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Pretrial Policy Change and Place:  
Evaluating the Impacts of Bail Reform in Maryland

DISSERTATION

submitted in partial satisfaction of the requirements  
for the degree of

DOCTOR OF PHILOSOPHY

in Criminology, Law and Society

by

Rebecca Tublitz

Dissertation Committee:  
Professor Charis Kubrin, Chair  
Professor Emily Owens  
Associate Professor Bryan Sykes

2025



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Criminology, Law & Society; Criminal Justice Reform; Courts and Corrections

## PUBLICATIONS

- Tublitz, R., Reiter, K., Augustine, D., Barragan, M., Chesnut, K., Gonzalez, G., Pifer, N., & Strong, J. Getting Boxed In: How Race and Gang Labeling Shape Solitary Confinement Use. (2025). *Punishment & Society*, 0(0).
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# **ABSTRACT OF THE DISSERTATION**

Pretrial Policy Change and Place:

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by

Rebecca Tublitz

Doctor of Philosophy in Criminology, Law and Society

University of California, Irvine, 2025

Professor Charis Kubrin, Chair

Despite the high-profile status of pretrial bail reform in criminal justice policy discourse, empirical evidence of its impacts remains remarkably sparse—generating uncertainty for policymakers pursuing effective and equitable system reform. Across three studies, this dissertation examines Maryland’s statewide bail reform to evaluate its effects on judicial decision-making and public safety, and to investigate how local court contexts shape pretrial outcomes. In the first study, I employ a regression discontinuity analysis on administrative court records to evaluate how bail reform affects judicial decision-making at the pretrial stage. In the second study, I draw on state-level longitudinal crime data to investigate the impact of bail reform on crime using a synthetic control group approach. In the final study, I integrate administrative court records with data on the organizational, social, and economic contexts of courts in Maryland to describe how pretrial decision-making varies across court jurisdictions and to explore how the social contexts of courts shape responses to new law. This

dissertation contributes to an ongoing discourse about the effects and limits of bail reform, and sheds light on the contextual contributors to local variation in pretrial practice.

## INTRODUCTION

Each year, millions of people in the United States come into contact with the criminal justice system. In 2023, local jails registered 7.6 million admissions nationwide (Zeng, 2025). Once arrested, individuals may maintain their liberty during the case process through money bail or other forms of non-monetary pretrial release. Defendants generally make an initial appearance in front of a judge, who determines whether and under what conditions to offer release pretrial. Judges wield considerable discretion at this stage, weighing factors such as public safety and likelihood of appearance at trial (Albonetti et al., 1989; Goldkamp, 1985). Though dated, available national data show that two-thirds of felony defendants in large urban jurisdictions were released pretrial in 2009, while 38 percent were detained until disposition—rates that have not changed substantially over the prior 30 years (Reaves, 2013). This apparent stability belies substantial change in the scale of pretrial detention across the United States: the size of pretrial population increased by nearly 470 percent between 1970 and 2023, when just under half a million people were detained pretrial in jails on any given day (Digard & Swavola, 2019; Zeng, 2025). Pretrial detainees also constitute a rising proportion of jail populations, increasing from 54 percent in 1990 to over 70 percent of people in jail in 2023 (Zeng 2025).

Evidence suggests that pretrial release outcomes are consequential for defendants: compared with those released pending trial, pretrial detainees are more likely to be prosecuted (Kellough & Wortley, 2002), plead guilty (Petersen, 2019), be convicted (Dobbie et al., 2018; Gupta et al., 2016), be incarcerated (Heaton et al., 2017; Oleson et al., 2016; Williams, 2003), and receive lengthier sentences (Sacks &

Ackerman, 2014; Stevenson, 2018b). These consequences are disproportionately borne by Black and Latino defendants, and by those unable to afford money bail (Demuth, 2003; Reaves, 2013; Sacks et al., 2015; Schlesinger, 2005). Not only do defendants often experience bail and pretrial detention as punishment (Dill, 1975; Feeley, 1979), scholars argue that pretrial systems' reliance on money bail contributes to social and economic inequalities (Scott-Hayward & Fradella, 2019; Page & Soss, 2021).

While injustices in the pretrial system have been a target of researchers and reformers since the early twentieth century (e.g., Beeley, 1927), calls to reform pretrial practices have grown dramatically in recent years. Nearly every state, and hundreds of local jurisdictions, have explored changes to law and policy governing pretrial practices (Doyle et al., 2019). Jurisdictions across the United States have enacted a variety of changes to their pretrial systems, including: modifying judicial guidelines on factors considered during the pretrial determination; implementing risk assessment tools; expanding access to counsel at pretrial hearings; increasing pretrial supervision and services; and, less commonly, limiting or abolishing money bail for certain offenses (Jorgensen & Smith, 2021). Not all pretrial reforms share the goal of limiting the use of money bail; however, efforts to move away from money-based pretrial systems have resulted in several high-profile statewide reforms, including in Maryland, New York, New Jersey, and Illinois.

The impacts of reforms that reduce or eliminate the use of money bail are not well-understood. With some exceptions, little peer-reviewed research examines how changes in policy and law governing the pretrial stage of the criminal process impact

pretrial practice or crime outcomes (Jorgensen & Smith, 2021; Stevenson, 2018a). This omission has generated uncertainty over the impacts of bail reform: opponents of reform assert that increases in crime can be linked to current bail reform efforts, while proponents argue that reforms achieve their aims with few adverse effects (Treadway, 2022). A small set of studies demonstrate that bail reform may increase pretrial release without financial conditions with negligible impact on court appearances or subsequent crime (Ouss & Stevenson, 2023; Stemen & Olson, 2020; Wu & McDowall, 2024; Zhou et al., 2023).

Generally, studies of bail reform examine local jurisdictions or measure only statewide impacts. How pretrial practices vary across jurisdictions and whether the impacts of bail reform vary across organizations responsible for implementing it, is less studied. This omission is important: statewide (or federal) criminal justice policy is put into action at local levels and wide variation across localities is observed even where formal law is the same (Lynch, 2011). Scholars have long documented significant differences in other criminal justice practices within and across states and local jurisdictions, including in prosecution (Lynch, 2016; Spohn & Fornango, 2009), sentencing (Engen & Steen, 2000; Ulmer, 1997), and community supervision (Lin et al., 2010). Scholars find that local variation in court outcomes is influenced by organizations and people responsible for implementing law, as well as the social, economic, and political contexts in which they are embedded (Eisenstein et al., 1988; Ulmer, 1997; Ulmer & Johnson, 2004).

Organizational and social contexts matter for policy change: when new policies or laws are enacted, they are put into action by actors embedded in those

organizations. Policy enacted on the frontlines of organizations is shaped not only by the requirements of new law, but also by local norms and established practices.

Research finds that moments of policy transition often result in gaps between formal law and its implementation on-the-ground (Grattet & Jenness, 2005; Lynch, 1998; Lynch & Omori, 2014; Rubin, 2021; Verma, 2015).

Local variation in policy and practice may lead to “justice by geography,” where those who encounter justice systems experience unequal treatment across place (Feld, 1991). In the pretrial context, place-based factors may influence how pretrial decisions are made: nonfinancial release rates and bail amounts vary widely across urban jurisdictions, beyond what is accounted for by case or criminal history characteristics (Hood & Schneider, 2019). Moreover, local policy choices, such as fixed bail schedules, and factors local to the justice system, like jail crowding, may also influence bail setting and pretrial detention (D’Alessio & Stolzenberg, 2002; Hood & Schneider, 2019; Pinchevsky & Steiner, 2016; Williams, 2016; Wooldredge et al., 2017).

Broadly, this dissertation responds to several gaps in the pretrial research field, contributing policy-relevant knowledge on the impacts of bail reform on pretrial decision-making and its effect on crime, and exploring how pretrial practices vary across jurisdictions and in response to reform. In this dissertation, I use the State of Maryland’s 2017 bail reform as a case study to explore questions about the effects and limits of bail reform, and to examine how local contexts shape responses to new law. This dissertation has several guiding research questions:

- 1) What is the impact of bail reform on judicial decision-making at the pretrial stage?
- 2) What is the impact of bail reform on crime?

- 3) How does judicial pretrial decision-making vary across counties in Maryland, both before and after bail reform?
- 4) To what degree is jurisdictional variation in pretrial decision-making influenced by court organizational characteristics and economic, demographic, and political contexts of courts?

Maryland's statewide bail reform provides a unique opportunity to examine these research questions. Maryland is among a handful of states that have undertaken major reforms to their pretrial procedures in recent years. Its bail reform was a high-profile effort to move Maryland's pretrial system away from reliance on money bail and to remake the pretrial process for all criminal defendants. This court-led reform rewrote the procedural rules governing how bail conditions are set, barring judges and court commissioners from setting unaffordable bail; however, it did not expressly outlaw the use of money bail. This reality provides a unique context under which to examine both the effects of bail reform and to examine variation in court responses to statewide policy change. This dissertation aims to contribute to several literatures on pretrial decision-making, the impacts of bail reform, and local variation in criminal justice practice. First, I provide empirical evidence on the impacts of bail reforms, examining bail reform's effects on pretrial decisions and on statewide crime. Second, I contribute to an emerging literature on the influence of court organizational features and contextual influences on judicial decision-making at the pretrial stage, and add to the literature on local variation in implementation of new law. Finally, I expand the types of cases and the jurisdictional contexts examined in relation to pretrial research—focusing on the state of Maryland, incorporating multiple case types, and examining the influence of a

range of demographic, economic, political, and organizational factors on pretrial decision-making.

### ***Outline of the Dissertation***

The dissertation is organized in the following manner: In **Chapter 1**, I lay the groundwork for three empirical studies of bail reform. I provide a brief primer on the pretrial process in the United States, and introduce the theoretical frameworks that ground this dissertation. I also review the research on the correlates and consequences of pretrial detention and release. Next, I turn to the current bail reform movement and introduce Maryland's bail reform, reviewing the development and rollout of the reform and detailing facets of the state policy environment. Finally, I outline the main data sources used to address the primary research questions posed by this dissertation. **Chapter 2** presents the first empirical study of this dissertation, examining the effects of bail reform on judicial decision-making at the pretrial stage. First, I discuss prior empirical research on the effects of bail reform, before presenting findings on the impacts of Maryland's bail reform on money bail setting, bail amount, nonfinancial release, and preventative detention using a regression discontinuity approach. The second empirical study is presented in **Chapter 3**, where I estimate the effects of bail reform on crime in Maryland. In that chapter, I discuss the theoretical mechanisms through which pretrial detention and release may influence crime. I then estimate the effects of bail reform on statewide crime in Maryland using a synthetic control group approach. The final empirical study of this dissertation is presented in **Chapter 4**, where I examine variation in pretrial decision-making across counties in Maryland. I ground this analysis in both focal concerns and courts-as-communities frameworks—two

complementary theoretical approaches used to describe court decision-making and explain variation in court outcomes. In this chapter, I use multilevel modeling to examine variation across counties in pretrial decisions, and examine how organizational and contextual factors of counties influence pretrial decisions and shape responses to bail reform. In the **Conclusion**, I review the empirical findings of these studies, and discuss the theoretical and policy-relevant contributions of this dissertation.

## **CHAPTER 1: PRETRIAL, COURTS, AND BAIL REFORM**

In this chapter, I lay the contextual groundwork for the three empirical studies of the dissertation. This chapter is organized into three main sections. In the first section, I provide an overview of the pretrial process in the United States, and introduce the theoretical frameworks that guide the questions and empirical studies in this dissertation. I then review extant knowledge on factors that influence pretrial decisions and outcomes, and the consequences of pretrial detention. In the second section, I outline the modern bail reform movement, situating Maryland's bail reform within the current context of pretrial justice reform. I then provide an overview of Maryland's pretrial process, outlining the steps from arrest to the pretrial determination, and detailing the key changes to pretrial procedure from the 2017 reform. Finally, in the last section, I provide an overview of the sources of data used in this dissertation.

### ***Part I. Pretrial Justice, Theory, and Prior Research***

#### **Overview of Pretrial Process in the United States**

In general, most offenses apart from homicide are eligible for some type of pretrial release (Scott-Hayward & Fradella, 2019). The pretrial process in the United States varies across jurisdiction and by offense severity, however, in most places, the judge (or bail magistrate or commissioner, depending on the jurisdiction) is the primary decision-maker at the pretrial stage. Following a custodial arrest, judges have an array of options for releasing defendants prior to trial, which vary along a continuum of restrictive conditions. At the least restrictive end, defendants may be “released on own recognizance” (ROR), without payment or condition, needing only to appear at their court hearings. Most often, defendants are offered a form of conditional release. The

most common condition of release is money bail (or financial release), where defendants may secure release from custody by paying money bail or bond directly to the court or through a commercial bail bond intermediary. Less commonly, defendants may be offered “unsecured bond,” which does not require a financial payment to be posted in order to be released. Payment is forfeited only if the defendant later fails to appear for their court hearings. Judges may impose an array of other conditions, such as drug testing, electronic monitoring, or pretrial supervision. Finally, on the most restrictive end of the spectrum, judges may deny bail altogether and hold defendants in jail custody until their case is disposed (“preventative detention”) (Scott-Hayward & Fradella, 2019).

National data suggest that preventative detention is relatively rare in felony cases: between 1990 and 2004, of those who were detained pretrial, just 1 in 6 defendants were denied bail, and in 2009, just 1 in 10 detained defendants were denied bail (Reaves, 2013). About 9 in 10 detained defendants were held in custody on a set bail amount, suggesting that for the majority of felony defendants held pretrial, the bail amount was not affordable. Among those released, the use of financial release conditions increased across this period, from 37 percent in 1990 to 61 percent in 2009. Nonfinancial releases were less common: just 2 in 5 felony defendants in 2009 were released without having to post financial collateral (Reaves, 2013).

### **Models of the Pretrial Decision**

Decision-making at the pretrial hearing is a multi-stage process for the decision-maker: judges first determine whether to release a defendant on ROR; if not, judges then determine whether to preventatively detain a defendant before, finally, setting a

bail amount or other condition of release (Frazier, Bock and Henrietta, 1980; Goldkamp & Gottfredson, 1979). However, the judge's decision is not the sole determinant of whether a defendant is released from custody. Most scholars view the pretrial process as having two distinct dimensions: (i) pretrial release *decisions*, such as the decision to detain, offer money bail or bond, or set the bail amount, and (ii) pretrial release *outcomes* for the defendant, namely the ability of a defendant to secure release on money bail or bond. Factors influencing a judicial decision to offer release may not explain a defendant's ability to obtain release (Demuth, 2003). Pretrial research generally examines a range of dependent variables within these two dimensions (e.g., Ball & Bostaph, 2009; Demuth, 2003; Demuth & Steffensmeier, 2004; Pinchevsky & Steiner, 2016; Schlesinger, 2005).

## **Theoretical Frameworks**

This dissertation draws from several complementary theoretical approaches to frame the study of pretrial decision-making and variation across courts, including focal concerns, courts-as-communities, and sociolegal perspectives on how formal law is interpreted and applied. I introduce these theoretical perspectives below, and discuss relevant perspectives at greater length in the empirical chapters.

### *Focal Concerns Theory*

The “focal concerns” theory is the predominant theoretical perspective applied to explain variation in court outcomes across race, gender, age, and other defendant characteristics (Lynch, 2019). While originally conceived to explain variation in sentencing outcomes, focal concerns theory is frequently used in pretrial research (e.g., Demuth, 2003; Schlesinger, 2005). This perspective argues that court decision-makers

are primarily concerned with three substantive considerations when making sentencing—and by extension, pretrial—determinations: blameworthiness, protection of the community, and practical considerations (Steffensmeier et al., 1998). Here, blameworthiness is linked with perceptions of a defendant’s culpability; protection of the community emphasizes concern over reducing risk of recidivism or risk to public safety; finally, practical constraints incorporate a range of considerations such as a defendant’s mental or physical health, disruption to ties to family, courtroom working relationships, efficient case flow, or use of scarce correctional resources (Steffensmeier et al., 1998).

Particularly at the initial stages of criminal case processing, judicial decision-making is characterized by both incomplete information on the facts of a case and defendants and by limited resources and time to make early stage decisions. In these conditions, judges employ “perceptual shorthands” to make determinations (e.g., who is a risk to public safety if released), relying on racial (or age and gender) attributions about membership in social groups perceived or stereotyped to be crime prone or dangerous (Albonetti, 1991; Steffensmeier et al., 1998). Focal concerns argues that these attributions may become shared informal norms or guides used to reduce uncertainty in decision-making, leading to disparate outcomes (Steffensmeier et al., 1998; Ulmer, 2012). Empirical research employing focal concerns theory generally distinguishes between the influence of measures *legally-relevant* to the goals of the pretrial determination (e.g., assuring appearance and protecting public safety), such as charge or criminal history characteristics, from measures deemed to be *extra-legal*, including demographic and non-legal case characteristics. Generally, this research

focuses on identifying extra-legal disparities in the pretrial phase (e.g., Demuth, 2003; Demuth & Steffensmeier, 2004; Katz & Spohn, 1995; Schlesinger, 2005).

### *Court Communities and Court Contexts*

Judges do not make decisions in isolation from other actors in the courtroom; bail determinations, plea agreements, and sentencing events are products of interactions among the judge, prosecution, defense, and other parties. Courts are also situated within specific social, economic, and political contexts, which may influence court operations (Ulmer, 1997). The “court community” perspective (Eisenstein et al., 1988) views courts as communities, or “social worlds” (Ulmer, 1997), that develop patterns of interactions and norms that shape case processing and court decision-making. These informal norms influence decision-making in most cases (Eisenstein et al., 1988; Ulmer, 1997), and are, in turn, influenced by the broader organizational, social, political, and economic contexts in which courts are embedded. This literature identifies a range of contextual contributors to court outcomes, including features of courts as organizations—the size of the court bureaucracy, the volume and diversity of cases, and the degree of autonomy of a given court to set its own policy and operations—and other contextual factors like community political pressures, local criminal justice resources, and demographic and economic characteristics of the community (Dixon, 1995; Eisenstein et al., 1988; Engen & Steen, 2000; Ulmer & Johnson, 2004). I discuss this framework at greater length in Chapter 4.

## *Responses to Legal Change*

Both focal concerns and court communities theories provide explanatory models for understanding variation in court outcomes across cases, courts, and contexts. Separate, but complementary, lines of scholarship also examine what shapes a law's effect, identifying both the role of individual discretion (Lipsky, 1980; Lynch, 1998; Rengifo et al., 2017), as well broader organizational, normative, political, and institutional factors that shape the application of law (Lynch & Omori, 2014; Verma, 2015). Broadly, this work articulates multiple, interacting processes that shape variation (or similarity) in implementation: at the individual-level, through the exercise of discretion and translation by street-level bureaucrats or mid-level managers (Lipsky, 1980); at the organizational-level, as leadership translate new law into policy and practice directives (Grattet & Jenness, 2005); and processes that operate across a population of organizations to create similarity in structures and practice (DiMaggio & Powell, 1983). Other arguments point to ambiguity in law itself, emphasizing that many laws do not specify clear instructions for their implementation (Edelman, 1990). Together, this scholarship highlights mechanisms that can account for misalignment between law-on-the-books and law-in-action.

Beyond individual actors, organizations play a role in shaping the meaning of law. Characteristics of an organization, along with their institutional environments, influence the process of translating formal law into specific procedure and practice (Grattet & Jenness, 2005; Eisenstein et al., 1988; Verma, 2015). Individual court communities are situated within a broader institutional field, and their decision-making processes are constrained by various rules, including criminal law, administrative rules,

and other policy influences (Ulmer, 2019). Neo-institutional theories in organizational sociology point to social processes by which organizations in a field come to adopt similar structures, policies, and procedures. New practices may spread across like organizations out of a need to conform to higher authority (e.g., through new law or court rule), by mimicking the practices of peer organizations, or by adopting practices promoted by standard-bearers (Meyer & Rowan, 1977).<sup>1</sup> Similar to the court communities concept of “going rates”, practices may come to be understood as “the way things are done” (Berger & Luckmann, 1967; Eisenstein et al., 1988).<sup>2</sup> Scholars also identify organizations as interpretative agents between higher law and local practice, and argue that variability in implementation of new law across organizations is likely when there is substantial discretion afforded to those responsible for implementing law, or where the letter of the law is ambiguous (Jenness & Grattet, 2005; Grattet & Jenness, 2005; Edelman, 1990). Past practices also influence the interpretation and implementation of new laws (Lynch & Omori, 2014; Verma, 2015).

Organizational features of courts and court systems, along with the criminal pretrial process itself, correspond to many of the mechanisms identified in sociolegal and court communities scholarship that account for variation in the application of law. For instance, the pretrial process involves (i) independent courts and court actors responsible for implementing pretrial policy; (ii) substantial discretion of judges and bail commissioners in the interpretation of the law’s meaning; and (iii) evidence of informal norms, or past practices, in operation across court case processing. Sociolegal

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<sup>1</sup> Other types of pressures have identified, as well—e.g., “actuarial pressures” (Grattet & Jenness, 2005).

<sup>2</sup> Meyer and Rowan (1977) note that “institutionalization involves the processes by which social process, obligations or actualities come to take on a rulelike status in social thought and action” (p. 341).

approaches to law and organizations, in conjunction with courts communities, suggest that not only will pretrial decision-making vary across court jurisdictions, but that community contexts may also differentially shape court responses to bail reform.

Together, these frameworks provide the theoretical grounding in which to situate this dissertation project. In particular, focal concerns and court-as-communities approaches provide ways of thinking about how courts might weigh factors in their pretrial determinations, and how the weighing of those factors are shaped or constrained by features of the organizational, local justice system, and broader community contexts in which these decision-makers are embedded. Further, sociolegal frameworks also provide grounding for how to think about how different jurisdictions respond to policy change. However, expectations derived from this literature are less clear as to the expected direction of change. Sociolegal and court communities approaches identify mechanisms that influence variation across courts in application of the law, and point to potential mechanisms of stability within courts in response to new law (e.g., Lynch & Omori, 2014, Verma, 2015). These lenses motivate my core research questions addressed in Chapter 4, on whether and how pretrial decisions vary across jurisdictions, and whether court contexts pattern responses to policy change. I now turn to a review of findings from the empirical literature on pretrial decisions, outcomes, and variation across jurisdictions.

### **Correlates and Consequences of Pretrial Detention and Release**

Below, I review the current state of knowledge on what factors influence pretrial decisions and outcomes, discussing first individual- and case-level correlates, then

turning to other influences, including organizational, and community- or neighborhood contextual factors.

### *Individual-Level Correlates of Pretrial Outcomes*

Studies consistently find that charge severity, along other case- or criminal-history measures, are the strongest predictors of pretrial release decisions and defendant pretrial outcomes, including ROR, bail amounts, and pretrial detention (Demuth, 2003; Foote, 1954; Goldkamp, 1980; Schlesinger, 2005). The severity of the charge significantly impacts the decision to release on ROR and to deny bail: those charged with more serious crimes are less likely to receive ROR (Ball & Bostaph, 2009; Frazier et al., 1980; Pinchevsky & Steiner, 2016), and more likely to be held in custody pending case adjudication (Demuth, 2003; Demuth & Steffensmeier, 2004; Pinchevsky & Steiner, 2016). Those with more serious charges also face higher bail amounts (Schlesinger, 2005; Turner et al., 2003; Turner & Johnson, 2005; Wooldredge, 2012; Wooldredge et al., 2017).

Criminal history, along with behavior while awaiting adjudication on a criminal case, also matter for pretrial outcomes: those with longer criminal histories and those who have previously failed to appear in court are more likely to be denied pretrial release (Schlesinger, 2005), and are less likely to receive ROR (Nagel, 1983; Pinchevsky & Steiner, 2016). Prior criminal history and active criminal justice status (i.e., on probation or parole at the time of the pretrial determination) also influence bail amounts (Katz & Spohn, 1995; Nagel, 1983; Pinchevsky & Steiner, 2016). Past performance on pretrial release does not appear to influence bail amounts, as studies largely fail to find any effect of prior failures-to-appear on bail amounts (Demuth, 2003;

Pinchevsky & Steiner, 2016; Schlesinger, 2005, but see Williams, 2016a). Qualitative research points to explanations for how these factors operate in certain jurisdictions: in New York City, Phillips (2012) finds that influence of charge severity on pretrial decisions is indirect, mediated by the prosecution's recommendation to the judge (see also Suffet, 1966). In two Southern Californian courts, judges overwhelmingly refer to fixed bail schedules when determining bail amounts in felony cases (Ottone & Scott-Hayward, 2018). Bail schedules assign a pre-determined bail amount to specific charges, and are heavily influenced by the severity of the charged offense (Scott-Hayward & Fradella, 2019).

As noted earlier, documenting racial and ethnic disparities at the pretrial stage is a central focus of research on pretrial outcomes. Though early studies find little evidence of an effect of defendant race on pretrial release decisions (Albonetti et al., 1989; Frazier et al., 1980; Goldkamp & Gottfredson, 1979; Nagel, 1983), more recent scholarship finds consistent evidence of racial and ethnic disparities. Black and Latino defendants are more likely to be detained pretrial and are less likely to receive ROR than their white counterparts, net of other factors (Demuth & Steffensmeier, 2004; Schlesinger, 2005; Sutton, 2013); while Latino defendants are more likely to receive higher bail amounts (Demuth, 2003; Sacks et al., 2015a; Schlesinger, 2005).<sup>3</sup>

Scholars also find evidence of gender disparities in pretrial outcomes. Despite early findings of no difference in release decisions between men and women

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<sup>3</sup> Several factors may account for differing results between the earlier and more contemporary research, including differences in the dependent variable(s) and sample differences. Early studies of racial disparities—published in the late 1970's through the 1980's—examined either pretrial release decisions or outcomes, thereby neglecting the full scope of the pretrial process. Later studies—primarily from the 2000's through present day—examine a variety of pretrial release decisions and outcomes, identifying significant racial and ethnic disparities in both.

(Goldkamp & Gottfredson, 1979), recent research finds that female defendants are treated more favorably throughout the pretrial period than men. Women are less likely to receive preventative detention than men, and are more likely to receive ROR, to be assigned a lower bail amount, and to secure pretrial release (Demuth & Steffensmeier, 2004; Katz & Spohn, 1995; Pinchevsky & Steiner, 2016). Gender also conditions the effects of other variables across both pretrial release decisions and outcomes, particularly offense type: Ball & Bostaph (2009) find that male defendants are more likely to be denied bail for property and violent offenses, while female defendants are more likely to be denied bail for drug offenses.

#### *Ability to Pay and Monetary Bail*

Money bail, and (in)ability-to-pay, has long been identified as a driver of pretrial release outcomes (for an early example, see Foote, 1954). Nationally, two-thirds of felony defendants released from custody do so by posting some form of bail (Reaves, 2013). However, limited research directly addresses the effect of poverty or socioeconomic status on pretrial outcomes. Existing data collections and court administrative datasets generally lack income, employment, or other financial or socioeconomic status information on defendants (Ulmer, 2012). Given the distinctive role of money bail in the pretrial process in the U.S., socioeconomic status and, in particular, access to financial resources, likely plays a substantial role in individuals' ability to obtain pretrial release. Studies document an inverse relationship between the amount of bail set and the probability of release (Reaves, 2013): at bail amounts of \$5,000 or less, about 70 percent of felony defendants in large urban counties were able to secure pretrial release; at bail amounts of \$100,000, just 10 percent of defendants were released

pretrial. Further, 9 in 10 felony defendants detained in jail custody awaiting trial were held on bail, meaning that a bail or bond amount needed to secure release was set, but not posted. While, in theory, defendants may choose not to post bail and to remain in jail, it is more likely that for those held pretrial, monetary bail was not within financial reach.

Studies of the pretrial process include proxy measures for socioeconomic status or ability-to-pay, such as assignment of indigent defense lawyer versus hiring of a private attorney, or draw on aggregate income data from areas around the defendant's residence. These studies find small positive associations between having a private attorney and pretrial release, suggesting that defendants who can afford private representation are more able to secure release pending trial (Goulette et al., 2015; Omori & Petersen, 2020; Sacks et al., 2015). Holmes et al. (1996), for example, find that defendants who retain private counsel are seven times more likely to be released prior to disposition. Heaton, Mayson, and Stevenson (2017) use median wealth of defendants' home zip codes to estimate the role of wealth on pretrial detention outcomes in Harris County, Texas. They find a negative relationship between median wealth in the defendant's zip code and likelihood of misdemeanor pretrial detention. Finally, using employment status drawn from pretrial investigation reports for a sample of 95 pretrial detainees in a county in Massachusetts, St. Louis (2020) finds that unemployment was not associated with bail amounts. However, being unemployed did significantly increase days spent in pretrial incarceration. These studies suggest that ability-to-pay, and socioeconomic status, exert an influence over pretrial outcomes.

### *Organizational and Contextual Factors*

Organizational and contextual factors also shape court outcomes, beyond the influence of individual case characteristics (Eisenstein et al., 1988; Ulmer & Johnson, 2004). Only a handful of studies of the pretrial process explore how judicial decision-making at the bail stage is influenced by contextual factors (D'Alessio & Stolzenberg, 2002; Hood & Schneider, 2019; Pinchevsky & Steiner, 2016; St. Louis, 2020; Williams, 2016; Sutton, 2013; Wooldredge et al., 2017; Williams 2018). Studies examine local jail crowding (Hood & Schneider, 2019; Pinchevsky & Steiner 2016; Williams, 2016), unemployment rates or economic contexts (D'Alessio & Stolzenburg, 2002; Hood & Schneider, 2019; Wooldredge et al., 2017; Sutton, 2013), crime rates (Pinchevsky & Steiner, 2016; St. Louis, 2020), and demographic factors (Pinchevsky & Steiner, 2016; St. Louis, 2020; Sutton, 2013; Williams, 2018). While findings across this small literature are mixed, this body of work suggests that contextual factors do matter for pretrial outcomes. I review this literature in depth in Chapter 4.

### **Consequences of Pretrial Detention & Release**

Compared with research on the pretrial release determination, the evidence base on the consequences of pretrial detention for defendants is less well-developed. However, recent studies generate convincing evidence that pretrial detention is consequential for both criminal justice outcomes and for future crime. Less is known about downstream outcomes: to date, only a few studies directly examine the social and economic consequences of pretrial incarceration (Dobbie et al., 2018; Gupta et al., 2016).

### *Failure-to-Appear in Court and Future Crime*

Correlational research links even short stays in pretrial incarceration to increases in failure to appear in court (Monaghan et al., 2022) and new criminal arrests (Lowenkamp, 2022). Generally, these studies use regression techniques to estimate the impact of pretrial detention on case outcomes, controlling for relevant defendant and case characteristics. However, correlational studies cannot rule out systematic, unobserved differences between those who are released pending trial and those who are detained.

Recently, scholars have attempted to improve upon correlational research, employing quasi-experimental methods to estimate the causal effects of pretrial detention and money bail on case outcomes. These studies find consistent evidence that defendants who are released pretrial have better case outcomes than those who are detained, but are also more likely to fail to appear in court and be rearrested in the pretrial period. However, studies also find evidence of a decreased likelihood of rearrest post-disposition, leading to negligible net impacts on reoffending. Dobbie, Goldin, and Yang (2018) find that for defendants who would have been detained if assigned to a different judge at the bail determination, pretrial release significantly increases the probability of failing to appear in court and the likelihood of rearrest during the pretrial period. However, they also find an offsetting decrease in the likelihood of rearrest post-disposition, leading to no discernable net impact on new crime. Leslie and Pope (2018) find that pretrial detention has offsetting impacts on future rearrest, decreasing the probability of arrest prior to disposition but increasing the probability of rearrest within two years following case disposition. Gupta, Hansman and Frenchman (2016) find that

defendants who are required to pay money bail are between 6-9 percent more likely to recidivate. Finally, Heaton, Mayson, and Stevenson (2017) find that pretrial detention is associated with an increase in the likelihood of committing future crime for misdemeanor defendants in Harris County, Texas.

### *Conviction and Sentencing*

Defendants detained pretrial are more likely to be prosecuted (Kellough & Wortley, 2002), be convicted (Stevenson, 2018b; Williams, 2003), receive a custodial sentence (Goldkamp, 1980; Oleson et al., 2016; Williams, 2003), and receive lengthier sentences (Sacks & Ackerman, 2014a; Stevenson, 2018b). Indeed, awaiting the resolution of a criminal case in the community may yield substantial advantages in case outcomes. Dobbie, Goldin, and Yang (2018), for example, find that being released pending trial decreases the probability of conviction by 14 percentage points, driven primarily by a reduction in the probability of pleading guilty. Studies in Philadelphia (Stevenson, 2018b) and New York City (Leslie & Pope, 2018) find comparable results. In the Federal system, Didwania (2020) finds that pretrial release is associated with shorter sentence length.

Setting money bail contributes to this effect: one study finds that defendants who are required to pay money bail are 12 percent more likely to be convicted (Gupta et al., 2016). However, no effect was observed for the amount of bail set, indicating that setting any financial release condition disadvantages defendants. Further, evidence indicates that the effects of detention on case outcomes may differ for felony and misdemeanor cases: Leslie and Pope (2018) find that the magnitude of effects of pretrial detention on case outcomes were larger for felony than for misdemeanor cases.

Heaton et al. (2017) find that pretrial detention increased the likelihood of conviction and of receiving a jail sentence, particularly for first-time misdemeanants.

Guilty pleas are a potential explanatory mechanism for the effect of pretrial detention on conviction. Qualitative studies offer insight into why those held pretrial have a higher rate of pleading guilty. Defendants held in custody pretrial may plead guilty in order to escape harsh jail conditions (Euvrard & Leclerc, 2017) or to avoid serving “dead time”, or time in jail awaiting disposition that may exceed an expected sentence length (Kellough & Wortley, 2002). Further, defendants may be induced to plead out because of prosecutorial practices such as time-delimited, “exploding” plea offers (Zottoli et al., 2016). Not only might incarceration prior to case dispensation induce guilty pleas, it may also speed up the timing of those pleas. Peterson (2019) finds that felony defendants detained pretrial pled guilty nearly 2.7 times faster than defendants who were released. Martinez et al. (2020) also find that the longer a defendant is detained, the more likely they are to be convicted and sentenced to incarceration.

### *Collateral Consequences to Employment and Families*

Research on the consequences of pretrial detention often overlooks the effects of detention beyond criminal justice outcomes. Two studies examine the social or economic consequences of pretrial detention outside of the criminal justice system. Dobbie et al. (2018) find that pretrial detainees were less likely to be employed and earned less following release compared to their released counterparts. Importantly, the effect identified by the authors is a narrow one, applying only to those detainees who would have received a different release decision if assigned to a different bail judge.

However, given the scale of pretrial detention in the United States, the actual effects of pretrial incarceration on employment may be much larger. A recent study in Denmark finds that pretrial detention is associated with a 6 percentage point drop in labor market attachment, and a higher risk of losing familial attachment (although the authors find no effect on family formation) (Wakefield & Andersen, 2020). Finally, a related literature examines the effects of jail incarceration on labor market outcomes, behavioral health, family, and child well-being (for a review, see Turney & Conner, 2019). While not specific to pretrial incarceration, this adjacent literature likely derives some of its results from pretrial detainees, given that they constitute over two-thirds of those incarcerated in local jails.

### **Summary and Proposed Contributions**

Research documents a range of extra-legal disparities with respect to race, ethnicity, and gender on both pretrial decisions and outcomes, and reveals evidence of unique impacts of pretrial detention on subsequent conviction and sentencing. Yet, key gaps in knowledge on the pretrial process and on the impacts of pretrial reform remain. In this section, I outline limitations to the existing body of research and discuss the contributions of this dissertation to this literature.

#### *Beyond “Individual-Level” Frameworks*

Most contemporary pretrial research is dominated by an “individual-level” focus, examining the relationship between various defendant- and case-characteristics on pretrial decisions and outcomes. Less is known about the influence of organizational, institutional, or ecological factors that shape judicial decision-making in the pretrial stage. Individual-level focus ignores structural or institutional forces that may produce

disparities, or may shape the ability of defendants to secure release. Given the wide variation in available release options, use of bail schedules, court resources and caseloads, and community contexts, key gaps in the literature remain on how pretrial decision-making and pretrial outcomes are shaped by organizational and social contexts. Specifically, questions remain about the organizational determinants of pretrial decisions, as well as whether the larger social contexts of courts influence outcomes. For example, to what degree do caseload pressure or volume affect pretrial release decisions? Does decision-making vary over time, and do informal norms persist even as policies change (Eisenstein et al., 1988)? Do community factors, such as crime rates, demographic composition, or economic conditions account for inter-jurisdictional variation in pretrial outcomes? Answering such questions requires information on court decisions and pretrial outcomes over time and across jurisdictions, as well as information beyond court administrative records. I turn briefly to limitations of existing pretrial samples below.

### *Limited Samples and Jurisdictions*

Many studies of the pretrial phase rely on just one source of national data—the Bureau of Justice Statistics’ State Court Processing Statistics (SCPS) series (see e.g., Demuth, 2003; Demuth & Steffensmeier, 2004; Helland & Tabarrok, 2004; Hood & Schneider, 2019; Pinchevsky & Steiner, 2016; Schlesinger, 2005; Williams, 2016; Williams, 2018). The SCPS series sampled felony cases from 40 urban jurisdictions, weighted to represent the 75 largest urban jurisdictions, biennially from 1988 to 2006 and again in 2009. Cases were tracked prospectively from charging to disposition, and contain a variety of defendant and case variables. While useful, several features of the

SCPS data limit its utility in pretrial research. First, the data are now quite dated, with the last available data collection occurring in 2009, limiting the ability to document more current trends in the pretrial process. Second, the SCPS omits misdemeanor cases and cases from suburban, ex-urban, and rural jurisdictions altogether, limiting their generalizability across place and case type. Finally, the SCPS does not contain information on pretrial policy and law that governs decision-making in sampled jurisdictions; given the wide variation in pretrial legal regimes documented across states and localities (Page & Scott-Hayward, 2022), studies making use of the SCPS often omit policy variables, which may confound estimates of contextual effects.

Other research draws on administrative data obtained directly from local court systems. Like the SCPS, these datasets are often missing defendant information on measures of socioeconomic status, income, and other stratifying variables (e.g., employment, education) that may be related to the ability to secure release (but see St. Louis, 2020). Information on how defendants paid bail—either through cash payment or bond—are often missing as well. Importantly, studies often rely on data from single local jurisdictions or several jurisdictions within a single state, limiting the nature of questions posed and the generalizability of the findings. A small group of large, urban jurisdictions – e.g., Philadelphia, Miami, Harris County, New York City – predominate in such studies, largely omitting smaller jurisdictions (as in the SCPS) (e.g., Dobbie et al., 2018; Gupta et al., 2016; Heaton et al., 2017; Leslie & Pope, 2018; Ouss & Stevenson, 2023).

This dissertation contributes to several gaps in the literature. First, a small, but growing number of peer-reviewed studies examine the impacts of bail reform on pretrial decisions and public safety (I discuss this literature in depth in chapters 2 and 3); this

study aims to contribute evidence on the effects of bail reform to this burgeoning literature. Second, this dissertation contributes to studies on the influence of court organizational and community context on pretrial decisions, and examines whether these contextual moderate responses to new bail law. Finally, I expand the types of cases and the jurisdictional context under study—focusing on the state of Maryland, incorporating multiple case types (i.e., not simply felony cases, or only misdemeanor cases), and jurisdictional features (e.g., large and small courts). In the next section, I introduce the bail reform movement and detail the Maryland site and reform as a case study.

## ***Part II. Pretrial Justice Reform and Maryland's Pretrial System***

### **Reforming Pretrial Justice in the United States**

Across the United States, a growing coalition of advocates, researchers, politicians, and media outlets have voiced concerns over a pretrial justice system that holds nearly a half million unconvicted people in jail each day and disproportionately impacts low-income people and people of color (Doyle et al., 2019). In the last decade, many states and hundreds of local jurisdictions enacted a wide range of changes to pretrial policy and practice to address the problem of rising pretrial detention. Reforms enacted include changes to court rules on pretrial release determinations; access to counsel at bail hearings; risk assessment tools to assist decision-making; automatic court date notifications to defendants; pretrial supervision services; and, in rare instances, limiting or prohibiting the use of cash bail (Jorgensen & Smith, 2021). Not all initiatives share an explicit goal of reducing or eliminating the use of cash bail; interventions variously focus on expanding due process rights for defendants at the bail

determination, changing how judges make determinations (rather than what bail conditions are available), and reducing pretrial misconduct through notification or supervision.

I use the term “bail reform” to refer to policy changes that intend to reduce, or eliminate, the use of money bail as a condition of pretrial release. Only a handful of states<sup>4</sup> have pursued large-scale reforms aimed at limiting the use of cash bail in recent years. In 2011, Kentucky enacted statewide reform, mandating the use of risk assessments in pretrial determinations, encouraging judges to release low- and moderate-risk defendants on their own recognizance (in lieu of monetary bail), and capping bail amounts for low-level misdemeanors. In 2017, New Jersey enacted a large-scale reform to shift nearly all pretrial determinations to a risk assessment-based system, and modified law enforcement practices on issuing citations in lieu of arrest. Following a 2014 New Mexico Supreme Court ruling prohibiting judges from assigning unaffordable bail, New Mexico voters amended their state constitution to ensure that no defendants were held in pretrial detention solely due the inability to pay bail. Alaska ended the use of cash bail for misdemeanors and low-level felonies in 2018; only serious felonies for high-risk defendants are eligible for secured bond. In 2020, New York State ended the use of cash bail for misdemeanors and low-level felony offenses, but rolled back some reforms just three months later, allowing the use of bail for felonies. In late 2023, Illinois ended its use of money bond across the state, changing

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<sup>4</sup> Many local jurisdictions have also enacted high-visibility reforms reducing the use of cash bail—notably, Harris County, Texas, Cook County, Illinois, and Philadelphia, Pennsylvania. Here, I focus on state-level reforms that are implemented across different sub-state jurisdictions because of my interest in examining how different courts respond to policy change.

the custodial arrest determination, allowing the use of preventative detention, and eliminating financial conditions of pretrial release.

Maryland is among the handful of states that have undertaken major, statewide reforms to their pretrial procedures with the goal of reducing reliance on money bail. Rather than targeting a specific population of arrestees, as in some other states, Maryland's bail reform affects almost all criminal defendants in the state. This broad-scale reform makes Maryland a useful site to study impacts of bail reform and to examine variation in court responses to state-wide policy change. Below, I detail Maryland's pretrial system and outline its bail reform effort.

### **The Pretrial Process in Maryland**

Maryland's pretrial process takes place in two stages. After arrest, most individuals are booked into jail custody and brought before a district court<sup>5</sup> commissioner – a judicial officer that presides over initial appearances and determines the initial conditions of pretrial release – within 24 hours.<sup>6</sup> At the initial appearance, the defendant is advised of the nature of the charges against them, the possible sanctions they face, and their right to counsel. Additionally, the court commissioner has the authority to set pretrial release conditions, such as personal recognizance, an

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<sup>5</sup> Not to be confused to Federal District Courts. In Maryland, the "District Court" is a trial court that hears both civil and misdemeanor and certain felony criminal cases, although it shares jurisdiction with the Maryland state "Circuit Courts" in criminal cases where the penalties authorized are three years or more in prison, a fine of \$2,500 or more, or both.

<https://mdcourts.gov/sites/default/files/import/publications/pdfs/mdjudicialsystem.pdf>

<sup>6</sup> District court commissioners are judicial officers in the Maryland judiciary, but with a more limited authority than judges. Commissioners have three main responsibilities: (i) to determine probable cause of applications for statement of charges; (ii) to conduct initial appearance hearings and set conditions of pretrial release; and (iii) to determine eligibility of applicants for indigent defense. Commissioners may not preside over trials. Only judges may render a disposition in a criminal case. (*Maryland Courts*, <https://mdcourts.gov/district/selfhelp/whodoeswhat#commissioners>)

unsecured personal bond, secured bond or money bail, and, in some counties, release to a pretrial supervision program.<sup>7</sup> Commissioners may also deny release altogether if no conditions of release can “reasonably protect against the danger that the defendant presents to an identifiable potential victim and/or to the community” (State v. Wheeler 2005, 864 A.2d at 1062). Further, court commissioners are prohibited by law from releasing certain groups of defendants, including those charged with: (i) crimes of violence or certain firearm offenses who have had a prior conviction for a crime of violence or a firearm offense; (ii) individuals charged with committing certain offenses while they were released pretrial; (iii) registered sex offenders; (iv) those charged as drug kingpins; and (v) correctional facility escapees (Md. Code. Ann. Crim. Proc. § 5-202).

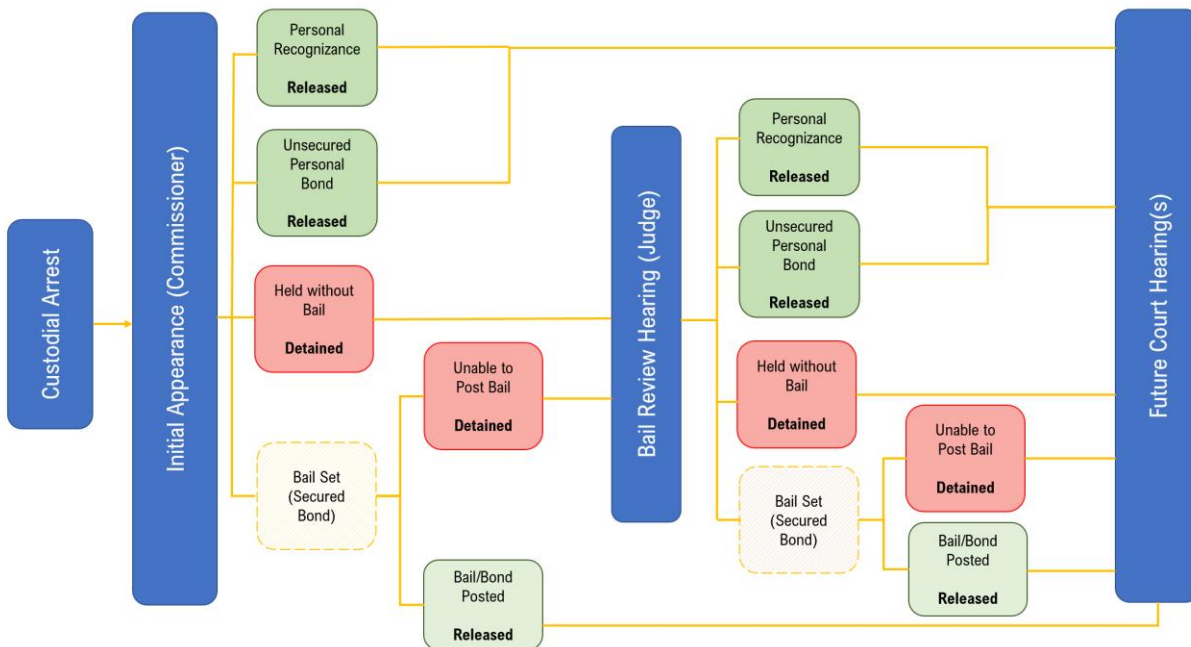
If a defendant is not released pretrial – either because the commissioner’s decision was to hold the defendant without bail, or the defendant was unable to post money bail or secured bond – they are held in detention until the next court day, when they will appear before a judge for a bail review hearing.<sup>8</sup> At the bail review hearing, the judge may decide to release on recognizance, set a secured or unsecured bond, or hold the defendant in custody without bail (“preventative detention”). Judges have wider authority than commissioners to set release conditions for defendants (MD Rule 4-216(d)). Figure 1 illustrates this process.

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<sup>7</sup> Nearly all counties and the City of Baltimore have a pretrial services agency operating to provide pretrial assessment and/or supervision services. However, little standardization in policy or practice exists: the type of assessment, supervision, and services vary widely from county-to-county. Court of Appeals Standing Rules Committee, Supplement 1 to the 192<sup>nd</sup> Report, Jan 17, 2017.

<sup>8</sup> District Court commissioners are available 24 hours a day, 365 days per year. However, judges work only on court business days, which exclude weekends and court holidays.

Figure 1. Diagram of the Pretrial Process in Maryland



In both the initial appearance and the bail review hearing, commissioners and judges have broad discretion to determine whether, and under what conditions, to offer pretrial release to defendants. Judicial officers are instructed to impose the least burdensome condition(s) of release necessary to ensure future appearance at court hearings, to protect safety of an alleged victim(s), and to ensure that the defendant does not pose a danger to another person or to the community. In making their determinations, judicial officers must consider factors including: the offenses charged and nature of the evidence; prior record of appearances at court proceedings; prior convictions; ties to family, community, and financial resources; and any recommendations from a pretrial services agency, the prosecution, and/or the defense. At both stages of the pretrial process—initial appearance and the bail review hearing—defendants have the right to an attorney, although this is often waived at the initial

appearance to expedite the determination (Maryland Office of the Public Defender, 2018).

### **The History of Bail Reform in Maryland**

Reforms to Maryland's pretrial system have long been sought by advocates and system actors, who argue that the state's reliance on money bail imposes substantial costs to defendants, communities, and local governments. Between 2011 and 2015, a study found that nearly 47,000 defendants were held in custody on money bail for more than 5 days at the start of their criminal cases; 1 in 3 had bail set at less than \$5,000 (Gupta et al., 2016b). During that same period, commercial bail bond companies posted \$2.56 billion in corporate bonds, costing defendants an estimated \$256 million in nonrefundable fees. These fees were paid disproportionately by Black defendants and concentrated in Maryland's poorest zip codes (Id.). In the years leading up to bail reform, Maryland's pretrial jail populations were also on the rise: between 2001 and 2015, the proportion of Maryland's jail population held pretrial increased from 55 percent to 66 percent (Governor's Commission to Reform Maryland's Pretrial System, 2014).

Maryland's 2017 bail reform was preceded by several unsuccessful efforts to reform the state's pretrial system. Between 1998 and 2002, a series of bills were introduced in the state's legislature proposing various fixes to the state's pretrial system, including creating a right to counsel at the first appearance and mandating ten percent cash deposits for money bail, among other reforms. In each case, the proposed legislation failed, succumbing to political opposition from the bail bond and insurance industries and from public defenders, in spite of broad support from high profile lawmakers, judiciary members, and civil rights organizations (Colbert & Starger, 2022).

Blocked in the legislature, reformers pursued change through litigation. In 2006, *Richmond v District Court of Maryland* was filed, asserting that state law gives indigent defendants the right to counsel “at all stages” of the criminal proceeding, including the initial bail appearance (Colbert, 2015). Seven years later, in 2013, the Maryland Court of Appeals ruled in favor of a right to counsel at the initial bail appearance, a victory for reform advocates. However, in practice, representation at the initial appearance has fallen short of its promise to improve defendant pretrial outcomes in Maryland; nearly 2 in 3 defendants waive this right for fear of being returned to jail custody while awaiting attorney appointments (Colbert & Starger, 2022). In 2013 and 2014, two high-profile committees, one formed by the Governor and another by the Judiciary, conducted comprehensive studies of Maryland’s pretrial process and issued wide-ranging recommendations to improve system outcomes.<sup>9</sup> Though both committees documented high rates of pretrial detention for those with bail amounts set and identified substantial racial disproportionalities, neither effort resulted in any change to policy or procedure, again facing strong opposition from judges, politicians, and bail bond industry.<sup>10</sup>

Legislative inaction prompted reform efforts through a different mechanism: court administrative rules. On October 11, 2016, the Maryland Attorney General’s office sent an advice letter to several members of the Maryland General Assembly arguing that defendants in Maryland are legally entitled to individualized inquiry on financial ability to pay prior to ordering money bail, and contending that the state’s appeals court might

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<sup>9</sup> The Maryland Judiciary’s Task Force on Pretrial Confinement & Release issued its final report in April 2014; and the Governor’s Commission to Reform Maryland’s Pretrial System issued its final report in December 2014.

<sup>10</sup> In fact, in its initial proposed rule order, the Rules Committee acknowledged the strong opposition to the proposed rule changes and that the “General Assembly...has consistently, for decades, been unwilling to enact any substantial reform measures” (page 7).

rule that money bail may not exceed a defendant's ability to pay. Just two weeks later, on October 25, the Attorney General, Brian Frosh, issued an official letter to the Maryland Judiciary's Standing Committee on Rules of Practice and Procedures ("Rules Committee"), requesting specific and substantial changes to the Court's procedural rules governing release. The Attorney General argued that current pretrial practices raised constitutional concerns. Bail set in unaffordable amounts would likely "violate...due process" and that the state Court of Appeals would "declare that the bail is excessive under the Eighth Amendment of the U.S. Constitution and Article 25 of the Maryland Declaration of Rights" (Maryland Attorney General, 2016: 2). The Attorney General's formal letter argued that state law required an individualized assessment of a defendant's ability to pay any financial bail conditions, and that current pretrial practices were neither effective at ensuring appearance nor in protecting public safety, and had a disproportionate, harmful impact on Black and Latino communities across the State. The letter also argued that there is substantial inconsistency in conformity to the existing pretrial rules across courts. Just one month later, on November 22, 2016, the Rules Committee issued draft amendments to its procedural rules ("Court Rules") to conform to the recommendations in the October 25<sup>th</sup> letter.<sup>11</sup> Those rules were formally adopted by the Court of Appeals on February 16, 2017, and took effect on July 1, 2017.

The Court Rules revised the procedures that govern the pretrial process outlined above. The new rules were designed to reduce unnecessary pretrial detention,

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<sup>11</sup> Section 18(a) of the Maryland Constitution provides the Court of Appeals with the power to adopt rules and regulations that are mandatory and have the force of law: "The Court of Appeals from time to time shall adopt rules and regulations concerning the practice and procedure in and the administration of the appellate courts and in the other courts of this State, which shall have the force of law until rescinded, changed or modified by the Court of Appeals or otherwise by law. The power of courts other than the Court of Appeals to make rules of practice and procedure, or administrative rules, shall be subject to the rules and regulations adopted by the Court of Appeals or otherwise by law."

eliminate unaffordable bail, and “promote release of defendants on their own recognizance, or when necessary, unsecured bond” (4-216.1). Foremost among these changes, the new rules expressly prohibited judicial officers from setting financial conditions that resulted in pretrial detention solely due to a defendant's inability to pay (4-216.1(e)(1)(A)). Moreover, the rules barred judicial officers from setting financial conditions solely to ameliorate perceived dangerousness or to punish defendants: “special conditions of release with financial terms...may not be imposed solely to prevent future criminal conduct during the pretrial period or to protect the safety of any person or the community; nor may they be imposed to punish the defendant or to placate public opinion” (4-216.1(e)(1)(B)). Judicial officers may only set financial conditions of release to ensure a defendant’s appearance in court. The use of bail schedules, which assign fixed bail amounts for specific offenses charged, was also outlawed.

The Court Rules also created explicit preferences in the law for certain types of pretrial release, instructing judicial officers to impose the least restrictive conditions necessary to ensure appearance and to prioritize the use of nonfinancial conditions. The new rules reaffirmed the use of preventative detention—or detention in custody without any bail or release condition—where no other conditions of release would ensure appearance or community safety. Under the new Court Rules, release on personal recognizance (or, “own recognizance”) is the preferred release type, followed by unsecured bond. Financial conditions are less preferred than nonfinancial or unsecured bond, followed by preventative detention, referred to as being “held without bail.” Finally, the Court Rules also expanded the set of authorized nonfinancial special

conditions available to commissioners and judges, including electronic monitoring, pretrial supervision, restrictions on travel; requirements to maintain employment or commence educational programming; imposing a curfew, or requiring that defendants refrain from drug or alcohol use.

Notably, Maryland's bail reform did not eliminate any pretrial release options available to judicial officers, nor did it eliminate judicial discretion in setting pretrial conditions. In fact, in its first issuance of the proposed rules changes, the Rules Committee expressly stated that the new rules "...do **not** prohibit the imposition of financial conditions (money bail); they do **not** do away with compensated sureties (bail bondsmen); and they do **not** permit the release of defendants who are significant flight risks or are likely to be a danger to the community or anyone in the community. Indeed, they expressly **prohibit** the release of such a defendant" (emphasis original; Standing Committee on Rules of Procedure and Practice, November 22, 2016).

### **Maryland as a Case Study of Bail Reform and Court Variation**

Maryland's bail reform provides a meaningful case study of recent bail reform efforts for several reasons. First, bail reform went into effect for all defendants across Maryland at the same time. This is largely unlike other bail reform initiatives implemented in other jurisdictions, which either target only some types of charges (Zhou et al., 2023; Albright, 2022), impact only single local jurisdictions (Heaton et al., 2017; Heaton, 2022; Lacoé et al., 2024; Ouss & Stevenson 2023), or were phased in across jurisdictions in a state over time (Albright, 2022; Stevenson, 2018a). Extant research on bail reform focuses on single-jurisdiction reforms or on aggregate effects (e.g., Golub et al., 2019; Stevenson 2018a; Zhou et al., 2023), leaving variation in implementation

across courts or contexts underexamined (but see Griffin et al., 2024; Lu & Rempel, 2022). Maryland’s statewide reform provides an opportunity to examine not only statewide impacts, but to explore how different courts and counties implemented the reform, holding other state policy variables constant across substate jurisdictions.

Second, Maryland’s bail reform is an effort to constrain the use of money bail without prohibiting its use. Maryland’s bail reform attempts to strengthen the *norm* of nonfinancial pretrial release by shaping judicial discretion rather than constraining its scope. This approach differs from other recent statewide efforts at bail reform in several respects. Other bail reform efforts, particularly legislative efforts, simultaneously altered other aspects of the pretrial process, which obscures the specific mechanism through which any changes are achieved. For example, New Jersey’s pretrial reform effort implemented a robust system of pretrial risk assessment and supervision to guide pretrial decision-making. However, the reform also substantially changed the law enforcement citations and summons processes, altering who gets custodially arrested—and therefore who is subject to pretrial determinations by judicial officers. Likewise, New York’s 2020 bail reform law, which limited the ability to set monetary bail for a range of offenses before being substantially rolled back, also significantly altered New York’s discovery rules, which heavily favored prosecutorial power and were often seen as a driver of guilty pleas to avoid a “trial penalty” (National Association of Criminal Defense Lawyers, 2021). Moreover, unlike other states, Maryland’s policy does not target specific subsets of offenses—instead, the rule changes focus on *how* judges release, rather than *who* judges release. For example, a Kentucky automatic release program eliminated bail for non-violent, non-sexual misdemeanors (Albright, 2022) and New

York's initial bail reform eliminated bail for most misdemeanors and certain non-violent felonies. Harris County, Texas and Cook County, Illinois also substantially altered bail practices for misdemeanors following court orders (Heaton, 2022; Stemen & Olson, 2020). Other policy reforms employ changes that circumvent judicial discretion entirely (Albright, 2022) or marshal prosecutorial discretion to influence bail determinations (Ouss & Stevenson, 2023).

Finally, Maryland's bail reform was put into motion several years prior to the COVID-19 pandemic. This timing allows for a meaningful post-reform period over which bail reform's impacts on pretrial and crime outcomes may be observed, without interference from the substantial shock of the pandemic that resulted in dramatic changes to daily operations of courts, jails, and law enforcement operations. Other prominent statewide bail reform efforts are more recent (e.g., Illinois' late 2023 bail reform) or were enacted during the COVID-19 pandemic (e.g., New York's 2020 bail reform).

### ***Part III. Overview of Data Sources for the Dissertation***

Below, I introduce the data sources that I draw on for this dissertation, and in Chapters 2, 3, and 4, I discuss each source, sample, and specific measures and operationalization in more detail.

#### **Administrative Court Data**

I draw from case-level criminal court data on all cases filed, pending, or disposed in district courts across 23 counties and the City of Baltimore from 2000 to early 2021. These data were obtained from Open Justice Baltimore (OJB), a non-profit organization

focused on transparency of criminal justice institutions and criminal justice reform.<sup>12</sup> OJB web-scraped court case records from the Maryland Judiciary Case Search (MJCS), a publicly accessible repository of court case information in the State, and compiled these public records into an accessible public database.<sup>13</sup> With special permission, I also obtained from OJB additional identifiable data elements redacted from their public exports (although publicly available on MJCS): defendant full name and date of birth information.

Pursuant to a 2020 law,<sup>14</sup> current MJCS records now automatically suppress all charges that are resolved in the defendants' favor, defined as charges where the disposition is acquitted, dismissed, "nolle prosequi," or not guilty. Such charges are neither expunged nor sealed, only suppressed from the online MJCS. These records remain in court case management systems and are available to the public in each courthouse location. Implementation of this law (removal of certain charges from MJCS) began in March 2021. Data used for this dissertation retain all records scraped from MJCS prior to the implementation of HB 1336, including those that are now no longer available via the MJCS.<sup>15</sup>

Each case in the record-level court data contains information on defendant and case characteristics. Defendant characteristics include full name and aliases, race, gender, and date of birth. Case characteristics include court type, court location, filing date, case type, and case status. Case characteristics also include case processing

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<sup>12</sup> Available here: <https://openjusticebaltimore.org/>. Data were obtained from the Case Harvester exports.

<sup>13</sup> Available here: <https://casesearch.courts.state.md.us/casesearch/processDisclaimer.jis>

<sup>14</sup> H.B. 1336. <https://mgaleg.maryland.gov/mgawebsite/Legislation/Details/HB1336?ys=2020RS>

<sup>15</sup> This suppression of records resolved in defendants' favor effectively limits OJB's complete data to cases resolved prior to early 2021.

information, including pretrial hearings and decisions; bail and bond setting information; charge information, including statute code, class, and level; and case disposition information.

### **Other Data Sources**

This dissertation also employs data from a variety of other sources. To examine the impacts of bail reform on crime, I use state-level annual crime data from Uniform Crime Reporting Program Data: Offenses Known and Clearances by Arrest (Return A), 1960-2020.<sup>16</sup> These data include the Part I violent crimes, including murder and non-negligent manslaughter, robbery, aggravated assault, and rape, and property crimes, including burglary, larceny-theft, motor vehicle theft and arson. For the analysis of county-level variation in pretrial outcomes, I draw on multiple sources of data. From Maryland Judiciary Annual Statistical Reports issued from FY2014 through FY2019, I use organizational characteristics of Maryland's district courts, including the number of new criminal filings for each court and the number of full-time judicial officers. Jail information is obtained from the Bureau of Justice Statistics 2019 Census of Jails and the 2014-2018 Annual Survey of Jails. I use data on county-level demographic and economic characteristics of each county from the Census Bureau's 2016 American Community Survey (ACS) 5-year estimates. Finally, I use data from MIT Election Lab on county-level voter returns for the 2016 Presidential election.

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<sup>16</sup> Kaplan, Jacob. Jacob Kaplan's Concatenated Files: Uniform Crime Reporting Program Data: Offenses Known and Clearances by Arrest (Return A), 1960-2020. Ann Arbor, MI: Inter-university Consortium for Political and Social Research [distributor], 2021-09-27. <https://doi.org/10.3886/E100707V17>.

## **CHAPTER 2: THE EFFECT OF BAIL REFORM ON JUDICIAL BEHAVIOR IN MARYLAND**

Limited research to-date suggests that initiatives aimed at reducing or eliminating cash bail have significant impacts on pretrial release conditions (Golub et al., 2019; Heaton, 2022; Ouss & Stevenson, 2023) and, in some cases, detention outcomes (Golub et al., 2019). This chapter adds to this nascent literature; I conduct an empirical study of the effect of Maryland's bail reform on pretrial decision-making. Using administrative court case data on new criminal cases filed in Maryland between 2014 and 2019, I use a discontinuity approach to estimate the effect of bail reform on money bail setting, bail amount, nonfinancial release, and preventative detention at the initial bail determination. I organize this chapter as follows: first, I discuss the prior research on bail reform initiatives and their effects on pretrial release conditions and detention outcomes. I then discuss the data and empirical approach used in the current study, and present the key findings on the effects of bail reform on pretrial decision-making in Maryland. I conclude with a discussion of study limitations and suggestions for future directions.

### ***Bail Reform Impacts on Pretrial Detention and Release***

Bail reform is not a monolith; initiatives have been driven by a range of forces, including mandated court-ordered reforms, bi-partisan legislative efforts, and discretionary policy changes within the courts or prosecution (Carroll, 2023). What unites these diverse efforts is the goal of reducing or eliminating altogether the use of

money bail. While the literature on the effects of bail reform is limited,<sup>17</sup> existing studies suggest that bail reform initiatives do influence pretrial release conditions and detention outcomes. In 2017, New Jersey's legislatively enacted bail reform all but eliminated cash bail, moving to a system of risk-based pretrial assessment and release decision-making, enhancing pretrial supervision, and allowing preventative detention. Golub et al. (2019) find that New Jersey's shift to risk-based pretrial decision-making led to a 9 percentage point increase in the number of defendants released without conditions, and a decline in those released to pretrial monitoring programs. They also find that a smaller percentage of defendants were held in jail due to preventative detention (a new option under the law) than would have been held in jail because they did not post bail. Though largely considered successful in reducing pretrial incarceration, fidelity to the risk-based decision-making framework has been variable; one study finds that judges and prosecutors in New Jersey courts sought to circumvent new processes or add pretrial monitoring conditions above the recommended release framework (Rengifo et al., 2021; Page & Scott-Hayward, 2022). One study of a court injunction in Harris County, TX that mandated release for misdemeanor defendants who cannot afford bail, finds that the injunction led to a 6.8 percentage point increase in pretrial release within 24 hours and a 3.2 percentage point increase in release within 72 hours of booking (Heaton, 2022). The injunction resulted in less time spent in detention for 12 percent of misdemeanor defendants. Likewise, Albright (2022) finds that a Kentucky law automatically releasing

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<sup>17</sup> Here, I intentionally exclude larger literatures on specific practices or programs that may be implemented to increase court appearance rates at the pretrial stage, such as court notification (e.g., Zottola et al., 2023) or pretrial supervision (e.g., Lowenkamp & VanNostrand, 2013). Likewise, the literature on pretrial risk assessments generally examines the power of such tools to predict future appearance and new criminal activity; much of the research examining their implementation is descriptive in nature or produced by the same organizations responsible for developing the assessment tools (Stevenson 2018a).

eligible low-level cases resulted in a 14 percentage point increase in release within one day, generated primarily from substitution away from the practice of setting money bail in low-level cases where small bail amounts resulted in pretrial incarceration. Finally, Griffin et al. (2024) descriptively examine the impacts of Illinois' bail reform that eliminated cash bail and largely prohibited pretrial detention for criminal defendants in the state, finding substantial declines in rates of detention following the reform.

Legislatively-driven and court-mandated reforms that limit judicial discretion are less common than reforms aimed at shaping the use of that discretion in pretrial policy (Page & Scott-Hayward, 2022). Reliance on the exercise of judicial discretion may blunt the effects of reform (Stevenson, 2018a). In 2018, Philadelphia's District Attorney implemented a policy that ended the practice of requesting cash bail for defendants charged with 25 common misdemeanor and felony offenses. This "no-cash-bail" policy did not change any rule or policy around the power of bail magistrates to set monetary bail; here, only the prosecutor's request for pretrial conditions changed. Ouss and Stevenson (2023) find that the no-cash-bail policy resulted in an 11 percentage point increase in release without financial collateral. These findings also suggest that other actors in the courtroom workgroup, besides the judge, are influential in shaping judicial decision-making (Ottone and Scott-Hayward, 2018; Ulmer, 2019). Despite its impact on nonfinancial release, the Philadelphia reform had limited effects on pretrial detention: the study finds no significant impact on rates of pretrial incarceration, as the shift away from setting cash bail conditions occurred among those defendants who would have otherwise had a very low bail set or have been assigned unsecured bail. Similarly, a court-mandated change in bail setting rules in San Francisco, CA, resulted in an overall

increase in pretrial release of 11 percent and a small impact on rates of detention (3 percent increase), driven by a shift away from cash bail toward the use of pretrial supervision programs (Lacoe et al., 2024).

Reforms, once implemented, may vary in their impact over time, as actors adapt their decision-making practices. In a study of the effects of a 2011 Kentucky reform, Stevenson (2018a) finds evidence that mandating the use of risk assessment resulted in an increase in release on nonfinancial conditions. However, that effect declined over time; by early 2016, over half of the increase in nonfinancial release attributed to the 2011 law had evaporated. At the same time, Stevenson (2018a) finds that the law was associated with an immediate and sharp decline in the share of defendants assigned low cash bail, but that this share increased over time as judges reverted to past practice. Similarly, Lu and Rempel (2022) conduct a descriptive analysis of trends in pretrial outcomes following New York State's 2020 legislative bail reforms, finding that initial responses to bail reform were not constant over time. They find that, in line with the aims of the law, bail setting decreased in the initial year of implementation before modestly increasing again, while rates of release on recognizance (ROR) followed the same trend: initial increases in use of ROR, followed by declining rates in the second year of implementation. Authors also find regional differences in trends, with upstate courts setting more restrictive conditions at the pretrial stage than courts in more urbanized areas of the state (Lu & Rempel 2022).

Jointly, these studies demonstrate that both mandatory and discretionary reforms to pretrial policy can influence decision-making at the pretrial determination and defendant detention outcomes. However, this small literature also suggests that

discretionary reforms may lead to variation in implementation, and in some cases, to outcomes that fail to meet the aims of bail reform.

### ***Prior Research on Maryland's Bail Reform***

A small set of descriptive studies examine the outcomes of Maryland's 2017 bail reform. Comparing pretrial conditions set in cases heard in the five months after bail reform (July to November 2017) to the same five month period two years earlier in 2015, Blumauer and co-authors (2018) find descriptive evidence of change in pretrial decisions following the reform. Researchers documented a drop of 22 percentage points in the percentage of defendants assigned money bail at their initial hearing, along with a \$31,000 decrease in the average dollar amount of bail assessed in 2017, compared with 2015. Likewise, they observe a smaller increase in the use of release on nonfinancial conditions (i.e., on recognizance and unsecured bail). Importantly, they document a rise in the use of preventative detention, increasing from 7 percent of defendants in 2015 to nearly 19 percent in 2017. The Maryland Judiciary released its own report on the impacts of changes to the pretrial release rules, examining data between July 2016 and September 2017, comparing periods before and after the Attorney General's Letter of Advice (issued in October 2016) (Maryland Judiciary, n.d.). The report documents similar findings, reporting small increases in the percentage of defendants released on nonfinancial conditions, as well as declines in the percentage of defendants held on cash bail after five days. Like Blumauer et al. (2018), the Judiciary also found increased use of detention without bail at initial appearance, although they also observe a decline in the proportion of defendants held without bail after a bail review hearing. Finally, a report examining pretrial outcomes for Baltimore City and

Prince George's County finds similar patterns at the local level, but also documents substantial variation between the two jurisdictions in both bail amounts and hearing outcomes (Glazener et al., 2023). Though these reports suggest that Maryland's reform impacted pretrial decision-making, each relied on simple pre/post or trend analysis to explore impacts and did not control for confounding influences on pretrial outcomes, limiting their utility in isolating the impacts of bail reform from other changes in case composition or policy.

### ***Current Study***

In this chapter, I estimate the effect of Maryland's bail reform on judicial decision-making at the pretrial stage. Specifically, I estimate the effect of Maryland's bail reform on four pretrial release decisions: money bail setting, bail amount, nonfinancial release, and preventative detention.

### **Data and Sample**

I use case-level criminal court data from all district courts in Maryland from the period October 2014 to September 2019, a period that encompasses the announcement and enactment of Maryland's bail reform (see Chapter 1 for a more detailed description of the MJCS data).<sup>18</sup> The study sample includes new criminal filings initiated under a "statement of charges," which are criminal cases initiated following "on-

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<sup>18</sup> This timeframe was chosen to examine cases in periods before and after bail reform. In 2014, the Maryland Judiciary implemented new rules allowing for the right to counsel at initial appearance in district courts for indigent defendants. Though limited, prior research suggests that representation at initial appearance may affect bail determinations (Colbert et al., 2001; Morgan, Shamserad & Huebner, 2024). As such, I begin the study period after the implementation of the new rules, and limit the pre-period to two years prior to the announcement. Further, on October 1<sup>st</sup> 2019, the State's Attorney for Prince George's County announced that the office would no longer request cash bail; this policy shift was limited to just one county, however, because it is one of the most populous counties in the state, I truncate the nominal sample for these analyses to end on September 30<sup>th</sup> 2019.

view” arrests.<sup>19</sup> Cases that do not uniformly require a bail determination (e.g., citations, summons), cases involving a probation or parole violation, warrants, and appeals are excluded. I also exclude cases missing information on bail hearings and on covariates.<sup>20</sup>

## Dependent Variables

I examine four judicial decision-making outcomes at the initial appearance: release on nonfinancial conditions, money bail set, bail amount, and detained without bail. *Nonfinancial release* is defined as release on either own recognizance (“ROR”) or unsecured personal bond (“UPB”) and is a dichotomous outcome, where the value is set to 1 for each case with an ROR or UPB release decision made at the initial pretrial determination, and 0 for all other release determinations. Money bail set is defined as the setting of cash bail or bond at the initial hearing, and takes a value of 1 if a judicial officer sets bail and 0 for all other outcomes. Bail Amount is defined as the dollar amount of bail set at the initial bail hearing. Bail amount is log transformed to remove the skew of the distribution (Fox, 2008), following other studies (Demuth, 2003; St. Louis, 2020; Wooldredge et al., 2017). This better reflects non-linearities in bail amounts – a \$100 increase at the low end of the distribution is likely to have a substantively different effect on defendants than a \$100 increase at the high end of the distribution.

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<sup>19</sup> On view arrests originate through an encounter between police and citizens that results in a custodial arrest. As noted by Scott-Hayward & Fradella (2019), from a bail determination standpoint, “on-view” arrests are circumstances where information regarding defendants is usually very limited at the pretrial stage; such cases provide the broad latitude for judicial officers to exercise discretion in applying applicable bail rules and procedures.

<sup>20</sup> The initial sample of new criminal cases between October 2014 and September 2019 consists of 311,079 cases. Nearly 14% of cases issued as a “statement of charges” were missing bail information; missing bail information is largely restricted to cases filed outside of the state’s three largest jurisdictions, Baltimore City, Montgomery County and Prince George’s County (which had fewer than 0.3% of cases missing bail information). The majority of cases missing bail information were issued a citation or summons following case filing. Of cases with bail information, an additional 10.5% of cases missing data on covariates were dropped (largely owing to missing information on offense type and prior history).

Bail amount is examined only for those individuals with money bail set at the initial bail setting.<sup>21</sup> Finally, I examine the use of preventative detention—a pretrial determination that results in a defendant being remanded to jail custody with no opportunity for release. Detained Without Bail takes a value of 1 for each case held in custody without bail at the initial appearance, and 0 for all other release determinations.<sup>22</sup>

## Covariates

In the main analyses, I control for individual demographic and case characteristics shown to influence pretrial determinations and outcomes in prior research. I include controls for *race* (Black, White, Asian/Pacific Islander, Indigenous/Native American, Other), where White is the reference category; defendant *age in years*, measured at the time of case filing; and *female* gender (where female is coded as 1 and male as 0). I control for case and criminal history characteristics, including *offense severity*, based on the Maryland Sentencing Guidelines charge severity scale; an indicator for *felony* offense (1 is felony; 0 all others); an indicator for *offense type* (person, property, drug, other), where property is the reference category; a *total count of charges* associated with the case, ranging from 1 to 40 (truncated to adjust for extreme outliers), and a *count of prior criminal cases filed*, a continuous variable ranging from 1 to 40 (truncated to adjust for extreme outliers).<sup>23</sup>

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<sup>21</sup> Cases released on ROR, UPB, and HWOB are not included in the bail amount analysis.

<sup>22</sup> This measure of detention captures judicial intent to ensure the pretrial incarceration of a given defendant; this is contrasted with other studies which examine pretrial detention measured as both those who are remanded to custody and those who have bail set, but are unable to post bond. In the MJCS data, I do not observe jail booking and release, nor is there complete information on bail amount posted. For this reason, I examine only judicial decision-making, rather than pretrial release outcomes.

<sup>23</sup> To create the criminal history measure, I use person-level identifying information, including name, date of birth, and sex to match people across cases in the data (using full data available from 2000-2019). Criminal history information is limited to cases occurring in the State of Maryland. 0.4% of cases had more than 40 prior criminal cases; 0.02% of cases had more than 40 charges on the current case.

## Empirical Approach

Maryland's bail reform was initiated by a formal letter sent from the state's Attorney General to the Court of Appeals raising constitutional concerns and urging specific, substantive changes to Maryland's pretrial practices. This letter was sent on October 25<sup>th</sup> 2016. Just weeks later, the Maryland Judiciary proposed a new set of procedural rules governing judicial determinations at the pretrial stage, including prohibiting judicial officers from setting financial conditions that resulted in pretrial detention solely due to a defendant's inability to pay. Following multiple public hearings, the rules went into effect on July 1<sup>st</sup> 2017. Importantly, the rule changes affected all defendants brought before commissioners and judges for bail hearings across the state at the same time. This poses a challenge for isolating the effects of bail reform on outcomes, as the policy change not only affected all jurisdictions across the state at the same time, but was also broadly tailored to all arrestees booked into custody, making the identification of a plausible in-time comparison group difficult.<sup>24</sup> However, this sudden change to the pretrial policy environment—initiated by the Attorney General's letter—provides natural experiment around which I may estimate the impacts of the reform. I treat the timing of the letter as exogenous to the outcomes of interest (i.e., pretrial release decisions), since the changes to pretrial policy were adopted in response to broad constitutional concerns on the affordability of bail and possible legal challenges to existing bail procedures.<sup>25</sup> The primary empirical strategy to estimate the

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<sup>24</sup> Here, there is no clear cross-sectional variation in treatment within Maryland. Moreover, comparable data on pretrial determinations are not readily available across other states or jurisdictions that might serve as a reasonable comparison group over the period in which Maryland reformed its pretrial system.

<sup>25</sup> As discussed in Chapter 1, concerns over pretrial practices across the state had been a long-standing issue, and the state had undertaken repeated, unsuccessful efforts at studying and proposing pretrial reforms since 1998, until the 2017 reforms (Colbert & Starger, 2022).

impact of the reform on key pretrial decision-making outcomes tests for discontinuities in outcomes in a short period around the initiation of bail reform (Carr & Packham, 2018; Hausman & Rapson, 2018; Ouss & Stevenson, 2023; Sloan et al., 2023).<sup>26</sup>

Regression discontinuity (RD) designs are considered one of the most credible quasi-experimental designs in the absence of random assignment to treatment conditions (Athey & Imbens, 2017). RD designs are applicable to settings in which the assignment into a given treatment is dependent upon a score on a running variable above or below a given threshold, or cutoff. This assignment rule implies that the probability of treatment assignment changes discontinuously at a known cutoff value. In a sharp RD setup, assignment<sup>27</sup> into treatment ( $T$ ) is deterministic and a discontinuous function of a running variable,  $X$ :

$$T_i = \begin{cases} 1 & \text{if } X_i \geq c \\ 0 & \text{if } X_i < c \end{cases}$$

where  $i$  represents each case and  $c$  is the known cutoff value. Units just above the cutoff are treated, while units just below are considered control units, whose outcomes serve as the counterfactual for treated units had they not been treated. The key assumption in the RD setting is based on continuity of average potential outcomes<sup>28</sup> across the cutoff. Stated another way, the assumption is that trends in potential outcomes in bail hearings would vary smoothly around the cutoffs absent any change in

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<sup>26</sup> This approach has been likened to a regression discontinuity in time, or “RDit”, as it shares similarities with traditional cross-section RD approaches, however, it also shares features with event studies or pre/post designs. For a full discussion, see Hausman & Rapson (2018).

<sup>27</sup> Here, “assignment” terminology is common in the RD literature, but does not necessarily imply that the researcher manipulated assignment to a treatment condition.

<sup>28</sup> Here, I follow potential outcomes notation, where  $Y(1)$  represents potential outcomes in a state of the world in which treatment occurred, and  $Y(0)$  represents potential outcomes where treatment did not occur (and distinct from observed outcomes  $Y_i$ ). A unit specific treatment effect is  $Y_i(1) - Y_i(0)$ . For any given unit, we can only observe one state of the world – either the unit received treatment, or they did not, but never both at once (Rubin, 1978).

treatment (Cattaneo et al., 2019). Formally, the average treatment effect is the vertical distance at the cutoff  $c$  is the treatment effect:  $E[Y_i(1) - Y_i(0) | X_i = c]$ . This quantity cannot be directly estimated; however, under continuity assumptions, the difference between the limits of average *observed* outcomes for treated (units just above the threshold) and control (units just below) units as the score approaches the cutoff is equal to the average treatment effect at the cutoff (Cattaneo et al., 2019).<sup>29</sup>

Here, I use time as the running variable, which determines assignment to treatment for bail hearings around the date of the policy announcement (cutoff date,  $c$ ): October 25<sup>th</sup> 2016.<sup>30</sup> Cases with bail hearings prior to the Attorney General’s letter (hereafter “policy announcement date”) are not exposed to the treatment, while bail determinations heard on or after the letter are considered exposed to the treatment. This temporal approach has been used in various contexts to evaluate effects of policy change, including cell phone bans (Burger et al., 2014), public benefits (Carr & Packham, 2018), and pretrial risk assessment implementation (Sloan et al., 2023). The basic model is articulated below:

$$Outcome_i = \beta_0 + \beta_1 Announcement_i + f(DaysfromCutoff_i) + Time_t + District_d + \varepsilon_i \quad (1)$$

where  $Outcome_i$  is one of four pretrial key outcomes (non-financial release, preventative detention, money bail set, bail amount) for each case  $i$ ;  $Announcement_i$  is a case-level indicator set to 1 for all cases with a bail hearing following the policy announcement (treated cases);  $District_d$  are fixed effects for county-level district courts and  $Time_t$  are day of the week fixed effects, which will capture any time-invariant court-specific effects

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<sup>29</sup>  $E[Y_i(1) - Y_i(0) | X_i = c] = \lim_{x \downarrow c} E[Y_i | X_i = x] - \lim_{x \uparrow c} E[Y_i | X_i = x]$

<sup>30</sup> In two prior analyses of Maryland’s bail reform, analysts have used the Attorney General’s letter as the “onset” of pretrial reform in lieu of the effective date of the bail rules, after observing change in pretrial determinations following the letter (see Glazener et al., 2023; Maryland Judiciary, n.d.)

along with any differences across days of the week. Finally,  $f(\text{DaysfromCutoff}_i)$  represents the running variable, or the number of days from the announcement of pretrial policy change. This variable is calculated as the difference between each case's initial bail hearing date and the policy announcement date, and is allowed to vary around the cutoff.  $\beta_1$  estimates the effect of the pretrial policy announcement on the outcomes of interest at the moment of the policy announcement. This approach generates an average treatment effect for cases near the cutoff date, providing an estimate of the immediate impact of the onset of bail reform. However, this approach has limited external validity for cases falling well before or after the cutoff dates.<sup>31</sup>

Several conceptual concerns apply to this approach. First, we may be concerned with the possibility of other changes occurring at the cutoff date that may affect the outcomes of interest. The continuity assumption implies that no other simultaneous treatments occur at the cutoff. I am not aware of any other policy changes or other shocks that impacted rates of pretrial release or detention, or substantially altered the composition of cases or defendants that occurred in the immediate period before or after the policy announcement.<sup>32</sup> Second, in traditional cross-sectional regression discontinuity designs, we may be concerned about individuals' ability to manipulate the assignment variable to select into (or out of) a treatment condition. Given that time is the

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<sup>31</sup> The continuity assumption described here is weaker than what is required for a pre/post analysis, which assumes that unobserved determinants of the outcome are not correlated with time; pre/post analysis that ignores time-varying confounding factors may yield biased estimates. The continuity assumption only requires that nothing changes discontinuously at the threshold date, other than the treatment (Hausman & Rapson, 2018).

<sup>32</sup> Unlike other states that also changed policies that impact the pretrial process or the composition of criminal cases, such as law enforcement citation in lieu of arrest, Maryland's bail reform was narrowly tailored to changing decision-making behavior of judicial actors. No other rule changes aimed at pretrial or court case processing were implemented at or around the time of the announcement of the policy change.

running variable in this approach, direct manipulation of the running variable is less of a concern (Hausman & Rapson, 2018). That said, it may be the case that actors alter their behavior in anticipation of the policy announcement date. In this context, in order to select into a more lenient bail reform regime, defendants would need to choose to commit crimes or be arrested on specific dates in anticipation of the announcement of pretrial policy change; likewise, law enforcement or court actors would need to modify their arresting behavior or court scheduling practices in order to select certain cases into (or out of) treatment in anticipation of the Attorney General's letter. Given Maryland's history of unsuccessful reform efforts, I do not expect that actors would specifically anticipate this policy change in advance. To examine the possibility of selection around the policy announcement date, I examine the continuity (or smoothness) in the volume of bail hearings around the announcement date, as well as test for possible discontinuities across all case-level covariates known to influence pretrial outcomes (e.g., charge severity, charge type, demographic characteristics, and criminal history).

Anticipatory or avoidance behavior might be of concern if I use the effective date of the bail rules (i.e., July 1 2017). The Attorney General's letter, proposed rule changes, and subsequent rule issuance were widely-advertised among the judiciary as well as the broader public (Snyder, 2016; Wiggins, 2016). Because of this, it is plausible that judicial officers could alter their behavior in setting bail conditions in advance of the actual date on which the new bail rules were made effective law. Prior reports document changes in bail setting practices at initial appearances in the months after the Attorney General's letter but before the final rules went into effect (Blumauer et al., 2018). If this is the case, the period immediately predating bail reform's effective date would no

longer be “untreated,” and the presence of anticipation effects would likely lead to an underestimate of the true effect of bail reform. I circumvent this by using the date of the Attorney General’s letter as the cutoff date in the discontinuity models. Consequently, the estimate generated here is most aptly described as an “intent-to-treat” effect of the announcement on pretrial outcomes,<sup>33</sup> rather than an estimate of the effect of the rule change.

I estimate local linear regressions within a given bandwidth (or “neighborhood”) on either side of the cutoff to generate estimates of the effect of the policy announcement on four outcomes (Cattaneo et al., 2019).<sup>34</sup> In RD approaches, the selection of a bandwidth generally involves a trade-off between bias and variance: narrower bandwidths may yield lower bias in estimates of the effect, but have higher variance owing to smaller sample sizes nearer to the cutoff (as there are fewer cases heard within shorter periods close to the announcement date). Conversely, larger bandwidths may lower variance (through larger sample sizes) but risk increasing bias, undermining the credibility of the continuity assumption. Following Calonico et al. (2014), I employ a data-driven approach to select the bandwidth size, using mean-square error (MSE) optimal bandwidths for each outcome. I further test the robustness of the results by estimating average effects along a range of bandwidth sizes. Finally, we may be concerned that unobservable variables are correlated with the running

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<sup>33</sup> In certain bandwidth specifications, estimates reflect a compound effect of any anticipatory behavior as well as the effect of the rule changes. Additionally, because the policy reform is aimed at modifying judicial behavior in a context where broad discretion applies, I make assumptions about judicial exposure to bail reform, but cannot know whether a given judicial officer has received the treatment, as in a typical RD set up. Here, the estimates reflect an “intent-to-treat” estimate of the effect of onset of bail reform, although a formal fuzzy discontinuity design is not possible in this context.

<sup>34</sup> I estimate separate weighted linear regressions on each side of the cutoff, using optimal bandwidths and a triangular kernel. I also separately explore whether allowing the running variable to vary quadratically impacts estimates of the effects.

variable (time) and have discontinuous impacts on potential outcomes; I include day of the week fixed effects to capture variation across days in unobservables, and county fixed effects to capture time-invariant variation across courts.

One limitation to the above approach is that discontinuity designs estimate a localized average effect of the policy on cases with bail hearings close to the announcement date. To recover the average treatment effect of the policy during periods farther away from the announcement date, we would need to assume a constant treatment effect over time. However, there may be reason to doubt that short-run impacts will remain stable over a longer period: for example, Stevenson (2018a) found that judges reverted to pre-reform patterns of pretrial release decision-making in her study of a Kentucky risk assessment reform. To explore the possibility of longer-run impacts or changes over time in the impacts of the policy, in supplementary analyses, I conduct an event study analysis, regressing each outcome on indicators of months before and after the policy announcement, controlling for individual demographic and case characteristics outlined above, along with court and time fixed effects.

## ***Results***

In this section, I first present summary statistics on the sample used in these analyses, and present tests of the continuity assumption underpinning the discontinuity approach, examining the smoothness of cases and observed covariates across the policy announcement threshold. I then present estimates of the impact of pretrial reform on pretrial decision-making.

## Sample Characteristics and Tests of Continuity Assumption

Table 1 presents summary statistics for each defendant in the sample between October 2014 and September 2019. Across the sample, most defendants are male (79%), Black (60%), and, on average, 33 years old. Most defendants were charged with a misdemeanor offense as the most serious charge (71%), with just 29 percent facing a felony charge. Person offenses were the most common type of charge (43%), followed by drug (31%) and property offenses (25%). On average, defendants in the sample were charged with 2.9 counts in the criminal case.

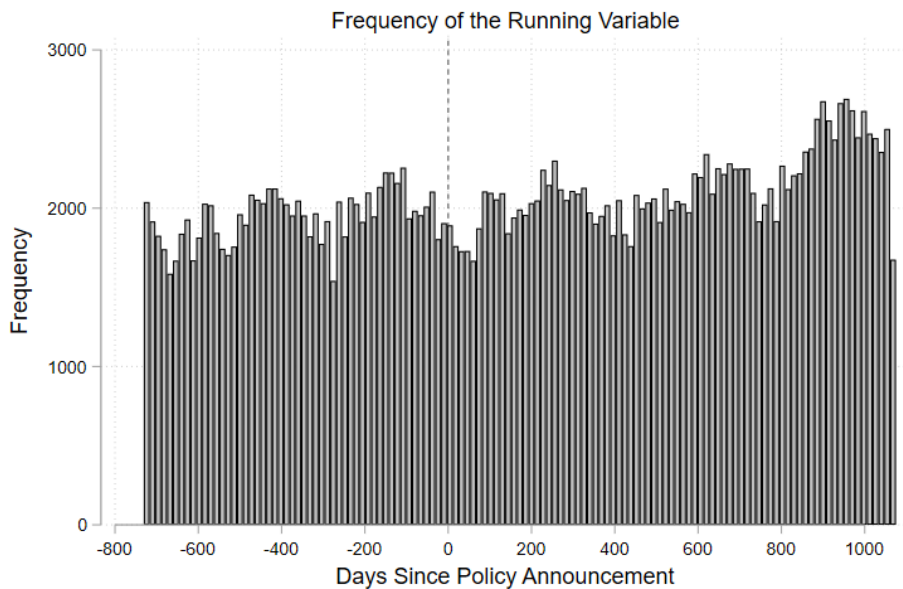
Table 1. Summary Statistics (N = 237,333)

<b>Variable</b>	<b>Mean</b>	<b>SD</b>	<b>Min</b>	<b>Max</b>
Nonfinancial Release	0.47	0.50	0	1
Money Bail Set	0.36	0.48	0	1
Detention without Bail	0.16	0.37	0	1
Log Bail Amount	8.96	1.38	1.25	15.42
Age at Filing (Years)	32.96	11.52	14.09	99.61
Female	0.21	0.41	0	1
Defendant Race: White	0.38	0.49	0	1
Defendant Race: Black	0.60	0.49	0	1
Defendant Race: Other	0.02	0.14	0	1
Offense Type: Property	0.25	0.44	0	1
Offense Type: Person	0.43	0.50	0	1
Offense Type: Drug	0.31	0.46	0	1
Offense Type: Other	0.01	0.09	0	1
Number of Charges	2.94	2.73	1	40
Charge Severity Scale	3.57	1.65	1	8
Felony Charge Indicator	0.29	0.45	0	1
Number of Priors	4.93	6.82	0	40

One central threat of the discontinuity approach is selection into treatment, or manipulation of the running variable. Common tests for manipulation in a cross-sectional setting are not applicable here, given that the running variable (time) cannot

be manipulated (Hausman & Rapson, 2018). Given the unexpected nature of the policy announcement, I argue it is unlikely that defendants, judicial officers, or other actors could have perfectly manipulated the timing of their arrests or bail hearings around the announcement date. Selection into (or out of) treatment may be apparent where unusually large decreases or increases in the volume of bail hearings are observed at dates near the cutoff date. In Figure 2, I present visual evidence that the number of bail hearings does not vary discontinuously at the policy announcement threshold.

Figure 2. Frequency of the Running Variable (Bail Date) around the Policy Announcement Cutoff



To further test the continuity assumption—which requires that all observed and unobserved factors that affect pretrial decisions are smooth across the policy announcement threshold—I conduct placebo studies using observed case-level and defendant factors as outcomes. I separately estimate model 1 using race, gender, age at filing, offense type, offense severity, number of charges, and prior criminal cases filed

as outcomes.<sup>35</sup> These covariates should not be affected by the policy announcement; if the RD design is valid, I should not find significant estimated effects at the cutoff. However, if the covariates that are known to strongly correlate with pretrial outcomes are discontinuous at the cutoff, the continuity assumption is unlikely to hold and the validity of this approach (and the estimates yielded) would be undermined (Cattaneo, et al., 2020). Figures 3 and 4 display the graphical results of the placebo studies, while Appendix Table 1 presents the point estimates for the estimated effects at the threshold. In all but one of the tests, the observed defendant and case characteristics do not vary discontinuously at the cutoff (policy announcement date). The estimated coefficient for number of charges is very small, but statistically significant using conventional standard errors; however, using robust bias-corrected standard errors, the difference is no longer statistically significant. These tests support the validity of the application of the discontinuity approach in this setting.

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<sup>35</sup> I control for day-of-week and county fixed effects, in addition to the running variable.

Figure 3. Regression Discontinuity Estimates: Age, Gender, Race

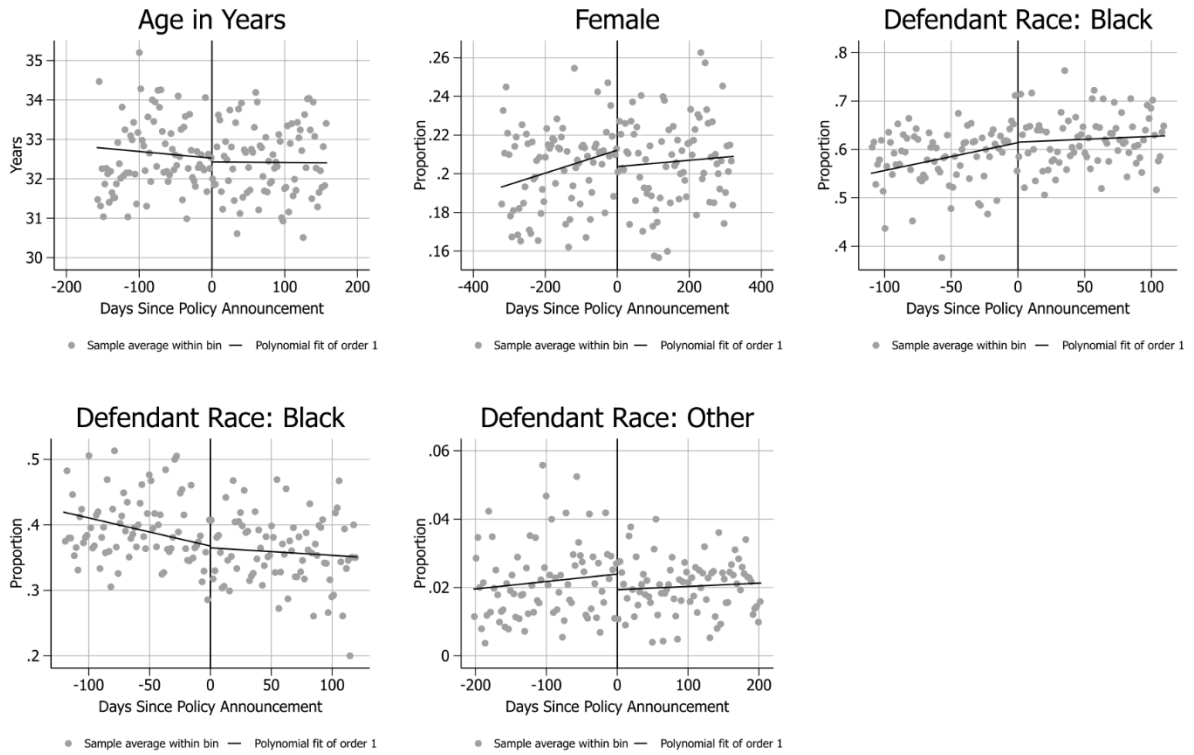
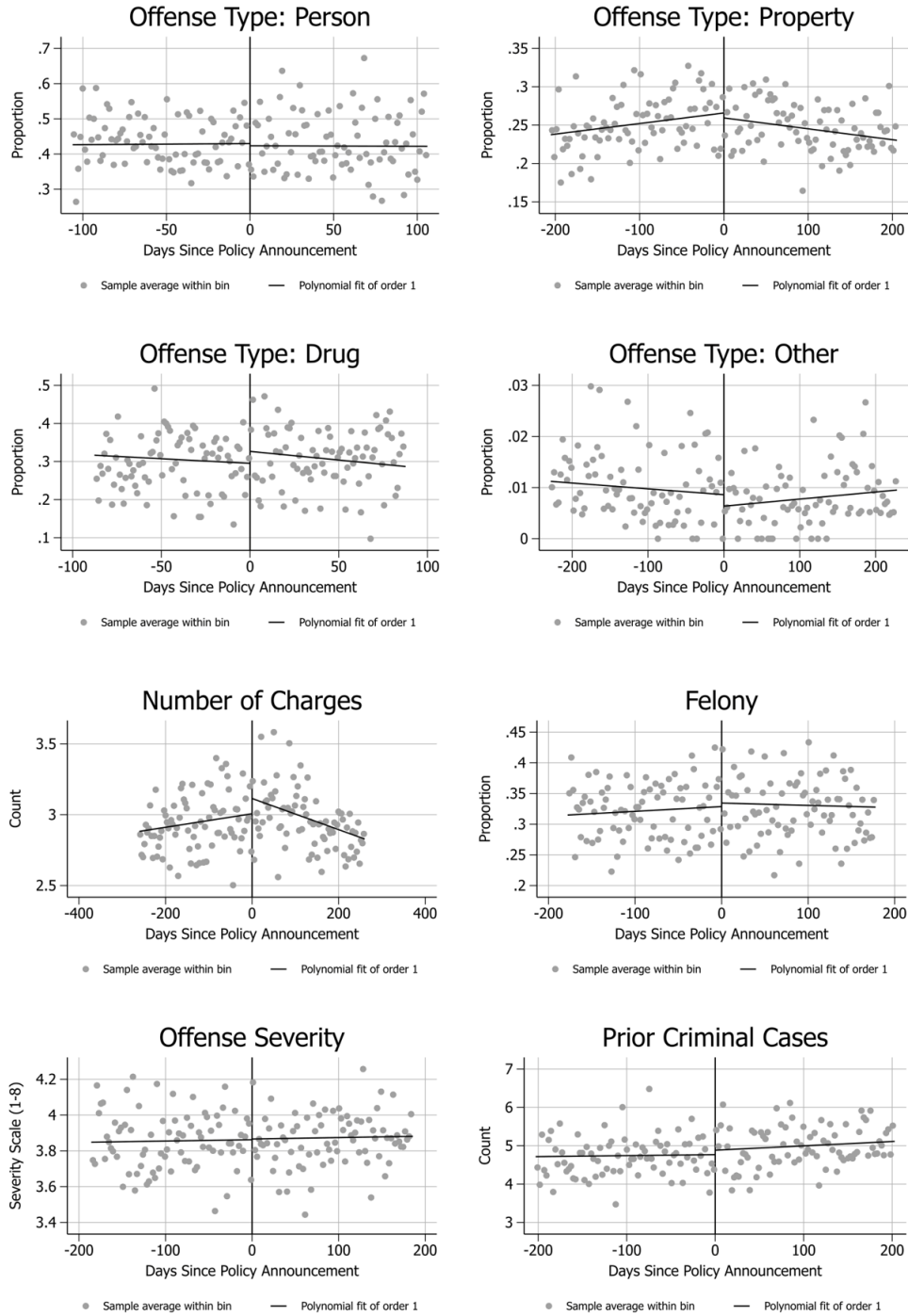


Figure 4. Regression Discontinuity Estimates: Offense Type, Severity, Prior History



# Discontinuity Estimates of the Effects of Bail Reform on Pretrial Decision-Making

## Bail Setting Behavior

The aim of this analysis is to examine whether bail reform affected pretrial decision-making in Maryland. Because the primary aim of the reform was to reduce the use of cash bail, I begin by discussing results with respect to money bail setting behavior. Figure 5 presents visual evidence of the key discontinuity results, plotting binned sample means within optimal bandwidths for each outcome. For each main outcome, the plots show clear discontinuities in the predicted probability of receiving bail, being released without financial condition, and detained without bail at initial appearance. Further, I observe a clear discontinuity in the log bail amounts.

Figure 5. Regression Discontinuity Estimates: Pretrial Decisions

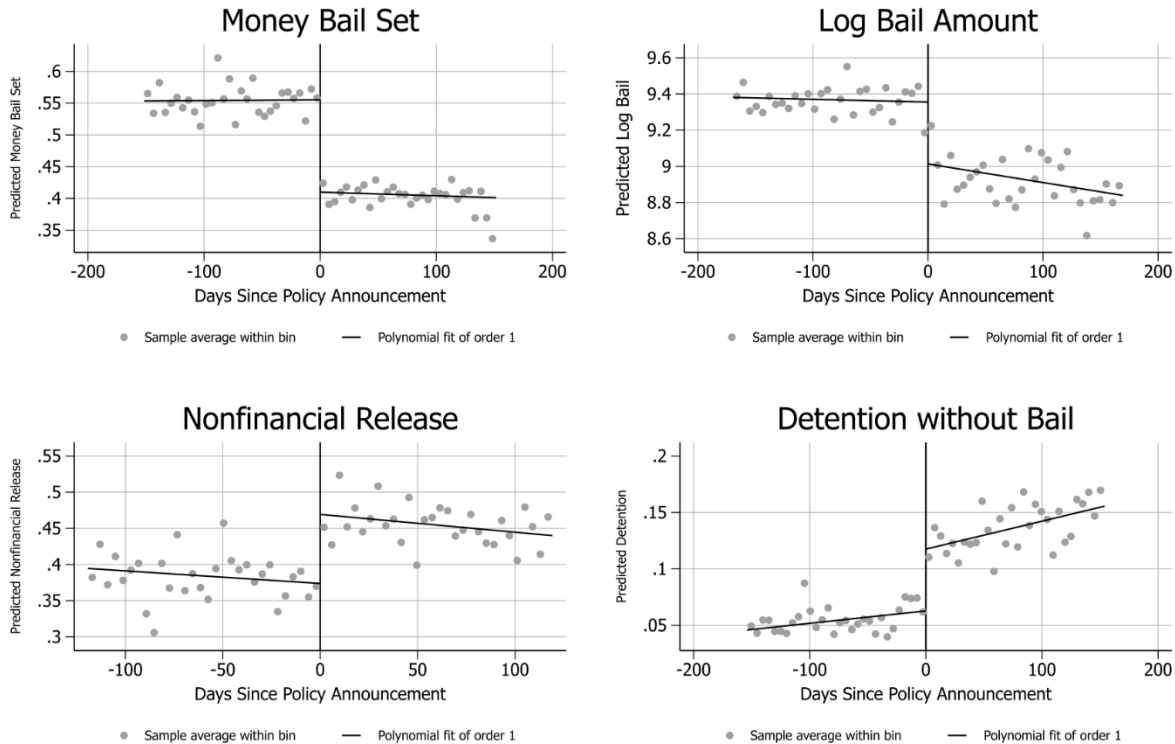


Table 2 presents point estimates from separate regressions for money bail set and logged bail amounts. Column 1 presents results using optimal (MSE) bandwidths (Calonico et al., 2014), the preferred specification, while remaining columns re-estimate the regressions varying the size of the bandwidth, for half of the MSE-optimal bandwidth (Column 2), 1.5 times the optimal bandwidth (Column 3) and double the optimal bandwidth (Column 4).

Results in Table 2 reinforce the conclusions drawn from Figure 5. I find that pretrial reform had immediate and substantial impacts on pretrial decision-making at the initial bail hearing. First, and most directly related to the aims of pretrial reform, I find that money bail setting practices change dramatically following the reform's announcement: pretrial reform resulted in a decline of 13.9-15.7 percent in the predicted probability of receiving bail. While the specific point estimates vary slightly across bandwidth size, the estimated effects are similar in size, direction, and statistical significance. Likewise, for cases where bail was set, I find that pretrial reform led to a decrease in the amount of money bail set, ranging from 19.2-32.8 percent decrease ( $b = -0.213$ ,  $b = -0.384$ ) in bail amounts. Like money bail setting, effect size estimates for logged bail amount are robust to changes in bandwidth size, with the smallest estimate yielded from the shortest bandwidth (although this is less precisely estimated), and relatively stable effect size estimates across longer periods.

Table 2. Regression Discontinuity Results: Money Bail Setting and Log Bail Amount

	Optimal Bandwidth (1)	0.5x Optimal Bandwidth (2)	1.5x Optimal Bandwidth (3)	2x Optimal Bandwidth (4)
<b>Outcome: Money Bail Set</b>				
Post-Announcement	-0.153*** (0.0121)	-0.139*** (0.0232)	-0.148*** (0.0134)	-0.157*** (0.0116)
Bandwidth	150.29	75.15	225.44	300.58
Effective Number of Cases	34797	16570	53718	72868
<b>Outcome: Log Bail Amount</b>				
Post-Announcement	-0.361*** (0.0424)	-0.213** (0.0748)	-0.362*** (0.0436)	-0.384*** (0.0378)
Bandwidth	171.18	85.59	256.76	342.35
Effective Number of Cases	19147	9093	28684	37206

*Notes: Each cell shows results for a separate regression. All models include day of week fixed effects, county fixed effects, along with individual case and demographic covariates. Robust standard errors are in parentheses. Column 1 uses optimal (MSE) bandwidths (Calonico et al., 2014), while columns 2-4 use bandwidths that are one-half, one-and-one-half, and double the size of the optimal bandwidth in Column 1. Models for log bail amount only include cases where bail is set.*

### *Nonfinancial Release and Preventative Detention*

Table 3 presents results for two additional outcomes of interest, nonfinancial release, and detention without bail. In line with the intended goal of the announced reforms—to encourage the use of the least restrictive conditions necessary to ensure appearance—the announcement of pretrial reforms led to an 8.8-10.9 percent increase in the predicted probability of nonfinancial release, a finding that is robust to changes in bandwidths. However, the announced reforms led to a statistically significant increase in the probability of detention without bail of between 3.9 and 5.1 percent. These findings are consistent with other reports on the impacts of Maryland’s reform (Blumauer et al., 2018; Maryland Judiciary, n.d.).

Table 3. Regression Discontinuity Results: Nonfinancial Release and Detention without Bail

	Optimal Bandwidth (1)	0.5x Optimal Bandwidth (2)	1.5x Optimal Bandwidth (3)	2x Optimal Bandwidth (4)
<b>Outcome: Nonfinancial Release</b>				
Post-Announcement	0.106*** (0.0126)	0.0879*** (0.0245)	0.105*** (0.0141)	0.109*** (0.0122)
Bandwidth	118.087	59.044	177.131	236.174
Effective Number of Cases	26978	13040	41556	56756
<b>Outcome: Detention without Bail</b>				
Post-Announcement	0.0479*** (0.00732)	0.0391** (0.0131)	0.0447*** (0.00783)	0.0506*** (0.00678)
Bandwidth	146.699	73.350	220.049	293.399
Effective Number of Cases	33825	16116	52466	71120

*Notes: Each cell shows results for a separate regression. All models include day of week fixed effects, county fixed effects, along with individual case and demographic covariates. Robust standard errors are in parentheses. Column 1 uses optimal (MSE) bandwidths (Calonico et al., 2014), while columns 2-4 use bandwidths that are one-half, one-and-one-half, and double the size of the optimal bandwidth in Column 1, respectively.*

## Robustness Tests

I assess the robustness of these results to different specifications of the main equation for each of the four outcomes of interest (Table 4). To examine whether estimated results are sensitive to the functional form imposed on the models, I re-estimate the main models above, allowing the running variable to vary quadratically (Panel A).<sup>36</sup> Plots of the quadratic models are in Appendix Figure 1. Next, I examine the sensitivity of results to the inclusion of covariates, estimating models controlling only for the running variable and no other covariates in both local linear (Panel B) and quadratic (Panel C) specifications. Optimal bandwidths are used to determine the samples for

<sup>36</sup> Out of concern for overfitting, I do not include higher-order polynomial controls in time (Gelman & Imbens, 2019).

each regression. Results across both linear and quadratic models, as well as models that exclude all covariates, are largely consistent, in magnitude and sign, with the effect estimates across all four pretrial decision outcomes above.

Table 4. Robustness Tests

	<b>Nonfinancial Release</b>	<b>Money Bail Set</b>	<b>Log Bail Amount</b>	<b>Detention without Bail</b>
<b>Panel A: All Covariates, Quadratic</b>				
Post-Announcement	0.108*** (0.0144)	-0.145*** (0.0150)	-0.356*** (0.0486)	0.0411*** (0.00852)
Bandwidth	223	228	269	211
Effective Number of Cases	53249	54560	29921	50130
<b>Panel B: No Covariates, Linear</b>				
Post-Announcement	0.0670*** (0.00950)	-0.148*** (0.0138)	-0.345*** (0.0512)	0.0539*** (0.00743)
Bandwidth	332	155	196	177
Effective Number of Cases	80938	35765	22013	41556
<b>Panel C: No Covariates, Quadratic</b>				
Post-Announcement	0.103*** (0.0155)	-0.150*** (0.0150)	-0.280*** (0.0648)	0.0534*** (0.00812)
Bandwidth	233	261	213	293
Effective Number of Cases	55921	63225	23927	70872

*Notes: Each cell shows results for a separate regression. Panel A models include day of week fixed effects, county fixed effects, along with individual case and demographic covariates. Panel B and C do not include covariates, controlling only for the running variable. Panel B allows the running variable to vary linearly on either side of the cutoff, while Panel C allows the running variable to vary quadratically. Robust standard errors are in parentheses. All models use optimal (MSE) bandwidths (Calonico et al., 2014).*

## ***Do Impacts Persist?***

Discontinuity approaches, by design, estimate local effects in a bandwidth around the cutoff. However, short-run impacts may not hold over time (e.g., Stevenson, 2018a), as judicial officers and others adjust to new policy (or revert to prior practices), or as other factors affect pretrial outcomes. Figure 6 below presents graphical results

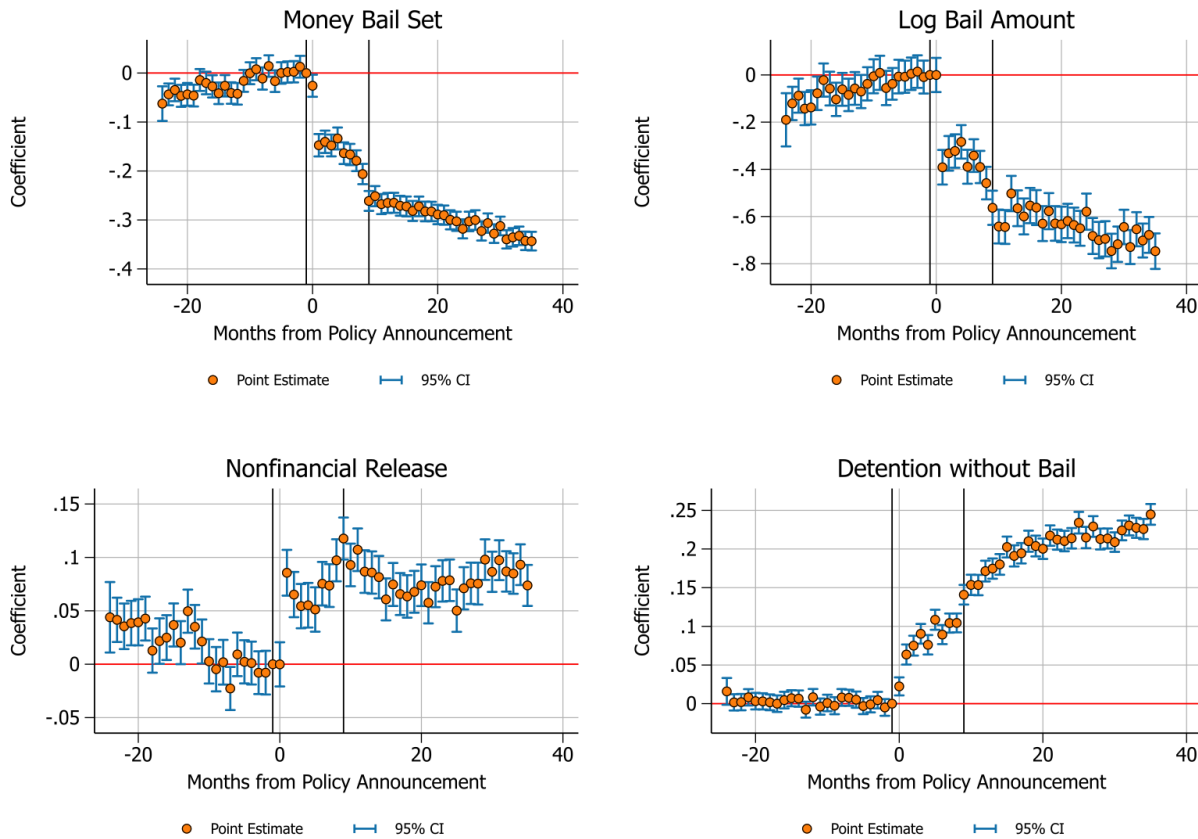
from separate event study models for each of the four pretrial outcomes.<sup>37</sup> In each panel, I denote two key time points with vertical lines: the first line (one month prior to the policy announcement) indicates the reference period for each of the monthly coefficients, while the second line indicates the month in which the bail rules were made effective (July 2017). This allows me to examine how the impacts on pretrial release decisions evolve over a longer time span.

Results suggest that for monetary bail setting behavior outcomes, as well as detention without bail, judicial decision-making did not revert to pre-reform practices in the three years following the onset of bail reform. Controlling for individual and case factors, along with county and day of week fixed effects, initial declines in money bail setting behavior—both in terms of the use of cash bail as a pretrial condition, along with the amount of bail set—continue to fall after rules were made effective law. In contrast to the aims of the reform, the use of detention without bail appears to increase steadily in the subsequent three years following the reform announcement, consistent with more recent public reporting (O’Neill, 2025). Finally, judges increased the use of nonfinancial release immediately following announced reforms; however, as the figure demonstrates, I find some suggestive evidence that impact of the new bail rules on the use of nonfinancial release abate over time.

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<sup>37</sup> I regress each outcome on a series of monthly indicators before and after the policy announcement, controlling for race, age, gender, offense type, number of charges, felony charge indicator, offense severity scale, prior criminal history, as well as fixed effects for day of the week and county (i.e., court). Each coefficient represents the predicted change in the outcome, net of above factors, from the reference month—September 2016.

Figure 6. Event Study Plots for Money Bail Set, Log Bail Amount, Nonfinancial Release and Detention without Bail



*Note: Each figure plots coefficients from separate regression of each outcome on indicators for months before and after the policy announcement. Individual controls for race, gender, age, charge severity, felony charge, number of charges, and prior history are included, along with fixed effects for day-of-week of the bail hearing and county. Each plot has a vertical line at the reference month (-1) and at the month in which the bail rules became effective (9), relative to the policy announcement.*

## Discussion and Limitations

Money bail, and the commercial bail bond industry, is often considered one dimension of the “predatory” nature of the criminal justice system, extracting resources from communities (Page & Soss, 2021). In this view, bail serves less as a rational mechanism to ensure pretrial appearance, and more as one that in its impacts, operates primarily to punish poverty (Scott-Hayward & Fradella, 2019). A key aim of Maryland’s bail reform was to move away from a pretrial system where cash bail serves the primary

mechanism for criminal defendants to secure release and where unnecessarily high bail amounts are routinely set. In that aim, this study shows that Maryland's bail reform was largely successful. Using a discontinuity approach, I find that the initiation of pretrial reform in Maryland resulted in a decrease in the probability of receiving money bail by 13.9-15.7 percent, and where bail is set, led to a 19.2-32.8 percent decrease in average bail amounts. Further, I find that bail reform led to an immediate 8.8-10.9 percent increase in the probability of nonfinancial release.

At the same time, Maryland's shift away from cash bail is accompanied by a smaller, but substantial and immediate, increase in the use of preventative detention. Moreover, longer-term trends suggest that the use of preventative detention has increased dramatically over time, a finding consistent with other reporting on Maryland's reform (Blumauer et al., 2019; O'Neill, 2025). These findings paint a mixed picture of whether Maryland's bail reform has achieved its larger aims of reducing pretrial detention across the state. While other bail reform initiatives have successfully moved away from cash bail, increasing rates of pretrial release with either null impacts on (Ouss & Stevenson, 2023) or even reductions in pretrial incarceration (Golub et al., 2019; Heaton, 2022), this study suggests that Maryland's courts appear to move in the opposite direction. While I cannot compare actual jail detention outcomes across periods,<sup>38</sup> aggregate jail statistics suggest that the proportion of people pretrial across Maryland has increased in the years following the reform (O'Neill, 2025). Aggregate data from the Governor's Office on Crime Prevention and Policy show that as jail

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<sup>38</sup> I do not observe actual pretrial release from custody; available court data reflect only bail hearing outcomes. For defendants with money bail set, available data limit my ability to observe whether bail was posted and the defendant was released from jail.

populations fell from 11,883 to 8,639 between 2014 and 2017, the proportion of the jail population that is pretrial was stable, averaging 68.2 percent (Maryland Governor's Office of Crime Prevention and Policy, 2024). Following bail reform in 2017, this patterned changed: statewide jail populations remained somewhat stable, while the proportion of those held pretrial increased: from an average of 68.6% in 2017 to 75.1% by 2019 (and increasing even further in 2020) (id). This suggests that bail reform did very little to impact the overall rate of pretrial detention in the state. This is likely not without consequence to defendants. Prior research suggests that pretrial detention has adverse effects on conviction and sentencing, resulting in a greater likelihood of pleading guilty (Gupta et al., 2016), conviction (Stevenson, 2018b), and being sentenced to jail (Heaton et al., 2017; Oleson et al., 2016). Though beyond the scope of this study, future research should examine whether, and how, the bail reform initiative impacted conviction and sentencing outcomes for defendants.

One observer of Maryland's bail reform quipped that "it felt as though judges 'went into the proverbial smoke-filled room' after bail reform took effect and decided to begin denying bail in cases where they previously would have issued cash bail" (O'Neill, 2025). Policy reform that hinges on the exercise of judicial discretion to achieve social aims may be limited in its impacts (e.g., Ouss & Stevenson, 2023). Scholarship on legal change points to the importance of individual discretion and normative factors in how law is applied (Lynch & Omori, 2014; Verma, 2015) and how reform may be subverted (Steiner et al., 2011; Rengifo et al., 2017). Buy-in from frontline workers is critical to successful implementation of public policy (Lipsky, 1980). My findings suggest that district court commissioners, as frontline court staff responsible for applying the new

rules, responded to bail reform in ways that both complied with new requirements of the law and undermined its larger intent. Nothing in the court rules barred courts from imposing cash bail; the immediate drop, and steady decline over time, in use of money bail may signal that judicial officers were responsive to larger policy goals of reducing reliance on money bail. However, the effects on nonfinancial release, which appear to dampen over time, coupled with sustained increase in the probability of preventative detention, point to possible implementation challenges with the new bail policy.

Added workload burdens and ambiguity in how to comply with new rules are often identified as issues leading to implementation gaps and variation in outcomes (Rengifo et al., 2017; Edelman, 1992). In court contexts, informational ambiguity and time-burdens are thought to generate conditions where cognitive attributions and perceptual short-hands influence judicial decision-making, and lead to disparate outcomes (Steffensmeier et al., 1998; Albonetti, 1991); in studies of reforms aimed at reducing punitive consequences of criminal justice involvement, the removal of information led to counter-intuitive and even adverse results in a hiring context (see e.g., Raphael, 2021). Here, bail reform imposed new requirements on judicial officers to assess the affordability of money bail imposed, but provided only minimal guidance on sources of information on defendants' finances and vague legal standards on what constitutes an affordable amount of bail (Maryland Office of Public Defender, 2018). In this sense, reductions in the likelihood of setting cash bail may partially reflect an avoidance of the additional workload burdens placed on judicial officers under the new rules. Moreover, Maryland's pretrial policy articulates only vague notions of "dangerousness," leaving questions on how to define harm and how to assess the

likelihood of those harms within the purview of each court to assess (Stevenson & Mayson, 2022). While bail reform did not alter rules on preventative detention, nor did it provide additional tools or resources to guide judicial assessments of risk. Research suggests that judges may behave in ways that are risk-averse at the pretrial stage, as they are subject to “asymmetric” blame for defendant misconduct in the community at the pretrial stage (Ouss & Stevenson, 2023; Page & Scott-Hayward, 2022). The shift from money bail, and particularly the decline in setting high bail amounts, toward the use of preventative detention is consistent with avoidance of added burdens on setting affordable cash bail, as well as with more conservative decision-making under conditions of ambiguity.

This study was not designed to explain the reasons for these findings or to test the mechanisms outlined above; I provide these as future directions for studies of bail policy implementation. Ethnographic and other qualitative methods are well-suited to explore the ways in which judicial officers and other court actors understand, experience, and apply the bail rules (e.g., Winter, 2023). This type of research would yield deeper understanding of the mechanisms that produce the results found here, and may provide new clarity on the sets of policy design and implementation challenges that jurisdictions should address when implementing new bail policy.

## **CHAPTER 3: ASSESSING THE IMPACT OF BAIL REFORM ON CRIME IN MARYLAND**

Concerns about the effectiveness of bail and the harms of pretrial detention have catalyzed policymakers to make significant changes to pretrial law and policy throughout the country, including in Maryland, New York, California, Illinois, New Mexico, New Jersey, Kentucky, and in many county and city jurisdictions (Pretrial Justice Institute, 2019). Though reforms vary, they aim to reduce pretrial detention and decrease reliance on cash bail as the primary mechanism for obtaining release during the pretrial period (Jorgensen & Smith, 2021). While many welcome such reforms, critics counter that changing existing bail policies puts public safety at risk (Southall & McKinely, 2020; Shea, 2020). Indeed, opponents have even found success in rolling back reforms (Sterne, 2023). Despite heated debate, the limited research to-date suggests that reforming pretrial policy does not increase crime or overall reoffending (Stemen & Olson, 2023; Wu & McDowall, 2024; Zhou et al., 2023). I add to this small, but growing evidence base. In this chapter, I conduct an empirical evaluation of the impact of Maryland's 2017 bail reform on crime. Using state-level Uniform Crime Report data from 1985-2019, I conduct a synthetic control group analysis to estimate the effects of bail reform on index crime, violent crime, and property crime. I organize this chapter as follows: first, I discuss the theoretical pathways through which bail reform may impact crime outcomes. Next, I review empirical evidence on the impacts of pretrial detention on crime and the effects of bail reform on recidivism and general crime. I then discuss the data and empirical approach used in the current study, and present the key

findings on the effects of bail reform on crime in Maryland. I conclude with a discussion of study limitations and suggestions for future directions.

### ***Crime Impacts of Pretrial Detention***

Pretrial detention can decrease criminal activity through multiple pathways, including incapacitation and general deterrent effects. Most directly, detention while awaiting trial may impact crime through an incapacitation effect: those who are detained in custody have less opportunity to commit new crimes compared to those who are released.<sup>39</sup> By expanding opportunity for release during the pretrial period, bail reform may influence crime by reducing the incapacitation effect of detention. Another mechanism by which bail reform may influence crime is through general deterrence. Here, a would-be law breaker might refrain from committing crime out of fear of being caught and punished (Loughran, Paternoster, Weiss, 2015). The threat of pretrial jail incarceration may deter future criminal activity by increasing the expected costs associated with committing crime (Becker, 1968; Yang, 2017). Research suggests that sanctions that are swift and certain may have a larger impact on deterrence than sanctions that are severe (Chalfin & McCrary, 2017); being detained in jail prior to case disposition imposes a sanction more immediately, and perhaps with more certainty, than post-disposition punishments. A reform that reduces the probability of pretrial detention and expands opportunities for pretrial release may lessen the potential general deterrent effect of pretrial incarceration. Pretrial incarceration, like postconviction imprisonment, may also have criminogenic impacts (Loeffler & Nagin,

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<sup>39</sup> Of course, people who are incarcerated may also commit new crime during their incarceration. In studies to date, in-custody reoffending is generally omitted from discussions of how subsequent reoffending is captured.

2022).<sup>40</sup> Most empirical research on bail and pretrial detention focuses on assessing impacts on reoffending or recidivism—more in line with theoretical questions of incapacitation and criminogenic effects of incarceration—while fewer studies explore its impacts on general crime.

Correlational research links pretrial detention to pretrial misconduct and reoffending, including increases in failure to appear (Monaghan et al., 2022) and new criminal arrests (Lowenkamp, 2022). Identifying a causal effect of pretrial detention on reoffending is complicated; defendants held in jail pretrial differ systematically from those who obtain release. Recent studies seek to overcome this methodological challenge by employing quasi-experimental designs, including instrumental variables and difference-in-difference designs. These studies find consistent evidence of little net effect of pretrial detention on future crime. Instead, the literature finds that pretrial detention may yield both incapacitative and criminogenic impacts, where those released pretrial are more likely to be rearrested during the pretrial period, but less likely to reoffend following disposition (Dobbie et al., 2018; Gupta et al., 2016; Leslie & Pope, 2018; Stevenson, 2018b).

In a study of pretrial release in Philadelphia, PA, and Miami-Dade County, FL, Dobbie, Goldin and Yang (2018) find that pretrial release increases the likelihood of rearrest during the pretrial period by 18.9 percentage points, providing evidence for a short-term local incapacitation effect for defendants who would have been detained had they been assigned a more punitive judge. Authors also find an offsetting decrease in

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<sup>40</sup> Though not examined here, Loeffler and Nagin (2022) find that the evidence for a recidivism effect of postconviction imprisonment is, at best, mixed, while they conclude that pretrial detention most likely has deleterious impacts on reoffending.

the likelihood of rearrest after the disposition of their cases, concluding that pretrial release leads to no discernable net impact on new crime. Leslie and Pope (2018) find similar effects in their study of pretrial detention in New York City: being detained pending trial reduces the probability of rearrest during the pretrial period by 12.2 percentage points for felony defendants and by 10.6 percentage points for misdemeanor defendants. This pretrial incapacitation effect is mostly offset by increases in the probability of rearrest within two years following disposition for both groups. Other studies find that pretrial detention is associated with an increase in the likelihood of committing future crime (Heaton et al., 2017). Whatever incapacitation benefits yielded from pretrial incarceration may be washed out by adverse consequences of incarceration on case outcomes through higher rates of guilty pleas and convictions (Dobbie et al., 2018; Peterson, 2019), as well as through disruption to defendants' social and economic lives (Dobbie et al., 2018; Turney & Connor, 2019). Defendants held until disposition often lose their jobs, housing, and other stabilizing forces (Turney & Connor 2019), as well as suffer from later reductions in employability via higher conviction rates and worse criminal records (Pager 2003; Uggen et al., 2014). These criminogenic factors may lead defendants to commit more crime, rather than less, following disposition.

### ***Crime Impacts of Bail Reform***

Evaluations of changes to pretrial law and policy have found little, if any, substantive impacts on either reoffending or aggregate crime rates (Stemen & Olson, 2023). Reports examining recidivism rates before and after bail reforms have observed declines in reoffending (Office of Chief Judge Circuit Court of Cook County, 2019). One

study of Cook County, Illinois' bail reform found no significant association between the reform and odds of new criminal activity or new violent criminal activity, after controlling for case and demographic factors (Stemen & Olson, 2020). Consistent with broader findings on the impact of pretrial detention, several studies of bail reform initiatives also suggest a small incapacitation effect associated with pretrial detention. A Harris County, TX, reform expanding pretrial release for misdemeanor crimes was associated with small, though statistically insignificant, increases in short-run recidivism for defendants exposed to the reform (Heaton, 2022). Similarly, a study of a Kentucky reform finds evidence of a slight increase in rates of rearrest pending disposition following the law's implementation (Stevenson 2018a); however, the author cautions that the increase is likely the result of a general upward trend in pretrial rearrest that predated the law. Lacoë, Skog, and Bird (2024) find no significant change in the probability of re-arrest within 6 months for San Francisco defendants released pretrial following a court decision requiring that bail be set according to ability to pay.

Turning to general crime impacts, limited research to date suggests that bail reform has no significant effect on aggregate crime. Most reports examining the impact of bail reform on crime rates conduct descriptive comparisons of crime rates before and after the reform, finding decreases or negligible increases in crime rates (e.g., Herring, 2020; Staudt, 2023). Such pre-post comparisons are problematic, as it is difficult to isolate the changes in crime attributable to bail reform absent rigorous controls. That said, studies employing more sophisticated designs reach similar conclusions. Two studies examine general crime impacts of bail reform in New York State, which overhauled its pretrial system in 2020, eliminating cash bail for many

offenses. Zhou et al. (2023) examine short-run effects of this bail reform on crime rates in New York City. Using both interrupted time series and synthetic control approaches, they conclude that bail reform had no significant effects on crime rates for violent assault, theft, burglary, and drug offenses in the immediate weeks following the law's implementation, although they do observe a small, positive increase in rates of violent robbery in some specifications. Wu and McDowall (2024) also estimate the effect of New York state's bail reform on statewide crime rates using a synthetic control approach. They find that while statewide rates of murder, larceny, and motor vehicle theft rose following bail reform, these increases were not statistically significant and could not be attributed to the reform. Studies of bail reform in other jurisdictions yield similar results: Ouss and Stevenson (2023) examine the effects of a "no-cash bail" policy implemented in Philadelphia, finding no evidence of increase in either failures-to-appear or new criminal charges within six months among eligible defendants. Stemen and Olson (2020) employ Bayesian Structural Time Series models to examine impacts of Cook County's court-ordered misdemeanor bail reform, finding no significant effects on property, violent, or violent gun crime counts. Looking at impacts of New Jersey's bail reform on gun violence, Jahn, Simes and Jay (2024) find no evidence of increases in firearm mortality or gun violence following the reform. Lastly, Craigie and Grawert (2024) use a staggered difference-in-difference strategy to estimate impacts of bail reform on crime across multiple local jurisdictions, finding no evidence to support a connection between bail reform and increases in crime.

## **Current Study**

The current research seeks to add to the growing literature on the effects of bail reform by examining the impact of Maryland's statewide bail reform on aggregate crime. I contribute to the literature by examining crime impacts of an understudied policy reform and by estimating the synthetic control method on a substantially longer pre-intervention<sup>41</sup> series than previously used in similar studies of bail reform. The current study addresses a key research question: what is the impact of bail reform on statewide crime?

## **Data and Methods**

### *Data and Dependent Variables*

I use annual state-level data from the Federal Bureau of Investigation's Uniform Crime Report Part I index crimes for the period of 1985-2019. I examine *total crime*, measured using annual state-level crime data, pooling all Part I index crimes (murder and non-negligent manslaughter, robbery, rape, aggravated assault, motor vehicle theft, larceny-theft, burglary, and arson); *violent crime* (murder and non-negligent manslaughter, robbery, rape, aggravated assault); and *property crime* (motor vehicle theft, larceny-theft, burglary, and arson).<sup>42</sup> I restrict the period used for the analysis to the post-1984 period, after the Federal Bail Reform Act of 1984 catalyzed many states to amend their pretrial release laws. The post-intervention period of interest spans the three years following the reform, 2017-2019.<sup>43</sup> I end the post-reform period in 2019,

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<sup>41</sup> Note that throughout this chapter, I use the terms "intervention" and "treatment" interchangeably to refer to the policy of interest.

<sup>42</sup> In supplemental analyses, I examine impacts on offense-specific crime rates for homicide, robbery, aggravated assault, larceny, burglary, and motor vehicle theft.

<sup>43</sup> Maryland's bail reform went into effect on July 1, 2017. However, as I discuss in Chapter 2, key parts of the reform were proposed and discussed by the Judiciary in early 2017. Thus, considering calendar year

prior to the onset of the COVID-19 pandemic in early 2020.<sup>44</sup> I transform statewide crime counts into per capita rates (per 100,000 people) to allow for comparisons between states that have different populations.

### **Analytic Strategy**

To estimate the impact of bail reform on crime, I use the synthetic control group method (SCM) to construct a “synthetic Maryland” that approximates the behavior of crime in Maryland had it not enacted bail reform (Abadie et al., 2010; Abadie & Gardeazabal, 2003). As a comparative case study method, SCM was developed to estimate the effect of an intervention with only one treated unit, and a small number of control units, where the intervention happens at an aggregate level (such as a statewide policy change) (Bartos et al., 2020; Bartos & Kubrin, 2018; Bird et al., 2018; Donohue et al., 2019; Kubrin & Bartos, 2020). This method has become a popular tool in empirical research across a range of disciplines and policy domains, with studies examining the effects of right-to-carry laws (Donohue et al., 2019), legalization of sex work (Cunningham & Shah, 2018), and criminal justice reform (Bartos & Kubrin, 2018, Bird et al., 2018, Kubrin & Bartos, 2020). Recently, the method has been used to estimate the impact of New York State’s bail reform on crime outcomes (Wu & McDowall, 2024, Zhou et al., 2023) and New Jersey’s bail reform on firearm mortality and gun violence (Jahn et al., 2024).

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2017 as a “treated” year should capture effects of both enactment of the new rules and alleviate any concern over anticipation effects in the months leading up to the effective date.

<sup>44</sup> The pandemic brought about substantial changes to social, economic, and crime conditions, along with unprecedented changes to court and bail operations (Viglione et al., 2023); because of this, any attribution of changes to crime rates during this period to the 2017 bail reform is difficult.

SCM estimates the counterfactual of the treated unit if there were no treatment by constructing a weighted average of multiple comparison units that “most closely resembles the actual one” (Abadie & Gardeazabal, 2003, p. 116). In circumstances where there are many possible units that may serve as controls, a combination of many units may serve as a better estimate of the counterfactual than any single unit alone (Abadie et al., 2010). Abadie et al. (2010, 2015) posit that SCM can be viewed as a generalization of the differences-in-differences (DD) design, which takes the differences in pre- and post-trends between a treated and control unit as the estimate of the effect of a treatment after an intervention. SCM has two main advantages over DD: (i) DD requires that treated and control units satisfy a strong assumption of parallel trends in the pre-treatment period, which can be difficult to justify, particularly over long pre-intervention periods; and (ii) in the instance where many possible comparison units exist, SCM provides a transparent way to select optimal comparison units. SCM relaxes the assumptions of the DD framework, allowing the effects of unobserved variables on the outcome to vary over time and does not rely on the assumption of parallel trends (Abadie 2021).

### **Overview of Synthetic Control Estimation**

$Y_{jt}$  is the outcome of interest for each state,  $j$ , of  $J + 1$  states at time  $t$ . The value  $j = 1$  corresponds to Maryland, and  $j = 2, \dots, J$ , correspond to each of the other states that are candidate contributors to the control group, or the “donor pool.” Because states in the donor pool are meant to approximate what would have happened in Maryland had bail reform not been enacted, donor pool states should not have passed a similar intervention. The synthetic control estimator models the effect of the intervention at time

$T_0$  on the treatment group (Maryland) using a linear combination of optimally chosen units as the synthetic control. For the post-intervention period, the causal effect is measured as:

$$Y_{1t} - \sum_{j=2}^{J+1} w_j^* Y_{jt}$$

where  $w_j^*$  is a set of weights,  $w_2, \dots, w_{J+1}$ , assigned to each donor unit. By design, weights assigned to each donor pool state are non-negative and sum to one (Abadie et al., 2010). Donor weights are optimally chosen to minimize the difference between the pre-intervention characteristics of the treated unit ( $X_1$ ) and synthetic control units ( $X_0$ ), measured as:

$$\|X_1 - X_0 W\| = \sqrt{(X_1 - X_0 W)' V (X_1 - X_0 W)}$$

Here,  $V$  is a matrix of weights assigned to the predictor variables in the model, which reflect the ability of the covariates to predict the outcome in the pre-intervention period. Donor weights ( $w_j^*$ ), then, are conditional on  $V$ . As described in Ferman et al. (2020), “[i]ntuitively,  $[W]$  is a weighting vector [that measures the relative importance of each unit in the synthetic control...while  $[V]$  measures the relative importance of each one of the...predictors” (p. 514). Generally, the weighting matrix is selected to minimize the root mean square prediction error (RMSPE) between the synthetic control and the treated unit’s time series during the pre-intervention period (Abadie et al., 2010). The RMSPE measures the lack of fit between the path of the outcome variable and the synthetic control group:

$$RMSPE = \sqrt{\frac{1}{T_0} \sum_{t=1}^{T_0} (Y_{1t} - \sum_{j=2}^{J+1} w_j^* Y_{jt})^2}$$

Two general approaches may be employed when selecting predictor variables: including covariates thought to have causal relationships with the outcome, or using variables based on pre-intervention observations of the outcome. Because there is no consensus in the literature on the causal mechanisms influencing long-term macro-level crime trends (for a review, see Baumer et al., 2018), and because it is not clear whether change in each causal variable is invariant across the panel or whether causal effects interact, I did not include causal covariates (Bartos et al., 2020, p. 133; McDowall et al., 2019, p. 165). Kaul et al. (2022) demonstrate that if all pre-treatment outcomes are used as predictors, other predictors receive no weight and are ignored. Two prior studies of bail reform on aggregate crime rates follow this convention and do not include causal covariates (Wu & McDowall, 2024, Zhou et al., 2023, *but see* Jahn et al., 2024). I extend these studies by matching on a much longer time series of pre-intervention outcomes, including as predictors all pre-intervention observations of each crime rate outcome for the period 1985-2016.<sup>45</sup> Including a long pre-intervention period helps to control for unobserved factors affecting the outcome variable, and for any heterogeneity in the effects of both measured and unmeasured factors. Underlying this is the intuition that only units that are alike in both observed and unobserved factors that influence state-level crime rates should produce similar time series of each outcome over prolonged periods of time (Abadie et al., 2010). I estimate separate synthetic control time series for each outcome, allowing the donor units to vary across outcome.

Defining the “donor pool” of untreated units is an important consideration for the synthetic control method. In general, “synthetic control time series should be

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<sup>45</sup> As will be discussed later, I also examine the sensitivity of effect estimates to different specifications of the synthetic control.

constructed from the widest practicable donor pools” (McDowall et al., 2019, p.169). As noted earlier, states that have been exposed to an intervention similar to Maryland’s bail reform should be excluded from the donor pool, as the synthetic control times series is meant to represent what would have happened in Maryland had bail reform never been enacted (Abadie et al., 2010). I exclude other states that passed similar statewide pretrial reforms in either the pre- or post-intervention periods. To determine which states enacted similar pretrial reforms, I use the National Conference of State Legislatures’ database of enacted legislation relating the pretrial process, peer-reviewed studies (e.g., Stevenson, 2018a), and other policy and research reports (Doyle et al., 2019; Jorgensen & Smith, 2021).<sup>46</sup> Several sub-state jurisdictions, including cities and counties (e.g., Cook County, IL and Harris County, TX), reformed their bail systems during the period under study. Because my interest is in statewide crime rate outcomes and because local reforms were restricted to specific jurisdictions within the state and were not expanded statewide, states including these local jurisdictions were retained in the donor pool.<sup>47</sup> In total, 42 states were included in the donor pool to create the synthetic control time series (“Synthetic Maryland”).

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<sup>46</sup> As discussed in Chapter 1, states such as New Mexico, Alaska, Kentucky, New York, Connecticut, and New Jersey enacted substantial reforms to their pretrial systems, via legislation, administrative rule and ballot proposition, aimed at reducing money bail; these states were excluded from the donor pool. California is also excluded from the donor pool; the state passed a landmark bail reform bill in 2018, which did not go into effect and was eventually repealed via ballot referendum in 2020. In addition, in January 2018, a California State Appeals court found that the state’s money bail system violated due process and equal protection, a ruling later affirmed by the California Supreme Court in 2021 (*In re Kenneth Humphreys*). Because both SB10 and *In re Kenneth Humphreys* might have influenced bail determination behavior during the post-treatment period for Maryland, I exclude the California from the donor pool.

<sup>47</sup> Further, where donor units are not selected as contributors to the synthetic control, their values do not influence the synthetic control estimates or estimates of treatment effects. However, in the main estimates of synthetic control groups for the total crime and property crime rates, Illinois is included as a donor unit. To assess the influence of Illinois on the estimates of treatment effects, I conduct “leave-one-out” tests, which iteratively removes each donor unit from the pool and re-estimates the synthetic control.

## Inference

Traditional statistical inference techniques are not applicable to the synthetic control approach, owing to the small sample size. Instead, I use in-sample permutation-based tests (“in-space placebo tests”) described in Abadie et al. (2010), which can be used to assess how unusual an estimated effect would be if it were due to chance. To implement this test, I apply the treatment year (or artificially assign the treatment) to every state in the donor pool of 42 states, adding Maryland to the set of states in the donor pool, and generate a synthetic control for each placebo state following the procedure described above. I then calculate the root mean squared prediction error (RMSPE) for the pre-treatment period, and generate a ratio of post- to pre-treatment RMSPE for each state. A large ratio for the treated unit (i.e., Maryland) implies that the difference between Maryland and its synthetic control is bigger after bail reform. Next, each ratio of post- to pre-treatment RMSPE for all 43 states is ranked from highest to lowest. A large ratio for Maryland, compared with the ratios generated by the placebo states, suggests that the impact is significant. Specifically, the probability that chance could have produced the Maryland results is the rank order of Maryland in that distribution, divided by the number of total units (i.e., 43), yielding an exact  $p$  value. This method allows me to examine whether the effect of bail reform in Maryland is large relative to the distribution of effects for states not exposed to bail reform, where effects are expected to be small (or the result of random variation) given that no reform was enacted. Because the distribution of in-space placebo effects may be generated from poorly-fitted synthetic controls for some units, I present  $p$  values for Maryland based on the full sample of 43 units, as well as  $p$  value computed from a placebo distribution of

states with pre-treatment fits of no more than 5 and 2 times that of the original synthetic control (Abadie et al., 2010).

### **Robustness and Sensitivity Tests**

“Synthetic Maryland” is a weighted combination of several donor units. To assess the influence of each donor unit on the estimate of the treatment effect, I conduct “leave-one-out” robustness tests, iteratively excluding the highest contributing donor unit from the donor pool, and re-estimating the synthetic control and treatment effects. To guard against specification searching, following Ferman et al. (2020), I also present additional specifications of the synthetic control, varying the sets of predictors and time periods used to generate a synthetic control series for each outcome. All models are estimated using the *synth2* package in Stata 17.1 with the nested optimization routine.

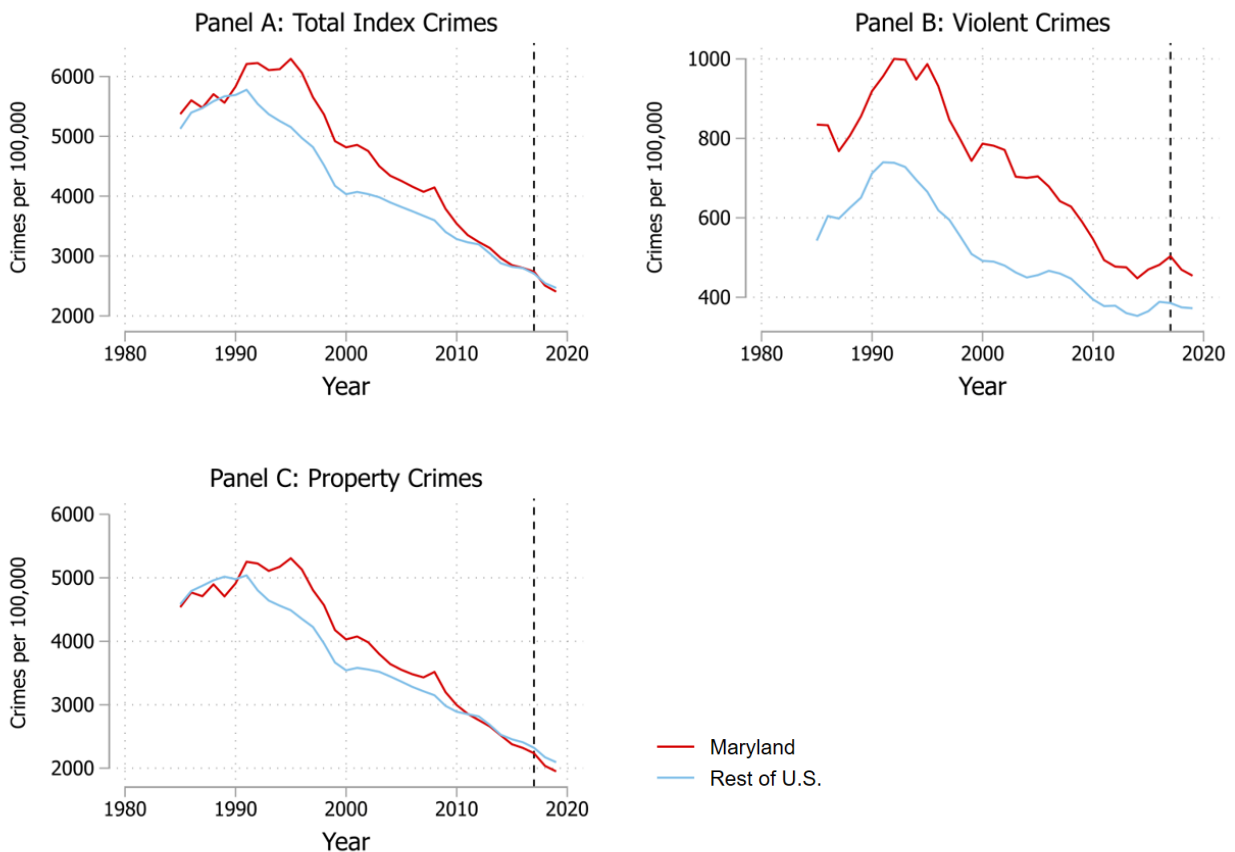
## **Results**

### **Crime Trends in Maryland and the United States**

Historically, Maryland’s crime trends have diverged from those of other states. Compared with the rest of the United States, Maryland’s crime rate was higher throughout the 1990s and early 2000s. By the mid-2010s, Maryland’s crime rate looked very similar to the rest of the country (see Figure 7). Property crime trends follow this same pattern: Maryland’s rate of property crime was higher than other states from the early 1990s through 2010, but mostly comparable in the immediate years leading up to the 2017 bail reform. Conversely, Maryland violent crime rates were consistently higher than other states’, and unlike property crime, remained high throughout the study period.

How does crime change, if at all, following Maryland’s bail reform? Bail reform went into effect in 2017. Compared with 2016 (the year prior to the reform), overall crime in Maryland declined, falling by 14.1% over the following three years. This decline is observed both in rates of violent crime—which, after increasing for several years before bail reform, dropped sharply from 2017 to 2019—as well as property crime. Whether these observed changes in crime rates are attributable to bail reform is an empirical question to which I turn next.

Figure 7. Crime Rates per 100,000, Maryland and United States, 1985-2019



## Effects of Bail Reform on Crime in Maryland

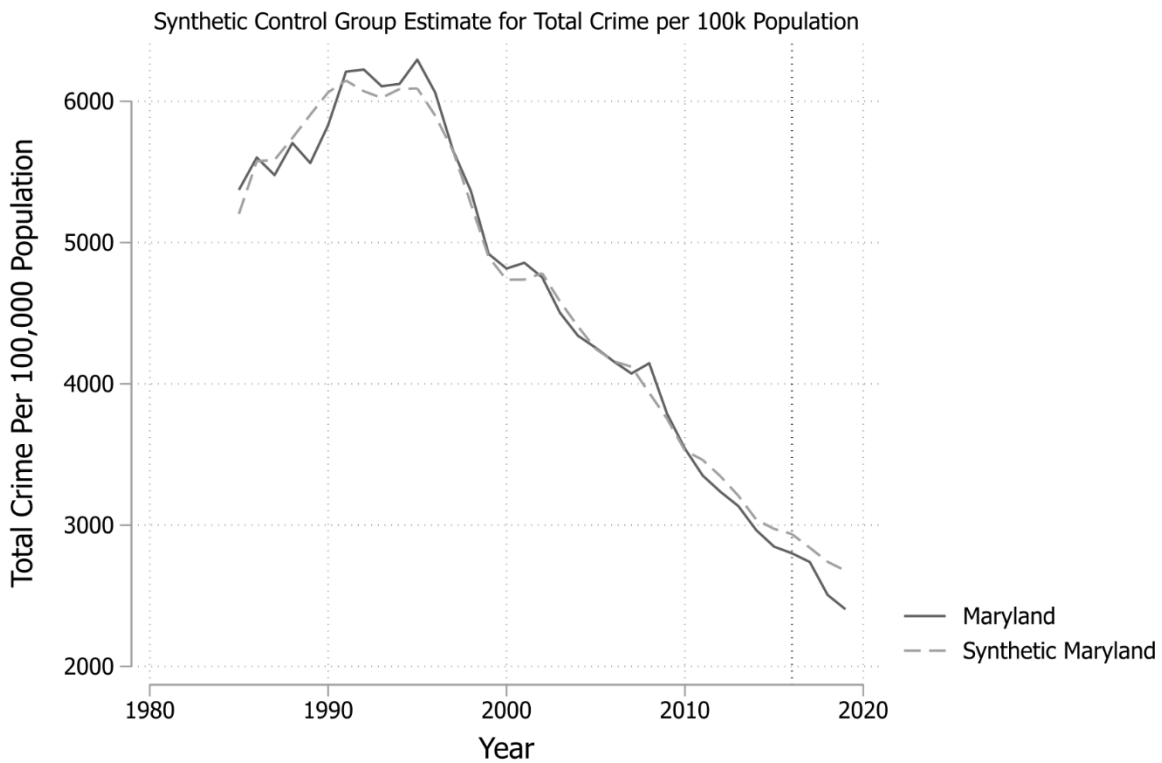
To estimate the impact of Maryland's bail reform on crime, I construct synthetic control groups to estimate what would have happened in Maryland to total crime, violent crime, and property crime rates had bail reform not been enacted. Figures 8-10 display Maryland (solid line) and its synthetic control (dashed grey line) for the total crime rate, violent crime rate, and property crime rate outcomes, respectively. The dotted vertical grey line marks 2017, the year of the enactment of bail reform. Overall, I find little evidence that Maryland's bail reform had any significant effects on crime rates, a finding that is robust to a range of specifications.

### *Overall Crime Rate*

The synthetic control algorithm generated a synthetic Maryland composed of a weighted average of six states: Illinois, Hawaii, North Carolina, Louisiana, Wyoming, and Florida. Table 5 presents the donor units for each outcome, along with their respective weights ( $w$ ). The RMSPE, is 121.82, indicating the average of the discrepancies between total crime rates in Maryland and Synthetic Maryland during the 32-year pre-intervention period is 121.82 (Abadie et al., 2010). Graphical inspection of Figure 8 shows a close pre-intervention fit between total crime rates in Maryland and Synthetic Maryland, as they followed a similar trend over the 32 year pre-intervention period. However, the trend does depart slightly before the bail reform took effect. The gap that emerges after bail reform's enactment is nearly 1.8 times larger than the pre-intervention RMPSE, suggesting that bail reform affected total crime rates in Maryland. The average post-reform annual crime rate for Maryland was 2,550.01 crimes per 100,000 people, while its synthetic control was 2,752.9 per 100,000. In other words, bail

reform decreased the total crime rate by 202.9 fewer crimes per year, representing an estimated 7.9 percent decrease in the crime rate. While compelling, I cannot draw conclusions about this estimated effect prior to post-estimation testing.

Figure 8. Synthetic Control Group Estimate for Total Crime

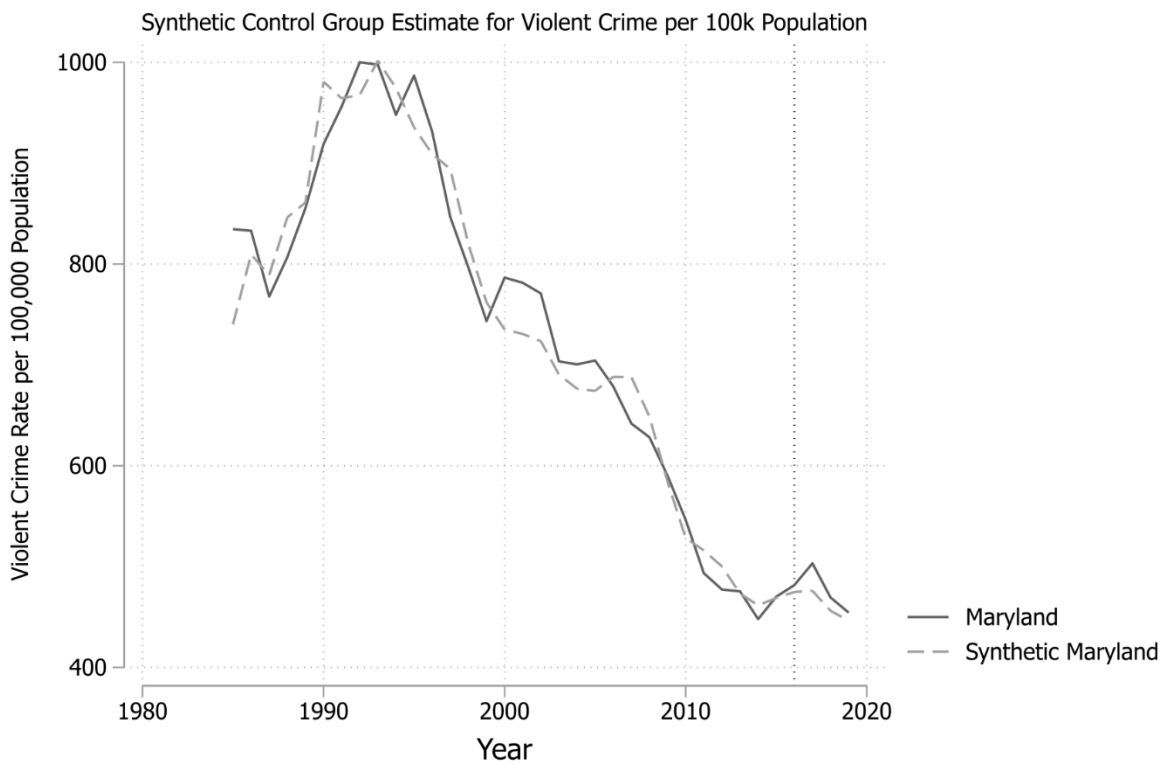


### *Violent Crime Rate*

The violent crime rate for Synthetic Maryland is composed of a weighted average of five donor units: Florida, South Carolina, Arizona, Louisiana, and Tennessee (see Table 5). The pre-intervention trends between Maryland and its synthetic control are substantially similar; the RPSME in the pre-intervention period is 33.9. Figure 9 displays violent crime rates in Maryland and Synthetic Maryland. During the post-intervention

period, Maryland had a violent crime rate that was slightly higher than its synthetic control: 475.7 violent crime per 100,000 per year, compared to its synthetic control, at an estimated 459.5 violent crimes per year. This suggests that bail reform led to an increase in the violent crime rate of an average of 16.2 crimes per year. However, the gap that emerges following bail reform is smaller than the model RMSPE, meaning that estimated effect falls within the range attributable to matching error and is not distinguishable from zero. Post-estimation tests, discussed below, reinforce this conclusion.

Figure 9. Synthetic Control Group Estimate for Violent Crime



### *Property Crime Rate*

Lastly, I estimate a synthetic control group for Maryland's property crime rates. The synthetic control is made up of a weighted combination of eight donor states: Wyoming, North Carolina, Florida, Hawaii, Montana, Illinois, Idaho, and Louisiana (see Table 5). Figure 10 shows a good pre-intervention fit in the property crime rate trends between Maryland and its synthetic control. The average discrepancy, or RMSPE, in the pre-intervention period is 101.8. During the post-intervention period, Maryland saw an average property crime rate of 2,074.4 per year, compared to its synthetic control, with an average of 2,176.5 property crimes per year. This suggests that the property crime rate is approximately 5% lower per year (-102.1) than it would have been without bail reform. However, as Abadie et al. (2010) note, a large post-intervention gap is not indicative of a large effect if the RMSPE is also large. Here, the post-intervention gap is only slightly larger than the RMSPE, suggesting that this effect may be difficult to distinguish from matching error. I explore this further in post-estimation testing.

Figure 10. Synthetic Control Group Estimate for Property Crime

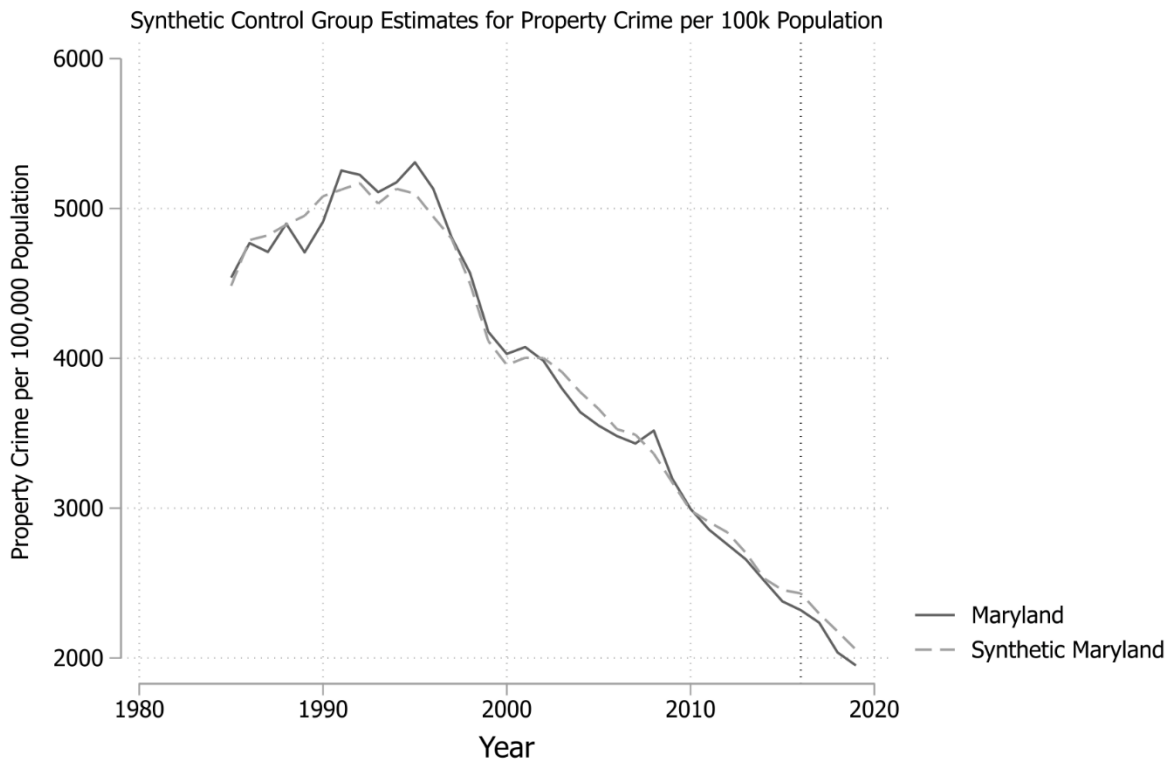


Table 5. Donor Weights for Synthetic Controls for Total Crime, Violent Crime, Property Crime

<b>State Name</b>	<b>State</b>	<b>Total Crime</b>	<b>Violent Crime</b>	<b>Property Crime</b>
Alabama	AL	0	0	0
Arizona	AZ	0	0.261	0
Arkansas	AR	0	0	0
Colorado	CO	0	0	0
Delaware	DE	0	0	0
Florida	FL	0.105	0.404	0.183
Georgia	GA	0	0	0
Hawaii	HI	0.202	0	0.094
Idaho	ID	0	0	0.045
Illinois	IL	0.287	0	0.051
Indiana	IN	0	0	0
Iowa	IA	0	0	0
Kansas	KS	0	0	0
Louisiana	LA	0.119	0.013	0.018
Maine	ME	0	0	0
Massachusetts	MA	0	0	0
Michigan	MI	0	0	0
Minnesota	MN	0	0	0
Mississippi	MS	0	0	0
Missouri	MO	0	0	0
Montana	MT	0	0	0.067
Nebraska	NE	0	0	0
Nevada	NV	0	0	0
New Hampshire	NH	0	0	0
North Carolina	NC	0.173	0	0.226
North Dakota	ND	0	0	0
Ohio	OH	0	0	0
Oklahoma	OK	0	0	0
Oregon	OR	0	0	0
Pennsylvania	PA	0	0	0
Puerto Rico	PR	0	0	0
Rhode Island	RI	0	0	0
South Carolina	SC	0	0.262	0
South Dakota	SD	0	0	0
Tennessee	TN	0	0.06	0
Texas	TX	0	0	0
Utah	UT	0	0	0
Vermont	VT	0	0	0
Virginia	VA	0	0	0
Washington	WA	0	0	0
West Virginia	WV	0	0	0
Wisconsin	WI	0	0	0
Wyoming	WY	0.114	0	0.316

## Placebo Analysis

Based on the above analyses, Maryland's bail reform reduced total crime rates by an average of 202.9 fewer crimes per year, reduced property crime rates by 102.1 per year, and increased violent crime rates by 16.2 per year. The credibility of the estimated effects is assessed using in-space placebo tests, which evaluate whether the estimated effects are large relative to the placebo effects observed in states that did not enact bail reform (Abadie et al., 2010). *P*-values constructed from the distribution of placebo effects are interpreted as the probability of identifying a treatment effect of equal or greater magnitude if the treatment had been randomly assigned to a unit in the donor pool. Intuitively, confidence in attributing a large estimated effect on crime to the bail reform is weakened if the magnitude of that effect falls inside the distribution of placebo effects (Abadie et al., 2015).

Figure 11 displays Maryland's ratio of post-intervention RMSPE to pre-intervention RMSPE relative to the donor pool states for total crime and property crime rates—the two outcomes that did not produce null effects. Relative to the distribution of ratios produced by the in-sample placebo tests, Maryland did not rank highly for either outcome. For total crime, 13 states had larger post/pre-intervention RMSPE ratios than Maryland. The probability of randomly observing at least as large a change in total crime rates in the 2017-2019 period as Maryland is 14/43, or 0.32. For property crime, 22 states had larger post/pre-intervention RMSPE ratios than Maryland, rendering the probability of observing a change in property crime rates at least that large at 23/43, or 0.54. In short, if I randomly picked a state from the sample, the chances of obtaining

effects of these magnitudes are high. While the analyses suggest that bail reform decreased total and property crime rates, these effects are not statistically significant.

The distribution of ratios of post-intervention gaps to pre-reform RMSPE may be produced by placebo estimates that were the result of poor pre-treatment fits. Following suggestions by Abadie et al. (2010), I also calculate  $p$  values using placebo effects that are restricted to good pre-treatment fits. Results suggest that, even after restricting pre-reform RPSME to no more than 5 and 2 times that of the original synthetic controls for both total crime and property crime, the estimated effects are still not significant at conventional levels (see Table 6).

Finally, the synthetic control produced a RPSME that was larger than the estimated post-intervention gap for violent crime rates, suggesting null effects of the bail reform on violent crime. To provide further support for this conclusion, I also conduct in-sample placebo tests for violent crime rate results. As expected, the in-sample placebo test suggests that the estimated average treatment effect for violent crime is not statistically significant, across all placebo effects (see Figure 12). In sum, results here indicate that bail reform did not impact crime rates—either total, property, or violent crime—in the three years following reform.

Table 6. Synthetic Control Tabular Results: Total Crime, Violent Crime and Property Crime

	<b>ATE</b>	<b>Pre- Reform R<sup>2</sup></b>	<b>RMSPE</b>	<b>p(RMSPE)</b>	<b>p(RMSPE) &lt; 5</b>	<b>p(RMSPE) &lt; 2</b>
<b>Total Crime</b>	-202.89	0.99	121.82	0.33	0.33	0.38
<b>Violent Crime</b>	16.16	0.96	33.90	0.88	0.88	0.90
<b>Property Crime</b>	-102.10	0.99	101.76	0.53	0.53	0.69

*Note: Average treatment effects (ATE) reflects the average gap between Maryland and its synthetic control over the post-reform period; RMSPE is the root mean square predicted error over the pre-intervention period; p(RMSPE) is the exact p value generated from the distribution of ratios of post/pre-RMSPE from the in-sample placebo tests; p(RMSPE) < 5 is the p value from the placebo tests, excluding estimates where the RMSPE for the placebo state was greater than 5 times that of the original synthetic control; p(RMSPE) < 2 is the p value from the placebo tests, excluding estimates where the RMSPE for the placebo state was greater than 2 times that of the original synthetic control.*

Figure 11. Post-Reform RMSPE to Pre-Reform RMSPE Ratios for Total Crime Rates and Property Crime Rates

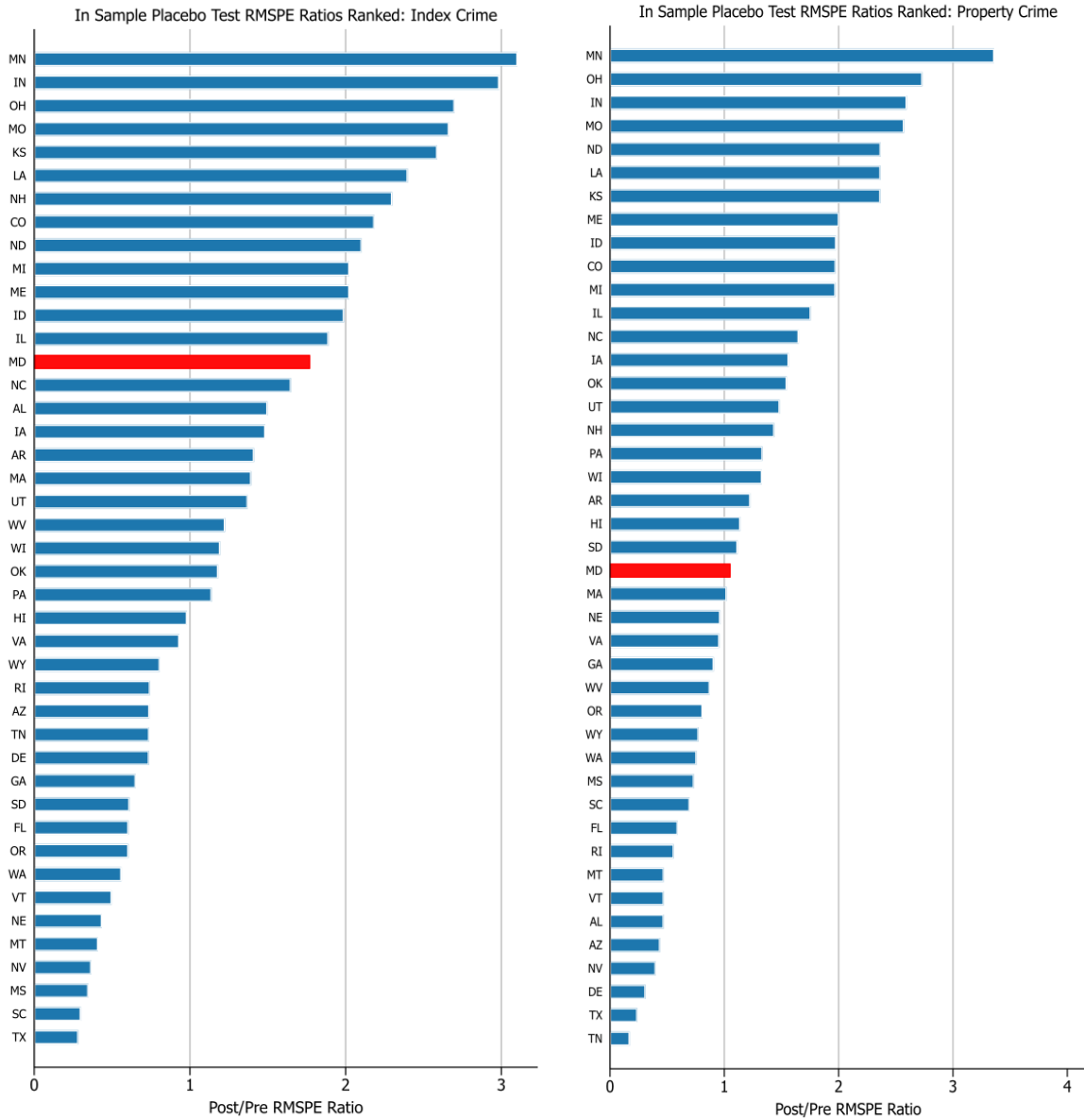
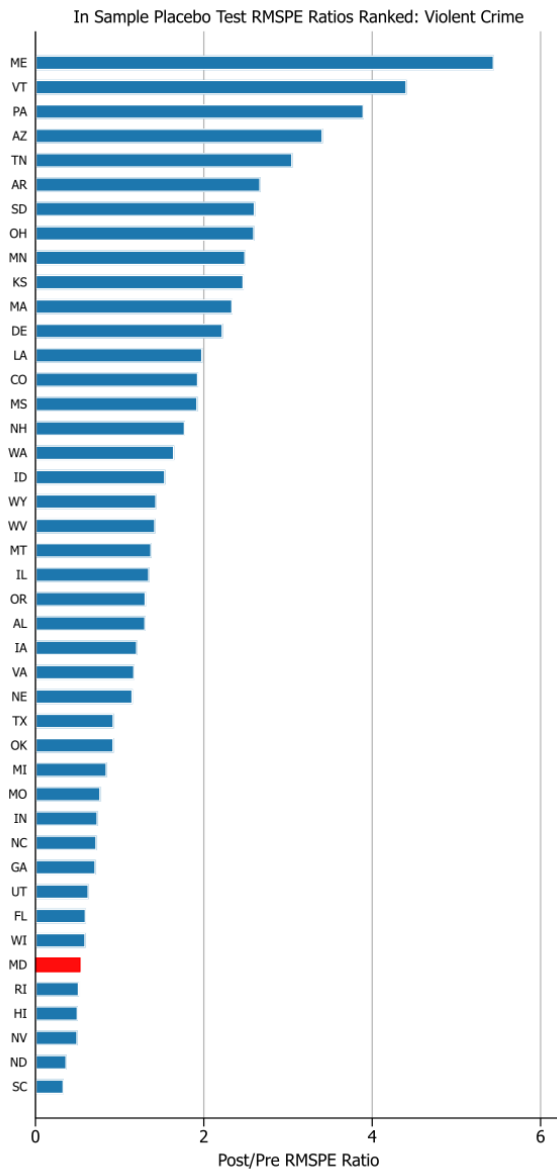


Figure 12. Post-Reform RMSPE to Pre-Reform RMSPE Ratios for Violent Crime Rates

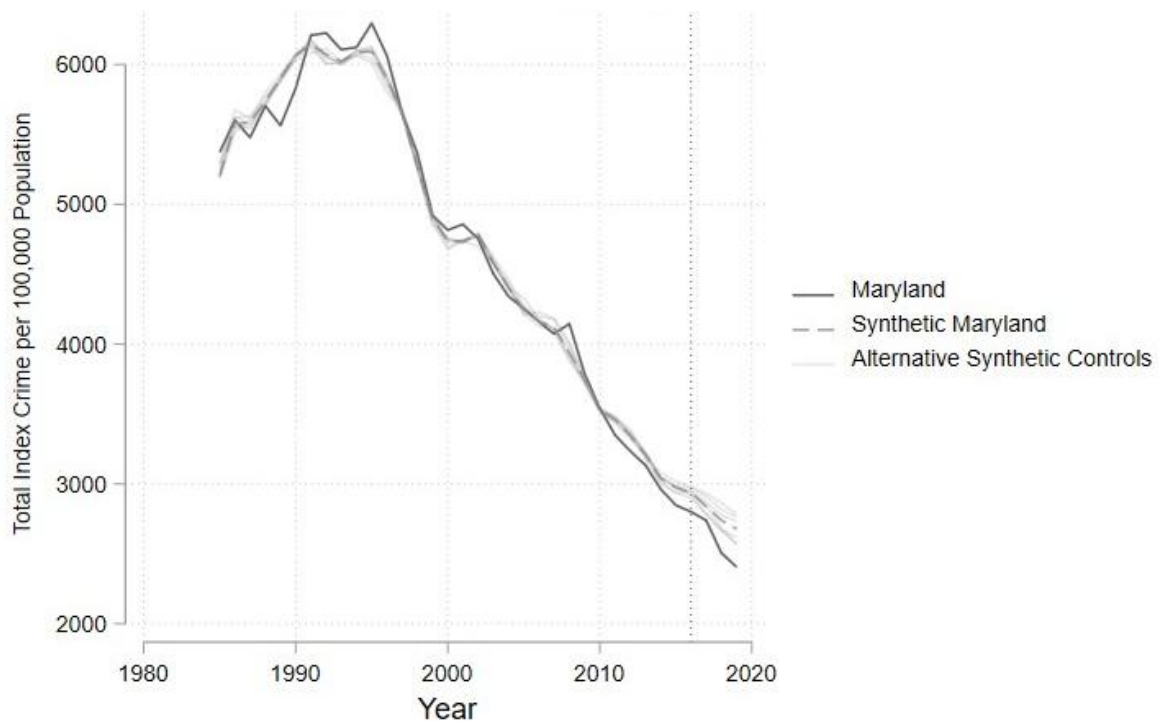


### ***Robustness and Sensitivity Tests***

To test the robustness of the results, I conduct a “leave-one-out” test to assess the influence of each individual donor state to the composition of Synthetic Maryland (McDowell et al., 2019). I summarize the results of the “leave-one-out” tests for each outcome of interest in tabular form (see Table 7). Figures 13-15 displays a series of alternative specification of synthetic controls generated by the “leave-one-out” tests

(light grey lines), the original synthetic control estimate (dashed grey line), and actual Maryland (solid black line) time series of the total index crime rate. Here, each alternative specification is produced by iteratively excluding each of the donor pool states that comprise the original synthetic control for the outcome, and re-estimating the synthetic control time series.

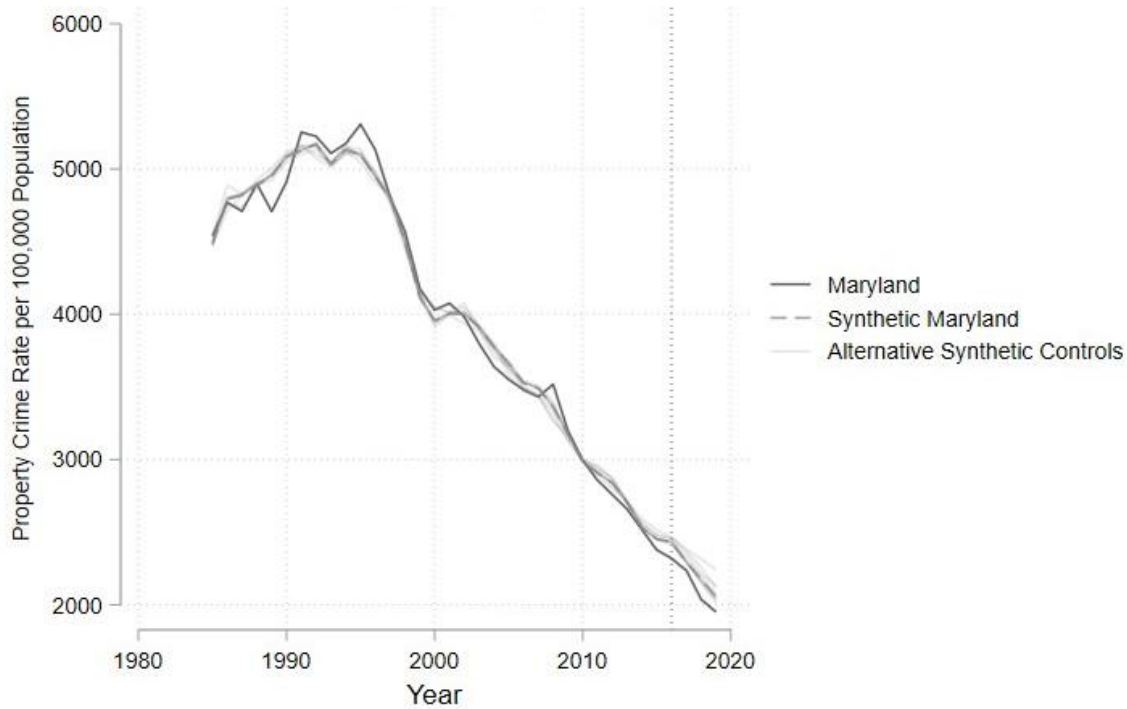
Figure 13. Leave-One-Out Plot for Total Crime Rate per 100,000



For total crime, the “leave-one-out” tests demonstrate that the direction of the estimated treatment effect is robust to alternate donor units, though the magnitude of estimated effects is variable (Table 7, Panel A). For example, when excluding Illinois from the donor pool, the estimated average treatment effect of bail reform on the total crime rate is smaller than when the synthetic control includes Illinois, while pre-treatment RMSPE remains similar. Conversely, when North Carolina is excluded, the

estimated treatment effect is larger in magnitude (a larger decrease in total crime rate relative to its synthetic control), however the pre-treatment fit is poorer.

Figure 14. Leave-One-Out Plot for Property Crime Rate per 100,000



Likewise, the results of the leave-one-out test for violent and property crime rates demonstrate the robustness of the main results to alternative constructions of the synthetic control. For violent crime rates (Table 7, Panel B), pre-intervention fits of the estimated synthetic control time series are worse as donors are removed from the donor pool; post-intervention differences were similar in sign, varying only slightly in magnitude. In each instance, the post-intervention gaps were smaller than the RMSPE, yielding further evidence for a null effect. For property crime rates (Table 7, Panel C), the average treatment effects estimated in the “leave-one-out” tests were in the same direction and of a similar magnitude to the main results. In only one case—Florida—did the state’s exclusion from the donor pool result in a change in the estimated magnitude

of the effect. In sum, I conclude that the “leave-one-out” tests support the primary conclusion of the analysis: bail reform had no significant impacts on total, violent, or property crime rates in Maryland.

Figure 15. Leave-One-Out Plot for Violent Crime Rate per 100,000

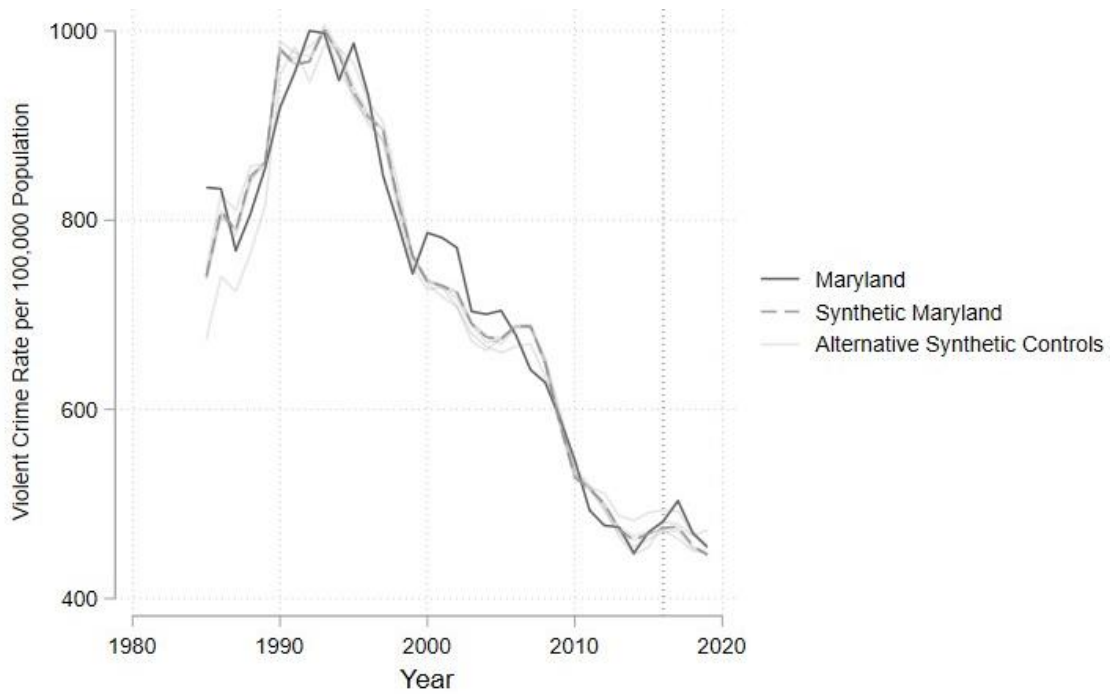


Table 7. Leave-One-Out Tests for Total, Property and Violent Crime Rates

<b>Panel A: Total Crime</b>				
<b>State</b>	<b>Original Donor Weights</b>	<b>Results from Leave-One-Out Test</b>		
		<b>Average Effect</b>	<b>RMSPE</b>	<b>Ratio of Post/Pre RMSPE</b>
IL	0.287	-125.90	126.40	1.08
HI	0.202	-150.60	132.85	1.16
NC	0.173	-310.03	130.00	2.47
LA	0.119	-140.18	126.25	1.25
WY	0.114	-243.37	122.84	2.09
FL	0.105	-280.27	124.92	2.34
<b>Panel B: Violent Crime</b>				
<b>State</b>	<b>Original Donor Weights</b>	<b>Results from Leave-One-Out Test</b>		
		<b>Average Effect</b>	<b>RMSPE</b>	<b>Ratio of Post/Pre RMSPE</b>
FL	0.404	3.12	46.10	0.38
SC	0.262	4.46	36.09	0.18
AZ	0.261	21.33	35.34	0.73
LA	0.013	18.72	34.09	0.62
TN	0.06	17.04	33.91	0.55
<b>Panel C: Property Crime</b>				
<b>State</b>	<b>Original Donor Weights</b>	<b>Results from Leave-One-Out Test</b>		
		<b>Average Effect</b>	<b>RMSPE</b>	<b>Ratio of Post/Pre RMSPE</b>
WY	0.316	-154.89	109.05	1.46
NC	0.226	-173.78	110.93	1.60
FL	0.183	-238.65	111.09	2.23
HI	0.094	-82.13	106.98	0.80
MT	0.067	-86.32	102.91	0.88
IL	0.051	-91.47	101.89	0.94
ID	0.045	-117.97	101.94	1.22
LA	0.018	-96.70	101.89	1.00

To further probe the sensitivity of the main results, I run additional models varying the set of predictors used to estimate each outcome’s synthetic control (see Ferman et al., 2020; McDowell et al., 2017). In the Table 8 below, I present results from a set of additional specifications: (i) using five-year averages of pre-treatment outcomes as predictors; (ii) using all pretreatment outcomes and their first differences (McDowell et

al., 2017); (iii) including only even years; (iv) discarding the first five years of pre-intervention outcomes; (v) discarding the first 10 years of the pre-intervention period; and (vi) discarding the first 20 years of pre-intervention outcomes (“discard-a-segment” tests; McDowell et al., 2017). Results demonstrate that when a long pre-intervention period is used—models 1 through 3—results are largely insensitive to variation in the sets of predictors used to construct synthetic Maryland. This pattern changes when the “discard-a-segment” test is used: dropping the first 5 years of the pre-treatment period available results in only small changes in the estimated treatment effects. However, in dropping increasingly larger segments—10 years and 20 years—of the pretreatment time series, I observe a decrease in magnitude and change in the sign of the estimated treatment effect for both overall crime and property crime rates. Estimated effects for violent crime rates are substantially similar for the “discard-a-segment” tests. Despite the change in sign, in-sample placebo tests find that the results are common relative to the distribution of placebo effects.

Table 8. Alternative Specifications for Synthetic Control Estimates

	Alternative Specifications					
	Means (1)	Levels and First Differences (2)	Even Years (3)	Drop First 5 Years (4)	Drop First 10 Years (5)	Drop First 20 Years (6)
<b>Total Crime</b>						
ATE	-119.25	-184.15	-102.86	-49.64	44.92	75.02
Pre-Tx R <sup>2</sup>	0.98	0.99	0.99	0.98	0.91	0.84
RMSPE	136.57	118.93	129.10	100.12	57.47	35.27
Post/Pre RMSPE	1.02	1.66	0.75	0.45	1.18	4.75
P(Ratio)	0.53	-	0.56	0.77	0.70	0.58
<b>Violent Crime</b>						
ATE	32.50	13.92	17.65	9.81	27.21	35.94
Pre-Tx R <sup>2</sup>	0.95	0.95	0.95	0.93	0.95	0.90
RMSPE	38.15	29.72	35.55	28.97	24.79	9.65
Post/Pre RMSPE	0.87	0.61	0.35	0.41	1.58	15.13
P(Ratio)	0.77	-	0.84	0.91	0.74	0.51
<b>Property Crime</b>						
ATE	-27.02	-39.65	-77.06	-41.85	37.31	13.27
Pre-Tx R <sup>2</sup>	0.98	0.99	0.99	0.98	0.89	0.87
RMSPE	114.34	104.52	103.50	87.72	45.13	34.70
Post/Pre RMSPE	0.06	0.44	0.59	0.27	0.97	0.66
P(Ratio)	0.98	-	0.65	0.79	0.74	0.91

Note: Placebo runs for model (2) would not complete; as such, p values were not generated for this specification.

### Supplemental Analyses: Crime Type

To examine whether Maryland's bail reform impacted offense-specific rates of crime, I generate synthetic control groups for rates of homicide, robbery, aggravated assault, burglary, larceny, and motor vehicle theft using all pre-intervention outcomes as predictors.<sup>48</sup> Appendix B summarizes the results of these supplemental analyses. Consistent with the main results, these offense-specific analyses yield little evidence in support of a crime rate effect of bail reform. For burglary, larceny, and motor vehicle theft, I find no significant impact of bail reform on crime. Similarly, I find null results for homicide, robbery, and aggravated assault, although the fit of the synthetic control time

<sup>48</sup> I exclude rape and arson from offense-specific estimates, owing both to changes in the UCR definition of rape in 2011 and to known issues in rape and arson crime statistics (National Academy of Sciences, 2016).

series for these outcomes is more variable, making the null findings less precisely estimated (see Appendix Table 2).

### ***Discussion and Limitations***

Despite concern over public safety impacts of bail reform, I find little evidence of substantive impacts on crime—either negative or positive. This study adds to a growing evidence base on the impacts of bail reform on crime, finding that bail reform in Maryland did not cause crime rates to change following its implementation. Using synthetic control methods, I find null results for the effects of bail reform on statewide crime rates, looking both at index crimes rates as well as offense-specific outcomes. These results are robust to a range of specifications and placebo tests, and are consistent with a growing number of studies of the impacts of bail reform (Zhou et al. 2023; Wu & McDowall, 2024, Heaton et al., 2017).

Added to other longitudinal research examining general crime effects of bail reform, which have not observed increases in crime rates, these results may suggest that bail reform does not affect general deterrence. However, there is reason to be cautious about this inference, particularly based on Maryland’s reform. Indeed, the null results found in this study may be due to the tempered impact of bail reform itself on defendants’ ability to secure pretrial release: though the use of money bail and bail amounts declined following the reform, preventative detention increased by 20.2 percentage points: 6.8 percent of cases in 2016 were held without bail at the initial bail hearing compared to nearly 27 percent of cases in 2019.<sup>49</sup> This countervailing trend

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<sup>49</sup> Based on an analysis of data obtained from the Maryland Judiciary Case Search on bail hearing outcomes across a sample of 332,056 new criminal cases filed between January 1, 2014 and December 31, 2019.

suggests that overall rates of release pretrial may not have changed dramatically following the reform, as judicial officers shifted away from high bail to explicitly remanding defendants to jail custody pending trial (O'Neill, 2025).<sup>50</sup> Aggregate jail statistics suggest that pretrial jail populations have not fallen following bail reform: in the three years prior to the passage of bail reform, an average of 68 percent of Maryland's jail population was held pretrial; in the three years following the reform, this increased 4.2 percentage points, to 72.2 percent from 2017-2019 (Maryland Governor's Office of Crime Prevention and Policy, 2024).

Although this study did not find evidence of significant impacts of bail reform on crime in Maryland, it may be the case any such impact may take more time to have an observable effect. The post-intervention period included only three years; a longer post-intervention period may identify impacts of bail reform, particularly as changes to bail policies become more widely understood by would-be offenders. Additionally, concurrent changes to criminal justice policies in Maryland may confound or otherwise obscure true effects of bail reform on crime. The most prominent policy change that occurred near Maryland's bail reform was the state's 2016 "Justice Reinvestment Initiative" (JRI) law, which encompassed a range of policy changes to reduce state prison populations for non-violent offenses, enhance penalties for certain serious and violent crimes, strengthen community supervision, and alter parole processes. The threat to identification posed by this policy change may be lessened by the fact that, between 2007 and 2019, over 35 other states that comprise the donor pool for Maryland

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<sup>50</sup> I do not observe actual pretrial release from custody; available court data reflect only bail determination outcomes, including money bail, held without bail, and nonfinancial release. For defendant with money bail set, data limit my ability to observe whether bail was posted and the defendant was released from jail.

passed similar laws through the federally-funded JRI initiative (Justice Reinvestment Initiative, n.d.).

This study focused on general crime effects of bail reform and did not investigate the effects of bail reform on reoffending outcomes, another mechanism through which the reform might affect crime. Prior studies of bail reform and pretrial detention suggest that changes to the probability of detention are likely to yield null net impacts to reoffending, through possible counterbalancing of incapacitation and criminogenic impacts (Dobbie et al., 2018; Heaton, 2022; Leslie & Pope, 2018). Additional analysis is necessary to investigate whether bail reform resulted in changes to reoffending in Maryland.

Given the shortage of evaluations of bail reform's impact on crime, this study offers lessons about possible effects to other localities and states considering such changes. Compared with more sweeping reforms (e.g., New York, Illinois) that have ended the use of cash bail for certain crimes altogether, Maryland's reform did less to constrain the discretion of judicial officers in making pretrial determinations. Because of this, results found here may not translate to all contexts or types of reforms broadly grouped under the umbrella of "bail reform," but may be more applicable to jurisdictions that have pursued more moderate approaches (Carroll, 2023). Future research should examine the relationship between specific policy levers (e.g., mandatory or discretionary reform) and effectiveness of reform in the pretrial process.

Finally, the analysis examined state-wide crime rates and did not investigate sub-state variation in crime effects of bail reform. Given well-documented variation in local implementation of law (e.g., Lynch, 2011), additional research that disaggregates

impacts by jurisdiction is needed to determine whether bail reform had more localized impacts on crime.

## CHAPTER 4. COURT CONTEXTS AND BAIL REFORM

Local variation in the implementation of law and policy is a consistent theme of scholarship across criminal legal and other domains, including policing (Simes et al., 2023), prosecution (Lynch, 2016; Spohn & Fornango, 2009), courts and sentencing (Engen & Steen, 2000; Flemming et al., 1988; Ulmer, 1997; Ulmer & Johnson 2006), parole (Lin et al., 2010), and juvenile justice (Armstrong & Rodriguez, 2005; Feld, 1991). The reasons identified in the literature are many; court scholars point to organizational and contextual factors, such as caseload pressure, local court culture, or normative orientations to punishment, along with the political and social environments of courts that shape sentencing and sanction outcomes (Eisenstein et al., 1988; Ulmer, 1997; Ulmer & Johnson, 2004). Less is known about jurisdictional variation in court decision-making before the sentencing event, but evidence suggests that similar organizational and contextual factors are likely at play (Hood & Schneider, 2019). Further, changes in law and policy are seldom implemented in a uniform manner across place, and may be translated and executed in ways that undermine their original intent (Edelman, 2016). Particularly during moments of policy transition, studies have found disjunctures between formal law and its implementation on-the-ground (Grattet & Jenness, 2005; Lynch, 1998; Lynch & Omori, 2014; Verma, 2015). Given the influence of pretrial outcomes in shaping later punishment outcomes (e.g., Dobbie et al., 2016; Gupta et al., 2016; Heaton et al., 2017; Peterson, 2019), and the role of monetary bail in contributing to racial disparities in the justice system (Demuth, 2003; Reaves, 2013; Sacks, Sainato, and Ackerman, 2015; Schlesinger, 2005), understanding how pretrial decisions and outcomes vary is of paramount importance.

In this chapter, I examine variation in pretrial decision-making across Maryland to explore whether courts and their broader social contexts matter for pretrial decision-making across distinct policy periods. Specifically, I describe (i) whether pretrial decision-making varies across courts before and after bail reform, and if so, (ii) whether features of the courts and their surrounding political, social, and economic contexts account for these variations; (iii) finally, I explore how courts social contexts shape responses to bail reform. I draw on several complementary theoretical perspectives on court variation, focal concerns and court as communities theories, as well as scholarship on legal change and implementation to inform this study. Using criminal court records data from the Maryland Judiciary Case Search, I use two-level multilevel linear and multilevel logistic models of cases nested in counties to examine the role of county-level sociopolitical, economic, demographic and court organizational characteristics in shaping pretrial determinations, both before and after bail reform.

## ***Theoretical Perspectives on Variation in Court Outcomes***

### **Individual-Level Frameworks**

Judicial decision-making is complex—weighing competing information and balancing various formal and substantive goals—and often repetitive. Particularly in the initial stages of case processing and in busy courts, legal decision-making is constrained by both incomplete information on the case, alleged crime, and defendant, and by limited time and judicial resources to gather additional information. No national, systematic data exist on the length of bail hearings, but reports from various jurisdictions across the United States note that bail hearings often take place within the span of minutes—ranging from one to three minutes in Harris County, Texas (Heaton et

al., 2017) and Philadelphia, PA (Stevenson, 2018b). These circumstances create uncertainty about arriving at a suitable pretrial determination, particularly when the overarching concerns for judicial officers at the bail stage are grounded in predictions of whether a given defendant will flee or commit new crime. Since judges “rarely, if ever, possess complete information” (Albonetti, 1991, p. 248) about cases or defendants, they depend on “perceptual shorthands” or attributions about defendant or case characteristics to manage uncertainty (Steffensmeier et al., 1998). Characteristics such as the current offense or a defendant’s criminal history are influential in assessing risk of flight or recidivism (Demuth, 2003; Demuth & Steffensmeier, 2004). However, other attributions, namely those based on race, ethnicity, or gender, may be projected onto judges’ expectations about whether that defendant is likely to flee or be a danger to public safety. In this way, such attributions may produce disparities in case processing and sentencing outcomes (Steffensmeier et al., 1998).

The focal concerns framework suggests that judges are guided in their decision-making by three substantive “focal” concerns: “the offender’s blameworthiness and the degree of harm caused the victim, protection of the community, and practical implications of sentencing decisions” (Steffensmeier et al., 1998, p. 767). First, judges consider the blameworthiness of the defendant, including the role they played in the crime, the harm caused to the victim, and mitigating circumstances. Similarly, protection of the community emphasizes concerns over the likelihood of re-offending and of incapacitating defendants perceived as “dangerous”. Court actors will seek more severe punishments—or, in the pretrial context, set more restrictive conditions of release or withhold release altogether—if they perceive certain defendants to be more culpable or

present a danger to others (Steffensmeier et al., 1998). Finally, practical considerations may be organizational or individual, including concerns over case processing efficiency, costs incurred by the criminal justice system, jail crowding, or a defendant's perceived "ability to do time" (Steffensmeier et al., 1998). Practical considerations may serve as constraints: for example, if less jail space is available due to overcrowding, judges may detain defendants perceived to be most dangerous or culpable. Conceptually, focal concerns is largely an account of decision-making at the individual-level, marshaled to explain demographic and other disparities in sentencing research (Lynch, 2019). However, the theory emphasizes that decision-makers are embedded in specific organizational and social contexts, which may shape shared understandings and attributions at the courtroom workgroup level (Ulmer, 2012). Normative understanding about typical cases or defendants may be influenced by, and differ across, local operational and community contexts of courts (Britt, 2000; Lynch, 2011; Ulmer, 2012).

Originally developed to explain sentencing disparities, the focal concerns framework has been extended to policing (Johnson et al., 2015), probation and parole (Stemen et al., 2015), solitary confinement (Logan et al., 2017), as well as pretrial decision-making (e.g., Demuth, 2003; Pinchevsky & Steiner, 2016; Schlesinger, 2005). Pretrial research employing the focal concerns perspective examines the role of individual-level factors on case outcomes. There, studies consistently find that *legally-relevant* characteristics, such as charge severity and other case or criminal history variables, are the strongest predictors of both pretrial release decisions and outcomes (Demuth, 2003; Schlesinger, 2005). Those charged with more serious crimes are less likely to receive release on recognizance (Ball & Bostaph, 2009; Pinchevsky & Steiner,

2016), are more likely to be detained pretrial (Demuth, 2003; Demuth & Steffensmeier, 2004; Pinchevsky & Steiner, 2016), and receive higher bail (Schlesinger, 2005; Turner et al., 2003; Turner & Johnson, 2005; Wooldredge, 2012; Wooldredge et al., 2017).

Specifying the influence of *extra-legal* factors, such as race, ethnicity and gender, has produced a robust scholarship at the pretrial stage (Demuth, 2003; Demuth & Steffensmeier, 2004; Katz & Spohn, 1995; Pinchevsky & Steiner, 2016; Sacks et al., 2015; Schlesinger, 2005). Studies find that Black and Latino defendants are routinely subject to worse pretrial outcomes, across contexts: Black and Latino defendants are less likely to receive nonfinancial release, while Latino defendants are more likely to receive higher bail amounts (Demuth, 2003; Sacks et al., 2015a; Schlesinger, 2005). Female defendants are treated more favorably throughout the pretrial period than men; women are less likely to receive preventative detention than men, and are more likely to receive ROR, to be assigned a lower bail amount, and to secure pretrial release (Demuth & Steffensmeier, 2004; Katz & Spohn, 1995; Pinchevsky & Steiner, 2016). Based on this research, in this study, I expect:

1. *Defendants facing more serious charges, multiple charges, and those with longer criminal justice histories will be more likely to face stricter pretrial outcomes, including higher bail, lower likelihood of nonfinancial release, and greater likelihood of detention without bail.*

Extra-legal considerations under focal concerns also include practical constraints. In the pretrial phase, the system constraint most examined is the bed capacity in local jails (D'Alessio & Stolzenberg, 1995; Hood & Schneider 2019; Pinchevsky & Steiner 2016; Williams, 2016). The growth of jail populations, and jail

crowding, have been leading concerns in policy dialogues over pretrial reform (Jorgensen & Smith, 2021). Early studies identified available resources, particularly jail bed space, as influential in bail policy and money bail setting practice (Flemming, 1982; Roth & Wice, 1980); more recent empirical evidence shows mixed results for its influence on pretrial outcomes. One study linked jail crowding with increases in the likelihood of nonfinancial release, and when bail is set, an increase in the likelihood of being held in custody on bail (Pinchevsky & Steiner, 2016). No significant association between jail crowding and bail amount is observed, suggesting that any amount may contribute to jail crowding (id). A study of eight Florida jurisdictions found the opposite relation: jail crowding was associated with a lower likelihood of nonfinancial release, along with lower bail amounts. There, the author suggests that judges are more willing to lower bail—rather than release outright—in contexts where jail capacity is a concern (Williams, 2016). A third study failed to detect any significant associations of local jail capacity on nonfinancial release or bail amounts, after accounting for political and economic contexts of counties (Hood & Schneider, 2019).<sup>51</sup> The nature—and direction—of this relationship is unclear and is unspecified in these correlational studies. Judges may be concerned with jail crowding pressures in their bail determinations, and consequently make more lenient decisions that facilitate release; however, many argue that overcrowding in local jails is a consequence of unaffordable bail (Scott-Hayward & Fradella, 2019). Following theoretical propositions of focal concerns, I expect that:

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<sup>51</sup> A study of pretrial detention in jails found positive association between jail capacity (not occupancy), or number of beds in a jail per 100,00 county residents, and use of pretrial detention (Ranson et al., 2023).

2. *Counties with more severe jail crowding will be associated with lower bail amounts, higher likelihood of nonfinancial release, and lower likelihood of detention without bail.*

## **Court Communities and Organizational Influences**

Decisions about individual cases and defendants are made in specific courts, involving routine interactions with many actors, including the judge, prosecution, defense, and other parties. Courts and court actors are also situated within organizational, social, economic, and political contexts, which influence court operations (Ulmer, 1997; Ulmer, 2019). The court community perspective views courts as communities, which are constituted by multiple actors' working relationships and by the court's relation to its sociopolitical environment (Eisenstein et al., 1988; Ulmer, 1997). In this framework, courts communities develop unique legal and organizational cultures—patterns of interactions, arrangements, and norms that shape case processing and court decision-making. These localized cultures and informal standards guide processing and sentencing for certain types of cases (Church, 1975; Eisenstein et al., 1988; Ulmer, 1997). This framework adds deeper contextual nuance to the largely individualized focal concerns approach to court decision-making, arguing that the influence of extra-legal demographic or socioeconomic characteristics on case outcomes is likely to be stronger in social contexts where biases exist (Kramer & Ulmer, 2009). Court communities theory identifies many possible contextual influences on court outcomes, including organizational features and constraints of courts, such as court size and caseload, community political pressures and local criminal justice resources, and wider demographic and economic characteristics of the community.

Organizational structures and bureaucratic concerns of courts may influence decision-making. The court communities framework argues that court size is crucial in shaping judicial decision-making, as the size of a given court as an organization is related to how visible routine court decisions are to those external to the court (e.g., to the public); how varied the types of cases are that are heard in court; and how independent a court's internal policies and operations are from external controls (Eisenstein et al., 1988). Larger courts are generally more autonomous and professionalized, possess more resources and bureaucratic processes, and have larger and more diverse courtroom workgroups compared with smaller courts. Owing to the volume of cases in large courts, day-to-day court decisions are also less visible to non-court actors. As such, large courts are expected to dispense more lenient decisions compared to smaller courts, an expectation that has found empirical support in sentencing research (e.g., Ulmer and Johnson, 2004; Johnson, 2006). Organizational goals, and bureaucratic concerns, of court systems may also pattern how cases are processed and disposed. Dixon (1995) argues that organizational efficiency is a paramount priority of courts, and judges are acutely concerned with caseload management and timely justice (Dixon, 1995; Engen & Steen, 2000; Hogarth, 1971). Judges with large caseloads may be less able to devote individualized attention and resources to each case, out of desire to provide timely resolution to court matters. Judges with fewer cases on their dockets may be less concerned about efficiency. To manage overwhelming dockets, organizational efficiency models argue that judges may encourage plea agreements (in lieu of lengthy and resource-intensive trials), and reward defendants who plead guilty with less severe sentences. Here, organizational pressures

that generate inefficiency, such as caseload pressure, may be expected to induce similar leniency at the pretrial phase. Organizational features of courts have only been examined in a single study on contextual determinants of pretrial decision-making, which found that jurisdictions with larger caseloads are associated with lower bail amounts and lower odds of being held on bail (Williams, 2018). Court communities and organizational efficiency models of court processing lead to the following expectations:

3. *Pretrial outcomes will vary across court jurisdictions, net of individual defendant and case factors.*
4. *Court jurisdictions with heavier caseloads will, on average, be associated with less restrictive bail conditions, including lower bail amounts, higher likelihood of nonfinancial release, and lower likelihood of pretrial detention.*
5. *Larger courts will behave in more lenient ways in the pretrial process, compared with smaller courts, including lower bail amounts, higher likelihood of nonfinancial release, and lower likelihood of detention without bail.*

### **Demographic, Economic, and Political Context**

Research finds that criminal justice outcomes may be influenced by a jurisdiction's demographic and economic context. A body of research has linked increasing minority populations with fear of crime (Liska, 1992), negative racial attitudes (Bobo & Hutchings, 1996), and increases in both arrest (Parker et al., 2005) and incarceration rates (Campbell et al., 2015). Group competition for resources and the unequal distribution of power across groups create disparities in the justice system (Liska, 1992). Blalock (1967) argued that as the relative size of a racial or ethnic minority group increases, members of the majority group perceive them as a growing

threat, either economically or politically. As a result, the majority group may demand more formal social control to maintain their advantage. Minority threat may operate through mechanisms identified by court community perspective: local, distinctive legal cultures may develop shared views on minority groups and shared perspectives on degree of threat posed by minority groups, incorporating those into normative understandings regarding court decisions.

In the context of sentencing, studies of minority threat yield mixed results: while some have found that defendants are more likely to be imprisoned in jurisdictions with larger Black population (Britt, 2000; Myers & Talarico, 1987; Wang & Mears, 2015), others have found little support (Fearn, 2005; Ulmer & Johnson, 2004; Ulmer, 1997). At the pretrial stage, few studies examine contextual-level demographics on pretrial decisions, with mixed results. Two studies find no evidence of influence of contextual-level racial and ethnic homogeneity (St. Louis, 2020) or racial composition on bail decisions (Williams & Rosenfeld, 2016). Conversely, Hood and Schneider (2019) find a significant negative relationship between the size of the Black population at the county level and the probability of nonfinancial release, net of case characteristics. Similarly, Williams, Vaughn, and Vogel (2024) find a negative association between county Black population and both ROR and bail amounts. However, they also find that these relationships are nonlinear—after a certain tipping point in the proportion of the county population that is Black, both the odds of ROR and average bail amounts begin to increase.

Economic conditions may also exert influence early-stage court outcomes. High unemployment or poverty may lead to more punitive practices out of fear of crimes

motivated by scarce resources (Liska 1992). Like minority threat, studies examining the role of jurisdiction-level economic characteristics in predicting pretrial outcomes have yielded mixed results. Three studies find positive associations between county-level unemployment and pretrial outcomes, including pretrial detention (D'Alessio & Stolzenberg, 2002) and bail amounts (Hood & Schneider, 2019; Williams et al., 2024), while other work finds a negative relationship between county levels of household disadvantage and bail amounts (Pinchevsky & Steiner, 2016). Wooldredge et al. (2017) use a composite measure of neighborhood disadvantage at the street block level, and find no significant association with bond amounts, but a positive association between neighborhood disadvantage and pretrial detention. These studies suggest that economic contexts may play a role in influencing pretrial decisions, though findings vary in part due to differences in measures employed and in the spatial unit of analysis. Following theoretical expectations, however, I expect:

6. *Jurisdictions with higher percentages of Black, Latino, or economically disadvantaged groups will be associated with more restrictive pretrial outcomes, including higher bail, lower likelihood of nonfinancial release and higher likelihood of detention without bail.*

Finally, political ideology and punitive attitudes have been associated with increased punitiveness in criminal justice settings (Beckett & Sasson, 2004; Campbell & Schoenfeld, 2013). Focal concerns and court communities perspectives argue that judges are cognizant and responsive to their political environments (Fearn, 2005; Ulmer et al., 2008). Areas with higher concentrations of residents who hold more conservative beliefs have been found to hold more punitive, law-and-order orientations (Unnever &

Cullen, 2006). How county political context influences pretrial decision-making has been explored in only two studies: Hood and Schneider (2019) find that counties with non-Democratic prosecuting attorneys are associated with lower likelihoods of nonfinancial release and higher bail amounts, while counties in states with a Republican governor are associated with lower rates of nonfinancial release and lower bail amounts. Williams (2018) finds that more conservative counties are associated with higher bail amounts, but lower likelihood of being held on bail. Courts embedded in more conservative communities may be more likely to hold more punitive orientations in the pretrial stage, as the few studies above generally suggest. This leads to the following expectation:

7. *Jurisdictions with more conservative political orientations will be associated with more restrictive pretrial outcomes, including higher bail amounts, lower likelihood of nonfinancial release, and higher likelihood of detention without bail.*

Finally, research examining how law is implemented has long identified gaps between formal law and law as it is enacted on the ground. This body of work examines local variation in the meaning and practice of law, demonstrating how higher laws are translated through existing operational norms and localized perspectives, both across place and over time (Grattet & Jenness, 2005; Lynch et al., 2021; Lynch & Omori, 2014; Rengifo et al., 2017; Verma 2015). Scholars contend that organizational, normative, political, and institutional factors shape the local application of law (Lynch & Omori, 2014; Verma, 2015). Scholarship across several disciplines documents various mechanisms accounting for variability in law: individuals' discretion and interpretation of rules (Lipsky, 1980; Lynch, 1998; Rengifo et al., 2017); organizations' intermediary role

in translating higher law into policy and procedure (Grattet & Jenness 2005); and processes that produce similarity in structures and organizational practices across a body of organizations (DiMaggio & Powell, 1993). The pretrial process corresponds with many of the conditions that generate local variation in the translation and application of law. Multiple, largely independent courts and court actors are responsible for translating procedural court rules into action, and substantial discretion is afforded to judges and bail commissioners in the interpretation of the law's meaning. As observed by Eisenstein et al., 1988: "[r]eform from above does not operate in a vacuum of Law. If changes occur, they must take place in the real world of criminal court communities, where the decisions have to be implemented" (296).

Scholars argue that discretion and legal ambiguity operate as facilitators to variation in implementation. Local actors often have discretion in interpreting and acting on law, from local organizational leaders to front-line workers (Lipsky, 1980). Policy changes that limit the discretion of front-line workers (Steiner et al., 2011) or challenge existing norms held by staff (Rengifo et al., 2017) may be resisted by those implementing it. Organizations, too, shape the meaning of law, both on their own and through interaction with their institutional and local environments (DiMaggio & Powell, 1973; Grattet & Jenness, 2005). Organizations serve as intermediaries in the process of translating higher law into actionable policy and specific procedure for frontline staff, and in that process, are subject to a variety of contextual, organizational, and historical influences. Historical practices and interpretative lenses may also shape how new law is made into action (Lynch & Omori, 2014; Verma, 2015). In their study of district-level variation in sentencing practices before and after major changes to federal sentencing

guidelines, Lynch and Omori (2014) find substantial interdistrict variation shaped by localized orientations to charging and sentencing, and conclude that “local legal practices not only diverge in important ways across place but also become entrenched over time such that top-down legal reform is largely re-appropriated and absorbed into locally established practices” (411).

Sociolegal scholars argue that variability in implementation is facilitated by ambiguity of the law itself. When key terms are not defined or where the text of the law is “vague” (Edelman, 1992) or “broad” (Edelman, Uggen and Erlanger, 1999), substantial discretion arises for those responsible for interpreting and implement it. This, in turn, is likely to increase variability in how law is operationalized at the local level (Grattet & Jenness, 2005). As discussed in Chapter 1, a stated goal of the new bail rules was to increase fairness and rationality in judicial decision-making, mandating “individualized consideration” of each criminal defendants financial status (192<sup>nd</sup> Supplement, p. 11). But those rules provided little additional guidance in how to gather specific financial information on each defendant, how much weight to give to various items, or what constitutes an unaffordable amount of bail. From the review of this literature, I derive two expectations: first, generally, I expect that the influence of court jurisdiction on pretrial decisions to matter less following bail reform, as judicial officers give greater consideration to individual case factors. Next, although there are no clear theoretical directions, sociolegal scholarship and court communities theory lead to an expectation that the implementation of bail reform will be moderated by the contextual features of courts. Formally:

8. *The influence of jurisdiction on pretrial decisions will decrease following bail reform.*
9. *Court organizational and contextual factors will moderate the impact of bail reform on pretrial decision-making.*

Together, the frameworks reviewed above provide the theoretical grounding that guides the empirical study of this chapter. Focal concerns and court-as-communities approaches provide ways of thinking about how judicial officers might weigh factors in their pretrial determinations, and how the weighing of those factors may be shaped or constrained by features of the court, local justice systems, and broader community contexts in which decision-makers are embedded. Sociolegal frameworks provide ways to think about jurisdictional response to policy change, and shape an expectation that courts translate new policy into action in ways that are patterned by their existing social contexts and past practices.

### ***Data Sources and Sample***

I use case-level criminal court data from all district courts across 23 counties and the City of Baltimore, from 2014 to 2019. The sample is restricted to new criminal filings initiated under a “statement of charges.” These cases are initiated pursuant to “on-view” arrests, which originate through an encounter between police and a citizen that results in a custodial arrest. Excluded are cases which do not uniformly require a bail determination (e.g., citations, summons), cases involving a probation or parole violation, warrants, and appeals.

Each case in the MJCS data contains information on defendants, case processing, and pretrial determination outcomes. Defendant characteristics include

race, gender, and date of birth. Case characteristics include court location, filing date, and case type, along with pretrial hearings and decisions; bail and bond setting information; and charge information. Prior research demonstrates the influence of criminal history on pretrial determinations (Nagel, 1983; Pinchevsky & Steiner, 2016; Sacks & Ackerman, 2014; Schlesinger, 2005). For this study, I construct a measure of criminal history, count of prior criminal cases filed, using existing court records.<sup>52</sup>

Drawing from prior research on organizational influences on court processes and outcomes (Dixon, 1995; Eisenstein et al., 1988; Ulmer, 1997), I have obtained information on organizational characteristics of Maryland's district courts. The number of new criminal filings for each district court and the number of full-time judicial officers are compiled from Maryland Judiciary Annual Statistical Reports for the fiscal years 2014 through 2019. I use annual county-level crime data from the Maryland Statistical Analysis Center, which compiles data from Maryland Uniform Crime Reporting Program. Jail information is obtained from the Bureau of Justice Statistics 2019 Census of Jails and the 2014-2018 Annual Survey of Jails. I use data on county-level demographic and economic characteristics of each county<sup>53</sup> from the Census Bureau's 2016 American Community Survey (ACS) 5-year estimates. Finally, data on political orientation at the county level is drawn from MIT Election Lab data on county-level voter returns for the 2016 Presidential election.

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<sup>52</sup> To do this, I use person-level identifying information, including name, date of birth, and sex to match people across cases in the data (using full data available from 2000-2019). Criminal history information is limited to cases occurring in the State of Maryland.

<sup>53</sup> Baltimore City is a separate administrative jurisdiction from other counties in the state. As such, it is treated as a group-level unit. For convenience, when referencing "county"-level variation in Maryland, this includes Baltimore City as unit.

## **Dependent Variables**

I examine the role of county contextual and organizational factors on variation in three pretrial decisions: nonfinancial release, detention without bail, and bail amount. *Nonfinancial release* is defined as release on either own recognizance (ROR) or unsecured personal bond (UPB) and is observed for each case in the data using court event activity. Release on non-financial conditions is a dichotomous outcome measure at the case level, where the value is set to 1 for each case with an ROR or UPB release decision made at the initial pretrial determination, and 0 for all other release determinations, including bail set and held without bail (HWOB). *Detained Without Bail* is defined as a pretrial determination that results in the defendant being preventatively detained in jail custody, with no release conditions set. Detained without bail is a dichotomous outcome measure at the case level, where the value is set to 1 for each HWOB release decision made at the initial pretrial determination, and 0 for all other release determinations including bail set, ROR and UPB. *Bail Amount* is defined as the dollar amount of money bail set at the initial bail hearing. Bail amount is log transformed (Fox, 2008; Demuth, 2003; Pinchevsky & Steiner, 2016; St. Louis, 2020; Wooldredge et al., 2017). Bail amount is examined only for those individuals who receive bail at the initial bail determination, as those who are released without monetary conditions or detained pretrial do not have a bail amount set.

## **Organizational and Contextual Variables**

The primary aims of this chapter are to examine variation in pretrial decision-making across court jurisdictions, describe the influence of contextual and organizational factors on pretrial decisions, and explore whether these contexts shape

counties' responses to bail reform. All contextual and organizational variables examined in this study are measured at the county level.<sup>54</sup>

The first set of contextual variables I include relate to county-level economic conditions. I use two measures of county economic context: *poverty*, or the percentage of county residents living under the Federal poverty line; and *unemployment*, or the percentage of county residents who are currently unemployed. Second, I include a set of measures of racial/ethnic composition of each county: *county Black population*, defined as the percentage of the county population that is Black, non-Hispanic or Latino (Bontrager et al., 2005; Hood & Schneider, 2019; Talarico & Myers, 1987; Ulmer & Johnson, 2004), *county Latino population*, defined as the percentage of the county population that is Latino or Hispanic. To capture larger sociopolitical environment, I include a measure of conservative political attitudes, measured as *percent Republican voters* in the 2016 presidential election (Fearn, 2005; Hester & Sevigny, 2016; Johnson, 2006; Pinchevsky & Steiner, 2016), as well as a measure of annual county *violent crime rate per 100,000 residents* (Lynch et al., 2021; Pinchevsky & Steiner, 2016) to explore the role of larger public safety concerns. To explore the role of organizational characteristics of courts, I include a measure of *court size*, measured by number of judges in each district court, categorized as small (1-6), medium (7-15), and large (16+) (Ulmer 1997; Thompson et al., 2019);<sup>55</sup> and *annual county caseload per judge*,

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<sup>54</sup> Case level data did not contain address information (only zip code), precluding analyses of contextual influences at spatial units smaller than counties.

<sup>55</sup> Historical data on commissioners in each county was not available. As a proxy, I use data on the number of judges, drawn from annual statistical reports generated by the Maryland Judiciary. The District Court division of the Maryland court system is organized into 12 geographical districts, which may include one or more political subdivisions (6 districts contain only a single county; 3 districts contain 2 counties; 1 district contains 3 counties; 1 district contains 4 counties; and 1 district contains 5 counties). Each county, however, has its own district court location where cases in that county are filed (larger counties contain several district courts locations). Data on the number of judges is reported by the Maryland Judiciary at

measured as the annual ratio of filed criminal cases per judge (Johnson et al., 2008; Lynch et al., 2021).<sup>56</sup> Lastly, I include annual percent *jail crowding*, measured as the percentage of rated capacity occupied on any given day (Hood & Schneider, 2019; Pinchevsky & Steiner, 2016; Williams, 2016).

### **Period Indicators**

Bail reform in Maryland went into effect on July 1, 2017, for all new criminal cases.<sup>57</sup> To examine the impact of bail reform on pretrial decisions, I include a dummy variable, *post-Reform*, coded as 1 for each case with an initial bail hearing on or after the post-bail reform period, and 0 if the initial hearing was before the reform. I also include dummy variables for each year, to control for any time invariant influences that affected all cases in each period.

### **Individual-Level Covariates**

This study includes individual-level variables to control for the influence of individual characteristics on pretrial determinations, each generated from the MJCS court data. Following prior research demonstrating the role of race, age, and gender in predicting pretrial outcomes, I include demographic characteristics, such as defendant *race* (Black, White, Asian/Pacific Islander, Indigenous/Native American, Other), where White is the reference category; defendant *age in years*, measured at the time of case filing; and defendant gender, *female* (where female is coded as 1 and male as 0). This study also controls for case characteristics known to be associated with pretrial

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the district level (rather than at the court location or county level). To assign a count of judges to each county, I divide the number of judges in a given year by the number of counties in each district.

<sup>56</sup> Annual criminal cases filed are reported at the county level.

<sup>57</sup> Analyses were also run using the policy reform announcement date and results were largely consistent with the main results presented here.

outcomes, including offense severity, offense type, and number of charges. *Offense severity* of the most serious charge offense is measured based on the Maryland Sentencing Guidelines charge severity scale, such that higher values represent more serious offenses and lower values represent less serious offenses.<sup>58</sup> *Offense type* of most serious charged offense is also included (person, property, drug, other), where property is the reference category. *Total count of charges* associated with the case is a continuous variable that indicates the number of charges associated with each case. Finally, I include a criminal history variable, *count of prior criminal cases filed*, a continuous variable ranging from 1 to 40 (truncated to adjust for extreme outliers).<sup>59</sup>

### ***Empirical Approach***

To examine how court jurisdictions varied in pretrial practices both before and after bail reform, I use two-level multilevel models of cases nested in counties.<sup>60</sup> Multilevel modeling techniques are an appropriate method for dealing with nested data structures, particularly where the substantive interest is in the impact of group-level factors (Bosker & Snijders, 1999). Because I aim to describe between-court variation and identify predictors of group-level variation, multilevel modeling techniques are ideal

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<sup>58</sup> The scale of the Maryland Sentencing Guidelines offense severity ranges from 1-7, with 1 representing the most serious crimes and 7 representing less serious crimes. Because the guidelines do not prescribe sentence recommendations and severity ratings for all crimes, I group additional lower level offenses found in the data (e.g., public order offenses) as a ranking of 8. In the analyses, these are reverse coded, such that lower values represent the least severe offense and higher values represent the most severe charges.

<sup>59</sup> For multilevel logistic models, total count of charges and count of prior criminal cases were transformed to binary indicator variables to achieve model convergence. While this may be a limitation with respect to examining associations between individual-level factors and pretrial outcomes, it may be less of a concern for this study, as the focus is on the influence of county-level factors on bail, non-financial release, and pretrial detention.

<sup>60</sup> Additional nesting structures were also explored, including three-level models of cases nested in individual district court locations nested in counties. However, the majority of counties (18 of the 24) have only a single district court location, and because of the concern over the small number of clusters at each level, a two-level model was preferred.

here. Multilevel modeling techniques for analyses of between-jurisdiction variation in court research has been used across sentencing studies (Britt, 2000; Fearn, 2005; Kautt, 2002; Lynch, Mona and Omori, 2014; Ulmer & Johnson, 2004), and a more limited number of pretrial studies (Pinchevsky & Steiner, 2016; St. Louis, 2020; Wooldredge et al., 2017; Williams, 2018). In this study, individual cases are nested within district court locations spread across 23 counties and the City of Baltimore (24 total groups). Given the nature of the dependent variables, I use multilevel logistic regression models to examine pretrial release outcomes, including nonfinancial release and preventative detention, and use multilevel linear models to assess log bail amounts.

**Step 1:** For each outcome (non-financial release, preventative detention, logged bail amount), I estimate unconditional models within counties and calculate the intraclass correlation coefficient (“ICC”) to capture the amount of variation in outcomes attributed to between-county variation. The ICC is the proportion of variance at the group (i.e., county) level out of the total variance (Bosker & Snijders, 1999). The ICC is commonly used as the measure of significant interjurisdictional variation in court research (e.g., Johnson, 2006; Wooldredge, 2010). Following the notation of Raudenbush & Bryk (2002), I show the equations for the individual-level model (level 1) and group-level models, or random effects, (level 2), separately. The unconditional model predicting logged bail amounts is shown in equations (1) and (2) below:

$$Y_{ij} = \beta_{0j} + r_{ij} \quad (1)$$

$$\beta_{0j} = \gamma_{00} + u_{0j} \quad (2)$$

where  $Y_{ij}$  is the logged bail amount for each individual  $i$  in county  $j$ , which is modeled as a function of a county-specific intercept,  $\beta_{0j}$ , and an individual-level error term,  $r_{ij}$ . The

county-specific intercept,  $\beta_{0j}$ , is estimated as an overall level 2 intercept,  $\gamma_{00}$ , and a county-level error term  $u_{0j}$  (random effect). For a two-level linear model, the ICC is estimated by:

$$\rho = \frac{\tau_{00}}{(\sigma^2 + \tau_{00})}$$

Where  $\rho$  is the ratio of the between-group variance to the total variance in the outcome,  $\tau_{00}$  is the between-group variance estimated by the  $u_{0j}$  parameter and  $\sigma^2$  is the within-group variance estimated by  $r_{ij}$ .

For binary outcomes (nonfinancial release and detention) estimated using multilevel logistic models, the unconditional model for the log odds is:

$$\log (y = 1|1 - y = 1)_{ij} = \beta_{0j} + r_{ij}$$

Where  $\log (y = 1|1 - y = 1)_{ij}$  is the log odds of individual  $i$  in county  $j$  being either released on nonfinancial conditions or detained without bail, estimated by a county-specific intercept,  $\beta_{0j}$ , and an individual-level error term,  $r_{ij}$ . Level 1 variance in multilevel logistic regression is taken to follow a standard logistic distribution ( $\pi^2/3$ ), and the ICC is:

$$\rho = \frac{\tau_{00}}{(\pi^2/3 + \tau_{00})}$$

To examine whether variation across counties changed from the pre-reform period to the post-reform period, for each outcome, I examine variation in pretrial decisions across counties over time by estimating the unconditional models for each policy period (pre- and post-reform) across my data and calculating period-specific ICCs.

**Step 2:** I then run a series of full random intercept and slopes models, with individual cases nested within counties to examine the extent to which county-level

variables influence between-court variation on pretrial decisions. Here, I include level 1 predictors of the outcome in the model, given by the matrix,  $\beta_{kj}X_{ij}$  in equation 3, along with a bail reform period indicator,  $\beta_{1j}Post_{ij}$ . I include year fixed effects to control for time trends across the sample period that are independent of bail reform (Starr & Rehavi, 2013). In the full models, I also include predictors of the random intercept, where the county-specific intercept ( $\beta_{0j}$ ) is a product of the overall mean,  $\gamma_{00}$ , as well as  $W_j$  level 2 predictors, and level 2 error term  $u_{0j}$ . I allow for the coefficient on the post-reform period to vary across counties (equation 5) in order to examine variation in the pretrial policy's effects between counties.

$$Y_{ij} = \beta_{0j} + \beta_{1j}Post_{ij} + \beta_{kj}X_{ij} + r_{ij} \quad (3)$$

$$\beta_{0j} = \gamma_{00} + \gamma_{01}W_j + u_{0j} \quad (4)$$

$$\beta_{1j} = \gamma_{10} + u_{1j} \quad (5)$$

**Step 3:** In order to understand whether the association between level 2 predictors and pretrial outcomes is moderated by bail reform, I re-estimate the model given in (3) and (4), but incorporate cross-level interactions between the  $Post_{ij}$  and the county-level predictors  $W_j$  shown in equation (6) below:

$$\beta_{1j} = \gamma_{10} + \gamma_{11}W_j + u_{1j} \quad (6)$$

Here, the level 2 predictors are being used to explain variation in the effect of  $\beta_{1j}$  across counties on pretrial outcomes.  $\gamma_{11}$  represents the cross-level interaction between  $Post_{ij}$  and  $W_j$ .

In all models, level 1 predictors are group-mean centered, which removes the between-group variation in individual-level covariates, while level 2 variables are grand-

mean centered.<sup>61</sup> An important assumption of multilevel models is that predictor variables included in the model do not covary with either the level 1 residuals,  $r$ , or the random effects,  $u$  (McNeish & Kelley, 2019). Group-mean centering of individual level predictors alleviates concerns over correlations between level 1 predictors and random effects ( $u$ ) (Bafumi & Gelman, 2006; McNeish & Kelley, 2019). Multilevel linear models were fit using REML using *mixed* command and multilevel logistic models were fit using *xtmelogit* in Stata 17.0.

### **Supplementary Analysis**

To supplement the primary multilevel modeling approach taken here, I also fit a series of fixed effects models (FE) to the data to examine how county-level contextual contributors moderate the influence of bail reform on pretrial decisions across counties. Unlike multilevel models, FE models do not use random effects or random coefficients (the  $u$  parameters above). Instead, dummy variables for each level 2 group (here, for each county, excluding a reference group) are entered into the model as predictor variables, yielding group-specific intercept estimates. The primary benefit of FE models is that there are fewer assumptions compared with multilevel models. All time-invariant variance attributable to group-level measures (whether explicitly included in the model or not) is consumed by the group dummy variables, making such models less susceptible to omitted variable bias at the group-level than the multilevel approach (McNeish & Kelly, 2019). Individual-level variables are generally the primary focus in the

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<sup>61</sup> Additional models that also included group-level averages of level 1 predictors were run to examine the between-group effects of level 1 predictors, or the impacts of aggregate caseload composition at the jurisdiction level (“the hybrid model”, Allison 2009). Group-level averages were largely insignificant in the models, and as such, were removed from the final models.

FE approach and coefficient estimates represent within-group effects. One consequence of this is that FE models are not able to directly estimate the effects of time-invariant level 2 variables—meaning that FE models cannot be used to model the influence of county-level contextual effects on pretrial outcomes. Because of this limitation and because my primary interest is in describing the association between group-level court organizational and social contexts on pretrial outcomes, I use a multilevel approach above as the primary analytical approach to the analysis. However, I can use a FE approach to examine how county contextual characteristics *moderate* the impact of bail reform, which may estimate the moderating effects with fewer assumptions (and less bias) than the multilevel approach. As a supplementary analysis, I fit FE models with interaction terms for each county-level contextual variable and the post-reform dummy indicator (excluding main effects for time-invariant county-level variables), adjusting for all individual-level covariates, year fixed effects, and county fixed effects. In each FE model, I use cluster-robust standard errors at the county level.

## **Results**

### **Descriptive Findings**

Table 9 provides descriptive statistics for outcome variables, individual level predictors, and county level predictors for the total sample, and before and after Maryland's bail reform. Across the sample, 47 percent of defendants were released on nonfinancial conditions, 36 percent were assigned a monetary bail condition, and 15 percent were detained pretrial. Of those assigned monetary bail, the average bail set was \$11,042. Most relevant to the dissertation study, most pretrial outcomes varied significantly before and after the bail reform period. Bail reform was specifically targeted

at judicial behavior with respect to money bail, and there, even descriptively, I observe substantial decreases in monetary bail setting and bail amounts from the pre-reform period: bail setting decreased from 49 percent to 22 percent, and average bail amounts dropped from over \$18,620 to \$2,174. Conversely, the use of pretrial detention rose significantly from just 7 percent in the pre-reform period to 25 percent following bail reform. The rate of nonfinancial release changed less between periods, increasing from 44 percent to 50 percent following bail reform. Although descriptive changes in pretrial outcomes cannot be attributed to the reform absent further analysis, these findings provide initial evidence of the influence of the new bail rules and motivate the analyses to come.

Looking descriptively at individual case characteristics of the sample, male defendants accounted for most of the sample (79%) and the average age at filing was 33 years. Black defendants comprised over 60 percent of the sample, followed by White defendants (38%). On average, defendants faced 2.9 charges per case; with respect to crime type, 42 percent of charges were person crimes, 31 percent drug crimes, 26 percent property, and just 1 percent other. Defendants also had an average of 4.9 criminal cases filed in Maryland courts prior to the current case.

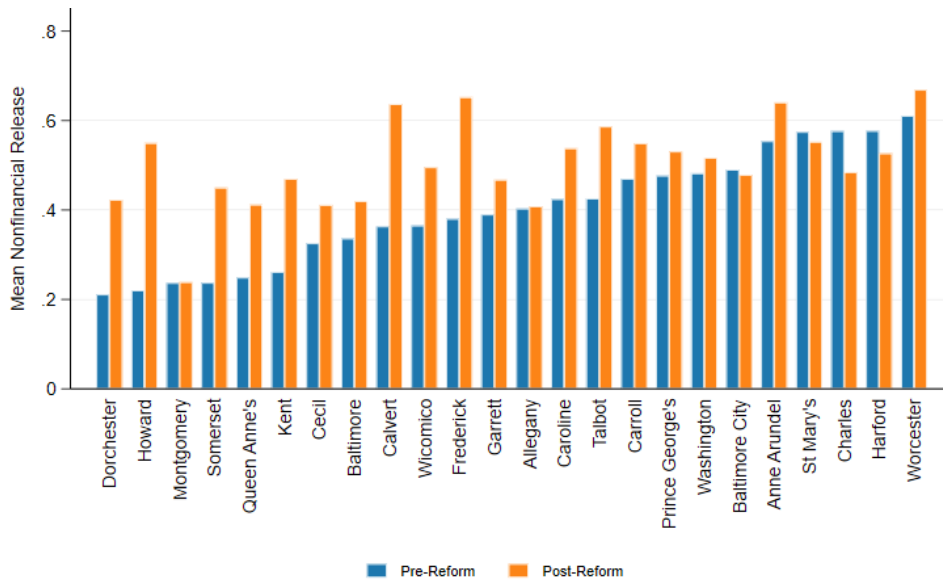
Table 9. Descriptive Statistics of Dependent and Independent Variables, Years 2014-2019 (n= 296,329)

	Pre-Reform	Post-Reform	Total
<b>Outcomes</b>			
Nonfinancial Release	0.44 (0.50)	0.50 (0.50)	0.47 (0.50)
Money Bail Set*	0.49 (0.50)	0.22 (0.41)	0.36 (0.48)
Bail Amount (\$)	18,620.48 (94,907.25)	2,174.36 (7,484.68)	11,041.94 (70,354.08)
Logged Bail Amount	9.25 (1.43)	8.39 (1.07)	9.01 (1.39)
Detained Pretrial	0.07 (0.25)	0.25 (0.43)	0.15 (0.36)
<b>Individual Level Factors</b>			
Age in Years	32.57 (11.49)	33.46 (11.57)	32.98 (11.53)
<b>Gender</b>			
Male	79.80%	77.80%	78.90%
Female	20.20%	22.20%	21.10%
<b>Race</b>			
Native American	0.10%	0.10%	0.10%
Asian/Pacific Islander	0.50%	0.80%	0.60%
Black	61.50%	53.90%	60.10%
White	36.50%	43.90%	37.80%
Other	1.30%	1.30%	1.30%
Offense Severity (1-8)	3.82 (1.73)	3.30 (1.53)	3.58 (1.66)
Count of Prior Cases	4.71 (6.46)	5.17 (7.14)	4.92 (6.79)
<b>Offense Type</b>			
Drug	34.20%	27.90%	31.30%
Other	1.10%	0.80%	1.00%
Person	39.60%	45.10%	42.10%
Property	25.10%	29.20%	25.60%
Total Charges	2.89 (2.64)	2.96 (2.87)	2.92 (2.74)
Total Cases (i)	159,779	136,550	296,329
<b>County (n=24) Factors</b>			
Annual Percent Jail Crowding	73.98 (18.26)	70.72 (18.18)	72.46 (18.25)
Annual Caseload Per Judge	1404.49 (708.92)	1271.10 (664.98)	1360.19 (701.56)
Percent Black	-	-	20.30 (16.68)
Percent Latino	-	-	6.20 (4.27)
Percent in Poverty	-	-	10.21 (4.82)
Percent Unemployed	-	-	5.00 (1.67)
Percent Republican	-	-	49.64 (17.77)
Annual Violent Crime Rate per 100k	358.81 (306.37)	347.35 (353.60)	351.96 (317.72)
<b>Court Size</b>			
Small	-	-	71%
Medium	-	-	21%
Large	-	-	8%

\*Money bail set is not examined as an outcome variable in the models. The rates here are shown for context. Standard deviations are in parentheses. County-level factors for racial/ethnic composition, economic context, court size, political context measures do not vary over time.

Of the 24 counties, 17 (71%) are small courts, 5 (21%) are medium sized courts, and just two (8%) are large courts, with over 16 judges. On average, judges across the counties heard 1,360 new criminal cases each year. Jails were occupied at an average of 72 percent capacity, with a low of 38 percent occupancy and a high of 133 percent of the rated capacity. With respect to racial/ethnic contexts of counties, on average, Black or African American people comprised 20 percent of county populations and Latino people comprised just over 6 percent. On average, just over 10 percent of county residents were living in poverty and 5 percent were unemployed. Counties had an average of 352 violent crimes per 100,000 residents, and on average, 49 percent voted for a Republican candidate in the 2016 presidential election.

Figure 16. Rate of Nonfinancial Release Before and After Bail Reform, By County



I observe significant county-level variation in the average bail amounts set, rates of nonfinancial release, and rates of pretrial detention at the initial bail determination, both before and after bail reform (Figures 16-18). Counties varied in the rate of release

on nonfinancial conditions before bail reform, with Dorchester County releasing just over 20 percent of defendants on nonfinancial release to Harford and Worcester counties using nonfinancial release options more often, at nearly 60 percent of defendants (Figure 16). Following bail reform, use of nonfinancial release increased in many, but not all, counties. Notably, in some of the largest jurisdictions, including Baltimore City and Montgomery County, nonfinancial release decreased slightly in the period following bail reform. Similarly, average bail amounts (Figure 17) showed wide between-county variability in the pre-reform period, ranging from averages of \$14,000 in Garrett and Montgomery counties to well over \$60,000 in Baltimore City.<sup>62</sup> Following bail reform, average bail amounts dropped dramatically across counties. Finally, prior to bail reform, mean rates of detention at the bail hearing ranged from just 2 percent to 12 percent across counties; following the new rules, use of detention rose substantially and non-uniformly across counties (Figure 18). These findings motivate further examination of what accounts for county-by-county variability in pretrial outcomes, evident in the periods before and after bail reform. In the next section, I turn to the multilevel models to explore what accounts for this variability.

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<sup>62</sup> As noted earlier, bail amounts are positively skewed, accounting for the high average figures presented in Figure 2. Median bail amounts range from \$3000 to a high of \$20000. For this reason, the analyses are conducted on natural log transformed bail amounts.

Figure 17. Average Bail Amount Before and After Bail Reform, By County

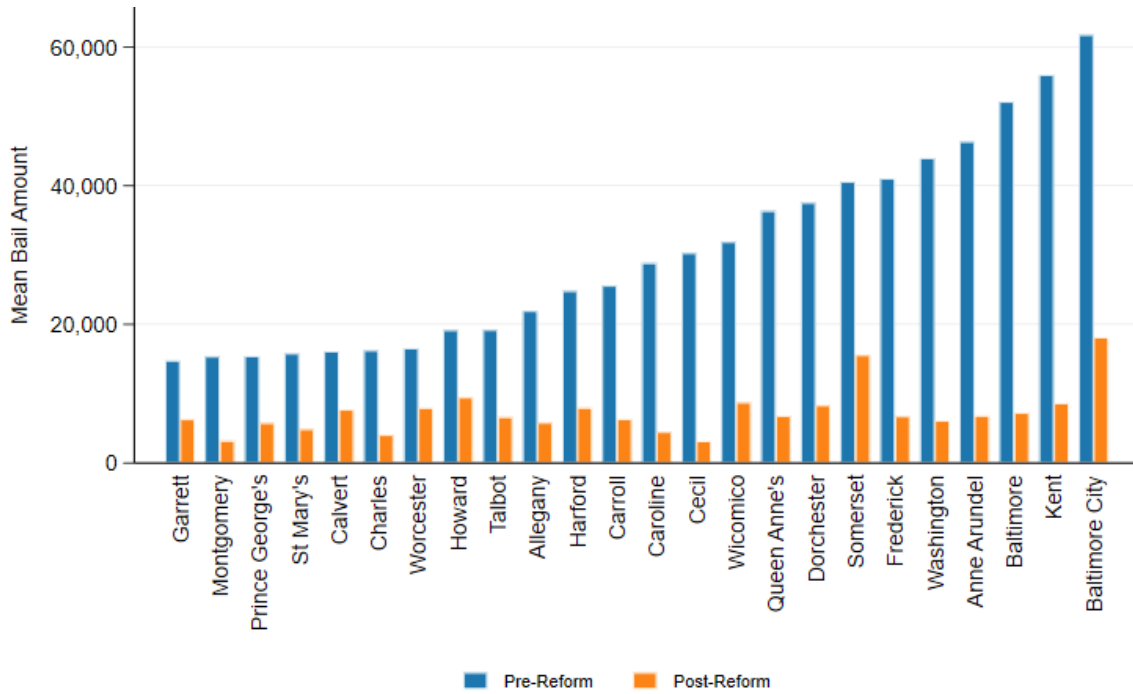
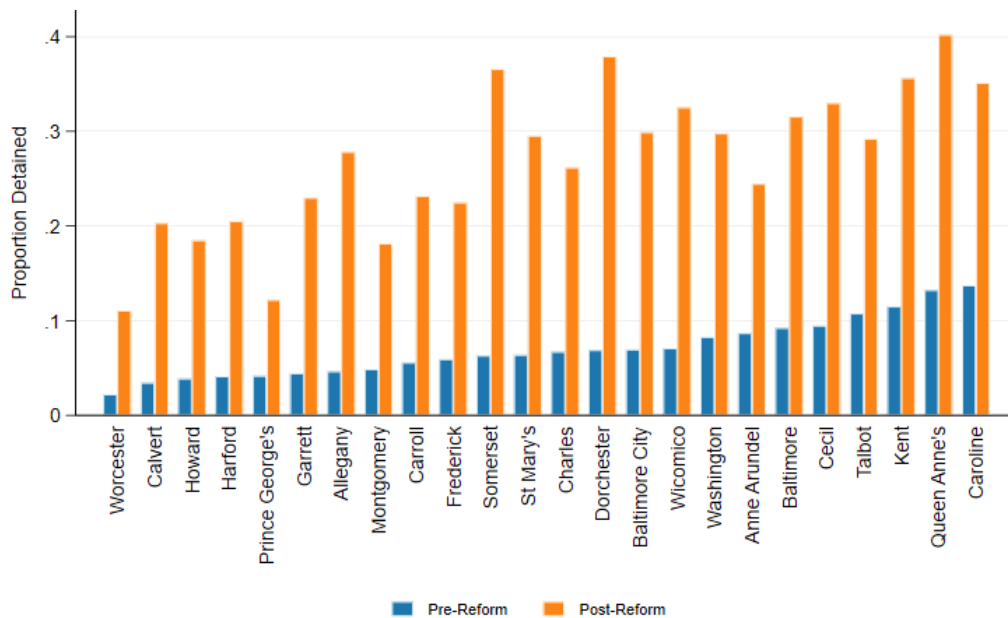


Figure 18. Mean Rate of Detention at Initial Bail Hearing Before and After Bail Reform, By County



## Between-County Variation in Pretrial Decision-Making

To address whether the county in which a case is heard matters for pretrial outcomes, I first examine the variance components and ICCs for individual cases nested in districts. Table 10 presents the unconditional models for logged bail amount, nonfinancial release, and detention without bail. The majority of variance in pretrial outcomes is accounted for at the individual case level compared with variation between counties. However, about 9 percent of the total variation in logged bail amount is due to differences across counties, whereas about 4.5 percent and 5.8 percent of the variation in the odds of nonfinancial release and detention, respectively, is due to between-county differences. Studies of variation in court outcomes across court jurisdictions find comparable levels of between-group variability (Lynch et al 2021; Lynch and Omori 2014; Johnson, 2006).

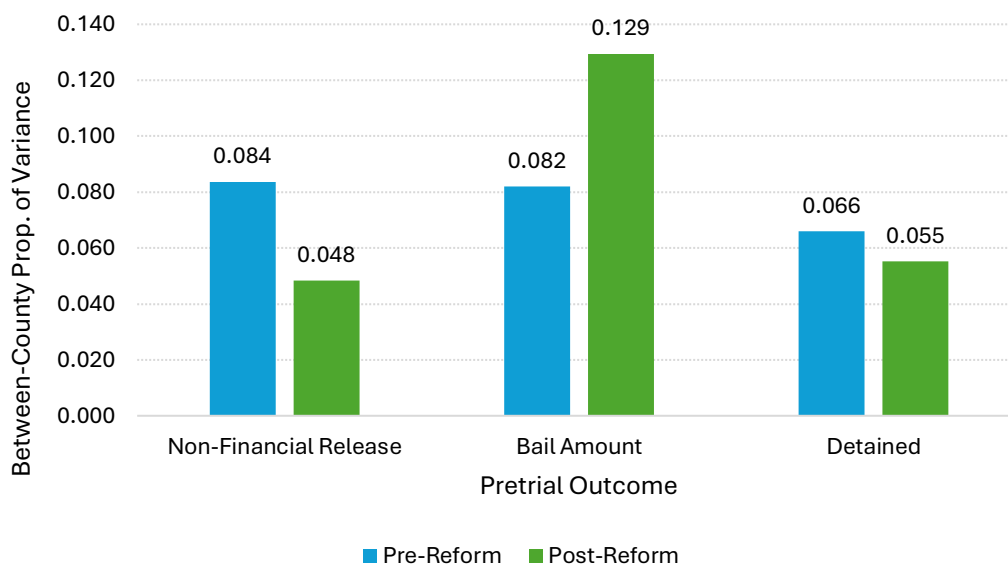
Table 10. Unconditional Logged Bail Amount, Nonfinancial Release, Detention Models

	Logged Bail Amount		Nonfinancial Release		Detained	
	Estimate	(SE)	Estimate	(SE)	Estimate	(SE)
<b>Intercept</b>	8.972	0.083	0.850	0.068	0.210	0.019
<b>Random Effects</b>						
<b>Level 2 - County</b>	0.163	0.048	0.154	0.045	0.201	0.059
<b>Level 1 - Case</b>	1.655	0.007				
<b>Between-County Proportion of Total Variance (ICC)</b>	0.090	0.024	0.045	0.012	0.058	0.016

One of the aims of the bail reform was to reduce arbitrariness of pretrial outcomes and focus judicial decision-making on individualized assessment of case factors and defendant characteristics. By extension, these reforms sought to reduce the influence of locale on judicial decision-making. To explore this question, I estimate unconditional models of cases nested in counties for pre- and post-reform periods and

examine the proportion of variance in the outcome that is accounted for by between-county variance, or the ICC (Raudenbush & Bryk, 2002). Figure 19 below displays ICC values for each outcome for cases filed before and after the reform. While in both periods, the majority of variance in pretrial decisions is accounted for at the individual-level, patterns of change in between-county variance do emerge. Between-county variance in the odds of nonfinancial release decreased, dropping from 8.4 percent to 4.8 percent, suggesting that county-level factors exert less influence in nonfinancial release decisions in the post-reform period, in line with the aims of the reform. Conversely, between-county variation is larger for bail amounts following reform, suggesting a persistent influence of jurisdiction (or, where a defendant is charged) in shaping money bail setting behavior. There, the proportion of variance in bail amounts that occurs between counties increases in the post-reform period, from 8.2 percent to 12.9 percent. Finally, inter-county variation in the odds pretrial detention outcomes remained stable between the two policy periods.

Figure 19. Between-County Variation in Pretrial Outcomes Pre/Post Bail Reform: ICCs



## Predicting Pretrial Decisions: Bail Reform, Case Characteristics, and Contexts

Table 11 below shows results from full multilevel models of pretrial decision-making. The first dependent variable, logged bail amount, was modeled using multilevel linear models, while the two binary outcomes, nonfinancial release and detention without bail, were modeled using multilevel logistic models. Coefficients in all models have been exponentiated, making the interpretation in the multilevel log-linear models a percent change in the bail amount for a one-unit change in the predictor variable, and for multilevel logistic models, a factor change in the odds of the outcome for a unit change in the predictor. In all models, level 1 predictors have been group-mean centered, while level 2 predictors are grand-mean centered. Finally, in the models presented in Table 11, I allow the slope on the policy period indicator, *Post Bail Reform*, to vary across counties.

Consistent with localized estimates of the effects of bail reform on pretrial decision-making discussed in Chapter 2, this analysis affirms that Maryland's bail reform is associated with significant change in judicial decision-making at the pretrial stage, although in directions that suggest that defendants did not face uniformly better outcomes following reform. On average, bail reform is associated with a 14.3 percent decrease in bail amounts ( $b = 0.857$ ,  $p < .05$ ), net of both individual and contextual factors. This finding suggests, for cases where money bail is set, defendants are less burdened by high bail following the reform's implementation. Conversely, in the model for detention without bail, the post-reform period is associated with an increase in the odds of being detained by a factor of nearly 2 (OR: 1.99,  $p < .001$ ), after accounting for individual and county-level factors. Finally, with respect to the odds of nonfinancial

release, although the coefficient on the reform indicator was positive, it was not statistically significant (OR: 1.227,  $p = 0.114$ ).<sup>63</sup>

The second guiding question of this chapter relates to whether bail reform's impacts varied across counties. There, I find significant between-county differences in main effect of bail reform. To test this hypothesis for each outcome, I first fit models without a random slope on post-reform indicator, then fit separate models with random slopes on the post-reform indicator (which allows the post-reform indicator to have a different slope for each county). Using a likelihood ratio test, I find significant differences between counties in the relationship between bail reform and log bail amounts ( $u_{1j} = .085$ ,  $p < .001$ ), nonfinancial release ( $u_{1j} = 0.192$ ,  $p < .001$ ) and odd of pretrial detention ( $u_{1j} = 0.113$ ,  $p < .001$ ).<sup>64</sup>

Panel B of Table 2 presents county-level predictors of the random intercept for each of the three pretrial outcomes. I structure the discussion of county-level predictors around the expectations outlined earlier in the chapter. First, both focal concerns and courts-as-communities perspectives anticipate that considerations outside of defendant or case characteristics may factor into judicial decision-making. Specifically, in setting conditions of release, judicial officers may be concerned with whether the local jail has capacity to detain a defendant, or whether conditions suffer due to overcrowding

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<sup>63</sup> In Chapter 2, I estimate that the policy reform was associated with an immediate increase in nonfinancial release rates; this effect estimate was local to the period just after the Attorney General's letter, and I observe some suggestive evidence that nonfinancial release rates were trending downward back to pre-reform levels over time. This may explain the lack of statistical significance in the main effect in the current modeling, which uses a larger time-frame over which to examine the associated between the new bail rules of the odds of nonfinancial release.

<sup>64</sup> As a supplementary analysis, I run county-level regression discontinuity models, estimating a separate effect of the policy announcement for each county ( $n=24$ ) and pretrial outcome, using the strategy outlined in Chapter 2. I use MSE-optimal bandwidths and control for individual-level covariates and day-of-week fixed effects. County-level RD effect estimates demonstrate the variability immediate impacts of the bail reform on pretrial decision-making across county. See Appendix Figures 8 and 9.

(D'Alessio & Stolzenberg 1995). I find that county-level jail crowding is significantly associated with pretrial decisions. Specifically, I find that a 10-percentage point<sup>65</sup> increase in county-level jail crowding is associated with a 2 percent increase in the odds of nonfinancial release (Odds ratio [OR]: 1.021,  $p < .001$ ). Odds of detention without bail are lower for defendants in counties with higher jail crowding (OR: 0.966,  $p < .001$ ). These findings may suggest that courts may be more inclined to favor pretrial decisions that allow for the possibility of release (and less inclined to detain without the possibility of release) where jail bed space is at a premium. I also find that counties with higher jail crowding are associated with slightly higher bail amounts (b: 1.021,  $p < .001$ ). Although this analysis is not able to disentangle the direction of the relationship, this finding may also suggest that, when monetary bail is set, high bail amounts continue to pose a barrier to release from custody for defendants.<sup>66</sup>

Court communities theory and organizational efficiency models of case processing contend that organizational characteristics of courts, namely caseload pressure and court size, influence court decision-making. Consistent with theoretical predictions, counties with higher caseloads are associated with more lenient pretrial decisions, including lower bail amounts and higher odds of nonfinancial release: an increase of 100 annual cases per judge<sup>67</sup> is associated with a 1.1 percent decrease in

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<sup>65</sup> Owing to the scaling of the original variable, a one-unit increase in jail crowding yields very small coefficients. To make this more meaningful, I rescale the unexponentiated coefficients by a factor of 10—corresponding to a 10 percentage point increase in jail crowding—then report the exponentiated results here. Odds ratios (OR) and  $p$  values reported in parentheses.

<sup>66</sup> I am unable to assess affordability of bail because of court case records do not include information on defendant employment, assets or reliable information on indigency. I am further unable to observe the ability to obtain release from custody after posting bail. As such, I cannot fully assess the direction of this relationship.

<sup>67</sup> Owing to the scaling of the original variable, a one-unit increase in annual caseload per judge yields very small coefficients. To make this more meaningful, I rescale the unexponentiated coefficients by a

average bail amounts ( $b: .989, p <.001$ ) and a 2.6 percent increase in the odds of nonfinancial release ( $OR: 1.026, p <.001$ ). However, counties with heavier caseloads are associated with a 3.3 percent increase in the odds of being detained at the initial bail hearing ( $OR: 1.033, p <.001$ ). Given the procedural feature in Maryland's pretrial system that those detained at the initial hearing receive a bail review hearing within 24 hours (see Chapter 1), increased odds of detention for jurisdictions with higher-than-average caseloads may reflect a desire for expedient decision-making at the initial pretrial hearing. In a sentencing context, organizational efficiency models predict that courts with high caseloads may pressure parties to produce pleas (rather than take cases to resource-intensive trials) in service of efficient management of caseloads. Here, courts with heavier caseloads, in lieu of carefully weighing individual factors and setting least restrictive release conditions to ensure appearance and maintain public safety, may choose to simply detain defendants. Finally, results indicate that, compared with smaller courts, counties with larger courts are significantly more likely to release defendants without monetary conditions ( $p < .05$ ), while medium sized courts are more likely to detain without bail relative to smaller courts ( $p < .05$ ). Court size was not predictive of bail amounts.

Sociopolitical context may also influence decisions at the pretrial stage. Two factors in particular, violent crime and political conservatism, are expected to result in more restrictive pretrial decisions across counties. In line with this expectation, counties with higher-than-average rates of violent crime are associated with lower odds of nonfinancial release ( $OR$  for a 10% increase in violent crime rate:  $0.983, p <.001$ ) and

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factor of 100—corresponding to an increase of 100 cases per year—then report the exponentiated results here. Odds ratios ( $OR$ ) and  $p$  values reported in parentheses.

higher average bail amounts ( $b: 1.027, p < .001$ ), net of case characteristics. Odds of detention were also significantly lower for defendants charged with crimes in counties with higher countywide violent crimes rates (OR: 0.967,  $p < .001$ ).<sup>68</sup> These results suggest that court communities may be responsive to external pressures related to crime, setting more harsh bail conditions in counties with higher violent crime rates. That the setting of any monetary condition of bail precludes preventative detention may explain the peculiar finding with respect to detention without bail in high-crime counties. While violent crime rates are associated with small, but significant variation in pretrial decision-making, a measure of political conservatism, percent Republican, intended to capture the influence of punitive criminal justice orientations, was not significantly associated with pretrial decision-making in any model.

County-level indicators of economic conditions were not significantly associated with bail amount decisions or odds of non-financial release. However, I find evidence that economic conditions are linked to pretrial detention: counties with higher poverty are associated with an increase in the odds of detention ( $p < .001$ ), while counties with higher unemployment are associated with a decline in the odds of detention ( $p < .001$ ). Consistent with other pretrial studies, county-level racial and ethnic composition were largely not associated with pretrial outcomes, except for percent Latino. On this factor, results show that counties with larger Latino populations were associated with a 3.3 percent decrease in bail amounts (OR: 0.966,  $p < .05$ ), net of other factors in the model.

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<sup>68</sup> Owing to the scaling of the original variable, a one-unit increase in violent crime rates per 100,000 yields very small coefficients. To make this more meaningful, I rescale the unexponentiated coefficients by a factor of 65—corresponding to an approximately 10 percent increase in average violent crime rates across the sample—then report the exponentiated results here. Odds ratios (OR) and  $p$  values reported in parentheses.

Table 11. Full Multilevel Models of Pretrial Outcomes

	Log Bail Amount Model 1		Non-financial Release Model 2		Detention without Bail Model 3	
	Exp. Coef.	(SE)	Odds Ratio	(SE)	Odds Ratio	(SE)
Post-Bail Reform	0.857*	(0.0546)	1.227	(0.159)	1.990***	(0.153)
<b>Panel A: Level 1 Predictors</b>						
Age at Filing (Years)	0.996***	(0.000302)	1.001***	(0.00037)	1.000	(0.0005)
Female	0.717***	(0.00622)	1.685***	(0.0173)	0.447***	(0.0078)
Race (ref = White)						
Black	1.131***	(0.00873)	0.790***	(0.00768)	1.321***	(0.0192)
Asian/Pac Islander	1.210***	(0.0540)	0.963	(0.0519)	1.198*	(0.0924)
Native American	0.947	(0.102)	0.923	(0.119)	1.006	(0.202)
Other Race	1.050	(0.0287)	0.687***	(0.0252)	1.306***	(0.0671)
Offense Type (ref = Property)						
Person	1.546***	(0.0134)	0.760***	(0.00830)	3.224***	(0.0568)
Drug	0.872***	(0.00889)	3.059***	(0.0393)	0.463***	(0.0101)
Other Type	1.459***	(0.0568)	0.886**	(0.0379)	3.739***	(0.301)
Number of Charges^	1.139***	(0.00136)	0.445***	(0.00399)	2.361***	(0.0363)
Severity of Top Charge	1.275***	(0.00313)	0.678***	(0.00209)	1.604***	(0.0072)
Count Prior Cases^	0.996***	(0.000496)	0.561***	(0.00525)	1.763***	(0.0255)
<b>Panel B: Level 2 Predictors of Random Intercept</b>						
Percent Black	0.998	(0.00635)	1.012	(0.0124)	1.014	(0.0139)
Percent Latino	0.966*	(0.0150)	0.966	(0.0295)	1.025	(0.0362)
Percent Below Poverty Line	1.008	(0.0222)	0.978	(0.0412)	1.219***	(0.0518)
Percent Unemployed	1.044	(0.0637)	0.902	(0.0846)	0.568***	(0.0734)
Court Size - Medium	0.992	(0.139)	1.004	(0.266)	2.069*	(0.691)
Court Size - Large	0.661	(0.167)	2.798*	(1.380)	2.018	(1.111)
Annual Caseload per Judge	0.999***	(0.000031)	1.000***	(0.00004)	1.000***	(0.0000)
Annual Jail Crowding Percent	1.002**	(0.00065)	1.002**	(0.0007)	0.997***	(0.001)
Annual Violent Crime Rate	1.000***	(0.000034)	0.999***	(0.00005)	0.999***	(0.000)
Percent Republican	0.991	(0.00696)	1.014	(0.0138)	1.008	(0.0157)
Intercept	9.269	0.172	0.477**	0.103	0.013	0.003
Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
<b>Level 2 Random Effects</b>						
Variance - Intercept	0.074	0.025	.1867	.058	0.155	0.058
Variance - Slope (Post-Reform)	0.085	0.026	0.192	.057	0.113	0.036
Cov (Slope, Intercept)	-0.055	0.022	-.089	.048	0.056	0.055
Level 1 Variance (Residual)	1.125	0.005				
Log Likelihood	-158382.9		-171435.8		-88201.1	
AIC	316831.7		342935.7		176466.1	
BIC	317147.9		343274.6		176805.1	
N	107121		294330		294330	

^In multilevel logistic models, number of charges is an indicator variable where 1 = more than 1 charge, 0 = single charge; and count prior cases = 1 if more any priors, 0 = no prior cases.

\* p < .05, \*\* p < .01, \*\*\* p < .001

Results for individual case predictors are presented in Panel A of Table 11. Consistent with expectation #1, I find that legally-relevant factors, including being charged with a person crime, facing multiple charges, and increased severity of the top charge, are strongly predictive of more restrictive pretrial decisions: each is associated with increased in bail amounts, lower odds of nonfinancial release, and higher odds of being detained pretrial within a given county. Likewise, prior history is also associated with lower odds of nonfinancial release and higher odds of detention without opportunity for release. These findings are consistent with a long line of prior research, which demonstrate that legal factors strongly shape pretrial outcomes (Frazier et al., 1980; Goldkamp, 1980; Goldkamp & Gottfredson, 1979; Walker, 2006; Wooldredge et. al, 2017).

Extra-legal factors also demonstrate associations with pretrial outcomes, after accounting for legal and contextual characteristics. Compared to their male counterparts, within a given county, female defendants are less likely be detained pretrial, more likely to be released without financial condition, and where bail is set, associated with lower bail amounts, consistent with prior work finding that women are treated more leniently throughout the pretrial process (Pinchevsky & Steiner, 2016). Models demonstrate some variation in pretrial decisions across racial groups: compared with White defendants, Black defendants are associated with a 13 percent increase in average bail amounts, 21 percent lower odds of being released without financial condition, and 32 percent higher odds of being detained ( $p < .001$ ).<sup>69</sup> Though not the

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<sup>69</sup> The “Other race” category is comprised of race values listed as “Other”. While the results suggest that such defendants face lower odds of nonfinancial release and higher odds of being held without bail, relative to white defendants, it is difficult to theorize what accounts for these associations.

focus of the study, these results largely follow prior research and are in the expected direction.

As the above analysis demonstrates, pretrial outcomes are not determined by case characteristics alone. I find that bail determinations across Maryland counties are shaped by the social and organizational context in which decision-makers are situated, even after accounting for individual-level case factors, such as offense type, severity, and prior history. I now turn to the final question posed in this chapter: whether, and how, organizational and contextual factors of courts shape pretrial decision-making after bail reform.

### **Do Organizational Features and Court Contexts Moderate the Association between Bail Reform and Pretrial Decisions?**

The analysis thus far demonstrates that while individual factors are most influential in pretrial decisions, court contexts are significantly associated with pretrial decisions across counties. These results contribute to a small, but growing literature demonstrating the influence of extra-legal and contextual factors on upstream criminal court decisions, and reinforce the importance of examining local variation in criminal justice outcomes. The final question of this analysis asks whether contextual influences moderate the influence of bail reform on pretrial decisions across counties. Just as existing law is filtered and translated into action at the local level (Lynch, 2011), so too, may the application of *new law* be influenced by the organizational, social, economic, and political contexts in which actors are embedded. To explore whether contextual factors moderate the impact of bail reform across counties, I include county-level factors as predictors of the random slope on the bail reform period. Cross-level interactions with

the post-reform indicator and county-level variables effectively ask the question: does relationship between the contextual factor and pretrial outcomes differ in the post-reform period, compared with the pre-reform period?<sup>70</sup> Table 12 presents the results of these cross-level interaction models. I separately examine FE models, fitting interactions between the post-reform indicator and both time-varying and time-invariant county-level measures; FE approaches control for all time-invariant group-level factors that may affect the outcomes of interest. Across both the multilevel approach and the fixed effects approach, I find support for the final hypothesis that organizational and contextual factors do moderate the effect of bail reform on pretrial decisions. I discuss each set of findings in turn, starting with multilevel models including cross-level interaction terms.

### *Bail Amount*

The full model predicting log bail amounts (Model 4) suggests that contextual characteristics, and in particular organizational features of courts, racial composition of counties, and the composition of the county electorate, all moderate the association of bail reform on monetary bail setting behavior. First, in the pre-reform period, counties with larger courts were associated with lower bail amounts, on average, compared to counties with small courts ( $b = .442, p < .05$ ). Following reform, counties with larger courts were associated with higher bail amounts, relative to smaller courts, following bail reform ( $b = 3.285, p < .001$ ).<sup>71</sup> This finding is counter to what court communities research would predict regarding larger counties. Further analysis reveals that this

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<sup>70</sup> In the multilevel logistic models, the interaction effects yield a ratio of odds ratios.

<sup>71</sup> To retrieve the effect for larger courts, compared to small courts, in the post period, I multiply the exponentiated coefficients for the main effect of large courts with its interaction term ( $0.442 \times 3.285 = 1.452$ ).

finding is driven almost entirely by Baltimore City, which had the highest average bail amounts both before and after bail reform (see Figure 1). This suggests that past bail setting practices in some court communities, which had previously established norms of setting high bail amounts, persist even as courts conform to the new bail rules.

The criminal justice and social contexts of courts do appear to moderate the impact of reform on bail amounts, but the practical significance of the estimated associations is in question. First, jail crowding moderates the impact of bail reform on bail amounts, although the magnitude of the coefficient is not large. The interaction term for jail crowding in the post-period is statistically significant ( $b = 0.994, p < 0.001$ ), although estimated association for jail crowding in the post-period is close to zero.<sup>72</sup> I find a similarly small association for violent crime rates ( $b = 0.999, p < 0.001$ ). Interestingly, county political orientation moderates the impact of bail reform on bail setting behavior: following bail reform, counties with higher percentages of Republican voters were associated with lower bail amounts. County political orientation did not have a significant association with bail amounts prior to reform, suggesting a greater correction to bail amounts in more conservative counties compared with less conservative counties following bail reform ( $b = 0.978, p < 0.01$ ).

Finally, racial and ethnic composition of the county also conditioned the impact of bail reform on bail amounts across counties: defendants charged with crimes in counties with larger Black ( $b = 0.979, p < 0.01$ ). and Latino populations ( $b = 0.935, p < 0.001$ ) were associated with lower bail amounts following reform. This finding is contrary

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<sup>72</sup> To retrieve the association for jail crowding on bail amounts in the post period, I multiply the exponentiated coefficients for the main effect of jail crowding with its interaction ( $1.004 * 0.994 = .998$ ), then subtract 1 and multiply by 100 to get a percent change  $(.998 - 1) * 100 = 0.2$  percent decrease.

to what might be expected based on the group threat hypothesis. Prior to reform, neither measure of racial or ethnic composition was associated with variation in bail amounts across counties. Like the finding on political orientation, the negative relationship may indicate a larger correction to high bail amounts, relative to counties with less diverse populations, following reform. Economic characteristics were unrelated to average bail amounts across courts following reform.

### *Nonfinancial Release*

Model 5 presents results for release without monetary conditions. Organizational features of courts moderate the association of bail reform on nonfinancial release: large courts appear to behave quite differently from small courts in the post-reform period. Prior to bail reform, large courts were associated with 4.2 factor increase in the odds of non-financial release, compared with small courts; following reform, this difference in the odds declined significantly ( $p < .001$ ), such that the odds of nonfinancial release in large courts is only seven percent greater in larger courts compared with small courts after bail reform.<sup>73</sup>

Economic contexts of courts condition the association of bail reform on nonfinancial release, as well. In the post-reform period, increases in county-level poverty are associated with a decrease in the odds of nonfinancial release compared with counties with lower poverty ( $b = 0.874$ ,  $p < .05$ ); poverty is not significantly associated with the odds of nonfinancial release prior to reform. Increases in county-level unemployment are associated with greater odds of nonfinancial release following

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<sup>73</sup> To calculate the odd ratio for large courts, relative to small courts, in the post-reform period, I multiply  $4.207 \times .255 = 1.073$ .

reform ( $b = 1.486, p < .05$ ); in the pre-reform period, however, increases in county-level unemployment were associated with lower odds of nonfinancial release. While difficult to interpret, these findings may suggest that social conditions related to poverty and to unemployment may be associated with different “perceptual shorthands” or attributions by local court communities (Steffensmeier et al., 1998); court communities in counties with high unemployment were more inclined to favor release under the least restrictive conditions following reform. As I discuss next, for counties with higher poverty, lower odds of nonfinancial release in the post-reform period were coupled with increased odds of being detained without bail; conversely, counties with higher unemployment were associated with lower odds of preventative detention. Lastly, like the results for bail amounts, the coefficient on jail crowding and violent crime rate in the post-reform period is statistically significant, but substantively small in size.

Table 12. Cross-Level Interactions

	Log Bail Amount Model 4		Nonfinancial Release Model 5		Detained without Bail Model 6	
	Exp. Coef.	(SE)	Odds Ratio	(SE)	Odds Ratio	(SE)
<b>Post-Bail Reform</b>	0.589***	(0.0709)	1.991*	(0.447)	3.124***	(0.400)
<b>Panel A: Level 1 Controls</b>						
<b>Age at Filing (Years)</b>	0.996***	(0.000302)	1.001***	(0.00037)	1.000	(0.0005)
<b>Female</b>	0.716***	(0.00622)	1.685***	(0.0173)	0.447***	(0.0078)
<b>Race (ref = White)</b>						
<b>Black</b>	1.132***	(0.00873)	0.790***	(0.00768)	1.321***	(0.0192)
<b>Asian/Pacific Islander</b>	1.211***	(0.0540)	0.962	(0.0519)	1.198*	(0.0924)
<b>Native American</b>	0.946	(0.102)	0.922	(0.119)	1.004	(0.202)
<b>Other Race</b>	1.049	(0.0287)	0.687***	(0.0252)	1.305***	(0.0671)
<b>Offense Type (ref = Property)</b>						
<b>Person</b>	1.547***	(0.0134)	0.760***	(0.00830)	3.227***	(0.0569)
<b>Drug</b>	0.872***	(0.00888)	3.059***	(0.0393)	0.463***	(0.0101)
<b>Other Type</b>	1.457***	(0.0568)	0.886**	(0.0379)	3.737***	(0.300)
<b>Number of Charges<sup>^</sup></b>	1.139***	(0.00136)	0.445***	(0.00399)	2.362***	(0.0363)
<b>Severity of Top Charge</b>	1.275***	(0.00313)	0.678***	(0.00209)	1.604***	(0.0072)
<b>Count Prior Cases<sup>^</sup></b>	0.996***	(0.000496)	0.561***	(0.00525)	1.763***	(0.0255)
<b>Panel B: Level 2 Predictors of Random Intercept</b>						
<b>Percent Black</b>	1.005	(0.00818)	1.018	(0.0130)	1.009	(0.0136)
<b>Percent Latino</b>	0.971	(0.0192)	0.985	(0.0305)	1.006	(0.0328)
<b>Percent Below Poverty Line</b>	0.996	(0.0277)	1.021	(0.0442)	1.251***	(0.0587)
<b>Percent Unemployed</b>	1.119	(0.0865)	0.769*	(0.0923)	0.549***	(0.0718)
<b>Court Size - Medium (ref = Small)</b>	1.114	(0.200)	1.127	(0.316)	1.946*	(0.578)
<b>Court Size - Large (ref = Small)</b>	0.442*	(0.144)	4.207**	(2.139)	1.834	(0.987)
<b>Caseload per Judge</b>	1.000**	(0.00003)	1.000***	(0.00005)	1.000***	(0.00008)
<b>Jail Crowding</b>	1.004***	(0.000935)	0.999	(0.00132)	0.994**	(0.00202)
<b>Violent Crime Rate</b>	1.000***	(0.000035)	1.000***	(0.00005)	0.999***	(0.00009)
<b>Percent Republican</b>	0.993	(0.00889)	1.025	(0.0144)	1.003	(0.0149)
<b>Panel C: Level 2 Predictors of Post-Bail Reform Effect</b>						
<b>Percent Black</b>	0.979**	(0.00641)	0.994	(0.0124)	0.992	(0.0063)
<b>Percent Latino</b>	0.935***	(0.0170)	0.999	(0.0333)	0.984	(0.0194)
<b>Percent Below Poverty Line</b>	1.046	(0.0267)	0.874**	(0.0392)	0.932*	(0.0273)
<b>Percent Unemployed</b>	0.912	(0.0613)	1.486**	(0.180)	1.335***	(0.0964)
<b>Court Size - Medium</b>	0.850	(0.125)	0.694	(0.192)	0.500***	(0.0723)
<b>Court Size - Large</b>	3.285***	(0.920)	0.255**	(0.134)	0.291***	(0.0892)
<b>Caseload Per Judge</b>	1.000	(0.00005)	1.000***	(0.00009)	1.000***	(0.00009)
<b>Jail Crowding</b>	0.994***	(0.00129)	1.007***	(0.00174)	1.004	(0.00225)
<b>Violent Crime Rate per 100k</b>	0.999***	(0.00016)	1.001*	(0.00025)	1.000*	(0.00024)
<b>Percent Republican</b>	0.978**	(0.00758)	0.990	(0.0140)	0.992	(0.00807)
<b>Constant</b>	9.369	0.188	0.400	0.093	0.011	0.002
<b>Year Fixed Effects</b>	Yes	Yes	Yes	Yes	Yes	Yes

<b>Level 2 Random Effects</b>						
Var - Random Intercept	0.0648	0.0193	.1591	.0475	.1702	.0563
Var - Random Slope (Post-Reform)	0.0356	0.0119	.1439	.0464	.0213	.0125
Cov (Slope, Random Intercept)	-0.0364	0.0133	-.0564	.0344	-.0068	.0219
Level 1 Variance (Residual)	1.1244	0.0049				
Log Likelihood	-158356		-171418.5		-88180.3	
AIC	316797.7		342920.9		176433.2	
BIC	317209.7		343365.8		176878.1	
N	107121		294330		294330	

<sup>^</sup>In multilevel logistic models, number of charges is an indicator variable where 1 = more than 1 charge, 0 = single charge; and count prior cases = 1 if more any priors, 0 = no prior cases.

\*  $p < .05$ , \*\*  $p < .01$ , \*\*\*  $p < .001$

### *Detention without Bail*

Turning to results for detention without bail, court organizational and county-level contextual factors condition the association of bail reform on the odds of preventative detention. Similar to nonfinancial release, economic conditions across counties moderate the impact of bail reform on the odds of detention: prior to bail reform, defendants charged in counties with higher poverty saw were more likely to be detained without bail ( $b = 1.251, p < .001$ ). Following reform, this association lessened ( $b = 0.874, p < .01$ ); however, increases in county-level poverty are still associated with greater odds of detention in the post-reform period. Before bail reform, unemployment was associated with lower odds of being detained without bail; following reform, this negative association was weakened ( $b = 1.335, p < .001$ ); however, following bail reform, a unit increase in percent unemployed was associated with a 27 percent decrease in the odds of being held in jail custody without bail. This, in conjunction with the findings on nonfinancial release, suggests that counties with higher unemployment responded to constraints in the use of money bail in a different manner than counties with high poverty; where the former were more likely to offer nonfinancial conditions in

lieu of bail, while the latter turned toward greater use of preventative detention. This pattern of results merits future investigation, something that is beyond the scope of this study.

Lastly, organizational features of courts also condition the impact of bail reform across counties on the odds of detention without bail. Prior to reform, counties with medium-size courts were significantly more likely to detain without bail, compared to counties with smaller courts ( $b = 1.946, p < .05$ ), though no difference is observed between large and small courts. Following reform, medium and large sized courts were somewhat less likely to detain defendants without bail, compared with smaller courts, suggesting that smaller courts shifted more toward preventative detention relative to other courts. Finally, although significant in the model, coefficients for caseload, jail crowding, and violent crime rate measures were substantively close to no effect.

### **Supplementary Fixed Effects Approach: Results**

In Appendix Table 2, I present three FE models predicting log bail amount (Model 7), release on nonfinancial conditions (Model 8), and detention without bail (Model 9). Broadly, the FE models affirm the multilevel results, supporting the contention that organizational and social contexts of courts moderate county responses to bail reform. Generally, coefficient estimates for the individual-level covariates for the FE models and the multilevel model results presented above are substantially similar—an expected result, given the choice to group-mean center the level 1 variables in the main multilevel models (McNeish & Kelly, 2019). Below, I briefly review results of the FE models, with attention to the county-level moderators of the impact of bail reform on pretrial decision and to where results depart from estimates above.

### *Bail Amount*

Results from the FE model (Appendix Table 2, Model 7) predicting bail amount affirm the general findings from the multilevel model (Model 4), both in direction of estimated associations and in statistical significance, although some differences arise. First, estimated coefficients on the interaction terms between the bail reform indicator and the contextual characteristics are generally larger in magnitude in the FE models, compared with the multilevel models. Like the multilevel models, court size is a significant moderator of the effect of bail reform across counties, with larger courts associated with higher bail amounts in the post-reform period. Additionally, medium size courts are also significantly associated with lower bail amounts, compared to small courts, following reform ( $b = .685, p < .001$ ). The interaction effect for jail crowding in the post reform period remains negative, but is no longer significant in the FE model, with a smaller coefficient estimate and larger standard error compared to the multilevel results (model 5). Finally, in the FE model, counties with higher poverty are associated with higher bail amounts after reform ( $b = 1.066, p < .05$ ); this was not significant in the multilevel results.

### *Nonfinancial Release*

More variation can be seen in the FE model results predicting the odds of nonfinancial release. Overall, FE models find less evidence that county contexts moderate the impacts of bail reform on the odds of nonfinancial release, with several county-level measures no longer statistically significant after accounting for time-invariant confounders across counties. Caseload, jail crowding, and violent crime are no longer significant in the FE models; however, while these measures were significant in

the multilevel approach above, the coefficients were substantively close to zero. More substantially, court size is no longer a significant moderator of bail reform impacts on the odds of nonfinancial release, nor is the county-level poverty rate. County unemployment remains a significant positive moderator of the impact of bail reform (OR = 1.554,  $p < .001$ ). Coefficients on interactions terms between bail reform and racial/ethnic composition at the county are statistically significant in the FE model: counties with higher Black and Latino populations are associated with lower odds of nonfinancial release in the post-reform period. Finally, sociopolitical context appears to matter for nonfinancial release: counties with larger Republican voter populations are associated with significantly lower odds of nonfinancial release following bail reform (OR = .956,  $p < .01$ ). This association was negative, but not statistically significant in the multilevel model.

#### *Detention with Bail*

Lastly, I turn to the FE models examining the county moderators of the impact of bail reform on the odds of detention without bail. Like nonfinancial release, county poverty is no longer a significant moderator of bail reform's impact on odds of detention without bail, although county unemployment remains a significant positive moderator (OR = 1.262,  $p < .001$ ). Racial composition of the county moderates the impact of bail reform on odds of detention: counties with larger Black populations are associated with slightly lower odds of detention without bail in the post-reform period (OR = 0.998,  $p < .01$ ). Finally, the coefficient on violent crime is no longer significant, while the interaction term on percent Republican is significant and negative in the fixed effect approach (OR = .986,  $p < .05$ ).

## ***Summary and Discussion***

This chapter examined the role of contexts—organizational contexts and community demographic, economic, political contexts—on the pretrial process and on court responses to Maryland’s bail reform. While analyses presented in this chapter affirm that individual case factors account for most variation in pretrial decisions, my findings support the argument that local contextual features—including court organizational characteristics, criminal justice contexts, and economic contexts—do affect pretrial decision-making. Specifically, I find significant variability across counties across pretrial decision-making, including in money bail setting practices, nonfinancial release, and pretrial detention. Counties diverged in varying ways following reform: while bail reform was not significantly associated with change in the odds of nonfinancial release, between-county variation in nonfinancial release declined following reform. Conversely, between-county variability in bail amounts increased in the post-reform period, despite a significant decline in bail amounts on average. Detention with bail demonstrated a different pattern of results: the odds of detention increased in the post-reform period and showed little change in variability between counties.

My findings align with expectations derived from prior research and focal concerns theory, which argues that considerations such as a defendant’s dangerousness, blameworthiness, along with practical considerations, shape judicial decision-making. I find that legal case factors, including charge severity, prior history, and crime type, are all strongly associated with more severe pretrial decisions, net of other factors. Results also suggest that extra-legal factors, including defendant race and gender are also predictive of outcomes: compared with white defendants, Black defendants, on average, received higher bail amount where bail was assigned, had higher likelihood of pretrial

detention, and lower likelihood of nonfinancial release (Demuth, 2003; Schlesinger, 2005). Consistent with prior research, female defendants also received more lenient pretrial outcomes, compared with their male counterparts (Demuth & Steffensmeier, 2004; Katz & Spohn, 1995; Pinchevsky & Steiner, 2016).

Results from this chapter imply that, even after adjusting for individual-level case factors, the contexts in which pretrial determinations are made play an important role in pretrial decisions, and moderate the impacts of reform. First, I find that pretrial decisions across counties are influenced by local criminal justice context—including organizational features of the courts and local jail crowding—supporting focal concerns and court communities theory propositions that courts, and actors operating in those courts, are responsive to their broader environments. In line with expectations, I find that jail crowding is associated with higher nonfinancial release and lower detention without bail across counties, suggesting that courts may be aware of jail usage when making pretrial determinations. Contrary to theoretical expectations, I find that jurisdictions with higher levels of jail crowding are associated with higher bail amounts. This finding supports contentions that unaffordable bail may lead to overcrowding in local jails, as individuals held on bail are unable to secure release (Scott-Hayward & Fradella, 2019).

Caseload pressure and court size were also associated with variation between counties in pretrial outcomes. Consistent with court communities and organizational efficiency perspectives, I find that heavier caseloads per judge were associated with greater leniency with some aspects of the pretrial process, including lower bail amounts and higher rates of nonfinancial release. However, heavier caseloads were also associated with greater odds of detention, contrary to overall expectations. Court size

appeared to matter less: although larger courts were more lenient with respect to nonfinancial release, compared with smaller courts (in line with theoretical expectations), court size did not explain variation between counties in bail amount in the main effects models. However, both court size and, to a lesser degree, caseload pressure did moderate the effects of bail reform across counties, providing evidence for claims that different organizational contexts pattern responses to new law (Grattet & Jenness, 2005; Lynch & Omori, 2014; Verma, 2015).

Community pressure from fear of or anxiety over levels of crime may also play a role. I find that jurisdictions with higher rates of violent crime are associated with lower likelihoods of nonfinancial release and higher bail amounts, but also lower odds of pretrial detention. And, contrary to expectations, I find little support for county-level punitive political orientation, percent Republican, in explaining variation in pretrial decisions across counties in the main effect models. Overall, this lack of association may indicate that political context of the county has little to do with pretrial decisions. Commissioners in Maryland's district courts are appointed, not elected as in other states, and this institutional feature may blunt direct effects of political context on judicial decision-making. However, results from the cross-level interaction models suggest that county political orientation conditions county-level responses to progressive reform. There, I find that counties with larger Republican electorates are associated with greater decreases in bail amounts following bail reform—an express goal of the reform.

In the full sample, this study did not find support for predictions aligned with minority threat theories, as demographic composition of counties did little to explain variation in pretrial outcomes, a finding that is consistent with some prior literature

(Pinchevsky & Steiner, 2016; St. Louis, 2020; Williams & Rosenfeld, 2016, but see Hood & Schneider, 2019; Williams 2018). Economic conditions variables were only significantly associated with variation in the pretrial outcome not expressly linked to financial considerations: detention without bail. There, higher level of poverty was associated with greater odds of detention, while unemployment was linked to lower odds of detention.<sup>74</sup> Coupled with null effects of county economic conditions on bail amounts and nonfinancial release, these results may suggest economic contexts may play a complex role, particularly as it relates to pretrial process. Indeed, the results of cross-level interaction models suggest that county economic factors—particularly unemployment—do play a role in shaping county-level responses to bail reform.

Finally, with respect to my final research question, I find that the impacts of Maryland’s bail reform were at least partially contingent upon county-level contextual factors and organizational characteristics. My findings demonstrate that court size significantly influenced responses to bail reform in Maryland: while larger courts were associated with lower bail amounts compared with small courts prior to bail reform, this relation was inverted following enactment of new bail rules. This pattern appears to be driven by historical practices of setting high bail amounts in one large court jurisdiction. Additionally, I find suggestive evidence that court size moderates response to bail reform in nonfinancial release decisions in the multilevel models: difference in odds of nonfinancial release between small and large courts declined significantly in the post-reform period. However, I caution over-interpretation of this result, as this interaction

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<sup>74</sup> To assess concerns over possible collinearity of poverty and unemployment measures, I ran additional models on all three outcomes wherein I excluded one variable and examined the coefficients on the other. Results from these models were substantially similar to models with both variables included, alleviated concerns over multicollinearity.

was not significant in the FE models. Finally, I find that economic contexts patterned county responses to bail reform in important ways: while all counties moved away from high bail setting behavior, counties with higher poverty appear to shift more toward the use of detention without bail rather than nonfinancial release, while counties with higher unemployment responded in the inverse manner. This raises important theoretical considerations about the divergent influences that poverty and unemployment may signal to court communities, a subject that requires greater examination. Broadly, these results demonstrate that county-level economic and organizational differences play a role in how policy changes are received and implemented by local courts. This research adds to a growing body of research that explores contextual influences on upstream court outcomes, and to research that explores variation in law's effect across locale.

While this analysis is revealing, there are limitations to these conclusions. First, these findings are intended to characterize variation in pretrial decisions across counties and over policy periods, and as such, are descriptive in nature. They are not intended to identify causal effects of any of the measures or constructs on pretrial decisions. Future work that explores specific explanatory factors would be useful to advance knowledge in this area. Second, these findings reflect cases originating from on-view arrests, and may not necessarily generalize to other types of cases, particularly those with more complex case characteristics such as those originating from arrest warrants or violations of community supervision. Third, a key limitation of this study is the inability to capture information related to financial status of defendants themselves, and as such, further research should explore individual and community level economic conditions and their interactions. Finally, although this chapter examined the role of organizational features

and community contexts on pretrial outcomes, future research should attend to variation across individual court locations and individual courtroom actors, such as judicial officers and prosecutors, which were beyond the scope of this study.

## **CONCLUSION. LESSONS FROM THE STUDY OF MARYLAND'S BAIL REFORM**

Curtailing the outsized role of money bail in pretrial justice has been a goal of reformers since the earliest days of the 20<sup>th</sup> century. Despite long-standing concern, research on the effectiveness and impacts of such efforts has received less focus and remains of pressing interest to advocates, policymakers, and the public alike. This dissertation examined a landmark change to how bail works in Maryland. After years of failed legislative efforts, in 2017, the Maryland Court of Appeals sought to move away from a reliance on money bail in its pretrial system and reduce unnecessary pretrial detention for defendants across the State. This court-led reform involved substantial revisions to the procedural rules governing decision-making on pretrial release and barred judges from setting money bail out of financial reach for defendants. This dissertation addressed the effects—and limits—of this reform, and examined questions about whether criminal defendants in Maryland are subject to pretrial “justice by geography” (Feld, 1991). Specifically, I explored four questions related to Maryland’s bail reform: (i) whether bail reform affects pretrial judicial decision-making; (ii) whether bail reform leads to increases in crime; (iii) whether pretrial decisions vary across counties; and, (iv) whether contextual factors, including organizational features of courts and the demographic, economic, and political contexts in which they are embedded, shape decision-making across counties. I summarize the major findings of the dissertation below.

First, I find that Maryland’s bail reform had immediate impacts on judicial decision-making at the pretrial stage. While the reform did not prohibit the ability of

judicial officers to set money bail, bail reform led to significant change in its use: using a regression discontinuity approach, I find that bail reform led to an immediate decrease in the probability of receiving money bail by roughly 14 to 16 percent, and for cases where bail is set, led to a 19 to 33 percent decrease in average bail amounts imposed on defendants at the initial appearance. In lieu of money bail, the reform led to an immediate increase in both the probability of being released without financial payment (by 8.8 to 10.9 percent) and the probability of being detained without opportunity for release (3.9 to 5.1 percent). Though the immediate shift in the probability of preventative detention is smaller in size than impacts on other pretrial determinations, over time, the probability of receiving detention without bail at initial appearance increased dramatically, after adjusting for case and defendant characteristics. Fewer than one in ten defendants were detained without bail in the two years prior to bail reform; in the two years following, 1 in 4 were preventatively detained. In the years following bail reform, the likelihood of receiving money bail continued to decline, while the likelihood of nonfinancial release remained relatively flat. These findings suggest that Maryland was successful in moving away from an over-reliance on money bail at the pretrial stage, but led to the unintended consequence of dependence on the most restrictive pretrial conditions for defendants across the State.

Second, despite persistent concern over the public safety implications of bail reform, little evidence exists to support general crime impacts. Responding to calls for more research into the crime impacts of bail reform (Wu & McDowall, 2024), I used synthetic control methods to estimate the impacts of bail reform on statewide crime in Maryland. I detect little evidence of substantive impacts of bail reform on crime rates—

either negative or positive—finding null results for the effects of bail reform on statewide crime rates, looking both at index crimes rates as well as offense-specific outcomes.

Third, this dissertation explored how pretrial decision-making varies across jurisdiction, assessing its extent of, and change in, between-county variation in pretrial decisions. Judicial officers rely primarily on individual case characteristics in deciding release conditions. Despite this, I find evidence that the county in which a defendant is brought to court matters for their pretrial outcomes: pre-reform, I find modest, but significant variability across counties in a range of pretrial decisions, including in set bail amounts, odds of nonfinancial release, and the odds of detention without bail. This changed after bail reform, but not in uniform ways: following bail reform, between-county variability in nonfinancial release declined, while variability across counties in average bail amounts increased. Finally, although the odds of detention without bail increased substantially post-reform, between-county variability did not change across reform periods, implying a uniform increase in its use across counties. In sum, jurisdiction matters for pretrial decision-making in Maryland following reform, though the degree to which location matters varies by specific pretrial decision. This may signal that the role of local legal cultures and their adaptations to new law varies across type of pretrial condition—with greater persistence of historical practices in money bail setting.

Lastly, in terms of what contributes to between-county variation in pretrial decisions, in line with court communities literature, I find that organizational features of courts, particularly court size (and to a lesser degree, caseload pressure), are significantly associated with variation in pretrial decision-making across counties. Contextual characteristics, particularly county-level jail crowding and violent crime rates,

were also associated with differences across counties in pretrial decisions. Moreover, court and contextual factors moderate the influence of bail reform on pretrial decision-making. Specifically, county economic contexts and organizational characteristics of county courts influenced the impacts of bail reform across counties. Broadly, these findings show that pretrial policy is not insulated from the larger contexts in which it is enacted, nor is policy change implemented in a consistent manner across place. The localized cultures that shape the everyday work in bail courts remain an important dynamic for understanding how policy—and policy change—is enacted on the front lines of the criminal justice system (Ulmer, 2019).

### ***Contributions of the Dissertation***

Overall, this dissertation makes contributions to empirical and theoretical scholarship on the pretrial process, and contributes policy-relevant knowledge to dialogues on the effectiveness of bail reform. I add to the emerging literature examining the effects of reforms aimed at curbing the use of money bail in pretrial systems in the United States. Others have lamented the dearth of rigorous evaluations of bail reforms, noting that policymakers lack a “clear blueprint for what works” (Jorgensen & Smith, 2021, 7). Rigorous understanding of the impacts of various bail reform initiatives—undertaken through different policy levers and in different contexts—is critical to providing a broad, and relevant, knowledge base for policy-makers looking to implement changes to pretrial justice systems.

Beyond this, the dissertation contributes to the body of work in several ways. First, this dissertation applies quasi-experimental methods to the study of Maryland’s bail reform, providing evidence of the effects of court-led reform on judicial decision-

making at the pretrial stage, after adjusting for a range of defendant, case, and contextual factors. These findings raise challenges for other policymakers and jurisdictions looking to enact bail reform like Maryland's, which was heavily reliant upon the exercise of judicial discretion to achieve policy aims. Many bail reform efforts focus on altering how judges decide bail conditions; the extent to which these efforts are considered successful lies in the extent to which court behavior changes in line with the aims of reform. Such efforts attempt to influence—but do not formally restrict—the discretion of judicial officers at the early bail stages, and judges retain wide latitude in exercise of that discretion (Scott-Hayward & Fradella, 2019). Advisory or voluntary policies intended to shape judicial decision-making can influence pretrial and sentencing outcomes (Bushway et al., 2012; Ouss & Stevenson, 2023). However, reliance on this form of voluntary change may attenuate the impacts of a policy, in ways that are complicated to unravel (Stevenson, 2018a; Stevenson & Doleac, 2019). This study affirms this contention. Research on the effects of Federal sentencing guidelines point to a layered interplay between courtroom workgroups and their contexts in shaping the exercise and influence of judicial discretion in sentencing events (Lynch & Omori, 2014; Rehavi & Starr, 2012). In most jurisdictions, pretrial decision-making takes place within ambiguous legal frameworks, and with even less information and fewer resources than at the sentencing stage. This dissertation casts doubt on the efficacy of bail reform efforts that fail to account for the ways in which judicial officers (and other courtroom actors) may exercise their discretion to explicitly or indirectly avoid, undermine, or subvert the aims of policy reform.

Second, I also contribute to court communities literature, applying its lens to the study of contextual influences on the pretrial process, and on court responses to policy change. I explored how pretrial decisions vary across county jurisdictions in a single state, providing a study of variation across substate jurisdictions in the application of the *same* policy. Other studies of contextual contributors to pretrial outcomes confound the role of such factors with omitted policy-level variables that vary widely across states (Scott-Hayward & Fradella, 2019). Contextual factors may also shape organizational response to new policy, an underexamined perspective in research on variation in the pretrial process. State-level examinations provide meaningful evidence to stakeholders interested in the outcomes of reform. However, understanding variation in the application of policy, and the contextual features that pattern that variation, can generate important theoretical and policy-relevant insights.

Third, I add to the literature on crime impacts of bail reform. Despite much public debate and political opposition, a growing consensus across several studies is that bail reform—in its varying forms—does not have substantial crime effects. In this dissertation, I applied synthetic control methods to estimate statewide crime impacts, extending the approach of recent studies by matching on a much longer time series of pre-intervention outcomes. I contribute to this nascent literature, finding that bail reform in Maryland did not significantly impact crime rates.

Finally, many contemporary studies on the pretrial phase overwhelmingly rely on a single, outdated source of national data, while other studies draw heavily from a small group of large metropolitan jurisdictions. Obtaining information on pretrial defendants, processes, and outcomes in other jurisdictions, particularly for non-felony cases and in

non-urban jurisdictions, is difficult given complexity and variation in the structure of court systems, and the fragmentation of court information (Tublitz, 2024). While this dissertation is a study of a single state reform—as in most other research on bail reform—it nonetheless widens the scope of criminal legal systems studied in pretrial research, relying on a unique administrative data source, and demonstrating that public repositories of criminal justice information are fruitful sources of research data on criminal legal system processes.

### **Policy Implications**

The results of this dissertation support the finding that Maryland’s bail reform reduced the use of money bail as a mechanism for release without any apparent impact on crime. Moreover, in many counties, judicial officers appear to have simply substituted money bail with preventative detention. Maryland’s bail reform story yields several lessons for other states and localities looking to reform their pretrial practices, and in particular, for policy change that is reliant on the exercise of judicial discretion to achieve policy aims.

Judicial officers face difficult decisions when making decisions about pretrial release. Notably, this reform was not accompanied by additional supports or tools for judicial officers in making determinations regarding ability to pay or to assess risk to public safety. Lacking any reliable or standardized process to collect or assess defendant information on ability to pay, judicial officers across Maryland appeared to rely on preventative detention in lieu of money bail. Other jurisdictions pursuing bail reform should carefully assess whether they have tools and capacity to conduct ability to pay determinations and should clearly articulate which governmental or judicial actor

in the criminal legal process is responsible for this task. Determining what constitutes affordability is not a straightforward exercise; however, analogous efforts to conduct individualized assessments with respect to monetary fines and fees have seen the adoption of individualized assessment tools and establishment of presumptions against the ability to pay any monetary amount (Johnson, 2024). Further, as noted above, reliance on the exercise of judicial discretion to achieve policy aims may invite unintended consequences and variability across court communities as they interpret and apply new law. Reformers should address the responsiveness of decision-makers tasked with on-the-ground implementation of bail policy reform (in Maryland's case, judicial officers) and develop trainings and tools to educate judges and courts on the aims of reform, identify implementation challenges, and develop solutions responsive to localized concerns.

### ***Future Directions***

Future research should extend and refine these analyses in several ways. First, in the studies of the impacts of Maryland's bail reform on pretrial decision-making and on crime, this dissertation examined average effects of bail reform for cases across the state. However, we might expect impacts to vary across certain groups of defendants or across certain types of cases; in particular, questions of differential impacts for Black or Latino defendants, or by specific charges. Correlational results show that certain demographic groups, particularly Black and Latino defendants, are more likely to experience more restrictive pretrial decisions at the bail hearing, net of case and criminal history factors. Though not examined in this study, unpacking the differential effects of bail reform might yield new insights into important questions about whether

the benefits or burdens of bail reform are borne disproportionately by some groups more than others. Moreover, public safety implications of bail reform may be more localized, or offense specific, in nature; studies of crime effects of changes to bail policy would benefit from the study of smaller geographic units.

Second, distinguishing the role of specific courtroom actors is unexamined in this dissertation, owing to data limitations. While this dissertation examined broader court organizational dynamics and social contexts, court communities research emphasizes the importance of individual motivations and the shared goals and norms of courtroom workgroups (Eisenstein & Jacob, 1977; Eisenstein et al., 1988). Few qualitative studies of pretrial courtroom dynamics and decision-making exist (but see Ottone & Scott-Hayward 2018; Suffet, 1966; Winter, 2023). More ethnographic and qualitative research may shed insight on individual and joint decision-making processes behind pretrial decisions and defendant outcomes, and highlight possible new avenues of reform.

Lastly, I echo many others in calling for more rigorous evaluation of bail reform initiatives, and their impacts on pretrial, case, social, and familial outcomes. The field of bail reform is broad and its approaches varied, yet little guidance on effective policies and paths forward exist for policymakers interested in reducing reliance on cash bail. Given the liberty interests at stake, the harms of pretrial detention to defendants and communities, the consequences of detention on case outcomes, and the scale of pretrial incarceration across the United States, bail and the pretrial criminal process more than deserves our full attention.

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Md. Code. Ann. Crim. Proc. § 5-202

MD Code. Ann. Crim. Proc. § 4-216(d)

MD Code. Ann. Crim. Proc. § 4-216.1(e)(1)(A)

MD Code. Ann. Crim. Proc. § 4-216.1(e)(1)(B)

# APPENDIX A. REGRESSION DISCONTINUITY ROBUSTNESS

Appendix Table 1. Regression Discontinuity Results for Case and Defendant Characteristics

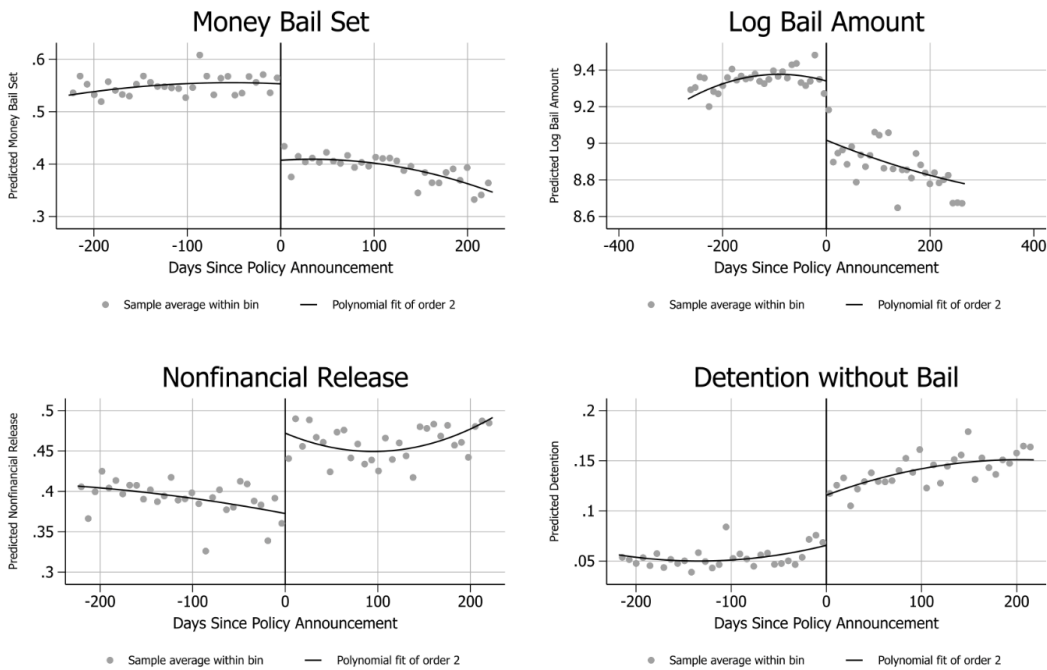
	Age in Years	Female	Defendant Race: Black	Defendant Race: Other	Defendant Race: White	Offense Type: Property	Offense Type: Person
Post-Announcement	0.047 (0.303)	-0.006 (0.00791)	-0.008 (0.0137)	-0.004 (0.00354)	0.005 (0.0131)	-0.003 (0.0105)	-0.003 (0.0162)
Optimal Bandwidth	158	324	110	203	121	205	106
Effective Num. of Cases	36824	78625	24970	48119	27462	48587	23942

	Offense Type: Drug	Offense Type: Other	Total Charges	Offense Severity Scale	Felony	Count of Prior Cases
Post-Announcement	0.024 (0.0157)	-0.002 (0.00216)	0.114 -0.0601	-0.021 (0.0415)	-0.004 (0.0119)	0.130 (0.161)
Optimal Bandwidth	88	228	260	186	178	203
Effective Num. of Cases	19369	54560	62933	43543	41556	47873

Notes: Each cell shows results for a separate regression. All models include day of week fixed effects, court jurisdiction (county) fixed effects.. Robust standard errors are in parentheses, and optimal (MSE) bandwidths are used (Calonico et al., 2014). \*  $p < .05$  \*\*  $p < .01$  \*\*\*  $p < .001$

Appendix Figure 1. Regression Discontinuity Results: Pretrial Decisions, Quadratic



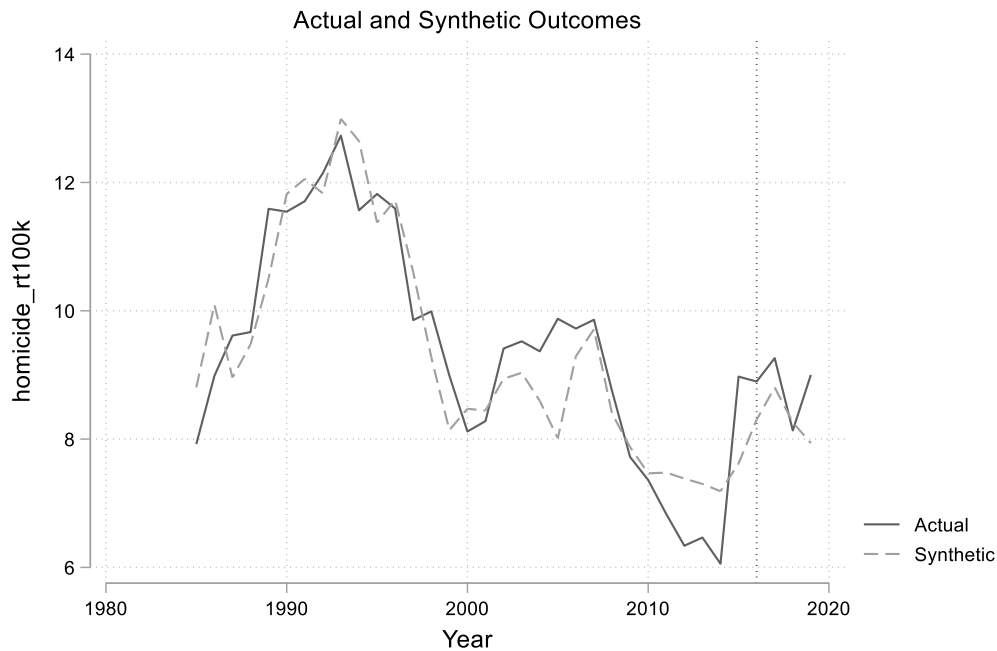
## APPENDIX B. SYNTHETIC CONTROL ESTIMATES BY CRIME TYPE

Appendix Table 2. Synthetic Control Estimates by Crime Type

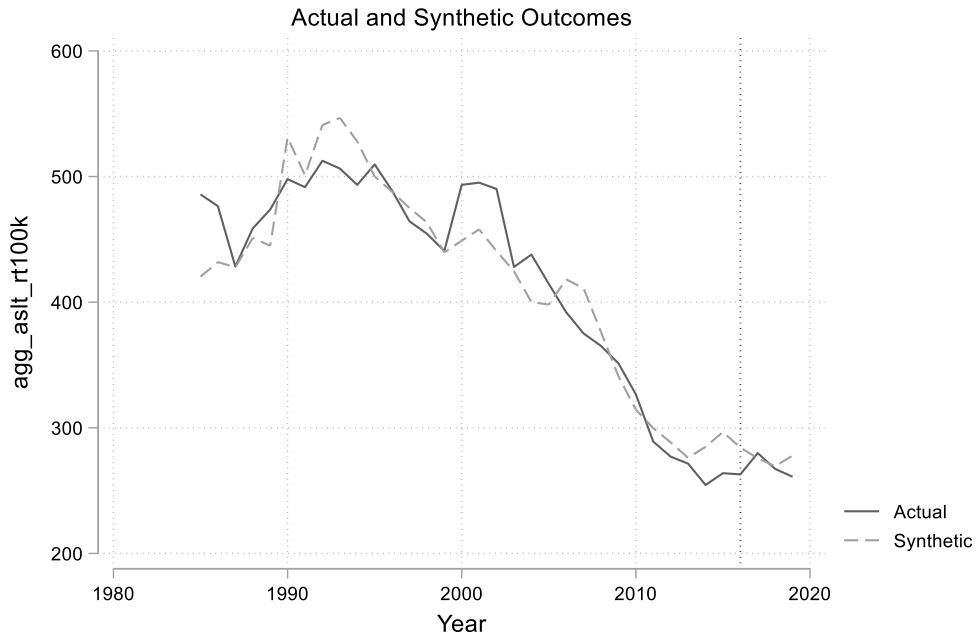
Outcome	ATE	Pre-Tx R-Squared	Pre-RMSPE	p(Ratio)
<b>Homicide</b>	0.46	0.80	0.75	0.72
<b>Aggravated Assault</b>	-4.93	0.88	27.87	0.98
<b>Robbery</b>	42.48	0.67	40.21	0.49
<b>Burglary</b>	-28.29	0.99	26.75	0.30
<b>Larceny</b>	-39.76	0.98	78.44	0.74
<b>Motor Vehicle Theft</b>	-67.49	0.97	29.50	0.21

Note: Average treatment effects (ATE) reflects the average gap between Maryland and its synthetic control over the post-reform period; RMSPE is the root mean square predicted error over the pre-intervention period;  $p(\text{RMSPE})$  is the exact  $p$  value generated from the distribution of ratios of post/pre-RMSPE from the in-sample placebo tests.

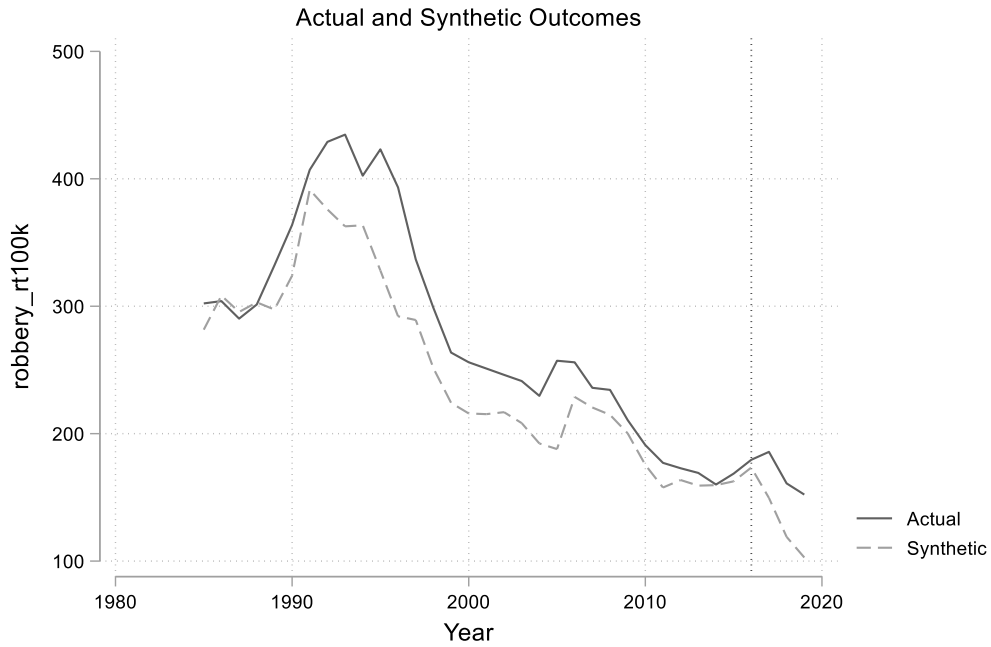
Appendix Figure 2. Homicide Per 100,000 Population: Actual and Synthetic Outcomes



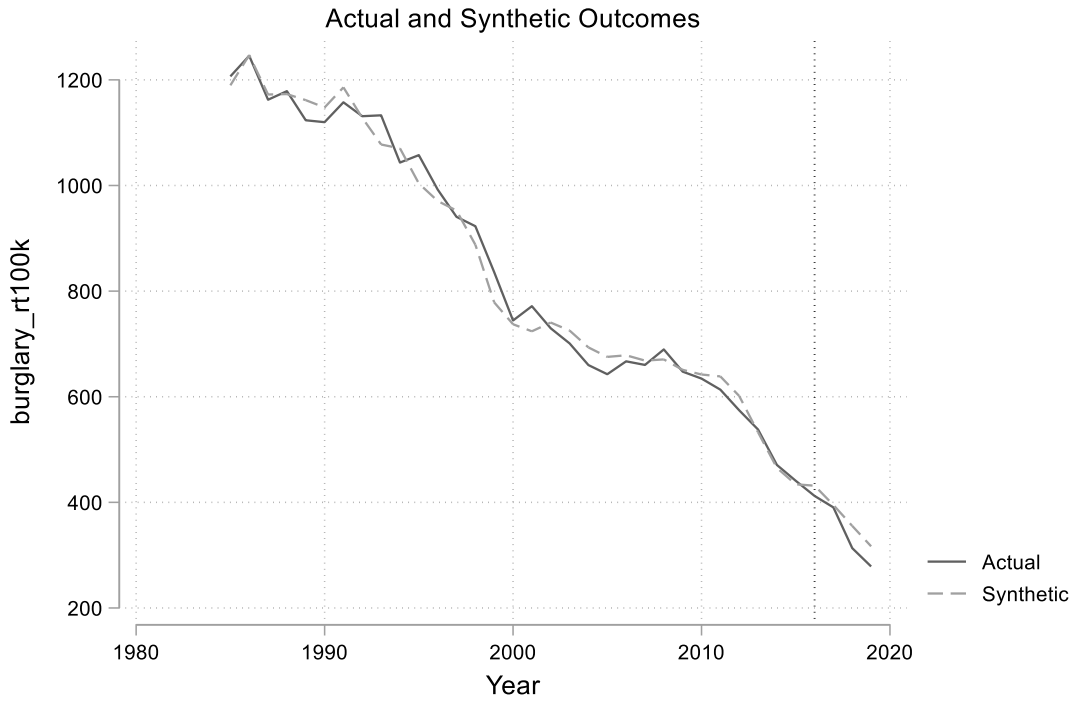
Appendix Figure 3. Aggravated Assault Per 100,000 Population: Actual and Synthetic Outcomes



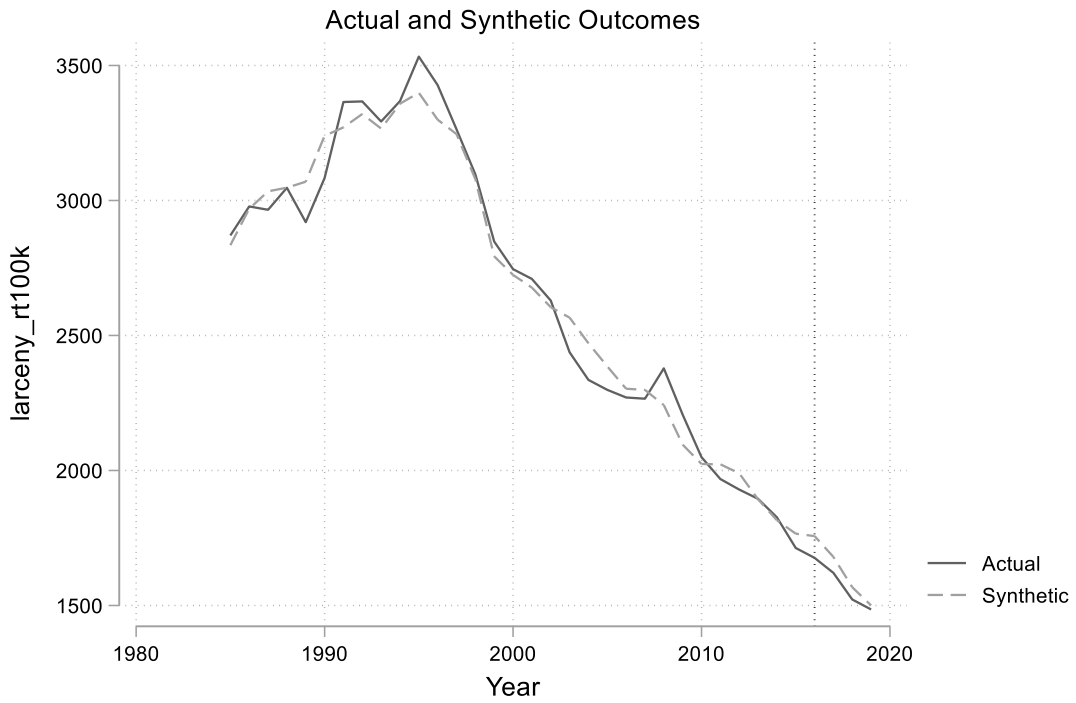
Appendix Figure 4. Robbery Per 100,000 Population: Actual and Synthetic Outcomes



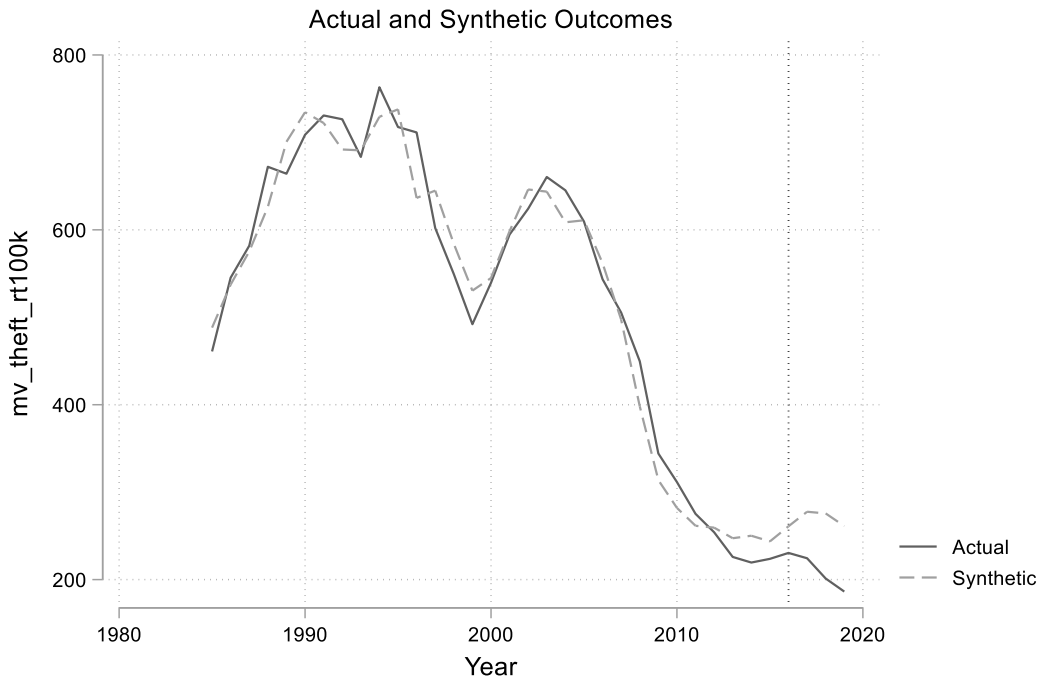
Appendix Figure 5. Burglary Per 100,000 Population: Actual and Synthetic Outcomes



Appendix Figure 6. Larceny Per 100,000 Population: Actual and Synthetic Outcomes

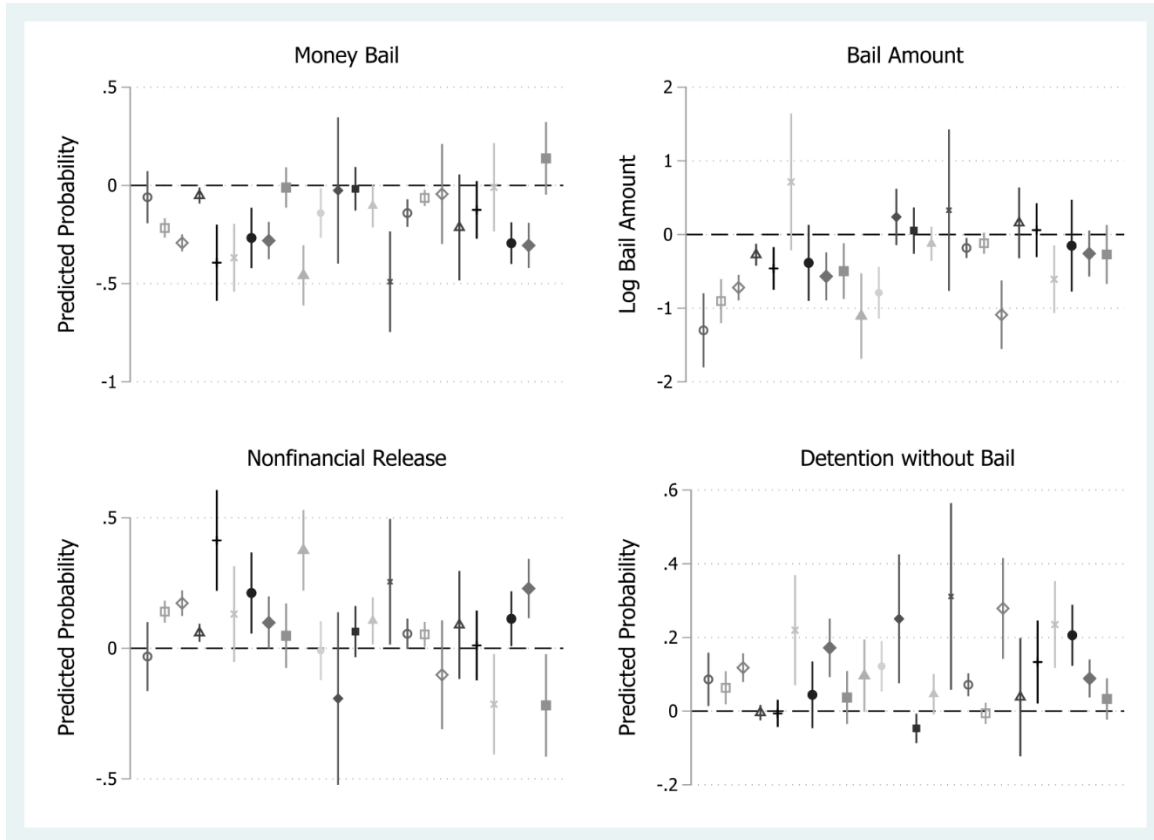


Appendix Figure 7. Motor Vehicle Theft Per 100,000 Population: Actual and Synthetic Outcomes



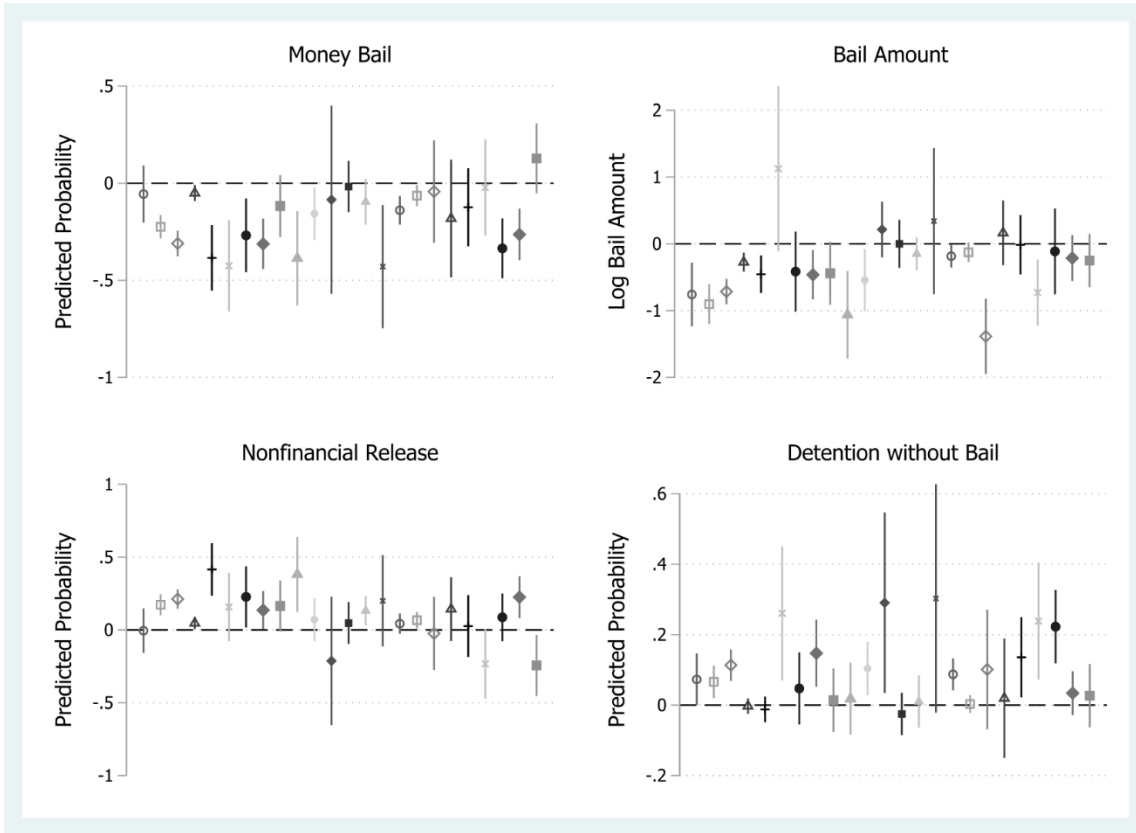
## APPENDIX C. COUNTY-LEVEL REGRESSION DISCONTINUITY ESTIMATES

Appendix Figure 8. County-Level Regression Discontinuity Estimates of Bail Reform on Pretrial Decisions



*Note: Each dot represents results from a separate regression for a single county (n=24). All models include day of week fixed effects along with individual case and demographic covariates, and use MSE optimal bandwidths. 95% confidence intervals are presented around each point estimate.*

Appendix Figure 9. County-Level Regression Discontinuity Estimates of Bail Reform on Pretrial Decisions, Fixed Bandwidth



*Note: Each dot represents results from a separate regression for a single county (n=24). All models include day of week fixed effects along with individual case and demographic covariates. 95% confidence intervals are presented around each point estimate. MSE-optimal bandwidths used in the statewide analyses in Chapter 2 are applied to each county here (i.e., each county-level RD estimate uses the same optimal bandwidth across each outcome).*

## APPENDIX D. FIXED EFFECTS MODELS, COUNTY-LEVEL MODERATORS OF BAIL REFORM

Appendix Table 3. Fixed Effect Model Results

	Log Bail Amount Model 7		Nonfinancial Release Model 8		Detained without Bail Model 9	
	Exp. Coef.	(SE)	Odds Ratio	(SE)	Odds Ratio	(SE)
<b>Post-Bail Reform</b>	0.746*	(0.0804)	1.148	(0.238)	2.822***	(0.305)
<b>Level 1 Controls</b>						
<b>Age at Filing (Years)</b>	0.996*	(0.00134)	1.001	(0.00346)	1.000	(0.00207)
<b>Female</b>	0.716***	(0.0288)	1.681***	(0.0832)	0.447***	(0.0269)
<b>Race (ref = White)</b>						
<b>Black</b>	1.131**	(0.0471)	0.792***	(0.0326)	1.320***	(0.0520)
<b>Asian/Pacific</b>						
<b>Islander</b>	1.206**	(0.0628)	0.968	(0.0944)	1.199	(0.150)
<b>Native American</b>	0.949	(0.0453)	0.913	(0.153)	1.005	(0.134)
<b>Other Race</b>	1.048	(0.0386)	0.682***	(0.0554)	1.303***	(0.0704)
<b>Offense Type (ref = Property)</b>						
<b>Person</b>	1.548**	(0.189)	0.764	(0.130)	3.230***	(0.572)
<b>Drug</b>	0.871***	(0.0132)	3.069***	(0.180)	0.464***	(0.0405)
<b>Other Type</b>	1.461***	(0.0613)	0.886	(0.163)	3.732***	(0.606)
<b>Number of Charges<sup>^</sup></b>	1.139***	(0.0221)	0.445***	(0.0618)	2.363***	(0.247)
<b>Severity of Top Charge</b>	1.276***	(0.0256)	0.677***	(0.0145)	1.603***	(0.0388)
<b>Count Prior Cases<sup>^</sup></b>	0.996**	(0.00136)	0.562***	(0.0268)	1.762***	(0.0879)
<b>Main Effects</b>						
<b>Caseload per Judge</b>	1.000	(0.0000925)	1.000	(0.000232)	1.000*	(0.000234)
<b>Jail Crowding</b>	1.002	(0.00251)	0.996	(0.00334)	0.994	(0.00348)
<b>Violent Crime Rate</b>	1.000**	(0.000157)	1.000	(0.000194)	1.000	(0.000340)
<b>Interaction Terms</b>						
<b>Reform * % Black</b>	0.969***	(0.00280)	0.970**	(0.0106)	0.988**	(0.00385)
<b>Reform * % Latino</b>	0.931***	(0.00777)	0.882**	(0.0394)	0.970	(0.0177)
<b>Reform * % Poverty</b>	1.066*	(0.0260)	0.895	(0.0520)	0.964	(0.0191)
<b>Reform * % Unemployed</b>	0.921	(0.0510)	1.554***	(0.171)	1.262***	(0.0507)
<b>Reform * Court Size - Medium</b>	0.615***	(0.0759)	0.804	(0.198)	0.553***	(0.0552)
<b>Reform * Court Size - Large</b>	2.249***	(0.389)	1.195	(0.565)	0.387***	(0.0722)
<b>Reform * Caseload Per Judge</b>	1.000*	(0.0000450)	1.000	(0.000109)	1.000***	(0.000068)
<b>Reform * Jail Crowding</b>	0.998	(0.00289)	1.002	(0.00409)	1.004	(0.00324)
<b>Reform * Violent Crime Rate</b>	0.999***	(0.000166)	0.999	(0.000572)	1.000	(0.000269)
<b>Reform * % Republican</b>	0.960***	(0.00535)	0.956**	(0.0157)	0.986*	(0.00649)
<b>County Fixed Effects</b>	Yes		Yes		Yes	
<b>Year Fixed Effects</b>	Yes		Yes		Yes	
<b>Log Likelihood</b>	-158365		-171591		-88099.5	
<b>AIC</b>	316776.0		343227.8		176245.1	
<b>BIC</b>	316996.4		343471.5		176488.7	
<b>N</b>	107121		294330		294330	