RESEARCH ARTICLE

RACIAL DISPARITY IN WAKE OF THE *BOOKER/FANFAN* DECISION

Racial disparity in the wake of the *Booker/Fanfan* decision

An alternative analysis to the USSC's 2010 report

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The U.S. Supreme Court ruled in *United States v. Booker* and a joint case *United States v. Fanfan* (2005, hereafter *Booker* and *Fanfan*, respectively) that the Federal Sentencing Guidelines (hereafter, Guidelines) would henceforth be advisory rather than presumptive in federal sentencing decisions. Many fear that the wake of *Booker/Fanfan* might have brought increased unwarranted disparity based on the social status characteristics of defendants (see reviews by Frase, 2007; Hofer, 2007). Hofer (2007) argued that if a primary goal of federal sentencing reform was a reduction of unwarranted disparity, the impact of *Booker/Fanfan* on disparity is among the most important questions facing sentencing policy makers. Chief among these concerns is the degree of disparity connected to race and ethnicity, the reduction of which was a key reason for the Guidelines' creation.

The U.S. Sentencing Commission (USSC) released a report in March 2010 concluding that racial disparity in federal sentence lengths has indeed increased in the wake of the *Booker* and *Gall v. Unietd States* (2007, hereafter *Gall*) decisions. Specifically, the report's "refined models" found that Black males had approximately 5% greater sentence lengths than White males in 2003–2004, 15% greater sentence lengths after the *Booker* decision, and approximately 21% greater sentence lengths post-*Gall*. Thus, from the report, it seems that racial disparity affecting Black males (and Black defendants in general) has become

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worse in the years since *Booker*, and especially since *Gall*. This is an alarming development for those who are rightly concerned with the racial fairness of federal justice.

Yet, the USSC 2010 report's analyses made some methodological choices that differ from those of several federal sentencing studies in the literature, and we detail these choices in the subsequent discussion. It is, therefore, important to examine whether the USSC 2010 racial disparity findings are apparent when different analytical and modeling choices commonly found in the sentencing literature are made. In addition, the USSC research staff was not directed in their 2010 report to present an analysis of whether disparity has increased post-*Booker* in sentences that depart/deviate from the Guidelines, and they did not compare their refined model findings with time periods earlier than the years when the PROTECT Act was in force (2003–2004). We, therefore, present such analyses because judicial discretion to deviate from the Guidelines has increased post-*Booker*, and Guidelines departures have been found to be the locus of extralegal disparity in research on pre-*Booker* sentencing (Albonetti, 1997; Hartley, Maddan, and Spohn, 2007; Johnson, Ulmer, and Kramer, 2008; Kempf-Leonard and Sample, 2001; Maxfield and Kramer, 1998; Mustard, 2001; Steffensmeier and Demuth, 2000).

Our analysis may present a fuller picture of the nature of racial disparity in the wake of the *Booker* and *Gall* decisions, as well as the relaxation of constraints on judicial discretion that they brought. According to Attorney General Holder (2009) as well as sentencing policy observers such as Paul Hofer (2007), this is one of the most pressing and timely questions faced by the federal sentencing community. If unwarranted disparity has increased in the post-*Booker/Gall* years, some argue that policy remedies are necessary to return the Guidelines somehow to a mandatory status and to attempt to roll back the judicial discretion granted by *Booker* and subsequent decisions.

In a recent essay in *Criminology & Public Policy*, Engen (2009) also noted the paucity of research on what happens in the wake of the repeal or relaxation of presumptive sentencing schemes. By examining sentencing in the aftermath of *Booker*, which loosened constraints dramatically on federal judicial discretion, we are helping to address the agenda Engen (2009) proposed. In sum, we provide a timely alternative analysis that we believe provides more specificity and guidance regarding questions vital to federal sentencing policy: (a) whether and how much racial disparity in federal sentencing has increased in the aftermath of *Booker* and *Gall*; (b) whether disparity has increased in particular kinds of sentencing decisions (i.e., sentence lengths, imprisonment, and Guidelines departures/deviations) or for particular offenses; and (c) whether the levels of racial disparity post-*Booker* are significantly greater compared with longer term federal sentencing patterns.

The Return of Federal Judicial Discretion: The *Booker* and *Gall* Decisions

From 1996 to 2005, legal developments moderately expanded judicial sentencing discretion, then sharply restricted it, and finally, culminating in *Booker*, dramatically expanded it again. From 1987 to 1996, discretion historically resting with the judiciary was tightly constrained

and shifted to the prosecutor (Stith and Cabranes, 1998). Congress continued to restrict judges' sentencing discretion during this period, sending directives to the Commission, and passing mandatory minimums to be incorporated into the Guidelines. Then, in *Koon v. United States* (1996, hereafter *Koon*), the Supreme Court restored some discretion to judges by establishing an "abuse of discretion" standard for appellate review of departures from the Guidelines. Congress later sought to counter *Koon* with the Feeney Amendment to the PROTECT Act of 2003, which replaced the "abuse of discretion" standard for departures with a "de novo" appellate review of sentences, gave prosecutors control over the third point of the "acceptance of responsibility" Guidelines reduction, and directed the Commission to reduce departure mechanisms.

Then, the *Booker* decision in 2005 ruled that the mandatory Guidelines could not constitutionally assess "real offense" conduct that increased sentences on factors not considered at trial by a jury. The Court's solution was that the Guidelines would become *advisory*. Judges must consider the Guidelines, but their discretion was returned to at least pre-PROTECT Act, although not to pre-Guidelines, levels. Also, in the wake of *Booker,* the standard of review now relies on the "reasonableness" of the sentence and on an "abuse of discretion" standard rather than on correct application of the Guidelines. Stith (2008: 1,427) stated: "*Booker*, the Sentencing Commission and Main Justice may still be calling signals but the decision makers on the playing field—judges and prosecutors—need not follow them."

Subsequently, the Court enhanced the judges' discretion restored in *Booker* by clarifying the meaning of "advisory" in *Rita v. United States* (2007), where it ruled that federal appellate courts *may* but are not *required* to presume Guidelines sentences to be reasonable. Consequently, sentences outside the Guidelines cannot be automatically regarded as unreasonable. In *Gall*, the Court went further and held that district judges *may not* automatically presume the Guidelines range to be reasonable and must "*make an individualized assessment based on the facts presented*" (*Gall*, pp. 596–597, emphasis added). *Gall* thus implies that district courts should make an individualized assessment of whether a Guidelines sentence is reasonable or whether a sentence outside the Guidelines is more reasonable. In *Kimbrough v. United States* (2007), the Court ruled that in cases involving crack cocaine, judges could reasonably conclude that Guidelines sentences were not reasonable in an individual case.

Policy observers have had different reactions to these developments. U.S. Attorney General Eric Holder (2009: 1) noted that uniformity and the control of judicial discretion per se do not guarantee justice: "The desire to have an almost mechanical system of sentencing has led us away from individualized, fact-based determinations that I believe, within reason, should be our goal." Some, including the USSC, have adopted a "wait-and-see" approach to post-*Booker* sentencing. For example, in 2005, an American Bar Association (ABA) Task Force Report recommended that sufficient time be allowed to evaluate the efficacy of the

new "advisory" guidelines, asserting that "the advisory remedy crafted in *Booker* may well prove as good or even better than the mandatory guidelines" (ABA, 2005: 339).

However, prominent U.S. Attorney John Richter (2008: 340), presenting a view held by many other federal prosecutors, argued that "[p]ost-*Booker* sentencing threatens equal justice under law." The dissenting opinions of Justices Stevens, Thomas, and Scalia in *Booker* each noted that Congress clearly intended to restrict judicial discretion to curb unwarranted disparity, and they argued that the Court majority's remedy of making the Guidelines advisory would jeopardize that goal.

Along these lines, then-Attorney General Alberto Gonzales (2005: 325) claimed that, since *Booker*, there has been "increasing disparity in sentences," and therefore, the *Guidelines* were in need of a legislative fix. Specifically, Gonzales (along with others) supported the proposed Consumer Privacy Protection Act,of 2005, which would have (a) transformed the Guidelines into a complex system of mandatory minimums (Bowman, 2005), (b) essentially forbidden the consideration of mitigating factors at sentencing (Berman, 2005), and (c) restricted severely the use of nonprosecutorial downward departures. Other sentencing scholars have proposed "hybrid" solutions. For example, legal scholar and former Special Counsel to the USSC Frank Bowman proposed simplifying the sentencing table to only nine base offense levels (down from the current 43) where no upward departures from the base sentencing range would be permissible (so as not to run afoul with *Booker*), although downward departures based on "acceptance of responsibility," motions by the prosecutor, or other relevant mitigating factors would be allowed (Bowman, 2005).

Research on Federal Sentencing Disparity

Much scholarly research on federal courts has assessed unwarranted disparity under the pre-*Booker* Guidelines. These studies often found small-to-moderate racial and ethnic sentencing differences benefitting Whites, although Guidelines-relevant factors exert much larger effects than offender status characteristics (e.g., Albonetti, 1997, 1998; Johnson et al., 2008; Kautt, 2002; Mitchell and MacKenzie, 2004; Mustard, 2001; Steffensmeier and Demuth, 2000; USSC, 2004). Evidence also suggests that extralegal differences in punishment are tied to departure sentences (Albonetti, 1997; Hartley et al., 2007; Johnson et al., 2008; Kempf-Leonard and Sample, 2001; Maxfield and Kramer, 1998; Mustard, 2001; USSC, 2004). Research using pre-*Booker* data showed that young minority males in particular were disadvantaged in incarceration decisions and sentence lengths (Doerner and Demuth, 2009); that defendant race, age, and gender influenced prosecutorial charge reductions, which in turn influence sentencing outcomes (Shermer and Johnson, 2010); that the degree to which race/ethnicity and gender influence sentencing varies significantly by judge (Anderson and Spohn, 2010); and that Hispanic defendants are most disadvantaged in sentencing in federal districts where Hispanics are least numerous, but not at all disadvantaged in districts with large Hispanic populations (Feldmeyer and Ulmer, 2011).

However, with the exception of two USSC reports published in 2006 and 2010, all the published research on federal sentencing disparity is based on pre-*Booker* data and most is based on pre-PROTECT Act data. The 2006 report showed that most federal cases continued to be sentenced in conformity with the Guidelines but that the rate of above-range, government-sponsored below-range, and other below-range sentences increased. Multivariate analyses showed that social status factors were associated moderately with sentence length but that their effects pre- and post-*Booker* were similar, and that race actually had more influence on sentence lengths in 1999–2000 than in the early post-*Booker* period (USSC, 2006). The report also examined conformity and departures by circuit and district from 2001 to January 2006 and concluded that regional sentencing differences have been relatively stable. A commentary on this report stated: "With a little over a year's experience under *Booker*'s new 'advisory' guidelines regime, the cumulative results can be summarized as 'much ado about nothing, or at least much ado about not very much'" (Thompson, 2006: 269). Overall, the 2006 report notes that disparity decreased in the PROTECT era, but after *Booker,* it returned to levels comparable with those of the pre-PROTECT act era.

However, the USSC 2010 report, which included data up to FY 2009, found that race disparity had increased in the post-*Gall* period compared with the PROTECT Act period. Their models first replicated the analyses in the 2006 report with the newer data included, and then they estimated a "refined model." Their "*Booker* report" model showed that Blacks received approximately 2% longer sentences than Whites (not significant) during the PROTECT era but that Blacks received 7% and 10% longer sentences than Whites in the post-*Booker* and post-*Gall* periods, respectively. Notably, Black–White sentence-length differences ranged from a high of 14% to a low of 8% in the pre-PROTECT years FY 1999–2002, as stated in the USSC 2006 report.

Their "refined model," which did not control for criminal history, but did differentiate Black, White, and Hispanic defendants by gender, found that Black males received 5.5% longer sentences than White males in the PROTECT period, 15% longer sentences post-*Booker,* and 21% longer sentences post-*Gall*. The 2010 report also found that noncitizens were increasingly sentenced more harshly than U.S. citizens and that gender disparity fluctuated across time periods. To be clear, the USSC 2010 report did not claim that *Booker* and *Gall* caused increases in racial disparity and recognized that other factors not related to the two decisions could be driving these increases. Nonetheless, the report's findings would seem to provide support for critics of the two decisions who call for remedies to reconstrain judicial discretion.

For our purposes, the USSC 2010 report's analyses have four notable methodological features: (a) the sentence-length models included nonimprisonment cases as sentence lengths of "0," thus combining the incarceration and length decision into one analysis (and used ordinary least-squares [OLS] regression, rather than tobit regression); (b) the report included immigration offenses in the analyses; (c) sentence-length models equated periods of alternative confinement with periods of imprisonment; and (d) the refined

model did not control for criminal history because of concerns about multicollinearity. Yet, previously published USSC studies of disparity in federal sentencing, such as the USSC's 2004 report, along with several studies published in the criminology literature, often made methodological choices that differed from these four features. Given these differing methodological choices, it is therefore important to examine whether the USSC 2010 racial disparity findings hold in the face of different analytical and modeling choices commonly found in sentencing studies.

To begin, the USSC's 2010 sentence length models included nonimprisonment cases as sentence lengths of "0," thus combining the incarceration and length decision into one OLS analysis. This strategy is relatively uncommon in the sentencing literature because it (a) assumes that there is no selection in the imprisonment decision relative to the length decision, (b) creates problematic distributional issues for standard OLS regression, and (c) offers opaque results regarding policy recommendations for the Guidelines. Although some might argue that nonincarceration sentences should be included as zeros because these offenders' "true" sentence lengths are not unobserved or censored, but are actually 0 months, we argue that this approach would be analogous to conducting research on wage disparity and including the unemployed, claiming that unemployed people actually receive wages of $0 (see Bushway, Johnson, and Slocum, 2007). We prefer to view only those *selected for incarceration* as eligible to receive sentence lengths. In other words, we view offenders' sentence lengths as conditional on whether they were sentenced to prison (and we will consider only imprisonment cases as sentence lengths, as we explain later).

This issue also raises the problem of the potential for selection bias, which is endemic to research on criminal justice decision making, and there is no definitive "right" way to handle it (Bushway et al., 2007; Stolzenberg and Relles, 1997). Most state and federal sentencing studies treat the imprisonment decision and the length decision as two related but distinct decisions (what Bushway et al. [2007] call the "two-part model"), and then these studies consider the issue of selection by including or not including a Heckman two-step correction for selection bias stemming from the imprisonment decision in sentence-length models (for some among many examples, see Anderson and Spohn, 2010; Doerner and Demuth, 2009; Johnson et al., 2008; Kautt, 2002; Peterson and Hagan, 1984; Spohn and Holleran, 2000; Steffensmeier and Demuth, 2000; Steffensmeier, Kramer, and Streifel, 1993; Ulmer, 1997; Ulmer, Eisenstein, and Johnson, 2010). The purpose of doing the Heckman correction is to generate estimates that refer to the *potential* population of everyone who could have been selected (Bushway et al., 2007). This strategy is in contrast to analyses that include only those who were sentenced to prison (i.e., second part of the two-part model) because these estimates refer only to the *actual* incarcerated population. Although the uncorrected two-part model may not capture potential selection *bias* because it focuses only on the effects on imprisonment length conditional on being imprisoned (Bushway et al., 2007), the approach used in the *Booker* reports does not assume any selection in the imprisonment decision by treating nonincarcerated offenders as incarcerated offenders for 0 months.

In other words, the latter approach assumes there is no distinction between those actually selected and those potentially selected, and it treats everyone as having a sentence length. It is for the preceding reasons that scholars attempted to use the Heckman correction; however, because the application of the selection procedure is problematic for several reasons in sentencing research, we opted to analyze sentence lengths for only those who were actually incarcerated.[1] That is, we employ a two-part model (although we do report the results of our "corrected" models in the text).

A second solution for problems with selection is the use of tobit regression (see Bushway et al., 2007), which treats the sentence-length variable as an instance of censoring. Because thousands of convicted offenders do not receive prison each year, these "zeros," when left in the distribution, create a problem with left censoring (i.e., there is a large category of individuals at the bottom of the distribution, which violates OLS assumptions regarding a normally distributed dependent variable). In this case, a tobit model treats the sentence-length distribution as a normal one but explicitly treats the zero sentence lengths as a point of censoring, and it assumes that the likelihood function would be normally distributed were it observed fully (for examples, see Albonetti, 1997, 1998; Bushway and Piehl, 2001; Kurlychek and Johnson, 2004). As an illustration of this censoring problem, we display the distributions of the sentence-length–dependent variables used by the USSC (panel A) and in our analysis (panel B) in Figure 1.

Both variables are displayed along a logarithmic scale. From a purely statistical standpoint, the distribution in panel A is highly problematic for an OLS regression equation. First, the variable does not approximate a normal or even near-normal distribution. Indeed, the modal category of this distribution is –4.61 [ln(0.01) = –4.61], which is the furthest left tail of the distribution.[2] As we demonstrate subsequently, this modeling choice has dramatic effects on how sentence-length results are interpreted. Panel B, in contrast, displays the distribution of the dependent variable used in our analysis. By analyzing only those who

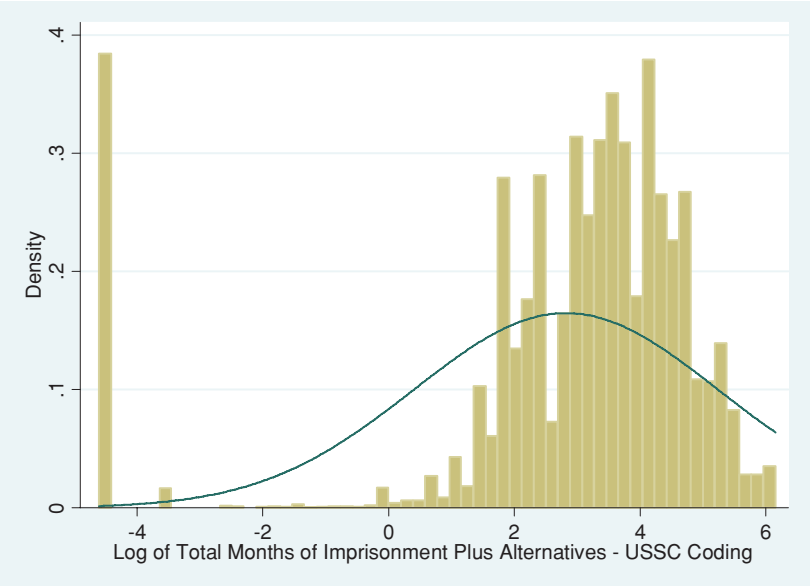
1. Rather than including "zeros" in standard OLS models, scholars have adopted several strategies to account for this possible selection bias, often using the Heckman two-step procedure, in which an individual's probability of being selected into the population of interest (in this case those receiving sentence lengths) is first calculated (using the inverse mills ratio), and then this conditional probability is entered into the OLS model. Although this selection correction may be justified theoretically, as Bushway et al. (2007) demonstrated, often its application is complicated and problematic because sentencing data usually do not include proper selection instruments that affect only an offender's likelihood of incarceration but not his or her length of imprisonment. As a result, the selection equation often includes many of the same predictors (i.e., criminal history, offense severity, race, etc.) as the substantive equation (sentence length) that introduces problems with multicollinearity, and model identification. Using similar procedures as Bushway et al. (2007), we find that using the Heckman procedure produced substantively similar results as those we present (discussed subsequently) but did in fact introduce problematic multicollinearity into our models of sentence length.

2. The USSC gave all zeros a value of 0.01 prior to logging because the log of 0 is not mathematically possible. Hence, all offenders who received probation or who were not incarcerated make up this category of –4.61 on the logarithmic scale.

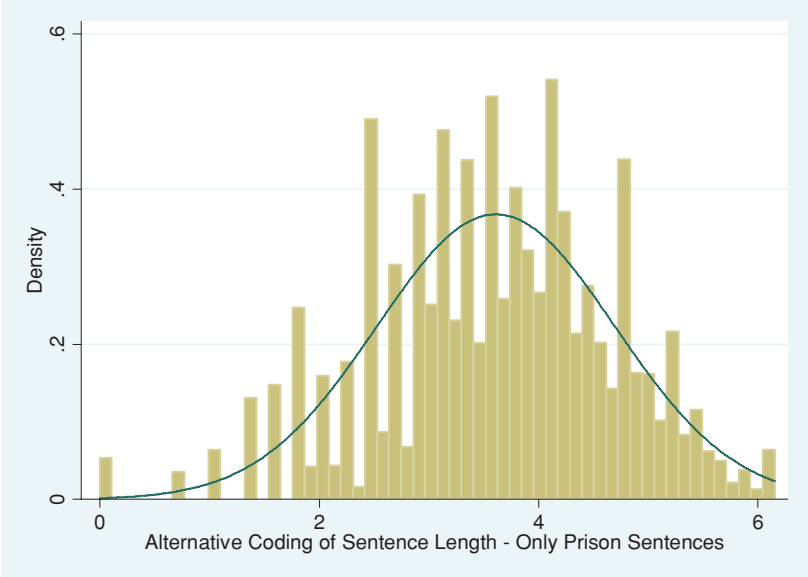
FIGURE 1

Histograms of Dependent Variables: Panel A - USSC Coding; Panel B - Authors' Alternative Coding

Panel A



Panel B



were actually sentenced to prison, logging approximates a normal distribution and does not require any correction for censoring. Again, although this modeling choice may be susceptible to selection bias, to ensure the robustness of our results, we also ran tobit regression models (discussed in text subsequently).

Perhaps the strongest argument against modeling sentencing decisions similar to the *Booker* report is the inability to separate out disparities occurring at either the incarceration stage or the sentence-length stage. As stated, although we acknowledge that our models of sentence length may ignore potentially problematic selection bias, from a policy perspective, we argue that our analytical approach is more appropriate because it does not conflate the incarceration and sentence-length decisions. By combining both decisions into one model (aside from the distributional and statistical issues discussed previously), the USSC model does not allow for the possibility that predictors might have different effects on imprisonment and sentence-length. However, this situation has often been found to be the case in sentencing research—and in fact, it is common to find that extralegal variables such as race/ethnicity have stronger impacts on incarceration than on sentence-length in sentencing research (see reviews by Spohn, 2000; Zatz, 2000; see also Doerner and Demuth, 2009; Johnson, 2006; Kramer and Ulmer, 2009; Steffensmeier et al., 1993; Ulmer and Johnson, 2004). In fact, the strategy adopted in the 2010 report differs from modeling choices made in previous USSC publications, in which incarceration and sentence-length analyses were modeled separately (see USSC, 2004: ch. 4). By combining the incarceration and length decisions into one model, the USSC report may be overstating the amount of sentence-length disparity, yet failing to pinpoint disparity in the incarceration decision.

The second methodological issue is that much of the previous research on federal sentencing has either excluded immigration or noncitizen cases from the analysis (Doerner and Demuth, 2009; Steffensmeier and Demuth, 2000; Ulmer et al., 2010) or analyzed these cases separately (Feldmeyer and Ulmer, 2011) for several reasons. First, often immigration offenses are handled differently than other federal crimes because of the intersection of immigration and criminal law, the possible involvement of foreign governments (Steffensmeier and Demuth, 2000), and the use of deportation as a sentencing option (only for non-U.S. citizens, who make up the overwhelming majority of immigration offenders). In fact, in the USSC's 15-year assessment of how well the Guidelines have accomplished the goals set out by Congress, the Commission excluded noncitizens from their analysis of racial, ethnic, and gender disparity because "inclusion of non-citizens, who are often non-White, confounds race and ethnicity effects of those with citizenship" (USSC, 2004: 120). Second, districts with comparatively large numbers of immigration cases commonly employ "fast-track" programs designed to expedite such cases (Bowman, 2003), whereas others do not have such fast-track programs. Fast-track programs present problems with uniformity in the system because the affected sentences are dependent not just on an offender's criminal conduct but on the district in which the offender is prosecuted (Maxfield and Burchfield, 2002). In the absence of controls for district variation (such as

fixed-effects models) or fast-track departures, this presents a potential omitted variable bias. Also, U.S. citizens would seldom be convicted of offenses involving "unlawfully entering or remaining in the U.S." (see §2L1.2 in the *U.S. Guidelines Manual*), which represent more than 70% of all immigration crimes. Again, this is not to say that immigration offenses should not be evaluated in sentencing outcomes. On the contrary, given the dramatic growth of such offenses in federal courts, we think this especially important issue deserves critical attention. However, we do argue that there are good reasons to suspect that immigration offenses are handled in distinct ways from most other offenses, and any analysis of federal data should be attuned to their distinctiveness, perhaps analyzing them separately.

The third methodological difference also involves the dependent variable. Whereas much previous research has examined sentences of incarceration to *prison* (which is the method we employ in our analysis; see also Bushway and Piehl, 2001; Doerner and Demuth, 2009, for similar analyses), the USSC uses a dependent variable that captures the months of *confinement* to either prison, home detention, community confinement, and intermittent confinement.[3] In other words, the racial disparities in "sentence lengths" reported by the Commission could be a result of different prison sentences or could be a result of different terms of community confinement or home detention. Although it is certainly important to research racial disparities in these other forms of confinement, we argue that sentences of home detention (and other forms of confinement) are qualitatively different from time in prison to the point where such sentences should not be analyzed as equivalent forms of incarceration.

Finally, the USSC 2010 report did not include controls for criminal history in their "refined" models because of issues of multicollinearity and because criminal history is one of the components of the presumptive sentence measure (see the Data section). However, criminal history has been shown to be an important independent predictor of sentencing outcomes beyond that captured by the presumptive sentence in published research on state and federal sentencing (Albonetti, 1998; Doerner and Demuth, 2009; Feldmeyer and Ulmer, 2011; Johnson and Betsinger, 2009; Johnson et al., 2008; Ulmer, 2005). These studies, along with our analysis, did not report severe multicollinearity with these two measures; however, criminal history was notably correlated with race (Black defendants tend to have higher mean criminal history scores).[4] Thus, any increase in racial disparity could possibly be because judges (or prosecutors) put more weight on criminal history in the wake of *Booker* and *Gall*.

Why would researchers want to control for criminal history in sentencing models above and beyond its influence through the presumptive Guidelines sentence? One answer is that, even if criminal history influences sentencing over and above the effect of Guidelines

3. This variable is SENSPLT0 in the USSC data files.

4. The bivariate correlation between criminal history and presumptive sentence is approximately 0.35 in all time periods.

minimums and is therefore a discretionary rather than a Guidelines-driven consideration of criminal history, sentencing variation explained by criminal history is not variation explained by race or ethnicity (or other defendant social statuses). That criminal history may *mediate* part of the effect or race/ethnicity or other characteristics indicates to us the importance of controlling for it when we try to identify the sentencing effect of race/ethnicity that is not attributable to other factors. Also, as we note, the USSC in their "*Booker*" models and several other federal sentencing studies include criminal history in sentencing models. However, as we discuss in the Results and Conclusion sections, there is legitimate debate as to the proper method for accounting for offender criminal history in sentencing studies that deserves additional attention.

It is also important to put the racial disparity findings from the USSC's refined model in broader temporal context. That is, how do post-*Booker* levels of sentencing disadvantage for Black males, for example, compare with Black male sentencing patterns in the pre-PROTECT Act era, or even before the important 1996 *Koon* decision? Perhaps the relatively low levels of racial disparity during the PROTECT Act era were atypical in the history of the Guidelines, and post-*Booker* racial disparity levels are comparable with earlier periods when the Guidelines were mandatory, but the PROTECT Act restrictions were not in effect. If this were the case, then it would not support arguments that the *Booker* and *Gall* decisions, and the increased judicial discretion they brought, produced a new trend of racial disparity in federal sentencing.

We attempt first to replicate the USSC's refined sentence length model (and also extend this analyses to the pre-PROTECT Act era) and then present alternative models that (a) examine disparity in the incarceration and length decisions separately, (b) control for criminal history, (c) do not equate alternative confinement with imprisonment, and (d) show levels of disparity with immigration offenses included in the models versus when they are excluded. We also extend the time period comparisons of racial disparity to the pre-PROTECT and the pre-*Koon* eras.

In addition, the 2010 report did not present an analysis of whether disparity has increased in sentences that depart/deviate from the Guidelines in the post-*Booker* periods, and it did not compare their refined model findings to time periods earlier than the years when the PROTECT Act was in force (2003–2004). We, therefore, present an analysis of whether disparity in departures (and which kinds of departures) has increased post-*Booker* and post-*Gall*, since judicial discretion to deviate from the Guidelines has increased post-*Booker*, and Guidelines departures have been found to be the locus of extralegal disparity in research on pre-*Booker* sentencing.

Data

The data come from the U.S. Sentencing Commission's Standardized Research Files, which are the same data used by the USSC for its reports. Consistent with the USSC's reports on the effects of *Booker*, we use the four time periods noted previously to assess the impact

of *Booker*: (a) cases sentenced in the pre-PROTECT Act period, which includes fiscal year 2002 (October 1, 2001–September 20, 2002) and fiscal year 2003 through April 2003; (b) cases sentenced in the PROTECT Act period which includes the second part of fiscal year 2003 (see footnote 1) and fiscal year 2004 through June 2004, which corresponds with the decision by the Supreme Court in *Blakely v. Washington* being handed down on June 24, 2004 (hereafter *Blakely*); (c) cases sentenced in the post-*Booker* period (January 2005 through November 2007); and (d) cases sentenced post-*Gall* (December 2007 through September 2009).[5,6,7] The unit of analysis is each sentenced case.

Dependent Variables

Our analysis examines the following three dependent variables for each of the four time periods: (a) length of sentence, (b) the imprisonment decision, and (c) the likelihood of receiving downward departures from the Guidelines, where substantial assistance and nonsubstantial assistance ("other" departures) are analyzed separately. Coefficients from the four separate time periods (pre-PROTECT, PROTECT, early post-*Booker*, and later post-*Booker*) are compared using z tests (Clogg, Petkova, and Haritou, 1995; Paternoster, Brame, Mazzerolle, and Piquero, 1998). The first dependent variable is the sentence length ordered for each offender (capped at 470 months). For the analyses after Figure 2, our dependent variable differs from that used in the USSC 2010 report in that we only use terms of imprisonment in our analysis, whereas their analysis includes months of alternative confinement including home detention, community confinement, and intermittent confinement. The USSC's sentence-length models also contain those who did not receive confinement sentences (e.g., probation) as sentence lengths of "0" (or 0.01, because 0 cannot be logged), whereas our analyses after Figure 2 do not. Because the sentence-length variable is skewed positively and regression diagnostics indicated problematic standard errors, we use the natural log transformation (as did the USSC in its reports). Our other dependent variables are dichotomies: (a) incarceration = 1, 0 if not; and (b) downward departures (of particular kinds) = 1, 0 if not.

Independent Variables

Consistent with prior research, we control for the *Guidelines-recommended sentence* by including a measure of the presumptive sentence equal to the minimum months of incarceration recommended by the sentencing guidelines after adjusting for any mandatory

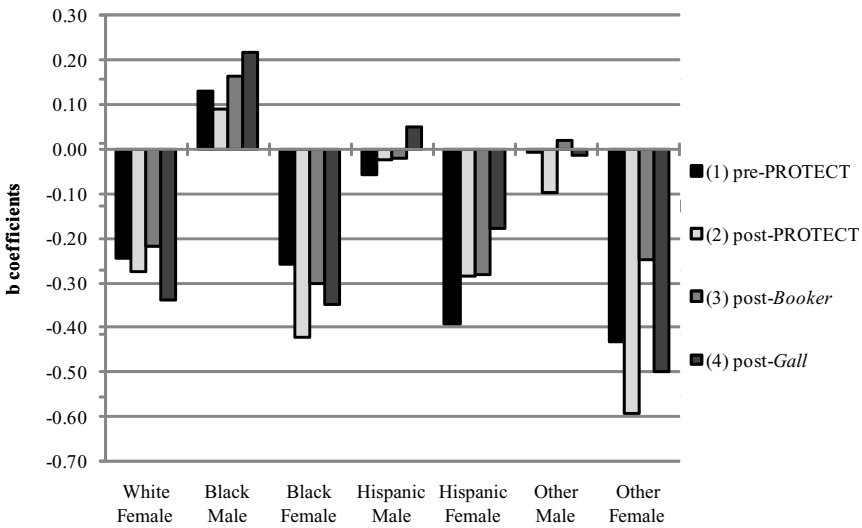
5. Seven months of fiscal year 2003 were prior to the effective date of the PROTECT Act (October 1, 2002–April 30, 2003), and 5 months were after (May 1, 2003–September 30, 2003).

6. We remove the period between the *Blakely* and *Booker* decisions to remove any potential *Blakely* effects.

7. The *Booker* decision was handed down on January 12, 2005, and *Gall* was decided on December 10, 2007.

FIGURE 2

U.S. Sentencing Commission "Refined" OLS Models of Sentence Length



	(1) pre-PROTECT	(2) post-PROTECT	(3) post-*Booker*	(4) post-*Gall*
Race-Gender Combinations	*Ln Length*[a]	*Ln Length*	*Ln Length*	*Ln Length*
	b	*b*	*b*	*b*
White Male (reference)				
White Female	-0.244*	-0.275*	-0.216[2*]	-0.340[1,2,3*]
Black Male	0.130*	0.089*	0.164[1,2*]	0.217[1,2,3*]
Black Female	-0.258*	-0.422[1*]	-0.300[2*]	-0.349[1*]
Hispanic Male	-0.058*	-0.023	-0.019[1*]	0.049[1,2,3*]
Hispanic Female	-0.391*	-0.286[1*]	-0.280[1*]	-0.177[1,2,3*]
Other Male	-0.008	-0.097[1*]	0.020[2]	-0.014[2]
Other Female	-0.432*	-0.592[1*]	-0.247[1,2*]	-0.499[3*]

[1] Coefficient is significantly different from pre-PROTECT Act estimate based on two-tailed z test ($p < .05$).

[2] Coefficient is significantly different from post-PROTECT Act estimate based on two-tailed z test ($p < .05$).

[3] Coefficient is significantly different from post-*Booker* estimate based on two-tailed z test ($p < .05$).

[a] Models include controls for all variables in Appendix A.

$p < .01$.

minimum trumps (Albonetti, 1998; Engen and Gainey, 2000; Johnson and Betsinger, 2009; USSC, 2004b). This measure incorporates the offense severity level and the criminal history, and it accounts for statutory sentencing provisions (i.e., mandatory minimum penalties) that affect the final presumptive sentence. As with sentence length, we cap the presumptive sentence variable at 470 months and take the natural log to reduce positive

skewness.[8] Although criminal history is included in the presumptive sentence measure, we follow previous research (e.g., Albonetti, 1998; Doerner and Demuth, 2009; Johnson and Betsinger, 2009; Johnson et al., 2008; Ulmer, 2005; Ulmer et al., 2010) and include an additional control for the offender's criminal history score.

We also control for the type of offense with a set of dummy variables (drug, violent, fraud, firearms, and other offenses, with property offenses as the reference category). We control for two case characteristics: whether the offender was detained prior to sentencing, coded 1 if the offender was detained and 0 otherwise; and whether the individual was convicted by trial, coded 1 for a trial conviction and 0 otherwise. Our sentence-length analyses include as predictors dummy variables for whether the defendant received an upward, downward, or substantial assistance (5K1) departure (coded 1 for these departures and 0 otherwise).

As in the USSC 2010 report's refined models, race/ethnicity and gender are combined into a set of dichotomous categories, a practice sometimes found in other sentencing studies as well (e.g., Doerner and Demuth, 2009; Kramer and Ulmer, 2009; Steffensmeier, Ulmer, and Kramer, 1998). In all analyses, we include dummy variables for Black males, Hispanic males, Black females, Hispanic females, White females, other race/ethnicity males, and other race/ethnicity females, with White males as the reference category. We also include a dummy variable for citizenship, with noncitizens coded as 1. Education is captured with four separate dummy variables: less than high school, high school graduates, some college, and college graduates as the reference.

Results

First, we present our replication of the USSC's refined model, adopting their sentence-length variable (with nonconfinement sentences-included and with alternative confinement counted as equivalent to imprisonment) as well as their coding of all independent variables, but we extend the time period of comparison to the pre-PROTECT Act era.[9] Second, we present our alternative sentence-length models across the four time periods. We then present similar models of the incarceration decision to compare racial/gender disparity across the different decision types. Fourth, incarceration and sentence-length decisions are reanalyzed without immigration offenses to evaluate the influence of these cases on demographic

8. A constant of 0.1 is added to all zero values for the presumptive sentence variable but not for the sentence-length–dependent variable. Taking the log of zero would exclude these values from the analysis. This is appropriate for the dependent variable because we want to analyze only those offenders who actually received a sentence length. The zeros are retained in the presumptive sentence variable (by adding 0.1 to all 0 values) because we want to retain those cases where an offender's minimum sentence was 0 months but he/she still received a prison sentence.

9. See the Appendix in *Demographic Differences in Federal Sentencing Practices: An Update of the Booker Report's Multivariate Regression Analysis* (USSC, 2010) for a description of all coding procedures used in USSC analyses.

disparities in sentencing outcomes. Fifth, we compare post-*Booker* sentencing to sentencing practices prior to *Koon v. United States* (1996) to test the validity that a return to more mandatory guidelines will "correct" the problems wrought by *Booker* and *Gall*. Finally, we examine the effects of race/ethnicity–gender categories on the likelihood of receiving different kinds of downward departures/deviations from Guidelines across the time periods. Our primary focus is on comparing and contrasting our findings with those of the USSC with regard to disparity connected to the race/ethnicity–gender categories across the various time periods, and on extending the analysis of post-*Booker* race/ethnicity–gender disparity to decisions that depart/deviate below the Guidelines.

Replication and an Alternative to the USSC 2010 Report

Figure 2 shows the results from our replication of the models run in the USSC 2010 report.[10]

The results in Figure 2 display the USSC models of sentence length (logged) regressed on offender characteristics, case processing factors, offense categories, and Guidelines factors for each of the four time periods.[11] Consistent with previous research, race/ethnicity and gender exert significant effects on sentence lengths in all time periods. Moreover, these results display similar patterns reported by the USSC, where certain forms of disparity have increased since *Booker* and *Gall*. For example, the Black male effect decreased from 0.130 in the pre-PROTECT era to 0.089 after the passing of the PROTECT Act, but then it increased to 0.164 in the wake of *Booker* and then again to 0.217 after *Gall*. Moreover, z tests show that these increases are statistically significant (see Appendix A). Figure 2 also shows that White female disparity has increased slightly since *Booker* and *Gall*, as has the Hispanic male effect.

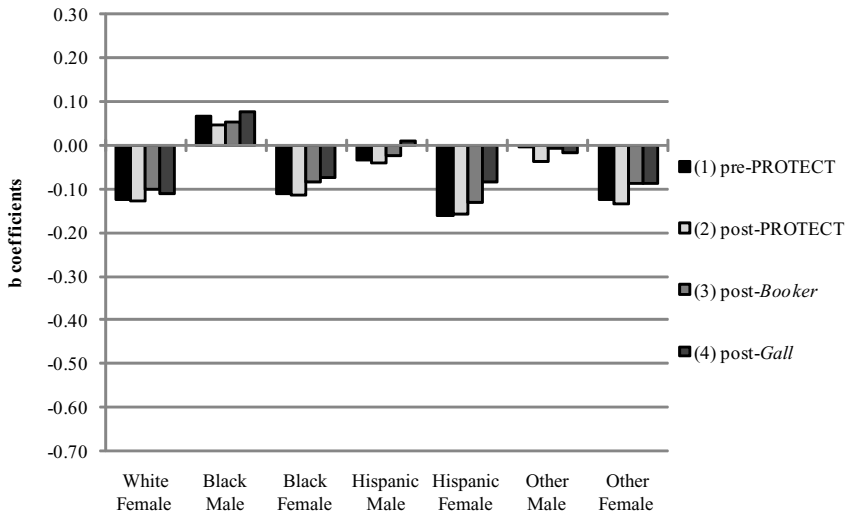
Not only do some forms of disparity show increasing trends over time, but also these effects are large compared with the results published in previous sentencing research. For example, the Black female effect for the post-PROTECT era is –0.422, which corresponds to 34% lesser sentence lengths ($\exp[-0.422] - 1 \times 100 = -34$) compared with White males, net of controls. Interpreted substantively, this effect indicates that at the mean sentence length (62.6 months), Black female offenders receive sentences that are nearly 2 years (21.0 months) less on average compared with their White male counterparts. Given the relatively narrow sentencing ranges in the Guidelines, this is perhaps a shocking result.

10. For parsimony, we display only the results for the race-gender combinations. The full models are shown in Appendix A.

11. The results shown here are not identical to those published by the USSC. We attempted several different coding procedures to try to obtain the exact results of the USSC without success. However, the patterns of results are generally consistent with those published by the USSC, and these differences do not account for the different results we display based on modeling choice. In fact, our results in places display greater disparity than the USSC report. For example, the Black male effects in Figure 2 are slightly greater in the post-PROTECT and post-*Booker* periods than reported by the USSC.

FIGURE 3

Alternative OLS Models of Sentence Length for Incarcerated Offenders



	(1) pre-PROTECT	(2) post-PROTECT	(3) post-*Booker*	(4) post-*Gall*
Race-Gender Combinations	*Ln Length*[a]	*Ln Length*	*Ln Length*	*Ln Length*
	b	*b*	*b*	*b*
White Male (reference)				
White Female	-0.125*	-0.128*	-0.099[1,2*]	-0.109*
Black Male	0.066*	0.045[1*]	0.053[1*]	0.077[2,3*]
Black Female	-0.112*	-0.115*	-0.084[1,2*]	-0.075[1,2*]
Hispanic Male	-0.034*	-0.039*	-0.025*	0.011[1,2,3]
Hispanic Female	-0.162*	-0.156*	-0.132[1*]	-0.085[1,2,3*]
Other Male	-0.004	-0.036*	-0.005[2]	-0.018
Other Female	-0.123*	-0.134*	-0.086*	-0.087*

[1] Coefficient is significantly different from pre-PROTECT Act estimate based on two-tailed *z* test ($p < .05$).

[2] Coefficient is significantly different from post-PROTECT Act estimate based on two-tailed *z* test ($p < .05$).

[3] Coefficient is significantly different from post-*Booker* estimate based on two-tailed *z* test ($p < .05$).

[a] Models include controls for all variables in Appendix B.

$p < .01$.

It is thus important to test the robustness of such findings against reasonable and common alternative modeling strategies.

Figure 3 reports the results from our alternative models of sentence length for all four time periods.[12]

12. Full results are available in Appendix B.

In separate models (not shown), we reran the analysis in Figure 3 without controlling for criminal history to assess the independent effect of excluding criminal history from models of sentence length. We find that criminal history has significant and substantial effects above and beyond the presumptive sentence. A one-unit increase in criminal history results in approximately 4% longer sentences, above that which is already captured by the presumptive sentence measure. Moreover, consistent with our predictions, including criminal history explains a significant portion of the race/ethnicity/gender effects. Put differently, criminal history seems to *mediate* a notable portion of the Black male effect. However, this was similarly true across time periods, and the inclusion or exclusion of criminal history does not change our conclusions about whether racial or ethnic disparity increased post-*Booker*/*Gall*. Across each time period, the racial/ethnic and gender disparities are approximately 20% larger when criminal history is not controlled for, and these effects vary across different racial–gender measures. For example, although criminal history accounts for virtually none of the Black female disparity, Black male disparity is more than 30% larger when a measure of criminal history is not included in the analysis. On the one hand, one could argue that by excluding criminal history from their "refined" models, the USSC may be overestimating racial and gender disparities in all time periods because part of the Black male effect in particular is explained by criminal history. On the other hand, one could argue that the true sentencing disadvantage of Black males is captured by not including criminal history because its Guidelines-based influence should occur through the presumptive sentence. Regardless, criminal history similarly mediates the Black male effect (in particular) across time periods.[13]

Although there are several differences in variable selection compared with Figure 2 (see the Data section for description), the most important difference between Figures 2 and 3 is the choice of dependent variable. In Figure 3, we include only those offenders who actually received a term of incarceration, whereas the USSC models included offenders who did not. The results reported in Figure 3 present a very different view of racial and gender disparity in the wake of *Booker* and *Gall*. First, the sizes of the disparity effects are substantially smaller. For example, whereas the Black male effect was 0.130, 0.089, 0.164, and 0.217 across the four time periods in Figure 2, they are 0.066, 0.045, 0.053, and 0.077 across the time periods in Figure 3. In other words, removing sentences of nonincarceration reduces the effect sizes by approximately 40% in each time period, and this pattern of results is generally consistent for the other racial–gender effects sizes. This reduction is almost entirely a result of removing the nonimprisonment cases—our omission of the alternative confinement cases as sentence lengths does not change the results notably. The USSC's

13. We also examined whether criminal history *moderates* the Black male effect by running supplemental analyses (available on request) that interacted criminal history by each race–gender dummy variable. We found a small moderation whereby the effect of criminal history was slightly greater for Black males (the Black male × Criminal history interaction term coefficients were as follows: pre-PROTECT = .005, PROTECT = .006, post-*Booker* = .004, post-*Gall* = .007. However, the differences in this interaction term across time periods were not statistically significant.

decision to include alternative confinement cases as sentence lengths therefore seems to be of negligible importance.

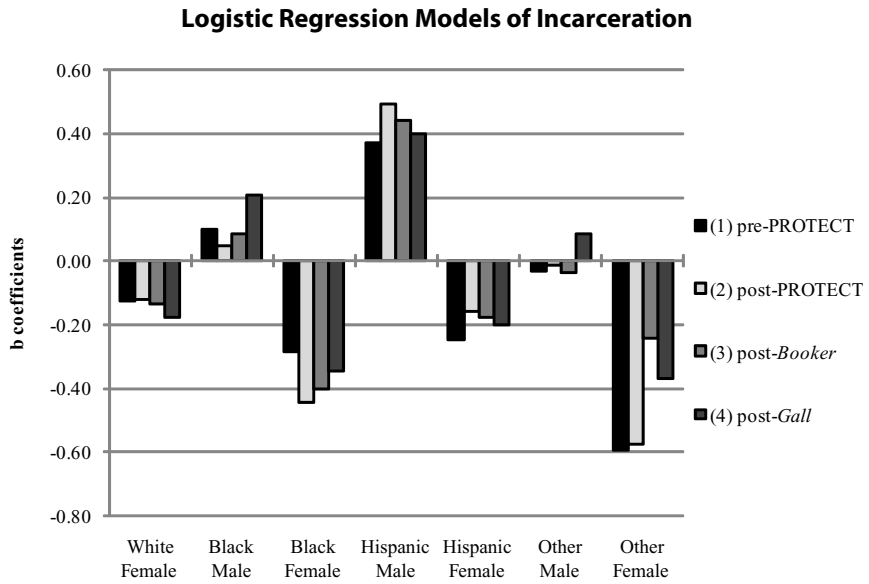
The second notable change in the pattern of results is the trends in effects over time. Although the Black male effect shows a similar pattern as Figure 2, where the effect dipped in the post-PROTECT era only to increase post-*Booker* and *Gall*, *z* tests show that the post-*Booker* effect is actually significantly *less than* the pre-PROTECT era, and there is no significant difference between the effects post-*Gall* and pre-PROTECT. In short, Black male disparity returned to the pre-PROTECT state in the wake of *Gall*. For other racial–gender effects, it seems that there is actually less disparity in sentence lengths after *Booker* and *Gall*. For example, compared with White males, the effects for Black females, Hispanic males, and Hispanic females are actually significantly *less* in the *Booker* and *Gall* periods than in the PROTECT era. In no case does it seem that racial–gender length disparities have substantially increased since *Booker* and *Gall*. Our findings, however, do raise serious questions about why our results differ from those of the USSC 2010 report. Because our dependent variable includes only terms of imprisonment, does this mean that disparity in the incarceration decision has increased after *Booker* and *Gall*? We explore this question in the next section.

Incarceration Decisions

Figure 4 reports the results of logistic regression models of the whether the offender was sentenced to prison regressed on the same independent variables reported in Appendix B.[14] The results offer mixed support for whether incarceration disparities have increased over time. Although the White female effect seems to have increased slightly, going from –0.125 in the pre-PROTECT era to –0.175 post-*Gall*, *z* tests show that none of the time period effects is significantly different from each other. This pattern of nonsignificant (or marginally significant) differences is generally true for nearly all the other race–gender effects as well but with one exception, the Black male effect. Consistent with the pattern of results for the sentence-length decision, Black male disparity decreased in the PROTECT era but then increased in the *Booker* and especially the *Gall* time periods. *z* tests confirm that post-*Gall* Black male *imprisonment* disparity is greater than in the previous time periods. These results explain the difference in sentence-length disparity between the USSC report and our models. Whereas they interpret their findings as increases in sentence-length disparity, we show that some differences in effects are actually caused by increased disparity in the incarceration decision. By including imprisonment and length decisions into the USSC's dependent variable, these two distinct patterns of results become conflated. Such results raise questions about the extent to which our sentence-length models in Figure 3 are biased by selection.

14. The only difference between the predictors in Figure 3 and 4 is the presumptive sentence variable is unlogged in Figure 4. The full table of incarceration results is available from the authors on request.

FIGURE 4



	(1) pre-PROTECT	(2) post-PROTECT	(3) post-*Booker*	(4) post-*Gall*
Race-Gender Combinations	*Ln Length*[a]	*Ln Length*	*Ln Length*	*Ln Length*
	b	*b*	*b*	*b*
White Male (reference)				
White Female	-0.125*	-0.122	-0.136*	-0.175*
Black Male	0.101	0.046	0.084	0.209[2,3,*]
Black Female	-0.283*	-0.444*	-0.401*	-0.346*
Hispanic Male	0.369*	0.492*	0.441*	0.400*
Hispanic Female	-0.245*	-0.158	-0.177*	-0.199*
Other Male	-0.032	-0.013	-0.038	0.084
Other Female	-0.592*	-0.573*	-0.240[1,2*]	-0.368*

[1] Coefficient is significantly different from pre-PROTECT Act estimate based on two-tailed *z* test ($p < .05$).

[2] Coefficient is significantly different from post-PROTECT Act estimate based on two-tailed *z* test ($p < .05$).

[3] Coefficient is significantly different from post-*Booker* estimate based on two-tailed *z* test ($p < .05$).

[a] Models include controls for all variables in Appendix B.

$p < .01$.

We ran our sentence length models both with and without a Heckman two-step correction factor, as discussed. For the purposes of the Heckman correction, we attempted to find exclusion restrictions and to estimate an incarceration model that was substantively different from the sentence-length model. This was difficult because most variables that significantly predict imprisonment also predict length, although the strength of the effects is sometimes different. Nonetheless, our selection model included a dummy variable for

presumptive disposition (despite whether the Guidelines-recommended imprisonment), instead of Guidelines minimum. The selection model also omitted defendant education because this did not exert significant effects on imprisonment in the selection probit model. The Black male length effects in the Heckman corrected model are as follows (all are significant at $p < .001$): pre-PROTECT = .063, PROTECT = .041, post-*Booker* = .048, and post-*Gall* = .08). In the Heckman corrected models, the Hispanic male effect was –.03 pre-PROTECT and –.02 PROTECT, and –.016 post-*Booker* and .02 post-*Gall*. The other race/gender effects are comparable with those in the results we present.

In supplemental analyses, we also included all cases for the four time periods together in models of incarceration and length, and we included interaction terms for each race/ethnicity/gender variable times each time period (with pre-PROTECT left out as a reference category). These terms then allow us to include all cases together in one model with the same error structure and to examine differences in Black male effects, for example, across time periods in the same model.[15] The results generally corroborate what we present in our alternative analyses previously in that sentence-length disparity for Black or Hispanic males is not significantly greater post-*Booker* or post-*Gall* than the pre-PROTECT or PROTECT eras. In fact, in the full time period interaction models, the increased incarceration odds post-*Gall* for Black males do not attain statistical significance. Furthermore, these models show that the Black male sentence-length effects post-*Booker* and post-*Gall* are slightly but significantly *less* than that in the pre-PROTECT era, whereas our models in Figure 3 show no significant differences between the pre-PROTECT and post-*Booker*/post-*Gall* Black male effects. The Black male × Post-*Booker* coefficient is –.023, and the Black male × Post-*Gall* coefficient is –0.017. Both indicate relatively small differences and are likely significant primarily because of the much larger number of cases in our combined-years model. Thus, the safest thing to say from our analyses is that the levels of sentence-length disparity affecting Black males seems to be nearly identical pre-PROTECT Act, post-*Booker*, and post-*Gall*. However, we present the separate models here as our main analysis for comparability with the USSC 2010 report, and these separate models make it easier to compare each time period with one another.

We also estimated tobit models that combined nonimprisonment and imprisonment sentences, treating 0 as a censoring point. The Black male effects are as follows (all are significant at $p < .001$): pre-PROTECT = .064, PROTECT = .041, post-*Booker* = .05, and post-*Gall* = .07. In the tobit models, the Hispanic male effect changed from –.04 pre-PROTECT and PROTECT, to –.03 post-*Booker* and .008 (not significant) post-*Gall*.

15. We would like to thank several helpful reviewers for suggesting this alternative modeling approach.

TABLE 1

Alternative Models of Sentence Length and Incarceration WITHOUT Immigration Offenses

Sentence Length[a]	(1) pre-PROTECT b	(2) post-PROTECT b	(3) post-*Booker* b	(4) post-*Gall* b
White Male (reference)				
White Female	−0.107*	−0.113*	−0.077[1,2,*]	−0.067[1,2,*]
Black Male	0.055*	0.028[1,*]	0.035[1,*]	0.040[1,*]
Black Female	−0.097*	−0.105*	−0.068[1,2,*]	−0.059[1,2,*]
Hispanic Male	−0.014	−0.033[1,*]	−0.013[2,*]	0.005[1,2,3]
Hispanic Female	−0.148*	−0.140*	−0.103[1,2,*]	−0.043[1,2,3,*]
Other Male	−0.005	−0.027	0.006[2]	−0.012
Other Female	−0.123*	−0.114*	−0.052[1,2,*]	−0.050[1,2,*]
N	60,226	46,400	121,625	75,209
Adjusted R2	0.820	0.832	0.847	0.849
Incarceration[a]	**(1) pre-PROTECT b**	**(2) post-PROTECT b**	**(3) post-*Booker* b**	**(4) post-*Gall* b**
White Male (reference)				
White Female	−0.119	−0.127	−0.136*	−0.166*
Black Male	0.126*	0.082	0.113*	0.228[2,3,*]
Black Female	−0.240*	−0.407[1,*]	−0.399[1,*]	−0.338*
Hispanic Male	0.321*	0.407*	0.405*	0.389*
Hispanic Female	−0.157	−0.074	−0.107	−0.122
Other Male	0.035	0.041	0.009	0.153
Other Female	−0.497*	−0.514*	−0.186[1,2]	−0.279
N	73,897	56,578	146,620	91,080
−2 log likelihood	31368.3	21318.6	50975.2	33249.8

[1]Coefficient is significantly different from pre-PROTECT Act estimate bas ed on two-tailed z-test ($p < .05$).
[2]Coefficient is significantly different from post-PROTECT Act estimate based on two-tailed z-test ($p < .05$).
[3]Coefficient is significantly different from post-Booker estimate based on two-tailed z-test ($p < .05$).
[a]Models include controls for all variables in Appendix B.
$p < .01$.

Immigration Offenses

With immigration crimes accounting for more than 25% of all federal sentences in 2009, immigration offenses are an important component of federal sentencing. However, as stated, these offenses offer unique challenges to researchers interested in comparability with other crimes, across time, and across federal courts. In Table 1, we evaluate whether immigration offenses have played a role in changing racial-gender disparity since *Booker* and *Gall* in both the incarceration and sentence-length decisions.

The results in Table 1 show the racial–gender effects for all four time periods across the incarceration and sentence-length decisions, excluding immigration offenses. For parsimony, we report only the race/ethnicity–gender effects (the full tables are available on request).

The results for the trends in disparity in sentence-length decisions, compared with those reported in Figure 2, show that a substantial amount of racial–gender disparity can be attributed to immigration offenses. For each racial–gender effect across the four time periods, immigration offenses alone account for roughly 25% of the effect size. However, the impact of immigration offenses varies substantially across groups, accounting for roughly 40% of the Hispanic male and Black male effects but for only 10% of the other male effect. These results show clearly that immigration offenses offer unique challenges to the federal criminal justice system. Even though Hispanics comprise the overwhelming majority of immigration offenders, the inclusion of these offenses without properly accounting for the degree of interdistrict variation that goes along with the unique district policies (i.e., use of fast-track departures) used to deal with them results in greater estimates of racial–gender length disparity than would be the case if immigration offenses were excluded.

The impact of immigration offenses seems to have only a modest effect on incarceration disparity. Whereas excluding immigration offenses actually shows slightly (although non-significant) increases in Black male disparity, immigration crimes account for approximately 10% of the Hispanic male and more than 40% of the Hispanic female disparity effects.

Advisory versus Mandatory Guidelines: A Broader Time Comparison

So far, our models have found little substantive change in sentence-length disparities based on race and gender when comparing the pre-PROTECT era with the post-*Booker* and *Gall* eras, but there has been an increase in Black male incarceration disparity. Also, roughly a quarter of all racial–gender disparities can be attributed to immigration offenses, likely resulting from the distinct methods certain federal districts use to handle the dramatic increase in immigration crimes.

As discussed, some commentators have claimed that increasing disparities post-*Booker* are caused by the increased discretion afforded judges (see, e.g., Gonzales, 2005; Richter, 2008), and to prevent such disparity, the Guidelines need to be made mandatory once again. However, none of the critics of the new advisory system have demonstrated that there was actually less disparity during the many years when the Guidelines were mandatory. More to the point, are the PROTECT Act period and pre-PROTECT years since 2000 the only relevant comparisons? What about the many years prior to the PROTECT Act when the Guidelines were also mandatory?

Since 1996, considerable "back-and-forth" struggling has occurred between the Supreme Court and Congress about the proper amount of judicial discretion at sentencing (see Stith [2008] for a detailed discussion). The Supreme Court decision in *Koon v. United States* (1996) was a watershed in this struggle, and the aftermath of this decision eventually led to Congress's attempts to restrict judicial sentencing discretion even more strongly with the PROTECT Act (see Stith, 2008). Recall that in *Koon*, the Supreme Court held that departure decisions made by district judges should be given due deference by appellate courts

TABLE 2

Sentence Length and Incarceration Models Comparing Pre–Koon with post-Booker and post-Gall

Sentence Length[a]	**(1) pre-*Koon* *Ln Length* *b***	**(2) post-*Booker* *Ln Length* *b***	**(3) post-*Gall* *Ln Length* *b***
White Male (reference)			
White Female	−0.200*	−0.110[1,*]	−0.109[1,*]
Black Male	0.122*	0.057[1,*]	0.077[1,*]
Black Female	−0.107*	−0.102*	−0.075*
Hispanic Male	0.012	−0.015[1,*]	0.011
Hispanic Female	−0.125*	−0.130*	−0.085[1,*]
Other Male	0.017	−0.007	−0.018 1
Other Female	−0.109*	−0.097	−0.087*
Incarceration[a]	**(1) pre-*Koon* *Incarceration* *b***	**(2) post-*Booker* *Incarceration* *b***	**(3) post-*Gall* *Incarceration* *b***
White Male (reference)			
White Female	−0.208*	−0.156*	−0.207*
Black Male	−0.008	0.078	0.175[1,*]
Black Female	−0.558*	−0.500*	−0.500*
Hispanic Male	0.489*	0.588*	0.437*
Hispanic Female	−0.419*	−0.200[1,*]	−0.282*
Other Male	−0.131	−0.053	−0.018
Other Female	−0.734*	−0.332[1,*]	−0.498

[1]Coefficient is significantly different from pre-*Koon* estimate based on two-tailed *z*-test ($p < .05$).
[a]Models include controls for all variables in Appendix B with the exception of "Pre-Sentence. Detention" because this information was not collected in the pre-*Koon* data.
$p < .01$.

and established that departures by judges should be examined by an "abuse of discretion" standard.

Thus, prior to *Koon* and its modest relaxation of restrictions on judges' ability to depart from Guidelines, the Guidelines were arguably more "mandatory" than at any other point in their history except perhaps the PROTECT era. We, therefore, use federal sentencing data from fiscal years 1994 and 1995 as a comparison time period versus post-*Booker* and post-*Gall*. If there is less disparity in the pre-*Koon* time period compared with *Booker* and *Gall*, this might mean that the post-*Booker* environment of advisory Guidelines has fostered greater disparity, and it would support calls for renewed restrictions on judicial discretion.

Table 2 shows the results for sentence length and incarceration decisions in the pre-*Koon*, post-*Booker*, and post-*Gall* time periods.

For parsimony, we display only the results for the racial–gender effects (full tables are available on request; note that the post-*Booker/Gall* effects are not identical to Figures 3 and 4 because we had to omit presentencing detention as a predictor.)[16] Beginning with the sentence-length results, it seems that the post-*Booker* and post-*Gall* disparities are considerably *less than* those found prior to *Koon*. Indeed, the White female, Black male, and Hispanic female effects are significantly less in either the *Booker* or *Gall* periods than prior to *Koon*. In no instance has there been a significant increase in sentence-length disparities since *Koon*. Put simply, racial and gender sentence-length disparities are less today, under advisory Guidelines, than they were when the Guidelines were arguably their most rigid and constraining.

However, disparities in the incarceration decision show considerably more stability among the three time periods. Of the seven racial–gender effects shown, three of them (Black female, Hispanic male, and other male) show no significant changes, two effects display significant reductions in disparity (Hispanic and other females), and only one effect shows a significant increase in disparity (Black male); this latter finding is specific to the post-*Gall* period.

These findings call into question the notion that mandatory guidelines, per se, result in reduced racial and gender disparities. Although we do find that Black male incarceration disparity has increased post-*Gall* compared with the pre-*Koon* period, we also find that Black male sentence-length disparity has been reduced considerably. Moreover, of the 14 racial–gender effects shown in Table 2 (seven effects across two sentencing decisions), seven show that post-*Booker* or post-*Gall* disparities are significantly *less than* those found prior to *Koon*, and the other six effects in general display slight (although nonsignificant) reductions in disparity.

It should be noted that incarceration and sentence-length decisions are not the only punishment decisions that judges make. In fact, much of the political and legal controversies surrounding the Guidelines have pertained to departures, and previous research has showed that departures from the Guidelines are a locus of disparity in federal courts (Albonetti, 1998; Johnson et al., 2008; Mustard, 2001). As our final test of whether *Booker* and *Gall* have resulted in greater disparities, Figure 5 reports the results from logistic regression models of whether an offender received a nonsubstantial assistance downward departure from the Guidelines.[17]

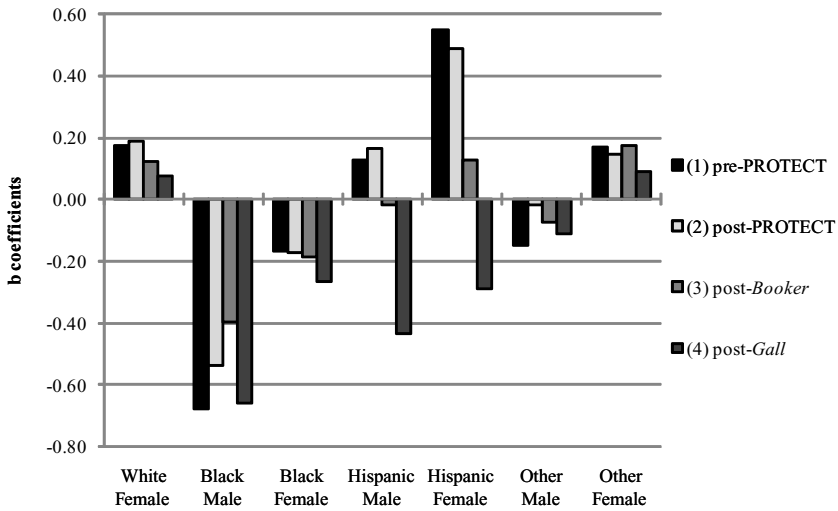
In all time periods, there is evidence of racial and gender disparity. Black males and females are less likely to receive an "other" downward departure compared with their White

16. All models in Table 2 include all variables from Figures 3 and 4 except whether the offender was detained pending sentencing. Although this variable is shown to have an effect on racial and gender disparities, information on this measure is not available in USSC data in the pre-*Koon* period. Thus, to compare across time periods directly, this measure was removed.

17. Full models are shown in Appendix C.

FIGURE 5

Logistic Regression Models of "Other" Downward Departures



	(1) pre-PROTECT	(2) post-PROTECT	(3) post-*Booker*	(4) post-*Gall*
Race-Gender Combinations	*Downward Dep* [a]	*Downward Dep*	*Downward Dep*	*Downward Dep*
	b	*b*	*b*	*b*
White Male (reference)				
White Female	0.173*	0.189*	0.122*	0.077
Black Male	-0.678*	-0.538[1,*]	-0.397[1,2,*]	-0.661[2,3,*]
Black Female	-0.167*	-0.172	-0.185*	-0.266*
Hispanic Male	0.130*	0.167*	-0.019[1,2,]	-0.432[1,2,3,*]
Hispanic Female	0.551*	0.486*	0.129[1,2*]	-0.290[1,2,3,*]
Other Male	-0.147	-0.017	-0.073	-0.111*
Other Female	0.168	0.145	0.177*	0.089

[1] Coefficient is significantly different from pre-PROTECT Act estimate based on two-tailed z test ($p < .05$).

[2] Coefficient is significantly different from post-PROTECT Act estimate based on two-tailed z test ($p < .05$).

[3] Coefficient is significantly different from post-*Booker* estimate based on two-tailed z test ($p < .05$).

[a] Models include controls for all variables in Appendix C.

$p < .01$.

male counterparts, net of controls, whereas White females are more likely to receive this form of sentencing discount. The trends in the effects, however, do not show that *Booker* and *Gall* have increased disparity substantially. z tests show that none of the Black female effects are significantly different across time, and this is true for the White female effects as well. The likelihood of a Black male receiving this sentencing discount actually improved significantly in the post-*Booker* period compared with the pre-PROTECT and post-PROTECT eras. However, since *Gall*, this form of sentencing disparity has returned to the effect found in

the pre-PROTECT time period, as evidenced by the nonsignificant z score between times (4) and (1). Interestingly, the disparity against Hispanic males and females seems to have increased in the post-*Gall* time period only. Whereas Hispanic offenders (male and female) were slightly more likely to receive an "other" downward departure in the pre-PROTECT and post-PROTECT periods, they are significantly less likely to receive this sentencing discount post-*Gall*.

In all, these results do not show that *Booker* and *Gall* produced greater disparity in the likelihood of minority offenders to receive nonsubstantial assistance departures. In most cases, the disparities returned to the pre-PROTECT effect sizes, although Hispanic disparity does seem to have increased since *Gall*. It should be noted, however, that this pattern of increased disparity against Hispanics is also true for substantial assistance departures. Figure 6 reports the results for the likelihood of receiving a substantial assistance departure across the four time periods.[18]

Again, there is clear disparity in the application of these departures in all time periods, specifically for Black and Hispanic males. Similar to the results in Figure 5, we find that Black male disparity increased in the post-*Gall* period, but this effect is not significantly different than in the pre-PROTECT period. This is not the case for Hispanic males, who have witnessed a significant increase in disparity post-*Gall* compared with all other time periods. These results are substantively important because, whereas most commentary on the effects of *Booker* and *Gall* has focused on how judges have reacted to their newfound discretion, little has been mentioned about how these cases may affect prosecutorial behavior. The results in Figure 6 suggest that disparity against Hispanic males in the prosecutorial use of substantial assistance departures has increased considerably since *Gall*.

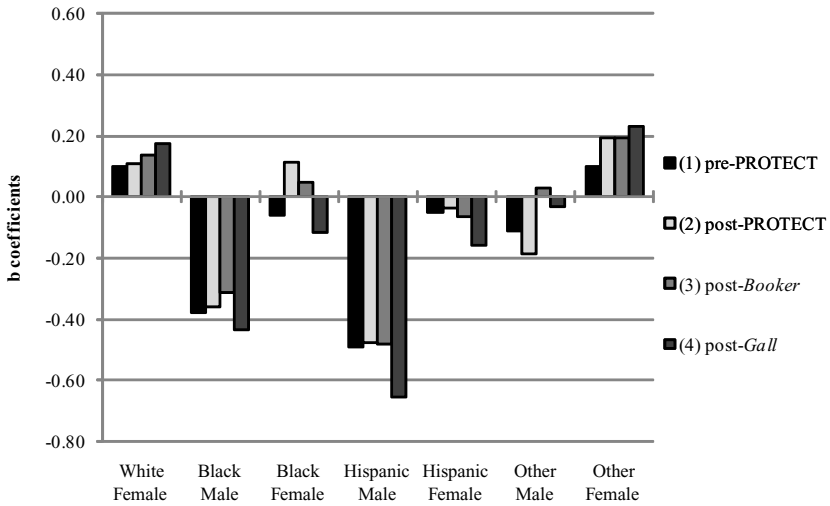
The idea that prosecutorial discretion, as opposed to judicial discretion, has been more of a locus of disparity in the wake of *Booker* and *Gall* receives additional support in Table 3.

Prior to *Booker*, the USSC did not keep detailed information on different types of downward departures except to indicate substantial assistance departures. However, government-sponsored downward departures and even fast-track departures were around long before *United States v. Booker*. After *Booker*, the USSC began keeping more detailed information on these specific departures types. Thus, in Table 3, we model the likelihood of receiving a judge-initiated downward departure, a government-sponsored downward departure, or a fast-track departure in the post-*Booker* and *Gall* time periods. The first part of the table shows that Black and Hispanic males are particularly disadvantaged in their likelihood of receiving a judge-initiated departure, and both forms of disparity have become significantly worse post-*Gall* compared with post-*Booker*. These results lend some support to those who claim that judges have used their newfound discretion in discriminatory ways. However, it is important to note that these effect sizes for "true" judge-initiated departures

18. Figure 6 includes all controls shown in Appendix C. Full tables are available from authors on request.

FIGURE 6

Logistic Regression Models of Substantial Assistance (5K1.1) Departures



	(1) pre-PROTECT	(2) post-PROTECT	(3) post-*Booker*	(4) post-*Gall*
Race-Gender Combinations	*5K1.1 Dep*	*5K1.1 Dep*	*5K1.1 Dep*	*5K1.1 Dep*
	b	*b*	*b*	*b*
White Male (reference)				
White Female	0.101*	0.109	0.135*	0.175*
Black Male	-0.378*	-0.361*	-0.313*	-0.436[3,*]
Black Female	-0.057	0.112[1]	0.050	-0.116[2,3]
Hispanic Male	-0.489*	-0.474*	-0.482*	-0.656[1,2,3,*]
Hispanic Female	-0.050	-0.037	-0.066	-0.157*
Other Male	-0.113	-0.186*	0.029[2]	-0.030
Other Female	0.102	0.194	0.195*	0.233

[1] Coefficient is significantly different from pre-PROTECT Act estimate based on two-tailed *z* test ($p < .05$).

[2] Coefficient is significantly different from post-PROTECT Act estimate based on two-tailed *z* test ($p < .05$).

[3] Coefficient is significantly different from post-*Booker* estimate based on two-tailed *z* test ($p < .05$).

[a] Models include controls for all variables in Appendix C.

$p < .01$.

are considerably less than those found in Figure 4, which contained all three of the different nonsubstantial assistance departure types combined into the "other" downward departure category. For example, in both the post-*Booker* and post-*Gall* periods, the effect for Black males in judge-initiated departures is only half the size of the effect for "other" downward departures, which suggest that prosecutor-sponsored departures are responsible for the other half. Moreover, the disparity against African Americans is considerably greater in both forms of government-sponsored departures, and it has increased to a greater extent since *Gall*.

TABLE 3

Logistic Regression Models of Different Departure Types in the post-Booker Era

	(1) post-*Booker*	(2) post-*Gall*
Judge Initiated Departure[a]	**b**	**b**
White Male (reference)		
White Female	0.124*	0.077
Black Male	−0.200*	−0.341[1,*]
Black Female	−0.020	−0.050
Hispanic Male	−0.213*	−0.308[1,*]
Hispanic Female	0.119*	−0.014[1]
Other Male	−0.024	0.015
Other Female	0.044	0.091
	(1) post-*Booker*	**(2) post-*Gall***
Government Sponsored Dep.[a]	**b**	**b**
White Male (reference)		
White Female	0.045	−0.061
Black Male	−0.299*	−0.599[1,*]
Black Female	−0.499*	−0.438*
Hispanic Male	0.264*	0.110[1]
Hispanic Female	0.561*	0.317[1,*]
Other Male	0.353*	0.474*
Other Female	0.473*	0.689*
	(1) post-*Booker*	**(2) post-*Gall***
Fast-Track Departures[a]	**b**	**b**
White Male (reference)		
White Female	0.274*	0.420*
Black Male	−2.580*	−3.595[1,*]
Black Female	−2.200*	−2.902*
Hispanic Male	0.059	−1.118[1,*]
Hispanic Female	0.148*	−1.148[1*]
Other Male	−0.511*	−1.184[1,*]
Other Female	0.784*	0.104[1]

[1] Coefficient is s ignificantly different from post-*Booker* estimate based on two-tailed *z*-test ($p < .05$)
[a] Models include controls for all variables in Appendix C.
$p < .01$.

Taken together, although much scholarly attention has been devoted to the changes in judges' discretion, the results in Figure 5, Figure 6, and Table 3 suggest that the post-*Booker* and post-*Gall* eras have observed equal or greater changes in prosecutorial behavior.

Conclusions

If a primary goal of federal sentencing reform was a reduction of unwarranted disparity, the impact of the *Booker/Fanfan* decision on disparity is among the most important empirical

questions facing sentencing policy makers (Hofer, 2007). Indeed, U.S. Attorney General Eric Holder (2009) emphasized the need for such research in recent remarks to Congress. We have provided an alternative to and extension of the USSC's 2010 report, which found that sentence-length disparity affecting Black males has increased relative to the PROTECT era. Our sentence-length findings differ in important respects with the USSC 2010 report, and our analyses go beyond theirs to provide a more extensive and fine-grained analysis of different sentencing decisions where disparity affecting Black males (and others) may occur.

First, in analytically separating the imprisonment decision from the length decision, we find that a considerable part of the USSC's Black male disparity findings are attributable to their analyses' combining of the imprisonment and length decisions into one model (and using OLS regression, rather than other options such as tobit models). We find that Black male incarceration odds have stayed relatively stable from pre-PROTECT up through post-*Booker*. Interestingly, the pre-PROTECT, PROTECT, and post-*Booker* periods show greater imprisonment decision disparity affecting Black males than the period before the 1996 *Koon* decision. However, Black male imprisonment odds do increase significantly post-*Gall*. This post-*Gall* increase in Black males' odds of imprisonment plays a big part in driving the USSC's findings of greater Black male sentence-length disparity. We have in fact shown that *post-Gall increases in Black male disparity are specific to the imprisonment decision* and not to sentence-lengths. This indicates that it matters a great deal for questions of disparity how one defines sentence outcome variables, and how one deals with selection into imprisonment, as well as the issue of censoring.

Second, we find that post-*Gall* sentence-length disparity disadvantaging Black males has increased significantly only with respect to the PROTECT era, and *not* in comparison with earlier periods. Notably, the post-*Booker*/*Gall* levels of Black male sentence-length disparity are lower than in the pre-*Koon* period, in addition to the pre-PROTECT era. Thus, 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.

Regarding racial disparity, the truly unusual period in the history of the Guidelines may be the PROTECT Act era, rather than the post-*Booker*/*Gall* eras. Taking the long view, the relatively low levels of disparity in the PROTECT period 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. If post-*Booker*/*Gall* racial/gender length disparity levels were comparable with or lower than levels in previous periods when the Guidelines were also mandatory, this calls into question the notion that the post-*Booker*/*Gall* eras of advisory Guidelines have produced *uniquely high* levels of racial disparity in sentence lengths. In our view, this also calls into question the need for blanket policy remedies that would attempt to curtail overall judicial sentencing discretion in the name of reducing disparity in sentence lengths.

Third, criminal history has an effect on sentences independent of Guidelines presumptive sentences, and criminal history *mediates* a notable portion of the Black male effect. The implications of this mediation should be considered further. On the one hand, Black males on average have higher criminal histories compared with White males (the Black male–criminal history correlation across all years is 0.25, whereas the White male–criminal history correlation is –.29). Thus, it could be argued that criminal history, even its discretionary consideration beyond its influence in establishing the presumptive sentence, captures "real" legally relevant differences—differences that are not attributable to race/gender categories. Therefore, the "true" degree of disparity is that which is left over after criminal history is taken into account. However, one could argue that the consideration of criminal history itself disadvantages Black male defendants and that the "true" degree of sentencing disadvantage is that which is produced by courts' discretionary consideration of criminal history beyond its influence on presumptive sentence because the consequences of such consideration fall harder on Black males.[19] Furthermore, the criminal records of Black males may themselves be the product of discriminatory processes and may be viewed subjectively and counted as more serious than those of other offenders. Although answering these questions is beyond the scope of this current article, it is safe to say that controlling for presumptive sentence and criminal history likely produces lower bound estimates of racial disparity, and our results suggest a need for future research and policy discussions about the consideration of criminal history, how it should be modeled properly, and its differential impact on Black males. It should be noted, however, that criminal history mediates the Black male effect similarly across each time period and, thus, does not account for the different trends in disparity between our analysis and the USSC 2010 report.

Fourth, a substantial portion of the sentence length disparity affecting Black males across time is attributable to *immigration offenses*, especially for the post-*Booker*/*Gall* period. When immigration offenses are removed from the models, Black male sentence-length disadvantage is notably less than when immigration offenses are included. What is more, when immigration offenses are removed, there is actually significantly *less* length disparity affecting Black males in the post-*Booker/Gall* periods than in the pre-PROTECT era. Incidentally, this is also true when we use the USSC model specification as in Table 1 without immigration offenses (available on request).

Booker and especially *Gall* gave judges more freedom to deviate from the Guidelines. Thus, if judges sentence Black males increasingly more severely compared with others post-*Booker/Gall*, logically we should view greater disparity affecting Black males (or others) in downward departures/deviations. We observe no such increase for Black males compared with the pre-PROTECT era. Black females are less likely to receive overall downward departures post-*Gall* (although not significantly), but there is no significant Black female

19. As one reviewer noted, "we need to avoid kitchen sink models when looking for racial disparity."

disadvantage in *judge-initiated* deviations (whereas there is for government-sponsored deviations). We also observe a greater Black male disadvantage in substantial assistance and government-sponsored departures than in judge-initiated departures. Furthermore, Hispanic males are significantly less likely to receive overall downward departures post-*Gall* compared with the pre-PROTECT and PROTECT eras. However, a substantial portion of this post-*Gall* disparity in overall downward departures affecting Hispanic males seems to be caused by dramatic post-*Gall* declines in the likelihood of Hispanic males receiving government-sponsored and fast-track departures, two decisions influenced heavily by prosecutors. Overall, the departure findings do not point to unique and comparatively large post-*Booker*/*Gall* racial/ethnic disparities in *judge-initiated* Guidelines deviations. In fact, greater disparity affecting Black and Hispanic males characterizes departures decisions heavily influenced by prosecutors more than judge-initiated departures.

Must the Guidelines be mandatory to be influential and to constrain disparity? Furthermore, why might *Booker* and *Gall not* have resulted in increased disparity? Perhaps the Guidelines serve a norm-setting function (Kramer, 2009) and have become embedded in the organizational and legal culture of federal courts. As Reitz (2005) observed, the Guidelines continue to structure federal sentencing in the aftermath of *Booker*—courts must continue to calculate and consider them and must provide legally defensible reasons for deviating from them. Furthermore, state court sentencing guidelines, such as Pennsylvania, Minnesota, Washington, Florida, and others have never been mandatory, and the federal Guidelines now have a legal status similar to such state sentencing guidelines (Kramer and Ulmer, 2009). Evidence exists that a major reason Pennsylvania's guidelines were influential was their norm-setting function: They became embedded in local court communities as taken-for-granted decision tools (Kramer and Ulmer, 2009; Ulmer, 1997). Although sentencing disparities affecting Black and especially Hispanic males, particularly in incarceration decisions, still exist under Pennsylvania's guidelines, these disparities have been reduced over time (Kramer and Ulmer, 2009).

Our study is certainly not the last word on the impact(s) of *Booker* and its aftermath on federal sentencing. We need to monitor levels of disparity continually, and our analysis raises some troubling questions. What accounts for the increase in the imprisonment odds of Black males post-*Gall*? What is responsible for the greater racial disparity among immigration cases, which are clustered in certain districts and processed in distinctive ways? Additional research on the role of race in immigration cases is needed. Why have Hispanic males become so much less likely to receive government-sponsored and fast-track departures post-*Gall*? We cannot answer these questions, but future research should continue to monitor more nuanced effects of *Booker* and *Gall* by evaluating sentencing outcomes for specific types of offenders, offenses, and specific decisions.

A chief limitation in our study is our inability to address disparities that might occur in earlier stages of case processing, such as charging and conviction processes. Our major goal in this article was to address important implications raised by the USSC 2010 report, which

focused on disparity in sentence lengths, and thus sentencing-stage discretion. However, we are acutely aware that prosecutors have always played a crucial role in federal sentencing, especially under the Guidelines. Our departure analyses differentiated substantial-assistance, government-sponsored, and fast-track departures from judge-initiated departures. However, offenders' exposure to Guidelines punishments is to a great extent a product of prosecutors' charging decisions and the plea agreement process, in which negotiated stipulations about Guidelines-relevant conduct and offense-specific behavior (which raise or lower the final offense level) are commonplace (Shermer and Johnson, 2010; Ulmer, 2005; Ulmer et al., 2010). It has been long recognized that changes in sentencing schemes affect the distribution of discretion among court actors (Engen, 2009; Reitz, 1998), and it is likely that federal prosecutors' decisions and behavior in the charging and plea agreement process have changed significantly in the wake of *Booker* and have changed in nonuniform ways. Some evidence for this is found in our results for substantial-assistance, government-sponsored, and fast-track departures. Our lack of presentence stage data means that we may be understating overall, process-wide disparity stemming from prosecutors' charging decisions and plea agreement behavior. If prosecutors have exhibited a greater tendency to consider extralegal factors in their charging decisions and in their plea agreement concessions in the post-*Booker* periods, our analyses would be unable to detect it.

The USSC 2010 report points to greater sentence-length disparity affecting Black males in the post-*Booker/Gall* periods, although it does not claim that *Booker* and *Gall* caused this increase. We have no wish to impugn the USSC or its commendable attention to the issue of unwarranted disparity. However, based on our differing results using alternative procedures that are reasonable in light of prior federal sentencing literature, as well as our analysis of Guidelines departures, we question the notion that *Booker* and *Gall* have caused increases in race/ethnic and gender sentence-length disparity compared with the full range of years when the Guidelines were mandatory.

We do find an unexplained increase in Black males' odds of imprisonment post-*Gall*, an empirical possibility that the *Booker* report cannot discern. There also seems to be notable disparity affecting Black males in immigration cases. These specific situations warrant additional scrutiny and perhaps discussions of policy changes targeted specifically to those two circumstances. Consideration of where disparities occur is fundamentally important to policy makers because, depending on where disparities are most prevalent, policy solutions differ. For example, based on the 2010 *Booker* report, a policy observer may favor restricting the sentencing ranges in the Guidelines table to reduce the amount of sentence-length disparity. However, if the bulk of disparity is located in the incarceration decision, such a "solution" would be misguided and would do little to help reduce this form of inequality. The same can be said for suggestions to "mandatorize" the Guidelines (see the Consumer Privacy Protection Act of 2005) to reduce sentencing disparities. In addition, if immigration cases are a particularly glaring locus of sentence-length disparity, but other kinds of offenses are not, then attention might be paid to the causes of such disparity and solutions drafted

to target immigration cases. We argue that there is insufficient empirical support for broad-based policies, such as the Consumer Privacy Protection Act, that would globally constrain federal judges' sentencing discretion as a remedy for disparity. Such a policy would not only be a blanket, blunt instrument solution to fairly specific loci of disparity but also would do nothing about prosecutorial decisions that affect sentencing outcomes (i.e., substantial-assistance, government-sponsored, and fast-track departures), which we have shown to be as great or greater a locus of disparity as judicial discretion.

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Appendix A.

U.S. Sentencing Commission "Refined" OLS Models of Sentence Length Based on 2010 *Booker* Report

	(1) pre-PROTECT Ln Length		(2) post-PROTECT Ln Length		(3) post-*Booker* Ln Length		(4) post-*Gall* Ln Length		(2) vs. (1)	(3) vs. (1)	(3) vs. (2)	(4) vs. (1)	(4) vs. (2)	(4) vs. (3)
Independent Variables	***b***	***SE***	***b***	***SE***	***b***	***SE***	***b***	***SE***	***z***[a]	***z***	***z***	***z***	***z***	***z***
White male (reference)														
White female	−0.244	0.023***	−0.275	0.024***	−0.216	0.015***	−0.340	0.021***	−0.930	1.003	2.050*	−3.100***	−2.043*	−4.772***
Black male	0.130	0.014***	0.089	0.015***	0.164	0.010***	0.217	0.013***	−1.942†	1.989*	4.182***	4.507***	6.398***	3.287***
Black female	0.258	0.026***	−0.422	0.028***	−0.300	0.018***	−0.349	0.027***	−4.359***	−1.343	3.687***	−2.480	1.901†	−1.529
Hispanic male	−0.058	0.016***	−0.023	0.017	−0.019	0.011†	0.049	0.014***	1.527	2.041*	0.192	5.106***	3.346***	3.938***
Hispanic female	−0.391	0.027***	−0.286	0.028***	−0.280	0.017***	−0.177	0.024***	2.676**	3.443***	0.184	5.920***	2.944**	3.496
Other male	−0.008	0.030	−0.097	0.030***	0.020	0.018	−0.014	0.025	−2.095*	0.795	3.336***	−0.155	2.115*	−1.094***
Other female	−0.432	0.056***	−0.592	0.056***	−0.247	0.034***	−0.499	0.049***	−2.008*	2.809**	5.254***	−0.889	1.254	−4.218***
>25 years old	0.033	0.012**	0.027	0.012*	0.033	0.008***	0.025	0.011*	−0.351	0.011	0.417	−0.539	−0.161	−0.651
U.S. citizen	−0.086	0.015***	−0.021	0.016	−0.052	0.010***	−0.074	0.013***	3.052**	1.956*	−1.682†	0.596	−2.608**	−1.369
Attending college	−0.103	0.012***	−0.073	0.013***	−0.077	0.008***	−0.075	0.011***	1.722†	1.807†	−0.268	1.687†	−0.154	0.105
Sexual abuse	−0.100	0.065	0.057	0.065	−0.012	0.040	−0.026	0.053	1.705†	1.153	−0.910	0.875	−0.994	−0.216
Pornography	−0.015	0.054	0.050	0.053	0.245	0.028***	0.329	0.034***	0.862	4.301***	3.230***	5.412***	4.398***	1.908†
Drug trafficking	−0.128	0.024***	−0.063	0.026*	−0.047	0.016**	−0.179	0.022***	1.870†	2.829**	0.522	−1.573	−3.427***	−4.809***
White collar	−0.492	0.026***	−0.519	0.028***	−0.480	0.018***	−0.558	0.024***	−0.710	0.369	1.161	−1.866†	−1.058	−2.565*
Immigration	0.022	0.027	0.025	0.029	0.054	0.018**	−0.003	0.024	0.071	0.978	0.854	−0.683	−0.735	−1.870†
Other offense	−0.337	0.025***	−0.303	0.026***	−0.297	0.017***	−0.414	0.023***	0.935	1.345	0.204	−2.315*	−3.206***	−4.179***
Upward departure	0.928	0.054***	1.050	0.054***	0.979	0.025***	0.884	0.031***	1.583	0.851	−1.186	−0.698	−2.637**	−2.382*
Substantial assistance dep.	−1.217	0.014***	−1.182	0.015***	−1.119	0.010***	−1.092	0.013***	1.772†	5.864***	3.580***	6.531***	4.528***	1.661†
Other downward departure	−1.076	0.014***	−0.885	0.016***	−0.913	0.008***	−0.979	0.010***	9.058***	10.261***	−1.575	5.645***	−4.998***	−5.182***
Mandatory minimum	0.333	0.014***	0.295	0.015***	0.354	0.009***	0.524	0.013***	−1.815†	1.197	3.265***	9.990***	11.542***	10.846***
Trial	0.224	0.026***	0.240	0.024***	0.284	0.015***	0.375	0.022***	0.442	2.023*	1.590	4.436***	4.156***	3.401***
Presentence detention	0.715	0.012***	0.744	0.013***	0.759	0.008***	0.867	0.012***	1.637	3.018**	0.987	9.133***	7.102***	7.601***
(ln) presumptive sentence	0.715	0.002***	0.686	0.002***	0.698	0.002***	0.642	0.002***	−8.741***	−6.046***	4.119***	−22.885***	−13.571***	−20.555***
Constant	0.738	0.031***	0.738	0.034***	0.621	0.022***	0.722	0.029***	−0.002	−3.077**	−2.927**	−0.363	−0.348	2.780**
N	88,856		70,505		187,793		124,118							
Adjusted R^2	0.685		0.715		0.678		0.620							

[a]Based on a two-tailed test. †$p < 0.10$; *$p < 0.05$; **$p < 0.01$; ***$p < 0.001$.

Appendix B.

Alternative OLS Models of Sentence Length for Those Who Received Incarceration Sentences

	(1) pre-PROTECT Ln Length		(2) post-PROTECT Ln Length		(3) post-*Booker* Ln Length		(4) post-*Gall* Ln Length							
									(2) vs. (1)	(3) vs. (1)	(3) vs. (2)	(4) vs. (1)	(4) vs. (2)	(4) vs. (3)
Independent Variables	***b***	***SE***	***b***	***SE***	***b***	***SE***	***b***	***SE***	***z***[a]	***z***	***z***	***z***	***z***	***z***
White male (reference)														
White female	–0.125	0.010***	–0.128	0.010***	–0.099	0.006***	–0.109	0.008***	–0.265	2.202*	2.412*	1.226	1.466	–0.975
Black male	0.066	0.005***	0.045	0.006***	0.053	0.003***	0.077	0.004***	–2.748**	–2.164*	1.172	1.547	4.424***	4.326***
Black female	–0.112	0.011***	–0.115	0.013***	–0.084	0.008***	–0.075	0.011***	–0.205	2.057*	2.105*	2.413*	2.449*	0.705
Hispanic male	–0.034	0.006***	–0.039	0.006***	–0.025	0.004***	0.011	0.005*	–0.610	1.292	1.941†	5.986***	6.388***	5.974***
Hispanic female	–0.162	0.011***	–0.156	0.011***	–0.132	0.007***	–0.085	0.009***	0.371	2.367*	1.833†	5.631***	5.041***	4.421***
Other male	–0.004	0.011	–0.036	0.011**	–0.005	0.007	–0.018	0.009*	–1.953†	–0.079	2.303*	–0.983	1.202	–1.190
Other female	–0.123	0.026***	–0.134	0.026***	–0.086	0.014***	–0.087	0.019***	–0.318	1.226	1.622	1.115	1.479	–0.013
Age (in years)	0.001	0.000***	0.001	0.000***	0.001	0.000	0.001	0.000***	0.335	2.743**	2.212*	2.189*	1.726†	–0.443
U.S. citizen	0.003	0.005	–0.007	0.006	–0.007	0.004*	0.026	0.005***	–1.273	–1.645	–0.060	3.229***	4.432***	5.810***
Less than high school	–0.008	0.009	–0.003	0.010	0.012	0.006*	–0.023	0.008***	0.322	1.851†	1.335	–1.297	–1.558	–3.668***
High school graduate	0.000	0.009	–0.002	0.010	0.018	0.006**	–0.007	0.008	–0.106	1.682†	1.676†	–0.555	–0.405	–2.576**
Some college	–0.015	0.009†	–0.005	0.010	0.008	0.006	–0.016	0.008*	0.731	2.105*	1.095	–0.015	–0.799	–2.388*
Drug	0.281	0.012***	0.319	0.014***	0.231	0.010***	0.215	0.014***	2.044*	–3.126***	–5.109***	–3.536***	–5.272***	–0.960
Immigration	0.120	0.013***	0.098	0.015***	0.050	0.010***	0.044	0.014***	–1.086	–4.220***	–2.697**	–3.937***	–2.655**	–0.346
Violent	0.284	0.015***	0.301	0.017***	0.183	0.011***	0.167	0.015***	0.762	–5.556***	–5.919***	–5.533***	–5.909***	–0.824
Fraud	0.005	0.013	0.010	0.015	0.013	0.010	0.023	0.014†	0.277	0.510	0.162	0.944	0.617	0.562

(Continued)

Appendix B.

(Continued)

	(1) pre-PROTECT Ln Length		(2) post-PROTECT Ln Length		(3) post-*Booker* Ln Length		(4) post-*Gall* Ln Length		(2) vs. (1)	(3) vs. (1)	(3) vs. (2)	(4) vs. (1)	(4) vs. (2)	(4) vs. (3)
Independent Variables	***b***	***SE***	***b***	***SE***	***b***	***SE***	***b***	***SE***	***z***[a]	***z***	***z***	***z***	***z***	***z***
Firearms	0.128	0.013***	0.148	0.015***	0.106	0.010***	0.133	0.014***	1.007	−1.320	−2.353*	0.258	−0.735	1.560
Other offense	0.068	0.015***	0.100	0.016***	0.127	0.011***	0.156	0.015***	1.464	3.278***	1.392	4.272***	2.563*	1.608
Upward departure	0.721	0.018***	0.783	0.019***	0.781	0.008***	0.751	0.010***	2.390*	3.046**	−0.098	1.477	−1.526	−2.419*
Substantial assistance dep.	−0.513	0.005***	−0.486	0.006***	−0.533	0.003***	−0.515	0.005***	3.469***	−3.342***	−7.132***	−0.382	−3.993***	3.132***
Other downward departure	−0.537	0.005***	−0.474	0.006***	−0.459	0.003***	−0.463	0.003***	8.104***	13.615***	2.279*	12.116***	1.500	−1.079
Trial	0.268	0.009***	0.261	0.008***	0.232	0.005***	0.257	0.007***	−0.623	−3.556***	−2.899**	−0.988	−0.318	2.917**
Presentence detention	0.182	0.005***	0.190	0.005***	0.180	0.003***	0.206	0.004***	1.131	−0.310	−1.581	3.811***	2.396*	4.862***
Criminal history	0.051	0.001***	0.049	0.001***	0.038	0.001***	0.030	0.001***	−1.355	−9.201***	−7.172***	−13.522***	−11.505***	−6.471***
(ln) presumptive sentence	0.669	0.002***	0.653	0.002***	0.712	0.001***	0.689	0.001***	−6.399***	21.610***	28.562***	9.476***	16.298***	−13.217***
Constant	0.820	0.017***	0.872	0.020***	0.721	0.013***	0.802	0.017***	1.974*	−4.600***	−6.472***	−0.752	−2.694**	3.782***
N	73,401		58,734		157,685		102,643							
Adjusted R^2	0.813		0.830		0.836		0.838							

[a]Based on a two-tailed test. †$p < 0.10$; *$p < 0.05$; **$p < 0.01$; ***$p < 0.001$.

Appendix C.

Logistic Regression Models of "Other" Downward Departures

	(1) pre-PROTECT Downward Dep		(2) post-PROTECT Downward Dep		(3) post-*Booker* Downward Dep		(4) post-*Gall* Downward Dep							
									(2) vs. (1)	(3) vs. (1)	(3) vs. (2)	(4) vs. (1)	(4) vs. (2)	(4) vs. (3)
Independent Variables	***b***	***SE***	***b***	***SE***	***b***	***SE***	***b***	***SE***	***z***[a]	***z***	***z***	***z***	***z***	***z***
White male (reference)														
White female	0.173	0.049***	0.189	0.064**	0.122	0.029***	0.077	0.034*	0.200	−0.881	−0.951	−1.607	−1.556	−1.017
Black male	−0.678	0.036***	−0.538	0.046***	−0.397	0.020***	−0.661	0.023***	2.401*	6.900***	2.793**	0.398	−2.387*	−8.715***
Black female	−0.167	0.060**	−0.172	0.082*	−0.185	0.037***	−0.266	0.044***	−0.040	−0.248	−0.149	−1.323	−1.015	−1.413
Hispanic male	0.130	0.032***	0.167	0.042***	−0.019	0.020	−0.432	0.022***	0.694	−3.925***	−4.002***	−14.340***	−12.627***	−13.817***
Hispanic female	0.551	0.049***	0.486	0.062***	0.129	0.031***	−0.290	0.038***	−0.810	−7.218***	−5.128***	−13.554***	−10.673***	−8.581***
Other male	−0.147	0.065*	−0.017	0.078	−0.073	0.035*	−0.111	0.041**	1.274	0.998	−0.650	0.470	−1.060	−0.700
Other female	0.168	0.116	0.145	0.143	0.177	0.063**	0.089	0.078	−0.125	0.064	0.202	−0.568	−0.347	−0.880
Age (in years)	0.005	0.001***	0.007	0.001***	0.003	0.001***	0.003	0.001***	1.233	−1.084	−2.337*	−1.586	−2.734**	−0.730
U.S. citizen	−0.174	0.030***	−0.229	0.038***	−0.289	0.018***	−0.245	0.022***	−1.148	−3.298***	−1.423	−1.927†	−0.372	1.503
Less than high school	−0.119	0.047*	−0.268	0.060***	−0.228	0.028***	−0.240	0.033***	−1.949†	−1.990*	0.599	−2.099*	0.405	−0.274
High school graduate	−0.283	0.048***	−0.389	0.061***	−0.276	0.028***	−0.262	0.033***	−1.378	0.120	1.691†	0.363	1.843†	0.333
Some college	−0.162	0.050***	−0.305	0.063***	−0.158	0.029***	−0.120	0.034***	−1.777†	0.062	2.105*	0.689	2.563*	0.848
Drug	1.090	0.072***	0.740	0.092***	0.641	0.044***	0.754	0.054***	−2.995**	−5.324***	−0.959	−3.753***	0.131	1.609
Immigration	1.548	0.074***	1.482	0.095***	1.228	0.046***	1.209	0.056***	−0.549	−3.663***	−2.414*	−3.648***	−2.483*	−0.260
Violent	1.048	0.085***	0.697	0.114***	0.550	0.054***	0.453	0.065***	−2.464*	−4.929***	−1.171	−5.539***	−1.862†	−1.142
Fraud	0.509	0.073***	0.398	0.093***	0.400	0.045***	0.524	0.054***	−0.944	−1.287	0.020	0.157	1.167	1.771†

(Continued)

Appendix C.

(Continued)

	(1) pre-PROTECT Downward Dep		(2) post-PROTECT Downward Dep		(3) post-*Booker* Downward Dep		(4) post-*Gall* Downward Dep		(2) vs. (1)	(3) vs. (1)	(3) vs. (2)	(4) vs. (1)	(4) vs. (2)	(4) vs. (3)
Independent Variables	***b***	***SE***	***b***	***SE***	***b***	***SE***	***b***	***SE***	***z***[a]	***z***	***z***	***z***	***z***	***z***
Firearms	0.872	0.078***	0.475	0.099***	0.407	0.047***	0.503	0.057***	−3.152***	−5.117***	−0.617	−3.837***	0.242	1.297
Other offense	0.748	0.081***	0.353	0.106***	0.476	0.048***	0.628	0.057***	−2.960**	−2.893**	1.052	−1.216	2.278*	2.031*
Trial	−0.602	0.063***	−0.577	0.072***	−0.301	0.028***	−0.344	0.036***	0.254	4.333***	3.565***	3.540***	2.897**	−0.934
Presentence detention	−0.248	0.026***	−0.312	0.035***	−0.549	0.017***	−0.583	0.019***	−1.447	−9.633***	−6.123***	−10.185***	−6.762***	−1.311
Criminal history	0.068	0.007***	0.057	0.009***	0.019	0.004***	0.028	0.005***	−1.007	−6.047***	−3.963***	−4.776***	−2.960**	1.411
Presumptive sentence	0.000	0.000	0.001	0.000***	0.003	0.000***	0.003	0.000***	3.289***	12.440***	6.511***	14.352***	8.332***	3.491***
Constant	−2.290	0.096***	−2.374	0.123***	−1.091	0.059***	−0.691	0.071***	−0.534	10.649***	9.401***	13.419***	11.860***	4.370***
N	72,807		58,662		156,406		104,390							
−2 log likelihood	68,128.8		46,263.2		180,084.9		129,834.2							

[a]Based on a two-tailed test. †$p < 0.10$; *$p < 0.05$; **$p < 0.01$; ***$p < 0.001$.