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Neighborhood Effects on Felony Sentencing

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The relatively high imprisonment rates of African American men from poor neighborhoods raise a question of whether felony sentences are influenced by ecological factors, separately from or in conjunction with a defendant's race. To provide insight on the topic, both legal and extralegal effects on imprisonment and sentence length were modeled for nearly 3,000 convicted felons from more than 1,000 census tracts in Ohio. Neighborhood effects were estimated with empirical bayes coefficients as outcomes, derived from hierarchical analyses, to adjust for the small ratio of defendants to tracts. Findings revealed that convicted felons from more disadvantaged neighborhoods were more likely to receive nonsuspended prison sentences, whereas a defendant's race was unrelated to imprisonment. By contrast, neighborhood disadvantage was unrelated to sentence length for imprisoned defendants, whereas African Americans received significantly shorter terms relative to Whites. The processes through which ecological context may operate to affect sentence severity are discussed.

Keywords: *neighborhood; extralegal effects; imprisonment; sentence length*

The overrepresentation of African American men in state prisons across the United States, particularly those from poor neighborhoods, has generated discussion about the impact of minority incarceration rates on economic well-being and social cohesion in predominantly Black communities (e.g., Rose and Clear 1998; Zatz 1998). One relatively simple explanation for this phenomenon focuses on the idea that African American men from poor neighborhoods are also more likely to engage in felony crimes, for various

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sociological and economic reasons (Wilbanks 1987). Another explanation, not necessarily contradictory to the first, focuses on disparate treatment of criminal defendants based on their race (Mauer 1999). Relative to convicted White defendants, convicted African Americans may face higher odds of imprisonment and longer sentences. Absent from the large body of research on racial disparities in sentencing, however, is consideration of whether sentences are more severe for minority defendants from more socially and economically disadvantaged areas within jurisdictions. Supporting evidence of disparate treatment based on a defendant's race *and* neighborhood has implications for the first perspective above in that legal authority may be undermined in such circumstances, at least in the eyes of African American men, possibly weakening the effectiveness of legal controls in communities with larger concentrations of African American residents.

When assessing an offender's culpability and risk for future offending, prosecutors and judges might also consider the individual's neighborhood. When confronted with defendants from poorer areas, particularly minority defendants, prosecutors and judges may pursue harsher sentences for these individuals in their efforts to reduce crime (a "focal concern" of court actors that may lead them to consider extralegal factors in their decisions, as described by Steffensmeier, Ulmer, and Kramer 1998). Prosecutors play an important role in sentencing via the guilty-plea process and the reduced charges and/or sentences they are willing to concede in plea agreements (Casper 1972; Heumann 1978; Jacob 1973; Jacoby 1980). To shed light on the relevance of neighborhood for understanding extralegal disparities in felony sentences, both legal and extralegal effects on the odds of imprisonment and the length of imprisonment were modeled for 2,954 defendants convicted on felony charges from 1,021 census tracts in Ohio, controlling for jurisdiction differences in defendant pools and disposition rates. It is argued that neighborhood differences in criminal case processing can be framed in similar fashion as previous studies of extralegal disparities solely conducted at the individual level of analysis.

Place of Residence as an Extralegal Consideration

The focus here is on the areas in which criminal defendants reside as opposed to the areas where their crimes are committed. Although there might be considerable overlap between these physical spaces, based on the "distance decay" function (Phillips 1980; for their discussion of "awareness spaces," see also Brantingham and Brantingham 1993), it must be

recognized that many offenders do *not* commit crimes close to their homes (Van Koppen and Keijser 1997). It has also been argued that the characteristics of neighborhoods in which crimes occur might play a role in police officers' use of discretion and whether they arrest a suspect (Klinger 1997). The focus here on a defendant's neighborhood of residence is no less relevant to the topic, however, based on the idea that African American men *residing* in poor neighborhoods are overrepresented in state prisons across the United States.

Recent bilevel studies of extralegal disparities in criminal case dispositions have included analyses of jurisdiction or county-level effects on sentence severity *beyond* the effects of individual-level characteristics such as a defendant's race or sex (e.g., Britt 2000; Johnson 2005; Kautt 2002; Ulmer and Johnson 2004). Scholars have grounded related research in theories of racial threat (Blalock 1967; Bobo and Hutchings 1996; Eitle, D'Alessio, and Stolzenberg 2002), but one of the underlying perspectives of these theories that is also applicable to a framework of *neighborhood* effects on sentence severity can be traced back to Blumer's (1955:13-4) commentary on race relations in the United States. Blumer discussed how "myths" associated with particular race groups can promote fear among members of the more socially, politically, and economically powerful group. Such fears can lead to "discrimination, segregation, and repression" of the less powerful group to insulate the dominant group from any threat to their social and economic interests. These processes then perpetuate the original myths and stereotypes surrounding particular race groups. Blumer noted that the quality of race group relations varies across different contexts, implying that levels of perceived threat, fear, repression, and so on might be higher or lower depending on the social environment.

Applying these observations to an understanding of race relations within municipalities, local elites' perceptions of minority group threat may vary across neighborhoods depending on the presence or absence of environmental "cues" associated with greater threat (e.g., higher concentrations of African Americans and higher poverty rates). Stronger perceptions of threat may be met with stronger efforts to control the residents of those areas, such as a harsher response by criminal justice officials to crimes committed by minorities (e.g., Liska and Chamlin 1984). The applicability of racial threat theory to a study of neighborhoods may lie in Blumer's (1955) ideas focusing on environmental differences in "ideological relations" and the "views and images which racial groups form of one another and the beliefs and myths supporting such views" (p. 10). As such, the relevance of neighborhood characteristics may fit well within a symbolic interactionist perspective.

Perspectives on Extralegal Disparities in Sentencing

Regarding studies of extralegal disparities in sentencing solely conducted at the individual level of analysis, research findings have consistently demonstrated that legal effects (type of offense and prior record) are stronger predictors of sentence severity relative to extralegal effects (as described by Ulmer 1997). There is some evidence, however, that judges consider extralegal factors, even if to a lesser extent. Perspectives on extralegal effects operating at the individual level have been grounded in symbolic interactionism, suggesting that judges may consider defendants' personal attributes when assessing risk for subsequent offending with limited available information (Albonetti 1991, 1997; Hawkins 1981; Steffensmeier et al. 1998). Defendants who fit judges' stereotypes of higher risk offenders may be sentenced more severely, such as individuals with characteristics reflecting greater social and economic disadvantage (Nobiling, Spohn, and DeLone 1998; Spohn and Holleran 2000; Steffensmeier et al. 1998). Hawkins (1981:230) referred to this type of decision-making as "perceptual shorthand," and Albonetti (1991) described how judges might consider extralegal attributes to reduce uncertainty in their decisions. Steffensmeier et al. (1998) have also discussed these considerations in the context of focal concerns and how judges weigh the impact of their decisions for crime control.

Because of concerns regarding the disproportionate overrepresentation of minorities in prison, related studies have focused heavily (but not solely) on the link between sentence severity and a defendant's race/ethnicity (summarized, in part, by Sampson and Lauritsen 1998; Spohn 2000; and Zatz 2000). Evidence of harsher outcomes for African Americans and/or Hispanics has been offered by Albonetti (1991, 1997), Holmes and Daudistel (1984), Johnson (2005), Kramer and Steffensmeier (1993), LaFree (1985), Nelson (1992), Spohn, (1990), Steffensmeier and Demuth (2000), Steffensmeier et al. (1998), Ulmer and Johnson (2004), and Zatz (1985), among others. There is also evidence that a defendant's race/ethnicity might *interact* with other characteristics such as a defendant's sex, employment status, and/or pretrial release status (e.g., Albonetti 1990, 1994; Dixon 1995; Kramer and Ulmer 1996; Moore and Miethe 1986; Nobiling et al. 1998; Spohn and Holleran 2000; Steffensmeier et al. 1998; Ulmer and Kramer 1996).

By contrast, some studies have produced no evidence that minority defendants receive more severe treatment relative to White defendants (e.g., Kleck 1985; Swigert and Farrell 1977; Thomson and Zingraff 1981; Unnever, Frazier, and Henretta 1980; Weisburd et al. 1991; Wheeler, Weisburd, and Bode 1982). Anomalous findings across studies could reflect differences in

the jurisdictions examined (Peterson and Hagan 1984), and the effects of a defendant's race might vary because of different court cultures (Ulmer 1997) and/or differences in population composition across jurisdictions (Ulmer and Johnson 2004). The focus here on 24 jurisdictions sheds additional light on this issue with an analysis of whether the effects of a defendant's race significantly varied across these courts.

Levels of Socioeconomic Disadvantage and Official Perceptions of an Offender's Risk

Perspectives of disparate treatment grounded in symbolic interactionism raise the possibility of extralegal effects on sentencing *beyond* those operating at the individual level. That is, uncertainty in legal decision-making might also be reduced with considerations of whether defendants reside in areas characterized by high levels of socioeconomic disadvantage that are, in turn, often associated with more crime-prone *environments*. Court actors might perceive certain neighborhood characteristics as cues for risk of repeat offending and may more vigorously pursue imprisonment for defendants residing in areas with more of these characteristics, particularly if the deterioration of community life in poor neighborhoods is being attributed to crime in those neighborhoods (consistent with Karp and Clear's 2000 discussion of "community prosecution"). For example, when reviewing sentence recommendations from presentence investigation reports for the defendants examined here, it was common to read commentaries on an offender's likelihood of failure while on probation because of the existence of drug markets and/or other criminal networks operating within proximity of an offender's residence.

Court scholars focusing on the dynamics of case processing have talked about court actors' perceptions of the more typical ecological contexts in which high-volume crimes occur, such as in poor or poverty-stricken neighborhoods (Eisenstein and Jacob 1977; Flemming, Nardulli, and Eisenstein 1992; Nardulli, Eisenstein, and Flemming 1988). Sudnow (1965) observed that "ecological patterns [of crime] are seen as related to socioeconomic variables and these in turn to typical modes of criminal and noncriminal activities" (p. 261). This observation underscores the idea that class-based stereotypes of higher-risk offenders might include the social and economic attributes of their neighborhoods. It also suggests that prosecutors and judges have knowledge about these attributes and may consider them in decision-making. Prosecutors play a critical role in sentencing via the

charge and/or sentence recommendations that accompany plea agreements (Casper 1972).

Consistent with the earlier observation that different characteristics of defendants may *interact* to disadvantage particular groups of individuals, ecological factors may also *condition* individual-level effects on sentencing if particular combinations of characteristics tend to flag certain defendants as "social dynamite" (Spitzer 1975). African American defendants who reside in poverty-stricken environments may be at an even greater disadvantage in the court system compared to either group separately considered. This combination of micro- and macro-level characteristics may be of particular concern to judges because they reflect the stereotypical higher-risk offenders (Hawkins 1981; Steffensmeier et al. 1998) who are perceived to be the greatest threat to social organization and cohesion in poor neighborhoods (Rose and Clear 1998). These ideas are consistent with Blumer's (1955) discussion of the "myths" surrounding subordinate race groups that are perpetuated by elites.

Racial disparities in sentencing could emerge in a more subtle fashion relative to the processes described above. In their study of whether a judge's race affects his or her sentencing decisions, Steffensmeier and Britt (2001) argued that African American judges might actually be *harsher* than White judges on minority defendants because of their "heightened sensitivity to social and personal costs of high levels of black crime (especially in black communities) and [these judges] see themselves not so much as representatives of black offenders but as advocates for black victims" (p. 754). Similarly, in their discussion of "community justice," Karp and Clear (2000) discussed how some prosecutors and judges are making conscious efforts to clean up particular neighborhoods by being harder on offenders from those communities. Unfortunately, given the higher concentrations of minorities in economically disadvantaged neighborhoods, this process may further tighten legal controls over individuals who are already disadvantaged in the system because of their individual-level attributes. This situation poses a potential conflict between the goal of improving quality of life in poverty-stricken neighborhoods and the goal of equity in sentencing. In an effort to act on behalf of the common good, are judges necessarily undermining due process? Given the overrepresentation of minorities in neighborhoods with the highest crime rates, are judges also feeding the negative images held by many African Americans toward the criminal justice system (Zatz 2000)?

Such a scenario is potentially damaging to African Americans and to the communities in which they reside. As argued by Rose and Clear (1998:442), higher levels of police enforcement in neighborhoods with higher crime

rates might contribute to *greater* social disorganization in those neighborhoods by disrupting social networks and interfering with a local population's capacity to "self-regulate." Although one might argue that arresting and incapacitating offenders should improve social cohesion among law-abiding residents, Rose and Clear observed that "offenders have complex relationships to the networks in which they are embedded. They may contribute both positively and negatively toward family and neighborhood life. Their removal in large numbers alters those networks both positively and negatively" (p. 442). The human and social capital necessary for social cohesion then drop to levels that make local control (vs. state control) ineffective for restoring any type of order.

Hypotheses and Method

The outcomes examined include whether a convicted felon received a nonsuspended prison sentence and the length of incarceration for convicted felons sent to prison. The study hypotheses focused on the main effects of a defendant's race on sentencing, the main effects of neighborhood socioeconomic disadvantage on sentencing, and the interaction effects between a defendant's race and neighborhood disadvantage (i.e., whether socioeconomic disadvantage conditioned the effect of a defendant's race on sentence severity):

1. African American defendants convicted on felony charges received more severe sentences relative to White defendants convicted on felonies, controlling for legal or case processing effects on case outcomes (main effect).
2. Convicted felons from neighborhoods with higher levels of socioeconomic disadvantage received more severe sentences, controlling for compositional differences in defendant pools across neighborhoods (main effect).
3. The effect of a defendant's race on sentence severity was stronger in neighborhoods with higher levels of socioeconomic disadvantage (interaction effect).

Samples and Data

Defendants included in the analysis described here were selected as part of a larger study of felony case processing in Ohio before and after the implementation of determinate sentencing guidelines in 1996. The larger sample included representative cross-sections of indicted suspects from 24 counties in the state, including the 6 most urban counties and a cross-section

of others based on population and geographic location. Systematic samples were drawn on site from prosecutors' indictment lists. These indictments occurred during a 2-year period surrounding the sentencing reform.[1]

From the larger sample of 5,648 indicted suspects, we excluded all defendants not convicted on felony charges (n = 1,613) and convicted Mexican American defendants and others who were not African American or White Anglo (n = 248). This decision was based on previous research demonstrating significant differences in the treatment of African American and Mexican American defendants (e.g., Albonetti 1997; Nobiling et al. 1998; Spohn and Holleran 2000; Steffensmeier and Demuth 2000). Also excluded were convicted felons who either did not reside in the jurisdiction where the case was processed or did not have an address that could be linked to a valid residence within the jurisdiction (including the homeless, persons residing in prison or jail at the time of the offense, and persons with invalid or missing addresses). The final pool for the analysis included 2,954 convicted felons from 1,021 census tracts across the 24 counties. All convicted felons in the sample were included in the analysis of imprisonment. The analysis of prison sentence length included 1,477 imprisoned felons within 722 tracts across the 24 jurisdictions.[2]

Census tracts were the neighborhood units for the analysis. Although this ecological unit is larger than the often preferred "face block" (Taylor 1997), it seems to better reflect how judges and prosecutors think about poor and high-risk neighborhoods (i.e., as block groups rather than hot spots). Census tracts were also created by the U.S. Census Bureau to be as internally homogeneous as possible in the sociodemographic characteristics of street block populations.[3] Although some of Ohio's metropolitan areas have formally defined neighborhoods, based on Park and Burgess's (1924) concept of a "natural area," other cities and towns in Ohio do not. Moreover, these natural areas exist at a considerably higher level of aggregation compared to census tracts, which masks important differences in levels of socioeconomic disadvantage within these areas.

Information on defendants' personal attributes and the legal aspects of their cases were obtained from prosecutors' case files, presentence investigation reports, and official records of the Ohio Department of Rehabilitation and Correction. Aggregate data for neighborhood (census tract) indicators of socioeconomic disadvantage were obtained from the 2000 U.S. Census of Population and Housing. Because of the hierarchical nature of the data (defendants within census tracts within jurisdictions), the following description of the statistical method should facilitate interpretation of the subsequent description of measures at each level of analysis.

Statistical Analysis

Given the hierarchical data, the method of statistical analysis had to adjust for (a) nonrandom error among defendants and census tracts within jurisdictions, (b) possible heteroscedasticity in census tract effects, and (c) differences in the appropriate degrees of freedom for hypothesis testing at the individual and aggregate levels of analysis. Although the choice of multilevel modeling seems obvious in this regard (Raudenbush and Bryk 2002), there is an additional problem of having too few defendants per census tract to provide stable maximum likelihood estimates of random, defendant-level effects across census tracts. The compromise involved creating a three-level data set (defendants at level 1, tracts at level 2, jurisdictions at level 3), estimating random effects for the level 1 model intercepts and race effects *only* (with all other level 1 effects fixed or averaged across census tracts), and modeling the empirical bayes (EB) estimates of the level 1 intercepts and race effects at level 2. Crudely defined, an EB estimate of a level 1 effect within each level 2 unit is derived by borrowing information on individuals from other similar aggregates to generate a more stable estimate per aggregate (see Raudenbush and Bryk 2002; Raudenbush et al. 2004).[4]

A three-level data set was created because grouping census tracts within jurisdictions permitted the adjustment of correlated error across tracts within jurisdictions. Adding the third level also was relevant for limiting explained variance in the outcomes to within jurisdictions (via centering the level 1 and level 2 measures on their means for each jurisdiction). This was important for reducing possible spurious effects owing to unmeasured influences on disposition rates that might also correlate with compositional differences in defendants and neighborhoods across jurisdictions (e.g., the heavier caseload demands of more urban courts). The models examined here are technically two-level models because they include only measures at the first two levels of analysis (defendants and census tracts). The limited degrees of freedom at the third level of analysis, with only 24 jurisdictions, prohibited any kind of meaningful examination of jurisdiction effects.

Nonlinear Bernoulli analyses were conducted for whether a convicted felon went to prison. A generalized least squares model was estimated for the analysis of prison sentence length. The examination of each outcome proceeded in three stages. First, an unconditional model with no predictors revealed the proportion of variance in each outcome at level 1 (among defendants) versus level 2 (between census tracts) versus level 3 (between jurisdictions). Significant variance in each level 1 outcome was found across tracts ($p < .001$).[5] Second, a model with *only* level 1 predictors was estimated

for each outcome, in which the model intercept and the relationship involving a defendant's race were allowed to vary randomly at level 2. All other level 1 effects were fixed because of the limited degrees of freedom existing within census tracts, as previously described. These models revealed the main effect of defendants' race on each outcome, and they also indicated whether the model intercepts and race effects significantly varied across tracts (necessary prerequisites for modeling the level 2 main and cross-level interaction effects of neighborhood socioeconomic disadvantage).

The final step in the analysis involved saving the EB estimates from the second step and modeling the level 2 main and interaction effects of neighborhood socioeconomic disadvantage. This procedure tested whether differences in the level 1 model intercepts (the adjusted means of each level 1 outcome measure across all tracts) and differences in the level 1 race effects might be explained by neighborhood disadvantage.[6]

Measures

All measures included in the models are described in Table 1 for the pools of convicted and imprisoned felons. The level 1 (defendant) effects of primary interest included the main effects of African American from hypothesis 1. The level 2 (neighborhood) effects of interest involved the effects of socioeconomic disadvantage on the level 1 model intercepts (the main effects from hypothesis 2) and the effects of disadvantage on the level 1 coefficients for African American (the interaction effects from hypothesis 3).[7]

Regarding the outcome measure of prison sentence length, the original scale of the number of months a defendant was sentenced to was logged because of its positively skewed distribution ($\bar{x} = 24$ months, $s = 31$ months). Logging the scale of sentence length to adjust for the skewed distribution is consistent with previous analyses of sentence length (discussed by Ulmer and Bradley 2006).

Based on observations by Steffensmeier et al. (1998) regarding a parabolic relationship between age and prison sentence length, the need to include a squared term for a defendant's age was explored for that model. The term was ultimately excluded because of nonsignificance ($p > .05$).

Several significant correlations emerged between the outcome measures and various indicators of a defendant's socioeconomic status, and so two factors were created (via principal components analysis) to capture this variation while limiting the number of predictors. The economic status factor taps a defendant's employment status at arrest, annual income, and receipt of public assistance. The education and residential stability factor

Table 1
Unweighted Means and Standard Deviations for All Predictors by Stage of Case Processing

	Convictions		Imprisonments	
	M	*SD*	*M*	*SD*
Level 1: Defendants[a]				
Outcomes				
Prison sentence	.49	.50	—	—
# months in prison (log)	—	—	1.18	.40
Extralegal predictors				
Age	30.66	9.52	30.76	9.32
Male	.81	.39	.87	.33
African American	.56	.50	.60	.49
# dependent children	.52	.85	.47	.78
Economic status (factor of . . .)	.00	1.00	.00	1.00
employed	.49	.41	.44	.39
gross annual income	12,353	8,345	11,663	8,077
receiving public assistance	.20	.40	.19	.40
Education and residential stability (factor of . . .)	.00	1.00	.00	1.00
no high school degree	.55	.41	.59	.39
bachelor's degree	.09	.28	.07	.25
# months at same address	49.0	94.1	43.2	79.5
Legal predictors				
Guidelines in effect	.48	.50	.44	.50
Felony 1 conviction	.04	.20	.07	.25
Felony 2 conviction	.07	.25	.10	.30
Felony 3 conviction	.20	.40	.25	.43
Felony 4 conviction[b]	.44	.50	.37	.48
Felony 5 conviction	.25	.43	.19	.39
# counts convicted on	1.45	1.06	1.55	1.15
# gun specifications	.01	.11	.01	.15
# prior prison terms	.69	1.26	.96	1.52
Case processing predictors				
Plea bargain	.87	.34	.86	.35
Court appointed attorney	.43	.50	.45	.50
Pretrial incarceration	.33	.47	.53	.50
n_1	2,954		1,477	
Level 2: Census tracts				
Socioeconomic disadvantage (factor of . . .)	.00	1.00	.00	1.00
proportion African American	.30	.34	.33	.35
proportion in nonfamily household	.22	.15	.22	.15

(continued)

Table 1 (continued)

	Convictions		Imprisonments	
	M	*SD*	*M*	*SD*
proportion female-headed household with children	.19	.14	.20	.15
proportion males without high school degrees	.23	.13	.25	.13
proportion civilian males unemployed	.09	.08	.09	.09
median household income	34,491	15,031	32,661	14,038
n_2	1,021		722	

a. Level 1 measures dummy coded (0 = no, 1 = yes), except # months in prison, # dependent children, economic status, education and residential stability, # counts convicted on, # gun specifications, and # prior prison terms.
b. Felony 4s and felony 5s combined for the reference category.

reflects variation in high school dropout, bachelor's degree, and number of months a defendant had lived at his or her address.

A factor of neighborhood socioeconomic disadvantage was created to capture several indicators of structural disadvantage (described in Table 1). All of these indicators, including the proportion of African Americans in a tract population, are intercorrelated above .70 (*r*). The factor was derived from a principal components analysis, and the components described in Table 1 produced a value of .76 for the Kaiser-Meyer-Olkin measure of sampling adequacy. In light of the focus on a defendant's neighborhood of residence, it is important to note that Ohio sentencing judges are provided with information on a defendant's place of residence via preliminary hearing summaries and presentence investigation reports.

A larger number of legal and case processing effects were also considered as statistical controls (for a list of these measures, see Wooldredge et al. 2002). The goal was to obtain a pool of the most important predictors of sentencing that might also be confounded with the measures of theoretical interest. Thorough examination led to the final list in Table 1. The measures displayed in Table 1 were ultimately selected after considering (a) empirical relevance as demonstrated in the literature, (b) the strength of the intercorrelations among all measures, and (c) multicollinearity in any of the models (i.e., highly correlated predictors that generated biased estimates when simultaneously included in the same model). When forced to choose between measures to include in an analysis because of multicollinearity,

both *a* and *b* were considered. This procedure is not the same as the one criticized by Berk, Li, and Hickman (2005:369) involving the selection of measures that correlate significantly with *only* outcome measures. Failure to include measures that are also correlated with race and uncorrelated with the outcomes could produce biased estimates of race effects that are *weaker* than the population parameters.

Some of the remaining measures in Table 1 also need elaboration. The measure of guidelines in effect compares defendants falling under determinate sentencing to those who did not. This control accounted for possible differences in sentencing behaviors resulting from the guidelines implemented in 1996. The measure of gun specifications is the number of possible prison sentence "enhancements" for possession or use of a gun. (This measure was excluded from the model of imprisonment because of its collinearity with the felony measures in the sample of convicted defendants only.) The court-appointed attorney variable compares defendants with court-appointed counsel to those with privately retained attorneys (e.g., Albonetti 1990). Pretrial incarceration reflects defendants who were not released prior to trial versus those who were released (e.g., Feeley 1979; Miethe and Moore 1985; Myers and Hagan 1979). Plea bargain compares defendants who pled guilty with prosecutorial agreements (reduced charges from the original indictment and/or reduced sentences) to all other defendants (e.g., Albonetti 1990; LaFree 1985).[8] Finally, a hazard of nonimprisonment derived from Heckman's (1976) "two-step" correction was included in the model of prison sentence length to adjust for possible sample biases associated with only examining imprisoned defendants.[9]

Results and Discussion

Defendant-Level Effects on Sentencing

The level 1 models predicting prison sentences and the length of prison sentences are displayed in Table 2. The model intercepts and race effects were the only estimates that varied randomly across census tracts, with all other effects fixed in each model. All coefficients reported in the table reflect pooled (average) effects across tracts.

The total variance in prison sentence is partitioned as follows: .60 at level 1 (between defendants within census tracts), .37 at level 2 (between tracts within jurisdictions), and .03 at level 3 (between jurisdictions). The level 1 and level 2 variance estimates were significant at $p < .0001$. Although the level 3 estimate was also significant ($p < .05$), its much smaller portion of

Table 2
Level 1 Models with Random Coefficients for Model Intercepts and Race Effects

	Prison		Months (log)	
Predictors	β	*SE*	β	*SE*
Intercept	.01		1.20	
Extralegal				
Age	.002	.002	.003	.002
Male	.29	.18	.03	.03
African American	.05	.08	−.06*	.03
# dependent children	−.04	.08	−.01	.01
Economic status	−.10***	.02	−.03*	.01
Education and residential stability	.06**	.02	.02	.01
Legal				
Guidelines in effect	−.12	.09	−.12***	.03
Felony 1 conviction	2.22***	.17	.80***	.05
Felony 2 conviction	1.57***	.15	.60***	.09
Felony 3 conviction	.79***	.06	.28***	.03
# counts convicted on	.20	.05	.03*	.01
# gun specifications	—	—	.14*	.07
# prior prison terms	.31***	.05	.01	.01
Case processing				
Plea bargain	−1.03***	.25	−.16***	.04
Court appointed attorney	.03	.07	.02	.03
Pretrial incarceration	2.31***	.51	−.10***	.02
n_1	2,954		1,477	
Proportion variation explained	.38		.55	

Note: Bernoulli model of prison sentences; generalized least squares model of months in prison. *$p \leq .05$. **$p \leq .01$. ***$p \leq .001$.

the whole reveals that imprisonment rates were considerably more uniform across jurisdictions than might be expected based on previous bilevel studies (e.g., Britt 2000; Ulmer and Johnson 2004). The model of imprisonment accounted for 38 percent of the level 1 variation.

The finding for a defendant's race in the imprisonment model refutes the first hypothesis, with no significant difference in imprisonment likelihoods between African American and White defendants convicted on felony charges. This nonsignificant finding reflects the *average* race effect across the entire sample. The significantly varying race effect at level 2, however,

indicates that racial disparities in the odds of imprisonment were larger in some tracts and smaller in others, thus introducing the possibility that neighborhood disadvantage might explain some of these tract-level differences. In other words, African American defendants from particular neighborhoods might still have faced greater disadvantages relative to White defendants from those neighborhoods based on levels of neighborhood disadvantage. Important to note is that the race effect did not significantly differ across the 24 jurisdictions ($p > .10$).

The general null effect of a defendant's race on imprisonment is consistent with Albonetti's (1999) finding in her study of suspended sentences in white-collar cases. She uncovered significant *in*direct effects of a defendant's race on sentencing, one of which operated through whether a defendant pled guilty (i.e., minority defendants were more likely to plead guilty, and, in turn, guilty pleas corresponded with higher odds of suspended sentences). When controlling for guilty-plea status and other mediating effects, she found that the direct effect of race on the odds of a suspended sentence was nonsignificant.

Also important to note are the other significant extralegal effects on imprisonment, where the odds of imprisonment were significantly higher for convicted felons with lower economic status and for those ranking *higher* on levels of education and residential stability. The main effect for education and stability appears counterintuitive, although the main effect for economic status was consistent with expectations. These findings suggest that socioeconomic indicators aside from race were more relevant than a defendant's race for predicting imprisonment among convicted felons in Ohio during the study period. However, the legal and case processing measures were stronger predictors of prison sentences compared to the extralegal measures, with offense levels and prior record maintaining by far the strongest effects. A stepwise analysis revealed that these measures accounted for more than 80 percent of the variation explained by the level 1 model, which is consistent with related studies (as noted by Ulmer 1997).

The analysis of months in prison (log) revealed a higher portion of total variance at level 1 (.74) compared to the analysis of imprisonment, with .23 at level 2 and .03 at level 3. Although the level 3 estimate was identical to the corresponding estimate for imprisonment, it is more surprising in light of the relatively broad sentence ranges with which judges are permitted to work in Ohio (Wooldredge, Griffin, and Rauschenberg 2005). Ohio jurisdictions could be considered more similar than expected regarding average prison terms, despite differences in geographic location and population demographics.

The sentence length model was also more efficient compared to the imprisonment model, accounting for 55 percent of the level 1 variation in prison terms. The effect of a defendant's race on sentence length was significant, unlike the race effect on imprisonment, yet *opposite* in direction to the hypothesized relationship. African Americans were sent to prison for, on average, 2 months of incarceration *less* than imprisoned whites (computed from the level 1 regression equation). Although statistically significant, the race effect might be considered modest.

The level 1 race effect on sentence length significantly varied across census tracts during the study period ($p < .05$) but did not significantly vary across the 24 courts ($p > .10$), similar to the race effect on prison sentences. Altogether, the nonvarying race effects across jurisdictions counter the idea that analyses of different jurisdictions are likely to produce different findings for racial disparities in sentencing, assuming identical models and estimation procedures. Important to underscore, however, is the focus here on multiple jurisdictions in the same state. It might be the different *states* examined across extant studies that have contributed to mixed findings. Important exceptions to this idea include Britt's (2000) and Ulmer and Johnson's (2004) analyses of race effects across Pennsylvania jurisdictions, which revealed significantly different effects across counties.

Economic status was also a significant predictor of months in prison. Unlike the race effect, however, the effect of economic status was in the predicted direction (i.e., higher status corresponded with significantly shorter prison terms). Economic status was the only extralegal measure that was significant in both models, suggesting that similar measures may be salient in future research on sentencing.

Neighborhood Disadvantage and Sentence Severity

The level 2 main and cross-level interaction effects of socioeconomic disadvantage are described in Table 3. The effects of neighborhood disadvantage were estimated by regressing the EB estimates obtained from the level 1 models described above on the level 2 measure of neighborhood socioeconomic disadvantage. As noted in the last section, significant differences in the level 1 intercepts and race effects across census tracts introduced the possibility that differences in neighborhood disadvantage might have corresponded with some of these tract-level differences in the level 1 coefficients.

Table 3
Level 2 Effects of Neighborhood Disadvantage on Random Level 1 Empirical Bayes Estimates

	Prison		Months (log)	
	γ	*SE*	γ	*SE*
Main effect				
Random level 1 intercepts as outcomes				
Socioeconomic disadvantage	.08***	.02	.02	.02
Intraclass correlation (ρ)	.38		.23	
Proportion between-tract variation explained	.30		.01	
Cross-level interaction effect				
Level 1 coefficients for African American as outcome				
Socioeconomic disadvantage	–.004	.02	–.003	.02
n_2	1,021		722	

Note: Level 1 intercepts and race coefficients estimated from a Bernoulli model of prison sentences and a generalized least squares model of months in prison. Intraclass correlation coefficient ρ = between-tract variance in outcome ÷ total variance in outcome.
*$p \leq .05$. **$p \leq .01$. ***$p \leq .001$.

The main effect of neighborhood disadvantage was significant and in the predicted direction in the model of prison sentence, thus favoring hypothesis 2. Even when controlling for the significant level 1 effects of a defendant's socioeconomic status, convicted felons from lower status areas were more likely to be sent to prison during the study period. This main effect was also fairly strong, accounting for 30 percent of the between-tract variation in imprisonment rates. By contrast, the neighborhood effect was nonsignificant in the model of prison sentence length, which refutes this particular application of hypothesis 2. In conjunction with the level 1 findings, these results indicate that sentence disparities based on a defendant's economic status and neighborhood disadvantage were more pronounced in imprisonment decisions. These specific forms of disparity were weaker in decisions regarding prison sentence length, based on the null effect of neighborhood disadvantage and the less powerful effect of economic status. Also, relative to the null race effect on imprisonment, the significant race effect on sentence length was *opposite* to the predicted relationship.

The different findings for the effects of neighborhood disadvantage on imprisonment versus sentence length could reflect the greater relevance of community factors for the incarceration decision as opposed to the sentence length decision. Consistent with the pursuits of some prosecutors and judges to clean up particular neighborhoods (Karp and Clear 2000), the immediate concern of court actors may be with getting offenders from those neighborhoods off the streets as opposed to for how long. It might also be easier for a judge to defend a decision to incarcerate in these circumstances as opposed to defending a specific length of imprisonment. The former decision requires only that a defendant is eligible for a prison sentence, and any defendant meets this criterion once he or she is convicted of a felony offense. Longer prison terms, however, require greater specificity in terms of offense type, the number of felony counts convicted on, and possible sentence enhancements (e.g., based on the possession or use of guns).

Despite the different effects of neighborhood disadvantage on imprisonment versus sentence length, the level 2 main effects were consistent with findings from a previous study of neighborhood effects on court dispositions in cases of misdemeanor assaults on intimates from Hamilton County, Ohio (Wooldredge and Thistlethwaite 2004). Findings from that study also revealed higher incarceration rates (in jail) for defendants from more disadvantaged areas in Cincinnati and no effect of neighborhood socioeconomic status on sentence length.

The cross-level interaction effect was also examined for each outcome because of the significant between-tract differences in race effects found at level 1. The interaction effect was not significant in either model, thus refuting the applicability of hypothesis 3 to decisions regarding imprisonment and sentence length. In short, the effects of a defendant's race on sentencing did not vary based on levels of neighborhood disadvantage.

The stronger main effects of the economic status indicators on imprisonment relative to the null race effect could reflect some of the geographic differences in minority concentrations across Ohio. The highest concentrations of African Americans are in the most urban counties of the state. In turn, class status might be defined more by economics rather than by race in these more urban areas. For example, in census tracts across Cleveland, Columbus, and Cincinnati, these tracts are geographically smaller and more internally homogeneous on economic indicators relative to census tracts in less urban or more rural jurisdictions. During the course of data collection, attorneys in urban courts talked about defendants from very specific areas or residential complexes, whereas attorneys in rural areas generally dichotomized their references, referring to defendants from a particular side of town (e.g., "east side"

or "other side"). It is possible that the main effects of economic status at the individual and neighborhood levels are most pronounced in more urban jurisdictions, which, in turn, include more racially heterogeneous populations. In these more racially mixed areas, there might be a greater tendency to define someone's class in strictly economic terms.

Implications

Across the Ohio jurisdictions examined, neighborhood disadvantage was a stronger predictor of prison sentences when compared to a defendant's race. Evidence was found for one of the two hypothesized main effects of neighborhood disadvantage (on prison sentences), versus neither one of the two hypothesized main effects of a defendant's race. A defendant's economic status was the only extralegal predictor that was statistically significant in both models, yet neighborhood disadvantage remained a significant and relatively strong predictor in the model of imprisonment, even when compositional differences in a defendant's economic status were controlled.

The relevance of neighborhood characteristics for understanding disparities in imprisonment may fit within a symbolic interactionist perspective of courtroom decision-making in that court actors may consider characteristics of a defendant's neighborhood when assessing an individual's risk for reoffending. If court participants' stereotypes of higher-risk offenders include elements of social and economic disadvantage, neighborhood characteristics would be theoretically relevant because a person's class status is shaped by both individual characteristics and characteristics of the community in which he or she resides. Studies of the ecology of crime consistently demonstrate the disproportionate distribution of crime across urban neighborhoods (Krivo and Peterson 1996, 2000; Ousey 1999; Sampson and Wilson 1995), possibly reinforcing a stereotype of residents from poorer neighborhoods as being more prone to crime. The disadvantages that accrue to felony defendants from poor neighborhoods in Ohio underscore Rose and Clear's (1998) argument regarding the possible disruption of social networks in poor communities because of the incarceration of large numbers of adult residents in those communities. This scenario potentially reduces the human and social capital necessary for social cohesion, which, in turn, could undermine the means of self-regulation within a community.

From a (slightly) different perspective, the higher likelihood of incarceration for individuals from more disadvantaged neighborhoods could be

a byproduct of courts trying to clean up these neighborhoods. From a community justice perspective (Karp and Clear 2000), court actors may be attempting to formally restore order to these communities because informal social controls (families, schools, etc.) are ineffective for controlling crime. Prosecutors retain a fair amount of power through the guilty-plea process in terms of the concessions and sentence recommendations they are willing to make in plea agreements (Jacoby 1980). For example, Jacoby (1977) discussed how prosecutors might consider a defendant's amenability to rehabilitation in plea bargaining. A prosecutor's perception of a defendant's amenability to rehabilitation, in turn, may be influenced by stereotypes of higher-risk offenders, such as defendants from poorer neighborhoods. This situation raises the possibility of inequities in case processing based on a macro-level, extralegal characteristic. In this scenario, the two goals of protecting communities and due process come into direct conflict with each other based on community characteristics generating inequities in case processing at the individual level. Yet this conflict may be unavoidable given the social processes that create and perpetuate the highest levels of socioeconomic disadvantage. By virtue of solely focusing on individual-level attributes, current sentencing policies are not designed to reduce community-level variation in the treatment of criminal suspects. Analogous to a fallacy of composition, there is an assumption underlying current sentencing schemes that equal treatment based on individual-level attributes should prevent inequities at the group level (e.g., neighborhood). It may be invalid to assume that excluding race from individual decisions necessarily prevents biases against African Americans as a group, especially given the high correlations between the racial makeup of neighborhood populations and other indicators of socioeconomic disadvantage. The multilevel analyses described here demonstrate that a defendant's race does not have to coincide with disposition severity while defendants from predominantly African American neighborhoods still face higher odds of incarceration.

A caveat to this discussion is that neighborhood disadvantage did not maintain significant conditioning effects on the empirical relationships between a defendant's race and the outcome measures. These findings refute the idea that the *greatest* disadvantages in case outcomes accrue to African Americans from poor neighborhoods (extrapolated from Blumer's 1955 discussion of environmental differences in the degree to which a minority group is perceived to threaten the interests of elites).

An important methodological issue to consider in future studies is whether the inclusion of neighborhood effects in sentencing models might improve prediction of jurisdiction differences in sentencing. In other words,

what proportion of between-jurisdiction differences in incarceration rates is attributable to organizational and political differences across jurisdictions versus relative differences in levels of crime, segregation, and poverty across neighborhoods within these jurisdictions? Yet although incarceration rates significantly varied across the 24 courts examined here, the effects of a defendant's race on sentencing did not. This last finding could reflect an analysis of multiple courts operating under the same sentencing scheme (i.e., within the same state). Previous observations of mixed findings for race effects across studies (Peterson and Hagan 1984; Ulmer 1997) could be because of analyses of different states across those studies rather than different courts per se (cf. Britt 2000; Ulmer and Johnson 2004). A direct test of this idea would require an examination of multiple state trial courts both within and between different states.

Notes

1. A 5 percent sample of indictments was drawn from each of the six largest counties during the two periods, a 15 percent sample from each of the next six largest counties, and a 35 percent sample from each of the last 12 counties. Twice as many rural counties were selected to ensure enough cases for a reliable analysis. Based on the sampling design, all cases were weighted for the multivariate analyses by the inverse of their selection probabilities, and case weights were normalized.

2. Period-specific models (before vs. after Ohio's sentencing reform) with individual-level predictors only were originally estimated for each sample of defendants, and z tests of significant differences in corresponding effects within each pair of models were conducted (Clogg, Petkova, and Haritou 1995). For each pair of models, despite significant differences in some of the other individual-level effects, the coefficients for a defendant's race were not significantly different between the two periods ($p > .05$). Therefore, cases were pooled across the two periods to increase the degrees of freedom for the analysis.

3. As of 2000, tract populations ranged from 1,500 to 8,000 residents, with an average of 4,000 residents per tract.

4. Using HLM6 (Raudenbush, Bryk, and Congdon 2004), these estimates were generated from the level 1 models and saved in a separate data file for the second stage of the analysis involving the estimation of neighborhood effects. Modeling neighborhood effects on the level 1 random empirical bayes (EB) estimates was necessary because, even with only two random effects per level 1 model, the relatively small ratio of defendants to census tracts might not have generated stable maximum likelihood (ML) estimates of these effects. The dispersion in EB estimates across aggregates should be smaller than the dispersion in ML estimates, based on the logic that the EB estimate for any one aggregate should be more similar to another if information is borrowed across similar aggregates. Examination of EB estimates should, therefore, make the level 2 null hypothesis tests more rigorous.

5. The significant between-tract differences might not be surprising given the small ratio of defendants to census tracts. This scenario can produce a relatively large portion of total variation in an outcome existing between tracts, in contrast to data sets with larger numbers of individuals nested within aggregates.

6. The proportions of variance in an outcome measure explained at each level of analysis are computed from estimates of error variance provided in the HLM6 output. Total variance in each outcome is computed as the sum of σ^2 (level 1), $\tau_{\pi 0}$ (level 2), and $\tau_{\beta 00}$ (level 3) from the unconditional models (with no predictors). Significant predictors added at each level will reduce these estimates, and differences in the estimates across models are used to compute the proportions of explained variance at each level. The focus here is on explained variance at level 1 (defendants) and level 2 (census tracts) because no predictors were added at level 3 (jurisdictions).

7. The level 1 model intercepts are interpreted as "adjusted" means on the individual-level outcomes (e.g., the proportion of convicted defendants sent to prison), controlling for compositional differences in defendant pools across census tracts based on the level 1 predictors included.

8. Steffensmeier, Ulmer, and Kramer (1998) examined the separate effects of bench versus jury trials, but this distinction did not offer additional insight into the analysis described here.

9. The applicability of the Heckman adjustment is limited to ordinary and generalized least squares models, so it was not considered for the analysis of imprisonment. A program for creating the Heckman correction is available in STATA 9.1. The procedure involved estimation of a probit model of selection (imprisonment), and these equations were then used to calculate a hazard of nonimprisonment, or the inverse Mills ratio (Berk 1983:391). An important challenge was selecting a set of measures for the selection equation that would prevent multicollinearity between the selection adjustment and the group of predictors of sentence length (Leung and Yu 1996). Most of these measures do not appear in Table 1 and were chosen from the larger data set based on their utility as instrumental variables. Descriptions of these measures and the tests performed for assessing the magnitude of selection bias in these data (Stolzenberg and Relles 1997) are available on request.

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