THREE PAPERS ON THE EFFECTS OF CRIMINAL PROCEDURE ON THE EXERCISE OF DISCRETION IN THE CRIMINAL JUSTICE SYSTEM

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For my Dad,

the first scientist I ever met

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ABSTRACT

THREE PAPERS ON THE EFFECTS OF CRIMINAL PROCEDURE ON THE EXERCISE OF DISCRETION IN THE CRIMINAL JUSTICE SYSTEM

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This dissertation examines how three systems of criminal procedure shape the exercise of discretion in the criminal justice system. Chapter 1 considers the relationship between sentencing guidelines and judicial sentencing decisions. Using simulation modeling, it challenges a widely held belief that robust sentencing guidelines increase uniformity in sentencing at the cost of fairness.

Chapter 2 turns to police regulation. Police departments and policymakers have implemented a range of mechanisms to regulate police discretion, but much of the scholarly literature has expressed skepticism about their effectiveness. One regulatory approach has largely escaped scrutiny—prosecutorial screening. This study examines the effect of prosecutorial screening on police charge decisions in one major metropolitan city in the United States. Exploiting the fact that the screening program only applies to suspects over seventeen years of age, it compares suspects arrested just a few weeks before and a few weeks after their seventeenth birthday. The analysis reveals a drop in felony charges against suspects arrested just after the age boundary for crimes subject to prosecutorial screening. The same pattern is not observed for crimes not subject to

screening, suggesting that officers file lesser charges against suspects over seventeen years of age in anticipation of the stringent screening process.

Chapter 3 explores the role of discovery rights on the plea bargaining process. It begins by extending prior work on civil discovery to develop a theory of criminal discovery. It then conducts the first systematic empirical investigation of the effects of expanding criminal discovery on case outcomes in one state that recently enacted legislation granting defendants wider discovery rights. A series of difference-in-differences models comparing felony and misdemeanor courts provide little evidence that the law promoted judicial efficiency by reducing the trial rate or that it produced more favorable outcomes for defendants by increasing the dismissal rate.

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Introduction

Discretion is a necessary feature of the criminal justice system. As just a few examples, police officers must decide whom to stop, search, and arrest every day. Prosecutors must decide what charges to file, plea offers to make, evidence to disclose, and sentences to recommend. And judges must regularly adjudicate guilt, select sentences for convicted defenders, and resolve complex legal issues without binding precedent. There can be no functioning criminal justice system without the exercise of discretion by unelected officials.

At the same time, scholars have long appreciated that discretion also has a dark side. Officials can abuse authority by violating individual rights, by engaging in unlawful discrimination, and by enforcing the coercive powers of the state against the innocent and undeserving.

The traditional strategy proposed by scholars to address the dark side of discretion is to eliminate, reduce, or move it somewhere else. This was the goal, for example, when many states adopted sentencing guidelines to constrain judicial discretion (Frase 1995). It was also the goal when some jurisdictions banned plea bargaining in the 1970s and 1980s (Rubinstein & White 1978). And it was the goal when states and police departments across the country adopted mandatory arrest policies for domestic violence cases (James 1994).

Yet this traditional regulatory strategy often produces surprising, unwanted and even perverse consequences. It sometimes pushes discretion to an earlier phase in the criminal process to officials who are less qualified to wield it, as in the case of mandatory

minimum sentencing statutes (Miethe 1987). Other times it may lead to unmanageable workloads because all cases are treated equally regardless of importance (Callan 1979; Holmes et al. 1992). And it may decrease social utility by causing violence (Sherman et al. 1992) or exacerbating inequality (Starr and Rehavi 2013).

Chapter 1 examines an area of criminal procedure that falls plainly within this traditional regulatory paradigm. It examines the relationship between sentencing guidelines and sentencing decisions. Using simulation modeling, it challenges a widely held belief that robust sentencing guidelines increase uniformity in sentencing at the cost of fairness.

The remaining two chapters examine alternative, more creative strategies for addressing the dark side of discretion, not by eliminating or moving it, but by designing systems of procedure that realign the natural incentives of actors in the system to regulate each other. Chapter 2 explores the effects of a charge screening program in which police officers must phone the prosecutor's office for approval before filing felony charges against suspects over seventeen years of age. It provides significant empirical evidence that officers seek lesser charges against suspects who are over seventeen years old in anticipation of the stringent screening process. It theorizes that officers learn the nuanced and complex screening standards over time by seeking frequent review of felony charges and by receiving immediate feedback from the prosecutor's office.

Chapter 3 turns to the plea bargaining process, where scholars and policymakers have expressed concerns that excessive prosecutorial powers allow the state to both charge and adjudicate guilt. Prosecutors' offices have implemented a range of traditional

strategies for addressing this dark side of discretion, including banning plea bargaining and establishing mandatory administrative reviews of plea offers. This Chapter examines an alternative regulatory approach, statutory expansions in criminal discovery. It argues that granting defendants a statutory right to all of the government's evidence early in the criminal process eliminates information asymmetries, and thus enables defense attorneys to regulate prosecutorial discretion by demanding bargains that fit the level of evidence available against the defendant.

Chapter 1. Questioning Blackmun's Thesis: Does Uniformity in Sentencing Entail Unfairness?

Ben Grunwald

Abstract

Criminal justice scholars commonly argue that sentencing guidelines increase uniformity in sentencing at the cost of fairness. They reason that guideline systems rarely take all relevant case characteristics into consideration, and as a result, impose sentences in particular cases that are biased relative to the ideal or best sentence. This bias effect is one of the primary theoretical and practical challenges faced by judges and sentencing commissions in the last thirty years and provides one of the strongest arguments against mandatory sentencing guidelines. This Chapter identifies a second effect of guidelines on fairness, which has not been sufficiently acknowledged by the scholarly literature: the variance effect increases the fairness of sentences directly by increasing uniformity. The Chapter then employs statistical simulation to examine the relationship between the variance and bias effects. The results provide substantial evidence that the variance effect is comparatively large, and that it will often outweigh the negative effects of bias. Under these circumstances, sentencing guidelines both increase uniformity and increase fairness.

I. INTRODUCTION

Until the 1970s, judges in the United States enjoyed nearly unlimited discretion in assigning sentences to criminal offenders. Judges were free to adopt their own theory of punishment, to determine how that theory applied to the facts of a case, and to select the most appropriate punishment scheme on that basis. Typically, only very wide statutory ranges constrained judges' power to individualize sentences (Stith & Koh 1993). Early empirical research from the 1960s to the early 1980s revealed that the existing system produced large sentencing disparities, finding that similar defendants with similar convictions often received different punishments. Scholars theorized that these disparities arose from variations in judges' ideological background (Forst & Wellford 1981; Partridge & Eldridge 1974), and from discrimination based on legally irrelevant characteristics such as race and ethnicity (Baldus et al. 1983). Contemporary research confirms that these phenomena continue to impact sentencing today (Bushway & Piehl 2001; Albonetti 1997; Abrams et al. 2012).

In response to evidence of disparity, federal and state legislatures established sentencing guideline systems to constrain judicial discretion. These systems typically used a limited number of variables to determine a sentence range for each defendant. Judges were then encouraged, or in some systems, required to impose a sentence within that range (Frase 1995). The level of discretion left up to the judge varied widely by jurisdiction, but no system was more restrictive than the Federal Sentencing Guidelines, which gave nearly all weight to crime severity and criminal history (Tonry 1993).

Almost immediately, the Federal Guidelines were criticized for promoting uniformity at the cost of fairness in individual cases (e.g., Ogletree 1987). Critics widely argued that the federal guidelines decreased the fairness of sentences by constraining judges' ability to take relevant case characteristics into consideration. I call this the bias effect of sentencing guidelines: guidelines can bias sentences away from the fairest or most appropriate sentence by limiting judges' ability to take all relevant case characteristics into consideration and to fully individualize punishment. While discussing disparities in the administration of the capital punishment, Justice Harry Blackmun articulated a well-known statement of the bias effect: "Experience has shown that … consistency and rationality ... are inversely related to [fairness]. A step towards consistency is a step away from fairness" (Callins v. Collins, 1994). Though Blackmun articulated this thesis in the context of the death penalty, I take his critique as a paradigmatic formulation of a broad and popular criticism of mandatory sentencing guidelines echoed by judges (Schwarzer 1991) and scholars (Ogletree 1987; Freed 1992; Alschuler 1991; Tonry 1993), which persists until today (Osler 2003; Kim 2004; O'Hear 2006).

This paper advances the sentencing literature in two ways. First, it develops a novel framework for conceptualizing the relationship between uniformity and fairness. Second, it tests Blackmun's Thesis by identifying and examining a second effect of sentencing guidelines that has not been acknowledged by the academic literature:

increasing uniformity through guidelines has a second effect—a variance effect—of directly increasing the fairness of sentences on average.[1]

This paper uses simulation modeling to examine the relationship between sentencing guidelines, uniformity and fairness. I begin by arbitrarily defining an "ideal" sentence for a set of equivalent criminal cases. I then randomly generate a pre-guideline distribution of sentences centered around the "ideal" sentence. Based on existing estimates from the literature, I then introduce a sentencing guideline system, and posit a series of plausible bias and variance effects on the sentence distribution. The average distance of sentences in the pre- and post-guideline distributions from the "ideal" sentence is then compared to determine whether, under these assumptions, Blackmun's Thesis would hold. This approach provides an important methodological benefit. Unlike many other analytic methods in the sentencing literature, this paper avoids the need to assume a thick normative theory about the purposes of punishment and the case characteristics that are relevant in sentencing.

The results of the analysis show that, under plausible conditions, the variance effect of sentencing guidelines is comparatively large, and may often outweigh the negative effects of bias. When outweighed, Blackmun's Thesis does not hold, and the

[1] The academic literature on sentencing guidelines is voluminous, and scholars have of course raised other criticisms of guidelines in addition to Blackmun's Thesis. They have, for example, questioned whether the normative principles of sentencing can be codified into a finite set of rules, and whether the technical nature of guidelines sap sentencing hearings of their moral force (Stith & Cabranes 1999). As discussed in greater detail below, they have also argued that sentencing guidelines may merely move disparities to earlier phases in the criminal justice system, most notably to the plea negotiation stage (Starr & Rehavi 2013; Alschuler 2005). While these criticisms are certainly important, they have received significant and extended scholarly attention. This paper focuses on Blackmun's Thesis, which has not yet been subjected to extended critical scrutiny.

guidelines both increase uniformity, and increase or maintain the existing level of fairness, thereby defusing one of the most formidable arguments against restrictive sentencing guideline systems.

The remainder of the Chapter proceeds as follows. I begin with a brief history of sentencing guidelines and a review of the empirical literature on sentencing disparity. Next, I develop a conceptual framework to clarify and explore the contours of Blackmun's Thesis, and then describe the basic design of the study. I conclude by reporting the results of the analysis, and by discussing the implications for the academic and policy debate on sentencing guidelines.

II. SENTENCING GUIDELINES

From the late 1970s to 1990s, Congress and a number of state legislatures established sentencing commissions to design guidelines that would increase uniformity in sentencing. Congress, for example, established the United States Sentencing Commission through the Sentencing Reform Act of 1984. The commission was authorized to develop general rules to regulate the form (e.g., fine, probation or imprisonment) and intensity of sentences on the basis of a list of variables including seriousness of the offense, criminal history, age, education, vocational skills, mental and emotional condition, family responsibilities, community ties, and extent of participation in the offense. In 1987, the commission passed a guideline system based primarily on two of those variables, seriousness of offense and criminal history. A limited number of additional case features were also given some weight (e.g., the amount of money stolen or the use of a weapon) (Ogletree 1987). Based upon these characteristics, the guidelines

were designed to output a narrow range of sanctions from which judges were required to select the most appropriate punishment.

A number of states such as Minnesota, Pennsylvania, Virginia and Massachusetts also enacted sentencing guidelines (Frase 1995). These systems varied widely, but in general, they were less restrictive, complex and controversial than the federal guidelines (Tonry 1993). Unlike the presumptive or mandatory system enacted by Congress, many state legislatures adopted advisory guidelines that served as recommendations rather than binding rules (Frase 1995). Many state guideline systems, including that of Minnesota and Pennsylvania, have received approval from legal scholars and social scientists (Tonry 1993).

Today, all federal and state guideline systems are advisory as a result of two Supreme Court cases, *United States v. Blakeley* (2004) and *United States v. Booker* (2005). The tension between uniformity and fairness in sentencing guidelines, however, remains a live policy debate. First, empirical evidence suggests that advisory guidelines continue to influence judges' sentencing practices (Pfaff 2006; Bushway et al. 2012). Second, the Supreme Court rendered sentencing guidelines advisory on a relatively narrow and technical issue, and scholars have noted the availability of mandatory systems that would survive *Blakely* and *Booker* review (Chanenson 2004). Third, the United States Sentencing Commission has recently proposed legislative and appellate court constraints on judicial discretion, which according to two scholars, would "restore the Guidelines very nearly to the legal status they enjoyed before *Booker*" (Starr & Rehavi 2013: 9).

III. EMPIRICAL RESEARCH ON SENTENCING DISPARITY

I review the empirical literature on sentencing disparity for two purposes. First, a review of existing empirical methodologies helps clarify the strengths of the current study. Second, the review also helps set plausible bounds on the parameters of the quantitative analysis by answering two key questions: what is the magnitude of sentencing disparity, and what is the effect of sentencing guidelines on disparity? Researchers have used two main methodological approaches to answer these questions: comparable distributions and identical cases. I discuss each literature separately.

A. The Comparable Distribution Approach

The comparable distribution approach assumes that certain groups of cases are statistically equivalent either by controlling for observable variables or by exploiting random assignment of cases to judges.

Studies that control for observable variables typically measure sentencing disparity by variation in the dependent variable. These studies find that total sentencing disparity decreases after the enactment of guidelines. Karle and Sager (1991), for example, report substantial reductions in the variation of sentences within broad categories of crime (e.g. robbery) after the enactment of the federal guidelines. A United States Sentencing Commission report examines narrower categories of crime (e.g., bank robbery of less than $10,000 with no criminal history), and finds that variation in sentences decreased by 15 to 60% for nearly all categories examined after the federal guidelines were enacted (USSC 1991). Stolzenberg and D'Alessio (1994) fit a linear

regression model with four independent variables[2] and measure disparity as "total sentencing disparity unexplained by legally mandated sentencing factors" (R2). The authors estimate a roughly 60% relative reduction in sentencing disparity after the enactment of the guidelines in Minnesota (Stolzenberg & D'Alessio 1994: 302).

As others have noted, sentencing studies that control for observable confounders have several important methodological limitations. First, the models may not include all relevant independent variables (Baumer 2013). This is particularly problematic for studies that measure disparity based on unexplained variation in sentence lengths (e.g., Stolzenberg & D'Alessio 1994). In these studies, the omission of any legally relevant variable will bias the measure of disparity regardless of its correlational structure. Thus, in practice, these studies capture both unwarranted and warranted sources of variation in their estimates of disparity.

Second, omitted variable bias is particularly problematic in the sentencing disparity literature because there is little normative consensus among judges and scholars about the variables that are relevant to sentencing (Hofer et al. 1999; Rhodes 1991). Some studies have attempted to address this problem by defining equivalent cases based on the categories of crimes defined by a sentencing guideline system (e.g., Rhodes 1991). But, unless one believes that the guideline system has correctly grouped similar cases, these studies merely confirm that "post-Guidelines sentences are more likely to be in accordance with the Guidelines" (Anderson et al. 1999: 280; Tonry 1993). Indeed, testing

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[2] Offense seriousness, criminal history, presence of a weapon, and whether the most serious conviction offense was a personal crime.

whether sentencing disparity itself changed would require a normative theory about the case characteristics that are relevant at sentencing—a theory over which many readers are likely to disagree.

Third, most sentencing studies that control for observable variables focus exclusively on sentencing, and do not capture disparities arising from earlier phases in the criminal justice system (see Baumer 2013). This criticism applies to most studies in the literature, including those that adopt other methodological approaches. But, they are particularly relevant for studies that examine the *effects* of guidelines on disparity. Guidelines may introduce disparities into the charging process by increasing the power of prosecutors during plea negotiations (e.g., Starr & Rehavi 2013; Miethe 1987). As a result, when studies find that sentencing disparity has decreased after guideline enactment, it is often unclear whether the disparity has merely moved to an earlier stage in the process.

Some scholars have taken an alternative methodological approach by exploiting the random assignment of cases to judges. Since random assignment ensures that judges receive roughly equivalent caseloads, social scientists have compared the mean sentence of each judge in the same district to assess inter-judge sentencing disparity. The earliest study to use this approach found that among six judges in one district, one judge imposed a median sentence of six months, while another imposed a median sentence of twelve (Gaudet et al. 1934).

More recent studies have used random assignment to estimate the effect of federal guidelines on inter-judge disparity. In a study of three federal districts, Waldfogel (1991)

estimates the mean absolute deviation—the average difference between the mean sentence length of each judge and the grand mean sentence length for all judges—before and after the enactment of the federal guidelines. Pre-guidelines, the mean absolute deviation was between 4.5 and 6 months, or between 12% and 26% of the mean sentence length respectively.[3] After enactment, the mean absolute deviation doubled in two of the districts. Anderson et al. (1999) use a similar approach with data from 140 federal judges. The authors find that, prior to the guidelines, two judges imposed sentences that differed from each other by 16 to 18% on average. That estimate declined by 8 to 13% after the guidelines were adopted. Other studies using random assignment of cases to judges have used an alternative measure of inter-judge disparity. Rather than comparing the mean sentences for each judge, they consider the proportion of variation in sentences attributable to the judge assigned to the case. Applying this approach, Payne (1997) and Hofer et al. (1999) find mixed results: disparity decreased for some crime categories after guideline enactment and increased for others.

Scholars have also examined the effect of changes to the Federal Sentencing Guidelines after their enactment. Most importantly, the United States Supreme Court rendered all sentencing guidelines effectively advisory in 2004 and 2005 (U.S. v. Booker; Blakeley v. Washington). Two later cases, *Gall v. United States* (2007) and *Kimbrough v. United States* (2007), further expanded judicial sentencing discretion. Scott (2010) uses random assignment in three federal districts to show that the mean absolute deviation increased from 4.6 months before *Booker* (or 15% of the average sentence length) to 6.2

[3] The author reports the overall average sentence lengths in bar chart form (1991: 154).

months after *Booker* (18%), and 9.0 months after *Gall*/*Kimbrough* (26%).[4] Yang (2014) reports substantively similar results with data from all federal districts.

Taken together, the random assignment literature suggests that prior to guideline enactment, the mean absolute deviation of judges' sentences were between 10 and 26% of the average sentence length (Waldfogel 1991; Anderson et al. 1999). The literature also suggests that guideline enactment and subsequent guideline policy changes may have resulted in changes in the mean absolute deviation that range between 0 and 26% (Anderson et al. 1999; Scott 2010).

Prior work has acknowledged several limitations in the random assignment approach. First, as Hofer et al. (1999) note, random assignment captures only the "tip of the iceberg" because it cannot capture disparities arising from sources other than judge assignment. Second, by focusing on the mean sentence of judges, the random assignment approach measures only a small fraction of all inter-judge disparity. Even if they share the same mean, judges' sentences may have different functional forms or standard deviations. And judges may impose the same sentence on average without imposing the same sentence in particular cases. Third, random assignment does not ensure that a court receives an equivalent caseload over time. As a result, these studies cannot rule out the possibility that changes observed after guideline enactment are caused by secular trends in the criminal justice system.

[4] Scott uses a different measure of inter-judge disparity, but provides sufficient data to calculate the mean absolute deviation (2010: 61).

B. The Identical Case Approach

The identical case approach measures sentencing disparity in individual cases rather than in distributions of comparable cases. The typical study provides judges with an identical set of real or hypothetical cases and requests a sentence recommendation for each. Unfortunately, no identical case studies in the literature are longitudinal. They provide insight on the magnitude of disparity at a given moment, but cannot estimate the effect of guideline changes over time.

Two early research initiatives (Seminar and Institute 1962; Sentencing Institute & Joint Council 1962) asked federal judges to assign sentences for a diverse set of hypothetical cases. The recommended sentences varied widely. One tax evasion case drew recommendations as lenient as a six-month suspended prison sentence and as harsh as a five-year prison sentence with a $20,000 fine. The recommended sentences for an embezzlement case ranged from probation to five years in prison. Similarly, Partridge and Eldridge (1974) conducted a study in which fifty federal district court judges were given complete pre-sentence reports and were asked to recommend a sentence for each. The authors find evidence of "substantial" disparities in the judges' sentencing recommendations.

Forst & Wellford (1981) distributed hypothetical bank robbery and fraud cases to 264 federal judges. Each hypothetical provided a limited number of facts about the case. Their results present evidence of large and sometimes huge disparities in recommended prison length. On average, the bank robbery cases received a sentence length of 8.7 years, and an average standard deviation of 5.2. Similarly, the fraud cases received an average

sentence length of 5.2 years and a standard deviation of 3.3. Thus, the inter-judge disparity was roughly 60% of the average sentence length.

Scholars have also applied the identical case approach to state court judges. Austin and Williams (1977) distributed descriptions of five hypothetical minor felony and misdemeanor offenses to forty-seven Virginia district court judges. The case descriptions "conveyed … defendant's name, the criminal charge and a synopsis of the testimony" (Austin & Williams 1977: 307). The authors find that even "when legal cases are equalized within offense categories, judges still show substantial disparity..." (Austin & Williams 1977: 309).

Diamond and Zeisel (1975) explore disparity through sentencing councils in New York and Chicago. In these councils, judges sought advice from each other on sentencing decisions for real cases on their dockets. Colleague judges were informed of the facts of each case through pre-sentence reports prepared by the probation department. The authors find that two judges on the same council "differ[ed], on the average, by between one-third and one-half of the mean sentence" (Diamond & Zeisel 1975: 122).

Taken together, the best studies using the identical case approach observe distributions of sentence recommendations with standard deviations that are 30 to 60% of the mean sentence length.

Scholars have identified some important limitations on the validity of data gathered through the identical case approach. First, judges may understand the aim of the study and "deviate from their normal sentencing practice" to "dispel an unwanted reputation" (1975: 116). The implication is that more extreme judges might recommend

sentences closer to the average leading to a conservative estimate of the true magnitude of disparity.

Second, scholars have argued that judges receive less information for hypothetical cases than real criminal cases and that less information will encourage judges to use their imaginations to fill in the "gaps" and inflate the estimate of disparity. This is a limitation, but it seems just as likely that information gaps would deflate the estimate. As Johnson (2003) has argued, hypothetical fact patterns with few details abstract away controversial case features over which there is little consensus and which could generate great differences in recommended sentences.[5]

Third, Diamond and Zeisel (1975) note that judges may recommend more severe sentences than in true criminal cases because it is easier to imprison a hypothetical defendant than a living person. These concerns are somewhat addressed by sentencing counsel data. Sentencing councils provide an optimal window to examine disparity because judges receive detailed pre-sentence reports and "know that their recommendations can and often do have a real impact on the sentence actually imposed… [N]o more realistic arrangement can be devised that will allow several judges to sentence one offender" (Diamond & Zeisel 1975: 116).

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[5] Moreover, most criminal cases are disposed by plea bargain. Thus, judges typically do not benefit from the wealth of information brought out at trial and instead receive only limited information through a pre-sentence investigation report and a brief sentencing hearing (Johnson 2003). This depth of information is similar to the information judges receive in at least some of the studies that use the identical case approach (e.g., Partridge & Eldridge 1974).

IV. CONCEPTUAL FRAMEWORK

Blackmun's Thesis asserts that imposing uniformity through sentencing guidelines introduces bias into sentencing that leads to an overall reduction in fairness. The following section defines several terms and concepts to clarify its precise contours of Blackmun's Thesis.

A. Definitions

The first key concept is the ideal sentence. The ideal sentence is the best sentence, or the fairest sentence in a particular case. It is the sentence that a defendant would receive in a perfect criminal justice system. Of course, views about the ideal sentence in particular cases will vary from person to person depending upon their normative theory of punishment (US Sentencing Commission 1987; Rossi, Berk & Campbell 1997). For example, under a retributive theory, the purpose of sentencing is to give offenders what they deserve (Tonry 2006). The ideal sentence, then, depends on a limited set of case characteristics related to the severity of the crime and the culpability of the defendant. In contrast, under utilitarian theories of punishment, the purpose of sentencing is to promote social utility. Utilitarian theories often emphasize rehabilitation or specific deterrence. Under these theories of punishment, the ideal sentence may depend upon a much larger group of case characteristics that correlate with recidivism, including age, gender, employment and family.

The current study does not attempt to resolve this longstanding debate about the correct theory of punishment or the case characteristics that are relevant to sentencing. Instead, it attempts to examine Blackmun's Thesis and the relevant tensions between bias

and uniformity—to the extent possible—without assuming any particular substantive theory of sentencing.[6] It does so by making assumptions about the numeric value of the ideal sentence that are favorable to Blackmun's Thesis and by examining the underlying mathematical relationships of the concepts of bias, uniformity and fairness. I leave the definition of the ideal sentence vague to be inclusive of diverse normative views about sentencing.

A relevant case feature is a feature of a criminal case that impacts the ideal sentence. The use of a weapon to facilitate a crime, for example, is likely a relevant case feature because it increases the ideal sentence by some months or years.

A sentence is unfair to the extent it differs from the ideal sentence. Sentence unfairness is, thus, some function of the difference between the ideal sentence and the actual sentence. An analysis of the first variable in Blackmun's Thesis, fairness, thus, involves a comparison of average sentence unfairness in a specific sentencing system before and after guidelines are introduced. The pre-guideline system is fairer than the post-guideline system if it has lower average sentence unfairness.

To analyze uniformity, the second variable in Blackmun's Thesis, some concept is needed to identify cases that are similar, and thus, deserve the same treatment. A set of morally equivalent cases refers to all criminal cases that share the same set of relevant case features, and as a result, share the same ideal sentence. In a perfect criminal justice

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[6] Indeed, Blackmun does not invoke any particular theory of sentencing in *Callins v. Collins*. There, he often refers to the "fair" sentence in a particular case as the "appropriate" sentence (e.g., 1994: 1149–50). Thus, another way to frame Blackmun's Thesis is: A step towards consistency is a step away from the most appropriate sentence, *however your normative theory of punishment defines it*.

system, judges would impose the same sentence in these equivalent cases. For example, imagine that one particular set of relevant case features is: (1) a robbery, (2) with a gun, (3) that is unloaded, (4) committed by an offender who has two prior felony convictions for aggravated robbery. For this hypothetical, all crimes that involve these four relevant case features, and no other relevant case features, are morally equivalent.[7]

Clearly, however, these cases will not all receive the same sentence. Discrepancies between sentences actually received may arise from any number of sources including differences in the ideological beliefs of the judge, the competence or resources of the prosecutor, and other unrelated variables. Sentencing variation among morally equivalent cases is measured by the standard deviation of the sentences imposed in those cases. Thus, an analysis of the second variable in Blackmun's Thesis, uniformity, involves a comparison of the standard deviation of sentences from equivalent cases before and after the introduction of sentencing guidelines.

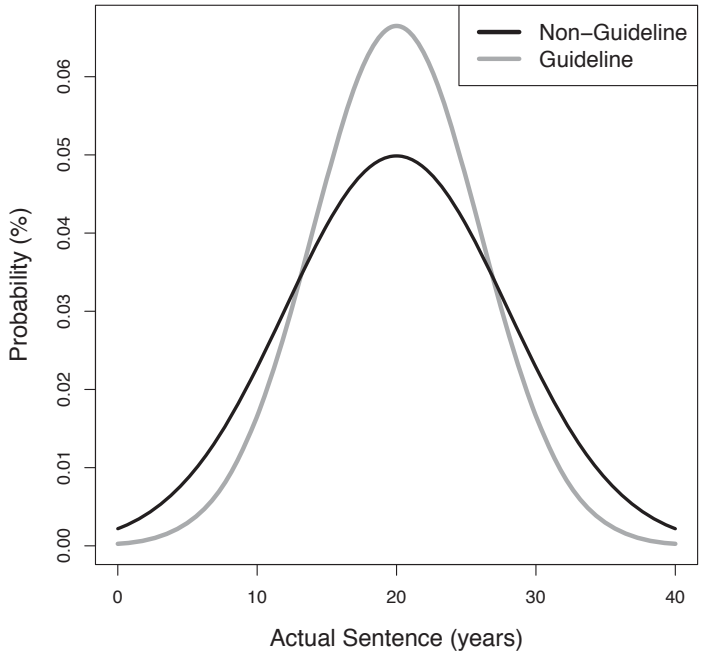
B. Defining Blackmun's Thesis

I now frame Blackmun's Thesis in the terminology defined above. The primary goal of sentencing guidelines is to increase uniformity in sentences among similar cases, that is, to decrease their standard deviation. Figure 1.1 provides an illustration of this process. Recall the unloaded gun robbery hypothetical discussed above. The black line in Figure 1.1 represents the pre-guideline sentences for these cases. The gray line represents post-guideline sentences, the sentences those cases would have received under a

[7] Importantly, a *set of morally equivalent cases* is not defined by the law in any particular jurisdiction. Rather, it is a collection of cases that would be deemed morally equivalent by an idealized or perfect criminal justice system.

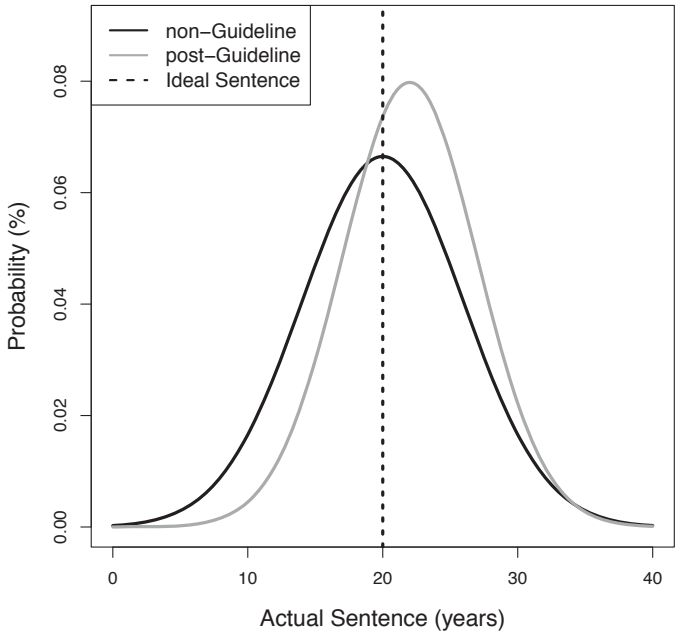
guideline system. Both distributions share the same mean, but the post-guideline sentences have a smaller standard deviation because guidelines increase uniformity.

Figure 1.1: Sentences for Equivalent Cases Before and After Guidelines



Proponents of Blackmun's Thesis assert that this reduction in the standard deviation is not the only effect of guidelines. Imposing greater uniformity, they reason, also has a bias effect. Once again, imagine that the solid black and gray lines in Figure 1.2 represent the pre-guideline and post-guideline sentences respectively. Imagine further that the ideal sentence for these hypothetical cases, represented by the dotted line, is the mean of the pre-guideline distribution. As expected, the standard deviation for the post-guideline distribution is smaller. But, the mean has also increased, representing a bias relative to the ideal sentence.

Figure 1.2: Sentences for Equivalent Cases Before and After Guidelines Increase Bias and Decrease SD



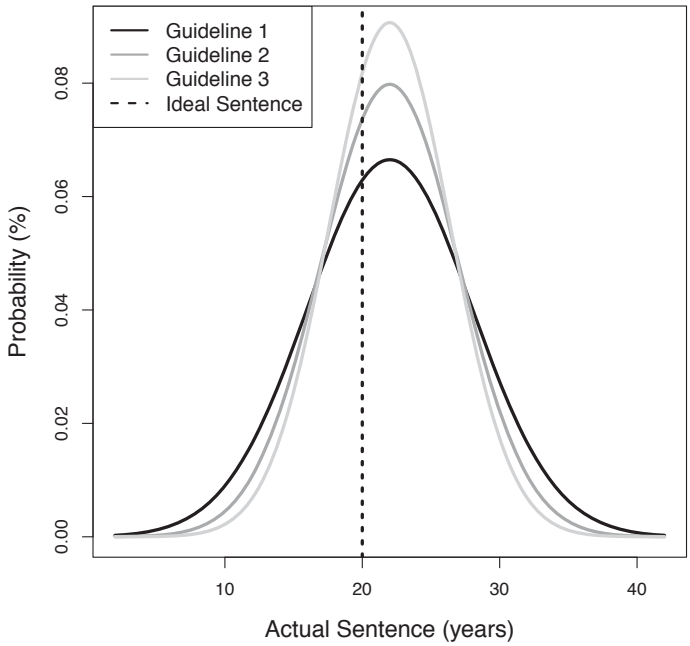
There are good reasons to expect that guidelines exert this kind of bias in the mean. Before the guidelines are enacted, judges have more discretion in identifying and weighing a wider range of case features during the sentencing process. The introduction of guidelines can bias the sentences imposed by judges by prohibiting them from considering case features they would have otherwise. To see this more clearly, suppose that pre-guidelines, most judges mitigate the sentence in our unloaded gun robbery hypothetical by roughly two years because the unloaded gun indicates diminished culpability. If the legislature adopts a guideline system that recognizes a firearm as an aggravator, but does not distinguish between loaded and unloaded firearms, then, judges can no longer mitigate the sentence. The post-guideline distribution would, as a result,

bias upwards by two years. Blackmun's Thesis asserts that, due to this bias effect, sentencing guidelines decrease fairness in individual cases on average.

There is, however, a second potential effect of guidelines on fairness not considered by proponents of Blackmun's Thesis. Increasing uniformity in sentences among equivalent cases, on average, directly increases their fairness. Figure 1.3 depicts three possible distributions of post-guideline sentences. Each distribution has a two-year bias in the mean relative to the ideal sentence, but has a different standard deviation. The black line has a standard deviation of six and represents sentences with the most unfairness. These sentences are, on average, 5 years away from the ideal sentence. The dark gray line has a standard deviation of five and is less unfair. These sentences are, on average, 4 years away from the ideal sentence. The light gray line has a standard deviation of four and is the fairest. These sentences are, on average, 3.5 years away from the ideal sentence. The implication is that increasing uniformity diminishes the average distance to the ideal sentence, and thus, increases fairness.[8] Blackmun's Thesis does not hold when the negative effects of bias are outweighed by the positive variance effects of uniformity. The remainder of this paper explores the potential relative sizes of these effects subject to a wide range of plausible conditions.

[8] Assuming that sentences are symmetrically distributed, increasing uniformity will increase fairness as long as there is at least one sentence that falls on both sides of the ideal sentence. The relationship between uniformity and fairness will hold as long as judges have not completely missed the mark.

Figure 1.3: Sentences for Equivalent Cases with Varying Levels of Uniformity



Before proceeding, two additional clarifying points are in order. First, in addition to criticizing the federal sentencing guidelines for promoting uniformity at the expense of fairness, scholars also commonly criticize guidelines for prescribing sentences that are, across the board, too severe (Stith & Cabranes 1998). This critique is distinct from Blackmun's Thesis. An increase in severity is not a necessary consequence of sentencing guidelines. Indeed, scholars have noted that many state guidelines successfully maintained historical sentencing averages (Tonry 1993). Moreover, the Federal Sentencing Commission deliberately increased sentences due to a perception that the federal courts were insufficiently punitive (Stith & Cabranes 1998). The severity of the federal system is not due to tension between uniformity and fairness in guideline systems, but rather, to a separate policy decision to increase the severity in the federal courts through guidelines and mandatory minimums.

Second, Blackmun's Thesis derives from a dissent in *Callins v. Collins*, which argued that judges must have the power to conduct individualized assessments while sentencing, particularly for capital cases. As a result, some scholars have interpreted Blackmun's Thesis to be about mercy (Garvey 1996), which is often described as a "means of ameliorating the injustice attributable to the strict application of rules" (1996: 1014). Under this interpretation, one might wonder whether Blackmun's Thesis should even acknowledge the concept of legally (or morally) equivalent cases defined by identical sets of relevant case characteristics. I think the answer is quite clearly yes. The relevance of mercy in sentencing is a controversial matter. But, if mercy does require a particular case characteristic to be considered, I surmise that Blackmun and others would agree that mercy would also require that characteristic to be considered in any other case that shares exactly the same set of relevant characteristics.

V. Analytic Method and Design

An analysis of Blackmun's Thesis involves four key parameters: (1) the magnitude of sentencing disparity among an equivalent set of cases prior to guideline enactment, (2) the effect of the guidelines on disparity, (3) the bias effect of the guidelines (i.e., effect on the average sentence length), and (4) the ideal sentence. A simulation modeling approach does not require perfect knowledge of these parameters. Indeed, they would no doubt vary by jurisdiction, time and crime category anyway. Instead, I look to the empirical literature for guidance on plausible bounds on the parameters and then test for all values between the bounds.

First, I rely on estimates from the identical case approach for guidance on the magnitude of disparity pre-guidelines. Diamond and Zeisel (1975) and Forst and Wellford (1981) observed sentence recommendations with standard deviations ranging from 30% to 64% of the mean sentence length. In the preliminary analysis below, I assume the pre-guideline disparity is 25, 50 or 75% of the average sentence length. A subsequent analysis then relaxes this assumption by testing a greater range of values including 0.

Second, I rely on the random assignment literature for guidance on the effect of sentencing guidelines on disparity because no identical case studies in the literature were conducted longitudinally. The random assignment literature observed post-enactment changes in the mean absolute deviation of judges' sentences by 0 to 26% (Anderson et al. 1999; Scott 2010; Waldfogel 1991). I, therefore, test for effects that range from 0 to 30%. Third, the empirical literature provides no guidance on the magnitude of the bias effect of sentencing guidelines. To be conservative, I assume wide bounds for bias effects that range from 0 to 50% of the average pre-guideline sentence length.

Finally, it is necessary to operationalize the ideal sentence for a set of morally equivalent cases. There is no non-arbitrary method to peg the value of the ideal sentence. Underlying Blackmun's Thesis, however, is the assumption that the pre-guideline average sentence for equivalent cases is closer to the ideal sentence than the post-guideline average. I, therefore, make the charitable assumption that pre-guidelines, judges on average get it exactly right. I assume that the ideal sentence equals the mean of the

pre-guideline distribution. Unfairness in both pre- and post-guideline sentences is measured relative to that benchmark.

A few further notes about this definition of the ideal sentence are in order. First, this specification favors Blackmun's Thesis and leads to a conservative test. The mean of the pre-guideline distribution is the value that minimizes unfairness.[9] Second, perceiving that historical sentences were insufficiently severe (or overly severe) for particular offenses, some sentencing commissions have aimed not only to decrease the standard deviation of sentences, but also to increase (or decrease) the mean. This reflects a belief that the ideal sentence is higher (or lower) than historical practice. By assuming the ideal sentence is the mean of the pre-guideline distribution, I cannot account for this kind of "prescriptive" change (Tonry 1993). But, if the sentencing commission is correct that the ideal sentence is higher (or lower) than historical practice, then, the test of Blackmun's Thesis is once again rendered more conservative because the true ideal sentence will be closer to the post-guideline distribution than the ideal sentence assumed in the analysis.

A. The Design: Statistical Simulation and Computation

This study uses statistical simulation to explore the relationship between uniformity and fairness proposed by Blackmun's Thesis.[10] I begin by examining this

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[9] It is not unreasonable to expect that the true ideal sentence is somewhere between the mean of the non-guideline and guideline distributions. If so, the argument against Blackmun's Thesis is even stronger than the evidence presented here.

[10] The methodology can be understood as a kind of Monte Carlo simulation that approximates the integral of the function of sentence unfairness (i.e., actual sentence minus some constant representing the ideal sentence) for a distribution of morally equivalent cases that is multiplied by X.

relationship for a particular pre-guideline distribution and later relax this assumption by exploring a wider range.

I start with a pre-guideline distribution with a mean of eight and a standard deviation of six. This ratio between mean and standard deviation is relatively large, but it is consistent with findings in the empirical literature. Some cases are likely subject to wide sentencing variation, particularly those involving multiple characteristics over which there is little consensus about their relevance to sentencing (e.g., young age, psychological disorders, history of family abuse, child dependents, stable employment, positive contributions to the community). The first step is to estimate unfairness in this pre-guideline distribution. A random sample of 10,000 elements is generated from a normal distribution with a mean of eight and standard deviation of six.[11] Each of the 10,000 elements represents a sentence from one of 10,000 hypothetical morally equivalent criminal cases. Because a sentence cannot take on a negative value, any of the 10,000 elements that are less than zero are set to zero.[12] Next, to calculate sentence

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[11] To address instability in some estimates, the final analysis actually draws 1,000 samples with 10,000 elements in each. This translates into an effective sample of ten million sentences per distribution.

[12] This deviation from the normal distribution is of only minor concern. Relatively few negative sentences were actually generated. Sensitivity analyses reveal that resetting those negative values to zero has little substantive effect on the findings of the study. Parallel analyses were conducted with precisely the same baseline values, but using means that were far enough from zero that no negative values were randomly generated. Little substantive change to the findings of the study were observed. Moreover, setting negative sentences to zero favors Blackmun's Thesis, and thus renders the test more conservative. Since the ideal sentence is always equal to or greater than zero, negative values are always further from the ideal sentence than zero. Thus, resetting negative values to zero increases the fairness of a distribution. As the bias in the mean of the post-guideline distribution is increased, fewer negative values are randomly generated. In contrast, the proportion of values set to zero in the pre-guideline distribution remains constant. The decreasing number of randomly generated negative values (reset to zero) in the post-guideline distribution

unfairness, the absolute value of the difference between each of the 10,000 sentences and the ideal sentence is computed. In this case, the mean of the pre-guideline distribution is eight, and so the ideal sentence is also eight. A sentence of five years, is therefore, associated with unfairness of |5 - 8| = 3 years. Finally, a summary of total sentence unfairness is calculated by computing the mean unfairness among the 10,000 sentences.

Calculating sentence unfairness for post-guideline sentences is more complicated because sentencing guidelines can have a wide range of possible effect sizes on the uniformity and bias of the distribution. Thus, all plausible effect sizes in 1% increments are estimated. First, I estimate the effect on fairness of a guideline system that exerts no change in the standard deviation or the mean. 10,000 sentences are randomly generated from a normal distribution with the same parameters as the pre-guideline distribution (i.e., mean of 8; standard deviation of 6). Of course, aside from small differences due to random chance, this post-guideline distribution will share the same level of unfairness as the pre-guideline distribution. Next, the effect on unfairness of a guideline system that biases the mean by 1% and exerts no change in the standard deviation is tested. 10,000 sentences are randomly generated from a normal distribution with a mean of 8 + (.01 * 8) = 8.08 and a standard deviation of 6. Once again, mean unfairness relative to the ideal sentence is computed. Next, the effect on fairness of a guideline system that biases the mean by 2% and exerts no change on the standard deviation is tested. 10,000 sentences

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decreases the fairness of the post-guideline distribution relative to the pre-guideline distribution, thereby, stacking the deck in favor of Blackmun's Thesis.

are randomly generated from a normal distribution with a mean of 8 + .02 * 8 = 8.16 and standard deviation of 6, and mean unfairness is computed. This process continues by 1% increments until the mean is 50% higher than its baseline value (.50 * 8 = 12). Next, I perform precisely the same procedure but with a standard deviation that is 1% lower than the pre-guideline standard deviation (6 – 6 * .01 = 5.94), and so on and so forth. This iterative procedure is repeated until mean unfairness is computed for a post-guideline distribution with a mean that is 50% larger than the baseline mean of eight (i.e., 12), and a standard deviation that is 35% smaller than the baseline standard deviation of 6 (i.e., 3.9).

At this point, we have estimates of the average unfairness resulting from a wide range of effect sizes on the mean and standard deviation of a pre-guideline distribution with a mean of eight and a standard deviation of six. For all post-guideline distributions that have lower average unfairness than the pre-guideline distribution, Blackmun's Thesis does not hold. For all post-guideline distributions, which have lower average unfairness than the pre-guideline distribution, Blackmun's Thesis does hold.

To explore how these effect sizes vary, I provide similar results for two other pre-guideline distributions with smaller standard deviations of 4 and 2. But we are not only interested in Blackmun's Thesis for three particular pre-guideline distributions. If we assume that pre-guidelines, a distribution of morally equivalent cases can have a mean between 0 and 60 years, and a standard deviation between 0 and 40, then there are 2,400 possible pre-guideline distributions. I perform the same analysis for all 2,400 pairwise combinations.

B. Assumptions of the Design

Having outlined the basic design of the study, I take this opportunity to make explicit several important methodological assumptions. The first assumption is that sentences for morally equivalent cases are distributed normally. The validity of this assumption will no doubt vary by context. Equivalent cases with average sentences that are close to zero, for example, cannot be normally distributed due to left-side censoring. As another example, it is possible that guidelines do not merely reduce the standard deviation of sentences, but also change the functional form. The normal distribution was selected, however, for a number of desirable properties.

First, unlike other probability distributions, the parameters of the normal distribution are defined by a mean and standard deviation. Thus, the parameters of the normal distribution map well onto the parameters of Blackmun's Thesis and allow for their direct manipulation.

Second, the normal distribution has relatively thin tails, meaning it generates few outliers that dominate the results. This feature is perhaps more important than the exact functional form. Indeed, the results of the analysis are not intended to be exactly right. Rather, they are intended to shed light on general trends in the tension between uniformity and fairness generated by the sentencing guideline debate. Similar analyses can, at least in principle, be conducted in future work for a wider range of distributional forms.

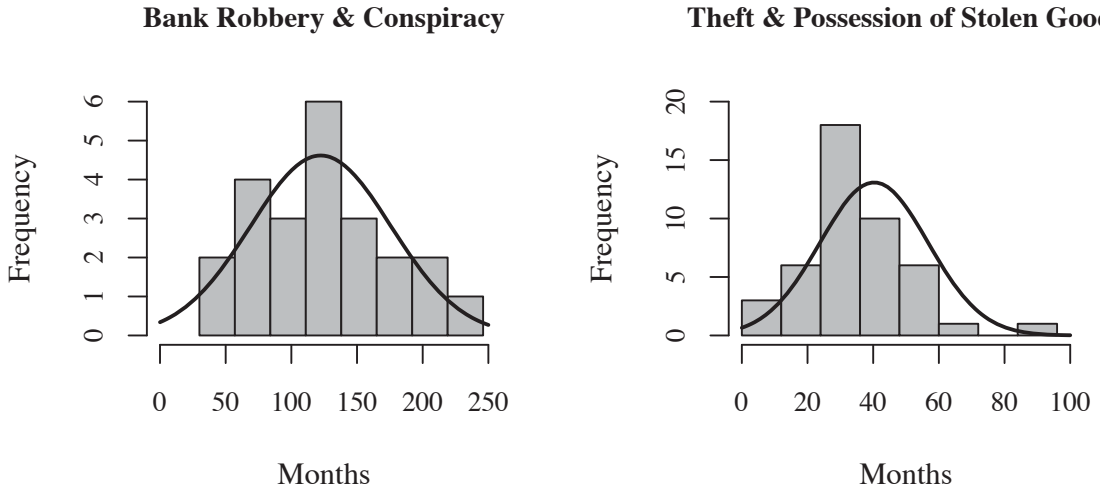
Third, normality is empirically plausible. The identical case approach likely provides the most useful evidence on this point. Yet, studies in the literature are over 30

years old, and thus, little data is still available. Fortunately, Partridge & Eldridge (1974) provide sufficient information to reconstruct some of the data from their study. The authors provided complete sentencing reports to fifty federal judges in the 1970s prior to the enactment of the federal sentencing guidelines. The left panel of Figure 1.4 depicts the sentences recommended for a male defendant with five prior convictions, two periods of incarceration and no history of drug use who was convicted at trial of four counts of bank robbery and conspiracy. While the sample size is small (n = 24), the distribution appears to approximate normality. The right panel of Figure 1.4 depicts sentences recommended for a male defendant who was convicted at trial of two counts of theft and possession of goods from an inter-state shipment, and who had no prior convictions, but did have other felony charges pending for acts committed after the instant offense. With a somewhat larger sample size (n = 45), the distribution also appears to approximate the normal distribution. Together, these distributions provide evidence that the assumption of normality is plausible for at least some sets of equivalent cases prior to the enactment of guidelines.

The second main assumption of the analysis is the conception of an ideal sentence. That conception involves three key components. First, as noted earlier, the design presupposes that every crime has an ideal sentence. Moral skeptics may wonder about this ontology, but Blackmun's Thesis itself invokes a comparison between the fairness of sentences under discretionary and guideline regimes. Such a comparison presupposes some ideal sentence against which to compare. Second, the design presupposes that the ideal sentence is a discrete point value (e.g., 9 years exactly), rather

than a range of values (e.g., 8 to 10 years). Third, the value of the ideal sentence is the mean of the pre-guideline distribution. As noted earlier, this is a charitable assumption that favors Blackmun's Thesis and renders the test more conservative.

Figure 1.4: Histograms of Sentences from Equivalent Cases[13]



Finally, we need to draw some assumptions for calculating the unfairness of deviations from the ideal sentence. My analysis begins with the assumption that unfairness is both linear and symmetric. If so, two-year and three-year deviations from the ideal sentence are twice and three times more unfair than a one-year deviation. This assumption appears plausible particularly when examining smaller margins such as a 10% or 20% change in bias. In a subsequent analysis, I relax the assumptions of linearity and symmetry. Given the difficulty of examining an unlimited number of possible non-linear and asymmetric functions, I explore several illustrative examples. First, I relax non-linearity by modeling fairness as (X^2), $(X^2)/3$ and $(X^2)/5$, where X represents the

[13] The data derive from Partridge and Eldridge (1974).

distance of a sentence from the ideal sentence. As I discuss in greater detail below, relaxing the assumption of linearity has only minor effects on the results of the analysis. Second, I also relax the assumption of symmetry by applying a different functional form for sentences below the ideal sentence than for sentences above. It is not possible to test all possible functional forms,[14] but these additional analyses provide added insight on the behavior of Blackmun's Thesis if unfairness increases at different rates above and below the ideal sentence.

VI. RESULTS AND ANALYSIS

I present my results in three stages. I begin by presenting what I refer to as conditional margin graphs. These graphs show the range of possible effects of sentencing guidelines conditional on a specific pre-guideline distribution. They provide granular information on whether Blackmun's Thesis holds subject to a wide range of plausible effects on uniformity (standard deviation) and bias (mean). I then present additional results in unconditional margin graphs that summarize the conditional margin graphs for all 2,400 possible pre-guideline distributions. Unsurprisingly, Blackmun's Thesis behaves predictably across different pre-guideline distributions. Finally, I illustrate how

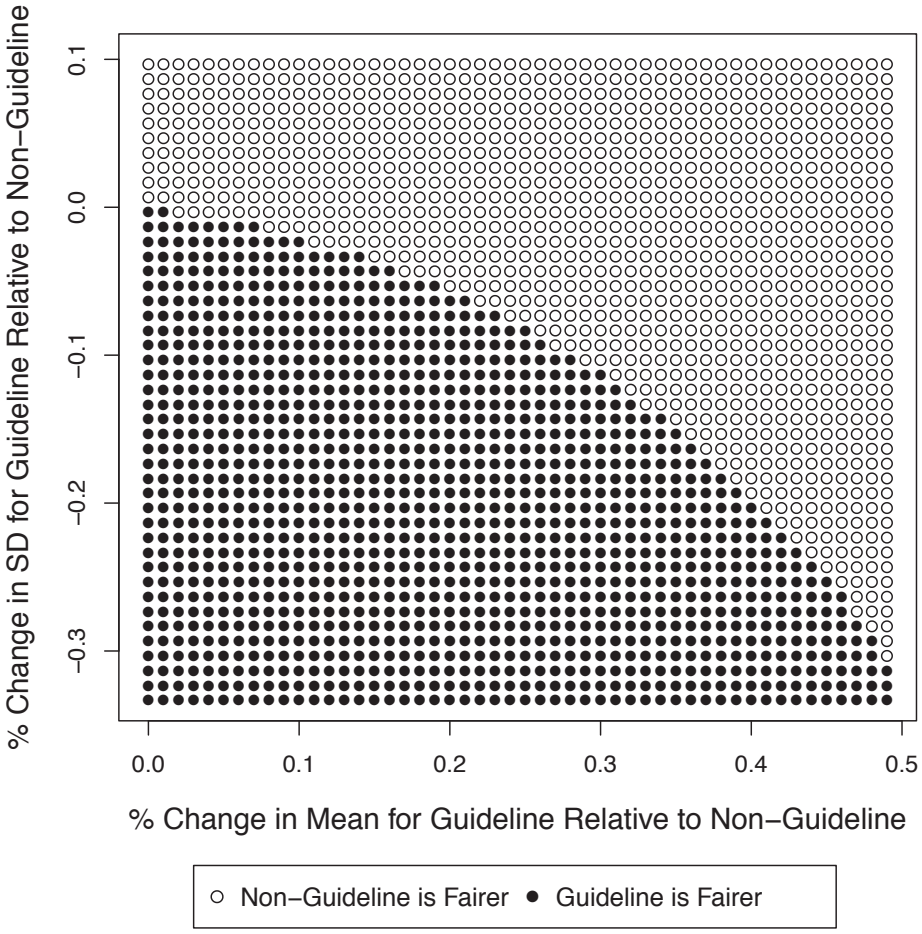
[14] For example, one reviewer noted that the pain or discomfort of an additional year of prison likely diminishes over a prisoner's tenure behind bars. As a result, a pure retributivist might believe that if a sentence over the ideal is lengthened, the marginal impact of each additional year on unfairness becomes smaller, and that as a sentence under the ideal is shortened, the marginal impact of each additional year becomes greater. The exponential functions of fairness I implement can account for the latter, but cannot account for the former. I merely note that such a nonlinear function of fairness would favor Blackmun's Thesis, and thus, render my test more conservative. As the mean of the post-guideline distribution increases, the fairness of a larger proportion of sentences in the distribution would be computed based upon the above-ideal-sentence function.

the results of the analysis are affected when we relax the assumptions of linearity and symmetry for fairness.

A. Conditional Margin Graph Analysis

Figure 1.5 is a conditional margin graph for a pre-guideline distribution with a mean of eight and a standard deviation of six. Each dot in the figure represents a comparison of fairness between the pre-guideline distribution and a post-guideline distribution with a percentage change in the mean and standard deviation. The X-axis represents a percent change in the mean relative to the value of the pre-guideline mean of 8, and the Y-axis represents a percent change in the standard deviation relative to the value of the pre-guideline standard deviation of 6. In other words, the point at 0.10 on the X-axis and -0.10 on the Y-axis represents a comparison between the pre-guideline distribution (mean= 8; SD = 6), and a post-guideline distribution upon which sentencing guidelines have exerted a 10% increase in the mean and a 10% decrease in standard deviation (mean = 8.8; SD = 5.4) relative to the pre-guideline distribution. A white dot signifies that the guidelines have decreased fairness and that Blackmun's Thesis holds. A black dot signifies that the guidelines have maintained or increased fairness overall and that Blackmun's Thesis does not hold. Accordingly, the black dot at the point (0.1, -0.1) indicates that a guideline system that decreases the standard deviation of the pre-guideline distribution by 10% will increase or maintain fairness even if the guidelines add 10% bias.

Figure 1.5: Conditional Margin Graph, Mean = 8; SD = 6
Fairness of Pre-Guideline and Post-Guideline Sentences Subject to Percentage Shifts in Mean and Standard Deviation of Post-Guideline Sentences



The implications of Figure 1.5 are striking. Point (0.28, -0.1) reveals that, for a pre-guideline distribution with mean of eight and standard deviation of six, a sentencing system that decreases the standard deviation by 10% and causes a 28% bias in the mean will increase or maintain fairness overall. At this cut point, standard deviation has a positive effect on fairness that is almost three times larger than the negative effect of bias. Similarly, point (0.4, -0.2) reveals that, a sentencing system that decreases the standard deviation by 20% and causes a 40% bias in the mean will increase or maintain fairness

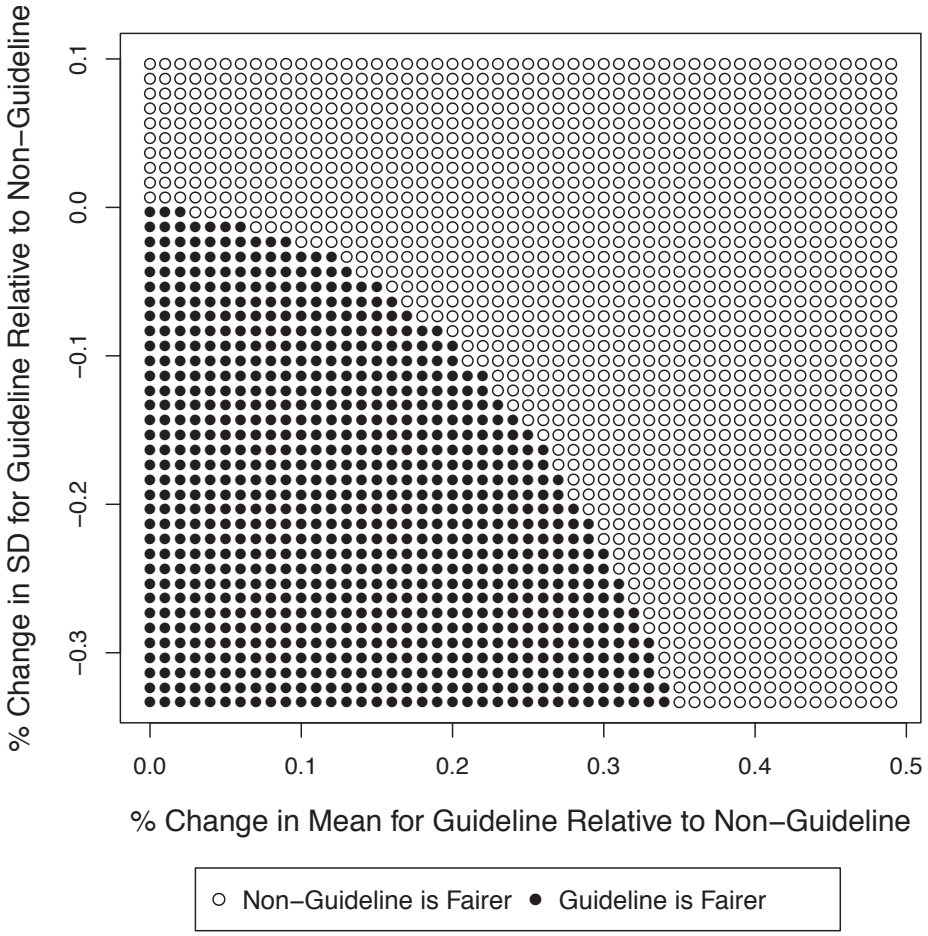
overall. At this cut point, standard deviation has a positive effect on fairness that is almost twice as large as the negative effect of bias in the mean. This is evidence against Blackmun's Thesis, as it suggests that decreases in uniformity can play a larger role in fairness than biases to the mean: even where guidelines increase the severity of an entire distribution of cases, small reductions in standard deviation may nonetheless deliver an overall increase in fairness.

Figure 1.6 is a conditional margin graph for a pre-guideline distribution with a mean of 8 and a standard deviation of 4. The implications of Figure 1.6 are similar. Point (0.2, -0.1) reveals that for this distribution a guideline system that decreases the standard deviation by 10% and causes a 20% bias in the mean will nonetheless increase fairness overall. At this cut point, standard deviation has a positive effect on fairness that is almost twice as large as the negative effect of bias in the mean. Similarly, point (0.28, -0.2) reveals that a sentencing system that decreases the standard deviation by 20% and causes a 28% bias in the mean will increase fairness overall. At this cut point, standard deviation has a positive effect on fairness that is almost 50% larger than the negative effect of bias in the mean.

Figure 1.7 is a conditional margin graph for a pre-guideline distribution with a mean of eight and standard deviation of two. Even where there is little sentencing variation to begin with, the story is similar: decreasing the standard deviation continues to have a substantial effect on fairness. Point (0.1, -0.1) reveals that, for this pre-guideline distribution, a sentencing system that decreases the standard deviation by 10% and causes a 10% bias in the mean will increase or maintain fairness overall. At this cut point,

standard deviation has a positive effect on fairness that is equal to the negative effect of bias. Point (.15, -0.2) shows that a guideline system that decreases the standard deviation by 20% will increase or maintain fairness overall, even if it also causes as large as a 15% bias in the mean. At this cut point, standard deviation has a positive effect on fairness that is 70% the size of the negative effect of bias.

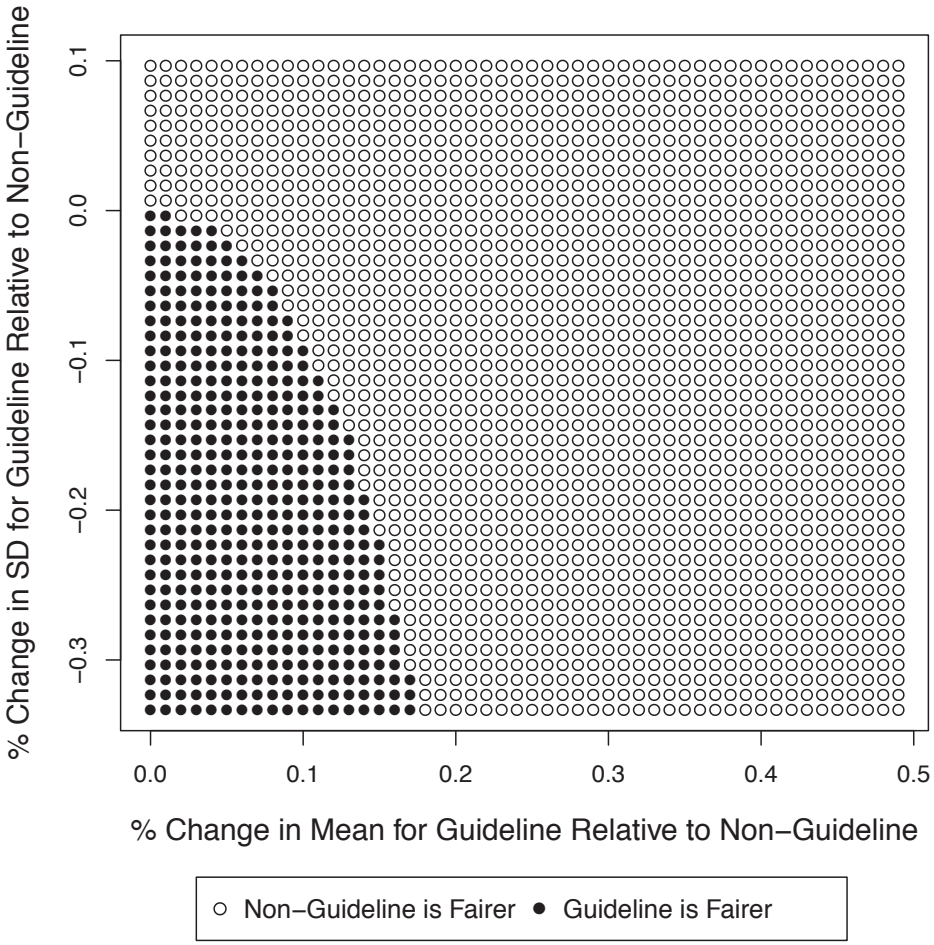
Figure 1.6: Conditional Margin Graph, Mean = 8; SD = 4
Fairness of Pre-Guideline and Post-Guideline Sentences Subject To Percentage Shifts in Mean and Standard Deviation of Post-Guideline Sentences



One clear generalization is that small reductions in the standard deviation can have a large effect on fairness when compared to the effects of bias. A second

generalization is that reductions in standard deviation have diminishing marginal returns on fairness: the first 10% reduction in standard deviation has a larger effect on fairness than a second 10%.

Figure 1.7: Conditional Margin Graph, Mean = 8; SD = 2
Fairness of Pre-Guideline and Post-Guideline Sentences Subject To Specified Percentage Shifts in Mean and Standard Deviation of Post-Guideline Sentences



B. Unconditional Margin Graph Analysis

Conditional margin graphs are "conditional" because they represent the relationship between uniformity, bias and fairness conditioned on a pre-guideline

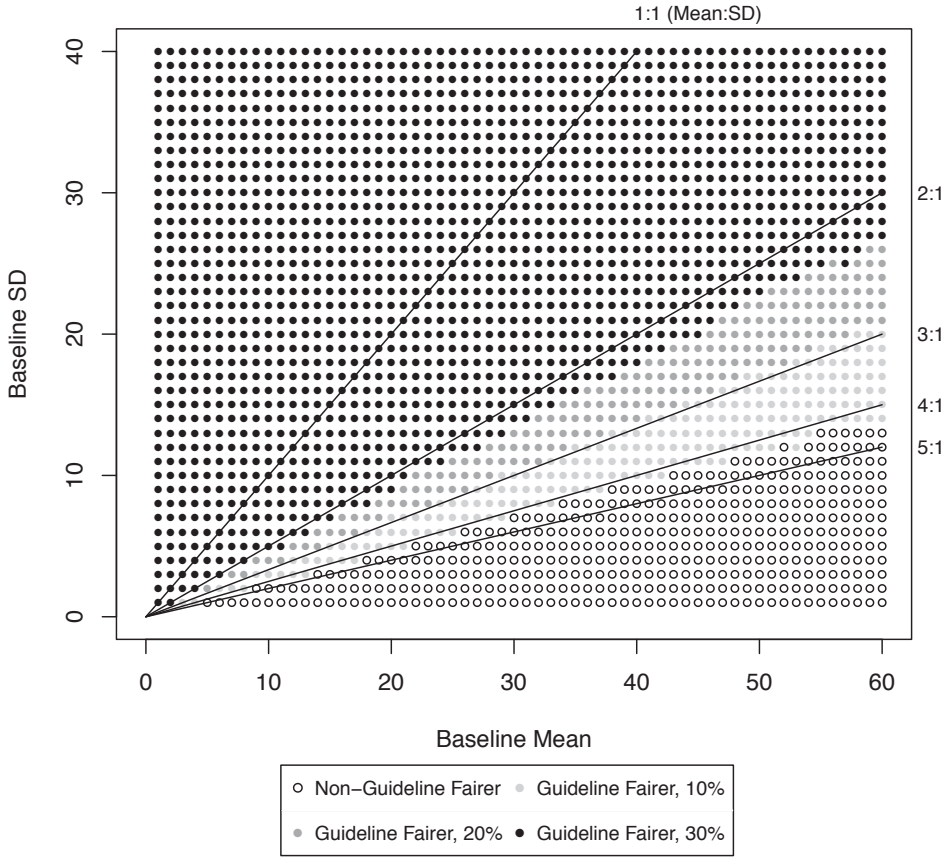
distribution with a specific mean and standard deviation. All possible 2,400 conditional margin graphs are summarized in two unconditional margin graphs.

Figure 1.8 is an unconditional margin graph that answers the question, does standard deviation or bias in the mean have a greater impact on fairness? Each of the 2,400 points in the figure represent a summary of the key findings from the possible pre-guideline distributions. For example, the point at 8 on the X-axis, and 6 on the Y-axis summarizes the findings of the conditional margin graph for a pre-guideline distribution with a mean of 8 and a standard deviation of 6. A white point indicates that a guideline system that decreases standard deviation by 10% and increases the mean by 10% would decrease fairness overall. Thus, the white point at (8, 1) indicates that, for a pre-guideline distribution with a mean of 8 and a standard deviation of 1, a guideline system that decreases the standard deviation by 10% and causes a 10% bias in the mean will decrease fairness overall. A light gray point indicates that a guideline system that decreases the standard deviation by 10% and increases the mean by 10% would increase or maintain the pre-guideline level of fairness. Thus, the light gray point at (8, 2) indicates that, for a pre-guideline distribution with a mean of 8 and standard deviation of 2, a guideline system that decreases the standard deviation by 10% and causes a 10% bias in the mean will increase or maintain fairness. A dark gray point indicates that a guideline system that decreases the standard deviation by 20% and increases the mean by 20% would increase or maintain fairness overall. Thus, the dark gray point at (8, 3) indicates that, for a pre-guideline distribution with a mean of 8 and a standard deviation of 3, a guideline system that decreases the standard deviation by 20% and causes a 20% bias in the mean will

increase or maintain fairness overall. Of course, since reductions in standard deviation have a diminishing marginal effect on fairness, all dark gray dots also satisfy the conditions for a light gray dot. Finally, a black point indicates that a guideline system that decreases the standard deviation by 30% and increases the mean by 30% would increase or maintain the pre-guideline level of fairness. Point (8, 5) is one such example. Once again, because of diminishing marginal returns, a black dot satisfies the conditions for a light gray dot and a dark gray dot.

The straight lines labeled by ratio in Figure 1.8 demonstrate that conditional margin graphs present consistent and predictable behavior. The line labeled "1:1" illustrates that any conditional margin graph associated with equal pre-guideline parameters (e.g., mean of 5, standard deviation of 5) is associated with a black dot. In other words, wherever the pre-guideline distribution has a mean and standard deviation of equal values, a 30% decrease in standard deviation and 30% bias in mean will increase or maintain the pre-guideline level of fairness. Similarly, conditional margin graphs associated with pre-guideline values with a ratio of 3:1 (e.g., mean of 6, standard deviation of 2) are associated with a light gray dot. Wherever the pre-guideline distribution has a mean and standard deviation in a ratio of 3:1, a 10% decrease in standard deviation and 10% bias in the mean will increase or maintain fairness. Finally, all conditional margin graphs associated with pre-guideline values with a ratio of 5:1 (e.g., mean of 10, standard deviation of 2) are white. Thus, wherever the pre-guideline distribution has a mean and standard deviation in a ratio of 5:1, 10% decrease in standard deviation and 10% bias in mean will decrease fairness overall.

Figure 1.8: Unconditional Margin Graph, Change in Mean and SD of 10, 20 and 30%



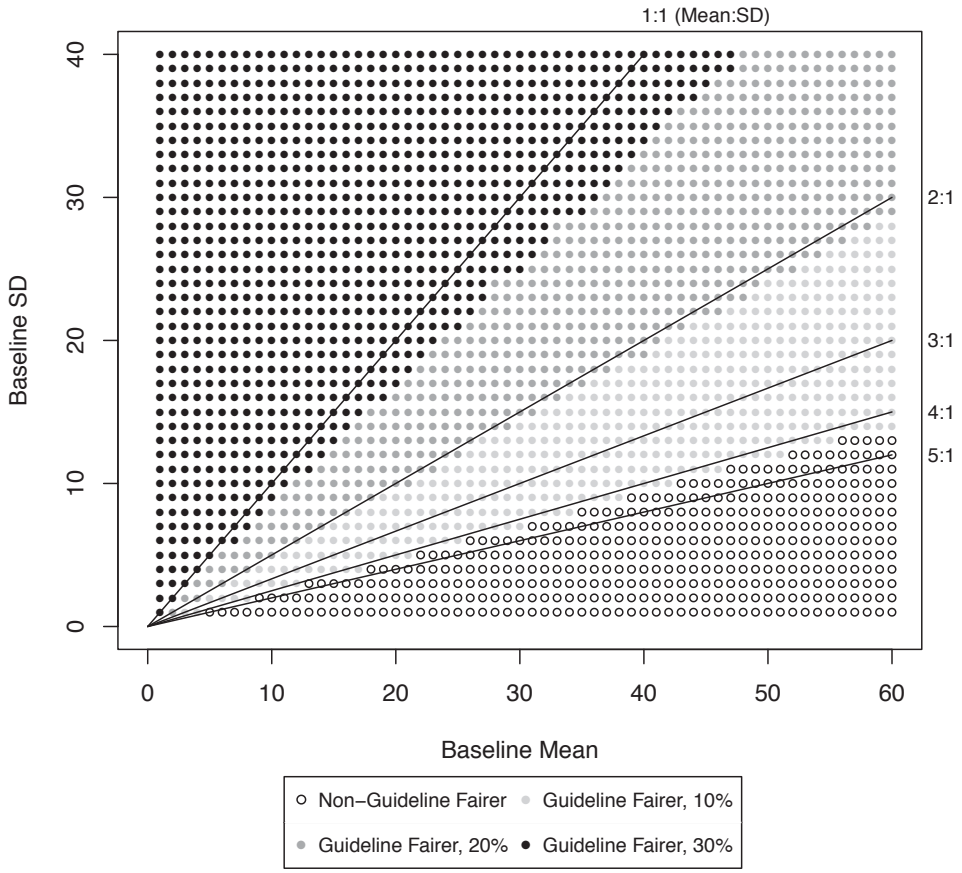
Unlike Figure 1.8, which depicts whether standard deviation or bias has a greater impact on fairness, Figure 1.9 depicts how much greater an impact standard deviation has on fairness. More specifically, Figure 1.9 indicates the level of bias needed to counteract the positive effect of a 10% decrease in standard deviation. A white point indicates that a 10% decrease in standard deviation has a smaller effect on fairness than a 10% bias in the mean. The point at (8, 1), for example, indicates that for a pre-guideline distribution with a mean of 8 and a standard deviation of one, a guideline system that decreases the standard deviation by 10%, and biases the mean by 10% decreases fairness overall. A light gray point indicates that a 10% decrease in standard deviation has an equal or larger

effect on fairness than a 10% bias in the mean. The point at (8, 2), for example, indicates that for a pre-guideline distribution with a mean of 8 and a standard deviation of 2, a guideline system that decreases the standard deviation by 10%, and biases the mean by 10% increases or maintains the pre-guideline level of fairness. A dark gray point indicates that a 10% decrease in standard deviation has an equal or larger effect on fairness than a 20% bias in the mean. Accordingly, dark gray points indicate that standard deviation has an effect on fairness that is over twice as powerful as bias in the mean. Finally, a black point indicates that a 10% decrease in standard deviation has an equal or larger effect on fairness than a 30% bias in the mean. Accordingly, black points indicate that standard deviation has an effect on fairness that is over three times as powerful as bias in the mean.

As in Figure 1.8, the findings are consistent within ratios. All pre-guideline distributions with equal mean and standard deviation values are associated with black dots, meaning that a 10% decrease in standard deviation has an effect on fairness that is equal to or greater than a shift in the mean that is three times larger. All pre-guideline distributions with mean and standard deviation values with a ratio of 2:1 are associated with dark gray dots, meaning that a 10% decrease in standard deviation has an effect on fairness that is equal to or greater than a bias effect two times larger. All pre-guideline distributions with mean and standard deviation values with a ratio of 4:1 are associated with light gray dots, meaning that a 10% decrease in standard deviation has an equal or greater effect on fairness than a comparable bias effect. All pre-guideline distributions with mean and standard deviation values with a ratio of 5:1 are associated with white

dots, meaning that a 10% decrease in standard deviation has a weaker effect on fairness than a comparable bias effect.

Figure 1.9: Unconditional Margin Graph, 10% SD Decrease & 10, 20 & 30% Mean Increase



C. Non-linear Fairness

I began with the assumption that unfairness is both linear and symmetric with respect to the ideal sentence. Here, I relax these assumptions by exploring several alternative functions of unfairness. I relax the linearity assumption by examining the behavior of Blackmun's Thesis if unfairness follows an exponential form of (X^2), $(X^2)/3$ or $(X^2)/5$ where X represents the difference between a sentence and the ideal sentence. I

then relax the symmetry assumption by allowing for different functional forms for sentences above and below the ideal sentence.

Table 1.1, Table 1.2, and Table 1.3 present the results for a pre-guideline distribution with a mean of 8 and a standard deviation of 6, 4 and 2 respectively. The left hand side of each of the tables shows the amount of bias necessary to equal the positive effects of a 10% decrease in the standard deviation after the enactment of guidelines. The right hand side represents the amount of bias necessary to equal the positive effects of a 20% decrease in standard deviation. The rows of the tables indicate the functional form of unfairness for sentences that are greater than the ideal sentence, and the columns indicate the functional form of unfairness for sentences that are less than the ideal.

The main diagonals present the results when the assumption of linearity is relaxed while maintaining symmetry. Perhaps unsurprisingly, relaxing linearity while maintaining symmetry has little impact on the results of the analysis. For example, Table 1.1 shows that a 10% decrease in standard deviation for a pre-guideline distribution with a mean of 8 and a standard deviation of 6 is equivalent to a 28% increase in bias if fairness is linear. The results are relatively stable if fairness is modeled using a different functional form (24%, 25% and 25%). Similarly, Table 1.2 shows that a 10% decrease in standard deviation for a pre-guideline distribution with a mean of 8 and a standard deviation of 4 is equivalent to a 20% increase in bias if fairness is linaer. Again, the results are nearly identical for other functional forms (19%, 20% and 21%).

Table 1.1: Increase in Bias Equal to 10% and 20% Decrease in SD (Mean 8; SD 6)

10 Percent Reduction in SD

		Under Ideal			
		Linear	**$X^2/5$**	**$X^2/3$**	**X^2**
Over Ideal	**Linear**	28%	43%	77%	190%
	$X^2/5$	18%	24%	39%	92%
	$X^2/3$	13%	16%	25%	63%
	X^2	11%	11%	13%	25%

20 Percent Reduction in SD

		Under Ideal			
		Linear	**$X^2/5$**	**$X^2/3$**	**X^2**
Over Ideal	**Linear**	40%	60%	84%	210%
	$X^2/5$	31%	37%	56%	99%
	$X^2/3$	25%	30%	37%	75%
	X^2	21%	21%	24%	38%

Table 1.2: Increase in Bias Equal to 10% and 20% Decrease in SD (Mean 8; SD 4)

10 Percent Reduction in SD

		Under Ideal			
		Linear	**$X^2/5$**	**$X^2/3$**	**X^2**
Over Ideal	**Linear**	20%	23%	42%	113%
	$X^2/5$	18%	19%	32%	69%
	$X^2/3$	12%	13%	20%	49%
	X^2	8%	8%	10%	20%

20 Percent Reduction in SD

		Under Ideal			
		Linear	**$X^2/5$**	**$X^2/3$**	**X^2**
Over Ideal	**Linear**	28%	31%	46%	115%
	$X^2/5$	26%	30%	38%	72%
	$X^2/3$	21%	21%	28%	56%
	X^2	13%	15%	18%	28%

Table 1.3: Increase in Bias Equal to 10% and 20% Decrease in SD (Mean 8; SD 2)

10 Percent Reduction in SD

		Under Ideal			
		Linear	**$X^2/5$**	**$X^2/3$**	**X^2**
Over Ideal	**Linear**	10%	7%	10%	33%
	$X^2/5$	18%	10%	17%	36%
	$X^2/3$	10%	7%	11%	26%
	X^2	5%	5%	5%	11%

20 Percent Reduction in SD

		Under Ideal			
		Linear	**$X^2/5$**	**$X^2/3$**	**X^2**
Over Ideal	**Linear**	15%	9%	13%	34%
	$X^2/5$	21%	14%	20%	37%
	$X^2/3$	16%	11%	14%	28%
	X^2	10%	8%	9%	14%

The results change more substantially when the symmetry assumption is also relaxed. The first column in each table provides the results when the fairness of sentences below the ideal is modeled linearly, but the functional form of unfairness above the ideal is varied. Here we can observe the relative magnitude of the variance and bias effects when the unfairness of sentences increases more rapidly for sentences above the ideal sentence. When the fairness of sentences over the ideal sentence follows the exponential function $(X^2)/5$ rather than a linear function, the impact of the standard deviation is

smaller. As Table 1.1 reveals, for a pre-guideline distribution with a mean of 8 and a standard deviation of 6, a 10% reduction in standard deviation has an equivalent effect on fairness of an 18% increase in bias. And, a 20% reduction in standard deviation has an equivalent effect on fairness of a 31% increase in bias. The variance effect is smaller when the fairness of sentences above the ideal is modeled as $(X^2)/3$. A 10% reduction in standard deviation is equivalent to a 13% increase in bias, and a 20% reduction in standard deviation is equivalent to a 25% increase in bias. Again, the effect is smaller when the fairness of sentences above the ideal sentence is modeled as (X^2). Here, a 10% decrease in standard deviation is equivalent to an 11% decrease in bias, and a 20% decrease in standard deviation is equivalent to a 21% increase in bias.

A similar pattern is observed in Table 1.3, and consistent with earlier resuts, the smallest variance effect is present in Table 1.3 for a pre-guideline distribution with a standard deviation of 2. Under the assumptions most favorable to Blackmun's Thesis—where unfairness for sentences above the ideal is modeled as (X^2) and the fairness of sentences below the ideal is modeled as linear—a 10 and 20% reduction in the standard deviation is equivalent to a 5 and 10% increase in the bias. To summarize, while the variance effect decreases under these assumptions of asymmetry and non-linearity, it is remarkable that the effects remain substantial in size given how much more rapidly unfairness is assumed to increase for sentences above the ideal (X^2) than for sentences below (linear).[15]

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[15] For example, a sentence of 13 years and a sentence of 3 years are both 5 years away from the ideal sentence of 8 years. If we assume that the fairness of sentences below the ideal follow a

The reverse pattern is observed for the relative magnitude of the variance and bias effects if the functional form for the fairness of sentences above the ideal sentence is held constant, while the form for the fairness of sentences below is varied. The first row in each table provides the results when the fairness of sentences above the ideal sentence is modeled linearly, but the functional form of unfairness below the ideal is varied. Across most rows in the tables above, the magnitude of the variance effect increases substantially.[16]

In summary, fairness may not always follow a linear or symmetric pattern above and below the ideal sentence. It is impossible, however, to examine all plausible non-linear functional forms of fairness. This section has attempted to illustrate how the primary results of the analysis may be affected when the assumptions of linearity and symmetry are relaxed. The results provide little evidence that relaxing the linearity assumption has a substantial effect on the results unless the symmetry assumption is also relaxed. Under the assumption of asymmetry, the magnitude of the variance effect relative to the bias effect may shrink but it remains substantial and substantively important. This is true even if the functional definition of fairness strongly favors Blackmun's Thesis, as it does when the fairness of sentences above the ideal is modeled as X^2, and the fairness of sentences below the ideal is modeled as linear.

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linear function, and the fairness of sentences above the ideal follow the exponential function X^2 then a 3-year sentence has an unfairness of 5 while a 13-year sentence has an unfairness of 25.

[16] The few exceptions are in Table 3 due to the substantial proportion of sentences that have a numeric value of unfairness that is between -1 and 1. In this small range of values, the exponential function is less steep than the linear function.

VII. DISCUSSION

This paper set out to critically examine Blackmun's Thesis, a widely expressed view among legal scholars and practitioners that increasing uniformity in sentencing through guidelines decreases the fairness of sentences in individual cases on average. To avoid the need to draw thick normative assumptions about morally equivalent cases and ideal sentences, and to overcome limitations in the availability of baseline data collected through the individual case approach, the paper did not use data on a specific guideline enactment. Instead, it took a more general approach by estimating plausible bounds of the relevant parameters and by exploring the effects for all possible values between these bounds. The paper also made some simplifying assumptions about the distribution of sentences from morally equivalent cases. It assumed, for example, that these distributions are normally distributed.

The quantitative analysis revealed that increasing sentencing uniformity (i.e., decreasing standard deviation) through guidelines can have a dramatic positive effect on fairness and that this effect may often outweigh the negative effects of bias in the mean. The analysis began with the assumption that the fairness of sentences is linear and symmetric with respect to the ideal sentence. For morally equivalent cases with larger standard deviations (mean to standard deviation ratio of 4:3), increases in uniformity under these assumptions can have a two or three times greater positive effect on fairness than the negative effect of bias when fairness is modeled linearly. Similar results were observed for cases with more moderate standard deviations (2:1). Reductions in standard deviation have a less impressive impact on fairness for pre-guideline distributions in

which the standard deviation is low relative to the mean (i.e., ratio of 4:1 or 5:1). However, where the mean to standard deviation ratio is 4:1, each percentage change in the mean represents a dramatically larger shift than each percentage shift in the standard deviation: where the pre-guideline distribution has a mean of 8 and a standard deviation of 2, a 10% change in the mean is 0.8, while a 10% change in the standard deviation is just 0.2. It is remarkable that even in this circumstance, where the bias in the mean is four times larger than the change in standard deviation, we still observe that a 10% decrease in standard deviation has a greater or equal effect on fairness than a 10% bias in the mean.

In a subsequent analysis, I relaxed the assumptions that the fairness of sentences follows a linear and symmetric function. Given the difficulty of examining the unlimited number of possible non-linear and asymmetric functions, I explored several illustrative examples. First, I relaxed non-linearity by modeling fairness according to several different exponential functions. This had only minor effects on the results. Second, I also relaxed the assumption of symmetry by applying a different functional form for sentences below the ideal sentence than for sentences above. The magnitude of the variance effect relative to the bias effect decreased when the slope of unfairness steepened more rapidly for sentences above the ideal sentence, but its size remained substantial and of continued substantive importance. This finding was observed even when functional forms of fairness were applied that strongly favored Blackmun's Thesis (X^2 for sentences above the ideal sentence, and a linear function for sentences below). The reverse pattern was observed when the slope of unfairness steepened more rapidly for sentences below the ideal sentence. Under these assumptions, the variance effect grew substantially.

Ultimately, the simulation models illustrate that decreasing the standard deviation of sentences from morally equivalent cases through sentencing guidelines may have large positive impacts on fairness that outweigh the negative effects of bias even when model assumptions favor Blackmun's Thesis. This is an important insight because it suggests that even where guideline systems increase (or decrease) the severity of sentences for an entire distribution of cases, modest increases in uniformity can yield a net increase in fairness. This bolsters the view that carefully developed guidelines likely increase rather than decrease sentence fairness on average in individual cases by increasing uniformity. In turn, this provides significant evidence against Blackmun's Thesis.

The results of the study have two general policy implications. First, the dramatic effect on fairness resulting from decreases in the standard deviation suggest that legislators and sentencing commissions can be less concerned about the potential to produce unfairness by constraining judicial discretion through robust sentencing guidelines. Second, the common legislative practice of enacting comprehensive guidelines that cover all criminal offenses, such as the Federal Sentencing Guidelines, may be misguided. Some crime categories are likely characterized by low levels of sentencing disparity. Attempts to decrease sentencing variation among those cases will have only small positive effects on fairness that will more likely be outweighed by the bias effect. Sentencing commissions should collect data through the identical case approach in order to identify offense types or case features associated with high levels of disparity. They should, then, develop guidelines that focus on those cases.

Chapter 2. The Role of Prosecutorial Screening in Police Charge Decisions

Ben Grunwald Charles Loeffler

Abstract

For nearly a century scholars have observed the highly discretionary nature of police work. Police departments and policymakers have implemented a range of mechanisms to regulate this discretion but much of the scholarly literature has expressed skepticism about their effectiveness. One regulatory approach has largely escaped scrutiny—prosecutorial screening. This study examines the effect of prosecutorial screening on police charge decisions by exploiting a sharp policy discontinuity in the Felony Review Unit of the Cook County State's Attorney's Office: charge screening only takes place for suspects who are seventeen years of age or older. We compare suspects arrested just a few weeks before and a few weeks after their seventeenth birthday. In general, we find large and sharp drops in the number of felony arrests just after the age boundary that range in size from 15 to 50%. After conducting a series of tests to rule out alternative explanations, we conclude that the drop in felony charges at the age of majority is likely the result of Felony Review. Officers appear to file lesser charges against those over seventeen years of age in anticipation of the screening process.

I. INTRODUCTION

Scholars have observed the discretionary nature of police work for over half a century (NAS 2004; Lipsky 2010; Bittner 1970). Police officers must regularly decide whom to stop, whom to arrest, what to charge, and whether to use force. A wide range of mechanisms have been considered by scholars and policymakers to encourage officers to make these decisions in accordance with policy goals and the law (Punch 1983; NAS 2004: 2). Police departments have implemented service academies, in-service training, recruiting requirements, complaint reviews, and administrative rules (Davis 1974; Amsterdam 1970; Locke 1967; Fyfe 1979). Policymakers have also regulated from the outside with legislative enactments, judicial supervision, and civilian oversight (Walker 2005; Williams 1983).

Recent systematic reviews have found few empirical studies on the effectiveness of these strategies for regulating arrest and charge decisions (Mastrofski 2004; NAS 2004). And with a few exceptions, much of the scholarly literature has expressed skepticism about their impact (e.g., Livingston 2004; Walker 1993; LaFave 1990). First, scholars have questioned the commitment of many departments to control police discretion in the face of other competing priorities (Walker 2012; Mastrofski & Rosenbaum 2011). Second, scholars have argued that preventative forms of regulation rely on administrative and judicial rules that are too vague in many police-citizen interactions (LaFave 1990; Walker 1993). And third, they have argued that regulatory strategies using corrective sanctions—such as internal complaint reviews and judicial

oversight—are too infrequent, inadequate and delayed to make a meaningful impact (Livingston 2004; Walker 2001; LaFave 1990).

Despite the variety of existing regulatory tools, one approach has largely escaped scrutiny—prosecutorial screening. Unlike other methods of regulation, prosecutorial screening can shift officers' incentives by controlling the kinds of cases that are ultimately prosecuted. If this legal authority is used immediately after a suspect is arrested, but before final charges are filed, prosecutorial screening can operate as a frequent and proximal feedback mechanism, informing officers of the level of evidence required to pursue further action, and of undesirable police conduct that inhibits successful prosecution (e.g., constitutional violations). Of course, establishing appropriate screening standards is a difficult normative challenge. Standards that are too high will discourage warranted arrests and well supported charges. Standards that are too low, on the other hand, may encourage unwarranted arrests and unsupported charges.

To date, no empirical study has examined the effect of prosecutorial screening on police discretion. The current study attempts to fill this gap by exploring the relationship between screening and arrest and charging practices. In Chicago, IL, the Felony Review division in the State's Attorney's Office screens all non-drug felony charges against adults. With only a few exceptions, felony review does not apply to juveniles, those under seventeen years of age.[17] This sharp policy discontinuity at the age of majority offers a window to examine the effect of prosecutorial screening on police arrest and

[17] During the study period, Illinois law defined an adult as any suspect who was seventeen years of age or older. In 2010, the Illinois legislature made some changes to this rule for misdemeanors offenses. All data in the current study precede this policy change.

charging practices. In particular, we compare suspects arrested just a few weeks before and a few weeks after their seventeenth birthday. In general, we find large and sharp drops in the number of felony arrests just after the age boundary, which range in size from 15 to 50%.

As a preliminary matter, the drop in felony arrests at the age of majority is consistent with a substantial effect of felony review on police charging practices. But it is also consistent with a number of alternative theoretical explanations. We, therefore, conduct a series of empirical tests of the frequency and characteristics of suspects arrested just before and after the age of majority to adjudicate between the following four most plausible theoretical explanations. First, the perceived severity of the adult criminal justice system may incentivize adolescents to engage in fewer serious crimes after their seventeenth birthday (Lane et al. 2002; Glassner et al. 1983). Second, the perceived severity of the adult system may incentivize police officers to protect individuals who are just over seventeen by arresting them less frequently. Third, the perceived leniency of the juvenile system may incentivize officers to charge adolescents just under seventeen more frequently with felonies to ensure that they receive a significant sanction as juveniles. Finally, some other procedural differences between the juvenile and adult systems, such as felony review, may differentially incentivize officers to arrest or charge suspects with felonies after their seventeenth birthday. Ultimately, we find significant evidence that the drop in felony arrests at the age of majority is primarily driven by Felony Review. Officers appear to file lesser charges against adults in anticipation of the screening process.

Our findings have important implications for at least three areas of the legal and criminological literature. First, the results expand our understanding of the effect of prosecutorial screening on the composition of cases prosecuted in the criminal justice system. We know remarkably little about the effects of screening on case processing. Prior studies in this area have focused on declination rates—the proportion of cases declined by the prosecutor's office (e.g., Frase 1980; Brosi 1979; Jacoby et al. 1982; BJS 2005; Neubauer 1974).[18] But as Wright and Miller have noted: "The number of cases declined depends on the number and quality of cases that the police . . . recommend. If the police anticipate the requirements of the prosecutor, they might recommend fewer cases after a change in screening policies, leaving the declination rate unchanged" (2002: 74). Furthermore, prior studies examining prosecutorial screening have used informal cross-jurisdictional comparisons or pre-post-designs that cannot control for secular trends in the criminal justice system (e.g., Brosi 1979; Wright & Miller 2002). The current study adds to this literature by providing evidence that prosecutorial screening substantially reduces the number of felony charges brought by the police. It does so by comparing charges against two similar groups of arrestees who are just a few weeks younger and older than the age of majority.

Second, our results suggest that prosecutorial screening may serve as a regulatory mechanism for police. Importantly, this finding is limited to charging, and does not extend to the decision to arrest. Nevertheless, we consider this a useful example of how

[18] One exception is Wright & Miller (2002), which marshals evidence that stringent prosecutorial screening enabled New Orleans to decrease charge bargaining.

an important form of police decision-making can be regulated by a proximate and frequent screening process—in this instance prosecutorial screening. To our knowledge, this paper provides the first empirical evidence of such a mechanism. Our findings are particularly notable because of the small number of empirical evaluations of more traditional methods of police regulation and because of an extensive theoretical literature that is skeptical of their impact on police decision making.

Third, our results provide systematic empirical evidence of substantial age-related charging disparities. A few important data limitations restrict our ability to explore the disparity in great depth. We cannot determine whether the disparity represents overcharging of juveniles or undercharging of adults. This normative question cannot be resolved without additional information about the quality of evidence in each case. Moreover, we lack data on downstream phases in the criminal justice system, and thus, cannot determine whether differential charging produces differential outcomes later on. Subsequent phases in the criminal justice system offer opportunities for officials to screen out inappropriate charges for juveniles and amend charges for adults. Still, our results provide evidence of a potentially important disparity that appears to arise from structural differences between the juvenile and adult justice systems.

The remainder of the paper is organized as follows. Section II discusses the traditional instruments of police regulation and argues that prosecutorial screening provides some strategic advantages. Section III reviews the literature on offender and police behavior at the age of majority. Section IV outlines in greater detail the four possible theories explaining the drop in felony arrests. Section V describes the study

setting and data. Section VI provides the central results of our analysis, and VII explores their substantive and theoretical implications.

II. Regulating Police Discretion

The wide discretion held by police officers derives from at least two features of their work. First, officers spend much of their time on the street without direct supervision. Second, officers have wide legal powers to decide whether to stop, investigate, arrest, pursue and use force against criminal suspects. Discretion is essential for effective policing (NAS 2004), and in recent years, legal scholars and social scientists alike have advocated for an expansion in police discretion to promote community-oriented policing (see Cole 1999). Yet, wide leeway also allows officers to exercise discretion in ways that conflict with departmental policy, public interest and the law.

Police departments and policymakers have implemented a range of methods to regulate police discretion. Departments use police academy and in-service training, internal administrative rules, internal investigations of citizen complaints, and formal sanctions. Policymakers have also regulated police discretion from the outside, with legislative enactments, judicial rules, and citizen review boards. The academic literature contains little empirical evidence on the effectiveness of these policies (see NAS 2004; Mastrofski 2004; Walker 2006). The few existing studies rely on limited correlational designs and problematic measures of police conduct (see Mastrofski 2004). A substantial academic literature has expressed skepticism about whether traditional regulatory mechanisms exert meaningful control.

In this section, we review the academic literature on police regulation. While some of the regulatory devices we discuss are not designed to influence arrest or charge decisions specifically, we draw on all such devices to consider structural features that limit police regulation more generally. We then propose that prosecutorial screening avoids several of these key limitations.

A. Traditional Methods of Police Regulation

Our discussion of the limits of traditional police regulation focuses on three key dimensions. First, prior work has emphasized whether the agency administering the regulatory policy is internal or external to the police department (e.g., Walker 1993; Human Rights Watch 1998; Christopher Commission 1991). Second, it has emphasized whether the regulation is preventative or corrective (e.g., Livingston 2004; Walker 2005). And third, it has emphasized the distinction between proximal and distal regulation (Amsterdam 1970). We consider the strengths and limitations that arise from each of these characteristics in turn.

1. Internal Versus External Sources of Regulation

Prior work has emphasized whether the agency administering a regulatory policy is internal or external to the police department (e.g., NAS 2004; Walker 1993; Christopher Commission 1991). The relevance of this dimension varies by context. In general, scholars have noted that police administrators are more effective at regulating line officer discretion than external agencies such as the courts (e.g., NAS 2004). Police sergeants and managers can exercise supervisory powers over line officers on a daily basis. Indeed, early qualitative scholarship concluded that sergeants have significant

influence on the behavior of their line officers (see Engel 2000). More recent quantitative research, however, has found little consistent evidence of such effects (see Engel 2000; Johnson & Billings 2010). Internal administrators can also formulate regulatory policies based on their substantive expertise in policing (McGowan 1972; Amsterdam 1974). Police departments are well positioned, for example, to teach technical policing skills (Bradford & Pynes 1999) and to craft administrative rules that are tailored to the realities of police work (LaFave 1990; Fyfe 1979). In contrast, external regulations created by legislatures and courts can be limited by a lack of familiarity with the particularities of everyday policing (Stoughton 2013; Williams 1983).

In some contexts, however, internal regulatory policies are less effective, especially where they are designed to foster systematic institutional change. Police leadership may not view certain changes as positive (Mastrofksi & Rosenbaum 2011; Human Rights Watch 1998).[19] Even where leadership backs a particular policy, there may be insufficient bureaucratic support from lower ranking officers to facilitate the transition. Internal reform efforts may also be hampered by civil service laws and collective bargaining agreements that restrict the hiring and termination decisions of the department (Harmon 2012).

Scholars have argued that in the absence of external pressure from the courts, many departments have mustered little internal momentum to establish (LaFave 1990) or enforce (NAS 2004) concrete administrative rules. No systematic reviews have recently

[19] Civil litigants and advocacy groups, for example, have tried to use the courts to overcome a strong commitment to stop-and-frisk practices by police leadership.

examined administrative rules for arrest and charge decisions, but older scholarship suggests that with the exception of mandatory arrest policies in domestic violence cases, these decisions remain "essentially ungoverned" (Walker 1993: 41). Police procedure manuals typically focus on techniques for stopping and arresting suspects, but they neglect substantive decision making (Walker 1993).

Similarly, efforts to change cultural norms in police departments (e.g., authoritarianism, sensitivity to diversity, community oriented policing) through police academy and in-service training appear limited in effect. This kind of institutional change likely requires strong support from department leadership. The most recent studies, however, suggest that many police academies have been slow to integrate significant cognitive, communication, ethics and diversity training into their curricula (Chappell 2008; Bradford & Pynes 1999; Marion 1998). Even with the support of leadership, internal training remains a difficult method for changing institutional norms without the commitment of lower-level officers who are needed to instruct the program (Moskos 2008; Prokos & Padavic 2002; Marion 1998). Some work has found that police attitudes and values about community relations may improve after family crisis or community-oriented policing training, but the effects are small (Quinet et al. 2003; Haar 2001; Buchanan 1985; Rosenbaum 1987).

Internal investigations of police misconduct appear to suffer from a related problem. Prior studies show that officers in some jurisdictions may discourage witnesses from filing complaints through intimidation and threats (Walker 2001) or simply refuse to record complaints at all (Human Rights Watch 1998). They also find that internal affairs

divisions may conduct only haphazard investigations (Prenzler 2009). In the small number of cases where a complaint is investigated and sustained, the accused officer sometimes receives no sanction at all (Human Rights Watch 1998; Christopher Commission 1991).

Due to problems associated with internal regulation, some have argued that "[e]xternal pressures are essential to force police administrators to improve accountability." (Human Rights Watch 1998: 61). Citizen oversight agencies, for example, were established to address problems in the internal operations of the traditional complaint review system (Perez 1994; Wickersham Commission 1933). Today, civilian organizations engage in two main kinds of activities. First, many citizen oversight agencies receive, investigate and dispose of citizen complaints. Second, some agencies monitor internal police processes and review administrative policy. No empirical studies have examined the impact of monitoring on police practices (Walker 2006), but qualitative assessments have found that oversight may increase accountability by shedding public light on hidden organizational processes (Walker 2001).

Importantly, regulatory oversight by an external agency does not guarantee effective regulation. Indeed, some external agencies lack sufficient independence or authority to exert control. As Walker (2001) has argued, for example, a civilian review board that lacks investigative powers, that "cannot require officer testimony and obtain all other facts relevant to an alleged incident, . . . that does not have the power to make binding determinations" may conduct oversight that is only superficially independent (2001: 78–79). Scholars have observed a similar problem in criminal prosecution of

police misconduct. Police and prosecutors work closely to investigate and process criminal cases. Prosecuting police officers can thus create "an impossible conflict of interest" (Hughes 2001: 241–42; Walker & Macdonald 2009).

2. Prospective Versus Responsive Regulation

Prior work has also emphasized whether police regulation is prospective or corrective (e.g., Livingston 2004; Walker 2005). Prospective regulation takes place *ex ante*, by anticipating that an officer will face a certain situation and by providing general guidance on the proper exercise of discretion. Police academy training and administrative, judicial and legislative rules are all common forms of prospective regulation.

Empirical research has identified limited evidence that some prospective forms of regulation may help influence police discretion. Scholars have observed, for example, that mandatory arrest policies are correlated with higher arrest rates (Eitle 2005; Lawrenz et al. 1989), though low levels of total compliance may remain (e.g., Ferraro 1989). Certain Supreme Court rulings—such as the requirement to provide a Miranda warning when arresting a suspect—have also clearly impacted police practice (Leo 1998; Tennenbaum 1994), even if officers sometimes attempt to circumvent the law (see NAS 2004).[20] And studies have observed reductions in police shootings after the enactment of administrative lethal-use-of-force policies (White 2001; Sparger & Giacopassi 1992; Fyfe

[20] In a limited sense, *Miranda v. Arizona* (1966) and other cases can be viewed as responsive regulation because the court responds to police misconduct in a particular case and may provide a remedy to the defendant. We view the prospective force of *Miranda* and other cases as more important as it regulates all future criminal cases.

1979). The designs of these studies provide only weak evidence of causal effects, but they suggest the possibility that prospective regulation may make a difference in contexts where mandatory rules are appropriate.

Scholars have emphasized three general limitations of prospective regulation. First, these policies, by necessity, regulate through abstraction, hypotheticals and general principles. To have any effect, officers must take the rules into consideration while making decisions, and it is not clear they always do (Moskos 2008; Cordner 1989). Even when officers consider the applicable rule, reasonable people can disagree on how the facts apply, especially in high-stress situations where rules represent a complex tradeoff of different values (Walker 1993). Second, prospective administrative, judicial and legislative rules are often too ambiguous to provide clear guidance in particular cases (Miller 2006; LaFave 1990; Williams 1983; Walker 2005). Third, legislative and judicial rules are often crafted at the state- or national-level without consideration for the conditions of local criminal justice systems (e.g., Sherman et al. 1992).

In contrast with prospective regulation, corrective regulation takes place *ex post*, and serves as a kind of feedback mechanism that informs a specific officer of the validity of a prior decision based upon specific factual circumstances. In many cases, this feedback is reinforced with a sanction. Corrective regulation may also indirectly influence other officers who are aware of the facts of the case and of the resulting sanction. Corrective police regulation is conducted by a number of different organizational actors, including internal affairs divisions within the police department, external civilian review boards, and the courts.

Scholars have emphasized two main limitations of corrective regulatory strategies. First, there are significant investigative challenges in resolving allegations of police misconduct after the fact. Allegations tend to arise in one-on-one confrontations, which neither produce physical evidence nor involve other witnesses (Livingston 2004). When in conflict, a police officer's testimony is usually given greater weight than the complainant's (Walker 2001). As a result, there is great ambiguity regarding the facts of most citizen complaints. Internal affairs divisions, external civilian review boards, and the courts all have similarly low rates of sustaining complaints against officers (Livingston 2004; Walker 2001).

Second, even if a review board or court sustains an allegation, the remedy is often mild. Officers sanctioned by internal affairs or external civilian boards may receive little or no punishment at all (Walker 2001). Similarly, the courts have little power to sanction police officers. In criminal cases, the only remedy is the exclusionary rule, which itself is subject to common exceptions. At worst, a court excludes critical evidence of the crime, and the prosecutors' office must drop the case. But even this consequence may fall primarily on the prosecutor's office rather than the police. It is, therefore, unclear on theoretical (Slobogin 1999) and empirical grounds (see NAS 2004) whether the exclusionary rule has any deterrent effect on police officers. Moreover, allegations of police misconduct in civil cases may occasionally result in large damage awards, but the empirical evidence suggests such remedies are weak deterrents on police misconduct (Hughes 2001).

3. Proximal versus Distal Regulation

Prior work has also emphasized the distinction between proximal and distal regulation on at least two dimensions. First, regulation can be institutionally distal (Amsterdam 1970). The legislature, for example, is a more distal regulator than a police sergeant who directly supervises officers on a daily basis. Some distal regulation by legislatures may impact the exercise of police discretion. Arrest rates have increased, for example, after states have enacted mandatory arrest policies in domestic violence cases (Simpson et al. 2006). But large institutional distances can mute the effect of regulation, which must "filter[] down . . . through the refracting layers of lower courts, trial judges, magistrates and police officials" (Amsterdam 1970: 791–92).[21] Scholars have noted a similar limitation on corrective judicial regulation. In many cases, police officers are not informed their behavior has lead to tarnished or excluded evidence at trial (Perez 1994).

Regulation can also be temporally distal (Walker 1993). Police academy training, for example, is temporally distal because officers participate at the beginning of their career and often receive little additional training thereafter. Empirical research has found training to be a modest predictor of legal knowledge (Perrin 1999; Hefferman 1991), but scholars have noted that without subsequent reinforcement—from in-service training, departmental incentives, or higher-level management—the short-term effects may disappear over time (NAS 2004; Haarr 2001; Wortley & Hommel 1995).

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[21] Some departments have established systems to inform officers of new legal developments in criminal procedure (Hirokawa 2000), but there is no empirical evidence that these systems influence police behavior in practice.

Corrective regulation faces a second kind of temporal distance: there are typically long delays between an inappropriate use of discretion and a sanction. Courts can take months or years before they resolve an allegation against an officer. Internal affairs and external complaint reviews also experience backlogs (Walker 2001: 108). As in traditional theories of criminal law, delayed and uncertain sanctions liely impose less deterrence (Kleiman 2009).

B. Prosecutorial Screening & Felony Review

The traditional goal of prosecutorial screening is to ensure that charges filed in criminal cases are appropriate for prosecution. The prosecutor must decide whether to decline a case in its entirety or to remove or add specific charges (Neubauer 1974). The exact procedures and policies for screening vary. In some jurisdictions, the prosecutor's office does not review charges until a preliminary appearance in court. In others, a designated screening unit reviews cases at an earlier stage, either before charges are filed in court, or soon afterwards. Office policies and standards for screening also vary. Many offices discard any charges for which there is insufficient evidence of probable cause on the elements of the offense (Mellon 1981). Some offices also screen out charges that will ultimately be unsupported at trial because of constitutional violations and evidence suppression. Some offices impose even higher standards, refusing to pursue a charge unless they are confident it will be won at trial (Wright & Miller 2002; Flemming 1990; Mellon 1981).

In the following subsections, we describe the policies and procedures of Felony Review—the prosecutorial screening process in Cook County. We then argue that

prosecutorial screening may serve as an effective regulatory device that avoids important limitations on traditional forms of police regulation described above.

1. Structure of the Felony Review Office

Felony Review is an independent charge-screening unit in the Cook County State's Attorney's Office. The unit is open 24 hours per day to provide legal guidance to police officers and to screen felony charges. In total, the office consists of over fifty attorneys. One Felony Review Supervisor and three Deputy Supervisors—who typically have over ten years of prosecutorial experience—lead the office. The remaining members of Felony Review are divided into four teams, each of which has a leader with experience managing a felony courtroom as a first chair. The teams also have two less experienced supervisors who have served as a second chair. Each team has ten to thirteen line assistant state's attorneys (ASAs) who typically have experience in the juvenile and criminal appeals divisions before they are assigned to Felony Review (Chicago Felony Courts 2007).

Line ASAs are generally responsible for approving felony charges. In more difficult cases, they may also seek guidance from team leaders, Deputy Supervisors or the Supervisor of Felony Review. Only the Supervisor and Deputy Supervisors have authority to approve charges in murder cases.

2. Procedures of Felony Review

Historically, police officers in Chicago had the authority to file felonies[22] and misdemeanors without prosecutorial approval. The basic contours of the felony review process derive from a memorandum of understanding formed in the early 1970s between the Police Department and State's Attorney's Office, which requires the police to obtain prosecutorial approval before filing non-drug[23] felony charges against an adult[24] suspect (Gilboy 1984; CPD G06-03, 2012).

In the typical case, the process works as follows. A police officer arrests a suspect, takes him to the police station, and books him in jail. Before formally filing felony charges in court, the officer must obtain approval from Felony Review. In more serious person cases, an ASA often goes to the scene of the crime or police station to interview suspects and inspect evidence (BJA 2005). In most cases, however, the

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[22] One exception is for homicide cases. Since the mid-1960s, police officers have needed approval from the state's attorney's office to file a homicide charge (Gilboy 1984).

[23] Felony Review also does not apply to arrests for syndicated gambling conducted by members of the Bureau of Organized Crime.

[24] During the study period (1999–2010), offenders under seventeen years of age were subject to the exclusive jurisdiction of the juvenile court, and offenders over seventeen were subject to the exclusive jurisdiction of the adult court. A number of Illinois statutes complicate this general principle. First, several statutes provide the adult court with exclusive jurisdiction for a subset of crimes committed by arrestees under seventeen. One statute provides exclusive jurisdiction to the adult courts for 15-year-olds for a subset of serious and mostly violent crimes. Another statute provides exclusive jurisdiction to the adult courts for 13-year-olds for first-degree murder committed in the course of a sexual crime or kidnapping. In all other cases, jurisdiction for arrestees under seventeen originates in the juvenile court. A second set of mandatory transfer statutes enable the prosecutor to request that the juvenile court transfer a defendant who is over fifteen years old for a subset of serious violent crimes that involve gang activity. Upon finding probable cause for any of the enumerated crimes, the juvenile court is required to transfer the juvenile. A third set of statutes enable the court to transfer a juvenile to the adult court on its own discretion.

arresting police officer conducts the felony review process over the phone. The officer describes the alleged crime, the circumstances of the arrest (e.g., where the arrest of effected, whether Miranda warning was given), the available evidence and how it was recovered. The officer also provides relevant police report numbers, and the contact information for suspects and witnesses (Devitt & Markovic 1987). The average phone call lasts about 20 minutes. Simpler cases like retail theft may only take ten minutes, but it is not uncommon for more complex cases to last 45 minutes. All of the information is inputted into a computer database at the State's Attorney's Office, and is used by an ASA at the bail hearing the next day.

Felony review can result in three outcomes. The office can (1) reject, (2) preliminarily reject or (3) approve the charge. When a charge is rejected, the police remain free to file a misdemeanor charge without approval from Felony Review. Charges are most frequently rejected due to problems related to victim credibility, witness non-cooperation, insufficient evidence, and weak eyewitness identification (BJA 2005).

When a charge is preliminarily rejected, the police officer is instructed to continue investigating the crime and to seek approval at a later time. An ASA that preliminary rejects a charge is responsible for the ongoing supervision of the investigation and the ultimate decision whether to grant approval.

When a felony charge is approved, the police department files paperwork in court to obtain a preliminary finding of probable cause from a Duty Judge on the approved felony charges and any additional misdemeanors. The case then goes to bond court where

a judge sets bail. The State's Attorney's Office has thirty days from the date of arrest to file an information or indictment.

The process differs somewhat for juvenile suspects. Illinois law requires that the arresting officer transfer a juvenile suspect to a designated juvenile police officer who has received special training and who acts as an advocate for the child. The juvenile officer decides whether to release the juvenile, give an informal or formal adjustment, or file formal charges and refer the case to court. In general, charging decisions for juveniles are not subject to felony review. There are exceptions, however, for homicide charges and any felony charges against juveniles transferred to the adult system (CPD G06-03, 2012). There is also a pilot juvenile felony review program for sex offenses, and violent crimes involving a firearm or causing bodily harm (CPD S06-04-07, 2000).

3. Substantive Standards of Felony Review

The general policy of Felony Review is to approve charges where there is sufficient evidence to support the statutory elements of a crime beyond reasonable doubt. This simple rule masks significant complexity in the process of reviewing felony charges. The meaning of beyond reasonable doubt is flexible, and it is often difficult to predict whether a jury would convict in a particular case. A few vignettes help illustrate how the standard applies in practice.

To convict a defendant of felony possession of a stolen motor vehicle (PSMV), the State must prove he knowingly possessed a stolen vehicle (Hunter 2012). In some cases, sufficient evidence will clearly be present. Suppose, for example, a police officer checks the license plate number of a vehicle and discovers it was reported stolen. When

the officer approaches the vehicle, he observes a pealed steering column, a punched lock or a broken window—signs that the vehicle has been broken into. He also finds that the individual in the driver seat does not have keys to the car. Felony Review will almost certainly approve a PSMV charge under these circumstances because the driver was on notice the vehicle was stolen. Other cases may be more difficult. Felony Review is less likely to approve a PSMV charge if, for example, the suspect has the keys to the car. And approval is highly unlikely if there is also no physical sign of a break-in and the driver claims he borrowed the car from someone else and was unaware it was stolen.

As another example, for a robbery charge the State must prove the defendant intentionally took property from another by force or by threatening the imminent use of force (Hunter 2012). In some robbery cases, sufficient evidence will clearly be present. Suppose, for example, that the perpetrator approaches a victim from behind in broad daylight, sticks a gun in his back, demands and receives the victim's cell phone, and runs away. Police officers across the street observe the crime in progress, and chase and arrest the perpetrator. Felony Review will almost certainly approve a robbery charge. At the other extreme, suppose the robbery takes place at night; there are no witnesses; the victim describes the perpetrator as male, of average height and wearing a black hoodie; and a suspect matching the description is apprehended an hour later in the same neighborhood. Felony Review is unlikely to approve a robbery charge because other men of average height could be wearing a black hoodie in the same area. Of course, many more difficult cases fall between these two poles. Whether Felony Review will decide to approve charges may be affected by a wide range of facts including visibility, the level of

specificity and accuracy of the suspect description, the presence and reliability of witnesses, whether the suspect was found with a gun, etc.

As one final example, suppose a bar fight spills out onto the street. One participant removes a gun from his pocket, shoots and misses the victim, and runs away. The police are called and a few minutes later the perpetrator is apprehended two blocks away with the gun in hand. Under these circumstances Felony Review is likely to approve charges for unlawful use of a weapon, aggravated discharge of a firearm, and perhaps even attempted murder. The case becomes more difficult if the suspect is arrested without the firearm, particularly if there are no witnesses other than the victim. If there is also no video recording of the fight inside or outside the bar, Felony Review is likely to reject the charges.

Although felony review is largely a collaborative process, tensions do arise over whether charges should be approved in individual cases. The Police Department may disagree with Felony Review about whether the available evidence meets the relevant evidentiary standard. It may also disagree about the standard itself. The decision to arrest and the initial judicial approval of charges filed in court are regulated by the probable cause standard, which is lower than beyond reasonable doubt. And as one officer explained, the ultimate goal of police officers is to build the "strongest case possible." As the strongest case possible does not always rise to beyond reasonable doubt, police officers may still believe that felony charges are warranted in cases rejected by Felony Review.

Police generally defer to the rejection of charges by Felony Review. The Police Department does, however, retain the authority to override Felony Review and file charges in court anyway.[25] To do so, a station supervisor must seek override approval from an area deputy chief or area commander (CPD S06-03).

Importantly, a police department override does not ensure that charges will be prosecuted. That decision is ultimately in the hands of the State's Attorney's Office. One police officer described the override process as a game of "chicken." Suppose the police have apprehended a suspect for a gun shooting. Felony Review rejects felony charges because, although the police obtained a video recording of the crime, they have not recovered the weapon. The police department is confident the suspect is guilty and thus asks Felony Review to reconsider. Felony Review rejects the charge again. The Police Department may then inform the State's Attorney's Office of its intention to exercise its override authority. The State's Attorney's Office might respond that if charges are filed, it will refuse to prosecute the case, and the charges will be dismissed by the court. Believing that the suspect should be charged, the Police Department may nonetheless exercise its override powers and file felony charges. The next day, the suspect appears before a judge for a bond hearing. In some cases, the State's Attorney's Office refuses to prosecute the case while in others it may decide otherwise.

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[25] Historically, the police department could only override rejections of charges, but could not override a temporary rejection pending further investigation. Perhaps due to a perception that the felony review unit was using the latter to avoid overrides, the police department recently gained the ability to override temporary rejections.

The threat of an override appears at times to trigger a process of blame shifting. One officer explained that the Police Department may encourage the State's Attorney's Office to approve charges by insinuating that the State's Attorney will be embarrassed or publicly criticized for letting a serious gun offender go free. The State's Attorney's Office may similarly discourage the Police Department from overriding felony review by suggesting the Department will be embarrassed or criticized when the State's Attorney refuses to prosecute a serious gun offender because the police failed to investigate the case properly.

This process of blame shifting highlights that, at least in some cases, charge screening is not merely a one-dimensional inquiry about evidence. Charge screening may also reflect other considerations—such as public safety, perceptions of public safety, political pressure and public scrutiny—that may affect the stringency of evidentiary standards applied in individual cases.

4. Theorizing Prosecutorial Screening

Legal and empirical scholars have closely examined the factors that influence prosecutorial screening decisions and the effect of screening on downstream phases of the criminal justice system. They have, for example, studied the effect of case characteristics on screening decisions for a range of different criminal offenses (Pyrooz et al. 2011; O'Neill 2004; Spohn et al. 2001; Frohman 1981; Frase 1980). They have also explored the significance of procedure (Brosi 1979; Jacoby 1977), administrative regulation (Barkow 2009; Bibas 2009), and environmental conditions (Mellon 1981) at the

screening stage. And they have considered the effect of screening on plea negotiations (Wright & Miller 2002; Lynch 2003).

While academic scholarship has recognized that the quality of arrests may affect screening, prior work has not considered the reverse: whether screening also affects police arrest or charge decisions. We suspect that screening systems like Felony Review may serve as a particularly effective regulatory mechanism because it enjoys several operational advantages relative to other methods of police regulation discussed earlier.

First, charge screening systems like Felony Review operate as a corrective form of regulation and thus avoid reliance on vague procedural rules common in judicial, legislative and administrative regulation. Screening is corrective because the prosecutor rejects charges due to specific evidentiary or constitutional deficiencies. Screening thus provides police officers an opportunity to observe in concrete cases the evidence required for a successful prosecution and the consequences of constitutional violations. As one officer explained, young police officers learn the "science" of charging—the basic elements of the offenses—in police academy. But they learn the "art" of charging on the job, from their partners and supervisors and from their direct interactions with Felony Review. An attorney in the Felony Review unit echoed a similar sentiment, acknowledging that young police officers can learn the nuances of charging in phone calls with felony review as their charges are accepted and rejected over time. Felony review also shifts officers' incentives as they have less reason to charge a suspect if they know the prosecutor will reject the charge anyway (Wright 2002). Police officers and prosecutors frequently disagree about the validity of charges in particular cases (Holleran

et al. 2008), and some prosecutors may view the rejection of "an exceptional number of cases . . . as a necessary part of training police officers to investigate more thoroughly" (Wright & Miller 2002: 65).[26]

Second, in jurisdictions like Chicago, charge screening is more proximal than corrective regulation by other agents such as courts, internal affairs, and civilian review. Charge screening is institutionally proximal because prosecutors work closely with the police on daily matters. It is especially proximal in jurisdictions where the police are responsible for filing formal charges, but must obtain approval from the charge screening office before doing so. In Chicago, for example, police obtain immediate and direct feedback from Felony Review over the phone each time they prepare to file felony charges. Charge screening is also temporally proximal. Courts, internal affairs and civilian review rarely sustain complaints against officers, but screening takes place every time a suspect is arrested for a felony charge.

Third, the prosecutor has the power of an external regulatory agent without many of the typical drawbacks. Though prosecutors may share similar overarching goals with the police, their institutional priorities differ. Prosecutors have stronger institutional incentives to produce convictions and may, therefore, place greater weight on the quality

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[26] Connick has explained the high declination rates in New Orleans based upon the fact that "Poor police work made declinations necessary" (Wright 2002: 65). It is difficult to assess empirically whether the police hypothesis or the prosecutorial hypothesis is correct on this issue. We found one potentially valuable empirical result in the literature to help resolve this issue. Brosi (1979) studied declination rates in a prosecutor's office that received charge filings from two different police departments. He finds that the prosecutor's office declined a larger proportion of charges across all crime categories from one department than the other. This finding provides some support for the proposition that the declination rate is partially determined by the quality of the arrests and investigations conducted by the police department.

of evidence against a suspect and on potential constitutional violations. Prosecutorial screening does not appear to suffer from many of the primary constraints on other external forms of external regulation either. Unlike judges and legislators, prosecutors have more substantive expertise on the nature of police work. Unlike citizen review boards, which rarely have final decision-making authority, a prosecutor's refusal to pursue a case cannot be overruled by the police department in most jurisdictions. And unlike criminal prosecutions of police misconduct, which are controversial and publicly visible, charge screening is a low visibility and relatively low consequence process that involves less intense conflicts of interest.

Of course, we do not regard charge screening as a panacea to all problems of police discretion. First, the regulatory effects are limited to the criteria of charge screening, which typically focus on the quality of evidence in support of a given charge against the suspect, and the presence of constitutional violations that would result in evidentiary exclusion. Second, prosecutorial screening is a preliminary assessment of evidence presented by the police officer. Jurisdictions vary in the amount of resources allocated to screening (Wright & Miller 2002), and in many jurisdictions, the prosecutor is unable to conduct any independent investigation. Some of the investigative challenges in complaint reviews, therefore, likely also apply to screening.

Third, structural and institutional variation across jurisdictions may influence whether prosecutorial screening can provide proximal and frequent feedback. In Chicago, the arresting officer generally contacts Felony Review directly. But, in other jurisdictions

such as Philadelphia, the police do not have charging responsibilities, which sharply limits the potential for feedback to officers.

Fourth, the effect of screening on police behavior depends upon the office's standards and policies. Standards that are too stringent will discourage police from conducting warranted arrests and filing supported charges. Indeed, prosecutors have incentives to apply unnecessarily stringent standards to avoid weak cases that threaten conviction rates. Prosecutors may also have incentives to apply standards that are too low to encourage higher charges that are later used as bargaining chips in plea negotiation (Bibas 2009).

Our discussion has focused primarily on individual prosecutors screening the charging decisions of individual officers, but screening may also be viewed at a higher level of analysis as a kind of agency-on-agency regulation. This concept of agency-on-agency regulation has received increased attention from criminal justice scholars in recent years. Richman (2003), for example, describes how the United States Attorney's Office's (USAO) exerts institutional pressures on the deployment of investigative resources by the Federal Bureau of Investigations through its gatekeeping monopoly on federal charging. And Rushin (2015) and Harmon (2009) examine how the USAO regulates local police departments through structural reform litigation under 42 U.S.C. § 14141. In the case of charge screening, prosecutors implement screening policies with varying levels of specificity that are adopted by upper-level management. These policies may not only influence the behavior of individual police officers, but also police department policy related to investigation and charging.

Agency-on-agency regulation is also a fruitful analytic lens because it highlights that the regulatory effects of prosecutorial screening do not go in only one direction. Police departments may pressure a prosecutor's office to accept charges in particular cases, or may even push back against unduly stringent standards in general (Richman 2003). The police department's authority to override felony review in Chicago, shows that an area deputy chief can override the charging decisions of the felony review office (CPD S06-03). Thus, the precise standards of felony review in some cases may be under ongoing negotiation between the two agencies.

III. Prior Evidence of a Discontinuity at the Age of Majority

As noted earlier, we observe substantial drops in felony charges among those arrested just days or weeks before their seventeenth birthday. These drops might be explained by a change in offender behavior or a change in police behavior. In this section, we review the empirical literature in search of evidence that can help adjudicate between these two possibilities. Drawing on prior studies of offending and police arrest practices, we arrive at two main conclusions. First, with only one exception (Levitt 1998), prior empirical studies have found that offending is nearly continuous across the age of majority (Wolfgang et al. 1987; Hjalmarrson 2009; Lee & McCrary 2009; Singer & McDowall 1998; Steiner & Wright 2006), suggesting that the drop in felony arrests is not explained by changes in offender behavior. Second, the literature offers no consistent evidence on changes in police behavior just days or weeks before and after the age of majority.

A. Offender Behavior at the Age of Majority

Two areas of the criminological literature shed light on the relationship between offending and age. First, criminologists have long observed strong associations between age and offending (Wolfgang et al. 1972; Hirschi & Gottfredson 1983). Yet, studies in this literature have not found sharp discontinuities at any particular age of the life course. One research group working on these issues concluded that "[c]riminal behavior . . . is continuous and develops independent of legal boundaries such as the switch from juvenile to adult criminal status" (Wolfgang et al. 1987: 6).

A second area of the literature has examined the relationship between criminal offending and prosecution in the adult criminal justice system. Early studies tested whether the risk of adult prosecution deters crime. Three studies have examined the effect of laws requiring certain juvenile offenders to be transferred to adult court (Singer & MacDowall 1988; Jensen & Metsger 1994; Risler et al. 1998). Another study examined the effect of a direct file statute that empowered prosecutors to transfer serious juvenile offenders to the adult system without judicial approval (Steiner & Wright 2006). All four studies found little evidence that the risk of prosecution in the adult system deterred juvenile offending.

Other studies have compared offending among adolescents slightly younger and older than the age of majority. Levitt (1998) compared the trend in arrest rates across the age of majority in all fifty states and found that the relative rate across the boundary tends to be substantially lower in states with adult systems that are relatively more punitive than their juvenile system. A prior study with a similar design found that these

differences were better attributed to police behavior than offender behavior (Ruhland et al. 1982). More recent studies with individual-level data have found little evidence of a large change in offending. Lee & McCrary (2009), for example, conducted a regression discontinuity on arrest data from Florida using the age of majority as the threshold.[27] The authors found that offending decreased by just 2% across the boundary. Hjalmarsson (2009) replicated these findings using self-report data on offending. This methodological approach is notable because it is the only study that can isolate changes in offender behavior from changes in police arrest practices.

Together, the literature provides significant evidence that offending does not decrease just after the age of majority. With only one exception, prior studies have found almost no change in offending. And the only study that can isolate offender behavior from police arrest practices supports this conclusion (Hjarlmasson 2009).

B. Police Behavior at the Age of Majority

The empirical literature on the decision to arrest and charge is one of the most extensive research programs in criminal justice scholarship. Yet, it provides little guidance on the effect of the age of majority on arrest and charging practices. Scholars have paid little attention to the role of age and juvenile status on the decision to arrest. The few studies that consider these variables merely control for them to investigate other variables of interest. Furthermore, scholars have paid little attention to charge selection, the process by which police officers select charges to file or recommend against a

[27] The age of majority in Florida is age eighteen.

suspect. The few studies in this area have not considered the effect of age or juvenile status (Sutphen et al. 1993; Phillips & Varano 2008; Phillip & Gillham 2010).

Researchers have devoted little energy to examining the effect of age directly. What can be gleaned from the literature derives from studies that control for age while examining the effects of other variables of interest. The results of these studies are inconsistent. Many have found that older suspects are moderately more likely to be arrested (Carrington 2003; Worden 1996; Friedrich 1977; Lundman 1974; Sealock & Simpson 1998), while others have found no association (Smith & Visher 1981; Lundman 1994; Visher 1983; Smith 1984; Smith et al. 1984). These inconsistencies are particularly notable because they derive from many of the same datasets. Data from at least seven studies were re-analyzed with inconsistent results for age, suggesting that the relationship between age and arrest is sensitive to model specification (Worden 1996; Lundman 1994; Lundman 1974; Smith & Visher 1981; Visher 1983; Smith 1984; Smith et al. 1984).[28]

A related variable—suspect's status as a juvenile or adult—is more relevant to the current study. The literature provides little guidance because few studies use data on both juveniles and adults. Several recent studies have estimated the effect of being a juvenile on the decision to arrest (Brown et al. 2009; Novak et al. 2002), but they did not otherwise control for the effect of age. A recent review of the literature by the National Academy of Sciences concluded that juvenile status "does not appear to affect police practice, in that patterns of decision making are based on the same criteria and [are]

[28] This unusually large number of replications results from a debate initiated by Klinger (1994) on the effect of demeanor on arrest, which lead to reanalysis of many prior studies.

weighed in the same ways" (NAS 2004: 116). The report attributes this continuity in arrest practices to the "well-documented trend … toward treating juveniles more like adults…" (2004: 117).

IV. Hypotheses and Theoretical Explanations of the Discontinuity

We test the seven most plausible hypotheses that could explain the drop in felony arrests observed at the age of majority. One group of hypotheses focuses on changes in offender behavior. Hypothesis 1 asserts that the drop is the result of a decrease in the total number of offenses committed after adolescents pass the age of majority. Hypothesis 2 asserts that the drop in felony arrests represents a kind of offending substitution, where adolescents do not commit fewer crimes after their seventeenth birthday, but instead commit less severe misdemeanor crimes (H2). Deterrence provides the primary theoretical explanation for both behavioral shifts, as the adult system is perceived as more punitive than the juvenile system (Lane et al. 2002; Glassner et al. 1983).

The remainder of our hypotheses focus on the police. Hypothesis 3 asserts that police arrest fewer adolescents just after the age of majority. Hypothesis 4 asserts that the drop in felony arrests represents a kind of charge substitution, where officers continue arresting adolescents at the same rate after their seventeenth birthday, but charge them with lesser misdemeanor crimes.

We test three different possible causal motivations for the behaviors in Hypotheses 3 and 4. According to Hypothesis 5, police officers charge juveniles for felony crimes more frequently than adults because they perceive that the juvenile system is not sufficiently punitive. On this view, the drop in felony arrests at the age of majority

represents an effort to ensure that juveniles do not merely receive a "slap on the wrist" in the juvenile system. We suspect that the police would be more likely to overcharge juveniles with longer criminal records. Hypothesis 6 asserts that officers charge juveniles for felony crimes more frequently than adults to protect some young adults from the negative consequences of a felony charge in the adult system. Though there has been little research on differential protective behavior by age, prior work suggests that officers engage in protective behavior for women whose behavior is consistent with traditional gender norms (Visher 1983). It is possible that officers also undercharge—and thus protect—adults with shorter criminal histories from the consequences of an adult felony record. Finally, officers might engage in differential arresting or charging across the age of majority due to screening differences between the juvenile and adult systems. There are a number of procedural differences between these systems, and it is difficult to disaggregate the effects of each. Hypothesis 7 asserts that officers engage in differential arresting or charging due to felony review—the prosecutorial screening program in the Cook County State's Attorney's Office.

V. DATA

We obtained a charge-level dataset for all 4,300,000 arrests between January 1999 and February 2013 from the Chicago Police Department. The data provide information on each arrest, including the date of arrest, statute charged, Uniform Crime Report (UCR) classification, offense type (felony or misdemeanor) and a brief description of the offense. The dataset also contains information on the arrestee, including an individual record identifier number (IR number), date of birth, gender, and race. The data also

indicate whether a case was processed through felony review. Finally, because the data is charge-level, we observe all charges entered into the police department information management system, including those that are dropped afterwards due to felony review or some other reason. To observe police charges prior to the felony review process, we convert the charge-level dataset into an arrest-level dataset by keeping the most serious initial charge entered into the system.

With this arrest dataset, we constructed a measure of prior felony arrests by counting the number of prior felony arrests associated with the same IR number. Next, we applied a number of exclusion restrictions. First, we excluded all 754,666 arrests from 2010 to 2012 due to a 2010 legislative change to the age of exclusive jurisdiction for the juvenile court for misdemeanor crimes. Next, we excluded 1,030,314 arrests that did not involve a misdemeanor or felony charges. Finally, we excluded 5,124 arrests in which the suspect was over 72 years old.

After excluding the observations noted above, 2,351,386 arrests remain in our analysis sample, of which 724,297 were felonies and 1,626,961 were misdemeanors. The average age is 28.6 (SD=11.5). Roughly 84% of the sample is male. About 26% of the sample is white, 73% is black, and less than 1% is defined as "other." The average number of felony priors is 1.2. Approximately 54% of the sample has no felony priors, 18% have 1 prior, 10% have 2 priors, 6% have 3 priors and 7% have 4 priors or more.

We use these data to conduct a series of empirical tests (i.e., histograms, tests for differences in mean) that discriminate between the different theories considered in Section III. We apply McCrary's (2006) density test to assess whether jumps and drops in

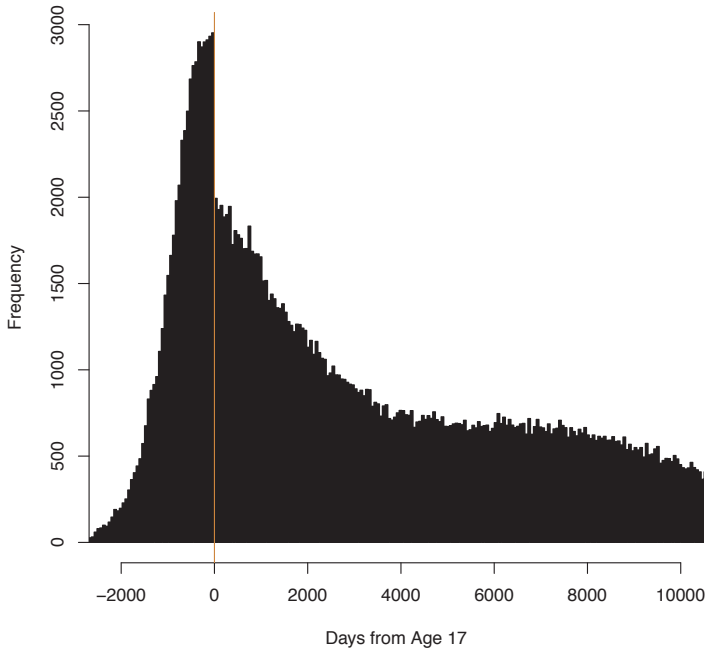
the distribution of charges at the age of majority are statistically significant. Table 2.1 also shows the results of a series of tests to better understand the size of the discontinuities. We first calculated the absolute and relative difference between each adjacent 60-day bin within 2 years of age seventeen. Column 2 shows the proportion of inter-bin differences that are larger than the difference between the two bins that are adjacent to the age 17 boundary. Column 3 provides the results when differences are calculated in relative rather than absolute terms. Next, we randomly selected hypothetical age thresholds and applied McCrary's density test. Column 4 shows the proportion of hypothetical age boundaries associated with a statistically significant discontinuity. Column 5 shows the proportion of hypothetical age boundaries associated with estimates of the size of the discontinuity that are larger than the estimated size of the discontinuity at the true age threshold. Finally, using the frequency of each 60-day bin in a given histogram, we randomly shuffled the frequencies to a different age range and fit a regression discontinuity model using a local linear regression. Column 6 shows the proportion of shuffle distributions with estimates of the size of the discontinuity at age seventeen that are larger than the estimate for the true distribution. This provides an assessment of the probability we would observe a discontinuity of this size under the assumption that arrest is uncorrelated with age.

VI. Analysis and Results

Figure 2.1 presents a histogram of the number of non-drug felony arrests by age in days with respect to suspects' seventeenth birthday. The figure shows the basic empirical result of the paper, a substantial drop in felony arrests at age seventeen. In this

section, we conduct empirical tests to assess the validity of the theoretical explanations of the discontinuity enumerated in Section III. We begin by testing whether the drop in charges at the age of majority can be attributed to shifts in offender behavior. We then consider whether changes in police behavior offer a stronger explanation.

Figure 2.1: Non-Drug Felony Arrests by Days From Age 17

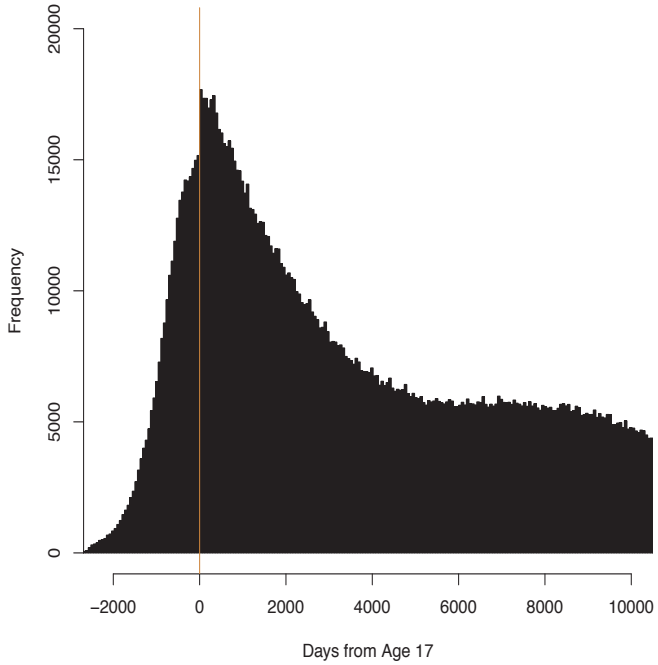


A. Theories of Offender Behavior

The first offender-based theory of the drop in felony arrests is that offending decreases as youth pass the age of majority (H1). On this view, individuals know that on their seventeenth birthday they become subject to prosecution in the adult system and perceive a threat of increased sanctions. We can test this theory by examining the distribution of all arrests (both felony and misdemeanor) by age: if youth commit fewer crimes after passing the age of majority, we should observe a drop in total arrests. Figure 2.2 displays a histogram of all felony and misdemeanor arrests by age in days with

respect to age seventeen. It reveals no drop in total arrests, and if anything, the number of arrests increases at age seventeen.[29] As a preliminary matter, Figure 2.2 appears to provide evidence that the discontinuity in felony arrests is not the result of a decrease in the total number of offenses.

Figure 2.2: All Arrests by Days from Age 17



If total offending does not decrease at the age of majority, the drop in felony arrest charges might still be explained by adolescents offending at the same frequency, but committing less serious misdemeanor crimes (H2). This kind of offense substitution is more plausible for some categories of crimes than others. We suspect, for example, that

[29] That total arrests increase at the age of majority is another interesting puzzle. The jump reflects a significant increase in misdemeanor arrests. Juvenile arrests entail significant additional processing costs on police, especially in the form of paperwork. Police officers amy be less likely to conduct a misdemeanor arrest of a juvenile relatively trivial infractions that are not worth this additional processing cost.

crimes of passion such as felony assault are least likely to decrease dramatically by this process. Indeed, these crimes are often impulsive and lack premeditation, and scholars commonly hypothesize they are more difficult to deter through criminal sanctions (e.g., Peterson & Bailey 1991). Yet, Figure 2.3 shows a very large drop in felony assaults at the age of majority ($p = .000$). The discontinuity is larger than at all other adjacent-bin pairs (Table 2.1, Columns 2 & 3), and when bins are randomly shuffled to remove the correlation between age and crime, less than 1% of the generated distributions have larger discontinuities (Column 6). Similarly, prior work has suggested that individuals who engage in drug trafficking may be especially aware of the significance of the age of majority (Moskos 2008; Leviton et al. 1994). But, as we discuss below, felony drug crimes show no drop in offending across many crime categories.

Figure 2.3: Felony Assault Arrests by Days from Age 17

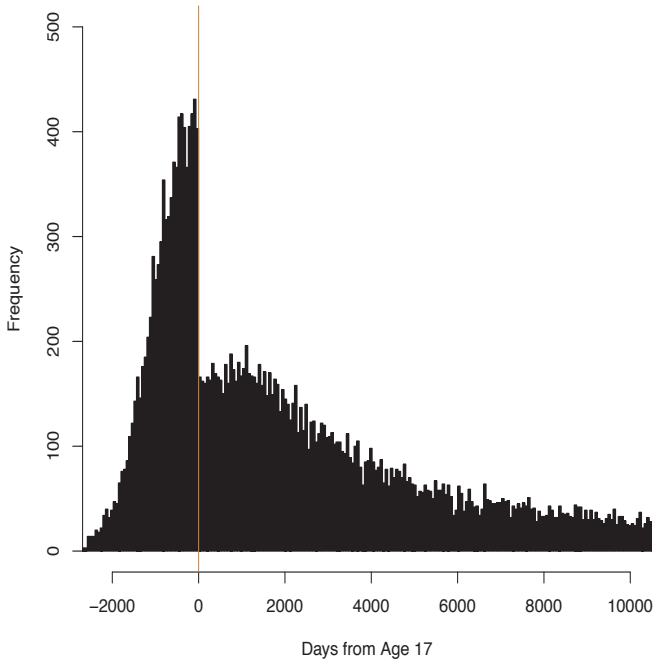


Table 2.1: Proportion of Hypothetical Thresholds with Significant Discontinuities

	True Thresh	Adjacent Bins, 2 Years		Shuffle Threshold		Shuffle Bins
	(1)	(2)	(3)	(4)	(5)	(6)
Crime Category	P-Value†	% Alt > True (Absolute)	% Alt > True (Relative)	% Alt P < .05†	% Alt B > True B†	% Alt B > True B††
Fig 2.1: F Non-Drug	0.000	0.0%	0.0%	15.2%	0.0%	0.2%
Fig 2.3: F Assault	0.000	0.0	0.0	6.4	0.0	0
Fig 2.4: F/M Assault	0.000	11.8	5.9	10.8	1.0	47.8
Fig 2.4: F/M Larceny	0.005	5.9	5.9	6.2	0.6	98.8
Fig 2.4: F/M Vehicle Theft	0.305	11.8	11.8	10.0	48.2	43
Fig 2.4: F/M Vandalism	0.035	64.7	64.7	4.4	9.4	97.6
Fig 2.5: F Retail Theft	0.000	0.0	0.0	10.2	0.0	0.2
Fig 2.6: F Retail Theft Prior	0.954	17.6	29.4	16.6	93.0	95
Fig 2.7: F Weapons Viol	0.878	76.4	76.4	13.4	91.8	75.6
Fig 2.8: F Agg. Assault	0.000	0.0	0.0	7.6	0.0	0
Fig 2.8: F Burglary	0.000	0.0	0.0	4.8	0.2	2.8
Fig 2.8: F Robbery	0.000	0.0	0.0	12.2	0.0	3
Fig 2.8: F Disord Conduct	0.003	0.0	17.6	11.0	0.0	0
Fig 2.8: F Larceny	0.000	0.0	0.0	21.2	0.0	2.2
Fig 2.8: F Vehicle Theft	0.000	0.0	0.0	3.8	0.0	0
Fig 2.8: F Vandalism	0.004	0.0	5.9	9.0	3.0	3.8
Fig 2.9: F Drug	0.394	100.0	100.0	12.8	39.6	74.4
Fig 2.10: F Homicide	0.274	0.0	0.0	7.2	23.2	26.8
Fig 2.11: Other F Offenses	0.339	100.0	100.0	9.4	15.0	2.8

† Estimated with McCrary density test

†† Estimated with local polynomial regression

Based on our data alone, we cannot reject with certainty all offender-based theories that could explain the discontinuity in felony arrests in Chicago. But our conclusion is reinforced by prior work, which has found little evidence of a behavioral change at the age of majority (e.g., Hjalmarsson 2009; Lee & McCrary 2009; Wolfgang et al. 1987). One study of particular relevance recently examined offending in Chicago. The author found no large discontinuities in self-reported offending rates among 16, 17 or 18-year-olds (Kirk 2006).[30] Coupled with the existing empirical literature, the patterns

[30] Consistent with our own results, Kirk (2006) also finds no evidence of a discontinuity in arrests at the age of majority because he does not disaggregate arrests by charge type.

observed for total offending, crimes of passion and drug offenses provide substantial evidence that decreases in total offending and offense diminution (H1 & H2) are inadequate explanations.

B. Theories of Police Behavior

1. Charge Substitution

If changes in offender behavior cannot explain the discontinuity in felony charges at the age of majority, then, changes in two forms of police behavior might offer a better explanation. The discontinuity might result from a decrease in the frequency of total arrests just after the age of majority (H3). Or, it might result from a process of charge substitution analogous to offense diminution: officers may charge certain suspects over seventeen years of age with a misdemeanor when they charge similar suspects under seventeen with a felony (H4).

To test these hypotheses, we consider three sources of evidence. First, as we have already noted, Figure 2.2 reveals no decrease in the total number of arrests at the age of majority. Instead, the frequency of total arrests increases. Second, the data provide evidence of charge substitution from felony to misdemeanor crimes. Detecting charge substitution is difficult because any single charge might substitute to multiple other lesser charges.[31] One particularly plausible form of charge substitution is across charge grades within Uniform Crime Reports (UCR) classifications. We examined the four felony UCR classifications that have a corresponding misdemeanor: assault, auto-vehicle theft,

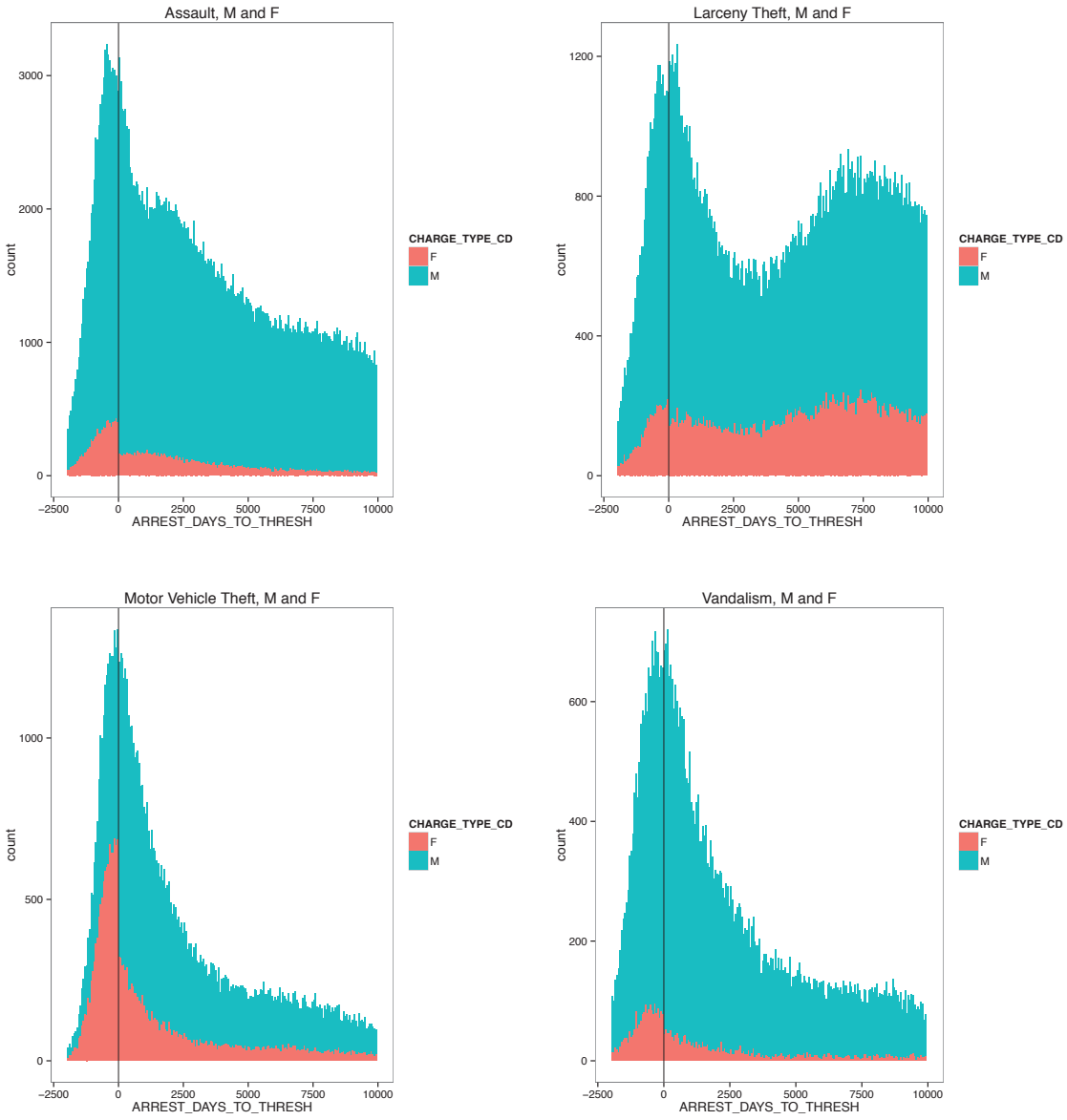
[31] Robbery, for example, could substitute to theft or assault. Burglary might substitute to criminal trespass or non-burglary theft.

larceny theft and vandalism. Figure 2.4 displays a series of stacked histograms by age in days relative to the seventeenth birthday. The red bars represent felony charges, and the green bars represent misdemeanor charges stacked on top.

These figures present several important patterns. All four felonies show sharp drops in frequency at age seventeen. More importantly, when felonies and misdemeanors are stacked together, they form a nearly continuous distribution. Felony and misdemeanor assaults form a relatively smooth distribution except for a moderate jump after the seventeenth birthday, which is not much larger than other neighboring jumps. The results are even more impressive for motor vehicle theft. The stacked vandalism histogram presents a small jump at the age of majority, but this jump is also similar in size to other neighboring jumps and drops. The stacked distribution for larceny theft is less smooth at seventeen, but again, the size of the jump is relatively similar to other jumps nearby.

Although all of the distributions except motor vehicle theft have statistically significant discontinuities based on the McCrary test (see Table 2.1, Columns 1), over 5% of all adjacent-bin pairs have a larger discontinuity (Columns 2 & 3). Moreover, if we randomly shuffle the bins to remove the correlation between age and crime, 43 to 99% of alternative shuffles have larger discontinuities (Column 6). In short, the large discontinuities observed in the misdemeanor and felony distributions all substantially diminish when stacked together. This finding is especially notable given the dramatic differences in volume: there are approximately 10 times as many misdemeanor assaults as felony assaults, 11 times as many for vandalism, 3.5 times as many for theft, and over twice as many for motor vehicle theft.

Figure 2.4: Stacked Felony and Misdemeanor Arrests by Days from Age 17



The third source of evidence in favor of charge substitution comes in the form of a falsification test. Charge substitution implies that a police officer has the discretion to substitute one charge for another. We should, therefore, expect no discontinuity among criminal charges that do not provide the officer with statutory discretion to charge a lower

substitutable offense. We identified two kinds of felony crimes for which charge substitution is difficult.

First, certain Illinois state statutes enhance a misdemeanor to a felony if the offender has a prior record. In these cases, the police officer has no power to charge the misdemeanor. The enhancement is triggered automatically by the prior conviction, and a prior conviction is not the kind of fact an officer can leave out of an arrest report. Retail theft is the only "prior enhancing" criminal charge in our dataset with sufficient sample size to detect a discontinuity. Figure 2.5 displays a histogram of felony retail theft[32] and Figure 2.6 displays a histogram of felony retail theft with a prior conviction. Low sample size leads to substantial variation across bins, but as expected, there is a statistically significant drop in felony arrests for retail theft at the age of majority ($p = .000$). No other adjacent-bin pairs have a larger discontinuity (Table 2.1, Columns 2 & 3). However, when bins are randomly shuffled to remove the correlation between age and crime, roughly 20% of alternative shuffles generate larger discontinuities, raising the possibility that the observed drop is due to chance. The distribution for retail theft with a prior conviction is flat at seventeen suggesting no charge substitution ($p = .954$). Approximately 18 to 29% of adjacent-bin pairs have larger discontinuities (Table 2.1, Columns 2 & 3), and when bins are shuffled, over 97% of generated distributions have a larger discontinuity (Column 6).

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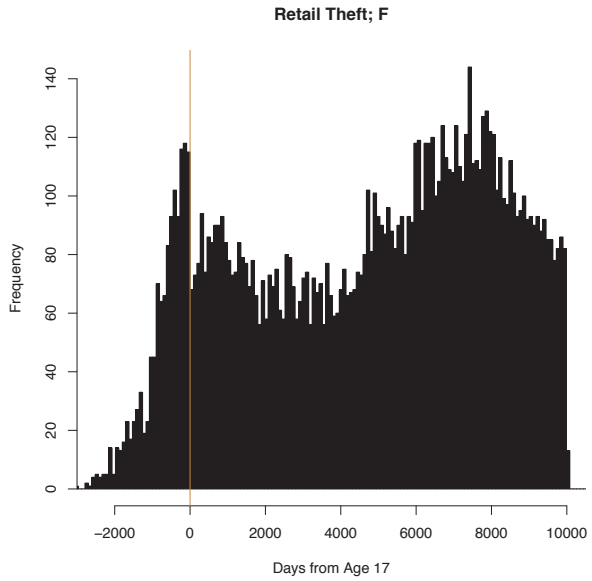
[32] It is possible that some crimes in the felony retail theft distribution are cases with enhancing prior convictions. But, the distribution contains a large fraction of cases without a prior enhancement.

Illinois' stringent weapons laws provide a second falsification test for the charge substitution theory. In Illinois, possessing a handgun as a seventeen year old is a felony (720 ILCS 5/24-3.1(a)(1)), and handguns account for nearly all illegal guns seizures (Pierce et al. 2003).[33] Police officers, therefore, have little statutory discretion to substitute a felony weapons charge to a misdemeanor weapons charge. Figure 2.7 displays a histogram of arrests for felony weapons violations. As expected, there is no evidence of a drop in offending at the age of majority (p = .879). Roughly 76% of adjacent-bin pairs, and 75% of random shuffles have larger discontinuities (Table 2.1, Columns 2, 3 & 6).

Figure 2.5: Felony Retail Theft Arrests by Days from Age 17



[33] Similarly, based on a review of our data and the Illinois statute (720 ILCS 5/24-1 24-1(a)(1)-(3)), non-firearm related weapon violation is almost always a misdemeanor.

Figure 2.6: Felony Retail Theft With Prior Conviction Arrests by Days from Age 17

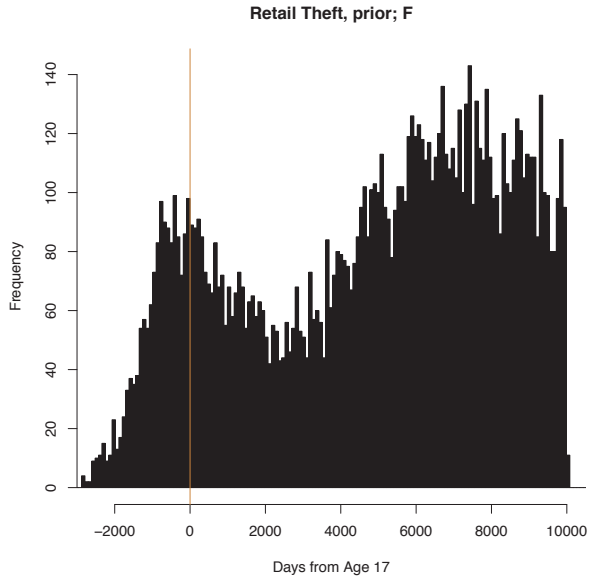
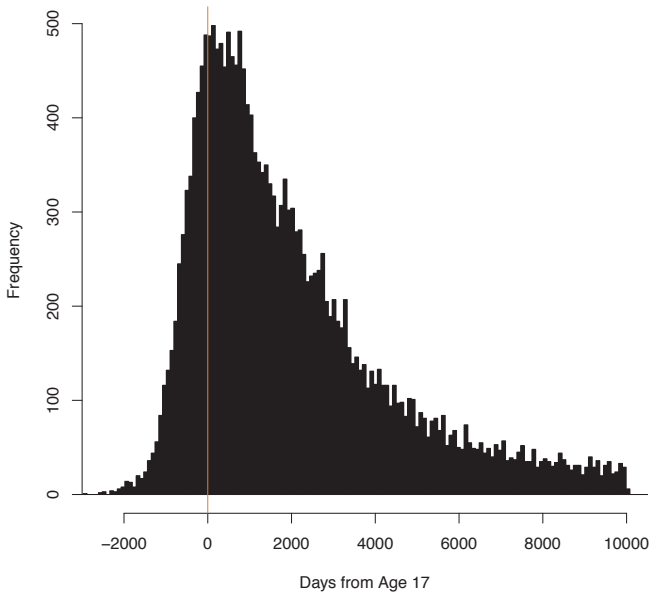


Figure 2.7: Unlawful Use of Weapons Arrests by Days from Age 17



2. Informal Processes for Charge Substitution: Protection and Slap-on-the-Wrist Theories

Charge substitution appears to account for a substantial fraction of the discontinuity in felony arrests. But we still have little understanding of the mechanisms

by which substitution takes place. The observed charge substitution may result from an informal exercise of police discretion based upon the personal characteristics of the suspect. At least two such processes could explain the sharp drop in felony arrests at age seventeen. First, officers may wish to protect certain adults from the harsh consequences of a felony charge in the adult criminal system (H5). Under this protection theory, officers may undercharge adults relative to similar juveniles. Second, in some cases, officers may worry that charging a juvenile suspect with a misdemeanor may result in little more than a slap on the wrist, and thus, overcharge him relative to an adult to ensure significant punishment (H6).

To test these informal theories, we examine prior arrest histories. Under the protection theory, officers tend to undercharge adults to prevent the harsh consequences of a felony record. On this view, an officer is most likely to substitute a felony to a misdemeanor for adults who are not experienced or "hardened" criminals, those with no or few prior arrests. In contrast, under the slap-on-the-wrist theory, officers tend to overcharge juveniles to ensure they receive significant punishment. On this view, an officer is most likely to substitute a misdemeanor to a felony for juveniles with longer criminal histories. As a coarse test of these hypotheses, we compare the average number of prior felony arrests for suspects arrested within a sixty-day period before or after their seventeenth birthday. Suspects below the age of majority had an average of 1.73 felony arrests, while suspects just above the age of majority had an average of 1.64 felony priors. This difference, while small, was statistically significant on both a t-test (p value = .039) and Wilcoxon ranked sum test (.045). The comparison thus provides some

preliminary evidence in favor of the slap-on-the-wrist theory and against the protection theory.

However, a simple difference in means, especially such a small difference, provides relatively little insight on informal charge substitution. By comparing the distribution of felony records on either side of the age of majority we can detect with greater specificity the kinds of offenders that are present or absent on either side. The first column of Table 2.2 presents the total number of suspects charged with a felony within 60 days of their seventeenth birthday by their number of prior felony arrests. Across nearly all criminal histories, the number of arrestees under seventeen outnumber the number of arrestees over seventeen. This is consistent with the general drop in felony arrests at the age of majority. Most of the "extra" below-seventeen suspects have short criminal histories with just zero to two priors.

Table 2.2: Prior Arrests Below and Above 17th Birthday, 60 Day Bandwidth

	Count		Density	
Priors	**Below 17**	**Above 17**	**Below 17**	**Above 17**
0	2181	1727	41.6%	40.2%
1	1124	885	21.4%	20.6%
2	687	565	13.1%	13.2%
3	450	425	8.6%	9.9%
4	307	240	5.9%	5.6%
5	182	153	3.5%	3.6%
6	113	107	2.2%	2.5%
7	64	74	1.2%	1.7%
8	47	46	0.9%	1.1%
9	32	31	0.6%	0.7%
10	24	16	0.5%	0.4%
11	35	26	0.7%	0.6%
Total	5246	4295	100%	100%

Column 1 illustrates the number of suspects above and below age seventeen by criminal history, but does not help determine whether there is differential charge

substitution across different criminal histories. Column 2 presents the same data, but represents each frequency as a proportion of the total number of suspects on the same side of the boundary. The black bar for 0 priors, for example, indicates that 41% of all suspects below 17 years of age had 0 priors, and that 40% of all suspects above seventeen had 0 priors. Similarly, 21% of all suspects below seventeen, and 20% of all suspects above seventeen had 1 prior. The proportions are remarkably consistent across all criminal histories. These results show very little or no differential charge substitution across criminal history, providing empirical evidence against both the protection and slap-on-the-wrist theories (H5, H6).

3. Formal Processes of Charge Substitution: Felony Review

If the informal mechanisms of charge substitution considered in the prior section cannot explain the drop in offending at the age of majority, more formal or structural differences between the adult and juvenile justice systems may offer better explanations. Felony Review—which only applies to adult cases and not juvenile cases—may explain the drop in felony arrests (H7). We consider three sources of empirical evidence to test this hypothesis.

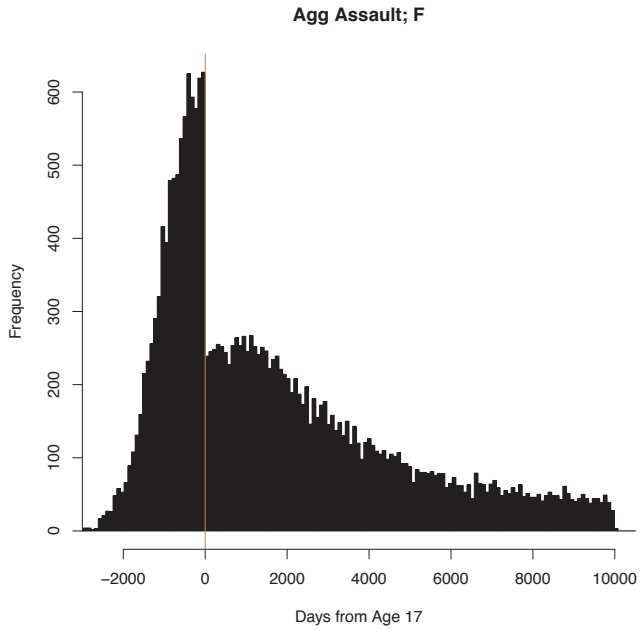
First, we observe a discontinuity in arrests at the age of majority for nearly all charges that are subject to felony review in the adult system and not in the juvenile system. Felony review applies to all non-drug adult crimes, and almost no juvenile crimes. Indeed, the data show that in juvenile cases, felony review was sought in just 3%, 0.1% and 4% of violent, property and other crimes respectively. In sharp contrast, felony

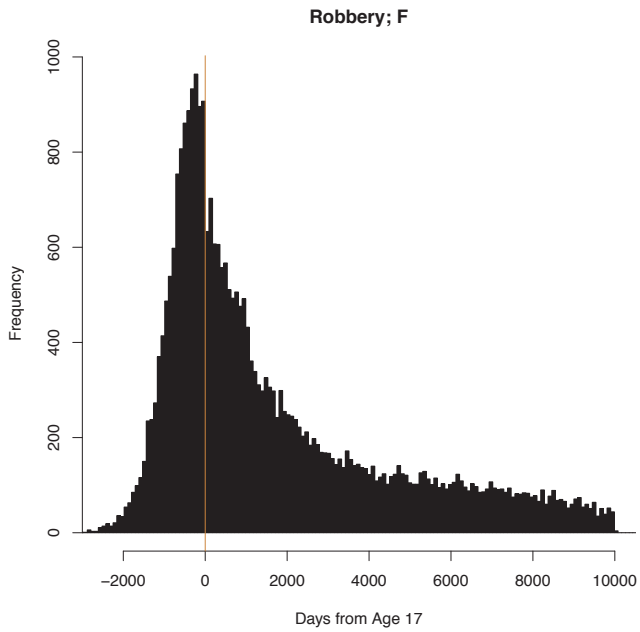
review was sought in adult cases 71%, 87% and 85% for violent, property and other crimes respectively.[34]

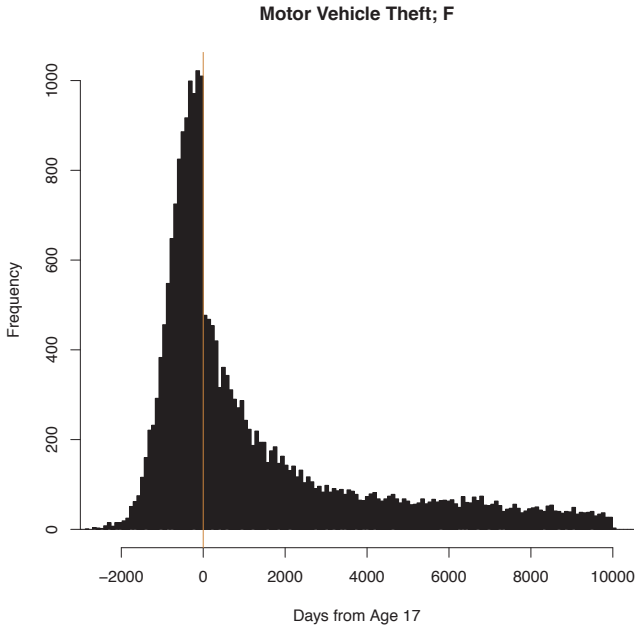
We constructed histograms on age with respect to the seventeenth birthday for all UCR categories for which there was significant volume around the age of majority. As Figure 2.8 reveals, large discontinuities were observed across five felony index crimes: aggravated assault ($p = .000$), robbery ($p = .000$), burglary ($p = .000$), larceny ($p = .000$), and motor vehicle theft ($p = .000$). We also observe large discontinuities in vandalism ($p = .003$) and disorderly conduct ($p = .004$). For nearly all of the histograms, almost no adjacent-bin pairs have a larger discontinuity, and only 2 to 4% of random shuffles generate distributions with larger discontinuities (Table 2.1, Columns 2, 3 & 6). The discontinuity in felony arrests appears to pervade the criminal code.

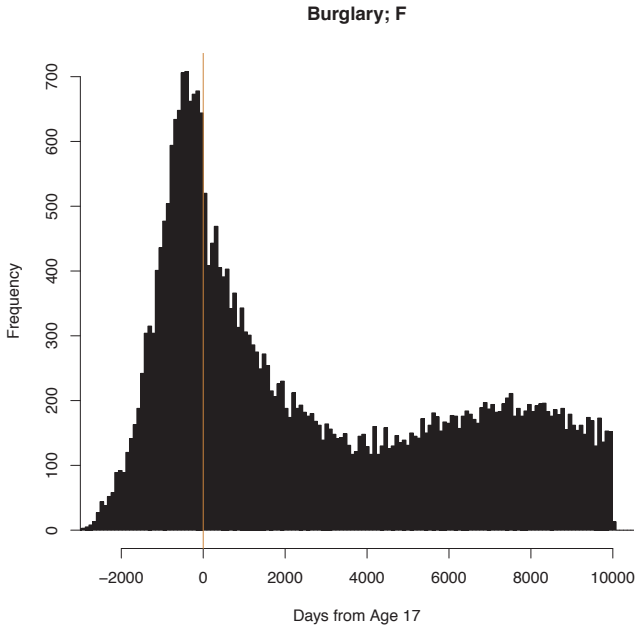
[34] The CPD data includes a flag to indicate whether an arrest was processed through felony review. This flag was used to compute these estimates. We are unsure of how consistently the CPD tracks this information, but the large differences in the proportion of cases confirms that adult arrests are processed through felony review far more frequently than juvenile arrests.

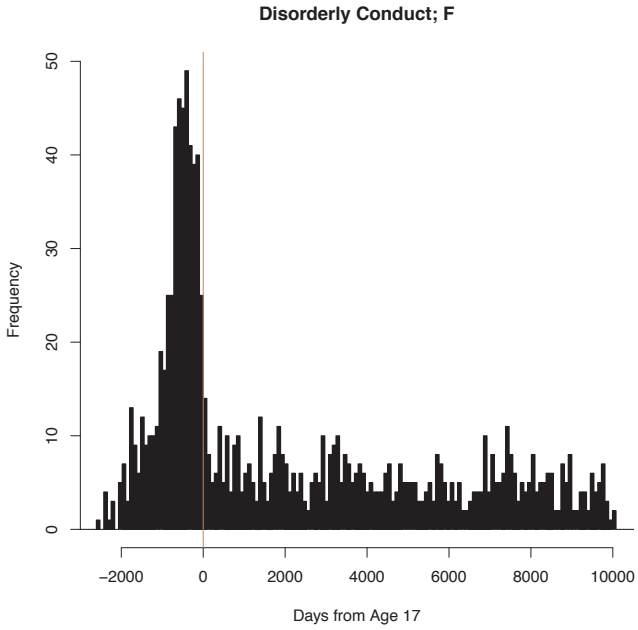
Figure 2.8: Arrests by UCR Categories With Felony Review by Days from Age 17

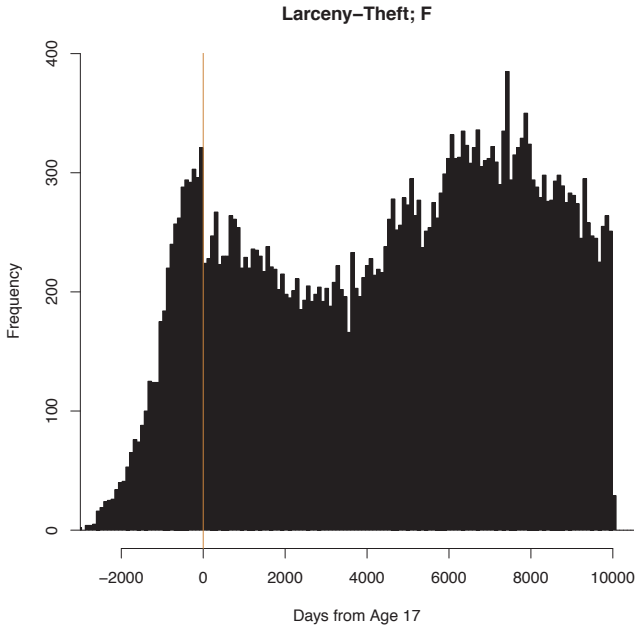


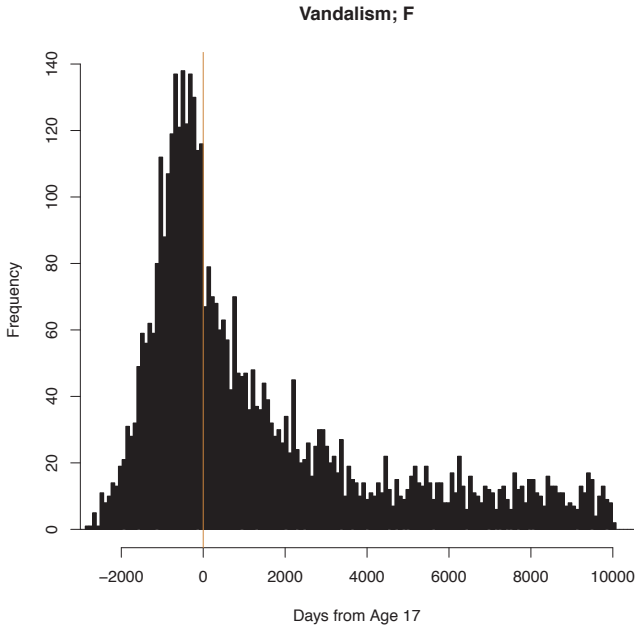






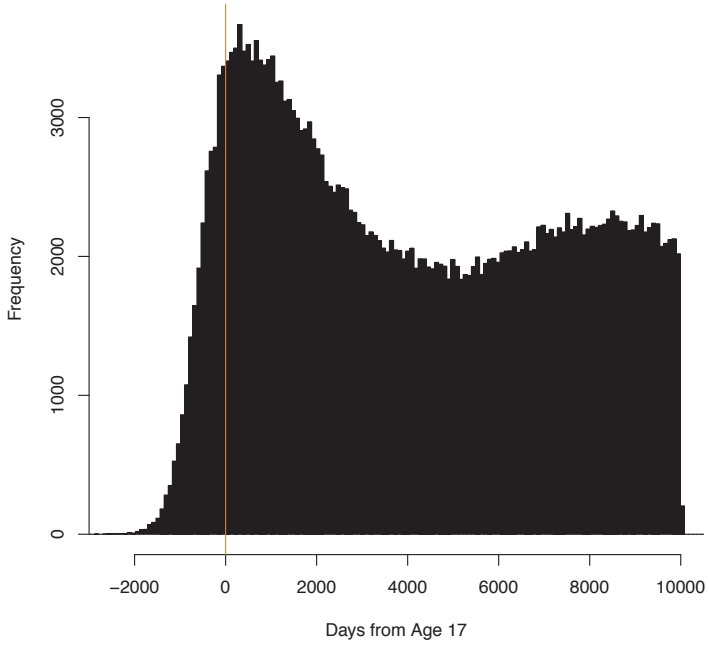






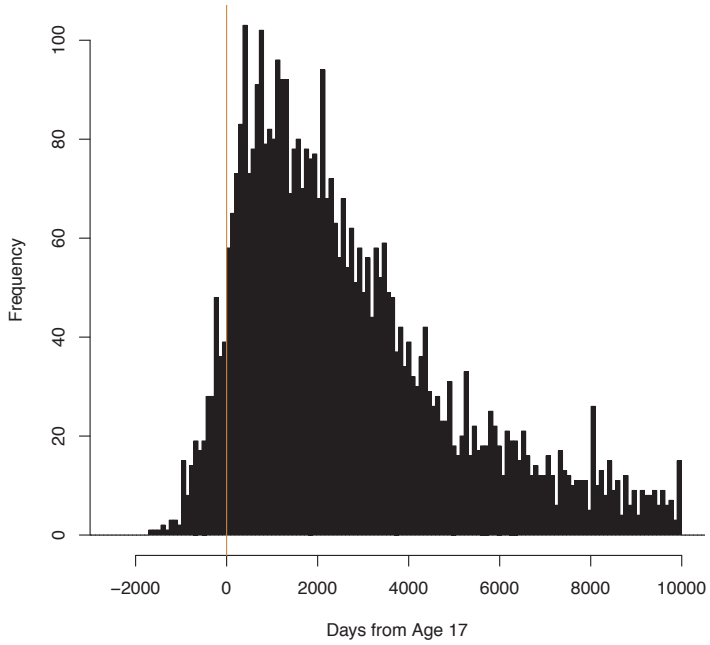
The second source of evidence in favor of felony review is the absence of a discontinuity among charges that are not subject to felony review in the adult or juvenile systems. Drug crimes are the only category of adult charges not subject to felony review. Indeed, the data show that just 0.1% of all juvenile drug arrests, and just 2% of all adult drug arrests resulted in felony review. Figure 2.9 reveals that there is no discontinuity in the distribution of felony drugs ($p = .394$). All adjacent-bin pairs have discontinuities that are larger (Table 2.1, Columns 2-3), and when bins are randomly shuffled to remove the correlation between age and crime, 74% of the distributions have larger discontinuities. To ensure this is not an artifact of statutory aggregation, we examined the distributions of the five most frequent felony drug crimes and the same patterns emerged (not shown).

Figure 2.9: Felony Drug Arrests by Days from Age 17



The third source of evidence in favor of felony review is the absence of a discontinuity among the small number of charges that are subject to felony review in both the adult and the juvenile system. Homicide charges against juveniles, for example, have always undergone felony review. We would, therefore, expect that the discontinuity for homicide would be small or non-existent. Figure 2.10 displays the distribution of arrests for murder and non-negligent homicide. Low volume and high variability across the distribution render interpretation difficult, but we observe little visual evidence of a discontinuity ($p = .274$). When the bins are randomly shuffled to remove the correlation between age and crime, 27% of alternative shuffles generate a larger discontinuity (Table 2.1, Column 6). Still, some caution is warranted as no other adjacent-bin pairs have a larger discontinuity (Column 2 & 3).

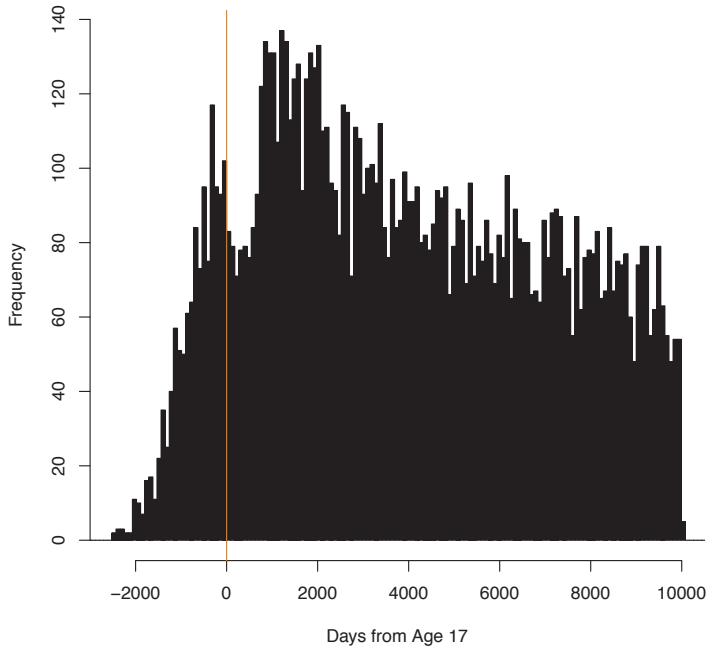
Figure 2.10: Murder & Non-Negligent Manslaughter By Days from Age 17



Only one UCR category in our data eludes the felony review pattern. Figure 2.11 reveals that there is no clear evidence of a discontinuity in charges labeled as "all other offenses (except traffic)." The McCrary test detects no statistically significant drop at the age of majority (p = .339), and many adjacent-bin pairs have larger discontinuities (Table 2.1, Columns 2 & 3). On the other hand, when the bins are randomly shuffled, only 3% of the distributions have a larger discontinuity (Column 6). Ultimately, even if there is no discontinuity here we do not find this theoretical inconsistency particularly troubling. First, we do not know how this category was constructed. Indeed, "all other offenses" consists of a miscellaneous list of crimes that do not fit into traditional UCR crime classifications. The ten most frequent crimes in this category are presented in Table 2.3. Second, the two most frequent charges in this classification are related to sex offender registries (which may very well be subject to juvenile felony review as a sex offense) and

escape from a peace officer. We suspect that felony review would not screen out a large number of either of these kinds of cases.

Figure 2.11: "All Other Offenses" Arrests by Days from Age 17



Taken together, the series of empirical tests conducted here provide significant evidence that Felony Review exerts a change in police arrest practices that generate the discontinuity in felony arrests at the age of majority. Nearly all charges that undergo felony review for adults and not juveniles show substantial drops in arrests. The only crime that does not undergo felony review in both the adult and juvenile system shows no evidence of a discontinuity. And the few crimes that undergo felony review in both the adult and juvenile system have little evidence of a discontinuity.

Table 2.3: Ten Most Frequent Charges in the "All Other Charges" Category

Rank	Statute	Statute Description	Frequency
1	720 ILCS 5.0/31-6-C	ESCAPE - FELON FROM PEACE OFFICER	1530
2	730 ILCS 150.0/3-A	VIOL SEX OFFENDER REGISTATION	1200
3	730 ILCS 150.0/6	FAIL REPORT CHANGE OF ADDRESS/SEX OFFENDER	942
4	720 ILCS 5.0/31-1-A-7	RESISTING OR OBSTRUCTING A PEACE OFFICER OR CORR EMP	838
5	15 ILCS 335.0/14B-B-1	ID CARD - POSSESS FRAUDULENT ID CARD	747
6	720 ILCS 5.0/31-7-F-5	ESCAPE - AID - PRISONER TO ESCAPE - PROBATION/PAROLE	666
7	720 ILCS 5.0/31-4	OBSTRUCTING JUSTICE	345
8	720 ILCS 5.0/32-10-A	VIOLATION BAIL BOND - CLASS 1 CONVICTION	326
9	720 ILCS 5.0/25-1-A-1	MOB ACTION - FORCE/2+ PERSONS	319
10	730 ILCS 150.0/3	SEX OFFENDER/ FAILURE TO REGISTER/FALSE INFO	304

These empirical observations are consistent with our previous empirical findings. First, the drop in felony arrests appears, at least in part, due to charge substitution from felonies to misdemeanors. Felony review may explain how this process takes place: when the felony review unit rejects felony charges for an adult suspect, the police remain free to charge a misdemeanor instead. In the juvenile system, there is typically no felony review process to screen out such charges. And there is little evidence that personal characteristics of the suspects influence charging through informal processes—the informal processes examined above—suggesting that certain adult suspects are screened out based upon the evidentiary characteristics of the case.

VII. Conclusion and Discussion

Our analyses provide evidence in favor of three empirical conclusions. First, the drop in felony arrests at seventeen in Cook County is not generated by a change in offender behavior. Second, the drop is more likely generated by a process of charge substitution, where officers file misdemeanor charges against adolescents who are just a few days over the age of majority, and file felonies against similar adolescents a few days under. Third, this charge substitution is likely driven by Felony Review, a charge screening policy in the Cook County State's Attorney's Office. The evidence suggests

that officers adjust their charging practices in anticipation of the standards and criteria of the review process.

No studies have rigorously examined Felony Review, but our results are consistent with limited anecdotal evidence about the process. One report found that, unlike the Chicago court system of the 1970s, which relied heavily on plea bargaining, the practice was "virtually eliminated" in the mid 1980s (Chicago Tribune 1985). This shift in court practice was attributed to, among other changes, the establishment of the Felony Review unit (Chicago Tribune 1985). Thus, at least at one point in time, Felony Review functioned similarly to the prosecutorial screening program in New Orleans, one of the most stringent screening policies in the country (Wright & Miller 2002). Second, the stringency of felony review has created tensions between the CPD and the state's attorney's office. Anecdotal evidence suggests that some police officers perceive that the state's attorney's office rejects cases to improve conviction rates, and as a result, releases dangerous criminals onto the street (Konkol 2013).

Our findings have important implications for at least three areas of the legal and criminological literature. First, the results expand our understanding of the effect of prosecutorial screening on the composition of cases prosecuted in the criminal justice system. Prior work in this area has focused on the predictors and downstream effects of charge declination. Though some scholars have acknowledged the possibility that screening may also have upstream effects on police discretion (Wright & Miller 2002), scholars have not examined this question empirically. Our analyses provide the first

empirical evidence that prosecutorial screening also affects the kinds of cases brought by the police.

Second, our results suggest that prosecutorial screening may serve as an effective regulatory mechanism for police. Importantly, our finding that felony review is an effective regulatory method is limited to charging decisions, and does not extend to the decision to arrest. Nevertheless, we consider this a useful example of how an important form of police decision-making can be regulated by a proximate and frequent screening process. An arresting officer calls Felony Review to obtain approval before filing felony charges. The prosecutor decides whether the charges are appropriate and typically informs the officer by phone or in person. Police officers thus receive immediate and direct feedback about the suitability of charges for criminal prosecution. Police officers engage this process frequently, nearly every time they conduct a felony arrest. Over time, police officers likely learn about the quality of evidence required to file felony charges. They may also learn about the kind of police activities that trigger evidentiary exclusion at trial. Officers have little reason to invest time and resources pursuing a dead end case through the review process when prosecution is the primary goal.[35] Thus, felony review provides proximate and frequent feedback to officers on their charging decisions. These kinds of feedback mechanisms might also be used to regulate other forms of police discretion.

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[35] In some case, officers may arrest a suspect exclusively for the purpose of arrest, and not for prosecution. Scholars have long acknowledged that the process of an arrest is a form of punishment even without formal prosecution (Feeley 1979).

As discussed earlier, prosecutorial screening has several organizational features that avoid some of the pitfalls of other methods of police regulation. First, the prosecutor's office is an external regulatory agent without the limitations of other external agents. Unlike judges, prosecutors have more substantive or recent expertise on the nature of police work. Unlike citizen review boards—which rarely have final decision-making authority—the prosecutor's refusal to pursue a case cannot be overruled by the police department.[36] And, unlike criminal prosecutions of police misconduct, which are publicly visible and highly contentious, charge screening is a low-visibility decision point that involves less salient conflicts of interest. Second, charge screening operates as a corrective form of regulation and thus avoids vague procedural rules common in judicial, legislative and administrative regulation. Through felony review, officers observe in concrete cases the evidence required for successful prosecution and the consequences of constitutional violations. Officers have less incentive to arrest or charge a suspect if they know the prosecutor will reject the charge anyway (Wright 2002). Third, in some cases, Felony Review provides a more proximal form of responsive regulation than courts, internal affairs, and civilian review boards because it takes place frequently and the outcome of the process is known to the police officer immediately.

As we have already noted, we do not consider prosecutorial screening a panacea to all problems of police discretion. Our findings suggests that, in its current form, Felony Review primarily influences charging decisions. We presume, therefore, that its effects

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[36] In very rare cases, the police department can file felony charges without the approval of the felony review unit through a first deputy override. But, if the prosecutor's office decides not to prosecute the case then it is dismissed.

are limited to the criteria of charge screening, which typically focus on the quality of evidence against the suspect, and the presence of constitutional violations. We also suspect that Felony Review might have a larger effect on arrest decisions if all charges, and not just felony charges, were subject to screening. Since misdemeanors are not subject to felony review, police officers can always charge an arrestee with a misdemeanor if felony charges are rejected. We expect this effect to be muted in departments that heavily emphasize arrest statistics as a performance measure for officers. Indeed, officers may continue to arrest in low quality cases to "get credit" for the arrest even if it will be dropped later on. And it is important to keep in mind that police officers do not always conduct arrests to facilitate a successful prosecution. They may, for example, arrest someone to defuse a violent confrontation, and there is little reason to expect that charge screening will regulate these kinds of decisions.

Moreover, the effect of screening on police behavior depends upon the standards and policies of the prosecutor's office. Prosecutors have incentives to apply unnecessarily stringent standards to avoid weak cases that threaten conviction rates. Standards that are too stringent will discourage police from conducting warranted arrests and filing supported charges. Prosecutors, on the other hand, may also have incentives to apply low standards, which encourage higher charges that are used as bargaining chips in plea negotiation (Bibas 2009). Standards that are too low will encourage police to conduct unwarranted arrests and file unsupported charges.

Our results also have implications for the research literature on criminal justice disparities. Specifically, the results provide evidence of age-related charging disparities.

The police appear to file less severe charges against suspects just over the age of majority than similar suspects who are just under. Two important data limitations restrict our ability to explore the disparity in greater depth. First, we cannot determine whether the disparity represents overcharging of juveniles, or undercharging of adults. This normative question cannot be resolved without additional information about the quality of evidence in each case. Future work may conduct a comprehensive review of the case files in a random subsample of similar juvenile and adult cases to assess whether the higher charges filed in juvenile cases, or the lower charges filed in adult cases, are better supported by the evidence. Second, we lack data on downstream phases in the criminal justice system, and thus, cannot determine whether differential charging produces differential outcomes later on. Subsequent phases in the criminal justice system provide several opportunities for officials to screen out inappropriate charges in juvenile cases and to amend charges in adult cases. In the juvenile system, a juvenile probation officer may screen out inappropriate charges prior to the first appearance in court. An assistant state's attorney or juvenile court judge may also dismiss inappropriate charges at the beginning of the judicial proceeding. In the adult system, the state's attorney may add criminal charges after the initial charges are formally filed by the police. Future work can trace cases from arrest to judicial disposition to determine if differential charging practices ultimately produce differential judicial outcomes.

Our analyses leave several additional questions unanswered. Why, for example, has the effect of prosecutorial screening not been observed in other jurisdictions? It is possible that prosecutorial screening affects police arrest and charging practices in many

jurisdictions, but the structure of the screening process renders detection of the effect difficult. In the current investigation, we are only able to detect the effects of felony review because of a sharp policy discontinuity that subjects adult cases, and not juvenile cases, to felony review. Our use of histograms with narrow 60-day bins is also important. The discontinuity is more difficult to detect with the more typical bin size of one year. It is also possible that prosecutorial screening only has an effect on police behavior if certain organizational characteristics are present. We suspect at least two features of criminal justice in Chicago play an important role. First, charge screening is conducted by an independent charge-screening unit. Independent screening units may perform more stringent review and decline a larger number of cases (Wright & Miller 2002). Second, the Chicago police are responsible for filing charges, and the prosecutor's office screens charges before the charges are filed. Screening may exert an especially large effect in Chicago because police must contact the prosecutor's office directly to seek approval before filing charges. If so, our results may not extend to all jurisdictions. In Philadelphia, for example, the arresting officer transfers the case to a detective who is then responsible for contacting the prosecutorial screening unit. Under this arrangement, it is less likely for the arresting officer to receive the kind of frequent and proximal feedback to affect his or her decision-making.

Chapter 3. An Empirical Study of Criminal Discovery: The Effects of Open File

Ben Grunwald

Abstract

The discovery rights of criminal defendants in federal court and a substantial number of states are severely restricted. As a result, many defendants must negotiate plea agreements in the dark and are poorly prepared for trial. Several states have recently taken dramatic steps to reform discovery by entitling defendants to nearly all of the prosecution's files, including investigative materials generated by the police. Yet scholars have given insufficient theoretical and empirical attention to these innovations. This paper begins by developing a theory of criminal discovery. It then conducts the first systematic empirical investigation of the effects of criminal discovery by examining the 2004 adoption of open file in North Carolina. A series of difference-in-differences models comparing superior and district courts provide little evidence that open file promoted judicial efficiency by reducing the trial rate. They also provide little evidence that open file produced more favorable outcomes for defendants by increasing the dismissal rate. The paper concludes by identifying avenues for future research to better understand the pathways by which expansions in criminal discovery affect case outcomes.

I. INTRODUCTION

At common law, defendants had no legal right to review the prosecution's evidence in criminal cases. The Supreme Court later established a constitutional right to criminal discovery in *Brady v. Maryland*, but the courts have narrowly construed its scope to exclude inculpatory evidence and have attached a stringent materiality standard that is rarely satisfied in practice. In many jurisdictions, *Brady* only attaches at trial, and thus, does not apply to the vast majority of cases, which are disposed by guilty plea. Legislatures have granted pre-trial discovery through statute, but the federal system and a substantial number of states retain restrictive statutory regimes that do not provide timely disclosure of witness lists, prior statements, tangible objects and other evidence. As a result, many criminal defendants are negotiating plea agreements in the dark and are poorly prepared for trial.

Responding to this problem, the North Carolina General Assembly enacted a dramatic expansion in criminal discovery in 2004 called full open file. In doing so, the state "moved from a highly traditional, restrictive discovery procedure that guaranteed only minimal disclosure to the defense" to a system that grants access to nearly all of the prosecution's files, including materials generated by law enforcement during the investigation (Mosteller 2008: 263). According to one legal commentator, the statute established the "broadest criminal discovery rights and duties in the nation" (Moore 2012: 1380). Since 2004, several other states have enacted similar statutes including Texas and Ohio.

Scholars, attorneys, and policy groups have widely advocated for open file discovery (e.g., Mosteller 2008; Moore 2012; Smith 2008; Yaroshefsky 2012; Medwed 2010; Burke 2009; Gregory 2012; Sarokin & Zuckerman 1991; Justice Project 2007). They forcefully argue that expansions in criminal discovery improve the accuracy of trials and plea agreements because defendants are better prepared to challenge the prosecution's evidence (Justice Project 2007; Schulhofer 1998; McMunigal 1988; Ostrow 1981). Presumably, this means that in practice defendants obtain more favorable case outcomes. Scholars and policy groups also argue that open file promotes judicial efficiency. They reason that information asymmetries preclude plea agreements by causing prosecutors and defendants to reach different estimates of the expected outcome of trial. On this view, open file decreases trials by reducing information asymmetries between the parties (Fox 2013; Moore 2012; Rosen 2006; Rayner 2004; Roberts 2004).

While scholars and policy groups have speculated about the effects of criminal discovery, they have engaged in relatively little extended theoretical analysis (but see Douglass 2001). Drawing on a larger body of work on civil discovery (e.g., Bebchuk 1984; Shavell 1989; Hay 1994), this paper develops a theory of criminal discovery that predicts the effects of open file on the content of settlements and the probability of trial. At least three important features of the criminal justice system must be accounted for to properly theorize criminal discovery. First, unlike in civil court where a broad right to discovery attaches at the start of litigation, criminal discovery generally proceeds in two distinct phases: the parties enjoy limited pre-trial discovery rights that expand once the case goes to trial. Second, high caseloads and tight criminal justice budgets mean that

natural incentives in the plea negotiation process are insufficient to ensure complete voluntary discovery without legal requirements. And third, unlike civil litigants who maximize profit and minimize costs, professional norms encourage many prosecutors to seek justice in addition to maximizing convictions.

The resulting theory of criminal discovery makes three core empirical predictions. First, expanding discovery increases the dismissal of cases where available evidence fails to support any of the charges filed. Second, expanding discovery leads to more lenient plea agreements. Third, and contrary to the predictions of scholars and policy groups, expanding discovery is unlikely to decrease the number of cases that go to trial. Indeed, increased disclosure of evidence favorable to the prosecution leads to fewer trials, but prosecutors already have strong incentives to disclose such evidence voluntarily even in the absence of legal requirements. Open file discovery has a bigger impact on prosecutorial disclosure of evidence unfavorable to the prosecution. But these disclosures are unlikely to encourage settlement and may sometimes have the reverse effect if the parties' estimates about the expected outcome of trial are biased (Babcock et al. 1995). If open file does in fact lead parties to settle and avoid trial, it is not due to decreased information asymmetries in particular cases, but rather due to a systemic increase in trust among defendants that additional relevant evidence will not be disclosed as a result of a decision to go to trial (Shavell 1989).

This Chapter conducts a partial test of this theory of criminal discovery by examining the first and third predictions.[37] North Carolina offers an ideal research setting for this purpose. Its open file law has received significant attention from both policy advocates and legal scholars (Mosteller 2008; Moore 2008). The state's abrupt shift from a highly restrictive discovery statute to the most expansive in the country increases the likelihood of a detectable effect. And unlike more recent open file statutes, North Carolina's law is over a decade old and most details about its implementation are already settled.

Estimating the effect of changes to the rules of discovery poses several difficult challenges. First, discovery is regulated by statute and constitutional law. Thus, changes to the rules of discovery tend to affect all courts in a given jurisdiction at the same time. To overcome this challenge, I exploit the fact that North Carolina's open file statute only applies to felony cases and not misdemeanors.[38] To estimate the effect of open file in the first few years of its operation, I fit a series of difference-in-differences models using district courts, which primarily handle misdemeanor cases, as a comparison group for superior courts, which primarily handle felonies. This approach provides useful correlational evidence about changes in case outcomes after open file went into effect. Second, identifying the effect of open file on case outcomes over time assumes that the composition of cases in the criminal justice system remains relatively stable. As I discuss

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[37] Data limitations preclude an examination of the second prediction.

[38] I also attempted to use courts in adjacent states and courts in adjacent counties in adjacent states as controls, but their outcome data did not follow a similar trend to that of the North Carolina courts prior to the enactment of open file.

in greater detail below, I exclude from the analysis a subset of crimes for which there is an observable compositional change during the study period—primarily drug and fraud-related offenses.

Consistent with theoretical prediction, my difference-in-differences models provide little evidence that open file had an effect on the trial rate in one direction or the other. While I cannot rule out all theoretically significant effect sizes due to statistical imprecision, most of the coefficients are small, and contrary to the expectation of scholars and policy advocates, some are positive. But inconsistent with theoretical prediction, the models provide little evidence that open file affected the rate of plea agreements and dismissals.

These findings are consistent across several alternative model specifications. First, I refit the models excluding the most serious felonies like murder, homicide, rape, and robbery. Focusing on less severe charges increases the similarity between cases in the treatment and control group. Second, I exploit the fact that some misdemeanor cases are appealed from the district court and disposed *de novo* in the superior court. These cases provide an opportunity to estimate the effect of open file using a control group of cases that are processed in the same court as cases in the treatment group. Third, I exploit the fact that certain district attorneys in North Carolina established open file policies prior to 2004. Given that the statute had a smaller impact on discovery practice in these counties, we should expect any observed effects to be smaller there. And fourth, I refit the models excluding counties with a public defender's office under the assumption that, as repeat

players (Eisenstein & Jacob 1977), public defenders likely obtained more voluntary discovery than private defense attorneys before the open file statute.

What explains the surprising empirical result that open file did not promote more favorable outcomes for defendants through an increase in dismissals? As I argue below, there are significant theoretical grounds to expect that open file has a larger effect on the content of plea agreements than on the probability of a dismissal. Future research should focus on the effects of discovery on the content of plea agreements using charge-level data, which will likely reveal larger effects.

The remainder of the Chapter proceeds as follows. Section II describes and contextualizes the discovery regime in North Carolina prior to 2004 in relation to the regimes in other states. It shows that North Carolina's discovery statute was representative of many other jurisdictions with narrow discovery regimes.

Section III provides a detailed account of the open file statute. It begins by describing the legislative history that led to its enactment and identifies categories of evidence that were not discoverable beforehand, but are now as a result of the law. Prior scholarly work on the open file system in North Carolina has focused on the statutory language and legal rules of the system. I supplement the existing literature by describing how open file works in practice. This discussion is enriched by qualitative data collected through interviews with prosecutors and defense attorneys in North Carolina, as well as primary drafters of the law.

Section IV develops a theory of criminal discovery, which is used to predict the effect of open file on case outcomes in North Carolina. Section V outlines the data and

design, and section VI provides the results. In section VII, I interpret the results with respect to the theory of criminal discovery outlined earlier in the paper, and suggest avenues for future research.

II. TRADITIONAL CRIMINAL DISCOVERY

A. Historical Background

Under the common law, American courts recognized no authority to order pretrial discovery (State v. Taylor 1990; Criminal Practice Manual 2013). There were, therefore, no formal discovery requirements on prosecutors or defendants up to the beginning of the twentieth century (Moore 1968). Just a few decades later, however, a majority of states had adopted at least some formal pretrial discovery in criminal cases (LaFave et al. 2014). A few states required the prosecution to provide the names of witnesses it intended to present to trial (Wigmore 1940). Others also required the defendant to give advance notice of an alibi defense (Epstein 1964). And a significant number of state courts recognized an inherent discretionary power to require disclosure of certain evidence (Note 1947) that was "essential" to the defendant's "capacity to meet the prosecution evidence" (LaFave et al. 2014). By the middle of the 20th century a majority of jurisdictions provided some discovery, but it was "treated primarily as an exceptional practice for a limited group of situations" (LaFave et al. 2014).

Expansion in civil discovery occurred more rapidly. Prior to the 1930s, some states had already experimented with more liberal approaches to civil discovery (Subrin 1998; Ragland 1932). And in 1938, the federal system adopted the Federal Rules of Civil Procedure, under which Rule 26 provided broad discovery in federal civil cases for "any

matter, not privileged, which is relevant to the subject matter involved in the pending action" (Wright et al. 2014; Rosenberg 1989; Hazard 1989). Within a few decades, broad civil discovery was available in nearly every state (Oakley & Coon 1986). The primary goals of these reforms were to diminish trial surprise and encourage settlement. There were also other parallel developments during this period that encouraged settlement, including the motion in limine, which clarified to the parties whether certain evidence would be admissible at trial (Wright et al. 2014).

The expansion in civil discovery also triggered a public debate on criminal discovery (LaFave et al. 2014), but several perceived differences between criminal and civil cases hindered reform. The courts had interpreted the Fifth Amendment right against self-incrimination to prohibit disclosure requirements on the defendant (Brennan 1963). As a result, early liberalization of criminal discovery focused on prosecutorial disclosure. Even these reforms were limited by the perception that asymmetric discovery requirements gave defendants an unfair advantage (Moore 1968; United States v. Garsson 1923; State v. Tune 1953). Courts and scholars also feared that criminal defendants were more likely to abuse discovery by intimidating witnesses (Brennan 1963) or by adjusting their testimony to comport with the prosecution's evidence (Brennan 1963; State v. Tune 1953; Commonwealth v. Caplan 1963).

Since the middle of the 20th century, there has been a clear movement towards broader criminal discovery at both the state and federal level (Brown 2005). Yet, the discovery rights of criminal defendants in most jurisdictions remain substantially narrower than that of civil litigants (Easton & Bridges 2008). The most prevalent

expansions in defendant discovery rights have focused on evidence that the prosecution will present at trial. Thus, most reforms have promoted disclosure of evidence favorable to the prosecution rather than to the defense (LaFave et al. 2014). The prosecution's right to discovery also remains limited and typically hinges on whether the defendant has requested discovery from the prosecution.

B. Constitutional Criminal Discovery

The Federal Constitution provides a baseline due process right to discovery in all criminal cases. The courts have narrowly interpreted the scope of this right, and numerous legal commentators have questioned whether the doctrine affords meaningful discovery.

Several early cases established that the Constitution prohibited prosecutors from knowingly introducing perjured testimony at trial (Mooney v. Holohan 1935; Napue v. Illinois 1959). In 1963, the Supreme Court extended this narrow rule in *Brady v. Maryland* to establish a limited constitutional right to discovery of material and exculpatory evidence. In *Brady*, a defendant and co-conspirator were both charged with murder. Prior to trial, the defendant had requested to examine all of the prior statements of the co-defendant. At trial, the defendant argued he had participated in the crime, but his co-conspirator had killed the victim. After the defendant was convicted and sentenced, he learned that the prosecution had withheld one of the co-conspirator's prior statements, in which he had confessed to killing the victim. The Supreme Court held that the prosecution's failure to disclose exculpatory evidence material to guilt or punishment, which was specifically requested, violated the defendant's right to due process. The Court

emphasized that the violation was "irrespective of the good faith or bad faith of the prosecution" (Brady v. Maryland 1963: 1197).

Subsequent cases have clarified the materiality standard. In *United States v. Agurs* (1976) the Supreme Court considered a defendant's constitutional right to disclosure of *Brady* material that was not requested or was requested in only general terms. The defendant was convicted of murder after raising an unsuccessful self-defense claim at trial. The defendant later discovered that the victim had a criminal record and argued that the prosecution's failure to disclose the record violated due process. The Court noted that prosecutorial failure to respond to a "specific and relevant request" is "seldom, if ever, excusable" (U.S. v. Agurs 1976: 106). But when there is no request, or only a general request, the standard is higher. The failure to disclose only violates the Constitution if it "creates a reasonable doubt that did not otherwise exist" given all of the evidence presented at trial (U.S. v. Agurs 1976: 112).

The Supreme Court later collapsed the formal distinction between specific requests, general requests and non-requests, and has adopted one uniform standard of materiality. In *United States v. Bagley* (1985), Justices Blackmun and White issued separate opinions that, together, formed the majority. Blackmun and White asserted that all non-disclosure cases should be adjudicated based upon one standard of materiality: whether there is "a reasonable probability" that disclosure would have led to a different result of the proceeding (U.S. v. Bagley 1985: 682). The opinions disagreed, however, on the continuing significance of specific request cases. Blackmun maintained that specific requests raise special materiality considerations because "the more specifically the

defense requests certain evidence . . . the more reasonable it is for the defense to assume from nondisclosure that the evidence does not exist, and to make pretrial and trial decisions on the basis of this assumption" (1985: 682–83). Justice White, in contrast, saw no reason to "elaborate on the relevance . . . of the specificity" given "the flexibility of the standard and the inherently factbound nature of the cases" to which it applies (1985: 685). Lower federal courts and a number of states continue to recognize the distinction between specific and general requests (LaFave et al. 2014), and some states provide a substantially higher standard for non-specific request cases (State v. Laurie 1995).

Other cases interpreting *Brady* have clarified the meaning of exculpatory evidence. The exculpatory standard includes impeachment evidence, which is used to undermine the credibility of witnesses at trial. In *Giglio v. United States* (1972), a defendant was charged with passing forged money orders. One Assistant United States Attorney (AUSA) promised immunity to a co-conspirator if he agreed to testify against the defendant. Another AUSA who was unaware of the agreement tried the case against the defendant. At trial, the co-conspirator stated he had not received inducement from the government to testify. The new AUSA on the case was unaware of the prior agreement, and did not correct the witness' false testimony. The defendant was convicted largely on the co-conspirator's testimony. The Supreme Court analyzed the case under *Brady* and held that the undisclosed impeachment evidence was both material and exculpatory.

In addition to extending *Brady* to impeachment evidence, *Giglio* also clarified that *Brady*'s disclosure requirements extend to information not readily available to the prosecuting attorney. *Giglio* implied that the prosecuting attorney has some obligation to

obtain and disclose *Brady* material held by other prosecutors in the office. Subsequent cases have clarified that prosecutors must obtain *Brady* material "known to others acting on the government's behalf . . . including the police" (Kyles v. Whitley 1995: 437-38).

The Supreme Court has not yet fully clarified the timing requirements for *Brady*. The lower courts generally agree that in cases that go to trial *Brady* is satisfied if the evidence is disclosed "in time for its effective use at trial" (United States v. Higgs 1983; United States v. Smith Grading and Paving, Inc. 1985; State v. Taylor 1996). For many kinds of evidence, disclosure at trial appears to be sufficient (LaFave et al. 2014). In the vast majority of cases that do not go to trial and are instead disposed by guilty plea, it is likely that *Brady* does not apply. Prior to 2002, at least five federal circuit courts held that prosecutors must disclose *Brady* material prior to a plea agreement under the theory that pleas are otherwise involuntary (United States v. Avellino 1998; Sanchez v. United States 1995; United States v. Wright 1994; White v. United States 1988; Miller v. Angliker 1988). But in that year the Supreme Court unanimously held in *United States v. Ruiz* (2002) that *Brady* does not require the disclosure of impeachment evidence prior to plea agreements. The Court explained that a defendant who pleads guilty "forgoes . . . a fair trial" and other "accompanying constitutional guarantees" (U.S. v. Ruiz 2002: 628). The Court also noted that a pre-plea *Brady* right "could seriously interfere with the Government's interest in securing ... guilty pleas ... to secure the efficient administration of Justice" (U.S. v. Ruiz 2002: 632). Characterizing *Brady* as a trial right, the Court found no constitutional right to pre-plea disclosure. It is possible that *Ruiz* does not extend to non-impeachment evidence. But the Court has already rejected a functional

distinction between impeachment and non-impeachment exculpatory evidence with respect to *Brady* in the past (U.S. v. Bagley 1985; Friedman v. Rehal 2010). And in the wake of *Ruiz*, many courts and commentators have concluded that *Brady* does not provide a right to pre-plea disclosure of non-impeachment evidence (U.S. v. Conroy 2009; U.S. v. Mathur 2010; Jones v. Cooper 2002; Friedman v. Rehal 2010; Gregory 2012; Justice Project 2007).[39] Even if *Brady* were extended to cases disposed by plea, the relevant standard would be so stringent that few defendants could meet it anyway (Douglass 2001).

C. Statutory Criminal Discovery

Brady is supplemented by discovery statutes, which vary widely in scope. Commentators often group provisions regulating defendants' discovery rights in relation to two models. First, the Federal Rule of Criminal Procedure 16 is the narrowest. It only grants defendants discovery of their own statements and criminal record, and "a limited list of evidence that is either material to the defense or that the prosecution intends to introduce" at trial (LaFave et al. 2014: §20.2(b)). Over a dozen states—and North Carolina prior to 2004—modeled their defense discovery rights on the federal rule. Second, the American Bar Association has published standards for criminal discovery. A dozen states provide broader defendant discovery than the federal rule by partially embracing the first edition of the ABA standards (ABA 1970). Another twenty states

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[39] One commentator notes that the "Supreme Court has severely *restricted Brady's* role in preplea discovery, if it did not eliminate it altogether." But, he also identifies "a narrow opening for the court to rule that due process requires a prosecutor to disclose evidence supporting factual innocence prior to a guilty plea" (Cassidy 2011: 1444).

provide even broader defendant discovery by embracing the standards more fully. Today, a small number of states—including North Carolina—provide the most expansive defendant rights modeled on the third edition of the ABA standards (ABA 1996; LaFave et al. 2014).

Commentators have similarly grouped prosecutorial discovery statutes in relation to the federal rule. Federal Rule 16 provides for narrow discovery rights for the prosecution by requiring advance notice of alibi and insanity defenses, and by requiring disclosure of expert testimony and tangible evidence that the defendant intends to present at trial. A majority of the states have adopted similar provisions, while the rest have adopted broader ones (LaFave et al. 2014).

1. Witness Lists

In the federal courts and many state jurisdictions, the defendant has no statutory right to discover the names and personal information of the prosecution's witnesses. In those jurisdictions, the trial court has the discretion to order such disclosure, but the defendant bears a heavy burden to show that disclosure is necessary (see, e.g, U.S. v. Alex 1992; U.S. v. Taylor 1989). This restrictive approach is based on fears that criminal defendants may interfere with or intimidate the prosecution's witnesses in advance of trial. Roughly two-thirds of the states provide broader discovery. Some states require the prosecution to disclose all its witnesses for the case in chief, while others also require disclosure of potential witnesses for rebuttal. A very small number of states go further by requiring disclosure of the names and addresses of any persons that have knowledge of

relevant facts even if the prosecution has no intention of introducing them at trial (LaFave et al. 2014).

Prior to 2004, North Carolina was among the most conservative states because it imposed no requirement on the prosecution or defendant to disclose a list of intended lay witnesses in advance of trial (Rubin 2004).

2. Prior Statements

In a large number of cases, the primary form of evidence available against the defendant is witness testimony. Nearly all jurisdictions provide defendants with the right to examine their own prior statements (Criminal Practice Manual 2013). Federal Rule of Criminal Procedure 16 and similar state provisions require discovery for relevant written or recorded verbatim statements by the defendant. They also provide discovery for the substance of relevant oral statements in response to an interrogation by a person the defendant knew was a government agent. States that have modeled their rule for defendant statements on the ABA standard have dropped the relevance requirement. The difference is minor, however, as courts have broadly interpreted the relevance standard to include all defendant statements made during the investigation of the crime (U.S. v. Caldwell 1974; LaFave et al. 2014). Prior to 2004, North Carolina required the prosecution to disclose all relevant written statements by the defendant, and the substance of oral statements regardless of to whom it was made (N.C.G.A §15A-903(a) 2003).[40]

[40] One exception to this rule was non-exculpatory oral statements made to a secret informant who would not testify at trial (N.C.G.A §15A-903(a)(2) 2003). Also, the state was required to divulge oral statements by the defendant to non-law enforcement the week before trial.

Discovery statutes are less generous for the statements of co-defendants. Federal Rule 16 and similar state statutes provide no right to the oral or written statements of co-defendants. Some states permit such discovery, but only where the co-defendants will be tried together, or where the state intends to introduce the statement at trial. Finally, many states permit defendants to discover both the recorded and oral statements of co-defendants without restriction (LaFave et al. 2014). Prior to 2004, North Carolina provided some discovery of co-defendant statements and was thus more liberal than the federal rule. But discovery was still narrow because it limited disclosure to statements the prosecution intended to introduce in a joint trial of the co-defendants (N.C.G.S. §15A-903(b) 2003).

All jurisdictions provide some discovery of the prior statements of witnesses the prosecution intends to present at trial. The conditions and timing of discovery, however, vary widely. In federal court, the Jencks Act prohibits the prosecution from disclosing prior statements by prosecution witnesses until after they have testified. And even afterwards, the defendant is only entitled to discover prior written statements that are signed or adopted by the witness, or "verbatim" transcriptions that were recorded contemporaneously. Oral statements that are not verbatim records are not discoverable. Statements disclosed under the Jencks Act must also "relate[] to the subject matter as to which the witness has testified." If statements are not provided until after testimony is given, the defendant may obtain a continuance to review them. The length of the continuance is left up to the discretion of the court (Levenson 2014) and there does not

appear to be any specific minimum (U.S. v. Stanfield 2004; Washington v. US 1985; U.S. v. Holmes 1983).

A number of states have adopted the language of the Jencks Act. Other states have adopted similar statutes that do not prohibit pre-testimony discovery of witness statements, but also do not require it. Roughly half of the states have rejected the approach of the Jencks Act entirely, and provide disclosure for written or recorded statements of prosecution witnesses, and roughly a quarter also provide disclosure of oral statements (LaFave et al. 2014). Prior to 2004, North Carolina had adopted the language of the Jencks Act (N.C.G.S §15A-903(f) 2003).

Discovery statutes are far less generous for the discovery of prior statements of witnesses the prosecution does not intend to present at trial. The vast majority of statutes do not require disclosure of such statements. A small number of states, however, do provide the statements of individuals who have knowledge of relevant facts. (e.g., Alaska R. Crim. P. 16(b)(1)(A)(i); Fla. R. Crim. P. 3.220(b)(1); Minn. R. Crim. P. 9.01(2)). Prior to 2004, North Carolina was among the most conservative states on this dimension, as it provided defendants no statutory right to the prior statements of witnesses the prosecution did not plan to present at trial.

3. Production of Physical Evidence

All discovery statutes provide some disclosure of physical evidence, which includes books, papers, documents, photographs, motion pictures, electronic recordings, weapons or any other tangible objects. The conditions for discovery, however, vary

widely. Nearly all jurisdictions provide defendants discovery of tangible evidence that the prosecution will use at trial, or which belong to the defendant.

Federal Rule 16 and a number of state provisions also enable defendants to discover tangible evidence that is "material to the preparation" of the defense (LaFave et al. 2014). When the defendant makes a request for a specific piece of tangible evidence, the burden is on the defense to show that the requested evidence satisfies the materiality standard. In practice, the meaning of materiality here is ambiguous, but mere relevance is insufficient. Some courts require the defendant to show the requested evidence "will play an important role in uncovering admissible evidence, aiding witness preparation, corroborating testimony, or assisting impeachment and rebuttal" (U.S. v. Felt 1979: 186; see also U.S. v. Ross 1975). The Supreme Court has limited materiality under Federal Rule 16 to evidence that undermines the state's case in chief (U.S. v. Armstrong 1996).

When the defendant makes a general request for "material" physical evidence—e.g., because he does not know what evidence the prosecution has—a "concrete showing of materiality is still required" (U.S. v. Ross 1975: 764). In practice, this materiality determination is left to the discretion of the prosecutor who must decide which physical evidence is material, and thus subject to discovery. Where the prosecutor has failed to disclose tangible evidence in response to a general request, Courts typically apply a "very high" standard of materiality similar to the *Brady* standard (LaFave et al. 2014). As in the *Brady* doctrine, prosecutors have substantial discretion in deciding whether to disclose evidence that is borderline material since they rarely lose on appellate review.

Prior to 2004, the North Carolina discovery provision for tangible evidence was consistent with Federal Rule 16. The defendant was able to discover any tangible evidence that was material to the preparation of the defense, intended for use by the prosecution at trial, or obtained from the defendant's possession (N.C.G.S. § 15A-903(d) 2003). As an example, the prosecution was only required to disclose the parts of the investigating officers' notes that were covered by the statutory categories (Rubin 2004).

4. Expert Evaluations and Tests

All jurisdictions provide the defendant with access to at least some medical examinations and scientific tests, but the conditions of disclosure vary (LaFave et al. 2014). Some states condition disclosure on the state's intention to present the results at trial (e.g., Oregon Rev. Stat. 135.815(1)). Federal Rule 16 and a number of states also provide such reports if they are "material to preparing the defense" (e.g., South Dakota Codified Laws § 23A-13-4; Tennessee R. Crim. P. 16(a)(1)(D); N.D. R. Crim. P. 16(a)(1)(D)). Some jurisdictions provide disclosure under an even broader condition that the relevant tests or reports were "made in connection with the particular case" (e.g., Maine. R. Crim. P. 16(b)(2)(B); Missouri R. Crim. P. 25.03(A)(5)).

Prior to 2004, North Carolina provided relatively liberal discovery of expert evaluations and tests. Defendants could discover the results or reports from any evaluations and tests "made in connection with the particular case" (N.C.G.S. § 15A-903(e) 2003). If no such reports were made, the court had the power to require the disclosing party to write one for purposes of discovery (State v. East 1997). They could

also discover the data or physical material that formed the basis of the analysis if the prosecution intended to present it at trial (N.C.G.S. § 15A-903(e) 2003).

The prosecution typically has narrow discovery rights with respect to expert examinations and tests. Most jurisdictions only require disclosure of materials that the defendant intends to present at trial (LaFave et al. 2014). Some also condition the prosecution's right on the defendant requesting similar discovery from the prosecution. Prior to 2004, North Carolina was in line with the majority of other states. The prosecution could discover an examination report or the underlying physical evidence if the defendant requested those materials and "reasonably expect[ed]" to present the examination results at trial (N.C.G.S. § 15A-905(b) 2003).

5. Criminal Record

Both Federal Rule 16 and the vast majority of states require the prosecution to provide the defendant a copy of his criminal record upon request (LaFave et al. 2014). A few discovery statutes do not expressly reference the defendant's criminal record, and thus, presumably leave disclosure up to the discretion of the trial court (Criminal Practice Manual 2013). As in the vast majority of other states, defendants in North Carolina prior to 2004 could discover their prior criminal record (N.C.G.S. § 15A-903(c) 2003).

In general, discovery statutes are less generous with respect to the criminal records of prosecution witnesses. Some statutes require disclosure in advance of trial. A few statutes go a step further by requiring the prosecution to disclose the criminal records of all potential trial witnesses (Easton & Bridges 2008). Prior to 2004, defendants in North

Carolina did not have the right to discover the criminal records of anyone other than themselves (State v. Brown 1982).

Taken together, North Carolina's discovery statute prior to 2004 was representative of narrow statutes in other states. The defendant and prosecution had no right to witness lists. Before trial, the defendant could only discover his own prior statements. Disclosure of trial witnesses' statements was not guaranteed until after they testified. And, even then, they were only discoverable if they were adopted by the witness or were contemporaneous and verbatim transcriptions. Defendants had no right to the prior statements of witnesses that the prosecution did not intend to present at trial. Tangible evidence was only discoverable if it was material to the preparation of the defense, intended for use by the prosecution at trial, or obtained from the defendant's possession. While the state discovery statute provided more liberal access to expert tests and defendants' criminal records, these rules were already common among most states by 2004.

D. Weaknesses in Traditional Discovery

Scholars have identified many problems with the traditional approach to discovery. For starters, the scope of discovery in many jurisdictions is too narrow and fails to cover evidence defendants need to negotiate guilty pleas and prepare for trial. *Brady*'s materiality standard is stringent. Courts often conclude that undisclosed exculpatory evidence is not material because it is cumulative of or outweighed by other evidence. Some courts have also limited materiality to admissible evidence, and thus denied *Brady* claims based on undisclosed materials that would have led to admissible

evidence (U.S. v. Morales 2014; Hoke v. Netherland 1996; Dennis v. Secretary 2015). *Brady* does not require disclosure of inculpatory evidence, which can be useful to develop a trial strategy and to challenge the prosecution's evidence (e.g., U.S. v. Kidding 1977). It also imposes weak timing requirements. Courts sometimes find no constitutional violation where material evidence is withheld until after the start of trial, and the doctrine is likely inapplicable to cases disposed by guilty plea. As discussed above, discovery statutes often extend beyond *Brady*, but in many jurisdictions they require only limited disclosure of witness lists, statements, and physical evidence.

Even when disclosure is legally required, it is not always carried out. Cultural norms in prosecutors' offices may overemphasize the maximization of convictions (Dunahoe 2005). At the extreme, prosecutors sometimes intentionally withhold discoverable evidence to protect conviction rates (Mosteller 2008). Intentional suppression is particularly likely in high-profile cases where prosecutors feel unusual pressure to obtain convictions. It is also more common where the prosecution receives exculpatory evidence late in the game after having investing months of effort into a case (Baer Forthcoming). Sanctions for discovery violations provide inadequate deterrence. Few violations are detected (Davis 2007; Editorial 2012), and when they are, professional discipline, civil liability and reversal of convictions are rare (Gregory 2012; Ridolfi & Possley 2010).

There are also more benign causes of discovery violations. A prosecutor may not know, for example, that specific evidence must be disclosed. The legal standards of

discovery are often vague and subject to disagreement.[41] Discovery rules demand prosecutors to make *ex ante* predictions about the materiality of exculpatory evidence before trial (Moore 2012; Gregory 2012). At that point, prosecutors are unlikely aware of all the evidence that will matter, particularly without knowing the defendant's trial strategy. High caseloads mean that prosecutors may also not have the time to examine their files for discoverable evidence (Gershowitz & Killinger 2011; Moore 2012). And prosecutors' discretionary judgment about discovery may be distorted by psychological biases (Burke 2009). As Professor Mosteller (2008) explains,

> [F]or a prosecutor who has reached the conclusion that the accused is guilty . . . there can be no true exculpatory evidence. If it is truly exculpatory, the case should be dismissed . . . . Otherwise, the evidence must be not really exculpatory, and therefore, is simply useful ammunition for the defense in the adversary battle of the criminal trial. Once the sweep of evidence has been examined and the prosecutor is committed to the rightness of the prosecution, *Brady* is not felt as a moral command, unless something stunning is observed.

These problems with the traditional approach to criminal discovery led North Carolina and other states to go open file.

III. OPEN FILE DISCOVERY IN NORTH CAROLINA

A. Legislative Background

North Carolina's first experience with open file discovery began in 1996 when the state legislature adopted open file for death row inmates seeking post-conviction review. The law's purpose was to increase the efficiency of post-conviction proceedings by

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[41] One study asked over thirty prosecutors to answer whether specific evidence was material under *Brady*, and found wide disagreement in a number of different factual circumstances (John Jay Clinic 2000).

providing "early and full disclosure ... so that [petitioners could] raise all potential claims in a single motion for appropriate relief" (State v. Bates 1998). The law gave access to the "complete files of all law enforcement and prosecutorial agencies involved in the investigation of the crime" (N.C.G.S. § 15A-1415(f)). Almost immediately, the state challenged the scope of the law, arguing that much of the prosecutorial file was protected under the work product privilege (State v. Bates 1998). The North Carolina Supreme Court rejected this reading and affirmed broad post-conviction access to the file (State v. Bates 1998).

As a result of the law, the convictions of at least ten death row inmates were reversed after they discovered the prosecution had failed to disclose exculpatory evidence (Mosteller 2008). One of these defendants, Alan Gell, was convicted of murder and sentenced to death in 1998 based on the testimony of two key witnesses. On post-conviction review, Gell's lawyers obtained the complete prosecutorial file and found interviews with the victim's brother and neighbor who had seen the victim alive after the date of the alleged murder. They also found a secretly recorded conversation in which one of the two prosecution witnesses stated the need to "make up a story" to tell the police about the murder. Gell's original conviction was reversed and he was acquitted on retrial (Mosteller 2008).

The Gell case led the North Carolina General Assembly in 2004 to extend open file beyond post-conviction to all felony cases prior to trial. The law requires the prosecution to provide the defense with the "complete files of all law enforcement and prosecutorial agencies involved in the investigation . . . or prosecution" of the case

(N.C.G.S. § 15A-903(a)(1) 2004). The "complete files" were defined broadly to include "the defendant's statements, the codefendants' statements, witness statements, investigating officers' notes, results of tests and examinations, or any other matter or evidence obtained during the investigation of the offenses" (N.C.G.S. § 15A-903(a)(1)). In practice, the law dramatically expanded defendants' access to law enforcement files and physical evidence. It also expanded access to prior witness statements and required the recording of all oral statements by witnesses (N.C.G.S. § 15A-903(a)(1); see also State v. Shannon 2007). The statute also provided the prosecution and defendant the right to seek a protective order against the discovery of specific items if disclosure would create a "substantial risk to any person or physical harm, intimidation, [or] bribery" (N.C.G.S. § 15A-908(a)).

Through these legislative changes, North Carolina "moved from a highly traditional, restrictive discovery [statute] that guaranteed only minimal disclosure to the defense . . . to a statute that entitles the defense to relatively full access to both prosecution and law enforcement files" (Mosteller 2008: 260). According to one legal scholar, the open file statute "created the broadest criminal discovery rights and duties in the nation" (Moore 2012: 1380).

Importantly, this dramatic shift in criminal discovery did not arise out of nowhere. On an *ad hoc* basis, a number of district attorneys had adopted "open file" policies in one form or another prior to 2004.[42] These policies varied by jurisdiction and were not as

[42] Through my own research, I have confirmed five such counties in North Carolina. Local open file policies appear in a number of jurisdictions in other states (*see e.g.*, Robinson v. State 1994;

expansive as the law enacted in 2004 (Klinkosum 2013; see also Douglass 2001). In the absence of a uniform statutory right to discovery, "open file" in some jurisdictions meant that the defense was "allowed to view what the prosecution and law enforcement had decided to place in their files and what the prosecution would allow the defense to see" (Klinkosum 2013: 29; see also Easton & Bridges 2008; Prosser 2006). Moreover, if a prosecutor failed to disclose information typically "covered by the policy, but not legally required, a defendant had little, if any, recourse" (Rubin 2004: 2). As a result, the 2004 open file statute likely expanded criminal discovery even in counties that already had open file policies.

The North Carolina General Assembly has made some "minor revisions" to the discovery statute since 2004 (Rubin & Grine 2013). In 2007, the legislature clarified police officers' duty to give all investigative files to the prosecution,[43] and permitted the prosecution to withhold the identity of confidential informants under certain circumstances. It also clarified that the requirement to disclose the files of all "prosecutorial agencies" includes "any public or private entity that obtains information on behalf of a law enforcement agency or prosecutor in connection with the investigation of the crimes committed" (Rubin 2007: 17). In 2011, the legislature established criminal sanctions against individuals who "willfully omit or misrepresent evidence or information

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State v. Crews 1989; People v. Bennett 1980; Luban 1993). Some assistant district attorneys may have also implemented an open file in individual cases (Joy 2007).

[43] Technically, this amendment effected little change in the legal obligations of law enforcement. It merely moved statutory language from one part of the code to another (Rubin 2007).

required to be disclosed under the open file statute" (Rubin 2011: 4). To my knowledge, no criminal prosecutions have yet been pursued.

B. The Effect of Open File Legislation on Statutory Discovery Rights

Historically, North Carolina had a narrow criminal discovery statute based on the Federal Rules of Criminal Procedure and the Jencks Act. This section outlines the effect of the law on discovery access to five main categories of evidence.

i. Lay Witness Lists.

Prior to 2004, neither the prosecution nor the defendant were required by statute to disclose a list of intended lay witnesses in advance of trial (Rubin 2004).

The open file statute substantially expanded discovery of witness lists. It created a new requirement that, at jury selection, the prosecution and defense must provide the names of witnesses they "reasonably expect[]" to call at trial (N.C.G.S. §15A-903(a)(3), 905(c)(3) 2004). Furthermore, access to the "complete" prosecutorial file in advance of trial gives the defendant contact information and prior statements for all witnesses interviewed by the government (N.C.G.S. §15A-903(a)(1) 2004). In most cases, the defendant can make reasonable predictions about which witnesses the state will present at trial.

ii. Prior Statements.

Prior to 2004, prosecutors were only required to provide relevant, adopted, written statements and verbatim, contemporaneously recorded oral statements of the defendant (N.C.G.S. §15A-903(a) 2003). They were required to disclose the statements of a co-defendant that the prosecution intended to introduce in a joint trial (N.C.G.S. §15A-

903(b) 2003). North Carolina's version of the Jencks Act prohibited disclosure of the statements of prosecution witnesses until after their testimony (N.C.G.S. § 15A-903(f)(1),(2) 2003). No discovery was required for the statements of any other witnesses that the prosecution did not intend to present at trial.

The open file statute dramatically expanded defendants' access to prior statements. First, it increased the number of oral statements available in the prosecutor's file by requiring the state to reduce all oral statements to writing. Second, it eliminated the narrow categories of discoverable statements (i.e., relevant, adopted written statements; verbatim, contemporaneously recorded oral statements) by providing discovery for all recorded statements. Third, it expanded the kinds of witnesses whose prior statements are discoverable by requiring disclosure for all individuals interviewed by the state during the investigation, including witnesses the prosecution does not intend to present at trial. North Carolina joined a very small minority of other states in doing so. Finally, the open file law required the state to disclose prior statements well in advance of trial so they are available during the plea negotiation process (N.C.G.S. §15A-903(a)(1) 2004; Rubin 2004).

iii. Production of Physical Evidence.

Prior to 2004, the prosecution was only required to disclose tangible evidence that was (1) material to the preparation of the defense, (2) intended for use by the state at trial, or (3) obtained from the defendant's possession (N.C.G.S. §15A-903(d) 2003).

The open file statute substantially expanded defendant discovery of physical evidence. First, the law likely increased the amount of tangible evidence in the

prosecutorial file by codifying a duty of law enforcement officers to provide the prosecution with all "materials and information acquired in the course" of a felony investigation (N.C.G.S. §15A-501(6) 2004).[44] Second, the law eliminated the narrow categories of discoverable tangible evidence and provided defendants the "complete files of all law enforcement and prosecutorial agencies involved in the investigation of the crimes committed" by the defendant" (N.C.G.S. §15A-903(a)(1) 2004). Perhaps most importantly, discovery of tangible evidence is no longer conditioned on the prosecutor's determination that the evidence is material to the preparation of the defense. As noted earlier, it is not clear whether prosecutors can do an effective job at this kind of screening. Now, the defendant can discover all physical materials regardless of the prosecutor's estimation of their importance in the case. This includes all documents created and all physical evidence collected during the investigation. Prior to 2004, for example, the prosecution was only required to disclose the parts of investigating officers' notes that were covered by the existing statutory categories. As a result of open file, all notes and arrest reports are now discoverable (Rubin 2004).

The statute had a smaller impact on the prosecution's right to discover physical evidence. Prior to 2004, the prosecution could discover tangible evidence that the defendant intended to introduce at trial if the defendant had already requested tangible evidence from the prosecution (N.C.G.S. §15A-905(a) 2003). The open file law relaxed the reciprocal discovery condition. Today, the prosecution can discover tangible evidence

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[44] This was technically the law prior to 2004, but it is possible that law enforcement agencies adhered less stringently to the rule beforehand (see State v. Pigott 1987).

if the defendant has requested any discovery at all from the prosecution (N.C.G.S. §15A-905(a) 2004).

iv. Expert Evaluations and Tests.

Prior to 2004, the prosecution was required to disclose the results or reports from any evaluations and tests "made in connection with the particular case" (N.C.G.S. §15A-903(e) 2003). If no such reports were made, the court had the power to require the disclosing party to write one for purposes of discovery (State v. East 1997). A defendant could also discover the data or physical material that formed the basis of an analysis if the prosecution intended to present the results at trial (N.C.G.S. § 15A-903(e) 2003).

The 2004 law left much of this framework in place, but made several modest changes. It expressly required the parties to create reports summarizing the analyses and results of expert examinations and tests, rather than leaving this up to the court's discretion (N.C.G.S. § 15A-903(a)(2) 2004; see also Rubin 2004). It required the state to give the defendant notice of any expert witness it "reasonably expects to call" at trial. And it required the state to provide the defendant with the curriculum vitae of the expert. The open file statute had a similarly modest effect on prosecution discovery of expert evaluations and tests. Prior to 2004, the prosecution could discover examination reports or the underlying physical evidence if the defendant had requested discovery of an examination from the prosecution and the defendant "reasonably expect[ed]" to present the examination or test at trial (N.C.G.S. § 15A-905(b) 2003). Open file made several minor changes. The prosecution can now discover examinations and tests whenever the defendant requests any form of discovery. The other expansions in prosecution discovery

mirrored those for defendants: a right to a report of the results of examinations and tests, advance notice of the defendant's intention to present the results of the examination at trial, and the curriculum vitae of the expert.

v. Criminal Record.

The open file statute had little effect on the discovery of defendants' criminal records. Defendants could discover their prior record prior to 2004 (N.C.G.S. § 15A-903(c) 2003), and they can do so today (N.C.G.S. § 15A-1340.14(f) 2004; Rubin & Grine 2013). The law had a larger effect on the disclosure of criminal records of state witnesses. Historically, the prosecution was not required under state statute to disclose the criminal record of state witnesses. (State v. Brown 1982). Under the open file law, if the state checks the record of one of its witnesses, it must be included in the "file" for the case, and is therefore, discoverable (Rubin & Grine 2013).

In summary, the 2004 open file statute in North Carolina dramatically expanded criminal discovery rights in the state. It required law enforcement to record the oral statements of witnesses. It provided discovery for all statements regardless of whether they are adopted or verbatim. It expanded discovery to the statements of all witnesses interviewed by the state, including those that will not testify for the prosecution. And it required the disclosure of all prior statements well in advance of trial so they are available during the plea negotiation process. The statute also expanded defendant access to tangible evidence. It increased the amount of evidence in the prosecutorial file by codifying a duty of law enforcement officers to provide the prosecution with all materials generated during the investigation. It eliminated narrow categories of discoverable

tangible evidence, providing defendants access to the "complete files of all law enforcement and prosecutorial agencies" involved in the investigation. The statute also enacted modest expansions in the discovery of materials related to scientific evaluations and tests.

C. Open File Discovery in Practice

The open file process begins with a police investigation of a felony crime. In a typical case, a police officer or detective interviews witnesses, and photographs and collects physical evidence from the crime scene. Officers are legally required to record all oral statements taken from any witnesses including the suspect (N.C.G.A. § 15A-903(a)(1) 2004). In some cases, the officer may send physical evidence to a crime lab for analysis.

Within a certain period of time, the officer is required to compile a file of all of the materials generated during the investigation. The statute stipulates that officers must provide the file to the district attorney's office "on a timely basis" (N.C.G.A. § 15A-501(6) 2004). Many districts have adopted local rules that impose a more specific timeline.

The statute expansively defines the materials that law enforcement must include in the file. Officers must transfer "all materials and information acquired in the course of all felony investigations" (N.C.G.A. §15A-501(6) 2004). This standard is frequently interpreted to cover "everything" collected and produced during an investigation, including arrest reports, evidence reports, handwritten notes, electronic notes, witness statements, physical evidence, forensic analyses, photographs, video recordings, legal

documents, the defendant's criminal record, and e-mails and text messages exchanged between officers (Campbell 2010; Klinkosum 2009). After assembling these materials, the law enforcement officer must number each page in the file before transferring it to the prosecution.

In many districts, the officer himself brings the physical files to the district attorneys office. In others, the files are stored electronically and are transferred online or by CD. After delivering the file, the officer must often sign a certificate indicating that all legally required materials have been provided. District attorney's offices may give the officer a letter documenting all of the items received and notifying the officer of the ongoing duty to deliver additional materials collected later on. If the case goes to trial, some district attorney's offices will also send a second letter to the officer indicating whether anything is missing from the file.

After the file is transferred, the assistant district attorney (ADA) assigned to the case must complete several tasks to prepare the file for discovery. The ADA must first determine if any materials are missing, and request these materials from law enforcement. When materials are missing from other agencies—e.g., victims services or forensic labs—the ADA usually requests them from law enforcement, rather than contacting the other agencies directly.

The ADA is required to add any materials to the file that the district attorney's office has generated during the investigation and prosecution. The same expansive principle of including "everything" applies at this stage except for privileged attorney

work product.[45] District attorney's offices have adopted different approaches to the disclosure of *Giglio* impeachment evidence drawn from internal police personnel files. Some offices do not include this kind of evidence as a matter of course and defendants must file a specific request. In others, an ADA automatically reviews the disciplinary records of officers involved in cases that proceed to a certain point in the process. In Mecklenburg, for example, a designated employee in the district attorney's office is responsible for checking whether officers have sustained violations related to truthfulness or bias on their record. Where an officer has such a record, a *Giglio* committee—which includes the district attorney, the deputy district attorney, and team leaders in the district attorney's office—votes on whether the violation should be disclosed.

The ADA may also identify any materials that would pose a "substantial risk" of "physical harm, intimidation, [or] bribery" if disclosed, and thus require a motion for protective order (N.C.G.A. §15A-908 2004). Little systematic data is available on the frequency of protective orders, but it appears to occur rarely in practice. One defense attorney interviewed for this study noted that "in ten years, [he's] only seen one case in his practice" where the prosecutor sought a protective order. Prior to 2004, the ADAs in many districts would also black out the names of witnesses and their statements unless their disclosure was required by law. In some counties they no longer do so in most cases.

Without systematic data on the frequency and volume of discovery, it is difficult to estimate how much more evidence was disclosed by prosecutors after the open file law

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[45] For example, prosecutorial work product and the personal information of some confidential witnesses are not subject to disclosure (N.C.G.A. 15A-904(a) 2004).

went into effect. As noted earlier, some district attorney's offices had already adopted informal open file policies. One public defender from Mecklenburg county, for example, perceived no change in the volume of discovery after open file went into effect at least partially because the county already had a generous open file policy. But the law probably had a bigger effect on discovery practice elsewhere, especially in smaller counties. A public defender from Pitt County, which is almost six times smaller than Mecklenburg, reported that prior to 2004 defendants often only received *Brady* material and anything else the prosecutor wanted the defendant to see. According to that attorney, the open file law was a "game changer" that led to discovery packages that are "a whole lot bigger."

I have focused primarily on the experience of the law enforcement officer and prosecutor as they investigate and compile discoverable materials prior to disclosure. It is also worth considering other procedural events taking place at the same time. In a typical case, the defendant is arrested and a police officer files charges against him.[46] Soon after, he is brought before a magistrate in district court for a bond decision. Next, defendants who are arrested without a warrant have a right to a probable cause hearing to determine whether there is sufficient evidence to proceed with the criminal process. A large majority of defendants waive this right (NC Courts 2005). Then, an indictment is filed against the defendant in superior court. The grand jury hearing is a secret proceeding administered and led by a law enforcement officer; neither the defendant nor the

[46] In North Carolina, only law enforcement officers and citizens can file charges.

prosecution are present. If the grand jury returns a true bill, the superior court has jurisdiction over the case.

After indictment, the defendant's statutory right to discovery attaches to felony charges and transactionally related misdemeanor charges (N.C.G.A. §15A-902(c) 2004). In practice, some defendants may enjoy discovery without strictly satisfying these conditions. For example, defendants may often receive partial discovery before indictment. Some prosecutors may also provide discovery in misdemeanor cases, but they are not legally required to do so, and there is no guarantee that they or law enforcement have included "everything" in the file (Loven 2008).[47]

Under the language of the statute, the defendant must request discovery from the state to exercise the right to open file discovery (N.C.G.A. §15A-902(a) 2004). The defendant first submits a request to the prosecution for voluntary discovery. If the defendant does not receive satisfactory discovery within seven days, the defendant may file a formal motion to the court. The same discovery rights attach regardless of whether the defendant receives discovery through a request for voluntary discovery or a motion to the court (N.C.G.A. §15A-902(a) 2004). In practice, discovery is frequently provided after the seven-day deadline due to heavy police and prosecutorial caseloads. Defendants do not all submit requests for discovery in order to receive it. Disclosure is now common practice, and some defense attorneys rely on prosecutors to provide discovery as a matter of course. In a few jurisdictions, the Office of Indigent Defense Services has a formal

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[47] Of course, federal constitutional rights to exculpatory evidence under *Brady* apply in misdemeanor cases, but as discussed earlier, they likely do not apply to cases disposed by plea agreement.

agreement with the district attorney's office to receive full open file discovery in all felony cases automatically.

The open file statute does not specify any particular method for providing the defendant access to the file. Some districts permit defense counsel to review and copy the physical file in the district attorney's office. The defense is often responsible for any copying costs. Other offices create and transfer a copy of the entire file to defense counsel directly. The more advanced systems are now electronic, and files are provided either on CD or online. Currently, online access may be limited to public defenders and not available to private defense counsel. The state intends to expand online discovery to all jurisdictions, and for defendants represented by both public and private counsel.

Once the state's file is transferred to the defense, counsel must review the materials to advise the client. Counsel generally provides the defendant with a summary of the files' relevant content. The North Carolina state ethics board has recently clarified that the defendant, and not just defense counsel, has a right to review the materials in the file (NC State Bar 2013; 2014). If a defendant requests to review the file, counsel must afford him "the opportunity to meaningfully review all of the relevant discovery material"(NC State Bar 2013). Some defense attorneys hesitate to allow clients to possess discovery materials. The risk is particularly salient for clients in prison who may lose the evidence or disclose it to other inmates.

The defendant's right to access the file is not absolute. Counsel can withhold materials if "it is in the best interest of the client's legal defense" or if disclosure will "endanger the safety or welfare of . . . others" (NC State Bar 2013: 49-50). Counsel may

also refuse to provide the client with a "physical copy" of the discovery materials, though some defense attorneys appear to be unaware of the rule. Interestingly, other states with expansive discovery laws handle these issues differently. The 2013 Michael Morton Act in Texas, for example, prohibits defense attorneys from allowing clients to "have copies of the information provided" other than copies of their own statements. In Ohio, the prosecution can designate "counsel only" materials that cannot be shown to the defendant (Ohio R. Crim. P. 16(c) 2014).

A number of procedures are set in place to ensure complete discovery before final disposition. First, as a general rule, the case cannot proceed to arraignment until after the defendant has received complete discovery. In some cases, however, the defendant may agree to proceed to arraignment before discovery is completed, perhaps to facilitate a better plea agreement. Second, in some jurisdictions the detective, defense attorney and ADA meet together after discovery is provided to review all of the materials and ensure nothing is missing. Some jurisdictions may also hold a second meeting prior to trial. Discovery rarely occurs all at once, and law enforcement and the district attorney's office have an ongoing legal duty to disclose new information that arises after initial discovery is provided. The open file statute does not impose a uniform time deadline for disclosing new evidence, but only requires the disclosure to be "prompt" or within a reasonable period of time (N.C.G.A. §15A-907 2014).

The open file discovery statute primarily regulates pretrial activity, but it continues to play a role at later phases in the judicial process. If one party attempts to introduce evidence at trial that was not previously disclosed, the opposing party must

object to its admission. Failure to do so defeats any appeal of the admission (State v. Mack 2008).

The statute also provides legal authority for the court to impose sanctions for discovery violations. The trial court has wide discretion to craft remedies for discovery violations and the appellate courts in North Carolina rarely reverse these decisions. In crafting a remedy, the courts have considered a variety of factors including the importance of the undiscovered evidence, the existence of bad faith, prejudice to trial preparation, prejudice to trial presentation, and whether the opposing party suffered any unfair surprise at trial (Rubin 2004).

The importance of each of these considerations vary by context. Prior to trial, one party may have failed to disclose all discoverable evidence. The typical judicial remedy is a corrective order of disclosure (Rubin 2004).

During trial, a party may attempt to introduce discoverable evidence that was not previously disclosed. In these circumstances, the most common judicial remedy is to provide a continuance to allow the opposing party to review the evidence (Rubin 2004). Trial courts are hesitant to impose more severe sanctions, (e.g., State v. Moncree 2008; State v. Blankenship 2006) but they occasionally exclude evidence (e.g., State v. Remley 2009; State v. Taylor 1984; State v. James 2007) or grant mistrials in more extreme cases (State v. Mills 1992), and are more likely to do so where there is evidence of bad faith (e.g., State v. McClintick 1986). As in other states, the North Carolina courts are particularly hesitant to grant mistrials, and apply a stringent standard before doing so (e.g., State v. Jaaber 2006). Dismissals are considered "an extreme sanction" reserved for

exceptional circumstances (State v. Adams 1984). In the most extreme cases, the state has the power to initiate criminal proceedings against state officials that fail to follow statutory disclosure requirements. A law enforcement officer's failure to provide legally required information to the prosecutor's office, and a prosecutor's failure to disclose legally required information to the defendant can be charged with a class H felony (N.C.G.A. § 15A-903 2014). I have found no evidence that any such charges have yet been brought since they were enacted in 2011.

After conviction, a defendant may discover new and relevant evidence that the prosecution failed to disclose. As elsewhere, the courts in North Carolina are reluctant to undermine the jury's decision to convict. When they find that the prosecution failed to disclose evidence, they frequently conclude that the failure imposed no prejudice on the defendant given other evidence in the case (State v. Tirado 2004).

D. What Open Files Solves, and What It Doesn't

North Carolina's open file statute helps address several problems with traditional criminal discovery. It requires disclosure of all materials associated with the case and thus preempts any argument the prosecutor was not obligated to disclose. More importantly, the statute strips prosecutors of their discretionary role as gatekeepers (Medwed 2010). They no longer need to make difficult *ex ante* predictions about whether evidence will be relevant at trial, and their biases are checked by the presumption that everything must be disclosed. Prosecutors who lack the time to review their files for discoverable evidence need not do so; instead, they can simply hand over the file. And as

described earlier, many counties have implemented robust procedures to decrease the chance evidence falls through the cracks by mistake or negligence.

Open file is less effective at eliminating intentional efforts to withhold evidence. For example, although police officers and prosecutors are required to disclose notes from all witness interviews, they can elude these requirements by failing to take notes (Medwed 2010; Cassidy 2012).

Still, open file likely reduces at least some intentional discovery violations. First, withholding any evidence associated with the case is prohibited by statute. Courts are thus more likely to find that non-disclosures are violations and impose sanctions. Second, procedures implemented after open file went into effect may increase detection of discovery violations. In some counties the prosecutor and police officer meet with the defense attorney prior to trial to confirm receipt of all relevant materials. Except in cases of actual collusion, prosecutors may be weary to withhold key evidence about which the police officer is aware. Third, the fact that discovery is usually given soon after charges are filed encourages prosecutors to review the record early in the criminal process. It may be easier for prosecutors to disclose exculpatory evidence early on before they have invested months in the case (Baer Forthcoming). Finally, over the long term, a robust discovery regime will likely encourage cultural change towards stronger norms of disclosure in prosecutor's offices.

IV. THEORIZING CRIMINAL DISCOVERY

A. The Basic Model of Litigation

Scholars of both civil and criminal law have widely adopted a basic model of litigation that predicts whether a dispute will be settled out of court, and if so, the content of the settlement agreement (Mnookin & Kornhauser 1979; Priest & Klein 1984; Easterbrook 1992; Scott & Stuntz 1992; Covey 2011). The model implies that parties bargain in the shadow of the trial, and thus, negotiate a settlement reflecting the expected outcome of trial. While this model is useful for understanding criminal litigation, structural asymmetries between the prosecution and defense may skew the plea bargaining process away from the trial's shadow (Bibas 2004).

The model begins by observing that a trial is an expensive way to resolve a dispute.[48] If the parties can accurately estimate the expected outcome of trial, they can obtain the same outcome through settlement and share the avoided costs of litigation (Cooter & Rubin 1994). Two key variables affect the likelihood and content of settlement.

First, the parties form estimates of the expected outcome at trial. The closer the parties' estimates are to each other, the more likely a settlement can be reached. And, the higher (or lower) the estimates, the higher (or lower) the settlement will be. Several factors influence the parties' estimates of the trial outcome including the balance of

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[48] This assumption is almost universal in the literature on civil litigation (Cooter & Rubin 1994). Interestingly, it assumption may not apply in all criminal cases. Some criminal defendants may place great value on maintaining their innocence and thus prefer trial despite a worse expected outcome to avoid pleading guilty.

favorable and unfavorable evidence they anticipate will be introduced at trial, the content and burden of proof for the relevant legal standard (Mnookin & Kornhauser 1979), and cognitive bias arising from overconfidence (Korobkin 2006; Loewenstein et al. 1993; Babcock et al. 1995; Babcock et al. 1997).

Second, the parties may be more or less willing to accept a suboptimal settlement offer, an offer that is worse than their trial estimates. Greater willingness among either party increases the likelihood of settlement. Greater willingness by the defendant leads to a more severe plea agreement, while greater willingness by the prosecution leads to a less severe one. Several factors influence the parties' willingness to accept a suboptimal settlement. Higher trial costs increase the cost savings from settlement, and thus increase willingness to accept a suboptimal offer (Mnookin & Kornhauser 1979; Priest & Klein 1984). Risk aversion also increases willingness among those who prefer to avoid the uncertainty of trial (Cooter & Rubin 1994). Parties with large stakes in the litigation may be particularly risk averse (Priest & Klein 1984).[49] Finally, parties may be more willing to accept a suboptimal settlement if they do not expect that new and favorable evidence would be uncovered at trial.

If the parties share similar and accurate estimates of the outcome of trial and neither party is more willing than the other to accept a suboptimal settlement, then the parties are likely to negotiate a settlement or plea agreement equal to the expected

[49] Empirical scholars have found mixed evidence on the effect of stakes on the settlement agreements (Kuziemko 2006; Perloff & Rubinfeld 1987; Huang et al. 2010; Danzon & Lillard 1975).

outcome of trial. But in the typical criminal case, asymmetries between the defense and prosecution may render these assumptions less plausible (Bibas 2004).

First, asymmetry in information may lead the parties to form different estimates of the trial outcome. Information asymmetries can therefore affect both the likelihood and content of a settlement. Such asymmetries are common in criminal cases for several reasons. As in civil cases, the defendant often has the best information that he is guilty. But, in many ways, the prosecution is better able to investigate the crime. The state has more resources for investigators and forensic analyses than a public defender or court-appointed counsel. The state also enjoys the investigative assistance of police officers who are the first to arrive at the scene of the crime and are well positioned to gather evidence and interview witnesses. These asymmetries are exacerbated by pre-trial detention, as a defendant behind bars is less able to assist in the defense's investigation (Roberts 2004). And weak criminal discovery rules in many jurisdictions mean that the defendant and prosecution are unable to eliminate information asymmetries by requiring the other party to disclose evidence that is otherwise unavailable.

Second, unlike in civil cases,[50] many criminal cases involve large stake asymmetries. As an extreme example, the stakes of a repeat drug or property offender facing many years in prison far outweigh the stakes of society or the prosecutor assigned to the case.

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[50] Civil cases are typically a zero sum game. Any award to the plaintiff is a loss of the same size to the defendant. Of course, in some civil cases, an award of $1 million dollars might be a huge sum of money for an individual plaintiff, and a drop in the bucket for a large corporate defendant. This asymmetry is better understood as a difference in risk aversion.

Third, unlike civil litigants who often carry similar financial costs, parties to a criminal case usually do not. Most defendants are indigent and the state pays for their attorneys. And some defendants may prefer trial over a plea agreement if they believe a delay may cause the witnesses' memory to fade or other evidence to grow stale. Still, many defendants bear significant trial costs including emotional suffering, public shame,[51] extended pre-trial detention, and a sentence penalty (Blume & Helm 2014; Abrams 2013).

Fourth, the settlement space—i.e., the universe of possible plea agreements—is both sparse and lumpy in criminal cases (Alschuler 1976). Settlement negotiations in civil cases often focus on damages, a continuous quantitative variable. The parties can, therefore, finely calibrate a settlement anywhere along a continuum based on gradations in the probability and size of the expected trial award. Negotiations in criminal cases, in contrast, often focus on charge bargaining. The settlement space for charges is sparse because there are only a limited number of relevant charges. The space is lumpy because the relevant charges often carry very different penalties. Charge bargaining thus "leaps from one charge to another" (Alschuler 1976: 1144), allowing little fine calibration (Bibas 2004).[52] A sparse and lumpy settlement space can decrease the chance that the parties will find a mutually agreeable settlement and may require parties to accept an unfavorable settlement if no more favorable one exists in the settlement space.

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[51] In some cases the defendant's trial costs may be lower than the costs of a plea agreement.

[52] Several recent papers examine the effect of the depth and distance of lesser included offenses (Wright & Engen 2006; Wright & Engen 2007)

B. Extending the Basic Model to Criminal Discovery

Scholars have engaged in little formal theoretical analysis of criminal discovery (Douglass 2001). A larger body of work on civil discovery emphasizes natural incentives in the pretrial negotiation process that lead to broad voluntary disclosure even in the absence of formal requirements. Drawing on this body of work, I outline a theory of criminal discovery to explain the effect of disclosure requirements on the content of settlements and the rate of trials.[53]

At least three important features of the criminal justice system must be accounted for to understand the effect of criminal discovery. First, unlike in civil cases where a broad right to discovery attaches at the start of litigation, criminal discovery in many jurisdictions proceeds in two distinct phases. The parties enjoy certain statutory discovery rights prior to trial and these rights expand when the case goes to trial. As I argue below, pre-trial and trial discovery requirements have different effects on both the rates of settlement and the accuracy of dispositions. Second, high caseloads and tight budgets mean that natural incentives in the plea negotiation process are insufficient to ensure complete discovery in criminal cases. And third, unlike civil litigants who seek to maximize profit and minimize costs, professional norms encourage many prosecutors to seek justice rather than merely maximize convictions

[53] Scholars of discovery often define the fair or accurate outcome as the outcome that a trial would arrive at based on a sound understanding of the relevant law and knowledge of all of the facts of the case (e.g., Cooter & Rubinfeld 1994).

For much of this section, I examine the process of discovery in the absence of prosecutorial norms and discovery requirements. I do so to explore the natural incentives in the discovery process that are independent of jurisdictional context.

1. The Effects of Information Asymmetry

Parties' knowledge of relevant and admissible evidence affects their estimates of the outcome of trial. Asymmetries in knowledge can, therefore, affect the distance between their trial estimates (Bebchuk 1984). The precise effects of information asymmetries depend upon the content of the evidence.

Suppose, for example, that the prosecution has evidence that is favorable to itself and the defense is unaware.[54] The evidence increases the prosecution's optimism about the outcome of trial without exerting a corresponding decrease in the defendant's. As a result, the distance between the parties' trial estimates expands and the probability of a plea agreement drops (Bebchuk 1984). Parties that settle prior to disclosure will negotiate a plea agreement that is, on average, biased against the prosecution relative to the available evidence. However, assuming that the prosecution would introduce the evidence at trial, the information asymmetry would not decrease the accuracy of the verdict.

The effects of information asymmetry are more complicated when the prosecution has evidence that is unfavorable to its case. As noted already, prosecutorial norms or discovery requirements may lead the prosecutor to disclose the evidence voluntarily. I

[54] The same basic analysis can be conducted in the reverse position with the defendant in possession of evidence that the prosecution does not know about. I focus on the prosecution because the open file statute primarily expanded discovery requirements on the prosecution.

ignore this possibility for now to consider the effects of information asymmetries on the parties' trial estimates. If the prosecutor believes that the defense will never obtain the evidence, then, the information asymmetry has no effect on the distance between the parties' trial estimates (Hay 1994). The defendant cannot incorporate the evidence into the trial estimate without knowing that the evidence exists. And assuming the prosecution will not introduce the unfavorable evidence at trial, the prosecution does not incorporate the evidence into its own estimate of the trial outcome either. Because the information asymmetry does not affect the distance between the parties' trial estimates, it has no effect on the probability of a plea agreement. But if the parties settle, they will negotiate a plea agreement that is, on average, biased against the defendant relative to the available evidence. Similarly, if the case goes to trial, the court will produce a verdict that is, on average, biased against the defendant (Bebchuk 1984; Hay 1994).

The effects of the information asymmetry differ if the prosecution anticipates that the defendant will obtain the evidence prior to the close of trial (Hay 1994). Once again, during the settlement process, the defense is unaware of the evidence and thus cannot incorporate it into the trial estimate. The prosecution, however, anticipates that the defense will obtain and present the evidence prior to the close of trial. The prosecution decreases its optimism about the outcome of trial, and thus, increases the chance of a plea agreement (Hay 1994). The information asymmetry may further encourage an agreement by giving the prosecution a special incentive to settle quickly before the defense obtains

the evidence.[55] If a plea agreement is reached, the agreement will on average be biased against the defendant relative to the available evidence. If the case instead goes to trial, the defendant will obtain and introduce the evidence. The resulting verdict will, on average, accurately reflect the available evidence.

2. Natural Incentives to Eliminate Information Asymmetry

Scholars studying civil litigation have argued that incentives in the negotiation process lead the parties to eliminate information asymmetries through a process of natural unraveling (Shavell 1994). A prosecutor or defendant seeking an optimal plea agreement has incentives to disclose favorable evidence voluntarily to decrease the optimism of the other party's trial estimate (Douglass 2001; Cooter & Rubinfeld 1994; Hay 1994; Shavell 1989).

The incentives for disclosure are less clear with respect to unfavorable evidence. Perhaps counter intuitively, Steven Shavell (1989) argues that civil litigants also disclose unfavorable evidence voluntarily through natural unraveling. When a plaintiff holds but does not disclose evidence, the defendant infers weakness from the plaintiff's silence and increases optimism about the trial outcome. If the defendant has overestimated the importance of the withheld evidence, the plaintiff now has incentive to disclose the evidence to correct the defendant's over-optimism. As a result, Shavell concludes that discovery requirements do not encourage settlement by eliminating information asymmetry. Rather, discovery requirements only affect the likelihood and content of a

[55] Others have noted, for example, that *Brady* creates a perverse incentive for prosecutors to obtain plea agreements to avoid the need to disclose exculpatory evidence (Douglass 2001).

settlement by providing parties with greater assurance that there is no additional favorable evidence of which they are not yet aware.

3. Limits on Natural Incentives and the Effect of Discovery Requirements

Certain conditions in the criminal justice system block the completion of disclosure through natural unraveling. Discovery requirements encourage settlement and promote accuracy by neutralizing the effect of these conditions.

i. Value of Trial Surprise.

The parties lack a natural incentive to disclose favorable evidence when the benefit of withholding until or just before trial exceeds the costs of trial. For example, in the absence of discovery requirements, a party can gain an advantage by surprising the opponent at or just before trial with previously undisclosed evidence (Hay 1994). Parties are more likely to withhold favorable evidence when the value of surprise exceeds the cost savings afforded by a plea agreement. A discovery requirement for the evidence neutralizes the value of surprise and, thus, removes an incentive to withhold the evidence. A discovery requirement therefore increases the chance of a plea agreement by removing the party's reason for trial and by decreasing the optimism of the opponent's trial estimate. It also increases the fairness of trial by eliminating the negative effects of unfair surprise.

ii. Complete Unawareness.

If we continue to assume away prosecutorial ethics and discovery requirements, the parties lack a natural incentive to disclose unfavorable evidence if the opponent is completely unaware of its existence (Cooter & Rubinfeld; Hay 1994). For example, if the

prosecution holds evidence that is unfavorable to itself and the defendant is completely unaware of its existence, the defendant cannot infer weakness from the prosecution's failure to disclose the evidence. As a result, the information asymmetry leads the defendant to a falsely pessimistic estimate relative to the evidence (Cooter & Rubinfeld; Hay 1994).

Under these circumstances, discovery requirements can play an important role in eliminating information asymmetry. The precise dimensions of this effect depend upon whether the discovery rule attaches at trial or attaches beforehand during the plea bargaining process. Under a trial discovery rule, the defendant remains unaware of the evidence during the negotiation process and only learns of the evidence if he goes to trial. The trial discovery requirement increases the chance of a plea agreement because the prosecution knows it must disclose if the case proceeds to trial (Douglass 2001). The trial discovery requirement may also improve the accuracy of the plea agreement, not by raising the defendant's estimate of the trial outcome, but by increasing the prosecution's willingness to accept a suboptimal plea to avoid disclosure at trial. Finally, the trial discovery requirement would likely improve the accuracy of the trial outcome by ensuring that the relevant evidence is presented to the court.

The effects of discovery differ if the requirement applies pre-trial. A pre-trial requirement compels the prosecution to disclose unfavorable evidence during the plea bargaining process, and thus, increases the optimism of the defendant's trial estimate. If there is no pre-existing trial discovery requirement, the pre-trial requirement has no effect on the probability of a plea agreement. Based on the new evidence, the defense increases

the optimism of its trial estimate. The prosecution also decreases its optimism accordingly now that the defense can present the evidence at trial. As a result, the new pre-trial requirement also increases the accuracy of any plea agreement relative to the available evidence. The accuracy of a trial verdict also increases now that the defense knows of the evidence and can present it to the court.

The effect of a pre-trial discovery requirement differs if there already exists a trial discovery requirement. For example, prior to the open file legislation in 2004, North Carolina courts recognized a trial discovery right to exculpatory and material evidence under *Brady*. The open file law extended discovery of *Brady* material to the pre-trial context. Extending a trial discovery requirement to the pre-trial context decreases the probability of a plea agreement in two ways. First, it increases the distance between the parties' trial estimates by increasing the optimism of the defendant's estimate without affecting the prosecution's (Hay 1994). Second, it decreases the prosecution's willingness to accept a suboptimal plea agreement by eliminating its special incentive to secure a plea to avoid disclosure at trial. Extending a trial discovery requirement to the pretrial context increases the accuracy of any plea agreement by correcting the defendant's overly pessimistic trial estimate. It does not, however, have any effect on the accuracy of the trial outcome because the defendant would have obtained the evidence through the existing trial discovery requirement in the event that the case went to trial.

iii. Time & Resource Constraints.

The natural unraveling process is time consuming and expensive. This process may be plausible in civil cases where litigants have deep pockets, but heavy caseloads sharply limit the time a prosecutor or public defense attorney can devote to a case.

Under the natural unraveling theory, parties voluntarily disclose favorable evidence to improve their bargaining position in the negotiation process (Shavell 1989; Hay 1994; Cooter & Rubinfeld 1994). The prosecutor has a strong incentive to disclose incriminating evidence to encourage the defendant to accept a plea agreement. Still, high caseloads and sharp resource constraints may limit their ability to disclose less critical evidence.

Resource constraints pose a greater obstacle to voluntary disclosure of unfavorable evidence. The unraveling theory predicts that when a defendant knows the prosecution has undisclosed evidence but does not know its content, the defendant will infer from the prosecution's silence that the evidence disfavors the prosecution. The defendant can propose a series of plea offers and then recalibrate his trial estimate based on the plaintiff's reaction. With an unlimited number of offers and counteroffers, the defendant could identify the precise value of the evidence. But resource constraints mean that parties in a criminal case do not have anything close to an unlimited number of offers and counteroffers to perform this calibration. If the defendant only has one more opportunity to make a plea offer before the case goes to trial, and if the costs of trial are high, the defendant may prefer a suboptimal plea agreement over trial. He may, therefore, make an offer based on a low estimate of the value of undisclosed evidence to secure

settlement (Shavell 1989; Hay 1994). Thus, resource constraints in the criminal justice system place sharp limits on the power of natural unraveling to produce accurate plea agreements.

A pre-trial discovery requirement provides a partial solution to this problem by mandating disclosure. The precise effects of the requirement depend upon whether it is encouraging disclosure of favorable or unfavorable evidence.

C. Applying the Model of Criminal Discovery to Open File in North Carolina

1. The Probability and Content of Settlement

Given the model articulated in the previous section, the open file statute may have affected the probability and content of settlements in criminal cases through three causal pathways.

First, the statute may have increased the disclosure of favorable evidence by the prosecution. Prior to 2004, disclosure of evidence favorable to the prosecution was not required by law. The previous discovery statute was narrow, and *Brady* imposed no requirements to disclose inculpatory evidence. Thus, the open file statute may have increased disclosure by requiring it. The disclosure of evidence favorable to the prosecution increases the probability and severity of plea agreements by increasing the defendant's estimate of the trial outcome. However, these effects were likely small. Although prosecutors were not legally required to disclose favorable evidence prior to 2004, natural incentives in the plea bargaining process likely led to voluntary disclosure of much favorable evidence anyway.

Second, the open file statute expedited the disclosure of unfavorable *Brady* material to an earlier phase in the process. Indeed, the right to *Brady* material does not attach until the case goes to trial, but the open file statute requires disclosure well before then. This change is particularly important for *Brady* witness statements, which under the previous statute, were sometimes withheld until after the witness testified. Expediting disclosure of evidence unfavorable to the prosecution decreases the probability and severity of plea agreements.[56] And where it reveals the prosecution has insufficient evidence to support any of the charges, it increases the probability the case will be dismissed.

Third, and most importantly, the open file statute likely increased the total amount of unfavorable evidence disclosed by the prosecution. Prior to 2004, narrow statutory rights in North Carolina meant that *Brady* was the major source of exculpatory evidence. But, *Brady* provides only weak discovery rights. The materiality standard is stringent, and scholars have questioned whether prosecutors are able to screen for exculpatory evidence without knowledge of the defendant's trial strategy. Professional and ethical norms encourage prosecutors to disclose, but otherwise they have little incentive to hand over unfavorable evidence unknown to the defendant. There is also less reason to expect natural unraveling. Prior to 2004, a defendant's only recourse was to submit a general request for exculpatory evidence. These requests are difficult for the prosecution to

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[56] The fact that disclosure happens all at once helps ensure fair settlements. Unlike in the civil system where discovery occurs gradually over time, a one-time disclosure in the criminal system decreases the chance a defendant will accept an unfair plea agreement without knowing additional exculpatory evidence is forthcoming.

satisfy in practice and the courts apply a higher *Brady* standard to general requests, which further weakens enforcement. Open file solves these problems by requiring the prosecution to provide all evidence to the defendant. The pre-trial disclosure of unfavorable evidence by the prosecution, which would otherwise not be disclosed, has no effect on the probability of a plea agreement unless the parties' trial estimates are subject to bias, in which case, such disclosure would decrease the probability. It does, however, reduce the severity of a plea agreement if one is reached. And where the prosecution has insufficient evidence to support any of the charges, it increases the probability the case will be dismissed.

Taken together, these causal pathways predict effects in multiple directions. Given pre-existing natural incentives to disclose favorable evidence, the second and third causal pathways likely exert the largest effect. Together, they predict that the open file statute decreased the severity of plea agreements, increased dismissals, and increased or had no effect on the trial rate.

2. Timing of Plea Agreements

In addition to affecting the content and probability of plea agreements, open file may also affect their timing. Early plea agreements are important because they promote efficiency and mitigate the negative consequences of pre-trial procedures including detention. The open file statute imposes no specific time requirement for discovery, and there is no publicly available data on the timing of disclosure (Rubin & Grine 2013). But attorneys interviewed in North Carolina reported that discovery began earlier in the criminal process after open file went into effect.

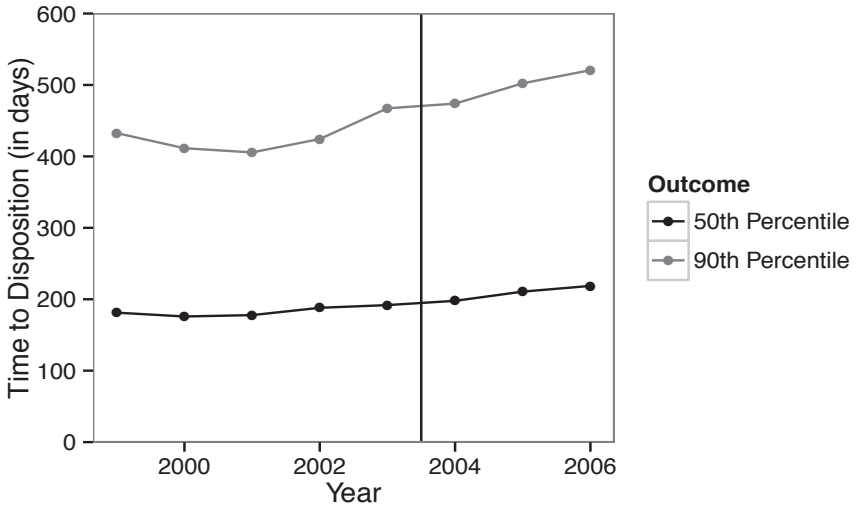
There are a number of reasons that early disclosure would speed up plea agreements. First, in some cases the content of a discovery package is the direct cause of settlement, and so earlier discovery means earlier settlement. Second, reasonable defendants who do not know what evidence the government has may wait to accept a plea agreement until they have received discovery. Third, even if the defendant is generally aware of the government's evidence, several defense attorneys explained it is easier to persuade clients to accept fair plea agreements when they have the evidence in their own hands. Fourth, a defense attorney who receives open file discovery early in the criminal process has less need to engage in time consuming investigative work. After receiving the file, an attorney can go straight to the witnesses already listed by the government without needing to identify them first. Finally, several defense attorneys explained that, prior to open file, they had to "jump through hoops" to obtain evidence from the prosecution when it was unwilling to share. Doing so frequently included filing time-consuming motions in court. The time and cost of these procedures are avoided now that full open file is required as a matter of course.

Some attorneys in North Carolina perceive that certain features of open file also slow down plea agreements. First, now that discovery includes more useful material, fewer defendants may accept plea agreements before receiving their discovery package. Second, the open file process is time consuming and burdensome, and it may take months for police and prosecutors to gather, compile and transfer evidence to the defense in compliance with the statute. Third, several attorneys in North Carolina explained that, especially in the last few years, open file coupled with the digitalization of records and

discovery has resulted in a deluge of information that is challenging to review. One attorney described an extreme case where discovery included 10,000 text messages exchanged by police officers during the course of an investigation. Finally, while open file may have decreased preliminary litigation about whether a defendant has a right to discovery of specific evidence, some attorneys reported that it has fueled an increase in litigation about whether the government has satisfied the defendants' statutory rights. One prosecutor claimed that, in some cases, more time is spent litigating discovery violations than the defendant's guilt.

The available data on time-to-disposition in North Carolina is limited, but we can glean some basic descriptive insights from the court system's annual reports. The black line in Figure 3.1 represents the county-level average of the median time-to-disposition in each court. It indicates that the time-to-disposition in the typical case—which is most likely disposed by guilty plea—increased slightly after 2004. The gray line represents the county-level average of the time-to-disposition for cases in the 90th percentile. It indicates that time-to-disposition for some of the longest cases in the system—a disproportionate share of which are disposed by trial—increased substantially after 2004. While we cannot draw firm causal conclusions based on these data alone, they offer little evidence the law led to quicker plea agreements.

Figure 3.1: Time to Disposition in Felony Cases



D. Literature on the Effects of Discovery Requirements

Few systematic empirical studies have examined discovery (Kakalik et al. 1998; McKenna & Wiggins 1998; Rosenberg 1989), and none have examined the effect of changes to the rules of discovery in criminal court. A small number of studies have evaluated changes to the rules of civil discovery, but most use subjective perceptions of attorneys and judges rather than objective case outcome data (Note 1972). One study using objective data examined the change in settlement rates in civil cases in Taiwan after a dramatic expansion in discovery rights (Huang et al. 2010). The author found that settlements increased after the law went into effect.

Several studies provide some support for the assumptions of the theory of criminal discovery outlined above. First, one study provides laboratory evidence that decreasing information asymmetries leads to faster settlements (Sullivan 2011). University students participated in a series of two-minute bargaining games where the level of information provided to each of the participants was randomly varied. The

author found that increasing information asymmetry between the participants caused up to a 95% increase in the time to settlement. The results suggest that information asymmetries prevent settlement and that discovery requirements may promote settlement by diminishing the asymmetries. The authors did not, however, investigate whether the effects of information asymmetries depend upon whether the evidence is favorable or unfavorable to the party holding it.

Second, limited empirical evidence confirms that, contrary to the unraveling theory, all relevant evidence is not voluntarily disclosed even in civil court where there are broad discovery rights. A survey of 180 civil litigators in Chicago found that parties frequently settled federal civil cases while they had "arguably significant information" that the other party had not discovered (Brazil 1980: 811). Lawyers estimated that this occurred in between a quarter and a half of their cases. Moreover, 83% of subjects reported having been "surprised at trial by evidence they had not uncovered during the discovery stage." (Brazil 1980: 817). They estimated that this took place in roughly 10% of their cases.

Taken together, the literature provides little guidance on the effect of open file in North Carolina. In the following sections, I conduct an empirical study to estimate the effect of the law on the rate of pleas, dismissals and trials in felony cases.

V. STUDY DESIGN

A. Data

This study uses annual court-level data on the number of felony and misdemeanor cases filed and disposed in North Carolina from July of 1999 to June of 2006. The data

were collected from caseload reports published by the North Carolina Court System (NC Courts 2000–2006) and processed using optical character recognition software. The data were closely examined to confirm the integrity of data transfer. They were then processed into one annual court-level dataset in which each row represents filing and disposition information for a given court in a given year of the study. Since each of the 100 counties in North Carolina have a superior and district court that handles criminal cases, the initial dataset contains 1,600 observations.[57] During the study period, the North Carolina Office of Indigent Defense Services established public defender offices in three prosecutorial districts, which account for nine counties in the state.[58] I remove these counties from the analysis to avoid potential sources of bias,[59] producing a final analytic dataset with 1,456 court-year observations.

I examine the effect of open file on three main outcome variables: the rate of guilty pleas, dismissals and trials. These measures were computed by dividing the number of pleas, dismissals, and trials in each court in each year by the total number of relevant dispositions.

Data on felony filings and dispositions are available by crime type including homicide, rape, other sexual assault, robbery, assault, burglary, larceny, arson, fraudulent activity, forgery and uttering, controlled substance, and other offenses. Data on

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[57] 100 counties * 2 courts * 8 years = 1,600 observations.

[58] The new public defender offices were established in Prosecutorial Districts 1, 10 and 21 which include Camden, Chowan, Currituck, Dare, Forsyth, Gates, Pasquotank, Perquimans and Wake County.

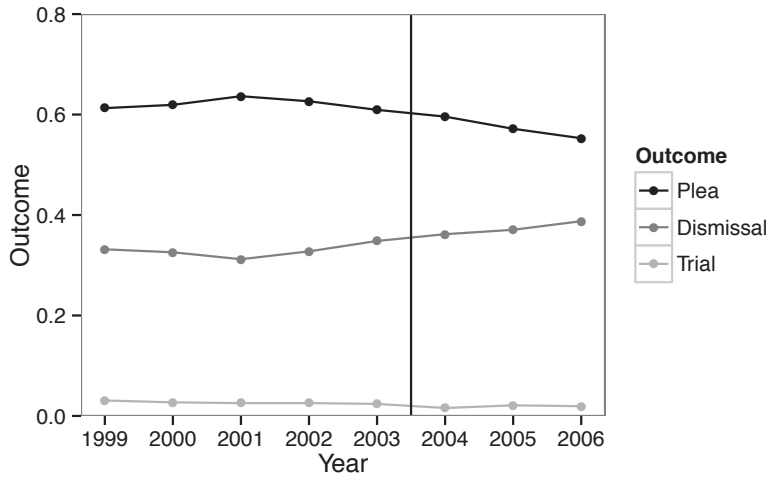
[59] There were no substantive differences in the results when these counties were included in the models.

misdemeanor filings, which exclude motor vehicle-related charges, are not available by crime type.

B. Descriptive Trends

Figure 3.2 shows the proportion of all felony cases disposed by plea, dismissal and trial before and after the open file law went into effect on September 1, 2004.[60] The figure reveals that guilty pleas decreased after the law went into effect and dismissals increased. The graph also reveals a slight decrease in the trial rate. While these trends are illustrative, it is difficult to draw causal inferences, particularly given that the trends pre-date 2004.

Figure 3.2: Descriptive Trends of Felony Outcomes, 1999–2006



C. Difference-in-Differences Models

To help distinguish between the effect of open file and other secular trends in the criminal justice system, I apply difference-in-differences (DID) models comparing the

[60] While the statutory requirements went into effect on this date, there was a delay of implementation in at least some counties. This slow roll may bias the estimates of the effect of the law downwards.

outcomes of felony cases with misdemeanors cases where open file does not apply.[61] In my first set of models, I compare the change in outcome variables in the superior courts (treatment group) before and after open file went into effect with the change in outcome variables in the district courts (control group). The model is as follows:

$$Y_{c,t} = B_0 + B_1*Superior_{c,t} + B_2*OpenFile_t + B_3*Superior_{c,t}*OpenFile_{c,t}$$

where Superior represents a dummy variable indicating whether court *c* is a district (=0) or superior court (=1); where OpenFile represents a dummy variable indicating whether an observation at time *t* is from before (=0) or after (=1) the open file statute went into effect. B_1 captures differences in level between the treatment and control group before 2004, and B_2 captures secular trends in the criminal justice system that may affect the outcome variable over time in the same way for both groups. B_3 is the relevant DID estimate of the effect of open file on disposition rates. I estimate all coefficients using ordinary least squares. In some models I also add fixed effects for each court in each county to adjust for time-invariant differences between courts. I estimate OLS and cluster-robust standard errors (clustered at the county-court level) to address concerns about serial correlation, which can be particularly acute where as here the treatment variable changes only once during the study period (Bertrand et al. 2004). I also collapsed the data into one pre- and one post-2004 period and refit the models (see Table 3.23 in the Appendix).

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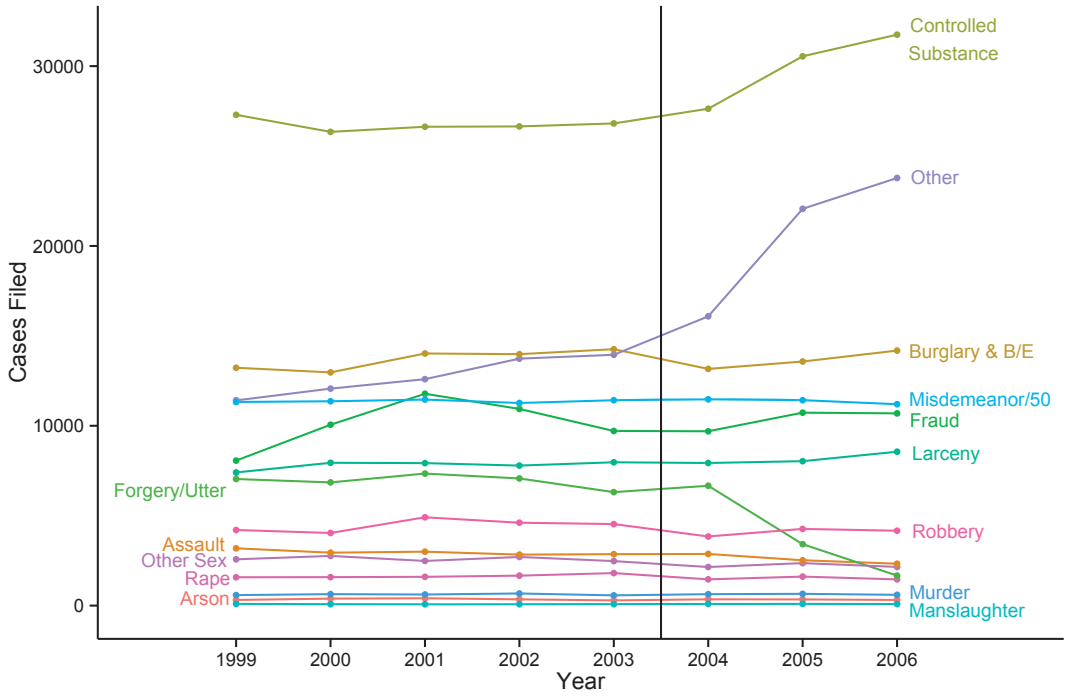
[61] This analytic approach is common in the empirical literature on the criminal courts (see, e.g., Helland & Tabarrok 2003; Yoon 2001; Cheng & Yoon 2005).

My identification strategy involves several assumptions, some of which are empirically testable. First, my models assume there are no changes in the composition of criminal cases during the study period. Figure 3.3 shows the number of filings by crime categories over time. For most categories—including burglary, larceny, robbery, assault, rape, other sex crimes, arson, murder, and manslaughter—there does not appear to be a substantial change in the number of filings.[62] There are, however, several exceptions. From 2004 to 2006, controlled-substance filings increased by roughly 15%. This gradual increase likely reflects a rise in drug arrests.

Figure 3.3: Number of Cases Filed by Crime Type



The sharp drop in forgery and uttering filings is more difficult to explain, but one of three phenomena is likely responsible: (1) prosecutors began charging forgery and

[62] Graphs presenting each crime category separately are available in Figure 3.6 in the Appendix.

uttering offenses less frequently in 2004; (2) prosecutors continued to charge forgery and uttering offenses at the same rate but began adding more serious charges, which caused forgery and uttering cases to be relabeled; (3) prosecutors continued to charge forgery and uttering offenses at the same rate, but revisions to the relevant statutes without revisions to relevant database linking tables caused forgery and uttering filings to be relabeled as "other" offenses.

Option (1) is unlikely for at least two reasons. First, the drop is too sharp—roughly 65% in two years. Second, the Chief Public Defender in a county for which the data indicate a large drop in forgery and uttering filings reported observing no change in prosecutorial charging practices during the study period.

Options (2) or (3) better fit the data because "other" filings increased at the same time that forgery and uttering filings fell. To investigate further, I reviewed every criminal-law bill passed by the North Carolina General Assembly during the study period. The state legislature enacted several fraud-related bills that became effective at the very end of 2002, which prohibited unlawfully accessing a government computer to commit fraud; fraudulently assisting another to file a tax return; failing to remit money from a taxpayer to the government; defrauding a drug and alcohol screening test; possessing forged or counterfeited instruments or securities; possessing or creating five or more counterfeit insurance instruments; using a scanning device to obtain information stored on a financial transaction card; fraudulently using the identity of a dead person; and trafficking in stolen identities (Smith 2002). In 2003, the General Assembly changed the criminal penalty provisions for securities-fraud offenses and created a new provision

against tax dumping—a practice of setting up dummy corporations to evade unemployment taxes (Smith 2003). While this legislation might explain some of the change in forgery and uttering filings, they affected narrow and idiosyncratic offense categories and went into effect a few years before the drop in forgery and uttering filings. The General Assembly later enacted the Identity Theft Protection Act in 2005, precisely the year in which forgery and uttering filings dropped. The Act, which was designed as a broad effort to prevent identity theft, prohibited certain uses of consumer information by businesses through civil rather than criminal liability. The Act also changed some of the language in the state's criminal financial identity fraud statute, including renaming the offense as "identity theft." Given the timing of the legislation, the Identity Protection Act is a potential candidate for explaining the apparent drop in forgery and uttering filings recorded in the data. Still, it is unclear precisely how it could have caused the drop because it did not add any new criminal statutes, renumber any existing criminal statutes or dramatically expand criminal liability for identity theft crimes.[63]

In summary, the decrease in recorded forgery and uttering filings and the increase in "other" offense filings in 2005, combined with some fraud legislation during the study period and anecdotal evidence indicating no change in prosecutorial behavior, all together suggest that the observed patterns in filing over time are due to changes in data management rather than prosecutorial behavioral. The precise process by which this change arose, however, is not entirely clear.

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[63] To further investigate, I obtained the linking table used by the North Carolina court system to generate the data used in this study. None of the statutes linked to forgery and uttering have been revised since 2002, well before the drop in forgery and uttering filings.

To minimize bias, I take a conservative approach and exclude the crimes for which there is observable evidence of a compositional change during the study period. This clearly covers controlled substance, forgery and uttering, and "other" offenses. I also exclude fraudulent activity due to some evidence of a compositional change from 1999 to 2003 and its close substantive connection to forgery and uttering offenses. For completeness, the primary results when each of these crime categories are included in the models are available in the Appendix (see Table 3.11 through Table 3.22). My primary analysis focuses on the remaining crime categories: burglary, larceny, robbery, assault, rape, other sex offenses, murder and manslaughter. As an alternative specification, I also fit models including all of the crime categories and with variables indicating the number of cases disposed in each court in each year by crime type. The results were substantively similar (see Table 3.24 in the Appendix).

Compositional changes other than the distribution of filed charges can also bias the models. Table 3.1 presents demographic data on arrestees in North Carolina from 2003 to 2005 (NC UCR 2003–2005). They reveal no change in the race, gender or age of offenders arrested during that period.

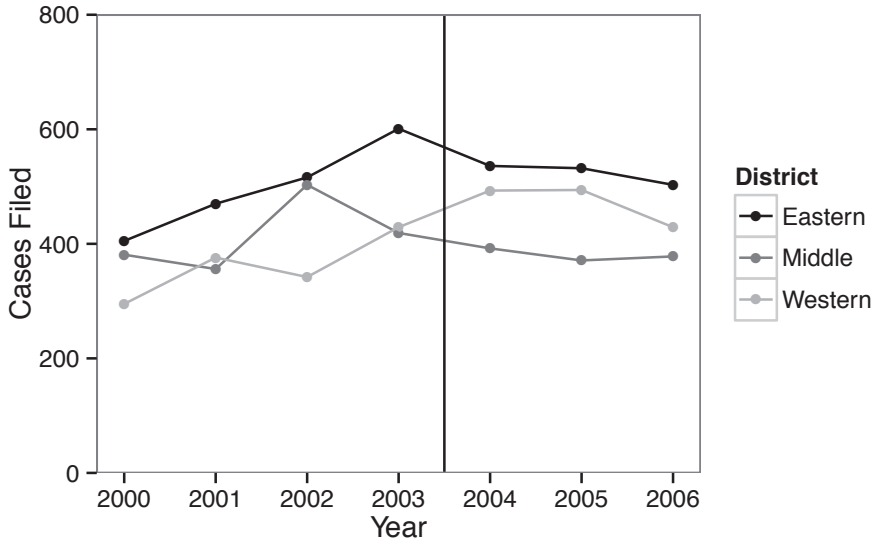
Table 3.1: Average County Arrestee Demographics and Charge Filings, 2003–2005[64]

	2003	2004	2005
Black	0.45	0.44	0.44
White	0.53	0.54	0.54
Other Race	0.02	0.02	0.02
Male	0.75	0.74	0.75
Age 18-25	0.28	0.27	0.27
25-34	0.28	0.28	0.28
35-44	0.21	0.22	0.22
45+	0.12	0.13	0.14

Some scholars have noted the possibility that open file encourages the government to divert state-court cases to federal court to avoid the discovery requirements imposed by open file (Baer Forthcoming). Figure 3.4 shows the number of cases filed in federal court in North Carolina before and after 2004. If the government diverted more cases to federal court to avoid the discovery requirements of open file, we would observe an increase in federal filings after the law went into effect. Filings in the Eastern District of North Carolina were relatively stable after 2004, and they decreased in the Middle District. Even if the increase of 100 federal cases in the Western District after 2004 can be attributed to the open file statute, it is highly unlikely such a small change would affect the results of the analysis.

[64] Data on demographics of arrestees derives from the North Carolina Uniform Crime Reporting Program (NC UCR 2003–2005). Arrest data are organized by calendar year (January to December).

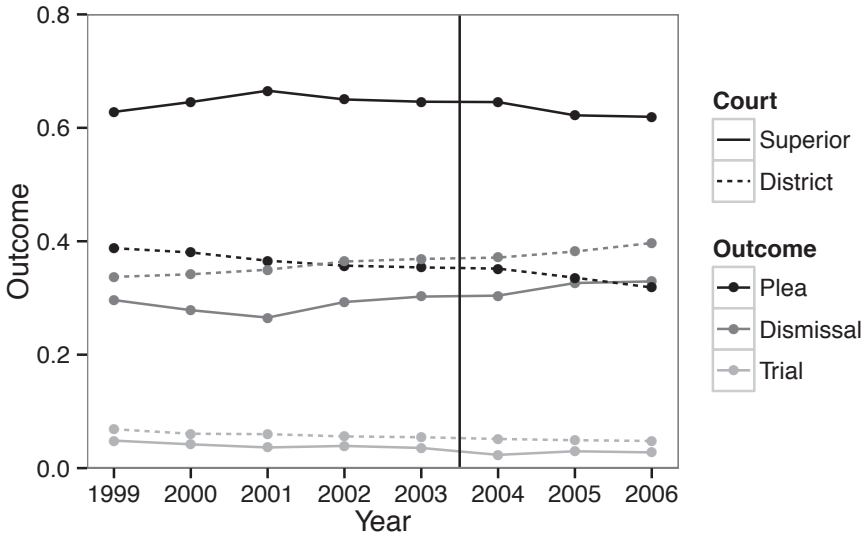
Figure 3.4: Federal Court Filings by Federal District



A second key assumption of the difference-in-differences design is the parallel trends assumption. Figure 3.5 presents the average county-level trial, plea and dismissal rate of crimes included in the analysis for the superior and district courts.[65] The superior courts (treatment group) are represented by solid lines and the district courts (control group) are represented by dotted lines. The average trial rate is in black, the plea rate in dark gray, and the dismissal rate in light gray. The most important takeaway is that the lines for the Superior and District Courts appear to follow similar trends, although there are some non-trivial differences in level. The other takeaway is that there are no dramatic changes in the rate of pleas, dismissals or trials after open file went into effect. Thus, whatever effects we observe in the difference-in-differences models will be small.

[65] The relevant graphs for the other contrasts are presented in Figure 3.7 through Figure 3.9 in the Appendix.

Figure 3.5: Average of Case Outcomes for Crimes by Court



My analytic approach—comparing felony and misdemeanor courts—is both a strength and a weakness of the study. Unlike previous empirical evaluations of changes to the rules of civil discovery—which merely compared outcomes before and after a law change (Weller et al. 1982; Huang et al. 2010)—the current study's use of district courts as a control helps account for the confounding effects of contemporaneous trends in the criminal justice system. The identification strategy is especially valuable given that criminal courts are local institutions. Comparing superior and district courts in the same county helps not only adjust for state-level, but also county-level secular trends.

Still, some important differences between superior and district court preclude firm conclusions about causality. First, the superior courts in North Carolina primarily handle felony cases, while the district courts handle misdemeanor cases that are comparatively less serious. Prosecutors, defense attorneys and defendants may invest less time, consideration and resources into the resolution of cases in the district court. This is a

common challenge in the court literature, which often uses less serious criminal charges as a comparison group for more serious charges affected by a legislative change.[66] Second, there may also be important procedural differences between the courts. The "assembly line" nature of criminal justice may be more pronounced in district courts, as higher case volumes must be disposed efficiently (Feeley 1979). The district courts, for example, try cases with bench trials, while the superior courts try cases with juries. And defendants in district court can appeal decisions to the superior court, while those in superior court appeal to the appellate division. Perhaps most importantly, legal representation for misdemeanor defendants in some states is rare. Fortunately, the statutory right to counsel in North Carolina extends to nearly all misdemeanors,[67] and a large majority of misdemeanor defendants in the state have legal representation.[68] While district courts may not provide a perfect comparison group, using them as a control helps remove some potential bias from secular trends in the criminal justice system and provides useful correlational evidence on the effects of open file.

Given these limitations, I take several further steps to improve the robustness of the analytic strategy. First, in addition to fitting the DID models on all felony and misdemeanor crimes, I also conduct alternative specifications which exclude the most serious felonies (i.e., murder, homicide, rape, robbery, burglary, other sex offenses).

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[66] Kuziemko (2006), for example, used burglary and robberies as a control group to test the effect of the availability of the death penalty on plea bargaining in first degree murder cases.

[67] Indigent persons have the right to counsel in "[a]ny case in which imprisonment, or a fine of five hundred dollars, or more, is likely to be adjudged" (N.C.G.A § 7A-451(a)(1)).

[68] Wright & Logan (2006) report that 65% of defendants charged with a misdemeanor in North Carolina in 2004 did not waive the right to counsel.

Focusing on less serious felonies increases the substantive similarity between cases in the treatment and control group. Second, I exploit the fact that some misdemeanor cases are appealed from the district court and are disposed *de novo* in the superior court. Open file discovery does not apply in these cases, even after they are appealed to superior court (State v. Cornett 2006; State v. Fuller 2006).[69] Focusing on these cases increases the procedural similarity between cases in the treatment and control group. Third, I exploit the fact that certain District Attorneys in North Carolina established informal open file policies prior to the 2004 statute. I have identified five such counties.[70] The open file statute affected a smaller change in discovery practice in these counties. We should, therefore, expect that any observed changes after enactment were smaller or non-existent in this subset of counties. Unfortunately, the estimates for these counties are statistically imprecise due to their small number. Fourth, I examine the effect of the open file statute on counties without a public defender's office under the assumption that private defense attorneys received less voluntary discovery prior to open file than public defenders who are repeat players.

VI. RESULTS

A. Main Models

Table 3.2 presents eight models that estimate changes in the rate of guilty pleas in North Carolina after open file went into effect in 2004. OLS standard errors are presented

[69] Some of the misdemeanor cases appealed to superior court will already have been tried in the district court. Defendants in those cases will have obtained some discovery through the course of the trial. The open file statute requires broader disclosure of information than would necessarily be presented at trial.

[70] They are Edgecomb, Mecklenburg, Nash, Robeson, and Wilson counties.

in parentheses and cluster-robust standard errors in square brackets. The first column presents the basic DID model without fixed effects. The coefficient for Superior is .278 and statistically significant reflecting the difference in level observed in Figure 3.5 between the plea rate in superior and district courts. The primary coefficient of interest is the DID term, Superior*OpenFile, which estimates a small and statistically insignificant .016 increase in the plea rate after the law went into effect. The second column presents Model 2, which excludes the most serious felonies from the treatment group. The coefficient is slightly smaller, at .014, and is also statistically insignificant.[71]

Models 3 and 4 add fixed effects for each court in each county. While the coefficients are similar, the R^2 increases dramatically. A high R^2 in some circumstances may imply overfitting and biased standard errors. Here, however, the fixed effects appear to be adjusting for real and large differences between courts in different counties. F tests reveal that the fixed effects in this table (and all others presented below) add statistically significant information to the models. And while the OLS standard errors may be biased by the fixed effects, the cluster robust standard errors appear unaffected.

In Models 5 and 6, I add a parameter for time for each court to adjust for linear trends. The estimates in these models are negative and slightly larger but remain statistically insignificant.

In Models 7 and 8 I add a separate DID term for five counties that adopted an open file policy prior to 2004. Any changes in the plea rate caused by the statute should

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[71] Three observations were dropped for this model and other models examining the minor crime data because there were no minor dispositions during the year of each of the three observations.

be lower among these early adopters. The main DID coefficient, Superior*OpenFile, is similar to that in Models 5 and 6. The coefficients for Superior*OpenFile*PreOpen show that early adopters experienced a larger reduction in the plea rate than other counties. While statistically imprecise, Models 7 and 8 estimate that early adopters experienced a reduction in the plea rate that was over 60% and 100% larger than other counties, respectively,

Taken together, the models suggest that open file did not affect the plea rate. None of the models estimate a statistically significant change. The coefficients in Models 1 through 4 are small, representing a 2% relative increase. While the estimates grow when linear time parameters are added for each court, the estimates remain small and statistically insignificant. These larger estimates may simply reflect greater noise in the models due to the increased number of parameters. And even if they represent real changes in the plea rate, Models 7 and 8 suggest the changes were not caused by the open file statute, as the reductions after 2004 are larger in counties that adopted open file long before then.

Table 3.3 presents the same set of models, but on a different dataset where the only cases in the control group are those appealed *de novo* to the superior court. Focusing on this subset of misdemeanor cases increases procedural similarity between the treatment and control group as all cases are processed and disposed in the superior court. The DID coefficients in these models are all small and statistically insignificant. Models 1 through 4 estimate coefficients similar to those in Table 3.2, and Models 5 and 6 estimate coefficients that are closer to zero.

Table 3.2: Main Models on Guilty Plea Rate

	(1) All	(2) Minor	(3) All	(4) Minor	(5) All	(6) Minor	(7) All	(8) Minor
Intercept	0.369	0.369						
	(0.005)**	(0.006)**						
	[0.008]**	[0.008]**						
Superior	0.278	0.232						
	(0.008)**	(0.009)**						
	[0.014]**	[0.015]**						
OpenFile	-0.034	-0.034	-0.034	-0.034	0.008	0.008	0.008	0.008
	(0.009)**	(0.010)**	(0.006)**	(0.008)**	(0.010)	(0.013)	(0.010)	(0.013)
	[0.004]**	[0.004]**	[0.004]**	[0.004]**	[0.006]	[0.006]	[0.006]	[0.006]
Superior*OpenFile	0.016	0.014	0.016	0.014	-0.031	-0.024	-0.03	-0.023
	(0.013)	(0.014)	(0.008)	(0.011)	(0.014)*	(0.019)	(0.014)*	(0.019)
	[0.012]	[0.013]	[0.012]	[0.013]	[0.019]	[0.023]	[0.020]	[0.025]
Superior*OpenFile* PreOpen							-0.019	-0.021
							(0.044)	(0.059)
							[0.039]	[0.045]
Court Fixed Effects	No	No	Yes	Yes	Yes	Yes	Yes	Yes
Court * Time Trend	No	No	No	No	Yes	Yes	Yes	Yes
R^2	0.6	0.45	0.824	0.731	0.858	0.792	0.858	0.792
N	1456	1453	1456	1453	1456	1453	1456	1453

Notes: * p<0.05, ** p<0.01

Table 3.3: Models on Guilty Plea Rate with Misdemeanor Cases Appealed De Novo

	(1) All	(2) Minor	(3) All	(4) Minor	(5) All	(6) Minor	(7) All	(8) Minor
Intercept	0.243	0.243						
	(0.006)**	(0.007)**						
	[0.010]**	[0.010]**						
Felony	0.404	0.359						
	(0.009)**	(0.010)**						
	[0.015]**	[0.016]**						
OpenFile	-0.032	-0.032	-0.032	-0.032	-0.026	-0.026	-0.026	-0.026
	(0.010)**	(0.011)**	(0.008)**	(0.009)**	(0.014)	(0.016)	(0.014)	(0.016)
	[0.008]**	[0.008]**	[0.009]**	[0.009]**	[0.017]	[0.017]	[0.017]	[0.017]
Felony*OpenFile	0.013	0.012	0.013	0.012	0.003	0.01	0.004	0.011
	(0.015)	(0.016)	(0.011)	(0.013)	(0.019)	(0.023)	(0.020)	(0.024)
	[0.014]	[0.015]	[0.015]	[0.015]	[0.025]	[0.029]	[0.025]	[0.030]
Felony*OpenFile* PreOpen							-0.019	-0.021
							(0.060)	(0.072)
							[0.039]	[0.045]
Court Fixed Effects	No	No	Yes	Yes	Yes	Yes	Yes	Yes
Court * Time Trend	No	No	No	No	Yes	Yes	Yes	Yes
R^2	0.694	0.596	0.833	0.781	0.848	0.822	0.848	0.822
N	1456	1453	1456	1453	1456	1453	1456	1453

Notes: * p<0.05, ** p<0.01

Table 3.4 presents the models for the dismissal rate. Model 1 estimates a near-zero and statistically insignificant decrease in the dismissal rate. Model 2, which excludes more serious felonies, is just slightly bigger. When a linear parameter for time is added for each court in Models 5 and 6, the estimates increase somewhat but remain statistically insignificant. Models 7 and 8 provide a separate DID term for early-adopter counties. The Superior*OpenFile*PreOpen term shows that the coefficient is dramatically larger for early adopters than other counties.

Table 3.5 presents the same set of models on the dismissal rate, but only using cases in the control group that were appealed *de novo* to the superior court. Contrary to the estimates in Table 3.4, Models 1 through 4 in Table 3.5 show a statistically significant increase in dismissals. The estimated effects all but disappear, however, when linear time parameters for each court are added in Models 5 and 6, suggesting that the estimated effects are explained by pre-treatment court-level time trends.

Table 3.6 presents the models estimating changes in the trial rate. Models 1 through 4 estimate a small and statistically insignificant decrease in the felony trial rate of -.003 or -.004. When a linear parameter for time for each court is added in Models 5 and 6, the coefficients remain relatively stable. As Models 7 and 8 reveal, counties that adopted open file prior to 2004 experienced a larger shift in the trial rate after 2004 than other counties.

Table 3.4: Main Models on Dismissal Rate

	(1) All	(2) Minor	(3) All	(4) Minor	(5) All	(6) Minor	(7) All	(8) Minor
Intercept	0.352	0.352						
	(0.006)**	(0.006)**						
	[0.011]**	[0.011]**						
Superior	-0.065	-0.016						
	(0.008)**	(0.009)						
	[0.016]**	[0.017]						
OpenFile	0.031	0.031	0.031	0.031	-0.006	-0.006	-0.006	-0.006
	(0.009)**	(0.011)**	(0.006)**	(0.007)**	(0.009)	(0.013)	(0.009)	(0.013)
	[0.004]**	[0.004]**	[0.004]**	[0.004]**	[0.006]	[0.006]	[0.006]	[0.006]
Superior*OpenFile	0.002	0.006	0.002	0.005	0.022	0.016	0.02	0.014
	(0.013)	(0.015)	(0.008)	(0.010)	(0.013)	(0.018)	(0.013)	(0.018)
	[0.011]	[0.013]	[0.012]	[0.014]	[0.017]	[0.022]	[0.018]	[0.023]
Superior*OpenFile* PreOpen							0.032	0.033
							(0.041)	(0.056)
							[0.040]	[0.042]
Court Fixed Effects	No	No	Yes	Yes	Yes	Yes	Yes	Yes
Court * Time Trend	No	No	No	No	Yes	Yes	Yes	Yes
R^2	0.077	0.016	0.673	0.580	0.741	0.693	0.741	0.693
N	1456	1453	1456	1453	1456	1453	1456	1453

Notes: * p<0.05, ** p<0.01

Table 3.5: Models on Dismissal Rate with Misdemeanor Cases Appealed De Novo

	(1) All	(2) Minor	(3) All	(4) Minor	(5) All	(6) Minor	(7) All	(8) Minor
Intercept	0.311	0.311						
	(0.007)**	(0.007)**						
	[0.012]**	[0.012]**						
Felony	-0.023	0.026						
	(0.010)*	(0.010)*						
	[0.016]	[0.018]						
OpenFile	0.002	0.002	0.002	0.002	0.006	0.006	0.006	0.006
	(0.011)	(0.012)	(0.008)	(0.009)	(0.014)	(0.016)	(0.014)	(0.016)
	[0.010]	[0.010]	[0.011]	[0.011]	[0.018]	[0.018]	[0.018]	[0.018]
Felony*OpenFile	0.03	0.034	0.03	0.033	0.01	0.004	0.008	0.002
	(0.016)	(0.017)*	(0.011)**	(0.013)*	(0.019)	(0.023)	(0.019)	(0.023)
	[0.015]*	[0.016]*	[0.016]	[0.017]	[0.024]	[0.028]	[0.024]	[0.028]
Felony*OpenFile* PreOpen							0.032	0.033
							(0.059)	(0.071)
							[0.040]	[0.042]
Court Fixed Effects	No	No	Yes	Yes	Yes	Yes	Yes	Yes
Court * Time Trend	No	No	No	No	Yes	Yes	Yes	Yes
R^2	0.006	0.021	0.511	0.510	0.575	0.628	0.575	0.628
N	1456	1453	1456	1453	1456	1453	1456	1453

Table 3.6: Main Models on Trial Rate

	(1) All	(2) Minor	(3) All	(4) Minor	(5) All	(6) Minor	(7) All	(8) Minor
Intercept	0.06	0.06						
	(0.002)**	(0.002)**						
	[0.004]**	[0.004]**						
Superior	-0.02	-0.025						
	(0.003)**	(0.003)**						
	[0.005]**	[0.006]**						
OpenFile	-0.01	-0.01	-0.01	-0.01	0.002	0.002	0.002	0.002
	(0.003)**	(0.004)**	(0.002)**	(0.003)**	(0.004)	(0.005)	(0.004)	(0.005)
	[0.002]**	[0.002]**	[0.002]**	[0.002]**	[0.002]	[0.002]	[0.002]	[0.002]
Superior*OpenFile	-0.003	-0.004	-0.003	-0.004	-0.007	-0.003	-0.006	-0.002
	(0.004)	(0.005)	(0.003)	(0.004)	(0.006)	(0.007)	(0.006)	(0.007)
	[0.004]	[0.004]	[0.004]	[0.004]	[0.006]	[0.009]	[0.006]	[0.009]
Superior*OpenFile* PreOpen							-0.01	-0.02
							(0.017)	(0.022)
							[0.012]	[0.017]
Court Fixed Effects	No	No	Yes	Yes	Yes	Yes	Yes	Yes
Court * Time Trend	No	No	No	No	Yes	Yes	Yes	Yes
R^2	0.074	0.083	0.509	0.512	0.586	0.652	0.586	0.652
N	1456	1453	1456	1453	1456	1453	1456	1453

Notes: * p<0.05, ** p<0.01

Table 3.7 presents the same models fit on data that only included cases in the control group if they were appealed *de novo* to the superior court. While Models 1 through 4 show a statistically significant decrease in the trial rate, Models 5 and 6 reveal that nearly all of the explanatory power of the DID term can be explained by pre-treatment court-specific time trends. Taken together, Table 3.6 and Table 3.7 provide little consistent evidence that open file affected the trial rate. While many of the coefficients are negative, the effects are relatively small. The statistical imprecision of the coefficients warrants some caution as even the most precise models can only rule out an effect size larger than .008.

Table 3.7: Models on Trial Rate with Misdemeanor Cases Appealed De Novo

	(1) All	(2) Minor	(3) All	(4) Minor	(5) All	(6) Minor	(7) All	(8) Minor
Intercept	0.03	0.03						
	(0.002)**	(0.002)**						
	[0.003]**	[0.003]**						
Felony	0.01	0.005						
	(0.003)**	(0.003)						
	[0.004]**	[0.005]						
OpenFile	-0.004	-0.004	-0.004	-0.004	-0.008	-0.008	-0.008	-0.008
	(0.003)	(0.004)	(0.003)	(0.003)	(0.005)	(0.006)	(0.005)	(0.006)
	[0.003]	[0.003]	[0.003]	[0.003]	[0.006]	[0.006]	[0.006]	[0.006]
Felony*OpenFile	-0.01	-0.011	-0.01	-0.011	0.003	0.007	0.003	0.008
	(0.005)*	(0.005)*	(0.004)*	(0.005)*	(0.008)	(0.009)	(0.008)	(0.009)
	[0.004]*	[0.005]*	[0.005]*	[0.005]*	[0.009]	[0.011]	[0.009]	[0.011]
Felony*OpenFile* PreOpen							-0.01	-0.02
							(0.024)	(0.027)
							[0.012]	[0.017]
Court Fixed Effects	No	No	Yes	Yes	Yes	Yes	Yes	Yes
Court * Time Trend	No	No	No	No	Yes	Yes	Yes	Yes
R^2	0.016	0.011	0.161	0.265	0.223	0.434	0.222	0.434
N	1456	1453	1456	1453	1456	1453	1456	1453

Notes: * p<0.05, ** p<0.01

B. Models by Public Defender Office

The previous section found little evidence that the open file statute affected the plea, dismissal or trial rate in North Carolina. It is possible, however, that open file had a larger effect in certain counties than others. Counties with public defender offices, for example, may be less affected by the statute because a larger number of defense attorneys are repeat players who form professional relationships with prosecutors (Eizenstein & Jacob 1977). These relationships may facilitate greater voluntary discovery even in the absence of legal requirements. Moreover, a centralized organization may help the defense bar advocate for more generous discovery practices from the local prosecutor's office. This section tests this hypothesis by refitting Models 1 and 2 on the plea and dismissal rates for the 76 counties that did not have a public defender's office during the entire

study period and the 15 counties that did.[72] The same tables for Models 5 and 6—with fixed effects and linear parameters for time for each court—are available in Table 3.25 through Table 3.27 in the Appendix.

Table 3.8 presents the models for guilty pleas. The first and second columns replicate the models for all counties in the state, and as before, estimate a small and statistically insignificant change in the plea rate. The third and fourth columns present the same models refit for the subset of counties that did not have a public defender's office during the study period. The DID terms, Superior*OpenFile, are similar in size. The fifth and sixth columns present the models refit for the subset of counties that have a public defender's office, and show DID coefficients that are larger but remain statistically insignificant.

Table 3.9 and Table 3.10 present a similar story for the dismissal and trial rates. Although the coefficients in Columns 3 through 6 in both tables point in opposite directions, they are all small and statistically insignificant.

In summary, the results remain substantively similar whether I examine all counties together or analyze counties without a public defender's office separately. The results consistently show no statistically significant change in the outcome variables after open file went into effect.

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[72] As noted, public defender's offices were established in three prosecutorial districts during the study period. The nine counties covered by these districts were excluded for all models.

Table 3.8: Model on Plea Rate, by Public Defender Office

	(1) All	(2) Minor	(3) All	(4) Minor	(5) All	(6) Minor
Intercept	0.369	0.369	0.374	0.374	0.345	0.345
	(0.005)**	(0.006)**	(0.006)**	(0.007)**	(0.009)**	(0.011)**
	[0.008]**	[0.008]**	[0.009]**	[0.009]**	[0.017]**	[0.017]**
Superior	0.278	0.232	0.262	0.217	0.361	0.31
	(0.008)**	(0.009)**	(0.009)**	(0.010)**	(0.013)**	(0.015)**
	[0.014]**	[0.015]**	[0.015]**	[0.017]**	[0.027]**	[0.028]**
OpenFile	-0.034	-0.034	-0.037	-0.037	-0.017	-0.017
	(0.009)**	(0.010)**	(0.010)**	(0.011)**	(0.015)	(0.018)
	[0.004]**	[0.004]**	[0.004]**	[0.004]**	[0.012]	[0.012]
Superior*OpenFile	0.016	0.014	0.015	0.01	0.019	0.032
	(0.013)	(0.014)	(0.014)	(0.016)	(0.022)	(0.025)
	[0.012]	[0.013]	[0.013]	[0.015]	[0.018]	[0.021]
Court Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Court * Time Trend	Yes	Yes	Yes	Yes	Yes	Yes
R^2	0.600	0.450	0.558	0.401	0.834	0.747
N	1456	1453	1216	1213	240	240

Notes: * p<0.05, ** p<0.01

Table 3.9: Model on Dismissal Rate, by Public Defender Office

	(1) All	(2) Minor	(3) All	(4) Minor	(5) All	(6) Minor
Intercept	0.352	0.352	0.342	0.342	0.403	0.403
	(0.006)**	(0.006)**	(0.006)**	(0.007)**	(0.010)**	(0.012)**
	[0.011]**	[0.011]**	[0.012]**	[0.012]**	[0.023]**	[0.023]**
Superior	-0.065	-0.016	-0.048	0.001	-0.153	-0.098
	(0.008)**	(0.009)	(0.009)**	(0.010)	(0.014)**	(0.016)**
	[0.016]**	[0.017]	[0.017]**	[0.019]	[0.030]**	[0.031]**
OpenFile	0.031	0.031	0.035	0.035	0.012	0.012
	(0.009)**	(0.011)**	(0.011)**	(0.012)**	(0.017)	(0.019)
	[0.004]**	[0.004]**	[0.004]**	[0.004]**	[0.012]	[0.012]
Superior*OpenFile	0.002	0.006	0.003	0.01	-0.003	-0.014
	(0.013)	(0.015)	(0.015)	(0.017)	(0.024)	(0.027)
	[0.011]	[0.013]	[0.013]	[0.015]	[0.020]	[0.023]
Court Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Court * Time Trend	Yes	Yes	Yes	Yes	Yes	Yes
R^2	0.077	0.016	0.049	0.018	0.429	0.217
N	1456	1453	1216	1213	240	240

Notes: *p<0.05, ** p<0.01

Table 3.10: Model on Trial Rate, by Public Defender Office

	(1) All	(2) Minor	(3) All	(4) Minor	(5) All	(6) Minor
Intercept	0.06	0.06	0.064	0.064	0.038	0.038
	(0.002)**	(0.002)**	(0.002)**	(0.003)**	(0.002)**	(0.002)**
	[0.004]**	[0.004]**	[0.005]**	[0.005]**	[0.005]**	[0.005]**
Superior	-0.02	-0.025	-0.021	-0.026	-0.011	-0.017
	(0.003)**	(0.003)**	(0.003)**	(0.004)**	(0.003)**	(0.003)**
	[0.005]**	[0.006]**	[0.006]**	[0.007]**	[0.006]	[0.006]**
OpenFile	-0.01	-0.01	-0.011	-0.011	-0.007	-0.007
	(0.003)**	(0.004)**	(0.004)**	(0.004)*	(0.003)*	(0.003)*
	[0.002]**	[0.002]**	[0.002]**	[0.002]**	[0.002]**	[0.002]**
Superior*OpenFile	-0.003	-0.004	-0.004	-0.005	0.002	0.003
	(0.004)	(0.005)	(0.005)	(0.006)	(0.005)	(0.005)
	[0.004]	[0.004]	[0.004]	[0.005]	[0.004]	[0.003]
Court Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Court * Time Trend	Yes	Yes	Yes	Yes	Yes	Yes
R^2	0.074	0.083	0.079	0.085	0.092	0.177
N	1456	1453	1216	1213	240	240

Notes: *p<0.05, ** p<0.01

VII. CONCLUSION

This Chapter began by contrasting the restrictive traditional rules of criminal discovery with the expansive open file system adopted by North Carolina in 2004. It also provided the first detailed discussion of how open file operates in practice. It next provided a theoretical analysis of criminal discovery, which showed that open file likely produced more favorable outcomes for defendants, and that—contrary to the predictions of legal scholars and advocates—open file is unlikely to result in fewer criminal trials. If open file does in fact decrease trials, it is not for the reasons provided by legal scholars and advocates. The theoretical analysis showed that open file is unlikely to avoid trials by eliminating information asymmetries in particular cases. It may, however, avoid trials by

fostering a system-wide faith among defendants that no additional evidence will be disclosed as a result of a decision to go to trial.

Finally, the Chapter conducted a partial test of these theoretical predictions by examining whether the open file statute affected the trial rate or improved case outcomes for defendants by increasing the dismissal of cases. Consistent with theoretical prediction, the models provide little empirical evidence that the open file statute affected the trial rate in one direction or the other. While I cannot rule out all theoretically significant effect sizes due to statistical imprecision, most of the coefficients are small, and contrary to the expectation of scholars and policy advocates, some are positive.

Furthermore, and inconsistent with theoretical prediction, the models provide little evidence that open file increased dismissals. Given the strong theoretical grounds for expecting that open file would improve case outcomes for defendants, what explains this result? One possibility is that enhanced access to discovery primarily helps defendants combat overcharging. Once the government decides there is sufficient evidence to charge a defendant with a particular offense, there are strong incentives to stack additional charges, which can be used as bargaining chips in plea negotiation and can be dismissed any time with little marginal cost to the prosecution. Open file may be particularly helpful to defendants to negotiate plea agreements that exclude such overcharged offenses. By contrast, the government has less incentive to overcharge in cases where there is insufficient evidence for any offenses. In this context, overcharging imposes significant costs as it requires the prosecution to process a case it wouldn't otherwise—a case that would likely lose at trial. Thus, expanding criminal discovery may

help defendants secure more favorable plea agreements even if it does not help increase the number of cases that are dismissed. Future research should use charge-level data to examine whether open file affects the content of plea agreements and not merely the probability of a case being dismissed.

Another important question is whether we can generalize the empirical results to other jurisdictions considering adopting an open file policy. There are good reasons to think we can. First, prior to open file the basic legal furniture of discovery in North Carolina was similar to that of the federal courts and many state courts. As federal constitutional law, *Brady* is in effect everywhere. And North Carolina's narrow discovery statute prior to open file was similar to that of the federal courts and a substantial number of states. Second, many of the implementation problems in traditional systems of criminal discovery are ubiquitous: misaligned prosecutorial norms about convictions and justice, difficult *ex ante* predictions about materiality, psychological bias, and insufficient resources. Third, while precise comparisons are impossible, North Carolina appears representative of other courts on some basic metrics. The median time-to-disposition in felony cases in North Carolina is roughly similar to that of the typical state and federal court, and the composition of charges appears roughly representative of the national state-court average (North Carolina Courts 2003; Sourcebook of Criminal Justice Statistics tbls. 5.50.2002, 5.44.2004). Discovery protections are perhaps most important in complex cases with a great deal of evidence, and high-profile cases where prosecutors experience significant pressures to secure convictions. Open file might exert even greater

effects in the federal system, where cases may be more complex and under greater public scrutiny.

A number of limitations in the current study leave open important avenues for future research. First, and as noted already, I was unable to investigate whether the content of plea agreements became more favorable to defendants after open file went into effect. Future research may examine whether the decrease in guilty pleas was primarily due to a decrease in straight pleas, or a decrease in pleas to a lesser offense. Even better, future research may examine case-level charge and conviction data.

Second, although significant anecdotal evidence confirmed an increase in the speed and volume of disclosure after open file went into effect, systematic data on these processes were not available. Future research should examine data on the volume and timing of discovery before and after a statutory expansion of discovery rights. It may be possible to use certain court filings, such as motions to suppress, as a proxy.

Third, I excluded several crime categories—controlled substance, fraudulent activity, forgery and uttering, and "other" offenses—from the primary analysis due to observable compositional changes in the data. The results for the trial rate are substantively similar when these crimes are included in the analysis. The results for nearly all of the crime categories were also substantively similar for the dismissal and plea rate (see Table 3.11 through Table 3.22 in the Appendix). The one exception is forgery and utterings, for which the models estimate a substantial reduction in pleas and increase in dismissals. Due to the absence of effects on the other crime categories, and due to the dramatic drop in forgery and uttering filings recorded in the data after 2004

(see Figure 3.3), these results likely represent an artifact of the compositional change rather than a true effect of open file. Still, the difference in results for forgery and utterings leaves some ambiguity about the causal effects of open file, and future research should examine forgery crimes more closely.

Fourth, even if open file does not decrease trials, it may contribute to judicial efficiency by decreasing the time required for defendants to accept a plea agreement or for the prosecution to dismiss a case. While this paper presented some descriptive data showing that the time-to-disposition for median and 90th percentile case increased slightly after the open file law went into effect, future research can apply econometric methods to determine whether expansions in criminal discovery affect timing.

Fifth, while the current study provides valuable correlational evidence about open file, data limitations and a simultaneous roll out in nearly all counties in North Carolina preclude firm claims about causality. The results may also be biased by a slow rollout of open file procedures when the law went into effect. Future research may consider examining natural experiments that allow for stronger identification strategies. Moreover, the current paper only examines the short-term effect of open file in its first few years of operation. This decision was driven by methodological challenges in identifying longer-term effects. It is possible, however, that the effects of open file change over time as the courts clarify the contours of the law, as counties improve their discovery systems, and as the culture of open file discovery takes hold.

Sixth, future research can examine the effects of statutory expansions to discovery in other jurisdictions. Texas, for example, enacted the Michael Morton Act in 2013,

which dramatically expanded the discovery rights of criminal defendants. As in North Carolina, the new statute entitles defendants to receive recorded witness statements, police reports, documents, and tangible evidence that is "material to any matter involved" in the case. While some attorneys have interpreted the law to provide discovery for "everything" in the prosecutorial file (Grissom 2013), some prosecutors have, as in the early days of the North Carolina statute, withheld evidence based on an expansive reading of the attorney work-product exception (Texas Appleseed & Texas Defender Service 2015). In one recorded case, a prosecutor claimed that the identity of an exculpatory witness was covered by the exception because he discovered the witness through his own investigative efforts and not the efforts of the police (Texas Appleseed & Texas Defender Service 2015). The effects of the Texas law will no doubt depend on how expansively the courts interpret the Act, but it is likely to have a similar effect on case outcomes as the North Carolina law. Other states that have enacted less expansive discovery reforms in recent years, such as Ohio, may also provide useful evidence on the effect of liberalized discovery (Ohio R. Crim. P. 16).

Finally, through the small number of interviews I conducted with attorneys in North Carolina, I found little anecdotal evidence that open file significantly increases the risk of witness intimidation or perjury. These empirical questions are at the heart of the open file debate. Systematic empirical evidence demonstrating that there is little risk of intimidation and perjury would be valuable to other states deciding whether to adopt open file policies.

APPENDIX

Table 3.11: Main Models on Plea Rate, Including Controlled Substance Offenses

	(1) All	(2) Minor	(3) All	(4) Minor	(5) All	(6) Minor	(7) All	(8) Minor
Intercept	0.369	0.369						
	(0.005)**	(0.006)**						
	[0.008]**	[0.008]**						
Superior	0.259	0.232						
	(0.007)**	(0.008)**						
	[0.014]**	[0.015]**						
OpenFile	-0.034	-0.034	-0.034	-0.034	0.008	0.008	0.008	0.008
	(0.009)**	(0.009)**	(0.005)**	(0.006)**	(0.008)	(0.009)	(0.008)	(0.009)
	[0.004]**	[0.004]**	[0.004]**	[0.004]**	[0.006]	[0.006]	[0.006]	[0.006]
Superior*OpenFile	0.016	0.018	0.016	0.018	-0.026	-0.021	-0.025	-0.021
	(0.012)	(0.013)	(0.007)*	(0.008)*	(0.011)*	(0.013)	(0.011)*	(0.013)
	[0.011]	[0.011]	[0.012]	[0.012]	[0.015]	[0.017]	[0.016]	[0.018]
Superior*OpenFile* PreOpen							-0.005	0.007
							(0.035)	(0.039)
							[0.033]	[0.032]
Court Fixed Effects	No	No	Yes	Yes	Yes	Yes	Yes	Yes
Court * Time Trend	No	No	No	No	Yes	Yes	Yes	Yes
R^2	0.587	0.506	0.857	0.839	0.899	0.897	0.899	0.897
N	1456	1453	1456	1453	1456	1453	1456	1453

Table 3.12: Main Models on Dismissal Rate, Including Controlled Substance Offenses

	(1) All	(2) Minor	(3) All	(4) Minor	(5) All	(6) Minor	(7) All	(8) Minor
Intercept	0.352	0.352						
	(0.006)**	(0.006)**						
	[0.011]**	[0.011]**						
Superior	-0.031	0.006						
	(0.008)**	(0.009)						
	[0.016]	[0.017]						
OpenFile	0.031	0.031	0.031	0.031	-0.006	-0.006	-0.006	-0.006
	(0.009)**	(0.010)**	(0.005)**	(0.006)**	(0.008)	(0.009)	(0.008)	(0.009)
	[0.004]**	[0.004]**	[0.004]**	[0.004]**	[0.006]	[0.006]	[0.006]	[0.006]
Superior*OpenFile	-0.002	-0.008	-0.002	-0.008	0.022	0.019	0.021	0.019
	(0.013)	(0.014)	(0.007)	(0.008)	(0.011)	(0.013)	(0.011)	(0.013)
	[0.011]	[0.012]	[0.012]	[0.013]	[0.015]	[0.017]	[0.016]	[0.018]
Superior*OpenFile* PreOpen							0.013	-0.003
							(0.035)	(0.039)
							[0.037]	[0.035]
Court Fixed Effects	No	No	Yes	Yes	Yes	Yes	Yes	Yes
Court * Time Trend	No	No	No	No	Yes	Yes	Yes	Yes
R^2	0.028	0.010	0.722	0.725	0.801	0.828	0.801	0.828
N	1456	1453	1456	1453	1456	1453	1456	1453

Table 3.13: Main Models on Trial Rate, Including Controlled Substance Offenses

	(1) All	(2) Minor	(3) All	(4) Minor	(5) All	(6) Minor	(7) All	(8) Minor
Intercept	0.06	0.06						
	(0.002)**	(0.002)**						
	[0.004]**	[0.004]**						
Superior	-0.03	-0.039						
	(0.002)**	(0.002)**						
	[0.005]**	[0.005]**						
OpenFile	-0.01	-0.01	-0.01	-0.01	0.002	0.002	0.002	0.002
	(0.003)**	(0.003)**	(0.002)**	(0.001)**	(0.003)	(0.002)	(0.003)	(0.002)
	[0.002]**	[0.002]**	[0.002]**	[0.002]**	[0.002]	[0.002]	[0.002]	[0.002]
Superior*OpenFile	0	0.004	0	0.004	-0.006	-0.002	-0.006	-0.002
	(0.004)	(0.004)	(0.002)	(0.002)	(0.004)	(0.003)	(0.004)	(0.003)
	[0.003]	[0.002]	[0.003]	[0.003]	[0.004]	[0.004]	[0.004]	[0.004]
Superior*OpenFile* PreOpen							-0.005	-0.007
							(0.012)	(0.010)
							[0.008]	[0.009]
Court Fixed Effects	No	No	Yes	Yes	Yes	Yes	Yes	Yes
Court * Time Trend	No	No	No	No	Yes	Yes	Yes	Yes
R^2	0.176	0.253	0.709	0.793	0.764	0.862	0.763	0.862
N	1456	1453	1456	1453	1456	1453	1456	1453

Table 3.14: Main Models on Plea Rate, Including Fraudulent Activity Offenses

	(1) All	(2) Minor	(3) All	(4) Minor	(5) All	(6) Minor	(7) All	(8) Minor
Intercept	0.369	0.369						
	(0.006)**	(0.006)**						
	[0.008]**	[0.008]**						
Superior	0.274	0.243						
	(0.008)**	(0.009)**						
	[0.014]**	[0.015]**						
OpenFile	-0.034	-0.034	-0.034	-0.034	0.008	0.008	0.008	0.008
	(0.009)**	(0.011)**	(0.006)**	(0.008)**	(0.010)	(0.014)	(0.010)	(0.014)
	[0.004]**	[0.004]**	[0.004]**	[0.004]**	[0.006]	[0.006]	[0.006]	[0.006]
Superior*OpenFile	0.004	-0.005	0.004	-0.006	-0.035	-0.037	-0.035	-0.037
	(0.013)	(0.015)	(0.009)	(0.011)	(0.014)*	(0.019)	(0.014)*	(0.020)
	[0.012]	[0.014]	[0.013]	[0.015]	[0.019]	[0.026]	[0.020]	[0.028]
Superior*OpenFile* PreOpen							0	0.01
							(0.044)	(0.060)
							[0.032]	[0.050]
Court Fixed Effects	No	No	Yes	Yes	Yes	Yes	Yes	Yes
Court * Time Trend	No	No	No	No	Yes	Yes	Yes	Yes
R^2	0.567	0.439	0.813	0.734	0.854	0.801	0.853	0.801
N	1456	1453	1456	1453	1456	1453	1456	1453

Notes: *p<0.05, ** p<0.01

Table 3.15: Main Models on Dismissal Rate, Including Fraudulent Activity Offenses

	(1) All	(2) Minor	(3) All	(4) Minor	(5) All	(6) Minor	(7) All	(8) Minor
Intercept	0.352	0.352						
	(0.006)**	(0.007)**						
	[0.011]**	[0.011]**						
Superior	-0.054	-0.012						
	(0.009)**	(0.010)						
	[0.016]**	[0.017]						
OpenFile	0.031	0.031	0.031	0.031	-0.006	-0.006	-0.006	-0.006
	(0.010)**	(0.011)**	(0.006)**	(0.008)**	(0.010)	(0.013)	(0.010)	(0.013)
	[0.004]**	[0.004]**	[0.004]**	[0.004]**	[0.006]	[0.006]	[0.006]	[0.006]
Superior*OpenFile	0.013	0.018	0.013	0.018	0.029	0.029	0.028	0.03
	(0.014)	(0.016)	(0.008)	(0.011)	(0.014)*	(0.018)	(0.014)*	(0.018)
	[0.012]	[0.014]	[0.013]	[0.015]	[0.019]	[0.025]	[0.020]	[0.026]
Superior*OpenFile* PreOpen							0.005	-0.007
							(0.043)	(0.056)
							[0.036]	[0.052]
Court Fixed Effects	No	No	Yes	Yes	Yes	Yes	Yes	Yes
Court * Time Trend	No	No	No	No	Yes	Yes	Yes	Yes
R^2	0.052	0.019	0.659	0.604	0.735	0.717	0.735	0.717
N	1456	1453	1456	1453	1456	1453	1456	1453

Table 3.16: Main Models on Trial Rate, Including Fraudulent Activity Offenses

	(1) All	(2) Minor	(3) All	(4) Minor	(5) All	(6) Minor	(7) All	(8) Minor
Intercept	0.06	0.06						
	(0.002)**	(0.002)**						
	[0.004]**	[0.004]**						
Superior	-0.025	-0.035						
	(0.003)**	(0.003)**						
	[0.005]**	[0.005]**						
OpenFile	-0.01	-0.01	-0.01	-0.01	0.002	0.002	0.002	0.002
	(0.003)**	(0.003)**	(0.002)**	(0.002)**	(0.003)	(0.004)	(0.003)	(0.004)
	[0.002]**	[0.002]**	[0.002]**	[0.002]**	[0.002]	[0.002]	[0.002]	[0.002]
Superior*OpenFile	-0.001	0.003	-0.001	0.003	-0.007	-0.002	-0.007	-0.001
	(0.004)	(0.004)	(0.003)	(0.003)	(0.005)	(0.005)	(0.005)	(0.005)
	[0.003]	[0.004]	[0.003]	[0.004]	[0.005]	[0.005]	[0.005]	[0.006]
Superior*OpenFile* PreOpen							-0.005	-0.012
							(0.014)	(0.017)
							[0.011]	[0.011]
Court Fixed Effects	No	No	Yes	Yes	Yes	Yes	Yes	Yes
Court * Time Trend	No	No	No	No	Yes	Yes	Yes	Yes
R^2	0.116	0.154	0.614	0.611	0.684	0.732	0.683	0.732
N	1456	1453	1456	1453	1456	1453	1456	1453

Notes: *p<0.05, ** p<0.01

Table 3.17: Main Models on Plea Rate, Including Forgery & Uttering Offenses

	(1) All	(2) Minor	(3) All	(4) Minor	(5) All	(6) Minor	(7) All	(8) Minor
Intercept	0.369	0.369						
	(0.006)**	(0.006)**						
	[0.008]**	[0.008]**						
Superior	0.293	0.28						
	(0.008)**	(0.009)**						
	[0.014]**	[0.015]**						
OpenFile	-0.034	-0.034	-0.034	-0.034	0.008	0.008	0.008	0.008
	(0.009)**	(0.010)**	(0.006)**	(0.008)**	(0.010)	(0.014)	(0.010)	(0.014)
	[0.004]**	[0.004]**	[0.004]**	[0.004]**	[0.006]	[0.006]	[0.006]	[0.006]
Superior*OpenFile	-0.004	-0.028	-0.004	-0.028	-0.043	-0.051	-0.043	-0.051
	(0.013)	(0.015)	(0.009)	(0.011)**	(0.014)**	(0.020)*	(0.015)**	(0.020)*
	[0.012]	[0.013]*	[0.013]	[0.014]*	[0.019]*	[0.024]*	[0.020]*	[0.026]*
Superior*OpenFile* PreOpen							-0.01	-0.003
							(0.045)	(0.062)
							[0.042]	[0.045]
Court Fixed Effects	No	No	Yes	Yes	Yes	Yes	Yes	Yes
Court * Time Trend	No	No	No	No	Yes	Yes	Yes	Yes
R^2	0.606	0.501	0.826	0.761	0.857	0.807	0.856	0.807
N	1456	1453	1456	1453	1456	1453	1456	1453

Table 3.18: Main Models on Dismissal Rate, Including Forgery & Uttering Offenses

	(1) All	(2) Minor	(3) All	(4) Minor	(5) All	(6) Minor	(7) All	(8) Minor
Intercept	0.352	0.352						
	(0.006)**	(0.007)**						
	[0.011]**	[0.011]**						
Superior	-0.072	-0.048						
	(0.008)**	(0.009)**						
	[0.016]**	[0.017]**						
OpenFile	0.031	0.031	0.031	0.031	-0.006	-0.006	-0.006	-0.006
	(0.010)**	(0.011)**	(0.006)**	(0.007)**	(0.010)	(0.013)	(0.010)	(0.013)
	[0.004]**	[0.004]**	[0.004]**	[0.004]**	[0.006]	[0.006]	[0.006]	[0.006]
Superior*OpenFile	0.017	0.038	0.017	0.038	0.04	0.05	0.039	0.05
	(0.014)	(0.015)*	(0.008)*	(0.010)**	(0.014)**	(0.018)**	(0.014)**	(0.019)**
	[0.012]	[0.013]**	[0.013]	[0.014]**	[0.018]*	[0.022]*	[0.018]*	[0.023]*
Superior*OpenFile* PreOpen							0.013	-0.002
							(0.042)	(0.057)
							[0.045]	[0.040]
Court Fixed Effects	No	No	Yes	Yes	Yes	Yes	Yes	Yes
Court * Time Trend	No	No	No	No	Yes	Yes	Yes	Yes
R^2	0.083	0.045	0.672	0.613	0.739	0.705	0.739	0.705
N	1456	1453	1456	1453	1456	1453	1456	1453

Notes: *p<0.05, ** p<0.01

Table 3.19: Main Models on Trial Rate, Including Forgery & Uttering Offenses

	(1) All	(2) Minor	(3) All	(4) Minor	(5) All	(6) Minor	(7) All	(8) Minor
Intercept	0.06	0.06						
	(0.002)**	(0.002)**						
	[0.004]**	[0.004]**						
Superior	-0.026	-0.036						
	(0.002)**	(0.003)**						
	[0.005]**	[0.005]**						
OpenFile	-0.01	-0.01	-0.01	-0.01	0.002	0.002	0.002	0.002
	(0.003)**	(0.003)**	(0.002)**	(0.002)**	(0.003)	(0.004)	(0.003)	(0.004)
	[0.002]**	[0.002]**	[0.002]**	[0.002]**	[0.002]	[0.002]	[0.002]	[0.002]
Superior*OpenFile	0	0.002	0	0.002	-0.009	-0.008	-0.009	-0.008
	(0.004)	(0.004)	(0.003)	(0.003)	(0.005)*	(0.006)	(0.005)	(0.006)
	[0.003]	[0.003]	[0.003]	[0.003]	[0.005]	[0.007]	[0.006]	[0.008]
Superior*OpenFile* PreOpen							-0.003	-0.003
							(0.015)	(0.018)
							[0.012]	[0.014]
Court Fixed Effects	No	No	Yes	Yes	Yes	Yes	Yes	Yes
Court * Time Trend	No	No	No	No	Yes	Yes	Yes	Yes
R^2	0.121	0.164	0.611	0.625	0.657	0.701	0.656	0.701
N	1456	1453	1456	1453	1456	1453	1456	1453

Table 3.20: Main Models on Plea Rate, Including Other Offenses

	(1) All	(2) Minor	(3) All	(4) Minor	(5) All	(6) Minor	(7) All	(8) Minor
Intercept	0.369	0.369	0.442	0.442	0.447	0.447	0.447	0.447
	(0.005)**	(0.006)**	(0.027)**	(0.035)**	(0.045)**	(0.061)**	(0.045)**	(0.061)**
	[0.008]**	[0.008]**	[0.002]**	[0.002]**	[0.001]**	[0.001]**	[0.001]**	[0.001]**
Superior	0.278	0.232	0.178	0.068	0.202	0.063	0.202	0.063
	(0.008)**	(0.009)**	(0.039)**	(0.050)	(0.063)**	(0.086)	(0.063)**	(0.086)
	[0.014]**	[0.015]**	[0.005]**	[0.005]**	[0.005]**	[0.006]**	[0.005]**	[0.006]**
OpenFile	-0.034	-0.034	-0.034	-0.034	0.008	0.008	0.008	0.008
	(0.009)**	(0.010)**	(0.006)**	(0.008)**	(0.010)	(0.013)	(0.010)	(0.013)
	[0.004]**	[0.004]**	[0.004]**	[0.004]**	[0.006]	[0.006]	[0.006]	[0.006]
Superior*OpenFile	0.016	0.014	0.016	0.014	-0.031	-0.024	-0.03	-0.023
	(0.013)	(0.014)	(0.008)	(0.011)	(0.014)*	(0.019)	(0.014)*	(0.019)
	[0.012]	[0.013]	[0.012]	[0.013]	[0.019]	[0.023]	[0.020]	[0.025]
Superior*OpenFile* PreOpen							-0.019	-0.021
							(0.044)	(0.059)
							[0.039]	[0.045]
Court Fixed Effects	No	No	Yes	Yes	Yes	Yes	Yes	Yes
Court * Time Trend	No	No	No	No	Yes	Yes	Yes	Yes
R^2	0.600	0.450	0.824	0.731	0.858	0.792	0.858	0.792
N	1456	1453	1456	1453	1456	1453	1456	1453

Notes: *p<0.05, ** p<0.01

Table 3.21: Main Models on Dismissal Rate, Including Other Offenses

	(1) All	(2) Minor	(3) All	(4) Minor	(5) All	(6) Minor	(7) All	(8) Minor
Intercept	0.352	0.352	0.211	0.211	0.202	0.202	0.202	0.202
	(0.006)**	(0.006)**	(0.026)**	(0.034)**	(0.042)**	(0.058)**	(0.042)**	(0.058)**
	[0.011]**	[0.011]**	[0.002]**	[0.002]**	[0.001]**	[0.001]**	[0.001]**	[0.001]**
Superior	-0.065	-0.016	0.099	0.25	0.033	0.26	0.033	0.259
	(0.008)**	(0.009)	(0.037)**	(0.048)**	(0.059)	(0.082)**	(0.059)	(0.082)**
	[0.016]**	[0.017]	[0.005]**	[0.005]**	[0.004]**	[0.005]**	[0.004]**	[0.006]**
OpenFile	0.031	0.031	0.031	0.031	-0.006	-0.006	-0.006	-0.006
	(0.009)**	(0.011)**	(0.006)**	(0.007)**	(0.009)	(0.013)	(0.009)	(0.013)
	[0.004]**	[0.004]**	[0.004]**	[0.004]**	[0.006]	[0.006]	[0.006]	[0.006]
Superior*OpenFile	0.002	0.006	0.002	0.005	0.022	0.016	0.02	0.014
	(0.013)	(0.015)	(0.008)	(0.010)	(0.013)	(0.018)	(0.013)	(0.018)
	[0.011]	[0.013]	[0.012]	[0.014]	[0.017]	[0.022]	[0.018]	[0.023]
Superior*OpenFile* PreOpen							0.032	0.033
							(0.041)	(0.056)
							[0.040]	[0.042]
Court Fixed Effects	No	No	Yes	Yes	Yes	Yes	Yes	Yes
Court * Time Trend	No	No	No	No	Yes	Yes	Yes	Yes
R^2	0.077	0.016	0.673	0.580	0.741	0.693	0.741	0.693
N	1456	1453	1456	1453	1456	1453	1456	1453

Table 3.22: Main Models on Trial Rate, Including Other Offenses

	(1) All	(2) Minor	(3) All	(4) Minor	(5) All	(6) Minor	(7) All	(8) Minor
Intercept	0.06	0.06	0.067	0.067	0.07	0.07	0.07	0.07
	(0.002)**	(0.002)**	(0.011)**	(0.013)**	(0.018)**	(0.022)**	(0.018)**	(0.022)**
	[0.004]**	[0.004]**	[0.001]**	[0.001]**	[0.000]**	[0.000]**	[0.000]**	[0.000]**
Superior	-0.02	-0.025	0	-0.057	0.04	-0.067	0.04	-0.067
	(0.003)**	(0.003)**	(0.015)	(0.019)**	(0.025)	(0.031)*	(0.025)	(0.031)*
	[0.005]**	[0.006]**	[0.001]	[0.002]**	[0.001]**	[0.002]**	[0.002]**	[0.002]**
OpenFile	-0.01	-0.01	-0.01	-0.01	0.002	0.002	0.002	0.002
	(0.003)**	(0.004)**	(0.002)**	(0.003)**	(0.004)	(0.005)	(0.004)	(0.005)
	[0.002]**	[0.002]**	[0.002]**	[0.002]**	[0.002]	[0.002]	[0.002]	[0.002]
Superior*OpenFile	-0.003	-0.004	-0.003	-0.004	-0.007	-0.003	-0.006	-0.002
	(0.004)	(0.005)	(0.003)	(0.004)	(0.006)	(0.007)	(0.006)	(0.007)
	[0.004]	[0.004]	[0.004]	[0.004]	[0.006]	[0.009]	[0.006]	[0.009]
Superior*OpenFile* PreOpen							-0.01	-0.02
							(0.017)	(0.022)
							[0.012]	[0.017]
Court Fixed Effects	No	No	Yes	Yes	Yes	Yes	Yes	Yes
Court * Time Trend	No	No	No	No	Yes	Yes	Yes	Yes
R^2	0.074	0.083	0.509	0.512	0.586	0.652	0.586	0.652
N	1456	1453	1456	1453	1456	1453	1456	1453

Notes: *p<0.05, ** p<0.01

Table 3.23: Models With Data Aggregated to Two Time Periods

	Plea	Dismissal	Trial
All Felonies	0.016	0.002	-0.003
	(0.021)	(0.023)	(0.007)
Minor Felonies	0.012	0.007	-0.004
	(0.022)	(0.024)	(0.007)
All Felonies, Appellate	0.013	0.030	-0.010
	(0.023)	(0.026)	(0.005)
Minor Felonies, Appellate	0.010	0.036	-0.011
	(0.024)	(0.026)	(0.006)

Notes: *p<0.05, ** p<0.01

Table 3.24: Models With Coefficients for Disposition Counts by Crime Type

	Plea Rate		Dismissal Rate		Trial Rate	
	Coef	SE	Coef	SE	Coef	SE
Intercept	0.4725	(0.0437)**	0.1606	(0.0439)**	0.08033	(0.01158)**
		[0.0300]**		[0.0251]**		[0.00709]**
Superior	0.0756	(0.0496)	0.2371	(0.0498)**	-0.03648	(0.01314)**
		[0.0306]*		[0.0258]**		[0.00715]**
OpenFile	-0.0332	(0.0050)**	0.0304	(0.0051)**	-0.01023	(0.00133)**
		[0.0045]**		[0.0045]**		[0.00171]**
Arson	-0.0002	(0.0008)	0.0004	(0.0008)	-0.00001	(0.00020)
		[0.0007]**		[0.0007]		[0.00014]
Assault	-0.0005	(0.0002)	0.0004	(0.0002)	-0.00003	(0.00006)
		[0.0003]**		[0.0003]		[0.00004]
Burglary	0.0001	(0.0001)	0.0000	(0.0001)	-0.00001	(0.00002)
		[0.0001]		[0.0001]		[0.00001]
Controlled Substances	-0.0001	(0.0000)**	0.0001	(0.0000)**	-0.00001	(0.00001)
		[0.0000]**		[0.0000]*		[0.00000]
Forgery & Utterings	0.0002	(0.0001)**	-0.0002	(0.0001)**	-0.00002	(0.00002)
		[0.0001]		[0.0001]		[0.00001]*
Fraudulent Activity	-0.0001	(0.0001)	0.0001	(0.0001)*	-0.00003	(0.00001)*
		[0.0001]**		[0.0001]		[0.00001]**
Larceny	-0.0001	(0.0001)	0.0001	(0.0001)	-0.00003	(0.00003)
		[0.0002]		[0.0002]		[0.00002]
Manslaughter	-0.0004	(0.0024)	-0.0007	(0.0024)	-0.00034	(0.00063)
		[0.0029]**		[0.0028]		[0.00049]
Misdemeanors	0.0000	(0.0000)	0.0000	(0.0000)	0.00000	(0.00000)
		[0.0000]		[0.0000]*		[0.00000]
Murder	-0.0003	(0.0008)	0.0008	(0.0008)	0.00000	(0.00021)
		[0.0009]		[0.0009]		[0.00014]
Other	0.0001	(0.0000)	-0.0001	(0.0000)**	0.00000	(0.00001)
		[0.0001]		[0.0001]*		[0.00001]
Other Sex Offenses	-0.0007	(0.0002)**	0.0006	(0.0002)**	0.00005	(0.00004)
		[0.0003]**		[0.0003]*		[0.00004]
Rape	-0.0003	(0.0002)	0.0004	(0.0002)*	0.00000	(0.00004)
		[0.0002]		[0.0002]		[0.00003]
Robbery	-0.0002	(0.0001)	0.0002	(0.0001)	0.00001	(0.00003)
		[0.0002]		[0.0002]		[0.00002]
Superior*OpenFile	-0.0180	(0.0077)*	0.0212	(0.0077)**	0.00194	(0.00203)
		[0.0131]		[0.0128]		[0.00286]
Court Fixed Effects	Yes		Yes		Yes	
Court * Time Trend	No		No		No	
R^2	0.8480		0.7312		0.7736	
N	1456		1456		1456	

Notes: *p<0.05, ** p<0.01

Table 3.25: Model on Plea Rate With Fixed Effects and Time Parameters for Each Court, by Public Defender Office

	(1) All	(2) Minor	(3) All	(4) Minor	(5) All	(6) Minor
OpenFile	0.008	0.008	0.009	0.009	0.001	0.001
	(0.010)	(0.013)	(0.012)	(0.016)	(0.016)	(0.021)
	[0.006]	[0.006]	[0.006]	[0.006]	[0.015]	[0.015]
Superior*OpenFile	-0.031	-0.024	-0.033	-0.032	-0.022	0.013
	(0.014)*	(0.019)	(0.016)*	(0.022)	(0.022)	(0.030)
	[0.019]	[0.023]	[0.022]	[0.027]	[0.029]	[0.031]
Court Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Court * Time Trend	Yes	Yes	Yes	Yes	Yes	Yes
R^2	0.858	0.792	0.834	0.762	0.952	0.922
N	1456	1453	1216	1213	240	240

Table 3.26: Model on Dismissal Rate With Fixed Effects and Time Parameters for Each Court, by Public Defender Office

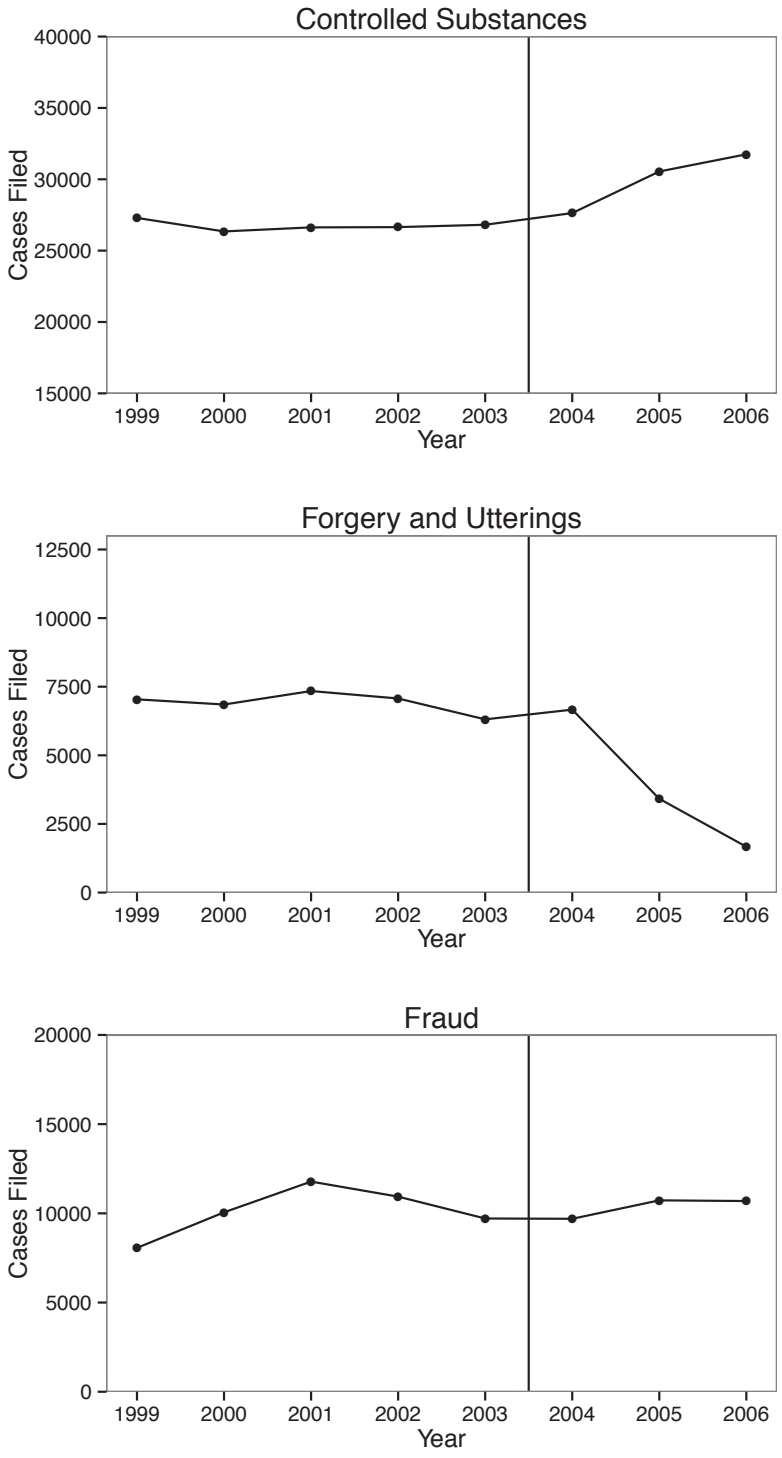
	(1) All	(2) Minor	(3) All	(4) Minor	(5) All	(6) Minor
OpenFile	-0.006	-0.006	-0.008	-0.008	0.002	0.002
	(0.009)	(0.013)	(0.011)	(0.015)	(0.015)	(0.021)
	[0.006]	[0.006]	[0.006]	[0.006]	[0.017]	[0.017]
Superior*OpenFile	0.022	0.016	0.024	0.022	0.009	-0.012
	(0.013)	(0.018)	(0.015)	(0.021)	(0.021)	(0.030)
	[0.017]	[0.022]	[0.019]	[0.025]	[0.032]	[0.036]
Court Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Court * Time Trend	Yes	Yes	Yes	Yes	Yes	Yes
R^2	0.741	0.693	0.720	0.683	0.871	0.783
N	1456	1453	1216	1213	240	240

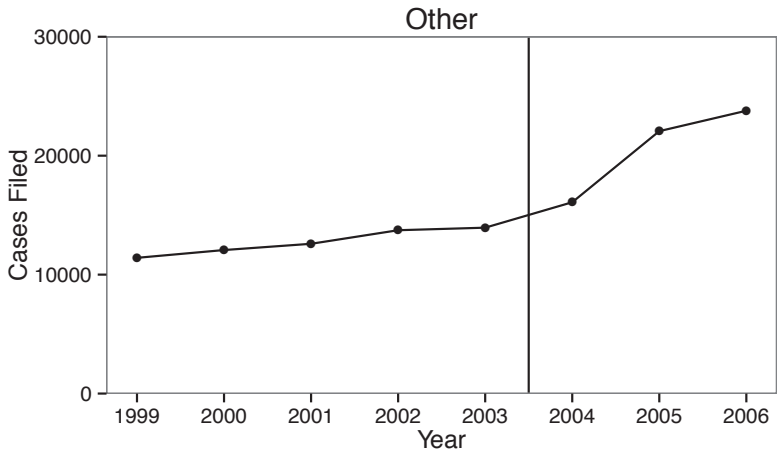
Table 3.27: Model on Trial Rate With Fixed Effects and Time Parameters for Each Court, by Public Defender Office

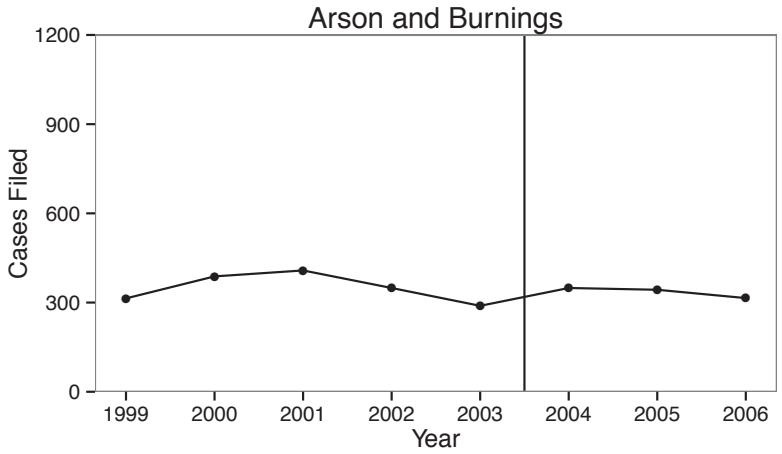
	(1) All	(2) Minor	(3) All	(4) Minor	(5) All	(6) Minor
OpenFile	0.002	0.002	0.003	0.003	-0.003	-0.003
	(0.004)	(0.005)	(0.005)	(0.006)	(0.004)	(0.004)
	[0.002]	[0.002]	[0.002]	[0.002]	[0.003]	[0.003]
Superior*OpenFile	-0.007	-0.003	-0.01	-0.005	0.006	0.006
	(0.006)	(0.007)	(0.007)	(0.008)	(0.006)	(0.006)
	[0.006]	[0.009]	[0.007]	[0.011]	[0.010]	[0.007]
Court Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Court * Time Trend	Yes	Yes	Yes	Yes	Yes	Yes
R^2	0.586	0.652	0.575	0.644	0.618	0.717
N	1456	1453	1216	1213	240	240

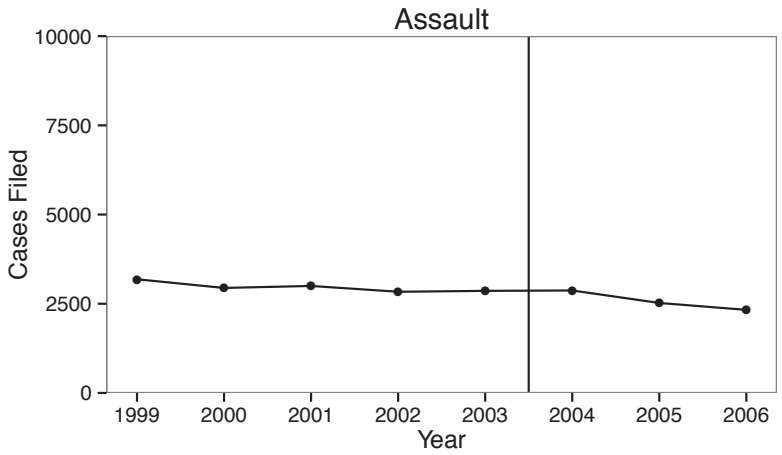
Notes: *p<0.05, ** p<0.01

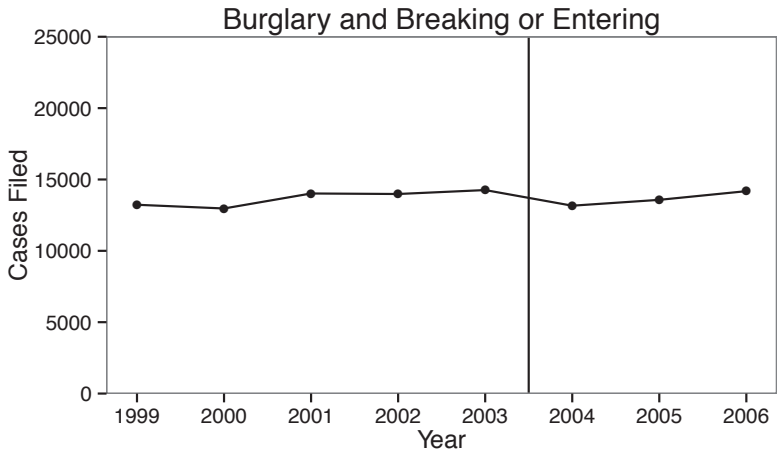
Figure 3.6: Total Number of Cases Filed by Crime Type

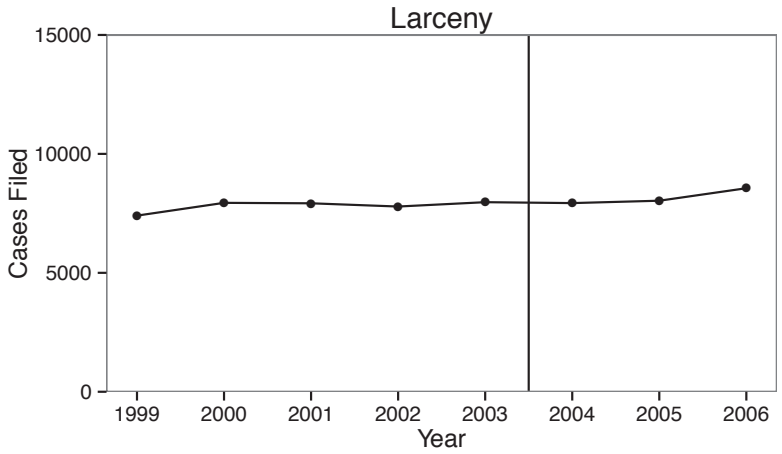


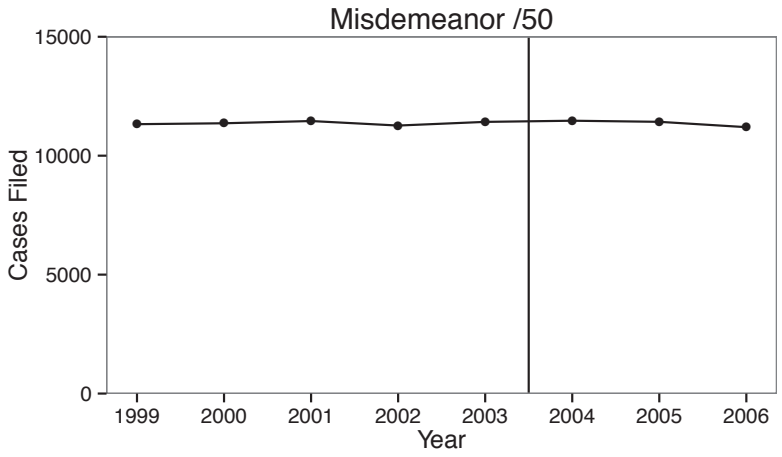


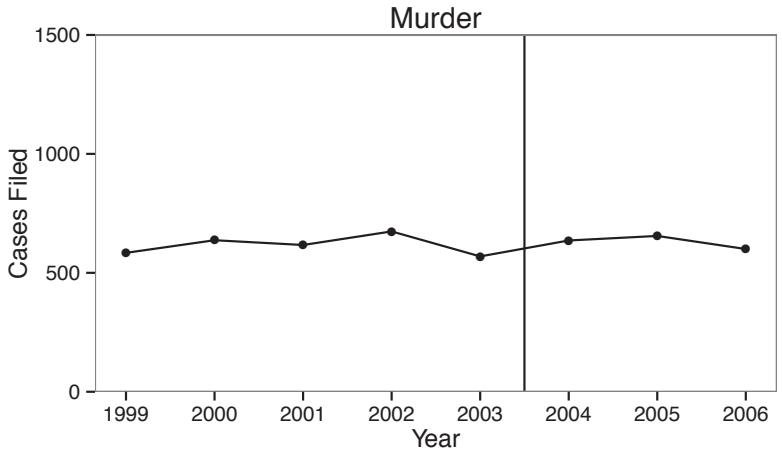


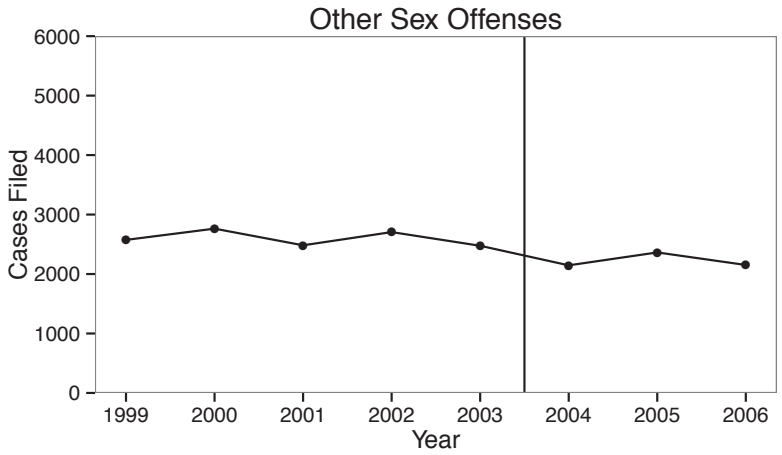


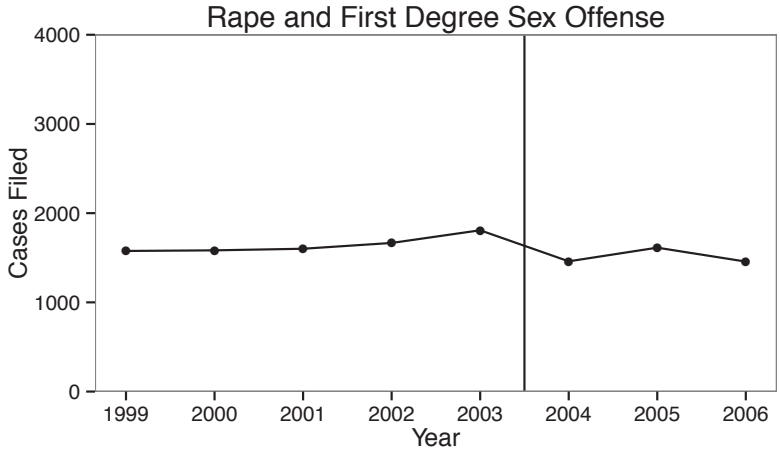












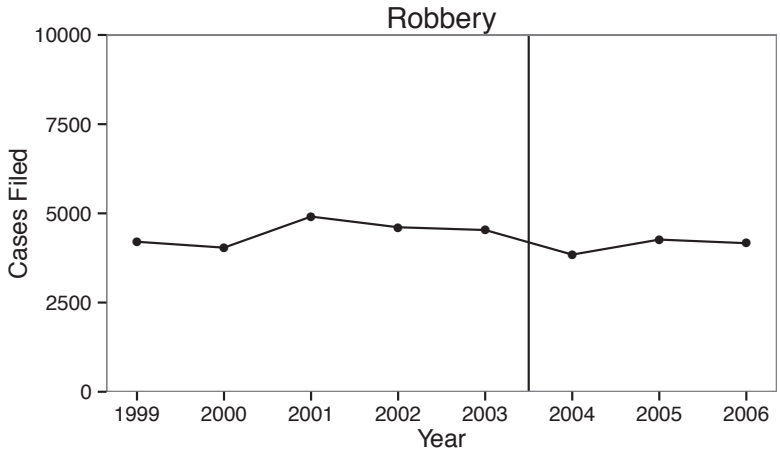


Figure 3.7: Average Case Outcomes for Minor Felonies and Misdemeanors

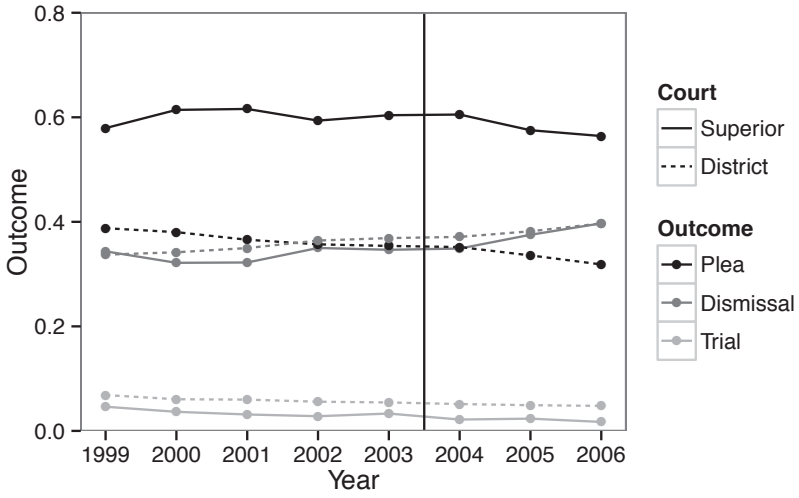


Figure 3.8: Average Case Outcomes for All Felonies and Appealed Misdemeanors

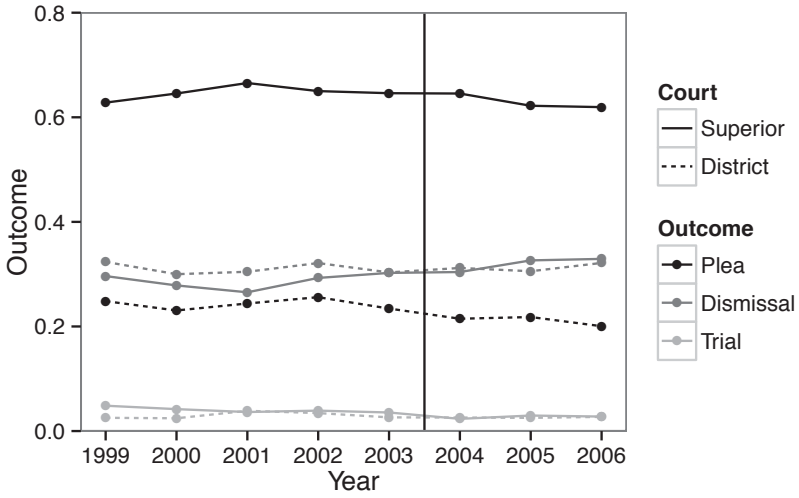
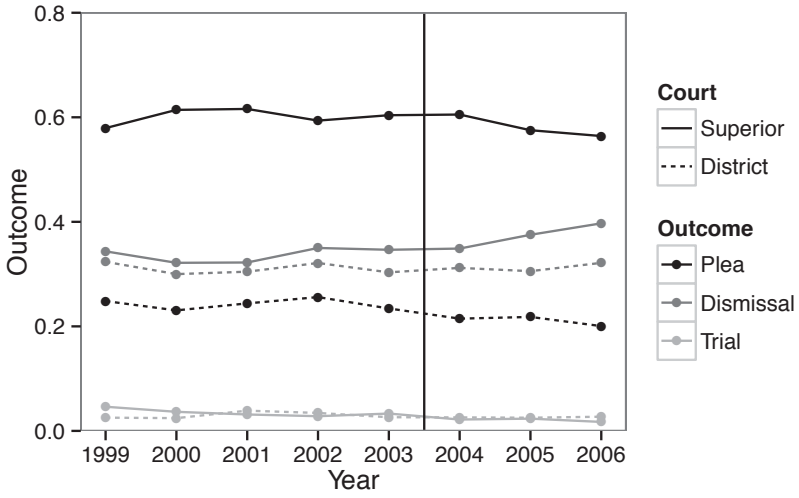


Figure 3.9: Average Case Outcomes for Minor Felonies and Appealed Misdemeanors



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