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Income Sorting: Measurement and Decomposition

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Abstract

Segregation of households on the dimension of income at the jurisdictional level is interesting to economists because, under some conditions, it is an equilibrium condition in the political economy models of jurisdiction choice that follow from [15]. This paper addresses the measurement of household income sorting across jurisdictions and the attribution of observed sorting to a pure Tiebout mechanism. A standard decomposition of income variance into within- and between-jurisdiction components is biased downward by roughly 50 percent due to measurement error and differences between transitory and permanent income. Adjusting US Census data accordingly, an average across US Metropolitan Areas (MSAs) of six to eight percent of income variation can be explained by differences across jurisdictions; approximately 21 percent in the much-studied Boston MSA. Variance decomposition overstates the role of locally provided public goods because jurisdictions are differentiated by both government and location. Comparing pairs of adjacent, randomly defined “neighborhoods” in the Boston MSA, I find that boundaries between physically adjacent jurisdictions explain no more than three to four percent of income variation.

JEL Classification Codes: D31, R21

1 Introduction

The causes, extent and consequences of the segregation of demographically heterogeneous populations into relatively homogeneous neighborhoods and jurisdictions are objects of considerable interest among economists and social scientists generally.¹ Segregation on the dimension of income (“income sorting”) at the jurisdictional level is particularly interesting because, under some conditions, it is an equilibrium condition in the political economy models of jurisdiction choice that follow from [15].² Whether local political economy has a large or small effect on housing choice is important for the social choice of school financing mechanisms.

This paper addresses the measurement of income sorting and the attribution of observed sorting to different causes. Using 1990 and 2000 US Census data, I find that the adjustment for measurement error and differences between permanent and transitory income increases a standard estimate of the extent of sorting by roughly 50 percent. On average, across all US metropolitan areas (MSAs), I find approximately six percent of the variation in household income within MSAs can be explained by differences across jurisdictions. Sorting is different from zero statistically in almost all MSAs, but the extent is generally quite small and varies widely across regions; the fraction of income variance explained by differences across jurisdictions ranges from less than one percent to almost 25 percent.

That substantial income variation exists within jurisdictions is hardly surprising given the many dimensions of preferences that enter housing choice and the visible differences in housing quality and locational amenity within even small jurisdictions. Indeed, [10] shows that most of the variation in US household income survives to the level of very tightly drawn neighborhoods. Further, empirical observation of significant differences in incomes across jurisdictions, combined with the fact that there are differences in public goods across jurisdictions, cannot be interpreted as proof that differences in government drive, or even enable income sorting in a “Tieboutian” fashion. Jurisdictions are differentiated not only by government but often by geographic amenity and housing quality. Difficulties in estimating

¹See, for example, [17], [2], [11], [8].

²Examples include: [5] [7] and [16].

hedonic values for location and amenity are compounded by the fact that amenity characteristics such as school performance are likely to be determined in part by the characteristics of the households using the amenity.³ Thus, if households sort by preferences over geography and not at all on the basis of public goods, we will find high income households in the more geographically desirable locations and also likely superior school performance and lower crime.

Two approaches to identifying the role of public goods in housing choice have come to prominence recently. [6] and [1] impose structural restrictions on choice to uncover underlying preference parameters. Another set of papers takes a more reduced form approach, using boundaries as a way around the identification problems caused by endogenous jurisdiction choice and formation. [3] shows that controlling for observables, virtually adjacent houses on opposite sides of school attendance lines within the same jurisdiction reflect quality differences in their associated schools in different prices. [9] uses the number of rivers in MSAs as a source of exogenous variation in the number of jurisdictions to estimate the effect of school choice on school quality. Both methodologies rely on the lack of unobservable amenity effects associated with being on one side or the other of these boundaries, independent of the associated difference in public goods.

I use a similar, but arguably more robust, boundary methodology to estimate the extent to which income sorting by jurisdiction is driven by differences in government, rather than locational characteristics. These extra-governmental locational characteristics might include housing quality (to the extent that variation does not simply reflect variation in present zoning) and access to regional amenities. In particular, I compare the extent of income sorting in two types of adjacent “neighborhood” pairs. The first type of neighborhood pair is a geographical split of the population of a single jurisdiction. The second type of adjacent neighborhood pairs are two half jurisdictions that are in different jurisdictions. It is natural to assume that the artificial boundaries within jurisdictions signal breaks in extra-governmental locational characteristics to no greater extent than jurisdiction boundaries, which are typically drawn as they are for a reason. If this condition is met, the difference

³As discussed in [1] and [13].

between the average extent of sorting exhibited by across-jurisdiction pairs and the average extent of sorting exhibited by within-jurisdiction pairs should be no smaller than the extent of sorting generated by a combination of geography and government (the expected extent of sorting across jurisdiction boundary pairs) minus the extent of sorting generated by purely geographic differences (the expected extent of sorting among within jurisdiction pairs).

In the Boston MSA, I find that no more than three to four percent of variation in income can thereby be attributed to political differences between adjacent jurisdictions. This is a relatively small fraction of the roughly 21 percent of income variation explained in the Boston MSA by differences across jurisdictions when the sampled population is not restricted to a small geographic area. Given that local governments are particularly powerful in the Boston area, the natural conclusion is that local government alone is not the sole, nor even dominant, cause of income segregation. The results in this paper do not imply that local government is a small factor in housing choice, because demand for location and public goods may be correlated. However, the results suggest that while [6] understate the extent of income sorting by ignoring measurement error, their analysis more seriously overstates the role of local government if the authors' caveat that some of the goods provided by jurisdictions are not governmentally provided is ignored and all differences in incomes across jurisdictions are attributed to different policy choices.

The second section of this paper discusses methodological issues in the measurement of income sorting. The third section discusses the data I use to estimate income sorting at the jurisdiction and neighborhood level within MSAs, and the fourth section summarizes the extent of sorting I find. The fifth section presents the decomposition analysis, and the sixth section concludes.

2 Measuring Income Sorting

A natural way to measure sorting by any characteristic within subregions (jurisdictions or neighborhoods within MSAs, “jurisdictions” hereafter when either can be meant) is to compare the average variance of the characteristic within jurisdictions to the variance at the

regional level.

[12] note that such a variance decomposition can be interpreted as the R^2 in a regression of the characteristic on a full set of dummy variables indicating individual residence in each of the jurisdictions. Indexing households by h and jurisdictions by j , and labeling income y , we have:

$$R^2 \equiv 1 - \frac{\sum_{j=1}^J \frac{H_j}{H} \sum_{h=1}^{H_j} \frac{(y_h - \bar{y}_j)^2}{H_j}}{\sum_{h=1}^H \frac{(y_h - \bar{Y})^2}{H}}, \quad (1)$$

where \bar{y}_j is mean income in jurisdiction j and \bar{Y} is mean income in the MSA. The numerator of the second term on the right hand side is the population weighted average of within jurisdiction variance. The denominator is the variance at the MSA level. If jurisdictions are close to homogenous, the fraction is small, and R^2 is large (with a maximum of one). If the average squared difference between households' income within jurisdiction is equal to the average squared difference between households at the regional level then there is no sorting and R^2 is zero. Decomposing total variance, we can also interpret the R^2 measure as the ratio of the population weighted average squared deviations of jurisdiction mean incomes from the population mean divided by total variance.

Two defects in the R^2 estimator of sorting must be addressed before taking it to the data. First, a well known problem associated with the unadjusted R^2 measure (1) is that increasing the number of regressors increases the expectation of R^2 in finite samples, even if the added regressors are orthogonal to the dependent variable (here, income). Hence in a world with no behavioral income sorting, MSAs with more jurisdictions would have greater R^2 values mechanically. Other widely used measures such as the Index of Dissimilarity and Thiel's index suffer from the same bias towards observed sorting when jurisdiction sizes are small.

The expectation of variance within a jurisdiction, when households are randomly taken from a sample of the MSA without replacement, is given by:

$$E \frac{1}{H_j - 1} \sum_{h=1}^{H_j} (y_h - \bar{y}_j)^2 = \frac{1}{H - 1} \sum_{h=1}^{H_j} (y_h - \bar{Y})^2.$$

Thus, replacing H_j with $H_j - 1$ in the numerator and H with $H - 1$ in the denominator of equation (1), with random assignment of households to jurisdictions we obtain an expected

R^2 of zero. With behavioral sorting, the expectation will be greater than zero.⁴ In the data I consider, populations are too large for this adjustment to make a noticeable difference.

2.1 Measurement Error

Measurement error in income will bias estimated R^2 toward zero. While R^2 itself does not have a clear economic interpretation, bias in estimating income dispersion will affect estimation of models such as [6] which use income distributions to recover preference parameters. Suppose that reported income is $y + v$, where v is mean zero and i.i.d. across households with variance σ_v^2 . Putting aside the small denominator adjustment discussed above, our estimate of R^2 becomes

$$1 - \frac{\sum_{j=1}^J \frac{H_j}{H} \sum_{h=1}^{H_j} \frac{(y_h - \bar{y}_j + v_h)^2}{H_j}}{\sum_{h=1}^H \frac{(y_h - \bar{Y} + v_h)^2}{H}}. \quad (2)$$

In expectation, randomness of v across individuals and jurisdictions gives us:

$$ER_{me}^2 = \frac{\sigma_y^2 - E(y_h - \bar{y}_j)^2}{\sigma_y^2 + \sigma_v^2} = R^2 \frac{\sigma_y^2}{\sigma_y^2 + \sigma_v^2}, \quad (3)$$

Where R_{me}^2 (R^2) denotes R^2 with (without) measurement error and σ_y^2 is the variance of true income in the population. Hence as the signal to noise ratio of income approaches zero so does the estimate of income sorting, regardless of the true level.

Measurement error v can come from several sources in cross sectional survey data. First, we are typically interested in a measure of sorting by permanent wealth rather than transitory income, but annual rather than lifetime income is reported in most survey data. This would not be a problem if annual income were simply equal to a constant fraction of lifetime income. However, this relationship is violated both by year-specific shocks to income and by a generally upward trending age-earnings profile. A young graduate student may exert at least as great a positive externality on neighbors as an old tenured professor, but will

⁴In general, adding more jurisdictions, or equalizing the population share of jurisdictions allows for a smaller value of adjusted R^2 . This only affects the expectation if behavioral sorting occurs. A finding that adding jurisdictions yields larger estimated adjusted R^2 's means only that there is sorting, not necessarily that sorting behavior is more pervasive in more fragmented regions.

show up in cross sectional data with low income. Unfortunately, widely available Census data includes only aggregate counts of income within jurisdictions and does not include covariates at that geographic level of detail. A second source of error is that households may misreport their earned income in the survey year. A third problem is that income is reported in bins in the data I use.

To estimate σ_y^2 and σ_v^2 separately, we recall the formula for attenuation bias in a regression where a single right hand side variable is measured with error. If we regress some variable Z on reported income \tilde{y} , a noisy measure of true income y ,

$$Z_h = a + b(y_h + v_h) + \epsilon_h, \quad (4)$$

then we have

$$\text{plim}(\hat{b}_{OLS}) = \frac{\text{Cov}(y, Z)}{\sigma_y^2 + \sigma_v^2}.$$

By contrast, if we find an instrument that is correlated with y , but not with v or ϵ , then the two stage least squares estimator \hat{b}_{IV} has the true coefficient on income as a probability limit:

$$\text{plim}(\hat{b}_{IV}) = b = \frac{\text{Cov}(y, Z)}{\sigma_y^2}.$$

Comparing the OLS and IV estimators yields the relationship

$$\text{plim}\left(\frac{\hat{b}_{IV}}{\hat{b}_{OLS}}\right) = \frac{\sigma_y^2 + \sigma_v^2}{\sigma_y^2}. \quad (5)$$

Multiplying observed R^2 in the jurisdictional analysis by the ratio (5) undoes the bias due to measurement error, pursuant to equation (3).

3 Data

I estimate the extent of income sorting using 2000 US Census (SF 3) data on the distribution of household incomes at the MSA and jurisdiction levels within 279 US MSAs. For each of these geographic entities, I observe the estimated number of households with 1999 income in each of 17 income ranges. I assume that all households deemed to have income in any income

bin reported the midpoint income of the bin.⁵ For example, I consider every household in the income category of \$10,000 to \$15,000 to have an income of \$12,500.

The Census Bureau aggregates data at the level of metropolitan area, physically continuous regions that arguably compose a single employment market. Incorporated jurisdictions typically compose only a portion of their encompassing MSA since some areas are not incorporated into political units below the county level. I define jurisdictions as “county subdivisions,” as organized by the Census Bureau. This level of analysis appears best to correspond to control over schools and police, to the extent that these are controlled below the county level.⁶

To estimate measurement error in income, I use a separate data set, the IPUMS 1990 Census microdata (which for confidentiality purposes does not have jurisdictional detail but does identify MSA) on household incomes, education, occupation and housing characteristics.⁷ This microdata includes a bounded integer value for income, which I transform into the midpoint of the corresponding bin that would be reported in the geography-specific SF 3 data (so that a household reporting income of \$12,300 is assigned the \$12,500 midpoint of the \$10,000 to \$14,999 bin).

I regress three different dependent variables on reported household income to obtain the OLS and IV estimates for comparison as in equation (5) above. The dependent variables are (1) the number of rooms in the household’s home, (2) the value of the household’s home if they are homeowners and (3) the monthly rent paid by households if they are renters. The IV estimate is obtained by instrumenting for the transformed income variable with one of two instruments. The first instrument is the mean income for the occupation and MSA cell in which the household head works (so that all households in the Pittsburgh, PA MSA headed by a real estate broker receive the same aggregated instrument). The second instrument is

⁵To topcoded households, I impute the jurisdiction average income for households in that category.

⁶One might argue that within-jurisdiction school attendance zones are the relevant level of analysis. However, Tieboutian sorting involves voting, which occurs at the jurisdiction level and many decisions are undertaken at this higher level rather than the school level. [9] considers variation in both the number of districts and the number of schools, with identification centering on districts.

⁷Not available for 2000 at the time of writing.

years of education. I perform these regressions in both logs and levels, at the MSA level. The assumption required for identification is that local occupation mean income and education affect housing purchases only through household income and not through any unobserved variables included in the error term ϵ in equation (4).

Neither of these assumptions is obviously tenable. Occupation mean income should be uncorrelated with individual shocks to 1989 income through measurement error or age-earnings profile effects to the extent that there is not age-based selection into occupations. However, macroeconomic shock are likely to survive to the occupation level and hence the instrument may not be purged of transitory variation. Hence we expect the IV estimate to be biased away from the true coefficient towards the OLS estimate. Considering education as an instrument, it is possible that education affects housing demand not just through income level, but also perhaps through a correlation with investment demand or access to capital.⁸

4 MSA-Level Sorting Results

I find considerable measurement error in household income, as indicated by comparing OLS to IV regressions with income alone on the right hand side.⁹ Table 1 shows a range of estimates more or less in line with those summarized in [14]. Column (1) of Table 1 reports results from regressions of the form

$$x = a + by + \epsilon,$$

where x is the number of rooms, the value of the home or the monthly rent. In each case, the coefficient estimate is the population-weighted mean ratio across the 279 MSAs. Column (2) reports an IV estimate of the same regression and column (3) reports the implied noise-to-signal ratio. I perform these regressions in both levels and logs.

Examining the result of Table 1, we see different noise to signal ratios depending on the right hand side variable, and a larger ratio when income is instrumented by education rather

⁸Using the method of moments approach proposed by [4] yields unreliable results with noise to signal ratios on the high end of the IV estimates.

⁹“Measurement error” is broadly defined to incorporate differences between permanent and transitory income.

than by occupational mean income. While this suggests that short term macroeconomic shocks persist when occupational mean is the instrument, we cannot be confident that the education instrument is valid. Further, the inconsistency of the education IV results suggest that we should not be too confident of the estimates. Using rent and price as dependent variables generates larger estimates of attenuation bias than does number of rooms. This may reflect misspecification of the rooms regression, in that rooms are unlikely to have the same price throughout an MSA; the single price assumption would be more likely to apply to a hedonic bundle of housing. Nevertheless, all specifications exhibit considerable attenuation bias due to measurement error in income. Based on these results I proceed with the analysis using the modal and median noise-to-signal ratio of 0.5.

With no correction for measurement error, across 279 US MSAs, at the jurisdiction level I find a mean R^2 for the level of income of 0.039. The unadjusted R^2 value is 0.051 in logs. Assuming a noise to signal ratio of 0.5 for both levels and logs based on the results reported in Table 1, the mean R^2 estimates increase to .057 in levels and .076 in log income at the jurisdiction level. Even with a noise to signal ratio of one, on the high end of the estimates presented in Table 1 and in [14], no more than 10 percent of income variation is explained by differences across jurisdictions on average.

The measured extent of income sorting may be small in part because a large share of population located in some jurisdictions implies that there must be some mixing of income. For example, a metropolitan area with just two jurisdictions cannot feature an R^2 value of one as long as there are more than two income categories with positive population. Large cities such as New York City are typically larger than the population in any single income category, and hence their associated metropolitan area must feature some income mixing.

Figure 1 plots two values of R^2 for each metropolitan area (for income levels). The vertical axis measures the observed extent of sorting by jurisdiction, corrected for measurement error by multiplying the observed values by 1.5. The horizontal axis is income sorting that would occur if “perfect” sorting were accomplished by locating the highest income households in the smallest jurisdictions and the poorest households in the largest jurisdictions, subject to the existing distribution of numbers of households across jurisdictions. If sorting were complete,

Table 1: OLS and IV estimates of coefficients on income

RHS Variable	Instrument	Functional Form	(1)	(2)	(3)
			OLS	IV	Implied noise to signal
Rent	Education	levels	.0034 (.0012)	.0051 (0.2228)	.5
Rent	Occupation Mean	levels	.0034	.0059 (.0020)	.7
Value	Education	levels	1.000 (.1877)	1.930 (.4873)	.9
Value	Occupation Mean	levels	1.0003	1.4816 (.3262)	.5
Rooms	Education	levels	2.42×10^{-5} (5.32×10^{-6})	3.66×10^{-5} (1.19×10^{-5})	.5
Rooms	Occupation Mean	levels	2.42×10^{-5}	3.17×10^{-5} (6.22×10^{-6})	.3
Rent	Education	logs	.2911 (.1760)	1.1221 (17.5494)	2.9
Rent	Occupation Mean	logs	.2911	.4221 (.2637)	.4
Value	Education	logs	.4580 (.0886)	.9482 (.2585)	1.1
Value	Occupation Mean	logs	.4580	.6777 (.1309)	.5
Rooms	Education	logs	.2236 (.0440)	.2880 (.1185)	.3
Rooms	Occupation Mean	logs	.2236	.2643 (.0537)	.2

Notes: Each row reports a regression of the form $X = a + by + v$. The OLS column reports the OLS coefficient on income y in a regression where X is the RHS variable. The IV column reports the estimated coefficient on income when income is instrumented for by the variable listed in the Instrument column. The estimated signal to noise follows from the comparison of the two coefficient estimates. Regression coefficients are MSA population-weighted means from individual level regressions for 289 MSAs in 1989 IPUMS Census data. The standard deviation in parentheses under each estimate reflects not the standard error of the single estimated coefficient, which we expect to vary by MSA, but rather the standard error of the estimate across MSAs.

the data would lie along the 45-degree line. The figure shows that while sorting exists, so that R^2 increases with the opportunity for larger values, sorting is highly imperfect. We also see that there is a considerable range of income sorting across MSAs. In all 279 MSAs, sorting is significant in the sense that an F-test of joint significance of dummy variables for each jurisdiction rejects the null hypothesis no effect of town mean income on household income. However, the corrected R^2 for the level of income varies from a minimum of .0001 in Lubbock, TX to a maximum of .2438 in Columbus, OH. Other highly income segregated MSAs include Philadelphia, PA (.2080); St. Louis, MO (.2074); Flint, MI (.2038); Milwaukee, WI (.2010); Cleveland, OH (.2007) and New York, NY(.1989). For log income, the similarly corrected range is from .0006 in Great Falls, MT to .3344 in Tallahassee, FL.

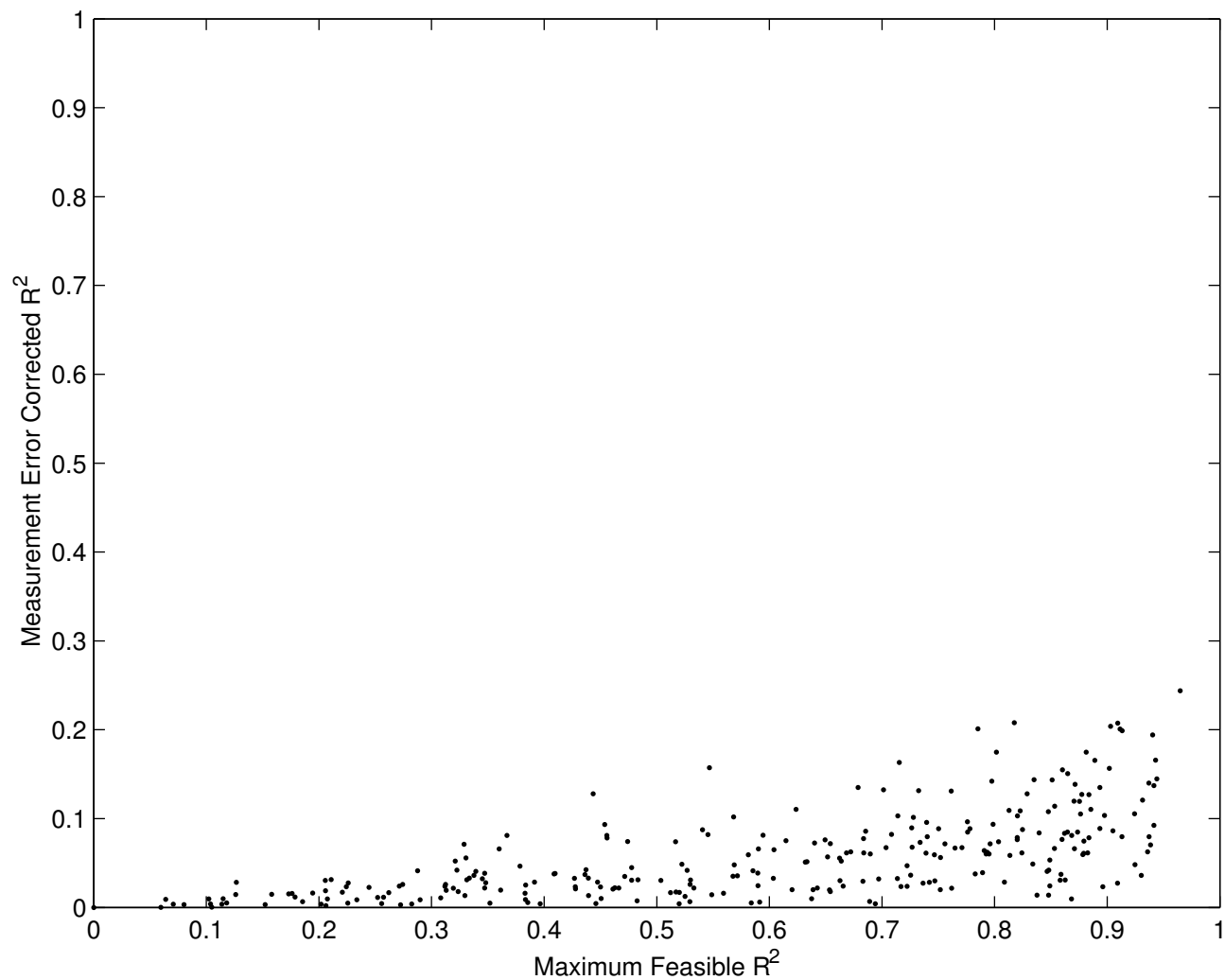
5 Decomposing Income Sorting

Beyond the mechanical correlation between feasible and actual income sorting, it is difficult to identify correlates of income sorting among the 279 MSAs. As suggested above, it would be difficult to assign a causal role to any MSA characteristic associated with sorting even if such a characteristic suggested itself in the data. However, comparing the extent of sorting along random boundaries against that along jurisdiction boundaries should provide an idea of the importance of locally provided public goods to the sorting process. Given the dominant role played by local governments in the theoretical literature on jurisdiction choice it is worthwhile to explore the empirical role.

The state of Massachusetts grants considerable power over local public goods such as schools, fire and police to jurisdictions,¹⁰ making Massachusetts a popular subject for analysis of jurisdiction choice. As we might expect, and supportive of the idea that jurisdictions drive sorting, the Boston MSA is more income sorted than average. For the 212 jurisdictions within both the state of Massachusetts and the Boston CMSA, I estimate an R^2 in the level of income as described above of .14, uncorrected for measurement error, which corresponds to a corrected value of approximately .21, assuming a noise-to-signal ratio of .5. This makes

¹⁰Subject to some limitations on the level of and changes to property taxes.

Figure 1: Income Sorting: Observed corrected R^2 values and maximized values with “perfect sorting”



Notes: Each observation is drawn from a different US MSA. Observed corrected R^2 is one minus the weighted average ratio of income variance within jurisdictions to MSA level income variance. This ratio is multiplied by 1.5 to correct for measurement error. Maximum feasible R^2 is estimated by assigning census households into maximally homogenous jurisdictions, as described in the text.

Boston the ninth most sorted of US MSAs. For log income the unadjusted figure is .10, which would be adjusted to .15 for measurement error, ranking 32nd among MSAs. The uncorrected variance decomposition results are consistent with those of [6], who find an R^2 of .11 using Boston MSA data from the 1980 census. However, without further analysis we cannot determine to what extent this income sorting is driven by differences in tax and expenditure policies across jurisdictions as opposed to different locational characteristics, such as different housing stock and access to regional amenity that are not directly related to local government policy.

To disentangle these possibilities, I estimate a sorting measure for adjacent artificially created neighborhoods. Each neighborhood lies strictly within the boundaries of a single jurisdiction. I describe the formation of these neighborhoods below. By comparing the extent of income sorting in “regions” composed of two adjacent neighborhoods when the neighborhood pairs are (a) in the same jurisdiction and (b) in neighboring jurisdictions, I can estimate the portion of income sorting that is directly attributable to local government. Under the assumption that a jurisdiction boundary involves no less separation of amenity and housing conditions than an essentially random boundary within a jurisdiction, and the admittedly unrealistic assumption that tax and expenditure policies are randomly distributed across locations within an MSA, the difference between sorting measures (a) and (b) can be interpreted as an upper bound on the sorting directly attributable to governmental differences. Realistically, underlying demand for amenity, lot size and high quality public goods are likely to be correlated. Recognizing this, a more modest use of the R^2 arising from comparisons of types (a) and (b) is to obtain an idea of how well or poorly a model in which local public goods and housing attributes approximate actual jurisdiction choice. If the difference in R^2 s of types (a) and (b) are close to the MSA-level R^2 , then we might infer that the approximation is close. If the difference is small, then we can reason that extra-governmental locational amenity should not be ignored.

I consider the assumption that a jurisdictional boundary conveys no less information on amenity than the line I draw within a jurisdiction to be a considerably weaker condition than two other boundary-related identifying assumptions that have come to prominence in the lit-

erature on local public goods. First, [3] examines the difference in property values associated with being on the side of a school attendance line associated with higher test scores, controlling for observable boundary characteristics such as major highways. For the difference to be meaningful, school attendance lines that are not observably topographically significant must not convey unobservable information about non-school neighborhood characteristics. Second, [9] argues that the number of rivers in a metropolitan area causes jurisdictional fragmentation, but does not cause economic segregation. This assumption implies that rivers do not mark changes in neighborhood characteristics when they do not mark changes in school districts.

Census block groups, the smallest geographic area for which income counts are available, are continuous geographic areas lying within census tracts, which themselves are subsets of county subdivisions. Tracts are designed to be homogenous areas within jurisdictions with respect to socioeconomic conditions, and block groups are yet more homogenous. Creating neighborhood boundaries within jurisdictions that are coterminal with block group boundaries would thus be unattractive because they would tend to overstate within-jurisdiction sorting. Instead, in each jurisdiction, I create one “neighborhood” which lies primarily in the northern portion of town and another lying primarily in the southern portion. To do this, I assign a fraction of the population in each income bin in each block group to one of the two neighborhoods. This fraction is constant across income bins within block groups, but varies across block groups. In particular, using data on the location of block group geographic centroids, I rank block groups within jurisdiction from north to south. The k th northernmost block group sends $\frac{k}{N+1}$ fraction of its population to the northern neighborhood and $1 - \frac{k}{N+1}$ to the south (one could draw something like a diagonal boundary satisfying this relationship if block groups were squares aligned in a grid. With real block group layouts, the physical boundary would be more contorted). The artificial boundary should yield within-jurisdiction sorting only because they are differentiated spatially, and we expect housing quality and access to amenity to vary spatially within jurisdictions. As one might expect, when the random neighborhoods are delineated by block groups, the difference in R^2 discussed below shrinks, suggesting an even more limited role for jurisdictional boundaries.

Whether jurisdictional boundaries are more comparable to random boundaries or to block group boundaries is difficult to know.

For each half jurisdiction, I calculate an R^2 measure as if the half jurisdiction in question along with a neighboring half jurisdiction jointly formed a metropolitan area; the larger the estimated R^2 the greater the difference in mean household incomes across jurisdiction halves relative to total household income variance, and hence the greater the extent of sorting. I estimate such an R^2 measure for each jurisdiction half along with (a) the other half of the same jurisdiction and (b) the nearest half jurisdiction to the north (for northern half jurisdictions) or south (for southern half jurisdictions). Pairs of type (a) allow estimation of within jurisdiction R^2 s and pairs of type (b) allow estimation of R^2 across jurisdiction boundaries. “Nearest” neighboring jurisdictions to the north or south are determined by comparing Euclidian distance of median block group centroids. When the neighboring relationship is symmetric, I delete one of the repeatedly observed pairs.

Turning to estimation of the effect of being in adjacent “neighborhoods” but different jurisdictions, I find that jurisdiction boundaries do, in fact, matter. Of 402 half-jurisdiction pair R^2 estimates, 196 are within jurisdiction comparisons, and they have a mean of .003 in both levels and natural logs, not correcting for measurement error. The remaining 206 variance decompositions are across jurisdiction boundaries, and have a mean of .023 in logs and .031 in levels. The difference in logs of .020 is highly significant as is the difference of .027 in levels. I present these differences in Table 2, where controls for distance are added in two specifications. The regressions have the form:

$$R_{ij}^2 = a + b * DIFJUR_{ij} + \gamma * DISTANCE_{ij} + u_{ij}, \quad (6)$$

where $DIFJUR_{ij}$ indicates whether halves i and j are in different jurisdictions, and $DISTANCE$ is Euclidian distance (in degrees divided by 1 million). Because each jurisdiction half is the basis of an R^2 variance decomposition estimate that includes another half in the same jurisdiction and another half in a different jurisdiction, including individual half fixed effects would not change the results.

We expect that $DIFJUR$ will be associated with differentiation in government policy, since pairs of half-jurisdictions within the same jurisdiction share the same government

(although there may be differences in average government service quality between neighborhoods). It is the effect on sorting of these policy differences that we wish to estimate through the coefficient b . The important question for estimation is whether $DIFJUR$ is correlated with the error term u through unobserved differences in conditions across jurisdiction boundaries that do not exist across random within-jurisdiction boundaries and are not directly attributable to government policy (or through differences that exist across random boundaries but not across jurisdictional boundaries). Such differences might include differences in housing quality (to the extent that these differences are not driven by zoning) and different topography and access to regional amenities. These unobserved differences are surely more likely to occur across non-random jurisdiction boundaries, so the coefficient b is plausibly an upper bound on the direct role of local governments.

Table 2 describes the results of the regression (6). Whether distance is included or not, we obtain almost identical estimates of the effect of being in a different jurisdiction. Correcting for measurement error in income, approximately four percent of the variation in the level of household income can be explained by being in a different jurisdiction when the different jurisdiction is nearby, and three percent of log income. This effect is statistically significant but economically small, and as discussed above is more plausibly biased upward than downward.

6 Conclusions

Jurisdictions are segregated by income relative to metropolitan areas. Correcting for measurement error in income increases the estimated extent of sorting, but sorting remains far from complete. In the Boston MSA, where jurisdictions have considerable authority, they explain approximately 21 percent of variation in household lifetime income. Observed sorting at the jurisdiction level may be generated by differences in tax and spending policies and the related quality of public goods such as schools, by differences in extra-governmental amenity and housing quality or, most likely, by a combination of these factors. The empirical results suggest that housing quality and extra-governmental amenity play large roles in

the sorting process. Jurisdictional differences account for only three to four percent of the variation in household income when the population is drawn from adjacent neighborhoods which presumably share many extra-governmental characteristics. Given these results, future development of the theory of local political economy should be embedded in a setting in which there is at least partly exogenous, spatially correlated variation in housing quality which affects households' locational choice.

Table 2: Regressions of Income R^2 for Neighboring Half-Jurisdictions on a Variable Indicating Different Jurisdictions

	(1)	(2)	(3)	(4)
DIFJUR	.0264**/ .0396 (0.0030)	.0282** / .0423 (0.0032)	.0200** / .0300 (.0016)	.0206** / .0309 (.0023)
<i>DISTANCE</i>		-.0432 (.0270)		-.0209 (.0170)
Constant	.0035 (.0021)	.0045* (.0022)	.0034** (.0015)	.0040** (.0017)
Income Measured in	Level	Level	ln	ln
Regression R^2	.16	.16	.15	.15
Observations	402	402	402	402

Notes: ** Denotes significance at 5 percent, * at 1 percent. Dependent variable is one minus the population-weighted average ratio of within quadrant code income variance to total variance in a region composed of two nearby jurisdiction halves. DIFJUR indicates whether the two halves are in different jurisdictions. The first coefficient on DIFJUR is the coefficient uncorrected for measurement error, the second is corrected for measurement error assuming a .5 noise to signal ratio. All other coefficients and all standard errors do not correct for measurement error. *DISTANCE* is the Euclidean distance (in millionths of degrees) between the centroids of each half-neighborhood.

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