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Racial Disparity in Formal Social Control

An Investigation of Alternative Explanations of Arrest Rate Inequality

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Prior research on racial disparities in arrest rates has been limited by an almost exclusive focus on two explanatory models, an inattention to the mediating processes identified in leading theories, and a relative neglect of nonindex crimes, for which police discretion is greater. This analysis contributes to research on race differences in social control by more comprehensively evaluating mediating factors in the racial threat and benign neglect models and by testing explanatory frameworks that link racial disparities to opportunities for bias that result from residential segregation and variations in police discretionary authority across crime types. Analyses of data from 136 cities reveal two key findings. First, an uneven distribution of Blacks and Whites is associated with higher arrest disparities for drug and weapons arrests, but not with violent or property crime arrest disparities. Second, there is little evidence in support of the venerable racial threat or benign neglect explanatory frameworks.

Keywords: *racial inequality; formal social control; racial threat; spatial opportunity*

Social science scholars have long debated whether interracial contact generates fear, negative attitudes, and discriminatory behavior among Whites or whether between-race exposure actually improves understanding and tolerance between racial groups (cf. Allport 1954; Blalock 1967; Dixon 2006; Ellison and Powers 1994; Fossett and Kiecolt 1989; Oliver and Wong 2003; Quillian and Pager 2001; Sigelman and Welch 1993; Taub, Taylor, and Dunham 1984; Taylor 1998). In criminology, this debate is reflected in a body of research investigating whether the prevalence of racial minorities

affects the operation of formal social control mechanisms. The most prominent theoretical statement on this issue, the racial threat thesis, suggests that an encroachment of Blacks (or non-Whites more generally) leads Whites to perceive threats to their economic and political standing and personal safety. In response, Whites are believed to invest in and deploy institutions of formal social control as a means of quelling this minority group threat. Generally consistent with this argument, previous studies have reported that cities or states with relatively large Black or non-White populations have more police personnel per capita, spend more money on policing and corrections, are more likely to impose the death penalty, and have greater rates of imprisonment (Jackson 1989; Jackson and Carroll 1981; Jacobs 1979; Jacobs and Carmichael 2001, 2004; Jacobs and Helms 1999; Kent and Jacobs 2005; Liska, Lawrence, and Benson 1981; Stults and Baumer 2006).

Despite the contributions of the noted studies, research on the relationship between minority group prevalence and formal social control processes is limited in salient ways. First and foremost, although arrests reflect an important social control process that serves as a formal gateway into the criminal justice system, relatively few studies have examined aggregate-level factors that affect race-specific arrest rates (Harer and Steffensmeier 1992; Liska and Chamlin 1984; Parker, Stults, and Rice 2005), and almost none have attempted to account for racial disparities in this particular dimension of formal social control (for a lone exception, see Eitle, D'Alessio, and Stolzenberg 2002). This limitation is both surprising and critical because one aspect of the racial threat perspective of Blalock (1967) deals specifically with the expected impact of minority group prevalence on racial discrimination, as measured by inequality in social processes. Second, the existence of certain patterns of racial segregation within cities may create important opportunity structures that affect racial disparities in arrests, but previous studies have neither theorized about nor empirically assessed these segregation-related opportunity effects. Third, variation in the level of police discretionary authority may create great opportunity for racial bias in arrests for some offense types (e.g., drug offenses) but little opportunity for bias in others (e.g., crimes with complaining victims or witnesses). Unfortunately, these important variations have not been assessed, because prior research has been primarily confined to a focus on serious violent and property offenses (i.e., index crimes). Finally, although evidence from studies of arrest has offered only weak support to the leading theoretical accounts of racial bias in formal social controls (e.g., racial threat and benign neglect), conceptual and measurement

shortcomings may have constrained the ability of those studies to provide comprehensive tests of these prominent explanatory frameworks.

In the current study, we addressed the above limitations to extend the macro-level research literature on racial disparities in formal social control. In particular, we responded to limited theoretical development in the literature by drawing out and then empirically testing two new opportunity-linked theoretical frameworks that may help explain racial inequality in arrest rates. Moreover, we incorporated measures that allowed us to more comprehensively investigate the explanatory processes outlined in current leading theories of racial bias in formal social control. Last, we expanded the empirical scope of previous scholarship by considering whether racial disparities in arrests, and the effects of key explanatory variables on those disparities, differ systematically across a wider range of offense categories than have been considered in prior scholarship.

In the next section, we briefly outline the theoretical models from which we derived the hypotheses to be tested in this study. We then discuss the data, measures, and analytical methods by which those tests were carried out. Next, we report the results of our analyses and describe the tentative conclusions that may be drawn on the basis of those results. We end with a brief articulation of various ways that future work may advance on contributions of the current investigation.

Theoretical Background and Prior Research

Racial Threat

Any theoretical discussion about the impact of minority group prevalence on formal social controls naturally starts with the racial threat thesis. Rooted in the conflict perspective, early versions of this viewpoint formally appeared in classic sociological works on intergroup relations, such as that of Blalock (1967). Drawing from Blalock's work, the most basic racial threat argument suggests that as a dominant social group, Whites view Blacks, and other non-White minority groups, as potential competitors who may challenge their ascendant position in society. Consequently, as Blacks (non-Whites) become more prevalent and less residentially segregated in a given area, it is hypothesized that Whites will perceive a greater threat and therefore move to protect the existing status quo via a variety of discriminatory methods, including unjustly focusing criminal justice resources at their non-White competitors. Simply put, the logic of racial threat theory proposes

that in the face of an increasing encroachment from Blacks, Whites will be more motivated to discriminate and therefore will use formal social control resources, such as arrests, as a means of controlling Blacks.

Lending general support to the racial threat thesis, a number of macro-level studies have reported that the relative size of the Black population is associated positively with several indicators of formal social control. For example, several studies have found that incarceration rates are higher in states that have higher percentages of Black residents (Jacobs and Carmichael 2001; Greenberg and West 2001; Myers 1990), whereas other studies suggest that the relative size of the minority population is a key determinant of police force size (e.g., Jackson and Carroll 1981; Kent and Jacobs 2005), criminal justice expenditures (Jacobs and Helms 1999), and the use of the death penalty (Jacobs and Carmichael 2004). Most important for the current research, prior work hints that an increased prevalence of non-Whites also may affect arrest rates (e.g., Liska and Chamlin 1984). However, relative to research on other formal social control mechanisms, the body of research on arrests does not yield compelling support for racial threat theory. Although some analyses have suggested that percentage non-White is positively related to overall arrest rates (Liska and Chamlin 1984), others have reported that percentage non-White is negatively associated with race-disaggregated arrest rates (Liska and Chamlin 1984; Parker et al. 2005).

Despite the limited support for racial threat as an explanation of arrest outcomes, there are at least three reasons to question whether extant arrest studies have effectively assessed the racial threat framework. First, past studies have focused primarily on the relationship between minority group prevalence and race-specific arrest rate levels, rather than inequality in arrest rates. This is a concern because Blalock's (1967) racial threat theory explicitly posits a relationship between the relative prevalence of a minority group and discrimination, a concept that reflects the differential treatment of one group relative to another. Moreover, theoretically salient explanatory variables actually may have qualitatively distinct relationships with race-specific arrest rate levels and with racial inequality in arrest rates. Therefore, the choice between these outcomes is important and should depend, in large part, on which more closely reflects the social process being explained. According to Blalock, in the absence of survey-based data that directly address discriminatory attitudes, measures of racial inequality are perhaps the best macro-level proxies for the key theoretical concept of discrimination.¹

Second, although the racial threat perspective clearly anticipates that Whites will be more threatened and motivated to mobilize formal social

control mechanisms against Blacks when they perceive the latter group to be more prevalent, prior studies have typically operationalized this prevalence-threat process simply with an indicator of the overall percentage of the city population that is Black (or non-White; but see King and Wheelock 2007). Yet it can be argued that the perception of minority prevalence and the experience of threat is most likely to occur when Blacks or non-Whites encroach on the proximal or intimate social space of Whites, such as their local residential communities. If so, it would seem preferable to not simply measure the relative size of the minority population but the degree to which Whites are exposed to Blacks in their residential communities. Perhaps in response to this concern, it has become standard practice for scholars to also include a measure of residential racial segregation, the index of dissimilarity, in their analyses of the racial threat thesis. Although we believe that the index of dissimilarity may indeed have very important effects on racial disparities in arrest rates (see the section on spatial opportunity below), its use as an indicator of racial threat is somewhat questionable because it more directly measures between-neighborhood variations in racial composition rather than the within-neighborhood experience of minority group prevalence and contact that we think may be a key contributor to threat processes.

A third concern is that past investigations of the racial threat model have often failed to measure key intervening mechanisms identified in that theoretical framework (for related discussion, see Eitle et al. 2002). Indeed, in his treatise on minority group relations, Blalock (1967) explicitly identified two important intervening mechanisms that presumably link racial composition to discriminatory outcomes. First, he proposed that the motivation to discriminate against Blacks (or other racial minorities) will be greater whenever Whites perceive that encroachment by the former group presents potential competition for desired labor market positions (economic threat). Thus, it is not the size of the Black population per se that is problematic but rather that as Blacks become increasingly visible, Whites may perceive that their unfettered access to valued jobs and economic resources is becoming less secure (e.g., see King and Wheelock 2007). Second, Blalock suggested that a greater prevalence of Blacks (or non-Whites) is threatening because it creates a potential challenge to Whites' long-standing control of the political machine (political threat). Because political power directly influences the allocation of public resources, any perceived threat to Whites' hegemony in this sphere of society may cause concerns about the consequences for the quality and quantity of public institutions and services in predominantly White communities.

In addition to the intervening processes articulated above, more recent scholarship has suggested that racial disparities in the application of formal social controls also may be spawned by a third intervening factor: Whites' concerns about criminal victimization at the hands of Black offenders (Liska and Chamlin 1984; Liska et al. 1981; see also Quillian and Pager 2001). Interestingly, although Blalock (1967) did not offer this minority crime threat process as an intervening mechanism on par with the economic and political threat hypotheses, he did mention that the relationship between minority group prevalence and discrimination may be enhanced in places such as the South, where Whites hold exaggerated fears regarding the criminal inclination of Blacks (p. 167). Consistent with Blalock's point of view, recent evidence supports the notion that Whites have deep concerns about their vulnerability to crime committed by Blacks (Bobo and Kluegel 1997; Russell 1998; Sniderman and Piazza 1993).

Most studies of racial threat and arrest rates have not measured one or more of the intervening processes described above (cf. Chamlin and Liska 1992; Eitle et al. 2002; Liska and Chamlin 1984; Parker et al. 2005), and those that have done so have provided only limited empirical support for the expectation that economic and political threats mediate the impact of interracial contact on arrests (Eitle et al. 2002; Parker et al. 2005). On the other hand, seemingly consistent with the interracial crime threat process, some research has indicated that crime and disorder are perceived to be worse and residents are more fearful about crime in places with a greater relative presence of Blacks (Liska, Lawrence, and Sanchirico 1982; Quillian and Pager 2001; Sampson and Raudenbush 2004). Likewise, other work reports that higher interracial crime rates are associated with elevated rates of arrest for non-Whites (Liska and Chamlin 1984) or with larger Black/White violent crime arrest ratios (Eitle et al. 2002).

Despite this modicum of support for the interracial crime threat position, the preponderance of extant research does not compel the conclusion that racial threat theory is particularly efficacious in explaining racial disparities in arrest rates. Yet because of the above-noted measurement limitations, it seems clear that additional inquiry is needed before definitive claims can be made. Toward that end, we tested several hypotheses extracted from the racial threat framework. First, we posited as follows:

Hypothesis 1: Cities in which the exposure between Whites and Blacks is higher will, on average, have greater Black-White arrest rate disparities (for all offense categories).²

In addition, because a comprehensive representation of the racial threat perspective identifies economic, political, and interracial crime threats as important mediating factors, we also investigated the following predictions:

Hypothesis 2: Cities that exhibit greater evidence of White-Black economic competition will have greater racial disparities in arrest rates.

Hypothesis 3: Cities that show greater evidence of political power among Blacks will exhibit larger racial differences in arrest rates.

Hypothesis 4: Black-White arrest rate disparities will be greater in cities where larger shares of person crimes involve Black offenders and White victims.

Benign Neglect

The racial threat thesis has been the most prominent theoretical argument in past studies of the impact of interracial contact on formal social control outcomes. However, in the literature on arrest outcomes, an alternative process referred to as the benign neglect hypothesis (Liska and Chamlin 1984) has also emerged. Briefly stated, the benign neglect model suggests that as Blacks become more prominent in the population of a given place, the racial composition of crimes committed by Black offenders changes. In particular, intraracial crimes become more prevalent and interracial crimes less prevalent. And for several reasons articulated below, this change in the racial composition of crime incidents yields a relative decline in Black arrest rates.

First, intraracial offenses perpetrated by Blacks tend to be viewed as personal matters that should be handled informally. According to the theory of social control developed by Donald Black (1976, 1998), legal intervention becomes less likely as the relationship between the disputants becomes more intimate. If, on average, Black offenders and victims are more likely to know one another or if the police are more likely to consider such situations personal affairs, it stands to reason that Black arrest rates will generally be lower. Second, Blacks are believed to be more distrusting of the legal system and therefore are reluctant to file official reports when they are victims of crimes. As Kubrin and Weitzer (2003) stated, “residents of these (disadvantaged and Black) neighborhoods have little or no confidence that the police are capable of protecting them if they provide information” (p. 175). Thus, if Blacks are likely to report a smaller proportion of serious crimes, arrest rates should also be lower. Third, Black crime victims may lack the social influence necessary to compel appropriate law enforcement responses to crimes committed against them (Liska and Chamlin 1984).

This is also consistent with Black's (1976) theory, because the law, as a means of redressing grievances, is generally less accessible to those with lower social status.

In sum, like the racial threat theory, the benign neglect argument suggests that an expansion of the minority population results in race-based disparities in arrests. However, rather than increasing arrest rate disparities by raising motivations for law enforcement to focus attention on Black offenders (as suggested by racial threat), the benign neglect perspective suggests that minority population encroachment decreases motivations to report and attend to Black-perpetrated crime incidents. Thus, a proportionally larger Black population yields a relative reduction in Black arrest rates and therefore decreases the racial disparity in arrests.³ Moreover, because crime among Blacks is viewed as an ecologically contained problem in segregated environments, the benign neglect model also leads to the expectation that increased levels of racial segregation (dissimilarity) will correspond with smaller racial disparities in arrests.

Unlike the muted support for racial threat hypotheses, previous studies of arrest rates have yielded considerable evidence consistent with the logic of the benign neglect model. Namely, these studies have reported that geographic units with higher percentages of Black (non-White) residents have lower Black arrest rates (Chamlin and Liska 1992; Liska and Chamlin 1984; Parker et al. 2005). In addition, there also is evidence of a negative relationship between racial segregation and Black arrest rates, although this latter effect appears to differ across time periods (cf. Chamlin and Liska 1992; Liska and Chamlin 1984; Parker et al. 2005).

The above evidence notwithstanding, most studies have fallen short of fully testing the benign neglect model. Indeed, as is evident in the above discussion, the benign neglect thesis contends that minority group prevalence indirectly affects Black (non-White) arrest rates (or the Black-White arrest disparity) through its impact on the intraracial/interracial crime ratio. To our knowledge, only Liska and Chamlin (1984) provided a direct test of that thesis, but their evaluation was limited insofar as it focused only on robbery arrest rates for a subset of 26 of the original 109 cities examined in their study. Moreover, because the logic of benign neglect theory suggests that the racial composition of crime victims is a central factor contributing to racial biases in arrests, we contend that it implies that the indirect effects outlined above will pertain primarily to crimes with identifiable victims (e.g., index offenses) rather than to victimless crimes (e.g., drug offenses). Unfortunately, prior studies have not evaluated the merit of this latter idea.

Thus, as an extension of the extant social science literature, we tested the following two “benign neglect” hypotheses:

Hypothesis 5: The relative prevalence of Blacks will indirectly affect Black-White disparities in arrest rates. Specifically, as the relative prevalence of Blacks increases, the ratio of intraracial to interracial crimes (committed by Black offenders) will also increase, and that latter increase will result in smaller Black-White disparities in arrest rates.

Hypothesis 6: The intraracial/interracial crime ratio will have discernible effects on Black-White arrest rate disparities for offense types with clear victims (e.g., violent and property offenses) but little appreciable impact on arrest disparities for offense types in which victims are less easily identifiable (e.g., drug and weapons law violations).

New Explanations: Spatial Opportunity and Police Discretion

With more than two decades behind them, the racial threat and benign neglect perspectives are fast becoming the “classic” theories of racial biases in formal social control outcomes, especially arrest rates. Although they differ in their predictions, they are similar to the extent that they suggest that interracial contact is likely to affect motivations for the discriminatory use of formal social controls. Although motivation is clearly an important explanatory mechanism, a new line of thinking suggests that racial disparities in arrest rates may vary as a result of structural opportunities for biased law enforcement. In the paragraphs below, we sketch two variations of this latter line of theorizing.

The first of the new structural opportunity viewpoints is what we call the “spatial opportunity” model. Essentially, this argument suggests that regardless of the nature or source of discriminatory motivations, the spatial distribution of Blacks and Whites can have an important impact on race-based disparities in arrest rates. In particular, cities with higher levels of unevenness are structural contexts that produce opportunities for conscious or unconscious racial biases to become manifest in racially disparate arrest rates. As Stuart (2004) notes, “unevenness makes it easier for . . . organizations to deliberately or unintentionally treat people of different races differently” (p. 8). Indeed, because unevenness makes it relatively easy to designate entire sections of a city as a “Black” or a “White” area, implicit biases rooted in cultural stereotypes that link Blacks with social disorder, crime, and violence (Bobo 2001; Bobo and Kluegel 1997; Quillian and Pager 2001; Sampson and Raudenbush 2004) can easily influence the geographic

deployment of law enforcement. For instance, because police administrators often operate with imperfect information on exactly who will commit a crime, they likely make decisions about the geographic distribution of police resources on the basis of a combination of considerations, including known racial, economic and geographic distributions of crime as well as powerful cultural stereotypes that “operate beneath the radar screen” (Sampson and Raudenbush 2004:320). Consequently, decisions that begin as legitimate attempts to efficiently and effectively deploy finite crime control resources may ultimately result in concentrating police attention on spatially distinct Black communities that are perceived to be crime “hot spots.” To the extent that this geographic selectivity occurs, even if not intended, it virtually ensures that Blacks (or other non-Whites) will be observed, questioned, and arrested at rates that substantially overstate objective racial differences in offending (e.g., see Beckett, Nyrop, and Pfingst 2006).

Complementing the above argument, a “police discretion” model suggests that the opportunity for racial disparity in arrests is greater for some categories of crime than it is for others. Similar to sentencing research that indicates that racial bias is more likely for less serious criminal cases in which judges wield more discretion (Spohn and Cederblom 1991), this model posits that racial bias in arrest activity, whatever its root source, is most likely to be evident for criminal offenses for which the police (as individuals and organizations) have more discretion with regard to arrest decisions. In our view, the potential for racial biases in arrests is particularly great for criminal offenses such as drugs and weapons violations. Indeed, because these offenses are often uncovered via proactive law enforcement strategies such as undercover buy-and-bust drug operations or field interrogations (Conlon 2004), they typically involve a relatively high level of police discretion when it comes to arrest decision making. In contrast, offenses that become known through the actions of a reporting crime victim or witness (e.g., violent and property crimes) may be less subject to racial bias because the presence of a complaining third party often places the police under external pressure to make an arrest. For example, the police likely have less discretionary authority if they have a bruised and bloodied violent-crime victim pointing the finger at his or her assailant. In contrast, when shaking down a suspected drug dealer or drug user, an officer has great discretion to make an arrest or turn the perpetrator loose, even when he or she has been found to possess drugs (see Conlon 2004; Simon and Burns 1998).

The spatial opportunity and police discretion frameworks have not been explicitly incorporated in prior work, but some evidence reported in prior studies is relevant for these frameworks. For instance, contrary to the logic of the spatial opportunity framework, some past studies have reported evidence of a significant negative (Liska and Chamlin 1984) or null (Chamlin and Liska 1992; Parker et al. 2005) relationship between racial segregation and non-White arrest rates. Notably however, those studies examined the impact of the index of dissimilarity only on arrest rates for serious violent and property offenses, which are likely to be reported to the police by victims or witnesses. It remains unclear if arrests for offenses that are more likely to be discovered via proactive law enforcement activities will also vary as a function of spatial unevenness in the distribution of Blacks and Whites. Other studies have reported that racial disparities in arrests for drug offenses in Seattle, Washington, are not simply explainable in race-neutral terms (Beckett et al. 2005, 2006). Rather, the arrest disparities are due to the fact that police attention is more heavily concentrated on crack cocaine offenders and other participants in predominantly non-White downtown drug markets than it is on predominantly White outdoor drug markets in which drugs such as methamphetamine, ecstasy, powder cocaine, and heroin are frequently peddled (Beckett et al. 2006). Building on those suggestive findings, we directly tested the merit of the spatial opportunity and police discretion arguments by examining the following two predictions:

Hypothesis 7: The degree of unevenness in the distribution of Blacks and Whites will be positively associated with the Black-White disparity in arrest rates.

Hypothesis 8: Black-White residential unevenness will have a positive association with the racial disparity in arrest rates for crime categories involving greater police arrest discretion (e.g., drugs and weapons offenses) but little association with crime categories for which police arrest discretion is more limited (e.g., index crimes).

Data and Methods

Units of Analysis

To test the hypotheses proposed above, we drew on data from 136 U.S. cities that have minimum total populations of 100,000 persons, at least 5,000 Blacks, and valid data for the dependent, independent, and control variables outlined next. The total and Black population counts used to select cities on the basis of the above criteria were obtained from the 2000 Census of Population and Housing.

Dependent Variable

As the preceding discussion indicates, in the current study, we focused on conceptual models that posit factors related to race-linked inequality in formal social controls, namely, arrests. Moreover, some of these models predict that racial disparities in arrests as well as the effects of salient predictor variables will differ across criminal offenses categories (e.g., between crimes with victims and “victimless crimes”). Thus, to test these hypotheses, our dependent variables tapped the relative Black-White disparity in arrest rates for several different crime types, including serious violent crimes (murder or nonnegligent manslaughter, rape, robbery, and aggravated assault), serious property crimes (burglary, motor vehicle theft, and larceny-theft), drug offenses (possession and sale or manufacture of cocaine and opiates), and weapons law violations. For each type of crime, we calculated for each city the natural logarithm of the ratio of Black-to-White arrest rates.⁴ Higher scores reflect a greater Black-White disparity in arrests for violent, property, drug or weapons offenses. Arrest data used to compute the ratio were drawn from the Federal Bureau of Investigation’s Uniform Crime Reporting Program for 2000, 2001, and 2002.⁵

Independent Variables

As discussed earlier, prior research has used percentage Black for an overall city (or state, metropolitan area, etc.) as the standard proxy for the prevalence of Blacks and the concomitant experience of racial threat among the White population. However, as we argued above, this measure is quite broad and may not effectively tap the extent to which Blacks encroach into the proximal social spaces (e.g., residential communities) that we believe are most likely to heighten the experience of threat among Whites. In contrast, we believe that an alternative measure, the White-Black exposure index ($xP \times y$) is more consistent with our aforementioned conceptualization of racial threat. The White-Black exposure index is computed as

$$xP \times y = \sum_{i=1}^n \left[\frac{x_i}{X} \right] \left[\frac{y_i}{t_i} \right],$$

where x_i , y_i , and t_i are the numbers of Whites, Blacks, and total residents in neighborhood (i.e., census tract) i , and X is the total number of Whites in the entire city. As computed herein, the White-Black exposure index reflects the percentage Black in the neighborhood (census tract) of the average White person; alternatively, it reflects the probability that a randomly

drawn White person living in a particular tract will encounter a Black person in that tract (Bell 1954). Recent work on the measurement of racial segregation suggests that exposure indices are particularly useful when researchers are interested in measuring the extent to which members of racial groups experience segregation from (or, in this case, interaction with) other racial groups (Stuart 2004).⁶ Although the overall city percentage Black and the White-Black exposure index are strongly correlated ($r = .81$ in our data), it is sometimes the case that the percentage Black in the neighborhood of the average White resident is substantially lower than the percentage Black in the city as a whole. For instance, although Blacks constituted nearly 61 percent of the overall population in Atlanta in 2000, the average White resident of Atlanta lived in a neighborhood (census tract) in which just under 21 percent of the residents were Black. Thus, although percentage Black and the White-Black exposure index may commonly yield similar data for a given city, there are prominent instances in which they paint starkly different pictures regarding the degree of interracial contact.

As in past studies, we also included in our analyses the index of dissimilarity, which measures the extent to which Blacks and Whites are unevenly distributed across the residential communities in a city (see Massey and Denton 1988 for the computational formula). However, unlike prior research in which this variable was viewed primarily as another measure of minority group prevalence and therefore as an indicator of motivation for the discriminatory use of formal social controls against Blacks and non-Whites, we conceive of this measure as an indicator of the degree to which there is spatial opportunity for the racially selective targeting of law enforcement resources.^{7,8}

Eitle et al. (2002) noted that extant racial threat research has often been limited by a failure to measure the economic, political, and interracial crime threat factors that underlie or help explain why racial composition may be linked to race-based differentials in the application of social control. We heeded Eitle et al.'s call for racial threat studies to assess these arguments by including measures of each intervening factor. First, we measured economic threat by the White/Black unemployment ratio, which has also been used to measure racial economic competition in prior sociological studies (Eitle et al. 2002; Olzak 1990).⁹ Second, we tapped political threat by including an indicator variable coded 1 if a city had a Black mayor and 0 if the mayor was not Black.¹⁰ Finally, we measured the extent to which racial disparity in social control may be motivated by Whites' concern about being crime victims at the hands of Black offenders by including a measure of the percentage of White homicide victims who are killed by Black

offenders. Outside of direct measures for fear of interracial crime or the perception of interracial crime risk, neither of which was readily available to us, we believe that this measure is a reasonable objective indicator of the threat of interracial crime. Indeed, it seems reasonable that homicide incidents in which Black offenders kill Whites are unusual enough that they will garner public attention and thereby elevate Whites' concern about their risks of criminal victimization at the hands of minority group members.¹¹ Finally, to assess the benign neglect hypothesis that the effects of interracial contact and residential segregation affect arrest rates indirectly via their impact on the racial composition of individuals victimized by Black criminal offenders, we computed the ratio of intraracial-to-interracial homicides committed by Blacks.¹²

Black-White exposure, the index of dissimilarity, and the White/Black unemployment ratio were each computed using data from summary file 3 of the 2000 U.S. census. The Black mayor indicator variable was derived from the 2000 version of the summary report on Black elected officials compiled and made available by the Joint Center for Political and Economic Studies (Bositis 2002). The measure of Black-on-White homicide and the intraracial/interracial homicide ratio was computed from data drawn from the 2000 to 2002 supplementary homicide report file compiled by James Fox (2005) and made available by the Inter-University Consortium for Political and Social Research.

Control Variables

Because arrest rates for Blacks and Whites may be driven in part by overall rates of criminal offending, we included a control for the index crime rate, computed from "crimes known to the police" for 2000 through 2002.¹³ These data are compiled as part of the Federal Bureau of Investigation's Uniform Crime Reporting Program and are reported in the 2001 and 2002 annual volumes of *Crime in the United States* (Federal Bureau of Investigation 2000, 2001, 2002a). In addition, because the Black/White arrest ratio may simply reflect race-based differentials in criminal offending, we included the Black/White homicide offender ratio, computed as the Black homicide offender rate divided by the White homicide offender rate. Data for this measure were drawn from the supplementary homicide report file noted above (Fox 2005). We also controlled for the local law enforcement crime control capacity, which we operationalized as the number of police officers per capita. Data on law enforcement personnel were taken from 2000 Police Employee (LEOKA) Data, collected as

Table 1
Descriptive Statistics for Variables Analyzed

Variable	<i>M</i>	<i>SD</i>	Minimum	Maximum
Black-White violent arrest ratio	4.18	2.42	0.69	20.32
Black-White violent arrest ratio (ln)	1.30	0.52	-0.38	3.01
Black-White property arrest ratio	2.88	1.61	0.67	11.97
Black-White property arrest ratio (ln)	0.95	0.46	-0.40	2.48
Black-White drugs arrest ratio	6.08	5.68	0.24	38.17
Black-White drugs arrest ratio (ln)	1.48	0.85	-1.43	3.64
Black-White weapons arrest ratio	3.85	2.43	0.63	11.89
Black-White weapons arrest ratio (ln)	1.15	0.65	-0.46	2.48
White-Black exposure	0.17	0.10	0.03	0.53
Unevenness	0.47	0.17	0.16	0.82
White/Black unemployment ratio	0.49	0.16	0.27	1.13
Black mayor = 1	0.18	0.39	0	1
Percentage Black-on-White homicide	6.63	5.74	0.00	42.86
Black intraracial/interracial homicide ratio	4.42	4.04	0.13	22.75
Black/White homicide offending ratio	4.01	3.04	0.11	21.73
Disadvantage index	80.86	17.60	40.46	125.88
Modified crime index	6358.37	2215.20	2416.28	14,618.43
Police per capita	2.17	0.89	0	5.11
City population	387,134	773,211	100,565	8,008,278
South = 1	0.36	0.48	0	1

part of the Uniform Crime Reporting Program (Federal Bureau of Investigation 2002b). In addition, we also included controls for the natural logarithm of city population, the level of socioeconomic disadvantage in a city, and location in the census-defined South region of the United States. Socioeconomic disadvantage was computed by summing together the percentage poor, the percentage unemployed and/or not actively looking for work, and the percentage of family households with female heads (no husband present). Southern location was measured by an indicator variable coded 1 for cities located in the South, as designated in the 2000 census. Descriptive statistics for the variables used in the analyses are presented in Table 1.¹⁴

Results

The descriptive statistics in Table 1 show several interesting features. First, these data indicate that, as anticipated, there were noteworthy differences across offense types in the Black-White arrest ratios. Specifically,

drug offenses, which are more likely to be recorded as a result of proactive policing, showed the greatest Black-White disparity, with the Black arrest rate being more than six times the White arrest rate across the cities that we observed. Violent and weapons offenses also showed fairly large disparities, with Black arrest rates being in the vicinity of four times the White arrest rates for both crime types. The smallest (though still substantial) disparity was observed for the property crime index, with Blacks being arrested at a rate nearly three times that of Whites.

Second, the data in Table 1 indicate that across our sample of large cities, the average White resident lived in a census tract in which 17 percent of the residents were Black. This estimate is noticeably lower than mean overall city percentage Black, which was 22.5 percent for the cities observed in our analysis (not shown in Table 1), consistent with our argument that racial composition and actual exposure are not one and the same. In addition, these data indicate that Blacks and Whites were fairly unevenly distributed across our sample of places, with the average index of dissimilarity being 0.47. This means that, on average, 47 percent of Blacks would have to relocate from their current neighborhoods to achieve an even distribution of Blacks and Whites across city census tracts. With regard to the other key predictor variables, we found that nearly one in five cities had African American mayors, the unemployment rate for Whites was less than half that for Blacks, nearly 7 percent of homicides involved Black offenders and White victims, and among Black homicide offenders, intraracial killing occurred four times as often as interracial killing.

Moving now to our test of the hypotheses posited above, we present in Table 2 a series of four “baseline” least squares regression models in which the logarithm of the Black-White arrest ratio for each of the four offense types (i.e., violent, property, drug, and weapons offenses) was regressed on the measures of interracial exposure, spatial unevenness, and the set of control variables. In the first model, the results of the regression equation predicting the logarithm of the ratio of Black to White violent crime arrest rates are presented. These estimates indicate that both White-Black exposure and the index of dissimilarity (unevenness) had statistically discernible effects on the Black-White disparity in violent crime arrests. More precisely, a 1 percent increase in the measure of Whites’ exposure to Blacks corresponded with a 0.136 percent decrease in the Black-White ratio of violent crime arrests.¹⁵ This finding does not support the logic of racial threat as specified in Hypothesis 1. Meanwhile, in agreement with the spatial opportunity framework (Hypothesis 7), cities with greater levels of dissimilarity in the distribution of Blacks and Whites showed greater racial disparity in

Table 2
Ordinary Least Squares Regression Models Predicting the
Black-White Gap in Arrest Rates for Four Offense Categories

Variable	Offense Category				
	Violent	Property	Drugs	Weapons	VIF
Minority prevalence/contact variables					
Unevenness (ln)	.526 ^a (.42)	.385 ^a (.35)	1.43 ^a (.69)	.830 ^a (.52)	2.97
White-Black exposure (ln)	−.136 ^b (−.16)	.058 (.08)	.067 (.05)	−.004 (−.003)	2.15
Control variables					
Black/White homicide offending ratio	.030 ^a (.38)	.022 ^a (.32)	.032 ^a (.24)	.025 ^a (.25)	1.16
Disadvantage index	−.015 ^a (−.49)	−.016 ^a (−.61)	−.026 ^a (−.53)	−.017 ^a (−.45)	3.10
Modified crime index	.007 ^{ad} (.28)	.006 ^{ad} (.30)	.008 ^a (.22)	.007 ^a (.24)	1.64
Police per capita	.092 (.16)	.013 (.03)	−.188 ^a (−.20)	.251 ^a (.34)	2.58
Log city population	−.147 ^a (−.21)	−.044 (−.07)	−.142 (−.13)	−.195 ^a (−.23)	1.73
South	.046 (.09) ^c	−.184 ^a (−.20) ^c	−.174 (−.21) ^c	−.073 (−.11)	1.69
Constant	3.68	2.73	6.40	4.49	
Adjusted R ²	.499	.313	.374	.531	
F ratio	13.58 ^a	8.15 ^a	15.83 ^a	17.42 ^a	
n	136	136	136	136	

Note: Standardized slopes are in parentheses. Robust standard errors were used in computing *t* ratios.

a. *t* ratio > 1.96.

b. *t* ratio > 1.65.

c. "Y-standardized coefficient."

d. Coefficient multiplied by 100 to reduce decimal places.

violent crime arrests than cities with lesser levels of Black-White dissimilarity. In particular, a 1 percent increase in dissimilarity was associated with a 0.526 percent increase in the Black-White violent crime arrest rate ratio. Of these two key explanatory variables, the measure of unevenness appeared to have a more robust impact, with a standardized coefficient of .42 (compared with -.16 for the White-Black exposure measure).

Looking next at the racial disparity in property crime arrests, the results in the second model show an important similarity and an important difference from the prior regression equation. As was observed in the violent crime disparity equation, the results here indicate that cities with greater unevenness in their Black-White distribution also had larger Black-White disparities in arrest rates for serious property crimes. A 1 percent increase in unevenness corresponded with a 0.385 percent increase in the Black-White property crime arrest ratio. However, unlike the results from the violent crime arrest model, the White-Black exposure variable had a statistically

negligible relationship with the measure of racial disparities in property crime arrest rates. Thus, these results are consistent with the spatial opportunity framework but do not support the racial threat model.

In the next two models, we examined the measure of Black-White disparity in drug crime arrests and the measure of Black-White disparity in weapons law arrests. Following the same general pattern as was observed previously, these latter analyses indicated that the index of dissimilarity (unevenness) had a discernible positive association with the racial disparity in drug and weapon violation arrests. Indeed, cities differing by 1 percent on this independent variable exhibited, on average, a 1.43 percent difference in the ratio of Black to White drug arrests. Meanwhile, in the weapons equation, a 1 percent increase in the index of dissimilarity corresponded with a 0.83 percent increase in the Black-White arrest rate ratio. Viewed collectively, the results displayed in Table 2 lend the strongest empirical support to the spatial opportunity model (Hypothesis 7), with the police discretion model (Hypothesis 8) also receiving some support.¹⁶ In contrast, the baseline racial threat effect specified in Hypothesis 1 did not receive much support. However, because the important “intervening” variables specified in both racial threat and benign neglect theories were not included in the analyses of Table 2, key propositions of those frameworks remain to be assessed.

In Table 3, we elaborate the above analyses by accounting for the influence of the potentially important intervening processes outlined earlier. In particular, we extended the models described above by adding four additional explanatory variables: the White/Black unemployment ratio, a dummy variable for cities that had Black mayors, the percentage of homicides with Black offenders and White victims, and the ratio of Black-on-Black to Black-on-White homicides. The former three variables reflect processes identified in racial threat and (in the case of Black mayors) Black political power arguments, while the latter represents the mediating process specified in the benign neglect model.

Compared with the earlier results, the addition of these four explanatory variables improved the overall predictive efficacy of the model, with the adjusted coefficient of determination rising from .499 to .655. Most of the additional explained variance resulted from two variables, the White/Black unemployment ratio and the Black intraracial/interracial homicide ratio. Yet contrary to expectations derived from the economic threat dimension of racial threat theory (Hypothesis 2), the White/Black unemployment ratio had a significant negative relationship with the measure of Black/White disparity in violent crime arrests. In other words, in cities in which White

Table 3
Ordinary Least Squares Regression Models Predicting the
Black-White Gap in Arrest Rates for Four Offense Categories

	Offense Category				
Variable	Violent	Property	Drugs	Weapons	VIF
Minority prevalence/contact variables					
Unevenness (ln)	.145 (.11)	-.054 (-.05)	.796* (.38)	.383* (.24)	4.01
White-Black exposure (ln)	-.193* (-.23)	-.031 (-.04)	-.024 (-.02)	-.074 (-.07)	2.42
Intervening variables					
White/Black unemployment ratio	-1.29* (-.39)	-1.36* (-.47)	-2.39* (-.44)	-1.29* (-.31)	2.00
Black mayor	-.024 (-.05) ^b	.046 (.10) ^b	-.025 (-.03) ^b	.020 (.03) ^b	1.22
Percentage Black-on-White homicide	.003 (.04)	.007 (.08)	.003 (.02)	-.001 (-.01)	1.90
Black intraracial/interracial homicide ratio	.015 ^c (.28)	.028 ^a (.25)	-.001 (-.003)	.030 ^a (.19)	2.67
Control variables					
Black/White homicide offending ratio	.065* (.38)	.043* (.29)	.058* (.21)	.067* (.31)	1.73
Disadvantage index	-.010* (-.34)	-.011* (-.44)	-.016* (-.33)	-.013* (-.34)	3.68
Modified crime index	.002 ^d (.09)	.002 ^d (.11)	.002 ^d (.05)	.003 ^d (.09)	1.81
Police per capita	.128* (.22)	.042 (.08)	-.110 (-.12)	.275* (.37)	2.66
Log city population	-.144* (-.21)	-.045 (-.08)	-.150* (-.13)	-.182* (-.21)	1.72
South	.062 (.12) ^b	-.192* (-.20) ^b	-.104 (-.06) ^b	-.079 (-.12) ^b	1.81
Constant	3.50	2.49	6.30	4.11	
Adjusted R ²	.655	.520	.493	.704	
F ratio	29.59 ^a	19.21 ^a	18.94 ^a	44.90 ^a	
n	136	136	136	136	

Note: Standardized slopes are in parentheses. Robust standard errors were used in computing *t* ratios.

a. *t* ratio > 1.96.

b. “Y-standardized coefficient.”

c. *t* ratio > 1.65.

d. Coefficient multiplied by 100 to reduce decimal places.

unemployment rates were higher relative to Black unemployment rates (traditionally viewed as greater “economic threat”), disparities in Black and White violent crime arrest rates were smaller. Moreover, inconsistent with the interracial crime and political threat arguments (Hypotheses 3 and 4), neither the percentage of homicides with Black offenders and White victims nor the Black mayor indicator variable showed a statistically discernible relationship with the logged ratio of Black to White violent crime arrest rates. Finally, contrary to benign neglect perspective expectations

(e.g., Hypothesis 5), the Black-White arrest disparity for violence appeared to increase when Black-on-Black homicide became more prevalent (relative to Black-on-White homicide).

With regard to measures of unevenness and White-Black exposure, two things are noteworthy. First, after controlling for the additional explanatory factors described above, the effect of unevenness, which was rather robust in the first model of Table 2, lost its punch. Exploratory analyses (not shown, available on request) suggested that the effect of the index of dissimilarity was attenuated primarily by the introduction of the White/Black unemployment ratio. Specifically, it appears that greater unevenness in the distribution of Blacks and Whites indirectly increased the relative disparity in Black and White violent crime arrest rates by increasing the relative employment disadvantage among Blacks. Second, the statistically discernible negative effect of the White-Black exposure index remained evident in the first model of Table 3. Again, this result is contrary to racial threat theory expectations, because cities where Whites had greater exposure to Blacks tended to exhibit smaller racial disparities in violent crime arrest rates.

In the next models, we predicted the log of the ratio of Black to White arrest rates for serious property offenses. Similar to the result for violent crime arrests, only the White/Black unemployment ratio and the Black intraracial/interracial homicide ratio had notable relationships with the measure of racial disparity in property arrests. And consistent with the above description, the direction of these coefficients was opposite of those predicted in the “mediation” hypotheses derived from racial threat and benign neglect models. Moreover, similar to the first model of Table 3, the inclusion of the intervening process variables accounted for the previously observed direct positive relationship between the measure of unevenness (index of dissimilarity) and the measure racial disparity in property crime arrest rates. Thus, for property offenses, the spatial opportunity model hypothesis that racial disparities in arrests would be higher in cities with greater levels of dissimilarity in the distribution of Blacks and Whites (Hypothesis 7) was not supported.

In the third model, we present the regression equation predicting racial disparities in arrests for drug offenses. As outlined in the theory section, these offenses often come to the attention of the police through direct observation, field interrogations, and buy-and-bust operations rather than through the reports of complaining citizens. And logic suggests that the absence of a complaining third party may increase the degree of discretion that police have in terms of which communities they patrol, which potential offenders they closely observe or frisk, and when they should make an arrest instead of using a more informal “warn-and-release” strategy. As a result of these differences

in police discretion, we expected that the selectivity mechanism that underlies the spatial opportunity framework was likely to be strongest for crimes that generally do not have direct victims (Hypothesis 8).

The results reported in Table 3 are generally consistent with the above logic. First, as was seen in the descriptive statistics presented in Table 1, the Black-White disparity in arrest rates for drug offenses was noticeably larger than the Black-White disparity in arrests for violent and property offenses. Moreover, in agreement with Hypothesis 7 and Hypothesis 8 and with the models presented in Table 2, we found that the index of dissimilarity had a discernible direct positive association with the log of the Black/White drug arrest ratio. Specifically, a 1 percent increase in the measure of unevenness was associated with a 0.796 percent increase in the Black-White drug arrest disparity outcome.

Seemingly consistent with the benign neglect model, the intraracial/interracial homicide ratio had no discernible effect on disparities in drug arrests, whereas it was a significant predictor in the models predicting disparities in violent and property crime arrests. However, because the relationships observed for the intraracial/interracial homicide ratio in the first two models were opposite to those predicted by the benign neglect argument, it would be dubious to conclude that this shift in the pattern of findings in the drug arrest disparity model represents a confirmation of the merit of the benign neglect explanatory framework.

Among the other intervening variables, only the White/Black unemployment ratio had a noteworthy effect on the racial disparity in drug arrests. However, as was the case earlier, the coefficient for this variable was negative, which contradicts racial threat theory (Hypothesis 2). White-Black exposure, a Black mayor, and the percentage of Black-on-White homicide did not have noteworthy associations with the relative racial disparity in drug arrests. Overall, then, the evidence obtained from the drug disparity arrest model accords best with the spatial opportunity and police discretion arguments. In contrast, virtually no support is offered for racial threat and benign neglect perspectives.

The last model presented represents the effects of White-Black exposure, the index of dissimilarity, and the intervening variables on the racial disparity in arrest rates for weapons law violations. Although perhaps less so than is the case for drug offenses, a substantial share of weapons arrests may occur as a result of proactive policing, such as “field interrogations” in which the police discover illegal weapons in the possession of “suspicious” individuals they stop and question. If so, a significant share of weapons law violations can be viewed as victimless offenses, and we would expect that the racial disparity in weapons offenses would behave similarly to the

disparity in drug offenses; that is, the spatial opportunity and police discretion model hypotheses (Hypotheses 7 and 8) should be particularly relevant.

The results in the weapons model basically support the preceding rationale. As was observed in the model for drug arrest disparities, there was a significant positive effect of the index of dissimilarity on the log of the Black/White weapons arrest rate ratio. Specifically, the results indicate that a 1 percent increase in unevenness was associated with a 0.38 percent increase in the measure of racial disparity in weapons arrests. Although this effect appears weaker than the parallel coefficient observed in the drugs model, it is consistent with the expectation that spatial opportunity effects will be evident for more discretionary offense categories. Also similar to the drugs models, the addition of the intervening variables reduced the direct impact of the unevenness variable on the racial disparity in weapons arrests. In particular, it appears that part of the influence of dissimilarity on the racial disparity in weapons arrests was indirect, because of its effect on the relative disparity in labor market difficulties of Whites and Blacks (i.e., the White/Black unemployment ratio). In addition, some of the effect appears to operate via the Black intraracial/interracial homicide ratio, which had a statistically discernible positive association with the racial disparity in weapons law arrests. Overall, these results offer little support for the racial threat and benign neglect frameworks. Contrarily, the spatial opportunity and police discretion models were generally supported.

Although our concern has primarily been with the effects of White-Black exposure, unevenness, and the four intervening variables, a summary of the impact of the control variables is in order. Looking broadly across the models, several controls had consistent effects worthy of mention. First, the Black/White homicide offending ratio, which we included to control for the possibility that racial disparities in arrests result simply from race differences in criminal offending, had a statistically discernible positive effect in each model. Thus, as we would expect, the overrepresentation of Blacks in criminal offending did appear to contribute to the explanation of arrest rate disparities. In addition, the overall level of socioeconomic disadvantage had a consistent negative association with the arrest rate disparity variables. That is, the results of each model indicate that cities with higher levels of overall socioeconomic disadvantage had smaller racial disparities in arrests. Likewise, arrest rate disparities appeared to be lower in cities that had larger populations, except in the case of property crime arrests. This generally suggests that smaller cities with less socioeconomic disadvantage had lower arrest disparities and that the large urban milieu, with higher levels of resource deprivation, effectively had a leveling effect across races in the arrest disparity.

The measure of law enforcement capacity, as measured by the number of officers per capita, had a discernible effect only in the violent crime and weapons law arrest models. In those models, the results suggest that cities with greater crime control capacity showed a greater Black/White arrest disparity. The modest performance of this variable is not surprising in light of a recent meta-analysis by Pratt and Cullen (2005) that made clear that policing and deterrence variables generally have a much weaker effect than sociological variables in macro-level crime models. The dummy variable for cities located in the South region of the country had only an inconsistent effect across the models, appearing as statistically significant only in the property arrest disparity model. Finally, the control for the overall volume of crime (i.e., “modified crime index”) was not associated with the outcome in any of the full models presented in Table 3.

Sensitivity Analyses

To check on the robustness of the results presented, we examined a variety of regression diagnostics as well as supplemental regression models. First, we assessed whether our results hinged on the influence of outlying data points by reviewing partial regression plots, plots of squared residuals against leverage values, and plots of Cook’s *D* statistics against Studentized residuals. For each of the outcomes predicted, we observed between six and eight observations that were outlying in the sense of having Studentized residuals greater than an absolute value of 2. For our sample size of 136, this is about what one would expect if the sample were drawn from a normal population. Of these “outlying” observations, between two and four observations per model had Cook’s *D* statistics above the size adjusted cut-off ($4/[n - p]$) commonly used by regression analysts (see Fox 1997). The impact of these cases on substantive results, however, was minimal. Supplemental ordinary least squares analyses that deleted the “influential outliers,” as well as robust regression models that reweighted outlying observations (iteratively reweighted least squares), all led to essentially the same findings as reported in Table 3. Thus, there is little evidence to suggest that the results reported above were sensitive to outliers.

In addition to examining the impact of outlying data points, we also considered the impact of multicollinearity in our regression models. As a summary tool for assessing collinearity, we include in Tables 2 and 3 a column that contains the variance inflation factor (VIF) associated with each explanatory variable included in the regression models. In general, the greater the VIF, the more the standard error of the regression coefficient is inflated (relative to what it would be if there were zero collinearity involving that

regressor). Although experts do not always agree on the specific value of VIF that reflects “too much” collinearity, a convention in the social science literature is that a VIF value of 4 or greater may be problematic. Although none of the VIF values exceeded this threshold in the “baseline” models presented in Table 2, the results in Table 3 indicate that the VIF for the unevenness variable was 4.01. Thus, there is some reason to be concerned that collinearity inflated the standard error of the unevenness coefficient and thereby decreased the t ratio for that variable in each of the regression models.

Because the unevenness coefficient in the violent and property arrest models was significant in the baseline models (Table 2) but not in the full models (Table 3), the variance inflation caused by multicollinearity could have potentially affected our substantive conclusions. However, when we compared the standard errors for the unevenness coefficient from the models in Table 2) which would be viewed as having “acceptable” levels of collinearity by most analysts) with the standard errors for the models reported in Table 3, only very small differences were noted. Clearly, on their own these differences in standard errors are very unlikely to be the source of the change in the significance level of the unevenness coefficients in the violent and property models.

In contrast to the minor fluctuation in the standard errors, there were more noticeable changes in the magnitudes of the unevenness coefficients as we moved from the baseline to full models. Indeed, across all four regression models, the unevenness coefficient was substantially smaller in Table 3 (full models) than in Table 2 (baseline models). As mentioned earlier, supplemental analyses indicated that the majority of the attenuation of the unevenness coefficients occurred when the White/Black unemployment ratio was added into the model, and indeed, these two variables were moderately correlated ($-.53$). Thus, there is reason to wonder whether our results reflect a classic case of partialling difficulties caused by collinearity among explanatory variables. From our perspective, the available evidence does not point to that conclusion. First, one prototype of partialling problems occurs when high levels of collinearity result in insignificant t ratios for two variables that obviously have an association with the dependent variable. That is not the case in our data, because in two models, there is evidence that both the unemployment ratio and unevenness had significant effects on the racial disparity in arrest rates. Second, another typical pattern of partialling problems occurs when two highly correlated predictors have only slightly different correlations with the dependent variable but one variable (usually the one with a slightly stronger association) gets assigned virtually all of the “explanatory credit” (and is generally significant), while the other variable gets little credit (and is not significant). This latter scenario also is not reflected in our data. Although correlations for the unemployment

ratio (−.62) and unevenness (.55) with the weapons disparity outcome were fairly similar in magnitude, the results in Table 3 indicate that both of those predictor variables had fairly strong and statistically significant effects on that dependent variable.

Finally, along with inflated standard errors and partialling problems, another hallmark of problematic multicollinearity is parameter instability. Indeed, it has been shown that when multicollinearity is great, small random changes in data may produce substantial instability in estimated regression parameters. Thus, one way to assess the consequences of collinearity is to repeatedly introduce small random measurement errors into one's data and then reestimate models. For Stata users, the *perturb* program written by John Hendrickx can be used to accomplish that task.¹⁷ Using this program, we ran 100 iterations of our model with minor random changes introduced into the unevenness and White/Black unemployment ratio at each iteration. The results of these analyses suggest that our parameter estimates were stable despite the evidence of collinearity indicated by the single VIF greater than 4.

In light of the above evidence, we see little reason to believe that our results are indicative of problematic levels of multicollinearity. Rather, they may suggest a substantively meaningful process in which racial inequality in unemployment (i.e., greater Black disadvantage) mediates (in part or full) the relationship between unevenness and the Black-White disparity in arrest rates. However, with cross-sectional data, that interpretation is speculative. An equally plausible alternative is that unevenness and racial inequality in unemployment are two correlated indicators that reflect a latent dimension of race-linked structural inequality. In either case, the evidence seems to support the notion that when Blacks are economically and residentially disadvantaged relative to Whites, they also tend to be overrepresented in arrest data, particularly for more discretionary offenses.

Discussion and Conclusion

The purpose of this study was to address several deficiencies in extant research on formal social controls. Focusing on a theoretically salient but empirically neglected outcome, the Black-White disparity in arrest rates, we tested hypotheses from two venerable explanatory frameworks: racial threat and benign neglect. We argued that the lack of empirical support for these frameworks in past studies of arrest outcomes may be linked to measurement weaknesses including the failure to adequately tap within-city variations in White-Black contact as well the general absence of proxies for the intervening mechanisms specified in racial threat and benign neglect arguments. Of equal importance, we also proposed two new arguments, which we call

spatial opportunity and police discretion, to help account for variations in racial disparities in arrest rate outcomes across multiple offense categories (e.g., violent, property, drugs, weapons).

Overall, the results of the analyses offer very little support to hypotheses drawn from the conventional racial threat framework. Despite our attempts to address measurement limitations in existing work, our results basically extend past studies of arrest rates that reported findings that contradict or fail to support the expectations of the racial threat model. Given the considerable support that racial threat theory has garnered in other studies of formal social control, the lack of empirical support found in most studies of arrest is somewhat surprising. Nonetheless, these findings may signal that scholars need to better theorize and empirically test the scope conditions that define when racial threat effects will and will not occur. From our reading of the literature and our empirical results, racial threat variables seem best suited to explain variations in the overall institutional capacity of formal social control, such as police force size or criminal justice expenditures, or perhaps to explain the overall use of certain forms of punishment, such as the death penalty. In contrast, it has fared less well as an explanation of formal control outcomes that are race disaggregated or that directly index racial disparities. Although we agree that the racial threat perspective has intuitive appeal and acknowledge that its key indicators (e.g., percentage Black) recently have been linked to Whites' perception of economic threat, personal safety, and punitive attitudes (e.g., King and Wheelock 2007), we also note that the racial threat argument rests on some very strong assumptions about the ease with which Whites can and will act as a collective entity to use the criminal justice machinery against Blacks (or other minority groups perceived as a threat). In light of incident-level data suggesting that arrest probabilities are higher for Whites than Blacks (D'Alessio & Stolzenberg 2003; Pope and Snyder 2003), perhaps those assumptions are too simplistic.

Similar to the racial threat perspective, we effectively find very limited support for the benign neglect model. Our main indicator for benign neglect, the ratio of intra- to interracial homicides, performed opposite from what is predicted by this model. Moreover, even the measures of minority group prevalence used in our study (White-Black exposure, percentage Black) show only weak or negligible effects on the arrest disparity outcomes. Thus, although some prior studies have reported evidence supporting this perspective (see, e.g., Liska and Chamlin 1984; Parker et al. 2005), our study, which incorporated mediating mechanisms (albeit roughly) ignored in prior analyses, does not. Nevertheless, our study is among the first to attempt to explicitly assess the mediating processes identified in benign neglect theory, so additional studies are certainly needed before definitive conclusions can be made.

In contrast to the pattern of results regarding racial threat and benign neglect predictions, our findings provide greater support for spatial opportunity and police discretion frameworks. In fact, the finding that the index of dissimilarity (unevenness) has a positive relationship with the arrest ratio for drug and weapons offenses but not for property and violent offenses suggests that a combination of those viewpoints may be the most effective explanation. In other words, although dissimilarity in the distribution of Blacks and Whites can structurally set the stage for implicit or explicit biases to result in racially disparate arrest rates, those disparities are most likely to occur at noteworthy levels for crimes in which the lack of a body, a victim, or a complaining third party provides the police with additional discretionary authority with regard to arrest decisions.

We consider the theoretical implications of these findings substantively important. Whereas other researchers have often failed to find substantial evidence of threat-motivated bias in arrest rates, our logic and empirical analyses move the literature forward by suggesting that disparity-generating processes may be more contextual than previously thought. By taking into consideration that racial residential unevenness may result in an opportunity structure that facilitates an imbalance in the allocation of criminal justice resources, our analyses suggest that discrepancies in arrest rates may not be driven by explicitly racist police officers, but they may be manifested by the fact that the officers get deployed in a manner that heightens their contact with Blacks while minimizing their contact with Whites.

Because our study is the initial attempt at testing the spatial opportunity and police discretion frameworks, the results remain fairly tentative. We readily acknowledge that the measures available to us do not directly tap into important factors such as the manner by which police resources are distributed within cities. Therefore, our interpretations must be viewed with caution, because much additional empirical inquiry is needed to more comprehensively evaluate the merit of spatial opportunity and police discretion frameworks. Ideally, subsequent work will build on the current research by attempting to obtain, across places with varying levels of racial residential unevenness, data that tap into the distribution of public services (including policing). Such data collection efforts will likely take tremendous amounts of time, effort, and money, but as is illustrated in the work of Beckett et al. (2005, 2006), the pay-off in new knowledge about racial disparities in formal social control can be enormous.

In addition to the data-gathering efforts noted above, we see several other promising avenues by which future research may extend the current study. First, although our results do indicate that racial unevenness is associated

with the arrest disparities for “softer” crimes such as drugs and weapons violations, it is not entirely clear whether this effect reflects implicit or explicit biases rooted in race, class, or some interaction of the two. Research has demonstrated that racial and economic segregation overlap considerably, and there is also substantial reason, particularly from mainline conflict perspectives, to believe that police behavior may be influenced by class as well as race. This is perhaps best laid out in Black’s (1976) classic work *The Behavior of Law*, in which he argued that those with lower vertical status have less access to use law in a beneficial way but are more likely to experience it in a repressive fashion. Following that line of thinking, it is possible that our findings actually reflect unmeasured class segregation and the discriminatory application of formal social controls occurs against communities at the low end of the economic-class spectrum. Although we are aware that class-stratified crime data are not readily available, an alternative way to index this is to use class-based segregation indices, which are readily computable from available census data (Jargowsky 1996; see also Eitle, D’Alessio, and Stolzenberg 2006). These data also can be derived in a race-specific manner, and therefore, researchers can attempt to untangle racial and class segregation effects on arrest rate disparities for more discretionary offenses.

A second avenue for future research that is suggested by our findings lies in expanding the scope of sample selection criteria. Although our focus on large cities is consistent with the bulk of prior research, our findings indicate that the racial arrest discrepancies are actually smaller in large cities and in those that have more socioeconomic disadvantage, which also tend to be larger cities. This suggests that midsized and smaller cities, as well as rural areas, would be a good place to explore the boundaries of these themes more extensively. It makes sense that in smaller and more homogeneous environments, the tenets of racial threat theory in particular are more likely to play out. Smaller communities should have a more pronounced capacity for the civic mobilization of police resources because of the more intimate nature of friendship and acquaintanceship networks (see Weisheit, Wells, and Falcone 2006). Furthermore, rural communities throughout the southern United States are home to substantial Black populations, and of course the South has historically exhibited substantial levels of violent crime among both the White and Black populations (Ayers 1984). Moreover, the rural context is unusual because crime clearance rates by arrest are significantly higher in this setting than in the metropolitan milieu. Thus, racial arrest disparities in settings other than the urban environs may be a profitable path to pursue.

Related to these themes, the recently evolving civic engagement and social capital perspectives on crime and community integration may bring significant insight to this debate (Messner, Baumer, and Rosenfeld 2004;

Rosenfeld, Messner, and Baumer 2001). Communities with substantial stocks of social capital should have a stronger capacity both to normatively reduce crime by inducing conformist behavior and to formally attack it by mobilizing police resources. Civically vibrant communities are able to hold accountable law enforcement, to dialogue with them about community needs, and to provide input on policing practices. Hence, the next chapter in the evolution of this literature should consider taking advantage of the ideas the social capital and civic engagement literature have to offer.

Appendix
Supplemental Ordinary Least Squares
Regression Models Using Percentage Black
(control variables not shown)

Variable	Offense Category			
	Violent	Property	Drugs	Weapons
Model 1: with percentage Black				
Unevenness (ln)	.176	-.050	.801 ^a	.394 ^a
Percentage Black	-.001	-.001	-.010 ^b	.005
White/Black unemployment ratio	-1.16 ^a	-1.34 ^a	-2.39 ^a	-1.23 ^a
Black mayor	-.060	.033	.037	-.025
Percentage Black-on-White homicide	-.010 ^c	.006	.005	-.003
Black intraracial/interracial homicide ratio	.009	.026 ^a	-.013	.021
Model 2: with percentage Black quadratic				
Unevenness (ln)	.252 ^b	-.057	.728 ^a	.377 ^a
Percentage Black	-.005	.002	-.007	.006
Percentage Black squared	.0003 ^a	-.002 ^c	-.0003	-.006 ^c
White/Black unemployment ratio	-1.28 ^a	-1.33 ^a	-2.28 ^a	-1.20 ^a
Black mayor	-.092	.036	.067	-.019
Percentage Black-on-White homicide	.002	.006	.004	-.004
Black intraracial/interracial homicide ratio	.009	.026 ^a	.013	.021
Model 3: with White-Black exposure quadratic				
Unevenness (ln)	.152	-.085	.792 ^a	.378 ^a
White-Black exposure (ln)	-.184 ^a	-.070	-.029	-.081
White-Black exposure (ln) squared	.020	-.089	-.012	-.015
White/Black unemployment ratio	-1.29 ^a	-1.33 ^a	-2.38 ^a	-1.28 ^a
Black mayor	-.029	.067	-.022	.024
Percentage Black-on-White homicide	.003	.007	.003	-.001
Black intraracial/interracial homicide ratio	.015 ^b	.029 ^a	-.001	.030 ^a

Note: Robust standard errors were used in computing *t* ratios.

a. *t* ratio > 1.96.

b. *t* ratio > 1.65

c. Coefficient multiplied by 100 to reduce decimal places.

Notes

1. Although we believe that indicators of racial disparity or inequality in arrests are a more direct and preferable measurement strategy, it is the case that models predicting race-specific arrest rate levels can be used to construct inferences about the raw difference in arrest rates. However, those inferences typically require additional analytic steps, such as the use of Oaxaca decomposition techniques (Oaxaca 1973). Unfortunately, those additional steps have rarely been used in the criminological research literature (but see Phillips 2002) and to our knowledge have not been used in past studies of racial inequality in formal social controls.

2. Blalock (1967) proposed that minority prevalence would have a curvilinear association with discrimination, with the shape of that curve determined by the degree to which the economic threat process (a positive decreasing slope) or the political threat process (a positive increasing slope) was predominant. Although we introduced additional variables to account for economic and political threat processes, we also followed Blalock and empirically assessed evidence of curvature in the relationship between relative minority presence and the racial disparity in arrest rates. The results of those analyses, presented in the bottom two panels of the Appendix, suggest little evidence of curvilinear effects.

3. One view of this negative relationship suggests that it reflects the right-hand side of the curvilinear relationship between minority prevalence and discrimination specified in Blalock's (1967) racial threat argument. Thus, from this standpoint, the benign neglect thesis is effectively part and parcel of the broader racial threat framework. A second viewpoint is that the benign neglect hypothesis reflects a linear causal process that is distinct from the racial threat model. Both viewpoints are eminently plausible, and therefore, we investigated the functional form of the relationship between minority prevalence and the racial disparity in arrests. Interestingly, our results suggest very limited evidence of either a curved or a linear effect of the minority prevalence measures.

4. We also estimated the models using an untransformed version of the Black/White arrest ratio for each offense type. The pattern of results from those models is consistent with that of the results reported herein.

5. We took race-specific three-year averages of the data to provide a more reliable estimate of the average volume of arrests circa 2000.

6. Although we believe that the White-Black exposure index is more consistent with theoretical conceptualizations of the racial threat that emphasize minority group encroachment on the physical and social spaces inhabited by Whites, we also computed supplemental analyses that used percentage Black in place of the exposure index. Conclusions from those analyses were similar to those discussed herein and are presented in tabular form in the Appendix.

7. We are not suggesting that this measure directly taps the extent to which racially biased policing or patrols are in fact taking place. Rather, we believe it taps whether the racial residential distribution of the city provides an opportunity context that is more or less favorable for the selective deployment of police resources to take place.

8. Exploratory analyses indicate that log transforming the White-Black exposure index and the index of dissimilarity results in a better fit to the data. However, we note that the basic pattern of results does not hinge on this transformation. Indeed, our findings and conclusions regarding these variables remain the same if the data are modeled in either way. We opted to report the results using the logged measures because they yielded the best fit, resulted in lower levels of multicollinearity in the set of explanatory variables, and allowed the exposure index and dissimilarity index coefficients to be interpreted as elasticities (i.e., the percentage change in the Black-White arrest ratio for a 1 percent change in the relevant predictor variable).

9. Blalock's (1967) seminal work suggests that economic threat may operate in a curvilinear manner (specifically, a positive decreasing slope). Thus, we tested for this possibility by alternatively including a logged version of the White/Black unemployment ratio. Although the results of these models suggested marginal evidence of curvature in the effect of the unemployment ratio, the findings were not materially affected by this alteration to the model, but levels of multicollinearity did increase somewhat. Given the similarity in the results across the models with logged and unlogged measure of economic threat, we report only the results from the latter.

10. We also tried two alternative measures: an indicator scored 1 for cities that had congressional districts with African American representatives, and the percentage of the city council that was non-White. The results of models using those measures were not materially different from those reported here.

11. However, to the extent that fear of Black-perpetrated crime among Whites is a subjective and irrational process, it may not respond to objective variations in interracial crime risks. If that is the case, the basis for expecting that a measure of interracial crime will affect the racial disparity in arrests and mediate the impact of minority prevalence is weakened. Yet it should be noted that past research has shown a relationship between minority prevalence and both objective measures of interracial crime and measures of crime fear (Liska et al. 1981). Likewise, there is empirical backing for measures of interracial crime as a mediator of the link between minority prevalence and non-White arrest rates (Liska and Chamlin 1984). Moreover, prior work has demonstrated that interracial crime is positively associated with Black/White violent crime arrest ratios (Eitle et al. 2002).

12. Because some cities reported zero Black-on-White homicide incidents, we added the constant of one to the numerator and denominator to avoid division by zero.

13. Index offenses included in this summary rate are homicide or nonnegligent manslaughter, robbery, rape, aggravated assault, burglary, motor vehicle theft, and larceny-theft.

14. All variables are expressed in their raw (e.g., not log transformed) metrics.

15. Because the dependent variable and both the White-Black exposure index and the measure of unevenness (index of dissimilarity) were converted to the natural logarithm scale, the coefficients associated with these latter two measures can be interpreted as elasticities. That is, these estimates indicate the percentage change in the dependent variable associated with a 1 percent change in the relevant independent variable.

16. For the unevenness variable, the evidence contradicting the null hypothesis (e.g., the t ratio) was greater in the drugs and weapons equations than in the violence and property arrest equations. Logically, these differences seem to imply some support for the police discretion argument. However, because the dependent variable differed across these regression models, these differences cannot be viewed as definite evidence of differences in effect size. Moreover, the significant association between unevenness and the arrest disparities for violent and property crime is not consistent with the expectations of the police discretion model as articulated in Hypothesis 8.

17. The perturb program can be obtained by typing "findit perturb" in the command window of Stata.

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