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AN INVESTIGATION OF WHITE-COLLAR CRIMINAL SENTENCING DISPARITIES IN SIX FEDERAL DISTRICT COURTS

By

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A dissertation submitted in partial fulfillment
of the requirements for the

Doctor of Philosophy – Sociology

Department of Sociology
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Abstract

Since the turn of the century, sentencing research has consistently shown that certain aspects of the social context generally condition individual-level sentencing variations. I further explore this postulation in assessing how legal changes affect courtroom decisions; and in analyzing how extra-legal offender characteristics and judicial attributes influence the likelihood and length of white-collar incarceration sentences. The study hypothesized an emergent socio-temporal trend, largely driven by implementation of white-collar sentencing legislation and a return to judicial discretion, whereby white-collar offenders sentenced in the years post-*Booker* would receive more lenient punishments (i.e., less likely to be incarcerated and shorter incarceration sentences) than those before the *Booker* decision. The study also hypothesized that the effect of extra-legal and judicial attributes on sentencing outcomes would be most pronounced post-*Booker* when Guideline statutes were deemed advisory. Results for binary logistic, OLS, hierarchical logistic, and hierarchical linear regression are discussed, as well as general implications for the study of white-collar offending.

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An Investigation of White-Collar Criminal Sentencing Disparities in Six Federal District Courts

CHAPTER 1: Introduction

In a 2010 survey marking the 25th anniversary of the Sentencing Reform Act (1984)[1], district judges ranked statutory mandatory minimums, charging decisions, judicial discretion, and regional differences as the greatest factors contributing to criminal sentencing disparities (U.S. Sentencing Commission 2010). These findings are quite revealing given the Federal Sentencing Guidelines[2] established by the SRA originally set out to temper some of these very issues by eliminating the possibility for unbridled, indeterminate sentencing, and establishing a mathematical means to mete out punishments (28 U.S.C. §991 (b)). The Guidelines base sentences primarily on two offense components: (1) the seriousness of the offense, and (2) the defendant's criminal history (Tiede, Carp, and Manning 2010). Pogdor (2007) has argued sentencing becomes problematic in the case of white-collar offenders because the two components conflict: determination of offense level—which includes a calculation of amount of fraud loss suffered—can result in excruciating penalties for first-time offenders who pose no bodily threat to society or its members. Additionally, Hagan and Nagel (1982) aver "white-collar criminality is particularly susceptible to variation in treatment according to the context considered…[and] changes in public opinion, political philosophy, and the priorities of judges and jurisdictions in which prosecutions take place are likely to influence sentencing outcomes" (p. 266).

[1] Pub.L. No. 98-473, 98 Stat. (1987). Henceforth referred to as SRA.
[2] Henceforth referred to as Guidelines.

The landmark Yale Studies on white-collar crime, conducted from 1983-1991 just prior to the introduction of the Guidelines, reveal factors auxiliary to Guideline components resulted in wide sentencing variations for these offenders. Mann, Wheeler, and Sarat (1980) conducted in-depth interviews to gain insight into judicial attitudes towards sanctioning white-collar defendants. The researchers concluded that judges' rationale for sentencing differed significantly between white-collar and street criminals, namely after considering the collateral damage the white-collar accused face (even if acquitted) as harsh enough punishment. Judges interviewed also made allowances in some cases for white-collar offenders' sensitivity to the prison environment, the prevention of injury to innocent parties (such as offender dependents), the ability to compensate for crime via restitution and/or community services, and the opportunity to impose harsh fines to achieve deterrence. Wheeler, Weisburd, and Bode's (1982) study examined sentencing disparities countenanced by judicial discretion, and found judges weighted both *act-related* (crime sophistication, dollar victimization, spread of illegality, and maximum sentence possible) and *actor-related* variables (socioeconomic status, prior criminal record, and defendant's role in the offense) quite heavily in white-collar criminal cases. Of the "extra-legal" variables measured (that is, factors exterior or auxiliary to the established legal process), white males in their forties comprised the majority of common offenders, while gender and age variables emerged as strong predictors of leniency.

Sentencing white-collar offenders poses an especially difficult problem under the Guidelines in that these nonviolent property crimes often injure victims in manners that elude systematic and direct measurement (Mann, Wheeler, and Sarat 1980; Pogdor 2007). Existing literature suggests have the potential to cause greater and more far-reaching consequences than the average street or "common" crime (Burns and Meitle 2020). Johnson (2006) notes "as a

fundamental mechanism of social control in society, criminal sentencing is a forum where broad sociological concerns, such as equality under the law, meet individuated decision-making constraints, such as locally varying courtroom norms and individual courtroom actor influences" (260). White-collar crime research informed by sociological theory is fundamental to understanding general crime motivations and to expanding the discipline; its findings are critical for the development and advancement of proactive crime prevention strategies and refined sentencing approaches, remaining important for researchers, policymakers, and fraud prevention teams alike. Most notably, explanations of white-collar sentencing disparities question the roles that power, status, and prestige—characteristics largely thought to correlate with opportunity to commit a white-collar offense—play in historically producing overly lenient punishments for this offending group (Mann, Wheeler, and Sarat 1980; Simpson 2013). The current study investigates the conditioning effect of the socio-temporal context on white-collar criminal sentencing disparities, and assesses how various legal, judicial, and extra-legal case characteristics might influence (1) incarceration decisions and, (2) the sentence length ruling once an "in" decision is handed down. I commence this study by reviewing white-collar criminological research, theoretical perspectives on sentencing disparities, and sentencing directives relevant to white-collar offenders.

CHAPTER 2: Literature Review

Despite the term's wide usage since Sutherland's (1940) original conception (as economic crimes perpetrated by respectable and respected professionals), the meaning of "white-collar crime" eludes consistent operationalization (Friedrichs 2020). Disagreement in definition typically revolves around focus on the types of crimes committed or the types of persons who commit them (Hagan and Nagel 1982; Shapiro 1990; Simpson 2013). Offender-based definitions emphasize the violator's use of power and influence in the commission and concealment of the crime; note Sutherland's initial focus on crimes of the elite within an occupational setting (Sutherland 1940; Simpson 2013). While "white-collar" and "corporate" crime have often been discussed interchangeably, the latter delimits misconduct in professions, business, or industry committed by organizational stakeholders for the overall benefit of the corporation (Coomber et. al 2015). Bribery, corruption, stock market manipulation, and misrepresentation of services/products (e.g., Ponzi schemes) fall under this category. Additionally, occupational crime describes transgressions involving the use of one's occupational resources "for personal enrichment through deliberate misuse or misapplication of the employing organization's resources or assets" (Holtfreter, 2005; Simpson 2013). These offenses include asset misappropriation, corruption, and making fraudulent statements on behalf of one's employer (Holtfreter, 2005).

The current study utilizes the broader offense-based approach, denoting white-collar crimes as *non-violent financial or property crimes committed through some form of trust contravention* (see Shapiro 1990). This definition underscores four crucial points of departure:

(1) "non-violent" meaning their commission does not involve physical force;

(2) “financial or property crimes” as distinct from crimes against persons, animals, the environment, or justice;

(3) evidence of “trust contravention” i.e., fraud or deception is necessary, whether public or private; and, most notably,

(4) violations range from those committed by low-level minor offenders to the rich and powerful (Galvin and Simpson 2020).

Offenses fitting the above description include antitrust violations, asset misappropriation, blackmail, bribery, corruption, counterfeiting, embezzlement, extortion, forgery, fraud/making fraudulent statements, money laundering, racketeering, and various tax offenses such as tax evasion. Examples of crimes that do not fit the given definition include environmental offenses, civil rights violations, immigration violations, shipping offenses, OSHA violations, or copyright infringement.

White-Collar Offending: A Challenge for Law Enforcement

White-collar criminality presents difficulties for law enforcement from reporting and detecting wrongdoing, to convicting and sentencing offenders. First, a relatively meager amount of white-collar cases reach the attention of crime control agencies and even fewer reach the bench. The USSC reports that in the 2015 fiscal year, fraud crimes accounted for the third largest portion (10.5%) of total federal criminal convictions, behind drug (31.8%) and immigration (29.3%) offenses. Dollar victimization in these cases ranged from $0 to more than $7 billion, with an average loss amount of about $2,900,000. The average sentence length imposed in fraud cases was 27 months (USSC 2016). Research further shows that over 90% of “federal sentencing cases result in a plea bargain and judges have little involvement in such cases

until the sentencing hearing" in which he or she can exercise discretion (Tiede, Carp, and Manning 2010; Bibas 2005).

According to a 2010 national survey, 55% of household victimizations were reported to an external agency such as a credit card company or personal attorney, while just under 12% of violations were reported to law enforcement. This was found despite the fact that these felonies were viewed as equally or slightly more serious than traditional crimes (Huff, Desilets, and Kane 2010; Coomber et al. 2015). In a study of small business employee embezzlement, Kennedy (2014) found that only 16% report incidents to the police. While research concerning reasons for non-reporting or underreporting violations of trust is scarce, Kennedy (2014) provides the following rationalizations:

(1) lack of confidence in the police and/or dissatisfaction with justice system outcomes;

(2) embarrassment;

(3) the loss was considered minimal; and/or

(4) business personnel had an emotional connection to the offending employee.

Several reasons can be generalized to the non-reporting habits of victimized individuals. Lack of awareness that one has been victimized, cost of pursuit, and insufficient evidence leading to a conviction might also explain general inclinations not to report white-collar crimes.

Shapiro (1990) argues the nature of trust relationships unavoidably invite potential trust violations, making them harder to detect and prosecute. "Principals exchange symbolic proxies—bank statements, stock certificates, mortgages, commodities future contracts, etc.—for tangible property. These pieces of paper or electronic impulses can be hidden, fabricated, or distorted more easily than the real commodities they represent" (p. 353). As a consequence, crimes can persist for years with very little evidence of their commission. She explains "the

social organization of trust taxes traditional deterrence methods of law enforcement, requires unique legal and private responses, and afford opportunities for intervention and punishment from which common criminals easily escape" (Shapiro 1990, p. 357). Albonetti (1998) notes that complex white-collar cases require "proactive" investigation to uncover evidence of wrongdoing, which creates avenues for defendants to "negotiate a desirable sentence reduction due to the greater leverage he/she derives from the greater complexity of the illegal activity" (p. 357; see also Poindexter and Moore 2016).

As for sentencing, a wide range of issues color deliberations over the fates of white-collar defendants. Yale Study findings coincide with Hagan and Nagel's (1982) assertion that equality before law "is perhaps nowhere more amorphous than in its application to the sentencing of white-collar offenders" (p. 264). Perpetrators are often charged as first-time offenders, even when their crimes persist for several years. It is clear that public perception, "including the need for the public to be wary of potential victimization to discourage potential offenders, and to ensure that those who commit such crime are appropriately punished" operates in addition to legalistic aspects of sentencing process (Burns and Meitle 2020, p. 289; Friedrichs 2020). Mann, Wheeler, and Sarat (1980) aver judges abstain from punishing offenders to the fullest extent of the law if restitution is possible. Even the United States Code cites "the need to provide restitution to any victims of the offense" as a pertinent factor to consider in imposing any criminal sentence (18 U.S.C. § 3553(a) (7)). Shapiro (1990) adds that leniency results from concern for the accused's victims and dependents who might be further devastated by and/or "feel the ripple effects of" an offender's internment (p. 356). White-collar offenders are assumed to have stable jobs and family lives (Burns and Meitle 2020). Defense counsel for the accused could argue that incarceration occasions excessive cruelty because the future dangerousness of

these offenders is diminished; the likelihood of recidivism is low since prosecution and conviction routinely results in loss of employment, status, and licensure among other resources, especially in high-impact cases involving "upstanding" citizens.

Theoretical Perspectives on Sentencing and Discretion

Rational choice models of decision-making posit that fully rational decisions can only be made with knowledge of all possible alternatives, of which information is limited (Albonetti 1987). Rationality denotes a style of behavior believed to be acceptable and conducive to the achievement of given goals within given contextual limits (Simon 1972; March and Simon 1958). Formal rational decisions also entail projections of preferred outcomes, further increasing the potential for uncertainty (Albonetti 1991). Constraints on information processing capacities can be explained by theories of *bounded rationality* which assume (1) the decision-making agent has incomplete information about alternatives; and (2) task complexity and environmental limitations are significant enough to hinder the actor from calculating the best course of action (Simon 1972, p.162; p.164). In the criminal justice system, limits on formal rationality are checked by formal policies and legal structures that work to "absorb" uncertainty, maintain uniformity, and reduce disparity (Albonetti 1987).

Research has documented how sentencing policies like the Guidelines have attempted to balance the dilemma between formally rational interests in uniformity with substantively rational interests in the flexibility to tailor individualized sentences when warranted (Savelsberg 1992; Steffensmeier and Demuth 2000; Ulmer and Kramer 1996). *Substantive political theory* explains the exercise of discretion as linked to the structure of and changes in sentencing policy (Ulmer, Light, and Kramer 2011, 801). "*Substantive rationality* means the intrusion of economic,

sociological, and ethical criteria upon formal rational reasoning and decision making" (Savelsberg 1992, 1346) – ideas external to law and the legal process (Ulmer 2012). In the exercise of social control through sentencing, this "entails consideration of defendants' particularistic circumstances, needs, or characteristics as well as the practical consequences for individuals and organizations" (Ulmer and Kramer 1996, p. 384). The resultant space for discretion increases potential for "wide variation in decision criteria between defendants of differing social statuses (Savelsberg 1992, p. 1348-49; Ulmer et. al 2011).

The *focal concerns* perspective on court decision-making further details the "substantively rational" criteria judges consider in making sentencing decisions, and the potential link between judicial discretion and sentencing disparity. It posits judges have incomplete information about defendants, and limited time in which to make crucial decisions that influence sentence severity (Albonetti 1987; Steffensmeier and Demuth 2000). To reduce uncertainty in rendering decisions, judges are guides by three focal concerns: (1) *blameworthiness,* (2) *protection of the community,* and (3) *practical constraints and consequences*. *Blameworthiness* relates to notions of offender culpability and whether justice was served. Assessments of culpability are influenced mainly by offense seriousness, the extent of harm caused, prior criminal history, and the offender's role in commission of the crime. Though calculating blameworthiness in white-collar crimes often poses problems for law enforcement officials, these concerns largely mirror the legal-rational considerations endorsed by the Guidelines to ensure uniformity in sentencing practices. Case information conducive to determining an offender's culpability should theoretically decrease uncertainty and permit calculated justifications of punishments rendered and their severity (Albonetti 1991). *Protection of the community* entails interest in deterrence efforts. Attributions of an offender's future dangerousness and propensity

for recidivism are often based on the nature of the offense and offender characteristics (Steffensmeier and Demuth 2000). While the collateral damage resulting from public accusation of a white-collar crime is thought to keep rates of offending and re-offending low (when compared to violent crimes), the possibility for great harm caused by crime commission behooves policymakers and officials to track characteristics, such as educational attainment or occupational status, that might correlate with increased opportunities to offend. Lastly, the focal concern regarding *practical constraints and consequences* describes organizational and societal limitations on decision-making; these include prosecutorial and pretrial decisions, concerns about the offender's ability to serve time, costs of pursuit, restitution and fines ordered, and disruption of ties to offender dependents (Bernstein, Kelly, and Doyle 1977). Here it is emphasized that sentence severity is not always a direct result of the offense; additional constraints include public concern for particular offenses, expectations of deviants and the legal system, and status labels from prior encounters with law enforcement (Bernstein, Kelly, and Doyle 1977). Common concerns of the judiciary emphasize federal judges may also rely on attributions linked to defendants' social characteristics and related offender labels (Steffensmeier and Demuth 2000), producing unwarranted biases which could influence the sentencing process (Ulmer et. al 2011). However, the selection and impact of criteria are largely contextual (Thomson and Zingraff 1981; Hewitt 2015).

This research assumes three main positions regarding theoretical connections between public processes, judicial discretion, and sentencing disparity. First, sentencing policies and the reasons for which they are implemented operate as practical constraints on judges' sentencing decisions (Crow and Bales 2006). Constraints in the white-collar context encompass issues related to availability of criminal justice resources, decision effects on commercial,

governmental, organizational and other societal arrangements, including public opinions toward these criminals and their crimes.

Second, the study follows previous research in assuming substantive rationality is increased to a greater extent under different policing systems, since the implementation of new laws can either "narrow" or "loosen" restrictions on sentencing. Consequently, differing degrees of criteria influence should characterize sentencing under different law periods (Crow and Bales 2006; Ulmer et. al 2011). I argue the socio-temporal variable is key to contextually situate the concerns officials might consider in the prosecution and punishment of white-collar offenders, and the impact of various legal, judicial, and extra-legal case characteristics on sentencing outcomes. For example, the early 2000s saw the deliberative political process altered by large scale financial scandals and resultant public opinion on white collar crimes (Poindexter and Moore 2006). Subsequent legislation led to a noted increase in white-collar imprisonment (USSC 2008).

Third, this study assumes that increased opportunity to sentence according to substantively rational interests also permits negative outcomes of flexibility in the legal process—namely, the permission of bias in sentencing. If bias begets extra-legal disparity, a more pressing issue becomes whether greater discretion creates opportunity for discrimination to go undetected and unchecked under formal-legal protections.

Legal-Rational and Temporal Aspects of White-Collar Sentencing

The two principal purposes of the U.S. Sentencing Commission[3] are to (1) "establish sentencing policies and practices for the federal criminal justice system, and (2) "develop the

[3] Henceforth referred to as USSC.

means of measuring the degree to which sentencing, penal, and correctional practices are effective in meeting the purposes of sentencing" (USSC 2010). Guideline parameters produce an extensive Sentencing Table based on intersections of crime seriousness and prior criminal history (**Appendix A**). For example, a defendant with no prior criminal record convicted of embezzlement resulting in a $100,000 loss involving 10 or more victims, and bringing about substantial hardship for at least five victims, could receive between 27-33 months of imprisonment possibly combined with other penalties. Sentence calculations are decided in post-conviction proceedings wherein a judge can exercise his/her discretion with regard to fact- and law-oriented findings (Schanzenbach and Tiller 2007). If facts constituting "aggravating and mitigating circumstances" pertinent to sentencing are discovered, the judge may adjust the base offense level (28 U.S.C. §991 (b)). These conditions, such as acceptance of responsibility or playing a major role in the crime, are specified in the Guidelines. A judge may also choose to depart upward or downward from the mandated sentencing range "based on law-oriented conclusions that have *not* been proscribed by the Guidelines" (Schanzenbach and Tiller 2007, p. 25, emphasis added). In these instances, the judge must provide "specific reasons for departure in open court at the time of sentencing;" circuit courts may subsequently reverse decisions in which circumstances are not sufficiently unusual as to warrant a sentence range departure (USSG §5K2.0 (e)).[4] In *Rita v. United States* (551 U.S. 338, 2007), the Supreme Court held that a court of appeals may apply a presumption of reasonableness to a sentence imposed by a district court within a properly calculated guideline range without violating the Sixth Amendment. Example rationales for departures include age of the offender, use of a weapon in commission of the crime, or mental impairment. Steffensmeier and Demuth (2000, p. 726) note these "windows of

[4] U.S. Sentencing Commission. 2016. "2016 Guidelines Manual." Retrieved from https://www.ussc.gov/guidelines/2016-guidelines-manual.

discretion" built into the Guidelines "allow court actors to temper formal rules that attach highly specific sentences to highly specific charges (formal rationality) and adapt sentences to fit individual defendants or organizational contingencies (substantive rationality)."

Pogdor's (2007) argument that "the passage of new laws places certain individuals in greater jeopardy for being held criminally culpable" succinctly makes a case for examining the influence of relevant legislation on punishment severity (p. 744). This sentiment echoes Thomson and Zingraff's (1981) call for crime research to account for changes in public attitudes toward crime, in judicial legislation, and the processual nature of court decision-making to construct a more dynamic depiction of sentencing and the legal system overall. Theoretically, changes in societal attitudes toward white-collar offending should vary with this offending group's outcomes. Further, excluding socio-temporal factors from quantitative analysis could result in a significant omitted-variable bias, which in turn causes incorrect slope coefficients for independent variables (Thomas and Zingraff 1981). Several studies have tested variable effects on sentencing before and after *Booker* (Tiede, Carp, and Manning 2010; Hewitt 2015), but none of those examining white-collar crimes have accounted for the impact of relevant economic legislation on sanction severity. In the following sections, I detail a timeline of sentencing directives meant to curtail and define proper punishment for economic and white-collar criminal activities following passage of the Sentencing Reform Act (SRA): (1) the 2001 Economic Crime Package, (2) the Sarbanes-Oxley Act (SOX), (3) the *U.S. v. Booker* decision, and (4) the 2015 Guideline Amendments.

Sentencing Reform Act. The SRA was not without its dissenters, with criticisms lodged namely regarding punishment appropriateness, confusion over Guideline provisions, and loss of control. Defense counsel asserted the Guidelines infringed upon due process. Trial court judges

decried limits on judicial discretion, calling the Guidelines too rigid (Schulhofer and Nagel 1997). In fact, after the formal adoption of the Guidelines in 1987, "more than 200 trial judges ruled the SRA and guidelines unconstitutional in whole or in part" (Goldsmith 2004, p. 935). The USSC and SRA statute faced its greatest challenge in *Mistretta v. United States* (1989),[5] called forth on the grounds that it violated both the non-delegation and separation of powers doctrines. With the *Mistretta* decision, the constitutionality of the SRA was upheld, as it was deemed Congress did not delegate excessive legislative authority to the USSC to structure the Guidelines (so long as they adhered to the criteria Congress supplied), and the Guidelines took full effect shortly thereafter in all 94 district courts in 1990 (Howard, Lazarus, and Glas 2015; Schanzenbach and Tiller 2007).

2001 Economic Crime Package. The USSC (2008) cites the passage of the Economic Crime Package of 2001 (USSG §2B1.1) and the Sarbanes-Oxley Act of 2002[6] as major influences on increased rates of imprisonment for white-collar offenders. The USSC's Economic Crime Package came about as a result of a six-year long deliberative process regarding two interrelated issues: (1) sentence severity in federal economic offenses, and (2) lack of a clear definition of what constituted "loss"—the variable considered most prominent in these cases—in a federal economic crime. Per the fraud Guidelines (USSG §2B1.1), the court should use the greater of actual or intended loss, if intended loss was different from actual loss. Further, "loss need not be determined with precision. The court need only make a reasonable estimate of the loss, given the available information" (see USSG §2B1.1, comment. n.3(c)). Many believed confusion over these two components of the (pre-2001) Guidelines resulted in overly harsh punishments for offenders who committed economic and property crimes, while others believed

[5] 488 U.S. 361 (1989).
[6] Pub. L. No, 107-204, 116 Stat. 745 (2002).

punishments produced were exceedingly lenient (Bowman 2001). The Economic Crime Package attempted to streamline punishments for these crimes by (1) consolidating theft and fraud Guidelines, (2) redefining actual loss in terms of causation as "the reasonably foreseeable pecuniary harm that resulted from the offense," and (3) modifying the economic loss table to include offense level adjustments for crimes involving numerous victims (Bowman 2001). Some conflicting issues with this Guideline addition vis-à-vis white-collar crime entail:

(1) limitation of loss to pecuniary harm at the exclusion of "emotional harm, damage to reputation, disruption of personal and business relationships, or even physical illness" (Bowman 2001, p. 49),

(2) broad definitions of *attributable loss* that can "produce lifelong sentencing ranges for defendants who neither cause much economic harm nor derive much economic benefit from their crimes" (Hewitt 2015, p. 1032);

(2) *intended loss* calculations that grounds punishment in premeditated pecuniary harms that never happened, or would have been impossible or unlikely to occur; and

(3) loss table increases on offense level that are so large they might "overwhelm other factors that are arguably more relevant to the defendant's culpability" (Hewitt 2015, p. 1034).

Sarbanes-Oxley Act. The Sarbanes-Oxley (SOX) Act was passed in response to high-profile accounting malpractice cases in the early 2000s such as those involving Adelphia, WorldCom/MCI, and Xerox.[7] The year 2002 also saw the creation of the Corporate Fraud Task Force (CFTF) "to enhance the DOJ's prosecution of corporate entities." The CTFT was replaced in 2009 with the Financial Fraud Enforcement Task Force (FFETF), which "includes

[7] All of these cases commenced in 2002 in the Southern District of New York.

representatives from over 20 federal agencies, 94 US Attorney's Offices, and state and local agencies" (Poindexter and Moore 2016, p. 1). The Act was meant to protect corporate shareholders from fraudulent representations in financial "symbolic proxy" statements (Shapiro 1990). Notable sections of the new legislation include requiring (1) corporate management to certify that they have reviewed financial statements for accuracy and truth (15 U.S.C. § 7241); (2) disclosures of any relevant off-balance-sheet debts, obligations, or transactions that may exist (15 U.S.C. § 7261); that management state whether or not the company's internal control procedures are adequate and effective (15 U.S.C. § 7262(a)); public updates of significant financial matters as they arise (amendment to 15 U.S.C. 78m); and imposing penalties for destroying, concealing, or otherwise altering records with intent to impede investigations (18 U.S.C. § 1519).

U.S. v. Booker decision. In the monumental court case, *United States v. Booker*[8], the Supreme Court "invalidated the provisions of the SRA that made the Guidelines mandatory. The Court found the binding Guidelines scheme unconstitutional because it (sic) increased sentences [solely] on the basis of judicial fact finding rather than fact finding by a jury" or admission by the defendant, effectively violating the Sixth Amendment (Hewitt 2015, p. 1029). However, following the decision in November 2005, judges must still "take account of the Guidelines together with other sentencing goals" and "consider the Guidelines 'sentencing range established for … the applicable category of offense committed by the applicable category of defendant,' pertinent Sentencing Commission policy statements, and the need to avoid unwarranted sentencing disparities."[9] According to the USSC's *Report on the Impact Booker* (2006), the average sentence length increased in 2006 while the rate of incarceration remained the same.

[8] 543 U.S. 220 (2005).
[9] 543 U.S. 220 (2005).

The rate of government-sponsored, below-range sentences—i.e., sentences less than that suggested in the Sentencing Table—increased after the passage of *Booker* to 23.7%, and non-government-sponsored, below-range sentences increased to 12.5%. Lastly, rates of within-range sentences decreased for each of the twelve judicial circuits following *Booker*. Hewitt (2015) found that between 2002 and 2012, within-range sentences for economic crimes nationwide decreased from more than 80% of sentences before *Booker*, to only 50.6% of sentences in 2012. Similarly, judges in the Southern District of New York imposed government-sponsored below-range sentences 90% (or between 40-60 months) shorter than the minimum Guidelines range following *Booker*. Non-government-sponsored, below-range sentences were, on average, 50-70% (or approximately 15-25 months) shorter than the minimum guidelines range.

2015 Guideline amendments. More recently, the 2015 Amendments to the Guidelines applicable to federal economic crimes resulted in at least three significant changes (USSC 2015). First, the 2015 amendment "revises the victims table in §2B1.1 (b)(2) to specifically incorporate substantial financial hardship to victims as a factor in sentencing economic crime offenders," in lieu of the previous measure of a victim headcount (USSC 2015, p. 24). The amendment also adds a "non-exhaustive" list of factors for courts to consider in determining "substantial" hardship, including insolvency, inability to obtain credit, filing for bankruptcy, suffering loss to savings (retirement, education, investment, etc.), and/or making significant changes to employment and living arrangements. Second, it redefines "intended loss"— "harm that was intended to result from the offense" (USSG §2B1.1, comment. 3(a)(2))—as "pecuniary harm that the defendant purposely sought to inflict." This modification was meant to deduce culpability based on the defendant's subjective intent, rather than relying on an objective standard determined by the court. Lastly, the amendment refocuses punishment of an economic crime

"intentionally engaged in or caused conduct constituting sophisticated means" squarely on the defendant. That is, the court should only consider adjustments based on this specific offense characteristic if the defendant's "own intentional conduct" was sophisticated (USSG 2015, p. 25).

Substantive political theory implies the greatest risk for sentencing disparity arises following the *Booker* decision, while the *focal concerns* lense adds that case measures of blameworthiness and additional "extra-legal" considerations made in the interest of deterrence differentially affect sentencing outcomes; their effects are contextually contingent on practical decision-making constraints (Ulmer et. al 2011). To account for the impact of legislation on sentencing decisions, this study compares three time periods characterized by relative restraint on judicial discretion in the sentencing of white-collar offenders; era demarcations are comparable to those employed in extant research (see Ulmer et. al 2011). Cases disposed during the *mandatory guideline era* include fiscal years[10] starting at the creation of the Guidelines in 1987 through 2000; cases disposed during the *narrow guideline era* include pre-*Booker* fiscal years 2001 through 2005; those disposed post-*Booker* during the *advisory guideline era* include fiscal years 2006 through 2015. These theorized temporal anchors also demarcate variations in severity of sentencing outcomes as a result of legislated constraints on judges' sentencing decisions.

Influence of Extra-legal Characteristics on Sentencing Outcomes

The court specifically prohibits the submission of departures based on race, sex, national origin, creed, religion, or socio-economic status on the simply stated grounds such "factors are

[10] A fiscal year runs October 1 through September 30 (FY 1987: October 1, 1987- September 30, 1988).

not relevant in the determination of a sentence" (USSG §5K2.0 (d)(1)). Variance in the effects of extra-legal variables throughout the Guideline's lifetime would signal that new policies affecting sentencing outcomes differ in their degree of constraint on discretion. Despite stipulations set out in the Guidelines, extant white-collar criminal sentencing research produced mixed results in tests of various extra-legal offender demographics and quantifiable effects on sentencing outcomes. For example, Schanzenbach and Yaeger (2006) contend "having more dependents, higher levels of education, being older, U.S. citizenship, being female, and being white are all associated with lower prison terms" (p. 781). On the other hand, Holtfreter (2013) analyzed sentencing outcomes using data reported to the Association of Certified Fraud Examiners (ACFE) and found that extra-legal variables of race, gender, age, education, and workplace position had no effect on either sentence severity or sentence length when controlling for offender blameworthiness (i.e., criminal history and crime seriousness). The following sections summarize findings for five commonly studied extra-legal attributes—race/ethnicity, sex/gender, age, education, and class/socioeconomic status (SES).

Race/ethnicity. Although the USSC's *Final Report on the Impact of United States v. Booker on Federal Sentencing* (2006) stated black offenders received sentences that are 4.9% longer than white offenders, research findings regarding the influence of race on sentencing outcomes for white-collar criminals are not entirely consistent. Wheeler, Weisburd, and Bode's (1982) pioneer study discovered that "race [bore] utterly no relationship to the sanction at this end of the criminal system" (p. 656). The same is true of Weisburd, Waring, and Wheeler's (1990) replication study and Maddan et al.'s (2012) investigation comparing embezzlement offenders to auto thieves. In Hagan, Nagel, and Albonetti's (1980) investigation, ethnicity proved

significant in models of common crimes but not white-collar crimes.[11] However, Benson and Walker's (1988) modified version of the Wheeler et al. model showed significant effects of race on the length of sentence, but not the decision to incarcerate. Per Albonetti's (1998) legal-bureaucratic model of indirect and direct influences on white-collar criminal sentencing, black defendants received significantly longer imprisonment terms than white defendants, likely due to the fact that they were also more likely to be charged with statutory offenses that carried longer sentences. The effect of race in her study operated both directly and indirectly (via case complexity) on sentencing outcomes. For Schanzenbach and Yaeger (2006), black and Hispanic defendants were both more likely than white defendants to be incarcerated and receive long prison sentences *after* offense level adjustments are made, but not before.

Sex/gender. According to the USSC (2006), male offenders continued to receive substantially higher sentences than female offenders after the *Booker* decision. In regards to white-collar crimes, men are more likely to commit these offenses (Eitle 2000), obtained greater economic gain in committing them (Daly 1989), are more likely to be sentenced to jail as punishment (Weisburd, Waring, and Wheeler 1990; Wheeler, Weisburd, and Bode 1982), and receive longer prison sentences (Maddan et al. 2012). In other analyses examining sentencing outcomes, offender sex/gender was insignificant when significant legal variables were held constant (Benson and Walker 1988; Eitle 2000; Wheeler, Weisburd, and Bode 1982).

Age and education. A 2010 survey of U.S. district judges confirmed the majority of respondents considered age and education relevant factors in determining within-range sentences and departures in sentencing decisions (USSC 2010). These characteristics, in addition to class, appear most relevant to white-collar cases since the commission of crimes like asset

[11] These studies compare racial sentencing disparities using a dummy variable (either white to nonwhite, or white to black).

misappropriation, antitrust violations, and securities fraud entail positions that have taken time and high status to acquire, and "lend the appearance of legitimacy" to illicit dealings (Wheeler et al. 1988). In studies of white-collar sentencing disparities, age is found to be statistically significant in models of sentence severity, indicating older defendants receive slightly longer sentences (Maddan et al. 2012, Schanzenbach and Yaeger 2006). When age is squared, results achieve even greater significance (Weisburd, Waring, and Wheeler 1990; Wheeler, Weisburd, and Bode 1982), while other studies found age to be wholly insignificant (Benson and Walker 1988; Wheeler, Weisburd, and Bode 1982). Education is possibly the least studied variable of those discussed, though almost half of all federal offenders sentenced in the fiscal year 2015 (45.3%) had not completed high school, and only 6.3% of offenders had completed college (USSC 2016). Wheeler et al. (1988) reported the majority of white-collar crimes are committed by individuals with at least a high school degree, and the most damaging and complex white-collar offenses (e.g., antitrust violations and securities fraud) are more likely to be committed by college graduates than those of other educational backgrounds. On the contrary, Daly (1989) found "if completing college is an indication of status or occupational power, then most of the defendants [in her study], including those convicted of securities fraud and antitrust violations, do not fit a high-status profile." An offender's educational background, however, produced no significant effects on white-collar sentence severity in all but two studies described in this paper. Hagan, Nagel, and Albonetti (1980), using education as a proxy for social standing, observed college-educated white-collar defendants were sentenced more leniently than their less-educated or common-crime counterparts. Additionally, Maddan et al. (2012) reported only a college degree mattered in white-collar sentencing, and only in decisions to incarcerate.

Class/SES. As with the previously discussed extra-legal white-collar offender attributes, researchers have not found unequivocal support of class bias exists in sentencing decisions. Both cultural and empirical studies suggest power and status provide advantages to those who possess them, especially in the legal arena. For example, Mann, Wheeler, and Sarat (1980) note judges offered alternatives to prison in white-collar cases based on the defendant's ability to compensate victims, a decision that must account for the accused's earning power and financial status. However, the possession of wealth or other social capital poses less a concern in sentencing and deterrence measures than "the social organization of trust" and the perceived structural opportunities for trust abuse expected at particular status levels (Shapiro 1990, p. 359). Using Duncan SocioEconomic Index (SEI) scores, Wheeler, Weisburd, and Bode (1982) found a positive correlation between socioeconomic status and sanction severity (i.e., both decisions to incarcerate and sentence length); Weisburd, Waring, and Wheeler (1990) achieved the same results using a categorical measure of class position based on ownership of the means of production and workplace authority. In fact, the inclusion of the class measures in addition to SEI scores increased the effect of status in their models. These results suggest those with higher social status carry a greater social obligation, as thus are perceived by judges as more culpable for deliberately breaching trust. On the contrary, Eitle (2000) found that principals who occupy the highest structural location in the organization of work are the least likely to receive a punitive sanction in securities violations, whereas legitimate workers are most likely to be punished. For other analyses, neither socioeconomic status (Benson and Walker 1988) nor income (Maddan et al. 2012) produced significant effects on sanction severity.

These study findings indicate disparities under the Guidelines might be linked *less* to general discriminatory sentencing practices, and *more* to the overall effects of differential

offender treatment by different judges (e.g., geographical/district variations, experience levels, or ideological differences). The following section describes a largely unexplored area of white-collar criminal sentencing research: attributes of district judiciary.

Influence of Judicial Attributes on Sentencing Outcomes

Put plainly, the majority of sentencing studies, especially those reliant on secondary crime data from official sources, do not account for the potential impact of individual judges (Thomson and Zingraff 1981). Research has demonstrated modest correlations between appellate, state, and Supreme Court judicial demographics of caseload, tenure, gender, race, and appointing president's ideology and decision-making in crime cases (Berry 2015; Carp, Manning, and Stidham 2009; Geyh 2008), and at the district level (Johnson 2006; Johnson and Songer 2002; Nash 2015; Schanzenbach and Tiller 2007; Tiede, Carp, and Manning 2010). Of these, only caseload (however measured) and the appointing president's party affiliation prove consistent. Caseload rate—measured as a function of the annual average number of cases filed per judge—was found to be negatively associated with average sentence length (Ulmer, Eisenstein, and Johnson 2010). Tenure, most often operationalized as judge age, has not produced consistent results as to whether or not years of service influence judicial decision-making; however, Hendershot and Tecklenburg (2011) found longer tenure was associated with a liberal decision trend (i.e., deciding in favor of the defendant). Schanzenbach (2005) observed no correlations between either judge gender or race and sanction severity for black and Hispanic offenders, though sentencing disparities are evident. While conventional wisdom would imply the voting patterns of minority and female judges are somewhat more liberal than white male

judges due to historical experience with racial and gender discrimination, Carp, Manning, and Stidham (2009) concluded that ideology mattered most in comparison.

Studies on the relationship between the ideology of the appointing president at the district court level and judicial decision-making are less prevalent (Hendershot and Tecklenburg 2011). For street crimes, judges appointed by Republican presidents meted out significantly longer imprisonment sentences than their Democrat-appointed counterparts (Schanzenbach and Tiller 2007, Tiede, Carp, and Manning 2010). Nash (2015) found the opposite, attributing this finding to President Clinton's "tough" crime policies (p. 683). In the only study analyzing the influence of ideology on the sentencing of white-collar criminals at the district level, results were inconclusive but weakly suggested Republican-appointed judges meted out shorter sentences for white-collar and environmental crimes than Democrat-appointed judges (Schanzenbach and Tiller 2007).

Despite the lack of consistent findings, the existence of these inquiries begs the question of why studies analyzing white-collar criminal sentencing have neglected to consider characteristics of the district judiciary. For one, databanks that house information on criminal proceedings—such as the Administrative Office of the U.S. Courts, the Federal Judicial Center (FJC), the Public Access to Court Electronic Records system (PACER), or the Inter-university Consortium for Political and Social Research (ICPSR)—do not commonly measure judge attributes (Tiede, Carp, and Manning 2010). In a 2010 survey of U.S. district judges, when asked if the Commission should report judge-specific sentencing data to promote transparency in sentencing, 53% of responding judges disagreed; only 24% of judges agreed with the proposal (USSC 2010). These issues problematize analyses concerning the impact of judicial characteristics on sentencing disparities. More significantly, the method of appointment for

district judges renders them (seemingly) impervious to election and retention pressures that might impel other judges to hand down stringent punishments. Judges in 94 federal judicial districts are appointed for life by the sitting President under Article III of the U.S. Constitution with the advice and consent of the Senate. Through the confirmation process, Congress determines which of the judicial nominees ultimately become federal judges. Once confirmed, district judges can only be removed from office via the impeachment process for misbehavior. These nomination and confirmation practices are meant to uphold an independent judiciary that decides cases "free from popular passion and political influence" (Administrative Office of the U.S. Courts n.d., p. 1). District composition variables measuring judge tenure, gender, race, and district caseload are included in hierarchical analytical models. Following previous research, the study also controls for effects of appointing president's political affiliation on sanction severity.

CHAPTER 3: Method

Hypotheses

This exploratory study investigates white-collar sentencing disparities by mapping broadly hypothesized effects temporal (contextual), legal, extra-legal (individual-level), and judicial (district-level) characteristics on decisions to incarcerate offenders and length of incarceration sentence. Results are compared across three time periods: (1) the *mandatory guideline era* (1990-2000); (2) the *narrow guideline era* (2001-2005); and (3) the *advisory guideline era* (post-*Booker*/2006-2015). Hypothesis 1 predicts a return to judicial discretion, following *Booker*, decreased sanction severity for white-collar offenders.

H1a: Offenders sentenced post-*Booker* will be significantly less likely to receive prison time than those sentenced in previous Guideline eras.

H1b: Offenders sentenced post-*Booker* will receive significantly shorter incarceration sentences than those sentenced in previous Guideline eras.

Hypothesis 2 suggests punishment severity is further affected by individual-level considerations of offender characteristics such as race/ethnicity, sex/gender, education, age, and having dependents:

H2a: Extra-legal offender characteristics will produce significant positive effects on the decision to incarcerate.

H2b: Extra-legal offender characteristics will produce significant positive effects on the length of incarceration sentence.

Research on white-collar criminal sentencing has not to date examined characteristics of district federal judiciary, though extant research has shown judge attributes and the appointing president's political ideology are associated with decisions to incarcerate street criminals (Johnson and Songer 2002; Johnson 2006). Hypothesis 3 uses hierarchical models to assess the influence of theoretically salient district judge attributes on sanction severity for white-collar criminals after controlling for effects of other relevant variables.

H3a: District judge attributes will produce significant positive effects on the decision to incarcerate.

H3b: District judge attributes will produce significant positive effects on the length of incarceration sentence.

Research also suggests effects of extra-legal offender and judge attributes (1) are nested within courtroom decision-making processes and, (2) are substantively constrained by the socio-temporal context. Hypothesis 4 predicts conditioning effects of individual- and district-level variables on sentencing outcomes will be more pronounced during periods of greater discretion i.e., post-*Booker,* after controlling for effects of other relevant variables.

H4a: Extra-legal and judicial attributes will produce greater positive effects on incarceration post-*Booker* than in previous Guideline eras.

H4b: Extra-legal and judicial attributes will produce greater positive effects on length of incarceration post-*Booker* than in previous Guideline eras.

Data

I use data from six U.S. federal districts (S. New York, W. Texas, N. Illinois, C. California, S. Florida, and N. Georgia) originally collected by the USSC between 1990 and 2015 and made available through the ICPSR. Districts were selected on the basis that they (1) have been used in other studies, thus providing a means for comparison, (2) are geographically diverse, and (3) are known to consistently prosecute large quantities of white-collar criminal cases (Hagan, Nagel, and Albonetti 1980; Wheeler, Weisburd, Waring, and Bode 1987). The year 1990 marks the start of widespread mandated Guideline implementation, while the 2015 Amendments represent the most recent large-scale legislative changes regarding sentencing. Following the given definition of white-collar offenses, I examine eight crimes measured in the ICPSR that are generally recognized as "white-collar" (Galvin and Simpson 2020): antitrust violations, bribery, embezzlement, forgery/counterfeiting, fraud, money laundering, racketeering/extortion, and tax offenses (**Appendix B**). Financial loss resulting from crime commission was capped at $450,000,000 to correct for skew due to extreme loss outliers with punishments more excessively severe than the sample means for each era (see Mann, Wheeler, and Sarat 1980). Additional data regarding caseloads, judicial vacancies, and characteristics of authorized judiciary active in the selected years were collected from district court websites and judge biographies available through the FJC (see **Data References**). A dichotomous variable represents *president ideology* where appointing Democratic presidents are coded (1) and Republican presidents are coded (0). Of 240 judges active in the years under study, 47% were appointed by Republican presidents. Approximately 74% of district judges in the subset were male, and the average judge tenure (capped at 2015) was 11.93 years.

Dependent variables

Primary analyses entail multivariate estimation models of (1) the decision to incarcerate white-collar criminal offenders, and (2) the length of incarceration sentence to determine associations between outcomes and relevant changes in the federal decision-making process, offender characteristics, and composite attributes of the district judiciary. I follow previous research in using binary logistic regression to model the incarceration outcome **(H1a, H2a, and H3a)** as represented dichotomously: (1) if the offender received an incarceration sentence and (0) if no incarceration (or '0' months, theoretically) (Ulmer and Johnson 2004; Johnson 2006). Ordinary least squares (OLS) regression was used to model the sentence length outcome for cases receiving an incarceration sentence **(H1b, H2b, H3b)**. I follow previous research in using the natural log of *sentence length* to correct for positive skew and reporting the anti-logged regression coefficients as percent change in discussion. OLS is appropriate because while no incarceration theoretically indicates '0' months, the data in this subset does not include a sentence of zero months in its original value range, i.e., an "in" sentence represents at least 1 month of incarceration.

The aforementioned analytic tests assume cases in the dataset are independent of one another. However, cases sentenced by the same judges in the same districts are likely to have similar characteristics possibly violating basic regression assumptions (Johnson 2006). Furthermore, the addition of theoretically contextual and district-level predictors affirm a tiered process, signaling stepwise model building may be most appropriate for accurate interpretations of data and governance trends. Hierarchical regression allows for more robust models of the multi-level nature of sentencing processes since offenders are nested within different districts with differing judge compositions. The final models provide tests of the unique contributions of

each predictor variable to explanatory power (variance) while controlling for all other variables at each analysis level (Ulmer and Johnson 2004; Johnson 2006). Thus, to adjust for error calculations, I follow previous research in modeling the conditioning effects of individual, district, and contextual variables on incarceration decision **(H4a)** using hierarchical logistic regression. Conditioning effects of individual, district, and contextual variables on incarceration length **(H4b)** were modeled using hierarchical linear regression.

Due to close relationships of legal-rational predictors, there rests the possibility for high correlations between two or more of the independent variables, which can skew regression results (Ulmer and Kramer 1996). Variance inflation values less than 10 are reported in **Appendix C** for variables under study (see Thompson, Kim, Aloe, and Becker 2017 for a thorough discussion).

Independent Variables

The analytic models include controls for theoretically salient variables in accordance with extant literature by incorporating individual-level factors related to the legal process, offender traits, and district-level composite attributes. Legal process variables consist of *offense type*, crime seriousness (measured as *final offense level*), *criminal history* (1 if any prior convictions, 0 if no prior convictions), and *case disposition* (1 if trial, 0 if plea). According to the Economic Crime Guideline (USSG §2B1.1; See Bowman 2001), the base offense level increases by 2 with each $5,000 increase in loss, thus heightening the severity of punishment and the likelihood of imprisonment. *Final offense level* represents crime seriousness on a 43-point scale; the calculation captures adjustments for loss amount resulting from the offense which is missing for most white-collar defendants in the subset before the year 2000. Offense levels can be reduced

depending on the offender's *acceptance of responsibility*, though levels are not reduced in all eligible cases (Ulmer, Eisenstein, and Johnson 2010). A dichotomous variable represents whether or not levels were actually subtracted in the calculation of the sentence outcome (1 if levels subtracted, 0 if no levels subtracted). Indications of *departures* granted (in eras where measured), number of *convictions*, and *presentence detention status* (in eras where measured) are added to the models as legal process control variables.

Extra-legal offender variables include race (1 if white, 0 if a person of color), *sex/gender* (1 if male, 0 if female), *dependents* (1 if yes, 0 if no), *education* level (ranging from "less than HS" to "college graduate"), *age* (in years) and *age squared*. The *age squared* variable was included based on evidence that younger and older offenders receive lighter sentences than their middle-aged counterparts (Hirschi and Gottsfredson 1989; Gottsfredson and Hirschi 1990). Composite judge attributes include *average tenure*, percent of judges having *prior prosecutorial experience* (e.g., *%Prior Exp*) at either the state or federal level, percent of judges appointed under a particular *president ideology* (e.g., *% Republican*), *sex/gender* composition (e.g., *% Male*), *race* composition (e.g., *% White*), and a proxy measure for mean district caseload pressure. Past studies have quantified caseload in dividing the number of cases a particular judge has sentenced, by the number of years under consideration (Johnson 2006) or incorporating the crime rate in the model (Nash 2015). The present study attempts to account for district vacancies using a proxy to model relative caseload pressure on the judicial district. The mean *caseload/vacancies* rate was calculated by subtracting judge vacancies from the number of authorized judgeships, and then dividing by number of criminal cases commenced in the district each year. Case, judgeship, and vacancy data were obtained from the U.S. Courts website, maintained by the Administrative Office of the U.S. Courts.

CHAPTER 4: Results

Descriptive Statistics

Table 1 shows descriptive statistics for all variables under analysis for each of three time periods: the (1) *mandatory* (1990-99), (2) *narrow* (2001-05), and (3) *advisory* (2006-15) white-collar policy eras. The columns report means, percentages, and standard deviations where appropriate. Prior to case exclusions, data specifications yielded over 650,000 cases for analysis across the six district courts under study. Missing data and data exclusions based on use of weapon in commission of crime reduced the sample size to 52,596 cases.

The greatest percentage of cases (37%) were disposed during the 10-year period categorized here as the *mandatory guideline era*. Of note, the shorter 6-year *narrow guideline era* saw a dramatic increase in cases tried, prosecuting over 27% more cases per year than either of the other two policy periods under study. The 10-year *advisory guideline era* reported the greatest percentage of offenders incarcerated, longest mean sentence length (M = 37.3, SD = 47.5), and highest mean *offense level* (M = 17.1, SD = 8.2) of the three time periods. With loss capped at $450,000,000, antitrust violations emerged as the "priciest" white-collar crime causing millions in monetary injury on average (M = 14,660,658.14), followed by money laundering (M = 2,102,377.18) and fraud/fraudulent statements (M = 1,377,337.02). Paradoxically, the sample included only 29 antitrust violation cases disposed during the lifetime of the Guidelines. Fraud emerged as the most common white-collar offense tried, accounting for more than 68% of the cases disposed in each era, while money laundering—the second most common crime—composed a mere 9% of the sample. Possible implications of this division of white-collar crime data are explored herein.

Over half of the offenders sentenced in each era had a criminal history, and the majority accepted responsibility for the crime tried, likely in exchange for leniency in the form of offense level reductions; most white-collar offenders—over 93% per era—accepted a plea deal in lieu of going to trial (Wheeler, Weisburd, and Bode 1982). Consistent with previous research white males in their early forties comprised the majority of offenders (Wheeler, Weisburd, and Bode 1982). As for district attributes, homogenous race and gender compositions decrease across policy periods while prior prosecutorial experience increases across policy periods. Overall district caseload rate increases throughout the lifetime of the Guidelines while average district tenure decreases as one might expect. Taken together, judicial attribute statistics for the sample signal a diversifying of general district composition resulting from new authorized judgeships added over time to deal with growing crime caseloads (FJC 2019).

TABLE 1. Descriptive Statistics for Independent and Dependent Variables: By Guideline Era

Variables	**Mandatory**	**Narrow**	**Advisory**
	N = 19,345	*N* = 17,175	*N* = 16,070
% Incarcerated	60.4	62.2	64.9
Mean Sentence Length	25.4 (38.7)	28.7 (39.7)	37.3 (47.5)
Mean Offense Level	12.7 (6.5)	14.3 (6.78)	17.1 (8.2)
Offense Type			
% Antitrust	0.2	0.2	0.1
% Bribery	3.6	2.3	2.1
% Embezzle	7.9	3.9	1.9
% Forgery/Counterfeit	10.0	9.0	5.8
% Fraud	58.7	64.7	72.8
% Money Launder	9.6	9.9	8.4

% Racket/Extort	3.6	5.0	4.6
% Tax offenses	6.4	5.0	4.3
Legal Process			
% Crim History	49.6	52.4	59.7
% Plea	93.5	94.5	93.8
% Accept Resp	82.2	89.1	90.0
% Single Convict Count	70.8	66.6	68.9
% Departure Applied	20.0	27.5	0.0[a]
% On Bail/Bond	0.0[a]	56.7	55.7
Extra-legal			
% Hispanic	7.4	25.5	35.0
% White	64.9	42.9	35.1
% Black	27.3	26.4	23.4
% Male	77.8	76.6	74.1
% HS Grad	42.7	27.1	28.2
% Some College	28.8	31.6	30.0
% Dependents	61.8	62.6	61.4
Mean Age	38.9 (11.8)	40.0 (12.2)	40.9 (12.7)
Judicial			
% Republican	48.5	46.2	38.2
% Prior Pros Exp	44.8	43.1	56.1
% White	63.4	64.6	72.6
% Male	76.1	70.8	68.9
District			
% S. NY	23.3	23.7	15.8
% W. TX	12.1	10.2	20.8
% N. IL	15.0	15.7	11.1
% C. CA	23.0	22.8	19.4

% S. FL	17.0	20.3	25.9
% N. GA	9.6	7.3	7.0

[a] Variable not measured in this time period

In/Out Decision

Tables 2 (unconditional model) and **3** (fully conditional model) report binary logistic regression results for the incarceration (in/out) outcome across the three time periods as odds ratios. Following previous research, hierarchical logistic regression was performed to allow multi-level analysis of variables of interest and account for the nested nature of sentencing processes (Ulmer and Johnson 2004). **Table 5** shows hierarchical logistic regression results for the incarceration decision, while **Table 7** reports random effects measures for select variables (Johnson 2006). **Tables 4** (fully conditional model) and **6** (hierarchical model) show Wald Chi-Squared statistics for the significance of individual regression coefficients across the three white-collar policy periods. The value table is included to show fit appropriateness of selected variables at the final step of the regression model (Bewick, Cheek, and Ball 2005).

H1a: Offenders sentenced post-Booker will be significantly less likely to receive prison time than those sentenced in previous Guideline eras.

H1a was confirmed. Controlling for legal and extra-legal variables, analyses reveal the socio-temporal context—measured as Guideline era in which a case disposed—was a strong predictor of incarceration ($SE = 0.02$, $p<.0001$). Offenders were least likely to receive incarceration during the *advisory* guideline era following the *Booker* decision ($OR = 0.78$, $SE = 0.03$, $p<.0001$); odds of receiving jail time were 52% higher for the *mandatory* era and 30% higher for the *narrow* era by comparison. The likelihood of being incarcerated was reduced

across the lifetime of the Guidelines in each of the six districts except S. FL, where the odds of incarceration instead increased by 78% between the *narrow* (*SE* = 0.14, *p*<.001) and *advisory* (*SE* = 0.0.09, *p*<.0001) policy eras.

TABLE 2. Unconditional Model of In/Out Decision: By Guideline Era

Variables	**Mandatory**			**Narrow**			**Advisory**		
	β	**OR**	**SE**	**β**	**OR**	**SE**	**β**	**OR**	**SE**
Intercept	-3.36	0.04	0.21	-3.37	0.03	0.11	-2.09	0.12	0.06
Offense Level	0.28	1.32*	0.01	0.26	1.30*	0.00	0.17	1.19*	0.00
Criminal History	-0.05	0.96***	0.02	0.87	2.39*	0.04	0.82	2.28*	0.04
Offense Type[b]	-0.07	0.94***	0.03	-0.04	0.96***	0.01	-0.02	0.97***	0.01
-*Antitrust*	-1.91	0.15	1.18	-0.22	0.80	0.43	-0.63	0.53	0.49
-*Bribery*	0.10	1.10	0.25	-0.03	0.97	0.16	1.11	3.04**	0.15
-*Embezzle*	0.33	1.39	0.22	0.17	1.19	0.13	0.79	1.17	0.14
-*Forge/Counterfeit*	1.41	4.11*	0.23	0.57	1.78*	0.11	1.47	2.32*	0.11
-*Fraud*	0.67	1.96*	0.16	0.29	1.33**	0.09	1.20	1.77*	0.08
-*Money Launder*	1.00	2.73*	0.24	0.04	1.04	0.12	1.16	1.70*	0.11
-*Racket/Extort*	-0.82	0.44***	0.42	0.11	1.12	0.16	1.38	2.11*	0.14
N		19,345			17,175			16,070	
Nagelkerke R^2		0.397			0.373			0.311	
-2 Log likelihood		3,187.428			13,194.867			15,783.927	
Model Accuracy		77.3%			77.7%			78.0%	

* *p*<.0001

** *p*<.001

*** *p*<.05

[b] Block variable

H2a: Extra-legal offender characteristics will produce significant positive effects on the decision to incarcerate.

H2a was partially confirmed as binary logistic regression results show significant conditioning extra-legal effects on decisions to incarcerate. Offender race was not a significant predictor of incarceration across time periods. Male offenders were over 75% more likely than female offenders to receive a prison sentence. An increase in age generally increased likelihood of receiving a prison sentence; however, results for the quadratic age^2 term revealed leniency directed towards elder offenders, especially during the *advisory guideline era* (*OR* = 0.83, *SE* = 0.27, $p<.0001$). Having a college degree conditioned incarceration decisions by reducing likelihood of incarceration, though this finding is not statistically significant during the *narrow guideline era* (*OR* = 0.86, *SE* = 0.10, *ns*).

TABLE 3. Binary Logistic Regression Results for In/Out Decision: By Guideline Era

Variable	**Mandatory**			**Narrow**			**Advisory**		
	β	**OR**	**SE**	**β**	**OR**	**SE**	**β**	**OR**	**SE**
Intercept	-5.14	0.00	0.83	-1.97	0.14	0.37	-0.19	0.83	0.27
Offense Level	0.49	1.64*	0.03	0.41	1.50*	0.01	0.20	1.23*	0.00
Criminal History	0.78	2.18*	0.12	0.73	2.07*	0.06	0.49	1.63*	0.05
Offense Type[b]	-0.0	0.97	0.04	-0.09	0.91*	0.02	-0.05	0.95*	0.01
-*Antitrust*	--	--	--	0.48	1.61	0.51	-0.61	0.54	0.59
-*Bribery*	0.92	2.51***	0.37	0.31	1.36	0.23	0.34	1.40***	0.16
-*Embezzle*	0.43	1.54	0.30	0.32	1.37	0.19	0.46	1.59***	0.16
-*Forge/Counterfeit*	0.77	2.17***	0.30	0.33	1.38***	0.16	0.22	1.24	0.12
-*Fraud*	0.59	1.80***	0.22	0.47	1.60**	0.13	0.27	1.31***	0.10

-*Money Launder*	0.81	2.25***	0.36	-0.25	0.78	0.18	-0.11	0.89	0.12
-*Racket/Extort*	-3.31	0.04***	1.37	0.32	1.38	0.23	.040	1.49***	0.17
Legal Process									
Accept Resp	-0.87	0.42**	0.26	-0.80	0.45*	0.17	-0.61	0.54*	0.15
Convictions	0.20	1.23	0.16	0.15	1.16***	0.07	0.20	1.22*	0.05
Disposition	-0.15	0.86	0.16	0.15	1.16	0.29	0.53	1.70***	0.20
Departure Applied	-2.12	0.12*	0.17	-2.44	0.09*	0.08	--	--	--
Presentence Status	--	--	--	-0.96	0.38*	0.05	-1.37	0.25*	0.04
Extra-legal									
Age	0.05	1.05	0.03	0.05	1.05**	0.02	0.06	1.06*	0.01
Age^2	-0.00	0.99***	0.00	-0.00	0.99**	0.00	-0.00	0.83*	0.27
College Grad	-0.69	0.50***	0.29	-0.15	0.86	0.10	-0.36	0.70*	0.07
Dependents	-0.30	0.74***	0.13	-0.05	0.95	0.06	-0.11	0.89***	0.05
White	-0.00	1.00	0.13	-0.11	0.90	0.07	-0.58	0.94	0.05
Male	0.61	1.83*	0.15	0.45	1.56*	0.07	0.52	1.67*	0.05
District [b]	0.13	1.13*	0.03	0.08	1.09*	0.02	0.21	1.24*	0.01
-*S. NY*	-0.51	0.60***	0.21	-0.84	0.43*	0.13	-1.03	0.39*	0.10
-*W. TX*	0.54	1.71***	0.24	-0.24	0.79	0.15	-0.12	0.89	0.10
-*N. IL*	-0.16	0.86	0.23	-0.16	0.85	0.13	-0.44	0.64*	0.10
-*C. CA*	-0.23	0.79	0.25	-0.61	0.54*	0.13	-0.65	0.52*	0.10
-*S. FL*	0.60	1.83***	0.23	-0.45	0.64**	0.14	0.35	1.42*	0.09
N		19,345			17,175			16,070	
Nagelkerke R^2		0.538			0.578			0.456	
-2 Log likelihood		1,678.607			6,829.588			12,873.944	
Model Accuracy		80.3%			83.9%			82.0%	

* $p<.0001$

** $p<.001$

*** $p<.05$

[b] Block variable

-- Variable not measured in this time period or variable removed from calculations due to negligible amount of cases

TABLE 4. Wald χ^2 Statistics for Select Covariates of In/Out Decision: By Guideline Era

Variable	**Mandatory**		**Narrow**		**Advisory**	
	Wald	**SE**	**Wald**	**SE**	**Wald**	**SE**
Intercept	28.798	1.396	49.461	0.517	50.630	0.560
Offense Level	214.179*	0.024	1950.824*	0.006	2218.167*	0.004
Criminal History	35.753*	0.120	230.243*	0.046	208.558*	0.045
Offense Type [b]	20.638**	--	27.360*	--	41.812*	--
Legal Process						
Accept Resp	11.471**	0.234	82.326*	0.105	17.881*	0.014
Convictions	5.866***	0.155	11.887**	0.052	53.500*	0.049
Extra-legal						
Age	8.636***	0.030	26.869*	0.011	31.374*	0.010
Age^2	13.159*	0.000	39.074*	0.000	49.850*	0.000
College Grad	5.127***	0.150	6.281**	0.058	16.300*	0.056
Dependents	6.751***	0.122	10.283**	0.047	--	--
Male	19.122*	0.136	134.822*	0.052	129.716*	0.048
District	**52.995**				**616.293***	
N	19,345		17,175		16,070	

* $p<.0001$

** $p<.001$

*** $p<.05$

[b] Block variable

-- Variable not measured in this time period or variable removed from calculations due to negligible amount of cases

H3a: District judge attributes will produce significant positive effects on the decision to incarcerate.

H3a was not confirmed; hierarchical logistic regression coefficients sloped in the opposite direction of predictions. District-level composition variables conditioned decisions to incarcerate during the *mandatory* policy era, with measures of judiciary *prior experience*, *white* composition, *male* composition, and *appointing president ideology* were associated with lower odds of incarceration for white-collar offenders. Significant results show reverse effects for the *narrow* and *advisory* eras whereby odds of incarceration increased dramatically beginning early 2000. However, large standard error calculations for composition variables suggest these results be interpreted cautiously.

H4a: Extra-legal and judicial attributes will produce greater positive effects on incarceration post-Booker than in previous Guideline eras.

H4a was partially confirmed per results of hierarchical logistic regression. In contrast to binary logistic regression results, offender *race* produced significant effects in the multi-level model, even after controlling for legal and district-level sentencing components. Specifically, white offenders were less likely than nonwhite offenders to be sentenced to prison for a white-collar offense; the strength of this association decreases and loses statistical significance between the *narrow* ($OR = 0.87$, $SE = 0.05$, $p<.05$) and *advisory* policy eras ($OR = 0.93$, $SE = 0.05$, *ns*); offender *race* produced null findings for the initial *mandatory* policy period ($OR = 1.00$, $SE = 0.13$, *ns*). Additionally, all other extra-legal characteristics except having *dependents* and

offender *gender* showed stronger associations with in/out decisions post-*Booker* when compared to previous guideline eras (see **Table 6**; full regression results summarized in **Table 14**). For judicial attributes, only associations for *district* in which a case disposed and *appointing president ideology* strengthened across policy eras, with the most pronounced effects manifesting during the *narrow guideline era*.

TABLE 5. Hierarchical Logistic Regression Results for In/Out Decision: By Guideline Era

Variable	**Mandatory**			**Narrow**			**Advisory**		
	β	**OR**	**SE**	**β**	**OR**	**SE**	**β**	**OR**	**SE**
Intercept	-4.721	0.00	0.72	-2.40	0.09	0.51	-4.49	0.01	0.37
Offense Level	0.39	1.43	0.02	0.27	1.31*	0.01	0.21	1.24*	0.00
Criminal History	0.66	1.92*	0.12	0.73	2.07*	0.05	0.68	1.98*	0.05
Offense Type[b]	-0.02	0.98	0.04	-0.04	0.97***	0.01	-0.03	0.97***	0.01
Accept Resp	-0.80	0.45**	0.24	-0.85	0.43*	0.13	-0.59	0.55*	0.14
Convictions	0.31	1.37***	0.15	0.17	1.19**	0.05	0.34	1.41*	0.05
Disposition	-0.11	0.90	0.15	0.33	1.38	0.20	0.49	1.63***	0.19
Extra-legal									
Age	0.07	1.07***	0.03	0.04	1.04*	0.01	0.05	1.05*	0.01
Age^2	-0.00	0.99**	0.00	-0.00	0.99*	0.00	-0.00	1.00*	0.00
Dependents	-0.02	0.98	0.01	-0.05	0.95*	0.02	-0.03	0.97***	0.01
Race	0.00	1.00	0.13	0.14	1.15***	0.05	-0.10	0.90***	0.05
White	0.02	1.02	0.12	-0.14	0.87***	0.05	-0.08	0.93	0.05
Gender	-0.61	0.55*	0.13	-0.62	0.54*	0.05	-0.56	0.57*	0.05
Male	0.61	1.84*	0.13	0.60	1.83*	0.05	0.56	1.75*	0.05
Education[b]	-0.14	0.87*	0.04	-0.07	0.93*	0.01	-0.10	0.90*	0.01
College Grad	-0.38	0.68***	0.15	-0.17	0.84***	0.06	-0.23	0.80*	0.06

District[b]	-0.29	0.75***	0.60	1.13	3.09	1.00	1.69	5.43***	0.55
% Republican	-3.35	0.04*	0.68	18.53	***[c]	6.04	4.73	***[c]	2.24
% Prior Pros Exp	-5.27	0.01*	1.11	-13.87	***[c]	4.86	-7.66	--[c]	5.81
% Male	-4.10	0.02**	1.24	-17.04	0.00***	9.23	-11.83	***[c]	12.47
% White	-1.93	0.15***	0.95	8.89	***[c]	3.67	2.35	10.52	2.91
Mean Tenure	-0.10	0.91	0.06	0.02	1.02	0.01	-0.43	0.65**	0.13
Mean Caseload	0.02	1.02*	0.00	0.01	1.01	0.01	-0.00	0.99	0.01
% Rep X District	-0.37	0.67	0.35	4.70	110.25*	1.19	-0.62	0.54	0.32
% Prior Exp X District	-0.86	0.42***	0.28	-0.02	0.99	1.19	-1.72	0.18	0.85
% White X District	-1.10	0.33***	0.35	-0.00	0.99	0.18	-1.49	0.23***	0.75
N		19,345			17,175			16,070	
Nagelkerke R^2		0.445			0.429			0.432	
-2 Log likelihood		1,890.403			12,521.626			13,546.744	
Model Accuracy		76.7%			78.3%			80.0%	

* $p<.0001$

** $p<.001$

*** $p<.05$

[b] Block variable

[c] OR greater than 3 decimal places

-- Variable not measured in this time period or variable removed from calculations due to negligible amount of cases

TABLE 6. Wald χ^2 Statistics for Hierarchical Logistic Regression: By Guideline Era

Variable	**Mandatory**		**Narrow**		**Advisory**	
	Wald	**SE**	**Wald**	**SE**	**Wald**	**SE**
Intercept	3.468	3.250	4.398	37.156	6.574	13.524
Offense Level	222.844*	0.025	2003.633*	0.006	2297.067*	0.004

Criminal History	29.985*	0.118	233.317*	0.046	218.844*	0.045
Offense Type [b]	0.341	0.037	9.261***	0.013	10.830**	0.012
Legal Process						
Accept Resp	11.286**	0.239	42.827*	0.128	17.948*	0.140
Convictions	3.677	0.152	10.717*	0.053	49.020*	0.050
Disposition	0.464	0.153	2.912	0.201	6.682***	.0189
Extra-legal						
Age	5.687***	0.029	26.962*	0.011	35.033*	0.010
Age^2	10.520**	0.000	40.049*	0.000	53.723*	0.000
Dependents	2.391	0.015	11.995**	0.015	5.757***	0.014
Race	0.000	0.125	8.312***	0.048	4.585***	0.047
Gender	20.582*	0.134	142.473*	0.052	135.325*	0.048
Education	12.435*	0.041	31.922*	0.013	63.010*	0.013
District[b]	**5.789*** **	**0.120**	**1.273**	**1.001**	**9.363*** **	**0.553**
% Republican	--	--	--	--	4.455***	2.241
% Male	--	--	4.676***	61.854	4.103***	15.053
% White	0.779	4.182	6.043***	4.641	0.653	2.913
Mean Caseload	12.559*	0.004	2.177	0.011	0.173	0.008
N	19,345		17,175		16,070	

* $p<.0001$

** $p<.001$

*** $p<.05$

[b] Block variable

-- Variable not measured in this time period or variable removed from calculations due to negligible amount of cases

TABLE 7. Hierarchical Logistic Regression: Variance for In/Out Decision By Guideline Era

Variable	Mandatory	Narrow	Advisory
Guideline Era			
Offense Level	0.016	0.011	
Criminal History	0.022	0.011	0.011
Legal Process			
Offense Type[b]	0.016	0.011	0.007
Accept Resp	0.082	0.037	0.026
Convictions	0.014	0.011	0.009
Extra-legal			
Age	--	0.000	0.000
	0.001		--
Dependents	0.363	0.405	0.475
Race	0.222	0.362	0.446
Gender	0.016	0.016	0.010
Education[b]			
District[b]			
% Republican			
% Prior Pros Exp			
% Male			
% White			
Mean Tenure			
Mean Caseload			
Between-district proportion of variance	**.376**	**.402**	**.473**
Between-district proportion of variance (pooled)		**0.414**	

Between-era proportion of variance	**0.389**

Sentence Length Decision

Tables 8 (unconditional model) and **9** (fully conditional model) report OLS regression results for the sentence length severity outcome; anti-logged results are discussed in text. Hierarchical linear regression was performed to allow multi-level analysis of variables of interest and account for the nested nature of sentencing processes (Ulmer and Johnson 2004). **Table 11** shows hierarchical linear regression results for sentence length decisions; **Table 13** reports random effects measures for select variables (Johnson 2006). **Tables 10** (fully conditional model) and **12** (hierarchical model) show *t*-test results for the equality of regression coefficients across three time periods. The *t*-test value table is included to show differences between group means on the logged sentence length variable.

H1b: Offenders sentenced post-Booker will receive significantly shorter incarceration sentences than those sentenced in the previous years.

H1b was confirmed; the era in which an offender was sentenced emerged as a strong predictor of incarceration sentence length ($SE = 0.01$, $p<.0001$). Two-tailed *t*-tests reveal the socio-temporal effect was most pronounced during the 6-year *narrow* guideline era ($t = 5.224$, $SE = 0.03$, $p<.0001$), in which the brunt of legislation passed to curtail white-collar offending were introduced. While no significant difference in sentence length emerged between the *mandatory* and *narrow* policy eras, offenders sentenced during the *advisory* period received sentences reduced by approximately 30% of those meted out pre-*Booker*. Additionally, all districts in the study experienced a tightening of restrictions on sentence length decisions during

the *narrow* guideline era; this finding was significant for all districts except W. TX when compared to N. GA. Lastly, analyses reveal that the C. CA district meted out the least severe punishments when compared to N.GA; being sentenced in this district was associated with sentence lengths reduced by approximately 68% (*SE* = 0.14, *p*<.0001), 25% (*SE* = 0.05, *p*<.0001), and 30% (*SE* = 0.05, *p*<.0001), in the *mandatory, narrow, and advisory* policy eras, respectively.

TABLE 8. Unconditional Model of Sentence Length: By Guideline Era

Variable	**Mandatory**		**Narrow**		**Advisory**	
	***b*[d]**	**SE**	***b*[d]**	**SE**	***b*[d]**	**SE**
Intercept	0.16	0.07	0.25	0.06	0.30	0.04
Offense Level	0.15*	0.00	0.14*	0.00	0.12*	0.00
Criminal History	0.11*	0.02	0.43*	0.02	0.49*	0.02
Offense Type[b]	-0.02	0.01	0.00	0.00	0.01	0.01
-*Antitrust*	-0.66	1.18	-0.16	0.30	-0.18	0.42
-*Bribery*	-0.19	0.17	--	--	-0.03	0.10
-*Embezzle*	-0.87*	0.16	-0.50*	0.08	-0.49*	0.11
-*Forge/Counterfeit*	0.26	0.15	0.21**	0.07	0.26**	0.08
-*Fraud*	0.00	0.11	-0.04	0.05	-0.00	0.06
-*Money Launder*	-0.04	0.13	-0.02	0.06	0.00	0.07
-*Racket/Extort*	-0.52***	0.17	-0.04	0.07	0.06	0.08
N	19,345		17,175		16,070	
F-value	476.889		895.158		781.874	
Adjusted R^2	0.404		0.466		0.356	

* *p*<.0001

** *p*<.001

*** $p<.05$

[b] Block variable

[d] Unstandardized coefficients reported

-- Variable not measured in this time period or variable removed from calculations due to negligible amount of cases

H2b: Extra-legal offender characteristics will produce significant positive effects on the length of incarceration sentence.

H2b was partially confirmed as OLS regression results show significant conditioning effects on sentence length for all extra-legal variables except offender *race* across white-collar policy eras. As with the incarceration decision, *age* proved a robust predictor of a lengthier prison sentence; severity was most lenient for the eldest offenders in the dataset. The negative effect of having *dependents* loses significance post-*Booker* when other pertinent legal and extra-legal sentencing factors are considered. Offender *education* loaded in later eras; by the *advisory* era, having a college degree was associated with 16% shorter sentences than having less than a high school degree as compared to 10% shorter sentences during the *narrow* era. Male offenders received significantly longer sentences than female offenders overall, and this finding was most pronounced during the *mandatory* period where judges meted out sentenced approximately 47% longer for men than for women.

TABLE 9. OLS Regression Results for Sentence Length: By Guideline Era

Variable	**Mandatory**		**Narrow**		**Advisory**	
	b [d]	**SE**	***b*** [d]	**SE**	***b*** [d]	**SE**
Intercept	-0.36	0.47	0.14	0.14	0.92	0.16
Offense Level	0.15*	0.01	0.14*	0.00	0.11*	0.00

Criminal History	0.59*	0.07	0.30*	0.02	0.31*	0.03
Offense Type[b]	0.00	0.02	-0.00	0.00	-0.00	0.01
-*Antitrust*	--	--	-0.07	0.27	-0.18	0.40
-*Bribery*	-0.03	0.22	0.04	0.10	-0.07	0.10
-*Embezzle*	-0.76*	0.20	-0.38*	0.08	-0.33***	0.11
-*Forge/Counterfeit*	0.02	0.18	0.18	0.07	-0.00	0.08
-*Fraud*	-0.05	0.14	-0.00	0.06	-0.11	0.06
-*Money Launder*	-0.08	0.18	-0.06	0.07	-0.22***	0.07
-*Racket/Extort*	-0.50	0.31	-0.06	0.07	-0.09	0.08
Legal Process						
Accept Resp	-0.32**	0.10	-0.18*	0.04	-0.16***	0.06
Convictions	0.17***	0.08	0.09*	0.02	0.25*	0.03
Disposition	-0.02	0.05	-0.07	0.06	-0.02	0.07
Departure Applied	-0.68*	0.10	-0.68*	0.03	--	--
Presentence Status	--	--	-0.22*	0.02	-0.52*	0.02
Extra-legal						
Age	0.04***	0.02	0.03*	0.01	0.03*	0.00
Age^2	0.00***	0.00	0.00*	0.00	0.00*	0.00
Dependents	-0.25**	0.07	-0.07***	0.02	-0.04	0.02
White	-0.07	0.08	0.03	0.02	-0.03	0.03
Male	0.39*	0.09	0.27*	0.03	0.19*	0.03
College Grad	-0.13	0.15	-0.10***	0.04	-0.18*	0.04
District[b]	0.02	0.02	0.02*	0.01	0.11*	0.01
-*S. NY*	-0.37***	0.13	-0.29*	0.04	-0.65*	0.05
-*W. TX*	-0.06	0.14	-0.02	0.05	0.04	0.05
-*N. IL*	-0.38***	0.13	-0.15**	0.05	-0.44*	0.05
-*C. CA*	-1.13*	0.14	-0.29*	0.05	-0.36*	0.05
-*S. FL*	-0.25***	0.13	-0.21*	0.04	0.02	0.04

N	19,345	17,175	16,070
F-value	53.163	292.854	332.846
Adjusted R^2	0.524	0.550	0.413

* $p<.0001$

** $p<.001$

*** $p<.05$

[b] Block variable

[d] Unstandardized coefficients reported

-- Variable not measured in this time period or variable removed from calculations due to negligible amount of cases

TABLE 10. Two-tailed *t*-tests for Select Covariates of Sentence Length: By Guideline Era

Variable	**Mandatory**		**Narrow**		**Advisory**	
	t	**SE**	***t***	**SE**	***t***	**SE**
Intercept	-2.355	0.431	-2.402	0.129	-1.482	0.147
Offense Level	17.864*	0.008	68.328*	0.002	67.340*	0.002
Criminal History	9.175*	0.076	16.459*	0.022	14.147*	0.026
Offense Type[b]	0.503	0.021	2.186***	0.005	2.721***	0.006
Legal Process						
Accept Resp	-4.270*	0.103	-6.288*	0.041	-3.781*	0.060
Convictions	0.814	0.085	3.092**	0.023	10.359*	0.026
Disposition	0.152	0.058	-1.327	0.055	-1.090	0.072
Extra-legal						
Age	2.061***	0.020	4.793*	0.006	3.119**	0.006
Age^2	-2.110***	0.000	-4.857*	0.000	-4.000*	0.000
Dependents	-2.945**	0.076	-4.296*	0.022	-2.084***	0.025
Male	5.177*	0.092	11.107*	0.028	8.030*	0.030

College Grad	-0.641	0.095	-3.181**	0.027	-7.312*	0.031
District[b]	**0.937**	**0.021**	**5.855**	**0.006**	**14.749**	**0.007**
N	19,345		17,175		16,070	

* $p<.0001$

** $p<.001$

*** $p<.05$

[b] Block variable

H3b: District judge attributes will produce significant positive effects on the length of incarceration sentence.

H3b was not confirmed. Hierarchical linear regression revealed district judge composition variables affected sentence length decisions across guideline policy eras in the opposite direction of predictions. *Male composition* corresponded with an overall decrease in sentence length across guideline eras; measures of judiciary *prior experience* and *white composition* significantly affected sentence length decisions, but these associations vary widely by guideline era. Larger percentages of Republican-appointed judges were associated with overall lengthier incarceration sentences for white-collar offenders.

H4b: Extra-legal and judicial attributes will produce greater positive effects on length of incarceration post-Booker than in previous Guideline eras.

H4b was only partially confirmed per hierarchical linear regression. Increases in offender *age* and being male corresponded with lengthier incarceration terms across time periods, while measures of offender *education* and *dependents* were associated with shorter incarceration terms. Associations between incarceration length and offender *education* strengthened across time periods (see **Table 12**; full results summarized in **Table 14**).

Additionally, the significance of having *dependents* decreased across guideline eras; having dependents corresponded with a 2.5% decrease in mean sentence length during the *mandatory* era and 6.8% by the *advisory* era when compared to offenders with no dependents. District composition variable associations were most pronounced during the *mandatory guideline era* prior to the *Booker* decision; findings for judge attribute variables should be interpreted with caution as this era was also missing the greatest amount of federal and judicial data. All judiciary composition variables under study produced significant effects on sentence length post-*Booker*, with districts having greater compositions of *prior prosecutorial experience*, *male*, and *white* judges corresponding to leniency during this policy period.

TABLE 11. Hierarchical Linear Regression Results for Sentence Length: By Guideline Era

Variable	**Mandatory**		**Narrow**		**Advisory**	
	b [d]	**SE**	***b*** [d]	**SE**	***b*** [d]	**SE**
Intercept	-4.48	0.85	0.06	0.17	3.23	0.40
Offense Level	0.15*	0.01	0.13*	0.00	0.12*	0.00
Criminal History	0.61*	0.07	0.35*	0.02	0.35*	0.03
Offense Type[b]	0.00	0.02	0.01	0.01	0.01	0.00
Accept Resp	-0.37*	0.10	-0.23*	0.04	-0.14***	0.06
Convictions	0.20***	0.08	0.11*	0.02	0.29*	0.03
Disposition	-0.01	0.06	-0.06	0.06	0.00	0.07
Extra-legal						
Age	0.05***	0.02	0.03*	0.01	0.03*	0.01
Age^2	-0.00***	0.00	0.00*	0.00	0.00*	0.00
Dependents	-0.28*	0.07	-0.12*	0.02	-0.08**	0.03
Race	0.04	0.08	-0.01	0.02	-0.04	0.03
White	-0.05	0.08	0.01	0.02	-0.03	0.03

Gender	-0.49*	0.09	-0.33*	0.03	-0.27*	0.03
Male	0.48*	0.09	0.33*	0.03	0.28*	0.03
Education[b]	-0.05***	0.02	-0.03*	0.01	-0.07*	0.01
College Grad	-0.05	0.09	-0.09**	0.03	-0.20*	0.03
District[b]	0.06**	0.02	-0.07***	0.02	0.12***	0.06
% Republican	--	--	2.38*	0.25	1.77*	0.17
% Prior Pros Exp	-2.22*	0.61	1.20*	0.27	-0.53***	0.21
% Male	6.31*	0.90	1.89***	0.82	-5.23*	0.43
% White	2.34*	0.59	-0.67*	0.19	-1.87***	0.85
Mean Tenure	-0.04	0.60	-0.08*	0.01	0.03***	0.01
Mean Caseload	-0.00*	0.00	--	--	0.00*	0.07
N	19,345		17,175		16,070	
F-value	69.092		440.738		390.606	
Adjusted R^2	0.495		0.465		0.383	

* $p<.0001$

** $p<.001$

*** $p<.05$

[b] Block variable

[d] Unstandardized coefficients reported-- Variable not measured in this time period or variable removed from calculations due to negligible amount of cases

TABLE 12. Two-tailed *t*-tests for Hierarchical Linear Regression: By Guideline Era

Variable	**Mandatory**		**Narrow**		**Advisory**	
	t	**SE**	***t***	**SE**	***t***	**SE**
Intercept	-5.270	0.850	0.364	0.165	8.181	0.395
Offense Level	17.960*	0.008	68.338*	0.002	70.419*	0.002
Criminal History	8.300*	0.073	15.789*	0.022	13.720*	0.025
Offense Type[b]	0.217	0.021	1.558	0.005	0.692	0.006

Legal Process						
Accept Resp	-3.640*	0.101	-5.434*	0.041	-2.407***	0.060
Convictions	2.404***	0.084	4.563*	0.023	11.129*	0.026
Disposition	-.0253	0.056	-0.834	0.055	0.011	0.071
Extra-legal						
Age	2.639***	0.019	5.690*	0.006	4.669*	0.006
Age^2	-2.566***	0.000	-5.572*	0.000	-5.151*	0.000
Dependents	-3.862*	0.074	-5.388*	0.022	-3.196**	0.025
Race	0.528	0.078	-0.297	0.023	-1.581	0.027
Gender	-5.564*	0.089	-11.815*	0.028	-9.264*	0.029
Education	-2.121***	0.024	-5.456*	0.006	-9.455*	0.007
District[b]	**--**	**--**	**-2.882*** **	**0.023**	**--**	**--**
% Republican	--		9.480*	0.251	10.485*	0.169
% Prior Pros Exp	-2.913***	0.529	4.393*	0.273	-2.511***	0.211
% Male	7.018*	0.900	--	--	-12.047*	0.434
Mean Tenure	--	--	-6.780*	0.012	--	--
Mean Caseload	-4.543*	0.000	--	--	12.053*	0.000
N	19,345		17,175		16,070	

* $p<.0001$

** $p<.001$

*** $p<.05$

[b] Block variable

TABLE 13. Hierarchical Linear Regression: Variance for Sentence Length By Guideline Era

Variable	**Mandatory**	**Narrow**	**Advisory**
Offense Level	42.763	45.10	67.31
Criminal History	3.90	0.25	0.24

Legal Process			
Offense Type[b]	**3.78**	0.011	0.007
Accept Resp	0.15	0.037	0.026
Convictions	0.21	0.011	0.009
Disposition	0.31		
Extra-legal			
Age	**139.33**	147.95	161.54
Dependents	0.24	0.405	0.09
Race	0.23	0.362	.021
Gender	0.19	0.016	0.06
Education[b]	286.40	**95.73**	**93.88**
District[b]	**2.76**	**2.67**	**2.54**
% Republican	0.01	0.01	**0.02**
% Prior Pros Exp	0.01	**0.02**	**0.03**
% Male	0.01	**0.00**	0.01
% White	0.01	**0.02**	0.01
Mean Tenure	1.35	**3.35**	**4.58**
Mean Caseload	--	**--**	**--**
Between-district proportion of variance	**.066**	**.053**	**.289**
Between-district proportion of variance (pooled)			
Between-era proportion of variance			

Legal-Rational, Temporal, and District Sentencing Variations

It appears the weight of key Guideline parameters, crime seriousness (measured as final *offense level*) and *criminal history*, decreases throughout the 26-year time period under study to their lowest values during the *advisory guideline era*, possibly signaling the gradual incorporation of substantive considerations into the decision-making process over time. For example, while a one-unit increase in white-collar *offense level* resulted in 32% greater odds of incarceration over the mean during the *mandatory* era, the same produced 19% greater odds of incarceration during the *advisory* era. Legally relevant variables including *acceptance of responsibility*, application of *departures* (where measured), and *presentence* detention status (where measured) were consistent predictors of incarceration across time periods. Specifically, final *offense level* and *departures* were the strongest and most consistent predictors of incarceration across districts and time periods, highlighting uniformity in the application of these considerations to sentencing decisions. Similarly, greater *offense levels*, having more *convictions*, and having a *criminal history* consistently predicted increases in incarceration length terms across white-collar policy periods; accepting responsibility for one's crime resulted in significantly shorter sentence terms for white-collar offenders. In contrast to previous studies, *case disposition* (plea vs. trial) did not significantly affect sentence length when other legally relevant variables are considered. This null finding might be attributed to sample rates of plea acceptance over 92% for each guideline era.

Additional hierarchical regression analyses were conducted to explore the impact of distinguishing between "high-loss" (i.e., antitrust violations and money laundering; $N = 4{,}939$) and "low-loss" (i.e., bribery, embezzlement, forgery/counterfeiting, fraud, racketeering/extortion, and tax offenses; $N = 47{,}619$) white-collar crimes. Results for the pooled sample reveal the delineation between committing a high- versus low-loss crime was associated with decisions to

incarcerate, but not incarceration length sentences; white-collar defendants on trial for high-loss crimes experience lower odds of incarceration when compared to defendants punished for low-loss crimes (*OR* = 0.76, *SE* = 0.06, $p<.0001$, Wald $\chi^2 = 19.883$). Results were negligible for the *mandatory era* (*OR* = 1.687, *SE* = 0.29, $p=0.66$, *ns*). However, during the *narrow* (*OR* = 0.66, *SE* = 0.10, $p<.0001$, Wald $\chi^2 = 16.755$) and *advisory* (*OR* = 0.76, *SE* = 0.08, $p<.0001$, Wald $\chi^2 = 12.536$) guideline eras, perpetrators of "high-loss" white-collar crimes experienced up to 24% lower odds of incarceration by comparison when all other legal, extralegal and contextual factors are considered. While this statistical insight could be a consequence of the relatively low number of high-loss crimes analyzed in the sample, it is interesting that this group experiences lower odds at all given, according to the Sentencing Table (**Appendix A**) and the Economic Crime Guideline (USSG §2B1.1; See Bowman 2001), exponentially greater financial loss should produce adjustments augmenting offense levels thus heightening the severity of punishment and the likelihood of imprisonment.[12]

Logistic regression revealed the district in which a case disposed produced significant positive effects on in/out decisions in the pooled sample (*OR* = 1.14, *SE* = 0.03, $p<.0001$, Wald $\chi^2 = 13.776$), but only during the *mandatory* guideline era (*OR* = 5.87, *SE* = 0.60, $p<.01$, Wald $\chi^2 = 8.820$) when other legal, extra-legal, and district factors were considered. Similarly, district variations in sentence length were observed during *mandatory* (*b* = 0.06, *SE* = 0.02, $p<.05$, *t* = 2.886) and *advisory* (*b* = 0.12, *SE* = 0.06, $p<.05$, *t* = 2.137) policy eras. S. NY and C.CA prosecuted the greatest volumes of white-collar crime yet boasted lower caseload rates by comparison due to these districts having high numbers of authorized judgeships. Odds of incarceration were lowest for S. FL where offenders in the sample experienced up to 60%

[12] As noted, levels are not reduced in all eligible cases (Ulmer, Eisenstein, and Johnson 2010).

decrease in odds of imprisonment when compared to N. GA. District interaction terms denote significant results across policy periods whereby districts with greater percentages of white judges mete out less severe punishments; this was only shown for the W. TX district (*SE* = 30.06, $p<.05$, Wald $\chi^2 = 4.706$) during the narrow era in comparison to N. GA. Likewise, S. FL and C.CA reported sentence lengths shortened by over 30% when compared to the N. GA district. A positive relationship presented between district caseload rate and likelihood of incarceration when compared to N.GA, and this relationship varied by policy period.

Random effects measures for hierarchical regression indicate the extent to which individual-level, district-level, and contextual sentencing components vary across white-collar policy eras for both the incarceration (**Table 7**) and sentence length (**Table 13**) outcomes. After controlling for individual sentencing factors, significant variation in the likelihood of receiving a prison sentence and the mean sentence length was observed across guideline eras. Study hypotheses and results are summarized below in Table 14.

TABLE 14. Summary of Regression Results

Description of Hypothesis (p. 26-27)	**Analytic Test/*Variables***	**Confirmed?**	
		In/Out	*Length*
Hypothesis 1 predicts the socio-temporal context conditions sentencing outcomes, decreasing sanction severity for white-collar offenders following a return to judicial discretion.	**H1a:** Binary Logistic Regression **H1b:** OLS Regression	**H1a:** Yes*	**H1b:** Yes*

Hypothesis 2 predicts individual-level extra-legal characteristics will produce significant positive effects on sanction severity for white-collar offenders.	**H2a:** Binary Logistic Regression/ Hierarchical Logistic Regression **H2b:** OLS Regression/ Hierarchical Linear Regression	**H2a:** Partial	**H2b:** Partial
	Age	Yes*	Yes***
	Age^2	Yes*	Yes***
	Dependents	Partial***	Yes**
	Race	Partial***	No, *ns*
	Gender	Yes*	Yes*
	Education[b]	Yes*	Yes***
Hypothesis 3 predicts salient district-level judge attributes will produce significant positive effects on sanction severity for white-collar offenders after controlling for other relevant variables.	**H3a:** Hierarchical Logistic Regression **H3b:** Hierarchical Linear Regression	**H3a:** No, negative effects observed	**H3b:** No, negative effects observed
	District[b]	Partial***	Partial**
	% Republican	No, *ns*	Yes*
	% Prior Pros Exp	Partial***	Partial***
	% Male	No***	Yes***
	% White	Partial***	Partial***
	Mean Tenure	Partial**	Partial***
	Mean Caseload	Yes*	Yes*
Hypothesis 4 predicts conditioning effects of individual- and district-level variables on sentencing outcomes will be more pronounced	**H4a:** Hierarchical Logistic Regression/ Random Effects	**H4a:** Partial	**H4b:** Partial

during periods of greater discretion/post-*Booker* after controlling for other relevant variables.	**H4b:** Hierarchical Linear Regression/ Random Effects		
	Age	Yes*	No
	Age^2	Yes*	No
	Dependents	No	No
	Race	No	No
	Gender	No	No
	Education[b]	Yes*	Yes*
	District[b]	Yes***	No
	% Republican	Yes***	Yes*
	% Prior Pros Exp	No	No
	% Male	No	Yes*
	% White	No	No
	Mean Tenure	No	No
	Mean Caseload	No	Yes*

See Tables 2-13 for detailed "partial" findings delineated by guideline era

* $p<.0001$

** $p<.001$

*** $p<.05$

ns = not significant

[b] Block variable

CHAPTER 5: Discussion

Since the turn of the century, sentencing research has consistently shown that aspects of the social context generally condition sentencing variations, potentially creating sentencing disparities (Thomson and Zingraff 1981; Ulmer 2012; Simpson 2013). I explore individual, district, *and* contextual effects on the likelihood and length of white-collar incarceration sentences by accounting for relevant legal acts introduced against the backdrop of the Federal Sentencing Guidelines, as well as extra-legal offender and judicial attributes. This study hypothesized an emergent socio-temporal trend, largely driven by implementation of white-collar sentencing legislation and a return to judicial discretion, whereby white-collar offenders sentenced in the years post-*Booker* would receive more lenient punishments than those sentenced before the *Booker* decision. The study also hypothesized effects of extra-legal and judicial attributes on sentencing outcomes would be most pronounced post-*Booker* when Guideline statutes were deemed advisory. Confirmation of study hypotheses merits consideration of study implications for methodological approach to white-collar sentencing inquiries, theoretical underpinnings, and the study of extra-legal and judicial attribute variables in white-collar research.

Methodological Approach

First, cross-sectional sentencing studies must account for the socio-temporal context resulting from legislative changes when analyzing white-collar crimes, especially in the wake of newly announced amendments to the Sentencing Reform Act that, for one, call for reductions in mandatory sentences for nonviolent crimes (USSC 2019). Exclusion could result in a significant omitted-variable bias and skew findings linking certain offender characteristics to sentencing

trends. Crime datasets in which sentence lengths are directly increased via legislative action are especially prone to errors resulting from omission of the temporal variable (Thomas and Zingraff 1981; USSC 2006). Additionally, the pre- / post-*Booker* analysis scheme alone might prove inadequate for models of white-collar criminal sentencing since targeted policies further altered restrictions on discretion—and consequently sanction severity—for this offending group (Breyer 1988; USSC 2010; Burns and Meitle 2020).

Second, while hierarchical modeling allows the researcher to account for nested effects of individual-level, district-level, and contextual variants, the addition of district-level variables measuring judicial attributes did not significantly improve the proportion of total variance explained by the hierarchical models ($\mathbf{R}^2$) over fully conditional binary logistic and OLS regression models. Nevertheless, controlling for effects of judicial attribute measures offers insight into the temporal/contextual changes affecting overall judicial composition, sentencing patterns, and practices across policy eras (Hendershot and Tecklenburg 2011). General district judiciary composition changed as a consequence of newly authorized judgeships and an increasingly diverse pool of judicial candidates throughout the lifetime of the Guidelines. It is possible such transitions have implications for sentencing outcomes, given judge focal concerns and constraints on discretion change along with the judicial body; however, high hierarchical logistic regression standard error calculations for some judge attribute variables suggest these particular results be interpreted with caution.

Lastly, exploratory analyses revealed "high-loss" white-collar crimes, herein defined as antitrust violations and money laundering, made up less than 10% of the white-collar criminal cases disposed during the lifetime of the Guidelines yet produced tens of millions of dollars in financial loss on average. These crimes were associated with significantly lower odds of

incarceration when compared to their "low-loss" criminal counterparts, suggesting white-collar crimes are delineated by loss in the federal practice of rendering incarceration sentences; the cost delineation was not significant in sentence length decisions. These results align with USSC (2016) reports that fines are commonly imposed as punishment for high-ticket crimes such as antitrust violations. That various temporal (contextual), legal, extra-legal (individual-level), and judicial (district-level) characteristics functioned to produce this result suggests certain powerful and high-status offenders experience advantages in the legal process (Mann, Wheeler, and Sarat 1980; Simpson 2013), since generally higher or more significant losses should result in more severe sentences (Burns and Meitle 2020) up to and including life in prison. White-collar research might consider further exploring definitional divisions within the field and how differences in crime operationalization might affect study interpretations.

Theoretical Underpinnings of White-Collar Sentencing Decisions

In line with *substantive political theory*, changes in policies affecting white-collar criminals appear to produce variations in their effects on sentencing outcomes (Thomson and Zingraff 1981; Crow and Bales 2006; Ulmer et. al 2011). Namely, offenders sentenced post-*Booker* had lower odds of incarceration and received shorter prison sentences when compared to previous eras; this was found despite the *advisory guideline era* reporting the greatest percentage of offenders incarcerated, longest mean sentence length, and highest mean offense level of the three time periods. Similar findings from Tiede, Carp, and Manning (2010) suggest *Booker* provisioned this trend as a result of affording federal judiciary greater discretion over white-collar sentencing outcomes. The overall weight of key Guideline parameters—*crime seriousness* and *criminal history*—in determining sentence severity for white-collar offenders decreases

across policy eras to their lowest values post-*Booker*. During this time, effects of crime seriousness/offense level produced a 20% reduction in likelihood of incarceration, and a 4% decrease of sentence length over time. Effects of criminal history resulted in a 4% *increase* in likelihood of incarceration, but a 40% *decrease* in sentence length over time.

Taken together, this research demonstrates *focal concerns* in courtroom decision-making involve complexly tiered legal-rational, socio-temporal, and discretionary components, evidence of attempts to balance formally rational interests with contextually situated concerns of federal judiciary and the wider public (Ribstein 2009). *Blameworthiness* as measured by criminal history and offense seriousness drives determinations of sanction severity for the sample. Further, legally-oriented considerations built into the Guidelines such as acceptance of responsibility, application of an upward or downward departure, and number of conviction counts consistently predicted enhancements and reductions of sentence severity for white-collar offenders, acting as "windows of discretion" for judges to lawfully adjust sentences on a case by case basis (Steffensmeier and Demuth 2000, p. 726; Galvin and Simpson 2020). Legislation introduced to enhance white-collar criminal penalties defined contextually acceptable punishments and sentence ranges, as well as the types of acts indicative of criminal intent (Burns and Meitle 2020).

As members of the federal judiciary, these public servants are serviced to protect the accused's victims and dependents, the falsely accused, and society at large from harm via punishment and deterrence. Judges in the sample appeared to *protect their respective communities* by combining legal-rational calculations with considerations of substantively rational ideals. That is, judges supplemented perceptions of future dangerousness and scope of crime problem with status characteristics that might correlate with increased opportunity to

offend and/or abuse trust relationships through force or fraud. Districts with greater compositions of prior prosecutorial experience corresponded to more lenient sentences for this offending group. Ulmer and Johnson (2004)—using a mixed felony sample—found support that perceived dangerousness of minority offenders provisioned harsher punishments in counties with large minority populations. Consistent with existing research, study results for white-collar crimes implied that the perceived future threat of this offending group might be *lowest* in districts with historically homogenous gender, racial, and ideological judicial compositions, occasioning more lenient sentences for a relatively homogenous sample of white male defendants. Mann, Wheeler, and Sarat (1980, p. 500) aver "evidence of the judges' understanding, indeed sympathy, for the person whose position in society may be very much like their own" is likely rooted in concerns that the crime may be separated from the offender's total personality, seriousness of trust violation, rehabilitation, special sensitivity to prison, and the desire to prevent further injury through non-incarcerative reparations.

Lastly, sentencing policies, public attitudes, organizational concerns of the federal judiciary, and events sparking law implementation operate as *practical constraints* on judges' sentencing decisions. While the specific *focal concerns* to which judges adhere appear to be contextually situated and vary with a changing judiciary composition, these constraints are thought to condition effects of legal, extra-legal, and district attributes on sentencing outcomes (see Ulmer et. al 2011). For example, Mann, Wheeler, and Sarat (1980) deduced from their interviews that "most judges share a widespread belief that the suffering experienced by the white-collar person as a result of apprehension, public indictment and conviction, and the collateral disabilities incident to conviction…completely satisfies the need to punish the individual" further with jail time (p. 483-84). Indeed, U.S. commissioner Stephen Breyer (1985-

89) who played a key role in forming original Guideline measures, noted courts granted leniency more frequently in white-collar fraud cases when compared to common or street crimes (see also Simpson 2013). The USSC then "mitigated [resultant] discrepancies" by ordering shorter sentences for these offenders than those imposed prior to the Guidelines (Breyer 1988). The current study also revealed significant socio-temporal era effects whereby white-collar sentences were most lenient post-*Booker* when the Guidelines were deemed advisory. Furthermore, during the *narrow guideline era*—in which several high-profile cases were tried and notable sentencing directives to curb white-collar criminality introduced—higher volumes of white-collar cases disposed and all districts under study meted out their shortest sentences.

Interpretations of Extra-legal and Judicial Attribute Findings

Extra-legal effects proved robust for this specific offending population and varied across policy eras. Men were more likely to be tried and convicted of white-collar crimes; odds of incarceration and sentence lengths were significantly higher for male offenders (Eitle 2000; USSC 2006; Galvin and Simpson 2020). Having dependents and higher levels of education were consistently associated with less severe sentences across policy periods, similar to Schanzenbach and Yaeger (2006) study conclusions. Similarly, having a college degree was related to decreased severity when compared to less educated offenders, mirroring reports from Hagan, Nagel, and Albonetti (1980), Maddan et al (2012) and the USSC (2010) regarding the relative importance of educational attainment in rendering decisions. Including the quadratic age term into models of sentencing outcomes revealed that while severity increased with offender age, leniency was reserved for the eldest offenders in the subset. The non-linear relationship between age and sentencing severity lends credence the general age-crime curve theorized by

Gottsfredson and Hirschi (1990), signaling leniency trends based on offender age might be generalizable to diverse criminal populations.

Contrary to extant study findings, hierarchical logistic regression revealed significant conditioning effects of offender race on incarceration decisions in the nested model; white defendants experienced lower odds of receiving jail time when compared to defendants of color suggesting considerations of this extra-legal characteristic factor into decisions made at earlier stages in the sentencing process. Hagan, Nagel, and Albonetti's (1980) findings insinuate race/ethnicity might matter more in common or street crime than white-collar outcomes. This makes theoretical sense given the relatively homogenous racial composition of white-collar offenders in the subset. Notwithstanding, manifest racial differences in sentencing are cause for concern as they call into question whether such disparities under the Guidelines are linked to differential offender treatment by different judges or resulting from general or widespread discriminatory sentencing practices. The present study utilized a dichotomous measure of race following existing research; further investigations comparing outcomes for specific racial groups across policy periods could inform interpretations. Overall, study findings imply sentencing disparities for white-collar offenders might be explained by differential offender treatment by different judges in different judicial districts *as a function of loosening restrictions on discretion.* Explicitly stated, while legal factors continue to be strongest predictors of sentence severity, it appears increased opportunity to render decisions according to substantively rational interests has also created the potential for sentencing biases to go unchecked.

Sentencing disparities were observed across district courts, even after accounting for legal and extra-legal case components. However, findings for judicial attribute variables should be interpreted with caution due to high volumes of missing data for the *mandatory guideline era*

and high standard error calculations. Namely districts prosecuting the highest volumes of white-collar crimes meted out the most temperate punishments. Per results of hierarchical logistic regression, districts with greater compositions of Republican-appointed, male and white judges and greater proportions of prior prosecutorial experience at the state or federal level were negatively associated with decisions to incarcerate during the *mandatory* policy era. Associations reverse for *%White*, and *%Republican*-appointed judges starting in the *narrow* era whereby odds of incarceration increased and persisted post-*Booker* into 2015. In contrast to Schanzenbach's and Tiller's (2007) study findings for white-collar and environmental crimes, judges appointed by Republican presidents meted out lengthier incarceration sentences than their Democratic appointed counterparts. Further, greater district compositions of prior prosecutorial experience, male, and white judges were associated with shorter prison sentences post-*Booker*. These findings contradict many studies that find null effects for judicial characteristics on criminal sentencing at the federal level (see Schazenbach 2005) and underscore the importance of including district-level variables in complex assessments of sentencing outcomes for white-collar offenders.

Findings for some study measures lead the researcher to question the designation of certain individual-level characteristics (i.e., age, education, having dependents) and district-level indicators with "extra-legal" labels in sentencing research if (1) these factors are considered relevant to sentencing by governing bodies, and (2) judges are expected to act according to the sentencing "climate" in their profession and respective jurisdictions (USSC 2010; Wheeler, Weisburd, and Bode 1982; see Wheeler, Weisburd, Waring, and Bode 1987, p. 331). For judges in the sample, indicators of future dangerousness, status, prestige, and opportunity influenced severity decisions, while the specific effects of these considerations on outcomes varied across

time periods. Research might instead frame these auxiliary indicators as substantively rational focal concerns separate from "purely extra-legal" attributes expressly prohibited from consideration by the courts (i.e., race, sex, national origin, creed, religion, or socio-economic status; USSG §5K2.0 (d)(1)). Maybe then dynamic discussion might rightfully ensue regarding the appropriateness of extra-legal observed effects on sentencing outcomes and resulting implications.

Limitations and Suggestions for Future Research

This exploratory study is not without its limitations. First, defendants in the dataset have been prosecuted and convicted. While findings from this study might be useful for ascertaining sentencing patterns in the selected federal districts, it leaves additional questions regarding the punishment of white-collar crimes unanswered (e.g., what factors influence whether a case is escalated to crime control agencies? In particular socio-temporal contexts?), and limits generalizability of the study's findings to specific geographical areas. Research on the extent of alternative punishments for the white-collar accused (see Schanzenbach and Yaeger 2006) and the (non-) reporting habits of victimized individuals (Kennedy 2014) would shed light of the use and efficacy of crime approaches employed in lieu of jail time.

A second related issue is one of criminal disposition: is incarceration the most appropriate outcome to investigate in white-collar criminal cases (see Braithwaite 1985)? Shapiro (1990) argues lenient punishment "arises from limitations in the dilemmas over the choice of sanctions for those convicted of abuse of trust" (p. 355). The prosecution of white-collar cases often utilizes more prosecutorial and federal resources compared to other crime types and requires specialization often unavailable in smaller districts (Burns and Meitle 2020).

Evidence from Kenneth, Mann, and Sarat (1980) suggests judges will forestall imprisonment for even the most serious economic violations if they deem the offender a staple in their family or the community. Hagan and Nagel (1982) found white-collar defendants benefit over other offender types since judges often consider specific sorts (e.g., fines) and combinations of punishments (e.g., fines and probation) in lieu of or to shorten prison sentences. To illustrate, in the 2015 fiscal year, "fines were most commonly imposed in antitrust cases (73.4%) …[while] restitution was most commonly ordered in cases involving embezzlement (86.3%) … tax (75.4%), and fraud (72.9%)" (USSC 2016). In the current study, use of these alternative punishments supports insignificant regression slope coefficients for particular offense types, and specifically antitrust violations and embezzlement. While rates of incarceration per era were high for the sample (over 60%), these values include offenders sentenced to 1 month of incarceration, typically noted as probation in official records.

Controversy surrounding overly lenient sentences meted out to white-collar offenders continues to pervade American culture and especially media. Reference a recent example of a recommended sentence reduced from 5 years to probation due to the offender's ability to pay restitution (Henning 2018), and 2-week sentences doled out to offenders for their roles in recent college admissions scandals disposed in California (Li 2019). Our attention is often drawn to large-scale and corporate crimes accompanied by harsh sentences and these sensationalized offenses typically mirror Sutherland's (1940) original definition of "white-collar" crimes (Friedrichs 2020); as mentioned, especially "salacious" high-loss antitrust violations and money laundering cases were rare in the federal dataset and this uniqueness (among other things) further increases media salability. Analyses revealed average sentences across eras were less than two years for this sample causing loss under $450,000,000, mirroring official average sentence length

penalty reports of 27 months imposed in fraud cases (USSC 2016). Nevertheless, Cullen, Hartman, and Johnson (2009) attribute overly punitive or "revenge-based" sentencing policies, like those introduced during the *narrow guideline era*, to constant media exposure that vilifies high-status defendants. Findings from Mann, Wheeler, and Sarat (1980) might shed light on these sentencing decisions examples, as judges consider collateral damages and these symbolic "status degradation ceremonies" crafted from trial and tabloid reporting as punishment enough for high-status offenders and especially so when restitution is possible (Simpson 2013, p. 321). Eitle (2000) notes that "legal tools such as the RICO statute and civil fining authority give regulators/prosecutors access to powerful sanctions outside the criminal justice system," useful considering the high cost of incarceration (p. 812). Future research might also investigate contextual factors influencing particular sanction outcomes (e.g., fines/restitution vs incarceration) pertinent to white-collar offending (Eitle 2000; Holtfreter 2013).

Data in this study did not contain information on district prosecutorial practices that occur prior to the final sentencing decision, although modern criminological research acknowledges the differing prosecutorial goals distinct to street and white-collar crimes and possible effects on outcomes (Burns and Meitle 2020). Braniff (1993) notes "judges cannot deal with cases until they are in the system after formal charging"—a decision which rests entirely on the prosecutor's decision to go to trial or press for harsher consequences should the defendant resist a plea. Furthermore, prosecutors individual pleading practices and sentence recommendations can contribute significantly to district sentencing variations, based on their own understandings of the cost of going to trial, local crime problems, and the organizational context among other things (Bibas 2005). While some centralized prosecution policies exist, many of these decisions can be resolved informally outside the restrictions of the Guidelines

(Koons-Witt 2002; Ulmer 2012). Hagan, Nagel, and Albonetti (1980) add that as a result of the proactive nature of white-collar investigations, prosecutorial negotiation becomes crucial to apprehending major players, and agreements reached between "prosecutorial and judicial subsystems" bring to fruition "the promises and concessions offered white-collar offenders" at the sentencing stage (p. 818). This idea is consistent with extant research (Tiede, Carp, and Manning 2010; Bibas 2005); in the fiscal year 2015 the vast majority of offenders (97.1%) pleaded guilty. Among those offenders who did, 50.7% received a sentence below the applicable sentencing guideline range (USSC 2016). It is possible the influence of prosecutorial negotiation might be reflected in this study's null findings related to rates of plea acceptance over 92%. Further, districts with greater compositions of prior prosecutorial experience meted out more lenient sentences for white-collar offenders, highlighting the relative importance of prosecutorial influence on sentencing outcomes (Friedrichs 2010). Going forward, research might better account for prosecutorial discretion and the organizational context of particular federal districts in models of sentencing outcomes for white-collar offenders by only analyzing white-collar cases decided by bench or jury trial.

Conclusions

While extant studies have parsed criminal sentencing data by policy era or compared severity scores pre- and post-*Booker* (e.g., Tiede, Carp, and Manning 2012; Hewitt 2015), white-collar sentencing research that accounts for changes in sentencing policy is scarce (Galvin and Simpson 2020). Using an offense-based operationalization of white-collar crimes and a tiered model of courtroom decision-making that accounts for nested effects of individual-level, district-level, and contextual variants, results denote sentencing outcomes varied significantly across

courtrooms and across time periods. Study results additionally strengthen the argument for using hierarchical analytic methods to account for dynamic sentencing processes; adjustments to error calculations afforded by the stepwise models presented unexpected insights not previously revealed with OLS and binary logistic regression (Hendershot and Tecklenburg 2011). A socio-temporal trend emerged whereby offenders sentenced in the years following the *Booker* decision were least likely to receive jail time and received the shortest overall sentences when compared to previous white-collar policy eras. Legislation introduced just prior beginning in the late 1990s was passed to curtail white-collar offending and streamline punishments, directly contributing to severity level observed during this time period (Pogdor 2007; USSC 2008; Burns and Meitle 2020).

Legal-rational case components provisioned by the Guidelines were found to consistently drive sentencing outcomes. Judge attribute variables also affected sentencing outcomes signaling such variants must be further studied in future criminological investigations of state and federal sentencing disparities. Extra-legal offender characteristics of age and gender were strong predictors of increased severity; educational attainment, being elderly, and having dependents generally decreased sentence severity since judges also consider collateral damages, offense level adjustments, and provisions for alternative punishments (Mann, Wheeler, and Sarat 1980). Surprisingly and in contrast to the majority of existing white-collar sentencing research, offender race emerged as a significant predictor of incarceration in the fully conditional hierarchical logistic regression model, even after controlling for legal and district-level sentencing factors (Benson and Walker 1988).

Maddan et. al's (2010, p. 14) study contrasting white-collar and street crimes concludes that white-collar offender sentences were based more on extra-legal factors, whereas street

offenders appeared to receive sentences based more on factors associated with legal Guideline parameters. I would re-interpret their conclusions as further indication that *focal concerns* of—and constraints on— district judiciary vary temporally and contextually. Perhaps for white-collar offenders, legal-rational case components generally absorb the perceptual shorthand that arise from substantive policy amendments, personal biases, inexperience, lack of resources and any unclear or confusing sentencing guideline specifications. However, some uncertainty in the ability of legal measures alone to render just sentences might lead judges to rely on past experiences with white-collar offenders, extra-legal "evidence" of culpability (blameworthiness), and social indicators of opportunity, contravention of trust, and abuse of power (Albonetti 1987; Steffensmeier and Demuth 2000) leaving many judges reluctant to impose harsh or lengthy prison sentences (Burns and Meitle 2020). Offender background characteristics representative of status and prestige both "lend the appearance of legitimacy" making victims easier to deceive and provide "more opportunity to occupy organizational positions with the greatest potential for large-scale offenses" (Wheeler, Weisburd, Waring, and Bode 1987, p.331). On the other hand, status symbols can also indicate rehabilitative and restitutive ability—factors for which judges reduce sentences or seek alternative punishment. White-collar offenders are assumed to have stable jobs, dependents, and other social capital; incarceration might prolong suffering of individuals and communities reliant on their services (Burns and Meitle 2020). For example, possessing a college degree—often considered a proxy of social status—was associated with less severe sentences for this sample, producing progressively larger effects on outcomes throughout the lifetime of the Guidelines. Therefore, it is not entirely clear why offender race emerged as a significant predictor of incarceration in the hierarchical model during later guideline eras,

especially given the relatively homogenous sample of offenders in the dataset, and null findings for the initial *mandatory* policy period.

In addition to notable findings for offender race, this study found some extra-legal and district-level factors were tied to *less* severe punishments, especially during the advisory guideline era following the *Booker* ruling (see Schanzenbach and Yaeger 2006). Burns and Meitle (2020) note invoking criminal law in white-collar cases is necessary; I add this is especially so given the far-reaching harm they cause necessarily warrants more stringent penalties as deemed by federal law. The greatest concern, then, rests in the obfuscation of discrimination in federal decision-making processes: Thomas and Zingraff (1981, p. 879) pose the question "is it preferable to conclude that discrimination may exist when in fact it does not, or to conclude that discrimination may not exist when in fact it does?" Especially when factors auxiliary to the legal process are shown to take precedence after a return to judicial discretion, we might validly question the specific reasoning behind such determinations, and if the flexibility granted through individualized sentencing merits the possibility of unjustly lenient or severe punishments. And since criminal sentencing is a crucial form of social control with broad reaching consequences, adherence to substantively rational interests that are exercised more readily and to a greater extent with particular offending groups can work to the detriment of Guideline enforcement, violating bureaucratic goals of uniformity and reducing public faith in the justice system.

Thus, conclusions warrant scrutiny of the Guidelines, specifically to include tighter measures of culpability for economic and fraud cases. According to Pogdor (2007), "culpability is basically non-existent as a sentencing concern, with the punishment resting on a numerical figure that correlates with the amount of loss occurring as a result of the crime" (p. 756). As

aforementioned, more precise approximations of a defendant's culpable mental state should work to decrease uncertainty in rendering appropriately severe for white-collar offenders (Albonetti 1991). Hewitt (2015) proposes amending white-collar Guidelines' assessments of defendant culpability by:

(1) reducing the severity of loss table enhancements,

(2) increasing offense levels based on offender's pecuniary gain, and

(3) replacing provisions for intended loss with measures of actual loss accrued.

Another resolution involves weighting the offender's role in commission of the crime more heavily in crime seriousness/offense level calculations.

As society relies increasingly on the exchange of symbolic proxies in government and commercial industry—and especially so in electronic form—opportunities to commit white-collar crimes will continue to abound (Shapiro 1990). As is demonstrated here, factors such as case "complexity, difficulty of gathering evidence, and the relative rarity of these cases" compounds problems in discretionary sentencing of these offenders, often resulting in more favorable plea agreements than warranted by the Guidelines and existing economic legislation passed to curtail white-collar criminality (Galvin and Simpson 2020, p. 391). Analysis of extra-legal effects on white-collar sentencing disparities draws our attention to the systemic ways in which symbols of status and power can "create and sustain a definition of legality that in itself reinforces those social structures" to the detriment of uniformity, and ultimately justice (Galvin and Simpson 2020, p. 384). Sociologically informed investigations of white-collar crime as a subclass with distinguishable offense, victimization, and punishment patterns can offer important contributions to criminal sentencing research so long as individual-, district-level and contextual factors are given their due credence in quantitative analyses. General study findings demonstrate

judiciary concerns vary contextually and as such, amendments to criminal sentencing guidelines *should* reflect the broad and probable sociological and organizational implications for disparate offending groups in order to justly render individualized penalties.

Appendix A

TABLE 15. Sentencing Table (in months of imprisonment)

		Criminal History Category (criminal history points)					
	Offense Level	**I** (0 or 1)	**II** (2 or 3)	**III** (4, 5, 6)	**IV** (7, 8, 9)	**V** (10, 11, 12)	**VI** (13 or more)
	1	0-6	0-6	0-6	0-6	0-6	0-6
	2	0-6	0-6	0-6	0-6	0-6	1-7
	3	0-6	0-6	0-6	0-6	2-8	3-9
	4	0-6	0-6	0-6	2-8	4-10	6-12
Zone A	**5**	0-6	0-6	1-7	4-10	6-12	9-15
	6	0-6	1-7	2-8	6-12	9-15	12-18
	7	0-6	2-8	4-10	8-14	12-18	15-21
	8	0-6	4-10	6-12	10-16	15-21	18-24
	9	4-10	6-12	8-14	12-18	18-24	21-27
Zone B	**10**	6-12	8-14	10-16	15-21	21-27	24-30
	11	8-14	10-16	12-18	18-24	24-30	27-33
	12	10-16	12-18	15-21	21-27	27-33	30-37
Zone C	**13**	12-18	15-21	18-24	24-30	30-37	33-41
	14	15-21	18-24	21-27	27-33	33-41	37-46
	15	18-24	21-27	24-30	30-37	37-46	41-51
	16	21-27	24-30	27-33	33-41	41-51	46-57
	17	24-30	27-33	30-37	37-46	46-57	51-63
	18	27-33	30-37	33-41	41-51	51-63	57-71
	19	30-37	33-41	37-46	46-57	57-71	63-78
	20	33-41	37-46	41-51	51-63	63-78	70-87
	21	37-46	41-51	46-57	57-71	70-87	77-96
	22	41-51	46-57	51-63	63-78	77-96	84-105
	23	46-57	51-63	57-71	70-87	84-105	92-115
	24	51-63	57-71	63-78	77-96	92-115	100-125
	25	57-71	63-78	70-87	84-105	100-125	110-137
	26	63-78	70-87	78-97	92-115	110-137	120-150
	27	70-87	78-97	87-108	100-125	120-150	130-162
	28	78-97	87-108	97-121	110-137	130-162	140-175
Zone D	**29**	87-108	97-121	108-135	121-151	140-175	151-188
	30	97-121	108-135	121-151	135-168	151-188	168-210
	31	108-135	121-151	135-168	151-188	168-210	188-235
	32	121-151	135-168	151-188	168-210	188-235	210-262
	33	135-168	151-188	168-210	188-235	210-262	235-293
	34	151-188	168-210	188-235	210-262	235-293	262-327
	35	168-210	188-235	210-262	235-293	262-327	292-365
	36	188-235	210-262	235-293	262-327	292-365	324-405
	37	210-262	235-293	262-327	292-365	324-405	360-life
	38	235-293	262-327	292-365	324-405	360-life	360-life
	39	262-327	292-365	324-405	360-life	360-life	360-life
	40	292-365	324-405	360-life	360-life	360-life	360-life
	41	324-405	360-life	360-life	360-life	360-life	360-life
	42	360-life	360-life	360-life	360-life	360-life	360-life
	43	Life	Life	Life	Life	Life	Life

Appendix B

TABLE 16. Variable Coding and Descriptions

Variable	Measurement
Socio-temporal:	
Year	25-level interval variable: coded (1) cases decided in 1990, (2) cases decided in 1991, etc. through 2015
Guideline Era	Coded (1) Mandatory: cases disposed 1990-1999; (2) Narrow: cases disposed 2000-2005; or (3) Advisory: cases disposed 2006-2015/ post-*Booker*
Legal Process:	
Acceptance of Responsibility	Binary: (1) yes, (0) no
Case Disposition	Binary: (1) trial, (0) plea
Conviction Counts	Binary: (1) multiple counts, (0) single count,
Departures[a]	Binary: (1) departure applied, (0) no departure applied
Criminal History	Binary: (1) yes, (0) no
Offense Level	43-level ordinal variable
Offense Type	8-category nominal variable and 8 dummy variables: antitrust violations, bribery, embezzlement, forgery/counterfeiting, fraud, money laundering, racketeering/extortion, and tax offenses*
Presentence Detention Status[a]	4-category nominal variable: (1) in custody, (2) out on bail/bond, (3) out on own recognizance, (4) other
Extra-legal:	
Age	In years
Age^2	To account for the theorized age-crime curve
Dependents	Binary: (1) dependents, (0) no dependents
Offender Race	Binary: (1) person of color*, (0) white
Offender Sex/Gender	Binary: (1) male, (0) female*
Offender Education	4-category ordinal scale indicating highest level of education attained, ranging from "less than HS*" to "college graduate"
District and Judicial Attributes:	
Appointing President Ideology	Composition: (%) Democrat*, (%) Republican
Prior Prosecutorial Experience	Composition: (%) prior state or federal experience, (%) no prior state or federal experience*
Judge Sex	Composition: (%) male, (%) female*
Judge Race	Composition: (%) non-white*, (%) white
Judge Tenure	Interval variable measuring average tenure in each district (capped at 2015)
District	6-category nominal variable and 6 dummy variables: S. New York, W. Texas, N. Illinois, C. California, S. Florida, N. Georgia*
Caseload/vacancies[a]	Interval variable measuring the annual average number of criminal cases filed in a district less the number of vacancies, divided by the number of authorized judgeships
Dependent/Outcome Variables:	
In/Out	Binary: (1) imprisonment, (0) no imprisonment
(Log) Length of Imprisonment	Log of months of imprisonment given for those imprisoned (capped at 470 months)

*Indicates variable excluded for comparison

[a]Indicates variable not measured in all years under study

Appendix C

TABLE 17. Multicollinearity Statistics for Independent Variables

Variable	**Tolerance** *(min >0.10)*	**Variance Inflation Factor** *(max < 10.0)*
Legal Process:		
Acceptance of Responsibility	0.556	1.797
Case Disposition	0.829	1.206
Conviction Counts	0.748	1.337
Departures	0.963	1.039
Criminal History	0.928	1.077
Offense Level	0.490	2.061
Offense Type	0.833	1.200
Extra-legal:		
Dependents	0.949	1.054
Offender Race	0.849	1.177
Offender Sex	0.962	1.040
Offender Education	0.940	1.064
District and Judicial Attributes:		
% Republican	0.264	3.792
% Prior Prosecutorial Experience	0.564	1.774
% Male	0.273	3.659
%White	0.973	1.028
Mean Tenure	0.228	4.393
District	0.837	1.195
Caseload/vacancies	0.239	4.175

Appendix D

Analysis Coding Commands

Recoding/Sorting Commands:

```
SORT CASES  BY WCDISTRICT.
SPLIT FILE SEPARATE BY WCDISTRICT.
DESCRIPTIVES VARIABLES=CASERATE PERWHITE PERMALE PERREP PERPRIOR
AVGTENURE
  /STATISTICS=MEAN STDDEV MIN MAX.


RECODE YEAR (1990 thru 1999=1) (2000 thru 2005=2) (2006 thru
2015=3) INTO GUIDEERA.
VARIABLE LABELS  GUIDEERA '3 GUIDELINE ERAS'.
EXECUTE.
SORT CASES  BY GUIDEERA.
SPLIT FILE SEPARATE BY GUIDEERA.
DESCRIPTIVES VARIABLES=CASERATE PERWHITE PERMALE PERREP PERPRIOR
AVGTENURE
  /STATISTICS=MEAN STDDEV MIN MAX.


RECODE DISTRICT (SYSMIS=SYSMIS) (8=1) (42=2) (52=3) (73=4)
(31=5) (32=6) (0 thru 7=0) (9 thru 30=0)
    (33 thru 41=0) (43 thru 51=0) (53 thru 72=0) (74 thru
99999999=0) INTO WCDISTRICT.
VARIABLE LABELS  WCDISTRICT '6 DISTRICTS UNDER STUDY'.
EXECUTE.
DATASET ACTIVATE DataSet1.

RECODE OFFTYPE2 (SYSMIS=SYSMIS) (18=1) (19=2) (20=3) (21=4)
(22=5) (23=6) (24=7) (33=8) (0 thru
    17=0) (25 thru 32=0) (34 thru 9999999999999=0) INTO WCOFF.
VARIABLE LABELS  WCOFF '8 WC OFFENSES UNDER STUDY'.
EXECUTE.
DATASET ACTIVATE DataSet1.

RECODE NUMDEPEN (SYSMIS=SYSMIS) (0=0) (1 thru 9999999999999=1)
INTO BIDEPEND.
VARIABLE LABELS  BIDEPEND 'BINARY DEPENDENTS'.
EXECUTE.
RECODE ACCTRESP (SYSMIS=SYSMIS) (0=0) (1 thru 9999999999999=1)
INTO BIACCTRESP.
VARIABLE LABELS  BIACCTRESP 'BINARY ACCTRESP'.
EXECUTE.
```

```
RECODE ACCTRESP (SYSMIS=SYSMIS) (0=0) (-4 thru -1=1) (-9999 thru
-5=SYSMIS) INTO BIACCTRESP.
VARIABLE LABELS  BIACCTRESP 'BINARY ACCTRESP'.
EXECUTE.
RECODE NOCOUNTS (SYSMIS=SYSMIS) (0=SYSMIS) (1=0) (2 thru 999=1)
INTO BICOUNTS.
VARIABLE LABELS  BICOUNTS 'BINARY COUNTS'.
EXECUTE.
FREQUENCIES VARIABLES=WCOFF WCDISTRICT BIDEPEND BIACCTRESP
BICOUNTS NEWRACE NEWEDUC NEWCNVTN MONSEX
    INOUT HISPORIG CRIMHIST PRESENT
  /ORDER=ANALYSIS.

RECODE LOSSHI (9999999997=SYSMIS) (5000000000 thru
9999999997=SYSMIS).
EXECUTE.
DESCRIPTIVES VARIABLES=LOSSHI
  /STATISTICS=MEAN STDDEV MIN MAX.

RECODE LOSSHI (9999999997=SYSMIS) (50000000 thru
9999999997=SYSMIS).
EXECUTE.
DESCRIPTIVES VARIABLES=LOSSHI
  /STATISTICS=MEAN STDDEV MIN MAX.

RECODE SENTTOT0 (SYSMIS=SYSMIS) (0 thru .99=0) (1 thru 470=1)
(471 thru 999999999999=SYSMIS) INTO
    WCPRISON.
EXECUTE.
FREQUENCIES VARIABLES=WCPRISON
  /ORDER=ANALYSIS.

RECODE WCOFF (SYSMIS=SYSMIS) (1=1) (2 thru 8=0) INTO FRAUD.
VARIABLE LABELS  FRAUD 'FRAUD  ONLY'.
EXECUTE.
RECODE WCOFF (SYSMIS=SYSMIS) (2=1) (1=0) (3 thru 8=0) INTO
EMBEZZLE.
VARIABLE LABELS  EMBEZZLE 'EMBEZZLE  ONLY'.
EXECUTE.
RECODE WCOFF (SYSMIS=SYSMIS) (3=1) (1 thru 2=0) (4 thru 8=0)
INTO FORGERY.
VARIABLE LABELS  FORGERY 'FORGE/COUNTER  ONLY'.
EXECUTE.
SPSSINC CREATE DUMMIES VARIABLE=WCOFF
ROOTNAME1=DUM
/OPTIONS ORDER=A USEVALUELABELS=YES USEML=YES OMITFIRST=NO.
```

```
SPSSINC CREATE DUMMIES VARIABLE=WCDISTRICT
ROOTNAME1=DUM
/OPTIONS ORDER=A USEVALUELABELS=YES USEML=YES OMITFIRST=NO.

RECODE WCOFF (8=1) (6=1) (0=SYSMIS) (1=0) (2=0) (3=0) (4=0)
(5=0) (7=0) INTO BIWCOFF.
VARIABLE LABELS  BIWCOFF 'hi ticket vs low ticket offenses'.
EXECUTE.
```

Regression Commands:

```
REGRESSION
  /MISSING LISTWISE
  /STATISTICS COEFF OUTS R ANOVA
  /CRITERIA=PIN(.05) POUT(.10)
  /NOORIGIN
  /DEPENDENT LOGLEN
  /METHOD=ENTER XFOLSOR CRIMHIST WCOFF BIACCTRESP BICOUNTS
NEWCNVTN AGE AGE2 COLLGRAD BIDEPEND
    WHITE MALE
  /METHOD=ENTER WCDISTRICT PERWHITE PERMALE PERREP PERPRIOR
AVGTENURE CASERATE.


REGRESSION
  /MISSING LISTWISE
  /STATISTICS COEFF OUTS R ANOVA
  /CRITERIA=PIN(.05) POUT(.10)
  /NOORIGIN
  /DEPENDENT LOGLEN
  /METHOD=ENTER XFOLSOR CRIMHIST BIWCOFF BIACCTRESP BICOUNTS
  /METHOD=ENTER BIRACE MONSEX EDUCATN AGE AGE2 BIDEPEND
  /METHOD=ENTER CASERATE WCDISTRICT PERWHITE PERMALE PERREP
PERPRIOR AVGTENURE
  /METHOD=ENTER GUIDEERA.

REGRESSION
  /MISSING LISTWISE
  /STATISTICS COEFF OUTS R ANOVA
  /CRITERIA=PIN(.05) POUT(.10)
  /NOORIGIN
  /DEPENDENT LOGLEN
  /METHOD=ENTER XFOLSOR CRIMHIST BIACCTRESP BICOUNTS HITICKET
  /METHOD=ENTER BIRACE MONSEX EDUCATN AGE AGE2 BIDEPEND
  /METHOD=ENTER CASERATE WCDISTRICT PERWHITE PERMALE PERREP
PERPRIOR AVGTENURE
  /METHOD=ENTER GUIDEERA.
```

```
LOGISTIC REGRESSION VARIABLES WCPRISON
  /METHOD=ENTER CRIMHIST XFOLSOR WCOFF BIACCTRESP BICOUNTS
NEWCNVTN AGE AGE2 COLLGRAD WHITE MALE
  /METHOD=ENTER PERWHITE PERMALE PERREP PERPRIOR AVGTENURE
CASERATE SNY WTX NIL CCA SFL NGA
    CASERATE*SNY PERWHITE*SNY PERREP*SNY PERPRIOR*SNY
PERMALE*SNY AVGTENURE*SNY CASERATE*WTX
    AVGTENURE*WTX PERWHITE*WTX PERMALE*WTX PERREP*WTX
PERPRIOR*WTX AVGTENURE*NIL NIL*PERPRIOR
    NIL*PERREP NIL*PERMALE NIL*PERWHITE CASERATE*NIL
AVGTENURE*CCA PERPRIOR*SFL PERREP*SFL PERMALE*SFL
    PERWHITE*SFL CASERATE*SFL AVGTENURE*SFL CCA*PERPRIOR
CCA*PERREP CCA*PERMALE CCA*PERWHITE
    CASERATE*CCA
  /CRITERIA=PIN(.05) POUT(.10) ITERATE(20) CUT(.5).

LOGISTIC REGRESSION VARIABLES WCPRISON
  /METHOD=ENTER CRIMHIST XFOLSOR WCOFF BIACCTRESP BICOUNTS
NEWCNVTN AGE AGE2 COLLGRAD WHITE MALE
  /METHOD=ENTER PERWHITE PERMALE PERREP PERPRIOR AVGTENURE
CASERATE SNY WTX NIL CCA SFL NGA
    CASERATE*SNY PERWHITE*SNY PERREP*SNY PERPRIOR*SNY
PERMALE*SNY AVGTENURE*SNY CASERATE*WTX
    AVGTENURE*WTX PERWHITE*WTX PERMALE*WTX PERREP*WTX
PERPRIOR*WTX AVGTENURE*NIL NIL*PERPRIOR
    NIL*PERREP NIL*PERMALE NIL*PERWHITE CASERATE*NIL
AVGTENURE*CCA PERPRIOR*SFL PERREP*SFL PERMALE*SFL
    PERWHITE*SFL CASERATE*SFL AVGTENURE*SFL CCA*PERPRIOR
CCA*PERREP CCA*PERMALE CCA*PERWHITE
    CASERATE*CCA
  /CRITERIA=PIN(.05) POUT(.10) ITERATE(20) CUT(.5).

GET
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DATASET-edited.sav'.
DATASET NAME DataSet1 WINDOW=FRONT.
RECODE WCOFF (8=1) (6=1) (0=SYSMIS) (1=0) (2=0) (3=0) (4=0)
(5=0) (7=0) INTO BIWCOFF.
VARIABLE LABELS  BIWCOFF 'hi ticket vs low ticket offenses'.
EXECUTE.
SPLIT FILE OFF.
LOGISTIC REGRESSION VARIABLES WCPRISON
  /METHOD=ENTER BIWCOFF XFOLSOR CRIMHIST NEWCNVTN BIACCTRESP
BICOUNTS
  /METHOD=ENTER MONSEX EDUCATN BIDEPEND AGE AGE2 BIRACE
  /METHOD=ENTER CASERATE PERWHITE PERMALE PERREP PERPRIOR
AVGTENURE WCDISTRICT CASERATE*WCDISTRICT
```

```
    PERWHITE*WCDISTRICT PERMALE*WCDISTRICT PERREP*WCDISTRICT
PERPRIOR*WCDISTRICT AVGTENURE*WCDISTRICT
  /METHOD=ENTER GUIDEERA
  /CONTRAST (DISTRICT)=Indicator
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LOGISTIC REGRESSION VARIABLES WCPRISON
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  /METHOD=ENTER MONSEX EDUCATN BIDEPEND AGE AGE2 BIRACE
  /METHOD=ENTER CASERATE PERWHITE PERMALE PERREP PERPRIOR
AVGTENURE WCDISTRICT CASERATE*WCDISTRICT
    PERWHITE*WCDISTRICT PERMALE*WCDISTRICT PERREP*WCDISTRICT
PERPRIOR*WCDISTRICT AVGTENURE*WCDISTRICT
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Curriculum Vitae

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EDUCATION

2020 Ph.D., Sociology, *University of Nevada, Las Vegas*
Areas of Specialization: Crime/Deviance & Criminology; Culture
Dissertation: "An Investigation of White-Collar Criminal Sentencing Disparities in Six Federal District Courts" (Final Defense Date: 1/31/2020)
Cumulative GPA: 3.90

2013 M.A., Communication Studies, *University of Nevada, Las Vegas*

2011 B.A., Communication Studies, Minor: French Studies, *University of Nevada, Las Vegas*

RESEARCH SPECIALIZATIONS AND INTERESTS

White-Collar Crime; Cultural Criminology; Media Studies; Quantitative Methods

PUBLICATIONS

2020 **Galloway, L. F. E.** "'A Conspiracy of the Nation': Case Study of Stokely Carmichael's and H. Rap Brown's Arguments in Support of Black Power." 2020, January. *The Journal of Black Studies* 51(1): 83-102. DOI: 10.1177/0021934719892296

2015 **Galloway, L. F. E.**, Engstrom, E., & Emmers-Sommer, T. 2015. "Does Movie Viewing Cultivate Young People's Unrealistic Expectations about Love and Marriage?" *Marriage and Family Review* 51(8): 687-712. DOI: 10.1080/01494929.2015.1061629

PRESENTATIONS

Invited Presentations

2017 **Galloway, L. F. E.** 2017, November. "'Dark' Celebrity Culture: Constructions of Infamy in True Crime Documentaries." Invited special conference plenary talk delivered at the Stardom, Celebrity, and Fandom Conference, Fort Worth, TX.

2012 **Galloway, L. F. E.** 2012, November. "Highly Apprehensive Students in the Basic Public Speaking Course: Assessment and Interventions for Increasing Student Retention and Successful Completion*."* Invited panel discussant to the 98th Annual Convention of the National Communication Association, Orlando, FL.

Professional Presentations

2018 **Galloway, L. F. E.** 2018, March. "The Influence of Formal and Bounded Rationality Concerns on White-Collar Criminal Sentencing Outcomes." Paper presented at the Pacific Sociological Association's 89th Annual Meetings/Conference, Long Beach, CA.

2018 **Galloway, L. F. E.** 2018, February. "An Investigation of White-Collar Criminal Sentencing Disparities in Six Federal District Courts." Paper presented at the 45th Annual Western Society of Criminology Conference, Long Beach, CA.

2016 **Galloway, L. F. E.** 2016, May. "How to Properly Fangirl/boy: Analysis of Social Media Texts." Paper presented at the 2016 Southwestern Social Science Association Conference, Las Vegas, NV.

2014 **Galloway, L. F. E.** 2014, May. "'A Conspiracy of the Nation': Case Study of the Newly Militant Student Non-Violent Coordinating Committee (SNCC)." Paper presented at the 2014 Rhetoric Society of America Biennial Conference, San Antonio, TX.

2013 Engstrom, E., & **Galloway, L. F. E.** 2013, August. "Does Movie Viewing Cultivate Unrealistic Expectations about Love and Marriage?" Poster presentation at the 2013 Association for Education in Journalism and Mass Communication Conference, Washington, D.C.

2013 **Galloway, L. F. E.** 2013, April. "Love, Marriage, and Movies." Poster Presentation at the Greenspun College of Urban Affairs Research Symposium, Las Vegas, NV.

2013 **Galloway, L. F. E.** 2013, March. "Accommodating Highly Apprehensive Students in the Basic Course." Presentation at the Spring 2013 UNLV Graduate and Professional Student Association Forum, Las Vegas, NV.

2012 **Galloway, L. F. E.** 2012, October. "The Old Spice Man Knows Best: Instructions for the Male Image." Paper presented at the 35th Annual Meeting of the Organization for the Study of Communication, Language, and Gender, Tacoma, WA.

Academic Workshops Developed and Hosted

2018 2018, February. "How to Write a Strong Statement of Support." Developed and presented on behalf of the Office of Undergraduate Research

2017 2017, December. "How to Write an Effective Academic Letter of Recommendation." Developed and presented to Engelstad Scholars Program Community Partners on behalf of the Office of Undergraduate Research.

2017 2017, July. "Curriculum Vitae (CV) and Crafting a Professional Email Workshop." Developed and presented at the OUR-UNLV Research Skills Academy on behalf of the Office of Undergraduate Research.

2016 2016, September. "Ethics and Data Collection." Developed and presented on behalf of the Office of Undergraduate Research.

2016 2016, July. "Crafting an Effective Lightning Talk." Developed and presented on behalf of the Office of Undergraduate Research.

2016 2016, February. "Crafting a Persuasive Presentation." Developed and presented on behalf of the Office of Undergraduate Research.

EXTERNAL FUNDING

2018 Pacific Sociological Association (PSA) Student Funding for attendance, membership, and registration to the 89th Annual Meetings/Conference, Long Beach, CA ($150).

2017 Southwest Airlines Student Travel Award, UNLV Foundation. Roundtrip airline voucher to the Stardom, Celebrity, and Fandom Conference, Fort Worth, TX (*Competitive* - $320 value).

INTERNAL FUNDING

2011-Present Internal Graduate Funding: $44,460 Total

2018	Summer 2018 Doctoral Research Fellowship, *UNLV* (*Competitive* - $7000)
2018	Patricia Sastaunik Scholarship, *UNLV* ($2500)
2017	Summer 2017 Doctoral Research Fellowship, *UNLV* (*Competitive* - $7000)
2017	Patricia Sastaunik Scholarship, *UNLV* ($2500)
2017	UNLV Department of Sociology Travel Funding. Conference travel to the Stardom, Celebrity, and Fandom Conference, Fort Worth, TX ($200)
2017	UNLV Graduate and Professional Students Association (GPSA) Travel Funding. Conference travel to the Stardom, Celebrity, and Fandom Conference, Fort Worth, TX ($310)
2017	Liberal Arts Ron Smith and Susan Thompson Scholarship, *UNLV* (*Competitive* - $490)
2014	Liberal Arts Ph.D. Student Summer Faculty Research Award, *UNLV* (*Competitive* - $3000)
2014	UNLV Graduate and Professional Students Association (GPSA) Travel Funding. Conference travel to the Rhetoric Society of America Biennial Conference, San Antonio, Texas. ($360)
2013	Graduate Access Recruitment Scholarship, *UNLV* ($1500)
2012	Alumni Association Graduate Scholarship, *UNLV* ($1500)
2012	Patricia Sastaunik Scholarship, *UNLV* ($2500)
2012	UNLV Graduate and Professional Students Association (GPSA) Travel Funding. Conference travel to Convention of the National Communication Association in Orlando, FL ($650)
2012	UNLV Department of Communication Studies Travel Funding. Conference travel to Convention of the National Communication Association in Orlando, FL ($500)
2012	UNLV Greenspun College of Urban Affairs, Dean's Associates Travel Funding. Conference travel to Convention of the National Communication Association in Orlando, FL ($300)
2012	UNLV Department of Communication Studies Travel Funding. Conference travel to Annual Meeting of the Organization for the Study of Communication, Language, and Gender in Tacoma, WA ($500)
2012	UNLV Graduate and Professional Students Association (GPSA) Travel Funding. Conference travel to Annual Meeting of the Organization for the Study of Communication, Language, and Gender in Tacoma, WA ($650)
2011-2019	UNLV Access Grant – Graduate ($12,000 Total)
2011	Summer Session Scholarship, *UNLV* ($1000)

HONORS AND AWARDS

2016-2019	UNLV Alliance of Professionals of African Heritage Outstanding Academic Achievement Award Recipient (Spring 2016, Spring 2017, Spring 2018, Spring 2019)
2018	Outstanding PhD Student, Department of Sociology, *UNLV*

2016	*Marriage & Family Review* Featured Article of 2016
2015	Honorable Mention, UNLV Graduate and Professional Student Association (GPSA) Research Forum
2013	Honorable Mention, UNLV Graduate and Professional Student Association (GPSA) Research Forum
2012	Induction into Lambda Pi Eta Communication Studies Honor Society, UNLV Chapter
2009-2010	UNLV Alliance of Professionals of African Heritage Outstanding Academic Achievement Award Recipient (Spring 2009, Spring 2010)
2008-2011	UNLV Greenspun College of Urban Affairs Dean's List (Spring 2011, Fall 2010, Spring 2010, Fall 2009, Spring 2009, Fall 2008)

ACADEMIC POSITIONS HELD

2013-Present	Adjunct Instructor, Department of Communication Studies, *College of Southern Nevada*
2016-2018	Graduate Assistant – OUR Communications Specialist, Office of Undergraduate Research, *University of Nevada, Las Vegas*
2014-2016	Graduate Assistantship as Instructor, Department of Sociology, *University of Nevada, Las Vegas* SOC 101: Principles of Sociology
2013-2015	Graduate Teaching Assistant, Department of Sociology, *University of Nevada, Las Vegas* SOC 415: World Population Problems SOC 441: Social Inequality SOC 421: Classical Sociological Theory
2011-2013	Research Assistant, Department of Communication Studies, *University of Nevada, Las Vegas*
2011- 2013	Graduate Assistantship as Lab Instructor, Department of Communication Studies, *University of Nevada, Las Vegas* COM 101: Fundamentals of Oral Communication

SERVICE

Community

2018	Volunteer, Color of Change #ServeOurSisters Event

Field (Sociology/Communications)

2018	Lead Volunteer, Pacific Sociological Association's 89th Annual Meetings/Conference

University

2017	Invited Lightning Talk Speaker, CCSD to UNLV Research Spotlight Event
2016	Member, UNLV Graduate College Search Committee
2016	Representative, UNLV College of Liberal Arts, Graduate & Professional Student Association (GPSA)
2016	Founding Member, UNLV Office of Undergraduate Research

2013-2015 Member, UNLV Student Conduct Hearing Board

Department

2017 Member, UNLV CSUN Scholarships Committee
2017 Coordinator, UNLV Spring 2017 Undergraduate Research Forum
2017 Volunteer, UNLV Graduate Research Forum
2016 Member, UNLV Office of Undergraduate Research Search Committee
2016 Member, UNLV CSUN Undergraduate Research Stipend Committee
2015-2017 Member, C. Wright Mills Sociology Club

ACADEMIC AND PROFESSIONAL AFFILIATIONS

2018 Pacific Sociological Association (PSA)
2018 Western Society of Criminology (WSC)
2012 Lambda Pi Eta Communication Studies Honor Society, UNLV Chapter
2008 Golden Key International Honour Society
2008 National Society of Collegiate Scholars (NSCS)