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A Multilevel Analysis of County and State Variation in the Severity of Sentences Imposed in Large Urban Courts

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A Multilevel Analysis of County and State Variation in the Severity of Sentences
Imposed in Large Urban Courts

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A Dissertation submitted to The Graduate School at the University of Missouri-St. Louis
in partial fulfillment of the requirements for the degree
Doctor of Philosophy in Criminology and Criminal Justice

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ABSTRACT

This study explored the structural sources behind variability in the sentences applied to felons convicted in state courts located across the U.S. Multilevel regression models were used to explore whether various state and county-level attributes help to account for why defendants experience a significantly higher probability of incarceration versus probation in certain jurisdictions.

Drawing upon a broad theoretical landscape, the analyses test several hypotheses derived from macro level theories of social control which predict that that the legal and organizational culture of courts, and the socioeconomic and political attributes of the communities they serve, influence sentencing outcomes. This study sought to fill two important gaps in the existing research. First, it broadened the theoretical framework used to interpret community variation in punishment to include the impacts of state sentencing policies that have been linked to the increase in mass incarceration among U.S. states. The second major goal of this study was to bring new data to bear on the issue of whether social and cultural attributes of communities are associated with the severity of the sentences their courts impose. The analysis examines this issue by linking individual sentencing outcomes to aggregate-level General Social Survey (GSS) responses that capture community variation in public sentiment.

The sentencing data used to test these hypotheses are derived from the State Court Processing Statistics (SCPS) for the years 1998, 2000, 2002, and 2004. Information on a sample of 26,000 felony cases in the SCPS were appended to a unique county and state-level database containing measures that capture variation in sentencing policy, criminal statutes, correctional resources, crime rates, court case load pressures, GSS survey

responses, and census-derived demographic attributes. The findings indicate that the average probability of being sentenced to incarceration varies significantly across court jurisdictions, and that differences in the types of cases courts process do not account for this variation. Consistent with previous studies, the analyses reveal that commonly considered attributes such as county racial composition, levels of crime, and adverse economic conditions, exert weak or null effects on the outcomes of criminal cases. Analyses of the effects of legal policy reveal that defendants processed in jurisdictions with certain punitive sentencing policies do not face significantly higher odds of being incarcerated. This finding contradicts much of the theoretical and policy literature, which highlights the role of more punitive sentencing policies as a key factor responsible for the growth in mass incarceration.

Certain aspects of the organizational context in which courts operate, including the amount of state correctional spending and higher monthly probation supervision fees, are associated with a significantly lower likelihood of going to prison. Models examining the effects of community social climate indicate that defendants convicted in communities with higher levels of anti-Black resentment among whites are significantly more likely to receive a prison versus a jail sentence, and that the odds of being sentenced to prison are significantly lower in jurisdictions where religious fundamentalism is more widespread. A series of supplementary models separating out drug versus non-drug felony cases suggests that the relationship between community context and sentencing outcomes is complex, and that certain aspects of the broader social and legal climate only impact the odds of incarceration for drug offenders. The implications of these findings for advancing contextual explanations of sentencing are discussed.

TABLE OF CONTENTS

ABSTRACT 2

TABLE OF CONTENTS 4

LIST OF TABLES AND FIGURES 5

ACKNOWLEDGEMENTS 6

CHAPTER 1 (INTRODUCTION)..... 8

CHAPTER 2 (PRIOR RESEARCH, THEORY, HYPOTHESS)..... 25

CHAPTER 3 (DATA AND METHODS)..... 116

CHAPTER 4 (FINDINGS) 185

CHAPTER 5 (CONCLUSION) 252

REFERENCES..... 268

APPENDICES..... 287

LIST OF TABLES AND FIGURES

TABLES

TABLE 2.1.....	35
TABLE 2.2.....	113
TABLE 3.1.....	126
TABLE 3.2.....	131
TABLE 3.3.....	150
TABLE 3.4.....	181
TABLE 3.5.....	183
TABLE 4.1.....	188
TABLE 4.2.....	196
TABLE 4.3.....	199
TABLE 4.4.....	210
TABLE 4.5.....	211
TABLE 4.6.....	221
TABLE 4.7.....	231
TABLE 4.8.....	234
TABLE 4.9.....	245
TABLE 4.10.....	248
TABLE 4.11.....	250

FIGURES

FIGURE 3.1.....	122
FIGURE 4.1.....	186
FIGURE 4.2.....	213
FIGURE 4.3.....	214
FIGURE 4.4.....	223
FIGURE 4.5.....	223
FIGURE 4.6.....	224
FIGURE 4.7.....	232
FIGURE 4.8.....	246
FIGURE 4.9.....	247
FIGURE 4.10.....	247

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CHAPTER ONE

INTRODUCTION

This research uses the State Court Processing Statistics (SCPS) to explore the sources behind the significant jurisdictional variation in the likelihood of incarceration across state felony courts. The central premise of this study is that amidst a broader national trend towards more punitive crime control and the expanded use of incarceration, the probability of being sentenced to a period of incarceration varies significantly across court jurisdictions. This study tests propositions derived from classic and contemporary theories of punishment, which argue that jurisdictional variation in the severity of sentencing can be attributed to differences in the legal cultures of states, and the social, political, and organizational climate in which county trial courts operate. I examine these questions using State Court Processing Statistics data on felony cases filed between 1998 and 2004 in county trial court jurisdictions that span 19 states.

Background and Significance

“Justice by Geography”

Sentencing scholars have long acknowledged that judges are not isolated actors, and that environmental conditions impact the priorities of courts and help to establish shared norms about what constitutes appropriate “going rates” of punishment (Eisenstein et al., 1988; Feld, 1991; Gibson, 1980; Kramer, 2009; Myers and Talarcio, 1987; Ulmer, 1997). These observations have important implications for sentencing. Despite a US legal culture emphasizing formal rationality and equal protection under the law, the potential for “justice by geography” undermines the core legal principles of most modern

democratic societies (Feld, 1991; Savelsberg, 1994; Ulmer & Johnson, 2004). Though there are legitimate reasons why jurisdictions may vary in the leniency of sentencing, local variations to state or federal law can signal troubling evidence of local courts' hostility to sentencing policy out of bias or prejudice (Bibas, 2005). Moreover, geographic variations in the sentencing of otherwise similar defendants that is "too great or too blatant comes at the steep price of inequality, unfairness, and reduced deterrence." (Bibas, 2005: 138).

Yet criminal courts in the U.S. are, by design, uniquely susceptible to geographic disparities for a number of reasons. Sentencing is the most visible stage of the criminal justice process in the U.S., and the only point at which citizens may openly observe and participate. Although most citizens have little direct contact with the courts, legal scholars maintain that court processing in the U.S. is unusually sensitive to political pressure since in most jurisdictions judges and prosecutors are locally elected, and because criminal justice authority in the U.S. is concentrated in state and local governments that can (and often do) quickly adjust crime control strategies to fit with changing structural pressures (Zimring & Johnson, 2006: 277; Helms, 2009; Helms and Jacobs, 2002). These features make courts in the U.S. a fertile context in which to study how sentencing decisions may be sensitive to the broader social climate in which courts are embedded.

The Current Context of Criminal Sentencing

Several recent developments in U.S. corrections have brought the social, political, and economic backdrop of sentencing into sharp focus. Two recent Supreme Court

decisions struck down provisions of the federal and state presumptive guideline schemes, leaving these jurisdictions struggling to find ways to maintain judicial discretion while also minimizing the potential for unwarranted sentencing disparities (e.g., Bushway and Piehl, 2007). In addition, spending on corrections is now the fastest growing general fund expenditure for states (Scott-Hayward, 2009). And for the first time in over two decades the annual rate of growth in state correctional populations has slowed, leading to speculation that budget deficits are prompting states to accelerate the rate of prison releases and to rethink the severity of criminal sanctions for nonviolent offenses (Lawrence, 2008; Sabol, West, and Cooper, 2009; Scott-Hayward, 2009; Wilhelm and Turner, 2002). Fiscal constraints have so far motivated at least six state legislatures to expand community corrections in order to control prison costs, and to help stem the flood of state revenue away from other important state functions (Lawrence, 2008; Warren, 2009).

This is a remarkable turnaround after decades of steady growth in state incarceration rates and it illustrates just a few of the ways in which broader structural changes impact the work of courts. However, signs of a slowdown in incarceration come with two caveats. The recent decline in the overall rate of incarceration among states between 2008 and 2009 was driven primarily by decreases in *jail* inmates, not prison inmates (Glaze, 2010). The second caveat is that while the annual rate of growth in state prison populations is leveling off, prison population declines appear to be driven by reductions in parole revocations more so than decreases in the rate of new court commitments to prison (Glaze, 2009). Moreover, while the magnitude of prison growth is slowing, the raw size of prison populations continued to increase between 2008 and

2009, at which point the U.S. reached a critical threshold- now more than 1 in 100 U.S. adults is incarcerated in prison or jail (Glaze, 2010; Warren, 2009).¹ Currently, the rate of incarceration in the U.S. remains the highest in the world at 750 inmates per 100,000 residents, an increase of 76% since 1987 (Langan, 1991; West, 2010). Remarkably, the period of steady growth in prison populations during the 1990s occurred amidst a sustained crime drop, and data show that the growing use of prison reflects changes in *sentencing* behavior more so than changes in reported crime or arrests (Blumstein and Beck, 1999; Langan, 1991; Raphael, 2009).

Though there are signs that local jurisdictions are easing their reliance on incarceration, there is little evidence to suggest that the *propensity* to incarcerate felony offenders has declined significantly. Since the 1990s, a number of studies have examined the factors behind the unprecedented levels of incarceration in the U.S. (Austin and Irwin, 2000; Beckett and Sasson, 2000; Garland, 2001; Langan, 1991; Tonry, 1995, 1996). A persuasive claim emerging from this work is that courts began to increasingly impose incarceration terms not because the composition of individual judges or prosecutors shifted, or because courtroom actors independently adopted a more punitive orientation to punishment (Tonry, 1996). Rather, the theme emerging from recent legal research is one of adaptation. Punishment philosophies began to change in the 1980s, and courts were forced in many jurisdictions to adopt sentencing practices triggered by legislative pressure to apply more “just,” determinate, and uniform sanctions, (Beckett and Sasson, 2000; Feeley & Simon, 1992; Garland, 2001; Lynch, 2005; Tonry, 1996).

¹ In 2009, Pennsylvania and Florida experienced the largest increases in the size of state prison populations (Glaze, 2010).

What are the sources of these pressures to mete out more uniform and punitive sanctions? The theoretical literature points to the increasing saliency of crime in everyday life, that political rhetoric on crime tacitly stoked anti-black racial prejudice, that public sentiment gained greater influence in forming sentencing policy, and that discourse regarding the appropriate response to crime has become more ideologically charged in recent decades (Beckett and Sasson, 2000; Chiricos, Welch, and Gertz, 2004; Garland, 2001; Steiger, 1998; Tonry, 1996). Some explain this newfound reliance on incarceration as the result of the war on drugs, which resulted in significant growth in the proportion of drug arrestees sentenced to imprisonment (Austin and Irwin, 2000; Beckett and Sasson, 2000; Blumstein and Beck, 1999). Others argue that a politicizing of crime paved the way for state lawmakers to experiment with parole abolishment, mandatory minimums, and ‘three-strikes’ laws that favor incapacitation and deterrence over rehabilitation (Garland, 2001; Raphael, 2009; Tonry, 1999; Zimring and Johnson, 2006). This policy explanation for the growing use of incarceration has gained considerable traction, and it is now “conventional wisdom” among criminologists that sentencing reforms are the primary factor responsible for changes in the severity of sentencing (Engen, 2009: 329). For example, in a highly publicized report documenting that 1 in 100 U.S. adults are now behind bars, researchers at the PEW Center concluded: “...lawmakers are learning that current prison growth is not driven primarily by an increase in crime, or a corresponding surge in the population at large. Rather, it flows principally from a wave of policy choices that are sending more lawbreakers to prison and, through popular ‘three-strikes’ measures and other sentencing enhancements, keeping them there longer” (Warren, 2008: 3). The underlying assumption of this

argument is that policies have significantly increased the severity sentencing above and beyond the effects of legally relevant factors such as offense severity and prior criminal history. While this seems reasonable, there is little evidence to support such conclusions (Engen, 2009). In fact, several macro level studies show that the adoption of sentencing guidelines and determinate sentencing either fail to significantly impact incarceration rates at all, or in some cases, are associated with *reductions* in incarceration rates (Engen, 2009; Greenberg and West, 2001; Marvell and Moody, 1996; Stucky, Heimer and Lang, 2005). Similarly, the passage of mandatory minimums in many states in the 1970s did not correspond with a swelling prison population of repeat offenders and those who committed offenses targeted by the mandatory provisions (Langan, 1991). Yet the popular explanations for why certain jurisdictions are more or less punitive favor the explanation that “policy choices drive growth” (Warren, 2008: 3). In sum, though scholars disagree on which factors shape the tendency of courts to mete out more punitive sanctions, the consistent theme of recent scholarship on incarceration and sentencing is that judges enjoy considerably less autonomy than in previous decades, and that the broader social and legal context in which sentencing occurs matters more than ever.

The Variability of Sentencing across State Court Jurisdictions

While reorienting theorizing on punishment back to the importance of social and political context, the tone of these explanations (most notably present in Garland’s ‘Culture of Control,’ 2001) is suggestive of a monolithic “get tough” approach to crime control within the US. This broad description is misleading. Though every state experienced some increase in the prison admissions over the last thirty years, the severity

of sanctions applied to felony defendants, and presumably the rationale for this severity, varies widely within the United States. For instance, during the 1980s the degree of variability in state incarceration rates was greater than the variability in incarceration observed across all of the countries in Western Europe (Zimring and Hawkins, 1991 cited in Frost, 2008: 277). New court-imposed prison sentences in Southern states typically double the number of new prison sentences imposed by trial courts located in the West and Northeast (Sabol, West, and Cooper, 2008). At the local level, data from the State Court Processing Statistics indicate that the proportion of convicted felons sentenced to incarceration in large urban courts rose for the fifth straight year to a new high of 75% in 2002 (Cohen & Reaves, 2006). However, this ranged from as low as 40% of convicted felons in Birmingham, Alabama to as high as 96% in Houston, Texas. These same data show that the overall mean prison sentence length for felony offenses in 2002 was 58 months; but this ranged from a low of 28 months in Fulton County, Georgia to a mean sentence of 253 months in Salt Lake County, Utah.

What explains this variation? A number of studies have examined jurisdictional variation in prosecutorial decision-making (Baumer and Martin, 2011; Cooney and Burt, 2008; Wooldredge and Thistlewaite, 2004), guideline departures (Johnson, 2005; Johnson et al., 2008), the odds of incarceration (Fearn, 2005; King et al., 2010; Myers and Talarico, 1987; Pardoe and Weidner, 2006; Wang and Mears, 2010; Weidner et al., 2004, 2005), and sentence lengths (Baumer and Martin, 2011; Britt, 2000; Cooney and Burt, 2008; Helms and Jacobs, 2002; Huang et al., 1996; Johnson, 2006; Myers and Talarico, 1987; Ulmer and Johnson, 2004). These studies consistently show modest but significant

inter-jurisdictional variation in sentencing severity, even within states that operate under structured sentencing schemes.

These studies have had less success, however, in identifying which community attributes consistently impact sentencing outcomes. Most of this research has focused on investigating the effects of variables emphasized in functionalist, conflict, Durkheimian, and Weberian theories of law and punishment. These theories typically emphasize that crime rates, the size of poor and minority populations, geographic region, political and religious conservatism, and variation in the size and caseload pressures of courts impact sentencing outcomes. Though there are some exceptions, the findings from this research reveal that most contextual variables exert no influence on the outcomes of individual cases. When studies do report significant relationships between county-level attributes and sentencing outcomes, the effects generally tend to be weak and at times inconsistent with theoretical expectations. Consequently, despite a sizeable body of theoretical work suggesting local context should shape the nature of punishment courts mete out, no consistent set of findings has emerged to answer the basic question of why some communities, on average, impose significantly more punitive sanctions in felony cases.

Though many studies report null findings, this area of research has still yielded some important insights that suggest a need for further research. For instance studies of sentencing outcomes in Minnesota, Pennsylvania, and Federal District courts highlight the importance of organizational constraints such as jail crowding, caseload pressure, and jurisdiction size (e.g. Dixon, 1995; Johnson, 2005; Ulmer and Bradley, 2006; Ulmer, Bader, and Gault, 2008). A handful of studies that analyze sentencing decisions from the State Court Processing Statistics (SCPS) also suggest that the racial and ethnic makeup of

communities may help to explain why defendants in some jurisdictions experience significantly higher odds of imprisonment upon conviction (Helms, 2009; King, Johnson, and McGeever, 2010; Pardoe and Weidner, 2006; Wang and Mears, 2010; Weidner, Frase, and Schultz, 2005). Last, recent studies have theoretically expanded their focus and find that more conservative religious climates, the characteristics of judges, and the racial makeup of the local legal workforce help to account for cross-jurisdictional variability in the severity of sentencing (Fearn, 2005; Johnson, 2006; King et al., 2010; Ulmer, Bader, and Gault, 2008).

Limitations of Prior Research

Despite these advances, contextual sentencing research remains limited in some important respects. Most prior research consists of studies conducted in a handful of states, the majority of which operate under presumptive guidelines. We know a good deal now about sentencing in Washington, Pennsylvania, Minnesota, and in the Federal system (Miller, 2005). However, the results from these studies may have limited generalizeability. Most states do not operate under sentencing guidelines, and analyses of sentencing using samples from presumptive guideline states likely provide a conservative test of the extent and sources behind geographic disparities in punishment because guidelines are designed in part to mitigate the potential for such disparities.

Multi-state studies that analyze case outcomes from the State Court Processing Statistics (SCPS) have helped broaden the geographic scope of contextual sentencing research, but suffer from crucial methodological and conceptual problems. First, these studies have employed two-level research designs that pool defendants and counties from

across the U.S. as if they were sentenced within the same state (Fearn, 2005; Helms and Jacobs, 2002; Pardoe and Weidner, 2006; Weidner, Frase, and Pardoe, 2004; Weidner et al., 2005). Yet, the states that participate in the SCPS exhibit diverse legal contexts. Some of the states have no structured sentencing scheme in place to minimize the potential for local variations in sentencing. Others have formal guideline grids that allow (and in some cases require) judges to consider more than the seriousness of the current offense, which may disadvantage defendants with more extensive criminal histories (Engen and Gainey, 2000; Frase, 1997). Likewise, there is considerable variation in the purposes and discretion (voluntary vs. presumptive) afforded by various guideline schemes. This is not to mention the variability across states in the adoption of mandatory sentencing policies and habitual offender laws. Yet the analyses from recent multi-state contextual sentencing studies treat these cases as if they operate under a single legal structure that performs in the same fashion for every defendant in every jurisdiction (Bushway and Piehl, 2007). As a result these studies have likely overstated the degree to which jurisdictional variation is due to local political and social dynamics, and underestimated the extent to which jurisdictional differences stem from state legal rules and guidelines that regulate judicial decision making. If so, these multistate studies may have confounded the estimated effects of conservative political climate, levels of crime, or county racial composition with variations across jurisdictions in the severity of sentencing options state penal codes provide judges. In other words, it may be that community levels of crime and a more politically conservative voter base help to foster punitive *legislation* rather than punitive judges. Thus, it remains unclear whether studies

of jurisdictional variation in punishment severity are modeling judicial or legislative behavior.

Ignoring state sources of variation also neglects one of the more critical policy issues that case-level data are uniquely situated to examine. The emergence of state sentencing reforms is among the most significant legal developments in the U.S. courts during the past century, yet there are few studies that compare the severity of sentencing across these various sentencing systems. For instance, prior studies have not addressed claims that controversial reforms such as mandatory enhancements, punitive drug penalties, and three-strikes laws truly increase the severity of sentencing above and beyond the effects of legally relevant case factors (Engen, 2009). This omission from the multilevel literature is surprising since a number of macro level studies reveal that state spending, revenue, and policy reforms play an important role in explaining growth in incarceration (Raphael, 2009; Spelman, 2009; Stemen, Rengifo, and Wilson, 2005). Though the findings from research on prison admission and incarceration rates are intriguing, incarceration rates confound arrest, conviction, incarceration, sentence length, and parole decisions. Thus, unlike case-level sentencing data, these aggregate level studies cannot isolate the stage at which sentencing reforms impact punishment, nor can they disentangle the effects of policy from those of legally relevant factors such as the severity of the offense. Accordingly, a major goal of this study is to broaden existing contextual research by incorporating measures of various state legal policies and organizational attributes that are predicted to directly impact the way courts sentence.

Another key shortcoming of the research is the lack of data that directly tap public sentiment and cohesion. For instance, classic Neo-Durkheimian perspectives on social

control argue that the severity of punishment in reflects the cultural sensibilities and attitudes of community members. A few of the community norms emphasized by this perspective are religious and political values, social cohesion and trust, and norms and attitudes that shape residents' tolerance for more or less repressive punishment styles. Numerous researchers have voiced a need to incorporate these types of direct measures into studies of social control (Liska, 1993; Ulmer and Johnson, 2004). However, data restrictions have forced researchers to rely primarily on Census and official crime data to capture community social context. This limitation is important for a number of reasons. First, census-derived measures of racial composition, poverty and unemployment, region, age structure, and political representation provide only indirect tests of the theoretical processes believed to shape variation in formal social control. For instance, community racial composition and unemployment have each been the subject of a good deal of research on spatial variation in various social control outcomes (e.g., police force size, sentencing, prison admissions). Most of this research is informed by conflict theory, which interprets the relationship between threatening populations and the severity of social control as evidence of social "threat" (Liska, 1992). An equally plausible possibility though, and one that is supported by survey research, is that larger populations of the poor and non-whites impact social control primarily in an indirect way by shaping levels of fear among local residents (e.g., Liska, Lawrence, and Sanchirico, 1981; Quillian and Pager, 2001; see also Baumer and Martin, 2011). Ulmer and Johnson (2004) recently concluded that for reason like these, future studies should consider more refined and specific survey measures of local public attitudes on a range of criminal justice concerns. Another reason why more direct measures of public sentiment can

advance contextual sentencing research is that a good deal of the theoretical literature highlighting the role of policy in shaping sentencing behavior has been careful to point out that punitive statutes such as three-strikes laws and mandatory minimums are successful only insofar as the public provides political support for them (Garland, 2001; Tonry, 1999). This insight suggests that depending on the local social climate, state legal policies may be met with varying degrees of tolerance in local communities and that local public sentiments play an important role in forming the broader social environment from which prosecutors and judges formulate “going rates” of punishment for most offenses (Kramer, 2009).

The Current Study

This research attempts to overcome some of these limitations by taking a more comprehensive theoretical approach that considers the relevance of a broad array of community and state attributes. This project builds upon recent work by Baumer and Martin (2011) which expanded the consideration of the social and organizational believed to shape punishment, while paying significant attention to the role of legal statutes and policies that shape the boundaries of judicial decision making and may serve to limit the influence of cultural and political features of communities on the sentencing process. The study develops and tests hypotheses derived from the macro structural theories of punishment which highlight the importance of social norms and values, organizational constraints and resources, and neo-Weberian perspectives which view sentencing in urban courts as highly “rationalized” and consistent with the rule of law. While previous studies have examined the effects of a few of these characteristics, no prior research has

systematically tested the propositions from much of the theoretical literature that focuses explicitly on the effects that more punitive legal statutes, financial resources, and public sentiments are believed to have on decisions to incarcerate felons.

The current study examines the effects of social, organizational, and legal climate on the likelihood that convicted felons are sentenced to incarceration versus probation. The data for this study come from the State Court Processing Statistics, which contains information on felony criminal cases processed in 60 large urban courts between 1998 and 2004. The SCPS is a biennial collection of felony cases that are followed from the original filing decision up until the final sentencing disposition. The SCPS includes information on the characteristics of defendants, their prior criminal history, the severity of the offense for which they were arrested and convicted, the mode of adjudication, and information on key decisions prior to sentencing such as bail and charging decisions. These data also contain geographic identifiers that were used to construct a multilevel database describing the social and legal contexts in which these cases were processed.

To test these hypotheses a unique three-level multilevel database was constructed that contains data describing the communities and states in which felony cases were processed. I incorporate more direct indicators of social and cultural context from a version of the General Social Survey (GSS) that contains census FIPS codes identifying the metropolitan areas in which respondents live. Using these codes, responses were aggregated to construct measures that have been identified in survey research as important indicators of more punitive orientations. These include GSS items tapping religious and political conservatism, support for harsh sanctions, levels of fear, social trust, and racial resentment among whites. In addition, another unique aspect of this

study is the inclusion of policy variables that tap jurisdictional variation in sentencing structure and the presence of substantive sentencing policies that aimed at increasing the overall severity of sentencing. These measures are derived from a sentencing policy database originally designed and made available by researchers at the Vera Institute (Stemen et al., 2005). This data base provided information on the presence of sentencing guidelines, determinate sentencing structures, truth in sentencing provisions, the breadth of mandatory minimum enhancements in each state's penal code, whether the jurisdictions in the SCPS have three-strikes policies for habitual offenders, as well as the minimum statutory sentences each state imposes for the possession and sale of cocaine. Finally, this research goes beyond prior studies by examining the effects of a broader array of organizational factors that constrain the sentencing options available to judges, and that may provide incentives for more lenient sanctions. Specifically, data were gathered that capture state variation in correctional spending, the ability of local and state authorities to collect fees that offset the costs of probation supervision, the availability of jail and prison space, and the workload of local prosecutors. Finally, this research replicates previous studies by examining the effects of crime rates, region, racial and ethnic composition, and economic conditions on sentencing.

Using a sample of over 26,000 felony cases that resulted in conviction, the analyses explore the outcomes of criminal cases using a dependent variable for sentence type that consists of prison, jail, and non-custodial sentences (i.e., probation and fines). A series of hierarchical multinomial regression models were estimated to address two general research questions. First, does the likelihood of incarceration vary significantly across county and state jurisdictions net of any differences across courts in the severity of

their caseloads? Second, do aspects of the jurisdictional legal, social, and organizational climate explain this variation? The models examining the effects of legal policies on sentencing address an additional set of research questions. First, what are the effects, if any, of sentencing policy on the severity of sentencing? Second, do sentencing policies such as presumptive guidelines and three-strikes laws that give special weight to prior offending *indirectly* increase the severity of sentencing by strengthening the weight courts place on having a prior felony record? Third, given the emphasis in the sentencing literature on the increase in the severity of sanctions for drug offenders over the past two decades, this study also conducts a separate analysis of variation in sentences meted out to drug offenders. Specifically, this study examines whether sentencing policies impact drug offenders in unique ways, and second, whether variation in state drug laws explain the higher probability of incarceration observed for drug offenders in certain jurisdictions. Prior research examining the social contexts of incarceration decisions has not explicitly considered the possibility that legal and social contexts may impact the sentencing of drug offenders in unique ways. This distinction may be important as certain social contexts may only increase punitive responses toward serious violent offenders, while prompting more therapeutic forms of sanctioning for drug offenders (e.g. Simmons, 1999).

Models assessing organizational perspectives address whether sentences are on average less punitive in jurisdictions experiencing prison and jail overcrowding, where there are higher correctional expenditures, greater caseload pressure on prosecutors, and greater fiscal offsets that promote the use of probation in lieu of jail and prison.

A series of models designed to test social conflict explanations of social control assess whether incarceration terms are more likely in jurisdictions that contain larger populations of non-whites and the poor, higher levels of economic inequality and unemployment. The analyses then explore whether levels of fear within the community mediate the relationships between social threat and sentence type.

Finally, data from the GSS are used to explore whether defendants face a significantly higher probability of being sentenced to incarceration in jurisdictions where residents are more socially conservative, where support for harsh sanctions is more widespread, and where mutual trust and interdependence are weaker.

The study is organized in the following manner. Chapter 2 briefly summarizes the state of the literature devoted to jurisdictional variations in sentencing severity and summarizes the hypotheses examined in the current research. Chapter 3 describes the methods and data used for assessing the effects of jurisdictional legal, organizational, and social climate on the outcomes of criminal cases. Chapter 4 presents the findings from a series of hierarchical multinomial regression models that test the hypothesized effects of various aspects of legal, organizational and social climate emphasized in the theoretical literature on sentencing and social control. Chapter 5 concludes the study by discussing the implications and limitations of the research.

CHAPTER TWO

PRIOR RESEARCH, THEORY, AND HYPOTHESES

This research examines whether, net of jurisdictional differences in caseloads, state courts' social, political, legal and organizational climate significantly impact the odds that a defendant will be confined to prison or jail versus probation or fines. In this chapter I review previous studies that have examined the effects of courtroom social climate on incarceration and guideline departure decisions. The latter half of this chapter describes the theories that have been used to interpret community variation in the severity of punishment. There are five distinct explanations that inform the literature: structural functionalist models, legal rationality models emphasizing procedural rules and a punitive legal culture, organizational efficiency models, conflict models which link harsh sanctions to the presence of "problem populations," and neo-Durkheimian models which emphasize community cohesion and the moralistic aspects of punishment. I conclude the chapter by summarizing the specific hypotheses that are tested in the current study.

BACKGROUND

Micro and Macro Approaches to Studying Punishment

Traditionally, sentencing research has been conducted at the individual level, with an emphasis on estimating the degree of variation in sentencing that can be attributed to defendant social status characteristics such as age, race, and gender (Bushway and Piehl, 2007; Sampson and Laub, 1993). The findings from this extensive body of research confirms that legal characteristics associated with the severity of the offense and prior

criminal history are the most powerful and consistent predictors of sentencing outcomes (see Engen and Gainey, 2000; Ulmer, 2000; Spohn, 2000 for reviews). This research also suggests that when racial and sex –based disparities arise, they are most pronounced in the decision of whether to incarcerate more so than the length of imprisonment (Hagan and Bumiller, 1983; Spohn, 2000; Spohn and Cederblom, 1991; Welch, Spohn, and Gruhl, 1985; Steffensmeier, Ulmer, and Kramer, 1998; Ulmer, 2000). Yet, the growing consensus among sentencing scholars is that the symbolic importance attached to defendant attributes as well as the ways in which the courts respond to felony crime in general are geographically contingent (Myers and Talarico, 1987; Peterson and Hagan, 1984; Spohn, 2000).

In this regard, macro level research on incarceration rates using states and counties as units of analysis have filled a void in our understanding of how and why the severity of sanctioning varies across U.S. jurisdictions. These studies demonstrate that the frequency with which states incarcerate and invoke the death penalty are determined in part by crime rates, the presence of populations perceived as threatening, residents' tolerance of certain types of crime, and the capacity to mobilize resources to respond to it (e.g., Crawford, Chiricos, and Kleck, 1998). Until recently these two streams of research (individual and macro level studies) remained relatively separate from each other. This resulted in an expansive but fragmented body of work that separated the micro level correlates of sentencing from the broader trends in the size and growth of prison populations across the states. Both micro and macro approaches to understanding the use of imprisonment provide critical insights on the nature of punishment in the U.S., but both are limited in important respects.

Individual-level sentencing studies leave out of account the widespread patterns of punishment in the U.S. that are suggestive of important state and county differences in their approach to crime control. For instance, the goals of sentencing are not uniform across jurisdictions (Eisenstein and Jacob, 1977; Ulmer, 1997). The purposes of sentencing may be incapacitation in some communities, retribution in others, while a greater emphasis may be placed on the rehabilitative ideal in other court contexts. Consider for example that the adoption of determinate sentencing schemes in California, Ohio, Kansas and Arizona coincided with legislative action to alter their respective penal codes to proclaim that “punishment” would become the primary purpose of sentencing.² This stands in contrast with the move by Wisconsin, Illinois, and New York to legislate that sentencing be carried out for the purposes of community safety (i.e., deterrence) and rehabilitation of the offender, rather than retribution. Claiming a retributive punishment philosophy may merely represent symbolic gestures on the part of politicians, but they underscore the need for a comparative assessment of individual felony case processing across multiple states and their local courts.

State level studies that draw from conflict, economic, and political models of punishment to explain prison populations and their growth provide an important point of departure from the individual level sentencing literature by recognizing that structural conditions other than crime provoke prison use, but these studies also suffer from some crucial limitations. First, although states dictate the legal parameters of sentencing, criminal court processing and sentencing is organized at the county level (Bridges,

² This information was gleaned from a variable contained in the Vera Institute Sentencing Policy Database collected by Stemen, Rengifo, and Wilson (2005). The variable provides a description of the purposes of punishment according to state penal codes. This stated purpose is usually provided at the very beginning of each state’s penal code.

Crutchfield, and Simpson, 1987; Eisenstein, Flemming, and Nardulli, 1988). Scholars familiar with legal decision making in guideline states seem to agree that while guidelines and other state-imposed laws provide meaningful boundaries that judges generally follow, they are nonetheless mediated by informal local community processes and organizational constraints that govern informal norms about appropriate penal sanctions (Engen and Steen, 2000; Gainey, Steen, and Engen, 2005; Johnson, 2003, 2005; Ulmer and Kramer, 1996; Ulmer, 1997). Second, aggregate level research using states and counties as units of analysis measure legal sanctioning using incarceration and prison commitment rates, but in doing so, may confound jurisdictional differences in punishment *severity* with differences in the *regularity* with which states and counties punish. For instance, a growing number of studies reveal that the certainty of prosecution and conviction is contingent upon the county and states in which defendants are arrested (Baumer and Martin, 2011; Cooney and Burt, 2008; Eisenstein and Jacob, 1977; Wooldredge and Thistlewaite, 2002). These studies provide important clues to uncovering the sources of geographic differences in the severity of felony sentencing; namely, that some jurisdictions and states will have a higher volume of incarceration (i.e., they appear to be more punitive) because they prosecute, deny bail, and convict more suspects that come to the attention of police and prosecutors. Using the same logic, studies of incarceration conducted exclusively at the state and county level may confound the affects of structural conditions such as the economy and political conservatism with differences across courts in the types of cases and defendants processed, as well as parole board decision making.

Contextual Studies of Criminal Case Dispositions

Recognition that legal processes vary across jurisdictions spurred several groundbreaking studies aimed at uncovering the organizational and social sources of variability in the application of law. In particular, case studies of nine court jurisdictions located in Illinois, Michigan, and Pennsylvania by Eisenstein and colleagues (1988) provided an important step towards merging these previously separate frameworks for understanding punishment. One of the key insights to emerge from this research is that the “going rates” of punishment differed across court jurisdictions as a function of three things: differences in the severity of state legal codes and varying prison capacities, differences in how courts informally negotiate pleas and charging, and the county’s political culture. Reiterating the importance of legal context, the authors concluded that minimum and maximum penalties dictated in state penal codes not only explains why punishment is more severe in certain jurisdictions (in this instance, Michigan), but that more severe penalties generally signal that “state political culture supports strict punishment, encouraging higher incarceration rates and longer terms,” and that higher prison capacities “provide the opportunity to translate that desire to punish into reality.” (Eisenstein et al., 1988: 225). However, this research also uncovered that legislative sentencing reforms “from above” had widely varying results across jurisdictions because local norms about punishment and organizational priorities differed greatly from county to county. On the latter point, the authors explain that more “moralistic” political cultures view government as a means of making judgments and improving society, and is defined to a good degree by the ideological and religious leanings of local residents. These conclusions point to the importance that the cultural sensibilities of communities, state

legal context, and the organizational barriers or opportunities to realize any influences that social and legal culture provide are key to explaining why punishment is on average more severe in some jurisdictions. This study encouraged a new theoretical interest in understanding the nuanced ways that local courts differ in their goals, strategies, political climate, and ideological values in ways that might contradict the goals of sentencing set forth by state legislatures (Eisenstein et al., 1988; Engen and Steen, 2000; Ulmer, 1997).

Other advancements include work by Spohn and colleagues on rape case processing in the 1980s, which highlighted the various success and failures among six counties as they adapted to state rape law reforms (Spohn and Horney, 1992). They found that prosecutors in certain jurisdictions were significantly more committed to pursuing convictions and stiff penalties in sexual assault cases, while statutory changes to rape law in other jurisdictions produced little change on the part of prosecutors (Spohn and Horney, 1992; Welch, Spohn, and Gruhl, 1985). Another significant study to lay the foundation for contemporary contextual research was conducted by Myers and Talarico (1987). The authors examined sentencing decisions handed down in Georgia courts during the 1970s and found that that the likelihood of incarceration is greater in urban courts and where there is a greater presence of blacks and the unemployed. More importantly, Myers and Talarico began the first systematic effort to replicate research emerging at the time that suggested that judicial decisions are sensitive to public opinion (Cook, 1977; Kuklinski and Stanga, 1979). In their conversations with mayors, judges and law enforcement, the authors noted a keen awareness among court personnel that judges face pressure by a public that perceives sentences as too lenient (Myers and Talarico, 1987: 31-32). In sum, each of the studies just discussed were among the first to

establish that sentencing decisions reflect a delicate balance between the role expectations of judges as fair and consistent arbiters and the pressures placed on them by public perceptions and organizational constraints.

Recognizing the merits of both approaches, contemporary multilevel sentencing literature merges considerations of individual, organizational, and community level factors in the study of criminal case disposition. In doing so, the empirical evidence from multilevel studies have helped clarify three points of contention in the study of sentencing: 1) they demonstrate that state and county variability in the rate at which they imprison offenders is tied primarily to the characteristics of the case and the way courts process them; 2) That despite the clear import of individual-level criteria for predicting sentencing outcomes, a significant amount of variability in sentencing severity remains unexplained by case and defendant characteristics, even within states with presumptive guideline structures in place; and 3) that a modest but significant portion of this variability can be explained by several aspects of the broader social and organizational context, including racial and ethnic composition, court case load pressure, religious conservatism, and levels of crime . In sum, individual studies of the legal process underscore that community differences in the severity of punishment reflects differences in the nature and severity of crime, but that this variability is also symbolic of local norms and interests; the very factors emphasized in classic macro level theories of social control and implied in the decision making processes described in current theories of sentencing (e.g., focal concerns and causal attribution theories).

The findings from contemporary contextual studies (most of which use more sophisticated hierarchical modeling procedures) are summarized in Table 2.1. This table

has several purposes. The body of research examining the social contexts of incarceration decisions is complex, owing largely to the diversity of the samples used and the multitude of ways in which researchers have operationalized various theoretical concepts. For example, a glimpse at the column headings of Table 2.1 demonstrates that there is little consensus on how to best measure levels of crime and adverse economic conditions (e.g., unemployment, economic inequality, poverty, overall disadvantage). Thus, this table is designed to succinctly summarize both the geographic and theoretical scope of prior research, while providing a snapshot of the empirical support for each of these perspectives in a way that gives attention to potentially important measurement differences across these studies.

Looking at the far-left column of the Table, the studies are grouped according to whether they analyze a sample of cases within a single system (i.e., one state or the Federal system) versus a sample of cases across courts located in multiple state jurisdictions. The first group of studies listed includes those that analyze sentencing within Georgia, Pennsylvania, Ohio, and the Federal system. These studies represent the bulk of contextual sentencing research. Underneath this grouping, studies that examine jurisdictional variability in sentencing using samples of cases pooled from across multiple states are listed. All but one of these multistate studies analyzes data from the State Court Processing Statistics (Helms, 2009 is an exception). Within each of these groupings, the studies are listed down the left-hand column in order of the year they were published.

The first theoretical “block” of predictors shown along the top left-hand side of the chart provides a summary of the empirical findings on whether levels of crime

explain geographic disparities in the probability of incarceration and/or guideline departures. As described in more detail in the coming sections of this chapter, these measures tap instrumentalist or functionalist theories of punishment, which argue that the severity of punishment will correspond closely with levels of crime in the community.

The second grouping of variables display results pertaining to the effects of punitive social climate. These indicators, which include conservatism and religious fundamentalism, are derived from classic Durkheimian arguments that view punishment styles as a reflection of cultural values and public sentiments rather than merely an instrumental response to crime (Garland, 2009). Traditionally, researchers have attempted to capture geographic variation in punitive attitudes using a dummy indicator of southern region as a proxy for communities with more collective support for retributive punishment styles. While this is a popular control variable in macro level studies of incarceration, these findings summarized here indicate that southern courts do not mete out significantly more punitive sentences compared to courts in other regions of the U.S. As the table shows, several recent studies have expanded beyond this focus to examine the influences of political and religious conservatism (Fearn, 2005; Johnson, 2006; Johnson, Ulmer, and Kramer, 2008; Ulmer et al., 2008). Though only two studies have examined the effects of religious fundamentalism, the finding that incarceration is more likely in communities containing with a greater concentration of adherents to evangelical and/or fundamentalist Christian faiths is consistent with a good number of survey-based studies that report significant associations between fundamentalist beliefs and more punitive attitudes. Borrowing on insights from the macro literature on social control, a greater number of contextual sentencing studies have examined whether

incarceration is more likely in conservative political climates (Fearn, 2005; Helms, 2009; Johnson et al., 2008; King et al., 2010; Weidner et al., 2005). Though there is some evidence to suggest that Federal sentencing is more lenient in liberal states (Johnson et al., 2008), and that incarceration is more likely in jurisdictions with a larger number of Republican voters, political explanations of sentencing do not receive much empirical support. Nevertheless, political explanations of punishment remain a popular focus of contextual research on sentencing and a handful of studies that examine only sentence length decisions suggest that conservatism may be more relevant for explaining variation in the average duration of sentences more so than incarceration decisions (Helms and Jacobs, 2002; Huang, Finn, Ruback, and Friedmann, 1996).

TABLE 2.1. SUMMARY OF COUNTY AND STATE FACTORS INFLUENCING INDIVIDUAL ODDS OF INCARCERATION AND UPWARD DEPARTURES

	Data and Sample	<i>Functionalism: Level of Community Deviance</i>					<i>Punitive Social Climate/Values</i>						
		Index Crime or Arrest Rate	Violent Crime Rate	Size Violent Caseload	Size Property Caseload	Size Drug Caseload	State Liberalism (Voting Record of South Senators in Congress)	% Chris. Mainline Prot.	% of Christians Catholic	% Evangelical/ Fundamentalist/ Conservative Christian	Index of Christian Diversity ^b	Percent Adhere to Civically-Engaged Denominations	Percent Votes Republican/% Voters Republican
Single-State/Federal Studies													
Myers, 1987 (DV: Prison vs. Other)	GA, 1976-1982 158 GA counties	(+)*											
Myers & Talarico, 1987 (DV: Incarceration vs. Other)	GA, 1976-1982 158 GA counties	(-)											
Britt, 2000 (DV: Incarceration vs. Other)	PCS, 1991-1994 67 PA counties	(+)	(+)										
Ulmer & Johnson, 2004 (DV: Incarceration vs. Other)	PCS, 1997-1999 67 PA counties	(+)		(+)	(+)	(-)							(-)
Johnson, 2005 (DV: Odds of Downward Departure) (DV: Odds of Upward Departure)	PCS, 1999-2000 60 PA counties												
Johnson, 2006 ^a (DV: Incarceration vs. Other)	PCS, 1999-2000 60 PA counties												(+)
Wooldredge & Thistlewaite, 2004 (DV: Jail vs. Other)	Misd. Dom. Viol. Defend. 204 Cincinnati Tracts												
Ulmer and Bradley, 2006 (DV: Incarceration vs. Other)	PCS- Violent Cases 60 PA counties		(+)										
Wooldredge, 2007 (DV: Prison vs. Other Sanction)	Felons in 24 Ohio counties/1,021 Tracts												
Ulmer, Bader, & Gault, 2008 (DV: Incarceration vs. Other)	PCS, 1997-2000 60 PA counties							(-)	(-)	(+)	(+)*	(+)	(-)
Johnson, Ulmer, and Kramer, 2008 (DV: Downward vs. No Departure) (DV: Sub Asst. vs. No Departure)	USSC, 1997-2000 89 Federal Districts	(+)									(+)*		
Farrell, Ward, and Rousseau, 2009 (DV: Prison vs. Other Sanction)	USSC, 2000-2002 89 Federal Districts	(+)	(+)*					(+)					
Multi-State Studies													
Weidner, Frase, & Pardoe, 2004 (DV: Prison vs. Other)	1996 SCPS 39 urban US counties	(+)						(+)					
Fearn, 2005 ^c (DV: Prison vs. Jail)	1998 SCPS 39 urban US counties		(+)*					(-)*		(+)*			(+)
Weidner, Frase & Schultz, 2005 (DV: Prison vs. Other Sanction)	1998 SCPS 39 urban US counties	(-)*											(+)
Pardoe & Weidner, 2006 (DV: Prison vs. Other Sanction)	1998 SCPS 39 urban US counties	(+)						(-)					(+)
Helms, 2009 (DV: Ordinal scale from suspended sentences (1) to prison (5)).	1990 OBTS 387 counties; 7 states		(+)*										(+)*
Wang and Mears, 2010 (DV: Prison vs. Probation) (DV: Jail vs. Probation)	1998-2002 SCPS 60 urban US counties	(-)						(+)					
King, Johnson, & McGeever, 2010 (DV: Incarceration vs. Other)	1990-2002 SCPS 272 urban U.S. county-years	(-)						(-)					(-)

Notes- *p<.05

TABLE 2.1. SUMMARY OF COUNTY AND STATE FACTORS INFLUENCING INDIVIDUAL ODDS OF INCARCERATION AND UPWARD DEPARTURES														
		Social Conflict: Economic Conditions/Threat							Social Conflict: Ethnic and Racial Composition/Threat					
		% Unempl./ Unemploy. Growth	Mean HH or Per Capita Income	Poverty Rate	Index of Economic Disadvantage	Economic Inequality/ Gini Index	% HH Headed by Single Parents	Defendant N'hood SES	White-Black Economic Inequality	Percent Black	Percent Black ²	Percent Hispanic	Percent Hispanic ²	Racial/Ethnic Diversity
(continued)														
Single-State/Federal Studies	Data and Sample													
Myers, 1987 (DV: Prison vs. Other)	GA, 1976-1982 158 GA counties					(+)				(-)	(+)*	(-)		
Myers & Talarico, 1987 (DV: Incarceration vs. Other)	GA, 1976-1982 158 GA counties	(+)*				(-)				(+)*	(+)*	(-)*		
Britt, 2000 (DV: Incarceration vs. Other)	PCS, 1991-1994 67 PA counties	(+) / (+)	(-)							(-)*	(+)*			(+)
Ulmer & Johnson, 2004 (DV: Incarceration vs. Other)	PCS, 1997-1999 67 PA counties				(+)					(-)		(+)		
Johnson, 2005 (DV: Odds of Downward Departure) (DV: Odds of Upward Departure)	PCS, 1999-2000 60 PA counties	(-) (+)										(-)* (+)*		
Johnson, 2006 ^a (DV: Incarceration vs. Other)	PCS, 1999-2000 60 PA counties	(+)										(+)		
Wooldredge & Thistlewaite, 2004 (DV: Jail vs. Other)	Misd. Dom. Viol. Defend. 204 Cincinnati Tracts								(-)*					
Ulmer and Bradley, 2006 (DV: Incarceration vs. Other)	PCS- Violent Cases 60 PA counties											(+)		
Wooldredge, 2007 (DV: Prison vs. Other Sanction)	Felons in 24 Ohio counties/1,021 Tracts								(+)*					
Ulmer, Bader, & Gault, 2008 (DV: Incarceration vs. Other)	PCS, 1997-2000 60 PA counties											(-)		
Johnson, Ulmer, and Kramer, 2008 (DV: Downward vs. No Departure) (DV: Sub Asst. vs. No Departure)	USSC, 1997-2000 89 Federal Districts					(-)* (-)						(-) (+)*		
Farrell, Ward, and Rousseau, 2009 (DV: Prison vs. Other Sanction)	USSC, 2000-2002 89 Federal Districts	(-)												
Multi-State Studies														
Weidner, Frase, & Pardoe, 2004 (DV: Prison vs. Other)	1996 SCPS 39 urban US counties	(-)										(+)		
Fearn, 2005 ^c (DV: Prison vs. Jail)	1998 SCPS 39 urban US counties					(+)*						(-)	(-)	
Weidner, Frase & Schultz, 2005 (DV: Prison vs. Other Sanction)	1998 SCPS 39 urban US counties				(-)							(+)*		
Pardoe & Weidner, 2006 (DV: Prison vs. Other Sanction)	1998 SCPS 39 urban US counties	(-)*										(+)*		
Helms, 2009 (DV: Ordinal scale from suspended sentences (1) to prison (5)).	1990 OBTS 387 counties; 7 states	(+)*	(-)					(+)				(+)*	(-)*	
Wang and Mears, 2010 (DV: Prison vs. Probation) (DV: Jail vs. Probation)	1998-2002 SCPS 60 urban US counties				(+) (-)							(-) (-)*	(+)* (+)*	(+) (-)*
King, Johnson, & McGeever, 2010 (DV: Incarceration vs. Other)	1990-2002 SCPS 272 urban U.S. county-years	(-)				(-)*						(+)*	(+)*	

Notes- *p<.05

TABLE 2.1. SUMMARY OF COUNTY AND STATE FACTORS INFLUENCING INDIVIDUAL ODDS OF INCARCERATION AND UPWARD DEPARTURES

		Organizational Context: Bureaucratic Constraints and Resources								Legal Context			Population Structure			
		Available Jail Space	Prison Capacity Constraints	Caseload Pressure/ # Filings	Court Specializes in Felonies	Large Jurisd.	Small Jurisd.	Urban Jurisd.	Trial Caseload	Guidelines Present ^d	Guideline Departure Rate	Alternative Sanctions Available	Pop. Density	Sex Ratio (F:M)	% Pop 65 and Older	% Pop Young (18-24)
(continued)																
Single-State/Federal Studies	Data and Sample															
Myers, 1987 (DV: Prison vs. Other)	GA, 1976-1982 158 GA counties															
Myers & Talarico, 1987 (DV: Incarceration vs. Other)	GA, 1976-1982 158 GA counties			(+)	(+)											
Britt, 2000 (DV: Incarceration vs. Other)	PCS, 1991-1994 67 PA counties															
Ulmer & Johnson, 2004 (DV: Incarceration vs. Other)	PCS, 1997-1999 67 PA counties	(+)*		(-)*		(-)*	(+)			(-)						
Johnson, 2005 (DV: Odds of Downward Departure) (DV: Odds of Upward Departure)	PCS, 1999-2000 60 PA counties	(-) (+)		(+)* (-)		(+)* (-)*	(-)* (+)			(-) (+)*		(+)* (+)*				(-) (+)
Johnson, 2006 ^a (DV: Incarceration vs. Other)	PCS, 1999-2000 60 PA counties	(+)*				(-)	(+)*			(-)		(-)*				
Wooldredge & Thistlewaite, 2004 (DV: Jail vs. Other)	Misd. Dom. Viol Defend. 204 Cincinnati Tracts															
Ulmer and Bradley, 2006 (DV: Incarceration vs. Other)	PCS- Violent Cases 60 PA counties			(+)*		(+)	(+)									
Wooldredge, 2007 (DV: Prison vs. Other Sanction)	Felons in 24 Ohio counties/1,021 Tracts															
Ulmer, Bader, & Gault, 2008 (DV: Incarceration vs. Other)	PCS, 1997-2000 60 PA counties			(+)*		(+)*										
Johnson, Ulmer, and Kramer, 2008 (DV: Downward vs. No Departure) (DV: Sub Asst. vs. No Departure)	USSC, 1997-2000 89 Federal Districts			(+)* (+)*		(+) (+)				(-) (-)						
Farrell, Ward, and Rousseau, 2009 (DV: Prison vs. Other Sanction)	USSC, 2000-2002 89 Federal Districts			(+)*								(-)*				
Multi-State Studies																
Weidner, Frase, & Pardoe, 2004 (DV: Prison vs. Other)	1996 SCPS 39 urban US counties															
Fearn, 2005 ^c (DV: Prison vs. Jail)	1998 SCPS 39 urban US counties										(+)			(-)	(-)	(+)
Weidner, Frase & Schultz, 2005 (DV: Prison vs. Other Sanction)	1998 SCPS 39 urban US counties										(-)*		(-)			
Pardoe & Weidner, 2006 (DV: Prison vs. Other Sanction)	1998 SCPS 39 urban US counties										(+)*					
Helms, 2009 (DV: Ordinal scale from suspended sentences (1) to prison (5)).	1990 OBTS 387 counties; 7 states															
Wang and Mears, 2010 (DV: Prison vs. Probation) (DV: Jail vs. Probation)	1998-2002 SCPS 60 urban US counties			(+) (+)								(-)* (-)				
King, Johnson, & McGeever, 2010 (DV: Incarceration vs. Other)	1990-2002 SCPS 272 urban U.S. county-years															

Notes- *p<.05

The table then presents the findings of studies that have tested hypotheses derived from the social conflict perspective. The conflict perspective, which argues that incarceration is more likely in communities where social conditions threaten the interests of elites and the middle class, has become a mainstay of the social control literature. Every study included in Table 2.1 tests the hypothesis that larger populations of the poor, non-whites, and higher levels of economic inequality are associated with more repressive punishment. Researchers have measured community social conflict a variety of ways, but on the whole, the findings summarized in this table confirm that there is little empirical evidence to suggest that communities mete out more punitive sanctions as a response to threats posed by the poor. However, the findings summarized in Table 2.1 suggest that racial and ethnic composition tend to exert more consistent and significant influences on the severity of sentencing. Specifically, a number of studies analyzing cases from the SCPS, Pennsylvania, and Georgia find that the odds of incarceration are significantly higher in jurisdictions where the relative size of the black and/or Hispanic population is higher (Britt, 2000; Johnson, 2005; King et al., 2010; Myers and Talarico, 1987; although see Johnson et al., 2008 and Wang and Mears, 2010). As a whole though, the findings from research testing threat-based hypotheses do not provide particularly strong or consistent support for conflict models of sentencing.

The remaining columns of the table display the findings from research that has examined the organizational and legal contexts that may promote variability in the use of incarceration across jurisdictions. Early research on courtroom functioning by Eisenstein and colleagues in the 1970s and 1980s identified several key aspects of the organizational work environment that might promote more or less punitive sentencing outcomes. These

include the size of the caseloads courts must process, the availability of adequate financial and physical resources to do so, and that the size of courts and the communities they serve help determine the ways courts “do business” and whether courts can be expected to rely on more harsh sanctions (Eisenstein and Fleming, 1977; Eisenstein et al., 1988; Feld, 1991; Ulmer, 1997). As Table 2.1 illustrates, multi-state contextual studies have generally neglected to consider the role that organizational context exerts on sentencing. Thus, the bulk of what we know about how caseload pressures, jurisdiction size, and jail and prison crowding influence incarceration decisions comes from research conducted in Pennsylvania (Johnson, 2005; Johnson, 2006; Ulmer and Bradley, 2006; Ulmer and Johnson, 2004; Ulmer, 1997). With respect to caseload pressure, prior studies have yielded inconsistent findings. The results from studies in Pennsylvania and the Federal court system suggest that higher caseloads tend to translate into a lower likelihood of being incarcerated and downward departures (Johnson, 2005; Johnson et al., 2008; Ulmer and Johnson, 2004). Yet several other studies listed in Table 2.1 report that higher caseloads increase the odds of incarceration (Farrell, Ward, and Rousseau, 2009; Ulmer and Bradley, 2006; Ulmer et al., 2008), suggesting that the effect of caseload pressure is likely sensitive to the choice of court or county-level variables that are included in model.

Finally, a handful of the contextual studies shown in Table 2.1 have examined aspects of legal context that include the presence of guidelines, the rate at which judges depart from guidelines, and whether states provide judges with alternative sanctions. As mentioned in Chapter 1, previous studies have afforded little attention to the possibility that county-level variation in the probability of incarceration reflects state-level

differences legal context. Instead, prior multilevel studies examining cases across multiple state jurisdictions have simply control for the presence of presumptive or voluntary guidelines. However, the theoretical rationale for why this variable has been included in models predicting incarceration is unclear (i.e., researchers usually do not offer a prediction about how or why the presence of guidelines is expected to impact the severity of sentencing above and beyond the effects of legally relevant case controls for offense and criminal history that guidelines direct judges to base sentencing on).

Typically, prior multistate studies analyzing the outcomes of SCPS felony cases include this as a control for “state differences in sentencing structure” (e.g., see Fearn, 2005; Wang and Mears, 2010). The findings from studies that include a control for whether sentencing occurs within a guideline jurisdiction indicate null or inconsistent effects of guidelines on incarceration outcomes. Similarly, in their analyses of outcomes in a sample of SCPS cases Weidner and colleagues report that the availability of alternative sanctions exert no significant impact on the likelihood of incarceration (Weidner et al., 2005). The six studies summarized in Table 2.1 that use data from the SCPS do not provide estimates of the degree to which residual variation in the probability of incarceration is due to county versus state-level sources of variation. Though doubtful, it may be the case that states that participate in the SCPS exhibit very little variation in the severity of their statutory penalties, which might explain why the presence of alternative sanctions exerts little influence on sentencing. Based on studies documenting the variability of state sentencing policies and statutory penalties for felonies, this is doubtful (Stemen et al., 2005), and there is a good possibility that these studies not only confound county-level and state-level sources of variability in the severity of sentencing, but also

that the absence of controls for state sentencing policy leaves open the possibility that the findings reported in these studies are biased.

Summary of Prior Research Examining Jurisdictional Variation in Incarceration

The findings from previous studies examining the community variation in incarceration decisions are somewhat difficult to summarize because these studies rely on diverse samples and examine the effects of a wide range of contextual predictors. On the whole, this research shows that individual level factors are the most powerful indicators of sentencing dispositions, but that even after adjusting for differences in the composition of cases jurisdictions exhibit significant variability in the tendency to grant departures and/or to incarcerate offenders. The multilevel sentencing literature has been predominantly concerned with how crime rates and the size of black, Hispanic, and poor populations may trigger more punitive formal control, as whether more conservative communities may be more supportive of law and order crime control campaigns traditionally used by Republicans (Fearn, 2005; Helms & Jacobs, 2002; Ulmer and Johnson, 2004; Johnson, 2006). The community conditions that appear to increase a defendant's odds of receiving incarceration on a fairly consistent basis are county racial and ethnic composition (though it is not clear why) and levels of violent crime. With the exception of research in Pennsylvania, organizational and legal contexts have garnered significantly less attention in contextual studies of sentencing despite early research documenting the salience of organizational factors and recent interest in assessing whether state sentencing policies increase the use of incarceration.

Three things are noteworthy about these findings. First, there is very little evidence that the size of the conservative population (an increasingly popular theoretical explanation for understanding variability in police force size, criminal justice spending, and growth in incarceration rates) exerts any discernable influence on the odds of incarceration. This appears to be the case across multiple time periods and across single and multi-state samples of felony case filings. Second, the direction of the effects for racial composition and organizational context depends on the data and state in question, suggesting a need for a broad sample of cases across multiple states. Equally puzzling is the inconsistent direction of the effect of guidelines in three studies that analyze the same sample of cases from the SCPS. The presence of sentencing guidelines appears to increase the odds of incarceration in one study (Pardoe and Weidner, 2006), yet decrease the odds of incarceration in another (Weidner et al., 2005), and appears irrelevant to jurisdictional variation in incarceration in another study (Fearn, 2005). Overall though, the dominant theme to emerge from a review of previous research is the relative *inability* for the majority of these contextual predictors to significantly account for jurisdictional variation in the odds of incarceration. The research provides little consensus as to why some jurisdictions are, on average, more punitive than others. Thus, despite a substantial body of research that demonstrates that the severity of sanctions applied to defendants varies across communities, our understanding of the sources that account for geographic variation in sentencing remains limited.

The Current Study's Contribution

In one of the earliest ethnographic studies to examine how courtroom environments shape sentencing, Eisenstein and Flemming concluded that although organizational resources, working styles, and interpersonal relationships among court personnel are key factors impacting sentencing, two elements of a court's environment are particularly powerful forces that shape the court's authority to punish: "the legislature as the source and definer of sanctioning authority, and public opinion as the context in which workgroup members develop expectations about punishment" (1977: 263). A review of the findings presented here demonstrates that these two aspects of courtroom environments are not typically examined in contemporary contextual sentencing studies. Perhaps the most significant shortcoming of existing studies is the lack of attention to the role that state sentencing policies may play in generating significant variability in the severity of sentencing outcomes. A comparative analysis of sentencing across a variety of sentencing structures would fulfill a gap in our understanding of the effects of policies on the propensity of courts to impose incarceration. As Stemen and Rengifo recently noted, the tradition of relying on socioeconomic variables to explain jurisdictional differences in incarceration and sentencing "dismisses the variation in state-level sentencing and corrections policies and confounds the impact of ideological and political covariates of imprisonment with state-centered interventions" (2011:175). Perhaps one reason why it has been a challenge to successfully explain jurisdictional variation in sentencing severity in studies that analyze cases pooled from multiple states is that these studies have ignored the types of state-level choices that exert the most powerful

influences on both the discretion of judges and the severity of the sanctions they are permitted to impose.

When multilevel studies have considered the effects of policy, they have restricted their focus to guidelines, and even then, have not offered a strong theoretical rationale for how guidelines should influence the severity of punishment. For those states that do operate under guidelines, including a binary variable indicating the presence of a guideline scheme acknowledges that there are constraints on judicial behavior in that state, which should help to reduce geographic variation in sentencing outcomes by promoting more uniform sentences. But merely accounting for the presence of guidelines assumes that the content, intent and forms (voluntary vs. mandatory) of these guidelines systems do *not* vary. Empirical research on the implementation and effectiveness of various guideline schemes shows that judicial adherence to the guidelines is shaped by whether the guidelines are legislatively mandated or merely advisory (Marvell, 1995; Tonry, 1999). In addition, prior research has focused exclusively on estimating the main effects of sentencing guidelines and has not yet considered the possibility that guidelines might *indirectly* increase the severity of punishment by giving greater weight to the effects of having a prior criminal history. While an important first step in providing baseline evidence about the effects of guidelines on sentencing across multiple state jurisdictions, this approach still leaves unaddressed variation in penalties for crimes across states without guidelines. For instance, prior work gives the impression that the only legal factor shaping jurisdictional variation in sentencing severity is the presence of structured sentencing. This is not the case. Every state has a penal code that dictates a minimum, maximum, or range of sentence lengths a judge may consider. As an example,

one area of statutory law that exhibits a remarkable degree of state variability are minimum penalties for the sale and possession of the smallest amount of drugs that typically qualify as a felony (usually 1 oz) (Pillsbury, 1989; Stemen et al., 2005). In addition, a recent survey of state sentencing policies documents considerable variability in the extent to which states have embraced mandatory enhancements, parole restrictions, and truth in sentencing provisions that require offenders to serve portions of their prison terms; these types laws were embraced by a larger number of states in an effort to crack down on crime (Pillsbury, 1989). However, the effects of these policies have not been examined using individual case data that can assess whether policies explain more punitive sentencing while adjusting for legally relevant aspects of each case.

The current study addresses these gaps by assessing both the main and interactive effects of a variety of sentencing structures and substantive laws that regulate the severity of punishment. This study also contributes to existing research by considering the effects of court organization and aspects of the social climate that are highlighted in theories of punishment, but that have yet to be tested in prior research. These precise factors are discussed in greater detail in the discussion describing the theoretical framework used to develop this study's hypotheses. The remainder of this chapter summarizes the major theoretical models of punishment that are used to develop specific hypotheses that tested in the current study.

THEORETICAL PERSPECTIVES GUIDING CONTEXTUAL SENTENCING RESEARCH

Recent efforts to extend theorizing on punishment reflect a new interest towards integrating structural, organizational, and cultural theoretical perspectives using multilevel statistical techniques. Drawing upon organizational efficiency models and macro-level theories of crime control, contemporary contextual research using explicitly multilevel regression-based techniques tie a fairly consistent set of community and state-level predictors to the disposition of criminal cases. These frequently include racial composition, economic conditions, crime rates, political conservatism, and local case processing strategies. These variables reflect a diverse theoretical field devoted to punishment that includes social threat, structural functionalist, economic perspectives, and organizational theories. These perspectives can be organized according to four theoretical traditions that center on a combination of societal and justice system forces that influence punishment: the Durkheimian emphasis on law as a reflection of collective values and punishment as the protection of commonly-shared interests; Marxist/conflict perspectives that view the mobilization of law as a means of protecting the socioeconomic interests of more powerful segments of society; economic/deterrence perspectives which argue that sentencing decisions are sensitive to organizational costs and constraints; and Weberian explanations that characterize “ideal” types of punishment as a dispassionate, highly “rationalized,” and calculable.

A key assumption that unites the first two perspectives is that the exercise of law reflects social norms, relations, and personal interests, rather they be rooted in consensus or conflict. Scholars such as Liska (1992) and Garland (1990; 2001) stress that values

and interests undergird why structural arrangements are believed to shape the intensity of crime control. This has prompted scholars to begin integrating more consistently indicators of political and religious ideology into models predicting legal outcomes (e.g., Fearn, 2005). Garland (2001) in particular has reasserted the importance of considering public sentiment and the political mobilization of interest groups for understanding the symbolic aspects sentencing (see also, Zimring, Hawkins, and Kamin, 2001; Zimring, 2003). For instance, he reasons that neo-Marxist theorists linking unemployment and inequality to penal power were “explicating penal culture and grounding it in the structures of social life,” but that ultimately, “in linking penal culture directly to social structure this way, these accounts tend to leave out of account large stretches of the mediating cultural framework in which penalty exists – particularly those forms of cultural life such as religious sentiment and humane sensibility which do not fit with their theoretical approach” (1990: 210). Efforts to sway public sentiment motivates political efforts to shift crime control authority away from criminal justice agencies, although there is some evidence to suggest that lawmakers are out of touch with a significant portion of Americans who express a desire for therapeutic justice measures, especially for drug users (Beckett and Sasson, 2000; Tonry, 1999). These arguments suggest that without fear, public distrust in the ability of criminal justice agencies to control crime, and some willingness to support punitive sentencing laws, there are fewer incentives for political parties to bear the mantle of ‘moral entrepreneurship’ over the crime problem as a means to gain public favor. For these reasons, the tone and strength of political partisanship, public attitudes about crime control, and fear of crime may significantly

contribute to geographic variation among states and local communities in sentencing behavior.

One way to advance the contextual study of sentencing behavior then is to evaluate the affects of more direct and proximate social conditions through which political party strength, unemployment, inequality, or race relations may impact the decisions of judges and prosecutors. As I elaborate on shortly, classic and contemporary explanations of community differences in legal sanctioning suggest several relevant indicators worth considering. The Durkheimian tradition emphasizes that religious values and a normative climate supportive of harsh sanctions affect the severity of legal sanctions. Contemporary interpretations of the Marxist/conflict perspective emphasize that “threatening” groups (e.g., the poor, racial and ethnic minority groups) can be expected to elicit more punitive social control insofar as they trigger fear of crime, insecurity about the judicial response to it, and foster a perception among the ruling class that their social position is in jeopardy (Chambliss and Seidman, 1971; Liska, 1992). These factors feature prominently in sociological theories of social control as well as current sentencing theories that predict that judges and prosecutors develop norms about appropriate reactions to crime based on their local surroundings (Eisenstein & Jacob, 1977; Steffensmeier et al., 1998). Estimating the link between structural arrangements and the local normative climate makes survey-based research on public attitudes increasingly important to understanding community differences in punishment severity. Survey research has established that fear and ideology are important predictors of punitive orientations, and that levels of fear (Liska et al., 1981), support for retributive punishment (Baumer, Messner, and Rosenfeld, 2003; Borg, 1997), and the racial

typification of crime (Quillian and Pager, 2001) all significantly vary across communities. These studies are an important part of the theoretical framework I draw upon for the current study and I discuss the implications of it for predicting variability in legal decision making throughout this chapter.

The normative and conflict-based perspectives detailed above clash with Weber's description of the ideal legal process as a mechanistic adherence to legal rules and precedent (Garland, 1990). The former characterizes punishment as vengeful reaction, the latter as a process of neutral uniformity. Neo-Weberian perspectives on sentencing are exemplified by state efforts to regulate the use of substantive, extralegal criteria during sentencing (Dixon, 1995; Salvesberg, 1994; Ulmer and Kramer, 1996). The most controversial of these reforms include efforts undertaken by some states to move away from the indeterminate sentencing model, to control judicial discretion through presumptive sentencing guidelines, and the adoption of habitual offender statutes such as the "three strikes" penalty. Although these relatively new legal procedures and sentencing options may clash with organizational efficiency and public interest, the thrust of neo-Weberian explanations of legal processing is that legal rules are the most important factor judges consider when disposing of cases. Consistent with this theme, Garland (2001) argues that public sentiment is important to understanding punishment, but perhaps primarily through its enduring effects on the adoption of more punitive sentencing laws and philosophies.³ Thus, a critical issue confronting sentencing research

³ A recurrent theme in classic and contemporary legal perspectives (e.g., focal concerns) is that public sentiment is expected to shape judicial norms about danger and culpability, and guide perceptions of appropriate punishment based on community standards. Garland makes a broader argument about criminal justice functioning though, summarizing the process as "the result of political choices and administrative decisions- but these choices and decisions are grounded in a new structure of social relations and coloured by a new pattern of cultural sensibilities" (2001:6). He goes on to argue that this has resulted in a chief focus of crime prevention being one of "fear reduction" alongside traditional concerns of prosecution and

is the need to distinguish between the affects of public sentiment and political interests on sentencing from that of *institutionalized* public outcry embodied in sentencing policies designed to increase the certainty, and in some cases the severity, of felony sentences. In doing so, we can begin to examine the tension between state policy and local context, but even more importantly, we can begin to systematically compare the consequences of implementing populist sentencing reforms by comparing variation in the disposition of individual defendants across these diverse systems. This study goes beyond existing research on jurisdictional variation in sentencing severity by incorporating differences across jurisdictions in policies intended to increase the certainty or severity of punishment (e.g., sentencing guidelines; three-strikes laws; the abolishment of parole), organizational constraints on the use of imprisonment (prison capacities, correctional spending), and policies that provide fiscal incentives to *not* imprison offenders (e.g., charging offenders fees for their state supervision). To date, this line of research has not systematically assessed the effects of each of these legal and organizational attributes on individual probabilities of imprisonment despite the widely held view that punitive sentencing reforms, not crime rates, are primarily responsible for growth in prison admissions over the last 30 years (Blumstein and Beck, 1999; Raphael, 2009).

Below I review the theoretical foundations influencing the study of jurisdictional variation in sentencing severity. The study of community variation in punishment includes a broad set of explanations that are usually integrated to provide a comprehensive understanding of the ways in which social contexts affects the outcome of

punishment (2001: 17). Thus, there are two related, but different arguments present in the literature- one argues that public sentiment impacts judicial decision making indirectly through judicial discretion to 'represent' public sentiment, the other being that punitive public sentiment affects the implementation of punitive sentencing laws which judges then implement in the courtroom.

criminal cases. I begin by first describing the theoretical foundation in the sociology of law for linking judicial behavior to the broader social environment. In relating social structure to individual level behavior, the multilevel literature assumes a subtle, interlocking relationship between sociological theories of social control and theories of institutional functioning and judicial decision making. Aggregate patterns of punishment are the result of decisions made by individual legal actors and the theoretical bridge between structural conditions and legal decision-making is important to establish.

Sociological Jurisprudence

Research examining the environmental aspects of legal decision making is guided by the theoretical principle that courts respond to deviance on behalf of collectives (e.g., the criminal justice system, the community, victim advocates, and influential groups), and that social structure, the political environment, and public interests impart subtle, but substantively important influences on judicial behavior. Sociological jurisprudence, a sub-discipline of legal realism, bridges sociological theory with social-psychological perspectives on legal decision making to contend that the ‘law in action’ frequently contradicts the ‘law in the books,’ (Pound, 1910). Arguing that the law achieves socioeconomic as well as legal purposes, legal realists argue that the law ‘in action’ can be understood in relation to other social institutions because “judges and courts are products of the broader social and political systems in which they exist” (Myers and Talarico, 1987: 9).

The distinction between sociological jurisprudence and the broader sociological study of punishment is that the former provides an explicit account of why other social

institutions penetrate courts to influence the legal process, while sociological theories of punishment make specific predictions about *which* features of the social context impact sentencing and the nature of that impact on sentencing severity. Much of the contextual research on sentencing owes a debt to the seminal work of Nonet and Selznick (2001), who provided a theoretical typology of the various forms and functions of the law. These models helped provide an entry point from which to merge the study of sentencing and legal decision-making with an explicit consideration of the ways in which law must contend with other social institutions, and with local public and political interests. Nonet and Selznick (2001) describe a three-part typology of repressive, autonomous and responsive legal forms that is designed to indicate the degree to which courts are sensitive to pressures placed on them by the economic conditions, the public, and various justice system stakeholders. These pressures are predicted to frequently disrupt the routine application of objective legal policies in favor of a law that adjusts to social reality. The first of these, *repressive law*, is characterized as a politically opportunistic style of legal behavior in which legal institutions may adapt legal reasoning to fit with current public mood and political interests. Numerous studies conducted over the last two decades provide evidence consistent with the opportunistic model of legal behavior. In one of the first systematic attempts to investigate environmental constraints on legal decision making in a criminal court setting, Gibson argues that trial judges actively “represent” community values and norms out of fear of electoral defeat, or out of a sense of moral obligation to the constituents that elected them (1980: 362). More recent analyses of sentencing in Pennsylvania also lends some support to this politicized view of the law by showing that judges issue significantly harsher sanctions as they near the date of their

retention election (Huber and Gordon, 2004). Thus, despite the fact that most judges face a low objective risk of electoral upset, these types of patterns have led some legal researchers to characterize courts as distinctly political institutions (Gibson, 1980).

In contrast to the repressive legal model, *autonomous systems* are preoccupied with preserving the authority and legal integrity of the court by keeping the court system insulated from the needs and interests of political figures and the public through a rule-centered system of “mechanical jurisprudence” (Nonet and Selznick, 2001: 61). This system closely resembles Weber’s model of legal formalism, which predicts that the most powerful predictor of legal behavior is the law itself, and that legal outcomes will reflect the goals of procedural fairness and equality. Ultimately, adopting either of these two systems (a strict repressive or autonomous system) poses a fundamental dilemma for courts. As Nonet and Selznick explain, courts must uphold legal principles (or else risk intrusion by the government) while remaining responsive to their respective constituencies. If courts remain closed and autonomous, they risk becoming socially irrelevant. This leaves the courts unable to enforce the decisions they hand down, and in the case of trial judges, vulnerable to political competition for their seat on the bench. In essence, courts cannot conflict too much with their social and political environment, even as they risk politicizing the legal process. Following a decade of controversial rulings by the Warren Court during the civil rights era, a crisis of judicial authority in the 1960s prompted a shift toward a *responsive style* of law. Providing a balance between repression and autonomy, responsive law represents an attempt to uphold legal doctrine and maintain institutional integrity, while also acknowledging social problems, remaining “responsive to social needs” and adjusting to changes in social context (Nonet and

Selznick, 2001; see also Hurst, 1971). The contemporary research on the social contexts of sentencing reflects the tenets of the responsive legal model- that judicial behavior should closely approximate the law, but that in practice legal decision making can often be understood in relation to public desires, political interests, and fiscal constraints confronting local criminal justice systems.

Contemporary Theories of Sentencing: Focal Concerns and Causal Attribution

Nonet and Selznick's framework advanced the view that legal behavior frequently achieves social, economic, or political purposes in addition to legal ones (Savelsberg, 2002). Although measuring the content of local norms and social needs is challenging, the notion that community standards influence the legal process is well established in the theoretical literature on law enforcement and sentencing. The focal concerns perspective of sentencing behavior, which has become a popular framework from which to analyze sentencing practices, shares these assumptions that courts absorb as well as regulate community life. According to focal concerns, public norms, the political environment, legislative pressures, and organizational values serve as environmental cues that impart subtle signals to judges that certain acts and defendants are perceived as more dangerous by the community, and that a deliberate message to offenders (i.e., retribution) and a need to deter future offending is desired by their constituency. The focal concerns framework blends insights from deterrence perspectives, Black's (1976) propositions tying stratification to the quantity of law, Albonetti's (1991) uncertainty avoidance theory of case processing, and the sociological literature on organizational efficiency. According to this perspective, judges weigh three focal concerns when sentencing defendants: the

offender's blameworthiness, protection of the community, and the practical implications of the sentence for defendants, victims, and other justice system agencies (Steffensmeier et al., 1998). First, the primary purpose of sentencing is to mete out sanctions that fit the severity of the crime. Steffensmeier et al. (1998) argue that regardless of where defendants are sentenced, explicitly legal criteria will be the strongest predictors of sentencing severity. Specifically, judges are expected to first take into account the nature of the defendant's role in the incident, the injury to the victim, and any mitigating factors that reduce their culpability when determining sentences. Consequently, defendants who play only a secondary role in the offense, the absence of a weapon or physical injury, very young offenders, or those who have experienced a history of victimization or mental illness may be viewed as less blameworthy by the court and are expected to receive more lenient sentences (Kautt and Spohn, 2007). A second focal concern of judges is community protection. Judges are expected to base sentencing in part on their perceptions of the defendant's dangerousness and likelihood of reoffending. Thus, defendants with more serious offending histories are expected to receive more punitive sentences in an attempt to incapacitate the defendant and deter future offending. In particular, those with violent or multiple prior convictions are significantly less likely to escape a felony conviction with probation given that these defendants will be viewed as less reformable (Steffensmeier et al., 1998). Thus, the main way judges address concerns of community safety and recidivism is through sentences that incapacitate the most dangerous offenders and deter crime.

Last, the focal concerns framework predicts that judges take into account the impact of their decisions on other agencies and the offender. From an organizational

standpoint, judges must maintain healthy relationships with other court actors, efficiently manage their caseloads, and often consider the impacts of their decisions on the fiscal costs to probation departments and prisons (Steffensmeier et al., 1998: 767; see also Dixon, 1995; Flemming et al., 1992; and Ulmer and Kramer, 1996). As a result, judges may mete out more lenient sentences offered through plea bargains in the interests of maintaining case processing efficiency for prosecutors and defense attorneys. These focal concerns represent potentially important sources of jurisdictional variability in sentencing that stem from community differences in perceptions of dangerousness, blame, and rehabilitation potential for offenders.

In recent applications of this perspective, Ulmer argues that judicial reliance on these focal concerns is universal but that importance, meaning, and interpretation of them is local (Ulmer, 1997; Ulmer and Johnson, 2004). Focal concerns proponents argue judges' interests in protecting the community and concerns about the costs of punishment are intertwined with the local political climate, criminal stereotypes and the fears of residents, and the local legal culture in ways that foster community variation in legal decision making (Steffensmeier et al., 1998; Ulmer and Johnson, 2004). The most potent public pressure judges may face is balancing the need to protect the community with the costs sustained from relying on incarceration or intensive supervision. Steffensmeier and colleagues point out that because judges are accountable to local politicians, victims, and their constituency, they are sensitive to the potential for community backlash to recidivism and to the perception that judges do not adequately protect the community (Eisenstein et al., 1988; Steffensmeier et al., 1998: 767; see also Myers and Talarico, 1987). Judges may feel vulnerable to public perceptions of leniency

for several reasons. Trials are rare, and the majority of residents will never be charged with a felony offense or given any firsthand insight into the sentencing process.

Consequently, research shows that the media and political messages play a pivotal role in informing the public about sentencing (Roberts and Doob, 1990). This is problematic for judges because politicians and the media often address only violent and ‘newsworthy’ cases that provide little detail about the sentencing process. This in turn may encourage a desire among the public for more punitive sentences that would incapacitate offenders and alleviate public anxiety that stems from the media construction of the crime problem (Roberts and Doob, 1990). Thus, conservative political climates and communities with more punitive public attitudes might signal to judges that one way to avoid uncertainty in determining the threat defendants may pose to the community is to err on the side of incapacitation.

In sum, the focal concerns perspective argues that community context shapes sentencing in two key ways: local community standards and tolerance for recidivism will shape the severity of sentencing, and second, that stereotypes that disadvantage certain defendants partly reflect the prevailing community sentiment about the causes of crime and the threats posed by certain groups (Steffensmeier et al., 1998). In some key respects, the focal concerns perspective represents a deterrence-based model of sentencing by predicting that judges maximize the goals of community safety and retribution while minimizing the social and fiscal costs of their decisions. These goals are often mediated by the reality of uncertainty in the legal process, and the compensation of that uncertainty through the reliance on community norms as well as racial and gender stereotypes. However, the focal concerns model does not offer specific predictions about

which or in what direction community characteristics might yield more or less punitive sentences; only that judges are sensitive to public and political pressures, and that they may share the community's stereotypical views about crime and certain defendants. Nevertheless, it provides a useful starting point for predicting a) How community conditions like crime, fear, and political composition may provoke less favorable treatment of offenders; b) That judges will consider prison crowding and organizational constraints when weighing the costs of incarcerating offenders relative to the uncertain risk of recidivism if given probation; and c) That judges may share in the community's stereotypical views that certain defendants are more dangerous. Below I describe the theoretical perspectives that identify the community factors and organizational constraints believed to be the most relevant macro level conditions for understanding jurisdictional variation in sentencing severity.

MACRO LEVEL THEORETICAL PERSPECTIVES

This study develops and tests hypotheses that are informed by four of the major sociological theories of punishment: neo-Weberian models that focus on procedural rules and the law, organizational efficiency models which emphasize the costs of punishment, social conflict models which view the purpose of punishment as a means to control threatening subordinate groups, and neo-Durkheimian perspectives which emphasize social solidarity and the emotional and ethical aspects of punishment. I begin the discussion with an overview of the functionalist perspective, which provides the primary alternative hypothesis to Weberian, Durkheimian, conflict, and organizational models of the law. In this regard, the factors emphasized by functionalist models are typically

treated as controls in multilevel studies of legal outcomes, despite the fact that legal factors consistently exert the strongest influences on sentencing outcomes.

FUNCTIONALIST EXPLANATIONS

Jurisdictional differences in the severity of sentencing may merely reflect differences in levels of crime and the composition of cases courts process. This view is consistent with instrumentalist or functionalist perspectives, which view the purpose of punishment in terms of controlling crime and fear (Tyler and Boeckman, 1997).

According to Garland, “although legal punishment is understood to have a variety of aims, its primary purpose is usually represented as being the instrumental one of reducing or containing rates of criminal behavior” (1990: 18). Viewed this way, sentencing is a means to an end, and the severity of sentencing is expected to increase in proportion to the severity of the offense (or at the macro level, crime rates). According to this perspective then, the majority of the variation should be explained by legally relevant case-level factors such as offense severity and the number of charges for which a defendant is convicted. It is also consistent with instrumentalist perspectives to expect that any residual variation in sentencing not explained by these legal factors might be explained by variation across jurisdictions in levels of crime and fear. The precise nature of these relationships and empirical evidence of their importance are described below.

Case Composition

Jurisdictional variation in the disposition of felony cases may reflect compositional differences in the types of cases and defendants that courts process

(Gibson, 1980; Huang et al., 1996). Contemporary research on sentencing demonstrates that legal criteria such as offense severity and mitigating or aggravating circumstances consistently exert the greatest influence on criminal case outcomes, although the magnitude of the effect may vary across courts and types of defendants (e.g., Bumiller and Hagan, 1983; Bushway & Piehl, 2001; Dixon, 1995; Engen & Gaaney, 2000; Eisenstein et al., 1988; Sampson & Lauritsen, 1997; Spohn, 2000). These findings lead to the expectation that once controlling for offense severity, prior criminal history, defendant attributes, victim attributes, and differences in the way cases are processed, any observed differences in sanctioning severity across jurisdictions should reduce substantially. The case level factors I review below are thus expected to exert the strongest influences on case outcomes.

Prior Criminal History

In addition to the severity of the offense, prior criminal history is an important factor judges weigh at sentencing. Prior research suggests that first time offenders are more likely to be diverted out of the felony court system while those with a prior criminal history are significantly more likely to face conviction and sentencing (Spohn, 2000; Wooldredge and Thistlethwaite, 2004). Although defendants previously arrested and convicted face significant disadvantages in the court system, defendants who have been *incarcerated* for any length of time can expect a substantially higher probability of going back to jail or prison (Welch, Gruhl, and Spohn, 1984).

Today the importance of prior criminal history has taken on new meaning. Serious offenses committed by repeat offenders ignite public outrage and harsh

sentencing reforms, as evidenced by the Willie Horton scandal during the 1992 Presidential election, and the Polly Klaas murder that triggered the passage of three-strikes legislation in California and sex offender registration laws. While concern over recidivism is fairly universal, the legal importance attached to prior criminal history varies across states, which may explain a substantial amount of variability in sentencing severity within the U.S. For example, habitual offender laws may magnify the impact of prior criminal history in some jurisdictions, suggesting that prior criminal history may explain a substantial portion of the variability in sentencing severity between jurisdictions. Judges are believed to be acutely sensitive to the public perception that the courts are ineffective at curbing the most dangerous offenders (Myers and Talarico, 1987; Steffensmeier et al., 1998). Thus, there is a good reason to expect that given the public and political fears about the threat posed by repeat offenders judges and prosecutors will view repeat offenders as more blameworthy, unsuitable for rehabilitation, and will reduce any uncertainty that these offenders may offend again by resorting to incapacitation (Ulmer et al., 2008). For these reasons, a history of felony arrests and convictions, and any record of prior jail or prison incarcerations should greatly increase the probability of imprisonment net of the severity of the offense. This may be especially true in jurisdictions where states grant discretion to judges and prosecutors to sentence defendants under habitual offender laws. Thus, one would expect that:

The positive effect of a prior criminal history on the odds of incarceration will be stronger in jurisdictions that have habitual offender laws.

In addition to habitual offender penalties, twenty-one states have adopted voluntary or mandatory sentencing guidelines that instruct judges to consider both prior

criminal history and offense severity when determining the type and duration of sentences, leading some to worry that sentencing guidelines may increase prison growth by simultaneously punishing offenders for their failure to learn from past mistakes on top of the sanction for the current offense (e.g., Engen and Gainey, 2000; see Kauder and Ostrum, 2008, Marvell, 1996, and Stemen and Rengifo, 2011 for reviews).⁴ The purpose of sentencing guidelines thus suggests that:

The effect of prior criminal history on the odds of incarceration will be stronger in jurisdictions that have sentencing guidelines.

Thus, prior criminal history is expected to exert both direct and conditioning influences on the probability of incarceration. This research examines the influence of criminal history on sentencing with a series of measures that capture prior felony and misdemeanor arrests and convictions, as well as prior jail and prison terms served.

Case Processing Factors

Research has shown that case processing factors such as mode of adjudication, type of counsel, and pretrial detention exert important influences on the disposition of felony cases. Penalizing defendants who exercise their right to trial through harsher sentences is particularly entrenched in U.S. legal culture. Blumberg's (1966) influential portrayal of prosecutor and defense attorney plea negotiations as a "confidence game,"

⁴ In their study of in/out and sentence length decisions in Washington jurisdictions, Engen and Gainey (2000) demonstrated that the relationship between prior criminal history and sentence lengths is not linear. They argue that in guideline states researchers should consider modeling the interactive or quadratic relationships between offense severity and criminal history and final sentence disposition, or alternatively simply enter in a 'presumptive sentence' in lieu of offense severity and criminal history. In a reply however, Ulmer (2000) demonstrated that this technique yields little to no improvement in model fit for equations predicting the decision to incarcerate. Ulmer concludes that controlling for the linear, additive effects of prior criminal history and offense severity provides better model fit than the substitution of these factors for a legally prescribed 'presumptive in/out' measure.

illustrated that defense attorneys often urge their clients to relinquish their right to trial in order to efficiency expedite cases in exchange for greater leniency and certainty about the final sentencing outcome. Indeed, across diverse jurisdictions and historical periods, research repeatedly documents that exercising one's right to trial is associated with more severe sentencing dispositions, a penalty believed to stem from the burdens trials place on case processing efficiency (Brereton and Casper, 1981-1982; Eisenstein et al., 1988; Eisenstein and Jacob, 1977; Johnson, 2003; Ulmer, 1995; McCarthy and Lindquist, 1985; Ulmer, 1995, 1997; Ulmer and Bradley, 2006). Although plea discounts are considered a universal feature of urban criminal case processing, the number and size of plea discounts may vary across communities and states. For example, in a general turn towards instituting more uniform sentences some states such as California adopted determinate and structured sentencing ranges that coincided with legal limits on prosecutors' discretionary power to plea bargain (King et al., 2008; Tonry, 1996). Despite some states' attempts to regulate plea bargaining trials are relatively uncommon in large urban courts (Heumann, 1974; Padgett, 1985). Data used in the current study indicate that only 5% of cases proceed to trial. Jurisdictions also differ in the types of pleas they offer. Depending on the type of plea allowed in the jurisdiction, pleas can yield dividends ranging from no sentence discounts, explicit sentence length reductions, a downgrade in charges, or the guarantee of a specific sentence agreed upon by judges (Padgett, 1985). While the trial 'tax' is widely regarded as pervasive across courts, other scholars argue that plea discounts are less frequently applied to defendants charged with more serious crimes (King, Soule, Steen and Weidner, 2005; Ulmer, 1997; although see Ulmer and Bradley, 2006). Thus, controlling for the mode of adjudication is important in modeling

sentencing outcomes, although there are important reasons to suspect it does not yield much leniency for serious crimes and in some jurisdictions.

The research remains mixed on whether private legal representation helps defendants avoid more punitive sentences. Some scholars argue that public defenders place defendants at a disadvantage and that quality of counsel serves as a key source of sentencing disparity for poor and minority defendants who are unable to obtain private counsel (Spohn, Gruhl, and Welch, 1981-1982). However, the degree to which attorneys shape sentencing outcomes appears to depend on court size and urban location. In one of the first contextual studies of court processing conducted in three courts in Pennsylvania, Illinois, and Michigan, Eisenstein and colleagues document that defendants tend to distrust attorneys working for the government and that low salaries for public defenders attracted less talented attorneys who were not committed to “zealous advocacy” (1988: 287). These reports substantiate the concerns voiced by Spohn and colleagues (1981-1982) that poor defendants (often synonymous being non-white in urban courts), face significant obstacles to obtaining a vigorous defense. Even so, the authors qualify their discussion by pointing out that in urban courts indigent defense is a cause supported by special interest advocacy groups that help insulate public defenders from bureaucratic and political pressures. Despite the concerns about the quality of public defense counsel, recent multilevel research offers little support to the claim that defendants with public defenders receive more punitive sentences (Fearn, 2005; Johnson et al., 2008; Wooldredge, 2007). However, type of legal representation may be consequential for pretrial outcomes (e.g., case dismissal or prosecutorial diversion) and the conviction phase, both of which directly impact the risk of being incarcerated. In the

present study, I control for county differences in the proportion of defendants represented by private legal representation and the proportion of cases plea-bargained with the expectation that both factors will be associated with less severe sentences.

Another potential source of jurisdictional variation in sentencing severity may stem from county differences in how judges set bail and the ability of defendants to secure pretrial release. There is a sizeable body of research that demonstrates a strong correlation between the severity of pretrial bail and release decisions and later conviction, incarceration, and sentence length outcomes (Albonetti, 1991; Free, 2001; 2002; Williams, 2003). Unfortunately, the majority of contextual sentencing studies have been conducted using data that do not document pretrial adjudication outcomes.

Consequently, its relevance for understanding final sentencing decisions remains an important but largely neglected component of the multilevel sentencing literature. This is a potentially critical limitation to understanding how state and community context shapes sentencing for several reasons. Pretrial confinement is a component of the adjudication process that has received little attention in the contextual study of sentencing despite some evidence that its influence on the decision to imprison defendants are nearly equal or greater in magnitude to the affects of offense seriousness (Fearn, 2005; Weidner et al., 2004; Wooldredge, 2007). While pretrial outcomes may seem relatively trivial, they deserve scrutiny because these decisions are less publicly visible than trial court decisions, defendants routinely negotiate initial pretrial proceedings without an attorney, and these decisions are not subject to judicial review as is often the case for sentencing decisions under presumptive guidelines (Free, 2002). Furthermore, these decisions are made without the benefit of witness testimony, evidence presented at trial, and mitigating

defense arguments. This makes judicial decision making at the pretrial stage highly discretionary, where assumptions of dangerousness and flight risk must be quickly made during arraignment. This opens the door to substantial substantive decision making criteria, due in large part to the absence of reliable evidentiary material sentencing judges typically have access to (Ulmer and Kramer, 1998). These factors combine to make this stage a locus of discretion, and potential class, racial, ethnic and gender and geographic disparities in punishment (Demuth, 2003; Schlesinger, 2005). Although scant, some research suggests that the severity of pretrial sanctions varies across jurisdictions and should be controlled in multilevel sentencing studies. Eisenstein and Jacob (1977) provide comparative aggregate and case level data on release decisions across Baltimore, Chicago and Detroit courts. Mean bail amounts were double in Baltimore what they were in Chicago and Detroit, so it is not surprising that less than half of Baltimore defendants secured release (compared to 61% in Chicago and 60% in Detroit). The authors concluded that “local culture” played a critical role in the exceptionally high bail fees in Baltimore compared to the other cities. They attributed this variability to the fact that Baltimore officials implemented harsher standards for release in response to the more conservative views of the local population. This research, combined with pretrial studies conducted at the individual level, suggest that during arraignment proceedings judges have much greater discretionary authority and less reliable or available legal information on which to base their decisions since bail decisions are typically made within 48 hours of arrest. If these pretrial decisions exert important influences on sentencing dispositions, and research suggests that they do, then this raises the possibility that many community factors shown to be associated with more punitive sentencing outcomes may actually

reflect pretrial decisions. I control for pretrial decision making with an indicator of whether defendants were denied bail or financially unable to secure their release.

Substantive Criteria

The literature remains mixed on the degree to which extralegal factors such as victim attributes, socioeconomic status, age, sex, race, and ethnicity factor into sentencing decisions. Information on victim attributes is rarely made available in large-scale sentencing data. A handful of studies suggest that cases involving murder and rape victims with lower social status, or victims who provoke or engage in disreputable conduct during the offense, are less likely to be prosecuted and result in significantly shorter prison sentences (Albonetti, 1987; Baumer, Felson, and Messner, 2000; Baumer and Martin, 2011; Spohn and Holleran, 2001; Spohn and Horney, 1993). In addition, several studies report an interactive relationship between victim-offender race and gender which suggest that black males convicted of assaulting white females face significantly greater risks of conviction and incarceration (LaFree, 1980; Spohn, 1994; Spohn and Spears, 1996). Offender race and ethnicity also exert significant influences on sentencing outcomes, although these effects tend to be small and indirect in nature (Sampson and Lauritsen, 1997). Additionally, studies show that race may exert more of an impact on earlier decisions such as charging and bail release, the effects of which accumulate as defendants move through the system (for reviews see Baumer, 2011; Sampson and Lauritsen, 1997; Spohn, 2000).

The research on sex disparities in sentencing suggests that while women contribute the greatest *growth* to the nation's prison systems (Blumstein and Beck, 1999),

by comparison males continue to receive more severe sanctions, even after controlling for the severity of the offense and criminal history. The most extensive meta-analysis on the issue reviewed studies covering 50 court data sets, most of which relied on data from the 1970s (Daly and Bordt, 1995). The authors report that over half of the studies reviewed report evidence that women receive more favorable treatment in the courts, and typically during the decision to incarcerate. Recent evaluations of the implementation of sentencing guidelines that aim to reduce sex-based disparities suggests that sentencing guidelines (and especially monitoring and publishing judicial decisions) have made sentencing a more objective process, but that defendant sex continues to be an important source of sentencing disparity (Griffin and Wooldredge, 2005; Koons-Witt, 2002).

Studies that control for defendant age indicate that very young offenders are more often given probation, but that offenders in their twenties, especially black males, face a significantly greater risk of imprisonment until age 30, at which point the odds of incarceration reduce substantially (Helms, 2009; Myers, 1987; Steffensmeier, Kramer, and Ulmer, 1995; 1998). Although the evidence remains mixed, the finding that youthful and much older defendants are more likely to avoid prison terms has been interpreted by focal concerns proponents as evidence of the type of ‘perceptual shorthand’ judges rely on to assess the potential for reoffending and chances of successful rehabilitation. Other recent studies that analyze sentencing data from multiple jurisdictions report no significant relationship between defendant age and imprisonment (Fearn, 2005; Maxwell, Robinson, and Post, 2003; Steffensmeier and Demuth, 2006; Weidner et al., 2005), although one study reports that older defendants receive significantly shorter sentences (Jacobs and Helms, 2002). Based on the existing literature I expect a curvilinear

relationship between age and the probability of confinement that leaves defendants between the ages of 20 and 50 most at risk of incarceration upon conviction.

To summarize, defendants charged with more serious offenses, those with more extensive prior records, males, black and Latino defendants, the very young and older offenders, those represented by public defenders and who exercise their right to trial, and those who receive pretrial confinement should explain why some jurisdictions more frequently incarcerate defendants. Each of these individual level and case processing factors, especially offense severity and prior offending, are consistently found to be among the most powerful predictors of sentencing outcomes regardless of geographic location. While early studies of community variation in punishment were unable to statistically partition the effects of case composition from structural attributes (e.g., Eisenstein et al., 1988; Myers and Talarico, 1987), recent multilevel studies show that although compositional differences account for the majority of the variation across counties in sanctioning severity. Local court jurisdictions still exhibit significant variability in sentencing behavior even after adjusting for case level attributes (Bongtrager, Bales, and Chiricos, 2005; Britt, 2000; Fearn, 2005; Johnson, 2005, 2006; Kautt, 2002; Ulmer and Bradley, 2006; Ulmer and Johnson, 2004; Weidner and Pardoe, 2004). In theory, this residual variation should be explained by the legal, organizational, and the social climate within which courts operate. Below I outline the legal and organizational factors that operate at the state and local level to shape variability in the severity of criminal sentencing.

Levels of Community Crime and Fear

It is now well established that communities vary in their levels of fear of crime (Liska et al., 1981), which may help to explain jurisdictional variation in the sentencing severity. The link between fear of crime and punitive sanctioning may reflect a pragmatic instrumental response to real or perceived concerns for public safety and personal victimization. This instrumentalist perspective has been used to examine public support for the death penalty as well as the passage of determinant sentencing policy in the state of Washington (Baumer et al., 2003; Steiger, 1998), and support for greater levels of crime control in the form of police force size (Liska, Lawrence, and Benson, 1981). While Garland (2000) specifically links fear in urban areas to demands for harsh justice, the literature has not established a consistent relationship between the two. For instance, some surveys reveal that trends in fear of personal victimization are not related to trends in support for the death penalty (Warr, 1995) and that the public supports punitive sanctions not because of personal fears about the risk of victimization, but because they view crime as a symptom of the decline of moral values and social cohesion (Stinchcombe et al., 1980; Tyler and Boeckmann, 1997).

However, I include fear of crime for two key reasons. First, as Garland (2001) has suggested fear of crime may be more salient among residents in or near major metropolitan areas that were largely the subject of media-fueled accounts of the crack and gun violence epidemics in the eighties and nineties. Even though survey data of adults do not provide a consistent link between fear of crime and public support for punitive measures, this is not necessary for models predicting criminal case processing. What is necessary is that you establish a plausible link between public fear and *judicial behavior*. It may be the case for example, that public fear does not fuel public support for punitive

responses to crime, but judges in these jurisdictions (influenced in part by media accounts and interactions with politicians and the public) may perceive shifts in levels of fear and feel it their duty to respond more harshly as gatekeepers of public safety (a key focal concern of judges), especially in jurisdictions where judges are elected. These arguments suggest the following hypothesis: *the odds of incarceration will be significantly higher in jurisdictions with higher levels of fear among local residents.*

NEO-WEBERIAN PERSPECTIVES:

LEGAL POLICIES AND ORGANIZATIONAL EFFICIENCY

Our understanding of the social contexts of punishment have been advanced greatly by empirical findings that highlight the tension between judicial and public sentiments about sentencing, what judges are expected to do according to the law, and what judges can do under various fiscal and organizational constraints (Eisenstein and Jacob, 1977; Gibson, 1983; Myers and Talarico, 1987; Kramer and Ulmer, 2002). Recognizing this tension, both multilevel and macro level studies of incarceration place a strong emphasis on assessing the ways in which policies both control judicial discretion, and dictate the severity of the sentencing options judges have to choose from. Much of the research describing the evolution of sentencing guidelines casts legal decision making in either “substantive” or “legally rational” terms (Salvesberg, 2002; Ulmer and Kramer, 1996). The concept of substantive versus rational law can be traced to Weber’s “ideal” type of law which views punishment as highly rationalized, especially in large complex bureaucracies such as urban courts. Researchers frequently draw upon Weber’s sociology

of law to describe how neoclassical reforms have altered the uniformity and severity of sentencing (e.g., Engen and Steen, 2000; Garland, 2000; Johnson, 2005; Kramer and Ulmer, 2002; Ulmer and Kramer, 1996; Zhang, Maxwell, and Vaughn, 2009).

Describing what he termed “formally rational law,” Weber argued that in modern democratic societies the ‘ideal’ system of law can be characterized as highly specialized, bureaucratic, and dominated by formal procedure (Engen and Steen, 2000; Kramer and Ulmer, 2002; Myers and Talarico, 1987; Weber, 1978). Under this type of legal system, the sentencing process is dispassionate and objective. It hinges on the neutral and uniform application of laws that are tailored to make sentencing highly predictable.

Viewed through this lens, legal decision-making in highly bureaucratized courts is devoid of the irrational forces of politics and public sentiment and is guided instead by formal-rational decision-making. Formally rational law occurs when courtroom decisions are guided by legal rules in a “neutral managerial” style that also seeks to “promote cost efficiency and administrative convenience” (Garland, 1990: 192). The key factor distinguishing this perspective from conflict and neo-Durkeimian models is that this perspective views highly rationalized bureaucracies as less accessible to the public, and insulated from public opinion about crime. Summarizing the development of more rational and bureaucratized punishment in the last 100 years, Garland remarks “these developments have had the effect of reducing the immediacy with which popular sentiment or political concern can be expressed within the act of punishing- as well as decreasing the direct knowledge and experience that most citizens have of this process,” (1990: 187). From this perspective, legal factors like offense severity and criminal history should dominate sentencing decisions, and any variability in the severity of sentencing is

believed to reflect jurisdictional differences in the severity of its legal code and sentencing philosophies.

Drawing on Weber's legal framework, Savelsberg maintains that the new brand of legal formalism views offenders as rational beings, that legal sanctions will reflect the goals of retribution and deterrence, that the modes of decision making will be determinate and predictable, and that prisons will be the preferred means of delivering punishment in lieu of therapy, probation, or welfare (1994). Although Savelsberg applied Weber's concepts to the adoption of sentencing guidelines, Garland (2001) and others point to other attempts to make the legal process more objective and retributive: determinate sentencing, mandatory enhancements, and habitual offender laws (Barker, 2006; Tonry, 1996, 1999). These policies reflect differing goals, but each are consistent with the neoclassical ethic of punishment as "without regard to substantive concerns about the offender, the perceived causes of criminality, or the anticipated consequences of punishment for the individual offender," (Engen and Steen, 2000: 1361).

In sum, this perspective points to a variety of contextual factors that create the overall legal culture in which judges and prosecutors process cases. These include other sentencing policies and rules besides guidelines that also control (or eliminate) judicial discretion as well as substantive penal statutes that guide the severity of sanctions judges may mete out, including determinate sentencing structures, mandatory minimums, three-strikes legislation and habitual offender statutes, truth in sentencing provisions, and the specific minimum or fixed penalties state legislatures mandate for certain offenses. Based on the Weberian perspective, one would expect that sentences are determined primarily by legally relevant factors associated with the case, and as a result structured

sentencing schemes such as guidelines and determinate sentencing should be expected to reduce the amount of residual variation among counties within states that operate under those laws. But the logic of this argument also suggests that when and if substantive legal policies that dictate harsher sanctions compared with the policies in other jurisdictions, we should expect to observe more punitive sentencing outcomes.

In addition, neo-Weberian perspectives also emphasize that legal-decision making is highly bureaucratized and should be expected to reflect a managerial style that values efficiency. Using this logic, researchers have identified several aspects of the organizational climate that are in keeping with Weber's view of institutions as preoccupied with issues of cost and efficiency. These include court case load pressures, inefficiencies that arise from prison and jail crowding, the amount of financial resources states devote to paying for the sanctions that courts mete out, and the ability of local and state agencies to offset the high costs of prison and jail by charging fees for community supervision. These two forces, legal rationality and organizational efficiency, often coexist in conflict with each other since the goals of law may undermine organizational priorities for efficiency (Engen and Steen, 2000; Ewing, 1987). Irrespective of the tension between the goals of formal legal rationality and the interests of organizational efficiency, these two aspects of court context are predicted to exert the most proximate influences on courtroom decision-making.

SENTENCING POLICY

Building off of Weber's emphasis that judges behave according to rules and laws, the current study examines the effects of several policies that the literature suggests might exert important influences on the overall severity and geographic uniformity of punishment. These policies include guidelines and determinate sentencing structures, mandatory minimum enhancements, truth in sentencing requirements designed to increase the certainty of punishment, habitual offender laws, and the severity of state penal codes that dictate the sentence ranges for felony offenses. A brief description of these policies and their hypothesized effects are discussed below.

A number of scholars attribute growth in the use of prisons (and the severity of punishment generally) to changes in sentencing legislation (Barker, 2006; Garland, 2001; Merritt, Fain, and Turner, 2006; Tonry, 1996, 1999). Sentencing practices have been dramatically refashioned by many legislatures to reflect new sensibilities about crime that are more in line with the goals of deterrence and retribution, while at the same time redistributing discretion across the justice system (i.e., to prosecutors, parole boards, etc.; Garland, 2000, 2001). Sentencing structures, the severity of legal penalties, and local courts' willingness to comply with state policies varies substantially. There has been a concerted effort in recent years to document the impact of sentencing policy choices on the levels and growth of state incarceration rates (see Stemen and Rengifo, 2011 for a review). Analyzing changes in prison admissions between 1973 and 1986, Langan (1991) concludes that changes in state sentencing practices explained just over half of the increase in prison populations during this period. In a similar analysis, Blumstein and Beck (1999) report that 88 percent of the rise in prison populations in the use can be attributed to rises in prison admissions due to sentencing policies, more so than

significant changes in the probability of arrest or conviction. A good deal of ambiguity remains however over whether the effects of sentencing policy on incarcerated populations is the result of changes in judicial propensities to incarcerate offenders or average sentence lengths. This ambiguity stems from the fact that the types of analyses highlighted above analyze aggregate data that do not contain information on cases, which means that some changes in sentencing could plausibly be due to changes over time in the severity of cases police and prosecutors bring into the court system. Nevertheless, empirical and theoretical research provide grounds to suggest that state policy choices may prove a significant source of geographic disparity in the probability of incarceration because they represent a more general shift towards a penal philosophy that emphasizes the goals of deterrence and incapacitation (Raphael, 2009; Stemen et al., 2005; Tonry, 1999).

While there seems to be a consensus that policies help determine the use of imprisonment, disagreement exists over *which* sentencing legislation matters and whether local courts even fully implement legislation. Some argue that the passage of punitive habitual offender laws during the 1990s was largely symbolic, that the implementation of sentencing reform at the local level is often mediated by the local legal culture and political environment, and that the most punitive laws only targeted a small group of repeat violent offenders and should contribute little to jurisdictional differences in the use of prison (Spohn, 2000; Merritt, Fain, and Turner, 2006; Kramer and Ulmer, 2002; Zimring, Hawkins, and Kamin, 2001). Yet, interest in the determinants and consequences of state policy choices remains at the forefront of efforts to explain local, state, and cross-national variability in punishment (e.g., Garland, 2001; Tonry, 1999;

Zimring et al., 2001). For instance, commenting on the adoption of determinate and mandatory sentencing laws, the director of The Sentencing Project observed that “The impact of these sentencing changes on prison populations has been dramatic, and far outweighs any change in crime rates as a contributing factor,” (Mauer, 2001: 6). In a recent exchange on the role of spending and sentencing policy in the expansion of state prison populations, another author similarly concludes that 83% of the increase in incarceration between 1984 and 2002 was attributable to changes in policy, remarking that “...behavior, in terms of variation in crime rates, is a bit player in the story, whereas policy is of first-order importance,” (Raphael, 2009: 91). What is more difficult to discern, and what often gets overlooked in discussions about the impact of reforms, is *how* policy may impact incarceration rates (e.g., by increasing the probability of incarceration, by lengthening sentences, or by removing parole release mechanisms?). Though the precise impacts of policy remain murky, empirical research on mass incarceration reviewed above and the tone of the debate surrounding the adoption of many of these reforms seems to implicitly argue that they have altered state incarceration rates over the last 10 years by *increasing the proportion of all offenders that are sentenced to incarceration*. Following this logic, the presence or absence of several key sentencing policies may signal more punitive legal contexts that increase the risks of incarceration for offenders, independent of defendant and offense characteristics.

Though the goals and content of guidelines vary, they generally function to increase the uniformity and predictability of sentences by requiring judges to mete out similar sentences to similar defendants convicted of similar crimes. Guideline systems achieve this through the form of a sentencing grid, which calculates a presumptive sentence based on scores that are determined by a defendant's prior criminal history and the severity of the conviction offense. Though judges are expected to impose the presumptive term, most states allow judges the discretion to depart from the recommended sentence. While guidelines in some states are merely advisory (e.g., Maryland), other states have instituted presumptive guidelines that typically require judges to document a rationale for departing from the guidelines. This departure decision may then be subject to judicial review. Between 1975 and 2002, nine states adopted presumptive sentencing guidelines (Stemen et al., 2005).

The primary purpose for adopting the guidelines appears to have implications for the severity of the sentences contained in the grid. Five state legislatures have followed Minnesota's model by passing guideline laws that require state sentencing commissions to draft sentencing ranges that pay special consideration to resource management (e.g., prison overcrowding). Other legislatures and the Federal government have placed an emphasis on risk management and sentencing equity when formulating guideline grids (Frase, 2005; Marvell, 1995). This variation in the goals across guideline systems has been cited as one possible factor that accounts for why determinate sentencing laws (many of which accompanied the adoption of guidelines) are associated with substantial decreases in incarceration rates in some states (particularly Washington and Minnesota) (Jacobs and Carmichael, 2001; Marvell and Moody, 1996; Smith, 2004; Stemen et al.,

2005). As others point out though, the use of sentencing policy by legislatures can be motivated by a desire for “get tough” sentencing practices or reforms can be motivated by interests in managing prison populations and justice system resources- thus, the exact effect of guidelines on the severity of punishment is unclear (Stemen et al., 2005).

Regardless, a number of aggregate level and multilevel studies find that the presence of presumptive guidelines is associated with significantly *lower* prison admission rates, slower growth in incarceration rates, and significantly lower likelihoods of being sentenced to prison (Nicholson-Crotty, 2004; Sorensen and Stemen, 2002; Spelman, 2009; Wang and Mears, 2010; Weidner et al., 2005). In sum, empirical evidence from a handful of multilevel and aggregate level studies suggests that: *all else being equal, defendants will face a lower likelihood of incarceration when processed in jurisdictions with guidelines.*

Guidelines may also impact sentencing severity in an indirect manner. For instance, though several studies report that guidelines are associated with lower incarceration rates and more lenient sentencing, the mechanisms by which this occurs is not entirely clear. One possibility is that states with presumptive guidelines crafted guideline grids with presumptive sentence ranges that were either sufficiently narrow or included enough options for alternative sanctions that these states were able to successfully regulate prison admissions and to manage correctional resources; such was the goal in Washington, Minnesota and other states that later passed guidelines (Stemen et al., 2005). However, as Engen (2009) aptly points out, aggregate level studies are unable to test the effects of guidelines in a way that disentangles the effects of reforms from the effects of legally relevant variables on which the guidelines base sentencing.

Thus, multilevel studies that control for the presence of guidelines may not find that guidelines exert any direct influence on sentencing above and beyond the effects of offense severity and prior criminal history (although two of the studies in Table 2.1 do report that guidelines significantly influence the overall severity of sentencing). Though it has not been explored in published research comparing sentencing across states with and without guidelines, this logic suggests that rather than expecting a simple negative relationship between guidelines and sentencing severity, guidelines may actually *increase* the severity of sentencing in an indirect manner by placing greater weight on having a prior criminal history than we would otherwise observe in jurisdictions without guidelines. This leads to the expectation that: *the presence of presumptive guidelines will significantly increase the odds of incarceration by strengthening the magnitude of the positive association between prior criminal history and sentencing severity.* Put another way, the odds of incarceration will be even higher for defendants with a prior criminal history when sentenced in guideline jurisdictions.

Determinate Sentencing and Truth in Sentencing Requirements (TIS)

Determinate sentencing describes sentencing structures that are designed to ensure that judges control or “determine” the actual amount of time offenders will serve in an effort to make punishment more certain and predictable. Under determinate systems, state legislatures control judicial discretion by removing broad sentencing ranges and replacing them with narrow or fixed sentence terms. Judges are expected to impose these terms, and offenders are released from prison at the completion of the term the judge imposes, minus any “good time” sentence reduction credits. Some states, such

as Virginia, have also abolished parole in conjunction with adopting presumptive sentences. Under indeterminate systems, judges typically choose from a broad range of sentencing options and parole boards determine the amount of time offenders serve, rather than the sentence imposed by the judge. Since the 1970s, seventeen states have adopted determinate sentencing, though not all of these states abolished parole release (McCoy, 1984; Shepherd, 2002; Stemen et al., 2005).

Like determinate sentencing, truth in sentencing (TIS) provisions require offenders to serve a certain proportion of their sentence. TIS provisions typically require offenders to serve at least three-fourths of their fixed or minimum sentence. Most states have adopted these requirements, which have allowed indeterminate states to still exercise control over the amount of time that offenders serve while maintaining parole release (Sabol, Rosich, Mallik-Kane, Kirk, and Dubin, 2002; Stemen et al., 2005). States vary widely though in their willingness to embrace TIS requirements. As of 2002, the percentage of sentences offenders must serve before they become eligible for parole ranged from as low as 33% in Alabama to 100% in Michigan (Stemen et al., 2005). Truth-in-sentencing requirements have undergone some significant changes in the last thirty years. For instance, in 1975 state offenders were required to serve an average of 70% of their minimum or fixed sentence. By 2002, this average had risen to 93% (Stemen et al., 2005).

Some argue that determinant sentencing structures and TIS provisions should increase incarceration rates through their impact on release decisions rather than any impact they may have on sentencing behavior (Stemen et al., 2005; Stemen and Rengifo, 2011). But there are several reasons to expect that the adoption of determinate

sentencing or time-served requirements may significantly increase or decrease the probability of incarceration independent of case-level characteristics. First, led by California, TIS and DSL came about as part of a broader push away from the rehabilitative ideal and toward crafting laws that promoted the goals of deterrence and incapacitation. Thus, these structural changes to state legal systems have been viewed as evidence of a more punitive legal culture generally (Mauer, 2001; Shepherd, 2002). Sabol and colleagues (2002) evaluated the impact of TIS reforms in the 1990s and concluded that by giving judges greater control over sentencing outcomes, the TIS model may increase the use of incarceration because “judges are in a better position to evaluate the effects of their decisions.” The authors found that in several states TIS reforms were associated with an increase in the proportion of felons sentenced to prison because “judges are in a position to expand the use of prison for more violent offenders, achieve some incapacitation or just deserts effects- while not changing appreciably the amount of time that violent offenders are required to serve, “ (2002: 11). Consistent with this argument, one study finds that the immediate adoption of the DSL contributed to a significant increase in the number of prison admissions in California (McCoy, 1984).

However, another possibility is that judges may be significantly *less* likely to impose incarceration for some offenders under DSL and TIS systems out of concern over the length of time that offenders are certain to serve on those sentences. A number of aggregate level studies find that support for this latter hypothesis, and report incarceration and prison admission rates are significantly lower in most states that have adopted determinate sentencing (Greenberg and West, 2001; Jacobs and Carmichael, 2002; Marvell and Moody, 1996; Smith, 2004; Stemen et al., 2005; Stemen and Rengifo, 2011;

Zhang et al., 2009). Consistent with this logic, the one published study that has examined the effects of varying time-served requirements on incarceration rates, found that states who require prisoners to serve most or all of their sentences have lower incarceration rates, although the effect is not significant once controlling for the presence of other sentencing policies (Stemen et al., 2005). Previous multilevel studies have not examined the effects of TIS or DSL on the odds of incarceration. On the whole though, several evaluations of the effects of DSL/TIS adoption and empirical research on state incarceration rates suggest that: *DSL and TIS time-served policies should exert important influences on judges' willingness to incarcerate offenders, but the direction of the relationship may be positive or negative.*

Three-Strikes Laws

The policies described above represent efforts by states to alter the *procedural* aspects of sentencing and release decisions (Stemen et al., 2005). States have also passed a number of policies in the past thirty years that impact the *substance* of penal law by targeting certain offenders and offenses for more punitive sanctions. Among the more controversial of these reforms is habitual offender legislation. Though habitual offender laws that increase punishment for repeat offenders have been in existence for decades, the concept was repackaged in the 1990s as “three-strikes and you’re out” (Clark, Austin, and Henry, 1997). These laws were first passed by public ballot initiative in California, Washington and Oregon between 1993 and 1994. Soon after, twenty-four states passed similar laws and mandated incarceration for third-time felony offenders. These laws were designed to not only incarcerate third-time offenders, but to do so for a substantially

longer period of time (typically at least twenty-five years). States vary substantially in their definition of a “strikeable” offense, how long in the past a prior offense must have occurred, and whether three-strikes laws mandate that judges impose incarceration if incarceration was not otherwise required for the current conviction (Stemen et al., 2005). The diversity of these laws makes it difficult to assess the effects of these provisions, but some evidence suggests that many states simply passed these laws as a symbolic gesture to appear tough on crime, and that only Georgia and California sentence offenders under three-strikes provisions with any regularity (Chen, 2002; Schultz, 2000; Zimring et al., 2001). Despite the relative irregularity with which courts actually use three-strikes laws, sentiment among legal observers was that three-strikes laws signaled a broader populist wave towards more punitive crime control that would increase the number of individuals sentenced to prison and the length of time they would serve (Zimring et al., 2001). For example, in the year California voters passed their three-strikes law, Austin (1994) cautioned that the law would increase the demand for jury trials, reduce the rate of pretrial bail release, delay court processing, and increase the number of prisoners sentenced to prison and the length of time they serve. Sutton elaborates on a more nuanced argument put forth by Feeley and Kamim (1996), who suggested that three-strikes laws are in reality an example of “symbolic crusades” that initially motivate lawmakers and judges to enthusiastically “swim with the political tide” and then following a period of uncertainty about how to absorb the impacts of the law, result in efforts by local authorities to use their discretion to undermine the intent of the law (Sutton, 2010). Thus, while three-strikes provisions may be expected to increase the odds of incarceration over and above what would be expected in non-three strikes

jurisdictions, another intriguing possibility that has not yet been examined with case-level data is that three-strikes provisions may provoke a “push back” effect on the part of local county courts who may either not agree with the intent of the law or who may feel that the law undermines the efficient processing of cases (i.e., obtaining guilty pleas).

Research on the implementation and impact of these laws supports this proposition (Austin, Clark, Hardyman, and Henry, 1999). A handful of aggregate level studies have assessed these claims. On the whole, these studies do not find that three-strikes laws are associated with significantly higher incarceration rates, and that in some instances they are actually associated with a significant decline in state incarceration rates (Spelman, 2009; Stemen et al., 2005). In fact, one study that examines the rate of new court commitments finds that three-strikes provisions (at least in a cross-sectional context) are associated with significantly *lower* rates of new court commitments to state prisons (Zhang et al., 2009). *The current study examines the possibility for three-strikes laws to impact the overall propensity to mete out incarceration, but the direction of this relationship may be positive or negative.*

No multilevel studies have yet compared the probabilities of imprisonment for defendants processed in jurisdictions with and without three-strikes laws. However, one study has used the SCPS to examine the impact of three-strikes in California on the processing of felony cases before and after the passage of the law. The author found that the law impacted average sentence lengths primarily by increasing the importance of having a prior criminal history, but had a negligible impact on the odds of incarceration (Sutton, 2010). Though this study did not find evidence of a significant interaction between three-strikes and prior criminal history in California for the decision of whether

to incarcerate, the intent of the law theoretically suggests that: *three-strikes laws may indirectly increase the probability of imprisonment by strengthening the magnitude of the effect of having a prior criminal history.*

Mandatory Minimums

Another controversial sentencing policy to emerge in the last thirty years is mandatory minimum or “enhancement” penalties. These laws, perhaps more so than any other recent reform, have been criticized as contributing substantially to the growth in state and federal incarceration rates (Barker, 2006; Beckett and Sasson, 2000; Stemen et al., 2005). These laws not only impact sentencing procedure by removing judicial discretion altogether, but they also alter the substance of criminal law by targeting certain types of offenses for more punitive sanctions. Between 1975 and 2002 every state adopted some form of mandatory minimums. The breadth of offenses targeted for mandatory enhancements varies substantially across states, but a few popular targets for these laws are offenses involving weapons (especially guns), offenses committed near schools, crimes against protected populations such as minors and members of certain race, sex, and religious groups, sex offenses, and any crime involving aggravated bodily harm (Stemen et al., 2005). In most states that have adopted these laws, a “triggering” offense requires a judge to impose a mandatory term of incarceration and/or requires the judge to impose a lengthier prison term.

Though these laws have received considerable attention, their effects on the probability of prison and on imprisonment rates are not frequently addressed in empirical studies of sentencing or in the macro level research on incarceration. In one study to

examine the effects that mandatory enhancements have on incarceration rates, Stemen and colleagues (2005) find that the number of mandatory enhancements that are triggered by weapon use, offenses against protected victims, and offenses committed while under state supervision are associated with significant growth in state incarceration rates, though the precise mechanism that explains this effect (i.e., prison admissions versus sentence lengths) remains unclear. In their conclusion, the authors of the study suggest that “we do not believe that these specific laws are leading to increased incarceration rates...Rather, these laws likely act as a proxy for a state’s overall use of mandatory sentencing policies.” (Stemen et al., 2005: 125). The fact that most states mandate incarceration for offenses that trigger mandatory penalties leads to the expectation that: *the odds of incarceration will be higher in jurisdictions with a larger number of mandatory minimum provisions.*

Punitive Drug Laws

Several observers have argued that a major factor driving growth in incarceration is the passage of more punitive drug penalties, which are believed to have sent a larger proportion of low-level drug offenders behind bars (Blumstein and Beck, 1999). Arrests of drug offenders tripled between 1980 and 2001, but the increase in imprisonment (1,195%) suggests that the war on drugs drove increases in incarceration rates primarily by increasing the proportion of offenders who were sentenced to jail or prison (Harrison and Beck, 2005). Given the significance of this shift, the current study also examines the impact of state variability in drug penalties on the probability of incarceration.

States have passed more complex and more punitive drug laws, beginning perhaps most notoriously with New York's Rockefeller drug laws. Passed in the 1970s, these laws were noteworthy for their strict minimum penalties for possessions of small amounts of drugs, and because the laws were accompanied by a ban on plea-bargaining. Many states have since adjusted their statutory penalties for drug crimes to include alternative sanctions such as drug courts that mandate treatment. Other states have maintained relatively severe penalties for the sale and possession of drugs, with some of these states also incorporating mandatory minimums based on quantity and where drugs are bought and sold. Such penalties have grown increasingly severe over time. For instance, Stemen et al. (2005) document that minimum sentences for possession of 1 ounce of cocaine averaged 13 months in 1975. By 2002, state statutes indicated that the average minimum sentence for possession of 1 ounce of cocaine increased to 28 months (a 115% increase) (Stemen et al., 2005: 22). Though there are no published studies which have examined how state variation in the severity of drug laws impacts the sentencing of drug offenders, there is some empirical evidence to suggest that states with higher minimum sentences for cocaine possession and sales and more mandatory minimum provisions for drug offenders, have experienced significant growth in their incarceration rates over the past thirty years (Stemen et al., 2005). It is not clear from these types of aggregate studies however if more punitive drug laws drive incarceration rates because they indicate a more punitive legal culture in general, or whether they actually contribute to an increased probability of imprisonment for drug offenders specifically. The current study examines this hypothesis with the expectation that: *drug offenders sentenced in*

jurisdictions with more punitive drug laws can expect a higher likelihood of being incarcerated upon conviction.

ORGANIZATIONAL EFFICIENCY

The goals of sentencing policy are often mediated by pressures to efficiently process cases and must be balanced by the costs of punishment. With respect to these issues, organizational and court community perspectives have greatly advanced our understanding of the relationship between court social contexts and sentencing dispositions (e.g, Dixon, 1995; Eisenstein et al., 1988; Ulmer, 1997). These perspectives highlight that the costs incurred by punishment, case processing efficiency, and local legal cultures that define the ‘going rates’ of punishment all impact sentencing dispositions. Although a comprehensive assessment of courtroom legal cultures across U.S. counties is not presently possible, important developments in the study of incarceration point to the importance of examining differences across jurisdictions in fiscal resources and offsets that may override the desires of communities and politicians to respond more harshly to crime, and may foster a local court room culture more open to alternatives to prison (Spelman, 2009). According to economic perspectives and deterrence theory, high conviction rates signal the certainty of punishment, prison crowding and corrections budgets signal the burdens of it, and attempts to offset the costs of expanding prison populations may be tied to changes over time in the use of probation. The present study examines the ways in which prison capacities exert independent influences on criminal case dispositions. In addition, I go beyond prior research by investigating the incentives *against* imprisonment by examining state differences in

corrections spending. Additionally, I investigate how state and local probation laws dictating the amount of supervision costs that may be charged to probationers may significantly alter the appeal of sentencing individuals to prison. These last two organizational attributes have not received explicit attention in prior multistate studies, but they may provide important clues as to why some jurisdictions send significantly fewer offenders to prison.

Physical and Fiscal Capacity to Incarcerate

Scholars have interpreted the relationship between bureaucratic arrangements and sentencing outcomes using economic/deterrence models of punishment and the court community theory of sentencing (Eisenstein and Jacob, 1977; Eisenstein et al., 1988; Ulmer, 1997). Both of these argue that sentencing outcomes will reflect interests to effectively manage the punishment process with minimal cost and greater efficiency. Economic perspectives of social control argue that crime control efforts will reflect an instrumental response to levels of crime across all jurisdictions, but that the severity of punishment can only be understood in relation to its certainty and costs (Chamlin and Langworthy, 1996; Cooney and Burt, 2008; Liska, 1992). Jurisdictional variation in the severity of sentencing then, is believed reflect a balance between the goals of deterring future offending and the costs imposed on the system by relying on harsh measures aimed at deterring crime. One important factor that may mediate the punitive-oriented goals of sentencing policies are the fiscal capacities to punish, the physical opportunities to punish in the form of prison and jail space, and the presence of incentives that help offset the burden of these costs. The capacity to punish has become so important that several states

have crafted sentencing schemes with the purpose of providing ‘cost-effective’ sanctions and assisting the state with avoiding prison overcrowding (Kauder and Ostrum, 2008).

Washington’s sentencing commission for example, states that a central goal of their guidelines is to sanction offenders by making “frugal use of the state’s and local government’s resources,” (Kauder and Ostrum, 2008: 26).

The key resources that may constrain the ability of states and local courts to impose more punitive sanctions are state spending on corrections and the availability of prison and jail space. A number of studies have explored the effects of jail or prison crowding on sentencing, but measures of crowding provide an incomplete portrait of the constraints facing state and local correctional systems because capacity measures reported by states and local governments do not capture the volume of prisoners that are in private prisons or housed in other states due to overcrowding. A more accurate picture of the full burden states bear to incarcerate offenders is corrections spending. As Spelman explains, “as we fill prisons beyond capacity or farm prisoners out to other states or institutions, we incur both the higher financial costs (increased transportation costs, profit, an inconvenience premium) and higher inchoate political and administrative costs (these prisoners remain our responsibility, but we cannot control and take care of them directly)” (2009: 43). One implication of these arguments is that as the costs of punishment increase, state and local governments will respond with less severe sentencing to help manage the expense of housing prisoners as well as the space to do so.

While there is some evidence to suggest this is true (Spelman, 2009), the economic model of punishment has received less attention in the multilevel sentencing literature. Only one existing multi-state contextual study of sentencing has assessed the

degree to which judges' propensity to incarcerate is shaped by state prison capacities and crowding (Wang and Mears, 2010), despite evidence from Pennsylvania which suggests that defendants face a significantly greater likelihood of confinement when sentenced in counties with greater jail space (Johnson, 2006; Ulmer and Johnson, 2004). In one metro county court in Pennsylvania, one judge explained the rationale for using jail overcrowding to depart from the sentencing guidelines for less serious offenders: "A retail theft or marijuana possession or something, I'm not going to put that person in jail. I say, 'Look, you might deserve to be there, but I don't have the space to waste on you'" (Ulmer, 1997: 87). This leads to the expectation that: *all else equal, the odds of incarceration will be lower in jurisdictions experiencing higher rates of prison and/or jail overcrowding,*

And also that:

All else equal, the odds of incarceration will be lower in jurisdictions within states that spend more money per capita on corrections.

Other factors besides prison crowding and corrections costs may provide incentives against the use of incarceration. Garland (2001: 177) points out that probation and parole have been refashioned in recent years to resemble "real" punishment instead of a means of rehabilitation and reintegration. Though a number of studies have examined this transformation of probation and parole, there is little research on how probation policies influence the decision to incarcerate. There are potentially important reasons to consider how community differences in probation policies may impact a jurisdiction's reliance on incarceration. One way that lawmakers can appear tough on crime while balancing the costs associated with incarceration is to expand the rationale

for probation. In other words, probation may serve as an organizational safety valve that helps to regulate prison populations.

In a climate of finite resources, some states and counties may offset the costs of punishment by charging offenders for their probation supervision (Diller, 2010; Ring, 1989; Wheeler et al., 1989). Probation has not only had to expand to include more serious offenders, but it has been reconfigured as a community-based extension of the prison (Bannon, Nagrecha, and Diller, 2010). Both adult and juvenile probation monitoring now frequently includes more intrusive measures such as electronic monitoring and drug testing, and operates under the goals of calculating and containing risk (Garland, 2001: 177). These all come at great cost to the state; costs the government may offset by charging felons a monthly supervision fee for the duration of their supervision (Morris and Tonry, 1990). All but four of the states included in the current study charge probationers for their supervision, a fee that varies from ten dollars in Washington to seventy-six dollars a month in Florida. In the state of Florida, where probationers are fully responsible for the costs of their supervision and nonpayment is considered grounds for revocation of parole or probation, supervision fees alone generated nearly 26 million dollars in revenue during the 2007-2008 fiscal year (Bannon et al., 2010; Diller, 2010; Florida Department of Corrections, 2009: 14).⁵ Similar to practices in other states, Georgia's probation program is supported by the supervision fees charged to all probationers (Morris and Tonry, 1990: 133-134). Although most

⁵ Most states are reluctant to revoke probation or parole for nonpayment of fees. The constitutionality for such punishment based on income is also questionable. In response to the legal concerns associated with tying punishments to socioeconomic status, Florida was among the first states to require payment of supervision fees; in lieu of revoking probation for nonpayment, the state allows probationers to complete their term of supervision with the requirement that they eventually pay all fees incurred during their supervision. Similar to income tax debt, probation fee debt will not be terminated until the probationer remits payment or proves their inability to pay.

states do not consider the inability to pay fees a probation violation, Morris and Tonry claim that the mere prospect that these fees can fuel the support and expansion of probation services provides a powerful incentive to collect such revenue that is not otherwise immediately available to a correctional system that relies heavily on imprisonment (although some states also charge parole supervision fees and debt incurred for health services). In sum, if states can finance probation in more creative ways and reduce the amount of non-violent offenders imprisoned, then defendants can be expected to face significantly lower odds of imprisonment. Although no prior research has examined the relationship between state efforts to offset the costs of supervision and sentencing decisions, according to the economic model we should expect that states and counties that charge a higher monthly probation supervision fee have a greater incentive to sentence defendants to some term of probation because they can offset more punitive methods of probation with costs recovered through probation fees.⁶ Thus, I expect that *defendants convicted in jurisdictions that charge a higher fee for probation supervision will, on average, be significantly less likely to receive a prison sentence.*

Caseload Pressures

The ability to obtain convictions and dispose of cases is influenced by the workload pressure placed on courts. Research by Eisenstein and colleagues found that higher caseloads provide incentives to plea bargain, which in turn, reduces the severity of sentencing (Eisenstein et al., 1988). Research in Federal districts and in Pennsylvania courts documents that defendants are significantly more likely to receive downward

⁶ In all but two of the states in the current study, the state controls probation supervision and sets a standard supervision fee. In New York and California, probation is organized at the county level, and local governments determine the supervision fees.

departures in jurisdictions where there is greater pressure to process a higher volume of cases (i.e., more cases per judge). However, sentencing studies that control for whether defendants plea bargain contradict these findings, demonstrating a higher likelihood of incarceration for defendants processed in courts with higher caseload pressure (Ulmer et al., 2008). Thus, while theory suggests that high caseloads will reduce the severity of sentencing indirectly by increasing conviction rates, the evidence from studies that control for plea bargaining rates across counties suggests the opposite. Thus, I expect that that: *caseload pressures will significantly influence the severity of sentencing, though the direction of this relationship may be positive or negative.*

COMMUNITY CONTEXT:

SOCIAL THREAT AND CONFLICT

Presently, conflict theory is the dominant framework guiding the literature on the social structure of legal sanctioning. Departing from the consensus-based portrayal of punishment as a social ritual carried out by the government for the collective good, conflict theorists maintain that legal sanctioning is a coercive tool carried out by the state to further the interests of the powerful at the expense of subordinate groups (Chambliss and Seidman, 1971). Classic neo-Marxist conflict focused on the threat to capitalist mode of production posed by surplus labor, stimulating a large body of research linking levels of unemployment to general imprisonment rates and the sentencing of individual defendants (Rusche, 1978). While economic interests remain at the forefront of conflict theory, competition for resources intersects with racial and political divisions. The social

threat hypothesis is a specific proposition drawn from conflict theory, which predicts that the severity of punishment will be greater against members of groups perceived as either a criminal or economic threat to 'elites.' Although elites were traditionally defined in Marxist terms as the government and property owners, scholars now recognize the middle class as an 'elite' demographic given that they are the least insulated from threatening groups and street crime, are powerful enough to politically mobilize and communicate their interests, and are more often employed as criminal justice actors with the authority to use the justice system to their advantage (Liska, 1993; Sampson and Laub, 1993). In the conflict tradition, the poor and racial and ethnic minorities are believed to represent threats to white, as well as upper and middle- class groups. According to the threat hypothesis, the relative size of these threatening groups should be positively associated with more severe sanctions levied against poor, Black, and Hispanic defendants (Chambliss and Seidman, 1971; Blalock, 1967; Liska, 1993).

Increasingly though, scholars now include the size of the poor, Black, and Hispanic population in studies that examine incarceration rates and sentencing decisions for offenders from both majority and subordinate groups, inferring that the presence of threatening populations should be expected to increase the severe sanctioning of all crime, primarily in an indirect way by increasing levels of fear and insecurity (Liska, 1992). Nearly every aggregate or multilevel study of penal sanctioning now includes measures unemployment, income inequality, and racial composition and these affects are almost always interpreted as the evidence or absence of threatening conditions. Yet from a theoretical standpoint, it is unclear why judges in communities with larger poor and minority populations would punish defendants from threatening and non-threatening

groups with equal severity. I describe several reasons why these conditions may impact sentencing behavior below, and ways in which we might begin to resolve conflicting interpretations about the affects of racial and ethnic composition on sentencing.

Unemployment and Economic Inequality

Economic threat models of punishment generally posit that during periods of economic prosperity, punishment is less necessary. In ‘Punishment and Social Structure’ (1939), Rusche and Kirchheimer provide one of the first explanations of the ways in which structural conditions are associated with distinct styles of punishment. They argue that in capitalist societies the frequency of imprisonment will be positively related to the changing size of ‘surplus labor’.⁷ During periods of labor surplus, the value of labor is diminished and the growing ranks of unemployed are perceived believed to pose a threat that is neutralized through a greater use of incarceration (Myers, 1993; Quinney, 1977). Motivated to reassert state legitimacy amidst declining prosperity, capitalist governments are even further motivated to punish workers so as to counteract the public fear that workers are redundant and economic advancement illusory (Chiricos and Delone, 1992). Thus, adverse economic circumstances are believed to form the perception that punishment becomes necessary in order to quell unrest and panic among the unemployed, and to reassert the legitimacy of the capitalist political economy.

Building on this early neo-Marxist formulation of the economy-punishment relationship, scholars have refined the bases for a link between unemployment and incarceration to characterize the threat of the unemployed in symbolic and cultural terms,

⁷ The Rusche-Kirchheimer hypothesis was originally formulated to explain historical trends in incarceration. Increasingly however, the proposition that unemployment is positively related to incarceration features prominently in cross-sectional studies of sentencing and incarceration.

and support for the position that the value of labor determines the instrumental use of incarceration has declined in favor of a general social threat hypothesis of formal social control (Melossi, 2003). Extending Rusche and Kirchheimer's (1939) work, Spitzer (1975) argues that high unemployment is related to greater coercive state because it produces two problem populations: those who are unproductive and unwilling to work but are otherwise harmless to elites; and second, those who are alienated by the conditions of underclass life (e.g., poor black residents) and are perceived as more likely to question their economic oppression (e.g., young males), a group Spitzer terms 'social dynamite.' Other scholars argue that economic threat is best captured by evaluating the gap between the rich and the poor. For instance, Chambliss and Seidman assert that "The more economically stratified a society becomes, the more it becomes necessary for dominant groups to enforce through coercion the norms of conduct that guarantee their supremacy," (1971:33; quoted in Jacobs and Carmichael, 2002). To summarize, the thrust of these neo-Marxist perspectives suggests that unemployment and income inequality trigger more punitive sentencing practices because they threaten the economic interests of upper income groups.

Another theoretical basis for the unemployment-punishment link is the perceived *criminal* threats posed by larger poor populations rather than threats to the economic hegemony of the upper class. Sampson and Laub (1993) for example, point out that the poor often do not come into contact with the rich, or their property. Thus, the threats posed by the poor are largely symbolic and signify a growing cultural divide between the poor and the middle and upper classes. According to this view, unemployment and poverty is believed to elicit more punitive sentencing because of ideological assumptions

that these conditions foster crime (Box, 1987), that the poor are subject to criminal temptation, and that prison is the only successful deterrent to economically motivated offending (Chiricos and Delone, 1992). Thus, unemployment is believed to shape sanctioning severity due to a prevailing cultural assumption that economic strain causes an increase in threatening acts (crime), and a growth in the size of threatening groups (the criminally-prone poor). Greenberg (1977) ties this logic to judicial decision-making to suggest that economic conditions will shape judicial assessments about the risks of recidivism. For instance, assumptions about the role of employment and crime risk have been codified in legal practices in Washington and North Carolina, where judges are permitted to consider defendant employment status (Greenberg & West, 2001), perceptions which may be shaped by larger aggregate conditions (e.g., Myers, 1993, 1987). To the extent that the poor make up a disproportionate share of those arrested and facing incarceration, adverse economic circumstances are expected to be positively associated with sentencing severity. Regardless of the economic status of suspects though, Greenberg asserts that economic insecurity is predicted to lower tolerance of *all* crime, primarily because economic uncertainty fosters fear and insecurity.

In conclusion, social threat explanations argue the use of incarceration is a response to unemployment and income inequality. In theory, these conditions 1) are perceived as threatening to elites' superior economic position, and/or 2) higher levels of unemployment and inequality affect the severity of sentencing indirectly because these conditions are believed to heighten fear and the intolerance of crime. These hypothesized affects are expected to persist after adjusting for levels of crime. Scholars continue to disagree on the causal connections between unemployment and punishment, and whether

unemployment and inequality should be expected to increase risks of imprisonment for *all* defendants, regardless of socioeconomic status. Notwithstanding these inconsistencies, the guiding assumption is that in the context of economic strain and uncertainty courts will intensify punishment in order to neutralize threats to middle and upper classes interests (Myers, 1993). The results in the expectations that:

Sentencing outcomes be more severe in communities experiencing relatively higher rates of unemployment and/or economic inequality; and that levels of fear should help account for a portion of the relationship between economic strain and sentencing severity.

Researchers have evaluated the validity of economic explanations of punishment using several indicators of economic conditions that range from unemployment rates, poverty, income inequality, and indices of socio-economic disadvantage. Generally, this perspective has received little consistent support in the contextual sentencing literature (e.g., see Table 2.1)

Linking Racial and Ethnic Composition to the Severity of Punishment

A large portion of the literature on sentencing and incarceration is concerned with modeling the ways in which the police and court decision making respond to minority-majority group relations. Social threat explanations argue that compared to white and upper income defendants, minority defendants are subject to more severe legal sanctions in an effort to insulate majority group members from potential threats to the existing social and economic hierarchy that benefits middle and upper class whites (Blalock, 1967). The degree to which majority group members perceive blacks and Hispanics as

threatening is believed to be strongly correlated with the size of their representation in the community. According to the social threat hypothesis, a greater presence of Blacks and Hispanics is expected to be met with greater attention from police and more severe legal sanctioning based on the rationale that a greater presence of minority groups cultivate perceptions of vulnerability and threat among majority group members (Blalock, 1967).

However, scholars frequently apply this reasoning to the sentencing and social control of the general population, and there is evidence that the severity of punishment in general is related to the size of racial and ethnic groups in the community. For instance, the results summarized in Table 2.1 suggest that among the more consistent predictors of more punitive sentencing is the relative size of the non-white population (Helms, 2009; King et al., 2010; Myers, 1987; Myers and Talarico, 1987; Weidner et al., 2005).

What explains this relationship between the racial makeup of the population and geographical and defendant level disparities in punishment severity? Due in large part to data restrictions limiting scholars to the use of Census data, we can only presume to know the meaning between racial composition and decisions regarding appropriate sanctions for individual felony defendants. This project goes beyond previous examinations of this issue to include more proximate measures of several processes hypothesized in the literature to mediate the link between racial composition and the intensity of formal social control. Two possible mechanisms can be gleaned from the literature on social disorder and fear (Sampson & Raudenbush, 2004) and social/minority threat theory (Blalock, 1967; Liska, 1992). Specifically, the disorder literature suggests that greater concentrations of racial minorities and the poor heighten public perceptions (among both blacks and whites) of disorder and crime, which may produce more punitive sanctions

towards *all* defendants through the more proximate mechanisms of fear or support for punitive sanctions. Thus, I expect that: *(a) the odds of incarceration will be higher in jurisdictions with a larger Black and Hispanic populations, but also that (b) racial and ethnic composition may also influence sentencing in an indirect manner through their impact on local levels of fear.*

NEO-DURKHEIMIAN PERSPECTIVES:

COLLECTIVE SENTIMENT AND SOCIAL COHESION

Durkheim's view of punishment as a reflection of moral consensus and the maintenance of social cohesion remains an influential force in contemporary studies of law and punishment (e.g., Garland, 2001). In its most basic sense, a neo-Durkheimian perspective on punishment argues that the community's reaction to crime flows from the basic moral and ideological orientation of its residents. Though Durkheim originally formulated a theory describing the functions of punishment more so than severity of it, his work has been influential for its suggestion that like crime, punishment can be viewed as expression of "collective sentiment" such that punishment's "real function is to maintain inviolate the cohesion of society by sustaining the common consciousness of all its vigor," (Durkheim, 1893, quoted in Cotterrell, 1991: 924). The assumption that public sentiment influences the legal process implies that in communities where cultural sentiments and norms support for punitive justice responses should be associated with more retributive or repressive forms of social control. According to Garland, the views of the public enter into the legal system in an "oblique" fashion by providing "a general

context of support for the laws and institutions which exist.....and they are referred to as a proper ‘consideration’ in the formation of policy and in individual sentencing decisions” (1990: 66). This conceptualization of punishment may be particularly relevant in the U.S. legal system because there is greater pressure to translate public sentiment into decision-making, and much more opportunities to do so either through jury participation or by pressuring the choices made by popularly elected prosecutors and judges (Salvesberg, 1992).

Attention to the role that the public plays in criminal sanctioning has prompted a stream of research concerned with identifying the sources of individual attitudes about crime and punishment. This research forms the basis for several of the hypotheses examined in this study that predict that public sentiment and levels of interpersonal trust exert moderate but important influences on how courts come to define appropriate “going rates” of punishment (Eisenstein et al., 1988). Below, I describe research identifying the social sources of support for punitive justice. If the community “consensus” is consistent with a punitive social climate then we should expect, all else equal, that defendants processed in such punitive social climates face a greater odds of incarceration than defendants processed elsewhere. Common examples of this highlighted in the literature are religious conservatism, political conservatism, social trust, racial resentment, and support for repressive measures of social control. The rationale for incorporating these types of mechanisms into the study of sentencing is rooted in research indicating that while the legislature defines the *boundaries* of sanctioning authority, it is the public and political sphere within which judges and prosecutors develop norms about appropriate reactions to crime (Eisenstein & Jacob, 1977). Assumptions about the ability of local

cultural and political climate to shape legal outcomes is consistent with contemporary theories of legal decision making which suggest that legal actors operate within a context of bounded rationality and base sentencing decisions on perceptions of risk, blameworthiness and organizational constraints that may reflect values and concerns of the local climate (Albonetti, 1991; Steffensmeier et al., 1998). Specific elements of local collective sentiments, and their hypothesized effects are detailed below.

Punitive Attitudes

Although it has received little attention in the literature on jurisdictional variation in criminal case processing, it is reasonable to expect that community variation in levels of support for punitive justice may have implications for jurisdictional variation in the types of sanctions applied to felony defendants. Building off of these ideas, much of the early American work in formal social control focused on the honor culture in the South believed responsible for support for retributive justice as a response to norm violations (Vidmar, 2002). There are a number of reasons why researchers have assumed that southern communities are more punitive. Survey research suggests that southerners are more likely to support harsh sanctions such as the death penalty, exhibit lower levels of social capital and trust, that southerners exhibit higher levels of religious and political conservatism, and because traditional forms of prejudice have been higher in the South. Each of these social characteristics has been linked to more punitive attitudes, which in the aggregate, should indicate greater support (or demand) for harsh sanctions on the part of criminal justice authorities.

Religious Fundamentalism

Because of religion's centrality to Durkheim's view that values and morals undergird the punishment styles of society, sociologists have long been captivated by the relationship between punitive mentalities and religious beliefs. The connection between religious faith and punitive attitudes may be particularly important for understanding American penology given the "persistence of religion in American society" and the powerful force of religion in politics (Applegate, Cullen, Fisher, and Vander Ven, 2000: 722; Grasmick et al., 1993). A long line of survey research has established a consistent, but complex relationship between individuals' adherence to fundamentalist faiths and more punitive orientations towards crime and rule breaking (Cook and Powell, 2003; Unnever and Cullen, 2006). A number of studies report that fundamentalist Christians are significantly more likely to support the death penalty, more punitive criminal justice legislation, harsher local courts, and more support for retributive penal philosophies (Borg, 1997; Britt, 1996; Grasmick et al., 1992, 1993; Unnever et al., 2005; Young, 1992). Some of these studies suggest that the key mechanism linking adherence to a fundamentalist faith to more punitive attitudes is the tendency toward biblical literalism (i.e., belief that the Bible is the literal word of God), which tends to be more prevalent in the South (Borg, 1997).

Other research though suggests a more complex relationship, and that some of these aforementioned studies overemphasize the authoritarian aspects of Christian fundamentalism. Applegate et al. (2000) find that fundamentalists may adhere more strongly to retributive punitive orientations, but that aspects of their faith also promote support for rehabilitation and a belief in forgiveness. Arguing that previous research

presents a “distorted” picture of religion by ignoring the more compassionate side of religion, the authors conclude that “one possible source of the tenacity of rehabilitation as a correctional ideology is the Judeo-Christian belief in forgiveness, and relatedly, the belief that sinners of all sorts can be transformed.” (Applegate et al., 2000: 741). In summary, the research on religion and punitive attitudes suggests that Christian fundamentalists exhibit strong views on punishment, although the nature of the relationship is complex. According to the most recent and comprehensive studies on the matter, the nature of the relationship between punitive ideology and religious fundamentalism turns on whether fundamentalists view God as loving and forgiving, and whether those who adhere to a fundamentalist doctrine interpret the Bible literally (Applegate et al., 2000; Unnever et al., 2005). Though there are some exceptions in the literature, as a whole research suggests that for jurisdictions in which a higher proportion of residents adhere to a Christian fundamentalist doctrine, support for harsh sentencing should be greater. This yields the expectation that: *all else equal, sentencing will be more punitive in jurisdictions that contain a larger population of residents who identify as Christian fundamentalists.*

Conservative Political Climate, Support for the Death Penalty, and Racial Prejudice

Political climate is the most consistently cited feature of communities expected to impact legal decision-making and aggregate differences in incarceration, pretrial bail and detention, and application of the death penalty (Beckett & Sasson, 2000; Eisenstein & Jacob, 1977; Garland, 2001; Greenberg & West, 2001; Helms & Jacobs, 2002; Huang et al., 1996; Jacobs & Carmichael, 2001, 2002; Smith, 2004; Ulmer & Johnson, 2004).

Research in this vein tends to focus on two aspects of the political climate: the effects of political *structures* and the effects of political *sentiment* or ideology on sanctioning.

There are several theoretical reasons to expect that conservative ideology might shape jurisdictional differences in the application of felony pretrial and sentencing sanctions.

Beckett and Sasson (2000) view the effects of politics in cultural terms, where conservative efforts to politicize the crime problem shifted public discourse away from the social sources of crime, effectively reframing the issue in terms of individual choices. Specifically, the authors make a compelling case that beginning in the 1964 Presidential election conservative Republicans have included crime control and “get tough” rhetoric as a cornerstone of their political platform. This platform called for penalties harsh enough to deter would be criminals from “choosing” violence, drugs and poverty, tacitly depicting the issue in urban, racialized terms (Beckett & Sasson, 2000; Tonry, 1995).

While provocative, this claim is not without merit. In a recent study of the link between racial prejudice, conservatism, and punitiveness, Chiricos et al. (2004) report findings demonstrating that among *conservatives*, net of respondent gender, fear of crime, and racial prejudice (all of which were reduced to non-significance for conservatives in the sample), it is perceptions of crime as a *black* phenomenon that most strongly predict support for harsher penal sanctions.

Consistent with assertions that conservatives support harsh sanctions, research has also shown that conservatives are more likely to support the use of the death penalty for murderers, which is a leading indicator of more punitive attitudes. According to the Gallup Poll and the General Social Survey, support for the death penalty and the belief that courts are not harsh enough has increased over time (Cullen et al., 2000). Roughly

50 percent of Americans supported the death penalty in 1970. By 1996, GSS data indicate that this figure had grown to 70 percent. Among Black Americans, research indicates less support for the death penalty, partly due to perceived injustice among Blacks (Johnson, 2008).

Accurately capturing Americans' attitudes about punishment is notoriously difficult due to the complexity of feelings individuals have towards crime. Nevertheless, this trend indicating a growing punitive orientation has led to suggestions that a shift in public support for punitive crime control is a major factor responsible for the growing use of incarceration in the U.S. Though overall support for the death penalty remains high among whites, this varies across communities, suggesting one possible factor that may account for community variation in the severity of sentencing (e.g., Baumer et al., 2003; Borg, 1997; Soss et al., 2003). On the whole, these streams of research suggest a more punitive social climate in communities with higher levels of political conservatism and support for the death penalty. This yields the expectation that: *sentencing will be more severe in jurisdictions characterized by higher levels of political conservatism and support for harsh sanctions.*

A relatively recent line of research has attempted to isolate some of the underlying attitudes that help to account for why some individuals (especially whites) tend to hold more punitive orientations. An intriguing finding to emerge from this work suggests that racial prejudice may be key to explaining the racial divide in support for the death penalty and punitive attitudes more generally (Johnson, 2008). Using a variety of data sources such as the GSS and the National Election Studies (NES), multiple studies show that racial prejudice among whites is one of the strongest and most consistent

predictors of support for the death penalty and support for other harsh crime control policies (Barkan and Cohn, 1994,1998; Bobo and Johnson, 2004; Soss et al., 2003; Unnever and Cullen, 2010; Unnever et al., 2008; Unnever et al., 2005). For instance, Unnever and Cullen (2005) find that nearly one-third of the support for capital punishment among whites can be explained by measures tapping white racism (see also Unnever and Cullen, 2010). Additional survey research finds that among whites, preferences for harsh sentencing are correlated strongly with perceptions of “racialized threat” and that harsh criminal sanctions offer whites a means with which to vent this racial animosity (Soss et al., 2003; Unnever and Cullen, 2007, 2010). What is particularly noteworthy about these studies is that they typically find that whites’ racial prejudice is associated to punitive attitudes generally, and not simply directed at blacks. Unfortunately, a more comprehensive examination of the precise reasons why white prejudice would stimulate more punitive reactions to criminals of all races has yet to emerge from this research. Nevertheless, these findings suggest that: *sentencing will be more severe in jurisdictions where whites’ racial resentment is higher.*

Social Trust

Lastly, I draw on classic propositions regarding the role of community social cohesion and informal social control to help explain variation in the severity of sentencing. Durkheim (1893), and more recently Donald Black (1989), describes community differences in the quantity and severity of formal social control as a function of weak informal social controls, the strength of which is reinforced by trust, cohesiveness and reciprocity among members of the community (Sampson et al., 1999). This community cohesion and coordination for the sake of punishment may take on one

of two forms. Braithwaite (1989) posits a negative relationship between social cohesion, trust and punitive sanctions. Specifically, he argues that communitarian locales characterized by more involvement, trust, and interdependencies may be less apt to rely on the state for punitive sanctioning in favor of informal shaming. Consistent with Braithwaite's theory, survey research reveals that individuals who express higher levels of interpersonal trust in others are significantly less likely to support harsh sanctions such as the death penalty (Soss et al., 2003). Researchers have interpreted this relationship to mean that trust is the foundation to civic engagement and that it expresses a commitment to the rights of other individuals in ways that promote forgiveness and a belief in second chances (Putnam, 1993; Soss et al., 2003).

In contrast to Braithwaite's view of community cohesion, Vidmar (2002) relies on a functionalist perspective to argue that close knit, cohesive communities will actually be motivated to punish defendants *more* harshly in order to restore the legitimacy of local rules and to maintain social cohesion. Legal violations in relatively cohesive, tight-knit communities challenge the norms governing acceptable behavior in these areas. This challenge may potentially trigger particularly retributive responses from the courts that are meant to reinforce the parameters of acceptable behavior. Tyler and Boeckmann (1997) present evidence consistent with this expectation. In a survey of California residents following adoption of the three strikes initiative, the central concern of individuals that supported the law was not fear of crime, but rather a concern about the decline of values and consensus.

Despite interest in the role of community trust, cohesion and involvement in relation to crime, only one multilevel study of sentencing has examined the effects of

social capital on individual legal outcomes (Baumer and Martin, 2011). While a promising start, the study by Baumer and Martin (2011) focused on outcomes for convicted murderers, and thus may not be generalizable to broad samples of felony cases (which are dominated by drug and property offenses). This study attempts to broaden our understanding of the effects of social trust by examining whether it impacts the sentencing outcomes for felonies in general, but also whether social trust may be particularly salient for understanding the legal sanctioning of non-violent drug offenders. Conflicting theoretical accounts highlighting the importance of trust and cohesion, and thus provide no single prediction about the direction of its effect on the severity of sanctioning.

SUMMARY

Four major theoretical models of punishment that guide contemporary macro and multilevel research on social control inform the hypotheses examined in this chapter. Each of these models underscores the importance of different state, county, and court-level attributes. Legal models draw upon Weberian theory to stress the formal and rational behavior of legal actors. Legalist perspectives thus view sentencing as a formal, bureaucratic, and predictable process guided by rules rather than moral outrage, political pressure, or organizational priorities. Viewed, this way, sentences are expected to be more geographically uniform in states that structure judicial discretion through the use of guidelines or mandatory penalties, and sentencing can be expected to be more severe in the context of a legal culture where laws embody a “get tough” approach to crime. Organizational efficiency models stress the importance of accounting for variability

across courts in workload pressure, resources required to mete out certain sanctions, and the ability of government to generate revenue to offset the fiscal burdens of crime control. Social conflict models, which remain a dominant force in the broader literature on social control, conceive of repressive punishment as a response to a perceived threat among elites and the middle-class to symbolic threats posed by the poor and minority racial and ethnic groups (Liska, 1992; Sampson and Laub, 1993). Though rarely tested in contextual or macro-level research on crime control (although see Stults and Baumer, 2007), an implicit assumption of this perspective is that fear and insecurity are critical intervening mechanisms that link the presence of “problem populations” to efforts to manage these populations through more severe sentencing. The final set of hypotheses examined in this study was formulated from neo-Durkheimian perspectives, which emphasize the moral and cultural roots of punishment (Garland, 1990). Viewed through this lens, punishment is a passionate response on behalf of the community, with roots in cultural sensibilities, morals, values and social cohesion; a view not inconsistent with research which shows that community norms and expectations help define court workers’ concepts of proper “going rates” of punishment (Myers and Talarico, 1987; Ulmer, 1997).

Table 2.2 summarizes the hypotheses developed from these theories that are tested in the current study. This table also serves as a guide to the order in which the results of these models are presented in Chapter 4 (findings).

Table 2.2. Summary of Hypothesized Relationships Between Jurisdictional Climate and the Severity of Sentences Applied to Felony Defendants

Theoretical Model of Sentencing Tested	Direction	Hypothesis Tested in Analyses
<i>Legal Models of Sanctioning</i>		<i>Hypotheses about the main effects of legal policies and statutory penalties on the severity of sentences imposed.</i>
<u>Structure of Sentencing Process in each State</u>		
Determinate Sentencing	+/-	No direction predicted.
Presumptive Sentencing Guidelines	-	A) Controlling for presence of presumptive guidelines will reduce residual variation in sentencing outcomes across jurisdictions. B) Defendants processed in jurisdictions operating under presumptive sentencing guidelines are expected to receive less punitive sentences on average compared to defendants processed in jurisdictions without presumptive guidelines.
Voluntary Sentencing Guidelines	-	A) Controlling for presence of voluntary guidelines will reduce residual variation in sentencing outcomes across jurisdictions. B) Defendants processed in jurisdictions operating under voluntary sentencing guidelines are expected to receive less punitive sentences on average compared to defendants processed in jurisdictions without presumptive guidelines.
Truth in Sentencing Time-Served Requirements	+/-	No direction predicted.
Mandatory Enhancement Score	+	Defendants processed in jurisdictions that have adopted a greater # of mandatory enhancement statutes are expected to receive more punitive sentences.
Mandatory Enhancement Score for Drug Offenses	+	Defendants processed in jurisdictions that have adopted a greater # of mandatory enhancement statutes related to drug offenses are expected to receive more punitive sentences.
Three Strikes Policy	+/-	No direction predicted.
Severity of Penalties for Possession 1 oz Cocaine	+	Defendants processed in jurisdictions with more severe penalties for possession of cocaine are expected to receive more punitive sentences.
Severity of Penalties for Sale 1 oz Cocaine	+	Defendants processed in jurisdictions with more severe penalties for sale of cocaine are expected to receive more punitive sentences.

Indirect Effects of Legal Culture:

Hypotheses about characteristics of legal climate expected to shape severity of sentences by conditioning the relationship between defendant criminal history and the odds of incarceration.

- | | | |
|--|---|--|
| Presumptive Guidelines x Prior Felony Conviction | + | Presumptive guidelines will increase the odds of incarceration by strengthening the magnitude of the effect that prior criminal convictions exert on the odds of being sentenced to prison or jail. |
| Three Strikes You're Out Law x Prior Felony Conviction | + | "Three Strikes' legislation will increase the odds of incarceration by strengthening the magnitude of the effect that prior criminal convictions exert on the odds of being sentenced to prison or jail. |

Organizational Models of Sanctioning

- | | | |
|---|-----|---|
| Jail Space Constraints | - | Jurisdictions with limited jail space will impose less punitive sentences (i.e., probation) than jurisdictions with greater availability of jail space. |
| Prison Space Constraints | - | Jurisdictions with limited prison space will impose less punitive sentences (i.e., jail or probation) than jurisdictions with greater prison space availability. |
| Per Capita Corrections Expenditures (in \$) | - | Jurisdictions that spend more money per capita on corrections are expected to sentence less punitively. |
| Average Monthly Probation Supervision Fee (in \$) | - | Jurisdictions that charge more on average for monthly probation supervision fees are expected to impose less punitive sentences (i.e. probation vs. incarceration). |
| Prosecutor Caseload Pressure | +/- | No direction predicted. |

Social Threat/Conflict Models of Sanctioning

- | | | |
|--------------------------------------|---|--|
| Size of Black Population | + | Jurisdictions with larger Black population will sentence more punitively. |
| Size of Hispanic Population | + | Jurisdictions with a larger Hispanic population will sentence more punitively. |
| Wealth Inequality (Gini coefficient) | + | Jurisdictions with greater income inequality will sentence more punitively. |
| Unemployment Rate | + | Jurisdictions with a larger population of unemployed persons will sentence more punitively. |
| Fear of Crime | + | Jurisdictions where a greater percentage of residents are afraid to walk in their neighborhood at night will sentence more punitively. |

Neo-Durkheimian Models of Sanctioning:

Public Sentiment and Social Cohesion

Southern Jurisdiction	+	Southern jurisdictions will sentence more punitively.
Levels of Support for Capital Punishment	+	Jurisdictions where a greater % of residents support use of capital punishment for murderers will sentence more punitively.
Levels of Social Trust/Cohesion	+/-	No direction predicted.
Levels of Religious Fundamentalism	+	Jurisdictions containing more residents who identify as religious fundamentalists will sentence more punitively.
Levels of Conservative Political Ideology	+	More politically conservative jurisdictions will sentence more punitively.
Levels of White Anti-Black Racial Animus	+	Jurisdictions with higher levels of anti-Black racial animus among whites will sentence more punitively.

CHAPTER 3

DATA AND METHODS

SAMPLE AND DATA

CASE-LEVEL DATA

I test the theoretical propositions outlined in Chapter Two using individual level data on adult felony case filings from the publicly available State Court Processing Statistics (SCPS) that have been merged with aggregate data capturing the local social, cultural, organizational and legal climate within which these cases are processed. The SCPS is a publicly available data set collected by the Pretrial Justice Institute on behalf of the Bureau of Justice Statistics. The data are publicly available through the Interuniversity Consortium for Political and Social Research (ICPSR study #2038). The SCPS is a biennial data collection effort that began in 1988. I use data on felony case filings for the most recent years of the SCPS series (1998, 2000, 2002, and 2004) that were publicly available at the beginning of the current study. Together, this cross-section of felony cases from 1998 to 2004 provides information on the processing of approximately 60,000 defendants initially arrested for felony crimes. The SCPS contains extensive data on prior criminal history, including the number and nature of prior arrests and convictions (misdemeanor, felony, and violent felony), any prior jail and prison incarcerations, and whether the defendant was on parole or probation at the time of arrest. One of the unique contributions of the SCPS is that it is the only large national sentencing database specifically designed to follow defendants throughout the adjudication process.

Information is recorded across major decision points between defendants' initial arraignment and the final adjudication of guilt and the sentencing disposition. The data include charging decisions at both the arrest and adjudication stage, the mode and outcome at adjudication (i.e., plea bargain, trial adjudication, diversion, or dismissal), data on the type of legal representation obtained by defendants, pretrial processing decisions regarding bail amount and the denial of release, the decision to confine an offender to jail or prison versus probation or other noncustodial punishment, and the duration of incarceration and/or probation.

SCPS Sampling

The geographic scope and sampling strategy of the SCPS are unique to sentencing research in several respects and merit a brief description. First, the design of the SCPS is intended to provide data on the processing of defendants within a representative sample of the 75 largest urban counties across approximately 20 states from each region of the US. Unlike the majority of sentencing studies in the literature, one of the goals of the SCPS is to situate the final disposition of felony cases within the broader context of urban court processing. Thus, the SCPS differs from datasets describing sentencing in single states (e.g., Pennsylvania and Washington) in that the data are not intended to provide estimates of sentencing behavior that are generalizable to the state in which these county jurisdictions are located.

The SCPS employs a multi-stage sampling procedure designed by the U.S. Census Bureau for selecting felony cases. Using a 2-stage stratified design, 40 of the 75 most populous counties in the US are selected during stage one (Kyckelhahn and Cohen,

2008; United States Dept. of Justice, 2010). Ten counties are included in the sample with certainty due to the size of their respective populations and felony caseload filings (e.g., Los Angeles; Cook; Miami-Dade; Maricopa). The remaining 30 counties are chosen from a pool of the remaining 65 largest counties in the US “based on the variance of their felony case filings” (U.S. Dept. of Justice, 2010: 6). The remaining 30 counties chosen to participate in the SCPS in a given year have a smaller sample of felony filings to draw from, and each are allocated to noncertainty strata with corresponding sampling weights.⁸ In the second stage of the sampling procedure, felony case filings within each of the 40 counties are systematically selected to represent all defendants with a felony charge filed with the court during the month of May. Each participating county for that year then provide data for each felony case filed on selected days during the month of May. A felony filing is followed for a period of one year, and murder cases are followed for two years. A small number of case filings involving death penalty cases may not be included in these data since capital cases require a lengthier adjudication period. When weighted according to their sample selection strata the approximately 15,000 cases sampled each year represent all felony cases (approximately 57,000 total in any given year) filed in the nation’s 75 most populous counties during the month of May (Cohen and Reaves, 2006). In 2004 the SCPS sampling frame was representative of 38% of the nation’s population (Kyckelhahn and Cohen, 2008), and according to the UCR, 45% of all serious violent crime reported in the nation in 2004, including over half of all robberies, 43% of assaults, and 43% of all murders. Therefore, the SCPS provides a unique glimpse into legal

⁸ The largest counties sampled with certainty receive a weight of 1.0 and smallest counties with fewer case filings receive the highest weight of 2.50. Because the remaining 30 counties are chosen with uncertainty, some counties will appear in the SCPS every year (i.e., the largest 10 counties) while some counties may only appear in 1998, 2000, 2002, or 2004, respectively.

decision making in the counties that process nearly half of all serious crime in the US; jurisdictions that are in a position to contribute significantly to the national incarceration rate through both prison commitment and duration decisions. All descriptive statistics and regression estimates in the current study are based in unweighted SCPS data. Details on the sample used in the current study, sample attrition out of the criminal justice system, and the removal of cases due to missing defendant or jurisdictional level data are discussed below.

Level 1 Sample

The SCPS is a database of accused defendants facing conviction and incarceration. Each defendant constitutes a ‘felony case filing.’ Thus, the unit of analysis in the current study is the defendant, rather than the charge. For each defendant, the most serious arrest and adjudication charge is identified as the primary offense. In the case of those arrested on multiple charges, there is also information on the second offense each defendant is initially charged with. The sample used for the present study is restricted to the portion of this sample convicted of a felony since in several states (e.g., Washington) sentencing guideline recommendations only apply to felony cases (Engen and Gainey, 2000).

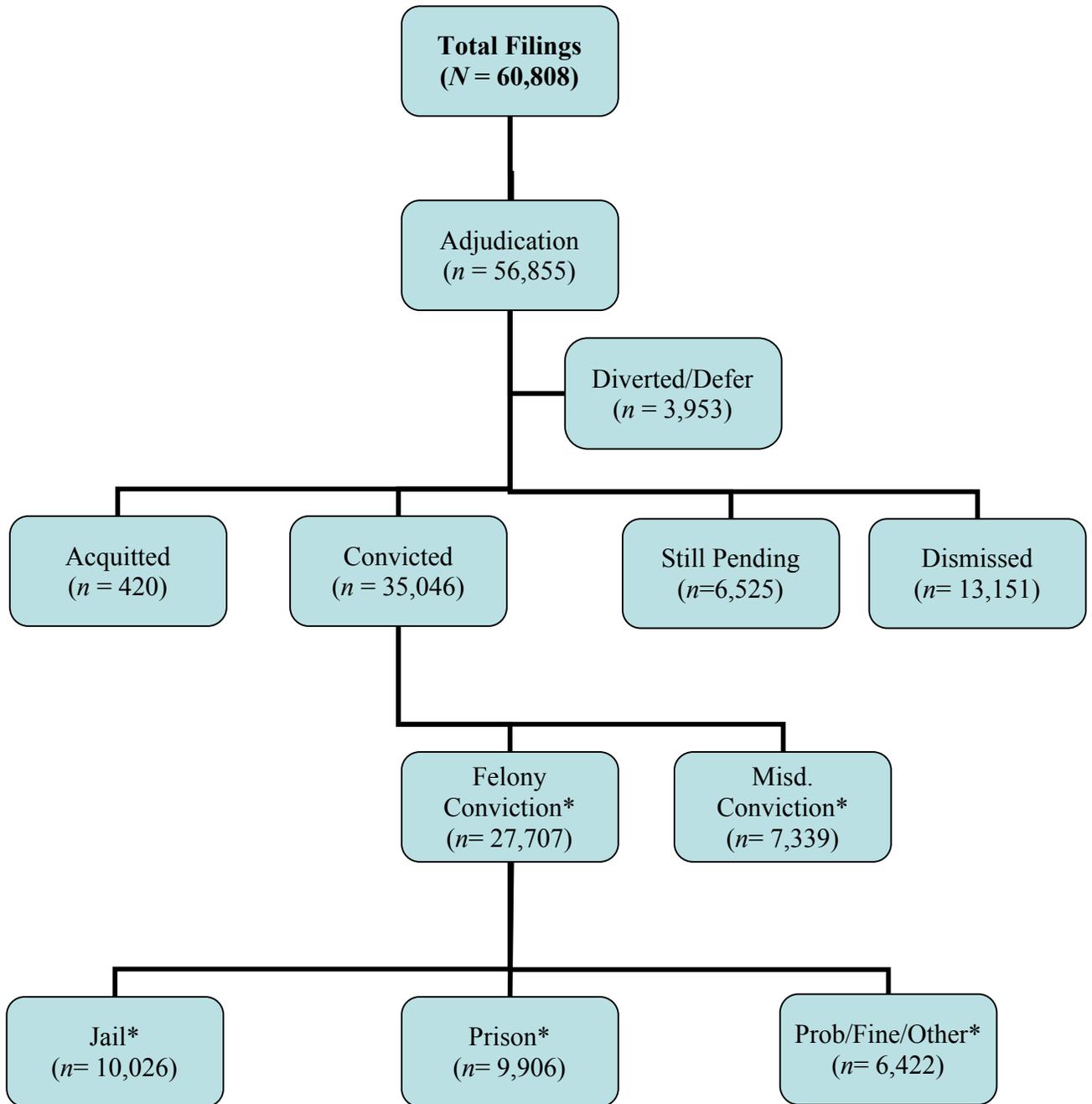
Figure 3 charts the extent and sources of sample attrition from the initial charging decision to final disposition, and identifies the initial sample sizes of all possible cases for use in the analyses of incarceration. An asterisk (*) indicates that sample sizes may not total to the original accused sample size due to missing data at key stages in the court process. At the individual level, sample attrition occurred when a defendant was initially

charged with a felony but convicted of a misdemeanor, the case was dismissed, the defendant acquitted, the case was still pending after one year, or the case was diverted out of the felony court to another adjudication setting (e.g., drug court) or an adjudication of guilt was “deferred.” Beginning with an initial sample of accused defendants whose cases moved forward past the arraignment stage and for which there is adjudication information ($N= 59,159$), and omitting 4,017 cases the prosecutor either diverted out of the court or deferred an adjudication of guilt, and those cases dismissed by the court or the prosecutor (13,151) provides a sample of 41,991 defendants adjudicated from 1998 to 2004. Of those, nearly 6,525 cases (mostly rape and murder cases) were still pending a year after data collection, 420 defendants were acquitted, and approximately 35,046 defendants were convicted. Of the latter, it could be determined that 27,707 defendants were convicted of a felony charge. From this pool of felony convictions, I removed an additional 1,353 defendants from the sample for three reasons. First, I removed 57 cases from three counties (Erie, NY; Westchester, NY; and Jefferson, KY) that had fewer than 25 convicted defendants per county. Although the consensus in multilevel modeling is that a large number of level two groups is more consequential for statistical power than a large number of individuals per group, larger samples of individuals within groups is necessary when the focus is on accurately probing for cross-level interactions, which is one component of the analyses examining the effects of legal policies on incarceration decisions. Reviewing simulation studies on the question of statistical power, Kreft and De Leeuw (1998: 125) conclude that at least 30 groups and 30 observations per group are required to obtain sufficient power to detect cross-level interactions. Next, sixty-eight cases were removed because data on the final sentencing disposition were missing.

Lastly, I removed 1,285 cases that were adjudicated in counties for which important jurisdictional level data were unavailable. Five of the counties in the 1998-2004 SCPS were not part of the General Social Survey sampling frame from 1990 to 2002.⁹ There were also no geographically proximate metropolitan areas near these counties that the GSS sampled that could be explored as potential substitutes for these five counties. After the omission of the cases from these eight counties from the level one defendant file, the final sample consists of 26,354 defendants that judges may sentence to prison, jail, or probation/fines.

⁹ These include Honolulu, HI; Salt Lake City, UT; El Paso, TX; Orange, FL; and Milwaukee, WI.

Figure 3.1 Case Flow Through Court System and Sample Attrition, 1998-2004 SCPS



Limitations of the SCPS

Although the SCPS is limited in important ways, it is currently the most comprehensive database available for studying multi-state adjudication processes from arrest to final disposition. Like many other data sets used to study sentencing, the SCPS does not contain information on defendant employment or family status, victim attributes, or data on judges and prosecutors involved in the case. These shortcomings are common across nearly all major sentencing datasets used in published research.

Another significant shortcoming of the SCPS, one that extends to almost all sentencing databases, is the lack of more precise detail on the severity (i.e. offense class) of arrest and conviction charges. The descriptions of the conviction offenses in the SCPS include crime types (murder, rape, robbery, assault, drug sales, drug possession, etc) but no specifics about each offense. For instance, it is not clear in the SCPS if the offender was armed with a gun (unless it was a weapons offense), if the offender knew the victim, if the victim was a minor, if there were multiple victims, or the time and location of the crime. This may hinder my ability to precisely predict the expected sentence for some offenses.¹⁰ Bushway and Piehl (2007) recently used the SCPS to compare sentencing and charging outcomes across two guideline states, one voluntary (Maryland) and the other presumptive (Washington). They point out that one disadvantage of using the SCPS to examine sentencing in a guideline state is that unlike most studies of sentencing in Washington and Pennsylvania, the SCPS does not provide the guideline worksheets that include the offense severity and criminal history scores that determine the presumptive sentence. The result is that the controls available in the SCPS for prior criminal history

¹⁰ For example, some states punish drug sales that occur on or near schools more punitively than drug sales that occur elsewhere.

and offense type may be less precise than the standard controls in studies from Pennsylvania and Washington that include the severity scores, the presumptive sentence from the guideline worksheet and whether a mandatory was applied by the prosecutor or judge. On the whole though, the content of the SCPS conform to most contemporary sentencing datasets, and contain more detail about a wide array of jurisdictions than any other data set on felony adjudication.

CONTEXTUAL DATA

The contextual data were compiled from several sources that describe various county, metropolitan, and state-level attributes predicted to affect sentencing severity. The sources for these data are provided in Table 3.1. Each contextual data source contained county and state census-derived FIPS codes that were used to attach cases in the SCPS (which also contain county and state FIPS codes) to data describing the county and state context in which defendants were processed. Data describing various aspects of each jurisdiction's sentencing structure and penal code are derived from a database of legal statutes dating from 1972 to 2002 that were collected by researchers at the Vera Institute (Stemen and Rengifo, 2005). Because the SCPS data extend beyond 2002, I consulted each state's penal code in 2003 and 2004 using Lexis-Lexis Academic Universe to verify whether each jurisdiction's legal policies were altered by state legislatures after 2002. To capture jurisdictional differences in organizational context, data were gathered on jail and prison capacities, state correctional spending, and the amount of supervision fees states and/or counties charge probationers each month. Information on county jail populations and capacities are available in the Annual Survey

of Jails and the 1999 National Jail Census, both of which were accessed through ICPSR. State prison population and capacity estimates were obtained from the National Prisoner Statistics bulletin series published by the Bureau of Justice Statistics (e.g., West, 2010). State correctional expenditures were obtained from the Expenditure and Employment Data for the Criminal Justice System database, which is collected annually for all states in the U.S. and is publicly available through ICPSR. One measure of organizational context required some primary data collection. The Vera database obtains information indicating the presence of monthly fees states charge probationers for the duration of their supervision up to 2002. I consulted state penal codes and contacted probation department representatives in every other state to verify probation supervision fee schedules for 2004. Also, the Vera database did not contain probation fee information for California and New York because such fees are determined at the local level rather than by the state legislature. I contacted these counties' local probation departments and conducted phone interviews with probation department supervisors to obtain monthly probation fee schedules from 1998 to 2004. Data describing the demographic makeup of each county are from the 2000 Decennial Census and the 2002 American Community Survey. County crime data are derived from the Uniform Crime Reporting program and were accessed through ICPSR. Data describing conservative religious and political ideologies and public sentiment were obtained from the General Social Survey (GSS). The GSS is conducted and made available by the National Opinion Research Center (NORC).

Table 3.1. Source and Description of Dependent Variable, Case Controls, and Contextual Predictors Included in Multilevel Regression Models of Sentencing	
Variable	Data Sources and Measurement
<i>Dependent Variable</i>	
Sentence Type	Nominal variable contrasting whether defendants' most severe sentence is prison, jail, or probation (1=prison; 2=jail; 3=probation).
<i>Case-Level Control Variables</i>	
<i>Source: State Court Processing Statistics, 1998-2004</i>	
<i>Defendant Characteristics</i>	
Black, non-Hispanic	Dichotomous variable identifying whether defendant was non-Hispanic black (0= no; 1= yes). (White, non-hispanic is omitted reference category).
'Other', non-Hispanic	Dichotomous variable identifying whether defendant was Asian, Pacific Islander, or Native American (0= no; 1= yes).
Hispanic, any race	Dichotomous variable identifying whether defendant is of Hispanic ethnicity (0= no; 1= yes).
Male	Dichotomous variable identifying the sex of the defendant (0= female; 1= male).
Age	Continuous variable indicating defendant age in years.
Prior Misdemeanor Arrest(s)	Dichotomous variable indicating defendant has at least one prior misd. arrest (0= no prior misd. arrests; 1= one or more prior misd. arrests).
Prior Felony Arrest(s)	Dichotomous variable indicating defendant has at least one prior felony arrest (0= no prior felony arrests; 1= one or more prior felony arrests).
Prior Misdemeanor Conviction(s)	Dichotomous variable indicating defendant has at least one prior misd. conviction (0= no prior misd. convictions; 1= one or more prior misd. convictions).
Prior Felony Conviction(s)	Dichotomous variable indicating defendant has at least one prior felony conviction (0= no prior felony convictions; 1= one or more prior felony convictions).
Prior Incarceration History	Dichotomous variable indicating defendant has served a prior prison sentence (0= no prior prison sentences; 1= one or more prior prison sentences).
Active Criminal Justice Status	Dichotomous variable indicating defendant under criminal justice supervision such as probation, parole, or on release pending other criminal charges (0=no active status; 1= active status).
<i>Case Characteristics</i>	
Violent Offenses	Five dichotomous variables indicating whether defendant was convicted of murder, rape, robbery, assault or other violent crime, respectively (0=no; 1=yes).
Property Offenses	Six dichotomous variables indicating whether defendant was convicted of burglary (reference category), larceny/theft, motor vehicle theft, fraud, forgery, or other property offense, respectively (0=no; 1=yes).
Drug Offenses	Two dichotomous variables indicating whether defendant was convicted of drug sales or other drug offense/possession, respectively (0=no; 1=yes).
Public Order Offenses	Three dichotomous variable indicating whether defendant was convicted of driving, weapons, or other public order charges, respectively (0=no; 1=yes).
Public Defender Counsel	Dichotomous variable indicating whether the defendant was represented by private attorney or another type of counsel (0=public defender or court-appointed attorney; 1=private attorney).
Guilty Plea	Dichotomous variable indicating whether the case was adjudicated through a plea agreement (0=no; 1=yes).
Year of Sentence	Four dichotomous variables where 1 represents 1998, 2000, 2002 or 2004 as year of sentencing. (1998 is omitted reference category).
Pretrial Detention	Dichotomous variable indicating defendant was incarcerated prior to sentencing (0= pretrial release; 1= pretrial detention).

Table 3.1 (cont.) Source and Description of Dependent Variables, Case Controls, and Contextual Predictors Included in Multilevel Regression Models of Sentencing

Variable	Data Sources and Measurement
<i>Jurisdictional Attributes</i>	
<i>State Legal Context</i>	
Determinate Sentencing	Dichotomous variable identifying counties located in states with a determinant sentencing structure (Vera Sentencing Policy Database from ICPSR #4456).
Presumptive Sentencing Guidelines	Dichotomous variable where 1 = county is located within a state operating under presumptive sentencing guidelines and 0 = not under presumptive guidelines (Vera Sentencing Policy Database, ICPSR #4456).
Voluntary Sentencing Guidelines	Dichotomous variable where 1 = county is located within a state operating under voluntary/advisory sentencing guidelines and 0 = not under voluntary guidelines (Vera Sentencing Policy Database, ICPSR #4456).
3-Strikes Policy	Dichotomous variable where 1 = county is located within a state that has adopted a three strikes you're out policy that mandates incarceration when a defendant is convicted of a third felony conviction. This variable captures only states that consider any current felony offense as a potential "strike" offense. (Vera Sentencing Database, ICSPR #4456).
Truth in Sentencing Requirements	Continuous variable that measures the percentage of sentence imposed that most felony offenders are required to serve before being considered for release. (Vera Sentencing Database, ICSPR #4456).
Mandatory Enhancement Score	Continuous variable developed by Stemen et al. (2006) capturing the sum of mandatory sentence enhancement laws passed by state that requires mandatory incarceration and/or a longer incarceration term for offenses involving 4 "triggers," which include use of a weapon, serious bodily harm, crimes while under state criminal justice supervision, and any offense committed against victims protected on basis of race, age, religion, etc. Higher scores represents a more punitive legal culture. (Vera Sentencing Database, ICSPR #4456).
Mandatory Enhancements for Drug Offenses	Continuous variable developed by Stemen et al. (2006) capturing the total # of mandatory sentencing enhancements that either mandate incarceration or increase underlying sentence for the sale or possession of marijuana, heroin, and cocaine. Higher numbers of enhancement scores represents a more punitive legal culture. (Vera Sentencing Database, ICSPR #4456).
Severity of Drug Possession Sanctions	Minimum sentence for possession of 28 grams (1 oz) of cocaine. (Vera Sentencing Database, ICPSR #4456).
Severity of Drug Sale Sanctions	Minimum sentence for sale/trafficking of 28 grams (1 oz) of cocaine. (Vera Sentencing Database, ICPSR #4456).
<i>Organizational Context</i>	
Monthly Probation Supervision Fee	Continuous variable indicating the state or county-determined monthly supervision fee charged to most felony probationers. (Data were gathered and verified through the use of three sources: a) the Vera Sentencing Policy Database from ICPSR #4456; b) The American Probation and Parole Association 2002 survey of county and state probation fees; and c) Personal phone interviews and email correspondence with chief probation officers in counties and states that participated in the 1998-2004 SCPS to verify information derived from the Vera databases and the APPA survey).
Prosecutor Caseload Pressure	Continuous variable representing the ratio of felony indictments in the SCPS per assistant prosecutor working in the county. (SCPS and 2001 National Prosecutor's Survey, ICPSR #3418).
Jail Capacity Constraints	Continuous variable indicating the average percentage capacity county jails operated at between 1997 and 1999. (Annual Survey of Jails, Jurisdiction-Level Data. ICSPR #2313, #2682 and 1999 National Jail Census, ICPSR #3318).
Prison Capacity Constraints	Continuous variable indicating the percentage capacity that state prisons are operating at between 1997 and 2001. (Prisoners Series, Bureau of Justice Statistics: http://bjs.ojp.usdoj.gov/index.cfm?ty=pbse&sid=40).
Corrections Expenditures	The amount of dollars spent on state and local corrections per 100,000 residents. 1997 data used for counties included in 1998-2000 SCPS; 2001 correction spending data used for counties included in 2002-2004 SCPS. (Source: Expenditure and Employment Data for the Criminal Justice System: CJEE Extracts File, 1997 and 2001; ICPSR study #s 3229 and 3962).
<i>Social Conflict/Threat</i>	
<i>Note: All Decennial Census data for 2000 and 2002 American Community Survey data available at American Factfinder: http://factfinder.census.gov/. 2000 Decennial Census data used for counties included in 1998/2000 SCPS; 2002 American Community Survey data used for counties included in 2002/2004 SCPS.</i>	
Percent Population Black	Percentage of county residents that are non-Latino black.
Percent Population Hispanic	Percentage of county residents that are of Hispanic ethnicity.
Unemployment Rate	Percentage of county residents in the civilian labor force who are unemployed
Income Inequality	Gini index measuring household income inequality. Continuous variable with values theoretically ranging from 0 (complete equality in distribution of income across county households) to 1 (total inequality, with county income concentrated in very few households).
Fear of Crime	Percentage of residents within MSA (1996-2002) who indicate being afraid to walk alone at night near their home (Aggregate GSS Data).
<i>Punitive Social Context</i>	
<i>Note: All GSS data obtained from the National Opinion Research Center (NORC).</i>	
Whites' Anti-Black Racial Animus	Seven-item standardized, additive scale that combines the percentage of white respondents within MSA agree/strongly agree that blacks should work their way up w/o favors to overcome prejudice; that black-white differences in SES are due to blacks' lack of motivation and willpower; that black-white differences in SES are not due to discrimination against blacks; that gov't does too much to help improve conditions for blacks; that gov't not obligated to give blacks special treatment to improve conditions for blacks; opposed to preferential hiring/promotion of blacks; and that it is somewhat or very likely that a black applicant would get a job or promotion over an equally or more qualified white applicant (Aggregate GSS Data, 1996-2002; Chronbach's Alpha=.86).
Religious Fundamentalism	Two item standardized, additive scale that combines the percentage of respondents within MSA that belong to a fundamentalist denomination with the percentage who believe bible is the actual word of God to be interpreted literally. (Aggregate GSS data, 1996-2002; Chronbach's Alpha = .86).
Levels of Trust/Cohesion	Two item standardized, additive scale that combines the percentage of residents within MSA who agree that generally most people can be trusted with the percentage of respondents who agree that most people are fair (Aggregate GSS Data, 1996-2002; Chronbach's Alpha=.84).
Support for Punitive Sanctions	Percentage of residents within MSA who support capital punishment for convicted murderers. (Aggregate GSS Data, 1996-2002).
Conservative Political Ideology	Average scores among GSS respondents in MSA on political ideology scale with theoretical values ranging from 1 (extremely liberal) to 7 (extremely conservative). (Aggregate GSS data, 1996-2002).
Southern Jurisdiction	Dichotomous variable identifying counties located in Southern states. (Source: Census 2000).
<i>Control Variables</i>	
County Index Crime Rate	Continuous variable of # Index crimes (excluding rape) per 100,000 residents averaged across 3 years (1996-1998 for 1998/2000 SCPS; 1999-2001 for 2002/04 SCPS; Source: ICPSR #s 9028, 2904, 3158, 3447, 3723).

CONSTRUCTING THE HIERARCHICAL DATA FILE

There are three implied levels of analysis in the current study. Data at the first level of analysis consist of felony case filings nested within fifty-two county state court jurisdictions for which individual case data are available (the second level of analysis). County court jurisdictions are “natural” aggregates for the study of legal decision-making because state trial courts and prosecutor offices are organized at the county level in all U.S. states (Weidner and Frase, 2003). These county court jurisdictions included in the SCPS between 1998 and 2004 are nested within 19 states (the third level of analysis). As previously mentioned, not every county in the SCPS is sampled with certainty and thus, will not appear in every year of the series. In all, 52 counties are included *at least once* during 1998-2004, some of which may only appear in 1998 or 2000.¹¹ There is a “break” in the county sampling frame in the SCPS after 1998. This is common among studies that use geographically stratified sampling designs to obtain nationally representative data (e.g., the General Social Survey). The break occurs because larger counties are sampled with certainty, and counties with smaller populations are chosen according to the size and variation of their caseload each year. Prior to the development of the American Community Survey, the sampling strata were based on decennial Census estimates of each county’s population. Therefore, the “break” between 1998/2000 in part reflects county population changes between 1990 and 2000. Table 3.1 summarizes the counties included in the analyses and the years for which they provided felony case filings. Twenty-one (40%) of the 52 counties appear in all four years of the SCPS. After

¹¹ These 52 counties are those included in the current sample after the omission of counties that had inadequate sample sizes at level one, or for which key aggregate level data were unavailable.

2000, the county sample remains virtually identical and forms a consistent grouping of 40 jurisdictions that participated in data collection for 2002 and 2004. Conceivably, each of these 52 counties could be treated as a single level two unit in hierarchical regression models. Given the structure of the SCPS though, it would be imprecise to treat each of these jurisdictions as static. Because some counties appear in multiple years, it was necessary to collect “real time” data in order to accurately reflect the shifting legal and social contexts that influence sentencing decisions. Legal and organizational characteristics changed considerably between 1998 and 2004 in some jurisdictions. For example, Florida and Michigan contribute felony cases in every year of the SCPS and both experienced significant changes to sentencing policy in 1999.¹² Ideally, one could simply nest each felony case within its year-specific jurisdictional context. In Table 3.2, this is denoted with an ‘*’ symbol and footnoted at the bottom of the table.

Unfortunately, partitioning each felony case by both location and year yields small cell counts for jurisdictions with fewer felony cases.¹³ This restricts the statistical power required for maximum likelihood estimation as well as the power needed to detect the presence of sentencing disparities since HLM estimates separate regression equations within each level two group. To increase within-county sample sizes while also accounting for shifts in the social and legal climate, I pooled cases in counties appearing in 1998 and 2000 together, and similarly pooled cases decided in 2002 and 2004 together.

The grouping procedure is illustrated in Table 3.2, with an ‘x’ denoting a county’s

¹² In late 1998, Florida did away with sentencing guidelines and adopted a determinate sentencing structure that abolished discretionary parole for most offenses. In conjunction with this shift, the state adopted the Criminal Punishment Code, which allowed for greater discretion to go above the legal statutes in determining punishment and lowered the threshold required for mandatory incarceration. In 1999, Michigan shifted from a voluntary to a presumptive guideline structure.

¹³ For example, Kings NY has 264 convicted defendants in the pooled 1998-2004 SCPS. If you break this down by year, however, there are only 42 defendants in 1998.

inclusion in a given year of the SCPS. Using Jefferson, AL as an example (the first row of the table), defendants processed in either 1998 or 2000 are nested within data describing the local context during that time period (1997-2000). Structurally then, these Jefferson, Alabama defendants are nested within a single level-two group. Using the same logic, for those defendants processed in Jefferson during 2002 or 2004, I appended contextual data that describes the jurisdictional climate in Jefferson from 2001 to 2004. These 2002 and 2004 SCPS defendants thus form the second level 2 group in the hierarchical file used in all analyses. If a county only appeared in the 1998 SCPS, then data describing the county during that time period (e.g., 1997) were appended to each defendant at level one. Likewise, if the county only provided cases in 2000, then jurisdictional data for the year 1999 were appended to each case at level two in the hierarchical file. This structure achieved dual purposes. It allowed for more temporally precise contextual data while creating level 2 groups with an adequate number of defendants necessary for multinomial logistic regression analyses. Structuring the felony cases this way yielded 91 county-year groups at level two nested within 19 states at level three. This relatively small sample of states precludes a formal three level model that incorporates state characteristics at level three. Therefore, all jurisdictional data described below are provided at the county level, such that state legal data and correctional spending data are included as variables at level two and capture whether a county is located in a state with that operates under various legal and organizational conditions.

Table 3.2. Counties and States Included in Analyses and Nested Data Structure

County and State	Level 2		Level 2	
	1998	2000	Case #	Case #
Jefferson, AL	{x	x}	= Case 1	{x x} = Case 2
Maricopa, AZ	{x	x}	= 3	{x x} = 4
Pima, AZ	{x	x}	= 5	{x x} = 6
Alameda, CA	{x	x}	= 7	{x x} = 8
Contra Costa, CA		{x}	= 9	{x x} = 10
Los Angeles, CA	{x	x}	= 11	{x x} = 12
Orange, CA	{x	x}	= 13	{x x} = 14
Riverside, CA		{x}	= 15	{x x} = 16
Sacramento, CA	{x}		= 17	
San Bernardino, CA	{x	x}	= 18	{x x} = 19
San Diego, CA		{x}	= 20	{x x} = 21
San Francisco, CA	{x}		= 22	
San Mateo, CA		{x}	= 23	{x x} = 24
Santa Clara, CA	{x	x}	= 25	{x x} = 26
Ventura, CA	{x}		= 27	
New Haven, CT		{x}	= 28	
Broward, FL	{x}	{x}	= 29 and 30* = 32 and	{x x} = 31
Dade, FL	{x}	{x}	32*	{x x} = 34
Hillsborough, FL	{x}		= 35	
Palm Beach, FL		{x}	= 36	{x x} = 37
Pinellas, FL		{x}	= 38	{x x} = 39
Fulton, GA		{x}	= 40	{x x} = 41
Cook, IL	{x	x}	= 42	{x x} = 43
DuPage, IL	{x}		= 44	
Marion, IN	{x	x}	= 45	{x x} = 46
Balt. County, MD		{x}	= 47	{x x} = 48
Baltimore City, MD	{x}		= 49	
Montgomery, MD	{x	x}	= 50	{x x} = 51
Macomb, MI		{x}	= 52 = 54 and	{x x} = 53
Wayne, MI	{x}	{x}	55*	{x x} = 56
Jackson, MO	{x}		= 57	
St. Louis, MO	{x}		= 58	
Essex, NJ		{x}	= 59	{x x} = 60
Bronx, NY	{x	x}	= 61	{x x} = 62
Kings, NY	{x	x}	= 63	{x x} = 64

Monroe, NY	{x}	= 65		
Nassau, NY		{x} = 66	{x	x} = 67
New York, NY	{x}	= 68		
Queens, NY	{x}	= 69	{x}	= 70
Suffolk, NY	{x}	= 71		
Franklin, OH		{x} = 72	{x	x} = 73
Hamilton, OH	{x}	= 74		
Allegheny, PA	{x}	= 75		
Montgomery, PA			{x	x} = 76
Philadelphia, PA	{x	x} = 77	{x	x} = 78
Shelby, TN	{x	x} = 79	{x	x} = 80
Dallas, TX	{x	x} = 81	{x	x} = 82
Harris, TX	{x	x} = 83	{x	x} = 84
Tarrant, TX		{x} = 85	{x	x} = 86
Travis, TX		{x} = 87	{x	x} = 88
Fairfax, VA		{x} = 89	{x	x} = 90
King, WA	{x}	= 91		

* Denotes a break in the sentencing structures for Florida and Michigan. This break only impacts counties that appeared in both 1998 and 2000. Thus, Broward, FL, Dade, FL, and Wayne, MI cases are appended to contextual data describing the year those cases were decided in order to reflect the shift in sentencing structures in 1999.

MEASURES

DEPENDENT VARIABLE

Judges must determine two things during sentencing: the appropriate type of punishment and its duration. The focus the current study is the final disposition, or sentence type, applied to convicted felons. Generally, researchers have gauged the decision to incarcerate in three ways. The majority of sentencing studies model this decision as the conditional probability of receiving any type of incarceration vs. a non-custodial sanction of probation, fines, and/or restitution (e.g., Britt, 2000; Johnson, 2006; Myers, 1987; Ulmer and Bradley, 2006; Ulmer and Johnson, 2004; Ulmer et al., 2008). This is commonly regarded as the ‘in/out’ or ‘total incarceration’ variable. Holleran and Spohn (2004) have questioned the standard practice of combining jail and prison

outcomes together, especially when examining the relationship between race, ethnicity, and incarceration. They argue that judges have several options to choose from and that the dependent variable should reflect the simultaneous probability of receiving either of those options. Though this issue is widely acknowledged in the sentencing literature, variation in state court sentencing in multilevel research conducted after the publication of Holleran and Spohn's (2004) study are almost always modeled using the total incarceration variable or as the log-odds of receiving a prison sentence versus jail or probation (e.g., King et al., 2010; Pardoe and Weidner, 2006; Ulmer and Bradley, 2006; Wooldredge, 2007; though Wang and Mears, 2010 and Fearn, 2005 are exceptions). In one of the only two multilevel studies that have modeled jail and prison as distinct types of incarceration, Fearn (2005) found that jurisdictional attributes play an important role in determining whether offenders receive prison versus jail, leading to speculation that previous evidence relating jurisdictional attributes to sentencing may more accurately reflect *imprisonment* patterns rather than incarceration generally, suggesting that the assessment of jurisdictional context on sentence type may be sensitive to the coding of the dependent variable.¹⁴ The SCPS includes information on both the most severe sentence handed down, as well as whether the most severe sanction was combined with a less serious sanction such as jail time followed by a period of probation. The most serious sanction is a term of incarceration in a state prison, followed by incarceration in a local jail, and then noncustodial sanctions such as probation and fines (non-custodial sanctions consisted of fines as the most serious sanction; all involved a period of probation, usually in conjunction with fines). Using information on the most serious

¹⁴ In a more recent multilevel sentencing study in Pennsylvania the authors acknowledge the importance of examining jail and prison decisions apart from each other, but are unable to do so because some communities may commonly use one type of incarceration (jail or prison) but not both (Ulmer et al., 2008).

sanction judges applied, sentence type is gauged using a trichotomous variable where prison sentences are coded as '1,' jail sentences are coded as '2', and noncustodial sanctions represent the third category of the dependent variable.

EXPLANATORY VARIABLES

I include as contextual predictors each of the jurisdiction-level measures highlighted in Table 3.1. These state and county level measures are derived from the five theoretical models summarized in Chapter 2 and thus are hypothesized, net of case level controls and levels of crime, to significantly influence sentence type. These state and county level measures capture legal and organizational context, social conflict and group threat, and variation in punitive social climate.

Measures of Legal Context

Each of the measures of legal context used in the current study are derived from the Vera Institute 'Fragment and Ferment' sentencing policy database, which contains a variety of indicators of state sentencing structure as well as information on the statutory sentence ranges, and substantive legal policies that alter the severity of punishment for certain types of offenders (e.g., repeat offenders) and certain types of offenses (i.e., violent crime involving a weapon). Sentencing structure for each state is measured using three dummy variables that are coded as '1' if a state has presumptive sentencing guidelines, determinate sentencing, and voluntary sentencing guidelines. The coding of these variables is consistent with the scheme used by Stemen and colleagues (2005). As Stemen et al. (2005: 10) explain, determinate sentencing refers to states that have

abolished discretionary parole release in an effort to ensure that the amount of time offenders serve is dictated by the length of sentences judges impose, as opposed to parole boards. Many determinate sentencing states such as California, Ohio, and Washington, also introduced presumptive recommended sentences for each offense class when they shifted from an indeterminate to determinate sentencing structure. Though the severity of the statutory ranges of these sentences varies across determinate sentencing states, such attempts to structure sentencing in determinate states should reduce the amount of residual variation in the log-odds of incarceration across counties and states.

Punitive legal culture is measured using six measures that tap variation across states in amount of prison time offenders are expected to serve when incarcerated, laws that target habitual offenders, the degree to which states embrace mandatory sentencing enhancements, and the severity of drug laws. The first variable, labeled *Truth in Sentencing Requirements* in Table 3.1, is a continuous level variable that measures variation across states in the minimum percentage of a sentence most offenders are required to serve before release (Stemen et al., 2005: 166). Although similar in spirit to determinate sentencing structures, many states have time-to-serve requirements while also maintaining discretionary parole release (Stemen et al., 2005). Higher values on this variable indicate more punitive “law and order” legal cultures, where some states such as Michigan require offenders to serve 100 percent of their sentence. Thus, this measure does not capture variability in the actual severity of state statutes (for example, Michigan may require offenders to serve 100 percent of sentences that are statutorily lower than other states’) it merely represents a commitment by states to increase the certainty of punishment.

Substantive sentencing laws are those that target certain types offenders and certain types of crimes for more punitive sanctioning (Stemen et al., 2005). I examine the effects of three laws that have been highlighted in the literature on mass incarceration as among the more controversial efforts by states to increase the severity and certainty of criminal sanctioning: three-strikes laws, mandatory enhancements, and more punitive penalties targeting drug offenders (Gottschalk, 2009; Spelman, 2009). The presence of a “*three-strikes*” law is measured with a binary variable coded ‘1’ if a state has a specific (and the most punitive) type of strike system, where any current felony conviction qualifies as a “strikeable” offense and mandates that the judge impose a term of incarceration. Coding the presence of three-strikes this way ensures that all felony offenders in these states qualify to be sanctioned under the three-strikes provision (as opposed to three-strike laws that target only violent felonies). The variable in Table 3.1 labeled *mandatory enhancement score* is a continuous variable originally constructed by Stemen and colleagues that represents the total number of mandatory enhancements that are triggered by four aspects of crime that are commonly considered to be aggravating factors. Specifically, this “score” summarizes the following: 1) the total number of mandatory minimums triggered when a weapon is used during the course of a crime, 2) the number of mandatory minimums triggered when the offender inflicts serious bodily harm or threatens to do so, 3) the number of mandatory minimum laws triggered when the crime is against a victim with “protected status” such as age, disability, race or religious affiliation, and 4) the number of mandatory minimum laws that target offenses committed while under state criminal justice supervision (i.e., probation, parole, imprisonment) (see, Stemen et al., 2005: 167). Higher scores indicate a more punitive

legal environment, where offenders convicted of otherwise similar crimes (e.g., burglary) may receive experience significantly different odds of incarceration in states that will enhance the sentence based on these aggravating factors versus states that do not mandate that judges increase the severity of sentences under such circumstances.

Given the attention to the war on drugs in the literature on sentencing and mass incarceration and evidence of a net-widening effect of arrest and incarceration for drug offenders (Beckett and Sasson, 2000; Blumstein and Beck, 1999), a series of models also explores the degree to which states and counties vary in the propensity to incarcerate drug offenders. Three variables were included that tap state variation in the severity of laws aimed specifically at drug offenders. The first of these is a continuous variable similar to the mandatory enhancement score described above. This variable, labeled *mandatory enhancements for drug offenses* in Table 3.1, consists of a composite “mandatory” score created by Stemen et al. (2005) that captures the variability across states in twelve different types of sentencing enhancements that target the most common drug offense types (i.e., sales and possession of cocaine, heroin, and marijuana). As explained by the authors, data on twelve enhancements for each of these three drugs “represent factors that may increase a sentence for the underlying offense if found by the jury at trial or by the judge at sentencing” (2005: 165). Five of these triggering factors include: 1) the location of the offense (i.e., near public housing, a church, or school property), 2) if the offense involved excessive quantities of drugs, 3) and drug offenses involving minors, 4) weapon use, 5) and gang activity. In addition to measuring the coverage of drug enhancements across various offense types (e.g., the above five criteria), the authors gauged the severity of each of the enhancements triggered by the above five criteria based on if the

enhancement is sale-related, possession-related, or both. Higher scores were given to states where mandatory enhancements targeted both sale and possession offenses. Using this method, three separate enhancement scores were calculated for each state: scores summarizing the severity and number of enhancements for marijuana, scores for enhancements targeting heroin, and scores that summarize the number and severity of enhancements that apply to either the possession, sale, or the manufacturing of cocaine. Additional details on the construction of each state's mandatory minimum score for drug offenses are provided in Stemen et al., (2005: 165).

Two additional variables are included in the regression models to examine whether more punitive legal contexts increase the severity of sentences applied to a subsample of SCPS drug offenders. These continuous variables capture state variation in the minimum statutory sentence (in months) for the sale of cocaine and the minimum statutory sentence for the possession of 28 grams (1 ounce) of cocaine.

Measures of Organizational Context

Various measures of organizational context are examined in order to estimate the effects of the costs of punishment to the state, states' efforts to offset those costs, constraints on the capacity to mete out incarceration, and variability in the workload pressure on local courts and prosecutors. The analyses include three indicators of organizational efficiency and workload: prosecutor caseload pressure, and the size of county jail and state prison populations as a percentage of their rated capacity. The variable labeled *prosecutor caseload pressure* in Table 3.1 is measured as the number of felony indictments (i.e., the aggregate number of felony indictments per county as

provided in the SCPS) divided by the number of chief and assistant county prosecutors per county. Although prior multilevel research has considered the effects of caseload pressure, these studies have conceptualized of workload pressures as a constraint on judges rather than prosecutors (Farrell et al., 2009; Johnson, 2005; Ulmer and Bradley, 2006; Ulmer, Bader, and Gault, 2008; Ulmer and Johnson, 2004). This is a logical choice for analyses of data that contain defendants processed within both small and large jurisdiction types. However, in the SCPS (a sample of large urban counties) 95% of all the cases are adjudicated via a guilty plea, which suggests that the burden of processing and disposing cases falls more squarely on prosecutors rather than judges.

Following the lead of Wang and Mears (2010) and the practices of the Bureau of Justice Statistics (West, 2010), state prison capacity is measured as the total size of the state prison population at year end expressed as a percentage of the total rated capacity of each state's prison system. Data on prison populations and capacities were obtained from the BJS 'National Prisoners Statistics' (NPS) series. It is important to note though that variation in state prison capacities is difficult to measure (Spelman, 2009). State prison population counts frequently omit prisoners held in private prisons and thus may not provide valid information in which to calculate overcrowding. Second, authors of the most recent NPS report note that there is no uniform measure of capacity across states (West, Sabol, and Greenman, 2010). Most states provide estimates of their official "rated" capacity, but some report only their design capacity, and others report their "operational" capacity. One state, Connecticut, has not report capacity data at all since 1994. For the current study, rated capacity was available for eleven states, design capacity had to be used for four states (NJ, PA, AZ and CA), and operational capacity

only was available for three states (MI, MO, and MD). For Connecticut, capacity information from 1994 was used. Similarly, jail space constraints were computed as the total county jail population expressed as a percentage of each county's rated jail capacity. Data on jail populations and capacities were obtained from the Annual Survey of Jails. This measure of jail capacity conforms fairly closely with measures used in prior studies (Ulmer and Johnson, 2004; Wang and Mears, 2010).

One important consideration commonly overlooked in research on sentencing is that the costs of punishment and the amount of state resources that are devoted to correctional punishment varies substantially across state and local jurisdictions (see e.g., discussions by Liska, 1992: 9; Sampson and Laub, 1993; Spelman, 2009). Variation in the extent to which states devote resources to punishing offenders is measured as the state's correctional spending per 100,000 residents. This measure was calculated using data available in the annual Criminal Justice Employment and Expenditures data bases available through ICPSR (see description in Table 3.1). These data show that the overwhelming bulk of state correctional spending is devoted to maintaining and building prisons. Macro level research on incarceration has also shown that the varying ability of states to fund or *offset* these costs through collecting revenue is associated with the size and growth of incarcerated populations (e.g., Greenberg and West, 2001; Spelman, 2009). In the context of sentencing decisions that more frequently result in jail and probation terms (functions that require funding at the local level), a more proximate source of fiscal offsetting that has not been considered in prior studies is the varying willingness of states and local authorities to force probationers to pay monthly fees toward the cost of their supervision as a means of offsetting the growing costs of prison

and jail terms. This method of generating revenue to offset the costs of punishing the most rapidly expanding share of the correctional population has become increasingly important as states have begun grappling with the soaring costs of imprisonment by cutting funding to probation functions (Diller, 2010; Ring, 1989). Measuring variability in the amount of dollars they charge probationers per month can capture the degree to which states and counties can conceivably offset probation costs, and thus avoid more costly sanctions involving incarceration. In the current study, this revenue is measured using a variable that captures the standard monthly probation service fee (i.e., the fee for simply being on felony probation) states and counties charge probationers. The measure excludes the costs associated with “specialized” probation programs such as drug treatment and high-risk electronic monitoring programs; the costs of which varied substantially across the jurisdictions included in the SCPS. The data for this measure was gathered in two stages. Initial state-level estimates of probation fees were gathered from the Vera Institute sentencing policy database, which provides both the amount states charge as well as the authorizing statute in each state’s penal code. Most states dictate in their penal codes whether they permit state and local probation departments to gather such fees. In most instances, state law also dictates either a fixed or maximum fee probation departments may charge. In two states, New York and California, state law directs local probation offices at the county level to determine the monthly supervision fees they charge probationers. Preliminary analyses of these fees reveal that over time they have increased in many states and that in several instances, state statutes did not dictate the amount of money probation departments are permitted to charge. A subsequent effort was made to collect this missing information. Phone interviews and

subsequent email correspondence with state and local probation supervisors provided more detailed “year-specific” fee schedules, clarified the maximum amount of fees probationers may be charged, as well as how the range of these fees varied across counties located in California and New York.

Measures of Social Conflict/Threat

As discussed in the previous chapter, social conflict and threat perspectives on punishment dominate contextual and macro level research on social control. The theoretical literature on conflict theory emphasizes five structural conditions that are predicted to increase the severity and certainty of punishment: unemployment, income inequality, and the size of the non-white population. In addition to the size of the poor and non-white populations, fear of crime has been identified in the literature as a key intervening variable linking these structural conditions to more punitive social control (Liska, 1987). Each of these measures is described below.

There are various ways scholars have operationalized the types of adverse economic conditions predicted to foster more repressive social control in a number of ways. Researchers have measured threatening economic conditions using the poverty rate, unemployment, median household income, per capita income, an index of disadvantage, and income inequality as economic indicators (e.g., see Table 2.1). I include a measure of each county’s civilian unemployment rate for several reasons. First, it is the most consistent indicator of economic conditions used in multilevel sentencing research (see Table 2.1). Second, classic conflict theories linking social structure to incarceration hinge on the assumption that states incarcerate more frequently in response

to the symbolic threats to order posed by a “labor surplus” (e.g., Rusche and Kirchheimer, 1939). Admittedly, unemployment may not be the most reliable indicator of economic insecurity since unemployment in some areas may be temporary, while in others it may represent the permanent elimination of a large sector of workers. Unemployment indicators likely also neglect the working poor. Nevertheless, given its centrality to conflict theories of punishment I follow the lead of prior multilevel sentencing studies and include a variable measuring county variation in the unemployment rate. This variable, labeled *unemployment rate* in Table 3.1, is calculated as the percentage of civilian residents ages 16 and older in the labor force that are unemployed. Income inequality is measured using the Gini index, the most commonly used measure of income inequality used in social science research (Swanson, Siegal, and Shryock, 2004). The index is a summary measure of income inequality that measures the distribution (or evenness) of income within a county across county households (Webster and Bishaw, 2007). It does so by first ranking households from poorest to richest, and then comparing the share of total cumulative income among each type of household. The Gini coefficient ranges theoretically from zero (complete equality) and 1 (complete inequality). Thus, larger values indicate more adverse (and threatening) economic conditions. Lower Gini scores mean that all households have about the same amount of income each year and that income inequality is relatively low. Higher Gini coefficient scores (approaching 1.0) indicate that the majority of income in a county is in the hands of relatively few households (i.e., greater inequality).

Prior research has primarily measured the size of the non-white population by including variables capturing variation in the size of the non-Latino black and the size of

the population of Latino ethnicity. The size of the black population is measured as the proportion of the total county population that is black (multiplied by 100) to create the variable labeled *percent Black*. The size of the Latino population is measured as the proportion of the total population that is Latino (multiplied by 100) and is labeled *percent Hispanic*.

Drawing upon survey research linking racial composition to perceptions of disorder and crime, variation in the size of non-white populations is thought to represent the degree to which the public perceives that crime and disorder are a problem. Thus, a key intervening variable included in the social threat models is a variable labeled *Fear of Crime*. Community variation in fear of crime was measured by aggregating responses for a GSS survey item asking respondents if there is a location within a mile of their home where they are afraid to walk alone at night (Warr, 1995). Though neither the main nor intervening effects of fear on sentencing have not been examined in previous multilevel sentencing research, prior research has shown that levels of fear vary substantially across communities (Liska et al., 1981); see also Stults and Baumer, 2007). It should be noted that the item tapping fear in the GSS suffers from some important limitations, chief of which stems from the fact that the question does not specifically ask residents about their fear of crime, uses only one item to tap a complex phenomenon, and is hypothetical and assumes respondents would actually walk alone at night (Kleck and Kovandzic, 2009: 55). Nevertheless, these are the only national data tapping individuals' fear, and given the emphasis placed on fear in the theoretical literature these data warrant at least some initial consideration in multilevel sentencing research. Details on the process of aggregating GSS responses are provided below.

Measures of Punitive Community Social Climate

In the previous chapter I described four dimensions of public sentiment predicted to foster a more punitive social climate: support for harsh sanctions such as capital punishment, southern jurisdiction, levels of social trust, conservative political and religious beliefs and racial resentment. One of the oldest and most common techniques for attempting to capture spatial variation in punitive culture is through the use of a dummy variable indicating if the jurisdiction is located in the South versus a non-Southern region (e.g., Farrell et al., 2009; Fearn, 2005; King et al., 2010; Wang and Mears, 2010; Weidner et al., 2004). In keeping with previous research on sentencing and incarceration, I include a variable labeled *South* that is coded '1' if a defendant was sentenced in a southern county jurisdiction and coded '0' if sentenced elsewhere.

I include measures of local public sentiment that are drawn from individual-level survey responses pooled across the 1996, 1998, 2000, and 2002 General Social Survey (GSS) that when aggregated, represent metropolitan area estimates of the social climate. Although the use of individual and aggregated GSS data is not new to studies of crime, it has only recently been used to study legal decision-making and warrants discussion.

The GSS is the leading source of time-trend research on social indicators in the United States and has been fielded biennially since 1994 (annually from 1972 to 1993). Each year NORC selects approximately 3,000 individuals to be interviewed in person. The questionnaire gauges public attitudes on a wide array of issues that range from abortion, levels of marital satisfaction, gun ownership, social activities, and a permanent set of questions about race relations, religious views, interpersonal trust, attitudes on punishment, and fear. The GSS uses a stratified, multistage area probability sample of

households to generate a nationally representative sample of English-speaking adults in the continental United States (Davis, Smith, and Marsden, 2009 - Appendix A). This multistage sampling process occurs in three major stages, each of which involve sampling at progressively smaller units of analysis. The GSS is administered through face-to-face interviews and consistently achieves a high response rate (approximately 71 percent) (GSS cumulative codebook, 1972-2006). The data I use are drawn from the 1990 sampling frame, which was in place between 1990 and 2002. This sampling frame contains the initial 100 primary sampling clusters (PSUs) from which households and respondents are sampled. Within each of these 100 PSUs, NORC selects blocks, and then housing units within each block.¹⁵ Because the GSS uses metropolitan county clusters (MSAs) and single nonmetropolitan counties as their primary sampling area from which to randomly select respondents, samples within each PSU are designed to be “self-representing.” That is, PSUs are initially selected so as to provide nationally representative survey estimates. These PSUs only remain nationally representative if

¹⁵ In the 1990-2002 GSS, 100 PSUs were selected at the first stage of sampling. Nineteen PSUs are sufficiently large that they are sampled with certainty. PSUs consist of counties, MSAs, and independent cities. Within each PSU, a second stage of sampling determines block groups are selected after first stratifying each block group within the MSA by race and income. Within block groups, blocks are selected with probabilities proportionate to the number of housing units. Within each PSU, anywhere from three to twenty six block segments were selected. The GSS uses full probability sampling only down to the block level. Within each block an average of five households are sampled using a less expensive “quota” procedure to select respondents. Interviewers are instructed to begin a travel pattern that begins with the first household located at the Northwest corner of the block and continuing until census-derived quotas have been met. These quotas require samples of respondents that correspond with 1990 census-tract figures for that area. These quotas ensure that each block group, when combined, provide for a sample of respondents that are proportionate to their share of the population. In this way, the samples from each MSA are “self representing.” That is, the sampling procedures are formally designed to yield respondents that represent the blocks within which they live, and these blocks represent the county or MSA within which they are located. Together, the responses from all PSUs provide nationally representative estimates. Additional details on GSS sampling procedures are provided in the GSS codebook, Appendix A and available at www.norc.org.

within each of these PSUs, respondents are then selected in ways that represent the local population.¹⁶

Although researchers primarily use the GSS to examine public sentiment at the individual level, geographic identifiers that link respondents to their respective PSU have recently been made available by NORC. Researchers are increasingly using these geocoded versions of the GSS to study the social context of individual attitudes (e.g., Baumer et al., 2003; Kleck, 1996; Kleck and Kovandzic, 2009; Taylor, 1998; Wong, 2007) and to aggregate survey responses to describe levels of racial prejudice (Baumer and Stults, 2007), gun ownership (Moody and Marvell, 2005), social capital (Kawachi et al., 1997; Rosenfeld et al., 2001), mutual trust (Hemenway, Kennedy, Kawachi, and Putnam, 2001), and conservative social ideology (Brace et al., 2002). In particular, the advancement of methods to assess the reliability of aggregated survey data has fueled the use of aggregated survey items in political science (e.g., Jones and Norrander, 1996) and in criminological studies on the neighborhood contexts of crime (e.g., Sampson, Raudenbush and Earls, 1997).

Following the procedures outlined in previous studies using these data, I aggregated individual responses to estimate levels of fear, racial animus, interpersonal trust, conservative ideology, support for the death penalty, and religious fundamentalism in the metropolitan areas within which the SCPS courts are located. In all, the 52 SCPS counties included in this study are located within 30 metropolitan areas sampled in the

¹⁶ Because the “natural” aggregate areas for sampling in the GSS are county clusters, most researchers caution that the data are not appropriate for aggregation to the state level (Kawachi, Kennedy, Lochner, and Prothrow-Stith, 1997: 1492). This is because GSS sampling procedures tend to favor large areas that only represent a fraction of the state population. However, Brace and colleagues (2002) show that some items in the GSS do exhibit adequate levels of reliability and face validity when individual responses are aggregated up to the state level. Given the focus on local social climate in the current study, all GSS data were aggregated to the PSU level, which provides an estimate of the social climate for the MSA in which each SCPS county is located.

GSS that yielded 4,788 survey respondents. All of the GSS items I include are part of a “core” set of items asked of respondents every year. I pooled responses from 1996-2002 (4 years of surveys) to create sample sizes large enough to produce reliable aggregate estimates of each item. Depending on the survey item, the average number of respondents per PSU ranged from 76 respondents on one item to an average of 220 respondents for another item. In a handful of PSUs however, the number of respondents that answer questions about racial perceptions is considerably lower (e.g., 10 to 20 respondents) because these items are only administered to half of GSS respondents each year. Thus, smaller PSUs with fewer respondents may yield less reliable aggregate estimates of the survey items.

Notwithstanding concerns about small within-PSU sample sizes, any study relying on samples rather than the population yields survey estimates with some degree of sampling error. However, a central concern when aggregating public opinion data is whether individual responses truly reflect the broader sentiment among residents who reside within each MSA. There are several ways scholars have grappled with estimating the reliability of aggregated opinion data, each of which rest on the basic tenet that aggregate reliability is a function of the ratio of aggregate to individual level variation in the item (Jones and Norrande, 1996: 298). Recently, Raudenbush and Sampson (1999) presented their method of assessing ‘ecometric’ reliability that provides an estimate of the precision with which survey based measures capture the aggregate climate. Rooted in generalizability theory, their proposed coefficient for reliability (λ) relies upon the degree to which individuals within aggregate PSUs “agree” and the number of respondents per

PSU.¹⁷ I used this formula to calculate aggregate reliability coefficients for each of the measures I initially considered in the GSS. Although the rules of thumb vary, Jones and Norrander (1996: 302) consider coefficients greater than or equal to .70 to be highly reliable; coefficients between .60 and .70 to be moderately reliable, and coefficients < .60 to be unreliable indicators at the aggregate level. Each of the GSS items I use to construct aggregate estimates of the local social climate yielded reliability coefficients greater than or equal to .60. These coefficients are presented below in Table 3.3.¹⁸

Below, I describe each of the aggregate measures constructed using these GSS items.

¹⁷ The formula for lambda is provided in equation 3.58 in Raudenbush and Bryk (2002: 49). Alternatively, the equation for lambda can be expressed as $\sum [\tau_{00} / (\tau_{00} + \sigma^2 / n_j)] / J$. As described by Morenoff et al. (2001: 526), the lambda coefficient is a function of (1) the sample size within each PSU and (2) the proportion of the total variance between PSUs (τ) relative to the amount of variance that is within each PSU (σ^2). These scores are calculated within each group and then averaged across the groups. A straightforward way of calculating lambda is to estimate an unconditional model in HLM where the individual survey item is the outcome. By default, HLM output includes an estimate of lambda labeled 'Reliability.'

¹⁸ Estimating these reliability coefficients is an important first step and one that to my knowledge is not frequently performed in studies using the aggregated GSS. However, there are still additional obstacles to using aggregated survey responses worthy of consideration. In PSU's with fewer respondents the precision of the aggregate estimate for a given item (e.g., fear to walk alone at night) could feasibly be lower than the precision of the estimate for a PSU like Chicago, which has nearly 500 respondents. This could be thought of as measurement error that varies across places. It is unclear if this is a problem in the current research because reliability coefficients generate precision estimates averaged over all PSUs. Raudenbush and Sampson (1999) propose estimating a multilevel latent variable model to assess the degree of inter-rater agreement and measurement error that arises from aggregating survey responses across larger geographic units. Unfortunately, their method appears to be exclusive to the construction of scales at the individual level and they do not illustrate or discuss methods of assessing general sources of error that may arise when aggregating single items across geographic units. This is an issue I will be addressing in more detail in future research.

Table 3.3. Ecological Reliability Coefficients for Aggregated GSS Survey Items Included in Analyses.

<i>GSS Item/Scale (R= Respondent)</i>	<i>30-PSU Sample (N= 4,788)</i>
	Reliability Coefficient
Religious Fundamentalism Scale Indicators (2-item scale)	
R Believes Bible Literal Word of God	.79
R Belongs to Fundamentalist Religion	.88
R Afraid to Walk in Neighborhood at Night	.70
Mean Level of Politically Conservative Ideology (on scale from 1 to 7)	.70
R Favors Capital Punishment for Convicted Murderer	.73
Social Trust/Cohesion Scale Indicators (2-item scale)	
R Agrees, Generally Most People Can Be Trusted	.65
R Agrees Most People are Fair	.62
Whites' Anti-Black Racial Animus (7-item scale)	
R Agrees Blacks Should Work Way Up w/o Favors to Overcome Prejudice	.72
R Agrees Black-White SES Differences due to Blacks' Lack of Motivation/Will	.64
R Disagrees Black-White SES Differences due to Discrimination Agst. Blacks	.68
R Agrees Govt. Does Too Much to Help Improve Conditions for Blacks	.80
R Opposes/Strongly Opposes preferential hiring/promotion of Black candidates	.60
R Agrees/Strongly Agrees Gov't Not Obligated to Give Blacks Special Treatment in Order to Improve Conditions for Blacks.	.72
R Agrees Somewhat/Very Likely Black Applicant Get Job/Promo Over Equally or More Qualified White Applicant	.60

Note: 'R' denotes term 'respondent.' Reliability coefficients calculated using HLM.
Coefficients estimated using data from PSUs included in the current study.

Christian Fundamentalism

Religious beliefs are an important source of cultural sensibilities about crime and punishment, and religious coalitions are particularly effective at mobilizing those beliefs to affect public policy (Unnever et al., 2006). Levels of religious fundamentalism in each county are estimated using a standardized additive scale containing two items from the GSS. The scale is designed to combine adherence to fundamentalist beliefs (i.e., fundamentalist ideology) with a measure of respondent affiliation with fundamentalist denominations. I combine the two dimensions to acknowledge that fundamentalist views

may not necessarily correspond with a punitive orientation. If they do, individual religious views do not necessarily influence government functioning in the absence of churches and religious coalitions that are capable of mobilizing their members. Thus, it is important to capture variation in ideology, as well as variation in the size of fundamentalist denominations across jurisdictions.

The first item consists of the percentage of respondents who indicate that their feelings come closest to “the Bible is the actual word of God and is to be taken literally, word for word,” compared with respondents who feel that “the Bible is the inspired word of God but not everything in it should be taken literally” and those who responded that “the Bible is an ancient book of fables, legends, history, and moral precepts recorded by men.” This measure is commonly used in individual-level survey research examining the influence of “biblical literalism” on punitive attitudes (Unnever et al., 2006). The ecological reliability of this item is .79.

The percentage of respondents in a county who reported affiliation with a fundamentalist Christian denomination was measured using an item originally designed by Smith (1990) and included in each year of the GSS. This measure of Christian fundamentalism is arguably most widely used indicator in the social sciences and was created to assist analysts (especially those not deeply familiar with religious studies) who did not wish to construct their own typology (Smith, 1990; Steensland et al., 2000). This variable, labeled ‘FUND’ in the GSS data, classifies each GSS respondent along a continuum of fundamentalist, moderate, and liberal Christian faiths (Smith, 1990: 227). Smith (1990) used five different criteria for classifying denominations along the continuum: “1) prior classification schemes used in the literature on religion, 2)

membership in theologically oriented ecumenical associations, 3) surveys of denominational members, 4) surveys of denominational clergy, and 5) theological beliefs of denominations,” (Smith, 1990: 226). This measure exhibits relatively high ecological reliability ($\lambda = .88$). Smith’s classification scheme has been criticized on a number of fronts. Steensland and colleagues (2000) argue that the measure is flawed because respondents do not identify themselves as fundamentalist, that the complexity of religions in the U.S. cannot be distilled down to a three-part typology, and that it cannot account for non-denominational faiths. Each of these are valid criticisms. However, given its longstanding use in the literature on religion, its broad conceptualization of fundamentalism generally (i.e., not simply Christian fundamentalism), and unique ability to capture fundamentalism among whites specifically, the GSS measure constructed by Smith is still preferable to measures used in prior research that merely tap the number of conservative or fundamentalist Christians within a county (e.g., Fearn, 2005; Ulmer et al., 2008).

Because each of these items were initially measured on different scales and for the purposes of assigning equal weight to each measure in the index, I standardized the aggregated responses (i.e., the percentage of respondents who believe that the Bible is the actual word of God and the percentage belonging to a fundamentalist denomination) using z-score transformations. This transformation consists of subtracting the average score for each item across counties and dividing by the standard deviation of each measure. Thus, once transformed, each measure included in the additive scale (but not the scale itself) will have a mean of zero and a standard deviation of one. Expressed this way, each county’s value for these two measures represents how far the county is from

the mean level of fundamentalism expressed in standard deviation units. The shape of the distribution of values among the counties remains the same; only the metric (or scaling) changes.

Support for Harsh Sanctions

Community variation in levels of support for punitive legal sanctions is gauged by aggregating the number of GSS respondents within each PSU that who indicate that they favor (versus oppose) the death penalty for persons convicted of murder. This GSS item is frequently used in individual level research examining the sources of punitive attitudes (e.g., see Applegate et al., 2000; Barkan and Cohen, 1994; Unnever et al., 2006). Ideally, other items that tap GSS respondent's feelings about the appropriateness of harsh sanctions would be used to form a multi-item composite measure of punitive attitudes. An initial two-item additive scale of punitive attitudes included an indicator of whether respondents feel the courts in their area were not harsh enough. However, this GSS item exhibited weak ecological reliability ($\lambda = .58$) at the aggregate level and did not perform well in a scale combining it with the item tapping support for the death penalty because the two items are not strongly correlated ($r = .422$).

Levels of Social Trust

Prior research has used aggregated GSS responses to capture spatial variation in levels of social capital and trust (Rosenfeld, Messner, and Baumer, 2001). Ideally, the goal of the current study was to capture variation in levels of social capital broadly (e.g., participation in volunteer organizations, time spent socializing with neighbors, etc).

However, several indicators included in the GSS that tap social ties and cohesion did not exhibit sufficient ecological reliability. Thus, I rely on a measure of social trust (rather than social capital in a broader sense), which has been emphasized as a key ingredient to more “communitarian” societies where mutual trust is believed to nurture mutual obligations and a greater reliance on others (Braithwaite, 1989). Higher levels of trust and communitarianism are predicted to foster less repressive punishment in favor of restorative justice measures. Social trust is measured a scale that combines two items from the GSS tapping levels of trust in others and perceptions of others as fair. The measure included in regression models is measured by summing the standardized scores on two items in the GSS. The first item asks respondents “Do you think most people would try to take advantage of you if they got a chance, or would they try and be fair?” At the aggregate level, the measure was constructed to represent the percentage of respondents who agree that most people try to be helpful. The second measure of trust was based on the following question: “Would you say most of the time people try to be helpful, or that they are mostly just looking out for themselves?” These two items were aggregated to the PSU level to represent the percentage of respondents who agree that most people try to be helpful and the percentage that agree that most people try to be fair. At the aggregate level these measures exhibited less than desirable, but still adequate, ecological reliability (i.e., $\lambda = .60$; see Table 3.3).

Conservative Political Climate

Deriving a valid measure of political conservatism is notoriously difficult, and there is little consistency in the literature on this issue. Many authors have measured

community variation in conservative ideology by measuring the percentage of county voters that are registered Republicans, or the number of votes cast for Republican candidates in general elections. These are among the most common measures used in previous multilevel sentencing studies examining the effects of conservative political climates, but these measures suffer from some important limitations. First, measuring how voters label themselves does not capture actual voter behavior that may fluctuate as different candidates and issues compete for public attention. Some researchers have understandably criticized using measures of votes cast for Republican candidates, arguing that in today's political climate, no single party is soft on crime (Greenberg and West, 2001). Others scholars have used dichotomous variables indicating the presence of a Republican governor, but state wide elections may be dominated by counties outside the larger areas considered in this study. Too, many Democratic governors have increasingly initiated sweeping criminal justice reforms, leaving little differentiation between the two parties on criminal justice issues.¹⁹ Thus, a better and more direct barometer of political conservatism was computed by aggregating responses to a GSS item that asks respondents: 'We hear a lot of talk these days about liberals and conservatives. I'm going to show you a seven-point scale on which political views that people might hold are arranged from extremely liberal (point 1)- to extremely conservative- (point 7). Where would you place yourself on this scale?' This measure, labeled *Conservative Political Ideology*, represents the mean level of political conservatism (or liberalism) for each county. Though not used in previous multilevel sentencing research, this measure of political orientation is commonly included in research that uses the GSS to understand

¹⁹ For example, in a move at odds with liberal ideology, Governor Jerry Brown (D) of California was one of the first Governors to abolish parole, along with prison rehabilitation programs.

individual-level variation in punitive attitudes such as support for the death penalty (see e.g., Barkan and Cohn, 1994; Borg, 1997; Unnever, Cullen, and Bartkowski, 2006). At the aggregate level this measure exhibited sufficient ecological reliability ($\lambda = .70$).

Whites' Racial Resentment

As discussed in Chapter two, one of the strongest and most consistent predictors of support for more punitive social control (e.g., capital punishment, criminal justice spending, police force size) is anti-black racial prejudice among whites (Barkan and Cohn, 1994; Cohn et al., 1991; Chiricos et al., 2004; Soss et al., 2003; Stults and Baumer, 2007; Unnever and Cullen, 2007; Unnever et al., 2006). Research in the area of whites' racial prejudice has traditionally distinguished between two dimensions of prejudice. The first type of prejudice identified in survey research consists of overt, or "old-fashioned" prejudice, is characterized by feelings that blacks are genetically inferior as well as a desire to avoid contact with blacks through segregation or by opposing interracial marriage (Stults and Baumer, 2007; Taylor, 1998; Unnever et al., 2006). The second, and subtler, form of prejudice is characterized by whites' denial that blacks' conditions are due to discrimination, and opposition to programs that change the status quo by elevating the social status of Blacks (Schuman and Bobo, 1988; Unnever et al., 2007). In their seminal study exploring the sources behind this contemporary form of racial animosity, Kinder and Sanders (1996) define this form of prejudice as "racial resentment," and find that this resentment is manifested in the belief that blacks are unwilling or unmotivated to improve their condition and are thus, unworthy of government assistance (e.g., affirmative action) (see also Unnever and Cullen, 2007). Bobo and Kluegel's analysis of

GSS responses over time reveal a paradox, whereby this newer “symbolic” form of prejudice has become more widespread even as adherence to traditional “Jim Crow racism” has declined in the past forty years (1993: 443).

Unnever and colleagues (2006) recently analyzed GSS responses and found that this contemporary form of racial resentment, rather traditional prejudice, was significantly and strongly related to support for capital punishment. This has helped refine our understanding of the relationship between racial animosity and punitive attitudes, while also suggesting that contextual and macro level research on the severity of social control should expand their models to include measures of racial animosity in addition to support for harsh sanctions and political and religious conservatism. I examine the effects of contemporary racial resentment on sentencing severity using a standardized additive scale containing items in the GSS that tap respondents’ agreement (either somewhat or strongly) with the following statements: 1) Blacks should work their way up without favors to overcome prejudice ($\lambda = .72$); 2) Black-white differences in socioeconomic status are due to Blacks’ lack of motivation or willpower ($\lambda = .65$); 3) Black-white socioeconomic differences are due to discrimination against Blacks (reverse coded; $\lambda = .69$); 4) The government does too much to help improve the condition of Blacks ($\lambda = .80$); 5) The government is not obligated to give Blacks special treatment to improve the condition of Blacks ($\lambda = .72$); 6) Opposed/strongly opposed to preferential hiring or promotion of Blacks ($\lambda = .60$); 7) and that it’s somewhat or very likely that a Black applicant will get a job or promotion over an equally or more qualified white applicant ($\lambda = .60$). The scale exhibits high internal consistency ($\alpha = .86$) and is consistent with prior research that has measured contemporary anti-black prejudice using

GSS responses (Bobo and Kluegal, 1993; Kinder and Sanders, 1996; Schuman and Bobo, 1988; Unnever et al., 2006; Unnever, Cullen, and Jones, 2008; Williams et al., 1999).

CONTROL VARIABLES

Individual-Level Case Controls

In each multilevel regression model I include a wide array of individual level variables research has shown to influence sentencing dispositions. It is well established that the severity of criminal sentences primarily reflect the seriousness of the offense (Spohn, 2000; Ulmer, 2000). Although the strength of the relationship may fluctuate depending on the race of the defendant or the jurisdiction, the general consensus is that offenders convicted of more serious offenses and those with more serious prior criminal histories are more likely to be incarcerated and for longer periods of time. Failure to control for inter-jurisdiction variability in the seriousness of caseloads may overestimate the relative importance of jurisdictional attributes in determining sentencing outcomes. I control for offense severity with a series of dummy variables that indicate the specific conviction charge, as well as the prior arrest, conviction, and incarceration history of each defendant. The specific coding of these items and other case level controls is summarized in table 3.1. Independent of these legally-relevant characteristics, there is a growing body of research documenting that judges manage uncertainty when determining punishment by relying on decisions made by other justice system actors at the arraignment stage. As a consequence, those defendants denied pretrial release prior are more frequently convicted and incarcerated (Albonetti, 1991; Williams, 2003). I control for pretrial decision-making with a dummy variable that is coded 1 when defendants were

detained prior to sentencing and coded 0 if they were released. Although evidence is mixed, some prior research suggests that extralegal defendant characteristics are associated with sentencing severity (e.g., see Zatz, 2000). I also control for defendant age, race, and sex to account for demographic differences in court caseloads. Research has consistently established evidence of a “trial tax” for those defendants who exercise their right to trial, and that severity of the trial tax is sensitive to jurisdictional attributes (Johnson, 2003; Ulmer and Bradley, 2006). Although there is very little variation in the mode of adjudication among cases in the SCPS (95% plea bargain), a failure to control for plea bargaining rates could yield biased estimates at each level of analysis. I include a dichotomous variable indicating whether the defendant plead guilty or was adjudicated via bench or jury trial. As with mode of adjudication, it is now customary to control for the type of legal counsel defendants secure. Predicting that defendants represented by public defenders may be more likely to secure more lenient sanctions and plea bargain deals, I include a control for whether defendants had a public defender appointed to them versus a private representation or court-appointed counsel from outside the public defender system. Finally, I control for any unobserved sources of temporal variability in punitiveness through the use of dummy variables that indicate the year of adjudication. Controlling for the year of case may help capture unobserved factors influencing sentencing such as election cycles that alter courtroom workgroups, and any bureaucratic changes that impact plea bargaining policies, charging decisions, or the nature of the cases before the court. Net of the contextual measures I describe below, differences across jurisdictions along any of these legal and extralegal dimensions should help explain why some jurisdictions more frequently incarcerate defendants.

County Crime Rate

The leading alternative hypothesis to the theoretical models presented in Chapter 2 is derived from functionalist models of punishment, which argue that the severity of punishment merely reflects differences across communities in levels of crime. As such, the functionalist perspective posits that higher crime rates should legitimately trigger more punitive sentencing within a jurisdiction. When measuring levels of crime and deviance, there is no consensus on what type of crimes to account for in multijurisdictional studies of sentencing. As evident in Table 2.1, some scholars rely on arrest data as a proxy for caseload or the certainty of punishment, some studies account for drug arrests, while some rely on measures of violent crime that should reflect the public's fear and demand for justice. Rather than control for county violent crime rates, I include measures of the overall index crime rate because the majority of felony cases heard before state courts in the US involve non-violent offenses. While violent offenses might understandably trigger fear among the public, so do burglaries, open-air drug sales, and the possession of illegal weapons. Plus, while the violent crime rate provides a valid barometer of how judges might sentence violent offenders, it does not necessarily provide an appropriate means with which to predict how judges will sentence those convicted of fraud, larceny, or minor drug possession charges. Consistent with the bulk of multilevel studies of incarceration, I measure variation in levels of crime by calculating an index crime rate using UCR data accessed at ICPSR. Because two Illinois counties participate in the SCPS each year, I calculated an index crime rate that excluded rapes because data

for these crimes are not available in the UCR due to differences in the definition of rape. To control for yearly fluctuations, I use a three-year average index crime rate.

ANALYTIC TECHNIQUE

METHOD OF ANALYSIS

Consistent with the focus of this project, a series of three-level hierarchical regression models are used to estimate the main effects of individual level and jurisdictional attributes on sentencing severity. These models have become standard for analyzing nested data. Multilevel models adjust for the fact that individual cases from the same county are likely to be more similar, which violates classic regression assumptions of independence among the observations. Multilevel models adjust the standard errors of the parameter estimates for non-independence and overcome any correlation among individual and group-level residuals by modeling unique error term for each level-one and higher level unit as a function of both individual and aggregate-level variables (Hofmann, 1997). These models are particularly useful because they allow researchers to maintain the appropriate level of analysis when examining the effects of group-level variables, providing significance tests for level 2 parameters that are based on the appropriate level-two sample size instead of the level-one sample size.

As mentioned previously, each of the 26,354 cases is nested within 91 county-years and 19 states. Due to the small number of states at level three ($n = 19$ states), only a random intercept parameter is modeled at level 3. What this means is that even though some of the jurisdictional attributes are actually state-level characteristics (i.e., legal statutes), these attributes are included at level 2 as a county level contextual measure.

Using determinate sentencing structure as an example, the level-two slope coefficient for determinate sentencing provides an estimate of the effect of being processed in a *county* court that operates under determinate sentencing, which is a partly a function of state-level variation in the overall odds of a particular sentencing outcome, plus county-level error. The primary purpose of each main effects model estimated is to assess the effects of Level 2 variables net of level-one characteristics. Thus, with the exception of several cross-level interaction models that were estimated, the Level-one slopes are treated as fixed in these models. Fixing the slopes assumes that the effects of all Level-one variables are assumed to be the same across counties and states.

Because the dependent variable consists of a categorical measure of three possible sentencing outcomes (i.e., prison vs. jail, prison vs. probation, and jail vs. probation), I estimated a series of HGLM (hierarchical generalized logit models), which are appropriate for nominal data. The HGLM for multinomial outcomes is an extension of the basic linear regression model. When the data are sampled from a multinomial distribution, the HLM program uses the log-link function to predict the odds of being in the m -th response category versus M -th reference category (Raudenbush et al., 2004: 101). In equation form, the log-link function is expressed as:

$$\eta_{mij} = \log\left(\frac{\phi_{mij}}{\phi_{Mij}}\right)$$

where phi (ϕ) represents the expected level-one outcome, which is the predicted probability of a observing a given sentencing outcome versus the reference category (M). For each set of models presented in the next chapter, two sets of multinomial regression models were estimated which provide the odds of three possible outcomes occurring: a) prison vs. jail, b) prison vs. probation, and c) jail vs. probation. Using the model for

prison versus probation as an example, the level 1 multinomial equation predicting the odds of prison (the ‘1’ category) versus probation (the ‘3’ category) is modeled for each defendant as a function of the county-level average odds of prison, a series of individual level predictors (X_{ijk}), plus random error (e):

$$\log(\varphi_{1ijk} / \varphi_{3ijk}) = \pi_{0,jk(1)} + \pi_{1,jk(1)}X_{1ijk} + \pi_{2,jk(1)}X_{2ijk} + \dots + e_{ijk(1)}$$

where $\pi_{0,jk(1)}$ is the mean log-odds of prison versus probation of county j in state k , and $e_{ijk(1)}$ is the level-one error term for each felony case. This error represents the deviation of each criminal case from the county-level average odds of prison. In the Level-two model the average log-odds of prison versus probation for each county ($\pi_{0,jk}$) becomes an outcome, which varies randomly around the county’s state-level mean log-odds of prison. This level-two model takes the basic form:

$$\pi_{pjk(1)} = \beta_{p0k(1)} + \beta_{pqk(1)}W_{qjk} + \dots r_{pjk(1)}$$

where $\beta_{00k(1)}$ is the mean log-odds of prison versus probation in state k and r_{0jk} is a random county-level effect that is the deviation of county jk ’s mean from the state mean odds of prison.²⁰ The level-three model estimates variability in the mean odds of prison among states. The level-three model takes the form:

$$\beta_{pqk(1)} = \gamma_{pq0(1)} + u_{pqk(1)}$$

where each state’s mean odds of prison versus probation is a function of the overall grand mean odds of prison ($\gamma_{000(1)}$), and a unique error term for each state (u) represents the deviation of each state’s estimated mean odds of prison from the overall grand mean.

Additionally, a series of random coefficient models and cross-level interaction models are estimated to examine the potential for any conditional relationships between

²⁰ Formulas for the basic three-level model are provided in Raudenbush and Bryk (2002: 229).

defendant prior criminal history, three-strikes laws and presumptive sentencing guidelines. In these cross-level interaction models, the slope parameters for two level-one indicators of prior record (having a prior felony conviction and having served a prior prison term) are allowed to vary across county-years at level 2 (they remain fixed at level three since there are no measures of legal context at level three to form a cross-level interaction). These models are discussed in greater detail in Chapter 4.

CENTERING

Because the focus of the analysis is estimating the effects of jurisdictional attributes on sentencing after adjusting for variation in the composition of county and state caseloads, all level-one variables were grand-mean centered for two reasons. First, centering reduced some of the instability stemming from moderate to high collinearity among several of the variables measuring prior criminal history. Second, grand mean centering is more appropriate than group-mean centering when the focus is on estimating the effects of level-two predictors net of compositional effects at level-one. This is because group-mean centered models lead to level 2 parameter estimates that confound individual and contextual level effects (Kreft and De Leeuw, 1998; Wu and Wooldridge, 2005). Depending on the theoretical goals of the research, Raudenbush and Bryk (2002: 34) also note that it is often useful to center level-one dummy variables, even though a value of zero for these variables is meaningful. If left uncentered for example, the intercept becomes the average odds of incarceration for any case scoring a zero on the dummy variables (e.g., the odds of prison for females, or for those with no prior criminal history), rather than providing the more useful estimate of the overall average odds of

incarceration that is adjusted for differences across counties in the proportion of court cases where the dummy variable (e.g., black defendant or male defendant) equals '1.' Under grand mean centering, the model intercept provides an estimate of the odds of incarceration for the average defendant processed in a "typical" county, and represents between-county variance in the average odds of the outcome after partialling out the effect of level-one case characteristics (Heck and Thomas, 2000; Hoffman and Gavin, 1998; Paccagnella, 2006). This allows one to gauge how counties and states deviate from the average or "normal" propensity to mete out incarceration terms (prison or jail) versus probation. Grand-mean centering the level-one predictors does not change the interpretation of the level-one slope estimates. The decision to center level-two variables is much less complex because the gamma coefficients can be easily interpreted in either raw or centered form (Enders and Tofighi, 2007; Raudenbush and Bryk, 2001: 35). However, it is particularly useful to grand-mean center continuous level-two predictors around their grand mean if any of the predictors are moderately or strongly correlated.²¹ In each of the models estimated, continuous level-two variables are centered at their grand-mean and dummy variables are level-two are not centered. This means the model intercept can be interpreted as the overall odds of a given sentencing outcome for the average defendant processed in a "typical" county that is not located in the south and that does not have present the structured sentencing schemes considered in the models (e.g., three-strikes, guidelines, and determinate sentencing).

²¹ Several level-two variables exhibited bivariate correlations that exceed .65. This was particularly the case for measures tapping state variation in penalties for sale and possession of cocaine ($r = .70^*$). All correlations among level-two variables are provided in Appendix B.

MODELING STRATEGY

Following the advice of Raudenbush and Bryk (2002), the analyses proceeded in several stages. First, an unconditional model was fitted which estimates the overall grand mean log-odds of being sentenced to prison vs. jail, prison vs. probation, and jail vs. probation. The unconditional model contains no level-one or level-two predictors and is a useful first step towards estimating the initial degree of cross-jurisdictional variation in the outcomes. If no such significant variation exists, then all regression models can be estimated using standard fixed-effects regression models that control for only level-one covariates. In the second step, all grand-mean centered level-one predictors were entered into the model to assess whether and to what degree any observed variation in the outcome across counties and states can be attributed to compositional differences across jurisdictions in the types of cases processed. The results of this model also provide a revised estimate of the overall mean-adjusted (for level-one covariates) log-odds of each sentence type. The next phase of the analysis focuses primarily on fitting a series of models that introduce blocks of theoretically relevant contextual attributes at level 2. Raudenbush and Bryk recommend that model building at level 2 be “theory-driven,” and that when the number of level 2 predictors is large relative to the number of level 2 groups researchers should consider “dividing the level-two predictors into conceptually distinct subsets and fitting a submodel for each. The strongest predictors from these submodels might then be combined in an overall model” (2002: 267). The results presented in Chapter 5 are displayed this way, beginning with legal and organizational models (the factors that should, in theory, exert the most proximate influences on court decision making), followed by a conflict model of sentencing, and then a punitive social

climate model that contains measures of public sentiment and trust. A final trimmed model is then estimated that retains significant level-two predictors from each theoretical “block,” along with all level-one case controls.

METHODOLOGICAL LIMITATIONS

Potential Selection Bias in the SCPS

The issue of sample selection bias is common in sentencing research, where the odds of reaching later stages in the adjudication process are not random across defendants. In nearly every study of legal decision making the sample of defendants facing a decision of incarceration represents a nonrandom sample of suspects that come to the attention of police and prosecutors. The SCPS is no exception. As seen in Figure 3.1, there is a considerable amount of sample attrition as cases move through the justice system. This merits scrutiny in the current study given the relatively high rate of dismissals (which can be observed in the SCPS) in some jurisdictions that resulted in the conviction of fewer and potentially different types of defendants.

A more pressing problem concerns the interplay between patterns of nonrandom selection with *omitted*, or “unobservable” variables that influence both the selection process (conviction) and the substantive outcome (incarceration). If samples for which we observe the outcome are not a random sub-sample of the population under study, Heckman (1976) and others have demonstrated that using only the observed cases can generate potential bias in the regression estimates (see also Bushway et al., 2007; Stolzenberg and Relles, 1997). Censored data do not necessarily yield biased regression estimates. Rather, potentials for bias arise from unobserved factors that underlie both the

selection and substantive outcomes. In the case of incarceration, unmeasured factors that influence the likelihood of conviction and the decision to incarcerate will enter both equations' error terms, inducing a correlation between the two (Bushway et al., 2007). This interdependency between processes of conviction and incarceration not only threatens external validity; it potentially biases all other parameter estimates in the equation. This occurs when the omitted factor related to both selection and the outcome is also correlated with another predictor such as race or sex. If this is the case, it becomes difficult to determine the "true" effect of defendant race or prior criminal history when predicting the likelihood of incarceration.

This is not a trivial issue in sentencing research where detecting the potential sources and extent of racial disparity or the impact of having a prior criminal record are of great interest. The threat of selection bias is virtually ubiquitous in criminology, a process Berk (1983: 392) terms "infinite regress."²² The question becomes whether this bias is small enough to ignore. If, for example, the strength of evidence and the odds of conviction are independent of the decision to incarcerate then this censoring is not a problem and the two processes can be modeled separately. A more acute problem arises if strength of evidence is unobserved *and* related to conviction and incarceration. The sub-sample of defendants that select into the incarceration stage effectively "carry" that unobserved error term with them. In this way, Heckman frames the problem of selection bias as a more complicated instance of omitted variable bias, where omitted variables

²² For example, in any study of formal social control, consider that only half of all crimes are typically reported to the police in any given year, that only some of these will result in a clearance by police, and so on. This even extends to studies of parole decision-making where the inmates *eligible* for parole are not a random subset of those defendants the state convicts and sends to prison. Likewise, any given longitudinal study of delinquency will experience some attrition of participants; particularly among those most at risk of delinquency.

predicting selection processes “interact” with the predictors in the substantive equation. Because the sources of selection bias are unobservable, it remains unclear the precise degree to which it threatens the validity of the parameter estimates for key exogenous regressors such as race. As Berk aptly puts it, in the presence of selection bias “one is essentially flying blind.” (1983: 390).

Although its application has been inconsistent, researchers typically employ the Heckman two-step method in an attempt to alleviate selection bias. This involves first estimating the odds of selection, and then including a selection “correction factor” in all substantive equations. Unfortunately, there are obstacles to implementing this approach for the current study. There is no formal statistical procedure for the two-step correction of censored discrete outcomes that allows for the calculation of a corrective factor (i.e., the inverse Mills Ratio) that then could be imported into a multilevel statistical package and included as a control in analyses of incarceration. Moreover, there is no estimator (two-step or otherwise) yet available that allows for any formal diagnoses of selection bias for nominal dependent variables. With no clear statistical remedy, I referenced earlier scholars’ treatment of selection bias for discrete outcomes, but the issue either garnered little discussion or scholars incorrectly implemented the Heckman two-step estimator or other “ad hoc” methods (for a review, see Bushway et al., 2007).²³

²³ In large part, researchers have limited their discussion on the sensitivity of their results to unobserved selection processes that impact *sentence length* equations. Researchers rarely acknowledge selection processes that may bias incarceration equations, especially in studies of jurisdictional variation in incarceration decisions. This reticence is understandable for a few reasons. Many large-scale sentencing databases only include samples of defendants who were ultimately convicted and sentenced, precluding attempts to gauge the degree of nonrandom censoring that occurs during the conviction stage. Second, and from an epistemological standpoint, problems with selection bias tend to be viewed through the narrow lens of understanding *sentencing* behavior, rather than criminal justice decision-making generally. As Bushway et al. (2007) point out, concerns of incidental selection bias are often predicated upon the assumption that judges’ decisions regarding incarceration and sentence length occur in two stages, both of which contain varying degrees of discretion. Perhaps because relatively less empirical attention is devoted to decisions to

Notwithstanding those limitations, I attempted to gain some intuition about the extent of selection bias at the incarceration stage using a derivative of Heckman's original estimator without the second step of calculating and saving the inverse Mills ratio.²⁴ Using Stata's *heckprob* command, I estimated the log odds of receiving incarceration *conditional* upon factors that maximize the likelihood of conviction (which is modeled in a separate first-stage probit equation). The model produces a coefficient, rho (ρ), which is an estimate of the degree of correlation between the error terms of the two equations (conviction and incarceration). While the "true" residual correlation is by definition unknown, examining the size, direction, and significance of the rho parameter helps to diagnose the general direction and magnitude of selection bias (Stolzenberg and Relles, 1997). Rho should be interpreted with caution though, as these estimates have been found to be extremely sensitive to model specification in simulation studies (e.g., Sartori, 2003). Keeping such cautions in mind, any observed non-zero correlation between the residuals suggests the two stages are related, and that unobserved factors not captured in the model explain this interdependence. Theoretically, rho ranges between -1 to +1 and the interpretation of rho is similar to the interpretation of a Pearson's correlation coefficient (Stolzenberg and Relles, 1997: 498). Although the "rules of thumb" for interpreting rho seem vague, Stolzenberg and Relles (1997) argue that a residual correlation closer to zero suggests a considerable amount of *random*

arrest, prosecute, and convict in the criminal justice system, sample selection bias often is limited to discussions of sentence length and relatively absent from studies regarding the decision to incarcerate.

²⁴ The Heckman two-step corrective method was originally applied to the problem of studying wages (a continuous outcome), rather than discrete outcomes. This restriction to continuous outcomes is necessary because the calculation of the inverse Mills ratio (or, lambda) that derives from the first step (the probit equation predicting selection) requires several estimated parameters (the product of sigma and rho), one of which (sigma) is a function of a standard normal probability density function. Ostensibly, this is the reason Stata does not calculate sigma or lambda following the 'Heckprob' procedure (the same is true of SAS).

unobservable factors determining the two processes. A residual correlation of 1 would occur if selection (i.e., conviction) and the substantive process (incarceration) were “accomplished by identical processes” (pp. 499). Given the interdependency of criminal justice system decision-making, I expected to observe a significant rho correlation coefficient. However, in the SCPS sample judges do not often make both the determination of guilt and incarceration since most guilt verdicts are pleas. Therefore, the rho correlation should not be extremely high (i.e., exceeding .80). In fact, in the SCPS 95% of convictions result from guilty pleas extended on behalf of prosecutors, suggesting that different processes should underlie the conviction and incarceration outcomes to a good degree.

I fit a Heckman’s probit model in Stata with the same level one coefficients included in all multilevel regression equations along with dummy indicators of jurisdiction.²⁵ The estimated residual correlation (ρ) is $-.67^*$ ($p < .05$). This suggests a moderate level of correlation between the processes of conviction and incarceration. The significance of rho is generally interpreted as evidence that the Heckman estimator should be used to fit the data. Not surprisingly, the negative direction of rho suggests that there are one or more unobserved factors that make conviction *more* likely, but incarceration *less* likely. There are two modal case dispositions in the SCPS- dismissal

²⁵ Following the advice of Bushway et al. (2007) I attempted to locate theoretically and empirically reliable exclusion restriction to enter into the selection equation predicting the likelihood of felony conviction. An exclusion restriction is a variable that theoretically should be related to the odds of conviction, but not to the decision to incarcerate. The exclusion restriction should help identify the sources and extent of incidental selection and hopefully, help to reduce any residual correlation between the selection and substantive equations. I considered three exclusion criteria: the speed of the process between arrest and conviction, the inability to afford pretrial release, and being charged with multiple offenses. These are the only data provided in the SCPS that might be related to conviction, but perhaps not incarceration. Unfortunately, these measures failed to reduce the initial rho correlation I obtained from models without exclusion restrictions (i.e., they do not appear to compensate for unobserved factors that determine selection) and, from an empirical standpoint, were observed as being significantly related to the risk of incarceration when modeling incarceration separately.

and guilty pleas. Thus, the court rarely decides guilt. The negative residual correlation suggests that some factor other than race, sex, criminal history, offense severity, and pretrial incarceration motivates defendants to plea, but lowers their odds of incarceration. Intuitively, this makes sense and it underscores how urban courts function. This pattern is consistent with Feeley's (1992: 27) portrait of adjudication as a "mixed strategy game," where plea bargains and dismissals allow prosecutors and defense attorneys to share in the gains and losses of case processing, while also expediting heavy caseloads. Plea bargains avoid the cost of trial, the uncertainty of losing at trial and the uncertainty of the punishment that awaits the defendant if they exercise their right to trial. On the other hand, if the case is dismissed the defendant avoids the stigma of conviction, and the prosecutor avoids a very public loss that may have resulted at trial. Thus, estimating community variability in punitiveness may require the consideration of court differences in their "success" at processing cases. In conclusion, I should stress that this rho correlation should be interpreted with caution and I present it only in the spirit of addressing potential selection bias in a more transparent way given its lack of attention in previous studies of incarceration. Furthermore, it does not provide a clear sense of whether this residual correlation may bias the meaning we infer to some of the individual level characteristics such as defendant race and ethnicity for predicting incarceration.²⁶ Despite that lingering ambiguity and no readily available means of empirically addressing it, estimating the Heckman probit does provide important intuition about the degree of integration between adjudication and sentencing outcomes. These diagnostics are not definitive by any means, but do suggest that some key unobserved factor may be

²⁶ With a continuous outcome, one could examine the correlation between lambda and defendant race and/or ethnicity. Unfortunately, that is not possible in cases where the outcome is discrete, since lambda cannot be calculated.

informal prosecutorial bargaining practices and organizational case processing strategies. Thus, any community level influences on sentencing behavior likely reflect to some degree how the courts informally process cases, which cannot be observed with these data.

Missing Data and Imputation Procedures

As is often the case in large data collections, the SCPS suffers from missing data. Some scholars rely on multiple imputation procedures in both individual (Demuth, 2003) and multilevel studies using the SCPS (Pardoe and Weidner, 2006). Some listwise delete all cases with any missing values (Schlesinger, 2005; Tomic and Hakes, 2008; Weidner et al., 2004; 2005).²⁷ Others do not address missing data, or the degree of sample attrition caused by it (Fearn, 2005; Maxwell, Robinson, and Post, 2003). I explored several options for handling missing data in the hopes of retaining statistical power while simultaneously generating valid regression estimates. Below I describe how I handle missing data in the current study. I address three issues. First, what is the extent of the problem? Second, are the data missing at random? Third, I describe the methods and rationale for the use of multiple imputation procedures in the current study.

It is customary in criminological research to handle missing data using listwise deletion. Preliminary individual-level regression models predicting the likelihood of incarceration resulted in the listwise deletion of 6,045 cases (23% of the initial sample of

²⁷ In at least one instance when researchers used only one year of the SCPS (Weidner et al., 2004), listwise deletion appears to have resulted in a loss of nearly half of all cases where the defendant had been convicted and the case was otherwise eligible for inclusion. Other authors did not report what the extent of attrition resultant from listwise deletion. Therefore, the results reported herein may not be compatible with that of prior published work.

26,354 defendants).²⁸ Over 4,000 of these cases were dropped from the analyses due to missing data on only one measure. The most common sources of missing data are items tapping race and ethnicity (5%), prior arrest (6%), prior prison sentences (7%), and type of defense counsel (7.5%).²⁹ Deleting over 6,000 cases also would remove from the analyses five to seven counties that have smaller caseloads. Thus, listwise deletion results in a considerable loss of data at both the defendant and county level, and reduces variation in the types of jurisdictions included in the study by disproportionately removing cases in jurisdictions with smaller caseloads.

There are several processes thought to generate incomplete data and these processes determine the most appropriate method of handling missing values. These processes can be classified in three ways.³⁰ The first is when data are missing “completely at random” (MCAR). MCAR narrowly defines the missing value process as being unrelated to *any* of the factors (X or Y) in the data matrix. In other words, missing values are equally likely for all groups. In practice, data are rarely MCAR. If the data are MCAR then listwise deletion is an inefficient use of the data, but it will not bias the parameter estimates since each case will have a random probability of being deleted. Unfortunately, missing data in the SCPS cannot be treated as MCAR. A t-test of mean differences indicated that those with missing data statistically differed from those with

²⁸ This analysis was restricted to defendants who had valid data for their conviction decision (since it determines the sample) and the outcome (prison, jail, probation). Any additional losses that would occur from missing data on those measures are not included in the figure above since those two measures are considered outcomes of the criminal justice system, and imputed values are not appropriate for outcomes.

²⁹ In particular, the codebook for the SCPS indicates that in 2004 prior arrest data were not available in four New York counties (Kings, Bronx, Nassau, and Queens). Fortunately, each of these counties appeared in previous years of the SCPS, leaving open the option that multiple imputation procedures could simulate imputed values for these counties informed by cases disposed in these counties prior to 2004.

³⁰ This is leaving aside for the moment processes such as censoring and when data are “missing by design” where respondent fatigue may prevent the completion of items towards the end of a survey.

complete data, and predictably, certain jurisdictions are more likely to contain missing cell values on indicators of race, prior criminal history, and attorney type.

A more realistic assumption is that the data are ‘missing at random’ (MAR). Data are MAR if the likelihood that an indicator is missing is unrelated to the unobserved value of that indicator, controlling for all other attributes (Acock, 2005; Little and Rubin, 1987; see also Raudenbush and Bryk, 2002: 199). For example, under assumptions of MAR the odds that a defendant is missing information on race cannot be the result of a process where coders systematically omit values for race if the defendant was Hispanic. Furthermore, other variables in the data should provide a “mechanism” to help to explain whether or not a respondent (or a jurisdiction) provided information (Acock, 2005; Schafer, 1997). In the SCPS court jurisdiction helps to explain why some criminal history data are more readily available in some jurisdictions. According to the codebook, some courts have computer access to FBI databases, while others are limited to local or state databases (1990-2004 codebook, ICPSR cite).³¹ Therefore, if the likelihood that prior criminal history is missing is a function of jurisdictional access to records but *not* a defendant’s actual unobserved criminal history, the data are MAR. Multiple imputation procedures, if carefully specified, can provide efficient and unbiased estimates when data are assumed to be MAR (Acock, 2005; Allison, 2002; King et al., 2001; Little and Rubin, 1987; Schafer, 1997).

³¹ Most discussions of missing data surround survey-based concerns of item non-response. Maltz provides a useful example of agency-based MAR processes (2007: 278). He describes several plausible scenarios by which UCR data could be incomplete. UCR statistics may be MCAR if a natural disaster strikes a state and those jurisdictions lose their data. Crime figures can be considered MAR when computer problems arise, such as the transition from the UCR reporting program to the more complicated NIBRS program. During these transitions, some agencies do not have access to UCR or NIBRS systems, resulting in non-reporting of some or all crime statistics for a particular year.

Last, missing data can arise from “non ignorable” (NI) or, alternatively, ‘not missing at random’ (NMAR) processes (Little and Rubin, 1987). Under these conditions, data are missing as a function of the unobserved value (e.g., income is missing only for low-income respondents) *and* other variables in the dataset cannot predict which cases are more likely to have lower incomes (i.e., the other explanatory variables cannot predict which cases will be missing). This is a more problematic process generating incomplete data for which neither listwise deletion nor multiple imputation procedures will provide unbiased estimates.

Missing Data Procedures in the Current Study

There are several ways to handle missing data, each of which depend on why the data are incomplete (i.e., MCAR, MAR, or NI). I carefully weighed the pros and cons of using listwise deletion versus multiple imputation, and with the exception of one item (defendant race) I estimated all multilevel models with imputed data.

Scholars strongly advise against using listwise deletion unless data are MCAR (King et al., 2001; Little and Rubin, 1987; Schafer, 1997). Intuitively though, listwise deletion is appealing because 1) it is convenient, 2) it has become standard in studies of legal decision making, and 3) it avoids the perception one is “making up” data. Moreover, in one simulation study Allison (2000) reports that listwise deletion yielded virtually no biased estimates when data were MCAR *or* MAR.³² On the other hand, other scholars have shown that listwise deletion does yield significantly biased regression

³² There has been some distortion of Allison’s findings in several studies where authors advocate for listwise deletion instead of multiple imputation. Allison’s now well-known “cautionary tale” was in reference to using multiple imputation estimates produced by the *SOLAS* program. However, the multiple imputation estimates generated by Schafer’s *NORM* program performed exceptionally well, and in several instances better, than listwise deletion.

coefficients (King et al., 2001: 51). Listwise deletion may also bias the results due to a loss of statistical power. For instance, simulations by King et al. (2001: 64) demonstrate that listwise deletion often produces results driven by a *lack* of information rather than a “true” relationship.

Multiple imputation (MI) has two appealing properties that alleviate these limitations. MI yields more efficient estimates by using all of information available to identify the conditions under which data are missing. Listwise deletion not only discards valuable information for each case, it assumes a very precise structure to that loss of information that is not reflected in the standard errors. Multiple imputation programs integrate that uncertainty by adjusting the standard errors for variability in the precision of imputed values. Second, under MAR assumptions, MI corrects for biases shown to result from ‘ad hoc’ methods such as case deletion and mean replacement. In principle, we cannot directly test the assumption that random processes generate missing values. However, *all* analyses that contain missing data rest on these untestable assumptions (Kenward and Carpenter, 2007: 204). Thus, potentially biased estimates coupled with the loss of statistical power warranted the extra step of imputation, and at the very least, comparing the results of models using listwise deletion versus imputed data.

Multiple imputation procedures are now integrated into Stata, S-PLUS, and SAS, and as well as in a few specialized software packages such as NORM (Schafer, 1997) and Amelia (Hornaker, King, and Blackwell, 2010). Two multilevel software packages (Mplus and HLM) permit the analysis of imputed datasets but do not actually impute data. I imputed five datasets using the ICE program developed by Royston (2005) and freely available for implementation in Stata. While the computational algorithms are

available elsewhere (e.g., see Royston, 2004), ICE provides a relatively user-friendly method of imputation that can be distilled down to three steps. First, ICE estimates imputed values for all items containing missing cells. More formally, the program uses “regression switching,” an iterative procedure that involves specifying separate regression equations to estimate values for all incomplete items. The program generates missing values by repeatedly sampling from the posterior distribution of the item using the observed values for that item in combination with a set of explanatory variables (Yu, Burton, and Rivero-Arias, 2007). The program then produces m number of data sets where observable values remain unchanged and imputed values are inserted into missing cells. The imputed values somewhat vary across each dataset to reflect uncertainty in the estimates. Imputation is more precise with large samples that contain relatively few missing values on any given item. Last, each imputed dataset was brought into HLM for regression analyses. HLM fits the substantive model using information from each dataset. The estimates are then pooled together to adjust for variability in the precision of each imputation iteration performed in Stata.

As Allison (2000: 302) points out, specifying multiple imputation models must be empirically and theoretically “correct in some sense.” Imputation models that contain less information than their analysis equations may bias the imputed values.³³ A common misstep is a failure to omit the outcome in the explanatory portion of the imputation model as it helps identify the formal “structure” of the data (i.e., the joint distribution of X and Y) (Acock, 2005; Kenward and Carpenter, 2007; Royston, 2005).³⁴ I specified

³³ King et al. (2001: 56) argues, “When the information is greater in the imputation analysis model, multiple imputation is more efficient than even the “optimal” application-specific method. This is the so-called efficiency property (see also, Rubin, 1996).

³⁴ While I include information on the outcome in the explanatory portion of the imputation equation, missing values for incarceration are not imputed since MI is only appropriate for missing explanatory variables.

models that included all variables to be included in the sentencing analyses, as well as information on pretrial decisions, initial charging offense, and any misconduct on the part of the defendant while on pretrial release.

Finally, although I initially imputed race and ethnicity, I do not include any imputed values for this item in my final regression models. I did this for two reasons. First, it is not clear to me how one reliably imputes race.³⁵ I searched for studies on this issue, and there is very little work on the implications of imputing race in social science settings. Hopefully, as MI grows increasingly popular researchers will devote simulation studies specifically to this issue. Second, because race and ethnicity are a central focus of this dissertation, I did not want to complicate efforts to compare the results to prior work that uses the SCPS in analyses of race and sentencing. In lieu of imputation, I opted to include a dummy variable where a value of 1 indicates the defendant was missing this information and 0 if the case contained valid data (see e.g., Cohen, Cohen, West, and Aiken, 2003). When the model is estimated, the results for race and ethnicity will resemble the same as those that would result from listwise deletion, but retain virtually identical statistical power in large samples (Acock, 2005). Table 3.4 provides an item-specific count of missing values, and identifies items used in multilevel regression models that contain imputed values.

To assess the sensitivity of the results to differences in imputed versus complete case (CC) samples, I estimated binary regression models of incarceration with and without imputed data. Table 3.5 provides a comparison of these estimates. I urge caution

Likewise, while I included information indicating if the defendant was convicted, I did not use any imputed values for conviction status in the regression models since this outcome determines my sample.

³⁵ I did impute defendant sex and kept those cases with imputed values since there were so few defendants missing this information (n= 14).

in conferring too much meaning to these comparisons, since CC samples only provide biased and efficient estimates when the data are MCAR. I present these comparisons to assess the implications of relying on imputed data. As I expected, the standard errors are lower (due in part to retaining a larger sample) and some of the estimates shift in magnitude. The overall theme though, is the results are generally similar, although the effects of race and ethnicity increase somewhat in the imputed models, and the effect of having a prior misdemeanor conviction is considerably attenuated because imputation increased the number of cases with valid data on prior arrests and incarcerations. The dummy variable indicating that race/ethnicity was missing is positive and significant, suggesting the odds of incarceration are significantly higher for these defendants, although for unknown reasons. Given these similarities and the desire for statistical power, I use imputed data in all final multilevel models.

Table 3.4. Imputed Data in Sample of Felony Defendants Used in Analyses of Incarceration

<i>(N= 26,354)</i>	# Missing Values	# Imputed Values
<i>Defendant Characteristics</i>		
Race/Ethnicity	1,349 (5.1%)	0
Sex	14 (.1%)	14
Age	392 (1.5%)	392
Most Serious Prior Arrest	1,603 (6.1%)	1,603
Most Serious Prior Conviction	928 (3.5%)	928
Served Prior Prison Term	1,782 (6.8%)	1,782
Active CJ Status at Time of Arrest	1,359 (5.2%)	1,359
<i>Legal/Case Characteristics</i>		
Year of Arrest and Adjudication	0	0
County Jurisdiction	0	0
Type of Attorney	1,971 (7.5%)	1,971
Pretrial Detention	0	0
Mode of Conviction (plea vs. trial)	350 (1.3%)	350
Conviction Offense	0	0
Incarceration Outcome ^a (Prison, Jail, Probation/Fine/Other)	1,340	0

^a There is no corresponding percentage for these missing values because they are not part of the final sample. Dependent variables are not suitable for imputation procedures.

One final issue merits comment. While multiple imputation is becoming increasingly popular in criminology, widely available imputation programs do not explicitly address the nested structure of data and I found little discussion of this issue in prior applications of MI using the SCPS (Demuth, 2003; Demuth and Steffensmeier, 2004; Pardoe and Weidner, 2006) or other widely used datasets where the data are nested and researchers have relied on MI procedures (e.g., ADD Health). Ideally, one could perform multiple imputation within HLM in a way that exploits the hierarchical structure of the data and its variance. Unfortunately, this is not yet possible. To date, HLM

merely analyzes imputed data, and has not yet integrated algorithms to impute clustered data.³⁶ To help alleviate any sensitivity to unobservable jurisdictional factors, I specified MI equations that included dummy indicators for county jurisdiction. That is, the estimate for an imputed value is determined in part by where the case was adjudicated.

Table 3.5. Predicting the Likelihood of Incarceration With and Without Imputed Data

³⁶ Recently, scholars have integrated a multilevel imputation algorithm into MLwin statistical package. While its use is documented in simulation studies involving large (40%) amounts of missing data, I uncovered no published research that actually implemented this technique to impute hierarchical data. Hopefully, as this application moves from its infancy there will be more opportunities to evaluate its use in common sources of social science data.

Predictors	Listwise Deletion	MI
	β	β
Race/Ethnicity Missing	--	.173*
(Reference= White, non-Hispanic)	--	(.086)
Black, non-Hispanic	.184*	.235*
	(.051)	(.044)
Hispanic, any race	.301*	.355*
	(.059)	(.051)
'Other', non-Hispanic	.032	.122
	(.161)	(.124)
Age	-.007*	-.003*
	(.002)	(.002)
Age (squared)	-.0003	-.0004
	(.0001)	(.0001)
Male	.369*	.368*
	(.050)	(.042)
Prior Felony Arrest	.214*	.197*
	(.058)	(.045)
Prior Felony Conviction	.287*	.263*
(Reference=No Prior Convictions)	(.068)	(.052)
Prior Misd. Conviction	.168*	.089
	(.061)	(.050)
Served Prior Prison Term	.921*	.773*
	(.067)	(.055)
Active CJ Status at Arrest	.176*	.191*
	(.048)	(.039)
Pretrial Detention	1.18*	1.18*
	(.043)	(.036)
Private Attorney	-.086	-.036
	(.051)	(.043)
Guilty Plea	-.117	-.149*
	(.082)	(.072)
Constant	.384	.559
	(.268)	(.227)
<i>N</i>	20,309	26,354
Log pseudolikelihood	-8067.01	-11268.33
Wald χ^2	3568.01*	4461.95*
McFadden's R^2	.26	.23

Note: Both models include controls for jurisdiction and year of adjudication.
 Robust standard errors reported in parentheses.

SUMMARY

To summarize, the data used in this study to understand jurisdictional variation in sentencing severity come from the State Court Processing Statistics. These data were used to construct a three-level hierarchical database that nests 26,354 criminal cases within 91 level-two county-years, which are in turn nested within 19 states. Data describing the legal, organizational, and social climate in which defendants were sentenced were obtained from a variety of secondary and original sources. In particular, this study extends prior research by including measures of legal context contained in the Vera Institute Fragment and Ferment sentencing policy database and aggregated GSS survey responses that more directly capture areal variation in punitive public sentiments and levels of mutual trust. Using these data, hierarchical multinomial regression models are estimated to examine 1) The extent to which the probability of three different sentencing outcomes varies across county and state court jurisdictions, 2) whether compositional differences in caseloads explain this variability, and 3) the role that court, community-level, and state context play in explaining jurisdictional variation in sentencing outcomes. In addition to exploring the main effects of jurisdictional context on sentencing outcomes, a series of cross-level interaction models are also estimated to explore whether presumptive guidelines and the presence of three-strikes laws significantly impact sentencing outcomes indirectly by strengthening the relationship between prior criminal history and sentence type. The results of these analyses are presented in Chapter 4. Chapter 5 concludes the study by briefly reviewing the contributions of the study, summarizing support for the various hypotheses examined, and discussing the study's limitations.

CHAPTER 4

RESULTS

INTRODUCTION

This chapter reports results of descriptive analyses, unconditional hierarchical multinomial regression models, and fully-specified random intercept and random coefficient models predicting the log-odds of receiving three possible outcomes: 1) a prison versus a jail sentence, 2) a prison versus a probation sentence, and 3) a jail sentence versus probation. The discussion of the findings proceeds in four steps. First, the results of descriptive analyses are presented and briefly discussed. Second, I present findings from the unconditional model predicting the overall probability of receiving each sentence type. The chapter then proceeds with models aimed at explaining the observed jurisdictional variability in the outcome. I begin with discussing a random intercept model including only fixed level-one effects. I then present the findings from fully specified random intercept models that estimate the main and conditioning effects of level-two variables while adjusting for all level-one characteristics. Legal context models are presented first since legal factors should, in theory, exert the most powerful and proximate influences on sentencing outcomes. The results for models examining the effects of organizational context are shown next. The analyses then shift to examining the effects of community social context, beginning with the social conflict model of sentencing, followed by models estimating the effects of public sentiment and ideology on sentencing. The chapter concludes with a summary of support for the hypotheses and a final trimmed model is estimated that contains only those level-two variables found to be significant in earlier models.

DESCRIPTIVE RESULTS

Table 4.1 displays descriptive statistics for variables included in the models predicting the odds of prison, jail, and probation. Thirty-eight percent of defendants are sentenced to prison upon conviction, a figure equal to that for jail. Though not reflected in the figures in the table, it is important to point out that the majority of these jail sentences (76%) were combined with a probation sentence to be served upon release from jail. For approximately one quarter of defendants (24%), probation and fines are the most severe sentence imposed. When combining prison and jail, clearly the modal sentence type for convicted felons is incarceration. However, as illustrated in Figure 4.1 below, the proportion of defendants that are incarcerated varies substantially, ranging from less than a third in some counties to as many nearly all convicted felons (i.e., over 90%) in other jurisdictions.

Figure 4.1. Percent of Defendants Incarcerated Across Counties in the SCPS, 1998-2004 (n = 91)

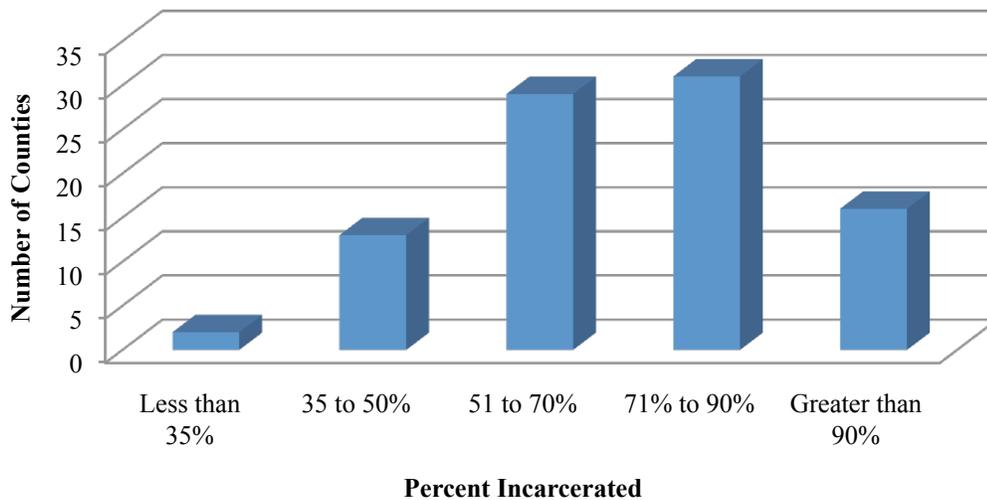


Table 4.1 also displays descriptive statistics for each of the level-one and level-two variables included in the analyses. The results show that the sample is quite diverse. Forty percent of the cases involve a black defendant, while Hispanics make up 23% of the sample, and whites make up just over a quarter of defendants (28%). The average age in the sample is 31 years old, and the sample is primarily male (83%). The sample consists primarily of offenders who have a criminal record. For example, 70% of the defendants have a prior felony arrest, and half of the sample has a prior felony conviction and has served jail time. Twenty-five percent of the cases involve offenders who have already served a prison sentence, and nearly half of the offenders (42%) were arrested while on “active” criminal justice status such as probation or parole. Over a third of all of the cases involved drug charges. The most common offenses for which defendants were convicted were drug possession (22%) and drug sales (16%), followed by theft (10%), driving-related offenses (8%), burglary (8%), and assault (8%). The majority of offenders were arrested on multiple charges (60%) and a high percentage of these defendants were detained prior to conviction (54%). Ninety-five percent of all cases were disposed via a guilty plea.

Table 4.1. Descriptive Statistics for Variables Included in Analysis of Sentencing Outcomes in Felony Cases (N = 26,354)

Dependent Variable	Mean	SD	Explanatory Variables (Cont.)	Mean	SD
<i>Sentence Type</i>			Organizational Climate Predictors		
Prison Most Serious Sanction Received	.38	.48	Jail Capacity Constraints (Operating Capacity)	98.19	17.86
Jail Most Serious Sanction Received	.38	.48	Prison Capacity Constraints (Operating Capacity)	110.98	25.76
Probation/Fines Most Serious Sanction Received	.24	.43	Per Capita Correctional Expenditures (\$)	18884.54	3482.35
<i>Jurisdictional Attributes (N=91)</i>			Average Monthly Probation Supervision Fee (\$)	34.62	23.80
			Prosecutor Caseload Pressure	3.82	2.80
Control Variables			Social Conflict/Threat Predictors		
County Index Crime Rate	5745.73	2285.22	Percent Population Black	17.86	13.88
Explanatory Variables			Percent Population Hispanic	18.94	14.82
Legal Climate Predictors			Percent Population Unemployed	6.87	2.38
Determinant Sentencing	.52	.50	Income Inequality (Gini)	.44	.03
Truth in Sentencing (% Sentence Must Serve)	61.40	26.74	Fear of Crime	47.02	10.37
Presumptive Sentencing Guidelines	.14	.35	Punitive Public Sentiment and Social Cohesion		
Voluntary Sentencing Guidelines	.11	.31	Southern Jurisdiction	.35	.48
Mandatory Enhancements for Violent/Property Crimes	9.77	6.26	Percent Support Capital Punishment for Murderers	64.03	9.03
Three Strikes and You're Out Policy	.44	.50	Mean Level of Social Trust and Cohesiveness	-.09	1.87
Min Sentence (months) for 1 oz. Cocaine Possession	21.63	22.08	Mean Level of Religious Fundamentalism	.06	1.93
Min Sentence (months) for 1 oz. Cocaine Sale	35.80	25.04	Mean Level of Political Conservatism (Scale of 1-7)	4.02	.24
Mandatory Enhancements for Drug Crimes	18.50	25.04	Mean Level of Whites' Anti-Black Racial Animus	.00	5.14

Case-Level Control Variables	Mean	SD	Control Variables (Cont.)	Mean	SD
<i>Defendant Characteristics</i>					
Non-Hispanic Black	.40	.49	Guilty Plea Entered	.95	.22
Hispanic, any race	.25	.44	Convicted of Murder	.01	.07
Non-Hispanic White	.28	.45	Convicted of Rape	.01	.09
'Other' Race/Ethnicity	.02	.14	Convicted of Robbery	.04	.20
Male Defendant	.83	.38	Convicted of Assault	.08	.27
Age (in years)	31.21	10.25	Convicted of 'Other' Violent Offense	.04	.19
Active Criminal Justice Status	.42	.49	Convicted of Burglary	.08	.27
Prior Felony Arrest(s)	.70	.46	Convicted of Larceny/Theft	.10	.30
Prior Misdemeanor Arrest(s)	.70	.46	Convicted of Motor Vehicle Theft	.04	.20
Prior Felony Conviction(s)	.50	.50	Convicted of Fraud/Forgery	.06	.24
Prior Misdemeanor Conviction(s)	.55	.50	Convicted of Other Property Offense	.05	.21
Served Time in Jail	.50	.50	Convicted of Drug Sale(s)	.16	.37
Served Time in Prison	.25	.43	Convicted of Drug Possession	.22	.41
			Convicted of Weapons Offense	.03	.18
			Convicted of Driving/Other Public Order/Other	.08	.27
			Felony Offense		
<i>Case Characteristics</i>					
Multiple Arrest Charges	.60	.49			
Pretrial Detention Prior to Conviction	.54	.50			
Public Defender Legal Representation	.62	.48			

The results in Table 4.1 also reveal a good deal of variation in the types of communities in which these defendants were processed. Turning first to legal context, just over half of the counties operated in states with determinate sentencing. The reason this figure is somewhat high is that many of the counties in the data set (40% of all county-years) come from California (one of the first states to enact determinate sentencing in the 1970s) owing to its large size and certainty of being selected into the SCPS sample. A quarter of the counties are in guideline states: 14% are located in jurisdictions with presumptive guidelines, while just over 10% of the counties operate under advisory guidelines. Jurisdictions also vary widely in the degree to which they function in states that have embraced more punitive legislative policies. For instance, 44% of the counties (many of which are located in California) are located in states where any current felony conviction potentially qualifies under a “three-strikes and you’re out” policy. The average percentage of sentences offenders must serve varies substantially. While the average is 66%, this ranges from zero in some counties to as high as 100% of the sentence imposed in Michigan and Ohio (see Appendix A). Jurisdictions vary considerably in the extent the availability of mandatory enhancements that may be applied at sentencing. States average approximately ten mandatory enhancements that are triggered by aggravating factors commonly targeted by state penal codes (i.e., weapons use, bodily harm, protected victims, and crimes committed while under state supervision). Arizona leads the sample in the number of mandatory enhancements with 26, while New Jersey has the fewest number of enhancements (2). More striking variation is observed in the minimum sentences state statutes impose for the possession and sale of the minimum felony amount (28g) of cocaine. The average sentence in

months for the sale of 1 oz of cocaine is 35 months, but this ranges from counties with no statutory minimum term of incarceration to a minimum as high as 120 months (10 years) in Georgia. The average statutory minimum is somewhat lower for possession of 1 oz. of cocaine (21 months), but yet again this ranges from a low of no minimum in some states up to 10 years in Georgia, and over five years in Tennessee (figures not shown).

Table 4.1 also indicates that defendants are processed in courts that vary considerably in their levels of jail and prison crowding, correctional spending, and prosecutor caseload pressure. Overall, the level of prison crowding is substantial. The typical county is located in a state that is over capacity by 10%. The monthly fees states and counties charge probationers exhibits substantial variation as well. Most states charge probationers supervision fees at an average of \$34 per month. However, several counties do not charge probationers for their supervision and Florida leads the sample with a base supervision fee of \$76 per month. Variables tapping social context of the communities demonstrate that most of the jurisdictions, on average, have diverse populations (18% black, 19% Hispanic). The average level of income inequality (measured by the Gini coefficient) was .44, which is slightly lower than the national average Gini coefficient of .46 in 2000. The aggregated GSS measures tapping public sentiment and interpersonal trust indicate that counties exhibit relatively high average levels of fear (47%) and support for capital punishment (64%), and are, on average, only moderately conservative (4.02 on a scale from 1 (extremely liberal) to 7 (extremely conservative)). Unfortunately, the index scores for social trust, fundamentalism and cohesion do not provide an indicator of the overall levels of these measures since the transformation of the raw metric to z-scores changes the metric of each variable (i.e., a

mean of zero and a standard deviation of 1). However, these standardized scales are useful at providing a 'rank ordering' of each county relative to the sample distribution overall. That data (not shown) show that Christian fundamentalism is highest in Alabama, parts of Florida, and Missouri counties, and lowest in Seattle, Phoenix, San Diego, and Palm Beach County, Florida. The GSS data also indicate that racial resentment is highest in Georgia, Alabama, and Tennessee counties, and below average in Seattle and San Francisco. Social trust varies substantially as well. Miami, St. Louis and Baltimore exhibit below average levels of interpersonal trust, while Michigan counties tend to exhibit average levels of social trust. Social trust is highest (by more than two standard deviations) in Palm Beach County in Florida, Seattle, Washington, and Rochester, New York.

RESULTS OF HIERARCHICAL MULTINOMIAL REGRESSION MODELS

UNCONDITIONAL MODEL

Table 4.2 displays the results of an intercepts-only model estimating the overall average probability of prison, jail, and probation, as well as the degree to which these mean estimates vary across counties and states. The model, which contains no level-one or level-two predictors, is a useful first step toward gauging the extent to which the probability of each outcome varies among county and state jurisdictions. The intercept (γ_{000}) in the unconditional model for the first outcome shown (prison vs. jail) is a point estimate of the average log-odds of receiving a prison vs. a jail sentence across jurisdictions. When converted to a conditional probability, the average odds of receiving a prison versus a jail sentence is $(\exp\{.642\}/(1+\exp\{.642\} + \exp\{.431\})) = .42$ (or 42%),

which closely approximates the observed prevalence of prison sentences in the sample (38%).³⁷ However, counties vary significantly around this overall mean probability of prison. Table 4.2 displays the conditional probabilities at 1 standard deviation above and below the overall mean probability. Going back to prison vs. jail, the results in Table 4.2 show that while the overall conditional probability of receiving a prison sentence is .42, this probability fluctuates between an average of .28 (a probability of 28%) in some counties up to .58 (i.e., a 58% probability of prison) for counties 1 standard deviation above the overall grand mean. The results are similar when comparing the conditional probability of prison versus probation. The intercept indicates that the overall expected log-odds of receiving prison versus probation is .210, or in terms of a conditional probability can be computed as $(\exp\{0.21\}/(1+\exp\{0.21\} + \exp\{-0.431\})) = .42$, or 42%.

³⁷ Conditional probabilities for the reference category (Jail in Model 1, Probation in Models 2 and 3) are calculated using the following equation:

$$\frac{1}{1 + e^{g^1} + e^{g^2}}$$

Conditional probabilities for prison in Models 1 and 2 are calculated using the following equation:

$$\frac{e^{g^1}}{1 + e^{g^1} + e^{g^2}}$$

The conditional probability for jail in Model 3 is calculated using:

$$\frac{e^{g^2}}{1 + e^{g^1} + e^{g^2}}$$

Estimation of the variance of the conditional probabilities for the non-reference categories in the models above are calculated by adding and subtracting 1 standard deviation of level 2 to the average effect (e.g. .642 ± .638 in Model 1) and repeating the above calculations. For Prison:

$$\frac{e^{g^1 \pm SD}}{1 + e^{g^1 \pm SD} + e^{g^2}}$$

For Jail:

$$\frac{e^{g^2 \pm SD}}{1 + e^{g^1} + e^{g^2 \pm SD}}$$

Examples of these formulas are provided in Holleran and Spohn (2002) and Raudenbush et al. (2004).

While the overall probability of prison should not change between these two outcomes (i.e., it should remain .42 in both outcomes), the degree to which counties vary around this estimate does. Thus, while the overall probability of prison versus probation is .42, this probability fluctuates between .27 and .601 for the counties in the sample. The unconditional mean probability of receiving jail versus probation is $(\exp\{-0.431\}/(1+\exp\{0.21\} + \exp\{-0.431\})) = .22$ (approximately 22%), but this too fluctuates between .116 in some jurisdictions and .393 in others.

The variance components displayed at the bottom of Table 4.1 indicate that the average log-odds of each of the outcomes varies significantly across counties and states, with jail versus probation exhibiting the largest degree of variation across counties ($s^2 = .636^*$) and states ($s^2 = 1.31^*$). One noteworthy finding revealed in this table is that the state-level variance in the probability of prison versus jail (.733*) is nearly double the size of the variance in the odds of prison vs. jail at the county-level (.407*). The results are similar for jail versus probation, where the amount of variance at the state level (1.31*) is more than double the size of county-level variance component (.636*). This suggests, at least initially, that state legal and organizational context may play an important role in explaining jurisdictional variation in sentencing. While intriguing, the results pertaining to the degree of state-level variance should be interpreted with caution due to the small sample size of states at level-three. Small group-level sample sizes challenge the ability of the models to generate reliable variance components (Raudenbush and Bryk, 2002). In linear hierarchical linear models, a level-one variance component is provided which allows for the calculation of an intraclass correlation coefficient (ICC).

The ICC decomposes the amount of variance within versus between counties and states.

This output is not available for nonlinear models.

LEVEL-ONE FIXED EFFECTS

Table 4.3 presents the results of a hierarchical multinomial regression model in which the effects of level-one case characteristics are estimated. Given the consistent finding that most variability in sentencing can be attributed to legally-relevant characteristics such as prior criminal history and offense severity, as well as process factors associated with mode of adjudication and pretrial detention, fitting a model that controls for these fixed effects is critical to isolating the effects of hypothesized level-two variables. Because level-one effects are not the primary focus of this research (they are treated merely as controls), my discussion of these findings is brief.

Table 4.2. Results for Unconditional HGLM Analyses Predicting the Average Log Odds of Prison, Jail or Noncustodial Sanction

	<i>Full Sample of Felony Cases (N = 26,354)</i>					
	Prison vs. Jail		Prison vs. Probation		Jail vs. Probation	
	<i>b</i>	SE	<i>b</i>	SE	<i>b</i>	SE
Fixed Effects						
Intercept g_{000}	.642*	.219	.210	.175	-.431	.287
County Variation in Estimated Probability						
-1 SD	.283		.270		.116	
Overall Mean Conditional Probability	.428		.428		.225	
+1 SD	.586		.601		.393	
Random Effects	<i>s</i> ²	SD	<i>s</i> ²	SD	<i>s</i> ²	SD
Defendants (Level 1)	--	--	--	--	--	--
Counties (Level 2)	.407*	.638	.493*	.702	.636*	.798
States (Level 3)	.733*	.856	.402*	.634	1.31*	1.14

Note: n = 91 at level 2; n = 19 at level 3; *p < .05.

Turning first to defendant characteristics, the findings are consistent with much of the prior research on sentencing. On average, male defendants face significantly higher odds of being incarcerated in prison or jail compared with females. When compared with whites, black and Latino defendants are significantly more likely to receive a prison sentence versus probation, and significantly more likely to go to jail versus probation. The effects of race and ethnicity are null for the model contrasting the odds of receiving prison versus jail. As expected, having a prior criminal record (i.e., having an active status, a prior felony arrest or conviction, prior incarceration) is generally associated with significantly higher odds of receiving a prison sentence versus jail or probation. One of the strongest predictors of receiving a prison sentence versus jail is pretrial detention ($b = .836^*$). The odds of prison versus jail is $[\exp(.836^*)]$ 2.3 times higher for those who do not secure their release before conviction. Consistent with expectations, the severity of the conviction charge is the strongest predictor of sentence type in the model. For instance, the odds of prison versus jail is $[\exp(4.83^*)]$ 125 times higher for defendants convicted of murder and $[\exp(1.29^*)]$ 3.5 times higher for defendants convicted of rape.

One of the primary purposes of estimating a random-intercept model controlling for level-one covariates is to obtain the overall log-odds of each outcome that is adjusted for variance in the types of cases courts process. In all but one exception (jail versus prison), introducing level-one covariates into the model does not reduce any of the significant residual variation initially reported in the unconditional model. Rather, taking into account cross-jurisdictional compositional differences marginally *increases* the amount of county and state variance in the overall log-odds of each outcome. The

remainder of the analyses in this chapter examines the effects of county-level variables that are hypothesized to explain this residual variation.

Table 4.3. Hierarchical Multinomial Regressions of Incarceration vs. Probation: The Effects of Case-Level Characteristics

Fixed Effects	Prison vs. Jail	Prison vs. Probation	Jail vs. Probation
<i>Defendant Characteristics</i>			
Male	.326* (.049)	.485* (.056)	.159* (.049)
Non-Latino Black (Reference = White)	-.003 (.047)	.249* (.054)	.252* (.050)
Hispanic (any race)	-.015 (.049)	.312* (.062)	.327* (.057)
Other Race	-.083 (.126)	-.033 (.158)	.050 (.137)
Race/Ethnicity Missing	.197* (.087)	.264* (.109)	.068 (.104)
Age	.004 (.002)	.004 (.002)	.0003 (.002)
Age (squared)	-.001* (.0001)	-.001 (.0002)	-.0004* (.0001)
Active Criminal Justice Status	.354* (.038)	.387* (.047)	.033 (.046)
Prior Misd. Arrest(s)	-.071 (.052)	-.042 (.059)	.029 (.055)
Prior Felony Arrest(s)	.149* (.052)	.271* (.060)	.123* (.054)
Prior Misd. Conviction(s)	-.147* (.051)	-.095 (.059)	.052 (.056)
Prior Felony Convictions(s)	.779* (.049)	-.701* (.059)	-.078 (.056)
Served Prior Jail Term(s)	-.052 (.049)	.302* (.057)	.354* (.055)
Served Prior Prison Term(s)	1.110* (.046)	1.180* (.061)	.069 (.063)
<i>Case Characteristics</i>			
Multiple Arrest Charges	.269* (.038)	.397* (.047)	.128* (.043)
Pretrial Detention	.836* (.039)	1.670* (.046)	.834* (.044)
Public Defender	-.239* (.039)	-.125* (.046)	.115* (.044)

	(.042)	(.050)	(.046)
Entered Guilty Plea	-.741*	-.352*	.390*
	(.089)	(.090)	(.096)
Murder Conviction (Reference = Assault)	4.827*	3.176*	-1.651
	(1.014)	(.557)	(1.139)
Rape Conviction	1.292*	1.530*	.238
	(.203)	(.255)	(.271)
Robbery Conviction	1.296*	1.093*	-.203
	(.106)	(.134)	(.142)
Other Violent Crime Conviction	.016	-.010	-.026
	(.098)	(.128)	(.120)
Burglary Conviction	.134	.175	.041
	(.080)	(.108)	(.104)
Larceny/Theft Conviction	-.430*	-.642*	-.213*
	(.080)	(.100)	(.093)
Motor Vehicle Theft Conviction	-.233*	.011	.244
	(.095)	(.140)	(.132)
Fraud/Forgery Conviction	-.401*	.887*	-.487*
	(.094)	(.112)	(.101)
Other Property Crime Conviction	-.290*	-.497*	-.207
	(.097)	(.120)	(.113)
Drug Sale Conviction	.084	-.038	-.122
	(.070)	(.090)	(.086)
Drug Possession Conviction	-.553*	-1.250*	-.697*
	(.070)	(.088)	(.082)
Weapons Offense Conviction	-.045	-.081	-.036
	(.104)	(.130)	(.126)
Driving/Other Public Order/Other Felony Conviction	-.252*	-.117	.134
	(.083)	(.106)	(.099)
Random Effects			
Level 2 Intercept	.429*	.647*	.665*
Chi-Square	1043.703	1071.996	1370.692
Level 3 Intercept	.969*	.584*	1.258*
Chi-Square	222.248	95.254	230.356

Note: Level 2 n = 91 counties; Level 3 n = 19 states; *p < .05, two-tailed test; standard errors in parentheses.

THE EFFECTS OF LEGAL CONTEXT

One of the primary questions guiding this research is whether state sentencing policy choices significantly impact the severity of sentencing above and beyond the effects of legally-relevant factors such as offense severity and prior criminal history. Sentencing policies are frequently invoked as the primary reasons why many states in the U.S. experienced growth in their incarceration rates after 1980 (Engen, 2009). Two recent studies investigating the factors associated with changes in incarceration rates over time have found evidence consistent with this argument (Spelman, 2009; Stemen et al., 2005). In a recent and exhaustive study, Spelman (2009) finds that public sentiment, social conditions, and political climate explain little of the escalation in prison use over the past 30 years. Rather, a central factor contributing to the expanded use of incarceration during a period of falling crime rates is changing penal policy. In particular, three of the findings from Spelman's study inform the current analysis: that presumptive guidelines tend to slow prison growth, while truth-in-sentencing laws and the war on drugs appear to fuel the growth of incarcerated populations (see also Sorenson and Stemen, 2003; Zhang et al., 2009). These findings join a number of earlier macro level studies of incarceration that suggest that sentencing policies (more so than social conditions and crime rates) exert significant influences on the likelihood that judges impose incarceration. The purpose of this analysis is to examine these possibilities using sentencing data, which are uniquely positioned to examine the impacts of policies on a specific stage of the legal process, thereby avoiding the confounding of arrests, convictions, incarceration, and release decisions (as stock incarceration rates do).

Several regression models were estimated that examine the effects of legal context on sentencing outcomes. Because scholars have placed significant emphasis on the war on drugs as a key factor responsible for the more punitive social control in the past 30 years (Beckett and Sasson, 2000; Spelman, 2009), I estimated the effects of legal context on both the full felony sample and then estimated a set of separate models examining the effects of legal context on the sentencing of drug offenders specifically. For these latter models, I broadened the specification to include laws specific to drug crimes. Having estimated the main effects of legal policies on sentencing, additional models were estimated that examined whether two policies that base sentencing on a defendant's prior record (three-strikes laws and presumptive sentencing guidelines) indirectly increase the severity of sentencing by increasing the impact of having a prior criminal history on sentencing.

I turn first to the results in Table 4.4 showing the effects of legal variables on the processing of all felony defendants. The results for models predicting sentencing outcomes for drug offenders are presented in Table 4.5, and the results for models that estimate cross-level interactions are displayed in Table 4.6. These models, and every other presented for the remainder of the chapter, control for each of the level-one predictors included in the Level-one model displayed in Table 4.3. The level-one estimates are not shown because they remained virtually identical in each of the models presented throughout the remainder of the chapter.

Model 1 of Table 4.4 presents the results of a baseline model that estimates changes in the average log-odds of prison, jail, and probation when accounting for variation in the structure and severity of state legal policies. Six variables are entered

into the equation predicting sentence type for the full sample of SCPS cases: presumptive and voluntary guidelines, determinate sentencing, the percentage of sentences most offenders must serve (i.e. truth in sentencing), the presence of a three-strikes law, and the mandatory minimum score. This baseline model reveals that, controlling for the effects of the level 1 covariates, the odds of prison versus probation doubles for defendants convicted in counties operating under determinate sentencing [$\exp(.798^*) = 2.22$]. Three-strikes laws and a higher mandatory minimum score are associated with a significant decrease in the odds of prison versus jail. Specifically, for every 1-unit increase above the average number of mandatory minimum enhancements in a jurisdiction, the odds [$\exp(-.069) = .93$] of prison versus jail declines by 7% ($[1-.93]*100$). Similarly, the odds of prison versus jail ($\exp[-.804] = .44$) is significantly lower in for defendants processed in counties with three-strikes laws.

The effect of three-strikes law is not consistent across outcomes. While three-strikes is associated with lower odds of prison, the results for models estimating the odds of jail versus probation reveal that defendants face significantly *higher* odds of jail versus probation in jurisdictions located within states with three-strikes laws. The odds of jail more than double ($\exp[1.05^*] = 2.85$) when defendants are processed in three-strikes jurisdictions. The presence of voluntary and presumptive guidelines and the severity of a state's time-served requirements are not significantly related to any of the sentencing outcomes. Thus, contrary to expectations, it does not appear that a more punitive legal culture independently increases the severity of sentencing. Somewhat unexpected, three-strikes laws are associated with a higher probability of jail versus probation, an outcome

that when compared to prison, should be less sensitive to the effects of three-strikes laws which mandate long periods of incarceration.

An additional model was fitted that includes controls for prison and jail capacities and county index crime rates because prior research has shown that these types of local concerns may mediate the potential impact of state-level regulations guiding sentencing (e.g., Engen and Steen, 2000; Merrit et al., 2006; Ulmer, 1997). These controls were entered into the equation because low crime rates may reduce the willingness of some jurisdictions to act upon more punitive laws that were passed in the wake of a peak in U.S. crime rates, and because the severity of state legal policies (or the willingness to comply with them) may be highly correlated with the severity of a jurisdiction's crime problem. Second, any influence of punitive sentencing laws that are believed to have contributed to a growth in incarceration may be highly correlated with jail and prison crowding that may flow from the passage of such laws.

Model 2 in Table 4.4 displays the results from this full specification. The results reveal that once controlling for crime rates and correctional capacities, only determinate sentencing retains its significant influence on sentencing, and for only the model predicting the log-odds of prison versus probation. The coefficient for determinate sentencing ($b = .759^*$) indicates that the average odds of prison versus probation double ($\exp[.759^*] = 2.14$) when sentenced in a jurisdiction operating under determinate sentencing. This finding is consistent with literature that casts the adoption of determinate sentencing as a move towards a philosophy of deterrence and incapacitation, but it contradicts findings from macro level studies that incarceration rates are significantly lower in states with determinate sentencing. Though only speculative, one

interpretation of the positive effect reported here might be that judges in determinate sentencing states may be reluctant to sentence offenders to prison if they know that prisoners have no prospect of parole release.

Consistent with macro level research on incarceration, the presence of presumptive guidelines is associated with less severe sentencing in general, but the effect does not achieve statistical significance. In addition, truth in sentencing provisions and the mandatory minimum score exert no significant influence over sentencing outcomes.

The variance components for the full model (Model 2) indicate that although few of the variables were significant, controlling for these legal factors explained a substantial portion of the between-county and state residual variance. For instance, entering these legal variables into the model reduced the state-level variance component for prison versus probation from .647* in the level-one fixed effects model to .233* (a 64% reduction). The largest amount of variance left unexplained by the level-one fixed effects model was for the log-odds of jail versus probation (1.258*). Table 4.4 reveals that after adjusting for legal context, the amount of between-state variation in the odds of jail is reduced by half (from 1.258 to .600*). While significant variation remains, this is a substantial drop in the residual variation and calls attention to a need for future studies that analyze SCPS data to model variability both across counties and states.

Several conclusions can be drawn from these results. There is widespread belief that certain sentencing policies have widened the net of offenders sent to prisons and jails. It is not evident from the case-level data used in the current study that more punitive sentencing policies necessarily disadvantage defendants. While the present study represents only a modest attempt at examining the effects of policies on sentencing, the

results reported here do not provide evidence to suggest that the risks of incarceration are lower on average in states with more structured sentencing (as found in macro level incarceration research), nor is the severity of sentencing significantly higher in jurisdictions with more punitive legal climates (measured by the presence of mandatory penalties and three-strikes laws). The next set of models examine whether these conclusions hold when examining legal outcomes in drug cases.

Table 4.5 presents results from a baseline and fully specified model examining the effects of legal context on the sentencing of drug offenders. Each model includes a control variable at level 1 which indicates whether the defendant was convicted of drug sales or manufacturing versus felony drug possession. Because the results do not change substantially across the two models, I discuss only the results of the fully specified model (Model 2). Turning first to the effects of *non-drug* related laws the results for Model 2 indicate that the odds of prison (versus jail or versus probation) are significantly lower for drug offenders convicted in counties that require offenders to serve a longer portion of their minimum sentence. The parameter estimate ($b = -.027^*$) for the model predicting the odds of prison versus jail indicates that a 1-standard deviation (26.74) increase in the percentage of sentences offenders must serve is associated with a .49 change in the odds of receiving a prison sentence [$\exp(-.027 \times 26.74) = .485$]. Expressed another way, a 1-standard deviation increase in the time-served requirement (26.74) corresponds to a 51% decrease [$(\exp(-.027 \times 26.74) - 1) \times 100$] in the odds of prison versus jail. The graph in Figure 4.2 illustrates the magnitude of the association in terms of changes in the conditional probability of prison. The graph shows that the estimated probability of prison versus jail is approximately .65 (or, a 65% probability of prison) in jurisdictions

that require offenders to serve only 15% of their incarceration terms. This probability of prison drops by over half (i.e., below .30) in counties that require most offenders to serve over 75% of their fixed or minimum sentence. This significant effect of TIS requirements is not observed for the felony sample over all and it persists independent of any influence that minimum statutory penalties for cocaine exert on sentencing. This finding may signal a reluctance among prosecutors and judges to sentence drug offenders to prison in jurisdictions where these offenders are certain to serve a higher proportion of their sentence before being eligible for parole. This might indicate that judges and prosecutors in these jurisdictions wish to reserve imprisonment for serious property and violent offenders.

The results in Model 2 also indicate that drug offenders are significantly less likely to be sentenced to prison versus probation in three-strikes jurisdictions. The slope coefficient ($b = -1.39$) indicates a -75% change in the odds of prison versus jail ($\exp[-1.39] = .249$) for drug offenders convicted in a three-strikes jurisdiction. Contrary to expectations, the mandatory minimum score for drug crimes was unrelated to the severity of sentencing for drug offenders. Supplementary models (not shown) were estimated to explore whether the number of drug-crime mandatory enhancements significantly influenced sentencing in the absence of controls for drug sale and possession statutory minimums. The results did not change. Though the implications of mandatory minimum enhancements are the subject of considerable debate, the results reported here suggest that sentencing is not significantly more punitive in states with more enhancements. Though the data cannot address the issue, one possible interpretation is that judges and

prosecutors in urban courts choose *not* to evoke mandatory enhancements for most drug offenders.

The findings in Model 2 (Table 4.5) also reveal that drug offenders face a significantly higher probability of going to prison versus probation in jurisdictions with higher minimum sentences for the sale of 1 oz. of cocaine. The effect is fairly substantial in magnitude and maintains significance after controlling for crime rates and prison and jail capacity constraints. Holding constant all other level-one and level-two predictors, a one standard deviation increase in the minimum sentence (25 months) for the sale of one ounce of cocaine corresponds to a 1.77 increase ($\exp[.023*25]$) (or 77%) increase in the odds of being sentenced to prison versus probation. The graph in Figure 4.3 illustrates more precisely the impact of more punitive drug statutes on the probability that drug offenders are sentenced to prison versus probation. Several states in the sample have no minimum sentence for the sale of 1 ounce of cocaine. In these jurisdictions, the probability of prison is estimated to be around 38% (.38). However, as you move along to the right of the graph, the probability of prison versus probation is estimated to be as high as 78% in jurisdictions that mandate a minimum sentence of at least 100 months (8 years). These findings suggest that state legal context exerts a significant influence on the outcomes of drug cases and helps to account for why drug offenders face substantially higher risks of incarceration in certain jurisdictions. Differences in state penal statutes are not often discussed in the extant contextual sentencing research, where researchers place a heavier focus on socioeconomic explanations for understanding community differences in sentencing severity. However, these findings suggest that states vary substantially in their legal approach to the sentencing of drug offenders in ways that generate significant

variability in the risks of incarceration when convicted of a drug offense. This finding provides support for legal models in the Weberian tradition, which view legal decision making as a highly rationalized process driven by rules and the law, rather than crime rates or organizational constraints. At the same time, the results as a whole also seem to suggest that policies labeled as inherently punitive appear to wield little influence on the severity of sentences above and beyond any impact of case-level factors. Another unexpected finding is that more punitive policies are associated with more lenient sanctions in certain cases. For instance, sentencing tends to be significantly more lenient for drug offenders convicted in jurisdictions with more punitive legal statutes. This may signal resistance among courtroom personnel in some jurisdictions to state-level attempts to institute harsher sanctions in ways that supplant judicial discretion by either mandating incarceration and/or a longer duration sentence.

Table 4.4. Hierarchical Multinomial Regressions of Incarceration vs. Probation in Felony Cases: The Main Effects of Legal Context

	(1)			(2)		
	<i>(Full Sample of Felony Cases; N = 26,354)</i>			<i>(Full Sample of Felony Cases; N = 26,354)</i>		
	Prison vs. Jail	Prison vs. Probation	Jail vs. Probation	Prison vs. Jail	Prison vs. Probation	Jail vs. Probation
Fixed Effects						
Intercept (γ_{000})	.785* (.326)	.207 (.327)	-.531 (.414)	.705* (.340)	.252 (.293)	-.454 (.380)
Presumptive Guidelines	-.263 (.327)	-.700 (.362)	-.478 (.389)	-.287 (.334)	-.461 (.355)	-.174 (.378)
Voluntary Guidelines	.098 (.478)	-.578 (.469)	-.894 (.565)	.239 (.475)	-.411 (.437)	-.650 (.529)
Determinate Sentencing	-.203 (.405)	.798* (.393)	.892 (.513)	-.067 (.416)	.759* (.247)	.826 (.467)
% Sentence Imposed Offenders Required to Serve	-.008 (.008)	-.007 (.007)	.004 (.010)	-.011 (.008)	-.010 (.007)	.001 (.009)
Three Strikes Law	-.804* (.403)	.196 (.398)	1.05* (.522)	-.821 (.423)	.093 (.350)	.913 (.474)
Mandatory Enhancement Score	-.069* (.035)	-.012 (.028)	.035 (.037)	.047 (.030)	-.013 (.025)	.034 (.033)
<i>Control Variables</i>						
Prison Capacity Constraints				-.005 (.005)	.008 (.005)	.013* (.006)
Jail Capacity Constraints				-.002 (.005)	-.001 (.005)	.001 (.005)
County Index Crime Rate				.00003 (.00005)	-.0001 (.0001)	.0001 (.00005)
Random Effects						
Level 2 Intercept	.428	.593	.611	.414	.591	.565
Chi-Square						
Level 3 Intercept	.418*	.374*	.766*	.489*	.233*	.600*
Chi-Square	99.68	62.4	101.9	112.07	46.12	87.18

Note: † < .056; *p < .05, two-tailed test. Level 2 n = 91 counties; Level 2 n = 19 states; standard errors in parentheses.

Table 4.5. Hierarchical Multinomial Regressions of Incarceration vs. Probation: The Main Effects of Legal Context

	(1) <i>(Drug Cases; N = 10,167)</i>			(2) <i>(Drug Cases; N = 10,167)</i>		
	Prison vs. Jail	Prison vs. Probation	Jail vs. Probation	Prison vs. Jail	Prison vs. Probation	Jail vs. Probation
Fixed Effects						
Intercept (γ_{000})	1.11* (.515)	.533 (.304)	-.581 (.469)	1.099* (.541)	.539 (.364)	-.560 (.595)
<i>Sentencing Structure</i>						
Presumptive Guidelines	-.393 (.432)	-.594 (.395)	-.201 (.455)	-.353 (.449)	-.562 (.409)	-.209 (.477)
Voluntary Guidelines	-.065 (.687)	-1.29* (.487)	-1.23 (.645)	-.126 (.703)	-.932 (.537)	-.807 (.740)
Determinate Sentencing	-.214 (.538)	.296 (.334)	.510 (.495)	-.219 (.565)	.389 (.393)	.607 (.625)
<i>Indicators of Punitive Legal Culture</i>						
% Sentence Imposed Offenders Required to Serve	-.026* (.011)	-.023* (.007)	.002 (.010)	-.027* (.011)	-.020* (.008)	.006 (.012)
Three Strikes Law	-1.45* (.611)	-.616 (.372)	.835 (.551)	-1.393* (.643)	-.809 (.435)	.583 (.702)
Mandatory Enhancement Score	-- --	-- --	-- --	-- --	-- --	-- --
<i>Punitive Legal Culture for Drug Offenses and Offenders</i>						
Mandatory Enhancements for Drug Offenses	-.004 (.034)	-.025 (.020)	-.020 (.031)	-.017 (.037)	.002 (.026)	.019 (.040)
Min Sentence (months) 1 oz Sale of Cocaine	.007 (.013)	.017* (.009)	.010 (.012)	.005 (.014)	.023* (.010)	.018 (.015)
Min Sentence (months) 1 oz. Possession of Cocaine	-.014 (.015)	-.030* (.010)	-.016 (.014)	-.013 (.016)	-.010 (.011)	-.019 (.017)

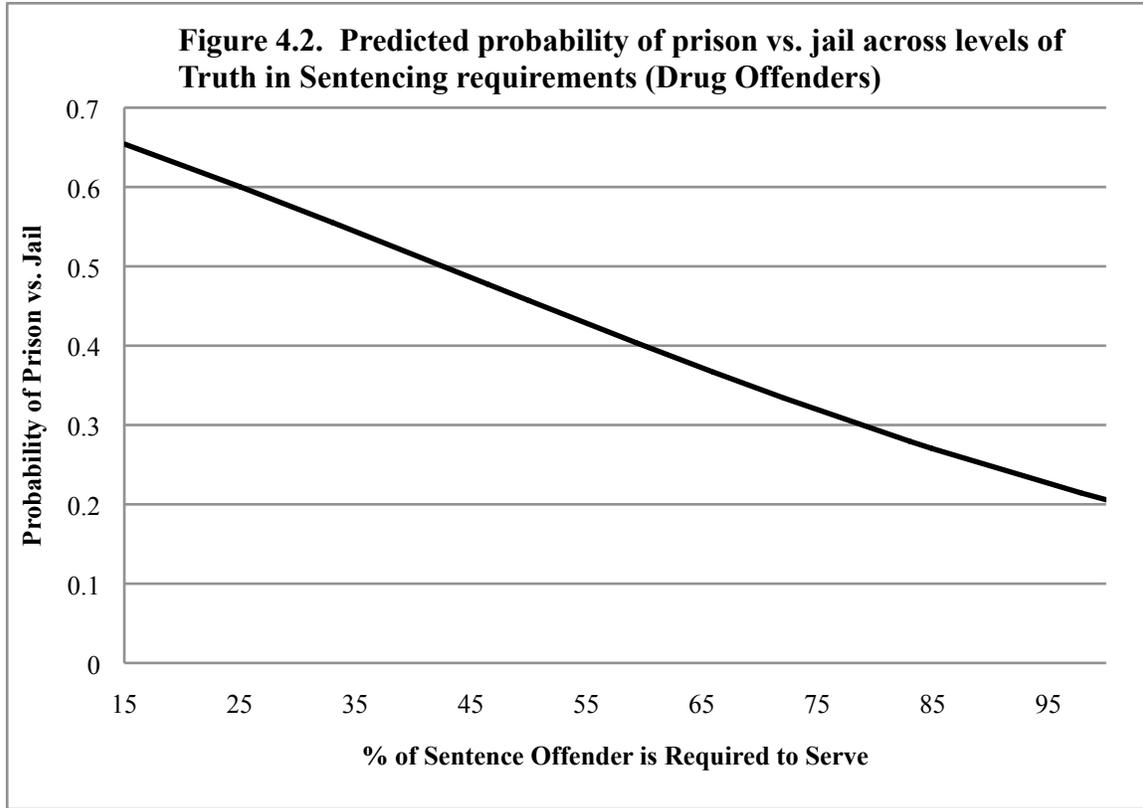
Control Variables

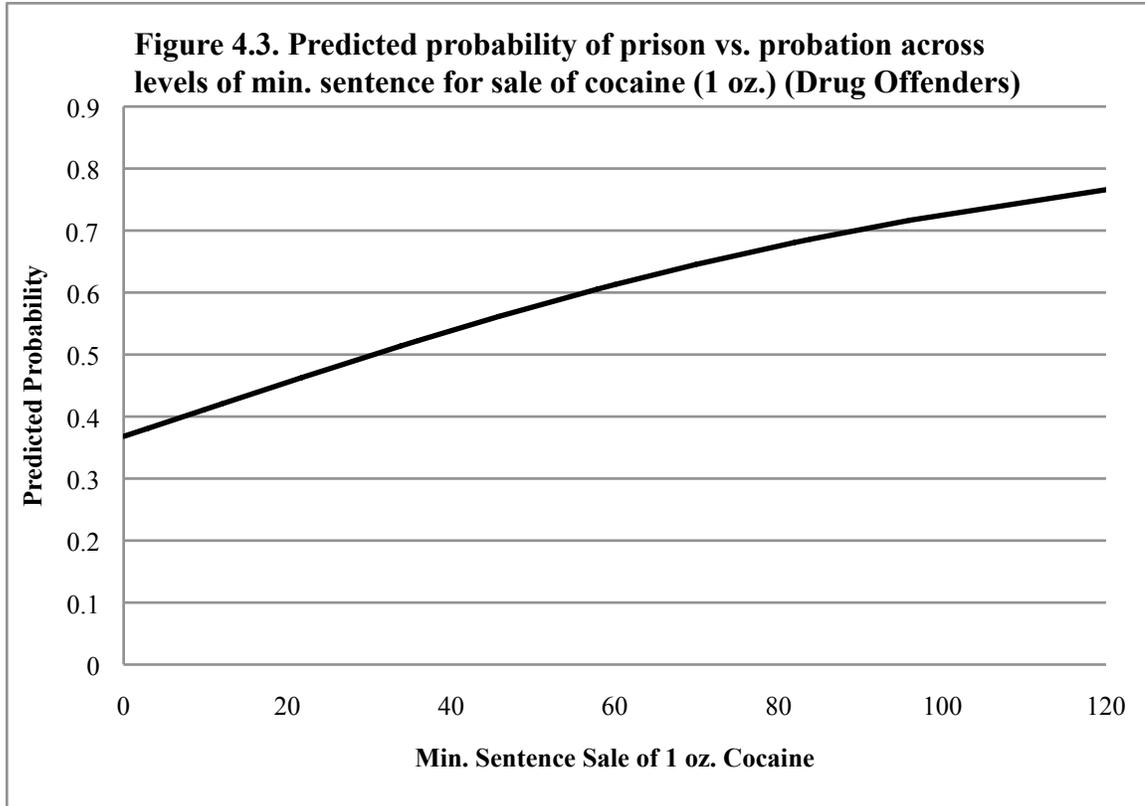
Prison Capacity Constraints						
Jail Capacity Constraints						
County Index Crime Rate						

Random Effects

Level 2 Intercept	.522	.872	.946	.493	.647	.578
Chi-Square						
Level 3 Intercept	.735*	.101*	.495*	.834*	.252*	1.041*
Chi-Square	121.5	30.3	60.04	134.51	47.47	131.55
<i>N</i> = 23,356						

Note: *p < .05, two-tailed test. Level 2 n = 91 counties; Level 2 n = 19 states; standard errors in parentheses.





THE CONDITIONING EFFECTS OF LEGAL POLICIES

Another possibility not considered in prior multilevel sentencing research (and which cannot be examined with purely macro level data) is whether sentencing reforms *indirectly* contribute to jurisdictional variation in punishment by modifying the effect of criminal history on the sentencing process. As a case in point, macro level research, which finds that presumptive guidelines are associated with lower incarceration rates and slower growth in those rates may be picking up on more nuanced indirect ways that policies have impacted the behavior of judges (see e.g., Marvell and Moody, 1996; Nicholson-Crotty, 2004; Sorenson and Stemen, 2003; Zhang et al., 2009). A series of random-slope and cross-level interaction models tested this hypothesis. The expectation is that although several multilevel and macro level studies report a significant negative relationship between guidelines and imprisonment, another possibility not considered in prior research is that sentencing in many jurisdictions in the U.S. has become more punitive because certain policies have elevated the importance judges give to prior criminal history.

There is good reason to expect that the meaning of having a prior record has changed in recent years. Sentencing guidelines were promulgated at the Federal level and in several states dating back to the 1980s as a way to formally rationalize the sentencing process (Ulmer and Kramer, 1996). Presumptive guidelines in states like Washington, Pennsylvania, and Minnesota expressly consider an offender's prior criminal history (in conjunction with offense severity) when deciding sentencing. This raises the possibility that the meaning of having a prior criminal record is different in these jurisdictions, where judges are not permitted (except in exceptional cases) to overlook prior record when sentencing. This suggests that presumptive guidelines may indirectly increase the probability of incarceration because it gives explicit, mathematical weight

to criminal history at sentencing. I tested this hypothesis in two models that interacted presumptive guidelines with having a prior felony conviction and having served a prior prison term.³⁸ The results shown in Model 1 (Panel A) of Table 4.6 indicate that for the outcome comparing the odds of jail versus probation, the odds of jail are significantly *higher* for defendants with a prior felony conviction who are convicted in presumptive guideline jurisdictions ($b = .509^*$). It should be noted that the effect should be interpreted with caution though since neither of the main effects of felony conviction or guidelines is significant. Nevertheless, the bar chart displayed in Figure 4.4 does suggest meaningful differences in the effects of prior felony conviction for defendants convicted in guideline versus non-guideline states. For instance, the far right bar of the graph represents the probability (.40, or 40%) of jail for prior felons convicted in a guideline state. This effect is noticeably larger than the effect of a prior felony conviction in non-guideline states. The conditional probability of jail nearly doubles for ex-cons who are convicted in guideline states. As you can see by looking at the second bar from the left, the predicted probability of jail for prior felons convicted in *non-guideline* states is approximately .25 (or 25%), compared with .40 for prior felons convicted in guideline states. These results suggest that all else being equal, a defendant with a prior felony conviction is expected to face a significantly higher probability of going to jail compared with defendants with prior felonies who are convicted in non-guideline states. These findings support the hypothesis that presumptive guidelines may indirectly increase the severity of sentencing by amplifying the effect of having a prior criminal record.

Contradicting this pattern, the results for Model 2 (Panel A of Table 4.6) contrasting the odds of prison versus jail indicate that offenders who have ever served a prior prison term are

³⁸ Initial estimates (not shown) revealed that both slopes (prior felony conviction and prior prison stay) varied significantly across county jurisdictions, and that the magnitude of the variance was surprisingly large, with the variance component exceeding .500 for prior felony conviction.

treated more *leniently* in guideline jurisdictions ($b = -.930^*$) than they are in non-guideline jurisdictions. The overall main effect of having a prior felony conviction is positive and significant ($b = .585^*$) which corresponds to $(1 - \exp(-.585)) * 100$ an 80% increase in the odds of prison versus jail if a defendant ever served a prior prison term. However, the magnitude of this positive effect is reduced substantially in guideline states. Figure 4.5 best illustrates the magnitude of the difference. The bars to the far right of the graph illustrate that guidelines appear to have an “equalizing” effect on the odds of prison versus jail for those who have served a prior prison term. That is, all else being equal, the predicted probability of going back to prison for these individuals is roughly equivalent (and actually marginally lower) to the probability of prison for defendants who have not served a prior prison term. However, the effect of having served a prior prison term results in substantially higher odds of going back to prison for defendants convicted in non-guidelines states. As shown in Figure 4.5, in non-guideline states the probability of prison for defendants with a prior prison term is over 50% (.53), compared with a conditional probability of 33% (.33) for prior prison inmates convicted in guideline states. The results from these models suggest that while guideline systems may increase the odds of some type of incarceration time (i.e., jail versus probation), that the guideline recommendations might actually constrain more punitive sanctions against ex-cons compared with states who may be permitted by law (i.e., have greater discretionary authority) to send a much more harsh message to repeat offenders. It is noteworthy that the ability of presumptive guidelines to reduce the punitive impact of being an ex-con is consistent with the theme from macro level incarceration research which finds presumptive guidelines to be significantly associated with a lower rate of prison admissions (Sorenson and Stemen, 2003). However, it is difficult to reconcile why guidelines might reduce prison for those who have

served a prior prison term but increase the odds of jail for defendants with a prior felony conviction. One reason for this may be that many states instituted guidelines that were tied into correctional resources or allow (as in PA) judges to depart from the guidelines if crowding is a problem (Stemen et al., 2005). Another logical possibility is that the guideline states included in this study (WA, PA, etc) have adopted guideline grids that either contain less punitive presumptive sentences compared with statutes in non-guideline states or that may give special consideration to the use of alternative sanctions, as Washington state does (Gainey et al., 2005).

Models 1 and 2 in Panel B (Table 4.6) display the results of the cross-level interaction models for prior criminal history and three-strikes laws. Arguably the most politicized effort to institute harsher sanctions during the 1990s came in the form of habitual offender laws, several of which were adopted by a direct ballot initiative and approved by voters in California, Washington, and Oregon (LaFree, 2002; Zimring, Hawkins, and Kamin, 2001). Many hypothesized that the passage of three-strikes might signal a general “wave” of more punitive sentencing (Stemen et al., 2005), but for the most part, the express purpose of three-strikes was to issue more punitive sanctions to *repeat* offenders more so than to increase the severity of sentencing generally (Spelman, 2009; Sutton, 2010; Zimring et al., 2001).

Similar to the interaction models for presumptive guidelines, I estimated two models that included cross-level interactions between three-strikes laws and having a prior felony conviction and having served a prior prison term. The results of these models are shown in Table 4.7. The first model interacts three-strikes with having a prior felony record. The results for the outcome contrasting the odds of prison versus jail indicate that the product term (Prior Felony Conviction * Three-Strikes) is positive and significant. While this is consistent with the hypothesis that three-strikes magnifies the punitive impact of having a criminal record, a closer inspection of the

results reveals that the interaction is substantively meaningless since the main effect of prior felony record is not significant at level 1 ($b = .251$), and because the overall effect of three-strikes is negative ($b = -.622$). The bar chart illustration in Figure 4.6 best illustrates this. The bars on the right hand side of each level-two group (i.e., three-strikes group vs. no three-strikes) represent the predicted probability of prison versus jail for felons with a prior felony conviction. If the interaction is meaningful, we would expect that bar on the far right hand side of the graph would be taller (with a correspondingly higher predicted probability) if the effect of having a prior felony conviction was magnified in three-strikes jurisdictions. Instead, because the overall effect of three-strikes is negative and non-significant ($b = -.622$) any additional effect of having a prior felony in a three-strikes state is effectively washed out. The results for Model 2 in Panel B in Table 4.6 display the results of cross-level interactions between having served a prior prison term and being sentenced in a three-strikes jurisdiction. The results reveal that three-strikes laws do not strengthen the positive effects of having a prior prison term. This finding contradicts the hypothesis that three-strikes laws significantly increase the severity of sentencing either directly or in an indirect manner. These findings are consistent with case studies that suggest that the adoption of three-strikes were primarily symbolic in states outside of California, and lend additional support to empirical research showing that new court commitment rates are significantly lower in states with three-strikes laws (Zhang et al., 2009). What is unclear from these findings, however, is precisely why, holding constant crime rates and prison capacity constraints, three-strikes laws would be associated with significantly more lenient sentencing outcomes.

As a whole, the findings suggest that presumptive guidelines exert an influence over sentencing primarily by conditioning the relationship between defendant criminal history and

sentence type. The results for the main effects models, however, do not support the hypotheses predicting the more punitive legal cultures where habitual offender laws and mandatory penalties are more widespread translate into significantly more punitive sentencing. Drug cases are an exception however; controlling for level-one characteristics (including a dummy variable of if the drug conviction was for sales versus possession), drug offenders sentenced in jurisdictions with more punitive minimum statutory penalties for drug sales face significantly higher odds (and increase of 26%) of going to prison compared with similar drug offenders convicted elsewhere.

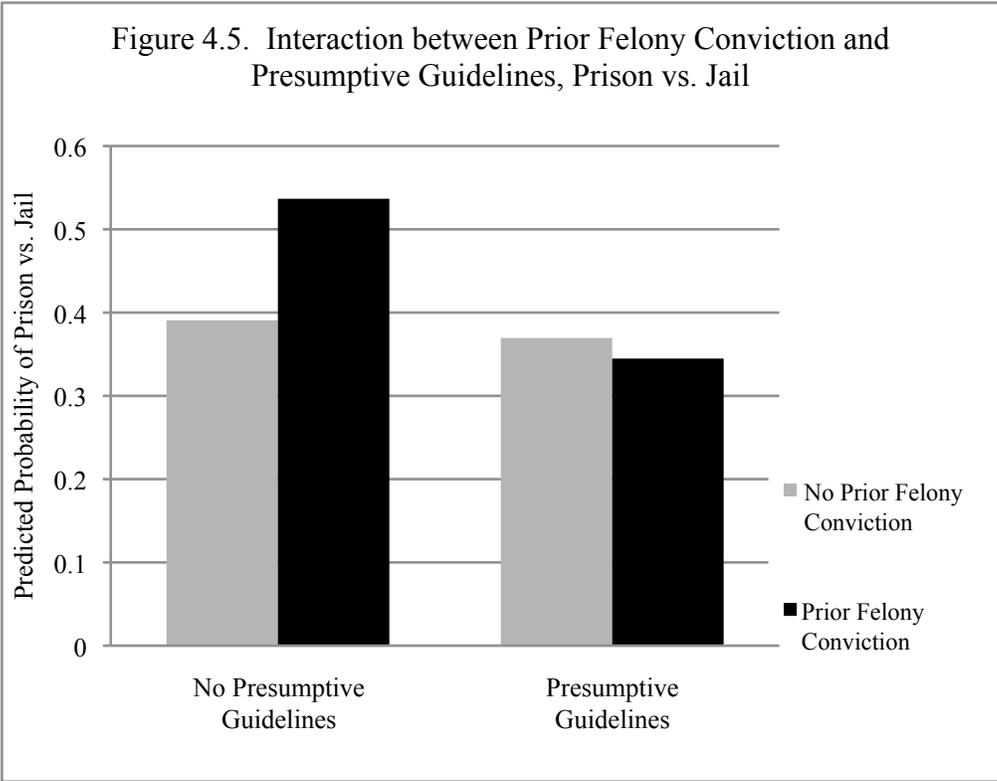
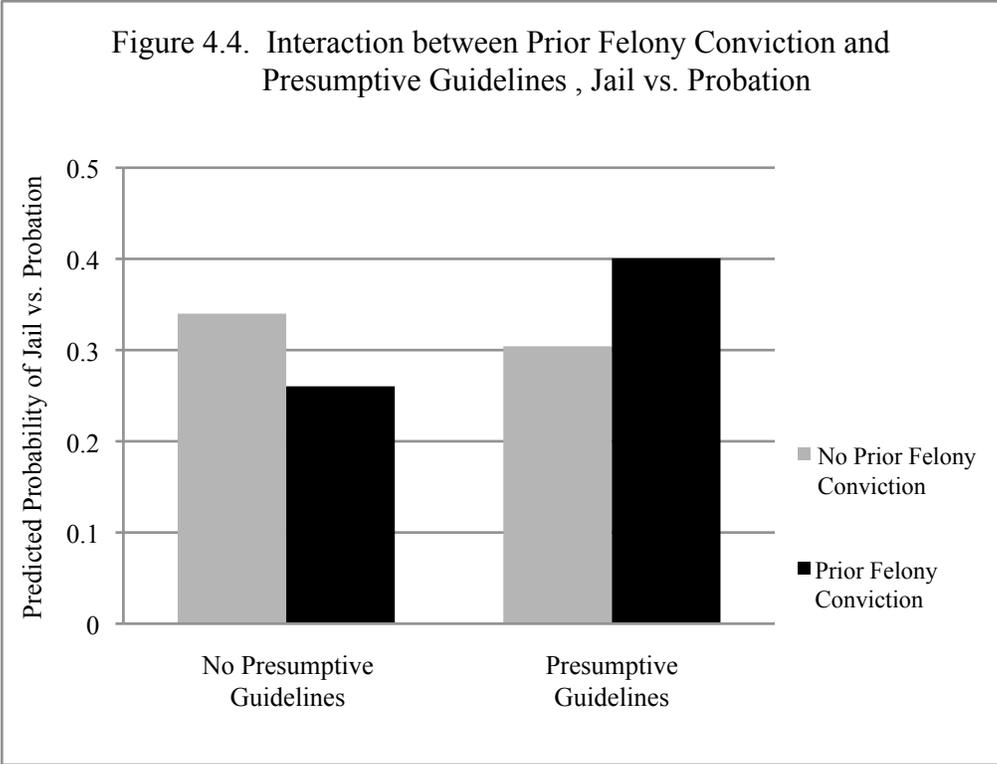
Table 4.6. Hierarchical Multinomial Regression Models Including Cross-Level Interaction Terms Between Prior Criminal History and Sentencing Policy (N = 26,354)

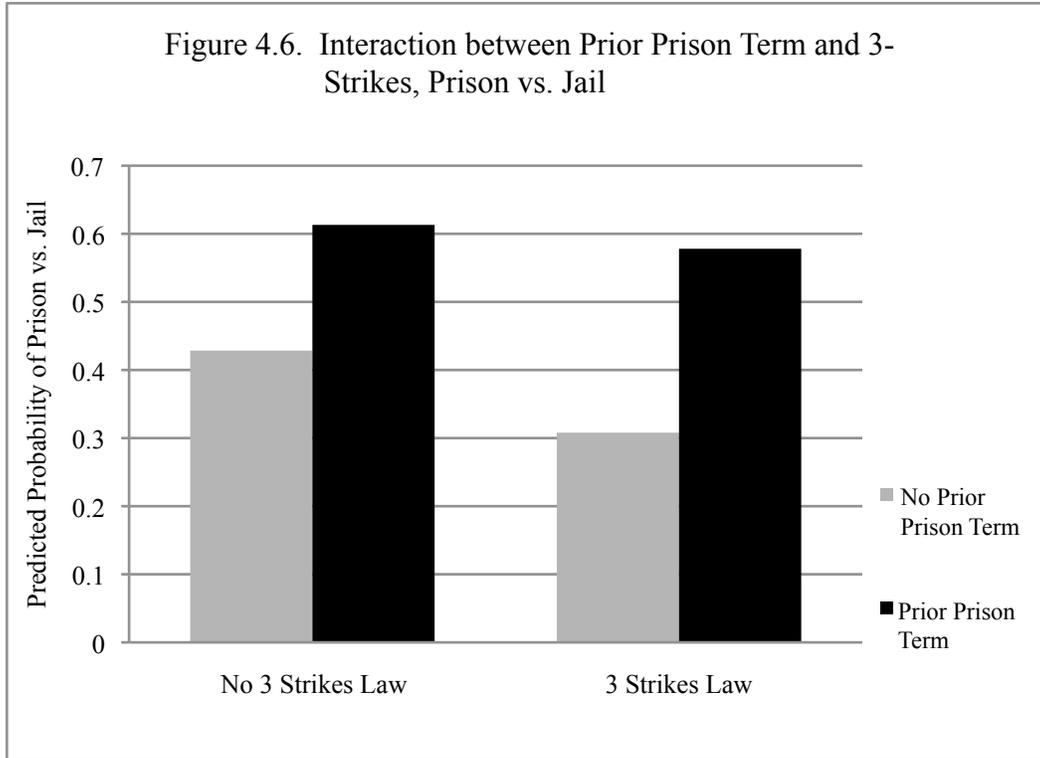
Panel A. Interactions with Presumptive Guidelines	(1)			<i>(x Prior Prison Term)</i>	(2)		
	Prison vs. Jail	Prison vs. Probation	Jail vs. Probation		Prison vs. Jail	Prison vs. Probation	Jail vs. Probation
<i>(x Prior Felony Conviction)</i>							
Fixed Effects				Fixed Effects			
Intercept (γ_{000})	.857* (.341)	.328 (.290)	-.528 (.382)	Intercept (γ_{000})	.696* (.342)	.258 (.322)	-.438 (.430)
Prior Felony Conviction(s)	.585* (.104)	.601* (.095)	.015 (.092)	Prior Prison Term(s)	.840* (.106)	1.08* (.095)	.241* (.112)
Presumptive Guidelines	-.413 (.326)	-.458 (.349)	-.044 (.384)	Presumptive Guidelines	-.083 (.324)	-.701* (.356)	-.618 (.394)
Prior Felony Conviction(s) *	-.930* (.259)	-.420 (.225)	.509* (.205)	Prior Prison Term(s) *	-.143 (.271)	-.003 (.231)	.140 (.265)
Random Effects				Random Effects			
Level 2 Intercept	.424	.562	.628	Level 2 Intercept	.430	.587	.667
Chi-Square				Chi-Square			
Prior Felony Conviction(s) Slope	.525* 518.7	.321* 243.1	.283* 290.1	Prior Prison Term(s) Slope	.537* 462.9	.258* 177.5	.433* 230.0
Chi-Square				Chi-Square			
Level 3 Intercept	.525* 160.2	.236* 48.7	.597* 89.6	Level 3 Intercept	.521* 132.9	.362* 61.3	.867* 126.8
Chi-Square				Chi-Square			

Panel B. Interactions with Three Strikes Law	(1)			(2)			
	Prison vs. Jail	Prison vs. Probation	Jail vs. Probation	Prison vs. Jail	Prison vs. Probation	Jail vs. Probation	
<i>(x Prior Felony Conviction)</i>				<i>(x Prior Prison Term)</i>			
Fixed Effects				Fixed Effects			
Intercept (γ_{000})	.750* (.344)	.303 (.293)	-.496 (.425)	Intercept (γ_{000})	.703* (.342)	.289 (.276)	-.414 (.378)
Prior Felony Conviction(s)	.251 (.136)	.537* (.116)	.294* (.111)	Prior Prison Term(s)	.583* (.134)	.910* (.115)	.327* (.142)
Three Strikes Law	-.622 (.434)	.157 (.351)	.959 (.537)	Three Strikes Law	-.769 (.429)	.025 (.327)	.794 (.475)
Prior Felony Conviction(s) *	.395* (.189)	.003 (.161)	-.388 (.150)	Prior Prison Term(s) *	.474 (.183)	.356 (.167)	-.116 (.201)
Random Effects				Random Effects			
Level 2 Intercept	.415	.560	.614	Level 2 Intercept	.419	.590	.665
Chi-Square				Chi-Square			
Prior Felony Conviction(s) Slope	.587* 539.6	.335* 255.5	.276* 282.4	Prior Prison Term(s) Slope	.472* 400.4	.252* 184.8	.431* 229.1
Chi-Square				Chi-Square			
Level 3 Intercept	.528* 153.7	.245* 49.9	.609* 88.6	Level 3 Intercept	.509* 129.9	.363* 61.3	.882* 128.2
Chi-Square				Chi-Square			

Note: Each cross-level interaction was estimated in a separate regression model while controlling for all individual-level and contextual effects shown in the models in Table 4.4.

*p < .05, two-tailed test. Level 2 n = 91 counties; Level 2 n = 19 states; standard errors in parentheses.





THE EFFECTS OF ORGANIZATIONAL CONTEXT

Table 4.7 presents findings for models examining the effects of several indicators of organizational context. With the exception of caseload pressure, each of these variables is predicted to significantly reduce the probability of incarceration. In keeping with the models presented for legal context, I estimated equations for the full sample as well as for the sub-sample of drug offenders, reasoning that concerns over prison crowding may dissuade judges from sending drug offenders to prison, but may not impact decision making in cases involving serious violent offenders. I turn first to the findings for the full SCPS sample of convicted felons.

The results in Model 1 in Table 4.7 reveal that three of the organizational variables I consider exert significant effects on sentence type. The negative and significant coefficient for average monthly probation cost ($b = -.012^*$) indicates that all

else being equal, the odds of being sentenced to prison versus jail are significantly lower in jurisdictions that charge higher monthly probation supervision fees. Every one-dollar increase in probation fees reduces the odds of prison by approximately 1% $[(1-(\exp(-.012)))=.011]$. Expressed another way, a 1-standard deviation increase in monthly probation fees (\$23.80) is associated with a 24% percent change in the odds of prison $[(1-(\exp(-.012*23.80))=.24]$. The chart in Figure 4.7 better illustrates how the probability of prison changes across varying probation fee contexts, controlling for all other level 1 and organizational characteristics included in the model. The far left side of the chart displays the estimated probability of prison for defendants convicted in counties that do not charge probationers supervision fees. The average probability of prison in this context is approximately fifty-fifty (i.e., .45, or 45%). As the amount of monthly supervision fees increase toward the mean for the counties in the sample (\$34.00), the probability of prison drops to less than 40%, and for defendants processed in jurisdictions that charge the highest supervision fees (\$76.00), the average probability of being sentenced to prison drops to under 30%.

At first glance this may not appear to support the hypothesis that courts better equipped to offset the costs of probation are more likely to steer convicted felons away from prison since this outcome contrasts prison with jail (as opposed to prison versus probation). However, the vast majority of jail sentences in the SCPS (76%) involve an additional period of probation. Thus, this may represent a preference among courts to issue a combination of jail and probation sentences in lieu of more costly prison sentences. This finding lends support to the hypothesis that in jurisdictions that can charge for probation supervision will adjust the severity of punishment to (i.e., use jail

and probation more often than prison) in an effort at shift the costs of punishment to offenders. Moreover, this effect persists amid controls for how much states spend on corrections (i.e., the severity of the fiscal burden for prisons that they currently bear) and crowding (which may further urge states to steer offenders away from prison).

As expected, the results for Model 1 also indicate that higher per capita correction spending (per 100,000 residents) is associated with significantly lower odds of prison versus jail ($b = -.0002^*$). A 1-standard deviation increase (\$3,482.34) in corrections spending per 100,000 state residents is associated with a 53% change in the odds of prison versus jail ($(1 - (\exp(-0.00022 * 3482.35)))$). This finding is consistent with the logic outlined in Chapter 2, which suggests that the burdens associated with high corrections spending will be met with efforts to “rethink” the severity of punishment (Wilhelm and Turner, 2002). There are multiple ways states can do this. They may alter penal codes in a more permanent attempt to manage correctional resources, but local jurisdictions may also react to these fiscal burdens (which usually means less state budget assistance for local government functions, and for state education, health care, and the like) by adjusting their overall propensity to incarcerate offenders in prisons versus jails or through more intensive probation supervision.

Contrary to expectations, prison crowding (not jail crowding) was associated with higher probability of being sentenced to jail versus probation. Also, the degree of prosecutor caseload pressure and county crime rates were unrelated to sentencing outcomes. Thus, contrary to the tenets of instrumentalist/functionalist perspectives, differences in the “inputs” to each court system do not substantively alter the severity of local jurisdictions’ crime control response (e.g., Liska, 1992:9).

Model 2 in Table 4.7 presents the results of regression models estimating the influence of organizational context on the sentencing of drug offenders. With one exception, the results mirror those for the full sample. The results in Table 4.7 show that the negative and significant influence of probation fees does *not* extend to the sentencing of drug offenders. Though probation fees are negatively associated with the probability of prison versus jail ($b = -.010$), the relationship is not statistically significant. This is somewhat surprising and contrary to expectations, since many states have sought to reduce the costs of imprisonment by developing alternative means of sanctioning non-violent drug offenders (i.e., drug monitoring, treatment). Thus, one would expect that higher probation fees would primarily motivate states to increase the use of probation for drug offenders since many of these individuals presumably pose less of a threat to society. Supplementary models not shown estimated whether the effect of probation fees depended on the severity of drug laws (specifically, the minimum sentence for the sale of 1 ounce of cocaine), reasoning that some jurisdictions' drug laws may be sufficiently harsh so as to remove any opportunity for judges to rely on a less costly sanction (probation and/or jail). In other words, it is reasonable to assume that we might observe a very different relationship between probation fees and the odds of prison if only analyzing a sample of drug offenders from courts in states with much less punitive drug laws. The results of this model including an interaction between minimum cocaine sale sentence lengths and probation supervision fees revealed no significant interactions (results not shown). Thus, there is no evidence to suggest that probation supervision fees might exert more powerful negative influences on the odds of prison in jurisdictions that have less punitive drug laws.

Looking at the column showing the results predicting the odds of jail versus probation the findings reveal that, contrary to expectations, higher probation fees are associated with a significantly *higher* probability of jail versus probation. It is not entirely clear why this is the case since most defendants who receive jail also receive a period of probation in conjunction with a jail sentence. Thus, jurisdictions receive no financial benefit either way- in theory, they will collect probation supervision revenue in either of these outcomes. One possibility (which cannot be observed with these data) relates to plea bargains and the ability of defendants to actually afford the costs of probation supervision. If the costs of probation supervision are high for drug offenders (owing to special costs of drug monitoring that go above and beyond regular supervision fees captured here), these defendants may request as part of their plea bargain to do a short stint in jail, rather than a longer, and potentially costly probation term.

As is the case in the full sample, correctional spending is also associated with a significantly lower probability of being sentenced to prison, giving further support to the interpretation that higher correctional spending may signal a desire among states to reduce the costs associated with prisons by reducing the overall severity of the sentences they mete out. Finally, and contrary to expectations, states with more crowded prisons appear be *more* likely to mete out prison and jail sentences to drug offenders. Additional models were estimated to explore if this effect remained after controlling for punitive laws (results not shown) and the effect remained significant in both the full sample of cases and for drug offenders. This positive and significant relationship between the odds of prison and state prison capacity constraints is unexpected. The expectation is that crowded conditions will provoke courts to ease their reliance on prison sentences than

otherwise would be the case. However, judicial “adjustments” to ease prison crowding may take years for states and local governments to effectively address (Spelman, 2009). In addition, it may not be reasonable to expect the courts to adjust for prison crowding. States may control crowding more easily, and with fewer disruptions to the legal system, by regulating *releases* from prison, which would remove any need for courts to adjust the severity of sentencing.

Prosecutor caseload pressure, crime rates, and jail capacities did not emerge as significant predictors of either prison or jail. After controlling for these variables, along with level-one fixed effects, a significant amount of county and state-level residual variation remained to be explained. On the whole, these models suggest that that organizational constraints and burdens exert important influences on sentencing, and appear to account for a larger portion of the between-county variation in sentencing outcomes than do legal policy variables.

Although organizational perspectives serve as the backbone of early contextual sentencing research, recent work (outside of Pennsylvania) has largely neglected to examine the costs of punishment, capacities to imprison, and the ability in theory to offset some of these burdens through fee revenue from probationers. The findings here suggest that may be a significant oversight. Controlling for level-one case characteristics and crime rates, the extent of prison crowding, per capita corrections expenditures, and the amount of monthly supervision fees jurisdictions may charge probationers exert significant impacts on the severity of sentences courts impose. For the model predicting sentencing outcomes for the full SCPS sample, these characteristics help to account for a substantial portion of the residual variance not explained in the initial level-one model

(see Table 4.3.). The model does a much better job explaining portions of the mean-adjusted state-level residual variation reported in the level-one model. For instance, the organizational characteristics shown in Table 4.6 explain 11% of the county-level and 48% of the state-level residual variance in the odds of prison vs. jail that remained after controlling for the level-one covariates in the earlier model shown in Table 4.3. At the county-level, this reduction in the variance components after controlling for organizational context was greater than that accounted for by sentencing policy variables for both the full and drug offender samples (see Table 4.4). In sum, these findings suggest that organizational factors should merit more attention in future studies comparing sentencing outcomes across multiple states.

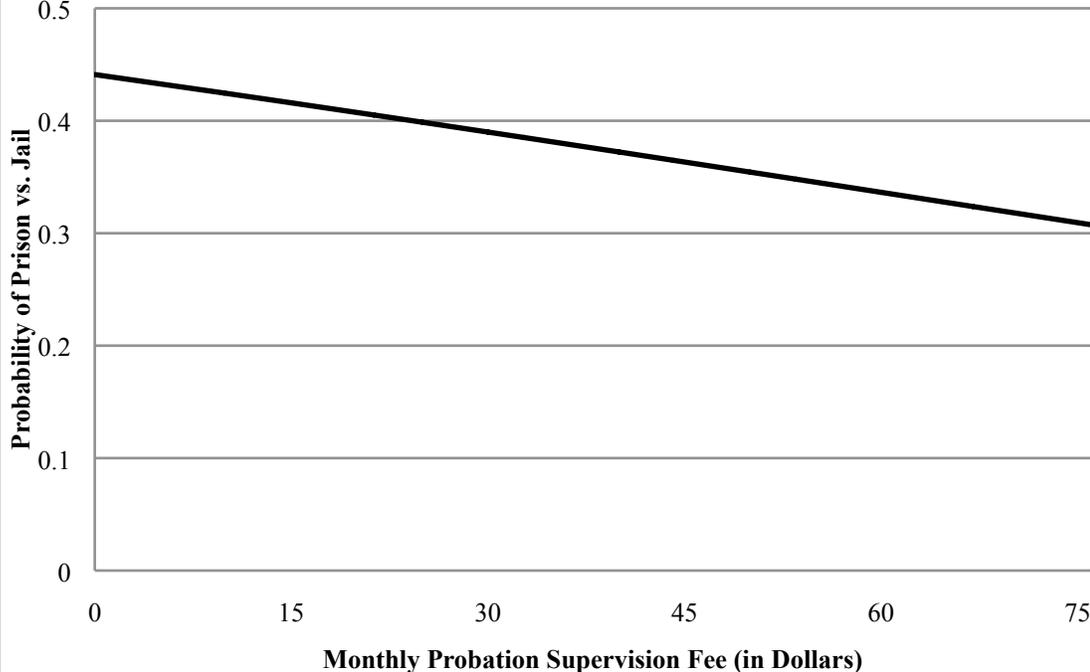
Table 4.7. Hierarchical Multinomial Regressions of Incarceration vs. Probation in Felony Cases: The Effects of Organizational Context

	(1) <i>(Full Sample of Felony Cases; N = 26,354)</i>			(2) <i>(Drug Cases; N = 10,167)</i>		
	Prison vs. Jail	Prison vs. Probation	Jail vs. Probation	Prison vs. Jail	Prison vs. Probation	Jail vs. Probation
<i>State and County Organizational Context</i>						
Prosecutor Caseload Pressure	.021 (.037)	.004 (.044)	-.0168 (.045)	.019 (.450)	-.002 (.050)	-.021 (.049)
Monthly Probation Supervision Fee (in \$)	-.012* (.005)	-.002 (.006)	.010 (.006)	-.010 (.006)	.005 (.007)	.015* (.007)
Jail Capacity Constraints	-.0005 (.004)	-.001 (.005)	-.001 (.005)	-.004 (.005)	-.005 (.006)	-.001 (.006)
Prison Capacity Constraints	-.007 (.005)	.011 (.006)	.017* (.006)	-.009 (.006)	.021* (.006)	.030* (.006)
Per Capita Correctional Expenditures	-.0002* (.00005)	-.0001 (.0001)	.0001 (.0001)	-.0001* (.00007)	-.0001 (.0001)	.0001 (.000)
<i>Control Variables</i>						
County Index Crime Rate	.00002 (.00004)	-.0001 (.0001)	.0001 (.0001)	-.000 (.0001)	-.0001 (.0001)	-.0001 (.0001)
Random Effects						
Level 2 Intercept	.379	.606	.575	.459	.659	.589
Chi-Square						
Level 3 Intercept	.501*	.445*	.776*	.988*	.648*	1.177*
Chi-Square	123.263	72.389	126.353	150.277	80.708	115.737

Note: Level 2 n = 91 counties; Level 2 n = 19 states; standard errors in parentheses.

*p < .05, two-tailed test

Figure 4.7. Predicted probability of prison vs. jail across levels of monthly probation supervision fees; Full sample (N = 26,354).



THE EFFECTS OF SOCIAL CONFLICT

The models estimating the effects of variables emphasized in conflict-based theories are presented in Table 4.8. Given the emphasis some researchers have placed on the role that the war on drugs plays in generating “symbolic” threats to the middle class (Sampson and Laub, 1993), both drug-specific and full sample models were estimated to separately examine the effects that racial and ethnic composition and threatening economic conditions might exert on sentencing. The results in Table 4.8 reveal that for both samples of offenders, none of the variables capturing community variation in social conflict exert any discernable influence on sentencing. Although the effects for racial composition are somewhat surprising given significant effects of percent black reported in several studies (see Table 2.1), the effects for economic threat mirror that of prior research and play no significant role in sentencing outcomes. Contrary to expectations, levels of fear yield no significant influence on the outcomes of cases in either the full sample or for drug cases. Moreover, the relationship is not in the theoretically expected direction. The most striking feature of the results reported in Panels A and B in Table 4.8 is the relative lack of any influence of social conflict measures on criminal case outcomes. This is surprising given the abundance of attention that most prior research affords to conflict-based explanations of formal social control (and sentencing especially). The findings here, though not definitive by any means, indicate that the importance of social conflict explanations of sentencing may be somewhat overstated and that the literature would benefit from expanding model specifications to include organizational and legal variables.

Table 4.8. Hierarchical Multinomial Regressions of Incarceration vs. Probation in Felony Cases: The Effects of Social Conflict

Panel A. All Felony Cases (N = 26,354)	(1)			(2)		
	Prison vs. Jail	Prison vs. Probation	Jail vs. Probation	Prison vs. Jail	Prison vs. Probation	Jail vs. Probation
<i>Indicators of Social Conflict/Threat</i>						
% Non-Hispanic Black	.007 (.015)	.018 (.016)	.011 (.017)	.003 (.013)	.018 (.016)	.015 (.017)
% Hispanic	-.003 (.009)	-.0001 (.010)	.003 (.011)	.011 (.009)	.006 (.011)	-.005 (.011)
Unemployment Rate	.033 (.070)	-.018 (.082)	-.051 (.084)	.025 (.064)	-.024 (.081)	-.049 (.081)
Economic Inequality (Gini coefficient)	4.23 (4.80)	-3.06 (5.46)	-7.29 (5.74)	1.55 (4.46)	-4.52 (5.50)	-6.08 (5.60)
Levels of Public Fear	--	--	--	-.022 (.031)	-.013 (.011)	.018 (.011)
<i>Control Variables</i>						
County Index Crime Rate	-.00005 (.00006)	-.0001 (.0007)	-.0001 (.0001)	.00001 (.0001)	-.0001 (.0001)	-.0001 (.0001)
Random Effects						
Level 2 Intercept	.398	.616	.602	.321	.613	.556
Chi-Square						
Level 3 Intercept	.965*	.555*	1.268*	1.012*	.492*	1.406*
Chi-Square	217.526	96.035	244.342	282.413	87.534	286.179

Panel B. Drug Cases (N = 10,167)	(1)			(2)		
	Prison vs. Jail	Prison vs. Probation	Jail vs. Probation	Prison vs. Jail	Prison vs. Probation	Jail vs. Probation
<i>Indicators of Social Conflict/Threat</i>						
% Non-Hispanic Black	.012 (.018)	.024 (.019)	.012 (.021)	.010 (.017)	.024 (.019)	.015 (.020)
% Hispanic	-.008 (.010)	.002 (.012)	.011 (.013)	.005 (.011)	.006 (.014)	.001 (.014)
Unemployment Rate	.054 (.083)	-.053 (.096)	-.107 (.101)	.045 (.079)	-.060 (.096)	-.105 (.099)
Economic Inequality (Gini)	3.19 (5.81)	-2.59 (6.49)	-5.78 (7.03)	.462 (5.59)	-3.44 (6.59)	-3.90 (6.95)
Levels of Public Fear	--	--	--	-.029 (.030)	-.009 (.013)	.020 (.013)
<i>Control Variables</i>						
County Index Crime Rate	-.0001 (.0001)	-.0001 (.0001)	-.00005 (.0001)	-.00003 (.0001)	-.001 (.0001)	-.0001 (.0001)
Random Effects						
Level 2 Intercept	.495	.813	.865	.432	.812	.805
Chi-Square						
Level 3 Intercept	1.397*	.658*	1.355*	1.439*	.614*	1.523*
Chi-Square	213.935	70.255	150.666	250.882	66.504	177.858

Note: Level 2 n = 91 counties; Level 3 n = 19 states; standard errors in parentheses. *p < .05, two-tailed test.

THE EFFECTS OF PUBLIC SENTIMENT

One of the primary goals of the current study was to examine the effects of variables that are emphasized in classic statements on the sociology of punishment, but have yet to be empirically tested due to a lack of community data that capture public sentiment. Without access to data like the GSS, researchers have been limited to estimating the influence of official crime data and Census-derived characteristics. As explained in the review of the literature in Chapter 2, when using these data researchers often come up short in their efforts to uncover which aspects of the local social climate matter most for understanding community variation in sentencing.

The variables considered in the models tested here stem from neo-Durkheimian perspectives on punishment, which emphasize the moral and cultural sensibilities that guide attitudes on crime and punishment. An implicit assumption of these models is that public opinion influences the decision making of prosecutors and judges. Multi-method contextual research documents that judges and prosecutors are in fact sensitive to public opinions (Myers and Talarico, 1987:31). Using theory as a guide, variables were chosen that tap the type of conservative values and ideologies that have been emphasized in Durkheim's work (e.g., religion) as well as contemporary survey literature as strong predictors of more punitive norms. The effects of these variables are assessed net of controls for the crime rate and all level-one case characteristics.

Turning first to the results for the full sample of defendants, Model 1 in Table 4.9 indicates that three of the variables measuring local normative climate exert significant influences on sentencing. Turning first to the odds of jail versus probation, the coefficient for *% favor capital punishment* ($b = .046^*$) indicates that defendants

experience a significantly higher probability of being sentenced to jail versus probation in jurisdictions where a higher proportion of GSS respondents support the death penalty. The odds ratio ($\exp[.046] = 1.05$), better illustrates the size of the effect. Every 1-percent increase in the percentage of respondents that support the death penalty corresponds to a 5-percent increase in the expected odds of jail versus probation. A 1-standard deviation increase (9%) in the percentage of respondents who support the death penalty is associated with a 9.5-percent increase in the log-odds of jail. These results are in line with theories that highlight the expressive aspects of punishment, and as such, that punishment will be more severe in jurisdictions with higher levels of support for more repressive sanctions. However, the effect is not highly consistent across the other two outcomes. For instance, the beta coefficients for prison vs. probation ($b = .031$) and prison vs. jail ($b = -.015$) are not significant, and in the latter case, not in the expected direction.

Contrary to a sizeable body of research to suggest otherwise, the findings indicate that religious fundamentalism is associated with more lenient sanctioning. The results for the model contrasting the odds of prison versus probation reveals that higher levels of Christian fundamentalism is associated with a significantly lower odds of being sentenced to prison ($b = -.167^*$). The odds of prison versus probation change by 27% for every 1-standard deviation (1.93) change in the fundamentalism index. Though numerous studies have shown that fundamentalists exhibit more punitive orientations, other studies have shown that Christians are also more likely to believe in forgiveness and redemption, a finding consistent with the pattern of effects displayed in Table 4.9.

One other variable entered into Model 1 equation is significant- all else equal, the odds of prison versus jail are significantly higher for defendants processed in jurisdictions characterized by higher levels of anti-black resentment among whites. This is also the case for the sample of drug offenders ($b = .084^*$). The graph in Figure 4.8 more clearly illustrates the effect of racial resentment on the sentences applied to the full sample. Recall that the racial resentment index is composed of a series of standardized GSS items. Thus, the mean of the overall summed index approximates zero and scores above zero represent counties with higher average racial resentment. Scores below zero represent counties with lower levels of racial resentment. The graph in Figure 4.8 charts changes in the probability of prison versus jail across counties with varying levels of racial resentment. In counties with below average prejudice, the probability of prison falls at or below about 30%. At average levels of prejudice, the probability of prison increases to around 40% and then, and then at relatively high levels of prejudice, the probability of prison approaches 60%- nearly doubling that of defendants convicted in the lowest-prejudice counties. Though no prior multilevel sentencing research has examined the effects of levels of racial prejudice within communities on sentencing outcomes, this finding conforms with a number of surveys which find that a key factor explaining whites' desire for more punitive crime control is racial stereotypes and prejudice (Barkan and Cohn, 1994; Unnever et al., 2006). Consistent with the Durkheimian tradition of punishment, these findings suggest that more punitive legal sanctions are a reflection of a punitive community social climate (measured here as prejudice).

Though it is not clear why racial attitudes are associated with more punitive sanctions toward all offenders (i.e., rather than just toward black defendants), this finding

is intriguing and suggests a need for future research to better understand the mechanisms responsible for linking racial prejudice and resentment to more punitive attitudes generally.

Turning next to the results for drug offenders displayed in Model 2, the findings exhibit some interesting patterns not observed for the full sample of offenders. First, the results for racial resentment and religious fundamentalism mirror those for the sample as a whole. The effects of Christian fundamentalism are significant and negative, again suggesting that sentencing is less punitive in jurisdictions containing where Christian fundamentalism is more prevalent. The sizes of the fundamentalism coefficients are not only slightly larger in the drug sample, but fundamentalism has a more sweeping effect on sentencing by impacting not only the odds of prison versus probation ($b = -.207$), but now also influencing the probability of being sentenced to jail versus probation ($b = -.213^*$) (an effect that is not significant for the full sample). Together, these two patterns suggest that fundamentalist religious beliefs may be especially relevant in the sentencing of drug offenders, and is consistent with research highlighting the compassionate and forgiving aspects of the Christian faith (Applegate et al., 2000). The magnitude of each of these effects is illustrated in Figure 4.9 for both prison and jail. The graph shows the range of estimated probabilities at varying levels of fundamentalism. Controlling for levels of crime and level-one case characteristics, the line depicting the relationship for jail shows that in counties where fundamentalism is highest, the probability of jail is as low as 10%. However, at the lowest levels of fundamentalism (between -1 and -3), the probability of jail doubles to .30 (or 30%). The line depicting the relationship for prison versus probation similarly demonstrates that the

odds of prison approach 50% in jurisdictions where fundamentalism is exceptionally low, and below 20% (.20) in jurisdictions where fundamentalism is the most widespread.

The findings in Model 2 also indicate that although support for capital punishment exerted significant influences on the odds of jail versus probation in the full sample, levels of capital punishment are not related to sentencing in drug cases. This makes intuitive sense, since this measure of punitive attitudes is related to support for a sanction that only applies to serious violent crime.

Finally, the results also reveal a negative and significant effect of social trust ($b = -.185^*$) on the odds of jail versus probation. Specifically, the beta coefficient suggests that a 1-unit increase in the social trust index is associated with a .83 decline in the average odds of jail [$\exp(-.185) = .83$], which corresponds to a 17-percent change in the odds. This is consistent with hypotheses grounded in Braithwaite's theory of reintegrative shaming, which argues that social trust and mutual interdependence are aspects of community social structure that promote informal forms of social control and forgiveness, and that discourage more repressive forms of control such as prison. The magnitude of the effect of social trust, expressed in conditional probabilities, is depicted in Figure 4.10. The graph shows that in communities where social trust is lowest (i.e., -5.9) the probability of jail versus probation is approximately 40% (.40). The probability declines substantially however as levels of social trust approach average levels (i.e., a score of '0'). At the mean level of trust, drug offenders have on average about a 25% probability of going to jail (compared to 40% for offenders in counties with very low levels of trust). As you can see though, this probability drops considerably and in counties that have the highest levels of social trust (i.e., scores of 4.6), the

probability of jail is only 13% (.13)- a 67% difference from the estimated probability (.40) of jail in counties with the lowest levels of social trust. It is noteworthy that levels of social trust within the community only impact the sentencing of drug offenders, rather than serious property or violent offenders. This suggests that the effects of social trust may hinge on the seriousness of the crime, as the public may consider certain violent offenders to have less rehabilitation potential. For example, an abundance of research suggests that over the past thirty years the public has grown more punitive in the sense they are likely to report supporting the death penalty, three-strikes policies, and incapacitation. However, these same studies also reveal that at the same time the public embraces punitive measures, they still cling to the belief that treatment is a necessary component of corrections (Cullen, Cullen, and Wozniak, 1988; Sundt, Cullen, Applegate, and Turner, 1998). In particular, this line of research has shown that when survey respondents are asked about non-violent offenders, that the American public is especially supportive of intermediate sanctions and restorative justice measures (Cullen, Fisher, and Applegate, 2000).

SUMMARY OF FINDINGS

Table 4.10 provides a summary of the results of each of the models presented thus far. Turning first to the results for the full sample, overall the analyses indicate that the strongest and most consistent contextual predictors of sentencing are the organizational aspects of the court and state correctional environment, along with measures of public sentiment. The findings that organizational context are highly relevant for understanding sentencing is consistent with organizational efficiency models which stress the

importance of considering cost and efficiency. Specifically, the hypotheses predicted that courts located in states burdened with higher costs of punishment would impose on average more lenient sentencing, and that the differential opportunity for jurisdictions to offset the high costs of prison through charging for probation would explain why some courts exhibit a lower average probability of imposing prison terms. Both hypotheses received support in the current study.

The analyses also revealed that public sentiment is highly relevant for understanding why certain jurisdictions impose more punitive sentences. Specifically, defendants face a significantly higher probability of being incarcerated in jail versus probation when sentenced in communities where support for the death penalty is more widespread, and that the probability of prison (versus jail) is significantly higher in communities where more whites express feelings of anti-black racial prejudice. It is important to point out however, that the effects of these variables (as well as others examined throughout the study) are not constant across outcome types. For example, the effect of capital punishment only impacts the probability of jail, but exerts no significant impact on prison. In a similar pattern, racial prejudice increases the probability of prison versus jail, but bears no influence on the odds of prison versus probation or jail versus probation. Though the importance of a few of these variables for understanding sentencing have been established in one other study (Baumer and Martin, 2011), these findings suggest a need for future research that replicates these types of models on different samples of cases, and with outcomes other than incarceration to explore whether the effects of these measures of sentiment are sensitive to the coding of the outcome or

stage of the criminal justice system (i.e. pretrial detention, sentence length, and conviction).

Contrary to expectations, legal policies do not figure prominently in sentencing outcomes. Determinate sentencing is one exception, though the positive direction of the relationship reported in this study is contrary to the findings from macro level studies that show that incarceration rates are lower, and their growth much slower, in the context of determinate sentencing (Greenberg and West, 2001; Jacobs and Carmichael, 2001; Smith, 2004; Zhang et al., 2009). The finding reported here does not necessarily contradict aggregate level incarceration research since those studies do not examine the cross-sectional influence of policy on actual sentencing decisions. The meaning of the relationship reported here is difficult to interpret with the data at hand, but two interpretations are possible. The first is that determinate sentencing may have been implemented in these jurisdictions as part of a broader package of reforms aimed at increasing the overall severity of punishment. For example, when California and a handful of other states adopted determinate sentencing in the 1970s and 1980s, these legislatures also amended their state penal codes to increase penalties for many felony offenses and to state that the express purposes of punishment would become deterrence. Second, and equally plausible, is that judges in determinate states may be more likely to impose incarceration than judges elsewhere because their decisions control both the sentence and time each offender will serve (Sabol et al., 2002).

Table 4.10 also illustrates the relative lack of support for the hypotheses derived from the social conflict perspective. The social threat perspective features prominently in the broader literature on social control, and it is particularly influential in sentencing

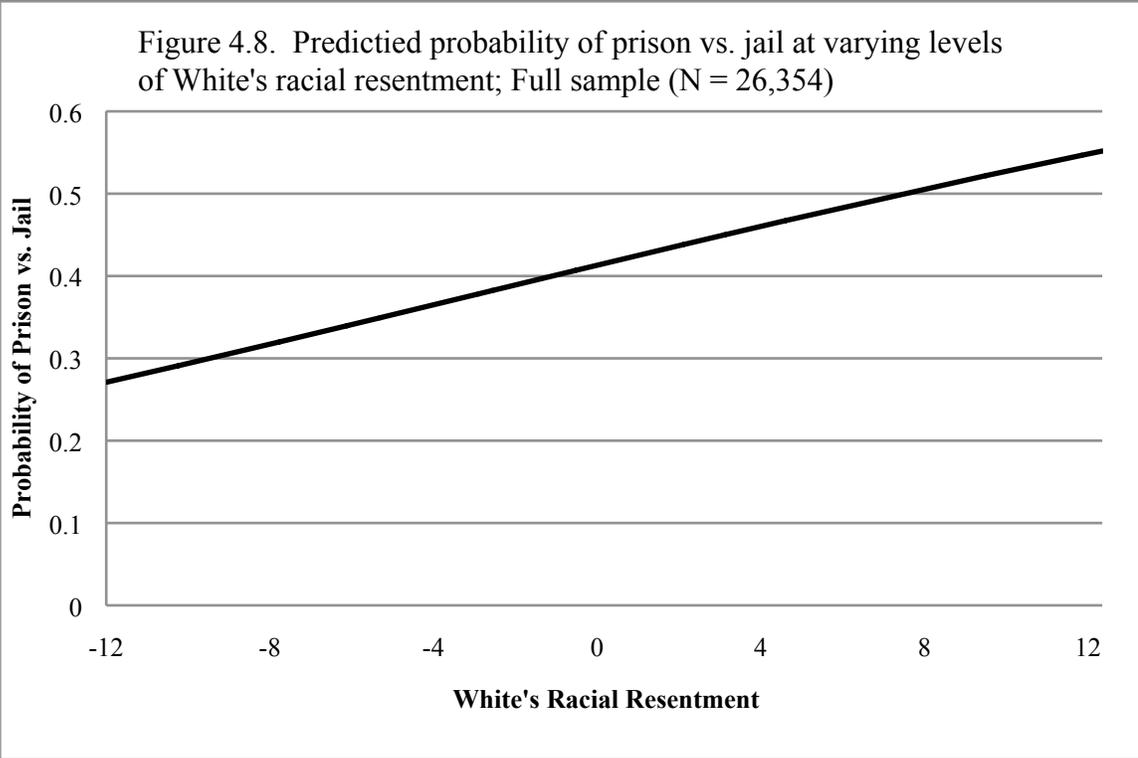
research (see e.g., Myers, 1987; Wang and Mears, 2010). However, the results reported here find that racial and ethnic composition, levels of fear, and the economic well being of communities have little relevance for predicting the outcomes of felony cases.

The results of the analyses for drug offenders are similar, although a few exceptions bear mentioning. First, higher probation fees do not appear to decrease the odds of prison for drug offenders. Second, support for capital punishment is not relevant for the sentencing of drug offenders, while social trust appears to uniquely impact the legal sanctioning of drug offenders. Third, legal statutes appear to be more relevant for understanding variation in penalties applied to drug offenders. The probability of incarceration for drug offenders is significantly higher in jurisdictions that require longer minimum sentence lengths for the sale of 1 ounce of cocaine. However, drug offenders receive significantly more lenient sentences in three-strikes states and in states that require offenders to serve a higher percentage of their minimum or fixed incarceration term. The nature of these findings are not altogether different from the effects of policy variables for the full sample, where both three-strikes and TIS are both negative (though not significant). The findings are a bit of a paradox; these two variables are typically interpreted as evidence of a more punitive legal culture. Yet, at least in urban court settings during this time period (1998-2004), the presence of these more punitive laws appear to be met with more lenient sentencing for drug offenders, even after adjusting for crime rates, crowding, and the severity of state drug laws.

Table 4.9. Hierarchical Multinomial Regressions of Incarceration vs. Probation: The Effects of Public Sentiment and Social Cohesion

	(1) <i>(All Felony Cases; N = 26,354)</i>			(2) <i>(Drug Cases; N = 10,167)</i>		
	Prison vs. Jail	Prison vs. Probation	Jail vs. Probation	Prison vs. Jail	Prison vs. Probation	Jail vs. Probation
Fixed Effects						
<i>Public Sentiment and Cohesion</i>						
Southern Jurisdiction	.288 (.521)	.446 (.481)	.157 (.488)	.010 (.640)	.524 (.569)	.514 (.574)
% Favor Capital Punishment	-.015 (.023)	.031 (.023)	.046* (.023)	-.031 (.027)	.021 (.027)	.052 (.028)
Religious Fundamentalism Scale	-.004 (.076)	-.167* (.083)	-.163 (.085)	.006 (.091)	-.207* (.099)	-.213* (.102)
Social Trust Scale	.034 (.057)	-.060 (.064)	-.094 (.066)	.072 (.069)	-.113 (.008)	-.185* (.079)
Conservative Political Ideology	-.404 (.818)	-.876 (.914)	-.472 (.934)	.118 (.941)	-.231 (1.07)	-.349 (1.09)
Racial Animus	.075* (.032)	.023 (.035)	-.052 (.036)	.084* (.037)	.024 (.041)	-.061 (.042)
<i>Control Variables</i>						
County Index Crime Rate	.00005 (.00005)	-.00005 (.0001)	-.0001 (.0001)	.00003 (.0001)	-.0001 (.0001)	-.0001 (.0001)
Random Effects						
Level 2 Intercept	.391	.566	.605	.464	.746	.792
Chi-Square						
Level 3 Intercept	.791*	.568*	.567*	1.179*	.764*	.739
Chi-Square	192.891	94.713	131.150	196.072	78.589	103.525

Note: Level 2 n = 91 counties; Level 3 n = 19 states; standard errors in parentheses. *p < .05, two-tailed test.



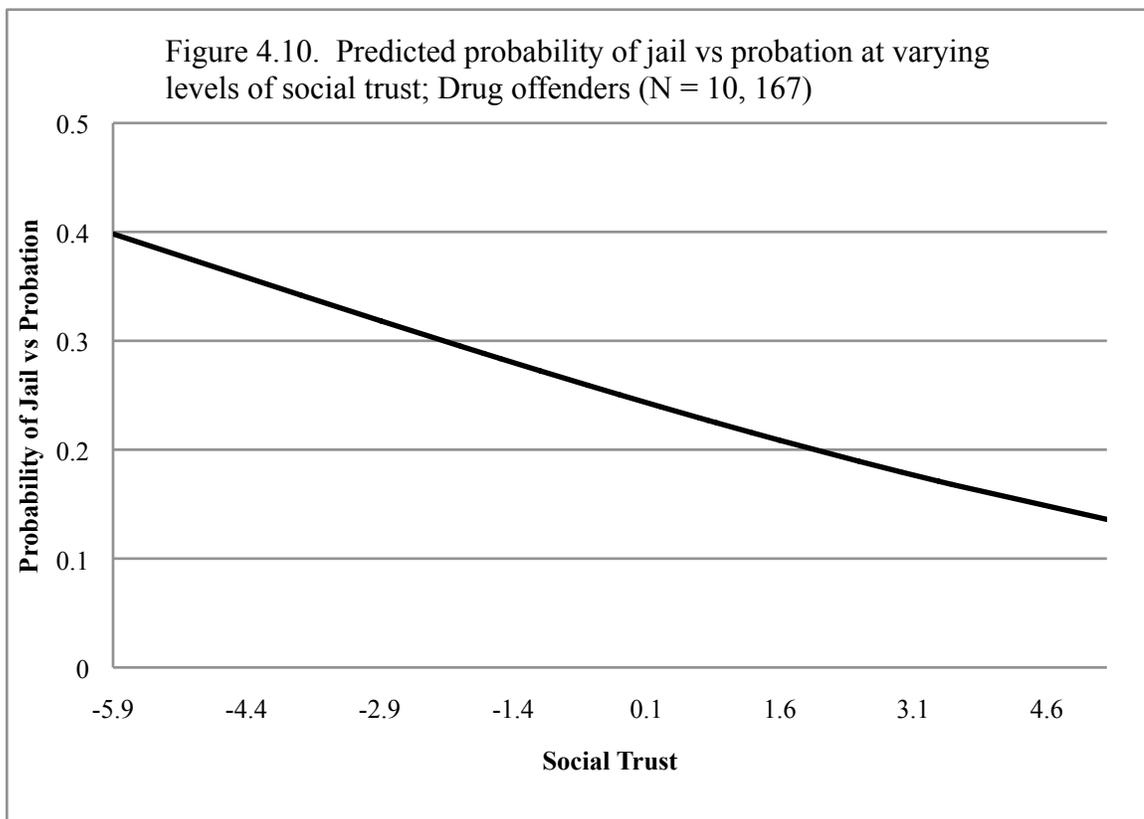
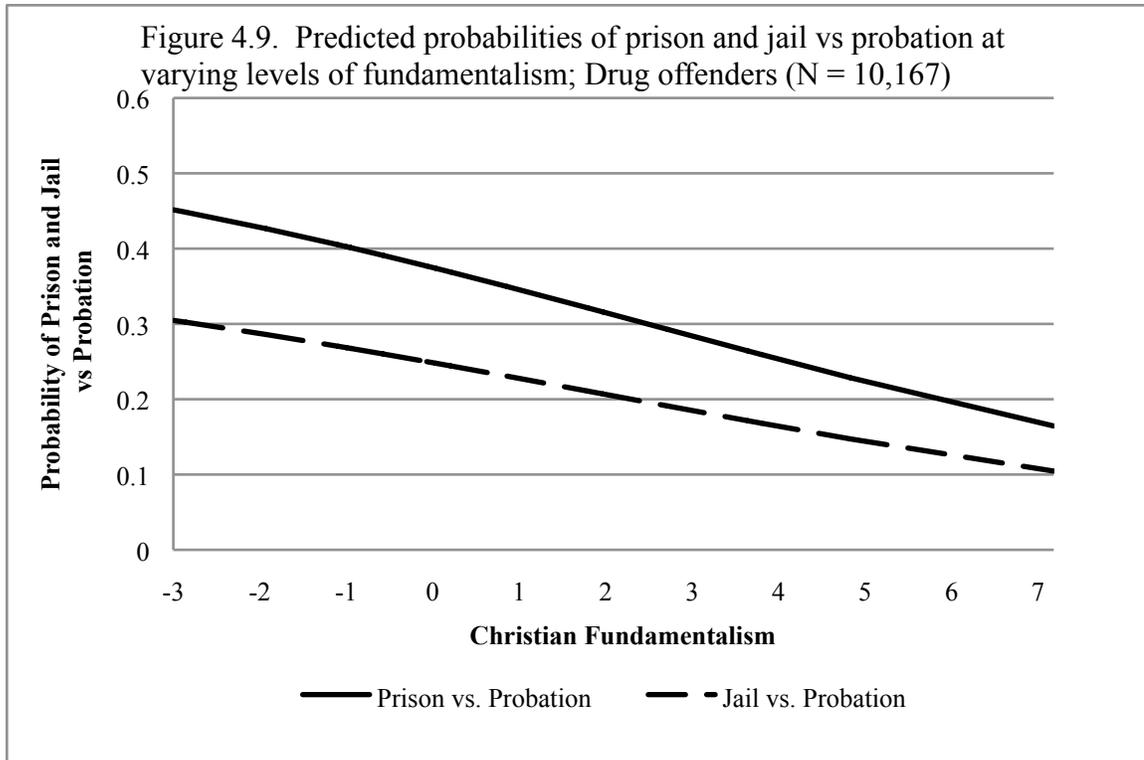


Table 4.10. Summary of Findings and Support for Hypotheses Predicting Jurisdictional Influences on Felony Sentencing Outcomes

	<i>(All Felony Cases; N = 26,354)</i>			<i>(Drug Cases; N = 10,167)</i>		
	Prison vs. Jail	Prison vs. Probation	Jail vs. Probation	Prison vs. Jail	Prison vs. Probation	Jail vs. Probation
Fixed Effects						
<i>Legal Context</i>						
Presumptive Guidelines	n.s.	n.s.	n.s.	n.s.	n.s.	n.s.
Voluntary Guidelines	n.s.	n.s.	n.s.	n.s.	n.s.	n.s.
Determinate Sentencing	n.s.	+	n.s.	n.s.	n.s.	n.s.
% Sentence Offenders Required to Serve	n.s.	n.s.	n.s.	—	—	n.s.
Three Strikes Law	n.s.	n.s.	n.s.	—	n.s.	n.s.
Mandatory Enhancement Score	n.s.	n.s.	n.s.			
# Mandatory Enhancements for Drug Offenses				n.s.	n.s.	n.s.
Min Sentence 1 oz Sale of Cocaine				n.s.	+	n.s.
<i>Organizational Context</i>						
Prosecutor Caseload Pressure	n.s.	n.s.	n.s.	n.s.	n.s.	n.s.
Monthly Probation Supervision Fee (in \$)	—	n.s.	n.s.	n.s.	n.s.	+
Jail Capacity Constraints	n.s.	n.s.	n.s.	n.s.	n.s.	n.s.
Prison Capacity Constraints	n.s.	n.s.	+	n.s.	+	+
Per Capita Correctional Expenditures	—	n.s.	n.s.	—	n.s.	n.s.
<i>Social Threat/Conflict</i>						
% Non-Hispanic Black	n.s.	n.s.	n.s.	n.s.	n.s.	n.s.
% Hispanic	n.s.	n.s.	n.s.	n.s.	n.s.	n.s.
Unemployment Rate	n.s.	n.s.	n.s.	n.s.	n.s.	n.s.
Economic Inequality (Gini)	n.s.	n.s.	n.s.	n.s.	n.s.	n.s.
Levels of Public Fear	n.s.	n.s.	n.s.	n.s.	n.s.	n.s.
<i>Public Sentiment/Cohesion</i>						
Southern Jurisdiction	n.s.	n.s.	n.s.	n.s.	n.s.	n.s.
% Favor Capital Punishment for Murderers	n.s.	n.s.	+	n.s.	n.s.	n.s.
Religious Fundamentalism Scale	n.s.	—	n.s.	n.s.	—	—
Social Trust Scale	n.s.	n.s.	n.s.	n.s.	n.s.	—
Conservative Political Ideology	n.s.	n.s.	n.s.	n.s.	n.s.	n.s.
Whites' Anti-Black Racial Animus	+	n.s.	n.s.	+	n.s.	n.s.
<i>Control Variables</i>						
County Index Crime Rate	n.s.	n.s.	n.s.	n.s.	n.s.	n.s.

Note: N.S. denotes null findings.

A final model that combines each of the variables found to be significant is provided in Table 4.11 for both the full sample and the subsample of drug offenders. The results in Panel A indicate that with the exception of the coefficient for determinate sentencing and support for capital punishment, all of the effects reported in previous models remain significant and in their original direction. The most consistent predictors of sentencing are organizational context and measures that tap punitive social climates—racial prejudice and religious fundamentalism. The results for drug offenders reported in Panel B indicate that the results are fairly robust. With the exception of the variable measuring time-served requirements, the effects reported in previous models remain statistically significant after controlling for other theoretically relevant predictors. The implications of these findings for theory and for advancing contextual sentencing research are discussed in Chapter 5.

Table 4.11. Full Regression Analyses of Competing Models Predicting Jurisdictional Influences on Felony Sentencing Outcomes

Panel A.		<i>(All Felony Cases; N = 26,354)</i>		
		Prison vs. Jail	Prison vs. Probation	Jail vs. Probation
Fixed Effects				
<i>Legal Context</i>				
	Determinate Sentencing	.193 (.395)	.582 (.443)	.389 (.444)
<i>Organizational Context</i>				
	Monthly Probation Supervision Fee (in \$)	-.012* (.005)	-.004 (.006)	.008 (.006)
	Prison Capacity Constraints	-.007 (.004)	.008 (.005)	.016* (.005)
	Per Capita Correctional Expenditures	-.0001* (.00004)	-.0001 (.00005)	.0001 (.0001)
<i>Public Sentiment/Cohesion</i>				
	% Favor Capital Punishment for Murderers	-.013 (.018)	.007 (.020)	.020 (.021)
	Religious Fundamentalism Scale	-.031 (.058)	-.154* (.069)	-.122 (.070)
	Whites' Anti-Black Racial Animus	.060* (.025)	.017 (.030)	-.042 (.031)
Random Effects				
	Level 2 Intercept	.364	.535	.575
	Chi-Square			
	Level 3 Intercept	.409*	.482*	.460*
	Chi-Square	110.7	90.4	81.6
Panel B.		<i>(Drug Cases; N = 10,167)</i>		
		Prison vs. Jail	Prison vs. Probation	Jail vs. Probation
Fixed Effects				
<i>Legal Context</i>				
	% Sentence Imposed Offenders Required to Serve	-.018 (.009)	-.015 (.008)	.003 (.010)
	Three Strikes Law (all current offenses qualify)	-1.20* (.574)	-.657 (.480)	.547 (.608)
	Min Sentence (months) 1 oz Sale of Cocaine	.011 (.012)	.031* (.010)	.020 (.013)
<i>Organizational Context</i>				
	Monthly Probation Supervision Fee (in \$)	-.009 (.005)	.009 (.006)	.018* (.006)
	Prison Capacity Constraints	-.006 (.005)	.023* (.006)	.029* (.006)
	Per Capita Correctional Expenditures	-.0001* (.00006)	-.0001* (.00006)	.00003 (.00007)
<i>Public Sentiment/Cohesion</i>				
	Religious Fundamentalism Scale	.005 (.077)	-.245* (.083)	-.249* (.068)
	Social Trust Scale	.079 (.063)	-.103 (.069)	-.182* (.068)

White Racial Animus	.063*	.047	-.017
	(.028)	(.031)	(.031)
Random Effects			
Level 2 Intercept	.408	.575	.506
Chi-Square			
Level 3 Intercept	.674*	.381*	.767*
Chi-Square	108.8	59.1	101.6

Note: Level 2 n = 91 counties; Level 2 n = 19 states; standard errors in parentheses. *p < .05, two-tailed test.

CHAPTER 5

CONCLUSION

SUMMARY OF GOALS

Prior research has shown that when controlling for legally relevant factors, the outcomes of criminal cases depend to a significant degree on the jurisdiction in which defendants are processed. Coining this pattern “justice by geography,” Feld (1991) observed over 20 years ago that both the functioning styles and punishment preferences of courts are sensitive to the differences in bureaucratic functioning and the diversity of the populations that courts serve. This further confirmed existing research showing that the courts draw upon their broader social environment to define acceptable going rates of punishment (Eisenstein et al., 1988; Myers and Talarico, 1987; Ulmer, 1997).

In the years since this groundbreaking research, the methodological sophistication and geographic scope of contextual sentencing research has grown exponentially. Aided by the availability of new and more diverse sentencing data, a number of studies document that the certainty and severity of punishment varies significantly across communities. These studies also indicate that the degree of jurisdictional variation tends to be smaller in studies analyzing data from guidelines states such as Pennsylvania and greater in studies using data collected from jurisdictions located across multiple states and diverse sentencing structures. This research has greatly advanced our understanding of the ways in which community context influences both state and federal court sentencing. As a whole though, the research examining the community contexts of sentencing has yielded few answers to the basic question of what social contexts appear

to make for more punitive legal sanctioning. Though many studies report that judge-level characteristics, the courtroom environment, and the broader social climate influence the severity of sentencing, the inconsistency of findings for any given variable across these studies makes it difficult to formulate a consistent narrative about which aspects of social context matter and precisely how they impact the severity of sentencing. Thus, despite a rich theoretical background encouraging contextual approaches to understanding legal decision-making, the sources responsible for jurisdictional variation in sentencing remain elusive (Myers and Talarico, 1987).

This ambiguity about which contextual factors are most relevant reflects three tendencies in the literature: a) this area of research is primarily informed by studies that analyze cases within only a handful of states (most of which operate under policies designed to limit the potential for community variation in sentencing); b) an over-emphasis on social structural characteristics at the expense of other theoretically-relevant variables; and c) a lack of serious consideration among multi-state contextual studies for the ways in which legal and organizational contexts contribute to geographic differences in the severity of sentencing within the U.S. Accordingly, a major goal of this study was to broaden contextual sentencing research in three ways: by employing a more comprehensive theoretical framework, by exploring policy questions surrounding the adoption of various sentencing reforms, and by bringing new (and more direct) measures to bear on the question of whether community sentiments impact the sentencing process in any significant way. The remainder of this chapter discusses the findings and their implications as they pertain to the above-mentioned goals.

SUMMARY OF FINDINGS

Legal Context

One of the questions that motivated the current study was whether sentencing policies that have been singled out as key causes for the growth in mass incarceration increase the probability of imprisonment net of legally relevant case characteristics. Specifically, the analyses sought to determine if offenders sentenced in jurisdictions with more punitive laws faced significant disadvantages compared with offenders sentenced in less punitive legal contexts. The findings provide little evidence that structured sentencing reforms significantly influence the severity of punishment. The findings also do not suggest that sentencing is more punitive in jurisdictions that have more mandatory minimum enhancements, three-strikes laws, or more punitive time-served requirements. These policies were intended to capture variation in the relative harshness of each jurisdiction's legal culture. If policies like these do drive incarceration growth, it does not appear to be doing so by increasing the probability of going to prison, at least during this time period of study (1998-2002) and in these urban counties. Perhaps this is an artifact of the sample being from urban courts, which may be more lenient than rural courts and resistant to state efforts to make sentencing more punitive. But there are reasons to doubt that this is true. In Pennsylvania, Ulmer (1997) found that urban court judges relied much more heavily on guidelines and rules because it helped large courts to process cases more efficiently. Also, if policies that aim to increase the severity of punishment are increasing incarceration rates, you would expect to observe the effects of policy in precisely these kinds of courts; when weighted, these data from the SCPS represent over half of all defendants sentenced in the U.S. each year.

Cross-level interaction models were used to test hypotheses predicting that presumptive guidelines and three-strikes laws indirectly impact the severity of sentencing by increasing the role that prior criminal history plays in sentencing decisions. Contrary to expectations, the findings do not indicate that offenders with a prior criminal history face a greater odds of prison in three-strikes states compared with repeat offenders sentenced elsewhere. Cross-level interaction models exploring the potential conditioning effects of presumptive guidelines reveal significant interactions between guidelines and having a prior felony conviction. The nature of the interaction is not uniform across different outcome categories, however. The findings show that offenders with a prior felony conviction face a significantly higher probability of jail versus probation than repeat offenders in non-presumptive guideline states. Thus, consistent with expectations, presumptive guidelines do appear to indirectly increase the severity of punishment by placing greater weight on prior criminal history. However, in guideline states the odds of prison versus jail is *less* impacted by a prior felony conviction compared to non-guideline states. In other words, having a prior felony conviction will generally increase the odds of prison versus jail for all offenders, but the effect of a prior record is actually larger in *non-guideline* states that do not direct judges to specifically consider prior offending history. This may reflect the inability of judges in guideline states to impose more severe sanctions than the guidelines dictate, whereas judges in states without guidelines typically sentence from broad penalty ranges and if they choose, may consider additional factors besides offense severity and criminal history at sentencing. This broad discretion may mean that sentencing is more severe for repeat offenders convicted in states that do not control judicial discretion in the way that presumptive guidelines do.

A substantial portion of the growth in incarceration rates can be attributed to the increase in incarceration for drug crimes (Blumstein and Beck, 2000). States have experimented greatly with the severity of their drug laws over the past thirty years, with some having a completely separate set of sentencing guidelines for drug offenders (e.g., Washington and Minnesota) (Stemen et al., 2005). Given the diversity of ways states have approached the punishment of drug offenders, the analyses also explored the effects of structured sentencing, mandatory minimums, three-strikes laws, time-served requirements and specific drug policies on the sentencing of drug offenders. The results reveal that policy differences bear a greater influence on the sentences applied to drug offenders, although not necessarily in the expected way. The findings show, for example, that the odds of prison are significantly *lower* in three-strikes states and lower in states that require offenders to serve a longer portion of their sentence before becoming eligible for parole. To the extent that the presence of three-strikes laws and more stringent truth in sentencing requirements indicate a punitive legal culture, these findings are unexpected. One could speculate that these negative effects indicate reluctance on the part of prosecutors and judges to using three-strikes on drug offenders, as well as a reluctance in TIS states to send drug offenders to prison where those will serve most of the duration of their sentence. Related to this point, one possibility is that three-strikes and TIS requirements may be correlated with the amount of money each state devotes to drug treatment for prisoners. For instance, some states offer comprehensive drug treatment in their prisons (Carmichael, 2010). If this is the case, then judges may be more likely to send drug offenders to prison in those states in hopes that the offender receives drug and alcohol treatment. Future research should explore if variation in the

resources devoted to drug treatment in prisons helps to explain why more punitive laws appear to reduce the chances that drug offenders will go to prison. If states with punitive laws spend more money on locking offenders up and keeping them there longer, and then devote little resources to rehabilitating them in prison, then this may reduce the propensity of prosecutors and judges to seek prison terms for drug offenders.

The findings regarding the effects of more punitive drug laws were mixed. Contrary to expectations, drug offenders sentenced in states with more mandatory enhancements for drug-related crimes do not face a significantly higher odds of being incarcerated compared to drug offenders convicted in states with fewer or no drug enhancements. However, the minimum penalties for the sale of cocaine exert a strong influence on the odds of going to prison versus probation. Compared with offenders sentenced in states with no minimum incarceration for the sale of 1 ounce of cocaine, the probability of prison versus probation doubles for drug offenders convicted in the most punitive states, where the minimum penalty for selling 1 ounce of cocaine is 10 years in prison (see Figure 4.3). Prior contextual sentencing studies have not examined the effects of drug policy on the specific sentences applied to these defendants, but the results reported here suggest that one way forward in understanding the impact of differences in the severity of laws may be to examine the effects of specific statutes on specific types of samples (e.g., the effects of mandatory enhancements on the sentencing of violent offenders, or comparing the effects of sex offender laws on the sentencing of sex offenders). More broadly, these findings suggest that differences in state legal codes are an important part of the story on why sentencing varies significantly across jurisdictions in the U.S. Future research that pools defendants from courts in multiple states should

explore more thoroughly the implications of this for both multilevel sentencing research and aggregate level research on incarceration rates. As one study's authors recently observed, the bulk of aggregate level research on incarceration rates is consumed with estimating the effects of socioeconomic structural factors and that this "traditional approach dismisses the variation in state-level sentencing and corrections policies and confounds the impact of ideological and political covariates of imprisonment with state-centered interventions." (Stemen and Rengifo, 2011: 175). To the extent that this is true, this raises doubts about the findings from previous studies that have used the SCPS to explain variation in sentencing. Without controls for differences in statutory penalties, the significant results reported in these studies for racial composition, crime rates, and religious fundamentalism may be misleading. It could be the case that these types of community attributes are more influential in shaping the severity of state law than the severity of judicial behavior at sentencing, and that social structural variables hold little relevance once controlling for variation in the content of the law. This study provided only a modest step towards examining the impact that penal codes have on generating variation in the sentencing of drug offenders within the U.S. Nevertheless, the findings are similar in spirit to those reported by Baumer and Martin (2011), who find that variation in the severity of state penalties for murder explain a substantial portion of inter-county variability in the sentencing of convicted murderers. Thus, future research should attempt to make strides toward better accounting for differences across states in the severity of penal codes.

Organizational Context

Another shortcoming of the literature this study attempted to address was the absence of variables measuring jurisdictional differences in the organizational constraints and opportunities to punish more harshly. The models presented here tested the hypotheses that as the costs of prison are higher, and the physical capability to imprison lower, that courts will be less likely to mete out prison terms. The analyses also tested whether fiscal offsets in the form of charging probationers supervision fees help to account for why some jurisdictions, on average, are significantly less likely to imprison offenders. On the whole, the results for both the full sample and drug cases support these hypotheses. Even after adjusting for level-one characteristics and other level-two variables, the final model (Table 4.11) shows that the amount of monthly probation fees courts charge probationers remains a significant negative influence on the odds of prison versus jail. As expected, correctional spending is also associated with less punitive sanctions for both the full sample as well as drug offenders, even after controlling for prison crowding (which could be expected to explain away the effect of high correctional costs) and levels of crime. The results for probation fees do not extend to the sentencing of drug offenders, however. Additional research is needed to explore whether drug offenders are a more costly probation burden for states (hence, removing any fiscal benefit of placing them on probation in lieu of incarceration), or if drug offenders are on average less likely to pay their probation supervision fees. Overall, the findings reported here suggest that these organizational attributes play an important, and frankly underappreciated, role in explaining why sentencing behavior varies across jurisdictions. Though state fiscal “crises” are gaining more attention in today’s economic context, the

burden of corrections costs on states has been an issue for over 10 years. In 2002, twenty-five states were forced to reduce their corrections budgets because state revenue could not meet the demands of correctional budgets. That same year, the Vera Institute conducted a study on the budgetary consequences of “get tough” sentencing policies and found that many states slowed corrections spending by repealing mandatory minimums and reclassifying certain offenses in such a way that they no longer automatically result in prison sentences (Wilhelm and Turner, 2002). These types of developments, coupled with the findings reported here, suggest that the costs of punishment are important to consider. These issues have been overlooked in the broader sentencing literature. As Engen recently observed, “The preoccupation with detecting and explaining unwarranted disparity in sentencing research has been accompanied by a near-exclusive emphasis on individual-level social psychological theories of decision-making and, to a lesser extent, on contextual theories (e.g., racial threat) that still emphasize subjective decision-making as the central causal mechanism. Theories must devote equal attention to understanding the institutional features such as sentencing laws, that provide the context in which these decision-making processes take place.” (2009: 333). The findings presented here echo Engen’s call that greater attention be given to institutional factors.

Social Threat

A particularly noteworthy finding is that measures of social conflict and fear of crime are unrelated to sentencing outcomes. With the exception of racial composition, this continues a trend observed in the literature of weak and inconsistent effects of economic conditions on sentencing severity. Conflict theories will undoubtedly retain

their importance in the macro and multilevel research on crime control, but the collective weight of the evidence shown in Table 2.2 and reported in the current study suggest that researchers seek out new variables and measures of social context to interpret community variation in sentencing. Though nearly every study reviewed in Chapter 2 considers the effects of economic conditions on sentencing, conflict models of sentencing might be better geared towards understanding the sentencing of disadvantaged and minority defendants instead of felons generally.

Public Sentiment

Finally, a major aim of this study was to expand on prior research by examining the effects of social attributes emphasized in theories that draw heavily from Durkheim's conceptualization of punishment as rooted in moral and cultural sensibilities. The findings from models that include measures of public sentiment suggest that the relationship between socio-cultural context and sentencing is complex. For the full sample, the final model (Table 4. 11) indicates that the probability of incarceration is lower in more fundamentalist communities, and greater in jurisdictions with higher levels of racial prejudice. For the drug sample, the results are similar with one exception- the sentencing of drug offenders appears to be sensitive to levels of social trust. Braithwaite stresses that more therapeutic forms of punishment will flourish in more "communitarian" social environments, where interpersonal trust and mutual interdependence thrive (1989). These findings bolster the arguments that public attitudes are an important part of understanding reactions to crime. Garland in particular has emphasized that while punishment has many purposes, one that has grown increasingly

important is conveying public sentiment. Evidence of this can be seen in political efforts to adopt crime control policies that sought to alleviate public fear and to articulate public sentiments about how best to control crime. What is intriguing about much of the theoretical work on the politics of crime control is that the effects of public sentiment are often assumed to be in the direction of *stricter* punishment. For instance, Garland observes that “punishment- in the sense of expressive punishment, conveying public sentiment- is once again a respectable, openly embraced, penal purpose and has come to affect not just high-end sentences for the most heinous offenses but even juvenile justice and community penalties” (2001: 9). While this is certainly true to some extent, the findings from this study also highlight those aspects of local culture that help reduce the severity of punishment for offenders. In sum, the results reported here suggest that variables tapping the normative environment in which sentencing occurs can help us understand why some communities are more punitive than others. Because the effects of these variables have not been examined in much previous research, future work should replicate these analyses with data from different jurisdictions, different time periods and for a broader range of court outcomes such as pretrial arraignment, charging, conviction, and sentence length decisions.

STUDY WEAKNESSES AND FUTURE RESEARCH

The weaknesses of this study are plentiful, but perhaps the most critical are the lack of more detailed information on the circumstances of each offense and the lack of a control for the “presumptive sentence” for each individual case. More detailed information on the specific circumstances and severity of each conviction charge would

permit a more comprehensive analysis of the extent to which sentencing variation is explained by jurisdictional differences in legal codes. For instance, if we know what felony class each offense in the SCPS fell into, whether it was “aggravated”, and perhaps victim and location attributes, it would be possible to search each state’s penal code for specific sentence ranges and then model variation in sentencing as a function of variability in the severity of statutes. As it stands, the current study is unable to control for presumptive sentences. While I attempt to partition any variance that might be due to state legal codes with a three-level model, this is insufficient. At the same time, the lack of a control for presumptive sentence may be less consequential when analyzing variation in the decision of whether to incarcerate. In an effort to gauge the extent to which states vary in the presumption of incarceration for each of the felony crime types included in the SCPS, I reviewed the state penal codes for each of the nineteen states included in the sample. Most of these states typically allow incarceration for *any* felony, and all states loosely define felonies as any crime punishable by incarceration. However, in guideline states the penalties become much more specific and for a number of felony offenses incarceration is not the presumptive sentence if an offender has no prior record. Thus, the current study likely does a poor job of accurately modeling variation in sentencing for defendants convicted in guideline states.

Other limitations of the study, and ones that pervade sentencing research generally, include the lack of information on victim attributes and defendant socioeconomic status, potential selection bias problems, and especially, more detailed information on prosecutorial decisions regarding initial charging and charge reductions. This latter point is particularly important for modeling sentencing outcomes in states that

have adopted determinate sentencing, mandatory minimums, and guidelines. As several studies have found, these reforms have displaced much of the discretion traditionally afforded to judges and placed it in the hands of prosecutors. Because these reforms make sentencing more predictable and certain, prosecutors are able to control the sentencing process through charging decisions. This unintended effect of reform has been observed in states such as Washington, Minnesota, Ohio, and in the case of mandatory minimums, Oregon (Engen, 2009; Merrit et al., 2006; Wooldredge, 2009). Thus, the current study may in fact not be modeling judicial behavior at all in jurisdictions that operate under determinate and/or presumptive guideline schemes. This raises a larger concern about the ability of this study to draw any conclusions about the effects of policies on sentencing. With the exception of the estimates pertaining to the effects of drug statutes, it is difficult to discern from this type of analysis exactly whether or how policy shapes sentencing. A more thorough analysis devoted strictly to policy questions would need to examine how various court outcomes (not just the in/out decision) are influenced by policy. For instance, the current study finds that for drug offenders TIS time-served requirements and three-strikes are significantly and negatively related to the risk of prison. If court personnel simply ignore those laws (perhaps they disagree with how punitive the statutes are), we would expect to observe a null effect of these punitive policies on sentencing. However, it is a challenge to explain why, controlling for crime rates and prison capacities, sentencing for drug offenders would be significantly more lenient in jurisdictions with these laws. There are several possibilities. One is that the effects of policy may be conditioned by community social context, or by a court's caseload pressure- suggesting that courts will blunt the effects of more punitive laws if

the laws interfere with case processing efficiency or if local prosecutors do not feel the public desires such punitive laws. There is evidence to suggest this is true in the case of three-strikes in California, where the law was passed as part of a voter ballot initiative. In their ethnographic study of chief prosecutors' implementation of three-strikes in California counties, Harris and Jesilow (2000) found that though many prosecutors were uncomfortable with the severity of the law, they fully implemented three-strikes provisions because it was popular among voters. Another possibility is that more punitive sentencing policies change how courts "do business" (Tonry, 2006). For instance, there is good reason to suspect that in states with some of the more punitive laws, plea bargains and charging "behave" differently, and not merely because prosecutors may disagree with the severity of the laws legislatures pass (e.g., see Merritt et al., 2006; Tonry, 2006). For example, one of the concerns about the effects of mandatory minimums and 3-strikes was that these laws would clog the courts with trials by reducing the incentive for a defendant to plea. Thus, the laws would increase the number of defendants who would rather take their chances at trial in order to avoid pleading guilty and being sentenced under such harsh terms (Clark, Austin, and Henry, 1997; Olsen, 2000). If this is true, then prosecutors and defense attorneys in these jurisdictions must "hustle" to secure plea bargains, and in order to secure them, might have to offer much more beneficial bargain terms than prosecutors in jurisdictions without such harsh policies (presumably, pleading is a less frightening proposition in a jurisdiction with less punitive statutes). If this is in fact happening, then plea bargain deals may offer more lenient sentencing options in these states compared with plea deals elsewhere. Consistent with a deterrence/economic perspective, this would increase

convictions (the certainty of punishment), and drive down the overall severity of punishment. Because a dummy indicator for plea bargain would not be able to capture these nuances, we could conceivably observe that sentencing is more lenient in three-strikes jurisdictions because most of the SCPS sample (95%) pleads guilty. An interesting question thus arises: are the benefits of plea bargains greater in jurisdictions with more punitive legal policies in ways that explain why in the current study nearly every indicator of punitive legal policy tends to be negatively associated with the probability of imprisonment (though not significantly).

There is some evidence from California that supports the argument that policy may actually lower the severity of punishment by altering plea bargaining practices. In the aforementioned study on the implementation of three-strikes in California, Harris and Jesilow (2000) document that judges were resistant to the passage of the law because it disrupted the ability of prosecutors to secure pleas, which in turn increased the caseload pressure on judges. The authors explain that before the passage of three-strikes plea bargains were easily accomplished because the court workgroup knew what the informal going rates were for most crimes. However, after the adoption of three-strikes, defendants became reluctant to plead guilty, which disrupted the efficient processing of cases. In response, judges in one county began circumventing the authority of prosecutors who were consistently charging defendants under the provisions of the law (at least initially). Judges frequently made promises to delete any prior strikes on a defendant's record in exchange for a plea bargain. By striking prior felonies to avoid sentencing under the three-strikes law, the authors of the study found that 80% of the

time judges did this, the plea deals offered the lowest possible minimum sentence in exchange for the defendant's plea (Harris and Jesilow, 2000: 197-198).

Future research on the effects of policy *could* explore these types of issues and form a more comprehensive understanding of the effects of policy on sentencing, if widely used databases like the SCPS and the Pennsylvania Commission on Sentencing included more detailed data on prosecutorial decision making. This is especially important in the present day context of sentencing in large urban courts where plea bargaining is almost always the mode of conviction, and where presumably the importance of the prosecutor and defense attorneys is more important than ever for understanding the disposition of felony cases. Finally, and building off of this theme of examining the *process* of punishment, future research should explore whether some of the relationships observed here between social context and sentencing might be mediated by earlier decisions such as the granting of bail and the ability to secure pretrial release. For example, a good deal of sentencing research uses data sets from Washington and Pennsylvania that do not contain information on pre-conviction decisions such as charging and pretrial detention (Bushway and Piehl, 2007). This is not a trivial limitation; pretrial detention in the current study is among the strongest predictors (second only to offense severity) of incarceration. It would be useful to know if the relationships between social context and sentencing that are reported in studies that rely on post-conviction data sets are mediated by earlier decisions, especially pretrial detention. Such research could greatly advance the broader literature on court processing by clarifying more precisely the stages and decision-makers that are most sensitive to the effects of social context.

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**APPENDIX A. SENTENCING STRUCTURES AND LAWS REGULATING HABITUAL
OFFENDING AND PROBATION SUPERVISION IN STATES USED IN ANALYSES, 1998-2004[†]**

State	3 Strikes Law ¹	Structured Sentencing	Determinacy and Type of Structured Sentencing	Adoption Date	Truth in Sentencing ²
Alabama	No	Yes	Voluntary Sentencing Guidelines	2000	33%
Arizona	No	Yes	Determinate Sentencing with Presumptive Terms (Single term for each offense class)	1978	85%
California	Yes	Yes	Determinate Sentencing with Presumptive Terms; Parole Retained (Presumptive single term for each specific offense)	1976	50%
Connecticut	No	No	No Structured Sentencing System in Place		50%
Florida ³	No	Yes	Determinate Sentencing with Presumptive Minimum Terms	1994	85%
Georgia	No	No	No Structured Sentencing System in Place		33%
Illinois	No	No	Determinate Sentencing		50%
Indiana	Yes	Yes	Determinate Sentencing with Presumptive Terms (Single term for each offense class)	1977	50%
Maryland	No	Yes	Voluntary Guidelines	1983	25%
Michigan	Yes	Yes	Voluntary Guidelines Presumptive Guidelines	1985-1998 1999-Present	100%

Missouri	Yes	Yes	Voluntary Guidelines	1997	15%
New Jersey	Yes	Yes	Presumptive Sentencing (Single term for each offense class)	1977	33%
New York	No	No	No Structured Sentencing System in Place		83%
Ohio	No	Yes	Determinate Sentencing with Presumptive Terms (Single term for each offense class)	1996	100%
Pennsylvania	No	Yes	Presumptive Guidelines	1982	100%
Tennessee	No	Yes	Presumptive Guidelines	1989	30%
Texas	Yes	No	No Structured Sentencing System in Place		25%
Virginia	No	Yes	Determinate Sentencing with Voluntary Guidelines	1995	85%
Washington	No	Yes	Determinate Sentencing with Presumptive Guidelines	1984	66%

†: Information on each state's sentencing criteria up to 2002 was adapted from Stemen, Rengifo and Wilson (2005). Information for 2004 collected by study author.

- 1: State has a 3 Strikes Law where any current felony conviction qualifies as a strikable offense.
- 2: Percentage of the minimum or fixed sentence that felony offenders will serve.
- 3: As explained in Stemen et al. (2005, p. 66) Florida repealed its presumptive guidelines in 1994 and replaced them with the "Criminal Punishment Code" which determines the lowest possible sentence that judges must impose without a departure. Judges may then sentence offenders up to the statutory maximum for the offense in question. As Stemen et al. (2005) explain, "Thus, the Code is not really a set of sentencing guidelines used to determine a specific sentence, but functions simply to determine the minimum sentence that a judge must impose without a departure."

Appendix B, continued. Correlations among Jurisdictional Variables included in Analysis of Legal Sanctions Applied in Felony Cases (N=91 county-years).

		(14)	(15)	(16)	(17)	(18)	(19)	(20)	(21)	(22)	(23)	(24)	(25)	(26)
(1)	Index Crime Rate	-.216*	-.209*	.510*	-.007	.257*	.497*	.016	.381*	-.126	.418*	.507*	.332*	.383*
(2)	Determinate Sentencing	.331*	-.024	-.518*	.305*	-.255*	-.212*	.080	-.162	.101	-.199	-.029	.434*	-.272*
(3)	Presumptive Guidelines	-.162	-.105	.267*	-.239*	.192	.067	.001	.028	.017	.080	-.026	.125	.197
(4)	Voluntary Guidelines	-.195	-.189	.144	-.299	-.216*	-.287*	.078	.256*	-.167	.129	-.043	-.228*	-.120
(5)	(TIS) % Sentence Offenders Must Serve	-.099	.370*	-.083	-.057	.086	.014	.241*	-.297*	-.128	-.231*	-.248*	-.124	-.079
(6)	Three Strikes Policy	.340*	-.061	-.254*	.214*	.005	-.221*	.023	-.281*	.284*	-.156	-.266*	-.008	-.374*
(7)	Mandatory Sentencing Enhancement Score	.026	.049	-.373*	.169	-.089	-.194	.314*	-.128	.186	-.237*	-.041	.375*	-.146
(8)	Mandatory Enhancements for Drug Offenses	-.624*	-.287*	.350*	-.273*	.100	.179	-.103	.397*	-.079	.368*	.392*	-.057	.414*
(9)	Severity of Cocaine Possession Sanctions	-.114	-.381*	.374*	-.105	.144	.333*	-.377*	.331*	.140	.352*	.511*	.400*	.539*
(10)	Severity of Cocaine Sale Sanctions	-.126	-.178	.148	.091	.066	.202	-.296*	.228*	.169	.203	.331*	.289*	.225*
(11)	Monthly Probation Supervision Fee	-.121	.178	-.295*	.196	-.229	-.015	.104	.278*	.005	.114	.410*	.448*	.155
(12)	Prosecutor Caseload Pressure	-.277*	-.364	-.067	-.081	-.114	-.255*	.071	.224*	-.099	.196	.074	.050	-.012
(13)	Jail Capacity Constraints	.114	-.063	-.020	-.117	-.107	-.192	-.089	.038	.049	-.068	-.106	-.015	-.106
(14)	Prison Capacity Constraints	--	.005	-.237*	.106	-.114	-.097	.047	-.524*	.171	-.308*	-.327*	.058	-.290*

	(14)	(15)	(16)	(17)	(18)	(19)	(20)	(21)	(22)	(23)	(24)	(25)	(26)
(15) Per Capita Corrections Expenditures		--	-.280*	.425*	.080	.113	.202	-.098	-.041	-.451*	-.230*	-.230*	-.364*
(16) % Population Black	(14)	(15)	(16)	(17)	(18)	(19)	(20)	(21)	(22)	(23)	(24)	(25)	(26)
(17) % Population Hispanic			--	-.301*	.605*	.597*	-.200	.238*	-.219*	.366*	-.227*	-.127	.478*
(18) Unemployment Rate				--	.284*	.253*	.374*	-.017	-.230*	-.069	-.058	.043	-.267*
(19) Income Inequality (Gini)					--	.633*	.049	-.109	-.169	.045	-.041	-.163	.135
(20) Fear of Crime						--	-.085	.144	-.224*	.212*	.327*	.068	.360*
(21) Southern Jurisdiction							--	-.099	-.384*	.129	-.194	-.229*	-.286*
(22) Levels of Social Trust								--	-.106	.507*	.631*	.234*	.471*
(23) Religious Fundamentalism									--	-.412*	-.008	.293*	-.031
(24) Politically Conservative Ideology										--	.460*	.005	.509*
(25) % Favor Capital Punishment											--	.602*	.737*
(26) Anti-Black Racial Animus												--	.383*

* p < .05