

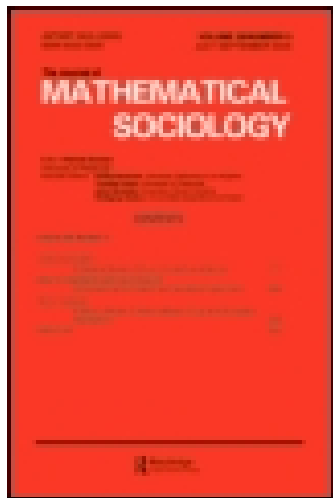
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Including Preference and Social Distance Dynamics in Multi-Factor Theories of Segregation

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All agree that many factors contribute to ethnic segregation, but controversy continues regarding the relevance of particular factors. In my paper I acknowledge the importance of discrimination, but I direct attention to the role of ethnic preferences and social distance dynamics. I do so because many dismiss the relevance of these factors for contemporary segregation without engaging formal theories and analytic models which suggest that dynamics associated with in-group attraction and out-group avoidance may take on increasing importance as past forms of discrimination slowly fade. Agent-based modeling shows promise for exploring the issue of whether prejudice against out-groups and affinity toward in-groups can build and sustain segregation in the absence of discrimination. Research drawing on this approach is at an early stage of development but is significant for highlighting two things. One is that the implications of preferences for segregation are strongly conditioned by the ethnic demography of the city. Another, closely tied to the first, is that different views about the implications of preferences for segregation often hinge on inconsistencies in how notions of integration and segregation are applied in discussions of individual location choices, the ethnic mix of single neighborhoods, and the ethnic distributions for all neighborhoods in a city. Critiques of agent-based models of Schelling-style preference effects will carry more force when they outline models indicating how location decisions guided by preferences documented in surveys can produce or sustain integration.

Keywords: agent-based model, simulation, ethnic segregation, urban ecology, prejudice, ethnic preferences, social distance

To begin, I wish to thank *JMS* and Professor Skvoretz, the editor for this issue, for providing me the opportunity to share my research and explore theoretical issues that I believe warrant attention. I also thank Professors Clark, Goering, Macy (writing with co-author Rijt),

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and St. John for offering thoughtful comments and criticism. I will try to respond to their remarks and at the same time respect the editor's request that I be "more or less" brief.

Clark, Macy and Rijt, and St. John are generally appreciative of at least some portions of the paper. Goering finds little to like. Some of the dividing lines in the literature are evident in the comments they offer. Many scholars and researchers, including Clark, Macy and Rijt, and St. John, take preference theory and social distance theory seriously as relevant for understanding contemporary ethnic segregation. Others, probably larger in number, are like Goering and view these theoretical perspectives with deep skepticism. Scholars and researchers in the former group tend to accept that it is worthwhile to explore the implications of preference and social distance theory within the context of modeling frameworks ranging from the analytic exercises described by St. John to Schelling's famous analytic and simulation models to more refined agent-based models made feasible by recent advances in computational methods. Those who are skeptical of these perspectives are reluctant to assign much weight to findings obtained using these methods.

Those who take preference theory and social distance theory seriously frame the relevance of preferences and social distance for ethnic segregation in a straightforward way. St. John's comments provide a nice illustration. He notes that many models suggest that preferences for contact based on social similarity (and avoidance based on dissimilarity) are relevant for understanding segregation. He also notes that theories of mutual attraction are routinely accepted as relevant for understanding congregation based on socioeconomic status, life-style, stage of the life cycle, and other social characteristics. He then poses the question, "Why is it so hard to accept that people also want to congregate voluntarily for positive reasons associated with ethnicity?" It is a compelling question. Skeptics of preference theory argue it is too simple because, for one thing, preferences and social distance are not exogenous but are embedded in a complex web of history and social relations. I doubt if many researchers would disagree that a full understanding of preferences should consider the origins of ethnic community and ethnic affiliation. But those who find St. John's question compelling believe it is legitimate to focus on the implications of affinity based on social similarity (aversion based on dissimilarity) *when it exists*. They note that ethnic preferences produce the same outcomes within analytic and simulation models regardless of whether affinity and social distance are seen as having particular historical origins.

Scholars and researchers will receive my efforts quite differently depending on whether they entertain this point of departure. Clark, Macy and Rijt, and St. John accept my strategy of formulating and using models to develop theoretical insights, and they take the insights gleaned from these efforts seriously even when the models involved do not strive for realism and high external validity. Accordingly, much of their commentary and discussion focuses on what kinds of findings can be established within the confines of different modeling frameworks. For example, Clark gives attention to how my findings may inform the broader literature on segregation, and he points out that future work with these models should give attention to the nature of the function linking neighborhood ethnic mix and preferences to residential satisfaction and choice. St. John wonders what would be found if simulations implemented minority ethnic preferences under conditions of majority indifference. These are excellent suggestions for future research and obviously presume the effort is worthwhile. Macy and Rijt go even further in this direction. They are enthusiastic about exploring the theoretical model and suggest many potentially useful model refinements (e.g., starting conditions of maximum segregation). In addition, they implement their own set of simulations to investigate refinements such as “noise” and alternative specifications of neighborhood scale and form, they raise questions about the scope conditions for particular simulation findings, and so on. For me, the comments, suggestions, and empirical explorations are stimulating and gratifying. Research of this type is at an early stage, and Clark, Macy and Rijt, and St. John have highlighted many good questions that need to be pursued in future research.

Researchers and scholars who are skeptical about the relevance of preference theory and social distance theory for ethnic segregation tend to be skeptical of the analytic approach I use. Goering for one raises many objections. These include a variety of criticisms of model specification and measurement strategy. I will comment on some of these, but I believe his major objection to my work is more fundamental; he questions the value of my point of analytic departure—trying to use abstract models to assess the implications of preferences and social distance dynamics for segregation.

Before addressing specific points in Goering’s comments, I first address something that I feel muddies the discussion. Goering mischaracterizes the goal of my paper many times saying that my “vote is for the primacy of preferences,” that I suggest that preferences have “the greater impact in affecting segregation,” and that I have offered a “single factor” explanation of segregation. This is not my intent, and it is easy to find statements in the paper where I acknowledge the reality

of discrimination even while considering the implications of long-term changes in discrimination. For the record, I fully acknowledge the existence of discrimination. I believe it is easy to observe and document. I have no hesitancy in describing it as an important force in residential systems. Consequently, I find Goering's suggestion that I believe "discrimination is but a quaint historical remnant of time and systems long gone" a troubling rhetorical tactic. My simulation program has algorithms that implement several major forms of discrimination (e.g., white exclusion of minorities, realtor steering, and loan discrimination).¹ When these dynamics are activated and set at high values, my model will produce high levels of ethnic segregation. Thus, my model highlights multiple factors, discrimination central among them, that are routinely hypothesized to contribute to ethnic segregation.

Nowhere in my paper or elsewhere do I offer quantitative assessments of the relative importance of discrimination, preferences, and other factors that may influence segregation. I am skeptical of such efforts for two reasons. First, I do not believe we possess the requisite data and formal modeling framework needed to accomplish the task with reasonable quantitative precision. Second, I have reservations about whether it is appropriate to offer a statement such as "10% of segregation is due to economic inequality, 30% is due to preferences, and 60% is due to discrimination." If multiple sufficient causes of segregation are operating, this kind of simple accounting can be highly questionable. For example, in simulation analyses, the impact of adding or subtracting a particular dynamic can vary considerably depending on what other dynamics are operating and on the order in which particular dynamics are added or removed.

If I acknowledge the importance of discrimination and eschew ranking its importance compared to other factors producing segregation, then why does my paper focus so much attention on preferences as a potential explanation of segregation? The answer is simple. Sociological theorizing about preference and social distance effects on segregation is often muddled and confused and should be improved. The literature on this subject offers many strong negative conclusions about the role of preferences and social distance dynamics in ethnic segregation that

¹Goering states that my definition of discrimination fails to include some aspects of discrimination. Since my paper does not report simulations that model discrimination dynamics, it is unclear to me why this is relevant. When my simulation program is used to model discrimination, it can produce very high levels of segregation based on a handful of discrimination processes (e.g., exclusion, steering, and differential treatment in loan qualifying). I am unaware of a crucial substantive point about discrimination that cannot be illustrated using my model.

appear to me to be unwarranted on theoretical and empirical grounds. This state of affairs promotes confusion not understanding. I feel no hesitation in focusing attention on these problems. Professor Goering raises the specter that the Heritage Foundation and the white population may draw selectively on the issues I raise. I worry more about the credibility of sociological theories of segregation and the possibility that theory and understanding are poorly served when we circumscribe debate.

The nature of the literature makes my task easier than it should be. For example, it is routine in the literature to characterize minority preferences as integration promoting and majority preferences as segregation promoting. I note that this characterization cannot easily be squared with standard conceptions of segregation at the city level. It is also routine to find Schelling's model-based insights criticized and even rejected based on discursive arguments unsupported by formal theory or modeling. I show that at least some of these criticisms are unfounded. These contributions would not be necessary if the literature did not so often casually dismiss the relevance of preferences. Thus, my paper outlines the case for preferences and social distance dynamics not to establish them as *the* primary explanation for segregation but to have them included in the discussion.

On more specific points, Goering provides an extended discussion raising concerns about various aspects of the model I use. He touches on the size of the city; the size of neighborhoods; using the index of dissimilarity to measure segregation, especially when minority populations are small; the level of the vacancy rate; the stability of the vacancy rate over time; the specification of central city, suburb differences in housing quality; how "tipping" is handled,² the absence of the absence of highways and ecological barriers; the absence of migration dynamics; the absence of gangs; the absence of garbage disposal plants; and more. For discussion purposes, I group these concerns into categories. First, is a group of concerns that can be set aside because they are mistaken. Goering states that the model does not include migration dynamics, but Macy and Rijt's comments highlight the importance of the parameter in the SimSeg model that represents precisely these dynamics. Goering states that the model does not have the capability to represent differential treatment of minorities in housing process, but rather allows for the specification of mortgage loan discrimination against minorities, ethnic steering, and direct exclusion. He also worries that the model does not take account of how the index of dissimilarity behaves when minority populations are small, when in

²Tipping is not "handled" in my simulations, it *occurs*.

fact the model uses the best available methods for adjusting index scores for this influence and makes both adjusted and unadjusted scores available.

A second grouping involves concerns that can be addressed by empirical study, at least in principle. Neighborhood size, city size, the vacancy rate, city-suburb differences in housing value, and city demography can all be varied (within limits) in the SimSeg model to explore their impacts. Goering *presumes* these factors make a difference for evaluating preference effects. For example, he authoritatively states "spatial complexity and scale matter." They may matter for some things, but I know of no theoretical or empirical basis for assuming they matter for my findings regarding preference and social distance effects. I have explored many of these issues in a methodological analysis that implemented simulations in which spatial scale and complexity were varied in many ways (Fossett and Dietrich, 2005). These analyses used large cities and small cities, large neighborhoods and small neighborhoods, fixed boundary neighborhoods and overlapping site-centered neighborhoods, neighborhoods with different shapes (i.e., diamonds, circles, and squares), distance-decay functions for weighting neighbors by distance, and so on. We found Schelling-style preference effects to be extremely robust across all combinations of variations in these spatial complications and spatial scales. In other methodological studies, I have measured segregation using a wide range of familiar segregation indices such as the index of dissimilarity, the gini index, the revised index of isolation, Atkinson's index, and Theil's entropy-based measure. I have not encountered evidence to support Goering's suggestion that Schelling-style preference effects are fragile and contingent upon particular choices for measuring segregation. These kinds of questions can be directly explored empirically. Macy and Rijt illustrate how this can be done in a constructive way. Goering's laundry list of criticisms is less helpful in advancing the issues because it is not accompanied by either empirical analyses or thoughtful arguments as to why key substantive findings from research on model systems will be crucially tied to particular aspects of model specification.

A third grouping of concerns consists of what I view as misplaced criticisms, at least relative to the goals I adopt for my analysis. My simulation analyses explore preference and social distance dynamics in an abstract world where complicating factors can be eliminated or controlled. This is a strategy of scientific inquiry that is widely used in many disciplines. Adding complications to the model in the form of roads, gangs, garbage disposal plants, discrimination, etc. is appropriate only when it serves the purposes of the study. I use simulation

methods to *eliminate* these complications to establish the effects of preferences under carefully specified conditions. I leave the task of pursuing more complicated models to future research as theory and debate warrants. Many sociologists do not appreciate this approach to theory building, but I believe it has much to commend it. Goering presents a mixed message on this issue. He sometimes criticizes the model for being abstract and intentionally narrow.³ Other times he worries that the model is too complicated and thus may be untrustworthy.

In the end, I come away with a strong sense that Goering's specific objections about the model are not really his main concern. Perhaps he is open to reconsidering his views on preference should future research demonstrate that the segregation behavior of the model is robust under alternative specifications that address his objections. However, he gives every impression of being confident that he knows the answers to the questions I view as open to debate and further inquiry. Based on this advantage, he can evaluate my model based on whether it produces correct answers. This is suggested in part by his reference to the Galster-Clark exchanges which, in his view at least, settled the standing of preference theory once and forever in Galster's favor. I assess the Galster-Clark exchange much differently than Goering and see the debate on preference theory as far from settled. I find it curious that most critiques of preference theory are discursive in nature and do not outline models of how preferences and social distance dynamics are irrelevant. For example, critics have rarely engaged Schelling's celebrated theoretical treatments directly and certainly have not refuted them. To date, at least, the work of researchers who have used model-based methods to raise questions about Schelling's findings (e.g., Laurie & Jaggi, 2003) has not fared well when subjected to close scrutiny (e.g., Fossett & Waren, 2005). Even at the discursive level, critics are quiet on key points raised by preference theorists. Would Goering have us believe there are no implications whatsoever from the fact that some survey data show that approximately a third of black respondents state they have no compunctions about entering predominantly white areas, but also state that they prefer to live in neighborhoods with substantial (50% or more) black presence.

I can appreciate anyone being cautious about embracing the results of new modeling efforts. However, I do ask that, in academic circles if nowhere else, these efforts be given fair consideration and be

³Macy praises the model on this count. This appears to highlight a basic difference in Goering's and Macy's views about theory and research strategy. I share many of Macy's views.

evaluated on grounds other than whether they confirm preconceived views and policy positions. I leave it to others to decide whether Goering gives the effort fair consideration. It appears to me that he is hasty and careless in raising objections. For example, one lengthy section of his comments argues that recent trends of modest declines in segregation pose serious problems for preference and social distance theories. The basis for this conclusion escapes me. First, we can simply set aside his suggestion that preference and social distance models imply that there will be *no* reductions in segregation under conditions of declining discrimination. I know no one who holds this view. I repudiate it and assert strongly that it is not implied by my modeling framework. To the contrary, my model readily provides evidence against this view.⁴

Second, a massive body of survey evidence suggests that preferences are changing in ways that could contribute to the modest declines in segregation observed in recent decades. Rather than posing serious problems for preference theorists, Goering lends some credence to their position. Preference theory indicates that increasing tolerance on the part of whites for low levels of contact with minorities creates opportunities for greater integration. Relevant analyses (e.g., Simulations 3 and 5) in my paper show that, in abstract models, reductions in preferences for same-group contact lead to reductions in segregation (but not necessarily to low levels of segregation). Thus, Goering's allusion to "simulated failure" is completely off target. Similarly, his review of evidence that discrimination is declining and changing form fits nicely with the arguments of those who believe questions about preference effects are increasingly salient. These mistakes suggest Goering does not take preference and social distance theory seriously. The simplistic implications and imagined failures of prediction he incorrectly attributes to preference theory can be found only in his representations of these perspectives.

I conclude by noting two points that commentators raised regarding directions for future research. One, research on multi-factor explanations of segregation is desirable. Goering is supportive of this general idea, but clearly he is not enthusiastic about the use of agent-based models in this regard. Clark, Macy and Rijt, and St. John see value in further research using agent-based approaches. A safe middle ground is to argue that agent-based modeling can serve as one among

⁴It is a simple matter to demonstrate this by activating and deactivating discrimination dynamics in the simulation exercises similar to those I report in my paper. When discrimination dynamics are activated, segregation increases to even higher levels. When they are deactivated, segregation drops back to lower but still high levels.

many approaches researchers can use to help move theory and research forward.

Another point of agreement is that future research and scholarship should give greater attention to the conceptual disconnect between neighborhood-level and metropolitan-level notions of integration. Macy and Rijt and St. John note that both scholars and the lay public tend to think about integration at the neighborhood level in a way that is fundamentally different from the standard thinking about integration at the city level. Preferences for ethnically diverse neighborhoods (e.g., 50/50) are viewed by many as noncontroversial, even socially laudable. In most cities, however, preferences for diversity will not promote proportional representation relative to the city's ethnic mix. Instead, realizing goals for diversity in some neighborhoods necessarily increases homogeneity in other areas of the city and promotes higher scores on standard segregation indices. Quinn and Pawasarat (2003) and Quinn (2004) have expressed concern about this issue, arguing that standard segregation indices are rooted in an implicit view that majority-minority neighborhoods are undesirable regardless of whether desired by minority households or not. The issues are thorny. Should *any* departure from uneven distribution automatically be seen as an undesirable state of affairs? The literature tends to project this view, possibly based on the assumption that segregation does not reflect ethnic community but is involuntary in every respect and would quickly give way to integration in the form of even distribution if discrimination and other constraints were eliminated. Preference and social distance models raise questions about the validity of such a strong and simplistic assumption. For this and other reasons I have noted, I believe these models and their implications deserve further attention and study.

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