings also provide an explanation for the argument that the public supports reforming the behavior of police (the intensive

Figure 9: Keyness statistics by the decision to support or oppose a candidate



Note: Shows terms mentioned with greatest relative frequency by respondents who supported a candidate, relative to respondents who opposed it.

margin) but not defunding or abolishing it (the extensive margin) (Vaughn, Kyle Peyton, and G. A. Huber 2022). Future research should further explore how attitudes along the intensive and extensive margin differ in other contexts of the criminal legal system.

Does push-back against criminal justice reform foreshadow a return to tough-on-crime attitudes? The recalled DA, Chesa Boudin, often made remarks such as: "We will not prosecute cases involving quality-of-life crimes" and "Crimes such as public camping, offering or soliciting sex, public urination, blocking a sidewalk, etc., should not and will not be prosecuted."[22] After the recall, pundits postulated that voters expressed their preference to return to "get-tough" crime control policies. Building and expanding the traditional punitive-progressive divide allows us to reach a more complex answer. This article demonstrates public support for reforming punishment intensity while maintaining a commitment to the current extensive margin. If we rely only on a punitive-progressive distinction, we risk learning the wrong lesson from the election, hurting democratic responsiveness.

[22] Boudin Will Not Prosecute Prostitution, Public Camping, And Other 'Quality-Of-Life Crimes'

8 Conclusion

This article offers a theory of crime control attitudes in the context of political transformations. It expands on the traditional punitive-progressive divide to explain why voters tossed out a leading reform advocate. After decades of ramping up the population under the control of the criminal legal system, multiple political attempts to reform crime control policy are taking effect. One prominent political reform movement is the emergence of competitive elections for DAs. Despite numerous political wins, we are still determining what explains voters' attitudes toward progressive DAs on the ballot. Utilizing the case study of a recall election for San Francisco's progressive DA and an online experiment on a national sample, this article argues that voters support DAs committed to reducing the severity but not the scope of crime control.

The findings suggest that voters may oppose progressive DAs while endorsing progressive reform. In explaining voters' revealed preferences in the recall election, this article posits that voters can concurrently advocate for less severe outcomes in the criminal legal system (the intensive margin) and maintain broad support for penalizing behavior (the extensive margin). These insights hold critical implications for electoral accountability within the politics of crime and justice. Misinterpreting electoral outcomes can obscure voters' true preferences and undermine efforts to dismantle mass incarceration. Understanding these nuanced attitudes is essential for driving meaningful reform in the criminal legal system.

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Supplementary Information

S1 Samples

Table S1.1: Recall Digital Exit Poll Sample

	Against Recall (N=466)	For Recall (N=422)	All (N=888)
Age			
Mean (SD)	56.0 (16.2)	53.6 (15.5)	54.9 (15.9)
Median [Min, Max]	57.0 [21.0, 89.0]	53.0 [18.0, 89.0]	55.0 [18.0, 89.0]
Gender			
Prefer to self-describe	7 (1.50%)	11 (2.63%)	18 (2.03%)
Man	247 (53.0%)	287 (68.5%)	534 (60.3%)
Woman	201 (43.1%)	117 (27.9%)	318 (35.9%)
Non-binary	11 (2.36%)	4 (0.955%)	15 (1.69%)
Political Ideology			
Neither liberal nor conservative	26 (5.58%)	96 (22.7%)	122 (13.7%)
Very conservative	1 (0.215%)	12 (2.84%)	13 (1.46%)
Somewhat conservative	3 (0.644%)	33 (7.82%)	36 (4.05%)
Slightly conservative	6 (1.29%)	30 (7.11%)	36 (4.05%)
Slightly liberal	39 (8.37%)	67 (15.9%)	106 (11.9%)
Somewhat liberal	179 (38.4%)	131 (31.0%)	310 (34.9%)
Very liberal	212 (45.5%)	53 (12.6%)	265 (29.8%)
Party ID			
Independent	60 (12.9%)	130 (30.8%)	190 (21.4%)
Democrat	356 (76.4%)	235 (55.7%)	591 (66.6%)
Something else	46 (9.87%)	25 (5.92%)	71 (8.00%)
Republican	4 (0.858%)	32 (7.58%)	36 (4.05%)
Race			
White	369 (79.5%)	233 (55.7%)	602 (68.3%)
Mixed	15 (3.23%)	24 (5.74%)	39 (4.42%)
Other	13 (2.80%)	18 (4.31%)	31 (3.51%)
Black	12 (2.59%)	12 (2.87%)	24 (2.72%)
Asian	23 (4.96%)	92 (22.0%)	115 (13.0%)
Hispanic	24 (5.17%)	29 (6.94%)	53 (6.01%)

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Table S1.1 – continued from previous page

	Against Recall (N=466)	For Recall (N=422)	All (N=888)
Middle Eastern	8 (1.72%)	7 (1.67%)	15 (1.70%)
Native American	0 (0%)	3 (0.718%)	3 (0.340%)
Homeowner?			
Other	21 (4.51%)	20 (4.75%)	41 (4.62%)
I own a home	233 (50.0%)	231 (54.9%)	464 (52.3%)
I rent	212 (45.5%)	170 (40.4%)	382 (43.1%)
Education			
High School	3 (0.644%)	12 (2.86%)	15 (1.69%)
Some College, No Degree	53 (11.4%)	51 (12.1%)	104 (11.7%)
Associate Degree or Bachelor Degree	194 (41.6%)	160 (38.1%)	354 (40.0%)
Master's Degree or higher	211 (45.3%)	189 (45.0%)	400 (45.1%)
Prefer not to answer	5 (1.07%)	8 (1.90%)	13 (1.47%)
Income			
Less than \$10,000	9 (2.00%)	12 (2.99%)	21 (2.47%)
$10,000-39,999$	47 (10.5%)	34 (8.46%)	81 (9.52%)
$40,000-89,999$	89 (19.8%)	76 (18.9%)	165 (19.4%)
$90,000-139,999$	96 (21.4%)	56 (13.9%)	152 (17.9%)
More than \$140,000	208 (46.3%)	224 (55.7%)	432 (50.8%)

Study 1 - Human Subjects Research Principles

All participants gave their explicit consent: "We want to invite you to participate in a research study being conducted by researchers from the [REDACTED] to consider your opinion regarding public safety in San Francisco. You can contact us through [REDACTED]. As we note in the survey, some campaign details presented are fictional, unrelated to any real District Attorney candidates. Participation in the research is completely voluntary. You are free to refuse to participate or withdraw your consent and discontinue participation in the research at ANY time." Each respondent was told: "Local district attorneys, like Chesa Boudin, are responsible for prosecuting those accused of violating the law. In this part of the survey, we want to know how you, as a resident of SF, voted on the June 7th election. All responses are anonymous. Did you vote in this election?" The survey did not intervene in political processes - respondents were only contacted after they voted or after voting was closed. Because the survey was administered through Political Data Inc., participants were not compensated in order to maintain their anonymity. Compensation would have required participants to add contact information which would have jeopardized their privacy.

Study 3 - Human Subjects Research Principles

All participants gave their explicit consent: "We want to invite you to participate in a research study being conducted by researchers from the [REDACTED] to consider your opinion regarding public safety. The survey will last approximately 7 minutes. Participation in the research is completely voluntary. You are free to refuse to participate or withdraw your consent and discontinue participation in the research at any time. As we note in the survey, some campaign details presented are fictional, unrelated to any real District Attorney candidates."[23] Lucid Theorem charges $1 per complete and pays the respondents according to contracts with suppliers not visible to the researcher.

[23] The cooperation rate was 98.1%.

Lucid Theorem Sample

	Overall (N=1030)
Age	
Mean (SD)	45.4 (17.2)
Median [Min, Max]	44.5 [18.0, 94.0]
Gender	
Female	531 (51.6%)
Male	499 (48.4%)
Political Ideology	
Neither liberal nor conservative	337 (32.7%)
Very conservative	146 (14.2%)
Somewhat conservative	127 (12.3%)
Slightly conservative	98 (9.51%)
Slightly liberal	107 (10.4%)
Somewhat liberal	104 (10.1%)
Very liberal	111 (10.8%)
Partisanship	
Democrat	460 (44.7%)
Independent/Other	234 (22.7%)
Republican	336 (32.6%)
Ethnicity	
Asian	64 (6.21%)
Black	124 (12.0%)
Native American	19 (1.84%)
Other	65 (6.31%)
Pacific Islander	4 (0.388%)
White	754 (73.2%)
Homeowner?	
I rent	422 (41.1%)
I own a home	519 (50.5%)
Other	87 (8.46%)
Education	
Some high school or less	53 (5.15%)
High school graduate	285 (27.7%)
Post high school vocational training	42 (4.08%)
Some college, but no degree	201 (19.5%)
Associate's degree	94 (9.13%)
Bachelor's degree	238 (23.1%)
Master's or professional degree	83 (8.06%)
Doctorate degree	28 (2.72%)
Other	6 (0.583%)
Income	
$0{-}19{,}999$	140 (13.6%)
$20,000{-}39{,}999$	380 (36.9%)
$40,000{-}69{,}999$	242 (23.5%)
$70,000{-}99{,}999$	155 (15.0%)
$100,000{-}149{,}999$	27 (2.62%)
$150,000+	79 (7.67%)
Other	7 (0.680%)

S2 Attitudes Scales

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Table S2.1: Attitudes Scales

	Q1	Q2	Q3	Q4
Progressive Sentiment	The proportion of Americans now serving time in the nation's prisons and jails is larger than that of any other country. In your opinion, should the U.S. . . . [Reduce the number of Americans serving time in the nation's prisons and jails; Make no changes; Increase the size of the prison and jail population]	In response to cases of police misconduct, some lawmakers have proposed reducing the amount of funding for the police. Others cite possible rising crime rates as justification for increasing police budgets. What, in your opinion, should be done about the level of funding for the police? [Reduce budgets on policing; Keep budgets on policing at about current levels; Increase budgets on policing].		

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Table S2.1 – continued from previous page

	Q1	Q2	Q3	Q4
Punitive Sentiment	Thinking about your residence area, what do you think about the current punishments for people who commit crimes? [Too harsh; Not harsh enough; About right]	We are faced with many problems in this country, none of which can be solved easily or inexpensively. When you think about halting the rising crime rate, are we spending too much, too little, or about the right amount on halting the rising crime rate? [Too little; Too much; About right]	Are you in favor of the death penalty for persons convicted of murder? [Favor death penalty; Oppose death penalty]	
Crime	Have you, or any family or friends to whom you feel close, been a victim of a crime in the past 2 years (approximately the course of the pandemic)?	How often do you feel angry about crime in your residence area?	How often do you fear someone breaking into your car?	How often do you fear being robbed?
Redeemability	Most convicted offenders can go on to lead productive lives with help from the government and the community. [Agree-Disagree]	Many offenders can turn their lives around and become law-abiding citizens if they choose to and work hard enough. [Agree-Disagree]	If a person is convicted for criminal conduct, they should never again receive the same rights other people enjoy, and they should always be treated differently. [Agree-Disagree]	

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Table S2.1 – continued from previous page

	Q1	Q2	Q3	Q4
Racial Sympathy	Michael is a young Black man who got a pat-down from a cop to see if he carries any concealed weapons. Michael is very upset by this treatment. How much sympathy do you feel for Michael? [A great deal - A little]	Laurette is a Black woman that got turned away from a nanny job for no apparent reason. Laurette is upset about the situation. How much sympathy do you feel for Laurette? [A great deal - A little]		
Racial Resentment	Irish, Italian, Jewish, and many other minorities overcame prejudice and worked their way up. Blacks should do the same without any special favors. [Agree-Disagree]	Generations of slavery and discrimination have created conditions that make it difficult for blacks to work their way out of the lower class. [Agree-Disagree]	It's really a matter of some people just not trying hard enough: if blacks would only try harder they could be just as well off as whites. [Agree-Disagree]	Over the past few years, blacks have gotten less than they deserve. [Agree-Disagree]

S3 Progressive Sentiment Scale

I measured a baseline of voters' support for reforming the criminal justice system. I asked participants to indicate their positions on two policy goals, regardless of how to achieve them. First, I tell participants: "The proportion of Americans now serving time in the nation's prisons and jails is larger than that of any other country." Then I ask them whether the US should reduce, increase, or make no changes to the size of the prisons' and jails' populations. Second, the participants read: "In response to cases of police misconduct, some lawmakers have proposed reducing the amount of funding for the police. Others cite rising crime rates as justification for increasing police budgets." Participants then indicate whether policing budgets should be reduced, increased, or stay at current levels. I combine the two questions to create a measure that places respondents on a scale of progressive criminal justice reform sentiment.

This measure was first tested using a representative sample of California voters. I partnered with the [REDACTED], for the March 29 - April 5, 2022 poll. The poll was administered online in English and Spanish, among 8,676 California registered voters. Email invitations were distributed to stratified random samples of the state's registered voters. Samples of registered voters with email addresses were provided to [REDACTED] by Political Data, Inc., a leading supplier of registered voter lists in California, and were derived from information on the official voter registration rolls. Before the distribution of emails, the overall sample was stratified by age and gender to obtain a proper balance of survey respondents across major segments of the registered voter population. To protect the anonymity of respondents, voters' email addresses and all other personally identifiable information were purged from the data file and replaced with a unique and anonymous identification number during data processing.

Figure S3.1 shows the average outcome per notable California counties, which gives simple face validity to the measure. Alameda county (Oakland), San Francisco, and Los Angeles have the highest proportion of progressive and mostly progressive voters. San Diego is close but less overwhelming. Orange county is a marker of a contested county in which no attitude is prominent. On the other hand of the spectrum, as expected, the most conservative counties in California also seem to be the least progressive on criminal justice issues: Lassen and Modoc (Trump 2020 vote share was about 70% in both counties).

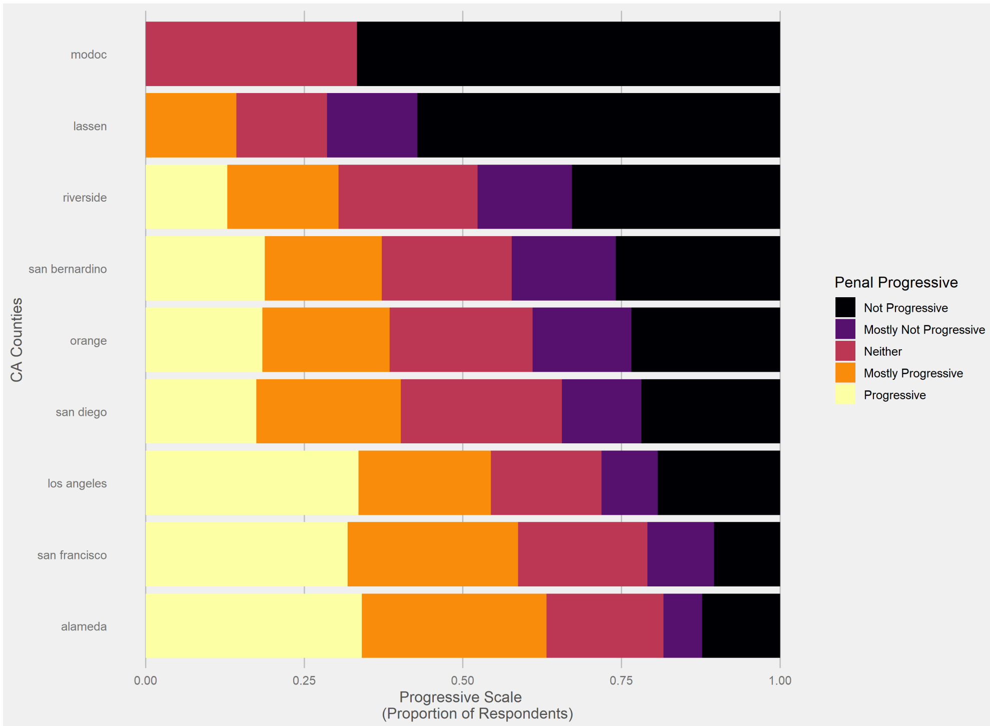

Figure S3.1: Statewide Voter Responses on Progressive Scale

Based on the statewide sample, Cronbach's Alpha turns out to be 0.727. the 95% confidence interval for Cronbach's Alpha is [0.714 0.740]. In the digital exit poll sample, Cronbach's Alpha is 0.751 [0.710, 0.791]. This is considered an acceptable level of internal consistency.

Another measure of validity can be the correlation with the punitive sentiment scale. Although, as this paper argues, these are separate and independent theoretical constructs, we can expect that usually, people who support harsher courts, the death sentence, and spending more to stop a rising crime rate would not also support ending mass incarceration and de-funding the police. Indeed, in the digital exit poll sample, the correlation is negative and statistically significant, $r(853)$ = -0.628, $p < .0001$. In the nationwide sample, the correlation is less pronounced but still strong, $r(951)$ = -0.386, $p < .0001$.

S4 DA Agendas - Examples

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Table S4.1: DA Agendas - Examples

DA	Statement	Construct	Source
Chesa Boudin - Reject the Recall Campaign (2022)	“Prosecute those who commit criminal acts, including violent crime, robberies, burglaries, and car break-ins, as well as police misconduct, retail theft, corporations exploiting workers, and hate crimes”	Get-Tough	https://www.chesaboudin.com/
Chesa Boudin - Original campaign website (2019)	“Stop using our jail as an ineffective and inhumane mental health facility. “ “Two-thirds of the cases that go to trial in San Francisco are misdemeanors – a tremendous waste of resources.”	Reduce Extent	https://web.archive.org/web/20191110134340/https://www.chesaboudin.com/issues
Larry Krasner - Reelection campaign (2021)	“He has shifted resources to solving serious crimes: Larry is actively working with city partners, non-profits, and doctors to explore public health solutions to gun violence, and to help the police increase its solve-rate for shooting crimes, as it currently hovers at 20%.”	Reduce Extent (with cooperation from external partners)	https://krasnerforda.com/promises-kept#

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Table S4.1 – continued from previous page

DA	Statement	Construct	Source
Larry Krasner - Original campaign (2017)	"As District Attorney, Larry Krasner will fundamentally change that culture, from a culture of seeking victory for prosecutors to a culture of seeking justice for victims." "While re-focusing the DA's Office on prosecuting serious crimes, he will seek alternatives to incarceration"	Reduce Intensity	http://web.archive.org/web/20170516210300/https://krasnerforda.com/platform/
Brooke Jenkins - Interim San Francisco DA (2022)	"reintroducing the option for prosecutors to charge 16- and 17-year-olds as adults in certain "egregious cases," a departure from former District Attorney Chesa Boudin's policy that banned the office from seeking adult charges for juveniles in any circumstance." Jenkins: "I don't believe that we should eliminate our discretion, that there may be some instances, that involve egregious circumstances, where we don't believe that we can rehabilitate somebody"	Get-Tough	https://www.sfchronicle.com/sf/article/S-F-D-A-Jenkins-to-seek-charges/-of-16-and-17438806.php
John Hamasaki - DA candidate (2022)	"intervening early to prevent crimes rather than simply waiting to prosecute crimes that could have been prevented with smart programs like keeping young people in school, job training, mental health treatment and other proven ways to reduce crime"	Reduce Extent	https://hamasakiforda.com/
John Hamasaki - DA candidate (2022)	"Investigate & prosecute the sale of dangerous drugs" "crack down on car break-ins" "Prosecute commercial burglaries"	Get-Tough	https://hamasakiforda.com/issues/

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Table S4.1 – continued from previous page

DA	Statement	Construct	Source
Kim Foxx - reelection campaign (2020)	"We are no longer punishing people simply because they are poor and cannot afford to pay their tickets. " "We prioritized violent crime and keeping our communities safe, rather than using resources to prosecute non-violent, low-level offenders."	Reduce Extent	https://www.kimfoxx.com/priorities
The Fair and Just Prosecution - A network of progressive DAs.	"Recent elections ushered in a new generation of prosecutors committed to changing office culture, enhancing transparency and accountability, and embracing prevention-oriented approaches to public safety that are rooted in local communities, based on data and evidence, and less punitive whenever possible."	Reduce Intensity	https://fairandjustprosecution.org/about-fjp/our-work-and-vision/

S5 Predictors of Vote Choice

Visualizing the Predictors of Vote Choice

Figure S5.1 presents the relationship visually and includes demographic variables. Compared to voters identifying as White, Asian voters were statistically significantly more likely to support the recall. I find no statistically significant effect for reported gender, age, partisanship, or home ownership.

Explaining the predictors of vote choice

As most research on the politics of crime stemmed from the rise in incarceration rates in the late-modern US, attitudes have been conceptualized by their respective position on incarceration, capital punishment, and public spending on law enforcement (Duxbury 2021; Enns 2016; Ramirez 2013). The literature focused on the progressives-punitive attitudes divide (Cullen, Fisher, and Applegate 2000; Pickett and Baker 2014; Unnever and Cullen 2010; Unnever, Cochran, et al. 2010). While progressives are expected to support reducing the reliance on incarceration, oppose the death sentence, and not support increased law enforcement spending, punitive individuals were traditionally conceptualized as the opposite. This paper contributes to this literature and theorizes that there is a politically crucial divide between two progressive agendas. The first seeks to lower only the intensive margin: an attitude that supports reducing the harshness of outcomes, yet not at the expense of reducing government intervention (for example, reducing the reliance on incarceration but not police funding). The second seeks to lower the extensive margin: a position that seeks to replace criminal legal system intervention with other, non-penal government responses.

Punitiveness is a challenging concept to measure (Adriaenssen and Aertsen 2015). There is "no adequate measure of the public's preferences for being tough on crime." (Enns 2016). Measures sought to make sense of public opinion on criminal justice have been based on either single ideological measures of punitive sentiment (such as opinions about the courts' harshness), specific policy sentiments (such as support for the death penalty), or aggregate estimates of public opinion regarding specific policy prescriptions. Aggregate estimates are preferred, as single indicator measures of policy opinion risk overstating temporal variation in punitive attitudes and underestimating the effects of punitive opinion on policymakers (Pickett 2019). Researchers also identified the relationship between change over time in citizens' punitive sentiment and policy outcomes such as incarceration rate (Enns 2016).

The first prediction for vote choice is the most commonly used measure of crime control attitudes - punitive sentiments, measured through a respondent assessment of abstract claims, most famously "how tough are the courts" (Enns 2016; Duxbury 2021). Ramirez 2013, defines the concept of punitive sentiment at the macro level as "the aggregate public support for criminal justice policies that punish offenders." The assumption that punitive sentiment, and more importantly, change in punitive sentiment, impacts policy relies on research that shows policy responsiveness to public attitudes as evolving from lawmakers anticipating the types of policies—rather than specific policies the public prefers (Bartels

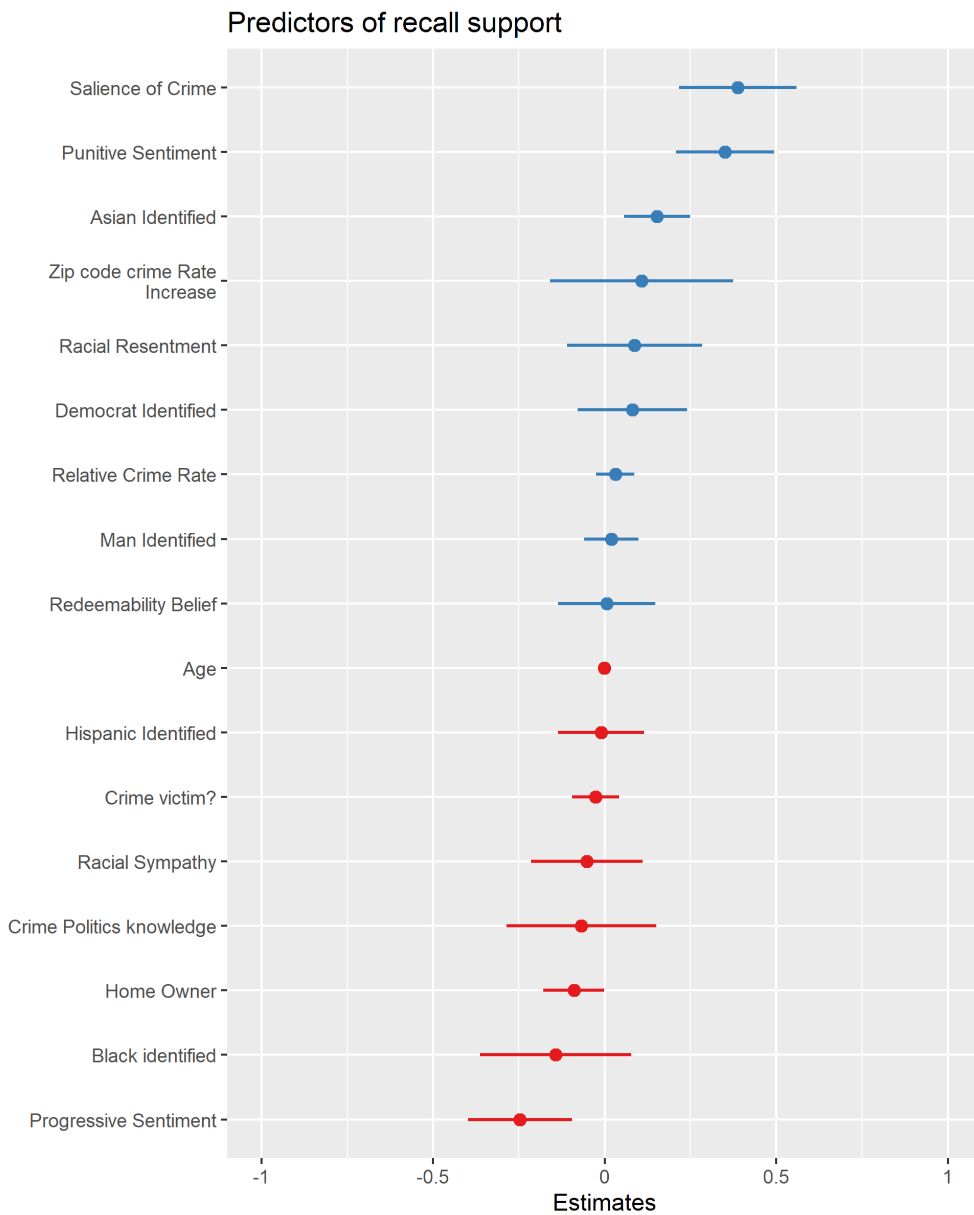


Figure S5.1: Coefficient estimates - multivariate model with demographics

and Stimson 1992; Stimson 2004; Stimson, Mackuen, and Erikson 1995). Enns 2016 demonstrated that rising public punitiveness from 1960 to 2010 significantly increased the national incarceration rate. American public's punitive sentiment is also known to move in parallel trend when accounting for race, political ideology, and gender (Duxbury 2021; Enns 2016; Ramirez 2013). Ramirez 2013 finds highly correlated trajectories in punitive sentiment for blacks and whites, men and women, and different age groups at the national level.

The second attitude that might inform vote choice is the belief that crime is rising. The punitive sentiment is said to follow people's beliefs about crime rates. Parallel shifts in punitive sentiment result from pronounced period effects where high violent crime rates and disproportionate crime news coverage increased American punitiveness at large (Enns 2016). The extant research shows that people's support for "tough on crime" politics changes in response to changes in the crime rate, and "both criminal justice policy and practice respond to opinion movements" (Pickett 2019). Thus, voting behavior in District Attorney elections might hinge on voters' punitive sentiment, which is affected by the crime rate and voters' personal feeling that crime is rising.

Researchers find significant gaps when accounting strictly for the difference between groups in cross-sectional studies. Racial differences in punitive sentiment are particularly pronounced. Between 1953 and 2006, only 11 percent of black respondents in 34 national polls supported capital punishment for convicted murderers, in contrast to 89 percent of non-blacks (Shirley and Gelman 2015). Women are less likely to support harsh criminal sanctions than men (Cochran and Sanders 2009). Anderson, Lytle, and Schwadel 2017 shows that support for capital punishment increases with age until respondents reach approximately 50, when death penalty support begins to decline. This leads to a third prediction: the demographics of voters predict the outcome.

Finally, people's support for tough-on-crime policies in America is interrelated with racial attitudes (Pager 2008; Tonry 2011). Weaver (2007) showed that political elites could influence how the public thinks about crime - following the civil rights movement, politicians helped bring race and crime together in the public's mind (Weaver 2007). Research consistently showed that racial animus is correlated with a preference for oppressive attitudes towards punishment, and the racial divide is meaningful for attitudes towards punishment and justice. Evidence range from support for the death penalty (Barkan and Cohn 1994; Messner, Baumer, and Rosenfeld 2006; Trahan and Laird 2018; Unnever, Cullen, and Jonson 2008), through abstract support for "get tough" politics (Brown and Socia 2017; Buckler, Wilson, and Salinas 2009; Morris and LeCount 2020; Unnever and Cullen 2010), to how group threat fueled the adoption of excessive punishment is America (Chiricos, Pickett, and Lehmann 2020; Duxbury 2020).

S6 Descriptive Findings - California Voters

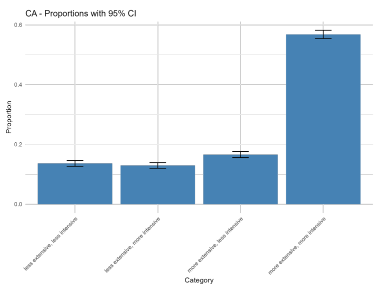
A survey of California voters was conducted to improve our understanding of voters' attitudes along the extensive and intensive margin. Respondents were asked to select their preferred general "policy package." Full details are in 2. The full distribution of preferences presented here supports the following conclusions regarding the cross-sectional attitudes of voters:

1. California voters, in general, can be described as punitive - more than 50% of voters prefer to increase both the extensive and intensive margins S6.1.

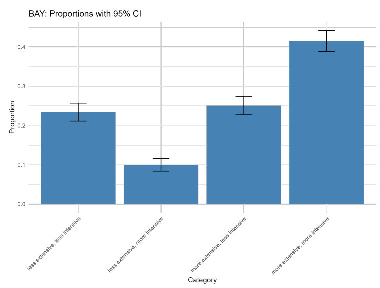
2. However, the other 50% of voters are almost evenly distributed between the three "progressive" options: There is no overwhelming consensus regarding which margin

progressive voters wish to lower; there is some small advantage to the option that includes lowering the intensive margin but not the extensive margin S6.1.

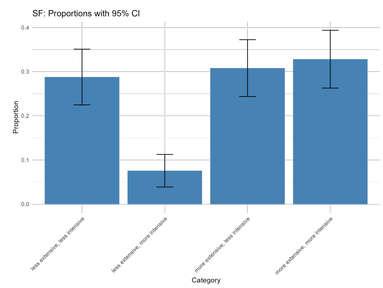
3. We witness a stark difference When examining only respondents from the Bay Area S6.1b and, to a much higher degree, only San Francisco Voters S6.1c. Specifically in San Francisco, the most prominent finding is the opposition to lowering both margins. In accordance with this article's main findings, Progressive voters are about evenly split between lowering both margins and lowering just the intensive margin. The broader sample of Bay Area voters reveals similar findings but with more "classic" punitive voters.



(a) California Proportions



(b) Bay Area Proportions



(c) SF Proportions

Figure S6.1: Descriptive Proportions of Preferences

S7 Study 1 - Additional Information and Analysis

The experimental condition appeared after collecting the covariates. The average verified reads of the original sent emails were 25.71%, and during the reminder phase it was 13.4%. Out of the verified email reads, the average click in the survey link rate was 9.25% and 11.43% in the reminder phase. Results for the entire sample are visualized in Figure S7.1:

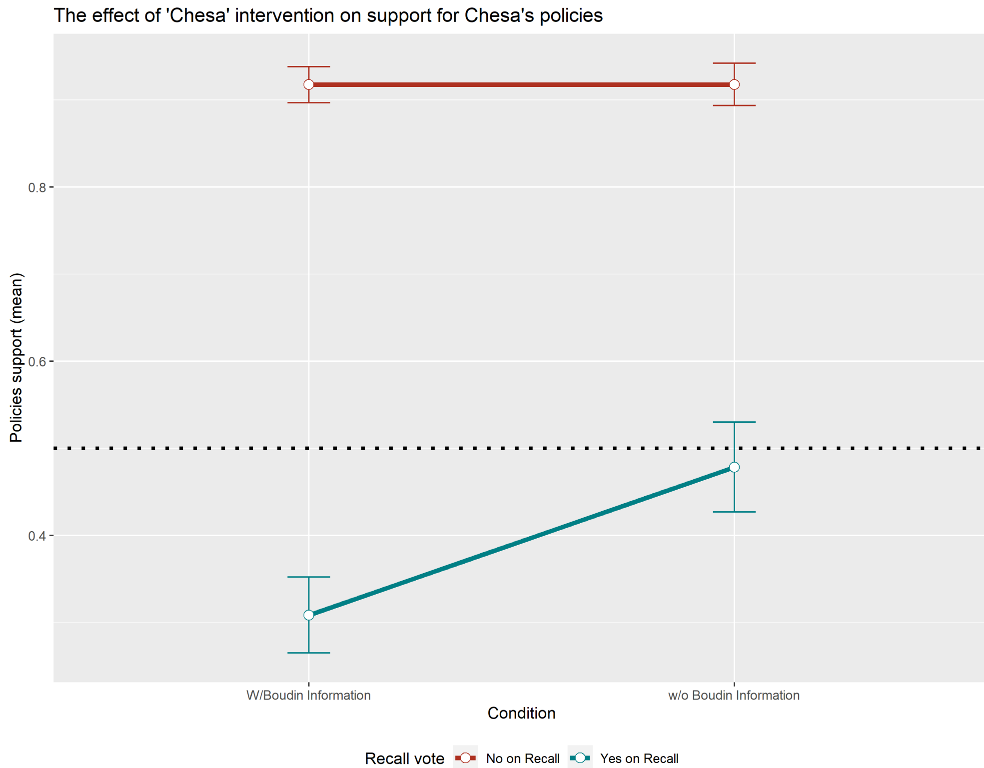


Figure S7.1: Chesa Context - Experiment Results

I conducted the main analysis using OLS regression with HC2 robust standard errors. The analysis tests an interaction model of vote choice and the experimental condition.

$$(1) Policy\bar{S}upport_i = \beta_0 + \beta_1 \text{w/Boudin Context} + \beta_2 \text{Recall Supporters} + \beta_3 \text{w/Boudin Context} \times \text{Recall Supporters} + \epsilon_i$$

Results

The results of the interaction model in Table 2.2 show that voters who both supported the recall and received the condition that the policies were "Boudin's policies" were significantly less likely to support the policies. The treatment did not affect voters who opposed the recall - they showed a consistent policy-voting attitude.

S8 Study 2 - Full Analysis

The experimental condition appeared after collecting the covariates. I conducted the analysis using OLS regression with HC2 robust standard errors. The results are presented in Table S8.1. To test the individual effect of each treatment condition against the control condition, model 1 estimated:

SI Table 2.2: The effect of 'Chesa Boudin' on Progressive Policies Support

	Support for Chesa's Policies (average)
Recall opponents x Boudin context	0.001
	[-0.032, 0.033]
	t = 0.042
	p = 0.966
Recall Supporters x Boudin context	-0.167
	[-0.243, -0.092]
	t = -4.350
	p < .001
Num.Obs.	849
R2	0.498

Note: The model reports the result of the effect of the experimental conditions on the average support of four progressive policies that Chesa Boudin implemented. Standard errors are clustered at the respondent's level.

$$\text{(1) Candidate Support}_i = \beta_0 + \beta_1\text{Get-Tough} + \beta_2\text{Reduce Extent} + \beta_3\text{Reduce Intensity} + \epsilon_i$$

To test whether the Reduce Extent and Reduce Intensity constructs differ, model 2 estimated the effect of each condition against the Reduce Intensity condition as a reference category:

$$\text{(2) Candidate Support}_i = \beta_0 + \beta_1\text{Get-Tough} + \beta_2\text{Reduce Extent} + \beta_3\text{Control} + \epsilon_i$$

To test heterogeneity in the treatment effect, models 3 and 4 repeated the estimation of model 1 but separately for voters who supported and opposed the recall. The reference category is, as in Model 1, the control condition.

Table S8.1: Effects of experimental conditions on supporting a DA candidate

	All voters		Recall supporters	Recall opponents
	(1)	(2)	(3)	(4)
Reduce Extent	-0.034	**-0.108**	**-0.391**	**0.281**
	[-0.093, 0.025]	[-0.166, -0.049]	[-0.466, -0.317]	[0.213, 0.350]

Continued on next page

Table S8.1 – continued from previous page

	All voters		Recall supporters	Recall opponents
	(1)	(2)	(3)	(4)
	t = -1.123	t = -3.607	t = -10.336	t = 8.058
	p = 0.262	p < .001	p < .001	p = 0.000
Reduce Intensity	**0.074**		**-0.099**	**0.209**
	[0.019, 0.128]		[-0.173, -0.024]	[0.138, 0.280]
	t = 2.662		t = -2.609	t = 5.815
	p = 0.008		p = 0.009	p < .001
Get-Tough	**-0.241**	**-0.314**	**-0.152**	**-0.344**
	[-0.302, -0.179]	[-0.375, -0.254]	[-0.236, -0.068]	[-0.397, -0.291]
	t = -7.722	t = -10.195	t = -3.568	t = -12.791
	p < .001	p < .001	p < .001	p < .001
Num.Obs.	885	885	421	464
R2	0.116	0.116	0.195	0.483

Note: All models report the effect of experimental conditions on supporting a hypothetical DA. Models 1, 3, and 4 use the Control condition as the reference category; model 2 uses Reduce Intensity. Standard errors clustered at respondent level.

S9 Study 3 - additional analysis

Placebo tests

Study 3 used multiple realizations of treatment conditions as a placebo test for the effect of the treatment constructs. As in the digital exit poll, respondents were randomized into one of: Reduce Extent, Reduce Intensity, or Get-Tough. This time, they were also randomized again into one of three versions of the treatment conditions. While the original conditions Subjects were: "first-time, nonviolent low-level criminal defendants," the placebo tests replaced the subjects with "Homelessness and drug use" and with no mention of behavior. For the Reduce Intensity conditions, the original "rehabilitation bootcamps" were replaced with "Community work and fines" and with no mention of a specific policy alternative. Table 2.3 details the exact wording.

SI Table 2.3: Placebo Test - Multiple Realizations

Construct	**Placebo 1**	**Placebo 2**
"Reduce Extent": Reforming the criminal legal system to minimize its scope - to take less action and leave some issues to other branches of the government.	A new possible candidate promised to keep criminals accountable. The candidate wants to downsize the conviction of nonviolent behaviors such as homelessness and drug use. According to the candidate: "Some offenders deserve help, not punishment, my office will not concern itself with taking such low-level offenses to court!"	A new possible candidate promised to keep criminals accountable. The candidate wants to stop pressing charges in some cases. According to the candidate: "Some offenders deserve help, not punishment, my office will not concern itself with taking such low-level offenses to court!"
"Get-Tough": Reforming the criminal legal system in a "get tough" direction - harsher outcomes.	A new possible candidate promised to keep criminals accountable. The candidate wants to increase prosecutions of nonviolent behavior, such as homelessness and drug use. According to the candidate: "These offenders do not belong in our city, my office will deter them by lengthening sentences and removing them from our streets!"	A new possible candidate promised to keep criminals accountable. The candidate wants to expand and press more charges. According to the candidate: "These offenders do not belong in our city, my office will deter them by lengthening sentences and removing them from our streets!"
"Reduce Intensity": Reforming the criminal legal system to reduce the harshness of outcome - replacing traditional imprisonment solutions with different initiatives.	A new possible candidate promised to keep criminals accountable. The candidate wants to exchange jail sentences for community work, supervision, and fines for nonviolent behavior such as homelessness and drug use. According to the candidate: "Some offenders do not belong in prison, my office will supervise them in the community!"	A new possible candidate promised to keep criminals accountable. In some cases, the candidate wants to replace pressing charges with alternatives to punishments. According to the candidate: "Some offenders do not belong in prison, my office will deal with them in new ways!"

The result of the placebo test confirms that there were no outstanding effects to any specific treatment version $\chi^2(2) = 2.066, p = 0.3559$. Every version had the same effect on the outcome.

Propensity Score Matching results for Study 3

To test for heterogeneity in treatment effect between respondents, I match respondents from the national sample to San Francisco voters. To pair observations, I construct propensity scores based on: average support for the four progressive policies, progressive and punitive sentiment, the salience of crime, and racial attitudes. Pairs were identified using MatchIt (Stuart et al. 2011) and 1:1 nearest neighbor matching without replacement. A distance is computed between each San Francisco unit and each national sample unit, and each San Francisco unit is assigned a national sample unit as a match. This method results in 835 observations from the national sample, each corresponding to a single observation from the San Francisco sample.

Figure S9.1 illustrates the results graphically for the entire sample by estimated vote choice. As expected, the results replicate Study 2 findings. The two groups of respondents differ significantly. Respondents similar to recall supporters supported the Reduce Extent candidate significantly less than the respondents similar to recall opposers. A similar yet reversed pattern emerged for the Get-Tough candidate. Finally, as expected, respondents from both sides are indistinguishable regarding their support for the Reduce Intensity candidate.

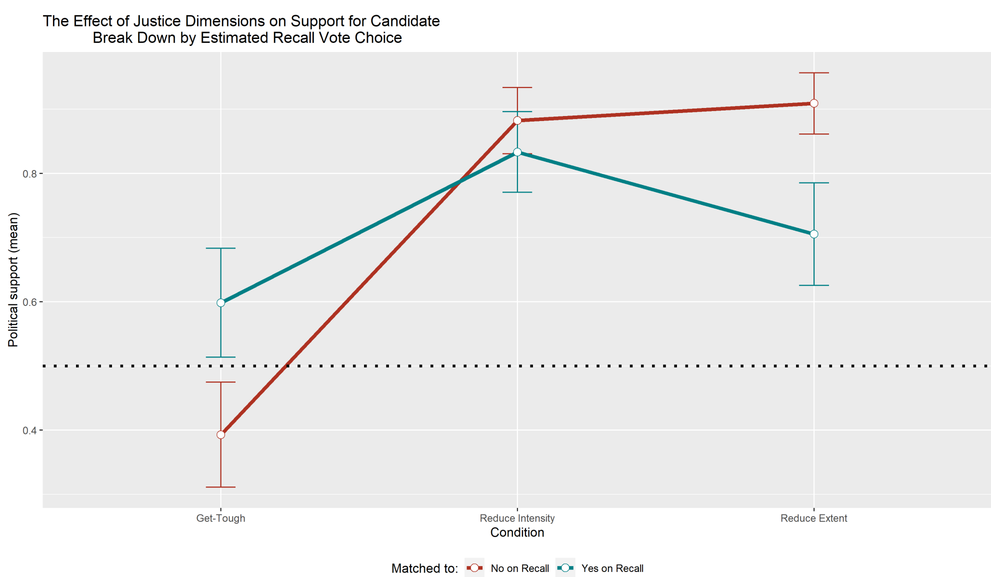


Figure S9.1: Treatment conditions effect on a national sample matched to San Francisco voters

S10 Computational analytical approaches - Additional Information

Random Forest

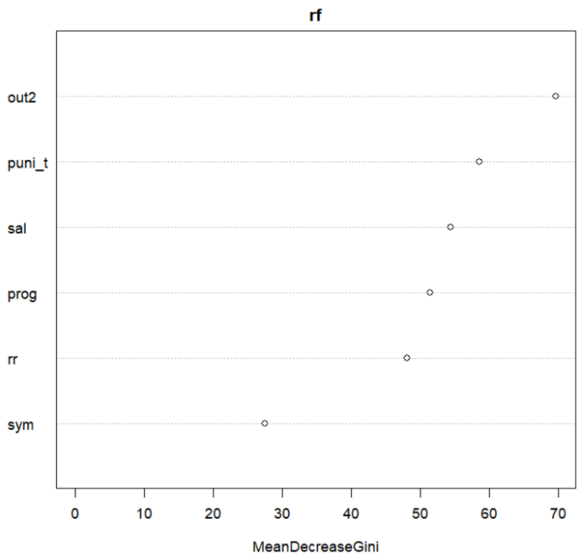
One purpose of Study 3 is to test whether a national sample would exhibit attitudes similar to those of the San Francisco sample. This article argues that San Francisco voters who voted to recall the DA did so for one of two reasons: opposition to reducing both the intensive and extensive margins or to reducing only the extensive margin of the criminal legal system. Estimating vote choice is required to test whether this result replicates outside of San Francisco.

Random Forest is a classification method, an algorithm that relies on repeated data partitioning to estimate the conditional distribution of a response given a set of explanatory variables. Random Forest can be used with all outcome variables and does not require distributional assumptions (Jones and Linder 2015). The importance of variables can be assessed by their impact on the accuracy of predictions, which allows for a quick assessment of the relevance of a predictor for the outcome of interest. In this case, the algorithm estimates the respondents' "vote choice" based on measures collected in Study 1: punitive sentiment, crime salience, progressive sentiment, racial attitudes, and average support of Boudin's policies are significant predictors of vote choice (Table 2, Figure S5.1).

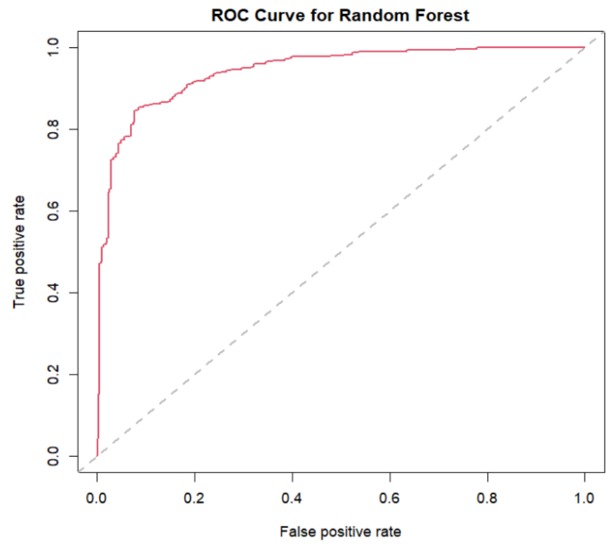
The algorithm works by considering every unique value in each predictor as a candidate for a binary split and calculating the homogeneity of the subgroups of the outcome variable that would result by grouping observations that fall on either side of this value. Using the randomForest package in *R* (version 4.7-1.1) with 2,000 Trees and one variable randomly sampled as candidates at each split, the algorithm aims to find homogeneous partitions of the outcome (vote choice).

I provide the performance metrics of a single random forest model generated through the same process used in the main analysis. The main analysis shows that my results are robust to using 100 randomly generated random forest models to overcome concerns about model selection. Nonetheless, to evaluate the accuracy of the specification I use, I provide the performance metrics of a model generated using a training set and a separate test set.

The OOB estimate of error rate is 11.98%. The AUC is 0.943 (0.8 to 0.9 is considered excellent, and more than 0.9 is considered outstanding). The predictors' significance and the ROC curve are presented in Figure S10.1.



(a) Predictors' significance



(b) ROC Curve

Figure S10.1: Performance metrics

Text Summarization Algorithms

Study 3 collects open-text responses to understand why respondents support or oppose a specific "kind" of hypothetical DA candidate. The assumption is that respondents who choose a specific response under an experimental condition do it for similar reasons. In other words, we assume that if a group of people supported a candidate, then not everyone had a unique reason to do that; instead, the group holds some underlying similar, limited set of reasoning methods.

I use computational text summarization to uncover the similarities between responses from respondents who received the same experimental condition and chose the same outcome variable. I chose to use BART (Bidirectional Autoregressive Transformer), a powerful Transformer-based encoder-decoder model that is particularly effective for text summarization tasks.

BART is pre-trained under self-supervision on a large text corpus, allowing it to learn natural language's complexities and nuances. During pre-training, BART learns to reconstruct the original text from corrupted or noisy input, which allows it to generate linguistically correct sequences even when the input text is erroneous or missing information.

For text summarization, BART can be fine-tuned on a dataset of documents and their corresponding reference summaries. The model takes the full document as input, encodes it using the bidirectional Transformer encoder, and then generates a summary using the autoregressive Transformer decoder. Specifically, I use the following model in this study: https://huggingface.co/facebook/bart-large-cnn. This model was pre-trained on English language, and fine-tuned on CNN Daily Mail (Lewis et al. 2019).

keyness statistics

Keyness analysis is a statistical method used to identify linguistic items, typically words, with significantly different frequencies between two corpora. The key idea is to compare the observed frequencies of words in a ”study” corpus against the expected frequencies based on a ”reference” corpus to identify words that are unusually frequent or infrequent in the study corpus (Gabrielatos 2018). However, a researcher can use a survey experiment with an open text-dependent variable to test for differences in the responses, an approach used in this study.

The most common approach is to use a statistical significance test, such as the chi-square (χ^2) or log-likelihood (G^2) test, to determine which words have a statistically significant difference in frequency between the two corpora. The resulting ”key” words have a very low p-value, indicating that the observed frequency difference is unlikely to have occurred by chance.

This study applies the keyness method to compare respondents' words in response to a survey experiment. One group opposes a hypothetical DA vowing to take one approach, and the other opposes a hypothetical DA vowing to take a slightly different approach (the experiment manipulates which margins are reduced or not). This analytical approach allows me to formally test the hypothesis that the reasons for opposing one kind of candidate are different from the reasons to oppose a different kind of candidate. An alternative would be to use a closed-end response with a fixed set of possible options for why a respondent opposed a candidate; without knowing the full range of possible reasons a person would oppose any candidate, this closed-ended approach is not advisable (Krosnick 2018).

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Chapter 3

Policing during Prosecutor Elections

11 Introduction[1]

District Attorneys (DAs) are the heads of the prosecutor's office. They are the elected officials who determine the jurisdiction's approach toward criminal justice policy. American prosecutors represent local jurisdictions and enjoy independence and a wealth of discretion in how criminal statutes are applied (Sklansky 2018; Tonry 2012). Typically, state prosecution is organized along county lines under the direction of an elected and autonomous prosecutor, designated as county attorney, district attorney, or state attorney. During the mass prison expansion between 1970 and 2000, the prosecutor's power has expanded considerably compared to judges, defense attorneys, and other actors in the criminal justice system (Simon 2007).

In recent years, the politics of prosecutor elections have changed dramatically. It used to be common wisdom that prosecutor elections are apolitical: rarely contested (Pfaff 2017; R. F. Wright 2014; Bibas 2016), and incumbents "win until they quit" (Bazelon 2020). However, new studies point toward a change in trend. In a study of prosecutor elections in 200 high-population districts in the US between 2012 and 2020, Wright and others find that the likelihood that an incumbent would run unopposed "fell by roughly eight percent for each passing year." (Wright, Yates, and Hessick 2021). Similarly, Hessick and Morse collected election results (2014 or 2016 cycle) for 2,315 districts across 45 states and found that in urban jurisdictions, elections were more likely to be contested and competitive (Carissa Byrne Hessick and Morse 2019).

In addition, some new DAs and DA candidates recently began making political promises to transform the criminal justice system from within through new visions of the district attorney's job (Wright, Yates, and Hessick 2021; Davis 2019). Colloquially, reform-minded DAs are known as "progressive prosecutors." By the beginning of 2023, America's five biggest cities by population have elected progressive district attorneys (including Los Angeles, Philadelphia, Boston, New York, Chicago, and Houston) (Carissa Byrne Hessick and Morse 2019).[1] A key tenet of the progressive prosecutor movement is police accountability (Sklansky 2016; Sklansky 2017); unintended consequences could emerge if, in response to heightened police accountability, public safety decreased. Furthermore, measuring the effect of progressive prosecutors' policies on crime may be confounded by any changes in police behavior that affect crime or crime reporting.

The changing nature of prosecutor politics, namely, the combination of increased competitiveness and attention from voters with a new political movement aimed at reform, might impact public safety unexpectedly due to its effect on police-prosecutor relationships. This relationship has been shown to affect police use of force (Stashko and Garro 2023). Legal scholars, practitioners, and advocacy groups have advocated for regulatory measures that circumscribe the involvement of DAs in cases pertaining to local law enforcement officers. In the vast majority of jurisdictions, cases implicating local law enforcement personnel fall

[1] Thanks to Anna Kyriazis and Lauren Schechter for coauthoring this project. The project is in a Working Paper status.

[1] New York has five district attorneys, one for each county/borough. New York County (Manhattan) and Kings County (Brooklyn) have elected progressive prosecutors.

under the purview of the resident DA. This dynamic, coupled with the orientations of contemporary progressive prosecutors, presents salient legal and policy-related considerations.

In this paper, we investigate the implications of a contentious DA recall election on San Francisco's policing, underscoring how political dynamics can shape police outcomes. Utilizing an Interrupted Time Series (ITS) estimator, with a discontinuity at the recall date, we identify significant pre- and post-recall behavioral shifts. While there is a pre-election decline, post-election police activities such as stops and felony arrests notably surge. However, citizen-initiated interactions with law enforcement remain essentially unchanged. A secondary yet salient observation is the pre-election decrease and post-election ascent in jail populations. The sole behavioral alteration in the prosecutor's office is an uptick in post-recall dismissals, hinting at supply-driven changes in prison populations. To bolster our findings' robustness, we provide bandwidth sensitivity analysis and a placebo test employing prior-year data to support our conclusions further.

12 Related Literature

We explore in this paper a model of police performance in the context of prosecutor elections. Police officers might explicitly or implicitly react to an upcoming prosecutor election. Specifically, they are incentivized to affect the voters' perceptions of the incumbent's performance. If the incumbent is up for reelection, police officers might improve their performance to improve public perceptions of the incumbent's effect on public safety or lower their performance to achieve the opposite results. Historically, prosecutors and police departments were working hand-in-glove, but the recent reform movement disrupted this relationship as it introduced prosecutors more critical of police accountability.

The burgeoning literature on prosecutor-police dynamics hints at significant behavioral interdependencies. Specifically, a notable study indicates that police officers might recalibrate their use of lethal force following the ousting of an incumbent DA, potentially due to uncertainties surrounding new DA relationships (Stashko and Garro 2023). Such ties often harbor conflicts of interest, with instances of questionable collaborations and campaign contributions underscoring the intricate nexus between prosecutors and law enforcement.[2] Our work augments this discourse by elucidating the profound implications of the police-prosecutor relationship on routine policing outcomes and its cascading effects on citizens' incarceration tendencies.

Previous studies utilizing comparable research designs have delved into how policing behaviors shift post-events like high-profile police shootings or subsequent protests. Evidence suggests transient reductions in police stops post such incidents, with no corresponding short-term crime spikes (Shjarback et al. 2017; Abrams, Fang, and Goonetilleke 2022; Cho,

[2]For instance, amidst an investigation involving the Fremont police union's leadership, DA Nancy O'Malley of Alameda County, California, received a $10,000 donation from the union during her re-election campaign. The subsequent exoneration of the officers in question exemplifies these conflicts (Carissa Bryne Hessick and Rossi 2018). The resultant calls for campaign finance reforms stem from perceived erosion in public trust and concerns over systemic shortcomings in addressing police misconduct (Westervelt 2020).

Gonçalves, and Weisburst 2021). Post George Floyd's murder, Minneapolis police exhibited a marked discontinuity in reporting race and gender data during stops—a decline from roughly 71% to 35%, a decrease of approximately 36 percentage points (United States Department of Justice, Civil Rights Division and United States Attorney's Office, District of Minnesota, Civil Division 2023). Our research underscores the responsiveness of policing to political landscapes, especially concerning calls for police accountability from prosecutorial entities. Numerous studies have delved into the nexus between accountability frameworks and police efficacy. While some found evidence of "de-policing" after the establishment of community oversight bodies or in the wake of external controversies (Ali and Nicholson-Crotty 2021; Mikdash 2022; Ba and Rivera 2019), others indicate crime surges following governmental inquiries into high-profile police incidents (Devi and Fryer Jr 2020). Furthermore, evidence suggests diminished arrest rates after targeted ambushes on officers (Sloan 2019). Given these findings, we posit that evaluations of legal system shifts, such as the induction of a new DA or prosecutorial policy changes, necessitate thoroughly examining consequent arrest patterns and nuances.

Our study exploits the June election to estimate a change in police behavior across multiple outcomes. In addition, we also estimate downstream effects on the San Francisco jail population. An existing strand of literature documents the effects of pre-trial detention and other short jail stays on defendants' future outcomes. Defendants detained pre-trial are more likely to plead guilty, less likely to engage in pre-trial misconduct (because of incapacitation) but more likely to recidivate, and are less likely to earn formal-sector income, receive government benefits, or file taxes than their counterparts who are not detained (Dobbie, Goldin, and Yang 2018). Furthermore, Black defendants sentenced to even short jail stays are less likely to vote following their incarceration sentences (White 2019). To the extent that these effects hold in San Francisco, these transitory changes in the jail population due to police responses to local political events could substantially affect defendants' subsequent outcomes.

13 Background

Opposition combined with high (and often confused) expectations from voters poses significant challenges to the new wave of district attorneys (Davis 2019; Cox and Gripp 2021). In one study, interviews with assistant district attorneys (the rank-and-file prosecutors) in a progressive office described a legitimacy crisis (Cox and Gripp 2021). Studies show that DAs are more punitive in an election year (Bandyopadhyay and Mccannon 2014; Dyke 2007; Nadel, Scaggs, and Bales 2017; Okafor 2021). This study explores the possibility of police officers' political opposition during a publicized conflict between San Francisco's progressive prosecutor and the SFPD before the prosecutor's recall election.

Documented incidents of excessive police violence against laypeople caused unprecedented protests against police violence in America and raised attention to police misconduct. Progressive prosecutors often sided, publicly, with protesters and vowed to press charges against police officers zealously (Holland and Zeidman 2023). When police officers face stricter legal scrutiny, they might avoid certain activities, thus affecting public safety. There are recorded instances of what is colloquially known as a "blue flu" - a type of strike action undertaken by police officers in which many simultaneously use sick leave - during calls to "defund the police" after the murder of George Floyd and when officers were prosecuted (Grim 2020; Hansen 2020). Moreover, police departments facing the threat of heightened legal accountability might choose to influence the elections of progressive prosecutors into office.

Chesa Boudin was elected to the DA's office on November 5, 2019. During Boudin's election campaign, The San Francisco Police Officers Association paid for ads calling Boudin "the #1 choice of criminals and gang members." In December of that year, Boudin's office charged a police officer in the first known excessive-force prosecution in the city's history. Moreover, on June 5, 2020, the DA's office released an official statement announcing "New Appointment and New Policy Designed to Protect the Public From Police Misconduct and Abuse."[3] In the statement, Boudin is quoted saying: "the national movement that has ignited around police abuse has illustrated the importance of having someone who deeply understands how to hold police accountable."

The recall efforts began on January 2, 2021. Richie Greenberg, a former Republican mayoral candidate, started a petition to recall Boudin. In August 2021, this recall attempt fell short due to a failure to achieve the required signatures from city residents. A second, separate campaign to recall Boudin started on April 19, 2021. This time, Mary Jung, former chair of the San Francisco Democratic Party, becomes chair and pitches the campaign as led by Democrats who support criminal justice reform but believe Boudin is ineffective. On November 9, 2021, this recall initiative forced a recall election after 83,000 signatures were gathered. The recall election is set for June 7, 2022. If the recall election results force Boudin out of office, Mayor London Breed would get to choose his successor. Mayor Breed supported Boudin's opponent during the 2019 election for DA. Boudin and Breed later clash after the Mayor declares a "state of emergency" in the high-crime neighborhood of Tenderloin while Boudin maintains that: "We can't arrest and prosecute our way out of problems that are afflicting the Tenderloin."

In early 2022, Boudin's office entered a conflict with the San Francisco Police Department (SFPD), which received broad media coverage. A 2019 agreement between SFPD and the DA's office made the DA's Office the lead investigating agency in police use-of-force incidents, police shootings, and in-custody death cases. The agreement was amended and signed by Boudin and the SFPD Chief in 2021. However, in January 2022, the police Chief said he intended to pull out of this memorandum of understanding.[4] The SFPD Chief said that: "trust between the two agencies was irrevocably damaged." [5] At the time, Boudin's office was prosecuting six officers in five separate use-of-force cases.[6]

Boudin argued that police officers are turning, as an institution, against the prosecutor's office: "When I was in office, as we got closer to the recall, we had videos that surfaced of

[3]"District Attorney Boudin Announces New Appointment and New Policy Designed to Protect the Public From Police Misconduct and Abuse" the DA's office, Jun. 5, 2020.

[4]Notice from the Chief of Police to DA Boudin, Feb. 2, 2022.

[5]SF Chronicle, Feb. 4, 2022.

[6]SF Chronicle, Feb. 2, 2022.

police officers in patrol cars, standing by and watching as businesses were being burglarized, making no attempt whatsoever to intervene to arrest suspects." (Holloway 2023).[7] In the June 7th election, voters split 55% to 45% in support of the recall (the turnout was 46.2% overall).

The recall results were known on the same day of the election. Still, Boudin's seat became officially vacant 10 days after the San Francisco Board of Supervisors certified the election results at the body's June 28 meeting. Moreover, despite Boudin's publicized defeat on June 7th, there was uncertainty about who will take his place and what the future holds for the city's leading law enforcement elected official: US Commission on Civil Rights Commissioner Michael Yaki told KRON4 there's a 50-50 chance Boudin could run again in November in a race to fill out the rest of this term.[8] Only a month later, on July 7th, Mayor Breed announced the appointment of Brooke Jenkins to serve as the city's interim DA. Jenkins was a prosecutor under Boudin but resigned from the San Francisco DA's Office in October 2021 due to mounting dissatisfaction with the direction of the office (Mayor 2022). SFPD supported her in her bid for the DA's seat in the following November general election, which she later won.[9]

During the DA recall campaign, San Francisco residents raised the alarm regarding the police ignoring crime and telling residents that they avoid arrests because the DA's office avoids charges; in response, the police Chief acknowledged to a reporter that the police has "serious morale issues" due to "intense scrutiny amid the police reform movement and tussles with District Attorney Chesa Boudin."[10] In this paper, we empirically test the claim that the police department changed their behavior in anticipation for San Francisco's DA recall campaign (June 7th, 2022) and DA elections (November 7th, 2022).

14 Data

We combine a variety of data sources to estimate the effect of the recall election of DA Boudin on policing, prosecution, and the resulting jail population in San Francisco. To measure officer-initiated police stops and police incident reports, we use calls for service and police incident reports data from the OpenDataSF portal. These two incident-level data sources allow us to construct detailed measures of daily police activity in each police district and for each type of stop or incident for the time period from January 2018 to November 2022. To measure arrests by SFPD and DA charging behavior, we use an additional arrest-level dataset provided on the OpenDataSF portal by the San Francisco District Attorney's Office showing arrests presented to the district attorney's office and actions taken on each arrest. Finally, we use jail roster data provided by the NYU Public Safety Lab's Jail Data

[7] In Portland, the police chief publicly called city cops to stop telling residents DA Mike Schmidt won't prosecute crimes (Kavanaugh 2023).

[8] "Who will replace Chesa Boudin as SF DA?" KRON4, Jul 7, 2022.

[9] "SF District Attorney Brooke Jenkins has cleaned house in one regard, now having dismissed charges in all three police shooting cases brought by her predecessor Chesa Boudin."

[10] SF Chronicle, Feb. 19, 2022

Initiative to measure the total daily jail population, admissions and discharges, and length of jail stays. By using all of these data sources, we can measure any changes in the way both police and prosecutors use their discretion throughout the stages of an encounter: 1) whether police stop and initiate contact with an individual, 2) whether police record a criminal incident when in contact with an individual, 3) whether police arrest in response to a criminal incident and 4) whether the district attorney's office files charges after an arrest has been made.

Police Stops

The "Law Enforcement Dispatched Calls for Service - Closed Calls" dataset, an SFPD-generated dataset available on the OpenDataSF portal, provides individual call-level data on police calls for service in San Francisco, including "calls [that]originate from the public via calls to the 911 call center or from law enforcement officers in the field upon viewing an incident ('On-View')" (OpenDataSF 2022).[11] We focus on the officer-initiated "on-view" calls, as these are the types of citizen-police interactions that are initiated by officers and in which officers have the most discretion, although we do compare with citizen-initiated 911 calls to ensure that any observed changes in stops do not appear to be police responses to changes in civilian demand for police services.

We use the call type description fields to categorize calls and on-view stops into different crime or non-criminal call types. We exclude from our sample irrelevant or rare call types such as those that appear only in 911 calls and never in on-view stops, administrative call types (e.g., meetings), 311 calls, citizen standby calls, or non-criminal calls in which police assist with obtaining medical or fire department services. We also exclude calls/stops related to protests or riots as our identification strategy relies primarily on abrupt changes over time, and those call types are disproportionately concentrated in the spring and summer of 2020, respectively. We also drop all calls/stops occurring outside of a regular San Francisco police district or that are handled by another agency such as the fired department or EMS.

Table 18.1 shows the summary statistics of the different categories of stops and calls. During the whole period of our analysis, police contacted, on average, 92 crime-related stops per day, with a little bit less than half of those being for public order offenses. The vast majority of stops are passing calls. "Passing call" is a radio code officers use to log that they are located in a particular place at a specific time, yet they are not performing any further action. It can be used to respond to directives from the chain of command or by the officer at their discretion. Many codes occur at parking lots owned by the transit authority under directed theft abatement.

Police Incident Reports

Each call for service may or may not result in a police incident report, depending on the events occurring and the responding officers' discretion. Police incident reports are recorded

[11] Data source: SFGOV Dataset explainers: gitbook

in the "Police Department Incident Reports: 2018 to Present" dataset, also available on OpenDataSF.[12] Officers file the vast majority of incident reports, while non-emergency police reports can be filed online by the public using SFPD's self-service reporting system. We drop from our sample all incidents not occurring in a regular San Francisco police district or not related to criminal activity. Reports are then collapsed on the day the report was filled–not the day the incident occurred.

Table 18.2 shows the summary statistics of the different categories of incident reports filed both by officers and online by the public.

Arrests

The next dataset used comes from the San Francisco District Attorney's Office and it is the "District Attorney Actions Taken on Arrests Presented", available on OpenDataSF.[13] The dataset includes information on arrests presented to the SFDA since 2011 and the subsequent actions taken by the District Attorney's Office for each arrest. Therefore, we first explore the number and types of arrests made to study police behavior and discretion, and then we analyze the charging behavior of SFDA. The arrests presented to SFDA are carried out by both the SFPD and other agencies operating in the region.

Table 18.3 displays the daily average number of arrests made by SFPD and other regional law enforcement departments, and the total number of arrests categorized by different crime types.

Table 18.4 shows the daily average number of actions SFDA took for each arrest presented by both SFPD and other regional police departments.

Jail Population

We use data from two different sources to measure changes in the jail population. The first dataset is obtained from the Jail Data Initiative operated by the NYU Public Safety Lab.[14] The data consists of daily detainee-level information compiled from the San Francisco County Jail's roster. It's important to note that the roster provides a daily headcount of the jail population at 5 a.m., so individuals booked and released within less than 24 hours may not be included in the data. To address this limitation, we also utilize a second dataset from the San Francisco Sheriff's Department, which records the number of daily bookings.

Table 18.5 shows summary statistics from the jail roster and bookings.

15 Empirical Strategy

To estimate the effect of the DA recall election on police actions in San Francisco, we employ an Interrupted Time Series (ITS) estimator with a kink at the recall election date. This

[12] Data source:SFGOV Dataset explainer: gitbook

[13] Data source: sfgov

[14] Data source: JDI Dashboard: JDI Dashboard

methodology allows us to draw inferences by comparing the slopes of each outcome's trend on either side of the election date discontinuity. The primary identifying assumption is that absent a police response to the election, the time trend in the outcome variables would have continued smoothly along the preexisting seasonal trend before and after the election date. If this assumption holds, the difference in the slopes on either side of the election date can be attributed to the effect of the prosecutor's election on police propensity to make a stop, record an incident, or make an arrest or prosecutors' propensity to charge a defendant after an arrest is made. The identification assumption relies on the fact that other than the recall election's results becoming public (The DA admitted defeat the same night), no other change that can affect our outcomes occurred; the DA remained in office for an additional month, during which it was unclear who will replace him or when.

The choice between a kink discontinuity or jump discontinuity estimator in the ITS design depends on the nature of the relationship between the treatment variable and the outcome variable in a given context. In our analysis, the treatment is the recall election event. The event did not force an instant and simultaneous change in officer behavior like a formal policy change would have. The kink design allows for a cumulative effect on stop, incident report, and arrest outcomes due to officers gradually altering their behavior as the election approaches, with the rate of change intensifying or diminishing around a specific point in time. A kink discontinuity estimator is better suited to this setting because the relationship between the election's proximity and police actions is likely to be driven by word-of-mouth communication between officers rather than a top-down directive, resulting in a gradual change in the propensity towards various actions around the election date.

To estimate the effect of the recall election on trends in our various outcomes Y_{wt} - officer-initiated stops, crime incident reports, arrests, DA actions, and jail population - we use the following linear interrupted time series model with a uniform kernel:

$$Y_{wt} = \beta_0 + \beta_1 W_{wt} + \beta_2 After_{wt} + \beta_3 (W_{wt} * After_{wt}) + FirstOfMonth_w + \alpha_d + \epsilon_{wt} \quad (3.1)$$

where Y_{wt} represents the count of outcome Y on date t in week w. The running variable W_{wt} is the number of weeks between date t and the DA recall election. For Example, $W_{wt} = 7$ means the observation is from seven weeks past the election date. $After_{wt}$ is a dummy for whether date t is before or after the election date, $FirstOfMonth_w$ is a dummy for whether week w contains the first of the month, and α_d is a day-of-week fixed effect, as crime patterns may vary both throughout the week and around the first of the month (Carr and Packham 2019). The coefficient β_3 on the interaction between the weekly linear time trend W_{wt} and the election date cutoff dummy $After_{wt}$ is our estimate of the change in the slope of the linear time trend in the jail population induced by the election. We use the same specification to estimate changes in the trend in daily jail bookings.

This regression specification estimates separate linear slopes for the time series trend of the San Francisco jail population for the periods before and after the election and then tests for the difference in slopes at the threshold. We use the same model for the effect of the recall election on placebo outcomes as such as the number of calls made by citizens to the

SFPD and the number of citizen-filed online police reports, to ensure that any changes in our main outcomes are not likely to be driven by citizen demand for police intervention.

We use a 10-week bandwidth in our main estimates for all outcomes due to the proximity of the recall election to other relevant events, including the general election in November in which DA Brooke Jenkins was elected to a full term. However, we test the robustness of all estimates to a set of narrower and wider bandwidths to ensure that this modeling choice is not overly influential.

The key identifying assumption underlying the kink ITS design is that, in the absence of the election, the slope of the relationship between the date and the daily number of stops, police-recorded crime incidents, or arrests would have been smooth and continuous. In other words, any observed discontinuity in this relationship at the election threshold can be attributed to the causal effect of the election itself. We also conduct placebo analyses of additional outcomes such as citizen-filed online crime reports (a measure of criminal activity that does not rely primarily on police recording) or 911 calls (a measure of demand for police intervention) to ensure that this change is police driven rather than citizen-driven.

16 Results

Table 18.6 summarizes our main results. When we compare changes occurring at different discretion points in the life of a criminal incident/case (summarized in Figure 18.1), we see that the daily counts of SFPD officer-initiated stops, criminal incident reports, and arrests were trending downward during the ten weeks leading up to the recall election, and immediately began increasing after DA Boudin was recalled.

If this increase were driven by underlying criminal activity or citizen demand for police services, we would likely see a corresponding increase in citizen-filed online criminal incident reports or citizen-initiated 911 calls. We find no statistically significant trend break in these outcomes; if anything, we see a noisily estimated decrease in these outcomes, suggesting that the increases in stops, incidents, and arrests were driven primarily by police discretion.

We also explore whether these additional arrests resulted in a change in the DA's propensity to charge an arrest presented by SFPD. There is a statistically significant increase in both the number and proportion of cases dismissed after the recall. Importantly, this increase in case dismissals began on the recall election date and continued after interim DA Jenkins was appointed about four weeks later. While the point estimate suggests a smaller and more noisy estimated increase in the number of cases charged, this increase in dismissed cases appears to be driven entirely by the increase in the number of arrests presented.

Lastly, we find that the increase in stops, resulting incident reports, and resulting arrests culminated in a substantial increase in the average daily San Francisco Jail population, largely driven by an increase in the daily number of bookings by SFPD officers.

Section 16 discusses the effect of the recall election on SFPD stops, incident reports, and arrests in more detail, finding overall that increases in police activity were related to non-violent offenses for which police may have a higher degree of discretion in how they respond. Section 16 explores DA actions in more detail, showing that the small increases in the number and proportion of cases declined at the time of the recall election are likely to result from changes in the number and composition of arrests presented. Section 16 discusses the increase in the jail population that resulted from increased arrests and bookings by the SFPD. Figure S0.1 shows the robustness of these effects to alternate bandwidths.

Police Behavior

We begin by exploring three discretion points at which officers choose whether and how to respond to a potential criminal incident. First, officers can either be dispatched to a scene by a resident-initiated 911 call or can choose to conduct an "on-view" stop in which they choose to respond to events they see during their shift. During the time period in our sample, SFPD received an average of about 551 crime-related 911 calls and made about 92 crime-related on-view stops each day. We focus first on on-view stops as officers typically have little discretion in responding to a 911 call and almost total discretion about whether to make an on-view stop.

Officers' Stops vs. Resident Calls

Table 18.7 shows that in the ten weeks before the recall election, the average number of on-few police stops per day had been decreasing by about 2.5 daily stops (2.8% of the pre-recall mean of about 88 stops per day, reported in Table 18.1) each week in the 10 weeks before the recall, and began increasing by about 3.6 daily stops (4.1% of the pre-recall mean). The point estimate in column 1 suggests a net slope increase of about 6.1 stops per day (6.9%) beginning on the recall election date. Columns 3-6 of table 18.7 decompose this effect by the type of crime reported as the initial reason for making the stop. In column 3, we find no statistically significant change in stops for violent offenses. Columns 4-6 show that property, traffic, and public order stops had been decreasing in the 10 weeks prior to the recall election and began increasing in the 10 weeks after the election; this trend reversal resulted in a net slope increase of about 0.7 property-related stops (4.7%), 2.6 traffic-related stops (6.6%), and 2.895 public order-related stops (36%) per day in each week following the recall. Figure 18.2 visualizes these changes, showing the weekly average of daily on-view stops, overall and for each offense type, with fitted lines showing the time trend in the ten weeks before and after the recall.

The increase in traffic stops is interesting in light of Boudin's stated intent not to prosecute cases centered around contraband found during pretextual traffic stops [LS ADD CITE LATER]; police may have increased traffic stops in anticipation of this policy possibly being reversed when Boudin left office a month later. Additionally, the large relative increase in stops related to public order offenses (sitting/lying on public sidewalks, vandalism, noise, trespassing, dumping, etc.) is consistent with police behavior changing the most in relation to non-urgent matters over which they have the highest level of discretion. Finally, the null finding for violence-related stops is unsurprising. Firstly, because on-view stops for violent offenses are far less common (see summary statistics in Table 18.1). Secondly, because officers have less discretion in whether to respond to an active violent situation.

Importantly, behavioral responses by police are not the only reason that on-view stops might increase. If underlying criminal activity or civilian demand for police intervention happened to increase around the time of the recall election, police stops could have increased for reasons other than police responses to prosecutor politics. Columns 1-6 of Table **??** present analogous estimates of changes in civilian 911 calls, both overall and by offense type. We find no statistically significant change in calls to police. If anything, there is a noisily estimated decrease in citizen requests for police assistance.

Additionally, Column 7 tests for a change in automated calls made by home alarm systems, a potential (albeit noisy) proxy for burglaries that should be unaffected by either civilian reporting behavior or police behavioral responses to the recall election. Again, we see no statistically significant change and, if anything, a noisy decrease. Weekly averages and estimated slopes are plotted in Figures 18.3 and 18.4. Overall, we find no evidence of any underlying changes in criminal activity or citizen demand for police services that would explain the increase in police stops.

Incident Reports

Once officers respond to a 911 call or choose to make an on-view stop, the next potential point of discretion is whether they report that a crime has occurred (See Figure 18.1). During the time period in our sample, SFPD officers recorded about 195 criminal incidents per day on average. Civilians can also file online incident reports, usually related to property or public order offenses, and filed about 74 reports per day within the same 10-week window around the recall.

Table 18.9 reports estimated changes in the trend in police reports per day at the recall election date, showing a small but statistically significant increase after the recall. In the 10 weeks leading up to the recall, police were recording an average of 188 incidents per day (see Table 18.2), and this average was relatively stable, decreasing by about 0.5 daily incidents each week (0.3%). This weekly average began increasing by about 1.8 daily incidents for each week after the recall (about 1%), for a total net slope change of 2.3 incidents per day or about 1.2%, as reported in Column 1.

Columns 3-6 again decompose this change by offense type. Consistent with the changes in on-view stops, the slope of the weekly average number of incidents recorded increased by 1.511 daily incidents (1.3%) for property offenses, 0.1626 daily incidents (8.7%) for traffic offenses, and 0.682 daily incidents (27.6%) for drug offenses. These trends before and after the recall are visualized in Figure 18.6. This pattern is consistent with the changes in on-view stops, and suggests that officers may have reported that some stops were initially made for public order offenses but resulted in a finding of a drug offense during the course of the stop.

We again test for a change in incident reports that cannot be driven by police behavior: those filed online by citizens, usually for theft, property damage, or noise. Table 18.10 shows no evidence of a statistically significant change in incident reports by San Francisco residents after the recall, and if anything, a small and noisy decrease. However, it should be noted that far fewer reports are filed online by civilians, so very small effects may not be detectable due to limited statistical power.

Arrest Presented to SFDA

After police make a stop or respond to a call, and if they determine that a crime has occurred, their next point of discretion is whether or not to make an arrest (Figure 18.1) to be presented to the DA for potential prosecution. On average, SFPD made about 18 daily arrests during the relevant time period - about 14 felony arrests and four misdemeanor arrests.

Table 18.11 estimates the trend break in arrests by SFPD at the recall date. In the ten weeks prior to the recall, SFPD averaged about 17 arrests per day (see Table 18.3), and the trend in the weekly average was decreasing by about 0.2 arrests (1.1%) each week as the recall approached. In the ten weeks after the recall, the trend reversed, with average daily arrests increasing at a rate of 0.6 average daily arrests (3.3%) in each week after the recall, for a total net slope change of 0.751 average daily arrests (4.4%). Columns 2 and 3 disaggregate by felony and misdemeanor arrests, finding a statistically significant slope increase of 0.813 average daily felony arrests (6.2%) each week. The analogous increase in misdemeanor arrests is not statistically significant. Still, the point estimate would suggest a slope increase of 0.1655 average daily misdemeanor arrests (4.3%) each week, and standard errors are large, so we cannot rule out that the relative magnitude of the change is the same for misdemeanors and felonies. Figure 18.7 plots these trends in overall, felony, and misdemeanor arrests, all of which were decreasing prior to the recall and began increasing after the recall. For comparison, it also shows trends in the relatively few arrests presented to the San Francisco DA's office by other law enforcement agencies such as the California Highway Patrol and the Bay Area Rapid Transit police. While these arrests are not frequent enough for a formal regression kink estimation to be informative, we see no evidence of a corresponding increase in arrests by these other agencies.

Columns 3-9 of Table 18.11 disaggregate these arrests by offense type. Unlike the changes in stops an incidents, there is no evidence of a statistically significant trend break in traffic, public order, or drug arrests; however, some traffic or public order offenses may result in citations rather than arrests. There is a marginally significant increase in the slope of the weekly average number of violent (0.3 daily arrests or 4.3%), property (0.4 daily arrests or 16.6%), and "other" (0.2797 daily arrests or 15.7%) arrests per day. Plots of these changes in trends are presented in Figure 18.8. This pattern of offense types differs from the increases in stops and incident reports, which were concentrated in public order, traffic, drug, and property offenses, suggesting that the relevant margin for some less serious offense types may be whether to make a stop or write a report given a certain observation and the relevant margin for serious or violent offense types may be whether to arrest or not given that a criminal incident has occurred.

Behavior of Prosecutors

We also examine whether the recall election might have influenced decisions by the District Attorney's office about which arrests to charge and which arrests to dismiss. Note that the recall election occurred on June 7th, and interim DA Brooke Jenkins was not appointed until July 7th, so any changes occurring immediately on the recall date would reflect a change by the Boudin DA's office during the lame-duck period rather than a new DA taking office. Importantly, given our detected changes in the number and possibly composition of arrests presented to the DA's office by the police, we expect corresponding changes in the trends of charges filed and arrests declined for prosecution. For this reason, we test for effects on both the number of arrests prosecuted/dismissed and the proportion of arrests prosecuted.

Table 18.12 shows that there was little to no change in the rate of charges. We detect an increase in the weekly average number of felony SFPD arrests charged per day (0.3 charges or 4%) in Column 3 of the top panel, as well as an increase in the weekly average of felony arrests dismissed per day (0.3 declinations or 7.7%) in column 5 of the top panel, consistent with the increase in the total number of felony arrests presented (discussed above in section 16. We find no statistically significant change in misdemeanor charges and that the average daily number of misdemeanor arrests declined increased, again consistent with increases in arrests. The overall change across felonies and misdemeanors was an increase in both charges and declinations due to more arrests being presented.

The overall proportion of arrests dismissed increased after the recall. Table 18.4 shows that the proportion of case dismissals out of arrests presented went up by 24% on average (on average, an additional case dismissed per week). It is difficult to assign a mechanism to this change given the uncertainty about the duration of the lame-duck period, the increase in the total number of arrests, and the possible change in the composition of arrests. The DA's office may have been reluctant to take on marginal cases given impending personnel changes, the increase in the total number of arrests may have caused capacity constraints to become binding, or the change in the composition of arrests may have resulted in lower-quality arrests for which there was not sufficient evidence to build cases. Regardless, the increased proportion of arrests dismissed suggests that the increases in the San Francisco jail population discussed below in section 16 are unlikely to be driven by changes in the DA's office and more likely driven by increases in arrests and bookings. The bottom panel of Table 18.12 includes the relatively few arrests presented by agencies other than SFPD, and the results are similar.

Regardless of the underlying causes for the decline in the proportion of cases charged, this phenomenon offers insight into possible influences on police behavior. It could be posited, and it often was, that officers were intentionally moderating their enforcement efforts prior to the recall, perceiving that their arrests might not culminate in charges. They might then logically intensify their efforts once the recall succeeded. Yet, this assumption conflicts with the reality that officers are rarely, if ever, apprised of the prosecutorial fate of their arrests on an individual basis. Contrary to their potential belief, the data show that the rate of arrests resulting in charges actually decreased following the recall. Moreover, the period between the recall election and DA Jenkins's appointment was uncertain, as the election results required certification before any appointment could occur. Given that officers likely operate with incomplete knowledge about the eventual outcomes of their arrests, it remains unclear whether their behavior was a response to a mistaken belief about the changing likelihood of charges or if it represented a strategic reduction in enforcement to potentially impact the election outcome, with a reversion to usual levels of effort once the electoral process concluded.

Jail Population

Lastly, we examine how each of the changes earlier in the pipeline of a criminal incident led to changes in jail outcomes. Unlike the other discretion points described in Figure 18.1, this outcome is potentially affected by both police and prosecutor discretion. Table 18.13 shows that the increases in police activity starting after the June 7th recall election had a significant effect on the local jail population. The rate of change in jail population per day decreased by 11 each week prior to the recall and increased by five after the election (visualized in Figure 18.10). Further, data on bookings - every person brought to the local jail regardless of whether they stay till the daily head count - confirms the finding that SFPD changed their behavior and decreased the rate of bookings before the election and increased after (see Figure 18.11). The total slope change in the weekly average daily jail population after the recall suggests a net slope increase of 16 inmates per day each week on average after the recall. While this is only a 2.2% weekly rate of increase compared to the pre-recall mean, by a rough back-of-the-envelope calculation,

$$JailDays = \sum_{w=1}^{10}(16.276 * 7 * w) = 6260.1 \tag{3.2}$$

The post-recall period witnessed an uptick in policing intensity, leading to approximately 6,260 additional person-days spent in jail over the ten weeks after the recall election. Considering the analyses from earlier sections, it is improbable that this increase stemmed from shifts in crime rates, civilian conduct, or the probability of pre-trial charges and detention. Rather, the San Francisco jail logs indicate a heightened frequency of bookings initiated by SFPD officers.

Concurrently, Figure 18.14 illustrates a pronounced reduction in the average jail stay duration (till final release) coinciding with a surge in inmate counts. Pre-election, we find a stable flat trend, yet after the recall, the average person in jail is released much faster (from about 270 days till release to less than 50). To quantify the relationship between the burgeoning jail population and the diminished duration of imprisonment, we employed a linear regression model as follows:

$$\text{AverageDays}_t = \beta_0 + \beta_1 \text{Population}_t + \beta_2 \text{afterRecall} + \beta_3 \text{Population}_t \times \text{afterRecall} + \epsilon_t \tag{3.3}$$

Here, AverageDays_t denotes the mean jail final stay at time t, Population$t$ represents the inmate count at time t, and afterRecall is a binary indicator which assumes a value of

1 post-June 7 (the recall date), and 0 otherwise. The term $\text{Population}_t \times \text{afterRecall}$ is the interaction of the inmate count and the post-recall period.

The coefficient for the interaction term, as reported in Table 18.14, suggests a post-recall reduction in the average jail stay by approximately one and a half days for each additional inmate. This pattern implies a potential shift towards more frequent arrests for minor offenses post-recall, which generally require shorter jail stays. This inference is further supported by Figure 18.15, which demonstrates an almost perfectly aligned decrease in the share of inmates incarcerated for violent crimes from roughly 35% to a mere 3% after the recall, as depicted in Figure 18.16 as well. These dynamics suggest that arrests for lower-level offenses subject to higher discretion predominantly drive the post-recall increase in the jail population. Normatively, the declining proportion of inmates held for violent crimes prompts critical discussions about the implications for public safety and the justice system, especially considering the broader impacts of pre-trial detention.

Figures 18.12 and 18.13 also show that this trend appears to have reversed when interim DA Jenkins was elected to a full term in the November general election. Formal hypothesis tests surrounding this date are beyond the scope of this paper. Still, these trends present suggestive evidence that criminal justice enforcement in San Francisco continued to be responsive to political events.

Robustness

While the 10-week bandwidth around the recall is the primary estimate to ensure consistency across outcomes, Figure S0.1 shows the robustness of the main results to a variety of alternate bandwidths from 5-15 weeks. In general, the main results are not sensitive to the choice of bandwidth. The only exception is 911 calls by citizens; while our main bandwidth of 10 weeks estimates no statistically significant change in trends in citizen demand for police services, some smaller bandwidths produce a statistically significant decrease in calls, and some larger bandwidths produce a statistically significant increase. This is unsurprising, as calls are the highest-frequency outcome, so small changes are estimated more precisely. Our main estimate is not an outlier in either direction.

The smallest bandwidth (5 weeks) may be particularly interesting because most of the post-recall period would constitute weeks when Boudin was still in office. Hence, any impact caused by the transition in the DA's office would not affect these estimates (in this period, the identity or timing of Boudin's replacement was unknown). Results estimated using this bandwidth are similar to the main results but, unsurprisingly, more noisily estimated.

Finally, we conducted a robust placebo test to validate that our results were not confounded by any endogenous factor synchronously occurring with the recall election date. We replicated our analysis using data from the previous year (2021) as a counterfactual scenario, where the recall election had not occurred. Figure S0.2 and Table S0.1, display these results. As hypothesized, the trends in police arrests and jail bookings in 2021 were statistically indistinguishable from a flat trend, indicating no underlying seasonal effects that could drive the 2022 outcomes. The decline in police stops at the onset of 2021, which gradually tapered off, suggests a continuing pattern that extends into 2022. This pattern aligns with our theoretical framework, positing a changing dynamic between SFPD and the progressive DA, albeit the magnitude and significance of these trends warrant further investigation to ascertain their contribution to the argument. The summer of 2021 did show some variability in citizen behavior, which, while carefully not attributed to any specific external factors due to a lack of evidence, did not mirror the 2022 data, thereby alleviating concerns regarding potential seasonality effects. This dissimilarity, particularly in directionality, suggests that the changes we observed in 2022 are likely attributable to the recall election rather than seasonal patterns inherent to the data set.

17 Conclusion

Overall, the totality of our results suggests that SFPD increased its effort after the recall of DA Boudin, resulting in increased stops, incident reports, arrests, and person-days in jail in the ten weeks following the recall compared to the ten weeks before, suggesting potentially suppressed police effort leading up to the Boudin recall. It is clear that police responded to the recall election; importantly, this response would be consistent with either the "blue flu" or "wildcat strike" hypothesis that police decrease their effort in response to unfriendly district attorneys or the finding that police tone down some activities in response to potential increases in liability when there is a new - or in this case, "unfriendly" - district attorney (Stashko and Garro 2023). Regardless, this arbitrary increase in policing resulted in additional days in jail for San Francisco defendants in the ten weeks after the recall compared to the ten weeks before, despite no evidence of increases in criminal activity. It is unclear which level of policing is optimal, but if this difference reflects under-policing before the recall, public safety may have been at risk; if it reflects over-policing after the recall, the post-recall increase resulted in an unnecessary increase in jail detention that is costly both to detainees and to the government. Furthermore, our findings highlight the need to consider the behavioral responses of police when evaluating the effects of progressive prosecutors' policies on crime and public safety.

18 tables/tables and Figures

Table 18.1: Summary Statistics: Stops and Calls, daily level

	Stops (On View)					
	weeks (-10 to +10)		weeks (-10)		weeks (0 + 10)	
	Mean	Std. Dev.	Mean	Std. Dev.	Mean	Std. Dev.
All criminal	91.94	24.09	88.18	20.67	95.23	26.47
Violent	5.71	4.00	5.33	3.22	6.05	4.57
Property	15.87	4.98	14.57	5.47	17.00	4.24
Public order	40.98	14.41	39.16	11.67	42.57	16.38
Traffic	29.38	11.19	29.12	10.56	29.61	11.79
Passing call	240.22	59.72	266.35	65.17	217.36	43.54
Alarm	0.23	0.47	0.27	0.49	0.20	0.44
public health	5.36	2.50	5.82	2.67	4.96	2.30
	911 Calls					
	weeks (-10 to +10)		weeks (-10)		weeks (0 + 10)	
	Mean	Std. Dev.	Mean	Std. Dev.	Mean	Std. Dev.
All criminal	550.80	31.85	549.10	34.64	552.29	29.43
Violent	158.46	15.77	154.67	13.95	161.77	16.63
Property	183.38	17.88	183.08	17.87	183.64	18.06
Public order	181.15	18.23	182.51	18.18	179.96	18.35
Traffic	27.81	7.32	28.84	7.48	26.91	7.13
Alarm	62.11	10.70	59.67	10.42	64.25	10.57
public health	93.65	16.01	102.88	13.34	85.57	13.68
Observations	105		49		56	

Table 18.2: Summary Statistics: Reports, daily level

	Incident Reports (Not Online)					
	weeks (-10 to +10)		weeks (-10)		weeks (0 + 10)	
	Mean	Std. Dev.	Mean	Std. Dev.	Mean	Std. Dev.
Incidents Criminal	194.93	30.97	187.94	28.69	201.05	31.83
Violent	35.38	6.96	33.35	5.08	37.16	7.89
Property	118.05	23.28	115.61	23.27	120.18	23.29
Public order	34.75	7.30	34.59	7.85	34.89	6.85
Traffic	1.96	1.30	1.86	1.15	2.05	1.42
Drugs	4.79	4.92	2.53	2.07	6.77	5.79
Total not criminal:	21.04	4.77	21.10	4.90	20.98	4.70
Other non-criminal	16.47	3.89	16.67	4.04	16.29	3.78
Admin	4.46	2.26	4.31	2.46	4.59	2.08
Suicide	0.11	0.40	0.12	0.39	0.11	0.41
	Online Incident Reports					
	weeks (-10 to +10)		weeks (-10)		weeks (0 + 10)	
	Mean	Std. Dev.	Mean	Std. Dev.	Mean	Std. Dev.
Incidents Criminal	74.26	18.07	67.80	10.63	79.91	21.19
Property	67.59	17.16	61.31	10.15	73.09	20.03
Public order	6.67	2.90	6.49	2.64	6.82	3.13
Observations	105		49		56	

Table 18.3: Summary Statistics: Arrests, daily level

	weeks (-10 to +10)		weeks (-10)		weeks (0 + 10)	
	Mean	Std. Dev	Mean	Std. Dev	Mean	Std. Dev
SFPD						
Total arrests	18.34	5.31	16.92	4.83	19.59	5.44
Total felony	14.30	4.44	13.14	4.15	15.30	4.47
Total misd.	4.05	2.37	3.78	2.22	4.29	2.48
Violent	8.09	3.22	7.18	2.69	8.88	3.45
Property	3.84	2.18	3.76	2.18	3.91	2.18
Traffic	0.11	0.32	0.12	0.33	0.11	0.31
Public order	1.80	1.24	1.67	1.13	1.91	1.32
Drugs	2.11	1.96	2.12	1.89	2.11	2.03
Other	2.14	1.45	1.78	1.37	2.46	1.45
Missing	0.25	0.57	0.29	0.65	0.21	0.49
Not by SFPD						
Total arrests	3.42	2.20	3.94	2.70	2.96	1.53
Total felony	1.70	1.48	2.08	1.73	1.36	1.14
Total misd.	1.72	1.50	1.86	1.78	1.61	1.20
Violent	0.89	1.22	1.02	1.52	0.77	0.87
Property	0.41	0.62	0.51	0.68	0.32	0.54
Traffic	0.03	0.29	0.06	0.43	0.00	0.00
Public order	1.38	1.30	1.59	1.57	1.20	1.00
Drugs	0.18	0.51	0.18	0.44	0.18	0.58
Other	0.43	0.63	0.45	0.61	0.41	0.65

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Table 18.3, continued

	weeks (-10 to +10)		weeks (-10)		weeks (0 + 10)	
	Mean	Std. Dev	Mean	Std. Dev	Mean	Std. Dev
Missing	0.10	0.34	0.12	0.39	0.09	0.29
All agencies						
Total arrests	21.76	5.77	20.86	6.06	22.55	5.45
Total felony	15.99	4.78	15.22	4.83	16.66	4.68
Total misd.	5.77	2.61	5.63	2.88	5.89	2.36
Violent	8.97	3.49	8.20	3.15	9.64	3.66
Property	4.25	2.28	4.27	2.36	4.23	2.22
Traffic	0.14	0.43	0.18	0.53	0.11	0.31
Public order	3.18	1.80	3.27	2.03	3.11	1.59
Drugs	2.30	2.13	2.31	1.94	2.29	2.30
Other	2.57	1.61	2.22	1.52	2.88	1.64
Missing	0.35	0.62	0.41	0.70	0.30	0.54
Observations	105		49		56	

Table 18.4: Summary Statistics: DA action of arrests, daily level

	weeks (-10 to +10)		weeks (-10)		weeks (0 + 10)	
	Mean	Std. Dev	Mean	Std. Dev	Mean	Std. Dev
SFPD						
Charged	10.80	3.81	10.14	3.27	11.38	4.17
Charged felony	9.01	3.40	8.39	3.03	9.55	3.63
Charged misd.	1.79	1.43	1.76	1.44	1.82	1.43
Discharged	5.30	3.29	4.33	2.63	6.14	3.59
Discharged %	0.28	0.14	0.25	0.12	0.31	0.16
Discharged felony	3.99	2.54	3.31	2.00	4.59	2.81
Discharged misd.	1.30	1.48	1.02	1.22	1.55	1.65
Further investigation requested	0.52	0.76	0.53	0.77	0.52	0.76
MTR/Referred to other agency	1.32	1.17	1.41	1.22	1.25	1.13
Other action	0.40	0.63	0.51	0.68	0.30	0.57
Not by SFPD						
Charged	2.30	1.64	2.67	1.93	1.98	1.26
Charged felony	1.13	1.19	1.37	1.42	0.93	0.89
Charged misd.	1.17	1.22	1.31	1.37	1.05	1.07
Discharged	0.74	1.01	0.73	1.17	0.75	0.86
Discharged %	0.21	0.29	0.17	0.26	0.25	0.31
Discharged felony	0.31	0.58	0.29	0.61	0.34	0.55
Discharged misd.	0.43	0.81	0.45	0.94	0.41	0.68

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Table 18.4, continued

	weeks (-10 to +10)		weeks (-10)		weeks (0 + 10)	
	Mean	Std. Dev	Mean	Std. Dev	Mean	Std. Dev
Further investigation requested	0.16	0.40	0.29	0.50	0.05	0.23
MTR/Referred to other agency	0.18	0.39	0.20	0.41	0.16	0.37
Other action	0.03	0.17	0.04	0.20	0.02	0.13
All agencies						
Charged	13.10	4.33	12.82	4.26	13.36	4.40
Charged felony	10.14	3.81	9.76	3.77	10.48	3.85
Charged misd.	2.96	1.80	3.06	1.85	2.88	1.76
Discharged	6.04	3.38	5.06	2.93	6.89	3.54
Discharged %	0.27	0.13	0.24	0.11	0.30	0.13
Discharged felony	4.30	2.62	3.59	2.07	4.93	2.90
Discharged misd.	1.73	1.73	1.47	1.65	1.96	1.78
Further investigation requested	0.69	0.82	0.82	0.88	0.57	0.76
MTR/Referred to other agency	1.50	1.20	1.61	1.24	1.41	1.17
Other action	0.43	0.65	0.55	0.71	0.32	0.58
Observations	105		49		56	

Table 18.5: Summary Statistics: Jail population and Bookings, daily level

	weeks (-10 to +10)		weeks (-10)		weeks (0 + 10)	
	Mean	Std. Dev	Mean	Std. Dev	Mean	Std. Dev
Number of inmates	744.48	28.36	732.63	23.30	754.84	28.51
Daily duration	420.77	17.56	430.42	12.67	412.33	16.96
Number of inmates serving:						
less than 24H	8.10	4.07	7.71	4.01	8.43	4.13
more than 24H	736.38	26.78	724.92	21.42	746.41	27.14
less than 48H	17.24	7.06	17.47	8.02	17.04	6.18
more than 48H	727.24	25.49	715.16	18.77	737.80	26.03
less than 72H	27.11	9.14	25.94	10.26	28.14	7.99
more than 72H	717.36	23.69	706.69	17.82	726.70	24.36
Total # of bookings	28.26	5.87	27.69	5.87	28.75	5.88
# of bookings by SFPD	20.73	5.24	19.73	5.00	21.61	5.33
Observations	105		49		56	

Table 18.6: Overview of Main Results

Outcome	Slope change	Trend pre-election	Trend post-election
Police Behavior			
Police stops			
All Stops (crimes only)	6.082***	-	+
Police reports			
All Incident Reports (crime)	2.33*	-	+
Police arrests (SFPD)			
All arrests	0.751***	-	+
All felony arrests	0.813***	-	+
All misdemeanor arrests	0.1655	-	+
Residents Behavior			
Residents Calls			
Crime related	-0.394	-	-
Non-crime related	-0.612	-	-
Residents Online Reports			
All residents' online reports	-0.433	-	-
DA Behavior			
All charges	0.231	+	+
All dismissals	0.608***	-	+
Charges (felony)	0.3019	-	+
Charges (misdemeanor)	-0.0712	+	+
Dismissals (felony)	0.395***	-	+
Dismissals (misdemeanor)	0.2127*	-	+
Jail Population			
Population	16.276***	-	+
Bookings (all)	1.068***	-	+
Bookings (SFPD)	0.991***	-	+

Note: All analyses utilize the rdrobust function to estimate the change in slope of the outcome concerning the weeks around the recall event. The specification spans a 10-week bandwidth before and after the recall. In essence, the estimate captures the difference in outcome trends before and after the recall over a 10-week period. All estimates rely on full police data: SFPD and other agencies.
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table 18.7: Police Stops

	(1) Crimes	(2) Passing calls	(3) Violent	(4) Property	(5) Traffic	(6) Public order
Conventional	6.082***	2.80	-0.0644	0.686**	2.566***	2.895***
	(0.948)	(2.16)	(0.1665)	(0.233)	(0.535)	(0.476)
slope.left	-2.46	-2.24	-0.07	-0.3	-0.98	-1.11
slope.right	3.62	0.56	-0.13	0.38	1.58	1.79
nobs.left	519	519	519	519	519	519
nobs.right	270	270	270	270	270	270
effective.l	70	70	70	70	70	70
effective.r	77	77	77	77	77	77

+ $p < 0.1$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table 18.8: Police Calls

	(1) Crimes	(2) MH/PH	(3) Violent	(4) Property	(5) Traffic	(6) PO	(7) Alarm
Conv.	-0.394	-0.612	-0.915	0.901	-0.093	-0.287	-0.364
	(1.407)	(0.690)	(0.693)	(0.846)	(0.335)	(0.838)	(0.472)
sl.L	-0.86	-0.78	-0.21	-0.77	0.13	-0.02	0.16
sl.R	-1.26	-1.39	-1.12	0.13	0.04	-0.31	-0.2
n.L	519	519	519	519	519	519	519
n.R	270	270	270	270	270	270	270
n.e.L	70	70	70	70	70	70	70
n.e.R	77	77	77	77	77	77	77

MH/PH:	Mental/public health
PO:	Public order
Conv.:	Conventional
sl.L/R:	slope left/right
n.L/R:	nobs left/right
n.e.L/R:	nobs effective left/right

+ $p < 0.1$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table 18.9: Police Incident Reports

	(1) Crimes	(2) Not crime	(3) Violent	(4) Property	(5) Traffic	(6) Public order	(7) Drugs
Conventional	2.33*	0.170	-0.0408	1.511*	0.1626*	0.0181	0.682***
	(0.91)	(0.230)	(0.3142)	(0.709)	(0.0816)	(0.3268)	(0.148)
slope.left	-0.52	0.01	0.02	-0.71	-0.02	0.06	0.13
slope.right	1.81	0.18	-0.02	0.8	0.15	0.08	0.81
nobs.left	1615	1615	1615	1615	1615	1615	1615
nobs.right	268	268	268	268	268	268	268
effective.l	70	70	70	70	70	70	70
effective.r	77	77	77	77	77	77	77

+ $p < 0.1$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table 18.10: Citizen Incident Reports (Online)

	(1) All incident reports	(2) Property	(3) Public order
Conventional	-0.433	-0.335	-0.0975
	(0.637)	(0.595)	(0.1909)
slope.left	-0.06	0.02	-0.08
slope.right	-0.49	-0.32	-0.18
nobs.left	1615	1615	1615
nobs.right	268	268	268
nobs.effective.left	70	70	70
nobs.effective.right	77	77	77

+ $p < 0.1$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table 18.11: Arrests

	(1) All	(2) Felony	(3) Misdemeanor	(4) Violent	(5) Property	(6) Traffic	(7) Public order	(8) Drugs	(9) Other
SFPD									
Conventional	0.751***	0.813***	0.1655	0.30813+	0.361***	-0.000698	0.0277	0.0544	0.2797**
	(0.216)	(0.207)	(0.1244)	(0.16021)	(0.108)	(0.019179)	(0.0646)	(0.0884)	(0.0895)
slope.left	-0.19	-0.24	-0.02	-0.09	-0.22	-0.01	-0.01	0.14	-0.09
slope.right	0.56	0.57	0.14	0.21	0.14	-0.01	0.02	0.2	0.19
All law enforcement									
Conventional	0.723**	0.787***	0.168	0.230	0.384***	0.00797	0.0439	0.0566	0.2945***
	(0.252)	(0.221)	(0.146)	(0.180)	(0.111)	(0.02006)	(0.0997)	(0.0900)	(0.0881)
slope.left	-0.15	-0.19	-0.02	-0.01	-0.23	-0.02	-0.04	0.15	-0.05
slope.right	0.58	0.6	0.15	0.22	0.15	-0.01	0.01	0.21	0.24
nobs.left	519	519	519	519	519	519	519	519	519
nobs.right	265	265	265	265	265	265	265	265	265
effective.l	70	70	70	70	70	70	70	70	70
effective.r	77	77	77	77	77	77	77	77	77

+ $p < 0.1$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table 18.12: DA action of arrests presented

	(1) All charges	(2) All Declinations	(3) Felony Charges	(4) Misdemeanor Charges	(5) Felony declinations	(6) Misdemeanor declinations	(7) Further investigation	(8) Refer to other agency	(9) % Declinations of arrests
SFPD arrests									
Conventional	0.3039+	0.529***	0.3390*	-0.0351	0.335**	0.1943**	0.0298	0.0871	0.02174**
	(0.1808)	(0.138)	(0.1543)	(0.0834)	(0.116)	(0.0734)	(0.0442)	(0.0595)	(0.00771)
slope.left	0	-0.24	-0.08	0.08	-0.13	-0.11	0	0.01	-0.01
slope.right	0.3	0.29	0.26	0.04	0.21	0.09	0.03	0.09	0.01
nobs.left	519	519	519	519	519	519	519	519	519
nobs.right	265	265	265	265	265	265	265	265	265
nobs.effective.left	70	70	70	70	70	70	70	70	70
nobs.effective.right	77	77	77	77	77	77	77	77	77
All arrests									
Conventional	0.231	0.608***	0.3019+	-0.0712	0.395***	0.2127*	0.00362	0.0810	
	(0.209)	(0.149)	(0.1745)	(0.0955)	(0.119)	(0.0877)	(0.04665)	(0.0617)	
slope.left	0.05	-0.26	-0.04	0.09	-0.16	-0.1	0.03	0.01	
slope.right	0.28	0.35	0.27	0.01	0.23	0.11	0.03	0.09	
nobs.left	519	519	519	519	519	519	519	519	
nobs.right	265	265	265	265	265	265	265	265	

Continued on next page

Table 18.12, continued

	(1) All charges	(2) All Declinations	(3) Felony Charges	(4) Misdemeanor Charges	(5) Felony declinations	(6) Misdemeanor declinations	(7) Further investigation	(8) Refer to other agency	(9) % Declinations of arrests
nobs.effective.left	70	70	70	70	70	70	70	70	
nobs.effective.right	77	77	77	77	77	77	77	77	

+ $p < 0.1$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table 18.13: Jail Outcomes

	Population	Bookings (all)	Bookings (SFPD)
Conventional	16.276***	1.068***	0.991***
	(0.358)	(0.233)	(0.208)
slope.left	-9.84	-0.25	-0.24
slope.right	6.44	0.82	0.75
nobs.left	146	154	518
nobs.right	203	210	210
nobs.effective.left	62	70	70
nobs.effective.right	73	77	77

+ p < 0.1, * p < 0.05, ** p < 0.01, *** p < 0.001

Table 18.14: Regression results of average jail stay duration (days) on jail population and after June 7 recall election.

Coefficient	Estimate	Std. Error	t value	Pr(¿—t—)
(Intercept)	166.72	42.28	3.94	¡ .001
Population	0.16	0.05	3.03	0.002
After Recall	1029.37	65.89	15.622	¡ .001
Population x After Recall	-1.50	0.08	-17.67	¡ .001

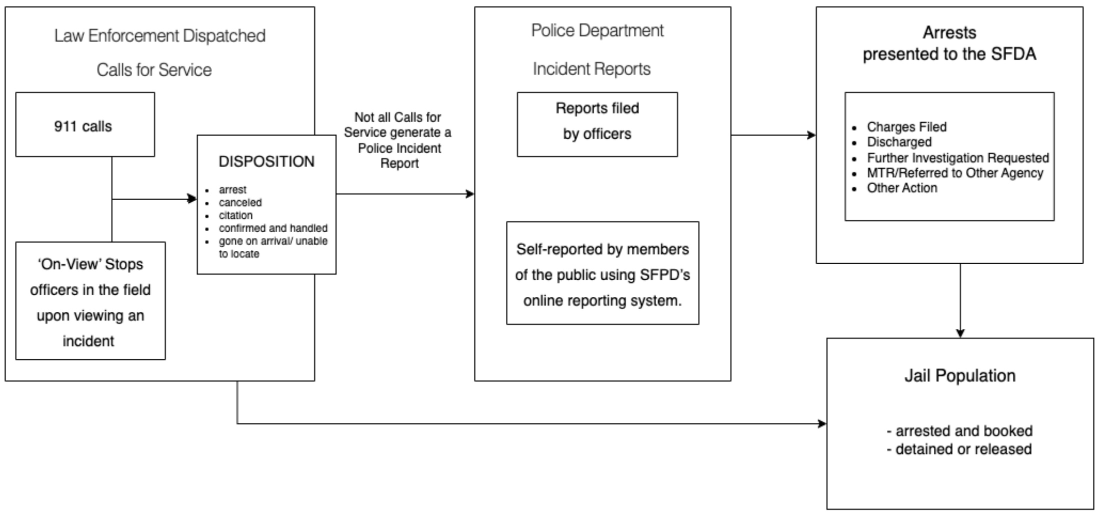
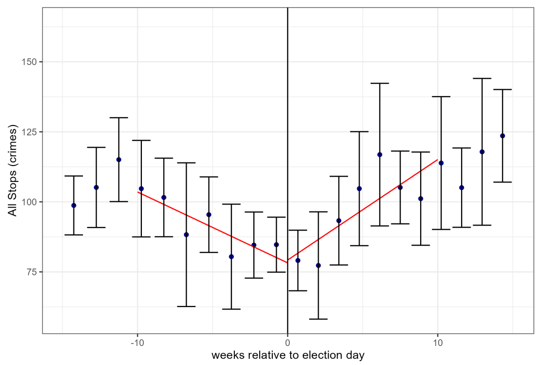
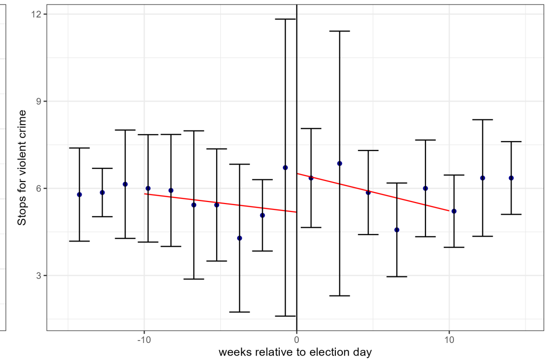


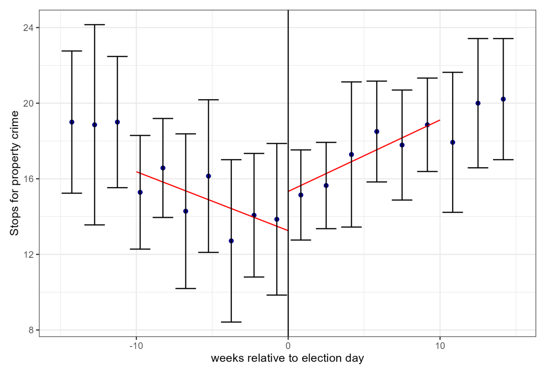
Figure 18.1: Discretion Points at Each Stage of a Criminal Incident



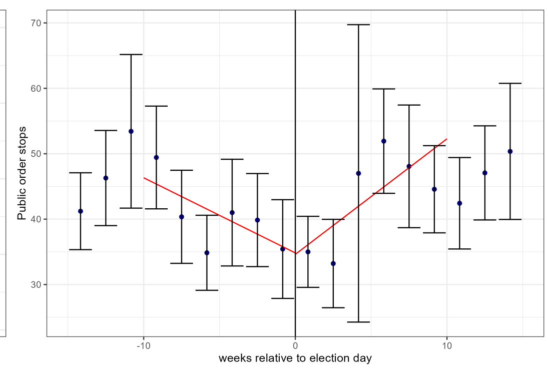
(a) All crime related stops



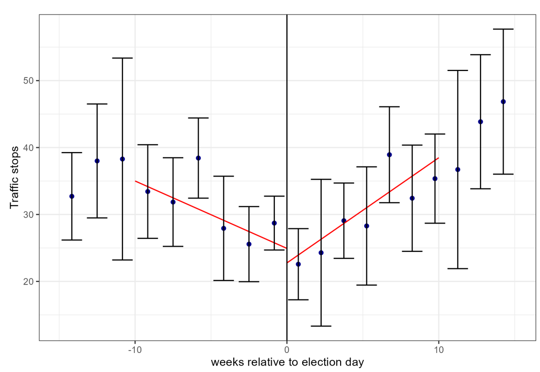
(b) Violent



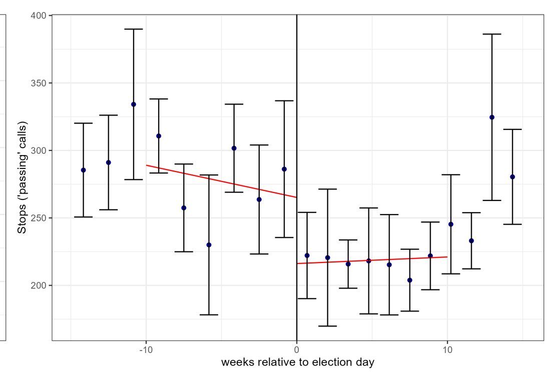
(c) Property



(d) Public order



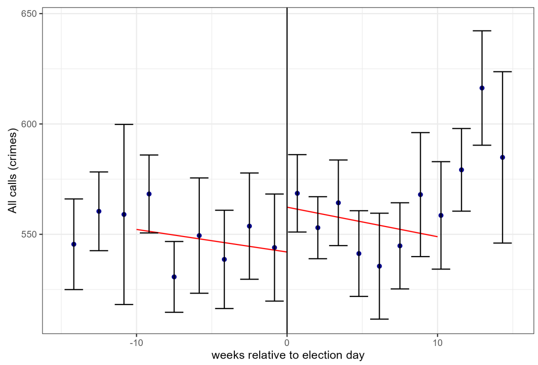
(e) Traffic



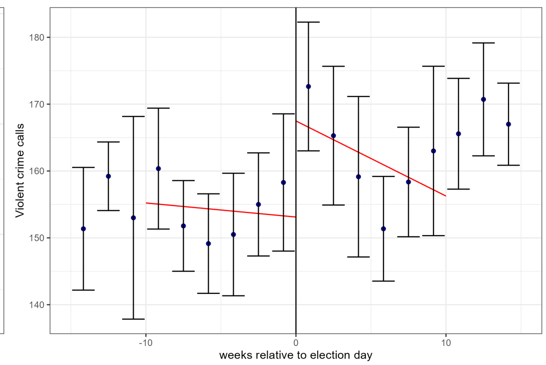
(f) Passing-calls

Figure 18.2: Officers' Stops, weekly 2022

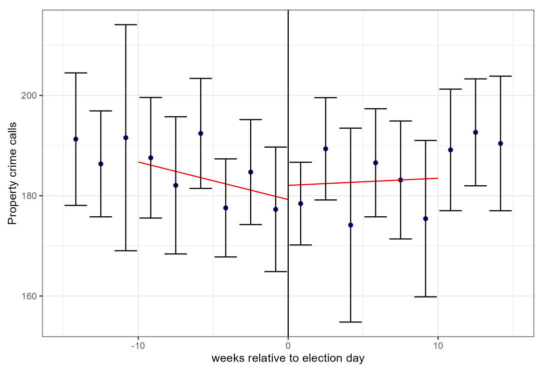
Note: Daily police stops data from 2022. The vertical line marks the recall election date (June 7th). Generated using the rdplot function in R's rdrobust package. Two local regression models are estimated, one for the period before the treatment week and one for the period after, using a bandwidth of 10 weeks to construct the fits.



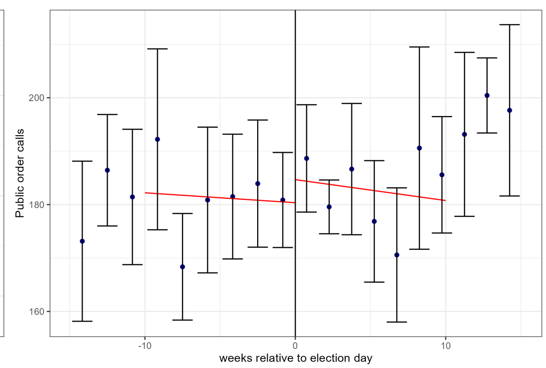
(a) All crime related calls



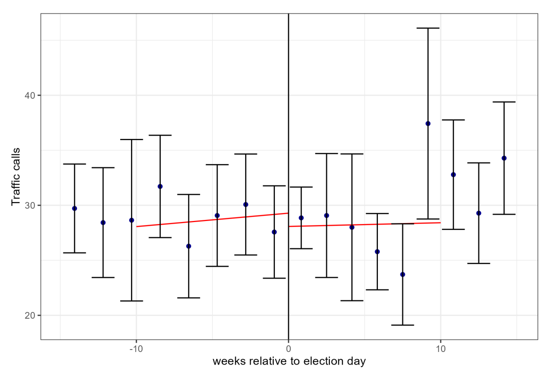
(b) Violent



(c) Property



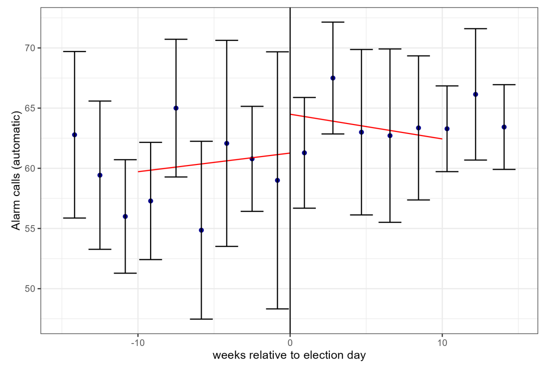
(d) Public order



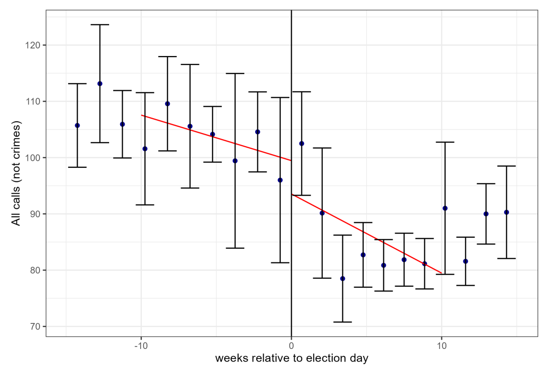
(e) Traffic

Figure 18.3: 911 Calls, weekly 2022

Note: Daily police calls data from 2022. The vertical line marks the recall election date (June 7th). Generated using the rdplot function in R's rdrobust package. Two local regression models are estimated, one for the period before the treatment week and one for the period after, using a bandwidth of 10 weeks to construct the fits.



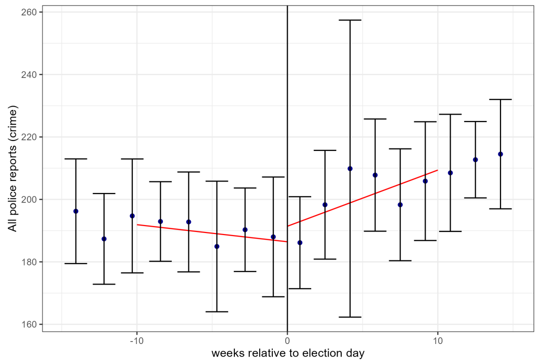
(a) Alarm



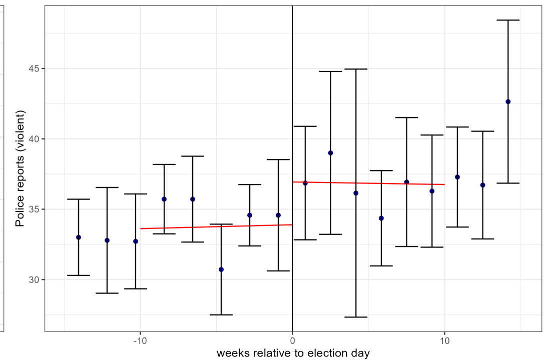
(b) Wellbeing, mental/public health

Figure 18.4: 911 Calls (placebo), monthly 2022

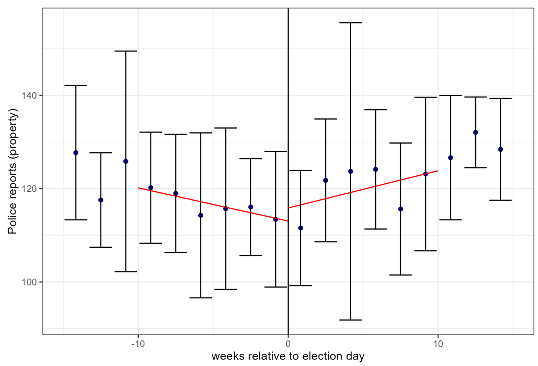
Note: Daily police calls data from 2022. The vertical line marks the recall election date (June 7th). Generated using the rdplot function in R's rdrobust package. The function uses the mimicking variance evenly-spaced method (esmv) to select the number of bins for the running variable (weeks relative to election day) to minimize the variance of the estimated treatment effect. The mean outcome variable and its standard error are calculated within each bin, with the latter used to generate 95% confidence intervals. Two local regression models are estimated, one for the period before the treatment week and one for the period after, using a bandwidth of 10 weeks to construct the fits.



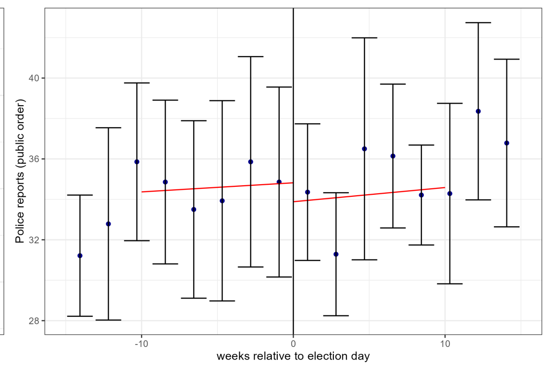
(a) All crime reports by officers



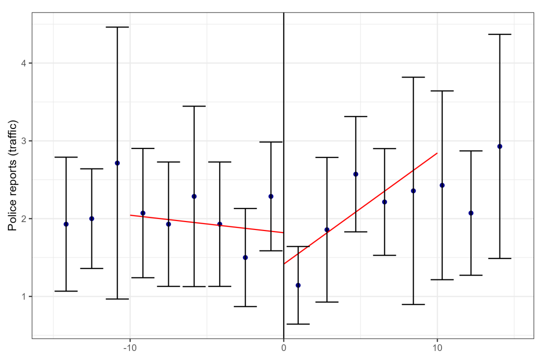
(b) Violent



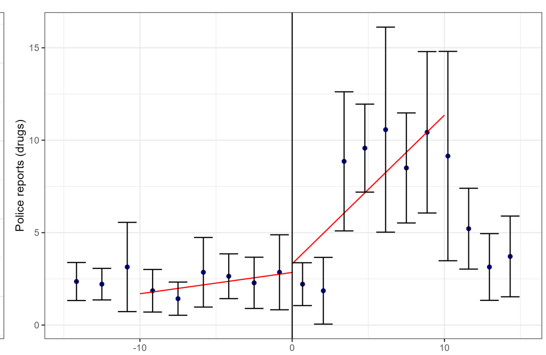
(c) Property



(d) Public order



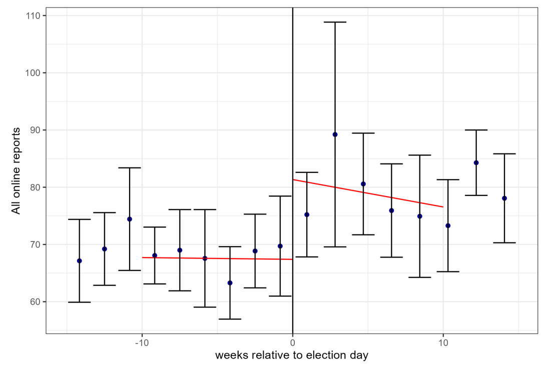
(e) Traffic



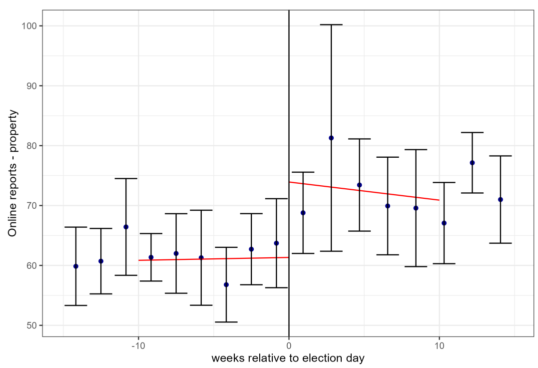
(f) Drug

Figure 18.5: Reports filled by officers, weekly 2022

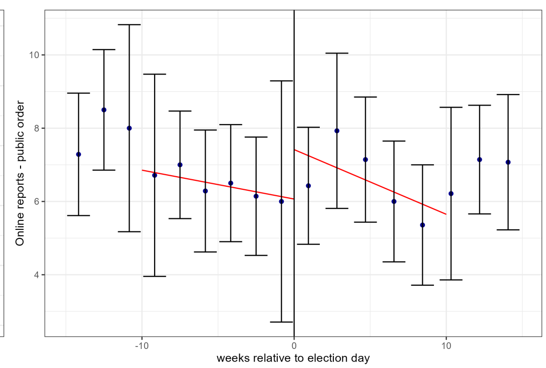
Note: Daily police reports data, 2022. The vertical line marks the recall election date (June 7th). Generated using the rdplot function in R's rdrobust package. Two local regression models are estimated using a bandwidth of 10 weeks to construct the fits.



(a) All crime reports filled online



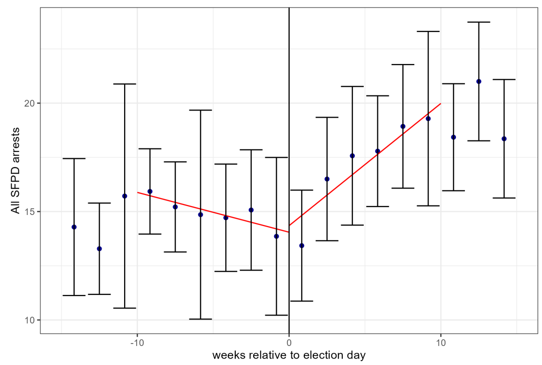
(b) Property



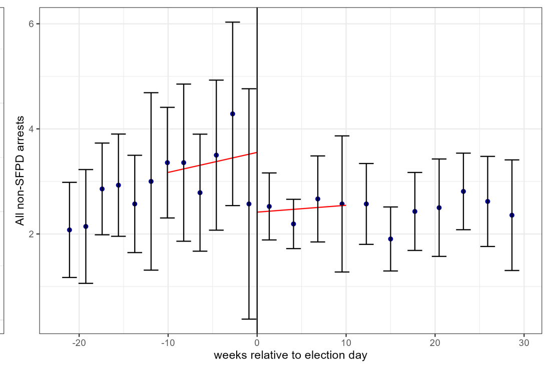
(c) Public order

Figure 18.6: Reports filled online, weekly 2022

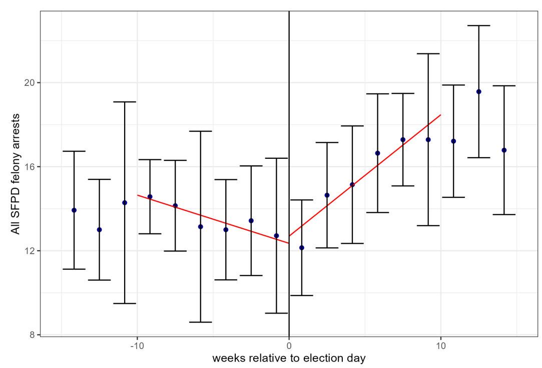
Note: Daily citizen reports data from 2022. The vertical line marks the recall election date (June 7th). Generated using the rdplot function in R's rdrobust package. The function uses the mimicking variance evenly-spaced method (esmv) to select the number of bins for the running variable (weeks relative to election day) to minimize the variance of the estimated treatment effect. The mean outcome variable and its standard error are calculated within each bin, with the latter used to generate 95% confidence intervals. Two local regression models are estimated, one for the period before the treatment week and one for the period after, using a bandwidth of 10 weeks to construct the fits.



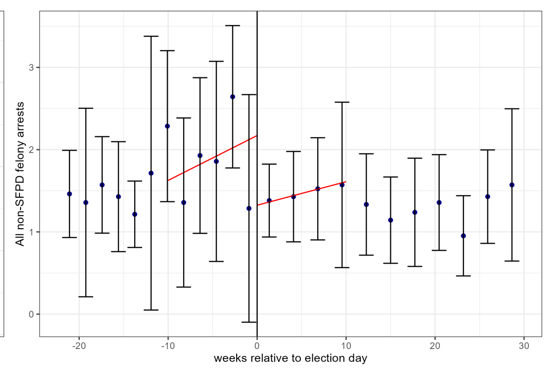
(a) Arrests of SFPD



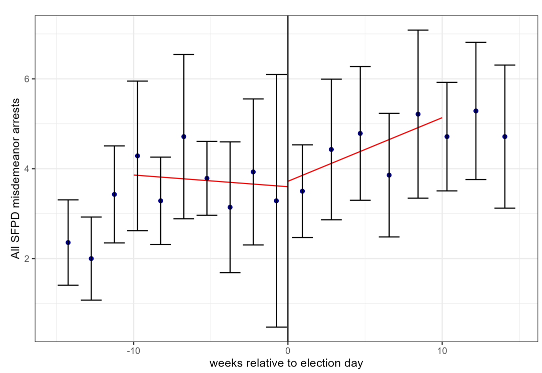
(b) Arrests by not SFPD



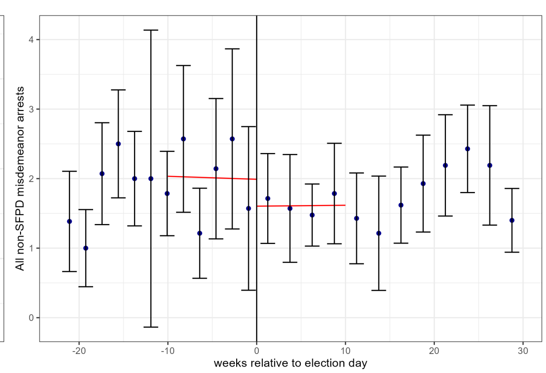
(c) Felony arrests of SFPD



(d) Felony arrests by not SFPD



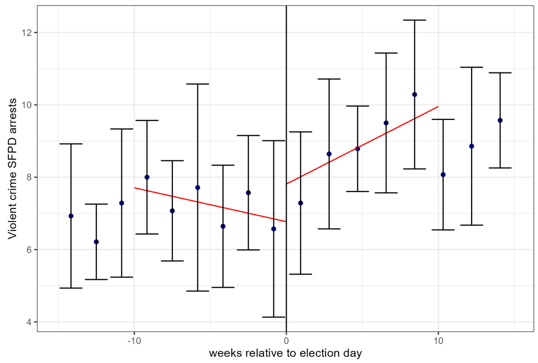
(e) Misdemeanor arrests of SFPD



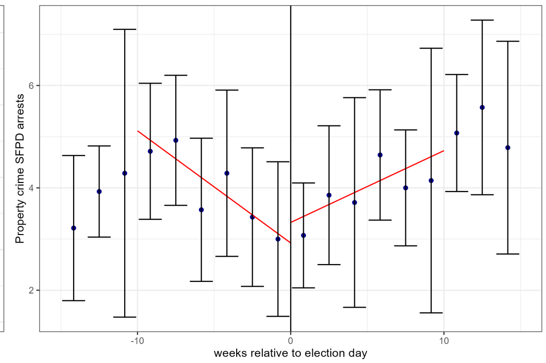
(f) Misdemeanor arrests by not SFPD

Figure 18.7: Arrests, weekly 2022

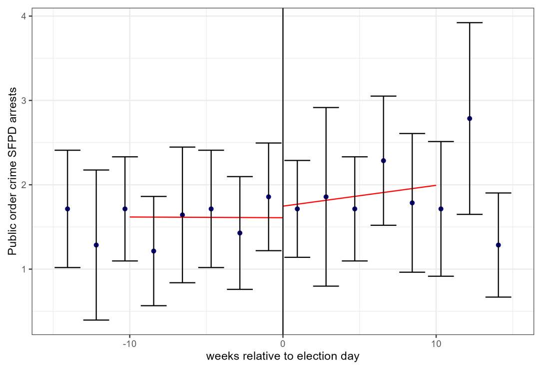
Note: Daily police arrests data from 2022. The vertical line marks the recall election date (June 7th). Generated using the rdplot function in R's rdrobust package. Two local regression models are estimated, one for the period before the treatment week and one for the period after, using a bandwidth of 10 weeks to construct the fits.



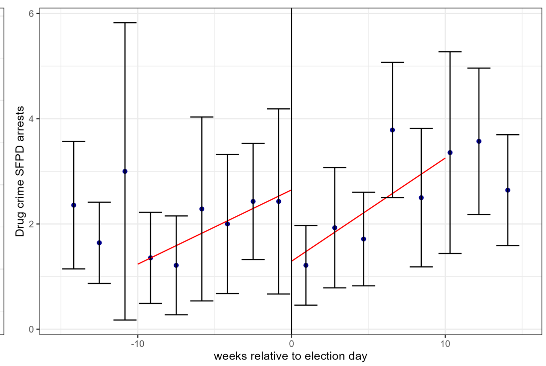
(a) Violent



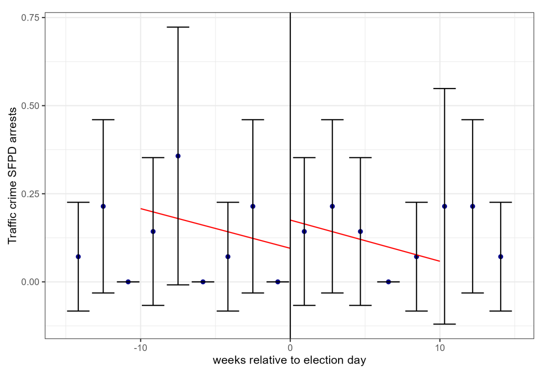
(b) Property



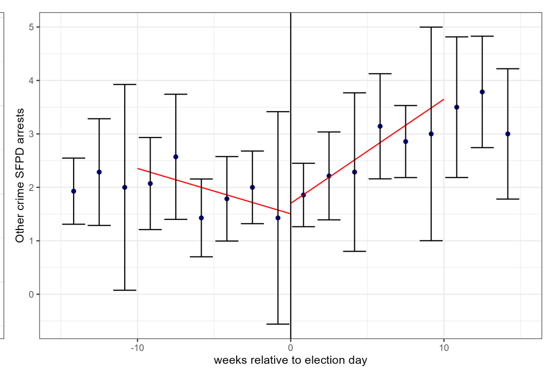
(c) Public order



(d) Drug



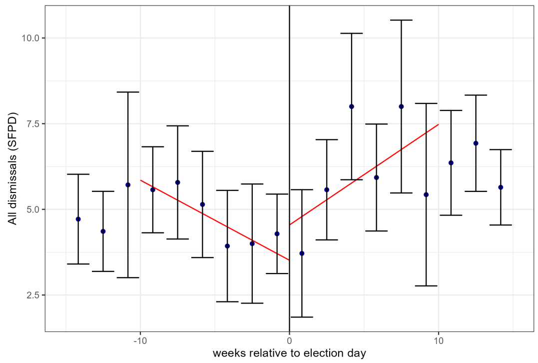
(e) Traffic



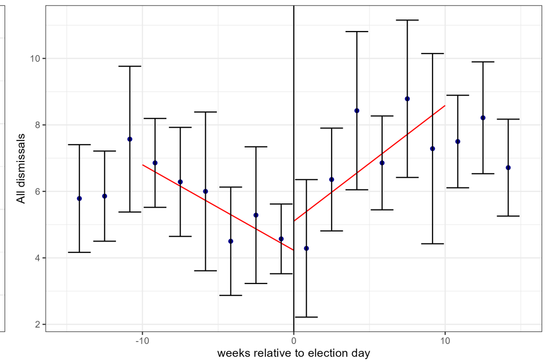
(f) Other

Figure 18.8: Arrests of SFPD by types of crimes, weekly 2022

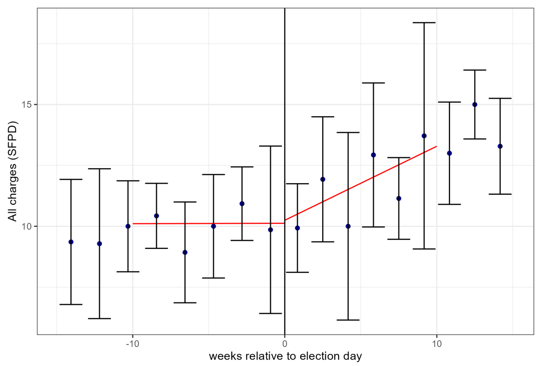
Note: Daily SFPD arrest data from 2022. The vertical line marks the recall election date (June 7th). Generated using the rdplot function in R's rdrobust package. Two local regression models are estimated, one for the period before the treatment week and one for the period after, using a bandwidth of 10 weeks to construct the fits.



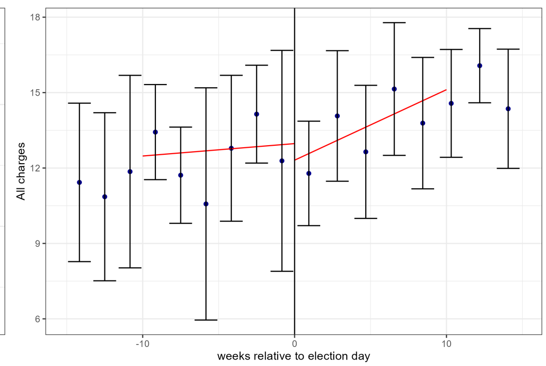
(a) Num of dismissals of SFPD arrests



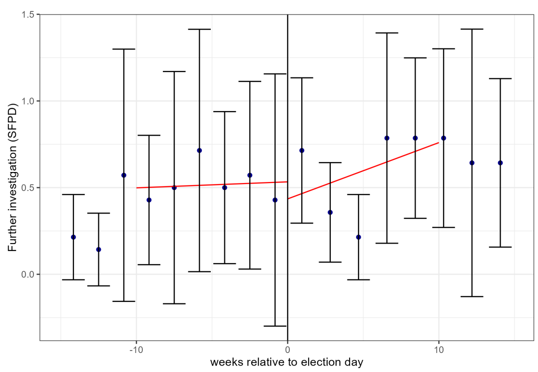
(b) Num of dismissals of all arrests



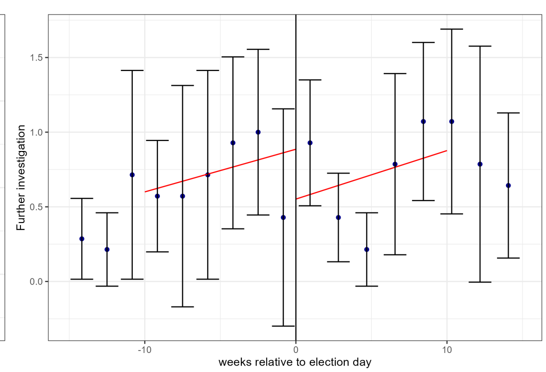
(c) Num of charges of SFPD arrests



(d) Num of charges of all arrests



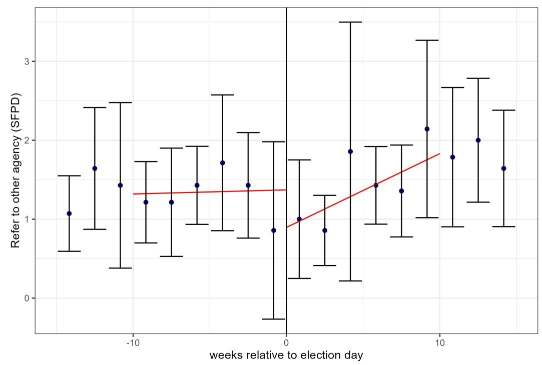
(e) Num of further investigation requested of SFPD arrests



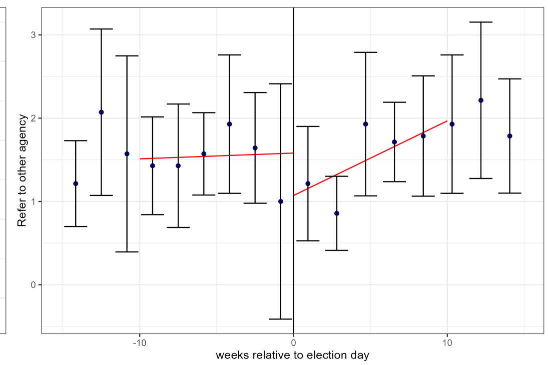
(f) Num of further investigation requested of all arrests

Figure 18.9: DA action of arrests, weekly 2022

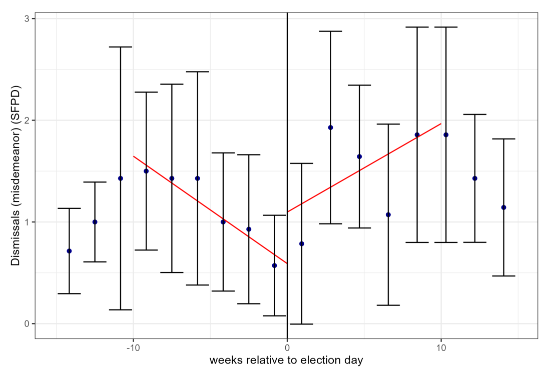
Note: Figure continued on the next page.



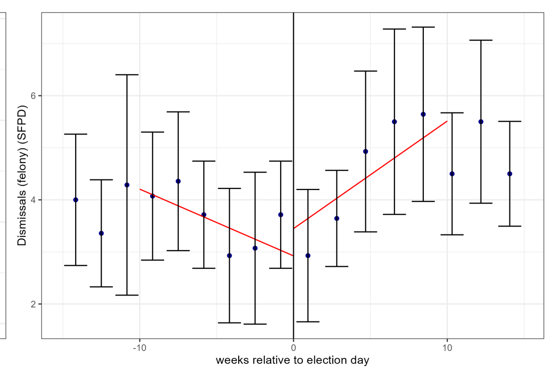
(g) Num of MTR/Referred to other agency of SFPD arrests



(h) Num of MTR/Referred to other agency of all arrests



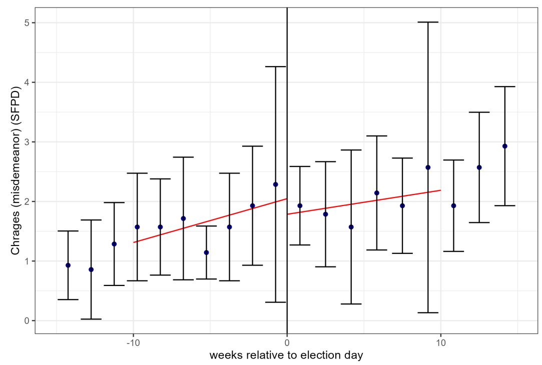
(i) Dismissals of misdemeanor SFPD arrests



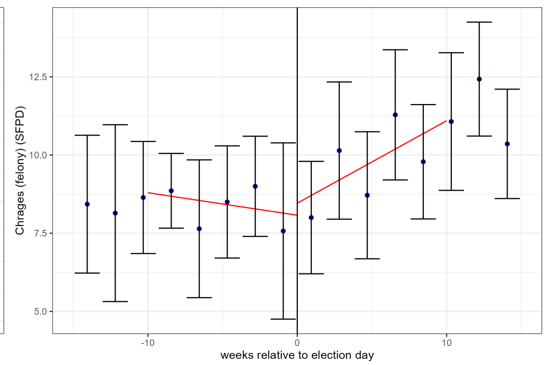
(j) Dismissals of felony SFPD arrests

Figure 18.9: DA action of arrests, weekly 2022

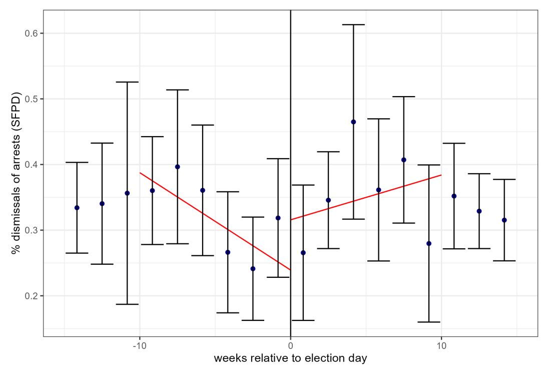
Note: Figure continued on the next page.



(k) Charges of misdemeanor SFPD arrests



(l) Charges of felony SFPD arrests



(m) % dismissals of SFPD arrests

Figure 18.9: DA actions of arrests, weekly 2022

Note: Daily DA action on arrests presented data from 2022. The vertical line marks the recall election date (June 7th). Generated using the rdplot function in R's rdrobust package. The function uses the mimicking variance evenly-spaced method (esmv) to select the number of bins for the running variable (weeks relative to election day) to minimize the variance of the estimated treatment effect. The mean outcome variable and its standard error are calculated within each bin, with the latter used to generate 95% confidence intervals. Two local regression models are estimated, one for the period before the treatment week and one for the period after, using a bandwidth of 10 weeks to construct the fits.

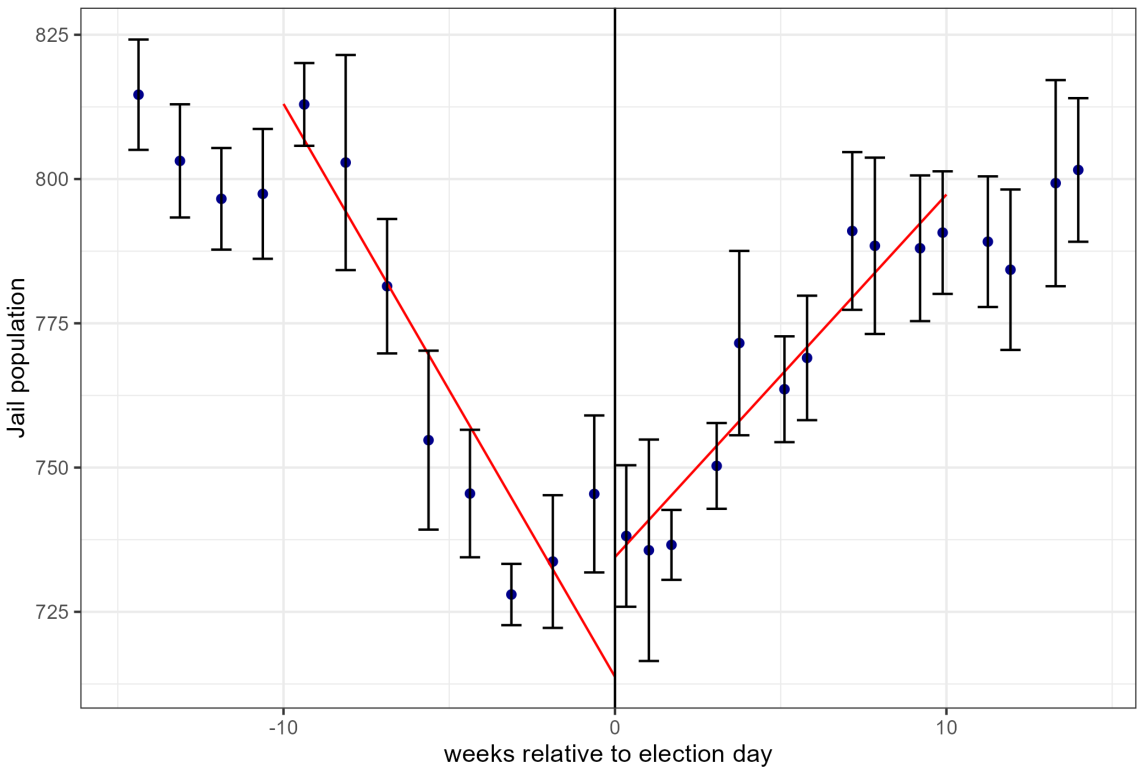


Figure 18.10: Daily jail population

Note: Daily jail population data from December 24th, 2021, until Decemebr 28th, 2022. The vertical line marks the recall election date (June 7th). This figure was generated using the rdplot function in R's rdrobust package. The function uses the mimicking variance evenly-spaced method (esmv) to select the number of bins for the running variable (weeks relative to election day), to minimize the variance of the estimated treatment effect. Within each bin, the mean jail population and its standard error are calculated, with the latter used to generate 95% confidence intervals represented by the error bars in the plot. Two local regression models are estimated, one for the period before the treatment week and one for the period after, using a bandwidth of 10 weeks to construct the fits.

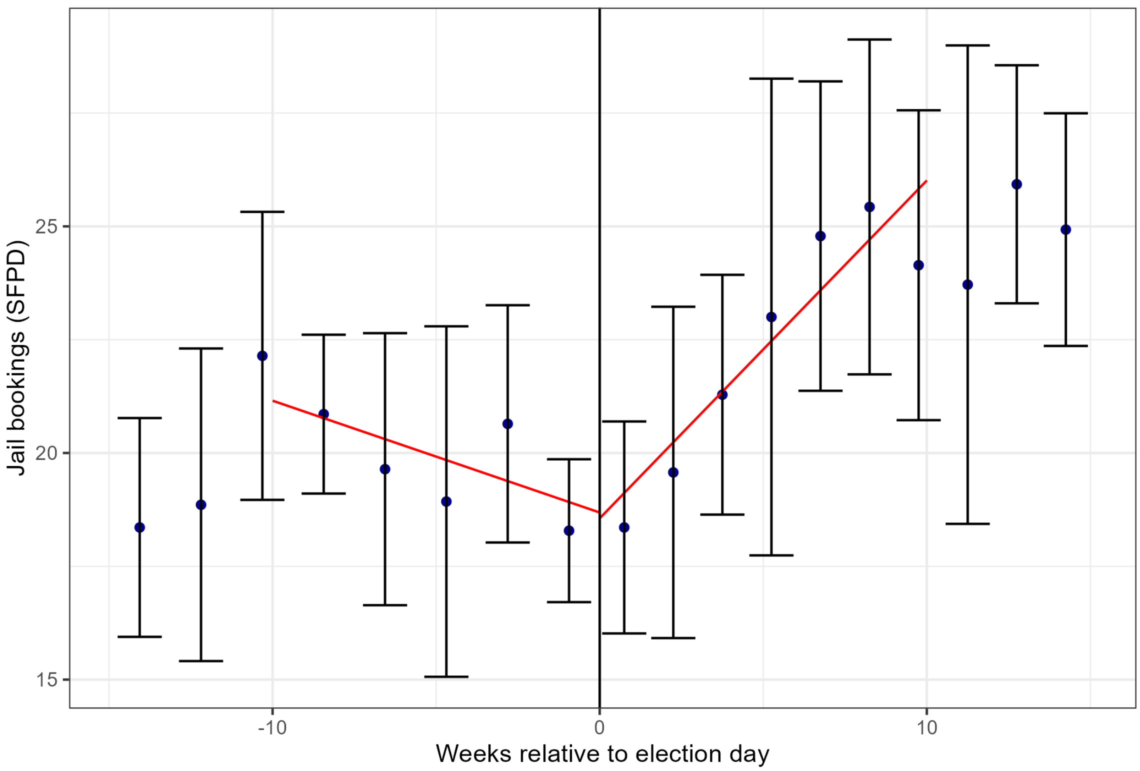


Figure 18.11: Jail bookings from SFPD

Note: This figure presents a regression discontinuity plot of weekly jail bookings in San Francisco by weeks relative to the recall election day. The analysis uses the rdrobust package in R with a polynomial order of 1. The plot is divided into bins of the running variable as determined by an evenly-spaced method that mimics variance ("esmv"), the default method used by the rdrobust package. Each point on the plot represents the average number of jail bookings within a bin. The error bars represent the 95% confidence interval around each bin's mean. The solid lines on either side of the discontinuity represent local polynomial fits, which are used to model the relationship between the running variable and the outcome variable within the specified bandwidth of 10 weeks in both directions.

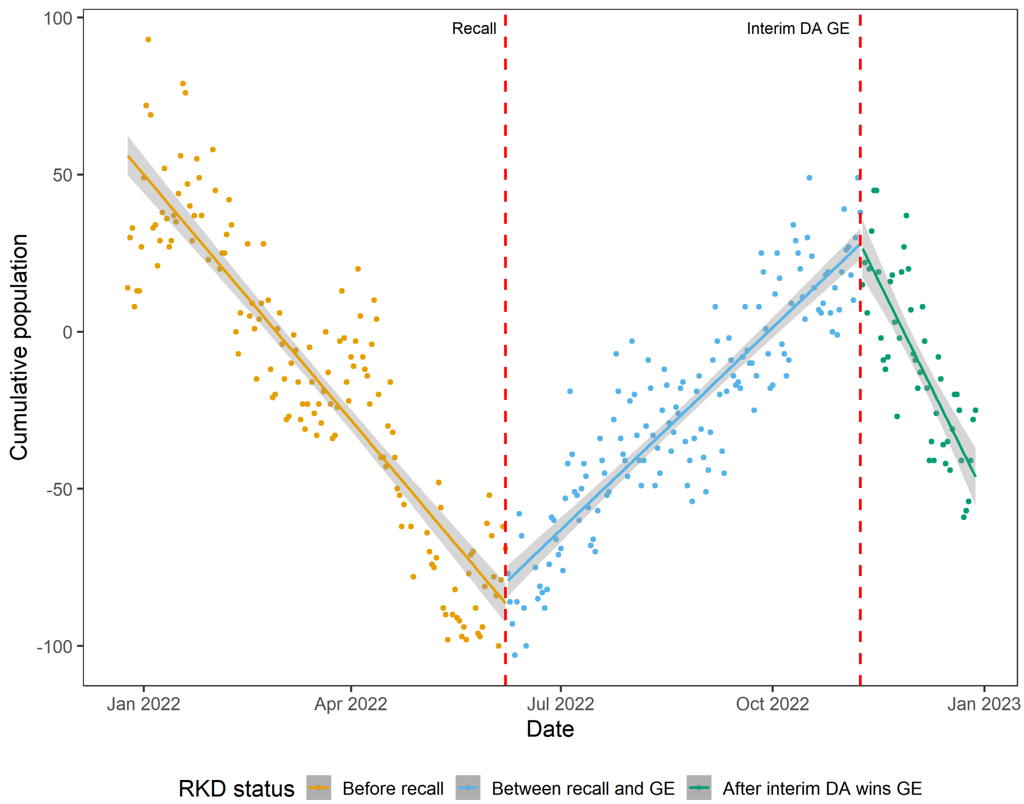


Figure 18.12: Cumulative jail population

Note: Cumulative daily jail population data from December 24th, 2021, until Decemebr 28th, 2022. The vertical lines mark the recall election date (June 7th) and the general election (November 8th). The regression lines are the first polynomial with 95% confidence intervals.

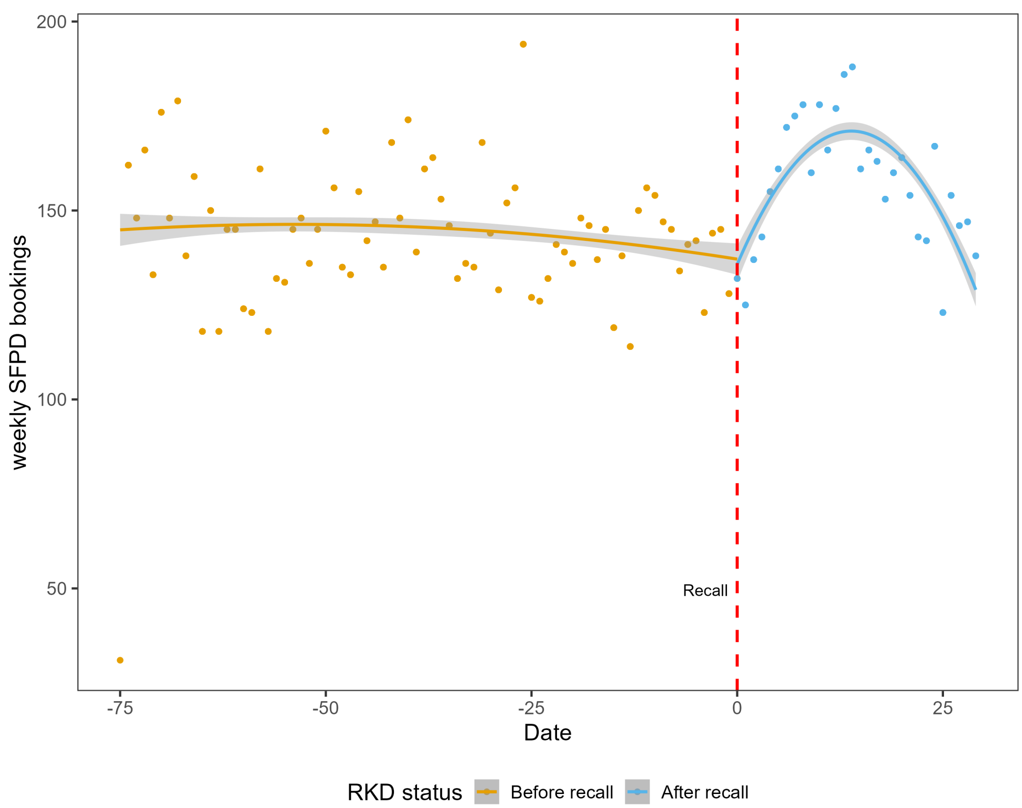


Figure 18.13: Jail bookings from SFPD

Note: This figure shows the total bookings made by SFPD by week relative to the recall election week (x = 0). The regression lines show the second polynomial relationship between the weekly bookings and the week of the recall election with 95% confidence intervals.

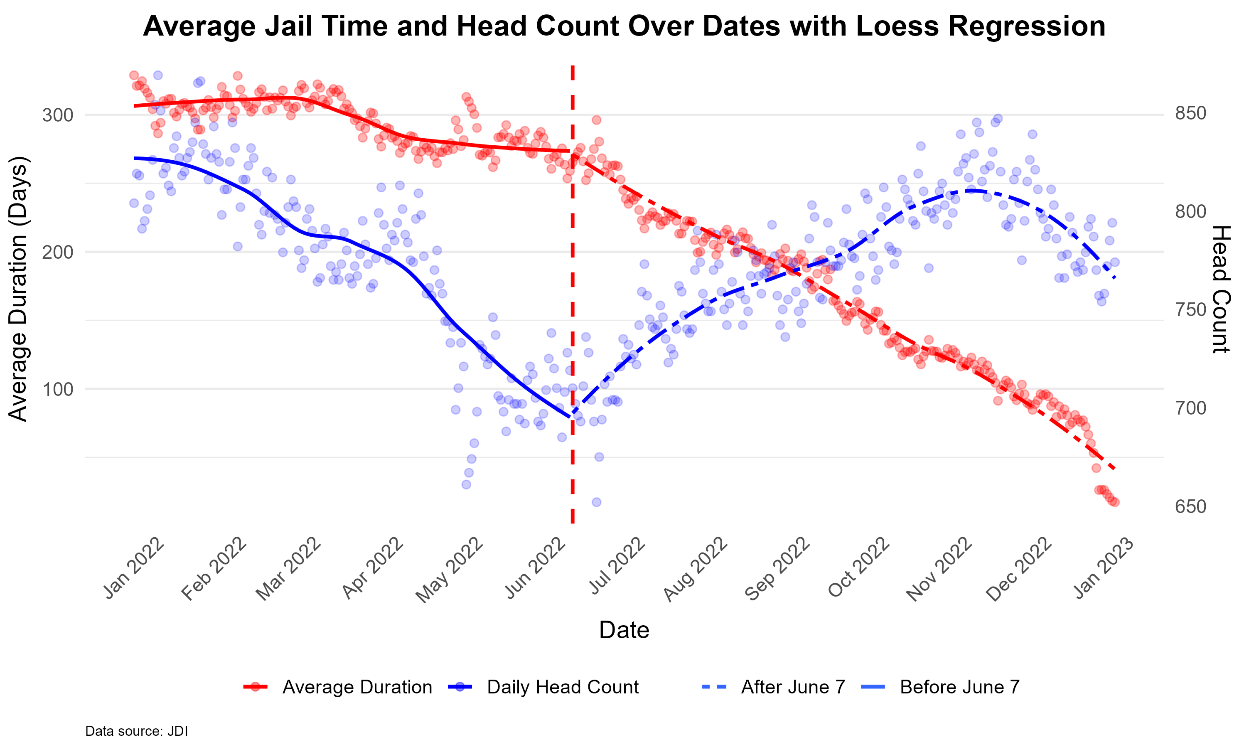


Figure 18.14: Average Jail Stay Duration

Note: This figure shows in red the average duration, till release, in days of all the people in San Francisco jail on a given day with a Loess-fitted line. In blue, the figure shows the jail population.

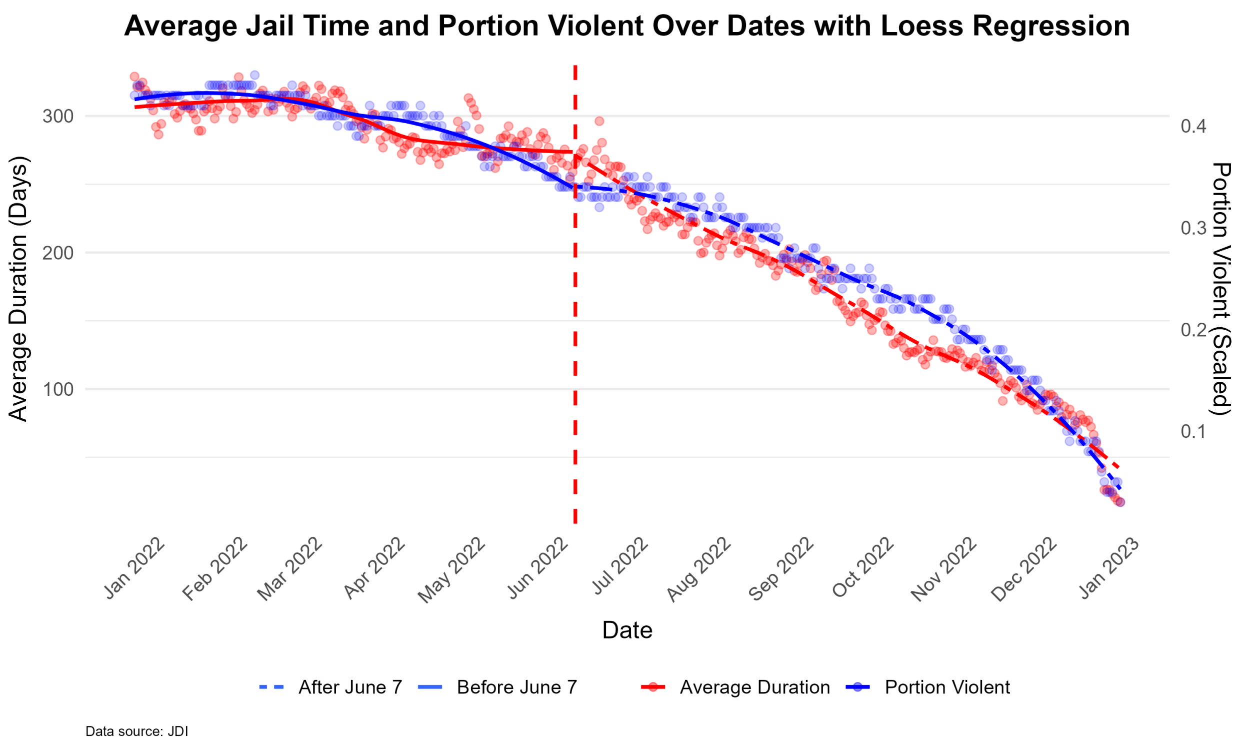


Figure 18.15: Average Jail Stay Duration

Note: This figure shows in red the average duration, till release, in days of all the people in San Francisco jail on a given day with a Loess-fitted line. In blue, the figure shows the proportion of people held in jail with a top offense ”violent.”

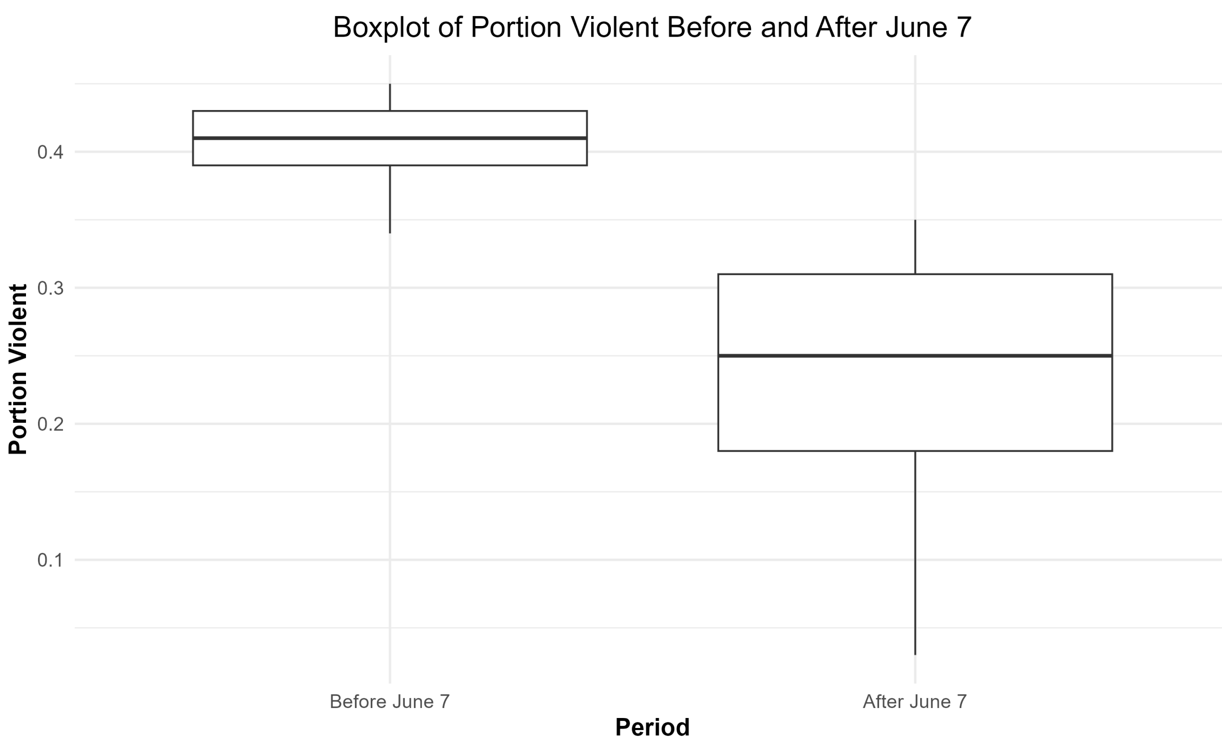
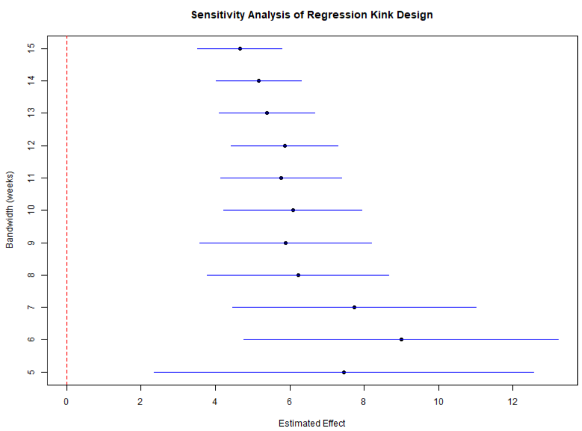


Figure 18.16: Average Jail Stay Duration

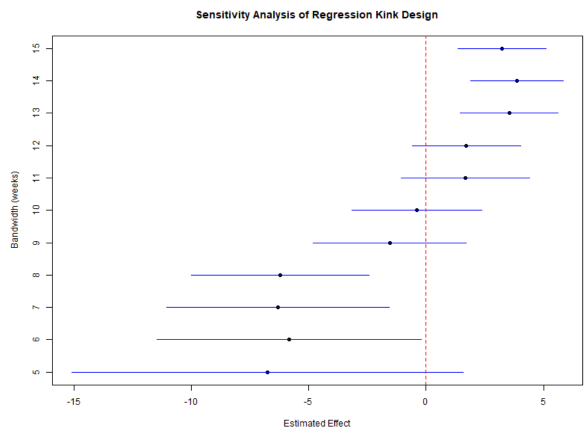
Note: This figure shows boxplots of the proportion of people held in jail with a top offense "violent."

Supplementary Information

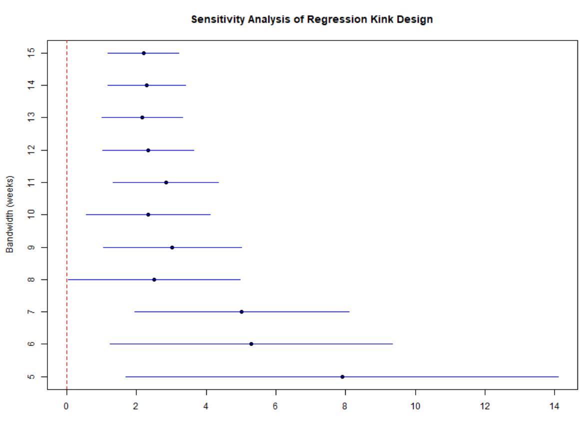
BW Sensitivity



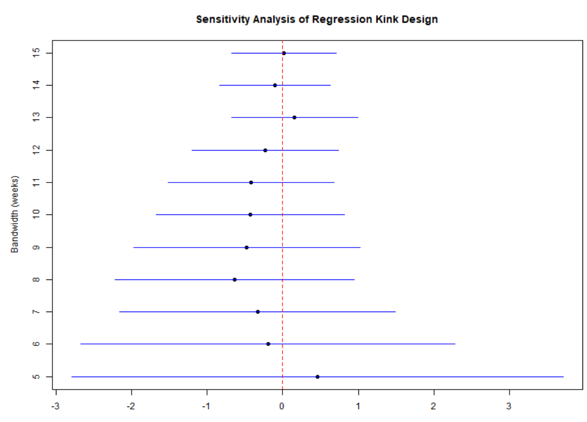
(a) All stops



(b) All calls

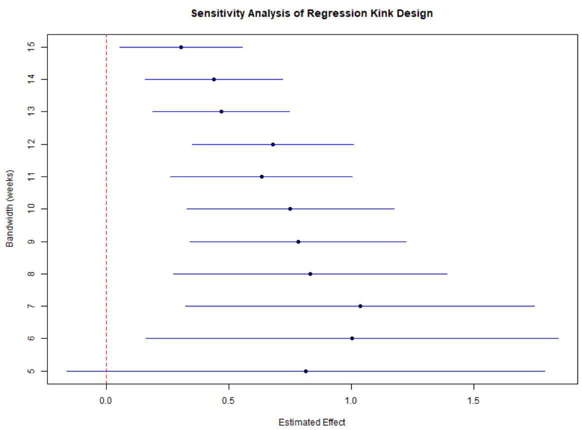


(c) Police report incidents

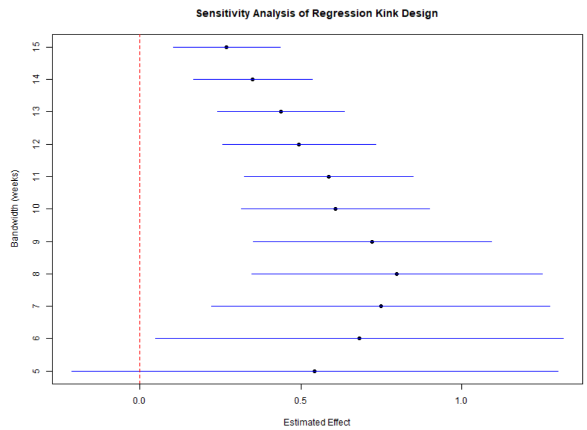


(d) Online incident reports

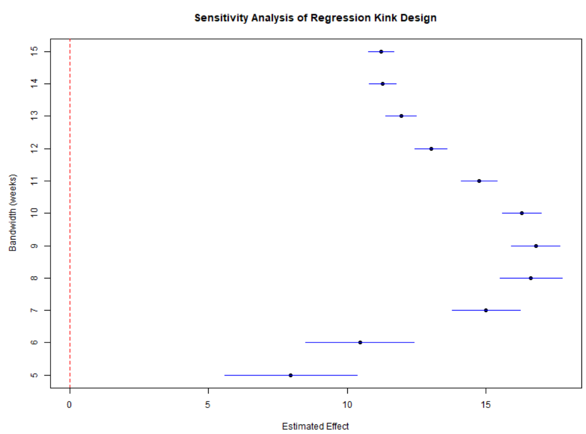
Figure S0.1: BW sensitivity analysis, Part 1



(e) All arrests of SFPD



(f) SFDA total dismissals



(g) Jail population

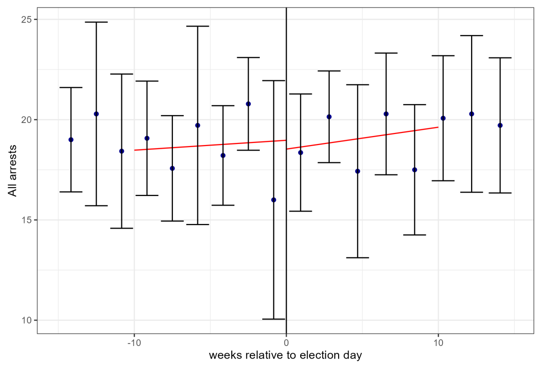
Figure S0.1: BW sensitivity analysis, Part 2

Table S0.1: Placebo test - 2021 data

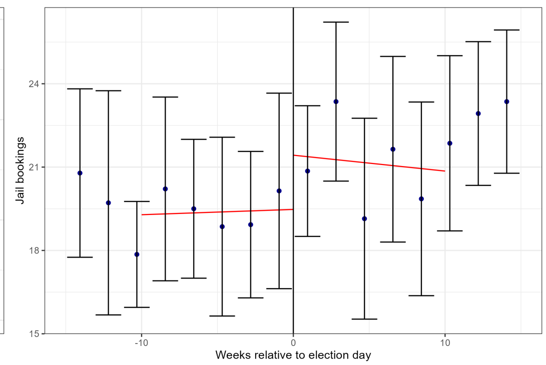
Outcome	Slope change	Trend pre-election	Trend post-election
Police Behavior			
Police stops			
All Stops (crimes only)	3.715*	-	-
Police reports			
All Incident Reports (crime)	-3.000	+	-
Police arrests (SFPD)			
All arrests	0.073	+	+
All felony arrests	0.043	+	+
All misdemeanor arrests	-0.187	+	-
Residents Behavior			
Residents Calls			
Crime related	-5.36***	+	-
Non-crime related	-0.953	+	-
Residents Online Reports			
All residents' online reports	-4.248***	+	-
DA Behavior			
All charges	-0.091	+	-
All dismissals	-0.239*	+	+
Jail Population			
Bookings (SFPD)	0.068	0	-

Note: All analyses utilize the rdrobust function to estimate the change in slope of the outcome concerning the weeks around June 7, 2021. We repeat the analysis of the 2022 data, replaced with 2021 data as a sanity test. The specification spans a 10-week bandwidth. All estimates rely on full police data: SFPD and other agencies.

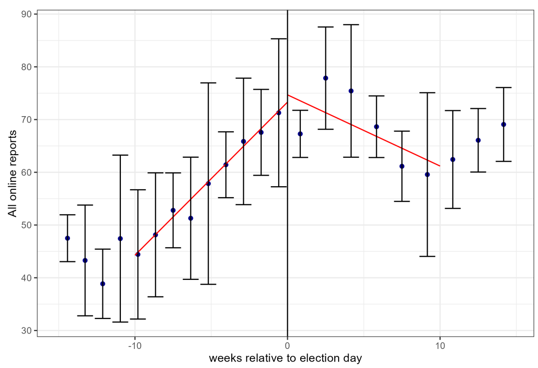
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$



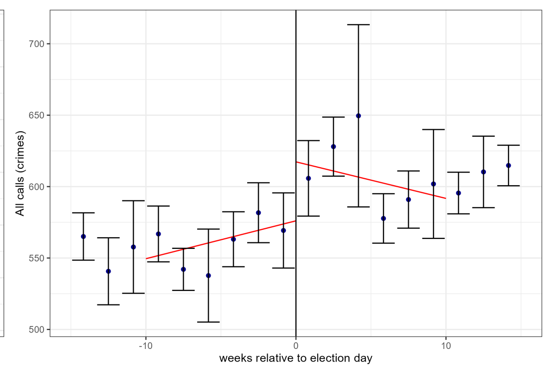
(a) Arrests



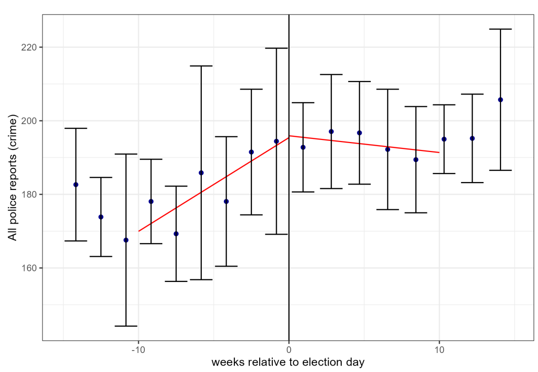
(b) Jail bookings by SFPD



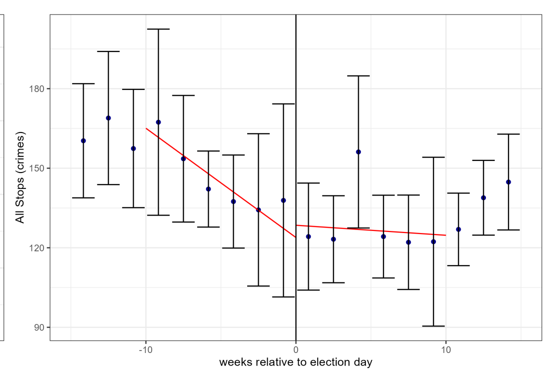
(c) Online residents reports



(d) Citizen 911 calls



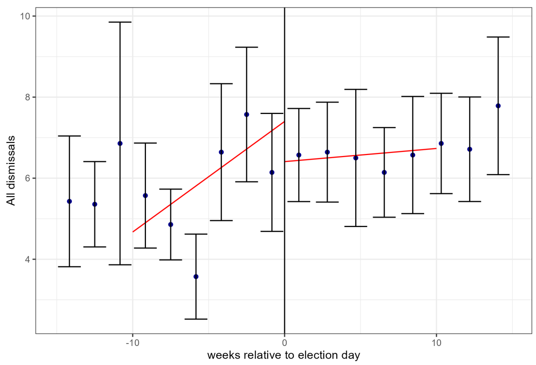
(e) Police reports (crime)



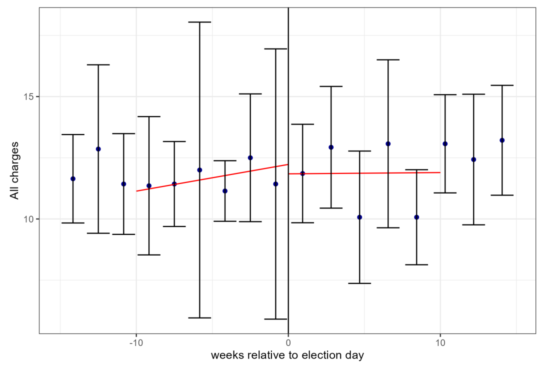
(f) Police stops

Figure S0.2: Main Placebo Results, weekly 2021

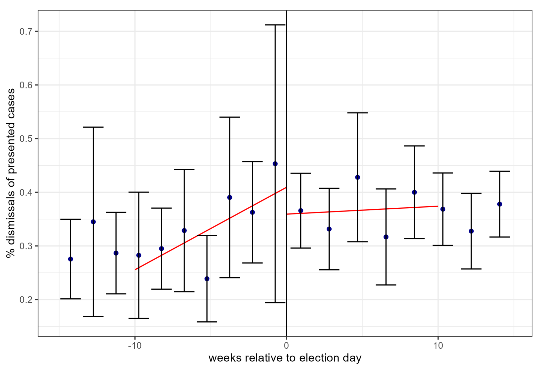
Note: Figure continued on the next page.



(g) Cases dismissed by the DA



(h) Cases charged by the DA



(i) Portion of dismissed cased out of arrests presented

Figure S0.2: Main Placebo Results, weekly 2021
Note: Januar 1st - Decmber 31st, 2021. The vertical line marks June 7th. Generated using the rdplot function in R's rdrobust package. The function uses the mimicking variance evenly-spaced method (esmv) to select the number of bins for the running variable to minimize the variance of the estimated treatment effect. The mean outcome variable and its standard error are calculated within each bin, with the latter used to generate 95% confidence intervals. Two local regression models are estimated, one for the period before the treatment week and one for the period after, using a bandwidth of 10 weeks to construct the fits.

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Chapter 4

Criminal Justice Policy Preferences and Group Cues

1 Introduction

Political communication about criminal legal reform is abundant in current American politics. President Biden promised voters to "reform our criminal justice system,"[1] while former President Trump signed the First Step Act in December 2018, announcing it would "give former inmates a second chance at life."[2] Both, in their 2020 campaigns, ran ads that depicted Black Americans discussing reforming the penal system.[3] In addition, social movements and political activists such as Black Lives Matter and the ACLU joined the call to adopt policies that restrict the power of police officers and empower prosecutors and judges to hold officers accountable after highly publicized instances of police brutality, often against Black Americans. Political interest groups raise millions to influence voters' attitudes to support criminal legal reform.[4] Yet, despite the enormous political effort and financial fortune invested in favor of progressive reform by various groups, we know little about how group identity and persuasion strategies shape public attitudes in this political context. This article shows that voters follow cues from racial minorities' when making decisions about progressive criminal legal reform support. I argue that this results from identifying criminal legal reform with racial justice.

In an environment saturated with information about the connections between race and crime, this article explains how racial groups matter for evaluations of policy reform. I argue that support for progressive criminal legal reform is closely tied to support for racial justice. Signals about the relationship between racial groups, racial attitudes, and the criminal legal system are common (Gilliam Jr and Iyengar 2000). In 2022, for example, Los Angeles City Council President Nury Martinez made these remarks while speaking about Los Angeles County District Attorney George Gascón: "F— that guy ... He's with the Blacks".[5] Trump's campaign ad for criminal legal reform echoed the style of Freedom and Civil Rights movement media from the 50s and 60s, binding criminal rights with civil rights ideas.[6]

Previous research has predominantly focused on public support for "getting tough on crime" to comprehend the political triumph of stringent law enforcement policies. But successfully transitioning away from punitive politics in which "the tougher, the better" requires understanding progressive policy preferences. Contemporary movements advocating for criminal justice reform receive millions of dollars in donations to support progressive criminal justice campaigns,[7]. The progressive movement emphasizes alternative strategies to achieve safety, such as reducing police presence and incarceration rates in lieu of adopting non-repressive, rehabilitative disciplinary measures. Given the limitations of relying on factual information to nudge preferences toward progressive change (Esberg, Mummolo, and

[1] Biden campaign.

[2] Trump announcement.

[3] Trump 2020 Super Bowl ad; Biden 2020 "Shop Talk" ad.

[4] For example, RealJusticePAC.org, ColorofChange.org, and Moveon.org publish their donations information which is available here: opensecrets.org.

[5] LA Times.

[6] Trump 2020 Super Bowl ad.

[7] See, for example, Real Justice donors, Color of Change spending by election cycle.

Westwood 2020), exploring the impact of attitudes towards social groups on shaping these preferences becomes crucial.

Across two studies, this article finds that Black and white voters are more likely to support progressive criminal justice reforms if they learn that Black voters support those reforms, conditional on positive racial attitudes. Study 1 utilizes a conjoint experimental design to causally gauge the marginal influence of varying policies and social as well as political group cues on respondents' advocacy for any kind of reform. Study 2 expands the analysis of group cues and racial attitudes and narrows the focus to the study of progressive reform.

2 Why Racial Group Cues Matter?

This article theorizes that attitudes toward other social groups and their interests affect criminal legal reform preferences. Specifically, support for criminal justice reform is related to an observed trend in White racial attitudes becoming more cognizant of racial justice concerns. The extant research shows that Whites in the United States are coming to terms with the pernicious impact of racial animus (Krysan and Moberg 2016; Hopkins and Washington 2020; Engelhardt et al. 2019). Moreover, people acknowledge the harm a discriminatory criminal legal system has inflicted on minority communities (Brenan 2020; Pew Research Center 2015; Pew Research Center 2020a; Pew Research Center 2020b). These two points are the basis for the argument that supporting criminal legal reform hinges on support for elevating minority citizens' civil and social rights. This aligns with evidence regarding support for the Black Lives Matter movement (Drakulich et al. 2021).

Citizens' policy preferences rarely hinge on perceptions of their material interests, and when it comes to crime, citizens rarely hold the correct knowledge about crime (Esberg and Mummolo 2018); instead, citizens often depend on loyalties to social groups (Converse 2006; Krosnick, Visser, and Harder 2010). Further, people can infer a group's position on policy, even when not explicitly known, and support a policy (Brady and Sniderman 1985; Elder and O'brian 2022) if they feel positively toward the social group that would benefit from it (Nelson and Kinder 1996). Thus, this article relies on group cues and heuristic projection theories (Bullock 2020; Broockman, Kaufman, and Lenz 2023) to extend the knowledge on criminal legal reform support by suggesting that exposure to the preferences of Black voters might serve as a lead when other voters decide on the desired policy. The following sections explain, firstly, the differences in criminal justice attitudes between racial groups and the relationship to racial attitudes, and secondly, the effect of out-group cues on in-group attitudes.

Factors related to racial identity and attitudes

Public opinion is "the central consideration in the making of penal policy" (Gottschalk 2006, p. 12). The prevalent theory suggests that changes in public punitive sentiment significantly impact policy, predicated on findings showing that policy responsiveness evolves from lawmakers' anticipation of the general policy direction—rather than the exact policies—that the public prefers (Bartels and Stimson 1992; Stimson 2004; Stimson, Mackuen, and Erikson 1995). The American public's punitive sentiment is known to move in parallel trends when accounting for race, political ideology, and gender (S. W. Duxbury 2021b; Enns 2016; Ramirez 2013). Ramirez (2013) found highly correlated trajectories in punitive sentiment for blacks and whites, men and women, and different age groups at the national level. However, public opinion on crime and justice is far from homogeneous, even if it changes in parallel over time (S. W. Duxbury 2021a; S. W. Duxbury 2021b). Researchers find significant gaps when accounting strictly for the difference between groups in cross-sectional studies (Jefferson, Neuner, and Pasek 2021).

Racial differences in punitive sentiment and trust in the police are notably stark (Jefferson 2022; Boudreau, MacKenzie, and Simmons 2019). Between 1953 and 2006, only 11 percent of black respondents in 34 national polls supported capital punishment for convicted murderers, in contrast to 89 percent of non-blacks (Shirley and Gelman 2015). Black Americans are not a homogeneous uni-dimensional group, and there is a gamut of opinions and attitudes regarding punitive policies (e.g., Dawson 2001; Forman Jr 2017; Jefferson, Neuner, and Pasek 2021; Jefferson 2019). Yet, evidence on heterogeneous racial reaction to racial and partisan group cues is sparse (Stephens-Dougan 2021).

Given the historical intersections of racial and criminal justice in America, it is unsurprising that racial attitudes are interrelated with punitive attitudes (Pager 2008; Tonry 2011). As civil rights reforms altered racial dynamics, crime became a pivotal battleground for those seeking to maintain the racial status quo. In 1960, 37% of America's prison population was African American. By 1995, it was 50%; it has remained at that level since. In 1960, the incarceration rate per 100,000 people was 126. In 2006, it was 943. Among black males, the number was 3042. Among black males in their late 20s, the rate exceeded 7000. In 2017, there were 1,549 black prisoners for every 100,000 black adults – nearly six times the imprisonment rate for whites (272 per 100,000) and nearly double for Hispanics (823 per 100,000). Notably, 32% of the US population are African Americans and Hispanics, compared to 56% of the US incarcerated population. If African Americans and Hispanics were incarcerated at the same rates as whites, prison, and jail populations, would decline by almost 40%. There are different crime rates in different demographic groups, but differences in crime rates do not account for the entire difference in the incarceration rate.

It remains unclear if racial identity similarly influences attitudes toward progressive reform (Boudreau, MacKenzie, and Simmons 2022). Reform-oriented politicians have succeeded mainly in urban areas characterized by racial diversity and liberal political leanings (Boudreau, MacKenzie, and Simmons 2019). Indeed, the persistent racial gaps in punitive sentiment and trust in the police, and more recently support for the BLM movement (Drakulich et al. 2021), reveal a structure of group association in which Black Americans support a punitive legal system less.

Furthermore, people's support for tough-on-crime policies in America is intricately interwoven with racial attitudes (Pager 2008; Tonry 2011; Rice, Rhodes, and Nteta 2022). The racial animus model is the most consistent and robust predictor of punitive attitudes (Unnever and Cullen 2010); it contends that negative racial attitudes mediate a preference for punitive policies. After the civil rights movement, politicians effectively linked race and crime in the public's mind (Weaver 2007). Empirical research consistently supports the correlation between racial animus and oppressive attitudes toward punishment, demonstrating the significance of the racial divide in shaping attitudes toward justice and punishment. This association is evident across various domains, including support for the death penalty (Barkan and Cohn 1994; Messner, Baumer, and Rosenfeld 2006; Trahan and Laird 2018; Unnever, Cullen, and Jonson 2008), abstract approval of "get tough" politics (Brown and Socia 2017; Buckler, Wilson, and Salinas 2009; Morris and LeCount 2020; Unnever and Cullen 2010), perceptions of criminal guilt (Rice, Rhodes, and Nteta 2022), and the role of group threat and racial social divides in promoting excessive punishment in America (Chiricos, Pickett, and Lehmann 2020; S. W. Duxbury 2021a; K. B. Smith 2004).

In recent years, there has been a noted decline in negative racial attitudes among white Americans, particularly white Democrats (Jardina and Ollerenshaw 2022; Engelhardt 2021). In line with current trends of diminished racial animus, the Willie Horton effect of "tough-on-crime" cued through racialized signals has shown signs of no longer holding politicians in a tight grip (Thielo et al. 2016; Mendelberg 1997). This shift has prompted calls for a broader consideration of positive racial attitudes in studying racial attitudes (Chudy 2021). This article, therefore, extends the discourse by examining both negative and positive racial attitudes' relationship to criminal justice politics. It seeks to enrich our understanding of how positive racial attitudes may be linked to support for progressive reform in the criminal justice system.

Factors related to group cues

Group cues are messages about which group supports which positions (Coppock 2023; Leeper and Slothuus 2014; Zaller 2012). A group can be a political group (most commonly parties), a religious group, a racial group, or any other social group a person might feel related to based on gender, age, education, or geography. It is important to note that people have many overlapping group identities.

Notably, group cues are distinguished from implicit priming.[8] Group cues do not include reasoning and rely on explicit signaling of social groups and are also processed orthogonally to persuasive information (Tappin, Berinsky, and Rand 2023). As such, it was proposed that they work as a heuristic mechanism, allowing people to take a shortcut toward policy positions without the hard work of learning about the policy (Brady and Sniderman 1985). Moreover, from a psychological perspective, it was suggested that conforming to one's group attitudes leads to pride and a sense of belonging, while deviance has the opposite pernicious effect (Suhay 2015).

The effect of group cues, for example, information about where a political party stands on an issue, depends on group membership (Barber and Pope 2019; Cohen 2003; Agadjanian et al. 2021). If a person is a member of a group, then they may follow the group position, but if a person is not a member of the said group, the group cue might have a negative

[8]Racialized rhetoric (or racial priming) also proved to be effective in influencing preferences for social policies (Hurwitz and Peffley 2005; White 2007; Mendelberg 2017).

effect. The strength of an in-group and out-group cue depends on group membership, the relationship between that group and the policy issue cued, and possibly other personal predispositions (Mason 2018; Cavaillé and Neundorf 2022). Research on party cues found that it has the expected heterogeneous effect - increasing policy support for the in-group and decreasing policy support for the out-group (Suhay, Grofman, and Trechsel 2020; Nicholson 2012; Cohen 2003; Conover 1984). These findings also replicated outside the United States to different degrees of magnitude (e.g., Arriola, Choi, and Gichohi 2022; Nordø 2021). Importantly, researchers found evidence that in-party leader cues influenced partisans' attitudes but that these group cues were integrated independently of non-partisan persuasive messages, suggesting that party cues and information are conceptually different (Tappin, Berinsky, and Rand 2023).

Studies about the effect of cues from other groups are less common. Generally, research suggests that people's political attitudes are affected by various social groups (Conover 1988; Miller, Wlezien, and Hildreth 1991; Green, Palmquist, and Schickler 2004) and the attitudes of their social network (Sinclair 2012). More specifically, studies show that church membership affects political behavior (Djupe and Gilbert 2008; Adkins et al. 2013). We know much less about explicit policy endorsement by racial groups on political behavior.

In understanding public support for criminal justice reform, the influence of group cues has not been adequately explored. For instance, a study focused on attitudes towards police accountability found that the positions of Black lawmakers versus those of law enforcement on police reform sparked a very modest polarizing effect among partisans, compared to a base-line of wide bipartisan support (Boudreau, MacKenzie, and Simmons 2022). Democrats tended to align their opinions with Black legislators' support and Republicans with law enforcement opposition. However, this polarizing effect was marginal. In another study that examined the impact of factual corrections on policy opinions, the imperative determinant for criminal justice policy preferences was found to be in-group pressure, not the corrected information itself (Esberg, Mummolo, and Westwood 2020). These two studies indicate the influence of group cues, but they provide only a fragmented understanding. Political science can expand on studying the role of group cues to equip policy advocates with the insights necessary to devise effective interventions.

3 Related Justice: racial and criminal justice linkage

The aggregate trend over time suggests a linear decline in punitive attitudes with a matching increase in support for progressive reform (Ramirez 2013).[9] For instance, evidence from Texas, often deemed a "red state," suggests a nascent consensus favoring rehabilitation, prison downsizing, and alternatives to incarceration (Thielo et al. 2016). Despite empirical evidence and publicized events that bolster the case for reform, past research reveals

[9]See also Gallup, (41% say justice system is "not tough enough," while 21% say it's "too tough", but compared to Gallup's initial reading of 83% in 1992 it is half of what it was); ACLU (voters express support for politicians that have a reform agenda); PEW (support for reducing spending on police has fallen significantly); PEW (support for the death penalty is still strong).

the inherent challenge of altering criminal justice policy preferences (Esberg, Mummolo, and Westwood 2020; Boudreau, MacKenzie, and Simmons 2022). Factors that previously bolstered political support for punitive policies, such as racial animus, necessitate fresh conceptualization and empirical scrutiny to formulate a blueprint for progressive reform support.

Previous scholarship has provided insights into the genesis of the prison boom. Still, our understanding of the emerging progressive trajectory (policies and legislation that promote decarceration, police accountability, and alternative sanctions) remains underdeveloped. One exception is a study that finds high bi-partisan support for police accountability policies, conditionally on citizens' attitudes toward the BLM movement (Boudreau, MacKenzie, and Simmons 2022). When public policy is associated with a racial group, voters' viewpoints towards that group influence their political convictions (Elder and O'brian 2022). As hostile racial attitudes decline and recognition of institutional racism has escalated (Engelhardt 2021), it could be inferred that the link between the positive shift in racial perspectives and the downturn in hard-line crime attitudes is due to the relatedness of criminal and racial justice in the public mind.

This article proposes a theory of racial and criminal justice relatedness by revisiting the racial animus model and extending its theoretical framework to encompass positive racial attitudes within the context of reform. The theory of racial and criminal justice relatedness (or "related justice") argues that attitudes toward racial justice determine attitudes toward criminal justice. According to the theory, support for criminal legal reform hinges on support for racial justice. Firstly, the theory contends a positive relationship exists between support for criminal legal reform, (1) a lack of negative racial attitudes (Study 1), and (2) positive racial attitudes (Study 2). Second, the theory suggests that people associate racial justice with criminal legal reform such that voters will follow criminal legal reform cues made by racial minorities (Study 2), possibly because they believe that criminal legal reform will affect mostly racial minorities (Study 2).

4 Study 1

Throughout the prison expansion period, public opinion mostly supported and contributed to the rise of mass incarceration (Stinchcombe et al. 1980; Baumgartner, Caron, and S. Duxbury 2022). However, the transferability of research on tough-on-crime politics to progressive reform politics is uncertain. It is necessary to shift the dependent variables to understand the process of political change. Here, I contrast punitive and progressive alternatives instead of relying solely on existing concepts of punitive sentiments.

Study 1 scrutinizes the impact of diverse policy parameters on public preferences: contrasting punitive and progressive policies, employing varied rationales to frame the reform, accounting for costs, and examining to what extent political, social, and interest group endorsements influence these preferences. Importantly, I explore the moderating effect of racial attitudes. Here, progressive reform is defined as policies that advocate for reducing the extensive and intensive margins of the criminal legal system: policies that shift the state's resources toward decriminalization, less severe punishment, or increased scrutiny of law enforcement. This contrasts with punitive policies that increase reliance on incarceration, capital punishment, and public spending on law enforcement (S. W. Duxbury 2021b; Enns 2016; Ramirez 2013).

Data

1433 American recruiters from Amazon MTurk during August 2021. To overcome concerns regarding sample quality, this study incorporated policy positions from the CES 2020 survey (questions CC20_334a-h). The analysis presented in full in the Supplementary Information tests the hypothesis that the true difference between the mean value of each policy in CES and my data is different from zero (Section S2); the null hypothesis cannot be ruled out across all eight comparisons between my estimations and the CES 2020's, thus providing further confidence in the quality of my data. As Coppock and McClellan (2019) note, convenience samples recover political attitudes of the U.S. population well (see also Coppock 2023; further discussion of this article's decision to rely on convenience samples is in the Supplementary Information, S2).

Design

Study 1 employs a conjoint experimental design, a methodological approach that allows for the identification of specific attributes' effects on respondents' preferences. The conjoint design enables the grouping of individual policies to simultaneously estimate the effect of the policy domain (such as policing, bail, prison sentencing, and fines) and policy agenda (progressive versus punitive) on preferences. The analysis groups 21 punitive and 21 progressive policies into punitive and progressive categories. [10]

In this study, I utilize a "single profile" conjoint design. The single profile design was selected for its enhanced ecological validity. When voters vote on policies, most commonly in the ballot proposition process, they do not choose between competing policies (as they often do when voting for political candidates). Moreover, to further increase ecological validity, the ballot propositions information environment was used to design the conjoint (See Section S1 in the Supplementary Information for examples of the official voter guide, which includes a short description, cost, arguments, official supporters, and opponents).[11]

The respondent views a table featuring the proposed policy and indicates whether they support or oppose it (4.1). Each iteration has its unique set of possible policies (not shared between iterations), and a single policy is drawn randomly for presentation. Respondents go through seven iterations of the conjoint table.

A methodological concern in conjoint designs is choosing uniform and target profile distributions (De la Cuesta, Egami, and Imai 2022). From an external validity standpoint, not all profile combinations are probable, and some uncommon combinations may challenge our treatment assumptions, although the extent to which this occurs remains unclear (Bansak

[10] Further methodological discussion in Supplementary Information, S3

[11] For example, see this website for voter information, and all California official pamphlets.

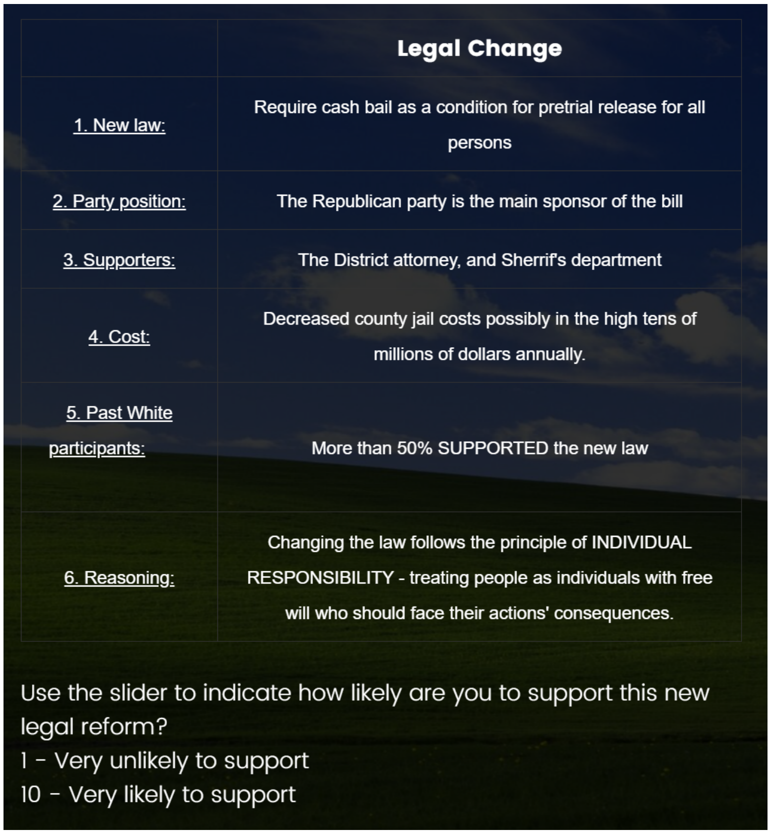


Figure 4.1: Conjoint Screen

Note: Participants were presented with seven iterations. Half were randomly assigned not to receive the "Party position" attribute." The reported race of the participant was inserted in the "Past [reported race] participants" attribute. Participants could not receive the same policy twice or view the progressive and punitive versions of the same policy.

and Jenke 2023). To address this, the theoretical distribution of interest is modeled in Section S3 in the Supplementary Information.

Independent Variables: Informational persuasion

The first four iterations display policies adapted from real-world policies (new legislation being proposed across the US). Each iteration draws from a distinct pool of policies. The first iteration randomly displays one policy related to bail, the second related to fines and misdemeanor punishment, the third related to policing, and the fourth related to maximum sentences. The last three iterations use policies adapted from CES surveys on minimum sentences, violent crime, and policing. I used a manipulation check for the respondent's subjective judgment on the policy's direction of change: more or less punitive than the status quo; the relationship between the classification of the policy as punitive and the respondents' assessment of the policy indicated a significant association between the researcher's classification and the respondents' assessments, $\chi^2(1, N = 8156) = 664.09, p < .001$ (Contingency tables in the Supplementary Information, Section S3).

The process resulted in a total of 42 policies, which are all presented in Figure 4.2 along with their average independent favorability. The most popular policy, all else equal, is to require police officers to wear body cameras while on duty. The least popular policy was to prohibit suing a police officer for damages.

Figure 4.2: All Policies.
Note: This figure presents all the policies considered in Study 1, categorized as either progressive or punitive, and by domain: six for fines and misdemeanors; 20 for policing; 10 for prison sentences; six for bail.

Independent Variables: Group cues, and racial attitudes

Each iteration presented the respondent with a randomized set of policy supporters: party, interest groups, administrative officials, and racial groups.[12] Regarding the measurement of racial animus, the most dominant scale is Racial Resentment, also known as Symbolic Racism (Kinder and Sanders 1996). The scale can also suggest the prevalence of positive racial attitudes for those scoring low on the battery of questions (Agadjanian et al. 2021).

Results

Study 1 supports a theory of group cues in understanding attitudes toward reform: party cues and racial group cues had significant effects on preferences. Respondents follow their racial group and mostly react in a way opposite to the other party's stance. Respondents' dispositional attitudes (partisanship and racial resentment) moderated their preferences.

Figure 4.3 presents the Average Marginal Component Effect (AMCE) analysis. When the opposite party supports the policy, predicted support is lower than both control (no information about party position) and congruence between PID and party support. The effect of group cues from the respondent's reported racial group was also significant and crucial in decision-making (the relative weight of each attribute is discussed below and presented in Figure 4.4, in both the positive direction (same race group supports the reform) and the negative direction (same race group opposes the reform).

Figure 4.4 presents the importance weights for the study's attributes. Importance weights explain the relative importance of different attributes in decision-making processes. The weights represent the change in preference for a unit change in an attribute, holding other attributes constant. The higher the weight, the more significant the attribute in influencing preference. The two most influential attributes were whether the policy is punitive or progressive and the cue regarding past racial group support of the policy.

Figure 4.3 shows that progressive policies affect support positively, compared to tough-on-crime policies. A breakdown of support by domain, using a Marginal Means analysis (Leeper, Hobolt, and Tilley 2020), revealed that most of the progressive support was driven by respondents' dislike of punitive police reform and punitive fines reform (Figure 4.5).

In terms of moderators, compared with those low on the Racial Resentment scale, participants who scored high on racial resentment were more supportive of tough-on-crime (Figure 4.6b) and less supportive of progressive reform (Figure 4.6a).

These results clearly show that Racial Resentment is a powerful predictor of both a preference toward tough-on-crime policies and opposition to progressive policies. This is also apparent in the analysis of Differences in Conditional Marginal Means (Leeper, Hobolt, and Tilley 2020), as shown in Figure 4.7, which also demonstrated that racial resentment moderates attitudes toward endorsement by BLM and the ACLU.

An analysis of Differences in Conditional Marginal Means (Leeper, Hobolt, and Tilley 2020) revealed that Republicans are significantly more likely to support tough-on-crime

[12] See SectionS3 in Supplementary Information for additional information.

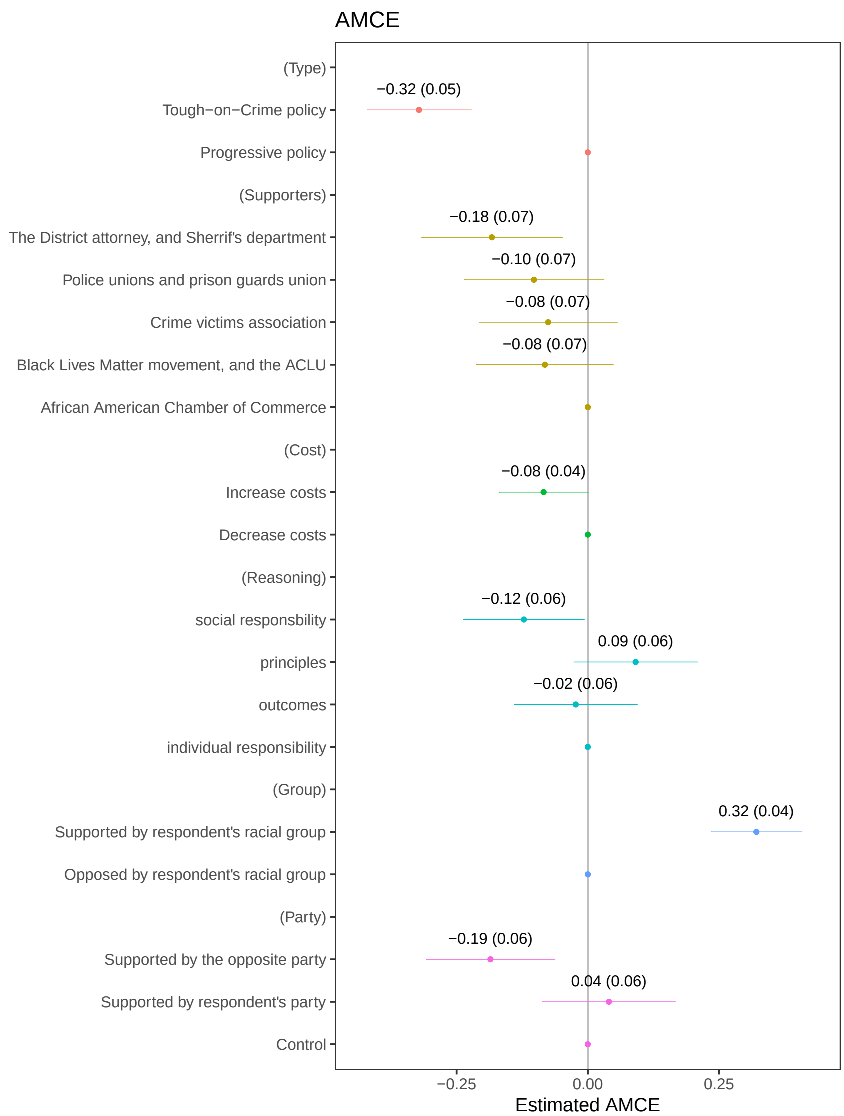


Figure 4.3: AMCE plot using the Binomial family.

Note: I used the Binomial family for the GLM in this analysis. The coefficients represent the change in the log-odds of the dependent variable for a one-unit change in the independent variable, holding all other variables constant.

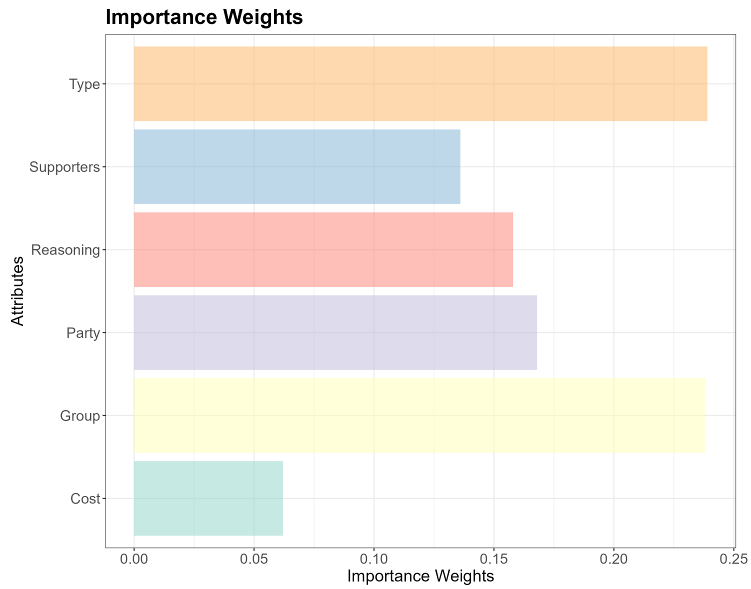


Figure 4.4: Importance weights of attributes from the conjoint analysis.
Note: These weights are calculated using an OLS regression-based method. Each attribute's range of part-worth utilities (i.e., the contribution of each attribute level to the overall preference) is computed. The weights are then the ranges for each attribute, normalized to sum to 100, representing the relative importance of each attribute.

policies (12.9 percentage points more than Democrats) and significantly less likely to support progressive policies (7.7 percentage points less) (Table S4.1).

To conclude Study 1, I find evidence for the importance of following group cues, both racial and partisan. Study 2 extends these results to out-group racial group cues, specifically regarding progressive reform. It will also explore how positive racial attitudes relate to progressive reform support. Finally, to explore a possible mechanism, Study 2 explores whether respondents associate the impact of progressive criminal legal reform with minorities.

5 Study 2

Study 2 extends the analysis of racial group cues beyond the respondents' groups. I examine how out-group cues effects differ from in-group racial group cues. In addition, racial attitudes continually evolve - changing from generation to generation and through time (DeSante and C. W. Smith 2019; Lee 2002; Valenzuela and Reny 2020; Engelhardt 2021). Importantly, we do not know how positive racial attitudes affect people's perceptions of crime and justice. Hence, Study 2 extends the study of racial attitudes to positive racial attitudes and asks whether respondents believe progressive reform would impact mostly minorities.[13]

[13]Study 1 also found that learning that the opposing party supports a policy has a negative effect on preferences when compared to no party cue present. Thus, Study 2 focuses on this finding to understand

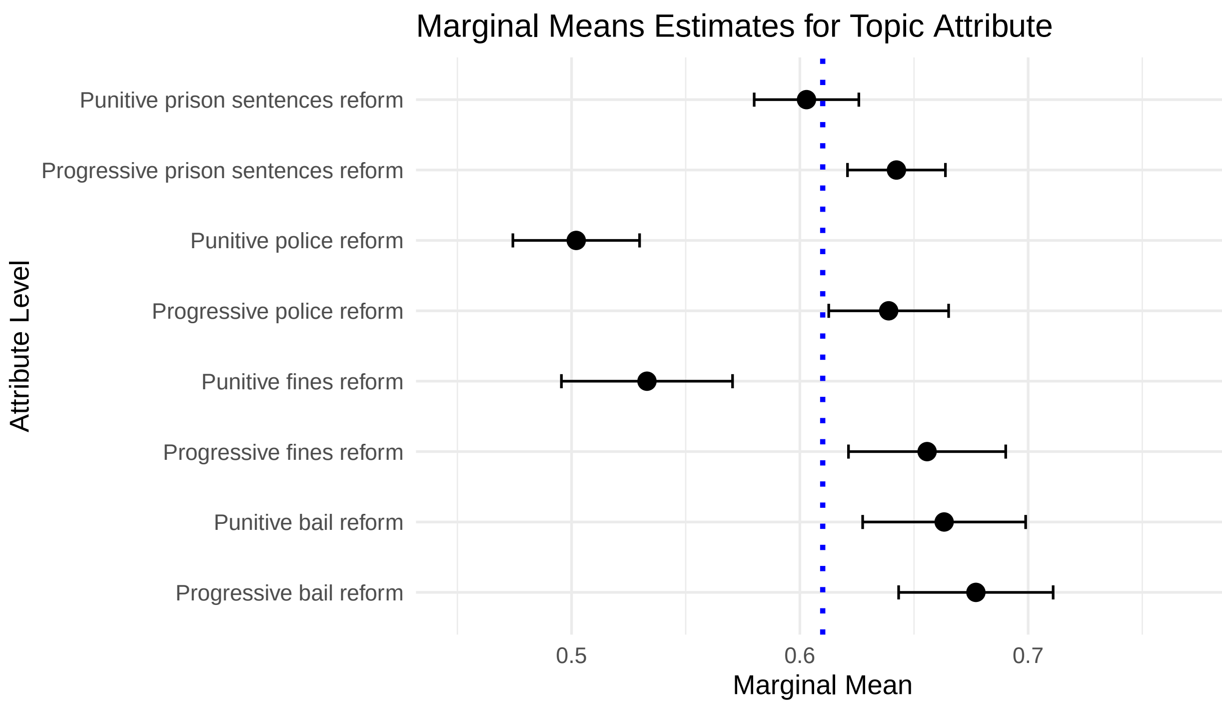
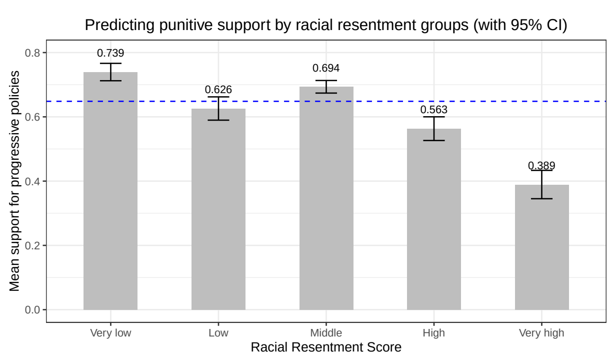
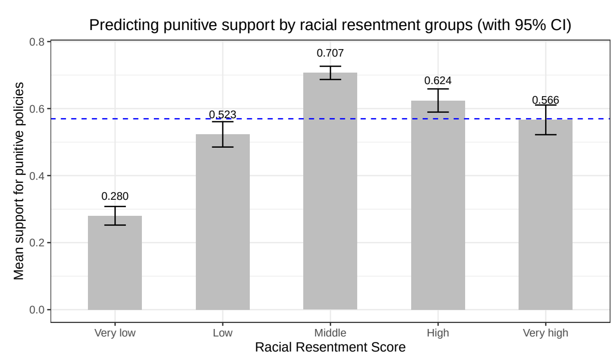


Figure 4.5: MM plot for Topic Attributes

Note: In this analysis, I show the marginal means, representing the mean outcome across all appearances of a particular conjoint feature level, averaging across all other features. Marginal means analysis provides a description of the preference structure for each attribute level.



(a) Support for progressive reforms.



(b) Support for punitive reforms.

Figure 4.6: Reform preferences by Racial Resentment scores

Note: The mean support for policy-group by racial resentment subgroups, with 95% CI. Racial resentment subgroups are calculated by grouping respondents between these mean scores on the scale: 0.2, 0.4, 0.6, and 0.8. The dotted line is the average support for the group of reforms.

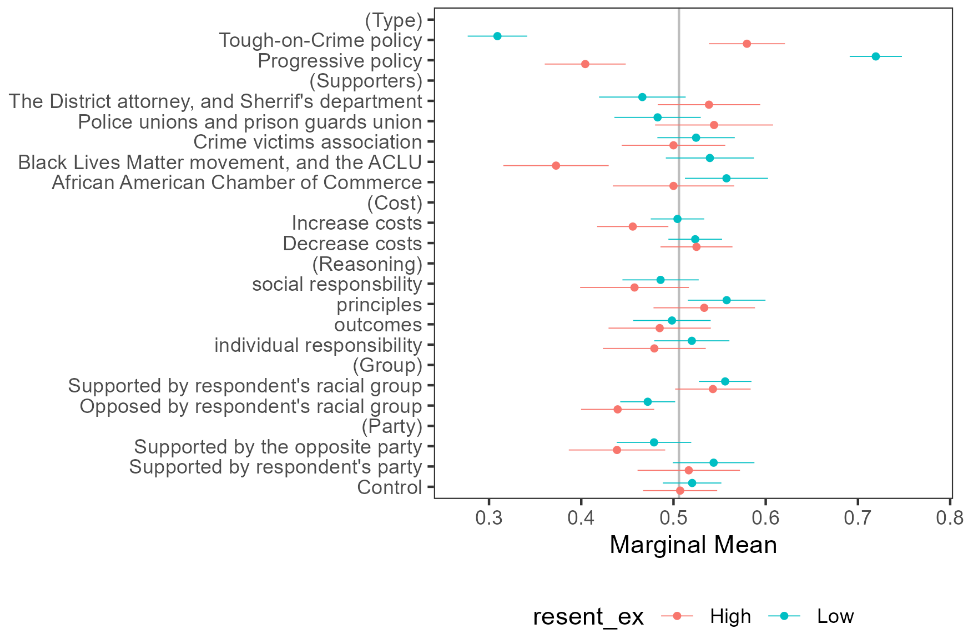


Figure 4.7: Subgroup MM Analysis

Note: In this analysis, I show the differences in marginal means, which represent the differences in mean outcomes across all appearances of a particular conjoint feature level, averaging across all other features.

Data and Design

The sample comprises both an MTurk sample (N=226) and a Lucid Theorem sample (N=284).[14] Lucid Theorem employs quota sampling to produce samples matched to the US population on age, gender, ethnicity, and geographic region; recent research demonstrates the suitability of the Lucid platform for evaluating social scientific theories (Coppock and McClellan 2019; Coppock 2023).

Respondents read four policies and answered: “Do you support this policy? How would you vote if you could?” The policies were chosen to represent different domains of criminal justice and four different baselines of support based on the results of Study 1. The policies were: (1) “The policy would make it possible for convicted felons to reduce up to 50% of their sentence, using ”good time credits.” (previous average of about 56% support); (2) “The policy would make it possible for people sentenced to one (1) year in jail or less, to apply for substituting the remainder of their sentence with a fine.” (previous average of 60% support); (3) “The policy would make it possible for people waiting for their criminal trial to

the effect of out-party support, reported in the Supplementary Information, Section S6.

[14]This allows my findings to be robust for systematic differences between the samples. See more regarding utilizing MTurk and Lucid in the Supplementary Information, Section S2; and Section S2.

apply for immediate release, without paying cash bail." (previous average of 64% support); (4) "The policy would allow some non-violent drug offenders to avoid mandatory minimum sentences." (previous average of 72% support).

Finally, all respondents were asked a final question before the end of the experiment: "If you had to guess, would you say these policies will have the most impact on which racial group? Choose as many as you like."

Group cues and positive out-group attitudes

Respondents were randomly assigned to receive racial group or political party cues. Racial group cues included five randomized conditions: "The policies we want your opinion on were previously SUPPORTED [/OPPOSED] by the majority of White [/Black] voters in [inserting the respondent's region in the US]. They believed these policies can lower crime and increase safety." The 5th condition was a control: "The policies we want your opinion on are new attempts to lower crime and increase safety."

Partisanship cues included four conditions: "The policies we want your opinion on are part of the Democratic party's [/Republican /bipartisan] new criminal justice reform campaign." The 4th condition was a control: "The policies we want your opinion on are part of a new criminal justice reform campaign." Respondents also must demonstrate they understand which party backs the reform before proceeding.

For positive racial attitudes, I use an adapted version of the FIRE scale (DeSante and C. W. Smith 2020) and the sympathy scale (Chudy 2021; Cullen, Butler, and Graham 2021) (full wording in the Supplementary Information, Table S5.1). Racial sympathy is a form of affect whereby whites, because of their discontent with the plight of Black Americans, will be inclined to support policies that benefit them; Racial sympathy is distinct from a more general sympathy, as it does not shape opinion related to other groups (Chudy 2021). A 2019 YouGov survey showed that racial sympathy is significantly related to the view that capital punishment is discriminatory and was positively associated with the idea that rehabilitation is the main goal of prison (Hannan et al. 2022).

To directly test for the differences in the effects between the two groups (based on reported racial identity), I test the significance of the interaction terms in a pooled model. My analysis used the following interaction model:

$$\begin{aligned}\text{Vote}_i = \beta_0 + \beta_1\text{Race group cue}_i * \beta_2\text{Reported race}_i \\ + \boldsymbol{\beta_3}\text{Demographics}_i + \boldsymbol{\beta_4}\text{Attitudes}_i + \beta_5\text{MTurk}_i + \varepsilon_i\end{aligned}$$

Results

Study 2 finds that for respondents identifying as white, a cue about Black voters supporting the reform had the most favorable effect. For Black respondents, the effect of Black opposition had the most impact. I also found that about half of the respondents believed progressive criminal legal reform would benefit racial minorities. People might follow minorities more because they believe the reform concerns them more.

Firstly, for people of color, the racial group cues treatment was statistically significant according to a chi-square test of independence for categorical variables ($\chi^2 = 11.723$, df = 4, $p = 0.019$). For white voters, the treatment had a statistically significant effect ($\chi^2 = 25.077$, df = 4, p ¡ .0001). Figure 5.1 shows the effect of treatment conditions with 95% confidence intervals. We notice stark differences in baseline progressive reform support in the control conditions (about 40% vs. 75%). For non-white respondents, information about Black opposition had a significant negative effect. White respondents were the most likely to be swayed by cues indicating the preferences of Black voters.

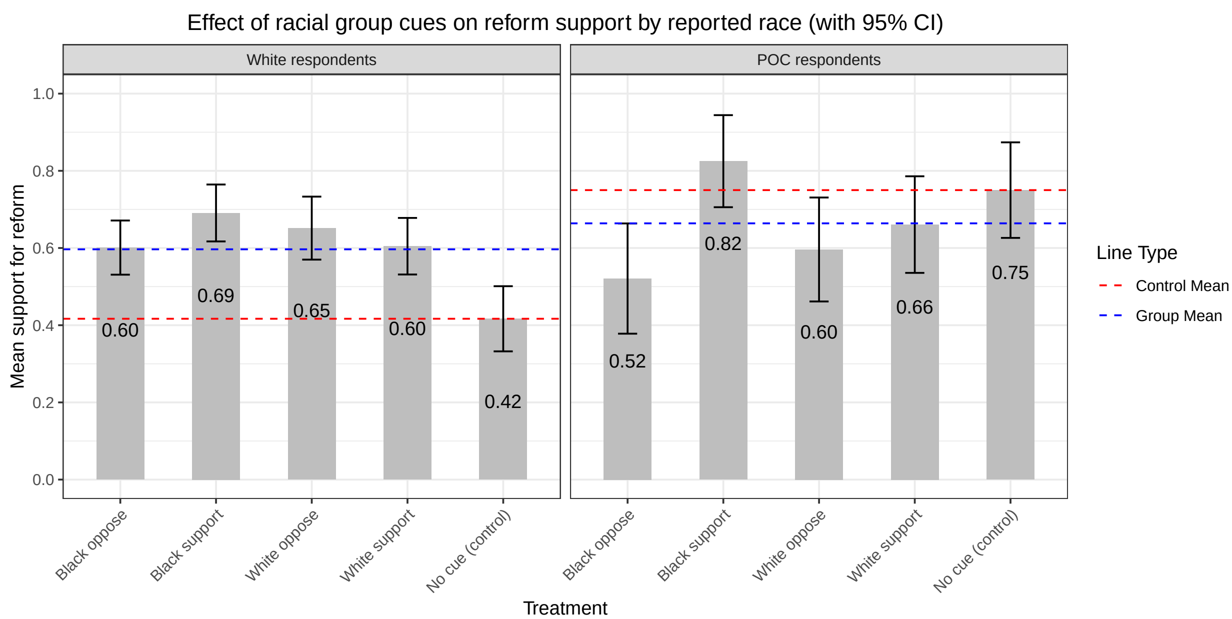


Figure 5.1: Racial group cues

Note: Mean support for the four progressive policies by racial group cues with 95% CI.

Because we are interested in how different cues about racial group behavior affect people reporting different racial groups, the analysis utilizes an interaction model (Table 5.1). Table 5.1 presents the estimated coefficients for the interaction terms between racial group cues and reported race (coded as 1 for reporting non-white identity and 0 for white); the control group is the reference category.[15]

The non-interaction coefficients in Table 5.1 estimate the difference in progressive support likelihood between the control group (no cue) and the other categories for respondents reporting a white identity. The interaction terms (”..∗ *non-white respondent*”) estimate how these differences change when the reported respondent identity becomes non-white. For example, *Black Opposition* ∗ *non-white respondent* is the interaction term for receiving a cue

[15] Full statistical results are reported in Supplementary Information, Section S6.

about Black citizens' preferences and reporting a non-white identity. The negative coefficient indicates that the effect of this cue (compared to the control group) decreases the likelihood of supporting progressive reform when respondents report a non-white identity (compared to reporting a white identity).

The results in Table 5.1 indicate significant interaction effects for some racial group cues with reported race. The most prominent effect for respondents reporting white identity (non-interaction coefficients) was the out-group support cue. For non-white identity respondents (interaction coefficients), information about any opposition had a statistically significant effect (keeping in mind the baseline high support for reform in their control group - 75%).

	Interaction Model
(Intercept)	$0.61\ (0.21)^{**}$
White Support	$0.16\ (0.08)$
Black Opposition	$0.17\ (0.08)^{*}$
Black Support	$0.22\ (0.08)^{**}$
White Opposition	$0.15\ (0.08)$
Non-white respondent (binary)	$0.30\ (0.11)^{*}$
White Support ∗ non-white respondent	$-0.26\ (0.14)$
Black Opposition ∗ non-white respondent	$-0.51\ (0.16)^{**}$
Black Support ∗ non-white respondent	$-0.20\ (0.14)$
White Opposition ∗ non-white respondent	$-0.39\ (0.14)^{**}$
R^2	0.15
Adj. R^2	0.12
Num. obs.	1012
F statistic	20.53
RMSE	0.46
N Clusters	253

$^{***}p$¡0.001; $^{**}p$¡0.01; $^{*}p$¡0.05

Table 5.1: Statistical models

Note: The interaction model tests the impact of all variables with their interaction effects on the outcome. Each cell provides the estimated effect size and the SE, controlling for all other variables (demographics, attitudes, and sample source) in the model; full statistical results are reported in Supplementary Information, Section S6.

Further, scoring high on racial sympathy predicts support for progressive reform, controlling for all other covariates and experimental conditions (Table 5.2). Moreover, the predictive effect of racial sympathy is the only racial attitude related to progressive reform support.

Finally, when asked whether the policies presented would benefit white people or other

	Predicting Progressive Policies Support
Racial sympathy	0.27^{***}
	(0.07)
Racial Resentment	-0.14
	(0.09)
FIRE scale	-0.13
	(0.13)
R^2	0.10
Adj. R^2	0.08
Num. obs.	2020
RMSE	0.47
N Clusters	505

$^{***}p < 0.001$; $^{**}p < 0.01$; $^{*}p < 0.05$

Table 5.2: Model predicting support for progressive policies

Note: The table displays the results of an OLS regression analysis. The dependent variable is a vote in favor of progressive policy reform, and the independent variables include demographics and controls for the treatment condition and the participant's recruitment source.

groups of people of color, about 48% of respondents indicated they believed a criminal justice reform would benefit people of color (POC).[16] On the other hand, approximately 34% of respondents indicated they believe the same reforms would benefit white people. Further, Table 5.3 indicates a perception gap among respondents where those who believe reform would benefit POC do not believe it would also benefit white people, and vice versa. There is also a statistically significant association between beliefs about the benefits of criminal justice reform for POC and white people (χ^2 (1, N = 477) = 231.74, p < .001.).

	Will not benefit whites	Will benefit whites
Will not benefit POC	82	164
Will benefit POC	231	0

Table 5.3: Distribution of beliefs about benefits of criminal justice reform

I find a perception gap: a belief that reform would benefit one group does not coincide with the belief that it would benefit the other. This gap suggests a cognitive mechanism for this article's findings. People might follow minorities more because they believe the reform concerns them more. When a policy is linked to a racial group, voters' attitudes toward that group shape their political beliefs (Elder and O'brian 2022). Indeed, moderated by positive

[16]There is no difference in these responses between the racial and party group cues treatments.

(and negative, as per Study 1), both people of color and white people are more likely to align with Black voters' support of a policy than any other information on group preferences.

6 Conclusion

The findings presented support a theory of Related Justice. I show a positive relationship between support for criminal legal reform and racial justice attitudes. Possibly because people believe that criminal legal reform would benefit racial minorities, I find that people will follow criminal legal reform cues made by racial minorities.

Racial attitudes and racial identity matter most for the politics of criminal legal reform (Boudreau, MacKenzie, and Simmons 2019). As racial attitudes have become less hostile and awareness of the systemic racism in political and legal institutions has grown (Engelhardt 2021), the correlation between positive changes in racial attitudes and the decline in tough-on-crime perspectives may be the result of criminal and racial justice relatedness in the public's mind. This is aligned with previous findings that support for police reform is tightly related to support for the BLM movement, surpassing and overcoming any effect of partisanship or elite (Black lawmakers and law enforcement agencies) cues (Boudreau, MacKenzie, and Simmons 2022). This study argues that amplifying the voices of racial minorities disproportionately affected by excessive punitiveness can shift policy preferences toward a more progressive direction. At the same time, cultivating an understanding and endorsement of positive racial attitudes is a key condition for catalyzing this transformation.

The American government's response to crime is inherently political, with public opinion shaping criminal justice policy. An increasing number of political organizations now focus on transforming criminal justice politics. Therefore, understanding the "politics of downsizing" is crucial for advocacy organizations and activists developing a politically viable alternative to excessively harsh penal policies (Petersilia and Cullen 2014). The potential success of progressive crime policies hinges on recognizing that voters do not rely on cost concerns, accurate information about crime rates, or straightforward partisanship cues (Esberg, Mummolo, and Westwood 2020). As the devastating impact of long-standing negative racial attitudes begins to recede, the most significant external influence emerges from trusting and amplifying the voices of communities affected by the carceral state and building broad social coalitions.

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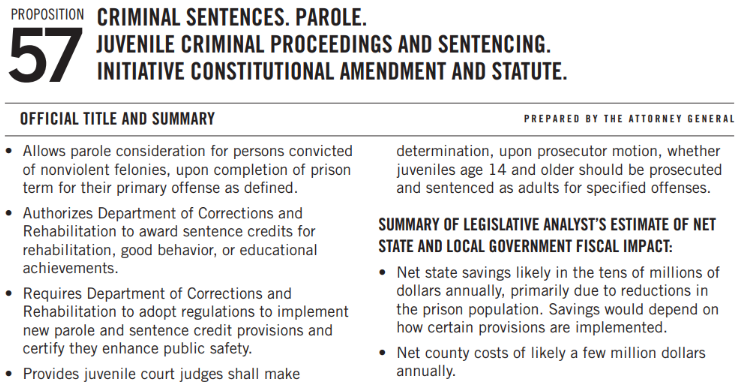
Supplementary Information

S1 Ecological Validity - Voter Information example



PROPOSITION **57** **CRIMINAL SENTENCES. PAROLE. JUVENILE CRIMINAL PROCEEDINGS AND SENTENCING. INITIATIVE CONSTITUTIONAL AMENDMENT AND STATUTE.**

OFFICIAL TITLE AND SUMMARY PREPARED BY THE ATTORNEY GENERAL

- Allows parole consideration for persons convicted of nonviolent felonies, upon completion of prison term for their primary offense as defined.
- Authorizes Department of Corrections and Rehabilitation to award sentence credits for rehabilitation, good behavior, or educational achievements.
- Requires Department of Corrections and Rehabilitation to adopt regulations to implement new parole and sentence credit provisions and certify they enhance public safety.
- Provides juvenile court judges shall make determination, upon prosecutor motion, whether juveniles age 14 and older should be prosecuted and sentenced as adults for specified offenses.

SUMMARY OF LEGISLATIVE ANALYST'S ESTIMATE OF NET STATE AND LOCAL GOVERNMENT FISCAL IMPACT:

- Net state savings likely in the tens of millions of dollars annually, primarily due to reductions in the prison population. Savings would depend on how certain provisions are implemented.
- Net county costs of likely a few million dollars annually.

Figure S1.1: Short description and cost estimate

S2 Samples

Summary Tables

Study 1

The mean age of the respondents was 38.6 (SD 11.8). 40.8% of the sample identified as Female, and 52.1% identified as Democrats. For race and ethnicity, 7% identified as Asian, 11.8% as Black, 4.7% as Hispanic, and 72.4% as White. The question of whether researchers should use weights in survey experiments analysis is complex and depends on the type of generalization (external validity) the researcher seeks to achieve (Egami and Hartman 2022) and on whether we can identify covariates that predict both treatment heterogeneity and selection into the sample (Miratrix et al. 2018). In this study, the difference in the composition of units in the experimental sample and the target population (voting-age Americans) does not raise generalization issues because selection into the experiment and treatment effect heterogeneity are unrelated to each other (Egami and Hartman 2022): In the Supplementary Information, I show that I detect only minor treatment effect heterogeneity on the partisanship and gender covariates, thus controlling for the pretreatment covariates can fulfill the ignorability of sampling and treatment effect heterogeneity assumption (Egami and Hartman 2022; Cole and Stuart 2010).

As mentioned, the analysis in Supplementary Information Section (S2) ensures the substantive attitudes captured in my sample are at least a close approximation to the general populations' by comparing various policy preferences among my sample to those captured on the CES - a high-quality probability sample survey. My estimates are indistinguishable from the 2020 CES results. Although I recommend caution in over-interpreting results from

Prop. 57 is long overdue.

Prop. 57 focuses our system on evidence-based rehabilitation for juveniles and adults because it is better for public safety than our current system.

Prop. 57 saves tens of millions of taxpayer dollars.

Prop. 57 keeps the most dangerous criminals behind bars.

VOTE YES on Prop. 57

www.Vote4Prop57.com

EDMUND G. BROWN JR., Governor of California
MARK BONINI, President
Chief Probation Officers of California
DIONNE WILSON, widow of police officer killed in the line of duty

(a) Supporters and arguments

The weakening of California's anti-crime laws has gone too far. Don't amend California's Constitution to give even more rights to criminals.

Crime Victims, Police, Sheriffs, Judges and Prosecutors urge a NO vote on 57.

HONORABLE JAMES ARDAIZ, Presiding Judge
5th District Court of Appeal (Ret.)
SANDRA HUTCHENS, Sheriff
Orange County
COLLENE THOMPSON CAMPBELL, Founder
Memory of Victims Everywhere

(b) Opposers and arguments

San Diego District Attorney Bonnie Dumanis—a Prop. 57 supporter—knows it is imperative to provide inmates with tools to stop the revolving door to prison. (*Daily Journal*, July 14, 2016).

And that makes our communities safer.

Join law enforcement officials, victims of crime and religious leaders: vote YES on Prop. 57.

EDMUND G. BROWN JR., Governor of California
MARK BONINI, President
Chief Probation Officers of California
DIONNE WILSON, widow of police officer killed in the line of duty

(c) Supporters and arguments

If you answered NO to these questions, then join District Attorneys, Courtroom Prosecutors, Police, Sheriffs, Crime Victims, Superior Court Judges and community leaders in voting NO on 57.

Violent crime was up 10% last year in California. Don't allow more violent and dangerous criminals to be released early. VOTE NO on 57.

MARTIN HALLORAN, President
San Francisco Police Officers Association
GEORGE HOFSTETTER, President
Association of Los Angeles Deputy Sheriffs
STEPHEN WAGSTAFFE, President
California District Attorneys Association

(d) Opposers and arguments

Figure S1.2: Example of Proposition 57 official voter information guide.

*Note:*See here for complete pamphlet: https://repository.uchastings.edu/cgi/viewcontent.cgi?article=2342&context=ca_ballot_props

non-probability samples, the quality control mechanisms and weighting scheme we utilize produce internally valid results, as well as externally valid results with regards to population (known issues of generalization across contexts and to behavioral outcomes, remain; see Egami and Hartman 2022).

Study 2

The use of weights in survey experiment analysis hinges on the researcher's intended generalization (external validity) (Egami and Hartman 2022) and the ability to identify covariates that predict both treatment heterogeneity and selection into the sample (Miratrix et al. 2018). In this study, the design assumes treatment heterogeneity based on reported race and political identity. The difference in the composition of units in the experimental sample and the target population (voting-age Americans) is a concern for the MTurk sample solely, which might compromise external population validity (x-validity, Egami and Hartman 2022) but not treatment validity (T-validity, Egami and Hartman 2022), which is the validity of

Table S2.1: Summary Table

Characteristic	Overall (N=1433)
Age: Mean (SD)	38.6 (11.8)
Median [Min, Max]	36.0 [19.0, 83.0]
Missing	1 (0.1%)
Gender: Female	585 (40.8%)
In another way	4 (0.3%)
Male	844 (58.9%)
Political View: Conservative	306 (21.4%)
Liberal	402 (28.1%)
Moderate	298 (20.8%)
Not sure	3 (0.2%)
Very Conservative	220 (15.4%)
Very liberal	204 (14.2%)
Party: Democrat	746 (52.1%)
Independent	264 (18.4%)
Republican	405 (28.3%)
Race: White	1038 (72.4%)
Black	169 (11.8%)
Asian	100 (7.0%)
Hispanic	67 (4.7%)
Other*	59 (4.1%)
Housing: Own	862 (60.2%)
Rent	514 (35.9%)
Other	57 (4.0%)
Education: Associate/Bachelor	816 (56.9%)
Master's or higher	358 (25.0%)
Some College, No Degree	155 (10.8%)
High School	97 (6.8%)
Other**	7 (0.5%)
Income: Above average	228 (15.9%)
Average	691 (48.2%)
High	76 (5.3%)
Low	438 (30.6%)

*Other includes: Something else, Middle Eastern, Mixed, Native American, Other
**Other includes: No Formal Schooling, Prefer not to answer, Missing

interest in this study. Thus, the assigned and target treatments generate identical average treatment effects in expectation. All participants gave their explicit consent to participate in the research study. The study was fielded in February 2022. latexCopybooktabs array caption

Table S2.2: Summary Table

Characteristic	Lucid Theorem (N=284)	MTurk (N=226)
Age: Mean (SD)	47.6 (16.8)	38.3 (10.3)
Median [Min, Max]	47.0 [20.0, 81.0]	36.0 [24.0, 68.0]
Gender: Female	146 (51.4%)	94 (41.6%)
Male	135 (47.5%)	131 (58.0%)
Other	3 (1.1%)	1 (0.4%)
Political View:		
Very liberal	36 (12.7%)	46 (20.4%)
Slightly/Somewhat liberal	67 (23.6%)	62 (27.4%)
Neither lib. nor cons.	76 (26.8%)	23 (10.2%)
Slightly/Somewhat cons.	55 (19.4%)	53 (23.4%)
Very conservative	50 (17.6%)	42 (18.6%)
Party: Democrat	127 (44.7%)	132 (58.4%)
Republican	75 (26.4%)	55 (24.3%)
Independent	61 (21.5%)	38 (16.8%)
Other	21 (7.4%)	1 (0.4%)
Race: White	207 (72.9%)	179 (79.2%)
Black	32 (11.3%)	12 (5.3%)
Asian	12 (4.2%)	13 (5.8%)
Hispanic	11 (3.9%)	8 (3.5%)
Other*	22 (7.7%)	14 (6.2%)
Housing: Own	172 (60.6%)	142 (62.8%)
Rent	97 (34.2%)	77 (34.1%)
Other	15 (5.3%)	7 (3.1%)
Education:		
Associate/Bachelor	117 (41.2%)	124 (54.9%)
Master's or higher	50 (17.6%)	62 (27.4%)
Some College, No Degree	55 (19.4%)	19 (8.4%)
High School	57 (20.1%)	19 (8.4%)
Other**	5 (1.8%)	2 (0.9%)
Income:		
Less than $40,000	105 (36.9%)	59 (26.1%)
$40,000 - $89,999	95 (33.5%)	129 (57.1%)
$90,000 - $139,999	38 (13.4%)	26 (11.5%)
$140,000 or more	46 (16.2%)	12 (5.3%)

*Other includes: Native American, Mixed, Middle Eastern, Other
**Other includes: No Formal Schooling, Prefer not to answer

Convenience samples - metrics and justifications

Utilizing MTurk and Lucid samples is a viable and efficacious approach for estimating treatment effects in survey experiments. A seminal study by Coppock, Leeper, and Mullinix (2018) in the Proceedings of the National Academy of Sciences, replicated 27 survey experiments encompassing 101,745 individual survey responses originally conducted on nationally representative samples using online convenience samples. The authors found a high degree of correspondence between conditional average treatment effects in original studies and their MTurk replications across various demographic subgroups, despite differences in sample composition. More broadly, the authors posited that the underlying reason for the strong correspondence between sample types across both unconditional and conditional average treatment effect estimates is the commonness of low treatment effect heterogeneity (see also, Coppock 2023). This finding is crucial as it suggests that the causal theories guiding the design of experiments extend in a straightforward manner to convenience samples like MTurk, despite differences in sample composition. This suggests convenience samples can yield reliable average treatment effect estimates like those obtained through nationally representative samples.

In a complementary vein, Coppock and McClellan (2019) in Research & Politics introduced Lucid as an aggregator of survey respondents, lauding its large subject pool, demographic diversity, and cost-effectiveness. The authors advocate for a “fit-for-purpose” framework, positing that convenience samples are apt for estimating sample average treatment effects if the sample is theoretically relevant. Lucid performed commendably in recovering estimates proximate to original studies. Furthermore, an analysis by Zachary Lorico Hertz corroborates the reliability of data obtained through Lucid. His research indicated that speeders have a limited effect on data quality and that Lucid’s data remained consistent with theoretical expectations. Collectively, these studies, especially the extensive replication effort by Coppock, Leeper, and Mullinix (2018), provide compelling evidence for the acceptability of MTurk and Lucid samples in survey experiments. Their utility lies in their ability to offer stable response times, limited effect of speeders on data quality, strong external validity, and demographically diverse samples that are fit for purpose. Researchers are encouraged to consider these platforms as robust tools for gathering insightful data in survey experiments.

Figures S2.1 and S2.2 show the distribution of completion times for Studies 1 and 2, accordingly. As expected, most respondents are bunched around the median time, which is the reasonable completion time for these kinds of studies.

CES Comparison

In my survey, I used the policies presented in questions CC20_334a-h from the CES 2020. I conduct t-tests to test the hypothesis that the true difference between the mean value of each policy in CES and my data is different from zero. By presenting all 8 comparisons in a single graphic (Figure S2.3), the forest plot provides a comprehensive view of the relationships between the datasets across all variables. The visualization can also help identify patterns

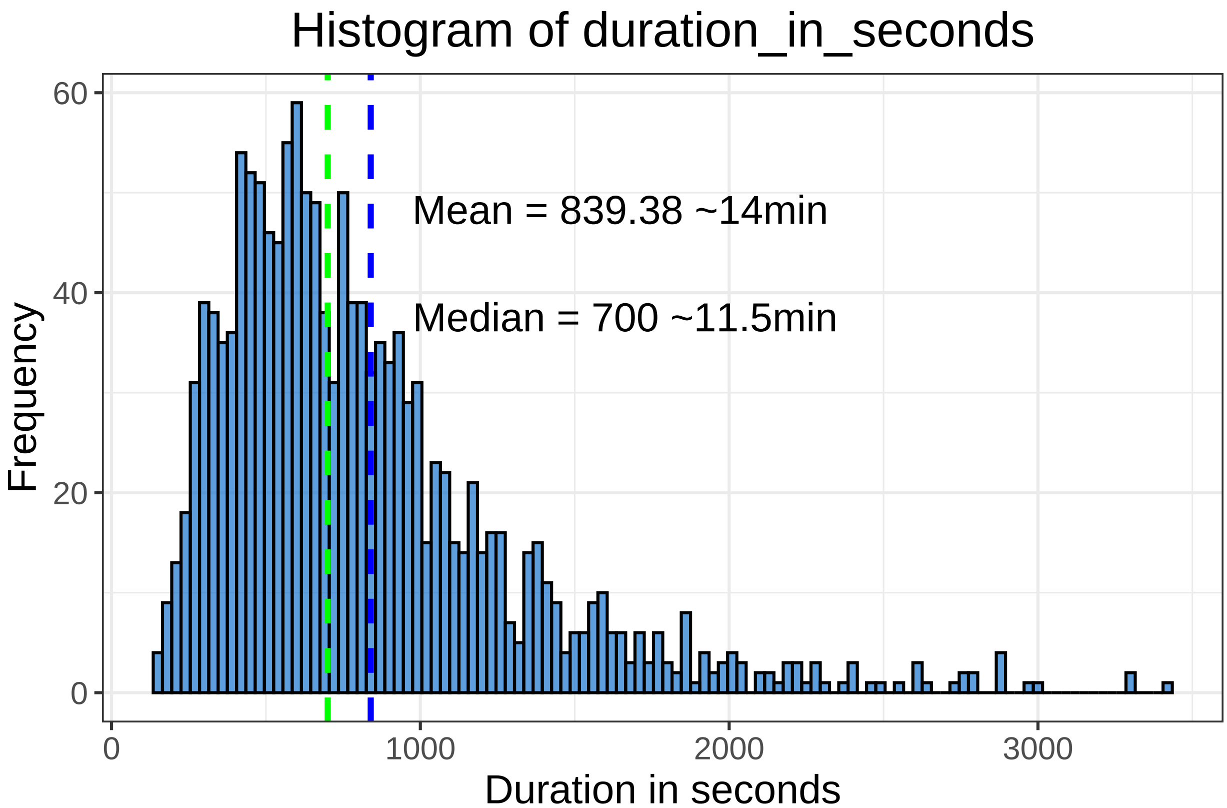


Figure S2.1: Study 1 - Duration in seconds
Note: The Histogram plots the survey duration in seconds for the respondents in Study 1.

or trends in the data, such as consistently larger or smaller effect sizes for certain variables. Lastly, the forest plot offers a clear and concise way to communicate complex statistical results to a wider audience, facilitating a better understanding of the data.

S3 Study 1 - methodological appendix

Additional attributes description

First, respondents were told which party supported the new law. For this attribute, I use a "pure control" by randomizing respondents to either receive a table that includes the attribute or without the attribute. Second, respondents were told that one of the following groups supports the reform: Police unions and prison guards union; The Black Lives Matter movement and ACLU; African American Chamber of Commerce; The District Attorney and Sheriff's Department; Crime Victims Association. Finally, I used the respondent's reported race to test whether people from the same racial group as the respondent supported or opposed the change.

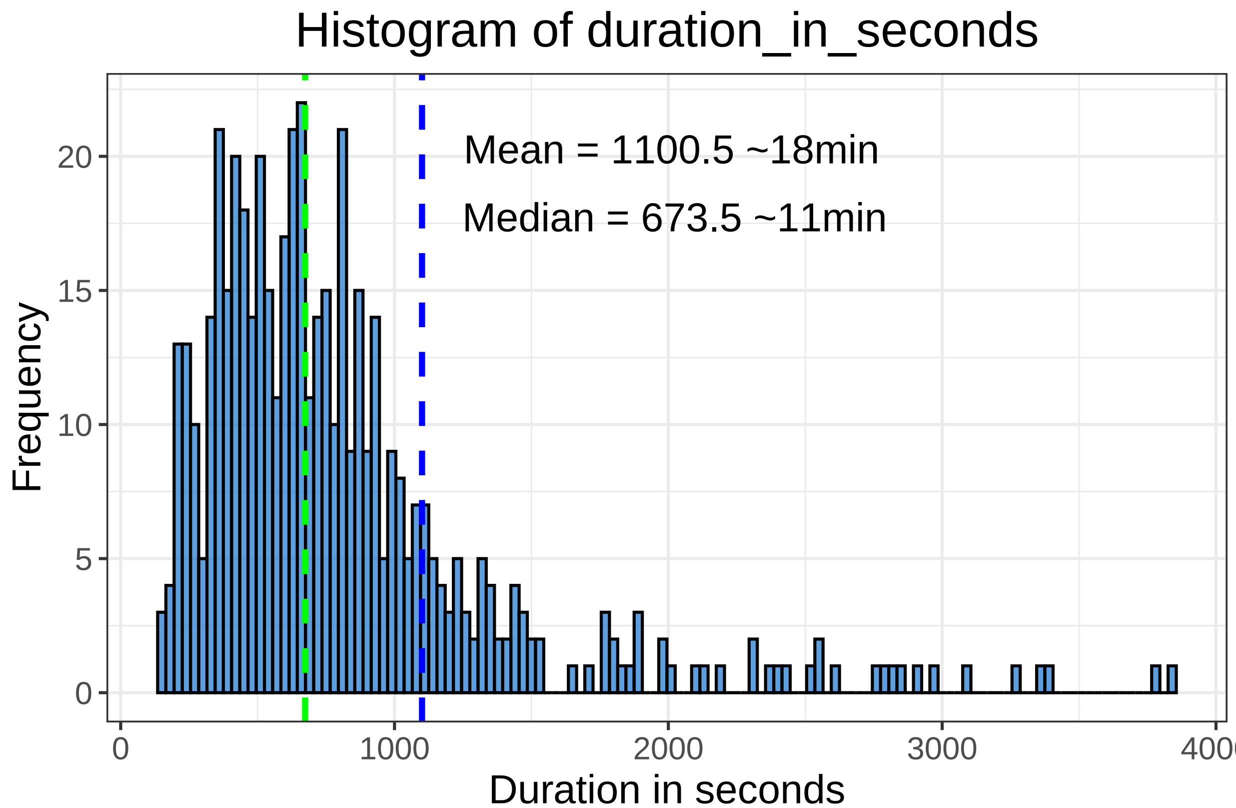


Figure S2.2: Study 2 - Duration in seconds
Note: The Histogram plots the survey duration in seconds for the respondents in Study 2.

A second type of informational treatment was framing. Each of the seven policy iterations presented additional reasoning for the proposed reform. The arguments were constructed to follow: (1) moral norms - general principles of justice and fairness; social responsibility for marginalized and historically disadvantaged groups; maximizing individual responsibility values; (2) escalating crime: achieving an outcome (reduce crime) at all costs. Finally, the respondents were also presented with random information regarding the cost of the reform and whether the law change is predicted to increase or decrease government costs.

Regarding the Racial Resentment scale, it has been used for over 30 years and is widely validated to capture whites' attitudes toward black Americans. The theory underlying symbolic racism is the belief that racial prejudice was transforming from an overt or blatant expression to a covert or subtle expression, in which African Americans were perceived to violate traditional norms and values like individualism. Underlying hatred had not changed, but social desirability norms altered the expression of anti-Black effect or racial prejudice into more coded language.

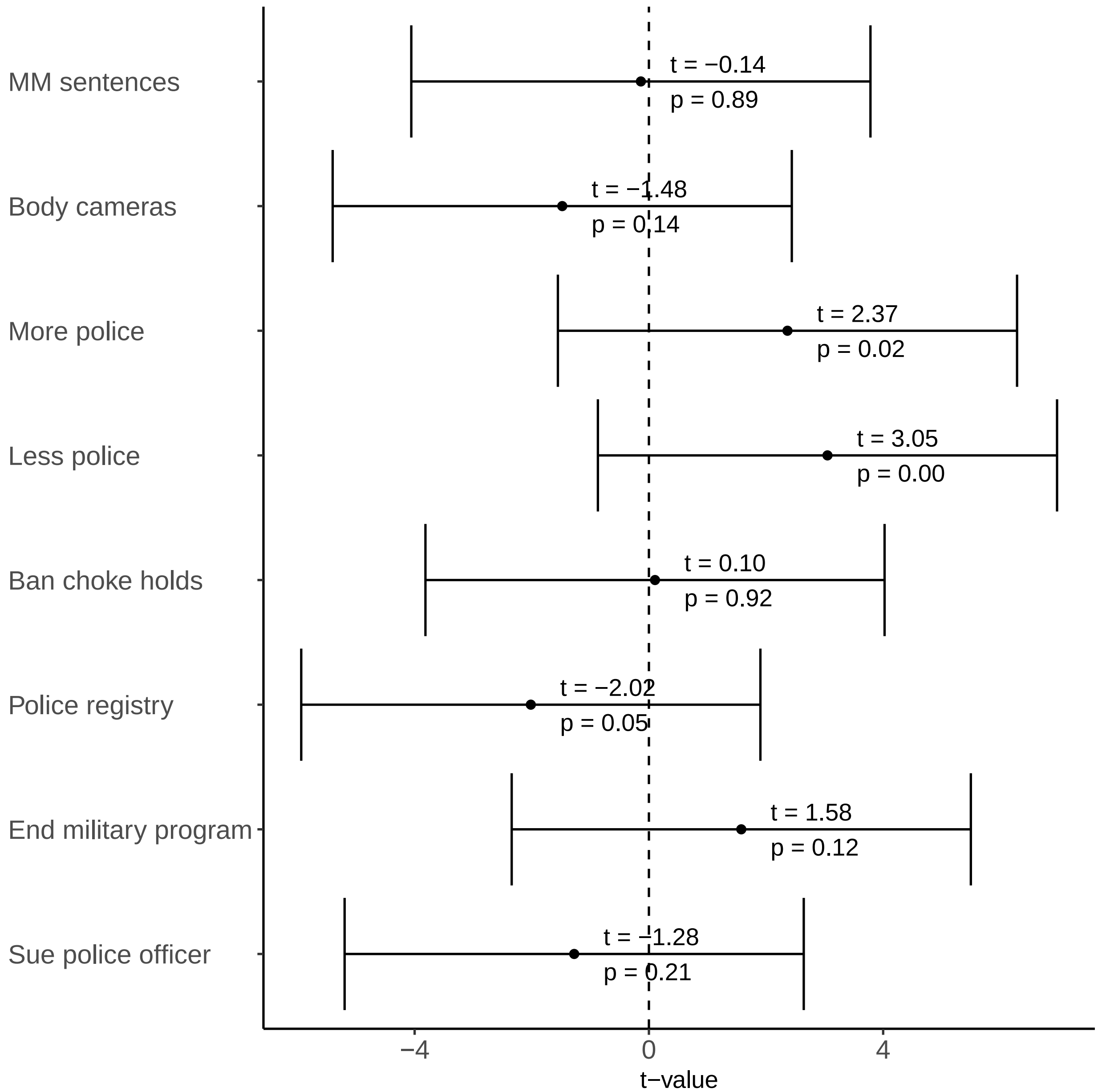


Figure S2.3: Forest Plot
Note: In the forest plot, the error bars show the 95% confidence interval (CI) for the mean difference between the two groups. When the p-value is very small, the difference between the two groups is highly unlikely to be due to chance. However, this does not necessarily mean that the true difference is large or practically significant.

AMCE

The AMCE analysis aims to estimate the average effect of each attribute level on the respondent's preferences while accounting for the presence of all other attribute levels in the profiles. The AMCE analysis estimates the average change in the probability of a profile being chosen or rated higher when a specific attribute level is present compared to when it's absent, averaging over all possible combinations of the other attribute levels. Positive AMCE coefficients indicate that an attribute level increases the likelihood of a profile being preferred, while negative coefficients suggest a decrease in preference.

Manipulation check

I use a combination of policies proposed or on the ballot in US states and hypothetical CES policies (N = 21). I coded each as either "punitive" or "progressive." Then, I created pairs: corresponding policies with the opposite effect on crime control, either tough-on-crime or progressive, depending on the original. For example, if the original policy were that police officers would be required to wear body cams at all times, I created another version, which is that police officers would get to decide if and when to wear a body cam.

The association between the researcher's policy classification as punitive and the respondents' assessment was further examined through a contingency table analysis. Respondents were asked, after each policy and measurement of the outcome, the following question: "If you had to guess, would you say this legislation would be harsher than current policy?" The results are summarized in Table S3.1. Table S3.2 presents contingency tables by police domain.

In the Bail policy, the responses appear more aligned when the policy is classified as more punitive. In Prison policies, the respondent alignment is stronger, especially when the policy is classified as more punitive. For the Police policies, the respondents are split relatively evenly. This suggests that the respondent's views are more diverse and that possibly the wording of the manipulation check as "harsher" makes less sense in the context of policing. In the Fine and Misdemeanor policies, similar to the Bail policies, respondents tend to agree more when the policy is classified as more punitive.

Table S3.1: Contingency table of researcher's classification and respondents' assessments

	Assessment "Harsher"	**Assessment "Not harsher"**
Coded "tough-on-crime"	2546	1529
Coded "progressive"	1385	2696

A properly implemented conjoint experiment can provide reliable measures of multidimensional preferences and estimate the causal impacts of multiple attributes on hypothetical choices or evaluations (Bansak et al. 2021). Moreover, this design allows us to deconstruct

Table S3.2: Contingency tables for four different policies

	Bail 0	**Bail 1**	**Prison 0**	**Prison 1**	**Police 0**	**Police 1**	**Fine 0**	**Fine 1**
0	351	269	1207	568	560	500	428	192
1	179	379	463	1322	555	601	188	394

the often competitive and noisy informational environment (Chong and Druckman 2007) by presenting the respondents with a comprehensive set of variables. In doing so, we can discern the factors within a "bundled treatment" that significantly influence attitudes.

Our interest lies in the collective effect of each policy group (punitive policies versus progressive policies) rather than the influence of any individual policies. By aggregating attitudes toward 42 specific policies (with each respondent evaluating only seven), we can distill these preferences into a broader punitive or progressive sentiment.

Odd profiles: Model-Based Exploratory Analysis

For data collection, this experiment employs the uniform distribution that equally weights each profile because uniform randomization is preferable in scenarios where a natural target profile distribution is absent. A robustness test is facilitated to estimate the coefficients under a profile distribution of theoretical interest, and as demonstrated here, the analysis of profiles, adhering to real-world theoretical importance, yields results consistent with the full uniformly distributed sample of profiles. Nonetheless, the paramount goal remains the internal validity of the conjoint experiment, akin to all experimental treatment effect studies.

To scrutinize the AMCEs utilizing a conjoint experiment with uniform profile distributions, a theoretical profile distribution of interest is modeled, and the analysis is reiterated. This method aids in excluding "odd profiles" from the analysis. Given the absence of a natural target profile distribution in this study, uniform randomization was employed. However, it is prudent to examine the robustness of the AMCE estimates to alternative profile distributions of theoretical relevance, albeit requiring additional modeling assumptions, aiding in exploratory and sensitivity analyses.

Presented herein are results for the subset of profiles strictly adhering to a designated joint distribution of interest: "Progressive" policies are predominantly supported by the Black Lives Matter movement, the ACLU, and the African American Chamber of Commerce, while "punitive" policies find backing from the District Attorney, Sheriff's Department, Police Unions, Prison Guards Union, and Crime Victims Association. Moreover, cues about non-White voters' preferences are more inclined to support policies endorsed by the Democratic party, mitigating the potential for erroneous signaling of bipartisan or consensus support. Overall, 1571 original "odd-profiles" were excluded from this analysis.

The findings reveal a remarkable consistency with the original results. Although differences among various supporters and cost factors are no longer significant, the principal outcomes of interest align identically with the full sample results, underscoring the robustness of the initial findings under varying profile distribution assumptions.

S4 Sub-group analysis

Unlike displays of conditional AMCEs, differences in conditional marginal means communicate subgroup differences for all feature levels, including the reference categories (Leeper, Hobolt, and Tilley 2020). Figure S4.1. When grouping the respondents by whether they reported being White or POC, we detected no group differences in response to attribute levels.

Table S4.1: Differences in Conditional Marginal Means: Republicans-Democrats

Level	Estimate	(Std. Error)	p-value	95% CI
Progressive policy	**-0.077**	0.020	.0001	(-0.115, -0.039)
Tough-on-Crime policy	**0.129**	0.020	¡.0001	(0.089, 0.169)
No party info	0.019	0.022	0.370	(-0.023, 0.061)
Respondent's party	0.023	0.027	0.393	(-0.030, 0.075)
Opposite party	0.046	0.028	0.102	(-0.009, 0.101)
AA Chamber of Commerce	0.034	0.028	0.223	(-0.020, 0.088)
BLM and the ACLU	-0.030	0.028	0.273	(-0.085, 0.024)
Crime victims association	0.024	0.026	0.373	(-0.028, 0.075)
Police and prison unions	**0.080**	0.027	0.003	(0.028, 0.133)
DA, and Sheriff	0.026	0.028	0.351	(-0.029, 0.081)
Decrease costs	0.043	0.019	0.025	(0.006, 0.081)
Increase costs	0.010	0.019	0.617	(-0.028, 0.047)
Individual responsibility	**0.049**	0.025	0.049	(0.0003, 0.098)
Outcomes	0.008	0.025	0.741	(-0.041, 0.057)
Principles	0.012	0.025	0.631	(-0.037, 0.061)
Social responsibility	0.036	0.025	0.152	(-0.013, 0.085)
Opposed by racial group	**0.049**	0.020	0.015	(0.009, 0.089)
Supported by racial group	0.003	0.018	0.874	(-0.033, 0.039)

Note: A comparison of the conditional marginal means between Republicans and Democrats. Conditional Marginal Means are the means, averaged over the distribution of the covariates, of supporting a policy for different groups after taking into account the influence of other covariates.

S5 Racial Attitudes Scales

Table S5.1.

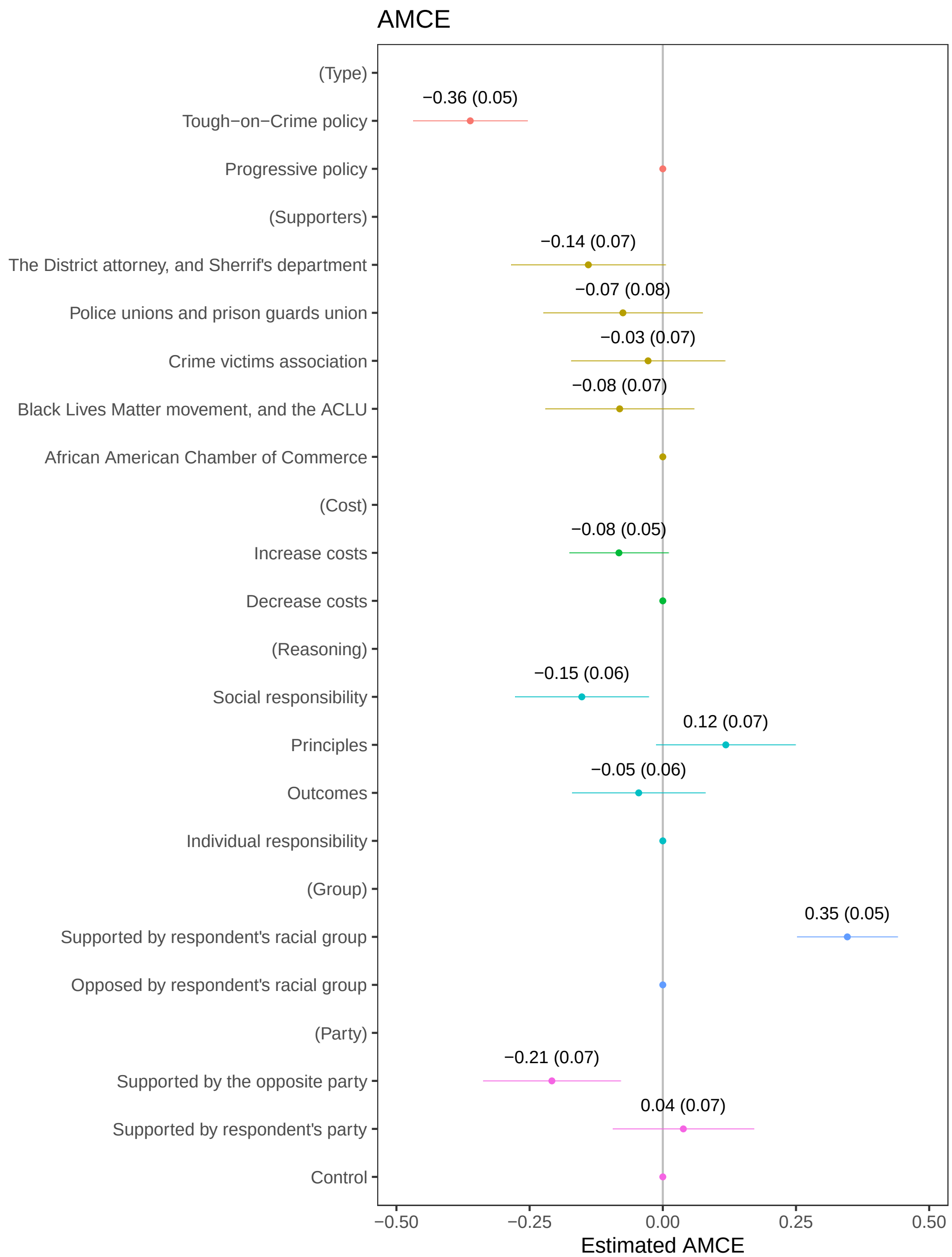


Figure S3.1: AMCE plot - excluding odd-profiles.
Note: In this analysis, I subset the sample to profiles with a joint distribution of theoretical interest. The sample includes 8309 observations (compared to the original 9880 full sample). I used the Binomial family for the GLM. The coefficients represent the change in the log-odds of the dependent variable for a one-unit change in the independent variable, holding all other variables constant.

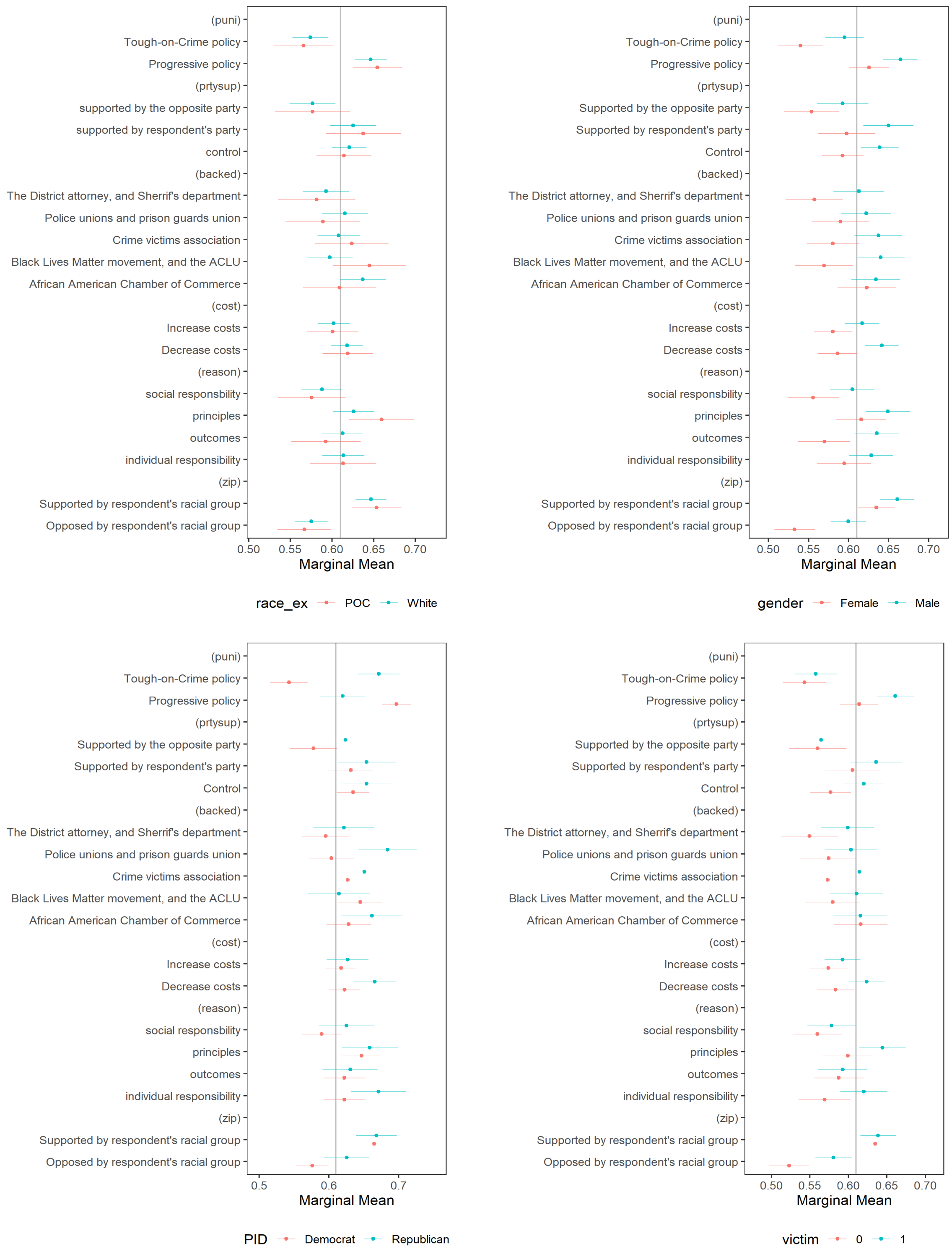


Figure S4.1: MM sub-group analyses. Clockwise from top-left: (1) Reported race, (2) Reported gender, (3) Reported partisanship, (4) Reported past victimization.

Table S5.1: Racial Attitudes Scales

Racial Resentment	Racial Sympathy	FIRE
Irish, Italian, Jewish, and many other minorities overcame prejudice and worked their way up. Blacks should do the same without any special favors.	Michael is a young black man who got a pat-down from a cop to see if he carries any concealed weapons. Michael is very upset by this treatment. How much sympathy do you feel for Michael?	I appreciate the company of people from diverse racial backgrounds.
Generations of slavery and discrimination have created conditions that make it difficult for blacks to work their way out of the lower class.	Laurette is a black woman that got turned away from a nanny job for no apparent reason. Laurette is upset about Mrs. Lewis' actions. How much sympathy do you feel for Laurette?	White people in the US don't have advantages because of the color of their skin.
Over the past few years, blacks have gotten less than they deserve.	The community leaders of a primarily black neighborhood fail in their attempt to relocate a pollution-heavy bus depot to a new location. They are very upset by the city's inaction. How much sympathy do you feel for the community leaders?	People from a racial minority are sometimes less successful because of prejudice and discrimination.
It's really a matter of some people just not trying hard enough: if blacks would only try harder, they could be just as well off as whites.		Race plays an important role in who gets sent to prison and who gets away with "a slap on the wrist."
		Racial problems in the US are rare, isolated situations.
		When I hear about acts of racial violence, I become angry or depressed.

S6 Study 2

Party groups cues

For respondents in the partisanship condition, I detect only minor effects. Importantly, I do not find any adverse effect of endorsement by the opposite party. Figure S6.1 shows that for Republicans, cues about party support had a minor effect on average support for reform (Chi-squared test, $\chi^2 = 7.333$, df = 3, p = 0.062). For Democrats, the treatment did not have an overall significant effect, possibly due to ceiling effects as progressive reform support is already quite high, as Figure S6.1 visualizes.

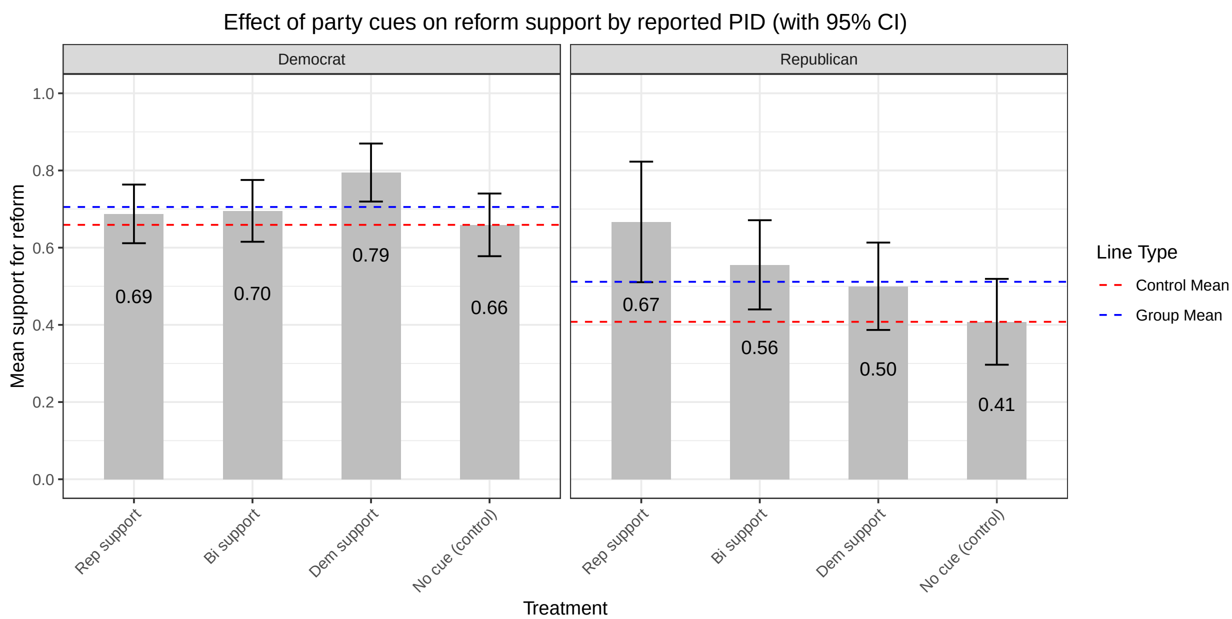


Figure S6.1: Party group cues
Note: Mean support for the four progressive policies by party group cues with 95% CI.

Existing literature demonstrates that voters frequently defer their policy preferences to align with their perceived party positions (Lenz 2013). Yet, concerning in/out-party cues, the only salient effect identified is opposition to the opposing party. When the rival party endorses a policy, predicted support diminishes; however, this effect is not observed when examining specifically progressive reform, which aligns with findings from related research (Esberg, Mummolo, and Westwood 2020). It is plausible that preferences for progressive reform are relatively constrained, thereby mitigating the influence of party cues (Bullock 2011). This implies, in comparison, that the observed racial group effects on progressive reform support may be notably substantial.

Complete analysis

Figure S6.2 show that by pooling all the respondents that received the racial group cue (column 1), we detect a significant effect for a group cue signaling support from Black voters. These coefficients are estimated while controlling for demographics, attitudes correlated with the outcome, and the MTurk sample.

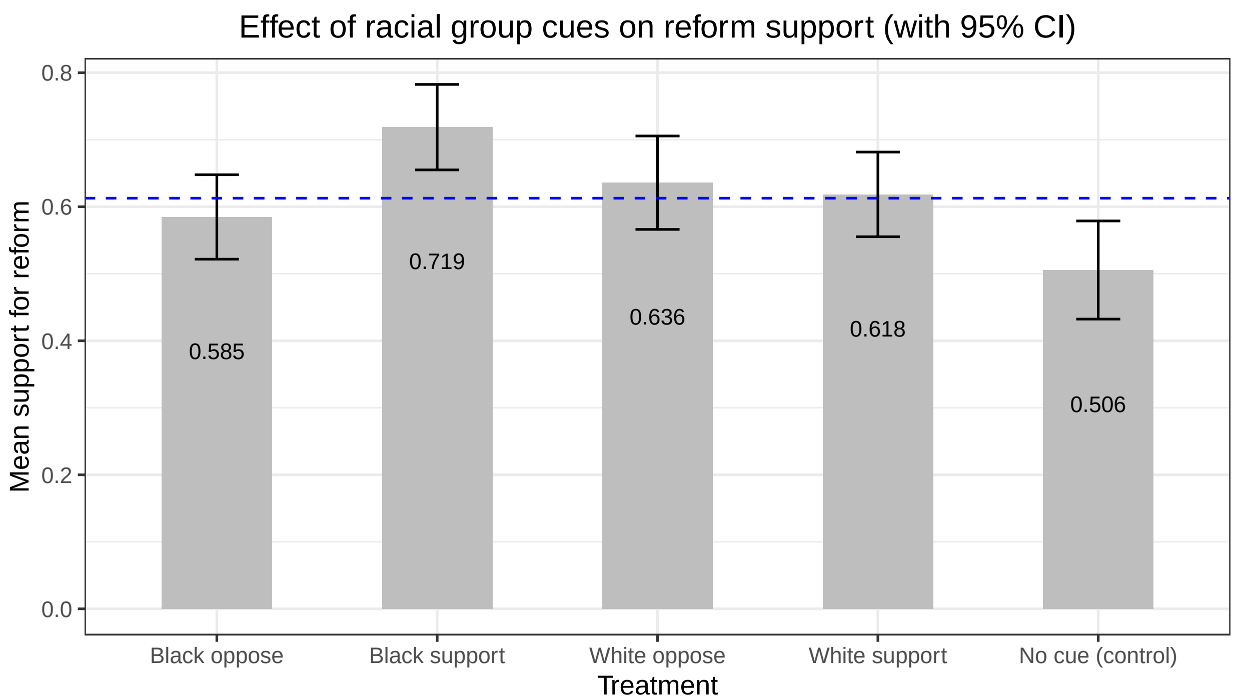


Figure S6.2: Racial group cues

Note: The effect of a racial group cue on all respondents. The dashed line represents the average support for progressive policies.

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Chapter 5

Conclusion

The dissertation demonstrates the centrality of political dynamics in shaping the American criminal justice system. At the core of these dynamics lies the complex interplay between voters, elected officials, and law enforcement agencies, each motivated by distinct incentives and operating within a broader socio-political context.

Voters' attitudes diverge both on the desired severity of punishment and their acceptance of penalization itself. This nuanced stance, characterized by the 'extensive and intensive margins' framework, underscores the need for political actors to gauge public sentiment accurately. Misinterpretations risk undermining the very reform efforts they seek to implement, as illustrated by the case study of the San Francisco District Attorney recall election.

Elected officials, particularly prosecutors who are elected on a reform platform, are caught between the mandate for reform, and the limits of their institutional authority. This tension is exacerbated by the strategic behavior of police departments, which may adjust their efforts in response to perceived political shifts, as evidenced by the changes in policing patterns surrounding the recall election. Such adaptations not only raise concerns about the equitable application of the law but also underscore the need to account for these institutional responses when evaluating the impact of progressive policies.

Lastly, the dissertation integrates the concept of Related Justice, illustrating that support for criminal justice reform is closely linked to racial justice attitudes. The dissertation investigates the deep-seated influence of race and identity in shaping public opinion on criminal justice reform. The positive correlation between support for reform and positive racial attitudes, particularly among white respondents who follow cues from Black voters, highlights the potential for building broad coalitions centered on shared values of justice and equity.

Taken together, these findings contribute to a more comprehensive understanding of the politics of criminal justice reform. They suggest that successful reform efforts must:

1. Accurately gauge and respond to voter preferences along both the extensive and intensive margins of criminal justice policy. 2. Account for and address potential strategic responses from law enforcement agencies. 3. Recognize and leverage the interconnectedness of racial justice and criminal justice reform in public opinion.

In conclusion, the path towards a more just and equitable criminal justice system is intrinsically political. It necessitates a deep understanding of the complex interactions between voters, elected officials, and law enforcement. It requires an appreciation of the nuanced nature of public opinion, which is shaped by both personal experiences and broader social forces. And it demands a commitment to building coalitions that transcend traditional political divides, uniting diverse groups around a common vision of a society where justice is not merely a concept, but a lived reality. Future reform efforts should focus on building broad coalitions - not just among voters but political institutions: prosecutors, police departments, mayors offices, and sheriffs. Only through proper institutional design, we create the foundations for reform. The benefits of reform cross racial and political lines, yet currently, they do not cross political institutions' incentives and structure, which benefits from the status quo.