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Dedicated to my students in Division X

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ABSTRACT

Over the last decade, there have been remarkable reforms in criminal legal policy and practice in Cook County, Illinois. In this project, I evaluate these changes in terms of their impact on the enduring problem of mass incarceration. First, I consider how prosecutors handle the influx of misdemeanor arrests from so-called broken windows policing. Analyzing 2016 jail data using competing risk models, I predict half of Black public order defendants to be released on dropped charges after spending two weeks in jail. From this, I argue that police and prosecutors use pretrial detention as an unregulated tool of racial control. Next, I evaluate the decarcerative impact of felony charging reform. In December 2016, newly-elected State's Attorney Kim Foxx more than halved the number of felony retail thefts charged in Cook County in less than a month. Using a decade's worth of courtroom event data, I build a regression discontinuity design to quantify how this intervention changed outcomes across the criminal process. On one hand, I find evidence that increased leniency did cause a slight month-to-month increase in crime. On the other hand, though, I find evidence of reduced levels of pretrial detention. I also find that prosecutors maintain pre-intervention drop rates despite handling fewer cases. Overall, I argue that the clear decarcerative benefits of felony charging reform outweigh the slight month-on-month post-intervention increase in retail theft. The third and final chapter evaluates a January 2018 law defining sentences for illegal gun possession. This new act bifurcates subpopulations based on their criminal history: those with no history may be offered probation, while those with multiple convictions face harsher prison terms. This reform intensified a pre-intervention upward trend in the use of probationary sanctions for first-time gun possession. The length of probationary terms also increased while the length of prison terms decreased. That said, these analyses also revealed a notable discontinuity in Summer 2016 caused by an internal policy change that increased the plea bargaining discretion of courtroom prosecutors. As with legislative reforms, this new organizational policy increased the length of probationary sentences for first-time offenders while decreasing the predicted length of prison terms for both groups.

CHAPTER 1

INTRODUCTION

The last decade has brought remarkable reforms in criminal legal policy and practice in state and local jurisdictions across the United States. In this project, I evaluate these changes in terms of their impact on the enduring problem of mass incarceration. At the beginning of the 21st century, the U.S. inmate population represented the largest number of incarcerated people in the history of the prison, worldwide (Pettit, 2012). This grim distinction has endured throughout recent decades. Although populations have declined marginally since 2009, there remain approximately 2.3 million people behind bars nationwide (Sawyer and Wagner, 2020). It is also crucial to recognize that mass incarceration often extends beyond prison or jail walls (Miller, 2021). There are approximately 7.7 million living under non-custodial surveillance, including warrants, parole, probation or community supervision, and over one hundred million adults have experienced the incarceration of a close family member (Sawyer and Wagner, 2020).

Mass incarceration is inextricably linked to systemic racial discrimination and economic inequality, both past and present (Alexander, 2012; Muhammad, 2011; Murakawa, 2014). One-quarter of incarcerated people are African-American, despite being less than 15 percent of the total U.S. population (Sawyer and Wagner, 2020). Latino men are twice as likely to be incarcerated as their white male peers (ibid.). These trends are exacerbated by poverty: those in custody earn a median annual income that is approximately half of the general population (ibid.). Over decades, scholars have documented the myriad harms that mass incarceration has directly caused poor communities of color (Clear, 2007; Pettit and Western, 2004; Tonry, 2011). These harms include a lack of voting rights, limited employment prospects, family disruption, and lack of housing, health care and educational opportunities (Comfort, 2016; Harding et al., 2014; Manza and Uggen, 2006; Wakefield and Wildeman, 2013; Western, 2002). Many have lost their fundamental faith in the legitimacy of key social and political institutions after coming into contact with the criminal legal system (Brayne, 2014; Goffman,

2014; Lerman and Weaver, 2014). Most recent scholarship attributes this unprecedented level of incarceration, not to crime rates, but rather to punitive policies and practices enacted by legislatures, courts, police and prosecutors (Barkow, 2019; Blumstein and Beck, 1999; Campbell and Schoenfeld, 2013; Goodman et al., 2014; Raphael and Stoll, 2013). In the following chapters, I focus on each of these institutional actors in turn.

The first chapter begins by considering how prosecutors handle the influx of misdemeanor arrests from so-called broken windows policing. In the 1980s, policing changed in racially segregated urban neighborhoods from scattershot brutality and neglect to a newly disciplined and organized tactical repertoire (Garland, 2001; Muhammad, 2011). Whereas traditional policing strategies react to a particular harm to an identifiable victim, police began to proactively surveil and incapacitate disorderly, "disreputable" or "unpredictable" people (Wilson and Kelling, 1982). This strategy assumes public disorder (a broken window, for example) signals neighborhood tolerance for any unlawful activity, big or small, leading to a criminogenic social environment.[1] Thus, 'disorderly' people should be preemptively controlled by police before they commit or encourage more serious crime (ibid., see also Harcourt 2001). It is well-established that people of color are surveilled, detained and arrested more often than their white counterparts for broken windows offenses (Meehan and Ponder 2002; Golub, Johnson, and Dunlap 2007; Harcourt and Ludwig 2007; Gelman, Fagan, and Kiss 2007; Antonovics and Knight 2009; Kutateladze et al. 2014; Peterson, Omori, and Lautenschlager 2018; Peterson and Omori 2020). This holds whether police are trying to meet a departmental arrest quota (Moskos, 2008); engaged in "therapeutic policing" (Stuart, 2016); or responding to third-party 311 complaints (Herring, 2019).[2]

In Chapter 1, I focus on one particularly harmful outcome of misdemeanor broken win-

1. Empirically, however, this outcome is questionable at best. While Skogan (1992) found early support for broken windows policing in reducing robberies, these results were later cast into doubt (Harcourt, 2001; Harcourt and Ludwig, 2006). Similarly, Sampson and Raudenbush (1999) cautioned that the mere coincidence of disorder and crime in neighborhoods does not demonstrate a causal link.

2. Arguably, the Supreme Court opened the door to formal, institutional practices of racial bias in policing when it legitimated the now-ubiquitous 'stop-and-frisk' tactic by lowering the threshold justifying a stop from probable cause to reasonable suspicion (*Terry v. Ohio*, 1968).

dows arrests: pretrial detention. Analyzing 2016 jail data using complementary competing risk models, I find over half of Black public order defendants are released on dropped charges after spending two weeks in pretrial detention. From this, I argue that police and prosecutors use the jail as an unregulated tool of racial control. This finding reveals an urgent problem, since even short-term periods of pretrial detention carry serious harms. Basic health care and hygiene is lacking in county jail facilities; they have been identified as a key vector of the spread of COVID-19 in the U.S. (ACLU Analytics, 2020).[3] Further, suicide has been the leading cause of death in jails for the last decade (Carson and Cowhig, 2020; Fernandes, 2020).[4] Beyond physical and mental illness, defendants who are detained until their minor charges are dropped also tend to lose trust in the legitimacy of key social institutions. Thus, short-term pretrial custody has been associated with decreases in voting among African-Americans (White, 2019), as well as avoidance of important educational, financial, labor market and medical institutions (Brayne, 2014; Dobbie, Goldin, and Yang, 2018). Worse, even factually innocent defendants may plead guilty to replace indeterminate pretrial detention with noncustodial punishment like court supervision or fines (Acker, 2017; Feeley, 1979; Natapoff, 2018). In turn, convicted misdemeanants must carry the mark of a permanent criminal record, with significant economic and political collateral consequences. These collateral consequences are often compounded by racial stigma (Albonetti and Hepburn, 1996; Pager, 2007; Jacobs, 2015; Dobbie et al., 2018), and minority misdemeanants are left uniquely vulnerable to cycles of re-criminalization (Barrett, 2017; Natapoff, 2018; Dobbie et al., 2018).

In Chapter 2, I evaluate the decarcerative effects of recent, dramatic shifts in the charging practices of the county prosecutor. After the 'tough on crime' turn solidified in the

3. Recently, 15.9% of all COVID-19 cases in Chicago were associated with short-term stays in Cook County Jail (Reinhart and Chen, 2020).

4. In Chicago, Tyler Lumar died in 2018 from brain injuries suffered from a 2016 suicide attempt in police lock-up. Lumar was detained on an outstanding warrant for a $25 dollar traffic fine that was paid four days late (Schmadeke, 2017). Elsewhere, this suicide rate has been tragically illustrated by the high-profile suicides of Sandra Bland and Kalief Browder (Gonnerman, 2015; Montgomery, 2019). Bland, Browder, and Lumar were all African-American. None were convicted of the charge for which they were detained.

early 1980s, local prosecutors became arguably the most powerful actor in the criminal legal process (Davis, 2007; Jacoby, 1980; Pfaff, 2017; Sklansky, 2018a). On one hand, aggressive policing supplied them with burgeoning caseloads (Gershowitz and Killinger, 2011). On the other, inflexible sentencing and expanded criminal liability provided prosecutors with an unprecedented level of control (Sklansky, 2018b). Discretionary authority shifted to the earlier charging decisions made by prosecutors as later sentencing options became increasingly limited and uniform (Sklansky, 2018b).[5] Generally, prosecutors used their increased discretion to file more charges of higher severity. An overcharging strategy is useful to win convictions under a system dominated by plea bargaining (Beckett, 2018; Davis, 2019; Gordon and Huber, 2009; Stuntz, 2011). If prosecutors begin with the most severe plausible charge, or pile on multiple charges, then defendants are confronted with the risk of lengthy prison sentences (or even death).[6] Faced with this dire prospect, individuals become much more likely to waive their trial rights and bargain for a lower punishment term. Of course, this incentive holds even if the defendant is factually innocent (Dervan and Edkins, 2013). Further, since prisons are largely state-funded while most prosecutors rely on county budgets, prosecutors had no financial incentive to limit prison populations (Barkow, 2019; Zimring and Hawkins, 1991). Thus, incarceration skyrocketed.

Recently, though, prosecutorial elections have emerged as a key battleground in the fight for decarceration: since 2016, fifty-one reformist candidates have won office in large urban jurisdictions around the country, including Chicago. (Balboni and Grometstein, 2020; Bazelon, 2019; Bellin, 2020; Davis, 2019; Sklansky, 2017).[7] Here, in December 2016, newly-

5. In the U.S., prosecutors enjoy a monopoly on the ability to pursue or drop criminal charges. They are constitutionally protected from legislative or judicial pressure in charging, and no private party may initiate or influence a criminal prosecution (*Inmates of Attica Correctional Facility v. Rockefeller*, 1973; Bellin, 2018; Davis, 2007)

6. This practice was condoned by the Supreme Court in *Brady v. United States* 1970 and *Bordenkircher v. Hayes* 1978.

7. In addition to the District of Columbia, the only states that appoint their head prosecutors are Alaska, Connecticut, Delaware and New Jersey. Federal prosecutors are appointed by the President. Electoral upsets have occurred in only a small fraction of the 2,500 local criminal prosecutors offices across the country. Hessick (2020) demonstrates great variation in electoral contestation across jurisdictions. The urban/rural divide is

elected Cook County State's Attorney Kim Foxx raised the threshold value defining felony retail theft from $300 to $1,000. Under the new policy, her administration more than halved the number of felony retail theft charges filed in the county by January 2017.

I use this sudden drop in felony retail theft charges to build a regression discontinuity design that quantifies the impact of charging reform at the beginning, middle and end of the criminal process. On one hand, I find evidence that this policy did result in a slight increase in the slope of retail theft incidents using data from the Chicago Police Department. On the other hand, I find it successfully decreased pretrial custodial booking using data from the Cook County Sheriff. Similarly, State's Attorney administrative data show that prosecutors maintained pre-intervention drop rates even when faced with dramatically fewer cases. Finally, punishment type and length remains broadly comparable to pre-intervention levels, with a small observed drop in the length of probationary terms. Overall, I argue that the decarcerative benefits of the policy reform outweigh a slight month-to-month increase in retail theft incidents, not only because of quantifiable decarcerative gains, but also as symbolic of a newly-expanded 'public' whose interests the prosecutor must both define and protect. While it remains to be seen if similar reforms succeed in other jurisdictions, I suggest that felony charging reform in Cook County offers a cautiously optimistic case study in the path to ending mass incarceration.

In the third and final chapter, I consider the decarcerative impact of recent sentencing reforms introduced by the Illinois state legislature. As with police and prosecutors, lawmakers have been identified as crucial architects of mass incarceration (Blumstein and Beck, 1999; Campbell and Schoenfeld, 2013; Goodman et al., 2014; Raphael and Stoll, 2013). From the 1980s onward, state lawmakers elected on punitive platforms expanded statutory criminal

particularly salient, with little evidence to suggest electoral reform will occur outside of dense, diverse and progressive-leaning urban areas. Instead, rural jurisdictions may follow historical trends where incumbent prosecutors run unopposed, and are re-elected at very high rates with low turnout (Wright and Miller, 2002). Barkow (2021) also notes that losses have also been suffered by reformist candidates running in sympathetic large urban jurisdictions. Though broad electoral trends are beyond the scope of the present study, this variation presents a pressing area for further research.

liability and increased the severity of sanctions (Barkow, 2019; Gottschalk, 2015). As more individuals entered the system, their alleged crimes became more likely to result in a longer, inflexible prison sentence with little room for judicial or parole-board discretion (Beckett, 2018). Importantly, not only offenders classified as violent or recidivist were incarcerated. Long prison sentences were also used to sanction (and control) those accused of property and drug crimes (Alexander, 2012; Garland, 2001). These tough on crime lawmakers were willfully ignorant of— even hostile to— credible scientific research into the factual origins and appropriate responses to crime (Garland, 2001).

Now, however, punitive lawmaking has lost much of its once-bipartisan appeal. The urgent problem of mass incarceration has become a rare point of consensus shared by fiscal conservatives and social progressives, establishment reformists and grassroots activists alike (Aviram, 2011; Dagan and Teles, 2014). Both Democrats and Republicans promote a cost-benefit approach to decarceration: they agree that it is a waste of tax-payer dollars to imprison individuals who pose no risk to public safety (Aviram, 2011; Richardson and Kutateladze, 2021). Among Democrats, however, these calculations also tend to highlight the well-documented psychological, economic, political and familial harms of mass incarceration on Black, Latinx and Indigenous communities, especially those living in poverty (Clear, 2007; Harding et al., 2014; Lerman and Weaver, 2014; Pettit and Western, 2004; Tonry, 2011; Manza and Uggen, 2006; Wakefield and Wildeman, 2013; Western, 2002). Left-leaning politicians now often promise to end the dramatic overrepresentation of poor people of color in U.S. prisons and jails populations (Drakulich et al., 2020).

Lawmakers across the ideological spectrum have embraced a new so-called smart on crime approach that aims to decarcerate using empirical evidence, administrative expertise and targeted, non-custodial interventions (Richardson and Kutateladze, 2021; Seeds, 2017). In practice, smart reforms generally use two sequential strategies: first, they define and bifurcate 'violent' from 'non-violent' crimes in order to reserve leniency for the latter (ibid.); second, they enact leniency by replacing prison terms with probationary surveillance (Beckett

and Murakawa, 2012; Miller, 2021). I define this reform strategy as bifurcated probation. I operationalize its decarcerative impact using the example of the 'Safe Neighborhoods Reform Act,' passed by the Illinois legislature in January 2018 (Illinois General Assembly, 2018). This new law bifurcates subpopulations convicted of gun possession based on their criminal history. Those with no history may be offered probation, while those with multiple serious convictions face harsher prison terms (Unified Code of Corrections, 1972, 4.5-110, 6-3.6). I find this legislative sentencing reform to have intensified a pre-intervention upward trend in the use of probationary sanctions for first-time gun possession. It also increased the length of probationary terms served by this subgroup, while decreasing their predicted prison terms. Surprisingly, though, this reform also decreased the predicted punishment term for gun possession recidivists sentenced to prison.

These analyses also revealed an earlier, unexpected discontinuity in the predicted length of both types of punishment. This is likely because, after being inaugurated in December 2016, State's Attorney's Kim Foxx instituted a new policy that allowed prosecutors to offer a plea deal *without* the prior approval of their unit supervisor. Individual prosecutors were therefore no longer subject to routine managerial oversight of the plea bargaining negotiations in their courtrooms. As with legislative reform, this new organizational policy increased the length of probationary sentencing for first-time gun possession while decreasing the predicted length of prison terms for both subpopulations. Although the final chapter is primarily focused on legislative change, future research will further develop the practical and theoretical implications of this unexpected two-fold discontinuity. It offers a unique opportunity to quantify the relative effect of internal, organizational factors versus external, legislative factors in sentencing outcomes. Future research will also supplement initial parametric results with non-parametric methods. I also intend to use segmented parametric survival analysis to test whether bifurcated probation increases the risk of probationary failure for first-time gun offenders. After all, it is plausible that this sentencing reform may in fact produce a comparatively longer custodial term for those first-time probationers who

violate the extensive surveillance requirements attached to their initial sanction.

Finally, a note on the benefits and limitations of the single-case focus on Cook County, Illinois. Police, prosecutors and lawmakers in this jurisdiction have historically been at the forefront of both punitive and decarcerative shifts in urban punishment (Muhammad, 2011). In 2020, for example, Illinois lawmakers became the first in the U.S. to directly legalize the recreational sale and possession of cannabis (Cannabis Regulation and Tax Act, 2019).[8] Similarly, State's Attorney Kim Foxx's 2016 victory was one of the first instances of the ongoing trend of upset prosecutorial elections in large urban jurisdiction across the U.S. Thanks to these early shifts, I am able to analyze years of data tracking the systemic outcomes of both charging and sentencing reforms. As other jurisdictions debate similar reforms, this work contributes timely empirical insights into how criminal law enforcement should be restructured in order to end mass incarceration.

That said, findings from Cook County cannot be immediately generalized to other jurisdictions across the U.S. For example, the county is home to one of the nation's busiest criminal courts (Gershowitz and Killinger, 2011). There can thus be little doubt that Cook County has a "high-crime, low-resource" legal system (Allen et al., 2016, 6). For this reason, results from this case make not be replicated in either the "high-crime, high-resource" federal system, or in the "low-crime, low-resource" systems of rural counties (ibid.). Further, Mayson and Stevenson (2020) describe Cook County as an exceptional locale even among similar large urban counties: compared with similar misdemeanor courts, Cook County has the lowest conviction rates, coupled with the highest racial disparities in public order case filings. With this caveat in mind, then, this project offers a template for approaching an emerging source of quantitative evidence that can be used to make inferences about how police, prosecutors and legislatures may maintain or disrupt the incarcerative status quo.

8. Before Illinois, all successful cannabis legalization efforts resulted from voter-lead ballot initiatives. This new statute explicitly recognizes and attempts to remedy the disproportionate "emotional, psychological and financial harms... [which] certain communities have disproportionately suffered" because of cannabis prohibition (ibid, §7-1e). As such, it includes provisions for the expungement of past convictions and requires cannabis businesses to employ formerly incarcerated people (Ahern, 2019).

CHAPTER 2

RACIAL DISPARITY IN BROKEN WINDOWS PRETRIAL DETENTION

When an individual is arrested, charged and convicted of a crime in the United States, it is most likely for a misdemeanor offense. Compared to felony case filings, estimates suggest there are three to four times as many misdemeanor cases per year: approximately 12 to 13 million (Stevenson and Mayson 2017, c.f. Bibas 2019). However, these numbers are imprecise, because quantitative misdemeanor data are "absurdly, embarrassingly, vanishingly" scarce (Stevenson and Mayson, 2017, 732). This scarcity is particularly severe post-arrest; there is an acknowledged lack of basic factual knowledge about how misdemeanor cases are charged, adjudicated and punished at scale in U.S. criminal courts (ibid., see also Natapoff 2018). How, then, do prosecutors handle the large number of misdemeanor arrests brought to them by police?

Two theories compete to explain how prosecutors deal with misdemeanor arrests: first, the assembly line model, and second, the managerial model. The first model expects prosecutors to quickly transform arrests into criminal convictions as if working on a legal assembly line (Packer, 1968; Barrett, 2017; Natapoff, 2018). In contrast, the second model argues that prosecutors actively categorize, evaluate and manage arrests to achieve the maximum level of defendant control with the minimal expenditure of court resources (Feeley, 1979; Garland, 2001; Sklansky, 2018a; Kohler-Hausmann, 2018). Both theories were developed through ethnographic observation; thus, it remains to be seen which theoretical model better explains how prosecutors handle misdemeanor arrests in aggregate.

In this chapter, I adjudicate between these two theories of misdemeanor prosecution using novel custodial data from Cook County, Illinois. My analysis is focused on institutional patterns of interaction between police and prosecutors. Both sets of actors possess considerable autonomy, yet are mutually interdependent: while prosecutors are constrained by the

information and arrests provided by the police, police cannot force prosecutors to pursue or drop criminal charges. Further, in busy misdemeanor courts, traditional law enforcement boundaries are blurred. Cook County, for example, grants police officers the authority to directly charge misdemeanor offenses upon arrest without immediate review (Code of Criminal Procedure, 1963, 109-3.1(a)). This abridged charging process is a "widespread practice" nationwide (Horwitz, 1998, 1306). Unfortunately, most empirical studies of misdemeanor law enforcement overlook this crucial nexus of discretionary authority between police and prosecutors.

Instead of police-prosecutor interactions, law enforcement scholars traditionally focus on the impact of the police in the neighborhoods they patrol (see for example Skogan 1992; Herbert 1997; Sampson and Raudenbush 2004; Desmond and Valdez 2012; Stuart 2016). It remains unknown, however, how prosecutors handle racially-skewed 'broken windows' arrests that police claim will maintain neighborhood order. Do prosecutors treat misdemeanor charges that allege a particular harm to an identifiable victim— shoplifting, for example— differently than alleged offenses against public order? Do they handle broken windows arrests differently depending on the race or ethnicity of the defendant? In this chapter, I offer evidence that prosecutors are more likely to quickly convict misdemeanor charges with a particular, identifiable victim, regardless of the defendant's race or ethnicity. Conversely, they are slow to drop charges filed by the police against Black broken windows defendants. These results suggest that misdemeanor prosecution depends upon both the type of policing and the race of the defendant. They also offer an institutional mechanism explaining persistent racial disparities documented by traditional neighborhood-focused research on broken windows policing. Broadly, then, I advocate a shift of disciplinary focus away from neighborhoods and towards key structural relations *within* the criminal justice system itself.

Finally, I propose one concrete policy reform based on my findings: police should not directly charge misdemeanors without review. Unlike prosecutors, police have no formal legal training, little to no professional or ethical oversight, and no obligations to adversar-

ial safe-guards like judges, juries and defense counsel (Kagan, 2019). My results suggest that police use their charging authority to temporarily appropriate the coercive tools of the court— specifically, pretrial detention— to control, manage, and perhaps even harass Black populations they perceive as 'disreputable' or 'unpredictable.' As a result, Black defendants are more likely to wait for their broken windows charges to be dropped in the degrading and dangerous environment of county jail.

2.1 Two Models of Misdemeanor Crime Control

It is contested how prosecutors deal with the influx of low-level arrests in our era of broken windows policing. In this section, I summarize two dominant models emerging from decades of courtroom observation: first, the assembly line model, and second, the managerial model. Despite their differences, however, both are positioned against the familiar adjudicative ideal of the criminal trial. Under this ideal, law enforcement must navigate a procedural "obstacle course" (Packer, 1968, 163). Police and prosecutors should work together to surmount these obstacles. Procedural fidelity should outweigh efficient crime control (*Mapp v. Ohio*, 1961). The defendant should be assumed innocent until convicted.[1]

Herbert Packer (1968) influentially argued that this adjudicative ideal is untenable in misdemeanor courts. Instead, he argued, lower courts follow the ideal of "crime control" (ibid.:89). Packer's argument proved prescient, as crime control would dominate the "countertrends" of criminal procedure throughout the 1970s and 80s (Aviram, 2011, 244). As the crime rate rose, politicians sought to control segregated urban neighborhoods that had become a metaphorical battleground of the War on Crime (Muhammad, 2011). Thus, police were encouraged to surveil, arrest, detain and incapacitate, not as a means to an eventual

1. Not so long ago, police and prosecutors used their discretionary power as an explicit weapon of racial discrimination and white supremacist violence (Klarman, 2000). From roughly 1953 until 1969, however, the Supreme Court lead by Chief Justice Earl Warren began to systemically constrain the legitimate methods available to racially-motivated law enforcement in Southern states (Packer, 1968; Steiker, 1996; Amar, 1996). This procedural revolution put forward an enduring and influential view of interaction between police and prosecutors: the adjudicative ideal.

conviction in the courts, but as an end in itself: to efficiently mitigate the risk of future crime. This ascendant ideal lead to a nationwide flourishing of broken windows policing (Feeley and Simon, 1992; Garland, 2001; Harcourt, 2001, 2007).

As broken windows arrests flooded the courts, prosecutors found themselves facing unprecedented caseload pressure (Kohler-Hausmann, 2018; Natapoff, 2018). In turn, they became increasingly trapped by the classic dilemma of the "street-level bureaucrat" (Lipsky, 1980). One one hand, prosecutors must quickly allocate scarce resources with an eye to treating like cases alike. One the other hand, though, they must carefully consider the specifics of a given case. Both models of broken windows prosecution insist upon one horn of this dilemma. The first model is focused on the mass scale, speed and uniformity of case processing. I refer to this as the the assembly line model. The second model highlights how law enforcement actors make discretionary, deliberate interventions tailored to specific situations. I refer to this as the managerial model. Both models define opposite "poles on a continuum" of practices observed over decades in misdemeanor courtrooms (Kohler-Hausmann 2018, 285 fn. 2; see also Natapoff 2019, 1669-1671). It remains to be seen, however, which model is more useful to explain how broken windows cases are prosecuted in aggregate.

It was also Herbert Packer who first analogized the lower courts to an "assembly line" (1968:345). Less than a decade later, the Supreme Court used it to describe the misdemeanor system as a whole (*Argersinger v. Hamlin*, 1972). This model expects prosecutors to quickly transform arrests and charges into criminal convictions. Cases that are factually tenuous or procedurally difficult should be immediately screened out lest they slow down the entire production line. The cases that remain are "probably guilty" (Packer, 1968, 160). Under this presumption of guilt, arrests should be dealt with en masse using "sloppy and fast" work routines (Natapoff, 2018, 64). Police and prosecutors work in passive isolation at the assembly line, performing "the same small but essential operation" on each case (Packer, 1968, 159). Over the last decades, the assembly line has had to work harder to process the massive amount of broken windows cases. To help, prosecutors tend to rely on racial

and ethnic stereotypes as heuristics to quickly resolve these cases; thus, Black and Latinx defendants are more likely to be presumed guilty and convicted on broken windows charges (Natapoff, 2018).

Some scholars have criticized the use of the assembly line metaphor as an inaccurate portrayal of misdemeanor courts. While Feeley (1979) offered the seminal formulation of this critique, Kohler-Hausmann (2018, 61) has recently elaborated upon this alternative "managerial model" of prosecution in the broken windows era. Here, the prosecutor's guiding purpose is neither protecting individual innocence nor predicting guilt, but identifying subpopulations of risk and adjusting court interventions accordingly (Garland, 2001; Kohler-Hausmann, 2018). Prosecutors are not passive industrial drones, but rather "redefine or translate... the arresting officer's... definitions to suit their own understandings and purposes" (Feeley, 1979, 117). Work is neither fast nor sloppy, but conducted at a purposive pace: police and prosecutors often use *delay itself* to ensure defendant compliance. For example, prosecutors may simply drop the charges of defendants after a period in jail. This "procedural hassle" degrades the defendant through arrest; delays the case through stressful, repeated court appearances; and denies the defendant liberty in order to remove 'disorderly' populations from the street (Kohler-Hausmann, 2018, 183). It also maximizes defendant compliance with a minimal expenditure of court resources. Targeted populations tend to be Black and Latinx, making "arrests that do not lead to a criminal conviction" the most "important site to study racial disparities in the subfelony world" (Hepburn, Kohler-Hausmann, and Medina, 2019, 1168).

2.1.1 Research Questions

These two models of crime control offer competing hypothetical answers to how broken windows arrests are handled once they reach misdemeanor courtrooms. I operationalize broken windows offenses as those misdemeanors that do not entail any particular harm to an identifiable victim. Specifically, they include: trespassing and loitering; panhandling; prostitution;

selling loose cigarettes; gambling; drinking alcohol in public or public intoxication; disorderly conduct; vandalism and property damage.[2] Throughout, I compare these broken windows offenses with shoplifting. Shoplifting is a useful comparison because, unlike broken windows offenses, it requires a particular harm to an identifiable victim: a retail store. Analytically, then, I assume retail theft is the result of traditional, reactive policing tactics that responds to a victim complaint. In contrast, I assume broken windows charges to be the result of the proactive policing of people perceived as "disreputable" by the police (Wilson and Kelling, 1982).

Do prosecutors treat traditional misdemeanor charges like shoplifting differently than alleged broken windows offenses? Do prosecutors handle broken windows arrests differently depending on the race or ethnicity of the defendant? The assembly line model would expect broken windows defendants to be more quickly convicted of a misdemeanor versus those charged with shoplifting. Similarly, Black and Latinx broken windows defendants would be processed more quickly than their white counterparts. In contrast, the managerial model would expect broken windows defendants to be more likely to have their charges dropped after waiting longer than comparable shoplifting defendants. Again, Black and Latinx broken windows defendants would be expected to wait longer for a dropped charge as compared to white defendants. In what follows, I adjudicate between these competing hypotheses using the case of misdemeanor prosecution in Cook County, Illinois.

2.2 Case, Data and Methods

My data are custodial administrative records capturing all broken windows and shoplifting defendants released from Cook County Department of Corrections (CCDOC) custody in 2016. The unit of analysis is the unique conjunction of case and individual. I cannot

2. Unlike previous broken windows research, I exclude all misdemeanors based on marijuana possession (c.f. Kelling and Coles 1997; Harcourt and Ludwig 2007; Geller and Fagan 2010; Peterson et al. 2018). This is because the Illinois state legislature decriminalized low-level marijuana possession mid-way through my 2016 sampling year.

identify individuals who may be released and booked multiple times throughout the year on different cases. Throughout, I define misdemeanors narrowly to include only charges based on the Illinois criminal code (Criminal Code, 2012).[3] I further limit my sample to non-violent misdemeanors, excluding, for example, any individual facing at least one assault, domestic battery, driving under the influence, or firearm-related charge. I exclude these charges because these sorts of misdemeanors are perceived as more serious, quasi-felony offenses (Barrett, 2017).[4] I exclude individuals facing simultaneous broken windows and shoplifting charges. The total sample size is $n = 5,340$.

All analyses occur within three defendant racial or ethnic groups: Black, white, and Latino.[5] I also adjust models for a variety of demographic covariates, including defendant gender, age, and residential poverty. From residential zip code, I match within-racial group poverty from the American Community Survey to the defendant's reported race/ethnicity and zip code. This "racially-bounded" poverty covariate follows the recommendation of Shihadeh and Steffensmeier (1994). Neighborhood poverty might also roughly capture the likelihood of a defendant using the services of the Cook County Public Defender, since defendants must demonstrate indigence to qualify for free counsel (General Administrative Order Number 2013-11, 2013). Unfortunately, I cannot directly observe whether the defendant used public or private defense counsel.[6] Finally, I include an indicator variable to capture defendants who are reported to be homeless at the time of booking.

3. Subsequent research would do well to reproduce my analyses using other subfelony charges like traffic and municipal ordinance violations.

4. I discuss the bifurcation of violent charges at greater length in Chapter 4.

5. Throughout the analyses, I use masculine forms and pronouns to represent the male reference group.

6. I can assume that every defendant had at least minimal access to an attorney, since indigent defense services are guaranteed by the Public Defender's Office for all state-level criminal offenses in Cook County. This guarantee includes misdemeanors that do not (or cannot) result in a custodial sentence. Thus, it offers indigent defendants more expansive representation than that which has been deemed constitutionally required in *Scott v. Illinois* (1979).

2.2.1 Legal Variables in Local Context

In 2016, over ninety-six thousand new misdemeanor cases were filed in Cook County, representing nearly two-thirds of all criminal prosecutions in the jurisdiction (Annual Report of the Illinois Courts, 2016, 47). Roughly, this translates to over five thousand new misdemeanor cases entering each courtroom in 2016, with each prosecutor opening approximately two thousand new cases.[7] Generally, prosecutors will have had only two to three years experience before being rotated into these busy misdemeanor courtrooms.

In Cook County, a misdemeanor defendant may be released from jail due to one of four kinds of events: bond release; dropped charges; post-conviction; or other, rarer outcomes like being found not guilty at trial,[8] transfer, or death. In my analyses, the primary event of interest is release after charges have been dropped. The rate of dropped misdemeanors evidences the extent to which prosecutors handle arrests using procedural techniques that are formal (i.e. conviction) or informal (i.e. pretrial detention). Further, the timing of dropped charges evidences if these techniques are used quickly, as in an assembly line, or if prosecutors take time to complete their managerial task. To my knowledge, no previous quantitative study has yet investigated the timing of dropped misdemeanor charges at the case level.[9]

The competing event is post-conviction release. I take drop and conviction outcomes to

7. Guidelines suggest attorneys should handle no more than four hundred misdemeanor cases per year (Gershowitz and Killinger, 2011; American Prosecutors Research Institute, 2002).

8. Less than 0.02% of defendants in my sample are found not guilty at trial. Unfortunately, I cannot distinguish those defendants whose trials conclude with a finding of guilt with those who plea guilty. I expect Cook County misdemeanor trial rates to hover at or below 1% following existing studies of similar jurisdictions (Kohler-Hausmann, 2013; Leslie and Pope, 2017; Mayson and Stevenson, 2020; Peterson and Omori, 2020).

9. Dropped events in Cook County take in one of two forms: first, *nolle prosequi* or 'nolle,' and second, "strik[ing] a case on leave to reinstate" or 'SOL' (*Ferguson v. City of Chicago*, 2004, ¶14). While a nolle disposition wholly terminates the charge at the prosecutor's discretion, for a SOL the criminal charge remains pending, "albeit in a dormant state" (ibid.). In Illinois, unlike other jurisdictions, this dormant charge requires no further action from any of the parties and automatically expires after five months (*People v. East-West University, Inc* 1994; cf. Kohler-Hausmann 2018). Since reinstatement almost never occurs in practice, I group both nolle and SOL dispositions in one dropped release event category (Annual Report of the Illinois Courts, 2016, 47).

be mutually exclusive. This assumes that prosecutors do not "split the difference" with misdemeanor charges as they tend to do with felonies, by offering to drop one charge in exchange for a guilty plea on the other (Feeley, 1979, 134). This sort of charge bargaining is rare for non-violent misdemeanors: over 90% of my sample is composed of single-charge cases. I use an indicator variable to denote case with multiple charges, and I use the most punitive exit event in the rare instances where one defendant experienced divergent outcomes. For example, a defendant who is convicted on one charge and not prosecuted on another will be classified as released post-conviction. All misdemeanors are eligible for non-custodial punishments, including time served, fines, conditional discharge, and probation (Unified Code of Corrections, 1972). In my sample, only 11% of convicted misdemeanants are sentenced to punitive custody.

My dependent variable is the length of detention until post-conviction or drop release. Figure 2.1 illustrates the typical stages followed in this process, which may stretch on over weeks, even months, or may occur simultaneously during the initial appearance.

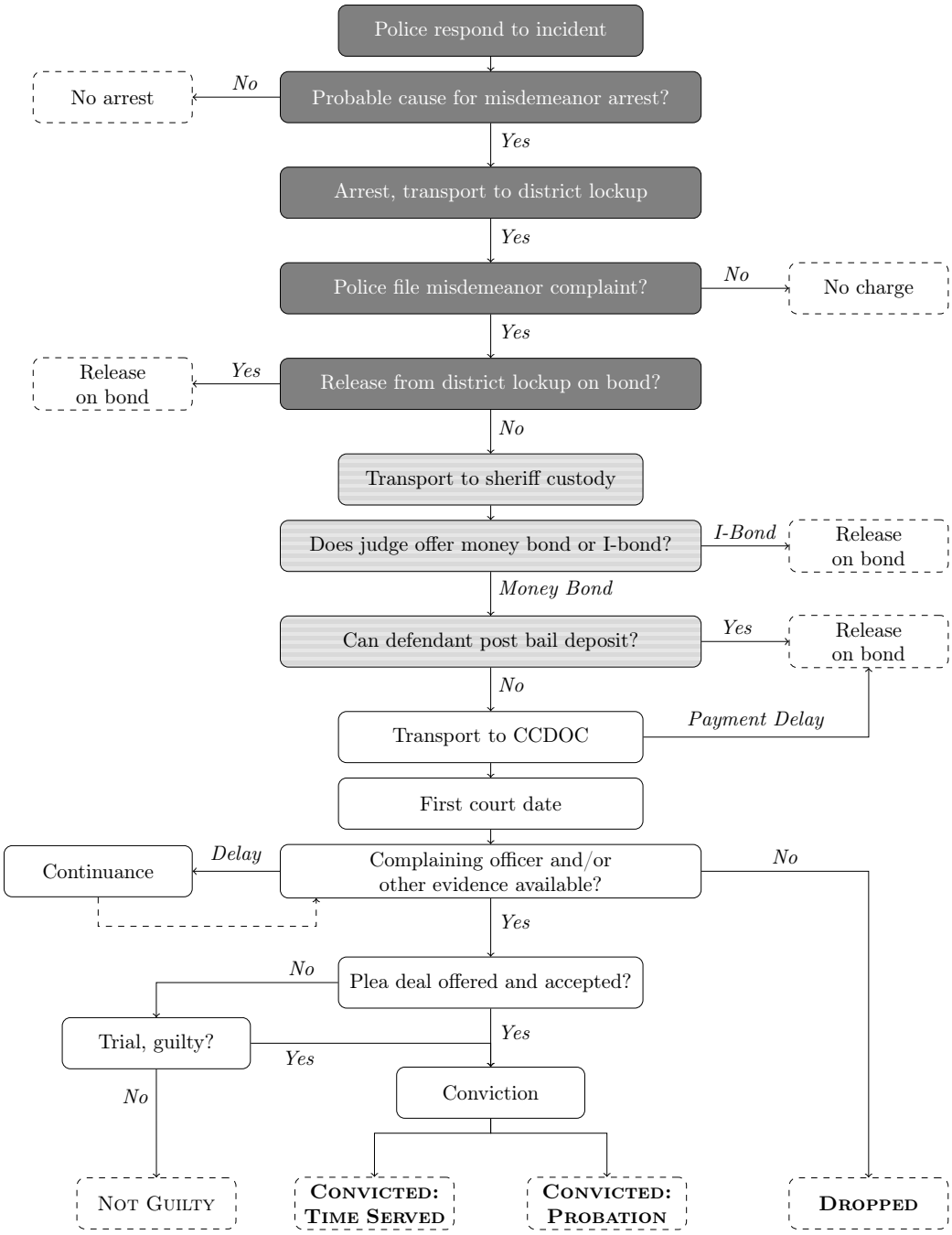


Figure 2.1: Decision points in the misdemeanor process

Note: Events exiting the criminal misdemeanor process are indicated by broken boxes. Dark solid grey boxes are unobserved selection mechanisms. Defendants enter my dataset when they are booked into sheriff custody. Thus, light gray lined boxes indicate bond decision-points that treated as censored observations.

It is important to note that this interval begins at jail intake. This leaves unobserved the

length and outcome of earlier police interactions as illustrated in Figure 2.1. I also cannot observe how many times the defendant appeared in court during this interval (cf. Feeley 1979). Instead, detainment length is calculated as the number of days between CCDOC intake and release date. I count those defendants who appear be booked into sheriff custody and released on the same day— usually on bond— to have been detained for a full day prior. One full day is a conservative way of accounting for the unobserved time the defendant spent in police custody before being transferred to county jail. By statute, this process must last no longer than 48 hours, since defendants arrested without a warrant must be given prompt judicial review of the probable cause for detention (Code of Criminal Procedure 1963, 109-1; see also *Gerstein v Pugh* 1975).[10]

Table 2.1 summarizes select legal and demographic covariates, including the proportion of missing observations completed using mean imputation.

10. In 2016, only 0.7% of my sample of misdemeanor defendants were released due to a lack of probable cause to detain.

	Broken Windows		Shoplifting	
	% or mean (sd)	% missing	% or mean (sd)	% missing
Booking Age	32.3 (11.7)	0	37.6 (12.67)	0
Male	81.9	0.2	70.6	0.3
Criminal Record	57.5	0	53.6	0
+1 Charges	5.2	0	6.0	0
I-Bond	34.1	0	29.0	0
Bail Amount	500.7 (1,155.9)	21.0	949.3 (2,194.0)	19.8
Poverty by Race	26.8 (11.3)	9.7	23.4 (11.7)	11.6
Homeless	7.2	40.7	8.4	36.4
Arrest				
Downtown	11.4	20.8	12.9	25.8
Chicago Police	72.5		47.8	
Suburban Police	23.2		40.7	
Bail Violation	4.2		11.4	

Table 2.1: Summary statistics of select covariates by charge type

Source: 2016 Cook County Dept. of Corrections custodial release records.

There are several additional legal covariates included in the models, such as an indicator variable for recognizance, or 'I-Bond,' release, or the deposit amount required to secure the defendant's release on money bail.[11] An indicator is also included as to whether the defendant has been convicted of a previous criminal offense in Cook County in the last six years. For charge severity, I take the most severe Class A misdemeanors as the reference

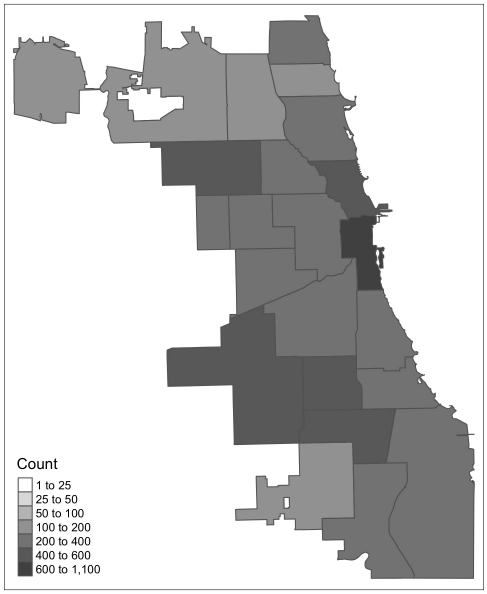
11. In Illinois, all misdemeanors are eligible for bond (Code of Criminal Procedure, 1963, 6.1c). Commercial bond agents are prohibited statewide. Recently, there have been several felony-level bond reforms in Cook County (see for example General Order Number 18.8A. Ill. Cir. Ct. Cook County, 2017). However, due to a lack of funding for pretrial services, these reforms were not extended to misdemeanor charges as planned on January 1, 2018 (see General Order Number 18.8B. Ill. Cir. Ct. Cook County, 2017), nor do they impact my 2016 data.

category, with other categories representing less frequent (and less severe) classes. Finally, I use booking date to construct an indicator for the month of arrest, and whether the defendant was booked on a weekday. Both may capture patterned variation in caseload and staffing levels over time.

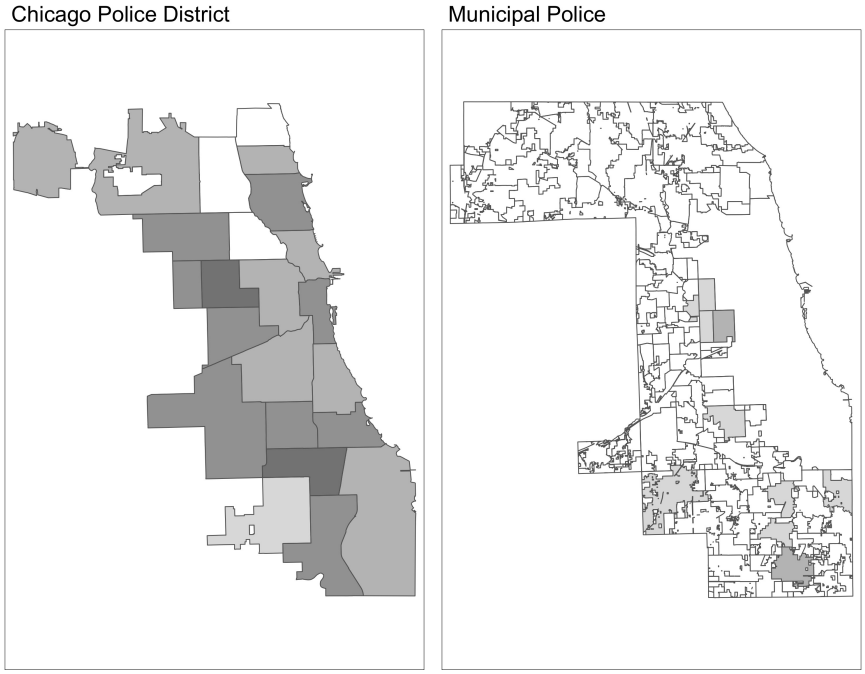
Previous research suggests that the location of arrest is associated with how misdemeanor offenses are both perceived by police and resolved in the courts (Sampson and Raudenbush, 2004; Peterson et al., 2018; Herring, 2019). Prosecutors may, for example, face negative reactions from business interests if they drop charges stemming from an incident in Chicago's downtown retail and tourism corridor. Or, they may be less eager to pursue criminal convictions against defendants arrested in seemingly "disreputable areas" (Sampson and Raudenbush, 2004, 319). For this reason, I include two ecological indicator variables: the first flags if the arrest took place in the downtown business and tourism district, while the second indicates if the arrest took place in Chicago's racially and economically segregated South or West side.

A final important covariate is the arresting police organization. The majority of arrests in Cook County are made by the largest police department in the county, and the second-largest in the U.S.: the Chicago Police Department (CPD). The CPD is organized into 22 geographic districts, 5 geographic areas, 6 specialized bureaus, and lead by a superintendent appointed by the city's mayor. Cook County is also policed by over one hundred smaller organizations, including suburban departments, university, and transport police. Figure 2.2A maps the total number of broken windows and shoplifting arrests reported by the CPD in 2016. Unfortunately, no other Cook County police department has published arrest-level data from this time period. In comparison, Figure 2.2B maps the CPD district or suburban police department of origin of those arrestees who enter my dataset after being transferred into sheriff custody. In all models, I include a categorical variable indicating the source of each arrest, whether by an independent suburban police department or CPD area. This variable also captures if the defendant was re-arrested by sheriff's deputies in court on a bail

violation.



(a) Focal misdemeanor arrests by Chicago Police district



(b) Arresting police department reported in custodial intake

Figure 2.2: Custodial selection after misdemeanor arrest in Cook County

Source: 2016 Cook County Dept. of Corrections custodial release records.

Comparing Figures 2.2A and 2.2B, it is clear that there are many misdemeanor arrests

that are not observed in custodial data. This is because police may resolve an alleged misdemeanor incident in a variety of ways without transferring the individual to county jail. The number of unique misdemeanor cases in CCDOC records comprise just under 40% of the total 2016 case dispositions in Cook County (Annual Report of the Illinois Courts, 2016, 47). Figure 2.2 also illustrates variation in bond decisions by Chicago Police district. While non-violent misdemeanor arrests are roughly evenly distributed across the city, districts on the South and West sides tend to send more arrestees to Cook County Jail.

2.2.2 Police Bond Release

Administrative jail records are produced by several unobserved police decisions that may bias the custodial data. From the onset, police may decline to formally arrest and/or charge. Instead, they may simply issue a move along order (Herring, 2019), write a summons or citation (ibid.), or "stop, question, and frisk" the suspect (Kohler-Hausmann, 2018, 42). Or, some individuals may be arrested, detained at the police station. and released without charge. Importantly, police in Cook County decide whether to charge a misdemeanor upon arrest without prosecutorial review (Code of Criminal Procedure, 1963, 111-2b).

These informal outcomes are especially prevalent in broken windows incidents. For example, in 2016, simultaneous arrest occurred in only 17 percent of the nearly thirty-nine thousand public order incidents reported by the CPD (Levy, 2020b). In contrast, the majority of shoplifting incidents were resolved with a simultaneous arrest. This difference may be explained by the police responding with formal, resource-intensive arrest when there is a particular harm alleged against an identifiable victim. A custodial sample is thus likely to underestimate the occurrence of informal contact between police and individuals suspected of disturbing the public order. To date, though, both the prevalence and impact of these informal interactions have been well documented in the literature on police-community relations. Here, I aim to study the relatively understudied interactions between police and prosecutors. Since prosecutors will only encounter defendants who have been formally ar-

rested and charged, I do not consider this selection mechanism to greatly impact my core analytic focus.

However, I expect a subsequent police decision to result in relevant bias in my custodial data: specifically, whether or not to release a charged misdemeanor defendant directly on bond. These misdemeanors bypass custodial data since defendants are never transferred into county jail. Comparing CCDOC booking and CPD arrest datasets (Levy, 2020a), there is a statistically significant difference in the proportion of broken windows versus shoplifting charges (χ^2 test $p < 0.001$). The relative proportions suggest Chicago Police may tend to release shoplifting defendants on bond more often than their broken windows counterparts. That said, there is no statistically significant difference in the relative proportion of racial and ethnic groups between datasets (χ^2 test $p = 0.7$). It is important to note that, in 2016, fewer than 1% of detainees were visited by an attorney in CPD lockup (Illinois Circuit Court of Cook County 2017, see also General Administrative Order Number 2017-01 2017). Thus, the police decision to offer bond is likely not related to the type of defense counsel used by the defendant, which I do not directly observe.

Substantively, I expect jail data to overestimate the rate of misdemeanor convictions while underestimating the time it take prosecutors to drop charges. This is because, on one hand, detained defendants have an incentive to plead guilty in exchange for release on probation or time served. On the other hand, bond release is likely positively correlated with the time it takes for prosecutors to drop charges against the defendant. Bonded defendants can more easily seek legal advice while maintaining employment, education and family obligations. In short, then, custodial data may inaccurately suggest that police and prosecutors work according to an assembly line model due to the uniquely severe consequences of pretrial detention.

2.2.3 Models

After being transferred from police lockup to the jail, approximately half of all sampled defendants in 2016 were released after their first judicial bond hearing. Of course, these bonded defendants will still have their charges dropped or be convicted and punished, but this outcome and its timing is no longer observed. I address this problem in several ways. I begin my analyses by exploring the average adjusted probability of three possible release outcomes— bond, drop, and conviction— by charge type and race using multinomial logit models. But, one must consider not only the release event, but also the timing of this outcome. How long did defendants wait in pretrial custody until their charges were dropped, or they were convicted? These issues are resolved using two complementary competing risk methods.

To this end, I treat judicial bond release events as right censored (Pintilie, 2006). I take bond censoring to be informative, and weight all competing risk models by the defendant's inverse probability of being released on bond at given detention times. This is necessary because the censoring mechanism is likely not independent of the focal event, since detained defendants face higher incentives to plea bargain (Putter, Fiocco, and Geskus, 2007). These weights are estimated using a Cox proportional hazards model. For univariate analyses, I report the results of the modified Wilcoxon test to emphasize the earlier part of the hazard curve. This choice is made because less than ten percent of release events in my sample occur after the first month of detention (Pintilie, 2006). All reported results are confirmed using a logrank test.

The first competing risk approach extends the standard multivariate Cox regression to compare the cause-specific hazards of each racial group by treating competing risks as censored observations. Put simply, this method compares the hazard of release events within each racial group, in an "ideal" situation where releases from other events did not occur (Pintilie, 2006, 155). Following Putter et al. (2007), I stratify these models to relax the assumption of proportional baseline hazards of release events across racial groups. This

strategy also make it possible to test whether different racial groups experience equal rates of dropped charges or post-conviction release. In this model, independence is assumed between dropped charges and post-conviction release. I model the cause-specific hazard of release event r for defendant i with covariate vector x as:

$$\lambda_r(t|x_i) = \lambda_{r,0}(t)exp(\beta_r^\top x_i). \tag{2.1}$$

where $\lambda_{r,0}(t)$ is the baseline cause-specific hazard of release event r. Vector β_r represents the covariate associations with regard to release event r. Each covariate vector includes Cox model estimates of the inverse-probability of bond release as a censoring weight.

However, there is likely to be dependance between the timing of dropped charges and post-conviction release. Again, this is because delayed dropped charges will likely encourage defendants to plead guilty to secure release from detention. In contrast to the 'ideal' cause-specific hazards, then, the second competing risk approach offers a more "empirical" look at differences across race and charge type that accounts for likely dependance between competing release events (Pintilie, 2006, 157). The final approach directly models the cumulative incidence function as proposed by Scheike et al. (2008), which allows simulation-based goodness-of-fit testing for non-proportionality as well as the integration of multiple time-varying covariates. Select covariates thus may have time-varying effects $\alpha(t)$ while others may be described parametrically by γ in the model

$$h\{P_1(t;x,z)\} = x^\top \alpha(t) + g(z,\gamma,t) \tag{2.2}$$

where h and g are known link functions. In the analyses, I fit a Fine-Gray proportional model which includes both nonparametric terms for covariates x found to have significant time-varying $\alpha(t)$ and parametric, constant covariates z. Also included are the inverse probability of censoring weights for bond release. Scheike and Zhang (2011, 7) define this formally as:

$$P_1(t; x, z) = 1 - exp\{-exp(x^\top \alpha(t) + z^\top \gamma)\} \tag{2.3}$$

Typically, relevant covariates for cause-specific hazards will also significantly alter the cumulative incidence function, and vice versa, but this need not be the case (Scheike et al., 2008). Thus, I report the both cause-specific hazards and flexible cumulative incidence results (Varadhan et al., 2010; Latouche et al., 2013).

2.3 Results

Over two thirds of the custodial sample is composed of broken windows defendants. Black defendants make up 70% of the sample. On average, defendants charged with shoplifting generally tend to be detained for longer than those charged with disrupting public order. Bond is the most common release event, followed by post-conviction release and dropped charges. Figure 2.3 illustrates the average adjusted probabilities of three release events by charge type and the racial or ethnic group of the defendant.

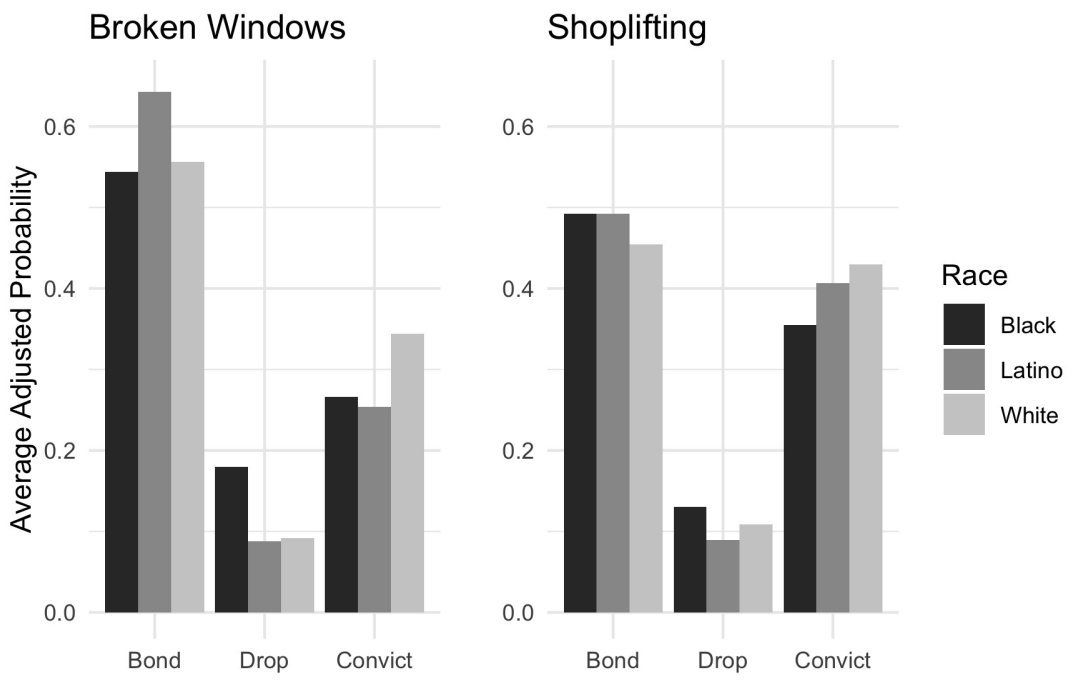


Figure 2.3: Average adjusted probability of competing release event

Source: 2016 Cook County Dept. of Corrections custodial release records.

Bond is predicted to be the most commonly experienced release event for both broken windows and shoplifting charges. This confirms the need to weight subsequent competing risk models by the defendant's inverse probability of being censored (i.e. released on bond) at given detention times. That said, Figure 2.3 offers preliminary evidence against the assembly line hypothesis, which expects prosecutors to win comparatively more convictions in broken windows cases. Instead, convictions are predicted to be more common for comparison shoplifting charges across all racial and ethnic groups. Further, within broken windows charges, white defendants are predicted to have the highest average adjusted probability of conviction: nearly 10% higher than their Black and Latino counterparts. Again, this result runs counter to the assembly line hypothesis that expects convictions to accrue to Black and Latino defendants more often than their white counterparts.

Instead, Figure 2.3 suggests that Black broken windows defendants may be uniquely managed by broken windows prosecution. This group is predicted to have the highest average adjusted probability of being released after charges are dropped by the prosecutor.

Specifically, Black public order defendants have an 18% probability of dropped release. This result is roughly nine percentage points higher than both Latinos and white defendants. This is also five points higher than the predicted probability of drop release for comparable Black shoplifting defendants. This suggests that Black broken windows defendants experience the outcomes expected in both managerial hypotheses: first, prosecutors are more likely to drop broken windows versus shoplifting charges, and second, prosecutors are more likely to drop charges against Black defendants as opposed to their white and Latino broken windows counterparts.

These preliminary results also suggest Black broken windows defendants may be more likely to have their charges dropped after waiting longer than their white and Latino counterparts. Thus, to fully adjudicate between the assembly line and management theories of misdemeanor justice, one must consider not only the release event, but also the *timing* of this outcome. How long did defendants wait in pretrial custody until their charges were dropped, or they were convicted? Questions of timing require two complementary competing risk methods: first, cause-specific hazards, and second, flexible cumulative incidence models. Both of these approaches take bond release as a censored observation, rather than as an outcome in its own right as above.

2.3.1 Cause-Specific Hazards

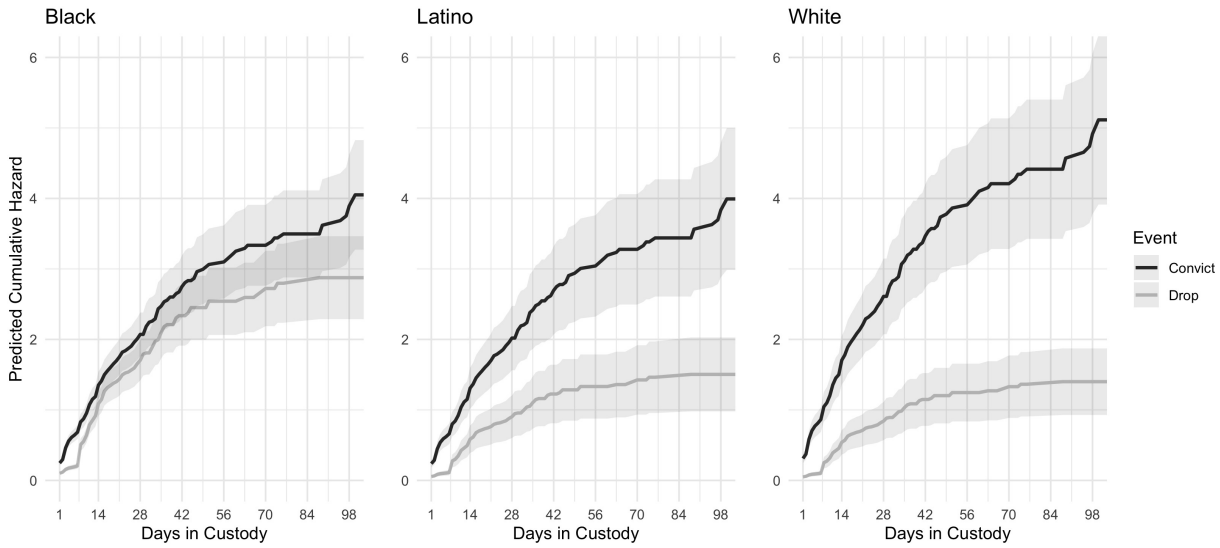
As expected by both theoretical models, there is a statistically significant difference in the hazards of both dropped and post-conviction release depending on whether the defendant is facing a shoplifting or broken windows charge ($p < 0.001$). Within broken windows charges, however, I find no statistically significant difference in the hazard rate of post-conviction release by racial or ethnic group ($p = 0.4$). Instead, each racial and ethnic group has a significantly different hazard rate of dropped release at $p < 0.001$. This may support the managerial hypotheses that prosecutors delay dropping broken windows charges more often against non-white defendants. To explore this possibility, I turn to multivariate cause-specific

models stratified by defendant racial or ethnic groups.

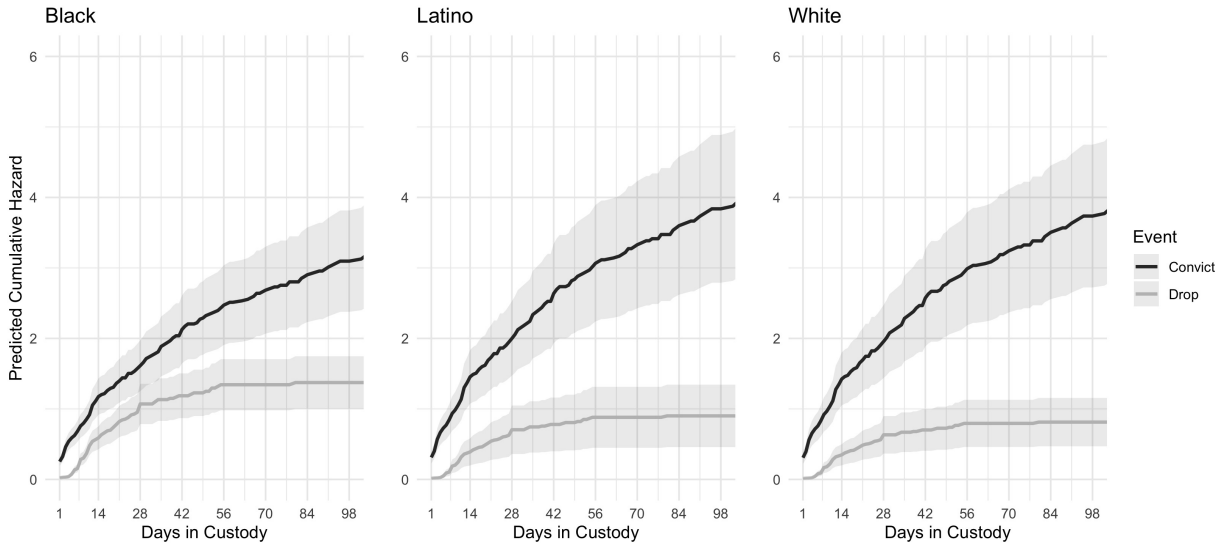
Again, for broken windows charges, defendant race/ethnicity is consistently associated with a significant difference in the hazard of experiencing dropped versus conviction release at $p \leq 0.01$. If a broken windows defendant is Black, then he has a significantly increased instantaneous risk of being released from county jail via dropped charges (h.r. 2.0; $p < 0.001$). In comparison, Black shoplifting defendants also have a significantly different instantaneous risk of drop release, but at a smaller magnitude (h.r. 1.5; $p = 0.02$). This evidence supports a managerial hypotheses for Black defendants: holding other covariates constant, those charged with broken windows are more likely to be released from jail because the prosecutor dropped their charges as compared to Black shoplifting defendants.

That said, prosecutors seem more likely to drop charges against both types of misdemeanor defendants if they are Black. In contrast, if a broken windows defendant is white or Latino, then there is a statistically significant decrease in their risk of instantaneous dropped release (Latino h.r. 0.6; $p = 0.001$; white h.r. 0.5; $p < 0.001$). Instead, broken windows defendants experience a significantly higher instantaneous hazard of post-conviction release—if they are white (h.r. 1.3; $p < 0.01$). This increase in post-conviction release hazard is not observed among white shoplifting defendants (h.r. 0.6; $p = 0.02$). Thus, it seems that, all else equal, white defendants tend to have a higher instantaneous risk of convicted release compare to their shoplifting counterparts. This result tends to support assembly line hypothesis concerning white defendants. Latino defendants, in contrast, do not have a significantly different hazard of being released after conviction for either broken windows ($p = 0.2$) or retail theft charges ($p = 0.2$).

Together, these results offer evidence for a modified managerial hypothesis: Black broken windows defendants are more likely to have their charges dropped after waiting in jail longer than their white and Latino counterparts. This is illustrated in Figure 2.4 using a cumulative hazard transformation, which more intuitively predicts the non-instantaneous phenomenon of waiting in pretrial detention.



(a) Broken windows



(b) Shoplifting

Figure 2.4: Predicted event-specific release hazards

Source: 2016 Cook County Dept. of Corrections custodial release records.

Black broken windows defendants are represented in the top left-hand graph of Figure 2.4; their predicted cumulative hazard function is noticeably distinct from all others. In particular, it displays a uniquely steep rise in the cumulative risk of dropped release at all times from one to approximately eleven weeks in custody. For example, take a hypothetical broken windows defendant who has been detained for about a month, or 30 days, in jail.

If this defendant is white or Latino, he has a higher cumulative risk of being released after having been convicted on his charges. But, if the defendant is Black, then his total risk of being released after being convicted (cum. haz 1.8; s.e. 0.2; c.i. 1.5 - 2.1) is statistically *equivalent* to having his charges dropped (cum. haz. 2.2; s.e. 0.2; c.i. 1.9 - 2.5).

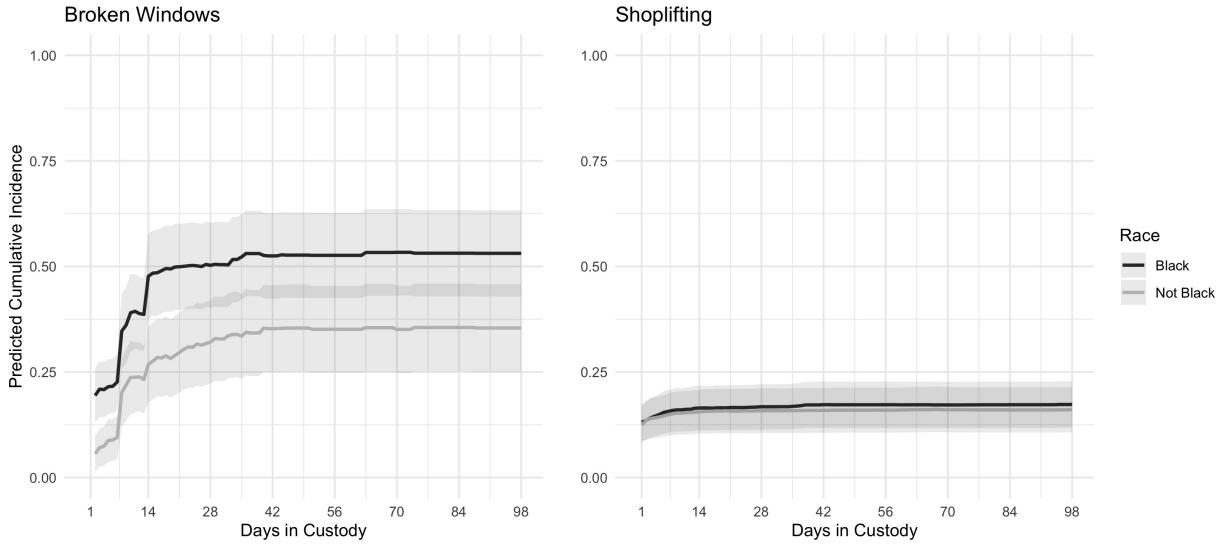
In sum, there is evidence that Black broken windows defendants do experience a uniquely managerial form of misdemeanor prosecution. They are more likely than other racial groups to wait in jail until the prosecutor decides to drop charges. To this point, I have used common competing risk methods that assume independence between release events. In my application, though, this is likely a dubious analytical foundation, since pretrial detention likely encourages plea bargaining. In the next section, then, I adopt a complementary analytical approach to competing risks that can simultaneously estimate the separate probability of both drop and post-conviction release events (Scheike and Zhang, 2011). Based on the above results, I also narrow my focus to comparing Black defendants, on one hand, and white and Latino defendants, on the other.

2.3.2 Flexible Cumulative Incidence Models

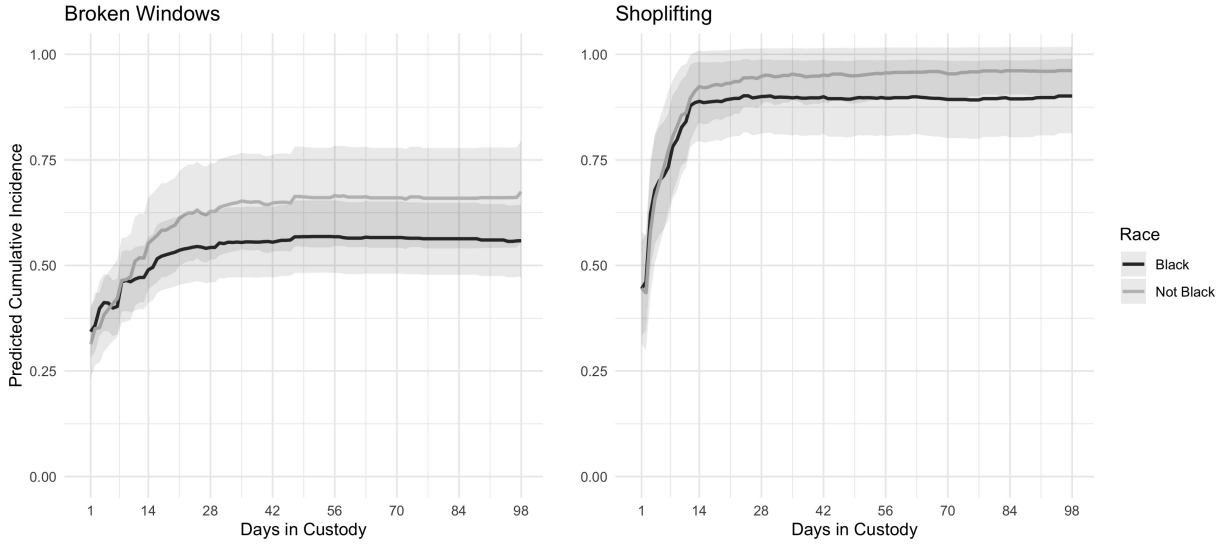
Using the univariate test designed by Gray (1988), there is evidence of differences in the cumulative incidence functions of broken windows and shoplifting charges for each competing release event at $p < 0.001$. Black defendants have significantly different cumulative incidence estimates from other racial and ethnic groups for both dropped and convicted broken windows ($p \leq 0.006$) and shoplifting charges ($p \leq 0.04$).

Figure 2.5 visualizes the predicted cumulative incidence functions for both competing events. Broadly, the right-hand column depicts retail theft charges being prosecuted as if on an assembly line. If released after two weeks in jail, the shoplifting defendant has an approximately 92% predicted probability of being convicted. There is also no difference in the predicted cumulative incidence functions for shoplifting by race. However, this pattern appears unique to shoplifting, an offense that requires particular harm to an identifiable

victim. When it comes to those defendants depicted on the left-hand side of Figure 2.5—those charged with being 'disreputable' or 'disorderly'— prosecutors seem to adopt a more managerial approach.



(a) Dropped



(b) Convicted

Figure 2.5: Predicted cumulative incidence of competing release events

Source: 2016 Cook County Dept. of Corrections custodial release records.

At all time intervals, broken windows defendants have a higher predicted probability of experiencing dropped release when compared to their shoplifting counterparts. For shoplift-

ing charges, the predicted probability of drop release stabilizes at approximately 16% after one week in detention. These charges also demonstrate no difference in the predicted cumulative incidence functions for dropped release events by race. In contrast, broken windows defendants are predicted to experience considerable jumps in the probability of dropped release after spending one and two weeks in detention. And, this probability is predicted to be significantly higher for those broken windows defendants who are Black.

Within broken windows charges, the predicted probability of drop release rises approximately ten percentage points for all defendants after one week in jail. For Black defendants, then, the predicted drop probability after one week in custody jumps to 35% (c.i. 26% - 44%); for white and Latino defendants, it reaches 20% (c.i. 12% - 28%). After two weeks in jail, Black defendants experience another spike in the probability of drop release. Of those released at 14 days, nearly one out of every two Black detainees is predicted to have had their broken windows charges dropped by the prosecutor (48%, c. i. 38% - 58%). This two-week increase is not mirrored by other broken windows defendants. Instead, during the same interval, only approximately one out of every four non-Black defendants is predicted to have had their charges dropped (27%, c.i. 18% - 36%). Over time, the predicted probability of drop release stabilizes at approximately 35% (c.i. 25% - 45%) for white and Latino detainees, and 53% (c.i. 43% - 63%) for their Black counterparts.

Results from these flexible cumulative incidence models complement the findings from cause-specific models above. Overall, there is clear evidence that prosecutors treat public order or broken windows charges differently than traditional misdemeanor charges like retail theft. Specifically, prosecutors handle broken windows arrests as expected by the managerial theory: broken windows defendants are more likely to have their charges dropped after waiting longer than comparable shoplifting defendants. Also, as hypothesized by the managerial approach, prosecutors are more likely to drop broken windows charges against Black defendants who are also predicted to spend longer in pretrial detention than Latino and white detainees.

2.4 Discussion

While the assembly line analogy may describe traditional misdemeanor charges like shoplifting, it is not an empirically useful model for the prosecution of broken windows misdemeanors in Cook County. In this jurisdiction, there is evidence that prosecutors tend to manage 'disorderly' or 'disreputable' defendants using informal tools like pretrial detention. When compared to their shoplifting counterparts, broken windows defendants have a consistently higher risk of experiencing release from jail after their charges have been dropped. These managerial techniques are also disproportionately used against Black defendants. This racial group tends to have their victimless charges dropped more often than comparable white and Latino defendants, and they must wait longer for this resolution in county jail.

Recall that, in this jurisdiction, police may directly file misdemeanor charges upon arrest without immediate prosecutor review. Judicial review is similarly truncated, with no standalone preliminary examination for misdemeanors (Code of Criminal Procedure, 1963, 109-3.1a).[12] By eliminating these review procedures, police become the *de facto* gatekeepers of the misdemeanor courts— courts which wield powerful tools of state control, including pretrial detention. Using this considerable charging authority, police may legitimately curtail the liberty of defendants for a variety of purposes.

On one hand, police may arrest and charge defendants with an eye to an eventual conviction. This motive may be particularly strong when attempting to redress a tangible harm to an identifiable victim. Retail stores, for example, may pressure both police and prosecutors to successfully convict shoplifting defendants in order to gain restitution or deter future theft. On the other hand, though, police may use their charging authority to immediately control and manage those they deem risky— often, Black populations— provisionally removing them from the streets to county jail. This alternative motive follows the logic of broken windows policing: 'disorderly' people should be preemptively surveilled and incapacitated

12. During a preliminary examination, a judge determines if there is probable cause to charge the defendant with a criminal offense.

before they commit or encourage more serious crime. Pretrial detention is an all-purpose management tool, suitable for use in most broken windows policing strategies. For example, this custodial delay may be justified as a therapeutic week-long jail detox for chronic addicts met on patrol, or remove a nuisance individual as reported by third parties. At worst, a week in jail is an effective, state-sanctioned way the police may harass and intimidate some of our society's most vulnerable members.

Clustered release times provide clues about when and how police and prosecutors interact, as well as the organizational incentives guiding their decisions. Recall that there is a marked spike in the predicted risk of dropped release for broken windows defendants after they have spent one or two weeks in custody (see Figure 2.5). Shoplifting charges also have a dramatic jump at the two week interval, but only for post-conviction release. I take these spikes to represent the aggregate prosecutorial response to police charging decisions once they enter the misdemeanor courts. Here, it is important to note the importance of the initial court appearance in Cook County. With no formal prosecutorial oversight, preliminary hearing or arraignment, the initial court appearance is usually the first time a prosecutor has reviewed pending charges. If there is no immediate guilty plea, the first appearance must occur no more than 14 days after the charges are filed (Rules of the Court Sec. 15, 2014). In my experience, for misdemeanors, the first court date is often set earlier: one week after arrest.

For the prosecutor, facing stacks of files, this initial review is by necessity "rapid and often perfunctory" (Feeley, 1979, 61). The prosecutor generally takes less than five minutes per case (see also Kohler-Hausmann 2018, 125). This rapid pace does not imply, however, that prosecutors work passively, performing the same task on each case as if on an assembly line (c.f. Packer 1968; Natapoff 2018). Instead, they actively interpret and respond to the perceived intent of the police. To do this quickly, they must rely on a variety of decision-making heuristics, including charge type and defendant race. Often, though, prosecutors do not have much of a case to work with.

For example, by the time broken windows defendants have their first court date, a week

or more will have passed from the time of arrest. If conviction was not the goal of arrest, then there remains little incentive for complaining police officers to testify in court.[13] Police will have accomplished their immediate management goals. From the prosecutor's perspective, there is no reason to spend scarce time seeking out wayward witnesses. It makes sense, then, that nine out of every ten dropped broken windows releases can be attributed to "strik[ing] a case on leave to reinstate," most often used when a witness fails to testify in court. And, it is not surprising that these so-called SOL drops are more likely to occur at the defendant's first court appearance after one or two weeks in jail. The same logic explains the dramatic spike in retail theft convictions during this two-week interval; after all, private security personnel and closed-circuit camera footage offer eager, convenient and reliable sources of evidence concerning a particular harm against an identifiable victim.

Here, it is worth considering the potential meaning of the SOL drop for misdemeanor prosecutors. The SOL allows the prosecutor to share the responsibility for non-conviction with the court, since the judge must approve the prosecutor's motion to strike the charges from the docket.[14] In theory, the prosecutor may motion to reinstate the charges if the police officer eventually decides to testify in court. In practice, though, reinstatement is virtually unheard of. The meaning of a SOL is thus symbolic, not practical: it acknowledges and validates the arresting police officer's strategic use of courtroom delay, and maintains the professional working relationship between police and prosecutors. More broadly, it reaffirms their shared commitment to a broken windows ethos of risk mitigation and crime management. It legitimates not any particular charging decisions, but rather the professional authority of proactive, broken windows law enforcement as such (Falk Moore, 1978).

To be clear, though, I do not claim to evidence any particular motives held by Cook County prosecutors, whether nefarious or altruistic. I remain agnostic on whether prosecu-

13. Here, I foreground responding police officers, as they are the most common witnesses of broken windows type misdemeanors. Of course, this logic may equally apply to other third-party complainants who use misdemeanor courts as tools of population management, as documented in Herring (2019).

14. This is in contrast to a *nolle prosequi* disposition, which places the responsibility for dropping charges entirely on the prosecutor.

tors purposively delay to keep defendants in pretrial detention, or if they seek to quickly drop inappropriate charges made by the police. I simply argue that prosecutors make active, managerial decisions when deciding how to handle broken windows charges brought to them by the police. After all, regardless of prosecutorial motive, these decisions hold serious consequences for Black broken windows defendants. Even short-term stays in county jail threaten virtually every aspect of a defendant's long-term well-being. Whether or not the defendant is found guilty, their physical and mental health is threatened by cramped, unhygienic custodial conditions. The lack of outside communication cuts crucial ties with family, friends, employers, landlords and legal counsel. The experience deeply damages the perceived legitimacy of key social institutions, from elections to health care and education.

Each year, these harms threaten millions caught in the U.S. misdemeanor justice system. Harms are not allocated according to legal guilt or innocence; rather, they are unevenly distributed by defendant race. In this chapter, I have argued that racial disparities in misdemeanor pretrial detention stem from interactions between two crucial street-level bureaucrats: the police and the prosecutor. In this vein, I urge all sociologists, criminologists and legal scholars to directly engage this crucial nexus of discretionary power. Beyond misdemeanors, the police-prosecutor relationship bears on every defining criminal justice topic of our time, from mass incarceration to police misconduct. It is time for scholars of the criminal legal system to seriously consider how prosecutors and police interact, not just with the neighborhoods they are supposed to protect and serve, but with each other.

By way of conclusion, I propose one concrete policy reform: police should not directly charge misdemeanors without review. This practice allows police to use the coercive tools of the court to control, manage, and perhaps even harass Black populations. In this chapter, I focused on pretrial detention as a particularly coercive form of "procedural hassle" (Kohler-Hausmann, 2018). But, defendants may be subject to a number of other hassles, indignities and harms given unchecked police charging authority. Indeed, for defendants released on bond, custodial data likely overestimates the rate of misdemeanor convictions and underes-

timates the time it take prosecutors to dropped charges. Bonded defendants must devote considerable time to making court appearances. Defendants may also perform compliance in exchange for charge dismissal, thus avoiding a criminal record (Kohler-Hausmann, 2018). Performances may range from community service to submitting a DNA sample to a forensic database (Roth, 2019).

In response, some may argue that direct charging is necessary to cope with crushing misdemeanor caseloads. This is precisely why direct charging is common practice throughout the United States (Horwitz, 1998, 1306). If more procedural safe-guards are imposed, it is argued, the process becomes even more slow and cumbersome for defendants and law enforcement alike. This criticism has been met with two responses. First, some legal scholars advocate targeted organizational reform in prosecutor's offices (Wright and Miller, 2002; Gershowitz and Killinger, 2011). Traditionally, resource allocation privileges the later stages of complex, felony-level prosecution in large urban jurisdictions like Cook County (Richman, 2003). Resources may be more efficiently allocated to earlier stages of the misdemeanor process. This shift may both weed out weak cases before defendants are unduly hassled, and also to actively dissuade police from strategically using tools of control ostensibly reserved for the prosecution and judge.

The second response goes beyond narrow organizational reform to advocate more sweeping ideological and organizational change. Specifically, critics take aim at the enduring adjudicative ideal in criminal justice (Lynch, 1998; Kagan, 2019). It is argued that, while this "governing ideology" imagines outcomes based in evidence and law, and protected by due process rights, it ignores the real, unchecked discretionary authority of police and prosecutors (Lynch, 1998, 2124). This authority is especially profound in charging the multitude of low-level, public order criminal offenses that impact millions each year. In Cook County, this decision is made by police, with no formal legal training, little to no professional or ethical oversight, and no obligations to adversarial safe-guards like judges, juries and defense counsel.

We should recognize that low-level, victimless infractions are not in fact processed as criminal offenses, but through administrative, or managerial, interventions. But, we must not take this to be a bad thing. Instead, we should decriminalize broken windows offenses, as the Illinois legislature itself did in 2016 regarding marijuana possession. This would remove the ability of law enforcement to jail the populations they deem disorderly, disreputable or unpredictable. It would allow for the *de jure* regulation of the *de facto* administrative decisions of law enforcement (Roth, 2019). Transaction costs and procedural hassles could be be recognized, accounted for and limited; above all, they can be forbidden as informal tools of racial control.

CHAPTER 3

THE IMPACT OF CHARGING REFORM ON CRIME AND PUNISHMENT

State prosecutors are increasingly tasked with fixing an urgent problem partly of their own making: mass incarceration (Barkow, 2019; Bazelon, 2019; Beckett, 2018; Bellin, 2018; Pfaff, 2017). Can prosecutorial charging reform reduce the unprecedented use of criminal courts, jails and prisons in the United States? Theoretically, this reform holds promise. If prosecutors simply file fewer felony charges, it is argued, then fewer people must wait in jail pretrial or end up in prison after conviction (Pfaff, 2017). Prosecutors, however, are also tasked with enforcing the criminal law in defense of public safety and social order (American Bar Association, 2015, 3-1.2). Thus, a second empirical question emerges: does prosecutorial charging reform impact crime rates, reporting or arrests? Though hotly debated, empirical evidence is rarely used to answer either question because sufficiently detailed charging data are vanishingly rare (Allen et al., 2016; Beckett, 2018; Davis, 2007; Pfaff, 2017). In this chapter, I address both questions empirically using a regression discontinuity research design. My goal is to offer a methodologically robust accounting of felony charging reform. Broadly, this work contributes to ongoing debates about how criminal law enforcement should be restructured in order to end mass incarceration in the United States.

Throughout, I focus on a dramatic policy shift in Cook County, Illinois that occurred on December 12, 2016. On this day, newly-elected State's Attorney Kim Foxx raised the threshold values defining felony retail theft from $300 to $1,000. By January 2017, this intervention more than halved the number of retail theft charges filed countywide, removing approximately 120 retail theft defendants from the felony criminal process. Crucially, though, this policy did not apply to felony thefts that are alleged to have occurred *outside* of a retail setting. I am thus able to use non-retail theft as a falsification test in order to isolate the causal impact of the specific intervention from broader historical trends over time

(Morgan and Winship, 2015). For a similar purpose, I also compare the reported incidents of retail theft pre- and post-intervention to non-theft crimes in retail locations. This gauges if the lenient charging policy impacted retail crime reporting rates. Using Chicago Police Department incident and arrest data, I find no discontinuity in crime reporting or police responsiveness (Levy, 2020b,a). That said, I do find evidence of a slight increase in the slope of reported retail theft incidents post-intervention.

A modest month-on-month increase in retail theft incidents may be acceptable to the reformist prosecutor, so long as there is also a simultaneous reduction in the myriad harms of criminal legal contact and mass incarceration. To this end, I next evaluate to what extent the charging intervention in fact reduced the use of courts, jails and prisons in Cook County. Fewer felony charges do not necessarily translate to decarcerative gains. For example, it is plausible that all of these defendants were nonetheless arrested and detained, not on felony charges, but on misdemeanors. While misdemeanors are less severe by statute, in practice they also exert serious social, psychological and economic harms (Feeley, 1979; Kohler-Hausmann, 2018; Natapoff, 2018). Or, perhaps the marginal gain of fewer total felony defendants is overwhelmed by more punitive and longer sentences for those that remain (Seeds, 2017). I test these hypotheses using data from the Cook County Sheriff's Office on pretrial incarceration rates as well as adjudication and sentencing outcomes from the Cook County State's Attorney's Office.

I do find evidence that misdemeanor arrests increased in rough proportion with the decrease in felony-level prosecutions. However, the number of misdemeanor jail bookings remained constant, since, unlike felonies, police may directly release misdemeanor defendants on bond. Similarly, even when they are faced with dramatically fewer felony cases, I find that prosecutors (but not judges) maintain pre-intervention rates of dropped charges. Finally, in terms of punishment, there is no evidence that felony retail thefts charged at the higher threshold result in more punitive adjudication or sentencing outcomes. Instead, the length of probationary terms has decreased after the intervention. Taken together, these results do

confirm that prosecutorial charging reform is a promising decarcerative strategy. I conclude by highlighting the symbolic and distributional value of new prosecutorial charging policies that include defendants' interests when defining and defending public order.

3.1 The Prosecutor's Shifting Role in Mass Incarceration

Prosecutorial elections have become a key battleground in the recent, seismic shift towards decarceration: since 2016, fifty one 'smart on crime' candidates have won office in large urban jurisdictions around the country, including Chicago, Philadelphia, Boston, Dallas and St. Louis (Balboni and Grometstein, 2020; Bazelon, 2019; Bellin, 2020; Davis, 2019; Sklansky, 2017). Newly-elected reformist prosecutors aim to tighten the "spigot" of overcharging in order to end mass incarceration (Flemming et al., 1992, 23). For example, Philadelphia District Attorney Larry Krasner has promised that "the era of trying to get away with the highest charge regardless of the facts is over" (Davis, 2019, 12). Theoretically, ending overcharging may offer a promising way out of mass incarceration. If prosecutors file fewer felony charges, then fewer people must wait in jail pretrial or end up in prison after conviction (Pfaff, 2017). Similarly, if prosecutors file more lenient charges, then fewer people will face years-long prison sentences. In turn, harsh sentencing and criminal legislation can be circumvented in practice without the lengthy and contentious process of decriminalization (Beckett, 2018). Theoretically, then, felony charging reform is one of the most promising leverage points to tackle the complex problem of mass incarceration (ibid).

3.1.1 Research Questions

Empirically, however, the claim that fewer felony charges will achieve decarcerative gains remains largely untested (Richardson and Kutateladze, 2021). This is because sufficiently detailed prosecutorial charging data are vanishingly rare (Allen et al., 2016; Pfaff, 2017).[1]

1. Although state-level data have been collected by the National Center for State Courts (NCSC) and by the Bureau of Justice Statistics's State Court Processing Statistics (SCPS), both datasets have substantial

This chapter combines a decade's worth of county-level administrative data— including felony courtroom events, criminal incident and arrest data, custodial booking records, and sentencing outcomes— to quantify the benefits and drawbacks of prosecutorial charging reforms aimed at ending mass incarceration. I approach this task in three parts, roughly corresponding to the beginning, intermediate, and final stages of the criminal adjudication process. First, I consider the impact of charging reform on initial crime reporting and arrests. Next, I look at intermediate pretrial detention and the decision to drop charges. Finally, I consider if charging reform hits its intended final target by decreasing the severity of punishment.

As prosecutors adjust criminal charging practices, they may also impact crime, reporting and arrest rates. For example, if prosecutors announce their intent to pursue more lenient criminal charges, they may also reduce the expected risk individuals associate with a given illegal activity. Thus, charging reform may inadvertently increase crime by weakening the deterrent effect of punishment. Following a similar logic, victims or witnesses may be reluctant to report criminal activity if they do not expect a sufficiently punitive outcome. Finally, police officers themselves may be reluctant to make arrests, collect evidence and conduct thorough investigations if they do not expect their efforts to result in a felony conviction. Together, these hypotheses are powerful and politically charged criticisms of any elected prosecutor's decarcerative aims (Barkow, 2019). They motivate my first research questions: Does prosecutorial charging reform increase crime? Does it decrease reporting or arrest rates?

A second problem emerges if prosecutorial charging reform is narrowly focused on reducing the lengthy formal sanctions and institutionalized stigma attached to a felony conviction (Pager, 2007; Manza and Uggen, 2006; Wakefield and Wildeman, 2013; Western, 2002). A

limitations. NCSC data rely on voluntary reporting and may suffer from validity problems (Bellin, 2018). SCPS uses superior sampling methods, but has problems measuring offense severity and criminal history across jurisdictions (Ulmer, 2012). Given the vital importance of these data, the Bureau of Justice Statistics has highlighted the need to improve the "record-keeping by State and local governments" necessary "to produce statistics on... the administration of justice" (Bureau of Justice Statistics, 2005, 10).

simple solution is to reduce felonies by simply shifting cases into an already-massive misdemeanor legal system (Mayson and Stevenson, 2020; Natapoff, 2018; Stevenson and Mayson, 2017). Even without a felony conviction, however, this outcome would maintain harmful criminal legal contact including arrest, pretrial detention and courtroom debasement rituals (Feeley, 1979; Kohler-Hausmann, 2018; Van Cleve, 2016). These concerns motivate my first intermediate-stage research question: Does felony charging reform simply shift arrests and pretrial detention into the misdemeanor criminal legal system?

Further, for those cases remaining in the felony system, the retail theft intervention may also change prosecutorial decision-making in criminal courtrooms. On one hand, it is plausible that the charging reform may cause prosecutors to also treat the remaining over-$1,000 shoplifting cases more severely. After all, court dockets did experience a sudden drop in caseload for one of the most common felony charges in the jurisdiction. Prosecutors may now have more time to win convictions for those remaining cases that are alleged to meet the higher threshold. To test this hypothesis, I evaluate a second research question at the intermediate stage of the criminal process. Before and after the charging reform, is there any discontinuity in the likelihood that a shoplifting defendant's charges will be dropped at the preliminary hearing?

Finally, it is possible that charging reform may not in fact have its intended decarcerative effect. It could even cause an increase in incarcerated populations. To illustrate, consider the following example. Under previous charging practices, the prosecutor may successfully convict five felony defendants. If each receives a one-year prison sentence, then this contributes five years to the total prison population. Post-intervention, though, the prosecutor may only be able to charge three of those same defendants with a felony offense. However, if the prosecutor also decides to pursue an increased two-year prison sentence for each defendant, then the total prison years *increases* from five to six. In short, there is no necessary relationship between reducing the number of felony defendants and decarceration, since those that remain may face more punitive and longer punishments (Seeds, 2017; Tonry, 2016). That said, it

is similarly plausible that even those charged with a felony under the increased threshold will be granted greater leniency under a new decarcerative ethos (Balboni and Grometstein, 2020; Bazelon, 2019). I test these competing hypotheses about the impact of prosecutorial charging reform on punishment with a third set of research questions. Does the intervention impact the likelihood that a convicted defendant will be sentenced to prison or probation? Does it increase or decrease the predicted length of sentence?

Figure 3.1 schematizes the task at hand by orienting the focal intervention and its potential decarcerative effects within the felony criminal process as a whole. I consider multiple discretionary points in order to capture the dynamic, cumulative and processual nature of punishment (Kutateladze et al., 2014; Ulmer, 2012). The black box locates the focal charging decision, while grey boxes denote other stages that may be impacted by the reform. Figure 3.1 is specific to the single jurisdiction that is under study throughout this chapter: Cook County, Illinois.

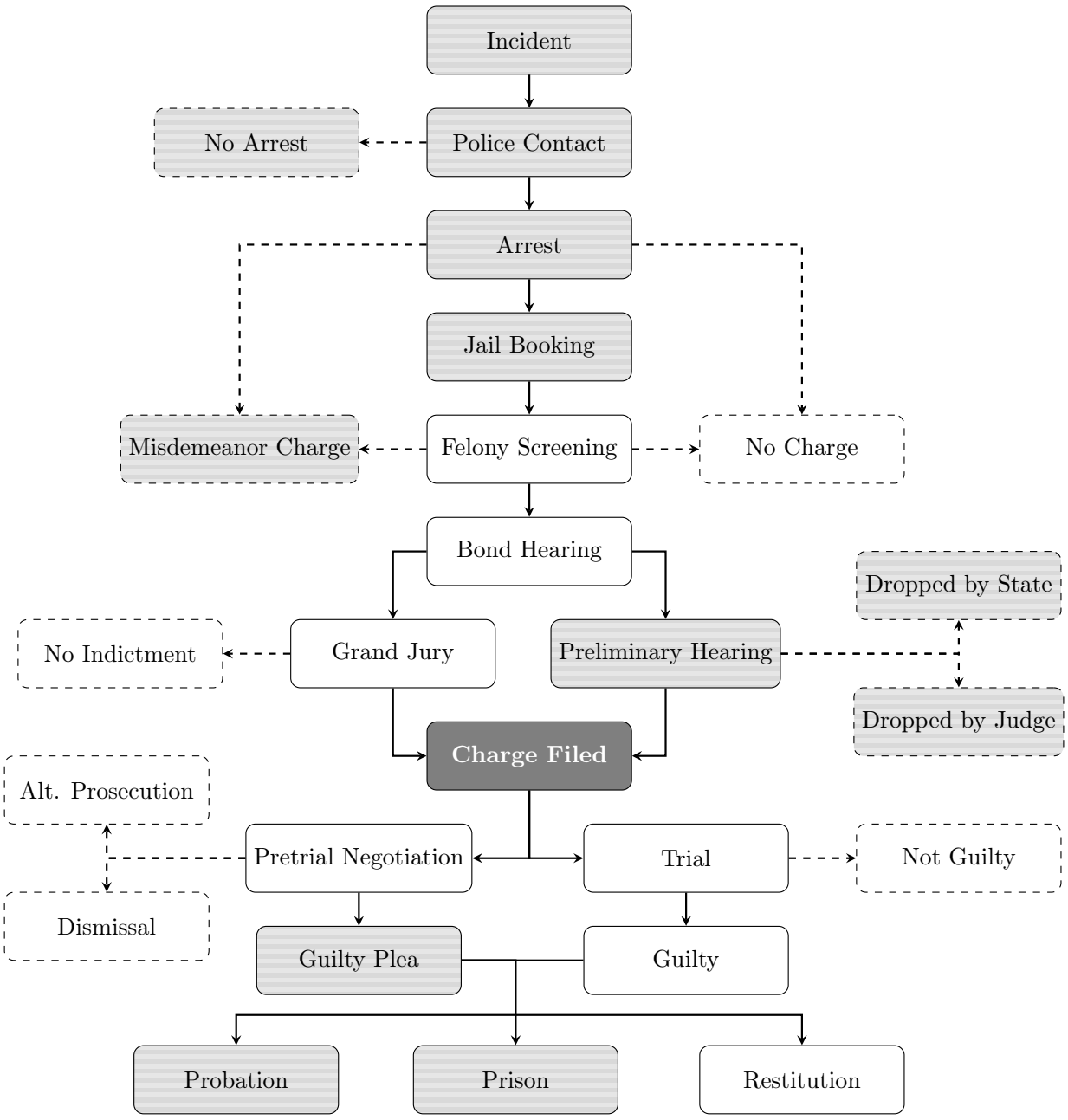


Figure 3.1: Felony case processing in Cook County.

Notes: The intervention occurs during at the formal felony charging stage, highlighted in black. The effect of this change is evaluated at each of the shaded decision-points above. Dotted boxes/lines indicate events that exit the traditional felony process with no conviction.

3.2 Case, Data and Methods

On December 12, 2016, after only two weeks in office, Cook County State's Attorney Kim Foxx raised the threshold value defining felony retail theft from $300 to $1,000. Under the new policy, her administration more than halved the number of felony retail theft charges filed in the county by January 2017. This dramatic drop only applied to retail thefts, however, leaving aside comparable non-retail thefts, as well as non-theft crimes that occurred in a retail location. This sudden, targeted intervention, coupled with useful opportunities for falsification tests, make Cook County uniquely well-suited to a regression discontinuity approach (Morgan and Winship, 2015). This design allows the causal effect of charging reform to be measured by comparing the outcomes of felony retail theft charges before and after the December 2016 intervention. Additionally, these estimates can be compared to similar charges that were not included in the policy change to isolate possible confounding factors over time— most importantly, the contemporaneous inauguration of the Foxx administration.

3.2.1 Prosecution in Cook County

With Chicago as its county seat, Cook County has been a consistent historical bellwether of shifts in the politics of urban punishment in the United States (Muhammad, 2011). Until recently, the office has been defined by a 'tough on crime' ethos of entrenched racism that faced little opposition at the ballot box.[2] For example, incumbent State's Attorney Anita Alvarez won her 2012 re-election campaign after running unopposed in the Democratic pri-

2. For decades, Chicago Democratic machine politics controlled Cook County's prosecutorial and judicial elections (Eisenstein and Jacobs, 1977). Mayor Richard J. Daley selected State's Attorney Edward Hanrahan, who orchestrated the assassination of Black Panther Party leader Fred Hampton in 1969 (Taylor, 2019). Daley's eldest son, Richard M. Daley, was State's Attorney throughout the 1980s before himself becoming Chicago mayor from 1989 until 2011. There is evidence to suggest that, as State's Attorney, the younger Daley actively ignored the Chicago Police Department's pervasive use of torture to compel confessions from hundreds of young Black men (ibid.). In 2008, Anita Alvarez was elected as the county's first female and first Latina head prosecutor. State's Attorney Alvarez preferred a cultural rhetoric of 'color-blindness' to the overt racism of the Hanrahan and Daley; nonetheless, this approach produced similar racial disparities as her predecessors (Van Cleve, 2016).

mary. In 2014, Alvarez filed no charges against white police officer Jason Van Dyke, despite a dash-cam video showing him shooting Black teenager Laquan McDonald sixteen times as he lay dying in the street (Davis, 2019). After the video was leaked nearly a year later, Van Dyke was found guilty of second-degree murder in 2018. Alvarez was also accused of under-charging police officer Dante Servin, who was acquitted of involuntary manslaughter in the killing of 22-year-old Rekia Boyd (Goodman, 2015).

The deaths of Boyd and McDonald sparked massive outrage. In 2016, Alvarez was defeated in the Democratic primary by current Cook County State's Attorney Kim Foxx. Foxx was elected on a reformist platform that promised voters increased transparency, conviction review, bond reform, and community-focused alternative prosecution (Davis, 2019). The Foxx administration is widely cited as a "genuine" model of the new 'smart on crime' prosecutor (Barkow, 2021, 8).[3] As the first African-American woman to hold the role of State's Attorney in Cook County, Foxx cites her personal background as formative of her policy stance. Several members of her family have been incarcerated or victims of gun violence; she herself survived multiple childhood sexual assaults (Bogira, 2017). Her policies are thus oriented to alleviating "the great tragedies of our current system... that many communities, particularly communities of color, are simultaneously ravaged by violent crime and plagued by destabilizing over-incarceration of nonviolent offenders" (Cook County State's Attorney, 2021). Here, State's Attorney Foxx captures the balancing act now faced by all reformist prosecutors: she must simultaneously maintain social order while also alleviating the decades of harm caused by mass incarceration. This task is especially urgent in the low-income Black communities where Foxx herself was raised. As she notes, the residents of these communities have been historically been both ignored and vilified by public law enforcement officials.

In contrast, the interests of retail business are usually allocated significant public safety resources. In Cook County, as elsewhere, retail theft is one of the most common criminal

3. In contrast, Barkow (2021) singles out Harris County, Texas prosecutor Kim Ogg as hypocritical for seeking to expand her office by one hundred attorneys (cf. Bazelon 2019).

complaints dealt with by law enforcement. Further, retail interests are often doubly protected since shoplifting is often also handled by an expansive for-profit security apparatus (Rappaport, 2018). Predictably, however, Cook County retailers baulked at the Foxx administration's decision to redistribute these resources by raising the felony retail theft charging threshold (Davis, 2019). Police departments are similarly critical of the new decarcerative reforms, as illustrated in Figure 3.2.

Figure 3.2: Billboard on W. Cermak Rd. and S. Canalport Ave, Chicago.

Photo by author, September 2020.

During Foxx's 2020 re-election campaign, the Chicago Fraternal Order of Police (FOP) rented the negative billboard, above, with a satirical job advertisement for a 'vacant' State's Attorney position.[4] The Chicago FOP also issued a vote of no-confidence in her leadership soon after her election (ibid.). In terms of political dynamics, then, Cook County offers a case study that will be familiar to many reformist prosecutors who have recently been elected

4. The Chicago billboard is not an original concept: the Philadelphia police union erected a near-identical billboard a year previously against District Attorney Larry Krasner (Barkow, 2021).

across the United States. While my specific results may not be immediately generalizable across jurisdictions, I use Cook County to provide rare causal insight into the consequences of these ongoing shifts in local prosecution and charging reform.

3.2.2 Regression Discontinuity Design

This chapter uses a regression discontinuity design to quantify the causal impact of the Foxx administration's new retail theft policy at several key points in the criminal legal process. Again, on December 12th, 2016, the State's Attorney's Office officially raised the threshold value of felony retail theft from $300 to $1,000. Throughout, I use an intervention cut-off point set at January 1st, 2017 to allow for an internal two week transition period.[5] This also allows for a more easily interpretable running variable of calendar months. All outcomes are assigned as 'treated' under this new policy if they occur during or after 2017. Any observed discontinuity in the conditional expectation of the outcome at the cut-off point is taken as evidence of the causal effect of the new charging policy on that outcome.

Importantly, though, December 1st, 2016 was also State's Attorney Foxx's inauguration date. It is crucial, then, to isolate the specific effects of the new charging policy from any confounding factors stemming from this contemporaneous change (Hausman and Rapson, 2018). To do this, two falsification tests are used (Morgan and Winship, 2015). The primary falsification test compares retail theft to felony theft charges below $10,000 that did *not* occur in a retail location. I limit the total value of alleged stolen goods in order to keep the class of felony and severity of punishment roughly equivalent between both types of theft. This group is used in all analysis except those intended to evaluate change in reporting rates; here, I instead compare all non-theft crimes reported to have occurred in a retail location. Common examples include fraud, battery, trespassing, and robbery. The former control assumes that those accused of theft in retail and non-retail locations are closely matched;

5. This choice was confirmed to be accurate by Chief Data Officer Matthew Saniie, personal communication, April 2021.

the latter assumes that those reporting crime in retail locations are similar regardless of crime type.

This comparison is first developed using segmented parametric regression to test for the intervention effect, the structural trend over time, and any post-intervention change in that trend. The type of model used depends upon the outcome variable under study. Linear regression captures the monthly number of charges filed, reported incidents, arrests, and number of jail bookings. Binomial outcomes are whether or not the charge is dropped by the prosecutor and what sort of punishment is assigned (i.e. prison or probation). The length of punishment is modeled as over-dispersed count data using zero-inflated negative binomial models. All general linear models include the appropriate exposure term. Finally, covariates related to bond and disposition are included where appropriate to improve model specification. Polynomial terms are introduced to improve model fit.

The linear version of the segmented parametric regression to be estimated can be expressed as (Lagarde, 2012, 77):

$$Y_t = \beta_0 + \beta_1 * time + \beta_2 * intervention + \beta_3 * postslope + \epsilon_1 \quad (1)$$

Here, Y_t is a continuous outcome variable at time t. β_0 is the baseline level of the outcome at time 0. B_1 is the non-treated or structural trend in the outcome independent of the intervention, while B_2 is the immediate impact of the intervention on the focal outcome. Finally, B_3 captures the treated, or post-intervention change in slope of the focal outcome (ibid.). This modeling strategy assumes that the error terms are not correlated; if this assumption is violated, then the statistical significance of coefficients may be biased. However, theft crimes may exhibit patterns of seasonal autocorrelation (Hausman and Rapson, 2018; Lagarde, 2012; Morgan and Winship, 2015). Seasonal effects must thus be distinguished from the impact of the intervention. I test each segmented model for autocorrelation with the Durbin-Watson statistic. If evidence of autocorrelation is found, I use the Prais-Winston generalized least squares estimator to correct for biased standard errors (Lagarde, 2012).

Finally, if initial parametric models suggest a significant impact of the felony charging reform on the outcome of interest, I then use a non-parametric approach to evaluate the robustness of these findings. Specifically, I use the local linear regression discontinuity techniques developed by Calonico, Catteneo, Titiunik and Farrell (Calonico et al., 2014, 2015, 2018, 2019, 2020). Instead of modeling the trend over all observation times, this approach allows for the data-driven optimal specification of a sample bandwidth of observations localized around the cut-off point (Calonico et al., 2020). I also explore the degree to which the findings remain at bandwidths both above and below the optimal, as well as when using triangular, uniform, and Epanechnikov kernel functions.

3.2.3 Administrative Data Sources

There are four sources of longitudinal data used in this chapter. The primary dataset is provided by the Cook County State's Attorney's Office and includes every felony courtroom event recorded in the jurisdiction from 2011 until 2019. These data have a total of 103 observed months with 69 months pre- and 34 months post-intervention. The total defendant $n = 19,647$ for felony retail theft and $n = 5,140$ for comparison non-retail theft. Theft charges are isolated by selecting those defendants who are only charged with either retail or non-retail theft under $10,000. I am confident that prosecutors have little incentive to misreport their activities since this system is used internally to schedule their courtroom work routines. The primary limit of these data, however, is that they do not capture misdemeanor-level prosecutions unless attached to a felony case.

This shortcoming is overcome by using supplemental incident and arrest data from the Chicago Police Department (CPD), as well as administrative booking records from the Cook County Sheriff's Department of Corrections. The incident dataset includes all crimes reported to the Chicago Police from 2011 until 2019, covering the same 103-month time period as is used in the State's Attorney data (Levy, 2020b). It also includes the location of the incident as well as whether a simultaneous arrest was made. I focus on two incident categories:

first, retail theft ($n = 90,434$), and second, all other crimes reported to have occurred in a retail setting ($n = 84,804$). Here, though, it is important to remember that CPD data do not include incidents reported to other, smaller police departments that cover university, transit or suburban jurisdictions. That said, the CPD handle the majority of incident reports and arrests in Cook County.

All CPD arrests are captured in a second dataset that further specifies whether the defendant was provisionally charged with a felony or misdemeanor-level offense (Levy, 2020a). Unlike the incident data, which include no demographic information, CPD arrest data also detail the race/ethnicity of the arrestee. These data have a retail theft arrest $n = 20,670$ and a comparison theft arrest $n = 7,859$. Unfortunately, though, these data offer a narrower view over time, ranging from 2014 until 2017. This time-frame is shared by the final data source used in this chapter: pretrial booking data from the Cook County Sheriff's Department of Corrections. Both have a total 48 of observed months with 36 months pre- and 12 months post-intervention. Unlike CPD data, though, the Sheriff's booking data do include all felony-level arrests made by all county-wide police departments, since they enter the sheriff's custody at the bond hearing stage.

Table 3.1 summarizes and compares the four datasets used in the analyses. Here, I including recognizance ('I-Bond') and money bond release types, bail amounts, case length, and diversion participation because these are used as covariates in model specifications.

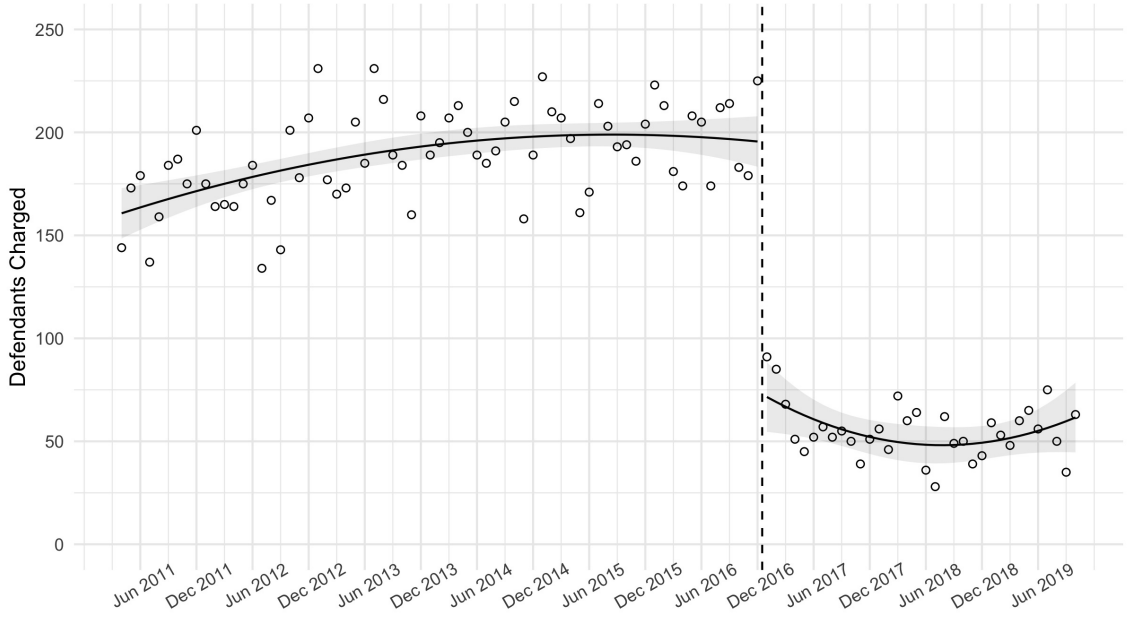
		Retail Theft		Comparison	
Data Source		*Pre*	*Post*	*Pre*	*Post*
Prosecutor	Months n	69	34	69	34
	Defendant n	17,203	2,444	3,925	1,215
	Mean Age	39	41	33	34
	% Male	68	75	79	74
	% Black	64	73	61	57
	Mean Charges	1.1	1.2	1.2	1.2
	% Money Bond	56	29	55	22
	Mean Bail	33,478	31,780	33,750	23,597
	% CPD Arrest	55	55	47	48
	% Diversion	19	18	15	24
	Mean Case Length (days)	104.0	102.2	188.1	174.1
Police, incident[1]	Months n	69	34	69	34
	Incident n	57,355	33,079	53,612	31,192
	% End in arrest	53	41	33	26
Police, arrest	Months n	36	12	36	12
	Arrest n	15,546	5,124	6,493	1,366
	% Felony	27	8	11	11
	% Black	67	71	70	69
Sheriff	Months n	36	12	36	12
	Booking n	11,046	2,886	3,420	984
	% Felony	33	29	46	55
	% Male	69	81	74	79
	% Black	64	67	69	63
	% Release on Bail	24	35	11	21
	% Release I-Bond	47	49	33	45

Table 3.1: Data summary by theft type and select covariates.

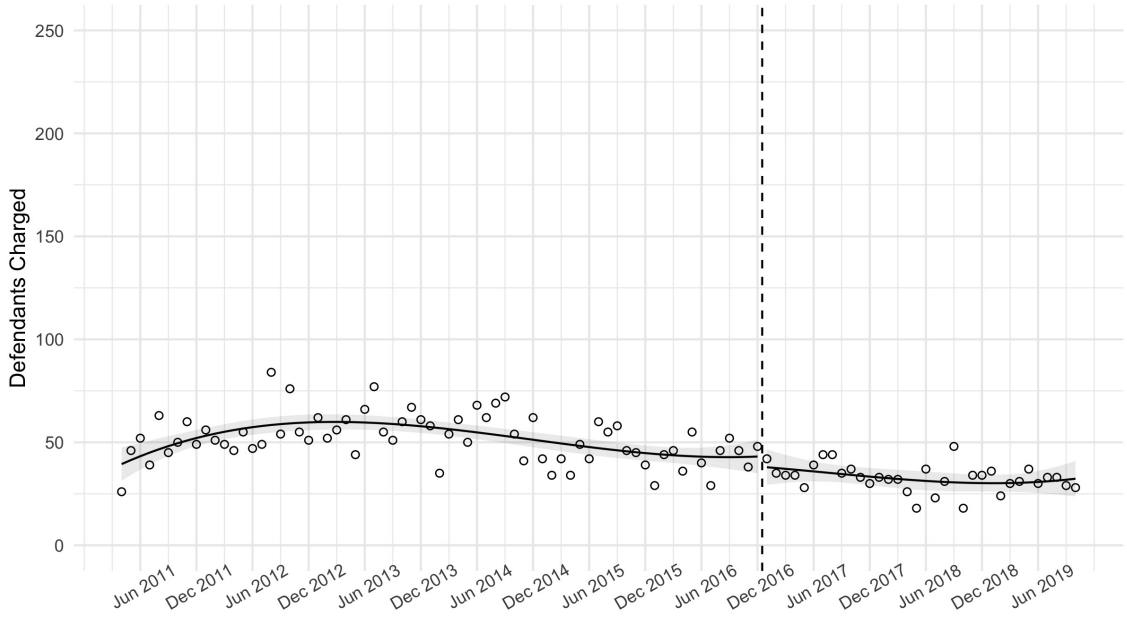
Notes: The comparison group is defined as non-retail theft below $10,000 for all datasets except incident data. [1]Here, the comparison group is defined as all non-theft crimes reported to have occurred in a retail location.

3.3 Results

I begin by offering evidence to support the key premise of the chapter: that by increasing the felony retail theft threshold from $300 to $1,000, the intervention rapidly and significantly reduced the number of defendants between December 2016 and January 2017. This step is necessary to demonstrate and quantify the impact of the felony charging reform intervention on the number of felony retail theft charges filed. Figure 3.3 illustrates the results from the full parametric linear model for retail theft ($R^2 = 0.93$) compared with the non-retail comparison theft group ($R^2 = 0.57$). The raw monthly count data are illustrated as series of points plotted alongside the model predictions and confidence intervals.



(a) Felony retail theft charges filed pre- and post-intervention.



(b) Comparison felony theft charges occurring in non-retail locations.

Figure 3.3: Segmented linear model predictions fitted to monthly felony theft charges

Source: 2011-2019 Cook County State's Attorney administrative records.

Figure 3.3A reveals that raising the felony threshold did indeed result in an immediate

and significant decrease of approximately 122 fewer defendants charged with felony retail theft ($s.e. = 11.4$, $p < 0.01$). There is evidence of autocorrelation with a Durbin-Watson test statistic of 1.7 ($p = 0.05$), but after the Prais-Winston transformation the intervention effect of -122 remains significant ($s.e. = 12.6, p < 0.01$, $R^2 = 0.91$). As expected, there is no significant treatment effect observed for in the comparison felony theft group ($p = 0.5$, $R^2 = 0.93$). Instead, these charges remained relatively constant over time with no evidence of autocorrelation (test stat = 2.0, $p = 0.5$). This is illustrated in the bottom Figure 3.3B.

These results are reproduced using non-parametric local linear models in Table 3.3. Across all bandwidth selections, the impact of the policy change on retail theft remains negative and statistically significant. All non-parametric estimates remain close to the approximately 120 defendant reduction of the parametric model. There is also no evidence that the retail-specific intervention meaningfully impacted the comparison theft group. There is just one statistically significant negative effect found using the uniform optimal bandwidth. Here, the estimated 14 fewer felony theft defendants is nonetheless quite small when compared to the magnitude of the treated retail theft group. This confirms the usefulness of the comparison theft group as a falsification test, since it was relatively unaffected by the retail-focused policy change. Having thus established that raising the charging threshold did indeed quickly and dramatically reduce the number of felony retail theft charges filed in Cook County, I next turn to evaluate if and to what extent this reform impacted other stages of the criminal legal process.

	Retail Theft			Comparison Theft		
	Triangular	*Uniform*	*Epanechnikov*	*Tri.*	*Uni.*	*Epan.*
Bandwidth						
5	-129.7***	-110.9**	-124.6**	-3.6	-5.1	-2.7
	(38.7)	(29.6)	(37.1)	(6.5)	(6.3)	(6.7)
Optimal	-121.3**	-128.6***	-121.2***	-9.3	-14.0*	-10.2
	(24.7)	(20.3)	(24.1)	(5.3)	(6.0)	(5.6)
15	-125.0***	-132.0***	-126.1***	-8.6	-5.9	-8.5
	(17.5)	(14.1)	(16.5)	(4.6)	(4.7)	(4.6)

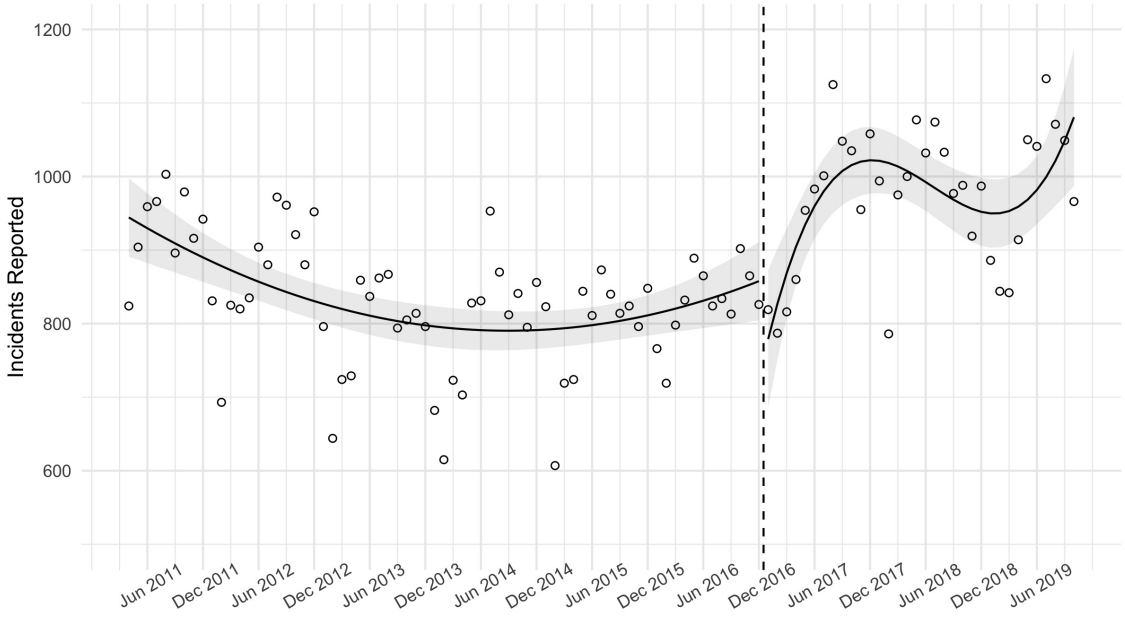
Table 3.2: Local linear estimated intervention effect on the number of monthly felony theft defendants charged

Notes: Total months $n = 103$ (pre $n = 69$; post $n = 34$). Optimal retail theft bandwidths are 9.1 for triangular, 9.2 for uniform and 8.7 for Epanechnikov kernel types. Optimal comparison theft bandwidths are 10.7, 7.9, and 9.7 for triangular, uniform and Epanechnikov kernel types, respectively. Standard errors reported in parentheses with robust bias-corrected significance reported at * = $p \leq 0.05$; ** = $p \leq 0.01$; *** = $p \leq 0.001$. Source: Cook County State's Attorney administrative records, 2011-2019.

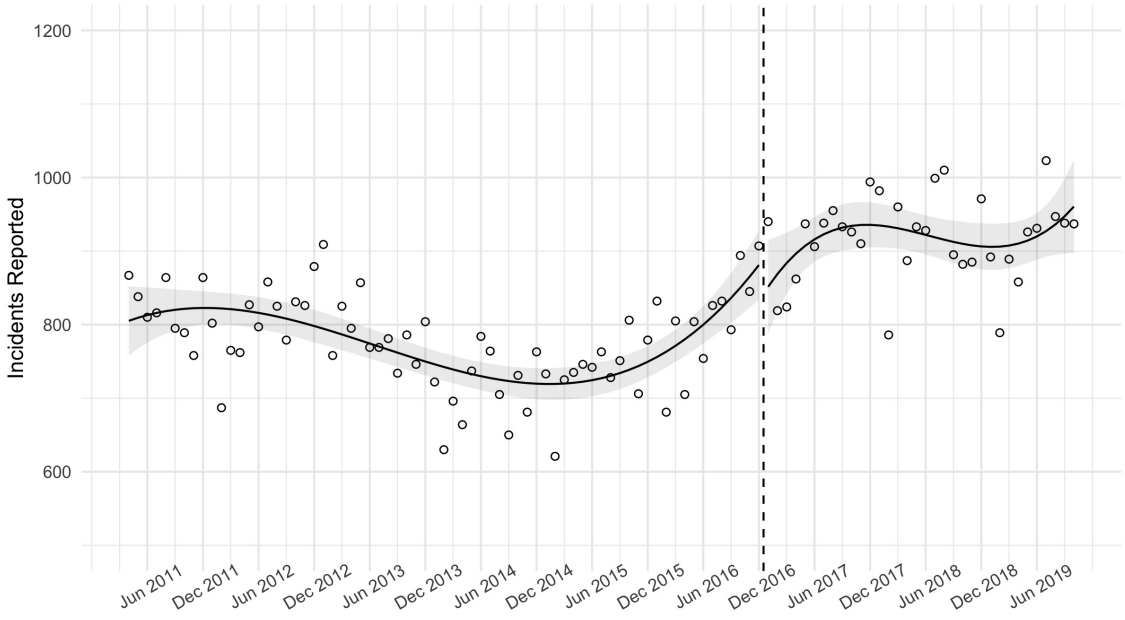
3.3.1 Crime Reporting and Police Response

Did the December 2016 charging reform impact retail thefts reported in Chicago? Figure 3.4A illustrates the predicted monthly number of reported retail theft incidents pre- and post-intervention ($R^2 = 0.52$). After correcting the retail theft model for seasonal autocorrelation (test stat = 1.1, $p < 0.00$), the immediate discontinuity at the cut-off point loses statistical significance ($p = 0.3$, $R^2 = 0.33$). However, the transformed model does find evidence of an upward month-on-month trend of reported incidents of retail theft after the charging reform ($\beta_3 = 42.4$, $s.e. = 19.9$, $p = 0.04$). Further, this increase appears specific to treated retail theft charges, since it is not replicated in the comparison model of non-theft crimes reported at retail locations ($p = 0.6$). This difference implies the month-on-month increase trend cannot be explained by either a general increase in criminal incidents or an overall increase

in reporting activity at retail stores.



(a) Reported retail theft incidents pre- and post-intervention.



(b) Comparison non-theft criminal incidents reported in retail locations.

Figure 3.4: Segmented linear model predictions fitted to monthly crime incidents reported in Chicago

Source: 2011-2019 Chicago Police Dept. incident data (Levy, 2020b).

This result is reproduced using non-parametric local linear models in Table 3.3. Here, I use kink regression discontinuity to test for a local change in the slope of retail theft incidents around the cut-off point. Using an optimal bandwidth, the positive impact of the new charging policy on the slope of monthly retail theft incidents remains significant under all kernel types. That said, using a narrower bandwidth, the estimated change in slope is not longer significant. And, the estimated month-on-month increase is lessened, but still significant when using a wider bandwidth. This uneven pattern may reflect the seasonal autocorrelation initially observed in the parametric models. Thus, the transformed parametric model may allow for the most defensible inference in this instance.

	Theft			Other Crimes		
	Triangular	*Uniform*	*Epanechnikov*	*Tri.*	*Uni.*	*Epan.*
Bandwidth						
5	35.5	35.4	36.5	-39.9	-13.9	-32.9
	(23.3)	(17.0)	(24.4)	(39.8)	(19.8)	(37.9)
Optimal	33.5***	44.1***	33.9***	-12.7	-5.1	-10.0
	(9.8)	(9.8)	(8.5)	(15.6)	(9.1)	(14.3)
15	16.3***	5.1***	13.3***	-7.1	-5.9	-6.6
	(4.6)	(4.6)	(4.8)	(6.5)	(5.0)	(6.2)

Table 3.3: Local linear estimated intervention effect on the slope (kink) of monthly reported criminal incidents in Chicago retail locations

Notes: Total months $n = 103$ (pre $n = 69$; post $n = 34$). Optimal retail theft bandwidths are 6.5 for triangular, 7.8 for uniform and 6.7 for Epanechnikov kernel types. Optimal comparison theft bandwidths are 8.1, 8.2, and 7.9 for triangular, uniform and Epanechnikov kernel types, respectively. Standard errors reported in parentheses with robust bias-corrected significance reported at * = $p \leq 0.05$; ** = $p \leq 0.01$; *** = $p \leq 0.001$. Source: 2011-2019 Chicago Police Dept. incident data (Levy, 2020b).

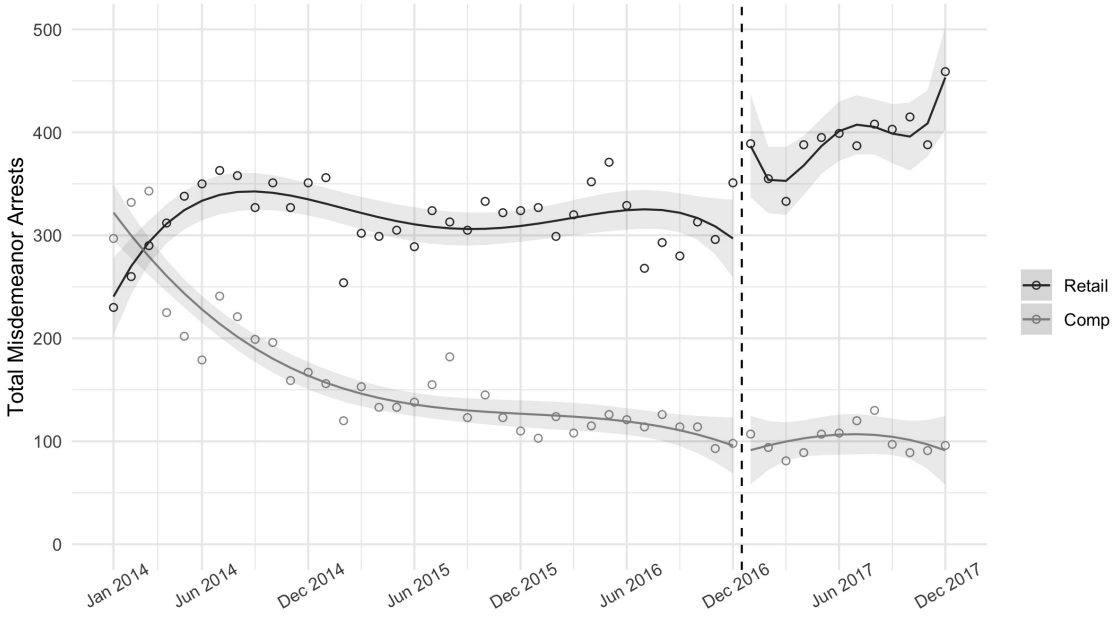
I also tested whether the new charging policy impacted police responsiveness operational-

ized using the number of monthly arrests-at-incident.[6] However, I found no evidence of any change in the monthly number of simultaneous arrests made by Chicago Police in both treated and comparison incident types: neither the immediate intervention effect nor the post-intervention trend were statistically significant in either parametric model.

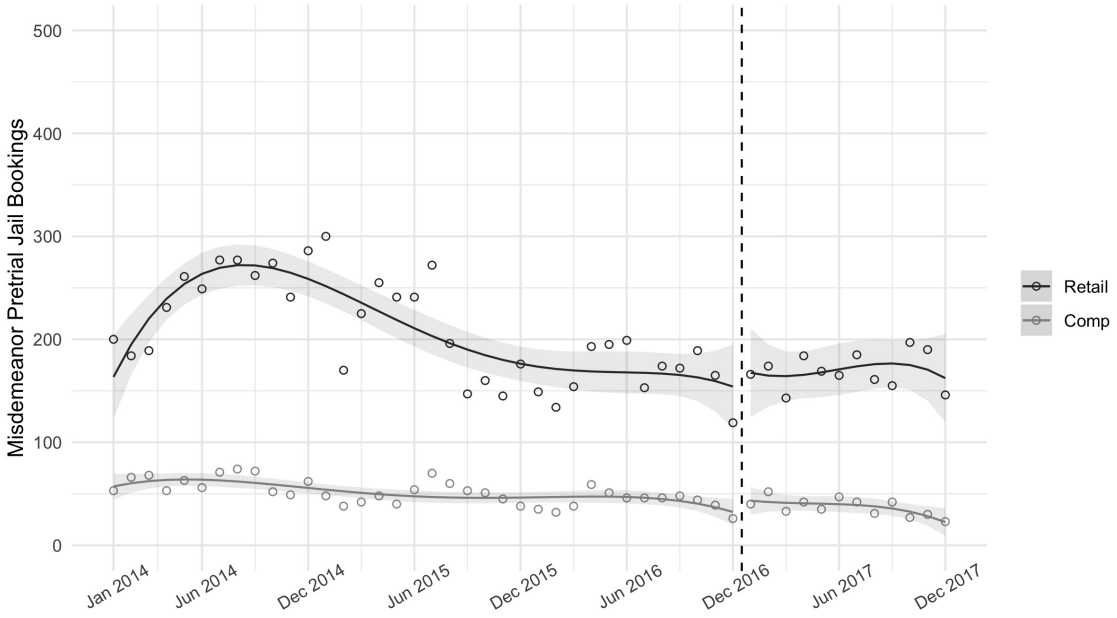
6. It is important to note here that arrest-at-incident is distinct from total arrests which can occur either at the time of the incident or afterwards. I consider total arrests in the following section.

3.3.2 Pretrial Detention and Dropped Charges

Next, I consider two questions at the intermediate stages of criminal legal contact. First, I evaluate whether felony charging reform simply shifts defendants into the misdemeanor courts. I operationalize misdemeanor legal contact at two points: arrest and booking into Cook County Jail. Unlike felonies, police have the discretion to immediately release misdemeanor theft defendants on bond (see Figure 1, above). Thus, even if misdemeanor arrests increase proportional to the drop in their felony counterparts, the new felony charging policy may still reduce the number of individuals subsequently booked into pretrial custody. Figure 3.5 offers evidence of this result.



(a) Total monthly Chicago Police Dept. misdemeanor theft arrests



(b) Total monthly misdemeanor theft bookings in Cook County Dept. of Corrections.

Figure 3.5: Segmented linear model predictions fitted to two measures of misdemeanor criminal legal contact.

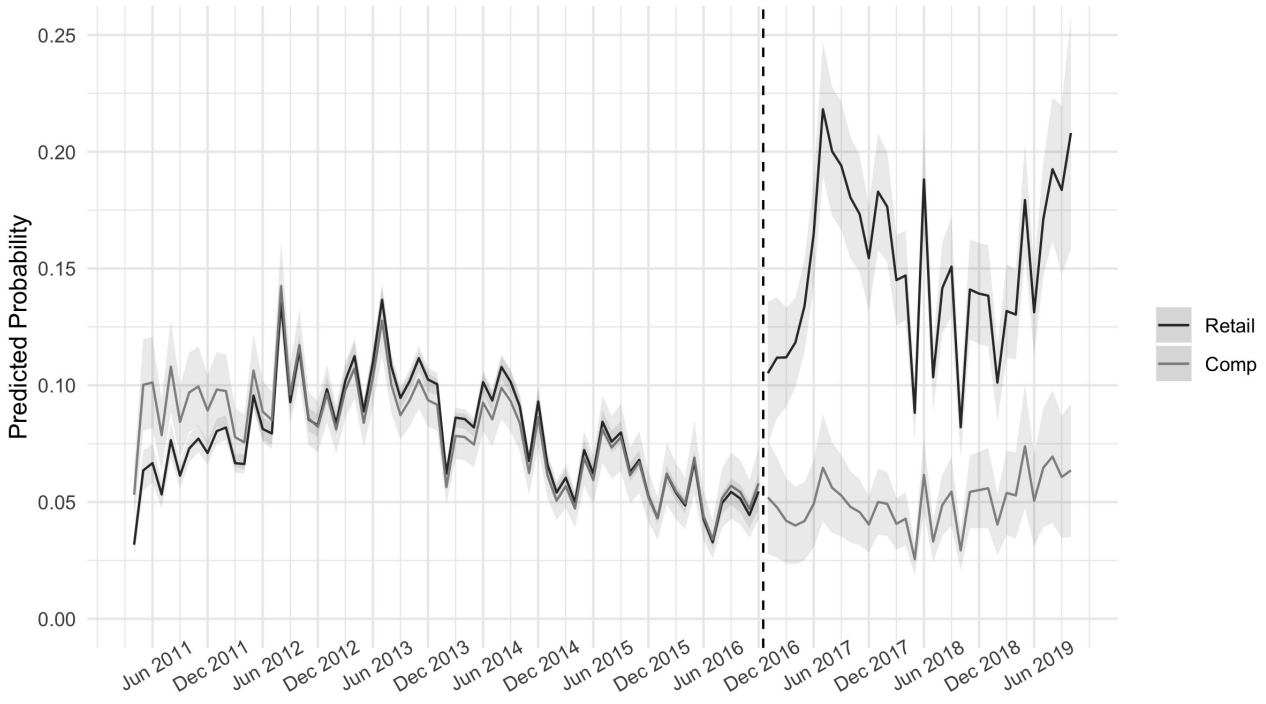
Source: 2014-2017 Chicago Police Dept. arrest (Levy, 2020a) and Cook County Sheriff booking records.

Figure 3.5A displays the raw and predicted monthly number of misdemeanor-level arrests for theft incidents pre- and post-intervention. For retail theft, there is an immediate impact observed after the new felony charging threshold is put in place. Specifically, there is an estimated increase of 175 (s.e. 70.7, $p = 0.02$, $R^2 = 0.68$) misdemeanor arrests at the cut-off. This impact lessens somewhat to an estimated 150 (s.e. 67.6, $p = 0.03$, $R^2 = 0.53$) after the model is adjusted for negative serial correlation over time (test stat = 1.6, $p < 0.01$). As expected, misdemeanor arrests for non-retail theft remain comparatively unaffected throughout the same time period (transformed $p = 0.8$, $R^2 = 0.83$). That said, I did not find evidence of this impact in subsequent non-parametric analysis.

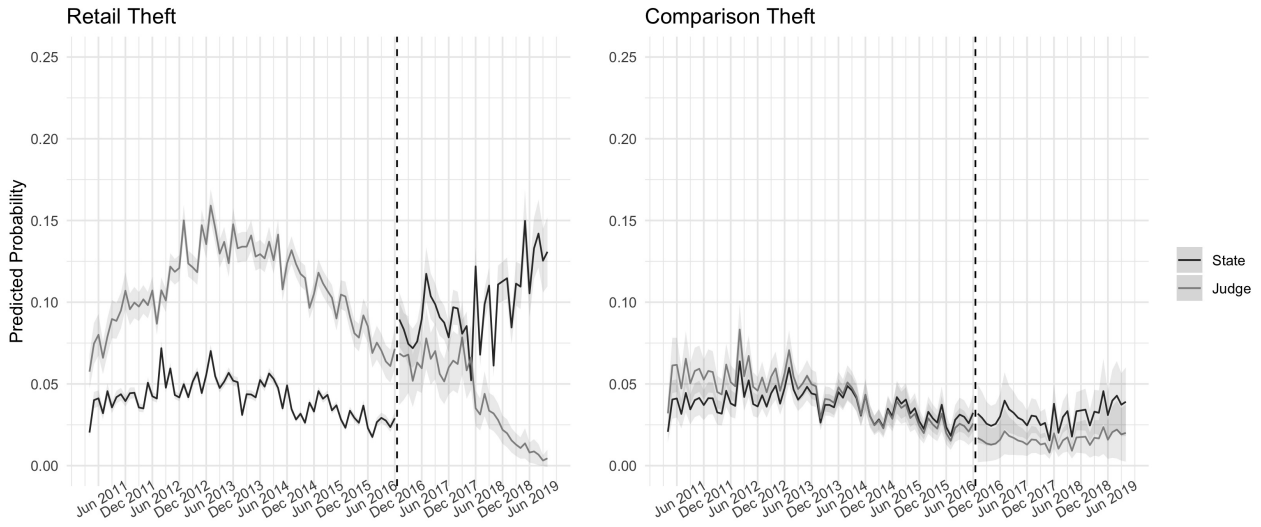
Despite evidence of an increase in the number of misdemeanor arrests, however, Figure 3.5B suggests that this did not translate into an increase in misdemeanor jail bookings. Instead, the number of monthly bookings is not estimated to have significantly changed through the new charging policy shift for both focal retail theft ($p = 0.5$) and comparison theft groups ($p = 0.2$). This null finding is confirmed using non-parametric local linear methods. In sum, then, while there is some evidence to suggest that police make more misdemeanor-level retail theft arrests under the new, higher threshold necessary to qualify for felony charges, there is no similar increase in the number of misdemeanor defendants booked into pretrial custody at Cook County Jail. This may be because police have greater discretion to directly release misdemeanor arrestees with no charges or on bond, unlike their felony counterparts.

The next intermediate question looks within felony courtrooms. Here, it is plausible that the charging reform may also cause prosecutors to change how they handle the remaining shoplifting charges that meet or exceed the new $1,000 threshold. I operationalize this question by measuring if there is any discontinuity in the likelihood that a shoplifting defendant's felony charges will be dropped at the preliminary hearing stage. Figure 3.6A offers a visual summary of a striking, immediate increase in the predicted probability of retail theft charges being dropped after the charging reform. Beforehand, the predicted probabilities

of a drop between retail and comparison theft are nearly indistinguishable, following very similar trends over time. Then, at the cut-off point, the predicted probability of dropped shoplifting charges jumps from just over 5% (c.i. 6.2-4.7) in December 2016 to over 10% (c.i. 7.4-13.6) in January 2017. This trend continues, with the predicted probability of dropped retail theft charges reaching nearly 22% (c.i. 18.9-24.6) in July 2016. This change is not replicated in the comparison theft group, where after 2013 the predicted drop probability remains stable at around 5% over time.



(a) Overall predicted probability of dropped charges.



(b) Predicted probability of charges dropped by the prosecutor versus the judge.

Figure 3.6: Segmented binomial model predictions of the probability of dropped theft charges in preliminary courtrooms.

Source: 2011-2019 Cook County State's Attorney administrative records.

In the preliminary hearing, however, a felony charge may be dropped in one of two ways: the judge may find that it is not supported by probable cause, or it may be dropped *nolle prosequi* by the prosecutor (refer to Figure 3.1, above).[7] Figure 3.6B distinguishes between the two courtroom actors responsible for dropped charges. This more specific approach reveals an even more dramatic, immediate impact of the retail-specific intervention with regard to prosecutorial decision-making. This is not observed in judges, however, whose probability of finding no probable cause for shoplifting charges has steadily declined from over 15% (c.i. 16.9-14.9) in July 2013 to less than 1% (c.i. 0.2-1.4) by June 2019. After the charging reform, then, Figure 3.6B reveals that is is the prosecutor who has driven the overall increase in dropped retail theft charges initially observed in Figure 3.6A. Before the intervention, in December 2016, there is just under 3% (c.i. 2.5-3.3) predicted probability that the prosecutor will drop these charges *nolle prosequi*. Afterwards, in January 2017, this probability is predicted to jumps to nearly 9% (c.i. 7.3-10.6). This trend is predicted to increase over time, reaching a 15% (c.i. 13.1-16.9) drop probability in May 2019.

Here, model specification is important, given potentially confounding changes in courtroom practice that came with the new Foxx administration. For example, in both the overall and prosecutor-initiated drop models, I include covariates to control for the effect of participating in an alternative prosecution program at the preliminary stage. All models also include a covariate for the number of hearings, intended to capture evidentiary complexity and delay (e.g. responding officer, defendant or witness failure to appear). Both specifications consistently improved the AIC-measured fit of these models. The exposure term used is the number of defendant who experienced a preliminary hearing, thus excluding the (rare) cases where the defendant was charged via grand jury.[8]

Table 3.4 suggests that this jump in the predicted probability of prosecutor-initiated

7. Like Kutateladze et al. (2014), I do not model the decision to reject felony charges at the earlier, informal screening stage because this decision is recorded in fewer than 1% of theft cases in the dataset.

8. Although nominally an independent charging body, the grand jury rubber-stamps virtually all charges put to them by the prosecutor (Kuckes, 2004). This is confirmed in my data, where there is no record of the grand jury rejecting any of the theft charges brought to them by the prosecutor in over a decade.

drops can be attributed to the combination of two factors. On one hand, as can be expected, the new felony threshold is estimated to have dramatically reduced the caseload in Cook County preliminary courtrooms by around 140 cases. In comparison, while non-retail theft preliminary hearings were also reduced, the magnitude of this decrease was relatively small. On the other hand, and unlike comparison charges, prosecutors did not proportionally reduce the number of charges they dropped *nolle prosequi* during the same period. Together, these two changes lead to a sharp post-intervention increase in the predicted probability of prosecutor-lead drops.

	Dropped Retail Theft			Dropped Comp. Theft		
	Triangular	*Uniform*	*Epanechnikov*	*Tri.*	*Uni.*	*Epan.*
Bandwidth						
5	-28.1*	-30.7***	-29.2	-1.1	-1.9	-1.2
	(2.5)	(1.8)	(2.8)	(0.8)	(0.7)	(0.7)
Optimal	-25.7***	-24.9***	-23.5***	-0.9	-2.0***	-1.3*
	(1.6)	(2.5)	(2.0)	(0.5)	(0.6)	(0.5)
15	-20.8***	-17.9***	-20.3***	-0.2*	0.1	0.1
	(2.2)	(3.1)	(2.4)	(0.4)	(0.4)	(0.4)
	Retail Theft Prelim. Hearings			Comp. Theft Prelims		
5	-155.2***	-133.7***	-151.4***	-8.1*	-9.4	-7.4*
	(23.2)	(16.7)	(22.2)	(5.2)	(4.1)	(5.0)
Optimal	-139.4***	-132.1***	-139.8***	-13.5***	-17.7***	-13.8***
	(16.0)	(12.9)	(16.0)	(3.8)	(4.1)	(3.9)
15	-133.2***	-139.7***	-133.6***	-8.1**	-4.3***	-7.4**
	(9.1)	(7.6)	(8.4)	(2.3)	(2.1)	(2.2)

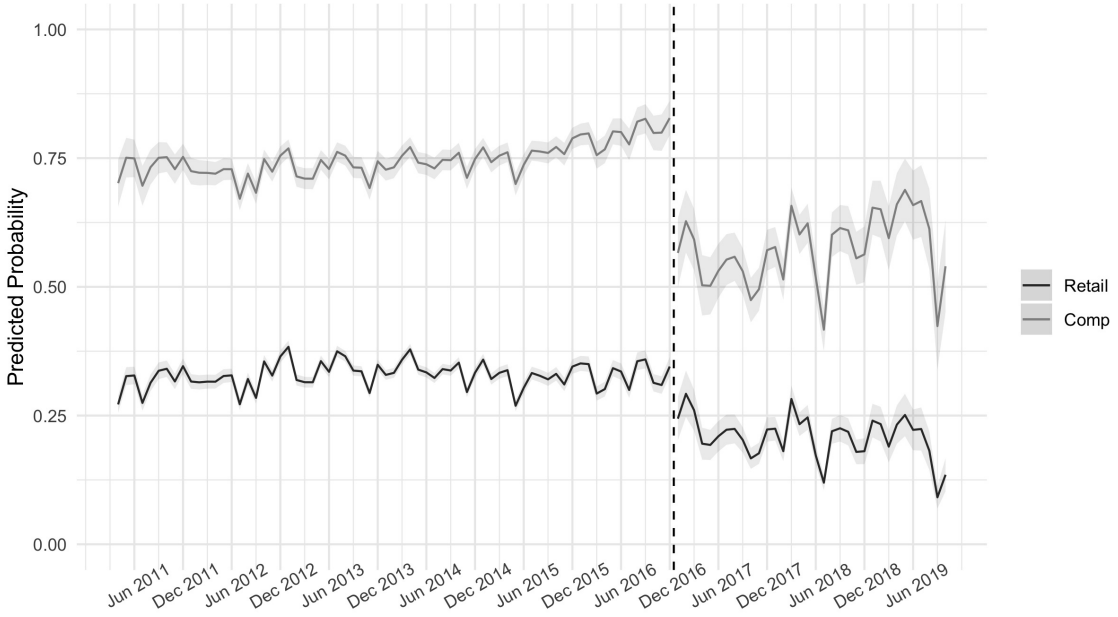
Table 3.4: Local linear estimated intervention effect on the monthly number of prosecutorial drops relative to preliminary theft hearings

Notes: Total months $n = 103$ (pre $n = 69$; post $n = 34$). Optimal drop retail theft bandwidths are 8.7 for triangular, 7.6 for uniform and 9.2 for Epanechnikov kernel types. Optimal retail theft preliminary hearings bandwidths are 7.7 for triangular, 7.2 for uniform and Epanechnikov kernel types. Optimal drop comparison theft bandwidths are 9.8, 6.8, and 8.7; comparison preliminary hearings bandwidths are 7.9, 6.1, and 7.2, for triangular, uniform and Epanechnikov kernel types, respectively. Standard errors reported in parentheses with robust bias-corrected significance reported at * = $p \leq 0.05$; ** = $p \leq 0.01$; *** = $p \leq 0.001$. Source: Cook County State's Attorney administrative records, 2011-2019.

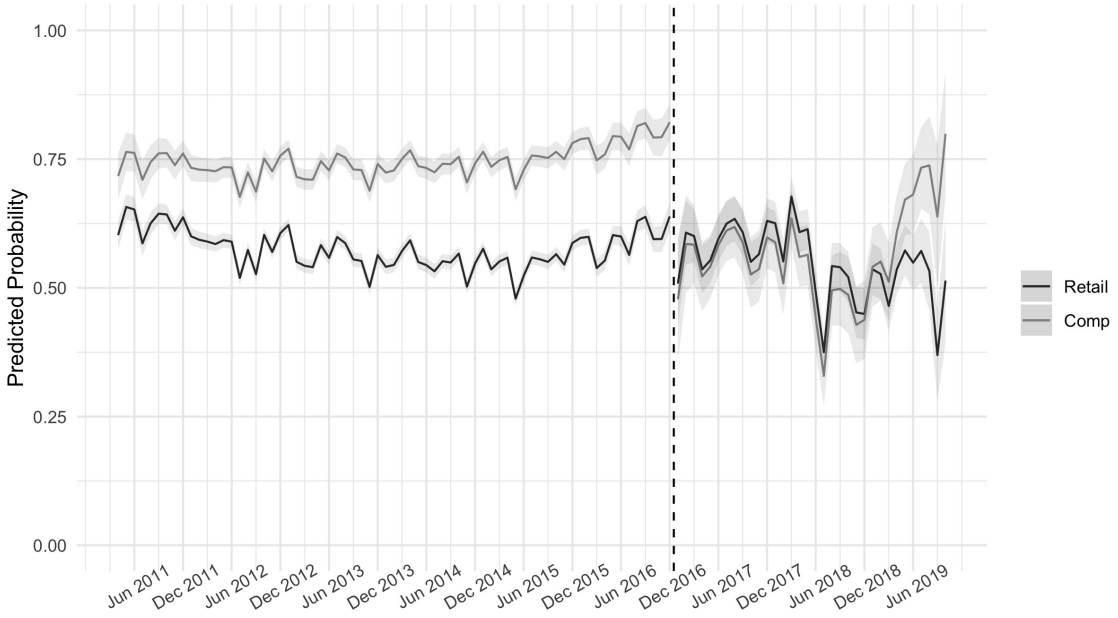
3.3.3 Punishment Type and Length

In the last step of the analysis, I evaluate if there is any direct impact of the felony charging reform on punishment. First, I consider punishment type, specifically, whether a convicted defendant will be sentenced to prison or probation. Next, I look at the length of both types of punishment in months. In all models with punishment outcomes, I include covariates that capture alternative prosecution, time to disposition, and whether the defendant took a plea deal. These two final factors have been shown to be relevant to subsequent punishment severity (Abrams, 2011; Bushway et al., 2014; Ulmer and Bradley, 2006). Indeed, these specifications consistently improved the AIC-measured fit of these models. The exposure term used is the number of convicted defendants. All predictions are built upon a hypothetical defendant who pleaded guilty.

Figure 3.7 illustrates the results of segmented binomial models of the likelihood that theft convictions will result in either probationary or custodial punishment terms. Here, the likelihood of both probation and incarceration drops quite sharply at the cut-off point for non-retail comparison theft charges. Since the comparison group was not treated with the specific policy intervention, there is evidence to suggest that the more relevant change in terms of punishment type was the broader contemporaneous change in administration. If there was any impact of the raising the felony retail theft threshold, it cannot be distinguished from this confounding shift.



(a) Predicted probability of probation.

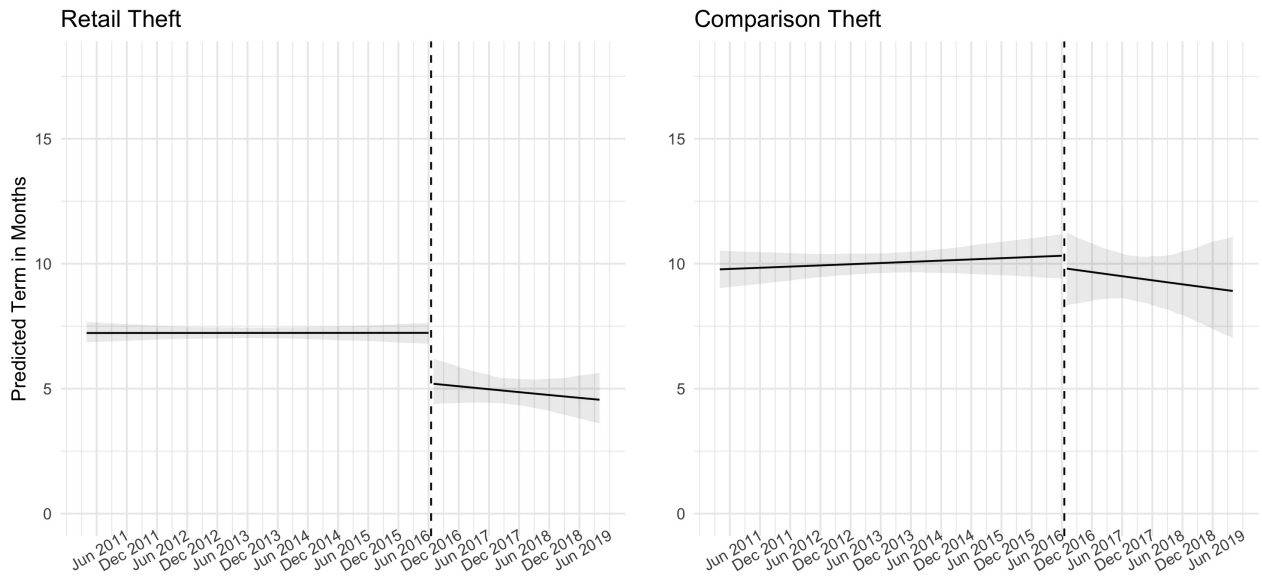


(b) Predicted probability of incarceration.

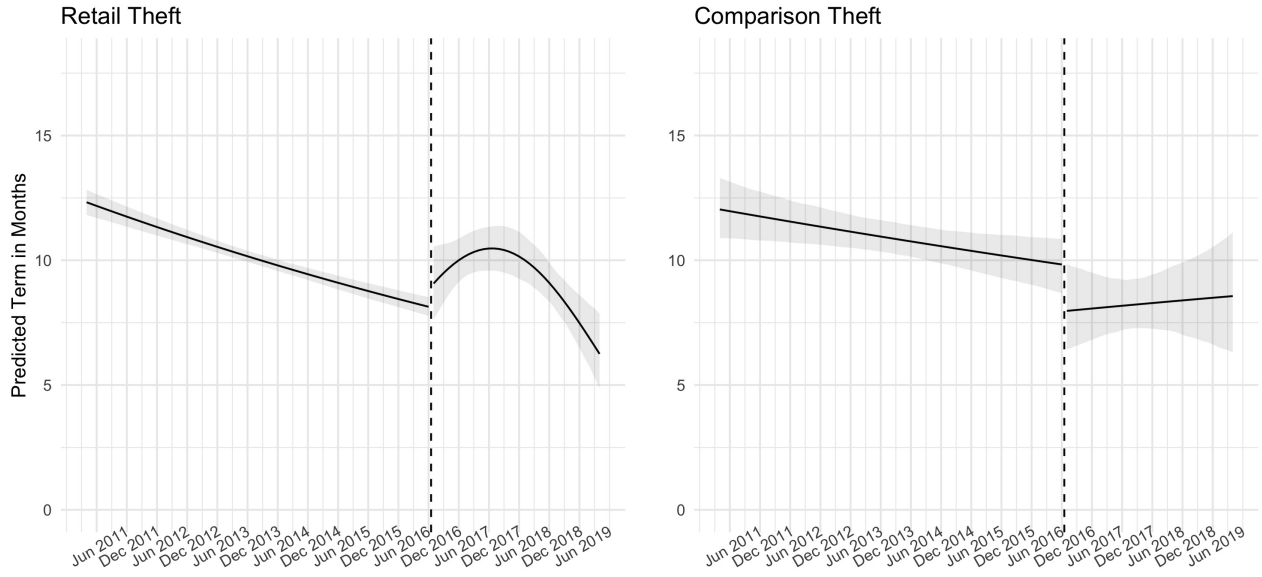
Figure 3.7: Segmented binomial model predictions of the probability of probationary versus incarcerative punishment.

Source: 2011-2019 Cook County State's Attorney administrative records.

In terms of the length of punishment, though, Figure 3.8A displays discontinuities in retail theft probationary terms not observed in the comparison theft group. This figure is built using the count results of segmented zero-inflated negative binomial models in order to account for the non-normal distribution of the outcome variable. Those defendants not sentenced to the punishment type in question are counted as zero. Before the intervention, shoplifting convictions are predicted to have a stable probationary term of around 7 months. After the intervention, in January 2016, this predicted probationary period dropped to just over 5 months (c.i. 4.4 - 6.2). Over time, there is also evidence of month-on-month trend that slightly reduces the length of probationary sanctions. This post-reform downward trend seems durable over time, and is in contrast to the stability of the predicted terms before the policy change.



(a) Predicted probationary term in months.



(b) Predicted custodial punishment term in months.

Figure 3.8: Segmented negative binomial model predictions of the length in months of probationary versus incarcerative punishment.

Source: 2011-2019 Cook County State's Attorney administrative records.

This finding is not replicated for predicted custodial terms, however. Instead, Figure 3.8B, illustrates no significant discontinuity at the intervention point for either the treated or control theft groups. That said, for shoplifting, it does predict that the monthly downward trend in custodial punishment length ended simultaneous with the felony charging reform. For example, for defendants charged in July 2014, there is a predicted custodial term of 9.7 months (c.i. 9.5 - 10.0); two years later, in July 2016, this predicted term has fallen by over a month to 8.4 (c.i. 8.1 - 8.8). In the summer after the charging reform, though, a new, upward monthly trend brings the predicted length of punitive custody up to just over 10 months (c.i. 9.3 - 11.0). That said, it does not appear that this upward trend continues over time: two years later, the predicted length of incarceration in months has fallen to 7.2 (c.i. 6.1 - 8.4) for those charged in July 2019. For the final sentencing stage, then, the long-term impact of the new charging policy is limited to reducing the probationary terms by a predicted two month period.

3.4 Discussion

In this chapter, the impact of felony charging reform was investigated at a variety of crucial stages in the criminal legal process: from incident reporting, arrest and pretrial detention, to preliminary hearing and sentencing decisions. This task was pursued using a regression discontinuity design built with a decade's worth of administrative data from the Cook County, Illinois State's Attorney's Office, as well as supplemental data from the Sheriff's Office and the Chicago Police Department. The focal charging reform occurred in December 2016, when the newly-elected State's Attorney Kim Foxx raised the felony charging threshold for retail theft from $300 to $1,000. I compared these 'treated' retail theft charges to their 'untreated,' non-retail theft counterparts in order to isolate the broader confounding effects of administrative change from the specific impact of charging fewer retail theft felonies.

First, I demonstrated that, by raising the felony charging threshold, the new policy more than halved the number of felony shoplifting charges in Cook County from December

2016 to January 2017. In contrast, this intervention was shown to have no impact on the comparison group of non-retail theft charges. Analyses were then divided into three main parts that corresponded to the early, intermediate and final stages of the felony adjudication process. Overall, I found the charging intervention to have had the largest impact on the intermediate case processing stage. At this intermediate stage, I evaluated the empirical basis for two hypothetical problems facing prosecutor-lead decarceration efforts.

One hypothetical problem expected fewer felony shoplifting charges to result in a proportional increase in misdemeanor caseloads. To test this problem, I operationalized misdemeanor legal contact at two points: arrest and booking into Cook County Jail. While there was evidence that raising the felony charging threshold did cause a proportional increase in misdemeanor-level retail theft arrests, there was no increase in the monthly number of misdemeanor defendants booked into Cook County Jail. This is because, unlike felonies, police have the discretion to immediately release misdemeanor defendants on bond. More broadly, then, these findings offer insight into how a reform focused on felony-level charges works to redistribute criminal legal contact downwards into the already-massive misdemeanor system (Mayson and Stevenson, 2020; Natapoff, 2018; Stevenson and Mayson, 2017).

There are benefits to reducing formerly felony-level retail theft prosecutions to misdemeanor offenses. For example, even when facing a misdemeanor conviction, a substantial number of retail theft defendants are spared the lengthy formal sanctions and institutionalized stigma attached to a felony criminal record (Jacobs, 2015; Miller, 2021; Pager, 2007; Manza and Uggen, 2006; Wakefield and Wildeman, 2013; Western, 2002). And, since most of these defendants are immediately released on bond by the police, they are also spared the considerable harm of even short-term pretrial detention. These harms range from an immediate lack of physical and mental health care and limited access to legal counsel to longer-term family instability, employment struggles and political disengagement (Comfort, 2016; Dobbie et al., 2018; Fernandes, 2020; Heaton et al., 2017; Irwin, 1985; Lerman and Weaver, 2014; Reinhart and Chen, 2020; White, 2019). More broadly, of course, fewer pre-

trial detainees will help to reduce the population of incarcerated individuals as a whole. In this chapter, my findings suggest that a reduced pretrial jail population is indeed a plausible outcome of felony-level charging reforms.

That said, misdemeanor-level prosecutions carry harms in their own right. Even when they are released on bond, misdemeanor defendants must maintain ongoing contact with the criminal legal system. For example, repeated, day-long court appearances may chronically disrupt employment responsibilities and family obligations (Feeley, 1979; Kohler-Hausmann, 2018; Van Cleve, 2016). Courtroom debasement rituals may cause shame and embarrassment that are both psychologically and socially damaging. When defendants perceive courtroom actors to be biased, untrustworthy or disrespectful, they tend to lose faith in the legitimacy of not only the law itself, but also non-punitive (even beneficial) surveilling state institutions (Brayne, 2014; Goffman, 2014; Tyler, 2006). Failure to appear in court may result in a warrant, further expanding the discretionary ability of law enforcement to arrest and detain individuals. Finally, if convicted, misdemeanants must nonetheless carry the mark of a permanent criminal record, even if they avoided the more-severe felony label. Although it is beyond the scope of this chapter, future research would do well to explore if and to what degree felony-focused decarcerative efforts may have proportionally expanded these non-custodial harms and "procedural hassles" of the misdemeanor court system (Kohler-Hausmann, 2018, 183).

A second hypothetical problem expected the decarcerative aims of charging reform to be frustrated if prosecutors treat remaining felony shoplifting cases more severely. This hypothesis is grounded in the reasoning that, when faced with a lighter caseload, prosecutors may now have more time to win convictions for those remaining cases. However, I did not find any evidence of this problem in Cook County. Instead, after the charging reform, I found a discontinuous increase in the likelihood that the remaining retail theft charges will be dropped by the prosecutor at the preliminary hearing stage. Additional non-parametric analysis suggested that this increase was driven by two factors. On one hand, there were

suddenly far fewer preliminary hearings for retail theft charges. Of course, this is to be expected given the higher charging threshold. On the other hand, though, prosecutors did not proportionately reduce the number of monthly shoplifting charges they dropped *nolle prosequi* after the intervention. Thus, the likelihood of a shoplifting charge being dropped by the prosecutor increased. I expect this outcome to have the largest decarcerative impact of all observed in this chapter; unlike the misdemeanor arrests discussed above, felony charges that are simply dropped *nolle prosequi* at the preliminary hearing stage fully exit the criminal legal process with no formal record, no sanctions, and no further compliance requirements. The decision to drop charges is thus seen as the most discretionary and consequential instrument of prosecutorial power (Bellin, 2018).

Unfortunately, quantitative analysis is not well-suited to answer what *motivated* prosecutors to continue dropping shoplifting charges at nearly pre-intervention levels despite dramatically reduced caseloads. Further qualitative research may fruitfully evaluate the merits of two reasonable explanations. It could be that, after the charging reform, prosecutors in Cook County preliminary courtrooms chose to actively drop more felony retail theft cases in order to align their day-to-day decisions with the increased leniency signaled by the executive. That said, it could also be that prosecutors have become accustomed to managing a high caseload using predictable routines to rapidly dispose criminal charges (Eisenstein and Jacobs, 1977; Feeley, 1979). Perhaps these routines remain even when caseload is reduced.

The question of motives deepens when one considers my findings in the context of the collaborative courtroom working group. Nearly fifty years ago, Eisenstein and Jacobs (1977) developed this fundamental concept in the same Cook County preliminary hearing courtrooms that produced the data for this chapter. In the last decade, though, there is evidence of dramatic shifts in the routine interactions of this seminal courtroom working group. Figure 6B, above, illustrates this change graphically: after the charging reform, prosecutors replaced judges as the courtroom actor most likely to drop a shoplifting charge. It remains to be explored, though, what on-the-ground interactions lead to this shift. It is possible,

as Flemming et al. (1992, 31) have observed, that "reformist" prosecutors typically become "initiators" who seek to exert more control and responsibility in their courtrooms (ibid.). In turn, this newfound exercise of agency could backfire, as judges reassert their authority in shaping the case docket, granting motions favorable to the defense, or even publicly berating their Assistant State's Attorney with relative impunity (Van Cleve, 2016).[9] Perhaps judges translate more prosecutor-initiated drops into a lessened responsibility to limit the number of cases flowing into trial courts. Or, they may even become concerned that too few charges are moving forward to justify their gatekeeping authority. Again, my quantitative findings open exciting new qualitative questions about how courtroom cultures may be adapting to (or resisting) a new decarcerative ethos.

Although the charging reform had its most profound impact at these intermediate stages, it also caused modest change in the punishment terms assigned to convicted individuals at the final sentencing stage. Here, I hypothesized that the intervention could be robbed of its intended decarcerative effect if any gains resulting from fewer felony defendants were overwhelmed by more punitive and longer sentences for those remaining (Seeds, 2017; Tonry, 2016). However, I did not find evidence to support this problem in the long-term. While the pre-intervention downward trend in custodial punishment length was predicted to have been interrupted by a new, post-intervention upward monthly trend, this did not persist over time. Instead, nearing the end of the observation period, there was a predicted return to pre-intervention levels in the length of punitive incarceration.

There was also evidence that the charging reform reduced the predicted length of probationary terms by two months. This suggests it is indeed plausible that even those charged with a felony under the increased threshold were granted greater leniency at the sentencing stage (Balboni and Grometstein, 2020; Bazelon, 2019). In turn, by reducing the length of

9. Recently, there have been surprising examples of judges refusing to accept the discretionary decision to drop charges form reform-minded prosecutors (Barkow, 2021). Although this no doubt oversteps the authority of the judicial branch over the executive, it evidences a newfound tension between prosecutors and their judges as charging practices shift.

probation, there is likely an indirect, but consequential, downward pull on the overall incarcerated population. Importantly, this impact reduces the amount of time of individuals must spend living under non-custodial surveillance (Miller, 2021). In turn, by shortening this period of surveillance, there is a lessened risk that individuals will be incarcerated due to a probation violation (Kingsnorth et al., 2002).

There is thus evidence that, by raising the felony charging threshold from $300 to $1,000 in December 2016, Cook County State's Attorney Kim Foxx did effect decarcerative change at both the intermediate and final stages of the criminal legal process in her jurisdiction. In conclusion, I touch upon two reasonable criticisms of this policy intervention. The first argues that any meaningful solution to mass incarceration cannot focus only on the "low-hanging fruit" of so-called non-violent crimes like drug possession, or indeed, retail theft (Seeds 2017, 592, see also Gottschalk 2015). While the majority of incarcerated individuals in the United States have been convicted of 'violent' crimes (Sawyer and Wagner, 2020; Pfaff, 2017), Beckett et al. (2016) find a wealth of evidence that most, if not all, governmental efforts to end mass incarceration are exclusively focused on crimes deemed non-violent, like retail theft. However, prison populations will not be substantially reduced if prosecutors restrict their decarcerative reform efforts (Sawyer and Wagner, 2020).

I fully agree with this criticism. I offer it here as a limiting factor of this chapter's narrow focus on retail theft charging practices. Future research should evaluate how reform-minded prosecutors have tackled (or ignored) incarceration rates for crimes understood to be violent (see Chapter 4). An optimist may expect that narrower reforms will gradually expand from retail theft to include more serious crime (Balboni and Grometstein, 2020; Bazelon, 2019). The pessimist would counter that, even if those facing non-violemt charges are incarcerated at lower rates, this marginal gain will nonetheless be overwhelmed by "patterns of expansion at punishment's upper end" (Seeds, 2017, 591). Finally, the moderate position would expect incarceration levels to slightly decline as narrow reforms "nibble at the edges" of mass incarceration (Tonry, 2016, 253). Indeed, the goal of this chapter was to offer a methodologically

robust accounting of the success of this sort of modest reform focused on felony retail theft charges.

A second and final criticism points to my finding that, after raising the felony charging threshold, there is evidence of a a slight increase in the month-on-month slope of reported retail theft incidents over time. This suggests that the State's Attorney's well-publicized move towards leniency in shoplifting prosecutions had the inadvertent effect of increasing the incidents of that crime in Chicago. By reducing the felony threshold, the prosecutor also decreased the expected risk individuals associate with retail theft, thus weakening the deterrent effect of punishment. This finding offers a powerful and politically charged retort to any elected prosecutor's decarcerative aims (Barkow, 2019; Davis, 2019). It is an empirical finding that could sway a 'smart' prosecutor back towards a regressive 'tough on crime' approach.

I argue that this finding should be interpreted differently. Specifically, it should be understood in the context of a fundamental duty of the prosecutor: to define who is included (and excluded) in the amorphous public she is tasked to represent. As stated in American Bar Association (ABA) standards, the prosecutor "serves the public and not any particular government agency, law enforcement officer or unit, witness or victim" (American Bar Association, 2015, 3-1.3). Here, the ABA standards explain that the public interest to be represented "should be determined by the chief prosecutor and designated assistants" (ibid.). Historically, retail business interests have been privileged by local prosecutor's offices, who allocate significant resources to handling shoplifting as a criminal offense. This choice may well have disincentivized retailers from investing in preventative measures that are in fact less costly, from a societal perspective, when compared to the collective burdens of arresting, detaining, convicting and punishing shoplifters (Rappaport, 2018). Thus, one could argue that the increase in retail theft incidents would be best dealt with by the retailers themselves.

This interpretation is strengthened when considering who the prosecutor has historically excluded from the public. In 2016, State's Attorney Kim Foxx was elected because voters of

color, and their allies, were outraged that the previous administration did not charge police officers with murder after they killed two young Black people: Laquan MacDonald and Rekia Boyd. State's Attorney Foxx was thus elected with a clear mandate to represent the interests of communities of color, whose members had historically been defined as second-class, "custodial" citizens (Lerman and Weaver, 2014). Here, I have offered evidence to suggest that the Foxx administration succeeded in reducing the use of criminal courts, jails and prisons with targeted felony charging reform. Now, there is an ongoing wave of newly-elected prosecutors who intend to pursue similar decarcerative policies. While it remains to be seen if similar reforms succeed in other jurisdictions, Cook County, Illinois offers a cautiously optimistic case study in the path to ending mass incarceration.

CHAPTER 4

BIFURCATED PROBATION AND PUNISHMENT FOR GUN CRIME

Can legislative sentencing reform end mass incarceration in the United States? State lawmakers generally aim to decarcerate using two sequential strategies. First, they define and bifurcate a subpopulation that is deemed deserving of increased leniency (Beckett et al., 2016; Gottschalk, 2015; Seeds, 2017; Richardson and Kutateladze, 2021). Next, legislators enact leniency for the bifurcated group by replacing prison sentences with probationary surveillance (Beckett and Murakawa, 2012; Miller, 2021). Theoretically, bifurcated probationary sentencing may indeed offer a way out of the incarcerative status quo. This argument usually follows a cost-benefit logic (Richardson and Kutateladze, 2021). Prisons may be socially necessary to incapacitate supposedly violent criminals, but the substantial psychological, community and economic costs of incarcerating 'non-violent' individuals are too high a price to pay (ibid.). Thus, these individuals should be identified and released on probation; in turn, incarceration rates will fall.

In the chapter, I use a regression discontinuity design to evaluate the decarcerative efficacy of bifurcated probation. Specifically, I use a decade of felony courtroom data from the Cook County State's Attorney's Office to gauge the impact of a new bifurcated sentencing law for gun possession enacted by the Illinois state legislature in January 2018. With this case, I test the empirical merit of two theoretical criticisms of bifurcated probation. The first expects prison populations will not be substantially reduced under a bifurcated reform logic, since most incarcerated individuals are excluded by definition (Sawyer and Wagner, 2020). At best, then, one may expect only a marginally reduced number of people behind bars (Tonry, 2016); at worst, custodial terms may lengthen for those categorized as violent (Seeds, 2017). The second criticism notes that probationary sentencing reforms work to expand the reach of carceral state control beyond prison walls, opening a new repertoire of "therapeutic"

techniques of control (Beckett, 2018; Cohen, 1985; Stuart, 2016). Further, if individuals fail to meet compliance requirements, they may will nonetheless experience custodial terms for probation violations (Kingsnorth et al., 2002; Phelps, 2013). This will not only maintain incarcerated populations, but could also result in a net increase in the length of punishment for those initially offered probation.

This chapter offers preliminary evidence of the decarcerative effects (or lack thereof) of sentencing reform. As expected, the new law intensified a pre-intervention upward trend in the use and length of probationary sanctions for first-time gun possession. Surprisingly, though, there is no clear evidence that it increased the predicted punishment term for gun recidivists sentenced to prison. I also identify an unexpected discontinuity in gun possession punishment after the Foxx administration took office in December of 2016. At this time, the State's Attorney's Office began a new policy that allowed prosecutors to offer a plea deal without the prior approval of their unit supervisor. Individual prosecutors were therefore no longer subject to routine managerial oversight of the plea bargaining negotiations in their courtrooms. This organizational change resulted in a smaller, but sudden, increase in the length of probationary sentencing for first-time gun possession. There is evidence that it also caused a simultaneous decrease in the predicted length of prison terms for both subpopulations.

Although this chapter is primarily focused on legislative change, future research will further develop the practical and theoretical implications of this unexpected two-fold discontinuity. This finding offers a unique opportunity to quantify the relative effect of internal, organizational factors versus external, legislative factors in sentencing outcomes. Future research will also supplement initial parametric results with non-parametric methods. I also intend to use segmented parametric survival analysis to test whether bifurcated probation increases the risk of probationary failure for first-time gun offenders. After all, it is plausible that this sentencing reform may in fact produce a comparatively longer custodial term for those first-time probationers who violate the extensive surveillance requirements attached to

their initial sanction.

4.1 Legislating Mass Incarceration

The so-called tough on crime era of lawmaking is widely recognized as a primary cause of the unprecedented, and enduring, reliance on prisons (Blumstein and Beck, 1999; Campbell and Schoenfeld, 2013; Goodman et al., 2014; Raphael and Stoll, 2013). Over the last two decades, however, punitive lawmaking has lost much of its political appeal (Beckett et al., 2016; Cadora, 2014). Now, mass incarceration is recognized as an urgent problem by fiscal conservatives and social progressives, establishment reformists and grassroots activists alike in a rare point of consensus (Aviram, 2011; Dagan and Teles, 2014). Both Democrats and Republicans have introduced sentencing reforms use two sequential strategies: first, they bifurcate 'violent' from 'non-violent' crimes and reserve leniency for the latter; and second, they enact leniency by replacing prison terms with probationary surveillance. In what follows, I define this reform strategy as bifurcated probation.

New sentencing laws enact bifurcated probationary reforms using two sequential steps. The first involves defining a subpopulation that is deemed deserving of increased leniency. Conversely, of course, this approach also identifies those unworthy of mercy. This division is justified under a cost-benefit logic: while the latter may be legitimately incapacitated given their risk to public safety and social order, the former should be released from prisons to save the high psychological, social and economic costs of incarceration (Richardson and Kutateladze, 2021). Seeds (2017) calls this a strategy of "bifurcation" (see also Gottschalk 2015). Beckett et al. (2016) find a wealth of evidence that most, if not all, governmental efforts to end mass incarceration rely on and reinforce bifurcation. For this reason, Seeds (2017, 608) argues this division is not merely a side-effect of reform, but "an underlying principle and key product of... a comprehensive criminal justice reassessment focused on the role of the prison."

The most commonly used bifurcating category is what (or who) counts as violent (ibid).

This aligns with the broader actuarial turn to preemptively maintain public safety through the predictive incapacitation of populations deemed dangerous or risky (Feeley and Simon, 1992; Floud and Young, 1981; Garland, 2001; Harcourt, 2001). Bifurcation is most commonly directed to categorizing criminal conduct. Generally, non-violent crimes are limited to property offenses like theft, burglary and vandalism, or to victimless vice offenses like sex work or the possession of small amounts of narcotics for personal use.[1] In contrast, violent crimes like robbery, kidnapping, and murder are usually categorically ineligible for bifurcated sentencing reforms. Similarly, any individual who has a documented history of these sort of offenses will be labelled as undeserving of leniency in subsequent interactions with the criminal legal system. In contrast, if (non-violent) criminal acts are understood to be symptomatic of addiction, mental illness and poverty, then an individual may qualify for a non-custodial probationary sanction.

Once a non-violent subpopulation has been identified, legislators enact leniency for the bifurcated group by replacing prison sentences with probationary terms (Beckett and Murakawa, 2012; Miller, 2021). Here, they aim to reduce custodial punishment by using other carceral state institutions capable of intensive, long-term surveillance. A year in prison may thus be replaced by three on probation. Granted, this approach may lower the number of people held in jail or prison. It may also relieve some of the harms of incarceration. After all, surveilled individuals are able to remain in the community and maintain crucial social, labor market and familial ties that are necessarily severed during a custodial sentence (Clear, 2007; Pettit and Western, 2004; Wakefield and Wildeman, 2013; Western, 2002). Rhetorically, this probationary sanction promotes a genre of "therapeutic" techniques of control that mobilize the coercive tools of the criminal legal system in service of economic redistribution and public health (Cohen, 1985; Kohler-Hausmann, 2018; Stuart, 2016).

1. Here, it is interesting to note that, unlike possession, the manufacture and/or delivery of narcotics has been defined as a violent act. For example, the National Association of Assistant United States Attorneys states that "drug trafficking is inherently violent... regardless of whether any individual offender engages in an act of violence during the commission of a drug offense." (National Association of Assistant United States Attorneys, 2015, 18).

4.1.1 Research Questions

In this chapter, my aim is to provide a methodologically robust account of the impact of bifurcated probationary sentencing reform as a solution to mass incarceration. To this end, I evaluate the empirical merits of two major theoretical criticisms that criminal legal scholars have leveled at this common approach to sentencing reform. The first criticism expects prison populations will not be substantially reduced under a bifurcated reform logic. Unfortunately, it is argued, a focus on supposedly non-violent crime cannot alone end mass incarceration. For example, drug possession charges account for only one-fifth of the total incarcerated population in the United States (Sawyer and Wagner, 2020). Since the War on Crime both preceded and intensified the War on Drugs, most scholars agree that mass incarceration will end only when an armistice is declared on both fronts (Gottschalk, 2015; Murakawa, 2014; Pfaff, 2017). That said, an optimist may argue that reforms will gradually expand from drug possession to include more serious crime (Balboni and Grometstein, 2020). The pessimist would counter that, even if those facing 'nonviolent' charges are incarcerated at lower rates, this marginal gain will nonetheless be overwhelmed by "patterns of expansion at punishment's upper end" (Seeds, 2017, 591). Finally, the moderate position would expect incarceration levels to slightly decline as bifurcated reforms "nibble at the edges" of mass incarceration (Tonry, 2016, 253). This debate motivates my first set of research questions: How many people avoid a custodial sentence after sentencing reforms are introduced? Do bifurcated reforms result in an increase in the number of 'violent' defendants sentenced to prison, or the predicted length of their custodial sentence?

A second criticism of this sentencing reform insists that the harms of mass incarceration cannot be entirely mitigated by non-custodial sanctions like probation. Of course, if individuals fail to meet compliance requirements, they will nonetheless experience custodial terms for probation violations (Kingsnorth et al., 2002; Phelps, 2013). This will not only maintain incarcerated populations, but may also result in a net increase in the length of punishment for those initially offered probation. Further, probation itself has been documented

to produce harms not dissimilar to those of a custodial sanction (Beckett and Murakawa, 2012; Miller, 2021). For example, meeting one's probation officer may chronically disrupt employment responsibilities and family obligations. Debasement rituals like urine testing and ankle bracelets may cause shame and embarrassment (Miller, 2021). In turn, if probationers experience bias or disrespect, they may lose faith in the legitimacy of not only the law itself, but also non-punitive (even beneficial) surveilling state institutions (Brayne, 2014; Goffman, 2014; Tyler, 2006). As Beckett (2018, 236) explains, then, penal reforms should not be limited only to reducing formal incarceration, but must also consider "those who are stopped, frisked, arrested, fined, and surveilled." My second set of research questions address these concerns. Does bifurcated sentencing reform increase the length of non-custodial probationary surveillance? Relatedly, does the risk of probationary failure increase under bifurcated sentencing reform? For first-time gun offenders, do probation violations result in a comparatively longer custodial term? Do probationary failures produce a longer combined punishment term?

4.2 Case, Data and Methods

I operationalize bifurcated probation using the Safe Neighborhoods Reform Act (Illinois General Assembly, 2018). This bill was first introduced in the Illinois Senate in February 2017 (100th Illinois General Assembly, 2017). It passed both legislative chambers on May 31st of that year, and was signed by the governor on June 23rd, 2017 (ibid). January 2018 was the date when the code of corrections would be formally amended by the new law. This act bifurcates subpopulations convicted of gun possession based on their criminal history. On one hand, those convicted of illegal gun possession who have previously been convicted of a qualifying felony offense face a higher sentencing range with a minimum of 7 and a maximum of 14 years in prison (Unified Code of Corrections, 1972, 4.5-110). Those not previously convicted of a felony face a sentencing range of between 6 to 7 years. This range is not wholly mandatory, permitting a judicial departure with reference to several mitigating

factors (ibid.).

On the other hand, though, this act also establishes a pilot probationary program for "first-time, non-violent offenders" who plead guilty to felony-level gun possession (Unified Code of Corrections, 1972, 6-3.6a, c). The rationale for this program is to "promote public safety, reduce recidivism, and conserve valuable resources" by diverting "young adults" raised in "areas of high crime or poverty" from prison (ibid., 6-3.6a) Interestingly, the General Assembly notes that these individuals "may have experienced trauma that contributes to poor decision making skills" (ibid.). Participants cannot be over 21 years old, and may not be charged with an act deemed violent related to the same incident. For example, if the first-time defendant is alleged to have discharged the firearm, or is accused of threatening harm, then a probationary sentence is not permitted (ibid. 6-3.6b1, 5). The prosecutor must consent to the probationary term, which must be at least 18 months but no more than 24 months. In the program, probationers must submit to drug tests via blood or urine; perform community service; maintain education or employment; attend counseling sessions; and maintain ongoing contact with the court. Electronic monitoring and psychiatric treatment may also be required.

Methodologically, the chapter follows a parametric regression discontinuity approach similar to that previously described in Section 3.2.2, above. Unlike these analyses, however, here I use only a parametric approach. I set an intervention cut-off point at January 1st, 2018. All outcomes are assigned as 'treated' by bifurcated sentencing reforms if they occur during or after 2018. Again, any observed discontinuity in the conditional expectation of punishment outcomes at the cut-off point is taken as evidence of the causal effect of the new law. Similar disposition covariates are included in parametric models, including the length of the case in days and whether the defendant went to trial. Seasonal autocorrelation is tested with the Durbin-Watson statistic and, if necessary, biased standard errors are adjusted using the Prais-Winston generalized least squares estimator. I model the length of probation or prison sentence using a segmented zero-inflated negative binomial model.

Table 4.1 summarizes select covariates by criminal history group using the Cook County State's Attorney's Office felony courtroom event and sentencing dataset from 2011 until 2019. These data have a total of 102 observed months with 78 months pre- and 24 months post-intervention. The total defendant $n = 6,648$ for first-time gun possession and $n = 6,973$ for recidivist charges. I exclude all gun cases with a simultaneous charge that would be understood as violent (e.g. firearm discharge, robbery, kidnapping or carjacking etc.).

	First-Time		Repeat	
	Pre	*Post*	*Pre*	*Post*
Months n	78	24	78	24
Defendant n	4,355	2,293	4,839	2,134
Mean Age	23	25	27	28
% Male	96	93	98	98
% Black	77	77	86	78
Mean Charges	5.5	5.1	6.2	7.0
% CPD Arrest	84	82	86	84
% Trial	7	5	11	6
Mean Case Length (days)	289	276	298	379

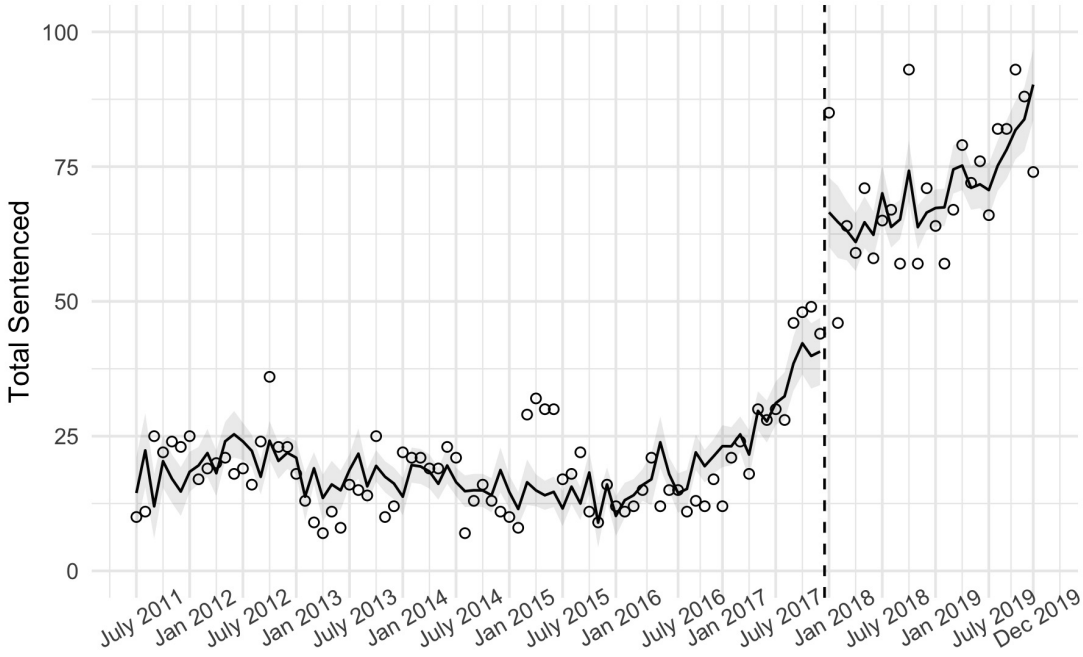
Table 4.1: Data summary by gun possession criminal history and select covariates.

Source: 2011-2019 Cook County State's Attorney administrative records.

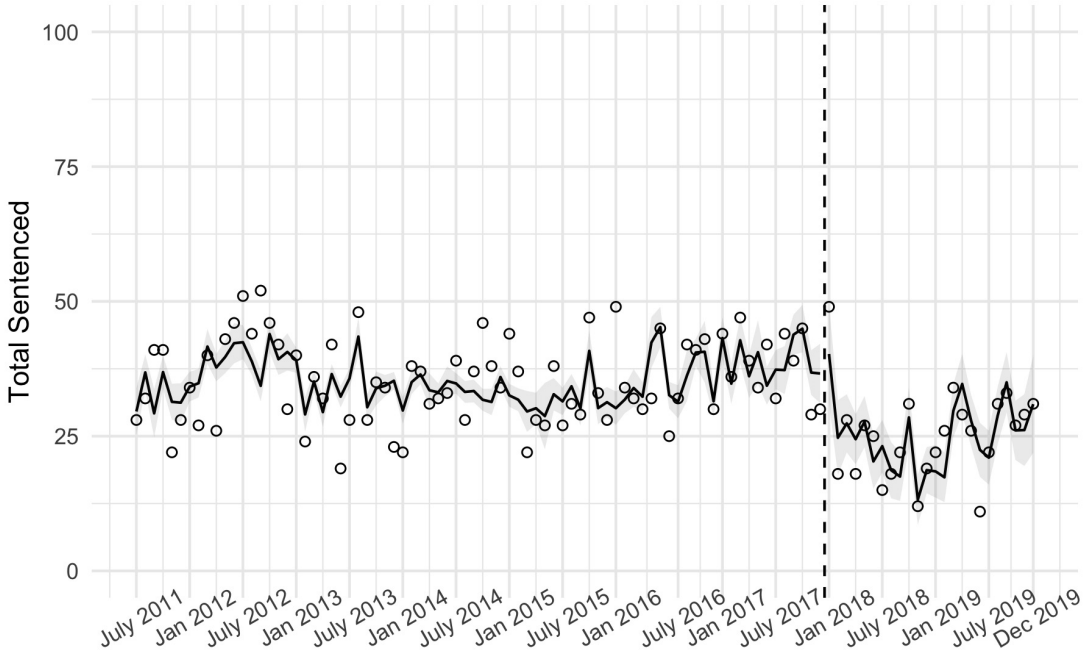
4.3 Preliminary Results

Figure 4.1A offers evidence that bifurcated probation did indeed result in an immediate and significant increase of approximately 24 additional first-time gun possession defendants sentenced to probation ($s.e. = 4.0$, $p < 0.01$, $R^2 = 0.90$). That said, the new law appears

to follow and intensify an existing pre-intervention upward trend in the use of probationary sanctions for first-time gun possession. There is evidence of autocorrelation with a Durbin-Watson test statistic of 1.6 ($p = 0.02$), but after the Prais-Winston transformation the intervention effect remains significant and is estimated to increase to just under 28 ($s.e. = 5.1, p < 0.01$, $R^2 = 0.83$). In contrast, there is no significant immediate treatment effect observed for prison sentences ($p = 0.9$, $R^2 = 0.50$). Over time, however, there does appear to be fewer first-time defendants being sentenced to prison terms. Here, non-parametric local linear methods will be useful to explore if this intervention gains significance with a more generous bandwidth, as well as any month-on-month decrease in prison terms for first-time offenders.



(a) Total probationary sentences pre- and post-intervention.

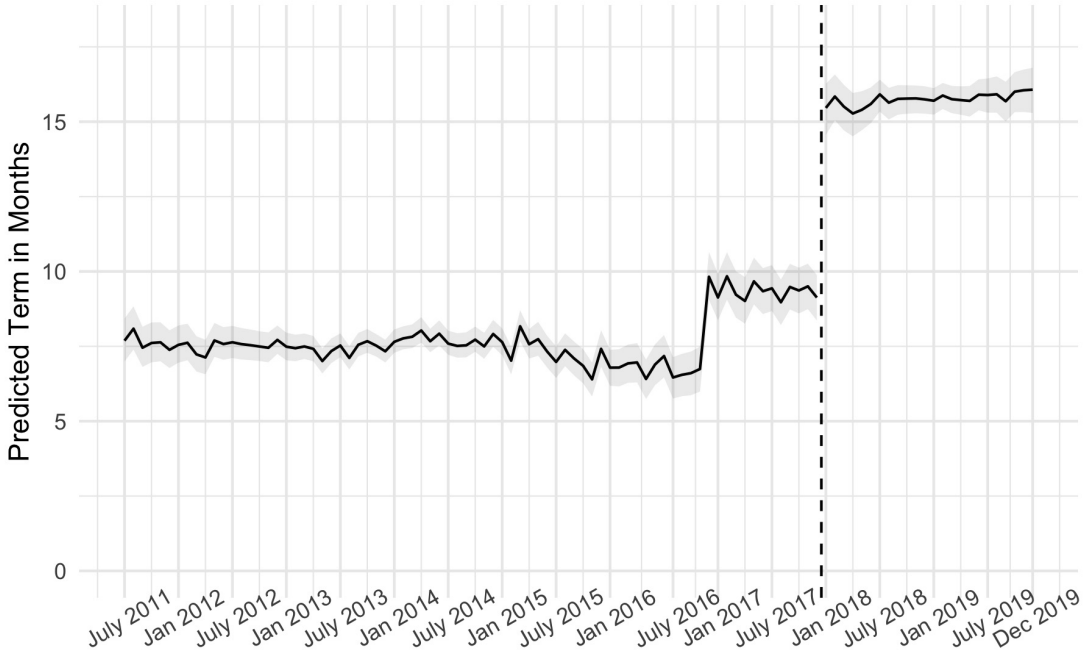


(b) Total prison sentences pre- and post-intervention.

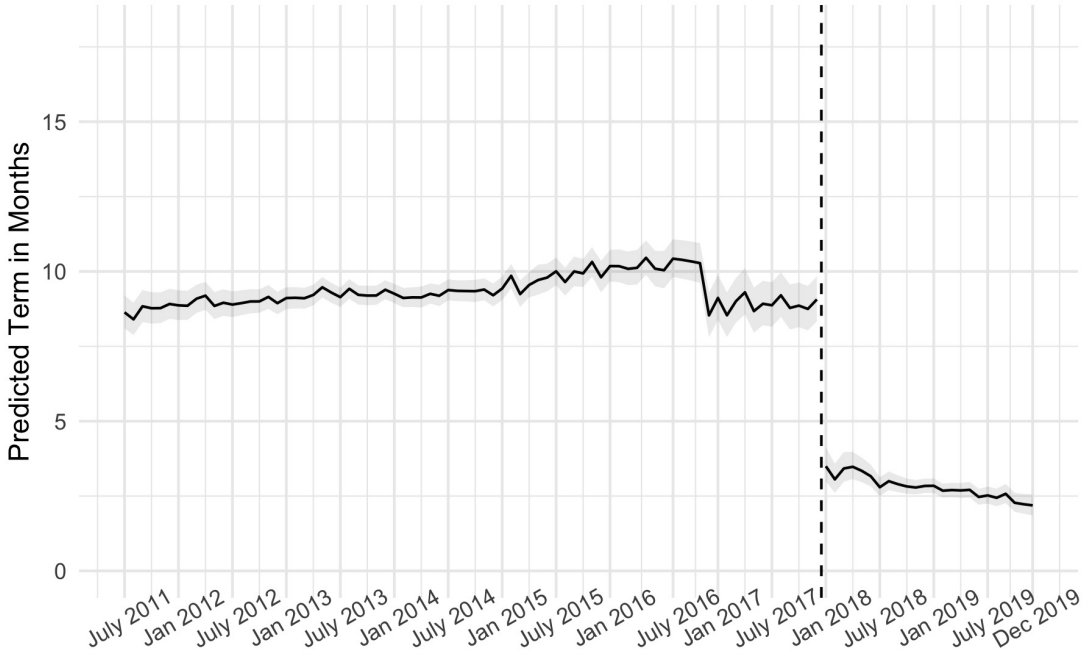
Figure 4.1: Segmented linear model predictions fitted to monthly gun possession sentencing outcomes for first-time offenders.

Source: 2011-2019 Cook County State's Attorney administrative records.

Figure 4.2 illustrates the predicted length of both probationary and prison terms for the same first-time offenders. Here, the sentencing legislation is observed to have an immediate yet opposite impact on the predicted length of both types of punishment. While the predicted probationary term for first-time gun offenders is predicted to increase from 9 months (c.i. 8.4-9.9) in December 2017 to over 15 months (c.i. 14.6-16.3) in January 2018 (Figure 4.2A), the predicted length of prison terms falls from 9 (c.i. 8.3-9.8) to over 3 months (c.i. 3.0-4.1) during the same period (Figure 4.2B).



(a) Predicted length of probationary sentence.



(b) Predicted length of prison sentence.

Figure 4.2: Segmented negative binomial model predictions of punishment term for first-time gun offenders.

Source: 2011-2019 Cook County State's Attorney administrative records.

Both subfigures in 4.2 also illustrate consistent discontinuities of a smaller magnitude after the change in managerial oversight of prosecutorial plea offers that occurred after State's Attorney Kim Foxx took office in December 2016. As with sentencing legislation, this organizational change is observed to have both an immediate and opposite impact on the predicted length of both types of punishment. The probationary term for first-time gun offenders is predicted to increase from just over 6 months (c.i. 5.6 - 6.9) in November 2016 to over 9 months (c.i. 8.4 - 9.9) in December 2016 (Figure 4.2A). Conversely, during the same administrative transition period, the predicted length of a prison term falls slightly from over 9 months (c.i. 9.0 - 10.2) to just under 8 months (c.i. 7.1 - 8.6) for the same subgroup with no previous serious criminal history (Figure 4.2B).

For repeat gun offenders, the intervention has no significant impact on the predicted monthly occurrence of either punishment type, as displayed in Figure 4.3. In terms of prison sentence length, Figure 4.4 suggests that the more salient change over time was the internal strengthening of plea bargaining discretion under the Foxx administration in the final months of 2016. Specifically, the predicted prison sentence dropped by nearly two months from 14 (c.i. 12.6 - 15.2) in November 2016 to 12 (c.i. 10.6 - 13.2) in December of the same year. In contrast, for the focal sentencing intervention, Figure 4.4 offers a puzzling, and counterintuitive, result: here, it appears that the predicted prison term of recidivist defendants was not significantly impacted by a sentencing intervention intended to *increase* the length of their punishment (pre-intervention 12.3 months, c.i. 11.0 - 13.5; post-intervention 11.2 months, c.i. 9.7 - 12.6).

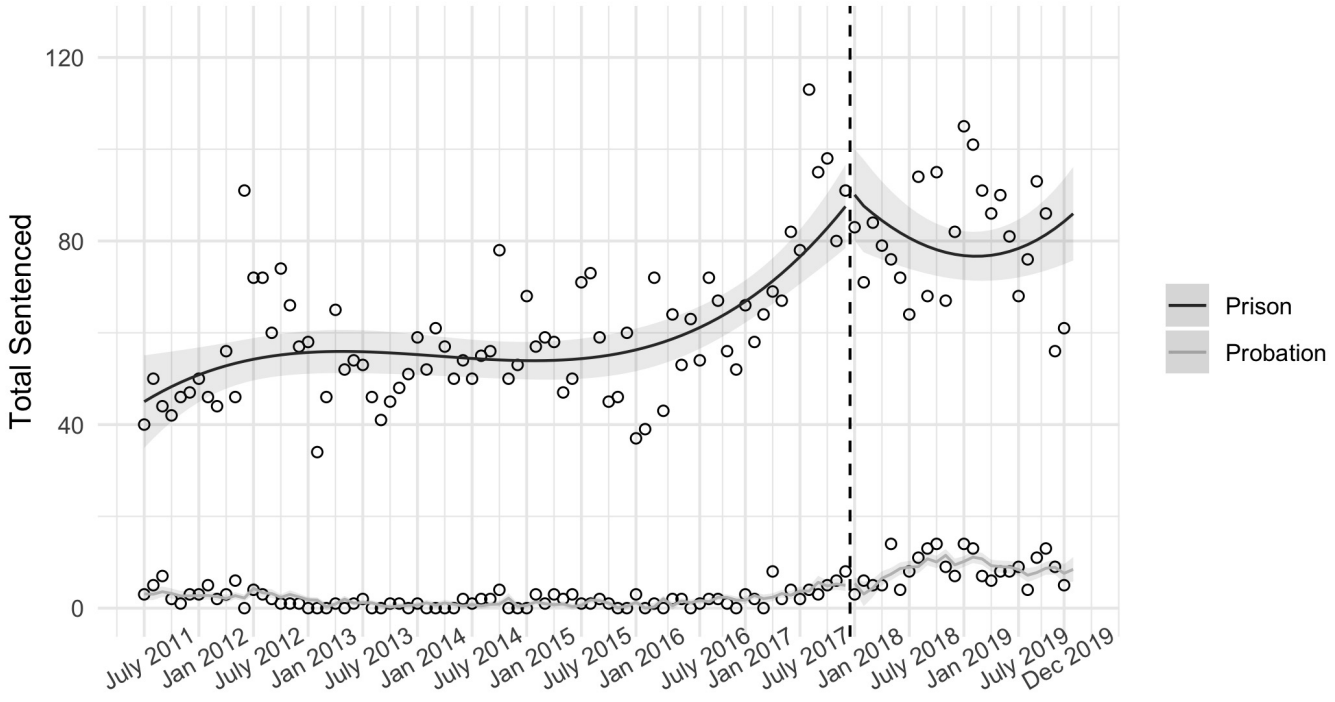


Figure 4.3: Segmented linear model predictions fitted to monthly gun possession sentencing outcomes for repeat offenders.

Source: 2011-2019 Cook County State's Attorney administrative records.

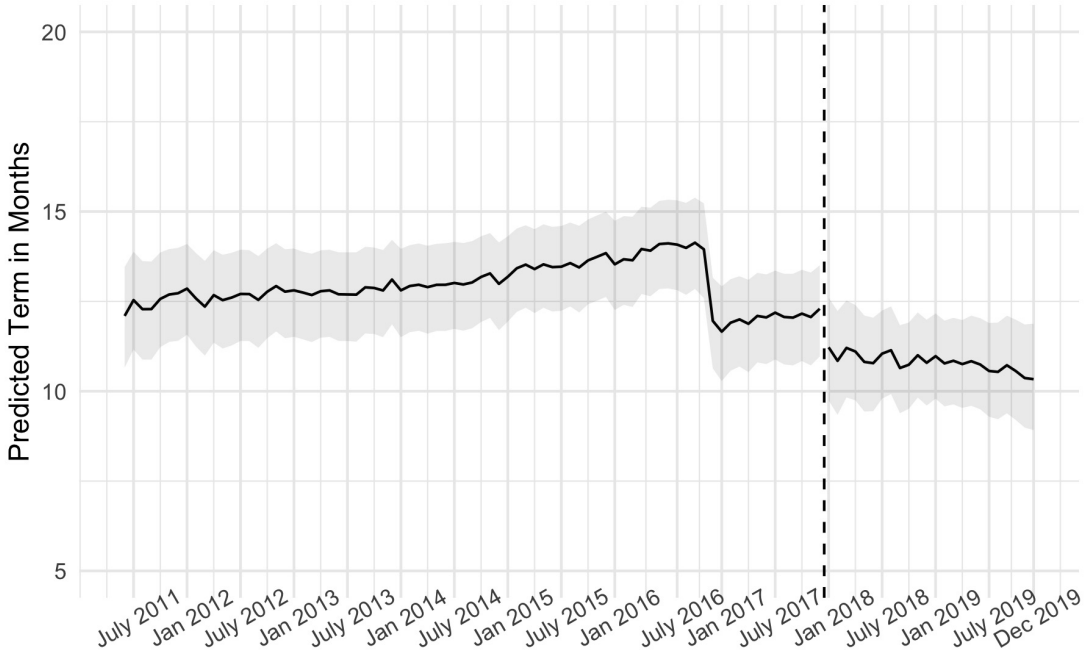


Figure 4.4: Segmented negative binomial model predictions of prison term for repeat gun offenders.

Source: 2011-2019 Cook County State's Attorney administrative records.

4.4 Preliminary Discussion and Future Research

The above analysis offers preliminary evidence to test the merits of two theoretical criticisms of bifurcated probation. First, there is no clear evidence of a substantial number of first-time gun possession defendants who avoided prison because of the new law. This suggests some empirical merit to the moderate concern that, at best, bifurcated reforms only "nibble at the edges" of mass incarceration (Tonry, 2016, 253). To confirm this null finding, in future research I will test for month-on-month reduced prison sentences using non-parametric local linear methods. That said, I also found no evidence to support the pessimistic claim of that bifurcated reforms will hasten "patterns of expansion at punishment's upper end" (Seeds, 2017, 591). Instead, bifurcated statutory reforms had no observed impact on the number

of 'violent' defendants sentenced to prison, nor on the predicted length of their custodial sentence.

The second criticism notes that probationary sentencing reforms work to expand the reach of carceral state control beyond prison walls. I did find that this reform was predicted to immediately increase the number of first-time defendants sentenced to probation from December 2017 to January 2018. Since there was no proportional reduction in the number of defendants sentenced to prison, this may suggest that the prosecutor's plea bargaining power has in fact been heightened by their new ability to offer more lenient probationary sanctions. Perhaps now, instead of dropping charges, the prosecutor can win a tenuous case by offering the defendant first-time gun offender probation in lieu of prison. The predicted length of probationary terms was also immediately increased by the new legislative reforms. This means that individuals must live under cumbersome probationary surveillance for longer periods of time.

This result is no doubt worrying for those critical of the non-custodial expansion of carceral surveillance. Individuals faced with longer probationary terms must endure more severe and long-lasting social, economic and psychological harms of probation. Relatedly, longer probationary terms may produce more individuals who are incarcerated after violating their probationary requirements. This will not only maintain incarcerated populations, but could also result in a net increase in the length of punishment for those initially offered probation. Moving forward, then, I intend to use segmented parametric survival analysis to test whether bifurcated probation increases the risk of probationary failure for first-time gun offenders.

Finally, these analyses also revealed an earlier, unexpected discontinuity in the predicted length of both types of punishment. This is likely because, after being inaugurated in December 2016, State's Attorney's Kim Foxx instituted a new policy that allowed prosecutors to offer a plea deal without the prior approval of their unit supervisor. Individual prosecutors were therefore no longer subject to routine managerial oversight of the plea bargaining

negotiations in their courtrooms. As with legislative reform, this new organizational policy increased the length of probationary sentencing for first-time gun possession while decreasing the predicted length of prison terms for first-time gun offenders. That said, the external legislative intervention appears to have produced a greater magnitude of change for first-time sentencing outcomes then that caused by internal, administrative change at the prosecutor's office. This was not the case with recidivist sentencing, however; here, the impact of increased prosecutorial discretion is the only immediate, significant driver of reduced custodial sentences. Although the preceding chapter has been primarily focused on legislative change, then, future research must further develop the practical and theoretical implications of this unexpected two-fold discontinuity. In future drafts, I intend to exploit this unique opportunity to quantify the relative effect of internal, organizational factors versus external, legislative factors in sentencing outcomes in Cook County, Illinois.

CHAPTER 5

CONCLUSION

In this project, I have empirically investigated how incarceration has shifted (or endured) during a remarkable decade of change in Cook County, Illinois. Broadly, this work used novel data to enrich ongoing debates about how criminal legal practice should be restructured in order to end mass incarceration in the United States. Each of the three chapters focused on the policies and practices of key institutional actors in the local criminal legal system. Though the findings are specific to this county, I aimed to build a template for approaching similar sources of quantitative evidence in other jurisdictions that can be used to make inferences about how police, prosecutors and legislatures may maintain (or disrupt) the incarcerative status quo.

The first chapter asked how prosecutors handle the influx of misdemeanor cases stemming from proactive, public order policing. Specifically, it focused on the length of pretrial detention for misdemeanor broken windows arrests. Analyzing 2016 jail data using complementary competing risk models, I found over half of Black public order defendants to be released on dropped charges after spending two weeks in pretrial detention. From this, I argued that police and prosecutors are neither deliberate adjudicators nor fast assembly line producers of convictions. Instead, there is evidence that these two law enforcement actors work together to use the jail as an unregulated tool of management and control against Black populations they deem risky, disorderly or disreputable.

The second chapter evaluated the decarcerative effects of recent, dramatic shifts in felony charging practice in this jurisdiction. In December 2016, newly-elected Cook County State's Attorney Kim Foxx raised the threshold value defining felony retail theft from $300 to $1,000. Under the new policy, her administration more than halved the number of felony retail theft charges filed in the county by January 2017. I used this sudden drop in felony cases to build a regression discontinuity design that quantifies the impact of charging reform on crime, reporting, arrests, pretrial detention, adjudication and punishment. Here, I employed both

parametric segmented and non-parametric local linear modeling strategies as well as falsification tests designed to isolate the specific intervention from contemporaneous administrative change.

I found evidence that the felony charging reform impacted crime, arrests, adjudication and sentencing practices. In terms of crime, there was a slight increase in the slope of retail theft incidents reported to the Chicago Police Department. In terms of arrests, felonies decreased alongside a proportional increase in their misdemeanor counterparts. That said, the shift from felony to misdemeanor arrests did successfully reduce the number of defendants booked into Cook County Jail. This is likely because police may directly release misdemeanor (but not felony) defendants without a judicial bond hearing. In terms of adjudication, State's Attorney administrative data show that prosecutors maintained pre-intervention drop rates even when faced with dramatically fewer cases. Finally, in terms of sentencing, both punishment type and length remain broadly comparable to pre-intervention levels, with a small observed drop in the length of probationary terms. Overall, I argue that felony charging reforms in Cook County offers a cautiously optimistic case study in the path to ending mass incarceration.

In the third and final chapter, I considered the decarcerative impact of recent legislative sentencing reform. I introduced the term 'bifurcated probation' as the typical decarcerative strategy enacted by statutory change. I operationalized this term using the example of the Safe Neighborhoods Reform Act, passed by the Illinois legislature in January 2018. This new law bifurcates subpopulations convicted of gun possession based on their criminal history; only qualifying first-time defendants may be offered probation. Harsher prison sentences are authorized for those convicted of repeat gun possession offenses. I found this legislative reform to have intensified a pre-intervention upward trend in the use of probationary sanctions for first-time gun possession. It also increased the length of probationary terms served by this subgroup, while decreasing their predicted prison terms. Surprisingly, though, this reform did not have its intended impact of increasing the predicted punishment term for gun

possession recidivists sentenced to prison. Future research will supplement initial parametric results with non-parametric methods. I also intend to use segmented parametric survival analysis to test whether bifurcated probation increases the risk of probationary failure for first-time gun offenders. After all, it is plausible that this sentencing reform may in fact produce a comparatively longer custodial term for those first-time probationers who violate the extensive surveillance requirements attached to their initial sanction.

This final analysis also revealed an earlier, unexpected discontinuity in the predicted length of both types of punishment from November to December 2016. At this time— two years before the focal legislative intervention— the length of probationary sentencing for first-time gun possession suddenly increased while the predicted length of prison terms decreased for both subpopulations. I attribute this discontinuity to a new management policy adopted by the incoming State's Attorney's Kim Foxx. Under her newly-elected administration, individual prosecutors were no longer subject to routine managerial oversight of the plea bargaining negotiations in their courtrooms. This unexpected two-fold discontinuity thus offers exciting opportunities for future research that quantifies the relative effect of internal, organizational factors versus external, legislative factors in sentencing outcomes.

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