(Un)anticipated Effects of Sentencing Reform on the Disparate Treatment of Defendants

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Despite concerns over racial disparities in imprisonment across the United States, little empirical attention has been paid to how changing the structure of sentencing might affect levels of disparity. This article examines whether Ohio's shift to determinate sentencing corresponded with significant changes in legal and extralegal effects on case outcomes, both generally and differentially for African American and white defendants. Bilevel analyses of felony defendants from 24 jurisdictions reveal relatively few substantive changes in these effects over time. Some changes involved reductions in race-related disparities (e.g., in the severity of charges convicted on), with others reflecting *increased* disparity (e.g., higher imprisonment likelihoods for African Americans). Findings underscore a modest link between restructured sentencing and actual case outcomes *overall*, with some relatively mixed effects on levels of disparity.

Considerable academic attention has been given to the overrepresentation of African Americans in U.S. prisons relative to their representation in the general population, contributing to discussions about the magnitude and sources of race-related disparities in sentencing (Zatz 2000). As described by Mauer (1999), scholars have posed different possible explanations for the problem, including race group differences in crime rates, criminal histories, and treatment by prosecutors and judges, as well as the differential impact of changing sentencing policies. Extant research on the topic has focused primarily on race group differences in treatment

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by prosecutors and judges, but with little attention paid to changes in sentencing policies and how these changes ultimately affect court actors' decisionmaking. This article provides a rare look at how these processes are linked with an examination of Ohio's shift to determinate sentencing guidelines and the corresponding changes to the conviction and sentencing of African American and white defendants indicted on felony charges. The focus on sentencing reform and race group differences in treatment is relevant to a broader question of how changes to the structure of legal decisionmaking affects case outcomes, both generally and differentially for subgroups of defendants.

Aside from examining differences in sentencing based on a defendant's race alone, scholars have also described evidence of race group differences in *legal* effects on case outcomes (e.g., Albonetti 1990, 1991, 1999; Kramer & Ulmer 1996; LaFree 1985; Miethe & Moore 1985; Petersilia & Turner 1985; Spohn et al. 1982; Ulmer & Kramer 1996; Zatz 1985). Miethe and Moore (1985) argued that judges might weigh legal factors such as offense severity and weapons use differently based on a defendant's race. Their observation taps into the broader idea that the way in which legal decisionmaking is structured may differentially affect case outcomes for subgroups of defendants, such as African Americans and whites. Fewer constraints on decisionmaking permit more discretion in how court actors consider various legal factors, and race-related differences in these considerations are difficult to control due to their more "subtle" nature (as discussed by Albonetti 1990; Miethe & Moore 1985; Zatz 1985). Sentencing reforms that impose more formal constraints on judicial discretion might therefore alter race group differences in legal effects on sentencing. However, prosecutors might use the plea bargaining process to maneuver around restrictions placed on judicial discretion (Lagoy et al. 1979), possibly resulting in no absolute change in final outcomes for African American and white defendants. The focus here on both sentence severity *and* the seriousness of charges convicted on, if any, offers some additional insight into the ramifications of restructured sentencing for other stages of case processing.

Processes Underlying Disparate Treatment

Explanations for race-related disparities in case outcomes contribute to a more realistic assessment of whether changes in disparity can be expected with more formal constraints on judicial decisionmaking. Discussions of race (and other extralegal) effects on sentencing are often framed in a symbolic interactionist perspective. For example, Hawkins (1981), Albonetti (1991), and

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Steffensmeier et al. (1998) have put forth perspectives describing how judges, when equipped with limited information for assessing a defendant's criminal propensity, may act on their own preconceptions of higher-risk offenders. Defendants may be sentenced more severely when they possess extralegal characteristics that are overrepresented relative to the distribution of these characteristics in the general population, such as characteristics reflecting lower social and economic status (Nobiling et al. 1998; Spohn & Holleran 2000; Steffensmeier et al. 1998). Therefore, uncertainty in decisionmaking is reduced (Albonetti 1991) through the use of this "perceptual shorthand" (Hawkins 1981:230), resulting in higher imprisonment likelihoods and longer prison sentences for minorities, men, and/or the poor because of judicial interest in incarcerating higher-risk offenders for the purpose of reducing crime (Steffensmeier et al. 1998).

Evidence of harsher outcomes for African American and/or Hispanic defendants *in general* (relative to white Anglo defendants) has been offered by Albonetti (1991, 1997), Holmes and Daudistel (1984), LaFree (1985), Kramer and Steffensmeier (1993), Myers and Talarico (1987), Nelson (1992), Spohn (1990), Steffensmeier and Demuth (2000), Steffensmeier et al. (1998), and Zatz (1985), among others.[1] Based on the perspectives above, however, a defendant's race may also *interact* with other extralegal characteristics to produce even more severe sentences for particular subgroups of minorities. There is evidence that minorities receive more severe sentences when they possess *other* attributes that reinforce court actors' stereotypes of more dangerous offenders, such as being male and/or unemployed (e.g., Daly 1989, 1994; Miethe & Moore 1985; Nobiling et al. 1998; Spohn & Holleran 2000; Steffensmeier et al. 1998).[2]

[1] Evidence also suggests harsher treatment of other defendant groups such as males (e.g., Albonetti 1991, 1997, 1999; Daly 1987, 1990; Steffensmeier et al. 1993, 1998), the unemployed (e.g., Kramer & Ulmer 1996; Nobiling et al. 1998; Spohn & Holleran 2000), and less-educated individuals (e.g., Albonetti 1997, 1999). However, conflicting evidence suggests that one or more of these defendant groups are *not* treated more severely (e.g., Kleck 1985; Swigert & Farrell 1977; Thomson & Zingraff 1981; Unnever et al. 1980; Weisburd et al. 1991; Wheeler et al. 1982).

[2] Steffensmeier et al.'s (1998) research on Pennsylvania's determinate sentencing scheme revealed that young African American men have the highest incarceration likelihood of any subgroup defined by age, race, and sex. In their analysis of three urban jurisdictions, Spohn and Holleran (2000) also found higher likelihoods of imprisonment among young African American and Hispanic men compared to middle-aged white men, as well as higher likelihoods for unemployed African American and Hispanic men relative to employed white men. Nobiling et al. (1998) found that, in Kansas City (Missouri), unemployment coincides with higher imprisonment likelihoods for African American men only. Unemployment also coincides with higher incarceration likelihoods for young Hispanic men in Chicago, and unemployment results in *longer* prison sentences for young men and African American men.

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Related to this theme, *legal* factors also may be considered differently by court actors in order to justify harsher dispositions for groups of defendants they identify as "dangerous classes" (Black 1976; Quinney 2001; Sheldon 2001). Miethe and Moore (1985) argued that limiting an analysis of racial disparities in treatment to *general* race effects could mask potential differences in treatment if the effects of legal measures operate differently across race groups. They demonstrated the importance of examining legal effects on case outcomes broken down by a defendant's race in order to see if the structure of sentencing is different across race groups.[3] Miethe and Moore's (1985) analysis of charge reductions and sentence negotiations in Minnesota revealed no significant general or *main* effects of race, but several significant interaction effects involving race and legal characteristics such as weapons use, number of counts charged, and severity of the initial charge.

Zatz (1985) found evidence of related disparities in the guilty plea process of California courts, where African Americans who pled guilty actually received shorter sentences compared to Hispanics who plead guilty. A separate analysis of California defendants revealed that race and ethnicity condition the effects of offense seriousness, guilty pleas, and prior record on sentence severity (Petersilia & Turner 1985).

In her description of race group differences in case processing in the Superior Court of Washington, D.C., Albonetti (1990) observed that, relative to whites, African Americans are more likely to be detained prior to trial, are less likely to have private counsel, and are less likely to plead guilty. Although she did not directly examine the ramifications of these differences for sentence severity, Albonetti discussed how such discrepancies might ultimately generate harsher outcomes for minorities. Subsequent research by Albonetti (1991, 1999) underscores her original argument. For example, she found that pretrial detention constitutes a greater disadvantage for African Americans relative to whites (Albonetti 1991). Albonetti (1999) argued that defendants of higher socioeconomic status (SES) are better equipped to take advantage of particular options, which, in turn, increase the odds of more favorable dispositions.

Scholars examining Pennsylvania's trial courts of general jurisdiction have offered additional evidence of interaction effects involving race and legal factors. Kramer and Steffensmeier (1993) originally found that the main effects of race and gender on sentencing were very weak, while subsequent analyses by Kramer and

[3] Analyses of race that focus only on main effects will ignore such differences by pooling cases across race groups and controlling for various legal influences on case outcomes. Such analyses produce estimates of legal effects that simply reflect "average" effects for all defendants regardless of race.

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Ulmer (1996) and Ulmer and Kramer (1996) revealed that variables such as race and sex interact with type of plea and jurisdiction in determining imprisonment and sentence length.

Implications of Formal Change to the Sentencing Process

Given the multiple sources of sentencing disparities described above, opinions vary on whether restructuring the sentencing process will reduce, have no effect on, or even exacerbate race-related disparities in case outcomes. Proponents of sentencing reform have argued that racial and other extralegal disparities in sentencing should be less likely under schemes that reduce judicial discretion, especially determinate sentencing schemes with guidelines that specifically outline the weight of legal factors in decisions regarding imprisonment and the length of imprisonment (e.g., Davis 1971; Frankel 1972; compare Smith & Cabranes 1998). In conjunction with narrower sentence ranges relative to those under indeterminate sentencing, determinate schemes might be more effective for prohibiting general racial inequities that were once introduced by judges inclined toward sentencing minorities at the higher end of a range. The more specific guidelines for how legally relevant characteristics are to be considered should also reduce judges' ability to focus more heavily on some of these characteristics versus others in order to distribute harsher sentences to defendants they perceive as more dangerous. In short, their argument is that legal effects on sentencing should become *stronger* while extralegal effects become *weaker* under determinate sentencing.

At the other extreme, Mauer (1999) contends that a move from indeterminate to determinate sentencing may only make matters worse for minorities:

> In many jurisdictions around the country, minorities constitute two thirds or more of the defendants in many offense categories. To what extent are "get tough" sentencing policies a reflection of the race and ethnicity of those likely to be affected by such policies? As the hue of the defendant population changes, do legislators and judges set different sentencing standards? (1999:132)

For example, if a sentencing scheme dictates more severe sanctions for selling crack cocaine, then this might affect African Americans more than whites if the former are arrested more often for the offense (Ohio Governor's Task Force 1990).

Between these opposite views is the argument that such reforms may coincide with little to no change overall. If legally relevant factors operate differently for African Americans and whites

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under indeterminate sentencing, then these differences may persist under determinate sentencing either because legal factors can still be weighed differently by prosecutors (in plea agreements) (Alschuler 1978; McCoy 1984; Savelsberg 1992; Tonry & Coffee 1987), or because judges still retain some discretion when considering these factors (in the form of sentence ranges and reasons for departures from sentence ranges) (Kramer & Ulmer 1996; Miethe 1987; Ulmer 1997).

Regarding the idea of "displaced discretion" from judges to prosecutors, sentencing guidelines may lead prosecutors to use more discretion in order to maneuver around the restrictions placed on judicial discretion (Lagoy et al. 1979), and so disparities may persevere through the guilty plea process via charge reductions (Rathke 1982). White defendants could still face less-severe sentences if prosecutors are more reluctant to expose them to mandatory prison terms.

Amidst these speculations are a limited number of pre- versus post-sentencing reform analyses. We are aware of only two such *independent* evaluations of state-level sentencing reform, and both studies focused on Minnesota. Moore and Miethe's (1986) analysis revealed that legal variables such as offense severity and criminal history became stronger predictors of sentencing under the Minnesota sentencing guidelines, while extralegal variables such as race, sex, employment, and education became insignificant predictors. The second study involving a time-series impact assessment, however, revealed that extralegal variables were weak predictors of sentencing immediately after the implementation of Minnesota's guidelines, although this pattern appeared to gradually reverse with time (Stolzenberg & D'Allessio 1994).

Other *internal* evaluations of determinate sentencing schemes have also rendered the general conclusion that the main effects of extralegal influences on disparities (particularly a defendant's race/ethnicity) have been reduced in Florida, Oregon, and Washington (Bales 1997; National Council on Crime and Delinquency 1996; Parent et al. 1996). However, these findings are limited because different measures were examined in the pre- and post-guideline studies.

A theme underlying the findings from these studies is that the general effects of a defendant's race and other extralegal characteristics on sentence severity may be weak to nonexistent in determinate sentencing states, at least in the short run. However, the external validity of those conclusions may be restricted because the guidelines for states such as Minnesota, Florida, Oregon, and Washington are more rigid than those implemented more recently (as in Ohio), so the question remains as to whether reductions in disparate treatment can be expected under more recent reforms.

Other states have opted for guidelines that place fewer constraints on judicial discretion in order to avoid overly severe sanctions that conflict with common sensibilities about fairness (National Council on Crime and Delinquency 1982; Tonry 1996).

Research Questions and Methods

The analysis described here focused on Ohio trial courts of general jurisdiction during a two-year period surrounding a statewide shift from indeterminate sentencing to determinate sentencing with guidelines (on July 1, 1996). Appendix 1 provides a description of Ohio's reform.

The outcomes examined include the most serious charge an indicted felony defendant was convicted on (if any), whether a convicted felony defendant went to prison, and the length of incarceration for convicted defendants sent to prison. (Convicted felons with suspended prison sentences were treated as *not* sent to prison.) The last two outcomes are relevant to whether Ohio's sentencing guidelines did restructure the sentencing process, both generally and differentially for minorities. The outcome tapping seriousness of conviction is relevant to the earlier discussion of possible "displaced discretion" under the guidelines. Since Ohio's reform placed a heavier emphasis on the seriousness of the conviction offense for the sentencing decision, there may have been some changes in race effects as well as other extralegal effects on the seriousness of the conviction offense. Stronger relationships between extralegal factors and the level of conviction *post*-guidelines could reflect a change in how prosecutors are using their discretion. An analysis of how legal and other extralegal effects changed for African Americans and whites separately might also reveal more dramatic changes in some of these effects for minorities compared to whites.

The focus on whether changes to the structure of Ohio's sentencing process coincided with significant changes to disparities in treatment led to the following null hypotheses:

1. Ohio's shift to more structured sentencing did *not* coincide with significant changes in the general effects of a defendant's race (and other extralegal and legal factors) on case outcomes (seriousness of the conviction, imprisonment, and the length of imprisonment).
2. Considering African American and white defendants separately, Ohio's shift to more structured sentencing did *not* coincide with significant changes in legal and other extralegal effects on case outcomes for either group.

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Samples and Data

An examination of whether the conviction and sentencing processes were restructured under Ohio's guidelines requires information on defendants processed before and after the reform. Since the guidelines became effective on July 1, 1996, we targeted persons indicted between July 1, 1995, and June 30, 1996 (pre-guidelines), as well as persons indicted between January 1 and December 31, 1997 (post-guidelines). Rather than targeting persons indicted immediately after the reform went into effect, we selected indictments beginning six months after implementation to reduce the odds of capturing cases where court participants were still learning the nuances of the new scheme.[4] An important limitation of the post-guideline study period is that an analysis of cases processed within 18 months of the reform will only capture short-term effects that may have been temporary, as Stolzenberg and D'Allessio (1994) found in their analysis of the Minnesota guidelines.

The complete sample includes 5,573 persons indicted on felony charges from 24 counties in Ohio (pre-guideline = 2,898 cases; post-guideline = 2,675). These counties include the six major urban counties in the state in addition to a cross-section of other counties based on population, geographic location, and the intake rate into the Ohio Department of Rehabilitation and Correction (ODRC). The sample of indicted suspects was obtained by drawing a 5% sample of all indictments from the six largest counties during the two time periods, a 15% sample from the next six largest counties, and a 35% sample from the last 12 counties. Twice as many rural counties were selected to ensure enough cases for a reliable analysis. For a description of the counties selected for the study, see Wooldredge et al. (2002).

The 24 county samples were drawn at the study sites. A systematic sampling method was employed using lists of indicted cases provided by the district attorney of each county. These lists are ordered by date of indictment and case number (within each date). Only the first codefendant listed on any case with multiple defendants was selected since the unit of analysis is the individual.

The analysis of convictions is based on 5,573 suspects indicted on felony charges. The analyses of imprisonment ($N = 4,040$) and length of imprisonment ($N = 2,019$) include smaller groups due to the outcome measures (i.e., only convicted felons are eligible for

[4] The indictment date ultimately determined whether a person was subject to the new guidelines. Roughly 25% of the felony indictments that occurred during fiscal year 1995–96 (pre-guidelines) were disposed after the new scheme was implemented, but these cases were not subject to the new guidelines.

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imprisonment, and variation in prison sentence length is limited to imprisoned felons). The race-specific analyses also exclude a relatively small number of Mexican American defendants ($N = 219$ indicted suspects), although these defendants are included in the pooled models along with a measure of a defendant's ethnicity. Scholars have demonstrated significant differences in the treatment of African American and Mexican American defendants (e.g., Albonetti 1997; Nobiling et al. 1998; Spohn & Holleran 2000; Steffensmeier & Demuth 2000), so we did not combine these two groups for the analysis.

The information for the study was gathered from prosecutors' offices, felony probation offices, and the ODRC. Prosecuting attorneys' case files provided much of the information pertaining to the general characteristics of cases (e.g., types of offenses, felony levels convicted on, sentences, etc.) as well as more specific case characteristics (e.g., pretrial detention, type of attorney, guilty plea versus trial, etc.). Police reports are an important part of the prosecutors' files. Probation office files and the ODRC provided data on the characteristics of defendants, mainly from pre-sentence investigations (e.g., criminal histories, family status, employment status, drug/alcohol addiction, etc.).

All of the measures examined here are described in Table 1. Information is broken down by a defendant's race (white and African American), and by the disposition groups examined (felony indictments, felony convictions, and imprisonments).[5] Appendix 2 provides more details on these measures, including justifications for some of the scales examined.

Statistical Analyses

Ethnographic studies of court communities have uncovered important jurisdiction-level differences in court cultures and case processing practices (Eisenstein & Jacob 1978; Flemming et al. 1992; Nardulli et al. 1988; Ulmer 1997), and so bilevel analyses are presented that control cross-jurisdiction differences in the outcomes examined. Britt (2000), Kautt (2002), and Ulmer and Johnson (2004) have demonstrated the usefulness of bilevel modeling for this purpose.[6] An important difference from their studies, however, is that we do not attempt to model any significant differences in outcomes *across* jurisidictions. The limited degrees of freedom at the jurisdiction level, based on the analysis of just 24 counties, severely limits the number of aggregate-level

[5] A more elaborate breakdown by the pre- versus post-guideline periods is available upon request.

[6] The statistical software used for the analysis was HLM5 (Raudenbush et al. 2000).

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Table 1. Variable Means (with Standard Deviations) by Defendant's Race

	Indictments		Convictions		Imprisonments	
	White	African Amer.	White	African Amer.	White	African Amer.
Outcomes						
Conviction level	2.5 (1.5)	2.6 (1.5)	–	–	–	–
Prison sentence	–	–	0.46 (0.50)	0.54 (0.49)	–	–
#months in prison	–	–	–	–	28.4 (34.0)	23.6 (31.4)
Legal Predictors						
Guidelines in effect	0.49 (0.50)	0.48 (0.50)	0.50 (0.50)	0.46 (0.50)	0.46 (0.50)	0.44 (0.50)
Plea bargain	0.75 (0.43)	0.78 (0.41)	0.85 (0.36)	0.87 (0.33)	0.84 (0.36)	0.86 (0.35)
F1 indictment/conviction	0.08 (0.27)	0.08 (0.28)	0.05 (0.21)	0.04 (0.20)	0.09 (0.29)	0.06 (0.25)
F2 indictment/conviction	0.12 (0.32)	0.12 (0.33)	0.08 (0.27)	0.08 (0.27)	0.13 (0.33)	0.11 (0.31)
F3 indictment/conviction	0.17 (0.37)	0.19 (0.39)	0.18 (0.39)	0.22 (0.41)	0.22 (0.42)	0.27 (0.44)
F4 indictment/conviction	0.42 (0.49)	0.39 (0.49)	0.45 (0.50)	0.42 (0.49)	0.37 (0.48)	0.35 (0.48)
F5 indictment/conviction	0.20 (0.40)	0.20 (0.40)	0.24 (0.43)	0.24 (0.43)	0.17 (0.37)	0.19 (0.39)
#counts indicted/ convicted on	1.9 (1.9)	1.8 (1.6)	1.6 (1.2)	1.4 (1.0)	2.3 (2.1)	2.2 (1.9)
Personal crime	0.21 (0.41)	0.20 (0.40)	0.22 (0.41)	0.21 (0.41)	0.32 (0.46)	0.28 (0.45)
Homicide	0.01 (0.09)	0.01 (0.10)	0.01 (0.11)	0.01 (0.10)	0.02 (0.15)	0.02 (0.14)
#gun specifications	0.03 (0.35)	0.05 (0.38)	0.01 (0.09)	0.01 (0.14)	0.01 (0.13)	0.02 (0.19)
#other specifications	0.11 (0.47)	0.21 (0.78)	0.03 (0.19)	0.03 (0.26)	0.04 (0.25)	0.05 (0.31)
Drug/alcohol addiction	0.67 (0.37)	0.68 (0.34)	0.67 (0.39)	0.70 (0.36)	0.71 (0.37)	0.74 (0.31)
Juvenile incarceration	0.12 (0.27)	0.14 (0.28)	0.12 (0.29)	0.15 (0.31)	0.17 (0.34)	0.17 (0.32)
#prior prison terms	0.48 (0.82)	0.78 (1.3)	0.48 (0.91)	0.86 (1.5)	0.73 (1.1)	1.1 (1.7)
Extralegal Predictors						
Male	0.80 (0.40)	0.82 (0.38)	0.80 (0.40)	0.83 (0.38)	0.88 (0.33)	0.88 (0.32)
#children living with def.	0.53 (0.79)	0.52 (0.81)	0.53 (0.85)	0.52 (0.86)	0.44 (0.73)	0.48 (0.81)
No high school degree	0.52 (0.40)	0.55 (0.37)	0.53 (0.42)	0.56 (0.39)	0.59 (0.41)	0.58 (0.38)
Employed	0.51 (0.39)	0.46 (0.37)	0.52 (0.42)	0.45 (0.39)	0.43 (0.40)	0.43 (0.37)
Other Controls						
Court-appointed attorney	0.42 (0.49)	0.39 (0.49)	0.45 (0.50)	0.42 (0.49)	0.48 (0.50)	0.45 (0.50)
Pretrial incarceration	0.25 (0.43)	0.30 (0.46)	0.29 (0.46)	0.35 (0.48)	0.46 (0.50)	0.54 (0.50)
N	2,703	2,651	1,908	1,960	898	1,073

predictors that can be examined (Bryk & Raudenbush 1992). Despite this limitation, the bilevel modeling strategy still allowed us to test whether the microlevel effects examined differ significantly across the 24 jurisdictions, similar to analysis of covariance tests of significant between-group differences in individual-level relationships (Bryk & Raudenbush 1992). This information is important for evaluating the stability of legal and extralegal effects across different court environments (e.g., Ulmer & Johnson 2004).[7]

Ordered logit models were estimated for the ordinal outcome measure of conviction severity described in Appendix 2.[8] (For these particular data, ordered logit was preferred over ordered probit because estimates from the latter are derived under the assumption that the distribution of probabilities associated with each category along the scale is cumulative normal.) Logistic models were estimated for the dichotomous outcome measure of prison sentence, and linear models were estimated for the outcome measure of number of months in prison.[9] All predictors were centered on the means for the jurisdiction from which the case came rather than the overall means for the entire sample. This approach restricts explanation to variation in the outcomes *within* (versus between) the 24 jurisdictions (Bryk & Raudenbush 1992).

The analysis of how case outcomes changed under Ohio's guidelines, both generally and for specific race groups, involved estimating models with *both* of the following: the full set of predictors described in Table 1 (excluding the measures of race/ethnicity

[7] The first step in the bilevel analysis involved an examination of the unconditional models (with no predictors) to estimate the proportion of variation in each outcome existing at the individual versus aggregate levels of analysis (Level 1 and Level 2, respectively). The second step involved entering the Level 1 predictors to estimate the relationships of interest (i.e., the "averaged" legal and extralegal effects across all 24 counties), and to see if these individual-level relationships vary significantly across counties. A finding that a Level 1 relationship does not vary significantly across counties implies uniformity in that particular effect across jurisdictions.

[8] The directions of relationships from an ordinal logit model are interpreted differently from logistic or linear regression due to the cumulative probabilities involved. Consider a simplified example with three ordered categories, and the third (highest) value on the outcome scale is the comparison group. The coefficient for any predictor of this outcome would reflect change in the expected log-odds of scoring the *lowest* value on the ordinal scale (relative to the other two values) combined with the expected log-odds of scoring *either* of the lowest two values relative to the third (Raudenbush et al. 2000:160). A positive coefficient, therefore, means that higher values on the predictor scale coincide with *higher odds* of scoring *lower values* on the outcome scale. A description of these models can be found in Raudenbush et al. (2000:120–2, 157–61).

[9] Several outliers on prison sentence length created a potential problem for the analysis because the vast majority of prison-bound felons received no more than 180 months (15 years) in prison, whereas about 40 defendants received well over this amount (some receiving several consecutive life terms). To adjust for problems related to nonrandom error, all cases greater than 180 months were collapsed into the category of 180 months.

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in the race-specific models), and a set of product terms consisting of "under-guidelines" multiplied by each predictor in the first set. By including both sets of predictors in a model, results for the first set represent the effects of all measures during the pre-guideline period, and results for the second set represent the *changes* in these effects for the post-guideline period. The sum of the two coefficients for any one measure reveals the actual post-guideline effect. All product terms were initially included in each model, and then reduced models were estimated without the nonsignificant product terms. (A nonsignificant term indicates no change in effect between the two periods examined.) There were no differences between the two sets of models in the statistical significance of the measures included, and so results for the reduced models are presented in order to simplify the presentation.

The pooled models (including all defendants) tap changes in effects over time in general, and the race-specific models tap changes in these effects for white and African American defendants *separately*.[10] For the analysis of race group differences, tests for statistically significant differences in legal and extralegal effects between race groups were conducted in order to see if any pre-guideline differences in these effects were reduced after the reform. The formula used here, introduced by Clogg et al. (1995) for samples larger than 120, is a *z*-test for a nonzero difference between race-specific regression coefficients, adjusted for nonindependent samples.

Scholars have noted the importance of adjusting sample biases associated with examining convicted and imprisoned defendants only. A sample of convicted defendants might not be representative of all defendants, so the race/imprisonment relationship could be biased if an unmeasured correlate of race also affects conviction likelihoods and sentence type (Albonetti 1991; Klepper et al. 1983; Zatz & Hagan 1985; Myers & Talarico 1987). To adjust sample bias, a defendant's likelihood of reaching a particular stage of case processing can be estimated and then entered as a control variable in a model predicting an outcome at the next stage of processing (see Heckman and Robb [1985] for methods to correct selection bias). This procedure was followed by first estimating logistic models predicting a felony conviction (for indicted defendants) and prison sentences (for convicted felons), then entering the likelihoods calculated from these equations into

[10] It is important to note that we also examined period-specific models and tested for differences in legal and extralegal effects *between* the two periods. Using a test for the equality of regression coefficients designed for nonindependent samples (Clogg et al. 1995), conclusions from these models were identical to those from the models described above. Therefore, to reduce the presentation of data, we present the aforementioned models.

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models predicting imprisonment and the length of imprisonment, respectively.[11]

Results and Discussion

Tables 2, 3, and 4 each display a set of pooled and race-specific models for a single outcome measure (conviction level, prison sentence, and prison sentence length, respectively). The "before" and "after" columns display *significantly* different pre- and post-guideline effects, respectively. A coefficient that is centered between the two columns is the pre-guideline effect, indicating no significant difference from the post-guideline effect.

Bilevel modeling provides a single estimate for each predictor that represents an "average" effect across all 24 counties, and it also tests whether the 24 county-specific estimates of that effect differ significantly (for evaluating the stability of the averaged estimate). The pooled estimates are displayed in the tables.

General Effects of Restructured Sentencing

The first pair of numerical columns in Tables 2, 3, and 4 each displays a pooled model for all race/ethnic groups combined. These models are relevant to understanding whether conviction and sentencing outcomes changed *in general* under Ohio's determinate scheme (i.e., whether there were significant changes in the strength of extralegal and legal effects on these dispositions).

The pooled model of conviction level (Table 2) reveals that legal factors were much stronger predictors of conviction seriousness relative to extralegal factors, for both the pre- and post-guideline periods. It also appears that a few legal effects became even stronger after the reform. The stronger impact of Felony 2 and Felony 3 indictments during the second period, in conjunction with the consistent and strong effect of Felony 1 indictments over time, reveal a tighter correspondence between indictment level and conviction level under the guidelines. (The negative coefficients for these predictors indicate that defendants in Group 1 of each dummy measure were less likely to be acquitted/dismissed and less likely to be convicted on less serious charges compared to the reference group of Felony 4 and Felony 5 indictments.) There was also a significant change in the impact of sentence specifications for crimes involving guns over time, although the separate effect during

[11] We also considered the inclusion of probabilities for specific *types* of felony convictions in the sentencing models. Results for the other predictors were similar in magnitude and statistical significance in models with the probability of a felony conviction in general versus models with the probabilities for specific felony convictions. For the purpose of parsimony, we include the general measure in the models presented here.

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Table 2. Random-Coefficient Ordered Logit Models of Conviction Level (Maximum-Likelihood Coefficients Reported with Standard Errors in Parentheses)

	Pooled		Whites		African Americans		
	Before	After	Before	After	Before	After	*z*-test
Intercept (group 0)	−2.92		−3.02		−3.09		
ΔIntercept (1)	1.42		1.46		1.46		
ΔIntercept (2)	2.54		2.54		4.84		
ΔIntercept (3)	4.83		4.87		6.74		
ΔIntercept (4)	6.62		6.55		8.26		
Legal Predictors							
Under guidelines	0.56***		0.91***		0.45*		
	(0.09)		(0.13)		(0.19)		
Plea bargain	−3.46***		−3.15***		−3.65***	−4.05***	3.91***
	(0.08)		(0.12)		(0.16)	(0.19)	
Felony 1 indictment	−3.53***		−3.58***		−3.84***		
	(0.12)		(0.18)		(0.19)		
Felony 2 indictment	−2.16***	−2.79***	−2.23***	−2.94***	−2.51***		
	(0.13)	(0.15)	(0.19)	(0.21)	(0.16)		
Felony 3 indictment	−1.70***	−2.13***	−1.72***	−2.22***	−1.93***		
	(0.09)	(0.13)	(0.13)	(0.19)	(0.12)		
#indicted counts	−0.10***		−0.09***		−0.14***		
	(0.02)		(0.02)		(0.02)		
Personal crime	0.29***		0.33**		0.36**		
	(0.08)		(0.12)		(0.13)		
Homicide	−3.12***		−4.08***		−2.42***		
	(0.36)		(0.62)		(0.43)		
#gun specs (indicted)	0.21	−0.15	−0.04		−0.10		
	(0.18)	(0.18)	(0.10)		(0.10)		
#other specs (indicted)	−0.08*		−0.12	−0.61*	−0.05		
	(0.04)		(0.09)	(0.29)	(0.05)		
Drug/alcohol addiction	0.06		0.19		−0.22		
	(0.07)		(0.10)		(0.12)		
Juvenile incarceration	−0.08		0.02		−0.25		
	(0.09)		(0.14)		(0.14)		
#prior terms	−0.01		0.07		−0.05		
	(0.02)		(0.05)		(0.03)		

(Continued)

Table 2. Continued

	Pooled		Whites		African Americans		
	Before	After	Before	After	Before	After	*z*-test
Extralegal Predictors							
African American	−0.29***	0.03	–		–		
	(0.08)	(0.10)					
Mexican American	−0.17		–		–		
	(0.19)						
Male	−0.04		−0.08		−0.04		
	(0.07)		(0.10)		(0.11)		
#children	0.01		−0.08		0.07		
	(0.03)		(0.05)		(0.05)		
No high school degree	−0.13*		−0.08		−0.30**		
	(0.06)		(0.10)		(0.11)		
Employed	0.01		0.32*	−0.06	−0.02		2.00*
	(0.07)		(0.14)	(0.16)	(0.11)		
Other Controls							
Court-appointed attorney	−0.38***	−0.04	−0.31***		−0.34**	0.12	−2.52**
	(0.07)	(0.08)	(0.08)		(0.11)	(0.09)	
Pretrial incarceration	−0.69***		−0.70***		−0.65***		
	(0.06)		(0.09)		(0.09)		
N defendants	5,573		2,703		2,651		
Prop. variation in outcome within jurisdictions	0.94		0.93		0.95		
Prop. within-variation explained	0.40		0.41		0.40		

Notes: Models estimated for defendants indicted on felony charges. Multiple intercepts reflect changes in the intercept when moving from one group to the next on the outcome scale, consistent with the possibility that change in the odds of conviction may not be constant for each unit increase on conviction severity.

*$p<0.05$; **$p<0.01$; ***$p<0.001$.

z-test values reflect significant differences between race groups in ML coefficients, based on Clogg et al.'s (1995) formula. For any row with separate before/after coefficients displayed for a race group, the specific ones for the test are described in the text.

either period was nonsignificant. The change itself is significant only because of the shift from a positive to a negative relationship.

An important change over time also occurred in the general effect of a defendant's race. African American defendants were more likely to be convicted on more serious felonies during the pre-guideline period, but this relationship became nonsignificant after the reform due to a significant change over time. While this change might reflect a shift in discretion to prosecutors, it actually coincided with more *similar* experiences for African Americans relative to whites after the reform. Regarding other extralegal effects, however, the reform did *not* change the significant effect of a defendant's education. Defendants without a high school degree were more likely to be convicted on more serious felonies during *both* periods examined.

The shift from indeterminate to determinate sentencing in Ohio, therefore, coincided with either comparable or *stronger* legal effects on conviction severity, and with either comparable or *weaker* extralegal effects. Contrary to predictions based on the thesis of displaced discretion, any increase in prosecutorial discretion that might have occurred after Ohio's sentencing reform did not magnify pre-guideline disparities in conviction severity for African American defendants relative to white defendants in general. On the contrary, the move to determinate sentencing actually coincided with more similar outcomes between these race groups overall.

The pooled coefficients displayed in Table 3 reveal that the shift from indeterminate to determinate sentencing had a rather modest influence on changing legal effects on imprisonment, reflected only in the higher likelihood of a prison sentence for a Felony 1 conviction. The modest effect could be explained by the more powerful effect of plea bargaining during the post-guideline period, where felony defendants who pled guilty with prosecutorial agreements were less likely to go to prison. This observation raises the possibility of prosecutors using the guilty plea process to maneuver around some of the guideline restrictions, consistent with the predictions of Alschuler (1978), Lagoy et al. (1979), and Tonry and Coffee (1987). Additional analyses reveal that the proportions of defendants pleading guilty with agreements did not change over time, suggesting that any real change involved the substance of these agreements. Given the tighter correspondence between indictment and conviction severity over time (noted above), the substance of these agreements may have focused more on sentence than on charge reductions.

While prosecutors may have offered more attractive sentence bargains under the guidelines, the results for extralegal effects indicate that convicted African Americans were more likely than convicted whites to go to prison *after* the reform, whereas a defendant's race had no significant impact on prison sentences prior

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Table 3. Random-Coefficient Logistic Models of Prison Sentences (Maximum-Likelihood Coefficients Reported with Standard Errors in Parentheses)

	Pooled		Whites		African Americans		z-test
	Before	After	Before	After	Before	After	
Intercept	0.12		0.06		0.79		
Legal Predictors							
Under guidelines	0.13		0.12		0.45		
	(0.22)		(0.32)		(0.41)		
Plea bargain	−0.04	−0.65**	0.12	−0.59*	−0.17	−0.78*	
	(0.17)	(0.21)	(0.26)	(0.30)	(0.25)	(0.30)	
Felony 1 conviction	1.85***	2.94***	2.15***		2.14***		
	(0.35)	(0.49)	(0.45)		(0.42)		
Felony 2 conviction	1.34***		1.22***		1.28***		
	(0.17)		(0.24)		(0.26)		
Felony 3 conviction	0.69***		0.57***		0.81***		
	(0.11)		(0.17)		(0.16)		
#convicted counts	0.16***		0.12*		0.21**		
	(0.04)		(0.05)		(0.08)		
Personal crime	0.43***		0.58***		0.22		
	(0.11)		(0.16)		(0.16)		
Homicide	0.86		4.08		0.26		
	(0.67)		(3.47)		(0.93)		
#gun specs (convicted)	2.12*		5.33		1.30		
	(0.99)		(5.51)		(1.04)		
#other specs (convicted)	0.37		0.76*		0.16		
	(0.22)		(0.38)		(0.27)		
Drug/alcohol addiction	0.40***		0.23		0.54***		
	(0.11)		(0.15)		(0.16)		
Juvenile incarceration	0.34*		0.62**		0.49	−0.32	2.61**
	(0.13)		(0.21)		(0.29)	(0.34)	
#prior terms	0.42***		0.73***		0.25 ***		4.66***
	(0.05)		(0.09)		(0.05)		

(Continued)

Table 3. Continued

	Pooled		Whites		African Americans		z-test
	Before	After	Before	After	Before	After	
Extra-legal Predictors							
African American	0.02	0.35*	–		–		
	(0.12)						
Mexican American	0.22		–		–		
	(0.28)						
Male	0.55***		0.52***		0.26	0.81**	
	(0.10)		(0.15)		(0.20)	(0.25)	
#children	−0.15*	0.02	−0.15*		0.02		
	(0.06)	(0.07)	(0.07)		(0.07)		
No high school degree	0.25*		0.38**		0.02		
	(0.10)		(0.14)		(0.15)		
Employed	−0.48***		−0.70***		−0.12		−2.90**
	(0.10)		(0.14)		(0.15)		
Other Controls							
Court-appointed attorney	−0.10		−0.36*	0.04	0.12	−0.28	
	(0.19)		(0.17)	(0.20)	(0.17)	(0.20)	
Pretrial incarceration	1.70***		1.34***		2.04***		−2.41*
	(0.14)		(0.20)		(0.21)		
Odds of felony conviction	1.43*		1.22		1.62*		
	(0.56)		(0.81)		(0.83)		
N defendants	4,040		1,908		1,960		
Prop. variation in outcome within jurisdictions	0.98		0.96		0.97		
Prop. within-variation explained	0.43		0.45		0.45		

Notes: Models estimated for defendants convicted on felony charges. *z*-test values reflect significant differences between race groups in ML coefficients, based on Clogg et al.'s (1995) formula. For any row with separate before/after coefficients displayed for a race group, the specific ones for the test are described in the text.

*$p<0.05$, **$p<0.01$, ***$p<0.001$.

to the reform. (A defendant's ethnicity was unrelated to imprisonment during both periods.) This finding runs counter to predictions made by sentencing reform advocates regarding a weaker race effect under more structured sentencing, yet it is consistent with Mauer's (1999) speculation (discussed earlier) that race-related disparities may actually *increase* under determinate sentencing.

In contrast to a defendant's race, the significant pre-guideline effect of "children" (regarding family size) became nonsignificant after the reform. Yet this is the only extralegal effect that was removed over time. Note that men were more likely to go to prison during both periods, as were defendants without high school degrees and those who were unemployed. Even the effect of pretrial incarceration posed a similar disadvantage over time. Along with the findings of limited change in legal effects, these results generally refute the arguments of sentencing reform advocates that more structured sentencing will elevate the importance of legal factors while reducing the importance of extralegal effects on imprisonment. The exceptions for Felony 1 convictions and a defendant's family status are noteworthy, but these must be considered against the *stronger* post-guideline effect of a defendant's race and the absence of significant changes in the vast majority of effects overall.

While these findings might seem counterintuitive to proponents of sentencing guidelines, the significant post-guideline race effect is actually consistent with Kramer and Steffensmeier's (1993) and Steffensmeier et al.'s (1998) analyses of Pennsylvania's guidelines (similar to Ohio's scheme), where they found a significant but "weak" main effect for race. The race effect found here is not weak, although it might be considered modest, translating into a probability of imprisonment that is 8% higher for African Americans (61% overall) relative to whites (53% overall).[12]

The pooled results for length of imprisonment (Table 4) are also inconsistent with the argument that legal effects should become stronger and extralegal effects weaker under sentencing guidelines. Only the change involving Felony 3 convictions reflects stronger legal effects over time, where prison terms nearly doubled in length for these offenses. This contrasts, however, with significantly *weaker* effects of Felony 1 and Felony 2 convictions under

[12] Related to the other significant extralegal effects, previous studies *not* focused on jurisdictions under sentencing guidelines have also revealed a significant main effect for education (Albonetti 1999) and employment status (Chiricos & Bales 1991; Farrington & Morris 1983; Myers 1987; Walsh 1987). These particular findings are also consistent with extant studies of more inclusive measures of socioeconomic status, such as those examined by Hagan et al. (1980) and Spohn et al. (1982). The lower imprisonment likelihoods found for women also reinforce the findings of Albonetti (1999), Atkinson and Newman (1970), Hagan et al. (1979), and Pope (1975).

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Table 4. Random-Coefficient Linear Models of Prison Sentence Length (Maximum-Likelihood Coefficients Reported with Standard Errors in Parentheses)

	Pooled		Whites		African Americans		z-test
	Before	After	Before	After	Before	After	
Intercept	26.50		27.28		26.91		
Legal Predictors							
Under guidelines	−2.80*		−5.30*		−1.80		
	(1.37)		(2.29)		(1.56)		
Plea bargain	−8.98***		−7.06 *		−11.54***		
	(1.81)		(2.91)		(2.62)		
Felony 1 conviction	69.06***	49.64***	74.01***	56.01***	65.69***	44.39***	
	(3.30)	(3.90)	(5.29)	(6.09)	(4.70)	(5.46)	
Felony 2 conviction	35.91***	18.98***	32.89***	20.28***	41.75***	18.21***	
	(2.61)	(3.27)	(4.28)	(5.31)	(3.50)	(4.36)	
Felony 3 conviction	5.65***	10.60***	7.91**	16.24***	4.46*		2.56**
	(1.70)	(2.50)	(3.03)	(4.18)	(1.93)		
#convicted counts	1.76***		1.60*		1.28*		
	(0.46)		(0.80)		(0.65)		
Personal crime	5.13***		4.78*		4.08*		
	(1.33)		(2.21)		(1.80)		
Homicide	22.76***		15.06*		28.84***		
	(3.87)		(6.07)		(5.44)		
#gun specs (convicted)	8.38**		26.93***		2.72		3.15***
	(3.15)		(6.75)		(3.66)		
#other specs (convicted)	6.66***		13.71***	−7.00	4.73*		2.05*
	(1.81)		(3.78)	(8.36)	(2.21)		
Drug/alcohol addiction	0.06		0.20		−0.14		
	(1.56)		(2.50)		(2.20)		
Juvenile incarceration	−0.54		0.95		−3.87		
	(1.58)		(2.58)		(2.98)		
#prior terms	−0.56		−0.58		−1.39		
	(0.44)		(0.97)		(2.16)		

(Continued)

Table 4. Continued

	Pooled		Whites		African Americans		z-test
	Before	After	Before	After	Before	After	
Extralegal Predictors							
African American	−2.36*		–		–		
	(1.17)						
Mexican American	−5.02		–		–		
	(3.50)						
Male	−0.22		3.18		−2.53		
	(1.70)		(2.78)		(2.30)		
#children	0.14		0.51		−0.15		
	(0.66)		(1.18)		(0.84)		
No high school degree	−1.23		−2.61		−1.14		
	(1.33)		(2.14)		(1.82)		
Employed	−0.24		−1.82		−0.69		
	(1.40)		(2.28)		(1.93)		
Other Controls							
Court-appointed attorney	0.63		−2.02		2.74*		
	(1.07)		(1.88)		(1.38)		
Pretrial incarceration	−7.63***		−6.38	−0.40	−10.54***		2.44**
	(2.15)		(4.07)	(3.00)	(2.89)		
Odds of imprisonment	12.69*		8.31		16.45*		
	(5.57)		(9.68)		(7.50)		
N defendants	2,019		898		1,073		
Prop. variation in outcome within jurisdictions	0.93		0.94		0.92		
Prop. within-variation explained	0.70		0.84		0.74		

Notes: Models estimated for convicted defendants sent to prison. *z*-test values reflect significant differences between race groups in ML coefficients, based on Clogg et al.'s (1995) formula. For any row with separate before/after coefficients displayed for a race group, the specific ones for the test are described in the text.

*$p<0.05$, **$p<0.01$, ***$p<0.001$.

the guidelines (although both remained powerful predictors of sentence length over time). Prison terms for Felony 2s were cut nearly in half under the new guidelines, and terms for Felony 1s were shortened by roughly 25%. The average reduction in sentence length for a Felony 1 conviction might reflect judicial discretion aimed at compensating for the higher imprisonment likelihoods for Felony 1 defendants (see Table 3), although no such "balance" appeared for Felony 2 convictions.

In contrast to the findings for imprisonment, no significant changes in *any* of the extralegal effects on sentence length occurred after the reform, and most of these effects were nonsignificant prior to the reform. African Americans faced significantly *shorter* terms than whites, on average, during both periods examined, although the difference of roughly two months is modest. (Kramer and Steffensmeier [1993] also found no significant main effect of race on the length of imprisonment under Pennsylvania's guidelines.) Since lower-status defendants did not face longer sentences prior to the reform, the guidelines would not necessarily reduce these already weak extralegal effects. By the same token, however, these findings also refute the argument that more structured sentencing leads to *greater* disparities in sentence length based on extralegal attributes (Mauer 1999).

It is worth underscoring the absence of significant extralegal effects *other* than race on sentence length, versus the number of significant extralegal effects on imprisonment. In their review of related research, Chiricos and Crawford (1995) observed that extralegal characteristics seem to maintain weaker influences on the length of imprisonment compared to the likelihood of imprisonment. This theme is reinforced with the findings presented here.

Also important to note is the lack of variation in race effects on imprisonment and sentence length *across* the 24 jurisdictions. The same observation also held for all other extralegal effects, suggesting that these defendant characteristics have more uniform effects across jurisdictions than would be expected based on recent bilevel studies (Britt 2000; Ulmer & Johnson 2004). While the only jurisdiction differences in *legal* effects on imprisonment involved a defendant's history of imprisonment, there were significant jurisdiction differences in the *length* of imprisonment based on convictions for Felony 1s, Felony 2s, and homicide. These between-jurisdiction differences existed both before *and* after the reform.[13]

[13] Implementation of the guidelines also did not coincide with significant changes *overall* in imprisonment likelihoods and the average length of prison sentences, controlling for all other predictors. A 6% drop in imprisonment rates occurred after the reform, but this zero-order difference could be attributable to compositional differences in caseloads over time (perhaps a reflection of the national drop in violent crime rates during the mid-1990s, as described by Eck and Maguire 2000).

Race-Specific Effects of Restructured Sentencing

Although findings from the pooled models suggest that Ohio's sentencing reform had a modest impact on restructuring the conviction and sentencing processes *in general*, it is possible that these processes were restructured differently between race groups. The race-specific models are displayed in the second and third pair of numerical columns in Tables 2, 3, and 4. For the purpose of data reduction, *z*-test values for differences in race-specific effects are presented for statistically significant differences only.

Regarding the models of conviction level (Table 2), race group comparisons of changes in the *y*-intercepts for each category of the ordinal outcome revealed no statistically significant race differences in these coefficients. (Coefficients *within* each group also remained stable over time.) The vast majority of legal and extralegal effects were also similar in magnitude between race groups for both periods examined. An exception to this theme for legal effects was a significantly stronger effect of plea bargaining for African Americans after the reform, which generated a significant post-guideline difference between the two race groups ($z = 3.91$; $p < 0.001$) versus no significant race group difference prior to the reform. Only after the reform were African Americans who pled guilty with agreements more likely than whites to be convicted on more serious charges.

Other changes in legal effects also occurred for white defendants that did not occur for African Americans, but none of these changes generated significant differences in treatment between the two groups. Whites indicted on Felony 2s and Felony 3s were more likely to be convicted on these charges after (versus before) the reform, but differences in the race-specific relationships during either period were *not* significant. Similar findings also emerged for "other specifications."

All but one of the extralegal effects on conviction levels were also similar between African Americans and whites during each period. The exception was a defendant's employment status, which maintained a modest effect for whites but not for African Americans prior to the reform, resulting in a significant race group difference for the pre-guideline period *only* ($z = 2.00$; $p < 0.05$). A significant post-guideline change in this effect for whites subsequently resulted in no relationship between employment status and conviction levels among whites, and no significant post-guideline difference in the effect between race groups.

Based on these findings, Ohio's sentencing guidelines appear to have had a modest effect on altering conviction likelihoods for African Americans relative to whites. Results for plea bargains demonstrate a disadvantage for African Americans relative to whites under the guidelines, but a previous advantage held by

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employed white defendants disappeared after the reform. In conjunction with the aforementioned change in the *main* effect of a defendant's race on conviction levels, moving from a significant advantage for whites overall to no advantage under the guidelines, these results provide little evidence that any shift in discretion that occurred from judges to prosecutors resulted in greater *dis*advantages for African American defendants. The significant changes in and of themselves, however, still imply that restructuring judicial decisionmaking may have ramifications for decisions made at *other* stages of case processing (Alschuler 1978; Packer 1968). Discretion may therefore be "displaced" from judges to prosecutors, to some extent, but not necessarily to the greater detriment of minority defendants.

The race-specific models of imprisonment (Table 3) also reveal some changes in legal and extralegal effects over time, but with very little impact on race group differences overall. For example, for both whites *and* African Americans, plea bargaining was unrelated to imprisonment likelihoods prior to the reform yet coincided with significantly lower likelihoods of imprisonment afterward. The magnitude of this change was comparable for each race group as well.

Two other significant changes also occurred for African American defendants only. The positive effect on imprisonment of being incarcerated as a juvenile remained stable over time for whites, but the positive pre-guideline effect for African Americans changed significantly in the *opposite* direction under the guidelines. This change created a significant disadvantage for whites relative to African Americans *after* the reform only ($z = 2.61$; $p < 0.01$).

The second change for African Americans involved a stronger post-guideline effect of being male, resulting in a significant post-guideline effect overall (versus a nonsignificant effect prior to the reform). The effect for whites remained stable (and significant) over time. However, no significant race group differences in the magnitude of this effect existed during *either* period.

A few race group differences also involved effects that were actually stable over time for *each* group. History of incarceration was a significantly greater disadvantage for whites relative to African Americans ($z = 4.66$; $p < 0.001$), employment status was a stronger predictor of imprisonment for whites compared to African Americans ($z = -2.90$; $p < 0.01$), and pre-trial incarceration coincided with higher imprisonment likelihoods for both groups while constituting an even greater disadvantage for African Americans ($z = -2.41$; $p < 0.05$). This last finding is consistent with Albonetti (1991).

The differential effect of employment status for African Americans and whites reflects an advantage for employed (versus

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unemployed) white defendants, but no such advantage for employed African Americans. This is different from the interaction effect more commonly described in the literature involving a *dis*advantage for *un*employed (relative to employed) minority defendants (e.g., Melossi 1989; Nobiling et al. 1998; Spohn & Holleran 2000). Regardless, the significant effect described here runs counter to the predictions of sentencing reform advocates that such interactions would subside under more structured sentencing. When considering that Ohio's reform did not actually *reduce* any of the pre-guideline disparities in imprisonment found here (related to criminal history, employment status, and pretrial incarceration), and that other race-related disparities actually emerged *post*-guidelines (including the differential effect of plea bargaining as well as the general race effect found in the pooled model), these findings indicate that more structured sentencing did not coincide with more equitable treatment across race groups in Ohio.

Turning to the race-specific models of prison sentence length (Table 4), both race groups experienced shorter sentences for Felony 1 and Felony 2 convictions after the reform (consistent with the pooled model), and the effects of Felony 1 and Felony 2 convictions were not significantly different between these groups during either period. By contrast, Felony 3 convictions resulted in significantly longer post-guideline sentences for whites but not for African Americans, generating a significantly greater post-guideline disadvantage for whites that did not exist prior to the reform ($z = 2.56$; $p < 0.01$).

While the pre-guideline effect of "other" specifications posed a significantly greater disadvantage for whites relative to African Americans ($z = 2.05$; $p < 0.05$), there was no difference between race groups in the magnitude of this effect *after* the reform. By contrast, the effects of gun specifications on sentence length remained stable over time for each race group, yet these effects differed significantly between the two groups during *both* periods ($z = 3.15$; $p < 0.001$). Convictions involving more gun specifications resulted in greater disadvantages for whites relative to African Americans.

Unlike the racial disparities in imprisonment decisions described earlier, the disparities in sentence length *consistently* demonstrated greater disadvantages for whites. Considering both sets of race-specific models as well as the main effects of a defendant's race from the pooled models, the majority of disparities uncovered here reflected greater disadvantages for whites. An important caveat to this observation, however, is that the vast majority of both legal and other extralegal effects on sentencing were *not* significantly different between African Americans and whites during either period. This provides, nonetheless, some very mixed

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evidence that does not fall neatly into just one of the three groups of predictions related to more structured sentencing. That is, full support is not offered for predictions of either *more* equitable sentences for African American defendants, no changes in these sentences, or *less* equitable sentences under more structured sentencing.

Summary

Table 5 provides a summary of the changes in legal and extralegal effects on conviction and sentencing under Ohio's reform. Information displayed for each pooled (main) effect indicates whether the effect became stronger or weaker under the guidelines, with blanks reflecting no change over time. For each race-specific effect, information indicates whether the treatment of African American and white defendants became significantly more or less equitable over time. Also indicated is whether there were inequities during both periods that did not change in magnitude over time. Blanks reflect no inequities during both periods examined.

This summary highlights the important links between formal changes to the structure of judicial decisionmaking and case outcomes, yet it also reveals that these effects were few in number (relative to the number of "stable" effects over time), and that there was no *consistent* evidence that racially disparate treatment will be either reduced *or* exaggerated with more formal constraints on judicial discretion.

Even the sheer number of stable effects on sentencing does not offer clear evidence of displaced discretion from judges to prosecutors, since the few significant changes in *general* (pooled) effects on conviction severity involved stronger legal effects and a weaker race effect. The few significant changes in race-specific effects on conviction severity also did not consistently demonstrate greater disadvantages for African American defendants under more structured sentencing, which is a prediction of scholars who argue that disparate treatment may persevere through the displacement of discretion. These findings reinforce Miethe's (1987) general observation that a shift to determinate sentencing may not translate into greater disadvantages for minorities prior to the sentencing stage. Important to recall, however, is that the stronger post-guideline effect of plea bargaining for African Americans coincided with more serious convictions for those who pled guilty with agreements (compared to whites who pled), and court scholars have argued that any displacement of discretion that does occur under more structured sentencing would ultimately impact the guilty plea process (Savelsberg 1992).

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Table 5. Summary of Changes in Legal and Extralegal Effects on Conviction and Sentencing Under Ohio's Guidelines

	Pooled Effects			Race-Specific Effects		
	Conviction	Prison	Length	Conviction	Prison	Length
Legal Predictors						
Plea bargain	–	stronger	–	<equity	–	–
F1 indict./conv.	–	stronger	weaker	–	–	
F2 indict./conv.	stronger	weaker	–	–	–	–
F3 indict./conv.	stronger	stronger	–	–	–	<equity
#counts	–	–	–	–	–	–
Personal crime	–	–	–	–	–	–
Homicide	–	–	–	–	–	–
#gun specs.	–	–	–	–	–	inequity/both
#other specs.	–	–	–	<equity	–	>equity
Addiction	–	–	–	–	–	–
Juvenile incar.	–	–	–	–	<equity	–
#prior terms	–	–	–	–	inequity/both	–
Extralegal Predictors						
African Amer.	weaker	stronger	–	N.A.	N.A.	N.A.
Mexican Amer.	–	–	–	N.A.	N.A.	N.A.
Male	–	–	–	–	–	–
#children	–	weaker	–	–	–	–
No h.s. degree	–	–	–	–	–	–
Employed	–	–	–	>equity	inequity/both	–

Notes: For pooled effects, "stronger" indicates a stronger effect under the guidelines. For race-specific effects, "<equity" indicates that the treatment of African American and white defendants became significantly less equitable over time.

Regarding specific changes to the imprisonment decision in general, the pooled findings revealed that convicted African Americans were actually more likely to go to prison *after* the reform, consistent with Mauer's (1999) speculation. Reducing judicial discretion also failed to eliminate pre-existing differences in imprisonment based on a defendant's sex, education, and employment status, although a weaker effect for family size (children) did emerge over time. The stronger post-guideline effect of Felony 1 convictions also provides some evidence of stronger legal effects after the reform. The stronger effect of plea bargaining under the guidelines raises the possibility that prosecutors have engaged in more plea bargaining in order to maneuver around the stricter sentences, but the race-specific models did not uncover any racial disparities in the effects of bargaining on prison sentences. No other legal effects changed significantly.

The majority of effects on imprisonment were not significantly different between race groups, with very few changes in these effects occurring over time. Other race group differences based on incarceration history and employment status persisted over time, but none reflected a greater disadvantage for African Americans. A significant change for African Americans in the effect of incarceration as a juvenile also resulted in a greater disadvantage for whites processed under the guidelines. While changing the structure of legal decisionmaking may affect case outcomes in some anticipated ways, these findings reveal that such changes may also generate some *un*anticipated effects on outcomes. The vast majority of stable effects over time also suggest that decisionmaking processes are somewhat resilient to formal change, at least when a certain level of discretion in decisionmaking is retained, as in Ohio.

The theme of predominantly stable effects and a few "mixed" changes in effects over time also emerged from the analysis of sentence length. Among the significant changes in pooled legal effects, Felony 3 convictions became stronger while both Felony 1 and Felony 2 convictions became weaker predictors over time. The general effect of a defendant's race persisted over time, reflecting slightly shorter sentences for African Americans relative to whites. The pooled *and* race-specific models also revealed no significant changes in other extralegal effects over time, and none of these effects were significant during either period.

The race-specific models of sentence length also revealed that Felony 3 convictions constituted a greater disadvantage for whites relative to African Americans *after* implementation of the guidelines. The reform also did not eliminate pre-existing disparities based on gun specifications, constituting a greater disadvantage for whites during both periods. In conjunction with the pooled effects, the findings for sentence length demonstrated either no race

group differences in treatment during both periods of study, or race group differences that consistently reflected greater disadvantages for whites. The latter, moreover, were split regarding their applicable time periods (pre- versus post-guidelines only, or both).

Implications

Formal changes to the structure of legal decisionmaking may only have a modest impact relative to that anticipated by legislators, and some of the effects that actually occur may *not* be anticipated (such as the higher odds of imprisonment for African Americans relative to whites during the post-guideline period only). In light of the vast majority of *stable* legal and extralegal effects on case outcomes described here, the link between formal changes to the decisionmaking process and actual changes in related decisions might not be particularly strong. Perhaps this scenario is more likely when formal change constitutes a less-radical shift in discretion, although the alternative (of a more radical shift) might pose an even greater threat of nefarious effects as court actors seek to maneuver around these restrictions in order to maintain their power and authority, potentially affecting decisions made throughout a court system.

All of this must be tempered, however, with the possibility that changes in case outcomes are less likely when the original structure of legal decisionmaking is generally perceived as "fair" by court participants (Griffin & Wooldredge 2001). Regarding the analysis of Ohio's sentencing process, the shift to determinate sentencing guidelines might have had a negligible effect on relationships involving extralegal characteristics simply because these relationships were weak to begin with. Legal measures were much better predictors, accounting for more than 85% of the *explained* variation in each outcome regardless of the time period examined. This figure is consistent with similar figures from related studies, which, when reported, generally indicate 80% or more of explained variation in sentencing accounted for by legal factors (see Ulmer 1997 for a review). Many scholars have also made the observation that specific measures such as offense severity and criminal history are much stronger predictors of case dispositions when compared to extralegal measures such as race and employment (e.g., Kramer & Steffensmeier 1993; Moore & Miethe 1986; Stolzenberg & D'Allessio 1994; Ulmer & Johnson 2004).

Sentencing reforms may have a greater impact on reducing extralegal disparities in jurisdictions with greater inequities. For example, if African American defendants had a 30% higher

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likelihood of going to prison compared to white defendants, a 10% reduction in this likelihood would be considered strong (and perhaps more realistic under sentencing reforms in these types of jurisdictions). However, in the counties examined here, the likelihood of imprisonment for convicted African Americans was 8% higher than for whites prior to the reform. This is also a zero-order difference that does *not* reflect compositional differences in legal characteristics between race groups that might contribute to the gap in imprisonment rates. (Recall that the pre-guideline relationship between race and imprisonment was nonsignificant when controlling for the legal measures in Table 3.) Significant reductions in extralegal disparities would be less likely under sentencing reforms when these disparities are modest to begin with, and when they do not consistently reflect greater disadvantages for lower-status defendants.

In light of this discussion, it appears counterintuitive that the general effect of a defendant's race on imprisonment actually became stronger over time. This is the type of disparity that researchers examine most often and that legislators show the greatest concern for, yet it appears to have been magnified under Ohio's sentencing guidelines. This observation, however, must be considered along with the earlier finding of a significant post-guideline decrease in conviction likelihoods for African American defendants in general.

Ohio's sentencing reform represents a scheme that has placed fewer constraints on judicial discretion when compared to the determinate sentencing schemes implemented in some other states. Perhaps this difference also accounts for the majority of stable effects as well as the few mixed changes over time. At best, more flexible schemes may only have a modest impact on very specific forms of disparity and, at worst, may simply transform these disparities over time. For example, the felony classification of possessing or selling small quantities of crack cocaine was changed under Ohio's reform so that imprisonment would be less likely for these offenders. Since African Americans are overrepresented in these types of cases, the overall effect should be a reduction in imprisonment for minorities. A separate analysis revealed that the imprisonment of African Americans convicted *only* on possessing or selling crack dropped by 5% under the guidelines. During the same period, however, there was an increase in the proportion of minority defendants involved in cases where crack was present (whether the defendant was possessing, selling, or under the influence of crack). Imprisonment for any felony conviction is more likely when drugs are also involved, which may have nullified (if not overcompensated for) the 5% drop in the imprisonment of African Americans attributable to the specific change in sentences

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for crack offenders. This situation could reflect either an actual increase over time in the use of crack by minorities *or* a greater tendency to trump up charges against minorities under the new guidelines. The second possibility exists by nature of retaining a fair amount of judicial discretion under the new sentencing scheme.

The Ohio legislature's interest in sentencing reform was driven, in part, by a concern with racial disparity in imprisonment. African Americans, constituting 11% of the general population in 1990, made up more than half of Ohio's prison inmates. A blue-ribbon panel studying the condition of Ohio's African American men recommended the creation of the Ohio Sentencing Commission in order to address this concern (Ohio Governor's Task Force 1990). However, efforts to achieve racial equity in treatment by the courts may not be very successful when these groups differ in *other* characteristics that also influence case processing. Wilson (1987) and Sampson and Wilson (1995) observed that larger portions of African Americans live in environments of extreme poverty, and urban neighborhoods of extreme disadvantage produce the highest violent crime rates. This raises the possibility that African Americans apprehended by the police are more likely to be more serious offenders, so a suspect's race becomes tied to other legally relevant characteristics that ultimately influence case processing.

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Appendix 1: Ohio's Sentencing Reform

Prior to July 1, 1996, Ohio Courts of Common Pleas operated under indeterminate sentencing. When deriving a determinate sentencing scheme, the Ohio Criminal Sentencing Commission (1996) decided in favor of a system based on presumptions, judicial discretion, and truth in sentencing (Amended Substitute Senate Bill No. 2 1996). Presumptive disposition decisions are based on the offense and prior record, and presumptive sentence lengths exist for particular levels of offense. The approach was designed to be a more flexible and just way to achieve many of the same goals that matrix-style grids (such as Minnesota's) can achieve. Under this scheme, felons are imprisoned under a determinate sentence called a stated prison term[14] chosen by the judge from a range. Table A1 describes, for each felony category, the range of basic prison terms, the presumptive sentences for first offenses, and the (actual) median prison term for convicted defendants in our sample who were sent to prison under the guidelines.

For any specific felony, judges select a prison term from the applicable range, and the time selected is the actual time served. The law guides the judge on the imprisonment decision (prison versus community) and on the length of any prison term via a series of factors and presumptions. Ohio's law has a rebuttal presumption in favor of imprisonment for first- and second-degree felons (a second-time first- or second-degree felon receives a mandatory prison term), guidance *against* prison for fourth- and fifth-degree felons, and no guidance for third-degree felons. Felony 5s were added to Ohio's felony classification scheme under Senate Bill 2. For prison-bound offenders, judges are presumed to use the

[14] *Stated prison term* is defined in Ohio Revised Code §2929.01, §2929.13, §2929.14 (1996).

[15] Rising prison populations and operating budgets were critical factors in the decision to add the "Felony 5" category, for which most offenders would receive a nonprison sanction while the remainder would face even lower maximum prison terms compared to Felony 4s. An example of an offense that was reclassified to a fifth-degree felony under the new scheme is possession of crack cocaine less than one gram. From the perspective of the Ohio Sentencing Commission, reclassifying these lower-level drug offenses would help reduce incarceration rates and speed up prison population turnover.

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Table A1. Prison Terms Under Ohio's Determinate Sentencing Guidelines

Felony Level	Range of Basic Prison Term	First Offense (Presumed)	Median Term (Actual)
1st degree	3 to 10 years	3 years	5 years
2nd degree	2 to 8 years	2 years	3 years
3rd degree	1 to 5 years	no guidance	2 years
4th degree	6 to 18 months	no prison	1 year
5th degree	6 to 12 months	no prison	8 months

minimum for a first trip to prison, except for the most serious cases.

Even with the presumptions, judges have wider discretion than some other states with structured sentencing. For example, under the prototypical Minnesota Sentencing Guidelines Grid, a first offender who uses a gun to commit robbery would have a presumed sentence of 48 months. The judge would have discretion within a range of 44 to 52 months. In sentencing that same armed robber under Ohio's template, the judge has a range of 36 to 120 months. There are factors to guide the judge toward a stated prison term within the range, but the judge still has broad discretion across the range. A Minnesota judge has a range of 9 months; an Ohio judge has a range of 85 months.

Appendix 2: Measures for the Analysis

A much larger pool of predictors was originally considered for inclusion in the analysis (see Wooldredge et al. 2002). Our goal was to obtain a pool of the most important legally relevant predictors of conviction and sentencing that could be included in every model examined. Hagan (1974) argued that a reliable analysis of extralegal influences, specifically referring to a defendant's race, requires adequate controls for legally relevant influences such as offense seriousness and prior record. Otherwise, the absence of adequate controls could generate spurious relationships involving extralegal variables.

The measures displayed in Table 1 were ultimately selected after considering (1) empirical relevance as demonstrated in the literature, (2) multicollinearity in any of the models (i.e., highly correlated predictors that generated biased estimates when included simultaneously in the same model), and (3) the strength of the zero-order relationships between the legally relevant predictors and the outcomes examined. When forced to choose between measures to include in an analysis due to multicollinearity, we considered both (1) and (3). The best example of this is our omission of a suspect's age. More than 60% of the variation in a suspect's age could be predicted by knowing several other defendant

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characteristics including number of children, education, employment status, type of attorney, bail status, drug and/or alcohol addiction at arrest, and history of incarceration (with younger defendants having fewer prior terms). Including age created problems with estimation, whereas dropping age resulted in more stable estimates for the other predictors. The included predictors that were correlated with a defendant's age permitted analysis of a larger number of relevant measures compared to the examination of age alone (criterion 1). These other predictors also offered more explanatory power compared to including age alone (criterion 3).

Most of the measures in Table 1 are dichotomous, with the variable label reflecting category 1 (versus 0) of the dummy code. Exceptions include any measure with a label beginning with "#," reflecting a raw count of a specific characteristic (e.g., # counts convicted on, # children living with defendant, etc.). In addition, the outcome measure of "conviction level" is an ordinal scale tapping the most serious charge convicted on, where 0 = none, 1 = misdemeanor, 2 = Felony 5 or Felony 4, 3 = Felony 3, 4 = Felony 2, and 5 = Felony 1. (Felony 4s and 5s are combined into a single category because the F5 category did not exist prior to the reform.) Variable labels for some of the other measures are also not completely self-explanatory and, therefore, need more explanation.

The measure "plea bargain" compares defendants who pled guilty with an agreement from the prosecutor to all other defendants combined. (Distinctions between types of guilty pleas are recorded in Ohio court sentencing reports. "Pled guilty with agreement from the prosecutor" reflects either one or more reduced charges from the original indictment, including the removal of any specifications, or a reduced sentence for the indicted charges.) Steffensmeier et al. (1998) examined the separate effects of bench versus jury trials, but this distinction did not offer additional insights into our own analyses. Albonetti (1990) and LaFree (1985) demonstrated significant links between a defendant's race/ethnicity, guilty pleas, and sentence severity. We also examined a dichotomous measure of whether a defendant pled guilty (with or without an agreement) and found the measure in Table 1 to be a stronger predictor of each outcome examined. Consistent with the earlier discussion of displaced discretion, sentences accompanying actual plea negotiations may have changed more dramatically for African Americans relative to whites under the new guidelines.

The dummy measures tapping the type of indictment/conviction (Felony 1 through Felony 5) reflect the *most* serious single charge that a defendant was indicted or convicted on. (The most serious indictment charge is described for the pool of indicted suspects only,

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whereas the most serious conviction charge is described for the pools of convicted and imprisoned defendants.) These predictors, in conjunction with some of the other legally relevant measures included, tap proportionately more of the variation in each outcome compared to other, more complex measures (such as a summed measure of the seriousness of all charges convicted on). For purposes of estimation, the dummy measures of F4 and F5 were left out of the models. Both measures were excluded because F5 indictments and convictions were only possible during the post-guideline period. Therefore, interpretation of the included measures is relative to the combined pool of F4s and F5s for *both* periods.

Following the strategy of Steffensmeier et al. (1998), we also explored a number of offense measures ranging from very general (e.g., property versus personal versus drug crime) to very specific (homicide, rape, selling crack cocaine, etc.). The dummy measures of "personal crime" (a general measure) and "homicide" (a specific measure) were the only variables adding explanatory power to the models beyond the other offense measures included. To a large extent this was due to the inclusion of "# gun specifications" and "# other specifications," which tap variation in several of the more specific offense measures dealing with weapons, drugs, violent crimes, and so on. Each measure of "specifications" refers to the number of specifications indicted or convicted on (depending on the outcome measure examined). "Other specifications" include specifications for (1) prior offense of violence/physical harm, (2) repeat violent offender, and (3) major drug offender, as well as other "habitual" specifications.

Defendants with a "drug/alcohol addiction" include persons deemed by court personnel to have had substance abuse problems at the time of case processing. "Juvenile incarceration" refers to any defendant who was incarcerated as a juvenile in an institution designed specifically for juveniles (i.e., local jails and/or temporary holding facilities did not apply). When determining a sentence, Ohio judges are allowed to consider a defendant's history of incarceration as a juvenile. It should also be emphasized that a series of both ratio and dummy measures of prior record were examined (# prior arrests, # prior convictions, ever served time in prison, etc.), and "# prior prison terms" and "juvenile incarceration" maintained the strongest zero-order relationships with the outcome measures.

The extralegal measure "employed" refers to individuals who were employed at least 20 hours per week and/or who were college students at the time of sentencing. The cutoff of 20 hours was based on our observation that the vast majority of defendants in the sample who were employed part-time worked *at least* 20 hours each week.

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The measures grouped under "other controls" (court-appointed attorney and pretrial incarceration) were included to provide a more reliable analysis. These variables are legal predictors of sentencing, but we did not classify them as such because they are not legally *relevant* predictors. In addition, we have no a priori reasons to suspect that these effects would have changed under the sentencing guidelines. "Court-appointed attorney" compares defendants with court-appointed counsel (whether public defenders or not) to those with privately retained attorneys (e.g., Albonetti 1990). "Pretrial incarceration" reflects convicted defendants who were not released prior to trial versus those who were released (e.g., Feeley 1979; Miethe & Moore 1985; Myers & Hagan 1979).

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