Fall 2022

Decision Making and Reform Within the United States Criminal Justice System

Rainita Narender
Claremont Graduate University

Decision Making and Reform Within the United States Criminal Justice System

Rainita Narender

Claremont Graduate University
2022

Approval of the Dissertation Committee

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

Gregory DeAngelo, Chair

Claremont Graduate University

Associate Professor of Economic Sciences

Melissa Rogers

Claremont Graduate University

Assistant Professor of Politics and Economics

Mark Hoekstra

Texas A&M University

Assistant Professor of Economic Sciences

Abstract

Decision Making and Reform Within the United States Criminal Justice System

Rainita Narender

Claremont Graduate University: 2022

The United States Criminal Justice System has undergone massive legislative reform in the past decade. These reforms have prompted a demand to analyze the benefits and potential unintended consequences of proposed policies and legislation. The following dissertation measures policy implications across three types of actors in the criminal justice system: 911 call-takes, prosecutors, and law enforcement. As most citizen-officer interactions arise from 911 calls for service, the first chapter of this dissertation is a study on the "priming" effect 911-call-takers have on officer decision to arrest and use force on calls for service in Dallas, Texas (2013-2018). In this work, I leverage random assignment of incoming calls to 911-call-takers to explore the effect of call-takers' high propensity to mention Hispanic/Latino, Black or White race on call outcomes. I find that marginal assignment of a call-taker with a high propensity to mention Black, Latino and White races results in a decrease in Black and Latino arrests and decrease in Black force use incidents compared to calls where no race is mentioned. The novel contribution of this research is to fill a gap in the empirical literature of 911-call-taker idiosyncrasies in transcribing information on officer decision-making.

The second chapter of this work explores the role of a recent California legislative reform, Proposition 57, on the role of prosecution. The value of a criminal case, often determined by identifying the level of punishment that is appropriate given the severity of the offense, can be determined by potential consequences for a defendant, harm being redressed, and public, office, or prosecutors' interests. Actors within the legal process must handle cases according to the value of the case while facing time and resource constraints. Legislation that changes the determinants of a case's value will likewise affect the behavior of the agents. Given

a case that is deemed to have a higher value, a rational agent prioritizes select cases and dispenses more readily with other lower value cases. Three testable predictions result from this model: (i) prosecutors seek the higher value for threshold cases; (ii) prosecutors resolve lesser cases quickly, through not filing, diversion, or plea; (iii) prosecutors adopt alternative consequences to offset any decrease in cases' value. We test this hypotheses within the context of California's recent legislation, Proposition 57, using difference-in-differences analysis. Our results support these predictions, legislation introduction contributes to case valuation.

The third chapter examines the effect of a low priority local initiative on contracted and non-contracted police and prosecutors. Moreover, the extant empirical literature has primarily focused on how norms affect the design of legal rules, paying less attention to their role in the enforcement or prosecution of laws. This work investigates whether social norms affect law enforcement and prosecution practices when laws and norms are not necessarily aligned. This empirical analysis focuses on the impact of the adoption of a low priority initiative (LPI) on police and prosecutor behavior in Los Angeles County. LPIs are not legally binding and while such initiatives signal underlying norms, they may conflict with police and prosecutor preferences. Results suggest that following the introduction of an LPI there is a rise in the number of misdemeanor arrests, but not in the rate that misdemeanor marijuana offenses are dismissed. We then relax our parallel trends assumption in our main analysis using synthetic difference-in-differences and show that results remain consistent. We conclude that law enforcement preferences have a stronger influence over law enforcement behavior than community norms, while prosecutor behavior is unchanged with regards to the local norms.

Acknowledgments

There are a number of people I would like to acknowledge for their continued support and willingness to invest their time and energy in me. I know that it would not have been possible for me to get this far without your support. First, I would like to thank the research community at Claremont Graduate University (CGU) and at the Computational Justice Lab (CJL). I have immense gratitude toward my advisor Greg DeAngelo for opening my eyes to the application of causal inference as a tool to evaluate policy, for your close and careful mentorship and for offering me numerous opportunities to develop my career throughout the program and beyond. Thank you for believing in me Greg. I would also like to thank my committee members Melissa Rogers and Mark Hoekstra for their professional and personal mentorship and support of me, especially when I questioned my place in the program, your reminder to keep my head up and focus on pushing research is priceless. I have a lot to thank of my co-authors and professors at CGU for your guidance as I first joined the program and lacked a sense of direction. I would like to give a special thanks to Hisam Sabouni, Pierangelo DePace, Jeffrey Borowitz and Paul Zak. I owe a special thanks to CJL for not only financial supporting my PhD program, but for more importantly creating a community of kind, thoughtful and engaging peers. I would like to personally thank Maryah Garner, Yunie Le, Josie Xiao, Morgan Stockham, Hunter Johnson, Jason Justo, Li-Hsin Lin, Minjae Yun, Ryan Quandt, Ben Feingold, Spencer Mueller and Will Wyatt for their encouragement and support of my research and academic pursuits. Maryah, thank you for believing in me and encouraging me to pursue research and teaching, it helped build the confidence and foundation for everything I accomplished in the program.

Prior to my graduate school experience, I was fortunate to have enthusiastic teachers in both my undergraduate and high school careers that inspired a sense of curiosity in me which sparked my interest in the broader social sciences. I would like to thank my undergraduate professors Urmee Khan, Analise Flores, Sarah Mednick and Yang Xie for fostering my love of

research, statistics and economics. A special thanks to my high school teachers Mr. Young, Mr. Little, Mrs. Holy and the late Dr. Blaker for being my support system during a turbulent time in my life and for pushing me to pursue a four year education when it did not seem possible at the time. I am certain that I would not have developed as a person and found myself at the rate that I did without your support. I cannot share enough appreciation for my lifelong friends who have stood by me through all of the ebbs and flows in the journey of life and have loved me like family, thank you Gabrielle Mata, Simran Singh, Raman Khinda, Sahira Rizvi and Rene Bui.

Lastly, I would like to give a special acknowledgement to my family and significant other for their support of me. I will always remember the day I found out about my acceptance at CGU, jumping with joy with my father and furthermore of my family's willingness to support my dreams in pursuing a doctorate in place of a full-time job. To my anchor, lifelong partner and best friend, I am thankful for your love and desire to spend each day in support of one another. I can say aside from my academic pursuit, CGU served as a place of finding my partner, and for that I am forever grateful. Thank you to all that have supported me and guided me along the way.

Contents

1 Chapter 1: "911, What's your Emergency?": Effect of Call-taker Proclivity to Mention Race on Arrest and Force Use Outcomes

1.1 Introduction

Concerns of racial disparities in citizen-police interactions have fueled widely contested debates on police reform in America. Many of these concerns are fueled by increased media attention on cases of officer force use that have resulted in citizen death. The majority of force incidents shaping citizen sentiment toward officers arise from citizen initiated police interactions via 911 calls for service Davis and Anthony Whyde (2018). The following paper explores a considerably understudied aspect of citizen initiated officer interactions, the "priming" effect of a 911 call-takers' transmission of caller information to dispatched officers. As officers' apriori beliefs arriving to a service call are shaped by information relayed by 911 call-takers, empirically evaluating the effects of call-taker discretion on what information is relayed requires further exploration. 911 call centers are comprised of call-takers and dispatchers. Call-takers are tasked to randomly attend to incoming calls, extract critical information from the caller, and assign priority to the call type; while dispatchers use a computer automated dispatch, CAD, system to assign available officers to service the call.[1] To ensure equitable servicing in each interaction with the public, call-takers are typically provided a transcript of prompts to inquire pertinent information from the caller that is later relayed to the dispatched officer.

The following work leverages call-taker specific idiosyncrasies in extracting suspect race information where call-takers can vary both the menu of potential suspect races and the

[1] 911 call centers are responsible for handling a variety of services including but not limited to theft, injuries, violent crimes and community welfare checks.

order of races presented to the caller. The nature of 911 call-takers answering calls at random as they are received by the call center allows for the application of a quasi-random experimental design. This design is used to measure the effect of calls answered at the margin by call-takers with a relatively high proclivity to mention Black, Hispanic or White races relative to their peers on downstream call for service outcomes. In the following sections, first a brief description on cases showcased in the media defining examples of call-takers' "priming" effects on call outcomes will be outlined, followed by a review of previous work establishing the importance of call-takers in officer-citizen interactions. Subsequently, the methods section will initially describe the 911 call center setting and the timeline procedure of a call for service followed by descriptive statistics of the data and will then lay out the formal empirical model being tested. Finally, a presentation of several checks on the exclusion restriction and its meaning in the setting of this paper will ensue followed by both OLS, 2SLS and reduced form results concluded with a discussion pertaining to insights on potential policy relevance.

A few cases that have generated longstanding societal discourse are the cases of Henry Louis Gate JR., Tamir Rice and George Floyd. In each of these cases, a citizen requested officer assistance by means of a 911 call for service, and the call resulted in a poor civilian outcome that is attributed to racially biased policing practices. Henry Louis Gates Jr.'s 911 call transcript has garnered widespread media coverage as the African American Harvard professor arrested upon entering his residence after a bystander made a 911 call for service for a potential breaking and entering. Notably, Gates provided proof of residence upon the dispatched officer's arrival, but due to Gate's unease and claims of racial profiling he was inevitably brought in to custody for disorderly conduct. The case of Tamir Rice is of a nine year old boy killed while playing with a toy gun at Cleveland Park. A bystander unsure of Rice's age or if his toy was real, called 911 and exclaimed that she saw an African American man holding a gun, later adding that the citizen was unsure if the individual was a youth

and if the gun was a toy. Unfortunately, the latter information was not relayed prior to the dispatched officer arriving to the scene, and within moments of arrival, Tamir was shot dead. The case of George Floyd has garnered arguably the most media and societal attention as of late and has been widely acknowledged as attributable to racially disparate treatment from officers to citizens of color. In particular, the circumstances leading to George Floyd's death spurred some of the most persistent protests against racially biased officer treatment of citizens. As many individuals are familiar with the case of George Floyd, an African American male restrained and eventually killed by an officer's choke hold for using counterfeit money to purchase items from a convenience store, less attention has been made to the fact that this event was initiated through a 911 call for service by the store's employee where the counterfeit currency was used. Consequently, the store worker had cameras pointed to the altercation of George Floyd with dispatched officers, and was one of the initial people to approach officers and inquire about Floyd's well being. It is unclear what information and how the information provided to officers prior to their arrival to these calls for service, such as citizen race, stature and other physical characteristics may have primed officer behavior before they ever interacted with the citizen. I do not observe the counterfactual of officers initial interaction, only the outcomes realized when information is relayed prior to arrival. Serving as a throughline across these calls for service is the belief of racial disparity in officer treatment toward citizens. As a considerable number of citizen-officer interactions arise from a citizen initiated call for service, better comprehension of this theme in the 911 call for service setting is necessary. To gain more insight in the 911 call service process, the author further explores how the dissemination of information impacts call for service outcomes.

As most events within a citizen-officer altercation begin with a bystander call for assistance to their local 911 agency, gaining a better understanding on how this transaction of information frames an officer's beliefs when arriving to the scene is of great importance (Davis and Anthony Whyde, 2018). In the following paper, I leverage 911 Dallas call-for-

service data to study the effect of call-taker's mention of citizen race as a priming mechanism on officer use of force and arresting behavior. Though this topic has been previously understudied (Gillooly, 2020a,b; Taylor, 2020), a recent contribution by Gillooly (2021) examines the effect of call-taker propensity to assign high priority to mental health calls on officer's priority assignment upon arriving to the call. This research is critical in anchoring and establishing the link between call-taker action on officer behavior (Gillooly, 2021). The following analysis extends prior contributions by focusing on all types of call for service beyond mental health, expanding the time frame and sample size allowing call-taker propensities to be flexible over time and by examining a different priming mechanism identified through call-taker's transcribed notes using a natural language process technique of regular expression extraction.

Racial bias within the Criminal Justice System has been widely studied regarding topics such as preference based versus statistical discrimination, judge sentencing leniency and even prevalence in use of force disparities(Antonovics and Knight, 2009; **?**; Fryer, 2019; Hoekstra and Sloan, 2020). The novel contribution of the following research is to examine how calls being randomly assigned to call-takers with higher race mention propensities effect downstream call for service outcomes. Causality is achieved through an instrumental variable approach, leveraging exogeneity from random assignment of incoming 911 calls to an available call-taker. Random assignment ensures that call-takers have an equal probability of having received calls of a specific call type and priority level. To address call-taker shift preferences being correlated with certain calls for service, the author residualizes the results with shift by day of week and week by year fixed effects. The author explores this research question with data from the Dallas Police Department (DPD) including calls for service spanning 2013-2019.

1.2 Previous Research

This research broadly fits two bodies of research that have been recently active. First, this research contributes to the literature on police-citizen interactions, especially as they relate to police use of force. Second, this research builds on previous work, largely conducted by criminologists, that examine the role and impact of 911 call takers and dispatchers on community safety.

1.2.1 Citizen-Police Interactions

The relationship across citizen characteristics, citizen demeanor and police conduct has received considerable attention in the literature. For example, Lundman (1996) and Engel et al. (2000) revisit a nearly forty year research program on the demeanor of citizens and police, confirming prior research findings that demeanor and suspect characteristics interact to produce differential patterns of citation, arrest, and the use of force. Indeed, Lundman (1996) concludes that the "analysis therefore provides little reason for questioning the agreement reached over four decades that demeanor and other extralegal variables shape police actions."

Nevertheless and in the wake of a number of high profile deaths[2] while in police custody, there has been a resurgence of research related to police-citizen interactions. For example,the introduction of technology such as body worn cameras (BWC) have been examined by Hedberg et al. (2017), noting that the use of BWC's significantly reduce complaints and use of force associated with calls for service. Williams et al. (2021) further argue that the benefits-cost ratio is favorable for the utilization of BWCs, thus making the argument that BWCs are cost-prohibitive inaccurate.

Other research has examined the impact of specific police personnel on community safety.

[2]Breanna Taylor, Elijah McClain, Philando Castile, Michael Brown and George Floyd to name just a few, are instances of citizen death by officers.

McCrary (2007) examined changes in the racial and gender composition of law enforcement agencies that resulted from affirmative action policies. He notes that the affirmative action litigation yields mixed results in terms of police performance, but some statistically significant changes in the number of serious arrests involving Black citizens. Although McCrary (2007) notes that compositional adjustments to the personnel of the agency do not yield large changes, Mummolo (2018) finds that law enforcement response to managerial directives regarding their stopping behavior of criminal suspects lead to significant increases in stops producing evidence of a suspected crime. Thus, there are fruitful adjustments to law enforcement agencies that can produce greater community safety.

More recent research has built on this work by focusing on outcomes, specifically police use of force, by the race of the citizen. Fryer (2019) and Johnson et al. (2019) find no statistical differences in use of force by the race of citizens.[3] Alternatively, Ba et al. (2021) and Hoekstra and Sloan (2020) find that minority police officers are less likely to engage in use of force against minority citizens, a result that is especially pronounced in minority communities. Lastly, Theobald and Haider-Markel (2008) notes that symbolic representation in police-citizen interactions does occur, as Black citizens are more likely to perceive police actions as being legitimate if there are Black officers present, while White citizens are more likely to perceive police actions as legitimate if the actions were conducted by White officers.

Finally, Knox et al. (pers) raise an important point with regards to administrative stop, call-for-service, and arrest data that are ubiquitously used in empirical criminal justice research. Specifically, administrative data could be contaminated due to "subset ignorability", as these data are a function of citizen detainments, which could be introducing bias that cannot be overcome in analyses.

[3] See also Knox and Mummolo (2020) who offer a criticism of the Johnson et al. (2019).

1.2.2 911 Call Takers and Dispatchers

Indeed, a large body of academic literature explore characteristics associated with lethal use of force on both the side of the officer and citizen (Rojek et al., 2012; McElvain and Kposowa, 2008; Mears et al., 2017). Much of this work has focused on citizen-officer specific characteristics related to force use or altercations leading to a poor outcome after an officer has began interacting with a citizen. Less has been established on the causal link between events leading up to officers interactions with the community, 911 call-takers deemed as the "gate-keepers" of the criminal justice system (Lum et al., 2020).

One of the most active researchers in establishing the call-taker - officer behavior link is (Gillooly, 2021). In this work, the author builds off from their prior research suggesting a qualitative relationship between call-taker assignment on call priority and the subsequent effects on officer assignment of call priority. More explicitly, (Gillooly, 2021) constructs a call-taker call priority assignment propensity to estimate the effect of random assignment to a call-taker with high propensity to assign calls with high priority on officer assignment of priority upon arriving to the call for service. Gillooly finds calls answered by call-takers with greater "alarmist" qualities result in officers assigning similar high priority to these incidents. This analysis is conducted in a subset of service calls related to mental health in Michigan from 2015-2016 (N = 20,764).

(Gillooly, 2020b) analyzes the role of a call-taker's interaction with a caller in assessing risk. By qualitatively and quantitatively measuring the norms and discretion practiced by call-takers and dispatchers, Gillooly studies to what extent these idiosyncrasies at the front-end impact officer-citizen interaction and altercation. Gillooly dissects the nature of a 911 call/dispatcher center learning the institutional norms such as call- taker discretion on what information is relayed and how this information sets the tone for an officer's prior beliefs when arriving to service calls. Call-taker discretion is evaluated through a field experiment, namely random assignment of call-takers to calls and showing that discretion on when to

assign higher risk is directly linked to officer use of force at the scene.

(Gillooly, 2020a) examines a case study of a 911 call resulting in the arrest of Louis Gates Jr. after a Caucasian female reported she saw two men she believed to be breaking and entering a home in Cambridge, Massachusetts. The level of fear in the caller's voice translated to a serious call type priority though the caller also mentioned she is unsure if the two men are entering their own home. When police arrived to the scene, they arrested Harvard professor Gates for disorderly conduct in his verified residence. This can in part be attributed to the fear translated from caller by the call-taker and subsequently to the officer.

Taylor (2020) employs a randomized controlled experiment with a police firearm simulator and 300 active law enforcement to study the effect of dispatcher information on officer use of force. Officers were randomly assigned to a priming group of citizen with a gun, citizen with a cell-phone, or a control group with no primed information. Results indicate when dispatchers provide officers inaccurate information, i.e. citizen is holding a gun when they are actually holding a cell-phone, officers shoot the citizen more than twice as much compared to the no priming scenario, though a cell-phone is visible in the citizen's hand. Moreover, when officers are provided accurate information, citizen is holding a cell-phone, officers have a misfire rate of 6% compared to a 62% with the inaccurate information scenario. Call-taker and dispatch priming is a critical and effective method of inciting biased prior beliefs to an officer before their arrival to service the call, with the serious consequence of a potential loss of life.

In this paper, the author explores call-taker priming on officer use of force and arrest behavior by leveraging variation in call-taker's propensity to disclose suspect race in a 911 call for service. Further, the author hypothesizes that officer knowledge of race will prime officer behavior by updating their beliefs and expectations before arriving to the scene. There are endogeneity concerns prevalent when naively examining the relationship between race mention and call for service outcomes. Such that race mention calls are systematically

occurring in neighborhoods with high crime rates or high call priority that are acting as the true determinants that prime an officers' behavior. To overcome this, the author makes use of a jacknife "leave-out" instrumental variable strategy to address potential endogeneity. Namely, this strategy leverages exogeneity of calls being randomly assigned to call-takers and each call-taker having exogenous variation in race mention propensity in the initial comment relayed to an officer. This call-taker race mention instrument is flexible across years of duty and has the current days worth of calls omitted as our "leave-out" unit. The author finds that calls randomly assigned to call-takers with high propensities to mention a minority race, Black or Latino mentions, result in an increased probability of force use and arrest. These results are robust when attributing for an array of call characteristics.

1.3 Methodology

1.3.1 911 Calls for Service in Dallas

The majority of police-citizen interactions are initiated by 911 calls for service in the United States (Davis and Anthony Whyde, 2018). The design relies heavily on a strong understanding of the 911 call for service process in Dallas, Texas which was achieved through interviewing internal personnel at dispatch centers as well as other dispatch offices across California. Combined, these resources have formed an institutionally backed understanding of the 911 call process. First, calls are routed to public-safety answering points (PSAP) where call-takers randomly answer calls as they come in the door. After answering a call, call-takers ask general questions regarding the incident such as the caller's safety, location and description regarding the call. In Dallas Police Department (DPD) Dispatch center call-takers are trained to follow a script with particular questions. For instance, if a residential burglary call for service comes in, the call-taker will ask some version of

> "911, this is Allison, how can I help you?"

The subsequent questions will evolve based on caller's response but call-taker's will generally inquire about the caller's safety, if the burglar is still present in the residence and ask about the physical characteristics to identify the suspect. In the current setting, the last question regarding physical characterization of a suspect is critical. Call-takers are instructed to inquire about physical attributes regarding the person, without priming the caller with a menu of options. For instance, a call-taker may ask

> "What does the burglar look like? What is their height, age, race if visible?"

An example of what call-takers are trained against asking is as follows:

> "Is the suspect Black? Or are they White, Latino, Asian?"

By framing the inquiry on suspect race with a menu of options, the call-taker is limiting the caller's responses to the set options provided and potentially priming the caller to respond based on the order effect of options supplied. Though call-takers are initially instructed and complete training to remain as consistent and standardized across calls for service, there is prevalence for call-takers to elicit race by providing different variations of the race inquiry option menu described above. Call-taker variation in mentioning some races more than other races in their menu of options is the exogenous variation leveraged in the following analysis. Since this data captures a truncated summary of the initial caller-call-taker interaction, one may wonder if we can sufficiently differentiate from callers mentioning differing levels of race across service calls compared to call-takers having varying race elicitation preferences, resulting in a range of different race mention propensities. We argue for the latter case since 1) call-takers answer calls at random, 2) they answer a similar volume of calls through their tenure and 3) they are assigned a variety of shift times which is also accounted for in the residualization process, ensuring each call-taker has an equal probability of answering each call type and priority. An additional concern about this identification strategy is the extent to which the implemented regular text natural language processing strategy identifies the

race of a suspect or complainant. Figure 1 notes the distribution of suspect mentions across call-types. It is believed that the distribution of suspect mentions described in this figure is indeed a lower bound estimate. Some call-takers may write "suspect" or various deviations on the word within their comment, while others do not note the word "suspect" when relaying the description. Therefore the author maintains all calls, even those where suspect is not directly mentioned since there is a plausible chance the description is referring to the suspect. To ensure complainant characteristics are not being captured in place of suspect, I remove all calls where race is mentioned, and the "complainant", or any derivation, is mentioned but there was no mention of the suspect. [4] A validation measure to test the limit of this assumption is made using arrest data. Race of suspects that were arrested by officers is identifiable and linked to the call-taker comments. From this linking it is observed that calls for service where White race is mentioned result in arrests of a person of White race 39% of the time, Black race mention result in the arrest of a Black individual 92% of the time and a call where Latino race is mentioned in call-taker's initial comment results in the arrest of a Latino individual 73% of the time. This validation test defines the limits of this assumption, call-taker race mentions have greater precision in identifying suspect race of minority groups compared to non-minority race mention.

There are 506 call-takers at DPD Dispatch in this 2013-2019 sample. These call-takers spend on average 53 months on duty answering an average of 20,582 calls throughout their tenure. After a call-taker answers an incoming call at random, the call-taker elicits and transcribes initial information regarding the incident into a computer where a dispatcher picks the incident from a queue, deems what they believe to be the most pertinent information, and relays this to the officer assigned to respond to the call[5]. Officers are then randomly

[4] In future robustness checks, I plan to drop any call-type where a suspect being present does not make practical sense coupled with when suspect is not frequently mentioned as is represented in the data. Therefore the implications from these results are constrained on the assumption that after censoring the data to calls with a greater relative likelihood of a suspect mention, race mentions are respective to suspects and not other individuals.

[5] It is a rare event that the call-taker is the same individual as the dispatcher, but this does occur for less

assigned to address the call for service based on their location and availability through a computer aided dispatch (CAD) system.[6] Though officers are assigned to a "home beat" in Dallas, lack of personnel require officers to move across beats and sectors to respond to service calls[7]. This movement suggests that the most accurate proxy or rather estimate for number of officers available in a geographical unit is achieved at the division level. Once an officer is assigned to a call, they are relayed initial information from the dispatcher and may have subsequent information relayed based on new caller information regarding the same incident or further elicitation of information by the dispatcher. As the following paper focuses on the exogenous variation from the initial interaction of 911 caller to call-taker, only the first comment recorded by a call-taker is considered. There are approximately 3.2 million calls for service incidents captured in the data.

Recently, DPD has released a number of datasets to the public in an initiative to foster transparency in policing practices. The following project combines a few of these datasets to empirically investigate the effects of call-taker race mention priming on call for service outcomes. All data considered ranges from 2013-2019 incidents, with the primary dataset consisting of 911 calls for service at the unique comment by incident level. Some incident types will have multiple calls to service the same incident, such as a roadside accident. To deal with such instances, we collapse the data at the unique geographic unit measured by latitude, longitude, time of call and call-type, maintaining only the initial comment placed for the incident. It is necessary to only maintain the first caller's comment to further ensure exogeneity in information transferred is maintained. Additionally, ensuring a specific call with many follow-ups is not over-weighted within the estimation, due to the duplicate rows

than 1% of calls in this sample.

[6]Officers are able to override the CAD by assigning themselves to a call via radio transmission to the dispatcher, though this is unobserved in this data. There is evidence that some officers may leave a current call for service if the new call has higher priority and the officer is the closest to the incident, though we find this to be an infrequent occurrence in this sample.

[7]A beat is the smallest geographical unit of officer assignment, multiple beats make up a sector and multiple sectors make up a division.

for the dame call, is mitigated by maintaining only the first comment within a call. A variety of information regarding the call is captured in this data covering the time and date, the call-taker who receives the call, the dispatcher assigned to the call and finally the problem type and priority level. Call-takers not only relay what they believe to be the most pertinent information for the officer to respond for service, but also they assign a problem type and priority for the call[8]. The call-types observed in this dataset can be generalized to three categories: theft, accident, and safety risks[9]. Call-takers also assign a priority of high or low for a subset of calls such as burglary, accident or miscellaneous crime to name a few, see figure 1.

Outcomes considered in the following analysis consist of whether an arrest was made and if force was used by an officer after an officer arrives to service the call. Arrest and force information is reported in separate datasets at the unique incident level which is merged onto the unique incident-by-call-taker comment data. Both arrests and use of force are rare events in the calls for service data we observe 0.16% of calls for service resulting in force and 1.51% of calls for service resulting in arrest. Information regarding the officer assigned to respond to the call for service is available at the incident level and is identifiable through badge number. Additional officer-level information on the beat, sector and division in which the incident takes place in is recorded.

The following section discusses natural language processing techniques implemented to identify race mention in the initial comment recorded by the call-taker.

[8]Though call-takers assign initial problem type and priority, dispatchers have the authority to modify one or both features if they believe it necessary therefore I do not analyze these outcomes as they may not be exogenous within the DPD setting

[9]Discrete call-types captured in this data are as follows: accident, assault, burglary in a business, burglary of motor vehicle, burglary in a residence, miscellaneous crime, disturbance, injury, robbery, shooter, theft, unauthorized use of motor vehicle or other. Descriptive statistics in table 1 display attributes of the call types in more detail.

1.3.2 Extracting Call-taker Race Mention Idiosyncrasies with NLP

To understand how the author treats language choices of call-takers, consider a simple model where each situation can potentially be described in many ways based on the ground truth facts. Call-takers exercise judgment by asking questions of the caller and choosing how to describe a situation.[10] The call-taker's goal is to provide useful context to the dispatcher and officer. We encode the call-taker's description of the incident using a bag of words model with the addition of a few aggregations of terms that are of particular interest to study.

Primarily, we consider race and gender information that may be conveyed. Though this information can be reflective of the underlying facts on the ground, individual call-takers also vary in the extent to which they find these characteristics important. Based on this underlying belief, call-takers can also vary in the extent to which they pursue questions to identify race/gender and in their propensity to mention race/gender in their notes after a caller mentions it. A second type of information is what could be thought of as "call specific effects". These are things which couldn't immediately be determined about the call by officers just from knowing the address but are clearly needed and in part show the nature of the call. For example, information about a gate or gate code suggests the location of the call is an apartment complex which can lead an officer to better gauge the nature of the situation. Other examples of this include call-taker mentions of "complex" , "floor" or other descriptors of the location in question.

The third type of information is one where the call-taker's judgment in how to ascertain details of the situation and then relay those details is potentially more important. This includes mentions of race and gender (which we separate as objects of study) but can be much broader. While race mention is sometimes a key aspect related to the suspect, there are other instances where race cannot be clearly observed or is mentioned with respect to

[10] Call-takers also exercise judgment by categorizing calls and assigning an associated dispatch priority. While priority and call type are not measured as they are subject to modification by dispatchers, the choice of call type and priority are also mechanisms that call-takers use to convey information.

other bystanders. An example of a concept like this is couching terms. Many aspects of a 911 call are reported by the caller. Call-takers sometime note that the "caller states" a specific fact. Compared to just stating the fact, noting that the caller is stating a fact is a way to minimize the likelihood or importance of a fact.

In calltaker/dispatch shorthand race and gender of individuals is coded as `w` for white, `l` for Latino, and `b` for black. Gender is expressed as `m` for male and `f` for female. These can be expressed with a range of spacing and punctuation, including for instance `wf` or `w/f` or `w. f.` for white females. To be as comprehensive as possible in identifying race in call-taker note records, we perform a regular expression extraction for all race identifiers including: "Black Male, Black Female, African-American, Latino Male, Latino Female, Hispanic, Mexican." The author is limited to capture race as a coupled statement with gender for "Black" and "White" race as some comments may state "Black" in contexts other than race, such as "Black SUV". Similar to race extractions, the author extracts gender through regular expression techniques extending acronyms to "Male, Female" labels. Gender characterization extractions will be used to further consider heterogeneity in treatment effects.

1.3.3 Descriptive Statistics

Table 1 presents descriptive statistics of the data. We break apart statistics across calls where race is mentioned. Panel A categorization supports the notion that call-taker characteristics such as average calls answered within a year, total months worked, and percentage of shifts worked are similar across calls where different races are mentioned. The similar composition of covariate characteristics across race mention categories suggest that call-takers who mention Black race look similar on observables to call-takers who mention White race. Among calls where Black, White, Latino or any race was mentioned, call-takers worked 49 months on average, with 26-42% of calls answered during the night shift, 34-39% during the mid-day shift and 19-21% in the morning shift where any race was mentioned. Call-takers

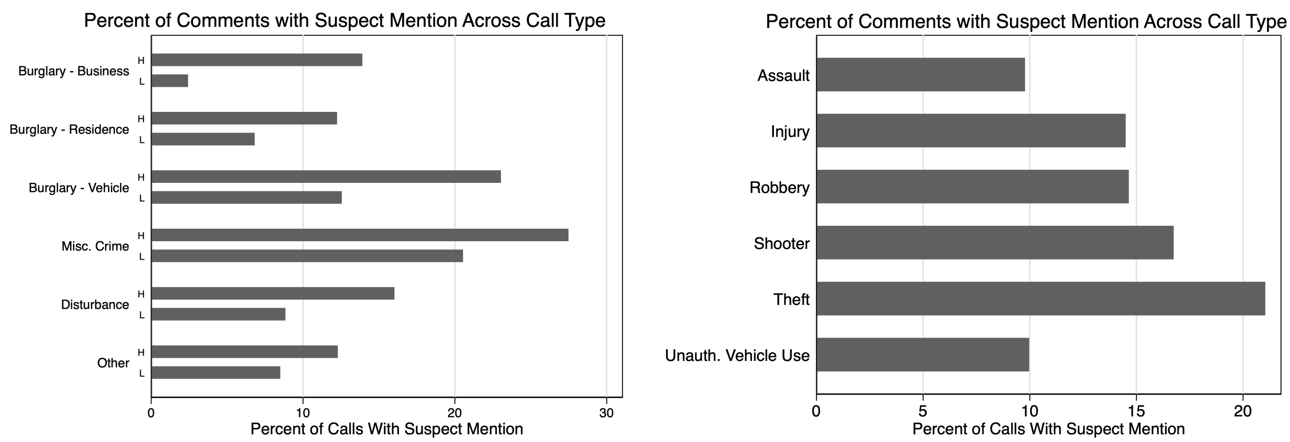
mention each race similarly within morning, mid-day and night shifts, with mid-day shifts having the largest deviation across race mentions. Panel B breaks apart race mention on call characteristics such as number of officers that respond to a call for service, total comments relayed to an officer by call-taker and dispatcher prior to officer arrival to service call, number of non-stop words relayed in the call-taker's first comment and whether the call was made on a weekend, holiday, during dark hours. All call characteristics look similar across calls where different races are mentioned, for instance the total non-stop words mentioned in the initial comment relayed by a call-taker is 21.48 on average for the total sample and 26-28 words across calls where race is mentioned. Panel C describes race mention across call for service categories. Similar to other call-characteristics, call problem types look similar across race mention except for in burglary of motor vehicle and theft, yielding a 5.2% and 1.3% increase in White race mentions compared to Black race mentions within this call type, respectively. Finally, Panel D concludes the descriptive statistics by measuring outcomes of interest, officer force use and arrest across race mentions. Officer force use is more frequent in calls where Latino is mentioned compared to calls where other races are mentioned while arrests occur most frequently when White race is mentioned. Overall, calls where any race is mentioned result in greater instances of arrest and force use compared to calls where no race is mentioned. This would suggest that race is elicited on calls for service that are deemed more serious. This notion is supported in figure 1(a), depicting that percentage of comments with suspect mention are concentrated within high priority calls.

Figure 1 summarizes percent of comments where the phrase suspect is mentioned across the uncensored sample. Namely, the figure contains two panels depicting (a) the percent of comments with suspect mentions across call types that are further broken down by priority level and (b) the percent of comments with suspect mentions across call types that are not broken down by priority level. Priority level is defined as "high" versus "low" priority for business burglary, residence burglary, vehicle burglary, criminal mischief, disturbance and

the bin for "other" call types. Panel (a) showcases that the percent of comments mentioning suspects is relatively larger for high priority versus low priority calls. This is likely in part due to the nature of call priority being correlated with whether the call is currently or recently in progress versus an after the fact call. Panel (b) showcases the percent of comments with a suspect mention on call types that are not broken apart by call-priority within the data. Given that the median percent of times a suspect is mentioned across all call-types is 16%, the author omits call-types with less than a 10% suspect mention. This censoring is completed one more time within the cleaned version of the data [11].

Figure 1: Percent of Comments with Suspect Mention Across Call Types

(a) (b)



[11] The cleaned version of the data refers to the removal of notes recorded after the call was dispatched.

Table 1: Descriptive Statistics of Call Characteristics by Race Mention

	Full Sample	Black Mentioned	Latino Mentioned	White Mentioned	No Race Mentioned
Panel A. Call-taker Characteristics					
Calls per Year	352,206	351,525	352,035	352,932	351,925
Months on Duty	49.7	49.6	50.63	49.40	49.75
Morning Shift	0.212	0.190	0.203	0.191	0.227
Mid-Day Shift	0.344	0.378	0.336	0.391	0.322
Night Shift	0.444	0.432	0.461	0.418	0.451
Panel B. Call Characteristics					
Number of Officers Responding	2.284	2.481	2.491	2.335	2.148
Comments Pre Officer Arrival	2.507	2.672	2.761	2.520	2.385
Non-Stop Words	21.48	26.57	27.94	27.55	16.93
Weekend	0.305	0.296	0.303	0.301	0.311
Holiday	0.116	0.115	0.115	0.113	0.118
Darkness	0.469	0.450	0.465	0.437	0.484
Panel C. Call for Service Reason					
Assault	0.00432	0.00411	0.00366	0.00493	0.00202
Burglary Motor Vehicle	0.0377	0.00842	0.0157	0.0600	0.00881
Miscellaneous Crime	0.151	0.178	0.191	0.117	0.235
Disturbance	0.390	0.436	0.416	0.376	0.326
Injury	0.00998	0.00955	0.0115	0.0103	0.00783
Other Crime	0.320	0.287	0.297	0.329	0.380
Disturbance	0.0213	0.0261	0.0194	0.0217	0.00845
Shooter	0.0146	0.0236	0.0213	0.0103	0.00855
Theft	0.0294	0.0236	0.0195	0.0357	0.0186
Use of Unauthorized Vehicle	0.0212	0.00366	0.00582	0.0348	0.00462
Panel D. Outcomes					
Force Used	0.00192	0.00265	0.00268	0.00257	0.00133
Arrest	0.0185	0.0245	0.0279	0.0293	0.0123
Observations	2,104,538	536,019	204,721	201,504	1,162,294

Note: This table presents average characteristics about the data broken apart by calls where race is mentioned. Total comments refers to number of total notes recorded per incident and total words represents the number of non-stop words in the first note recorded by the call-taker.

The following section discusses the motivation and construction of the instrument utilized for analysis.

1.4 Identification Strategy

The DPD Dispatch center is an ideal location to study call-taker race mention priming effects since it is the central office receiving calls for service in the Dallas area. There are anywhere from 30-50 call-takers available on a given shift. The morning shift spans from 7:00AM-3:00PM, while the evening shift is from 3:00PM-12:00AM and the night shift is 12:00AM-7:00AM. Call-takers rotate shifts throughout their tenure allowing exposure to different types of incidents. Subsequently, this allows propensities to be comparable across

call-takers. Evidence for call randomization is shown in Figure 2, depicting that baseline call characteristics are insignificant to call-taker race mention propensity. Baseline call characteristics in the main results are limited to include officer division to condition for officer availability and census block group fixed effects to condition for correlation of calls coming from similar locations.[12] Additional baseline characteristics considered in randomization tests include proportion of block group that has less than high school education, whether the call occurred during dark hours, rush hour, season of year, and the number of comments relayed by the dispatcher prior to the officer arriving to the scene.

This jackknife instrumental variable design builds on a strong literature estimating the effect of judge leniency on future incarceration rates (Aizer and Doyle Jr, 2015; Kling, 2006). Similar to instruments implemented in previous literature, this empirical strategy leverages variation in call-takers' race mention propensity. The research design is described in detail in the following section.

1.4.1 Research Design

The following model specifies the instrument construction and estimation approach. We focus on mentions of Black, Hispanic and White mentions in all estimations since these races are mentioned at the highest frequency within the data. [13] If in call i a $Black$, $Latino$ or $White$ race is mentioned by call-taker j then $Race\ Mention$ is in unity, if none of these races are mentioned then $Race\ Mention$ is zero. Consider the following model that describes the relationship of race mention on downstream calls for service outcomes:

$$Y_i = \beta_0 + \beta_1 \mathbb{1}(Race\ Mention)_{ij} + \beta_2 \mathbf{X}_i + \epsilon_{ij} \tag{1}$$

[12]These include the least number of fixed effects and controls to achieve randomization.

[13]Though understanding the impacts of call-taker mention of Asian American, Native American, Arab, Indian and other races is important, mentions of these races occur at such low frequencies that drawing an inference given this data set would be unlikely. This is likely due to the nature of the racial distribution in Dallas Texas during this time period.

where Y represents considered outcomes arrest and/or officer use of force for call i, X_{it} refers to a vector of baseline controls for call i on day t, ϵ_{ijt} is an error term [14]. The ordinary least squares estimation in equation (1) presents an endogenous relationship between call for service outcomes and race mention in call i. Particularly, bias in examining this relationship can arise from correlations in race mention with unobserved call characteristics that are correlated with calls for service outcomes. To contextualize the issue of bias in OLS, call-takers may use their discretion to elicit race on calls they believe yield a greater threat to public safety which would make the call have a higher disposition for arrest and force use by the officer. An example is a call for service regarding an active shooter, call-takers may inherently have a higher likelihood to elicit race with a specific menu due to the call being time sensitive and a great public safety risk, while simultaneously this type of call would have a higher probability of arrest or use of force given the call-type and severity. In this example, OLS would find that race mention substantially increased the probability of arrest and force use to occur. Alternatively, if these high emergent calls occur at lower frequencies compared to other call-types in the data, it is possible for call-takers race elicitation to be correlated with lower rates of arrest and force use. In this example, OLS would find that race mention substantially decreases the probability of arrest and force use to occur. Such correlation in outcomes and call-taker discretion make simple OLS endogenous and biased suggesting the use of an identification strategy that leverages exogenous variation. I exploit variation in call-taker propensity to mention race across past calls and future calls relative to calls answered in the current day. Using the random assignment of calls for service to call-takers, the author estimates the causal effect of call-taker race mention propensity on call for service outcomes. We can interpret differences in calls for service outcomes for calls assigned to call-takers with more or less of a race mention propensity as the causal effect

[14]Baseline controls are depicted in descriptive statistics and include variables such as census block and officer division.

of the change in probability in race mention associated with the call-taker assigned to the call. Through this estimation strategy, I estimate the local average treatment effect (LATE) as the causal effect of call-taker race mention for calls that were selected by call-takers with high proclivity to mention race at the margin.

Instrumental Variable Calculation. Previous research has established a precedent for the use of jacknife, or leave-out, instrumental variable strategy when the number of fixed-effects (call-takers) is likely to increase with sample size (calls for service) (Dobbie et al., 2018). To remain consistent in the 911 context, I omit the current week's worth of calls for a specific call-taker when calculating the instrument to deal with bias. I make the modification of omitting a week worth of calls for that call-taker in the instrument construction since the dispatch setting varies from the settings of other leave-out papers where the volume of calls received by call-takers is far larger compared to the number of cases a judge deliberates on in a year (Dobbie et al., 2018). I follow suit in Dahl et al., (2014) to residualize the instrument for time fixed effects, year by season by day of week by call-taker-shift. In doing so, the author aims to account for call-taker preference to substitute for shifts during a specific time of day or shift. This is pertinent as various calls for service occur more often during different segments of the day. For instance auto theft and DUIs occur disproportionately during evening and night shifts compared to day shifts. Residualizing the instrument helps account for these structural differences in call-taker availability[15].

Let the residual action of call-taker race mention after removing time fixed effects be as follows:

$$Race\ Mention^{*}_{jT} = Race\ Mention_i - \tau_T = Z_{jT} + \epsilon_{jiT} \tag{2}$$

where τ_T are the time fixed effects described above and the residual $Race\ Mention^{*}$ decision is represented by call-taker j's race mention propensity, Z_{jT}, flexible to change across years

[15]Call-taker propensity is also made to be flexible across a call-taker's years of service. This allows for the nature of call-taker's race mention proclivity to partially be explained by their experience

T, plus the idiosyncratic call level variation ϵ_{jiT}. Below describes the construction of the leave-out mean for each call-taker j in month T using the residual $Race\ Mention_{jT}$ decision.

$$Z_{jm} = \left(\frac{1}{n_{jT} - n_{jt}}\right) \left(\sum_{k=0}^{n_{Tj}} (Race\ Mention^{*}_{jkT}) - \sum_{k=0}^{n_{tj}} (Race\ Mention^{*}_{jkt})\right) \tag{3}$$

where n_{Tj} is the total number of calls call-taker j answers in year T and n_{tj} represents the number of calls the call-taker j answers in the current week t. The leave-out mean removes the residualized call-taker race mention propensity decision for all calls received by the call-taker in the current week across calls within a given year.

The call-taker leave-out measure in equation (3) can be interpreted as the race mention rate for the call-taker answering a service call after attributing for year by season by day of week by shift time fixed effects. Leaving out the current week's worth of calls is critical to avoid bias in regressing outcomes of call-for service i on call-taker j's race mention propensity. Omitting calls answered by call-takers within the current week is a more conservative estimation approach compared to omitting the current call [16]. If the author were to estimate residualized call-taker race mention propensity without omitting the current week's worth of calls, the same estimation errors would result on both left and right hand side of the regression producing biased estimates on the causal effect of race being mentioned in call-taker's initial comment. The two-stage least squares results use this predicted call-taker race mention propensity, Z_{jT}, as the instrument for whether the call had a specific race mention occur in the call.Preferred specifications include all call types as listed in Table 1 descriptive statistics[17] [18].

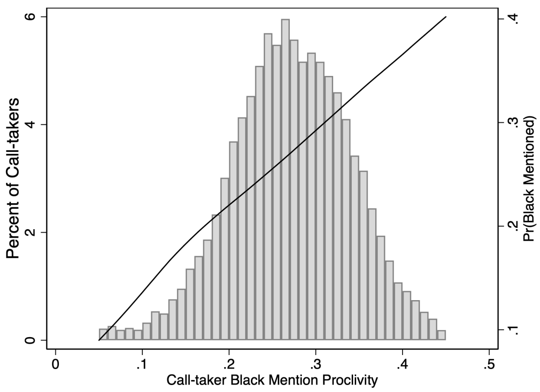
[16]Given scenarios where the call-taker's current day's interactions with callers influences interactions in the following day's calls, a week exclusion allows a longer buffer window to be realized

[17]Calls that are considered to have a lower likelihood of having a suspect such as call-types labelled as accident are omitted from the analytical sample.

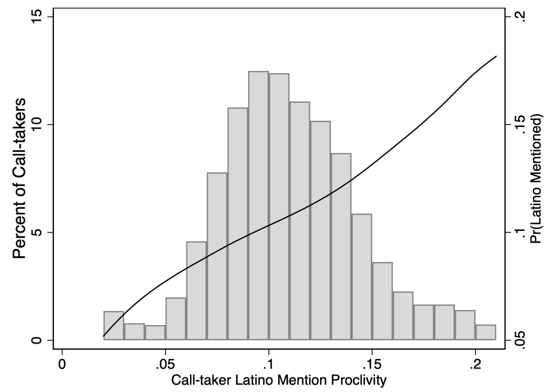
[18]For less than 2% of the data, the call-taker and dispatcher are reported to be the same individual. Accounting for dispatcher fixed effects also accounts for variation in dispatcher's discretion to report more or less of a call-taker's transcribed notes, this is not included in the main specification to not avoid introducing endogeneity for instances where dispatchers with more experience are selecting to dispatch officers to crimes

Figure 2: Distribution of Call-taker Propensity and First Stage

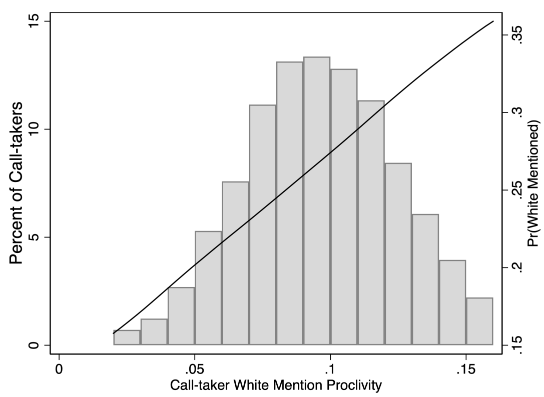
(a) Black Race Mention



(b) Latino Race Mention



(c) White Race Mention



Note: This figure reports the distribution of call-taker propensities to mention Black race (a), Latino race (b), and White race (c) across calls answered within the year. A flexible local polynomial is used to plot first stage coefficients of all-taker race mention propensity on the likelihood of that race being mentioned on the call.

Call-taker Variation Figure 1 presents the distribution of residualized call-taker's race mention propensities at the call-taker by year level. Propensities are residualized with time fixed effects and re-centered at the mean by adding in the mean predicted coefficient from each instrument construction. Panel (a) presents call-taker's propensity to mention Black race where the mean Black race mention propensity is approximately 0.27 and approximately 6% of call-takers mention Black race at the mean. Panels (b-d) can be interpreted similarly where approximately 12% of call-takers mention Latino race in 10% of their calls, approximately 15% of call-takers mention White race in 8% of calls answered within the year. This sample includes 673 number of call-takers throughout the sample period of 2013-2019 censoring out call-takers spending less than 7 months on duty, results do not change when maintaining all call-takers in the sample.[19]

Call-taker race mention propensity varies due to call-taker idiosyncratic preferences. I leverage this variation by instrumenting for race mention to identify the local average treatment effect of race mention on calls where initial race mention occurs at the margin based on call-taker assignment. The conditional assumptions to interpret results as local average treatment effects are relevance, call-taker assignment is related to race mention; validity, call-taker assignment only impacts call for service outcomes through the probability of race being mentioned; monotonicity, or calls where race is mentioned by a call-taker with a low propensity to mention race would also have race mentioned if the call had been assigned to a call-taker with a high race mention propensity. The next section of the paper explores the context in which these assumptions hold in the Dallas dispatch center setting.

First Stage. To establish relevance in the instrumental variable strategy, the author examines the strength of the relationship between call-taker race mention propensities and

of the highest severity, such as calls where a shooter is involved.

[19]It is pertinent to omit call-takers with less experience as to not misattribute results to lack of experience. If call-takers are systematically mentioning race more or less at the beginning of their tenure due to lack of training or recent training, results may be biased upwards or downwards.

whether race was mentioned on a call for service. To establish this relationship, equation (4) for call i and call-taker j is estimated using a linear probability model:

$$Race\ Mention_i = \alpha_0 + \alpha_1 Z_{jy} + \alpha_2 \mathbf{X}_i + \alpha_3 \tau_T + \epsilon_{ijTy} \tag{4}$$

where the vector $\mathbf{X}_i$ refers to baseline call characteristics for call i such as the division of the reporting officer and census block group fixed effects where the call was made. Time fixed effects attributed for in the residualization process are also included in τ_T attributing for year by season by day of week by shift fixed effects. As previously discussed, Z_{jy} is the jacknife instrument leaving out calls answered by the call-taker within the given week and made flexible across years. Robust standard errors are clustered at the call-taker level. Overlaid the distribution of call-taker race mention propensities in Figure 1 is a graphical representation of the first stage relationship between residualized call-taker race mention propensity and probability of race mention after controlling for time fixed effects. This plot is estimated using a local linear regression as described in equation (4) and presents call-taker race mention to be monotonically increasing in a linear fashion as call-taker race mention propensity increases. Table 2 presents first stage estimates from equation (4). Each column represents an estimation of any race being mentioned on the propensity to mention White, Hispanic or Black race[20]. Columns (1), (3) and (5) include time fixed effects, while Columns (2) (4) and (6) present the preferred specification including additional call-level fixed effects such as reporting officer division and census block fixed effects. Columns (1-2) omit the current week's worth of calls whereas Columns (3-4) presents the first stage when only the current call observation is omitted and columns (5-6) present first stage estimates where no observations are omitted when calculating call-taker proclivity to mention race.

Similar to Figure 1, race mention propensities exhibit a strong first stage signifying that

[20] Any race mention is defined as White, Black or Latino being mentioned in the call.

the residualized call-taker instrument is highly predictive of whether the initial call-taker notes indicate a race being mentioned. Attributing for additional characteristics such as division of reporting officer(s) and census block does not significantly impact the magnitude or direction of the estimates. Likewise, adding baseline call characteristic controls does not significantly alter the magnitude of estimates. The fully saturated model suggests calls for service randomly assigned to a residualized call-taker that is 10 percentage points more likely to mention Black race, Latino race and White race, results in 9.8, 9.9, and 10, respectively, percentage point increase in the probability of race being mentioned on the call. The probability at which race is mentioned on a call for service is increasing approximately at the same rate as call-taker race mention propensity when not omitting any observations, 10.1%. This is likely attributed to the fact that call-takers receive a similar volume of calls across each problem-type after residualizing for time fixed effects. Below each call-taker race mention propensity estimate are $F - statistics$ on the instrument. Lee et al. (2021) suggest an F-statistic deemed relevant for a model should exceed 104.7. Each of the estimated instrument F-statistics exceed this threshold with ease providing evidence that call-taker race mention propensities are relevant in their relationship to whether race is mentioned on a call for service.

Instrument Validity. To interpret the two-stage least squares estimates as the local average treatment effect of call-taker race mention, two additional assumptions must be satisfied: exclusion restriction and monotonicity. For the exclusion restriction to be met, call-taker random assignment to service calls should only impact call for service outcomes through the probability of race to be mentioned. Figure 2 plots the coefficient from regressions estimating 1)race mention on a service call on call characteristics and 2)call-taker race mention proclivity on call characteristics. Panel A presents results for the Black Race mention category, Panel B presents Latino Race Mention Propensity and Panel C presents White Race Mention Propensity. Estimates from the endogenous relationship of call-taker race mention

Table 2: Call-taker Race Mention Proclivity and Race Mention in Calls for Service

	Dependent Variable:					
	Any Race Mention		Any Race Mention		Any Race Mention	
	(1)	(2)	(3)	(4)	(5)	(6)
Black Race Mention Proclivity	0.981***	0.983***	0.992***	0.995***	1.009***	1.012***
	(0.00867)	(0.00911)	(0.00832)	(0.00880)	(0.00690)	(0.00765)
Mean of Outcome	0.270	0.270	0.270	0.270	0.270	0.270
Latino Race Mention Proclivity	0.910***	0.922***	0.913***	0.925***	0.916***	0.928***
	(0.00864)	(0.00879)	(0.00824)	(0.00841)	(0.00780)	(0.00799)
Mean of Outcome	0.102	0.102	0.102	0.102	0.102	0.102
White Race Mention Proclivity	0.794***	0.780***	0.790***	0.775***	0.798***	0.780***
	(0.0170)	(0.0177)	(0.0181)	(0.0191)	(0.0167)	(0.0183)
Observations	2,104,538	2,040,796	2,104,538	2,040,796	2,104,538	2,040,796
Mean of Outcome	0.100	0.100	0.100	0.100	0.100	0.100
F-Statistic	866.15	-	876.02	-	915.44	-
Fixed Effects						
Time FE	Y	Y	Y	Y	Y	Y
Block Group & Division FE	-	Y	-	Y	-	Y

Note: The following table presents first stage estimates on whether race was mentioned on a certain call on assigned call-taker race mention proclivity. Columns 1, 3 and 5 include the baseline specification of time fixed effects: year by season by day of week by call-taker shift. While columns 2, 4 and 6 account for census block group and the assigned officer's division. Columns (1-2) omit the current week's worth of data relative to the current call for that call-taker, columns (3-4) omit the current call only and columns (5-6) do not omit any data when constructing the instrument. Standard errors are clustered at the call-taker level and reported below each estimate in parentheses.

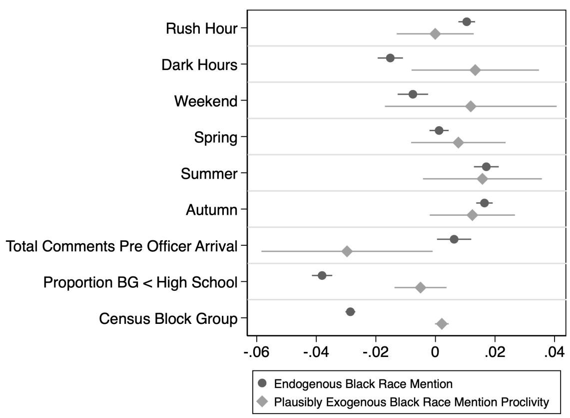
in a given call is presented by dark grey circles indicating statistical significance on a variety of call characteristics while the diamond coefficients present the exogenous call taker race mention proclivity yielding statistically insignificant estimates. These results further support the necessity to leverage exogenous variation in call-taker proclivity compared to endogenous race mention on a particular call, suggesting satisfaction of the exclusion restriction.

Though tests for randomization support call-taker race mention propensities as unrelated to observed call-characteristics, the exclusion restriction assumption is untestable. The nature of this exclusion restriction being untestable is caused by the possibility of unobserved channels impacting call for service outcomes other than call-taker race mention decisions. Some of the channels we consider are the amount of information that is relayed to an officer. We believe that ambiguity is fostered when there are not many comments relayed prior to an officer's arrival increasing the strength of race mention priming compared to instances where a large volume of information is relayed. If the exclusion restriction is violated, call-taker race mention propensity influences downstream call for service outcomes through a channel other than race mention, interpretation of LATE would include the additional effects associated with call-taker assignment. However, we believe the Dallas 911 call-taker setting is ideal for the exclusion restriction to hold since call-takers only role is to answer incoming calls at random, note down pertinent information from the caller and designate a category to the problem type. All subsequent follow-up interactions with the call for service are assigned through processes unrelated to call-taker random selection of incoming calls. Dispatchers select a call for service at random in their computerized queue where the call is then assigned using a CAD system to an officer available in the area[21]. These institutional details of the 911

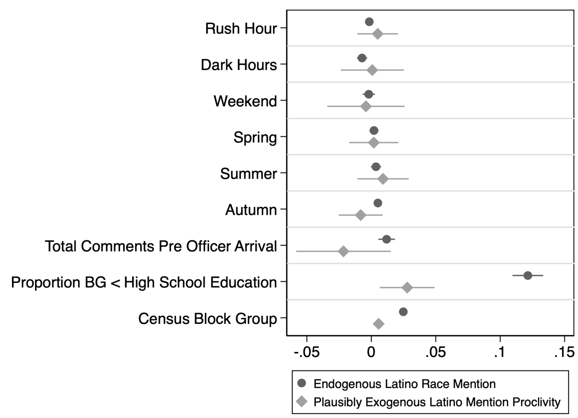
[21] We believe there to be certain serious call-types, such as active shooter, that merit either more experienced dispatchers or the chief dispatcher to address the call. Such serious events are less frequent in this sample occurring 0.97 percent of times compared to other call types. As discussed in section II officers currently servicing a call for service can radio in to dispatchers to assign themselves to a serious call if the new call has higher priority and they are located closest to the incident. We estimate officers moving from one active service call to another to be an infrequent event.

Figure 3: Test of Randomization

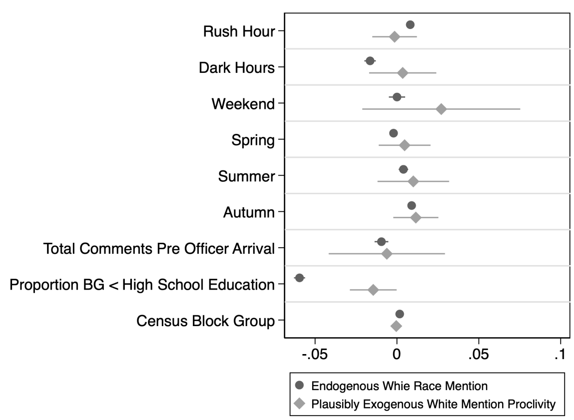
(a) Black Race Mention



(b) Latino Race Mention



(c) White Race Mention



Note: This figure reports the coefficients of the endogenous call-taker mention on call and call-taker characteristics compared to coefficients of the plausibly exogenous call-taker race mention instrument on call and call-taker characteristics.

call for service procedure suggest that call-taker random selection of a service call is unlikely correlated to dispatcher and officer assignment to the call [22] Another point of contention in supporting the exclusion restriction is that assigned call-takers only influence call for service outcome through the act of mentioning race. To test if call-taker descriptiveness is priming officer behavior I control for number of total comments a call-taker shares prior to the officer arriving to the scene, as noted in Figure 3, which is uncorrelated to the plausibly exogenous instrument. In essence this finding suggests that the race mention propensity is not acting as a proxy for call-takers being more descriptive in their dissemination of information.

In the case of the exclusion restriction to be violated, we can interpret the reduced form estimates as the causal effect of being assigned to a call-taker with higher or lower race mention propensity. The magnitude and sign of these results follow similarly with all two stage least squares estimates supporting that either two-stage least square or reduced form estimate interpretation would yield similar conclusions. These reduced form results can be found in Table 3 alongside OLS and 2SLS results.

If the exclusion restriction holds up, the monotonicity assumption must hold for the two stage least squares estimates to be interpreted as LATE. In the Dallas 911 call for service setting, the monotonicity assumption holds under two conditions. First, if a call-taker with low race mention propensity mentions race on a call for service, race would have also been mentioned if a call-taker with high race mention propensity answered the call and second if race is not mentioned for a call for service by a call-taker with high race mention propensity then race would not have been mentioned if the call had been answered by a call-taker with low race mention propensity. For the two-stage least square estimates to be interpreted as LATE, monotonicity must not be violated. If the monotonicity assumption does not hold, the two-stage least square estimates can be interpreted as the weighted average of marginal treatment effects (Angrist et al., 1996; Aakvik et al., 2005; Dobbie et al., 2018).

[22] Call-takers are the same individual as the dispatcher for 0.63 percent of calls in this sample.

There are a few ways to test the different implications of the monotonicity assumption holding. One way is to ensure that the first stage estimates are non-negative for all subsamples. We test this by plotting the first stage relationship of race mention to call-taker race propensity within different call type categories and find across all specifications a non-negative relationship with considerable magnitude supporting the monotonicity assumption.

Understanding the LATE. The following table below present the two-stage least squares estimate for arrest and force use. Each column presents specifications of call-taker race mention instrumented by call-taker race mention proclivity on the probability of an outcome to occur measured through a linear probability model. The estimates represent the local average treatment effect (LATE) for calls for service that would have received a different race mention decision had the call been answered by another call-taker.

1.5 Results

The following section describes the main results for the effect of call-taker race mention on call for service outcomes. We compare the endogeneous relationship between race mention in a specific service call (columns 1-2) with the plausibly exogeneous call-taker IV (columns 3-6). OLS estimates suggest that mentions of White, Black or Latino race statistically increases the likelihood for arrest and force use. Conversely, 2SLS and reduced form estimates have flipped signs indicating a decrease in arrests and force use for call-takers with high proclivities to mention Black and Hispanic race. The likelihood for arrests is still increasing for calls where call-takers have higher proclivity to mention White race [23] The percentage point increase for arrests and force are relatively small due to the nature of these events being rare events in the sample. To gain a comprehensive understanding regarding the magnitude of these estimates we interpret them with respect to the percentage change from their respective means. The

[23] For robustness, we link call-records to arrest records that indicate the race of suspect arrested to back out what percent of calls where race is mentioned have the same race associated with the arrest record as referenced in the methods section.

mean for each outcome is reported below each of the separate race mention estimates, for example in Table 3 Panel A the outcome mean refers to the average number of calls where arrest occurred. By evaluating the magnitude of estimates relative to the mean, one is able to interpret the estimates through a relevant and meaningful lens.

Tables 3-4 presents linear probability models results on the impact of race being mentioned on the likelihood for officer arrest and force use behavior. Panel A presents the endogenous ordinary least square estimates on the likelihood for an outcome to occur if Black, Latino or White race was mentioned. Panel B estimates reduced form estimates, if one fails to believe the exclusion restriction is met, we can interpret reduced form estimates as the local average treatment effect (LATE) of a call being marginally assigned to call-takers with higher propensity to mention race. Panel C estimates two-stage least squares (2SLS) estimates where any race mention is instrumented by call-taker's Black mention, Latino mention and White mention proclivities. Each OLS race mention specification indicates a moderate to strong effect, though the relationship is mis-attributed due to correlation of race mention with other call characteristics. In columns 3-6, 2SLS and reduced form estimates present strong results in the other direction for Black and Latino races suggesting that being assigned to call-takers with high race mention proclivities at the margin is associated with less arrests and force use. We observe a larger magnitude in effects across reduced form and 2SLS estimates by leveraging the plausibly exogeneous call-taker race mention propensity instrument.

Columns 1, 3 and 5 present the baseline specification including time fixed effects required to account for temporal differences in call-types coming in the door. To ensure call-takers have an equal probability to be exposed to similar types of calls we account for year by season by day of week by shift time fixed effects. These are the same fixed effects used to residualize the data prior to constructing the instrument, we follow suit in Dobbie et al. (2018) when conducting this procedure. Columns 2, 4 and 6 contain additional fixed effects for reporting

officer division and census block group fixed effects. Officer-division fixed effects help account for varying number of officers available to service incoming calls within a division the call was made in. Census block group fixed effects help account for correlation in calls coming from the same location on call-taker preference to elicit and relay information in calls coming from these specific census blocks in a specified manner.

Table 3: Effect of Call-taker Black, Latino and White Race Mention Proclivity on Arrests

	OLS		Reduced Form		2SLS	
	(1)	(2)	(3)	(4)	(5)	(6)
Black Race	0.0105***	0.0106***	-0.0257***	-0.0259***	-0.0332***	-0.0334***
	(0.000402)	(0.000409)	(0.00514)	(0.00519)	(0.00677)	(0.00699)
Latino Race	0.0138***	0.0140***	-0.00896*	-0.00899*	-0.00799	-0.00829
	(0.000689)	(0.000702)	(0.00534)	(0.00528)	(0.00544)	(0.00545)
White Race	0.0147***	0.0137***	0.0385***	0.0377***	0.0553***	0.0563***
	(0.000648)	(0.000609)	(0.00799)	(0.00791)	(0.0124)	(0.0134)
Observations	2,104,538	2,040,796	2,104,538	2,040,796	2,104,538	2,040,796
Mean of Outcome	0.019	0.019	0.019	0.019	0.019	0.019
Fixed Effects						
Time FE	Y	Y	Y	Y	Y	Y
Block Group & Division	-	Y	-	Y	-	Y

Note: The following table reports least squares, reduced form and two stage least square estimates of call-taker's Black, Latino and White race mention proclivities on the likelihood of arrest. Call-taker proclivities are flexible at the year level with a leave-out on the current week's worth of calls for that call-taker. Column (1), (3) and (5) report coefficients with time fixed effects including year by season by day of week by shift. Columns (2), (4) and (6) include time, block group and division fixed effects. Standard errors are clustered at the call-taker level and reported below estimates in parentheses.

Reasons why service calls result in arrests are rarely straightforward, typically involving complex decision-making and processing of information by the officer. The current estimation strategy does not claim to disentangle this complex officer decision process but rather captures the effect of race mention on calls where race mention occurs at the margin. Table 3 presents OLS, 2SLS and reduced form results on the effect of call-taker race mention and race mention propensity on the probability for an arrest to occur. OLS estimates in columns 1-2 indicate that any race being mentioned on a service call significantly increases the likelihood for an arrest to occur. For example in Column 1, with the sole inclusion of time fixed

effects, estimates indicate that Black race mention is associated with a 1.05% increase in the likelihood of arrest, Latino race mention is associated with a 1.4% increase in the likelihood of arrest and White race mention is associated with a 1.5% increase in the likelihood of arrest compared to calls where no race is mentioned. These results suggest an increase in arrests at an order of 55.3%, 72.6% and 77.4% increase in the mean of arrest, respectively. In columns (3-4), reduced form estimates improve the reliability of this estimation by leveraging exogenously random variation in call-taker race mention propensity that can be interpreted on calls where assignment to call-takers with high race mention propensity occurred at the margin. The random assignment of call-takers to calls make it so observed and unobserved characteristics correlated to call outcomes and race mention are no longer being mis-attributed in observed estimates. Estimates in columns (3-6) suggest a reversal in sign, assignment to call-takers with higher race mention propensity is associated to a statistically significant decrease in likelihood for arrest, excluding Latino two stage least squares estimates which are insignificant. If we fail to believe in our exclusion restriction being met, we refer to reduced form estimates interpreted as the marginal assignment of a call to a call-taker with higher proclivity to mention race across calls answered within the year in columns (3-4). Estimates from column (4) depict estimates when assignment to a call-taker with high race mention occurs at the margin, a 2.6% and 0.9% reduction in the likelihood for arrests is realized for calls assigned to call-takers with high propensities to mention Black and Latino race, a 135% and 47% reduction in the mean for arrests, respectively, compared to calls assigned to call-takers with low to race mention propensity. Whereas for calls where assignment to a call-taker with high proclivity to mention White race occurs at the margin, the likelihood for arrests increases by 3.8%, a 202% increase in the mean of arrests compared to calls assigned to call-takers with low to no race mention proclivities.[24]. Columns

[24] In addition to no race, it is possible that the sample includes rare instances when other races not flagged for in this analysis are mentioned such as Asian American, Indian, Native American which are not easily identifiable.

(5-6) report similar and consistent results to reduced form estimates except reductions in the likelihood for arrest are not statistically significant for calls where Latino race mention occurs at the margin. In column (6), we can interpret the first estimate for when Black race being mentioned at the margin is associated with a reduction in arrests by 3.3% at the 0.01 significance level, a 173% decrease in the mean for arrests. A 5.6% increase in the likelihood of arrests is observed at the 0.01 significance level for calls where mentions of White race occur at the margin.

Table 4: Effect of Call-taker Black, Latino, and White Race Mention Proclivity on Force Use

	OLS		Reduced Form		2SLS	
	(1)	(2)	(3)	(4)	(5)	(6)
Black Race	0.00127***	0.00125***	-0.00209**	-0.00218**	-0.00224**	-0.00231**
	(8.22e-05)	(8.06e-05)	(0.000887)	(0.000906)	(0.00110)	(0.00113)
Latino Race	0.00117***	0.00118***	-0.000230	-0.000366	-0.000178	-0.000305
	(0.000115)	(0.000120)	(0.000832)	(0.000844)	(0.000851)	(0.000854)
White Race	0.00112***	0.00102***	-0.00113	-0.00138	-0.000897	-0.00123
	(0.000126)	(0.000128)	(0.00133)	(0.00137)	(0.00184)	(0.00197)
Observations	2,104,538	2,040,796	2,104,538	2,040,796	2,104,538	2,040,796
Mean of Outcome	0.0019	0.0019	0.0019	0.0019	0.0019	0.0019
Fixed Effects						
DOW x Shift x Year x Season	Y	Y	Y	Y	Y	Y
Block Group & Division	-	Y	-	Y	-	Y

Note: The following table reports least squares, reduced form and two stage least square estimates of call-taker's Black, Latino and White race mention proclivities on the likelihood of force use. Call-taker proclivities are flexible at the year level with a leave-out on the current week's worth of calls for that call-taker. Column (1), (3) and (5) report coefficients with time fixed effects including year by season by day of week by shift. Columns (2), (4) and (6) include time, block group and division fixed effects. Standard errors are clustered at the call-taker level and reported below estimates in parentheses.

Table 4 presents the same results shown in Table 3 but for the outcome of force use. Officers decision to use force is a complex process likely attributable to a variety of reasons and causes that differ from call to call. The current analysis does not capture all of the complexities involved in this decision but focuses on the variation spurred by call-taker's race mention propensity. Columns 1-2 estimate the effect of race mention on the probability of officer force use with linear probability models. Similar to arrest results, ordinary least

square estimates present an opposite sign finding from reduced form and two stage least square estimates suggesting that race mention increases the likelihood for force use when not accounting for endogeneity in correlations of race mention and other call characteristics. On average, the effect of race being mentioned in these estimates on a call is shown to significantly increase the probability of force use by 0.13%,0.11%,0.11%, a 66%,62% and 54% increase in the mean of force use respectively. Columns (3-4) present reduced form estimates interpreted as the marginal assignment of a call being assigned to a call-taker with high race mention proclivity. Column (4) estimates indicate the likelihood of force use decreases for calls where Black, Latino and White race is mentioned at the margin but is only statistically significant for Black race mentions at the 0.05 significance level, a 0.21% reduction or 1.1% decrease in the mean of force use compared to calls assigned to a call-taker with little to no race mention propensity. Columns (5-6) present 2SLS estimates that are similar in sign and magnitude to reduced form estimates. 2SLS estimates are interpreted through a causal lens as the marginal race mention instrumented by call-taker propensities. Column (6) estimates showcase a reduction in likelihood for force use by 0.23% for calls where Black race is mentioned at the margin, a 1.21% decrease in the mean of force use compared to calls where no race is mentioned.

If there is a belief that the exclusion restriction is violated, the reduced form estimates in columns (3-4) present the causal effect of being assigned to a call-taker with a higher propensity to mention race compared to a call-taker with lower propensity to mention race. All reduced form estimates are similar to the 2SLS estimates presented in columns (5-6) and are consistent with the first stage estimates on assigned call-taker propensity to mention race and call for service outcomes.

1.6 Discussion

This paper estimates the impact of race mention on call for service outcomes where race mention is a marginal occurrence. As previously noted, all of the explored outcomes are complex processes in the criminal justice system, the author does not intend to uncover the many complex processes associated with citizen-officer interactions but would like to note the direct effect of call-taker systematic preference to elicit specified suspect races on call for service outcomes. The probability of each outcome explored, arrest and force use increased drastically compared to calls where no race was mentioned in the endogenous ordinary least squares estimation. Applying the constructed plausibly exogenous leave-out instrument yielded opposing results, a decrease in arrests for calls where Black and Latino races are mentioned at the margin and an increase in arrests for calls where White race is mentioned at the margin relative to calls where no race is mentioned. Estimates on the likelihood of force use based on call-taker assignment and race mention yielded few statistically significant results when applying the instrument. This is likely due to the rarity of force use to occur in our sample period making it difficult to draw inferences for this outcome. Force use, similar to arrests showcased a sign flip when comparing least squares estimates to reduced form and two-stage least squares estimates with a statistically significant decrease in force use for calls where Black mention occurred at the margin. Reduced form estimates were consistent to all two stage least square estimates.

Drawing policy relevance in research similar to the current work is generally encouraged to gain insight on the generalizability and extension of this research for real world application, practitioners and future extensions to the current research. To remain cautious of over-extending the estimates from the current sample too drastically, I will discuss a few points on how estimating these results can help inform 911 call-centers. Primarily, as one of the primary functions of police officers is to service calls for service initiated by citizens, an equal treatment for each call coming in the door by call-takers is likely a desired outcome.

As there are inevitably idiosyncratic differences in call-taker preference to ask questions, elicit information and gain a response by means of varying tactics and varying success, we can focus less on attempting to standardize these idiosyncrasies and focus more on how some differential preferences in these practices related to race mention lead to inequitable treatment by officers as a function of calls leading to an arrest or force use more often. More precisely, if call-taker propensity to mention certain races more frequently than is the expected frequency of race mention across call-types, and this preference is resulting in more force use compared to call-takers that do not hold this preference, a call to enforce training or track these occurrences to help improve equitable outcomes can be considered. Given the results described in this research, the contrary is found to be true, race mention for minority race is associated with less force use to calls where call-taker's do not have high race mention proclivity. An extension to the current work would be to inquire and work closely with an agency to uncover other institutional reasons why call-takers with greater minority race mention proclivity result in less force and arrests. Provided the data observable within this current framework, the author does not find other call-taker facets correlated with race mention proclivity that would drive these results.

In all, the primary work of call-takers and 911 call centers is of great importance in maintaining social welfare and public safety within society, acting as the bridge between citizen emergency and reporting service members certainly is a complex job with various constraints. The following work sheds light to how idiosyncrasies in the considered "gate-keepers" of the criminal justice system (Gillooly, 2020a) impact officer discretion to arrest and use force when arriving for a call for service based on marginal race mention occurrence. This research contributes to existing literature on the role of 911 call centers on officer decision-making by being the first causal empirical analysis of such a large volume (2.1 million call-taker comment logs) via natural language processing to construct call-taker race mention propensity. Extensions to this work can consider the effect of race mention and

other call-characteristic mentions such as suspect behavior on other outcomes of interest when considering officer-citizen interactions such as citizen complaints on officers.

2 Detriments of the Legislative Pendulum: An Empirical Case Study of Proposition 57

Coauthored with Greg DeAngelo, Ryan Quandt and Minjae Yun

2.1 Introduction

The effect of legislation on criminal justice systems and their actors cannot be assumed. A popular line, championed by Stuntz (2011) and, more recently, Pfaff (2017), states that federal and state legislation often has limited effects on the practices and norms of police, courts, parole boards, and prisons.[1] Even when a law directly alters a system, agents may adjust to maintain past outcomes or, if outcomes change, they may return to prior levels soon after. Bjerk (2005) and Sutton (2013) argue that, for California's *Three Strikes laws*, prosecutors adjusted their behavior to preserve past outcomes or lessen the severity of punishment. Bjerk (2005) also shows that failing to account for potential adjustments in agents' behavior biases the estimates of a law's effect. Often, agents' decisions throughout the legal process are unobserved, some of which defy formal models, so omitted variables bias threatens analyses of criminal justice system reforms.[2] The effects of legislation depend on how agents adjust, if at all, and measuring anticipation of these adjustments are crucial to reach the intended policy outcomes.

Prosecutors are uniquely influential in the criminal justice system. They decide which cases to pursue or not file, which charges to level, plea agreements, and how much time and energy to spend on a case. Prosecutor decision-making can enable or block legal reform. But, as noted in Sutton (2013), the mechanism for the effect of legislation on prosecutor

[1]Lipsky (1980) argued for the important role of communities, institutions, and their practices much earlier.

[2]See, in the case of prosecutors, Wright et al. (2014). One case they mention is how certain district attorney's offices incentivize certain prosecutorial practices.

discretion is unknown. One reason for this gap has been inadequate data on prosecution. District Attorney's offices are complex institutions, which can have considerable differences across jurisdictions. From arrest to conviction, plea , or dismissal, a series of decisions occur that are often unobserved. To compensate, select decisions proxy prosecutor behavior, such as the decision to detain a defendant before trial Sutton (2013), the charge Bjerk (2005), the decision to go to trial Baker and Mezzetti (2001); Boylan (2012), whether the defendant plead guilty Reinganum (1988); Sutton (2013), the conviction Rasmusen et al. (2009), or the final sentence Kessler and Piehl (1998). The challenge is to isolate the causal effect of legislation on prosecutor behavior given observed and unobserved decisions made by a District Attorney's office.

A hypothetical model on practitioner behavior that may predict changes in prosecution brought on by legislation would help shed light on explaining past events, anticipate the results of future ones, and pinpoint a mechanism for effective legislation. Our hypothesis can be summarized as follows. District Attorney offices are supplied with cases submitted by local police departments. Given the role of district attorneys, the value of these cases depend on potential consequences for a defendant, harm being redressed, and public, office, or plaintiff interests. These elements are often positively correlated: the consequences of a crime are larger when the crime results in greater harm; wider interest in the crime often rises likewise. Prosecutors handle cases according to this value while constrained by time and resources. If correct, legislation that adjusts determinants of a case's value will affect prosecutors' calculus of which cases to prioritize. Importantly, legislation may not explicitly effect court outcomes yet adjust case values nonetheless if post-court outcomes change and prosecutors anticipate those changes. Our hypothesis is that prosecutors adjust their behavior relative to such outcomes. Three testable predictions result from our hypothesis that allow us to support or reject this hypothesis: (i) prosecutors seek the higher value for threshold cases;

(ii) prosecutors resolve lesser cases quickly, through not filing, dismissing, diverting [3], or disposing the case by plea; (iii) prosecutors adopt alternative consequences to offset any decrease in case value.

One debate in the literature on prosecutor discretion concerns whether prosecutors are aggressive or lenient. Some argue that since prosecutors have the most power over cases, they are a main driver for mass incarceration. Under this theory there is a belief that prosecutors maximize sentences or suggest charges[4] with the most certainty of conviction. And they often add that since crime has decreased yet prison populations rise, crimes or arrests are not driving prison growth. Prosecutors are the culprit, they continue, because the number of convictions relative to arrests and the average severity of charges increases while dismissals lessen. This view is nearly taken as self-evident in much of the literature.[5] Others argue for leniency, at least with respect to unduly harsh punishments (e.g., Three Strikes laws). Their argument centers on how certain crimes are handled, such that the composition of charges, dismissals, and convictions change, and how aggregation leads to misleading results. With more detail, censure of prosecution becomes less convincing. Still others find that many legislative changes are absorbed into office and court routine practices without long-run effects. Norms and expectations for how certain cases should be handled remain unchanged. Legislation alters how these norms surface. The argument goes that if a prosecutor can no longer charge a case with an expected effect, they will charge like cases differently to achieve a similar effect.

Previous research often notes that data on prosecution limits their studies so that prosecutor behavior may not be inferred from available sources. Many studies describe prosecution

[3] We do not currently observe diversion in our data, and so will not report changes in diversion below. We leave such analysis for future work.

[4] Prosecutors do not have discretion on the final set of charges of a case, rather charges and sentence length are negotiated and agreed upon through the course of a trial, in most cases the courts have a final determination on case charges and sentence lengths.

[5] See Bellin (2019) for extensive citations. He finds the claim that prosecutors are the most powerful actors in criminal justice to be general, vague, and dubious.

trends without causally identifying the effect of legislation on prosecution. But describing trends is inadequate given unobserved variables: offender's prior offense history, initial arresting information, filing or dismissing, district attorney's dismissal, assigned prosecutor, case loads, history of individual prosecutors, time spent building a case, initial charge, initial and resulting plea deal, and court dismissal. The lack on observable features in the data partially explains why scholars disagree on what prosecutors optimize (Boylan, 2005; Glaeser et al., 2000). The data are unable to accurately determine what prosecutors are attempting to optimize. Take the decision of the review prosecutor to file or dismiss a case. One of two outcomes occur: either the reviewer files or dismisses the case. As Bjerk (2005) has argued,[6] if the review prosecutor tends to file cases that may qualify as serious nonviolent felonies or violent while dismissing others, the average sentence length for nonviolent felonies may go up,[7] while the average sentence length for violent offenses decreases.[8] Someone may argue prosecutors are becoming more severe with nonviolent felonies and more lenient with violent ones, though that is not so. After the initial review prosecutor's decision to file, there is another review in which charges are decided.[9] In this second review, if the prosecutor charges certain cases that could be either violent or nonviolent within an offense category as violent and others as nonviolent, their behavior will go unobserved. They may not convict when charging more harshly, judges may adjust their sentencing accordingly, or plea deals may be easier with more severe (or less severe) charges. Changes in behavior of select agents will wash out in aggregate.

A main contribution of this paper is the use of microdata for a D.A. office in a large,

[6]Bjerk argues that, after California's severe Three Strikes law passed, prosecutors charged lesser, strikable offenses as more severe non-strike offenses, which increases how many lesser offenses are charged, increased their average sentence length (because more severe offenses are being charged as the lesser offense), and increases the average sentence length of strikable offenses (since there are less mild offenses being charged as such).

[7]Since the review prosecutor only files the more severe ones.

[8]More lesser violent felonies bring down the average.

[9]After the district attorney assigns the case to a horizontal or vertical review board.

southwestern county to test a theory for prosecutor valuation of cases. On this theory, prosecutors optimize their time and resources to resolve as many valuable cases as possible. A Difference-in-Differences causal model is used to test this theory by leveraging a legislative shock to case value. One advantage of using this legislation is that it did not explicitly or formally alter prosecution. Laws and formal sentencing held, but a crime's actual consequences altered. Our results support the hypothesis that prosecutors adjust their behavior to the expected consequences of a conviction, not merely the formal sentence. As a result of legislation, prosecutors reallocate their resources. However, our analysis is restricted to one large D.A. office during a four year time frame. Future work should test this theory across offices, states, and periods, and with different shocks and causal models.

2.2 Background & Case

Over the last decade in the United States, criminal justice systems have again come under scrutiny. Not only have their been public calls for reappraisal but for sweeping reforms and even dissolution.[10] Many (not all) oppose the "tough on crime" mentality of past decades. Federal, state, and local governments have responded with proposals and legislative changes, which continue to the present. Reducing recidivism, lowering prison populations, improving rehabilitation, and sentencing reform—all while maintaining public safety and rule of law—are common motifs in public discourse and legislation as policy-makers and institutional leaders act for a more just society. Long-term effects of these reforms remain to be seen, but early effects can be tracked. State-based reforms, especially, help us see which efforts merit replication or expansion.

Californians have witnessed and enacted many vanguard legislative changes, which have drawn scholarly attention. These reforms were prompted by a federal court order in 2011 (*Brown v. Plata*) to lower prison populations by roughly 40,000 persons (bringing prison

[10] Including, recently, popular criticism of prosecutors (Bazelon (2019)).

populations down to 137.5% design capacity) as the culmination of multiple lawsuits filed against the state in 2007 for poor prison conditions and lacking resources. Before the court order, Senate Bill (SB) 678 passed in 2009, offering financial incentives for counties to house felons in their jails rather than state prisons. The same year, SB 18 introduced non-revocable parole thereby removing low-level offenders on parole from active supervision. Assembly Bill (AB) 109 moved low-level felons from prisons to jail or out on parole in 2011, the year of the federal court order. Subsequent to these congress-led legislation changes, Californians passed Proposition 36 in 2012, mitigating the three strikes law, and Proposition 47 in 2014, reducing penalties associated with certain lower-level drug and property offenses. Also in 2014, by court order, the California Department of Corrections and Rehabilitation (CDCR) increased the use of "good time" credits and rehabilitation incentives.[11]

Our study focuses on a recent legislative change, again enacted by Californians, Proposition 57, or "The Public Safety and Rehabilitation Act of 2016." Like prior reforms, Prop. 57 aims to prevent the federal courts from "indiscriminately releasing prisoners," along with protecting the public, reducing costs, and strengthening rehabilitation.[12] It claims to meet these goals in three ways: (1) expand parole eligibility; (2) enlarge the potential efficacy of sentence credits (to be decided by the CDCR); (3) require judges, not prosecutors, to decide whether a juvenile is transferred to adult court. Expanded parole eligibility will be our focus here. We will put aside youth offenders to focus on charging decisions for adults. The reason we leave out expanded sentence credits is that (a) there is uncertainty as to whether they will be applied in a given case (the offender may be ineligible, not admitted into a program, or on a waitlist, and so the credits apply at an unknown time), (b) there is selection based on which prison an individual is sentenced to, and (c) whether incarcerated individuals are released due to credits is unobserved in our data. Our causal model below assumes that prosecutors

[11] For a timeline and summary of these changes, see Lofstrom and Raphael (2016).

[12] See SEC. 2 of "Proposed Law" in the Legislative Analysis prepared by the Attorney General.

are not adjusting their behavior in anticipation of sentencing credits, but rather solely on expansion of parole eligibility. The uncertainty of credits being rewarded, related to how one would adjust practices with respect to credits, is one reason to believe this assumption is met.

The major change introduced by Prop. 57 is that someone convicted of a non-violent felony becomes eligible for parole once the "primary sentence" is served. "The full term for the primary offense" is defined in Section 32(a)(1)(A) as "the longest term of imprisonment imposed by the court for any offense, excluding the imposition of an enhancement, consecutive sentence, or alternative sentence." Someone convicted of a nonviolent felony may be released by a parole board, then, without serving a consecutive sentence or alternative sentence (if convicted of more than one charge), or serving an enhancement. A key exception with respect to enhancements are those that recategorize the charge from a nonviolent felony to a violent one.[13] Prosecutors can expect only the primary offense to be served in nonviolent cases at a minimum, and, even then, sentencing credits can be earned for good behavior or for participating in work, training, or education programs that further reduce time served (although we are not evaluating the latter for reasons stated above).

Critics of Prop. 57 worry about the definition of violent felony given in PC 667.5.[14] The legal definition of "violent" is not intuitive since certain crimes one may expect to be included as a violent felony are not considered under California Criminal Statute Code: for example, rape of a drugged victim (PC 261(A)(3)), rape of an unconscious person (PC 261(A)(4)), rape of a minor older than 14 (PC 264(C)(2)), torture (PC 206), sodomy with someone between the ages of 14 and 18 (PC 286(B)(1) and 286(B)(2)), lewd or lascivious act

[13]For example, if a firearm is used (PC 12022.3a, 12022.5, or 12022.55) or Great Bodily Injury (PC 12022.7, 12022.8, 12022.9; formerly, PC 213, 264, 461).

[14]See "Argument Against Proposition 57" by Martin Halloran, George Hofstetter, and Stephen Wagstaffe in the Legislative Analysis (pg. 59). In a rebuttal by Governor Edmund G. Brown, Jr., Chief President of Probation Officers of California, Mark Bonini, and Dionne Wilson, they cite Penal Code 290 as excluding sex offenders from Prop. 57 (ibidem).

with someone 14 or 15 years old (PC 288(C)), and shooting at an unoccupied dwelling (PC 247(B)). This threshold between violent and nonviolent felonies is central to our analysis and so the formal definition has been included in the appendix section 1.1.

Except for a few qualitative or summary studies Davis (2021); Darbinyan (2019), there is no published empirical analysis on the impact of Proposition 57 on prosecutors.[15] One reason may be the bill's opacity on what is changing (it assigns CDCR the task of awarding credits) and no formal changes to prosecution.[16] Our object is to measure whether prosecutor behavior changes from a new expectation of sentence conviction outcomes, an unintended channel of variation of the policy. In this way, we intend to measure changes in discretion of filing charges by leveraging the plausibly exogenous shock of Prop. 57.

We hypothesize that, in response to Prop. 57, prosecutors will file more offenses on the threshold of violent and nonviolent. This testable predictions result from the theory of case value. Violent felonies are more valuable than nonviolent, in the sense that offenders are punished equally to the severity of the offense compared to nonviolent felonies, so that prosecutors will seek the higher case value for threshold cases. At the same time, nonviolent felonies lose value, and so prosecutors will shift more resources to violent felonies.

[15] Prior legislative changes have been studied extensively: Realignment (AB 109), Prop. 36, and Prop. 47 have been studied in terms of homelessness (Yun, 2022), racial and ethnic disparities (MacDonald and Raphael (2020); Lofstrom et al. (2020); Mooney et al. (2018)), incarceration and crime rates (Dominguez Rivera et al. (2019); Barts and Kubrin (2018); Sundt et al. (2016); Lofstrom and Raphael (2016)), jail and prison populations (Grattet et al. (2016); Lofstrom and Martin (2014, 2015)), recidivism (Bird et al. (2018); Bird and Grattet (2016)), criminal justice systems (Lofstrom and Martin (2015); Petersilia (2014)), and the consequences of federal orders on governmental finances (Boylan and Mocan (2014)).

[16] Except for disabling prosecutors from deciding whether a juvenile should be tried in adult court.

2.3 Data

2.3.1 California Department of Justice (CalDOJ) California Offense Record Information (CORI) Data

California Offense Record Information (CORI) is our primary data source, which was accessed through an open research initiative by the California Department of Justice (CalDOJ) pursuant to penal code section 13202. Data is recorded at the unique offender-statutory code-sentence length level allowing us to observe each step within an offense "cycle." As defined within CORI, the cycle of an offense begins with an offender, identified by a unique identifier, being arrested [17] where each statutory code suggested by the arresting agency upon an arrest being made is recorded. Subsequently, a court action is recorded as the second step of the cycle that includes the final disposition sentence type and length associated to each statutory code the offender is convicted of [18]. Often times, a single charge disposition results in numerous sentence locations (i.e. time in prison, parole, fine) and within CORI each sentence location is recorded within a separate row. To avoid inflating offense cycles with multiple sentence locations for each conviction, we drop the duplicate observations that only differ by sentence location. Additionally, there are instances that offenders have additional steps within their cycle, such as diversion, mental health supervision, Federal out of state custody, and offenses made while in custody. As these offense types are not applicable to the legislation change evaluation at hand, we censor our analytical sample of CORI to offense life cycles that contain an arrest followed by a court action. Given that this data expands across a panel from 2015-2018, there are numerous instances where a specific offender re-appears within the data for a new offense. This facet serves to be a potentially difficult obstacle if, for instance, an individual is arrested for an offense, is released on their own recognizance, commits a new offense while released but the subsequent row court action listed is associated

[17] We observe both the date of the arrest and the arresting agency name/location.

[18] We observe both the court date and court name/location.

with the first arrest while the new offenses are handled within a new case/court action. This is especially important to take into consideration given the nature of offenses being handled by prosecutors within the county at which the offense took place. Further, we note that it is common that an offender may purposefully or without noticing the arbitrary unseen county lines that divide two prosecutor offices, re-offend across a variety of counties. As the jurisdictions of prosecuting cases are contained within the county, we know for certain any follow-up arrest within a different county will be handled by the county in which the offense occurred. CORI includes a variable denoting the cycle date to combat any potential misattribution that would include multiple independent offenses within the same offense cycle. This date variable differs from the event date that is unique for every arrest and court action in that the cycle date remains constant for each offender's arrest and follow-up court action contained within the same cycle. Therefore if there are any other arrests made between the time of the original arrest and final court action or follow-up recidivism after the court action, this information is not encapsulated within the analysis of the current offense life-cycle.

While the CORI data provides granularity on the various steps involved within the life-cycle of an offense, two of the most pertinent components for our analysis are omitted: prosecutor decisions and realized sentence length served. Due to this limitation, we refrain from conducting any statistical analysis with these data but rather motivate the presence of a treatment effect with respect to Proposition 57 as is described within Figure 1 [19]. To remain consistent with our goal of evaluating the effect of Proposition 57 on prosecutor decision making, we will run our main results on prosecutor data obtained from an agency in a large county within California. For the latter of the two aforementioned missing components, CDCR is the only agency, to our knowledge, that observes actual time served after court

[19] We note that figure 1 includes all offenses that result in a court action therefore omitting cases disposed of by pre-trial methods. If charges disposed of by pre-trial methods are systematically different to charges disposed of by court action, interpretation of state-wide trends must be interpreted with this limitation in mind.

sentencing. Since this specific research focuses on the second channel of Proposition 57 sentence reduction, that is that all non-violent felony offenders are eligible for early parole after serving the entirety of their primary sentence, irrespective of credits earned, we do not require data from post-incarceration. An extension to the current research is to evaluate the first channel of early parole consideration by linking offense outcomes with total time served under CDCR jurisdiction coupled with any plausibly exogenous awardment of credits to offenders.

County-level Prosecutor Data

Our secondary data source is administrative data from a large county in California, henceforth referred to as County A. Table 1 compares the composition of socioeconomic factors within County A to California. [20] Further, table 1 showcases that unemployment rates, median earnings, racial composition of the population and median age within County A and California share similar trends during the panel. Comparing these covariates is pertinent in establishing generalization of our county-wide analysis to other counties in California.

[20] We obtain this data from the Census using the data explore platform here. and from the American Community Survey from IPUMS.

Table 1: Summary Statistics of Covariate Balance in California and County A

Variables	**California** Before Proposition 57	After Proposition 57	**County A** Before Proposition 57	After Proposition 57
Population Size	78,394,835	79,093,698	2,189,641	2,418,185
Median Income (Dollars)	31,600	35,000	63,110	61,656
Proportion Completed High School	0.66	0.67	0.78	0.77
Racial Composition				
Black	0.07	0.07	0.06	0.06
Hispanic	0.39	0.39	0.53	0.52
Asian	0.16	0.17	0.05	0.05
White	0.64	0.63	0.32	0.32
Other	0.15	0.16	0.04	0.04

Notes: The following table depicts summary statistics on state versus county covariate characteristics before and after the legislation change. Columns (2-3) present summary statistics sourced from the American Community Survey in California before and after the legislation change. Columns (4-5) present summary statistics sourced from the Census Bureau in County A before and after the legislation change.

An example is most instructive in describing the sequences of events that are captured within our county-wide administrative data. Suppose an individual is arrested for burglary, after the individual is booked by an officer, the arresting agency will submit the statutory code sections they deem to correspond with the burglary severity (i.e. first or second degree) as well as any additional charges such as a firearm enhancement to the countywide prosecutor office. If the arrested individual is held in county jail, the prosecutor's office will have a 42 hour period in which they must decide whether to file or dismiss the charges. If the arrested individual posts bail or is not required to be housed in jail due to the nature of the offense, e.g. offense deemed as non-violent, then prosecutors have more time to determine a filing decision. A prosecutor specialized in filing, referred to as a filing or review prosecutor, handles incoming cases from arresting agencies in a randomized fashion. The filing prosecutor will then read the information provided by the arresting officer as well as any evidence or eye witness testimony when making a decision to file or dismiss the offender-case. Some cases include multiple defendants, for example if the individual charged with burglary had a friend that

was also arrested for being the "get away" driver. In these instances, the filing prosecutor can decide to file charges for one defendant but not the other or to file charges for both defendants. Some argue that the first avenue in which prosecutors practice their discretion is in deciding whether to file or not file charges [21]. Suppose the filing prosecutor decides to file the burglary case, but the arresting agency had suggested a first degree statutory code [22] but after reviewing the facts of the case the filing prosecutor decides the evidence aligns more with second degree burglary with no enhancement[23]. The filing prosecutor can then file the case with the revised statutory codes they deem the evidence supports, and this decision is believed to be the second channel of discretion by the prosecutor.

We caveat that though the term "prosecutor discretion" is used widely in the literature, it is difficult if not impossible to define what discretion means in this context. Moreover, there is no established "ground truth" or correct level of punishment for an offense. Further, we do not observe all of the arresting information a filing prosecutor observes when deciding to file or not file. One way we can think of identifying discretion within this setting is by the decision a filing prosecutor makes to file charges with greater case value, violent or non-violent felonies with substitutes, more frequently to cases with lower case value, non-violent felonies with no substitutes or misdemeanors. The second way we can think of discretion in this setting is when a filing prosecutor modifies the filing charge to be different from the initial arresting charge. A potential argument made against defining discretion in this way is if arresting agencies typically fail in correctly identifying the statute code associated with an offense, which then results in a prosecutor substitute to a violent felony that is independent of practicing discretion. That is, substitution is not a function of discretion but rather a function of a systematic problem in arresting agencies' ability to identify arresting

[21] The decision to not file charges will be referred to as a charge being dismissed henceforth though this is not the vernacular used institutionally

[22] First degree burglary, penal code 459/460(a) is considered a violent felony.

[23] Second degree burglary is not considered a violent felony.

charges appropriately. We counter this argument by noting that a study on substitution before and after a legislation shock, allows us to discern the rate at which substitution occurs as a plausibly exogenous test. So long as our arrest trend tests are not significantly different before and after the legislation (see table 8), we can ensure any variation in a filing prosecutor's filing decision or charge substitution is identified purely by the legislation.

Within this data set, we observe both arresting agency suggested and filing prosecutor filed statutory code sections at the offender-statutory code-final disposition level from 2004-2021. We censor our data to include cases filed from 2015-2019 in order to avoid three levels of misattribution in treatment effects. First, we limit to 2015 onward due to a major legislation change in California enacted in 2014, Proposition 47, which re-defined specified felony lower level drug crimes to misdemeanors. By censoring after this legislation we do not require to account for the offense type severity shifting for specific non-violent felony statute codes in 2014 that would, if not accounted for, surely bias or taint our results. By censoring the data to look up until 2019, we avoid misattributing treatment effects due to shortages and shutdowns in various criminal justice systems, such as the courts, during the COVID-19 pandemic 2020 onward [24]. Third, if we believe that changes in the elected District Attorney (DA) influences internal office practices, we avoid misattribution by limiting to a time-window where only a singular DA is appointed.

2.3.2 Life Cycle of a Case

Once a case has been filed, a supervising attorney will either assign the case to a horizontal or vertical review unit. Horizontal prosecutors deal with more common, reduced severity offenses such as misdemeanors and drug offenses, while vertical prosecutors deal with more

[24] A wide body of research exists citing appropriate time horizon for panels within the context of difference-in-differences and other causal inference analyses. We believe maintaining 5 years of data, two years prior and three years post is appropriate due to the granularity of the data being contained at the individual defendant-charge level. Especially when compared to other analysis that these panel recommendations are based on being at the county-month or other aggregated levels

serious and violent felony offenses. Within the vertical prosecution, various units exist that specialize on cases such as gang, domestic violence and murder. Assignment of cases to some horizontal prosecutors is stated to be semi-random within this county corresponding to the court number though it seems that attorney's prior experience dealing with similar case types and current case load are two potentially non-random factors that inform case assignment, this is especially true for more serious case types[25]. For this reason, the current analysis will only focus on the decisions made by the filing prosecutor as we understand assignment of cases coming in the door to the various filing prosecutors to be plausibly exogenous.

Once a prosecutor has been assigned a case, a preliminary hearing is set where the filed charges and associated sentence length are brought forth to a judge. The offender then pleas guilty or not guilty under the advisement of their defense attorney present. A common misconception is that prosecutors have total discretion on what charges to bring forth and what sentences to attach to each charge. In actuality, prosecutors in California are guided by a sentencing triad system for felony offenses[26]. Each felony has a lower, middle and upper term sentence length as defined within the statutory code being charged. To our knowledge, there exists no formal guide encapsulating the list of triads for each felony offense. To obtain the "exposure" for the felony, lawyers utilize annual editions on the statute codes in California, also found on California Legislative Information online. For example penal code section 220, assault with intent to commit other felony, has a lower term of two years, mid term of four years and maximum term of six years. The third proposed channel in which prosecutors exert discretion is in deciding which tier of the sentence triad to suggest for the primary offense[27]. Here, the primary offense is referred to as the statutory code that has the longest sentence length attached to it. This is a case where office or even county-wide

[25] Vertical prosecutors deal with serious and violent cases and are typically specialized within a unit such as gang, domestic violence, etc. These prosecutors are not randomly assigned to cases

[26] Refer to the Legislative Analyst's Office summary located here for a for an overview on felony sentencing

[27] Prosecutors do not get to decide the charge length, as this is determined as a result of court convictions or across back and forth discussions with defense counsel

practices inform the norms of filing decisions. For instance, a considerably "tough on crime" county may always charge the mid or high term while a progressive county may always start at the lower tier[28]. Within a perfectly constrained theoretical state of the world, variability in sentence options is meant to allow prosecutors to treat each case and the facts/evidence supporting the statutory code charges uniquely rather than apply a blanket sentence length for all types of burglary, for example [29]. Further care is made to attach sentence lengths to cases with multiple statute codes, where the usual sentencing math is to serve 100% of the primary sentence, 1/3 of any consecutive sentences and enhancements and to not serve any time on any statutory code that has the same or a similar offense definition to the primary term [30]

Prior to and after a preliminary hearing, prosecutors can make plea offers to an offender which in essence requires the offender to admit guilt to the offense with the outcome of reduced punishment compared to counterfactual sentence that would be served after a guilty conviction by court or jury Bellin (2019). The defense attorney will attempt to advise their client on the optimal strategy of whether to admit guilt and accept the plea deal or continue with the trial. The motivation to dispose of a case through these pre-trial channels is to reduce the load of cases handled by the courts as the courts are often constrained with a lack of judges and court spaces. Suppose, in our example the individual that committed burglary did not accept a plea deal and the charges were not dismissed by the court during the preliminary hearing, then the case will be set to trial by court or jury based on if the offender waives their right to a jury trial. Once the case is heard by the court or jury, both the assigned prosecutor [31]and defense attorney will have the opportunity to argue the facts

[28]Though most county-wide charging practices remain constant across time, one may argue that internal policies are a direct function of the elected district attorney's policies. We note that there is no significant change in the policy preferences of the elected district attorney (DA) during our panel since elections run every four years and there was no change to the elected DA from 2015-2019.

[29]A common saying within the prosecutor's office is that not all crime is created equally, each offense has is committed by a unique person.

[30]Penal code section 654 prohibits the conviction of two similar or same charges.

[31]At times, an attorney other than the assigned prosecutor, the appearing attorney will go to court to

and evidence of the case that they believe warrants a conviction or dismissal, respectively. Upon the completion of the back and forth statements, testimony, and witnesses/victims called forth, a disposition on the case is made by either the judge or jury [32]. This disposition includes a guilty or not guilty verdict associated with each charge an offender is convicted or acquitted of and the associated sentence length. If a guilty outcome is realized, the burglary offenders will be likely sentenced to prison based on the severity of the offense and previous offense history [33].

Table 2 reports the count of Felony filings, dispositions and percent disposed by jury trial across California and County A from 2015-2019. These data suggest that most cases are disposed of prior to jury trial, either by plea deal or dismissal. This data is reported by the California Courts Judicial Branch of California Statistics Report.

Table 2: Court Statistics Report - Felony Filings, Dispositions and Court & Jury Trials

Fiscal Year	California			County A		
	Average Filings	Average Dispositions	Avg. % Jury Trial	Average Filings	Average Dispositions	Avg. % Jury Trial
Before Prop. 57	204,265	203,497	2.35	29,997.5	27,422	1.9
After Prop. 57	186,045	155,438	2.43	35,542	28,451	1.43

Note: The following table represents summary statistics sourced from the California Court Statistics Report, Column (1) from Figures 24 and Figure 57, Column (2) from Table8a, Table8b, Table9a and Table9c contained within Appendix G. Columns (2-5) depict filings, dispositions and percent of jury trial dispositions across California during the panel. Columns (6-9) depict filings, dispositions, and percent of jury trial dispositions across County A during the panel. The underlying data is reported at the end of each fiscal year.

Misdemeanor offenses are typically served in county jail whereas felony offenses in state prison, one exception to this comes from recent legislation change in 2011, AB-109, which allows relatively less violent offenses to be served under the jurisdiction of county jail also

appear on behalf of the assigned attorney if the assigned attorney is actively engaged in another jury trial case.

[32]There is another process within a life cycle of a case that includes prosecutor and defense attorney selection of a jury of one's peers. We do not discuss this selection process in detail due to the fact that this feature is not studied within this research design

[33]There are various formats in which a sentence can be served, such as jail, prison, probation, home confinement, fine, community service, restitution to name a few.

known as a penal code 1170H offender.[34] Once a felony disposition results in a prison sentence, the individual is transferred to the specific institution based on their offense severity and security level necessity. During the duration of the sentence, this offender is under the jurisdiction of CDCR, where the offender will take part in work duties, rehabilitation programming and other activities. The role that CDCR plays with respect to this legislation change is awarding sentence credits and assigning offenders to specific rehabilitation programs. As previously mentioned, we stray away from investigating this channel in sentence reduction due to the endogeneity in the types of offenders being assigned to specific programs that are not opt-in, such as the vocational fire-fighting training program, as decided by CDCR.

2.3.3 Variables of Interest

As previously noted, we limit the use of CORI data to descriptive statistics to summarize trends in convictions as well as the rate of arrests with no subsequent follow-up court action parsed apart by violent and non-violent felony arrests (see Figure 2). The pertinent variables of interest within this data include statutory codes used to define offense type, action type defined as either arrest or court action and sentence length location and duration. Greater attention is placed on our secondary county-level data source as it contains prosecutor-level decision making. Though this data captures a myriad of decisions and outcomes related to prosecution, special attention is made to retain the following variables: charges suggested by the arresting agency, arresting agency name, charges filed by the review prosecutor, review prosecutor identifier, offense date, arrest date, filing date and whether the case was dismissed. An extension to this paper will be to measure a second stage, the rate at which a plea was offered and/or accepted by the defendant [35]. We focus on the exogenous exchange between

[34]Since 1170H offenders are housed within the county jail, proposition 57 credits and early parole are not potential outcomes, therefore we exclude these individuals within our analysis.

[35]We transition to describe offenders as defendants once a charge has been filed to be consistent with the naming practices within the county-wide administrative data though the step in which to make this

the arresting agency and filing agency at first, but we plan to extend this analysis to observe if conditional on a treated and untreated charge being filed, how does the rate of pleas offered on each type of charge vary [36] . Our treatment variable is defined through the comparison of violent and non-violent felonies while our outcome variable is defined as (1) decision to file or (2) dismiss a charge. This outcome is defined as binary, estimated through linear probability models (LPM) as described within the research design section.

Though the county-wide data is far more granular than the state-wide arrest and court trends, heterogeneity within the county is prevalent especially given the size of the county. With such a large county, multiple prosecutor branches are present where each branch has a different likelihood of interacting with various offense types, i.e. some branches within rural areas are attuned to a different composition of crime compared to branches that are located within urban hubs. Therefore, we take into consideration prosecutor branch fixed effects to absorb variation attributed to branch practices to help better isolate the variation in the legislation's treatment effect. Further consideration is made to compare filing decisions across individuals that commit similar offenses, that is we ensure the decision to file a burglary is not compared to the decision to file a murder. Despite the fact that using statutory code or charge fixed effects seem like a seemingly straightforward solution to this concern, there are two pitfalls in utilizing this level of fixed effect. First and foremost the level of treatment is at the statutory code level and any statue level fixed effect would absorb all of the variation we hope to estimate. Second, adding a statutory code fixed effect would necessitate multiple defendants within a prosecutor's branch, within a quarter and year were arrested and/or filed with the same exact statute code or set of codes. Determining the appropriate amount of fixed effects to absorb enough variation while not observing all variation is the delicate

distinction, if to even ever make it, is widely debated in the literature.

[36]Due to the endogenous nature of assigned prosecutor workload and preferences being a determinant on offering plea deals, we plan to apply a jacknife IV strategy to leverage plea offer idiosyncrasies across prosecutors in the future.

trade-off faced by social scientists wanting to isolate the effects of a specific policy change [37]. We avoid the aforementioned trap of over-absorbing charges by using a generalized crime category fixed effect as provided by the agency. Finally, we observe cyclicality within our state-wide arrest and court trends which motivate us to check for such trends within our county-wide data that we can further seasonally adjust for. Aside from the nuanced nature of crime being widely cited as fluctuating across the various seasons, the number of cases filed by a filing prosecutor also fluctuates across various days of the week. We therefore include two temporal fixed effects, one for the year by quarter level and second for the day of the week.

2.3.4 Defining Violent Felonies and Serious Felonies

One of the primary contributions of this work is to identify statutory code definitions associated to violent and serious felonies as defined by the legislature and attach these definitions to an administrative data set. To our knowledge, this is one of the first research analyzing legislation at the unique charge code level across the entire population of cases brought forth to a large county's District Attorney office. To carefully construct the violent and serious felony definitions, we first referred to code section 667.5(c) to define violent felonies and code sections 1192.7(c) and 1192.8 to define serious felonies [38]. A limitation in utilizing these publicly available resources to define offense categories is that certain offense types do not have a statutory code listed accompanied with the offense description. For example within violent felonies: robbery, attempted murder, murder, mayhem, kidnapping and any felony punishable by life or death do not contain associated code sections. Whereas, for instance arson is defined as a violation of subdivision (a) or (b) of Section 451. This is even more

[37] To our knowledge there is no arbitrary or proven cutoff for the number of minimum observations required after absorbing the various levels of fixed effects to maintain power. This is likely due to the nature of sample size constraint fluctuating based on the features of various policy and data settings.

[38] These definitions can be sources through California Legislative Information here:Violent Felonies,Serious Felonies, 1192.7(c) and Serious Felonies, 1192.8 or through the California Department of Corrections and Rehabilitation here: Violent Felonies and Serious Felonies.

prevalent within serious felonies: rape, sodomy/oral copulation by force, violence, duress, menace, threat of great bodily injury, holding a hostage, selling/furnishing heroin to a child do not list associated code sections just to name a few[39].

One concern in the generalization of specific offense labels is the nature of ever-changing statute code definitions of offenses within California. We account for such concerns by ensuring to take into consideration any amendments made in the definition of offenses being charged one way for a specific set of years and another way after new legislation. Most importantly, upon reviewing the various revisions in code sections attached to violent and serious felonies, there was a single revision realized during the timing of our panel that would elicit such concern[40]. We account for this by only using the revised definition within our violent felony definition scraper.

Our primary concern in making educated guesses in attaching a statutory code to the generalized offense types is that these guesses will not adequately compare to the definitions attorneys would attach given their extensive legal training. To avoid such expected errors, we utilize a sentencing guideline document disseminated to all prosecutors' offices in California that includes 2006 onward mapping of violent and serious felony offense description to statute codes[41]. We received confirmation that there is no discretion in charging any of the generalized label definitions with any other code section other than the one which is listed within the sentencing guidelines.

[39] A full list of serious felony offenses can be found in the appendix section 01. Any offenses that list an offense with no associated statutory code are considered to be generalized labels

[40] There were two changes to violent felony offense statutory code definitions. The first change was made for oral copulation by force, threat, in concert, or with a child formerly charged under penal code section 288a(c) modified to now be charged as penal code 287(c)(d) as of January 2019. The second modification was made in January 2012 to offenses such as explosion with intent to murder, explosion causing bodily injury, explosion causing great bodily injury or death formerly charged as penal code 12308, 12309, 12310 and currently charged under 18745, 18750, and 18755 respectively.

[41] These definitions also include any adjustments or amendments in statute codes associated with various offense types.

2.3.5 Regular Expression Scraping Extractor: Violent, Serious and Non-Violent Felonies

Upon receiving confirmation regarding the list of violent and serious felony statute codes associated to each offense, a regular expression extractor was constructed to create dichotomous variables for each offense type. The procedure to create this extractor followed Stata's "ustrregexm" syntax leveraging both the flexible and rigid features of the syntax application. Namely, a subset of offense types are constrained to specific paragraphs within a subsection of a penal code subdivision, e.g. rape by force, threat, or in concert defined by penal code 261(a)(2); 261(a)(6) and 264.1. While other offense types have a flexible definition such as assault with intent to commit a specified felony, penal code 220, where any subsection and paragraph within penal code 220 is considered a violent felony within the penal code 667.5(c) definition. An example of an offense description that the extraction was applied to is: "459 PC-BURGLARY:FIRST DEGREE" or "518 PC-EXTORTION". In the first example, both the penal code section and the description is required to identify that this specific offense is considered a violent felony [42] First, variables storing penal, vehicle, united states, military & veterans, health & safety and welfare & institution codes were created to separate the storage of each statute code type. Second, only the code section was retained, dropping the "PC" or "WI" suffix, for example. Third, code sections with a strict definition contained within a specific subdivision or paragraph included a "blank space" suffix to denote that once the code section being searched is found, there should be no parentheses or other alpha or non-alpha numeric characters that follow. An example of this type of extraction is showcased below:

```
gen  spsl_rape = " "
```

[42] First degree burglary is considered violent while second degree is not, though both share the same penal code definition. This is the only offense that requires the coupling of description with statute code to be defined.

```
foreach  w  in  262(A)(1) 262(A)(4) {
replace spsl_rape  =  ustrregexs(0)  if   ustrregexm(pc_offense, "^`w'[\ s]")
 }
```

While flexible code sections include a suffix search of "[/ s|/ W]" which allows the inclusion of a space or any non-alpha numeric characters, such as a parentheses embedded after the code section to call on a subsection or paragraph within the statute. All searches are prefixed with a carrot, to ensure the offense description begins with the queried statute code, ensuring the extractor does not pull codes that are embedded within a larger code section, i.e. defining PC 18205 as the same offense as PC 205.

The same extraction methods are utilized on the full list of serious felony offenses as defined within penal code 1192.7(c) and 1192.8 (Appendix 1.2).

2.3.6 Descriptive Statistics

Table 3 depicts state-wide summary statistics utilizing CORI to compare average violent felony, non-violent felony and serious felony arrests and court actions before and after the legislation. We note that the difference in the aggregate arresting and conviction trends for violent felonies, non-violent felonies and serious felonies remain unchanged before and after the legislation change. Noticeably, there is a slight reduction in arrests after the legislation for all felonies, coupled by an increase in convictions, most noticeably for violent felonies. Table 4 showcases similar statistics for County A, arrests and cases filed without the inclusion of serious felonies. Similar to state-wide differences, we observe no significant change in arrests or cases filed over time, with an increase in violent felony filings and decrease in non-violent felony filings.

Table 3: Summary Statistics of Violent and Non-violent Felonies Across California

	Arrests		**Court Actions**	
Offense Type	Before Proposition 57	After Proposition 57	Before Proposition 57	After Proposition 57
Violent Felonies	0.0323	0.0302	0.0211	0.0239
Non-Violent Felonies	0.2689	0.2618	0.2019	0.1938
Serious Felonies	0.0681	0.0662	0.0505	0.0566
Observations	1,275,071	868,986	953,686	1,150,105

Notes: The following table depicts summary statistics on state-wide violent, non-violent and serious felony arrests and convictions. Average proportions are shown in columns 2-5.

Table 4: Summary Statistics of Violent and Non-violent Felonies in County A

	Arrests		**Cases Filed**	
Offense Type	Before Proposition 57	After Proposition 57	Before Proposition 57	After Proposition 57
Violent Felonies	0.0010	0.0016	0.0014	0.0018
Non-Violent Felonies	0.0325	0.3148	0.3244	0.3145
Observations	255,477	364,539	242,695	341,177

Notes: The following table depicts summary statistics on county-wide violent, non-violent and serious felony arrests and charges filed. Average proportions are shown in columns 2-5.

Figure 1: Conviction Trends Across California

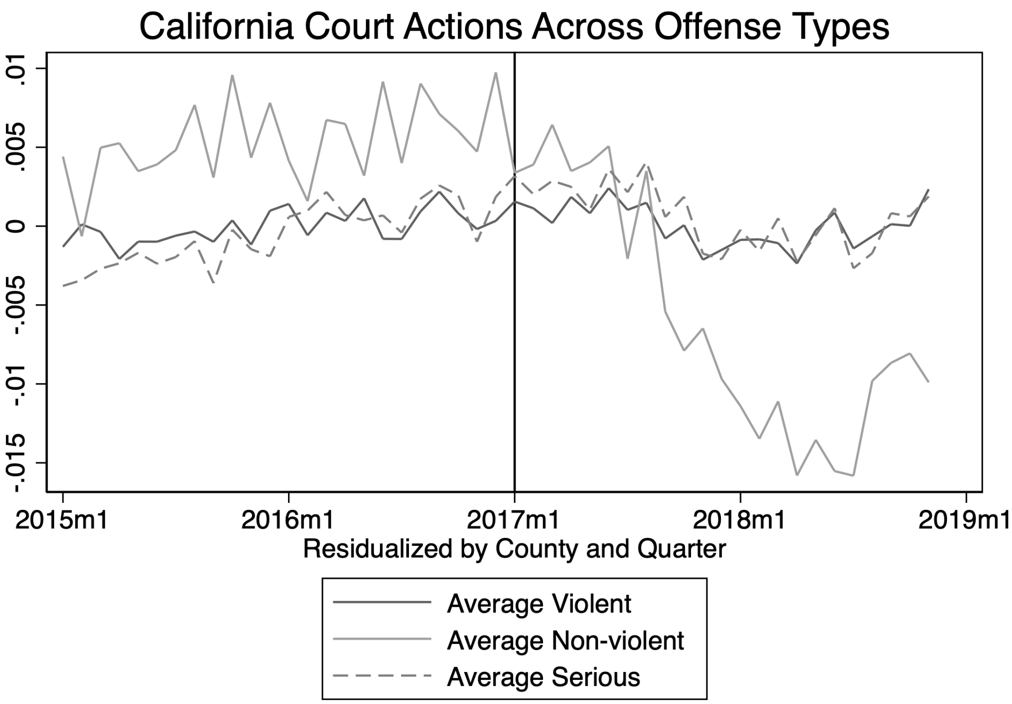


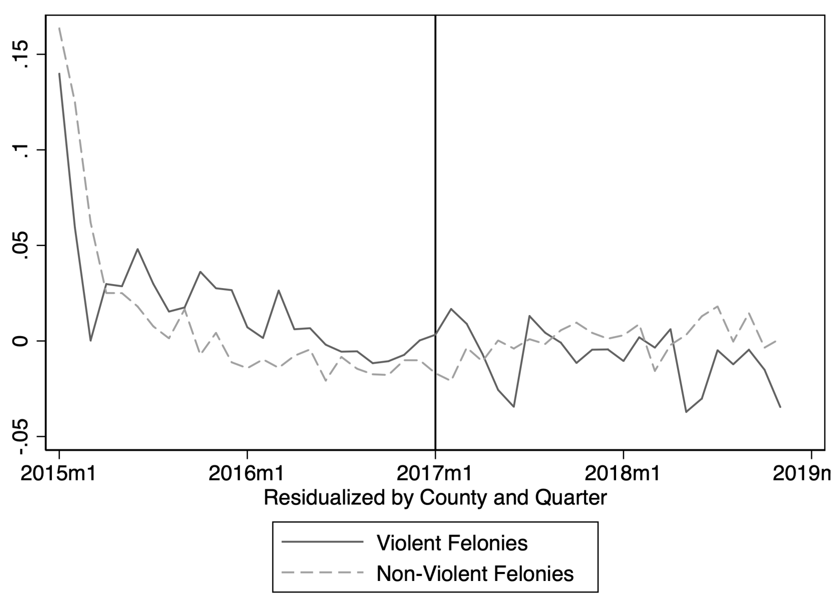
Figure 1 depicts violent, non-violent and serious felony offense trends utilizing CORI data across California. We residualize the data to absorb any heterogeneity in county practices, for instance if we believe progressive versus conservative prosecutor offices will be deferentially influenced by the legislation, such varying practices may cancel out treatment effects. We also attribute for seasonality in crime by including quarter by year fixed effects.

Another statistic we are interested in measuring within CORI is the number of non-violent felony arrest and violent felony arrests that have no subsequent court action over time. We describe these trends in figure 2. Figure 1 coupled with figure 2 support our hypothesis that though violent felony offenses remain constant over time, resource re-allocation is observed where non-violent felony arrests observe a sudden drop-off in court actions after the legislation change is realized. Figure 2 and figure 3 depict the average number of non-violent and violent court arrests with no-follow up court action. Namely, figure 2 describes instances

where a non-violent felony arrest is made with no follow-up court action conditional on there being at least one previous non-violent felony arrest for that offender which also did not result in court action. We interpret the post-legislation incline in non-violent felony arrests that do not result in court-action conditional on there being no previous court action suggestive of two things. First, is that arrests may at times be made without substantive evidence to prove the offense in court. Conditional that an offender does not deviate significantly in the types of offenses being committed, prosecutors may practice Bayesian updating when viewing that prior arrests resulted in no successful court action and therefore decide to dispose of these cases through dismissals or deciding not to file. Second, the sentencing exposure and charging ability of prosecutors is enhanced with respect to certain prior offenses. One may argue that in order for prosecutors to value a non-violent felony case as worth allocating limited resources toward, it would require some prior conviction history to be present. Another way of explaining this same phenomena, is that prosecutors re-assess their attention to the most violent and egregious offenses, especially offenders that show a trend of increasing offense severity with each re-offense. Comparatively, figure 3 depicts isolated offense cycles of a singular non-violent felony arrest that result in no court action. Notably the trends of average violent and non-violent felony arrests with no follow-up court action when only honing in on the specific cycle are unchanged after the legislation. This further suggests that previous criminal history and the outcome of such history influences method of disposition for incoming cases. Indeed, filing and review prosecutors are provided not only the current details of the offense but also prior criminal history within the county when making the decision to file or dismiss incoming charges. These descriptive plots provide further evidence that any policy shock is being directly realized by prosecutors and not explained by a shifts in offending or arresting behavior.

Figure 2: Violent and Non-violent Felony Arrests with No Follow-up Court Action Conditional on Previous Non-violent Arrest with No Cout Action

Figure 3: Violent and Non-violent Felony Arrests with No Follow-up Court Action Conditioned to the Current Offense Cycle



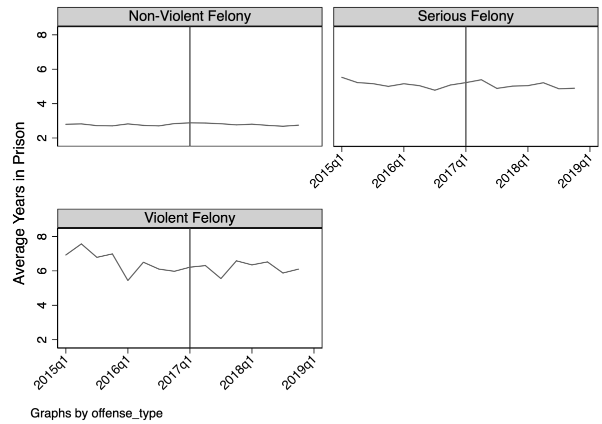
Another facet worth explicitly discussing is the nature of this legislation intended to

directly influence post-sentencing outcomes. This means that the way in which sentence time to be served as well as convicted sentence length remains identical from before and after the legislation change. The legislation gives (1) offenders the chance to be eligible for early parole after serving the entirety of their primary non-violent felony sentence [43] and (2) direct discretion for CDCR to award credits that once accumulated can be used for early parole consideration. Given that the only administrative data that contains actual time served is recorded by CDCR, all other data sources will record convicted sentence length found on the abstract of judgement at time of case disposal. Though the sentence reduction from early parole eligibility is not realized in either CORI or County A's data, we might be interested to further check our hypothesis of substitution to measure if the average sentence length and/or location for a non-violent felony conviction changes over time. Though we expect there to be substitution in charging, we do not anticipate any significant changes in sentence length duration or location. Figure 4 showcases average number of years served across the various institutions recorded within CORI for non-violent felony, violent felony and serious felony convictions. Namely, we observe there are no significant changes in the trends pre and post legislation consistent with our expectations.

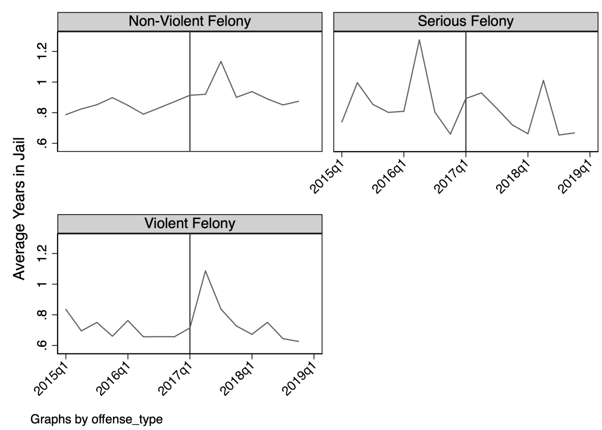
[43] Once an individual is eligible, they must prepare a post-incarceration plan which is then presented in a parole hearing and is then subject to being accepted or denied based on conduct history while incarcerated.

Figure 4: Average Sentence Lengths Across Incarceration Type

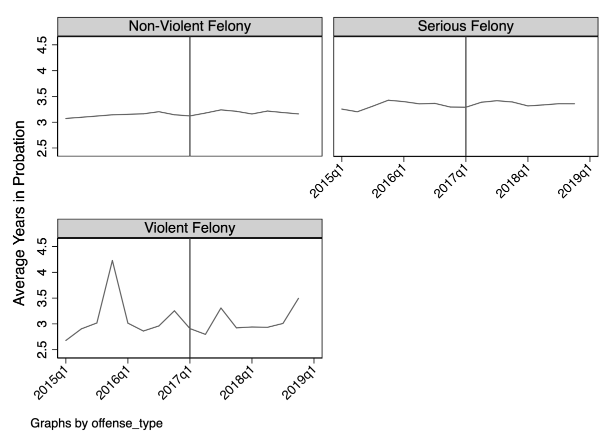
(a) Prison



(b) Jail



(c) Probation



2.4 Methods

A novel contribution made in this research is to be one of the first papers to carefully attach violent, serious felony and non-violent felony definitions to the statute code sections. The necessity to define non-violent felonies is straightforward given the intended measurement of treatment effects. More explicitly, as proposition 57 introduces a channel to automatically introduce early parole consideration once the entirety of a non-violent primary sentence is served, we anticipate our treatment effect to be a function of non-violent felonies. A less obvious channel the legislation influences is its impact on violent felony offenses. Due to the nature of violent felony offenses being ineligible to the same extent of sentence reduction as non-violent felony offenses, we believe prosecutors will substitute a non-violent felony offense with a violent felony offense when substitution is an available option. We define non-violent felonies as any statute code that is charged a felony and not contained within the violent felony definition. Violent felonies are constrained to the 667.5(c) definition. We also go through the arduous efforts to attach serious felony offense definitions to each statute code in light of proposition 57 directly impacting the sentence duration for strike offenses. Namely, that "strike" offenses considered under Three Strikes Law would no longer receive mandatory full-term completion of consecutive sentences for second and third "strikers." Though the legislation is not written in a way to highlight the modification in Three Strike sentencing, we identify serious felony offenses to measure any substantive changes within serious felony offenses. We do not observe a shift in charging practices with these offenses. This notion is further supported by institutional knowledge that little to know attention was made to charge serious felonies since modifications in charging behavior within non-violent felonies were the most approachable application. For completeness, we still include the trends and analysis for serious felonies as these offenses as they are listed as being impacted by the legislation.

2.4.1 Defining Non-violent to Violent Offense Substitutability

We define substitution effects of non-violent felony statute codes to violent felony statute codes in two ways within CORI. As a first pass, we subset the CORI data to contain offense cycles with only a singular non-violent felony arrest conditional on having at least one violent felony court action from before the legislation was passed in January 2017.[44] We stratify within this criteria due to the nature of any intermediary steps after the arrest and before the court action being unobserved within CORI. Further, this means that if we evaluated substitution on any offender that commits at least one non-violent felony, we would have no way in identifying which statutory code of the set of codes was substituted to be violent or even if it was a non-violent, serious or misdemeanor charge that was substituted. Though this methodology provides assurance on potential errors with inappropriately defining substitution, a major drawback in this approach is self-selection of offender behavior. More explicitly, one can easily imagine that the type of offender and offense that is singular and non-violent is systematically different to the types of offenders that commit multiple offenses within the same point in time. We are concerned with self-selecting to only offenses that commit a singular non-violent felony in that the generalization of our results decreases. This motivates us to perform a second pass substitution test. Within the second pass framework, we stratify our sample to include arrests where the only statutory codes charged are non-violent felonies and compare this to the grouped statutory code court actions. In essence, by grouping the set of non-violent felony charge codes, we measure whether a non-violent assault and family offense arrest translates to a violent felony court action, such as a penal code 220 violation. To further enhance measurement of substitution, we group the violent felony court actions to then compare how the set of non-violent felony arrests is substituted for a set of violent felony court actions. The upside of the second pass is greater generalization of offenses that

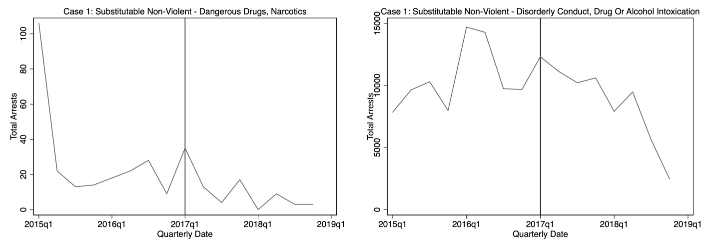
[44] We use pre-legislation observations to define treatment as this is common practice when learning from the data. We also test differences in the statute codes that are being changed to a violent felony in the post period and find that there is more substitution occurring on codes that originally did not have substitution.

normally contain multiple statute codes within an arrest, but the pitfall is the lack of power this type of grouping elicits. More explicitly, power in our sample diminishes when grouping arrest and court action statutory codes as it requires multiple offenders are arrested for the same set of statute codes and convicted of the same set of substitution charges. Due to the sparsity in the second pass substitution, there are a handful of non-violent charge sets to violent felony charge sets that occur once or twice and are too noisy to appropriately measure. Figure 5 depicts a subset menu of singular non-violent felony arrests that result in at least one violent felony court action [45]. This figure is labelled based on both code section and offense category as defined by CalDOJ for each offense category. Figure 6 depicts a subset of trends for the set of non-violent felony charges substituted to a violent felony court action, our second pass. An easy error to make in this second pass is to have arrests that have burglary listed first and assault listed second treated independently to arrests where the assault is listed first and burglary is listed second. We ensure that the unique charge set does not take into account order effects but rather groups by the contents of the statute codes.

[45]The plot for every non-violent felony that is substituted to a violent felony court action can be found within the appendix, figure 10

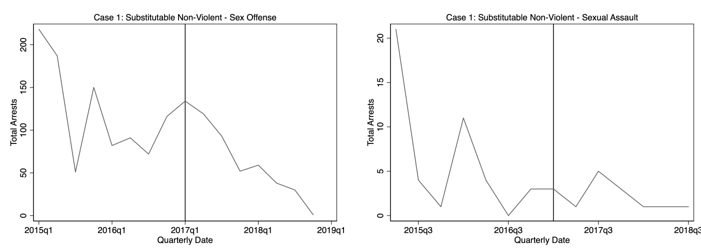
Figure 5: First Pass: Non-violent Arrests Substituted to Violent Felony Court Action(s)

(a) Drug and Alcohol Related Crimes



(b) Driving Under the Influence

(c) Sex Offenses



(d) Theft

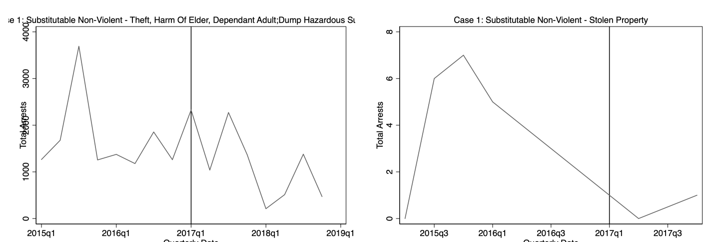
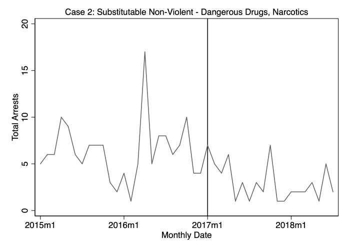
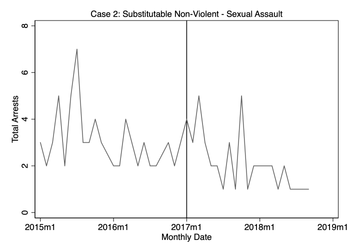


Figure 6: Second Pass: Non-violent Arrests Substituted to Violent Felony Court Action(s)

(a) Drug and Alcohol Related Crimes

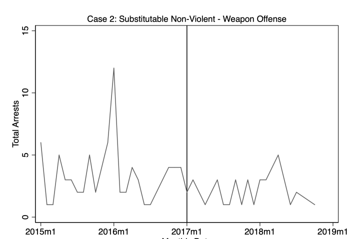


(b) Sexual Assault



(c) Burglary

(d) Weapon Offense



2.4.2 Defining Treatment Within the Context of Proposition 57

One purpose in plotting arrest trends for non-violent single and multiple substitutes is to learn from the state-wide data, what types of non-violent arrests are commonly substituted

into violent felony final dispositions. Though we attempt to garner a substitution code book from prosecutors in various counties across California, we learned that any individual prosecutor's substitution preference of converting a non-violent arrest to violent felony court action is indeed a preference at risk of selection bias and endogeneity correlated to that specific prosecutor's preferences on charging practice, sentence triad preference, experience, unit in which that experience was obtained and county-wide office policy. Therefore, our exploration on the non-violent to violent felony substitutes is our effort to identify treated and untreated groups in the aggregates of state-wide data. As previously discussed, we first test our hypothesis that constrained prosecutor resources, i.e. time, will re-adjust filing behavior to statutory codes that have the greatest incarceration length by measuring if substitution is prevalent and those trends of substitution over time. We do not theorize the qualitative rationale behind why prosecutors may adjust their behavior, for instance it could mean that the prosecutor has always held a view to be punitive toward sentencing, or it could be that the prosecutor empathises with the domestic violence victim that regularly visits their office to discuss their experiences of chronic victimization. In all, we do not observe qualitative data on if or why prosecutors adjust their behavior, but rather this analysis is motivated to shed light on how unintended consequences of legislation can be realized when introducing policy that is intended to inform one set of actors but in actuality has a overarching effect on the various actors handling cases within the criminal justice system.

A second reason for plotting arrest trends for non-violent singular and multiple substitutes is to statistically test our exclusion restriction that arrests and offenders do not change their behavior as a function of the policy change. In other words, any observed effect can be plausibly explained by changes in prosecution and not changes in other actors or steps within the life cycle of an offense, such as offenders and policing. We discuss the methods used to run this statistical test within the identifying strategy section below.

An extension to our first definition of treatment, whether a non-violent felony arrest

is ever substituted to a violent felony filed charge or court action, is to define treatment as a dosage effect. In essence, specific violent felonies have a larger universe of potential non-violent felonies. For example murder, penal code section 187, does not have another code section substitute since all murders are defined as violent [46]. For cases where the violent felony is considered extremely egregious, e.g. Murder, Homicide, Manslaughter, all subsections/similar offenses are also deemed violent within the legal landscape. While, other violent felonies such as burglary, sex offenses and carjacking have ample comparable non-violent felony substitutes statute codes. We plan to further measure treatment effects within this setting, by defining a dosage of treatment based on the ratio of non-violent felony substitutes for a given violent felony statute code. To do this, we simply measure the number of non-violent arrest to violent filed charge/court action co-occurrences to create a scale of treatment. Further, we create a binary variable describing the likelihood of an offense being most or least substituted by a median split, non-violent felony offense codes that have equal to or above the median number of substituted violent felonies are considered more treated while non-violent felony codes with at least one substitute but less than the median number of violent felony substitutes are considered less treated. We plan to re-define treatment utilizing this dosage design to explore treatment effects across a threshold of most and least treated in order to take a closer look at the distribution of weight within our average treatment effect.

2.4.3 Identification Strategy

An imperative check within the framework of this project is to ensure channels other than prosecutor decision-making are not changing as a function of the reform. Namely, as prosecution is a link within a chain of processes in the life cycle of an offense, we must ensure previous steps in the cycle are not also being impacted by the legislation. We believe the

[46] Figure 11 in the appendix contains a subset list of more and less treated offenses categorized by offense categories listed in CORI.

two actors of importance in satisfying this exclusion restriction are offenders and arresting officers. First, we must ensure that offenders are not impacted by the reform as a function of changing perceived risks in committing future crime through reduced penalty, i.e. reduction in deterrence effects. Second, we must ensure arresting officers are not 1) changing the types of offenders they arrest after learning about the policy and/or 2) changing the suggested charges brought forth due to the reform. Though we cannot formally test effects that the policy has on offender decision-making, we proxy for changes in criminal activity in two-folds. First through changes in total arrests, if we believe arrests serve as a function of ongoing criminal activity, we propose the number of arrests would only deviate if more offenses occur or if there is a substantial change in funding or grant availability that would directly alter the number of patrolling officers within arresting agencies (see Tables 3 & 4). Second, we leverage the granularity of the CalDOJ data being at the unique offender-action level to compare arrest trends across all non-violent felony charge codes that were ever substituted to a violent felony court action[47]. As shown in figures 5 and 6, visually showcasing the trends does not allow for a thorough test on if there are any statistical differences in arrest trends pre and post the legislation for non-violent arrests with violent felony substitutes. To further test if our exclusion restriction is violated we run a piecewise regression that compares the arrest trends before and after the policy shock. This approach, though similar to the mean-difference test, is more suited to address observational trends or serial correlation that may influence a simple mean-difference test. While a simple mean-difference test will check if the average number of arrests differs before and after the policy, a piecewise regression will measure if the slope of observations before and after are statistically different. Specifically, the mean-difference test will reject the null hypothesis if there is already a trend approaching upwards or downwards, concluding that the policy incurred a change in the trend. However, the piecewise regression will take the entire trend into consideration. Tables 5 and 6 and

[47]We discuss methods applied to construct this charge set within the methods section.

figure 7 present the subset of piecewise regression slope checks that indicate significant differences in arrests across (1) first pass non-violent felony arrests with a violent felony court action and (2) second pass non-violent felony arrest group with a violent felony court action. Indeed, we do find a number of our statute codes are statistically different in the pre and post periods though we do not know exactly why this is true. It could be that the policy is directly impacting these offenders or arresting behavior, or it could be that there are other unobserved changes that influence this change. The benefit of evaluating such granular data is that we can run our analysis with all observations and then omit statistically significant changes in arrest trends as a robustness check, which we plan to do in future iterations of this research.

Table 5: Statistically Significant Non-violent Felony Arrest Trends Changing after Legislation

Graph index	Statute Code	P-value	t-stat
6	11357(A) HS	0.0159*	6.9332
7	11358 HS	0.000*	49.1189
18	186.22(B)(1) PC	0.034*	5.1807
27	243.4(A) PC	0.0185*	6.5749
37	261 PC	0.0118*	8.7954
61	368(D) PC	0.006*	9.4571
75	487(A) PC	0.0301*	5.4494
80	496(A) PC	0.0114*	7.7629
87	550(A)(1) PC	0.008*	8.6872

Note: Column one represents the graph index on the specified charge code, while column two is the arrest statute code, columns four and five present the associated p-value and t statistics computed from the test. Any p-values containing an * signify the slope of arrests in the pre period is statistically different to the slope of arrests in the post period at the 0.05 significance level. These significant statute codes are omitted within our robustness checks.

Table 6: Statistically Significant Non-violent Felony Arrest Charge Set Trends Changing after Legislation

Graph index	Offense Categories	P-value	t-stat
15	FAILURE TO APPEAR;PAROLE VIOLATION;PERJURY	0.0098*	8.2344
16	FALSE IMPRISONMENT	0.0408*	5.08
18	FAMILY OFFENSE;TERRORIST THREATS	0.0101*	10.505

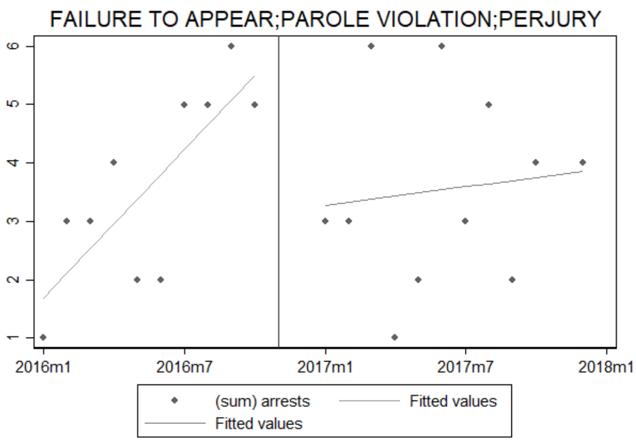
Note: Column one represents the graph index on the specified charge code, while column two is the arrest statute code, columns four and five present the associated p-value and F statistics computed from the test. Any p-values containing an * signify the slope of arrests in the pre period is statistically different to the slope of arrests in the post period at the 0.05 significance level. These significant statute codes are omitted within our robustness checks.

When conducting a Difference-in-Differences (Diff-in-Diff) analysis, there are two identifying assumptions that must be satisfied for your results to be treated within a causal framework. First, we must satisfy Stable Unit Treatment Variable Assignment (SUTVA) which requires (1) the likelihood of a unit being treated is not impacted or influenced by the likelihood of another unit's treatment assignment and (2) there is no spillover in treatment effects. In the typical Diff-in-Diff setting, treatment and control units are defined across geographical locations. Some locations realize the policy shock while other locations, similar on observables, are not treated with the new policy. Within this landscape, examples of each SUTVA violations is (1) a county's treatment take-up is dependent on the outcomes of a similar county's treatment effects suppose from a pilot program or (2) the treatment of one county will have spillover if the residents of the treated location migrate in or out of the county to neighboring areas and impact outcomes within those non-treated locations as a consequence of the new policy.

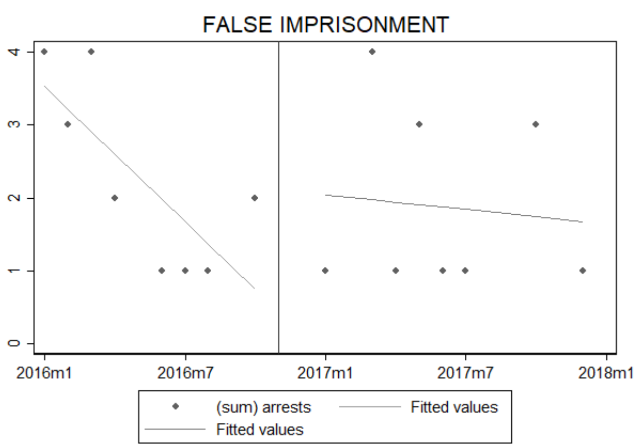
Defining SUTVA in this setting is substantially different from the typical diff-in-diff setting. The first channel of SUTVA can be satisfied so long as treatment of non-violent substitutable statutes does not impact the treatment status of other statute codes. Given the legislation directly impacts violent felonies by excluding these offenses from post-incarceration

Figure 7: Case 2: Piecewise Arrest Trends

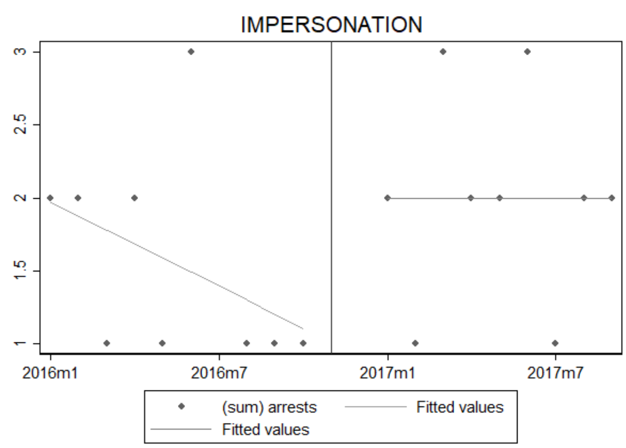
(a) 15



(b) 16



(c) 18



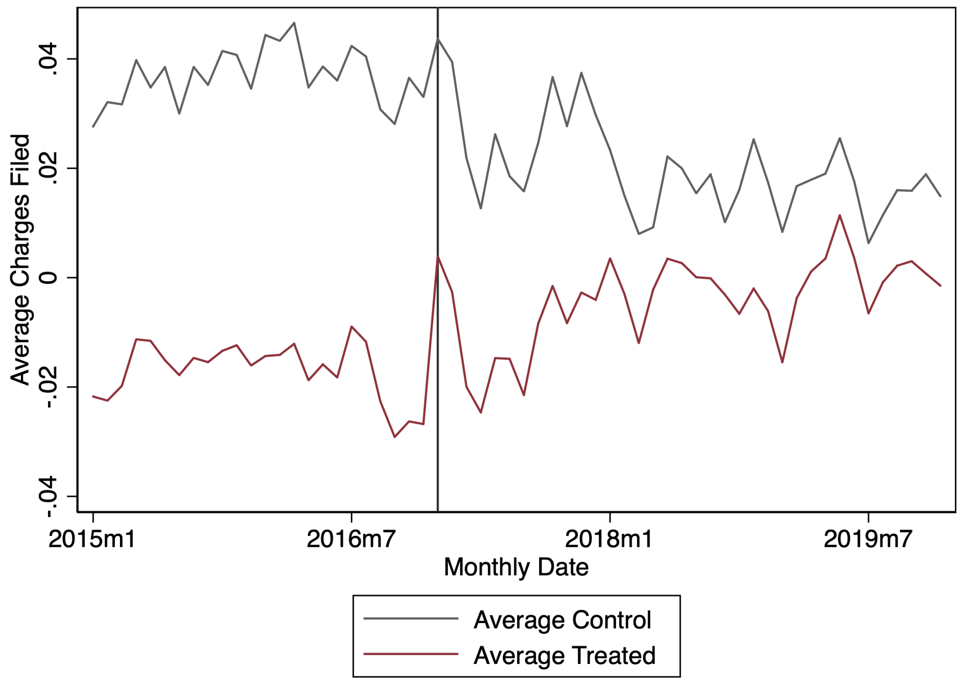
sentence reductions, violent felonies are considered treated within this setting. Non-violent felonies with no chance of substitution cannot be treated as a control given our hypothesis predicting these offenses will retain the least case value, leading to a reduction in dismissals. The only set of statute codes we believe has no plausible impact or violation of SUTVA are misdemeanor offenses, since there is no substitution or impact of the legislation on offenses that result in an incarceration outside of state prison [48]. Though we believe there may be correlation across types of offense categories that are substituted, we do not know of any reason to believe one charge code having substitution effects impacts the chances of another code being substituted. Of the two assumptions to satisfy, we are more likely to violate the latter. One way to violate spillover within this setting is if the treatment of one non-violent felony statute being substituted to a violent felony influences the way filing prosecutors treat other statute codes as substitutable. Our theory quite explicitly violates this assumption if we utilize any non-violent or violent statute as our uncontaminated comparison group.

The parallel trends assumptions is satisfied if trends on the outcome variable, i.e. choosing to file or dismiss, across treatment and control non-violent felony groups are parallel within the pre-legislation period, that is, no significant convergence or divergence is realized. Given the treatment and control look similar prior to the legislation, therefore satisfying parallel trends, any observed shifts in treatment group trends after the legislation can be compared to a counterfactual trend in a state of the world where the legislation did not occur. This counterfactual trend shares the slope of the control group within the post-legislation period. We are able to superimpose the control group's slope due in fact to the treatment and control groups sharing "parallel" trends within the pre legislation period. We fist establish proof of satisfying this assumption within figure 1 across state-wide data. We observe both non-violent and violent felony charges following the same trends within the pre-legislation

[48] An exception to this are "wobbler" offenses that can be charged as either felony or misdemeanor, we exclude these statutes as a robustness check.

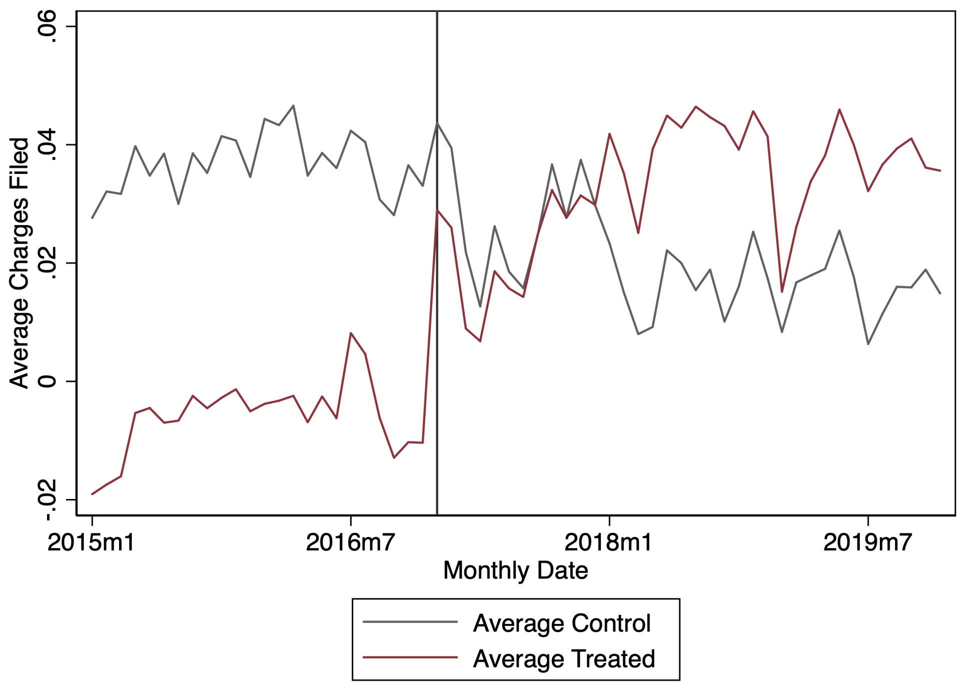
with a substantial divergence in the post-period. We do note that, as anticipated, there is a lag in the divergence which we explain as a result of internal lag in prosecutor's learning on the new legislation. Our second proof of satisfying parallel trends assumption is within County A in figure 8. We show that the trends for filing non-violent substitutable felonies relative to misdemeanor offenses remain stable over time when we first censor our sample to estimate trends on case-defendants that are only arrested for non-violent felonies with a violent felony substitute or misdemeanor arrest charges. Second, we relax our sample to depict average charges files when an incoming arrest is any non-violent felony arrest relative to misdemeanor incoming arrests and observe a dramatic increase in the rate of non-violent felony filing outcomes (see figure 9). This figure is evidence that (1) filing prosecutors understand that non-violent felonies hold a greater case value than misdemeanors since some subset of non-violent felonies can be translated to a violent felony, even if this is to occur at the pre-liminary hearing or at some point later down the line or (2) we must expand our substitution definition by looking at other time periods of the data to garner a more comprehensive set of non-violent felony substitutable offenses. Both figure 8 and 9 are residualized to absorb office branch, quarter, year and crime type fixed effects.

Figure 8: Filing Outcomes in County A - Censored Sample



*Note:*This figure presents average charges filed, residualized with branch, quarter, year and crime type fixed effects. The sample is censored to describe filing decisions on non-violent felony requested arrest charges with a substituable non-violent felony treated filing charge compared to the control group of misdemeanor requested arrest charges.

Figure 9: Filing Outcomes in County A - Relaxed Sample



*Note:*This figure presents average charges filed, residualized with branch, quarter, year and crime type fixed effects. The sample is relaxed to describe filing decisions on any non-violent felony requested arrest charges defined as treated compared to the control group of misdemeanor requested arrest charges.

2.5 Research Design

Analysis Within a Large County in California

Our empirical strategy defined within the diff-in-diff setting can be described by the equation below, where our outcome Y_{it} is a dichotomous variable defined by 0 for dismissal or 1 for filed within arrest i at time t. [49] Due to the dichotomous nature of the outcome, our models will be estimated through a linear probability model (LPM). The variable $(Post)_t$ describes if an observation is in the post-legislation period or equal to one January 2017

[49]Given our observations are at the unique offender-statute-decision level, time is with respect to the filing date.

onward, equal to 0 otherwise. The $(Treated\ NVFEL\ Codes)_i$ is a dichotomous variable equal to 1 if the statute code for the specific case-defendant-statute is a non-violent felony with a violent felony substitute, 0 otherwise. Our treatment effect can be explained by $(Post * TreatedNVFELCodes)_{it}$ which is the interaction of whether an observation is in the post-legislation period and also a treated statute code for arrest/filing i on day t. Similarly, δ describes the diff-in-diff estimator, or the average treatment effect from the policy shock. Fixed effects are described by τ_t and λ_i. Time fixed effects are presented by τ_t which absorb any time-variation within years and quarters while time-invariant office characteristics are presented by λ_i. All standard errors are two-way clustered at the defendant and filing prosecutor identifier level.

The first three columns in table 7 describe estimates where treatment is defined equal to one for any non-violent felony arrest that has the potential to be upgraded to a violent felony filed charge[50]. As mentioned in the methods section, we do not observe which specific charge within the set of charges is upgraded. So in order to avoid misattribution, we focus on the pure set of non-violent felony arrests that had no other type of charge, i.e. misdemeanor or violent felony, for a case-defendant. Though this estimation strategy limits the scope of our results, it allows us to evaluate the policy in an identifiable way and provides us lower bound estimates on the treatment effect. For instance, it is indeed prevalent that an arresst with some non-violent felony charges and some violent felony charges may result in a violent felony filing as a function of substituting the charge code from the non-violent felony arrest. Another reason for censoring our treatment definition is to allow us to cleanly identify our comparison group, misdemeanor requested charges. As discussed in the methods section, both non-violent and violent felonies are considered treated statutes. We hypothesize that (1) non-violent felony charges that have a violent felony substitute will be substituted and

[50] We define a statute as having the potential to be upgraded by censoring our sample to the pre-legislation period and measuring which arrested non-violent felony charges result in a violent filed charge. We then define all charges within the total sample that match this list as treated potential.

filed, (2) non-violent felony charges with no substitute will have a diminished case-value and be dismissed and (3) violent felonies will have the greatest case-value and be prioritized [51] following Proposition 57. Therefore, our untainted comparison group within this setting is defined as misdemeanor offenses that will not be directly influenced by the policy shock. To identify validity of our comparison group we plot residualized charging trends across non-violent felony substitutes and misdemeanor requested charges over time.

The fourth through sixth columns in table 7 describe estimates where treatment is defined equal to one for non-violent felony incoming charges. We hypothesize in light of the legislation change, these non-violent felonies, though on face value have the lowest case value, the option to have the charge be upgraded later down the line of prosecutor decision-making is possible, and is certainly not possible for misdemeanors. Therefore though these charges hold less relative case value to an incoming violent offense, they hold more relative case value to an incoming misdemeanor arrest. Columns (1) and (4) present the unsaturated model, columns (2) and (5) present quarter and year fixed effects and columns (3) and (6) attach time, branch and crime type fixed effects. As discussed in the methods section, large counties contain various branches that handle varying offenses correlated to covariates such as geography, urbanization, earnings, population, and educational attainment. Coupled with that, internal policies and norms differ within a specific branch. Accounting for differences in branch type will therefore help better isolate the effect of the policy shock. All standard errors are two-way clustered at the defendant and prosecutor identifier. This is done to take into consideration correlation of offenses committed by the same individual re-appearing in the data, and likewise correlation in filing decisions made with a prosecutor across their tenure.

[51] Here, we rely on the fact that arresting charges that are requested as violent felony are likely in part due to the increased evidence and therefore have a higher chance of being filed and remaining classified as violent. An extension to the current research would be to measure the rate at which violent felony requests are downgraded to a non-violent felony in light of the legislation.

$$Y_{it} = \beta_0 + \beta_1(Post)_t + \beta_2(Treated\ NVFEL\ Codes)_i$$
$$+ \delta(Post * Treated\ NVFEL\ Codes)_{it} + \lambda_i + \tau_t + \epsilon_{it}$$

2.6 Results

The following section lays out our diff-in-diff results with time-varying and time-invariant fixed effects. Table 7 estimates the effect of Proposition 57 on filing charges across treatment and control incoming arrest statutes. As described in the methods section, defining of treatment varies slightly from the state-wide description in that a substitution is recorded when a non-violent incoming arrested charge is substituted to a violent felony filed charge.

Table 7: Estimates on the Effect of Proposition 57 on Filed Charges in County A

	Dependant Variable: Charges Filed					
	(1)	(2)	(3)	(4)	(5)	(6)
Post*Treated	-0.0292***	-0.0290***	-0.0304***	-0.0526***	-0.0527***	-0.0553***
Mean of Dependant Variable	0.935	0.935	0.935	0.942	0.942	0.942
Observations	447,497	447,497	447,490	522,687	522,687	522,682
Fixed Effects						
Time FE	-	Y	Y	-	Y	Y
Branch FE	-	-	Y	-	-	Y
Crime Type FE	-	-	Y	-	-	Y

Notes: The following table showcases difference-in-difference LPM estimates on two types of treatment. Columns (1-3) define treatment where a non-violent felony can be substituted and columns (4-6) define treatment when there is no substitution. Columns (1) and (4) contain the estimates without any fixed effects or controls, columns (2) and (5) contain time fixed effects defined by quarter and year and columns (3) and (6) contain office location fixed effects. All estimations contain two-way clustered standard errors at the defendant and review prosecutor identifier level. Statistical significance thresholds are defined by *** p<0.01, ** p<0.05, * p<0.1.

Table 7 showcase our county-wide granular analysis of the effect of Proposition 57 on prosecutor chargining behavior across offense type. Each column represents the output from a linear probability model estimation where our outcome is defined as charges filed, 1, or

not filed, 0. Models (1-3) estimate the effect of proposition 57 on filing decisions for non-violent felony offenses that have a violent felony substitute. Whereas, models (4-6) estimate the effect of proposition 57 on filing decisions for non-violent felony offenses with no violent felony substitute at the filing stage. Columns (1) and (4) depict the unsaturated base model with no fixed effects or controls, while columns (2) and (5) add time fixed effects defined by year and quarter. Finally, columns (3) and (6) depict the fully saturated model including crime type and branch fixed effects. Each model reports a statistically significant negative coefficient, though the greatest magnitude is observed in the fully saturated model. We refer to the fully saturated model for interpretation as it takes into consideration both time variant and time invariant characteristics that could influence our analysis on the legislation. Column (3) reports a coefficient of -0.304 that is statistically significant at the 99% confidence level. This coefficient represents that the likelihood of filing charges moving from 0 to 1, i.e. charges are filed, is by 3.04% less likely in the post period (2017-2019) within the treated category (non-violent felony charges with a violent felony substitute) compared to the pre period (2015-2017) and control category (misdemeanor charges). Though this estimate touts statistical significance, economic significance is lacking given that the decrease in likelihood of charges being filed is a mere 3.2% reduction from the mean. Comparatively, column (6) presents a reduction in likelihood of a charge being filed by 5.5% for treated charges in the post period compared to control charges in the pre-period, a 5.4% reduction in the mean of charges filed. Though both estimations show a reduction, breaking apart the likelihood of a charge being filed by non-violent felony substitutable and non-substitutable estimations suggests that there is a general trend to decrease time spent on handling non-violent felony charges in light of the legislation, and this finding is especially true in cases that have a slim to no chance in increasing case-value at a later point in the prosecution. One potential explanation as to why we observe a negative estimate can be gleaned from referencing figure 9. Namely, that the difference in treated and control trends in the pre-period is large, but

since the treated group charges pass over, or rather has an inflection point, in 2018 to there on after be above the number of misdemeanor charges, the gap observed in the pre-period is closed. In summation, our estimates showcase a decrease in charges for non-violent felonies with a violent felony substitute in the post period compared to the control group in the pre-period, which is further showcased in our residualized parallel trends graphs. We plan to further expand this analysis by measuring changes in filing charges for the complement of our current treatment, violent felony statutes with a non-violent felony substitute. Further analyses will test falsification, robustness and other outcome variables such as number of and types of plea offers.

2.7 Discussion

There are a number of limitations to consider within this research. First, defining treatment and control is difficult if not nearly impossible to do given the nature of the policy being wide sweeping across California. Rather than the typical assignment of untreated locations as control, a deeper analysis into the types of offense categories that are comparable is used as a control group. Using misdemeanor charges is a theoretically sound definition for a control group, due to the nature of these charges not being at risk of violating SUTVA. Practically, it is difficult to counter any argument that posits wide institutional differences in misdemeanor and non-violent felonies making them incomparable. A less clean, but perhaps a practically sound methodological approach would be to utilize extremely egregious violent felony offenses, such as murder, as a control group since we do not expect there to be any change in offender behavior, arresting behavior, or prosecutor behavior when charging these offense types. This would get to a closer definition given that the control group would compose of felony offenses, but still remains at risk of being substantially different in nature to the control group.

Other general limitations to consider is our lack of observation on plea bargaining.

Though we do observe if pleas are offered which we intend to explore in future iterations of this work, we have no way in knowing which charge of the set of charges was plead down. Additionally, we attempt to control for crime category in order to make better "apples to apples" comparisons such as comparing charging among statute codes within theft to other theft charges and not to assault charges. However this controlling crime type category is not perfect but rather a good faith effort to categorize charges within a category. Finally, the limitation that is most considerable to note within this design framework is the chance that the county being evaluated is systematically different to other counties and therefore decreases the generalizability in findings. Though, we try our best to compare covariate characteristics across California and County A, as well as average outcomes, there is no way in knowing for certain which counties this county looks most similar to. A way to test this, is to conduct propensity score matching across a matrix of covariate characteristics for each county in California for the duration of this panel. By doing so, we can then list the counties that look most similar to County A. We plan to run this exercise in a future draft of this research.

Within the policy evaluation research realm, papers are often concluded with a note on policy relevance. Suggesting policy relevance at this stage of the research seems irresponsible given the nature of the county-wide analysis being at a preliminary stage. A general policy relevance tying back to the goal of this project is to note the potential unintended consequences of enacting legislation without taking into consideration the upstream and downstream effects made. Proposition 57 is a unique landscape to evaluate this phenomena. Though this legislation was targeted to have an isolated effect on the decision-making of CDCR personnel in awarding sentence credits, our results support the notion that filing prosecutor decision-making was also altered as a result of this legislation. We conclude by positing that legislators take into consideration the transdisciplinary and dependent nature of the California Criminal Justice System when making recommendations on future policy

reforms.

2.8 Appendix

2.8.1 Violent Felony as defined in PC 667.5(c)

1. Murder or voluntary manslaughter.
2. Mayhem.
3. Rape (PC 261(a)(2) or (6); PC 262(a)(1) or (4)).
4. Sodomy (PC 286(c) or (d)).
5. Oral copulation (PC 287 (c) or (d); formerly, PC 288a).
6. Lewd or lascivious act (PC 288(a) or (b)).
7. Any felony punishable by death or imprisonment in state prison for life.
8. Any felony in which the defendant inflicts great bodily injury (PC 12022.7, 12022.8, 12022.9; formerly, PC 213, 264, 461).
9. Any felony in which a firearm has been used (PC 12022.3, 12022.5, 12022.55).[52]
10. Any robbery.
11. Arson (PC 451(a) or (b)).
12. Sexual penetration (PC 289(a) or (j)).
13. Attempted murder.
14. Violation of PC 18745, 18750, 18755.

[52] In the definition, this line is grouped with the last one. For clarity, we have separated them.

15. Kidnapping.

16. Assault with the intent to commit a specified felony (PC 220).

17. Continuous sexual abuse of a child (PC 288.5).

18. Carjacking (PC 215(a)).

19. Rape, spousal rape, or sexual penetration, in concert (PC 264.1).

20. Extortion (PC 518) in felony violation of PC 186.22.

21. Threats to victims or witnesses (PC 136.1) in felony violation of PC 186.22.

22. Any burglary of the first degree (PC 460) in which another person, other than the accomplice, was present.

23. Any violation of PC 12022.53.

24. A violation of PC 11418(b) or (c).

2.8.2 Serious Felony as defined in PC1192.7(c)

1. Murder or voluntary manslaughter

2. mayhem

3. rape

4. sodomy by force, violence, duress, menace, threat of great bodily injury, or fear of immediate and unlawful bodily injury on the victim or another person

5. oral copulation by force, violence, duress, menace, threat of great bodily injury, or fear of immediate and unlawful bodily injury on the victim or another person

6. lewd or lascivious act on a child under 14 years of age
7. any felony punishable by death or imprisonment in the state prison for life
8. any felony in which the defendant personally inflicts great bodily injury on any person, other than an accomplice, or any felony in which the defendant personally uses a firearm
9. attempted murder
10. assault with intent to commit rape or robbery
11. assault with a deadly weapon or instrument on a peace officer
12. assault by a life prisoner on a noninmate
13. assault with a deadly weapon by an inmate
14. arson
15. exploding a destructive device or any explosive with intent to injure
16. exploding a destructive device or any explosive causing bodily injury, great bodily injury, or mayhem
17. exploding a destructive device or any explosive with intent to murder
18. any burglary of the first degree
19. robbery or bank robbery
20. kidnapping
21. holding of a hostage by a person confined in a state prison

22. attempt to commit a felony punishable by death or imprisonment in the state prison for life

23. any felony in which the defendant personally used a dangerous or deadly weapon

24. selling, furnishing, administering, giving, or offering to sell, furnish, administer, or give to a minor any heroin, cocaine, phencyclidine (PCP), or any methamphetamine-related drug, as described in paragraph (2) of subdivision (d) of Section 11055 of the Health and Safety Code, or any of the precursors of methamphetamines, as described in subparagraph (A) of paragraph (1) of subdivision (f) of Section 11055 or subdivision (a) of Section 11100 of the Health and Safety Code

25. any violation of subdivision (a) of Section 289 where the act is accomplished against the victim's will by force, violence, duress, menace, or fear of immediate and unlawful bodily injury on the victim or another person

26. grand theft involving a firearm

27. carjacking

28. any felony offense, which would also constitute a felony violation of Section 186.22

29. assault with the intent to commit mayhem, rape, sodomy, or oral copulation, in violation of Section 220

30. throwing acid or flammable substances, in violation of Section 244

31. assault with a deadly weapon, firearm, machinegun, assault weapon, or semiautomatic firearm or assault on a peace officer or firefighter, in violation of Section 245

32. assault with a deadly weapon against a public transit employee, custodial officer, or school employee, in violation of Section 245.2, 245.3, or 245.5

33. discharge of a firearm at an inhabited dwelling, vehicle, or aircraft, in violation of Section 246

34. commission of rape or sexual penetration in concert with another person, in violation of Section 264.1

35. continuous sexual abuse of a child, in violation of Section 288.5

36. shooting from a vehicle, in violation of subdivision (c) or (d) of Section 26100;

37. intimidation of victims or witnesses, in violation of Section 136.1

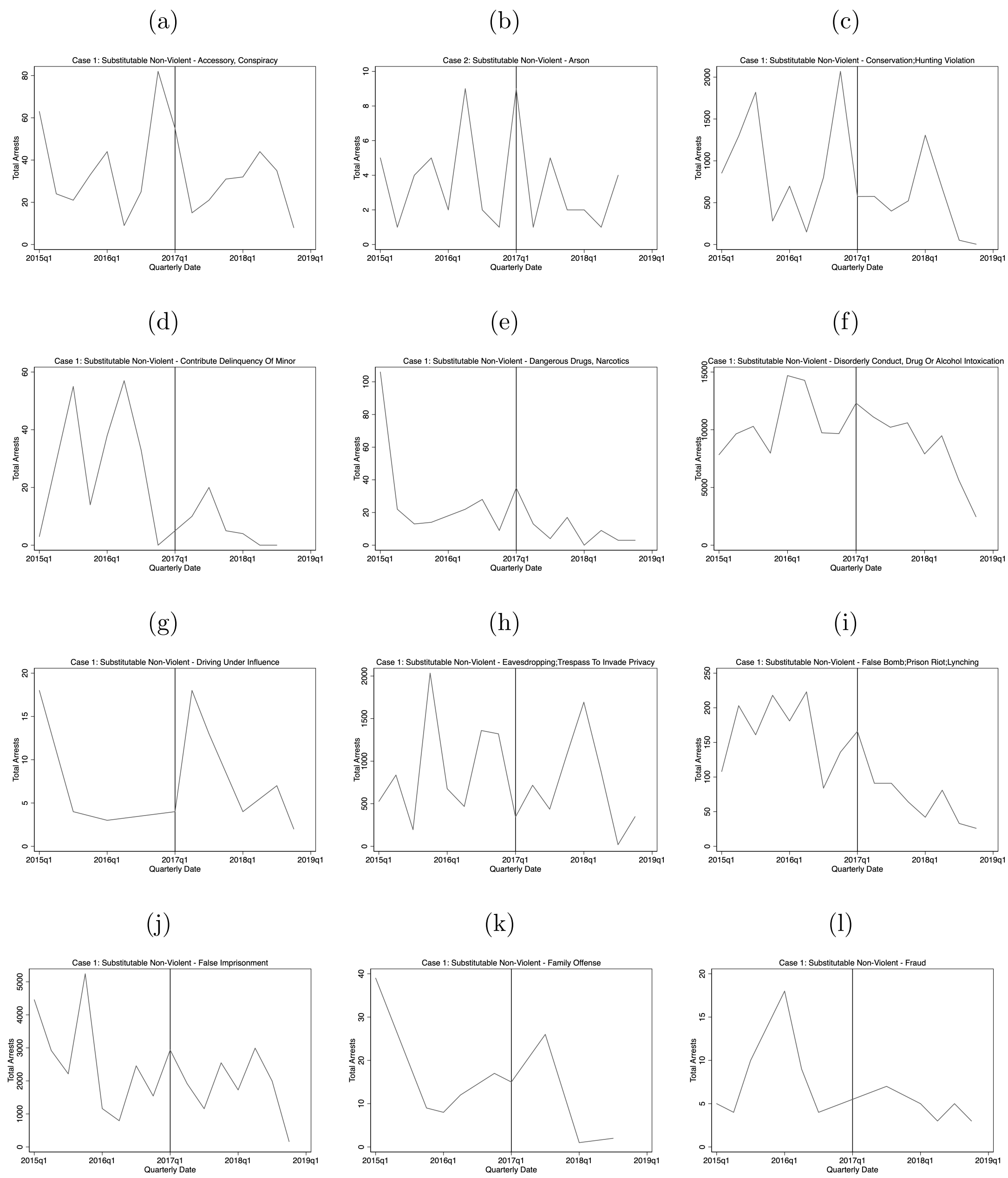
38. criminal threats, in violation of Section 422

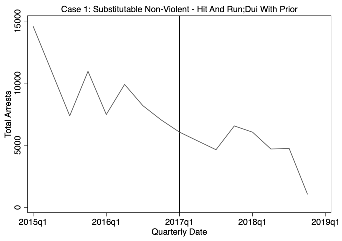
39. any attempt to commit a crime listed in this subdivision other than an assault

40. any violation of Section 12022.53; (41) a violation of subdivision (b) or (c) of Section 11418

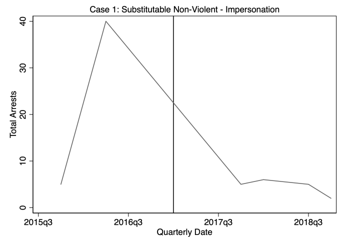
41. any conspiracy to commit an offense described in this subdivision.

Figure 10: First Pass: Non-violent Arrests Substituted to Violent Felony Court Action(s) Extended

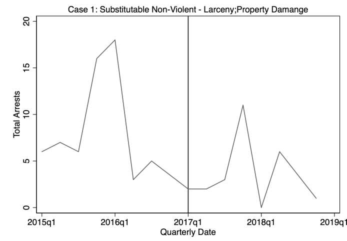




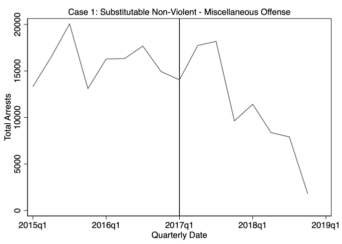
(m)



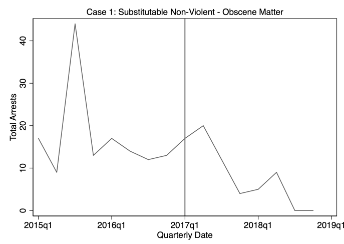
(n)



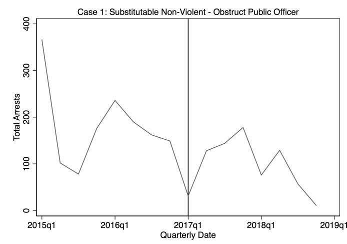
(o)



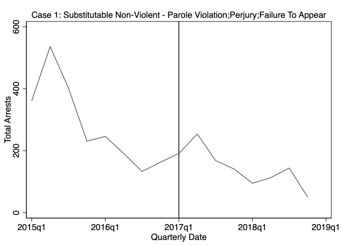
(p)



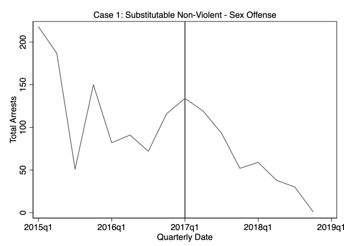
(q)



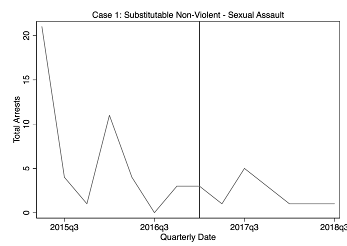
(r)



(s)

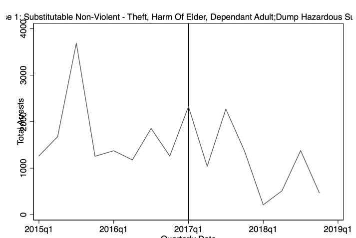


(t)

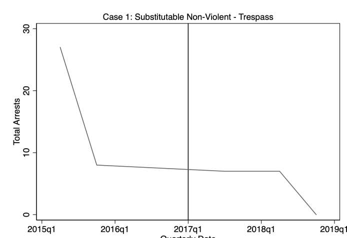


(u)

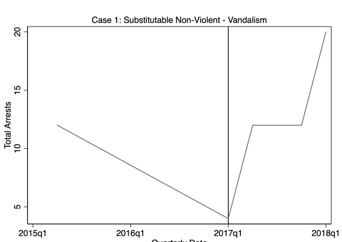
(v)



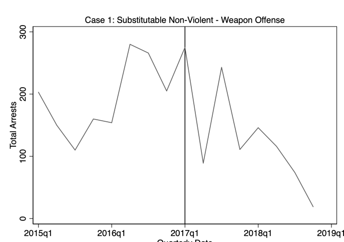
(w)



(x)



(y)



(z)

Table 8: First Pass Piecewise Regression Arrest Trend Test

Graph index	Statute Code	P-value	t-stat
1	10851 VC	0.1521	2.2173
2	10851(A) VC	0.301	1.1274
3	11350(A) HS	0.1346	2.4322
4	11351 HS	0.6138	0.2629
5	11352(A) HS	0.9645	0.002
6	11357(A) HS	0.0159*	6.9332
7	11358 HS	0*	49.1189
8	11359 HS	0.3109	1.081
9	11360(A) HS	0.0659	3.7856
10	11377(A) HS	0.0753	3.5207
11	11378 HS	0.0962	3.0467
12	11379(A) HS	0.1973	1.7788
13	118(A) PC	0.5469	0.3756
14	136.1(C)(1)PC	0.145	2.3007
15	166(C)(4) PC	0.0628	3.8822
16	182 PC	0.968	0.0017
17	186.22(A) PC	0.2857	1.2034
18	186.22(B)(1) PC	0.034*	5.1807
19	21310 PC	0.2242	1.573
20	215 PC	0.8969	0.0172
21	22210 PC	0.2975	1.1441
22	236 PC	0.9688	0.0016
23	236.1(A) PC	0.3282	1.0045
24	242 PC	0.5599	0.3516
25	243(D) PC	0.5905	0.299
26	243.4 PC	0.9497	0.0042
27	243.4(A) PC	0.0185*	6.5749
28	245(A)(1) PC	0.5786	0.3188
29	245(A)(2) PC	0.8925	0.0187
30	245(A)(4) PC	0.0591	4.0053
31	245(B) PC	0.4358	0.6324
32	245(C) PC	0.8491	0.0372
33	246 PC	0.7336	0.1191

Graph index	Statute Code	P-value	t-stat
34	246.3(A) PC	0.9895	0.0002
35	25400(A)(1) PC	0.1622	2.1065
36	25850(A) PC	0.5457	0.3778
37	261 PC	0.0118*	8.7954
38	261(A)(1) PC	0.7347	0.1181
39	261.5(A) PC	0.4587	0.5708
40	261.5(C) PC	0.6984	0.1545
41	261.5(D) PC	0.4565	0.5767
42	273.5 PC	0.1558	2.1757
43	273.5(A) PC	0.6949	0.1584
44	273.5(F)(1) PC	0.0996	2.9812
45	273A(A) PC	0.5487	0.3721
46	2800.2 VC	0.3901	0.7717
47	2800.2(A) VC	0.1161	2.698
48	286(A) PC	0.4343	0.638
49	286(B)(1) PC	0.8996	0.0163
50	288 PC	0.8626	0.0312
51	288(C)(1) PC	0.8509	0.0362
52	288.2(A) PC	0.1517	2.2219
53	288A(B)(1) PC	0.9714	0.0013
54	288A(B)(2) PC	0.3478	0.9243
55	289(B) PC	0.8143	0.0567
56	29800(A)(1) PC	0.8885	0.0202
57	30305(A)(1) PC	0.148	2.2646
58	3056 PC	0.863	0.0305
59	32 PC	0.8026	0.0642
60	368(B)(1) PC	0.0878	3.2207
61	368(D) PC	0.006*	9.4571
62	422 PC	0.2889	1.187
63	422(A) PC	0.9674	0.0017
64	451 PC	0.9539	0.0034
65	451(C) PC	0.0892	3.1909
66	451(D) PC	0.1993	1.7621
67	452(B) PC	0.5025	0.4663

Graph index	Statute Code	P-value	t-stat
68	4573 PC	0.3383	0.9624
69	4573.6 PC	0.629	0.2407
70	4573.8 PC	0.6404	0.2249
71	459 PC	0.463	0.56
72	460(B) PC	0.8602	0.0318
73	470(A) PC	0.7146	0.1376
74	487 PC	0.6269	0.2438
75	487(A) PC	0.0301*	5.4494
76	487(C) PC	0.139	2.3743
77	487(D)(1) PC	0.7817	0.0788
78	487(D)(2) PC	0.7708	0.0872
79	496 PC	0.4132	0.6983
80	496(A) PC	0.0114*	7.7629
81	496(D) PC	0.8428	0.0404
82	496D(A) PC	0.8253	0.05
83	529(A)(3) PC	0.3102	1.0843
84	530.5 PC	0.893	0.0186
85	530.5(A) PC	0.9593	0.0027
86	532(A) PC	0.9644	0.002
87	550(A)(1) PC	0.008*	8.6872
88	591 PC	0.3999	0.7399
89	594(A) PC	0.9768	0.0009
90	594(A)(1) PC	0.6282	0.2419
91	594(B)(1) PC	0.1743	1.9846
92	646.9(A) PC	0.7626	0.0938
93	646.9(B) PC	0.726	0.1264
94	69 PC	0.9778	0.0008
95	978.5 PC	0.1256	2.5553

Note: Any p-values containing an * signify the slope of arrests in the pre period is statistically different to the slope of arrests in the post period at the 0.05 significance level. These significant statute codes are omitted within our robustness checks.

Table 9: Second Pass Piecewise Regression Arrest Trend Test

Graph index	Offense Category	P-value	t-stat
1	ACCESSORY/CONSPIRACY	0.4658	0.5621
2	ARSON	0.1787	2.0397
3	ASSAULT	0.7807	0.0796
4	ASSAULT;FAMILY OFFENSE	0.3213	1.2107
5	BRING CONTROL SUBST./ALC./NARCOTIC IN PRISON	0.0619	5.2465
6	BURGLARY	0.9428	0.0053
7	BURGLARY;FAILURE TO APPEAR;PAROLE VIOLATION;PERJURY	0.1681	2.4557
8	BURGLARY;LARCENY;PROPERTY DAMANGE	0.0688	5.3407
9	BURGLARY;STOLEN PROPERTY	0.7628	0.095
10	BURGLARY;STOLEN VEHICLE	0.9967	0
11	BURGLARY;VANDALISM	.	.
12	DANGEROUS DRUGS/NARCOTICS	0.1954	1.7945
13	DANGEROUS DRUGS/NARCOTICS;DANGEROUS DRUGS/NARCOTICS	0.37	0.9694
14	DUMP HAZARDOUS SUBSTANCE;THEFT/HARM OF ELDER/DEPENDANT ADULT	0.7624	0.0953
15	FAILURE TO APPEAR;PAROLE VIOLATION;PERJURY	0.0098*	8.2344
16	FALSE IMPRISONMENT	0.0408*	5.08
17	FAMILY OFFENSE	0.2022	1.7389
18	FAMILY OFFENSE;TERRORIST THREATS	0.0101*	10.505
19	IMPERSONATION	0.246	1.4573
20	LARCENY;PROPERTY DAMANGE	0.5754	0.3242
21	MISCELLANEOUS OFFENSE	0.4378	0.6379
22	OBSTRUCT PUBLIC OFFICER	0.1629	2.1696
23	SEX OFFENSE	0.4751	0.53
24	SEXUAL ASSAULT	0.9663	0.0018
25	STOLEN PROPERTY	0.1147	2.7209
26	STOLEN VEHICLE	0.8725	0.0267
27	TERRORIST THREATS	0.6356	0.2315
28	VANDALISM	0.1697	2.1902
29	WEAPON OFFENSE	0.0662	3.7746

Note: Any p-values containing an * signify the slope of arrests in the pre period is statistically different to the slope of arrests in the post period at the 0.05 significance level. These significant statute codes are omitted within our robustness checks.

Figure 11: Subset of Less and More Effected Offense Types

Less Affected Offenses

1. 09 - Homicide, Murder, Manslaughter
2. 13 - Larceny
3. 39 - Gambling
4. 52 - Weapons offenses

More Affected Offenses

1. 10 - Kidnapping
2. 11 - Rape
3. 22 - Burglary
4. 12 - Robbery
5. 36 - Sex Offenses (sodomy, L&L, etc.)
6. 23 - Carjacking

3 Chapter 3: Deterrence, Norms and the Enforcement of Laws

Coauthored with Greg DeAngelo and Rustam Romaniuc

3.1 Introduction

Law is traditionally defined as a set of formal rules, promulgated by legislatures, regulatory agencies, and courts, and that is backed by the threat of monetary punishment or imprisonment (Raz, 1972). However, rules of conduct can also be informal insofar as they do not depend on government for either their promulgation or enforcement. When norm-enforcement results in the refusal to interact with the offender or in the expression of disapproval of one's actions, for example, behavior is considered to be influenced by informal norms. As Ellickson noted, "in the mid-1990s norms became one of the hottest topics in the legal academy" (Ellickson, 1998). The quantity and the quality of published papers on this topic rose significantly, as evidenced by the development of an area of research referred to as the law-and-economics of norms (Feldman and Perez, 2009) and by the attention given to the topic in prominent law journals.[1]

The increased interest in the interplay between norms and laws resulted in new empirical studies that complemented earlier qualitative research showing that formal enforcement of law is not independent of social norms (Westley, 1953; Bittner, 1967; Hunt, 1985; Engel, 2021; Herman and Pogarsky, 2021; Loughran et al., 2021; DeAngelo and Charness, 2012; DeAngelo

[1]In the second half of the 1990s, there have been at least eight major symposium issues on the subject of laws or formal rules and norms. Symposium, Law, Economics, and Norms, 144 University of Pennsylvania Law Review (1996); Symposium, Law and Society & Law and Economics, 375 Wisconsin Law Review (1997); Symposium, The Nature and Sources, Formal and Informal, of Law, 82 Cornell Law Review (1997); Symposium, Social Norms, Social Meaning, and the Economic Analysis of Law, 27O Journal of Legal Studies (1998); Symposium, Corporate Law and Social Norms, 99 Columbia Law Review (1999); Symposium, The Legal Construction of Norms, 86 Virginia Law Review (2000); Symposium, Norms, Law, and Order in the City, 34 Law and Society Review (2000); Symposium, New and Critical Approaches to Law and Economics: Part II, Norms Theory, 79 Oregon Law Review (2000).

and Gee, 2020). However, much of the research related to the implications of norms on law enforcement decisions focus on criminal actors.[2], while the factors that impact criminal context are often influenced by criminal justice system actors (Bittner, 1970; Goldstein, 1990). Indeed, it could be the case that system actors, such as police and prosecution, are impacted by expressions of social norms (e.g., demonstrations such as the Black Lives Matter movement impacting arresting or charging decisions). This article aims to study whether law enforcement actors are influenced by prevailing norms. Furthermore, this is one of the rare studies that explores empirically the specific behavioral responses of law enforcement agents *inside* and *outside* the communities that have social norms misaligned with prevailing laws. Prevailing norms in one community could indeed affect law enforcement within that specific location but also in the nearby areas that have not expressed a preference to not enforce certain laws.

To study the interplay between norms and the enforcement of laws by system actors, we focus on the impact of the adoption of a low priority initiative (LPI) on police and prosecutor behavior in Los Angeles County that was adopted in June 2006. LPIs are local mandates which state that police should make the enforcement of minor marijuana offenses the "lowest enforcement priority." Within Los Angeles County, two jurisdictions adopted such initiatives – Santa Monica and West Hollywood. While local policy-makers and voters may want such a policy in place, this does not necessarily mean law enforcement and prosecutors do not have their own views on marijuana, or other biases, that may cause them to act in contrast to the norms of the public. The LPI passed by city councils is a non-legally binding resolution. Therefore, their enforcement greatly depends on the law enforcement agents' incentives to comply with the initiative in the particular jurisdictions where it has been enacted while continuing to enforce minor marijuana possession offenses in areas that did not enact such initiatives.

[2]See Charness and DeAngelo (2018) for a review of this literature.

Given that there is no legal retribution to a law enforcement officer for not complying with the resolution, LPIs are equivalent to what the law and norms literature call expressive policies (McAdams, 2015).[3] There is evidence that such expressive policies can deter antisocial conduct because they are expressions of social approval or disapproval (Feldman and Perez, 2009). However, the existing empirical evidence has mostly focused on criminal actors' behavior in response to norms that may conflict with the prevailing laws. The rare studies that sought to understand the influence of norms on law enforcement agents' behavior are mainly qualitative (Westley, 1953; Bittner, 1967; Hunt, 1985) or theory-based (Kahan, 1999). LPIs may offer new insights into the interaction between laws and norms because they are the result of local voters' and politicians' preferences when it comes to arrests and charges associated with marijuana possession. However, the legal actors who make arrests and decide about charges are not bound by low priority laws. Therefore, LPIs can be seen as an indicator of preferences of the local jurisdiction, with no formal mechanism through which it can hold the relevant legal actors accountable.[4]

There are various factors that could explain the legal actors' decision to comply (or not) with locally enacted LPIs. The willingness to seek social approval in human beings has been shown to play an important role in our societies (Veblen, 1889; Elster, 1989; Bicchieri, 2005) and may be one factor explaining why police officers/management may comply with local LPIs. In the case of police officers, they may seek approval of members of the community. Indeed, a City Council's decision to enact an LPI is most likely the expression of the preferences of its constituents. If police officers want to guarantee that their actions are in line with the local community's preferences, as argued by Nix et al. (2020), then this may increase police's compliance with the LPI. As shown in Rengifo et al. (2018) using survey

[3]We discuss potential financial fallout for the law enforcement agency in section 2.3.

[4]LPIs are voted by City Councils composed of council members who act as a legislative body, proposing bills, holding votes, and passing laws to help govern the city. As council members act on behalf of voters, low priority laws can be seen as an indicator of preferences of the local jurisdiction and therefore as an expression of local norms.

data, the more individuals see the police as aligned with their own values, the more they are willing to support and cooperate with them. The Sheriff's decision can also be affected by a desire to obtain approval from local communities, as this is an elected position. Furthermore, the Sheriff's department may have pressure to comply with local norms to obtain renewal of their contractual relationship with a municipality that enacted an LPI. Indeed, the municipality may decide to provide police services internally in case it is dissatisfied with the Sheriff's department services. Similarly, the District Attorney serves an elected position which may introduce pressure to align charging practices with the local norms of the community. Though a major difference between the Sheriff's and District Attorney's offices are the way in which actors interact within the community. Officers are typically assigned to geographically designated locations known as beats for patrolling and therefore have a greater likelihood of being aware of the local level community norms and preferences when making citations. Conversely, cases brought forth from the Sheriff's Department to the District Attorney's office are handled in a centralized fashion where cases from all cities within a county are typically reviewed, filed and tried in court by the same personnel and the only specialization occurs based on offense severity.[5] These facets suggest the prosecutor's office is unlikely to differentiate cases coming from specific cities when facing a non-binding local norm given their duties align with the county of Los Angeles.

However, despite the approval-seeking factor that may explain law enforcement agents' compliance with low priority initiatives, adopting new enforcement practices may not seem natural to many police agents (on learning costs within the police, see DeAngelo and Owens (2017)). Furthermore, as has been shown by Dharmapala et al. (2016), police agents have a special taste for punishment, which could provoke resistance from law enforcement agents if they are explicitly mandated (but not by law) to not enforce specific laws. Finally, the

[5]Some big counties have separate division offices handling cases from specific regions across the county, but even in this case there is no differentiation of cases being reviewed at the granular city-level. Specialization within the DA's office is usually across horizontal and vertical cases based on the severity of the offense type.

threat from the municipality to provide police services internally if the Sheriff's department does not comply with an LPI may be perceived as non-credible.

To study whether formal law enforcement is influenced by the dominant norms within the community that the enforcement agents patrol, we examine two parts of the criminal justice system in our analysis. The first data set utilizes incident-level arrests from the Los Angeles Sheriff's Department (LASD). The second data set utilizes prosecution data obtained from the California Department of Justice, which contains information about the initial charging decisions and final disposition of all charges brought against a citizen. Prosecution is a unique setting to study the impact of local norms on system actors' behavior since there are no contractual relationships between a particular municipality and the prosecutor's office. Therefore, the prosecutor's behavior can only be impacted indirectly. First, prosecutors could choose to follow local norms by complying with LPIs, thus reducing the number of charges and increase dismissal/pleas after the implementation of an LPI. Second, there may be fewer cases arriving at the DA's office if officers are making fewer marijuana arrests.

Section 3 of this paper discusses both of these data sets. Importantly for our analysis, while LPIs were adopted in two cities in Los Angeles County - Santa Monica and West Hollywood - we focus on the latter because, as described in section 2, Santa Monica has its own police department, while West Hollywood is the only jurisdiction with an LPI that uses the law enforcement services of the LASD. Indeed, while there is uniformity in the laws that the LASD officers are employed to enforce, there are differences in local norms as to which practices are considered acceptable or not as evidenced by the enactment of the LPI in West Hollywood but not in other areas where the LASD deploys its patrol forces.

Our first set of results show that, after the introduction of the LPI, the number of misdemeanor arrests significantly rises throughout the regions of Los Angeles County that the LASD patrols with the exception of West Hollywood. To better understand the dynamics of what is driving this result, we explore heterogeneous results by the region where arrests

are being made. The LASD patrols cities that are contracted to use its law enforcement services, such as West Hollywood, and unincorporated areas that do not have their own police department and are not part of any official city. We expect that LASD officers would not adjust their behavior in unincorporated areas, as no contractual arrangement exerts outside pressure on enforcement behavior in the unincorporated areas. Alternatively in contract cities, the LASD performs policing services on behalf of the contract city and may feel pressure to alter their behavior in response to the contract city's norms. We find that there is a small effect of the LPI on misdemeanor marijuana arrests when comparing West Hollywood to neighboring contract cities, while there is a large decrease in the number of misdemeanor marijuana arrests in West Hollywood compared to unincorporated areas. Thus, it appears that the LASD especially increased their arresting behavior in unincorporated areas that neighbor West Hollywood. The norms of West Hollywood, expressed through the introduction of the LPI, activated law enforcement preferences to punish illegal behavior, especially in neighboring regions where the LASD are not engaged in a contractual arrangement.

Our second set of results examine the behavior of the prosecutor's office in response to the LPI. Unlike the LASD, which has contractual arrangements with some parts of Los Angeles County and is, by law, to perform policing services in other parts of the county, the prosecutor's office provides prosecution services to the entire county.[6] As such, the prosecutor's office does not face the same contractual pressures that the sheriff's department faces. Our difference-in-difference analysis finds that the fraction of misdemeanor marijuana charges fell, which is a product of law enforcement decisions. The rate of misdemeanor marijuana charge dismissals remain unchanged when considering all possible dismissals.[7] Lastly, the rate of plea bargaining declined, which we discuss in Section 4.2. We mea-

[6]There are a select number of cities that are out of Los Angeles Prosecutor's office jurisdiction which are excluded from the following analysis. The list of cities are Burbank, Hawthorne, Hermosa Beach, Inglewood, Long Beach, Los Angeles, Pasadena, Redondo Beach, Santa Monica and Torrance.

[7]We estimate the effects of the LPI on review prosecutor initiated case dismissals and find an increase in the rate of these dismissals in West Hollywood post LPI (Appendix Table 8)

sure robustness of these results with the application of a novel empirical strategy, synthetic difference-in-differences. When comparing our difference-in-difference estimates to our synthetic difference-in-difference estimates, we find the same magnitude and direction in our results across both methods. We discuss the underpinning methodological differences across the two methods extensively in the Robustness section.

Our study contributes to the extant literature on the interaction between social norms and laws. While previous research shed light on how the design of legal rules is influenced by the prevailing social norms (Benson, 1989; Carbonara et al., 2008, 2012, 2011) and how norms influence the enforcement of norms by system agents (Westley, 1953; Hunt, 1985; Acemoglu and Jackson, 2017) using theoretical models and qualitative studies, we extend the current analysis to an empirical investigation of how social norms affect law enforcement and prosecution practices when laws and norms are not necessarily aligned. Furthermore, we examine how norms in a specific community affect law enforcement agents' behavior in the adjacent areas, thus providing evidence on spillover effects.

Given the nature of our data, there are a number of limitations associated with our research. Social norms are typically measured in an experimental framework where researchers can identify people's beliefs about what is socially appropriate in a specific context. However, experimental studies suffer from external validity. As we attempt to identify social norms in practice, a limit of our work is that we do not measure citizen and criminal justice actors' beliefs about the appropriateness of arresting and incarcerating someone for a minor marijuana offense before and after a municipality enacted an LPI. However, given the institutional arrangements described above, it is reasonable to assume that LPIs are the expression of a community's norms that can be taken into account by police and prosecution or not depending on the system actors' preferences and constraints.

3.2 Background

Law Enforcement in Los Angeles County

Law enforcement in Los Angeles County is largely broken into three groups. First, the Los Angeles Police Department (LAPD), with its approximately 10,000 sworn officers, patrols all of the city of Los Angeles and most Metro trains and buses. Second, the Los Angeles Sheriff's Department (LASD), which also has approximately 10,000 sworn officers, patrols all unincorporated areas, contract cities, and runs the jails in Los Angeles County. Finally, there are more than 70 independent law enforcement agencies (e.g., Santa Monica Police Department) in Los Angeles County that are not associated with either the LAPD or LASD.

For the purposes of this analysis we focus exclusively on LASD since our main analysis will focus on one location (West Hollywood) under the LASD's jurisdiction that imposed an initiative requiring the LASD to change their enforcement behavior. As such, we explain the difference between patrol locations within the LASD's purview. If a jurisdiction is an officially incorporated city, then they are considered a contract city. Alternatively, if a jurisdiction is not part of an incorporated city, then it is considered an unincorporated area. Contract cities have a choice between providing their own police department (e.g., Compton) or paying for law enforcement services to be provided by the LASD (e.g., West Hollywood). Importantly, if the contract city hires the LASD to act as their enforcement agency, then a contract between the city and the LASD is drawn up. This contractual relationship could be a driving force behind enforcement behavior. Alternatively, unincorporated communities are not a part of any official city and so are governed by the Los Angeles County Board of Supervisors and have their laws enforced by the LASD. In unincorporated areas the LASD does not face any concern about losing a contract.

Examining contract cities enforced by the LASD as separate from unincorporated areas presents a fruitful opportunity to determine if social norms or law enforcement biases drive

changes in enforcement behavior. Given the social pressure that LPIs introduce for law enforcement to adjust their behavior, we can leverage contract cities versus unincorporated areas to determine the net effect of social norms and law enforcement biases in deciding how to change their behavior.

Prosecution in Los Angeles County

Unlike law enforcement in Los Angeles County, there is a single prosecutor's office with an elected district attorney that is charged with prosecuting criminal conduct. Indeed, the Los Angeles District Attorney's Office (LADA) was lead by the elected district attorney, Steve Cooley, for the entirety of our analysis.[8] There are nearly 1,000 deputy district attorneys that prosecute felony crimes throughout Los Angeles County and misdemeanor crimes in unincorporated areas of the county and in all cities except Burbank, Hawthorne, Hermosa Beach, Inglewood, Long Beach, Los Angeles, Pasadena, Redondo Beach, Santa Monica and Torrance. Thus, the prosecutor's office has jurisdiction over West Hollywood and most of the surrounding regions.

Importantly for our analysis, except in rare circumstances, the prosecutor's office is at the mercy of law enforcement agencies, as they can only make charging decisions on cases where law enforcement has made an arrest. As such, the cases that are brought to the LADA have already been impacted by law enforcement response to social norms. Since the LADA does not have any explicit contract with specific regions within Los Angeles County to perform prosecution services, the response of the office is purely to social norms and potential re-election pressure that the elected district attorney faces.

[8]Of note, Steve Cooley believed "undermining those laws via their ordinance powers is counterproductive, and quite frankly, we're ignoring them" when expressing his plan to undermine council member ordinance to lift bans on prosecuting charges related to medical marijuana dispensaries (Hoeffel, 2009).

3.2.1 Low Priority Initiatives in Los Angeles County

The low priority initiative (LPI) mandates that minor, misdemeanor marijuana possession offenses be considered the lowest priority for the law enforcement agency with jurisdiction.[9] The mandate is only intended to affect offenses where marijuana was intended for adult personal use, which does not include possession or selling of marijuana to minors. Also, the LPI only applies to the private use of marijuana. So, offenses committed on public property are not impacted by the LPI.

Santa Monica and West Hollywood were the only municipalities that adopted LPIs in Los Angeles County. Since Santa Monica has its own police department and is not prosecuted by the LADA, we cannot compare citations and prosecution behavior in Santa Monica to other locations, since the law enforcement and prosecution agencies differ.[10] Since West Hollywood is a contract city that employs the LASD for policing services and falls under the jurisdiction of LADA, we can examine the impact of the LPI in West Hollywood on changes in LASD enforcement and LADA prosecution behavior within West Hollywood and in neighboring jurisdictions that did not implement an LPI. As noted above, the relationship between the city of West Hollywood and the LASD is governed by a somewhat complicated contractual structure. On the one hand, the LASD is a law enforcement agency and the LPI is not a legal change that requires that the LASD alter their behavior.[11] On the other hand, West Hollywood could end their contract with the LASD if the agency does not honor West Hollywood's request to make misdemeanor marijuana arrests the lowest priority effort of the law enforcement agent's activities.[12] Similarly, the LPI should not impact prosecution

[9] Note that the LPI does not apply to felony drug offenses.

[10] Unfortunately, we do not have data from this jurisdiction so we cannot conduct any analyses.

[11] West Hollywood City Council has no authority to compel the LASD to enforce the LPI since it was passed by the city council as a resolution, which is not a law and is not legally binding. The city council member who proposed the resolution acknowledged this, stating that the resolution should "send a message to law enforcement that they should focus on more serious crimes."

[12] There is also a political force at work, although the effect is more indirect. Since the county sheriff is an elected position, it could be that the sheriff aims to comply with potential voter's desires to increase their likelihood of re-election.

behavior by the LADA since it does not change the laws, but to the extent that the LADA responds to social norms, we can examine this behavior.

LPIs present one way that local communities can express their desire for legal changes without going through the formal channels of changing laws. In this way, LPIs can be thought of as a social norm, and used to evaluate the effect of local norms on the decision-making actors in the criminal justice system. And while criminal justice leadership might believe that it is necessary to adhere to the norms of the community for financial and political reasons, such pressure might not impact patrol officers or line prosecutors.[13]

3.3 Data

To examine the effect of changes in local norms on the enforcement and prosecution of low-level marijuana offenses, we utilize two data sets in our analysis. The first data set utilizes incident-level arrests from the Los Angeles Sheriff's Department. The second data set utilizes prosecution data obtained from the California Department of Justice, which contains information about the final disposition of all charges brought against a citizen. We discuss both of these data sets independently.

3.3.1 Los Angeles Sheriff's Department Data

The first data set includes all arrests obtained from the Los Angeles Sheriff's Department (LASD) from 2000-2007. Since these are arrest-level data, we can identify low-level, misdemeanor marijuana offenses, which are precisely the offenses that the LPI initiative aimed to prevent from being issued. These data are also geo-coded so that we can determine the jurisdiction where the arrest occurred. Also, the police reporting district (RD) where the arrest occurred is coded into these data. Lastly, we can determine if the jurisdiction is a

[13] Indeed, Sheriff's Office officials were among the few public opponents of the LPI resolution, who noted concern regarding "inflexibility" among patrol officers.

contract city or an unincorporated community.

Importantly for our analysis, while the LASD have law enforcement jurisdiction throughout Los Angeles County, many jurisdictions have their own police force (e.g., Long Beach, Los Angeles, Pomona, and Santa Monica) and so the LASD does not patrol these regions. Given that Santa Monica has their own police department, the only jurisdiction that the LASD patrol that implemented an LPI is West Hollywood. Thus, throughout our analysis, we will only be examining jurisdictions where the LASD patrol and West Hollywood will be the only "treated" location in our analysis.

3.3.2 California Department of Justice Data

The second data set we utilize is the Criminal Offender Record Information (CORI) from 2000-2010, which is managed by the California Department of Justice (CA DOJ). The raw data are available at the individual step event unit, where individuals show up during each instance of their interaction with the criminal justice system. For instance, each individual's reason for arrest, arresting agency and case disposition after a court action is recorded. In this analysis we subset our data to Los Angeles county, aggregating data to the unique city-week level.[14] We leverage the granularity in the prosecutor data by aggregating misdemeanor marijuana offenses to the weekly-level. We conduct our analysis of prosecutor data at the weekly-level to avoid mis-attribution due to the process time lag between the time of arrest and initial intake of an arrest to the prosecutor office. While arrests occur at a specific date and time, a number of factors could impact the length of time until a case arrives at the prosecutor's office.[15] Due to the potential time delay in cases arriving at the prosecutor's

[14] The following cities are excluded from our data set as LADA does not have jurisdiction over misdemeanor offenses: Burbank, Hawthorne, Hermosa Beach, Inglewood, Long Beach, Los Angeles, Pasadena, Redondo Beach, Santa Monica and Torrance.

[15] Variance in the length of time between an arrest being made and the case arriving in the prosecutor's office could be due to many, often unobserved factors. Examples of such factors include case backlog in the prosecutor's office, shortages of prosecution staff and/or court room shortages, a delay in the time taken to collect evidence by the law enforcement agency, etc.

office, we aggregate data in an attempt to mitigate potential mis-attribution when cases actually reach to the prosecution stage.

As these data includes unique offense descriptions for an offender's arresting and charged offense type, we are able to further subset our data to identify low-level misdemeanor marijuana charges as defined by the offense description. For the purposes of this analysis, we are able to identify three important features pertaining to each arrest that the LADA would handle. First, we can construct a city-week aggregate misdemeanor marijuana charge rate, which is constructed as the number of misdemeanor marijuana charges divided by the total number of incoming misdemeanor marijuana arrests. Second, conditional on the LADA receiving suggested charges from the LASD, we construct the rate at which misdemeanor marijuana charges are dismissed. Mechanically, this is the fraction of dismissed misdemeanor marijuana charges divided by the total number of misdemeanor marijuana charges at the city-week unit of analysis. Finally, conditional on the LADA pursuing the misdemeanor marijuana charges, we construct a measure of the rate at which plea deals are reached, which is the fraction of pursued misdemeanor marijuana charges reaching a plea agreement divided by the total number of pursued misdemeanor marijuana charges.

3.3.3 Analytical Plan

Given the institutional composition of the criminal justice system in Los Angeles County, as well as the passage of the LPI, we intend to empirically examine the impact of the LPI on the behavior of institutional actors in the criminal justice system. We interpret the LPI as a reflection of the community's norms toward low-level marijuana offenses, which creates a unique environment to determine how different aspects of the criminal justice system, each with their own organizational preferences and pressures, responds to the expression of the community norm.

The LASD is the agency most directly impacted by the passage of the LPI. As noted

above, West Hollywood contracts law enforcement services from the LASD. As such, we expect that the LASD would be the criminal justice agency most likely to respond to the expression of the community's norm regarding low-level marijuana offenses by reducing the number of arrests or citations associated with low-level marijuana offenses. However, it could also be the case that law enforcement preferences could go against community norms if community norms are geared toward being less punitive, as law enforcement has shown a steadily increasing level of punitiveness (Enns, 2014).

It is our prior that prosecutor behavior is unaffected by the expression of the community norm, other than the influence of changes in arrests that result from LASD officers adjusting their behavior. Since the LADA's office is tasked with prosecuting criminal behavior throughout Los Angeles County and is not contracted to perform these services, there is no clear reason that prosecutors should adjust their behavior in response to the passage of this local initiative. In light of the anti-drug stance of the presiding District Attorney, we can expect the LADA's office to remain unchanged in their handling of low-level misdemeanor marijuana offenses in response to the LPI.

Therefore, we expect the only change in charge rates observed to be a function of changes in the volume of incoming arrests made by the LASD. Subsequently, we expect the rate of cases dismissed for low-level marijuana charges to remain unchanged after the passage of the LPI. Finally, we expect the rate at which a plea agreement is offered by a prosecutor to remain unchanged, while changes on plea up-take may be subject to change based on defense attorney's updating beliefs on the optimal strategy in response to the initiative. Further, it could be the case that defense attorneys encourage their clients to not accept plea agreements in West Hollywood because they believe that the community's preference toward low-level marijuana offenses have shifted, which could result in the defendant being found not guilty by a jury of their peers. To the extent that this belief is true, we would expect to see a reduction in the number of cases disposed of by plea agreement.

3.4 Results

3.4.1 Policing

To examine the effect of LPI's on misdemeanor marijuana arrests we build on the work of DeAngelo et al. (2018)[16], but further refine the analysis to get closer to the causal effect of the LPI. We start by identifying RDs that are fully contained within West Hollywood, where the LPI was enacted. We also identify RDs that are partially contained in West Hollywood, but partially contained in a neighboring jurisdiction. In total there are 48 RDs that are either fully or partially contained in West Hollywood, which constitute 6.5% (167,435/2,542,640) of the observations in our data. While West Hollywood is a contract city, the RDs that are partially within West Hollywood and partially in a neighboring jurisdictions include both contract cities and unincorporated areas, which we will leverage in our analysis.

Our primary outcome variable involves whether an arrest involved a misdemeanor marijuana arrest. We will analyze a standard difference-in-difference framework where we define the treatment group (*Treat*) as those arrests occurring within West Hollywood and our control group as arrests occurring in RDs that are partially contained in West Hollywood, but where the actual arrest did not occur in West Hollywood. Lastly, we will define the treatment period (*Post*) as the time period after the LPI was passed (July 1, 2006). The main specification that we utilize is described in equation 1:

$$Y_{it} = \alpha + \beta_1 LPI_i + \beta_2 Post_t + \beta_3 Post_t \times LPI_i + \delta_i + \gamma_t + \epsilon_{it}. \tag{1}$$

[16] As discussed in this section, this work deviates from the previous research in several substantive ways. First, when examining arrest data, we employ a much more refined geographic measure for the treatment. Also, the comparison group is geographically limited to reporting districts immediately adjacent to West Hollywood or, in some instances, partially contained in West Hollywood and partially outside of West Hollywood. Perhaps most substantively, we incorporate data from the Los Angeles District Attorney's Office to examine prosecution behavior in conjunction with arresting behavior. We further extend on previous research by applying a novel empirical identification strategy, synthetic difference-in-differences as discussed in the robustness section.

The coefficient of interest (β_3) is our difference-in-difference estimate that explains the effect of the LPI on misdemeanor marijuana arrests. To ensure that we are not falsely attributing unobserved time-invariant variation in RDs, we include fixed effects for RDs (δ_i) in our analysis. We also include yearly time controls (γ_t) to absorb time-varying unobserved variation across all RDs. Lastly, we include controls for time-varying RD-specific variation in the gender and racial composition of RDs, which include the average number of male, white, Black, Hispanic, and Asian residents in each RD.[17] Undoubtedly these controls act as a subset of observed factors associated with the variation in misdemeanor marijuana arrests. We believe gender and racial composition to be two of the most predictive and endogenous attributes associated with low-level unconcealed drug arrests, which have been identified in the literature (Parker and Maggard, 2005; Gaston, 2019; Mitchell and Caudy, 2015; Mauer et al., 1999). In particular, our goal in adding controls and fixed effects in the model is to better isolate the effect of LPI on low-level misdemeanor marijuana arrests by accounting for other observed variables that contribute to this relationship. While we include exhaustive time, location and demographic controls, it is still possible that unobserved factors could influence our results. For instance, if LADA's office is short staffed leading to less emphasis being made on low-level offenses at the same time as the passage of the LPI, we would not be able to capture the effects of this hypothetical reality on our estimations, leading to misattribution of the treatment effects.

Table 1 displays the average number of misdemeanor marijuana arrests for the full sample, West Hollywood, contract cities (excluding West Hollywood), and unincorporated areas before and after the introduction of the LPI.[18] We also examine the difference in misdemeanor

[17]This information is sourced from the American Community Survey data.

[18]We further capture the overall trend of crime across Los Angeles County by comparing the average number of misdemeanor arrests pre and post LPI. We find an average of 2,444 misdemeanor arrests pre LPI and 3,375 arrests post LPI across the county. We further subset our descriptive measurement to treated and untreated locations and find that West Hollywood had an average of 1,426 arrests while untreated locations had an average of 2,456 arrests pre LPI. In the post LPI period, West Hollywood has an average of 2,238 arrests and untreated locations had an average of 3,388 arrests. We further note that the increase in misdemeanor arrests in West Hollywood post LPI differs from the trend of misdemeanor marijuana arrests

marijuana arrests and perform a t-test in the third column. The number of misdemeanor marijuana arrests significantly rises after the introduction of LPI throughout the regions of Los Angeles County that the LASD patrol, however they do not significantly rise in West Hollywood. Both contract cities and unincorporated areas experience an increase in misdemeanor marijuana arrests by approximately the same amount.

Table 1: Unconditional Differences in Marijuana Arrests

	Pre	Post	Difference
Full Sample	0.016	0.026	-0.010***
	(0.125)	(0.159)	
West Hollywood	0.009	0.010	-0.001
	(0.094)	(0.101)	
Contract Cities	0.014	0.024	-0.010***
	(0.117)	(0.152)	
Unincorporated	0.020	0.031	-0.011***
	(0.141)	(0.174)	

Notes: This table displays the average number of misdemeanor marijuana arrests for the overall sample, for West Hollywood, for contract cities (excluding West Hollywood), and for unincorporated areas. The difference in misdemeanor marijuana arrests are reported in the third column and a t-test is performed on these differences. *, **, *** denote statistical significance at the 10%, 5%, and 1% levels, respectively.

While the unconditional means display interesting changes in the number of misdemeanor marijuana arrests before and after the introduction of the LPI, they could be masking unobserved variation in citizen and law enforcement behavior over time. In Table 2 we examine the effect of the LPI on misdemeanor marijuana arrests using a fixed effects OLS model. In column I we include RD fixed effects, while column II includes the gender and ethnicity controls listed above. Finally, in column III we include RD and year fixed effects as well as gender and ethnicity controls.

post LPI in the treated location as showcased in Table 2.

Table 2: Effect of Low Priority Initiatives on Misdemeanor Marijuana Arrests

	I	II	III
LPI	-0.002	-0.005	-0.005
	(0.005)	(0.006)	(0.006)
Post	0.007***	0.005*	0.006**
	(0.003)	(0.003)	(0.003)
LPI x Post	-0.007**	-0.008***	-0.008***
	(0.003)	(0.003)	(0.003)
Reporting District FEs	✓	✓	✓
Controls		✓	✓
Year FEs			✓
Mean of Dep. Var.	0.015	0.015	0.015
Number of Obs.	167,435	167,435	167,435

Notes: This table produces the results of a difference-in-difference analysis of the effect of LPI on misdemeanor marijuana arrests. The sample of observations included in this table includes all observations within reporting districts that are fully contained in West Hollywood as well as reporting districts that are partially contained in West Hollywood. *, **, *** denote statistical significance at the 10%, 5%, and 1% levels, respectively.

Since the outcome variable in this analysis is an indicator variable for whether an arrest involved a misdemeanor marijuana arrest, we are utilizing a linear probability model. The coefficient of interest (LPI $\times$ Post) is consistently statistically significant, indicating that the number of misdemeanor citations fell by approximately 50% (0.008/0.015) in West Hollywood after the passage of the LPI. As noted in DeAngelo et al. (2018), however, this result is driven by law enforcement increasing arrests for misdemeanor marijuana arrests in regions immediately adjacent to West Hollywood, which drives the main result.[19] Thus, it appears that law enforcement preferences for enforcing laws outweigh social norms to reduce the enforcement of low-level marijuana offenses.

To dissect the dynamics of what is driving our main result, we explore the relationship

[19]DeAngelo et al. (2018) examines the effect of the LPI on law enforcement behavior by distance from West Hollywood, noting that the treatment effects increase with distance from West Hollywood.

between the LASD and the region where arrests are being made. As noted above, the LASD patrols contract cities and unincorporated areas. Indeed, RDs in our control locations contain both contract cities and unincorporated areas. To determine if the new norm in West Hollywood had any impact on misdemeanor marijuana arrests elsewhere, we break our results apart by control locations that are either exclusively in contract cities or exclusively in unincorporated areas. Since the LASD may be under contractual pressure in contract cities, but no such dynamics exist in the unincorporated areas, we hypothesize that the impact of the LPI could have a different impact on each of these regions. We explore this in Table 3.

Table 3: Effect of Low Priority Laws on Misdemeanor Marijuana Offenses by Area Type

	Contract			Unincorporated		
	I	II	III	IV	V	VI
LPI	-0.004	-0.008	-0.008	0.004*	0.005**	0.005**
	(0.007)	(0.007)	(0.007)	(0.002)	(0.002)	(0.003)
Post	0.005*	0.004	0.005**	0.015***	0.013***	0.015***
	(0.003)	(0.003)	(0.002)	(0.004)	(0.003)	(0.003)
LPI x Post	-0.004	-0.006*	-0.006*	-0.014***	-0.015***	-0.015***
	(0.003)	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)
Reporting District FEs	✓	✓	✓	✓	✓	✓
Controls		✓	✓		✓	✓
Year FEs			✓			✓
Mean of Dep. Var.	0.013	0.013	0.013	0.014	0.014	0.014
Number of Obs.	141,636	141,636	141,636	82,493	82,493	82,493

Columns I-III of Table 3 examines RDs in the control set that are located in contract cities, while columns IV-VI examines RDs in the control set that are located in unincorporated areas. Once again, we conduct a linear probability model to explore the effect of the LPI on misdemeanor marijuana arrests. The difference in effects between contract cities and unincorporated areas is quite stark. There is a small and weakly significant effect of the LPI on misdemeanor marijuana arrests when comparing West Hollywood to neighboring contract

cities. However, in comparing West Hollywood to neighboring unincorporated areas, we find a large statistically significant reduction in misdemeanor marijuana arrests. Thus, it appears that the LPI had a differential impact on law enforcement behavior depending on the dynamics of the relationship between the LASD and the location where the RD was located. While the LASD appears to not have adjusted their behavior, in response to the LPI, in contract cities, it does appear that they increased their arresting behavior in unincorporated regions where no contractual relationship exists.

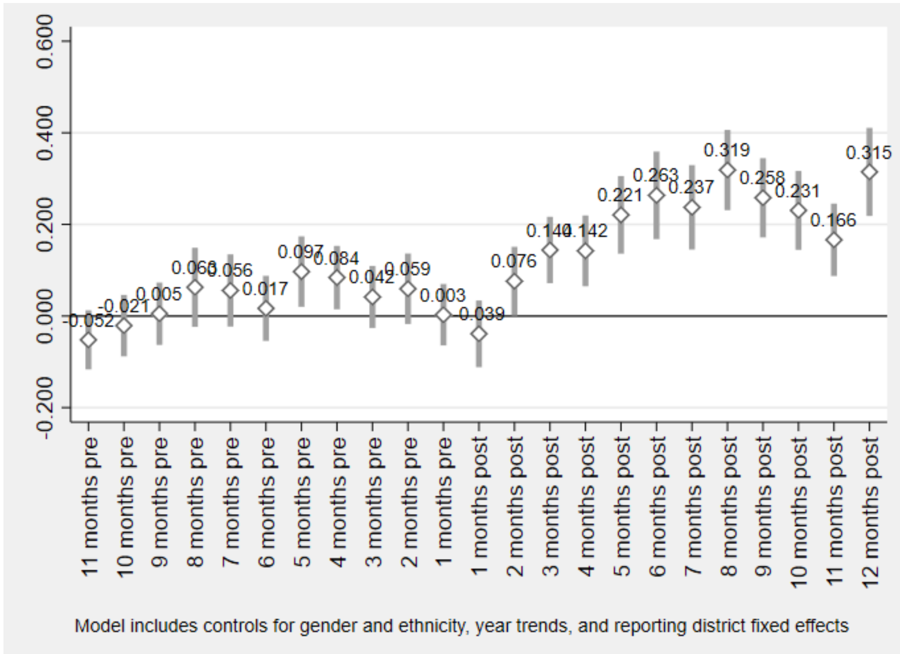
To more generally explore whether the introduction of the LPI had an effect even beyond the RDs directly surrounding West Hollywood, we broaden our analysis to examine the effect of the LPI on all of Los Angeles County. To conduct this analysis we estimate a basic event study plot with the unit of analysis at the monthly level. Specifically, we estimate Equation 2:

$$Y_{it} = \alpha + \sum_{i=-12}^{-1} \beta_i Pre_i + \sum_{j=1}^{12} \beta_j Post_j + \gamma_i + \epsilon_{it}, \tag{2}$$

where Pre_i and $Post_i$ are indicators for the 12 periods leading up to and after the passage of the LPI, respectively. We plot the county-wide monthly marijuana arrest coefficients in Figure 1, where the unit of analysis is an RD-month. It is evident that, in the aftermath of the passage of the LPI, misdemeanor marijuana arrests grew by, on average, 0.2 - 0.3 arrests per month in an RD. Given that the average number of misdemeanor marijuana arrests per RD-month is 0.56 in the pre-period, it appears that the number of misdemeanor marijuana arrests grew by approximately 50%, on average, in Los Angeles County after the passage of the LPI.

Figure 1: **Misdemeanor Marijuana Arrests**

All of Los Angeles County

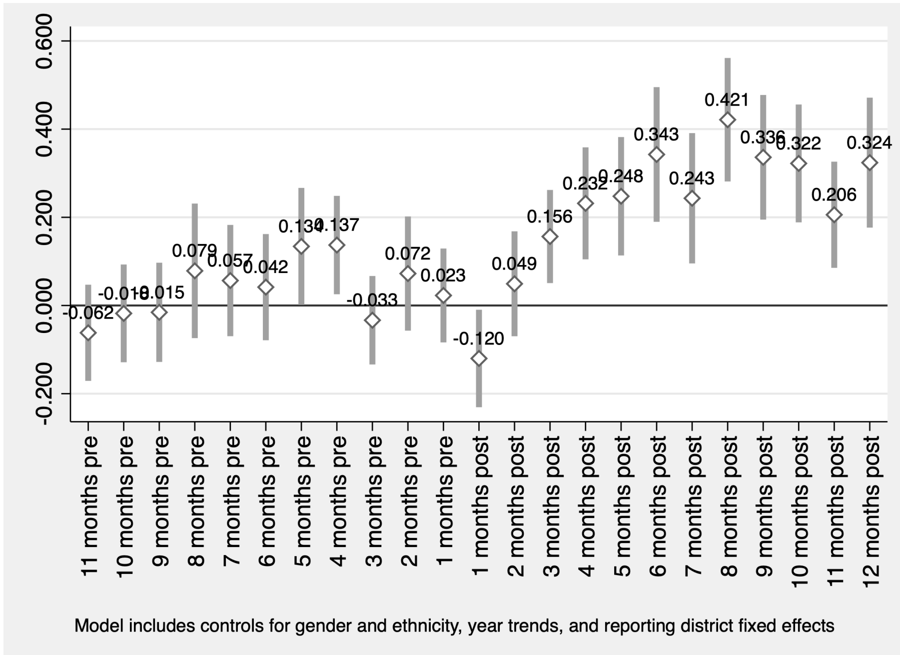


Note: Figure 1 showcases event-study coefficients on misdemeanor marijuana arrests across months pre- and post- the LPI in Los Angeles County. We omit 12 months pre, the first month of the data, to be the omitted reference time period. Standard errors are clustered at the reporting district level.

To further explore the locations that are contributing to the significant increase in misdemeanor marijuana arrests, we further explore arrest behavior by those locations where the LASD are contracted to perform policing services versus unincorporated regions where the LASD are statutorily mandated to act as the enforcement agency in Figures 2 and 3, respectively. We exclude all arrests in West Hollywood to isolate the effect of the LPI on other regions within Los Angeles County.

Figure 2: **Misdemeanor Marijuana Arrests**

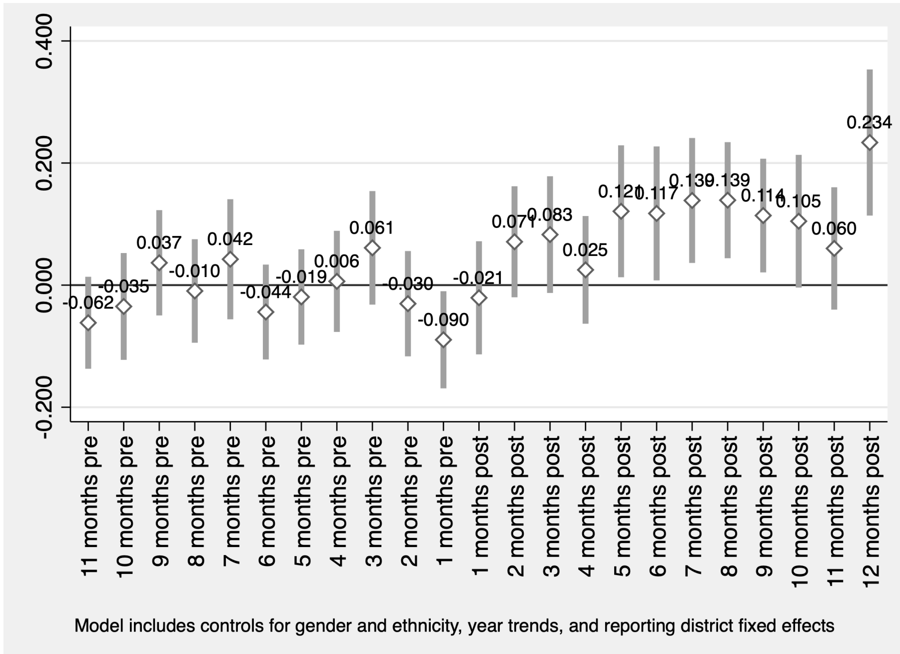
Contract Cities (excluding West Hollywood)



Note: Figure 2 showcases event-study coefficients on misdemeanor marijuana arrests across months pre- and post- the LPI in Los Angeles County, excluding West Hollywood. We omit 12 months pre, the first month of the data, to be the omitted reference time period. Standard errors are clustered at the reporting district level.

Figure 3: **Misdemeanor Marijuana Arrests**

Unincorporated Areas (excluding West Hollywood)



Note: Figure 3 showcases event-study coefficients on misdemeanor marijuana arrests across months pre- and post- the LPI in Unincorporated Areas, excluding West Hollywood. We omit 12 months pre, the first month of the data, to be the omitted reference time period. Standard errors are clustered at the reporting district level.

Interestingly, misdemeanor marijuana arrests significantly increased in contract cities, but did not significantly increase in unincorporated areas. This result is in contrast to the results presented in Table 3, but speaks to the tension between social norms, law enforcement biases, and the dynamics between contract cities and the LASD. While previous research has established that social norms could be driving law enforcement behavior (Acemoglu and Jackson (2017)), it appears that law enforcement biases and the contractual dynamics between the LASD and contract cities are a stronger driving force in explaining the changes in misdemeanor arrest behavior. As noted by the *Drug Reform Coordination Network*, law enforcement announced their opposition to being told how to perform their job.[20] Moreover,

[20]See https://stopthedrugwar.org/chronicle-old/441/westhollywood.shtml.

Sheriff Baca highlighted the importance of the LASD satisfying the terms of the relationship with each contract city. Taken together, it appears that the passage of the LPI resulted in the LASD not merely maintaining the number of misdemeanor marijuana arrests in non-LPI contract cities, but perhaps sending a signal that they would not be shirking on their contractual responsibilities.

3.4.2 Prosecution

The following section extends our previous results by examining the effect of the LPI on prosecutorial behavior. Since the LPI is intended to change policing behavior, less is known regarding the initiative's effect on altering prosecutor decisions. We now explore if changes in policing behavior extend to prosecutorial behavior by examining whether misdemeanor marijuana charges from West Hollywood are penalized more stringently compared to misdemeanor marijuana charges brought forth by other cities in Los Angeles County. As the local initiative is a non-binding reform, we hypothesize that the prosecutor's office will be unchanged in charging, dismissing and plea offering practices post LPI. Though the initiative potentially serves as a signal on community preferences, prosecutors handle a large volume of cases across many cities within the county. The nature of the prosecutor's office to handle cases in such a fashion reduces the likelihood of this low priority reform to meaningfully shift practices in the office. Furthermore, our hypothesis is informed by institutional knowledge of the presiding district attorney at the time of the initiative, Steve Cooley. As many news outlets noted, Cooley held a negative sentiment toward the legalization of marijuana dispensaries, even for medical use, as he continued to pursue cases involving dispensaries regardless of the city's lift on medical dispensaries (Hoeffel, 2009). This information, coupled with other "tough on drug crime" beliefs associated with Cooley's time in office, suggest that the district attorney's office would remain unchanged after the introduction of this non-binding legal reform.

In the prosecutorial setting, charges are brought forth by different arresting agencies across the county and prosecuted by the Los Angeles county district attorney's office. As our unit of treatment is at the city level, we aggregate cases brought forth in each city i within a given week t to be our unit of analysis. Of the cases brought forth, a select number of cases are charged, and of these cases a subset will be dismissed, plead out, or result in a conviction. We follow suit in the California Department of Justice (CA DOJ) definition of charges, dismissals and pleas when identifying these case outcomes in the CORI data.[21]

We accept that reasons why a prosecutor may charge, dismiss or offer a plea deal can be attributed to a myriad of reasons. Some reasons encompass specific offender-case-level facets, such as prior criminal history, offense severity, or the prosecutor's belief of the offender's risk to re-offend. We are less concerned with the interaction of prosecutor's priors with the LPI, as this reform is a small low-level initiative in the grand scheme of prosecutor policy reforms. To better isolate the effect of prosecutor behavior, we do not focus on estimating the change in the levels of prosecutor charges, dismissals or plea offers but rather we estimate effects on the rate of charging, dismissing and plea take-up. By estimating the effect of the LPI on the rate of misdemeanor marijuana charges/dismissals/pleas we control for the fact that incoming misdemeanor cases are a function of arrests made within that particularly city. Comparatively, the rate of misdemeanor marijuana offenses charged is a decision made by the prosecutor.

We further explore prosecutor behavior by examining dismissal and plea take up rates. Within the CalDOJ data, a total of 913 disposition codes related to each incoming arrest are recorded. Of these disposition codes, a subset are related to the arrest made, others to prosecutor decisions, and finally decisions resulting from a court disposition. A set of

[21] In the CORI data, case dispositions are organized by various disposition codes. For this analysis, we define charges with codes associated with cases that result in conviction or dismissal. Dismissals are defined as cases resulting in a review prosecutor dismissal, court dismissal, and dismissal for various reasons such as "furtherance of justice" and pleas are identified by a regular expression extraction detecting the phrase "PLEA" in the offense disposition description for an individual's case.

offense codes are attributed to dismissals conducted by review prosecutors as arrests come in the door, which we further explore in Appendix table 8. These dismissals will not include a dismissal made by other actors within the criminal justice system or dismissals made by prosecutors at later steps within a criminal case proceeding.

A plea offer is extended at the sole discretion of a prosecutor, which we also estimate in our prosecutor analysis. It is true that when aggregating from the single individual case-level to the city-week level we lose the identifying characteristics associated with the individual facing prosecution. However, we are not concerned with losing specific case-level information so long as the guiding principles a prosecutor refers to when deciding to dismiss, plea , or charge a case remain unchanged by the LPI. Further if we believe the types of offenses the DA's office attends to remain unchanged by the local initiative, any observed differences in outcomes can be associated with a shift in overall prosecution behavior in response to the LPI.

We estimate the effect of the LPI in West Hollywood by measurement on the rate of misdemeanor marijuana charges, rate of misdemeanor marijuana cases dismissed, and the rate of misdemeanor marijuana cases plead out. First, we formalize our estimation strategy through a difference-in-difference design, nearly identical to the estimation in Equation 1, where our treatment group ($Treat$) is defined by charges brought forward in the city of West Hollywood and our treatment period ($Post$) is the time period after the LPI was initiated (July 1, 2006). This framework is similar to that estimated in our policing results, but differs in that our unit of analysis is at the city level ($City_i$) rather than the reporting district level (RD_i). Additionally, we adopt the most conservative definition of treated location as only containing the city of West Hollywood. This definition is pertinent as the district attorney's office operates at the county level excluding a select few cities previously described. The unit of analysis between our analysis of police and prosecution differs due to the timing of criminal justice processes. While arrest timing is precisely determined, the date that cases arrive at

the prosecutor's office are a function of a number of features (backlogs in law enforcement agencies, time required to gather evidence, etc.). As such, we aggregate our unit of analysis to the week level in examining the prosecution data in an attempt to overcome some of these issues.

As previously noted, there are three outcomes of interest: misdemeanor marijuana charge rates, misdemeanor marijuana dismissal rates and the rate of misdemeanor marijuana cases plead out.[22] The coefficient of interest, β_3, is a difference-in-difference estimate that explains the effect of the LPI on the rate of misdemeanor marijuana cases charged, dismissed and plead out. To account for unobserved time-varying changes across cities, we include year controls, τ_y, and to absorb time-invariant variation across cities, we include city controls, $City_i$. We estimate various specifications in the Appendix section Tables 6-9 including month fixed effects and a vector of city-level characteristics and find consistent results to our main findings. The magnitude of our observed estimates are strongest in our model including a vector of control characteristics, discussed extensively in the appendix section. Provided that our location level fixed effects account for both observed and unobserved variation across cities, we retain these estimates within our main findings.

Table 4 depicts summary statistics for the various rates considered for the full sample, West Hollywood and non-West Hollywood cities. Column 1 presents unconditional averages prior to the initiative, column 2 presents the sample average after the initiative and column 3 formally tests the difference in pre- and post- initiative means with a t-test. We observe statistically significant differences in all three panels in the Full Sample and non-West Hollywood cities, while West Hollywood experiences significant differences for charge and dismissals rates. Namely, we observe an increase in charge and dismissal rates after

[22]These rates are constructed as follows: (1) number of misdemeanor marijuana charges divided by total misdemeanor marijuana arrests, (2) number of misdemeanor marijuana cases dismissed divided by total misdemeanor marijuana charges and (3) number of misdemeanor marijuana charges plead out divided by total misdemeanor marijuana charges.

the intervention in both the full sample and non-west Hollywood cities when testing the unconditional means. In the third panel, we observe the average rate of cases plead out to be larger prior to the intervention relative to after intervention in both the full sample and non-West Hollywood cities. In West Hollywood we observe statistically significant differences in means for charge and dismissal rates before and after the initiative, but we do not observe a statistically different average plea rate. To further examine the LPI, we address potential caveats in relying on a simple difference in means by estimating an OLS model across cities and weeks pre- and post-initiative to better capture unobserved variation in prosecutorial behavior over time.

Table 4: Unconditional Differences in Marijuana Charges

	Charge Rates			Dismissal Rates			Plea Rates		
	Pre	Post	Difference	Pre	Post	Difference	Pre	Post	Difference
Full Sample	0.011	0.0200	-0.008***	0.613	0.786	-0.173***	0.247	0.228	0.019***
	(0.0003)	(0.0005)		(0.008)	(0.005)		(0.007)	(0.006)	
West Hollywood	0.007	0.011	-0.004*	0.758	0.929	-0.171***	0.027	0.190	0.226
	(0.001)	(0.002)		(0.076)	(0.040)		(0.084)	(0.056)	
Non-West Hollywood	0.011	0.021	-0.009***	0.611	0.784	-0.143***	0.247	0.228	0.0189***
	(0.0004)	(0.0005)		(0.008)	(0.005)		(0.008)	(0.006)	

Notes: This table displays the average number of misdemeanor marijuana charge rates, dismissal rates and rate of cases plead out for the overall sample, for West Hollywood, and for non-West Hollywood cities. The difference in misdemeanor marijuana charge rates, dismissal rates and rate of cases plead out are reported in the third column and a t-test is performed on these differences. *, **, *** denote statistical significance at the 10%, 5%, and 1% levels, respectively.

Table 5 presents OLS estimates on the effect of the LPI in West Hollywood after the initiative across our three outcomes of interest. The OLS estimation includes city fixed effects in column (I) and city and year fixed effects in column II. As the three outcome variables examined are rates bounded between 0 and 1, we interpret our results through a linear probability model. In the first set of results, our coefficient of interest ($LPI \times Post$) is consistently statistically significant and suggests a smaller charge rate of 0.479% in West Hollywood compared to other cities in the sample post LPI, which translates to a 32% reduction from the mean. Our second set of results suggest there is no statistically significant

change in the rate of overall misdemeanor marijuana case dismissals in West Hollywood compared to other cities in the sample after the passage of the LPI.[23] As is discussed in section 3.3, these estimates suggest that the number of marijuana misdemeanor charges decreased as a function of fewer arrests made by officers in West Hollywood post-initiative.[24] Of the arrests made, prosecutors do not appear to change their behavior by increasing or decreasing the rate of overall misdemeanor marijuana charge dismissals. Similar to our posited hypotheses, we observe unchanged prosecutor practices in the form of no statistically significant reduction or increase in overall case dismissal rate after the passage of the LPI. Our final panel of results indicate a statistically significant decrease in the rate of cases plead out in West Hollywood post LPI, representing a 30% decrease from the mean. These results are consistent with the notion that the people of West Hollywood have deemed misdemeanor marijuana cases to be a low priority charge that should not receive harsh penalties, potentially motivating defense attorneys to advise their clients against accepting a plea agreement.[25]

[23]See robustness table 8 for a subset of dismissals pertaining to review prosecutor initiated dismissals

[24]If we find a comparative reduction in misdemeanor marijuana arrests in West Hollywood to other reporting districts, we would expect there to be mechanically fewer cases charged in West Hollywood compared to other cities.

[25]To test this hypothesis, we run a separate set of estimates where our outcome of interest is the rate of misdemeanor marijuana charges resulting in a guilty verdict divided by total misdemeanor marijuana charges. We find that the rate of guilty convictions decreased by 39% post LPI in West Hollywood compared to other cities in the sample, a 121% decrease from the mean. This finding suggests that post- LPI, misdemeanor Marijuana infractions in West Hollywood result in a guilty plea at much smaller rates.

Table 5: Effect of Low Priority Initiatives on Misdemeanor Marijuana Charges

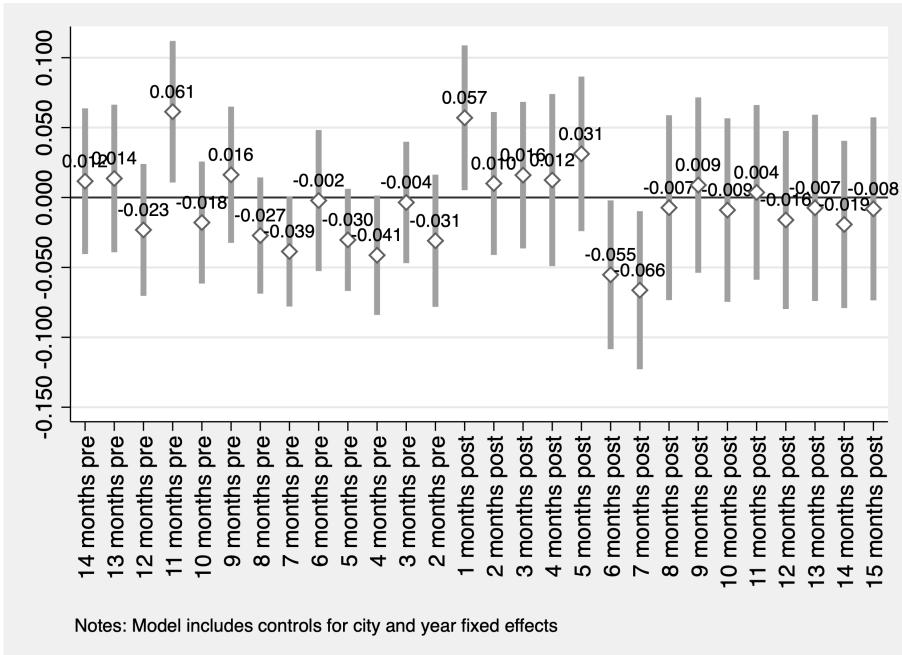
	Charge Rates		Dismissal Rates		Plea Rates	
	I	II	III	IV	V	VI
LPI x Post	-0.00479***	-0.00481***	0.00901	0.00484	-0.0709***	-0.0746***
	(0.00137)	(0.00138)	(0.0281)	(0.0270)	(0.0200)	(0.0184)
City FEs	✓	✓	✓	✓	✓	✓
Year FEs		✓		✓		✓
Mean of Dep. Var.	0.015	0.015	0.717	0.717	0.236	0.236
Number of Obs.	29,640	29,640	7,312	7,312	7,312	7,312

Notes: This table produces the results of a difference-in-difference analysis on the effect of LPI on misdemeanor prosecutorial charges, dismissals and cases plead out. Columns (1) and (2) estimate the effect of LPI on the rate of marijuana charges of total charges within a city-week. Columns (3) and (4) estimate the effect of LPI on the rate of dismissed marijuana charges of total marijuana charges. Columns (5) and (6) estimate the effect of LPI on the rate of marijuana cases plead out to total marijuana charges. *, **, *** denote statistical significance at the 10%, 5%, and 1% levels, respectively.

Similar to our policing analysis, we conduct event-study analyses based on equation 2 but within the context of our prosecutor outcomes. We are interested in estimating the effect of the LPI on the charge rate, dismissal rate and rate of cases plead out across Los Angeles County. Figure 4 plots the county-wide monthly misdemeanor marijuana charge rate pre- and post- coefficients at the city-month unit of analysis. We observe no statistically significant increase or decrease in charge rates across the entire county except for in 6^{th} and 7^{th} months after the initiative, where charge rates are negative. Similarly, Figure 5 suggests that there are some statistically significant negative dismissal rates pre and post LPI (4 months pre and 6-7 months post). Lastly, figure 6 showcases a null effect for the rate of cases plead out in Los Angeles county both pre- and post-LPI with a few exceptions (8 months pre and 11 months post).[26]

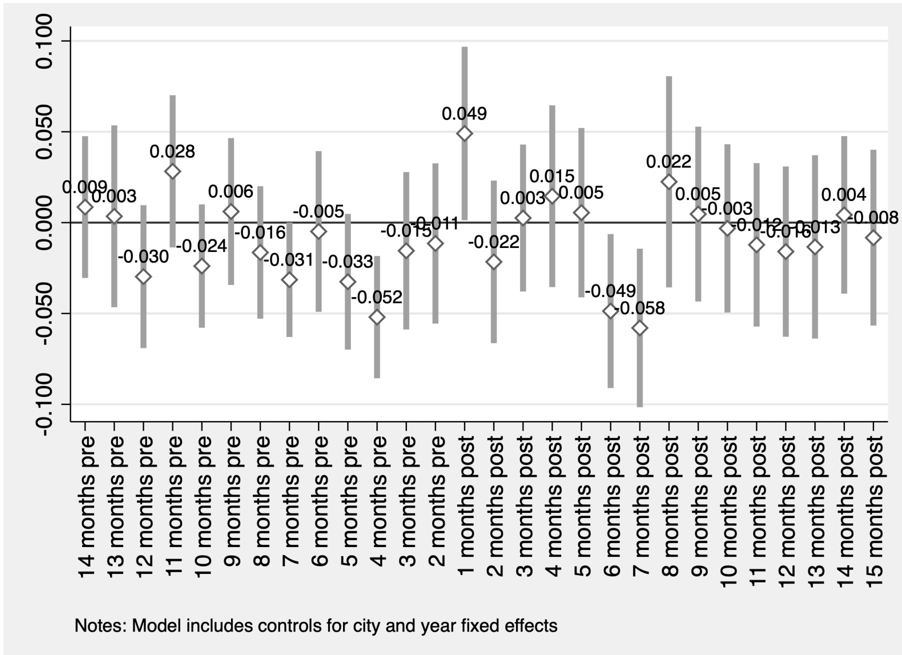
[26]The relatively large standard errors and insignificant estimates are likely attributable to pleas being observed at low frequencies in our data, making it difficult to precisely estimate changes in cases plead out.

Figure 4: **Misdemeanor Marijuana Charge Rate**



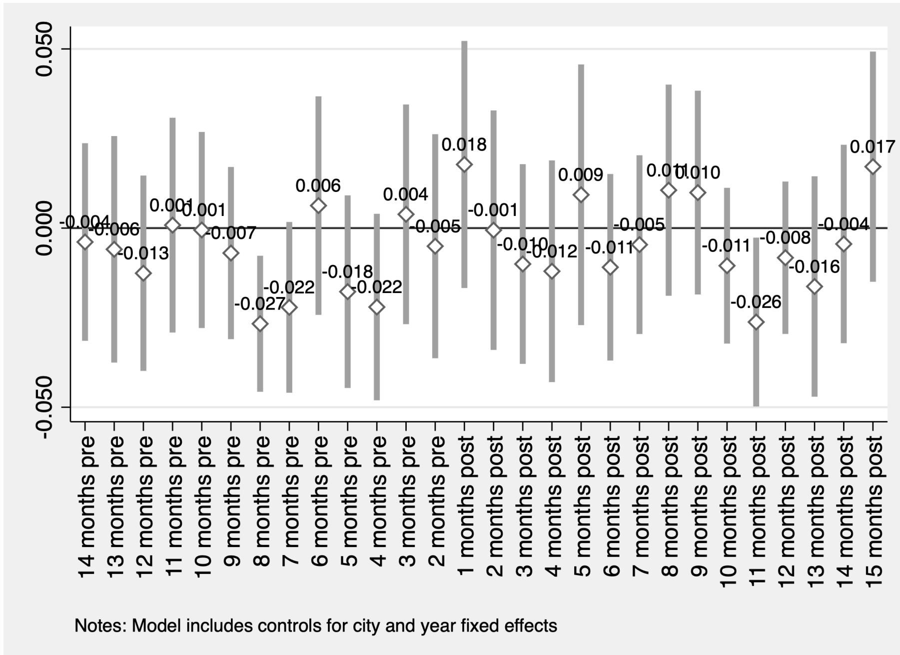
Note: Figure 4 showcases event-study coefficients on misdemeanor marijuana charge rates across months pre- and post- the LPI. We omit the month prior to the LPI and 15 months post as our reference omitted time groups. Standard errors are clustered at the city-level.

Figure 5: **Misdemeanor Marijuana Dismissal Rate**



Note: Figure 5 showcases event-study coefficients on misdemeanor marijuana dismissal rates across months pre- and post- the LPI. We omit the month prior to the LPI and 15 months post as our reference omitted time groups. Standard errors are clustered at the city-level.

Figure 6: **Misdemeanor Marijuana Plea Rate**



Note: Figure 6 showcases event-study coefficients on misdemeanor marijuana plea rates across months pre- and post- the LPI. We omit the month prior to the LPI and 15 months post as our reference omitted time groups. Standard errors are clustered at the city-level.

3.5 Robustness

We explore robustness checks to verify the results presented thus far. A natural extension to a difference-in-difference (DiD) analytical approach is a synthetic control (SC) approach, especially when the current framework estimates on a single treated location. A recent innovation in the empirical methodological literature is a method that couples the difference-in-difference estimation strategy with the synthetic control estimation strategy, the synthetic difference-in-difference (SDID) (Arkhangelsky et al., 2021). We explore the application of SDID within the LPI framework as one of the first empirical applications of this estimation strategy. Below we layout the empirical differences across the three noted strategies as well as the reasons SDID should be considered the most appropriate identification method within the LPI setting.

As noted in previous literature, synthetic control is arguably the most influential innovations in policy reform evaluation (Athey and Imbens, 2017). This sentiment is highlighted within the current context where our "treatment", the low priority initiative, is evaluated on a singular treated "unit", West Hollywood. Another comparative advantage in synthetic control is the robustness gained by comparing the treated unit to another unit that matches on pre-treatment exposure trends and other relevant covariate characteristics. In doing so, this strategy relaxes the strong assumption required within difference-in-differences (parallel pre-trends between treated and control units). If parallel trends are established in the DiD context, we further must believe pre-trends in the treated unit would have evolved similar to the control unit in the absence of intervention. In contrast, the synthetic control design relies on computing a "synthetic unit" composed of weighted untreated "donor" units that match on the treated unit's pre-treatment outcome. SDID integrates the two approaches by first re-weighting untreated "donor" units to ensure time trends for the outcome are parallel pre-intervention and subsequently measure DID on the revised re-weighted panel. SDID is an attractive alternative method due to the data-driven nature of selecting time weights rather than allowing each time unit to have equal weight in the pre-period, such as in DID, or over-weighting the last pre-treatment period as is typically done in event study analysis (Arkhangelsky et al., 2021).

Within the LPI framework, the SDID application is as follows. First, the "synthetic control" estimation determines the city locations that most mimic the outcome of interest and re-weights those cities to ensure time trends are parallel pre-intervention. Second, the difference-in-difference estimation is applied to the re-weighted panel to measure the effect of the LPI on misdemeanor marijuana arrests, prosecutor charge rates, prosecutor dismissal rates and prosecutor plea rates. Another deviation from previous results is the unit of analysis. Due to the nature of census data reported at the decennial frequency as well as the noisiness of rate data, SDID is analyzed at the annual-city level where all cities in California

are included in the potential donor pool. Similar to our prior analysis, locations directly within a 12 mile radius from the treated location are excluded from the analysis to avoid spillover effects. We believe SDID has a comparative advantage in estimating robust results as locations geographically distant from the treated location can also be used in creating the synthetic donor unit, further decreasing potential spillover effects.

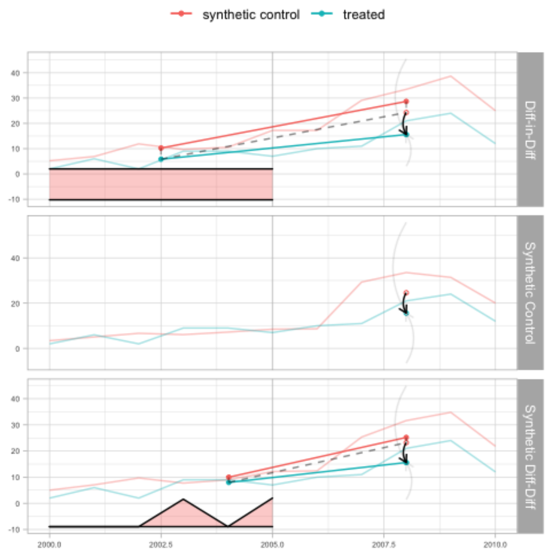
We explore SDID across four outcomes: incoming arrests for low-level misdemeanor marijuana offenses, low-level misdemeanor marijuana charge rates, low-level misdemeanor marijuana dismissal rates and low-level misdemeanor marijuana plea rates. Our goal in implementing SDID is to create a synthetic West Hollywood that emulates misdemeanor marijuana offense trends in the pre-treatment period, 2000-2006. Our estimated SDID average treatment on the treated (ATT) estimate is graphically showcased in Figure 7 below. Each panel is estimated on data from CalDOJ censoring misdemeanor marijuana offenses that co-occur with other offense types. Indeed, a benefit of estimating the effect of LPI on CalDOJ data is two-fold. First, we observe each of the arresting department's suggested charges for each person-date. Second, we observe arrests across cities other than those contained in Los Angeles County. A drawback in utilizing the CalDOJ data in this analysis is the lack of geographical granularity. We cannot observe or measure arrests at the reporting district level as was done in the analysis presented in Table 2. Each panel consists of three graphs. The top figure depicts the difference-in-difference trends. The middle figure displays the synthetic control trends. The bottom figure displays the SDID trends. The arrow represents the average treatment on the treated (ATT) estimate of the LPI in West Hollywood. Specifically, the red line within each panel represents the general trend of the synthetic West Hollywood unit from pre to post LPI, the dashed line describes the counterfactual trend, and the blue line represents the actual trend of the treated location. The counterfactual trend can be thought of similar to difference-in-difference methodologies. Since SDID ensures the synthetic unit and treated unit share parallel trends in the pre-period, we are

certain the treated unit would have followed the same trend as the counterfactual had it not been treated. We can then re-anchor the trend the counterfactual had from the pre to post period (the dotted line) at an intercepting point on the treated, blue line. The final step is to compare the gap between the counterfactual dashed line and the real evolution of the treated location (blue line), which is denoted with the arrow.

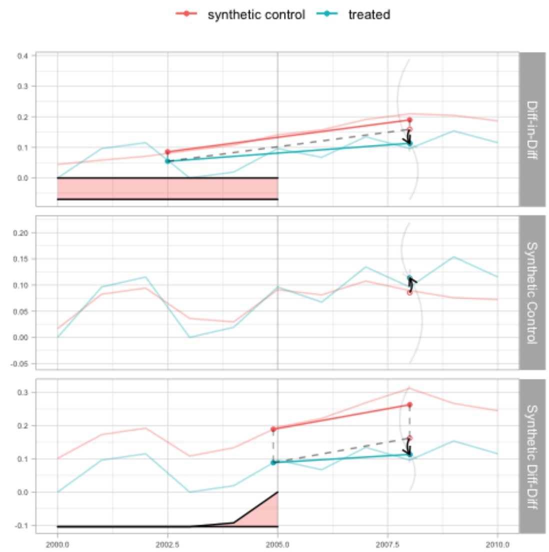
Figure 7, panel (a) displays the effect of the LPI on misdemeanor marijuana arrests. The effect size is consistent with the previously stated difference-in-differences results in Table 2. Panels (b),(c) and (d) depict the effect of the LPI on misdemeanor marijuana charge rates, dismissal rates and plea rates. Each of these estimates also retain consistency with previously measured difference-in-difference analysis in Table 5. A major takeaway from the research introducing the SDID design is the nature of DID and SC methods to commonly overstate the treatment effect while SDID produces more conservative estimates for the magnitude of the ATT. Interestingly, within our current framework, SDID reports similar magnitudes to DID across all estimations.

Figure 7: Synthetic Difference-in-Differences

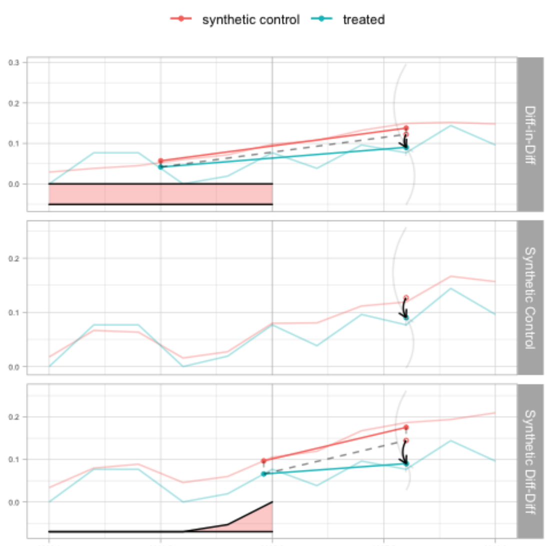
(a) Arrests



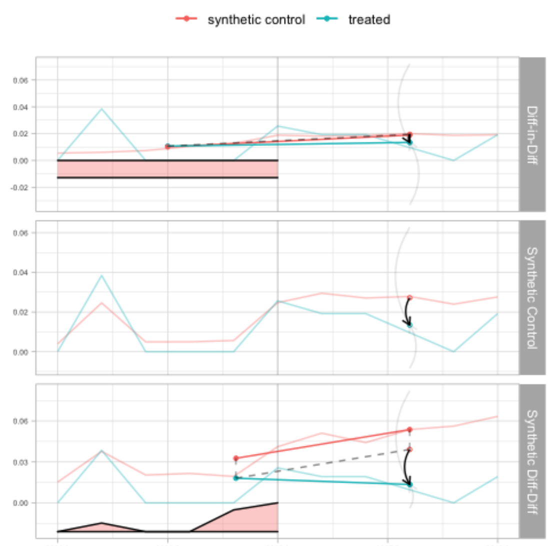
(b) Charge Rate



(c) Dismissal Rate



(d) Plea Rate



Note: Panel (a) estimates the effect of the LPI on arrest outcomes while Panels(b-d) estimate the effect of the LPI on misdemeanor marijuana charge rates, dismissal rates and plea rates. Within each panel, a comparison of difference-in-difference, synthetic control and synthetic difference-in-differences is estimated on the effect of the LPI at the annual-city level.

3.6 Conclusion

This research examines the effect of a low priority initiative, which mandated that misdemeanor marijuana offenses be considered as the lowest priority for law enforcement. We explore whether social norms toward specific laws (namely low-level marijuana offenses) lead to changes in the behavior of actors in the criminal justice system. Building up on previous research showing that formal enforcement is not independent of the prevailing social norms (Westley, 1953; Hunt, 1985; Acemoglu and Jackson, 2017), we examine the interplay between social norms and police and prosecutor behavior. We note that there is likely a tension between the preferences of local policy makers, voters and actors in the criminal justice system.

We hypothesized that law enforcement could potentially respond to the expression of the social norm by reducing the number of arrests and citations for low-level marijuana offenses. But, we also note that law enforcement preferences could have a counterbalancing effect given that law enforcement actors might be inherently punitive. Importantly, though, the Los Angeles Sheriff's Department is contracted to perform policing services in West Hollywood and could lose this contract if the municipality became dissatisfied with the services offered.

Alternatively, we expected prosecutors would not be impacted by the LPI with the same magnitude as the Sheriff's office. This hypothesis was based on the facts that the initiative was non-binding and consisting across a singular city while the DA's office handles cases across the county. Therefore, we hypothesize prosecutors would respond to misdemeanor marijuana charges no differently after the LPI, especially in light of the elected LADA's, Steve Cooley, stance against marijuana sales as noted in section 4.2. Prosecuting attorneys are tasked with holding individuals accountable for violating the law. Since low level marijuana offenses remain violations of the law post LPI, there remains an expectation for prosecutors to charge these cases.

We are able to utilize this low priority initiative as an opportunity to examine how law enforcement and prosecutors respond to social norms. This analysis empirically examines the change in arresting behavior by law enforcement using a difference-in-difference framework and find that the social norm appears to activate law enforcement to engage in making more arrests in areas that neighbor the region that passes the low priority initiative. We also identify that financial contracts appear to play a driving role in determining how law enforcement make arrests in the aftermath of the expression of the social norm. We also explore changes in prosecution behavior in response to the passage of the low priority initiative, finding that prosecutors filed fewer charges in the treated location, while overall dismissals in West Hollywood remained unchanged. Plea bargain rates for low-level misdemeanor charges fell, which could be a product of changes in strategies that the defense pursues, especially given that the community has expressed a desire to not punish such offenses. Thus, unlike the law enforcement agency, the prosecutor's office did not appear to respond to the social norms, as evidenced by no change in the rate of dismissing cases.[27] However, the prosecutor's office did experience changes in the number of low-level marijuana arrests made by LASD and possibly a change in defense attorney's strategies. Further, our extension in estimating the effect of the LPI under the SDID framework showcases that our main results are consistent in magnitude and direction. By implementing SDID, we ensure that the results showcased are not present due to chance and allow the relaxation of the parallel trends assumption. We can therefore provide greater confidence in the nature of our estimates to be estimating the effect from the LPI reform.

A common extension to the empirical measurement of legislative reform is to discuss the policy relevance. The scope of the current LPI framework is limited to an expansive

[27] As discussed above, we subset dismissals to include only review prosecutor initiated dismissals as charges come in the door, these include reasons such as lack of evidence and for the interest of justice, in the appendix. We include a more extensive set of dismissals observed during every step that a case is handled by the prosecutor in the main results. The main result dismissal rates include case dismissals that are likely attributable to prosecutors dismissals at later points of the case or trial.

discussion of policy implications due to the small-scale nature of the initiative. Nevertheless, we can draw on our analysis for some policy insights, particularly how a non-binding initiative take-up is related to actor incentives. A conclusion one may extrapolate from our analysis is that under the currently established election driven positions of the Sheriff's and Prosecutor's offices, constituent sentiment may influence heterogeneity in how the law is upheld across geographic locations. The nature of the Sheriff's office, in particular, having a contractual relationship with specific locations spurs greater awareness of the norms within the jurisdictions with a contractual relationship.

With respect to policy implications, one major obstacle in modifying legislation to reflect the evolution of community beliefs is the slow and adversarial nature of legislative reform. Our analysis may introduce an alternative avenue when considering small-scale reforms in decriminalizing non-violent low-level offenses. Though an important consideration to be aware of from our arrest analysis is that the community concerned with the low-level misdemeanor marijuana offenses observed a reduction in arrests, the reduction was a function of neighboring locations experiencing an increase in arrests. This result shows that a non-binding norm had significant implications for law enforcement behavior within the community of interest, despite being informal and non-legally binding. But, law enforcement behavior does not seem to have been altered in regions where the community norm did not apply. With respect to the prosecutor's office, the nature and quantity of cases across all cities encompassed in LA County makes it so that this local norm only played a significant role in reducing the volume of incoming arrests and in defendant's optimal plea take-up rate strategy for a very small share of cases.

Future theoretical research could inform empirical research. Acemoglu and Jackson (2017) examine the interplay between social norms and enforcement, but this theoretical model would be significantly improved by considering the economic relationship between law enforcement agencies, the communities that they enforce, and the social norms of these

communities. Theoretically understanding how such financial contracts can influence enforcement choices can also inform the effectiveness of the expression of informal initiatives versus formalized laws.

We note that while democratically implemented legal changes in the criminal justice system have been enacted, which could be viewed as an expression of a community social norm, these legal changes are often large (e.g., state-wide) and complex (i.e., impacting numerous agencies and institutional actors). The scale and complexity of these legal changes complicates the effort of identifying the impact of a social norm on the behavior of institutional actors in an empirical analysis, often resulting in researchers turning to laboratory environments. By leveraging the low priority initiative passed in West Hollywood, which was passed in a single geographic location and had direct implications for one criminal justice agency, we are able to isolate the effect of the social norm on the behavior of institutional actors that are directly and indirectly impacted by the LPI.

3.7 Appendix

Tables 6-7 expand on original results with the inclusion of month and city-year control characteristics. Undoubtedly, the use of two-way fixed effects is a widely practiced application when estimating the effect of a "shock" or policy change within a panel data setting. Particularly within the LPI framework, we include either a combination of reporting district and year fixed effects when measuring arrest outcomes or city and year fixed effects when measuring prosecution outcomes. Arguably, the addition of geographical and temporal fixed effects is considered the standard application within panel data in light of there being unobserved characteristics within the geographical unit that effect the outcome in a meaningful way. Put differently, geographic and temporal fixed effects control for measured characteristics the researcher believes impact the outcome while also controlling for unmeasured characteristics that also impact the outcome. Furthermore, hand picking the control covariates the researcher believes are the most pertinent when measuring the outcome leads to over weighting the importance of these observable characteristics which are limited to how often this data are sampled and what variables are measured in survey data. Though it can be argued that the selection of specific covariates based on institutional and theoretical framework is the preferred approach. To ensure previously reported estimates are robust when including a vector of covariates, we report columns (3) and (6) in tables 6-9. Our full set of controls considered are gathered from the decennial Census encompassing the years 2000 and 2010. [28] We include an exhaustive vector of city-level covariates that were available across our panel including population size, ratio of males to female, percent of population that received a high school education, percent of Black population, percent of White population, percent of Asian population, percent of Other population, median income, percent of female population ages 65 and over, and percent of male population ages 65 and over.

[28] Though a preferential source for covariate characteristics is the American Community Survey (ACS) as this survey is reported more frequently, our treated location of West Hollywood is not located within the list of cities surveyed.

Another potential concern when interpreting our results is the nature of misdemeanor marijuana offenses occurring concurrently with other offense types. A possible scenario could be that our results are conditional on whether a misdemeanor arrest occurs coupled with other offense types. We need not be concerned with the coupling of offense types so long as the nature of the types and frequency of co-occurring offenses remain unchanged after the introduction of the LPI. To ensure our results are not attributed to the "stacking" of offenses or types of co-occuring offenses post LPI, we censor our sample to include arrests where the only offense type is misdemeanor marijuana offenses in columns (3-6). We find that our estimates retain magnitude, direction and remain statistically significant, showcasing that upon removing arrests where misdemeanor marijuana arrests co-occur with other offense types our results are robust.[29]

Table 6: Robustness Checks: Effect of LPI on Misdemeanor Marijuana Arrests

	Dependent Variable: Arrests					
	I	II	III	IV	V	VI
LPI x Post	-0.531***	-0.531***	-0.795***	-0.455***	-0.455***	-0.559***
	(0.0472)	(0.0472)	(0.0659)	(0.0321)	(0.0322)	(0.0449)
City FEs	✓	✓		✓	✓	
Controls			✓			✓
Month FEs		✓			✓	
Year FEs	✓	✓	✓	✓	✓	✓
Mean of Dep. Var.	0.6109	0.6109	1.0417	0.215	0.215	0.215
Number of Obs.	39,468	39,468	27,664	39,468	39,468	29,744

Notes: This table showcases difference-in-difference analysis on the effect of LPI on misdemeanor arrests. Columns (1-3) includes all misdemeanor charges when estimating the likelihood of an marijuana arrest, while columns (4-6) censor observations where multiple offenses co-occur with misdemeanor marijuana arrests. Columns (1) and (4) estimate the effect of LPI on arrests at the city-week level conditioning for city and year fixed effects. Columns (2) and (5) add month fixed effects to control for any possibility of seasonality in the outcome. Columns (3) and (6) replace city controls with a vector of covariates described below in the description. *, **, *** denote statistical significance at the 10%, 5%, and 1% levels, respectively.

[29]We remove 0.5% of our data in this exercise, our reported observation size remains constant in Table 6 since our data is aggregated to the week by year level and so the number of cities captured in our week-year panel remain constant.

Table 7: Robustness Checks: Effect of LPI on Misdemeanor Charge Rates

	Dependent Variable: Charge Rates					
	I	II	III	IV	V	VI
LPI x Post	-0.110***	-0.111***	-0.123***	-0.0895***	-0.0895***	-0.102***
	(0.0116)	(0.0116)	(0.0138)	(0.0119)	(0.0119)	(0.0143)
City FEs	✓	✓		✓	✓	
Controls			✓			✓
Month FEs		✓			✓	
Year FEs	✓	✓	✓	✓	✓	✓
Mean of Dep. Var.	0.1456	0.1456	0.1602	0.1427	0.1427	0.1604
Number of Obs.	35,818	35,818	24,995	35,818	35,818	24,995

Notes: This table showcases difference-in-difference analysis on the effect of LPI on misdemeanor marijuana charge rates. Columns (1-3) includes all misdemeanor charges when estimating the likelihood of a marijuana charge rate, while columns (4-6) censor observations where multiple offenses co-occur with misdemeanor marijuana offenses. Columns (1) and (4) estimate the effect of LPI on arrests at the city-week level conditioning for city and year fixed effects. Columns (2) and (5) add month fixed effects to control for any possibility of seasonality in the outcome. Columns (3) and (6) replace city controls with a vector of covariates described below in the description. Standard errors are clustered at the city level. *, **, *** denote statistical significance at the 10%, 5%, and 1% levels, respectively.

Table 8: Robustness Checks: Effect of LPI on Misdemeanor Dismissal Rates

	Dependent Variable: Dismissal Rates					
	I	II	III	IV	V	VI
LPI x Post	0.0435***	0.0435***	0..0443***	0.0454***	0.0454***	0.0466***
	(0.00117)	(0.00117)	(0.0012)	(0.00137)	(0.00137)	(0.00102)
City FEs	✓	✓		✓	✓	
Controls			✓			✓
Month FEs		✓			✓	
Year FEs	✓	✓	✓	✓	✓	✓
Mean of Dep. Var.	0.0029	0.0029	0.0027	0. 0027	0. 0027	0.00223
Number of Obs.	39,468	39,468	29,744	39,468	39,468	29,744

Notes: This table showcases difference-in-difference analysis on the effect of LPI on misdemeanor marijuana dismissal rates. Columns (1-3) includes all misdemeanor dismissals when estimating the likelihood of a marijuana dismissal rate, while columns (4-6) censor observations where multiple offenses co-occur with a misdemeanor marijuana offense. Columns (1) and (4) estimate the effect of LPI on arrests at the city-week level conditioning for city and year fixed effects. Columns (2) and (5) add month fixed effects to control for any possibility of seasonality in the outcome. Columns (3) and (6) replace city controls with a vector of covariates described below in the description. Standard errors are clustered at the city-level. *, **, *** denote statistical significance at the 10%, 5%, and 1% levels, respectively.

Table 9: Robustness Checks: Effect of LPI on Misdemeanor Plea Rates

	Dependent Variable: Plea Rates					
	I	II	III	IV	V	VI
LPI x Post	-0.0116***	-0.0119***	-0.0167***	-0.0293***	-0.0293***	-0.0345***
	(0.00413)	(0.00417)	(0.00613)	(0.00421)	(0.00423)	(0.00633)
City FEs	✓	✓		✓	✓	
Controls			✓			✓
Month FEs		✓			✓	
Year FEs	✓	✓	✓	✓	✓	✓
Mean of Dep. Var.	0.0141	0.0141	0.0144	0.0141	0.0141	0.0142
Number of Obs.	35,818	35,818	24,995	35,831	35,831	25,013

Note: This table showcases difference-in-difference analysis on the effect of LPI on misdemeanor marijuana plea rates. Columns (1-3) includes all misdemeanor offenses when estimating the likelihood of a marijuana plea rate, while columns (4-6) censor observations where multiple offenses co-occur with a misdemeanor marijuana offense. Columns (1) and (4) estimate the effect of LPI on arrests at the city-week level conditioning for city and year fixed effects. Columns (2) and (5) add month fixed effects to control for any possibility of seasonality in the outcome. Columns (3) and (6) replace city controls with a vector of covariates described below in the description. Standard errors are clustered at the city-level. *, **, *** denote statistical significance at the 10%, 5%, and 1% levels, respectively.

Conflict of Interest Statement

The authors report no conflicts of interest associated with this research.

References

Aakvik, A., Heckman, J. J., and Vytlacil, E. J. (2005). Estimating treatment effects for discrete outcomes when responses to treatment vary: an application to norwegian vocational rehabilitation programs. *Journal of Econometrics*, 125(1-2):15–51.

Acemoglu, D. and Jackson, M. O. (2017). Social norms and the enforcement of laws. *Journal of the European Economic Association*, 15(2):245–295.

Aizer, A. and Doyle Jr, J. J. (2015). Juvenile incarceration, human capital, and future crime: Evidence from randomly assigned judges. *The Quarterly Journal of Economics*, 130(2):759–803.

Angrist, J. D., Imbens, G. W., and Rubin, D. B. (1996). Identification of causal effects using instrumental variables. *Journal of the American statistical Association*, 91(434):444–455.

Antonovics, K. and Knight, B. G. (2009). A new look at racial profiling: Evidence from the boston police department. *The Review of Economics and Statistics*, 91(1):163–177.

Arkhangelsky, D., Athey, S., Hirshberg, D. A., Imbens, G. W., and Wager, S. (2021). Synthetic difference-in-differences. *American Economic Review*, 111(12):4088–4118.

Athey, S. and Imbens, G. W. (2017). The state of applied econometrics: Causality and policy evaluation. *Journal of Economic perspectives*, 31(2):3–32.

Ba, B., Knox, D., Mummolo, J., and Rivera, R. (2021). The role of officer race and gender in police-civilian interactions in chicago. *Science*, 371(6530):696–702.

Baker, S. and Mezzetti, C. (2001). Prosecutorial resources, plea bargaining, and the decision to go to trial. *The Journal of Law, Economics, and Organization*, 17(1):149–167.

Barts, B. and Kubrin, C. E. (2018). Can we downsize our prisons and jails without compromising public safety? findings from california's prop. 47. *Criminology and Public Policy*, 17(3):693–715.

Bazelon, E. (2019). *Charged: The New Movement to Transform American Prosecution and End Mass Incarceration*. Random House, New York.

Bellin, J. (2019). The power of prosecutors. *New York University Law Review*, 94(2):171–212.

Benson, B. L. (1989). The spontaneous evolution of commercial law. *Southern economic journal*, pages 644–661.

Bicchieri, C. (2005). *The grammar of society: The nature and dynamics of social norms*. Cambridge University Press.

Bird, M. and Grattet, R. (2016). Realignment and recidivism. *The Annals of the American Academy of Political and Social Science*, 664(1):176–195.

Bird, M., Sofstrom, M., Martin, B., Raphael, S., and Nguyen, V. (2018). The impact of prop. 47 on crime and recidivism. Public Policy Institute of California.

Bittner, E. (1967). Police discretion in emergency apprehension of mentally ill persons. *Social Problems*, 14(3):278–292.

Bittner, E. (1970). The functions of the police in modern society: A review of background factors, current practices, and possible role models.

Bjerk, D. (2005). Making the crime fit the penalty: The role of prosecutor discretion under mandatory minimum sentencing. *Journal of Law and Economics*, 48:591–625.

Boylan, R. and Mocan, N. (2014). Intended and unintended consequences of prison reform. *Journal of Law, Economics, and Organization*, 30:558–586.

Boylan, R. T. (2005). What do prosecutors maximize? evidence from the careers of u.s. attorneys. *American Law and Economics Review*, 7(2):279–402.

Boylan, R. T. (2012). The effect of punishment severity on plea bargaining. *Journal of Law and Economics*, 55:565–592.

Carbonara, E., Parisi, F., and Von Wangenheim, G. (2008). Lawmakers as norm entrepreneurs. *Review of Law & Economics*, 4(3):779–799.

Carbonara, E., Parisi, F., and von Wangenheim, G. (2011). Countervailing norms. In *Encyclopedia of Law and Economics*. Edward Elgar Publishing Limited.

Carbonara, E., Parisi, F., and Von Wangenheim, G. (2012). Unjust laws and illegal norms. *International Review of Law and Economics*, 32(3):285–299.

Charness, G. and DeAngelo, G. (2018). Law and economics in the laboratory. In Teitelbaum, J. C. and Zeiler, K., editors, *Research Handbook on Behavioral Law and Economics*, Chapters, chapter 12, pages 321–346. Edward Elgar Publishing.

Darbinyan, J. (2019). Proposition 57: Implementation and perception. Master's thesis, California State University, Northridge.

Davis, C. (2021). *Proposition 57: Psychological Development, Social and Fiscal Implications for African American and Other Incarcerated Youth of Color in California*. Proquest dissertation publishing, The Chicago School of Professional Psychology.

Davis, E. and Anthony Whyde, B. S. L. L. (2018). Contacts between police and the public, 2015.

DeAngelo, G. and Charness, G. (2012). Deterrence, expected cost, uncertainty and voting: Experimental evidence. *Journal of Risk and Uncertainty*, 44(1):73–100.

DeAngelo, G. and Gee, L. K. (2020). Peers or police?: The effect of choice and type of monitoring in the provision of public goods. *Games and Economic Behavior*, 123(C):210–227.

DeAngelo, G. and Owens, E. G. (2017). Learning the ropes: General experience, task-specific experience, and the output of police officers. *Journal of Economic Behavior Organization*, 142:368 – 377.

DeAngelo, G. J., Ross, A., and Gittings, K. R. (2018). Police Incentives, Policy Spillovers, and the Enforcement of Drug Crimes. *Review of Law & Economics*, 14(1):1–29.

Dharmapala, D., Garoupa, N., and McAdams, R. H. (2016). Punitive police? agency costs, law enforcement, and criminal procedure. *The Journal of Legal Studies*, 45(1):105–141.

Dobbie, W., Goldin, J., and Yang, C. S. (2018). The effects of pretrial detention on conviction, future crime, and employment: Evidence from randomly assigned judges. *American Economic Review*, 108(2):201–40.

Dominguez Rivera, P., Lofstrom, M., and Raphael, S. (2019). The effect of sentencing reform on crime rates: Evidence from california's proposition 47. University of California Berkeley: Working Paper.

Ellickson, R. C. (1998). Law and economics discovers social norms. *The Journal of Legal Studies*, 27(S2):537–552.

Elster, J. (1989). Social norms and economic theory. *Journal of economic perspectives*, 3(4):99–117.

Engel, C. (2021). The decision for crime as a conditional rule violation. *Working Paper*.

Engel, R. S., Sobol, J. J., and Worden, R. E. (2000). Further exploration of the demeanor hypothesis: The interaction effects of suspects' characteristics and demeanor on police behavior. *Justice Quarterly*, 17(2):235–258.

Enns, P. K. (2014). The public's increasing punitiveness and its influence on mass incarceration in the united states. *American Journal of Political Science*, 58(4):857–872.

Feldman, Y. and Perez, O. (2009). How law changes the environmental mind: An experimental study of the effect of legal norms on moral perceptions and civic enforcement. *Journal of Law and Society*, 36(4):501–535.

Fryer, R. G. (2019). An empirical analysis of racial differences in police use of force. *Journal of Political Economy*, 127(3):1210–1261.

Gaston, S. (2019). Producing race disparities: A study of drug arrests across place and race. *Criminology*, 57(3):424–451.

Gillooly, J. (2020a). *"911, Is This an Emergency?": How 911 Call-Takers Extract, Interpret, and Classify Caller Information*. PhD thesis.

Gillooly, J. W. (2020b). How 911 callers and call-takers impact police encounters with the public: The case of the henry louis gates jr. arrest. *Criminology & Public Policy*, 19(3):787–804.

Gillooly, J. W. (2021). "lights and sirens": Variation in 911 call-taker risk appraisal and its effects on police officer perceptions at the scene. *Journal of Policy Analysis and Management*.

Glaeser, E. L., Kessler, D. P., and Morrison, A. (2000). What do prosecutors maximize? an analysis of the federalization of drug crimes? *American Law and Economics Review*, 2(2):259–290.

Goldstein, H. (1990). *Problem-oriented Policing*. McGraw-Hill.

Grattet, R., Tafoya, S., Bird, M., and Nguyen, V. (2016). California's county jails in the era of reform. Public Policy Institute of California.

Hedberg, E. C., Katz, C. M., and Choate, D. E. (2017). Body-worn cameras and citizen interactions with police officers: Estimating plausible effects given varying compliance levels. *Justice Quarterly*, 34(4):627–651.

Herman, S. and Pogarsky, G. (2021). Morality, the situation, and crime decisions. *Working Paper*.

Hoeffel, J. (2009). D.a. will prosecute medical marijuana dispensaries – even if l.a. does not ban sales [updated].

Hoekstra, M. and Sloan, C. (2020). Does race matter for police use of force? evidence from 911 calls. *Working Paper*.

Hunt, J. (1985). Police accounts of normal force. *Journal of Contemporary Ethnography*, 13(4):315–341.

Johnson, D. J., Tress, T., Burkel, N., Taylor, C., and Cesario, J. (2019). Officer characteristics and racial disparities in fatal officer-involved shootings. *Proceedings of the National Academy of Sciences*, 116(32):15877–15882.

Kahan, D. M. (1999). The secret ambition of deterrence. *Harvard Law Review*, pages 413–500.

Kessler, D. and Piehl, A. (1998). The role of discretion in the criminal justice system. *Journal of Law, Economics, and Organization*, 14:256–276.

Kling, J. R. (2006). Incarceration length, employment, and earnings. *American Economic Review*, 96(3):863–876.

Knox, D., Lowe, W., and Mummolo, J. (Working Papers). Can racial bias in policing be credibly estimated using data contaminated by post-treatment selection?

Knox, D. and Mummolo, J. (2020). Making inferences about racial disparities in police violence. *Proceedings of the National Academy of Sciences*, 117(3):1261–1262.

Lee, D. S., McCrary, J., Moreira, M. J., and Porter, J. R. (2021). Valid t-ratio inference for iv. Technical report, National Bureau of Economic Research.

Lipsky, M. (1980). *Street-Level Bureaucracy: Dilemmas of the Individual in Public Services.* Russell Sage Foundation, New York.

Lofstrom, M. and Martin, B. (2014). Key factors in california's jail construction needs. Public Policy Institute of California.

Lofstrom, M. and Martin, B. (2015). Public safety realignment: Impacts so far. Public Policy Institute of California.

Lofstrom, M., Martin, B., and Raphael, S. (2020). Effects of sentencing reform on racial and ethnic disparities in involvement with the criminal justice system: The case of california's proposition 47. *Criminology and Public Policy*, 19:1165–1208.

Lofstrom, M. and Raphael, S. (2016). Incarceration and crime: Evidence from california's public safety realignment reform. *Annals of the American Academy of Political and Social Sciences*, 664:196–220.

Loughran, T. A., Nguyen, H., and Topalli, V. (2021). Crime, consumption and choice: On the interchangeability of licit and illicit income. *Working Paper*.

Lum, C., Koper, C. S., Stoltz, M., Goodier, M., Johnson, W., Prince, H., and Wu, X. (2020). Constrained gatekeepers of the criminal justice footprint: A systematic social observation study of 9-1-1 calltakers and dispatchers. *Justice Quarterly*, 37(7):1176–1198.

Lundman, R. J. (1996). Demeanor and arrest: Additional evidence from previously unpublished data. *Journal of Research in Crime and Delinquency*, 33(3):306–323.

MacDonald, J. and Raphael, S. (2020). Effect of scaling back punishment on racial and ethnic disparities in criminal case outcomes. *Criminology and Public Policy*, 19:1139–1164.

Mauer, M., Potler, C., and Wolf, R. (1999). *Gender and justice: Women, drugs, and sentencing policy*. Sentencing Project Washington, DC.

McAdams, R. H. (2015). *The Expressive Powers of Law: Theories and Limits.* Harvard University Press.

McCrary, J. (2007). The effect of court-ordered hiring quotas on the composition and quality of police. *American Economic Review*, 97(1):318–353.

McElvain, J. P. and Kposowa, A. J. (2008). Police officer characteristics and the likelihood of using deadly force. *Criminal justice and behavior*, 35(4):505–521.

Mears, D. P., Craig, M. O., Stewart, E. A., and Warren, P. Y. (2017). Thinking fast, not slow: How cognitive biases may contribute to racial disparities in the use of force in police-citizen encounters. *Journal of Criminal Justice*, 53:12–24.

Mitchell, O. and Caudy, M. S. (2015). Examining racial disparities in drug arrests. *Justice Quarterly*, 32(2):288–313.

Mooney, A. C., Giannella, E., Glymour, M. M., Neilands, T. B., Morris, M. D., Tulsky, J., and Sudhinaraset, M. (2018). Racial/ethnic disparities in arrests for drug possession after california's proposition 47, 2011-2016. *American Journal of Public Health*, 108(8):987–993.

Mummolo, J. (2018). Modern police tactics, police-citizen interactions and the prospects for reform. *The Journal of Politics*, 80(1):1–15.

Nix, J., Pickett, J., and Wolfe, S. (2020). Ltesting a theoretical model of perceived audience legitimacy: The neglected linkage in the dialogic model of police–community relations. *Journal of Research in Crime and Delinquency*, 57(2).

Parker, K. F. and Maggard, S. R. (2005). Structural theories and race-specific drug arrests: What structural factors account for the rise in race-specific drug arrests over time? *Crime & Delinquency*, 51(4):521–547.

Petersilia, J. (2014). California prison downsizing and its impact on local criminal justice systems. *Harvard Law and Policy Review*, 8:327–358.

Pfaff, J. F. (2017). *Locked In: The True Causes of Mass Incarceration - and How to Achieve Real Reform*. Basic Books, Inc, New York.

Rasmusen, E., Raghav, M., and Ramseyer, M. (2009). Convictions versus conviction rates: The prosecutor's choice. *American Law and Economics Review*, 11(1):47–78.

Raz, J. (1972). Legal principles and the limits of law. *The Yale Law Journal*, 81(5):823–854.

Reinganum, J. F. (1988). Plea bargaining and prosecutorial discretion. *American Economic Review*, 78(4):713–728.

Rengifo, A. F., Slocum, L. A., and Vijay, C. (2018). From impressions to intentions: Direct and indirect effects of police contact on willingness to report crimes to law enforcement. *Journal of Research in Crime and Delinquency*, pages 1–39.

Rojek, J., Alpert, G. P., and Smith, H. P. (2012). Examining officer and citizen accounts of police use-of-force incidents. *Crime & delinquency*, 58(2):301–327.

Stuntz, W. J. (2011). *The Collapse of American Criminal Justice*. Belknap, Cambridge and London.

Sundt, J., Salisbury, E. J., and Harmon, M. G. (2016). Is downsizing prisons dangerous? the effect of california's realignment act on public safety. *Criminology and Public Policy*, 15(2):315–341.

Sutton, J. R. (2013). Symbol and substance: Effects of california's three strikes law on felony sentencing. *Law and Society Review*, 47(1):37–71.

Taylor, P. L. (2020). Dispatch priming and the police decision to use deadly force. *Police Quarterly*, 23(3):311–332.

Theobald, N. A. and Haider-Markel, D. P. (2008). Race, Bureaucracy, and Symbolic Representation: Interactions between Citizens and Police. *Journal of Public Administration Research and Theory*, 19(2):409–426.

Veblen, T. (1889). 1992. *The theory of the leisure class. New Brunswick, NJ: Transaction.*

Westley, W. (1953). Violence and the police. *American Journal of Sociology*, 59(1):34–41.

Williams, Morgan C, J., Weil, N., Rasich, E. A., Ludwig, J., Chang, H., and Egrari, S. (2021). Body-worn cameras in policing: Benefits and costs. Working Paper 28622, National Bureau of Economic Research.

Wright, R. F., Levine, K. L., and Miller, M. L. (2014). The many faces of prosecution. *Stanford Journal of Criminal Law and Policy*, 1(27):27–47.

Yun, M. (2022). *Three Essays in Law and Economics: Consequences and Formation of Legal Rules in Other Markets*. PhD thesis, Claremont Graduate University.