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Research Article

A Comparison of Aspatial and Spatial Measures of Segregation

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Abstract

The modifiable areal unit problem arises when the boundaries that define neighborhoods affect perceived levels of segregation. Scholars postulate that this problem is exacerbated when one uses a definition of neighborhoods that is based on administrative units; doing so leads to an aspatial measure of segregation, which may or may not adequately account for the spatial relationships among residential locations. In this article, we assess whether aspatial and spatial definitions of neighborhoods produce different perceived levels of income segregation. Using an original individual-level dataset on income in San Mateo County, California, we define each individual's neighborhood in three ways – two aspatial and one spatial. On the basis of these definitions of neighborhoods, we then estimate residential income segregation using the local Moran's *I* statistic. We report two primary findings. First, the three measures generate different perceived levels of income segregation. Specifically, we observe *less* income segregation when using the aspatial measures as compared with the spatial one. Second, the inconsistencies between these measures are systematic in such a way as to lead to different inferences when used to predict individual voter turnout.

1 Introduction

Measuring residential segregation requires that we: (1) define the neighborhood of each individual; and (2) quantify the extent to which these neighborhoods vary across individuals. The most common measures of segregation – including the widely used dissimilary index – are aspatial (Grannis 2002; Massey and Denton 1988; Morrill 1991; Reardon and Firebaugh 2002; Reardon and O'Sullivan 2004; Wong 1993, 2002). These measures define neighborhoods as administrative units with exogenously established boundary lines. All individuals within those boundary lines are said to be members of the same neighborhood,

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and therefore are affected equally by their neighborhood, regardless of the individual's spatial location within the boundary lines. As such, these measures may not adequately account for the continuous spatial properties of neighborhoods.

With GIS, we are now able to measure segregation using a definition of neighborhoods that is, indeed, spatial.[1] Rather than stating that each individual within a specified areal unit is equally affected by a fixed neighborhood, we suggest that each individual is a member of a unique neighborhood. That neighborhood is centered at the individual, and emanates outward from that center, forming a circle around the individual. Further, within that circle, we posit that individuals consider those who live closer to the center of the circle to play a more pronounced role in their neighborhood. Thus, neighbors who live close to the target individual are weighted more heavily than those who live farther away.

These two competing definitions of neighborhoods – one aspatial and one spatial – require shifting the areal unit that defines the neighborhood. Whenever the areal unit of analysis changes in measures that may contain spatial correlation, researchers must account for the modifiable areal unit problem (MAUP). As noted by Openshaw (1984), "the areal units (zonal objects) used in many geographical studies are arbitrary, modifiable, and subject to the whims and fancies of whoever is doing, or did, the aggregating." Therefore, the units chosen can induce bias into the measure of segregation.

MAUP can be decomposed into two constituent effects: a scaling effect and a zoning effect (Wong 1997). The scaling effect refers to differences in measures caused by aggregating data at different levels of geographical size. A measure of segregation may produce different results when the areal unit is geographically small (such as a census block) than it does when the areal unit is geographically large (such as a census block group or census tract). The zoning effect, meanwhile, describes variation in statistics caused by the regrouping of data into different configurations at the same scale.

MAUP and its constituent effects are widely acknowledged problems in the neighborhood effects literature (Massey and Denton 1988, Reardon and O'Sullivan 2004, Wong 1997). However, scholars have disagreed about the relative magnitudes of these two effects. Some have argued that the zoning effect does not lead to biased estimates of segregation, but rather that scaling is the more biting problem (Cowgill and Cowgill 1951, Iceland and Steinmetz 2003). Cowgill and Cowgill (1951) say that valid measures of segregation can be calculated as long as neighborhoods are defined with sufficiently small areal units, such as census blocks. Others contend that both the scaling and the zoning effect introduce bias into the measure of segregation (Fotheringham and Wong 1991, Openshaw 1984, Reardon and O'Sullivan 2004, Wong 2008).

Previous studies that have attempted to evaluate the magnitude of bias induced by MAUP and its constituent parts have been constrained by data limitations: scholars, often, must rely on data that is only available for administrative units. We circumvent this obstacle by utilizing a novel individual-level data set for the population of San Mateo County, California, that contains household incomes. We geocode the individuals in this data set to construct three measures of income segregation using the local Moran's *I* statistic. In the first measure, neighborhoods are defined as census blocks. In the second measure, neighborhoods are defined as census block groups. In the third, neighborhoods are defined as emanating from each individual (as explained above), and having the same geographical area as the average block group in the county. Hence, the former two measures of income segregation are aspatial, and the latter is spatial.

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Each of the three measures generates different perceived levels of income segregation. This leads us to two primary findings. First, we observe *less* income segregation when the local Moran's I is computed with the two aspatial definitions of neighborhoods. This difference is not a result of a scaling effect; nor can it be accounted for by shifting to a definition of neighborhoods based on smaller administrative units. Second, the inconsistencies between these measures are systematic in such a way as to lead to different inferences when used to predict individual voter turnout.

The article proceeds as follows. In Section 2, we describe our data and the construction of the three measures of segregation. Section 3 provides a discussion of the modifiable areal unit problem and its constituent parts, illustrated using our data. In Section 4, we provide descriptive statistics for the three measures of segregation calculated using different definitions of neighborhoods. Having shown that the definition of neighborhoods influences the measurement of segregation, we address whether these differences lead to biased causal inference. We regress individual voter turnout on each measure of income segregation in Section 5 to address this empirical question. Section 6 concludes and considers future avenues for research.

2 Measuring Residential Income Segregation

To what degree do aspatial versus spatial definitions of neighborhoods influence perceived levels of income segregation? We attempt to answer this question by comparing measures of income segregation calculated using three different definitions of neighborhoods. Each measure of segregation is calculated with the same individual-level data on income, allowing us to isolate the effect of changing the definition of neighborhoods.[2] These data come from the San Mateo County postal service list. To obtain a proxy for income, we match individuals' addresses with the price of their homes, obtained from Zillow.com in October and November of 2010.[3] Of the 496,881 individuals in our data set, we are able to match 282,445 with an estimated home value.[4] These individuals constitute our sample. The distribution of home values has a pronounced right skew; when the log is taken, though, the distribution is approximately normal. Thus, when computing neighborhood income segregation, we use a logged version of this variable.

We define each individual's neighborhood in three ways. First, we measure neighborhoods as census block groups. This definition is consistent with much of the extant literature (e.g. Elliott et al. 1996, Coulton et al. 1999, Cohen et al. 2000).[5] To assess the effect of zoning, we next define neighborhoods using inverse distance (as explained in the introduction). To operationalize inverse distance neighborhoods, we first geocode individuals' addresses from the postal service list.[6] Then, we use ArcGIS to calculate the unique neighborhood of each individual. Space is conceptualized as continuous, and individuals are considered to be influenced by all those neighbors within a set circle of influence – though nearer neighbors have more influence over the target individual than farther away ones (at a rate of 1/distance). The circle of influence for these inverse-distance based neighborhoods is set as 1154.75 m. This is the radius that results in each circle of influence having an area equal to that of the average block group in San Mateo County, allowing us to hold constant the geographical size of the neighborhood. Therefore, if this spatial measure of segregation differs from the aspatial measure outlined above, this difference is independent of the scaling effect, and hence attributable to the zoning effect.

Finally, we vary the size of administrative units, defining neighborhoods as census blocks. Since both census block groups and census blocks are administrative units, they

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generate aspatial measures of segregation. If these two aspatial measures are inconsistent, we can conclude that MAUP is present when comparing differentially scaled administrative units.

Having defined neighborhoods, we need to quantify the extent to which segregation in these neighborhoods varies across individuals. To do so, we calculate the local Moran's *I* statistic. For each individual, three spatial weights matrices – one for each definition of neighborhoods – are calculated.[7] These weights matrices are then used to estimate the local Moran's *I* statistic in ArcGIS. This statistic captures the degree to which each individual is clustered or dispersed in space. Specifically, each individual is defined as high or low, based on whether or not her income exceeds the global mean (i.e. her log house price exceeds the mean log house price in San Mateo County, which is equivalent to $566,266). Then, each individual's neighborhood income is defined as high or low, according to whether the weighted average of all neighbors' incomes is above or below the county mean. Based on these classifications, individuals are placed into four mutually exclusive categories. Each category references both the individual's income and the neighborhood income, with the former always preceding the latter. If an individual is segregated (i.e. clustered), she is categorized as high-high or low-low. If an individual is an income outlier, she is classified as high-low or low-high. If the individual's income and her neighborhood income do not jointly significantly deviate from the county mean, the p-value on the *I* statistic will not exceed 0.05, and the observation is classified as insignificant.[8] We henceforth refer to the measure of segregation (based on the local Moran's *I* statistics) estimated with block groups as the definition of neighborhoods as *BG*; the measure estimated with the inverse distance definition of neighborhoods is referred to as *ID*. Last, we refer to the measure estimated with the definition of neighborhoods as blocks as *B*.

The local Moran's *I* statistic possesses a number of desirable properties for our purposes. First, using this statistic, we can calculate separate estimates of segregation for each individual in the data set. This is not true of some more prevalent measures of segregation, like the dissimilarity index, which are generally used to summarize segregation in a larger geographical area, such as a county or census tract. Second, the local Moran's *I* allows the researcher to define a neighborhood in a way that is consistent with his or her theory of neighborhood effects by specifying the structure of the spatial weights matrix. In our case, we use this statistic to generate measures of spatial segregation with the neighborhood defined as the block, block group, and inverse distance area. By using the same statistic – and the same data – to operationalize income segregation across different definitions of neighborhoods, we are able to draw valid comparisons that shed light on how MAUP affects the measurement of segregation.

3 Empirical Illustrations of MAUP

The modifiable areal unit problem is not, in fact, problematic unless: (1) individuals' neighborhoods are not contained within areal units (the zoning effect); and/or (2) high levels of aggregation obfuscate local patterns of segregation or dispersion (the scaling effect). This section discusses three ways in which MAUP may manifest. First, we may find that neither the zoning nor the scaling effects influence the measurement of segregation. Second, it may be the case that the zoning effect exists independent of the scaling effect. Last, the zoning and scaling effects may jointly influence the measurement of segregation.[9]

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The case in which neither the zoning nor the scaling effects influence the measurement of segregation is, perhaps, the most intuitive. Standard economic approaches predict that individuals sort into homogeneous neighborhoods (Alonso 1964, Schelling 1969, Tiebout 1956). If this is true, individuals will live with those who have similar incomes, and switching the definition of neighborhoods should not change perceived levels of income segregation. Indeed, it is not difficult to find examples in which neither scaling nor zoning is problematic in San Mateo County. For example, in the extremely affluent town of Atherton, residents are almost universally above the county mean income. Switching the definition of neighborhoods does not change that these individuals live in high-high clusters. Similarly, when we measure segregation in the poor city of East Palo Alto, shifting the definition of neighborhoods does not change that residents are classified as low-low.

However, scholars have found that there is substantial income heterogeneity within neighborhoods (Farley 1977, Massey and Fischer 2003, McKinnish et al. 2010, Fischer 2003). It is in these cases that MAUP is particularly problematic. One instance in which we observe neighborhood income heterogeneity is in the city of Millbrae. In Millbrae, the income distribution is bimodal. In the south, the demographic composition mirrors that of Burlingame, its wealthy southern neighbor. In the north, however, it resembles San Bruno, the relatively poor city sharing its northern border.

Figure 1 shows segregation as measured by *ID* in Millbrae (traced by the turquoise line). The figure focuses on two individuals in the same block group, one living in the southern corner of the block group, and the other in the northern corner. The southern individual's home has an estimated value of \$735,500, leading to the classification of this individual as high income. The northern individual's home has an estimated value of \$117,500, leading to her classification as low income. Though these individuals live in the same block group, we expect that they consider themselves to be part of different neighborhoods. Most of the individuals within 100 m of both individuals share their respective income classifications; the same is true when we expand the distance band to 500 m. Only when we relax the distance band to 1154.75 m (the average area of a block group) are the two individuals technically in the same neighborhood; however, they are very much on the periphery, and the majority of those within this band are members of the same income group.

Figure 1 is illustrative of a case in which the measurement of segregation is influenced by the zoning effect *independent of the scaling effect*. Specifically, consistent with the zoning effect, we observe different levels of segregation with *BG* than with *ID*. The block group is, on average, low income. The inverse distance neighborhood is high income for the southern, wealthier individual, and low income for the northern, low-income individual. Hence, with *BG*, the low-income individual is classified as low-low and the high income individual as high-low. However, with *ID*, both individuals appear to be segregated. That is, the low-income citizen is still in a low-low cluster, but the high-income individual is now in a high-high cluster. Thus, switching from an aspatial to a spatial definition of neighborhoods (while holding constant the level of aggregation) affects the measurement of segregation for the individual whose income classification does not match the block group's. Furthermore, when a spatial definition of neighborhoods is adopted rather than an aspatial one, we observe *greater* income segregation.[10]

The zoning and scaling effects may jointly influence the measurement of income segregation, as is illustrated in Figure 2. The figure shows a block group in Millbrae that is, on average, wealthy relative to the county mean. Most of the blocks within this county

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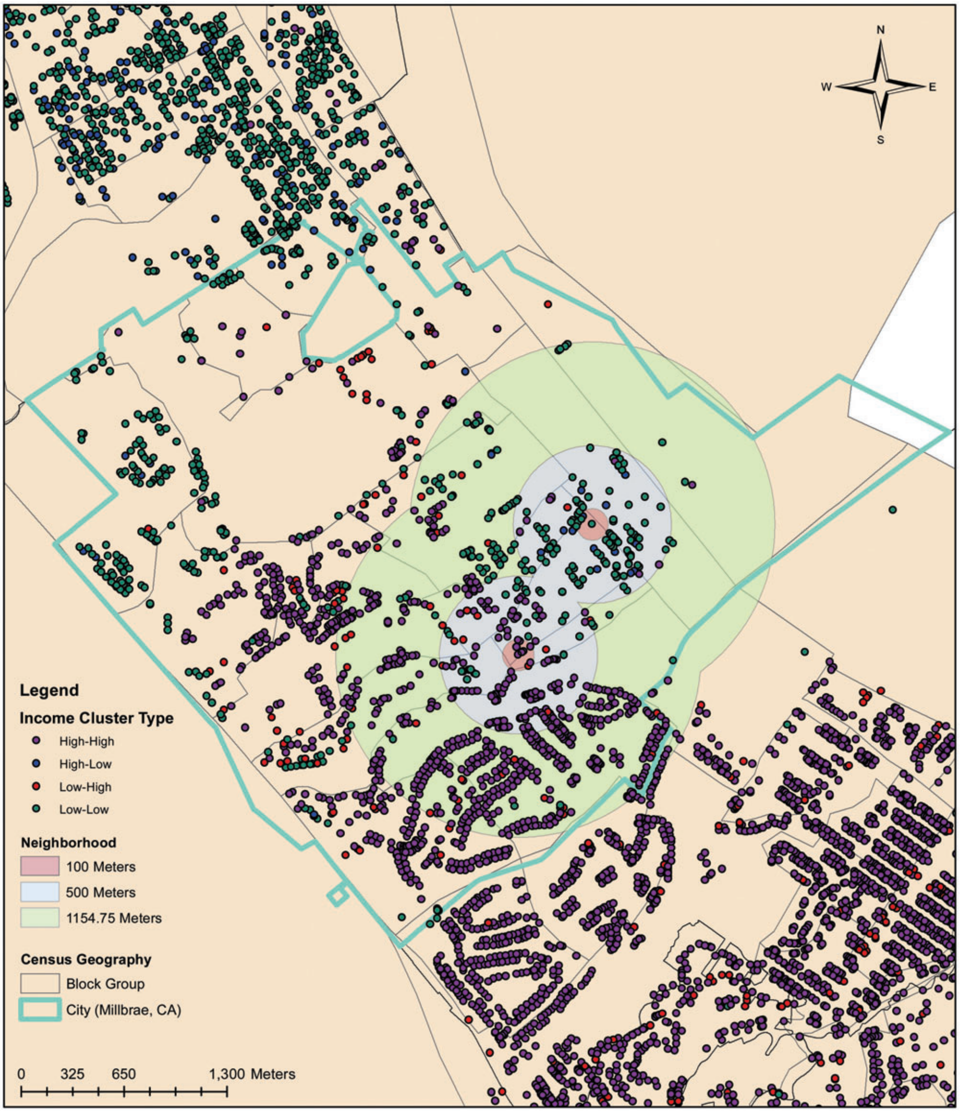

Figure 1 Income segregation in Millbrae. The income cluster types shown here are based on the spatial measure of segregation (*ID*)

are fairly homogeneous and high income, with housing values typically ranging from $900,000 to $1.6 million. On the western border of this block group, there are a few lower income blocks, such as the one outlined in pink in Figure 2. This block is predominantly comprised of houses valued between $100,000 and $500,000 (below the county mean). With *BG*, the low-income individuals in this neighborhood – such as the individual indicated with a star in Figure 2 – are classified as low-high, thus appearing to be income outliers. However, in their more immediate neighborhood, they are segregated. Consequently, with *B*, low-income individuals in this block are classified as low-low.[11] The two aspatial measures of segregation, then, produce inconsistent esti-

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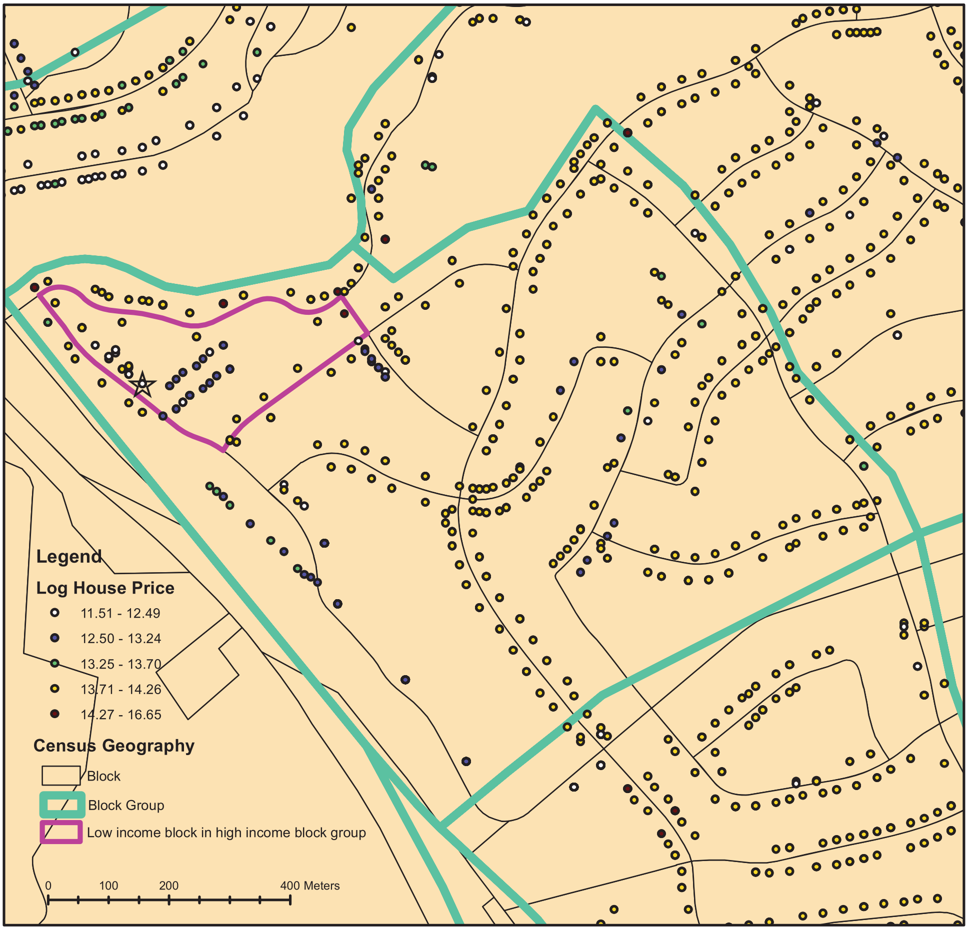

Figure 2 Distribution of income in census block and block groups in Millbrae

mates of segregation. Since *BG* and *B* are estimated with neighborhoods that differ in their areal units *and* levels of aggregation, both constituent elements of MAUP are jointly influencing the measurement of segregation.

4 Descriptive Statistics

We have provided examples of ways in which the zoning effect and the scaling effect may introduce bias into measures of segregation. However, it does not follow deductively that these effects will lead to different measures of segregation when we consider the three definitions of neighborhoods proposed above. This, at its core, is an empirical question. To answer this, we turn now to data. We find that all three measures of income segregation (*BG*, *ID*, and *B*) are notably distinct from each other.

To assess whether the zoning effect introduces bias into the measurement of income segregation, we begin by comparing *BG* and *ID*. Table 1 presents the cross-tabulation of these two computed statistics, with the number of observations classified into each of the

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Table 1 Comparison of *BG* and *ID*

Neighborhood: Census Block Group	Neighborhood: Inverse Distance					
	High-High	High-Low	Low-High	Low-Low	Insignificant	Total
High-High	47,030	236	0	0	37,163	84,429
	55.70	0.28	0.00	0.00	44.02	100.00
	93.78	10.28	0.00	0.00	21.60	29.89
High-Low	1,311	1,891	0	0	43,078	46,280
	2.83	4.09	0.00	0.00	93.08	100.00
	2.61	82.40	0.00	0.00	25.03	16.39
Low-High	0	0	2,032	7,282	13,149	22,463
	0.00	0.00	9.05	32.42	58.54	100.00
	0.00	0.00	89.52	13.08	7.64	7.95
Low-Low	0	0	119	45,319	59,343	104,781
	0.00	0.00	0.11	43.25	56.64	100.00
	0.00	0.00	5.24	81.43	34.49	37.10
Insignificant	1,809	168	119	3,055	19,341	24,492
	7.39	0.69	0.49	12.47	78.97	100.00
	3.61	7.32	5.24	5.49	11.24	8.67
Total	50,150	2,295	2,270	55,656	172,074	282,445
	100.00	100.00	100.00	100.00	100.00	100.00
	17.76	0.81	0.80	19.71	60.92	100.00

Each cell contains (in order of top to bottom) the raw count, the row percentage, and the column percentage.

five categories based on the local Moran's I (high-high, high-low, low-high, low-low, insignificant). The first number in each cell provides the raw cell count. The second number is the row percentage. The row percentage indicates the likelihood that an observation is in a given column, conditional on being in a given row. The third number is the column percentage.

If *BG* and *ID* were perfectly correlated, every observation would fall into one of the five diagonal cells in Table 1, and the row and column percentages in each of these five cells would be 100%. This is clearly not the case. Two discrepancies between the measures are of particular note. First, among significant observations, *ID* detects a great deal more income segregation than *BG*. With *ID*, approximately 96% of significant individuals are identified in either a high- or low-income cluster, while 4% are income outliers. In contrast, with *BG*, 73% of significant individuals are characterized as segregated, while 27% are classified as income outliers. This disparity appears to stem in large part from differences in the two measures' classifications of the neighborhoods of low income individuals: examining the off-diagonal cells, we see that *ID* classifies 32% of individuals as living in low-low clusters that are, according to *BG*, low-high outliers.

Second, looking now to the diagonal cells, we find that the greatest difference in the two measures is in their classification of insignificant observations. Reference to the marginal distributions shows that *ID* is far more likely to classify individuals as insig-

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nificant (61%) than *BG* (9%). This amounts to 147,582 more observations that are not classified as statistically significant when using *ID*. If *ID* classifies an observation as significant, that observation is likely to be placed in the same category using *BG* (ranging from 81 to 94%). However, if *ID* classifies an observation as insignificant, *BG* only classifies that observation as insignificant 11% of the time. The converse is also true; those classified as insignificant by *BG* are highly likely to be classified the same by *ID*, but being classified as significant by *BG* is not a good predictor of classification by *ID*.

We find some evidence to suggest that this difference between the two measures generates a systematic bias, as those individuals classified as insignificant by *ID* are more than twice as likely to be classified by *BG* in low neighborhoods than in high neighborhoods (60% to 29%). These facts highlight that *BG* and *ID* have observably different distributions, indicating that the zoning effect does independently affect segregation measures when comparing spatial and aspatial measures. However, we also note that, at least for significant observations, *BG* and *ID* produce relatively consistent measures of income segregation.

In order to judge whether two aspatial measures are affected by MAUP in a way that influences the measurement of segregation, we next compare *BG* and *B*. The cross-tabulation of these two measures is presented in Table 2. Contrary to previous research (Reardon and O'Sullivan 2004), we find that greater aggregation leads to higher percep-

Table 2 Comparison of *BG* and *B*

Neighborhood: Census Block Group	Neighborhood: Census Block					
	High-High	High-Low	Low-High	Low-Low	Insignificant	Total
High-High	24,724	35,900	0	0	23,805	84,429
	29.28	42.52	0.00	0.00	28.20	100.00
	54.61	59.49	0.00	0.00	29.70	29.89
High-Low	15,114	18,056	0	0	13,110	46,280
	32.66	39.01	0.00	0.00	28.33	100.00
	33.38	29.92	0.00	0.00	16.36	16.39
Low-High	0	0	6,695	9,372	6,396	22,463
	0.00	0.00	29.80	41.72	28.47	100.00
	0.00	0.00	15.56	17.46	7.98	7.95
Low-Low	0	0	33,717	41,263	29,801	104,781
	0.00	0.00	32.18	39.38	28.44	100.00
	0.00	0.00	78.38	76.89	37.19	37.10
Insignificant	5,436	6,394	2,606	3,029	7,027	24,492
	22.20	26.11	10.64	12.37	28.69	100.00
	12.01	10.59	6.06	5.64	8.77	8.67
Total	45,274	60,350	43,018	53,664	80,139	282,445
	16.03	21.37	15.23	19.00	28.37	100.00
	100.00	100.00	100.00	100.00	100.00	100.00

Each cell contains (in order of top to bottom) the raw count, the row percentage, and the column percentage.

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tions of segregation. As noted above, *BG* classifies 73% of significant observations as segregated (i.e. either high-high or low-low). The corresponding figure for *B* is 49%.

More generally, we find that *BG* and *B* are poor predictors of each other. Of the 10 conditional probabilities given in the diagonal cells in this table, eight are beneath 40% (with one at 55% and another at 77%). Moreover, comparing the diagonal cell percentages to the off-diagonal cell percentages, we see that each individual's neighborhood classification in *B* is uncorrelated with that individual's neighborhood classification in *BG*. That is, knowing whether a high-income individual is classified as high-high or high-low with *BG* provides little information about the classification of that individual with *B*. The same is true of low-income individuals. This suggests that MAUP *strongly* influences the measurement of segregation when comparing aspatial measures that are estimated using neighborhoods of varying geographical size.

Last, we compare *B* to *ID*. Table 3 shows their cross-tabulation. Again, these two measures paint different pictures of income segregation in San Mateo County. While 49% of significant observations are classified as segregated with *B*, 96% are classified as segregated with *ID*. In addition, the diagonal cells indicate very little correlation between the two measures; of the 10 conditional probabilities listed, only one is above 41%. Thus, it appears that *B* is an even weaker predictor of *ID* than it was of *BG*. We can

Table 3 Comparison of *ID* and *B*

Neighborhood: Inverse Distance	Neighborhood: Block					
	High-High	High-Low	Low-High	Low-Low	Insignificant	Total
High-High	14,200	22,065	0	0	13,885	50,150
	28.32	44.00	0.00	0.00	27.69	100.00
	31.36	36.56	0.00	0.00	17.33	17.76
High-Low	738	872	0	0	685	2,295
	32.16	38.00	0.00	0.00	29.85	100.00
	1.63	1.44	0.00	0.00	0.85	0.81
Low-High	0	0	629	1,017	624	2,270
	0.00	0.00	27.71	44.80	27.49	100.00
	0.00	0.00	1.46	1.90	0.78	0.80
Low-Low	0	0	18,383	21,509	15,764	55,656
	0.00	0.00	33.03	38.65	28.32	100.00
	0.00	0.00	42.73	40.08	19.67	19.71
Insignificant	30,336	37,413	24,006	31,138	49,181	172,074
	17.63	21.74	13.95	18.10	28.58	100.00
	67.01	61.99	55.80	58.02	61.37	60.92
Total	45,274	60,350	43,018	53,664	80,139	282,445
	16.03	21.37	15.23	19.00	28.37	100.00
	100.00	100.00	100.00	100.00	100.00	100.00

Each cell contains (in order of top to bottom) the raw count, the row percentage, and the column percentage.

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therefore conclude that MAUP influences the measurement of segregation when one varies both the size and spatial nature of the definition of neighborhoods.

5 Measuring Segregation and Causal Inference

As we show in the previous section, the way in which we define neighborhoods changes the amount of segregation we observe. Is this variation across measures systematic in such a way as to change causal inferences when segregation is used as an explanatory variable? In this section, we attempt to answer this question by using our three measures of income segregation to predict voter turnout.

Voter turnout – the dependent variable in our subsequent analyses – pertains to the 2008 general election. We focus on this variable for two primary reasons. First, this is a substantive area of research that has received little attention. Despite an extensive literature connecting individual income to political participation, we are, to our knowledge, the first to examine the effects of income *segregation* on political participation. Though this is an understudied area of research, past work suggests that *residential* segregation is correlated with political participation. Previous authors have linked ethnic clustering to political participation, finding that homogeneously African American (Schlichting et al. 1998) and Asian (Cho et al. 2006) neighborhoods have higher participation. The same mechanisms linking ethnic clustering to higher political participation, such as an increased sense of political efficacy or group concern, may also link income segregation to political participation. We are able to provide the first explicit test of this hypothesis.

The second advantage of using voter turnout as our dependent variable is that data on this variable is available for the population of San Mateo County at the individual level. We would like to make inferences about how income segregation affects individual behavior; by using a dependent variable that is measured at this level of analysis, we avoid problems of ecological inference. We obtain data on individual voter turnout from the 2008 California voter file.[12] This voter file indicates whether or not each registered voter cast a ballot in every primary and general election. We create a dummy variable coded one for those individuals who turned out to vote in 2008. Individuals who did not vote (both registered and unregistered eligible voters) are coded as zero. We find that approximately 50.5% of eligible voters cast ballots; the corresponding figure is 84% among the 171,638 registered voters for whom we are able to acquire housing price data.

As our independent variables, we code five indicators of segregation or dispersion. Recall that, after calculating the local Moran's I for each definition of neighborhoods (BG, ID, and B), individuals are classified into five categories. For each measure of segregation based on the different neighborhood types, we code five mutually exclusive and exhaustive dummy variables: *high-high*, *low-low*, *low-high*, *high-low*, and *insignificant*. Individuals are coded as one if they fall into each category of segregation, and zero otherwise.

We estimate three logit models, regressing voter turnout on the five indicators of clustering or dispersion for each measure of segregation. *Insignificant* is omitted as the baseline category. Results are presented in columns 1 to 3 of Table 4. *Clarify* (Tomz et al. 2003) is used to simulate predicted probabilities corresponding to the estimated coefficients, which are given in Table 5.

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Table 4 The effect of income segregation on voter turnout (logit results)

	Block Group (1)	Inverse Distance (2)	Block (3)	Block Group (4)	Inverse Distance (5)	Block (6)
High-High	0.24**	0.40**	–0.04**	0.23**	0.23**	–0.02
	(0.01)	(0.01)	(0.01)	(0.08)	(0.06)	(0.06)
High-Low	–0.23**	–0.41**	0.31**	–0.09	–0.43*	0.23**
	(0.02)	(0.04)	(0.01)	(0.09)	(0.21)	(0.06)
Low-High	0.08**	0.44**	–0.38**	0.14	0.16	–0.21**
	(0.02)	(0.04)	(0.01)	(0.10)	(0.22)	(0.07)
Low-Low	–0.41**	–0.29**	–0.10**	–0.24**	–0.19**	–0.11^{+}
	(0.01)	(0.01)	(0.01)	(0.08)	(0.05)	(0.06)
Age				0.03**	0.03**	0.03**
				(0.00)	(0.00)	(0.00)
Gender (female = 1)				–0.06	–0.08^{+}	–0.07^{+}
				(0.04)	(0.04)	(0.04)
Constant	0.17**	0.04**	0.07**	0.98**	0.95	0.95**
	(0.01)	(0.00)	(0.01)	(0.10)	(0.07)	(0.08)
N	282,445	282,445	282,445	28,789	28,789	28,789
χ^2	5,293.78**	3,368.61**	3,071.68**	493.15**	445.56**	449.51**
Pseudo R^2	0.01	0.01	0.01	0.03	0.03	0.03

Standard errors in parentheses.
Significance tests are two-tailed.
$^{+}p < 0.10$; $^{*}p < 0.05$; $^{**}p < 0.01$.

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Table 5 Predicted likelihoods of voting (from logit results)

	Full Sample (no controls)			Full Model (with controls)		
	Block Group	Inverse Distance	Block	Block Group	Inverse Distance	Block
High-High	59.58	60.89	50.79	92.88	92.75	91.00
	[59.64, 60.33]	[60.50, 61.32]	[50.34, 51.21]	[92.30, 93.46]	[92.06, 93.44]	[90.08, 91.88]
High-Low	48.49	40.88	59.33	90.48	86.61	92.81
	[48.05, 48.92]	[38.91, 42.79]	[58.93, 59.59]	[89.47, 91.34]	[81.38, 90.79]	[92.17, 93.42]
Low-High	56.03	61.84	42.27	92.24	92.10	89.32
	[55.40, 56.67]	[59.76, 63.80]	[41.79, 42.74]	[91.05, 93.29]	[88.39, 94.79]	[88.14, 90.46]
Low-Low	43.90	43.66	49.33	89.10	89.42	90.17
	[43.61, 44.18]	[43.24, 44.06]	[48.89, 49.76]	[88.25, 89.90]	[88.44, 90.38]	[89.30, 91.04]
Insignificant	54.14	51.00	51.76	91.21	92.75	91.14
	[54.54, 54.78]	[50.77, 51.22]	[51.41, 52.11]	[89.99, 92.31]	[92.06, 93.44]	[90.40, 91.87]

95% confidence intervals in brackets.
For the full model, the baseline category is a 50-year-old female.

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We find that, when using *BG*, the average income in one's neighborhood – as opposed to the individual's income – is the most important determinant of participation. High-income neighborhoods are associated with greater levels of participation than low-income neighborhoods, regardless of the income level of the individual. For *BG*, we find that *high-high* clusters have the highest likelihood (59%) of turning out. *Low-high* outliers have a 56% chance of turning out. So, though the difference between *high-high* and *low-high* participation rates is statistically significant, it is substantively small. Participation rates for those in low-income neighborhoods drop dramatically: *high-low* outliers have a 48% chance of turning out, while *low-low* clusters have only a 44% chance of voting. Thus, we find that while high-income individuals are (in keeping with conventional wisdom), more likely to vote, the greatest determinant of voter turnout is the income level of the neighborhood.

When the independent variables of interest are based on *ID*, we again find that the neighborhood income classification is the strongest predictor of turnout. However, the difference between turnout in wealthy and poor neighborhoods is much more pronounced than with *BG*. Individuals from high-income neighborhoods have a 60 to 63% chance of turning out. Individuals from low-income neighborhoods, on the other hand, have a 39 to 44% chance of voting. Within neighborhoods, the turnout propensities of high- and low-income residents are statistically indistinguishable.[13]

When blocks are used to define neighborhoods, we obtain starkly different results. Segregation, independent of individual income, has the same effect on individuals' likelihoods of turning out: regardless of whether one lives in a *high-high* or *low-low* cluster, she has a 50% chance of voting. On the other hand, the *type* of outlier has a large effect – nearly 20% – on voter turnout. *High-low* income outliers have the greatest propensity to vote, near 60%. *Low-high* outliers are the least likely to vote, with a turnout rate of 42%. Therefore, with this measure, the neighborhood type is irrelevant, as is segregation. Instead, dispersion (and, more specifically, the *way* in which individuals' incomes differ from those of their neighbors) is the most important determinant of voter turnout when using *B*.

Some may object that the models presented here are underspecified. One reason for this is the difficulty involved with obtaining individual level data for the sample. However, the voter file does provide some information about individuals that can help mitigate concerns of omitted variable bias. In particular, it contains data on age and gender. Both variables have been shown to predict turnout (Fowler and Dawes 2008). Moreover, there is a link between each and income. Women, on average, have less money than men (Bobbitt-Zeher 2007). Age is likely to weaken the link between housing price and income (since older individuals may have bought their homes before the market spike), and is, for this reason, an especially important control. We include both variables in our model specifications, and present the results in columns 4 to 6 of Table 4. Our sample size is greatly reduced, and now only contains registered voters who, on their registration form, listed their age and gender (28,789 individuals). Predicted probabilities corresponding to the estimated coefficients are simulated with *Clarify* (Tomz et al. 2003), and presented in columns 4 to 6 of Table 5; the baseline is a 50-year-old female.

In keeping with expectations, age is a strong, positive predictor of turnout across all models. While gender is a consistently negative predictor of turnout (women, on average, are less likely to vote than men), the coefficient does not reach conventional levels of statistical significance.

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Since we are only looking at registered voters, the predicted voting propensities (shown in Table 5) increase substantially, now ranging from 89 to 93%.[14] Despite the dramatically reduced sample size, our results are relatively robust, though our coefficients are estimated with less precision. Coefficients on variables based on *BG* and *ID* again indicate that neighborhood income, rather than individual income, is the strongest predictor of turnout. Specifically, individuals in high-income neighborhoods are generally more likely to turnout than those in low-income neighborhoods.[15] In both models, individual income has no significant effect on turnout beyond that of neighborhood income.

For *B*, we again find that turnout rates among those in *high-high* and *low-low* clusters are not significantly different; individuals in both cluster types have a 90 to 91% chance of casting a ballot. Turnout rates among those in the *low-high* group are approximately 89%; thus, individuals in this category are, on average, less likely to vote than those in segregated neighborhoods, but the difference is not statistically significant. *High-low* individuals are significantly more likely to vote than those in any other category, ceteris paribus, with a 93% likelihood of voting. So, these full models tend to produce similar results to those generated with the larger sample, though the magnitude of effects appears to attenuate. And, more generally, we find consistent support for MAUP: the relationship between income segregation and turnout appears to be contingent on the definition of neighborhoods utilized in the construction of the independent variables. Shifting from *BG* to *ID* has a large effect on the magnitude of the coefficients, while using *B* leads to entirely different causal inferences.

6 Discussion and Conclusions

In this article, we explored the effects of the modifiable areal unit problem under differing definitions of neighborhoods. Specifically, we examined how MAUP influences the measurement of income segregation (operationalized with the local Moran's *I* statistic) when one switches from an aspatial definition of neighborhoods to a spatial one. Utilizing an original individual-level dataset on income and voter turnout in San Mateo County, California, we constructed three measures of income segregation – two aspatial and one spatial. We reported two primary findings. First, more income segregation is observed when using a spatial definition of neighborhoods. This difference is not a result of the scaling effect, nor is it accounted for by adopting a definition of neighborhoods based on smaller administrative units. Second, the three measures lead to different inferences about the relationship between income segregation and voter turnout, which implies that the inconsistencies in the aspatial and spatial measures are systematic.

In noting that aspatial and spatial definitions of neighborhoods produce different measures of segregation, we do not claim that one definition of neighborhoods is more valid than the other. Rather, we highlight that this definition may itself carry implications for measurement and causal inference. As such, when operationalizing residential segregation, researchers should choose a definition of neighborhoods that is consistent with their conception of how neighborhoods influence individual behavior. If neighborhoods are believed to affect individuals only through a highly local mechanism (like daily interaction), a definition based at a low level of aggregation may be most appropriate. In addition, when defining neighborhoods, scholars should consider the physical features of the social environment that they are analyzing. Census administrative units are more

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accurate in defining neighborhoods that are divided by obstacles that impede contact between subunits (like highways, bodies of water, or major intersections), since boundaries are drawn to take these barriers into account. However, physical obstacles do not separate all neighborhoods (as theoretically conceived), particularly in urban areas. In these cases, a spatial definition of neighborhoods may be more valid than an aspatial one.

While we believe that these preliminary findings strongly suggest the sensitivity of perceived income segregation to different definitions of neighborhoods, we recognize that there are limitations to our study. First, we have only looked at San Mateo County, which is an outlier with regard to its income distribution. In future work, we intend to examine additional counties with different income distributions in order to make sure that our findings are not an artifact of case selection. Second, our analysis focuses on one particular measure of segregation – the local Moran's *I* statistic. Many other measures of spatial segregation exist (e.g. Getis' Local Index, Geary's Index, the Generalized Neighborhood Sorting Index, and the Isolation and Interaction Indices). Future work would do well to explore the effect of MAUP when utilizing these alternative clustering statistics. Last, we have focused on three particular definitions of neighborhoods in order to highlight the effects of MAUP when contrasting aspatial and spatial neighborhoods. However, a multitude of neighborhoods can be defined by varying: (1) their geographical size; (2) whether they are defined aspatially or spatially; and (3) the weight that individuals place on their neighbors. Considering different permutations of these variables may yield new findings regarding the empirical implications of MAUP. Additionally, exploring new definitions of neighborhoods – both aspatial and spatial – may allow for the more precise identification of how each element in the definition of neighborhoods influences the measurement of spatial segregation.

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Notes

1 Numerous authors have used GIS to define neighborhoods using various spatial criteria (e.g. Benenson et al. 2002, Brown and Chung 2006, Chaix et al. 2005, Jargowsky and Kim 2005, Omer and Benenson 2002). We attempt to show how shifting to a spatial definition–which is more computationally intensive–changes the measurement of segregation, and inferences reliant on this measure.

2 Individual-level data is relatively common in the neighborhood effects literature. In their review of this literature, Sampson et al. (2002) find that 17 of the 40 articles they survey use individual-level data. In a few cases, authors have data for the full population in a given geographic area (e.g. Chaix et al. 2005). However, for the most part, authors obtain their individual-level data through surveys. Then, in order to measure features of neighborhoods, they must rely on data at higher levels of aggregation, such as census data on administrative units, which forces them to rely on an aspatial measure of segregation. Looking at the full population of San Mateo County residents allows us to sidestep this limitation.

3 There is evidence to suggest that housing prices are a relatively good proxy for income. A number of authors demonstrate that housing prices and income are cointegtrated (e.g.

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Abraham and Hendershott 1996, Malpezzi 1999, Capozza et al. 2002, Meen, 2002), suggesting that they are linked by a stable long-run relationship. However, housing prices are certainly an imperfect proxy for income. In recent years, housing prices have outpaced the growth of household income (Hilsenrath 2002). Houses purchased before the mid-1990s were significantly less expensive than houses purchased more recently. Between 1997 and 2002, real house prices rose 28% while real per capita personal income only rose about 15%. During the previous 20-year period, real house prices rose only 8% while real per capita income rose 35% (Gallin 2003). Thus, individuals who bought their homes prior to the housing market boom may have low incomes relative to their 2010 home value. We appreciate this limitation as a necessary one, as housing prices are, to our knowledge, the most accurate proxy for income available at the individual level (the lowest level of aggregation at which the census provides this information outside of a secure data center is the block group). Moreover, we attempt to mitigate this concern in our analysis by controlling for age (we assume that older individuals are more likely to have purchased their homes earlier).

4 For point of reference, the U.S. Census estimates that, between 2005 and 2009, the population in San Mateo County over the age of 18 is 544,073 (United States Census Bureau 2005–2009).

5 Even more common is to define neighborhoods as census tracts. We are unable to do so due to computational limitations.

6 When we obtained the data, addresses had already been converted to latitudes and longitudes. We used the "Display XY Data" tool in ArcGIS to geocode these coordinates, and, in this way, we were able to geocode all 282,445 individuals in the sample.

7 For theoretical reasons, we do not row standardize. We believe that individuals living in isolated areas are less affected by each of their neighbors. Row standardization would create the opposite effect.

8 For a more thorough treatment of the local Moran's *I* statistic, see Anselin (1995).

9 We do not discuss two other hypothetical possibilities: that the scaling problem exists independent of the zoning problem, or that both problems exist but cancel each other out. The former is not logically possible. Meanwhile, the case in which both problems exist but cancel each other out is observationally equivalent to that in which neither effect exists.

10 We would further note, for the two individuals in this example, *BG* and *B* produce the same measures of segregation.

11 This is commonly referred to as the checkerboard problem.

12 Using voter lists to study turnout has some great advantages, but also significant limitations. One advantage of voter lists is that turnout is recorded for the *entire population*, allowing us to, on the basis of addresses, match each individual's income information and turnout behavior. In this way, we avoid the ecological inference problem that plagues designs that look at aggregate turnout to make inferences about individual level behavior (King 1997). Second, these lists record observed behavior; therefore, we do not have to worry about over-reporting, which is inherent to surveys that ask about voter history (see Clausen 1968, Anderson and Silver 1986, Bernstein et al. 2001). However, we are limited by working with voter files, as these lists do not contain the dearth of attitudinal questions that are in most surveys (Dyck and Seabrook 2010). Also, this measure is flawed to the degree that voter turnout lists are inaccurate. Voter registration lists, like any large-scale data entry project, are prone to data entry errors on names, addresses, and other information recorded in the file (McDonald 2007). Further, voter lists contain the names of individuals who have moved or passed away (Burden and Ezra 1998). Though the true amount of error in voter lists is unknown, and, thus impossible to adjust for (Rhine 1995), scholars have estimated that the amount of "deadwood" on registration lists ranges from 9 to 20%, depending on the state (Knack 1995; Piven and Cloward 1988, 1989; Squire et al. 1987).

13 This may be due to the small number of individuals who are counted as outliers, according to this measure; as a result of low cell counts, the standard errors on *high-low* and *low-high* are much larger than those on the cluster variables. Perhaps with greater variation in these categories, we would see results more similar to those generated by the *BG* variable (i.e. we would be able to distinguish the voting probabilities of high- and low-income individuals from the same neighborhood types).

14 These predicted probabilities may seem surprisingly high, since, in the larger sample, registered voters turnout at a rate of 84%. This effect is likely due to the fact that our truncated sample

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consists only of those individuals who listed their demographic information on their registration forms; taking the time to fill out the form more completely is plausibly correlated with a higher propensity to vote.

15 The coefficients on *high-low* and *low-high* are not statistically distinguishable in the *ID* model; this is, in large part, due to a lack of variation in these variables in the truncated sample. Nevertheless, the point estimates reflect the same patterns in the data as were previously found.

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