# UNBIASED INDICES OF UNEVEN DISTRIBUTION AND EXPOSURE: NEW ALTERNATIVES FOR SEGREGATION ANALYSIS


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## ABSTRACT

This paper introduces new versions of popular indices used to measure the two most widely studied dimensions of residential segregation, uneven distribution and exposure. The new versions have the desirable characteristic of being unbiased; that is, under random distribution the new versions of indices of uneven distribution have expected values of zero and the new versions of indices of exposure have expected values that track group representation in the city population. In contrast, the expected values of conventional versions of indices of uneven distribution and exposure are subject to bias that can be substantial in some circumstances. The impact of bias can complicate the interpretation and comparison of conventional index scores; in some circumstances it can render segregation indices untrustworthy altogether. Consequently, researchers' concerns about the undesirable impact of index bias has led them to routinely restrict the scope of segregation studies as a means of dealing with the issue. The new versions of popular segregation indices introduced here are free of bias and this brings several benefits for research. First, the new versions extend the feasible scope of segregation studies because they can be used in a wider range of circumstances. Second, the new versions are easy to compute and support interpretations that are substantively appealing and easy to explain to broad audiences. Third, the benefits of using the new versions of popular indices come at little cost. In circumstances where conventional versions of popular indices are trustworthy, the scores for the new versions will yield similar results. The new versions only yield different, and superior, results when the conventional versions are adversely impacted by bias. In view of this, researchers can use the new versions in parallel with, or as substitutes for, the conventional versions in most research applications.

Residential segregation is a multi-dimensional phenomenon (Stearns and Logan 1986; Massey and Denton 1988) but two dimensions – uneven distribution and inter-group exposure – have been the primary focus of empirical studies. Inter-group contact is typically assessed with indices of same-group exposure ($P_{XX}$ and $P_{YY}$) and cross-group exposure ($P_{XY}$ and $P_{YX}$) introduced originally by Shevky and Williams (1949) and Bell (1954) and later popularized by Lieberson (1980; 1981). Uneven distribution is usually assessed with summary indices of distributional dispersion such as the index of dissimilarity (D), the gini index (G), the variance ratio (V), Atkinson's index (A), Hutchens' square root index (R), and Theil's index (H). Methodological studies characterize these popular indices as useful and attractive options for assessing exposure and uneven distribution (e.g., Duncan and Duncan 1955; James and Taeuber 1985; Stearns and Logan 1986; White 1986; Massey and Denton 1988; Hutchens 2001; 2004; Reardon and Firebaugh 2002; Reardon 2006). Most empirical studies of residential segregation draw on these indices.

All of these indices have something in common; they all are inherently biased. Specifically, under random assignment the standard versions of indices of uneven distribution have expected values above zero and the standard versions of exposure indices have expected values that differ from city-wide group composition. As I review in more detail below, methodological analysis establishes several basic points about the nature of the bias in these indices: bias is inherent and is rooted in their basic formulation; the magnitude of bias varies with circumstance and across indices; bias can be non-negligible in many research situations; and, non-negligible bias complicates the interpretation and comparison of index scores. Concerns about index bias have had important and unwelcome impacts on segregation research. Specifically, they have caused researchers to limit the scope of segregation studies and to narrow the range of questions they investigate. This is a direct byproduct of researchers routinely imposing restrictions on the research designs of segregation studies to avoid situations where bias carries the potential to render index scores untrustworthy.

My goal in this paper is to introduce unbiased versions of popular indices of uneven distribution and exposure that can eliminate index bias and thereby eliminate the need to restrict the research designs of segregation studies. To accomplish this, I show that the bias in popular segregation indices can be traced to a relatively simple aspect of their construction. Understanding this makes it possible to develop revised constructions that yield new versions of the indices that are free of bias. The resulting new versions of popular segregation indices are attractive alternatives for research based on several considerations: they are unbiased; they are conceptually and methodologically and conceptually straightforward; they support appealing substantive interpretations; and they are relatively easy to implement and compute. Significantly, the scores of unbiased indices track those of conventional indices quite closely when bias is negligible; so the benefits of using the unbiased versions of popular indices come at little cost.

## An Introduction to the Issue of Index Bias

The index of dissimilarity (D) is by far the most widely used index of uneven distribution. Accordingly, it has been subjected to greater methodological scrutiny than other indices and is the obvious example to consider first. Early on researchers gravitated to D based on its ease of calculation and interpretation. In more recent decades it continues to be widely used to maintain continuity with past research and also because methodological studies suggest that, despite various technical shortcomings, D correlates closely with other indices that might be seen as superior on technical grounds (Duncan and Duncan 1955; Massey and Denton 1988). An important, early methodological review of D's qualities by Taeuber and Taeuber (1965: Appendix A) noted that zero, the value that signals the absence of segregation defined in relation to exact even distribution, does not obtain under random distribution and often is logically impossible owing to the nature of the integer counts in residential distributions (1965:231-235). Later methodological studies characterized D's positive expected value under random assignment (i.e., E[D] > 0) as "bias" and expressed concern about the behavior of D based on the fact that its bias varies in magnitude with circumstance and can be problematic under certain conditions (e.g., Cortese, Falk, and Cohen 1976; Winship 1977). For over three decades now, methodological studies have continued to examine various aspects of the issue of index bias – most often in relation to D and occasionally in relation to G, V, and other indices – and debated its practical implications for segregation studies (e.g., Taeuber and Taeuber 1976; Blau 1977; Winship 1978; Massey 1978; Cortese, Falk, and Cohen 1978; Falk, Cortese, and Cohen 1978; Farley and Johnson 1985; Carrington and Troske 1997; Ransom 2000; Allen, Burgess, and Windmeijer 2009).

Three points of broad agreement have emerged from the debate and discussion on this topic. First, there is general agreement on several technical issues: D is subject to bias in the sense that its expected value under random distribution is not zero (i.e., E[D] > 0); the magnitude of the bias in D varies with circumstance; the bias can be large and non-negligible in some circumstances; and, in such circumstances, bias can lead investigators to draw incorrect conclusions about the levels and variation of uneven distribution. Relatedly, methodological studies establish that the magnitude of bias in D varies as a complex (i.e., nonlinear, non-additive) function of two basic factors; the average total population per areal unit and the relative size of the two groups in the segregation comparison (Cortese, Falk, and Cohen 1976; Winship 1977; Farley and Johnson 1985; Boisso, Hayes, Hirschberg, and Silber 1994; Carrington and Troske 1997; Ransom 2000; Allen, Burgess, and Windmeijer 2009). Methodological studies further establish that bias may be affected by additional factors (e.g., variation in area population counts). Below I note that bias also is affected by two factors that heretofore have not been widely appreciated; namely, the relative size of population groups other than the two groups being compared and the degree of segregation of these other groups from the two groups being compared.

A second point of agreement is that random distribution is a valid and desirable reference point for assessing segregation, particularly for addressing questions of whether race (or gender or other social characteristics) plays a role in segregation over and above the consequences of chance or artifactual aspects of index design. The potential desirability of indices possessing this quality has been noted often (e.g., Jahn, Schmidt, and Shrag 1947; Reiner 1972; Zelder 1972; Cortese, Falk, and Cohen 1976; Winship 1977; Blau 1977; Boisso, Hayes, Hirschberg, and Silber 1994; Carrington and Troske 1995; 1997).[1] For example, Cortese, Falk, and Cohen state that it is "natural" to "construct an index which takes a value of zero when the distribution is random" (1976:631). No significant objection has been raised against the goal of seeking "unbiased" segregation indices. Taeuber and Taeuber (1976) and Winship (1977) note that segregation resulting from randomness can sometimes be substantively interesting in its own right. But this does not undercut the potential value of unbiased indices whose scores provide a reliable signal that segregation departs from levels expected under random distribution. Winship argues that this quality is especially desirable when interest is focused on the causes of segregation rather than its consequences (1977:1065).

A third point of agreement in the current literature is that conventional versions of D and other popular indices can be used if researchers take appropriate measures to avoid the potential complications of index bias. This view is grounded in the assumption that no realistic alternatives are available because no proposed method for achieving unbiased index scores has been accepted as viable, practical, and compelling (Taeuber and Taeuber 1976; Massey 1978). It additionally stresses that segregation is an important social phenomenon and that, since bias in D can be small and negligible in many research situations, it is useful to work with D if appropriate precautions are taken (Taeuber and Taeuber 1965; 1976). This view has had great influence on the research designs of segregation studies. I argue that it produced a less than ideal situation in key regards. Specifically, the standard precautions adopted to guard against undesirable consequences of index bias are crude and uneven in their effectiveness and, even more importantly, they have the unwelcome consequence of substantially restricting the scope of segregation studies.

At this point I broaden the discussion beyond D to include all measures of uneven distribution and also indices of same-group and inter-group exposure. I do so by offering the following two summary statements. All conventional indices of uneven distribution that take a value of zero under *exact* even distribution – including D, G, A, R, H, and V – are biased; they have positive expected values under random assignment (i.e., E[•] > 0) that can sometimes be large and non-negligible. Similarly, all conventional indices of same-group and inter-group exposure are subject to bias in the sense that their expected values under random assignment depart from what would be expected based on the population representation of groups and the magnitude of this bias varies with circumstance and can sometimes be large and non-negligible. The nature of bias has been investigated most thoroughly for D (as reviewed above). I review the general basis for bias in all conventional indices of uneven distribution and exposure below. For the moment I note simply that the main points concern regarding bias in D generalize broadly to all conventional segregation indices.

[1] The issue is also taken up in discussions of measuring segregation in occupation and other nominal outcomes. Some of the references listed here give attention to this issue (e.g., Blau 1977; Carrington and Troske 1997; Ransom 2000).

The current situation then is this. Indices that assess segregation in relation to random distribution are desirable, but they are not available. Conventional indices are often problematic because they are affected by non-negligible bias under certain circumstances. But, researchers have gone forward with empirical research by adopting precautionary practices – typically implemented in the form of relatively crude "rules of thumb" for restricting study designs – to guard against the potentially adverse consequences of index bias.

In the remainder of this paper I introduce new, unbiased versions of popular segregation indices that give researchers expanded options for studying residential segregation. I hold that the new versions of popular indices bring significant benefits while imposing minimal costs and thus should be routinely used in parallel with, or as substitutes for, conventional versions of popular indices.

I organize the discussion in the paper as follows. I first review the basic correlates of index bias and how concerns about index bias have come to influence research designs and restrict the scope of segregation studies. I then review previous attempts to deal with bias and explain why they have not succeeded. Next I identify the feature of segregation index construction that is the basic source of index bias and I then identify how relatively simple revisions in index construction eliminate bias. I then review selected empirical analyses to demonstrate the desirable qualities of the new indices and the new possibilities they provide for expanding the scope of segregation studies. Finally, I conclude by noting some implications the new measures carry for several issues in segregation measurement.

**Patterns of Index Bias and the Consequences of Concerns about Bias**

The exact nature of index bias varies from index to index and I do not review all popular indices in full detail here. However, the nature of the variation in bias for D is typical and can be briefly summarized as follows. For D; bias (i.e., E[D]) is a non-linear, non-additive function of two key factors. The first factor is effective neighborhood size (ENS) – the size of the combined population counts for the two groups in the comparison over areal units. The second factor is the relative size of the reference group as registered by its proportion (P) or , alternatively, by the group ratio (GR) – the ratio of the size of the smaller group to the size of the larger group. The mathematical relationship governing bias (E[D]) is a complex non-linear, non-additive function of these two factors, but its main features are easy to describe. All else equal, bias increases at an increasing rate as ENS becomes smaller. All else equal, bias increases at an increasing rate as GR falls from balanced (i.e., 1) to highly imbalanced (i.e., nearing 0). And, in addition, ENS and GR interact such that declines in ENS produce greater bias when GR is low and declines in GR produce greater bias when ENS is small.

The literature suggests that, in principle, bias can be estimated from formulas for E[D] such as those reported in Winship (1977) or by computation intensive methods (e.g., bootstrap simulations) that involve fewer assumptions (Allen et al 2009; Carrington and Troske 1997). This then permits scores for D to be adjusted to remove the impact of bias. But researchers rarely adopt this practice in empirical studies. Instead, researchers more often deal with bias by following methodological "rules of thumb" that relate to E[D] in relatively crude ways. For example, segregation studies nowadays tend to limit their focus to situations involving broad population groups, larger spatial scales, relatively balanced group ratios, and full count data. These restrictions on study design do generally help researchers avoid circumstances where E[D] is likely to be high. But this benefit comes at a significant cost; it greatly restricts the scope of segregation studies. At a minimum, standard restrictions in study design largely preclude analyses involving one or more of the following: narrowly defined population subgroups, small spatial scales, groups that are small in relative size, and analyses based on sample data instead of full count data.

The impact of concerns about index bias on the scope of segregation studies is pervasive. One example of this is the near total disappearance from the literature of studies that assess segregation at smaller spatial scales. For example, analysis of segregation based on block-level data once was common (Taeuber and Taeuber 1965; Sorenson, Taeuber, and Hollingsworth 1965; Farley and Taeuber 1975; Van Valey, Roof, and Wilcox 1978). But nowadays it is rare if not nonexistent. This is not for lack of substantive relevance. Assessing segregation at this spatial scale has obvious value because it can potentially detect segregation that might otherwise be missed. As one example, block-level analysis might be useful for studying the emergence of segregation patterns for newly arriving migrant or immigrant populations whose patterns of segregation during the phase of initial settlement might not be evident when assessing segregation at larger spatial scales such as the level of census tracts. But, even as advances in computing technology in recent decades make block-level analysis easier than ever before, segregation studies have "retreated" to the census tract level to avoid problems of bias that arise when measuring segregation based on small area counts (i.e., low ENS).

Another example is that empirical studies avoid examining segregation in metropolitan areas where one of the populations in the analysis is a relatively small proportion of the population or is small in absolute population size. For example, Farley and Frey's (1994) important study of trends in segregation for blacks, Latinos, and Asians considered metropolitan areas only when the minority group under consideration either reached 20,000 in overall population or represented 3 percent or more of the city population. As a result, out of 318 total metropolitan areas, their analysis included only 232 areas for blacks, only 153 areas for Latinos, and only 66 areas for Asians. The metropolitan areas excluded from comparison were those for which minority group size was small in both relative and absolute size. Similarly, in a study of segregation patterns for five Asian-origin groups (Chinese, Japanese, Korean, Vietnamese, and Asian Indian), Massey and Denton (1992) restricted their analysis to metropolitan areas where the size of the Asian-origin group in question was 5,000 or higher. This limited the scope of their analysis to no more than 11 metropolitan areas for any group. In addition, they reported segregation scores only for group comparisons where both groups in the segregation comparison had 5,000 persons and this eliminated 20-30% of possible comparisons involving other Asian-origin groups. They explicitly justified these restrictions in terms of concerns about index bias stating the following.

> Since the index of dissimilarity is inflated by random variation when group sizes get small (Massey 1978), we only compute indices when the group size in the SMSA exceeds 5,000" (Massey and Denton 1992:171).

Massey and Denton did not adopt these restrictions on study design based on theoretical interest or substantive concerns. They adopt the restrictions solely as a means of guarding against the potential adverse consequences of index bias. Unfortunately, the restrictions obviously limit the scope of segregation analysis. For example, it precludes the investigation of how segregation emerges when new population groups arrive in a city and how segregation changes over time as new groups grow in absolute and relative size.

A final example is the impact on research examining racial segregation between racial-ethnic groups matched on important social characteristics such as socioeconomic status. Empirical investigations of this subject routinely limit their analyses sharply to a very small number of cities. Furthermore, to accomplish this they must use broad socioeconomic groupings. These restrictions in study are adopted solely to avoid complications associated with index bias as the following statements from two important studies indicate.

> "Since the number of minority members is small in some socioeconomic categories, particularly those at the upper end of the socioeconomic spectrum, we focus attention on three sets of 20 SMSAs that have the largest numbers of blacks, Hispanics, and Asians ... Focusing on the top 20 SMSAs for each group maximizes the number of minority members within each socioeconomic category and increases the stability of the segregation indices." (Denton and Massey 1988:799-800)

> "Since dissimilarity indices become unreliable and difficult to interpret when the number of minority members is very small (Massey 1978), we only compute figures for those metropolitan areas where the minority population reached 5000." (Massey and Fischer (1999:318)

I could easily introduce more examples. But by now the main point should be clear; empirical studies of segregation routinely adopt restrictions on study designs to avoid situations where index bias can complicate interpretations and comparisons of conventional index scores. In the absence of better alternatives, the conventional practice for dealing with the issue of index bias can be justified as a necessary correction to the earlier practice of ignoring the issue. At the same time, however, it is important to recognize that this strategy for dealing with the problem of index bias is less than ideal. Explicit discussion of the issue of index bias is not common. Instead, the practical implementation of the strategy relies on crude "rule-of-thumb" guidelines without rigorous demonstration that the design restrictions in fact reduce complications associated with index bias to acceptable levels without eliminating cases unnecessarily. In the ideal, studies would explicitly acknowledge concerns about index bias as in Massey and Denton (1992) and restrictions on study designs would then be guided by concrete goals for minimizing the potential complications of bias (e.g., exclude cases from analysis when E[D] > 2). In addition, studies would re-examine prevailing practices that can in some situations promote unjustified confidence that index bias is negligible.[2] This is not the current practice.

In sum, contemporary segregation studies routinely adopt a variety of "rule-of-thumb" practices to deal with concerns relating to index bias. These practices have had a pervasive, but largely unappreciated, adverse influence on the literature; they have caused researchers to restrict the scope of segregation studies and to limit the kinds of questions they seek to answer.

**Why Previous Efforts to Deal with Index Bias Have Not Gained Wide Acceptance**

A variety of technical solutions for dealing with index bias have been proposed over the years but none have gained wide acceptance in empirical research. Most proposed solutions have focused on strategies for adjusting or evaluating index scores in relation to their expected values under a baseline model of random distribution (e.g., Cortes, Falk, and Cohen 1976; Winship 1977; Farley and Johnson 1985; Carrington and Troske 1997). Winship (1977) and Carrington and Troske (1997) have proposed a "norming" adjustment that has intuitive appeal.[3] Using the example of D, the normed score D* is obtained from the calculation $D^* = (D-E[D])/(1-E[D])$ and registers the extent to which observed departure from uneven distribution (D) exceeds the departure from uneven distribution expected under a baseline model of random distribution (E[D]). In principle this adjustment can be applied to any conventional index of uneven distribution. In addition, if desired, it can be applied based on estimating E[D] from bootstrapping or related computation-intensive methods that require fewer formal assumptions.

Unfortunately, there are significant obstacles to wide adoption of this procedure. To start with, the interpretation of D* is more technical and abstract than the interpretation of D. This negates one of the appealing aspects of D; namely, the ease with which its interpretation can be conveyed to broad audiences as well as professional audiences. More importantly, researchers view the task of obtaining E[D] values for all segregation comparisons considered as impractical because rigorous estimates of E[D] require burdensome, computation-intensive calculations which need to be performed separately for all measures considered.[4]

Finally, closer consideration of this approach reveals that D* does not perform well in circumstances that are fairly common in empirical segregation studies. The issue is this. The techniques for calculating E[D] outlined in Winship (1977) and Carrington and Troske (1997) can perform well. But this is only certain under certain narrow conditions; namely, when the city population consists only of the two groups in the comparison and areas are uniform in population

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[2] Focusing on counts of individuals rather than households inflates the apparent the number of independent observations (Winship 1977: footnote 1) by a factor of 2-4. Similarly, using population counts reported in census tabulations based on sample data, as is common in studies of segregation by income and other social characteristics, inflates the apparent number of observations on which segregation scores are based by a factor of 5-7. In many situations these practices produce a false sense of confidence that index bias under random assignment is negligible.

[3] Allen and colleagues (2009) suggest a similar strategy.

[4] The calculations for E[D] under a relatively simple binomial model (per Winship [1977:1061-1063]) are non-trivial. The calculations under more complex models such as those suggested by Cortese, Falk, and Cohen (1976) are even more demanding.

size. The techniques can perform poorly when the city population includes other groups or when areas vary in population size. These are common circumstances.

The complication resulting from variation in area population size can in principle be addressed either by using more complex formulas for calculating E[D] or by using bootstrap simulation methods to establish values of E[D]. Either approach imposes burdens on researchers that make wide adoption of the practice unlikely.

The complications associated with the presence of other groups in the population are much more difficult to address. The basic problem is that values of E[D], which are needed to calculate D*, are not necessarily correct when other groups are present in non-negligible numbers. Consequently, the resulting values of D* can be misleading, possibly more so than unadjusted values of D. The underlying problem is that the value of effective neighborhood size (ENS), a key factor in calculating E[D], is uncertain when other groups are present in the population. Indeed, the value of E[D] can vary dramatically depending on the joint combination of two factors: the overall representation of other groups in the city population and the extent to which the two groups in the segregation comparison co-reside with (i.e., are integrated with) other groups in the population. Regarding the first factor, the practical significance of the issue is minimal when other groups represent only a small share of the city population. But in the current era of growing ethnic diversity in metropolitan areas, this cannot be routinely assumed.

Regarding the second factor, I discuss two hypothetical limiting cases to highlight the nature of the problem. At one extreme, the other groups in the population are distributed evenly (or randomly) in relation to the two groups in the segregation comparison. In this simple situation the value of ENS can be obtained by dividing the sum of the city-wide counts of the two groups in the comparison ($N_T = N_1 + N_2$) by the number of areas (T) per Winship (1977) and Carrington and Troske (1997). Thus, ENS is given by $N_T/T$ and values of E[D] based on this will be reasonable. At the other extreme, the two groups in the segregation comparison are completely segregated from all other groups in the population. In this case, ENS calculated by the method just described will be too low. The reason for this is that the two groups in the comparison are no longer distributed evenly over all areas; they are under-represented – in this extreme example absent – in areas where other groups are present and they are over-represented in other areas. ENS based on $N_T/T$ will then be too low. This will in turn cause values of E[D] to be too high and thus values of D* to be too low because the adjustment to remove the impact of bias will be too aggressive. Of course, the usual situation will be the intermediate case where other groups are partially, but not completely, segregated from the two groups in the segregation comparison.

These issues are not generally appreciated. But they cannot be safely neglected. To make this point more rigorously, I demonstrate its potential significance in more detailed analyses I present in later sections of this paper. In these analyses I use simulation methods to "exercise" indices to reveal the nature and potential magnitude of this particular problem. For the moment, I highlight the relevance of the issue by noting that calculations of ENS and E[D] are likely to introduce greater error in D* for comparisons of white-black segregation for high-income households than for white-black segregation overall. The reason is that high-income households are substantially segregated from other households. Thus, high-income households are not distributed evenly across areas is implicitly assumed in conventional calculations of ENS and E[D]. Instead, they are disproportionately concentrated in higher-income areas. Conventional calculations of ENS will thus be too low. This will lead to over-estimation of E[D] and, in turn, under-estimation of D* (i.e., overcorrection for bias).

The irony of the situation is that the segregation comparison of white and black high-income households is clearly more likely to be subject to substantial bias. It involves smaller counts (especially if nominal counts are deflated to take account of sampling, as they should be) and also the proportion black is likely to be lower among high income households than in the full population. Both promote a higher level of bias. Yet correction for bias using methods similar to those suggested by Winship (1977) and Carrington and Troske (1997) may be prone to greater error in this situation. Dealing with this situation requires that more complex strategies for obtaining values of ENS and E[D]. One option is to use bootstrap simulation methods to obtain expected values of D under random assignment based on the *observed* distribution of $n_{Ti}$ (where $n_{Ti} = n_{1i} + n_{2i}$) over areas of the city. However, the complexity and added practical burdens of adopting this approach make it unlikely to be widely adopted.

*Issues of Statistical Significance: A Brief Aside*

Expected values of segregation indices under random distribution have been a focus of attention for a purpose different from that of correcting for index bias; namely, establishing the statistical significance of index scores in relation to expected index values under a null model of random assignment. Ransom (2000) and Allen, Burgess, and Windmeijer (2009) have suggested model-based approaches for establishing standard errors for index values. Blau (1977) and Carrington and Troske (1995) have used chi-square tests to test deviation of observed distributions from distributions expected under random assignment. Taeuber and Taeuber (1965), Massey (1978), Farley and Johnson (1985), and Boisso, Hayes, Hirshberg, and Silber (1994), have either used or suggested the use of data-based, computation intensive methods such as bootstrap sampling techniques to aid in establishing sampling distributions of index scores. Allen, Burgess, and Windmeijer (2009) have suggested a combination of model-based and data-based approaches.

To date these approaches have not been widely used in empirical studies because their application is computationally burdensome. But the common finding when they are used is that moderate-to-high index scores are usually, but not necessarily always, statistically significant at conventional levels (e.g., at $p < 0.05$) It is important to recognize, however, that this finding, is relatively narrow in key respects. Certainly, it is useful to know whether a given observed index score is statistically different from segregation expected under a null hypothesis of random assignment (e.g., E[D]) or from zero or any other value of interest. But it does not eliminate the problems that bias presents for interpreting and comparing index scores when expected values under random distribution vary across cases as is typical. Thus, one is still left with the problem of making sense of what the value of an index score reveals about the role of race (or other social characteristics) in segregation and the similar problem of making sense of how this varies over time, across cities, or across groups based on comparing index scores that are subject to differing amounts of bias.

## The Surprisingly Simple Source of Bias and How it Can Be Eliminated

I pursue a different approach to solving the problem of bias in conventional segregation indices. Instead of using complicated techniques to adjust, test, or otherwise work directly with biased scores of conventional indices, I identify new versions of popular segregation indices that are free of bias. I obtain these new versions by introducing revised computing formulas that eliminate the root source of bias in the conventional formulations of the indices.

The key elements of the strategy I introduce here can be summarized as follows. First, I note that all popular indices of uneven distribution can be formulated as simple differences of group means on scores based on group exposure ($p_i$) calculated from area population counts. Next, I note that group exposure ($p_i$) calculated from area population counts is biased in a simple, obvious way; the exposure calculations for individual households include the household itself. I then note that the exposure means are biased in opposing directions for the two groups in the comparison and the resulting difference produces the bias in the index score. This traces the root source of index bias to a surprisingly simple source. Based on knowledge of this, it is possible to eliminate index bias by modifying the exposure calculation by basing them on counts for neighbors – that is, excluding the focal household from the counts used to calculate $p_i$ – instead of basing them on counts for area population. This simple modification eliminates the bias in the group-specific means based on exposure scores. This in turn eliminates bias from indices of uneven distribution when they are calculated from the difference of group means on unbiased scaled exposure. Accordingly, indices of uneven distribution computed using this relatively simple refinement take an expected value of zero under random assignment.

I first discovered this measurement strategy when examining a segregation measure used by Laurie and Jaggi (2003). These researchers used an agent simulation model to investigate model-generated segregation patterns assessed at an extremely small spatial scale – a von Neumann or "rook's" neighborhood which is defined based on the 4 neighbors who share sides with a household in a housing lattice or housing grid. Ordinarily, segregation assessed at this fine-grained spatial resolution would be subject to extreme, pathological levels of index bias. But Laurie and Jaggi (2003) measured segregation using a novel index of their own construction which they termed the "ensemble averaged, von Neumann segregation coefficient". They designated the index as "S" and they claimed, correctly, that it was unbiased under random distribution; that is E[S] = 0.

In examining S more closely, I found that it can be understood as being essentially equivalent to Bell's (1954) revised index of isolation ($I_R$). Bell's $I_R$ expresses a group's average same-group exposure ($P_{XX}$) as a proportion of its logical range (i.e., as $[P_{XX}-P_X]/[1-P_X]$) where $P_{XX} = (1/N_X)\cdot\Sigma(x_i\cdot p_i)$, $P_X = (N_X/T)$, T is the total population, $N_X$ is the group population, $x_i$ is the count of the group in area i, and $p_i$ is the group proportion or "exposure". However, where Bell's measure is subject to substantial positive bias when implemented at small spatial scales, S is unbiased. What distinguished S from Bell's $I_R$ is one simple, but ultimately very important thing; Laurie and Jaggi did not include the focal household in the counts used in calculating exposure outcomes for households. That is, Laurie and Jaggi calculated same-group exposure ($p_i$) based on counts for *neighbors* (i.e., $p_i = (x_i-1)/(t_i-1)$). In contrast, Bell's formulation, like all conventional formulations of popular indices of exposure and uneven distribution used in sociological studies of segregation, calculates exposure based on counts for *area population* which includes the focal household in both the numerator and the denominator of the calculation (i.e., $p_i = x_i/t_i$). The difference may seem simple, even trivial; but in fact it often has important consequences in many research situations.

It was not immediately obvious that the interesting result for the Laurie and Jaggi measure would be relevant for standard segregation studies. Segregation studies typically measure segregation based on data for "bounded areas"; that is, using population counts for a relatively small number of mutually exclusive, non-overlapping areas with fixed boundaries. But Laurie and Jaggi calculated their measure using data for "site-centered" areas – that is, using over-lapping areas defined separately for each household based on its particular spatial location – a practice common in agent models of spatial dynamics. Two methodological discoveries indicated that the Laurie and Jaggi measure had broader significance beyond the stylized application in agent models of segregation. The first was that the same desirable results obtained for the Laurie and Jaggi measure when I applied their computational approach to small, non-overlapping bounded areas of 2x2 units and 3x3 units. In this circumstance, the conventional version of $I_R$ took high average values under random assignment while the Laurie and Jaggi version took average values of zero. The second discovery was that all conventional segregation indices can be expressed in terms of household-level calculation formulas that make it possible to compute them using the site-centered areas commonly used in agent simulations.[5]

In light of this, I directed attention to the question of whether the Laurie and Jaggi measurement strategy could be adapted to broader application. My first step in this direction was to draw on the well known fact that Bell's $I_R$ is equivalent to the variance ratio (V) in the special case where the population consists of only two groups (James and Taeuber 1985; Stearns and Logan 1986; White 1986). V of course is a popular measure of uneven distribution known also as eta squared (Duncan and Duncan 1955) Zoloth's S (Zoloth 1976), and Coleman's $r_{ij}$ (Coleman, Kelly, and Moore 1975; Coleman, Hoffer, and Kilgore 1982). So, *for the special two-group situation only*, it was reasonable to describe Laurie and Jaggi's S as an unbiased index of uneven distribution.

This discovery prompted me to explore whether it was possible to similarly eliminate bias in the general formulation of V by adopting the Laurie and Jaggi strategy of performing segregation calculations based on counts for neighbors instead of counts for area population. This ultimately

[5] Based on this finding, the criticism that popular indices such as G, D, A, R, H, and V are "aspatial" is not strictly correct. The indices are indeed aspatial when computed using data for bounded areas; any index is. But they can be implemented as spatial indices by using household-level computing formulas based on data for over-lapping, household-specific, site-centered areas as noted in Fossett and Warren (2005).

proved to be the case, but discovering a viable implementation required thinking about V in a new way. The next key step was to formulate V in terms of group-specific exposure calculations. I pursued this approach based on realizing that bias in the conventional formulation of $I_R$ arises from the fact that including the focal household in exposure calculations has differential effects on the exposure outcomes for the two groups in the comparison. I found that this new formulation could be achieved by casting V as a simple, arithmetic difference of group means on exposure; specifically, $V = P_{XX} - P_{YX}$, where $P_{XX} = (1/N_X)\cdot\Sigma(x_i\cdot p_i)$, $P_{YX} = (1/N_Y)\cdot\Sigma(y_i\cdot p_i)$, $p_i = x_i/t_i$, and $t_i = (x_i+y_i)$.[6] Significantly, this formulation of V is mathematically equivalent to more well known formulations such as $V = 1/NPQ\cdot\Sigma t_i(p_i-P)^2$ (James and Taeuber 1985:6) which draws on (pair-wise) area population counts ($t_i = x_i+y_i$), (pair-wise) area group proportion ($p_i = x_i/(x_i+y_i)$), and (pair-wise) city proportions ($P=N_X/T$, $Q=N_Y/T$, and $T=N_X+N_Y$). I derived the equivalence independently. Later I found that an alternative derivation of this equivalence had been reported earlier in an interesting but largely overlooked methodological paper by Becker, McPartland, and Thomas (1978).

I then investigated the impact of applying Laurie and Jaggi's strategy of calculating exposure ($p_i$) based on counts for neighbors instead of counts for area population. I discovered the following: exposure indices $P_{XX}$ and $P_{YX}$ computed using *area population* counts are biased, but exposure indices $P'_{XX}$ and $P'_{YX}$ computed using counts for *neighbors* are unbiased. Consequently, V based on area population counts (i.e., $V = P_{XX} - P_{YX}$) is biased and V' based on counts for neighbors (i.e., $V' = P'_{XX} - P'_{YX}$) is unbiased. Specifically, using area population counts, $E[P_{XX}] > P_X$; $E[P_{YX}] < P_X$, and $E[V] = E[P_{XX}-P_{YX}] > 0$. In contrast, using counts for neighbors, $E[P'_{XX}] = E[P'_{YX}] = P_X$, and $E[V'] = E[P'_{XX}-P'_{YX}] = 0$.

The source of bias in the conventional formulation of V is especially easy to highlight in this formulation. Consider the familiar example of white-black segregation and, for simplicity, assume the city is not unusually small, consists of only whites and blacks, and that area population ($t_i$) is constant for all areas.[7] Next assume that households are distributed randomly across areas. For any household, white or black, the expected exposure to whites ($p_i$) based on counts for *neighbors* is unbiased and is equal to proportion white in the city (i.e., $E[P'_{WW}] = E[P'_{BW}] = P_W = W/(W+B)$).[8] In contrast, the expected exposure to whites ($p_i$) based on counts for area population is biased because the exposure score for each household is a weighted average of two components; the household's exposure based on area neighbors, which has an expected value

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[6] As is the case for all indices of uneven distribution, the calculations draw only on counts for the two groups in the comparison. So "exposure" ($p_i$) here should be understood as "pair-wise" exposure in contrast to "overall" exposure as calculated in Bell's isolation and exposure indices. These are identical in the two-group case but may differ otherwise.

[7] The assumption that the city is not small simplifies things by making the individual household's impact on the city-wide values for $P_X$ and $P_Y$ negligible. Thus, bias in both groups' expected exposure can be assessed in relation to a single value (i.e., $P_X$ or $P_Y$). When the city population is small, bias in expected exposure should be assessed in relation to group-specific versions of $P'_X$ and $P'_Y$.

[8] If the combined count of the two groups is small (e.g., < 500), the focal household may have a non-negligible impact on the value of P for the city as a whole. This can usually be ignored in situations where the combined city-wide count of the two groups in the comparison is sufficiently large (e.g., 500 or higher).

equal to city-wide proportion white ($P_W = W/(W+B)$)), and the household's exposure to itself which of course is fixed as either white or black and does not vary under random assignment. Thus, for whites expected contact with whites ($E[P_{WW}]$) can be given by $E[P_{WW}] = ((t_i - 1)/t_i) \cdot E[P'_{WW}] + (1/t_i) \cdot 1$. The first term is not affected by bias because $E[P'_{WW}] = \cdot P_W$ but the second term is a fixed source of upward bias. Accordingly, the resulting sum ($E[P_{WW}]$) is biased upward with the degree of bias being greater when area population ($t_i$) is small. Similarly, for blacks expected contact with whites ($E[P_{BW}]$) is given by $E[P_{BW}] = ((t_i - 1)/t_i) \cdot E[P'_{BW}] + (1/t_i) \cdot 0$. The first term is unbiased and the second term is biased downward. So the sum ($E[P_{BW}]$) is biased downward.

Based on this, it is clear that V is biased upward since $E[V] = E[P_{WW}] - E[P_{BW}]$. The following sequence shows that, as previously noted by Winship (1977:1064), amount of bias in V will be $1/t_i$.

$$E[V] = E[P_{WW}] - E[P_{BW}]$$

$$E[V] = [((t_i - 1)/t_i) \cdot E[P'_{WW}] + (1/t_i) \cdot 1] - [((t_i - 1)/t_i) \cdot E[P'_{BW}] + (1/t_i) \cdot 0]$$

$$E[V] = [((t_i - 1)/t_i) \cdot E[P'_{WW}] - [((t_i - 1)/t_i) \cdot E[P'_{BW}] + [(1/t_i) \cdot 1] - (1/t_i) \cdot 0]$$

$$E[V] = [((t_i - 1)/t_i) \cdot P_W - ((t_i - 1)/t_i) \cdot P_W] + [(1/t_i) \cdot 1] - (1/t_i) \cdot 0]$$

$$E[V] = (1/t_i) \cdot 1 - (1/t_i) \cdot 0$$

$$E[V] = 1/t_i$$

The discussion here adds two points not previously noted in methodological studies of bias in segregation indices. One is that it shows how the bias in the conventional formulation of V can be understood as a consequence of computing $p_i$ for households based on area population. The second is that it reveals that an unbiased version of V (i.e., V' where $E[V'] = 0$) can be obtained by computing $p_i$ for households based on neighbors as the following sequence shows.

$$E[V'] = E[P'_{WW}] - E[P'_{BW}]$$

$$E[V'] = P_W - P_W$$

$$E[V'] = 0$$

V is a convenient first example to review because the mathematics of bias for it are relatively simple and straightforward. But V is not a special case. The key elements of the approach used here to identify the source of bias in V and point the way to a viable strategy for eliminating it can be applied to all popular indices of uneven distribution. That is, while it is not generally appreciated, all popular indices of uneven distribution can be expressed as a simple, arithmetic difference of means on group exposure scores. Once they are examined in this formulation, it is straightforward to establish that index bias traces to bias that inherent is in the group-specific means on exposure scores. That bias traces to the fact that exposure scores calculated from area population counts reflect the weighted average of two components; an unbiased component registering exposure calculated from counts of neighbors and a group-specific bias component registering households' exposure to themselves. Eliminating the latter component from the exposure calculations yields unbiased group means on exposure scores and the difference of the unbiased group means yields an unbiased index score. More succinctly, the following two conclusions apply to all popular indices of uneven distribution (including G, D, A, R, and H) and also to indices of isolation and exposure: bias in the conventional index formulation traces to calculating group exposure ($p_i$) for households based on area population counts and an unbiased version of the index can be obtained by calculating group exposure based on counts for neighbors.

I now briefly review how these conclusions generalize and apply to the index of dissimilarity (D), the most popular and widely used index of uneven distribution, and also to the closely related and technically superior gini index (G). Later I similarly review how the conclusions apply to the less widely used Hutchens index (R) and the Theil index (H). To begin I note that, while it not generally recognized, D and G are similar to V in that they can be understood as reflecting simple differences of group means on scores based on exposure ($p_i$) calculations for households. However, D and G register group means on exposure in a different metric than V. To facilitate discussion of the similarities and differences among indices on these points, it is useful to examine them within the following framework. Each index can be understood as registering group exposure scored on the basis of a scaling function $y_i = f(p_i)$ where $y_i$ is a scaled exposure score and $f(p_i)$ is the function that scores $y_i$ on the basis of the value of $p_i$. For V, the scaling function is the identity function (i.e., $y_i = f(p_i) = p_i$). For D, the scaling function $f(p_i)$ is different but it is still relatively simple. Specifically, $f(p_i)$ for D is a step function wherein $y_i$ is scored 0 when $p_i \leq P$ and 1 when $p_i > P$. D for white-black segregation, then, reflects the white-black difference of means on scaled exposure to whites ($y_i$) scored by $f(p_i)$ for D; that is, $D = Y_W - Y_B$ where Y denotes the group mean on (scaled) exposure to whites and the subscript denotes the group.

In the case of D, the group means ($Y_W$ and $Y_B$) register the proportion in the group where exposure to whites ($p_i$) exceeds the city-wide value (P). Thus, the value of D also can be described as the white-black difference on the proportion of each group residing in areas where exposure to whites ($p_i$) exceeds the value for the city (P). This easy-to-grasp interpretation of D is rarely mentioned in methodological and empirical studies. But it is not unknown as at least one previous discussion of this interpretation can be found in Becker, McPartland, and Thomas (1978:349) who also show that this interpretation can be depicted graphically in the context of the segregation curve (1978:353).

This formulation of D makes it clear how bias in D traces to bias in the group means for whites and blacks on (scaled) exposure. Understanding this also makes it clear that *bias in D can be eliminated by the simple adjustment of calculating exposure based on counts of neighbors instead of counts for area population*. When households are randomly assigned to areas, the expected distribution of raw exposure ($p'_i$) calculated using counts for *neighbors* is the same for both whites and blacks. As a result, expected values for group means on scaled exposure ($y'_i$) scored based on *any* scaling of "raw" exposure ($p'_i$) will be the same for whites and blacks (i.e., $E[Y'_W] = E[Y'_B]$). The binomial model provides the expected distribution of $p'_i$ and from it the distribution and expected means for $y'_i$ as scored from $f(p'_i)$. For example, if area size ($t_i$) is 20 and city proportion white (P) is 0.90, the probability of $p'_i$ exceeding P in a random draw of 19 neighbors is 0.4203. Thus, $E[Y'_W] = E[Y'_B] = 0.4203$ and $E[D'] = 0$.

When exposure is calculated from area population, bias enters the picture because now $E[Y_W] \neq E[Y_B]$. The basis for this is straightforward. The expected distribution of whites among 19 random *neighbors* remains the same for both whites and blacks. But area population includes one additional household – namely, the household itself – which of course is always white for white households and always black for black households. This makes it more likely for white households than black households that raw exposure ($p_i$) calculated from *area population* will exceed 0.9 causing scaled exposure ($y_i$) to be scored 1 instead of 0. How much more likely? In the present example, $y_i$ is scored 1 when $p_i > 0.90$ which occurs when 19 or more of the 20 households in the area are white. Under the binomial, the probability that exactly 18 of 19 neighbors will be white is 0.2852 and the probability that exactly 19 of 19 neighbors will be white is 0.1351. Based on these probabilities, the expected proportion of households scored 1 on $y_i$ will be at least 0.1351 for both whites and blacks. For blacks, this does not change when the focal household is added to the distribution because the additional household is always black. Thus, $E[Y_B] = 0.1351$. For whites, the addition of the focal household always increases the count of whites by 1 and households who have 18 white *neighbors* will then have 19 white households in their area population. As a result, the proportion of white households scored 1 on y will be 0.4203 (i.e., the sum of 0.1351 and 0.2852, the expected proportions of households with 19 and 18 white neighbors, respectively). Thus, $E[Y_W]$ is 0.4203 and $E[D] = 0.2852$ based on the expected difference of means $E[Y_W] - E[Y_B]) = 0.4203 – 0.1351$. Note that this value is identical to the value obtained using the formula for calculating E[D] given in Winship (1977:1063).

In sum, when D is formulated as a difference of group means on exposure to whites, it becomes clear that calculating D based on counts for area population is intrinsically biased (i.e., $E[D] > 0$) because counting households as exposed to themselves biases the mean on exposure up for whites and down for blacks. In contrast, calculating D based on counts for neighbor is unbiased (i.e., $E[D'] = 0$) because in the long run random draws of neighbors are the same for whites and blacks.

Like D and V, the gini index (G), also can be formulated as a simple arithmetic difference of group means on (scaled) exposure. In the case of G the scaling function $y_i = f(p_i)$ is the percentile scoring of raw exposure ($p_i$). That is, individual households are assigned scores for scaled exposure to whites ($y_i$) based on the percentile scoring of the value of $p_i$ for the area they reside in. G for white-black segregation can then be obtained from $G = 2(Y_W - Y_B)$ where Y denotes the group mean on (scaled) exposure to whites and the subscript denotes the group. Multiplication by 2 is involved because, based on the nature of percentile scoring, the logical range for $Y_W - Y_B$ is 0.5 instead of 1.0. In substantive terms, the value of G thus is equal to the white-black difference on exposure to whites scored on the basis of area rank position on proportion white ($p_i$).

The fact that G assesses group differences in average area rank on $p_i$ is rarely noted in methodological discussions of segregation measurement. But this quality of G has been noted in the literature on measurement of inter-group inequality. Lieberson alludes to it when he describes G as "analogous" to the index of net difference (ND), a measure explicitly interpreted as an index of rank-order inequality between groups (1976:281). In fact, G and ND are mathematically equivalent. This has been previously noted in Fossett and South (1983:860-861). Fossett and Siebert (1997: Appendix A) discuss this in greater detail and show that G can be obtained as a simple difference of group means on percentile scores and that both ND and G are equivalent to Somers' $d_{yx}$, a well known measure of ordinal association.[9]

As with D, viewing G as registering a group difference of means on scaled exposure helps clarify the source of bias in G under random distribution. The example just reviewed for D established that the expected distribution on raw exposure ($p'_i$) is the same for whites and blacks when $p'_i$ is computed from counts of *neighbors*. As a result, the expected means for whites and blacks on scaled exposure to whites ($y'_i$) based on percentile scoring of $p'_i$ is the same for whites and blacks (i.e., $E[Y'_W] = E[Y'_B] = 0.50$) and the expected value of G under random distribution is unbiased (i.e., $E[G'] = 2(E[Y'_W]-E[Y'_B]) = 0$). In contrast, when raw exposure ($p_i$) for households is calculated in the conventional way using counts for area population, the expected average for rank position on exposure to whites ($y_i$) is biased up for whites and biased down for blacks for obvious reasons. Thus, applying a binomial model for the example just reviewed above, $E[Y_W] = 0.5204$, $E[Y_B] = 0.3164$, $E[Y_W-Y_B] = 0.2040$, and $E[G] = 0.4080$. Note that E[G] is even higher than E[D] for a simple reason; G registers all differences between whites and blacks on $p_i$ where D registers only differences in the proportion of each group residing in areas where $p_i > P$.

In sum, formulating G as a difference of group means on exposure to whites reveals why calculating G in the conventional way (i.e., based on counts for area population) involves intrinsic upward bias (i.e., $E[G] > 0$) under random distribution. The by now familiar reason is that the conventional calculations treats households as exposed to themselves and this biases the group mean on (scaled) exposure to whites up for whites and down for blacks. In contrast, means on (scaled) exposure based on counts for neighbors are not biased and thus G based on this difference of group means on (scaled) exposure is unbiased (i.e., $E[Y'_W] = E[Y'_B] = 50$, so $= E[G'] = 2(E[Y_W]-E[Y_B]) = 0$).

Without going into as much detail as in the discussions, I now note that the same result applies to the two other measures of uneven distribution I examine in this paper, Hutchens' square root index (R) and the Theil entropy index (H).[10] As with V, D, and G, R and H can both be expressed as a simple difference of group means on (scaled) contact with whites. The relevant scaling functions (i.e., $y_i = f(p_i)$) for these indices are more complex and less intuitive than the corresponding scaling functions for V, D, and G.[11] I have reviewed the derivation of their

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[9] In the application to segregation, $d_{yx}$ assesses the individual-level ordinal association between race coded 0 for black and 1 for white and rank on area proportion white ($p_i$).

[10] R has close mathematical relations to Atkinson's index (A); specifically, for the symmetric version of A (i.e., for A with tuning parameter 0.5) $R = 1-\sqrt{A}$ and $A = 2\cdot(R-R^2)$. R is mathematically more tractable than A. A practical consequence of this is that I was able to express R as a difference of group means on scaled contact. I have not been able to do this for A.

[11] For the Hutchens index (R), $R = Y_W-Y_B$ where $y_i = 0.5$ when $p_i$ is *exactly* equal to P. Otherwise, $y_i = Q + (1$

formulas, as well as those for V, D, and G, in more detail elsewhere (Fossett 2008). Here I briefly note that in the cases of R and H the scaled exposure score ($y_i$) is a continuous, ever-rising, non-linear function of raw exposure ($p_i$). When graphed, the non-linearity of the y-p relationships yields a continuously increasing, forward-leaning "S" rising from (0,0) to (1,0). For any particular group comparison, the S-curve for R is more pronounced than that for H in the sense that it departs further from the linear y-p relationship for V.

By now the points that follow should be familiar and straightforward. Under random distribution, the expected distribution of raw exposure ($p'_i$) calculated using counts of neighbors is the same for whites and blacks. Accordingly, the expected distributions of scores on (scaled) exposure to whites ($y'_i$) are identical for whites and blacks and so the expected mean for (scaled) exposure to whites is the same for both whites and blacks (i.e., $E[Y'_W] = E[Y'_B]$). For the example introduced above (with 19 neighbors), the expected (scaled) exposure means relevant for R' are $E[Y'_W] = E[Y'_B] = 0.5644$ and the expected (scaled) exposure means relevant for of H' are $E[Y'_W] = E[Y'_B] = 0.7388$. As a result, $E[R'] = E[Y'_W]-E[Y'_B] = 0$ and $E[H'] = E[Y'_W]-E[Y'_B] = 0$.

As reviewed previously for V, D, and G, the results are quite different when raw exposure ($p_i$) is calculated using counts for area population. Upward bias in raw exposure ($p_i$) for whites translates into upward bias in scaled exposure ($y_i$) for whites and downward bias in raw exposure ($p_i$) for blacks translates into downward bias in scaled exposure ($y_i$) for blacks. As a result, the expected distributions of scaled exposure to whites ($y_i$) shift up for whites and down for blacks leading the expected group means on scaled exposure to differ. In the example where area population is 20, 19 neighbors plus the household itself, the results for R are $E[Y_W] = 0.5705$, $E[Y_B] = 0.4614$, and thus $E[R] = 0.1091$. In the case of H, $E[Y_W] = 0.7457$, $E[Y_B] = 0.6579$, and thus $E[H] = 0.0878$. In sum, formulating R and H as differences of group means on (scaled) exposure to whites reveals that: calculating these indices in the conventional way based on counts for area population introduces intrinsic upward bias, and this bias can be eliminated by simply excluding the focal household and calculating the indices based on counts for neighbors.

Table 1 provides a summary of the various results for the example binomial-based calculations reviewed in the above discussion. For each of the five indices of uneven distribution examined − G, D, R, H, and V – the table presents the expected group means on scaled contact with whites, and the expected value of the index based on the group difference of means. These results are reported separately for the case where $p_i$ for households is calculated in the conventional way based on area population counts and for the case where $p_i$ for households is calculated using counts for neighbors. The values of the individual terms have already been introduced so I limit my comments to a few general patterns. One important pattern is that, when calculations are based on counts for neighbors, the expected group means on scaled exposure to whites are the same for whites and blacks and thus the expected index score is always 0. Another important pattern is that, when calculations are based on area population, the expected mean for

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$- \sqrt{[p_i q_i/PQ]}\ ) / [(p_i/P) - (q_i/Q)]$. For the Theil index (H), $H = Y_W - Y_B$ where $y_i = Q + 1/([p_i/P - q_i/Q] \cdot [(E - e_i)/E])$ where $e_i$ and E are entropy calculations for area and city as outlined in James and Taeuber (1985). When $p_i$ is *exactly* equal to P, $y_i$ must be obtained by taking limiting values of $y_i$ based on setting $p_i$ arbitrarily close to P. See Fossett scaled exposure to whites is biased up for whites and biased down for blacks and this produces upward bias in the index score. Another pattern is that the magnitude of the bias in conventional index scores varies by index. It is highest for G and D and lowest for H and V. A full, detailed discussion of this pattern is beyond the scope of this paper.[12] But the essence of the matter is this; the more dramatic the non-linearity in the $y_i = f(p_i)$ relationship, the greater the bias in conventional index score, especially when P is near the extremes of 0 and 100. Given this observation, I now note that the various indices also are listed in Table 1 in rank order of the degree of non-linearity in the $y_i = f(p_i)$ relationship involved.

—— Table 1 About Here ——

This last point warrants brief additional comment. The variance ratio (V) is unique among indices of uneven distribution in that it registers exposure based on the identify function $y_i = p_i$. As a result, plotting values of $y_i$ by values of $p_i$ produces a diagonal line rising from (0,0) to (1,1). For all of the other indices, the $y_i$-$p_i$ relationship is non-linear and, accordingly, the plot of $y_i$ by $p_i$, while always starting at (0,0) and ending at (1,1), departs substantially from the diagonal with values of $y_i$ being either markedly lower or higher than values of $p_i$ over some ranges of $p_i$. This has relatively straightforward substantive implications. V registers differences in "simple" (unscaled) exposure equally over all levels of exposure (i.e., over the full range of $p_i$).[13] Thus, an exchange of a white and black household between two neighborhoods that produces a change in the white-black difference in average exposure to whites of a given direction and amount will have the same impact on V regardless of the initial values of proportion white for the two neighborhoods. For other indices of uneven distribution, that is not the case. Instead, non-linearity in the $y_i$-$p_i$ scaling relationship dictates that the index will respond to the changes in group differences in simple (unscaled) exposure unequally depending on the initial values of proportion white in the two neighborhoods. For example, it is well known that D does not respond at all to exchanges involving households from two neighborhoods where proportion white is below the city average (both before and after the exchange). This can be understood here as resulting because exchanges that produce changes in simple exposure to whites ($p_i$) do not produce changes in scaled exposure to whites ($y_i$) because $y_i$ is scored 0 for all values of $p_i$ below P and thus scores for $y_i$ are unaffected by exchanges in that range.

### Documenting Bias in Conventional Versions of Indices of Uneven Distribution

I have used three different methods to verify that conventional indices of uneven distribution are biased and that revised versions of these indices based on the measurement strategy outlined in the previous section are unbiased. The three methods include: formal analysis using binomial probability models, empirical application in agent simulation

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(2008) for derivations of these expressions and alternative expressions.

[12] I provide an extended review elsewhere (Fossett 2008).

[13] That is, an exchange of a white and black household between two neighborhoods that produces a change in the white-black difference in average exposure to whites of a given direction and amount will have the same impact on V regardless of the initial values of proportion white for the two neighborhoods.

experiments, and analysis of bootstrap simulations using census data. Here I review selected results from agent simulation experiments because this approach provides good options for systematically "exercising" the various indices under a wide range of conditions. I used the SimSeg agent simulation model which has been described elsewhere (e.g., Fossett and Waren 2005; Fossett 2006; Fossett and Dietrich 2008; Clark and Fossett 2009). There is one notable difference; the version of SimSeg I use here is one that implements routines to calculate both conventional and unbiased versions of G, D, R, H, and V. I designed experiments that implemented a two-group city in which segregation is assessed using bounded neighborhoods of uniform size. The two groups in the simulation are of course "virtual", but for convenience of discussion I refer to them as whites and blacks. I varied the conditions of the experiments to exercise index behavior by varying the ethnic mix of the city randomly from 2-98% white separately in each experiment. I then ran 2,500 experiments separately for each of eight neighborhood sizes based on a square housing grid for the bounded area ranging from 3-10 houses on a side. The resulting neighborhood sizes were 9, 16, 25, 36, 49, 64, 81, and 100.

In each simulation experiment households are initially distributed randomly to housing units. Then the simulation implements a Schelling-style dynamic of ethnic congregation based on preferences to achieve minimum levels of co-ethnic contact at the neighborhood level. Household movement under this process proceeds for five cycles (i.e., time periods). The level of co-ethnic presence sought by households was varied randomly across simulations from zero to 90%. This produced considerable variation in the degree of segregation observed in the intermediate-state and end-state residential distributions of the simulation experiments.

My primary interest at this point in the paper is with index behavior under random distribution at the initialization of the city landscape. This is depicted in Figure 1 which provides six graphs, one for each of the following six indices: G, D, A, R, H, and V. Each graph plots the values of the relevant index score at the beginning of the simulation experiment (i.e., cycle 0) against percent white in the city population. The graphs plot observed segregation outcomes from simulations in which effective neighborhood size (ENS) is 9, 16, 25, 49, and 100.[14] In addition, each graph also plots a black line tracing the expected index score (e.g., E[D]) based on calculations using a binomial model per Winship (1977).

—— Figure 1 About Here ——

The results presented in Figure 1 are straightforward. All of the indices take values above zero in each one of the separate 12,500 simulation trials for which data points are plotted. The gray points indicate that index scores calculated from the random residential distributions at initialization vary in relatively narrow ranges around their expected values. The black lines show that the expected values of the indices vary systematically with effective neighborhood size and percent white in the city population. As noted earlier, the nature of the systematic variation in index scores is relatively simple in its main features. For all indices, scores for both the expected

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[14] Outcomes are plotted only for four sizes to facilitate graphical presentation. As one would expect, curves for

values under random assignment and the observed random segregation at initialization are systematically higher when effective neighborhood size (ENS) is lower. Thus, the highest curve is always for the lowest value of ENS (in this case 9) and the curve is systematically lower for each successively higher value of ENS. Also, for all indices except the variance ratio (V), both the expected values and the observed random outcomes are systematically higher when proportion white for the city (P) departs from balance at 0.50 and they become especially high when P falls below 0.10 or rises above 0.90.

The methodological literature has documented similar patterns of random variation for D many times before and also occasionally for G. But patterns of random variation for A, R, H, and V have not been reviewed often (in some cases perhaps not at all). Comparing the figures shows reveals several noteworthy differences between indices. One pattern is that indices vary considerably in the magnitude of bias under random assignment. The highest scores under random assignment are observed for G followed closely by D and then A. The lowest scores under random assignment are for V followed by H and R. This documents that D, the most popular and widely used index, consistently has higher expected and observed values under random assignment than all but one of the other indices. The one exception is G and it is worth noting that the reason D is lower than G is because D is in essence a crude version of G since the value of D can be understood as the value of G when G is calculated using only two categories of area proportion white ($p_i \leq P$ and $p_i > P$) instead of using all values of $p_i$ as is the case for the usual calculation of G.[15]

Another pattern is that, for any given index, bias is lower when effective neighborhood size (ENS) is larger. This provides at least some justification for two crude rules-of-thumb guiding the designs of contemporary segregation studies; examine segregation scores calculated using data for spatial units with larger population counts (e.g., use tracts over blocks) and avoid analysis of comparisons involving groups that are small in absolute population size (e.g., segregation of detailed income groups, Asian or Latino subgroups, etc.). Another pattern is that, for all indices except V, index bias is highest, sometimes alarmingly so, when group size is imbalanced. This provides justification for another crude rule-of-thumb restriction on research designs for contemporary segregation studies; that of avoiding analysis of comparisons for groups that are small in relative size.

At the same time, the graphs in Figure 1 reveal a few things that are not widely appreciated. One is that for most indices, and especially for G and D, effective neighborhood size (ENS) and group ratio (GR) interact such that index bias is far greater when ENS is low *and* GR is highly imbalanced. This has an unfortunate practical implication. It indicates that the standard rules-of-thumb for restricting research designs are imprecise and not necessarily reliable for their intended purpose because the rules are applied in a simple additive way that does not take this potentially

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intermediate values of ENS fall between the curves in the graph.

[15] The crude nature of D also accounts for the "lumpiness" in the curves for D as contrasted with the smooth curves for G. Lumpiness in curves for D results because D responds only to group differences in exposure to one particular value of area proportion white (i.e., $p_i > P$) and, unlike G, does not register group differences in exposure to whites above or below this value.

important interaction into account,. As a result, the conventional guidelines can exclude cases where bias may low enough to be viewed as negligible (e.g., E[•] < 2); particularly when using R, H, and V. And they can include cases where bias is high and problematic; particularly when using G, D, and A. In sum, not only do current practices based on concerns about bias greatly restrict the scope of segregation studies, they also are less reliable and effective for their intended purpose than researchers may often assume.

**Documenting the Attractive Behavior of Unbiased Indices of Uneven Distribution**

I now review the behavior of the new *unbiased* versions of popular indices of uneven distribution under random distribution at the initialization of the city landscape. This behavior is depicted in Figure 2 which provides five graphs, one each for G', D', R', H', and V'.[16] Each graph plots the values of the relevant index score at the beginning of the simulation experiment (i.e., cycle 0) against percent white in the city population. As with Figure 1, the individual graphs plot observed segregation outcomes from simulations in which effective neighborhood size (ENS) is variously set to 9, 16, 25, 49, and 100. A key difference here is that the expected value of each index (e.g., E[D']) is zero under calculations using a binomial model per Winship (1977). So the resulting curves for the indices all take the general form of a straight line centered on zero on the vertical axis. The figure reveals that index scores based on the 12,500 random residential distributions vary near symmetrically around zero and can be negative as well as positive. A negative score indicates that even distribution is *greater* than would be expected based on random distribution. The dispersion of scores around the expected value of zero gives intuitive insight into the sampling distribution of the scores for the unbiased versions of the different indices. Substantively, scores that fall within the center of the sampling distribution do not surprise. But scores above and below the range of the sampling distribution for an unbiased index suggest that a policy or social dynamic is promoting either greater segregation or greater integration, respectively, than would be expected based on chance.

—— Figure 2 About Here ——

The main differences across the five unbiased indices are seen in two areas: the general level of variation in their scores around their expected values of zero and the degree to which this variation differs by effective neighborhood size (ENS) and proportion white in the city (P). Figure 2 indicates that the scores for the unbiased indices are distributed symmetrically around zero at all levels of proportion white for the city as expected based on formal analysis. Second, the degree of variability in the distribution of scores around zero varies by index. To help highlight this pattern, points are plotted in successively darker shades of gray as ENS increases in size from 9 to 16 to 25 to 49 to 100. Figure 3 presents similar patterns using box plots to help

[16] There is no graph for A' because I have not discovered an unbiased formulation of this index because I have not found a way to express A as a difference of means on scaled scores (y) based on area proportion white (p). Researchers who find A appealing, should consider Hutchens R, a conceptually similar index with an available unbiased formulation.

highlight variation in central tendency and dispersion of scores for indices by relative group size and effective neighborhood size.

—— Figure 3 About Here ——

Table 2 reports means and standard deviations for index scores by ENS and category of city proportion white. The results reveal that random variation tends to be higher when ENS is small. This finding itself is not very surprising. But it is interesting that the extent of variation of random scores differs by index. Holding ENS constant, dispersion in unbiased scores is higher for G' and D' and lower for R', H', and V'. Dispersion in scores for the unbiased variance ratio (V') does vary with proportion white in the city (P). But it is unique in this regard. In general, G', D', R', and H' exhibit greater sampling variability when city proportion white departs from balance (i.e., 0.50) and particularly when it and approaches the extremes of 0.0 and 1.0.

—— Table 2 About Here ——

In sum, under random distribution, dispersion in scores of unbiased indices varies in magnitude depending on the index, the value of effective neighborhood size (ENS), and, with the lone exception of V', proportion white in the city (P). Furthermore, for G', D', R', and H', the ENS and P interact such that dispersion at low ENS is even greater when P departs further from balance and dispersion when P is high or low is greater when ENS is low. These patterns indicate that one must be mindful of these distinctive sampling distributions for different indices when evaluating the statistical significance of particular index scores. Formal equations for accurately describing standard errors of index scores under varying circumstances are not yet available. So, for now, bootstrapping and related computation-intensive approaches that require only minimal assumptions are probably the best available option for establishing whether particular non-zero index scores are different from zero at a given confidence level.

**Further Documenting the Desirable Behavior of Unbiased Indices**

I now review the behavior of conventional and unbiased versions of popular indices of uneven distribution in multi-group situations where “norming” adjustments proposed by Winship (1977) and Carrington and Troske (1997) can be problematic. The essence of the problem with norming adjustments is that index bias is more complicated in multi-group situations than is currently appreciated. Specifically, the expected value of conventional indices is not a constant as assumed by the logic of performing “norming” adjustments. Instead, the expected value of conventional indices is uncertain and can vary substantially. This undermines the effectiveness of norming adjustment procedures that have been proposed as means of dealing with the impact of bias on the scores of conventional indices.

I present results from simulation analyses to highlight the complex problems of bias in conventional indices. The simulations all involve three groups; one large minority group, and two smaller minority group. At the initialization of each simulation trial the households in the majority group are highly segregated from the households in the two minority groups but the households in the two minority groups are randomly distributed in relation to each other. This is depicted in the top graph in Figure 4.[17] The simulation is then run for ten cycles (i.e., time periods). During each cycle, 25% of households are chosen at random and are assigned randomly to a new residential location. Not surprisingly, systematic segregation between the majority group and the two minority groups quickly dissipates under this process of random movement resulting in majority households being randomly intermixed with minority households. This is depicted in the bottom graph in Figure 4. At all times, from initialization to conclusion, the households in the two minority groups are randomly distributed in relation to each other.

—— Figure 4 About Here ——

The simulation experiments I used to generate the results for the analysis here follow the basic design used in the simulations described earlier. The simulations here use the same neighborhood size (25) and the same city size and area configuration (i.e., 256 areas and 6,400 housing units). The ethnic composition of the city is set at 80-15-5. A total of 2,500 separate simulation experiments are run using this setting.

Index behavior is depicted in Figure 5 which provides four graphs, two on the top row for the unbiased formulation of the dissimilarity index (D') and two on the bottom row for the conventional formulation of the dissimilarity index (D). The graphs in the left column depict majority-minority segregation; the graphs in the right column depict minority-minority segregation. The box plots in the top left graph show how D' for the majority-minority comparison starts at very high levels and falls to zero as the ten cycles of random movement dissipate the initial segregation at the start of the simulation. The box plots in top right graph show that the distributions of D' for minority-minority segregation are always centered on zero as expected since households in the two minority groups are distributed randomly in relation to each other over the entire course of the simulation.

—— Figure 5 About Here ——

The box plots in the bottom left graph depict the distribution of scores for the conventional version of the index of dissimilarity (D) for majority-minority segregation. This shows that D is very high at the beginning of the experiment and then falls sharply as households move randomly for ten cycles. But D does not fall to zero due to the intrinsic bias in D. Thus, the final level of D essentially reflects a bootstrap estimate of the expected value of D for majority-minority segregation under random assignment (i.e., E[D]). The box plots in the bottom right graph depict the distributions of scores for D for minority-minority segregation which, of course, reflects only random residential variation over the course of simulation. The surprising finding here is that D *increases* over the course of the simulation. Why does this occur when the two minority groups are distributed randomly in relation to each other over the entire simulation? The answer traces to

[17] Note that to facilitate visual inspection the example city depicted in the figure is smaller (about 1/3rd size) than the city size used in the simulations.

the complicated nature of effective neighborhood size in residential patterns involving three or more groups.

As illustrated in Figure 4, the simulations begin with the two minority groups being highly segregated from the majority group. Under this pattern, effective neighborhood size (ENS) for the minority-minority segregation comparison is approximately 25 (i.e., the size of the neighborhoods) because households from the two minority groups live together in a small subset of the city's areas where majority households are absent. But this changes over the course of the simulation. Under the final pattern of random distribution for all groups, effective neighborhood size (ENS) for minority-minority segregation falls to approximately 5 (i.e., 20% of the neighborhood size of 25).[18] The change in ENS has important implications for the expected value of D under random assignment (i.e., E[D]) because E[D] is a negative function of effective neighborhood size. Consequently, over the course of the simulation, ENS falls from 25 to 5 and the value of D for minority-minority segregation under random assignment increases.

Figure 6 graphically summarizes results from additional analyses which replicate the analysis just reviewed using multiple ethnic demographic distributions for the virtual city. The findings closely parallel those presented in Figure 5. This documents that the unbiased version of D behaves as desired under a wide range of conditions and that the conventional version of D behaves in an undesirable way under the same wide range of conditions.

—— Figure 6 About Here ——

The findings here document that previous suggestions for dealing with index bias by adjusting observed values of D in relation to its expected value under random distribution (i.e., E[D]) face a serious obstacle. The obstacle is that adjustments such as those proposed by Winship (1977) and Carrington and Troske (1997) presume, implicitly and incorrectly, that a single value of E[D] is relevant for the adjustment calculation and that this value can be estimated either by formula or by bootstrap methods grounded in baseline models of random distribution. The results here show that the value of D for minority-minority segregation under random assignment, which can be interpreted as a bootstrap estimate of E[D], is significantly affected by an important factor; namely, the residential relationship between the two minority groups and the majority group. In more general terms, the findings here indicate that E[D] for any two-group comparison is complicated in the multi-group situation and will be affected by: (a) the extent to which the two groups in the comparison are jointly segregated from other groups and (b) the relative size of other groups in the city population.

Space does not permit a detailed review of the issue, but in analyses not reported here, I establish that this finding applies to all conventional indices of uneven distribution and that two broad conclusions hold in multi-group situations. One is that expected values of index scores under random assignment (i.e., E[•]) can potentially vary over a wide range. The other is that

[18] I say approximately because a precise discussion of effective neighborhood size would take account of the city vacancy rate (which is 6% in these simulations).

adjustments of index scores in relation to expected values (E[•]) can be inappropriate and misleading if estimates of E[•] do not take account of this complication.

This may help explain why adjustment methods such as those proposed by Winship (1977) and Carrington and Troske (1997) are rarely used in empirical analyses. My own experience has been that the adjustment methods do work well in methodological studies where the underlying assumptions of the method are met and they also work reasonably well in analyses where a simple two-group situation is approximated. However, when the adjustment methods are applied in the context of multi-group situations, they can "break down" and yield surprising and substantively implausible results.

It is possible that the general approach can be "salvaged" by refining the methods used to estimate expected index values under random assignment (i.e., E[•]). But, since the necessary refinements would be very demanding to implement, it is unlikely they will gain wide use.[19] The results reviewed here show that the unbiased indices introduced in this paper provide an attractive alternative that both conceptually appealing and much easier to apply in empirical analyses. The new indices eliminate the source of bias at the root and therefore do not require one to attempt after the fact adjustments to eliminate the complex manifestations of bias. Accordingly, the expected values of the unbiased indices are zero regardless of whether other groups are present in the population and, if so, regardless of the nature of the residential segregation pattern between the two groups of interest and other groups. Indeed, the only impact I have been able to discern so far is that the dispersion of the sampling distribution of the unbiased indices is affected by the presence of other groups. More specifically, while the mean for unbiased indices is always approximately zero, the standard error of the mean varies inversely with ENS. But this is what basic sampling theory would lead one to expect and the pattern holds for the expected distributions of scores of both conventional and unbiased versions of indices of uneven distribution.

### Conceptual and Practical Issues and Potential Impact on Research

When should the new unbiased versions of indices of uneven distribution be used? One reasonable answer is that they almost always be used. They are not burdensome to compute; they support familiar substantive interpretations and also expand the available substantive interpretations; and using them eliminates potential concerns about index bias from discussion. A related question might be, will using the new unbiased versions of familiar indices lead to major

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[19] I have found that bootstrap methods can be used to obtain serviceable context-specific estimates of E[•]. One such approach is to take the observed distribution across areas of the combined count of the two groups in the segregation comparison. Then perform bootstrap simulations wherein households from the two groups in the comparison are assigned randomly to areas until the observed area counts for the two groups combined are duplicated. Performing a sufficiently large number of bootstrap simulations (e.g. 500) will then establish the expected value of the index of interest under random assignment. Estimates of E[•] obtained in this way are specific, not only to the nature of the multi-group residential pattern, but also to other potential complicating factors such as variation in area size. In the simulation results just reviewed, values of E[D] would have to be recalculated for every time period of the simulation and even that would be a compromise because the observed value of E[D] is changing rapidly in the early stages of the simulations.

changes in research findings, or alternatively, will it make a practical difference? I answer this in two parts. The first part notes that findings from studies that have used research designs that follow the now conventional practices for guarding against index bias are not likely to be contradicted in dramatic ways. The reason for this is straightforward; to the extent the design practices protected against serious problems with index bias, replications that use unbiased versions indices with the same study design will not be likely to yield dramatically different results. The second part of the answer is that I believe the potential for greater practical impact on research results will come from the fact that future studies will be able to conduct expanded analyses that may draw on many cases that previously were not included in studies because conventions in restricting study designs caused them to be excluded.

On balance, I argue that it will always make good sense to examine the new unbiased versions of indices. One can never worse off for this. It is not easy to definitively rule out the possibility that they will yield different results in some respects even in replication studies. Current "rule-of-thumb" practices aimed at minimizing undesirable complications associated with index bias are not precise and may occasionally be hit and miss in effectiveness. Concerns on this point can be easily set aside by examining the unbiased versions of the indices even if one in the end elects to report the conventional versions.

Perhaps more importantly, examining unbiased indices can free investigators from some of the routine restrictions that limit the scope of segregation studies and the range of questions researchers investigate to guard against complications associated with index bias. Relaxing the need for such restrictions and limitations can only be a good thing. It will allow researchers to expand samples and explore a broader range of research questions. The following is a brief list of research applications where the benefits of using unbiased indices are especially likely to be seen.

- studies assessing segregation at small spatial scales such as the census block and block group;
- studies assessing segregation for small population groups such as Asian and Latino populations in areas of new settlement;
- studies assessing segregation when groups are imbalanced in size;
- studies assessing segregation for subgroups within broader populations, for example, the segregation of high-income Whites and high-income African Americans;
- studies assessing segregation based on published tabulations for sample data (in contrast to tabulations based on 100% count data).
- studies assessing segregation based on forthcoming tabulations from the American Community Survey that will be based on small samples;
- studies assessing across small units such as classrooms within schools; and
- studies assessing segregation in simulation analyses where analysis of neighborhoods of small spatial scale are typical.[20]

[20] In fact, I pursued the development of unbiased measures of uneven distribution to cope with the problem of bias in measuring segregation in simulation studies. In that context, the unbiased measures allow researchers to explore a much wider range of combinations of neighborhood scale and population composition than can be considered

To conclude, one is never worse off for examining the new unbiased versions of popular indices and there are many ways they may yield benefits. So I encourage researchers to take advantage of these new measurement options.

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using conventional versions of segregation indices.

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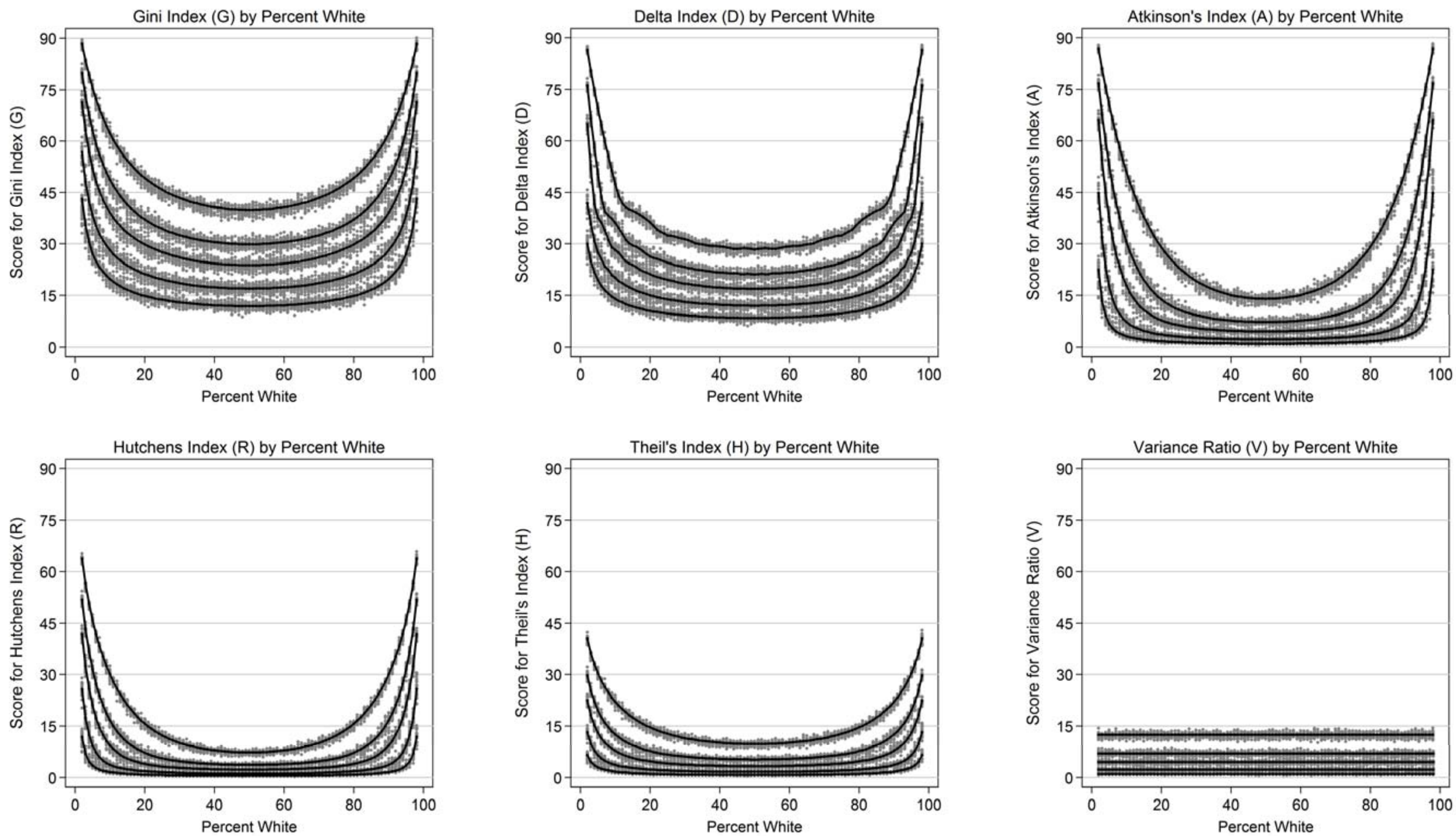
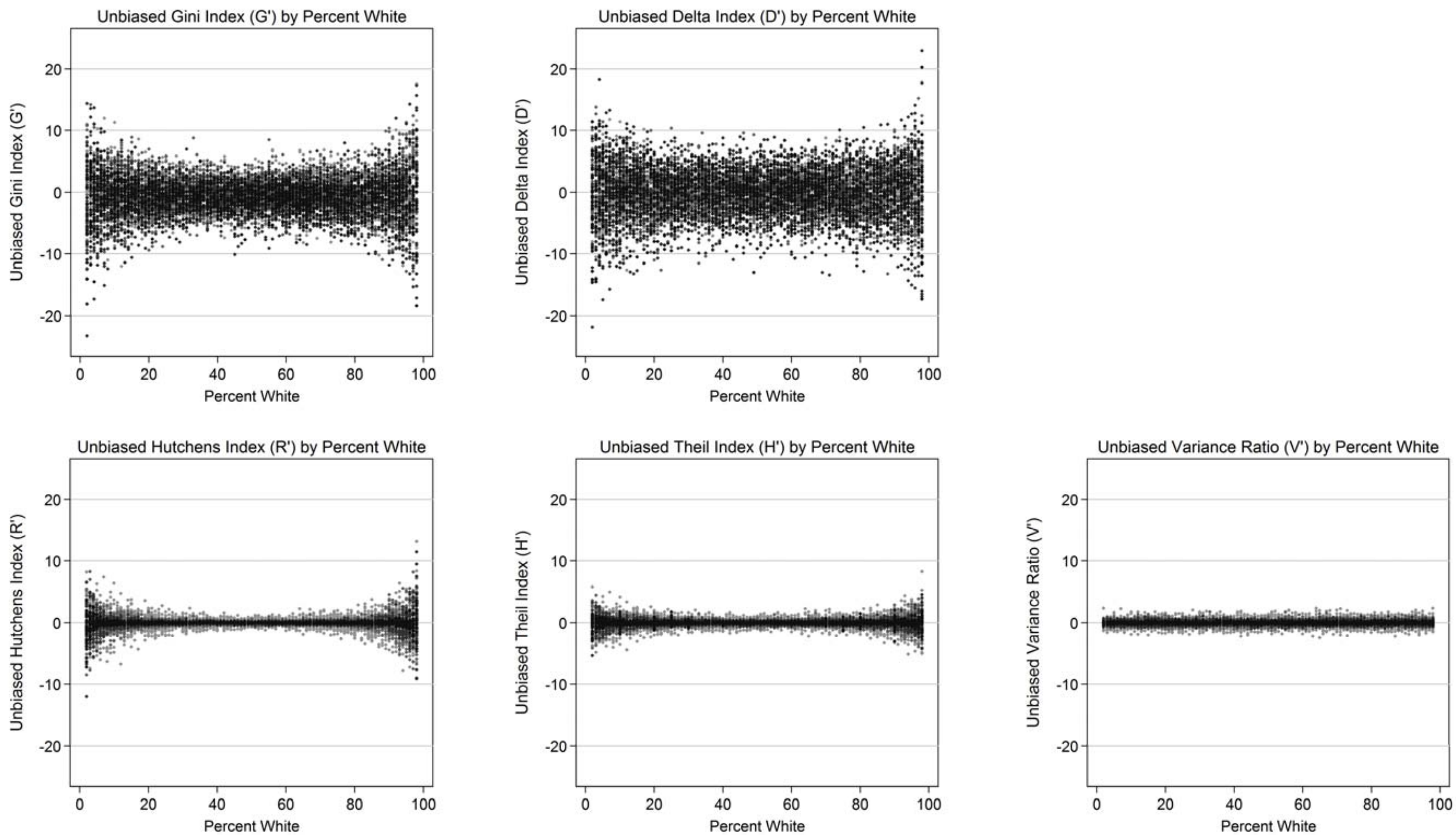
Figure 1. Scores for Segregation Indices Under Random Assignment by Percent White and Neighborhood Size



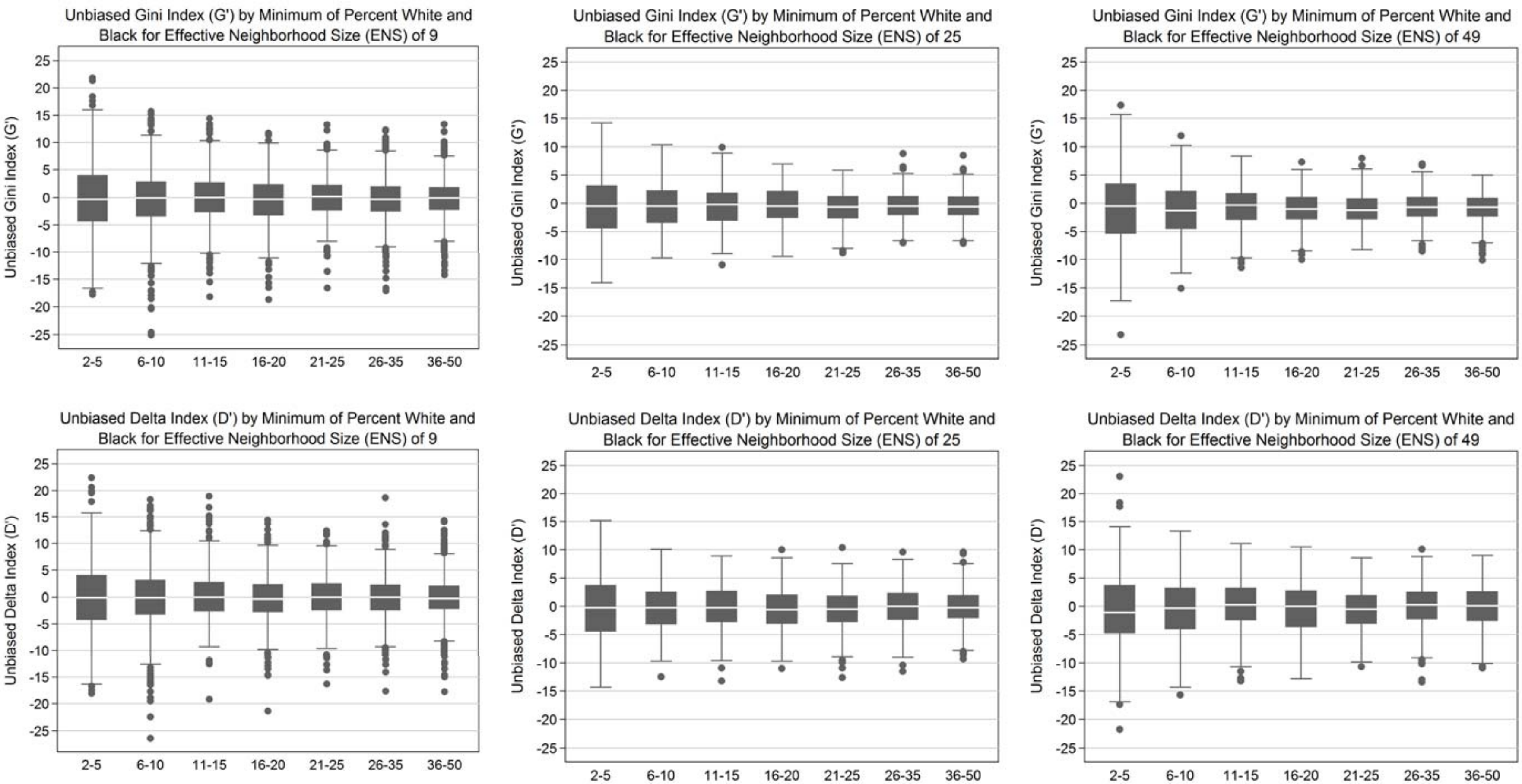

Note: Points plotted in light gray are values calculated from random residential distributions. Points plotted in black are expected values (e.g., E[D]). Values for effective neighborhood size (ENS) are 9, 16, 25, 49, 100.

Figure 2. Scores for Unbiased Segregation Indices Under Random Assignment by Percent White and Neighborhood Size



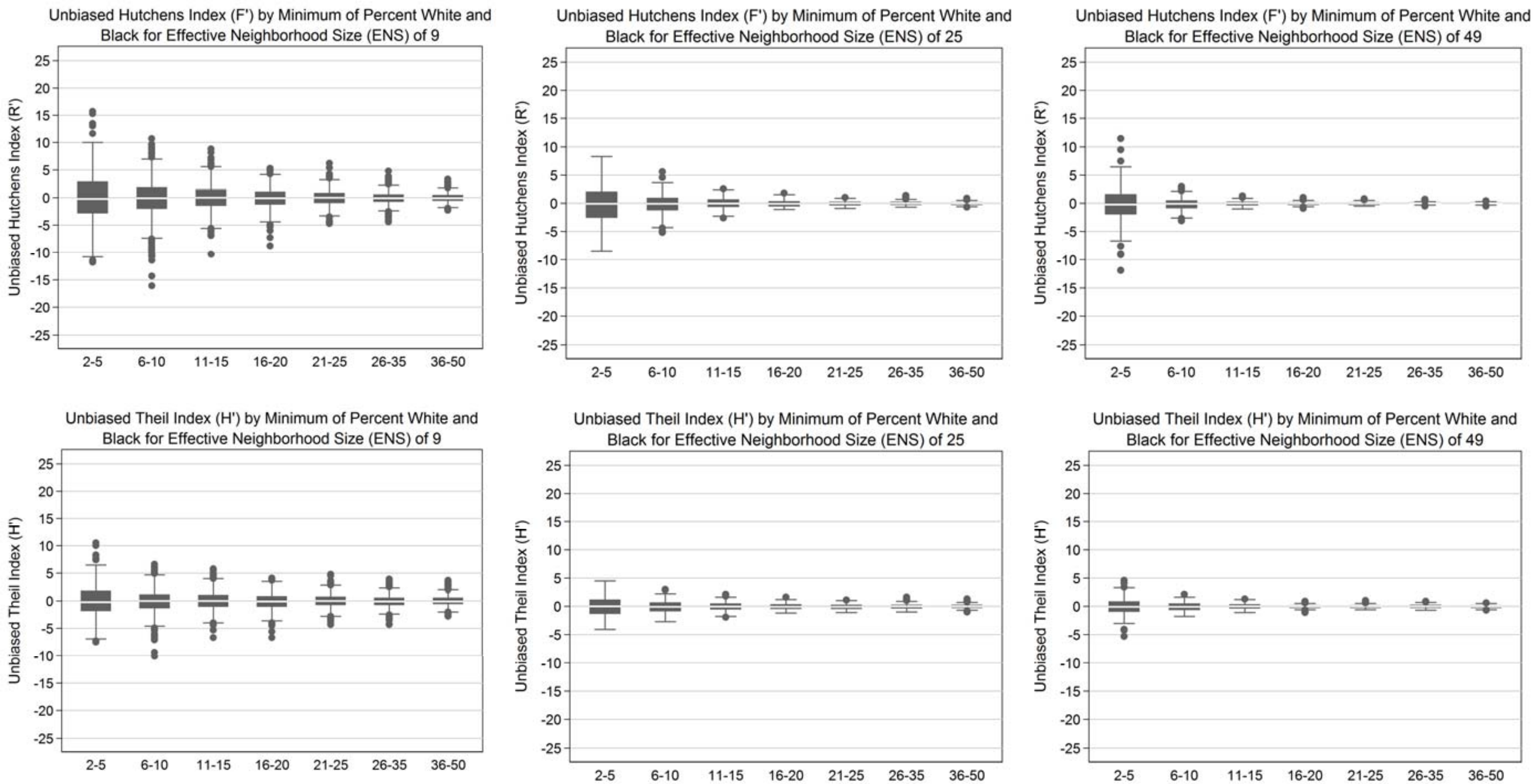

Note: Values for effective neighborhood size (ENS) are 9, 25, 49, and 100. Cases for higher values of ENS are plotted in darker shades.

Figure 3. Scores for Unbiased Segregation Indices Under Random Assignment by Group Percentage and Neighborhood Size



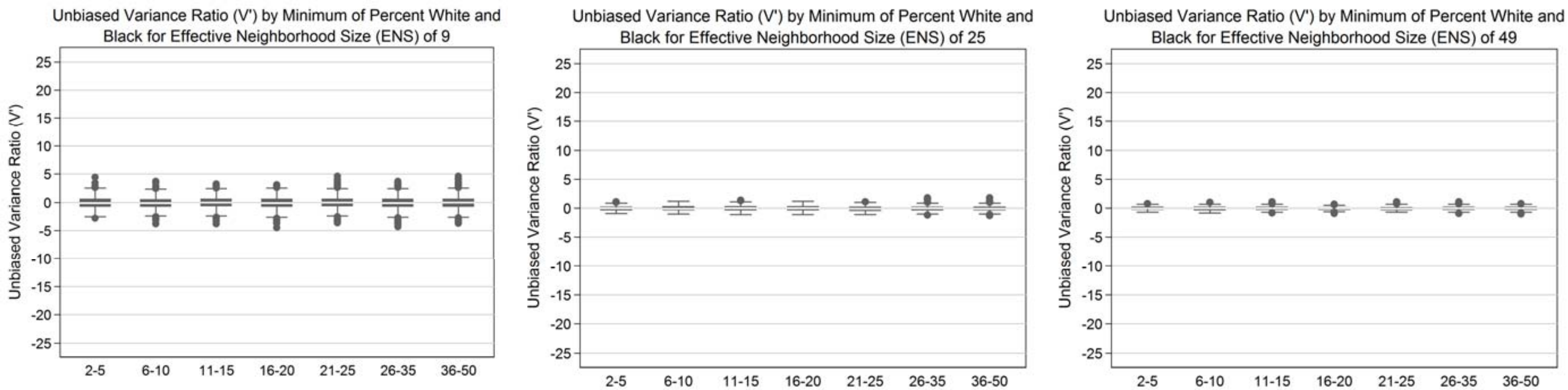

Note: Group Percentage is the smaller of the two group percentages.

Figure 3 (Continued. Scores for Unbiased Segregation Indices Under Random Assignment by Group Percentage and Neighborhood Size



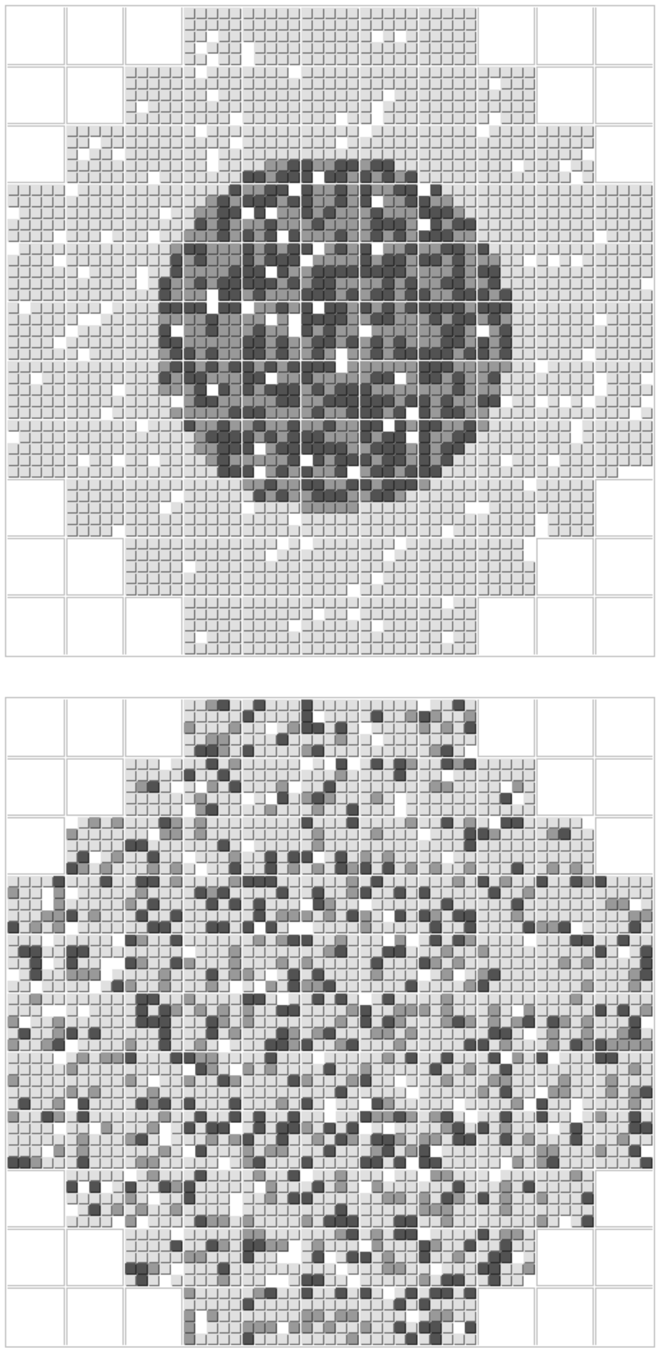

Note: Group Percentage is the smaller of the two group percentages.

Figure 3 (Continued). Scores for Unbiased Segregation Indices Under Random Assignment by Group Percentage and Neighborhood Size



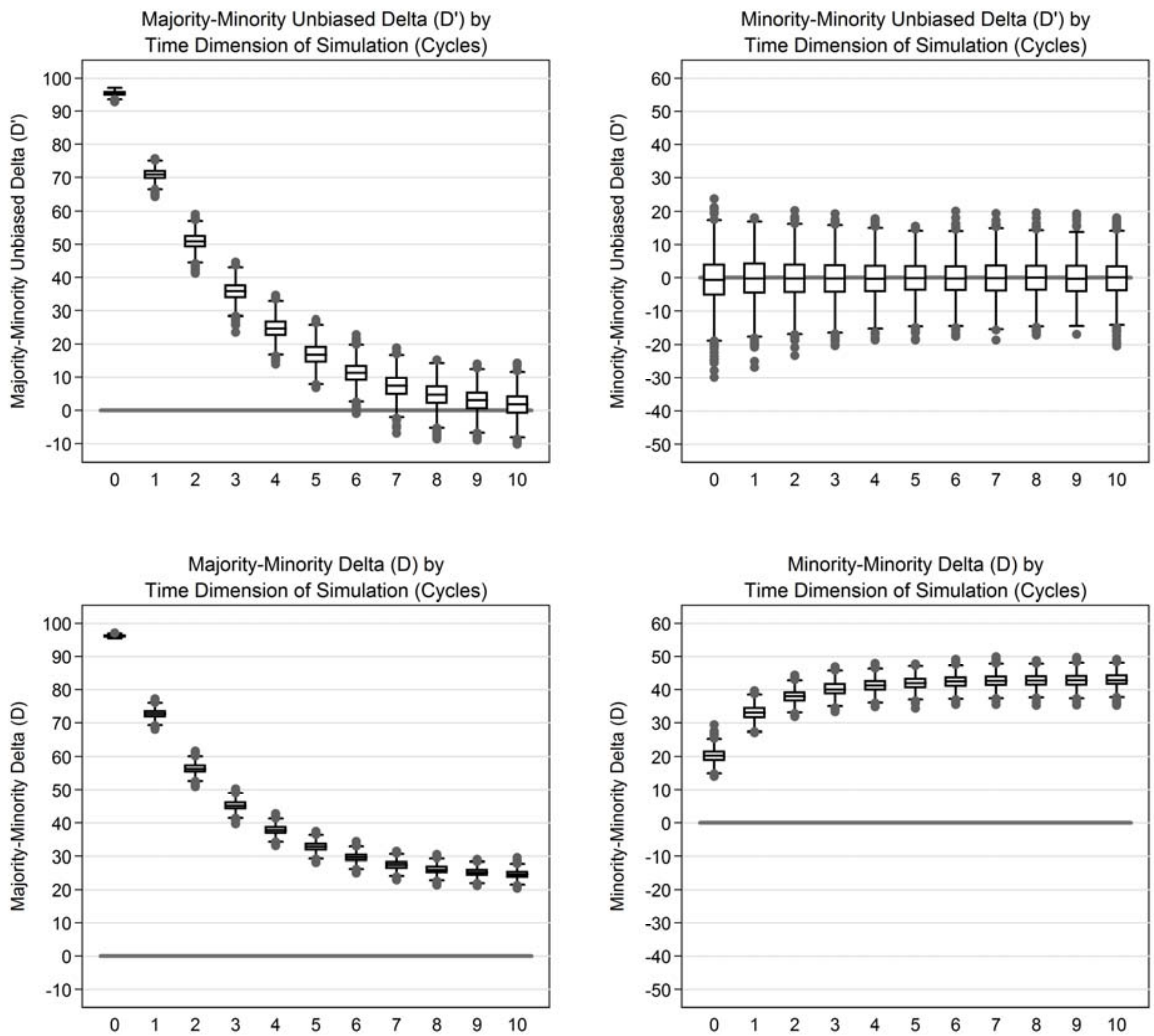

Note: Group Percentage is the smaller of the two group percentages.

Figure 4 Illustration of the Transition from the Initial State of Minority-Minority Integration and High Majority-Minority Segregation to the End State of All-Way Integration (Random Distribution)



Note: Households from the majority group and two minority groups are depicted in shades of gray (light, medium, and dark gray, respectively). Vacant housing units are in white. Grid lines delimit areas. For easy visual review, the city here is 40% the size of the city in the simulations but faithfully depicts city shape and residential patterns.

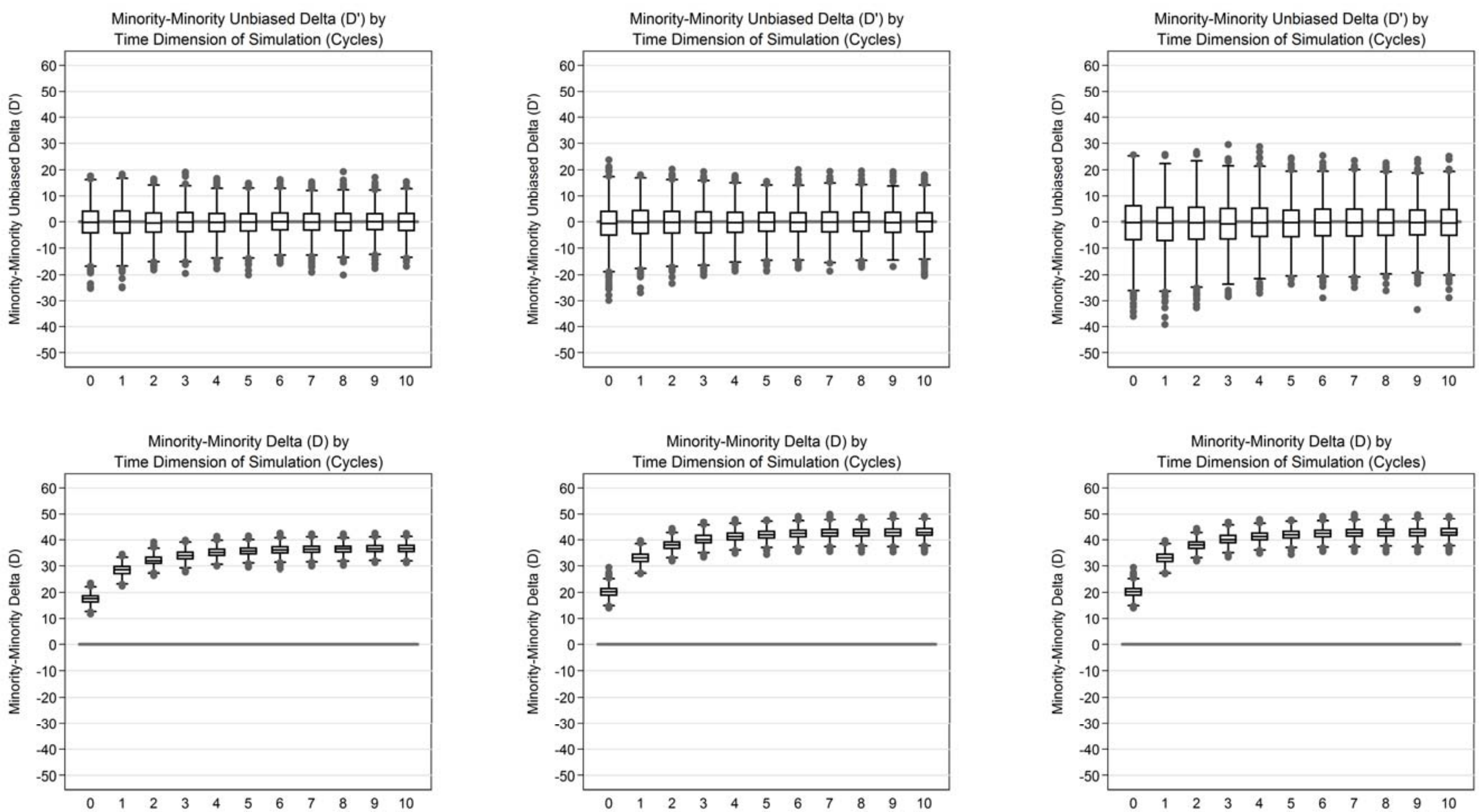
Figure 5. Box Plots Depicting Distributions of Scores for Unbiased and Conventional Delta Index (D' and D) for Majority-Minority Segregation and Minority-Minority Segregation over Ten Simulation Cycles




Note: The graphs in the top row depict unbiased delta (D') for majority-minority segregation on the left and minority-minority segregation on the right. The graphs on the bottom row depict conventional delta (D) for the same comparisons. See text for details regarding the simulation designs. .

Figure 6. Scores for Unbiased and Conventional Delta Index (D' and D) for Minority-Minority Segregation over Time for Three Combinations of Ethnic Mix

Ethnic Mix 80/10/10 | Ethnic Mix 80/15/5 | Ethnic Mix 91/6/3




Note: Top row is unbiased delta (D') for minority-minority segregation; bottom row is conventional delta (D) for minority-minority segregation. Each simulation begins with an initial residential distribution in which the majority group is very highly segregated from two minority groups and the two minority groups are randomly distributed in relation to each other. Ten periods of random residential movement follow and the segregation pattern moves rapidly toward random distribution for *all* groups. Ethnic mix settings are 80/10/10 (column 1), 80/15/5 (column 2), and 91/6/3 (column 3). Neighborhood size is 25.

Table 1. Summary of Expected Values of Terms Based on Example Using Binomial Model for 20 Households Per Area and City Proportion White 0.90.[1]

| Index of Uneven Distribution | Expected Group Means on Scaled Exposure to Whites (y=f[p]) $E[Y_W]$ | $E[Y_B]$ | Index Score from Difference of Means $E[Y_W-Y_B]$ |
|---|---|---|---|
| Gini Index (G)[2] | | | |
| Conventional (based on area population) | 52.04 | 31.64 | 40.80 |
| Unbiased (based on neighbors) | 50.00 | 50.00 | 0.00 |
| Dissimilarity Index (D) | | | |
| Conventional (based on area population) | 42.03 | 13.51 | 28.52 |
| Unbiased (based on neighbors) | 42.03 | 42.03 | 0.00 |
| Hutchens Square Root Index (R) | | | |
| Conventional (based on area population) | 57.05 | 46.14 | 10.91 |
| Unbiased (based on neighbors) | 56.44 | 56.44 | 0.00 |
| Theil Entropy Index (H) | | | |
| Conventional (based on area population) | 74.57 | 65.79 | 8.78 |
| Unbiased (based on neighbors) | 73.88 | 73.88 | 0.00 |
| Variance Ratio (V) | | | |
| Conventional (based on area population) | 90.50 | 85.50 | 5.00 |
| Unbiased (based on neighbors) | 90.00 | 90.00 | 0.00 |

[1] Group means on scaled contact are multiplied by 100 as is standard reporting of results for segregation scores.

[2] In the case of G, the group difference of means is multiplied by 2.

Table 2. Means for Conventional Versions of Popular Indices of Uneven Distribution Computed for Random Residential Distributions by Relative Group Size (P) and Neighborhood Size

| | | Neighborhood Size | | | | | |
|---|---|---|---|---|---|---|---|
| Index | P[1] | 9 | 16 | 25 | 49 | 100 | 225 |
| Gini (G) | ≤ 5 | 81.4 | 70.5 | 60.1 | 45.2 | 33.0 | 22.0 |
| | 11-15 | 56.5 | 43.7 | 35.1 | 25.1 | 17.6 | 11.7 |
| | 36-50 | 40.2 | 30.1 | 24.0 | 17.1 | 12.0 | 8.0 |
| Dissimilarity (D) | ≤ 5 | 77.7 | 62.8 | 48.0 | 33.4 | 24.0 | 15.6 |
| | 11-15 | 40.8 | 31.6 | 25.2 | 17.9 | 12.5 | 8.3 |
| | 36-50 | 28.7 | 21.4 | 17.1 | 12.2 | 8.5 | 5.7 |
| Atkinson[2] (A) | ≤ 5 | 78.4 | 64.4 | 49.8 | 28.4 | 12.9 | 4.5 |
| | 11-15 | 41.9 | 23.3 | 13.0 | 5.5 | 2.6 | 1.1 |
| | 36-50 | 14.5 | 7.5 | 4.7 | 2.4 | 1.1 | 0.5 |
| Hutchens (R) | ≤ 5 | 54.0 | 40.8 | 29.6 | 15.6 | 6.7 | 2.3 |
| | 11-15 | 23.8 | 12.4 | 6.7 | 2.8 | 1.3 | 0.5 |
| | 36-50 | 7.6 | 3.8 | 2.4 | 1.2 | 0.6 | 0.3 |
| Theil (H) | ≤ 5 | 34.8 | 24.1 | 16.9 | 9.0 | 4.4 | 1.8 |
| | 11-15 | 18.8 | 10.5 | 6.4 | 3.1 | 1.5 | 0.6 |
| | 36-50 | 9.9 | 5.3 | 3.3 | 1.7 | 0.8 | 0.4 |
| Variance Ratio (V) | ≤ 5 | 12.3 | 6.9 | 4.4 | 2.2 | 1.1 | 0.5 |
| | 11-15 | 12.3 | 7.0 | 4.4 | 2.3 | 1.1 | 0.5 |
| | 36-50 | 12.3 | 6.9 | 4.4 | 2.3 | 1.1 | 0.5 |

[1] P is the city-wide group percentage for the smaller group in the comparison. [2] Atkinson index (A) is computed with δ set at 0.5, the only value at which A is symmetric such that its value is the same for white-black and black-white segregation.

Table 3. Standard Deviations for Conventional Versions of Popular Indices of Uneven Distribution Computed for Random Residential Distributions by Relative Group Size (P) and Neighborhood Size

| | | Neighborhood Size | | | | | |
|---|---|---|---|---|---|---|---|
| Index | P[1] | 9 | 16 | 25 | 49 | 100 | 225 |
| Gini (G) | ≤ 5 | 4.9 | 6.6 | 7.2 | 7.4 | 5.7 | 3.9 |
| | 11-15 | 2.4 | 2.4 | 2.4 | 2.1 | 1.4 | 1.0 |
| | 36-50 | 1.2 | 1.2 | 1.2 | 1.2 | 0.9 | 0.6 |
| Dissimilarity (D) | ≤ 5 | 6.4 | 9.6 | 10.3 | 5.9 | 4.2 | 2.8 |
| | 11-15 | 1.8 | 2.2 | 1.9 | 1.5 | 1.1 | 0.8 |
| | 36-50 | 0.9 | 0.9 | 0.9 | 0.9 | 0.6 | 0.4 |
| Atkinson[2] (A) | ≤ 5 | 6.1 | 8.9 | 10.4 | 10.7 | 6.3 | 2.0 |
| | 11-15 | 3.8 | 3.3 | 2.5 | 1.1 | 0.4 | 0.2 |
| | 36-50 | 1.2 | 0.6 | 0.5 | 0.3 | 0.2 | 0.1 |
| Hutchens (R) | ≤ 5 | 6.7 | 7.6 | 7.5 | 6.5 | 3.4 | 1.0 |
| | 11-15 | 2.5 | 1.9 | 1.3 | .06 | 0.2 | 0.1 |
| | 36-50 | 0.6 | 0.3 | 0.2 | 0.2 | 0.1 | 0.1 |
| Theil (H) | ≤ 5 | 3.9 | 3.9 | 3.5 | 2.7 | 1.5 | 0.6 |
| | 11-15 | 1.4 | 1.1 | 0.9 | 0.5 | 0.2 | 0.1 |
| | 36-50 | 0.6 | 0.4 | 0.3 | 0.2 | 0.1 | 0.1 |
| Variance Ratio (V) | ≤ 5 | 0.7 | 0.5 | 0.4 | 0.3 | 0.2 | 0.1 |
| | 11-15 | 0.6 | 0.5 | 0.4 | 0.3 | 0.2 | 0.1 |
| | 36-50 | 0.7 | 0.5 | 0.4 | 0.3 | 0.2 | 0.1 |

[1] P is the city-wide group percentage for the smaller group in the comparison. [2] Atkinson index (A) is computed with δ set at 0.5, the only value for which A is symmetric such that its value is the same for white-black and black-white segregation.

Table 4. Means for Unbiased Versions of Popular Indices of Uneven Distribution Computed for Random Residential Distributions by Relative Group Size (P) and Neighborhood Size

| | | Neighborhood Size | | | | | |
|---|---|---|---|---|---|---|---|
| Index | P[1] | 9 | 16 | 25 | 49 | 100 | 225 |
| Gini (G) | ≤ 5 | -0.1 | -0.5 | -0.4 | -0.7 | -1.0 | -1.5 |
| | 11-15 | -0.2 | 0.2 | -0.5 | -0.6 | -0.7 | -1.1 |
| | 36-50 | -0.2 | -.04 | -0.5 | -0.8 | -0.8 | -1.0 |
| Dissimilarity (D) | ≤ 5 | -0.0 | -0.4 | -0.4 | -0.8 | -0.2 | -0.6 |
| | 11-15 | -0.0 | 0.1 | -0.1 | 0.3 | -0.1 | 0.1 |
| | 36-50 | -0.2 | -0.2 | -0.1 | -0.1 | -0.1 | 0.1 |
| Hutchens (R) | ≤ 5 | -0.1 | -0.2 | -0.1 | -0.1 | -0.1 | -0.1 |
| | 11-15 | -0.1 | 0.2 | -0.0 | 0.0 | 0.0 | -0.0 |
| | 36-50 | -0.0 | -0.0 | -0.0 | -0.1 | -0.0 | 0.0 |
| Theil (H) | ≤ 5 | -0.0 | -0.1 | -0.1 | -0.1 | -0.1 | -0.1 |
| | 11-15 | -0.0 | 0.1 | -0.0 | 0.0 | 0.0 | -0.0 |
| | 36-50 | -0.0 | -0.0 | -0.0 | -0.0 | -0.0 | -0.0 |
| Variance Ratio (V) | ≤ 5 | -0.0 | -0.0 | -0.0 | -0.0 | -0.0 | -0.0 |
| | 11-15 | -0.0 | 0.0 | -0.0 | -0.0 | 0.0 | -0.0 |
| | 36-50 | -0.0 | -0.0 | -0.0 | -0.0 | -0.0 | -0.0 |

[1] P is the city-wide group percentage for the smaller group in the comparison.

Table 5. Standard Deviations for Unbiased Versions of Popular Indices of Uneven Distribution Computed for Random Residential Distributions by Relative Group Size (P) and Neighborhood Size

| | | Neighborhood Size | | | | | |
|---|---|---|---|---|---|---|---|
| Index | P[1] | 9 | 16 | 25 | 49 | 100 | 225 |
| Gini (G) | ≤ 5 | 4.9 | 5.3 | 5.8 | 6.7 | 5.2 | 3.2 |
| | 11-15 | 3.3 | 3.5 | 3.6 | 3.6 | 2.5 | 1.9 |
| | 36-50 | 2.4 | 2.4 | 2.4 | 2.4 | 1.8 | 1.2 |
| Dissimilarity (D) | ≤ 5 | 4.8 | 5.4 | 5.8 | 6.9 | 6.1 | 4.3 |
| | 11-15 | 3.3 | 3.6 | 3.9 | 4.6 | 3.5 | 3.3 |
| | 36-50 | 2.5 | 2.7 | 2.9 | 3.7 | 3.0 | 2.6 |
| Hutchens (R) | ≤ 5 | 3.4 | 3.3 | 3.2 | 3.0 | 1.6 | 0.5 |
| | 11-15 | 1.7 | 1.3 | 0.9 | 0.5 | 0.2 | 0.1 |
| | 36-50 | 0.5 | 0.3 | 0.2 | 0.2 | 0.1 | 0.0 |
| Theil (H) | ≤ 5 | 2.2 | 1.9 | 1.7 | 1.4 | 0.8 | 0.3 |
| | 11-15 | 1.2 | 0.9 | 0.7 | 0.4 | 0.2 | 0.1 |
| | 36-50 | 0.6 | 0.4 | 0.3 | 0.2 | 0.1 | 0.1 |
| Variance Ratio (V) | ≤ 5 | 0.8 | 0.5 | 0.4 | 0.3 | 0.2 | 0.1 |
| | 11-15 | 0.7 | 0.6 | 0.5 | 0.3 | 0.2 | 0.1 |
| | 36-50 | 0.8 | 0.5 | 0.4 | 0.3 | 0.2 | 0.1 |

[1] P is the city-wide group percentage for the smaller group in the comparison.

Table 6. Count of Simulation Trials for Random Residential Distributions by Relative Group Size (P) and Neighborhood Size

| | | Neighborhood Size | | | | | |
|---|---|---|---|---|---|---|---|
| $P^2$ | Percent White | 9 | 16 | 25 | 49 | 100 | 225 |
| ≤ 05 | ≤ 05 \| ≥ 95 | 212 | 201 | 189 | 220 | 210 | 197 |
| 11-15 | 11-15 \| 90-94 | 277 | 270 | 256 | 269 | 253 | 289 |
| 36-50 | 36-50 \| 51-64 | 716 | 759 | 747 | 752 | 754 | 743 |

[1] Group percentage is For each simulation neighborhood size, [2] P is the city-wide group percentage for the smaller group in the comparison.