ABSTRACT

Title of Dissertation: PROSECUTION AND SENTENCING OF WHITE COLLAR CRIME IN FEDERAL COURT: UNDERSTANDING PATHS AND PROCESSES

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The question of whether 'white collar' crimes are treated more leniently by society dates to the first use of the phrase by Edwin Sutherland in 1939. One of the most important ways in which this leniency may manifest is in the criminal prosecution and sentencing of violations of law. And yet, the question of whether and how white collar cases are treated differently than other types of crimes has not received sufficient attention. Prior research findings have been clouded by inconsistent effects and inconsistent definitions of white collar crime and may have limited generalizability under modern sentencing regimes. This dissertation reconsiders the question of white collar leniency for a sample of white collar and comparable crimes referred for federal criminal prosecution between 2009-2011 and followed through 2013 using the Federal Justice Statistics Program. Specifically, this research considers how case complexity affects the likelihood of plea bargains, and how these bargains in turn affect sentencing outcomes. Additionally, this dissertation explores

whether white collar cases receive more lenient sentencing outcomes, and the effect that different definitions have on substantive conclusions.

PROSECUTION AND SENTENCING OF WHITE COLLAR CRIME IN FEDERAL COURT: UNDERSTANDING PATHS AND PROCESSES

by

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CH.1 INTRODUCTION

> Sociologists and social psychologists have often noted that deviance on the part of high status offenders of a group provokes extreme reactions. Sanctions tend to be much less severe than those imposed on lesser group members… or much more severe… but apparently rarely are sanctions the same. (Wahrman, 1970:485)

When Sutherland (1939) first called attention to "white collar crime" in his presidential address to the American Sociological Association, his intention was to shine light on a topic that, although prevalent, was largely overlooked by scholars who studied crime. In contrast to the long-held assumption that crime was confined to the socially disadvantaged, Sutherland argued that people in power, particularly in the course of their occupations, committed acts that were against the law with great regularity. Since this time, the United States has experienced several waxing and waning periods of increased awareness of and attention to white collar crime, including (but not limited to) Sutherland's first documentation of widespread corporate wrong-doing (coinciding with a great deal of regulatory action on the part of the federal government), the Savings and Loan crisis of the 1980s (see Simpson 2002 e.g.), up through the accounting scandals of the early 2000s and the Great Recession of 2008.

Over time, the public demand for criminal sanctions for white collar crime has increased (Cullen et al. 1982; Cullen et al. 2009, Holfreter et al. 2008; Unnever et al. 2008). And yet, Sutherland (1949) and other white collar scholars have long noted that white collar crimes, especially those committed in the course of business, are often sanctioned in forums other than criminal court (i.e., civil and regulatory proceedings). As a result, research on corporate sanctions has generally been focused on punishments meted out by other agencies in other venues (e.g. Ayres

and Braithwaite 1992; Clinard and Yeager 1980; Simpson 1986; Sutherland 1949). From these studies it is clear that the bulk of punishments levied at corporations for white collar crime are administrative or civil in nature. It is the rare case in which corporate crime is tried in the same venue as other, more seemingly traditional crimes, such as robbery or homicide, even though some forms of white collar crime are seen as equally serious, if not more serious, than traditional offenses (Cullen et al. 1982; Holland 1995; Huff et al. 2010 Simpson et al., *In Progress*). In some cases, corporations and individuals involved in them are subject to harsh criminal penalties; Jeffrey Skilling was sentenced to 24 years for his involvement in the Enron violations (Brickey 2006). Yet these cases are the exception, rather than the rule. There were fewer than 200 organizations criminally sentenced each year between fiscal years 2012 and 2016 (United States Sentencing Commission 2016) in spite of widespread estimates of corporate wrongdoing (Sutherland 1949; Clinard and Yeager 1980). The limited research on organizations receiving federal criminal sentences suggests that firms that make it to the point of criminal sentencing are a select group: they are often firms that exist solely for criminal gains (opposed to legitimate firms caught doing bad acts) or are often small and family owned enterprises (Cohen 1991; Piquero and Davis 2004).

In recent years, some have argued that it is not possible (or, at least, advisable) to punish many white collar crimes, that some companies, especially financial institutions, (and the individuals within them), are "too big to jail". Such arguments have prompted concern from academics (Garrett 2014; see also Pontell, Black, and Geis 2014), politicians (Hensarling 2016, Morgenson 2016), and the

public (Taibbi 2013) after the most recent financial crisis. The phrase has become synonymous with punishment avoidance for powerful people and powerful companies. What this rallying cry does not acknowledge however, is that simply because very few notable cases of white collar crime are *sentenced* criminally does not mean that there are few white collar cases that are eligible to be criminally *prosecuted*. In other words, the belief that certain white collar crimes are beyond the reach of the criminal justice system focuses far too much on the end of the process (sentencing, especially incarceration) without acknowledging that criminal sentencing exists within a larger system through which cases are moved and diverted at various decision points. To understand whether some or all white collar crimes receive leniency requires a wider lens than just sentencing.

Previous research on white collar sentencing, mostly conducted during the 1970s and 1980s, tended to focus on differences between the sentences of white collar offenders and those of traditional offenders. Were white collar offenders, who are more social privileged, treated more leniently in criminal proceedings as well? Unfortunately, in spite of nearly a decade of concentrated attention, multiple approaches, and labor-intensive data scraping, the sum total of this research offered little clarity to the question. Research found mixed effects, including: (a) no effect of status, (b) a negative relationship between status and sanction, and (c) a positive relationship between status and sanction. The effect of status thus seemed to be predictably inconsistent. The picture is even further muddied by inconsistent definitions of white collar crime in these studies – regulatory crimes, financial crimes, and specific offenses (like Medicaid fraud) have each been used to test the

question of white collar leniency, with little discussion or understanding of how these selections may affect the likelihood of certain conclusions.

In addition, most early white collar sentencing research is limited by its inability to respond to two key issues in modern sentencing research. First, most studies were conducted, or use data, from time periods before the implementation of sentencing guidelines in many states. Guidelines were installed to limit judicial discretion, which may mean that white collar offenders are no longer beneficiaries of leniency. Further, the implementation of some guidelines systems (in particular, those for the federal system) were designed to bring about harsher sentences for white collar crimes in the face of this perceived leniency (Stith and Cabranes 1998, Alexander et al 1999, Murphy 2002). The generalizability of these studies to modern sentencing regimes thus seems tenuous at best. Second, these early studies were not informed by broader sentencing research; the theories employed to explain white collar sentencing lacked the broader context of how judges sentence. While the scholars approaching these issues have asked related questions and offered similar explanations at times, nevertheless the two lines of inquiry have intersected only tangientially. These limitations suggest that findings from this early body of research have limited generalizability for contemporary questions of white collar leniency.

In addition, to date, there is very little understanding of the criminal case processing of white collar crimes, whether defendants are organizations or individuals. Studies of white collar cases in the criminal justice system are both outdated and segmented, generally viewing as isolated the practices of referring

cases for prosecution (Shapiro 1980), prosecution and defense from prosecution (Mann 1985), and sentencing (e.g., Albonetti 1994; Wheeler et al. 1988). However, a key insight of broader sentencing research, particularly in the last five years, has been the acknowledgement that judges are increasingly not the sole determinants of offender sentences, both because of increased attention to the role of prosecutors and because of particular forms of influence prosecutors have under guidelines systems. In a review of sentencing research, Baumer (2013) notes that future research should seek to understand the ways in which early case decisions, e.g., those made by prosecutors, shape and inform the sentencing options that are accessible to judges later on. Where guidelines are presumptive or mandatory or have very high rates of fidelity, prosecutors may use expected sentences for given charges to manipulate defendant outcomes either through initial charges filed (e.g., filing or not filing a mandatory minimum charge; Ulmer et al., 2007), through fact-bargaining (thus limiting information available to judges at sentencing; Sarner, 1996), or through charge reductions during plea deals. Decisions to change the formal charges in a case, for example, can structure a judge's available sentencing options (both due to statutory restrictions and sentencing guidelines). Additionally, actors in the criminal justice system may rely on cues from other agents to make their own decisions; in this way, judges may receive "signals" from prosecutors, and use these signals to upwardly or downwardly adjust punishment (Hill, Harris, and Miller, 1975). In doing so, some cases may be “tracked” into more severe case processing outcomes at sentencing because they have not received leniency at previous prosecution decision-points. In practice, this means that cases which

receive advantage at one stage in the process may be more likely to receive them in later stages as well (though some theories suggest otherwise), producing multiple interacting processes that work to produce disparity in sentences.[1] Alternatively, judges may see these bargains as signals of cases receiving unfair discounts, and react with harsher sentences. Unfortunately, even the most recent white collar crime sentencing research has yet to take this into consideration (e.g., Maddan et al., 2012). There has been little acknowledgement or attempts to unpack the role of earlier decisions by prosecutors on later decisions made by judges. This is problematic given that prosecutorial decisions are particularly influential in determining sentences at the federal level (Ulmer et al., 2016), and potentially exert even larger effects on white collar crime cases due to the rarity, complexity, and unique role of the prosecutor in white collar cases (see e.g., Katz, 1979).

This research seeks to advance both white collar sentencing literature as well as sentencing literature more broadly by responding to these calls for studies of sentencing that look beyond judges and toward the role of prosecutors. In doing so, I draw on both the rich history of white collar sentencing research with the equally rich literature on sentencing of traditional crimes. I seek to describe, compare, and explain the sentencing outcomes of white collar cases and a set of comparable crimes in the federal criminal justice system. Drawing on the insights

[1] No study of sentencing would be complete without the caveat that disparity in sentences does not necessarily mean that there is *discrimination* in sentences. However, this research aims to identify (and unpack) the perceived problem of disparity in white collar case outcomes at the federal level. The *source* of this disparity, whether it is related to case characteristics (both legally relevant and otherwise), offender characteristics, or others, will determine whether or not the difference suggests practices that are *discriminatory*, i.e, that result from decisions to treat otherwise like cases differently based on legally irrelevant considerations, particularly as related to offender characteristics.

of recent literature, I also acknowledge the interdependencies and contingencies that develop across actors in the criminal justice system by unpacking prosecutorial decision-making at earlier points as well as considering the way in which these outcomes ultimately influence sentences. Understanding white collar sentencing requires a broader consideration of the ways in which cases are systematically differentiated and diverted at various points. These distinctions become meaningful for future decision-making, both within and across agencies of the justice system.

This dissertation has three research goals: (1) describing the process of white collar case movement through the federal criminal justice system, (2) exploring the demographic and case characteristics associated with prosecution and sentencing outcomes and (3) considering the effect of different white collar definitions and identification strategies on substantive conclusions. The analyses reflect prior white collar sentencing literatures by offering both within-white-collar analyses and between-crime-type analyses to explore potential disparities between white collar and traditional crimes.

To do so, I employ data from the Federal Justice Statistics Program [FJSP]. These data are collected from administrative data spanning multiple stage of the criminal justice system, including the United States Marshals Service [USMS], Executive Office of the U.S Attorneys [EOUSA], the Administrative Office of the U.S. Courts [AOUSC], and the U.S. Sentencing Commission [USSC]. I focus my analyses on decisions made by prosecutors and judges at multiple points in the criminal justice process including count and charge reductions and sentencing outcomes. These data are of particular value not only in the access to the same cases

across decision-makers and agencies, but also because federal (opposed to state) courts offer the best opportunity to study white collar offenses (Nagel and Hagan 1982). I use multiple theories of sentencing and prosecutorial decision-making to guide this research, drawing on commonalities and insights from uncertainty avoidance, focal concerns theory, typescripts theory, and the status-shield and liability hypotheses.

SUMMARY AND OUTLINE

The current study thus offers a contribution both to white collar and criminal sentencing literature. It responds to calls from reviews of white collar research for renewed attention to white collar criminal sentencing, particularly using new and updated data (Simpson 2013; see also Maddan et al. 2012). While a small body of more recent research has developed since Simpson's (2013) review, in general these analyses have not paid much attention to broader trends in sentencing research. In addition, this study also offers a contribution to sentencing research by taking a processural analytic approach, looking at multiple stages in the criminal justice system, an approach suggested by multiple reviews of sentencing research (Baumer, 2013; Spohn, 2015; Ulmer, 2012). Finally, this research offers a new glimpse into the window of prosecutorial discretion. Johnson et al. (2016) note that prosecutor decisions are key and yet generally not well studied in sentencing literature.

This dissertation proceeds as follows. In Chapter 2, I discuss the extant literature relevant to the problem at hand, beginning with a discussion of definitional issues. Also discussed is the literature on prosecutors and sentencing

research and theories to explain these outcomes - in general but also with specific focus on white collar crimes. I highlight the limitations of this research in regards to both its method and theory. The chapter concludes with several research hypotheses. Chapter 3 introduces the FJSP and discusses the methodological approach that will be taken to test those hypotheses. Chapter 4 describes the matching procedures used to link the data across successive stages and presents some descriptive statistics about the sample. Chapter 5 develops and presents a comprehensive descriptive picture of the movement of white collar cases through the federal criminal justice system across the agencies in the data. Chapter 6 explores the case- and individual-level characteristics associated with prosecution of white-collar cases. Chapter 7 considers the ways in which prosecutorial outcomes (specifically, negotiated pleas) affect sentencing leniency for white collar cases and whether white collar cases are treated differently than other crimes. Finally, Chapter 8 offers a concluding discussion of the results and suggests directions for future research.

CHAPTER 2. PROSECUTING AND SENTENCING WHITE COLLAR CRIME

INTRODUCTION

> Since Sutherland, sociologists and criminologists have argued that the lenient treatment extended to high-status, white-collar criminals can be found at all stages in the system, but that it is particularly evident at two points: first, in the decision to prosecute, and second, in sentencing decisions. (Tillman, Calavita, and Pontell, 1996: 53)

Scholars typically have been dually focused on the incidence and punishment of white collar offenses. This is likely in part because there appear to be few means through which we can identify white collar crimes when they happen other than when they are detected and responded to by either regulatory or criminal justice agencies (Reiss and Biderman 1980, Simpson and Yeager 2015). Even studies concerned with the origin of white collar offending inevitably seem to select on the dependent variable, lending themselves also to studies of white collar punishment.[2] For example, the Yale White Collar Studies selected on convicted cases from the Administrative Office of the United States Courts in ten federal districts, and spent considerable time studying both the characteristics of offenders (Weisburd 1991), as well as how they are sentenced (Weisburd 1991; Wheeler et al. 1982, 1988). Thus, it is no surprise that there is a rich extant literature concerned with the sentencing of white collar crimes. However, even the earliest studies of white collar scholars have also noted that white collar cases do not randomly (or even consistently) arrive at the criminal sentencing stage; Sutherland's (1949) seminal work acknowledged that regulatory filtering reduced the cases even

[2] An exception may be recent efforts to identify white collar crimes using open source data. This work may allow scholars to identify crimes (at least those that have been identified as crimes by media outlets and the like) without waiting until the offenses are processed through the regulatory, civil, or criminal systems. See Maimon and Simpson (2016).

referred for criminal prosecution, and both Shapiro (1980) and Mann et al. (1985) note the ways in which regulators, prosecutors, and defense attorneys are involved in constructing the pool of offenders that are ultimately processed through the criminal justice system.

The purpose of this chapter is to discuss what is known about the prosecution and sentencing of white collar cases. While studies exist at both stages specific to white collar cases, this chapter situates these offense-specific findings within the broader prosecution and sentencing literature. In doing so, it redresses the unfortunate habit of white collar and traditional sentencing research operating in parallel planes. It is worth noting that while this dissertation explores white collar *cases*, prior literature is inconsistent in focusing on white collar offenders and white collar cases. The primary distinction between these is the sampling frame; studies of white collar *offenders* identify relevant cases based on defendant characteristics, while studies of white collar *cases* may be interested in the treatment of individuals but identify "white collar crime" through the use of offense characteristics. Both sets of literature focus on the possibility of differential treatment and can offer insight; thus both are discussed herein.

I address first the issue of definitional ambiguity as a unique and defining feature of white collar crime research, develop a working definition for this research, and discuss the implications of definitional debate for this project. Next, I explain the motivation for a multistage sentencing process, as well as the results of prior multistage analyses. Then I move on to a brief summary of prosecutorial decision-making research and theory, with a greater focus on white collar specific

studies. Subsequently, I provide an overview of the current state of sentencing research, including predictors of criminal sentences in general offense samples as well as studies with a focus on white collar offenders. To make sense of these findings and direct the focus of this study, I then address several theoretical perspectives that have been used to explain both prosecutors' and judges' decisions (both in general and white-collar samples). The final portion of this chapter addresses the contribution of this dissertation to this literature, and organizes a set of hypotheses drawn from bodies of research herein.

DEFINING WHITE COLLAR CRIME

All studies of white collar crime share the same burden where, unlike studies of so-called "traditional crime", authors need to define the focal offense(s) of analysis. This need for definitional clarity stems from the fact that in spite of the acceptance of the term "white collar crime" in the modern lexicon, it is not a term of any legal significance. Compare this to "property crime", a legally meaningful word, that refers to the theft of money or physical property, generally without interaction between the victim and the offender. While "property crime" includes a set of statutory offenses (like "white collar crime"), the specific offenses that fall into this category are clearly defined through the characteristics of the offense and the interaction between victim and offender. What exactly, then, is 'white collar crime'?

The answer to that simple question is deceptively complex. Scholars who have studied white collar crime (including specific variants, such as corporate crime, occupational crime, or elite deviance) have reached little consensus on the

meaning of this term since it was first defined by Edwin Sutherland in 1939: he noted that, "Income and amount of money involved in the crime are not the sole criteria… 'white collar' as used in this paper means 'respected,' 'socially accepted and approved,' 'looked up to'" (1939: fn 2). The original definition was so stated as a direct response to the common belief at the time that crime was a condition of the impoverished classes. Sutherland's work offered a powerful counterpoint to that assumption. Crime, he argued, was present at much higher frequencies among the upper class than traditionally believed. In calling attention to this, Sutherland highlighted the futility of explaining crime solely through (the lack of) economic resources. Sutherland later clarified the definition of 'white collar crime' as, "crime committed by a person of respectability and high social status in the course of his occupation" (1983[1949]:7). The definition he offered was both class-based (i.e., "high status") and offender-based, not necessarily because white collar crime was inherently different in its consequences or causes, but rather because he wished to draw attention to the *similarity* of behavior across the social classes. While crimes like burglary, assault, and murder may be much more common among individuals of the lower classes, Sutherland's definition made clear that this did not mean that the upper class was uninvolved in law-violating behavior.[3]

[3] I specify "law-violating", as opposed to criminal, behavior, because in many cases (particularly occupation-related offenses), individuals are subject not only to criminal jurisdiction, but also to regulations. Law violating behavior by both individual and organizational actors in the course of occupational settings are most often responded to through regulatory, as opposed to criminal, means. Nevertheless, these behaviors are against codified law, and are arguably thus roughly equivalent to the violation of criminal law. Indeed, many regulations carry the option to be resolved through either regulatory or criminal means (see Ayers and Braithwaite, 1992). There are some who would disagree with extending the word "crime" to these behaviors (e.g. Tappan, 1947), though this extension is generally accepted among criminological scholars.

Arguably setting the stage for confusion, Sutherland's seminal study, *White Collar Crime* (1983[1949]), focused on corporate actors despite his definitional focus on individual actors. In the nearly 80 years since Sutherland first coined the term, there has been general agreement that crimes committed by organizations merit the label 'white collar crime,' though generally it is more specifically referred to as "organizational" or "corporate crime." However, there is great debate about who, what, and when, a crime committed by an *individual* should be labeled "white collar."

Generally, definitions of white collar crime have been either offender- or offense-based, and may or may not incorporate contextual clarifiers. Offender-based definitions of white collar crime draw heavily on Sutherland's original work, emphasizing the "person of respectability and high social status." Scholars who take such an approach typically identify white collar crime by isolating well-educated or affluent individuals among broader criminally involved samples (see, for example, Hagan and Nagel, 1982). Other work that utilizes offender-based definitions of white collar has used Marxian labor classifications to distinguish between the *class* of individuals (i.e., laborers, managers, employers, owners; for example, see Eitle, 2000 and Hagan and Parker, 1985). However, in many cases, the data required to identify offenders who are "white collar" according to these offender-based is difficult to attain (particularly for class-based definitions). Further, purely offender-based definitions ignore the second component of Sutherland's definition: "crime committed…in the course of [the offender's] occupation." If a wealthy person murders his or her spouse, does that really stay

true to the essence of Sutherland's intention? After all, his purpose was in highlighting the unseen yet somewhat common behavior of privileged persons, not the aberrant, but still visible. Unfortunately, the occupational context of offenses is often excluded from traditional sentencing data sources, keeping context out of many offender-based studies.

To avoid including more traditional crimes committed by elite offenders in their definition of "white collar crime," some scholars have turned to offense-based definitions of 'white collar crime.' In these approaches, white collar crime is taken to refer to a subset of offenses that share a set of characteristics consistent with abuses of trust (e.g. Shapiro 1980). Edelhertz (1970) defined white collar crime as illegal acts that are financial in nature, are nonviolent, and use deception or guile. It bears noting that his definition is quite popular among law enforcement agencies; the Federal Bureau of Investigation defines white collar crime in the same way (Barnett 2011). Often, scholars interested in criminal sentencing of white collar cases are somewhat backed into taking such an approach because of data limitations (i.e., the inability to identify dimensions of defendant privilege), even though many of these 'white collar crimes' may not be committed by persons meeting Sutherland's requirement of "respectability and high social status" (e.g., Daly 1989; Holtfreter 2013).

In another seminal study of white collar crime, the Yale White Collar Crime Project, the authors took an offense-based approach, highlighting cases that involved one of eight crime types, including postal and wire fraud, embezzlement, anti-trust, securities and exchange fraud, false claims and statements, tax fraud, and

bribery (see Mann et al. 1980, e.g.). Weisburd (1991) in fact dubs the majority of these offenses "crimes of the middle classes,"[4] though the authors also noted that this varies by offense type (p.48). Weisburd (1991:40) suggests that within these eight offenses, securities and antitrust violations represent one end of a continuum of complexity more associated with white collar offending, while offenders of federal bank embezzlement, tax or credit frauds represent the opposite, across a variety of metrics.[5] The question then becomes, however, whether an offense based approach is sufficient to capture "real" white collar crime. If it cannot, how might researchers identify white collar cases in meaningful ways, when employment is typically not part of traditional data collection? How can researchers isolate white collar crime?

It is worth noting here that there exists a long tradition of studying white collar crime that has generally avoided this issue entirely. These scholars have done so by focusing not on Suthlerand's "high social status", but rather tuning in on his

[4] Weisburd et al. (1991:180) argue that an offense based definition is a more useful study than an offender-based one, remarking, "Those who would limit white-collar crime solely to the crimes of the upper classes and the corporate world, and would compare it to common crime, street crime, or the usual conception of 'the crime problem,' are examining the top and bottom of the stratification and virtually leaving out the entire middle." However, the extent to which power and privilege equally protect the middle and upper classes is questionable; if scholars are interested in the role of individual privilege in treatment within the criminal justice system, a broad-based offense definition of white collar crime may be biased toward finding smaller or null differences between lower-class and high(er) class offenders.

[5] The authors also note that offenders convicted of antitrust and securities violations are more consistent with an offender-based definition of white collar as well (Weisburd 1991: 48). In contrast, offenders convicted of credit or tax fraud or making false claims and statements are more consistent with the image of traditional criminals with limited financial means and younger offender profiles. Similarly, Daly (1989) observed variation in gender gap across offense types using the same sample. According to Weisburd (1991)'s conception, she finds women were more likely to be involved in lower end, less complex white collar offenses, with the greatest female representation among bank embezzlers (45%), credit frauds (18%), postal fraud and credit fraud (15%) (Daly 1989: 776).

"in the course of [the offender's] occupation."[6] To do so, scholars have generally relied on particular offenses that are committed in the course of business activities, namely, regulatory violations. Businesses, and those in their employ, are subject to overlapping systems of control including both criminal law and regulatory law. Regulatory laws are often generated by regulatory agencies, as opposed to through legislative acts, and generally violations are at least initially pursued by the same agencies. In focusing on regulatory laws, scholars have largely been able to isolate law-violating behavior that is consistent with Sutherland's white collar offender descriptions: typically mid- and upper-level individual employees are held responsible for violations of regulatory law (supervisors, owners, etc.), and will almost certainly include cases committed within the course of legitimate business activities. These supervisors, managers, and owners are arguably consistent with Sutherland's definition of white collar crime, including the offender's position of "respectability and high social status"; additionally, this also removes cases of common crimes committed by 'white collar' persons. Finally, a focus on regulatory law will include corporate offenders, whereas most other offense-based definitions (such as the Yale White Collar Project) will exclude these actors (Weisburd, 1991: 20), in spite of their key role in historical studies of white collar crime.

These definitional disagreements may be particularly important to consider in a study of white collar crime sentencing. In discussing the relevance of offense and offender characteristics in criminal sentencing, Nagel and Hagan (1982) note

[6] Focusing on crime committed in the course of an occupation is sometimes referred to as occupational crime (Friederichs 2002), though "occupational crime" typically includes basic theft from employers that white collar scholars would seek to avoid as this is not offending *within the course of legitimate work*.

that while white collar offenses are generally considered very severe due to the greater harm inflicted, white collar offenders are typically good candidates for leniency based on offender characteristics, such as the lack of a prior criminal record (1982:1431-1436). This tension has been highlighted by scholars who see the current federal sentencing guidelines as too offense-driven, and ignorant of reasons to be lenient based on offender characteristics (Podgor, 2007). Together, this implies that studies defining white collar crime solely based on a strict set of offenses that are more serious (without regard for offender characteristics) may systematically bias themselves toward a finding of white collar punitiveness while those identifying white collar crime by offender characteristics alone (collecting a mix of not-so-serious and extremely serious offenses) may be more likely to produce findings of leniency.

A Working Definition of "White Collar Crime"

It is not the purpose of this dissertation to argue about the relative validity of each of these different approaches to white collar crime. The debate surrounding what is "real" white collar crime has been ongoing for nearly a century (Friederichs, 2002; Tappan, 1947) and this work will not resolve it. However, the above discussion serves to highlight the importance of method for isolating white collar cases to be studied. In doing so, I consider both the original definition of white collar crime as described by Sutherland, as well as the way in which this phrase is understood by the public.

From the foundation Sutherland set, white collar crime has two primary defining components: (1) the status or respectability of the offender, together

describing his or her social position, and (2) the offense's situation within an occupational setting as part of legitimate work. The latter component is consistent with laymen's' definitions of white collar crime. Merriam-Webster defines "white-collar crime" as "a crime that typically involves stealing money from a company and that is *done by people who have important positions in the company*" (2017, emphasis added). Similarly, the popular web source Wikipedia[7] identifies white collar crime as "financial motivated nonviolent crime *committed by business and government professionals*" (2017, emphasis added). While the two sources offer undoubtedly distinct definitions that could, conceivably, each include crimes that the other does not, they share a focus on the occupational position of the offender and the importance of this criterion in defining the offense. Even the FBI's webpage on white collar crime specifies that the major current programs focus on "business-related" offenses (Federal Bureau of Investigation, 2017). Given that this is also consistent with Sutherland's original definition in 1939, this dissertation, too, prioritizes this dimension of 'white collar' crimes.

In this work, the primary definition of white collar crime is as follows: "*white collar offenses are any violations of (federal) law committed by persons or organizations in the conduct of their legitimate occupational roles or organizational functions,"* (Simpson and Yeager 2015: 47). White collar crime, then, is not defined precisely based upon offender or offense characteristics, but upon the context of the offending. It can be expected that these offenders will be more educated, or of a higher socioeconomic status, compared to the average

[7] While this is certainly an odd citation in an academic work, the popularity of Wikipedia belies its value to provide insight into the understanding and acceptance by the general public.

offender but not necessarily. The crimes identified as 'white collar' under this definition will largely be consistent with prior offense-based studies, but may also include others, such as workplace safety violations that result in injury, that would not otherwise be included according to Edelhertz (1970). This approach is also consistent with Hagan and Parker's observation that,

> Class position influences involvement in white-collar criminal *behavior* as well as the *punishment* of this behavior… the punishment of white-collar crime is not simply a function of class position, but also of the kinds of organized white-collar criminal behavior that certain class positions make possible. (1985: 304, emphasis in original)

Regulatory violations (i.e., a specific subset of regulatory violations) are most consistent with this definition; however, breaking with much of the previous work on regulatory offenses, I am focusing on violations not as handled by a specific regulatory agency, but on the criminal prosecution and sentencing of regulatory offenses. In doing so, I attempt to isolate the specific types of offenses that may be committed by relatively high status persons in the course of legitimate business activity.

However, because this working definition is not the only definition of white collar crime that has been used in white collar sentencing literature (or the most popular), nor is there evidence of a consensus for appropriate isolation of white collar cases, a fundamental part of this work will consider whether definitional differences (with associated differences in cases identified as white collar) result in different findings for the questions at hand. This is itself an important question that is generally not considered within a singular study of white collar sentencing.[8]

[8] However, at times, conflicting results have prompted authors to consider the effect of different measures. For example, see Hagan et al. (1982), responding to Hagan et al. (1980)

SENTENCING AS PROCESS

The cumulative effect of prior decisions on criminal justice processing (sentencing) outcomes has long been recognized (e.g. Hagan, 1975; Hill, Harris and Miller, 1985), though it has only recently been problematized in sentencing research. In several recent reviews of sentencing research, a general call has been issued for scholars to acknowledge and methodologically incorporate the fact that judicial sentencing decisions do not occur in a vacuum, and are in fact dependent on several prior decisions (see Ulmer, 2012; Baumer, 2013). The ultimate sanction issued is the result of the totality of criminal justice system processing, which incorporates decisions made by police (e.g., whether to stop, question, and frisk, whether to arrest), prosecutors (e.g. whether to file a case, whether to negotiate a plea, whether to motion for substantial assistance departures), as well as judges (e.g., whether to include certain facts in guidelines calculations, what weight to give the guidelines, whether to depart from presumptive sentences). For those interested in sentencing disparity, early processing studies have suggested that failure to consider multiple decision points may lead to the erroneous conclusion that offender characteristics, such as race, are not significantly related to outcomes at any single stage, while total effects *across* stages are significant (Stolzenberg et al., 2013; Wooldrege et al., 2015). While the earliest empirical attempt to model the interconnectedness of the process can be seen in the work of John Hagan (1975), thus far multistage outcome analyses within the same study are the exception rather than the rule; those exceptions are briefly discussed below.[9]

[9] This is not to deny the ever-growing literature on early criminal justice system outcomes, which focus on directly modeling prosecutorial and early judicial (e.g. bail) decisions. However, because

A handful of studies have explicitly explored cumulative effects across multiple sentencing outcomes in state courts. Findings suggest that race affects sentencing outcomes, at least partially, through its relationship to prior outcomes in both drug courts (Brennan, 2009) and broader criminal courts (Kutateladze et al., 2014; Spohn, 2009; Stolezenberg et al., 2013; Wooldredge et al., 2015). Specifically, black defendants are more likely to be detained pretrial (Wooldrege et al., 2015), less likely to receive charge reductions and more likely to be offered custodial (rather than noncustodial) pleas (Kutateladze et al. 2016, Spohn 2009). These early stages then affect the likelihood of incarcerative sentences (Kutateladze et al., 2014; Stolzenberg et al., 2013; Sutton, 2013; Wooldrege et al., 2015). Fewer studies have explored these effects in the federal system. However, some evidence suggests that men are less likely to receive charge reductions (Shermer and Johnson, 2010) and that minorities are more likely to be held before trial (Spohn, 2009), each of which affects the severity of sentences received.

What is clear from this review of studies is that investigations of sentencing as a process remain a relatively small portion of the overall sentencing literature. Wooldredge et al. (2015) attribute this dearth to limitations in data accessibility and statistical modeling. Additionally, a relatively small number of early case processing decision points are explored, with few studies looking beyond pretrial detention. What is needed is a broader concept of process, beginning with prosecutorial case initiation decisions, as well as a wider consideration of what

these studies are not primarily interested in the sequential chain of decision-making that occurs *across stages*, I limit my discussion here to studies that explicitly attempt to model multistage outcomes.

offender and case characteristics may exert indirect effects on sentencing via these early decisions.

The Importance of White Collar Processing

Like reviews of traditional sentencing research, recent reviews of white collar crime research reach similar conclusions about the importance of process. "No research…follows cases from the initial point of contact with the justice system through sentencing outcomes. These studies are especially important for understanding how cases are moved forward and which cases are diverted from the justice process" (Simpson, 2013: 321-22). Indeed, there is good reason to believe that process decisions may be particularly important for white collar cases above and beyond traditional offenses. For many white collar crime crimes, police are less likely to be involved than for traditional crimes and prosecutors are involved at an earlier stage (Mann, 1985). Prosecutors are more involved in the collection of evidence prior to formal prosecution and often solicit cooperation from defendants in order to attain that evidence (see discussion below). This dependency on cooperation for evidence may result in more culpable managers benefiting from favorable charging decisions by prosecutors (Hagan and Parker, 1985; however, see Brickey, 2006). More complex white collar cases also appear more likely to receive leniency at sentencing (Albonetti, 1994, 1999). In a broader study of prosecutorial decision-making, Nagel and Schulhofer (1992) note that in at least one district, white collar cases commonly benefited from charge bargaining practices that other crimes did not (see p. 516). Variation in how cases are treated at these early decision-making points may help to explain why some research (e.g.,

Benson and Walker, 1988) found that white collar offenders did not receive longer prison sentences than other offenders – in other words, the "filtering out" of cases across stages may be particularly salient for understanding the nature of leniency afforded to white collar defendants.

Altogether, this suggests that the nature of white collar offenses, and in particular, offenses related to occupations, may operate to the advantage white collar offenders in ways that may supersede or interact with other predictors of decision-making across stages in the criminal justice process. In addition to cases being filtered out (selection) or receiving greater discounts it is also possible that white collar cases receive advantages at sentencing related to earlier decisions, especially plea bargains and charge reductions (Albonetti, 1994; Szockyj, 1998). The potential consequence of this is that cases that appear "like" at a single stage may not be alike at all. However, some expectations may be gleaned through the examination of research that has focused on white collar prosecution and, independently, white collar criminal sentencing.

PROSECUTION

The present work is focused on the criminal justice processing of white collar cases through the federal court system. In doing so, it is critical to consider the role of the prosecutor's office as a gatekeeper to the criminal courts (Coffee, 2006). Yet, in spite of decades-old widespread agreement on the importance of studying prosecutors, comparatively little research exists to shed light on these actors (Johnson et al., 2016; Ulmer, 2012). Further, it is only in recent years that researchers have begun to problematize prosecutorial decisions as structurers of

criminal sentencing outcomes (e.g. Kutateladze et al., 2014, 2016; however, see Hagan, 1975). Prosecutors are often said to have "unfettered" discretion. While this is arguably hyperbolic language, it is nevertheless true that prosecutors have a sizable amount of discretion whose consequences are not well understood. This discretion is problematic not *only* because it is not understood, but also because prosecutors (or, more accurately, prosecutor's offices) hold sway over more key case decision points than any other criminal justice agent. While police have discretion in pursuing and arresting suspects, and judges in their sentencing (and conviction in bench trials), prosecutors decide who is *eligible* for criminal sentencing, and under what circumstances. Specifically, prosecutors decide among referrals which cases will be pursued formally, among these, which will be charged, when, and under what statutes. In the majority of cases, prosecutors also negotiate the terms of conviction through plea agreements. In doing so, they not only shape the composition of cases eligible for sentencing (through case and filing decisions) but also restrict the sentences that later decision-makers can impose by charging individuals under particular statutes, reducing or modifying charges in plea agreements, supporting downward departures[10] and sometimes recommending sentences to judges.

While prosecutorial decisions are often difficult to study for lack of data (Forst, 2002; Johnson et al., 2016), the research that has been done paints a

[10]In districts with sentencing guidelines. Here, I refer in particular to the federal sentencing guidelines, which offer prosecutors the opportunity to support lower sentences when defendants offer "substantial assistance" to prosecutors in other investigations. These "5K" motions are, in practice, subject to wide variety between prosecutors and districts in their applications (Nagel and Shulhofer, 1992).

somewhat consistent picture across stages. In charging decisions, prosecutors appear to act with an eye on a case's eventual disposition; in other words, prosecutors evaluate a case's "convictability" and act accordingly (e.g., Frohmann, 1997), regardless of their own beliefs about the events that occurred (Spohn et al., 2001). Prosecutors are likely to move forward when it is likely a case will end in a conviction, and less likely when 'convictability' is low (e.g., Albonetti, 1986). For example, the strength (and type) of evidence (both physical and testimonial) appears to be an important consideration (Albonetti, 1986, 1987, 1990, 1992; Beichner and Spohn, 2005, 2012; Holleran et al., 2010; Holmes et al., 1987; Kingsnorth and Macintosh, 2007; Kingsnorth et al., 1998, 2001; Spohn and Holleran, 2001; however, see Kutateladze et al., 2016; Spears and Spohn, 1997; Spohn and Horney, 1993). Additionally, victim behavior appears to be important, including provocation of the incident (Albonetti, 1987; Baumer et al., 2000) and behaviors indicating morality, such as sexual history or suggestive clothing (Holleran et al., 2010; Spears and Spohn, 1996; Spohn and Holleran, 2010; Spohn and Spears, 1997).[11]

Evidence suggests that other offender characteristics, such as race, may affect not only prosecutor decisions to charge (Adams and Cutshall, 1987; Henning and Feder, 2005; Holleran et al., 2010; Miethe, 1987; Myers, 1982; Spohn et al., 1987) but also whether to plea (Albonetti, 1990; Frenzel and Ball, 2007; Kellough and Wortley, 2002) and whether to offer charge reductions (Farnworth and Teske,

[11] Often studies of victim behavior and prosecutorial decision-making focus on domestic violence or sexual assault cases.

1995; Kingsnorth and MacIntosh, 2007; Kutateladze et al., 2014, 2016; Shermer and Johnson, 2010) and the sentences negotiated as part of those pleas (Kutateladze et al., 2014, 2016), largely to the disadvantage of minority defendants (Kutateladze et al., 2016).[12] Minorities also may be less likely to be diverted from criminal courts (Albonetti and Hepburn, 1996), less likely to receive prosecutor-sponsored sentencing departures (Spohn and Fornango, 2009), and more likely to be charged with mandatory minimums (Ulmer et al., 2007). Consistent with sentencing literature, women may receive preferential treatment by prosecutors as well (Adams and Cutshall, 1987; Albonetti, 1986; Albonetti and Hepburn, 1996; Barnes and Kingsnorth, 1996; Baumer et al., 2000; Bjerk, 2005; Farnworth and Teske, 1995; Frenzel and Ball, 2007; Henning and Feder, 2005; Kingsnorth and Macintosh, 2007; Kutateladze et al. 2014, 2016; MacMurray, 1989; Myers, 1982; Pyrooz et al., 2011; Sherman and Johnson, 2010; Sorenson and Wallace, 1999; Spohn et al., 1987; Spohn and Fornango, 2009; Spohn and Spears, 1997; Ulmer et al., 2007; Webb and Rodriguez, 2006), [13] though effects are often sensitive to modeling strategies or appear only for gender-race dyads. Thus, while evidence generally supports that prosecutors' decisions are affected by case strength, they are also affected by legally irrelevant characteristics.

[12] On the other hand, a still sizable number of studies have found no evidence of a race effect on prosecutorial decision-making. See: Albonetti, 1986, 1987, 1992; Ball, 2006; Barnes and Kingsnorth, 1996; Baumer et al., 2000; Bjerk, 2005; Beichner and Spohn, 2005, 2012; Elder, 1989; Franklin, 2010a, 2010b; Freiburger and Jordan, 2011; Kingsnorth et al., 1998, 2001; Miethe and Moore, 1985,1986; Spears and Spohn, 1997; Spohn and Holleran, 2001; Spohn et al., 1987; Spohn and Spears, 1997; Vance and Oleson, 2014; Webb and Rodriguez, 2006)

[13] However, see: Albonetti, 1987, 1992; Ball, 2006; Elder, 1989; Franklin, 2010a, 2010b; Freiburger and Jordan, 2011; Miethe and Moore, 1985, 1986; Sacks and Ackerman, 2012; Vance and Oleson, 2014)

There is reason to believe that prosecutorial decisions about how to charge offenders, whether to plead cases, and under what terms, are also particularly consequential for offenders under the federal system. The federal sentencing guidelines are fairly restrictive and while only advisory, are usually followed unless there is a motion by the prosecutor (USSC Sourcebook 2016). Johnson et al. (2016) argue that when sentencing guidelines are in place (as in the federal system) prosecutors may have increased influence if not discretion, a hypothesis known as hydraulic displacement; according to this hypothesis, when judicial discretion is constrained by guidelines, disparities and discretion do not disappear from the system but rather are shifted (displaced) to other actors, namely, prosecutors. There is little overt evidence for hydraulic displacement (Miethe, 1987; Miethe and Moore, 1985; Vance and Oleson, 2014; Wooldredge and Griffith, 2005) via increased disparities in prosecution, but this does not mean that prosecutors lack discretion and influence under guidelines systems. Some evidence suggests that plea agreements (and thus, prosecutors) may have larger impacts on sentences in districts with presumptive as opposed to voluntary guidelines (Piehl and Bushway, 2007); yet their influence on sentences is likely predominantly via impacts on presumptive sentences due to negotiated charges (Shermer and Johnson, 2010). Additionally, perhaps even more so than in state courts, federal prosecutors have enormous power in determining whether or when cases enter and exit the criminal justice system, as well as the conditions under which they do so. Unlike state prosecutors, whose primary source of cases stem from police activity, federal prosecutors (under the Department of Justice) have investigative groups of their

own. For example, federal prosecutors currently have working groups dedicated to antitrust activity, fraud, and environmental and natural resource crimes, among others (Department of Justice, 2017). While the U.S. Attorney's caseload is informed by referrals from other agencies, including the United States Marshall's Service (which may be considered analogous to the state and local police in some respects) and regulatory agencies, in these working groups, the U.S. Attorneys serve *both* as police and prosecutor.

Prosecuting White Collar Crime

While there is a rich literature on white collar crime and sentencing, there is less research related to the prosecution of white collar offenses. What literature there is also tends to be dated, particularly to the 1970s and 1980s, likely (in part) inspired by the events of Watergate and the Savings and Loan crisis (Tillman et al., 1997; Katz, 1980). As such, there is less on which to draw in order to understand the process and practices of modern white collar prosecution. One advantage of this concentrated time period, however, is that much of the literature is grounded in deep observational research which provides context when merged with prosecutor studies of traditional crime. A common theme is that white collar cases are at a particular advantage in even the earliest of case processing decisions (Albonetti, 1994; Hagan and Parker, 1985; Tillman et al., 1997; Jesilow et al., 1986). White collar crimes, particularly those committed by organizations, or individuals within organizations, have several key characteristics that make it more likely that these cases are dropped or otherwise diverted from full criminal prosecution.

Congressman Annunzio commented in a Congressional hearing following the Savings & Loan crisis that,

> I don't think the administration has the interest in pursuing Gucci-clad white-collar criminals, These are hard and complicated cases, and the defendants often were rich, successful, and prominent members of their upper-class communities. It is far easier putting away a sneaker-clad high school dropout… (1990; as cited in Tillman et al. 1997)

What makes these cases so hard to prosecute? It is partly due to the nature of the offense itself. Typically, white collar or corporate cases (especially those involving financial transactions) generate vast amounts of evidence of wrong-doing. In principle, this should advantage prosecutors and make conviction more likely; consequently, prosecutors could be more willing to pursue these cases. However, unlike traditional crimes, where the evidence of the crime is readily apparent to the victim and easily documented, the evidence in white collar cases is often shrouded behind walls of proprietary information. Prosecutors (or the police) cannot simply collect the evidence of these crimes. As Katz notes,

> Many white-collar jobs offer distinctive opportunities to cover up the commission of crime by virtue of the social distance between their occupants and victims or outside witnesses. The organizational barriers between price fixer and consumer, stock manipulator and investor, prevent the latter from perceiving his victimization. Victims of large-scale consumer fraud typically are so removed from the inner workings of the firm, dealing only with salesmen on the fourth or fifth organizational tier, that they have no basis to discount explanations that nondelivery is due to mistake, incompetence, or bankruptcy (1979:437-438).

In order to obtain proof that a white collar crime has occurred, prosecutors must often engage in negotiations with the very people and organizations that they wish to use that evidence to convict (Mann, 1985). Prosecutors are unlikely to obtain access to this information without also offering concessions, which may include or produce sentencing discounts later on (e.g. Albonetti, 1994, 1999). This is especially likely when criminal behavior takes place under the guise of legitimate business activity (Benson and Simpson, 2015).

Additionally, white collar cases are often advantaged at prosecution by their complexity. White collar crimes, with layers of individual and organizational responsibility, hierarchies, and often long-term offending behavior appear as case file Rubix cubes (Weisburd, 1991; Wheeler and Rothman, 1982). Pinning responsibility (and intent) clearly on the shoulders of a single party, or even parties, is difficult within organizational hierarchies; when management encourages accountants to produce fraudulent returns, who is the criminal, the manager, the accountant, or both? Prosecutors appear unwilling to clog court dockets with unclear cases that may take a long time, citing fear of judicial backlash (Jesilow et al., 1986). Work by Albonetti (1994, 1999) underscores the influence of complexity, suggesting that because prosecutors are interested in securing convictions, complex cases are more likely to result in plea agreements; in agreeing to plea deals, prosecutors ensure that cases resolve in their favor. However, plea agreements increase the likelihood of suspended sentences, with the greatest effects for the most complex cases (Albonetti, 1994); the relationship between case complexity and leniency appears to cascade across multiple decision-points and actors. These plea agreements not only work to increase the odds of an advantageous sentence through structure, but also may be seen by judges as signaling a lower risk of defendant recidivism (Hagan and Bernstein, 1979: 472).

Rarity may also play a role. While the average federal prosecutor has significant experience working on drug cases (which make up the largest portion of federal prison sentences; Carson and Anderson, 2016), they are potentially not as familiar with white collar crimes. Research in federal courts in the 1970s found that

only one of the ten districts was observed to have a sizable white collar crime case load (Nagel and Hagan, 1982). In the others, it is reasonable to assume that prosecutors have limited experience with white collar cases, and potentially, no desire to become familiar. Even districts with high rates of white collar prosecution, however, may also treat white collar cases leniently – "proactive" districts that pursue white collar cases with more regularity appear to engage more in sentence discussions during negotiations (Hagan and Bernstein, 1979).

White collar cases, and specifically, their defendants, are also posed to be problematic for prosecutors because of the resources that they (typically) have at their disposal, manifesting in particularly challenging legal defenses. The prosecutor's office has a set amount of resources (and often not enough) for a wide variety of crimes. Given the case complexity of many white collar cases and the lengths to which prosecutors must go to attain evidence, they represent a significant resource burden for prosecutors. This, in itself, is problematic. However, it becomes more problematic when coupled with defendants who have significant resources to expend to hamper the prosecutor's efforts. Large corporate defendants, for example, typically have legal teams on which they can rely. Individual white collar defendants will often have access to private counsel as well, either because of their own financial resources or because firms may feel pressure to defend accused employees (Ribstein, 2009). Private defense attorneys for white collar cases generally intervene early in the prosecutorial process and instigate substantive defenses long before trial (Mann, 1985). The consequence of this is that many white collar cases may be terminated in the prosecutor's office long before charges

are even filed. This is just one of many reasons that the present study's focus on process, particularly early stage processing, is of importance particularly in white collar cases.

Beyond these overarching advantages for white collar defendants, it appears that there are some distinguishing case features that *increase* the likelihood of prosecution for white collar cases. For example, Gurney (1985), in a study of a local economic crimes prosecutorial unit, noted that cases with individual, as opposed to corporate, defendants were more likely to be prosecuted. This is consistent with the above arguments regarding resources, complexity, and availability of evidence. Individual white collar defendants may be weak links, especially if corporations choose not to (or in the case of smaller firms, perhaps cannot) offer employees legal assistance (Ribstein, 2009). This also suggests that smaller firms, who are less likely to be able to devote significant time and resources to fighting prosecutors, may be at a relatively increased risk for prosecution, which is consistent with the findings that small firms make up the bulk of organizations sentenced under the United States Sentencing Guidelines (see e.g., United States Sentencing Commission, 2016).

In addition, Gurney (1985) also found that prosecutors were more likely to accept and prosecute cases referred by certain regulatory agencies; these cases came "ready-made" for prosecution, requiring little resource burden for prosecutors. In the present study, this may mean that cases referred to the U.S. Attorney's Office by outside agencies may be more likely to move forward than those originating from within the agency or its associated workgroups. However, this practice itself

may result in unequal outcomes; Hagan and Parker (1985) find evidence that employers are more likely to be charged with regulatory rather than criminal code charges by regulatory agencies, resulting in less severe sentence exposure. In other words, the cases referred to prosecutors by regulatory agencies may already have been affected by the privilege of defendants.

Since the mid-1990s, a "new" prosecutorial tool has emerged in the form of Deferred Prosecution Agreements and No Prosecution Agreements (DPAs and NPAs, respectively). These are forms of diversion in which corporate defendants agree not to fight prosecutors, thus preserving resources, accept responsibility, but also do not admit criminal wrongdoing. These agreements require defendants to refrain from criminal acts, i.e., be compliant with the law, for a certain period. If these conditions are met during the time period, companies receive no charge nor sanction. However, if companies at any point become noncompliant, prosecutors reserve the right to pursue them for the original charges, in addition to charges from new violations. Large organizations, in particular, may be more eligible for this resolution strategy, particularly if they have an "effective compliance program" in place at the time (Murphy, 2002). The primary difference between the two is that DPAs are usually filed in court, while NPAs are not. Thus far, there is little empirical research on these types of "prosecution," their predictors, or their consequences. However, such agreements may have reduced the total number of white collar cases that are prosecuted as a means of conserving resources. This reduction is likely more noticeable for organizational, opposed to individual, defendants.

CRIMINAL SENTENCING

Research on criminal sentencing, also sometimes called judicial decision-making, has remained a vibrant line of research for decades without pause. Much of research has focused on identifying and explaining disparity in sentences. However, traditional sentencing research has primarily focused on racial and gender disparity, rather than on disparities in sentences between categories of offenses (e.g., white collar compared to other crimes), while white collar scholars have prioritized the latter over the former. Rarely have authors in these works intersected (with some exception), resulting in bodies of literature that are almost entirely distinct. Thus, I separately discuss both 'traditional' sentencing research and white collar sentencing research, though in practice many 'white collar' cases may be part of the samples on which traditional sentencing research is based. After discussing the empirical findings, I present and compare several theoretical paradigms used to explain these findings.

Discretion and Disparity in Criminal Sentences

There have been multiple waves of sentencing research, each with its own particular focus and methodological tools (Johnson and Lee, 2014; Spohn, 2000; Zatz, 1987). However, the questions of interest have largely remained the same across these periods: What are the predictors of criminal sentences? What role do legally relevant variables, such as prior record and offense seriousness, play? What role do supposedly irrelevant ("extralegal") factors[14], such as the victim and offender's race and gender, play? Which is more important? At the heart of this is

[14] Thaler (2014)

a fundamental unease with disparate sentences, and a concern that these differences originate from biases (either conscious or subconscious). While it has not always been so, evidence suggests that the most influential factors in criminal sentences are legally relevant (Spohn, 2000); that is, crime seriousness, culpability, and the offender's criminal record are robust predictors that capture the bulk of explained variance in regression models. However, there is also robust evidence that extralegal variables, including but not limited to offender characteristics, matter as well.

Perhaps the most well researched extralegal variable in criminal sentencing is race[15]; Zatz (1987) argues that nearly all of criminal sentencing research can be said to study this issue. While the most recent meta-analysis concludes that race does indeed have an effect on sentencing outcomes (Mitchell, 2005), researchers have argued that determining "if" race matters is not as informative as determining "when" and "for whom" (Spohn, 2000; Spohn and DeLone, 2000). For example, race appears to affect sentencing in different ways for state and federal courts. Federal courts appear to have larger race discrepancies for property offenses,[16] compared to being largest for drug offenses in state courts (Mitchell, 2005). In capital cases, the victim's race appears to interact with the race of the defendant to produce particularly harsh outcomes for black offenders who kill white victims (Paternoster and Brame, 2008). Sentencing guidelines may have reduced the effect of race and gender effects at the federal level, though they may continue to exert at

[15] Though the conceptualization and operationalization of race in these works is limited (see Baumer 2013, Johnson and Lee 2014)
[16] Schanzenbach and Yeager (2006) argue that income and fines explain a large portion of this disparity.

least small effects on sentencing (Mustard, 2001; however see Mitchell, 2005). In an early analysis of the federal guidelines implementation, Mustard (2001) notes that at the federal level minorities receive longer sentences, which is largely explainable through the differential likelihood of receiving departures. Some scholars have suggested that extralegal variables may exert more influence in less serious offenses (Myers, 1982; Spohn and Cederblom, 1991). However, a large portion of the race effect may operate through prior decision points, especially bail and plea agreements (Bjerk, 2005; Hagan, 1975; Holmes et al., 1987; Shermer and Johnson, 2010; Kutateladze et al. 2014, 2016, 2016; Thomson and Zingraff, 1981). This is consistent with a "subtle" (Zatz, 1987) or "contextual" (Spohn, 2000) effect. In other words, "offender race/ethnicity affect[s] judges' sentencing decisions in some jurisdictions, for some types of offenses, and for some types of offenders" (Spohn and DeLone, 2000: 25).

The variation of the race effect ties into several other forms of disparity that are prominent in contemporary sentencing research, including the relationship between gender and sentencing. While less often explicitly researched than race, defendants' gender is arguably even more consistent in its effect on criminal sentences. A recent meta-analysis (Bontrager et al., 2013) confirmed findings from an earlier narrative review (Daly and Bordt, 1995) – women on average receive more favorable sentencing outcomes than similarly situated men.[17] However, both studies also note that there is variability in this finding; departures from guidelines

[17] While many of the sentencing theories explicitly address this finding, it is also worth noting that feminist scholars have advanced their own explanations for this. Among these include the selective chivalry hypothesis (or the evil woman hypothesis; Visher, 1983), which parallels typescripts at times, and Daly (1989b)'s concept of "familied justice"

ranges, the incarceration decision, and sentence length are more likely to find effects than more broad sentencing severity measures (Bontranger et al., 2013). More recent data appears less likely to find support according to Bontranger et al. (2013), though the overall level of consistency is similar to that observed by Daly and Bordt (1995).

Additionally, in the last decade sentencing scholars have also been increasingly aware of disparities that exist between districts (e.g., Britt, 2000; Ulmer and Johnson, 2004), judges (Johnson, 2006), and prosecutors (Spohn and Forngano, 2009).[18] These results have held at both the state (Johnson, 2005) and federal (Johnson, et al. 2008) level. However, in general these types of variation explain only a small portion of case outcomes (Ulmer and Johnson, 2004).

Sentencing White Collar Crime

While the research on criminal sentencing has certainly both wide-reaching and deep roots in criminology, little of this research has been conducted with an eye toward the specific processes and definitional issues that are central to studying white collar crime. For example, many traditional demographic correlates of crime, such as age and race, are not associated with white collar crime in the same way (e.g., Benson, 1989; Benson and Walker, 1988; Weisburd et al., 1991); given this, do the same heuristic or attributional concerns affect decision-making in the same way for white collar offenses? Are white collar offenses subject to a different set of processes than traditional crimes? However, neither has white collar sentencing

[18] Though it is worth noting that scholars have been aware of these variations, at least across districts, for quite some time (e.g. Eisenstein and Jacobs, 1977; Myers and Talarico, 1987; Nagel and Schulhofer, 1992)

research picked up on many of these traditional sentencing research questions, rarely problematizing the role of race or gender alone or in tandem.[19] Instead, white collar and traditional sentencing research have existed in relatively separate spheres. This section discusses both historical and contemporary studies of white collar sentencing, as well as how these efforts have been limited by theory and methodology.

Unfortunately, as noted in a recent review of white collar research, the bulk of what we know about white collar sentencing is dated (Simpson, 2013). The most prolific period of time for white collar sentencing research was in a post-Watergate flurry of concern. This conveniently coincided with a surge of critical criminological perspectives that drew attention to the role that power and privilege play in the development and execution of law. However, the limited window in which much of white collar sentencing research was developed does not belie the *concentration* of research in this period. Scholars like John Hagan, Michael Benson, Robert Parker, Stanton Wheeler, and Ilene Nagel Bernstein produced study after study on which to draw inferences. Recently, there has been greater attention to white collar sentencing in criminological literature, perhaps inspired by the most recent spate of white collar crime violations.[20] These contemporary studies are discussed separately from the rest of white collar sentencing research due to the

[19] With the exception of some studies related to gender, discussed below.
[20] Katz (1980) suggests as much when he notes that without continual public focus, the social movement against, or overall public concern with, white collar crime will fade. Both Simpson (2002) and Holtfreter et al. (2008) suggest that we are in a new stage of heightened attention to white collar crimes.

dramatic changes in sentencing regimes between most white collar studies and these.

Regulatory Sanctions

The study of punishment for white collar crime has a long history, and much of it has not been limited to studies of criminal sentencing. Most white collar cases will never even see a prosecutor's desk, let alone a court docket. Thus, a great deal of what we know about the punishment of white collar crime has been learned by studying predictors of regulatory sanctions. We know, for example, that firms with histories of law violating behavior are more likely to be closely monitored for future violations, and thus more likely to be sanctioned if violations occur (Simpson and Schell, 2009). For individual offenders, research has also suggested that social class influences the likelihood of punitive regulatory sanctions (i.e., in excess of the amount of illegal gains, or right to work); however, contrary to the assumptions that have driven much of white collar criminal sentencing research, Eitle (2000) notes that, ceteris paribus, legal workers tend to be punished more punitively than illegal ones. This could be a definitional issue, particularly related to Eitle's coding of "punitive sanctions" – offenders who have no legal place in the market have no securities license to be revoked, and thus are less likely to be *at risk* for "punitive" sanctions.

However, the process through which cases are criminally sanctioned is significantly different than the process through which regulatory agencies sanction offenses. Regulatory agents tend to not see themselves as enforcers of criminal law (Shapiro, 1980) and indeed most regulatory goals are better described as securing

compliance rather than punishing deviance (Ayers and Braithewaite 1992). In this way, we should be cautious in inferring that predictors of regulatory sanctions should similarly predict criminal case processing outcomes.

Traditional White Collar Sentencing Research

A recent review of white collar crime research noted that, "Sociological research on the sentencing of white collar offenders reached an apex in the 1980s" (Simpson, 2013: 321). This arguably remains true, despite the recent spate of contemporary studies (discussed below). In general, these studies use data that was collected before the 1980s, or at least before the implementation of guidelines. But, this is nevertheless an important literature given the number of treatments of the focal question of this dissertation. In general, these studies follow one of two strategies. The first is championed by John Hagan and collaborators, who divide a mixed sample of white collar and other cases by offender and offense, producing four categories: white collar offender who commit white collar offenses, white collar offenders who commit "common crimes", non-white-collar offenders who commit white collar crimes, and non-white-collar offenders who commit "common crimes" (see, e.g., Hagan and Nagel, 1982). The second approach involves predictors of sentences within a sample of white collar offenders, notably by authors of the Yale White Collar Crime Studies, but also others (e.g., Benson, 1989; Weisburd, 1991). The findings of both approaches suggest more inconsistency than a clear pattern of white collar leniency (or liability).

From studies taking the first approach, there may be an advantage to being both a white collar defendant and a criminal who commits a white collar crime.

Educated offenders who commit 'white collar' crimes receive more lenient sentences, while cases where *either* the offense or offender merits the label "white-collar" seem to receive benefits only where white collar cases are more routine (Hagan and Nagel, 1982; Hagan et al., 1982; however see Nagel and Hagan, 1982 and Hagan and Parker, 1985). The pattern also holds when 'white collar' offenders are identified through income instead of education (Hagan, Nagel, and Albonetti, 1982). While not using the same sample or categories of cases, Tillman and Pontell (1992) also find evidence of white collar leniency in the imprisonment decision, but not length of sentence. Cases with only a white collar offense or offender may not always receive discounts, while cases satisfying a more stringent definition of white collar do appear to generate consistent leniency (Hagan and Nagel, 1982; Hagan et al., 1982; Nagel and Hagan, 1982). The discounts for white collar offenders committing white collar offenses may also be larger in districts with more white collar case regularity (Hagan et al., 1982), consistent with "going rates" for "normal crimes" (Sudnow, 1965). Some evidence also suggests that white collar scandals may produce increases in severity for white collar offenses after the fact (Benson and Walker, 1988[21]; Hagan and Palloni, 1986), or at least a renewed focus on white collar crimes (see Katz, 1980).

A more complex picture emerges within white collar crimes. The Yale White Collar Crime data produces evidence of *harsher* sentences for well-off

[21] Benson and Walker (1988) find no evidence of an effect of income or impeccability on the imprisonment decision, but do find that impeccability is associated with an increase in sentence length. Because the authors are responding to findings that suggest *leniency* afforded to white collar offenders, I interpret the loss of a leniency effect after Watergate as an increase in severity. This interpretation is consistent with Hagan and Palloni (1986), who run split sample regressions to demonstrate the onset of increases in punishment for white collar cases (also after Watergate).

offenders (Weisburd et al., 1990; Weisburd 1991; Wheeler et al., 1982; Wheeler et al., 1988), the different measures of status used (such as education, class, respectability) produce variable effects (Weisburd et al., 1990; Weisburd, 1991); additionally, it is unclear if these status measures would operate similarly in a mixed offense sample. However, the same data also suggest, *ceteris paribus*, that offenders with greater community involvement and lower criminal justice histories receive more lenient outcomes (Weisburd, 1991; Wheeler et al., 1982, Wheeler et al., 1988). Social impeccability may also benefit offenders through suspended sentences (Albonetti, 1994, 1999), as may assets and education (Albonetti, 1999).[22] There are some parallels to broader sentencing research in these studies as well; offense seriousness (e.g., duration, scope) is positively related to the imprisonment decision and sentence length (Wheeler et al., 1982), though some evidence suggests that these factors may have differential effects for men compared to women (Albonetti, 1998). Complexity, on the other hand, may be associated with lenient sentencing outcomes (Benson, 1988; though see Weisburd et al., 1991), in addition to its advantages at prosecution. Certain classes of offenses also appear to receive differential sentencing. For example, securities offenses (which may be more serious) are more likely to result in imprisonment than credit frauds, but do not receive sentences that are as long (Weisburd, 1991).

Both for white collar only and mixed samples, generally women receive less severe sanctions on sentence severity scales (Hagan and Nagel, 1982; Hagan et al.,

[22] This is consistent with work by gender scholars. Krutchshnitt (1982) notes that offenders can be legally or socially reputable and disreputable, and finds some evidence that socially disreputable offenders fare better at sentencing, even if they are legally disreputable (i.e., have prior records).

1982; Nagel and Hagan, 1982). Women are also more likely to receive some types of downward departures (Nagel and Johnson, 1994), are less likely to be imprisoned (Albonetti, 1998; Weisburd, 1991; Wheeler et al., 1982; Hagan and Palloni, 1986), and, when they are, receive shorter sentences (Albonetti, 1998; Hagan and Palloni, 1986; Nagel and Hagan, 1982; Nagel and Johnson, 1994; Weisburd, 1991) that are more likely to be suspended (Albonetti, 1994, 1999).[23] The effect of race has been less consistent with prior research in these samples, however, with some studies finding null effects (Albonetti, 1994; Benson, 1989; Hagan and Palloni, 1986; Tillman and Pontell, 1992; Weisburd, 1991; Wheeler et al., 1982), others finding that minorities get an advantage at sentencing (Albonetti, 1998; Hagan and Nagel, 1982; Hagan et al., 1982), and still others finding an effect in the opposite direction (Albonetti, 1998; Nagel and Hagan, 1982).

Contemporary White Collar Research

While the bulk of research on white collar criminal sentencing took place before the turn of the century, this does not mean that there has not been any contemporary research. The question of whether there is indeed an advantage to being a white collar offender is still important in the modern criminological era because the contemporary response to white collar crime has changed substantially since the 1980s. This is evident in both public opinion research (Huff et al., 2010; Holtfreter et al., 2008; Rebovich and Kane, 2002), and in qualitative work among criminal justice officials (Tillman and Pontell, 1992). The short span between white

[23] However, Benson and Walker (1988) find no effect of gender for either the imprisonment decision or sentence length in a different federal court, nor do Tillman and Pontell (1992) in their selected offense set.

collar crime crises in this century (which themselves followed not long after the Savings and Loan crises of the 1980s) may be exactly the sustained attention that Katz (1980) argued was necessary to change the severity of sanctions levied at white collar crime. In this vein, a handful of scholars have returned to the question of whether or not there is a "white collar advantage" at the sentencing stage.

In general, modern white collar sentencing research has used datasets common to more traditional sentencing research and that reflect the guided sentencing structure present in many modern courtrooms. These studies typically identify white collar cases by single offense types, such as fraud (Payne et al., 2011; Shanzenbach and Yeager, 2006; Van Slyke and Bales, 2012), embezzlement (Gottschalk and Rundmo, 2014; Maddan et al., 2012; Shanzenbach and Yeager, 2006; Van Slyke and Bales, 2012), bribery (Shanzenbach and Yeager, 2006; Van Slyke and Bales, 2012), and others, compared to automotive theft (Maddan et al., 2012), or burglary (Van Slyke and Bales, 2012). A handful of these studies offer within-white-collar case comparisons focusing on status (Schanzenbach and Yeager, 2006; Holtfreter, 2013).

As with traditional white collar sentencing studies, the contemporary research findings suggest a muddled picture of status advantage. White collar offenses and offenders may be less likely to go to prison (Maddan et al., 2012), but may serve longer sentences when they do (Maddan et al., 2012). Schanzenbach and Yeager (2006) suggest a potential mediator between status and incarceration, finding that an inability to pay fines may be behind longer prison sentences for low income offenders; additionally, their data suggest that educated offenders are less

likely to go to prison and are more likely to receive downward departures. In contrast, in a select sample of Medicare Fraud cases, Payne et al. (2011) find evidence of both status advantage and liability; high status offenders are more likely to get the most severe sanction (prison) but are also more likely to receive a sentence of community service. These conflicting findings across studies are potentially the result of studying different types of white collar crime that may be responded to with different levels of severity (Van Slyke and Bales, 2012). Definitional issues are compounded by variation within samples; for example, high profile cases may produce more severe penalties, though thus far this research has only been conducted in civil court cases (McShane et al., 2012).

Race effects appear equally complex; depending on the analytic strategy taken, minorities may receive harsher (Schanzenbach and Yeager, 2006) or more lenient (Schanzenbach and Yeager 2006;[24] Van Slyke and Bales, 2012) sentences relative to whites, or no difference at all (Maddan et al., 2012; Van Slyke and Bales, 2012[25]). Income appears to exacerbate these differences (Schanzenbach and Yeager, 2006). Evidence is more conclusive for gender, with somewhat consistent evidence that men get harsher punishment (Maddan et al., 2012; Schanzenbach and Yeager, 2006; Van Slyke and Bales, 2012, 2013). However, authors have suggested that differences in offending behavior or roles may explain this (Holtfreter, 2013; see also Steffensmeier et al., 2013). Additionally, Van Slyke and Bales (2013) note

[24] Their results suggest that controlling for base offense level show advantages for minorities, while controlling for *final* offense levels show disadvantages; this may suggest some racial disparity in the practice of applying the guidelines.
[25] Hispanics are less likely to be incarcerated than whites, but blacks are no more or less likely.

that the gender effect in their sample seems driven by male judges giving discounts to female *street offenders*, but not white collar offenders.

Theorizing Prosecution and Sentencing Decisions

Scholars who study prosecution and judicial decision-making have adopted various theoretical approaches to explain behavior at these stages. The paradigms of uncertainty avoidance and focal concerns share a murky, overlapping, approach that has been applied across multiple decision points and actors. Courtroom community and workgroup theories similarly share conceptual territory and are more often applied to explain plea bargaining decisions by prosecutors, but also suggest a relationship between prosecution and sentencing outcomes. These theories share broad agreement on many concepts but differ in their emphases and levels of analysis (Johnson et al., 2016). In addition to these general theories of prosecution and sentencing, there are also theories that predict either leniency or liability for white collar offenses specifically. There are more similarities than differences in these approaches, though some have risen to prominence in the field (e.g., the focal concerns perspective), while others have achieved only limited resonance (e.g., Gottfredson and Gottfredson's theory of criminal justice decision-making, 1988). While it is not the purpose of this dissertation to summarize the varying degrees of the support for these theories, or to debate their relative conceptual merits, it is worth discussing several explanations for the apparent disparities between offender sentences. These theories often generate similar predictions for how cases likely will be treated, and many are at least partially rooted in a rational choice theory, assuming that actors are rational, weighting

benefits and consequences of particular possible outcomes by the probability that the outcome occurs.

Perhaps the most heavily rooted in the rational choice tradition is the "shadow of the trial" approach to explain prosecutorial decisions to engage in plea bargaining. The shadow of the trial literature suggests that both prosecutors and defendants are rational actors that seek to maximize their utility; the prosecutor seeks to maximize the probability of conviction using the least possible resources, while the defendant seeks to minimize the probability of conviction and, if convicted, receive the least severe possible sentence (Landes, 1971). Both of these calculations involve estimations of the probability of conviction at trial and the probable sentence at trial. In an effort to conserve resources, and to ensure conviction (trials, after all, are never certain), the prosecutor may offer a plea deal for a lesser charge or sentence. The "discount" afforded to pleas is said to reflect the probable sentence at trial, downwardly adjusted for the increase in certainty (Bibas, 2004; Bushway et al., 2014).[26] However, there is mixed empirical evidence for this purely rational interpretation of the shadow of the trial (e.g., Bushway and Redlich, 2012; Bushway et al., 2014; Smith, 1986).

One step removed from this pure rationality is the focal concerns perspective. Focal concerns is arguably the dominant theoretical tradition for criminologists studying sentencing, and has also been applied to prosecutors. The paradigm was introduced by Steffensmeier et al. (1998) to explain persistent

[26] That is to say that the Pr(Conviction at Trial)*(Sentence at Trial)=P(Conviction with Plea)*(Sentence with Plea). Because conviction is assured with a plea agreement, Pr(Conviction with Plea)=1, producing the following equilibrium: Pr(Conviction at Trial)*(Sentence at Trial)=Sentence with Plea.

sentencing differences experienced by offenders who are, young, male, and black. According to the theory, at sentencing, judges take into consideration three "focal concerns": (1) offender blameworthiness, (2) protection of the community, and (3) practical constraints. Offender blameworthiness is a somewhat abstract concept that encompasses both the harm done and culpability; often, judges rely on attributions of internal or external sources of criminality to determine blameworthiness. Protecting the community involves a judgment about an offender's future likelihood of criminal activity if released on not charged. Practical constraints vary depending on the actor and decision and may be argued to contain a wide variety of concerns. For judges, practical constraints are typically interpreted as relating to jail and prison capacity in a district. However, this construct can be much broader than correctional resource constraints. For example, work by Daly (1987, 1989) suggests that judges are also concerned with costs to children (particularly when the offender is the primary caregiver). Additional work by Jesilow et al. (1986) suggests that judges may be less likely to incarcerate white collar (Medicare fraud) offenders because the community would suffer a greater loss in services. Practical constraints also can include perceptions about an offender's ability to "do time" – this may be due to an offender's advanced age, but it also could include white collar defendants as a special class. Judges have long seen white collar defendants as less capable of surviving in prison, a contention known as the "special sensitivity hypothesis" (Mann et al., 1980; Pollack and Smith, 1983). Practical constraints on prosecutors have been interpreted slightly differently; these constraints may reflect a wide variety of concerns, including finite resources (Farnworth et al.,

1991), a desire for high conviction rates (Frohmann, 1997), political capital (McConville and Mirsky, 2005; Vogel, 1999), evidence strength (Holleran et al., 2010), and victim cooperation (Kingsnorth et al., 1998).

While judges and prosecutors would ideally have perfect information on all of these concerns, they are forced at times to make shorthand judgments that are influenced by the limited information they have about an offender and thus are forced to rely on attributions and heuristics that facilitate determinations in the absence of exhaustive information. Thus, a defendant's prior record comes to be seen as a quickly accessible proxy for a defendants' future risk of offending (protection of the community) while race, sex, age, and offense severity come to imply blameworthiness through various stereotypes (Johnson et al., 2016; Pyrooz et al., 2011). Similarly, protection of the community gets reduced to a focus on offender criminal history, employment, and marriage, as well as race and sex; to the extent a defendant has characteristic that is associated with increased risk of criminal involvement if not charged or imprisoned, they are unlikely to receive leniency (e.g. Spohn and Spears 1997; Spohn et al. 2001). These attributional assessments produce differences in sentences along race, sex, and other demographic lines (see also Kramer and Ulmer, 2009).

The practical constraints of focal concerns as applied to prosecutors parallel the idea of "uncertainty avoidance" initially raised by Albonetti in the late 1980s and early 1990s (e.g. 1986, 1987, 1990; Albonetti and Hepburn, 1996). The theory draws heavily on the assumption that judges and prosecutors operate under conditions of bounded rationality (March and Simon 1958); they lack both the time

and critical information to make fully informed decisions about the future risk of an offender. In determining offender dangerousness, for example, judges rely on stereotypes about individuals of different races and sexes to make their decisions (what Simon, 1956, calls "satisficing behaviors"). Relatedly, Albonetti argues that prosecutors are motivated by securing a conviction, and in the case of uncertainty (weak evidence, uncooperative witnesses, chance), prosecutors will be more likely to attempt to secure plea agreements. In addition, according to Albonetti, judges also reflect on prior decisions made by other criminal justice decision-makers, especially prosecutors. For example, cases in which prosecutors oppose bail may be interpreted as involving more serious offenders. Similarly, offenders whose bails are set higher are also presumed to be more serious. In the case of both uncertainty avoidance and focal concerns, prosecutors are risk averse and seek to maximize convictions.

The idea that the outcomes of prior decision-making affects subsequent decision-makers is a central feature of the heuristic processing model discussed by Hill, Harris, and Miller (1985). They draw on the same notion of bounded rationality and attributions as Albonetti (1991); given that there are constraints on knowledge, actors in the criminal justice system develop and rely on heuristics, mental shortcuts, to simplify complex decisions in a semi-rational way. The development of these heuristics are often grounded in the perceptions of social realities, resulting in bias. For example, a heuristic may develop as follows: most poor people are black, and poverty is associated with increased criminal activity, therefore, minorities are more likely to be involved in criminal activity; a police

officer considering making an arrest draws on this link between minorities and crime and chooses to arrest the minority suspect, who is "probably guilty of something". Hill et al. (1985) argue that decisions are organized according to trait display (how much the case/the offender looks like the average case/offender of its type), the payoffs to the decision-maker of moving forward, and the cost of choosing *not* to act (for example, the cost to the judge for letting an offender avoid prison if the offender then commits another crime). Their approach aligns closely with sociological work on "type-scripts" (Harris, 1977). When offenders resemble the offenders usually associated with a given crime type, they are typically treated as such, and are advanced through the criminal justice process. When offenders do *not* resemble the usual type, however, they often are diverted at these early stages, resulting in bias according to offender characteristics. Yet, given that judges reflect on prior decision-makers, Hill et al. (1985) argue that judges are likely to be *more* punitive toward offenders who are not consistent with the usual "type", inferring that these offenders must be particularly serious if they were not filtered out. This fits with the findings of leniency by Hagan and his colleagues for educated offenders who committed white collar crimes (e.g. Hagan and Parker, 1985). This is also consistent with Sudnow's (1965) discussion of "going rates" for "normal crimes" (see also Swiggert and Farrell, 1977), as well as the empirical finding of sentence discounts for white collar cases only in districts where white collar cases are somewhat common (Nagel and Hagan, 1982).[27] Importantly, this theoretical

[27] Each of the theories discussed in this chapter focuses on explaining individual decisions as though independent from the larger environment in which these decisions are made. However, as noted, there is evidence to suggest that is not entirely the case. Some approaches, notably "courtroom community theories" but also applications of racial threat theory, situate individual

approach also suggests that the same characteristic can prompt both leniency and punitiveness by decisionmakers, depending on the stage of processing.

These same themes are paralleled in Gottfredson and Gotfredson's (1988) theory of criminal justice decision-making. They argue that prosecutors and judges are influenced (as are all agents in the criminal justice system) to advance cases based on offense seriousness, the offender's prior criminal record, and the relationship between the victim and the offender. The more serious the crime and the longer the offender's history of criminal activity is, the more likely cases are to be advanced. However, cases where the victim knows the offender are less likely to move forward, because these "private issues" are less likely to be seen as appropriate issues for the criminal justice system. They further argue that discretion is most prevalent with less serious offenses and offenders (as well as crimes between nonstrangers). In having this discretion, however, decision-makers are notably concerned with what will happen after they decide to move a case forward, or let it exit the criminal justice system (also consistent with some versions of "protection of the community" in the focal concerns perspective).

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prosecutor and judge decisions within larger contexts of courtroom workgroups and district-level variation. According to this approach, local "culture" develops and replicates itself over time, resulting in standard practices, or "going rates" for similar crimes (Emmelman, 1996; Nardulli et al., 1988; Sudnow, 1965). These become baseline agreements, from which all other similar cases are negotiated in reference (Feeley, 1979). Supporting evidence for these claims is found in numerous studies that evidence markedly different early case processing and plea practices across courts (e.g., Eisenstein and Jacobs 1977; Franklin 2010; Haynes et al., 2010; Johnson 2014; Nagel and Scholhofer 1992). In addition, sentencing research has (especially since the 2000s) has shown that a consistently non-negligible portion of case processing variation is explainable by district level features (e.g., Britt, 2000; Johnson, 2006; Ulmer and Johnson, 2004). However, this district-level variation is not the focus of this dissertation, and is this only briefly discussed. Modeling adjustments are made to account for residual variation related to district, discussed in Chapter 3.

Of the theories applied to prosecutors, Wright and Engen (2007) note, "All of these [theories], however, try to answer a common question: '*What* does the prosecutor want to accomplish and what considerations affect how the prosecutor pursues those goals?'" (emphasis in original; 2007:11). The common theme across all the theories discussed above is that prosecutors' decisions are generally believed to be driven by a desire to maximize both convictions and the severity of crimes convicted while using minimal resources to secure those ends. In addition, the concept of offense seriousness and offender history appear in each theory of sentencing, as well as many theories to explain prosecutor decision-making. Several of the theories also mention the importance of prior decision-making in informing judges' perceptions of offenders, beyond the limiting role that prosecutorial decisions can have in shaping the correctional options available to judges. Additionally, most theories note that while judges and other criminal justice actors would ideally make purely rational decisions, their opportunities to do so are limited both by time and available information. In the absence of perfect information and infinite time, decision-makers use mental short-cuts (often using readily-available demographic characteristics of defendants, or looking backward to prior decisions) to simplify the process. The ultimate goal of these behaviors is to increase the success of correctly identifying cases in which the offender can be "safely" given a lesser sentence than might be implied by deserts alone. However, these theories vary in whether other factors should be expected to influence actors' decisions. For example, an overburdened jail or a defendant with multiple minor dependents would not influence a judge's sentencing decision under Albonetti's

attribution theory, but would under focal concerns theory. In addition, typescripts theory might suggest that young minority male defendants are funneled through early stages, but are granted more lenient outcomes than atypical defendants at sentencing, in direct contrast to focal concerns theory. None of these theories speaks directly to how "white collar" cases might be sentenced. Yet, as discussed below, the historical theories used to explain white collar sentences discuss concepts that in many ways parallel these traditional ways of thinking about prosecution and sentencing (Holtfreter, 2013; Maddan et al. 2012).

Explaining White Collar Outcomes

While there exist no specific theories of white collar prosecution, the common thread that prosecutors seek to maximize convictions with minimum resources offers a prediction for white collar offenders. White-collar crimes present obstacles to those ends because they are complicated and require heavier resource expenditure. They are also rarer, suggesting that workgroup norms will not have developed around these types of offenses. Accordingly, we might expect that white collar cases receive more lenient outcomes for prosecutors. Hagan and Bernstein (1979) found that where white collar crimes were more commonly prosecuted, sentences were lower and more consistent, reflecting workgroup norms and paralleling Sudnow's (1965) "going rates."

In contrast, a number of theories have attempted to explain white collar sentences. Much of this work focuses on explaining why white collar cases may be sentenced more leniently than traditional crimes. This is sometimes referred to as "status advantage" (Simpson, 2013). The assumption is justified by empirical work

that suggests that judges may identify with white collar criminals on a personal level, and choose to treat them more leniently (Mann et al., 1985; Wheeler et al., 1988). Relatedly, judges may also worry about political (Giordano, 1983) or community fallout from sentencing these high-status, well-integrated, offenders (Jesilow et al., 1986), a concern that may well be extended to politically minded prosecutors. However, this is not the only reason that white collar offenders may receive more lenient punishment; there is also the assertion that these, less-hardened-but-still-criminal, offenders may not be able to adapt well to prison, or that prison would be an unfairly difficult experience for white collar offenders (known as the "special sensitivity hypothesis") (see, e.g. Pollack and Smith, 1983; Wheeler et al., 1988). Additionally, given that white collar offenders have "farther to fall," they may also suffer additional punishments, such as loss of a prestigious job or community exclusion, producing a cumulatively larger punishment (see Benson, 1989).

Yet, there is reason to believe that white collar offenders may also be at risk for particularly *harsh* sentences, according to the status liability hypothesis (Rosoff, 1989; Skolnick and Shaw, 1994; Wiggins et al., 1965). This hypothesis argues that high status is associated with "goodness" (legal behavior) and criminal involvement violates these expectations in an unpleasant way; as a result, high status offenders receive harsher punishments than those who do not present such mentally discordant images. Other interpretations argue that judges may sentence white collar defendants more seriously because status may be perceived of as a good, in exchange for which legal behavior is expected; those who are socially privileged

and commit crime have "welched" on their side of the bargain (Wahrman, 1970, 1972). Relatedly, attribution theory may also suggest harsh sentences because white collar offenders evidence a greater difference between our expectations and observations, which we then react toward with increased punitiveness compared to those whose offending behavior is less divergent from our expectations (Giordano, 1983). This is consistent with some of the findings from the Yale White Collar Crime Project (Wheeler et al. 1982, for example).

However, this liability may only be present for those at the extreme upper bounds of the status hierarchy; additionally, if the criminal act is consistent with the expectations we have for a particular actor, the liability may be ignored entirely (Wahrman, 1970). Thus, while in general high status actors may be punished harshly, particularly if they are key figures, there may be different levels of liability for some than others. For example, recently both comedian Bill Cosby and (then-candidate) President Donald Trump have faced allegations of sexual impropriety by numerous women. Bill Cosby's career has been built on "clean", family-focused comedy; the allegations had a significant detrimental impact on his career. In

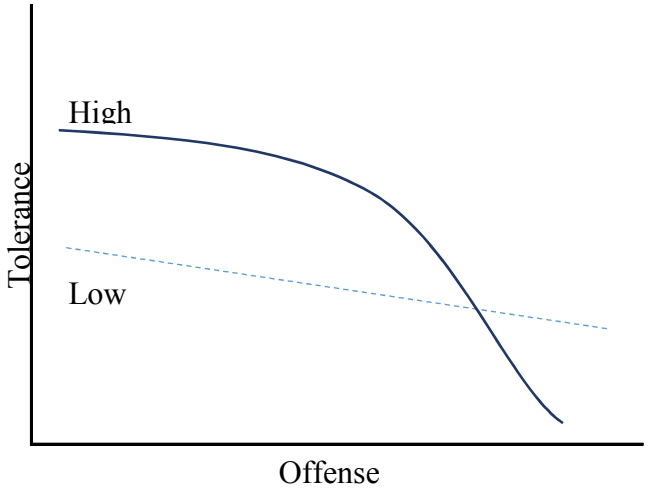


Figure 1. Tolerance for Deviance by Seriousness for Low and High Status Offenders

contrast, President Trump's image as a "playboy", with multiple marriages and infidelities, was not harmed significantly, in that he was able to secure the presidency. Rosoff's (1989) study suggests that for extreme deviance, status may serve as a liability, while lesser forms of deviance may benefit from "status-shield" effects (Tillman and Pontell, 1992; see Figure 1 for a hypothetical depiction of this relationship).

Rosoff's (1989) study is of particular interest for the study at hand because he considered the effect of relative status within an already high status category of offenders (doctors; see also Wiggins et al., 1965). However, the focus on regulatory offenses committed by licensed professionals may increase the odds of observing a status-liability effect (Skolnick and Shaw, 1994); in the present sample, offenders may already have already been selectively filtered out to leave only the most serious regulatory offenders.

In contrast to the sociological concern of status liability and status-driven leniency, work by Wheeler et al. (1988), as part of the Yale White Collar Crime Studies, parallels traditional sentencing theories. Using interviews with federal judges discussing real cases, the authors identified three primary concerns of judges: the offense's harm, the offender's blameworthiness, and the consequences of sentencing. Generally, judges report perceiving white collar crime as less harmful than traditional crimes (with some exceptions, particularly long lasting, complex offenses). In addition, stereotypical white collar offenders typically do not

have criminal records and are generally seen as posing little risk of recidivism.[28] Judges also report concern over other costs, such as job loss, to white collar offenders, consistent with the special sensitivity hypothesis. Altogether, this suggests that white collar offenders, because of their histories, social ties, and perceptions of seriousness for the types of offenses involved, may be in favorable positions compared to street offenders.

Limitations of Extant Research

In trying to understand how exactly white collar cases come to be punished in the (particularly federal) criminal courts, there are limits to what can be gleaned from prior research in the area. As mentioned, white collar crime sentencing research has remained a fairly limited academic enterprise, and the bulk of this literature is severely dated (Simpson, 2013). While this in itself is not a key limitation (Clinard and Yeager's 1980 work is still considered seminal in the field, for example, as is Sutherland's 1949 book), there are reasons to question the modern generalizability of these older studies. First, the nature of sentencing has changed markedly since the 1970s. Sentencing guidelines have been implemented at the federal and (often) state level, which has been effective in reducing some forms of disparity (e.g. Miethe and Moore, 1985; Stolzenberg and D'Alessio, 1994). It is not clear if this same process has reduced or eliminated sentencing disparities between white-collar and street offenders. While there are (as noted above) a handful of studies that have explored status advantage in a modern

[28] Podgor (2007) argues that this is an important part of white collar sentencing, one that the updated offense harm table has overlooked in the current version of the federal sentencing guidelines.

sentencing context, these studies are few in number and restricted to certain populations and certain jurisdictions. Further, as argued previously in this work, the present time appears to be one of decreased tolerance for white collar (especially corporate) crime. This suggests that the status advantage gained by offenders for less serious offenses may be gone, and replaced with an exacerbated status liability.

In addition to being dated, the literature is also limited by its heavy reliance on particular datasets. Sentencing data from the 10 federal district courts of the Yale White Collar Crime Studies make up one-third of the data sources in white collar sentencing studies before 2000 (Albonetti 1994, 1999; Benson, 1989; Weisburd et al., 1990; Weisburd, 1991; Wheeler et al., 1982, 1988). Hagan's work on data from the Administrative Office of the U.S. Courts make up nearly another third (Hagan and Nagel, 1982; Hagan et al., 1982; Hagan and Palloni, 1986; Nagel and Hagan, 1982). The remaining works either offer no empirical analysis (Benson and Cullen, 1988; Giordano, 1993; Jesilow et al., 1986), focus on only a small subset of crimes (Hagan and Palloni, 1986; Tillman and Pontell, 1992), or also use data collected from federal courts (Benson and Walker, 1988). Modern studies are little better in this regard, with Florida's guidelines providing just shy of one-third of the studies (Van Slyke and Bales 2012, 2013), federal guidelines data another third (Schanzenbach and Yeager, 2006; Maddan et al., 2012), and specific crime types providing the remainder (Payne et al. 2011; Holtfreter, 2013), with one international study (Gottschalk and Rundmo, 2014).

White collar sentencing research, like much of traditional sentencing research, is also limited by its conceptual focus on judges as primary determiners

of sentences. The studies of white collar sentencing discussed in preceding sections are generally based on convicted defendants, particularly when relating to criminal sentences (opposed to regulatory sanctions). If research aims to fully unpack not only the existence but also the mechanisms of disparities in sentencing between different groups, it becomes necessary to move to studies of process that begin long before sentencing hearings, particularly focusing of prosecutors (Ulmer 2012). The lack of attention may even partially explain the complicated and mixed findings of potential disparity in treatment of white collar cases and white collar offenders. Thomson and Zingraff (1981), speaking of a different type of disparity, note that

> Populations in the later stages of the judicial process may be homogenous in terms of status… If, as the research indicates, discrimination is concentrated in the earlier decision-making stages, research which does not account for the processural nature of decision making or which analyzes populations at just the later decision points will tend to produce findings of no discrimination. (1981: 871)

Thus, in the evaluation of potential disparate treatment of white collar cases (perhaps especially for white collar cases, as argued above), prior research has limited its efficacy by taking a narrow view of sentencing.

THE PRESENT STUDY

This dissertation offers a contribution to both white collar and sentencing literatures. It responds to a general dearth of white collar sentencing studies in the modern era and is reflective of the context in which most sentencing decisions are currently made. It also takes a more nuanced approach to explaining sentencing outcomes by looking not only the proximal decision-makers, but also at contingent processes that cross criminal justice agencies. The hypotheses below address questions of case processing that are both "within" white collar offenses and

"between" white collar crime and so-called traditional crimes, and will be reflected in complementary analytic strategies.

For white collar crime, the entrance to the federal criminal justice system is guarded by many gatekeepers; the last of these, and arguably the most influential, is the prosecutor. Federal prosecutors determine which, of eligible cases, will be filed in criminal court and the conditions under which they will be filed. Prosecutors' roles extend beyond gatekeeping, however, in that they continue to act as guides as cases move through the system, diverting or tracking cases one way or another through the potential offer and conditions of plea agreements. Theories of prosecutor decision-making suggest that many of these decisions are a function of prosecutors' desires to close a case "successfully" through the securement of a guilty verdict, regardless of the means of that conviction. Studies of white collar crime have suggested that case complexity of white collar cases stands as a primary obstacle to otherwise willing prosecutors by reducing the likelihood that prosecutors will be able to get sufficient evidence or convince judges and juries of the facts of a case. When prosecutors are faced with complex cases that undercut their confidence in attaining a conviction, they will engage in satisficing behavior by attempting to secure convictions for lesser offenses, either through their choice of initial charges or in their decisions to offer and enter plea bargains with white collar defendants. In addition, complex cases may require more resources from prosecutors.[29] In an effort to dispose of cases more quickly, prosecutors should

[29] Case complexity is an abstract term that is not perfectly captured by the presence of or number of codefendants. Albonetti (1999) and several studies from the Yale White Collar Crime Studies each include the number of codefendants involved. These studies also considered duration of the offense, organizational involvement, and a preordained plan (see also Weisburd et al., 1990;

engage in similar strategies as though they were unsure. Thus, I expect that, ceteris paribus:

> **H1: Case complexity will be associated with more lenient prosecutorial outcomes for white collar cases, as indicated by charge reductions, count reductions, and charge dismissals.**

Recent developments in sentencing suggest that these prosecutorial outcomes will influence judicial outcomes (e.g., Kutateladze et al. 2014, Wooldredge et al. 2015) i.e., prosecutors and judges are not independent actors. There is both theory and evidence to suggest that prosecutors may act as either or both "structurer" and "signaler" for judicial decisions. Prosecutors fundamentally construct (structure) the legal characteristics of cases that judges based their decisions on through decisions of initial charge severity and charge bargaining. In other words, prosecutorial decisions may be thought to have an indirect effect on sentencing outcomes through guidelines offense scores.

However, theories of criminal justice processing suggest that judges do not sentence on legal characteristics alone. Instead, judicial decision-making can be described as "satisficing behavior," in which judges attempt to make rational decisions in imperfect environments. Judges may look toward defendant attributes to inform their decision-making, such as defendants' race and marital status. However, judges' behaviors may also be informed by decisions made by anterior criminal justice agents; in environments of uncertainty, often the status quo remains

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Weisburd et al., 1991; Wheeler et al., 1982). Unfortunately, none of these other variables is present in the FJSP. Antitrust and securities frauds offenses have often been considered to be more complex for generally involving more technical knowledge, longer lasting offenses, higher dollar loss, and involving larger conspiracies. An attempt was made to isolate these offenses as another measure of case complexity, but the number of cases was generally too small.

unchallenged, or is even confirmed by selective attributions (Thaler, 2014; see also Hill et al., 1985). Thus, regardless of the legal characteristics of terminating charges, judges rely on outside signals as to the risk that defendant's pose to the general public future crimes, as well as their blameworthiness for the offense committed. A great deal of literature has suggested that individual characteristics such as race, sex, and prior record, may act as signals to judges when direct measures of blameworthiness and risk are lacking. Yet, judges may also consider decisions by anterior decision-makers as "signals" about the relative risk of particular defendants. Cases receiving leniency at one stage may thus be seen as more deserving of leniency at a later stage. If so, prosecutorial outcomes such as negotiated pleas may have direct effects on sentencing above and beyond their effect on legally relevant variables (offense severity). This suggests:

H2: Prosecutorial decisions will have indirect influence on sentencing outcomes via legal characteristics

H3: Prosecutorial decisions will have direct influence on sentencing outcomes

Literature suggests that there is "something different" about white collar cases that manifests in differences in sentencing outcomes. However, the nature of that difference is not clear. Theories of status leniency, focal concerns, and the special sensitivity hypothesis argue that white collar cases are likely to see less severe sentencing outcomes than similar traditional crimes because judges may identify with offenders to a greater degree, may worry about the community's loss of otherwise prosocial members, or may fear that white collar offenders are less able to tolerate harsh forms of punishment. Evidence is not wholly supportive of these theories however, as discussed above. Interviews with judges suggest that

white collar cases often pose particularly challenging for sentencing decisions, as they often capture conflicting elements of severe harm inflicted and individual characteristics that are typically associated with lesser risk of future crime (employment histories, family connections, etc.). As a result, it seems prudent to state that literature suggests a relationship in either direction. Thus:

H4: White collar crimes will be treated differently than comparable crimes at sentencing.

However, the term "white collar crime" has a less-than-agreed-upon definition. Variations in defining and identifying white collar crime may, in part, explain the inconsistencies in previous studies predicting white collar leniency. Because "white collar crime" was originally intended to describe crimes committed by a certain class of *offenders*, the inherent need of crime researchers to identify subjects according to characteristics in administrative data often requires relying on *offense* characteristics and produces a set of "white collar" crimes with varying shares of the types of offenders to whom Sutherland (1939) originally referred. Weisburd (1991) argues that broader offense-based definitions (e.g., all fraud and embezzlement are "white collar" crimes) will inherently include more traditional offenders and fewer high-status, occupationally-involved offenders. "Embezzlers" in the Yale sample were more often middle-class, and were much younger than, for example, antitrust and securities offenders (see also Daly 1989). This, along with the previous patterns of both significant and insignificant relationships between a case's distinction as "white collar" and sentence severity suggests that the process of defining and identifying white collar cases likely has substantive effects on the type of relationship observed. It can be reasoned that more restrictive definitions of

white collar (i.e., those that focus on high-status offenders committing occupation-related offenses) should evidence greater divergence from traditional crimes than those that are less restrictive (i.e., definitions that focus on nonviolent financial and fraud offenses). Thus:

> **H5: More restrictive definitions of "white collar crime" will evidence greater differences in sentencing outcomes relative to comparable crimes than will broader definitions of "white collar crime"**

Finally, due to the relative rarity, complexity, and resource-intensive nature of white collar offenses compared to traditional crimes, I have argued that the role of the prosecutor is particularly important to understand white collar sentencing. This argument is more than justification for the research at hand. Rather, the notion that prosecutors' impact on sentencing outcomes is of interest for white collar crimes similarly implies a substantive claim that the impact of the prosecutor, either indirect or direct, should be larger for white collar crimes than for traditional crimes:

> **H6: The impact of charge and count reductions or dismissals on sentencing outcomes should be larger for white collar cases than for traditional crimes.**

CH 3. DATA & METHOD

INTRODUCTION

This chapter discusses the data series used to conduct the analyses in this dissertation, the Federal Justice Statistics Program, as well as the stages of processing that will be analyzed. I also provide a detailed discussion of the four identification strategies used to capture white collar crime in the data, the variables of interest, and relevant controls. Finally, the chapter concludes with a description of the analytic strategy that I will use to test the hypotheses outlined in the preceding chapters.

DATA

Collectively known as the Federal Justice Statistics Program (FJSP), the data are collected from multiple independent departments within the federal Department of Justice, including the U.S. Marshals Service (USMS), the Executive Office of the United States Attorney (EOUSA), the Administrative Office of the United States Courts (AOUSC), the United States Sentencing Commission (USSC), and the Bureau of Prisons (BOP). The Bureau of Justice Statistics (BJS) performs annual extracts of these agencies' administrative records to generate comprehensive data on criminal cases from the earliest stages in the federal justice system through their conclusions. BJS also provides a unique "link" datafile that allows a case to be identified from one stage (for example, the filing of a case) to the next (e.g., conclusion of a case). Datasets from each stage, as well as the link file, of the FJSP are available from the Interuniversity Consortium for Political and

Social Research (ICPSR), in the National Archives of Criminal Justice Data (NACJD).

Despite the richness of the data, the linking file of FJSP does not have a long history of use in criminology. Only a handful of peer-reviewed papers make use of the linking file to examine multiple stages (Shermer and Johnson, 2010; Starr and Rehavi, 2014). Much more common has been use of individual components of the FJSP, particularly the USSC sentencing data, which have been used quite extensively. However, given the recent emphasis on early stage influencers of sentencing and cumulative impacts, a focus on convicted cases alone can offer only so much insight. Instead, I make use of the link file to follow cases from initial criminal filing through their conclusion, considering multiple pathways, outcomes, and processes.

Sample

This dissertation follows a cohort of matters referred for criminal prosecution between 2009 and 2011 using data spanning 2009-2013 to allow the majority of these cases to come to conclusion.[30] The collection of matters referred includes matters that are referred to U.S. Attorneys for prosecution by regulatory and justice agencies, as well as cases that are developed through the special units of the Office

[30] ICPSR studies used in this dissertation include the following: (1) EOUSA Matters In - 2009 (30793), 2010 (34335), 2011 (35049), 2012 (35500), and 2013 (36336), (2)EOUSA Matters Concluded – 2009 (30790), 2010 (34332), 2011 (35055), 2012 (35498), and 2013 (36333), (3) EOUSA Defendants in Criminal Cases In – 2009 (30791), 2010 (34333), 2011 (35056), 2012 (35489), 2013 (36334), and 2014 (36704), (4) EOUSA Defendants in Criminal Cases Terminated – 2009 (30792), 2010 (34334), 2011 (35048), 2012 (35499), and 2013 (36335), (5) AOUSC Cases Filed – 2009 (30781), 2010 (34324), 2011 (35041), 2012 (35490), and 2013 (36324), (6) AOUSC Cases Terminated – 2009 (30784), 2010 (34326), 2011 (35043), 2012 (35492), and 2013 (36327), and (7) USSC Cases Sentenced – 2009 (30795), 2010 (34337), 2011 (35051), 2012 (35502), and 2013 (36338).

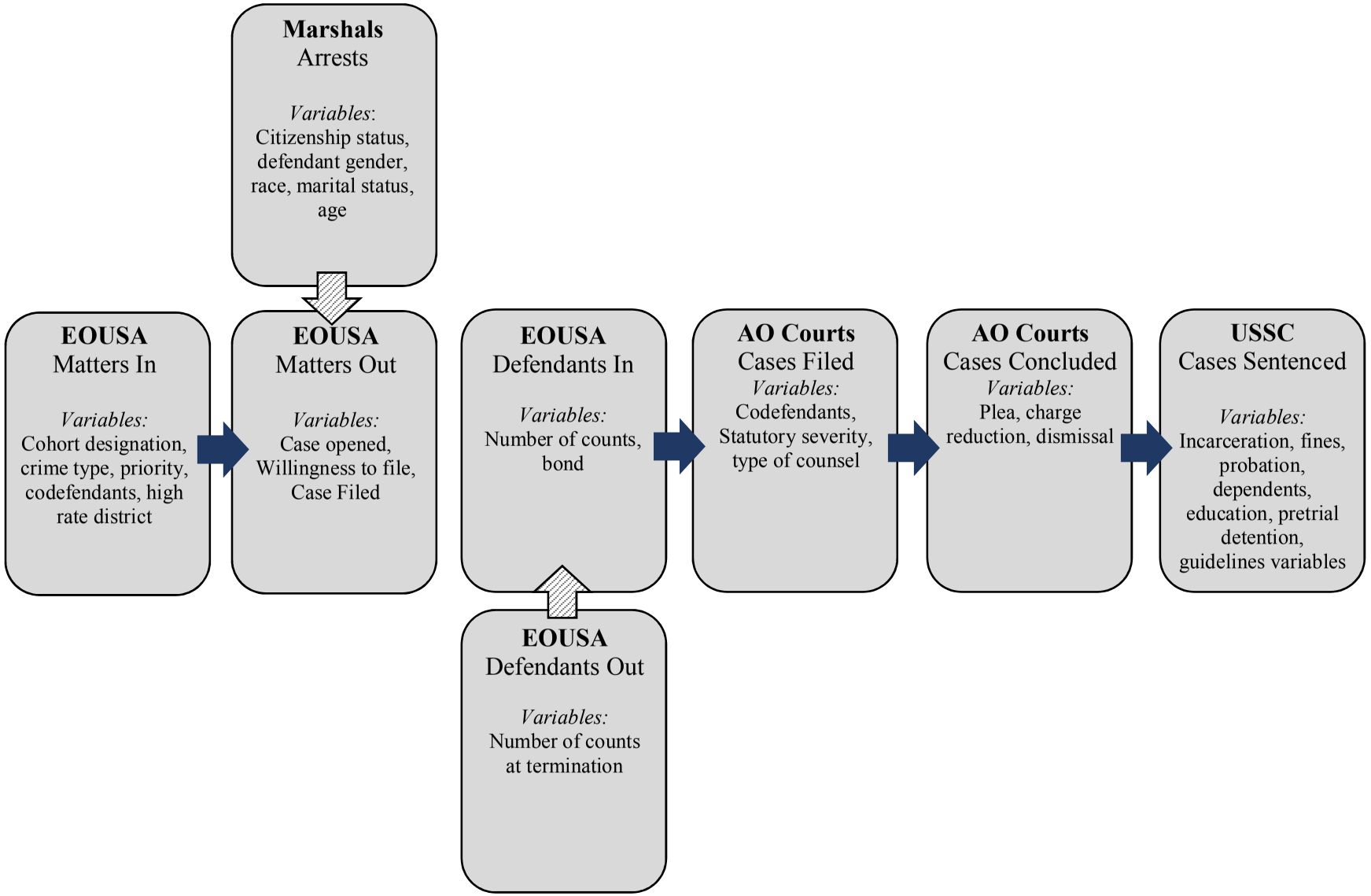
of the U.S. Attorneys. I identify white collar and comparable crimes at the point of referral and then follow them through the successive stages of the criminal justice process (see Figure 2, which includes a discussion of the important variables provided by each of these datasets).

To be sensitive to the ways in which white collar cases referred for prosecution ultimately do (or do not) arrive at criminal court, Chapter 5 includes contextualizing (i.e., descriptive) analyses that follow cases from matters referred through the prosecutors' decision to file a case in order to offer some insight as to the total criminal justice system process. Ideally, this dissertation would explore the important decision point of matter declination (i.e., which cases are rejected by the prosecutor). However, due to a lack of individual characteristics (e.g., suspect race, sex) or evidentiary variables at this early stage, this dissertation cannot reliably make causal arguments about this decision. While unarguably a critical point in a study of case processing, developing a model of this decision would inherently suffer from omitted variable bias. Noncitizens and juveniles are excluded from analysis.

While the FJSP data are available prior to 2009, this dissertation does not use earlier data for methodological and substantive reasons. The ability of researches to link files across the FJSP has improved over time, leading to a prioritization of the most recent data. In addition, the financial recession may have affected case processing outcomes (both prosecutor and judicial decisions) for white collar cases initiated after 2008, regardless of whether the case was related to the offense. The current time period thus reflects the processing of federal criminal

cases *after the financial crisis of 2008*. Inclusion of cases initiated during an earlier time period may introduce temporal effects on decision-making, which is beyond the scope of this research.

Figure 2. Stages of the Federal Justice Statistics Program Used



LINKING DATA ACROSS STAGES

The FJSP's structure allows research to be done on each dataset in the series, or by linking data from one stage of case processing to another. That link is made possible through the use of "linkfiles." The FJSP series incorporates ten linkfiles, each of which allows data from a "dyad" (a specific set of two data series) to be linked. This process is facilitated through the use of unique case identifiers present in each of the FJSP individual data files. The linkfiles act as crosswalks, allowing identifiers from one data series to be matched with identifiers to a different series. For example, one linkfile allows for case identifiers from the Administrative Office of the U.S. Courts dataset on concluded cases to be matched to case identifiers from the United States Sentencing Commission. By linking multiple pairs of dyads, it becomes possible to link data from the earliest point of case processing (the point at which a matter is referred for criminal prosecution) to the point of sentencing, and even beyond (through the use of Bureau of Prison data, which are not used in this dissertation). To follow cases through these points and to maximize the information available about these cases, I have performed six linking procedures (see Figure 2).

The first link allows me to follow cases from the point at which they are referred for prosecution (matters in) to the point at which the matter is concluded.[31] This conclusion may include the matter being closed without any action from the

[31] The number of merges required to link datasets is actually *twice* the number of dyads (in this case, 12). The original (or "master") dataset must first be linked to the associated dyadic linkfile. This "master + link" dataset can then be used to merge with data from the second ("using") dataset. This, unfortunately, also increases the number of places in which cases can be lost due to their inability to be matched.

prosecutor, or the prosecutor may open a case. After a case is formally opened, the prosecutor could decline to prosecute, or may file charges, at which point the case can be dismissed by a judge, fail to reach indictment, or any number of other case outcomes.

The next dyad links the matters series to the U.S. Marshal's arrest records. This link is important because it allows the capture of key demographic characteristics – gender, race, marital status, and citizenship (which is used for exclusion purposes). It is important to note that, as depicted in Figure 2, the U.S. Marshal's arrests follow after the matters stage, about the same time that a U.S. attorney files a formal case. *This is not necessarily true for all crimes*. For example, in the case of supervision violations, an arrest may be made without prior knowledge of the case by the prosecutor. However, in the case of white collar crimes and many other offenses, an arrest may be made *after* the decision to file a case has been made. The order of events depicted here shows the order and process of linking the data from different court actors, as required by the FJSP. Once the Marshal's data were linked to the outgoing matters file, a second link was made between the Executive Office of the U.S. Attorney's Defendants In data set and the outgoing matters file, which is then linked to Terminated Defendants data. The fifth dyad links the charging data from the U.S. Attorney's office to case filings in the U.S. District Courts. Using both datasets available for this particular point in the criminal justice system is necessary because each offers unique variables that the other does not. The AOUSC new filings are then linked to concluded cases from the same agency using yet another dyadic linkfile. The seventh and final link allows

concluded cases to be located in the United States Sentencing Commission individual defendants datafile.

Outcome of Linking Procedure

The initial sample was identified prior to any matches taking place in order to reduce cloudiness of match rates that included cases referred outside the time period of interest or for offenses not related to any of the white collar or comparable crime definitions. Exploratory analyses (not shown) indicated that the degree of success for linking the variables from one stage to the next vary according to the year in which the matter originated. The specific cohort followed (matters referred in fiscal years 2009-2011) was chosen, in large part, because cases from these years had the highest match rates when linking consecutive stages of the criminal justice process. This makes intuitive sense, because cases can take several years to resolve. By closing the cohort to cases opened after this period, it should result in the highest likelihood of cases being present in later stages of the system, thus resulting in higher match rates. This seems to be the case, even at the early stages of the criminal justice process.

As shown in Table 1, there is varying success in matching cases across datasets, though match rates overall are very high. The "Cases" column presents the number of cases eligible to match at the start of the dyad. This number excludes cases which did not match previously, either because they were not eligible (i.e., have a "null" value), or because the dyadic linkfile did not contain a match ("unmatched" cases).

Table 1. Match rates across stages of the FJSP

		Cases[a]	% Matched	% Null	% Unmatched	% Eligible Match
Dyad 1	Matters In Linkfile	92,214	96.19%	-	3.81%	96.19%
	Matters Out	88,697	88.11%	9.7%	2.16%	97.54%
Dyad 2	Matters Out Linkfile	78,149	100.00%	-	0.00%	100.00%
	Arrest	78,149	44.05%	55.01%	0.61%	98.90%
Dyad 3	Matters Out Linkfile	42,986	99.94%	-	0.06%	99.94%
	Defendants In	42,963	80.73%	19.21%	0.00%	99.99%
Dyad 4	Defendants In Linkfile	34,684	95.99%	-	4.01%	95.99%
	Defendants Out	33,293	92.79%	7.07%	0.14%	99.86%
Dyad 5	Defendants In Linkfile	30,893	100%	-	0.00%	100.00%
	AOUSC Cases In	30,893	90.54%	9.20%	0.26%	99.71%
Dyad 6	AOUSC Cases In Linkfile	27,969	99.92%	-	0.08%	99.92%
	AOUSC Cases Out	27,946	98.74%	1.26%	0.00%	100.00%
Dyad 7	AOUSC Cases Out Linkfile	27,594	94.81%	-	5.19%	94.81%
	USSC Cases	26,162	87.78%	2.65%	9.57%	90.17%

[a] Excludes cases not matched previously

The rates reported as "Percent Matched" in Table 2 refer to the percent of the total sample at that stage that matches either to the dyadic linkfile or to the subsequent stage, as indicated. The "Percent Null" column refers again to the total sample at the stage, and reflects the percent of cases that have a "NULL" value for the second part of the dyadic link. Note that matches to the linkfile cannot possibly have a "NULL" value as they would not uniquely identify cases and also the first

link temporally precedes the second stage of a link. The "Percent Unmatched" column reports the percent of cases that did not have a formally indicated "NULL" value but also did not match, relative to the total sample. Because "NULL" cases are guaranteed not to match because these cases have failed to reach the next stage of the criminal justice system, the final column reports the percent of *eligible* cases that matched (i.e., excluding nulls).[32]

In a small percentage of cases at each stage, the linkfile either did not include a crosswalk for a particular case, or a case had an assigned crosswalk identifier but the second stage identifier was out of range (e.g., an arrest was made in 2014 for a matter filed in 2011) or reflected an error in the algorithm; I refer to this type of unmatched case as a "non-match" because the linkfile could or should be able to provide a crosswalk between datasets but does not. Yet by far the most common reason for cases failing to match was that a case had an identifier of "null" – i.e., that the case was present in the first file of a dyad, but not the second. These null values indicate that a case is not found, or has not progressed to the next stage for some reason. This could be that cases are either still pending (if it is between opened and terminated cases extracted from the same agency, for example) or because the case has is not eligible to be found in the second stage (e.g., an acquittal will not be found in sentencing data). For unmatched cases between Matters Out and Arrests, it is also possible that at least some of the unmatched cases are

[32] For some dyads, identifiers did not uniquely identify cases without apparent cause. This suggests either an error in the algorithm, or, for example, that a defendant was arrested multiple times related to a single matter. To avoid clustered errors, the first of these duplicate observations was retained, while the remainder were dropped.

corporate defendants, who would not appear in arrest data. Yet, most of the failures to match were in general very slight otherwise.

A detailed review of match rates and rates of null was undertaken for each link between stages and to the crosswalk file. Fuller descriptive tables are available in Appendix A, though the broad conclusions of this analysis are discussed here. In general, there were only small differences in match rates to both linkfiles and from one stage to the next. For many links, only a handful of cases did not match, and descriptive differences were not considered. However, some patterns did emerge. White collar cases (according to any definition) were often slightly more likely to be unmatched, though the difference in match rates was often within 2%; while sometimes resulting in a large difference compared to the base rate of non-matches, the rates of non-matches for white collar crimes were still small. Cases with codefendants were differentially likely to match to linkfiles, though the magnitidude, significance, and direction of this difference varied by stage. For match rates (i.e., algorithmic error) there was generally no significant difference in the likelihood of matching for cases referred during 2009-2010 compared to 2009-2011.

In contrast, cases from the 2009-2011 cohort were often more likely to have "NULL" values at a given stage; in other words, focusing on an earlier cohort reduced the rate at which cases were not found in secondary stages. This makes intuitive sense, as more cases that are pending or still under investigation will come to conclusion if the longest followup period is increased by a year. This difference was predominantly observed for the links between matters in to matters out, matters

out to cases filed, and stages between case filing and conclusion. There were few observed differences in the rate of nulls according to defendant characteristics, although cases with white defendants and cases with male defendants appear to take longer, as indicated by a higher rate of nulls matching defendants in cases filed to defendants in cases concluded. White collar cases under the agency definition were less likely to have null values matching to the USSC sentencing data, though the strict and loose offense-based definitions of white collar produced slightly higher null rates, though the magnitude of differences is small.

WHITE COLLAR AND COMPARABLE CRIMES

This research includes analyses of process outcomes both within a white collar case sample as well as between white collar cases and non-white collar cases. As such, it is important to be thoughtful about which cases to include in both categories so as to make meaningful comparisons. Below are the strategies for identifying white collar crimes according to different definitions that have been employed previously, as well as the approach most consistent with the definition laid out in Chapter 2 of this dissertation. Following this are the parallel strategies for identifying the relevant comparison group for each definition.

Identification Strategy – White Collar Cases

Earlier in this dissertation, a working definition of white collar crime was developed such that: "*white collar offenses are any violations of (federal) law committed by persons or organizations in the conduct of their legitimate occupational roles or organizational functions*" (Simpson and Yeager 2015). Unfortunately, as with many administrative datasets, the FJSP does not contain a

specific variable to identify occupation-related white collar crimes, or white collar crime in general. However, I argue it is possible to identify many of these offenses by keying in on the means through which cases arrive in the federal prosecutor's office.

Unlike a local prosecutor, U.S. attorneys receive case referrals from hundreds of agencies. Some of these cases are referred by agencies like the U.S. Secret Service, and a large portion are referred by immigration and customs officials. A small but still notable proportion, however, are referred by federal regulatory agencies. Briefly discussed earlier, regulatory agencies are the primary overseers of corporate and other organizational behavior, as well as individuals working within them. Often these agencies have the duty to generate rules and regulations for businesses as well as enforce them. However, most agencies are limited to administrative sanctions for violations; others, like the Securities and Exchange Commission (SEC) and Environmental Protection Agency (EPA), also possess the power to independently pursue civil cases against violators. If agencies prefer to seek criminal charges, they must refer these cases for action by the U.S. Attorneys, though there is not guarantee that the attorneys will ultimately file charges. Because of the role of regulatory agencies in managing business behavior, I argue that by identifying key referring agencies that are engaged in regulatory roles it is possible to isolate a sample of cases consistent with the definition of "white collar" crime stated above. This is consistent with the traditional focus on the work of specific regulatory agencies by white collar scholars, primarily focusing on work by the SEC, FTC, and EPA (see e.g., Shapiro, 1980; Simpson,

1986; Simpson and Schell, 2009). Specific agencies to be included are listed below in Table 1; this specific set of agencies was selected based on their roles in regulating occupation-related behavior. A discussion of the types of cases captured using this definition will be included in the next chapter. From this point on, this strategy of identifying white collar cases will be referred to as the "agency-based definition" of white collar crime.

However, as discussed previously, while there is a long tradition of studying white collar crime and sentencing, scholars have used a variety of means of defining and identifying white collar crimes. As specified in Hypothesis 5, I expect that the definition of white collar used may affect the substantive differences between white collar and other crimes. As such, it is necessary to consider other means of identifying white collar crimes. These strategies coalesce around the idea that white collar crimes can be identified in administrative data not by the source of their referral, but by their statutory offense. I consider three offense-based identification strategies for white collar crime: one consistent with the FBI's definition ("loose offense-based"), one consistent with the Yale White Collar Studies Project ("Yale offense-based"), and another based on fine-grained offense descriptions ("strict offense-based").

The Federal Bureau of Investigation (FBI) considers white collar crime to be a financial crime that is "characterized by deceit, concealment, or violation of trust and are not dependent upon the application or threat of physical force." Included in the FBI's definition are all embezzlements, frauds, forgeries, identity thefts, and money laundering, without regard to the context of these offenses.

Notably excluded from this definition are regulatory offenses, while *included* are welfare frauds, evasion of income tax, and impersonation of another person. I refer to this set of identified cases as "loose offense-based" white collar crime.

The Yale Studies (discussed earlier) attempted to use a more nuanced version of white collar crime. They considered eight federal offenses that were common enough, and consistent (enough) with most scholars' definitions of white collar crime. Their study included securities and exchange violations, antitrust violations, bribery, bank embezzlement, mail fraud, tax fraud, false claims and statements, and credit fraud. The first five of these offenses are arguably "quintessential white collar crimes" (Wheeler et al., 1988: 332) – though none of them are limited to individuals using occupational power, they have all been extensively studied by white collar scholars. The latter three offenses were more controversial. This set of eight offenses includes some offenses that are not included in the FBI's definition, but excludes many more that are. In future sections, I refer to this as the set of "Yale offense-based" white collar crimes.

Finally, I also consider an offense-based identification scheme that is possible only through the uniquely granular detail of the FJSP. It includes some forms of embezzlement, some frauds, bribery, violations of regulatory laws (including fair labor standards, antitrust act, environmental violations, and others). The complete list specific offenses are included below in the Table 1. This "strict offense-based" definition of white collar is in some ways more consistent with this dissertation's working definition of white collar crime than the Yale offense-based

definition (e.g., excluding tax fraud), but is also more exhaustive in its list of offenses. The overlap of these definitions can be found in Table 2.

Table 2. White Collar Crime and Comparison Offenses, by Identification Strategy

	White Collar Crime	*Comparison Crimes*
Agency-Based	Crimes referred by the following agencies: International Trade Administration, Health Care Financing Administration, Food & Drug Administration, Bureau of Health Professions, DHHS Office of the Inspector General, Treasury U.S. Trustees, Bureau of Labor Standards, Mine Safety and Health Administration, Pension & Welfare Benefits Administration, Employment Standards Administration, Commodities and Futures Trading Commission, Consumer Product Safety Commission, National Credit Union Administration, Equal Employment Opportunity Commission, Environmental Protection Agency, Farm Credit Administration, Federal Communications Commission, National Labor Relations Board, Nuclear Regulatory Commission, National Science Foundation, Office of Personnel Management, Pension Benefit Guarantee Corporation, Small Business Administration, Securities and Exchange Commission	Larceny Motor Vehicle Theft Robbery Transactions in Stolen Goods Damage/Destruction of Property Bribery Fraud Embezzlement
Loose Offense-Based	Fraud Embezzlement	Larceny Motor Vehicle Theft

	Forgery Counterfeiting Money laundering Identity theft	Robbery Transactions in Stolen Goods Damage/Destruction of Property False Claims and Statements Bribery
Yale Offense-Based	Securities Fraud Antitrust violations Bank Embezzlement Bribery Mail & Wire Fraud Tax Fraud False Claims and Statements Credit and Lending Fraud	Larceny Motor Vehicle Theft Robbery Transactions in Stolen Goods Damage/Destruction of Property Other Fraud Other Embezzlement
Strict Offense-Based	Embezzlement of public monies and property Embezzlement from credit/lending institution Embezzlement by officers Embezzlement by government officer/employee Credit and Lending Fraud Bank Fraud Marketing and Commodities Fraud Securities Fraud Bribery Agriculture Violations Agriculture Insecticide Violations Antitrust Violations Fair Labor Standard Violations Food and Drug Act violations Energy facility violations Violation of federal statutes for public officers Violation of federal statutes on foreign relations Violation of federal statutes on banking Violation of federal statutes on money and finance Violation of federal statutes on public health and welfare	Larceny Motor Vehicle Theft Robbery Transactions in Stolen Goods Damage/Destruction of Property Other Fraud Other Embezzlement

	Violation of federal statutes on commerce and trade Violation of federal statutes on transportation Communication Acts violations Aircraft regulations	

Identification Strategy – Comparable Cases

In general, the possible pool of comparable cases includes cases eligible to be sentenced under the economic crimes portion of the United States Sentencing Guidelines. Fraud, larceny, and embezzlement are considered "basic economic offenses" and fall under the provisions of Chapter 2, Part B. Fraud and embezzlement are, to varying degrees, included in many definitions of white collar crime, while larceny is often used as a comparable crime in status leniency research; thus this section of the guidelines seems an appropriate frame for comparable offenses. Also subject to these provisions are offenses involving stolen property and damage/destruction of public property. These offenses similarly are nonviolent in nature and are either motivated by financial gain or result in financial loss to the victim. Motor vehicle theft has also been suggested as a comparison crime for studies of white collar leniency (Maddan et al., 2012). In each of these types of crimes, the offenses are presumed to be motivated by the desire for financial gains, without physical harm to another person. [33] For this reason, I also include offenses that involve transactions in stolen goods and property damage or destruction as part of the comparison sample. However, white collar crimes, particularly those

[33] "Joy-riding" may be an exception; unfortunately, it is not possible to distinguish instrumental from non-instrumental motor vehicle thefts in these data,

committed in the course of an occupation, are not always without physical harm; workplace safety violations can maim and adulterated food can kill. For this reason, I also include robbery as a comparable crime; robbery is financially motivated (similar to occupational crimes), and offers the threat of physical harm to the victim, which may or may not be realized. Yet, the inclusion of a violent offense in the "comparable crimes" category may exacerbate extant differences between white collar and traditional crimes, or worse, create the appearance of difference where one would otherwise not be observed. Sensitivity analyses consider the extent to which the findings hold when robbery is not included in the sample of comparable crimes.

The set of cases to which the white collar sample is compared for between-crimes analyses varies depending on the definition of white collar crime used. For some definitions, a case may be 'white collar' crime but for another, it may be excluded and relegated to a comparable crime. For example, fraud for government benefits (e.g., welfare fraud) would be identified as "white collar" under the loose offense-based definition of white collar crime employed by agencies such as the FBI, but would not qualify under the Yale or strict offense-based definitions. Such behavior may be identified as white collar in the agency-based definition if and only the crime was referred for prosecution by one of the agencies listed in Table 1. For this reason, and for the general similarity between specific subcategories of an offense, the identifier of "comparable crime" also changes depending on the definition of white collar crime employed. For the agency-based white collar crimes, the comparison offense group includes all types of frauds, bribery, larceny,

embezzlements, theft of motor vehicles and other transactions in stolen goods and robberies originating from all agencies and other sources not specifically listed as a white collar agency. Because frauds and embezzlements are considered white collar crime according to the loose offense-based definition, these are not part of comparable crimes for that definition. Similarly, some types of fraud and embezzlement are included as "white collar" under both the strict and Yale offense-based definitions, while all other subtypes of those offenses are retained as comparable crimes.

VARIABLES

Because this research is focused around case processing, the traditional delineation of "independent" and "dependent" variables is at times misleading. Intrinsic to the processural approach taken is the assumption (and indeed, hypothesized relationship) that early outcomes influence later ones. Thus, what is a dependent variable in one model subsequently becomes an independent variable. This research considers each of the following outcomes independently as well as how they may inform one another.

Prosecution Outcomes

Charge Reduction – This dummy variable indicates whether a case received a reduction in the potential incarceration category (see above) or a reduction in fine for the most serious charge between case filing and case termination. The variable is coded "1" for all cases in which the potential incarceration *or* potential fine for the most serious charge at filing is greater than for the potential incarceration or fine for the most serious charge at

termination and "0" for all other cases.[34] *[Values calculated using "1st /2nd/ 3rd/ 4th/ 5th Filing Offense Severity Code" in conjunction with the variable "Most Serious Filing Charge" in the AOUSC – Cases Filed data and "1st /2nd/ 3rd/ 4th/ 5th Terminating Offense Severity Code" in conjunction with the variable "Most Serious Terminating Charge" in the AOUSC – Cases Terminated data]*. Note, this variable is likely a conservative measure of charge severity reduction because it is not able to identify cases in which the possible incarceration or fine was decreased but did not result in a change in the ordinal categories.

Count Reduction – This dummy variable indicates whether or not a case-defendant received a reduction in the number of counts s/he faced between filing and termination. The variable is coded "1" for case in which the case-defendant faced fewer counts for all charges at termination than at filing, and "0" for all others. *[Values calculated using "Total Counts" variable in the EOUSA – Defendants In and EOUSA – Defendants Out data]*.

Charges Dropped/Dismissed – Another possible mechanism for prosecutors to influence sentence outcomes is to drop or dismiss certain charges. This is a dummy variable coded "1" for all cases in which any of the five most serious charges were indicated as dismissed (without prejudice), nolle prosequi, or withdrawn, but *not* all charges and not

[34] Potential incarceration at charging and conclusion is indicated eleven ordinal categories (none; less than 6 months; 7 months – 1 year; 1 year and 1 day – 2 years; 2 years and one day – 3 years and 364 days; 4 years – 5 years and 364 days; 6 years – 10 years and 364 days; 11 years – 5 years and 364 days; 16 years – 20 years and 364 days; 21 years – 25 years and 364 days; more than 25 years, including life and death penalty cases). Potential fines are indicated by ten categories (none, $1-100, $101-500, $501-1,000, $1,001-5,000, $5,001-10,000, $10,001-20,000, $20,001-50,000, $50,001-99,999, $100,000 or more).

including charges that were dismissed with prejudice. [*This is determined using the "1st /2nd/ 3rd/ 4th/ 5th Disposition" in the AOUSC – Cases Terminated data]*.

Sentencing Outcomes

Discretionary Incarceration – This is a dichotomous variable that is coded "1" if a case was eligible to receive a non-prison sentence (i.e., in Zone A or B – with some conditions – in the guidelines; see Analytic Strategy) and received any term of incarceration as part of the sentence. Cases eligible to receive a non-prison sentence that did not receive any incarceration are coded "0"; those case not eligible to receive a non-prison sentence are coded missing. *[This is determined using the "Type of Sentence Imposed", "Total Months in Prison Ordered" and "Prison Sentence for Defendants Eligible to Receive a Nonprison Sentence" variable in the USSC – Defendants Sentenced data]*.

Judicial Departure – This is a binary indicator variable coded "1" if a case received a sentence below the range suggested by the federal sentencing guidelines through a departure or variance and "0" if the case received a sentence either within the range or above the range. Cases receiving a government-sponsored departure are coded as missing. *[This is determined using the "Collapsed Post-Booker Reporting Category – 5 category" variable in the USSC – Defendants Sentenced data]*.

Prosecutor-Sponsored Departure – This is a binary indicator variable coded "1" if a case received a government-sponsored departure (also known

as a substantial assistance or 5K departure) and "0" if the case received a sentence either within the range or above the range. Cases receiving a sentence less than prescribed but not through a government motion are coded as missing. *[This is determined using the "Collapsed Post-Booker Reporting Category – 5 category" variable in the USSC – Defendants Sentenced data]*.

Any Departure – This is a dichotomous variable that is coded "1" if the case received a sentenced less than prescribed by the guidelines through *either* a judicial departure or variance or a government-sponsored substantial assistance departure.

Independent Variables

White Collar Crime – White collar crime is defined in multiple ways, as discussed above. Each of these definitions are operationalized as dummy indicators where "1" indicates that a case meets a particular definition of white collar crime and "0" if it does not. Cases are identified as white collar at the point of referral for prosecution. Cases referred by any of the following agencies are "white collar" according to the agency based definition: International Trade Administration, Health Care Financing Administration, Food & Drug Administration, Bureau of Health Professions, DHHS Office of the Inspector General, Treasury U.S. Trustees, Bureau of Labor Standards, Mine Safety and Health Administration, Pension & Welfare Benefits Administration, Employment Standards Administration, Commodities and Futures Trading Commission, Consumer

Product Safety Commission, National Credit Union Administration, Equal Employment Opportunity Commission, Environmental Protection Agency, Farm Credit Administration, Federal Communications Commission, National Labor Relations Board, Nuclear Regulatory Commission, National Science Foundation, Office of Personnel Management, Pension Benefit Guarantee Corporation, Small Business Administration, and the Securities and Exchange Commission. All fraudulent property offenses meet the definition of white collar for the loose offense-based definition. The Yale definition includes eight specific offenses (securities fraud, antitrust violations, bank embezzlement, bribery, mail and wire fraud, tax fraud, false claims and statements, credit and lending fraud), while the strict offense definition of white collar crime includes the following offenses: embezzlement of public monies and property, embezzlement from credit/lending institution, embezzlement by officers, embezzlement by government officer/employee, credit and lending fraud, bank fraud, marketing and commodities fraud, securities fraud, bribery, agriculture violations, antitrust violations, fair labor standard violations, Food and Drug Act violations, energy facility violations, violation of federal statutes for public officers, violation of federal statutes on foreign relations, violation of federal statutes on banking, violation of federal statutes on money and finance, violation of federal statutes on public health and welfare, violation of federal statutes on commerce and trade, violation of federal statutes on transportation, Communication Acts violations, aircraft regulations.*[The*

follow variables from the EOUSA – Matters In data were used to generate these indicators: "Referring Agency" (agency), "BJS Secondary Offense" (loose), and "AOUSC Offense Category" (Yale and strict)]

Any/Number of Codefendants – Following prior literature, one indicator of case complexity is the number of codefendants. This is indicated by two variables: the number of codefendants and a dummy variable indicating any codefendants *[These are calculated using the Case ID and Defendant ID variables in the EOUSA – Matters In File]*.

Other Variables

Maximum Incarceration – The maximum incarceration for the most serious charge is indicated by eleven ordinal categories of varying width: none; less than 6 months; 7 months – 1 year; 1 year and 1 day – 2 years; 2 years and one day – 3 years and 364 days; 4 years – 5 years and 364 days; 6 years – 10 years and 364 days; 11 years – 5 years and 364 days; 16 years – 20 years and 364 days; 21 years – 25 years and 364 days; more than 25 years, including life and death penalty cases) *[This is determined using the values in "1st /2nd/ 3rd/ 4th/ 5th Filing Offense Severity Code" in conjunction with the variable "Most Serious Filing Charge" in the AOUSC – Cases Filed data]*.

Maximum Fine – Potential fine severity for the most serious charge is characterized by ten categories (no fine associated; $1-100; $101-500; $501-1,000; $1,001-5,000; $5,001-10,000; $10,001-20,000; $20,001-50,000; $50,001-99,999 ; $100,000 or more). *[This is determined using the*

values in "1st /2nd/ 3rd/ 4th/ 5th Filing Offense Severity Code" in conjunction with the variable "Most Serious Filing Charge" in the AOUSC – Cases Filed data].

Number of Charges: The AOUSC data contain information on up to five most serious charges. The number of charges variable indicates how many unique charges a defendant faces, up to five. *[This is determined using the values in "1st /2nd/ 3rd/ 4th/ 5th Filing Offense"]*.

Plea – A case is considered to have resolved in a plea if any of the charges were resolved via a plea, regardless of the conditions of that plea. *[This is indicated from the values in "1st /2nd / 3rd / 4th / 5th Disposition" in the AOUSC – Cases Terminated data]*

Race – A series of dummy variables is used to capture race: white, black, other or unknown race. While there is information on Asian and Native American defendants' race, they make up less than 6% of the sample and are thus grouped together. Unfortunately, there is no way to distinguish Hispanics using the FJSP sample.[35] *[This is drawn by the values in "Race of Arrestee" in the USMS – Arrests and Bookings file]*

Male – A dummy variable indicating defendant sex is coded "1" if the case-defendant is male and "0" if otherwise. *[This is drawn from the values in "Sex of Arrestee" in the USMS – Arrests and Bookings file]*

[35] Hispanic origin is available for many convicted cases using the USSC data; Hispanic will be included as a separate (non-mutually-exclusive) variable as a sensitivity analysis for sentenced cases.

Age – Age is indicated by defendant's age at arrest *[Using values in "Age of Arrestee" in the USMS – Arrests and Bookings file]*

Type of Counsel – To capture the potential advantages of private counsel, a dummy variable is coded "1" if the defendant retained private counsel. *[This is indicated by the values in "Type Counsel Code" in the AOUSC – Charges Filed data*]

Marital Status – This variable is coded "1" if the defendant is married and "0" otherwise. *[This is drawn by the values in "Marital Status of Arrestee" in the USMS – Arrests and Bookings file]*

Violent Crime – This variable is coded "1" if the referring offenses is a violent crime and "0" otherwise. *[This is determined using the variable "BJS Primary Offense Categories" in the EOUSA – Matters In File]*.

Drug Crime – This variable is coded "1" if the referring offenses is a drug crime and "0" otherwise. *[This is determined using the variable "BJS Primary Offense Categories" in the EOUSA – Matters In File]*.

Regulatory Offense - This variable is coded "1" if the referring offenses is a regulatory offense and "0" otherwise. *[This is determined using the variable "BJS Secondary Filing Offense Categories" in the EOUSA – Matters In File]*.

Misdemeanor/Petty – This variable is coded "1" if the filing offenses contain a misdemeanor or petty offense and "0" if the filing offenses contain only felony. [*This is determined using the "1st /2nd/ 3rd/ 4th/ 5th Offense Level" in the AOUSC – Cases Filed data]*.

Number of Counts – This variable reflects the total number of counts across all charges at filing. *[Values calculated using "Total Counts" variable in the EOUSA – Defendants In data]*.

Prior Record[36] – This variable is the criminal history category a defendant was determined to have based on calculations specified in the guidelines. Offenders are given points for several factors including the number and length of prior sentenced for adults and juveniles, the defendant's status as under or not under criminal justice supervision at the time of the offense, and the relative recency of previous offenses. *[This is indicated by the values in "XCRHISSR" in the USSC – Defendants Sentenced data]*

Education[32] – Education is captured as a series of dummy variables: less than high school education, high school graduate or G.E.D, any college, any vocational school, and other *[This is indicated by the values in "Education Level Completed" in the USSC – Defendants Sentenced data]*

Dependents[32] – Two measures of dependents are considered; the first is a dummy variable indicating any dependents. The second indicates the number of dependents; the number is topcoded at 7; less than .5% of the sample report a number of dependents in excess of this. *[This is indicated by the values in "Number of Dependents" in the USSC – Defendants Sentenced data]*

[36] These variables are only included for sentencing outcomes, as they are only available in the USSC data (convicted cases).

Pre-Sentence Detention Status[32, 37] – This dummy variable is coded "1" for defendants who were in custody during their trial, and "0" for those who were either out on bail or bond or released on their own recognizance. *[This is captured by the variable "Pre-Sentence Detention Status" in the USSC – Defendants Sentenced data]*

Acceptance of Responsibility[32] – While technically taken into account in guidelines final scores, and thus influences sentences, it is possible that this operates as a heuristic for judges. This is captured as an ordinal (0-4) point variable with higher scores indicating greater acceptance. *[This is captured by the variable "Acceptance of Responsibility" in the USSC – Defendants Sentenced data]*

Guidelines Offense Seriousness Category[32] – The offense severity score from the completed guidelines calculation. This acts as an overall indicator of offense severity. *[This is captured by the variable "XFOLSOR" in the USSC – Defendants Sentenced data]*

Guidelines Minimum Sentence – The minimum term of incarceration suggested by the federal sentencing guidelines, in months. *[This variable is determined using the variable "Adjusted Guidelines Range Minimum" in the USSC – Defendants Sentenced Data]*

Amendment Year[52] – A series of dummy variables are used to account for the specific set of sentencing guidelines used to determine a defendant's

[37] The FJSP EOUSA data does include variables for bond type and bond amount. However, these fields are not well populated (~15% of the prosecution sample) and thus are not reliable indicators for prosecution analyses.

sentence. More than 99% of cases are sentenced under guidelines between 2007-2011, though of those that are not included in that range, the guidelines year ranges from 1986-2006. For both practical and substantive reasons,[38] these amendment year dummy variables are as follows: 1986-2002, 2003-2006, 2007, 2008, 2009, 2010, and 2011.

ANALYTIC STRATEGY

The first goal of this research is to describe the processing of white collar cases through the federal criminal justice system. Using the FJSP, it is possible to track these cases from referral for prosecution forward, through several points at which cases may or may not advance in the system. This research objective will be completed through the use of descriptive statistics tracing the funneling of white collar cases of various types through the criminal justice process, paying particular attention to reasons provided for case disposition.

The remainder of the research objectives and tests of hypotheses laid out in the previous chapter require the use of multivariate models. Unfortunately, due to the relative novelty of multistage processing studies, there has yet to emerge a disciplinary standard "best practice" for capturing the independent and cumulative effects of particular variables at multiple stages in the criminal justice process. Below, I address several strategies suggested by the literature, including their corresponding strengths and weaknesses for the problem and data at hand.

[38] In addition to a very small number of cases, precluding individual year dummy variables, there were significant changes to the penalties for basic economic offenses that went into place during FY 2003 as part of the Sarbanes-Oxley Act.

Both Hagan (1975) and Wooldrege (2015) employ a structural equation model (SEM) to model the effect of defendant and case characteristics on early and later decision points. This approach seems appealing for its ability to capture multiple time points in the same model while reflecting independent and mediated effects. However, their work has been criticized for their reliance on convicted cases. This is not necessarily a problem for the present problem, as not only does this work also rely on a sample of convicted cases (which is necessary to predict bargaining as well as sentencing), and this dissertation is not attempting to predict sentence length[39]. However, the data structure nevertheless is such that cases are lost between the modeling of prosecution and sentencing outcomes due to failures to match between the last two stages. Using SEM requires complete data at each time point, and would result in the loss of cases for the analysis of prosecution outcomes. Thus, SEM appears to be a less-than-ideal solution to the problem of process modeling. Kutateldze et al. (2014) proceed in a fashion more traditional within sentencing research by employing logistic regressions for each outcome across stages. When necessary, they control for prior decision outcomes (e.g., for bail decisions predicting sentencing). To generate estimates of cumulative impacts, the authors calculate conditional probabilities of particular outcomes given case characteristics. This simple yet demonstrative tool may be the emergent method of process studies. The present analyses follow Kutateladze et al. (2014) by running

[39] SEM is most problematic for sentencing studies that predict sentence length, as they either (a) result in the loss of cases that are "missing" for sentence length (including both those who are missing and those who are ineligible because they were not incarcerated), or (b) model sentence lengths of "0" for those not sentenced to prison, generally not taking into account a zero-inflated or hurdle model to account for selection into sentence length.

separate independent regressions across the stages of prosecution and sentencing. While this is not a perfect model for process and does not model outcomes simultaneously nor produce estimates of direct and indirect effect estimates (as would SEM), the importance of retaining the variation of unmatched cases is preferred to the loss of these cases. This method is not able to produce estimates of the effects of characteristics *across* selected stages, yielding only conditional estimates; in other words, this strategy produces estimates of the effect of a given characteristic, *given that a case has advanced to this stage*.

Modeling Prosecution and Sentencing Outcomes

All models of prosecutorial and sentencing outcomes are analyzed using fixed effects for districts. Fixed effect regressions account for persistent within-individual (i.e., within-district) variation in the dependent variable, also known as persistent heterogeneity. Between-district variation is not predicted by the model. The extensive evidence for differences in sentencing and bargaining practices both at the state and federal level support the use of fixed effects. In practical terms, fixed effect models allow for variation in the dependent variables, the probability of negotiated pleas, discretionary prison sentences, and departures across district; this model is justified if districts vary in these outcomes. Evidence from multilevel models and prior qualitative work as well as theoretical models of courtroom workgroups suggest that this is likely. Some work has suggested that the fixed effects estimator is problematic in nonlinear models, both because of its cumbersomeness and because of the "incidental parameters problem"; the consequence of this is that coefficients may be biased away from zero at the same

time that standard errors are biased toward zero, inflating the odds of a significant result (see e.g., Greene 2002; Lancaster, 2000). Because of the large number of districts, this is likely a problem for these data. In spite of the potential bias, however, Greene concludes that, "this does not imply… that one should simply ignore the issue and fit the model with one constant" (2002: 15-16). However, it is possible to *condition* the fixed effects (across-district variation) out of the likelihood function, leaving only within-district variation (Allison, 2009); the fixed effects logit model in Stata (*xtlogit, fe*) is equivalent in outcomes to conditional logit (*clogit*; Williams, 2017), which does not suffer from the same bias in parameters as a more traditional fixed effects logit (Greene, 2002).[40]

An alternative to the fixed effects model is a random effects model, which also allows the intercept of the model to vary according to the grouping variable (district), but also assumes the between-district variation is a random variable (i.e., normally distributed), and (more importantly) that this random variation is uncorrelated with any of the independent variables (Allison, 2009; Greene, 2002). However, this is a strong assumption for most research. For example, the relative prevalence of white-collar cases is known to affect sentencing outcomes in those districts; i.e., between-district variation in white collar punishment (the fixed effects) are likely related to the value of "white collar" in that district (an independent variable; Hagan and Bernstein, 1979). Thus, it is likely that the assumptions of a random effects model are violated, and a fixed effects model,

[40] Another problem with fixed effects estimators is that they exclude groups (districts) that lack variation in any of the dependent or independent variables. However, because the districts are generally comprised of a large number of cases, this rarely affects the models in this dissertation.

which is less efficient but still consistent, is preferred. Random effect models are included in the Appendix (A4) but are not discussed in the main body of the document for these reasons. It is worth noting that fixed effects models imply that there is between-district differences, but only provide results for within-district effects; this means that these models do not explain *why* some districts sentence differently than others. Future work can and should unpack the ways in which the effects of certain case characteristics vary across districts using multilevel models.

The prosecution outcomes considered here are binary indicators of the decision to engage in negotiated pleas resulting in charge reduction, count reduction, or dropped charges. Based on descriptive analyses (Chapter 4), charge and count reduction have prevalence rates of 6.6-8.5% and 15.1-17.3%, respectively, across the different definitional samples; dismissal of charges is the rarest of the three outcomes, with 5.2-7.7% of the definitional samples. Both logit and probit regression are viable options for a binary limited dependent variable. The primary difference between the two functions is in the shape of the curve; the logistic cumulative density function has slightly more area in the upper and lower tails of the distribution. Due to the need for conditional fixed effects to account for district variation, logistic regression is preferred; the probit model does not allow for conditional fixed effects, subjecting estimates to error (Allison, 2009).

Traditionally, sentencing research has often focused on prison as the primary sentencing outcomes of interest. In the federal guidelines, there are four possible sanctions: prison, probation, fines, and alternative confinement. Fines are required to be imposed under the guidelines, except where the defendant is unable

to pay and unlikely to be able to pay in the future.[41] Fines are also secondary to restitution payments (United States Sentencing Commission 2016: 451), which are themselves contingent on ability to pay. Together, this suggests that fines are more likely to be a proxy for socioeconomic status than to represent a punitive outcome and as a consequence, would almost certainly reflect status liability if predicted as a dependent variable. In other words, defendants who can afford to pay both fine and restitution are anomalies (94% of the sample receives a waiver for inability to pay fines). This creates a situation in which fines given are either zero or very large, with only the very rich able to pay, and thus given the largest fines. This is consistent with findings from Schanzenbach and Yeager (2006), who find that the amount of restitution has a positive relationship with incarceration length. As a result, this dissertation does not consider fines as a primary sentencing outcome. However, total economic penalties (restitution, cost of supervision and criminal fines) are included in sentencing models to account for potential influence on other sentencing outcomes.

With the exclusion of financial punishments, this leaves five possible "packages" of sanction components: probation only, probation with alternatives, alternative confinement only, prison with alternative confinement, and prison only. It is tempting to model these packages using ordinal or multinomial regression. However, while this endeavor would certainly prove interesting conceptually, the

[41] Unfortunately, there is no way to determine, given the federal sentencing data available, the degree to which fines are imposed exclusively based on ability to pay, versus judicial discretion. Arguably, the imposition of a fine reflects a combination of both. However, it is highly unlikely that this decision is entirely or even predominantly discretionary, and thus its use as an outcome to determine leniency is not advisable.

point of this dissertation is to consider the effect of white collar status and prosecution on sentence *leniency*. What is critical, then, is a model that clearly illustrates leniency. Of the three possible sanction components and the five resultant possible packages, what, then, is leniency? Probation, certainly, is more lenient than prison; prison results in number of negative consequences for the individual, both during incarceration and lasting into release. Alternative confinements then, too, are also likely to be less serious than a prison term. But the answers are less clear when it comes to ranking the severity of probation versus alternative sanctions (especially since they are not mutually exclusive categories), or the packages to one another. Because of the variation in alternative confinements available to judges and the special conditions potentially applied to probation, it is difficult to uniformly say that alternatives are more punitive than probation, or that prison alone is more serious than a prison sentence that is followed by a period of alternative confinement. In addition, conflating cases that received combinations of prison and alternative confinement with cases who received probation and alternative confinement would muddle the meaning of the alternative sanction category, potentially creating a set of "like" cases that are in fact distinct. Finally, the question of unequal length poses an issue – is a long term of probation less severe than an offender who receives a month of incarceration? While simplistic sanction categories cannot illuminate these differences (problematically), neither can one day in prison reliably be equated to a certain number of days of probation to ordinally rank these cases. As a result, it makes most sense for the question at

hand to focus (white collar leniency) on the decision to impose a term of incarceration, rather than focusing on the imposition of other sanction types. [42]

There is a great deal of variation in modeling strategy related to incarceration (both in/out and length) in sentencing literature – from two-stage Heckman corrections involving probit and ordinary least squares regression to Tobit to zero-inflated negative binomial regressions to hurdle models. Each varies slightly in how a "0" (a sentence of 0 months) is conceptually created; some argue that there are two processes at play (*structural* zeroes – defendants who will never be incarcerated – and *discretionary* zeros – defendants that could be incarcerated but are not), while others argue that structural and discretionary zeros are one in the same. The absence of a reliable exclusion restriction in criminal sentencing research (see Bushway et al., 2007) seem to imply, at the very least, that the factors related to the incarceration decision and the factors related to incarceration length are highly similar, calling into question the two-stage decision model. Yet none of these methods fully account for the potentially differential processes influencing which cases that are sent to prison. In the federal system, all cases are eligible to receive prison, thereby eliminating structural zeroes. However, not all cases are eligible to *avoid* prison. Cases sentenced in the upper two zones of the guidelines table (Zone

[42] Another possibility, creating a sum of sanction types variable, will not be discussed because it conflates combinations that are substantively different. For example, a case that received prison and probation would, using a summative measure, appear the same as a case receiving fine and restitution. For this reason, a summative measure is not appropriate to study sentence packages. Relatedly, there has been some attempt to use sentence severity scales that create ratio interpretations of different types and amounts of sentence types. However, due to the variation in perceived seriousness and exchange rates across persons, this is not a strategy pursued here (see e.g., Flory et al., 2006; May and Wood, 2010; Moore et al. 2008).

A and B)[43] are eligible to receive prison, but may also be sentenced to community corrections or probation. For this subset, the imposition of a period of incarceration, or a non-prison sentence, is purely discretionary, making this an ideal decision to study whether some cases are treated differently than others. Focusing on these less serious offenses is also warranted given findings that extralegal disparities are often most problematic for minor offenses (e.g., Spohn and Cederblom 1991). The in-out decision is also arguably more relevant to questions of potential white collar leniency. Incarceration is almost presumed in the federal system, with most offenders receiving at least some term. The avoidance of a term of incarceration thus reflects leniency in that it is a departure from the norm. Additionally, previous work on white collar sentencing and judicial rationale highlight the in-out decision as particularly consequential and the primary focus for judges who may treat white collar offenders differently (see e.g., Benson and Cullen, 1988; Wheeler et al., 1988) .

Another way in which cases might receive leniency is through the receipt of a sentence less severe than prescribed by the guidelines.[44] Less serious sanctions can come in the form of a judicial downward departure, a judicial downward variance, or a prosecutor motioning for a "5K" Substantial Assistance Departure. Previous work has demonstrated that there is significant variation in the use of 5Ks across districts and prosecutors (e.g., Hagan and Bernstein 1979; Spohn and

[43] Offenders in zone B are eligible to receive a non-prison sentence if the court imposes probation with conditions of any of the following alone or in combination: intermittent confinement, community confinement, home detention (United States Sentencing Commission 2015).
[44] Very few cases receive sentences higher than prescribed than the guidelines in this sample, and are thus not focally considered here.

Fornango 2009), suggesting that this is in fact a discretionary outcome. In this dissertation, judicial variances and departures are treated as the same and modeled as "judicial departures", whereas 5K motions are referred to as "prosecutorial departures". Each is considered independently, as well as in a combined "any departure" variable. Unfortunately, it is not possible to use a multinomial logit with fixed effects for district; to preserve the fixed effects option, these outcomes are modeled using logistic regression. Arguably this is not a perfect reflection of reality, as departure options are conditional on one another, rather than independent.[45] Supplemental analyses consider a multinomial model and are available in the Appendix (A7).

Testing Hypotheses

Hypotheses 1-3 rely on a white collar only sample, and the related analyses are referred to as "within" white collar. Hypothesis 1 predicts leniency for more complex cases (i.e., those with codefendants). This hypothesis will be fully supported if the presence and number of codefendants are positively associated with charge/count reductions and having charges dropped or dismissed by the prosecution. The hypothesis will be partially supported if only some of these coefficients are significant, and unsupported if all of the coefficients are either not significant or are in the wrong direction. The models are considered both for each subsample of white collar crimes, as well as an aggregate white collar sample containing all cases meeting any of the four definitions.

[45] Recall (as discussed above) that judicial departures are coded as missing for analysis of prosecutorial departures and vice versa, preserving the reference to receiving no departure.

Hypothesis 2 requires a test of mediation. This hypothesis predicts that prosecutors have an indirect effect on sentencing outcomes via negotiated guidelines offense scores. This relationship is described in Figure 3.

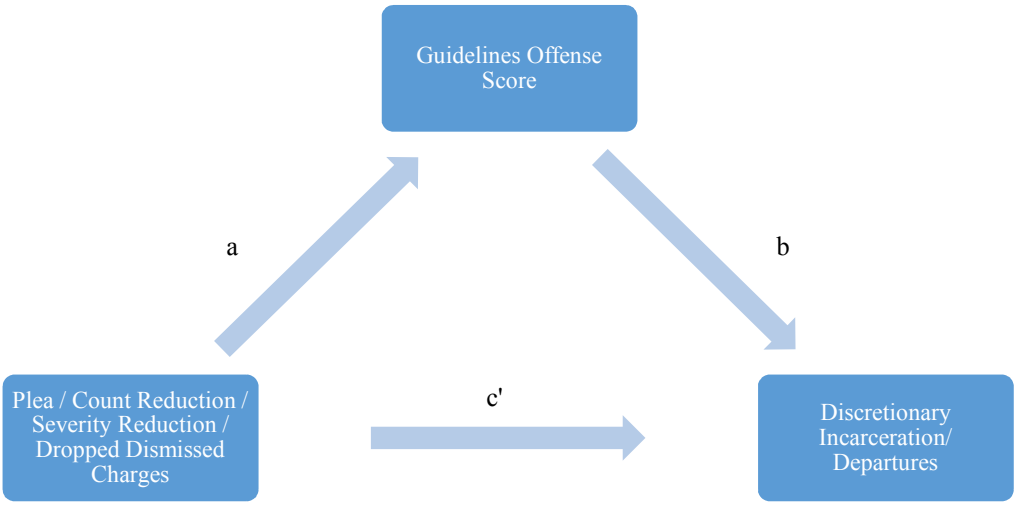


Figure 3. Relationship Between Prosecution, Guidelines, and Sentencing Outcomes

To determine whether prosecutors have indirect, i.e., mediated effects, on sentencing outcomes via guidelines offense scores, I use the rescaling method proposed by MacKinnon and Dwyer (1993) (see also description by Kenny, 2013). This method accounts for the fact that each logistic regression has a different scale for coefficients due to changes in residual variance for the latent variable, Y*. First, I estimate the following regressions, where X is the prosecutorial outcomes of interest, M is the guidelines offense score predicted using a linear regression, Y' is the binary outcome of interest modeled without the mediator with coefficients rescaled to account for residual variance, and Y'' is the binary outcome of interest *including the mediator variable* and rescaled to account for residual variance:

$$(1)\ Y' = c_1X_1 + c_2X_2 + c_3X_3 + c_4X_4 + \alpha_1 + \varepsilon_1$$

$$(2) M = a_1X_1 + a_2X_2 + a_3X_3 + a_4X_4 + \alpha_2 + \varepsilon_2$$

$$(3)\ Y'' = c'_1X_1 + c'_2X_2 + c'_3X_3 + c'_4X_4 + bM + \alpha_3 + \varepsilon_3$$

The rescaling of the coefficients across Equations 1 and 3, is accomplished multiplying each coefficient by the ratio of its standard deviation to the standard deviation of the outcome variable (Y' or Y''). Equations 4 and 5 provide the standardization coefficients for Equations 1 and 3, respectively, where Y'* and Y''* are the predicted scores. The coefficients in Equation 2 do not require standardization as it is not a binary outcome.

$$(4)\ \frac{\sqrt{\frac{\sum(x_i - \bar{x})}{n-1}}}{\sqrt{\frac{\sum(Y'^*_i - \bar{Y}'^*)}{n-1}} + \frac{\pi^2}{3}}$$

$$(5)\ \frac{\sqrt{\frac{\sum(x_i - \bar{x})}{n-1}}}{\sqrt{\frac{\sum(Y''^*_i - \bar{Y}''^*)}{n-1}} + \frac{\pi^2}{3}}$$

The direct (c) and indirect effects (c') of a given X on Y'' is determined using Equation 6:

$$(6) c'\left(\frac{\sqrt{\frac{\sum(x_i-\bar{x})}{n-1}}}{\sqrt{\frac{\sum\left(Y''^*_i-\bar{Y}''^*\right)}{n-1}+\frac{\pi^2}{3}}}\right) + \left[a * b\left(\frac{\sqrt{\frac{\sum(x_i-\bar{x})}{n-1}}}{\sqrt{\frac{\sum\left(M'^*_i-\overline{M'}^*\right)}{n-1}+\frac{\pi^2}{3}}}\right)\right]$$

$$\cong c\left(\frac{\sqrt{\frac{\sum(x_i-\bar{x})}{n-1}}}{\sqrt{\frac{\sum(Y'^*_i-\bar{Y}'^*)}{n-1}+\frac{\pi^2}{3}}}\right)$$

If Equation 6 holds true, then an indirect effect, or mediation, is likely. When c' is nonzero, the ratio of the standardized values of ab to c can also be thought of as the proportion of the effect that is explained by the mediator. When this value is less

than one, there is partial mediation, while values close to or above one suggest complete mediation. This ratio is only calculated when there is a significant relationship between X and Y in the reduced model (Equation 1, above).

Hypothesis 2 will be supported if there is a significant relationship between prosecution and sentencing outcomes and there is a nonneglibile portion of the relationship explained by the guidelines offense score. In contrast, Hypothesis 3 will be supported if the prosecutorial outcomes retain their statistical significance both before and after the addition of offense severity measures; in this case, prosecutorial decisions might be said to act as a signal to judges about how to treat cases. It is possible that there is only partial mediation of the effects of prosecutorial outcomes, (i.e., Hypotheses 2 and 3 are both supported), suggesting that prosecutorial decisions act as both structure and signal. [46]

Hypotheses 4-6 test the question of white collar leniency and liability and rely on samples that include both white collar and comparison crimes. Hypothesis 4 suggests that white collar crimes will be treated differently at sentencing than comparable crimes. For each of the four samples, this Hypothesis will be supported if there is a significant coefficient in either direction for the white collar variable. Relatedly, Hypothesis 5 predicts that the substantive conclusions drawn about white collar leniency and liability will be varied across the four definitions used.

[46] An alternative method to this type of rescaling has been proposed by Karson et al. (2012), and is known as the "KHB Method". The KHB test of mediation relies on calculating a regression of the mediator (i.e., guidelines offense score) on the independent variable (prosecution outcomes) and using the residuals of that linear regression as an independent variable in the regression similar to Equation 1 (above). In doing so, they do not need rescale the coefficients across models and are able to calculate the difference using a common scale (rather than scaling each coefficient according to its model). This method has been shown to produce reliable estimates of mediation (Karlson and Holm, 2011). Because there is no consensus in the field about the appropriate method, results for a KHB analysis are included in the appendix.

Specifically, Hypothesis 5 predicts that more restrictive definitions, that are more closely attuned to Sutherland's notions of power and occupational involvement, will be more likely to produce findings of leniency than others. This Hypothesis will be supported if the agency or strict definition of white collar crime produces findings of leniency but the loose and Yale definitions produce findings of no effect for white collar crime, or produce findings of liability. Finally, Hypothesis 6 suggests that there should be an additional benefit for prosecutorial bargains on sentencing for white collar cases due to the reliance of prosecutors on early cooperation from white collar defendants; this requires the addition of an interaction term representing cases that are both white collar and received the benefit of charge/count reductions and charge dismissals. If these interaction terms are significant, it will offer support for Hypothesis 6.

CONCLUSION

This dissertation uses the Federal Justice Statistics Program data to track a cohort of white collar and comparable crimes referred 2009-2013 up to defendant sentencing through 2013. In general, linking the data resulted in high match rates, especially after taking into account cases that were not eligible to match due to a variety of reasons (including still pending, declined, etc.). The rich data from the FJSP allow both descriptive and explanatory analysis for multiple decision points, from case referral to sentenced cases; this dissertation focuses on reductions in charge severity, reductions in the number of counts, whether charges are dropped or dismissed, and sentence leniency in the forms of avoiding discretionary prison sentences and receiving downward departures from either a judge or a prosecutor.

Testing the hypotheses derived in Chapter 2 will be accomplished primarily through fixed effect logistic regressions, supplemented by mediation analyses where appropriate. Chapter 5 provides a descriptive look at case processing in the FJSP while Chapters 6 and 7 present results of the state hypotheses as divided by prosecution analysis and sentencing, respectively.

CH 4. DESCRIPTIVES

INTRODUCTION

In this chapter, I discuss what 'white collar crime' looks like according to the four definitions laid out in the previous chapters, specifically focusing on the commonalities and differences in the types of cases included under each definition. In addition, I provide descriptive statistics for my key outcomes of interest across prosecution and sentencing outcomes, as well as for independent variables, and controls.

WHITE COLLAR CRIME IN THE FJSP

Across the four definitions of white collar crime and the four comparison groups, there are 92,214 cases eligible for prosecutions that were referred between 2009 and 2011. The percentage of the total sample of referred matters that is identified as either white collar crime or a comparable crime according to the different identification strategies and definitions. Table 3, below, describes the relative proportion of the total sample made up by each definitional subsample (i.e., white collar and comparison crimes according to each definition, as indicated in Table 1). Depending on the definition employed, white collar crime makes up between approximately 7% to nearly two-thirds of the different analysis samples. Yet, none of the definitional samples encompasses the full 92,214 cases; this reinforces the notion that employing different definitions of white collar crime (and the requisite comparison crimes for a test of leniency) produce distinctly different samples. In other words, different definitions of white collar crime are not a simple

matter of coding some cases as 'white collar' under one definition and 'not white collar' under another.

Table 3. Percent White Collar and Comparison Crimes of Total Sample, by Definition/Identification Strategy

N=92,214	White Collar	Comparison	Total
Agency Sample	7.38	84.02	91.40
Loose Sample	65.58	25.65	91.22
Yale Sample	36.84	53.72	90.56
Strict Sample	19.07	69.19	88.26

Not only are the samples not necessarily fully inclusive of one another, the definitions of white collar are also not nested within each other. Table 4 demonstrates this fact that there is a notable difference in which cases are counted as 'white collar' under each definition. Less than one-eighth of cases identified as 'white collar' according to an agency based definition are also identified as white collar crime according to a strict offense-based definition, while more than a third of these same cases qualify as white collar crime under the loose offense-based definition. Even where definitions seem to share similar degrees of overlap (e.g., the strict and Yale offense-based definitions each share approximately 15,000-17,000 cases with the loose offense-based definition), the notion that many of these common cases might also be common to each other (i.e., that the Yale and strict offense-based definitions should share a large number of cases), in fact they share only about 2,000 cases.

Table 4. Overlap of Cases Across Definitions of White Collar Crime

	Loose (N=60,470)	Yale (N=33,975)	Strict (N=17,583)
Agency (N=6,806)	2,332	903	835
Loose	-	16,274	15,505
Yale	-	-	2,043

Table 0.5. Broad Offense Categories

	N	Pct
Violent Offenses	5,281	5.73
Fraudulent Property Offenses	73,882	80.12
Other Property	4,163	4.51
Drug Offenses	612	0.66
Regulatory Public Order	2,449	2.66
Other Public Order	5,506	5.97
Weapon	101	0.11
Immigration	59	0.06
Unknown	1	0.00
Other	160	0.17
Total	92,214	100.00

Table 5 describes the broad categories of the offenses in the sample; the combined white collar and comparison crimes are predominantly identified as fraudulent property offenses (more than 80%), which is not surprising given the consistent presence of economic offenses in definitions of white collar crime. Non-regulatory public order offenses make up the next largest portion of the sample (approximately 6%), with violent, non-fraudulent property, and regulatory public order offenses making up about 13% all inclusive. Drug offenses are also present in the sample, though are a very small proportion of all cases. However, there is likely variation in how these offense types are distributed across the different definitions of white collar crime.

What is White Collar Crime under an Agency-Based Definition?

The use of an agency-based definition, opposed to more traditional offense-based definitions for administrative data, potentially introduces variation in the types of offenses identified as "white collar crime." Thus, it is particularly important to unpack the characteristics of these cases for this definition; this

definition might also be expected to contain cases most dissimilar to other definitions.

Table 6. Broad Offense Categories, Agency Definition White Collar Crime

	N	Pct
Violent Offenses	122	1.79
Fraudulent Property Offenses	2,670	39.23
Other Property	62	0.91
Drug Offenses	612	8.99
Regulatory Public Order	1,695	24.90
Other Public Order	1,324	19.45
Weapon	101	1.48
Immigration	59	0.87
Unknown	1	0.01
Other	160	2.35
Total	6,805	100.00

Table 6 shows the same categories of offenses as Table 5, for only cases identified through the agency-based definition of white collar crime. Compared to the broader sample, these cases are much less likely to be fraudulent property offenses (39.23% compared to 80.12%), and are much likely to be identified as public order offenses (regulatory or otherwise). All in all, nearly 25% of the agency-based white collar sample is made up of regulatory offenses including antitrust, food and drug, transportation, communications, civil rights, and custom laws (not shown). More than 10% of these cases are ultimately categorized as "miscellaneous general offenses" in the most detailed offense classification variable, which would preclude these cases from being categorized as white collar crime using two out of the three offense-based definitions. There is also a higher rate of drug involved offenses. While drugs are not typically associated with the idea of 'white collar crime', it is worth noting that doctors were among the first potential classes of offenders discussed by Sutherland (1939). Similarly, other white collar scholars have studied

prescription and healthcare fraud (Tillman and Pontell 1995). Notably, almost all of these cases are drug *trafficking* opposed to drug *possession* charges (not shown).

The agencies contributing to the cases identified as white collar crime through this identification approach are displayed below in Table 7. The Office of the Inspector General in the Department of Health and Human Services and "other" Department of Justice[47], supply the most cases, with the EPA and the Food and Drug Administration providing large contributions as well.

Table 7. Referrals for Prosecution by Agency, Agency-Based Definition of White Collar Crime

Agency	%	Agency	%
Bureau of Health Professionals	0.37	International Trade Administration	0.03
Bureau of Labor Standards	0.90	Joint EPA/ Local Taskforce	0.12
Commodities Futures Trading Commission	0.16	Mine Safety & Health Administration	0.85
Consumer Product Safety Commission	0.15	National Labor Relations Board	0.01
Employment Standards Administration	0.01	National Science Foundation	0.28
Environmental Protection Agency	13.46	Nuclear Regulatory Commission	0.09
Farm Credit Union	0.18	Occupational Safety and Health Administration	0.15
Federal Communications Commission	0.07	Office of Personnel Management	1.54
Federal Deposit Insurance Corporation (Banks)	1.54	Office of the Inspector General in the Department of Health and Human Services	21.91
Federal Deposit Insurance Corporation (Savings & Loans)	0.01	Other Justice	36.96
Federal Maritime Commission	0.01	Pension and Welfare Benefits Administration	0.29
Federal Reserve System	0.44	Pension Benefit Guarantee Corporation	0.03
Federal Retirement Thrift Investments	0.01	Securities and Exchange Commission	0.50
Food and Drug Administration	11.49	Small Business Administration	1.76

[47] "Other" Justice referring to a source within the Department of Justice other than the following: Bureau of Prisons, Drug Enforcement Administration, Joint DEA/Local Task Force, Inspector General of the Justice Department, Immigration and Naturalization Services, U.S. Marshals Service, Joint Marshals Task Force, Office of Justice Programs, Federal Prison Industries, and the U.S. Trustees.

Health Care Financing Administration	0.50	U.S. Trustees	6.16

Interestingly, traditional foci of white collar research for corporations, the Securities and Exchange Commission and the Federal Trade Commission provide 0.5% and 0.0%, respectively. This suggests that these agencies may be heavily involved in administrative and civil sanctions for white collar crime, but that they are less likely to be involved in criminal prosecutions. From a scholarly perspective, this indicates that the types of white collar crime handled by criminal justice agents may not be simply a winnowed sample of the types of cases, but rather that the types of white collar cases that are processed criminally may be systematically and fundamentally different from those processed through regulatory means.

What is White Collar Crime under an Offense-Based Definition?

It is also illustrative to consider the types of referring agencies and offenses that describe white collar crime when using one of the three offense-based definitions employed in this dissertation. Among a loose offense-based definition, cases are referred by dozens of agencies. The Federal Bureau of Investigation accounts for 20,573 of these cases (approximately 34%), suggesting that this group of "white collar" offenders may have more in common with traditional offending. Among the largest contributors to the agency-based definition (Office of the Health Inspector General, other Department of Justice, the EPA, the Food and Drug Administration, U.S. Trustees, and the Small Business Association), these agencies contribute only a small proportion of the cases identified as white collar under the loose offense-based definition (less than 6% combined), and are arguably lost amid the much broader range of agencies referring this type of offense for prosecution.

The loose offense-based definition captures a much more defined set of offenses. Postal fraud makes up the largest single offense type, accounting for nearly one-quarter of this group of white collar crimes. Embezzlement of public monies and properties, bank fraud, embezzlement of public monies and properties, and social security fraud each comprise nearly 12% of the sample. There are also notable numbers of forgery, counterfeiting, conspiracy to commit fraud, passport fraud, and other frauds (See Appendix A2.1). [48]

The Yale offense-based definition cases are referred by a similarly broad spectrum of agencies even for this fairly limited set of offenses. The Federal Bureau of Investigation refers nearly one-third of these cases. Other large contributions come from the Bureau of Customs and Border Protection (16%), the Internal Revenue Service (12%), the Secret Service (9%) and U.S. Postal Service (7%). Notably, even though the Yale offenses were selected with the intention of identifying "true" white collar crime, the largest portion of cases are not referred by traditional white-collar agencies. Similarly, while these cases extend beyond fraud, fraudulent property offenses still make up nearly 90% of the cases identified as white collar through this approach. Most of the remaining cases are non-regulatory public order offenses (not shown). Table 8 displays the distribution of specific offenses for these cases. The vast majority of the Yale white collar cases are postal fraud (41%) and false claims and statements (40%). Income tax fraud accounts for the next largest portion (nearly 10% across five offenses). Antitrust and Securities fraud cases account for less than 3% of the total, in spite of

[48] Due to the size of many of the tables in this section, these tables appear in the Appendix.

representing the most "pure" white collar crimes of the Yale sample (Weisburd, 1991).

Table 8. Distribution of Offenses, Yale Offense-Based Definition

Offense	Percent (N=33,975)
Bank Embezzlement	1.46
Income Tax Evasion	4.91
Other Income Tax Felony	3.86
Failure to File Income Tax	0.64
Other Income Tax Misdemeanor	0.02
Lending and Credit Fraud	3.05
Postal and Wire Fraud	40.84
Securities and Exchange Fraud	2.55
Other Tax Fraud	0.24
False Claims and Statements	39.48
Bribery	2.69
Antitrust	0.27
Total	100.00

The final white collar definition aims to overcome some of the limitations of the loose and Yale offense-based definitions by making use of the most granular offense-level data available in the FJSP. These cases, like the previous offense-based definitions, are referred by a large group of federal agencies (See Appendix A2.1). The FBI still refers nearly one-third of cases and the Secret Service Agency refers approximately 13.5%. Notably, the number of cases referred by key traditional white collar agencies, like the EPA and SEC, are fewer than are identified through an agency-based approach. This suggests that offense-based definitions may be failing to capture substantively important portions of the offenses they intend to study, as critics like Geis (1977) note.

The single largest offense type captured by the strict offense-based definition is frauds, closely followed by embezzlement. In Table 9, you can compare this to the distribution of offense types under the other offense-based white

collar definitions. While frauds make up the largest part of the strict offense-based sample, the share of the total group of white collar offenses made up of frauds is notably smaller than either of the previous offense-based definitions. Similarly, the share of regulatory offenses is larger (with the exception of tax law) than either of the other offense-based definitions of white collar, though not as large as for the agency-based definition (see Appendix A2.2). Accordingly, charges of embezzlement of public monies or property and bank fraud account for more than 80% (together) of the specific offense types, with bribery and credit and lending fraud accounting for more than 10% additional. The remaining less than 10% of the offenses are primarily regulatory in nature (see Appendix A2.2).

Table 9. Broad Offense Categories (percent), Offense-Based Definitions White Collar Crime

	Loose	Yale	Strict
Embezzlement	18.67	1.46	41.08
Fraud	74.75	85.92	47.10
Forgery	4.73	0.00	0.00
Counterfeiting	1.85	0.00	0.00
Agriculture	0.00	0.00	0.06
Antitrust	0.00	0.27	0.52
Food and Drug	0.00	0.00	2.58
Transportation	0.00	0.00	0.78
Communications	0.00	0.00	0.07
Other Regulatory Laws	0.00	0.00	2.04
Tax Law	0.00	9.66	0.00
Bribery	0.00	2.69	5.20
National Defense	0.00	0.00	0.34
Environmental Offenses	0.00	0.00	0.23
N	60,470	33,975	17,584

ANALYSIS SAMPLES

This dissertation uses two extracts of the FJSP to explore the prosecution and sentencing of white collar crimes in the federal criminal justice system. I refer to these extracts as “analysis samples”. The analysis samples are distinguished by

which stage of the federal justice system they describe. The raw counts of cases that appear in these samples is shown in Table 10, below.

Table 10. Analysis Samples

	N
Prosecution analysis sample	27,594
Sentencing analysis sample	22,966

The first extract is the prosecution analysis sample. The prosecution sample includes white collar and comparable cases referred to federal prosecutors between 2009 and 2011 that were matched across all of the following datasets: EOUSA Matters In, EOUSA Matters Terminated, US Marshals Arrests, EOUSA Defendants In, EOUSA Defendants Out, AOUSC Cases Filed, and AOUSC Cases Terminated. The prosecution sample is used to analyze negotiated plea outcomes (severity and count reductions and any charges dropped or dismissed). The second extract is the sentencing analysis sample and consists of white collar and comparable crimes from the referral cohort that were matched across all eight FJSP datasets used in this dissertation. Descriptive statistics will be discussed for each of these samples independently and in relation to one another.

PROSECUTION ANALYSIS SAMPLE

Because the prosecution outcomes considered are matched across most of the links used in this dissertation, the prosecution sample is much smaller than the number of cases at the start of the link process (27,594 cases out of 92,214 at referral). These cases are used to predict the decision to reduce or dismiss charges. These variables, as well as independent variables and controls for this sample, are described in Table 11, both for all cases used in any sample, as well as in definition-

specific samples. However, the samples are generally similar to one another, so only the full sample is discussed.

About 90% of cases are resolved by plea. However, most of these pleas do not appear to be negotiated (or are not detectable as negotiated with these measures); only 7% of cases have charges withdrawn or dropped. Eight percent experience a reduction in the number of counts faced between filing and case conclusion, and 17% experience a reduction in severity. It is rare that these negotiatied components of plea occur in common with each other; slightly more than 3% of cases receive more than one type of negotiated plea, and only 0.33% of cases at this stage receive all three (not shown).

Table 11. Descriptives, Prosecution Sample (All Definitions)

	Full Sample					Agency	Loose	Yale	Strict
Variable	Min	Max	Miss.	Mean	SD	Mean	Mean	Mean	Mean
Plea	0	1	0.00%	0.90	-	0.90	0.90	0.90	0.90
Charges Dropped/Dismissed	0	1	0.00%	0.07	-	0.07	0.07	0.07	0.07
Count Reduction	0	1	0.00%	0.08	-	0.08	0.08	0.08	0.08
Severity Reduction	0	1	0.00%	0.17	-	0.17	0.17	0.17	0.17
Agency Sample	0	1	0.00%	0.89	-	-	-	-	-
Loose Sample	0	1	0.00%	0.94	-	-	-	-	-
Yale Sample	0	1	0.00%	0.90	-	-	-	-	-
Strict Sample	0	1	0.00%	0.88	-	-	-	-	-
White Collar - Agency	0	1	0.00%	0.04	-	0.05	-	-	-
White Collar - Loose	0	1	0.00%	0.68	-	-	0.72	-	-
White Collar - Yale	0	1	0.00%	0.27	-	-	-	0.30	-
White Collar - Strict	0	1	0.00%	0.17	-	-	-	-	0.19
Any Codefendants	0	1	0.00%	0.34	-	0.33	0.34	0.33	0.33
Number of Codefendants	0	100	0.00%	2.18	9.25	2.32	2.26	2.26	2.33
Violent Offense	0	1	0.00%	0.11	-	0.12	0.11	0.12	0.12

Drug Offense	0	1	0.00%	0.01	-	0.01	0.00	0.00	0.00
Regulatory Offense	0	1	0.00%	0.01	-	0.01	0.00	0.00	0.01
Number of Charges	1	5	0.00%	1.99	1.24	2.01	2.01	2.00	2.01
Total Counts Filed	0	687	0.00%	3.67	8.72	3.64	3.60	3.70	3.63
Most Severe Incarceration	1	13	0.00%	4.73	2.42	4.77	4.74	4.69	4.75
Most Severe Fine	0	9	0.00%	6.16	2.51	6.14	6.07	6.20	6.13
Private Counsel	0	1	0.00%	0.15	-	0.15	0.14	0.16	0.15
Male	0	1	0.05%	0.71	-	0.70	0.70	0.70	0.70
Black	0	1	0.04%	0.29	-	0.28	0.30	0.29	0.29
Other Race	0	1	0.04%	0.05	-	0.05	0.05	0.05	0.05
Age	18	89	1.78%	38.97	12.40	39.02	38.54	39.37	38.99
Married	0	1	11.57%	0.40	-	0.40	0.39	0.41	0.40

The analysis samples make up similar portions of the total sample as at the referral stage (see Table 3); however, the agency sample is slightly reduced in relative size, while the loose sample is slightly increased. This implies that the agency based sample is more likely to suffer from attrition or nonmatches compared to the other definitions, while the loose definition is slightly less. The variables identifying white collar crime also suggest differential attrition, though most are less common in the samples than at referrals. White collar crimes are from ~4% more prevalent in the prosecution sample than in the referral sample to 42.5% less common (for the agency sample). This suggests that, for at least three out of four definitions of white collar crime, white collar cases are more likely to either drop out or still be pending.

About one-third of cases have codefendants; all cases have an average of 2.18 codefendants, but, among those with codefendants, the average is 6.58. There is also substantial variation in the number of codefendants for white collar cases

(not shown): the loose and Yale offense-based white collar cases have average number of codefendants 7.96 and 11.64, respectively, compared to 6.50 and 3.56 for the agency and Yale definitions. Surprisingly, as a measure of complexity, this suggests that the definitions more attuned to the occupational component of white collar crime are 'less complex' than looser definitions. Violent offenses make up a sizeable portion of the sample, primarily due to the inclusion of robbery as a comparison crime; this emphasizes the importance of sensitivity analyses excluding these offenses. Drug offenses make up a small portion of the sample overall, but are only present in the agency sample (as they are contained exclusively in the agency definition of white collar crime). There is tremendous variation in the number of charges cases have at filing, but on average cases have fewer than 4 charges. The average risk of fine at filing for the most serious charge is fairly large, between $10,000 and $50,000; in contrast, the average risk of incarceration is less than three years. Defendants are generally male and white, with only one third of the sample being black or another race. While some defendants are elderly, the average defendant is slightly younger than 40. Only 15% of defendants have private counsel representing them, while 40% are married.

SENTENCING ANALYSIS SAMPLE

The sentencing analysis sample is slightly smaller than the prosecution sample because it requires the additional link to the sentencing commission data. This sample is used to predict sentencing outcomes and is comprised 22,966 cases. The characteristics of this sample can be found in Table 12. Most cases receive prison (71%), though this is notably less common than general estimates of prison

sentences for federal offenders; according to the United States Sentencing Commission, approximately 90% of all offenders sentenced receive some sort of prison term (Table D, Sourcebook 2017). This is not surprising given the focus on offenses that are arguably less serious than many under the federal guidelines, and the exclusion of most types of drug cases. In fact, more than one-third of offenders were eligible to receive a non-prison sentence. Departures are somewhat common in the sample, with 17% of offenders receiving judicial departures and 23% receiving prosecutor sponsored departures. Notably, these are less common than for federal offenders overall (Table N, Sourcebook 2017); it is possible that because these offenses are less serious on average, the impetus to circumvent harsh guideline prescriptions is lesser for these offenses. However, the difference is generally small, and could reflect variability across years.

Table 12. Descriptives, Sentencing Sample (All Definitions)

	Full Sample					Agency	Loose	Yale
Variable	Min	Max	Miss.	Mean	SD	Mean	Mean	Mean
Any Prison	0	1	0.05%	0.71	-	0.71	0.72	0.71
Judicial Departure	0	1	17.34%	0.27	-	0.27	0.26	0.28
5K Departure	0	1	23.40%	0.21	-	0.22	0.21	0.22
Any Downward Departure	0	1	0.88%	0.39	-	0.39	0.38	0.40
Eligible No Prison	0	1	0.00%	0.34	-	0.35	0.35	0.34
Agency Sample	0	1	0.00%	0.89	-	-	-	-
Loose Sample	0	1	0.00%	0.94	-	-	-	-
Yale Sample	0	1	0.00%	0.9	-	-	-	-
Strict Sample	0	1	0.00%	0.88	-	-	-	-
White Collar - Agency	0	1	0.00%	0.04	-	0.04	-	-
White Collar - Loose	0	1	0.00%	0.67	-	-	0.72	-
White Collar - Yale	0	1	0.00%	0.26	-	-	0.00	0.29
White Collar - Strict	0	1	0.00%	0.16	-	-	0.00	-
Any Codefendants	0	1	0.00%	0.33	-	0.32	0.33	0.31

TABLE 12 (Continued). Descriptives, Sentencing Sample (All Definitions)									
	Full Sample					Agency	Loose	Yale	Strict
Variable	Min	Max	Miss.	Mean	SD	Mean	Mean	Mean	Mean
Number of Codefendants	0	88	0.00%	1.91	8.11	2.02	1.96	1.96	2.02
Plea	0	1	0.00%	0.95	-	0.95	0.95	0.95	0.95
Charges Dropped/Dismissed	0	1	0.00%	0.06	-	0.06	0.07	0.06	0.06
Count Reduction	0	1	0.00%	0.08	-	0.09	0.08	0.08	0.09
Severity Reduction	0	1	0.00%	0.17	-	0.18	0.17	0.17	0.18
Accept Responsibility	0	3							
Total Financial Sanctions	0	$584m	1.65%	$390,944	$6.34m	$416,193	$393,921	$423,277	$427.68
Offense Score	1	43	1.59%	14.23	7.21	14.44	####	14.43	14.39
Criminal History	1	6	1.08%	2.01	1.61	1.97	2.04	1.94	1.96
Violent Offense	0	1	0.00%	0.12	-	0.13	0.12	0.13	0.13
Regulatory Offense	0	1	0.00%	0.01	-	0.01	0.00	0.00	0.01
Number of Charges	1	5	0.00%	1.99	1.24	2.00	2.00	1.99	2.00
Presentence Detention	0	1	5.99%	0.47	-	0.48	0.48	0.47	0.48
Private Counsel	0	1	0.00%	0.15	-	0.15	0.14	0.16	0.15

TABLE 12 (Continued). Descriptives, Sentencing Sample (All Definitions)									
	Full Sample					Agency	Loose	Yale	Strict
Variable	Min	Max	Miss.	Mean	SD	Mean	Mean	Mean	Mean
Male	0	1	0.05%	0.71	-	0.71	0.71	0.71	0.71
Black	0	1	0.05%	0.29	-	0.28	0.29	0.28	0.28
Other Race	0	1	0.05%	0.05	-	0.05	0.05	0.06	0.06
Age	18	89	1.96%	38.52	12.28	38.59	38.11	38.93	38.58
Married	0	1	11.65%	0.4	-	0.40	0.39	0.41	0.40
High School Graduate	0	1	7.98%	0.34	-	0.34	0.34	0.33	0.34
Any College	0	1	7.98%	0.4	-	0.41	0.39	0.41	0.41
Other Education	0	88	7.98%	0.02	0.13	0.02	0.02	0.02	0.02
Any Dependents	0	1	0.00%	0.62	-	0.63	0.62	0.63	0.62
AY Pre-2003	0	1	6.69%	0	-	0.00	0.00	0.00	0.00
AY 2003-2007	0	1	6.69%	0.01	-	0.01	0.01	0.01	0.01
AY 2008	0	1	6.69%	0.14	-	0.14	0.15	0.14	0.14
AY 2009	0	1	6.69%	0.28	-	0.28	0.28	0.28	0.28
AY 2010	0	1	6.69%	0.36	-	0.36	0.36	0.36	0.36

There is little change in the distribution of samples or white collar crime from the prosecution analysis sample, which is not surprising given the addition of only a single dataset; the same is true for other variables common across both samples, so they will not be belabored here. The introduction of the USSC data, however, also introduces several new variables for analysis. The average offender has an offense score of approximately 14 and a criminal history score of 2; however, the full range of possible offense seriousness and criminal history are present in the sample. In other words, while white collar cases and comparable offenders and offenders are generally not as serious as other crimes, some are nevertheless very serious. Nearly half of offenders are detained pretrial. Nearly one-fourth of offenders have less than a high school education (reference category), though 40% have at least some college education. Finally, while the cohort of offenses in this dissertation was referred for prosecution between 2009-2011, not all offenses are sentenced under these guidelines. A very small proportion of cases were sentenced before the passage of Sarbanes-Oxley (after which point Chapter 2B was substantially changed), Most cases were sentenced under the 2008-2010 guidelines.

CONCLUSION

The data discussed in this chapter set the stage for a more comprehensive descriptive look at the processing of white collar crime in the next chapter, and also illustrated the importance of studying both process and definition. The proportional representation of some white collar crimes (i.e. agency-based and the Yale definitions) changes markedly from referrals to case conclusion by prosecutors and

sentencing. Yet, other definitions remain approximately as common throughout the process. This implies there may be significant differences in case processing across the definitions.

CH 5. WHITE COLLAR CASE PROCESSING IN THE FEDERAL CRIMINAL JUSTICE SYSTEM

INTRODUCTION

In this chapter, I provide descriptive analyses of white collar case movement through the federal criminal justice system. I explore key points at which cases exit the system, including the lack of an arrest and a matter being closed without action. I also consider variation in outcomes for those cases remaining in the system, including cases still pending, the incidence and degree of prosecutorial bargaining, and sentencing outcomes.

WHITE COLLAR PROCESSING

The first goal of this dissertation was to describe the movement of cases through the federal criminal justice system, beginning with referral for criminal prosecution through case sentencing. The linking of each stage as discussed in Chapter 3 (see also Appendix A1) in conjunction with some definitional differences in descriptive statistics presented in Chapter 4 provide some insight as to potential differences across white collar and comparison crimes, as well as across definitions. Out of more than 92,000 case-defendants referred for potential prosecution between 2009 and 2011, fewer than 28,000 resulted in concluded criminal cases up through 2013, and fewer than 23,000 are sentenced by the end of 2013. This tapering is depicted in Figure 4. The greatest degree of tapering occurs at the front end of the system, as cases are not filed in criminal court or do not result in arrest. However, more than 9% of the matters sample has yet to reach a conclusion by the end of the observation window. Others are filed in court, but still ongoing.

Figure 4. Criminal Case Processing, Full Sample

Overall, the greatest source of exit from the criminal justice system is through the decision to decline prosecution. More than 30% of all closed matters were declined (Table 13).[49] Nevertheless, the majority of matters move forward, and the most common outcome for a matter is a guilty verdict.

Table 13. Disposition of Matters Concluded

Disposition	**Percent**
Closed Without Action	0.02
Declined	31.63
Dismissed with Prejudice	0.36
Dismissed	2.49
Juvenile Delinquent Adjudication[50]	0.02
Criminal Guilty Verdict	38.64
No indictment returned	0.00
Nolo Contendre	0.01
Criminal Not Guilty Verdict	0.15
New Filing	1.26
Included in Another Case	1.24
Opened in error	0.04
Removal (Rule 40)	1.62
Transferred	0.85
Total	78.33%
Unknown	21.67%

Both of these common outcomes can result from a number of factors. Table 14 reports some of the most common disposition reasons reported for both those declined and those not declined. Nearly 80% of matters not declined are concluded through plea agreements, either in district or magistrate court. The next most common outcome is Rule 40 transfers, which occur when individuals are arrested for a crime but are also in violation of conditions of release for another crime, or

[49] Unfortunately, disposition values are missing for a significant portion of cases (more than 20%), indicating that while these results are suggestive, they are by no means conclusive.
[50] Juvenile cases are removed after the second dyad, with the age variable from the U.S. Marshals Arrest data.

found to be in violation of a subpoena; in these instances, the original case supersedes the new filing. Similarly, cases can be declined for a number of reasons. The most common reason reported for a declination is that the case lacks clear evidence of criminal intent (23.36%), followed closely by weak evidence. However, a notable 15% of cases are declined due to agency request. In nearly 10% of cases, matters are declined because the suspect is prosecuted either by another agency related to the same behavior or on another charge. Interestingly, approximately 4% of declined cases are not moved forward because there is no federal interest in the case.

Table 14. Selected Disposition Reasons, for Cases Closed Without Action or Declined and Others

	Other	Closed Without Action/ Declined
By Agency Request	-	15.33%
Lack of Evidence of Criminal Intent	-	23.36%
Minimal Federal Interest/No Deterrent Value	-	4.03%
No Federal Offense Evidence	-	5.08%
Suspect Prosecuted Other Authorities	-	6.42%
Suspect Prosecuted Other Charge	-	3.19%
Weak Evidence	-	21.47%
Plea – District Court	59.68%	-
Plea – Magistrate	20.09%	-
Rule 40 Transfer	3.63%	-
Total	83.40%	78.88%

[1] Some value labels sourced from LIONS Data Appendix when not determinable using ICPSR materials.

Table 15 reports bivariate tests of difference in the likelihood of certain outcomes according to key variables. The table reports observed differences for when a dummy variable takes a value of "1" versus a value of "0". Thus, a positive observed difference indicates an increase in the likelihood that a case with that characteristic is declined, for example. The difference in specific declination reasons are tested among cases that were declined. Differences are statistically

significant at $p<.005$ unless otherwise specified. White collar cases are generally more likely to be closed without action, though the degree of difference varies substantially by definition (4%-14%). The loose offense-based definition results in the largest difference in case declination, followed by the strict definition.

Table 15. Differences in Disposition Outcomes (percent) Across Key Variables

Variable	Declined/ CWA	Agency Request	No Federal Interest	Weak Evidence	Other Authorities
White Collar (Agency) [1]	0.07	-0.03	0.00†	0.01†	-0.02
White Collar (Loose) [1]	0.14	-0.02	0.02	-0.01‡	-0.02
White Collar (Yale) [1]	0.04	0.01	-0.01	0.00†	-0.02†
White Collar (Strict) [1]	0.11	0.02	0.01	0.03	-0.03
Comparison (Agency) [1]	-0.04	0.01†	0.00†	0.00†	0.01
Comparison (Loose) [1]	-0.15	0.02	0.01	0.02	0.03
Comparison (Yale) [1]	-0.03	0.00†	0.01	0.00†	0.01
Comparison (Strict)[1]	-0.07	-0.02	-0.01	0.02	0.02
Any Codefendants[1]	0.17	0.00†	0.00†	0.00†	0.00†
Number of Codefendants[2]	-0.49	-0.13	0.61	0.06†	0.49

[1] Z test for difference in two sample proportions
[2] T-test for difference in two sample means (unequal variances assumed)
† Not significant at p<.05
‡ Significant at p<.05

The Yale offense-based definition is the least different from non-white collar cases in the likelihood of declination, but the difference is still significant (likely in part due to sample size). In contrast, comparison crimes are uniformly less likely to be declined, with similar patterns of variation in magnitude across definitions. Cases with codefendants are more likely to be declined; however, cases that are declined have, on average, fewer codefendants than those cases that are not declined. This

suggests that while complexity due to the multiple individuals involved may increase the odds of declination, greater numbers of codefendants are not necessarily indicative of more lenient treatment at this stage.

There are fewer differences across groups for the specific reasons for declinations. While most differences observed are significant, they are generally not large enough to be substantive. The greatest differences observed are for cases involving codefendants; cases that are deemed to have minimal federal interest or are prosecuted by other authorities generally have more codefendants, while those declined due to agency request tend to have fewer. Given these differences, how does the total graduated system processing of cases vary across crime type? For the sake of avoiding redundancy, these figures are not presented here, but are available in the Appendix (A2.3). These figures suggest that, at an aggregate level, the definition of white collar crime has very little impact on the overarching view of case processing through the federal system – if you consider the *total sample* of white collar and comparable crimes. However, the number and processing of white collar cases *relative to comparable crimes* does appear to vary by definition starting with referral. Though discussed previously, it is worth noting that the overall makeup of the sample that is white collar crime varies substantially across definitions, with agency-defined white collar crime generating the appearance of less than 7,000 matters referred and the loose offense-based definition dominating the sample with 60,470 matters referred.

While there are similar patterns of concluding matters for white collar and comparable crimes for all four definitions, with white collar crimes slightly more

likely to be concluded, this difference is larger when using the more inclusive loose-offense based definition. However, the pattern of arrests varies across definitions. White collar crimes are much less likely to be arrested relative to comparison crimes using the agency-based definitions; this difference is smaller for the loose and strict offense-based definitions, but disappears when using the Yale definition. As argued before, this may reflect the inclusion of tax fraud cases, which dominate the Yale white collar sample. Among cases with arrests there are even more diverse patterns across definitions. The percent of these cases that do not have a corresponding criminal case (recall these cases are largely processed through magistrate court). Under the Yale definition, 36.31% of white collar cases do not have a corresponding case in criminal court (relative to 9.07% of comparable crimes), but are much less likely (relative to both comparable crimes and the Yale rate) not to have a criminal case using the strict or loose offense-based definition, and approximately equally likely under the agency-based definition.

However, beyond the initial stages of arrest and case filing in criminal court, there are relatively few differences in the patterns of white collar crimes relative to comparable crimes across the four definitions. Under the loose offense-based definition, white collar cases are more likely to extend beyond the observation window (i.e., are not concluded) suggesting that these cases may take longer; this difference is also observed for two of the three other definitions, though the magnitude of the difference is smaller. Otherwise, the relative case processing of white collar and comparable crimes is generally similar across definitions as well as relative to comparable crimes.

If white collar cases do not differ in the macro-level processing, either from each other or relative to comparison crimes, do they vary in substantive outcomes? The above descriptions began to explore this question, and found some differences in the likelihood of certain outcomes based on white collar status, as well as variation in this effect across definitions. The next section unpacks these findings further by looking at several questions, including the rate of matters closed without action, the rate of cases filed, the rate of cases still pending, and the conclusions of cases across the four different white collar definitions. In addition to the descriptive value of these findings, this analysis also offers insight into whether or not definitional differences produce different descriptions of white collar processing.

Table 16 reports the rates and averages for a number of case processing outcomes from referral to sentencing across the four white collar definitions. While the overall picture of processing is similar across the four definitions, as noted above and presented in detail in Figures A2.3a-d in the Appendix, there are interesting differences in case outcomes across the different white collar samples.

Table 16. Case Outcomes Across White Collar Definitions

			Agency	Loose	Yale	Strict
Referred			6,806	60,470	33,975	17,583
	Declined		47.20%	45.41%	42.70%	49.66%
	Matters Still Pending		11.87%	10.18%	11.23%	10.81%
	Filed		23.35%	39.46%	28.93%	32.38%
		Concluded[1]	73.63%	78.46%	71.65%	80.10%
		Dismissed/Nolle Prosequi[2]	5.38%	4.98%	4.42%	6.84%
		Acquittal[2]	0.60%	0.35%	0.35%	0.37%
		Plea[2]	89.74%	92.53%	92.52%	90.44%
		Reduction in Severity	16.84%	16.12%	17.32%	15.07%

		Reduction in Counts	8.55%	7.08%	8.20%	6.64%
		Charge Dropped	5.21%	6.97%	7.80%	6.36%
		Imprisoned[3]	45.32%	67.53%	70.52%	58.20%
		Average Prison Length[3,4]	59.20	26.71	21.09	26.50
		Alternatives[3]	21.08%	17.07%	17.29%	21.40%
		Fine Waiver[3]	87.30%	93.59%	91.02%	93.28%
		Fine Amount[3,5]	$88,288.11	$276,545.60	$364,187.60	$64,378.21
		Judicial Departure	28.97%	22.74%	24.96%	25.78%
		Prosecutorial Departure	16.80%	17.70%	16.25%	15.15%

[1] Percent of matters filed concluded
[2] Percent of concluded matters, using prosecution sample
[3] Using sentencing sample
[4] Of those incarcerated
[5] Of those without fine waivers

The rate of matters declined is similar across definitions, but is highest for the agency and strict offense-based definitions of white collar crime. In comparison to other definitions of white collar crime, the agency based definition is the least likely to result in a case being filed in criminal court. Once filed, these cases also have a lower rate of concluding within the observation window. Yet, these cases are also slightly more likely to be dismissed or result in an acquittal. While the rate of pleas is similar across groups, the rate of negotiated pleas varies slightly. Using an agency-based definition, white collar cases are slightly more likely to receive a reduction in charge severity or a reduction in counts, but less likely to have a charge dropped.

Punishment outcomes are also markedly different across groups. The agency and strict offense-based definitions are imprisoned at much lower rates and more likely to receive alternative confinement sanctions than other white collar crimes, yet the average length of imprisonment for agency white collar crimes are

nearly twice the average for other definitions. Fine waivers, which are given for defendants who are financially unable to pay for fines, are given to the vast majority of the sample, though agency-based white collar cases are slightly less likely to receive this waiver than others. This suggests that defendants identified using an agency-based approach are the most likely to have significant financial assets (in line with assumptions about many white collar offenders), though most defendants still have limited means, consistent with prior research (Weisburd, 1991). Perhaps the most striking difference in case outcomes is in the average amount of fine among offenders who do not receive a waiver. The loose and Yale offense-based definitions of white collar suggest much higher fines for the average offender than using either the agency or strict-offense based definitions. This may be the result of tax offenses, which are likely to incur large amounts of interest and penalties, while simultaneously not likely to incur restitution obligations. Departures are somewhat common for white collar cases, with nearly 40% of white collar cases receiving either a judicial or prosecutor-sponsored sentence below the guidelines range. Agency based white collar cases are also slightly more likely to receive judicial departures, but not prosecutorial departures, and the differences are slight. While these numbers do not include comparisons to other crimes, they nevertheless imply that there may be some differences in punishment depending on the definition of white collar crime used.

Based on prior literature, several other case characteristics may potentially influence processing and outcomes. For example, cases that have a private attorney may be able to influence early case processing outcomes, or better argue for

leniency at sentencing. Complex cases, i.e., those with codefendants, may also be processed differently than less complex cases, given the resources they require; they might also be treated more harshly because of the relative seriousness of these offenses compared to the acts of individual offenders. Table 17 presents the outcomes of cases across these dimensions, focusing on case conclusion and sentencing (based on availability in the data). As evidenced by these descriptive statistics, there are stark differences in several case outcomes across these characteristics, as well as strong similarities. For example, few cases are acquitted, regardless of type of attorney or whether they are single defendants or have codefendants. Compared to cases without private counsel, defendants with private attorneys receive some advantages – they are less likely to be imprisoned, receive a shorter average length when they are, and are more likely to receive alternative confinement. However, they are also less likely to be dismissed or result in dropped prosecutions. Cases with private counsel are less likely to receive negotiated pleas across any of the three measures, but are more likely to receive government-sponsored substantial assistance departures. While other attorneys may focus on reducing charges, counts, or charge severity, private attorneys may target substantial assistance departures. Similarly, they are less likely to receive a fine waiver (not surprising given the financial assets required to attain private legal assistance), and receive fines more than ten times as high as other cases. The Yale and agency-based definitions of white collar crime tend to have higher rates of private counsel than either the loose or strict offense-based definitions (not shown).

Table 17. Case Outcomes Across Counsel and Codefendant Dimensions

		Private Counsel	Other Counsel	Any Codefendants	Individual Offender
Concluded		4,130	23,464	9,254	18,340
	Dismissed/Nolle Prosequi[1]	1.48%	5.19%	6.03%	3.93%
	Acquittal[1]	0.32%	0.37%	0.54%	0.26%
	Plea[1]	96.32%	91.91%	90.24%	93.74%
	Reduction in Severity	9.42%	17.99%	25.08%	12.48%
	Reduction in Counts	6.25%	8.06%	10.97%	6.19%
	Charges Dropped	3.37%	8.00%	10.63%	5.63%
	Imprisoned[2]	64.88%	72.29%	74.23%	69.73%
	Average Prison Length[2,3]	33.88%	41.67	46.11	37.70
	Alternatives[2]	23.00%	13.88%	13.57%	16.03%
	Fine Waiver[2]	85.60%	94.57%	94.54%	92.62%
	Fine Amount[2,4]	$524,763.00	$40,416.09	$292,444.80	$157,810.90
	Judicial Departure	29.98%	21.23%	19.74%	23.87%
	Prosecutorial Departure	22.90%	15.34%	20.74%	14.40%

[1] Percent of concluded matters, using prosecution sample
[2] Using sentencing sample
[3] Of those incarcerated
[4] Of those without fine waivers

Codefendants are similarly advantaged in some case processing outcomes and disadvantaged in others. Cases with codefendants are more likely to result in nullified prosecution or dismissed cases, and slightly more likely to result in acquittal, which may relate to difficulty in establishing individual blame (consistent with Albonetti, 1994). However, cases with codefendants are also imprisoned at higher rates, for longer periods of time, and are less likely to receive any alternative confinements. They are slightly more likely to receive fine waivers, but when they do not, receive sentences approximately twice that of defendants who do not have codefendants.

Given that there are different rates of negotiated pleas and departures, what are the consequences of these agreements on sentencing outcomes? Figures 5 and 6 display the probability of incarceration and alternatives and the length of incarceration (for those who received any incarceration) across different "discounts" defendants received. In general, discounts are received in isolation from each other, so these basic descriptives should reflect average sentencing outcomes.

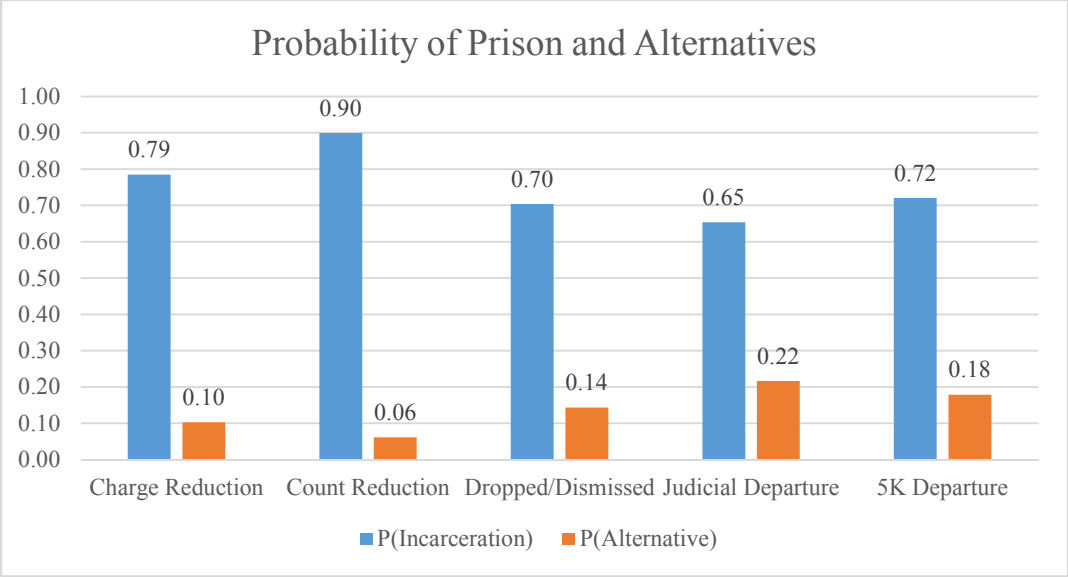


Figure 5. Probability of Prison and Alternative Confinement by Discount Type

In general, count reduction appears to be the least "successful" discount for defendants, given that these cases have very high rates of incarceration, have the lowest rate of alternative confinement, and the longest length of incarceration on average. Judicial departures, in contrast, tend to have the most favorable outcome for defendants; they are the least likely to result in a prison term and the most likely to receive some alternative confinement. They are also associated with the second shortest average sentence length. Prosecutor-sponsored departures, however, have

similar rates of alternative confinement and have the lowest average sentence length. If private attorneys are preferring to target substantial assistance departures instead of other types of negotiated reductions for their clients, they may be doing so strategically. In contrast, attorneys who prioritize count reduction may be doing so to achieve nominal success; however, this relationship could reflect a correlation between offense seriousness and the number of counts.



Figure 6. Length of Incarceration, by Discount Type

CONCLUSION

This chapter delved into the patterns of criminal case processing after referral for prosecution in the FJSP to describe the flow of cases through the federal criminal justice system. In large part, while there were differences between white collar and comparison crimes for most definitional samples, the picture of white collar case processing *across definitions of white collar crime* was generally similar. White collar cases were more likely to be declined than comparison crimes, with the most common reasons cited being a lack of criminal intent or weak

evidence. Yet, one-sixth of these declinations were by agency request, suggesting that regulatory agents have some influence over prosecutors above and beyond referral of cases. It could be that regulatory agencies engage in do-not-prosecute agreements of their own, or that they use criminal case referral as a tool to encourage compliance; once compliance is achieved, the additional benefits of prosecution may be limited from a regulator's perspective. The most pronounced differences in case processing between white collar crime and comparison crimes were observed for decisions prior to case filing.

The effect of definition on substantive conclusions is most pronounced in three areas: (a) the overall magnitude of cases identified at referral, which carries through the system, (b) the proportion of cases that exit the system after matters referred (i.e., have no resulting arrest; the vast majority of these cases are declined), and (c) whether cases are filed in criminal court. For all but the Yale offense-based definition, white collar cases are more likely to exit the criminal justice system due to no arrest or declination than comparable crimes; however, those white collar cases that do have an associated arrest are then *more likely* to have a criminal case filed than comparable crimes. This reflects the fact that white collar cases in the federal system may involve an arrest later in case development than other types of crimes, given the role of the prosecutor in identifying and investigating white collar crimes. In contrast, using a Yale offense-based definition suggests that white collar cases are no more likely to involve an arrest, but these cases are then generally more likely to fail to match to a corresponding criminal case. This seems likely to be driven by the sizeable portion of tax fraud cases in the Yale sample; because many

tax fraud prosecutions may result from audits, prosecutors likely do not need to engage in negotiation to access documents as with they may need to for other financial crimes. The most pronounced effect of definition, however, was for sentencing outcomes, particularly imprisonment, average sentence length, and the amount of fine. Depending on what definition is employed, prison may be the modal response, but also may not. The length of incarceration for white collar cases also varied substantially, and varied in the opposite direction as the likelihood of incarceration. This chapter also offered a preliminary analysis of the effect of type of counsel and case complexity on case outcomes. Cases with private attorneys received a mix of advantages and disadvantages. When represented by a private lawyer, defendants were less likely to have their cases result in either dismissal or acquittal and plead at a higher rate. In spite of this higher rate of pleas, they were less successful in attaining negotiated pleas. However, private attorneys were generally more successful in attaining substantial assistance departures, which were associated with more lenient sentencing outcomes; they were second only to judicial departures. While these results are suggestive, the relationship between case characteristics and prosecution and sentencing outcomes, as well as definitional effects, are considered more fully in the next chapters.

CH. 6 WITHIN WHITE COLLAR CASE PROCESSING

INTRODUCTION

This chapter presents the formal hypothesis tests for Hypothesis 1. Specifically, these analyses will test whether case complexity is related to lenient prosecutorial outcomes during plea negotiations in the form of charge severity reductions, count reductions, and having any charges dropped or dismissed. These analyses are conducted "within" white collar crime samples; i.e., these results do not attempt to speak to the validity of theories predicting leniency or liability afforded to white collar cases relative to comparable traditional crimes (which will be tested in Chapter 7). While the primary definition of white collar crime used in this dissertation is an agency-based definition, these findings are presented in comparison to findings using each of the three offense-based definitions. Note, to best tap into negotiated outcomes, these analyses follow Shermer and Johnson (2010) in considering only discounts achieved for cases that also resolved via a guilty plea agreement.

THE ROLE OF CASE COMPLEXITY

As discussed in Chapter 2, previous literature (both quantitative and qualitative) suggests that prosecutors are predisposed to dislike complex cases – this may be because their resources are limited or because they undermine the likelihood of conviction, which may benefit them for personal or group reasons (e.g., Emmelman, 1996; McConville and Mirsky, 2005; Vogel, 1999). Several scholars have argued that early role of the prosecutor in white collar investigation and prosecution creates a situation in which case complexity is particularly

influential (e.g. Albonetti, 1994; Mann et al., 1985; Wheeler and Rothman, 1982). Thus, Hypothesis 1 predicted that case complexity would be associated with more lenient prosecutorial outcomes, specifically: (a) increased likelihood of charge reductions (b) increased likelihood of count reductions, and (c) increased likelihood of charge dismissals. This analysis requires the use of the prosecution analysis sample, which includes all cases successfully linked through the AOUSC Cases Concluded dataset.

Charge Reductions

The prosecutorial outcomes discussed in this chapter take place after initial filing. Each is modeled using logistic regression and includes additional controls for the initial charge severity as given by fines and incarceration and is modeled only for those cases resolving through plea to best tap into prosecutorial discretion, following prior research (Shermer and Johnson, 2010).[51] The first such outcome considered is a reduction in charge severity, as indicated by a reduction in the maximum incarceration or fine amounts for the most serious charge at termination based on the statutory maximum for incarceration and fine for the most serious charge at filing. Tables 18 reports the odds ratios for two models per definitional sample: (1) a model where the primary variable of interest is the presence of any codefendants and (2) a model where the primary independent variable is the number of codefendants. Table 19 reports a pooled sample of all cases meeting any of the four definitions of white collar crime. Due to the similarity of the effect of control variables across definition, these coefficients are only presented and discussed in

[51] Supplemental analyses (Appendix) consider these effects for the full sample and are substantively similar.

the context of the pooled sample. Full tables with all coefficients are available in the appendix (A2).

Table 18. Logistic Regression Predicting Reduction in Charge Severity, Definitional Samples (Fixed Effects for District)

	Agency				Loose			
	Odds Ratio				Odds Ratio			
Any Codefendant[1]	1.67	*	-		1.24	***	-	
Number of Codefendants[1]	-		0.96		-		0.98	***
N	742		742		14,646		14,646	
LR X^2	140.23		137.58		2445.92		2440.25	
	Yale				Strict			
	Odds Ratio				Odds Ratio			
Any Codefendant[1]	1.51	***	-		0.90		-	
Number of Codefendants[1]	-		0.99		-		1.00	
N	5,887		5,887		3,600		3,600	
LR X^2	1083.79		1067.48		6229.13		628.60	

[1] One-tailed p-value. All others two-tailed unless noted
* p<.05
** p<.01
***p<.005

For three of the four crime types, cases with codefendants are 24-67% more likely to receive a severity reduction in the process of a plea bargain than are cases with only individual defendants. However, there is no effect for the number of codefendants across any of the single definition samples. In the pooled sample of white collar cases (Table 19), the likelihood of receiving a reduction in charge severity between filing and termination is more than 30% higher for cases with any codefendants. In addiiton, each additional codefendant appears to significantly *decrease* the odds of receiving such a reduction. This, in conjunction with the close proximity of this odds ratio to 1.00 could suggest that either (a) there is no true relationship, and the model is overpowered or (b) a non-linear effect of the number of codefendants. Regardless of the source of this finding, the number of

codefendants is not associated with lenient outcomes via increased likelihood of charge reduction. Altogether, this suggests only partial support for the hypothesis that more complex cases will be more likely to receive a reduction in charge severity during plea negotiations.

Table 19. Logistic Regression Predicting Reduction Charge Severity, All Definitions (Fixed Effects for District)

	Odds Ratio		Odds Ratio	
Any Codefendant[1]	1.31	***	-	
Number of Codefendants[1]	-		0.98	***
Male	0.92		0.92	
Age	0.99	***	0.99	***
Black	1.09		1.09	
Other Race	0.86		0.87	
Married	0.94		0.94	
Private Counsel	0.64	***	0.61	***
Violent Offense	2.06	*	4.01	*
Drug Offense	0.64		0.70	
Regulatory	0.99		1.00	
Misdemeanor/Petty	3.24	***	2.89	***
Total Counts at Filing	1.00		1.00	
Number of Charges	2.15	***	2.20	***
Risk of Incarceration at Filing	0.93	***	0.95	***
Risk of Fine at Filing	1.15	***	1.16	***
N	18,250		18,250	
LR X^2	3085.20		3086.70	

[1] One-tailed p-value. All others two-tailed unless noted
* p<.05
** p<.01
***p<.005
Note: Model also contains controls for all four definitional samples. Because these categories are not mutually exclusive, these coefficients are not readily interpretable.

Beyond the effect of case complexity, several controls exerted consistent effects both across definitional samples and in the pooled sample. Private attorneys were associated with a decreased likelihood of attaining a reduction in the severity of the charges during plea negotiations; it is possible that public defense attorneys develop working relationships with prosecuting attorneys, which then can be leveraged into better deals for their clients. Cases eligible for higher fines are more

likely to receive reductions in charge severity; interestingly, this relationship holds even when the dependent variable of any reduction in fine or incarceration severity is changed to only include a reduction in incarceration (not shown). It is possible that the fine severity measure is more sensitive to changes in statutes given the large range of possible sentences for some values of the severity scale (though the fine categories are also wide). Having a higher maximum sentence (risk of incarceration) associated with the most serious charge at filing is generally *not* advantageous; rather than giving attorneys more room to negotiate, it may represent a constraint on discretion, where more serious crimes are less likely to receive any concessions (e.g., the liberation hypothesis). However, this result is consistent in only two of the four definitional samples, so caution is warranted in interpreting this coefficient; it is also inconsistent with some prior literature (Kutateladze, 2017). In general, the more unique charges a case has increases the odds that the severity of the most serious charge will be reduced; it is possible that prosecutors are more willing to negotiate when defendants will also be sentenced for additional charges. This is consistent with Kutateladze (2017), who suggests that this may reflect overcharging practices, or, that cases with more charges offer more "room to move." Similarly, cases with misdemeanor or petty charges included at filing are also more likely to receive severity reductions in the process of plea negotiations. There are few relationships between demographic characteristics and severity reduction, with the exception of the loose offense-based sample. In this broadest of classifications, black defendants are more likely to receive severity reductions; this could reflect a number of phenomena, including racial differences in the rate or

degree of initial overcharging or differences in the quality of evidence against defendants. The lack of demographic predictors is largely consistent with previous findings by Shermer and Johnson (2010), who found support only for certainly jointly held characteristics (e.g., young white females and young Hispanic females).

Count Reductions

Charge severity is not the only reduction mechanism prosecutors have at their disposal. While the most serious charge is very influential on sentencing, the number of counts can also lengthen sentences. Prosecutors can thus offer a benefit to defendants by reducing the number of counts they are facing. As with reduction in charge severity, Tables 20 and 21 report the definition-specific and pooled white collar sample for models predicting cases receiving a reduction in the number of counts between filing and termination.

Table 20. Logistic Regression Predicting Count Reduction, Definitional Samples (Fixed Effects for District)

	Agency				Loose			
	Odds Ratio				Odds Ratio			
Any Codefendant[1]	1.92	*	-		1.43	***	-	
Number of Codefendants[1]	-		1.16	*	-		1.02	*
N	555		555		14,440		14,440	
LR X^2	61.89		62.27		350.18		333.25	
	Yale				Strict			
	Odds Ratio				Odds Ratio			
Any Codefendant[1]	1.36	**	-		1.52	**	-	
Number of Codefendants[1]	-		1.02		-		1.05	*
N	5,523		5,523		3,126		3,126	
LR X^2	136.22		131.08		111.53		109.58	

[1] One-tailed p-value. All others two-tailed unless noted
* p<.05
** p<.01
***p<.005

As with receiving a reduction in severity, having any codefendant is associated with an increased likelihood of receiving a reduction in the number of counts a defendant is facing; this result holds for all four definitions. In contrast to a reduction in charge severity, the number of codefendants is significantly associated with increased odds in receiving a reduction in the number of counts for three out of four definitions. Yet, in the pooled sample, the effect observed is not significant. This suggests there may be some types of cases in which the number of codefendants are associated with better odds of receiving a reduction in counts. Interestingly, the effect for the number of codefendants is significant in both the agency and strict offense sample; these samples are most likely to contain offenses similar to the types of cases in which Albonetti (1994, 1999) originally observed leniency for white collar crime cases with codefendants. These results are again interpreted as partial support for Hypothesis 1.

The effect of many variables in the model are also similar to patterns observed for receiving a reduction in charge severity, with several notable exceptions. Both sex and race are significantly related to the odds of receiving a reduction in the number of counts; defendants who are male and black are more likely to receive this particular type of plea bargain. While this is not consistent with Shermer and Johnson (2010), this is a different type of plea bargaining than examined in their work, and it is possible that, since different discounts are generally not found in the same case, that different considerations drive each type of negotiated plea considered here. Consistent with severity reductions, private counsel is associated with worse outcomes for the defendant.

Table 21. Logistic Regression Predicting Count Reduction, All Definitions (Fixed Effects for District)

	Odds Ratio		Odds Ratio	
Any Codefendant[1]	1.46	***	-	
Number of Codefendants[1]	-		1.01	
Male	1.31	***	1.32	***
Age	1.01	***	1.01	*
Black	1.22	**	1.22	**
Other Race	1.16		1.18	
Married	1.08		1.09	
Private Counsel	0.80	*	0.78	**
Violent Offense	9.71	***	9.08	***
Drug Offense	1.02		1.02	
Regulatory	3.44	***	3.45	***
Misdemeanor/Petty	0.51	***	0.45	***
Total Counts at Filing	1.01	***	1.01	***
Number of Charges	1.35	***	1.38	***
Risk of Incarceration at Filing	1.02		1.04	*
Risk of Fine at Filing	1.06	***	1.06	***
N	18,090		18,090	
LR X^2	493.63		465.97	

[1] One-tailed p-value. All others two-tailed unless noted
* p<.05
** p<.01
***p<.005
Note: Model also contains controls for all four definitional samples. Because these categories are not mutually exclusive, these coefficients are not readily interpretable.

Violent offenses are much more likely to receive count reduction, as are cases with more counts and more unique charges at filing. Altogether, suggests that more serious offenses are more likely to receive a reduction in the number of counts. Count reduction may be somewhat of a conciliatory offering that prosecutors can offer defendants to encourage them to plead instead of going to trial, but with a lesser benefit than would be achieved through other forms of discounts. Such forms of bargaining may save prosecutors significant effort and uncertainty while simultaneously not affecting sentences significantly (particularly given that sentences for multiple counts may be served concurrently; see also Chapter 5's discussion of sentencing across various types of prosecutor and judicial discounts).

Unfortunately, many of these effects varied across the definitional samples, and caution is warranted in seeking to generalize these results.

Charge Dismissal

The final prosecutorial outcome considered here is whether a case had any charges dropped by the prosecutor (nolle prosequi) or dismissed without prejudice between filing and termination. Tables 22-23 report odds ratios for definitional and pooled samples. For three of the four samples, having any codefendant is associated with an increased likelihood of any charge being dropped or dismissed; in the fourth (agency) sample, a small sample likely overpowered the model, reducing the ability to find significant effects. In the pooled sample (Table 23), the effect of any codefendants is again consistent with the prediction of leniency. For both the definitional subsamples and the pooled sample, the number of codefendants is not related to having any charges dropped or dismissed.

Table 22. Logistic Regression Predicting Charge Dropped or Dismissed, Definitional Samples (Fixed Effects for District)

	Agency			Loose		
	Odds Ratio			Odds Ratio		
Any Codefendant[1]	1.40		-	1.26	**	-
Number of Codefendants[1]	-		1.22	-		1.00
N	276[2]		276	11,312		11,312
LR X^2	26.46		27.51	640.65		634.44
	Yale			Strict		
	Odds Ratio			Odds Ratio		
Any Codefendant[1]	1.74	***	-	1.46	*	-
Number of Codefendants[1]	-		1.02	-		0.99
N	4,012		4,012	2,226		2,226
LR X^2	310.47		296.73	163.97		161.15

[1] One-tailed p-value. All others two-tailed unless noted
[2] This sample is dramatically smaller as a result of districts lost for lack of variation in the dependent variable
* p<.05
** p<.01
***p<.005

Drug cases are more likely to have some charges dismissed, as are those cases that include at least one misdemeanor or petty charge. Counts and the number of unique charges are also associated with the odds of prosecutorial leniency, suggesting that when prosecutors have more to bargain with, they are more likely to engage in negotiated pleas; this could be reflection of a greater number of charges/counts that could possibly be reduced or dropped, or could reflect a desire by prosecutors to not reduce cases below a level of certain punishment.

Table 23. Logistic Regression Predicting Dropped/Dismissed Charges, All Definitions (Fixed Effects for District)

	Odds Ratio		Odds Ratio	
Any Codefendant[1]	1.36	***	-	
Number of Codefendants[1]	-		1.00	
Male	1.01		1.01	
Age	1.01	*	1.00	
Black	1.02		1.02	
Other Race	1.09		1.10	
Married	1.96		0.96	
Private Counsel	0.46	***	0.46	***
Violent Offense	<0.01		<0.01	
Drug Offense	3.69	***	3.41	**
Regulatory	1.15		1.13	
Misdemeanor/Petty	1.88	***	1.71	***
Total Counts at Filing	1.01	***	1.01	***
Number of Charges	1.94	***	1.98	***
Risk of Incarceration at Filing	0.94	***	0.96	*
Risk of Fine at Filing	1.01		1.01	
N	14,512		14,512	
LR X^2	870.35		856.41	

[1] One-tailed p-value. All others two-tailed unless noted
* p<.05
** p<.01
***p<.005
Note: Model also contains controls for all four definitional samples. Because these categories are not mutually exclusive, these coefficients are not readily interpretable.

As with reductions in charge severity, demographics have little effect on this outcome. The risk of incarceration and fine at filing exhibit a different pattern than for either other plea discount; fine risk has no effect on dropped charges

while incarceration risk has a small suppressive effect on the likelihood of dropped charges.

CONCLUSION

The results presented in this chapter offered moderate support for Hypothesis 1, that case complexity as measured by codefendants would be associated with more lenient prosecutorial outcomes. As shown in Table 24, there is consistent evidence across measure and definition that cases with codefendants are treated more leniently by prosecutors; they are more likely to receive reductions in the severity of the most serious charge, more likely to receive a reduction in counts, and more likely to have at least one charge dropped or dismissed without prejudice. This is somewhat consistent with qualitative evidence that suggested that case complexity would place a greater burden on prosecutors and result in a desire to move cases through the system more quickly by opting to negotiate convictions with defendants through charge and count bargaining and dropped or dismissed charges.

Table 24. Summary of Hypothesis 1 Coefficients for Codefendants

		Agency	Loose	Yale	Strict	Pooled
Presence	Severity Reduction	+	+	+	NS	+
	Count Reduction	+	+	+	+	+
	Dropped/ Dismissed	NS	+	+	+	+
Number	Severity Reduction	NS	NS	NS	NS	-
	Count Reduction	+	+	+	+	NS
	Dropped/ Dismissed	NS	NS	NS	NS	NS

- = Less likely to receive outcome
+ = More likely to receive outcome
NS = Coefficient not significant

However, there was very little support for Hypothesis 1 using the *number* of codefendants. It is possible that the involvement of *any* codefendants is the more important factor in determining which cases will require more work on behalf of

prosecutors. It is also possible that cases with multiple codefendants offer prosecutors the opportunity to pit one defendant against another, offering better pleas to get defendants to cooperate against other codefendants. In that case, the number of codefendants would not be expected to matter so much as the incidence.

Several control variables also showed interesting patterns of findings. Having a private attorney, for example, was associated with decreased charge severity (not shown). However, once filed, private counsel was generally *not* effective in securing more advantageous plea agreements for their clients – in fact, they were often less able to do so. This suggests that private attorneys may be beneficial to defendants at initial filing (analyses not shown; this effect may reflect differences in the type of cases involving defendants who can afford private attorneys), but become a liability thereafter. Private counsel may be most effective for white collar defendants before charging by engaging in negotiations with prosecutors and restricting access to potentially damaging information. The number of charges is typically associated with an increase in the likelihood of each of the three bargaining outcomes. The two measures of severity (risk of fine and incarceration) are not consistently associated with prosecutorial outcomes; higher risk of fines is associated with an increased likelihood of count and charge reductions, while higher risk of incarceration has inconsistent effects. It is also worth noting that there is variation across the definitional samples in some of these effects. Some of this is likely due to small sample sizes (especially the agency sample) but this suggests that there is variation in prosecutorial decision-making depending on the types of offenses in question. Chapter 7 next explores sentencing

outcomes, focusing on the effect of prosecutorial decisions on sentencing, white collar leniency and liability, and whether definition matters for substantive conclusions.

CH. 7 SENTENCING WHITE COLLAR CRIME

INTRODUCTION

This chapter explores four primary questions. First, what direct and/or indirect effects does prosecution have on sentencing outcomes? Second, do white collar crimes (as identified through the agency definition and strategy) receive leniency in sentencing outcomes, compared to non-white collar crimes? Third, do findings of white collar leniency (or liability) vary depending on the definition of white collar crime employed? Finally, do white collar crimes receive an additional benefit of prosecutorial leniency at sentencing – i.e., are pleas, count and charge reduction, and having charges dropped or dismissed more influential for white collar crimes, above and beyond any benefit at sentencing in general? This chapter answers each in turn, using samples that include both white collar and comparable traditional crimes.

THE PROSECUTOR'S INFLUENCE ON SENTENCING

Recent reviews of sentencing research have urged sentencing scholars to think beyond judges as primary architects of sentences (Baumer 2013; Ulmer 2012). The arguments laid out by these reviews suggest that the legal influences on sentence outcomes (primarily the charge at sentencing) are themselves the product of multiple decisions earlier in the criminal justice process. As such, Hypothesis 2 predicts that prosecutorial decisions earlier in the criminal justice process, specifically pleas, charge or count reductions, and dismissals of some but not all charges, will have an indirect effect on sentence severity via legal characteristics. However, several theories (notably typescripts but also attribution theories) suggest

that prosecutors' decisions may also act as *signals* to judges about the relative deservedness of the case for lenient treatment. Accordingly, Hypothesis 3 suggests that prosecutorial decisions to bargain or selectively dismiss some charges may still have a direct effect on sentencing controlling for legally relevant variables.

To test these hypotheses, I conduct three related sets of regressions (full models in the Appendix, A4): the first includes the four prosecutorial outcomes *without* the guidelines offense seriousness score in the model – this is the unconditioned (or, unmediated) effect of these variables on sentencing. The second set of regressions predicts the guidelines offense score, establishing a relationship between the prosecutorial outcomes and the mediator. The third set includes both the prosecutorial outcomes and the guidelines offense score as mediator. These regressions are used to calculate the total, direct, and indirect effect of prosecutorial variables, following MacKinnon and Dwyer (1993). If the prosecutorial variables are significant in the final set of equations, and the proportion of the total effect explained is less than 1.00, Hypothesis 3 will be supported. If prosecutorial outcomes are significant in the first and second set of equations but not significant in the third *and* the proportion of the unconditioned effect is substantively large, this is consistent with an indirect effect of prosecutorial negotiation via guidelines offense score. To avoid repetition with the next section and to focus on the mediation, only the rescaled coefficients are provided. Table 25 provides the unmediated or total (c), direct (c') and indirect (a*b) effects of the four prosecutorial outcomes for each of the four sentence outcomes. If the unmediated effect (e.g., charge reduction without the inclusion of offense score) is significant

and the direct path (charge reduction with offense score) is *not* significantly different from zero, this is considered complete mediation. If, however, the unmediated effect is statistically significant and the direct effect is nonzero, the "Percent Mediated" column provides the ratio of the unmediated path explained by the indirect effect of prosecutor variables through the guidelines offense score. These numbers are drawn from the coefficients of fixed effect logistic regressions using the agency sentencing sample that are then standardized to account for differences in residual variation across models.

Table 25. Direct and Indirect Paths for Prosecution on Sentencing, Agency Sample (Fixed Effects for District)

	c		a*b		c'		Percent Mediated[1]
Discretionary Prison							
Plea	0.074		0.001		0.083		
Drop/Dismissed	-0.104		-0.003		-0.099		
Count Reduction	0.201	*	-0.003		0.219	*	1.4%
Severity Reduction	0.025	*	-0.004		0.036	*	15.9%
Judicial Departure							
Plea	-0.060	*	-0.003		-0.047	*	4.3%
Drop/Dismissed	-0.039	*	-0.001		-0.034	*	3.0%
Count Reduction	-0.103	*	0.003		-0.122	*	-3.3%
Severity Reduction	-0.023	*	<|0.001|		-0.022	*	1.6%
5K Departure							
Plea	0.013		-0.003		0.024		
Drop/Dismissed	0.005		-0.002		0.012		
Count Reduction	-0.138	*	0.005		-0.162	*	-3.8%
Severity Reduction	0.013		<|0.001|		0.014		
Any Departure							
Plea	-0.036	*	-0.003		-0.020		Complete
Drop/Dismissed	-0.014		-0.001		-0.007		
Count Reduction	-0.099	*	0.004		-0.121	*	118.1%[52]
Severity Reduction	-0.007		-0.001		-0.005		

[1] Calculated as the ratio of the indirect effect (a*b) to the full effect (c)
*p<.05

[52] When percent mediated is greater than 100%, this suggests a "suppression effect" (MacKinnon 2000). In this case, the addition of the guidelines offense score *increases* the predictive power of the independent variable; when failing to control for this in the unmediated regression, the actual relationship is "suppressed."

Variables not shown: white collar, Substantial Assistance (discretionary prison only), eligible for non-prison sentence, criminal history points, male, black, other race, age, married, high school graduate, any college, other education, any codefendant, private counsel, violent crime, drug crime, regulatory offense, number of charges, presentence detention, any dependent, acceptance of responsibility, amendment year (pre-2003, 2003-2007, 2008, 2009, 2010), amount of financial penalties, risk of incarceration at charging (offense score only)

The results suggest that both the indirect and direct effects of prosecution on sentencing outcomes vary significantly based on the outcome considered. Looking at the column with unmediated effects, count reduction emerges as positively associated with imprisonment and negatively associated with the receipt of departures. This is not surprising given the findings that cases with more counts and charges were more likely to receive count reduction (Chapter 6). Based on these findings, it seems that more serious cases are more likely to receive count reduction. In a mediated model, the guidelines offense score should mitigate the effect of count reduction. For all four outcomes, count reduction continues to exert a direct effect (c'). However, only for the outcome of "any departure" is the unmediated effect of count reduction largely explained by the guidelines offense score; otherwise, the percent of the total effect explained is small. Judicial departures seem to be more affected by prosecutorial outcomes than any other sentencing outcome considered. All four prosecution outcomes have significant total and direct effects. Further, there is little evidence for an indirect effect – the proportion of the total effect explain by the mediator is very small. For this outcome only, there is consistent evidence in favor of Hypothesis 3, that prosecutorial outcomes should have a direct signaling effect on sentencing outcomes. This is not entirely surprising. If prosecutorial outcomes earlier in the process should affect sentencing, it makes sense that the outcomes evidencing signaling are those in which judges

exercise the most discretion (i.e., departing from the guidelines). Full models, including odds ratios, other variables, and model fit statistics, are provided during the discussion of Hypotheses 4-6 (below).

Overall, this is not strong support for Hypothesis 2, and only partial support for Hypothesis 3. It is possible that prosecutorial variables do not matter for departures or the in/out decision, but rather matter for length. It could be that because prosecutorial discounts are primarily given to more serious cases, they may have more effect on sentencing outcomes focused on punitiveness, and (at least for incarceration) in samples involving more serious crimes. Results for each of the other three definitional samples are provided in the Appendix. An alternative method of mediation analysis, the Karlson-Holms-Breen (KHB) method, also produces similar conclusions; these results are also provided in the Appendix (A5).

WHITE COLLAR LENIENCY/LIABILITY

Hypothesis 4 suggests that white collar crimes should be treated differently than comparable crimes, either receiving leniency or liability. As discussed previously, in this dissertation, two measures of "leniency" are considered: (a) the avoidance of a prison term among cases which are structurally eligible for either prison or a non-prison sentence and (b) a sentence less than structurally prescribed either through a downward departure, downward variance, or prosecutor-led government motion. Table 41 shows the odds ratios for the agency sample (both white collar and comparable crimes) across these four outcomes. Odds ratios below one are indicative of a lower likelihood of receiving a given outcome, while odds ratios above one are indicative of a higher likelihood of receiving that outcome.

Hypothesis 4 will be supported if the odds ratio for the nominal variable "white collar crime" is significantly different than one. White collar leniency will be suggested if white collar cases have a lower likelihood of discretionary incarceration and a higher likelihood of downward departures, while liability will be suggested if white collar cases have a greater likelihood of receiving discretionary incarceration and a decreased likelihood of downward departures.

As shown in Table 26, white collar cases are significantly less likely to go to prison for those cases in which a non-prison sentence is structurally possible. In addition, white collar cases are significantly more likely to receive judicial departures or any departure. This is strong support for Hypothesis 4, suggestive of leniency. Cases with count reduction are in fact more likely to go to prison and less likely to receive departures, suggesting that these cases may be subject to real offense sentencing. However, most prosecutorial outcomes had little evidence of a direct effect on the imposition of a prison sentence of the receipt of a departure. Cases that were already eligible to receive a non-prison sentence were approximately 80% less likely to receive a departure either from a judge as well as overall, suggesting that judges prefer to use structurally available less punitive outcomes before departures or variances. That these cases are also less likely to receive prosecutor-initiated departures also suggests that 5k agreements more likely to be given to more serious cases either because these defendants are more likely to be able to give information on other criminal activity or because 5Ks are used instrumentally by prosecutors to purposefully reduce offender sentences.

Men are more likely to receive discretionary prison than women consistent with traditional sentencing work (Bontrager et al. 2013), and less likely to receive leniency through departures. Education appears to influence only judicial discretionary decisions to impose a prison sentence within the guidelines, but not the likelihood of receiving a departure. There is no difference in this sample in the likelihood of going to prison by race. This is consistent with a previous meta-analysis finding that federal sentencing data are less likely to show racial disparity, though generally property offenses are more likely to show disparities (Mitchell 2005). Each additional unique charge increases the odds of incarceration in a non-prison eligible case by 24%, and decreases the odds of judicial or any departures by 7-8%. Private counsel is associated only with the odds of achieving a substantial assistance departure, suggesting that, in spite of potentially undeveloped workgroup relationships with prosecutors, they are still able to negotiate some favorable agreements for their clients. Acceptance of responsibility also increases the odds of receiving a substantial assistance departure, those this relationship is likely reflective of the requirements of these motions. Departures are less likely for cases sentenced under Amendment Year 2009 relative to 2011. It is possible that the 2009 guidelines were perceived as fairer than the 2011 guidelines, prompting a decreased desire to impose sentences outside those prescribed. However, it is also possible that this represents a slight temporary uptick in punitiveness after the Great Recession. Future research should explore this relationship in depth.

Table 26. Odds Ratios for Logistic Regression Predicting Any Prison or Departure with Fixed Effects for District (Agency Sample)

	Discretionary Prison		Judge Downward Departure/Variance		5K Departure		Any Departure	
White Collar	0.34	***	1.46	***	1.30		1.40	***
Substantial Assistance	0.98		-		-		-	
Plea	1.38		0.78	*	1.13		0.89	
Dropped/Dismissed Charges	0.66	*	0.83		1.07		0.96	
Count Reduction	2.46	***	0.47	***	0.39	***	0.44	***
Severity Reduction	1.21		0.86	*	1.10		0.96	
Offense Score	1.19	***	1.03	***	1.04	***	1.04	***
Eligible No Prison	-		0.21	***	0.19	***	0.22	***
Criminal History	1.78	***	0.85	***	0.96		0.89	***
Male	1.84	***	0.69	***	0.88	*	0.76	***
Black	0.96		0.99		1.15	*	1.05	
Other Race	1.23		0.78	*	1.08		0.87	
Age	0.97	***	1.01	***	0.99	***	1.00	
Married	0.92		1.10		1.04		1.07	
High School	0.52	***	0.93		0.98		0.93	
Any College	0.56	***	0.98		1.06		0.99	
Other Education	0.43	**	1.18		0.79		0.98	
Any Codefendants	0.93		1.06		2.05	***	1.38	***
Private Counsel	0.85		0.88	*	1.31	***	1.05	
Violent	1.11		0.89		0.41	***	0.62	***
Drug	4.11		0.67		0.57	***	0.57	*
Regulatory Offense	0.57		0.59	*	0.74		0.63	*
Number of Charges	1.24	***	0.92	***	0.93		0.93	***
Presentence Detention	9.53	***	0.48	***	0.41	***	0.47	***
Any Dependents	0.99		0.97		1.10		1.02	
Accept Responsibility	0.84		1.00		1.51	***	1.15	***
AY Pre-2003[1]	0.37		1.23		1.43		1.09	
AY 2003-2007	0.61		0.72		0.54		0.66	
AY 2008	0.83		0.89		0.93		0.91	
AY 2009	0.97		0.87	*	0.83	*	0.86	***
AY 2010	1.13		0.99		0.96		0.98	
Amount of Financial Penalties	1.00	***	1.00		1.00		1.00	
N	5,090		13,301		12,228		15,923	
LR X^2	1835.31		1784.99		2186.42		2844.20	

[1] AY= Amendment Year of the Guidelines
* p<.05
** p<.01
*** p<.005

The amount of financial penalties imposed affects only incarceration. The approximate coefficient is 0.99999, suggesting that for each additional dollar, the likelihood of incarceration decreases by 0.001% While this appears to be a small effect that could be driven by sample size, this effect size translates to large odds for the average, and even the 25th and 50th percentile of financial penalties. Supplemental analyses (not shown) tested for a potential quadratic effect of the financial penalties imposed; these results suggest that each additional dollar imposed on a defendant has an initial increase in the likelihood of going to prison, but at high amounts (most fines), fines reduce the likelihood of incarceration at an increasing rate.

Does Definition Matter?

While there is evidence that white collar cases are treated differently than comparable crimes based on the previous analysis, as discussed previously, the evidence for white collar leniency at sentencing has historically been mixed. One possible explanation is that the definitions that have been employed vary in the crimes included to such an extent that they create differences in the likelihood of observing leniency. Hypothesis 5 predicted that not only would there be variability in the likelihood of observing leniency across definitions, but also that definitions which were more mindful of Sutherland's definition by attempting to focus on crimes involving power in the course of an occupation, would produce more lenient outcomes at sentencing. Tables 27-29 present results for the same models discussed above, but for the different definitional samples.

Cases that meet the label of "white collar" according to the loose offense-based definition are all non-violent property offenses. This definition is sometimes referred to as the Edelhertz (1978) definition, and is commonly used by law enforcement. As shown in Table 27, cases meeting the loose-offense based definition are not significantly more or less likely to receive a prison term as part of their sentence. However, they may be more likely to receive prosecutor-led or any departure.

Together, this offers mixed evidence for leniency or liability, and offers preliminary support for Hypothesis 5. Otherwise, the findings are generally consistent with the agency case sample. White collar crime is again significantly related to sentencing outcomes for the Yale offense-based sample (Table 28), though in the opposite direction. White collar crimes as defined in this sample are 26% more likely to receive prison when non-prison sentences are structurally allows and are less likely to receive downward departures or variances. The effect of controls, however, are highly consistent with other definitional samples. Finally, Table 29 displays results for the strict offense-based definition. These offenses were selected with particular attention to types of crimes that might be committed in the course of a legitimate occupation. White collar crime in this sample is treated differently than comparable crimes in the direction of leniency for three of the four outcomes

Table 27. Odds Ratios for Logistic Regression Predicting Any Prison or Departure with Fixed Effects for District (Loose Sample)

	Discretionary Prison		Judge Downward Departure/Variance		5K Departure		Any Departure	
White Collar	0.93		1.12		1.16	*	1.18	***
Substantial Assistance	1.00		-		-		-	
Plea	1.24		0.80	*	1.07		0.89	
Dropped/Dismissed Charges	0.54	***	0.78	*	1.05		0.93	
Count Reduction	2.34	***	0.47	***	0.41	***	0.44	***
Severity Reduction	1.23		0.91		1.12		1.00	
Offense Score	1.19	***	1.03	***	1.05	***	1.04	***
Eligible Non Prison	-	-	0.21	***	0.20	***	0.22	***
Criminal History	1.74	***	0.84	***	0.96	*	0.89	***
Male	1.76	***	0.69	***	0.88	*	0.76	***
Black	0.93		0.95		1.14	*	1.03	
Other Race	1.19		0.78	*	1.10		0.89	
Age	0.97	***	1.00	*	0.99	***	1.00	
Married	0.94		1.10	*	1.02		1.06	
High School	0.54	***	0.94		0.96		0.94	
Any College	0.57	***	0.99		1.04		1.00	
Other Education	0.45	***	1.24		0.75		1.01	
Any Codefendants	0.93		1.06		1.94	***	1.35	***
Private Counsel	0.83		0.88	*	1.31	***	1.05	
Violent	0.93		1.00		0.46	***	0.71	***
Drug	-		-		-		-	
Regulatory Offense	-		-		-		-	
Number of Charges	1.23	***	0.93	***	0.93	***	0.94	***
Presentence Detention	9.24	***	0.48	***	0.41	***	0.47	***
Any Dependents	0.97		0.96		1.10		1.01	
Accept Responsibility	0.79	*	1.00		1.53	***	1.16	***
AY Pre-2003[1]	0.39		1.51		1.04		1.14	
AY 2003-2007	0.60		0.81		0.51	*	0.72	
AY 2008	0.82		0.91		0.97		0.94	
AY 2009	1.04		0.90		0.86	*	0.89	*
AY 2010	1.11		1.00		0.99		1.00	
Amount of Financial Penalties	1.00	***	1.00		1.00		1.00	
N	5,432		14,172		13,023		16.876	
LR X^2	1924.75		1892.83		2219.58		2974.18	

[1] AY= Amendment Year of the Guidelines
* p<.05
** p<.01
*** p<.005

Table 28. Odds Ratios for Logistic Regression Predicting Any Prison or Departure with Fixed Effects for District (Yale Sample)

	Discretionary Prison		Judge Downward Departure/Variance		5K Departure		Any Departure	
White Collar	1.26	*	0.86	***	0.76	***	0.81	***
Substantial Assistance	0.95		-		-		-	
Plea	1.34		0.80	*	1.17		0.92	
Dropped/Dismissed Charges	0.65	*	0.75	*	0.99		0.89	
Count Reduction	2.59	***	0.47	***	0.39	***	0.44	***
Severity Reduction	1.15		0.89		1.09		0.98	
Offense Score	1.19	***	1.02	***	1.05	***	1.04	***
Eligible No Prison	-		0.20	***	0.20	***	0.21	***
Criminal History	1.79	***	0.83	***	0.96	*	0.88	***
Male	1.84	***	0.69	***	0.89	*	0.76	***
Black	0.96		0.96		1.12		1.02	
Other Race	1.21		0.83		1.07		0.90	
Age	0.97	***	1.01	***	0.99	***	1.00	
Married	0.89		1.12	*	1.06		1.09	*
High School	0.52	***	0.93		0.94		0.92	
Any College	0.56	***	0.97		1.03		0.98	
Other Education	0.43	***	1.20		0.80		1.00	
Any Codefendants	0.94		1.04		2.00	***	1.36	***
Private Counsel	0.81		0.95		1.33	***	1.10	
Violent	1.00		0.87		0.38	***	0.59	***
Drug	-		-		-		-	
Regulatory Offense	-		0.23		1.56		0.62	
Number of Charges	1.24	***	0.93	***	0.93	***	0.94	***
Presentence Detention	9.36	***	0.49	***	0.41	***	0.47	***
Any Dependents	1.00		1.00		1.12	*	1.04	
Accept Responsibility	0.87		1.03		1.49	***	1.16	***
AY Pre-2003[1]	0.38		1.29		1.53		1.16	
AY 2003-2007	0.62		0.78		0.53		0.70	
AY 2008	0.81		0.93		0.97		0.94	
AY 2009	1.00		0.88	*	0.85	*	0.87	**
AY 2010	1.12		0.99		0.97		0.98	
Amount of Financial Penalties	1.00	***	1.00		1.00		1.00	
N	5,031		13,482		12,252		16,128	
LR X^2	1784.23		1887.99		2137.04		2888.89	

[1] AY= Amendment Year of the Guidelines
* p<.05
** p<.01
*** p<.005

Table 29. Odds Ratios for Logistic Regression Predicting Any Prison or Departure with Fixed Effects for District (Strict Sample)

	Discretionary Prison		Judge Downward Departure/Variance		5K Departure		Any Departure	
White Collar	0.82	*	1.17	**	1.02		1.13	*
Substantial Assistance	0.99		-		-		-	
Plea	1.38		0.78	*	1.08		0.88	
Dropped/Dismissed Charges	0.66	*	0.79	*	1.03		0.92	
Count Reduction	2.47	***	0.47	***	0.40	***	0.44	***
Severity Reduction	1.16		0.90		1.10		0.98	
Offense Score	1.19	***	1.03	***	1.05	***	1.04	***
Eligible No Prison	-		0.22	***	0.20	***	0.22	***
Criminal History	1.80	***	0.84	***	0.96		0.89	***
Male	1.83	***	0.69	***	0.89		0.76	***
Black	0.97		0.97		1.16	*	1.04	
Other Race	1.21		0.78	*	1.06		0.87	
Age	0.97	***	1.01	***	0.99	***	1.00	
Married	0.90		1.11	*	1.04		1.07	
High School	0.52	***	0.94		0.95		0.92	
Any College	0.56	***	0.98		1.02		0.98	
Other Education	0.42	***	1.25		0.78		1.01	
Any Codefendants	0.92		1.07		2.03	***	1.39	***
Private Counsel	0.83		0.89		1.31	***	1.06	
Violent	0.91		0.91		0.41	***	0.62	***
Drug	-		-		-		-	
Regulatory Offense	0.23	*	0.67		1.24		0.78	
Number of Charges	1.24	***	0.92	***	0.93	***	0.93	***
Presentence Detention	9.34	***	0.50	***	0.40	***	0.47	***
Any Dependents	0.99		0.97		1.11		1.02	
Accept Responsibility	0.85		1.01		1.52	***	1.16	***
AY Pre-2003[1]	0.36		1.24		1.41		1.10	
AY 2003-2007	0.63		0.78		0.51	*	0.68	
AY 2008	0.79		0.92		0.96		0.93	
AY 2009	0.96		0.89		0.84	*	0.87	**
AY 2010	1.11		1.01		0.97		0.99	
Amount of Financial Penalties	1.00	***	1.00		1.00		1.00	
N	5,009		13,025		11,980		15,582	
LR X^2	1801.41		1733.90		2141.27		2771.05	

[1] AY= Amendment Year of the Guidelines
* p<.05
** p<.01
*** p<.005

Taken together, these results offer support for Hypothesis 5, which predicted that stricter definitions of white collar crime would be more likely to produce findings of leniency at sentencing. Table 30 offers a summary of the significance and direction of white collar crime coefficients for each of the four samples.

Table 30. Summary of White Collar Leniency/Liability Findings

	Discretionary Prison	Judge Downward Departure/Variance	5K Departure	Any Departure
Agency White Collar	-	+	NS	+
Loose White Collar	NS	NS	+	+
Yale White Collar	+	-	-	-
Strict White Collar	-	+	NS	+
- = Less likely to receive outcome + = More likely to receive outcome NS = Coefficient not significant				

Both the agency and strict offense-based definitional samples suggested that white collar samples were treated differently than traditional crimes, and, consistent with Hypothesis 5, the pattern of results suggests leniency. Under these definitions, white collar crimes were less likely to receive prison when they were eligible for a non-prison sentence, relative to comparable crimes; these white collar cases were also more likely to receive judicial departures and variances resulting in sentences less severe than prescribed by the guidelines. However, white collar crimes were not more likely to receive prosecutor-led departures through 5K Substantial Assistance departures. In contrast, the Yale white collar definition was consistently associated with liability, with white collar cases in this sample more likely to receive prison when eligible for a less severe sanction and less likely to receive

departures across all three measures. The loose white collar definition offered some evidence of neither leniency nor liability for white collar offenses, though white collar cases under this definition were more likely to receive substantial assistance departures or any departure.

Several other variables exerted consistent effects across definitional samples. Presentence detention exerted strong positive effects on the likelihood of receiving a prison sentence, while suppressing the likelihood of receiving a departure across each of the four samples. Men were more likely to go to prison, with little evidence of racial differences in the allocation of a prison term (consistent with federal findings in Mitchell 2005, however see also Ulmer et al. 2011). Education demonstrated protective effects, but not uniformly across outcomes, appearing to affect only incarceration. Criminal history operates as might be expected, with each additional point associated with an increased likelihood of a prison sentence relative to a non-prison sentence and a decrease in the odds of receiving a departure by any means. However, the offense score exhibits a different pattern of effects; among those eligible for a non-prison sentence, each additional point in the offense score is associated with approximately a 19% increase in the odds of receiving an incarceration sentence. Yet, each additional point is also associated with a small increase in the odds of receiving departures. It is possible that the significance of this coefficient is an artifact of a large sample size;

alternatively, as offense seriousness increases, judges may seek to avoid increasingly harsh penalties.[53]

The number of charges and the number of counts are generally associated with harsher outcomes, both in an increased likelihood of receiving prison and a decreased likelihood of receiving downward departures relative to being sentenced within guideline range or above. This might reflect either real offense sentencing, in which judges take into account the original number of counts (which are generally higher for those cases receiving count reduction; see Chapter 6) or a signal that these cases have already receive leniency and are not prime candidates for further leniency. Pleas are generally only related to leniency through judicial departures (but not 5K departures), either reflecting a plea discount/trial penalty, or suggesting that prosecutors may engage in sentencing negotiations with judges as part of plea negotiation.

Do Prosecutorial Decisions Benefit White Collar Cases More than Comparable Crimes?

Finally, Hypothesis 6 predicted that prosecutorial outcomes, specifically pleas, reductions in counts, charge severity, and dropped or dismissed charges, should have a larger effect for white collar cases due to the early involvement of

[53] Supplemental analyses (not shown) considered the argument that status liability is only an issue for serious crimes by separately considering an interaction term between white collar status and crime seriousness using the guidelines offense score and also considering a quadratic offense severity term interacting with white collar status. Both the quadratic term and the interacted quadratic term were significant for all departure samples. In brief, the results suggested that additional increases in offense severity increased the odds of receiving a departure, however, there was a small decline in these increased odds as offense score continued to rise. These declining marginal effects were largely mitigated by white collar status, with a similar in magnitude increase in odds of departure. This is inconsistent with the argument of status liability, which would predict a *sharper* decline in leniency for white collar offenses.

the prosecutor; this may produce conditions of "negotiated justice" that favor white collar defendants. Table 31 reports the odds ratios for the interaction terms only for the four definitional samples. Other variables are omitted for the sake of parsimony but are provided in the Appendix.

Table 31. Odds Ratios for Logistic Regression Predicting Any Prison or Departure with Fixed Effects for District – Interaction Terms

	Discretionary Prison		Judge Downward Departure/Variance		5K Departure		Any Departure	
Agency Sample								
Plea* White Collar[1]	-		-		-		-	
Dropped/Dismissed Charges* White Collar	0.40		3.50	**	3.11	*	2.80	*
Count Reduction* White Collar	2.03		1.48		1.15		1.39	
Severity Reduction* White Collar	2.64		0.47	*	0.66		0.58	*
N	5,090		13,301		12,228		15,923	
LR X^2	1838.78		1798.87		2192.07		2856.12	
Loose Offense-Based Sample								
Plea* White Collar	0.66		0.73		0.50		0.68	
Dropped/Dismissed Charges* White Collar	1.31		1.09		1.09		1.11	
Count Reduction* White Collar	0.47		0.78		0.52		0.67	***
Severity Reduction* White Collar	0.64		1.02		0.82	***	0.93	
N	5,439		14,172		13,023		16,876	
LR X^2	1934.70		1896.26		2233.99		2984.39	
Yale Offense-Based Sample								
Plea* White Collar	1.96		0.82		0.65		0.75	
Dropped/Dismissed Charges* White Collar	0.74		0.77		0.81		0.77	
Count Reduction* White Collar	3.05		0.67	*	0.82		0.70	*
Severity Reduction* White Collar	1.12		1.16		0.89		1.04	
N	5,039		13,482		12,252		16,128	
LR X^2	1794.98		1865.82		2141.20		2898.30	

Strict Offense-Based Sample								
Plea* White Collar	0.11	***	0.64		0.60		0.63	
Dropped/Dismissed Charges* White Collar	0.55		1.12		1.00		0.97	
Count Reduction* White Collar	1.06		0.73		0.90		0.75	
Severity Reduction* White Collar	0.41	***	1.20		1.02		1.08	
N	5,017		13,025		11,980		15,582	
LR X^2	1825.78		1739.70		2143.10		2776.44	
[1] Omitted because only one white collar cases resolved without a plea * p<.05 ** p<.01 *** p<.005								

Several things emerge from this table of results. First, there is notable variation across definition, with more evidence of a white collar benefit for prosecution outcomes for departures rather than imprisonment. To better demonstrate these relationships, Figures 7-8 show the average predicted probabilities using the agency sample for eight types of cases: white collar cases with charges dropped or dismissed, white collar cases without charges dropped or dismissed, non-white-collar cases with charges dropped or dismissed, and non-white-collar cases without charges dropped or dismissed.[54]

[54] Predicted probabilities are illustrative only and are generated assuming the fixed effect is zero, which is likely inaccurate.

Figure 7. Predicted Probabilities of Departures

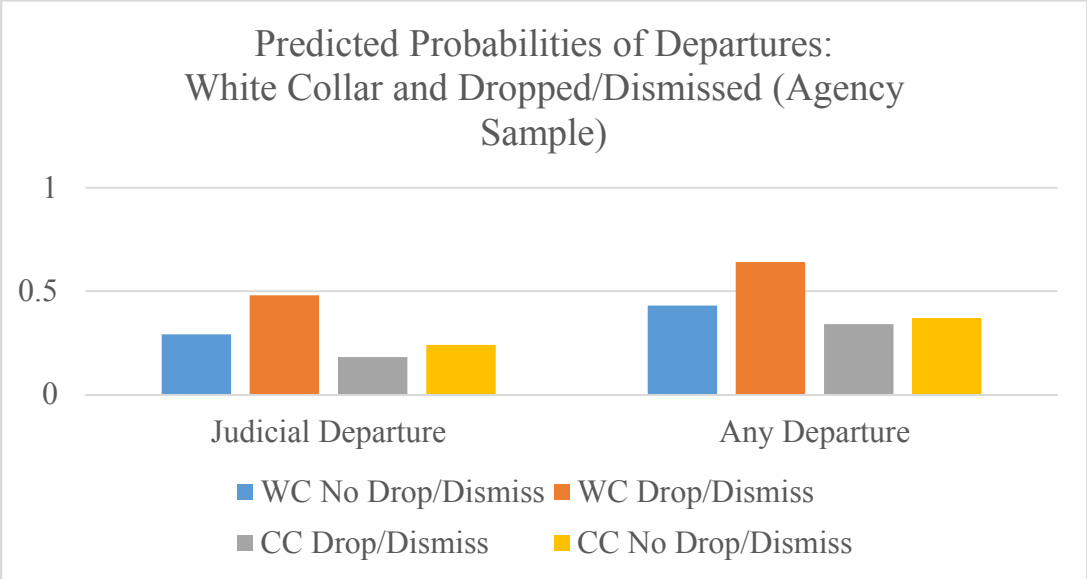
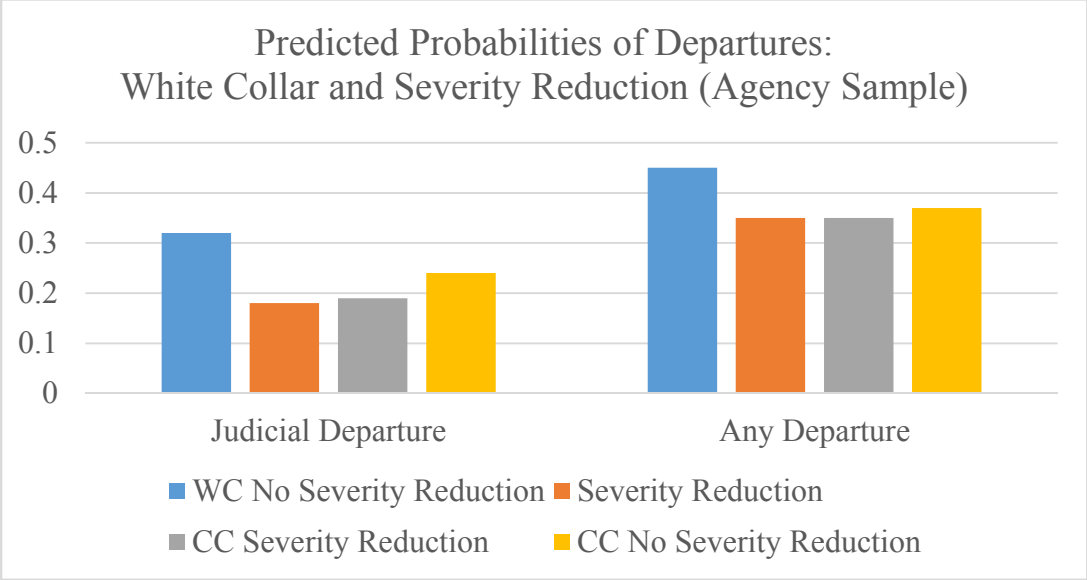


Figure 8. Predicted Probabilities of Departures



Based on the predicted probabilities, there is some evidence of additional leniency for white collar cases. White collar cases with dropped or dismissed charges are more likely to receive both judicially initiated departures and departures from any source. However, in spite of significant coefficients, there is little practical

difference between white collar and non-white-collar benefits for severity reductions; additionally, severity reduction generally disadvantages defendants when it comes to receiving departures. These predicted probabilities offer additional insight to the findings of leniency from the previous hypotheses. However, these findings are only suggestive as there are few significant interactions between prosecutorial outcomes and being a "white collar" case. Overall, these results are interpreted as providing little support for Hypothesis 6.

CONCLUSION

This chapter explored whether prosecutor outcomes had a direct or indirect effect on sentencing outcomes. Results from these analyses suggest that there is little evidence of an indirect effect of prosecutorial outcomes via their influence on guidelines offense scores. While prosecutorial outcomes have strong relationships to the ultimate guidelines offense score, this relationship mediates a very small percent of the total effect of prosecutorial variables on sentencing outcome. Thus, Hypothesis 2 is generally unsupported. However, Hypothesis 3, which predicted a direct signaling effect of prosecution outcomes on sentencing, received some support, especially when focused on the outcome of judicial departures. Overall, leniency for prosecutorial outcomes (i.e., receiving reductions in counts or charges, any charges dropped) was associated with less lenient judicial outcomes; while not always significant, cases that received discounts from prosecutors generally were more likely to be sentenced to prison when eligible for a non-prison sentence and were less likely to receive departures. Count reduction, in particular, was consistently associated with less lenient outcomes. Rather than signaling that these cases should be treated leniently, this could be a function of the fact that more

serious cases are more likely to receive discounts. Judges, either due to "real offense" sentencing or recognizing that these cases have already benefited from leniency, are less likely to offer cases with negotiated pleas additional leniency. However, because not all prosecution variables were significant across all sentence outcomes, this is interpreted as only partial support for Hypothesis 3.

This chapter also considered whether white collar crimes were treated differently relative to comparable crimes (Hypothesis 4), whether this leniency/liability finding varied based on the definition of white collar crime used (Hypothesis 5), and whether white collar crimes receive an additional benefit of prosecutorial bargaining relative to non-white-collar crimes (Hypothesis 6). Using the preferred agency definition of white collar crime, Hypothesis 4 was strongly supported; agency white collar cases were less likely to be sentenced to a period of imprisonment than comparable crimes for those both structurally eligible to receive a non-prison sentence; white collar cases were also more likely to receive downward departures overall and specifically initiated by judges. Hypothesis 5 was also supported. White collar leniency was observed for both the agency based definition and the strict offense-based definition, but the Yale definition was largely suggestive of liability, consistent with the findings from the Yale Studies (e.g. Weisburd et al. 1990; Wheeler et al.1982). Interestingly, however, the loose offense-based definition was associated with *both* lack of leniency or liability for incarceration decisions and with an increased likelihood of receiving departures.

Finally, Hypothesis 6 received little to no support. No prosecutorial outcome was significantly more beneficial for white collar offenders across all

outcomes or across all samples for a single outcome. There is some evidence that, using an agency-based definition, white collar cases that receive dropped charges are more likely to receive departures than are similarly situated comparable cases; however, the inconsistency of findings across outcomes and samples suggest these findings should be interpreted cautiously. The next and final chapter of this dissertation discusses the findings for each hypothesis, limitations of this research, and implications for policy and future research.

CH. 8 DISCUSSION AND CONCLUSION

INTRODUCTION

This dissertation responds to recent calls for sentencing scholars to broaden their focus on judges as sole arbiters of sentences. Instead, those interested in sentencing have been urged to look toward prosecutors as structurers of judicial decision-making (Baumer, 2013; Ulmer, 2012). Previous research provides evidence of the importance of prosecutorial decisions, suggesting that mandatory minimums and other offense characteristics that may be negotiated by prosecutors during plea agreements drive sentencing outcomes (e.g., Ulmer et al., 2007).

This dissertation uses data on the federal criminal justice system from the Federal Justice Statistics Program (FJSP); a set of white collar and comparison cases are tracked beginning with referral for prosecution in 2009-2011 through arrest, case filing, case conclusion, and sentencing. In doing so, I addressed three goals: (1) describing the movement of white collar cases through the criminal justice system, (2) exploring the demographic and case characteristics associated with prosecution and sentencing outcomes, and (3) considering the effect of different white collar definitions and identification strategies on substantive conclusions. I developed and tested six hypotheses, focusing on the role of case complexity in plea negotiations, the role of prosecutorial outcomes on sentencing outcomes, white collar leniency in sentencing, and the importance of white collar definition.

DISCUSSION

Overall, the hypotheses were met with mixed support. Hypothesis 1 drew on both white collar-specific and general prosecution research that suggests that prosecutors desire high conviction rates for any of a multitude of personal and group goals (e.g., Eisenstein and Jacobs, 1977; McConville and Mirsky, 2005; Sudnow, 1965); as a result, prosecutors should attempt to minimize uncertainty and resource burden through attractive (discounted) plea offers. Cases that are more complex, especially those with codefendants (Albonetti, 1999), should more likely to receive this prosecutorial leniency. This dissertation considered three different outcomes for prosecutorial leniency (reduction in charge severity, reduction in the number of counts, and any charges dropped or dismissed) and two indicators of case complexity (any and the number of codefendants). Across all three outcomes, the presence of any codefendants was almost uniformly associated with increased likelihood of leniency, while the number of codefendants was associated only with count reduction. It is possible that the marginal benefit (or cost) of an additional codefendant is so small that it does register statistically, however the large sample size suggests that this is unlikely, especially for the loose offense-based sample (n=14,440) or in the pooled sample (n=18,090). If a lack of power is not to blame, the number of codefendants may have no uniform relationship with the odds of receiving a negotiated plea. Why might this be the case? While the burden for prosecutors is larger for cases with multiple defendants, but that the *number* of codefendants may be indicative of offense severity, which might not be expected to relate to leniency (or might be related in the opposite direction); in light of other

controls for offense (charge) severity, the number of codefendants could lose its significance. Additional analyses (not shown) showed that the effect of codefendants is quadratic in nature; each additional codefendant increases the odds of receiving reductions, but the effect diminishes at high numbers of codefendants. This suggests that codefendants generally may be pitted against one another to reduce prosecutorial resource expenditure (resulting in an increase in the odds of bargains), but that offenses involving large numbers of codefendants are not necessarily at an advantage. These more involved schemes may be seen as too serious to bargain, or, these cases may be initiated by single whistleblowers, reducing or eliminating the need to solicit cooperation from charged defendants.

Hypotheses 2 and 3 predicted that prosecutorial decisions to negotiate pleas would have both signaling (direct) and structuring (indirect) effects on sentencing outcomes. In determining the terms of charging and the terms of case conclusion through negotiated pleas, prosecutors have tremendous potential influence on the details of offenses and the seriousness of charges for which offenders are sentenced. Prosecutorial outcomes thus indirectly influence – structure – sentencing outcomes by affecting one of the major determinants of sentencing outcomes in the federal system (the guidelines offense score); this structuring is particularly consequential in the federal system because judges often comply with guidelines prescribed sentences even though not required. However, previous research has also suggested that judges look to the decisions of previous decision-makers to inform their own decisions (e.g., Hill et al., 1974); if this is the case, prosecutorial outcomes will still have an impact even after guidelines relevant variables are controlled (i.e., these

decisions act as "signals" about how judges should treat a case). Hypothesis 2, which predicted an *indirect* effect of prosecutor decisions on sentencing outcomes, was not well supported. Mediation analysis suggested that, while outcomes at the prosecutor stage predict both guidelines severity and sentencing outcomes, and guidelines severity predicted sentencing outcomes, guidelines offense severity mediated only a small proportion of the total effect of negotiated prosecutorial outcomes. In addition, the results suggest that there is a resilient direct effect (in support of Hypothesis 3) of plea discounts; these outcomes matter above and beyond the structuring of guidelines offense factors. This stands in contrast to many of the arguments that motivated this dissertation: that prosecutor's decisions are important for sentencing outcomes because prosecutors, through charging and plea negotiations, significantly influence sentences (especially in guidelines systems). Surprisingly, the direct effects observed were such that receipt of certain prosecutorial discounts was associated with *increased severity*, rather than leniency. One possible explanation is that, because more serious cases tended to receive plea discounts, this reflects "real offense sentencing". In this way, the discounts are not signals at all, but rather capture additional variation in the underlying offense behavior.

These results do not imply that prosecutors' decisions do not matter for sentencing. They may suggest that prosecutors create the most structure for sentences not through negotiation, but through initial charging, consistent with findings by Hagan and Parker (1985), looking at securities violations. Another possibility (and limitation of this research) is that the simple status of a case as

negotiated is not as important as is the degree of discount. Some research has suggested that plea discounts can be thought of in terms of "depth" and "distance"; charges with "depth" have related (lesser) charges that can be offered in plea negotiations, whereas "distance" is the degree of discount received (Wright and Engen, 2006). Future research should unpack the role of distance traveled in plea agreements in sentencing outcomes (admittedly, the available charge severity measure is not ideal) in white collar cases. Relatedly, this research did not address whether white collar cases were more likely to receive negotiated pleas, or if they received more favorable terms. Research on racial disparity has suggested that defendants are differentially likely to receive negotiated pleas, and the terms of those pleas similarly vary across groups (e.g., Kutateladze et al., 2016). Prior work on white collar prosecution, as well as prosecution literature more broadly, suggest that white collar cases should receive better agreements than similarly situated comparable crimes because of the cooperative investigations required for white collar crimes and the desire to reduce resource burden and conviction uncertainty associated with unfamiliar and often complex offense types. While there was no additional benefit in the signaling effect of negotiated pleas on sentencing leniency for white collar crime (Hypothesis 6, discussed further below), this does not preclude the possibility that the structuring effect is larger for white collar offenders (i.e., that there is a greater magnitude of discount); future research should consider this "distance traveled" approach to white collar leniency in prosecution. It is also important to note that in focusing on discretionary incarceration and departures, the

effect of bargains on structure as related to sentence *length* may is not fully considered.

Hypothesis 4 drew on a wide variety of both empirical and theoretical work that suggested that white collar crimes are treated differently at sentencing than comparable crimes. Most theoretical research has suggested that white collar crimes should be treated more leniently. Judges may be less willing to harshly sentence offenders who share their social status (Wheeler et al. 1982), or because communities lose when these otherwise prosocial offenders are incarcerated (Jesilow et al., 1986; Wheeler et al., 1988), or because white collar offenders may not be able to withstand the harsh conditions in prison (Benson and Cullen, 1998; Pollack and Smith, 1983). Other theories, however, suggest that white collar offenders should be *more* likely to receive harsh sentences. The status liability hypothesis suggests that white collar offenders may be more likely to be treated leniently for mild offenses, but serious offenses (such as those that reach the gravity of federal prosecution) are seen as a violation of the social contract (e.g., Wahrman, 1970, 1972). The results from Chapter 7 suggest that white collar cases (using an agency-based definition) were more likely to receive downward departures and were less likely to receive prison when non-prison sentences were structurally eligible. This was interpreted as strong support for Hypothesis 4, in the direction of white collar leniency. Of course, "leniency" is an imprecise term. It may be that white collar cases are less likely to go to prison but receive longer sentences when they do; Maddan et al. (2012) find just such an effect in recent federal sentencing data. It is also conceivable that white collar cases are more likely to receive non-

prison sentences but that the conditions of probation and alternative confinement are more severe for white collar offenders than for others.

But why might white collar offenders be more likely to receive leniency? The most common first reason judges provide for departures is for "offender characteristics" – white collar offenders may be more likely to be employed, or employed in the types of occupations that judges associate with reduced risk. White collar offenders may also be more likely to have financial assets that suggest that offending is not likely to be motivated by financial need in the future (though Weisburd, 1991, suggests that "white collar" offenders are much more likely to be middle class than wealthy); income may be negatively associated punishment severity (Schanzenbach and Yeager, 2006; though see also Wheeler et al., 1982). However, neither of these is inconsistent with the theoretical underpinnings of theories that predict white collar leniency; white collar cases would be treated more leniently because these types of cases, offenders, and offenses, have characteristics that make them fundamentally different than other types of crime. At the core of the scholarly interest in white collar offenders is an interest in why some offenders or offenses are treated differently. Should respectability, wealth, or employment or some combination thereof reduce the explanatory power of the "white collar" label to null, this would not reduce the importance of studying "white collar" crime (it would also be surprising, given the role of heuristics in decision-making).

The evidence presented in this dissertation in support of white collar leniency is consistent with a large number of white collar sentencing studies; however, it is also inconsistent with a large number of similar studies. Differences

in time, public attitudes toward white collar crime, or sentencing regimes could be the cause of this discrepancy. However, it may also be that the inconsistency between this study and some prior studies, as well as the difference across previous studies, is driven by data or definitional differences. Hypothesis 5 predicted that different definitions of white collar crime in the same data would produce different substantive conclusions about white collar punishment leniency or liability; specifically, Hypothesis 5 stated that definitions of white collar crime more closely related to offenses tied to crimes committed in the course of a legitimate occupation (i.e., the agency and strict offense-based definitions) would be more likely to produce findings of leniency. While there have been a large number of studies using a myriad of definitions and approaches to identifying white collar crimes (and comparable crimes), no study to the author's knowledge has tested the effect of different definitions on findings within the same sample. As demonstrated in Chapter 4, choice of definition is not a trivial matter – depending on the definition employed, "white collar crime" produces different types of offenses, even including violent or drug offenses. As presented in Chapter 7, there is strong support for Hypothesis 5 across all four sentencing outcomes. Ceteris paribus, the agency and strict offense based definitions of white collar crime were associated with a decreased likelihood of receiving a prison sentence and an increased likelihood of receiving a departure. In contrast, the loose offense-based definition was unrelated to sentence leniency, and the Yale offense-based definition of white collar produced a finding of white collar status liability. These findings are not surprising. The agency-based definition and the strict offense-based definition were designed to

isolate cases most consistent with Sutherland's notion of an offense committed by a high status offender in the course of legitimate business activity. It is not surprising, then, that these definitions exhibited the leniency that Sutherland first described (1939). The loose offense-based definition, in contrast, was developed by a scholar who argued that offender characteristics were not important, that crime type was all that mattered; given that offender characteristics bely the theoretical arguments, we should not expect that white collar cases identified purely on the basis of offense characteristics are treated differently. Altogether, the findings related to this hypothesis suggest that definition matters, not just in the magnitude of findings but in the substantive conclusions we draw about the administration of justice.

These findings emphasize the need for researchers who are interested in "white collar crime" to ask themselves what precisely they want to understand – are they interested in the role of individual characteristics like power, prestige, and social position in affecting the processing of cases through the justice system? If so, a broad stroke approach to identifying white collar crimes similar to Edelhertz (1970) or the Yale Studies (e.g. Weisburd, 1991) will likely create a mixed group of offenses that contain cases with defendants both similar and dissimilar to the population of interest. In doing so, these types of approaches apparently dilute and even mask important differences in offending. This is particularly true due to the relative number of cases identified through these definitions. The Yale definition, for example, includes securities fraud and antitrust offending, which inarguably speak to power and occupational context. Yet, these offenses are present in such

small numbers that any potential leniency they receive is overshadowed by the treatment of mail and wire fraud and tax frauds. The same may be said of the loose-offense based definition, which not only omits regulatory offenses, but includes a much broader set of frauds that only further obscures the treatment of securities offenses. These findings do not resolve the nearly century-old debates about what qualifies as white collar crime; instead, these findings validate the necessity of these intellectual sparring matches. In researching and making claims about white collar crime, definition matters. Unfortunately, the results presented in this dissertation also suggest that without broad-based agreement on what is white collar crime, the literature surrounding its treatment in the criminal justice system (and perhaps even its etiology) will likely continue to be clouded by inconsistent findings.

I argue that careful consideration of the original use and intention of the phrase "white collar crime" suggest the dimensions of power and legitimate occupation as critical characteristics of these offenses and offenders. As such, an identification strategy that capitalizes on the context of offending (such as the agency-based definition) or the granular features of the offense in an effort to capture both context and power (the strict offense-based definition) should be preferred. In an era of increasingly accessible administrative criminal justice data, the onus is on white collar scholars to think deeply and innovatively about ways to capture dimensions of white collar offending in traditional datasets. We know from analyses of the original Yale data that, within a sample of "white collar" cases, impeccability and respectability matter (Albonetti, 1994; Weisburd et al., 1991; Wheeler et al., 1982), typically producing leniency. Absent these measures,

scholars can still tap into these constructs indirectly with careful consideration of the offenses considered and produce similar results.

Finally, this dissertation argued (Hypothesis 6) that white collar crimes should receive greater benefits from plea agreements than should traditional crimes. This was tested using interaction terms, predicting an additional leniency benefit for white collar crimes in terms of discretionary prison and downward departures. Ultimately, this hypothesis was not well supported. Using the agency based sample, there was some evidence that white collar cases with dropped or dismissed charges were more likely to receive either judge-initiated or any departure, above and beyond the benefits for white collar leniency. However, most coefficients were not significant, and others suggested that white collar cases that received certain types of negotiated pleas (severity reduction) were *less* likely to receive leniency. Altogether the results suggested that there is no additional signaling benefit for white collar crime in most cases. Yet, as discussed above, this does not answer the question of whether white collar crimes receive more favorable plea agreements than traditional offenses. Future work in this area should consider whether white collar cases receive a larger reduction in potential imprisonment or fines than comparable crimes, a larger reduction in the number of counts or charges, and a departure greater in magnitude.

While not considered as a primary dependent variable in this dissertation, financial penalties emerged as a consistent predictor of the likelihood of receiving a discretionary incarceration sentence – defendants who paid more in financial penalties were less likely to receive a prison sentence. This is consistent with work

by Shanzenbach and Yeager (2006), who argue that white defendants, who pay more in fines, also spend less time in prison than fine-paying black defendants, at approximately a $4,000 per month "savings". However, Schanzenbach and Yeager's work stops short of addressing the question of how the amount of fines and total financial penalties themselves affect prison length, instead demonstrating that income, which affects both, is negatively related to sentence length. This dissertation begins to address the question of how fines are related to punishment outcomes in partially confirming their argument that financial penalties are at odds with incarceration; cases with more financial obligations may have less harsh outcomes for other types of punishments. While the coefficient on these estimates were small (~0.001% reduction in the likelihood of going to prison for every additional dollar in financial obligations), in the context of large financial obligations,[55] this amounts to a large reduction (even with decreasing marginal effects). These findings are consistent with the notion that judges create "packages" of sentence options, rather than independently sentencing defendants to different sanction options. Understanding the types of sentence combinations available to federal judges and the relationship between amounts of incarceration, alternatives, probation, and financial obligations is an important direction for future work.

IMPLICATIONS FOR POLICY AND PRACTICE

While this dissertation is in large part motivated by theoretical reasons, it nevertheless has several implications for the practice of criminal justice administration. First, findings from the descriptive analysis suggested that white

[55] The 25th, 50th, and 75th percentile of financial penalties are as follows: $3,752, $20,875, and $116,708.

collar cases, for all definitions, take longer to process than traditional crimes. This could be due to the complexity or rarity of these cases or simply reflect the time it takes to build cases that are often based on testimony and paper trails. White collar cases were also generally much more likely not to involve an arrest. For cases closed without action, weak evidence was cited in more than 20% of cases. To prevent the delay of justice for these offenses, or the lack of justice proceedings entirely, more resources should be dedicated toward white collar prosecution. However, in another 15% of cases declined for prosecution, the referring agency requested that the case not be filed – this poses another kind of problem. To the extent that cases are otherwise eligible for criminal prosecution but are not, this introduces another potential source of leniency, both between different white collar cases and across offense types. Given that this early decision to close a case without action accounts for the largest portion of cases exiting the criminal justice system, additional oversight of this decision may be advisable. Prosecutorial guidelines, similar to the structured prescriptions for judges, may provide some control over these decisions. However, given that the federal criminal justice system already uses memoranda to direct prosecutors and priorities may change with administrations, as well as variation in oversight (e.g., Nagel and Scholhofer, 1982) the degree to which such guidelines would succeed in increasing consistency in this decision is debatable. It is also possible that there are differences in these and other decision-points examined herein across prosecutors; Spohn and Fornango (2009) confirm interprosecutor disparity documented in qualitative work (e.g., Hagan and

Bernstein 1979). As with sentencing, the degree of disparity in prosecution outcomes may be reduced with guidelines, but not eliminated (Mustard, 2001).

In general, findings suggested that more serious cases – as defined by complexity, violent offenses, the number of counts and charges, and at higher risk of incarceration or fines – were more likely to receive negotiated pleas resulting in fewer counts, less serious charges, or fewer unique charges. While these cases also have greater possible distance to travel, this is nevertheless troubling because it suggests that those defendants who have done the most harm are most likely to receive leniency. One explanation is that prosecutors use the terms of negotiated pleas to circumvent guidelines they feel are too harsh and attempt to manipulate sentence outcomes through these reductions. If this is the case, one (admittedly radical) solution is to discontinue the use of federal sentencing worksheets by judges so that prosecutors are not able to "select" the sentence that they feel is appropriate. While it is possible that disparity could reenter sentences, federal sentencing guidelines have not necessarily eliminated disparities across race or sex (Mustard 2001); efforts could be undertaken to systematically review sentence patterns for consistency within and between judges. Another possibility is to increase oversight for plea agreements, such that the terms of an agreement are mutually agreed upon between defense, prosecution, and the judge. These cooperative agreements would reduce the singular power of the prosecutor and better distribute that power among multiple actors. Prosecutors may also use negotiated pleas to conserve resources that might otherwise be spent on protracted legal battles, which may be more common for more serious cases and

correspondingly more serious consequences. This explanation for the leniency afforded to serious cases would also benefit from a cooperative plea strategy by increasing the resources required to engage in negotiated pleas and discouraging prosecutors from offering discounts that are too large to facilitate speedy (or speedier) resolution. There is evidence that judges do not follow prosecutors' leniency at sentencing, with cases receiving count and severity reduction generally *less* likely to receive leniency from judges. This suggests that judges do not necessarily agree with prosecutorial outcomes and may seek to act as a counterbalance to some extent.

One of the most striking implications to come from these results is that private attorneys may put their clients at a disadvantage in achieving desirable plea results. In spite of the commonly held notion that public defenders are less competent or less devoted to their clients than private counsel, defendants with private counsel were generally less successful in achieving reductions in the course of plea agreements. Private attorneys may be less able to draw on longstanding working relationships with prosecutors, ultimately putting their clients at a disadvantage. The question of how to address this issue is complicated; should public defender-prosecutor relationships be randomized to prevent unfair leniency? Yet, this arguably removes the benefit of established workgroups from poor defendants who may already be at risk for harsher sentences given limited financial means (e.g., Mustard 2001; Shanzenbach and Yeager 2006). This amounts to a question of what is "fair" in terms of equitable distribution of benefit versus what is "fair" given differences in status. Instead of trying to eliminate structural causes

of this difference, the best policy may be for private legal professionals who work in the federal criminal justice system to be made aware of this potential risk to their clients so that they might work to build more consistent relationships with particular prosecutors in particular districts. This might be accomplished by accepting more pro-bono clients, or by accepting clients who will likely be tried in districts familiar to them.

LIMITATIONS

This dissertation, like all research, suffers from a number of limitations, not the least of which is omitted variable bias, which occurs in both analysis samples and in the descriptive analyses, albeit in varying degrees. In the prosecution sample, a measure of evidentiary strength would have been an ideal independent variable. However, the FJSP EOUSA extracts do not contain any objective measures of evidence strength, like many prosecutorial datasets (Johnson et al. 2016). The EOUSA does offer some insight into the role of evidentiary strength in case declinations, however, through the review of declination reasons. Weak evidence was to blame for approximately one-fifth of all cases closed without action; there was little evidence that this reason was more or less common for particular cases. Yet this does not reveal how evidentiary strength might have affected plea negotiations or sentencing outcomes. Similarly, the linked data used to study negotiated pleas do not contain any indicators of a defendant's prior record. This is problematic because prior record may influence initial charge severity (Ulmer et al. 2007) as well as prosecutor decisions to engage in bargaining with defendants. Measures of charge severity may in part capture this unmeasured influence.

Sentencing outcome analyses are not affected by omission of prior history as the final stage of linking (USSC Cases Sentenced Data) are able to provide many of these variables. While it is possible to conduct the prosecution analysis with the sentencing sample, the nonrandom loss of cases between the court and sentencing data may problematic for conclusions.

Relatedly, I was unable to move beyond simplistic racial comparisons (black/white); while available for the final stage of the FJSP used in this dissertation, ethnicity is not available to be used in prosecution analyses; both "white" and "black" include Hispanic and non-Hispanic individuals of each race. The likely effect is a suppression of black-white differences in outcomes; differences between these groups may emerge if ethnicity information were available (sentencing analyses with the inclusion of Hispanic can be found in the Appendix, A8). In addition, due to small sample sizes, other races (Asian and Pacific Islanders, Native Americans) are not discussed individually but rather in a joint category. Future research may employ weighted samples to explore racial effects for these groups, or undertake comparisons for case processing across races. Also missing is information on defendant class or income; this limits the comparability of this research to early sentencing studies of white collar crime, which employed income as a means of identifying white collar offenders (e.g., Hagan and Bernstein, 1979; Hagan and Nagel, 1982). Several variables used in the analyses are arguably related to income, however; offenders with private counsel can be thought of as meeting some minimum income threshold, and offenders who receive larger financial penalties are able to pay more. If private counsel is a proxy

for income, wealthier defendants are less likely to receive discounts in plea negotiations, but are more likely to receive substantial assistance departures. The amount of financial penalties imposed was also negatively related to the likelihood of receiving a discretionary prison sentence, which may indicate that wealthier defendants are less likely to go to prison. Relatedly, models of prosecution outcomes are not able to control for bail status of defendants, in spite of evidence that suggests that bail affects both the likelihood and conditions of pleas (Wooldredge et al., 2015). In spite of these omitted variables, there is reason to find merit in the findings presented here. To the extent that income (and relatedly, bail status) is unevenly distributed across white collar and traditional crimes, this would only confirm the measurement strategies employed as successfully capturing crimes committed by persons of high social status and class. There is some reason to suspect that complex white collar cases are more difficult to prosecute in part because it is more difficult to amass evidence, and that these cases might have weaker evidence. However, among declined cases, white collar cases were *not* more often cited as be declined for weak evidence. If there is no relationship between white collar crime status and evidentiary quality, the omission of this variable should not affect the substantive conclusions presented in this dissertation.

Notably for a study of white collar crime, this dissertation does not consider crimes referred for prosecution that involve *organizational* (including corporate) defendants. While these defendants should appear in the EOUSA and AOUSC data extracts, there is no way to identify them in the data. The Bureau of Justice Statistics reports that while such a variable is contained in the EOUSA administrative record

system from which the FJSP files are pulled, the variable is not populated enough to be reliable; similarly, name recognition algorithms in the AOUSC data are not efficient enough to warrant dissemination to researchers.[56] Even if these cases could be identified in court and U.S. Attorney data, it would not be possible to link these cases to sentencing outcomes, as the United States Sentencing Commission Organizational datafiles are not part of the FJSP; in the present analysis, these defendants drop out when not matched to an arrest.

An inherent complication of using data from different agencies is the imperfect nature of the linking procedure, particularly given that so many links were performed. While some match-eligible rates were near perfect, others were notably less than. However, most of the case loss from the link procedures were due to "NULL" cases – i.e., cases that should not have been expected to match. These are not failures of the algorithm, but rather cases that are either still pending or not eligible to appear in the next stage. While there were sometimes large differences in rates of null for different types of cases (e.g., between white collar and comparable crimes), these differences reflect real differences in processing rather than an error, and therefore do not pose a risk to generalizability. Failures to *match*, in general, were small even when statistically significant. The largest issue with match rates was the addition of the sentencing data to court data; at this stage, general unmatched rates were higher, and unmatched rates were also generally higher for white collar offenses. The loss of these cases could be an issue for generalizability if these cases are sentenced differently than their counterparts that

[56] Motivans, Mark. Personal communication with the author.

did match. However, supplemental analysis (not shown) found that there were very few differences for white collar cases that matched and those that did not match. White collar cases that did not match were slightly more likely to be white, and less likely to be single, similar to the match rates overall, suggesting that this failure to match will not likely have a significant effect on the white collar leniency findings.

Some may argue that this research is also limited by its focus on conditional effect models; i.e., this dissertation does not fully account for selection into a given criminal justice stage, and thus the coefficients cannot be taken to represent the "full" effect of, for example, white collar crime, on sentencing outcomes. However, in spite of the methodological and conceptual emphasis on full effects, our abilities to in fact present realistic estimates of total, direct, and indirect effects of particular variables across (and even within stages) of the criminal justice system are generally limited. Two-stage models of selection, while in principle helpful, are limited in their utility because of the absence of a reliable exclusion restriction; rather than reflecting a flaw in data, it seems more likely that the absence of an exclusion restriction suggests that the same variables are driving both selection and the observed values of the dependent variable. While some within-stage selection (such as incarceration length) can be modeled with zero-inflated models, selection *across* stages is not readily remedied with these approaches (particularly with dichotomous outcomes). Providing an estimate of a single direct effect also does not necessarily accurately describe variables that may have different effects at each stage. For example, in the present analyses, type of attorney was significantly related to the terms of negotiated pleas but was associated with an increased

likelihood of a substantial assistance departure; the likely consequence is that a "total effect" of attorney type might appear to be null, when in fact private attorneys have effects in two different ways at two different points. For individual defendants, the experienced effect of a private attorney likely depends on the magnitude of the departure and the original charges filed.

Finally, this dissertation is limited in that it focuses on a cohort of cases that are referred for prosecution. It cannot speak to the case processing outcomes that occur before this point, or explain how cases are funneled into referral or to other case resolutions. White collar scholars have long noted that criminal justice interventions are the rarest form of formal response to white collar crime (Ayers and Braithwaite 1992). Thus, it is important to caution that this dissertation speaks to the criminal justice case processing, and may not be representative of the effect that case and defendant characteristics have on other white collar case processing outcomes. Future work should consider the effect of these characteristics on civil processes.

CONCLUSION

The field of white collar criminology has long been motivated by the assumption that there is something different about the way in which white collar cases and offenders are treated, relative to so called 'street crime.' Sutherland (1939, 1983[1949]) first called attention to the ways that the crimes of powerful individuals and organizations escaped society's (and scholars') attention; these cases often were routed through other justice systems, avoiding the label of 'crime' other more serious consequences, in spite of behavior against the law. In the three-

quarters of a century that has followed, an entire subsection of criminological interest has emerged to study that 'something different' about white collar offenses. And yet, the findings from this research have been plagued with definitional and empirical inconsistencies that have prevented a robust evaluation of that difference. While the earliest scholarly work on white collar crime highlight the role of power, privilege, class, and occupational context features of these offenses, the methodological difficulty of identifying cases meeting this definition has, seemingly more from pragmatic necessity than theoretical drive, prompted a surplus of definitions of 'white collar' crime. Coinciding with these many definitions have been mixed findings related to the assumed differential treatment of these cases in criminal courts; there is not even a strong tilt in the findings toward difference or status quo (Simpson, 2013). One possible explanation for these mixed findings is the promulgation and use of many white collar definitions without careful consideration of the resultant differences in samples of 'white collar' cases and the differences between these offenses and comparison crimes. At the same time, studies of white collar leniency have generally been conducted apart from traditional sentencing research. Over the past few years, a key theme of this literature has been a widening of focus to understand sentencing not as the provision of judges alone, but rather to see sentencing as the ultimate outcome of a process involving multiple actors; among the actors most important in this process are prosecutors (Baumer, 2013; Ulmer 2012; Ulmer et al. 2007).

The goal of this dissertation was to marry the two literatures, drawing on insights of both to explore the prosecution and sentencing of white collar crime in

federal criminal courts. To do so, I identified a cohort of white collar and comparable crimes referred for criminal prosecution to (or initiated by) the United States attorneys between 2009-2011. I then used dyadic linkfiles to link these cases to other data in the Federal Justice Statistics Program series, including arrest data from the United States Marshals Service, criminal case information from the Administrative Office of the United States Courts, and sentencing details from the United States Sentencing Commission through fiscal year 2013. Doing so allowed me to track cases from referral through case conclusion, and thereby to both describe case processing as well as consider the effect that prior decisions have on subsequent ones.

One of the most striking findings was that, depending on the definition employed, white collar cases were either incredibly common or exceedingly rare. In addition, the definitions of white collar crime that included fewer cases were *not*, as might be expected, subsets of those that resulted in larger referral samples. The agency-based and strict offense-based definitions were generally more similar to one another than to either the Yale or loose offense-based definition. The largest exit from the criminal justice system occurs at the point of case filing; nearly half of white collar matters referred for prosecution were declined; the proportion of cases filed is highest when including all nonviolent property offenses, and lowest using an agency-based definition. Prosecution outcomes vary slight across definitions but are generally similar. However, definition matters most profoundly for sentencing outcomes, with anywhere from 45-71% of cases resulting in any incarceration sentence. Sentence length and the amount of fines also vary notably;

yet, no definition produces uniformly lenient or harsh penalties based on descriptive statistics. Cases with private counsel, however, typically fared less well (from the perspective of the defendant) for negotiated pleas, but more favorably at sentencing with the exception of fine amounts. Cases with codefendants, which may be perceived by prosecutors as more complex, generally received greater prosecutorial advantages but fared slightly worse at sentencing.

Case complexity was also associated with case processing outcomes in more formal multivariate tests. Having codefendants was associated with an increased likelihood of receiving discounts during plea negotiations, though each codefendant was not associated with any additional leniency. Codefendants were *not* associated with judicial decisions to impose a discretionary prison sentence or to issue a sentence less than prescribed by the guidelines. Consistent with the prosecution analyses, cases with codefendants were more likely to receive substantial assistance departures. These finds are largely consistent both with prior literature on the role of case complexity but also with the argument made in this dissertation that complex cases impose larger resource burdens on prosecutors and introduce uncertainty into case outcomes. As a result, prosecutors may seek to reduce both by offering favorable plea agreements to defendants with complex cases. Yet, case complexity had no appreciable effect on decisions made by judges, whose work is not necessarily as dramatically affected by case complexity.

The role of the prosecutor on sentencing outcomes, while a primary motivation for this dissertation, was met with mixed empirical findings. Not only was there little evidence of an indirect (structuring) effect of prosecutor decisions

to engage in plea bargaining through the manipulation of offense scores, there was also only partial evidence of a direct effect of plea bargains on sentencing. Receiving a reduction in the number of counts was generally associated with harsher punishment, either in the imposition of a purely discretionary prison sentence or in a reduced likelihood of receiving a departure. One possible explanation is that judges seek to "balance the scales" by penalizing defendants who have received advantages through prosecutorial decisions. However, it is equally plausible that this reflects judicial efforts to engage in "real offense sentencing". To the extent that prosecutors are able to reduce guidelines offense scores through their plea negotiations, they may in fact succeed in reducing sentence outcomes for white collar offenders. Very few cases were sentenced above the guidelines prescribed ranges, so even if negotiated pleas are less likely to benefit from judicial leniency, they may nevertheless receive discounts relative to the offense score they would have received without the benefit of a plea bargain. Contrary to expectations, there was no evidence of an additional signaling effect for white collar cases receiving these plea discounts. Perhaps unsurprisingly, there were mixed results as to the role of white collar case status in sentencing leniency. Under the primary working definition of this dissertation (agency-based), white collar cases were treated more leniently than comparable crimes. However, this finding of leniency did not hold across definitions (consistent with Hypothesis 5). The Yale offense-based definition was associated with punishment liability, consistent with prior findings using this definition (e.g., Weisburd, 1991).

These findings suggest several promising and important directions for future research. For white collar crime research, it is abundantly clear that definition matters. While definitional debates may have been thought to be little more than narcissistic self-promotion, argument for the sake of argument, or pragmatically required to study white collar crime, this dissertation has demonstrated that, in the context of federal criminal courts, definitional choice has profound impact on the types of substantive conclusions that are drawn about white collar crime processing both on its own and in relation to other crimes. Second, while prosecution may matter for sentencing, this dissertation offers initial evidence that the largest effect may be in the selection of initial charging decisions. There may also be significant variation in the quality of negotiated pleas, such that the impact of these bargains is only found when discounts are large. Future research should extend these analyses to incorporate the "distance traveled" in plea negotiations, and consider the extent to which these discounts vary across groups. Complex cases (e.g., those with codefendants) and white collar cases may receive greater discounts. While not specifically hypothesized, cases with private attorneys generally received worse outcomes during plea negotiations, but were more likely to receive departures from both prosecutors and judges. In strict terms of magnitude, these case outcomes are associated with the shortest sentence length, suggesting that private attorneys may in fact be shrewd negotiators (though their benefit is not always apparent). Finally, these results generally affirm the notion that there is something special about white collar crimes in the context of administering justice. Whether that 'something' is to

their advantage or disadvantage, however, depends on the stage and the definition employed.

APPENDIX

A1. UNPACKING MATCH RATES – ERRORS VS. EXITS

Table 32. Differences in Non-Match rates, Matters In to Linkfile

Variable	Difference in % Unmatched	Z	
White Collar (Agency)	1.89	-7.85	***
White Collar (Loose)	1.47	-11.03	***
White Collar (Yale)	1.64	-12.52	***
White Collar (Strict)	0.25	-1.59	
Comparison (Agency)	-1.16	6.76	***
Comparison (Loose)	-2.4	16.66	***
Comparison (Yale)	-1.26	9.98	***
Comparison (Strict)	-0.28	2.03	*
Cohort 2009-2010[1]	-0.96	10.22	***
Any Codefendants	1.81	-14.17	***
	Difference[2]	**T**	
Number of Codefendants	-0.45	10.04	***

[1] Comparison between 2009-2011 referral cohort and 2009-2010 referral cohort.
[2] T-test assumes unequal variances.
* p<.05
** p<.01
*** p<.001

Table 33. Differences in Rate of Null, Matters In to Matters Out

Variable	Difference in % Null	Z	
White Collar (Agency)	2.72	-7.41	***
White Collar (Loose)	2.39	-11.82	***
White Collar (Yale)	2.97	-14.94	***
White Collar (Strict)	1.8	-7.34	***
Comparison (Agency)	-2.15	8.2	***
Comparison (Loose)	-4.92	22.42	***
Comparison (Yale)	-2.63	13.67	***
Comparison (Strict)	-2.12	10.21	***
Cohort 2009-2010[1]	-1.7	9.2	***
Any Codefendants	5.51	-28.38	***

	Difference[2]	T	
Number of Codefendants	0.68	-9.00	***

[1] Comparison between 2009-2011 referral cohort and 2009-2010 referral cohort.
[2] T-test assumes unequal variances.
* p<.05
** p<.01
*** p<.001

Table 34. Differences in Non-Match rates, Matters In to Matters Out

Variable	Difference in % Unmatched	Z	
White Collar (Agency)	-0.40	1.86	
White Collar (Loose)	0.58	-5.08	***
White Collar (Yale)	0.26	-2.33	*
White Collar (Strict)	0.34	-2.46	*
Comparison (Agency)	-0.08	0.54	
Comparison (Loose)	-0.82	6.72	***
Comparison (Yale)	-0.12	1.12	
Comparison (Strict)	-0.25	3.00	**
Cohort 2009-2010[1]	-0.03	0.38	
Any Codefendants	-0.05	0.43	
	Difference[2]	**T**	
Number of Codefendants	-0.49	4.60	***

[1] Comparison between 2009-2011 referral cohort and 2009-2010 referral cohort.
[2] T-test assumes unequal variances.
* p<.05
** p<.01
*** p<.001

Table 35. Differences in Rate of Null, Matters Out to Arrests

Variable	Difference in % Null	Z	
White Collar (Agency)	18.45	-26.58	***
White Collar (Loose)	8.47	-22.84	***
White Collar (Yale)	-0.90	2.41	*
White Collar (Strict)	12.92	-28.32	***
Comparison (Agency)	-7.30	14.86	***

Comparison (Loose)	-14.36	36.06	***
Comparison (Yale)	-1.22	3.42	***
Comparison (Strict)	-10.48	27.08	***
Cohort 2009-2010[1]	0.38	-1.36	
Any Codefendants	7.46	-20.45	***
	Difference[2]	**T**	
Number of Codefendants	-0.76	15.12	***

[1] Comparison between 2009-2011 referral cohort and 2009-2010 referral cohort.
[2] T-test assumes unequal variances.
* p<.05
** p<.01
*** p<.001

Table 36. Differences in Non-Match rates, Matters Out to Arrests

Variable	**Difference in % Unmatched**	**Z**	
White Collar (Agency)	0.77	-3.35	***
White Collar (Loose)	-0.35	3.42	***
White Collar (Yale)	0.12	-1.13	
White Collar (Strict)	0.03	-0.24	
Comparison (Agency)	-0.24	1.67	
Comparison (Loose)	0.30	-2.78	**
Comparison (Yale)	-0.26	2.64	**
Comparison (Strict)	-0.14	1.28	
Cohort 2009-2010[1]	-0.14	1.70	
Any Codefendants	-0.26	2.36	*
	Difference[2]	**T**	
Number of Codefendants	-1.38	13.31	***

[1] Comparison between 2009-2011 referral cohort and 2009-2010 referral cohort.
[2] T-test assumes unequal variances.
* p<.05
** p<.01
*** p<.001

Table 37. Differences in Rate of Null, Matters Out to Cases Filed

Variable	Difference in % Null	Z	
White Collar (Agency)	4.64	-5.24	***
White Collar (Loose)	-26.33	67.55	***
White Collar (Yale)	26.61	-67.18	***
White Collar (Strict)	-8.05	15.18	***
Comparison (Agency)	8.52	-15.44	***
Comparison (Loose)	29.71	-73.20	***
Comparison (Yale)	-22.82	59.70	***
Comparison (Strict)	10.1	-23.36	***
Cohort 2009-2010[1]	1.03	-3.42	***
Any Codefendants	-18.87	46.19	***
White	11.93	-29.17	***
Black	-11.51	26.63	***
Other Race	-6.93	8.20	***
Male	-6.45	15.54	***
Married	-6.43	16.95	***
	Difference[2]	**T**	
Number of Codefendants	-2.14	39.12	***
Age	-5.65	38.83	***

[1] Comparison between 2009-2011 referral cohort and 2009-2010 referral cohort.
[2] T-test assumes unequal variances.
* p<.05
** p<.01
*** p<.001

Table 38. Dispositions of NULL cases, Matters Out to Defendants in Criminal Cases

	Percent
Closed Without Action	0.01%
Declined	10.86%
Dismissed with Prejudice	0.36%
Dismissed	4.82%
Criminal Guilty Verdict	65.93%
Nolo Contendre	0.00%
Criminal Not Guilty Verdict	0.01%
New Filing	0.96%
Included in Another Case	2.35%

Opened in error	0.01%
Removal (Rule 40)	7.56%
Transferred	1.58%
Total	94.45%
Missing	5.44%

Table 39. Court Type for Null Cases

	Percent
U.S. District Court	0.01%
Magistrate Court[1]	87.30%
Not in Court	12.69%
Total	100.00%

[1] Label values identified using the LIONS Data Appendix

Table 40. Differences in Rate of Non-Match, Defendants in Cases Filed to Defendants in Cases Concluded Linkfile

Variable	**Difference in % Unmatched**	**Z**	
White Collar (Agency)	2.58	-5.13	***
White Collar (Loose)	0.26	-1.19	
White Collar (Yale)	3.77	-16.14	***
White Collar (Strict)	0.56	-1.97	*
Comparison (Agency)	-1.03	3.49	***
Comparison (Loose)	-1.77	7.22	***
Comparison (Yale)	-2.64	12.11	***
Comparison (Strict)	-0.68	2.93	**
Cohort 2009-2010[1]	-1.16	7.42	***
Any Codefendants	0.11	-0.53	
White	-0.16	0.72	
Black	0.06	-0.24	
Other Race	0.46	-1.01	
Male	0.81	-3.47	***
Married	1.95	-8.62	***
	Difference[2]	**T**	
Number of Codefendants	-0.32	1.45	
Age	5.26	-15.34	***

[1] Comparison between 2009-2011 referral cohort and 2009-2010 referral cohort.

[2] T-test assumes unequal variances.
* p<.05
** p<.01
*** p<.001

Table 41. Differences in Rate of Null, Defendants in Cases Filed to Defendants in Cases Concluded

TABLE A1.10.			
Variable	**Difference in % Null**	**Z**	
White Collar (Agency)	10.21	-4.95	***
White Collar (Loose)	2.88	-9.5	***
White Collar (Yale)	1.97	-6.26	***
White Collar (Strict)	-0.93	2.46	*
Comparison (Agency)	-0.25	0.63	
Comparison (Loose)	-4.29	13.2	***
Comparison (Yale)	-0.71	2.47	*
Comparison (Strict)	1.35	-4.34	***
Cohort 2009-2010[1]	-1.17	5.47	***
Any Codefendants	4.83	-16.52	***
White	0.23	-0.8	
Black	-1.39	4.5	***
Other Race	4.29	-7.18	***
Male	1.31	-4.21	***
Married	3.02	-9.86	***
	Difference[2]	**T**	
Number of Codefendants	3.68	-12.41	***
Age	2.95	-10.81	***

[1] Comparison between 2009-2011 referral cohort and 2009-2010 referral cohort.
[2] T-test assumes unequal variances.
* p<.05
** p<.01
*** p<.001

Table 42. Differences in Rate of Null, Defendants in Cases Filed to Defendants in Cases Concluded

Variable	Difference in % Null	Z	
White Collar (Agency)	1.05	-1.29	
White Collar (Loose)	0.72	-2.05	*
White Collar (Yale)	1.13	-3.06	
White Collar (Strict)	-0.58	1.29	
Comparison (Agency)	0.50	-1.08	
Comparison (Loose)	-0.85	2.27	*
Comparison (Yale)	-0.61	1.80	
Comparison (Strict)	0.78	-2.15	*
Cohort 2009-2010[1]	-0.25	0.99	
Any Codefendants	2.13	-6.16	***
White	-1.53	4.43	***
Black	0.86	-2.37	*
Other Race	3.27	-4.53	***
Male	1.50	-4.14	***
Married	0.87	-2.46	***
	Difference[2]	**T**	
Number of Codefendants	0.06	-0.40	
Age	0.61	-2.40	*

[1] Comparison between 2009-2011 referral cohort and 2009-2010 referral cohort.
[2] T-test assumes unequal variances.
* p<.05
** p<.01
*** p<.001

Table 43. Differences in Rate of Null, AOUSC Cases Filed to AOUSC Cases Concluded

Variable	Difference in % Null	Z	
White Collar (Agency)	0.36	-1.07	
White Collar (Loose)	0.03	-0.25	
White Collar (Yale)	0.12	-0.79	
White Collar (Strict)	0.2	-1.16	
Comparison (Agency)	-0.11	0.54	
Comparison (Loose)	-0.09	0.60	
Comparison (Yale)	-0.22	1.58	

Comparison (Strict)	-0.13	0.85	
Cohort 2009-2010[1]	0.08	-0.73	
Any Codefendants	0.06	-0.41	
White	-0.15	1.03	
Black	0.21	-1.40	
Other Race	-0.19	0.65	
Male	0.08	-0.55	
Married	0.14	-0.99	
	Difference[2]	**T**	
Number of Codefendants	-0.70	3.56	***
Age	0.93	-1.32	

[1] Comparison between 2009-2011 referral cohort and 2009-2010 referral cohort.
[2] T-test assumes unequal variances.
* p<.05
** p<.01
*** p<.001

Table 44. Differences in Rate of Unmatched, AOUSC Cases Concluded to Linkfile

Variable	**Difference in % Unmatched**	**Z**	
White Collar (Agency)	1.81	-2.73	**
White Collar (Loose)	0.90	-3.16	**
White Collar (Yale)	-0.39	1.29	
White Collar (Strict)	2.68	-7.48	***
Comparison (Agency)	0.43	-1.15	
Comparison (Loose)	-0.74	2.42	*
Comparison (Yale)	0.22	-0.80	
Comparison (Strict)	-1.48	5.03	***
Cohort 2009-2010[1]	-0.19	0.92	
Any Codefendants	2.41	-8.49	***
White	-0.58	2.04	*
Black	0.62	-2.09	*
Other Race	-0.14	-0.09	
Male	-1.83	6.23	***
Married	0.22	-0.76	
	Difference[2]	**T**	

Number of Codefendants	5.33	-9.99	***
Age	0.88	-2.47	*

[1] Comparison between 2009-2011 referral cohort and 2009-2010 referral cohort.
[2] T-test assumes unequal variances.
* p<.05
** p<.01
*** p<.001

Table 45. Differences in Rate of Null, AOUSC Cases Concluded to USSC Cases Sentenced

Variable	**Difference in % Null**	**Z**	
White Collar (Agency)	1.45	-2.92	**
White Collar (Loose)	-0.77	3.60	***
White Collar (Yale)	-0.30	1.34	
White Collar (Strict)	-0.93	3.43	***
Comparison (Agency)	0.16	-0.60	
Comparison (Loose)	0.80	-3.51	***
Comparison (Yale)	0.17	-0.84	
Comparison (Strict)	0.67	-3.07	**
Cohort 2009-2010[1]	0.10	-0.63	
Any Codefendants	0.11	-0.53	
White	0.42	-2.01	*
Black	-0.39	1.78	
Other Race	-0.30	0.66	
Male	0.46	-2.09	*
Married	-0.12	0.57	
	Difference[2]	**T**	
Number of Codefendants	0.32	-0.89	
Age	-1.47	3.26	**

[1] Comparison between 2009-2011 referral cohort and 2009-2010 referral cohort.
[2] T-test assumes unequal variances.
* p<.05
** p<.01
*** p<.001

Table 46. Differences in Rate of Unmatched, AOUSC Cases Concluded to USSC Cases Sentenced

Variable	Difference in % Unmatched	Z	
White Collar (Agency)	5.52	-5.87	***
White Collar (Loose)	1.71	-4.27	***
White Collar (Yale)	5.82	-13.85	***
White Collar (Strict)	3.23	-6.39	***
Comparison (Agency)	-1.59	3.04	**
Comparison (Loose)	-4.08	9.59	***
Comparison (Yale)	-3.91	10.10	***
Comparison (Strict)	-2.42	5.86	***
Cohort 2009-2010[1]	-4.61	17.37	***
Any Codefendants	-0.37	0.77	
White	0.01	-0.02	
Black	-0.08	0.18	
Other Race	0.28	-0.33	
Male	-1.19	2.89	**
Married	3.07	-7.56	***
	Difference[2]	**T**	
Number of Codefendants	-0.70	6.96	***
Age	4.44	-16.83	***

[1] Comparison between 2009-2011 referral cohort and 2009-2010 referral cohort.
[2] T-test assumes unequal variances.
* p<.05
** p<.01
*** p<.001

A2. EXPANDED DESCRIPTIVES

A2.1 Detailed Offense Codes by Definition

Table 47. Detailed Offense Codes by Definition

	Agency		Loose		Yale		Strict	
Offense	Freq.	%	Freq.	%	Freq.	%	Freq.	%
Missing	1	0.02	-		-		-	
Conspiracy	2	0.03	-		-		-	
Conspiracy	4	0.06	-		-		-	
Conspiracy	80	1.2	-		-		-	
Conspiracy	35	0.53	-		-		-	
Murder, 1st Degree	21	0.32	-		-		-	
Murder, 1st Degree, Govt Officials	3	0.05	-		-		-	
Manslaughter	5	0.08	-		-		-	
Manslaughter - Govt official	1	0.02	-		-		-	
Robbery, Bank	15	0.23	-		-		-	
Aggravated or felonious assault	18	0.27	-		-		-	
Fair Housing Law	1	0.02	-		-		-	
Assault, Other	2	0.03	-		-		-	
Obstruction Of Justice-Interference	18	0.27	-		-		-	
Burglary, Postal	1	0.02	-		-		-	
Larceny & Theft, Bank	1	0.02	-		-		-	
Larceny & Theft, Postal	4	0.06	-		-		-	
Larceny & Theft, Interstate Commerce	1	0.02	-		-		-	
Transportation Stolen Property	1	0.02	-		-		-	
Larceny & Theft, Felony Other	10	0.15	-		-		-	
Bank Embezzlement	9	0.14	496	0.82	496	1.46	-	

Postal Embezzlement	11	0.17	1,131	1.87	-		-	
Embezzles Public Moneys Or Property	207	3.11	7,087	11.72	-		7,087	40.31
Embezzle - Lending, Credit, Insurance Institutions	1	0.02	56	0.09	-		56	0.32
Embezzle by Officers Of A Carrier	-		1	0	-		1	0.01
Embezzle - Veterans Relief	-		14	0.02	-		-	
Embezzle by Officer Or Employee Of U.S. Govt.	9	0.14	79	0.13	-		79	0.45
Embezzlement, Other	141	2.12	2,426	4.01	-		-	
Income Tax, Evade Or Defeat	17	0.26	-		1,667	4.91	-	
Income Tax, Other Felony	17	0.26	-		1,310	3.86	-	
Income Tax, Failure To File	-		-		219	0.64	-	
Income Tax, Other Misdemeanor	-		-		7	0.02	-	
Fraud, Lending, Credit Institutions	86	1.29	1,037	1.71	1,037	3.05	1,037	5.9
Bank Fraud	150	2.26	7,245	11.98	-		7,245	41.2
Fraud, Postal, Interstate Wire, Radio, Etc.	340	5.12	13,876	22.95	13,876	40.84	-	
Fraud, Veterans And Allotments	-		18	0.03	-		-	
Fraud, Bankruptcy	346	5.21	1,126	1.86	-		-	
Fraud, Marketing Agreements & Commodity Credit	-		12	0.02	-		-	

Fraud, Securities & Exchange	35	0.53	865	1.43	865	2.55	-	
Fraud, Other Tax	10	0.15	-		80	0.24	-	
Fraud, Railroad Retirement & Unemployment	-		2	0	-		-	
Fraud, Food Stamp Program	61	0.92	333	0.55	-		-	
Fraud, Social Security	239	3.6	6,919	11.44	-		-	
Fraud, False Personation	13	0.2	309	0.51	-		-	
Fraud, Nationality Laws	3	0.05	1,505	2.49	-		-	
Fraud, Passport Fraud	18	0.27	2,209	3.65	-		-	
Fraud, False Claims & Statements	338	5.09	-		13,412	39.48	-	
Fraud, Conspiracy To Defraud, Other	23	0.35	637	1.05	-		-	
Fraud, Conspiracy (General), Other	114	1.72	2,803	4.64	-		-	
Fraud, False Entries, Other	-		31	0.05	-		-	
Credit Card Fraud	17	0.26	2,548	4.21	-		-	
Computer Fraud	21	0.32	1,584	2.62	-		-	
Health Care Fraud	1	0.02	7	0.01	-		-	
Fraud, Other	446	6.71	2,133	3.53	-		-	
Transport Etc. Stolen Vehicles, Aircraft	2	0.03	-		-		-	
Auto Theft, Other	24	0.36	-		-		-	
Forgery, Postal	1	0.02	89	0.15	-		-	
Forgery, Other U. S.	15	0.23	2,772	4.58	-		-	
Counterfeiting	25	0.38	1,120	1.85	-		-	
Sexual Abuse of Adult	36	0.54	-		-		-	

White Slave & Importing Aliens	11	0.17	-		-		-	
Sex Offenses, Other	86	1.29	-		-		-	
Controlled Sub-Continuing Criminal Enterprise	554	8.34	-		-		-	
Controlled Sub-Sell,Distribute,Or Dispen	4	0.06	-		-		-	
Controlled Sub-Possession	3	0.05	-		-		-	
Driving Under Influence Alcohol/Drugs	2	0.03	-		-		-	
6812 - Undocumented Code	1	0.02	-		-		-	
6882 - Undocumented Code	1	0.02	-		-		-	
6883 - Undocumented Code	1	0.02	-		-		-	
6884 - Undocumented Code	9	0.14	-		-		-	
Controlled subs-possession	17	0.26	-		-		-	
Controlled sub-import/export	20	0.3	-		-		-	
Bribery	36	0.54	-		914	2.69	914	5.2
Traffic Offenses, Other	3	0.05	-		-		-	
Escape	27	0.41	-		-		-	
Escape, Jumping Bail	51	0.77	-		-		-	
Escape, Aiding Or Harboring	20	0.3	-		-		-	
Extortion, Racketeering, & Threats	64	0.96	-		-		-	
Threats Against The President	19	0.29	-		-		-	

Racketeering, Murder	2	0.03	-		-		-	
Racketeering, Conspiracy, Murder, Kidnap	1	0.02	-		-		-	
Racketeering, Attempt/Conspir e, Assault	1	0.02	-		-		-	
Racketeering, Monetary Laundering	1	0.02	-		-		-	
Racketeering, Extortion Credit Transact.	1	0.02	-		-		-	
Kidnapping	1	0.02	-		-		-	
Perjury	5	0.08	-		-		-	
Firearms & Weapons	2	0.03	-		-		-	
Firearms, Other	99	1.49	-		-		-	
Misc General Offenses, Other	718	10.8	-		-		-	
Juvenile Delinquency	1	0.02	-		-		-	
Immigration Laws, Illegal Entry	2	0.03	-		-		-	
Immigration Laws, Illegal Re-Entry	27	0.41	-		-		-	
Immigration Laws, Other	30	0.45	-		-		-	
Agriculture Acts	-		-		-		10	0.06
Game Conservation Acts	9	0.14	-		-		-	
Agriculture, Insecticide Act	36	0.54	-		-		41	0.23
National Park/Recreation Violations	1	0.02	-		-		-	
Antitrust Violations	15	0.23	-		92	0.27	92	0.52
Fair Labor Standards Act	4	0.06	-		-		24	0.14
Food & Drug Act	288	4.33	-		-		453	2.58

Migratory Bird Laws	1	0.02	-		-		-	
National Defense, Selective Service Acts	4	0.06	-		-		-	
National Defense, Illegal Use Of Uniform	1	0.02	-		-		-	
Alien Registration	1	0.02	-		-		-	
Energy Facility	-		-		-		59	0.34
Treason	1	0.02	-		-		-	
Espionage	1	0.02	-		-		-	
National Defense, Other	6	0.09	-		-		-	
Defense Contractors	1	0.02	-		-		-	
Civil Rights	36	0.54	-		-		-	
Election Law Violators	1	0.02	-		-		-	
Federal Statutes - Public Officer/Employee	-		-		-		20	0.11
Federal Statutes - Foreign Relations	-		-		-		14	0.08
Federal Statutes- Money And Finance	-		-		-		76	0.43
Federal Statutes- Public Health & Welfare	-		-		-		36	0.2
Communication Acts	-		-		-		13	0.07
Federal Statute- Copyright Laws	1	0.02	-		-		-	
Federal Statutes- Commerce And Trade	2	0.03	-		-		188	1.07
Contempt	24	0.36	-		-		-	
Contempt - Congressional	4	0.06	-		-		-	

Customs Laws (Except Narcotics & Liquor)	39	0.59	-		-		-	
Peonage	2	0.03	-		-		-	
Terrorist Activity	3	0.05	-		-		-	
Maritime & Shipping :aws	232	3.49	-		-		-	
Federal Boat Safety Act Of 1971	-		-		-		5	0.03
Postal, Violations By Postal Employees	2	0.03	-		-		-	
Postal, Other	1	0.02	-		-		-	
9991Destroying Federal Property	7	0.11	-		-		-	
Intimidation Of Witnesses, Jurors, Etc.	21	0.32	-		-		-	
Aircraft Regulations	1	0.02	-		-		133	0.76
Explosives (Except On Vessels)	10	0.15	-		-		-	
Federal Statutes, Other	1,071	16.11	-		-		-	
Total	6,646	100	60,470	100	33,975	100	17,583	100

A2.2 Definition-Specific Process Models

Figure 9. Agency Definition Case Processing

Figure 10. Loose Offense-Based Definition Case Processing

Figure 11. Yale Offense-Based Definition Case Processing

Figure 12. Strict Offense-Based Definition Case Processing

A3. FULL TABLES CHAPTER SIX

Table 48. Logistic Regression Predicting Reduction in Charge Severity, Agency and Loose Offense-Based Definitions (Fixed Effects for District)

	Agency				Loose			
	Odds Ratio				Odds Ratio			
Any Codefendant[1]	1.67	*	-		1.24	***	-	
Number of Codefendants[1]	-		0.96		-		0.98	***
Male	1.14		1.10		0.94		0.95	
Age	0.98		0.98		1.00		0.99	*
Black	0.57		0.56		1.14	*	1.14	*
Other Race	0.42		0.45		0.90		0.91	
Married	1.16		1.21		0.89	*	0.89	*
Private Counsel	0.86		0.78		0.65	***	0.64	***
Violent Offense	8.14		7.02	*	-		-	
Drug Offense	0.65		0.88		-		-	
Regulatory	1.09		1.08		-		-	
Misdemeanor/Petty	3.67	*	3.70	*	4.24	***	3.91	***
Total Counts at Filing	1.00		1.00		1.00		1.00	
Number of Charges	2.39	***	2.53	***	2.16	***	2.20	***
Risk of Incarceration at Filing	0.96		0.95		0.91	***	0.93	***
Risk of Fine at Filing	1.23	***	1.22	***	1.19	***	1.19	***
N	742		742		14,646		14,646	
LR X^2	140.23		137.58		2445.92		2440.25	

[1] One-tailed p-value. All others two-tailed unless noted
* p<.05
** p<.01
***p<.005

Table 49. Logistic Regression Predicting Reduction in Charge Severity, Yale and Strict Offense-Based Definitions (Fixed Effect for District)

	Yale				Strict			
	Odds Ratio				Odds Ratio			
Any Codefendant[1]	1.51	***	-		0.90		-	
Number of Codefendants[1]	-		0.99		-		1.00	
Male	0.94		0.95		0.83		0.82	
Age	0.99		0.99	*	1.00		1.00	
Black	1.21		1.20		1.11		1.11	
Other Race	0.92		0.94		0.79		0.78	
Married	0.95		0.96		0.83		0.83	
Private Counsel	0.58	***	0.57	***	0.60	***	0.60	
Violent Offense	-		-		-		-	
Drug Offense	-		-		-		-	
Regulatory	<0.01		<0.01		0.78		0.77	
Misdemeanor/Petty	1.46		1.19		6.16	***	6.28	***
Total Counts at Filing	0.99	*	0.99	*	1.00		1.00	
Number of Charges	2.15	***	2.21	***	2.29	***	2.27	***
Risk of Incarceration at Filing	0.98		1.00		0.87	***	0.87	***
Risk of Fine at Filing	1.17	***	1.16	***	1.27	***	1.26	***
N	5,887		5,887		3,600		3,600	
LR X^2	1083.79		1067.48		6229.13		628.60	

[1] One-tailed p-value. All others two-tailed unless noted
* p<.05
** p<.01
***p<.005

Table 50. Logistic Regression Predicting Count Reduction, Agency and Loose Offense-Based Definitions (Fixed Effect for District)

	Agency				Loose			
	Odds Ratio				Odds Ratio			
Any Codefendant[1]	1.92	*	-		1.43	***	-	
Number of Codefendants[1]	-		1.16	*	-		1.02	*
Male	0.70		0.69		1.34	***	1.35	***
Age	1.00		1.00		1.01	***	1.01	***
Black	1.85		2.02		1.26	**	1.26	***
Other Race	5.63	***	6.99	*	1.19		1.20	
Married	1.47		1.51		1.04		1.05	
Private Counsel	0.75		0.70		0.90		0.89	
Violent Offense	8.41	*	9.46	**	-		-	
Drug Offense	0.50		0.39		-		-	
Regulatory	1.53		1.61		-		-	
Misdemeanor/Petty	0.45		0.49		0.64		0.58	*
Total Counts at Filing	1.04	*	1.04	*	1.01	***	1.01	***
Number of Charges	1.47	*	1.50	**	1.38	***	1.41	***
Risk of Incarceration at Filing	1.02		1.02		0.99		0.99	
Risk of Fine at Filing	1.23	*	1.24	*	1.06	***	1.06	***
N	555		555		14,440		14,440	
LR X^2	61.89		62.27		350.18		333.25	

[1] One-tailed p-value. All others two-tailed unless noted
* p<.05
** p<.01
***p<.005

Table 51. Logistic Regression Predicting Count Reduction, Yale and Strict Offense-Based Definitions (Fixed Effect for District)

	Yale				Strict			
	Odds Ratio				Odds Ratio			
Any Codefendant[1]	1.36	**	-		1.52	**	-	
Number of Codefendants[1]	-		1.02		-		1.05	*
Male	1.14		1.15		1.20		1.21	
Age	1.00		1.00		1.01		1.00	
Black	0.94		0.95		1.36		1.38	
Other Race	0.76		0.77		1.38		1.40	
Married	1.14		1.15		1.10		1.09	
Private Counsel	0.83		0.82		0.78		0.79	
Violent Offense	-		-		-		-	
Drug Offense	-		-		-		-	
Regulatory	<0.01		<0.01		2.65	***	2.61	***
Misdemeanor/Petty	0.14		0.12	***	1.13		1.09	
Total Counts at Filing	1.01	***	1.01	***	1.02	*	1.02	*
Number of Charges	1.27	***	1.29	***	1.44	***	1.45	***
Risk of Incarceration at Filing	1.10	***	1.12	***	0.89	**	0.89	**
Risk of Fine at Filing	0.98	***	0.97		1.11	*	1.10	*
N	5,523		5,523		3,126		3,126	
LR X^2	136.22		131.08		111.53		109.58	

[1] One-tailed p-value. All others two-tailed unless noted
* p<.05
** p<.01
***p<.005

Table 52. Logistic Regression Predicting Dropped/Dismissed Charges, Agency and Loose Offense-Based Definitions (Fixed Effect for District)

	Agency				Loose			
	Odds Ratio				Odds Ratio			
Any Codefendant[1]	1.40		-		1.26	**	-	
Number of Codefendants[1]	-		1.22		-		1.00	
Male	0.54		0.51		1.02		1.02	
Age	1.01		1.01		1.01	**	1.01	*
Black	0.69		0.74		0.97		0.99	
Other Race	1.16		1.17		1.03		1.03	
Married	0.67		0.70		0.93		0.93	
Private Counsel	0.70		0.66		0.52	***	0.51	***
Violent Offense	<0.01		<0.01		-		-	
Drug Offense	3.79		3.61		-		-	
Regulatory	1.30		1.34		-		-	
Misdemeanor/Petty	6.67	*	7.48	*	1.40		1.30	
Total Counts at Filing	1.00		1.00		1.01	***	1.01	***
Number of Charges	1.69	*	1.70	**	1.95	***	1.98	***
Risk of Incarceration at Filing	0.88		0.88		0.98		0.99	
Risk of Fine at Filing	1.31	*	1.32	*	1.00		1.00	
N	276[2]		276		11,312		11,312	
LR X^2	26.46		27.51		640.65		634.44	

[1] One-tailed p-value. All others two-tailed unless noted

[2] This sample is dramatically smaller as a result of districts lost for lack of variation in the dependent variable

* p<.05

** p<.01

***p<.005

Table 53. Logistic Regression Predicting Dropped/Dismissed Charges, Yale and Strict Offense-Based Definitions (Fixed Effect for District)

	Yale				Strict			
	Odds Ratio				Odds Ratio			
Any Codefendant[1]	1.74	***	-		1.46	*	-	
Number of Codefendants[1]	-		1.02		-		0.99	
Male	0.87		0.86		1.04	***	1.05	
Age	1.00		1.01		1.02		1.02	***
Black	0.81		0.81		1.26		1.27	
Other Race	1.11		1.12		1.45		1.45	
Married	0.99		1.02		0.93		0.94	
Private Counsel	0.44	***	0.45	***	0.56		0.57	
Violent Offense	-		-		-		-	
Drug Offense	-		-		-		-	
Regulatory	<0.01		<0.01		0.55		0.57	
Misdemeanor/Petty	1.59		1.26		2.03		1.86	
Total Counts at Filing	1.02	***	1.02	***	1.02	**	1.02	*
Number of Charges	1.79	***	1.84	***	2.11	***	2.15	***
Risk of Incarceration at Filing	0.97		0.99		0.92		0.93	
Risk of Fine at Filing	1.00		0.99		0.94		0.95	
N	4,012		4,012		2,226		2,226	
LR X^2	310.47		296.73		163.97		161.15	

[1] One-tailed p-value. All others two-tailed unless noted
* p<.05
** p<.01
***p<.005

A4. MACKINNON DWYER MEDIATION

Table 54. Models Predicting Full and Direct Effects of Prosecution, Any Prison

	C			C'		
	Coef.	Std. Err.	P>z	Coef.	Std. Err.	P>z
White Collar Crime	-1.02	0.26	0.00	-1.08	0.26	0.00
Substantial Assistance	0.24	0.17	0.16	-0.02	0.17	0.92
Plea	0.31	0.22	0.15	0.32	0.22	0.14
Dropped/Dismissed Charges	-0.44	0.19	0.02	-0.41	0.19	0.03
Count Reduction	0.83	0.20	0.00	0.90	0.20	0.00
Severity Reduction	0.13	0.12	0.25	0.19	0.12	0.11
Guidelines Offense Score	-	-	-	0.17	0.02	0.00
Criminal History	0.45	0.05	0.00	0.58	0.05	0.00
Male	0.60	0.08	0.00	0.61	0.08	0.00
Black	-0.04	0.10	0.67	-0.04	0.10	0.70
Other Race	0.17	0.17	0.34	0.21	0.18	0.24
Age	-0.03	0.00	0.00	-0.03	0.00	0.00
Married	-0.08	0.09	0.35	-0.09	0.09	0.34
High School Grad	-0.62	0.10	0.00	-0.65	0.10	0.00
Any College	-0.55	0.10	0.00	-0.57	0.10	0.00
Other Education	-0.83	0.30	0.01	-0.83	0.30	0.01
Any Codefendants	-0.07	0.10	0.45	-0.07	0.10	0.49
Private Counsel	-0.18	0.13	0.18	-0.16	0.13	0.23
Violent	-0.03	0.43	0.95	0.10	0.44	0.82
Drug	1.29	0.78	0.10	1.41	0.77	0.07
Regulatory Offense	-0.82	0.87	0.35	-0.57	0.90	0.53
Number of Charges	0.24	0.04	0.00	0.21	0.04	0.00
Presentence Detention	2.26	0.09	0.00	2.25	0.09	0.00
Any Dependent	-0.01	0.08	0.88	-0.01	0.09	0.92
Accept Responsibility	-0.21	0.11	0.06	-0.17	0.11	0.12
AYpre2003	-1.17	1.04	0.26	-1.00	1.03	0.33
AY2003-7	-0.59	0.37	0.11	-0.49	0.38	0.19
AY2008	-0.28	0.14	0.05	-0.19	0.14	0.19
AY2009	-0.13	0.12	0.28	-0.03	0.12	0.78
AY2010	0.11	0.11	0.34	0.12	0.11	0.30
Amount of Financial Sanctions	0.00	0.00	0.26	0.00	0.00	0.00

Table 55. Model Predicting Indirect Effects of Prosecution, Any Prison

	Coef.	Std. Err.	P>t
White Collar Crime	0.37	0.16	0.02
Substantial Assistance	1.70	0.13	0.00
Plea	0.09	0.17	0.61
Dropped/Dismissed Charges	-0.24	0.14	0.08
Count Reduction	-0.21	0.16	0.19
Severity Reduction	-0.29	0.09	0.00
Criminal History	-0.76	0.03	0.00
Male	-0.12	0.06	0.05
Black	0.08	0.07	0.25
Other Race	-0.46	0.13	0.00
Age	0.01	0.00	0.01
Married	0.02	0.06	0.78
High School Grad	0.02	0.08	0.76
Any College	0.05	0.08	0.55
Other Education	-0.13	0.21	0.53
Any Codefendants	0.03	0.07	0.69
Private Counsel	-0.07	0.09	0.42
Violent	-0.94	0.32	0.00
Drug	-0.79	0.54	0.15
Regulatory Offense	-1.74	0.36	0.00
Number of Charges	0.15	0.03	0.00
Presentence Detention	0.11	0.07	0.11
Any Dependent	-0.16	0.06	0.01
Accept Responsibility	-0.15	0.09	0.08
AYpre2003	-0.76	0.76	0.32
AY2003-7	-0.75	0.24	0.00
AY2008	-0.78	0.10	0.00
AY2009	-0.64	0.09	0.00
AY2010	-0.06	0.08	0.48
Risk of Incarceration	-0.07	0.01	0.00
_cons	9.01	0.27	0.00

Table 56. Models Predicting Full and Direct Effects of Prosecution, Judicial Departure

	C			C'		
	Coef.	Std. Err.	P>z	Coef.	Std. Err.	P>z
White Collar Crime	0.40	0.12	0.00	0.38	0.12	0.00
Plea	-0.33	0.11	0.00	-0.25	0.11	0.03
Dropped/Dismissed Charges	-0.21	0.11	0.05	-0.18	0.11	0.09
Count Reduction	-0.65	0.08	0.00	-0.76	0.09	0.00
Severity Reduction	-0.16	0.07	0.01	-0.15	0.07	0.02
Guidelines Offense Score	-	-	-	0.03	0.00	0.00
Eligible No Prison	-1.77	0.06	0.00	-1.56	0.07	0.00
Criminal History	-0.17	0.02	0.00	-0.16	0.02	0.00
Male	-0.35	0.05	0.00	-0.37	0.05	0.00
Black	-0.01	0.05	0.81	-0.01	0.05	0.83
Other Race	-0.26	0.11	0.02	-0.25	0.11	0.02
Age	0.01	0.00	0.00	0.01	0.00	0.00
Married	0.10	0.05	0.04	0.09	0.05	0.06
High School Grad	-0.06	0.06	0.32	-0.07	0.06	0.25
Any College	0.00	0.06	0.96	-0.02	0.06	0.72
Other Education	0.17	0.17	0.33	0.17	0.17	0.33
Any Codefendants	0.06	0.05	0.25	0.06	0.05	0.28
Private Counsel	-0.09	0.06	0.15	-0.13	0.06	0.04
Violent	0.08	0.07	0.26	-0.12	0.08	0.16
Drug	-0.25	0.28	0.36	-0.40	0.28	0.15
Regulatory Offense	-0.57	0.26	0.03	-0.53	0.26	0.04
Number of Charges	-0.06	0.02	0.00	-0.08	0.02	0.00
Presentence Detention	-0.69	0.06	0.00	-0.73	0.06	0.00
Any Dependent	-0.03	0.05	0.58	-0.03	0.05	0.55
Accept Responsibility	0.03	0.03	0.39	0.00	0.03	1.00
AYpre2003	0.12	0.55	0.84	0.21	0.55	0.70
AY2003-7	-0.34	0.25	0.16	-0.33	0.25	0.18
AY2008	-0.15	0.08	0.06	-0.11	0.08	0.14
AY2009	-0.16	0.06	0.01	-0.14	0.06	0.03
AY2010	-0.01	0.06	0.82	-0.01	0.06	0.89
Amount of Financial Sanctions	0.00	0.00	0.69	0.00	0.00	0.55

Table 57. Model Predicting Indirect Effects of Prosecution, Judicial Departure

	Coef.	Std. Err.	P>t
White Collar Crime	0.72	0.22	0.00
Plea	-2.41	0.20	0.00
Dropped/Dismissed Charges	-1.08	0.18	0.00
Count Reduction	3.21	0.14	0.00
Severity Reduction	-0.35	0.11	0.00
Eligible No Prison	-7.90	0.10	0.00
Criminal History	-0.35	0.03	0.00
Male	0.46	0.09	0.00
Black	-0.05	0.09	0.62
Other Race	-0.45	0.18	0.01
Age	0.03	0.00	0.00
Married	0.28	0.09	0.00
High School Grad	0.18	0.10	0.09
Any College	0.64	0.11	0.00
Other Education	-0.16	0.29	0.58
Any Codefendants	-0.03	0.09	0.74
Private Counsel	1.25	0.12	0.00
Violent	6.67	0.14	0.00
Drug	3.75	0.53	0.00
Regulatory Offense	-1.98	0.47	0.00
Number of Charges	0.56	0.04	0.00
Presentence Detention	0.93	0.09	0.00
Any Dependent	0.05	0.08	0.56
Accept Responsibility	0.94	0.06	0.00
AYpre2003	-2.76	1.00	0.01
AY2003-7	-0.48	0.39	0.21
AY2008	-1.11	0.13	0.00
AY2009	-0.86	0.11	0.00
AY2010	-0.13	0.11	0.21
Risk of Incarceration	0.25	0.02	0.00
_cons	12.65	0.31	0.00

Table 58. Models Predicting Full and Direct Effects of Prosecution, Prosecutorial Departure

	C			C'		
	Coef.	Std. Err.	P>z	Coef.	Std. Err.	P>z
White Collar Crime	0.29	0.15	0.06	0.26	0.15	0.09
Plea	0.07	0.14	0.65	0.12	0.14	0.40
Dropped/Dismissed Charges	0.03	0.11	0.83	0.06	0.11	0.57
Count Reduction	-0.80	0.10	0.00	-0.94	0.10	0.00
Severity Reduction	0.09	0.07	0.21	0.10	0.07	0.17
Guidelines Offense Score	-	-	-	0.05	0.01	0.00
Eligible No Prison	-1.99	0.08	0.00	-1.65	0.09	0.00
Criminal History	-0.05	0.02	0.01	-0.04	0.02	0.06
Male	-0.09	0.06	0.12	-0.13	0.06	0.03
Black	0.14	0.06	0.02	0.14	0.06	0.02
Other Race	0.05	0.12	0.66	0.08	0.12	0.51
Age	-0.01	0.00	0.00	-0.01	0.00	0.00
Married	0.05	0.06	0.38	0.04	0.06	0.50
High School Grad	-0.01	0.07	0.88	-0.02	0.07	0.75
Any College	0.10	0.07	0.19	0.06	0.07	0.42
Other Education	-0.23	0.22	0.29	-0.24	0.22	0.29
Any Codefendants	0.71	0.06	0.00	0.72	0.06	0.00
Private Counsel	0.32	0.07	0.00	0.27	0.07	0.00
Violent	-0.60	0.09	0.00	-0.89	0.09	0.00
Drug	-0.36	0.32	0.25	-0.57	0.32	0.08
Regulatory Offense	-0.39	0.30	0.20	-0.31	0.31	0.32
Number of Charges	-0.04	0.02	0.06	-0.07	0.02	0.00
Presentence Detention	-0.85	0.07	0.00	-0.90	0.07	0.00
Any Dependent	0.11	0.05	0.04	0.10	0.05	0.07
Accept Responsibility	0.52	0.05	0.00	0.41	0.05	0.00
AYpre2003	0.16	0.67	0.81	0.36	0.67	0.59
AY2003-7	-0.61	0.32	0.06	-0.62	0.33	0.06
AY2008	-0.13	0.09	0.14	-0.07	0.09	0.42
AY2009	-0.22	0.07	0.00	-0.19	0.07	0.01
AY2010	-0.05	0.07	0.45	-0.05	0.07	0.50
Amount of Financial Sanctions	0.00	0.00	0.65	0.00	0.00	0.42

Table 59. Model Predicting Indirect Effects of Prosecution, Prosecutorial Departure

	Coef.	Std. Err.	P>t
White Collar Crime	0.59	0.25	0.02
Plea	-1.74	0.21	0.00
Dropped/Dismissed Charges	-0.90	0.19	0.00
Count Reduction	2.82	0.14	0.00
Severity Reduction	-0.21	0.12	0.07
Eligible No Prison	-8.47	0.11	0.00
Criminal History	-0.39	0.03	0.00
Male	0.54	0.09	0.00
Black	0.02	0.10	0.84
Other Race	-0.52	0.19	0.01
Age	0.03	0.00	0.00
Married	0.28	0.09	0.00
High School Grad	0.14	0.11	0.22
Any College	0.50	0.11	0.00
Other Education	-0.31	0.32	0.34
Any Codefendants	-0.05	0.09	0.59
Private Counsel	1.04	0.12	0.00
Violent	6.31	0.15	0.00
Drug	2.74	0.57	0.00
Regulatory Offense	-2.15	0.50	0.00
Number of Charges	0.50	0.04	0.00
Presentence Detention	0.66	0.10	0.00
Any Dependent	0.12	0.09	0.15
Accept Responsibility	1.22	0.07	0.00
AYpre2003	-3.13	1.08	0.00
AY2003-7	-0.45	0.41	0.28
AY2008	-1.17	0.14	0.00
AY2009	-0.82	0.12	0.00
AY2010	-0.12	0.11	0.31
Risk of Incarceration	0.29	0.02	0.00
_cons	11.79	0.34	0.00

Table 60. Models Predicting Full and Direct Effects of Prosecution, Any Departure

	C			C'		
	Coef.	Std. Err.	P>z	Coef.	Std. Err.	P>z
White Collar Crime	0.36	0.11	0.00	0.34	0.11	0.00
Plea	-0.21	0.10	0.03	-0.12	0.10	0.22
Dropped/Dismissed Charges	-0.09	0.09	0.33	-0.04	0.09	0.64
Count Reduction	-0.69	0.07	0.00	-0.83	0.07	0.00
Severity Reduction	-0.05	0.05	0.33	-0.04	0.05	0.45
Guidelines Offense Score				0.04	0.00	0.00
Eligible No Prison	-1.83	0.05	0.00	-1.52	0.06	0.00
Criminal History	-0.12	0.01	0.00	-0.11	0.01	0.00
Male	-0.25	0.04	0.00	-0.27	0.04	0.00
Black	0.05	0.04	0.25	0.05	0.04	0.26
Other Race	-0.15	0.09	0.09	-0.13	0.09	0.13
Age	0.00	0.00	0.72	0.00	0.00	0.78
Married	0.08	0.04	0.07	0.06	0.04	0.12
High School Grad	-0.06	0.05	0.28	-0.07	0.05	0.17
Any College	0.03	0.05	0.58	-0.01	0.05	0.91
Other Education	-0.02	0.15	0.92	-0.02	0.15	0.91
Any Codefendants	0.33	0.04	0.00	0.32	0.04	0.00
Private Counsel	0.10	0.05	0.06	0.05	0.05	0.33
Violent	-0.19	0.06	0.00	-0.49	0.07	0.00
Drug	-0.34	0.24	0.16	-0.56	0.24	0.02
Regulatory Offense	-0.52	0.22	0.02	-0.47	0.22	0.03
Number of Charges	-0.04	0.02	0.01	-0.07	0.02	0.00
Presentence Detention	-0.72	0.05	0.00	-0.76	0.05	0.00
Any Dependent	0.02	0.04	0.57	0.02	0.04	0.65
Accept Responsibility	0.19	0.03	0.00	0.14	0.03	0.00
AYpre2003	-0.07	0.48	0.88	0.09	0.48	0.86
AY2003-7	-0.42	0.21	0.05	-0.41	0.21	0.05
AY2008	-0.15	0.06	0.02	-0.10	0.06	0.13
AY2009	-0.19	0.05	0.00	-0.15	0.05	0.00
AY2010	-0.03	0.05	0.53	-0.02	0.05	0.64
Amount of Financial Sanctions	0.00	0.00	0.53	0.00	0.00	0.27

Table 61. Model Predicting Indirect Effects of Prosecution, Any Departure

	Coef.	Std. Err.	P>t
White Collar Crime	0.65	0.21	0.00
Plea	-2.37	0.19	0.00
Dropped/Dismissed Charges	-0.96	0.17	0.00
Count Reduction	3.03	0.13	0.00
Severity Reduction	-0.37	0.10	0.00
Eligible No Prison	-7.90	0.09	0.00
Criminal History	-0.32	0.03	0.00
Male	0.57	0.08	0.00
Black	-0.07	0.09	0.40
Other Race	-0.42	0.17	0.02
Age	0.03	0.00	0.00
Married	0.29	0.08	0.00
High School Grad	0.25	0.10	0.01
Any College	0.71	0.10	0.00
Other Education	-0.16	0.28	0.56
Any Codefendants	0.01	0.08	0.92
Private Counsel	1.15	0.11	0.00
Violent	6.31	0.13	0.00
Drug	3.77	0.49	0.00
Regulatory Offense	-2.14	0.44	0.00
Number of Charges	0.55	0.03	0.00
Presentence Detention	0.91	0.09	0.00
Any Dependent	0.11	0.08	0.14
Accept Responsibility	1.18	0.06	0.00
AYpre2003	-3.04	0.96	0.00
AY2003-7	-0.35	0.38	0.36
AY2008	-1.18	0.13	0.00
AY2009	-0.88	0.10	0.00
AY2010	-0.13	0.10	0.20
Risk of Incarceration	0.29	0.02	0.00
_cons	11.99	0.30	0.00

A5. KHB MEDIATION

Table 62. KHB Mediation Analysis of Discretionary Prison (Agency Sample)

	OR	Std. Err.	P>z	Percent Mediated
Plea				
Reduced	1.38	0.30	0.133	
Full	1.35	0.29	0.163	
Diff	1.02	0.04	0.581	N/A
Severity Reduction				
Reduced	1.15	0.14	0.233	
Full	1.18	0.14	0.151	
Diff	0.97	0.04	0.488	N/A
Count Reduction				
Reduced	2.32	0.46	<0.001	
Full	2.38	0.47	<0.001	
Diff	0.97	0.04	0.514	-3.23%
Charges Dropped/Dismissed				
Reduced	0.62	0.12	0.011	
Full	0.66	0.13	0.027	
Diff	0.94	0.04	0.144	N/A

Table 63. KHB Mediation Analysis of Judicial Departure (Agency Sample)

	OR	Std. Err.	P>z	Percent Mediated
Plea				
Reduced	1.63	0.59	0.179	
Full	1.43	0.52	0.323	
Diff	1.14	0.33	0.657	N/A
Severity Reduction				
Reduced	0.78	0.13	0.126	
Full	0.98	0.16	0.889	
Diff	0.80	0.23	0.436	N/A
Count Reduction				
Reduced	0.58	0.18	0.074	
Full	0.66	0.20	0.168	
Diff	0.88	0.26	0.671	22.81
Charges Dropped/Dismissed				
Reduced	0.42	0.12	0.002	
Full	0.68	0.19	0.157	
Diff	0.62	0.18	0.102	55.06

Table 64. KHB Mediation Analysis of Prosecutor Departure (Agency Sample)

	OR	Std. Err.	P>z	Percent Mediated
Plea				
Reduced	2.21	1.14	0.123	
Full	1.98	1.02	0.183	
Diff	1.12	0.24	0.608	N/A
Severity Reduction				
Reduced	1.33	0.28	0.181	
Full	1.49	0.32	0.057	
Diff	0.89	0.19	0.576	N/A
Count Reduction				
Reduced	0.58	0.25	0.202	
Full	0.67	0.28	0.34	
Diff	0.87	0.19	0.525	N/A
Charges Dropped/Dismissed				
Reduced	0.57	0.20	0.114	
Full	0.74	0.26	0.403	
Diff	0.77	0.16	0.217	N/A

Table 65. KHB Mediation Analysis of Any Departure (Agency Sample)

	OR	Std. Err.	P>z	Percent Mediated
Plea				
Reduced	1.86	0.57	0.044	
Full	1.61	0.49	0.123	
Diff	1.16	0.30	0.579	23.36
Severity Reduction				
Reduced	0.92	0.13	0.563	
Full	1.10	0.15	0.479	
Diff	0.84	0.22	0.498	N/A
Count Reduction				
Reduced	0.59	0.15	0.037	
Full	0.70	0.18	0.157	
Diff	0.84	0.22	0.512	31.97
Charges Dropped/Dismissed				
Reduced	0.49	0.11	0.002	
Full	0.73	0.17	0.164	
Diff	0.68	0.18	0.137	54.89

A6. RANDOM EFFECTS MODEL

Table 66. Odds Ratios for Logistic Regression Predicting Any Prison or Departure with Random Effects for District (Agency Sample)

	Discretionary Prison		Judge Downward Departure/Variance		5K Departure		Any Departure	
White Collar	0.33	***	1.46	***	1.30		1.40	0.33
Substantial Assistance	0.98		-					0.98
Plea	1.39		0.78	*	1.11		0.89	1.39
Dropped/Dismissed Charges	0.64	*	0.84		1.11		0.97	0.64
Count Reduction	2.47	***	0.47	***	0.39	***	0.44	2.47
Severity Reduction	1.19		0.85	*	1.09		0.95	1.19
Offense Score	1.19	***	1.03	***	1.05	***	1.04	1.19
Eligible No Prison	-		0.21	***	0.19	***	0.22	-
Criminal History	1.75	***	0.85	***	0.97		0.90	1.75
Male	1.87	***	0.70	***	0.88	*	0.77	1.87
Black	0.96		0.98		1.14	*	1.04	0.96
Hispanic	1.32		0.79	*	1.11		0.89	1.32
Other Race	0.97	***	1.01	***	0.99	***	1.00	0.97
Age	0.91		1.09		1.03		1.06	0.91
Married	0.52	***	0.93		0.98		0.93	0.52
High School	0.56	***	0.98		1.07		1.00	0.56
Any College	0.45	**	1.15		0.78		0.97	0.45
Other Education	0.94		1.05		2.03	***	1.37	0.94
Any Codefendants	0.86		0.89		1.32	***	1.06	0.86
Private Counsel	1.05		0.89		0.42	***	0.62	1.05
Violent	3.79		0.68		0.57		0.58	3.79
Drug	0.57		0.57	*	0.73		0.62	0.57
Regulatory Offense	1.25	***	0.92	***	0.93	***	0.93	1.25
Number of Charges	9.98	***	0.48	***	0.41	***	0.47	9.98
Presentence Detention	1.00		0.97		1.10		1.02	1.00
Any Dependents	0.83		1.00		1.52	***	1.16	0.83
Accept Responsibility	0.37		1.21		1.33		1.08	0.37
AY Pre-2003[1]	0.57		0.71		0.54		0.66	0.57
AY 2003-2007	0.84		0.89		0.93		0.90	0.84
AY 2008	0.96		0.87	*	0.83	*	0.86	0.96
AY 2009	1.14		0.99		0.96		0.98	1.14
AY 2010	1.00	***	1.00		1.00		1.00	1.00
Amount of Financial Penalties	0.09	***	1.28		0.15	***	1.09	0.09
Sigma_u	0.66		0.57		0.60		0.51	0.66
Rho	0.12		0.09		0.10		0.07	0.12

[1] AY= Amendment Year of the Guidelines
* p<.05
** p<.01

*** p<.005

Table 67. Odds Ratios for Logistic Regression Predicting Any Prison or Departure with Random Effects for District (Loose Sample)

	Discretionary Prison		Judge Downward Departure/Variance		5K Departure		Any Departure	
White Collar	0.92		1.12		1.17	*	1.18	***
Substantial Assistance	0.99		-		-		-	
Plea	1.24		0.80	*	1.06		0.88	
Dropped/Dismissed Charges	0.54	***	0.79	*	1.09		0.94	
Count Reduction	2.38	***	0.47	***	0.40	***	0.44	***
Severity Reduction	1.21		0.91		1.11		0.99	
Offense Score	1.19	***	1.03	***	1.05	***	1.04	***
Eligible No Prison	-		0.21	***	0.20	***	0.22	***
Criminal History	1.71	***	0.84	***	0.97		0.89	***
Male	1.78	***	0.70	***	0.88	*	0.77	***
Black	0.93		0.95		1.13	*	1.02	
Hispanic	1.29		0.79	*	1.14		0.91	
Other Race	0.97	***	1.00	*	0.99	***	1.00	
Age	0.93		1.10		1.02		1.06	
Married	0.54	***	0.94		0.96		0.94	
High School	0.56	***	1.00		1.04		1.00	
Any College	0.45	**	1.21		0.74		1.00	
Other Education	0.93		1.06		1.93	***	1.35	***
Any Codefendants	0.83		0.89		1.31	***	1.06	
Private Counsel	0.89		1.00		0.47	***	0.71	***
Violent	-		-		-		-	
Drug	-		-		-		-	
Regulatory Offense	1.24	***	0.93	***	0.93	***	0.94	***
Number of Charges	9.66	***	0.48	***	0.41	***	0.47	***
Presentence Detention	0.98		0.95		1.10		1.01	
Any Dependents	0.79	*	1.00		1.53	***	1.16	***
Accept Responsibility	0.39		1.49		0.97		1.14	
AY Pre-2003[1]	0.56		0.81		0.50	*	0.71	
AY 2003-2007	0.82		0.91		0.97		0.93	
AY 2008	1.03		0.90		0.86	*	0.89	*
AY 2009	1.11		1.00		0.99		1.00	
AY 2010	1.00	***	1.00		1.00		1.00	
Amount of Financial Penalties	0.11	***	1.15		0.13	***	0.91	
Sigma_u	0.64		0.58		0.60		0.52	
Rho	0.11		0.09		0.10		0.08	

[1] AY= Amendment Year of the Guidelines
* p<.05
** p<.01

*** p<.005

Table 68. Odds Ratios for Logistic Regression Predicting Any Prison or Departure with Random Effects for District (Yale Sample)

	Discretionary Prison		Judge Downward Departure/Variance		5K Departure		Any Departure	
White Collar	1.28	**	0.87	***	0.75	***	0.81	***
Substantial Assistance	0.95		-		-		-	
Plea	1.33		0.80	*	1.16		0.92	
Dropped/Dismissed Charges	0.63	*	0.76	*	1.03		0.90	
Count Reduction	2.58	***	0.47	***	0.39	***	0.44	***
Severity Reduction	1.13		0.89		1.08		0.97	
Offense Score	1.19	***	1.02	***	1.05	***	1.04	***
Eligible No Prison			0.20	***	0.20	***	0.21	***
Criminal History	1.76	***	0.83	***	0.96	*	0.88	***
Male	1.87	***	0.70	***	0.89		0.77	***
Black	0.95		0.96		1.10		1.02	
Hispanic	1.30		0.84		1.08		0.90	
Other Race	0.97	***	1.01	***	0.99	***	1.00	
Age	0.89		1.12	*	1.05		1.08	
Married	0.52	***	0.93		0.95		0.92	
High School	0.56	***	0.97		1.03		0.98	
Any College	0.43	***	1.17		0.80		0.99	
Other Education	0.94		1.04		1.99	***	1.35	***
Any Codefendants	0.81		0.96		1.34	***	1.11	*
Private Counsel	0.96		0.88		0.39	***	0.59	***
Violent	-		-		-		-	
Drug	-		0.21		1.49		0.56	
Regulatory Offense	1.25	***	0.93	***	0.93	***	0.94	***
Number of Charges	9.84	***	0.49	***	0.41	***	0.47	***
Presentence Detention	1.01		0.99		1.12	*	1.04	
Any Dependents	0.86		1.03		1.49	***	1.17	***
Accept Responsibility	0.39		1.27		1.45		1.17	
AY Pre-2003[1]	0.57		0.78		0.52		0.69	
AY 2003-2007	0.82		0.92		0.97		0.94	
AY 2008	0.99		0.88	*	0.85	*	0.87	**
AY 2009	1.14		1.00		0.98		0.98	
AY 2010	1.00	***	1.00		1.00		1.00	
Amount of Financial Penalties	0.08	***	1.24		0.16	***	1.08	
Sigma_u	0.64		0.58		0.60		0.53	
Rho	0.11		0.09		0.10		0.08	

[1] AY= Amendment Year of the Guidelines
* p<.05

** p<.01
*** p<.005

Table 69. Odds Ratios for Logistic Regression Predicting Any Prison or Departure with Random Effects for District (Strict Sample)

	Discretionary Prison		Judge Downward Departure/Variance		5K Departure		Any Departure	
White Collar	0.80	*	1.17	**	1.02		1.13	*
Substantial Assistance	0.99		-		-		-	
Plea	1.38		0.78	*	1.07		0.88	
Dropped/Dismissed Charges	0.64	*	0.79	*	1.07		0.93	
Count Reduction	2.48	***	0.47	***	0.40	***	0.44	***
Severity Reduction	1.14		0.89		1.09		0.97	
Offense Score	1.19	***	1.03	***	1.05	***	1.04	***
Eligible No Prison	-		0.22	***	0.20	***	0.22	***
Criminal History	1.77	***	0.84	***	0.97		0.89	***
Male	1.86	***	0.69	***	0.89		0.77	***
Black	0.97		0.97		1.14	*	1.04	
Hispanic	1.31		0.78	*	1.07		0.87	
Other Race	0.97	***	1.01	***	0.99	***	1.00	
Age	0.89		1.11	*	1.03		1.07	
Married	0.52	***	0.94		0.95		0.92	
High School	0.55	***	0.98		1.03		0.98	
Any College	0.43	***	1.21		0.77		1.00	
Other Education	0.92		1.07		2.02	***	1.38	***
Any Codefendants	0.83		0.90		1.32	***	1.06	
Private Counsel	0.87		0.92		0.41	***	0.63	***
Violent	-		-		-		-	
Drug	0.23	*	0.68		1.26		0.79	
Regulatory Offense	1.24	***	0.92	***	0.93	***	0.93	***
Number of Charges	9.76	***	0.50	***	0.40	***	0.47	***
Presentence Detention	1.00		0.97		1.10		1.02	
Any Dependents	0.85		1.01		1.53	***	1.17	***
Accept Responsibility	0.37		1.21		1.30		1.09	
AY Pre-2003[1]	0.57		0.78		0.50	*	0.68	
AY 2003-2007	0.79		0.91		0.95		0.92	
AY 2008	0.96		0.89		0.84	*	0.87	**
AY 2009	1.12		1.01		0.97		0.99	
AY 2010	1.00	***	1.00		1.00		1.00	
Amount of Financial Penalties	0.09	***	1.17		0.16	***	1.03	
Sigma_u	0.66		0.57		0.60		0.52	
Rho	0.12		0.09		0.10		0.08	

[1] AY= Amendment Year of the Guidelines

* p<.05
** p<.01
*** p<.005

A7. MULTINOMIAL MODEL FOR DEPARTURES

Table 70. Multinomial Sentencing Outcomes, Agency Definition

	Judicial Departure vs. Within Range		5K vs. Within Range		5K vs. Judicial Departure	
White Collar Crime	1.53	***	1.27		0.83	
Plea	0.82		0.98		1.20	
Charges Dropped/Dismissed	0.80	*	1.24	*	1.55	***
Count Reduction	0.50	***	0.38	***	0.75	*
Severity Reduction	0.83	***	1.00		1.19	*
Guidelines Offense Score	1.03	***	1.05	***	1.02	***
Eligible No Prison	0.23	***	0.23	***	1.00	
Criminal History	0.87	***	0.98		1.13	***
Male	0.75	***	0.93		1.25	***
Black	0.96		1.08		1.12	
Othe Race	0.94		1.23		1.31	*
Age	1.01	***	0.99	***	0.98	***
Married	1.05		0.98		0.93	
High School Grad	0.91		0.99		1.08	
Any College	1.00		1.07		1.07	
Other Education	0.98		0.75		0.77	
Any Codefendants	1.03		1.73	***	1.67	***
Private Counsel	0.95		1.36	***	1.44	***
Violent Offense	0.90		0.44	***	0.49	***
Drug Offense	0.83		0.57		0.69	
Regulatory Offense	0.44	***	0.66		1.49	
Number of Charges	0.93	***	0.95	*	1.02	
Presentence Detention	0.49	***	0.45	***	0.92	
An Dependents	0.98		1.07		1.10	
Acceptance of Responsibility	1.02		1.54	***	1.52	***
AYpre2003	0.88		0.94		1.07	
AY2003-7	0.67		0.53	*	0.78	
AY2008	0.85	*	0.94		1.10	
AY2009	0.87	*	0.82	***	0.94	
AY2010	1.00		0.94		0.93	
Amount of Financial Sanction	1.00		1.00		1.00	

_cons	1.08		0.15	***	0.13	***
N	15,923					
Pseudo R2	0.1178					

Table 71. Multinomial Sentencing Outcomes, Loose Definition

	Judicial Departure vs. Within Range		5K vs. Within Range		5K vs. Judicial Departure	
White Collar Crime	1.14	*	1.30	***	1.13	
Plea	0.84		0.95		1.13	
Charges Dropped/Dismissed	0.77	**	1.24	*	1.61	***
Count Reduction	0.51	***	0.39	***	0.76	*
Severity Reduction	0.89		1.01		1.13	
Guidelines Offense Score	1.03	***	1.06	***	1.03	***
Eligible No Prison	0.23	***	0.24	***	1.06	
Criminal History	0.86	***	0.98		1.14	***
Male	0.75	***	0.94		1.25	***
Black	0.94		1.07		1.14	*
Other Race	0.92		1.30	*	1.41	***
Age	1.01	***	0.99	***	0.98	***
Married	1.06		0.96		0.91	
High School Grad	0.93		0.98		1.05	
Any College	1.02		1.05		1.03	
Other Education	1.03		0.74		0.72	
Any Codefendants	1.04		1.66	***	1.61	***
Private Counsel	0.96		1.37	***	1.43	***
Violent Offense	1.02		0.54	***	0.53	***
Drug Offense	-		-		1.00	
Regulatory Offense	-		-		1.00	
Number of Charges	0.94	***	0.95	*	1.01	
Presentence Detention	0.48	***	0.45	***	0.93	
An Dependents	0.96		1.07		1.12	*
Acceptance of Responsibility	1.02		1.56	***	1.52	***
AYpre2003	1.10		0.73		0.67	
AY2003-7	0.76		0.49	*	0.64	
AY2008	0.86	*	0.96		1.12	
AY2009	0.90		0.85	*	0.94	
AY2010	1.03		0.96		0.93	

Amount of Financial Sanction	1.00		1.00		1.00	
_cons	0.91		0.11	***	0.12	***
N	16,876					
Pseudo R2	0.1177					

Table 72. Multinomial Sentencing Outcomes, Yale Definition

	Judicial Departure vs. Within Range		5K vs. Within Range		5K vs. Judicial Departure	
White Collar Crime	0.90	*	0.74	***	0.82	***
Plea	0.85		1.02		1.21	
Charges Dropped/Dismissed	0.73	***	1.16		1.58	***
Count Reduction	0.51	***	0.38	***	0.74	**
Severity Reduction	0.87	*	1.00		1.15	
Guidelines Offense Score	1.02	***	1.05	***	1.03	***
Eligible No Prison	0.21	***	0.23	***	1.09	
Criminal History	0.85	***	0.97		1.14	***
Male	0.76	***	0.96		1.26	***
Black	0.95		1.04		1.09	
Other Race	0.94		1.19		1.27	*
Age	1.01	***	0.99	***	0.98	***
Married	1.08		0.99		0.92	
High School Grad	0.92		0.95		1.04	
Any College	1.00		1.04		1.04	
Other Education	0.99		0.80		0.81	
Any Codefendants	1.02		1.69	***	1.66	***
Private Counsel	1.03		1.40	***	1.36	***
Violent Offense	0.90		0.40	***	0.45	***
Drug Offense	-		-		-	
Regulatory Offense	0.21		1.27		6.16	
Number of Charges	0.94	***	0.95	*	1.01	
Presentence Detention	0.50	***	0.45	***	0.89	
An Dependents	1.00		1.10		1.10	
Acceptance of Responsibility	1.04		1.52	***	1.46	***
AYpre2003	0.94		1.17		1.25	
AY2003-7	0.73		0.48	*	0.66	
AY2008	0.87		0.97		1.11	

AY2009	0.88	*	0.84	*	0.95	
AY2010	1.01		0.95		0.94	
Amount of Financial Sanction	1.00		1.00		1.00	
_cons	1.01		0.16	***	0.15	***
N	16,134					
Pseudo R2	0.1180					

Table 73. Multinomial Sentencing Outcomes, Yale Definition

	Judicial Departure vs. Within Range		5K vs. Within Range		5K vs. Judicial Departure	
White Collar Crime	1.19	***	1.01		0.85	*
Plea	0.82		0.95		1.15	
Charges Dropped/Dismissed	0.77	**	1.21		1.57	***
Count Reduction	0.51	***	0.39	**	0.75	*
Severity Reduction	0.87	*	0.99		1.14	
Guidelines Offense Score	1.03	***	1.05	***	1.02	***
Eligible No Prison	0.23	***	0.23	***	1.01	
Criminal History	0.86	***	0.98		1.15	***
Male	0.75	***	0.95		1.27	***
Black	0.95		1.07		1.13	
Other Race	0.89		1.18		1.32	*
Age	1.01	***	0.99	***	0.98	***
Married	1.07		0.97		0.91	
High School Grad	0.92		0.95		1.03	
Any College	1.00		1.03		1.03	
Other Education	1.02		0.74		0.73	
Any Codefendants	1.04		1.71	***	1.64	***
Private Counsel	0.96		1.36	***	1.41	***
Violent Offense	0.92		0.43	***	0.47	***
Drug Offense	-		-		-	
Regulatory Offense	0.85		1.18		1.39	
Number of Charges	0.93	***	0.95	*	1.02	
Presentence Detention	0.51	***	0.44	***	0.86	*
An Dependents	0.98		1.08		1.10	
Acceptance of Responsibility	1.03		1.55	***	1.51	***
AYpre2003	0.85		0.93		1.10	

AY2003-7	0.74		0.47	*	0.64	
AY2008	0.87		0.96		1.11	
AY2009	0.89		0.83	**	0.93	
AY2010	1.03		0.94		0.92	
Amount of Financial Sanction	1.00		1.00		1.00	
_cons	0.95		0.15	***	0.16	***
N	15,589					
Pseudo R2	0.1184					

A8. ETHNICITY COVARIATE

Table 74. Odds Ratios for Logistic Regression Predicting Any Prison or Departure with Fixed Effects for District (Agency Sample)

	Discretionary Prison		Judge Downward Departure/Variance		5K Departure		Any Departure	
White Collar	0.31	***	1.31	*	1.25		1.28	*
Substantial Assistance	0.93		-	-				
Plea	1.35		0.78	*	1.13		0.90	
Dropped/Dismissed Charges	0.73		0.79	*	1.10		0.94	
Count Reduction	3.16	***	0.45	***	0.38	***	0.42	***
Severity Reduction	1.22		0.83	**	1.10		0.94	
Offense Score	1.19	***	1.03	***	1.05	***	1.04	***
Eligible No Prison	-		0.21	***	0.21	***	0.22	***
Criminal History	1.85	***	0.84	***	0.97		0.89	***
Male	1.75	***	0.69	***	0.91		0.77	***
Black	1.07		0.97		1.13		1.04	
Hispanic	2.00	***	0.99		0.93		0.98	
Other Race	1.37		0.81		1.12		0.92	
Age	0.97	***	1.01	*	0.99	***	1.00	
Married	0.88		1.08		1.03		1.06	
High School	0.57	***	0.90		0.95		0.90	
Any College	0.68	***	0.94		1.01		0.96	
Other Education	0.49	*	1.24		0.83		1.03	
Any Codefendants	0.91		1.06		2.10	***	1.41	***
Private Counsel	0.84		0.86	*	1.34	***	1.05	
Violent	0.74		0.93		0.42	***	0.64	***
Drug	4.92		0.80		0.58		0.64	
Regulatory Offense	0.29		0.67		0.85		0.74	
Number of Charges	1.23	***	0.90	***	0.94	**	0.92	***
Presentence Detention	8.92	***	0.48	***	0.38	***	0.45	***
Any Dependents	0.97		0.97		1.09		1.01	
Accept Responsibility	0.89		0.99		1.52	***	1.15	***
AY Pre-2003[1]	0.36		1.13		1.03		0.95	
AY 2003-2007	0.74		0.66		0.55		0.62	*
AY 2008	0.79		0.89		0.96		0.92	
AY 2009	0.91		0.87	*	0.84	*	0.86	*
AY 2010	1.09		0.98		0.95		0.97	
Amount of Financial Penalties	1.00	***	1.00		1.00		1.00	
N	4,401		11,566		10,680		13,853	
LR X^2	1621.97		1547.7		1953.08		2491.02	

[1] AY= Amendment Year of the Guidelines
* p<.05

** p<.01
*** p<.005

Table 75. Odds Ratios for Logistic Regression Predicting Any Prison or Departure with Fixed Effects for District (Loose Sample)

	Discretionary Prison		Judge Downward Departure/Variance		5K Departure		Any Departure	
White Collar	0.89		1.14		1.25	**	1.22	***
Substantial Assistance	0.96		-	-	-	-		
Plea	1.23		0.82		1.06		0.90	
Dropped/Dismissed Charges	0.63		0.73	**	1.08		0.90	
Count Reduction	3.06	***	0.46	***	0.39	***	0.43	***
Severity Reduction	1.18		0.90		1.12		0.98	
Offense Score	1.19	***	1.03	***	1.05	***	1.05	***
Eligible No Prison	1.80	***	0.21	***	0.22	***	0.23	***
Criminal History	1.68	***	0.83	***	0.97		0.89	***
Male	1.01		0.69	***	0.90		0.76	***
Black	1.87	***	0.93		1.13		1.01	
Hispanic	1.32		1.00		0.98		1.02	
Other Race	0.97	***	0.82		1.15		0.95	
Age	0.90		1.00		0.99	***	1.00	
Married	0.61	***	1.08		1.02		1.06	
High School	0.69	***	0.91		0.92		0.90	
Any College	0.52	*	0.97		0.99		0.97	
Other Education	0.92		1.26		0.79		1.04	
Any Codefendants	0.84		1.06		1.97	***	1.37	***
Private Counsel	0.58		0.87	*	1.33	***	1.04	
Violent	-		1.07		0.51	***	0.76	***
Drug	-		-		-		-	
Regulatory Offense	1.21	***	-		-		-	
Number of Charges	8.88	***	0.91	***	0.94	*	0.93	***
Presentence Detention	0.97		0.47	***	0.38	***	0.45	***
Any Dependents	0.83		0.96		1.07		1.00	
Accept Responsibility	0.37		0.99		1.53	***	1.15	***
AY Pre-2003[1]	0.72		1.43		0.58		0.99	
AY 2003-2007	0.79		0.77		0.52		0.69	
AY 2008	0.96		0.91		1.00		0.95	
AY 2009	1.04		0.91		0.88		0.91	
AY 2010	1.00	*	1.01		0.98		1.00	
Amount of Financial Penalties	1.00	***	1.00		1.00		1.00	
N	4,707		12,332		11,390		14,695	
LR X^2	1690.98		1645.79		1990.76		2615.05	

[1] AY= Amendment Year of the Guidelines
* p<.05
** p<.01
*** p<.005

Table 76. Odds Ratios for Logistic Regression Predicting Any Prison or Departure with Fixed Effects for District (Yale Sample)

	Discretionary Prison		Judge Downward Departure/Variance		5K Departure		Any Departure	
White Collar	1.28	*	0.88	*	0.75	***	0.82	***
Substantial Assistance	0.91		-					
Plea	1.29		0.80		1.17		0.93	
Dropped/Dismissed Charges	0.72		0.72	***	1.01		0.87	
Count Reduction	3.38	***	0.46	***	0.38	***	0.43	***
Severity Reduction	1.16		0.85	*	1.08		0.95	
Offense Score	1.19	***	1.02	***	1.05	***	1.04	***
Eligible No Prison	1.85	***	0.20	***	0.21	***	0.22	***
Criminal History	1.78	***	0.83	***	0.96	*	0.88	***
Male	1.07		0.69	***	0.92		0.77	***
Black	1.97	***	0.95		1.08		1.01	
Hispanic	1.28		0.99		0.92		0.98	
Other Race	0.97	***	0.87		1.06		0.94	
Age	0.85		1.01	***	0.99	***	1.00	
Married	0.57	***	1.10		1.06		1.08	
High School	0.67	***	0.90		0.91		0.89	*
Any College	0.46	*	0.93		0.98		0.95	
Other Education	0.92		1.26		0.82		1.05	
Any Codefendants	0.79		1.06		2.06	***	1.39	***
Private Counsel	0.61		0.93		1.36	***	1.10	
Violent	-		0.93		0.39	***	0.62	***
Drug	-		-		-		-	
Regulatory Offense	1.22	***	0.23		1.80		0.64	
Number of Charges	8.89	***	0.91	***	0.94	*	0.93	***
Presentence Detention	0.97		0.49	***	0.38	***	0.46	***
Any Dependents	0.92		1.00		1.10		1.03	
Accept Responsibility	0.38		1.02		1.50	***	1.16	***
AY Pre-2003[1]	0.74		1.18		0.94		0.97	
AY 2003-2007	0.76		0.68		0.54		0.64	
AY 2008	0.92		0.93		0.99		0.96	
AY 2009	1.07		0.89		0.86		0.88	*
AY 2010	1.00	*	0.98		0.96		0.97	
Amount of Financial Penalties	1	***	1.00		1.00		1.00	
N	4,345		11,689		10,681		14,001	

LR X^2	1581.64	1629.99	1914.24	2528.1

[1] AY= Amendment Year of the Guidelines
* p<.05
** p<.01
*** p<.005

Table 77. Odds Ratios for Logistic Regression Predicting Any Prison or Departure with Fixed Effects for District (Yale Sample)

	Discretionary Prison		Judge Downward Departure/Variance		5K Departure		Any Departure	
White Collar	0.80	*	1.19	***	1.06		1.16	***
Substantial Assistance	0.94		-		-			
Plea	1.33		0.79		1.08		0.90	
Dropped/Dismissed Charges	0.73		0.74	*	1.06		0.90	
Count Reduction	3.18	***	0.46	***	0.39	***	0.42	***
Severity Reduction	1.17		0.87	*	1.09		0.95	
Offense Score	1.19	***	1.03	***	1.05	***	1.05	***
Eligible No Prison	1.86	***	0.22	***	0.21	***	0.23	***
Criminal History	1.76	***	0.83	***	0.97		0.88	***
Male	1.08		0.68	***	0.91		0.77	***
Black	2.00	***	0.95		1.14		1.03	
Hispanic	1.27		1.00		0.97		1.01	
Other Race	0.97	***	0.81		1.12		0.93	
Age	0.86		1.00	*	0.99	***	1.00	
Married	0.58	***	1.09		1.03		1.06	
High School	0.68	***	0.90		0.91		0.89	*
Any College	0.46	*	0.94		0.97		0.95	
Other Education	0.89		1.31		0.84		1.07	
Any Codefendants	0.81		1.08		2.08	***	1.42	***
Private Counsel	0.56		0.87	*	1.34	***	1.05	
Violent	-		0.97		0.43	***	0.66	***
Drug	0.14	*	-		-		-	
Regulatory Offense	1.22	***	0.47	*	1.22		0.64	
Number of Charges	8.78	***	0.90	***	0.94	*	0.92	***
Presentence Detention	0.97		0.50	***	0.37	***	0.46	***
Any Dependents	0.91		0.98		1.09		1.01	
Accept Responsibility	0.35		1.00		1.53	***	1.16	***
AY Pre-2003[1]	0.76		1.14		1.01		0.99	
AY 2003-2007	0.74		0.72		0.52		0.64	
AY 2008	0.90		0.91		0.99		0.94	
AY 2009	1.06		0.89		0.85	*	0.87	*
AY 2010	1.00	*	1.00		0.96		0.98	
Amount of Financial Penalties	1.00	***	1.00		1.00		1.00	

N	4,329	11,293	10,453	13,532
LR X^2	1591.3	1506.35	1909.4	2428.53

[1] AY= Amendment Year of the Guidelines
* p<.05
** p<.01
*** p<.005

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