

## ABSTRACT

Title of Dissertation: SENTENCING IN A NEW ERA: EXAMINING THE  
IMPACT OF JUDICIAL DISCRETION AND  
CONTEXTUAL DYNAMICS IN FEDERAL  
CRIMINAL SENTENCING AFTER  
*BOOKER/FANFAN*

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For nearly twenty years, federal judges were required to strictly adhere to the federal sentencing guidelines (“the Guidelines”), which calculated sentences based on the offender’s current offense level and his or her prior offense history, and which tightly controlled which aggravating and mitigating factors could be considered. However, the U.S. Supreme Court effectively altered the course of federal sentencing with its decision in *Booker/Fanfan* in 2005, which made the Guidelines advisory; although judges are still required to consider the Guidelines and to provide reasons for departing from their recommendations, *Booker* has minimized the concern for appellate interference and, as such, has presumably opened the door for increased judicial discretion. The current dissertation examines the impact of the Supreme Court’s decision and extends prior work by paying particular attention to its effect on

drug and immigration offenses and by incorporating unique dynamic measures to examine contextual changes over time.

Because *Booker* provides a natural experiment, a quasi-experimental pre-test post-test design is employed to examine the decision's impact, with separate models estimated for the full study period (2000-2008), the pre- and post-*Booker* periods, and each quarter in the study period. In addition, the impact of the decision is examined both through individual-level models, which focus on the role of offense and offender characteristics, and multilevel models, which more closely investigate how *Booker*'s impact may be contextualized by district-level factors.

The results provide very little evidence of a "*Booker* effect" during the years examined; instead, they largely suggest that judges continued to sentence according to pre-*Booker* sentencing patterns. When noticeable differences were found pre- to post-*Booker*, closer examination of quarterly trends often revealed that shifts occurred prior to *Booker*, during the PROTECT Act period. However, because there was initially a large amount of federal-level confusion regarding the interpretation of *Booker*'s application – subsequent clarification was provided by the Supreme Court in *Kimbrough* and *Gall* – it is possible that the true effects of *Booker* became evident well after the study period ended. Future research should expand the types of contextual variables included, incorporate qualitative data, and more precisely estimate *Booker*'s causal influence on sentencing outcomes.

SENTENCING IN A NEW ERA: EXAMINING THE IMPACT OF JUDICIAL  
DISCRETION AND CONTEXTUAL DYNAMICS IN FEDERAL CRIMINAL  
SENTENCING AFTER *BOOKER/FANFAN*

by

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## DEDICATION

This work is dedicated to my mother, Mary Jo Betsinger. I wish she could be here to see me finally finish. I will always miss you, Mom.

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I would like to express my most sincere gratitude to each of the individuals who have agreed to serve on my committee. Dr. Brian Johnson, in particular, has greatly contributed to this work being improved through his willingness to endure countless revisions; somehow he has never wavered in his encouragement, and he has taught me so much.

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## CHAPTER I: INTRODUCTION

For nearly twenty years, the federal sentencing guidelines (hereafter, “the Guidelines”) required that judges strictly adhere to sentences calculated based on an offender’s current offense level and his or her prior offense history, and the aggravating and mitigating factors that could be considered in order to arrive at a sentence were tightly restricted (Mustard, 2001; Tonry, 1996; USSC, 2006).<sup>1</sup> This was deliberate, as the intended aims of the Guidelines were to reduce judicial discretion and, at the same time, to reduce sentencing disparities and lenient sentences. Academics and judges alike criticized the Guidelines for their complexity, for their severity, for their limited flexibility and inadequate range of sentencing options, and for the possibility that prosecutors would experience increased discretion and accordingly be able to manipulate Guidelines sentences (Albonetti, 1997; Anderson, Kling, and Stith, 1999; Burns, Elden, and Blanchard, 1997; Frase, 2000; Kempf-Leonard and Sample, 2001; Nagel and Schulhofer, 1992; Ruback, 1998; Spohn, 2002; Stith and Cabranes, 1998; Tonry, 1996; Weisburg, 2007).

Still, the Guidelines withstood this criticism as well as numerous challenges concerning their Constitutionality, and they became synonymous with federal sentencing. However, the course of federal sentencing was effectively altered in 2005, when the U.S. Supreme Court decided the *Booker/Fanfan* cases (hereafter “*Booker*”). Put simply, *Booker* changed the process by which sentences could be increased beyond the Guidelines range. Prior to the decision, judges were able to use

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<sup>1</sup> As noted by Tonry and Hatlestad (1997: 7-8), “the guidelines are presumptive in the sense that they set sentencing standards for individual cases that were presumed to be appropriate and that judges were expected to follow.” The terms “presumptive” and “mandatory” are used interchangeably throughout this document.

a preponderance of evidence standard to find facts that would increase offenders' sentences (Seghetti and Smith, 2007; USSC, 2006; Yeh and Doyle, 2009). *Booker* demanded that such facts either be provided by the defendant's own admission or be subjected to a jury using a beyond a reasonable doubt standard. Not wishing to expand the limited role of the jury in the federal system, the Supreme Court instead opted to make the Guidelines advisory so that judges could continue to drive federal decision-making.<sup>2</sup> Although judges are still required to consider the Guidelines and to provide reasons for departing from their recommendations, *Booker* has minimized the concern for appellate interference and, as such, has presumably opened the door for increased judicial discretion.

The current dissertation addresses the following research questions:

1. *How have sentencing patterns changed since the Supreme Court's decision?*
2. *Have disparities in sentencing outcomes (in general, and specifically for drug and immigration offenses) increased now that judges are afforded greater discretion?*
3. *Are crack and powder cocaine cases sentenced more similarly in the post-Booker period?*
4. *How has sentencing in immigration cases changed since Booker?*
5. *What role do district-level factors play in sentencing outcomes?*

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<sup>2</sup> The terms "voluntary" and "advisory" are used somewhat similarly throughout this document. However, "advisory" is preferred when discussing the post-*Booker* federal sentencing guidelines, as judges are still required to consult the recommendations of the Guidelines, while "voluntary" guidelines, as in some state systems, imply a looser level of judicial adherence.

The obvious questions facing researchers and criminal justice personnel in the wake of *Booker* are what effect it has had on federal sentencing outcomes and whether unwarranted disparities have increased now that judges' decisions are less constrained. *Booker* provides a unique type of natural experiment in which the impact of legitimized judicial discretion can be gauged. Although a growing body of empirical research has investigated the impact of this monumental shift in sentencing policy (see Hofer, 2007; Ulmer and Light, 2010; Ulmer, Light, and Kramer, 2011a, 2011b; USSC, 2006, 2010a, 2012), the current dissertation substantially contributes to and extends these studies in a number of key ways. First, it provides an in-depth and longitudinal examination of sentencing trends for drug offenses, with particular attention paid to the way crack and powder cocaine cases have been handled over time. Second, because immigration cases represent the fastest growing offense category in federal criminal caseloads, the present work examines these cases separately and investigates the extent to which inter-district variation in the sentencing of these offenses exists and has changed over time. Third, this dissertation reaches beyond typically examined contextual factors to incorporate unique dynamic measures that capture changes in district-level demographic factors over time. Finally, in addition to making the unique contributions outlined here, the current dissertation represents an important effort to independently replicate prior research efforts, and the importance of replication should not be underestimated in social research (see King, 1995).

Given that drug cases make up a substantial portion of the federal caseload, in addition to historical disparities associated with the sentencing of different types of

drug offenses, there are ample reasons to devote attention to sentencing outcomes for federal drug cases. The current work examines the influence of changing demographic factors and other unique contextual measures of interest, such as relative district-level drug caseload, on sentencing outcomes for drug offenders over time. In addition, because *Booker* (and the Supreme Court's later clarification in *Kimbrough*) left judges freer to depart from the Guidelines in cases specifically involving crack cocaine - which were, until very recently, subject to particularly severe and widely criticized punishments - the present work also investigates the extent to which the sentencing patterns for crack and powder cocaine cases have been altered in the post-*Booker* era.

Due to their expanding share of the federal caseload, immigration offenses also warrant a separate examination. Immigration cases are comprised of a demographically different group of offenders than federal sentencing cases more generally, and these cases are also likely to benefit from sentencing discounts in the form of fast-track departures.<sup>3</sup> While a growing body of research (e.g., Hartley and Armendariz, 2011; Wolfe, Pyrooz, and Spohn, 2011) focuses on the role of citizenship status in federal sentencing outcomes, few studies are devoted to investigating outcomes for those individuals specifically sentenced for immigration offenses. The Commission's (2006; 2010a) analyses conceal the unique nature of these offenses by examining them in models with all other federal criminal offense types, while other work (Ulmer and Light, 2010; Ulmer, Light, and Kramer, 2011a, 2011b) acknowledges their distinctive characteristics but drops them from analysis.

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<sup>3</sup> The Early Disposition Program seeks to expedite, or fast track, case processing and allows courts to approve downward departures of up to four levels.

Hartley and Tillyer (2012) devote their attention to immigration offenders, but their analyses are limited to a single year of federal sentencing data and concentrate solely on sentence length outcomes. This dissertation extends this body of research by examining immigration cases separately and by investigating how contextual factors influence different sentencing outcomes for these unique offenses.

Prior research on the influence of *Booker* is also quite limited in terms of the contextual factors that it has examined. Analyses completed by the Commission (2006; 2010a) and by Hofer (2007) excluded districts as controls in their analyses. More recent research by Ulmer and Light (2010) explores whether inter-district variation increased after the Supreme Court's decision, but it uses unconditional multilevel models to complete this investigation. Ulmer, Light, and Kramer (2011a) expand this line of inquiry to include several contextual variables (i.e., districts' departure rates, caseload per judge, and districts' mean offense levels) and find that inter-district variation did not increase substantially after *Booker*. Still, the role played by other contextual elements remains unexplored. The current dissertation extends beyond prior research by controlling for additional district characteristics and by incorporating the effects of both static and dynamic measures of racial composition and caseload pressure in federal punishment.

The dissertation proceeds as follows. Chapter II is devoted to explaining the *Booker* decision and placing it within the historical context of federal sentencing reform. Chapter III provides a review of relevant federal level sentencing studies, including those specific to drug and immigration offenses. Chapter IV outlines the specific hypotheses to be explored, and Chapter V details the data and methods to be

utilized. The overall models are presented in Chapter VI; the findings for drug-specific models are detailed in Chapter VII; and immigration-specific results are detailed in Chapter VIII. Chapter IX provides a summary of the results, and Chapter X discusses policy implications of the present research, and suggests directions for future research efforts.

## CHAPTER II: THE *BOOKER* DECISION IN CONTEXT

In order to provide a fuller understanding of the impact of the *Booker* decision on federal sentencing practices, it is first necessary to place the decision within its proper historical context. This chapter provides an overview of the changes precipitating the development of sentencing guidelines in general and at the federal level in particular. It then examines the legal challenges leading up to *Booker*, and it ends by detailing the particulars of the U.S. Supreme Court's ruling in *Booker* and by discussing the uncertain sentencing environment left in its wake.

### **Sentencing Reform and the Guidelines Ideal**

Prior to the 1970s, indeterminate sentencing allowed judges broad discretion to customize appropriate sentences for individual offenders based upon a wide range of aggravating and mitigating factors (Reitz, 1998; Spohn, 2002; Stith and Cabranes, 1998). The underlying philosophy of punishment was rehabilitation, with judges determining the minimum and maximum terms but prison and parole authorities determining the actual release date.<sup>4</sup>

Ultimately, support for indeterminate sentencing and the rehabilitative ideal were undermined by the confluence of several factors, including an increase in crime during the 1960s, a rising suspicion that rehabilitation programs did not work (e.g., Martinson, 1974), and a growing discontent with the criminal justice system that transcended political party lines (Anderson, Kling, and Stith, 1999; Frase, 2000; Reitz, 1998; Stith and Cabranes, 1998). In a scathing critique published in 1972, a federal judge, Marvin Frankel, noted that indeterminate sentencing was characterized

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<sup>4</sup> At the federal level, this meant that release dates were set by the U.S. Parole Board, usually after one-third of the sentence had been served (Seghetti and Smith, 2007).

by unjustifiably divergent sentences handed down to different offenders charged with the same offenses (a cause for liberal concern), and he discussed the wide discretion the system afforded to judges (a concern for conservatives). Not only did Frankel question the skills of both judges and prison and parole officials, but he questioned the validity of rehabilitation: “Until or unless we have some reasonable hope of effective treatment, it is a cruel fraud to have parole boards solemnly order men back to their cages because cures that do not exist are found not to have been achieved” (Frankel, 1972: 34). Importantly, Frankel concluded his essay by advocating the use of sentencing commissions to study sentencing practices and to make sentencing recommendations.

With the need to address sentencing disparities and judicial discretion clearly identified and the rehabilitative ideal in doubt, many jurisdictions began to experiment with alternative sentencing schemes.<sup>5</sup> In the late 1970s and early 1980s, a handful of individual states latched onto Frankel’s (1972) idea of sentencing commissions as a way to achieve determinate reform. Since that time, commissions have been adopted in eighteen states and the nation’s capital, in addition to the federal system (Engen, 2009; Frase, 2005). These commissions are tasked with studying sentencing trends, developing sentencing guidelines, monitoring the impact of the guidelines, and enacting amendments as necessary (Spohn, 2002). Guidelines vary

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<sup>5</sup> Five states adopted statutory determinate sentencing structures, in which the punishment options available to judges were constrained to specified sentencing ranges, and every state and the federal system created mandatory sentences for specific offenses (e.g., offenses involving violence, firearms, drugs, etc.) or for habitual offenders. Although broadly popular with the public, mandatory penalties have often been criticized for being excessively severe and have been plagued by problems including inflated discretion for prosecutors, wide circumvention, enlarged prison populations, and exacerbated sentencing disparities, among other things (Reitz, 1998; Spohn, 2002; Tonry, 1996; Zimring, Hawkins, and Kamin, 2001). Some studies indicate that “three strikes” laws, for instance, may even be associated with increases in homicide rates (Kovandzic, Sloan, and Vieraitis, 2002, 2004; Marvell and Moody, 2001).

significantly by jurisdiction (Frase, 2000, 2005; see also Kauder and Ostrom, 2008),<sup>6</sup> and although state sentencing commissions and their guidelines have generally been viewed as success stories, their level of success is similarly context-dependent (Tonry, 1996; for an opposing viewpoint, see Bushway and Piehl, 2001). Their success is partially attributable to their insulation from sudden political whims, as well as the fact that state sentencing commissions often seek input from both legal authorities and practitioners (Frase, 2000). Accordingly, almost all state “systems leave plenty of room for the consideration of unique offense and offender characteristics, crime-preventive as well as retributive sentencing purposes, local community values and resources, and emerging sentencing theories such as restorative and community justice” (Frase, 2000: 443).

### **The Federal Sentencing Commission and its Guidelines**

Congress shared with the states the desire to reduce judicial discretion and eradicate unwarranted disparities. However, the federal experience with sentencing reform is significantly differentiated from the state experience by the high level of politicization in the reform process and by the rigidity and severity ultimately embodied in the resulting guidelines. The federal sentencing commission was originally envisioned in legislation before Congress in 1974, just two years after Frankel’s famous treatise (Tonry, 1996). However, a decade passed before the United States Sentencing Commission (hereafter referred to as “the Commission”)

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<sup>6</sup> Guidelines may range from voluntary (i.e., judges are not required to follow them) to presumptive (i.e., judges are expected to follow them unless providing documented reasons for departure), and they may be either descriptive (i.e., modeled after prior sentencing patterns) or prescriptive (i.e., prescribing new sentencing patterns). Although guidelines in a handful of jurisdictions include misdemeanors, most guidelines cover only felony offenses (Frase 2000, 2005; Spohn, 2002; Tonry 1996). The decisions to retain parole, to integrate intermediate sanctions into guidelines schemes, and to tailor the guidelines to deal with prison crowding also vary by jurisdiction (Engen, Gainey, Crutchfield, and Weis, 2003; Frase, 2000, 2005; Marvell, 1995; Reitz, 1998; Tonry, 1996; Weisburg, 2007).

was created as part of the 1984 Sentencing Reform Act (SRA). By that time, there had been a seismic shift in the ideology surrounding criminal justice policy.

Although the legislation was cosponsored by Edward Kennedy and Strom Thurmond, prominent senators representing both ends of the political spectrum (Anderson, Kling, and Stith, 1999), the Reagan administration was already firmly established and its “get tough” approach was widely embraced.<sup>7</sup> Ultimately, the seven members appointed to the Commission were determined to prove their abidance to this new approach, and they sought to demonstrate their intolerance for lenient judges and their willingness to be tough on crime (Tonry, 1996).

The “get tough” approach is clearly evident in the SRA, which not only created the Commission but also abolished federal parole and capped sentence reductions for good behavior at 15%. The groundwork for the Guidelines was also spelled out in the SRA, which embraced uniformity, proportionality, and honesty as its goals.<sup>8</sup> The Guidelines were to be presumptive and to require lengthy terms of incarceration for specific offenses involving violence, drugs, and white-collar crimes, and for repeat or “career” offenders (Breyer, 1988; USSC, 2006). The SRA also delineated grounds for mitigating or aggravating sentences, and it established a range

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<sup>7</sup> Despite this broad support for “get tough” strategies in the 1980s, there was still no single underlying sentencing philosophy evident in the federal sentencing guidelines. This should come as no surprise, given that state level guidelines are similarly characterized. As underscored by Reitz (1998: 545), “the last twenty-five years can be characterized as a period in which no single policy viewpoint has stood squarely behind the operation of U.S. sentencing structures – unless it has been the view that rehabilitation was *not* the way to go” (emphasis in original). Breyer (1988) discusses the philosophical compromise embodied by the federal guidelines; he notes that the Commission, by creating “evolutionary” guidelines based on typical past practices, sought to balance just deserts and deterrence goals (see also USSC, 2006).

<sup>8</sup> Mustard (2001: 289) defines each of these terms: “An honest sentence avoids the confusion that occurs when judges impose an indeterminate sentence that is subsequently reduced by ‘good-time’ credits. Sentencing uniformity narrows the disparities in sentences imposed by different federal courts for similar criminal conduct by similar offenders. Proportionality imposes appropriately different sentences for criminal conduct of different degrees of severity.”

of no more than 25 percent between minimum and maximum sentences in the guideline range. Further, in designing the Guidelines, the Commission was prohibited from “considering the race, sex, national origin, creed, and socioeconomic status of offenders, and [the SRA] instructed that the sentencing guidelines should reflect the general inappropriateness of considering certain other factors that might serve as proxies for forbidden factors, such as current unemployment” (USSC, 2006: 4).

The resulting federal sentencing guidelines, which took effect on November 1, 1987, make use of a sentencing grid, with six “Criminal History” categories (i.e., prior convictions and time served) represented on the horizontal axis and 43 “Offense Level” categories (i.e., the severity of the current offense) represented on the vertical axis (see Appendix A). Unlike state guidelines systems, which have often been lauded for their fairness and flexibility, the federal guidelines have been criticized for being overly rigid, complex, and unnecessarily harsh (Frase, 2000; Spohn, 2002; Tonry, 1996). The Commission chose to depart from the proven approaches used by state commissions, arguing that “state guidelines systems which use relatively few, simple categories and narrow imprisonment ranges . . . are ill suited to the breadth and diversity of federal crimes” (USSC, 1987: 14; see also Breyer, 1988).<sup>9</sup>

The criticisms leveled against the federal guidelines are multifaceted and well-documented (c.f., Anderson, Kling, and Stith, 1999; Ruback, 1998; Spohn, 2002;

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<sup>9</sup> At the same time, it is telling that no state has chosen to base its guidelines system around the model provided by the federal guidelines. In fact, Tonry (1996) points out that several states specifically denounced the federal guidelines model as they were developing their own guidelines.

Stith and Cabranes, 1998; Tonry, 1996; Weisburg, 2007).<sup>10</sup> Still, although Tonry (1996: 72) calls them “the most controversial and disliked sentencing reform initiative in U.S. history,” a sizeable body of research suggests that they were effective in reducing unwarranted disparities (Anderson, Kling, and Stith, 1999; Hofer, Blackwell, and Ruback, 1999; McDonald and Carlson, 1993; USSC, 1991, 2004). Even Tonry (1996) admits that the Guidelines have been effective if they are judged on their intended goals of reducing the number of cases sentenced to probation and increasing the average federal prison sentence (see also USSC, 1991). Further, he states: “However misguided the U.S. Sentencing Commission’s policies, and however ineffective its efforts to elicit acceptance from practitioners, it has become a specialized agency of technical competence and has managed through its guidelines radically to alter sentencing practices in the federal courts” (Tonry, 1996: 26).

### **Legal Challenges Leading up to *Booker/Fanfan***

Criticisms and misgivings aside, the Guidelines remained in place largely as the Commission outlined them for close to twenty years. During that time, their basic legality has been repeatedly questioned and upheld by a series of Supreme Court cases, beginning with *Mistretta v. United States* in 1989, which upheld the Constitutionality of both the Guidelines and the Commission by ruling that the SRA did not violate the doctrine of separation of powers (USSC, 2006, 2008).

Over time, other legal challenges have followed. Most central are the challenges that have invoked the Sixth Amendment, which calls for speedy and

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<sup>10</sup> It should be noted that there is even criticism of all of the criticism that the Guidelines have received. Weisburg (2007) contends that academics have been too focused on condemning the federal guidelines instead of imagining a suitable alternative. He additionally argues that the obsession with all things federal has meant that the “less visible, harder to quantify conditions of state law get ignored” (Weisburg, 2007: 9).

public jury trials for criminal defendants by an impartial jury of their peers (see Table 1). The history of these challenges is fairly recent, starting with *Apprendi v. New Jersey* (2000), in which the U.S. Supreme Court ruled that, aside from a prior conviction, facts that lead to an aggravated sentence beyond a statutorily prescribed maximum must be decided by a jury using the “beyond a reasonable doubt” standard.<sup>11</sup> Two years later, the Court decided *Ring v. Arizona* (2002), which applied the *Apprendi* decision to the determination of facts related to an offender’s eligibility for a sentence of capital punishment (Frase, 2007).<sup>12</sup>

More relevant to the nature of guidelines sentencing, the Supreme Court decided *Blakely v. Washington* in 2004. The Court ruled that a judge’s application of an enhanced sentence under the Washington State guidelines violated the defendant’s Sixth Amendment right to a jury trial, as the defendant had not made admissions that supported the court’s finding of “deliberate cruelty” – a statutorily defined ground for departure – in his kidnapping case. Thus, the Court’s ruling redefined “statutory maximum” as “the maximum sentence that a judge may impose solely on the basis of

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<sup>11</sup> *Apprendi* was arrested in 1994 after shooting into the home of his black neighbors, whom he initially admitted to targeting based on their race. He pled guilty to a federal firearms violation carrying a prison term of 5 to 10 years, and the judge accepted his plea. However, the State of New Jersey later filed a motion to enhance *Apprendi*’s sentence under the state’s hate crime statute, and the judge found “by a preponderance of evidence” that the crime was racially motivated and thus imposed a 12-year sentence. The U.S. Supreme Court ultimately reversed this decision, ruling in 2000 that, aside from a prior conviction, facts that lead to an aggravated sentence beyond a statutorily prescribed maximum must be decided by a jury using the “beyond a reasonable doubt” standard. Frase (2007) provides an excellent description of the way *Apprendi* changed the standards of proof required in sentencing.

<sup>12</sup> Under the State of Arizona’ death penalty statute, after the jury decided the defendant was guilty of first-degree murder, the trial judge was allowed to determine whether the aggravating factors required by Arizona law to impose the death penalty were present. The Supreme Court ruled that the Arizona statute violated the Sixth Amendment since the aggravating factors acted as the “functional equivalent” of a greater offense, thereby necessitating that a jury should decide the aggravating factors. Although important, the impact of the *Ring* decision was limited “because many jurisdictions were already applying jury rights and the reasonable-doubt standard to death penalty eligibility facts” (Frase, 2007: 412).

the facts reflected in the jury verdict or admitted to by the defendant, not the maximum the judge may impose after making additional findings” (USSC, 2006: 12).

*Blakely* sent tremors of doubt through the sentencing community. In essence, the ruling meant that upward departures from the guidelines could only be imposed after the alleged facts warranting a longer sentence were decided by a jury using the beyond a reasonable doubt standard (Tonry, 2006). Although “upward departures are rare in every guidelines system” (Tonry, 2006: 3), the *Blakely* decision still meant that states had to come up with a workable solution for dealing with those rare instances in which such departures may arise. Frase (2007) suggests that most affected jurisdictions opted to comply with *Blakely* by providing the relevant jury rights, although some states attempted to get around the decision by broadening sentencing ranges (thereby allowing for longer sentences and making upward departures unnecessary) or by instituting “topless” guidelines (i.e., statutory maximums replace the high ends of the guidelines ranges). Still, because the impact of *Blakely* was limited to Washington State and a handful of other presumptive guideline states, Bushway and Piehl (2007) contended that the affected states set about “Blakelyizing” their sentencing systems, and they argue that apparently little changed in those states or at the state level in general.

At the same time, uncertainty also arose regarding how *Blakely* would affect the federal guidelines (Seghetti and Smith, 2007; USSC, 2006; Weisburg, 2007; Yeh and Doyle, 2009). Some argued that the Guidelines were exempt from *Blakely* (and *Apprendi*) because, unlike Washington State’s guidelines, which were statutorily created by the legislature, the federal guidelines were created by an independent

sentencing commission (Frase, 2007). Five circuits eventually ruled that *Blakely* did not affect the Constitutionality of the federal guidelines, but two others decided that the Guidelines were in violation of the Sixth Amendment (the Seventh Circuit decided *United States v. Booker* (2004), and the Ninth Circuit decided *United States v. Ameline* (2004)) (USSC, 2006). *Blakely* thus disrupted the attempt to uniformly apply the Guidelines across the country and ultimately led to the Supreme Court's 2005 ruling in *United States v. Booker* (USSC, 2006).

### ***Booker/Fanfan***

Questions regarding *Blakely*'s application to the federal guidelines were answered when the Supreme Court made its decision in *Booker/Fanfan* in January 2005.<sup>13</sup> Both the *Booker* and *Fanfan* cases, which were consolidated and heard together before the Court, involved sentencing for drug offenses. In both cases, appeals were made on the basis of whether a federal judge has the authority to decide facts that will enhance the offender's sentence beyond that which was authorized by the jury's verdict or by the offender's guilty plea. Essentially, the "merits" opinion in *Booker*, which was written by Justice Stevens, applied the Court's *Blakely* decision to the federal system. As in *Blakely*, the Court ruled that, after either a defendant's admission or a jury's verdict authorizes a certain range of punishment, a judge cannot

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<sup>13</sup> Originally, a jury convicted Booker on two counts of possession with intent to distribute at least 50 grams of cocaine base, thereby authorizing a sentence under the federal guidelines of 17 to 21 years in prison. However, at sentencing the judge found, by a preponderance of the evidence, that Booker had distributed a quantity of drugs above that which was found by the jury and that he had also obstructed justice. Under the Guidelines, these facts increased Booker's sentencing eligibility to a range of 30 years to life. Fanfan's case was similar: a jury convicted Fanfan of conspiracy to distribute more than 500 grams of cocaine hydrochloride, which, under the Guidelines, authorized a sentence of five to six years in prison. However, the prosecutor argued at sentencing that, although he was not formally charged, the defendant should be sentenced for possession and sale of crack cocaine. These facts would have increased Fanfan's sentence to a prison term of 15 to 16 years, but the judge sentenced Fanfan based solely on the crime for which he was found guilty (Yeh and Doyle, 2009).

issue a sentence beyond that range based on a finding of facts that he/she makes using a preponderance of the evidence standard (Seghetti and Smith, 2007; USSC, 2006; Yeh and Doyle, 2009). Instead, such facts (other than a prior conviction) must be either provided by the defendant's admission or proved beyond a reasonable doubt to a jury.

The quandary thus facing the Court was how best to "restore the jury's significance" (Seghetti and Smith, 2007: 4) while at the same time maintaining a workable federal sentencing process. Justice Breyer, writing the Court's "remedial" opinion, recognized that the day-to-day functioning of federal proceedings would be hindered by requiring juries to play a larger fact-finding role, and he also acknowledged that the use of real offense conduct in sentencing would be incompatible with having juries play such a role (USSC, 2006).<sup>14</sup> Accordingly, the option of expanding the utilization of juries in the federal system to meet the demands of the Sixth Amendment was abandoned.

Instead, in line with the exceptions for indeterminate systems outlined in its earlier decisions in *Apprendi-Blakely*, the Court sought to remedy the situation by modifying the provisions of the SRA that were found unconstitutional (Frase, 2007). Specifically, it excised 18 U.S.C. § 3553(b)(1), which made the Guidelines mandatory, as well as 18 U.S.C. § 3742(e), which outlined the grounds for appeal related to guideline departures (Seghetti and Smith, 2007; USSC, 2006; Yeh and Doyle, 2009). Although these modifications disrupted Congress' vision of a

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<sup>14</sup> Under §1B1.3 of the Guidelines, offenders can be sentenced for "relevant conduct," or "real-offense conduct," including behaviors for which they were never convicted or even formally charged (Burns, Elden, and Blanchard, 1997; Spohn, 2002; Tonry, 1996). This was designed to transfer the responsibility for the determination of offense-related facts to judges (Albonetti, 1997; Nagel and Schulhofer, 1992) and thus curtail potential abuse of plea bargaining by prosecutors.

mandatory guidelines system, Justice Breyer wrote that Congress' goal of linking the imposed sentence to the offender's real offense conduct was maintained (*Booker*, 2005: 253).<sup>15</sup>

The Court's ruling in *Booker* transformed the Guidelines overnight from a mandatory system into an advisory system. Still, district courts initially reacted with confusion to the instruction that, although judges are not required to apply the Guidelines, they still "must consult those guidelines and take them into account when sentencing" (*Booker*, 2005: 264). Much of the confusion derives from the Court's failure to more explicitly define what it meant when it said that sentences falling outside of the Guidelines would be subject to reversal if they failed to meet a "reasonableness" standard (Frase, 2007; Hofer, 2007; Seghetti and Smith, 2007; Yeh and Doyle, 2009). Subsequently, two opposing viewpoints developed regarding the extent to which the Guidelines must be considered. At one end of the spectrum was the viewpoint that significant weight should still be placed on the Guidelines (*United States v. Wilson*, 2005), and at the other end was the view that the Guidelines are only one of the factors detailed in 18 U.S Code §3553(a) to be considered in determining a sentence (*United States v. Ranum*, 2005) (Seghetti and Smith, 2007; USSC, 2006). Ultimately, the Court ruled in *Rita v. United States* (2007) that sentences within the range prescribed by the Guidelines may be presumed to be reasonable and that district judges are afforded the discretion to prescribe non-guideline sentences both by

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<sup>15</sup> The voting of the justices for both the merit and remedial opinions is of note. For the merits opinion, justices Scalia, Souter, Thomas, and Ginsburg joined Justice Stevens, who delivered the opinion. Justice Ginsburg swapped sides to take part in the 5-4 vote for the remedial decision, the opinion for which was delivered by Justice Breyer and which effectively made the Guidelines advisory.

departing and applying § 3553(a), although the Court asserted that reasons should be provided for individual sentencing decisions (Seghetti and Smith, 2007).

The advisory nature of the Guidelines, as laid out by *Booker*, was further reinforced by two additional cases decided in 2007 - *Gall v. United States* and *Kimbrough v. United States*. The High Court's decision in *Gall* indicated that sentences outside of the Guidelines were not necessarily "unreasonable." The *Kimbrough* decision further legitimized sentencing outside of the Guidelines by confirming that, in relevant cases, judges may consider the crack- versus powder-cocaine disparity as justification to deviate from prescribed sentencing ranges.

Opinions and predictions about the impact of *Booker* on the federal sentencing landscape have varied widely. For instance, former Attorney General Alberto Gonzales (2006) predicted that "shorter sentences and disparities among sentences will occur under a system of advisory guidelines." The major fear here is that unwarranted disparities will return, as judges will be able to use any criteria they deem appropriate in determining an individual's sentence. On the other hand, others have argued that repaving the way for judicial discretion in an overly harsh federal system is a positive move, as it will allow for sentences to be tailored to meet the individual needs of offenders and to again treat different cases differently (Hofer, 2007). There is a relatively small body of empirical research that examines the full impact of the *Booker* decision on federal sentencing practices, and it is examined in the next chapter.

### CHAPTER III: LITERATURE REVIEW

A considerable body of research exists surrounding sentencing guidelines at both the federal and state levels, most of which has been focused on whether guidelines systems have been effective in reducing unwarranted sentencing disparities. In general, statewide studies have found that legally relevant factors (i.e., prior criminal record and offense seriousness) are the primary determinants of sentencing outcomes. At the same time, some of this research presents evidence that non-legal factors, including age, race, and gender, continue to play a role in how punitively the criminal justice system treats offenders (Engen and Gainey, 2000; Kramer and Steffensmeier, 1993; Steffensmeier, Kramer, and Striefel, 1993; Steffensmeier, Kramer, and Ulmer, 1995; Stolzenberg and D'Alessio, 1994). A large body of research completed on sentencing outside the guidelines suggests that departures represent a significant source of disparate treatment (Engen and colleagues, 2003; Johnson, 2003, 2005; Kramer and Ulmer, 1996, 2002; Ulmer, 1997). Additionally, some research, particularly in Pennsylvania, suggests that there is significant variation in sentencing depending on characteristics of the local courtroom context (Johnson, 2005, 2006; Kramer and Ulmer, 1996, 2002; Ulmer and Johnson, 2004).

Because state guidelines systems vary significantly from the federal system and, indeed, from each other with respect to both sentencing law and culture, it may be difficult to extrapolate the results from state-level studies to the federal system. This chapter focuses on federal sentencing research and begins by detailing the research conducted on the pre-*Booker* period, before the Guidelines were transformed

from mandatory to advisory. Particular attention is then focused on research examining the way both drug and immigration offenders have been sentenced under the Guidelines. Finally, the chapter concludes by turning to the handful of studies that have thus far examined the impact of *Booker* on federal sentencing outcomes.

### ***Pre-Booker* Research on the Federal Guidelines**

*Pre-Booker* research on the federal guidelines largely focused on three different areas: the role of extra-legal factors in sentencing outcomes, the shift of discretion from judges to prosecutors, and the way specific offenses are sentenced under the Guidelines. Studies falling into each of these categories are outlined below.

#### *The Role Played by Extra-Legal Factors*

As with sentencing research in general, studies conducted at the federal level have chiefly focused on whether different groups (e.g., racial groups, genders) have been treated comparably in the post-Guidelines era. Such research has largely utilized the publicly available data maintained by the Commission.<sup>16</sup> An early annual report indicated that 81% of cases were sentenced within the applicable sentencing range and that disparity in terms of both sentences given and time served decreased under the Guidelines (USSC, 1991). Other early evaluations also found that the Guidelines reduced unwarranted disparity (McDonald and Carlson, 1993), even if

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<sup>16</sup> This approach is not without criticism. Wellford (2007) points out that the advancement of sentencing research has been stunted, both in terms of theory development and in terms of understanding the wide range of factors that may actually influence sentencing decisions, by researchers' willingness to rely solely upon officially collected data (see also Everett and Wojtkiewicz, 2002). Additionally, others have noted that the road to sentencing may be a long one, and offenders may be subjected to disparate treatment at earlier, more hidden stages in the criminal justice system, which were not subject to reform and the importance of which may have been exacerbated by the Guidelines (Anderson, Kling, and Stith, 1999; Bushway and Piehl, 2001, 2007; Farabee, 1998; Kempf-Leonard and Sample, 2001). Still, due to the dearth of data in these earlier stages, as well as the wide availability of sentencing data, it is perhaps not surprising that the sentencing phase has received a great deal of attention from researchers seeking to examine disparities in the criminal justice system.

surveys administered by the General Accounting Office in 1992 indicated that most judges and defense attorneys were skeptical of such reports (see also Farabee, 1998). Later research examining the impact of the Guidelines on inter-judge differences in the average sentence lengths prescribed for criminal defendants indicated that these disparities had been reduced since the introduction of the Guidelines (Anderson, Kling, and Stith, 1999; Hofer, Blackwell, and Ruback, 1999; USSC, 2004).

Although much of the research conducted at the federal level suggests that legal factors (i.e., current offense and criminal history) are the primary consideration of federal judges in making sentencing decisions, a sizeable body of evidence does suggest that extralegal factors also make their way into sentencing decisions, albeit more subtly (Albonetti, 1997, 2002; Everett and Wojkiewicz, 2002; Hartley, Maddan, and Spohn, 2007; Hebert, 1997; Johnson and Betsinger, 2009; Johnson, Ulmer, and Kramer, 2008; Kautt and Spohn, 2002; Kempf-Leonard and Sample, 2001; Mustard, 2001; Spohn and Fornango, 2009; Steffensmeier and Demuth, 2000; Zatz, 2000). Kempf-Leonard and Sample (2001), for example, examined cases sentenced in the Eighth Circuit in 1993-1994 and found that, although only legally relevant variables factored into the incarceration decision for female offenders, extra-legal factors were relevant to sentencing outcomes for males. More specifically, younger men and men who were poor were more likely to be incarcerated, and younger men were also subject to lengthier sentences. In addition, regardless of gender, white defendants were more likely to benefit from downward departures.

Similarly, Mustard (2001) found sentencing disadvantages for blacks and males in the incarceration decision, the length of sentence, and the likelihood and

extent of departure. Work by Steffensmeier and Demuth (2000) found that Hispanic males were generally more susceptible to longer sentences than white males and that disparities were especially great when downward departure cases were considered separately. Additionally, Hispanics were less likely than both whites and blacks to be the recipients of substantial assistance departures, especially in drug cases. At the same time, work by Johnson and Betsinger (2009) generally found that, even after controlling for legally relevant factors, Asian Americans were sentenced similarly to, and sometimes more leniently than, white offenders, while blacks and Hispanics were subject to more punitive treatment at the hands of federal judges. Finally, Johnson and colleagues used multilevel modeling and found that individual racial differences in sentencing “were exacerbated in socioeconomically disadvantaged districts and in districts with larger minority populations” (Johnson, Ulmer, and Kramer, 2008: p 769).

*Shifting Discretion: Plea Bargaining and §5K1.1 Departures*

Because judge-initiated (§5K2.0) departures have historically been subject to a rather limited appellate review (i.e., appeals courts simply determined whether judges appropriately departed from the Guidelines based on factors not considered by the Commission), they have been acknowledged as a potential entry point for the consideration of offender and offense characteristics prohibited by the SRA (Farabee, 1998; USSC, 2003). However, because judicial discretion was the primary focus of the reform efforts enacted by the Guidelines (Farabee, 1998), and because judges have been fairly limited in the number of ways they could legitimately depart from the recommended sentencing ranges, a growing body of federal sentencing research

has focused on plea bargaining and prosecutor-initiated departures as avenues for the possible reintroduction of unwarranted disparities (Albonetti, 2002; Farabee, 1998; Hartley, Madden, and Spohn, 2007; Spohn and Fornango, 2009). While the architects of the Guidelines acknowledged the potential for prosecutorial abuse of plea bargaining as a technique for achieving sentencing reductions for offenders, prosecutors still possess the power to determine which charges are filed and to facilitate sentencing reductions for offenders who plead guilty and for those who qualify for a two-level sentencing reduction for “acceptance of responsibility” (Albonetti, 1997, 2002; Gyurci, 1994; Hartley, Madden, and Spohn, 2007; Nagel and Schulhofer, 1992; Spohn, 2002; Spohn and Fornango, 2009; Stith and Cabranes, 1998).<sup>17</sup> As Spohn and Fornango (2009: 814) state, “By choosing the charges the defendant will face and by determining the ‘facts’ (for example, the type and amount of drugs for which the defendant will be held responsible) that will be proved at trial, the AUSA [Assistant United States Attorney] essentially determines the range of penalties from which the judge must choose.”

In addition, it is prosecutors who remain largely in control of §5K1.1 departures, which are sentencing discounts that may be supplied to offenders who offer substantial assistance to the government.<sup>18</sup> These departures are especially common in cases involving drug offenses; when approved, they may remove the mandatory minimum sentences that offenders would normally face (Hartley, Madden, and Spohn, 2007; Spohn and Fornango, 2009). Surprisingly little accountability is

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<sup>17</sup> Disparities in the use of acceptance of responsibility discounts have been demonstrated by some research (Everett and Wojkiewicz, 2002).

<sup>18</sup> For a list of the factors considered by judges when determining whether or not to grant a substantial assistance departure, see Spohn and Fornango (2009).

required for these decisions (Farabee, 1998; Gyurci, 1994; Steffensmeier and Demuth, 2000), and, although judges can ultimately deny substantial assistance motions, this is rare, and only the prosecutor can move for such a departure in the first place (Gyurci, 1994; Maxfield and Kramer, 1998).<sup>19</sup> The concern over §5K1.1 departures is that disparities will arise, especially because no guidelines exist to ensure that similar discounts are supplied for similar levels of assistance to the government (Hartley, Madden, and Spohn, 2007; Maxfield and Kramer, 1998; Richman, 1998).<sup>20</sup>

Much of what we know about prosecutorial decision-making comes either from qualitative research (Nagel and Schulhofer, 1992; Schulhofer and Nagel, 1989) or from studies that have used the Commission's data to examine prosecutorial decision-making indirectly (Albonetti, 1997; 2002; Johnson, Kramer, and Ulmer, 2008; Mustard, 2001; Steffensmeier and Demuth, 2000).<sup>21</sup> One exception is the work by Spohn and Fornango (2009), which supplemented publicly available Commission

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<sup>19</sup> Still, the judge's discretion in granting substantial assistance departures should not be minimized. As Burns, Elden, and Blanchard (1997: 1327, emphasis in original) state: "Once a section 5K1.1 recommendation is made, the sentencing fate of the defendant is entirely up to the judge, who decides whether, and to what degree, the sentence should be reduced for the cooperation. . . . The prosecutor cannot cause a sentence to be reduced even one day below the Guidelines range unless the court also deems it appropriate. The prosecutor possesses only the executive power to *seek* a departure" (see also Hartley, Madden, and Spohn, 2007; Spohn and Fornango, 2009).

<sup>20</sup> Steffensmeier and Demuth (2000) worry that the increased discretion afforded to prosecutors by the option to initiate §5K1.1 departures puts minorities at risk for greater sentencing disadvantages while, at the same time, making prosecutorial decision-making even less transparent (see also Albonetti, 1997; Anderson, Kling, and Stith, 1999). This concern seems justified by the Commission's own study indicating that the decision to file for §5K1.1 departures was unevenly embraced by individual prosecutors and across U.S. attorney offices (USSC, 1991; see also Maxfield and Kramer, 1998). Johnson, Ulmer, and Kramer (2008: 772) note that the changes brought about by *Booker* mean that "the issue of interdistrict variation in departures and their role in producing unwarranted sentencing disparities will likely become even more important."

<sup>21</sup> Maxfield and Kramer (1998: 6) note that, although the Department of Justice requires that all U.S. attorney offices document their use of substantial assistance departures, "there are no standards given to the individual U.S. attorney offices defining how the information is to be maintained, nor are the data required to be compiled or reported to the central DOJ offices in Washington, D.C."

data with court documents in three federal district courts (Minnesota, Nebraska, and the Southern District of Iowa) during fiscal years 1998 through 2000. These data allowed for the identification of the AUSA assigned to handle the case and revealed considerable differences among individual prosecutors with respect to filing motions for substantial assistance departures, even after controlling for characteristics of the individual offender and case. The authors found that discounts were more likely in cases involving offenders who had more dependents and for those who were younger, white, female, better educated, citizens of the United States, and using drugs at the time of the crime. Importantly, these recent findings are consistent with the bulk of prior work that suggests guidelines departures are one of the primary mechanisms leading to sentencing disparities in federal court (Albonetti, 1997; Johnson, Kramer, and Ulmer, 2008; Mustard, 2001).

#### *Offense-Specific Research: Drug Offenses*

Much of the federal sentencing literature has focused specifically on the punishments faced by drug offenders, with good reason. Until 2009, when immigration offenses surpassed them, drug offenses were the most commonly prosecuted offenses in the federal system (Hartley and Armendariz, 2011). As a report by the Commission notes, “the annual number of offenders sentenced for [drug trafficking] has increased from 13,521 in fiscal year 1991 to 24,332 in fiscal year 2007, an increase of 80% in the number of drug trafficking offenders” (Reedt and Widico-Stroop, 2009: 7).

The prevalence of drug cases in the federal system stems from sentencing policies associated with the “war on drugs” begun by President Nixon. By the 1980s,

the primary targets of this “war” were crack and powder cocaine (Boyum and Reuter, 2005). Crack was particularly vilified as a drug which was not only extremely addictive, but which also threatened the unborn children of users and which generated violence, especially in inner city communities (Hartley and Miller, 2010; Loshin, 2007; Vagins and McCurdy, 2006; Yeh and Doyle, 2009). As a result of the “moral panic” surrounding crack (Chiricos, 2004; Hartley and Miller, 2010), the Anti-Drug Abuse Act of 1986 was enacted by Congress to address continuing concerns associated with the ills associated with this illicit substance. Although the Act was responsible for creating the reductions associated with substantial assistance departures (Maxfield and Kramer, 1998), it was also, more controversially, responsible for requiring that offenses involving crack be subject to particularly severe penalties. The alleged more serious danger associated with crack compared to powder cocaine was quantified in a 100:1 ratio, requiring that five hundred grams of powder cocaine be present to trigger the same mandatory minimum penalties that were demanded for just five grams of crack cocaine. The battle against crack offenses was escalated by Congress’ Omnibus Anti-Drug Abuse Act of 1988; while simple possession of any other drug (including powder cocaine), regardless of quantity, was associated with a maximum penalty of 1 year, possession of at least 5 grams of crack was subject to a mandatory 5 to 20 years in prison (Vagins and McCurdy, 2006).

There has been an abundance of criticism for policies directed at crack offenders, with many pointing out that the 100:1 ratio, in particular, has not only failed to result in targeting serious drug traffickers (Hartley and Miller, 2010; Loshin, 2007; Vagins and McCurdy, 2006), but that it unfairly targeted blacks, as crack is

more commonly found in black communities than in those of whites (Hebert, 1997; Kautt and Spohn, 2002; LaFree, 1994; Tonry, 1996; Vagins and McCurdy, 2006). Hartley, Madden, and Spohn (2007) cite statistics showing that blacks are both disproportionately arrested and subject to lengthier sentences for crack-related offenses. Both the Commission and Congress acknowledged the inequality associated with this sentencing policy over time, and, as detailed by Yeh and Doyle (2009), Congress made some attempts in the past to address inequities associated drug sentencing. For instance, in 1994 it enacted the Violent Crime Control and Law Enforcement Act, which created a safety valve provision designed to mitigate the mandatory minimum sentences associated with certain categories of drug offenses. Still, repeated recommendations by the Commission that the 100:1 ratio be revised to more accurately reflect the actual differences between crack and powder cocaine were rejected by Congress, and the policy stood for more than two decades, resulting in sentences “three to over six times longer for crack cocaine than for powder cocaine offenses” (USSC, 2006: 126; Yeh and Dolye, 2009; see also Loshin, 2007).<sup>22</sup>

Public perceptions of the elevated dangerousness associated with crack cocaine have diminished over time. Hartley and Miller (2010) analyzed the content of 78 *USA Today* articles related to narcotics offending that were published in the year 2000; despite the fact that more than 10,000 cocaine cases went through the federal system that year, only 12 articles were specifically related to crack, while 53

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<sup>22</sup> The Commission amended the Guidelines in 2007 in order to assign base levels for crack cocaine that corresponded to guidelines ranges including mandatory minimums (USSC, 2010b). “Crack cocaine offenses for quantities above and below the mandatory minimum threshold quantities similarly were adjusted downward by two levels” (USSC, 2010b: 2). In March 2008, the amendment was made retroactive, enabling some eligible incarcerated offenders to seek reductions in their sentences. President Obama signed the Fair Sentencing Act in August 2010, thereby reducing the 100:1 ratio and eliminating mandatory minimum penalties associated with crack-cocaine possession (American Civil Liberties Union, 2010).

focused on powder cocaine offending. Neither the offender's race nor the violence typically identified with cocaine was generally a focus in the sampled articles. Using the Commission's data for the same year, Hartley and Miller found that sentence length for crack and powder cocaine defendants was largely predicted by legally relevant factors and that most extra-legal factors were non-significant.

Still, a study by Hartley, Madden, Spohn (2007), which also used Commission data for the year 2000, found that the prosecutor's decision to file for a §5K1.1 departure motion was motivated by a combination of legally relevant and irrelevant factors. Motions for substantial assistance departures were less likely in cases involving crack rather than powder cocaine, while such motions were more likely in instances where a mandatory minimum sentence was involved and in which the offender was white, female, had more education, was charged with a more serious offense, and, in some instances, had a more extensive criminal history. Interactions between race and gender also proved to be particularly salient, as white males were more likely than black and Hispanic males to benefit from §5K1.1 departures. Among females, race played a significant role only in powder cocaine guidelines cases. Similar to the previous work by Kautt and Spohn (2002), which hypothesizes that judges may react to the criticism regarding disparate sentencing patterns for blacks and whites in crack cocaine cases by using whatever discretion they have to purposely decrease the sentences for black offenders, Hartley, Madden, and Spohn (2007) hypothesize that prosecutors may use substantial assistance departures in those instances involving particularly severe offenses or offender histories so that judges may prescribe more appropriate punishments.

### *Offense-Specific Research: Immigration Offenses*

Illegal immigration into the United States has increasingly become a focus of the federal government since the turn of the millennium. Since that time, Congress has passed both the Victims of Trafficking and Violence Protection Act of 2000 and the PROTECT Act of 2003 to control the penalties for illegal immigration, and the Department of Homeland Security (DHS) has turned its attention to securing the nation's borders, and to the southern border in particular, after the terrorist attacks on September 11, 2001 (Hartley and Tillyer, 2012; Logue, 2009). At the same time, immigration has become the source of heated ideological debate. As noted by Hartley and Armendariz (2011:47), "Current political discourse regarding national security threats from Mexican drug cartels and a concurrent media induced panic regarding immigration and crime has produced a fear of immigrants in the United States (Jaret, 1999; Logue, 2009)."

DHS estimates that there were approximately 10,790,000 unauthorized immigrants in the United States in 2010. Perhaps not surprisingly, immigration cases represent the fastest growing offense category in federal criminal caseloads; a 2009 report by the Commission indicates that the number of immigration cases increased by 165% during the previous decade, thereby contributing to the fact that these offenses now represent the largest percentage of the federal caseload. Yet, shockingly little federal sentencing research has focused specifically on immigration offenses.<sup>23</sup> This omission is especially astounding given the unique characteristics of both these offenses and the offenders who commit them. Compared to other federal

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<sup>23</sup> While Wolfe, Pyrooz, and Spohn (2011) discuss illegal immigration and negative perceptions of immigrants, their analyses focus on the role of citizenship status in the incarceration and sentence length decisions and are not specific to immigration offenses.

offenders, greater percentages of immigration offenders are Hispanic, possess lower levels of education, and are non-citizens (Schmitt, 2009); as noted by Hartley and Tillyer (2012), non-citizens may be more susceptible to prison terms because they are ineligible for alternative sanctions. Immigration offenders are also likely to benefit from fast-track, or Early Disposition program, departures, which are authorized in seventeen federal districts (Gorman, 2009). These sentencing discounts were first recognized by Congress in the PROTECT Act and were incorporated into the Guidelines in §5K3.1, which allowed for courts to approve downward departures of up to four levels at the behest of prosecutors in Early Disposition cases (Hartley and Tillyer, 2012).<sup>24</sup>

### **Post-Booker Sentencing Research**

The *Booker* decision has signaled a shift toward a renewal of judicial discretion in the federal system. While judges must still consult the Guidelines in all cases, they now have the option of deviating from their recommendations with little fear of appellate reprisal, although they are still bound to impose the sentences mandated by mandatory minimum statutes (Seghetti and Smith, 2007; USSC, 2006). The advisory status of the Guidelines, and the discretion that judges are accordingly afforded, has led to the general hypothesis and, in some cases, fear that judges will increasingly impose less severe sentences than those that were imposed during the era when the Guidelines were mandatory (pre-*Booker*).

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<sup>24</sup> The PROTECT Act was passed by Congress in 2003 to reduce the number of judge-initiated departures from the Guidelines. It limited the grounds for departures to what was laid out in 18 U.S.C. § 3553(a)(2) and required that the court provide written reasons for departures, that the Chief Judge of each district submit documentation concerning departures to the Commission, and that appeals courts provide *de novo* reviews of district courts' departure decisions (USSC, 2003).

A small but growing group of studies have examined the impact of *Booker/Fanfan* on federal sentencing outcomes. The Commission's (2006) *Final Report on the Impact of United States v. Booker* examines sentencing in three separate time periods: pre-PROTECT Act, post-PROTECT Act, and Post-*Booker*. Surprisingly, the Commission found that many pre-*Booker* sentencing patterns remained in place following the High Court's decision, with the vast majority of cases (85.9%) sentenced in conformance with the federal sentencing guidelines.<sup>25</sup> Further, the Commission found that most offenders were still sentenced to prison terms, with no significant difference being found in the imprisonment decision between the pre- and post-*Booker* time periods. In addition, the average sentence length actually increased following the decision, continuing a pre-*Booker* trend; this was true across offense types (except for some immigration offenses), and the factors associated with the sentence length outcome were basically unchanged.<sup>26</sup>

Despite these consistencies, pre- to post-*Booker* differences were evident in out-of-range sentences.<sup>27</sup> Above-range sentences doubled after *Booker*, with those sentenced after the decision finding themselves 20.7% more likely to receive an upward departure.<sup>28</sup> Below-range sentences also increased following the *Booker* decision, with government-initiated downward departures (e.g., substantial assistance

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<sup>25</sup> At the same time, judges continued to select within-range sentences from the low end of the applicable guideline range (i.e., the presumptive sentence) 60% of the time.

<sup>26</sup> The Commission does note that the trial variable was not significant in the post-*Booker* time period.

<sup>27</sup> In *Irizarry v. United States* (2007), the Court reasoned that, because *Booker* made the Guidelines advisory, advance notification of deviations from the Guidelines – now known as “variances” – is not required. Still, the term “departure” is used throughout this dissertation for ease of description between the pre- and post-*Booker* periods.

<sup>28</sup> Still, the rate of upward departures remained very low, with only 1.6% of cases receiving a departure post-*Booker* (USSC, 2006).

and Early Disposition Program departures) accounting for a large percentage of these sentences.<sup>29</sup> Judge-initiated downward departures also experienced an increase following the *Booker* decision, although monthly analyses showed the trend to be stabilizing.<sup>30</sup> The multivariate analyses completed by the Commission reveal that the factors associated with downward departures changed somewhat following *Booker*, with non-citizenship, greater criminal history points, career offender status, and the application of a mandatory minimum sentence all decreasing the likelihood of downward departures post-*Booker*.

The Commission (2006) also explored the impact of the *Booker* decision on the sentencing of some specific offense and offender types. While district court judges continued to sentence crack cocaine cases in accordance with the Guidelines in the initial period following the *Booker* decision, they appeared to be seizing the opportunity to exercise their discretion in cases involving career offenders (most often involving drug trafficking offenses), as there was a post-*Booker* increase in the rate of below-range sentences for these individuals (see also Hofer, 2007).

Differences in the sentences of subgroups of offenders persisted after the *Booker* decision, with men and blacks continuing to receive longer sentences than women

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<sup>29</sup> Those sentenced after the *Booker* decision found themselves 61.4% more likely to receive a government-initiated downward departure (excluding substantial assistance); at the same time, the likelihood of receiving a substantial assistance departure actually decreased by 6.2% in the post-*Booker* period compared with the post-PROTECT Act period (USSC, 2006).

<sup>30</sup> Although the finding that average sentence length increased post-*Booker* seems to be at odds with the increased use of downward departures, government-initiated or otherwise, the sentence length increase is largely explained by the fact that presumptive sentences have been trending upward since Fiscal Year 2003 (see also Hofer, 2007). The Commission (2006) and Hofer (2007) point out that several guideline and statutory amendments may be responsible for this increase, including the 2001 “Economic Crime Package,” the 2002 Sarbanes-Oxley Act, and the PROTECT Act. The Commission (2006:73) also suggests the increase in presumptive sentences may be due to the possibility that “more serious offenses may have been brought for prosecution” (see also Hofer, 2007).

and whites; interestingly, post-*Booker* analyses by the Commission showed no difference between the sentences of whites and Hispanics.

Hofer (2007) extends the work of the Commission (2006) by examining the reasons given by judges for departing from the Guidelines in the post-*Booker* era. He finds that inadequate criminal history (§4A1.3) was the reason most commonly cited by judges. Judges seemed to be particularly wary of applying the “career criminal guideline” (§4B1.1) to more harshly sanction those convicted of repeat drug trafficking and violent offenses; in the post-*Booker* era, such career offenders had significantly higher rates of departure (22.3%) than the overall caseload (13.0%). Judges in the post-*Booker* era also increasingly cited offender characteristics (e.g., age, physical condition, family ties, etc.) as reasons for departing from the guidelines; recall that, prior to *Booker*, judges could not generally consider these characteristics as reasons for departure, as they were deemed “not ordinarily relevant” under Chapter 5, Part H of the *Guidelines Manual*.

In 2010, the Commission revisited its earlier analyses (USSC, 2006) in order to better assess how the *Booker* decision has affected long-term sentencing trends by demographic factors; this more recent examination is limited to sentence length outcomes. In their first set of analyses, the Commission (2010a) utilized the original methodology used for the 2006 report but updated the time frames to better highlight sentencing patterns that have occurred since the *Booker* decision; accordingly, they divided the data into a post-PROTECT Act period, a post-*Booker* period (i.e., following the *Booker* decision but preceding both the *Kimbrough* and *Gall* decisions), and a post-*Gall* period (i.e., following the *Kimbrough* and *Gall* decisions and ending

with the conclusion of fiscal year 2009). The Commission found that sentence length differences between black and white offenders were not significant during the post-PROTECT Act period but that significant differences between these two race groups did emerge during the post-*Booker* period; since that time, the difference between the two groups has been growing, with the post-*Gall* period witnessing sentence lengths that were 10% longer for blacks. As in its 2006 report, the Commission found that sentence lengths for white and Hispanic offenders did not differ significantly in any of the three time periods examined. At the same time, sentence length patterns by gender demonstrated significant differences across all three periods, with males experiencing significantly longer sentences than females.

Using a “refined model” to account for race and gender interactions, to control for pre-sentence detention status, and to exclude variables highly correlated with the presumptive sentence,<sup>31</sup> the Commission (2010a) found that differences in sentence lengths between black and white male offenders have been increasing since the post-PROTECT Act period and that they were the greatest in the post-*Gall* period, with black males receiving sentences that were 23.3% longer than those of white males during that period. Although no significant difference in sentence lengths was evident between Hispanic males and white males in the post-*Booker* period, Hispanic males received significantly longer sentences in the post-*Gall* period.

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<sup>31</sup> Excluded variables include: “(1) the number of criminal history points; (2) whether the offender received a “safety valve” adjustment (in drug cases); (3) whether the offender was convicted of an offense under 18 U.S.C. § 924(c); (4) whether the offender received a specific offense characteristic (“SOC”) enhancement for use of a weapon; (5) whether the Career Offender enhancement applied; (6) whether the Armed Career Criminal enhancement applied; and (7) the offender’s role, if any, in the offense (as reflected by a mitigating role adjustment, an aggravating role adjustment, or no role adjustment)” (USSC, 2010a: 20-21).

Opposing findings have been reached in three recent articles by Ulmer and his colleagues (Ulmer and Light, 2010; Ulmer, Light, and Kramer, 2011a, 2011b). After providing a critique of the methodology used by the Commission (2010a), Ulmer, Light, and Kramer (2011b) provide a re-analysis of the impact of *Booker* that: 1) separates the imprisonment and sentence length decisions; 2) defines the incarceration decision purely with respect to receipt of prison or not; 3) controls for criminal history; 4) excludes immigration offenses; and 5) investigates whether departures have increased following the *Booker* and *Gall* decisions. Their analyses utilize data broken into pre-PROTECT, PROTECT, post-*Booker*, and post-*Gall* periods. With these changes in place, the authors conclude that the increased odds of imprisonment for black males in the post-*Gall* period, and not any evident sentence length disparities, may have influenced the Commission's (2010a) findings regarding increased racial disparity after *Booker* and *Gall*. Importantly, they note that this post-*Gall* sentencing disadvantage for black males is significant relative only to the PROTECT period, which they argue stands out as "the truly unusual period in the history of the Guidelines" due to "relatively low levels of disparity [that] were an anomaly compared with the earlier years when the Guidelines were also mandatory (particularly the pre-*Koon* period), as well as the post-*Booker* years" (Ulmer, Light, and Kramer, 2011b: 1105). In addition, the authors report that controlling for criminal history mediates the effect of being a black male and that, when immigration offenses are excluded from the models, sentence length disadvantages for black males are reduced and that "there is actually *less* length disparity affecting Black males in the post-*Booker/Gall* periods than in the pre-PROTECT era" (Ulmer, Light, and

Kramer, 2011b: 1106). Finally, though Ulmer and colleagues report no post-*Booker/Gall* increases in disparity for judge-initiated departures, they do indicate that such disparities for black and Hispanic males were apparent for prosecutor-initiated departures.

Ulmer and Light (2010) further investigate *Booker*'s impact by surveying federal court personnel about their perceptions and by using multilevel modeling to examine whether between-district variation has increased following the High Court's decision. Their survey results indicate that federal judges and defense attorneys associate the post-*Booker* era with increases in the use of waivers of right to appeal, negotiations around relevant conduct, and open pleas. At the same time, these individuals do not believe that defense attorneys have gained a bargaining advantage since the Supreme Court's decision, and they believe sentences have stayed stable. These perceptions are substantiated by statistical models that utilize federal sentencing data for the pre-PROTECT, post-PROTECT, and post-*Booker* periods. While the authors find that the post-*Booker* period is associated with shorter sentences for both drug and violent offenders, their results also show that extralegal disparities and inter-district variations have not increased since the Supreme Court's decision.

Ulmer, Light, and Kramer (2011a) focus more explicitly on variation between districts in incarceration and sentence length decisions following the Supreme Court's ruling. Consistent with their other work (Ulmer, Light, and Kramer, 2011b), these authors partition the data into pre-PROTECT, post-PROTECT, post-*Booker*, and post-*Gall* (extending to November 2008) periods. Their analysis again excludes

immigration offenses, but it extends the range of independent variables previously examined in post-*Booker* research to incorporate a limited range of contextual variables, including districts' departure rates, caseload per judge, and mean offense level. Ulmer and colleagues conclude that extralegal disparity generally did not increase in either the post-*Booker* or post-*Gall* periods. Additionally, while federal courts exhibited substantial variation in terms of the role played by extralegal characteristics in sentencing outcomes, there was not significantly more inter-district variation in the post-*Booker* or post-*Gall* periods than in earlier time periods.

#### *Sentencing for Drug Offenses in the Post-Booker Era*

As is the case more generally, post-*Booker* research focusing specifically on drug offenses is fairly limited. Initial research by the Commission (2006) indicates that, contrary to expectations, federal judges did not seem to be using *Booker* as an excuse to compensate for the huge sentencing discrepancy between cases involving crack versus powder cocaine in the period immediately following the decision.<sup>32</sup> Instead, 84.8% of crack cocaine cases were sentenced in accordance with the Guidelines post-*Booker*, and this was roughly the same degree of guidelines conformance as for other drug types. Later research by Ulmer and Light (2010) and by Ulmer, Light, and Kramer (2011a) indicates that the post-*Booker* period was actually associated with shorter sentences for both drug and violent offenders. Ulmer and colleagues note that these findings are “not surprising given the very prevalent view among federal judges and other commentators that the drug Guidelines in particular were too severe” (Ulmer, Light, and Kramer, 2011a: 830). Another study

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<sup>32</sup> The Commission's (2006) report was released before the Supreme Court decided *Kimbrough v. United States* (2007), which clarified that the parts of the Guidelines specifically pertaining to the crack/powder cocaine differential, like the Guidelines more generally, are now effectively advisory.

by Hartley and Armendariz (2011) finds that noncitizens generally received shorter sentences than citizens in federal drug cases and that differences in the effects of presumptive sentences and departures on sentence length varied by district and by citizenship status. Unfortunately, these researchers used only a single year (2008) of the Commission's data, making it impossible to determine *Booker's* influence on these findings.

#### *Sentencing for Immigration Offenses in the Post-Booker Era*

Post-*Booker* research centered on immigration offenses is even more limited than the body of research focused on drug offenses. Importantly, the Commission's (2006; 2010) post-*Booker* work masks the way immigration cases are handled by ignoring the unique nature of these offenses and including them in multivariate models with all other offenses. On the other hand, research by Ulmer and his colleagues (Ulmer and Light, 2010; Ulmer, Light, and Kramer 2011a; 2011b), consistent with prior work by Steffensmeier and Demuth (2000), excludes these offenses all together due to both the unique way in which they are processed (e.g., the use of fast-track departures) and because citizens cannot be charged with illegally entering or staying in the country.

Hartley and Tillyer (2012) use federal sentencing data for 2008 to examine sentence lengths for immigration offenders who were issued pre-sentence investigation reports (PSR). They find that males, those detained prior to sentencing, those with longer presumptive sentences, and those with more extensive criminal histories were subject to longer prison terms, while Hispanics, younger offenders, and U.S. citizens received shorter terms. Immigration offenders charged with trafficking

were subject to sentences that were 28% longer than those sentenced for illegal entry, while offenders sentenced for illegal documentation were the recipients of sentences that were 18% shorter. In addition, immigration offenders sentenced in the five southwestern border districts received longer average sentences than those in non-border districts. Hartley and Tillyer (2012) further partitioned the data based on the three different types of immigration offenses and for the five southwestern border districts. The results of these disaggregated analyses revealed substantial district-level variation in sentence lengths. Unfortunately, Hartley and Tillyer's (2012) study was limited to a single year and was not focused on assessing the impact of the *Booker* decision on sentencing patterns for immigration offenses over time.

#### *Summarizing Post-Booker Research*

To summarize, existing post-*Booker* research suggests that many of the pre-*Booker* trends continued into this new era of sentencing, although some research indicates that extralegal disparities following the decision were not as large as was originally anticipated. While informative, extant research on *Booker*'s influence is limited in several important ways: 1) it has paid only limited attention to drug offenses, with little consideration of the specific impact the decision has had on crack and powder cocaine sentencing; 2) it largely ignores immigration offenses; and 3) it has yet to fully consider important jurisdictional-level differences in the effects of *Booker* across federal district courts. Accordingly, the next chapter provides the framework for addressing these limitations by outlining the specific hypotheses that this dissertation will examine.

## CHAPTER IV: THEORY AND HYPOTHESES

Extant post-*Booker* studies generally indicate that judges have continued to sentence according to the Guidelines since the High Court's decision. However, these studies do not focus specific attention to changes in drug or immigration sentencing trends, and they examine only a limited range of contextual factors. Using previous research as a guide, this chapter outlines predictions about the impact of *Booker* on federal sentencing outcomes in general and on outcomes for drug and immigration offenses in particular.

### **Understanding the General Influence of *Booker* on Sentencing Outcomes**

The limited body of research that has examined the influence of *Booker* on federal sentencing outcomes has largely found that the use of imprisonment has not changed significantly in the aftermath of the Supreme Court's decision and that offenders sentenced in the period following *Booker* actually received longer prison terms than those sentenced prior to the decision (Hofer, 2007; USSC, 2006). Several scholars have provided explanations for why sentencing patterns following the Supreme Court's decision have largely stayed consistent with pre-*Booker* trends. Their arguments primarily center on the idea that the Guidelines "have become embedded in the organizational and legal culture of federal court communities" (Ulmer and Light, 2010: 175; see also Engen, 2009; Engen and Steen, 2000; Hunt and Connelly, 2005). The federal courts have had two decades to adjust to the changes necessitated by the SRA and Guidelines. Accordingly, it can be argued that, in that time, judges and other federal criminal justice personnel have come to more or less embrace and internalize the ideals of these reform efforts (see Engen, 2009) and that

they are used as a way to “maintain organizational efficiency” (Engen and Steen, 2000: 1387). To paraphrase one federal district judge, a whole generation of attorneys and judges has known only sentencing under mandatory guidelines (Chasanow, 2009; see also Hunt and Connelly, 2005). Additionally, it is possible that courtroom communities have incorporated the recommendations of the Guidelines into what are believed to be appropriate punishments, or “going rates,” for commonly seen crimes (Eisenstein, Flemming, and Nardulli, 1988; Kramer, 2009; Sudnow, 1965); as such, judges may be inclined to continue with the patterns that were established before *Booker* made the Guidelines advisory. Still, an alternative viewpoint posits that this stability in sentencing patterns will erode over time.<sup>33</sup>

This position contends that judges may have kept their decisions in line with preexisting sentencing patterns during the time immediately after *Booker* because they anticipated that Congress would step in to revise the Guidelines and once again make them mandatory. Hunt and Connelly (2005: 239) hypothesized that “judges will continue to comply with guidelines recommendations, at least in the typical cases for which the guidelines are designed, if the likely alternative is the perceived failure of advisory guidelines and more direct legislative control of sentencing, such as an expansion of mandatory minimum sentences.” As one federal judge described it, although judges still felt “tethered” to the Guidelines immediately following the *Booker* decision, they may feel more inclined to deviate from the sentences

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<sup>33</sup> More generally, clearer patterns will become evident as more time passes since the decision. The Commission (2006) notes that, “As appellate jurisprudence evolves, uncertainties are resolved, the system becomes more predictable, and a more complete picture of the impact of *Booker* on federal sentences can be developed” (USSC, 2006: v). Additionally, Wooldredge (2009) notes that it is important to include long follow-up periods when studying sentencing reform, as initial fluctuations in sentencing outcomes following reform efforts may only be temporary.

prescribed by the Guidelines with time (Chasanow, 2009). Thus, it may be that, with each passing year following the *Booker* decision, sentences will start to deviate more and more from those previously demanded by the Guidelines.

The latter viewpoint is embraced as a possible explanation for evidence confirming the emergence of greater extralegal disparities as more time has passed since the *Booker* decision. Theoretical perspectives articulated by both Albonetti (1991) and Steffensmeier and colleagues (Steffensmeier, Kramer, and Ulmer, 1995; Steffensmeier, Ulmer, and Kramer, 1998) may be used to help explain why minority offenders, in particular, have been historically sentenced more harshly in the federal system. Albonetti's (1991) uncertainty avoidance/causal attribution theory posits that courtroom actors, in an effort to make rational decisions with only incomplete pictures concerning defendants and their crimes, depend upon "patterned responses" (March and Simon, 1958), or a "perceptual shorthand" (Hawkins, 1981: 280), based around stereotypes about the offender's characteristics as well as the characteristics of the crime (see also Albonetti, 1997; 2002). As such, more severe sentences are likely to be imposed upon defendants who are viewed by judges to be dangerous and/or likely to recidivate, and minorities may be particularly susceptible to such attributions (see also Kautt and Spohn, 2002). Similarly, the offender's predicted dangerousness and the need to protect the community are included as one of the individual level "focal concerns" considered by courtroom personnel when making sentencing decisions (Steffensmeier, Kramer, and Ulmer, 1995; Steffensmeier, Ulmer, and Kramer, 1998). Other focal concerns include the offender's culpability and the degree of harm the offender inflicted upon the victim, as well as practical constraints

related to both the individual (e.g., health of the offender, ties to family members) and the organization (e.g., case flow, prison crowding). Building on the groundwork laid out by Albonetti (1991), Steffensmeier, Ulmer, and Kramer (1998: 768) posit that “judges, both as citizens and as elected officials, may share in the general stereotyping predominant in the community; and that racial (as well as age and gender) attributions will intertwine with the focal concerns . . . to influence judges in deciding whether to incarcerate an offender and the length of the incarceration.”

If increased judicial discretion allows for an increased reliance on stereotypical attributions, as these perspectives suggest, then one would expect sentencing disadvantages for young minority males to be exacerbated in the post-*Booker* era. Existing research suggests that some of the extralegal disparities for males and minorities that were apparent when the Guidelines were mandatory continued under the advisory system (USSC, 2006; 2010a). While existing studies are in disagreement over the precise role played by *Booker* (and subsequently by *Gall*) in exacerbating extralegal differences in incarceration outcomes (Ulmer, Light, and Kramer, 2011b; USSC, 2010a), they do suggest that these disparities have worsened over time. Sentence length outcomes have been found to be more nuanced, with Ulmer and colleagues noting that “one concludes that the post-*Booker* era has brought greater sentence-length racial disparity disadvantaging Black males *only* when one’s basis of comparison is the PROTECT era” (Ulmer, Light, and Kramer, 2011b: 1105, emphasis in original). The current dissertation is expected to confirm these previous findings and, accordingly, predicts that:

*H1: While post-Booker imprisonment and sentence length trends will largely resemble those found pre-Booker, differences between whites and minorities will be most apparent in more recent years of data.*

Although extant work suggests that the use of upward departures has grown in the post-*Booker* period, these deviations are still fairly uncommon, whereas the use of both government- and judge-initiated downward departures has continued to increase more steadily (Hofer, 2007; USSC, 2006). There is some evidence to suggest that disparities in the application of the substantial assistance (§5K1.1) departures initiated by prosecutors have increased over time, especially in the post-*Gall* period, with particularly pronounced disparities evident for Hispanic males (Ulmer, Light, and Kramer, 2011b). At the same time, because *Booker* has enabled an increase in judicial discretion, one would expect judge-initiated (i.e., §5K2.0) departures to be those most affected in the post-*Booker* period. Indeed, the work by Hofer (2007) seems to suggest that judges view these departures as a legitimate avenue for adjusting sentencing inequities based on a sense of fairness. In accordance with these findings, the following prediction is made:

*H2: Disparities will be evident between whites and minorities in the application of government- and judge-initiated departures from the Guidelines in the post-Booker period, particularly in more recent years of data.*

The focal concerns perspective presented by Steffensmeier, Ulmer, and Kramer (1998) acknowledges the fact that, beyond the individual level characteristics considered by the judge in any given case, there are additional factors that weigh into judicial decision-making. Indeed, characteristics of the courtroom community

(Eisenstein, Flemming, and Narduli, 1988), in addition to factors associated with the environment in which the courtroom is located, combine with individual case-level factors to influence sentencing decisions (Johnson, Ulmer, and Kramer, 2008; Spohn and Fornango, 2009; Ulmer, 1997; Ulmer and Kramer, 1998). While Steffensmeier, Ulmer, and Kramer (1998) give examples of organizational factors, such as case flow, which may serve as practical constraints to be considered in sentencing decisions by courtroom personnel, Dixon (1995) provides a more detailed explanation of the role played by the organizational context. She argues that “political, social, and organizational environment” (Dixon, 1995:1164) in which sentencing occurs varies from place to place and is highly influential in the sentencing outcomes that result.

As Johnson, Ulmer, and Kramer (2008) have expressed, the very nature of federal sentencing warrants a contextual approach. Because federal districts extend to every corner of the United States, there is likely to be variation in what constitutes justice from one jurisdiction to the next, despite the fact that each is encompassed by a cohesive federal system. The limited collection of studies that have examined *Booker*'s impact on sentencing outcomes have either paid limited attention to the contextual nature of federal sentencing (Hofer, 2007; USSC, 2006) or have found that inter-district variation has not increased since the decision (Ulmer and Light, 2010; Ulmer, Light, and Kramer, 2011a). However, the influence of the role played by contextual influences is worthy of further examination. Given that the *Booker* decision allowed for an expansion of judicial discretion and sentencing flexibility, one would expect that any inter-district variation in punitive practices should increase in the post-*Booker* period, as the formalized constraints of the Guidelines have been

relaxed, leaving judges' decisions more susceptible to community-level influences. Accordingly, the current dissertation examines the following prediction:

*H3: Inter-district variation in sentencing outcomes will increase significantly in the post-Booker era.*

Prior theorizing and research suggests that several factors may account for inter-district variation in sentencing. Some research has shown that lenient sentences are more likely to be granted in those jurisdictions experiencing a higher caseload to personnel ratio (Johnson, 2005, 2006; Kramer and Ulmer, 1996, 2002; Ulmer and Johnson, 2004). In such jurisdictions, court personnel may attempt to accommodate heavy caseloads and increasing demands on criminal justice resources by prescribing less severe punishments to the offenders sentenced therein. With formal constraints loosened under *Booker*, judges under heavy or increasing caseload pressure in the new sentencing era may feel justified in departing from the recommendations of the Guidelines in order to optimize case processing and maximize the usage of correctional resources. Accordingly, the following hypothesis is offered:

*H4: Judges in districts characterized by heavy or increasing caseload pressure, net of individual factors, will hand down less punitive sentences in the post-Booker period.*

Another important contextual factor in sentencing in the post-*Booker* period may be district-level history with departures. Judges in those districts that have traditionally high rates of departures may feel even more comfortable using them in the current sentencing environment, since the Court excised the portion of the Sentencing Reform Act (SRA) that dealt with departure-related appeals (18 U.S.C. §

3742(e)). Alternatively, it may be that judges in those districts with historically lower rates of departure may now be more empowered to grant departures, now that the threat of appeal no longer looms as large. Accordingly, the following bidirectional prediction can be made:

*H5: Historical district-level departure rates will influence the likelihood of individual departures in the post-Booker era.*

Prior research has also shown that sentencing disadvantages for minority offenders are often intensified in those districts characterized by large minority populations (Johnson, 2005; Johnson, Ulmer, and Kramer, 2008; Ulmer and Johnson, 2004). This research has been grounded in racial group threat theory, which posits that *large or growing* numbers of minorities are perceived as threatening to the dominance of the white majority (Blumer, 1958; Bobo and Hutchings, 1996; Dixon and Rosenbaum, 2004; Quillian, 1995). In the post-*Booker* era, as the legitimacy of judicial discretion is ostensibly renewed and judges are freer to consider a broader array of factors, the role of district-level racial composition may play a more prominent role in sentencing outcomes.

Previous studies have examined contextual variables in a somewhat static way, by using measurements of these variables at a single point in time. However, racial group threat perspectives clearly argue for the importance of both static (i.e., the percentage of minorities already living in an area) and dynamic factors (i.e., whether the minority population is experiencing significant growth), and the current study will attempt to account for both of these factors. Based on previous research grounded in the racial threat perspective, it is anticipated that:

*H6: Less favorable sentencing outcomes will be handed down in those districts in which blacks constitute a sizeable percentage of the population.*

*H7: Less favorable sentencing outcomes will be handed down in those districts in which blacks constitute a growing percentage of the population.*

*H8: The influence of district-level racial composition and growth will be greater in the post-Booker period.*

### **Booker's Role in Sentencing for Drug Offenses**

The role of renewed judicial discretion may be particularly salient in drug cases, given their controversial place in the federal sentencing narrative. Many judges have been among the group particularly critical of 100:1 crack versus powder cocaine sentencing mandates and their disparate impacts on minority offenders. Accordingly, *Booker* (and the clarification provided by *Kimbrough*) may present an opportunity for the bench to make sentences prescribed for these two drug offenses more equitable. Although research published by the Commission in 2006 found that federal judges continued to sentence the majority of crack cocaine and other drug types in accordance with the Guidelines after the Supreme Court's decision, their analyses are of limited utility because the Supreme Court had not yet decided *Kimbrough*, which effectively eliminated much of the uncertainty surrounding the application of *Booker* in crack and powder cocaine cases. Because this dissertation uses more data than were available when the Commission (2006) published its report, it is possible to get a more fully developed picture of the impact *Booker* has had on drug sentencing, and on sanctions for crack and powder cocaine offenses in particular. Accordingly, it is hypothesized that:

*H9: Sanctions for crack cases will become less punitive over time in the post-Booker era, making sentencing outcomes for crack more comparable to those found in cases involving powder cocaine.*

*H10: Disparities between whites and minorities in sentencing outcomes for drug case will decrease in the post-Booker era, particularly in more recent years of data.*

It is also anticipated that the relative district-level drug caseload composition specifically affects sentencing outcomes for drug offenders. Because the 100:1 crack-cocaine policy has been heavily criticized, it may be the case that those districts characterized by large or increasing numbers of drug cases will be simultaneously characterized by a greater willingness to grant more favorable sentences in drug cases in the post-*Booker* period. As such, it is expected that:

*H11: Judges in those districts characterized by heavy relative drug caseloads will grant favorable sentencing outcomes to drug offenders in the post-Booker period.*

*H12: Judges in those districts characterized by increasing relative drug caseloads will grant favorable sentencing outcomes to drug offenders in the post-Booker period.*

### ***Booker's Influence in Sentencing Outcomes for Immigration Offenses***

Very little research attention has been devoted to investigating how the Supreme Court's decision has influenced sentencing outcomes for immigration offenses. Existing *Booker* research has either masked the unique nature of immigration cases by including them in models with other types of offenses (USSC, 2006; 2010a), or it has excluded these offenses from examination altogether (Ulmer

and Light, 2010; Ulmer, Light, and Kramer, 2011a; 2011b). Although a recent study does pay particular attention to sentencing outcomes for these offenses (Hartley and Tillyer, 2012), this research was limited to a single year (2008) and was not intended to gauge the impact of *Booker* on sentencing trends for immigration offenders over time.

Although the research by Hartley and Tillyer (2012) provides the most meaningful findings relevant to immigration cases (i.e., that longer prison terms have largely been reserved for those immigration offenders who were male, were detained prior to sentencing, had more extensive criminal histories, and were charged with trafficking offenses), it is difficult to draw inferences regarding *Booker*'s influence from a study using only a single year of data. Two opposing viewpoints may apply. If judges are using *Booker* as an excuse to deviate from what are believed to be overly harsh sanctions for immigration offenses prescribed by the Guidelines, the post-*Booker* period may be characterized by less punitive sanctions for these offenses with each passing year. On the other hand, judges may use the increased discretion afforded to them by *Booker* to apply more punitive than prescribed sentences for the fastest growing segment of their caseload, particularly given the concomitant increase in anti-immigration sentiment since the beginning of the 21<sup>st</sup> century. Because little research exists to guide predictions, the analyses must be seen as exploratory in nature. As such, it is simply hypothesized that:

*H13: Sanctions for immigration offenses will exhibit differences in the post-Booker period, with differences becoming most apparent in more recent years of data.*

*H14: Disparities in sentencing outcomes will be evident between non-Hispanics and Hispanics in the post-Booker period, particularly in more recent years of data.*

Finally, because previous research has shown that differences exist between non-border and border-districts, it is predicted that:

*H15: Inter-district variation in sentencing outcomes for immigration offenses exists and will increase significantly in the post-Booker era.*

The next chapter is devoted to describing the data and methods to be used to examine these hypotheses.

## CHAPTER V: DATA AND METHODS

This chapter describes the data used in the current dissertation. Individual and case-level data provided by the United States Sentencing Commission, and contextual data obtained from a variety of sources, are used to investigate the hypotheses presented in the previous chapter. These data, and all of the statistical modeling approaches employed, are explained in detail below.

### Data

The primary source of data for the current analyses is the United States Sentencing Commission's annual *Monitoring of Federal Criminal Sentences* series, which contains information about the characteristics of the defendant and his or her case.<sup>34</sup> These data are available upon request from the Inter-University Consortium for Political and Social Research ([www.icpsr.umich.edu](http://www.icpsr.umich.edu)). The years 2000 through 2008 were selected to provide comparable samples of pre- and post-*Booker* cases. Cases were limited to those sentenced within the 90 districts in the United States (including the District of Columbia); U.S. territories - Guam, North Marianas, Puerto Rico, and the Virgin Islands – were excluded.

In addition to these individual-level (“Level 1”) data, community- and court-level (“Level 2”) data are also utilized in some of the analyses that follow. These data were derived from a variety of different sources, including statistical websites maintained by the United States Census Bureau, Fedstats, and the Administrative

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<sup>34</sup> The federal court system has domain over cases that involve the U.S. Constitution, laws passed by Congress, or crimes committed on federal property (Federal Judicial Center). Arrests may be made by federal law enforcement agencies including the Bureau of Alcohol, Tobacco, Firearms, and Explosives (ATF), the Drug Enforcement Administration (DEA), the Federal Bureau of Investigation (FBI), the Secret Service, U.S. Customs and Border Protection, and the United States Marshal Service (USMS), among others.

Office of the U.S. Courts on behalf of the Federal Judiciary. As discussed in more detail below, Level 2 data are only included in those models investigating the way contextual factors (e.g., caseload pressure, racial makeup) affect sentencing outcomes.

### *Dependent Variables*

In order to examine the study's hypotheses, multiple sentencing outcomes are examined; all outcome variables are found in the Level 1 data provided by the Commission. Since the inception of the Guidelines, the starting point for judges' decisions has been based upon where the defendant's criminal history score and current offense level intersect on the Sentencing Table (see Appendix A). The first decision facing sentencing judges is whether or not defendants should be incarcerated; this decision is captured in the data by the Commission's PRISDUM dummy variable, which is coded such that the receipt of a prison sentence is coded as 1.<sup>35</sup> The second decision facing judges is the length of prison term to which defendants are to be sentenced. This outcome is derived from the SENTTOT variable provided by the Commission and represents the total sentence in months, with values capped at 470 months to represent life sentences (Kitchens, 2010); cases not receiving a prison sentence are excluded. Consistent with prior research (Bushway and Piehl, 2001; Johnson, 2006; Johnson and Betsinger, 2008; Wooldredge, 2007), due to the positively skewed nature of federal sentences, this dissertation uses a logged measure of sentence length.

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<sup>35</sup> Because the current dissertation seeks to examine the role of *Booker* in determining the incarceration decision, the PRISDUM variable is chosen over the INOUT variable, which also measures the receipt of incarceration but also accounts for offenders' eligibility for non-prison sentences.

Judges may choose to sentence outside of the ranges prescribed by the Guidelines by utilizing one of several types of departures. Although judges have increasingly made use of sentences above what the Guidelines recommend in the post-*Booker* era (Hofer, 2007; USSC, 2006), these upward departures are extremely rare and are therefore not examined as a separate outcome.<sup>36</sup> On the other hand, two different types of downward departures are examined here. Government-initiated downward departures include substantial assistance departures (§5K1.1) as well as Early Disposition (§5K3.1) departures, both of which are initiated at the discretion of the prosecutor but which still require judicial approval. Judges may also initiate departures in those instances in which they perceive mitigating circumstances that were not embodied by the Guidelines. The presence of either government-initiated or judge-initiated departures is captured in the data by separate dummy variables.<sup>37</sup> Those cases that are not eligible for either type of downward departure (i.e., those falling within Zone A of the Sentencing Table), those receiving upward departures, and those missing departure information are excluded from the analyses examining departure outcomes.

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<sup>36</sup> Only 1% of cases sentenced between 2000 and 2008 received upward departures.

<sup>37</sup> The U.S. Sentencing Commission's coding of departures changed significantly over the nine years included in these analyses. From 2000 through 2002, departures were captured using a categorical variable, DEPART, which indicated the absence/inapplicability of a departure or the presence of either an upward departure, a downward departure, or a substantial assistance downward departure. In 2003, the DEPART variable was modified to include additional categories for departures based on the Early Disposition Program (EDP) as well as combinations of multiple departures (i.e., Substantial Assistance and Other Downward Departure; EDP and Other Downward Departure; EDP and Substantial Assistance; EDP and Substantial Assistance and Other Departure). In 2004, and prior to the *Booker* decision in 2005, departures were captured using the DEPART\_A variable; this variable largely followed the format laid out by the DEPART variable as it was used from 2000 to 2002 but this version also included a category for departures based on the EDP. Finally, following the *Booker* decision in 2005, the Commission captured deviations from the Guidelines using BOOKERCD, a twelve-category variable which provided for differentiations between within range sentences and the various upward or downward departures, with distinctions made between departures based on Guidelines reasons and those using §3553 as a justification.

### *Level 1 Independent Variables*

The primary Level 1 variable of interest is a dichotomous variable coded to indicate whether the offender's case occurred before or after the Supreme Court's ruling in *Booker*, which was decided on January 12, 2005. Because the data are collected on a fiscal basis, the *Booker* decision falls in the midst of the year 2005; thus, in addition to all of the cases sentenced in 2006 through 2008, nearly half of the cases sentenced in 2005 are classified as post-*Booker* cases.

A wide range of independent variables from the Commission's data is included to account for a host of offense- and case-related factors. First and foremost, a measure of presumptive sentence length is included; based on the offender's criminal history score and his/her current offense severity, this variable represents the starting point for judges in deciding a suitable sentence and is considered "appropriate for 'removing' variation in sentence length attributable to the guidelines, and it may be a superior predictor relative to other legal measures such as offense type/classification and prior record" (Wooldredge, 2009: 295; see also Bushway and Piehl, 2001; Engen and Gainey, 2000; Johnson, Ulmer, and Kramer, 2008; Spohn and Fornango, 2009; USSC, 2004). The presumptive sentence is based on the adjusted guidelines minimum, but it also accounts for instances in which a mandatory minimum sentence trumps the recommended sentence.<sup>38</sup> Consistent with prior

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<sup>38</sup> The presumptive guidelines length was coded to adjust for mandatory penalties that supplant the adjusted guidelines minimum. Some of these mandates were present for all years in the dataset (i.e., drug and gun minimums), while others were instituted in later years; the latter include mandatory penalties for repeat sex offenders and for offenses involving explosives, pornography, illegal sexual activity (i.e., coercion and enticement to engage in prostitution/illegal sexual activity, transportation of minors), immigration, possession of false identification documents, and those committed while on release, in addition to "other" mandatory minimums not considered in this list. In instances in which the safety valve was applied in drug cases, the presumptive guidelines length was coded as the adjusted guidelines minimum rather than the mandatory minimum (Spohn and Fornango, 2009).

research (Hofer and Blackwell, 2001; Johnson, Ulmer, and Kramer, 2008), the defendant's criminal history score is also included in the analyses. Even though criminal history is one of the two major components (along with final offense severity) that decide the presumptive sentence length, the two variables are only weakly correlated ( $r = .343$ ).

The type of offense is captured by a set of six dummy variables – violent, property, drug, fraud, firearms, and other – with property offenses serving as the omitted reference category; as is discussed in more detail below, due to their unique characteristics, cases involving immigration offenses are examined separately. Finally, detention prior to sentencing and conviction by either a bench or jury trial are each captured by dummy variables.<sup>39</sup>

Because the Commission only collects a limited amount of data on factors associated with defendants' personal backgrounds, there are many variables for which the current analyses simply cannot control (e.g., socioeconomic status, family history, etc.). Still, the data do contain information on the offender's race/ethnicity, age, sex, citizenship status, number of financial dependents, and level of educational achievement. The race of the defendant is recoded into a set of dummy variables such that whites, blacks, Hispanics, and members of other racial/ethnic groups are represented, and a separate binary variable is included for cases in which race/ethnicity is missing.<sup>40</sup> Age is provided in years, and a dummy variable is used to

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<sup>39</sup> Consistent with work by Johnson, Ulmer, and Kramer (2008), I elected not to include the variable for "acceptance of responsibility" in the analyses. As noted by those researchers, not only does the presumptive sentence length variable account for acceptance of responsibility discounts, but it is also strongly associated with mode of conviction (i.e., correlation with plea bargaining,  $r = .613$ ).

<sup>40</sup> Only 1% of offenders were indicated as being both black and Hispanic; these cases were recoded into the black dummy variable.

indicate male gender. Citizenship status is captured by a dummy variable coded so that non-citizens are equal to one, and a separate dummy variable is included to account for cases missing information on citizenship. The number of dependents is also recoded as a dummy variable such that having one or more dependents is distinguished from having no dependents; a separate dummy variable is used to account for those cases missing information on dependents. In addition, educational attainment is also represented by a series of dummy variables – less than a high school diploma, high school graduate, some college, college graduate, and education level missing – with high school graduates being the omitted reference category. Basic descriptions of relevant individual-level (“Level 1”) variables are provided in Table 2.

Finally, in those models not considering contextual effects (see below), dummy variables are included to represent each of the federal districts. Descriptive statistics for Level 1 data are provided in subsequent chapters.

### *Level 2 Independent Variables*

Given that the study period spans nine years, attention must be paid to the fact that, not only did many sentencing changes occur during that time period, but it is very likely that many contextual changes occurred as well. Accordingly, the second level of data consists of an assortment of static and, where appropriate, dynamic district-level independent variables reported for the 90 federal districts. Descriptions of these Level 2 variables are provided in Table 3. Court size is represented by the number of authorized judgeships in each federal district; because it exhibits little change over time, court size is captured by a static variable, with the number of

authorized judgeships averaged over 2000 through 2008. Historic departures are calculated by averaging the aggregate rate of departures, including upward as well as government- and judge-initiated downward departures, over time, using the Commission's data. Caseload pressure, or the total number of filings (civil, felony criminal, and supervised release hearings) per district judge, is more variable over time. Accordingly, a static measure of caseload pressure is calculated by averaging filings per judge over time, while a dynamic measure is used to account for the average percentage change in filings.

Because group threat theories point to the need to examine the way that large *and* growing racial and ethnic minority populations affect sanctions, the current study uses both static and dynamic measures to account for shifts in these groups over time. Yearly county-level population data available through the U.S. Census Bureau were aggregated to provide percentages of blacks in each federal district. Percent changes were then averaged over time.

### **Analytic Approach**

The effect of the *Booker* decision is examined in multiple ways. Because *Booker* provides a natural experiment, a quasi-experimental pre-test post-test design is employed to examine the decision's impact. First, for each outcome, models including all cases sentenced throughout the study period are explored. These "full" models include a dummy variable to indicate whether cases were sentenced in the post-*Booker* period, and they serve as a baseline for examining the effects of the decision.

Next, the results of period-specific (pre- and post-*Booker*) linear models are compared using a z-test for the equality of regression coefficients (Brame, Paternoster, Mazerolle, and Piquero, 1998), using the equation

$$z = \frac{\theta_2 - \theta_1}{\sqrt{SE\theta_2^2 + SE\theta_1^2}} \quad (5.1)$$

where  $\theta_1$  represents pre-*Booker* coefficients and  $\theta_2$  represents post-*Booker* coefficients. In the case of nonlinear regression models, because coefficients are scaled by an unknown variance, the ratios of coefficients are compared using the equation

$$\frac{\beta_2}{\beta_1} = \frac{\alpha_2/\sigma}{\alpha_1/\sigma} = \frac{\alpha_2}{\alpha_1} \quad (5.2)$$

where  $\alpha$  represents underlying coefficient and  $\sigma$  represents the standard deviation of the unobserved variation (Allison, 1999; Hoetker, 2007). Statistical significance of the ratio is determined using a Wald chi-square test, using the formula

$$\frac{(\theta_2 - \theta_1)^2}{[SE(\theta_2)]^2 + [SE(\theta_1)]^2} \quad (5.3)$$

where  $\theta_1$  and  $\theta_2$  again represent pre- and post-*Booker* coefficients (Allison, 1999).

Finally, because the more nuanced hypotheses set forth in this dissertation assert that important changes will not be apparent immediately following *Booker* but will emerge with time, each outcome is also examined using quarterly regression models. Depending on the model, quarterly odds ratios or percent changes for variables of interest are graphed so that trends may be inspected.

The approach to examining the hypothesized effects of *Booker* described above is implemented in two ways. First, individual-level models, which include

Level 1 predictors and control for district-level differences using fixed effects, are used to explore predictions concerning general changes in sentencing outcomes before and after the decision. Second, in order to investigate how the impact of the *Booker* decision may be contextualized by district-level factors, multilevel models are employed and include both Level 1 and Level 2 predictors. Each of these approaches is described in more detail below.

### *Individual-Level Models*

Hypotheses concerning general changes anticipated in the wake of the Supreme Court's decision are investigated using models which include only individual-level predictors and which control for average differences across districts using fixed effects. The incarceration decision (i.e., the likelihood of receiving a prison sentence versus being released or receiving a sentence not involving imprisonment) is modeled using logistic regression. Ordinary least squares (OLS) regression is used to model decisions concerning the length of prison sentences. The likelihood of receiving a government-initiated or judge-initiated departure (or not receiving either type of departure) is modeled using multinomial logistic regression. The equations representing these statistical models are summarized as follows:

$$X_i = \log\left(\frac{\pi_i}{1 - \pi_i}\right) = \beta_0 + \beta_j B_{ij} + \beta_k C_{ik} + \varepsilon_i \quad (5.4)$$

$$Y_i = \log(\hat{\mu}) = \beta_0 + \beta_j B_{ij} + \beta_k C_{ik} + \varepsilon_i \quad (5.5)$$

$$Z_i = \log\left(\frac{\pi_i = d}{\pi_i = D}\right) = \beta_0 + \beta_j B_{ij} + \beta_k C_{ik} + \varepsilon_i \quad (5.6)$$

where, for individual  $i$ ,  $X_i$  represents the log odds of imprisonment,  $Y_i$  represents the natural log of the months of imprisonment, and  $Z_i$  represents the log odds of receiving a  $d$  departure compared to receiving no departure,  $D$ . Across all models,  $B_{ij}$  signifies a vector of  $j$  offense and case-related factors, and  $C_{ik}$  stands for a block of fixed effects for federal districts.

### *Contextual Models*

Predictions focused on the contextualized nature of sentencing attempt to extend the research scope beyond only the individual- and case-level factors typically shown to influence sentencing decisions to consider factors that may influence these outcomes in a more nuanced or indirect way. Multilevel models are utilized to depict the nested relationship between individual cases sentenced within districts and to overcome the problems created by the dependent nature of this relationship (Johnson, 2005; Ulmer and Johnson, 2004). This dependency often results in residual errors that are “correlated within [districts], violating the OLS assumption of independent error terms and risking a misestimation of standard errors” (Ulmer and Johnson, 2004: 152). As Johnson, Ulmer, and Kramer (2008: 756) argue, “Hierarchical models correct misestimated standard errors caused by data clustering, provide properly adjusted statistical significance tests, and offer analytical advantages such as the parceling of variation across levels of analysis, the modeling of heterogeneity in regression coefficients, and the proper estimation of cross-level interaction effects.”

Whereas most regression models assume uniform effects across aggregate units, multilevel models also have the ability to model variation in both the model intercept and the slopes for specified coefficients (Field, 2009). In the present

context, this means that, rather than predicting that the *Booker* decision will have similar effects on all individuals regardless of where they are sentenced, the decision can be viewed as affecting individuals differently across districts. Accordingly, Equations 5.5-5.7 below represent the multilevel models used in the present research.

$$Y_{ij} = \beta_{0j} + \beta_{1j}B_{ij} + \varepsilon_{ij} \quad (5.7)$$

$$\beta_{0j} = \beta_0 + u_{0j} \quad (5.8)$$

$$\beta_{1j} = \beta_1 + u_{1j} \quad (5.9)$$

In Equation 5.6,  $Y_{ij}$  is used to represent each outcome of interest (i.e., incarceration, sentence length, and likelihood of departure) and  $B_{ij}$  represents a vector of  $j$  offense and case-related factors. The main difference between this equation and Equations 5.4-5.6 is that this model accounts for individual cases,  $i$ , and districts,  $j$ . Equations 5.8 and 5.9 account for random intercepts,  $\beta_{0j}$ , and random slopes,  $\beta_{1j}$ , respectively (Field, 2009).

### **Data Limitations**

Although the Commission's data provide a great deal of information about individual offender and offense characteristics, some caveats should be noted. First, the fact that the data set consists, in essence, of a large population rather than a sample means that statistical tests for significance are more sensitive, and even trivial differences are likely to appear to be significant. Although these statistical tests are reported to be consistent with previous research, the focus of the discussion of results is on substantive differences. At the same time, the statistical tests for contextual

models are based on the number of districts (n=90), so the likelihood of finding all predictors statistically significant at this level of analysis is much less.

The Commission's data may be subject to omitted variable bias because information on some potentially important variables (e.g., socioeconomic status, employment history, family history, level of evidence) is not recorded (Johnson, Ulmer, and Kramer, 2008). This may be due, in part, to the initial limitations placed on the Commission by Congress regarding the factors deemed appropriate for creating the federal sentencing guidelines. The SRA specifically precluded the Commission from considering the personal characteristics of offenders, or proxies for these characteristics, in its creation of the Guidelines. Information on other variables is simply no longer collected.<sup>41</sup> Unfortunately, these omissions prevent researchers from directly examining what role certain factors play in federal criminal sentencing. Similarly, although the Commission collects identifying information on judges for internal usage, judicial identifiers are removed from the data that are made available to researchers.

Despite these limitations, though, the Commission's data are very high quality and contain a great deal of information about individual cases sentenced at the federal level. Although some cases had to be dropped because they were missing information on key variables, this did not result in the loss of a significant number of

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<sup>41</sup> Up through 2003, the Commission collected information on the type of attorney obtained by the defendant (e.g., private attorney, public defender, etc.). As is noted in the 2004 Monitoring of Federal Criminal Sentences codebook, the variable was often not defined in presentence reports, and it was accordingly discontinued.

cases, and the current dissertation makes use of data on a large sample of federal cases.<sup>42</sup>

Findings are presented as follows. Results from the overall models are presented in Chapter VI; the findings for drug-specific models are detailed in Chapter VII; and immigration-specific results are detailed in Chapter VIII. Chapter IX provides a summary of all of the results, and Chapter X discusses policy implications of the present research and suggests direction for future research efforts.

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<sup>42</sup> Less than 8% of the cases had to be dropped from the dataset due to missing information; earlier years of data has larger proportions of cases with missing information on key variables.

## **CHAPTER VI: ALL OFFENSE TYPES (EXCLUDING IMMIGRATION)**

This chapter explores the overall impact of the Supreme Court's decision by comparing pre-*Booker* sentencing outcomes to those in the post-*Booker* era using both individual- and district-level data. Consistent with other research studies (Ulmer and Light, 2010; Ulmer, Light, and Kramer, 2011a, 2011b), the current examination excludes immigration offenses, which are noticeably different from cases involving other federal offenses. Unlike prior work, though, immigration offenses are examined separately in Chapter VIII.

### **Individual-Level Analysis**

Table 4 includes descriptive statistics for the individual-level data. Forty-three percent of the cases included in the study occurred after the *Booker* decision. Due to the large sample size, a number of statistically significant differences are evident between the two time periods, though, substantively speaking, the two groups are generally very similar, particularly with respect to demographics. At the same time, offenders sentenced after *Booker* had more extensive criminal histories and longer presumptive sentences, and they were more frequently detained prior to sentencing. While slightly smaller percentages of offenders were sentenced for violent, property, drug, and fraud offenses in the post-*Booker* period, slightly larger percentages of offenders were sentenced for cases involving firearms and other offenses following the Supreme Court's decision.

Consistent with extant research (Hofer, 2007; Ulmer, Light, and Kramer, 2011a; USSC, 2006), the descriptive statistics suggest that a significantly higher percentage of cases received imprisonment following *Booker* (87%) than in the years

prior to the decision (84%), and that the average term of imprisonment for these offenders also increased significantly, from 64 months to 72 months. In addition, the percentage of cases receiving government-initiated departures increased significantly between the two periods, from 19% to 27%, and there was a significant but negligible change in the percentage of cases receiving judge-initiated departures following *Booker*.

#### *Imprisonment and Sentence Length – Individual-Level Models*

The first hypothesis predicts that imprisonment and sentence length trends following *Booker* will largely resemble trends that existed prior to the Supreme Court's decision but that differences will emerge with time, as judges become more comfortable with breaking with the sanctions dictated by the Guidelines. These differences are predicted to differentially affect black and Hispanic offenders. Results for imprisonment and sentence length models are detailed separately below. Although models included all of the relevant variables detailed previously, tabled results are limited in the interest of space, and the discussion of results focuses primarily on hypothesized relationships between specific predictors and outcomes of interest.

#### Imprisonment

In order to assess predictions related to the imprisonment decision, three separate logistic regression models were estimated, and the results are displayed in Table 5. The first model includes all cases during the entire study period, and the results suggest that the *Booker* decision was not related to significant increases in the likelihood of imprisonment. During the same time frame, there were substantial

differences in the incarceration decision based on race/ethnicity such that, relative to whites, blacks were 4% more likely to be incarcerated, and Hispanics were 31% more likely to be incarcerated.

Predictions about the changing influence of race and ethnicity on the incarceration decision can be investigated by comparing results in the pre-and-post *Booker* models in Table 5. The results suggest that the odds of imprisonment for both blacks and Hispanics increased after the *Booker* decision. Compared to whites, blacks were 4% more likely to be incarcerated prior to the decision, and they were 6% more likely to be incarcerated in the period following the High Court's ruling. Hispanics were 27% more likely than whites to be incarcerated in the time period prior to the Supreme Court's decision, while they were 39% more likely to be incarcerated after the decision. However, the chi-square statistic in the far right column of the table shows that pre- and post-*Booker* differences in imprisonment outcomes were not statistically significant for blacks, though they were significant for Hispanics at the  $p < .05$  level.<sup>43</sup>

In order to more specifically address the question of whether deviations in sentencing trends for blacks and Hispanics are becoming more apparent with time, logistic regression models were estimated for the incarceration decision for each quarter in the data set.<sup>44</sup> To account for sudden quarterly fluctuations and to establish a clearer trend line, a three quarter moving average of the odds ratios was calculated, and the results are graphed in Figure 1. Incarceration outcomes for whites, the

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<sup>43</sup> Again, consistent with other work (Allison, 1999; Hoetker, 2007), a Wald chi-square test is used to assess the statistical difference between nonlinear regression models because coefficients are scaled by an unknown variance.

<sup>44</sup> Disaggregating the data by quarters reduced the number of cases included in the regression models. In some quarters, some districts had few or no cases and were therefore removed from the models.

omitted reference group, are represented by the thick, horizontal line at 1.0. Notably, the trend line for blacks only dropped below 1.0 once, suggesting that, for nearly all of the study period, blacks were more likely to be the recipients of imprisonment than whites. Further, the trend line for Hispanics was much higher than that of blacks for the entirety of the study period, indicating that they experienced even higher odds of incarceration.

Figure 1 not only provides more context for interpreting the results of Table 5, but it provides little evidence of a “*Booker* effect” with respect to the incarceration of either blacks or Hispanics. Although the graph suggests that imprisonment trends were slightly less stable for blacks following the decision, a separate t-test comparing the average odds of incarceration for these offenders prior to and following *Booker* reveals no statistically significant differences ( $t=.29$ ). Further, while the results shown in Table 5 indicate that Hispanics’ odds of incarceration significantly increased in the post-*Booker* period, and a separate t-test comparing the mean odds of incarceration for each time period supports this finding ( $t=2.22, p<.05$ ), the quarterly results graphed in Figure 1 suggest that their odds of incarceration actually began to increase during the PROTECT period, well before the *Booker* ruling. Finally, though the incarceration trend lines for both blacks and Hispanics were increasing in the last fiscal year of the study period, there is little prima facie evidence to suggest that *Booker* is responsible for these upward shifts, as the trends for both groups were trending downward in the preceding quarters. Additional years of data are required to investigate whether these patterns were sustained, as well as whether they are connected to *Booker* or to the *Gall* and *Kimbrough* decisions.

## Sentence Length

Given that the incarceration decision is only the first of many choices that affect offenders' overall sentences, it is necessary to additionally examine sentence length outcomes to determine whether important differences have emerged as more time has passed since the *Booker* decision. OLS models representing the sentence length decision were estimated for the entire study period as well as for both the pre- and post-*Booker* periods; due to the skewed distribution of sentence lengths, logarithmic transformations of this outcome were completed. The results for these logged sentence length models are reported in Table 6. The results of the first model, which includes all cases sentenced from 2000 through 2008, suggest that slightly longer sentences were handed down during the post-*Booker* period. However, during this time frame, sentence lengths were slightly shorter for both blacks and Hispanics relative to their white counterparts.

Comparing the coefficients for minority groups across the pre- and post-*Booker* models suggests very little change over time. Although blacks were the recipients of significantly shorter sentences than whites during both periods, sentences for Hispanics were not significantly different from those of whites during either time frame. However, the z-scores provided in the far right column of Table 6 indicate significant changes between the two time periods such that both blacks and

Hispanics received slightly shorter sentences in the post-*Booker* period.<sup>45</sup>

Accordingly, it is instructive to examine sentence lengths for cases on a quarterly basis to investigate when shifting patterns emerged. As such, a three quarter moving average was calculated, and quarterly percent differences for these two groups are graphed in Figure 2. Consistent with the findings already discussed, this graph offers no evidence that sentence lengths for black and Hispanic offenders became noticeably more punitive after the *Booker* decision. Relative to whites, sentence lengths for these two groups began to decline prior to the PROTECT Act. Although these trends appear to have changed substantially over time, sentence lengths were very similar for blacks, Hispanics, and whites by the end of the study period. Moreover, t-tests for both groups confirm that mean percentage differences in sentence length were not significantly different prior to and following the Supreme Court's decision ( $t=.65$  for blacks;  $t=.59$  for Hispanics). Additionally, it is important to emphasize the modest magnitude of the effect sizes in Figure 2. At no point during the study period were sentence lengths greater than 3% longer for Hispanic and black defendants than for whites, and at no point were differences favoring these groups larger than 5% and 7% respectively.

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<sup>45</sup> Alternative models were estimated to separately account for the presumptive guidelines minimum and the application of any mandatory minimum. Although the model for the full study period indicated that significantly longer sentences were handed down post-*Booker*, the coefficient for Hispanics was non-significant. Separately estimated regression models for the pre- and post-*Booker* periods indicated that, though Hispanics received significantly longer sentences in the pre-*Booker* period, their sentences were not significantly different from those of whites in the post-*Booker* period. The application of mandatory minimums was non-significant in the post-*Booker* model but was significant in the models for the pre-*Booker* period and for the overall time period. Other findings were not substantively different from those presented in the text.

### *Departures – Individual-Level Models*

Disparities between whites and minorities in the receipt of both government- and judge-initiated departures are predicted to become more apparent as more time has passed since the *Booker* decision. In order to assess these predictions, multinomial logistic regression models were estimated to represent the decision to use either government-initiated or judge-initiated downward departures (relative to using no departure). The results for the first model, which includes all eligible cases sentenced throughout the entire study period, are presented in Table 7. These results indicate that the *Booker* decision is associated with positive and significant increases in the likelihood of receiving both types of departures. After the Supreme Court's decision, offenders were 79% more likely to receive government-initiated departures, and they were 22% more likely to receive discounts initiated by judges on average. The results for this model also indicate that, during the nine years included in the study period, both blacks and Hispanics were less likely to be the recipients of downward departures, regardless of type of departure.

Period specific multinomial logistic regression models, representing the pre- and post-*Booker* eras, were also estimated, and the results are presented in Table 8. These results indicate that, both before and after the Supreme Court's decision, blacks and Hispanics were significantly less likely to benefit from both government- and judge-initiated departures. Changes between the two time periods are significant for Hispanics only for government-initiated departures, while significant changes are evident for blacks over time only for judge-initiated departures. These results suggest that, though these groups were still less likely than whites to receive these departures,

their odds of receiving these respective sentencing discounts improved in the period following *Booker*.<sup>46</sup>

In order to further investigate racial disparity in the pre-and-post *Booker* eras, quarterly multinomial logistic regression models were examined. Moving averages of odds ratios over three quarters were calculated for blacks and Hispanics, and the results are graphed in Figure 3 for government-initiated departures and in Figure 4 for judge-initiated departures. As is most apparent in Figure 3, the odds of receiving government-initiated departures rose dramatically during the PROTECT Act period and peaked for both blacks and Hispanics right before the Supreme Court's decision; for Hispanics, this increase was almost to the point of parity with whites. The odds of receiving a government-initiated departure dropped for both groups after the decision, and declining odds of these departures continued through the remainder of the study period. T-tests respectively comparing the mean odds of receiving a government-initiated departure for black and Hispanic offenders prior to and following *Booker* reveals no statistically significant differences ( $t=.38$  for blacks;  $t=0$  for Hispanics).

Trends for judge-initiated departures, presented in Figure 4, are more suggestive of a possible *Booker* influence. From the beginning of the study period through the passage of the PROTECT Act, Hispanics were more likely than whites

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<sup>46</sup> Additional analyses examined the extent to which the magnitudes of both government- and judge-initiated departures were influenced by the *Booker* decision. Contrary to expectations that judges would use longer departures as a vehicle for providing what are believed to be more just and appropriate sentences, the magnitude of government-initiated departures did not significantly differ prior to and following the Supreme Court's decision, and the magnitudes of judge-initiated departures were significantly *shorter* in the post-*Booker* era. Period-specific models indicated that the magnitude of government-initiated departures were significantly shorter for Hispanics in the post-*Booker* era, though graphing the quarterly percent differences suggested that disparities between Hispanics and whites were improving somewhat after the decision. Both blacks and Hispanics were found to have received significantly shorter judge-initiated departures following *Booker*, but graphing the quarterly percent differences in this outcome suggests that departure lengths for these groups began declining in the PROTECT Act period, prior to the Supreme Court's decision.

(represented by the horizontal line at 1.0) to be the recipients of departures brought forth by judges. However, by the end of the PROTECT Act period, their odds of receiving these departures had dropped dramatically and were, in fact, lower than both their black and white counterparts. For black offenders, the odds of judge-initiated departures hovered below .8 prior to the *Booker* decision. After the Supreme Court's ruling, the likelihood of receiving these departures gradually increased for both blacks and Hispanics, peaking near the end of fiscal year 2007, right before both *Gall* and *Kimbrough* were decided. T-tests confirmed that the mean odds of receiving a judge-initiated departure increased significantly for black and Hispanic offenders following the *Booker* decision ( $t=3.60$ ,  $p<.01$  for blacks;  $t=2.84$ ,  $p<.01$  for Hispanics). It is worth noting that the trends for blacks and Hispanics much more closely mirrored each other after the *Booker* decision, but the odds of departure for neither group approached a level on par with those of whites.

To summarize, the overall regression results for both government- and judge-initiated departures lend little credibility to the notion that *Booker* has created an environment in which sentencing discounts are increasingly being used for whites rather than for racial and ethnic minorities. While whites were significantly more likely to benefit from departures in both time periods, regardless of who initiated them, the odds of receiving government-initiated departures increased for Hispanic offenders by 4% on average, and the odds of receiving judge-initiated departures increased for black defendants by 8% on average, following the *Booker* decision. Examination of the quarterly trends additionally suggests that the odds of receiving judge-initiated departures for minority offenders have been steadily increasing since

the *Booker* decision, though they still remained well below the level of whites at the end of the study period.

### **Multi-Level Analysis**

This section explores the role of contextual factors in each of the sentencing outcomes described above. Descriptive statistics for district-level variables are provided in Table 9. During the study period, an average district in the sample had 7 judges, with approximately 463 filings per judge each year; average caseloads declined slightly between the periods prior to (468 cases) and following (457 cases) the *Booker* decision. Averaged across the entire study period, case filings increased roughly 4%, but they increased nearly 11% when only the pre-*Booker* years are considered, and they declined by slightly more than one-half of one percent during the post-*Booker* period. District-level departure rates hovered around 34% during both the pre- and post-*Booker* periods. Although the average district-level racial composition was slightly less than 10% across the years in the study period, the district-level percentage increase in the black population was 91%, averaged across all of the years in the study period; the racial composition increased more substantially during the pre- than post-*Booker* period.

In general, inter-district variation was predicted to increase over time as *Booker* allowed judges to shape their decisions to be consistent with the desires of the communities in which they were embedded. This prediction can be assessed by comparing the unconditional models for the pre- and post-*Booker* periods for each outcome (Table 10). Consistent with the findings of Ulmer, Light, and Kramer (2011a), these results provide little evidence to support the notion that *Booker* has

contributed to decreased uniformity in the application of the Guidelines across federal districts.<sup>47</sup> In both the pre- and post-*Booker* periods, districts accounted for approximately 6-7% of the variance in incarceration outcomes (6.5% versus 6.1%), and about 5% of the variance in sentence length outcomes (5.1% versus 4.6%). Although the proportion of the variance accounted for by districts in the likelihood of receiving either government- or judge-initiated departures was comparatively more substantial, changes between the two time periods were modest (9.2% versus 12.6% for government-initiated departures; 11.2% versus 10.0% for judge-initiated departures).

Table 11 summarizes the findings for the district-level predictors for each sentencing outcome;<sup>48</sup> individual-level predictors are excluded from this table because the findings were not substantively different from the individual-level models previously presented. Judges in districts characterized by either heavy or increasing caseload pressure were predicted to sentence less punitively, and the impact of caseload pressure was hypothesized to be more evident in the post-*Booker* period. The only result consistent with these expectations was the finding that, in the post-*Booker* period, judges in districts in which caseload pressure was increasing were significantly more inclined to grant judge-initiated departures. Contrary to expectations, heavy caseloads were significantly and positively related to the likelihood of receiving government-initiated departures during the period prior to

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<sup>47</sup> Separate analyses also indicate that the pre-to-post change in the proportion of variance accounted for by districts in the magnitude of government-initiated departures was negligible (8.6% versus 11.1%). Although the change in district-level variance for the judge-initiated departures was more substantial (8.4% versus 2.1%), this result contradicts expectations by suggesting that districts were actually becoming *more* uniform in the lengths of these departures after the Supreme Court's decision.

<sup>48</sup> A correlation matrix for district-level predictors is found in Appendix B.

*Booker* but not during the period following the Supreme Court’s decision. Caseload pressure was significantly related to judge-initiated departures following the Supreme Court’s decision, but in the opposite of the predicted direction, such that judges in districts characterized by heavy caseloads were significantly *less* likely to grant these departures.

Historical district-level rates of departure were predicted to influence the likelihood of granting departures in the post-*Booker* era. As is apparent in Table 11, districts with higher historical departure rates were more likely to grant both government- and judge-initiated departures both prior to *and* following the Supreme Court’s decision in *Booker*. Historical departures rates were also related to significantly shorter average sentences in both time periods.<sup>49</sup>

Finally, it was hypothesized that less favorable sentencing outcomes would be handed down in districts in which blacks constituted either a sizeable or growing percentage of the population. Further, the influence of racial composition and growth was predicted to be more influential on sentencing outcomes in the period following the *Booker* decision. As is shown in Table 11, these predictions were met with limited support. Consistent with expectations, judges in districts with large black populations were less likely to grant judge-initiated departures in the post-*Booker* period; in addition, districts with *growing* black populations were less likely to grant government-initiated departures after the Supreme Court’s decision. More

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<sup>49</sup> Separate analyses also revealed that judges in districts with higher historical departure rates granted significantly longer departures, regardless of who initiated them.

equivocally, the likelihood of incarceration was higher in districts with a higher concentration of black residents during both time periods.<sup>50</sup>

## Summary

The results presented in this chapter do not offer much evidence in support of the notion that *Booker* has directly altered the sentencing landscape (Table 12).

Though the post-*Booker* period was characterized by significantly longer sentences and increased odds of receiving either a government- or a judge-initiated departure, evidence that *Booker* directly contributed to these outcomes is lacking. This point is underscored when the focus is placed on racial/ethnic disparities. Though significant pre-post differences are sometimes evident, the role of *Booker* is less apparent when quarterly trends are examined.

The next chapter examines sentencing outcomes as they specifically pertain to drug offenses. Particular attention is paid to *Booker*'s influence in outcomes for crack and powder cocaine offenses, which have historically been the source of much disparity and controversy.

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<sup>50</sup> Although the pre-*Booker* period was associated with shorter sentence lengths for districts with growing black populations and increased odds of government-initiated departures in districts in which blacks constituted a sizeable proportion of the population, neither of these findings was significant in the post-*Booker* period.

## CHAPTER VII: DRUG OFFENSES

Separate models are used to examine hypotheses specifically dealing with the sentencing differences between different types of drug offenses. As detailed in previous chapters, the *Booker* decision is anticipated to differentially affect the sentencing patterns for crack and powder cocaine offenders in particular.

Accordingly, this chapter pays specific attention to the treatment of these offenders. Both individual- and district-level effects on sentencing outcomes for these offenses are explored.

### **Individual-Level Analysis**

The drug-specific models presented in this chapter use the same dependent variables and most of the same independent variables as the overall models presented in the previous chapter. However, the data set is limited to include only those offenders sentenced for drug offenses. Therefore, instead of general offense type dummies, the current models include two sets of dummy variables specific to drug offenses. First, a group of drug type variables is included to distinguish between cases involving powder cocaine, crack cocaine, amphetamines, opiates, psychedelics, other drug or paraphernalia offenses, and cases in which multiple types of drugs are present.<sup>51</sup> These cases are compared to those involving marijuana or hashish, the omitted reference category. An additional dummy variable is included to account for those cases identified as drug offenses but for which no drug type was listed in the data. Second, a group of dummies is included to distinguish between different types

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<sup>51</sup>A complete breakdown of the drugs included in each of these categories is included in Appendix C.

of drug offending; these variables represent trafficking, communication facility,<sup>52</sup> and possession (the omitted reference category).

The descriptive statistics for the drug cases examined in this chapter are presented in Table 13. Independent samples t-tests were used to test for significant differences between pre- and post-*Booker* cases, and statistical significance at the  $p < .01$  value is indicated on the far right hand side of the table. It bears repeating that the large sample sizes mean that tests of statistical significance are very sensitive to even small differences between samples, so the discussion of results focuses on substantively meaningful differences between samples.

Although the patterns of descriptive statistics for drug offenders largely mirror those of the broader federal criminal offender population, imprisonment and sentence length figures for drug offenders are much higher. As reported in the previous chapter, 85% of the federal offenders (including drug offenders) sentenced between 2000 and 2008 were imprisoned, while Table 13 indicates that 95% of drug offenders were sent to prison during the same period. Additionally, while the broader group of offenders sentenced during the study period were sentenced to an average of 68 months in prison, drug offenders were sentenced to an average of 79 months.

Although Table 13 indicates significant differences in most of the sentencing outcomes examined, the difference between pre-*Booker* and post-*Booker* incarceration for drug offenders is slight (94% versus 96%). More substantial differences are apparent in sentence lengths for drug offenders, from 76 months prior to *Booker* to 83 months afterward. Yet the percentage of cases receiving departures

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<sup>52</sup> Communications facility is the use of means of communication, such as telephones, to commit a drug offense.

also increased after the decision from 14% to 18% for government-initiated departures, and from 14% to 19% for judge-initiated departures.

With the exception of a declining percentage of cases involving marijuana/hashish after *Booker*, few substantial pre-post differences are evident with respect to drug types. The types of drug offenses sentenced in the federal system similarly appear to be relatively unchanged in the time after the Supreme Court's decision; both before and after *Booker* was decided, federal drug cases were dominated by trafficking offenses, while offenses involving both possession and communications facilities comprised only 4% of cases combined.

Compared to the broader sample of cases examined in the previous chapter, cases involving drug offenses are comprised of more Hispanics (38% compared 25%). Additionally, drug offenders are slightly younger (33 versus 35 years old), more likely to be non-citizens (29% versus 20%), and are less educated (48% have no high school diploma compared to 39%) than federal offenders more generally. Still, although significant pre-to-post *Booker* differences are apparent for the demographic characteristics of drug offenders, these differences are relatively small. On the other hand, notable pre-to-post differences for drug offenders are evident with respect to criminal history (2.17 versus 2.32), presumptive guideline length (88.39 versus 97.95), and presentence detention (71% versus 77%).

#### *Imprisonment and Sentence Length – Individual-Level Models*

Sentencing trends for crack and powder cocaine offenders are predicted to become more comparable as more time passes in the new sentencing era, as judges are hypothesized to use the freedom afforded by *Booker* and subsequent Supreme

Court decisions to deviate from the particularly severe sentences prescribed by the Guidelines for these offenses. In addition, sentencing outcomes for white and minority drug offenders are hypothesized to become less disparate in the wake of *Booker* (and subsequently *Kimbrough*). As in the previous chapter, results for imprisonment and sentence length models are detailed separately, and the discussion of results focuses primarily on hypothesized relationships between specific predictors and outcomes of interest.

### Imprisonment

The left column of Table 14 displays the findings of the logistic regression model for the imprisonment decision for drug cases over the full study period. These results indicate that drug offenders sentenced after the *Booker* decision were 22% more likely to be imprisoned. Relative to marijuana/hashish offenders, crack cocaine offenders were 43% more likely to be imprisoned, while powder cocaine offenders were 34% more likely to be imprisoned. Drug offenders sentenced for trafficking offenses were 11.36 times more likely to be imprisoned than those sentenced for possession offenses. Compared to their white counterparts, black offenders were 19% more likely, and Hispanics were 48% more likely, to be imprisoned during the study period.

Separate pre- and post-*Booker* logistic regression models for drug cases, also presented in Table 14, indicate that offenders charged with powder cocaine offenses were significantly more likely than those whose cases involved marijuana/hashish to be incarcerated in both time periods. At the same time, while crack offenders were significantly (1.52 times, or 52%) more likely to face incarceration than

marijuana/hashish offenders during the pre-*Booker* period, differences between these two groups were non-significant in the post-*Booker* era. Importantly, the 33% reduction in the coefficient for crack between the two periods was significant, while the 9% reduction in the coefficient for power cocaine offenses was not, as indicated by the chi-square statistic in the right column of the table. Similarly, the 18% reduction in the coefficient for blacks was significant, while the change in the coefficients for Hispanics was non-significant.

To further examine how sentencing trends for drug offenders have changed over time, separate logistic regression models were estimated for each of the quarters in the dataset. Three quarter moving averages of the odds ratios were calculated, and the results for blacks and Hispanics are graphed in Figure 5. Dividing the graph into pre- and post-*Booker* periods suggests that both blacks and Hispanics were subject to higher odds of incarceration than whites (represented by the horizontal line at 1.0) throughout most of the pre-*Booker* period. On the other hand, the post-*Booker* era was generally characterized by a continued decline in the odds of incarceration for Hispanics, while the odds of incarceration for blacks hovered below those for whites for most of the period. T-tests comparing the mean odds of incarceration before and after the *Booker* decision indicate that differences were significant for blacks ( $t=2.35$ ,  $p<.05$ ) but not for Hispanics ( $t=.44$ ). However, the graph suggests that disparities between whites and minorities were most pronounced during the period between the passage of the PROTECT Act and the Supreme Court's decision, and declining odds of incarceration for both groups began during this period and were continued in the period following *Booker*.

Because offenders sentenced for crack and powder cocaine offenses were hypothesized to be particularly affected by *Booker*, Figure 6 graphs the quarterly trends for crack offenses. For purposes of these quarterly analyses only, powder cocaine offenses were used as the omitted reference group and are, accordingly, represented by the horizontal line at 1.0. Evidence offered from this graph is not strongly supportive of a “*Booker* effect.” A t-test comparing the mean odds of incarceration for crack offenders during the pre- versus the post-*Booker* era confirms no statistically significant difference ( $t=1.21$ ). Consistent with prior research, the graph shows that crack cocaine offenders were overwhelmingly more likely to experience incarceration than offenders sentenced for powder cocaine offenses, with a peak occurring in the pre-PROTECT period. Although another, lesser peak occurred around the time of the *Booker* decision, the back-and-forth, up-and-down trend that occurred after the decision appears to be a continuation of an already ongoing pattern.

#### Sentence Length

Sentence length trends for drug offenders are similarly hypothesized to be significantly affected by the Supreme Court’s decision in *Booker*. The findings for the first OLS model in Table 15, including all drug offenders sentenced between 2000 and 2008, indicate that sentence lengths were 1% greater for drug offenders sentenced after *Booker*. Relative to offenders sentenced for marijuana/hashish offenses, only offenders sentenced for other drug types and paraphernalia experienced significantly shorter sentence lengths during the study period; the sentence lengths for all other drug types were greater than those for marijuana/hashish. Most relevant to the current study, crack offenders experienced sentences that were 57% greater, while

powder cocaine offenders experienced sentences that were 54% greater. Contrasted with white drug offenders, sentence lengths for blacks were 3% greater, and terms for Hispanic offenders were 4% greater.

The impact of the Supreme Court's decision on sentence lengths for drug offenders is further explored through separate OLS regression models for the pre-*Booker* and post-*Booker* periods, also presented in Table 15. For both periods, the results closely mirror those found for the full study period. Both blacks and Hispanics received longer sentences than whites during both periods, although the z-statistic on the right side of the table indicates that the increase between the two time periods was significant only for blacks. Again, consistent with the results of the full model, most drug types (except for those in the other/paraphernalia category) were sentenced more harshly than marijuana/hashish. Yet, with the exception of psychedelics, z-scores on the right side of the table do not suggest that significant changes in the individual drug types occurred between the two times periods.

Figure 7 graphs the three quarter moving average of the quarterly percent difference in sentence lengths (logged) for black and Hispanic drug offenders. This graph shows that sentence length trends for these two groups tracked together fairly closely throughout the entire study period, and, for nearly all of this time, Hispanics experienced the longest sentences. Sentence lengths for both groups began decreasing in the middle of 2000 and reached their lowest points in 2004, in the middle of the PROTECT Act period. Sentence lengths for both groups increased thereafter. By the end of the study period, both groups continued to receive more severe sentences for drug offenses than their white counterparts. Although t-tests

comparing the average percent change in sentence lengths prior to and following *Booker* indicate significant differences for blacks ( $t=2.87$ ,  $p<.05$ ) but not Hispanics ( $t=.62$ ), the graph is suggestive that shifts in sentencing trends began occurring during the PROTECT Act period rather than following the *Booker* decision.

Perhaps more interestingly, the quarterly percent difference in logged sentence lengths for crack relative to powder-cocaine is graphed in Figure 8. This graph shows that, prior to the Supreme Court's decision, crack offenders typically met with stiffer penalties than did offenders who were sentenced for powder cocaine offenses (represented by the horizontal line at 0). Pre-*Booker* crack sentence lengths dropped at the beginning of the study period but rose again to a smaller peak around the beginning of 2004, during the PROTECT Act period. After the *Booker* decision, sentences for crack offenses exhibited a marked decrease, falling to roughly the same level as powder cocaine sentences in the midst of 2006. The sentence lengths for crack offenses peaked again, at around 4% higher than those of powder cocaine offenses, near the middle of 2007, and they fell in nearly every quarter thereafter; in fact, sentence lengths for crack near the end of the study period fell to roughly 6% below those of powder cocaine. Notably, a t-test comparing the average percent change for the pre- and post-*Booker* periods indicates that the difference between the two is significant. However, it should be stressed once again that this analysis is not causal and that other factors may have influenced the trends. For instance, the Commission revised its Guidelines in 2007 (prior to the *Kimbrough* and *Gall*

decisions) to lower the base offense level by two levels for crack cocaine offenses.<sup>53</sup> Thus, further investigation is warranted.

#### *Departures – Individual-Level Models*

Departures from the Guidelines have traditionally provided an avenue for lessening the severity of sentences, and departures for providing substantial assistance to prosecutors have been frequently used in drug cases in particular. It is hypothesized that, in the post-*Booker* era, both government- and judge-initiated departures have provided a way to mitigate historical sentencing disparities, particularly for crack and powder cocaine. In addition, disparities in the odds of departure for white and minority drug offenders are hypothesized to become less evident in the post-*Booker* period.

To investigate these predictions, a multinomial logistic regression model representing the decision to depart downwardly from the Guidelines (relative to not departing) was estimated for drug cases for the entire study period, and the results are presented in Table 16. The findings in the left column suggest that, controlling for other important factors, the likelihood of government-initiated departures increased by 70% in the post-*Booker* era. Drug offenders who took their cases to trial were much less likely to receive these departures, as were offenders who were detained prior to sentencing; males, blacks, Hispanics, offenders with no high school diplomas, and those with longer criminal histories were also less likely to receive departures initiated by prosecutors. While departures of this type were no more likely for crack cocaine than for marijuana/hashish offenses, offenders sentenced for powder cocaine

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<sup>53</sup> Because the Fair Sentencing Act was not passed until 2010, it cannot explain sentencing patterns discussed in this dissertation.

offenses were 1.31 times more likely to receive these discounts during the full study period.<sup>54</sup>

Departures initiated by judges are of particular interest, as the *Booker* decision afforded them wider latitude to deviate from the Guidelines. As is shown in the right column of Table 16, offenders sentenced after the Supreme Court's ruling were 82% more likely to receive judge-initiated departures. Blacks and Hispanics were both significantly less likely than white drug offenders to be the recipients of these departures during the full study period, as were males, non-citizens, those without high school diplomas, those with more extensive criminal histories, those who took their cases to trial, and those detained prior to sentencing. At the same time, none of the specific drug categories were significantly more or less likely than marijuana/hashish to receive judge-initiated departures during the study period.<sup>55</sup>

To further disentangle the role of *Booker* in departure outcomes for drug offenders, separate multinomial logistic regression models for the departure decision were estimated for the pre- and post-*Booker* periods. The results for are displayed in Table 17. During both periods, offenders sentenced for powder cocaine experienced significantly higher odds of receiving government-initiated departures than those

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<sup>54</sup> Separate analyses found that the *Booker* decision was associated with significantly shorter government-initiated departures for drug offenders. Though departure lengths for both black and Hispanic drug offenders were slightly shorter in the post-*Booker* era, changes over time were non-significant. Significant changes between the pre- and post-*Booker* period were evident with respect to government-initiated departure lengths for crack cocaine offenders; prior to the decision, these offenders received departures that were 15% larger than those of marijuana/hashish offenders, while their departures were 24% larger in the period following the decision. However, graphing the three quarter moving averages of percentage changes for crack versus powder cocaine offenders suggested that post-*Booker* trends were initiated prior to the decision, at the start of the PROTECT Act.

<sup>55</sup> Separate analyses indicated that the magnitude of judge-initiated departures decreased for drug offenders after the Supreme Court's decision. In addition, the magnitude of these departures decreased significantly in the post-*Booker* period for Hispanics. Significant reductions in departure lengths between the two time periods were also evident for powder cocaine offenders; changes in the magnitude of judge-initiated departures for crack cocaine offenders were not statistically significant.

sentenced for marijuana/hashish. Yet, significant differences between time periods, as indicated by the chi-square statistic, were not evident for any drug type. During both time periods, both blacks and Hispanics were significantly less likely than whites to be the recipients of departures initiated by prosecutors, but differences between the two time periods were statistically significant only for Hispanics; Hispanics were 18% less likely to receive government-initiated departures prior to the decision, but their odds of departure improved such that they were approximately 9% less likely than whites to receive these departures in the period after the decision.

With respect to judge-initiated departures, the results in Table 17 indicate that both black and Hispanic drug offenders were significantly less likely than whites to receive departures before and after the decision, and the change over time was non-significant.

Quarterly multinomial logistic regression models of the departure decision were also estimated, and the three quarter moving average of the odds ratios were calculated. The results for government-initiated departures for black and Hispanic drug offenders (relative to whites) are graphed in Figure 9. This graph is not particularly suggestive of a “*Booker* effect,” despite the fact that t-tests comparing the average odds of receiving these departures before and after the decision indicate significant differences for Hispanics ( $t=1.97$ ,  $p<.05$ ) but not for black offenders ( $t=1.35$ ). Instead, the graph shows that both groups were generally less likely to receive government-initiated departures than their white counterparts throughout the study period. Although the odds of departure for blacks appeared to rise during the period following *Booker*, the trend line suggests that these offenders’ odds of

departure dropped during the PROTECT Act period and rebounded thereafter. For Hispanics, higher post-*Booker* odds of departure appear to fit with an ongoing, increasing pattern.

Quarterly odds ratios for judge-initiated departures by race/ethnicity are displayed in Figure 10. This graph suggests that departure trends for blacks and Hispanics tracked each other more closely in the post-*Booker* era than they did prior to the Supreme Court's decision. T-tests comparing the average odds of departure for the two periods indicate that differences were significant for blacks ( $t=2.45$ ,  $p<.05$ ) but not for Hispanics ( $t=.90$ ). However, the graph suggests that trends for both groups appeared to shift in the year prior to the *Booker* decision, in the midst of the PROTECT Act era. From that point onward, blacks and Hispanics were generally less likely to be the recipients of departures, relative to white offenders.

Separate graphs focusing on quarterly government- and judge-initiated departures for crack offenders (relative to powder cocaine offenders, the omitted reference group) also fail to provide convincing evidence that the Supreme Court's decision has substantially altered these sentencing outcomes. Figure 11 suggests that the odds of receiving a government-initiated departure spiked for crack offenders in the middle of the PROTECT Act period and dropped dramatically in the two quarters leading up to the *Booker* decision before briefly raising again. After *Booker*, the odds of departure for crack offenders continued to trend downward and reached their lowest point in the quarter that *Kimbrough* and *Gall* were decided before, notably, beginning to trend upward thereafter. A t-test comparing the mean odds of departure

for crack offenders indicates that the odds were not significantly different between the pre- and post-*Booker* periods ( $t=1.26$ ).

The quarterly trends for judge-initiated departures are graphed in Figure 12 and exhibit more variation than those of government-initiated departures. Still, this graph fails to provide substantial evidence of a “*Booker* effect,” and a t-test comparing the mean odds of crack offenders receiving judge-initiated departures confirms no significant difference between the pre- and post-*Booker* period ( $t=.60$ ). Although the odds of departure were generally much higher in the pre-PROTECT period, they dropped off dramatically immediately following the passage of the PROTECT Act. Beginning in the quarter prior to the Supreme Court’s decision in *Booker*, trends reversed again to briefly favor crack offenders, but this trend leveled out around the time the Commission revised its Guidelines in 2007, with both groups appearing to receive comparable treatment. Additional years of data are needed to confirm whether or not these trends have continued since the end of the study period.

### **Multi-Level Analysis**

The next set of models examines the impact of district-level characteristics on sentencing outcomes in drug cases. District-level descriptive statistics are presented in Table 18 and are largely the same as those presented in the previous chapter. However, relative drug caseload specifically is substituted for caseload pressure.<sup>56</sup> Roughly two-fifths (40%) of cases sentenced in these districts were drug cases; however, the percentage of drug cases varied across districts from a low of 20% to a high of 65%. The percentage of district-level drug cases remained relatively stable

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<sup>56</sup> Relative drug caseload represents the proportion of district-level drug cases, averaged over the relevant number of years (i.e., pre-*Booker* period, post-*Booker* period).

between the pre-*Booker* (40%) and post-*Booker* (39%) periods; over the course of the study period, relative drug caseloads across districts actually decreased by less than 3%, though some districts experienced reductions in their caseloads as big as 48%, while others experienced increases as large as 33%.

Unconditional models are presented in Table 19 and confirm that substantial district-level variation was present both before and after the *Booker* decision.

However, similar to the results presented in Chapter VI, these findings suggest little change over time in the proportion of variance in sentencing outcomes explained by districts.<sup>57</sup>

Judges in districts characterized by heavy or increasing relative drug caseloads were predicted to grant more favorable sentencing outcomes in the post-*Booker* period. The findings in Table 20 can be used to assess these predictions; again, individual-level predictors are not tabled due to their substantive similarity with the individual-level models previously examined. Contrary to expectations, the results indicate that heavier relative drug caseloads were not significantly related to any of the outcomes examined. Changes in relative drug caseload pressure were significantly related to departure outcomes in select time periods; however, these findings were again opposite of expectations, as increases in relative drug caseload pressure in the post-*Booker* period were not significantly related to more favorable outcomes in any of the models examined.

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<sup>57</sup> Separate analyses revealed that the proportion of variance explained by districts changed most substantially with respect to the magnitude of judge-initiated departures. Prior to the decision, districts accounted for 12% of the variances in judge-initiated departure lengths; after the decision, districts accounted for only 4% of the variance in this outcome.

## Summary

In summary, there is very little evidence to suggest that *Booker* has greatly altered the sentencing landscape for drug offenses (Table 21). Rather, the majority of the findings suggest that outcomes evident in the post-*Booker* period were initiated well before the decision, beginning in the PROTECT Act period. Further, because other changes have been enacted with respect to federal drug sentencing, particularly around 2007 and 2008, it is difficult to isolate *Booker*'s influence on outcomes for drug offenders. Finally, district-level drug caseloads were less influential in sentencing outcomes for drug offenders than was hypothesized.

The next chapter examines sentencing outcomes for immigration offenses.

## CHAPTER VIII: IMMIGRATION OFFENSES

Because they are substantially different from other cases, immigration cases are examined separately. Although the models used to examine these cases are substantively the same as those presented in previous chapters, important model specification differences are discussed in detail below, followed by a discussion of the results.

### **Individual-Level Analysis**

The data set used for the analyses presented in this chapter is limited to only those offenders sentenced for immigration offenses and, accordingly, excludes general offense type dummy variables. Because 80% of offenders sentenced for immigration offenses were Hispanic, the analyses included in this chapter utilize binary comparisons of Hispanics to their non-Hispanic counterparts. Similarly, because two-thirds (67%) of this sample did not graduate from high school and only a small minority attained more than a high school diploma (4% had some college, and approximately 1% graduated from college), controls for education are limited to binary comparisons of those who did not finish high school to those with high school diplomas or more. A dummy variable was included to differentiate border districts from non-border districts; 73% of immigration cases sentenced during the study period were from border districts.<sup>58</sup>

Additional descriptive statistics for the immigration cases examined in this chapter are displayed in Table 22. Pre-and post-*Booker* differences were assessed using independent samples t-tests, and significant differences are indicated in the far

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<sup>58</sup> Because a large number of border districts are also districts that participate in Early Disposition or Fast Track programs, the analyses could not control for both fast track districts and border districts due to high correlation.

right column of the table. Between 2000 and 2008, 96% of federal immigration offenders were incarcerated, and these offenders spent an average of nearly 25 months in prison. A significant decrease in sentence length occurred between the pre-*Booker* period, when offenders spent an average of 27 months in prison, to the post-*Booker* period, when offenders were sentenced to an average of 23 months. Even more dramatic changes between the two time periods are evident with respect to the percentage of immigration cases receiving departures. The percentage of cases receiving government-initiated downward departures increased from 12% prior to the decision to 33% in the period after the decision. The opposite trend was true for judge-initiated departures; 26% of immigration cases in the sample received these departures in the pre-*Booker* period, while only 10% of these cases received these departures in the years following the decision. Notably, the Commission did not begin capturing “fast track” departures as a category of departure until 2003, which likely explains some of the shifts in the use of these departures, and the increase in government-initiated departures in particular, over time.

As noted above, the majority of offenders (80%) in the immigration sample were Hispanic, but their representation did decline significantly over time, from 84% prior to *Booker* to 77% following the Supreme Court’s decision.<sup>59</sup> Average criminal histories of immigration offenders also dropped significantly between the two time periods, from 3.01 prior to the Supreme Court’s ruling to 2.76 after the decision. The presumptive guidelines recommendation exhibited a similar decline over time, from

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<sup>59</sup> The percentage of white immigration offenders increased from 6% to 7% between the pre- and post-*Booker* periods, while the percentage of immigration offenders classified as being of another race/ethnicity (including Asians) increased from 2% to 7%. The percentage of immigration offenders whose race/ethnicity was missing in the data also increased in the post-*Booker* period (6%, compared to 4% pre-*Booker*).

28 to 23 months. Other individual-level characteristics exhibited more stability over time. The average age of immigration offenders during the study period was roughly 33 years old, and the majority of these offenders (93%) were male. Interestingly, only 88% of immigration offenders were classified as non-citizens in the data, while the citizenship status was missing for an additional 1% of offenders.

Due to the relatively small body of literature on federal sentencing for immigration offenses, particularly with respect to the *Booker* decision, the hypotheses examined in this chapter are necessarily exploratory and broad. It is predicted that immigration sentencing will exhibit differences in the post-*Booker* period and that disparities between Hispanics and non-Hispanics will be most apparent in the years following the decision.

Because nearly all immigration offenders were imprisoned both prior to and following the *Booker* decision, the incarceration outcome lacks sufficient variation to justify examination here. Accordingly, hypotheses specific to immigration offenses are explored first via models estimating the impact of the *Booker* decision on sentence length.

#### *Sentence Length – Individual-Level Models*

Similar to previous chapters, the effect of *Booker* on sentence lengths for immigration offenses is assessed using three separate OLS regression models, and logarithmic transformations were completed to account for the skewed nature of sentence lengths. The first set of results in Table 23 indicate that cases sentenced after the decision received significantly shorter prison terms than those sentenced

before the Supreme Court's ruling. Relative to non-Hispanics, sentences for Hispanic immigration offenders were nearly one-fifth (19%) greater between 2000 and 2008.

Table 23 also displays sentence length outcomes for immigration cases during both the pre- and post-*Booker* periods. These findings indicate that Hispanics were the recipients of significantly longer sentences during both time frames. Prior to *Booker*, Hispanics sentenced for immigration offenses received prison terms that were approximately 4% greater than those of their non-Hispanic counterparts. After the Supreme Court's ruling, Hispanics received sentences that were 24% greater than those of non-Hispanics. This increase represents a significant change between the two periods, as is indicated by the z-statistic in the far right column.

In order to further investigate the timing of the shift in sentence lengths for Hispanics, quarterly regression models for the sentence length decision were also estimated, and the moving averages of the percent differences for Hispanics (relative to non-Hispanics) are graphed in Figure 13. This graph suggests the possibility of a “*Booker* effect,” and a t-test comparing the mean percent changes in sentence length for Hispanic offenders sentenced before and after the decision indicates a significant change ( $t=5.76$ ). The graph shows that, although sentence lengths for Hispanics were increasing during the period prior to the PROTECT Act, they remained relatively flat throughout much of the PROTECT Act period. In the quarter prior to the *Booker* decision, sentence lengths for Hispanic immigration offenders increased dramatically, and they continued to rise in the two quarters following the decision. During much of the post-*Booker* era, sentence lengths for Hispanics were between 27% and 33% longer than those of non-Hispanics sentenced for immigration offenses. This trend

began to reverse in the period prior to the *Gall* decision. Though not causal, this graph is certainly suggestive and points to the need for further examination of *Booker*'s role in sentence length outcomes for immigration offenses.

#### *Departures – Individual-Level Models*

Departures play an important role in the sentencing of immigration offenders. In particular, so-called “fast-track” departures have traditionally been utilized in these cases as a way to save the government time and money while providing sentencing discounts to offenders for ostensible acceptance of responsibility. Table 24 displays the results of the multinomial logistic regression model examining the likelihood of departures for offenders sentenced for immigration offenses during the full study period. These results indicate that immigration offenders were more likely to benefit from both types of departures in the period after the Supreme Court decided *Booker*. While these offenders were 1.48 times more likely to receive judge-initiated departures following the decision, they were 4.32 times more likely to receive departures initiated by prosecutors, including fast-track departures. Although these results are suggestive of increased popularity of government-initiated departures following the *Booker* decision, they must be interpreted cautiously because, as noted previously, the post-*Booker* increase in these departures may be at least partially attributable to the fact that the Commission did not capture fast tracks as a category of departures in its data until 2003.

Separate pre- and post-*Booker* departure models were also estimated to further investigate period-specific differences (Table 25). These results indicate that, although the odds of government-initiated departure were statistically similar for

Hispanic and non-Hispanic immigration offenders prior to the Supreme Court's ruling, Hispanic offenders were approximately 12% less likely to receive these departures in the years following the decision. However, the chi-square for difference indicates that change in magnitude between the two periods was non-significant at the  $p < .05$  level.<sup>60</sup>

Significant pre- and post-*Booker* differences were evident with respect to Hispanics' odds of being granted judge-initiated departures. Consistent with the results for government-initiated departures, the results in Table 25 suggest that, though the odds of judge-initiated departures were similar for Hispanics and non-Hispanics prior to *Booker*, Hispanics were 13% less likely to be granted these departures after the Supreme Court's decision. The declining likelihood of judge-initiated departures between the two time periods was statistically significant at the  $p < .05$  level, as is indicated by the chi-square statistic in the far right column of the table.<sup>61</sup>

In order to further investigate changes over time, multinomial logistic regression models were estimated for each quarter of the study period. Figure 14 graphs the three-quarter moving average of the odds of departure for Hispanic

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<sup>60</sup> Separate analyses indicate that immigration cases sentenced after the *Booker* decision received significantly greater government-initiated departure lengths. Although Hispanics were the recipients of significantly shorter government-initiated departures in both the periods leading up to and following the Supreme Court's ruling, there is evidence to suggest that the magnitude of Hispanics' departures improved significantly between the two time periods. However, graphing the three-quarter moving averages of the percent differences for Hispanics versus non-Hispanics reveals that shifting trends in the magnitudes of government-initiated departures took place prior to *Booker*, during the PROTECT Act period.

<sup>61</sup> Additional analyses indicate that immigration cases sentenced after the *Booker* decision received significantly shorter judge-initiated departure lengths. Both prior to and after the Supreme Court's decision, Hispanics received judge-initiated departures that were similar in magnitude to those of non-Hispanics. Graphing the three-quarter moving average for the percent difference for Hispanics versus non-Hispanics indicates that departure lengths had generally been increasing since the middle of the PROTECT Act period.

(relative to non-Hispanic) offenders for both government- and judge-initiated departures. The graph is not particularly suggestive of a “*Booker* effect” for either type of departure. Hispanics’ odds of receiving government-initiated departures peaked during the PROTECT Act period. Their odds of being granted these departures dropped somewhat at the beginning of the post-*Booker* period but remained relative stable – at around 1.4 times higher than non-Hispanics – until the end of 2006, when they began to drop sharply; throughout 2007, and for the remainder of the study period, Hispanics appeared to be less likely than non-Hispanics to receive government-initiated departures. A t-test indicates that Hispanics’ mean odds of departure were not significantly different between the two time periods ( $t=.59$ ).

Judge-initiated departure trends for Hispanic immigration offenders reveal a different pattern. The odds of these departures appeared to be generally declining from the beginning of the study period until the end of the PROTECT Act period. Although Hispanics’ likelihood of departure increased somewhat during the beginning of the post-*Booker* period, judges’ application of these departures was relatively similar for Hispanics and non-Hispanics for much of this period. While the odds of departure for Hispanics declined again somewhat near the end of 2006, they appeared to rebound after the *Gall* decision. Thus, while the multinomial logistic regression model presented in Table 25 suggested significant changes between the pre- and post-*Booker* periods, this graph suggests that the odds of judge-initiated departures for Hispanics were in decline well in advance of the decision. Further, a t-

test comparing their mean odds of departure between the two time periods does not suggest significant differences ( $t=1.49$ ).

### **Multilevel Analysis**

This section explores the role played by contextual factors in sentencing outcomes for immigration offenses. Again, because of the relative dearth of literature regarding *Booker*'s impact on sentencing for immigration offenses, the research questions examined in this chapter are fairly general. As such, it is simply hypothesized that inter-district variation in sentencing outcomes for immigration offenses exists and will increase significantly in the post-*Booker* era. The district-level variables included to explore this hypothesis are largely the same as those included in Chapter VI and are summarized in Table 26.<sup>62</sup>

The unconditional models presented in Table 27 confirm that significant district-level variation existed both prior to and following the Supreme Court's decision. Unlike the findings presented in the two previous chapters, these findings indicate substantial change over time in the proportion of variance in sentencing outcomes explained by districts for several outcomes. Districts accounted for 19% of the variance in sentence lengths in the pre-*Booker* era and 26% of the variance in the post-*Booker* era. The amount of district-level variation in government-initiated departures also increased substantially, from 23% in the period prior to the decision to 33% in the period following the decision. On the other hand, districts accounted for 22% of the variance in the likelihood of judge-initiated departures before the decision but only 14% of the variance in this outcome following the Supreme Court's ruling.

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<sup>62</sup> Caseload pressure measures overall number of case filings per judge, averaged over time; it is not specific to immigration cases, as large immigration caseloads are largely concentrated in a small proportion of districts.

However, it must be kept in mind that these large shifts in the proportion of variance accounted for by districts may be an artifact of the way the federal sentencing data were collected prior to 2003, when fast track departures were not captured as a distinct departure category. Thus, the limited support for the hypothesis that the *Booker* decision increased inter-district variation in sentencing outcomes (in the cases of sentence length and government-initiated departure) must be viewed with caution.

The models presented in Table 28 explore possible factors behind these changes. Both prior to and following the *Booker* decision, judges in districts with higher historical departure rates were more likely to grant both government- and judge-initiated departures. While factors related to caseload pressure exhibited statistically significant influence on sentence lengths only in the period prior to *Booker*, they were significantly related to departures only after the High Court's decision. Post-*Booker*, government-initiated departures were significantly more likely to be granted in districts with heavy caseloads; concomitantly, these departures were significantly less likely to be granted in districts where caseloads were increasing. On the other hand, judges were significantly more likely to initiate departures after *Booker* in districts where caseload pressure was increasing.

### **Summary**

Consistent with the results presented in previous chapters, the findings of models focused specifically on *Booker*'s influence on sentencing outcomes for immigration offenders largely suggest that significant pre-to-post changes were likely the result of either ongoing trends or of changes initiated during the PROTECT Act period (Table 29). However, sentence length outcomes provided an exception, as

graphing quarterly percent changes in the sentence lengths suggested that the significantly longer sentences experienced by Hispanics following the *Booker* decision were likely a result of post-*Booker* increases. The predicted increase in the role played by district-level factors following the Supreme Court's decision was met with limited support, although the findings do suggest that further inquiry into the influence of increasing caseload pressure is warranted.

## CHAPTER IX: SUMMARY

This chapter summarizes the key findings found in each of the previous three chapters. Conclusions and directions for future research are offered in Chapter X.

### ***Booker's Role in Sentencing Outcomes for All Offenses (Excluding Immigration)***

The intended goal of the analyses in Chapter VI was, in large part, to replicate previous research that has examined the impact of the *Booker* decision on sentencing outcomes for all federal offenses (excluding immigration). A series of regression models were used to evaluate the prediction that post-*Booker* incarceration and sentence length trends would largely resemble pre-*Booker* trends but that differences between whites and minorities would emerge as more time passes since the decision. Findings in Chapter VI indicate that incarceration was no more or less likely in the post-*Booker* era than in the period prior to the Supreme Court's decision. Also consistent with prior work (Hofer, 2007; USSC, 2006), the coefficient representing the *Booker* decision was found to be significantly related to longer sentences.

*Booker's* influence on incarceration and sentence length outcomes for minority offenders is more nuanced. Though period-specific logistic regression models indicate that Hispanic offenders' likelihood of incarceration increased significantly in the period following the Supreme Court's ruling – Hispanics were 27% more likely than whites to be imprisoned prior to *Booker* and 39% more likely to be imprisoned after the decision – graphing their quarterly odds of incarceration relative to whites suggests that Hispanics' odds of incarceration actually began to increase during the PROTECT Act period, well before the Supreme Court's decision.

Evidence to support the notion that the *Booker* decision contributed to lengthier sentences for black and Hispanic offenders is lacking. Period-specific models indicate that sentence lengths for both blacks and Hispanics became significantly shorter in the period following the High Court's decision, while graphing the quarterly percent changes in sentence lengths for these two minority groups (relative to whites) similarly offers no evidence that sentence lengths for these groups became more punitive in the post-*Booker* period. These results are consistent with previous findings by Ulmer, Light, and Kramer (2011b), who found that there was less disparity in sentence lengths of black males in the post-*Booker/Gall* periods than there was in the period preceding the passage of the PROTECT Act.

Chapter VI also examined the prediction that disparities between whites and minorities in the use of departures would become more apparent with time. Though higher percentages of offenders sentenced after the decision received both government- and judge-initiated departures, pre-to-post changes with respect to race/ethnicity varied by departure type. Period-specific multinomial logistic regression models indicate that Hispanics' odds of receiving a government-initiated departure improved significantly following *Booker*, but graphing trends based on models for each fiscal quarter suggests that the odds of receiving departures initiated by prosecutors were elevated for both blacks and Hispanics during the PROTECT ACT period – almost to the point of parity for Hispanics and whites – but dropped substantially thereafter. Particularly with respect to Hispanic offenders, though their odds of government-initiated departure were lower following *Booker* than during the PROTECT Act period, they were still higher following the decision than they were

during the pre-PROTECT period. At the same time, however, the graph is suggestive of declining odds of government-initiated departures for both blacks and Hispanics toward the end of the study period. This finding is in line with previous work by Ulmer, Light, and Kramer (2011b), who found that disparities in the application of §5K1.1 departures were particularly pronounced for Hispanic males in the post-*Gall* period.

Perhaps the clearest evidence of a “*Booker* effect” is offered with respect to judge-initiated departures. Period-specific models indicate that odds of benefiting from a judge-initiated departures improved for blacks following the Supreme Court’s decision, and graphing the quarterly odds of judge-initiated departures shows that the odds of these departures gradually increased for both minority groups (relative to whites) after the Supreme Court’s ruling. Although these findings stand opposed to the prediction that disparities in the use of judge-initiated departures would increase over time – both before and after the Supreme Court’s decision, blacks and Hispanics were less likely to benefit from these departures, though there was evidence that their odds were improving with time – they are consistent with work by Ulmer, Light, and Kramer (2011b), which also failed to find evidence of post-*Booker/Gall* increases in disparities for blacks in the application of judge-initiated departures (though they did find post-*Gall* disparities in the use of these departures for Hispanics). These results also align with previous work by Hofer (2007), which found that judges viewed offender characteristics (though not specifically race/ethnicity) as legitimate reasons for departing from the Guidelines after the decision.

Because prior work has either ignored contextual factors or has reported findings that seemed counter-intuitive (i.e., that inter-district variation did not increase in the post-*Booker* period), the current dissertation sought to re-examine *Booker*'s influence on inter-district variation. Consistent with prior research by Ulmer, Light, and Kramer (2011a), the findings in Chapter VI suggested that *Booker* did not decrease uniformity in the application of the Guidelines across federal districts. Further, the results in Chapter VI indicated that districts' historical departure rates were influential on sentence lengths and odds of departure both prior to and following the Supreme Court's decision, rather than specific to the post-*Booker* era, as predicted.

Though predictions regarding the role of heavy and increasing caseload pressure were largely met with opposing evidence, some findings are worth reiterating. Specifically, results in Chapter VI indicate that, following *Booker*, judge-initiated departures were significantly more likely when judges were in districts facing increasing caseload pressure. At the same time, however, judges in districts where caseload pressure was already heavy were significantly less likely to initiate departures in the period following the decision. These seemingly opposing findings are worthy of further investigation. In particular, future investigations should examine whether there is a certain threshold or tipping point at which caseload pressure operates to individual offenders' disadvantage.

Further attention is also warranted with respect to the role played by both large and growing black populations in sentencing outcomes after *Booker*. As was predicted, findings in Chapter VI show that, in districts with large black populations,

judges were less likely to grant judge-initiated departures following the Supreme Court's decision. At the same time, government-initiated departures were significantly less likely in the post-*Booker* period in districts with growing black populations. Future research should more closely examine how racial threat dynamics operate across districts, with particular attention paid whether there is an important interaction between racial composition and growth. For example, is there a difference in departure outcomes between districts that have traditionally been non-minority jurisdictions but have growing minority populations compared to jurisdictions that have always had large minority populations but are experiencing more minority population growth?

### ***Booker's Role in Drug Sentencing***

Given the controversy surrounding what have been characterized as overly-severe prescribed sentences for crack cocaine offenses (and drug offenses more generally) under the presumptive Guidelines, it was anticipated that judges would seize the opportunity presented by *Booker* to mitigate the traditionally differential sentencing patterns of crack and powder cocaine offenders and to balance the sentencing outcomes for different racial/ethnic groups as well. Chapter VII investigated these predictions and found that, in general, prison sentences for drug offenders became more certain and severe in the post-*Booker* era; following the decision, drug offenders were 22% more likely to be incarcerated, and, contrary to prior findings by Ulmer and Light (2010) and Ulmer, Light, and Kramer (2011a) their sentences were 1% *greater* than those of offenders sentence prior to the decision. At the same time, the likelihood of government-initiated departures in drug cases

increased by 70% after the decision, while the likelihood of judge-initiated departures increased by 82%.

With respect to differences in sentencing outcomes for specific types of drugs, findings in Chapter VII indicate that, relative to offenders sentenced for marijuana/hashish offenses, the likelihood of incarceration for crack offenders decreased by 33% between the pre- and post-*Booker* periods, while 9% the reduction in the likelihood of imprisonment for powder cocaine offenders following the decision was non-significant. However, graphing the quarterly odds of imprisonment revealed that likelihood of incarceration of crack offenders (relative to powder cocaine offenders) peaked prior to the PROTECT Act and that odds of incarceration were generally decreasing over time, though the trend line exhibited a back-and-forth, up-and down pattern over time. Although changes between the pre- and post-*Booker* period with respect to sentence length were non-significant for both crack and powder cocaine offenders (relative to marijuana/hashish offenders), graphing the quarterly percent difference in logged sentence lengths for crack (versus powder) cocaine offenders is somewhat more suggestive of a “*Booker* effect,” as sentences for crack offenders dropped markedly after the decision and fell to roughly the same level as sentences for powder cocaine in the midst of 2006. However, caution is warranted because other important changes surrounding crack-versus-powder cocaine sentencing, including revisions to the Guidelines, occurred during the post-*Booker* period.

Contrary to expectations, significant changes were not evident with respect to the use of either government- or judge-initiated departures for crack or powder

cocaine offenders (relative to marijuana/hashish offenders) in the post-*Booker* period. Graphing the quarterly odds of government-initiated departure indicated that the odds of receiving such a departure actually peaked for crack offenders (relative to powder cocaine offenders) during the PROTECT Act period and declined thereafter. With respect to judge-initiated departures, using the same graphing technique also failed to provide evidence of a “*Booker* effect,” instead suggesting that the PROTECT Act period was the anomalous period, wherein departures for crack offenders plummeted; although trends reversed again post-*Booker* to briefly favor crack offenders, they leveled out again by the time the Commission revised its guidelines with respect to crack-cocaine sentencing in 2007.

With respect to race/ethnicity, results of the models presented in Chapter VII are equally non-supportive of a hypothesized “*Booker* effect.” Although the odds of incarceration reduced by 18% for blacks following the decision, the change in odds between the two time periods was non-significant for Hispanic offenders. Further, graphing the quarterly odds of imprisonment for these two groups (relative to whites) suggested that the declining odds of incarceration for both groups began during the PROTECT Act period. Similarly, though period-specific regression models suggested that sentence lengths increased significantly for black drug offenders following the Supreme Court’s decision, graphing the quarterly percent difference in the logged sentence lengths for black and Hispanic drug offenders (relative to whites) suggested that sentence lengths for both groups reached their lowest points in the midst of the PROTECT Act period; increases during the post-*Booker* period appeared to be a continuation of an existing trend.

Although results presented in Chapter VII suggested that Hispanics' likelihood of receiving government-initiated departures improved following the Supreme Court's decision, both blacks and Hispanics were less likely than whites to receive these departures in drug cases in both the pre- and post-*Booker* periods. Similarly, both groups were significantly less likely than whites to receive judge-initiated departures in drug cases during both periods, and changes between the two periods were non-significant. Again, graphing the quarterly odds of these two minority groups (relative to whites) receiving either type of departure underscores the importance of the PROTECT Act period rather than suggesting a "*Booker* effect" for either.

Chapter VII also investigated the hypothesis that districts characterized by heavy or increasing relative drug caseloads would grant more favorable sentencing outcomes in the post-*Booker* period. Multilevel findings not only suggested that the proportion of variance attributed to districts changed very little over time but also indicated that, contrary to expectations, heavier relative drug caseloads were not significantly related to any of the outcomes examined. In addition, increases in relative drug caseloads in the post-*Booker* period were not significantly related to more favorable outcomes in any of the models examined.

### ***Booker's Role in Immigration Sentencing***

Given their unique role in the federal sentencing picture, immigration offenses were also examined separately. Because relatively few prior studies have focused on immigration cases, the stated hypotheses were necessarily exploratory in nature, with sanctions for immigration offenses expected to differ before and after the *Booker*

decision. Because 96% of immigration offenders were imprisoned during the study period, the incarceration decision was not modeled. Unlike the overall or drug-specific results presented in previous chapters, the findings specific to immigration offenders indicated that offenders sentenced after the Supreme Court's ruling were the recipients of significantly shorter sentences than those who were sentenced in the pre-*Booker* era. In addition, immigration offenders sentenced in the post-*Booker* period were 1.48 times more likely than those sentenced prior to the decision to benefit from judge-initiated departures, and they were 4.32 times more likely to benefit from government-initiated departures.

Additional predictions anticipated that Hispanic and non-Hispanic differences would be most apparent in the years following the *Booker* decision. Sentence lengths for Hispanic offenders increased significantly between the two time periods, from 4% greater than non-Hispanics in the pre-*Booker* period to 24% greater in the post-*Booker* period. A graph of the quarterly percent differences for Hispanics (relative to non-Hispanics) was suggestive of a "*Booker* effect," as sentence lengths that were relatively flat throughout the PROTECT Act period increased dramatically in the quarter prior to the *Booker* decision and continued to rise and remained relatively high until the period prior to the *Gall* decision.

As noted in Chapter VIII, results related to government- and judge-initiated departures must be interpreted cautiously because data collection issues related to fast track departures may be at least partially responsible for pre- and post-*Booker* differences. Bearing this caveat in mind, the results suggest that reductions in the likelihood of government-initiated departures for Hispanic offenders across the two

time periods were non-significant, and graphing the quarterly odds of these departures were also not particularly suggestive of a “*Booker* effect.” Though period-specific models indicate that Hispanic offenders odds of receiving a judge-initiated departure declined significantly in the period following the decision, graphing the quarterly odds of judge-initiated departures was similarly non-supportive that the *Booker* decision alone has fundamentally changed how these departures are handed down with respect to ethnicity.

Finally, multilevel results presented in Chapter VIII, unlike the multilevel results presented in the other two results chapters, indicate that significant changes in the proportion of variance attributable to districts were evident for both government- and judge-initiated departures. However, the same caveat regarding the Commission’s data collection casts some suspicion on this finding. Other findings presented in Chapter VIII suggest the need to further explore the role played by contextual factors in immigration sentencing outcomes.

## CHAPTER X: CONCLUSIONS

With a few exceptions, the results presented in the previous chapters find very little evidence of a “*Booker* effect” during the years examined. When noticeable differences were found pre- to post-*Booker*, closer examination of quarterly trends often revealed that shifts occurred prior to *Booker*, during the PROTECT Act period. These findings are largely consistent with prior research, including work by the Commission, and they underscore the challenge of explaining why judges have largely continued to sentence according to the Guidelines even though they are now ostensibly re-afforded a tremendous amount of discretion.

The first step toward understanding these findings is to consider whether the methodological approach employed may be masking *Booker*’s role. However, it should again be emphasized that, due to the large sample sizes employed in the current work, tests of statistical significance are very sensitive to even small differences between groups. As such, one would expect that *rejection* of the null hypothesis would be more likely. As Engen (2011:1142) points out, “the problem is not, as I have heard it described, that very large ‘*Ns*’ somehow produce significant differences or effects where none exist but that large *Ns* allow us to measure associations with great precision, thus increasing our confidence that the slope coefficients in our models were not obtained by chance (which is not a bad problem to have). The consequence, however, is that even small associations achieve ‘significance,’ and we often will reject the null hypothesis of no difference in slope coefficients obtained in two or more time periods, or across groups, even when those differences are small.” Thus, it is perhaps more surprising that the models presented

in the current study, and in prior work, do not yield a greater number of significant differences between the pre- and post-*Booker* time periods.

Another possible methodological issue involves determining what time frame is appropriate for uncovering a “*Booker* effect.” The current dissertation utilizes data from 2000 through 2008 to provide relatively equal pre- and post-*Booker* sample sizes. However, many of the clarifications for *Booker*, including *Kimbrough* and *Gall*, were decided near the end of the study period, and it is likely that the effects of these clarifications were evident after the study period ended. In fact, a recent report by the Commission (2012:3), points to these cases as a possible explanation for the fact that “unwarranted disparities in federal sentencing appear to be increasing.” Thus, additional years of data may be necessary to examine the true impact of *Booker*, which may have come much later than what was allowed for by the current dataset. Of course, such analyses are further complicated by other changes – not just Supreme Court decisions - that have occurred since *Booker*. Most notably, in the case of drug offenses, the Commission revised its Guidelines to deal with disparities in specific drug offenses in 2007, and President Obama signed the Fair Sentencing Act to further address disparities between crack and powder cocaine in 2010. Due to such complications in drawing causal inference, it may be that an alternative methodological approach, such as a regression discontinuity design that is focused more closely on the periods immediately before and after the Supreme Court’s decision, would be better suited to more precisely determining *Booker*’s effects (see Starr and Rehavi, 2012).

The current dissertation, and sentencing research more generally, is also limited by the variables that it can consider. The SRA restricted the personal characteristics of offenders that could be incorporated into the Guidelines and captured in the Commission's data, which means that it is not possible to control for socioeconomic status, family involvement, and other possibly relevant individual factors. In addition, one possible proxy for socioeconomic status, type of defense attorney, is no longer captured in the data maintained by the Commission.

Aside from these individual-level factors, federal sentencing research would benefit from the inclusion of a broader array of contextual factors than has typically been examined. Though researchers, often using the focal concerns theoretical orientation, have given consideration to caseload pressure and other organizational factors, less attention has been paid to the role of prison crowding as a practical constraint considered by judges in making sentencing decisions. The Government Accountability Office (GAO, 2012) reported that, between 2006 and 2011, the Bureau of Prisons (BOP) increased capacity by approximately 8,300 beds, "but because of the population expansion, crowding (or population in excess of rated capacity) increased from 36 to 39 percent...BOP's 2020 long-range capacity plan projects continued growth in the federal prison population from fiscal years 2012 through 2020, with systemwide crowding exceeding 45 percent through 2018." Because such system pressures are driven, in large part, by judicial decision making, future research should examine the extent to which judges' sentencing decisions are, in turn, affected by burgeoning prison populations.

Characteristics of the judges themselves may also be influential in determining sentencing outcomes for offenders. Though collected, judge-level data are not made publicly available by the Commission and therefore could not be considered in the current study. Because prior research has demonstrated differences in outcomes based on interactions between the characteristics of judges and offenders, researchers should revisit these analyses if and when the Commission makes judge-level data available. At the same time, it is worth noting that researchers would not be able to glean judges' motivations for continuing to sentence according to the Guidelines in the post-*Booker* era even if such data were made available. Federal sentencing researchers should be mindful of prior criticisms of their overreliance on officially collected data (see Wellford, 2007) and make better use of qualitative data, including surveys and interviews with judges, to investigate how judicial attitudes and opinions – not just the characteristics of the districts where they sit on the bench – contextualize their decisions.

The current study's findings with respect to immigration offenses are limited due to the fact that possible "*Booker* effects" in the use of departures for these cases are confounded by data issues surrounding the use of Early Disposition Program, or fast track, departures. Given the dearth of research attention devoted to examining these offenses more generally, future research efforts should investigate how district-level attitudes and population characteristics affect sentencing outcomes for immigration offenders therein. In particular, the role played by large and increasing *Hispanic* populations should be investigated to determine whether immigration

offenders are treated more harshly when they represent a perceived ethnic group threat to the district in which they are sentenced.

More generally, more research attention should be devoted to the PROTECT Act period, which has been given fairly limited consideration in the research literature. The PROTECT Act was passed by Congress in 2003 and “not only repudiated the abuse-of-discretion standard of review announced in *Koon* but also specified that an appellate review of sentences would be *de novo*, directed the Commission to reduce requirements for non-guideline sentences, and directed prosecutors to resist downward departures” (Scott, 2011: 1134). Scott (2011) cites prior research (Stith, 2008; USSC, 2003; 2006) as showing that judge-initiated downward departures dropped to 5% after the PROTECT Act was passed. Yet, despite this dramatic change in federal sentencing policy and practice, most researchers have devoted little attention to the PROTECT Act period, other than as a period to which the post-*Booker* period may be compared. The current work suggests that sentencing outcomes were dramatically different in the PROTECT Act period than in the periods leading up to or following it. Accordingly, researchers wishing to better understand the impact of shifts in sentencing policy should more closely investigate what happened in the wake of the PROTECT Act.

Researchers should also be mindful of the fact that the degree of disparity uncovered at the sentencing stage is likely much smaller in magnitude than what may be uncovered at earlier stages in the criminal justice system, where decision making is more hidden and has not been subject to dramatic reform efforts or intense, systematic scrutiny (Anderson, Kling, and Stith, 1999; Bushway and Piehl, 2001,

2007; Farabee, 1998; Kempf-Leonard and Sample, 2001). Recent research (Rehavi and Starr, 2012; Starr and Rehavi, 2012) utilizes data from the U.S. Marshals' Service, the Executive Office of the U.S. Attorneys, the Administrative Office of the U.S. Courts, and the United States Sentencing Commission and finds that longer sentences for black males were largely driven by initial charging decisions; in particular, prosecutors were twice as likely to file mandatory minimum charges against black offenders. Starr and Rehavi (2012) also use a regression discontinuity analysis to investigate *Booker*'s immediate effects on sentencing, and they find that, though departures immediately increased, sentencing disparities did not; again, however, they found that blacks were more likely to be charged with mandatory minimums (and have longer average sentences). They speculate that "prosecutors may have reacted to the Guidelines' weakening by charging more harshly, and in particular by seeking to constrain judges with mandatory minimums instead" (Starr and Rehavi, 2012: 4). However, their results suggest that this pattern was only temporary and that the use of mandatory minimums eventually returned to pre-existing trends. Future research should revisit and extend these findings, with particular attention paid to how *Booker* impacted charging decisions related to drug and immigration offenses.

In addition to these directions for future research, there also exists a need to broaden the types of theories typically applied to sentencing research and to enhance those that are already being used. In particular, more work is needed to explain why courtroom workgroups appear to have largely maintained the status quo even in the face of monumental changes to the rules governing the behavior of workgroup

members. Commonly applied theories that account for organizational factors, including the focal concerns perspective, have not yet been extended to explain whether and how legal rulings such as *Booker* impact organizations and, ultimately, sentencing outcomes. Perhaps Newton's First Law of Motion – that objects at rest will stay at rest, and objects in motion will stay in motion, unless acted upon by an unbalanced force – is appropriate for explaining why there is so little evidence of district-level variation in sentencing outcomes, and for why these outcomes have largely remained consistent with prior trends, in the wake of *Booker*.

Finally, ten years after the *Booker* decision, policymakers still face the question of what should be done to prevent disparities in sentencing outcomes now that the Guidelines are advisory. Paternoster (2011) critiques sentencing researchers who have taken part in this policy debate for rejecting a return to a mandatory Guidelines system (with ostensibly more uniformity) in favor of allowing judges to exercise greater discretion under an advisory system. He believes “that at least part of the issue among federal sentencing scholars is that the mandatory guidelines were too severe for their taste and they fear any attempt to return to them” (Paternoster, 2011: 1070). At the same time, if much of the evidence points to the fact that judges continue to sentence in accordance with the Guidelines and that disparities have not dramatically increased, is a return to mandatory Guidelines really necessary (see also Albonetti, 2011; Ulmer et al., 2011)? Perhaps the Commission's own viewpoint is most instructive. In their 2012 report on *Booker*, the Commission stopped short of advocating for a return to a mandatory system and instead outlined recommendations for improving the appellate process and for clarifying the extent to which judges

could consider some offender characteristics. In addition, the Commission called on Congress to statutorily clarify how much weight should be given to the Guidelines, noting that the uncertainty caused by Supreme Court decisions subsequent to *Booker* may have opened the door for increased disparities. Ultimately, it may be that more research is needed before a policy course is chosen. As Scott (2011:1136) notes: “To move policy makers . . . evidence of a trend in race disparity will have to be robust and sustained. So far, the race disparity research, standing alone, is insufficient to justify sweeping changes.”

**Table 1. U.S. Supreme Court Decisions Dealing with the Sixth Amendment**

Case Name	Year Decided	Summary
<i>Apprendi v. New Jersey</i>	2000	The Supreme Court ruled that, aside from a prior conviction, facts that lead to an aggravated sentence beyond a statutorily prescribed maximum must be decided by a jury using the “beyond a reasonable doubt” standard.
<i>Ring v. Arizona</i>	2002	The Supreme Court ruled that the Arizona statute that allowed trial judges to determine whether the aggravating factors to impose death were present violated the Sixth Amendment since the aggravating factors acted as the “functional equivalent” of a greater offense. Such aggravating factors should be decided by a jury.
<i>Blakely v. Washington</i>	2004	The Supreme Court thus ruled that a judge’s application of an enhanced sentence under the Washington State guidelines violated the defendant’s Sixth Amendment right to a jury trial, as the defendant had not made admissions that supported the court's finding of this fact.
<i>United States v. Booker</i>	2005	As in <i>Blakely</i> , the Court ruled in its "merits" opinion that facts (other than a prior conviction) that increase the offender's sentence beyond what is authorized by the Guidelines must be either provided by the defendant’s admission or proved beyond a reasonable doubt to a jury. The Court’s “remedial” opinion excised the provisions of the Sentencing Reform Act that made the Guidelines mandatory and which outlined the grounds for appeal related to guideline departures.
<i>Gall v. United States</i>	2007	The Supreme Court ruled that sentencing outside of the Guidelines was not necessarily "unreasonable."
<i>Kimbrough v. United States</i>	2007	The Court reaffirmed that the Guidelines are only one factor that judges should consider in determining a sentence and that the built-in disparity between crack and powder cocaine sentences in the Guidelines is a valid reason for judicial variation from the sentences prescribed by the Guidelines.
<i>Irizarry v. United States</i>	2007	The Supreme Court reasoned that, because judges are now using the factors laid out in 18 U.S.C. §3553(b) in an advisory system, any resulting deviations from the Guidelines are not “departures” but “variances.” Accordingly, defendants do not have a Constitutional right to know of these variances in advance.

**Table 2. Descriptive Summaries of Level 1 Variables**

<b>Variables</b>	<b>Coding Specifications</b>	<b>Description</b>
<b><u>Dependent Variables</u></b>		
PRISDUM	1=Yes	Dummy indicating receipt of prison sentence
SENTLOG	Ln Months	Natural log of the total prison sentence in months (capped at 470)
GOVDEP	1=Yes	Dummy indicating receipt of government-initiated downward departure
DOWNDEP	1=Yes	Dummy indicating receipt of judge-initiated downward departure
<b><u>Independent Variables</u></b>		
	1=Post-Booker (01/12/05 or later)	
BOOKPOST		Dummy indicating sentencing period in reference to the <i>Booker</i> decision
RACE DUMMIES	3 Dummy Variables	Dummies indicating defendant's race/ethnicity; white is the reference category
RACEMISS2	1=Missing	Dummy indicating that information on defendant's race/ethnicity is missing
AGE	Years	Defendant's age at time of offense
MALE	1=Male	Dummy indicating defendant's sex
NONCITZ0	1=Not a U.S. citizen	Dummy indicating that the defendant is not a U.S. citizen
CITMISS	1=Missing	Dummy indicating that the defendant's citizenship status is missing
DEPEND	1=One or more dependents	Dummy indicating one or more dependents
DPENDMIS	1=Missing	Dummy indicating that information on dependents is missing
EDUCATION DUMMIES	3 Dummy Variables	Dummies indicating educational level; high school graduate is the reference category
EDUCMISS	1=Missing	Dummy indicating that information on defendant's education is missing
OFFENSE DUMMIES	6 Dummy Variables	Dummies indicating offense type; property offense is the reference category
XCRHISSR	Scale of 1 to 6	Defendant's final criminal history category as determined by the court
PRESUMPTIVE TRIAL	Months	Adjusted minimum months of incarceration recommended by the Guidelines
DETAINED	1=Yes	Dummy indicating presentence detention
DEPARTURE DUMMIES	3 Dummy Variables	Dummy variables indicating presence of departure; No Departure is the reference
DEPMISS	1=Missing	Dummy variable indicating that information on departures is missing
DISTRICT DUMMIES	89 Dummy Variables	Dummy variables for district in which offender was sentenced

**Table 3. Descriptive Summaries of Level 2 Variables**

<b>Independent Variables</b>	<b>Coding Specifications</b>	<b>Description</b>
COURT SIZE	Continuous Variable	Average number of federal judgeships in the district, 2000-2008
DEPARTURE RATE	Percentage	Average percentage of departures per district, averaged over time
CASELOAD	Continuous Variable	Number of case filings per authorized judgeship, averaged over time
CASELOAD CHANGE	Percentage	Percentage change in filings per judgeship, averaged over time
RACIAL COMPOSITION	Percentage	Percentage of blacks per district, averaged over time
RACE CHANGE	Percentage	Percentage change in racial composition, averaged over time

**Table 4. Level 1 Descriptive Statistics for Federal Criminal Cases (Excluding Immigration), FY2000-2008**

	<b>Full Study Period N=445,481</b>		<b>Pre-Booker N=252,907</b>		<b>Post-Booker N=192,574</b>		
<i>Dependent variables</i>	<b>Mean</b>	<b>S.D.</b>	<b>Mean</b>	<b>S.D.</b>	<b>Mean</b>	<b>S.D.</b>	<b>  t  </b>
Incarceration	.85 (N=445,481)	.35	.84 (N=252,907)	.37	.87 (N=192,574)	.34	26.84 *
Sentence Length	67.92	74.82	64.48	73.07	72.28	76.76	31.86 *
Sentence Length (Logged)	3.62 (N=377,889)	1.32	3.57 (N=211,374)	1.27	3.68 (N=166,515)	1.38	25.03 *
Government-Initiated Down. Departure	.22 (N=399,013)	.42	.19 (N=221,986)	.39	.27 (N=177,027)	.44	56.74 *
Judge-Initiated Downward Departure	.16 (N=399,013)	.37	.16 (N=221,986)	.37	.16 (N=177,027)	.36	2.68 *
<i>Independent variables</i>							
Booker decision	.43	.50	--	--	--	--	--
White (reference category)	.36	.48	.37	.48	.36	.48	9.43 *
Black	.31	.46	.31	.46	.32	.47	4.05 *
Hispanic	.25	.44	.25	.44	.25	.43	2.36
Other Race	.05	.22	.04	.20	.06	.24	24.85 *
Race Information Missing	.02	.13	.02	.14	.01	.12	13.64 *
Age of Defendant	34.99	11.16	34.65	11.09	35.44	11.24	23.31 *
Male	.85	.36	.84	.37	.85	.36	10.01 *
Defendant is a Non-Citizen	.20	.40	.20	.40	.20	.40	3.78 *
Citizenship Status Missing	.02	.13	.02	.13	.02	.12	3.80 *
Offender has Dependents	.59	.49	.59	.49	.60	.49	7.04 *
Information on Dependents Missing	.04	.19	.04	.20	.04	.19	5.32 *
No High School Diploma	.39	.49	.39	.49	.38	.49	5.03 *
High School Graduate (ref. category)	.33	.47	.33	.47	.34	.47	6.13 *
Some College	.19	.39	.19	.39	.18	.39	2.16
College Graduate	.07	.25	.07	.25	.07	.25	1.48
Education Information Missing	.03	.17	.03	.17	.03	.17	.17
Violent Offenses	.05	.21	.05	.22	.04	.21	9.81 *
Property Offenses (reference category)	.04	.20	.05	.22	.03	.18	27.93 *
Drug Offenses	.49	.50	.50	.50	.48	.50	12.23 *
Fraud Offenses	.22	.41	.22	.42	.21	.41	11.52 *
Firearms Offenses	.13	.34	.11	.32	.15	.36	40.54 *
Other Offenses	.07	.25	.06	.24	.08	.27	19.35 *
Criminal History Category	2.31	1.72	2.24	1.68	2.39	1.77	28.22 *
Presumptive Guideline Length	68.42	80.52	64.07	77.95	74.13	83.44	41.41 *
Trial	.05	.22	.05	.21	.05	.23	12.03 *
Presentence Detention	.62	.49	.59	.49	.65	.48	43.57 *
Block of Departure Dummy Variables	--	--	--	--	--	--	--
Block of District Dummy Variables	--	--	--	--	--	--	--

\* p<.01

**Table 5. Logistic Regression Models for Incarceration Decision, Federal Criminal Cases (Excluding Immigration), FY2000-2008**

Independent Variables	Full Study Period N=445,481				Pre-Booker N=252,907				Post-Booker N=192,574				Ratio of Coeff.	Chi-Square for Difference
	$\beta$	S.E.	Exp ( $\beta$ )		$\beta$	S.E.	Exp ( $\beta$ )		$\beta$	S.E.	Exp ( $\beta$ )			
Constant	-1.34	.04	--	**	-1.26	.05	--	**	-1.47	.07	--	*	--	--
Booker Decision	.02	.01	1.02		--	--	--		--	--	--		--	--
Black	.04	.02	1.04	**	.04	.02	1.04	*	.06	.03	1.06	*	1.59	.49
Hispanic	.27	.02	1.31	**	.24	.03	1.27	**	.33	.03	1.39	**	1.38	4.52 *
Other Race	.05	.03	1.05		-.03	.04	.97		.17	.04	1.18	**	-5.00	13.88 **
Age of Defendant	-.01	.00	.99	**	-.01	.00	.99	**	-.01	.00	.99	**	1.67	8.00 **
Male	.36	.01	1.44	**	.36	.02	1.43	**	.37	.02	1.45	**	1.04	.25
Defendant is a Non-Citizen	.78	.02	2.18	**	.70	.03	2.01	**	.91	.04	2.47	**	1.29	21.51 **
Offender has Dependents	-.03	.01	.97	*	-.04	.02	.96	**	-.01	.02	.99		.25	1.66
No High School Diploma	.14	.02	1.15	**	.15	.02	1.17	**	.12	.03	1.13	**	.77	1.09
Some College	.04	.16	1.04	*	.04	.02	1.04		.03	.03	1.03		.64	.18
College Graduate	.08	.02	1.08	**	.11	.03	1.12	**	.03	.03	1.03		.24	4.00 *
Violent Offenses	.80	.04	2.23	**	.86	.06	2.36	**	.71	.07	2.03	**	.82	2.72
Drug Offenses	.82	.03	2.27	**	.84	.03	2.32	**	.83	.04	2.30	**	.99	.02
Fraud Offenses	.72	.02	2.05	**	.73	.03	2.07	**	.72	.04	2.06	**	.99	.01
Firearms Offenses	.83	.03	2.29	**	.86	.04	2.36	**	.79	.05	2.20	**	.91	1.46
Other Offenses	.19	.03	1.21	**	.21	.03	1.24	**	.17	.04	1.19	**	.80	.61
Criminal History Category	.35	.01	1.42	**	.36	.01	1.43	**	.35	.01	1.42	**	.97	.50
Presumptive Guideline Length	.09	.00	1.10	**	.09	.00	1.09	**	.11	.00	1.11	**	1.21	162.00 **
Trial	.78	.04	2.17	**	.84	.06	2.31	**	.68	.07	1.97	**	.81	3.25
Presentence Detention	1.41	.02	4.08	**	1.47	.02	4.34	**	1.34	.02	3.81	**	.91	16.62 **
Government-Initiated Downward Departure	-1.61	.02	.20	**	-1.59	.02	.20	**	-1.71	.03	.18	**	1.07	9.65 **
Judge-Initiated Downward Departure	-1.22	.02	.30	**	-1.29	.02	.28	**	-1.14	.03	.32	**	.89	16.71 **
Upward Departure	2.17	.14	8.74	**	1.88	.19	6.54	**	2.46	.19	11.66	**	1.31	4.53 *
Block of District Dummy Variables	--	--	--		--	--	--		--	--	--		--	--

Reference Categories: White, Female, Citizen, High School Graduate, Property Offenses, No Departure

\*\*p<.01; \*p<.05

**Table 6. OLS Regression Models for Sentence Length (Logged) Decision, Federal Criminal Cases (Excluding Immigration), FY2000-2008**

Independent Variables	Full Study Period N=377,889				Pre-Booker N=211,374				Post-Booker N=166,515				z-score
	$\beta$	S.E.	Exp ( $\beta$ )		$\beta$	S.E.	Exp ( $\beta$ )		$\beta$	S.E.	Exp ( $\beta$ )		
Constant	1.56	.01	--	**	1.56	.02	--	**	1.57	.02	--	**	.06
Booker Decision	.02	.00	1.02	**	--	--	--		--	--	--		--
Black	-.03	.00	.97	**	-.02	.01	.98	**	-.03	.01	.97	**	-2.25 *
Hispanic	-.01	.01	.99	**	-.01	.01	.99		-.01	.01	.99		-2.34 **
Other Race	-.05	.01	.95	**	.00	.01	1.00		-.08	.01	.93	**	-9.79 **
Age of Defendant	.00	.00	1.00	**	.00	.00	1.00	**	.00	.00	1.00	**	--
Male	.28	.00	1.32	**	.29	.01	1.33	**	.28	.01	1.32	**	-.29
Defendant is a Non-Citizen	-.07	.00	.93	**	-.08	.01	.92	**	-.06	.01	.94	**	2.49 **
Offender has Dependents	.02	.00	1.02	**	.02	.00	1.02	**	.02	.01	1.02	**	-.68
No High School Diploma	.00	.00	1.00		.00	.00	1.00		.00	.01	1.00		.00
Some College	.00	.00	1.00		-.00	.01	1.00		.01	.01	1.01		11.95 **
College Graduate	.09	.01	1.09	**	.08	.01	1.08	**	.11	.01	1.11	**	2.04 *
Violent Offenses	.82	.01	2.26	**	.85	.01	2.35	**	.76	.02	2.14	**	-.63
Drug Offenses	.82	.01	2.27	**	.84	.01	2.31	**	.80	.02	2.22	**	-.31
Fraud Offenses	.11	.01	1.12	**	.10	.01	1.11	**	.13	.02	1.13	**	1.13
Firearms Offenses	.64	.01	1.90	**	.66	.01	1.94	**	.62	.02	1.85	**	-.40
Other Offenses	.39	.01	1.47	**	.36	.01	1.43	**	.41	.02	1.51	**	.82
Criminal History Category	.05	.00	1.06	**	.06	.00	1.06	**	.05	.00	1.05	**	-4.91 **
Presumptive Guideline Length	.01	.00	1.01	**	.01	.00	1.01	**	.01	.00	1.01	**	--
Trial	.10	.01	1.10	**	.13	.01	1.13	**	.06	.01	1.06	**	-5.37 **
Presentence Detention	.33	.00	1.40	**	.29	.00	1.34	**	.39	.01	1.48	**	2.75 **
Government-Initiated Downward Departure	-.51	.00	.60	**	-.45	.01	.64	**	-.59	.01	.56	**	-2.45 **
Judge-Initiated Downward Departure	-.45	.00	.64	**	-.45	.01	.64	**	-.44	.01	.64	**	.14
Upward Departure	.58	.01	1.79	**	.58	.02	1.79	**	.57	.02	1.78	**	-.09
Block of District Dummy Variables	--	--	--		--	--	--		--	--	--		--

Reference Categories: White, Female, Citizen, High School Graduate, Property Offenses, No Departure

\*\*p<.01; \*p<.05

**Table 7. Multinomial Logistic Regression Models for Departure Decision, Federal Criminal Cases (Excluding Immigration), FY2000-2008**

Independent Variables	Full Study Period N=394,443							
	Government-Initiated				Judge-Initiated			
	$\beta$	S.E.	Exp( $\beta$ )		$\beta$	S.E.	Exp( $\beta$ )	
Intercept	51.08	1.81	--	**	27.64	1.90	--	**
<i>Booker</i> Decision	.58	.01	1.79	**	.20	.01	1.22	**
Black	-.30	.01	.74	**	-.18	.01	.83	**
Hispanic	-.36	.01	.70	**	-.08	.02	.92	**
Other Race	--	--	--		--	--	--	
Age of Defendant	-.00	.00	1.00	**	.00	.00	1.00	**
Male	-.39	.01	.68	**	-.47	.01	.63	**
Defendant is a Non-Citizen	-.07	.01	.94	**	-.05	.02	.95	**
Offender has Dependents	.07	.01	1.08	**	.00	.01	1.00	
No High School Diploma	-.10	.01	.91	**	-.04	.01	.97	**
Some College	.10	.01	1.11	**	.10	.01	1.10	**
College Graduate	.28	.02	1.32	**	.28	.02	1.32	**
Violent Offenses	-.14	.04	.87	**	.26	.04	1.29	**
Drug Offenses	1.06	.03	2.88	**	.56	.03	1.75	**
Fraud Offenses	.51	.03	1.66	**	.28	.03	1.32	**
Firearms Offenses	.11	.03	1.12	**	.39	.03	1.48	**
Other Offenses	.01	.04	1.01		.47	.03	1.59	**
Criminal History Category	-.07	.00	.93	**	-.03	.00	.97	**
Presumptive Guideline Length	.01	.00	1.01	**	.00	.00	1.00	**
Trial	-3.31	.05	.04	**	-.57	.02	.57	**
Presentence Detention	-.76	.01	.47	**	-.76	.01	.47	**
Block of District Dummy Variables	--	--	--		--	--	--	

Reference Categories: White, Female, Citizen, High School Graduate, Property Offenses, No Departure

\*\*p<.01; \*p<.05

**Table 8. Pre- and Post-Booker Multinomial Logistic Regression Models for Departure Decision, Federal Criminal Cases (Excluding Immigration), FY2000-2008**

Independent Variables	Government-Initiated								Judge-Initiated							
	Pre-Booker N=220,220			Post-Booker N=174,223			Chi- Square Ratio of Coef- ficients		Pre-Booker N=220,220			Post-Booker N=174,223			Chi- Square Ratio of Coef- ficients	
	$\beta$	S.E.	Exp( $\beta$ )	$\beta$	S.E.	Exp( $\beta$ )			$\beta$	S.E.	Exp( $\beta$ )	$\beta$	S.E.	Exp( $\beta$ )		
Intercept	34.06	2.46	-- **	69.69	2.69	-- **	--	--	2.29	2.38	--	60.66	3.35	-- **	--	--
Black	-.28	.02	.76 **	-.31	.02	.74 **	1.09	1.15	-.25	.02	.78 **	-.15	.02	.86 **	.60	14.31 **
Hispanic	-.38	.02	.69 **	-.31	.02	.73 **	.83	5.28 *	-.11	.02	.90 **	-.17	.02	.85 **	1.54	3.75
Other Race	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--
Age of Defendant	-.00	.00	1.00 **	-.01	.00	1.00 **	5.00	12.50 **	.00	.00	1.00 **	.01	.00	1.01 **	2.50	4.50 *
Male	-.35	.02	.71 **	-.44	.02	.64 **	1.27	13.31 **	-.49	.02	.61 **	-.45	.02	.64 **	.92	2.09
Defendant is a Non-Citizen	-.13	.02	.88 **	-.03	.02	.97	.23	14.69 **	.03	.02	1.03	-.13	.02	.88 **	-4.61	26.53 **
Offender has Dependents	.06	.01	1.06 **	.09	.01	1.09 **	1.52	2.66	.04	.01	1.05 **	-.04	.02	.96 **	-.93	17.16 **
No High School Diploma	-.10	.01	.90 **	-.10	.02	.91 **	.97	.02	-.02	.02	.99	-.06	.02	.95 **	3.80	3.04
Some College	.10	.02	1.11 **	.10	.02	1.10 **	.95	.04	.11	.02	1.12 **	.08	.02	1.08 **	.69	1.52
College Graduate	.24	.03	1.27 **	.32	.03	1.38 **	1.36	4.60 *	.34	.03	1.41 **	.21	.03	1.23 **	.60	10.82 **
Violent Offenses	-.01	.05	.99	-.34	.06	.72 **	33.50	17.32 **	.39	.05	1.47 **	.01	.06	1.01	.03	25.45 **
Drug Offenses	1.12	.04	3.07 **	.94	.05	2.55 **	.83	8.82 **	.85	.04	2.33 **	.09	.05	1.09	.10	151.32 **
Fraud Offenses	.53	.04	1.69 **	.45	.05	1.56 **	.85	1.59	.42	.04	1.53 **	.02	.05	1.02	.04	42.90 **
Firearms Offenses	.13	.04	1.14 **	.05	.05	1.05	.34	1.66	.52	.05	1.68 **	.11	.05	1.12 *	.21	38.83 **
Other Offenses	.14	.05	1.15 **	-.15	.06	.87 **	-1.01	15.56 **	.48	.05	1.61 **	.25	.05	1.29 **	.53	10.35 **
Criminal History Category	-.06	.00	.95 **	-.09	.00	.91 **	1.65	40.50 **	-.02	.01	.99 **	-.06	.01	.95 **	3.80	35.28 **
Presump. Guideline Length	.01	.00	1.01 **	.01	.00	1.01 **	1.17	--	.00	.00	1.00 **	.00	.00	1.00 **	.75	--
Trial	-2.89	.06	.06 **	-3.83	.08	.02 **	1.32	90.81 **	-1.32	.04	.27 **	-.06	.03	.94 *	.05	690.95 **
Presentence Detention	-.76	.02	.47 **	-.74	.02	.48 **	.97	.83	-.68	.02	.51 **	-.87	.02	.42 **	1.28	64.22 **
Block of District Dummies	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--

Reference Categories: White, Female, Citizen, High School Graduate, Property Offenses, No Departure

\*\*p<.01; \*p<.05

**Table 9. Level 2 Descriptive Statistics for Federal Criminal Cases (Excluding Immigration), FY 2000-2008**

<i>District-level Variables (n=90)</i>	<b>Mean</b>	<b>S.D.</b>	<b>Min</b>	<b>Max</b>
District Size (2000-2008)	7.35	5.62	1.50	28.00
Caseload Pressure (2000-2008)	462.96	135.75	180.78	862.22
Caseload Pressure (2000-2004)	467.71	135.47	168.60	918.00
Caseload Pressure (2005-2008)	457.02	164.52	172.75	1267.50
Change in Caseload Pressure (2000-2008)	4.31	45.77	-41.38	335.63
Change in Caseload Pressure (2000-2004)	10.54	42.71	-31.92	355.76
Change in Caseload Pressure (2005-2008)	-0.56	23.71	-44.72	131.27
Departure Rate (2000-2008)	34.27	10.64	13.79	66.96
Departure Rate (2000-2004)	34.32	10.54	14.01	67.13
Departure Rate (2005-2008)	34.20	10.83	13.25	66.73
Racial Composition (2000-2008)	9.69	12.29	0.07	70.19
Racial Composition (2000-2004)	9.64	12.49	0.05	71.17
Racial Composition (2005-2008)	9.75	12.10	0.10	68.96
Change in Racial Composition (2000-2008)	91.14	321.30	-24.65	2098.94
Change in Racial Composition (2000-2004)	40.77	162.10	-14.80	1027.83
Change in Racial Composition (2005-2008)	12.84	33.94	-10.31	221.04

**Table 10. Unconditional Multilevel Models, Federal Criminal Cases (Excluding Immigration), FY2000-2008**

	Incarceration		Sentence Length		Likelihood of Departure (vs. No Departure)			
					Government-Initiated		Judge-Initiated	
	Pre-Booker	Post-Booker	Pre-Booker	Post-Booker	Pre-Booker	Post-Booker	Pre-Booker	Post-Booker
<b>Variance</b>	.23	.21	.08	.09	.33	.47	.42	.36
<b>Intra-class Correlation</b>	6.5%	6.1%	5.1%	4.6%	9.2%	12.6%	11.2%	10.0%
<b>n</b>	252,907	192,574	211,374	166,515	220,020	174,223	220,020	174,223

**Table 11. Conditional Multilevel Models, Federal Criminal Cases (Excluding Immigration) FY2000-2008**

	Incarceration				Sentence Length			
	Pre-Booker		Post-Booker		Pre-Booker		Post-Booker	
	<i>b</i>	S.D.	<i>b</i>	S.D.	<i>b</i>	S.D.	<i>b</i>	S.D.
District Size (2000-2008)	.007	.01	.010	.01	-.000	.00	-.002	.00
Caseload Pressure†	-.064	.04	-.004	.03	-.004	.01	.010	.01
Change in Caseload Pressure‡	.014	.01	-.004	.02	.001	.00	-.005	.01
Departure Rate‡	.072	.04	.046	.04	-.065	.01 **	-.057	.02 **
Racial Composition‡	.092	.03 **	.101	.03 **	.007	.01	.003	.01
Change in Racial Composition‡	.001	.00	-.001	.01	-.001	.00 *	.002	.00
	<i>N</i> = 252,907		<i>N</i> = 192,574		<i>N</i> = 211,374		<i>N</i> = 166,515	

	Gov.-Initiated Departures				Judge-Initiated Departures			
	Pre-Booker		Post-Booker		Pre-Booker		Post-Booker	
	<i>b</i>	S.D.	<i>b</i>	S.D.	<i>b</i>	S.D.	<i>b</i>	S.D.
District Size (2000-2008)	-.002	.01	-.000	.01	.001	.01	.033	.01 **
Caseload Pressure†	.067	.03 *	.034	.02	.039	.05	-.131	.03 **
Change in Caseload Pressure‡	-.008	.01	-.012	.01	-.016	.01	.064	.02 **
Departure Rate‡	.498	.04 **	.614	.04 **	.555	.04 **	.400	.04 **
Racial Composition‡	.068	.02 **	.031	.02	-.011	.02	-.080	.03 *
Change in Racial Composition‡	.002	.00	-.038	.01 **	.003	.00	.009	.01
	<i>N</i> = 220,020		<i>N</i> = 174,223		<i>N</i> = 220,020		<i>N</i> = 174,223	

\*\*p<.01; \*p<.05

† 100 unit change

‡ 10% change

**Table 12. Summary of Support for Hypotheses**

Hypothesis	Supported?
H1: Incarceration and sentence length differences between whites and Hispanics will emerge as more time passes since the decision.	No
H2: Disparities between whites and minorities in the application of government- and judge-initiated departures will become more apparent with time.	No
H3: Inter-district variation in sentencing outcomes will increase significantly in the post-Booker era.	No
H4: Judges in districts characterized by heavy or increasing caseload pressure will hand down less punitive sentences in the post-Booker period.	Limited support
H5: Historical departure rates will influence the likelihood of individual departures in the post-Booker era.	Limited support
H6-8: Less favorable sentencing outcomes will be handed down in those districts in which blacks constitute a large or growing percentage of the population; the influence of district-level racial composition and growth will be greater in the post-Booker period.	Limited support

**Table 13. Level 1 Descriptive Statistics for Drug Cases, FY2000-2008**

	Full Study Period N=220,316		Pre-Booker N=127,099		Post-Booker N=93,217		t
	Mean	S.D.	Mean	S.D.	Mean	S.D.	
<b>Dependent variables</b>							
Incarceration	.95 (N=220,316)	.22	.94 (N=127,099)	.23	.96 (N=93,217)	.20	14.62 *
Sentence Length	79.10	75.93	75.94	75.21	83.36	76.69	22.10 *
Sentence Length (Logged)	3.90 (N=208,785)	1.14	3.85 (N=119,780)	1.10	3.96 (N=89,005)	1.18	19.93 *
Government-Initiated Downward Departure	.16 (N=188,765)	.37	.14 (N=102,370)	.35	.18 (N=86,395)	.39	25.51 *
Judge-Initiated Downward Departure	.16 (N=188,765)	.37	.14 (N=102,370)	.34	.19 (N=86,395)	.39	30.96 *
<b>Independent variables</b>							
Booker decision	.42	.49	--	--	--	--	--
Marijuana/Hashish (reference category)	.27	.44	.29	.45	.24	.43	22.59 *
Cocaine	.19	.39	.19	.39	.20	.40	4.37 *
Crack	.16	.37	.16	.37	.17	.37	1.12
Amphetamines	.16	.36	.15	.35	.17	.38	17.41 *
Opiates/Opioids	.06	.25	.07	.25	.06	.24	6.78 *
Psychedelics	.02	.15	.03	.16	.02	.14	9.19 *
Other Drug Types/ Paraphernalia	.01	.08	.01	.08	.01	.07	2.31
Multiple Drug Types	.11	.31	.10	.30	.12	.33	18.67 *
Drug Type Missing	.02	.13	.02	.14	.02	.12	8.02 *
Trafficking	.96	.19	.96	.20	.97	.18	10.47 *
Possession (reference category)	.02	.15	.02	.15	.02	.13	11.55 *
Communications Facility	.02	.13	.02	.13	.02	.12	2.53
White (reference category)	.26	.44	.27	.44	.26	.44	5.42 *
Black	.30	.46	.30	.46	.31	.46	4.86 *
Hispanic	.38	.49	.39	.49	.38	.49	4.37 *
Other Race	.03	.18	.02	.15	.04	.20	23.23 *
Race Information Missing	.02	.13	.02	.14	.01	.11	13.65 *
Age of Defendant	32.96	9.80	32.67	9.81	33.35	9.77	15.90 *
Male	.87	.34	.87	.34	.87	.33	4.39 *
Defendant is a Non-Citizen	.29	.45	.29	.46	.28	.45	4.49 *
Citizenship Status Missing	.01	.12	.02	.13	.01	.11	9.74 *
Offender has Dependents	.62	.48	.62	.49	.64	.48	9.89 *
Information on Dependents Missing	.04	.19	.04	.20	.03	.18	7.64 *
No High School Diploma	.48	.50	.49	.50	.48	.50	5.47 *
High School Graduate (reference category)	.32	.47	.31	.46	.33	.47	6.72 *
Some College	.14	.35	.14	.35	.15	.35	1.98
College Graduate	.03	.16	.03	.16	.03	.16	1.97
Education Information Missing	.03	.16	.03	.17	.02	.15	8.95 *
Criminal History Category	2.23	1.66	2.17	1.61	2.32	1.72	21.48 *
Presumptive Guideline Length	92.44	84.74	88.39	83.27	97.95	86.39	26.19 *
Trial	.04	.20	.04	.20	.04	.21	4.40 *
Presentence Detention	.74	.44	.71	.45	.77	.42	28.90 *
Block of Departure Dummy Variables	--	--	--	--	--	--	--
Block of District Dummy Variables	--	--	--	--	--	--	--

\* p<.01

**Table 14. Logistic Regression Models for Incarceration Decision, Drug Cases, FY2000-2008**

Independent Variables	Full Study Period N=220,316			Pre-Booker N=127,099			Post-Booker N=93,217			Ratio of Coefficients	Chi-Square for Difference
	$\beta$	S.E.	Exp ( $\beta$ )	$\beta$	S.E.	Exp ( $\beta$ )	$\beta$	S.E.	Exp ( $\beta$ )		
Constant	-2.00	.08	-- **	-1.94	.10	-- **	-1.90	.14	-- **	--	--
Booker Decision	.20	.03	1.22 **	--	--	--	--	--	--	--	--
Cocaine	.30	.04	1.34 **	.31	.05	1.36 **	.24	.07	1.27 **	.77	.65
Crack	.36	.06	1.43 **	.42	.07	1.52 **	.17	.10	1.19	.41	4.14 *
Amphetamines	.37	.05	1.45 **	.42	.06	1.53 **	.17	.08	1.18 *	.40	5.96 *
Opiates/Opioids	.14	.06	1.15 *	.26	.07	1.29 **	-.02	.09	.99	-.06	5.27 *
Psychedelics	-.02	.06	.99	.03	.08	1.03	-.27	.12	.76 *	-10.84	4.41 *
Other Drug Types/ Paraphernalia	-1.21	.08	.30 **	-1.36	.10	.26 **	-1.07	.13	.34 **	.79	3.08
Multiple Drug Types	.24	.06	1.27 **	.34	.07	1.41 **	.04	.09	1.04	.12	6.63 **
Trafficking	2.43	.05	11.36 **	2.61	.06	13.66 **	2.10	.09	8.14 **	.80	22.91 **
Communications Facility	1.43	.08	4.17 **	1.64	.10	5.13 **	1.07	.13	2.90 **	.65	12.22 **
Black	.17	.04	1.19 **	.24	.05	1.27 **	.06	.06	1.06	.24	5.04 *
Hispanic	.39	.04	1.48 **	.38	.05	1.46 **	.39	.07	1.48 **	1.03	.02
Other Race	.09	.06	1.09	-.07	.09	.93	.27	.10	1.31 **	-3.66	6.73 **
Age of Defendant	-.00	.00	1.00 **	-.00	.00	1.00	-.01	.00	.99 **	--	6.13 *
Male	.55	.03	1.74 **	.56	.04	1.76 **	.54	.05	1.71 **	.95	.19
Defendant is a Non-Citizen	1.29	.05	3.65 **	1.40	.06	4.06 **	1.09	.08	2.98 **	.78	9.27 **
Offender has Dependents	.04	.03	1.04	.01	.03	1.01	.08	.05	1.08	9.75	1.54
No High School Diploma	.22	.03	1.25 **	.26	.04	1.29 **	.16	.05	1.18 **	.64	1.95
Some College	-.12	.03	.89 **	-.11	.04	.90 *	-.10	.06	.90	.94	.01
College Graduate	-.25	.06	.78 **	-.26	.08	.77 **	-.22	.10	.81 *	.83	.12
Criminal History Category	.32	.02	1.37 **	.31	.02	1.36 **	.34	.03	1.41 **	1.10	.88
Presumptive Guideline Length	.04	.00	1.04 **	.04	.00	1.04 **	.06	.00	1.06 **	1.71	312.50 **
Trial	.50	.14	1.66 **	.46	.17	1.58 **	.42	.24	1.53	.92	.02
Presentence Detention	1.57	.03	4.78 **	1.58	.04	4.87 **	1.54	.05	4.67 **	.97	.41
Government-Initiated Downward Departure	-1.58	.03	.21 **	-1.65	.04	.19 **	-1.60	.06	.20 **	.97	.52
Judge-Initiated Downward Departure	-1.34	.04	.26 **	-1.42	.05	.24 **	-1.26	.07	.28 **	.88	4.10 *
Upward Departure	2.07	.31	7.94 **	2.14	.44	8.52 **	2.15	.45	8.56 **	1.00	.00
Block of District Dummy Variables	--	--	--	--	--	--	--	--	--	--	--

Ref. Categories: Marijuana/Hashish, Possession, White, Female, Citizen, High School Graduate, No Departure

\*\*p<.01; \*p<.05

**Table 15. OLS Regression Models for Sentence Length (Logged) Decision, Drug Cases, FY2000-2008**

Independent Variables	Full Study Period N=208,785			Pre-Booker N=119,780			Post-Booker N=89,005			z
	$\beta$	S.E	Exp ( $\beta$ )	$\beta$	S.E	Exp ( $\beta$ )	$\beta$	S.E	Exp ( $\beta$ )	
Constant	.83	.02	-- **	.94	.02	-- **	.68	.03	-- **	-1.43
Booker Decision	.01	.00	1.01 **	--	--	--	--	--	--	--
Cocaine	.43	.01	1.54 **	.40	.01	1.49 **	.46	.01	1.58 **	1.15
Crack	.45	.01	1.57 **	.44	.01	1.56 **	.45	.01	1.57 **	.20
Amphetamines	.49	.01	1.64 **	.46	.01	1.58 **	.52	.01	1.69 **	1.00
Opiates/Opioids	.36	.01	1.44 **	.35	.01	1.42 **	.36	.01	1.43 **	.17
Psychedelics	.32	.01	1.38 **	.27	.01	1.31 **	.38	.02	1.46 **	1.79 *
Other Drug Types/ Paraphernalia	-.38	.03	.69 **	-.37	.03	.69 **	-.37	.05	.69 **	-.01
Multiple Drug Types	.43	.01	1.54 **	.42	.01	1.52 **	.44	.01	1.55 **	.42
Trafficking	1.58	.02	4.87 **	1.54	.02	4.65 **	1.62	.03	5.05 **	.24
Communications Facility	1.42	.02	4.15 **	1.44	.02	4.20 **	1.36	.04	3.91 **	-.22
Black	.03	.01	1.03 **	.02	.01	1.02 **	.05	.01	1.05 **	6.98 **
Hispanic	.04	.01	1.04 **	.04	.01	1.04 **	.05	.01	1.05 **	1.11
Other Race	.02	.01	1.02 *	.01	.01	1.01	.04	.02	1.04 **	5.57 **
Age of Defendant	.00	.00	1.00 **	.00	.00	1.00 **	.00	.00	1.00 **	.00
Male	.20	.01	1.22 **	.19	.01	1.20 **	.21	.01	1.23 **	1.02
Defendant is a Non-Citizen	-.01	.00	.99 **	-.03	.01	.97 **	.00	.01	1.00	14.19 **
Offender has Dependents	.04	.00	1.04 **	.03	.00	1.03 **	.04	.01	1.04 **	.89
No High School Diploma	-.00	.00	1.00	-.01	.00	.99 *	.00	.01	1.00	18.65 **
Some College	-.02	.01	.98 **	-.01	.01	1.00	-.03	.01	.97 **	-9.41 **
College Graduate	-.04	.01	.97 **	-.03	.01	.97 *	-.03	.02	.97 *	-.36
Criminal History Category	.04	.00	1.04 **	.05	.00	1.05 **	.04	.00	1.04 **	-2.60 **
Presumptive Guideline Length	.01	.00	1.01 **	.01	.00	1.01 **	.01	.00	1.01 **	2.97 **
Trial	.02	.01	1.02 **	.03	.01	1.03 **	-.00	.01	1.00	-10.54 **
Presentence Detention	.27	.00	1.31 **	.22	.01	1.24 **	.36	.01	1.43 **	4.16 **
Government-Initiated Downward Departure	-.57	.00	.57 **	-.54	.01	.59 **	-.62	.01	.54 **	-1.38
Judge-Initiated Downward Departure	-.48	.01	.62 **	-.51	.01	.60 **	-.37	.01	.69 **	2.87 **
Upward Departure	.45	.02	1.57 **	.52	.03	1.68 **	.40	.03	1.49 **	-1.01
Block of District Dummy Variables	--	--	--	--	--	--	--	--	--	--

Ref. Categories: Marijuana/Hashish, Possession, White, Female, Citizen, High School Graduate, No Departure

\*\*p<.01; \*p<.05

**Table 16. Multinomial Logistic Regression Models for Departure Decision, Drug Cases, FY2000-2008**

Independent Variables	Full Study Period N=184,967							
	Government-Initiated				Judge-Initiated			
	$\beta$	S.E.	Exp ( $\beta$ )		$\beta$	S.E.	Exp ( $\beta$ )	
Intercept	-8.63	3.88	--	*	-12.16	3.75	--	**
<i>Booker</i> Decision	.53	.01	1.70	**	.60	.01	1.82	**
Cocaine	.27	.08	1.31	**	.05	.09	1.05	
Crack	.12	.08	1.12		.02	.09	1.02	
Amphetamines	.22	.08	1.24	**	-.12	.09	.89	
Opiates/Opioids	.13	.13	1.14		-.09	.14	.91	
Psychedelics	.15	.20	1.16		.36	.19	1.44	
Other Drug Types/ Paraphernalia	.04	.16	1.04		-.35	.17	.70	*
Multiple Drug Types	.11	.08	1.11		-.17	.09	.84	*
Trafficking	--	--	--		--	--	--	
Communications Facility	--	--	--		--	--	--	
Black	-.09	.02	.92	**	-.17	.02	.84	**
Hispanic	-.14	.03	.87	**	-.08	.03	.92	**
Other Race	--	--	--		--	--	--	
Age of Defendant	-.00	.00	1.00		.01	.00	1.01	**
Male	-.26	.02	.77	**	-.32	.02	.73	**
Defendant is a Non-Citizen	.14	.03	1.15	**	-.10	.03	.91	**
Offender has Dependents	.16	.01	1.17	**	.01	.01	1.01	
No High School Diploma	-.08	.02	.92	**	-.04	.02	.96	*
Some College	.08	.02	1.09	**	.03	.02	1.03	
College Graduate	.30	.02	1.34	**	.16	.02	1.17	**
Criminal History Category	-.12	.01	.89	**	-.08	.01	.92	**
Presumptive Guideline Length	.01	.00	1.01	**	.00	.00	1.00	**
Trial	-2.70	.06	.07	**	-.34	.03	.71	**
Presentence Detention	-.86	.02	.42	**	-.83	.02	.44	**
Block of District Dummy Variables	--	--	--		--	--	--	

Ref. Categories: Marijuana/Hashish, Possession, White, Female, Citizen, High School Graduate, No Departure

\*\*p<.01; \*p<.05

**Table 17. Pre- and Post-Booker Multinomial Logistic Regression Models for Departure Decision, Drug Cases, FY2000-2008**

Independent Variables	Government-Initiated									Judge-Initiated							
	Pre-Booker N=100,869			Post-Booker N=84,098			Ratio of Coef- ficients	Chi- Square for Difference	Pre-Booker N=100,869			Post-Booker N=84,098			Ratio of Coef- ficients	Chi- Square for Difference	
	$\beta$	S.E.	Exp ( $\beta$ )	$\beta$	S.E.	Exp ( $\beta$ )			$\beta$	S.E.	Exp ( $\beta$ )	$\beta$	S.E.	Exp ( $\beta$ )			
Intercept	-31.76	5.40	-- **	10.69	5.60	--	--	--	--	-29.57	5.27	-- **	1.69	5.35	--	--	--
Cocaine	.27	.13	1.30 *	.23	.11	1.26 *	.86	.05	.04	.12	1.04	.21	.12	1.23	5.57	.94	
Crack	.15	.13	1.16	.07	.11	1.07	.43	.26	-.04	.13	.96	.20	.12	1.23	-5.67	1.81	
Amphetamines	.34	.12	1.40 **	.10	.11	1.10	.28	2.21	-.13	.12	.88	-.03	.13	.97	.21	.35	
Opiates/Opioids	.15	.20	1.17	.10	.17	1.11	.66	.04	-.32	.21	.72	.18	.19	1.19	-.55	3.13	
Psychedelics	.22	.30	1.24	.02	.27	1.02	.09	.25	.22	.28	1.25	.58	.27	1.79 *	2.62	.87	
Other Drug Types/ Paraphernalia	.02	.24	1.02	.01	.21	1.01	.50	.00	-.30	.25	.74	-.28	.24	.75	.95	.00	
Multiple Drug Types	.10	.14	1.10	.05	.10	1.05	.48	.08	-.20	.14	.82	.02	.12	1.02	-.12	1.53	
Trafficking	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	
Communications Facility	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	
Black	-.10	.02	.90 **	-.07	.03	.93 **	.66	1.02	-.21	.03	.81 **	-.15	.02	.86 **	.70	3.20	
Hispanic	-.20	.04	.82 **	-.09	.04	.91 **	.46	4.63 *	-.08	.04	.93 *	-.10	.04	.90 **	1.36	.30	
Other Race	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	
Age of Defendant	-.00	.00	1.00	-.00	.00	1.00	--	.00	.01	.00	1.01 **	.01	.00	1.01 **	.83	.50	
Male	-.20	.03	.82 **	-.33	.03	.72 **	1.65	11.84 **	-.35	.03	.71 **	-.29	.03	.75 **	.83	2.57	
Defendant is a Non-Citizen	.13	.04	1.14 **	.15	.04	1.17 **	1.18	.24	-.02	.04	.98	-.19	.04	.83 **	10.05	11.41 **	
Offender has Dependents	.10	.02	1.10 **	.22	.02	1.24 **	2.28	18.61 **	.06	.02	1.06 **	-.02	.02	.98	-.29	7.03 **	
No High School Diploma	-.06	.03	.94 *	-.10	.03	.91 **	1.63	1.16	-.02	.03	.98	-.06	.03	.94 *	3.00	1.41	
Some College	.06	.03	1.06 *	.11	.03	1.12 **	1.85	1.85	.04	.03	1.04	.03	.03	1.03	.63	.17	
College Graduate	.24	.03	1.27 **	.36	.04	1.43 **	1.52	6.54 *	.20	.03	1.23 **	.12	.03	1.13 **	.61	2.94	
Criminal History Category	-.11	.01	.90 **	-.13	.01	.88 **	1.20	4.94 *	-.05	.01	.95 **	-.12	.01	.89 **	2.27	43.11 **	
Presumptive Guideline Length	.01	.00	1.01 **	.01	.00	1.01 **	1.20	.50	.00	.00	1.00 **	.00	.00	1.00 **	.50	2.00	
Trial	-2.20	.08	.11 **	-3.29	.11	.04 **	1.49	68.31 **	-.91	.05	.40 **	.02	.04	1.02	-.02	227.36 **	
Presentence Detention	-.89	.02	.41 **	-.84	.02	.43 **	.94	2.17	-.77	.02	.46 **	-.89	.02	.41 **	1.16	13.91 **	
Block of District Dummies	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	

Ref. Categories: Marijuana/Hashish, Possession, White, Female, Citizen, High School Graduate, No Departure

\*\*p<.01; \*p<.05

**Table 18. Level 2 Descriptive Statistics for Drug Cases, FY 2000-2008**

<i>District-level Variables (n=90)</i>	<b>Mean</b>	<b>S.D.</b>	<b>Min</b>	<b>Max</b>
District Size (2000-2008)	7.35	5.62	1.50	28.00
Relative Drug Caseload (2000-2008)	39.66	9.69	19.86	65.18
Relative Drug Caseload (2000-2004)	39.80	9.74	19.99	64.82
Relative Drug Caseload (2005-2008)	39.48	9.65	19.70	65.62
Change in Relative Drug Caseload (2000-2008)	-2.35	15.91	-48.47	32.93
Change in Relative Drug Caseload (2000-2004)	-3.00	86.98	-56.14	30.84
Change in Relative Drug Caseload (2005-2008)	0.71	3.04	-13.99	8.39
Departure Rate (2000-2008)	34.27	10.64	13.79	66.96
Departure Rate (2000-2004)	34.32	10.54	14.01	67.13
Departure Rate (2005-2008)	34.20	10.83	13.25	66.73
Racial Composition (2000-2008)	9.69	12.29	0.07	70.19
Racial Composition (2000-2004)	9.64	12.49	0.05	71.17
Racial Composition (2005-2008)	9.75	12.10	0.10	68.96
Change in Racial Composition (2000-2008)	91.14	321.30	-24.65	2098.94
Change in Racial Composition (2000-2004)	40.77	162.10	-14.80	1027.83
Change in Racial Composition (2005-2008)	12.84	33.94	-10.31	221.04

**Table 19. Unconditional Multilevel Models, Drug Cases, FY2000-2008**

	Incarceration		Sentence Length		Likelihood of Departure (vs. No Departure)			
	Pre-Booker	Post-Booker	Pre-Booker	Post-Booker	Government-Initiated		Judge-Initiated	
					Pre-Booker	Post-Booker	Pre-Booker	Post-Booker
<b>Variance</b>	.55	.50	.13	.16	.25	.37	.30	.31
<b>Intra-class Correlation</b>	14.2%	13.1%	12.0%	11.8%	7.0%	10.1%	8.4%	8.6%
<b>n</b>	127,099	93,217	119,780	89,005	100,869	84,098	100,869	84,098

**Table 20. Conditional Multilevel Models, Drug Cases, FY2000-2008**

	Incarceration				Sentence Length			
	Pre-Booker		Post-Booker		Pre-Booker		Post-Booker	
	<i>b</i>	S.D.	<i>b</i>	S.D.	<i>b</i>	S.D.	<i>b</i>	S.D.
District Size (2000-2008)	.012	.01	.009	.01	-.005	.00 *	-.006	.00
Relative Drug Caseload†	-.926	.68	-.756	.69	-.187	.14	-.411	.22
Change in Relative Drug Caseload‡	-.068	.04	-.024	.23	.005	.01	.022	.06
Departure Rate‡	.093	.06	.000	.06	-.069	.01 **	-.069	.02 **
Racial Composition‡	.142	.07 *	.126	.05 *	-.007	.02	-.002	.03
Change in Racial Composition‡	.006	.00 *	-.014	.03	-.001	.00 **	.006	.00
	<i>N</i> = 127,099		<i>N</i> = 93,217		<i>N</i> = 119,780		<i>N</i> = 89,005	

	Gov.-Initiated Departures				Judge-Initiated Departures			
	Pre-Booker		Post-Booker		Pre-Booker		Post-Booker	
	<i>b</i>	S.D.	<i>b</i>	S.D.	<i>b</i>	S.D.	<i>b</i>	S.D.
District Size (2000-2008)	.013	.01 *	.019	.01 **	.001	.01	.019	.01 **
Relative Drug Caseload†	.018	.46	-.469	.36	-.694	.41	-.330	.51
Change in Relative Drug Caseload‡	.044	.02 *	.027	.10	.034	.02	-.091	.14
Departure Rate‡	.344	.04 **	.460	.04 **	.436	.04 **	.319	.05 **
Racial Composition‡	.011	.03	.018	.02	-.027	.03	-.073	.03 *
Change in Racial Composition‡	-.002	.00	-.003	.01	.003	.00 **	.010	.01
	<i>N</i> = 100,869		<i>N</i> = 84,098		<i>N</i> = 100,869		<i>N</i> = 84,098	

\*\*p<.01; \*p<.05

† 100 unit change

‡ 10% change

**Table 21. Summary of Support for Hypotheses, Drug Offenses**

Hypothesis	Supported?
H9: Sentencing outcomes for crack will become more comparable to those found in cases involving powder cocaine in the post-Booker era.	Limited
H10: Disparities between whites and minorities in drug cases will decrease in the post-Booker era, particularly in more recent years of data.	No
H11-12: Judges in those districts characterized by heavy or increasing relative drug caseloads will grant favorable sentencing outcomes to drug offenders in the post-Booker era.	No

**Table 22. Level 1 Descriptive Statistics for Immigration Cases FY2000-2008**

	<b>Full Study Period N=124,611</b>		<b>Pre-Booker N=59,171</b>		<b>Post-Booker N=65,440</b>		
<i>Dependent variables</i>	<b>Mean</b>	<b>S.D.</b>	<b>Mean</b>	<b>S.D.</b>	<b>Mean</b>	<b>S.D.</b>	<b>  t  </b>
Incarceration	.96	.19	.96	.20	.97	.18	9.15 **
Sentence Length	24.51	21.06	26.74	22.14	22.50	19.82	34.72 **
Sentence Length (Logged)	2.76	1.07	2.9	1.03	2.66	1.09	35.55 **
	(N=118,253)		(N=56,065)		(N=62,188)		
Government-Initiated Downward Departure	.18	.38	.12	.32	.33	.47	85.78 **
	(N=107,354)		(N=51,530)		(N=55,824)		
Judge-Initiated Downward Departure	.23	.42	.26	.44	.10	.29	74.53 **
	(N=107,354)		(N=51,530)		(N=55,824)		
<i>Independent variables</i>							
Booker decision	.53	.50	--	--	--	--	--
Hispanic	.80	.40	.84	.37	.77	.42	29.99 **
Age of Defendant	33.16	8.88	32.71	8.68	33.56	9.04	16.92 **
Male	.93	.25	.94	.24	.93	.26	6.85 **
Defendant is a Non-Citizen	.88	.33	.88	.33	.88	.33	.28
Citizenship Status Missing	.02	.13	.01	.12	.02	.14	7.28 **
Offender has Dependents	.59	.49	.60	.49	.58	.49	6.93 **
Information on Dependents Missing	.14	.35	.11	.32	.16	.37	26.44 **
No High School Diploma	.67	.47	.68	.47	.67	.47	5.91 **
Criminal History Category	2.88	1.63	3.01	1.66	2.76	1.59	27.43 **
Presumptive Guideline Length	25.63	23.13	28.26	24.22	23.24	21.82	38.47 **
Trial	.01	.11	.01	.11	.01	.11	2.02 *
Presentence Detention	.88	.33	.88	.33	.88	.33	.34
Border District	.73	.44	.71	.45	.75	.43	17.56 **
Block of Departure Dummy Variables	--	--	--	--	--	--	--
Block of District Dummy Variables	--	--	--	--	--	--	--

\*\*p<.01; \*p<.05

**Table 23. OLS Regression Models for Sentence Length (Logged) Decision, Immigration Cases FY2000-2008**

Independent Variables	Full Study Period N=118,253				Pre-Booker N=56,065				Post-Booker N=62,188				z
	$\beta$	S.E.	Exp ( $\beta$ )		$\beta$	S.E.	Exp ( $\beta$ )		$\beta$	S.E.	Exp ( $\beta$ )		
Constant	1.23	.01	--	**	1.40	.02	--	**	1.10	.02	--	**	-1.28
Booker Decision	-.02	.00	.98	**	--	--	--		--	--	--		--
Hispanic	.18	.01	1.19	**	.04	.01	1.04	**	.22	.01	1.24	**	9.51 **
Age of Defendant	.00	.00	1.00	**	.00	.00	1.00	**	.00	.00	1.00	**	--
Male	.17	.01	1.19	**	.14	.01	1.15	**	.19	.01	1.21	**	2.02 *
Defendant is a Non-Citizen	-.20	.01	.82	**	-.14	.01	.87	**	-.22	.01	.80	**	-3.56 **
Offender has Dependents	.03	.00	1.03	**	.03	.01	1.03	**	.04	.01	1.04	**	1.87 *
No High School Diploma	.11	.00	1.12	**	.10	.01	1.11	**	.12	.01	1.13	**	1.87 *
Criminal History Category	.09	.00	1.09	**	.09	.00	1.09	**	.09	.00	1.09	**	.36
Presumptive Guideline Length	.03	.00	1.03	**	.03	.00	1.03	**	.04	.00	1.04	**	--
Trial	.04	.02	1.04	*	.04	.02	1.04		.00	.02	1.00		-6.38 **
Presentence Detention	.10	.01	1.10	**	.05	.01	1.05	**	.15	.01	1.16	**	6.45 **
Government-Initiated Departure	-.19	.01	.83	**	-.17	.01	.85	**	-.23	.01	.79	**	-3.04 **
Judge-Initiated Downward Departure	-.30	.01	.74	**	-.26	.01	.77	**	-.32	.01	.73	**	-1.46
Upward Departure	.73	.02	2.08	**	.62	.04	1.86	**	.76	.02	2.13	**	.90
Border District	.02	.00	1.02	**	.02	.01	1.02	**	.04	.01	1.04	**	5.97 **

Reference Categories: Non-Hispanic, Female, Citizen, High School Diploma or Higher, No Departure

\*\*p<.01; \*p<.05

**Table 24. Multinomial Logistic Regression Models for Departure Decision, Immigration Cases, FY2000-2008**

Independent Variables	Full Study Period N=106,302							
	Government-Initiated				Judge-Initiated			
	B	S.E.	Exp( $\beta$ )		$\beta$	S.E.	Exp( $\beta$ )	
Intercept	-1.67	.13	--	**	-1.45	.13	--	**
<i>Booker</i> Decision	1.46	.04	4.32	**	.39	.04	1.48	**
Hispanic	-.09	.04	.91	*	-.04	.04	.96	
Age of Defendant	-.01	.00	.99	**	-.00	.00	1.00	
Male	-.60	.08	.55	**	-.66	.08	.52	**
Defendant is a Non-Citizen	-.40	.08	.67	**	-.02	.08	.98	
Offender has Dependents	-.05	.04	.95	**	.09	.04	1.10	*
No High School Diploma	-.19	.04	.83	**	-.10	.04	.90	*
Criminal History Category	-.07	.01	.94	**	-.17	.01	.85	**
Presumptive Guideline Length	.03	.00	1.03	**	.03	.00	1.03	**
Trial	-2.39	.28	.09	**	-.82	.13	.44	**
Presentence Detention	-.38	.07	.69	**	-.40	.07	.67	**
Border District	1.06	.05	2.90	**	1.08	.04	2.94	**

Reference Categories: Non-Hispanic, Female, Citizen, High School Diploma or Higher

\*\*p<.01; \*p<.05

**Table 25. Pre- and Post-Booker Multinomial Logistic Regression Models for Departure Decision, Immigration Cases, FY2000-2008**

Independent Variables	Government-Initiated						Judge-Initiated									
	Pre-Booker N=51,306			Post-Booker N=54,996			Ratio of Coef- ficients	Chi- Square for Difference	Pre-Booker N=51,306			Post-Booker N=54,996			Ratio of Coef- Difference	Chi- Square for Difference
$\beta$	S.E.	Exp( $\beta$ )	$\beta$	S.E.	Exp( $\beta$ )	$\beta$			S.E.	Exp( $\beta$ )	$\beta$	S.E.	Exp( $\beta$ )			
Intercept	-1.36	.21	-- **	-.35	.16	-- *	--		-1.57	.19	-- **	-.99	.18	-- **	--	
Hispanic	.02	.08	1.02	-.13	.05	.88 *	-6.60	2.66	.06	.06	1.07	-.14	.06	.87 *	-2.22	5.80 *
Age of Defendant	-.00	.00	1.00	-.01	.00	.99 **	8.00	2.56	-.01	.00	.99 **	.01	.00	1.01	-.56	10.89 **
Male	-.65	.13	.52 **	-.59	.10	.56 **	.91	.13	-.55	.12	.57 **	-.75	.10	.47 **	1.36	1.57
Defendant is a Non-Citizen	-.41	.12	.66 **	-.38	.10	.69 **	.92	.04	-.01	.12	.99	-.01	.12	.99	1.14	.00
Offender has Dependents	.19	.07	1.20 *	-.17	.05	.85 **	-.90	15.82 **	-.01	.05	.99	.21	.06	1.23 **	-34.50	7.22 **
No High School Diploma	-.21	.07	.81 **	-.19	.05	.83 **	.89	.08	-.07	.06	.94	-.16	.06	.86 **	2.31	1.23
Criminal History Category	-.25	.03	.78 **	.02	.02	1.02	-.09	80.95 **	-.16	.02	.85 **	-.17	.02	.84 **	1.11	.36
Presumptive Guideline Length	.02	.00	1.02 **	.03	.00	1.03 **	1.17	3.20	.03	.00	1.03 **	.03	.00	1.03 **	.85	12.50 **
Trial	-1.77	.37	.17 **	-2.85	.42	.06 **	1.61	3.69	-1.16	.21	.31 **	-.49	.18	.61 **	.42	6.09 *
Presentence Detention	-.32	.12	.73 **	-.39	.09	.68 **	1.23	.23	-.42	.11	.66 **	-.39	.11	.68 **	.93	.04
Border District	1.35	.07	3.85 **	.82	.06	2.27 **	.61	31.42 **	1.48	.06	4.40 **	.53	.07	1.70 **	.36	110.61 **

Reference Categories: Non-Hispanic, Female, Citizen, High School Diploma or Higher

\*\*p<.01; \*p<.05

**Table 26. Level 2 Descriptive Statistics for Immigration Cases, FY2000-2008**

<i>District-level variables (n=90)</i>	<b>Mean</b>	<b>S.D.</b>	<b>Min</b>	<b>Max</b>
District Size (2000-2008)	7.35	5.62	1.50	28.00
Caseload pressure (2000-2008)	462.96	135.75	180.78	862.22
Caseload pressure (2000-2004)	467.71	135.47	168.60	918.00
Caseload pressure (2005-2008)	457.02	164.52	172.75	1267.50
Change in caseload pressure (2000-2008)	4.31	45.77	-41.38	335.63
Change in caseload pressure (2000-2004)	10.54	42.71	-31.92	355.76
Change in caseload pressure (2005-2008)	-.56	23.71	-44.72	131.27
Departure Rate (2000-2008)	34.27	10.64	13.79	66.96
Departure Rate (2000-2004)	34.32	10.54	14.01	67.13
Departure Rate (2005-2008)	34.20	10.83	13.25	66.73

**Table 27. Unconditional Multilevel Models, Immigration Cases, FY2000-2008**

	Sentence Length		Likelihood of Departure (vs. No Departure)			
			Government-Initiated		Judge-Initiated	
	<i>Pre-Booker</i>	<i>Post-Booker</i>	<i>Pre-Booker</i>	<i>Post-Booker</i>	<i>Pre-Booker</i>	<i>Post-Booker</i>
<b>Variance</b>	.22	.36	.98	1.65	.94	.52
<b>Intra-class Correlation</b>	18.6%	26.1%	23.0%	33.4%*	22.3%	13.6%*
<b>n</b>	56,065	62,188	51,306	54,996	51,306	54,996

**Table 28. Conditional Multilevel Models, Immigration Cases, FY2000-2008**

	<b>Incarceration</b>			
	<i>Pre-Booker</i>		<i>Post-Booker</i>	
	<i>b</i>	S.D.	<i>b</i>	S.D.
District Size (2000-2008)	.003	.00	.008	.00
Caseload Pressure†	.022	.02	.014	.02
Change in Caseload Pressure‡	-.009	.00 **	-.007	.01
Departure Rate‡	.002	.02	-.014	.02
	<i>N = 56,065</i>		<i>N = 62,188</i>	

	<b>Gov.-Initiated Departure</b>				<b>Judge-Initiated Departure</b>			
	<i>Pre-Booker</i>		<i>Post-Booker</i>		<i>Pre-Booker</i>		<i>Post-Booker</i>	
	<i>b</i>	S.D.	<i>b</i>	S.D.	<i>b</i>	S.D.	<i>b</i>	S.D.
District Size (2000-2008)	-.013	.01	.002	.03	.002	.01	.017	.01
Caseload Pressure†	.062	.09	.342	.11 **	-.048	.08	-.093	.06
Change in Caseload Pressure‡	-.004	.02	-.182	.06 **	.020	.03	.068	.03 *
Departure Rate‡	.589	.12 **	.731	.10 **	.585	.11 **	.357	.09 **
	<i>N = 51,306</i>				<i>N = 54,996</i>			

\*\*p<.01; \*p<.05

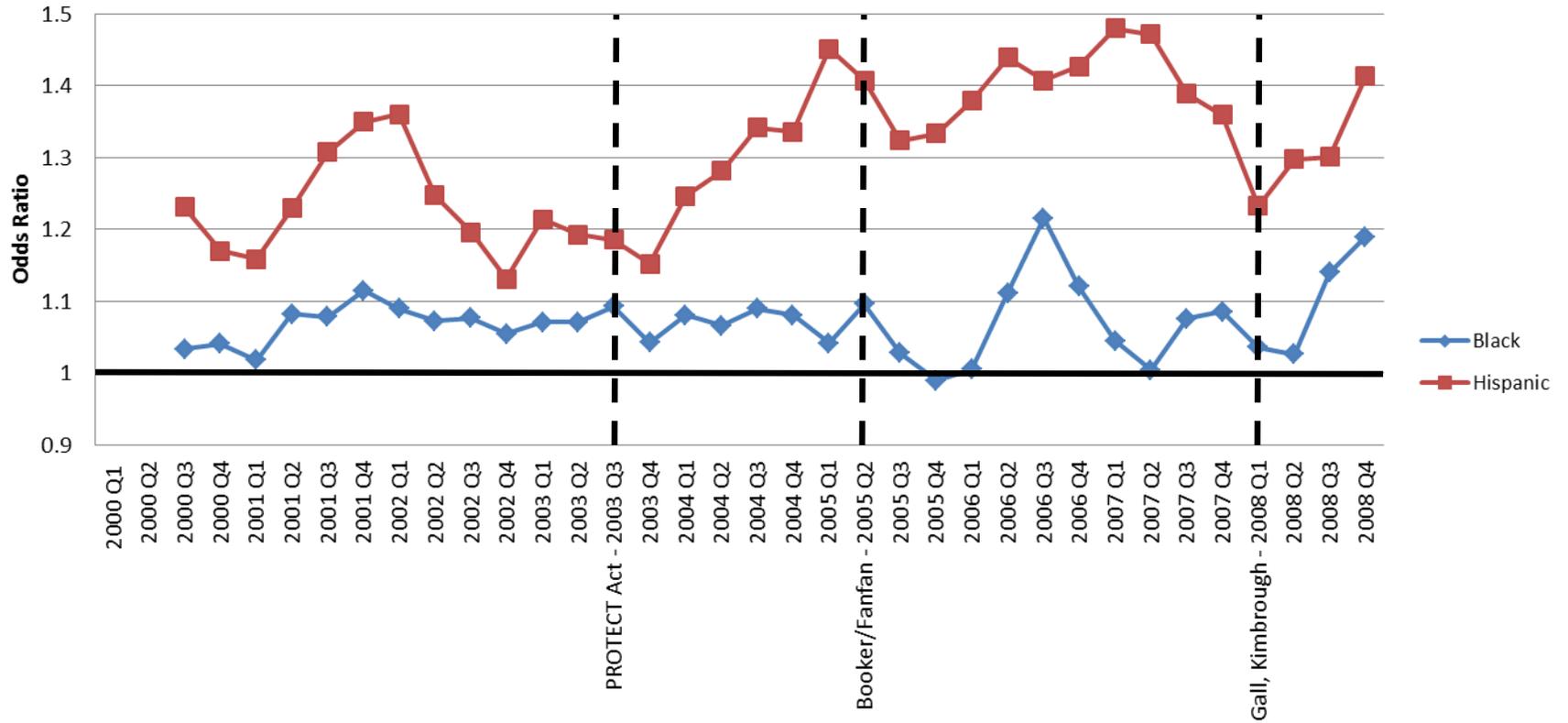
† 100 unit change

‡ 10% change

**Table 29. Summary of Support for Hypotheses, Immigration Offenses**

<b>Hypothesis</b>	<b>Supported?</b>
H13: Sanctions for immigration offenses will exhibit differences in the post- <i>Booker</i> period.	Yes
H14: Disparities in sentencing outcomes will be evident between non-Hispanics and Hispanics in the post- <i>Booker</i> period, particularly in more recent years of data.	Limited
H15: Inter-district variation in sentencing outcomes for immigration offenses exists and will increase significantly in the post- <i>Booker</i> era.	Limited

**Figure 1. Quarterly Odds Ratios - Incarceration Decision (Three Quarter Moving Average)**



**Figure 2. Quarterly Percent Differences,  
Logged Sentence Length (Three Quarter Moving Average)**

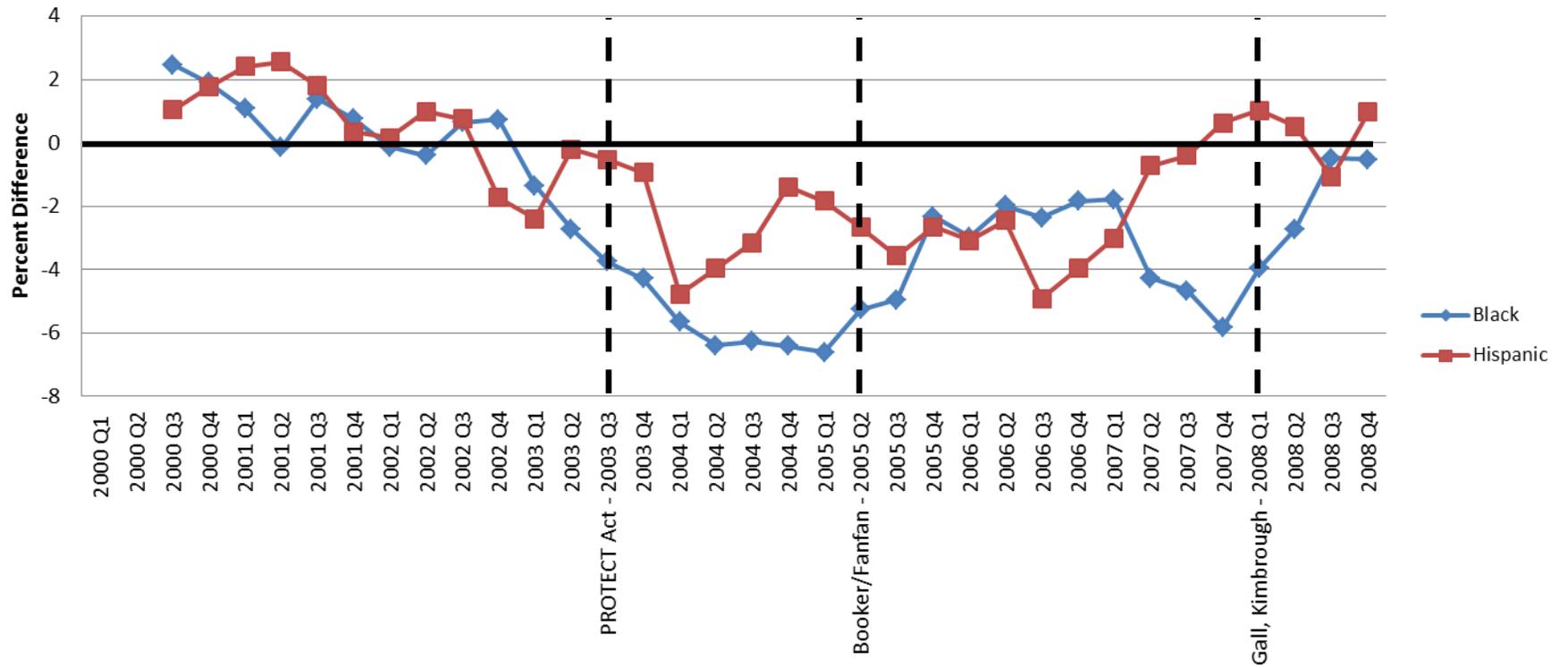


Figure 3. Quarterly Odds Ratios - Government-Initiated Departures (Three Quarter Moving Average)

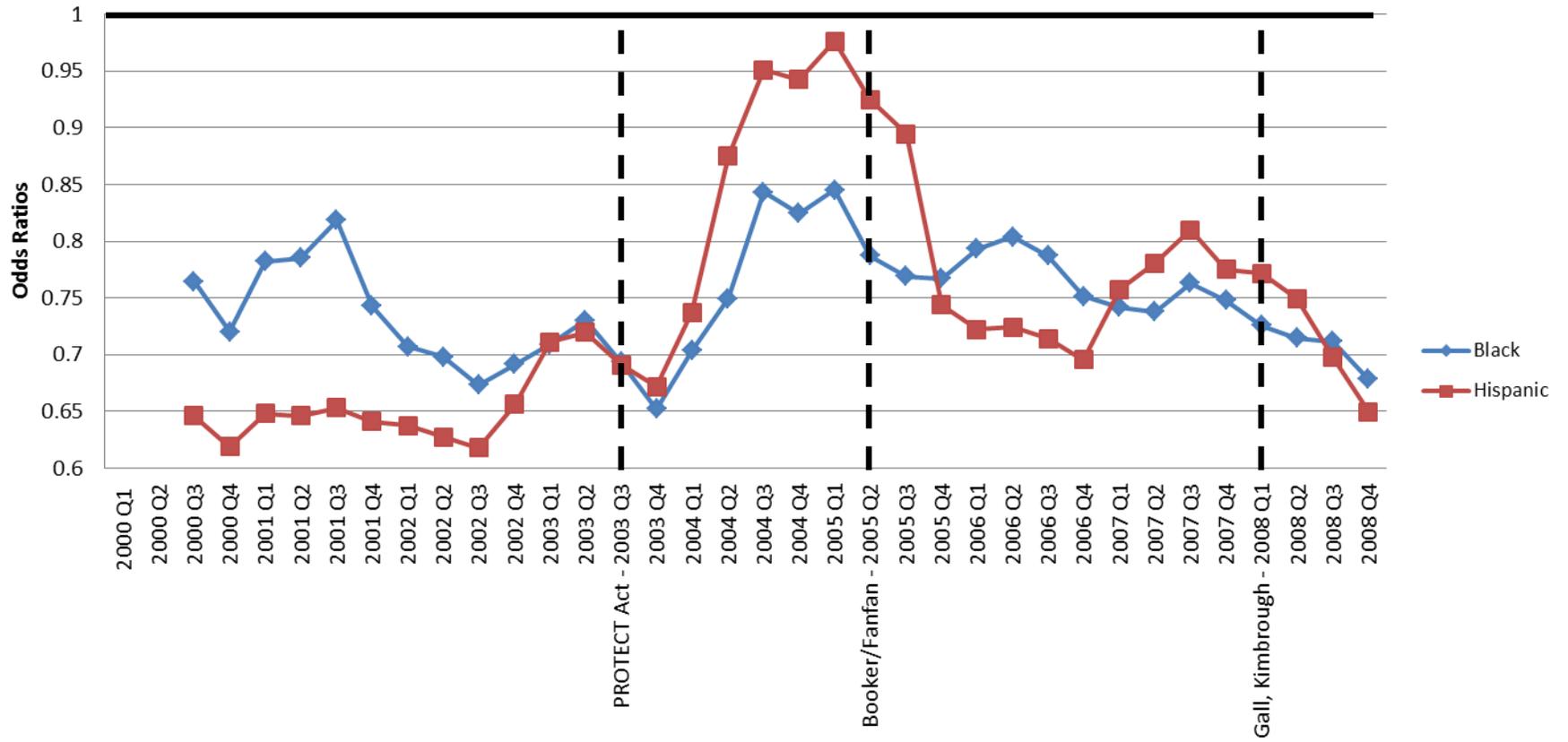
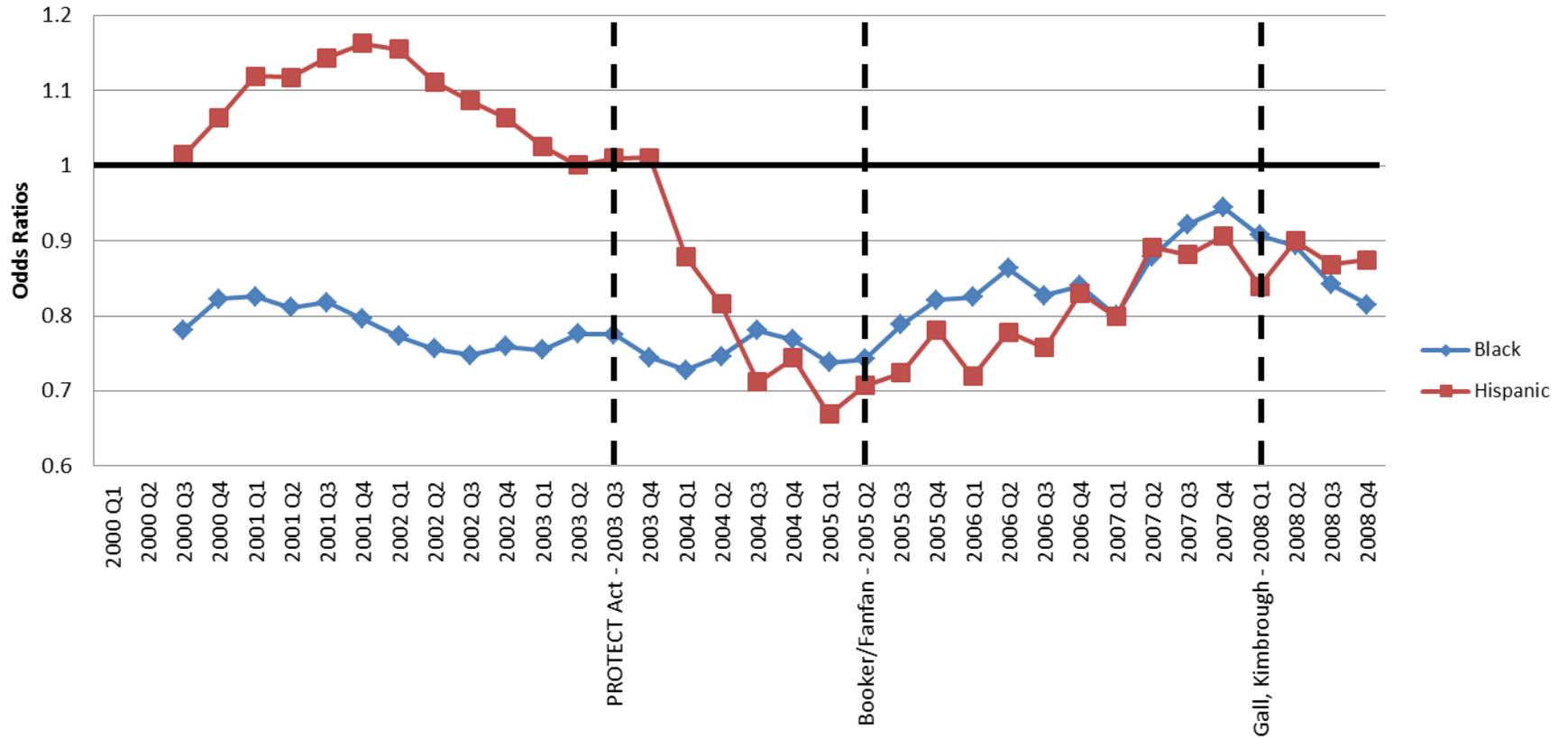
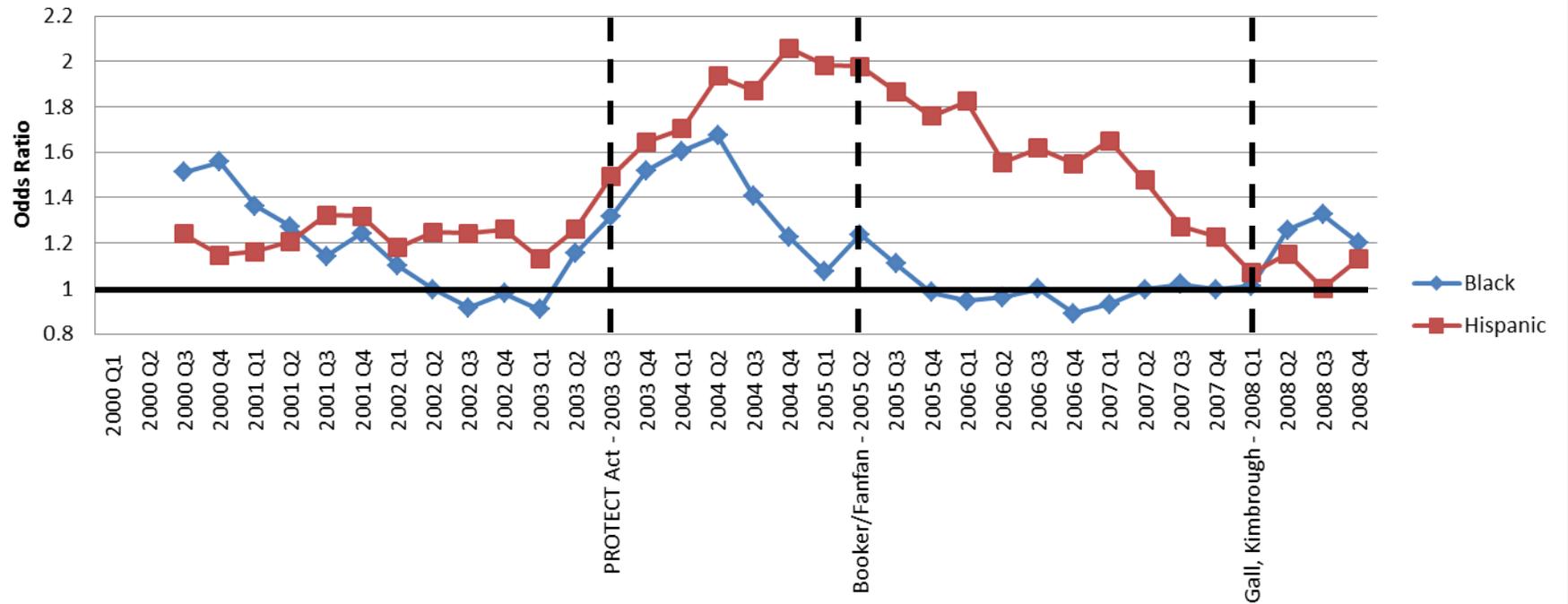


Figure 4. Quarterly Odds Ratios - Judge-Initiated Departures (Three Quarter Moving Average)



**Figure 5. Quarterly Odds Ratios - Incarceration Decision:  
Drug Offenses by Race/Ethnicity (Three Quarter Moving Average)**



**Figure 6. Quarterly Odds Ratios - Incarceration Decision:  
Crack Cocaine Offenses (Three Quarter Moving Average)**

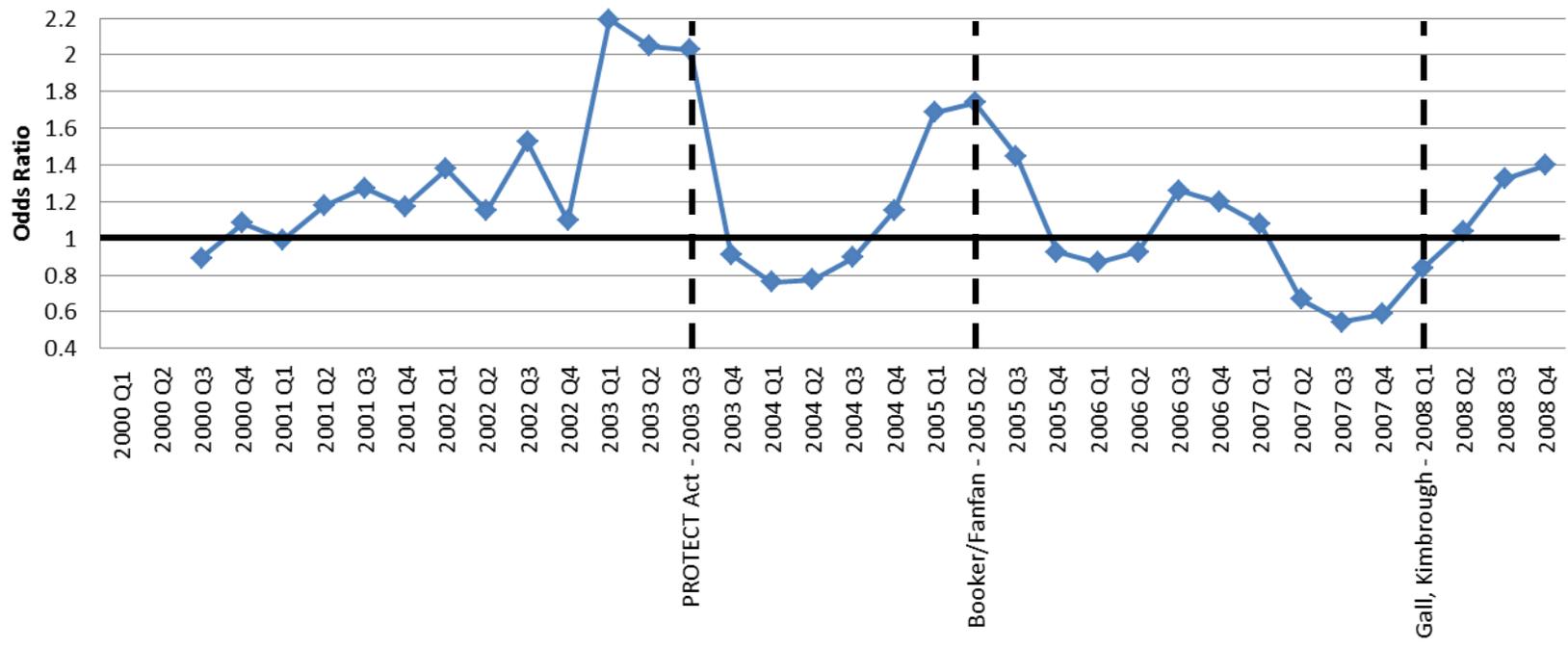
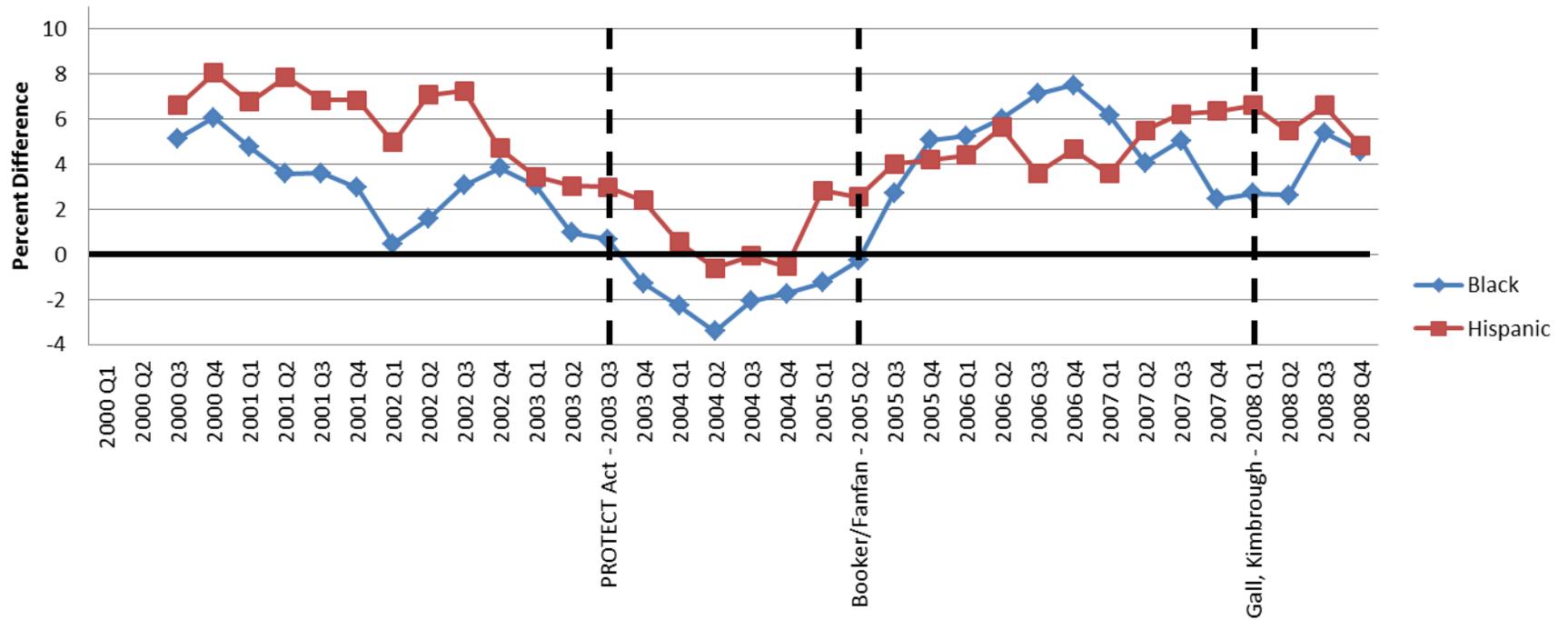
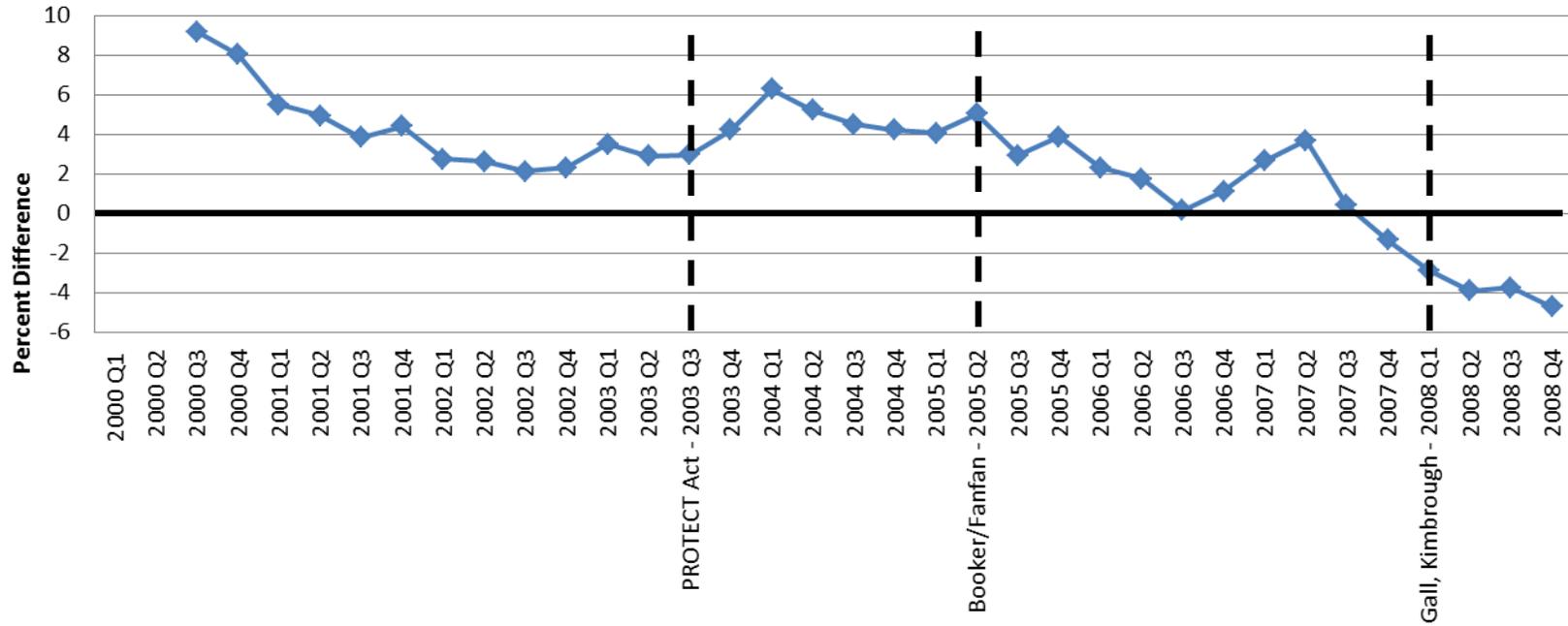


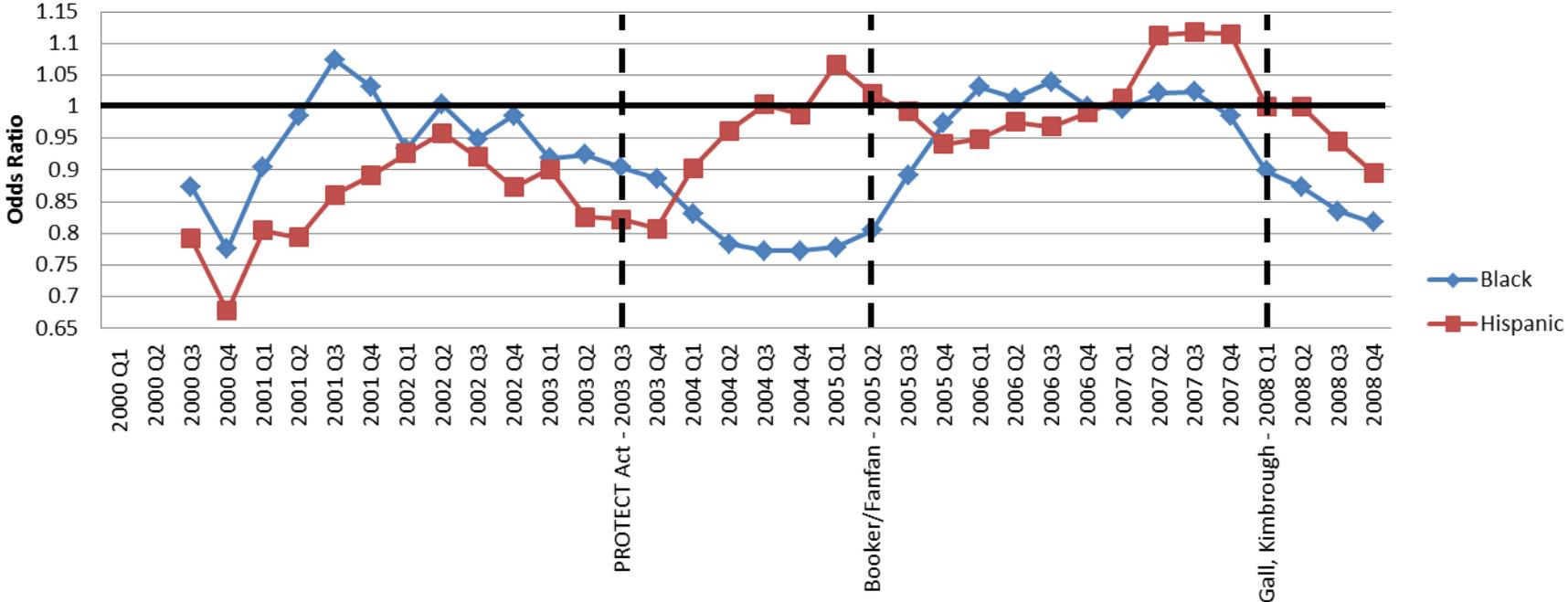
Figure 7. Quarterly Percent Differences, Logged Sentence Length - Drug Offenses by Race/Ethnicity (Three Quarter Moving Average)



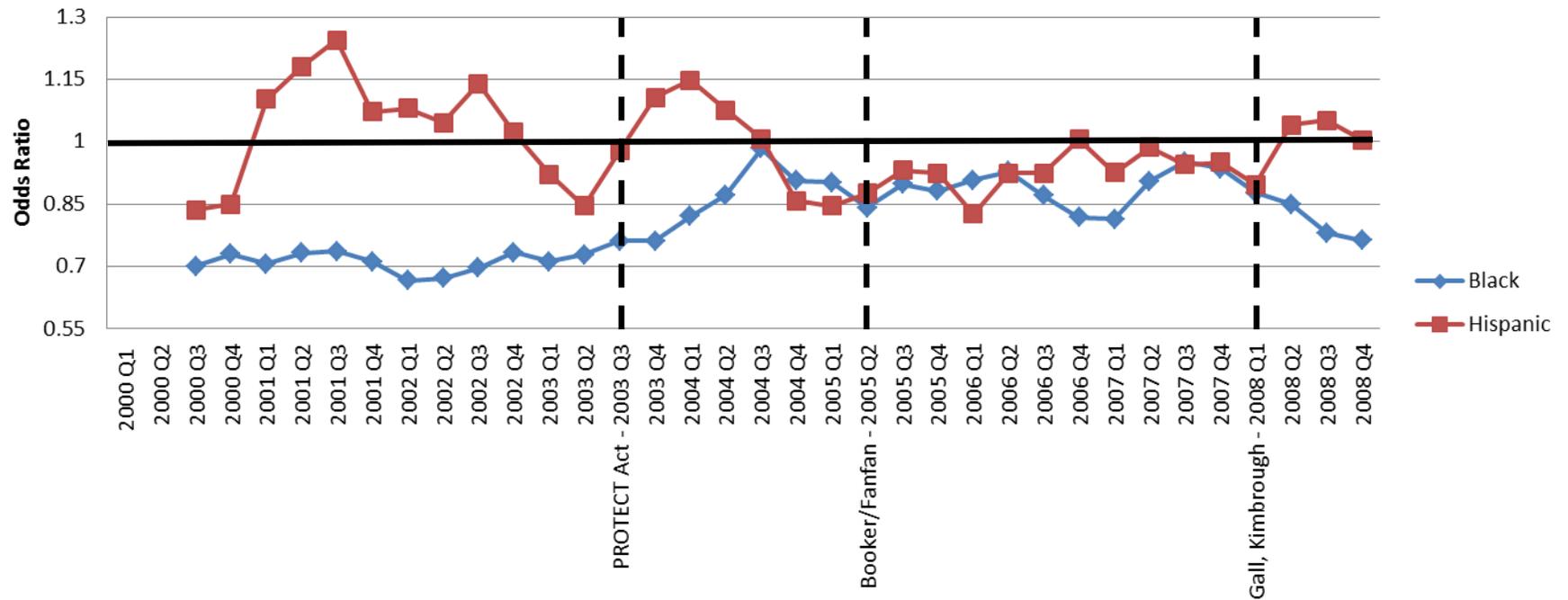
**Figure 8. Quarterly Percent Differences, Logged Sentence Length - Crack Cocaine Offenses (Three Quarter Moving Average)**



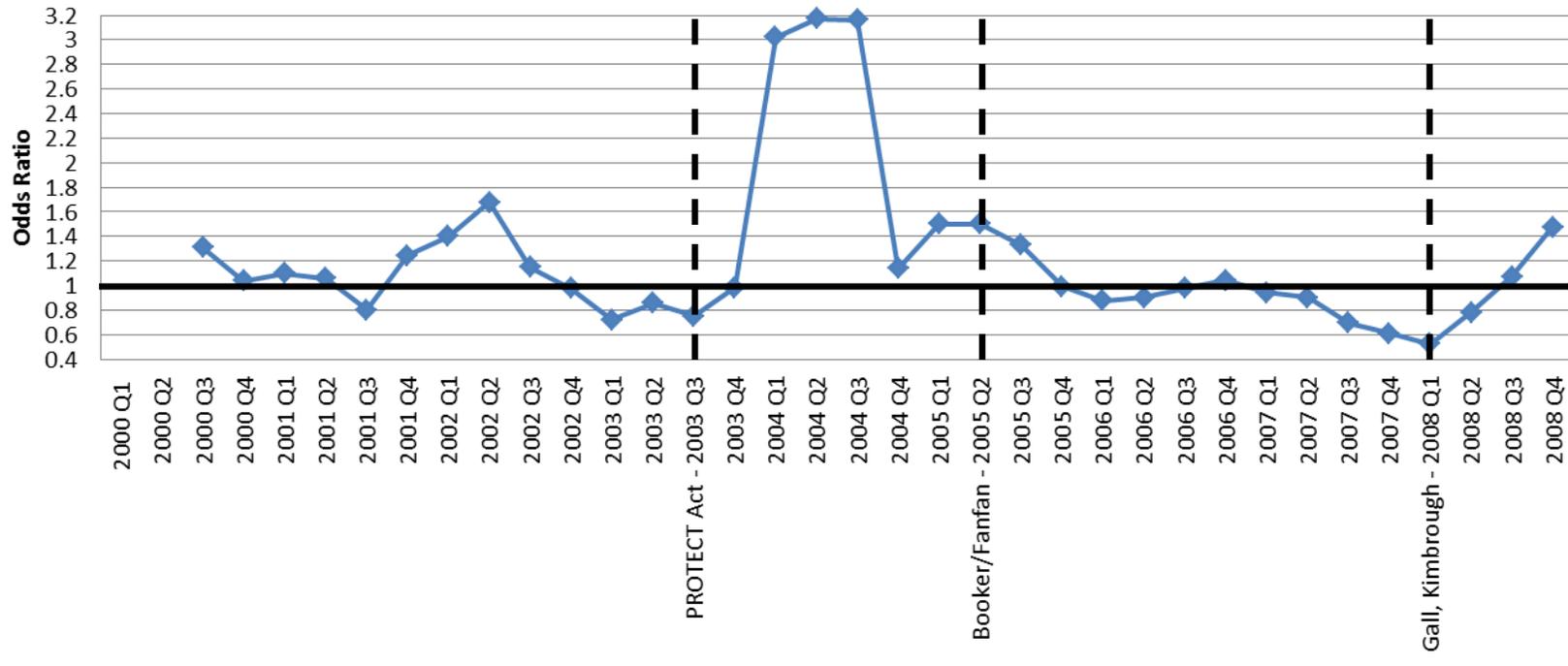
**Figure 9. Quarterly Odds Ratios - Government-Initiated Departures:  
Drug Offenses by Race/Ethnicity (Three Quarter Moving Average)**



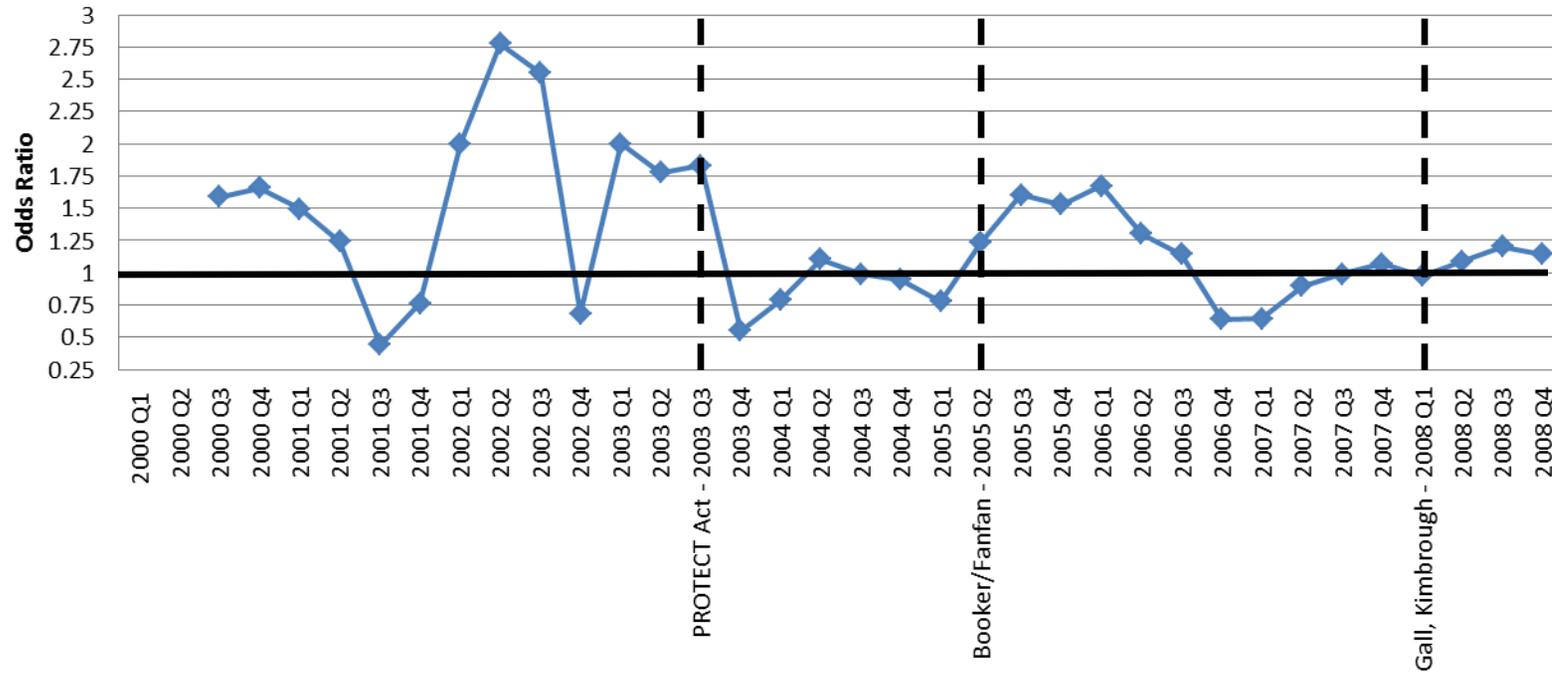
**Figure 10. Quarterly Odds Ratios - Judge-Initiated Departures:  
Drug Offenses by Race/Ethnicity (Three Quarter Moving Average)**



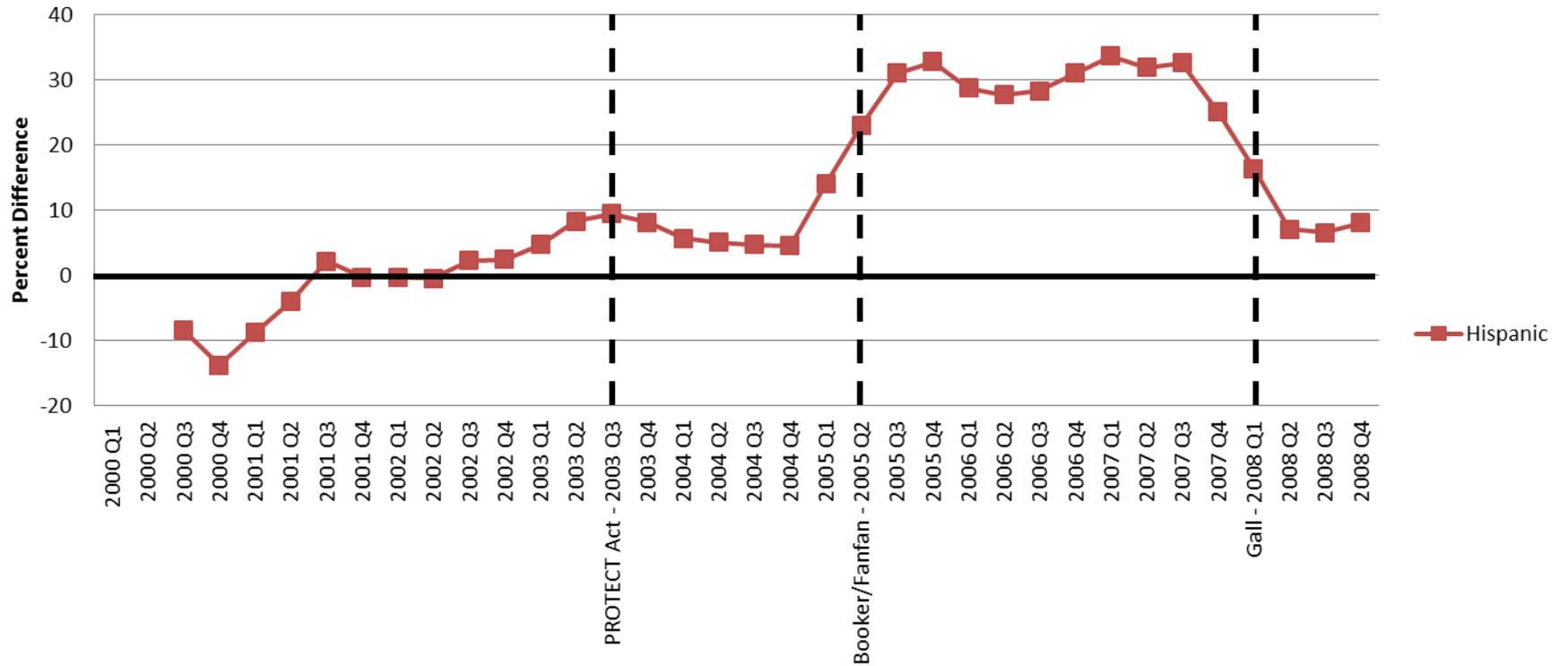
**Figure 11. Quarterly Odds Ratios - Government-Initiated Departures:  
Crack Cocaine Offenses (Three Quarter Moving Average)**



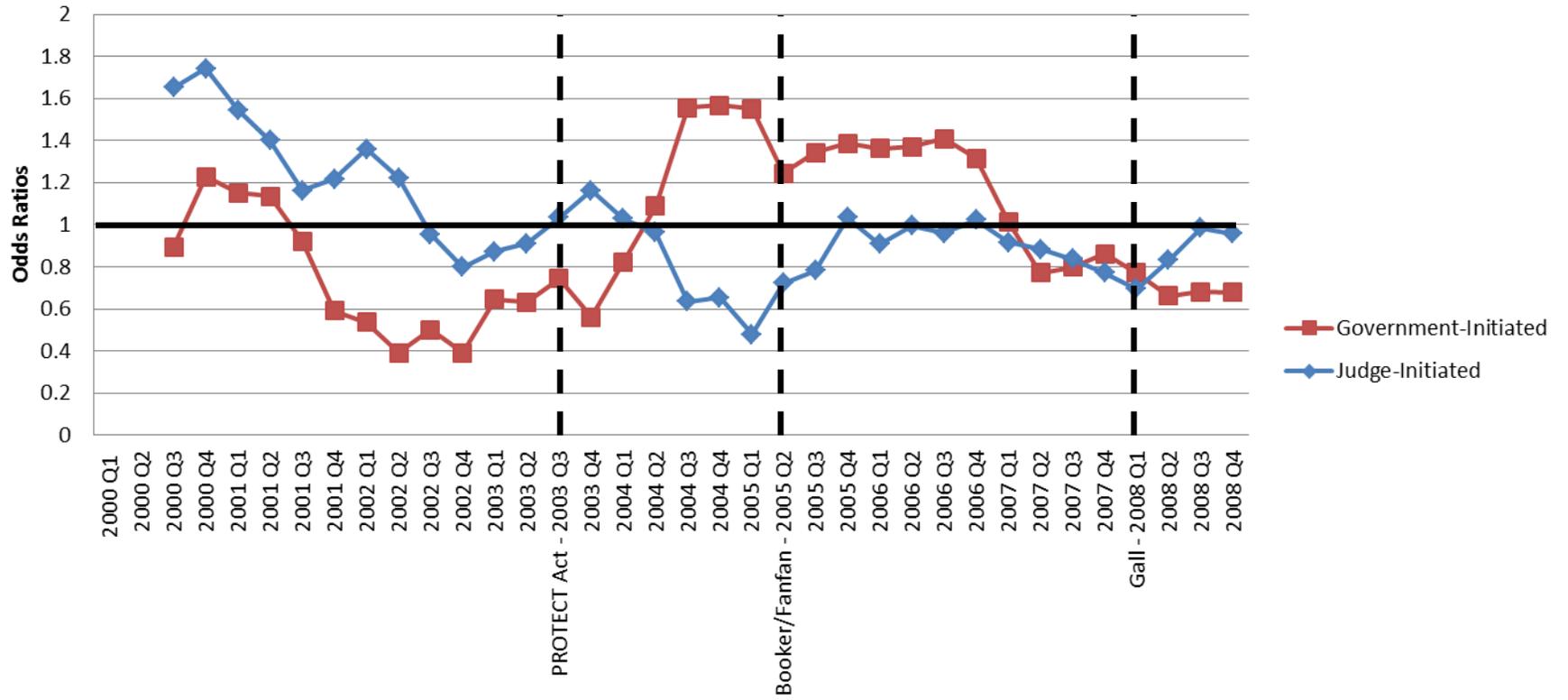
**Figure 12. Quarterly Odds Ratios - Judge-Initiated Departures:  
Crack Cocaine Offenses (Three Quarter Moving Average)**



**Figure 13. Quarterly Percent Difference, Logged Sentence Length - Immigration Offenses (Three Quarter Moving Average)**



**Figure 14. Quarterly Odds Ratios - Departures: Immigration Cases, Hispanic (vs. Non-Hispanic) Offenders (Three Quarter Moving Average)**



## APPENDIX A: SENTENCING TABLE

### SENTENCING TABLE (in months of imprisonment)

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

**APPENDIX B: CORRELATION MATRIX FOR LEVEL 2 VARIABLES**

	District Size (2000-2008)	Caseload Pressure (2000-2008)	Caseload Pressure (2000-2004)	Caseload Pressure (2005-2008)	Change in Caseload Pressure (2000-2008)	Change in Caseload Pressure (2000-2004)	Change in Caseload Pressure (2005-2008)	Departure Rate (2000-2008)	Departure Rate (2000-2004)	Departure Rate (2005-2008)	Racial Composition (2000-2008)	Racial Composition (2000-2004)	Racial Composition (2005-2008)	Change in Racial Composition (2000-2008)	Change in Racial Composition (2000-2004)	Change in Racial Composition (2005-2008)
District Size (2000-2008)	1															
Caseload Pressure (2000-2008)	.36**	1														
Caseload Pressure (2000-2004)	.29**	.92**	1													
Caseload Pressure (2005-2008)	.37**	.91**	.67**	1												
Change in Caseload Pressure (2000-2008)	.21*	.37**	.03	.66**	1											
Change in Caseload Pressure (2000-2004)	.03	.31**	.35**	.23*	.32**	1										
Change in Caseload Pressure (2005-2008)	.23*	.29**	.03	.50**	.82**	.22**	1									
Departure Rate (2000-2008)	.14	.10	.06	.12	.04	-.03	.02	1								
Departure Rate (2000-2004)	.13	.10	.06	.12	.05	-.03	.02	1.0**	1							
Departure Rate (2005-2008)	.15	.10	.06	.12	.04	-.03	.02	1.0**	.99**	1						
Racial Composition (2000-2008)	.11	-.03	.01	-.07	-.12	.00	-.05	-.05	-.05	-.04	1					
Racial Composition (2000-2004)	.11	-.03	.02	-.07	-.12	.01	-.05	-.05	-.05	-.04	1.0**	1				
Racial Composition (2005-2008)	.11	-.04	.00	-.08	-.11	-.01	-.04	-.05	-.05	-.04	1.0**	1.0**	1			
Change in Racial Composition (2000-2008)	-.11	-.11	-.14	-.06	.07	.05	-.03	-.12	.10	-.13	-.20	-.21**	-.18	1		
Change in Racial Composition (2000-2004)	-.05	-.09	-.12	-.04	.06	.04	-.03	-.10	-.09	-.11	-.18	-.19	-.16	.97**	1	
Change in Racial Composition (2005-2008)	-.19	-.02	-.08	.05	.12	.12	-.07	-.01	.01	-.02	-.30**	-.31**	-.30**	.39**	.24*	1

## APPENDIX C: DRUG CATEGORIZATION FOR DRUG OFFENSE MODELS

Drug Category	Drug Types Included	
Marijuana/Hashish	Marijuana Marijuana plant Hashish	Hashish oil Tetrahydrocannabinol (organic) Tetrahydrocannabinol (synthetic)
Cocaine	Cocaine	
Crack	Crack / cokebase	
Amphetamines	Amphetamine Amphetamine actual Dextroamphetamine Ephedrine Ice Khat Methamphetamine	Methamphetamine actual Methamphetamine pre-cursor (2D1.11 cases) N-n-dimethylamphetamine Phenylacetone-p2p (for meth manufacturing) Phenylacetone-p2p (other) Phenylpropanolamine Pseudoephedrine
Opiates/Opioids	Heroin Opium 3-methylfentanyl Alpha-methylfentanyl Alphaprodine Dextromoramide Dextropropoxyphene Dilaudid Dipipanone Ethylmorphine Fentanyl n-phenyl Laam	Levorphanol Meperidine / Pethidine Methadone Mixed alkaloids-opium / Papveretum Monoacetylmorphine Morphine MPPP Oxycodone actual Oxycontin Oxymorphone Pepap Racemorphan
Psychedelic	Bufotenine DET DMT DOB DOM LSD MDA MDEA MDMA / ecstasy Mescaline	PCE PCP PCP actual Dry peyote Wet peyote Dry psilcybin (mushrooms) Wet psilcybin (mushrooms) Psilocin Psilocybin TCP
Other Drug Types / Paraphernalia	Aminorex Codeine Fenethyline Flunitrazepam Gamme Butyroleactone Gamm-hydroxybutyric acid Hydrocodone Ketamine Methcathinone Methylaminorex N-ethylamphetamine PCC	Phenmetrazine PHP Prescription drugs and Schedule I/II depressants Ritalin Schedule III substance Schedule IV substance Schedule V substance Steroids 1,4 -Butanediol Other Drug paraphernalia

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