THE TYRANNY OF QUANTITY: HOW THE OVEREMPHASIS ON DRUG QUANTITY IN FEDERAL DRUG SENTENCING LEADS TO DISPARATE AND ANOMALOUS SENTENCING OUTCOMES

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Submitted to the Graduate Faculty of

Graduate School of Public and International Affairs in partial fulfillment

of the requirements for the degree of Doctor of Philosophy

University of Pittsburgh

2006

UNIVERSITY OF PITTSBURGH

GRADUATE SCHOOL OF PUBLIC AND INTERNATIONAL AFFAIRS

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June 30, 2006

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This dissertation examined evidence for disparate and anomalous drug sentencing outcomes arising from an overemphasis on drug quantity in the federal sentencing guidelines and mandatory minimums. Data from the *1997 Survey of Inmates in Federal Correctional Facilities* were used to investigate the appropriateness of drug quantity as a measure of offense seriousness, the determinants of sentence length, mandatory minimum sentencing outcomes, and the application of firearm sentence enhancements in drug cases. The multivariate analyses employed a range of sentencing predictors, including measures of drug offense seriousness (e.g., drug type and quantity, role in the offense, firearm use), criminal history, case processing factors (e.g., guilty plea, charge bargaining), and sociodemographic characteristics (e.g., race, gender, citizenship). Methods employed to reduce bias and improve the efficiency of the model estimates included imputation of missing values to deal with item nonresponse, design-based estimation to account for the survey's complex sampling design, and truncated-censored regression to handle limited response on the dependent variable. The main findings revealed that (1) drug quantity—as a measure of harm—is a poor surrogate for culpability- and dangerousness-based offense factors, (2) the overemphasis on quantity

results in excessive uniformity in sentencing and creates pressures for guideline evasion, (3) the current 100-to-1 quantity ratio between crack and powder cocaine fosters anomalous and disparate sentencing outcomes by targeting the least culpable crack cocaine offenders with the harshest sanctions, (4) quantity-driven mandatory minimums interact with the guidelines to create sentencing anomalies that fail to differentiate adequately between offenders of varying culpability and dangerousness, and (5) circumvention of firearm sentence enhancements appears to be driven by equity concerns over already severe drug sentences and case pressures to avoid trial. The clearest and most far-reaching implication of this research is that drug quantity is simply too blunt an instrument to meet the demands of principled sentencing. The major policy implication is that the central, organizing role of drug quantity in federal drug sentencing needs to be replaced with a more balanced approach that more equitably focuses on factors of harm, culpability, and dangerousness in assessing sentencing liability.

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ACKNOWLEDGEMENTS

I would like to thank my committee for helping to make this dissertation a success. As my dissertation advisor, Phyllis Coontz has been invaluable with her encouragement and guidance. Both professionally and personally, her support has helped me along the way through triumphs and tribulations alike, and I thank her for her matchless commitment. The insights, collaborations, and guidance from my other committee members—Bill Dunn, Jon Caulkins, and Lisa Nelson—have also been invaluable in completing this project. My three years serving as Bill's graduate research assistant grounded me in the methods of public policy analysis, and I thank him for his continued commitment while facing new administrative duties and responsibilities. Jon's substantive knowledge and thoughtful feedback have benefited me immensely throughout this process. His guidance and collaboration have contributed greatly to my growth as a scholar, and I thank him for his valued mentorship. Finally, Lisa has been a thoughtful commentator and enthusiastic supporter throughout this project, and I thank her for her keen insights.

This dissertation is dedicated to my wife, Emily. This accomplishment is as much hers as it is mine. Without her, this journey would have been less meaningful and enriching. Emily, your sacrifices and devotion sustained me throughout and, yes, it is now my turn to do the dishes!

Finally, this dissertation is dedicated to the memory of my brother, Keith Joseph Sevigny, 1968-2004. You are missed.

1.0 INTRODUCTION

1.1 UNWARRANTED DISPARITY AND SENTENCING REFORM

For the better part of the twentieth century, criminal justice systems throughout the United States all-but-universally operated under an indeterminate sentencing model. Indeterminate sentences were linked to individualized assessments of an offender's rehabilitative progress. Judges garnered unbridled discretion to impose sentences within broad statutory ranges, and parole boards held ultimate authority over inmate release decisions (Blumstein, Cohen, Martin, and Tonry 1983). By the 1970s, however, the rehabilitative ideal that undergirded indeterminate sentencing for three-quarters of a century had fallen into disrepute. Capped by Martinson's (1974; Lipton, Martinson, and Wilks 1975) influential review of offender treatment outcomes, rehabilitation was deemed ineffective at reducing recidivism—the measure of rehabilitation's success. Despite criticisms this research was flawed, the mantra "nothing works" became the dominant perception of offender rehabilitation programs. Weakened by this loss of justification, liberals attacked indeterminate sentencing on grounds that unguided judicial discretion led to unwarranted disparity and racial discrimination. They claimed that sentences were based more on the ascribed personal characteristics of the offender or the idiosyncrasies of the sentencing judge than on principled notions of justice. Conservatives were likewise critical of indeterminate sentencing, but on the alternative grounds that a lack of accountability on the part of judges and corrections officials

resulted in sentences that too often failed to reflect the gravity of the crime or the seriousness of the offender (Blumstein et al. 1983; Stith and Koh 1993).

In light of these concerns, reformers advocated determinate or structured sentencing systems that would provide principled guidance on imprisonment decisions and terms of incarceration. The first such proposals for sentencing reform embraced a philosophical shift from rehabilitation to a retributive or "just deserts" model of punishment (Twentieth Century Fund 1976; von Hirsch 1976). Central to the just deserts model is the principle of proportionality, which holds that punishment should be commensurate with the gravity of the crime. Rehabilitation, in focusing on the offender and not the offense, failed to safeguard this fundamental tenet of justice. Under the just deserts framework, offense seriousness is determined by the *harms* caused by the offense and the defendant's *culpability* for those harms (von Hirsch 1985). By making sentences presumptive and basing them on a retrospective assessment of offense seriousness, these reformers sought to make criminal punishments more predictable and fair.

At about the same time desert theorists were advocating replacing rehabilitation with a justice model, crime control proponents, responding to rising crime rates of the late 1960s and early 1970s, argued for greater emphasis in sentencing on the utilitarian objectives of incapacitation and deterrence (Becker 1968; Wilson 1975). Viewing rehabilitation as ineffective and soft on crime, these critics called for selective incapacitation of habitual offenders, long determinate sentences, and mandatory minimums (MacKenzie 2001). In short, these "law and order" reformers advocated changes that would make sentences for criminals expressly determinate and uniformly severe to lessen their likelihood of committing future crimes.

With the disenchantment over rehabilitation and indeterminate sentencing coming from a wide political cross-section, sentencing reform was pursued vigorously and garnered an increasingly urgent slate on both state and federal legislative agendas (Bureau of Justice Assistance 1996; Frase 1995; Stith and Koh 1993). California was first to overhaul its criminal justice system with the Uniform Determinate Sentencing Law of 1976, which set legislatively-prescribed fixed sentences for certain offenses and abolished parole. Several other states followed suit in short order (BJA 1996), but perhaps the most influential sentencing reform proposal of this era was framed by Marvin Frankel in his book *Criminal Sentences: Law Without Order* (Frankel 1972). Responding to the "almost wholly unchecked and sweeping powers" of judges to impose "widely unequal sentences…for crimes and criminals not essentially distinguishable from each other," Frankel recommended the creation of an expert, independent "Commission on Sentencing" that would study, formulate, and enact guidelines for sentencing (Frankel 1972: 5, 8). Frankel's proposal has had great appeal, as no less than 18 states and the federal government have implemented some type of guideline system (Reitz 2001).

At the federal level, Judge Frankel's recommendations were championed by Senator Edward Kennedy, who first introduced a sentencing reform bill in 1975 calling for the creation of an independent commission to promulgate federal sentencing guidelines as "the beginning of a concerted legislative effort to deal with sentencing disparity" (quoted in United States Sentencing Commission 1991a: 12). Though initially unsuccessful, Senator Kennedy introduced similar bills in each of the next four Congresses. Eventually, legislative compromise led to the passage of the Sentencing

Reform Act of 1984 (SRA) with its central premise of establishing guidelines for fair and just sentencing (Stith and Koh 1993).

In enacting the SRA, Congress established the United States Sentencing Commission (USSC) and charged it with promulgating guidelines and policies in pursuance of several goals (USSC 2004). First, Congress sought *purpose* and *rationality* in sentencing. Specifically, the SRA directed the Commission to consider the four traditional purposes of punishment—retribution, deterrence, incapacitation, and rehabilitation. Rationality was to be achieved by furthering these four purposes through the establishment of binding, predictable rules of sentencing, which were to be based on relevant offense and offender characteristics while remaining "entirely neutral as to the race, sex, national origin, creed, and socioeconomic status of offenders" (28 U.S.C. 994(d)). In addition, the guidelines would be regularly assessed and modified to reflect new research and advances in knowledge. Second, Congress sought *certainty* and *severity* in sentencing. Certainty, or honesty, was to be achieved by establishing "truth-in-sentencing" through the abolition of parole. Congress also instructed the Commission to increase sentences beyond past practice for certain offenders and offenses to "reflect the fact that, in many cases, current sentences do not accurately reflect the seriousness of the offense" (28 U.S.C. 994(m)). For example, the SRA required "a term of imprisonment at or near the maximum term authorized" for certain repeat violent and drug offenders (28 U.S.C. 994(h)). Third, Congress sought the reduction of unwarranted sentencing disparity. The guidelines were to achieve this by ensuring *uniformity* for similarly situated defendants while also maintaining *proportionality* with respect to legally relevant differences across offenses and offense behaviors.

Of the several goals Congress sought to achieve by enacting the Sentencing Reform Act of 1984, reducing unwarranted sentencing disparity was *the* primary objective (Stith and Cabranes 1998). Indeed, Kenneth Feinberg, special counsel to the Senate Judiciary Committee during the late 1970s and one of the SRA's principal drafters, asserts "the first and foremost goal of the sentencing reform effort was to alleviate the perceived problem of federal criminal sentencing disparity," noting further that "all other considerations were secondary" (Feinberg 1993: 295, 296). There are two primary forms of unwarranted sentencing disparity (Schulhofer 1992a; USSC 1987). The first involves the imposition of different sentences on similar offenders. This is the kind of disparity Judge Frankel (1972) railed against and Congress was most concerned with when it passed the Sentencing Reform Act of 1984 (Blumstein et al. 1983; USSC 1987). Greater uniformity in sentencing reduces this first form of disparity. The second type of unwarranted disparity involves the imposition of similar sentences on different offenders. Congress also recognized this type of disparity in the SRA when it required the guidelines to maintain "sufficient flexibility to permit individualized sentences when warranted by mitigating or aggravating factors not taken into account in the establishment of general sentencing practices" (28 U.S.C. §991(b)(1)(B)). Greater proportionality in sentencing reduces this second form of disparity. Thus, the standard of fair and just sentencing that the SRA sought was "similar treatment for similar offenders and different treatment for different offenders" (USSC 2004: 79).

1.2 STATEMENT OF THE PROBLEM

Almost twenty years after the federal sentencing guidelines went into effect, federal sentencing still stands indicted for failing to provide equitable justice. Though federal sentences have become more transparent, certain, and severe, the goal of reducing unwarranted sentencing disparity—the major impetus for reform—remains an elusive, if partially achieved, objective (Stith and Cabranes 1998; USSC 2004). In promulgating practical sentencing guidelines, the Sentencing Commission struggled mightily to strike an appropriate balance between the goals of uniformity and proportionality and their respective imperatives of "broad, simple categorization and detailed, complex subcategorization" (USSC 2004: 10). In the end, the Sentencing Commission's guidelines may have placed too great an emphasis on uniformity at the expense of proportionality. Ilene Nagel, one of the first United States Sentencing Commissioners, intimated as much in admitting that "while every effort was made to treat like offenders alike, less attention was given in the first set of guidelines [to] insuring the likeness of those grouped together for similar treatment" (Nagel 1990: 934). This failure to adequately address and assure proportionality in sentencing was a major reason Paul Robinson, also one of the first United States Sentencing Commissioners, stood alone in dissent of the guidelines (Robinson 1986). In short, in attempting to reduce disproportionality, the Commission may have overreached in its quest for uniformity by severely restricting the factors that bear on sentencing.

Nowhere has this criticism been levied with more force or regularity than in the area of drug sentencing (Alschuler 1991; Schulhofer 1992a, 1992b). Under the guidelines, drug sentences are based primarily on the type and quantity of drugs involved

in the offense. The emphasis on quantity overshadows other important elements of offense seriousness, such as the culpability or dangerousness of the defendant. This narrow focus has created the problem of "excessive uniformity" in sentencing in which offenders of differing culpability and dangerousness are treated similarly based solely on the amount of drugs involved in the offense (Schulhofer 1992a, 1992b). The problem of disproportionality in drug sentencing in which similar offenders are treated differently has also persisted under the sentencing guidelines (USSC 1991b, 1995). The central problem addressed by this dissertation, then, is that the federal drug sentencing guidelines fail to ensure similar treatment for similar offenders and different treatment for different offenders. The specific mechanisms that give rise to these forms of unwarranted disparity are varied and complex. As detailed in the following section, the problem comprises the following aspects: an overemphasis on drug quantity as a measure of offense seriousness, anomalies formed by the interplay between guideline and mandatory minimum sentencing, and guideline circumvention.

1.2.1 Aspects of the problem

1.2.1.1 Overemphasis on quantity as a measure of offense seriousness Perhaps the single greatest cause of disparity in federal sentencing is that drug quantity drives punishment for drug crimes. Other important factors, such as the offender's degree of culpability or the presence of especially pernicious offense behaviors (e.g., weapon use, use of minors in drug distribution), are relatively underweighted in this calculation (Goodwin 1992; Hofer and Allenbaugh 2003; Schulhofer 1992a; USSC 2004; Wasserman 1995; Weinstein and Bernstein 1994; Young 1990). Drug quantity is often a

poor proxy for these other offense seriousness factors. For instance, a large amount of drugs attributed to a mid-level dealer may be indicative of the defendant's high level of responsibility and trust in a drug organization, central role in distributing that drug, or large earning potential. This connection does not hold for couriers and other low-level participants who, though they may be in possession of a large quantity of drugs, have limited responsibility or stake in the conspiracy (Goodwin 1992; Young 1990). As Schulhofer and Nagel (1997: 1307) describe it, "often, drug quantity is an adequate surrogate for culpability, especially when the principal dealer is involved. But quantity of drugs becomes only a crude or entirely misleading surrogate when applied to incidental participants or very low-level subordinates in a conspiracy." The consequent lumping of organizers and bit part players into the same sentencing bin based on a single, overriding factor—drug quantity—can lead to similar sentences for dissimilar offenders.

Another major criticism is that the rigidity and heavy influence of quantity-driven guidelines impede the ability of judges to adjust sentences to reflect a defendant's lower level of culpability. Not only is the application of mitigating sentence adjustments constrained by the guidelines and relevant case law, their impact on sentencing outcomes is rather limited when they are applied (Tobin 1999; USSC 2004). For instance, drug quantity alone can effect a sentence anywhere from probation to life in prison, whereas a minor role adjustment can reduce the base sentence about 25% (Hofer and Allenbaugh 2003). Thus, the upside sentencing potential of quantity-driven punishment is hardly counterbalanced by downside mitigating role adjustments (Schulhofer 1992a, 1992b). In short, critics contend drug quantity simply has too much influence in sentencing outcomes.

Many argue that the central role of drug quantity in determining offense seriousness also gives rise to unprincipled and incoherent sentencing rules. For example, the guidelines base punishment for drug offenses on the "entire weight of any mixture or substance containing a detectable amount of a controlled substance" (United States Sentencing Guidelines [USSG] 2D1.1(c)(A)). Consequently, determining the weight of drugs when they are combined with nonconsumable substances, applied to carrier mediums of differing weight (as with LSD), or mixed in a solution of water has led to widely varying interpretations in federal courts of how to determine the appropriate quantity for sentencing purposes (Berman 1994; Geiger 1998; Gonyer 1998; Meier 1993; Perry 1998; Quivey 1993; Tafe 1994). Similar anomalies have occurred when determining the weight of unharvested marijuana plants or marijuana that has substantial moisture content (Balding 1999; Quivey 1993; USSC 1992a). Failure to consider purity for sentencing purposes also precludes the ability to distinguish potentially more culpable offenders connected to the source from those closer to the street (Meierhoefer 1999). Thus, using the total weight of mixtures and not the pure weight of the controlled substance leads to disproportionate punishment for lower-level offenders who are more likely to dilute the drugs they sell (Meierhoefer 1999; Quivey 1993; Tafe 1994; Vincent and Hofer 1994; Young 1990). In short, these commentators argue that the guidelines' rule of determining sentences based on the entire quantity, irrespective of the purity or usability of the substance or how it is weighed, can lead to disparate outcomes.

Easily, the most notorious quantity-based sentencing provision is the 100-to-1 crack-powder cocaine quantity ratio (Blumstein 2003). The guidelines, for instance, provide a sentencing range of 63 to 78 months for trafficking 500 grams of powder

cocaine, but it takes only five grams of crack cocaine—the weight of two pennies—to reach this same sentencing level. The apparent effect has been to sweep lower-level operators in the crack trade, the majority who are black, into punishment ranges reserved for high- to mid-level powder cocaine dealers (Angeli 1997; Blumstein 2003; Spade 1996; USSC 1995, 2002).

Critics maintain another way quantity-driven sentencing produces disparate outcomes occurs through the application of the relevant conduct standard. The relevant conduct standard exposes all participants of jointly undertaken criminal activity to sentences based on the amount of drugs attributed to their "foreseeable level of involvement," irrespective of any particular member's low-level or mitigating role in the conspiracy (Goodwin 1995; Schulhofer 1992a). Thus, depending upon the court's findings of foreseeability, coconspirators of widely differing culpability can garner similar, and thus disparate, sentences (Meierhoefer 1999). Such broadly sweeping conspiracy laws in particular impact female coconspirators because women typically play supporting roles in conspiracies or become involved solely on the basis of their dependent relationships with men (Gaskins 2004; Huling 1995; Jackson 2003; Nagel and Johnson 1994; Raeder 1993; Tinto 2001). Furthermore, in what has been labeled the "cooperation paradox," this type of disparity can be exacerbated when the leaders and organizers of the conspiracy provide substantial assistance in the prosecution of other coconspirators in exchange for more lenient sentencing, a benefit minor participants are unable to take advantage of because they have no valuable information to trade with the prosecutor (Hrvatin 2002; Maxfield and Kramer 1998; Simons 2002).

1.2.1.2 Interaction between the guidelines and mandatory minimums Not soon after passing the Sentencing Reform Act of 1984 and before the Sentencing Commission could issue its first set of guidelines, Congress enacted the Anti-Drug Abuse Act of 1986. This act created five- and ten-year mandatory minimum sentences for trafficking in certain quantities of drugs, and since they are statutorily-based the minimums always supersede or trump the guidelines. Thus, to preserve some semblance of proportionality, the Sentencing Commission pegged guideline sentences to these anchor points and then extrapolated quantities for sentencing purposes upwards, downwards, and in between (Feinberg 1993; Hofer 2001; Scotkin 1990). Despite this attempt to meld the sentencing guidelines with the mandatory minimums, they form a bifurcated sentencing system that is often at cross-purposes (Schwarzer 1992). Indeed, observers of guideline and mandatory minimum sentencing maintain the interplay between the two systems has created new sentencing anomalies and wreaked havoc with proportional sentencing (Lowenthal 1993; Lutjen 1996; Oliss 1995; Schulhofer 1993). Whereas the guidelines allow for *some* sentence mitigation on the basis of a defendant's degree of culpability or dangerousness, mandatory minimum drug sentences are based almost entirely on the quantity of drugs involved in the offense (with additional consideration of the defendant's prior record).

Since mandatory minimums always trump the guidelines, the single focus on drug quantity produces a "tariff effect" that militates against just outcomes by "short-circuit[ing] the guideline's design of implementing sentences that seek to be proportional to the defendant's level of culpability" (USSC 1991b: 28). Thus, for example, whether a defendant was a bit part player or the leader of a drug ring has no bearing on the final

sentence in mandatory minimum cases. A related anomaly is the "cliff effect" which occurs when quantity-driven mandatory minimums "produce large sentence differentials that override the guidelines approach of providing incremental increases in punishment" (USSC 1991b: n31). To the extent that small variations in drug quantity differentially expose otherwise similar offenders to mandatory minimum sentencing, additional disparities will be introduced. Thus, in perhaps the most acute example, a first-time offender convicted of simple possession of 4.9 grams of crack cocaine would be subject to a *maximum* guideline sentence of one year whereas a similarly situated offender possessing 5.1 grams would be subject to a mandatory *minimum* sentence of five years.

1.2.1.3 Guideline circumvention A fairly ubiquitous concern in federal sentencing is that disparity can be exacerbated as a consequence of "hidden" guideline circumvention or evasion. Guideline circumvention operates mainly through prosecutorial discretion, plea and charge bargaining practices, and judicial acquiescence to overriding equity concerns. In contrast to overt departures for factors not ordinarily taken into account by the guidelines and which must be stated for the record by judges at sentencing, guideline circumvention occurs covertly and lacks accountability (Schulhofer and Nagel 1997). In drug cases, common mechanisms of guideline circumvention include charging offenses that carry less sentencing exposure, holding the defendant accountable for or alleging reduced drug quantities, not pursuing or granting culpability-based adjustments when warranted, and not charging or imposing applicable firearm sentence enhancements (Nagel and Schulhofer 1992). Oftentimes, such evasions are pursued (or at least not rejected) by courtroom actors for the purpose of achieving local norms of justice. For

example, prosecutors may decide not to pursue relevant firearm sentence enhancements or judges may overlook the facts of the case for certain drug defendants whose sentences are deemed sufficiently severe on the basis of drug quantity alone (Hofer, Blackwell, Burchfield, and Stevens-Panzer 2000; USSC 1991a). From a national perspective, regional inconsistencies in the frequency and nature of guideline circumvention can lead to unwarranted sentencing disparity (Berman 2000). In short, to the extent that substantively different offenders end up being grouped together because of hidden departures, disparity is reintroduced into the guidelines system (Nagel 1990).

1.3 PURPOSE AND OBJECTIVES OF THE STUDY

The federal prison system now houses more inmates than any single state, having recently passed Texas and California for that distinction. Sentencing reform has dramatically increased both the number of drug offenders sentenced to prison and their average time served (Austin, Bruce, Carroll, McCall, and Richards; Bureau of Justice Statistics 1988; Hofer and Semisch 1999; McDonald and Carlson 1992; Scalia 2001). Between 1984 and 2004, the number of convicted drug offenders sentenced to a term of imprisonment increased from 77% to 93%, and the average sentence imposed increased from 66 to 83 months (Maguire and Pastore 2006). The increased severity in drug sentencing, together with an overall rise in the number of drug offenders arrested and prosecuted for federal drug crimes, accounted for the majority of the growth in the federal prison population (Blumstein and Beck 1999; Meierhoefer 1999; Simon 1993). Notably, between 1984 and 2004, the proportion of drug offenders in federal prison climbed from 30% to 54%, peaking at 61% in 1994 (Maguire and Pastore 2006). Given

this growth in the federal drug offender prison population, the scope of the problem is considerable. As noted above, this study is motivated by ongoing evidence of unwarranted disparity in federal drug sentencing. Using data from the *Survey of Inmates in Federal Correctional Facilities, 1997* (Bureau of Justice Statistics and Federal Bureau of Prisons 2000a), the purpose of this study is to assess the nature and extent of this unwarranted disparity. In particular, this dissertation sets forth six specific research questions:

1. How well does drug quantity serve as a proxy for other elements of offense seriousness, such as the culpability of the offender and the dangerousness of the instant offense conduct?

2. How much influence does drug quantity have on sanctioning severity compared to other elements of offense seriousness?

3. To what extent and in what ways does the 100-to-1 crack-powder quantity ratio disadvantage crack cocaine offenders?

4. Is there evidence of the tariff effect in the interaction between guideline and mandatory minimum sentencing?

5. Is there evidence of the cliff effect in the interaction between guideline and mandatory minimum sentencing?

6. What is the nature and extent of circumvention of firearm sentence enhancements in drug sentencing outcomes?

The empirical and legal evidence addressing each of these issues is examined in the following section.

2.0 LITERATURE REVIEW

This literature review contains three sections. Section 2.1 describes the development and operation of the sentencing guidelines for drug offenses. Section 2.2 frames the theoretical perspective backing the federal drug sentencing guidelines. Finally, Section 2.3 reviews the empirical literature in relation to the dissertation's six research objectives.

2.1 DEVELOPMENT AND OPERATION OF THE DRUG GUIDELINES

As authorized under the Sentencing Reform Act of 1984, the staff and commissioners of the United States Sentencing Commission convened in October 1985 to begin work on structuring federal sentencing. This undertaking culminated in the promulgation of the federal sentencing guidelines on November 1, 1987.[1] The sentencing guidelines evolved through much compromise (Breyer 1988; Nagel 1990; USSC 1987; Wilkins 1988), but the development of the drug guideline in particular was influenced by two key events that caused drug sentences to depart considerably from past practice (Bowman 1996; Hofer 2001; Scotkin 1990). First, Congressional directives in the Sentencing Reform Act of 1984 instructed the Commission to consider the fact that past sentencing practices did not reflect the seriousness of certain offenses and offenders. Recidivist drug traffickers, for

[1] In many federal districts, constitutional challenges delayed full implementation of the guidelines. These issues were ultimately resolved in *Mistretta v. United States*, in which the Supreme Court upheld the constitutionality of the Sentencing Commission and the Sentencing Reform Act (United States Sentencing Commission 1991a). The date of this decision—January 18, 1989—marks the effective date of the sentencing guidelines in *all* federal districts.

example, were singled out for especially severe punishment in the SRA. Second, and more significantly, before the Sentencing Commission had finished developing the drug guideline, Congress passed the Anti-Drug Abuse Act of 1986.[2] This law tied mandatory minimum five- and ten-year sentences for drug trafficking offenses to specific quantities of controlled substances. The statutory minimums effectively constrained the Commission's options, and it was forced to incorporate the quantity-based rationale into the drug offense guidelines. In effect, as Bowman (1996: 692) describes it, "mandatory minimums were a set of fixed, immovable points around which the Guidelines architects were obliged to design their edifice." In order to maintain proportionality between different offenders, the Commission extrapolated drug quantities above, below, and in between the mandatory minimum anchor points (Hofer 2001; Scotkin 1990). The Commission then tied these quantities to 17 base offense levels, beginning with level 6 and increasing in two-level increments to level 38 (higher offense levels are possible with aggravating circumstances). When the mandatory minimum sentence was applicable, consideration of other offense seriousness factors was severely constrained because the applicable mandatory minimum "trumped" the guideline sentence. In short, the drug mandatory minimums Congress enacted two years after the SRA influenced the development of the drug sentencing guidelines, and continue to affect their operation (Wilkins, Newton, and Steer 1993).

The key to understanding the guidelines is its sentencing grid, formed on one axis by 43 offense levels and on the other axis by six criminal history categories. This

[2] The Anti-Drug Abuse Act of 1988 continued this drive toward mandatory minimum sentencing by making conspiracies punishable similarly to the substantive offense, and by extending mandatory minimum sentencing to simple possession of crack cocaine.

structure takes the form of a 258 box grid, each box stipulating a presumptive sentencing range for that particular combination of offense seriousness level and criminal history category. The sentences within the grid range between probation and life imprisonment (USSC 2004). All federal offenses are governed by specific rules and policy statements that place offenders within one of these presumptive sentencing ranges. In arriving at the appropriate guideline sentence for drug offenders, the court follows several steps. First, the judge determines the statute of conviction and the applicable guideline. For most federal drug offenses, this would lead to the United States Sentencing Guideline (USSG) §2D1.1. Second, the judge determines the type and amounts of drugs involved in the offense, which determines the *base offense level*. Third, the judge adjusts the sentence for *specific offense characteristics* associated with the offense guideline. For example, possession of a firearm during a drug trafficking crime calls for a two-level increase in the base offense level. Fourth, the judge adjusts the sentence for *generic offense characteristics* applicable to all offenses. This includes, for example, the two- to four-level adjustment for a defendant's aggravating or mitigating role in the offense. The *final offense level* is obtained after all such adjustments are made. Fifth, the judge determines the offender's *criminal history category*. Finally, the judge looks up the sentence range in the *sentencing table* that occurs at the cross-section of the final offense level and criminal history category.

2.2 THEORETICAL PERSPECTIVE

As discussed in the Chapter 1, Congress sought federal sentencing guidelines that would further the four basic purposes of criminal punishment: just deserts, deterrence,

incapacitation, and rehabilitation. Congress directed the Sentencing Commission to balance these four purposes in crafting a principled sentencing system. However, the Commission never expressly justified the guidelines with a clearly stated philosophy of punishment, calling the issue largely "academic." Some have described the Commission's failure to articulate a clearly stated philosophical basis for the guidelines as one of its greatest blunders (Feinberg 1993). In order to evaluate whether the guidelines sentence appropriately and reduce unwarranted sentencing disparity, the purposes of the guidelines need to be articulated (Lynch 2003; von Hirsch 1987). As Hofer and Allenbaugh (2003: 37) put the question, "Should we judge the guidelines largely by how successfully they punish proportionately, how well they target the most dangerous offenders for incapacitation, or how successfully they provide offenders with the training or treatment they need?"

Without an official Sentencing Commission declaration on the purposes of punishment, observers have attempted to construct the theory behind the guidelines by focusing on the guideline rules themselves, justifications for subsequent amendments, and other Sentencing Commission policy statements (Hofer and Allenbaugh 2003; Rappaport 2003). Paul Hofer, both independently and through his work at the Sentencing Commission, argues that the underlying philosophy of the guidelines is a hybrid theory of punishment that blends the goals of just deserts and incapacitation (Hofer and Allenbaugh 2003; USSC 2004). Labeled "modified just deserts," this approach places primary emphasis on just punishment (i.e., punishment should be proportionate to the gravity of the offense) and secondary emphasis on incapacitation (i.e., punishment should be extended for dangerous and repeat offenders). In short, the modified just deserts

perspective holds that the sentencing guidelines as a whole "are best explained by their primary emphasis on proportionate punishment and a secondary concern with the incapacitation of higher-risk offenders" (Hofer and Allenbaugh 2003: 52).

A few commentators have suggested that a coherent sentencing philosophy may be more attainable if sentencing purposes are defined for specific groups of offenders rather than for the sentencing system as a whole (Miller 1992). This issue was raised in Congress prior to passage of the SRA. For example, a Senate Judiciary report from 1980 noted that "different purposes may play greater or lesser roles in sentencing for different types of offenses committed by different types of offenders" (quoted in Feinberg 1993: 300). Thus, the question of whether this broad framework is an equally valid philosophical justification for federal drug sentencing is an important one. However, punishment theory for specific crimes is relatively underdeveloped, and particularly so for drug offenses (Husak 1998). Indeed, as Hofer (2001: 1, 6) has emphasized

> We have no theory of punishment for drug trafficking…Without a theory explaining why the rules treat different drug traffickers differently, sentencing guidelines risk becoming an exercise in drawing "distinctions without a difference." Without a theory, we cannot assess whether the guidelines have eliminated *unwarranted* disparity....the very concept of unwarranted disparity assumes a background theory that identifies which characteristics of offenders are relevant to sentencing and which may be ignored.

In light of this void, the following section frames a theoretical perspective of the federal sentencing guidelines for drug offenses, drawing heavily on the work of Paul Hofer (2001; Hofer and Allenbaugh 2003; USSC 2004), Andrew von Hirsch (1979, 1985, 1993), and Arnold Loewy (1988).

2.2.1 The theoretical rationale behind the federal drug sentencing guidelines

In their "harm-based version of modified just deserts," Hofer and Allenbaugh (2003) present a well-argued perspective on the philosophical rationale that sits behind the federal drug sentencing guidelines. Put simply, *harm-based modified just deserts* implies that punishment is scaled foremost to the harmfulness of the offense conduct, subsequently adjusted to reflect the offender's need for incapacitation based on prior record. This section describes and further develops the components of this theory as they relate to federal drug sentencing.

Desert theory is concerned with fair punishment. Paramount to determining the appropriate quantum of punishment is the principle of proportionality, which requires that sanctioning severity be commensurate with the gravity of the offense (von Hirsch 1985, 1998). This basic structure is implicit in all criminal codes that rank offenses along a continuum from petty misdemeanors to aggravated felonies. Thus, the requirements of proportionality dictate that defendants convicted of similar conduct receive sanctions of comparable severity and defendants convicted of dissimilar conduct receive sanctions correspondingly different (von Hirsch 1998). This fundamental premise of desert theory—that punishment be proportionate to the seriousness of the offense—stands as the central organizing principle of the federal sentencing guidelines (USSC 2004).

The idea of commensurate proportionality is fairly straightforward: as the seriousness of the offense increases, so goes the punishment. Key to this understanding are the concepts of *punishment* and *seriousness*. In the first instance, federal punishment comprises two basic options for the most part: probation and incarceration. Thus, it is a simple stipulation that incarceration is more burdensome than probation, and longer

sentences are more onerous than shorter ones. As for the second concept, von Hirsch (1979, 1985, 1992, 1993, 1998) defines crime seriousness as involving two key factors: harm and culpability. Harm refers to the degree of injury caused or risked by an offender's actions. In assessing harmfulness, von Hirsch (1976: 80, emphasis added) stresses that "the emphasis should be on the harm *characteristically* done or risked by an offense of that kind." That is, a crime's seriousness should be based on its typical harmfulness. Although the taxonomy of drug-related harms is quite broad (MacCoun and Reuter 2001), the harms typically associated with drug offenses relate to the use and abuse of illicit substances. Certain drugs cause greater and more types of harm, and a greater amount of any drug is potentially more harmful. Thus, "drug type and quantity are reasonable first measures of the harm for which a drug trafficker should be held accountable" (USSC 2004: 49-50). Culpability, von Hirsch's second criterion, refers to an offender's degree of blameworthiness for any harm or wrongdoing. Thus, the extent to which a defendant can be justly held accountable for harmful conduct hinges on his or her level of intent, involvement, or importance in bringing about that harm. Under the sentencing guidelines, a defendant's degree of culpability is determined by the role played in the offense, whether any special skills were brought to bear, and whether the defendant was a leader or organizer of a drug trafficking network. In short, a crime's gravity can be expressed by the following formula: *offense seriousness = harmfulness of the offense conduct x culpability of the offender* (von Hirsch 1985).

The "harm x culpability" framework for drug offenses is too simplistic, however, and in the judgment of some observers requires further theoretical development (Hofer and Allenbaugh 2003). The formula simply does not account for everything the drug

guidelines consider in determining offense seriousness. Arnold Loewy (1988) provides a framework of criminal liability that can inform a more fully developed perspective of the drug sentencing guidelines. In particular, he highlights a third dimension of offense seriousness—dangerousness—that, when considered with harm and culpability, provides a more complete characterization of how the guidelines assess drug offense seriousness. The concept of dangerousness is often encompassed within the concept of harm, but Loewy (1988) argues for its separate consideration. Dangerousness has different meanings in the literature (see e.g., Moore, Estrich, McGillis, and Spelman 1984), but here it is taken to refer to conspicuously reprehensible offense conduct that causes or risks serious harm.

To more fully distinguish between harm and dangerousness for purposes of sentencing, it helps to refer back to von Hirsch's emphasis on *characteristic* harms, that is, those harms typically associated with a particular offense. Again, for drug offenses, this is tied to the type and quantity of drugs. By way of contrast, "uncharacteristic" harms could include those facets of criminal conduct that are not integral to the underlying criminal offense, but which nevertheless aggravate the seriousness of the offense in a material way (e.g., firearms use, using minors to distribute drugs). These are the types of harms captured by the term dangerousness. In practice, the drug guidelines already account for dangerous behaviors above and beyond the typical harms tied to drug type and quantity, and this theoretical elaboration only makes explicit what is already inherent in the drug guideline's operation. For example, the drug guidelines increase drug offense seriousness for the following uncharacteristic harms: serious bodily injury, the discharge of toxic substances into the environment, abuse of a position of trust, operating a

continuing criminal enterprise, selling near protected locations or to underage or pregnant individuals, and using weapons. Thus, drug offense seriousness as measured by the drug guidelines is best depicted by the following formula: *offense seriousness = harmfulness of the offense x dangerousness of offense-related conduct x culpability of the offender*. The most serious drug offenses, then, would involve trafficking in large amounts of drugs, conducted in especially reprehensible fashion and with a high degree of culpability on the part of the defendant.

These three factors—harm, culpability, and dangerousness—do not receive equal weight in the guideline's calculation of drug offense seriousness, however. Offense seriousness is determined first and foremost by the harmfulness of the offense, that is, by the type and quantity of drugs involved. Under the drug guideline, the dangerousness of the instant offense conduct and the culpability of the offender aggravate or mitigate the base offense level only secondarily. Because drug type and weight are the dominant indicators of drug offense seriousness, the theoretical framework is most appropriately characterized as *harm-based* just deserts.

The final component pertains to how the calculus of offense seriousness is modified by resort to an incapacitation rationale. Specifically, in the guidelines, an offender's criminal history category operates on one axis in conjunction with the offense seriousness level on the other axis to pinpoint an appropriate punishment range. Thus, all things equal, defendants with more serious and extensive prior records will be subject to increased sentencing exposure. The guidelines also single out recidivist drug trafficking and violent offenders for particularly severe sentence increases. Thus, the core logic underlying the guideline's criminal history provision is one of crime control (Hofer and

Allenbaugh 2003). Together, these elements constitute the *harm-based modified just deserts* theoretical perspective thought to best characterize rationale behind the federal drug sentencing guidelines.

2.3 REVIEW OF THE EMPIRICAL AND LEGAL EVIDENCE

Relatively little empirical research has been conducted on the specific problems raised by this research. Much of the available evidence derives from descriptive analysis, legal commentary, and anecdotal evidence. This body of literature, together with the available empirical research, is used to identify the bounds of the issues this dissertation addresses and to form specific research hypotheses. The subsections that follow correspond to the six objectives of this dissertation specified in Section 1.3.

2.3.1 Drug quantity as proxy

When Congress passed the Anti-Drug Abuse Act of 1986, it tied the five- and ten-year mandatory minimum drug sentences to specific types and quantities of drugs. As suggested by the legislative history, Congress intended the five-year penalty to apply to "serious" traffickers (i.e., the "middle-level dealers" and "managers of the retail level traffic") and the ten-year penalty to "major" traffickers (i.e., the "kingpins," "masterminds," and "heads of organizations") (Hofer 2001; USSC 1995). The Sentencing Commission indicates that, in enacting quantity-driven mandatory minimums, "Congress had in mind a tough penalty scheme under which, to an extent, drug quantity would serve as a proxy to identify those traffickers of greatest concern" (USSC 1995: 118). The Sentencing Commission extended this drug-quantity-as-proxy rationale to the guidelines

when it incorporated the quantity-based mandatory minimum anchor points into the guidelines' architecture (Scotkin 1990). Citing evidence from case law, many observers of federal drug sentencing contend that drug quantity—as a measure of harm—is often an inadequate surrogate for other elements of offense seriousness, such as the culpability or dangerousness of the offender (Goodwin 1992; Schulhofer 1992a; Wasserman 1995; Weinstein and Bernstein 1994; Young 1990).

Few studies have directly examined the ability of drug quantity to serve as a proxy for other elements of offense seriousness, however, so the empirical evidence on this matter is rather limited. The best available evidence indicates drug quantity is positively correlated with indicators of culpability and dangerousness, but just how strong these associations are remains unclear. Two recent studies suggest a strong albeit imperfect relationship between quantity and culpability. The Sentencing Commission's *2002 Cocaine Report* concludes that for both powder and crack cocaine offenders sentenced during 1995 and 2000, "the most culpable offenders…generally were held accountable for greater drug quantities than lower level offenders" (USSC 2002: 45). The notable exception was that low-level couriers, mules, and drug handlers were responsible for greater quantities than offenders operating at higher-levels (e.g., managers, wholesalers, pilots, chemists). Sevigny and Caulkins (2004) likewise found that larger drug quantities were generally associated with higher levels of culpability among federal drug inmates incarcerated in 1997, but again with important exceptions. For example, their analysis shows that the largest median drug quantities were associated with importers and money launderers, and the smallest quantities were associated with retailers and user/possessors. Yet, manufacturer/growers possessed drug amounts more in

line with user/possessors and wholesale dealers possessed mid-range drug amounts similar to peripheral role participants (e.g., go-betweens, couriers, loaders).

In contrast, a Department of Justice (1994) study of federal drug inmates sentenced in 1992 found that drug amounts were similarly distributed across different roles in the offense (i.e., high-level dealer, street-level dealer, courier, peripheral role), showing drug quantity to be a poor indicator of an offender's level of culpability. The DOJ (1994) study suffers from an important bias, however, because the sample was originally selected to exclude "sophisticated" drug criminals and those who received an aggravating role adjustment at sentencing. Thus, the very nature of the sample confounds the quantity-culpability relationship. In short, DOJ's (1994) results are not representative of drug offenders with higher degrees of culpability. Including such offenders would likely alter the nature of the observed relationship. On balance, then, the evidence indicates that larger drug amounts are generally associated with higher levels of culpability, but also that this association is imperfect.

With respect to offender dangerousness, there is strong evidence of a connection between drug distribution and weapons, especially with the advent of crack cocaine in the 1980s (Blumstein 1995; Blumstein, Rivara, and Rosenfeld 2000; Johnson, Golub, and Dunlap 2000; MacCoun, Kilmer, and Reuter 2003). In the business of drug dealing where there is no legal recourse, guns are deemed necessary to settle territorial disputes, protect against robbery, ensure payback of drug-related debts, and keep subordinates in line. While the drug-gun connection is well-established, there is comparatively limited empirical evidence indicating that trafficking in greater drug quantities is related to higher levels of gun activity. The only study to examine such a direct association was

conducted by Lizzotte, Krohn, Howell, Tobin, and Howard (2000), who found that "high quantity" drug selling was associated with greater odds of carrying a gun among a general population sample of urban adolescents. Based on this evidence, it is a plausible supposition that distributors running larger scale operations involving greater amounts of drugs and cash are more likely to possess and use guns. The available evidence supports the following hypothesis.

> H1: Larger drug quantities will be associated with more serious offense characteristics, such as higher-level roles in the offense, leadership and managerial responsibility, and gun use. However, there will be important exceptions for relatively minor traffickers who are held responsible for substantial drug quantities.

2.3.2 The influence of drug quantity on sentence length

It is a pervasive criticism in the literature that drug quantity—as a measure of harm—has undue influence on federal drug sentencing outcomes, creating disparities with culpability- and dangerousness-based offense characteristics, such as a defendant's role in the offense or the use of weapons (Alschuler 1991; Schulhofer 1992a; Young 1990). Many of these claims have been made by observers of the legal system citing drug sentencing jurisprudence and specific examples from case law, yet solid empirical evidence is less ubiquitous.[3] This section reviews the available evidence that speaks to

[3] There is a long line of research addressing federal drug offense sentencing outcomes, especially with respect to racial and gender disparities (e.g., Albonetti 2002b; Bushway and Piehl 2001; Everett and Wojtkiewicz 2002; Koons-Witt 2002; Mitchell 2005). However, most of this research has used the sentencing guideline's final offense level as the main control for offense seriousness. In many studies, especially during the early guidelines era, the underlying measures that determine the final offense level (e.g., drug quantity, role in the offense, weapon use) were not available in the datasets analyzed. Even when these measures became more readily available, many researchers rightly

the relative impact of drug quantity and other offense seriousness factors in federal drug sentencing outcomes. The review focuses on multivariate analyses of federally sentenced drug offenders in which sentence length was regressed on measures of drug quantity and at least one other relevant offense seriousness factor (e.g., role in the offense, weapon use). These studies typically included controls for demographics (e.g., age, race, gender), criminal history (e.g. criminal history category, career offender status), case processing factors (e.g., guilty plea, acceptance of responsibility), sentencing adjustments (e.g., substantial assistance), and, occasionally, region (e.g., circuit, county). The discussion below centers just on the sentencing effects of the relevant drug offense seriousness characteristics (e.g. drug type and quantity, role in the offense, weapon use).[4] Unless

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opted to use the final offense score in their analyses because it parsimoniously controls for a host of legally relevant offense factors and sentencing adjustments that are usually of secondary interest to the investigator (Engen and Gainey 2000). However, these are the factors of prime interest when examining how legally prescribed criteria differentially impact drug sentencing outcomes. In this specific area, then, the commentary of the legal profession has far outpaced the empirical research of the social science community. Nevertheless, a number of multivariate sentencing analyses have been reported in the literature that include the relevant offense seriousness factors with which this dissertation is concerned.

[4] The offense seriousness factors are operationalized in different ways across studies, and this is noted for each study reviewed. A prefatory note on drug quantity is necessary, however. Marijuana equivalencies describe a quantity measure in which the weights for different drugs are converted into a common metric that is consistent with the sentencing guidelines' drug-specific penalty levels. For example, 1 gram of heroin equals 1 kilogram of marijuana and 1 gram of powder cocaine equals 200 grams of marijuana for sentencing purposes. The Sentencing Commission uses marijuana equivalencies to establish commensurate base offense levels when, for example, an individual defendant is sentenced for multiple drugs. For sentencing research purposes, the use of marijuana equivalencies is not only consistent with Sentencing Commission practice, but it represents a parsimonious way to model the effects of drug quantity. Some investigators in the studies reviewed here use marijuana equivalencies. Others use an unadjusted quantity measure, which, if the analysis involves different drug offenses, entails simple summation of quantities without first converting to marijuana equivalents. Still other

noted, interpretation and discussion of relative effects is based on the reported standardized beta coefficients (b^s).

Two studies of pre-guideline drug sentencing show that quantity was an important sentencing factor (Peterson and Hagan 1984; Rhodes 1991). For example, Peterson and Hagan (1984) examined federal drug sentencing outcomes for several periods between 1963 to 1976. In their main model, they included measures for 'big dealer' (identified as either a 'prime mover' or in possession of more than 100 lbs of a drug), 'drug user,' and gun use. While recognizing that 'big dealer' combines quantity and role considerations into a single measure, the results show that 'big dealer' status led to significantly longer sentences during the periods examined. Conversely, being a drug user led to significantly shorter sentences, and gun use had no significant effect on sentence length.

Rhodes (1991) analyzed federal cocaine cases prosecuted between 1984 and 1988. The sample was further limited to "leaders" and "more culpable" defendants, eliminating those who were "less culpable" or "acted alone." Thus, a notable bias exists in the sample. Rhodes included measures for cocaine quantity and continuing criminal behavior (i.e., "multiple recurrent acts that were planned as a long-range scheme") in the model. The main results revealed that both cocaine quantity and more culpable status resulted in longer sentences. Together, the studies by Peterson and Hagan (1984) and Rhodes (1991) provide evidence for the role of quantity and culpability considerations (but not gun use) in federal drug sentencing prior to the guidelines, but the time periods analyzed, selection issues, and variable operationalization all raise questions about the generalizability of these findings to the current sentencing regime.

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investigators use separate quantity measures for each drug. The use of these various constructions is noted in the text.

Two recent studies by Kautt (2001-2002; Kautt and Spohn 2002) show drug quantity and the presence of weapons to have little to no effect, or even a significant negative effect, on sentence length. Inspection of the estimation models used in these studies reveals the outcomes have more to do with model overspecification, however, than any truly negative or invariant drug quantity or weapon effect. For example, Kautt (2001-2002) analyzed sentence length outcomes for federal drug offenders sentenced in FY1999. She ran four analyses stratified by race and gender controlling for drug type (crack, powder, heroin, marijuana, and methamphetamine), quantity (marijuana equivalencies), and receipt of a weapon enhancement, *as well as* measures for guideline offense level, offense level squared, mandatory minimum status, and the statutory mandatory minimum sentence length. Her results indicate drug type was largely inconsequential to sentence length outcomes, and that drug quantity had an insignificant effect for whites but a significant negative effect for blacks, males, and females. The effect of the weapon enhancement was insignificant for all but females, for whom it had a significant negative effect. These counterintuitive findings are easily explained by the highly significant effects of offense level, offense level squared, and the statutory minimum sentence in all four models. These measures are themselves direct functions of the underlying offense characteristics, and this severe overspecification erases or convolutes the independent effects of quantity and weapons use on sentence length.[5]

[5] To be fair, the studies by Kautt (2001-2002) and Kautt and Spohn (2002), as well as most of the other studies reviewed here, were originally designed to investigate different issues than this dissertation. Kautt and Spohn (2002), for example, were interested in uncovering racial disparities in drug sentencing, and they defended as appropriate the model specifications they employed for this particular purpose. More broadly, however, they argue that *not* including both the individual and aggregate offense seriousness measures would result in severe model misspecification. While there is considerable

The study by Kautt and Spohn (2002) examined sentence length outcomes for federal drug offenders sentenced in FY1998, stratified into six subgroup regressions by sentencing regime (i.e., guidelines, "hybrid," and mandatory minimum) and race (i.e., black and white). The analysis also suffers from overspecification, but also from some other hard-to-interpret modeling choices. For example, the authors included six separate quantity measures for different drug types, which makes it nearly impossible to assess the aggregate effect of quantity. In addition to quantity, they included measures for drug type (crack, powder, heroin, marijuana, methamphetamine, and other drugs), the presence of a weapon enhancement, and nominal offense codes (e.g., possession, importing, manufacturing), *as well as* the final offense level and the minimum recommended sentence. These latter measures together account for the largest share of the variance, largely wiping out the effects of the specific offense characteristics. However, manufacturers tended to be sentenced more leniently than importers, and, for two of the six models, possession offenders were sentenced more severely than importers. In explaining this latter finding, the authors point to possible selection effects related to federal prosecution of only the most serious possession offenders.

Bush-Baskette's (2000) study also finds no effect for drug quantity on sentence length. Her main analysis examines FY1996 federal sentencing outcomes for female drug offenders. Relevant offense characteristics included drug type (crack vs. all others), quantity (unadjusted amount), aggravating role adjustment, and a series of nominal

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debate over proper model specification in sentencing research, especially for investigations of racial and gender disparities (Engen and Gainey 2000), it is clear that including the aggregate offense seriousness measures confounds the independent effects of specific sentencing factors—which are the variables of interest in the present research.

offense codes (e.g., importation, distribution/manufacturing, simple possession). Bush-Baskette's model is not overspecified, so it is not clear why drug quantity had no effect. Some of the strongest effects in the model occurred for mandatory minimum status and crack cocaine offending (versus all other drugs), so there could be an interaction or moderating effect with these variables. More likely, it is an issue with the operationalization of the drug quantity measure since marijuana equivalencies were not used to account for the differential sentencing exposure of the various drugs. Other notable results included an aggravating role adjustment that had a small but significant *negative* effect, and simple possession offense status (relative to distribution/manufacturing) that had one of the largest mitigating effects on sentence length.

Pasko (2002) examined federal drug sentences for FY1995. Like Kautt (2001-2002) and Kautt and Spohn (2002), this analysis is also overspecified. Measures are included for drug type (crack, powder, heroin, marijuana, methamphetamine, and other drugs), quantity (nominal weight indicators[6]), aggravating and mitigating role adjustments, and gun possession, *as well as* measures for both the base and final guideline offense levels. Her results indicate that the offense level measures account for the largest share of the variance. In terms of specific offense factors, gun possession had the strongest positive effect on sentence length, followed by aggravating role status. Also,

[6] Pasko (2002) controls not for the quantity of drugs per se, but oddly includes dummy variable indicators for whether the quantity was reported or measured in grams, kilograms, pounds, ounces, or some other metric. Gram weight serves as the reference category. Thus, only in a very rough and artificial way does the analysis control for quantity.

both crack offending and involvement with "larger" quantities significantly predicted longer sentences. Finally, mitigating role status was not significant in the model.

In contrast to the dated or poorly specified studies reviewed above, several analyses have been reported in the literature that provide more tenable assessments of differential effects of relevant offense seriousness factors on federal drug sentencing. For example, a Department of Justice (1994) study of federal drug offenders sentenced in 1992 examined the effects of drug weight (marijuana equivalencies), mitigating role status, and role in the offense (i.e., mid- to high-level dealer, money launderer/manufacturer, street-level dealer, courier, and peripheral role offenders) on sentence length.[7] In this study, drug quantity had by far the greatest effect on sentence length, accounting for about half the variance in the model. Mitigating role status was the next strongest offense seriousness indicator, resulting in shorter sentences as expected. The effects of being either a money launderer/manufacturer or mid- to high-level dealer (relative to peripheral offenders) were nearly as strong in the opposite direction. Street-level dealers and couriers, in contrast, were sentenced no differently than peripheral role offenders.

Using a special Sentencing Commission drug sample dataset, Semisch's (2000) study of federal drug offenders sentenced in 1995 included measures for drug type (crack, powder, heroin, marijuana, and methamphetamine,) quantity (unadjusted quantity), role

[7] Like Rhodes (1991) did with high-level offenders, the DOJ study sample was selected for low-level offenders. These selection issues clearly affect the associations between the observed variables. As Roth (1994: 20) comments, "if the results are intended to present an unbiased comparison of the influences of role in the offense, prior criminal history, and violence with that of drug quantity, a new sample is needed that has not been preselected in terms of the former three variables."

in the offense (operationalized as a 20-level ordinal variable of decreasingly culpable roles), and gun use. Of the three specific offense seriousness factors (i.e., quantity, role, guns), role in the offense had the strongest effect (b^s = -.18) on sentence length. The next strongest effects were for gun use (b^s = .15) and drug quantity (b^s = .11). Thus, drug quantity was the weakest predictor among the three offense seriousness factors. However, as indicated by the relatively strong effects of the individual drug type measures in the model—especially for crack (b^s = .13) and marijuana (b^s = -.22)—Semisch's use of unadjusted drug quantities depresses the quantity measure's direct effect. This interpretation is supported by Semisch's parallel analysis of the Sentencing Commission's main datafile in which the guidelines' base offense level proxies drug quantity. In this alternate model, the base offense level is by far the strongest predictor of sentence length (b^s = .51), followed by role in the offense (b^s = .20),[8] the weapon mandatory minimum (b^s = .18), and the guideline weapon enhancement (b^s = .10).

McDonald and Carlson (1993) analyzed federal crack and powder cocaine sentencing outcomes for 1989-1990. They analyzed each drug separately and included measures for quantity (crack or powder cocaine weight), role in the offense ('less,' 'same,' and 'more' culpable conspirators), participation in a criminal enterprise, firearms activity (none, present, threatened, used), and receipt of a weapon enhancement. Unfortunately, MacDonald and Carlson do not report beta coefficients in their models to aid interpretation of relative effects, so conclusions are drawn from 'unstandardized' percent effect estimates. For the powder cocaine model, threatening with a firearm (40%)

[8] Under this alternate model, the effect for role in the offense is in the opposite direction because Semisch operationalized the variable to increase with greater culpability, whereas she operationalized the role in the offense variable in the initial specification to decrease with greater culpability.

and receiving a firearm sentence enhancement (31%) had the strongest significant positive effects on sentence length, followed by quantity (21%), being a more culpable coconspirator (19%), and participation in an ongoing criminal enterprise (8%). Significant negative effects were noted for being a less culpable coconspirator (-19%) and having no firearm in connection with the offense (-18%). For the crack cocaine model, receiving a firearm enhancement (25%) had the strongest significant positive effect on sentence length, followed by quantity (21%), and being a more culpable coconspirator (12%). Conversely, significant negative effects were found for being a less culpable coconspirator (-12%) and having no firearm in connection with the offense (-12%).

In summary, the best available evidence suggests that drug quantity is the most influential offense-related predictor of federal drug sentences. Role- and weapon-related factors are also influential, impacting sentences to fairly similar degrees. Based on the empirical evidence, legal commentary, and the quantity-based structure of the guidelines and mandatory minimums, the evidence supports the following hypothesis.

> H2: Drug quantity will be the strongest independent predictor of sentence length. Culpability- and dangerousness-based factors (i.e., role, weapon use) will have smaller, but still significant, net effects.

2.3.3 The 100-to-1 crack-powder cocaine quantity ratio

The 100-to-1 crack-powder cocaine quantity ratio, statutorily set by Congress in the Anti-Drug Abuse Act of 1986 and subsequently incorporated into the sentencing guidelines, defines a penalty structure in which it takes 100 times the quantity in powder cocaine to reach the same sentencing level reserved for a given quantity of crack cocaine. For

example, an offense involving 50 grams of crack cocaine calls for the same ten-year mandatory minimum sentence as 5,000 grams of powder cocaine. The 100-to-1 quantity ratio was originally justified by Congress because crack was seen as the more dangerous drug, associated with greater rates of crime, violence, and physical addiction (USSC 1995). Subsequent evidence has demonstrated that the pharmacological differences between powder and crack cocaine have been overstated (Hatsukami and Fischman 1996). Moreover, commentators have pointed out that the crime and violence associated with crack markets has waned and that sentencing can be handled more justly and efficiently through specific sentence enhancements for dangerous offense behaviors (Blumstein 2003). Not only has the original justification for the 100-to-1 quantity ratio not held up, but it has spawned a host of anomalous and disparate sentencing outcomes. For example, street-level sellers of crack cocaine can receive substantially longer sentences than high-level powder cocaine suppliers and importers. Moreover, the imprisonment burden has fallen most heavily and disproportionately on blacks. While there has been considerable debate on this issue (Sklansky 1995; Stevens 1997; Stunz 1998; Tison 2003), the Sentencing Commission has on several occasions strongly recommended eliminating or reducing the 100-to-1 quantity ratio (USSC 1995, 1997, 2002). Each time, Congress has rejected these proposals.

Crack sentences are more severe on average than powder cocaine sentences, but the 100-to-1 quantity ratio does not result in sentences for crack offenders that are 100 times greater than powder cocaine offenders. The actual sentencing differential is less, although by how much depends on the time periods and specific moderating charactertistics examined. For example, between 1992 and 1997, the percent difference of

crack cocaine sentences over powder cocaine sentences increased steadily from 25% to 54%, and thereafter remained relatively stable through 2000 (USSC 2002). These figures are overall averages, however, and do not account for the similar (or different) characteristics of offenders. DOJ (2002) compared crack and powder cocaine sentences for the period 1996-2000 for similar drug amounts by weapon use and criminal history category. The analyses revealed that crack sentences were 60% greater than powder cocaine sentences on average, but this ranged anywhere from 30% to 730% greater depending on specific drug amounts and offense/offender characteristics. Notably, the largest difference was for offenders with minimal criminal histories (category I) who were involved with less than 25 grams of cocaine. Examining just simple possession defendants sentenced in FY1993, the Sentencing Commission found that crack sentences were 856% greater than powder cocaine sentences (USSC 1995).

The 100-to-1 crack-powder quantity ratio has been criticized for sweeping lower-level operators in the crack trade into punishment ranges reserved for high- to mid-level powder cocaine dealers (Angeli 1997; Blumstein 2003; Spade 1996). In a series of studies of cocaine sentencing, the United States Sentencing Commission reported that crack offenders tended to operate at the lower end of the distribution chain, especially at the street-level, and, compared to powder cocaine offenders, they were underrepresented at the mid- to upper-levels of the drug trade (USSC 1995, 2002). However, despite trafficking in substantially smaller drug quantities and dollar values of drugs, the average sentences for low-level crack offenders were similar to or even exceeded those of high-level powder cocaine offenders. Based on this evidence, the Sentencing Commission's *1995 Cocaine Report* concluded that "crack's unique distribution pattern, in combination

with the 100-to-1 quantity ratio, can lead to anomalous results in which retail crack dealers get longer sentences than the wholesale drug distributors who supply them the powder cocaine from which their crack is produced" (USSC 1995: 174). The Commission's follow-up *2002 Cocaine Report* provided even more convincing evidence in concluding that "the current penalty structure's almost exclusive reliance on quantity-based penalties…fails to provide adequate sentencing proportionality" between offenders of differing culpability (USSC 2002: 101).

The 100-to-1 quantity ratio has also led to disproportionately severe sentences for blacks (Mauer 2004; USSC 1995, 2002). According to the Sentencing Commission, "the 100-to-1 crack cocaine to powder cocaine quantity ratio is a primary cause of the growing disparity between sentences for Black and White federal defendants" (USSC 1995: 163). Several studies have attempted to estimate the effect of eliminating or reducing the 100-to-1 quantity ratio on racial disparities in sentencing. For example, the Sentencing Commission estimated that replacing the 100-to-1 ratio with a 20-to-1 ratio would reduce the sentencing gap between black and white drug traffickers by 17.8 months on average. MacDonald and Carlson (1993) simulated the effects of sentencing crack and powder cocaine traffickers equivalently for FY1990 and determined that the average sentence for blacks would have dropped 33 months (from 96 to 63 months) compared to just 4 months for both whites (74 to 70 months) and Hispanics (97 to 93 months). Thus, relative to the average sentence for whites, the average sentence for Hispanics would have remained essentially unchanged, but the average sentence for blacks would have been 10% shorter rather than 30% longer. DOJ (2002) estimated the impact of eliminating the 100-to-1 quantity ratio in general for 1999-2000 and determined that crack sentences would have

been 40% shorter than powder cocaine sentences rather than 50-60% longer. The evidence cited above supports the following hypothesis.

> H3: Crack cocaine offenders will experience a significant and substantial sentencing disadvantage compared to powder cocaine offenders. Less culpable crack offenders (e.g., retailers) will experience greater sentencing exposure than more culpable powder cocaine offenders (e.g., importers and wholesalers). Blacks will be most adversely affected by the 100-to1 quantity ratio; thus, sentencing crack and powder cocaine equivalently will benefit blacks the most.

2.3.4 The "tariff effect"

The tariff effect engenders "misplaced equality" in sentencing when a predominant sentencing factor leads to similar sentences for otherwise differently situated offenders (Oliss 1995; USSC 1991b). Whereas the sentencing guidelines exemplify a calibrated approach to sentencing in which gradations in offense seriousness, dangerousness, and culpability are taken into account, mandatory minimums represent a flat or tariff-like approach to sentencing that obscures important offender distinctions (USSC 1991b). Federal mandatory minimum drug sentences base five- and ten-year penalties exclusively on drug type and quantity, leading to situations in which offenders with different levels of culpability and dangerousness can receive similar sentences (Schulhofer 1993). The tariff effect is most egregious when it exposes the least culpable and dangerous offenders to harsh mandatory minimum sentences. Congress recognized as much when it passed the "safety valve" legislation in 1994 allowing certain low-level offenders to escape mandatory minimum sentencing and, as amended in 1995, to also receive a two-level reduction in their base offense level (Froyd 2000).

Empirical evidence of the tariff effect is rather limited. Meierhoefer (1992a, 1992b) analyzed changes in federal drug sentencing outcomes between 1984 and 1990 to

assess the sentencing impacts of the drug mandatory minimums introduced in 1986 and the sentencing guidelines promulgated in 1987. Her results indicated that sentencing exposure increased substantially for dangerous weapon-involved offenders after the sentencing law changes. With respect to culpability, sentencing exposure appeared to increase for offenders with "middle" roles in the offense to the point they were treated more like "major" role offenders. It not possible to draw any firm conclusions from Meierhoefer's (1992a, 1992b) analyses, but they suggest that (1) the capacity to differentiate among dangerous offenders for sentencing purposes was maintained and even expanded after the introduction of the guidelines and mandatory minimums and (2) offender differentiation based on culpability may have undergone a shift so that "middle" and "major" role offenders were less distinguishable under the new sentencing laws. This latter finding is supported by more recent research showing that mandatory minimum exposure for both crack and powder cocaine offenders sentenced in 2000 did not substantially decrease with the offender's level of culpability (USSC 2002).

Probably the most sophisticated analysis of this issue to date is the Sentencing Commission's *1991 Mandatory Minimum Report*, which examined quantity-based and drug-related firearm mandatory minimum sentencing outcomes for FY1990 (USSC 1991b). Bivariate analyses showed that, aside from the most peripherally involved defendants, offenders with different functional roles in the offense were sentenced at roughly equivalent rates to the requisite mandatory minimum. In addition, whether offenders were involved in a single event or an ongoing conspiracy did not differentially affect mandatory minimum sentencing outcomes.

The study also reports the results of a probit analysis predicting sentencing to at least the indicated mandatory minimum (versus not), while controlling for race, gender, prior convictions, role in the offense, and base offense level. Other variables were dropped from the final analysis due to either multicollinearity (drug type, guilty plea) or insignificance in preliminary models (firearm use, scope of criminal activity, number of codefendants, and citizenship). Thus, dangerousness as measured by gun activity and culpability as measured by the scope and size of the conspiracy had no bearing on sentencing to the applicable mandatory minimum. However, the offender's functional role in the offense (i.e., peripheral offender, street-level dealer, above street-level dealer, and high-level producer/importer/financier) was significant in the final model. These results indicated that street-level dealers had the greatest probability of being sentenced to the requisite mandatory minimum. For instance, compared to street-level dealers, peripheral offenders were considerably less likely (β = -.41) to be sentenced to the mandatory minimum, which accords with proportionality. However, the more culpable above-street dealers (β = -.03) and high-level traffickers (β = -.10) were also less likely than street dealers to be sentenced to the requisite mandatory minimum. These outcomes deviate from proportionality and are indicative of penalty inversion whereby lower-level offenders experience greater sentencing exposure than higher-level offenders. The estimated differences are fairly small, especially between street and above-street dealers, so at a minimum these results are indicative of unwarranted uniformity in federal mandatory minimum sentencing. In contrast, the offender's base offense level (representing a modified proxy for drug quantity) significantly increased the probability of sentencing to the mandatory minimum. Taken together, these results indicate that

mandatory minimums, by keying primarily on a single factor (i.e., drug quantity), fail to adequately account for important differences in levels of dangerousness and culpability. Based on this evidence, the following hypothesis is supported.

> H4: Exposure to mandatory minimum drug sentencing will not be proportional to the culpability or dangerousness of the offender. Quantity-based mandatory minimum sentencing will result in excessive uniformity across other important elements of offense seriousness.

2.3.5 The "cliff effect"

The "cliff effect" occurs when quantity-driven mandatory minimums produce large differences in sentences on the basis of small differences in drug quantity for otherwise similarly situated defendants (USSC 1991b). Despite the guideline's attempt to maintain proportionality for increasing quantities of drugs, certain conditions exist whereby a small difference in drug amount can drastically increase sentence exposure for some defendants and not others who are otherwise similar in all legal respects. As the Commission's *1991 Mandatory Minimum Report* (USSC 1991b: 30) explains,

> [mandatory minimums can result in] sharp differences in sentence between defendants who fall just below the threshold of a mandatory minimum compared with those whose criminal conduct just meets the criteria of the mandatory minimum penalty…they distinguish far too greatly among defendants who have committed offense conduct of highly comparable seriousness.

For instance, Schulhofer (1993) describes an example in which a five gram difference (i.e., 495 vs. 500 grams) in the amount of powder cocaine sold by two virtually identical offenders results in a 2.5 year sentencing differential, because the larger amount is subject to the rigid five-year mandatory penalty whereas the smaller amount is controlled by the more flexible guidelines. Steer (2000) describes another example in which a

conviction for simple possession of 5.01 grams of crack cocaine carries a mandatory minimum five-year sentence, but a simple possession conviction for 5.0 grams of crack carries a *maximum* of one year in prison.

Sentencing Commission data from 1993 for crack and powder cocaine offenders suggest that the circumstances in which the guideline maximum is less than the mandatory minimum occur in approximately 20-30% of these cases (USSC 1995), although a report by the General Accounting Office using FY1990 data found this to be the case in only 5% of the cases examined (General Accounting Office 1993). Just how big the actual difference is and whether the effect remains after the 1994 "safety valve" legislation remain important questions.

No empirical studies have directly investigated the impact of the cliff effect. However, evidence for the cliff effect can be demonstrated indirectly if, after all legally relevant factors are accounted for, simple exposure to mandatory minimum sentencing significantly predicts longer sentences.[9] Examined this way, the mandatory minimums typically create a sentencing differential of 16% to 33%, which varies across offender and demographic subgroups (Albonetti 2002a, 2002b; Bush-Baskette 2000; Kautt 2000, 2001-2002; McDonald and Carlson 1993). In short, the available evidence indicates that mandatory minimum sentencing creates "cliffs" by sentencing offenders above and beyond what they would otherwise receive under the guidelines. Importantly, none of the studies reviewed include more nuanced controls for role and weapons use. These

[9] It is important to note that not all the studies control for the same factors, nor do they always account for similar offense seriousness factors. Nevertheless, there is a fair amount of consistency across studies because they all use USSC data and, therefore, use similar constructs.

considerations, and the empirical evidence reviewed above, motivate the following hypothesis.

> H5: Mandatory minimums will produce "cliffs" by creating significant and substantial sentencing differences between offenders falling just above versus just below the mandatory minimum quantity triggers.

2.3.6 Guideline circumvention of firearm sentence enhancements

Concern with the use of firearms by drug distributors and violent criminals prompted Congress and the Sentencing Commission to establish sentence enhancements for drug traffickers who use guns (and other weapons) in the course of their criminal activity. There are two distinct types of firearms sentence enhancements (FSEs) for drug crimes that can be applied in federal courts (Hofer 2000). The first entails a guideline increase of two offense levels for possessing a dangerous weapon, including a firearm, in the course of drug trafficking. The guideline FSE increases sentences proportionately to the underlying base offense level, so that the two-level increase results in an approximate 25% increase in sentence length. In contrast, the mandatory minimum FSE governed by 18 U.S.C. § 924(c) calls for a minimum five-year sentence add-on, regardless of the underlying offense level (Hofer, Blackwell, Burchfield, and Stevens-Panzer 2000; Hofer 2000).

Procedurally, application of the guideline and statutory FSEs is quite different. Prosecutors need to include statutory 924(c) counts in the indictment, and they must be proved beyond a reasonable doubt (or plead to). In contrast, the guideline FSE is applied by the judge at sentencing based on the lower threshold of a preponderance of the evidence. Empirical evidence indicates the guideline FSE enhancement results in an

average sentence increase of 28 months—about half the statutory 924(c) enhancement (Hofer et al. 2000).

Prosecutorial discretion in deciding whether or not to pursue the guideline or statutory FSE can dramatically affect sentence outcomes. The available evidence indicates that firearm sentence enhancements are not applied evenly or consistently in all cases they appear warranted (Hofer 2000; Nagel and Schulhofer 1992; Schulhofer and Nagel 1997). For example, the Sentencing Commission's analysis of FY1990 data found that statutory 924(c) counts were not filed in 45% of eligible drug cases, and charges were subsequently dropped in 26% of the cases in which the count was initially filed (USSC 1991b). Subsequent estimates based on USSC data from 1995 and 2000 found circumvention of both the guideline and statutory FSE occurring in 29-42% of the cases they appeared legally warranted (Hofer 2000; USSC 2002). Hofer (2000) found the exact nature of the circumvention varied by the egregiousness of the offense conduct and the type of FSE. For example, as the defendant's offense conduct increased in seriousness from facilitating possession to carrying to actually using a firearm, so did the likelihood of receiving the statutory 924(c) enhancement (18%, 27%, and 34%, respectively). Alternately, the guideline FSE was applied most often when the offender facilitated possession (37%) and about equally in the other two situations (28-30%).

There are several possible reasons for these differences and for FSE circumvention in general (Hofer 2000; Nagel and Schulhofer 1992; Schulhofer and Nagel 1997). First, evidentiary limitations may dissuade prosecutors from filing 924(c) charges due to the statutory provision's higher standard of proof. Second, prosecutors may drop 924(c) counts in exchange for the defendant's plea to the underlying drug offense. Third,

equity concerns may lead prosecutors and judges to avoid FSEs when the underlying offense provides sufficiently severe punishment. For example, research by Hofer (2000) and Schulhofer and Nagel (1997; Nagel and Schulhofer 1992) suggests that FSEs are less likely to be applied to defendants responsible for larger drug quantities. The evidence leads to the following hypotheses.

> H6: FSEs will not be applied in a large percentage of cases they appear legally warranted. FSE circumvention will have a mitigating effect on sentence length. Finally, FSE application will be conditioned by the quantity of drugs involved in the offense, mandatory minimum exposure, and plea and charge bargaining practices.

3.0 DATA AND METHODOLOGY

This chapter outlines the dissertation's data and methodology. Section 3.1 describes the research design and data source. Section 3.2 defines the drug offender subsample used in the analyses. Section 3.3 presents an overview of the analyses and research plan. Section 3.4 discusses preliminary data cleaning, and Section 3.5 provides a complete description of the dependent and independent variables. Section 3.6 addresses issues of measurement reliability and validity. Section 3.7 discusses multivariate model specifications, and Section 3.8 details the study's multivariate analytic methods. Section 3.9 addresses missing data problems and responses. Finally, Section 3.10 introduces the sensitivity analyses that are presented in detail in Appendix A.

3.1 RESEARCH DESIGN AND DATA DESCRIPTION

The research for this dissertation analyzes data from the *1997 Survey of Inmates in Federal Correctional Facilities* (hereinafter called the Inmate Survey), a nationally representative survey of federal inmates cosponsored by the Bureau of Justice Statistics and the Federal Bureau of Prisons (Bureau of Justice Statistics and Federal Bureau of Prisons 2000a, 2002b).[10] Administered between June and October 1997,[11] the Inmate

[10] This dissertation employs secondary data analysis as its primary research method. By definition, secondary data analysis involves the analysis of survey data, official records and statistics, or historical documents originally collected for other purposes. Using secondary data provides a low-cost and efficient means of conducting research, and ensures a return on the investment in the original data collection. The growing volume of

Survey collected self-report information from prisoners on a wide array of topics, including offense and sentencing information, incident characteristics, criminal history, socioeconomic status, alcohol/drug use and treatment history, health status, and conditions of confinement. Since much of this information is not collected by other surveys or monitoring systems, the Inmate Survey provides a unique and expansive set of data for research purposes. This dissertation actually represents the first empirical use of these survey data for multivariate sentencing outcomes research, and in this regard the Inmate Survey provides an underutilized adjunct to the Sentencing Commission's more commonly analyzed Monitoring Dataset. Part of the reason for this relative underutilization is that the Inmate Surveys appear every 5-6 years, whereas the Monitoring Files are released yearly.

The Inmate Survey dataset is maintained and distributed through the National Archive of Criminal Justice Data (NACJD), which is a division of the Inter-university Consortium for Political and Social Research (ICPSR) at the University of Michigan. A public-use version of the Inmate Survey is available to all requesters, but access to a restricted-use version is limited to investigators with a demonstrated research interest. The restricted-use Inmate Survey dataset contains additional survey design variables needed for proper multivariate analysis, but which also pose problems of confidentiality. For this dissertation, access to the restricted-use version was secured by obtaining

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data collected by public agencies and private institutions, together with the ease of electronic distribution, has made secondary data analysis an increasingly viable option for researchers.

[11] An earlier version of the Inmate Survey was fielded in 1991 and a more recent version was completed in 2004—with an anticipated release date of late 2006 or early 2007.

research approval from the University of Pittsburgh's Institutional Review Board and entering into a data confidentiality agreement with NACJD.[12]

3.2 SUBSAMPLE DEFINITION

A total of 4,041 inmates of all offense types completed interviews for the *Survey of Inmates in Federal Correctional Facilities*. The subpopulation of interest for this research includes just those drug offenders who were sentenced after the guidelines went into effect (hereinafter referred to as New Law cases). Identification of the sample proceeded as follows. Initially, sixteen cases were excluded from the sampling frame altogether because the inmates were awaiting sentencing, leaving a new base sample size of 4,025. Of this number, the Bureau of Justice Statistics/Bureau of Prisons (BJS/BOP) classified 1,520 as primary drug offenders.[13] Close inspection of the data revealed that, for two reasons, this figure underestimates the size of the drug offender subsample by as much as 9 percent. First, BJS/BOP classified certain 'drug' crimes as nondrug offenses. For example, BJS/BOP labeled money laundering and racketeering crimes as public order offenses, even if the underlying offense conduct was drug-related as determined by data describing the offense characteristics. Second, BJS/BOP categorized offenders with multiple convictions according to the individual offense that carried the longest sentence. For example, offenders convicted of using a weapon in connection with a drug trafficking

[12] The *1997 Survey of Inmates in Federal Correctional Facilities* was actually distributed together with its companion but functionally distinct state-level counterpart, the *1997 Survey of Inmates in State Correctional Facilities*.

[13] Drug offenders were identified by their offense of conviction. For offenders with multiple convictions, the offense with the longest corresponding prison sentence was classified as primary.

crime were generally classified as public order offenders, even though the weapon conviction was contingent upon the underlying drug offense, because such weapon offenses can comprise a larger share of the total sentence.

Despite BJS/BOP's coding scheme, federal sentencing practice typically sentences these offenders under the drug trafficking guideline (USSG §2D1.1). For example, according to the *Guidelines Manual* the sentence for a defendant convicted of money laundering should be based on "the underlying offense from which the laundered funds were derived" (USSG §2S1.1(a)(1)). Consequently, 210 'nondrug' cases with obvious drug-related incident characteristics were reclassified as drug offenses, expanding the drug subsample from 1,520 to 1,730.[14]

Not all 1,730 drug offenders were New Law cases, however. The guidelines officially became effective on November 1, 1987, and they apply to offenses committed on or after that date. In many federal districts, constitutional challenges delayed full implementation of the guidelines. These issues were ultimately resolved in *Mistretta v. United States*, in which the Supreme Court upheld the constitutionality of the Sentencing Commission and the Sentencing Reform Act (United States Sentencing Commission 1991a). The date of this decision—January 18, 1989—marks the effective date of the sentencing guidelines in *all* federal districts. Thus, New Law cases are defined here to include cases in which (1) the date of arrest occurred after the guidelines took effect and (2) the date of sentencing occurred post-*Mistretta*. Fifty-nine of the 1,730 drug cases did

[14] Although BJS/BOP categorized the 210 cases as 'nondrug' in the Inmate Survey's offense classification scheme, interviewers flagged three-quarters as drug offenders during the course of the interview. Consequently, these respondents were administered the survey's drug-specific incident characteristic questions, supporting the decision to expand the drug sample in this manner.

not meet these two conditions, leaving a final estimation subsample of 1,671 New Law drug offenders.[15] Analyses involving specific drug offender subpopulations (e.g., crack cocaine offenders) are subsets of this final estimation subsample.

3.3 ANALYSIS OVERVIEW

Six interrelated research objectives concerning unwarranted disparity in federal drug sentencing outcomes guide the analyses for this dissertation (see Table 3.1). Specific evidence was examined for disparate outcomes in sentence severity, mandatory minimum exposure, and the application of firearm sentence enhancements. Independent variables used in the multivariate analyses included offense seriousness factors (e.g., drug type and quantity, role in the offense, firearms use), criminal history, case processing factors (e.g., plea status, pretrial release), and sociodemographic characteristics (e.g., race, age, gender, citizenship).

Descriptive, bivariate, and multivariate analytic techniques were employed in this research. Analyses generally proceeded in the following stages: (1) preliminary inspection of the data, (2) simple descriptive analysis, (3) correlation analysis and graphing of bivariate associations, (4) multiple regression estimation, (5) diagnostic assessment, and (6) postestimation parameter tests, model predictions, and graphical presentation of outcomes. All analyses were performed using Stata/SE, version 8.2 (StataCorp 2005). Table 3.1 collates the study's research questions, hypotheses, and the associated analytic approach.

[15] It is possible that some of the 59 excluded drug offenders were sentenced pursuant to the guidelines, particularly if these cases occurred in federal districts in which the constitutionality of the guidelines was upheld or never questioned. It was not possible to make this distinction from the available data, so all 59 cases were summarily excluded.

Table 3.1: Research Questions, Hypotheses, and Analytic Strategy

Research Questions	Hypotheses	Analytic Strategy
Q1: How well does drug quantity serve as a proxy for other elements of offense seriousness, such as the culpability of the offender and the dangerousness of the instant offense conduct?	H1: Larger drug quantities will be associated with more serious offense characteristics, such as higher-level roles in the offense, leadership and managerial responsibility, and gun use. However, there will be important exceptions for relatively minor traffickers who are held responsible for substantial drug quantities.	Correlation analysis between drug quantity and other offense seriousness factors, with supplementary graphical presentations.
Q2: How much influence does drug quantity have on sanctioning severity compared to other elements of offense seriousness?	H2: Drug quantity will be the strongest independent predictor of sentence length. Culpability- and dangerousness-based factors (i.e., role, weapon use) will have smaller, but still significant, net effects.	Regression of sentence length on offense and offender factors, with comparison of standardized coefficients to assess relative effects.
Q3: To what extent and in what ways does the 100-to-1 crack-powder quantity ratio disadvantage crack cocaine offenders?	H3: Crack cocaine offenders will experience a significant and substantial sentencing disadvantage compared to powder cocaine offenders. Less culpable crack offenders (e.g., retailers) will experience greater sentencing exposure than more culpable powder cocaine offenders (e.g., importers and wholesalers). Blacks will be most adversely affected by the 100-to1 quantity ratio; thus, sentencing crack and powder cocaine equivalently will benefit blacks the most.	Graphs of bivariate associations. Regression of sentence length on offense and offender factors in subsamples of crack and powder cocaine offenders. Postestimation analyses testing the equality of role in the offense coefficients across the crack and powder cocaine models. Postestimation adjusted predictions for sentencing crack offenders “as if” they were powder cocaine offenders.
Q4: Is there evidence of the tariff effect in the interaction between guideline and mandatory minimum sentencing?	H4: Exposure to mandatory minimum drug sentencing will not be proportional to the culpability or dangerousness of the offender. Quantity-based mandatory minimum sentencing outcomes will result in excessive uniformity across other important elements of offense seriousness.	Logistic regression of mandatory minimum exposure and sentencing outcomes on offense and offender factors.

Table 3.1 (continued)

Q5: Is there evidence of the cliff effect in the interaction between guideline and mandatory minimum sentencing?	H5: Mandatory minimums will produce "cliffs" by creating significant and substantial sentencing differences between offenders falling just above versus just below the mandatory minimum quantity triggers.	Regression of sentence length on offense and offender factors in five drug type submodels. Use of contrast -coded dummy variables to compare sentencing outcomes across quantity levels, with supplementary graphical presentations.
Q6: What is the nature and extent of circumvention of firearm sentence enhancements (FSEs) in drug sentencing outcomes?	H6: FSEs will not be applied in a large percentage of cases they appear legally warranted. FSE circumvention will have a mitigating effect on sentence length. Finally, FSE application will be conditioned by the quantity of drugs involved in the offense, mandatory minimum exposure, and plea and charge bargaining practices.	Descriptive analysis of FSE outcomes. Logistic regression of FSE status on offense and offender factors.

3.4 DATA PREPARATION

Secondary data rarely come ready for analysis. The Inmate Survey data were no exception. This section describes the measures taken to clean and prepare the data prior to analysis. Primarily, this involved inspecting relevant questionnaire items, the associated skip patterns, and the actual data values to ensure that the data were appropriately coded and within acceptable ranges. In many instances, this simply involved recoding the survey data into a form amenable for statistical analysis. For example, the Inmate Survey datafile recorded responses for dichotomous measures as follows: '-1' for don't know, '-2' for refused, '1' for yes, and '2' for no. These variables were recoded to match the following format: '0' for no, '1' for yes, and '.' as a missing value indicator.

It was also common for questionnaire items to be skipped based on the pattern of prior responses, with the value '9' (or '99,' '999,' etc.) representing this blanked or

skipped status. Although these blanked values indicate that the respondent provided no direct response, they do not represent missing data due to nonresponse per se. For example, consider the following consecutive survey questions:

> *During the month before your arrest in [MM/YY], did you have a job or a business?*
>
> *[IF YES] Was this full-time, part-time, or occasional work?*

Respondents who did not have a job prior to their arrest, or who did not know or refused to answer this question, were skipped over the subsequent question. Thus, it is necessary to examine responses to the first question to infer whether a blank '9' code on the second item is due to nonresponse or a valid response of not having a job. In general, this process of inferring blanked values was fairly straightforward, although complex skip patterns occasionally required examining multiple prior and/or subsequent responses. Lastly, if data values were miscoded or out of range, an attempt was made to recover the true value from auxiliary information. If this was not possible, the values were recoded to missing.

3.5 MEASURES

3.5.1 Dependent variables

Several sentencing outcome measures were used in this research. Most analyses focus on sentence length, but specific analyses also address the application of mandatory minimums and firearm sentence enhancements. Table 3.2 presents descriptive statistics for the dependent variables.

3.5.1.1 Sentence length in months The Inmate Survey asked respondents to report the length of their sentence imposed by the court. This interval-level variable codes the court-imposed maximum sentence length in months for the current offense(s), including any suspended time.

3.5.1.2 Mandatory minimum sentencing Two binary dependent variables were analyzed regarding mandatory minimum sentencing outcomes. The first reflects *mandatory minimum exposure*. Since a definitive indicator of mandatory minimum application was not included in the Inmate Survey, this measure was constructed from the survey's drug type and quantity information (refer to Sections 3.5.2.1 through 3.5.2.3). The variable is coded '1' for defendants who were exposed to either the five- or ten-year mandatory minimum and '0' for defendants who were not exposed.

The second dependent variable reflects *sentencing at or above the requisite mandatory minimum* among offenders exposed to mandatory minimum sentencing (n = 1,165). The variable was coded '1' if offense characteristics called for a mandatory minimum of five (ten) years and the actual sentence imposed was at least five (ten) years, and '0' otherwise.

3.5.1.3 Firearm sentence enhancements Two binary dependent variables were analyzed regarding the application of a firearm sentence enhancement (FSE) for using or possessing a firearm during a drug trafficking crime. The first represents a mandatory five-year sentence enhancement under 18 U.S.C. 924(c), and is actually more than an enhancement because it requires a separate conviction. The second represents a two-level

increase in the base offense level as provided for under the drug trafficking guideline (USSG 2D1.1(b)(1)). Both variables are coded '1' for positive status and '0' otherwise. Refer to Section 3.5.2.7 for additional details on these items.

Table 3.2: Dependent Variable Descriptive Statistics

Dependent Variables	Sample n (n = 1,671)	Weighted Estimate (N = 55,481)
Sentence Length in Months	1,671	$\bar{x}$ =149.0
Mandatory Minimum Exposure		
None	506	30.6%
Five or Ten Years	1,165	69.4%
Sentenced At or Above the Requisite Mandatory Minimum	(n = 1,165)	(N = 38,521)
No	387	28.9%
Yes	778	71.1%
924(c) Conviction		
No	1,497	90.9%
Yes	174	9.1%
Guideline FSE		
No	1,510	89.6%
Yes	161	10.4%

3.5.2 Independent variables

The independent variables cover a range of offense and offender characteristics, including offense seriousness factors, criminal history, case processing factors, and sociodemographic characteristics. The following subsections describe these measures, and Table 3.3 presents the descriptive statistics.

3.5.2.1 Primary drug type The Inmate Survey asked respondents to report the specific drug types involved in their offense:

> *You said that you were serving time for [CURRENT OFFENSES]. What drugs were involved: (1) Heroin, (2) Other opiates or methadone, (3) Amphetamines or methamphetamine (Uppers), (4) Methaqualone, (5) Barbiturates (Downers), (6) Tranquilizers, (7) Crack cocaine, (8) Cocaine other than crack, (9) PCP, (10) LSD or other hallucinogens, (11) Marijuana or hashish, (12) Other?*

Respondents were asked to identify all applicable drugs. While the majority of drug offenders (89%) acknowledged serving time for a single drug type, 11% reported involvement with multiple drugs. For these polydrug offenders, the drug with greatest sentencing potential based on quantity was denoted as the primary drug type. This information was then recoded into six dummy variables representing the following drug categories: heroin, methamphetamine, crack cocaine, powder cocaine, marijuana, and other drugs.

3.5.2.2 Marijuana equivalent quantity For each drug, respondents were asked "*Approximately what amount of [DRUG] was involved?*" Drug amounts could be recorded using several standard metrics such as grams, kilograms, pounds, and ounces.

Nonstandard metrics such as rocks, pills, and joints were also accepted, as were respondent-specified units such as baggies and plants. For each drug type, these various metrics were transformed into gram weight and then summed. For nonstandard metrics, quantities were first transformed into gram weight based upon conversion factors specified in the *Guidelines Manual* (USSC 1997b) or, if necessary, on supplementary law enforcement and ethnographic information (Office of National Drug Control Policy 1995-2002; Community Epidemiology Work Group 2000-2002). For example, one bag or paper of heroin was assumed to be 0.1 gram; one rock or vial of powder or crack cocaine to be 0.2 gram; and one marijuana joint to be 0.5 gram.

This process resulted in separate quantity measures for each drug type, but guideline rules weight drugs differently for purposes of determining the base offense level. Therefore, a second step entailed generating "marijuana equivalencies" as specified by the sentencing guidelines' Drug Equivalency Tables[16] and, for polydrug offenders, summing across drug types. This procedure resulted in a single, properly weighted quantity measure that mirrors the guideline practice of determining a commensurate base offense level for different amounts and types of drugs.

[16] As set forth in USSG §2D1.1(c), the Drug Equivalency Tables specify conversion rules for obtaining "marijuana equivalent" drug quantities. For instance, the conversion rules indicate that 1g of powder cocaine = 200g marijuana, 1g of heroin = 1,000g marijuana, and 1g of crack cocaine = 20,000g marijuana. Within the guidelines, marijuana equivalencies are used to determine base offense levels for drugs not otherwise mentioned by statute or to combine controlled substances in order to obtain a single offense level for polydrug offenders. For example, a defendant convicted of trafficking 80 grams of powder cocaine and five kilograms of marijuana would garner the same base offense level (i.e., 18) as a defendant convicted of trafficking 21 kilograms of marijuana [(80 g * 200 g) + 5 kg = 21 kg].

3.5.2.3 Mandatory minimum exposure As enacted by the Anti-Drug Abuse Act of 1986 and codified in 21 U.S.C. §841, defendants convicted of manufacturing or distributing heroin, powder cocaine, crack cocaine, PCP, LSD, or marijuana are subject to five- and ten-year mandatory minimum sentences based upon the quantity of drugs involved. In November 1988, methamphetamine was added to the list and, under 21 U.S.C. §844, the five-year mandatory minimum penalty was extended to the *simple possession* of crack cocaine (but not other drugs). While the Inmate Survey does not include an item that measures actual mandatory minimum status, it was possible to construct a measure of mandatory minimum *exposure* from the drug type and quantity data collected by the survey. Accordingly, two dummy variables were created to measure five- and ten-year mandatory minimum (MM) exposure based on the above statutes' effective dates and respective quantity triggers: heroin (100g/1kg), powder cocaine (500g/5kg), crack cocaine (5g/50g), PCP (100g/1kg), LSD (1g/10g), marijuana (100kg or plants/1,000kg or plants), and methamphetamine (100g/1kg).[17]

[17] Unique to PCP and methamphetamine, two sets of quantity triggers have been statutorily defined: one based upon a pure amount of the substance (10g/100g) and the other based upon a mixture containing the substance (100g/1kg). The quantity triggers for the latter were used here. Also, while not applicable to these data, a 1998 legislative amendment halved the five- and ten-year methamphetamine mixture quantity triggers to 50g and 500g, respectively (USSC 1999).

Table 3.3: Independent Variable Descriptive Statistics

Independent Variables	Sample n (n = 1,671)	Weighted Estimate (N = 55,481)
Marijuana Equivalent Kilograms	1,671	$\bar{x}$ = 47,175.6
Five-Year MM Exposure	491	28.2%
Ten-Year MM Exposure	674	41.2%
Primary Drug Type		
Heroin	150	7.7%
Methamphetamine	183	10.6%
Crack Cocaine	444	27.3%
Powder Cocaine	601	37.1%
Marijuana	248	14.7%
Other Drugs	45	2.7%
Role in the Offense		
Money Laundering	138	6.2%
Importing	236	12.7%
Producing	99	6.4%
Wholesaling	302	18.5%
Distributing NOS	484	29.5%
Retailing	304	19.9%
Possessing	108	6.8%
Aggravating Role		
None	1,545	92.9%
+2	29	1.7%
+3	41	2.5%
+4	56	2.9%
Mitigating Role		
None	1,526	92.1%
-2	76	4.1%
-3	56	3.3%
-4	13	0.5%

Table 3.3 (continued)

Safety Valve Eligibility	411	21.5%
924(c) Conviction	174	9.1%
Guideline FSE	161	10.4%
Used/Possessed Firearm	133	7.4%
Criminal History Category		
I	1,063	59.4%
II	179	11.9%
III	235	15.9%
IV	63	4.3%
V	23	1.5%
VI	108	7.1%
Guilty Plea	1,140	66.9%
Charge Bargain	890	53.4%
Pretrial Release	672	36.3%
Race/Ethnicity		
White	389	22.7%
Black	676	41.4%
Hispanic	551	32.6%
Other	55	3.3%
Male	1,241	91.5%
Age at Offense	1,671	$\bar{x}$ = 33.1
Educational Attainment	1,671	$\bar{x}$ = 10.9
Noncitizen	354	22.2%

3.4.2.4 Role in the offense The Inmate Survey asks respondents to report all applicable drug-related activities they were involved with at the time of their arrest:

> *The next few questions are about drug-related activities you may have been involved with. At the time of your arrest for [CONTROLLING OFFENSE] were you: (1) Importing or helping others import illegal drugs into the United States? (2) Illegally manufacturing, growing or helping others manufacture or grow drugs? (3) Laundering drug money? (4) Distributing or helping to distribute drugs to dealers? (5) Selling or helping to sell drugs to others for their use? (6) Using or possessing illegal drugs?*

From a market-level perspective, these activities represent differing degrees of culpability and will be referred to respectively as importing, producing, money laundering, wholesaling, retailing, and possessing. Since question phrasing refers to both primary and supportive activities, these roles should be interpreted as representing the "average participant."[18]

[18] The following subsidiary question attempted to further distinguish the degree of market-level involvement: "*Were you: (1) a street-level dealer, (2) a dealer above the street-level dealer, (3) a bodyguard, strongman or debt collector, (4) a go-between or broker, (5) a moneyrunner, (6) a courier, mule or loader?*" However, the item performs poorly in this regard for several reasons. First, only respondents who reported involvement with importing, wholesaling, or retailing were asked this follow-up question. Thus, the survey made no attempt to discern finer distinctions among the other roles of producing, money laundering, or possessing. Second, the questionnaire was designed to ask offenders involved with importing whether they were a "courier, mule, or loader." However, a programming error inadvertently skipped importers over the subsidiary question if they did not also acknowledge being a wholesaler or retailer. Consequently, for more than 60 percent of respondents who acknowledged "importing or helping others import illegal drugs," it was not possible to determine whether or not their participation was limited to low-level transport. Third, for respondents involved with wholesaling or retailing, the additional information did not contribute to clear-cut culpability distinctions. For instance, is the wholesaler who operates at the street level more culpable than the retailer who operates above the street level? Ultimately, given these shortcomings, it was not feasible to obtain finer market-level distinctions with any degree of confidence, so these data were not utilized.

Notably, a unique data quality issue was encountered with these items: about one-quarter of the sample respondents answered 'no' to all six drug-related activities. Question phrasing (i.e., "*At the time of your arrest... "*) may partly explain why so many respondents failed to specify an activity, especially considering that an arrest for drug conspiracy—a substantial share of federal drug caseloads—can occur well before or after the substantive act was completed. Fortunately, as detailed in Section 3.9.3.1, it was possible to augment these measures with alphanumeric conviction offense data that coded detailed offense characteristic information such as "Importation of Heroin" or "Manufacturing Methamphetamine." However, since it was difficult to distinguish between the wholesaling and retailing roles from this additional information, a seventh "distributing, not otherwise specified" category was created as a generic placeholder for those indeterminate offenders involved in some manner of trafficking or sales.

Again, multiple activities could be reported, so the final step entailed creating a series of seven dummy variables coding each defendant's most culpable role in the offense. The sentencing guidelines do not articulate explicit rules for how these differences should factor into sentencing decisions, and therefore provide no insight into how they should be ranked. Nevertheless, prior research and commentary (Miller and Freed 1994; USSC 2002: Appendix C) suggests the following rank from most to least culpable: money laundering, importing, producing, wholesaling, distributing NOS, retailing, and possessing. For offenders reporting multiple roles, the dummy variables were coded according to the most culpable role indicated.

3.5.2.5 Aggravating and mitigating role adjustments The sentencing guidelines' aggravating and mitigating role adjustments gauge culpability as a function of the *relative* seriousness among coconspirators. Specifically, under USSG §3B1.1 the following aggravating role adjustments can be applied: a four-level increase in the base offense level for organizers or leaders of criminal activity involving five or more participants, a three-level increase for managers or supervisors of criminal activity involving five or more participants, or a two-level increase for organizers, leaders, managers, or supervisors of criminal activity involving less than five participants. Similarly, under USSG §3B1.2 the following mitigating role adjustments can be applied: a four-level decrease in the base offense level for "minimal participants," a two-level decrease for "minor participants," and a three-level decrease for defendants falling in between these levels.[19] Defendants who were not drug organization participants are viewed neutrally for purposes of this guideline adjustment. The Inmate Survey asks the following relevant questions:

> *In the year before your arrest on [DATE], were you a part of any group or organization that engaged in drug manufacturing, importing, distribution or selling?*
>
> *[IF YES] Altogether, about how many people would you say regularly participated in that group or organization?*
>
> *Which of these best describes your role in that group or organization: (1) A leader or organizer, (2) A middle man, (3) An underling, such as a carrier, runner, etc., (4) A seller, (5) Other?*

[19] Under this guideline, minimal participants are defined as "plainly among the least culpable of those involved in the conduct of a group" (USSG §3B1.2, comment, n.4), and minor participants as "less culpable than most other participants, but whose role could not be described as minimal" (USSG §3B1.2, comment, n.5).

These items were used to create two variables modeling the sentencing guidelines' aggravating and mitigating role adjustments.[20] First, open-ended responses to the organizational role question were examined and recoded to match one of the existing response options if applicable. The remaining offenders consisted of low-level coconspirators (e.g., "girlfriend," "wife," "bystander"), so this subset remains as a fifth peripheral role category. Beginning with the most culpable, aggravating role status was operationalized as follows: leader/organizers of large organizations (i.e., involving five or more members), middlemen of large organizations, leader/organizers or middlemen of small organizations, and all others with no aggravating status. Likewise, beginning with the least culpable, mitigating role status was operationalized as follows: peripheral coconspirators, sellers, underlings, and all others with no mitigating status.

3.5.2.6 Safety valve eligibility Effective September 23, 1994, the sentencing guidelines were amended to limit the applicability of mandatory minimum sentences for certain low-level offenders (USSG §5C1.2). Commonly referred to as the "safety valve" amendment, this change enabled defendants who met certain criteria to escape sentencing under statutory mandatory minimum laws. On November 1, 1995, the safety valve was extended to nonmandatory minimum cases in which the defendant had an offense level of at least 26 (i.e., 63-78 months). While the Inmate Survey does not include an item

[20] Even though these questions refer to the "year before arrest," which may include conduct for which the defendant was not expressly convicted, the principles and limits of sentencing accountability are not the same under the guidelines as those for criminal liability (USSG §1B1.3, comment, n.1). Indeed, the sentencing guidelines' relevant conduct rules state that "conduct that is not formally charged or is not an element of the offense of conviction may enter into the determination of the applicable guideline sentencing range" (USSG §1B1.3, comment, backg'd).

regarding safety valve status, it was possible to construct a measure of *safety valve eligibility* based upon the guideline's constituent criteria.[21] Thus, for offenders sentenced after the effective date, safety valve eligibility was operationalized to exclude offenders who (1) had more than one criminal history point, (2) received a firearm sentence enhancement or otherwise acknowledged using or possessing a dangerous weapon in connection with their offense, (3) were concurrently convicted of a violent offense or caused injury to others, (4) were leader/organizers or middlemen of a drug organization or were convicted of engaging in a continuing criminal enterprise, or (5) did not reach a plea or charge bargain agreement with the government. That is, ineligible offenders by date or those who met any of these criteria—as operationalized in the relevant subsections—were coded '0' on this measure and '1' otherwise.

3.5.2.7 Firearm sentence enhancements The sentencing guidelines enhance sentences for defendants who use or possess dangerous weapons in connection with a violent or drug trafficking crime. For drug offenders, a firearm sentence enhancement (FSE) can be brought to bear through either a concurrent firearm conviction under 18 U.S.C §924(c),

[21] These criteria, as set forth in USSG §5C1.2, include the following:

(1) the defendant does not have more than 1 criminal history point . . .;
(2) the defendant did not use violence or credible threats of violence or possess a firearm or other dangerous weapon . . . in connection with the offense;
(3) the offense did not result in death or serious bodily injury to any person;
(4) the defendant was not an organizer, leader, manager, or supervisor of others in the offense, as determined under the sentencing guidelines and was not engaged in a continuing criminal enterprise, as defined in 21 U.S.C. §848; and
(5) not later than the time of the sentencing hearing, the defendant has truthfully provided to the Government all information and evidence the defendant has concerning the offense or offenses that were part of the same course of conduct or of a common scheme or plan . . .

which carries a fixed minimum sentence of five years, or a guideline-based weapon enhancement under USSG §2D1.1(b)(1), which carries a two-level increase in the base offense level. Conviction under 924(c) requires a prosecutor to prove the offense beyond a reasonable doubt or for the defendant to plead guilty, whereas application of the guideline FSE requires only that the judge be convinced of weapon behavior by a preponderance of the evidence.

Data from the Inmate Survey were used to create a dummy variable for each type of FSE. First, for the 924(c) enhancement, conviction offense data were examined to identify offenders convicted of using or possessing a firearm in connection with the underlying drug offense. Second, evidence for receipt of the guideline-based FSE was derived from the following question: "*Did you receive an increase in your sentence because of a firearms violation?*" Given the generality of the question, it is conceivable that respondents could have interpreted this question to refer to either a 924(c) conviction or the guideline FSE. However, the guidelines prohibit defendants from receiving both FSEs for the same offense conduct; they are mutually exclusive outcomes. Therefore, to prevent "double counting," the guideline FSE variable was recoded to '0' for defendants who were convicted of a logically prior 924(c) offense.

3.5.2.8 Offense-related firearm use Irrespective of whether an FSE was applied, the Inmate Survey also asked respondents about offense-related weapon use: "*Did you use, carry or possess a weapon when the [CONTROLLING OFFENSE] occurred?*" A subsequent question asked those who responded affirmatively to identify the type of weapon (e.g., handgun, rifle, knife, blunt object). A dummy variable representing the use

or possession of a firearm during the offense was coded '1' if a firearm was reported as the type of weapon and '0' otherwise.

3.5.2.9 Criminal history category The Inmate Survey collects detailed information on up to ten prior sentences to jail or prison (e.g., offenses of conviction, admission dates, times served), as well as less detailed information on the total number of custodial and noncustodial sentences previously imposed, whether as an adult or juvenile. More than 400 variables in the Inmate Survey capture these various aspects of criminal history. These data were used to model the Criminal History Category guideline (USSG §4A1.1). Specifically, the total number of criminal history points was operationalized and calculated per guideline criteria as follows:

(a) Three points were added for each prior adult conviction and sentence of imprisonment exceeding one year and one month (excluding drunkenness, vagrancy, loitering, disorderly conduct, and minor traffic crimes) for which any portion was served within fifteen years of the defendant's commencement of the current offense.

(b) Two points were added for each prior adult conviction and sentence of imprisonment exceeding sixty days not counted in item (a) (excluding drunkenness, vagrancy, loitering, disorderly conduct, and minor traffic crimes) for which any portion was served within ten years of the defendant's commencement of the current offense.

(c) One point was added for each prior adult conviction not counted in items (a) or (b), up to a total of four points.

(d) Two points were added if the defendant committed the current offense while on parole, probation, or escape status.

(e) Two points were added if the defendant committed the current offense less than two years after release from prison on a sentence counted in items (a) or (b) or while on escape status. Only one point was added if two points were added under item (d).

(f) For each prior sentence involving multiple violent convictions, one point was added for each individually convicted violent offense beyond the first, up to a total of three points.

The criminal history category was then determined by the total number of criminal history points as provided in the Sentencing Table (USSG §5A): I (0 or 1), II (2 or 3), III (4, 5, 6), IV (7, 8, 9), V (10, 11, 12), VI (13 or more).[22]

The sentencing guidelines also provide for enhanced terms of imprisonment for "career offenders" (USSG §4B1.1) and "armed career criminals" (USSG §4B1.4).[23] These guideline adjustments operate by increasing an eligible offender's criminal history category to the maximum allowable. The following Inmate Survey item was used to proxy the imposition of either of these habitual offender enhancements: "*Did you receive an increase in your sentence because of a second or third strike?*" Thus, in mimicking guideline operation, the combination of receiving a habitual offender enhancement and having seven or more criminal history points resulted in the offender's criminal history category increasing to level VI.

[22] Although the impact is not likely to be great, the final criminal history score will be underestimated on two accounts. First, criminal history calculations under the guidelines are based on the sentence length imposed, but the Inmate Survey data only support calculations based on actual time served, which is generally of shorter duration. Second, data on prison sentences occurring before the most recent ten were not collected and, therefore, would not be included in the calculations if they were applicable.

[23] Career offenders are defined as current violent or controlled substance offenders with two or more prior violent or drug felony convictions. Armed career criminals are defined as offenders convicted of possessing or transporting firearms in violation of 18 U.S.C. §922(g) who have three or more prior felony violent or drug convictions.

3.5.2.10 Guilty plea In addition to saving the criminal justice system the costs of going to trial, pleading guilty is a strong signal to the court of the defendant's acceptance of responsibility. Under USSG §3E1.1, a defendant is entitled to a two-level decrease in his offense level if he "clearly demonstrates acceptance of responsibility for his offense." To measure these effects, a guilty plea dummy variable was created from the following survey item: "*In your trial for the [CURRENT OFFENSES], did you enter an Alford plea, a no contest plea, a guilty plea, or did you plead not guilty?*"[24] Respondents could report multiple plea types, and since Alford and no contest pleas are not open admissions of responsibility, only those defendants who submitted a straight guilty plea were coded '1' on this dummy variable.

3.5.2.11 Charge bargain Federal rules of criminal procedure allow plea agreements between the government and a defendant to dismiss criminal charges, as long as "the remaining charges adequately reflect the seriousness of the actual offense behavior" (USSG §6B1.2). This charge bargain behavior is measured by the following item: "*Before your trial for the [CURRENT OFFENSES], did you reach an agreement with a prosecutor to plead guilty to a lesser charge or to fewer counts?*"[25]

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[24] An Alford plea, not an official plea under the *Federal Rules of Criminal Procedure*, is "a guilty plea to a charge by the defendant, despite his claim of innocence, because the evidence of guilt is overwhelming" (BJS 2000b: 161).

[25] Under the sentencing guidelines, a defendant's substantial assistance to authorities in the prosecution of others is recognized as a mitigating sentencing factor (USSG §5K1.1). Since application of this departure requires an explicit government motion in recognition of the defendant's assistance (e.g., providing information on or testifying against codefendants, working undercover), such motions ostensibly follow from *quid pro quo* agreements. However, since §5K1.1 departures are not plea agreements per se, it is plausible that they are not measured by this survey item. Unfortunately, no other item in

3.5.2.12 Pretrial release Release prior to trial has been found in prior sentencing research to be related to sentencing outcomes. The following item measures this case processing outcome: "*Were you released between the time of your arrest (notification of charges) and the start of your trial?*"

3.5.2.13 Sociodemographic measures A number of sociodemographic measures were controlled for in the analyses. These include the usual suspects in sentencing outcomes research (i.e., race/ethnicity, gender, and age at offense), as well as measures for educational attainment and noncitizenship status. Specifically, educational attainment is an interval level measure of the number of traditional (i.e., non-GED) years of education completed (0-18), and noncitizenship status is an indicator of non-U.S. citizenship.

3.6 RELIABILITY AND VALIDITY OF THE MEASURES

The self-report method is a major part of the social science toolkit. Measurement error and the associated problems of reliability and validity, however, have long been major concerns for survey researchers (Carmines and Zeller 1979). This is particularly true for criminological and drug-related research on sensitive and stigmatized behaviors. While few studies have addressed measurement issues among inmates, the general consensus from other criminal and general population samples is that self-report is an acceptably reliable and valid, albeit far from perfect, method of measuring criminal behavior (Junger-Tas and Marshall 1999; Thornberry and Krohn 2000). Of the two measurement

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the Inmate Survey addresses this topic, so there is potential for measurement error and omitted variable bias.

properties, the evidence for validity is generally weaker and more controversial than for reliability (Des Jarlais 1998; Harrison and Hughes 1997; Johnson, Taylor, Golub 2005; Mathur, Dodder, and Sandhu 1992).

In general, it is difficult for secondary data analysts to perform reliability and validity checks on survey measures. For the present study, reliability has to be assumed. However, it proved feasible to assess the criterion validity of specific measures against United States Sentencing Commission data. Specifically, this criterion validity assessment entailed comparing aggregate measures from the Inmate Survey and the Sentencing Commission's *1996 Sourcebook of Federal Sentencing Statistics* (USSC 1996). The *1996 Sourcebook* covers sentencing statistics for the fiscal year from October 1, 1995 to September 30, 1996; Inmate Survey data on offenders sentenced during the same time period were selected for comparison.

According to the *Sourcebook*, a total of 15,652 drug offenders were sentenced to prison in FY1996 (excluding 348 with split sentences). The Inmate Survey estimate is substantially lower at 11,941 (95% C.I. = 10,647 to 13,235). There are two likely sources for this discrepancy. The first, as evinced by the confidence interval, is sampling error. The second arises from differences between stock and flow data. The Commission's data represents a complete census of the flow of inmates into prison, whereas the Inmate Survey provides a cross-sectional estimate of the stock of drug offenders in prison. Naturally, for any given period, flow estimates will be larger than stock estimates because the prison stock decreases as inmates are released.[26]

[26] Comparing FY1997 data did not eliminate the discrepancy between data sources because data collection for the Inmate Survey waned toward the end of the fiscal year. Thus, the drug offender subsample for FY1997 actually contained fewer observations

Table 3.4 compares specific Inmate Survey measures with similar Sentencing Commission measures for drug offenders sentenced during FY1996. Note that the base N for the Sentencing Commission data is 17,267, which includes drug offenders sentenced to probation as well as prison. It was not possible to disaggregate these two groups in the *1996 Sourcebook*, so all 17,267 sentenced drug offenders served as the reference group. Unfortunately, this will introduce inconsistency to the extent that prisoners and probationers are substantively different on the measures examined. However, as a criterion data source, it is the best available.

Overall, despite certain large deviations, the aggregate measures matched well across data sources. For example, the Inmate Survey estimates for race/ethnicity, citizenship, criminal history, and mandatory minimum status were highly consistent with the Commission data. For other measures, such as gender and firearm sentence enhancement status, the Commission's data points fall on or just outside the Inmate Survey's 95% confidence bounds. The drug type estimates varied in one respect with Commission data: the numbers of powder cocaine and marijuana offenders were over- and underestimated, respectively. Finally, the estimates for role adjustment and the safety valve status showed large deviations from Commission data. The issue with the role adjustment variable concerns the serious underestimate of mitigating role status, which suggests that the Inmate Survey item as operationalized does not fully capture the true extent to which mitigating role adjustments are applied. Safety valve status is overestimated, although part of reason is that the Inmate Survey item captures eligibility whereas the Commission variable captures actual application of the safety valve.

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than the FY1996 subsample (which explains why the latter period was used for comparison).

Table 3.4: Criterion Validity of Specific Inmate Survey Measures, FY1996

Measures	USSC Data (N = 17,267)	Inmate Survey Data (N = 11,941)	
	%	%	[95% C.I.]
Race/Ethnicity			
White	25.7	21.4	[17.1, 26.4]
Black	35.2	37.8	[29.8, 46.4]
Hispanic	37.1	36.7	[27.9, 46.4]
Other Race	2.0	4.2	[2.4, 7.3]
Gender			
Male	87.3	90.5	[87.5, 92.8]
Female	12.7	9.5	[7.2, 12.5]
Citizenship			
U.S. Citizen	72.1	78.9	[71.3, 84.9]
Non-U.S. Citizen	27.9	21.1	[15.1, 28.7]
Primary Drug Type			
Heroin	10.3	8.0	[4.8, 13.0]
Methamphetamine	9.5	11.7	[7.3, 18.3]
Crack Cocaine	26.8	25.0	[19.2, 32.0]
Powder Cocaine	26.0	37.2	[30.1, 45.0]
Marijuana	24.7	14.5	[11.0, 19.0]
Other Drugs	2.7	3.5	[2.0, 6.1]
Criminal History Category			
I	55.8	58.4	[49.9, 66.4]
II	13.0	13.3	[9.8, 17.8]
III	14.9	14.8	[11.1, 19.5]
IV	6.1	4.8	[2.1, 10.5]
V	2.9	1.7	[0.6, 5.1]
VI	7.4	7.0	[3.8, 12.7]

Table 3.4 (continued)

Received 924(c) or Guideline FSE			
No	85.5	81.3	[76.3, 85.4]
Yes	14.5	18.7	[14.6, 23.7]
Role Adjustment			
Mitigating	20.2	4.1	[2.3, 7.2]
None	71.1	89.8	[85.0, 93.2]
Aggravating	8.8	6.1	[3.7, 9.9]
Mandatory Minimum Status			
None	34.0	30.0	[24.2, 36.5]
Five-Year	28.9	32.7	[25.7, 40.6]
Ten-Year	37.1	37.3	[31.2, 43.8]
Safety Valve			
No	80.8	62.7	[54.2, 70.6]
Yes	19.2	37.3	[29.4, 45.8]

In summary, many of the Inmate Survey estimates matched exceptionally well with the Commission's census data, and nearly all were within acceptable bounds. Nevertheless, certain large discrepancies were evident. This could be due to the different operationalization of the measures, as with safety valve status, or simply to poor measurement, as with the mitigating role adjustment. By and large, however, this validity check leaves an overall favorable impression of the quality of the Inmate Survey data.

3.7 MULTIVARIATE MODELING AND CODING SPECIFICATIONS

3.7.1 Model specification and functional form

In order to maintain distributional assumptions, sentence length and certain continuous independent variables (i.e., drug quantity, age at offense) were natural log transformed

prior to multivariate analysis.[27] Such transformations affect how model coefficients are interpreted, and this varies according to the functional form relationship between the logged dependent variable—sentence length—and a given independent variable. For logged independent variables, for example, the effects are interpreted as an elasticity (i.e., the percentage change in *y* for a one percent increase in *x*). For interval-level independent variables, the effects are interpreted as the percentage change in *y* for a one unit increase in x based on the formula $100*(\exp(\beta) - 1)$. For dichotomous independent variables, the effects are interpreted as the percentage change in *y* for having the denoted *x* characteristic based on the formula $100*(\exp(\beta - \frac{1}{2}\sigma^2) - 1)$ (DeMaris 2004; Kennedy 1981). All coefficients from multivariate analyses involving sentence length were interpreted in this manner; logistic regression coefficients were reported as odds ratios, which have their conventional interpretation.

3.7.2 Specialized dummy variable coding schemes

Specialized dummy variable coding schemes were used in two instances. First, *effect coding* was used for the role in the offense variables in all analyses. Traditional dummy coding identifies a reference group against which all coefficients in the set of dummies are interpreted. Effect coding changes the reference group to the "average" or "typical" case, and the individual regression coefficients can be interpreted as the "uniqueness" or "eccentricity" of the specified group (Hardy 1993). This provides for an intuitive interpretation of role in the offense effects. Moreover, effect coding enables the coefficients for all the dummies to be expressed through a respecification of the dummy

[27] Hereinafter, all general references to log transformations refer to the natural log.

coefficients and the constant term (Hardy 1993). Thus, all seven effect-coded role in the offense dummies were reported in the estimation results, where the reference group is the offender of "average" culpability. This respecification was accomplished using Stata's postestimation -devcon- command.

The second dummy coding scheme entailed *backward adjacent difference coding*, which was used in the analyses examining the cliff effect. For an ordered set of dummies, this coding scheme contrasts the effect for a given dummy group against the effect of the immediately prior group, where the lowest group serves as the reference category. Backward adjacent difference coding is particularly useful for the cliff effect analyses, because it enables statistical comparison of sentence length outcomes between groups of offenders who possessed drug amounts, say, just above versus just below the five-year mandatory minimum quantity trigger. Thus, in this way, sentence outcomes were contrasted for different groups of offenders depending on the quantities possessed in relation to the five- and ten-year mandatory minimums.

3.8 MULTIVARIATE ANALYTIC METHODS

3.8.1 Design-based analysis

When researchers wish to gather representative information from a given target population, survey methodologists commonly employ complex sampling designs that deviate from simple random sampling for both methodological and cost considerations. Complex surveys generally contain three important design elements: stratification, clustering, and weighting. Strata define divisions or parameters in the population of interest within which sampling is performed independently. Clusters, or primary

sampling units (PSUs), are higher-level groups within which the individual units of interest are ultimately selected. Sampling weights reflect the number of observations a sample case represents in the population (Chamber and Skinner 2003; Korn and Graubard 1999; Lee and Forthofer 2006; Skinner, Holt and Smith 1989).

The target population for the Inmate Survey consisted of inmates housed in federally owned and operated correctional facilities. Survey administrators sampled this population using a stratified two-stage design. In the first stage, forty correctional facilities were randomly selected from within seven strata defined by security level and gender.[28] In the second stage, inmates were randomly selected from within the sampled facilities. The final sampling weights reflect these initial probabilities of selection, modified by a number of poststratification and nonresponse adjustment factors (Bureau of Justice Statistics and Federal Bureau of Prisons 2000b).

When survey data are collected using complex sampling designs, the subsequent challenge—which can be particularly vexing to the secondary data analyst—is to make valid inferences about the target population from the realized probability sample. Analytic methods designed to make inferences from simple random samples are not always valid when the data are derived from complex designs. Indeed, if complex design features are ignored, the analysis results can be biased and misleading (Lee and Forthofer 2006). Ignoring stratification and clustering can produce erroneous standard errors, confidence intervals, and test statistics; omitting sampling weights can result in biased point estimates and regression coefficients (StataCorp 2003b). In the present context, for

[28] There were a total of 127 correctional facilities. Males were organized into five strata defined by minimum, low, medium, high, and administrative security levels. Females were organized into two: minimum and all other security classifications.

example, the Inmate Survey undersampled drug offenders by a factor of three to enable efficient parameter estimation for less prevalent offense types (Bureau of Justice Statistics and Federal Bureau of Prisons 2000b). Not accounting for this undersampling, which is reflected in the sampling weights, could lead to seriously biased results. Accordingly, this research adopts a *design-based* analytic approach, which uses Stata's complex survey analysis capabilities to appropriately account for the Inmate Survey's complex sampling design.[29]

3.8.1.1 Subpopulation estimation As discussed in Section 3.4, this research focuses on the subpopulation of New Law sentenced drug offenders. Unlike with simple random sampling, subgroup analysis of complex survey data is not performed by simply selecting out the subsample of analytic interest; that approach can destroy the integrity of the underlying sampling design and lead to incorrect variance estimation (Lee and Forthofer 2006). Instead, proper subgroup analysis of complex survey data requires that *all* sample observations be included in the estimation process, which is accomplished by setting the sampling weights of observations falling outside the analytic domain to zero. In this way, all sample cases are retained for purposes of variance estimation but only the subsample cases are included in point estimation (Korn and Graubard 1999; Lee and Forthofer 2006). For this study, proper design-based subsample estimates, whether on the full

[29]An alternative methodology is a *model-based* approach, which advocates including design elements in an estimation model as auxiliary independent variables. The model-based approach is susceptible to model misspecification, however. This is especially problematic for secondary data analysts who have limited access to and knowledge of intricate survey design features; any such attempt at explicit modeling of the sampling design is likely to be highly misspecified (Binder and Roberts 2003).

subsample of New Law drug offenders or smaller subsets thereof, were obtained using Stata's complex survey subpopulation analysis procedures.

3.8.1.2 Degrees of freedom limitations In a traditional model-based context, the number of degrees of freedom is a function of sample size. In a design-based framework, the formula for degrees of freedom is *df* = #(sampled PSUs) – #(strata). With 40 sampled clusters and seven strata in the Inmate Survey, *df* = 40 – 7 = 33. Thus, with one constraint taken up by the constant term, at most 32 independent variables can be included in any single estimation model using federal Inmate Survey data. Although the model may otherwise be correctly specified, violating degrees of freedom limitations prevents Stata from reporting an overall model test (StataCorp 2005, 2003a, 2003b). Thus, at most 32 independent variables could be included in any single estimation model.

This limitation affects all multivariate analyses, but it has a particular bearing on modeling decisions for the firearm sentence enhancement analyses. Since the three possible outcomes (i.e., no FSE, Guideline FSE, 924(c) conviction) are mutually exclusive and independent, multinomial logistic regression would ordinarily be the model of choice to perform this analysis. Multinomial logistic regression is a multiple equation model that simultaneously tests all possible independent variable contrasts for *j*-1 categories of the dependent variable. However, in the design-based context, estimating a multinomial logistic regression model would exceed the 32 degrees of freedom limitation for providing an overall model test. Specifically, with a three-category dependent variable and 29 independent variables in the planned analysis, 58 degrees of freedom

would be required to perform an appropriate design-based model test (the adjusted *F* statistic in this case)—far exceeding the available 32 degrees of freedom.

There are several alternative model specifications that can address the degrees of freedom problem. One option would be to ignore the misspecification and assume the model parameters are valid despite the lack of a universal model test. As Stata's software documentation indicates, "There is no mechanical problem with your model, but you need to consider carefully whether any of the reported standard errors mean anything" (StataCorp 2005). Thus, the parameter estimates could be accepted by assuming that the model as a whole is significant and meaningful. A second option would be to relax the design-based specification in order to increase the degrees of freedom. For example, by ignoring clustering, degrees of freedom could be increased sufficiently to provide an overall model test. However, this would also distort the underlying sampling design and adversely affect variance estimation (Korn and Graubard 1999). As a final option, the analysis could be performed by running two independent binary logistic regression models on subsamples formed by the FSE categories. For example, 924(c) and non-FSE offenders would form one subsample, and Guideline FSE and non-FSE offenders would form the other. This method provides parallel, albeit less efficient, estimates to multinomial logistic regression (DeMaris 2004; Long 1997; Long and Freese 2001). The third option was adopted here because it allows for a universal test of model significance without too great a loss of efficiency. In addition, the postestimation options are more expansive and reliable under this specification.

3.8.2 Analysis of limited response data

Nonrepresentative samples of one form or another are pervasive in social science research (Berk 1983; Berk and Ray 1982). Often referred to as problems of "sample selection bias," limited response distributions are defined by nonrandom selection on a given response variable, *y*, creating a distribution that is not fully representative of the true range of values present in the original population (DeMaris 2004). Standard OLS analysis of nonrandomly selected samples tends to produce estimates that are biased and inconsistent. Notably, *this problem extends even to properly realized random samples of a target population* if the target population itself is a "selected" population of the original population (Berk and Ray 1982).

These concerns extend to the present research, because the population from which the Inmate Survey sample was drawn represents a nonrandomly selected subset of more severely sanctioned inmates relative to the original population of imprisoned inmates. That is, since inmates with shorter sentences are released earlier from prison, they are systematically underrepresented in the Inmate Survey's target population of federally imprisoned inmates. Not accounting for limited response in the estimation process has serious implications for the validity of the estimation results. Accordingly, this dissertation employs a truncated-censored regression model to counter the bias of OLS. The ensuing subsections expand on the general patterns and problems of limited response, and present a more detailed exposition of this study's approach to the problem.

3.8.2.1 Patterns of limited response Several mechanisms characterize samples with limited distributions on a continuous response variable: truncation, censoring, and

incidental selection (Breen 1996; DeMaris 2004; Maddala 1983). *Truncation* occurs when a sample is drawn from a selected population that has been restricted at some threshold value, *c*, of the response variable, *y*. Thus, sample data for all *x* and *y* variables are available only for cases in which *y* does not exceed *c*. For example, a survey of household incomes in which the target population has been limited to households with incomes 1.5 times the poverty line or below constitutes a truncated sample. No *x* or *y* data is available in the sample for households with incomes greater than 1.5 times the poverty line, so truncation represents a limitation in the population from which the sample is drawn.

Censoring, on the other hand, occurs when a sample is drawn from the full population, but the values of the response variable *y* have been constrained at some threshold value, *c* of *y*. For example, a survey of household incomes in which all observations above $100,000 were recorded as '≥ $100,000' constitutes a censored sample. Unlike with truncation, censored data are observed for all *x* and *y* variables. With censoring, then, the limitation lies not in the target population, but in the measurement of the response variable, *y*.

Truncation and censoring are sometimes referred to as explicit selection mechanisms, because whether *y* is observed depends on the values of *y* itself. In contrast, *incidental selection* occurs when a sample observation for *y* is observed only when another variable, *z*, achieves some threshold value, *c*. For example, a survey of household incomes in which only the sample members on welfare (*z*) reported the amount of their assistance income (*y*) constitutes an incidentally selected sample, because *y* is observed only when $z = 1$.

3.8.2.2 The problem of limited response OLS estimation tends to produce biased and inconsistent results in samples with limited response distributions (Breen 1996). Berk (1983; Berk and Ray 1982) frames the problem in terms of threats to both external and internal validity. In the presence of limited response, external validity is threatened because OLS estimates from a nonrandomly selected sample cannot hold for the population as a whole—the sample and population regression lines would differ as a function of the available data (Breen 1996). In the present study, for example, estimates from the subset of relatively more serious drug offenders who are in prison at any given time cannot automatically be generalized to the population of imprisoned drug offenders.

Internal validity is also threatened by OLS analysis of limited response data. To see why, take the regression equation

$$y_i = \beta X_i + u_i$$

where y_i is sentence length, X_i is a vector of independent variables, and the error term $u_i \sim IN(0, \sigma^2)$. For left-truncated data characteristic of the Inmate Survey, for example, we would observe y_i only if $y_i \quad c$, where c is a given threshold. This constraint implies that

$$\beta X_i + u_i \quad c, \text{ or equivalently } u_i \quad c - \beta X_i.$$

Therefore, the expectation, $E(u_i \mid u_i \quad c - \beta X_i)$, cannot equal zero; in fact, the residual will be correlated with the set of independent variables, X_i, producing inconsistent β estimates through the method of OLS (Breen 1996; DeMaris 2004; Maddala 1983). In the present context, β will be upward-biased and thereby overestimate the true effects of offense and offender factors on sentence length outcomes. Notably, the investigator "cannot dismiss the problem by claiming interest only in the nonrandom subset of cases represented by

the sample at hand" (Berk 1983: 388). OLS remains an improper method for the analysis of limited response data.

A variety of models have been developed to counter the threats to external and internal validity posed by OLS estimation of limited response data, where the choice of model depends on the pattern of limited response in the realized sample. These models include, for example, the truncated regression model for truncated data, the censored or tobit model for censored data, and the Heckman two-step model for incidentally selected data (Breen 1996; DeMaris 2004; Maddala 1983). As explained in the next section, this dissertation uses the truncated-censored regression model in analyses predicting sentence length due to the variable's specific pattern of limited response.

3.8.2.3 The truncated-censored regression model The Inmate Survey sample is both left-truncated and right-censored with respect to sentence length[30]—left and right simply identify the limited part of the distribution. Left-truncation is evident because, as noted above, inmates with shorter sentences are released from prison earlier than inmates with longer sentences. As an added complexity, the truncation point at which these cases go unobserved is not constant; it occurs at increasingly higher thresholds the further back in time inmates were sentenced. Right censoring is evident because the sentence length of inmates with an effective life or death sentence is constrained above a certain number of years, usually taken to be the effective maximum for non-lifers. Accounting for these

[30] The Inmate Survey sample could also be described as incidentally selected because of the nonrandom selection process inherent in the judge's in/out decision given conviction. However, it is not possible to model this selection process because the Inmate Survey sample contains no data on earlier stages in the criminal justice system.

limited response components define what is referred to here as the truncated-censored regression model with multiple thresholds. This estimator is not common in the literature and requires further elaboration.

The truncated-censored regression estimator is based on the method of maximum likelihood (ML). In a very general sense, ML operates by converging upon a set of parameter estimates having the greatest likelihood of generating the observed sample.[31] ML estimation requires derivation of a log-likelihood function, which is a specification of how the data are believed to have been realized. The specific log-likelihood function for the truncated-censored regression model employed in this study is a generalization of the censored regression model (Breen 1996; Maddala 1983). It has two distributional components regarding the underlying population variable y^*

$$y_i = y_i^* \text{ if } c_i \leq y_i^* \leq d_i \qquad (1a)$$

$$y_i = d_i \text{ if } y_i^* > d_i \qquad (1b)$$

where c_i is the lower truncation threshold and d_i the upper censoring threshold.

The complete log-likelihood is the sum of the individual log-likelihoods corresponding to each part of the distribution (Breen 1996; StataCorp 2003a). The log-likelihood corresponding to equation (1a) actually reduces to the log-likelihood for the truncated regression model (Breen 1996), where the lower truncation point, c_i, varies by

[31] ML estimators have several desirable large sample properties. First, they are asymptotically unbiased, i.e., as n approaches infinity, the estimator $E(\hat{\theta})$ converges on parameter θ. Second, they are consistent, i.e., as n approaches infinity, the sampling distribution of $\hat{\theta}$ becomes increasingly concentrated over θ. Third, they are asymptotically efficient, i.e., as n approaches infinity, the sampling variance is the smallest among consistent estimators. Finally, they are asymptotically normal, i.e., as n approaches infinity, the estimator's sampling distribution tends toward normality, even if the distribution of the original variable deviates from normality (DeMaris 2004).

observation. The log-likelihood corresponding to equation (1b) contributes a term relating to the probability that y^* exceeds the upper censoring threshold, d_i. This has been set to a constant of 63 years, which marks the longest prison term in the drug offender subsample that does not represent an effective life sentence. A total of 53 cases were censored at this point, including 47 cases with imposed life or death sentences and an additional five cases with effective life sentences of 87 to 120 years. In summary, the truncated-censored regression model with multiple thresholds is used in the present research to address problems of limited response with the primary dependent variable, sentence length.

3.8.2.4 Limited response and discrete choice models Two sets of planned multivariate analyses in this dissertation utilize binary response variables, namely those involving mandatory minimum exposure and the application of firearm sentence enhancements. Do the preceding concerns with limited response apply just as equally to discrete choice models? Once again, the answer depends on how the data were realized. For example, bias is a possible concern in discrete models with incidental selection, and a correction exists in the two-stage probit selection model. In the case of explicit selection, however, bias may not be an issue depending on the requirements of the analysis. For instance, even when there is explicit selection on a binary response variable, parameter estimates for logistic regression have been shown to be asymptotically unbiased with respect to β_1 coefficients and standard errors; any bias ends up concentrated in β_0, the constant term (Hosmer and Lemeshow 2000; Scott and Wild 1989). Thus, if β_1 is of primary interest,

which is the case with most research, then the bias that may be contained in β_0 will not hamper inference.

Researchers in the field of epidemiology were the first to demonstrate this property of the logistic regression model by showing that odds ratios were invariant between cohort studies, where sampling *is not* conditional on the response variable, and case-control studies, where sampling *is* conditional on the response variable (Breslow 1996). As Hosmer and Lemeshow (2000:208, emphasis in original) note, "the implication of this is that *analysis of data from case-control studies via logistic regression may proceed in the same way and using the same computer programs as cohort studies*." By extension, any survey in which selection depends on the response variable is, roughly speaking, a case-control study (Scott and Wild 1989), so the above implication is broadly applicable.

It is important to note, however, that these properties of the logit estimator were developed primarily from case-control studies with stratification on the response. Although the underlying theory and statistical properties are not as well developed for complex sampling designs, the general consensus is that these conditions are probably robust in a design-based framework (Hosmer and Lemeshow 2000; Scott and Wild 1989). In conclusion, concern with limited response on a binary dependent variable is moot given the favorable properties of the logit estimator. Thus, logistic regression constitutes the model of choice for analyses of binary sentencing outcomes.

3.8.3 Design-based maximum likelihood estimation in Stata

The complex survey and model estimator components of the analysis strategy have been discussed separately to this point, but they form an integrated suite of commands in Stata estimated via maximum likelihood methods. Maximum likelihood theory actually assumes sample observations are random and independent, but this condition is violated within the design-based framework (Lee and Forthofer 2006). Thus, as an approximation of the true likelihoods, Stata derives "pseudo-likelihoods" for complex survey analysis (Gould, Pitblado, and Sribney 2003; StataCorp 2003a, 2003b). The specific Stata commands for the truncated-censored and logistic regression models used in this dissertation are -svyintreg- and -svylogit-, respectively. The -svyintreg- command is a generally applicable command for interval, censored, and truncated regression analysis of survey data.

3.8.3.1 Model fit Stata does not report an R^2 analog for the design-based logistic and truncated-censored regression models, so a common measure of fit was calculated after each model estimation. McKelvey and Zavoina proposed a measure of fit equal to the ratio of the variances of the latent predicted dependent variable and the latent observed dependent variable, M&Z R^2 = Var(predicted y^*)/Var(y^*) (Long and Freese 2001). Since both the logistic and truncated-censored regression models can be defined in terms of a latent dependent variable y^*, McKelvey and Zavoina's pseudo-R^2 was reported for both estimators.

3.8.3.2 Postestimation analyses The analysis plan calls for a number of postestimation analyses, which consist of additional parameter tests and model predictions subsequent to the primary model estimation. In each instance, the postestimation analyses accounted for both the survey design and model-specific features of the preceding estimation. For example, postestimation tests for the equality of coefficients were performed using the adjusted Wald test, which provides an appropriate correction for design-based degrees of freedom restrictions (StataCorp 2003b).

Model predictions are also contingent upon the underlying estimation model, including functional form specifications. Thus, postestimation predictions of ln(sentence length) were exponentiated to return results in the original metric, and predictions involving binary dependent variables were reported as predicted probabilities. Moreover, depending on the particular aims of the analysis, postestimation predictions were performed by (1) allowing the independent variables to vary "as is," (2) setting the variables to their means, or (3) constraining them to specific values of interest. Particular combinations of these strategies were used to obtain predictions for a typical case or hypothetical situation. For analyses examining the 100-to-1 crack-powder cocaine quantity ratio, for example, adjusted postestimation predictions were used to estimate the mean prison term for crack offenders sentenced "as if" they were powder cocaine offenders.

3.9 DEALING WITH MISSING DATA

Missing data are a pervasive problem in social science research (Junger-Tas and Marshall 1999), yet most multivariate statistical methods were not developed to analyze

incomplete data matrices. When faced with missing data, investigators commonly employ complete-case methods that exclude from the analysis all units with missing values in the covariates. Unfortunately, this approach can lead to serious bias and inefficiency in estimation, especially when the amount of missing data is nontrivial. These concerns are compounded when making subpopulation inferences from complex survey data. First, missing data occurring within the target sample can distort the underlying weighting scheme, jeopardizing accurate inference to the target population. Second, missing data occurring anywhere in the full data matrix can alter the design-based variance estimates (Lee and Forthofer 2006; Little and Rubin 2002). These represent serious concerns for the present study since complete-case methods would discard fully one-quarter (433 of 1,671) of the cases in the drug subsample. Fortunately, there are a number of methods—some more sophisticated and defensible than others—that analysts can use to counter the problems posed by missing data. Knowing how the missing data were generated, the subject of the next section, is crucial to the choice of missing data method.

3.9.1 Missing data mechanisms

Missing or unobserved data can arise in a number of ways, whether by refusal, noncontact, or some other process. Survey methodologists frame the problem according the governing *missing data mechanism*, that is, how missingness is related to the observed values of study variables (Little and Rubin 2002). The missing data mechanism is considered 'nonignorable' if the data are *not missing at random* (NMAR), i.e., missingness depends on unobserved values of *y*. Nonignorable missing data mechanisms must be modeled as part of the estimation process in order to achieve good parameter

estimates. This should sound familiar to the reader, because NMAR characterizes the problem of limited response within a missing data framework. The Inmate Survey's nonignorable missing data mechanism, characterized by left-truncation and right-censoring, was fully addressed in Section 3.8.2 and its subsections under this limited response framework. The remainder of this section, therefore, addresses specific problems of 'ignorable' missing data.

The missing data mechanism is considered 'ignorable' if the data are (a) *missing at random* (MAR), i.e., missingness is unrelated to observed values of *y*, and (b) *distinct*, i.e., missingness is unrelated to the parameters to be estimated (Allison 2002; Little and Rubin 2002). Ignorability essentially means that the missing data mechanism does not need to be modeled in the estimation process; it does *not* mean that missing data can be disregarded without consequence to the estimation results.[32] Ignorable missing data arises from both unit and item nonresponse. Unit nonresponse occurs when a subset of sampled individuals fails to complete the interview due to noncontact or refusal to participate. Item nonresponse occurs when respondents refuse or are unable to answer specific questionnaire items. It happens that both types of nonresponse affect the Inmate Survey. The following subsections describe the specific methods implemented to counter problems of bias and inefficiency associated with missing data.

[32] Under the more restrictive *missing completely at random* (MCAR) assumption, i.e., missingness is unrelated to both observed and missing values of *y*, complete-case analysis would produce unbiased (though less efficient) results because the observed data are a random subsample of the original data. For this reason, complete-case analysis is often preferable when the data are MCAR because it avoids the inferential errors that go along with missing data methods. In practice, however, data are rarely MCAR, so some type of correction for ignorable missing data usually is warranted (Allison 2002).

3.9.2 Missing data methods for unit nonresponse

The Inmate Survey had a total unit nonresponse rate of 9.8%, as 438 of 4,479 selected inmates refused to participate in the study. Although an overall response rate greater than 90 percent is exceptional, bias could be introduced if certain groups were less likely to cooperate. Typically, unit nonresponse is handled by the data collection agency because it has access to auxiliary information on nonresponders. BJS/BOP (2000b) dealt with unit nonresponse in the Inmate Survey by adjusting the base sampling weights as a function of nonresponse rates within age-gender-race-stratum subgroups. These adjustments were incorporated into the survey's final sampling weights and, consequently, factored into this study's design-based analyses. It is important to note, however, that biases will remain to the extent that respondents and nonrespondents differed *within* the adjustment groups formed by BJS/BOP.

3.9.3 Missing data methods for item nonresponse

The task of dealing with item nonresponse is usually left to the secondary data analyst (Lee and Forthofer 2006; Lehtonen and Pahkinen 2004). The amount of missing data on individual study variables in the drug subsample ranged from zero percent for gender to 11 percent for powder cocaine quantity. Total item nonresponse on study variables would result in about 25 percent of drug subsample cases being dropped from a complete case analysis, leading to potentially severe bias and loss of efficiency. To counter these effects, a sequence of imputation procedures was employed to obtain a complete data matrix for this study's analyses. Recall from Section 3.8.1.1 that subpopulation estimation of complex survey data requires that all sample observations be included in

the estimation process. Accordingly, missing data on study variables were filled in not just for the drug subsample but for the full Inmate Survey sample of 4,025 cases.

3.9.3.1 Missing value inference from auxiliary information As a first step, missing values were inferred from a supplementary alphanumeric datafile if at all possible. The conviction offense information in this supplementary datafile typically denoted detailed aspects of the offender's role and the type of drugs involved (e.g., "Importation of Heroin" and "Manufacturing Methamphetamine"), information that was used to infer valid responses on these measures. Recall that about one-quarter of drug subsample cases did not report a specific role in the offense, so the supplementary datafile was particularly effective for augmenting these survey items.

3.9.3.2 Missing value imputation After making these inferences from the supplementary alphanumeric datafile, stochastic regression imputation was used to predict the remaining missing values. Stochastic regression imputation operates by executing "random draws from a predictive distribution of plausible values…rather than from the center of this distribution" (Little and Rubin 2002: 64). Thus, the main advantage of this approach is that it ameliorates the best prediction distortions introduced through other methods (e.g., conditional mean imputation). The main criticism of stochastic regression imputation is that it does not go far enough in accounting for imputation uncertainty. As a practical matter, however, more advanced imputation methods either have not been implemented in standard statistical software, call for

specialized knowledge, or require complete access to auxiliary survey design variables.[33] Thus, compared to feasible alternatives, stochastic regression imputation presented the most advanced option for reducing nonresponse bias and improving the precision and validity of the estimates.

Procedurally, stochastic regression imputation was performed using Stata's -uvis- (univariate imputation sampling) command (Royston 2004). Sampling weights and strata were accounted for in the imputation model, although it was not possible to adjust for clustering. As a general rule, imputation was performed not with the final constructed variables, but with the original precursor variables. For example, the individual drug quantity measures were imputed, not the combined marijuana equivalent quantity measure. As recommended by Little and Rubin (2002), study covariates with missing data were imputed from study covariates with observed data, with the dependent variable and a dummy indicator for censoring on the dependent variable included in the list of covariates. Conditioning on observed covariates preserves the multivariate associations between observed and missing variables. In general, variables with the least amount of missing data were imputed first. Once a variable was imputed, it was eligible for inclusion as a covariate in subsequent imputations. The underlying estimation model called by -uvis- (e.g., logistic or standard regression) corresponded to the imputed

[33] These methods include explicit and unique variance estimators, bootstrap or jackknife resampling techniques, and multiple imputation (Little and Rubin 2002). Multiple imputation (MI) is probably the current gold standard and, as implemented in Stata, performs stochastic regression imputation *n* times creating *n* complete data sets ($n = 5$ is the generally recommended minimum). Results from *n* independent analyses of the *n* data sets are then combined into a single set of estimates, thereby incorporating between-imputation uncertainty across the *n* data sets. Unfortunately, in addition to their added complexity and computational intensity, MI procedures for complex survey data are not fully integrated into the Stata platform.

variable's level of measurement. Finally, if the imputed values for a variable had logical implications for missing values on other variables, then these values were filled in prior to subsequent imputations. For example, missing values for heroin quantity were set to zero if the prediction for heroin offense status was negative (i.e., '0' versus '1'). In summary, the preceding missing data methods for item nonresponse resulted in a complete data matrix.

3.10 SENSITIVITY ANALYSIS

The multivariate analytic and missing data methods introduced in Sections 3.8 and 3.9 include many assumptions and modeling decisions. In order to assess their impact on estimation results, Appendix A presents a sensitivity analysis that alternately relaxes each modeling assumption for the main multivariate analysis regressing sentence length on offense and offender characteristics. In total, a complete set of eight models were estimated with respect to the following dichotomies: the truncated-censored vs. OLS estimator, design-based vs. model-based estimation, and imputed vs. complete case analysis. With few exceptions, the coefficients were markedly similar across models. Generally, differences were more likely concerning statistical significance. The overall pattern of results supports the analytic strategies and modeling decisions adopted throughout this research.

4.0 RESULTS

As stated in Chapter 1, this study examines problems of unwarranted disparity in federal drug sentencing outcomes. Presentation of the results in this chapter is organized around the research questions and hypotheses discussed in Chapters 1 and 2 and summarized in Table 3.1. Section 4.1 examines the association between drug quantity and the culpability- and dangerousness-based offense seriousness factors. Section 4.2, which introduces the multivariate analyses, investigates the relative influence of the legally relevant offense seriousness factors on sentence length. Section 4.3 examines evidence for disparate outcomes related to the 100-to-1 crack-powder quantity ratio. Sections 4.4 and 4.5 investigate the tariff and cliff effects, respectively, that arise from the interplay between guidelines and mandatory minimum sentencing. Finally, Section 4.6 investigates the circumvention of firearm sentence enhancements in federal drug sentencing outcomes.

As noted in Chapter 3, the data for these analyses come from the *Survey of Inmates in Federal Correctional Facilities, 1997*. The main subsample includes guideline-era drug offenders, representing an estimated 55,481 drug inmates or about 62% of all federal inmates. Table 4.1 presents the sample *n* and weighted *N* frequency distributions by role in the offense and drug type. Certain roles are more prevalent for particular drugs. For example, producing is hardly represented among heroin offenders but is the most populous role among methamphetamine offenders. Several of the

Table 4.1: Sample *n* and Weighted *N* Frequency Distributions by Role in the Offense and Primary Drug Type

Role in the Offense	Primary Drug Type													
	Heroin		Meth-amphetamine		Crack Cocaine		Powder Cocaine		Marijuana		Other Drugs		Row Total	
	n	*N*	*n*	*N*	*n*	*N*	*n*	*N*	*n*	*N*	*n*	*N*	*n*	*N*
Laundering	8	171	12	279	15	353	69	1,942	33	684	1	3	138	3,432
Importing	46	949	12	419	12	350	96	3,011	69	2,321	1	13	236	7,063
Producing	1	13	51	1,960	15	428	4	155	21	728	7	276	99	3,559
Wholesaling	25	689	34	855	102	3,609	97	3,568	33	1,153	11	370	302	10,244
Distributing NOS	40	1,348	30	878	136	4,479	207	7,320	62	2,038	9	287	484	16,350
Retailing	23	863	32	1,074	142	5,135	87	3,221	10	361	10	387	304	11,041
Possessing	7	237	12	394	22	778	41	1,354	20	848	6	180	108	3,792
Column Total	150	4,270	183	5,860	444	15,132	601	20,571	248	8,132	45	1,516	1,671	55,481

Note: *n* represents raw sample observations and *N* represents weighted estimates.

following analyses are performed within subsamples formed by these primary drug type stratifications. Accordingly, for certain subgroup analyses, some categories are collapsed due to small *n* considerations. Lastly, as referenced in the text, additional tables and figures that supplement specific analyses are included in Appendix B.

4.1 THE RELATIONSHIP BETWEEN DRUG QUANTITY AND OTHER OFFENSE SERIOUSNESS FACTORS

This section examines the extent to which drug quantity, as the foremost legal determinant of federal drug sentences, is an adequate proxy for other elements of offense seriousness. In particular, the analysis evaluates how well drug quantity and quantity-based mandatory minimums correlate with culpability- and dangerousness-based offense seriousness factors. The following specific associations are examined: (1) drug quantity by role in the offense, (2) mandatory minimum exposure by role in the offense, (3) mandatory minimum exposure by organizational role, (4) drug quantity by the use or possession of a firearm during the offense, and (5) drug quantity by firearm sentence enhancement outcomes. Spearman's rho (r_s), which has a similar interpretation to Pearson's *r*, is used to assess the bivariate correlation between measures. Boxplots and bar graphs of the bivariate distributions supplement the statistical analysis. The boxplots exclude outside values for presentation purposes.

4.1.1 Drug quantity and role in the offense

The association between drug quantity and role in the offense is examined independently for each of the five primary drug types: heroin, methamphetamine, crack cocaine, powder

cocaine, and marijuana. As indicated in Table 4.1, however, the number of sample observations for certain role by drug type combinations is too sparse to provide stable quantity estimates. For example, there is only a single observation representing offenders involved in heroin production. Thus, the boxplot graphs omit roles for which the corresponding sample $n < 10$. However, all data points are included in the Spearman's rho calculations.

4.1.1.1 Heroin quantity and role in the offense Among primary heroin offenders (N = 4,270), there is a weak positive correlation between heroin quantity and role in the offense ($r_s = 0.28$, $n = 150$, $p < 0.001$). Figure 4.1 shows the boxplot of this association, excluding money laundering, producing, and possessing roles due to small *n*. As the graph depicts, the statistical association is weak because of the substantial quantity overlap across roles. Nevertheless, in terms of central tendencies, the graph clearly shows three distinct quantity tiers. With a median (P_{50}) quantity of 14 grams, heroin retailing clearly represents the bottom tier. The middle tier consists of unspecified distributing and wholesaling roles with median quantities of 344 grams and 200 grams, respectively, and nearly identical interquartile distributions. Finally, heroin importing, with a median quantity of just under one kilogram (P_{50} = 930g), comprises the top tier. In short, these results confirm a significant positive, albeit weak, association between the quantity of heroin trafficked and the defendant's level of culpability.

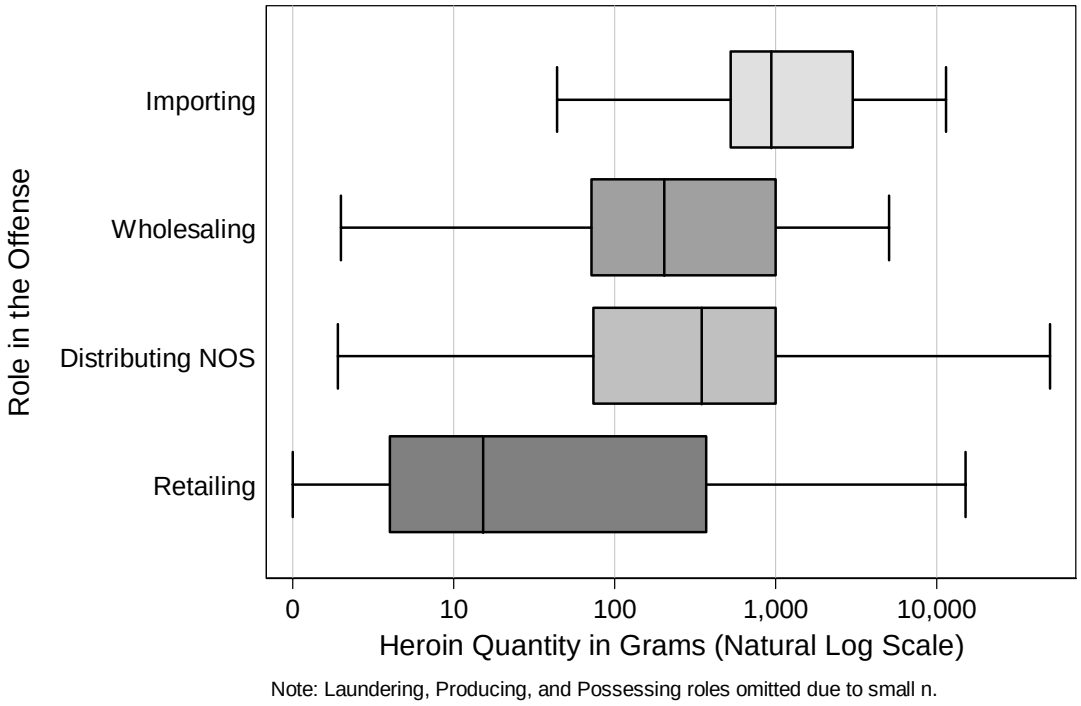

Figure 4.1: Boxplot of Heroin Quantity by Role in the Offense

4.1.1.2 Methamphetamine quantity and role in the offense Among primary methamphetamine offenders (N = 5,860), there is a fairly weak positive correlation between methamphetamine quantity and role in the offense (r_s = 0.31, n = 183, p < 0.001). Figure 4.2 shows the boxplot distribution. Once again, while there are substantial quantity overlaps, three fairly distinct tiers form across the various roles. For example, median methamphetamine quantities for the bottom tier roles of possessing (P_{50} = 193g) and retailing (P_{50} = 225g) are right around 200 grams, whereas median quantities for the middle tier of unspecified distributing and wholesaling roles are roughly twice as much at 454 grams. Finally, median methamphetamine quantities are in the 1 to 1.5 kilogram range for the top tier, which includes producing (P_{50} = 1,532g), importing (P_{50} = 1,068g), and money laundering (P_{50} = 1,000g). Thus, these results also confirm a significant

positive but weak association between the quantity of methamphetamine trafficked and the defendant's level of culpability.

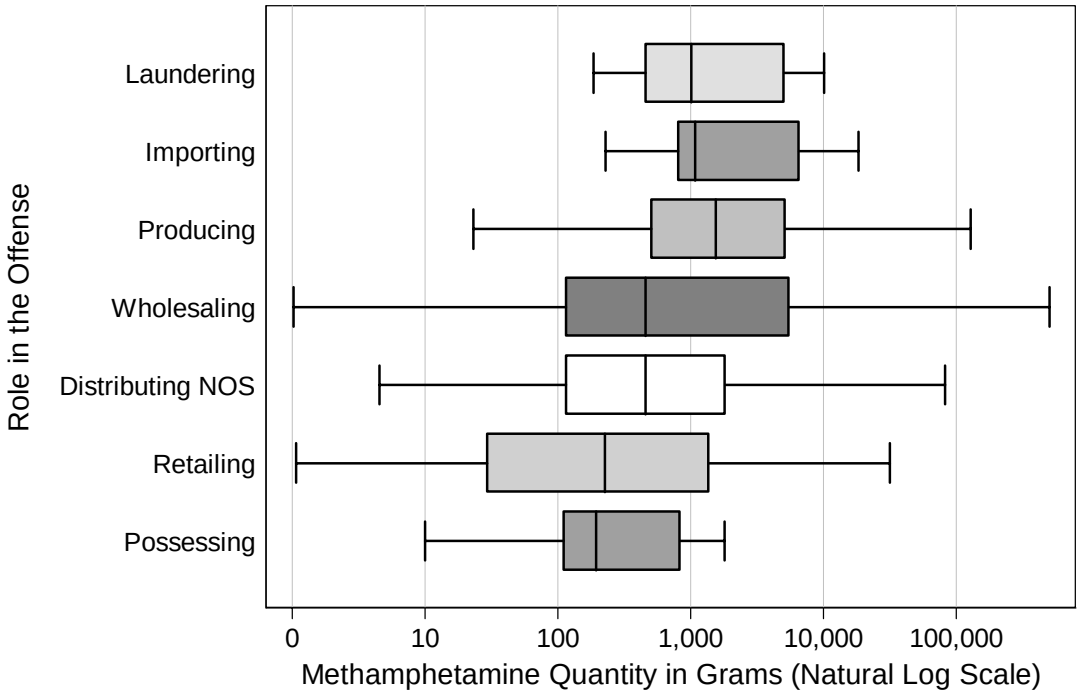

Figure 4.2: Boxplot of Methamphetamine Quantity by Role in the Offense

4.1.1.3 Crack cocaine quantity and role in the offense Among primary crack cocaine offenders (N = 15,132), the correlation between crack quantity and role in the offense is also positive but weak (r_s = 0.28, n = 444, p < 0.001). Figure 4.3 presents the boxplot graph of this association. The wide distributional overlap in quantity across roles explains the relatively weak association. Similar to the other drugs, the quantity distributions coalesce into tiers across the various roles, although the demarcation points are less distinct. A reasonable division might place possessing (P_{50} = 60g) and retailing (P_{50} = 62g) in the bottom tier and money laundering in the top tier (P_{50} = 500 g), with

unspecified distributing (P_{50} = 109g), wholesaling (P_{50} = 195g), producing (P_{50} = 251g), and importing (P_{50} = 220g) in a middle tier. In summary, although there is wide variation across roles and the relationship is fairly weak, crack offenders of greater culpability tend to traffic in larger quantities

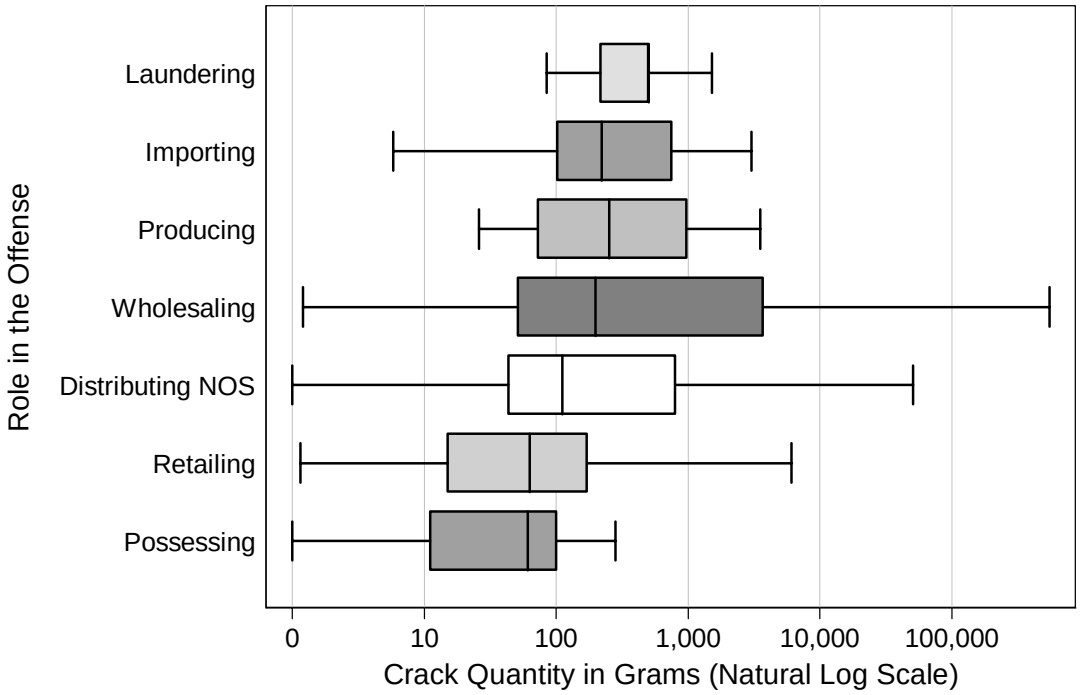

Figure 4.3: Boxplot of Crack Cocaine Quantity by Role in the Offense

4.1.1.4 Powder cocaine quantity and role in the offense Among primary powder cocaine offenders (N = 20,571), there is a weak positive correlation between powder cocaine quantity and role in the offense (r_s = 0.27, n = 601, p < 0.001). Figure 4.4 shows this association graphically, excluding the producing role due to small n. Note that quantity is now expressed in kilograms. Once again, three quantity tiers are evident. Powder cocaine possessing (P_{50} = 1,000g) and retailing (P_{50} = 700g) offenders comprise

the bottom tier with median amounts of one kilogram or less. The middle tier consists of unspecified distributing (P_{50} = 3,429g) and wholesaling (P_{50} = 2,268g) roles involving median quantities between one and five kilograms. The top tier includes importing (P_{50} = 5kg) and money laundering (P_{50} = 10kg) with median quantities of five kilograms or more. Thus, powder cocaine is similar to the other drugs in that offenders of greater culpability traffic in larger drug quantities, although the association is fairly weak due once again to the considerable overlap in powder cocaine quantity across roles.

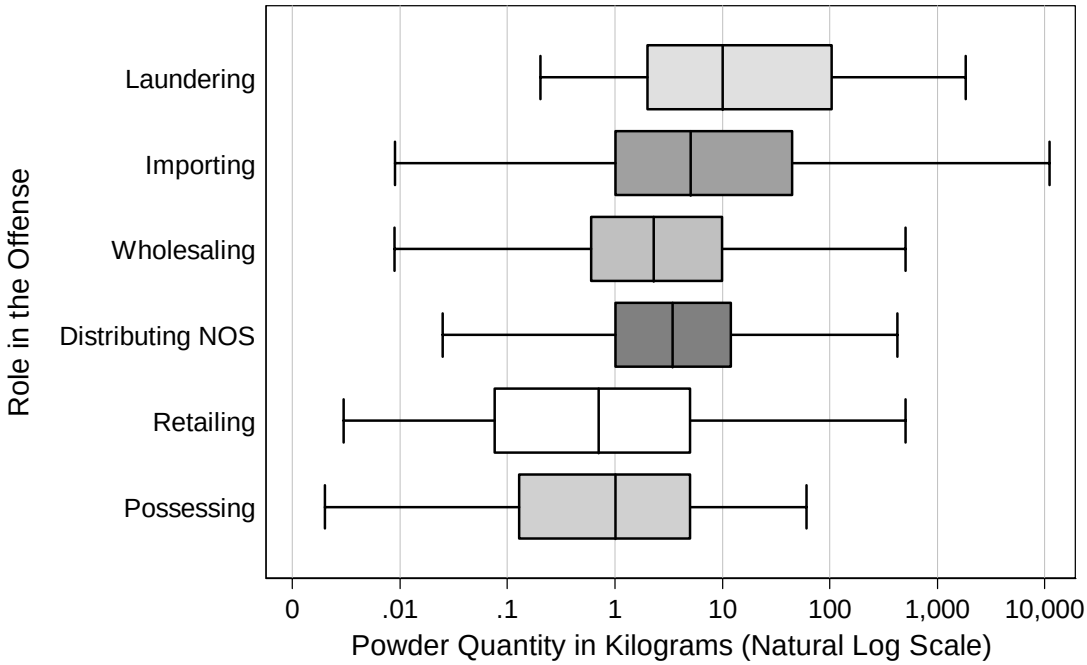

Figure 4.4: Boxplot of Powder Cocaine Quantity by Role in the Offense

4.1.1.5 Marijuana quantity and role in the offense In contrast to the other drugs, there is no statistically significant association between marijuana quantity and role in the offense among the estimated 8,132 primary marijuana offenders (r_s = 0.04, n = 248, p =

0.506). The lack of association is evident from Figure 4.5. With a median quantity less than 100 kilograms, possessing is the only that role stands out from the others; otherwise, there is no discernable pattern between the amounts of marijuana trafficked and levels of culpability.

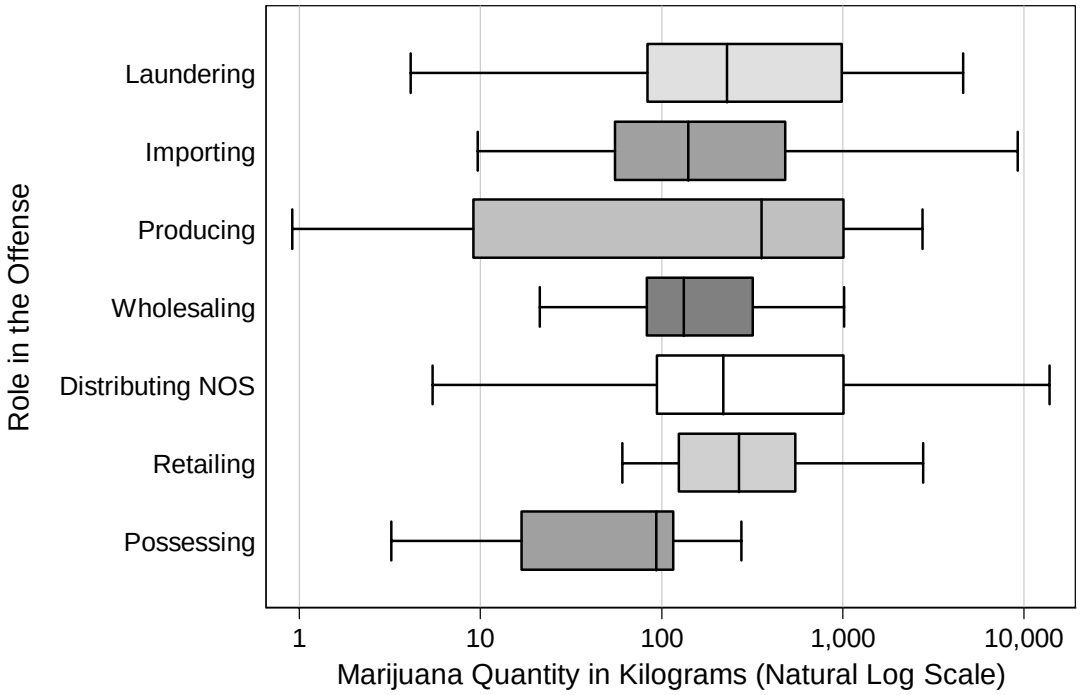

Figure 4.5: Boxplot of Marijuana Quantity by Role in the Offense

4.1.2 Mandatory minimum exposure and role in the offense

This section examines quantity-based mandatory minimum sentencing exposure across levels of culpability. Combining data for all drugs, Figure 4.6 presents a stacked bar graph of mandatory minimum exposure by role in the offense.[34] Overall, the correlation

[34] Refer to Table B1 in Appendix B for the sample *n* and weighted *N* frequency distributions corresponding to this analysis.

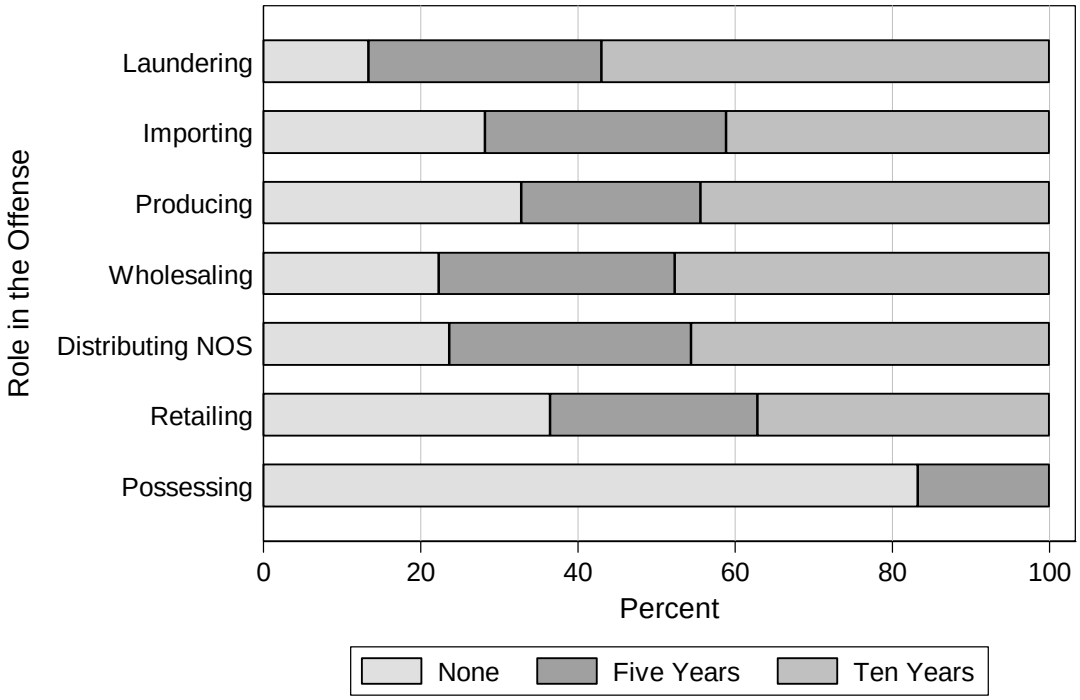

Figure 4.6: Mandatory Minimum Sentencing Exposure by Role in the Offense

between the two measures is positive but extremely weak (r_s = 0.19, n = 1,671, p < 0.001). As the graph makes clear, however, there is a distinct difference between the least and most culpable roles. Offenders at the level of possessing have the smallest share of defendants (17%) exposed to mandatory minimum sentencing.[35] At the other extreme, 87% of money laundering offenders face either the five- or ten-year mandatory minimum. Between these two extremes, there is a slight trend of increasing mandatory minimum exposure from the retailing through wholesaling roles, but this breaks down for importing and producing. In sum, mandatory minimum sentencing exposure has a very

[35] Since crack is the only drug for which mandatory sentencing applies to simple possession behavior, this 17% is comprised entirely of crack cocaine offenders subject to the five-year mandatory minimum.

weak positive correlation with role in the offense. Of particular note, however, is the breakdown in proportionality with respect to mandatory minimum exposure for the higher-level roles of importing and producing.

4.1.3 Mandatory minimum exposure and organizational role

Culpability is also reflected in whether a defendant participated in an organized drug trafficking group and, if so, what the defendant's role was within that organization. Figure 4.7 presents a bar graph of the association between mandatory minimum exposure and organizational role.[36] As the graph suggests, the correlation between mandatory minimum exposure and role in the offense is positive but extremely weak to the point of being nearly linear (r_s = 0.08, n = 1,671, $p < 0.001$). The most noticeable aspect of this association is that only 5% of the peripherally involved coconspirators escape mandatory minimum sentencing exposure. While these peripheral role offenders account for just 295 individuals, 39% are women. The overall female proportion in the sample is 8%, and no other organizational role contains more than 14% women (underlings). Thus, few of these small-time participants actually end up in prison, but exposure to mandatory sentencing plays a big part when they do—particularly for female coconspirators. Aside from this disparate outcome, there is a perceptible trend of increasing mandatory minimum exposure as a function of greater responsibility and leadership within a drug trafficking organization. As a practical matter, however, the difference between mandatory

[36] Just 271 of 1,671 respondents reported organizational involvement, so small *n* considerations preclude splitting the analyses by drug type as with the role in the offense in Section 4.1.1. Refer to Table B2 in Appendix B for the sample *n* and weighted *N* frequency distributions corresponding to this analysis.

minimum exposure rates for nonmembers (68%) and leaders of organizations (75%) is small.

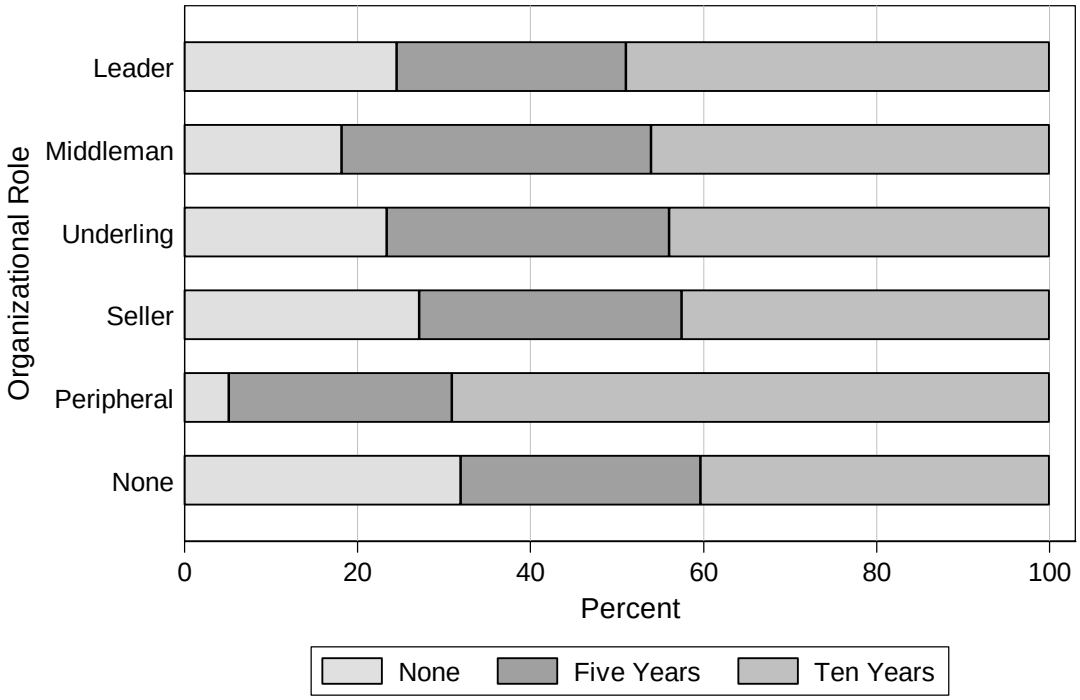

Figure 4.7: Mandatory Minimum Sentencing Exposure by Organizational Role

4.1.4 Drug quantity and firearms use

This section turns the focus to the dangerousness-based offense seriousness factors. Figure 4.8 shows the distribution of drug quantity by the acknowledged use or possession of a firearm during the offense.[37] This association is assessed independently for each of the five primary drug types. As the graph shows, drug quantity is inversely related to firearms activity in each instance. That is, offenders who reported using or possessing a

[37] Refer to Table B3 in Appendix B for the sample *n* and weighted *N* frequency distributions corresponding to the analyses in this and the following section.

firearm are responsible for smaller median drug quantities than offenders who did not acknowledge such weapon activity. However, only the association for powder cocaine offenders is statistically significant (r_s = -0.12, n = 601, p < 0.01). Thus, despite a general tendency for more dangerous drug offenders to possess smaller drug quantities, this conclusion applies only to powder cocaine offenders with any degree of confidence.

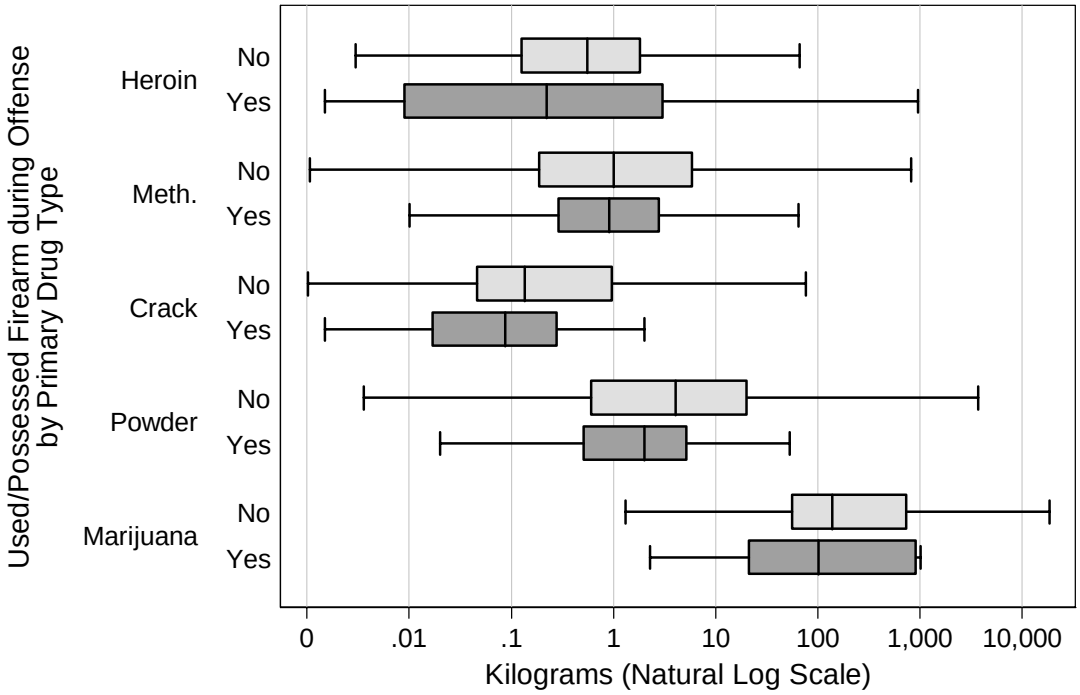

Figure 4.8: Boxplot of Drug Quantity by Firearm Use and Drug Type

4.1.5 Drug quantity and firearm sentence enhancement status

Irrespective of self-reported firearm use, the application of a firearm sentence enhancement (FSE) is a marker of perceived offender dangerousness. Recall that a 924(c) conviction generally carries greater sentencing potential than the Guideline FSE (USSG §2D1.1.) and is, therefore, considered the more severe enhancement. Figure 4.9 shows

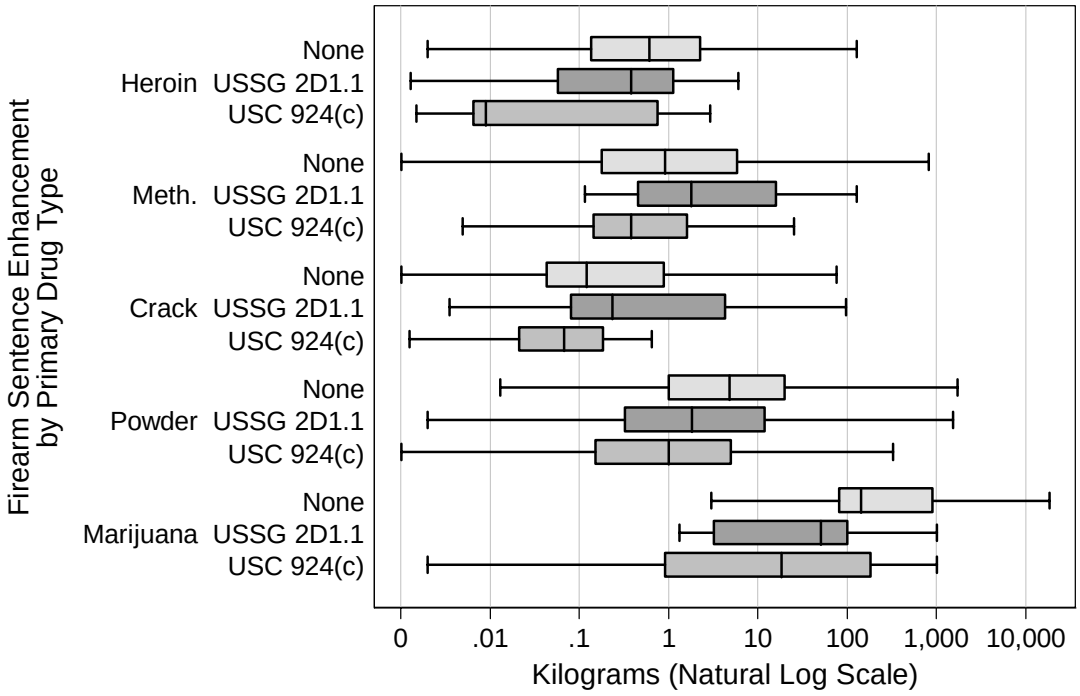

Figure 4.9: Boxplot of Drug Quantity by FSE Status and Drug Type

the distribution of drug quantity by FSE status for each of the five primary drug types. The graph shows that in general drug quantity is inversely associated with firearm sentence enhancement status. That is, offenders with a 924(c) conviction are responsible for smaller median quantities than those who received the Guideline FSE, and recipients of the Guideline FSE are in turn responsible for smaller median quantities than offenders who received neither enhancement. Exceptions to this pattern occur for methamphetamine and crack cocaine offenders. For these two drugs, a 924(c) conviction is still associated with the smallest median drug quantities, but recipients of the Guideline FSE alternatively possess the largest median quantities. Because of this curvilinear association, statistical significance is achieved only for heroin (r_s = -0.18, n = 150, p < 0.05), powder cocaine (r_s = -0.16, n = 601, p < 0.001), and marijuana (r_s = -0.25, n = 248,

$p < 0.001$) offenders. In short, these results also reveal that the more dangerous offenders are responsible for smaller drug quantities on average.

The findings in this and the preceding subsection raise the question: Why do more dangerous offenders traffic in smaller quantities of drugs? The answer lies, at least partly, in the differential rates of weapon use across roles in the offense. In particular, for all drugs combined, possessing (10.0%) and retailing (11.0%) offenders are more than twice as likely as laundering (4.8%) and importing (4.9%) offenders to report using or possessing a firearm during their offense, with the rates for the other roles generally falling in between (see Figure B1 in Appendix B). Thus, the more dangerous firearm-involved offenders tend to operate at the lower distribution levels, which is also where the average drug quantities are smaller.

4.1.6 Summary of bivariate offense factor analyses

These analyses examined how well drug quantity and quantity-based mandatory minimum sentencing correlate with culpability- and dangerousness-based offense factors. As detailed in Section 4.1.1, the culpability-based factors are generally positively correlated with the amount of drugs trafficked and the defendant's role in the offense. However, with Spearman's rho coefficients in the .27 to .31 range, these associations are fairly weak. The graphical analyses reveal a tendency for drug quantity to coalesce into distinct tiers across roles. For example, possessors and retailers generally form the bottom tier; unspecified distributors and wholesalers the middle tier; and producers, importers, and launderers the top tier. Nevertheless, the boxplot distributions indicate the statistical associations are not stronger because of the wide overlap in drug amounts across roles.

For example, one-quarter of both powder cocaine possessors and importers were held responsible for between 1 and 5 kilograms of powder cocaine, although for possessors these quantities represented the third quartile and for importers the second quartile. Thus, while quantity distinguished powder cocaine possessors and importers on average, fully one-quarter from each group were held responsible for similar quantities.

As detailed in Sections 4.1.2 and 4.1.3, the correlations between mandatory minimum exposure and culpability-based factors are also significant and positive, but generally weaker. Indeed, recognizing certain exceptions, these results are more suggestive of the relative uniformity in mandatory minimum exposure across both roles in the offense and roles within drug trafficking organizations. The notable exception for organizational participants concerns peripherally involved coconspirators who experience greater mandatory minimum sentencing exposure than more culpable leaders and middle men. Moreover, although few peripheral participants end up in prison—they account for less than 1% of all drug offenders—they are disproportionately women.

The dangerousness-based factors, in contrast, are generally negatively correlated with drug quantity, although significant outcomes occur only with respect to (1) firearms use for powder cocaine offenders and (2) firearm sentence enhancement outcomes for heroin, powder cocaine, and marijuana offenders. Part of the reason for this generally inverse association, however, is that the more dangerous firearm-involved offenders are more likely to function at the lower distribution levels where drug quantities are smaller. It is also likely that selection issues are operating in that it takes smaller drug quantities to expose dangerous offenders to prison time.

4.2 THE RELATIVE INFLUENCE OF DRUG QUANTITY AND OTHER OFFENSE FACTORS ON SENTENCE LENGTH

This section introduces the main multivariate analysis predicting sentencing outcomes from a range of offense and offender characteristics. The primary objective is to compare the relative effects of drug quantity and other offense seriousness factors on sentence length, after controlling for other variables. A secondary objective involves interpreting the case processing and sociodemographic factors in light of the offense seriousness outcomes. Statistics are reported for regression coefficients (β) and standard errors, percent change, and standardized regression coefficients (b^s). For models with log-transformed dependent variables, the percent change statistic provides a more accurate estimate of effect size than β (DeMaris 2004; Kennedy 1981). Because the independent variables are coded in different metrics that prevent direct comparison, the standardized betas (b^s) are calculated to provide a commensurate measure of effect size. Finally, the truncated-censored regression model reports both a constant and an error (sigma) term; only β coefficients and standard errors are reported for these terms.

4.2.1 Offense and offender seriousness factors

The overall model is statistically significant and explains a moderately large share of the variance ($F(31, 3) = 77.23$; $p < 0.01$; M&Z $R^2 = .46$). The design-based truncated-censored regression estimates are reported in Table 4.2. The results indicate that a 1% increase in marijuana equivalent drug quantity results in a significant 0.07% net increase in sentence length. Although this is a comparatively small effect in terms of percent change, the standardized beta coefficient ($b^s = .241$) reveals that drug quantity is actually

the strongest predictor in the model, net of other factors. Notably, exposure to the five- or ten-year mandatory minimum does not significantly impact sentence length after partialing out the effects of drug quantity. Better measures of mandatory minimum status would probably change the results. Among the drug type factors, only the effect for marijuana is significant. In particular, marijuana offenders receive a 27% net sentence benefit relative to powder cocaine offenders—ranking it among the top five predictors in the model (b^s = -.133).

Two general conclusions can be drawn about the effects of the role in the offense variables. First, in contrast to the effects of drug quantity, an offender's role in the offense has relatively little influence on sentence length. Indeed, the standardized beta effect sizes for all seven roles are in the bottom half of all predictors. Second, the effects of the role in the offense variables themselves are not proportionately scaled to their levels of culpability. For example, sentence lengths for offenders involved with importing, wholesaling, unspecified distributing, retailing, and possessing are not statistically different from the average offender, or from each other.[38] Only the effects for money laundering and producing achieve significance. However, despite being atop the chain of culpability, money laundering offenders actually receive 13% shorter net sentences relative to offenders of average culpability. Only the 22% net sentence increase for producing appears to fit in relation to the lower-level roles. Even if we limit our attention to the eight in ten offenders with roles at producing or below (44,986 of 55,481) where sentences are not expressly disproportionate, sentences are nevertheless extremely

[38] All tests of the equality of coefficients among these five roles failed to reject the null hypothesis (not shown).

Table 4.2: Design-Based Truncated-Censored Regression Predicting ln(Sentence Length)

Independent Variables (0-1 Dummies Unless Noted)	β [S.E.]	Percent Change[a]	b^s [Rank]
ln(Marijuana Equivalent Grams)	0.066 [0.016]***	0.07%	.241 [1]
Five-Year Mandatory Minimum	0.049 [0.051]	4.9%	.026 [26]
Ten-Year Mandatory Minimum	0.116 [0.078]	12.0%	.067 [15]
Primary Drug Type (Reference: Powder Cocaine)			
Heroin	-0.048 [0.086]	-5.1%	-.015 [31]
Methamphetamine	-0.099 [0.081]	-9.7%	-.036 [22]
Crack Cocaine	-0.091 [0.056]	-8.9%	-.048 [20]
Marijuana	-0.319 [0.054]***	-27.4%	-.133 [5]
Other Drugs	-0.100 [0.164]	-10.7%	-.019 [28]
Highest Role in the Offense (Effect Coded)			
Money Laundering	-0.132 [0.049]*	-12.5%	-.038 [21]
Importing	-0.048 [0.043]	-4.7%	-.019 [28]
Producing	0.203 [0.066]**	22.2%	.059 [17]
Wholesaling	0.038 [0.054]	3.7%	.017 [30]
Distributing NOS	0.053 [0.043]	5.4%	.029 [24]
Retailing	0.057 [0.051]	5.7%	.027 [25]
Possessing	-0.170 [0.104]	-16.1%	-.051 [18]
Aggravating Role Adjustment (0-3)	0.131 [0.038]**	13.9%	.092 [8]
Mitigating Role Adjustment (0-3)	0.063 [0.035]	6.5%	.034 [23]
Safety Valve Eligibility	-0.288 [0.050]***	-25.2%	-.140 [4]
USC 924(c) Conviction	0.325 [0.076]***	38.0%	.110 [6]
Guideline FSE	0.219 [0.059]***	24.3%	.079 [12]
Used Firearm During Offense	-0.015 [0.062]	-1.7%	-.005 [32]
Criminal History Category (1-6)	0.040 [0.015]*	4.0%	.069 [14]
Guilty Plea	-0.428 [0.046]***	-34.9%	-.237 [2]
Charge Bargain	-0.185 [0.044]***	-16.9%	-.109 [7]
Pretrial Release	-0.342 [0.041]***	-29.1%	-.194 [3]

Table 4.1 (continued)

Race (Reference: Black)			
White	-0.181 [0.066]**	-16.8%	-.089 [9]
Hispanic	-0.147 [0.057]*	-13.8%	-.081 [11]
Other Race	-0.102 [0.124]	-10.4%	-.021 [27]
Male	0.273 [0.070]***	31.0%	.089 [9]
ln(Age at Offense)	0.204 [0.057]**	0.2%	.070 [13]
Years Education Completed (0-18)	-0.017 [0.006]*	-1.7%	-.061 [16]
Non-U.S. Citizen	-0.100 [0.044]*	-9.6%	-.049 [19]
Constant	3.532 [0.323]***	--	--
Sigma	-0.408 [0.026]***	--	--

*p<0.05, **p<0.01, ***p<0.001

Model: F(31, 3) = 77.23; p = 0.002; McKelvey & Zavoina's pseudo-R^2 = .46
Number of strata = 7; Number of PSUs = 40
Population (all sentenced inmates): n = 4,025; N = 88,807
Subpopulation: n = 1,671 (53 right-censored); N = 55,480

[a] Denotes the percent change in y for a corresponding unit, percent, or discrete change in x, holding other variables constant. For logged independent variables, the coefficient is directly interpretable as the percent change in y for a one percent increase in x. For interval-level independent variables, the effect is interpreted as the percent change in y for a one unit increase in x based on the formula $100*(\exp(\beta)-1)$. For dummy- and effect-coded independent variables, the effect is interpreted as the percent change in y for having the denoted x characteristic based on the formula $100*(\exp(\beta - \frac{1}{2}\sigma^2) - 1)$ (DeMaris 2004; Kennedy 1981).

similar for the roughly eight in ten offenders (37,635 of 44,986) who played wholesaling, unspecified distributing, and retailing roles in their offense.

The guideline-based culpability adjustments reveal mixed effects. For example, a one unit increase in aggravating role status results in a significant 14% net increase in sentence length, placing it among the top predictors in the model (b^s = .092). Mitigating role status, conversely, is neither significant (p = 0.076) nor in the expected direction—an outcome that is likely due to poor measurement of this construct. However, safety valve eligibility, which is based in part on culpability considerations, leads to a 25% shorter net sentence—the fourth strongest predictor in the model (b^s = -.140). Thus, aside from the poor prediction of mitigating role status, the guideline-based culpability adjustments influence sentence length in expected ways.

The firearm sentence enhancements are also relatively strong and significant predictors of sentence length. In particular, imposition of the Guideline FSE results in a 24% longer net sentence (b^s = .079), and receiving a 924(c) conviction leads to a 38% longer net sentence (b^s = .110). However, after partialing out the effects of FSE factors, offenders who used or possessed a firearm during their offense receive no added sentencing exposure. Finally, criminal history is significantly related to sentence length: for each unit increase in the criminal history category, sentence length increases by 4% (b^s = .069), net of other factors.

4.2.2 Case processing and sociodemographic factors

As a group, the case processing factors are among the strongest predictors of sentence length. Offenders who plead guilty benefit from a 35% net reduction in sentence length

(b^s = -.237), and those who are granted pretrial release receive 29% shorter net sentences (b^s = -.194). These are the second and third strongest predictors in the model, respectively. Charge bargaining is also highly influential, resulting in a 17% net sentence reduction (b^s = -.109).

Turning to sociodemographic factors, even after controlling for other variables, significant disparities remain for race, age, gender, education, and citizenship. For example, males receive 31% longer sentences than females, and blacks receive sentences that are 17% longer than whites and 14% longer than Hispanics. Older offenders are also at a relative sentencing disadvantage. Notably, the standardized betas for race, gender, and age are in the top half of effect sizes. Finally, higher educational attainment and noncitizenship are associated with significantly shorter sentences.

4.2.3 Summary of sentence length analysis

In summary, this section investigates the relative effects of offense and offender characteristics on federal drug sentence lengths. The results show that drug quantity has the strongest independent effect on sentence length: as quantity increases, so goes the imposed sentence. The next most influential predictors are not the other offense seriousness factors, however, but case processing details such as whether the defendant pled guilty or was released from detention prior to trial. In fact, the relative influence of pleading guilty on sentence length (b^s = -.237) falls just short of the effect of drug quantity (b^s = .241). In other words, after drug quantity, procedural matters governed by prosecutors and the courts have the strongest influence on federal drug sentencing outcomes.

The culpability- and dangerousness-based offense seriousness factors generally form the next most influential group of predictors. Notably, the importance of these measures hinge in part on whether the specific offense behavior corresponds to a formal guideline-based sentencing rule. For example, the aggravating role adjustment, safety valve, and firearm sentence enhancements all significantly influence sentence length in expected ways. In contrast, the role in the offense variables, which are not explicitly accounted for by the guidelines, have no predictable influence on sentence length.

Finally, these results indicate that drug sentences remain disparate under the guidelines for particular sociodemographic subgroups. For example, females and noncitizens receive relatively lenient treatment under the guidelines, and blacks experience a significant and substantial sentencing disadvantage relative to both whites and Hispanics. Moreover, all else equal, older offenders are treated relatively more severely and more educated individuals relatively less severely.

4.3 EFFECTS OF THE 100-to-1 CRACK-POWDER QUANTITY RATIO

The analyses in this section examine the effects of a specific aspect of federal drug sentencing: the 100-to-1 crack-powder cocaine quantity ratio. The 100-to-1 quantity ratio defines a penalty structure in which it takes 100 times the quantity in powder cocaine to reach the same sentencing level reserved for a given quantity of crack cocaine. For example, an offense involving 50 grams of crack cocaine warrants the same ten-year sentence under the guidelines as 5,000 grams of powder cocaine.

The following analyses investigate various aspects of the 100-to-1 quantity ratio. First, descriptive analyses compare crack and powder cocaine offenders across relevant

sentencing factors. Second, multivariate analyses estimate the extent of the crack-powder cocaine sentencing differential after controlling for other variables. Third, a series of postestimation analyses (1) isolate the differential effects of drug quantity on crack and powder cocaine sentence lengths, (2) assess whether disproportionality occurs across levels of culpability, and (3) estimate the racial impact of eliminating the 100-to-1 quantity ratio.

4.3.1 Descriptive analyses comparing crack and powder cocaine offenders

This section compares crack and powder cocaine offenders by role in the offense, sentence length, and drug quantity. There are an estimated 36,651 drug offenders incarcerated for offenses involving any amount of crack or powder cocaine, which represents about two-thirds of all federal drug inmates. Not all of these offenders trafficked in just crack or powder cocaine, however. Thus, to remove the confounding effects of other drugs, all analyses focus on the subsample of offenders involved only with crack (N = 12,653) or powder (N = 18,965) cocaine. Accordingly, these are smaller subsamples than those reported in Table 4.1. Together, these 'crack-only' and 'powder-only' subgroups account for 57% of all drug inmates.

Figure 4.10 compares the role in the offense of crack and powder cocaine offenders. The graph illustrates the unique drug trafficking patterns associated with each drug. For instance, at the top of the supply chain, powder cocaine offenders dominate the money laundering and importing roles, whereas crack offenders are foremost at the level of production. Overall, nearly one-quarter (24.6%) of powder offenders are involved in these upper-level trafficking activities compared to just 6.8% of crack offenders. For the

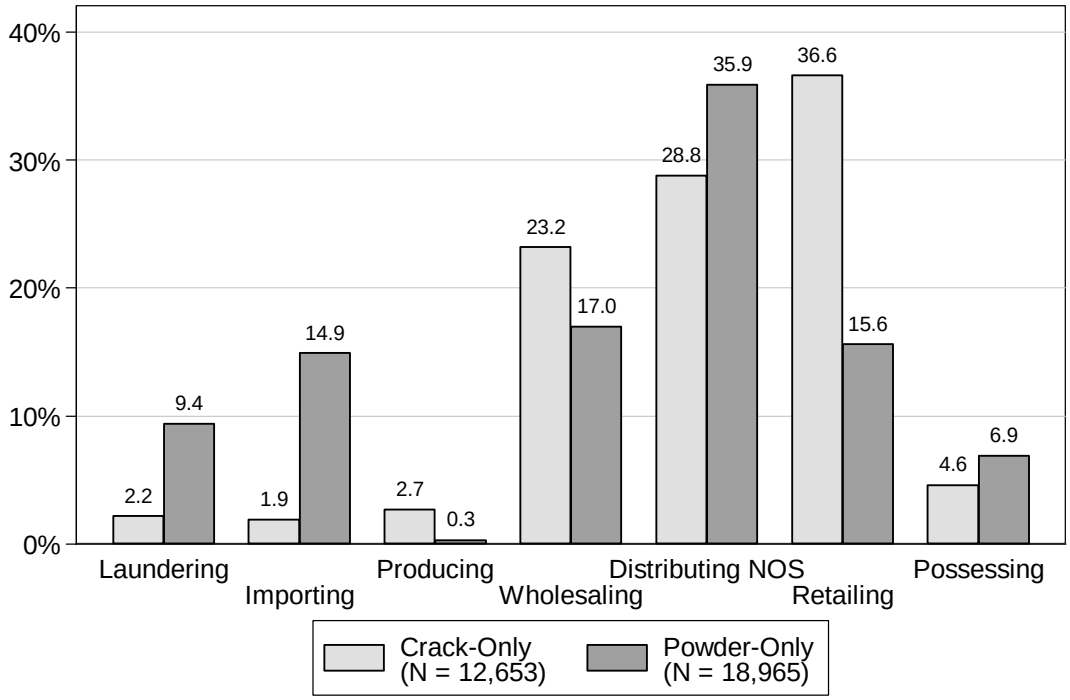

Figure 4.10: Role in the Offense of Crack and Powder Cocaine Offenders

mid-level roles of wholesaling and unspecified distributing, the proportion of crack and powder cocaine offenders is nearly identical (52.0% vs. 52.9%, respectively). Lastly, crack offenders are more likely to operate at the lower distribution levels (41.2% vs. 22.5%), a finding driven primarily by the overwhelming number of crack retailers. Indeed, retailing is the only role where the absolute number of crack offenders exceeds that of powder offenders (4,636 vs. 2,954). These results reveal, in short, that powder cocaine offenders are more prevalent in the upper- to mid-level functional roles (77.5%), whereas crack cocaine offenders predominate in the mid- to low-level functional roles (93.2%).[39]

[39] This general conclusion still holds if the imprecise unspecified distributing role is set aside. For instance, 41.6% of powder cocaine offenders versus 30.0% of crack cocaine

Crack cocaine offenders are also less likely than powder cocaine offenders to be involved in an organized drug distribution group and, when they are, to be the leader or organizer of that group. For example, about 9% (N = 1,156) of crack cocaine and 13% (N = 2,406) of powder cocaine offenders acknowledged involvement with a drug trafficking organization in the year before their arrest. Among these organizational participants, crack offenders were more likely than powder cocaine offenders to be peripheral players (6.2% vs. 2.6%, respectively) and sellers (44.7% vs. 19.3%), but much less likely to be a leaders or organizers of the drug ring (5.2% vs. 22.9%). Overall, 31.3% of crack offenders versus 46.9% of powder offenders played a high-level leadership or middleman role; conversely, 68.7% of crack offenders versus 53.1% of powder offenders played a low-level peripheral, seller, or underling role.

Even though powder cocaine offenders are generally more culpable, the sentences for crack offenders are more severe on average (168 vs. 152 months). Figure 4.11 compares the mean sentences of crack and powder cocaine offenders stratified by role in the offense. Only for possessing is the mean sentence less severe for crack than powder cocaine. For every other role, crack offenders receive longer average sentences than their powder cocaine counterparts. The mean differences range from just two months for unspecified distributing to almost seven years for producing. This sentencing differential is also evident in comparisons across roles. The mean sentence for crack retailing, for example, is greater than the mean sentences for powder cocaine offenders at any higher level role, including money laundering, importing, and wholesaling.

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offenders operate at wholesaling or above, which is nearly opposite the frequency distribution for retailing and below (i.e., 22.5% powder vs. 41.2% crack).

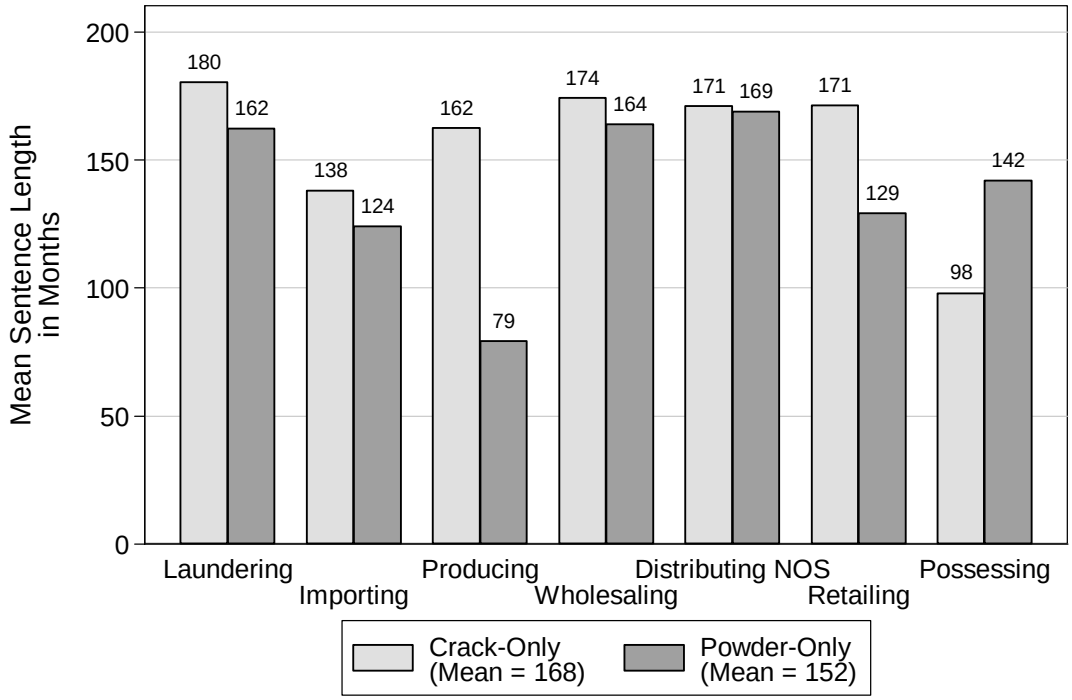


Figure 4.11: Mean Sentence Length by Role in the Offense for Cocaine Offenders

The influence of the 100-to-1 quantity ratio becomes immediately apparent when one examines the underlying drug quantities that lead to these sentences. On the whole, powder cocaine offenders possessed or were responsible for an average of 101.2 kilograms compared to just 4.4 kilograms for crack cocaine offenders. This translates, on average, to 38 months in prison per kilogram of crack cocaine against just 1.5 months per kilogram of powder cocaine. Thus, evaluated at the means, this represents a per kilogram crack-powder penalty ratio of approximately 25:1. This differential is even greater for certain roles. Figure 4.12 presents the mean crack and powder cocaine quantities by role in the offense. For every role, average powder cocaine quantities dramatically exceed those of crack cocaine.

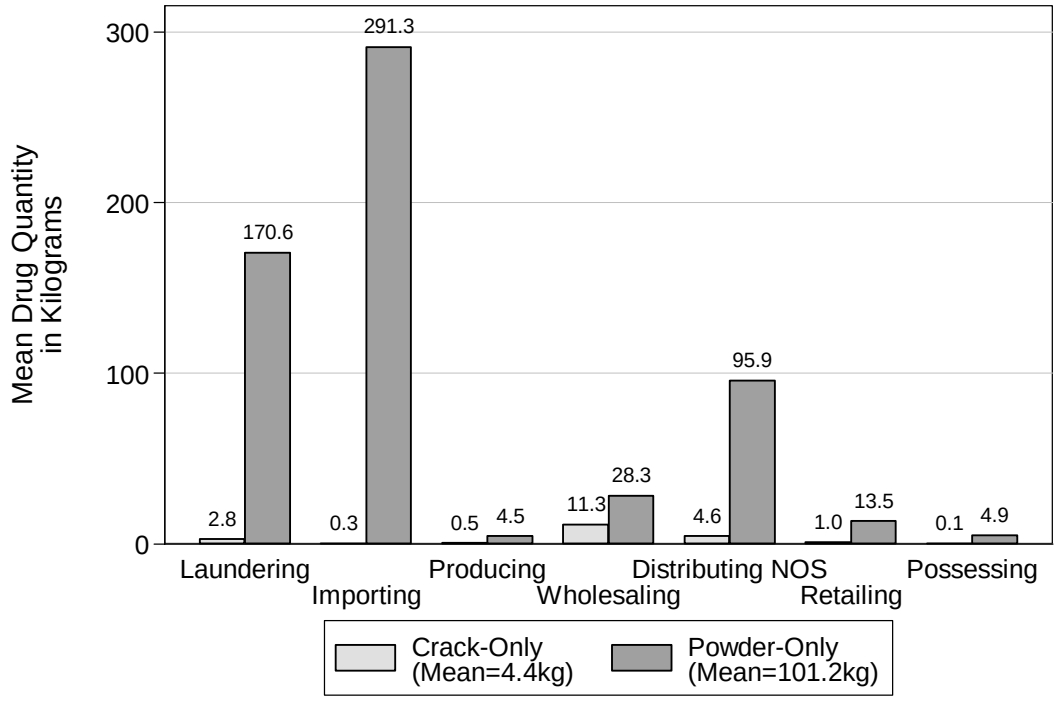


Figure 4.12: Mean Quantity by Role in the Offense for Cocaine Offenders

The disproportionate outcomes possible under the guidelines' 100-to-1 sentencing scheme are best noted by a specific example. Powder cocaine importers, for instance, receive sentences about four years shorter on average than crack cocaine retailers, even though the former are relatively more culpable and responsible for an average of nearly 300 times the amount of cocaine.

In summary, despite being responsible for substantially smaller drug amounts and being relatively less culpable, crack cocaine offenders receive longer average sentences than their powder cocaine counterparts. This conclusion generally holds both within and across roles in the offense. It is well-documented, however, that crack offenders have relatively more serious criminal histories and rates of weapons use than powder cocaine offenders (DOJ 2002; USSC 1995, 2002)—a finding also supported by the Inmate

Survey data[40]—so the crack cocaine sentencing disadvantage may not be as dramatic as these descriptive results suggest. The next set of analyses examine this issue in a multivariate context.

4.3.2 Multivariate analysis of the crack cocaine sentencing disadvantage

How big is the crack-powder cocaine sentencing differential after controlling for other variables, and which factors significantly impact any observed differences? Three truncated-censored regression models predicting sentence length are estimated to investigate these questions. The first is a combined regression of crack- and powder-only offenders (N = 31,618). This model provides a base estimate of the net sentencing differential between crack and powder cocaine offenders. The second two models are subgroup regressions of crack-only (N = 12,653) and powder-only (N = 18,965) offenders. These regressions facilitate cross-model comparison of sentencing effects.

The results are presented in Table 4.3. For ease of presentation and model comparison, the coefficients are presented in percent change format only. There are a few changes to the predictors from the main model in Table 4.2. First, an unadjusted crack and powder cocaine quantity measure replaces the marijuana equivalency measure. Since these analyses deal with offenders sentenced for crack or powder cocaine only, the need for a commensurate quantity measure for multiple drug types was obviated. Also, necessarily, the drug type measures are dropped from the analysis; only a crack offender dummy is retained in the full cocaine model. Finally, the roles of importing and

[40] Refer to Figures B2 and B3 in Appendix B for comparison by firearms use and criminal history, respectively.

producing are combined because of the relatively small *n* among crack importers and powder cocaine producers.

As reported in Table 4.3, the full regression model is significant and explains a moderate share of the variance ($F(26, 8) = 41.05$, $p < 0.001$; M&Z $R^2 = .40$). The results confirm that crack offenders experience a significant net sentencing disadvantage. Specifically, crack offenders receive sentences that are about 21% longer than powder cocaine offenders, net of other factors. In real terms, this translates into an estimated conditional mean sentence of 124 months (95% C.I. = 114, 136) for crack cocaine offenders compared to 103 months (95% C.I. = 94, 112) for powder cocaine offenders—a 21 month difference.[41] In other words, all else equal, crack offenders receive sentences almost two years longer on average than powder cocaine offenders.

Table 4.3 also presents the estimates for the crack-only ($F(25, 9) = 30.38$, $p < 0.001$; M&Z $R^2 = .48$) and powder-only ($F(25, 9) = 16.09$, $p < 0.001$; M&Z $R^2 = .40$) subgroup regressions. Both models are highly significant, with the crack model explaining a slightly larger share of the variance. The results show that the amount of crack involved in the offense has a stronger and more significant net effect on sentence length than the amount of powder cocaine. The role in the offense measures are not as easily interpreted, but appear more influential among crack offenders in terms of the range of effect sizes. However, only simple crack possessing activity bestows a significant and substantial net sentencing benefit relative to other crack distributing roles. In contrast, involvement with unspecified powder cocaine distributing activity leads to

[41] Using Stata's -adjust- command, these estimates were obtained by stratifying the prediction by cocaine type, setting all other variables to their mean values, and exponentiating the estimate.

Table 4.3: Design-Based Truncated-Censored Regression Predicting ln(Sentence Length) among Crack- and Powder-Only Cocaine Offenders

	[1] Crack- and Powder-Only	[2] Crack-Only	[3] Powder-Only
Independent Variables (0-1 Dummies Unless Noted)	Percent Change[a]	Percent Change[a]	Percent Change[a]
ln(Unadjusted Grams)	0.07%***	0.08%***	0.06%**
Five-Year Mandatory Minimum	0.9%	17.1%	5.1%
Ten-Year Mandatory Minimum	11.5%	29.3%	7.5%
Crack Cocaine Offender	20.7%*	--	--
Highest Role in the Offense (Effect Coded)			
Laundering	-9.5%	-2.3%	-11.3%
Importing/Producing	-4.7%	12.7%	-4.9%
Wholesaling	8.4%	15.5%	3.5%
Distributing NOS	7.2%	1.5%	9.0%*
Retailing	4.1%	13.7%	-6.1%
Possessing	-5.1%	-34.7%*	10.4%
Aggravating Role Adj. (0-3)	13.8%*	9.2%	16.0%*
Mitigating Role Adj. (0-3)	2.6%	-2.7%	9.1%
Safety Valve Eligibility	-24.6%***	-19.0%	-29.1%***
924(c) Conviction	27.8%**	26.3%	28.7%
Guideline FSE	19.1%	27.4%	13.1%
Used Firearm During Offense	-0.5%	2.9%	-11.5%
Criminal History Category (1-6)	4.4%**	6.9%*	1.3%
Guilty Plea	-34.7%***	-33.1%***	-33.9%***
Charge Bargain	-15.9%**	-22.7%***	-12.7%
Pretrial Release	-25.8%***	-25.6%***	-24.7%**
Race (Reference: Black)			
White	-18.4%	-28.9%	-18.8%
Hispanic	-15.0%*	-1.4%	-15.9%*
Other Race	-14.1%	59.5%	-26.2%
Male	27.5%**	24.7%**	32.8%*

Table 4.2 (continued)

ln(Age at Offense)	0.16%	0.16%	0.2%
Years Education Completed (0-18)	-1.4%	-0.2%	-1.9%
Non-U.S. Citizen	-5.7%	22.9%*	-13.9%
Constant [coef.]	4.02***	3.80***	4.01***
Sigma [coef.]	-0.45***	-0.53***	-0.44***

*p<0.05, **p<0.01, ***p<0.001

All Models: Number of strata = 7; Number of PSUs = 40
Population (all sentenced inmates): n = 4,025; N = 88,807

Model [1]: F(26, 8) = 41.05, $p < 0.001$; McKelvey & Zavoina's pseudo-R^2 = .40
Subpopulation (powder or crack inmates only): n = 919 (29 right-censored); N = 31,618

Model [2]: F(25, 9) = 30.38, $p < 0.001$; McKelvey & Zavoina's pseudo-R^2 = .48
Subpopulation (crack inmates only): n = 370 (10 right-censored); N = 12,653

Model [3]: F(25, 9) = 16.09, $p < 0.001$; McKelvey & Zavoina's pseudo-R^2 = .40
Subpopulation (powder inmates only): n = 549 (19 right-censored); N = 18,965

[a] Results reported as percent effects, which denote the percentage change in y for a corresponding unit, percent, or discrete change in x, holding other variables constant. For logged independent variables, the coefficient is directly interpretable as the percent change in y for a one percent increase in x. For interval-level independent variables, the effect is interpreted as the percent change in y for a one unit increase in x based on the formula $100*(\exp(\beta)-1)$. For dummy- and effect-coded independent variables, the effect is interpreted as the percent change in y for having the denoted x characteristic based on the formula $100*(\exp(\beta - \frac{1}{2}\sigma^2) - 1)$ (DeMaris 2004; Kennedy 1981).

significantly longer net sentences relative to the average powder cocaine offender. No other role effects are significant in either model.

Comparison of other differences across models reveals that the aggravating role adjustment (increase) and the safety valve (decrease) significantly impact powder, but not crack cocaine, sentences. Conversely, criminal history (increase), charge bargaining (decrease), and noncitizenship status (increase) significantly impact crack, but not powder cocaine, sentences. Notably, among powder cocaine offenders, blacks are sentenced more severely than Hispanics and whites, although the latter contrast is only marginally significant ($p = 0.058$); Hispanics and whites are not sentenced significantly different ($F(1, 33) = 0.10$, $p = 0.751$). Among crack cocaine offenders, blacks and Hispanics are sentenced no differently and, even though the blacks receive longer net sentences than whites, the effect fails to reach significance ($p = 0.105$).[42] Finally, the contrast between white and Hispanic crack offenders is not significant ($F(1, 33) = 1.79$, $p = 0.190$).

4.3.3 Postestimation analyses

In the following subsections, a series of postestimation analyses based on the regression models in Table 4.3 aim to (1) isolate the differential effects of drug quantity on crack and powder cocaine sentencing outcomes, (2) assess whether disproportionality occurs across roles in the offense, and (3) estimate the racial impact of eliminating the 100-to-1 quantity ratio.

[42] The black white contrast in the full model is also large and marginally significant ($p = 0.052$). Given the relatively small sample sizes, it is hard to discount a race effect in these analyses even though strict conventional significance levels were not obtained.

4.3.3.1 Isolating the impact of the 100-to-1 quantity ratio The full model estimation from Table 4.3 shows that, all else equal, crack offenders experience an average 21 month sentencing disadvantage. The subgroup results also indicate that crack quantity has a stronger net effect on sentence length than powder cocaine quantity. The analyses in this section attempt to isolate the effect of the 100-to-1 quantity ratio on the differential crack and powder cocaine sentencing outcomes. Specifically, based on Model [1] estimates from Table 4.3, this section generates and compares a series of alternately specified postestimation predictions of crack and powder cocaine sentence lengths.

The results are presented in Table 4.4. Row (a) reports the fully controlled model prediction in which all variables are set to their mean values. As noted above, this prediction estimates an average 21 month sentencing differential disfavoring crack offenders. The influence of the 100-to-1 quantity ratio on this sentencing differential is demonstrated through two alternately specified prediction equations. The prediction equation in row (b) relaxes the constraint on drug quantity by allowing it to vary by observation. Under this specification, in which everything but quantity is controlled, the sentencing differential is eliminated and even slightly reversed to favor powder cocaine offenders by two months. Row (c) changes the specification by retaining the control on quantity and relaxing the constraint on all others. This equation predicts a 35 month sentencing differential that once again disfavors crack offenders. Together, these results demonstrate that the crack-powder cocaine sentencing disparity is by and large a product of the 100-to-1 quantity ratio. Indeed, if quantity was the lone sentencing determinant, the crack cocaine sentencing disadvantage would be nearly three-years. The collective impact of the other predictors actually mitigates the full, disparate impact of the 100-to-1

quantity ratio, reducing the predicted crack cocaine sentencing disadvantage by more than a year (from 35 to 21 months).

Table 4.4: Predicted Crack-Powder Cocaine Sentencing Differentials

Prediction Criteria	Predicted Sentence Length in Months[a]		Crack Minus Powder Sentencing Differential
	Crack Offenders	Powder Offenders	
(a) All variables set to their mean values	124	103	+21
(b) Quantity allowed to vary by observation and all other variables set to their mean values	110	112	-2
(c) Quantity set to its mean value and all other variables allowed to vary by observation	133	98	+35

[a] Predictions based on Model [1] in Table 4.3.

The differential impact of the 100-to-1 quantity ratio on crack and powder cocaine sentencing outcomes is perhaps best demonstrated graphically. Figure 4.13 presents the linear fit (and 95% confidence intervals), controlling for all other factors, of the predicted sentence lengths as a function of quantity. As the graph shows, for all but the smallest drug amounts, the predicted crack sentences significantly exceed those for powder cocaine. The confidence bands are narrowest at the center of the quantity distributions, and this naturally shades to the left for crack and to the right for powder cocaine. Therefore, the sentencing differential is most meaningfully compared where these narrow bands overlap, say between the medians of the respective quantity distributions. The median quantity for crack is 85 grams and for powder cocaine it is 3 kilograms; the

estimated net sentencing differential between these points ranges from 19 to 24 months. In short, these results demonstrate that, all else equal, the 100-to-1 quantity ratio leads to a typical crack sentencing disadvantage on the order of 1.5 to 2 years.

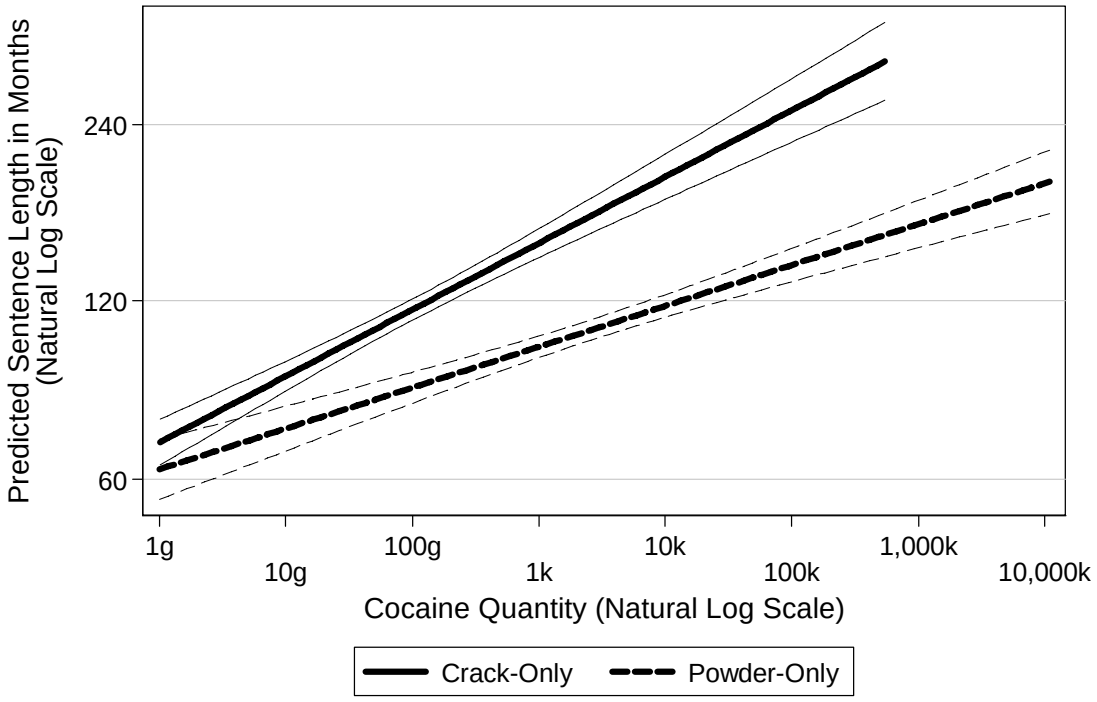


Figure 4.13: Linear Fit (95% C.I.) of the Predicted Sentence and Quantity for Crack and Powder Cocaine Offenders

4.3.3.2 Examining disproportionality across roles in the offense This section addresses the issue of disproportionality across roles in the offense in crack and powder cocaine sentencing outcomes. In this instance, the problem of disproportionality arises when offenders who function in less culpable roles are sentenced more severely than offenders who function in more culpable roles. In light of the disparate impacts of the 100-to-1 quantity ratio, it is expected that crack cocaine offenders will be the relatively

disadvantaged group. In particular, it is a primary expectation that, all else equal, crack retailers will experience a significant sentencing disadvantage relative to powder cocaine wholesalers and importers.

The analysis, based on the crack and powder cocaine subgroup regression estimates from Table 4.3, tests for differential sentencing effects across roles in the offense. The results are presented in Table 4.5. Predicted sentence lengths from the subgroup regressions are displayed in the column and row headings for each role in the offense, holding all other variables to their means. The cells denote the crack minus powder cocaine differences in these predictions. Under the hypothesis that crack sentences will be more severe, one-tailed adjusted Wald tests of the cross-model equality of coefficients are performed for every role in the offense contrast. Significant differences at the $p < 0.10$ level or better are bolded to aid visual interpretation.

The table reveals a general pattern of sentencing outcomes disfavoring crack cocaine offenders. For example, in 25 of the 36 role in the offense contrasts, the predicted sentences for crack offenders are greater than those for powder cocaine offenders. Moreover, in 6 of the 11 contrasts in which powder cocaine offenders receive the longer average sentence, the comparison is to the least culpable crack possessing role. In other words, with the exception of leniently sentenced crack possessors, crack offenders in general experience a near across-the-board sentencing disadvantage.

While just eight role in the offense contrasts confirm a statistically significant crack disadvantage, the six that appear within and above the diagonal support the disproportionality hypothesis. That is, for these six effects, the sentences for less culpable crack offenders are significantly greater than the sentences for more culpable powder

Table 4.5: Testing for Crack minus Powder Cocaine Differences in Sentence Length by Role in the Offense

Powder Cocaine Role in the Offense [Predicted Sentence]	Crack Cocaine Role in the Offense [Predicted Sentence]					
	Laundering [108]	Importing/ Producing [123]	Wholesaling [126]	Distributing NOS [111]	Retailing [124]	Possessing [72]
Laundering [94]	14 (p = .269)	**29 (*p* = .025)**	**32 (*p* = .009)**	17 (p = .106)	**30 (*p* = .007)**	-22 (p = .843)
Importing/ Producing [101]	7 (p = .407)	22 (p = .131)	**25 (*p* = .045)**	10 (p = .226)	**23 (*p* = .039)**	-29 (p = .936)
Wholesaling [110]	-2 (p = .219)	13 (p = .251)	16 (p = .182)	1 (p = .434)	14 (p = .206)	-38 (p = .987)
Distributing NOS [115]	-7 (p = .469)	8 (p = .377)	11 (p = .264)	-4 (p = .613)	9 (p = .323)	-43 (p = .989)
Retailing [100]	8 (p = .394)	**23 (*p* = .074)**	**26 (*p* = .028)**	11 (p = .235)	**24 (*p* = .056)**	-28 (p = .920)
Possessing [117]	-9 (p = .515)	6 (p = .432)	9 (p = .351)	-6 (p = .520)	7 (p = .414)	-45 (p = .992)

Note: Sentence length predictions are from Table 4.3 subgroup estimates. Each cell denotes the crack minus powder cocaine difference in predicted mean sentence lengths. The reported p-values are from one-tailed adjusted Wald tests of the cross-model equality of coefficients from the original submodels. Significant differences at the $p < 0.10$ level or better are bolded.

cocaine offenders. In particular, the net sentencing effects for crack retailing and wholesaling are significantly greater than for powder cocaine laundering and importing/producing. In addition, the effect of crack importing/producing on sentence length is significantly greater than the effect for powder cocaine money laundering. To summarize, these findings provide evidence of disproportionality in sentencing by revealing a reliable pattern of outcomes in which less culpable crack offenders experience a significant sentencing disadvantage compared to more culpable powder cocaine offenders.

4.3.3.3 The 100-to-1 quantity ratio and racial disparities Evidence of racial and other disparities was a primary impetus behind passage of the Sentencing Reform Act of 1984. Race continues to be a contentious issue, especially with respect to disparate crack and powder cocaine sentencing outcomes. Figure 4.14 shows the race/ethnicity of crack and powder cocaine offenders, revealing the extent to which blacks are overrepresented both overall and among crack cocaine offenders in particular. The above multivariate analyses indicated that among all cocaine offenders combined blacks received net sentences that are 15-18% longer than Hispanics and whites, although with a p-value of 0.052 the black-white contrast is arguably inconclusive if conventional significance levels are followed.

The analysis in this section explores the following question: What would be the racial impact of eliminating the 100-to-1 quantity ratio, that is, if crack and powder cocaine were sentenced equally? Regression estimates from Model [1] in Table 4.3 are used to generate postestimation model predictions of this “what if” scenario. The analysis proceeds in the following manner. First, a base prediction isolating the effects of drug

quantity is derived by setting quantity to its mean value and allowing all other variables to vary by observation (i.e., identical to row (c) predictions in Table 4.4). Second, the prediction equation is reestimated treating crack offenders "as if" they were powder cocaine offenders, in effect, statistically eliminating the 100-to-1 quantity ratio. Third, these two sets of predictions are compared, and estimates of prison time averted are calculated to see who benefits most and by how much.

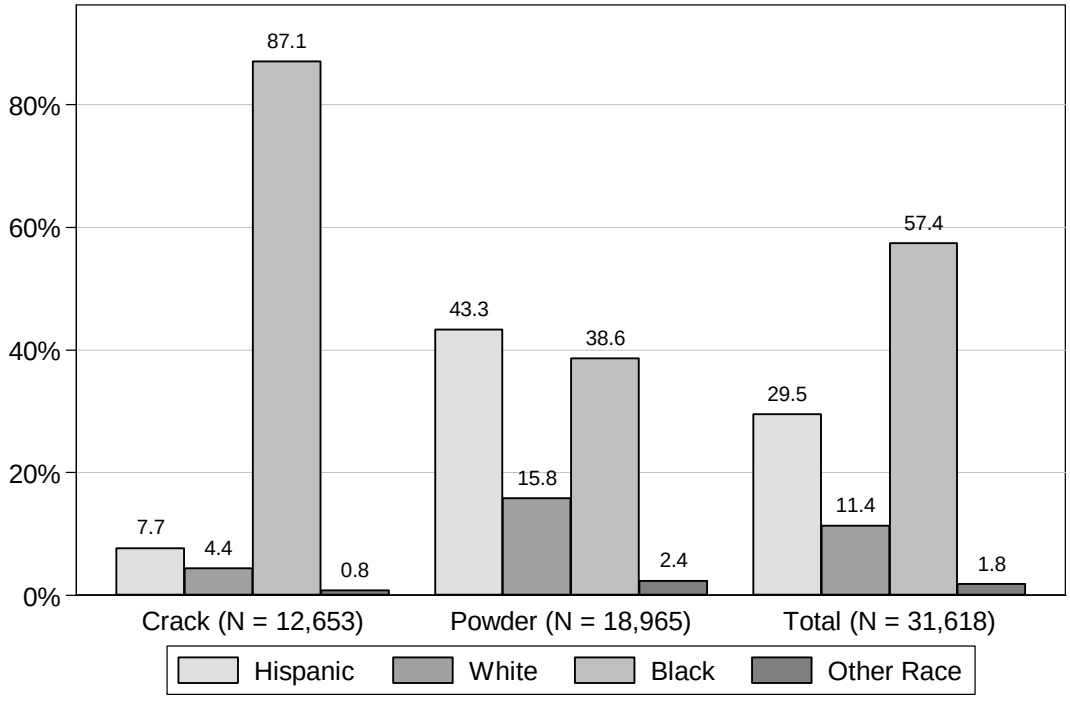


Figure 4.14: Race/Ethnicity of Crack and Powder Cocaine Offenders

The results are presented by race/ethnicity in Table 4.6 (excluding offenders of other race). Row (a) shows the predicted sentences for crack and powder cocaine offenders combined, and row (b) shows similar predictions if crack offenders had been

sentenced "as if" they were powder cocaine offenders. In each case, the predicted sentences for blacks are the most severe, with the sentences for whites and Hispanics essentially equal. Row (c) shows the difference between these two sets of estimates and represents the estimated mean number of prison months averted due to "eliminating" the 100-to-1 quantity ratio. Notably, although the sentences for blacks remain higher, the gap between blacks, on the one hand, and whites and Hispanics, on the other hand, would have been reduced by roughly 11-12 months on average (or about 10-11%).

Table 4.6: Estimated Prison Time Averted from Eliminating the 100-to-1 Quantity Ratio

	Black	White	Hispanic
(a) Predicted Sentence in Months for Crack and Powder Cocaine Offenders Combined[a]	128.7	91.5	90.7
(b) Predicted Sentence in Months for Crack and Powder Cocaine Combined if the 100-to-1 Quantity Ratio was Eliminated[a]	114.9	88.5	88.7
(c) Mean Sentence Averted in Months	13.8	3.0	2.0
(d) Number (Row %) of Incarcerated Crack and Powder Cocaine Offenders	18,615 (60.0%)	3,135 (10.1%)	9,296 (29.9%)
(e) Mean Number (Row %) of Prison Years Averted	21,407 (90.2%)	784 (3.3%)	1,549 (6.5%)

[a] Predictions based on Model [1] in Table 4.3.

To present the broader picture of the racial impact, row (d) presents the total number of incarcerated crack and powder cocaine offenders, and row (e) provides an estimate of the mean number of prison *years* averted, calculated by expressing averted prison time in years and multiplying by the number of inmates. Examined this way, the overall effect of the 11 to 12 month reduction is readily apparent in the large proportional

increase in prison years averted by blacks relative to their representation in the population of cocaine offenders. Specifically, blacks account for 60% of crack and powder cocaine offenders combined, but would benefit from 90% of the averted prison years. To put this in perspective, the estimated number of black prison years averted were crack and powder cocaine sentenced equally represents more than 4,000 individual five-year sentences, compared to approximately 150 for whites and 300 for Hispanics.

4.3.4 Summary of the 100-to-1 crack-powder quantity ratio analyses

This section examined various aspects of the 100-to-1 quantity ratio. The descriptive analyses revealed that, despite being responsible for substantially smaller drug amounts and being relatively less culpable, crack cocaine offenders receive longer average sentences than their powder cocaine counterparts. The multivariate analyses estimate the net crack cocaine sentencing disadvantage to be 21 months on average. The analyses comparing sentence length across roles in the offense indicated that less culpable crack offenders are generally sentenced to longer average sentences than more culpable powder cocaine offenders. Finally, if crack and powder cocaine had been sentenced equally, the sentencing gap between blacks, on the one hand, and whites and Hispanics, on the other hand, would have been reduced by an estimated 11-12 months on average. Moreover, 90% of the nearly 24,000 prison years averted from eliminating the 100-to-1 quantity ratio would have benefited blacks.

4.4 INVESTIGATING THE TARIFF EFFECT

As discussed in Chapter 2, the mandatory minimums' tariff-like approach to sentencing can result in similar sanctions for markedly different offenders, undermining the guidelines' aim of achieving sentences that are proportional to the defendant's level of culpability and dangerousness. This section presents two complementary analyses investigating this "tariff effect." The first analysis examines whether simple exposure to the five-year drug mandatory minimum or above is proportional to other elements of offense seriousness. The second analysis relates mandatory minimum exposure to actual sentencing outcomes and, in particular, replicates a United States Sentencing Commission (1991b) analysis examining whether or not defendants with mandatory minimum offense behaviors are sentenced to the requisite mandatory minimum.

4.4.1 Mandatory minimum exposure and the tariff effect

The results reported in Section 4.1.2 indicated there were differences in mandatory minimum exposure for the highest- and lowest-level functional roles in the offense, but that otherwise exposure was fairly uniform across roles. This section examines these outcomes in a multivariate context. Model [1] in Table 4.7 presents the design-based binary logistic regression results predicting mandatory minimum exposure, coded '1' for offenders with either the five- or ten-year mandatory minimum offense characteristics and '0' otherwise. Note that the model excludes drug quantity as a predictor because mandatory minimum exposure is derived directly from the drug quantity measures, and including it would be circular. The overall model is significant and explains a small to moderate share of the variance ($F(28, 6) = 10.09$, $p < 0.01$; M&Z $R^2 = .24$). Trafficking in

certain drug types has significant effects on mandatory sentencing, most notably for crack cocaine offenders who face nearly nine times the odds of mandatory minimum exposure relative to powder cocaine offenders and net of other factors. Conversely, marijuana and other drug offenders face 47% and 85% lesser odds of mandatory minimum exposure, respectively.

The pattern of results across roles in the offense indicates that, with key exceptions, the odds of mandatory minimum exposure are greater for more culpable defendants. For example, net of other factors and relative to offenders of average culpability, possessors have 92% lesser odds and money launderers 294% greater odds of mandatory minimum exposure, with the effects for retailers (39% lesser odds), unspecified distributors (58% greater odds) and wholesalers (72% greater odds) falling proportionately between these two extremes. This proportionality breaks down for producers and importers, however, as indicated by relative odds of mandatory minimum exposure that fit incongruously between wholesaling and laundering.[43] In short, with the exception of importers and producers, the odds of exposure to mandatory minimum sentencing become proportionately greater as one moves up the chain of culpability.

[43] The lack of statistical significance simply indicates that these roles are no different from offenders of "average" culpability. In fact, importing is marginally significant (p = .059) whereas producing is not (p = .320), which indicates that producing is closer to the average in terms of mandatory minimum exposure. The key finding is that, aside from these two roles, mandatory minimum exposure increases as a function of greater culpability.

Table 4.7: Design-Based Logistic Regression Predicting [1] Mandatory Minimum Exposure and [2] Mandatory Minimum Sentencing Outcomes

Independent Variables (0-1 Dummies Unless Noted)	[1] Exposed to a Drug Mandatory Minimum versus Not	[2] Sentenced At or Above the Requisite Drug Mandatory Minimum versus Not
	Odds Ratios	Odds Ratios
ln(Marijuana Equivalent Grams above the Requisite Mandatory Minimum)	--	1.03
Primary Drug Type (Reference: Powder Cocaine)		
Heroin	0.75	0.73
Methamphetamine	1.14	0.68
Crack Cocaine	8.77***	0.71
Marijuana	0.53***	1.49
Other Drugs	0.15**	0.73
Highest Role in the Offense (Effect Coded)		
Laundering	3.94***	0.69
Importing	1.61	0.74
Producing	1.26	1.10
Wholesaling	1.72**	0.78
Distributing NOS	1.58**	0.93
Retailing	0.61***	1.09
Possessing	0.08***	2.27
Aggravating Role (0-3)	1.21	1.09
Mitigating Role (0-3)	1.07	1.03
Safety Valve Eligibility	1.10	0.26***
924(c) Conviction	0.60*	1.73
Guideline FSE	0.99	1.55
Used Firearm During Offense	1.40	2.12
Criminal History Category (1-6)	0.90	1.07
Guilty Plea	0.61**	0.43**
Charge Bargain	1.26	0.49***

Table 4.7 (continued)

Pretrial Release	0.84	0.47**
Race (Reference: Black)		
White	1.24	0.59
Hispanic	1.43	0.75
Other Race	1.14	0.82
Male	0.92	1.81*
ln(Age at Offense)	1.33	1.43
Years Education Completed (0-18)	1.03	0.96
Non-U.S. Citizen	1.06	0.99

*p<0.05, **p<0.01, ***p<0.001

All Models: Number of strata = 7; Number of PSUs = 40
Population (all sentenced inmates): n = 4,025; N = 88,807

Model (1): F(28, 6) = 10.09, p = 0.004; McKelvey & Zavoina's pseudo-R^2 = .24
Subpopulation: n = 1,671; N = 55,481

Model (2): F(29, 5) = 8.32, p = 0.013; McKelvey & Zavoina's pseudo-R^2 = .27
Subpopulation: n = 1,165; N = 38,521

The effects of aggravating and mitigating role adjustments are not significant, nor is the safety valve, indicating that guideline-based determinations of relative culpability have no bearing on mandatory minimum sentencing exposure, net of other factors. Notably, offenders who receive a 924(c) firearm conviction have 40% lesser odds of mandatory minimum exposure, net of other factors, yet neither of the other firearm-related factors are significant. Thus, offenders who receive the five-year mandatory minimum 924(c) penalty face significantly reduced odds of also receiving a drug mandatory minimum. Offenders who plead guilty also experience significantly lesser odds of mandatory minimum exposure, net of other factors. Lastly, criminal history, other case processing factors, and sociodemographics are invariant with respect to mandatory minimum exposure.

4.4.2 Mandatory minimum sentencing outcomes and the tariff effect

While the results from Model [1] speak to mandatory minimum *exposure*, they do not reflect actual mandatory minimum sentencing *outcomes*. This is a key difference as mandatory minimums are not always applied when the quantity of drugs is sufficient to trigger the minimum. Indeed, of the 38,521 drug offenders with drug mandatory minimum behaviors, only 71% were sentenced at or above the indicated mandatory minimum level. What are the factors that determine differential receipt of mandatory minimum sentences when they appear warranted based on quantity, and are the differential effects incongruent with the guidelines' aim of achieving proportionality in sentencing? In order to investigate these questions, a second logistic regression analysis examines whether or not defendants with mandatory minimum drug amounts were

sentenced at the requisite five- or ten-year mandatory minimum level. The dependent variable is coded '1' if offense characteristics called for a mandatory minimum of five (ten) years and the actual sentence imposed was at least five (ten) years, and '0' otherwise. Model [2] in Table 4.7 presents the results of this analysis. Note that the independent variable for drug quantity is modified to reflect the number of marijuana equivalent grams *above* the requisite mandatory minimum. This specification parallels the United States Sentencing Commission's (1991b) mandatory minimum study.

Overall, the model is significant and explains a small to moderate share of the variance ($F(29, 5) = 8.32$, $p = 0.013$; McKelvey & Zavoina's pseudo-$R^2 = .27$). The amount of drugs above the requisite mandatory minimum trigger is not a significant predictor of mandatory minimum sentencing outcomes, net of other factors. That is, offenders who possess or are held responsible for drug amounts just above the mandatory minimum quantity trigger are no more or less likely than those who possess amounts that far exceed the quantity trigger to receive a sentence at the appropriately indicated level. Notably, drug type, role in the offense, aggravating or mitigating role status, firearm-related factors, and criminal history all have no significant bearing on whether the requisite mandatory minimum is applied. The only significant offense-related factor is safety valve eligibility, which is associated with 74% lesser odds of sentencing to the requisite mandatory minimum level after controlling for other variables. These findings confirm that the safety valve is working as intended to limit the applicability of mandatory sentences to certain low-level defendants characterized by minimal criminal histories, lack of concomitant violence or weapon use, nonleadership roles, and acceptance of responsibility.

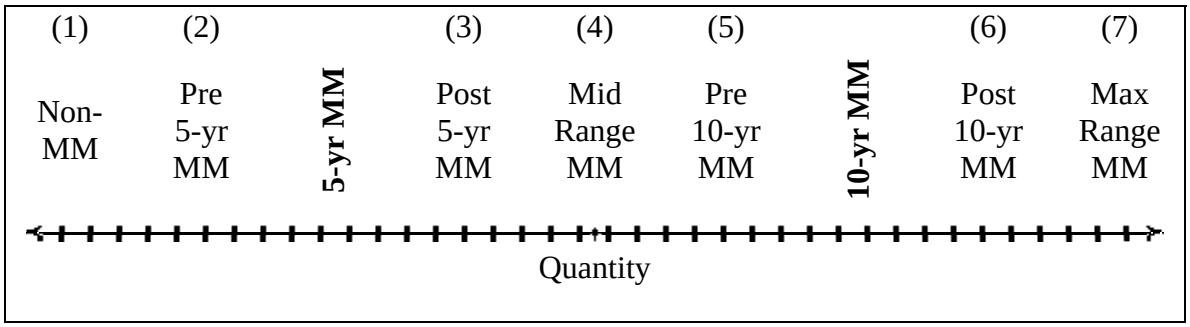
These results also confirm the strong role of procedural case processing factors in mandatory minimum sentencing outcomes. In particular, offenders who plead guilty, charge bargain with the prosecutor, or are released prior to trial have approximately half the odds of being sentenced to the requisite mandatory minimum, net of other factors. The only other significant effect in the model indicates that, net of other factors, females have 45% lesser odds (i.e., $1.81^{-1} = 0.55$) than males of receiving the requisite mandatory minimum.

4.4.3 Summary of mandatory minimum and tariff effect analyses

These results appear mixed regarding the question of whether quantity-based mandatory minimums undermine proportionality along other dimensions of offense seriousness. First, regarding mandatory minimum *outcomes*, they provide strong evidence of the safety valve's success in limiting the applicability of mandatory minimums to certain low-level offenders. However, they also reveal that no other culpability- or dangerousness-based factors influence whether a mandatory minimum is applied. Thus, barring alleviation under the safety valve, offenders of varying culpability, dangerousness, and criminal backgrounds are treated uniformly with respect to mandatory minimum sentencing. This uniformity extends even to offenders with dissimilar roles in the offense, despite their differential *exposure* to mandatory sentencing. Notably, these results also confirm the impact of case processing factors—and perhaps most striking, prosecutorial discretion in plea and charge bargaining decisions—on mandatory minimum sentencing outcomes.

4.5 INVESTIGATING THE CLIFF EFFECT

One type of cliff effect that can occur in federal drug sentencing arises from the interaction between mandatory and guidelines sentencing, whereby small differences in drug quantity (e.g., 1 or 5 grams) surrounding the mandatory minimum quantity trigger result in large differences in sentence outcomes for otherwise similarly situated defendants. Evidence for this cliff effect is assessed in the present analysis by comparing the sentences of offenders immediately above the mandatory minimum quantity trigger with those falling just below it. Methodologically, this is accomplished using a series of backward adjacent difference coded dummy variables. As noted in Section 3.7.2, for an ordered set of dummy variables, this coding scheme contrasts the effect for a given dummy variable against the effect of the immediately prior dummy. As detailed in the following schematic, seven cliff effect dummy variables were created reflecting the drug amounts offenders possessed in relation to the five- and ten-year mandatory minimum (MM) quantity triggers.



A critical modeling choice concerns where in the quantity distribution to establish the dividing points. Obviously, the marks are already set with respect to the five- and ten-year quantity triggers. For these analyses, the pre and post mandatory minimum

thresholds were defined as occurring 50% above and below the mandatory minimum quantity triggers, respectively. For example, the five-year heroin mandatory minimum quantity trigger is 100 grams; therefore, the pre five-year MM quantity level extends from 50 to 99.9 grams and the post five-year MM quantity level extends from 100 to 150 grams. Analysis of the cliff effect over smaller quantity ranges was attempted using both 10% and 25% multipliers. Unfortunately, small *n* considerations in the resulting quantity ranges precluded using these alternate thresholds. Thus, because the 50% threshold is fairly large, it is possible that the analysis will capture legitimate proportional increases in sentencing exposure rather than unwarranted disparity related to small differences in drug quantity. For instance, the 50 to 150 gram quantity range cited in the example above corresponds to guideline offense levels 20 to 26, or a guideline sentencing range of 33 to 78 months. Interpretation of the results will need to account for this unsatisfactory, but necessary, modeling choice. For instance, following the above example, a sentencing differential greater than 78 – 33 = 45 months could more plausibly be interpreted as a cliff effect than a smaller, and thus within-guideline, differential.

Evidence for the cliff effect is examined for each of the five major drugs: heroin, methamphetamine, crack cocaine, powder cocaine, and marijuana. Offenders convicted of offenses involving multiple substances are excluded from the analyses to avoid confounding effects. Thus, these analyses include 87% (48,400 of 55,481) of the full drug sample after excluding other drug and polydrug offenders. Table 4.8 presents the frequency distributions of the cliff effect groups by drug type. Separate design-based truncated-censored regression models are estimated for each drug subsample. The cliff effect dummy variables replace the drug quantity and mandatory minimum exposure

variables used in previous analyses. The complete regression estimates are presented in Table B4 in Appendix B. The analyses below focus exclusively on the magnitude and significance of the cliff effect dummy coefficients. Presentation of the results is framed around graphs of the predicted sentences (and 95% confidence intervals) across the seven cliff effect quantity levels.

Table 4.8: Cliff Effect Frequency Distributions by Drug Type

			Quantity Level							
Drug Type	Five- and Ten-Year MM Qty Triggers		Non MM	Pre Five-Year MM	Post Five-Year MM	Mid-Range MM	Pre Ten-Year MM	Post Ten-Year MM	Max Range MM	Total
Heroin	100g	*N*	733	308	213	600	627	423	676	3,580
	1,000g	*n*	22	9	8	21	27	15	25	127
Meth.	100g	*N*	789	147	387	925	604	353	1,987	5,192
	1,000g	*n*	24	4	10	30	22	10	60	160
Crack	5g	*N*	1,053	343	696	895	857	1,946	6,863	12,653
	50g	*n*	31	13	20	26	29	51	200	370
Powder	500g	*N*	3,474	862	809	3,914	1,425	2,207	6,274	18,965
	5,000g	*n*	95	27	25	121	46	58	177	549
Marijuana	100kg	*N*	1,881	1,091	1,376	1,416	658	660	927	8,009
	1,000kg	*n*	65	36	39	42	19	17	27	245

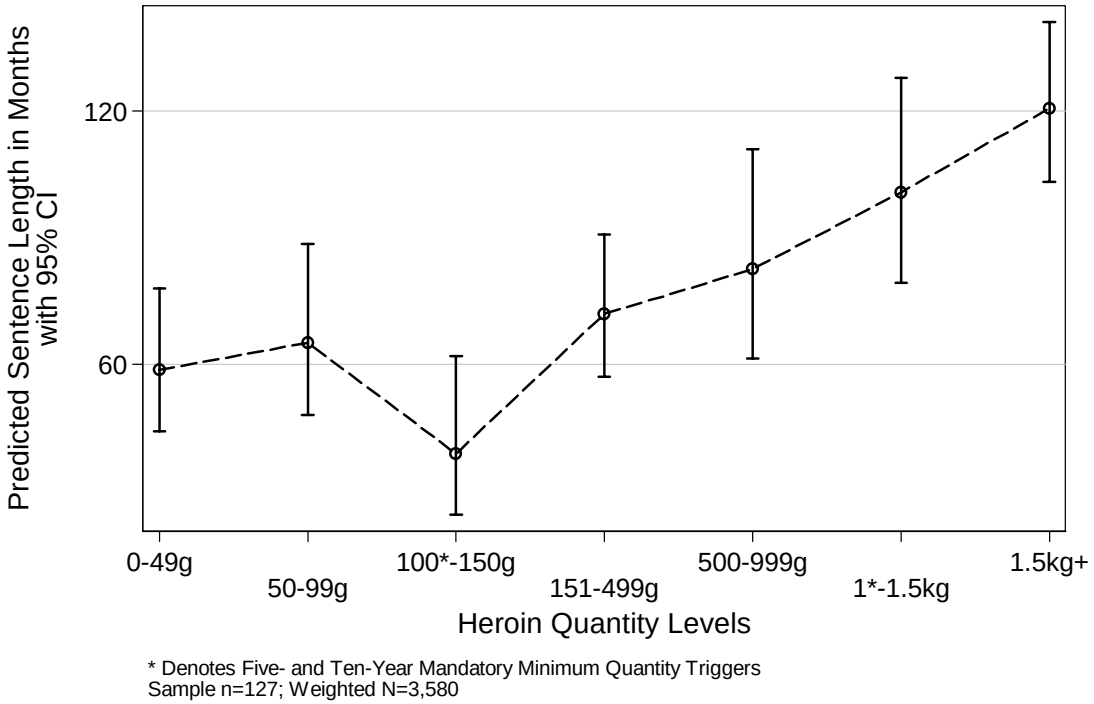


Figure 4.15: Predicted Sentence Length by Heroin Quantity Levels

4.5.1 Heroin sentencing outcomes and the cliff effect

The heroin model is highly significant and explains a substantial share of the variance ($F(28, 6) = 42.94$, $p < 0.001$ M&Z $R^2 = .70$). Figure 4.15 presents the predicted sentence lengths with 95% confidence intervals across the seven quantity levels. Overall, with the exception of an anomalous dip at the post five-year MM level (100-150g), predicted mean sentences increase steadily from 59 to 121 months. The only significant outcomes occur around this anomalous dip. Specifically, the predicted mean sentence for heroin offenders declines by 26 months ($p = 0.035$) from a pre five-year MM (50-99g) prediction of 65 months to a post five-year MM (100-150g) prediction of 39 months, net of other factors. The predicted mean sentence then jumps to 72 months at the mid range MM level (151-499g), a significant net increase of 33 months ($p = 0.008$). Sentence

length predictions then increase steadily but insignificantly across higher quantity levels. In short, this analysis fails to find evidence of the cliff effect in heroin sentencing outcomes. In fact, it finds a significant deviation in sentencing outcomes around the mandatory minimum exactly opposite than expected. However, given the relatively small heroin subsample size (n = 127) and the small n at the post five-year MM level (n = 8), this deviation is probably an artifact of the data rather than a sign of any systematic difference for this particular group of heroin offenders.

4.5.2 Methamphetamine sentencing outcomes and the cliff effect

The methamphetamine model is highly significant and explains a large share of the variance ($F(28, 6) = 150.44$, $p < 0.001$ M&Z $R^2 = .59$). Figure 4.16 presents predicted sentences by quantity level for methamphetamine. Similar to heroin offenders, the predicted mean sentence increases from 57 to 120 months, but the rise is more erratic and none of the contrasts achieve significance. Although the 40-month jump in the predicted sentence from 65 months at the pre five-year MM level (50-99g) to 105 months at post five-year MM level (100-150g) is substantial, the increase is not significant at conventional levels (p = 0.075). The lack of significance is partly attributable to imprecision in the estimate for the pre five-year MM category, which includes just four sample cases. Above the 100-150 gram category, there is no strong linear trend in sentence outcomes, nor is there evidence of a cliff effect. In fact, relative to the pre ten-year MM level (500-999g), the predicted mean sentence for the post ten-year MM level (1-1.5kg) actually drops from 111 to 89 months.

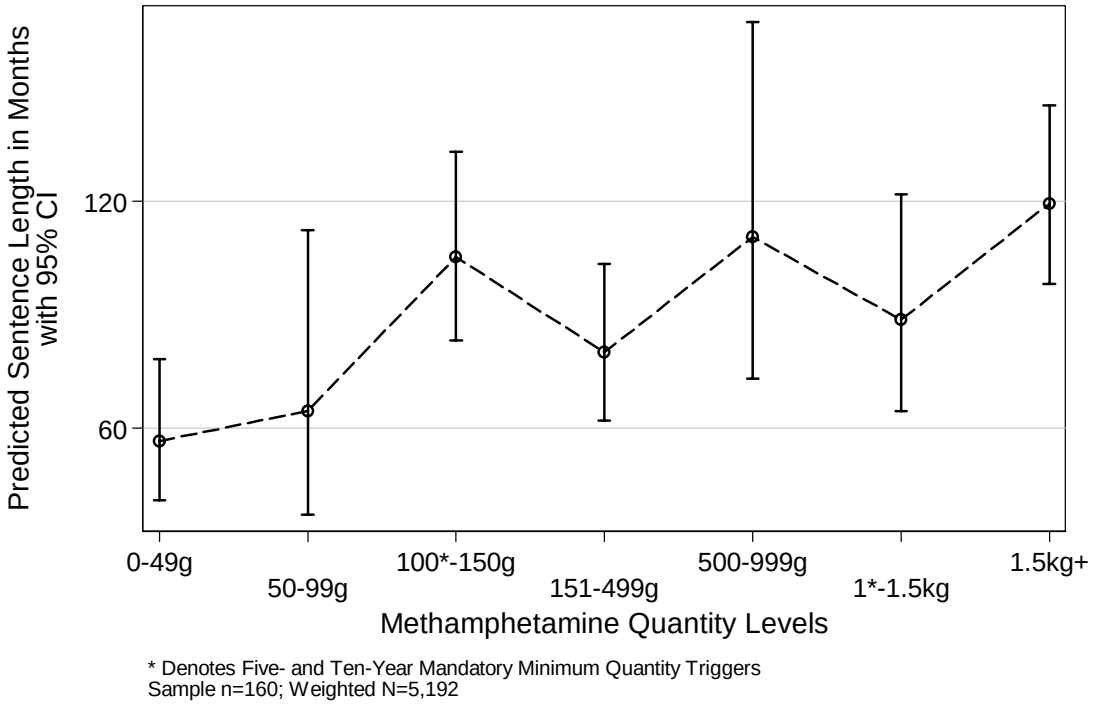


Figure 4.16: Predicted Sentence Length by Methamphetamine Quantity Levels

4.5.3 Crack cocaine sentencing outcomes and the cliff effect

Figure 4.17 presents the graph for crack cocaine. The overall model is significant and explains a large share of the variance ($F(29, 5) = 7.98$, $p = 0.015$ M&Z $R^2 = .49$). Notably, predicted crack sentences, which increase from 70 to 144 months, are relatively more severe than for the preceding drugs. None of the contrasts across quantity levels provide evidence of a crack cocaine cliff effect. Indeed, at the lowest three quantity levels—which includes the five-year mandatory minimum—predicted sentences hover around 70 months. Sentences increase sharply over the next two quantity levels, but only the contrast between the post five-year (5-7.5g) and mid range (7.51-24.9g) mandatory minimum quantity levels is even marginally significant ($p = 0.053$). Similar to the five-year mandatory minimum, the predicted mean sentences for the pre and post ten-year

MM levels are practically identical. Finally, the significant jump (p = 0.022) to a 144 month predicted mean sentence for the last quantity level is reflective of the relatively large crack quantities on the whole, as 64% of crack offenders possess or are held responsible for quantities above 75 grams. In short, these findings disconfirm evidence for a cliff effect in crack cocaine sentencing outcomes at both the five- or ten-year mandatory minimum levels.

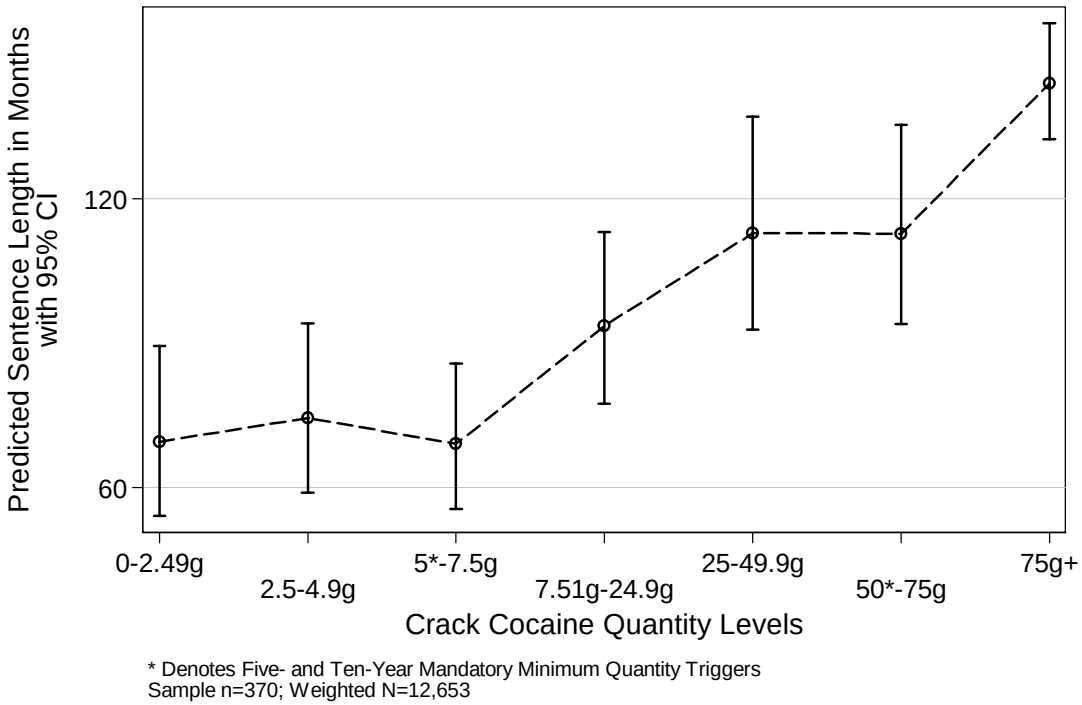


Figure 4.17: Predicted Sentence Length by Crack Cocaine Quantity Levels

4.5.4 Powder cocaine sentencing outcomes and the cliff effect

Figure 4.18 presents outcomes for the powder cocaine model, which is significant and explains a moderately large share of the variance (F(28, 6) = 6.62, p = 0.013 M&Z R^2 = .40). Similar to crack cocaine, powder cocaine sentences are relatively elevated,

increasing from 82 to 134 months. Notably, the sentencing differential between the pre five-year MM prediction of 72 months and the post five-year MM level prediction of 105 months is highly significant ($p = 0.010$), providing evidence of a net 33 month sentencing differential around the five-year powder cocaine mandatory minimum. However, this difference is not larger than the sentences called for by the corresponding guideline ranges. No other contrasts in the model are significant. Moreover, the predicted sentences for the pre and post ten-year MM quantity levels actually decrease from 121 to 116 months.

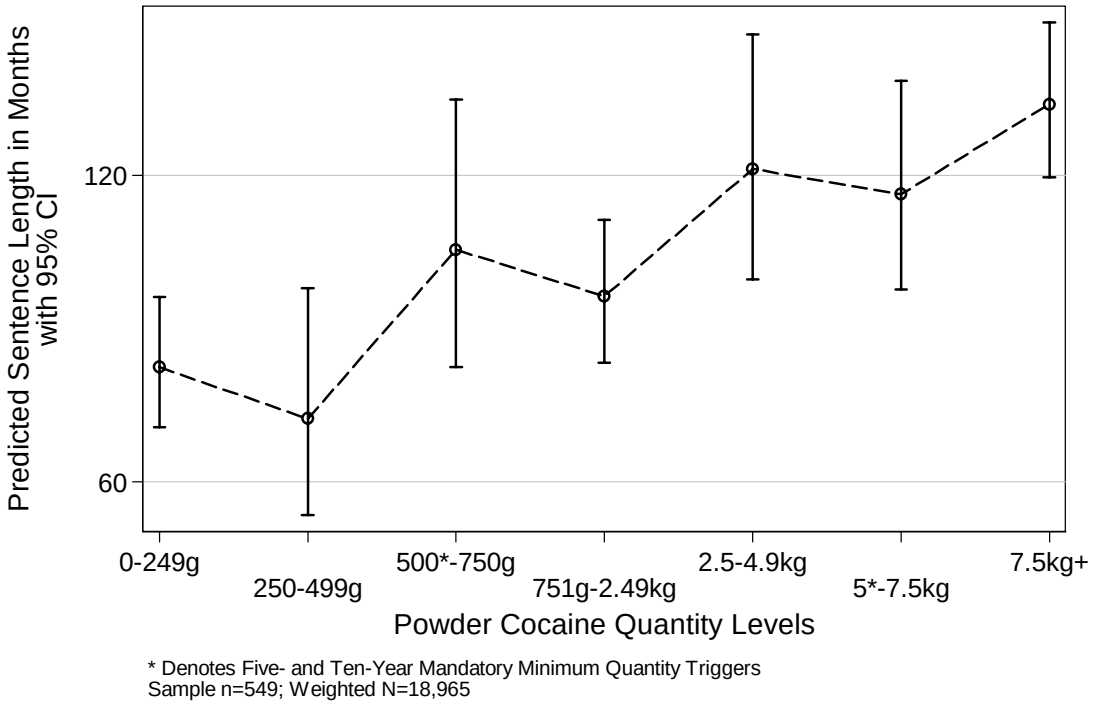


Figure 4.18: Predicted Sentence Length by Powder Cocaine Quantity Levels

4.5.5 Marijuana sentencing outcomes and the cliff effect

The final model examines marijuana outcomes, which are presented in Figure 4.19. The model is significant and explains a large share of the variance ($F(29, 5) = 33.10$, $p < 0.001$ M&Z $R^2 = .44$). Compared to the other drugs, the sentences for marijuana are relatively deflated, ranging from a low of 40 months to a high of 94 months. Notably, the contrast between the pre and post five-year MM quantity levels is significant ($p = 0.021$), net of other factors. In particular, the predicted mean sentence increases from 45 to 69 months across the five-year mandatory minimum, but again this two-year differential is within the corresponding guideline ranges. No other contrasts are significant. The predicted sentence actually decreases from 76 months at the pre ten-year MM level to 71 months at the post ten-year MM level.

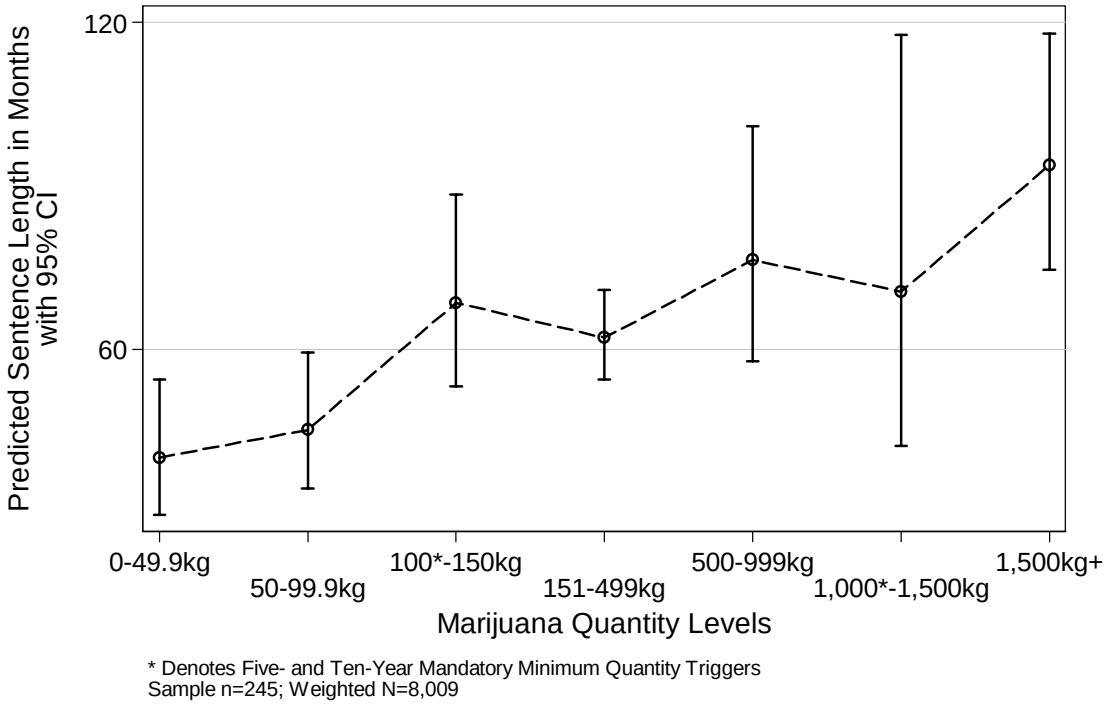


Figure 4.19: Predicted Sentence Length by Marijuana Quantity Levels

4.5.6 Summary of cliff effect analyses

Based on these results, evidence for the cliff effect in federal drug sentencing is generally disconfirmed. Only two specific contrasts indicate that sentences for offenders possessing quantities in the range above the quantity trigger are significantly longer than those falling below it—both of these occurring for powder cocaine and marijuana offenders around the five-year mandatory minimum. In these cases, the sentencing differential is estimated to be 33 months for powder cocaine offenders and 24 months for marijuana offenders—both of which are reasonably encompassed within the guideline sentencing ranges for the quantity levels examined. Sentences for heroin, methamphetamine, and crack cocaine across the five-year mandatory minimum and for all drugs across the ten-year mandatory minimum are not significantly different. In fact, most of these the contrasts reveal flat or inverse sentencing effects rather than the expected sharp increases.

Given the relatively wide quantity ranges over which the cliff effect was examined (i.e., 50% above and below the mandatory minimum threshold), it is possible these analyses were not sensitive enough to detect unwarranted disparity between offenders who were held accountable for drug quantities *just* above and below the mandatory minimum thresholds. However, that relatively few drug offenders in this sample actually possessed drug quantities in narrow bands around the mandatory minimum quantity triggers suggests that the cliff effect potentially impacts a limited number of federal drug offenders. For example, less than 10% of the offenders for all five drugs combined (4,613 of 48,400) possessed quantities within a 5% range on either side of both the five- and ten-year mandatory minimum thresholds (corresponding, for example, to a range of 4.75 to 5.25 grams around the five-year crack cocaine mandatory

minimum). In short, the cliff effect is probably not a common source of disparity in federal drug sentencing. In fact, these results show that a potentially greater source of disparity in the interplay between the guidelines and mandatory sentencing—which might be referred to as the "plateau effect"—manifests as a general flattening (and even inversion) of sentences around the mandatory minimums. For example, the results show that sentences for offenders with drug amounts in the range below versus above the ten-year mandatory minimum were not statistically different for all drugs, and this relative sentencing uniformity around just the ten-year threshold impacts 20% of drug offenders in the sample (9,760 of 48,400).

To see how this may operate in practice, it is helpful to examine the interrelation between the mandatory minimum and guideline sentencing ranges. Figure 4.20 displays this schematic using crack cocaine as an example. The mandatory minimum line shows the stepped increases that occur at the five- and ten-year quantity thresholds. The guideline sentencing range is anchored to the mandatory minimum thresholds, but increases proportionally with quantity. When a sentence is governed by the guidelines, it presumptively falls within the specified range, but guideline departure jurisprudence allows judges to adjust sentences up or down based on aggravating or mitigating factors (although this has been severely restricted in recent years). However, when the guideline range is at or just above the five-year mandatory minimum threshold, for example, mitigating guideline departures are constrained by the minimum. As the guideline range increases past the threshold, the leeway between the two systems once again allows guideline departures to come into play—that is, until the guideline range reaches the ten-year minimum threshold, where the process is repeated.

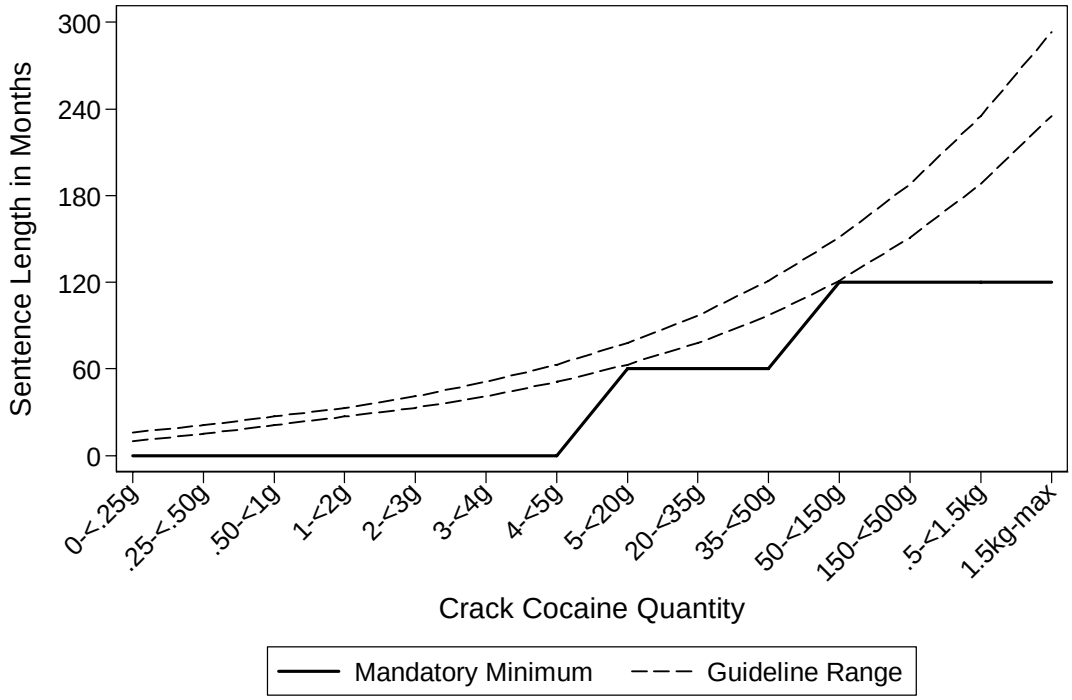


Figure 4.20: Mandatory Minimum and Guideline Range for Crack Cocaine Quantity

The general pattern of results uncovered by these analyses suggests judges are exercising considerable discretion under the guidelines to adjust sentences downward (e.g., using mitigating role and safety valve departures) when this discretion is not otherwise constrained by the mandatory minimums. In doing so, however, this creates aggregate sentencing plateaus and even inversions (possibly due to substantial assistance departures) between offenders with different levels of seriousness. In short, these results suggest that the "plateau effect" is a potentially greater source of unwarranted disparity in federal drug sentencing than the "cliff effect."

4.6 CIRCUMVENTION OF FIREARM SENTENCE ENHANCEMENTS

Prior research has found that firearm sentence enhancements (FSEs) are not applied evenly or consistently in cases where they appear warranted, creating disparities in sentencing outcomes among similarly situated offenders (Hofer 2000; Nagel and Schulhofer 1992; Schulhofer and Nagel 1997). Several possible reasons for FSE circumvention have been postulated, including plea bargaining practices and equity concerns over already severe drug sentences. This section examines the circumvention of firearm sentence enhancements for federal drug offenders. First, descriptive analyses characterize the scope and extent of the problem. Second, multivariate logistic regression analyses investigate the factors that significantly impact firearm sentence enhancement application. It is expected that drug quantity, mandatory minimum exposure, pleading guilty, and charge bargaining will condition the application of firearm sentence enhancements.

4.6.1 Characterizing the extent of the FSE circumvention problem

To what extent are firearm sentence enhancements circumvented when they appear legally warranted? Table 4.9 presents the bivariate distribution of firearm sentence enhancement status by the defendant's acknowledged use or possession of a firearm during the offense. Overall, the majority (78.5%) of federal drug offenders are not connected to firearms in any way: they neither received a firearm sentence enhancement nor admitted to offense-related firearms activity. Another 5.3% acknowledged both using a firearm during the offense and receiving either of the firearm sentence enhancements. Thus, the application (or not) of a firearm sentence enhancement concurs with the

defendant's reported gun activity in 83.8% of the cases. In the remaining 16.2% of the cases, there is a discrepancy between FSE outcomes and self-reported firearms involvement. In the majority of these cases, the discrepancy is due not to possible circumvention of a firearm sentence enhancement, but to the application of a firearm sentence enhancement where gun behavior is denied by the defendant. Each of these situations warrants separate attention.

Table 4.9: Firearm Sentence Enhancement Status by the Use or Possession of a Firearm

	Used/Possessed Firearm During Offense		
FSE Status	No	Yes	Row Total
None	78.5%	2.0%	80.5%
Guideline FSE	8.4%	2.0%	10.4%
924(c) Conviction	5.8%	3.3%	9.1%
Column Total	92.6%	7.4%	N = 55,481

Note: Percentages may not sum due to rounding.

First, among those offenders who denied using or possessing a firearm, the data indicate that "unwarranted" application of an FSE occurs in 15.3% of the cases (7,852 of 51,397). While this could partly be due to underreporting given the sensitive nature of weapons violations, it is more likely a product of federal firearms jurisprudence which extends criminal liability for guns to (1) coconspirator conduct and (2) constructive possession (e.g., having a gun in the trunk of a car during a drug deal or in a house where marijuana is being grown). Second, among those offenders who acknowledged using or possessing a firearm during their offense, the data indicate that "unwarranted" evasion of

an FSE occurs in 27.5% of the cases (1,123 of 4,084). It is not possible to discern from these data, however, whether this is due to true evasion or to evidentiary issues with proving the defendant's firearm involvement. In short, a liberal estimate indicates that FSE circumvention occurs in about 28% of the cases in which an FSE appears legally warranted.

Does FSE circumvention have an unwarranted mitigating effect on sentence length? Table 4.10 displays mean sentence length stratified by firearm sentence enhancement status and reported firearm involvement. Among offenders who used or possessed a firearm during their offense, those who evaded an FSE have an average sentence of 120 months versus the 179- to 187-month sentences for similarly situated offenders who ended up receiving an FSE. Thus, to the extent that offense-related firearms activity among the evaders was readily provable, they experience an approximate five-year unwarranted sentencing advantage, on average, compared to offenders who ultimately received an FSE. Table 4.10 also reveals that, among offenders who received either FSE, there is a two- to three-year average sentencing differential between those who deny and acknowledge offense-related firearm involvement. There are at least two possible interpretations of this difference, both of which could be operating at the same time. On the one hand, to the extent the deniers are not being forthright about their gun involvement, a warranted sentencing benefit is bestowed upon offenders who accept responsibility for their actions. On the other hand, to the extent the deniers are being truthful, their greater punishment for ostensibly less egregious offense conduct (e.g., constructive possession versus active use) appears unwarranted.

Table 4.10: Mean Sentence Length in Months by Firearm Sentence Enhancement Status and the Use or Possession of a Firearm

	Used/Possessed Firearm During Offense		
FSE Status	No	Yes	Row Total
None	134	120	134
Guideline FSE	226	179	217
924(c) Conviction	212	187	203
Column Total	148	166	149

4.6.2 Multivariate analyses of FSE application

The next set of analyses investigates the factors that lead to uneven application of firearm sentence enhancements. Table 4.11 presents two design-based logistic regression models predicting 924(c) and Guideline FSE outcomes.[44] Note that safety valve eligibility is not included in either model because it perfectly predicts both outcomes. Both models are significant at the $p < 0.01$ level, although Model [1] predicting the 924(c) conviction explains about twice the variance of Model [2] predicting the Guideline FSE. As expected, self-reported use or possession of a firearm during the offense is by far the strongest predictor of receiving a firearm sentence enhancement, net of other factors. The

[44] Since the three possible FSE outcomes (i.e., no FSE, Guideline FSE, and 924(c) conviction) are mutually exclusive and independent, multinomial logistic regression would ordinarily be the model of choice to perform this analysis. However, recall from Section 3.8.1.2 that degrees of freedom limitations prevent the design-based multinomial logistic regression estimator from reporting an overall model *F* test. For this reason, two logistic regression models are estimated in subsamples formed by the FSE categories (with non-FSE offenders forming the reference group in both). For comparative purposes, the independent variable coefficients from a design-based multinomial logistic regression model (without an overall model test) are reported in Table B5 in Appendix B. Comparing these estimates with those in Table 4.11 indicates they are remarkably consistent. The only substantive differences are that 'male' achieves significance in the 924(c) vs. no FSE model, as does 'importing' in the Guideline FSE vs. no FSE model.

Table 4.11: Design-Based Logistic Regressions Predicting Receipt of [1] a USC 924(c) Conviction or [2] the Guideline FSE

Independent Variables (0-1 Dummies Unless Noted)	[1] 924(c) Conviction versus No FSE	[2] Guideline FSE versus No FSE
	Odds Ratios	Odds Ratios
ln(Marijuana Equivalent Grams)	0.88**	1.01
Five-Year Mandatory Minimum	1.11	0.81
Ten-Year Mandatory Minimum	0.89	1.08
Primary Drug Type (Reference: Powder Cocaine)		
Heroin	0.57	1.31
Methamphetamine	1.50	1.55
Crack Cocaine	1.02	0.86
Marijuana	0.86	0.49
Other Drugs	0.58	0.24
Highest Role in the Offense (Effect Coded)		
Laundering	1.34	0.51
Importing	0.26**	0.47
Producing	1.84	1.72
Wholesaling	0.80	1.35
Distributing NOS	1.17	0.77
Retailing	0.90	1.36
Possessing	1.82	1.74
Aggravating Role Adjustment (0-3)	1.18	1.34
Mitigating Role Adjustment (0-3)	0.85	1.24
Used Firearm During Offense	21.12***	8.58***
Criminal History Category (1-6)	1.07	0.88
Guilty Plea	0.77	0.66*
Charge Bargain	1.14	1.09
Pretrial Release	0.53*	0.60*
Race (Reference: Black)		
White	0.57	0.82

Table 4.11 (continued)

Hispanic	0.39*	0.59
Other Race	0.50	0.49
Male	1.63	1.03
ln(Age at Offense)	0.74	0.71
Years Education Completed (0-18)	0.99	0.97
Non-U.S. Citizen	0.34***	0.92

*p<0.05, **p<0.01, ***p<0.001

All Models: Number of strata = 7; Number of PSUs = 40
Population: n = 4,025; N = 88,807

Model [1]: F(28, 6) = 12.72, p = 0.002; McKelvey & Zavoina's pseudo-R^2 = .25
Subpopulation: n = 1,510; N = 49,698

Model [2] F(28, 6) = 7.72, p = 0.009; McKelvey & Zavoina's pseudo-R^2 = .12
Subpopulation: n = 1,497; N = 50,451

likelihood is more than twice as great for the 924(c) conviction (OR = 21.12) compared to the Guideline FSE (OR = 8.58).

Four specific factors were hypothesized to result in the uneven application of firearm sentence enhancements: drug quantity, mandatory minimum exposure, guilty pleas, and charge bargaining. The results in Table 4.11 reveal that drug quantity significantly impacts 924(c) but not Guideline FSE application. Specifically, each log-unit increase in marijuana equivalent grams results in 12% lesser odds of receiving a 924(c) conviction. In comparison, the probability of receiving the Guideline FSE does not change significantly as a function of drug quantity.

Since the interpretation of log-unit changes is not particularly intuitive, the effect of drug quantity is supplemented with a graphical analysis. Figure 4.21 displays the predicted probabilities of gun-involved offenders receiving either a 924(c) conviction or the Guideline FSE as a function of drug quantity, after setting all other variables to their mean values.[45] As the graph makes clear, there is a dramatic and significant drop-off in the probability of defendants receiving a 924(c) conviction as quantity increases. For example, all else equal, a gun-involved defendant who was responsible for 2.5 marijuana equivalent kilograms (corresponding to guideline sentencing range of 10-16 months) has a .63 probability of receiving a 924(c) conviction, whereas a gun-involved defendant convicted of 30,000 marijuana equivalent kilograms (corresponding to guideline sentencing range of 253-293 months) has a .33 probability of receiving a 924(c)

[45] The 95% confidence intervals are not included because the graph becomes too convoluted. However, separate graphs of 924(c) and Guideline FSE predicted probabilities that include the 95% confidence intervals are presented in Figures B4 and B5 in Appendix B.

conviction. Similar probability estimates for receipt of the Guideline FSE are .40 and .43, respectively. This supports the circumvention hypothesis that offenders with larger amounts of drugs, and thus sentencing exposure, are less likely to receive the more severe 924(c) conviction.

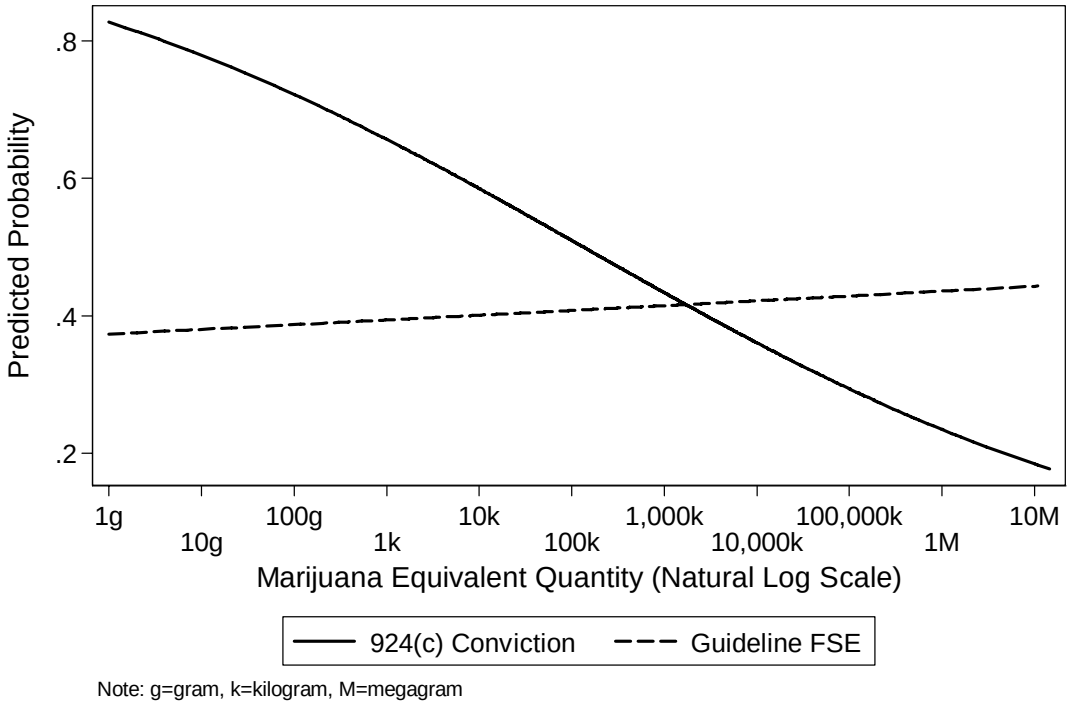


Figure 4.21: Predicted Probability of Gun-Involved Offenders Receiving an FSE as a Function of Drug Quantity

The effects of five- and ten-year mandatory minimum exposure are not significant in either model after partialing the effects of drug quantity. However, it is likely that a more accurate measure of mandatory minimum status would achieve a different result. The outcomes regarding plea and charge bargaining are rather surprising. Only the effect of pleading guilty impacted FSE outcomes, and just for the Guideline FSE. In particular,

offenders who pled guilty have 34% lesser odds of receiving the Guideline FSE, net of other factors. This suggests that prosecutors systematically avoid stipulating warranted gun activity to the court in exchange for a defendant's guilty plea. Why this type of manipulation fails to show in other situations is not clear. It could be that drug quantity is absorbing most of the variance with respect to guilty pleas in 924(c) convictions. As far as charge bargaining is concerned, these results indicate that, aside from possible equity concerns with already severe quantity-based sentences, prosecutors are not systematically using their discretion to drop otherwise warranted firearm sentence enhancements.

The results presented in Table 4.11 reveal some other notable findings. First, although role in the offense is largely unrelated to FSE outcomes, importers face 74% lesser odds of receiving a 924(c) conviction relative to offenders of average culpability, and the 53% lesser odds associated with the Guideline FSE is marginally significant (p = 0.053).[46] This suggests that, as a group, traffickers importing drugs into the United States reduce their risk of apprehension by not carrying firearms. Pretrial release is also associated with 40-47% lesser odds of receiving either firearm sentence enhancement, net of other factors—an indication that judges are less likely to grant release to detainees with more dangerous offense behaviors.

4.6.3 Summary of FSE circumvention analyses

These results indicate that as many as 28% of drug offenders evade a firearm sentence enhancement when one appears legally warranted. This evasion confers an approximate

[46] Because the multinomial logistic regression estimator is more efficient, this effect achieves significance at conventional levels in the model reported in Table B5 in Appendix B.

five-year unwarranted sentencing advantage compared to similarly situated offenders who end up receiving an FSE. The possibility of unwarranted sentencing disparity occurring between those who deny and acknowledge offense-related firearm involvement was also noted.

The multivariate analyses confirmed that certain factors are associated with the uneven application of firearm sentence enhancements. Most notably, as drug quantity increases, the odds of receiving a 924(c) conviction decline significantly. This outcome supports the hypothesis that prosecutors are less likely to pursue a severe 924(c) conviction as drug quantity (and sentencing exposure) increases. In comparison, application of the typically less severe Guideline FSE does not depend on drug quantity. In general, mandatory minimum exposure and plea and charge bargaining practices do not significantly impact FSE outcomes. The exception occurs for guilty pleas in Guideline FSE outcomes, which results in 34% lesser odds of receiving the enhancement.

5.0 SUMMARY AND DISCUSSION

This dissertation research focused on how legally prescribed offense seriousness criteria and sentencing rules influence drug sentencing outcomes. The central problem addressed by this research is that the federal drug sentencing guidelines fail to ensure similar treatment for similar offenders and different treatment for different offenders. Drawing from the sentencing literature and a 'harm-based modified just deserts' theoretical framework, the analyses were structured on the premise that an overemphasis on quantity in the drug sentencing guidelines, in concert with quantity-based mandatory minimums, opens the door to two kinds of unwarranted disparity. The first involves the imposition of different sentences on similar offenders (i.e., unwarranted differentiation), and the second involves the imposition of similar sentences on different offenders (i.e., unwarranted similarity).

Data from the *1997 Survey of Inmates in Federal Correctional Facilities* were used to perform a series of six analyses examining evidence for unwarranted disparity in drug offense sentencing outcomes. The main findings relate to multivariate analyses focusing on sentence length, mandatory minimum, and firearm sentence enhancement outcomes. The multivariate analyses employed a comprehensive array of relevant sentencing predictors, including measures of drug offense seriousness (e.g., drug type and quantity, role in the offense, firearm use), criminal history, case processing (e.g., guilty plea, charge bargaining), and sociodemographic characteristics (e.g., race, gender, citizenship). Several advanced methods were employed to reduce bias and improve the

efficiency of the model estimates, including imputation of missing values to deal with item nonresponse, design-based estimation to account for the survey's complex sampling design, and the truncated-censored regression estimator to handle limited response on the dependent variable, sentence length.

Section 5.1 summarizes the results of the dissertation's six research objectives, and Section 5.2 discusses and interprets these findings in greater detail. Section 5.3 draws theoretical and policy implications from the study. Finally, Section 5.4 suggests options for future research.

5.1 SUMMARY OF RESULTS

5.1.1 How well does drug quantity serve as a proxy for other elements of offense seriousness?

In general, drug quantity correlated weakly with the culpability- and dangerousness-based offense seriousness factors. As expected, drug quantity was found to be positively correlated with higher-level roles in the offense for four out of five primary drug types (i.e., heroin, methamphetamine, and crack and powder cocaine), but the associations were weak. For marijuana, there was no observed linear association. Thus, aside from marijuana offenders, possessors and retailers generally handled the smallest drug amounts; unspecified distributors and wholesalers mid-range amounts; and producers, importers, and money launderers the largest drug amounts. Nevertheless, despite these propensities, there were wide quantity overlaps across roles, indicating that drug quantity is ultimately an imperfect indicator of the defendant's role in the offense.

Exposure to quantity-based mandatory minimums was also found to be positively correlated with both higher-level roles in the offense and leadership/managerial positions in drug trafficking organizations, although these associations were even weaker. Indeed, exposure to mandatory minimum sentencing was uniform across all roles except possessing (less) and money laundering (more). This relative uniformity was also observed with respect to an offender's participation and function in a drug trafficking organization. In particular, there was little separation in mandatory minimum exposure between nonparticipants and low-end organizational sellers, on the one hand, and organizational leaders and middlemen, on the other hand. As expected, however, there was one clear exception for the most peripherally involved coconspirators: nearly all were exposed to mandatory minimum sentencing. Moreover, these peripheral participants were disproportionately women (e.g., wives, girlfriends, hangers-on). In short, quantity-based mandatory minimums failed to target the most culpable drug traffickers, and even disproportionately exposed some of the least culpable offenders to the severest sanctions.

The association between drug quantity and the dangerousness-based offense factors generally ran counter to the expectation that offenders responsible for larger drug quantities would be more likely to possess and use firearms. In fact, gun activity was negatively correlated with quantity across all drug types, although the associations were significant only for (1) self-reported firearms use/possession among powder cocaine offenders and (2) firearm sentence enhancement outcomes among heroin, powder cocaine, and marijuana offenders. Nevertheless, the overall pattern of results indicates that the more dangerous offenders tended to traffic in smaller drug quantities. Examining the basis for this set of findings revealed that the more dangerous firearm-involved

offenders were more likely to function at the lower distribution levels, where the quantities trafficked were characteristically smaller. It is also likely that selection issues are operating in that it takes smaller drug quantities to expose dangerous offenders to prison time. In short, based on these data, quantity was not a good indicator of offender dangerousness. Overall, drug quantity was imperfectly associated with other offense seriousness factors.

5.1.2 How much influence does drug quantity have on sentence length relative to other elements of offense seriousness?

Consistent with the sentencing guidelines, drug quantity was the strongest predictor of sentence length. Aside from the poorly measured mitigating role adjustment, other guideline-based offense seriousness factors also had significant net effects. In particular, the aggravating role adjustment, safety valve, and firearm sentence enhancements significantly influenced sentence length in the expected ways and, with the exception of the Guideline FSE, all were in the top third in strength of effect. In contrast, functional role in the offense, which is not explicitly accounted for by the guidelines, had minimal impact on sentence length. Moreover, the relative effects across roles were not proportional to culpability, instead revealing a pattern of sentencing inversion and uniformity. Specifically, the sentences for wholesalers, unspecified distributors, and retailers were no different from each other, and money launderers and importers had shorter average sentences than all other roles except possessors. In short, as expected, drug quantity was by far the strongest predictor of sentence length among the offense factors, with the culpability- and dangerousness-based guideline factors adjusting

sentences up or down consistent with their import in the guidelines. However, the sentencing effects of the non-guideline role in the offense factors were negligible and inconsistent with proportionality.

Notably, after drug quantity, case processing factors such as pleading guilty, charge bargaining, and pretrial release status were among the strongest predictors of sentence length. In fact, the absolute standardized effect of pleading guilty on sentence length was nearly equal to that of drug quantity, and pretrial release status—the third strongest predictor in the model—was not too far removed. Thus, as a group, procedural matters governed by prosecutors and the courts were stronger predictors of sentence length than indicators of offender culpability or dangerousness.

5.1.3 To what extent does the 100-to-1 crack-powder quantity ratio disadvantage crack cocaine offenders?

Several aspects of the 100-to-1 crack-powder quantity ratio were investigated. Both the descriptive and multivariate results confirmed a distinct crack cocaine sentencing disadvantage, as expected. The descriptive analyses showed that powder cocaine offenders predominated in upper- to mid-level roles in the offense, whereas crack cocaine offenders were concentrated in mid- to low-level roles. The results also showed that average quantities for powder cocaine dramatically exceeded those for crack cocaine. However, despite being responsible for substantially smaller drug amounts and being relatively less culpable, crack cocaine offenders received longer average sentences than their powder cocaine counterparts. Overall, this translated into a crack-powder penalty ratio per kilogram of drug of approximately 25:1. The multivariate analyses indicated

that, after controlling for other variables, crack offenders experienced an average 21-month sentencing disadvantage. Evaluated over the range where crack and powder cocaine quantities most overlap (i.e., between 85g and 3kg), the net differential ranged between 19 and 24 months, or about 1.5 to 2 years.

The results also confirmed a consistent pattern of disproportionate sentencing outcomes disfavoring lower-level crack cocaine offenders. That is, less culpable crack offenders were nearly always sentenced to longer average sentences than more culpable powder cocaine offenders. The more notable and significant findings indicated that sentences for crack retailers and wholesalers were approximately two to three years longer on average than powder cocaine importers/producers and money launderers. Possessors represented the only role among crack cocaine offenders to receive consistently shorter sentences than all powder cocaine offenders, and this was because crack possessors were sentenced relatively leniently. In short, with the exception of crack possessors, lower-level crack cocaine offenders were sentenced more severely than higher-level powder cocaine offenders.

The final analysis attempted to estimate the racial impact of eliminating the 100-to-1 crack-powder quantity ratio. This was achieved statistically by predicting adjusted mean sentences for crack offenders sentenced "as if" they were powder cocaine offenders. The estimates revealed that the sentencing gap between blacks, on the one hand, and whites and Hispanics, on the other hand, would have been reduced by an estimated 11-12 months on average. Moreover, fully 90% of the prison years averted from eliminating the 100-to-1 quantity ratio would have benefited blacks.

5.1.4 Is there evidence of the tariff effect in federal drug sentencing?

Two complementary analyses were conducted to explore whether quantity-based mandatory minimums undermine proportionality along other dimensions of offense seriousness by engendering flat, tariff-like sentences. The first analysis examined simple exposure to mandatory minimums, and the second examined actual sentencing outcomes among those offenders with mandatory minimum behaviors based on drug quantity. As expected, these results confirmed that quantity-based mandatory minimums produce excessive uniformity among other offense seriousness factors. The results also showed that the safety valve is working as intended to limit the applicability of mandatory sentences to certain nonviolent, low-level offenders. Finally, the results highlight the strong role of prosecutorial discretion in determining mandatory minimum sentence outcomes.

Specifically, the first analysis revealed a general proportionate increase in mandatory minimum exposure as one moved up the chain of culpability from possessors to money launderers. Exceptions to this pattern occurred for importers and producers, both of which had lesser odds of mandatory minimum exposure than lower-level wholesalers and distributors. In contrast, aggravating and mitigating role adjustments, the safety valve, the guideline-based firearm sentence enhancement, self-reported firearm use, and criminal history all had no bearing on mandatory minimum sentencing exposure. Notably, however, offenders who received the five-year mandatory minimum 924(c) penalty faced significantly *reduced* odds of also receiving a drug mandatory minimum. Finally, offenders who pled guilty also experienced significantly lesser odds of mandatory minimum exposure. Thus, as expected, mandatory minimum exposure was

generally not proportional to the culpability or dangerousness of the offender. Only the offender's role in the offense was partially scaled to proportionate increases in mandatory minimum exposure, and some factors (i.e., 924(c) conviction, plea-bargaining) were inversely related.

The second analysis examined whether offenders who had mandatory minimum behaviors based on drug quantity were sentenced to the applicable penalty. The analysis was even more revealing regarding the uniformity-producing effects of mandatory minimum sentencing. Virtually all offense-related factors had no influence on whether an offender was sentenced to the requisite quantity-based mandatory minimum. Notably, the safety valve was the only factor to significantly impact mandatory minimum outcomes, demonstrating its success in limiting the applicability of mandatory minimums to certain nonviolent, low-level offenders who accept responsibility for their crimes. In other words, barring alleviation under the safety valve, offenders of varying culpability, dangerousness, and criminal backgrounds were treated uniformly with respect to the application of tariff-like drug mandatory minimums. The results also revealed the strong impact of prosecutorial discretion in plea and charge bargaining decisions on mandatory minimum sentencing outcomes. Similar to sentencing in general, this finding indicates that procedural matters governed by prosecutors and judges have more influence on mandatory minimum sentencing outcomes than the culpability or dangerousness of the offender.

5.1.5 Is there evidence of the cliff effect in federal drug sentencing?

This analysis examined evidence for the cliff effect occurring around the five- and ten-year mandatory minimum quantity triggers for the five major drugs (i.e., heroin, methamphetamine, crack cocaine, powder cocaine, and marijuana). The evidence for this type of cliff effect in federal drug sentencing is generally disconfirmed. The sentences for heroin, methamphetamine, and crack cocaine across the five-year mandatory minimum threshold and for all drugs across the ten-year mandatory minimum threshold were not significantly different. In fact, most of these the contrasts revealed a flat or inverse sentencing effect rather than the expected sharp increase. Significant differences in sentence length across the mandatory minimum thresholds were observed only for powder cocaine and marijuana around the five-year mandatory minimum. However, these sentencing differentials—33 months for powder cocaine and 24 months for marijuana—were not large enough to suggest a "cliff" between the pre and post mandatory minimum quantity levels examined. In fact, the corresponding guideline sentencing ranges could readily account for the differences.

Although the cliff effect was examined across relatively wide quantity ranges due to small *n* considerations (i.e., 50% above and below the quantity triggers), this very fact highlights that few drug offenders actually possessed drug quantities in the narrow bands around the mandatory minimum quantity triggers (i.e., .01 to 5 grams) typically cited by observers of the cliff effect (Froyd 2000; Oliss 1995; Schulhofer 1993; Steer 2000; USSC 1991b; Vincent and Hofer 1994; Wilkins, Newton, and Steer 1993). This suggests that even if the cliff effect causes a degree of sentencing disparity that this study could not detect, it is not likely to be a widespread source of disparity in federal drug sentencing. In

fact, this study finds that a more likely source of disparity in the interaction between the sentencing guidelines and mandatory minimums is the flattening (and inversion) of sentences that occurs around the mandatory minimum thresholds. In short, this "plateau effect" may be a greater disparity-inducing problem than the "cliff effect" in federal drug sentencing.

5.1.6 What is the nature and extent of firearm sentence enhancement circumvention in federal drug sentencing?

This study investigated the extent to which firearms sentence enhancements are circumvented, the consequent sentencing implications of this circumvention, and the factors that lead to uneven FSE application. The results indicated that as many as 28% of drug offenders evaded a firearm sentence enhancement when it appeared legally warranted. However, to the extent that evidentiary matters precluded the legitimate pursuit of a warranted FSE (which cannot be determined by these data), the actual percentage could be correspondingly smaller. A trivariate analysis of mean sentence length by FSE status and self-reported firearm use/possession suggests that offenders who evade an FSE receive an approximate five-year sentencing advantage relative to similarly situated offenders who end up receiving an FSE. The results also suggest the possibility of a two- to three-year sentencing disadvantage for those who denied offense-related firearm involvement, but nevertheless received an FSE. However, it was not possible to discern how much of this difference actually represented unwarranted disparity related to harsher punishment for ostensibly less egregious offense conduct

(e.g., constructive possession versus active use) or a warranted benefit bestowed upon those who accepted responsibility for their actions.

The multivariate analyses confirmed that certain factors are associated with the uneven application of firearm sentence enhancements. Most notably, the odds of receiving a 924(c) conviction declined significantly as drug quantity increased. In comparison, the application of the typically less severe Guideline FSE was not associated with changes in drug quantity. This outcome supports the hypothesis that prosecutors, yielding to equity concerns of overly harsh punishment, are less likely to pursue the relatively more severe 924(c) conviction as drug quantity (and sentencing exposure) increases. In general, mandatory minimum exposure and plea and charge bargaining practices did not significantly affect FSE outcomes. The exception occurred for guilty pleas in Guideline FSE outcomes, which resulted in 34% lesser odds of receiving the enhancement. In summary, these results suggest that prosecutors systematically (1) manipulate the applicability the Guideline FSE in sentence calculations (possibly with the acquiescence of other court actors) as an incentive for the defendant to plead guilty and (2) circumvent 924(c) charges out of equity concerns with already severe quantity-based sentences. Otherwise, prosecutors do not appear to use their discretion systematically to drop warranted firearm sentence enhancements.

5.2 DISCUSSION

5.2.1 The "crude surrogate"

This study finds that drug quantity—as a measure of harm—provides at best a rough approximation of the defendant's level of culpability. Yet, both the sentencing guidelines

and the mandatory minimums accord drug quantity a central role in determining sentences for drug offenders. A primary justification for granting drug quantity this central role has been that it adequately accounts for the defendant's level of culpability and importance in the drug trade. As Charles Tetzlaff of the United States Sentencing Commission testified to Congress:

> For the overwhelming majority of drug offenders, the drug quantity serves as a reasonable initial proxy both for the harm caused by the offense and the trafficking function performed by the offender. In other words, offenders who perform higher trafficking functions, such as organizers, manufacturers, supervisors, and managers, tend to be held accountable under the guidelines for the largest quantities of drugs, and offenders who perform lesser functions tend to be held accountable for smaller quantities. *Thus, for the overwhelming majority of offenses, there does not appear to be any tension between the assignment of the offender's offense level based on drug quantity and the role of the offender.* (Tetzlaff 2001-2002: 234, emphasis added).

Mr. Tetzlaff's statement is based on the Sentencing Commission's *2002 Cocaine Report* that concluded, for both powder cocaine and crack cocaine offenders sentenced in 1995 and 2000, "the most culpable offenders (i.e., importers/high-level suppliers) generally were held accountable for greater drug quantities than lower level offenders" (USSC 2002: 45).

This dissertation's quantity-by-role distributional analyses covering a wider array of drugs revealed the generality to be much less precise.[47] While higher-level money launderers, importers, and producers were held responsible for the largest average drug quantities, and lower-level retailers and possessors were held responsible for the smallest

[47] Sevigny and Caulkins (2004) analyzed the same 1997 Inmate Survey data using different operationalizations for drug quantity (i.e., a single measure of "standard retail amounts") and role in the offense. Their results are generally consistent with those reported here and will not be reviewed further.

average drug quantities, the observed associations were weak. Indeed, there were substantial drug quantity overlaps across every role in the offense. To provide an example, one-quarter of both powder cocaine possessors and importers were held responsible for between 1 and 5 kilograms of powder cocaine. For possessors this quantity range represented the third quartile of the distribution, and for importers it was the second quartile. Thus, while quantity distinguished between the two roles on average, fully one-quarter of these differently culpable offenders faced the same quantity-based penalties. Thus, despite the tendency for average quantities to coalesce into distinct tiers across roles in the offense, the wide distributional overlap creates considerable tension between quantity and role in a large number of cases.

Notably, this dissertation found more differentiation in quantity across functional roles than the Department of Justice's (DOJ 1994) study of federal drug inmates sentenced in 1992. Combining different drugs into a single marijuana equivalency measure and comparing this distribution across high-level dealers, street-level dealers, couriers, and peripheral role offenders, DOJ concluded that "regardless of the functional role a defendant played in the drug scheme, the drug amounts involved in the offense are similar across roles" (DOJ 1994: 2119). Two issues mitigate the force of this conclusion. First, the use of marijuana equivalencies probably masks some of the variation. Second, the study employed an unrepresentative sample that excluded offenders who "participat[ed] in sophisticated criminal activity" or received an aggravating role adjustment. Naturally, then, the analyses would be biased toward not finding a difference (Roth 1994).

In short, this dissertation finds that drug quantity serves as a rough, although in many cases woefully inadequate, proxy for the defendant's level of culpability. This is somewhat more encouraging than DOJ's assessment, but far less optimistic than the Sentencing Commission's evaluation. To be sure, the Commission's reports and staff testimony have highlighted the need to address the anomalous results that occur for the "limited number" of couriers, mules, and drug handlers who are held accountable for exceptionally large drug quantities (Tetzlaff 2001-2002; USSC 2002). However, the present study finds the association between quantity and role to be less exact than the Sentencing Commission's analysis would suggest.

The discrepancy between quantity and role also extends to mandatory minimums. Consistent with the existing research (DOJ 1994; USSC 2002), this study finds a high level of uniformity in mandatory minimum sentencing exposure across different roles in the offense and levels of responsibility in drug trafficking organizations.[48] In short, the studies concur in finding that the five- and ten-year mandatory minimum penalties have generally failed to target the "serious" and "major" drug traffickers, as was Congress' intent. Not only have the mandatory minimums failed in this regard, but, as this dissertation indicates, they also open the door to perverse outcomes by exposing some of the least culpable coconspirators to the most severe sanctions.

[48] While it may seem peculiar that the Sentencing Commission concluded in the *2002 Cocaine Report* that, on the one hand, "the most culpable offenders…generally were held accountable for greater drug quantities than lower level offenders" and, on the other hand, "exposure to mandatory minimum penalties does not decrease substantially with offender culpability" (USSC 2002: 45, 48), this study would have reached similar conclusions had it looked only at central and not distributional tendencies.

This dissertation also examined how drug quantity relates to gun activity, and found, contrary to expectations, that quantity is inversely related to offender dangerousness. That is, the more dangerous drug offenders who used and possessed firearms tended to traffic in smaller drug quantities. The original hypothesis was derived from the available empirical research (Lizzotte, Krohn, Howell, Tobin, and Howard 2000) and the commonsense notion that larger scale distributors with more cash and drugs would be more likely to use and possess firearms. This hypothesis was obviously not supported by the results. Rather, it turns out, the more dangerous firearm-involved offenders were more likely to function at the lower distribution levels, namely retailing and possessing, where drug quantities are characteristically smaller.[49] Thus, this dissertation finds that drug-related gun activity has more to do with the functional role of the offender and perhaps the context of the drug transaction than it does with the amount of drugs trafficked. In short, drug quantity is simply a poor indicator of offender dangerousness, as measured by firearms activity.

[49] Conceptually, there are several possible explanations for this finding. The competitive and uncertain nature of retail markets is an obvious factor. As one observer puts it, "defendants, both buyers and sellers, involved in simple possession or small quantity illegal drug transactions frequently justify their need to carry firearms as protection against being robbed by rival dealers, gang members or others" (Wulff 2005: 6). This is particularly true regarding the subculture of handguns that predominated in the open-air retail-level crack markets of the late 1980s and early 1990s (Johnson, Golub, and Dunlap 2000). These violent markets garnered substantial law enforcement attention, which put many retail-level dealers in prison for lengthy terms. Lower-level distributors also tend to be younger, and thus at peak ages for committing violence. Another possibility is that lower-level operators are more apt to employ violence in order to move up the distribution ladder than higher-level agents are to keep them down (MacCoun, Kilmer, and Reuter 2003). Some of these possibilities are amenable to further analysis using the Inmate Survey data.

In summary, drug quantity, as the main sentencing factor under the guidelines and mandatory minimums, fails to distinguish adequately between offenders with differing levels of culpability and dangerousness. While drug quantity may be consistent in the aggregate with a defendant's role in the offense or organizational responsibility, quantity-driven sentencing is simply too blunt to meet the demands of proportionality. Nor is quantity a good indicator of drug-related gun activity. These findings are succinctly captured in comments by Stephen Schulhofer (1992a: 854): "Drug quantity is sometimes a plausible surrogate for an offender's dangerousness, culpability or level of organizational responsibility. At best it is a crude surrogate." To be sure, quantity is an important sentencing factor and may even be a good starting point for tailoring sentences, but quantity cannot be the sole or even prime sentencing factor without running the risk of introducing unwarranted sentencing inequalities.

5.2.2 The tyranny of quantity

The problems with the quantity-dominated sentencing guidelines and mandatory minimums are borne out by this dissertation's multivariate sentence length analysis. As expected, drug quantity was the primary determinant of sentence length among the offense seriousness factors, with the culpability- and dangerousness-based guideline adjustments significantly influencing sentences to a lesser degree and in expected ways. However, the analysis also showed that quantity-driven sentencing, together with guideline-based adjustments that are too limited in scope or applicability, led to inverted and flattened sentences across important dimensions of culpability. This failure to ensure proportionality in sentencing is further compounded by discretionary plea and charge

bargaining practices that had equal, if not stronger, effects on sentence length than legally prescribed offense seriousness factors.

That drug quantity was the primary determinant of federal drug sentences is not surprising given its central role in the guidelines and mandatory minimums. Prior research has also found that quantity plays a strong role in drug sentencing outcomes (McDonald and Carlson 1993; Peterson and Hagan 1984; Rhodes 1991; Semisch 2000), but only the Department of Justice's (1994) study substantiates the current finding that quantity plays the *strongest* role. As explained in Chapter 2, however, studies that found drug quantity to have a lesser (or even inverse) effect on sentence length (1) employed estimation models that were overspecified (Kautt 2001-2002; Kautt and Spohn 2002; Pasko 2002), (2) used alternate (and occasionally ill-specified) variable operationalizations of drug quantity (Bush-Baskette 2000; Kautt and Spohn 2002; Pasko 2002; Peterson and Hagan 1984; Semisch 2000), or (3) failed to report standardized regression estimates to enable direct comparison of effects (McDonald and Carlson 1993; Rhodes 1991). In short, these studies were poorly specified to answer questions about the relative importance of drug quantity as a sentencing factor. In this regard, both DOJ (1994) and the present study provide the most accurate assessments of the relative influence of drug quantity in sentencing outcomes.

After drug quantity, the culpability- and dangerousness-based offense seriousness factors influenced sentence length secondarily and only to the extent they were explicitly accounted for by the sentencing guidelines. For instance, both the Guideline FSE (USSG 2D1.1 (b)(1)) and 924(c) conviction (USSG 2K2.4) significantly increased sentences for dangerous firearm-involved offenders, and the aggravating role adjustment (USSG

3B1.1) and the safety valve (USSG 5C1.2) adjusted sentences up and down, respectively, for offenders of differing culpability. Only the mitigating role adjustment (USSG 3B1.2) failed to show the intended effect, but, as noted in Section 3.6, this outcome is likely due to poor measurement of this construct. Thus, with the exception of the mitigating role adjustment, these outcomes are consistent with prior drug sentencing research (Albonetti 2002b; McDonald and Carlson 1993; Pasko 2002; Semisch 2000).

Although the guideline-based sentencing factors generally operated as intended, they were too limited in scope and applicability to counterbalance the proportionality-distorting impacts of drug quantity. Indeed, the sanctioning outcomes across functional roles in the offense reveal a toppled and flattened pyramid of sentencing liability: Sentences for offenders at the very top of the distribution chain fell at or below those for lower-level offenders, and the sentences for mid- to low-level distributors were all but indistinguishable. This latter condition typifies the problem of unwarranted similarity (or excessive uniformity) of sentences. In particular, the sentences for wholesalers, unspecified distributors, and retailers—who together account for 68% of all drug offenders—were practically identical. Moreover, from a statistical standpoint, the sentences for importers and possessors—representing another 20% of drug offenders—were no different from the wholesalers, unspecified distributors, and retailers. In other words, after controlling for other variables, the sentences for nearly nine in ten drug offenders were effectively uniform across functional roles in the offense.

The toppled part of the drug distribution pyramid typifies the problem of unwarranted inversion of sentences. In this instance, importers and money launderers at the top of the distribution chain received sentences disproportionately shorter than lower-

level offenders. Given that money launderers and importers were typically held accountable for the largest drug amounts, there are two plausible explanations for the inverted sentencing outcomes. First, the selective granting of 5K1.1 substantial assistance departures can result in what has been called the "cooperation paradox," whereby higher-level offenders who have more knowledge and responsibility in the drug trade exchange information with the prosecutor in order to avoid sentencing under the harsh mandatory minimums—something lower-level agents are unable to take advantage of due to their limited knowledge of the conspiracy (Hrvatin 2002; Simons 2002). Exploratory research by Maxfield and Kramer (1998), however, found that substantial assistance departures were more likely to be granted to less culpable defendants, perhaps because they were viewed sympathetically. Substantial assistance departures were not explicitly controlled for in this dissertation because no such variable existed in the Inmate Survey. Second, the analysis may not adequately account for mitigating role sentence reductions granted to minor coconspirators (e.g., couriers, mules, and moneyrunners) who operate at the upper distribution levels. In short, better controls for substantial assistance departures, the mitigating role adjustment, and courier/mule status might erase the observed inverted sentencing effect, although probably not to the point that excessive uniformity ceases to be a problem for these offenders.

Notably, this dissertation provides the only multivariate sentence length analysis in the literature that independently controls for an unrestricted range of functional roles in the offense. Semisch (2000) includes a 20-level ordinal variable that ranges from importer/high-level supplier to user-only in her analysis, and finds a significant effect on sentence length. Unfortunately, this construction does not allow for an independent

assessment of specific functional roles. DOJ (1994) examined sentencing outcomes for launderers/manufacturers, mid- to high-level dealers, street-level dealers, couriers, and peripheral role offenders (after selecting out the highest-level offenders). The results showed that launderers/manufacturers and mid- to high-level dealers received significantly longer sentences than peripheral role offenders. However, street-level dealers and couriers received sentences that were no different from peripheral role offenders. These last three roles represent about two-thirds of the offenders in the sample, which is consistent with this dissertation research finding excessive sentencing uniformity for the majority of drug offenders.

The analysis also revealed the strong mitigating influence of plea and charge bargaining in drug sentencing outcomes. The absolute effect of pleading guilty on sentence length was nearly equal to that of drug quantity and charge bargaining had an equal absolute effect to receiving a 924(c) conviction. That prosecutorial discretion in the granting of plea and charge bargains can impact sentences to the same extent as the legally relevant offense seriousness factors raises additional concerns about the success of the guideline effort. For one, it appears to confirm fears that the guidelines and mandatory minimums have not channeled judicial discretion as much as they have transferred discretion from judges to prosecutors (Alschuler 1991; Nagel 1990; Stith and Cabranes 1998; Wilmot and Spohn 2004). Prosecutorial plea and charge bargaining practices represent unreviewable and unsystematic downward departures that operate outside the purview of the guidelines To the extent that this type of discretionary circumvention results in different sentences for like offenders, unwarranted disparity will result.

While uncontrolled prosecutorial discretion thwarts the intent of the guidelines, Nagel and Schulhofer (1992: 557) note that "sentences resulting from guideline evasion are [not] necessarily 'wrong.' Sometimes they are. But sometimes guideline circumvention produces arguably just results." While it is not possible to discern the motivating reasons behind the granting of plea and charge bargains for offenders in the Inmate Survey sample, empirical research on prosecutorial practices reveals that guideline circumvention in drug cases is often framed by a desire to achieve fair sentences for sympathetic defendants (Bowman and Heise 2002; Nagel and Schulhofer 1992; Schulhofer and Nagel 1989; USSC 1991a). This body of research found several ways in which charge bargaining operates in federal drug cases, including dismissing or not filing 924(c) charges, not pursuing career offender enhancements, dismissing more severe charges in exchange for a plea to a lesser charge such as a "phone count" or simple possession. Other methods of manipulation included stipulating to a different substance (i.e., powder vs. crack cocaine) or to a lesser drug amount and recommending a mitigating role reduction when it is not warranted by the facts of the case. The bottom line is that "prosecutors, and the judges they appear before, use their discretion liberally, but irregularly, to reduce drug sentences" (Bowman and Heise 2002: 559).

In summary, these findings suggest that the overemphasis on drug quantity in the guidelines and mandatory minimums results in a number of anomalous outcomes. Essentially, the dominance of drug quantity as a sentencing factor, when combined with guideline-based sentencing adjustments that fail to adequately account for important offender differences, leads to excessive uniformity where "the big fish, the minnows, and the superminnows wind up in the same sentencing boat" (Schulhofer 1992b: 170).

Finally, the harsh sentences that arise from quantity-based sentencing result in a significant amount of guideline circumvention. While this evasion is often undertaken to achieve just results, it nevertheless undermines consistency under the guidelines.

5.2.3 The 100-to-1 quantity ratio: Rule-based disparity and disharmony

The 100-to-1 quantity ratio defines a penalty structure in which it takes 100 times the amount of powder cocaine as it does crack cocaine to achieve the same sentence. This dissertation found this rule-based differential resulted in an average net sentencing gap of 21 months (103 months for powder versus 124 months for crack), or about a 21% net differential. This estimate sits below the low end of previous Department of Justice and Sentencing Commission estimates (i.e., 25-60%). There are several reasons for this. First, the crack-powder cocaine sentencing gap was generally smaller during the earlier part of the 1990s (DOJ 2002; USSC 2002), which are the years covered by the Inmate Survey. Second, the Sentencing Commission and Department of Justice estimates are based on sentencing flow data, whereas the this study's estimate is based on prison stock data. Analyses of the crack-powder sentencing gap using prison stock rather than sentencing flow data will be biased toward finding a smaller differential since powder cocaine offenders are released from prison systematically earlier than crack cocaine offenders, leaving a relatively more serious pool of powder versus crack cocaine offenders. Finally, the present study's estimate was derived from a multivariate analysis that controlled for a host of other offense and offender characteristics, whereas the Sentencing Commission and Department of Justice estimates were derived from simple bivariate sentence length by drug type associations. Factoring in differences in criminal history and weapons use—

for which crack cocaine offenders have historically higher rates—mitigates some of the difference. In short, this study is consistent with previous studies in the literature in finding a substantial crack-powder cocaine sentencing gap, but differences in years of coverage, data sources, and analytic methods explain the comparatively low-end estimate reported here.

The analyses also revealed that the crack-powder cocaine sentencing gap could be entirely accounted for by the 100-to-1 quantity ratio. On its face, then, the crack-powder sentencing gap does not represent unwarranted sentencing disparity. The 100-to-1 quantity ratio was legally set by Congress in 1986 to reflect the belief that crack cocaine was more physically addictive, associated with greater levels of crime and violence, and particularly damaging to vulnerable groups such as pregnant women and youth (USSC 1995). However, research since 1986 suggests the 100-to-1 quantity ratio overstates the relative harms of crack and powder cocaine (Blumstein 2003; Hatsukami and Fischman 1996; Spade 1996; USSC 1995, 1997, 2002). For example, the Sentencing Commission evaluated the cumulative evidence and concluded that "the 100-to-1 drug quantity ratio was established based on a number of beliefs about the relative harmfulness of the two drugs and the relative prevalence of certain harmful conduct associated with their use and distribution that more recent research and data no longer support" (USSC 2002: 91).

If this were the only indictment of the 100-to-1 quantity ratio, legislators might be excused for their inaction in the face of repeated Sentencing Commission proposals to amend the penalty structure. However, the empirical evidence also indicates that the 100-to-1 quantity ratio distorts uniformity and proportionality in sentencing (DOJ 2002; MacDonald and Carlson 1993; USSC 1995, 2002, 2004). In particular, the evidence

indicates the 100-to-1 quantity ratio leads to sanctions that (1) apply most often to crack cocaine offenders at the lowest rungs of the distribution ladder, (2) overstate the culpability of most crack cocaine offenders and fail to assure adequate proportionality, and (3) adversely impact blacks and other minorities (USSC 2002).

This dissertation examined and found evidence to support all three claims. First, the results confirmed Sentencing Commission findings that the current 100-to-1 penalty structure fails to target the most serious crack cocaine traffickers. Specifically, compared to powder cocaine offenders, crack offenders were overrepresented among low-level possessors and retailers, and underrepresented among high-level launderers, importers, and producers. Moreover, crack offenders were less likely to be involved in a drug trafficking organization and, if they were, to be a leader, organizer, or middleman. These results are consistent with Sentencing Commission research (USSC 1995, 2002) showing that federal crack cocaine penalties "apply most often to offenders who perform low-level trafficking functions, wield little decision-making authority, and have limited responsibility" (USSC 2002: 99-100).[50]

The results of this dissertation also confirmed Sentencing Commission analyses showing that the 100-to-1 quantity ratio fails to ensure adequate sentencing proportionality between crack and powder cocaine offenders of differing culpability (USSC 1995, 2002). Both are congruent in finding that crack cocaine offenders received

[50] Notably, the Sentencing Commission's analysis of year 2000 data indicates the functional roles of crack and powder cocaine offenders were more equally distributed than in 1992 or 1995. This shift was driven primarily by an increase in the number of powder cocaine street-level dealers and couriers/mules, which the Commission suggests is at least partly attributable to greater law enforcement and interdiction efforts on the southern U.S. border since 1995 (USSC 2002). It will be interesting to see if the 2003 Inmate Survey mirrors these findings.

longer average sentences than their powder cocaine counterparts despite being responsible for substantially smaller drug amounts (or dollar amounts of drugs) and playing relatively less culpable roles in the offense. Notably, this dissertation is the first multivariate analysis in the literature to examine and provide evidence of disproportionality in crack and powder cocaine sentencing outcomes across functional roles in the offense. Specifically, with the exception of crack cocaine possessors who were sentenced relatively leniently, the analysis revealed a consistent pattern of inverted penalties whereby less culpable crack offenders received sentences that were longer than more culpable powder cocaine offenders. For example, all else equal, crack cocaine retailers received sentences that were 23-30 months longer on average than (1) similarly situated powder cocaine retailers and (2) more culpable powder cocaine importers/producers and money launderers. In other words, not only do current cocaine penalties apply more often to low-level crack offenders, but the 100-to-1 quantity ratio commonly leads to penalty inversions that thwart the goals of uniformity and proportionality.

Finally, it is well established in the drug sentencing literature that blacks and Hispanics are sentenced more severely than whites, even after accounting for important offense and offender differences (Albonetti 1997, 2002b; Everett and Wotjkiewicz 2002; Mitchell 2005; Spohn 2000). Probably the most contentious aspect of the federal 100-to-1 crack-powder cocaine quantity ratio revolves around perceptions that it creates unwarranted racial disparities in sentencing, particularly for blacks. Consistent with other estimates in the literature (DOJ 2002; MacDonald and Carlson 1993; USSC 2004), this study found that reducing the 100-to-1 quantity ratio would primarily benefit blacks. In

particular, if crack and powder cocaine had been sentenced equivalently, blacks would have averted an average of 14 months on their sentences, which represents a net 11-12 month reduction compared to whites and Hispanics. Put differently, though blacks account for 60% of crack and powder cocaine offenders combined, they would have benefited from 90% of the nearly 24,000 averted prison years. Granting clemency to all federal crack cocaine defendants for a full six-month period would achieve the same effect.[51]

In summary, the current 100-to-1 quantity ratio between crack and powder cocaine fosters anomalous and disparate sentencing outcomes by targeting the least culpable crack cocaine offenders with sanctions that are more severe than the highest-level powder cocaine offenders. This reality contravenes Congress' expressed objectives of assuring uniformity and proportionality in sentencing in general, and that high-level powder cocaine traffickers receive longer sentences than low-level retail crack traffickers in particular (Tison 2003). Moreover, that the 100-to-1 crack-powder penalty scheme disproportionately burdens blacks stands as a further indictment of the fairness of the system. As Alfred Blumstein testified to Congress, "the 100:1 disparity is widely seen as blatant proof of racial discrimination by the criminal justice system, and thereby contributes in important ways to serious challenge to the legitimacy of that system. It is crying for careful reconsideration…" (Blumstein 2002: n.p.). If not discrimination, this

[51] This back-of-the-envelope estimate was calculated by taking the mean number of crack cocaine prison years sentenced in 1997, and then taking as a percentage of this figure the present study's estimated number of prison years averted. Specifically, 4,497 crack offenders were sentenced to an average prison term of 10.4 years in 1997 (USSC 1997), which represents 46,769 prison years. This study's estimate of 23,740 prison years averted represents 51% of that figure, or about six month's worth of sentencing.

dissertation suggests at the very least that the 100-to-1 quantity ratio is a significant source of rule-based disharmony.

Yet, despite general calls for reform and numerous proposals to amend the 100-to-1 quantity ratio by independent scholars and analysts, the United States Sentencing Commission, and even the Clinton Administration (Blumstein 2003; Tison 2003; USSC 1995, 1997, 2002), Congress has failed to act on and even expressly rejected these proposals. Recognizing there are legitimate differences over exactly how and to what extent the 100-to-1 quantity ratio should be amended, Congress' inaction in the decade since the initial *1995 Cocaine Report* cannot be supported by the findings of this dissertation.

5.2.4 Mandatory minimums: excessive uniformity, the safety valve, and prosecutorial discretion

This dissertation's analysis of "misplaced equality" in mandatory minimum sentencing offers three, interrelated assessments. First, the drug mandatory minimums, by relying exclusively on drug type and quantity to determine the applicable penalty, result in excessively uniform sentences for substantively different offenders and, thereby, undermine the goal of proportional punishment. Second, the safety valve is working as intended to limit the applicability of mandatory minimums to certain low-level defendants, yet, at the same time, appears to be so narrowly construed that it fails to address misplaced equality among the majority of drug defendants. Third, the impact of prosecutorial discretion on sentencing outcomes—and the potential for this hidden and unreviewable form of sentence manipulation to reintroduce unwarranted sentencing

disparity—is quite substantial under the mandatory minimum sentencing regime. Each of these issues is discussed in turn.

Unlike the sentencing guidelines, mandatory minimums offer no flexibility to federal judges to take into account legitimate offender differences; drug type and quantity are the only factors that matter for determining the applicable sentence. Uniformity is achieved only insofar as the type and amount of drugs is the same. As Oliss (1995: 1864) points out, "offenders whose only shared circumstance is that the amount of drugs involved in their offenses falls into the same broad penalty category may have extremely different levels of culpability, criminal histories, violent proclivities, and mitigating circumstances." Thus, while principled uniformity is an important goal of federal sentencing, the mandatory minimums reach too far in this regard and essentially "*mandate inequality* by requiring that different cases be treated alike (Schulhofer 1992a: 854, emphasis in original). Consistent with these statements and prior research (USSC 1991b), this study's analyses showed that actual mandatory minimum sentencing outcomes were invariant with respect to gradations in offender culpability and dangerousness. Specifically, differences in functional roles in the offense, aggravating or mitigating role status, and firearms activity had no bearing on whether a defendant was sentenced to the applicable quantity-based mandatory minimum sentence. Put simply, the mandatory minimums' one-size-fits-all approach to sentencing fails to ensure proportionality and fairness in punishment.

Congress recognized as much—at least for certain low-level defendants—when it passed Section 80001 of the Violent Crime Control and Law Enforcement Act of 1994, otherwise known as the "safety valve" amendment. In the face of mounting criticisms of

the harsh effects of mandatory penalties, "Congress came to realize that it could strengthen the integrity and effectiveness of the drug-related mandatory minimums by creating a limited 'safety valve' to release the least culpable, low-level drug offenders from the application of these penalties" (Froyd 2000: 1496). In particular, the provision created a specific mandatory minimum exemption for nonviolent, low-level offenders who have minimal criminal histories and cooperate with the prosecution in a timely manner. Some commentators contend this definition is too narrow to have a substantial impact on overall sentencing equity (Froyd 2000; Oliss 1995; Vincent and Hofer 1994). For example, Oliss (1995: 1890) argued that

> An offender who does not meet the safety valve's criteria, but whose crime nevertheless involved mitigating circumstances that substantially differentiate him or her from other offenders dealing in the same quantity of drugs, will still have no recourse from the mandatory sentence. Thus, the safety valve will not significantly abate the excessive uniformity that arises from misplaced equality.

The results of the present study support this contention. On the one hand, offenders who met the safety valve criteria had 74% lesser of odds of being sentenced to the applicable mandatory minimum. This finding is consistent with prior empirical research on the safety valve (Albonetti 2002a, 2002b), and observers of federal drug sentencing will certainly commend the safety valve's success in limiting some of the most intolerable consequences of mandatory sentencing. On the other hand, the results show that, barring alleviation under the safety valve, offenders of vastly differing culpability and dangerousness continue to be treated uniformly under the mandatory minimum sentencing laws. Whether an offender plays an aggravating or mitigating role in the

offense, uses a weapon, or has an extensive criminal history has no bearing on the application of a harsh mandatory minimum drug sentence.

This contrasts sharply with the operation of the sentencing guidelines. As this dissertation's sentence length analysis demonstrated, guideline-based adjustments work as intended, albeit imperfectly, to calibrate an offender's sentence up or down based on the presence of specific aggravating or mitigating offense characteristics. Unfortunately, these adjustments are not allowed to operate under the mandatory minimums, because whenever the two systems conflict the mandatory minimums always trump the guidelines in controlling the final sentence. One has to doubt the sustainability of such a bifurcated sentencing system, especially when the sentencing guidelines can achieve the goals of just punishment, crime control, and disparity reduction equally or better than the mandatory minimums (USSC 1991b).

A further criticism of mandatory minimums is that they shift discretion in tailoring sentences from judges to prosecutors (Bowman 2005; Lutjen 1996; Vincent and Hofer 1994; Weinstein 2003). This study's result confirmed the strong role prosecutors play in determining the applicability of mandatory minimum sentences. In particular, defendants who pled guilty or bargained with the prosecutor had half the odds being sentenced to the otherwise applicable mandatory minimum. Based on these results, it is not inaccurate to say that prosecutors have through their plea and charge bargaining powers become the primary decisionmakers regarding who ultimately gets a mandatory minimum sentence. Proponents of mandatory minimums point to their ability to induce cooperation, which aids in the prosecution of other defendants, and pleas, which reduce burdens on attorneys and trial courts (Mueller 1992; USSC 1991b). The problem is that

prosecutorial discretion is largely hidden and unreviewable. Moreover, individual prosecutors set the incentives for cooperating and pleading guilty, and these can vary. As Oliss (1995: 1873) explains, "where prosecutors apply mandatory minimum statutes against some defendants, but not against others guilty of the exact same conduct, the mandatory minimum statutes' objective of removing disparity from the sentencing system is undermined."

While it is not possible to determine reasons and facts behind the systematic mandatory minimum circumvention uncovered here, it is noteworthy that prior research on plea and charge bargaining practices in mandatory minimum cases has found that much of this evasion takes place in order to avoid applying mandatory minimums to sympathetic or low-level defendants (Nagel and Schulhofer 1992; Schulhofer and Nagel 1989; USSC 1991b). Such evasion may represent a shared view among court actors that the mandatory minimum is inappropriate, but it is a haphazard and covert way of achieving justice (Vincent and Hofer 1994). As Nagel and Schulhofer (1992: 561) emphasize, "so long as mandatory minimum sentences, and the guidelines anchored by mandatory minimums, are tied to charges for which the defendant is convicted and prosecutors exercise unfettered discretion in charging decisions, the goals of certainty, uniformity, and the reduction of unwarranted disparity are at risk."

In short, from the perspective of treating offenders fairly and equitably, one can only conclude that quantity-driven mandatory minimums represent bad policy. They are divorced from any sort of proportionality review and result in unwarranted sentencing uniformity for dissimilar offenders. Congress' enactment of the safety valve in 1994 was an explicit acknowledgement that mandatory sentencing was not working—at least for

certain low-level defendants. One could also argue that this action represents implicit disapproval of the very backbone of mandatory minimum sentencing: rigid, quantity-driven penalties. Congress' attempt to salvage the mandatory minimum system with a politically viable fix—whether it works as intended or not—does not lend itself to coherent sentencing policy. It is a like patching the roof when the foundation is crumbling. When coupled with hidden and unsystematic prosecutorial discretion that heavily influences whether the indicated mandatory minimum is ultimately applied, it is easy to see how mandatory sentencing thwarts the goals of certainty, uniformity, and proportionality in sentencing. In the final analysis, mandatory minimum sentencing in theory and in effect is grossly at odds with the tenets of principled uniformity and proportionality.

5.2.5 Should we be more concerned with "plateaus" than "cliffs" in federal drug sentencing?

The sentencing literature is replete with hypothetical examples describing how small, incremental differences in drug quantity around the mandatory minimum threshold can result in sharp sentencing cliffs between otherwise similarly situated defendants (Froyd 2000; Oliss 1995; Schulhofer 1993; Steer 2000; USSC 1991b; Vincent and Hofer 1994; Wilkins, Newton, and Steer 1993). For instance, Schulhofer (1993) describes an example in which a five gram difference (i.e., 495 vs. 500 grams) in the amount of powder cocaine sold by two identical offenders results in a 2.5 year sentencing differential, because the larger amount is subject to the rigid five-year mandatory penalty whereas the smaller amount is controlled by the more flexible guidelines (see also Froyd 2000 and Oliss

1995). Another example is provided by Steer (2000), USSC (1991b), and Wilkins, Newton, and Steer (1993), describing a situation in which a conviction for simple possession of 5.01 grams of crack cocaine carries a mandatory minimum five-year sentence, but a simple possession conviction for 5.0 grams of crack carries a *maximum* of one year in prison. Mueller (1992) refers to this last example as "illusory," because a defendant who possessed 5.0 grams of crack cocaine would likely be charged not with simple possession but with trafficking, which has a corresponding guideline sentencing range of 63-78 months. Notably, despite these various claims, there has not been a single empirical analysis of the cliff effect in the sentencing literature.

This dissertation, which represents the first empirical investigation of the cliff effect, found little evidence to indicate that the cliff effect is a widespread source of disparity in federal drug sentencing. In particular, the study found that the net sentencing differentials across the mandatory minimum thresholds were generally not large or significant and, where they were (for powder cocaine and marijuana around the five-year mandatory penalty, in particular), the differences were well accommodated within the proportional structure of the guidelines. Moreover, it found that few drug offenders actually possessed or were held responsible for drug quantities within the small ranges around the mandatory minimum threshold typically cited in examples of the cliff effect. For instance, just 1.6% of powder-only offenders (N = 312) possessed quantities in the 490-500 gram range, and 4.3% of crack-only offenders (N = 547) possessed quantities in the 4.8-5.0 gram range.[52] Overall, less than 10% of offenders for the five major drugs

[52] Recall that the cliff effect analyses were restricted to offenders convicted for one substance. The respective estimates for all powder and crack cocaine offenders, regardless of their polydrug status, are 2.7% (N = 508) and 4.7% (N = 589). It is also

combined possessed quantities within a 5% span on either side of the five- and ten-year mandatory minimum thresholds (e.g., extending from 4.5 to 5.5 grams for crack cocaine and 450 to 550 grams for powder cocaine around the five-year mandatory minimum). In short, based on the results of this analysis, the potential for disparity arising from quantity-based cliff effects in federal drug sentencing remains more hypothetical than real.

This study finds that a more likely source of disparity in the interaction between the sentencing guidelines and mandatory minimums is the flattening (and inversion) of sentences that occurs around the mandatory minimum thresholds—which might referred to as a "plateau effect." Steer (2000: 350) describes how the interaction between the sentencing guidelines and mandatory minimums can produce such an anomalous outcome:

> In cases in which the guideline sentence is higher than the mandatory minimum, any applicable mitigating factors recognized by the guidelines (i.e., acceptance of responsibility, reduced role in the offense) will operate to provide a proportionally lower sentence than would apply to a similarly situated offender who lacked these mitigating characteristics. Ironically, however, for the very offenders who arguably most warrant proportionally lower sentences—offenders who by guideline definitions are the least culpable—mandatory minimums generally operate to block the sentence reflecting mitigating factors. This means that the least culpable offenders may receive the same sentence as their relatively more culpable counterparts.

This probably represents as accurate portrayal as any of the "plateau effect" uncovered by this study's analyses. The process is probably more complex, however, involving

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noteworthy that the drug amounts for approximately nine of ten powder and crack cocaine offenders fell exactly on the respective mandatory minimums of 500 and 5 grams, which suggests a different problem with sentence manipulation and entrapment (Fisher 1996; Johnson 1996).

interaction between the guidelines and mandatory minimums, substantial assistance departures, the safety valve, and prosecutorial charge and plea-bargaining practices. While investigating these nuances would require additional analyses and different data, the results suggest considerable discretion is being exercised to depart from the guidelines within the constraints established by the mandatory minimums.

5.2.6 Evasion of firearm sentence enhancements: Bargaining-chip justice and equity concerns

Consistent with prior research (Hofer 2000; Hofer, Blackwell, Burchfield, Gabriel, and Stevens-Panzer 2000; Nagel and Schulhofer 1992; Schulhofer and Nagel 1989, 1997; USSC 1991b), this dissertation found that firearm sentence enhancements were not applied in a large number of cases where they appeared legally warranted. Of those drug offenders who reported using or possessing a firearm during their offense, 28% did not ultimately receive a firearm sentence enhancement. This circumvention conferred an approximate five-year sentencing advantage upon FSE evaders compared to similarly situated defendants who did not escape receiving an FSE.

The broader question this study sought to answer concerned why this evasion takes place. Prior research identifies three general reasons why firearm sentence enhancements have not be applied when they are otherwise warranted by the facts of the case (see e.g., Hofer 2000; Schulhofer and Nagel 1997). First, weak evidence and concerns with meeting the high standards of proof necessary for a statutory conviction may make prosecutors reluctant to seek a 924(c) conviction. This is less of a concern regarding the Guideline FSE, which operates under the less strict preponderance of the

evidence standard, but still may play a role. Second, in responding to case pressures, prosecutors may offer plea and charge bargains as inducement to settle cases outside of trial. Third, equity concerns with overly harsh quantity-based penalties may cause prosecutors and judges to seek ways to avoid the additional sentencing exposure that a firearm sentence enhancement brings. The Inmate Survey data are not amenable to addressing the first circumvention issue related to weak evidence. Nevertheless, it was possible to investigate the other two. Specifically, the analyses revealed that guilty pleas and drug quantity were significantly associated with the uneven application of the Guideline FSE and 924(c) counts, respectively. Charge bargaining and mandatory minimum exposure were not related to FSE outcomes.

The finding that drug offenders are significantly less likely to receive a 924(c) conviction as drug quantity increases is particularly salient to this research. The implication is that equity concerns with overly harsh quantity-based sanctions leads to systematic circumvention of the statutory five-year 924(c) penalty. This is consistent with Schulhofer and Nagel's (1997: 1310) impressions of plea and charge bargain practices in federal sentencing:

> Several factors appear to contribute to our finding that evasion seemed to be prompted, to an unexpectedly significant extent, by local perceptions of excessive severity. First, because of policy changes and Congressional enthusiasm for mandatory minimums, many cases—especially drug cases, 924(c) cases, and those with career offender charges—now involve extremely high stakes. In addition, the gap between past practice and new norms can be substantial. Under these circumstances, the inevitable resistance to change is greatly intensified and pressures for evasion become powerful.

Interestingly, the same pressure for evasion of overly harsh penalties did not reveal itself in the association between drug quantity and the Guideline FSE. Two important

distinctions between the Guideline FSE and 924(c) conviction may explain this difference. First, the Guideline FSE is generally less harsh than the 924(c) in its effect and, therefore, the corresponding pressure for evasion is less. Second, judges and not prosecutors control imposition of the Guideline FSE and, to the extent that factual stipulations about weapon use are made in the presentencing report, judges must either impose the Guideline FSE or state their reasons for departing on record, which is obviously not a popular choice when dealing with firearm-involved offenders. Naturally, judges would rather leave this type of evasion to the back room. Lastly, mandatory minimum exposure was invariant with respect to both of the firearm sentence enhancements.[53] It is possible, however, that a more accurate measure of actual mandatory minimum charging and conviction outcomes would achieve a different result.

Aside from the significant association between pleading guilty and the Guideline FSE, the generally insignificant role of plea and charge bargaining in predicting FSE

[53] While this study failed to find such an association, Hofer (2000) found higher rates of Guideline FSE application for quantities falling below versus above the five-year mandatory minimum threshold. My initial thoughts were that this discrepancy could be explained by differences in methodology (i.e., bivariate vs. multivariate), especially since this dissertation's drug quantity measures were used to derive the mandatory minimum variables and, therefore, the former would account for much of the variance of the latter in a multivariate analysis. However, a supplementary bivariate analysis presented in the following table still revealed results contradictory to Hofer's (2000). Just how contradictory is not clear, however, since Hofer reported his results descriptively without the support of hard data.

	Firearm Sentence Enhancement Status		
Mandatory Minimum	None	Guideline FSE	924(c) Conviction
No Mandatory	29.9%	27.0%	40.2%
Five-Year Mandatory	28.2%	23.8%	33.2%
Ten-Year Mandatory	41.8%	49.2%	26.6%
Total N	44,669	5,783	5,029

outcomes was surprising given this study's earlier analyses, which found these factors to have a strong, across-the-board influence on sentence length and mandatory minimum outcomes. Schulhofer and Nagel's (1989, 1997; Nagel and Schulhofer 1992) qualitative research provides a rich and varied set of findings that may shed light on these apparent discrepancies. First, Schulhofer and Nagel (1997: 1293) concluded that "by far, the most important vehicle for Guidelines evasion…is charge bargaining which leads to the dismissal of readily provable counts." This general conclusion is consistent with this dissertation's sentence length and mandatory minimum analyses. Speaking directly to the issue of 924(c) circumvention, however, Nagel and Schulhofer (1992: 549, 551) noted that

> since *Mistretta*, [the 1989 Supreme Court decision affirming constitutionality of the guidelines,] U.S. Attorneys, AUSAs [Assistant U.S. Attorneys], and supervisors have been particularly sensitive to the dismissal of 924(c) counts. Defense counsel reported that whereas before the issuance of the Thornburgh memorandum 924(c) counts were routinely dismissed, after the issuance of the memorandum AUSAs were reluctant to dismiss such counts.
>
> …the data suggest that AUSAs simply drop 924(c) counts to avoid the mandatory minimum consecutive five-year sentence in cases in which they think dismissal will prompt a guilty plea, or when they consider the additional mandatory consecutive sentence too harsh.

While these two statements seem contradictory, they appear to capture the essence of this study's results. For example, as already noted, the finding that equity concerns lead prosecutors to "simply drop" 924(c) counts is consistent with the results showing that 924(c) application decreases as drug quantity increases. At the same time, their finding that supervisors exercise tight control over 924(c) charging and bargaining practices is

also supported by the dissertation's results showing that plea and charge bargaining had no independent effect on 924(c) outcomes.

Finally, there is the question of why plea-bargaining significantly predicts uneven application of the Guideline FSE. Schulhofer and Nagel (1989) uncovered several plea bargain practices in their research, one of which they refer to as 'guideline-factor bargaining.' Under this type of prosecutorial manipulation, aggravating or mitigating factors that bear on sentencing are changed or misrepresented to the court in exchange for the defendant's guilty plea. Schulhofer and Nagel (1989: 273) provide an example of such guideline-factor bargaining:

> Defendant, arrested at the scene of a large cocaine buy, admitted to possessing a gun found in the back seat of his car. The defendant argued, however, that the presence of the gun at the time of the offense was fortuitous…but the defendant's story, even if plausible, was not necessarily decisive under section 2D1.1(b)(1) [i.e., the Guideline FSE], which requires a two-level enhancement for any firearm "possessed during the commission of the offense." Nonetheless, in order to get the plea, the AUSA agreed to both drop the section 924(c) count, and not to assert the Guideline enhancement for the weapon.

Based on the FSE analyses reported here, it appears the practice of guideline-factor bargaining around the Guideline FSE is a common and influential means of inducing drug defendants to plead guilty to the underlying drug charge.

In summary, this study finds that circumvention of firearm sentence enhancements in federal drug cases is specific to the type of FSE, arising from two apparently different concerns. First, evasion of 924(c) counts appears to be highly driven by equity concerns over already severe drug sentences. Otherwise, the practice of 924(c) dismissals appears tightly controlled and restricted. Second, evasion of the Guideline FSE appears to be driven by case pressures to avoid trial by inducing plea agreements. In

particular, this is accomplished through guideline-factor bargaining in which the prosecutor agrees to downplay or hide the defendant's weapon involvement from the court in exchange for the plea bargain.

These conclusions should not overshadow the fact that offenders who use firearms in the course of their drug trafficking activity are *highly* likely to receive a firearm sentence enhancement in the federal system. Nonetheless, the findings indicate the presence of systematic FSE circumvention in federal drug sentencing cases. While these evasions are often carried out in the name of individual justice, they are just as likely to promote system-wide disparities. Once again, as Nagel and Schulhofer (1992: 535-536) put it,

> The problem with such equity judgments is that they are made by individual prosecutors without regard to the nationally set sentencing rules, thereby introducing sentencing disparity and compromising the uniformity and certainty goals of the guidelines. Further, such individually made equity judgments open the door to race, gender, and social-class bias, notwithstanding the good intentions of individual AUSAs hoping to 'save' sympathetic defendants.

In short, the ad hoc basis on which much of this discretion is exercised undermines the goals of uniformity and proportionality in sentencing and introduces unwarranted disparities the guidelines were meant to reduce, no matter how much justice is done in each individual case. Of course, this highlights a tension between the front-line individuals who operate within local norms of justice and national policymakers who seek system-wide consistency. So long as front-line actors consider the underlying drug sentences excessively severe, these types of evasions will continue.

5.3 IMPLICATIONS OF THE STUDY

5.3.1 Theoretical implications

Does the *harm-based modified just deserts* perspective presented in Section 2.2.1 provide an accurate description of the rationale behind the federal drug sentencing guidelines? In many ways, the results of this research provide strong support for this framework. For example, drug type and quantity—as measures of harm—were the main drivers of sentence length. This is consistent with the harm-based focus of the theory. The other two elements of offense seriousness—culpability and dangerousness—played secondary but important roles in tailoring sentences. This is consistent with the just deserts perspective. Indeed, after drug quantity, the specific sentencing factors related to culpability and dangerousness accounted for much of the variance in drug sentencing outcomes and influenced sentence length in ways commensurate with the principle of proportionality. Finally, criminal history, as the crime-control modifier of base offense seriousness levels, played a significant role in increasing sentences for repeat offenders. In short, the harm-based modified just deserts perspective provides a fairly accurate theoretical description of the operation of the federal drug sentencing guidelines.

This assessment is both complicated and weakened, however, by the influence of the drug mandatory minimums, which tend to operate under a competing set of rationales (e.g., deterrence, certainty, severity) and produce widely divergent sentencing outcomes. For example, this research showed that the drug mandatory minimums, though clearly harm-based, hardly comport with a just deserts or incapacitation rationale. Indeed, both criminal history and the culpability- and dangerousness-based offense factors had no discernable impact on mandatory minimum sentencing outcomes. A similar assessment

prevails for the 924(c) mandatory minimum firearm sentence enhancement. The study also revealed the strong influence of plea and charge bargaining practices on sentencing outcomes—influences that both distort proportionate punishment and compete with legitimate offense factors for control over the final sentence.

That the sentencing guidelines and mandatory minimums coexist does not mean they do so without conflict. The rationales for the two systems and the sentencing outcomes they produce stand in stark contrast. Indeed, as this research has suggested, as long as the drug guidelines remain both wedded and subservient to the mandatory minimums, the intent of the drug sentencing guidelines will tend to be frustrated. This research also suggests that even were the mandatory minimums abolished, the harm-based rationale of the drug guideline itself would tend to distort sentencing proportionality so long as culpability and dangerousness remained the secondary factors they are now. Alschuler (1991: 909) comments that "a system grounded on 'just deserts' need not—indeed should not—focus primarily on harm." If it does, at some point, it stops being just deserts. Whether this point has been reached is difficult to judge because of the distorting influence of the mandatory minimums, but the implication stands as a challenge to the descriptive accuracy of the 'harm-based modified just deserts' perspective as it applies to the federal drug sentencing guidelines.

5.3.2 The current sentencing regime: *Booker* and beyond

The landscape of federal sentencing has undergone momentous change in recent years due to a series of Supreme Court decisions addressing the Sixth Amendment right to trial by jury. These decisions culminated in *United States v. Booker*, decided on January 15,

2005, which effectively made the guidelines advisory rather than presumptive. The essential elements of *Booker* are recounted in USSC (2006):

> According to the Court, the basic precepts of the right to trial by jury and the right to have the crime proved beyond a reasonable doubt were undercut by a new trend in the legislative regulation of sentencing, which placed an increasing emphasis on facts that enhanced sentencing ranges. It thus became the judge, and not the jury, who determined the upper limits of sentencing based upon facts not required to be raised before trial or proved beyond a reasonable doubt. The legislative trend thereby operated to increase the judge's power and decrease that of the jury. (p.14)
>
> After considering the legislative intent underlying the SRA, the Court concluded that the Sixth Amendment requirement that a jury find certain sentencing facts was incompatible with components of the SRA. The Court concluded that the severability question must be answered by excising from the SRA those provisions that made the sentencing guidelines mandatory. (p.15)
>
> By severing these provisions, the Court rendered the sentencing guidelines effectively advisory, thereby permitting the courts to continue factfinding under a preponderance of the evidence standard. (p.16)

The *Booker* decision did not apply to mandatory minimums, but it effectively expanded judicial discretion in guideline sentencing. Legal analysts and commentators have advised restraint and caution in the rush to a "fix" in order to let the repercussions of *Booker* work their way through the system (Bowman 2005). Initial assessments suggest that the majority of federal sentences continue to be within the now advisory guideline ranges (USSC 2006). However, many courts have begun using their expanded judicial discretion to challenge the 100-to-1 crack-powder cocaine quantity ratio (King and Mauer 2006; USSC 2006). Although most crack cocaine cases that have been sentenced under a reduced ratio (e.g., 20-to-1) have not stood up on appeal, King and Mauer (2006: 20) conclude that

> The post-*Booker* world has changed the mechanics of crack cocaine sentencing and opened up opportunities for judges to contemplate a host of relevant factors to determine a sentence that is appropriate for the defendant, while still maintaining principles of fairness, equity, and the opportunity for rehabilitation.

These types of pressures will continue to test the limits of *Booker*, especially in drug cases where the punishment is perceived as too harsh. At some point, Congress is sure to act on these developments, so the implications of this dissertation are especially pertinent in this post-*Booker* policy window.

5.3.3 Policy implications

The clearest and most far-reaching implication of this research is that the central, organizing role of drug quantity in federal drug sentencing needs to be rethought. Under the sentencing guidelines, drug quantity can effectuate a sentence anywhere from probation to life in prison. No other single factor has near as much influence. This study has demonstrated that drug quantity is simply too blunt an instrument to meet the demands of principled sentencing. Drug quantity cannot adequately differentiate between offenders of varying culpability, dangerousness, and circumstance. The overemphasis on drug quantity devalues other, legitimate sentencing factors and, as this study has shown, this leads to widespread disparities and anomalies in sentencing. Correcting this imbalance stands as the core recommendation of this research.

Currently, the amount of drugs an offender is held responsible for can result in a guideline offense level anywhere from 6 to 38, which, for a first offender, ranges from no prison time to almost 25 years in prison. Under the drug trafficking guideline, the use or possession of a weapon in connection with a drug crime can increase the guideline range

by just 2 offense levels. Likewise, whether an offender is the organizer of an extensive drug trafficking organization or a minimal participant with little responsibility can result in a difference of at most 8 offense levels under the sentencing guidelines. This study has demonstrated the inequities that can result from such an imbalanced sentencing regime. Bringing balance to the federal drug sentencing guidelines would require deemphasizing drug quantity while placing greater weight on other factors in the sentencing equation.

A first step in this direction might entail decoupling considerations of culpability and dangerousness from current harm-based penalty levels. The severe penalties tied to large drug quantities have often been justified on grounds that the sanctions will target the major and serious drug traffickers of greatest concern or the most dangerous gun-toting dealers. By separating out these "uncharacteristic harms," overall quantity-based sanction levels can be reduced in a principled manner while, at the same time, enhancements targeting the most noxious and high-level offenders can be adjusted to carry greater weight. This consideration applies to all drugs, but is perhaps most cogent with respect to crack cocaine. Both prior research and the results of this study have shown that the 100-to-1 quantity ratio between crack and powder cocaine overstates the culpability and dangerousness of most crack offenders. This rule-based disparity should be corrected, which could easily be accomplished, and justified, within a balanced sentencing system.

Such a shift from a harm-based to a balanced approach would necessitate greater differentiation among the culpability- and dangerousness-based offense factors than now exists. The current two-level increase in the base offense level for possessing a gun is not sufficient within a balanced system. Thus, the weapon-related enhancement could be

written to account for different levels of gun involvement, including, for example, constructive possession, active possession, carrying, brandishing, and firing. Scaled enhancements for other dangerous behaviors could be constructed in similar fashion. Indeed, the guidelines already do this to an extent for additional harms to the environment and minors caused by the illicit manufacture of methamphetamine and similar drugs.

Existing adjustments for culpability are also inadequate. The guidelines' role in the offense adjustment that applies to drug traffickers is a generic guideline that also applies to other federal offenses. Moreover, it applies only to charged conspiratorial activity in which there are multiple participants. That is, assessment of culpability for mitigating or aggravating purposes is done only against one's accomplices. Offenders acting alone or charged separately are generally unable to receive culpability-based sentencing adjustments. This research suggests that such adjustments need to be expanded to include an offender's functional role in the offense that is independent of his or her involvement in group conduct.

Nowhere is the imbalance of the current drug sentencing regime more apparent than with drug and firearm mandatory minimums. These should be repealed. Indeed, they *must* be repealed for a balanced sentencing regime to function. This research demonstrates that mandatory minimums do not forward the goals of uniformity and proportionality. Instead, they create unwarranted disparities by driving severity levels uniformly higher for all offenders, not just the worst ones. It is not enough to create "safety valves" and similar exceptions to these existing structures. Not only do such fixes

fail to address the larger problem of excessive uniformity, but they also add to the complexities and distortions of the current bifurcated system.

Mandatory minimums also create pressures for evasion and circumvention, and this can have a "spill-over" effect into the guidelines. In many ways, this research found that mandatory minimums create a "prosecutor's paradise" in which prosecutors have become the primary decisionmakers regarding who gets a mandatory minimum. While much of this discretion is exercised out of equity concerns with severe quantity-based punishments, this type of ad hoc justice compromises consistency and rationality in sentencing despite the individual justice done in any individual case. Prosecutorial plea and bargaining practices would probably not be so problematic were quantity-based sanctions not so great. Nevertheless, the great extent to which government charging and bargaining decisions impacted sentencing outcomes in this research implies that additional oversight may be necessary.

5.4 SUGGESTIONS FOR FUTURE RESEARCH

There are many opportunities for future research. The Inmate Surveys are conducted every 5-6 years, and the most recent version of the survey is set to be released in late 2006 or early 2007. This dataset will afford additional opportunities for replication and original research. This study proves the Inmate Surveys can be a valuable source of data for sentencing research. Indeed, the Inmate Surveys can be used to triangulate the findings of more commonly analyzed datasets, such as the Sentencing Commission's Monitoring Files, or to provide state-level estimates where no similar data exists.

More substantively, disparity research should focus on the structure and impact of alternate sentencing regimes. This could be accomplished through simulation studies or evaluation in jurisdictions that have recently changed their drug sentencing laws (e.g., Michigan's repeal of the 650-lifer). Research on identifying taxonomies of harm, culpability, and dangerousness associated with drug distribution would advance discussion of sentencing rationales and contribute to theory-based assessments of punishment policy.

More research is warranted on pre-indictment and presentencing processes in gun and drug cases that occur "under the radar screen" yet significantly affect sentencing outcomes. This would probably require a multiyear qualitative research project in similar vein to Schulhofer and Nagel's (1989, 1997; Nagel and Schulhofer 1992) work, but it stands to increase our understanding of these vital, but hidden, processes.

This study's results indicated that the "cliff effect" is probably not a great source of disparity in federal drug sentencing, and identified a "plateau effect" that could be a greater problem. Future research on the interaction between guidelines and mandatory sentencing would be worthwhile to confirm or disconfirm these findings.

Finally, this study highlighted several advanced methods for dealing with missing data, limited response distributions, and complex survey data. This study shows that these types of methods can be employed by nonstatisticians in policy research. To the extent possible, researchers should employ such methods in order to improve their statistical models and the validity of their results.

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APPENDIX A

SENSITIVITY ANALYSES

Table A1: Sensitivity Analyses for Alternate Modeling Assumptions♦

Independent Variables (0-1 Dummies Unless Noted)	(1) ML Svy Imp	(2) ML Nonsvy Imp	(3) ML Svy CC	(4) ML Nonsvy CC	(5) OLS Svy Imp	(6) OLS Nonsvy Imp	(7) OLS Svy CC	(8) OLS Nonsvy CC
ln(Marijuana Equivalent Grams)	**0.07*** **	**0.06*** **	**0.07*** **	**0.07*** **	**0.06*** **	**0.06*** **	**0.07*** **	**0.07*** **
Five-Year Mandatory Minimum Exp.	0.05	0.06	0.03	0.03	0.06	0.07	0.03	0.04
Ten-Year Mandatory Minimum Exp.	0.12	**0.14***	0.08	0.12	0.12	**0.14***	0.09	0.13
Primary Drug Type (Reference: Powder Cocaine)								
Heroin	-0.05	-0.05	-0.03	-0.05	-0.05	-0.05	-0.03	-0.05
Methamphetamine	-0.10	**-0.15***	-0.10	**-0.18***	-0.10	**-0.15***	-0.10	**-0.18***
Crack Cocaine	-0.09	**-0.10***	-0.05	-0.05	-0.10	**-0.10***	-0.06	-0.05
Marijuana	**-0.32***	**-0.33*** **	**-0.34*** **	**-0.34*** **	**-0.32*** **	**-0.33*** **	**-0.34*** **	**-0.34*** **
Other Drugs	-0.10	-0.06	-0.14	-0.16	-0.10	-0.06	-0.13	-0.16

Table A1 (continued)

Highest Role in the Offense (Reference: Retailing)								
Money Laundering	-0.19	-0.13	**-0.17***	-0.11	**-0.18***	-0.13	-0.16	-0.11
Importing	-0.10	-0.13	-0.09	-0.12	-0.09	-0.12	-0.08	-0.11
Producing	0.15	0.11	0.18	0.16	0.15	0.11	0.17	0.15
Wholesaling	-0.02	-0.01	0.03	0.05	-0.02	-0.01	0.03	0.05
Distributing NOS	0.00	0.00	-0.01	-0.01	0.00	0.00	0.00	-0.01
Possessing	-0.23	-0.15	-0.18	-0.09	-0.22	-0.14	-0.18	-0.08
Aggravating Role (0-3)	**0.13****	**0.13*****	0.09	0.07	**0.12*****	**0.12*****	0.08	0.06
Mitigating Role (0-3)	0.06	**0.07***	0.03	0.06	0.06	**0.07***	0.03	0.06
Safety Valve Eligibility	**-0.29*****	**-0.29*****	**-0.31*****	**-0.30*****	**-0.30*****	**-0.30*****	**-0.32*****	**-0.31*****
USC 924(c) Conviction	**0.32*****	**0.29*****	**0.37*****	**0.35*****	**0.31*****	**0.28*****	**0.34*****	**0.33*****
Guideline FSE	**0.22*****	**0.25*****	**0.22****	**0.24*****	**0.20*****	**0.23*****	**0.20****	**0.22*****
Used Firearm During Offense	-0.01	-0.08	-0.04	-0.09	-0.01	-0.07	-0.03	-0.08
Criminal History Category (1-6)	**0.04***	**0.04****	**0.04***	**0.04****	**0.04***	**0.04****	**0.04***	**0.04***
Guilty Plea	**-0.43*****	**-0.45*****	**-0.43*****	**-0.46*****	**-0.41*****	**-0.43*****	**-0.41*****	**-0.44*****
Plea Agreement	**-0.18*****	**-0.18*****	**-0.19*****	**-0.18*****	**-0.18*****	**-0.17*****	**-0.19*****	**-0.17*****
Pretrial Release	**-0.34*****	**-0.35*****	**-0.31*****	**-0.31*****	**-0.34*****	**-0.34*****	**-0.31*****	**-0.31*****
Race (Black Reference)								
White	**-0.18****	**-0.14***	-0.13	-0.08	**-0.17****	**-0.13***	-0.12	-0.08
Hispanic	**-0.15***	**-0.15****	-0.13	**-0.12***	**-0.15***	**-0.15****	**-0.14***	**-0.12***

Table A1 (continued)

Other Race	-0.10	0.02	**-0.26***	-0.08	-0.11	0.00	**-0.26***	-0.08
Male	**0.27*** **	**0.28*** **	**0.32*** **	**0.30*** **	**0.27*** **	**0.27*** **	**0.31*** **	**0.30*** **
ln(Age at Offense)	**0.20*** **	**0.18** **	**0.26** **	**0.27*** **	**0.20*** **	**0.18** **	**0.26** **	**0.27*** **
Years Education Completed (0-18)	**-0.02***	**-0.02*** **	**-0.02** **	**-0.02*** **	**-0.02** **	**-0.02*** **	**-0.02** **	**-0.02*** **
Non U.S. Citizen	**-0.10***	-0.10	-0.08	-0.09	**-0.10***	-0.09	-0.08	-0.09
Constant	**3.59*** **	**3.71*** **	**3.38*** **	**3.37*** **	**3.61*** **	**3.72*** **	**3.40*** **	**3.38*** **
Sigma	**-0.41*** **	**-0.39*** **	**-0.44*** **	**-0.43*** **	--	--	--	--

*p<0.05, **p<0.01, ***p<0.001

♦ML = Maximum Likelihood Estimation (Truncated-Censored Regression); OLS = Ordinary Least Squares; Svy = Design-based; Nonsvy = Model-based; Imp = Imputed; CC = Complete Case

Model (1): F(31, 3) = 77.23, p < 0.01; M & Z's pseudo-R^2 = .46; Subpopulation: n = 1,671 (53 right-censored); N = 55,480
Model (2): LR $\chi^2(31)$ = 948.27, p < 0.001; M & Z's pseudo-R^2 = .46; Subpopulation: n = 1,671 (53 right-censored)
Model (3): F(31, 3) = 71.95, p < 0.01; M & Z's pseudo-R^2 = .48; Subpopulation: n = 1,238 (34 right-censored); N = 42,813
Model (4): LR $\chi^2(31)$ = 760.34, p < 0.001; M & Z's pseudo-R^2 = .47; Subpopulation: n = 1,238 (34 right-censored)
Model (5): F(31, 3) = 62.77, p < 0.01; R^2 = .42; Subpopulation: n = 1,671; N = 55,480
Model (6): F(31, 1639) = 41.20, p < 0.001; R^2 = .44; Subpopulation: n = 1,671
Model (7): F(31, 3) = 134.36, p < 0.001; R^2 = .45; Subpopulation: n = 1,238; N = 42,813
Model (6): F(31, 1206) = 33.74, p < 0.001; R^2 = .46; Subpopulation: n = 1,238

APPENDIX B

SUPPLEMENTARY TABLES AND FIGURES

Table B1: Role in the Offense by Mandatory Minimum Frequency Distribution

	Mandatory Minimum Exposure							
	None		Five-Year		Ten-Year		Row Total	
Role in the Offense	*n*	*N*	*n*	*N*	*n*	*N*	*n*	*N*
Laundering	22	458	45	1,019	71	1,955	138	3,432
Importing	65	1,992	77	2,167	94	2,905	236	7,063
Producing	32	1,168	22	811	45	1,579	99	3,559
Wholesaling	69	2,286	97	3,076	136	4,882	302	10,244
Distributing NOS	115	3,873	149	5,028	220	7,449	484	16,350
Retailing	112	4,026	84	2,917	108	4,099	304	11,041
Possessing	91	3,156	17	635	0	0	108	3,792
Column Total	506	16,959	491	15,652	674	22,869	1,671	55,481

Note: *n* represents raw sample observations and *N* represents weighted estimates.

Table B2: Organizational Role by Mandatory Minimum Frequency Distribution

	Mandatory Minimum Exposure							
	None		Five-Year		Ten-Year		Row Total	
Organizational Role	*n*	*N*	*n*	*N*	*n*	*N*	*n*	*N*
Leader	15	530	18	572	38	1,055	71	2,157
Middle Man	10	328	20	647	25	832	55	1,806
Underling	16	529	30	737	30	994	76	2,260
Seller	14	501	18	559	24	782	56	1,842
Peripheral	1	15	3	76	9	204	13	295
None	450	15,057	402	13,063	548	19,002	1,400	47,122
Column Total	506	16,959	491	15,652	674	22,869	1,671	55,481

Table B3: Firearm-Related Factors by Primary Drug Type Frequency Distributions

Firearm-Related Factors	Primary Drug Type									
	Heroin		Methamphetamine		Crack Cocaine		Powder Cocaine		Marijuana	
	n	*N*	*n*	*N*	*n*	*N*	*n*	*N*	*n*	*N*
Used/Possessed Firearm										
No	142	4,035	156	5,080	404	13,781	561	19,317	236	7,831
Yes	8	235	27	780	40	1,351	40	1,254	12	301
Firearm Sentence Enhancement										
None	128	3,599	127	4,020	338	11,633	486	16,835	220	7,291
Guideline FSE	16	464	29	1,028	57	1,954	49	1,952	8	325
924(c) Conviction	6	207	27	813	49	1,545	66	1,784	20	517
Total	150	4,270	183	5,860	444	15,132	601	20,571	248	8,132

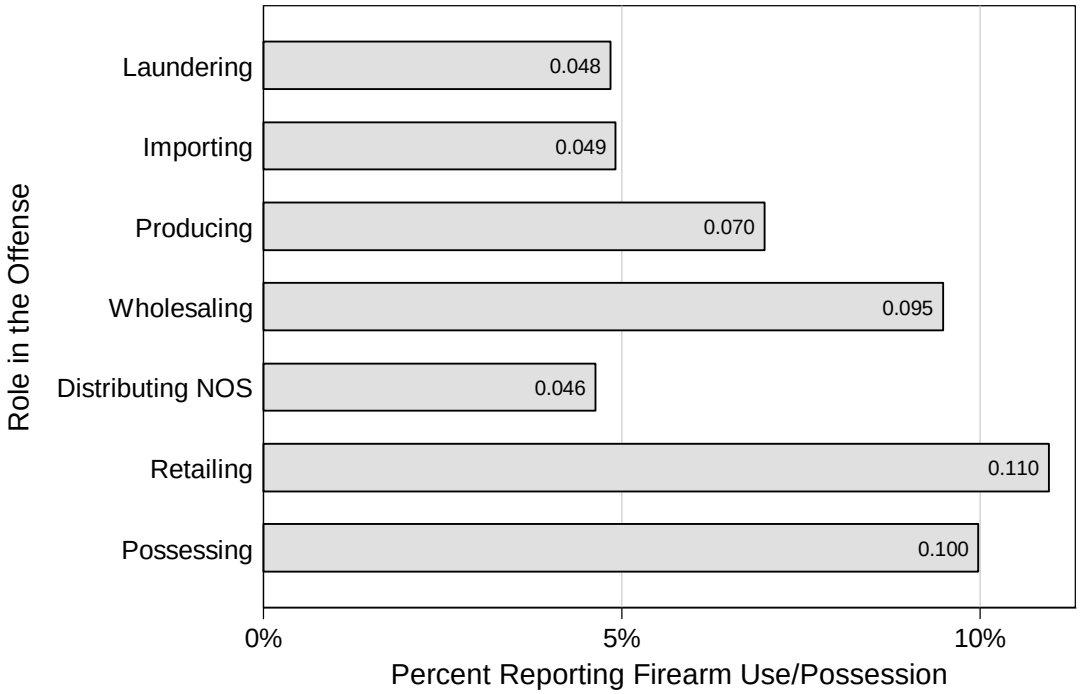

Figure B1: Firearm Use/Possession by Role in the Offense

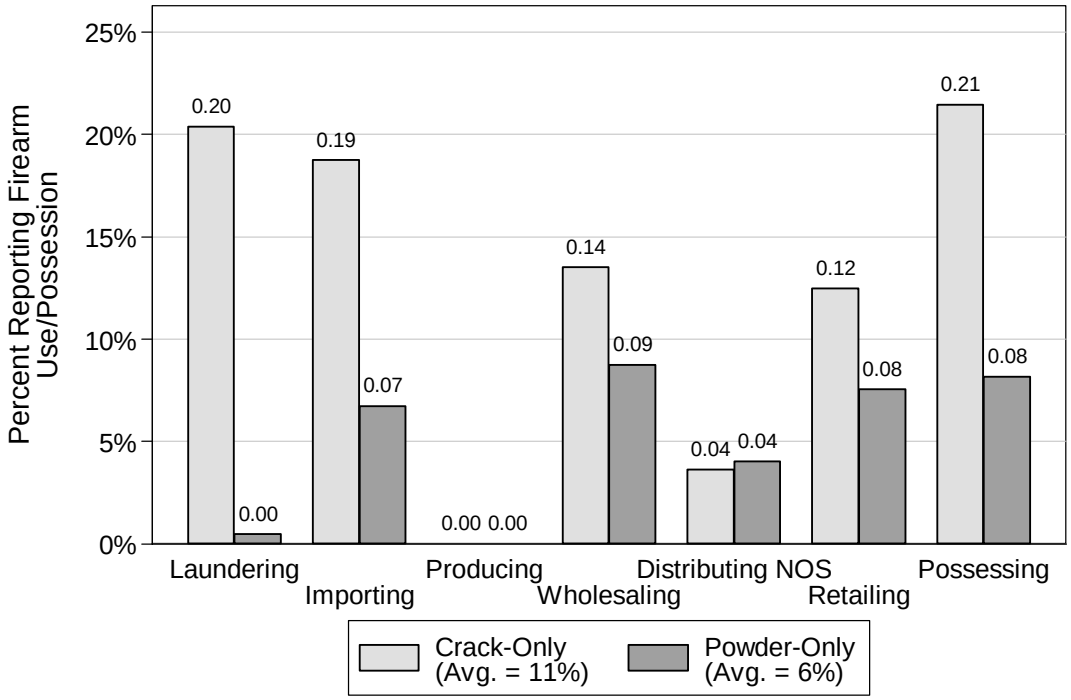

Figure B2: Firearm Use/Possession by Role in the Offense for Cocaine Offenders

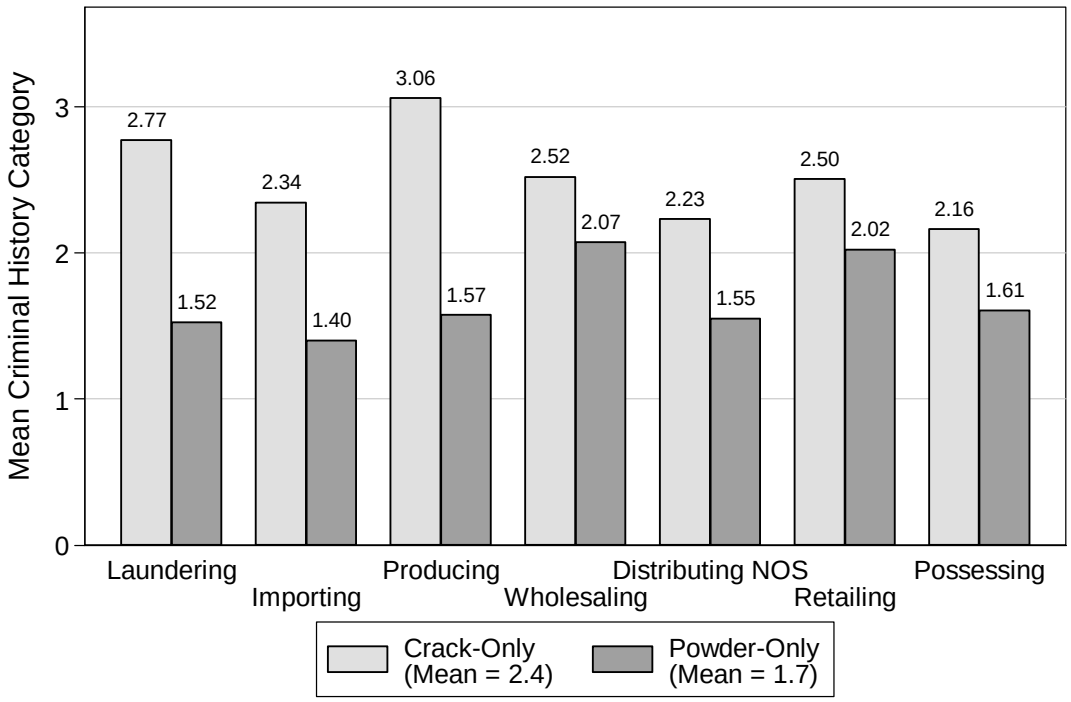

Figure B3: Criminal History by Role in the Offense for Cocaine Offenders

Table B4: Design-Based Truncated-Censored Regression Models Testing for the Cliff Effect

Independent Variables (0-1 Dummies Unless Noted)	[1] Heroin		[2] Meth.		[3] Crack Cocaine		[4] Powder Cocaine		[5] Marijuana	
	β	S.E.	β	S.E.	β	S.E.	β	S.E.	β	S.E.
Cliff Effect Dummies (Backward Adjacent Difference Coded; Reference: Non MM)										
Pre Five-Year	0.10	0.18	0.13	0.28	0.07	0.16	-0.13	0.16	0.12	0.22
Post Five-Year	**-0.52***	0.23	0.49	0.27	-0.07	0.16	**0.38****	0.14	**0.42***	0.17
Mid Range MM	**0.62****	0.22	-0.27	0.16	0.30	0.15	-0.09	0.14	-0.10	0.10
Pre Ten-Year MM	0.14	0.15	0.32	0.26	0.19	0.14	0.23	0.13	0.21	0.15
Post Ten-Year MM	0.20	0.17	-0.22	0.22	0.00	0.12	-0.04	0.10	-0.08	0.24
Max Range MM	0.18	0.15	0.30	0.15	**0.24***	0.10	0.14	0.11	0.28	0.24
Highest Role in the Offense (Reference: Retailing)										
Laundering	-0.33	0.26	-0.32	0.17	-0.14	0.22	-0.04	0.10	0.02	0.21
Importing (and Producing)	0.02	0.20	**-0.61****	0.19	-0.02	0.18	0.03	0.11	0.03	0.10
Producing	--	--	-0.13	0.13	-0.03	0.21	--	--	**0.32***	0.15
Wholesaling	**0.45***	0.2	**-0.46***	0.18	0.08	0.09	0.10	0.12	0.14	0.18
Distributing NOS	0.14	0.20	-0.21	0.17	-0.09	0.12	0.15	0.09	0.04	0.09
Possessing	0.29	0.31	**-0.67***	0.31	**-0.66****	0.21	0.11	0.12	-0.07	0.29
Aggravating Role (0-3)	0.03	0.10	0.03	0.09	0.10	0.09	0.15	0.08	0.09	0.07
Mitigating Role (0-3)	0.14	0.10	0.00	0.09	-0.04	0.10	0.09	0.08	0.11	0.09
Safety Valve Eligibility	**-0.50****	0.22	-0.41	0.21	**-0.26***	0.10	**-0.33*****	0.08	-0.24	0.15
USC 924(c) Conviction	**-0.91***	0.39	0.39	0.22	0.20	0.13	0.27	0.13	0.53	0.35

Table B4 (continued)

Guideline FSE	-0.10	0.17	0.29	0.16	0.28	0.16	0.16	0.11	-0.16	0.23
Used Firearm During Offense	**1.12***	0.45	-0.12	0.16	0.01	0.15	-0.12	0.12	0.15	0.20
Criminal History Category (1-6)	0.10	0.05	0.05	0.05	**0.07***	0.03	0.01	0.03	-0.02	0.05
Guilty Plea	**-0.66****	0.19	**-0.36****	0.11	**-0.38*****	0.07	**-0.40*****	0.08	**-0.59*****	0.14
Charge Bargain	0.04	0.08	-0.22	0.12	**-0.27*****	0.06	-0.13	0.07	-0.10	0.08
Pretrial Release	**-0.45****	0.16	-0.23	0.12	**-0.28*****	0.07	**-0.26****	0.08	**-0.50*****	0.06
Race (Reference: White)										
Black	-0.02	0.23	--	--	0.31	0.19	0.20	0.11	0.22	0.15
Hispanic	-0.20	0.20	0.12	0.22	0.34	0.24	0.02	0.11	0.11	0.12
Other Race	**-0.51***	0.21	0.33	0.25	0.88	0.60	-0.08	0.23	0.49	0.46
Male	0.15	0.15	**0.30****	0.09	**0.24****	0.07	**0.29***	0.13	0.19	0.15
ln(Age at Offense)	-0.46	0.25	-0.05	0.27	0.18	0.10	0.21	0.12	**0.30***	0.13
Years Education Completed (0-18)	-0.03	0.02	-0.01	0.03	0.00	0.01	-0.02	0.01	-0.02	0.01
Non U.S. Citizen	-0.09	0.10	-0.27	0.27	**0.22***	0.09	**-0.15***	0.07	-0.25	0.16
Constant	**6.80*****	0.86	**5.19*****	1.16	**3.83*****	0.42	**4.18*****	0.51	**3.82*****	0.54
Sigma	**-0.71*****	0.10	**-0.57*****	0.08	**-0.54*****	0.04	**-0.45*****	0.06	**-0.42*****	0.08

*p<0.05, **p<0.01, ***p<0.001

All Models: Number of strata = 7; Number of PSUs = 40; Population: n = 4,025; N = 88,807
Model [1]: F(28, 6) = 42.94, p < 0.001; M & Z's pseudo-R^2 = .70; Subpopulation: n = 127 (1 right-censored); N = 3,580
Model [2]: F(28, 6) = 150.4, p < 0.001; M & Z's pseudo-R^2 = .59; Subpopulation: n = 160 (2 right-censored); N = 5,192
Model [3]: F(29, 5) = 7.98, p = 0.015; M & Z's pseudo-R^2 = .49; Subpopulation: n = 370 (10 right-censored); N = 12,653
Model [4]: F(28, 6) = 6.62, p = 0.013; M & Z's pseudo-R^2 = .40; Subpopulation: n = 549 (19 right-censored); N = 18,965
Model [5]: F(29, 5) = 33.10, p < 0.001; M & Z's pseudo-R^2 = .44; Subpopulation: n = 245 (2 right-censored); N = 8,009

Table B5: Design-Based Multinomial Logistic Regression Predicting Receipt of [1] a USC 924(c) Conviction or [2] the Guideline FSE

Independent Variables (0-1 Dummies Unless Noted)	[1] USC 924(c) Conviction versus No FSE	[2] Guideline FSE versus No FSE
	Relative Risk Ratio	Relative Risk Ratio
ln(Marijuana Equivalent Grams)	0.87**	1.01
Five-Year Mandatory Minimum	1.07	0.87
Ten-Year Mandatory Minimum	0.89	1.14
Primary Drug Type (Reference: Powder Cocaine)		
Heroin	0.56	1.30
Methamphetamine	1.65	1.59
Crack Cocaine	1.03	0.89
Marijuana	0.85	0.52
Other Drugs	0.60	0.26
Highest Role in the Offense (Effect Coded)		
Laundering	1.42	0.53
Importing	0.26**	0.46*
Producing	1.66	1.71
Wholesaling	0.86	1.30
Distributing NOS	1.19	0.80
Retailing	0.93	1.31
Possessing	1.69	1.77
Aggravating Role Adjustment (0-3)	1.21	1.25
Mitigating Role Adjustment (0-3)	0.90	1.21
Used Firearm During Offense	22.88***	8.57***
Criminal History Category (1-6)	1.05	0.89
Guilty Plea	0.76	0.62*
Charge Bargain	1.09	1.12
Pretrial Release	0.52*	0.59*

Table B5 (continued)

Race (Reference: Black)		
White	0.57	0.90
Hispanic	0.38*	0.59
Other Race	0.54	0.43
Male	1.55*	1.05
ln(Age at Offense)	0.88	0.62
Years Education Completed (0-18)	0.99	0.97
Non-U.S. Citizen	0.31***	0.94

*p<0.05, **p<0.01, ***p<0.001

Population: n = 4,025; N = 88,807
Subpopulation: n = 1,671; N = 55,481

Note: No overall model test is provided due to violation of degrees of freedom limitation.

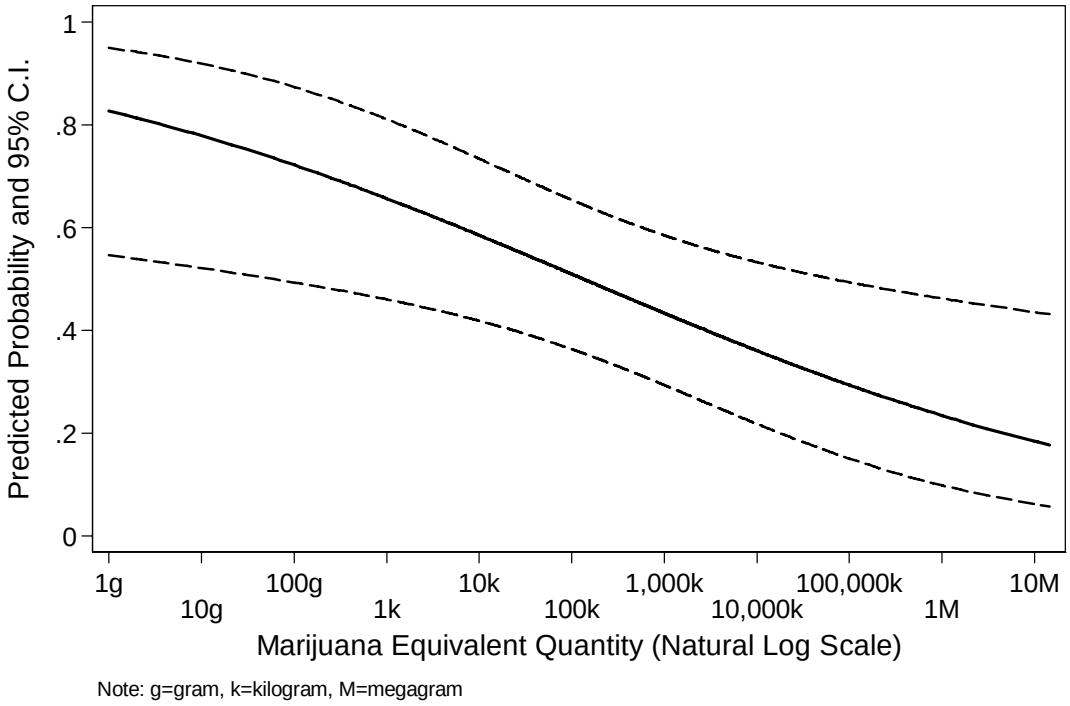

Figure B4: Predicted Probability of Gun-Involved Offenders Receiving a 924(c) Conviction as a Function of Drug Quantity

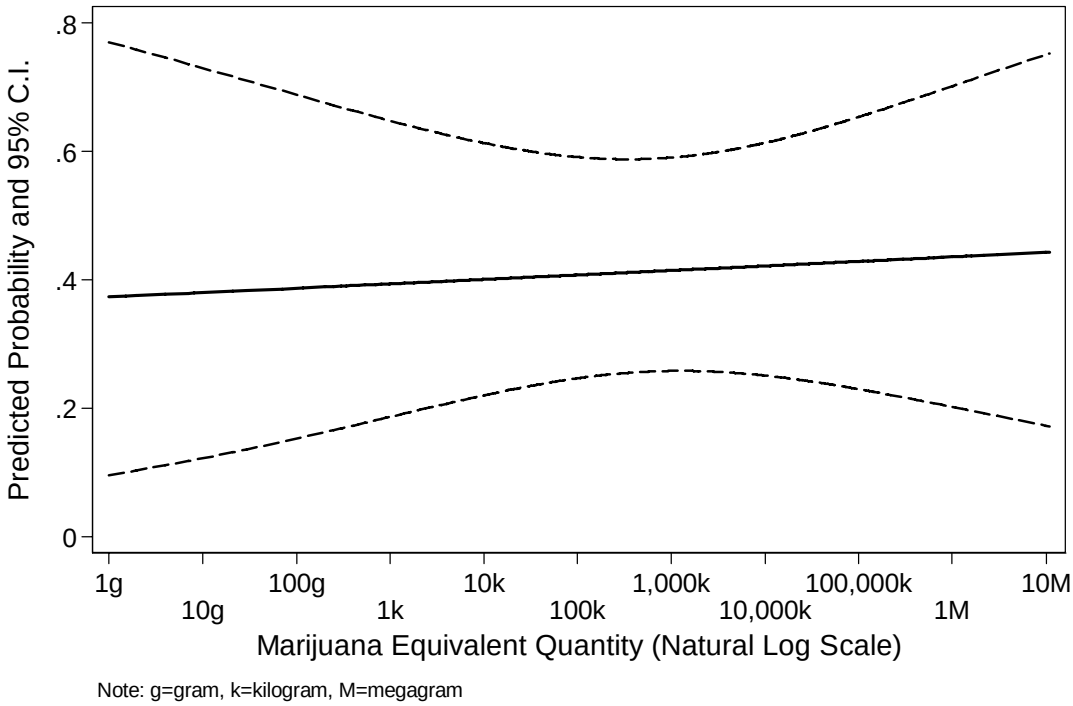

Figure B5: Predicted Probability of Gun-Involved Offenders Receiving the Guideline FSE as a Function of Drug Quantity