Neighborhood Sorting Obscures Neighborhood Effects in the Opportunity Atlas

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Abstract: A standard practice for ordering neighborhood effects is to rank neighborhoods according to observable characteristics. However, neighborhood rankings based on observables could reflect either neighborhood effects or neighborhood sorting. The Opportunity Atlas (OA) is an innovative data set that ranks neighborhoods according to children's adult outcomes in several domains, including income. Neighborhood rankings based on outcomes could also reflect either neighborhood effects or neighborhood sorting. This note formally states the identification problem and empirically documents sorting by income, by race, and over time that suggests bias when using observed outcomes to estimate potential outcomes.

Keywords: Neighborhood Effect; Neighborhood Sorting; Opportunity Atlas
JEL Classification Codes: C21, C81, R23, I38, J15

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1 Introduction

Researchers and policymakers are increasingly interested in understanding economic mobility and inequality. What are the factors that contribute to human capital growth and translate to higher earnings and productivity? The variation in outcomes across space suggests that place could be a central determinant of these factors. In recent years interest has grown in creating policies that leverage the effects of place to improve outcomes (OI (2021)).

Researchers traditionally measure the influence of neighborhoods using the characteristics that are *believed* to affect the outcomes of a neighborhood's residents. Such characteristics might include crime rates, school resources, and the socioeconomic status of residents. But we are unable to know for certain that the characteristics we measure do indeed affect residents' outcomes.[1] This obstacle arises due to the fundamental problem of neighborhood effects research: selection bias. We do not know if an observed relationship between a neighborhood and its residents is driven by the causal effect of the neighborhood or by a systematic spatial selection process.[2] If researchers could control spatial selection, we could solve this problem by randomly assigning individuals to neighborhoods with varying characteristics. With rare exceptions, however, researchers tend to have limited influence on neighborhood sorting.

Is there any way to improve the measurement of neighborhood effects relative to traditional practice? An alternative approach to measuring neighborhood effects uses the outcomes of a neighborhood's prior residents. Chetty et al. (2020) introduce an innovative new data set called the Opportunity Atlas (OA) that is based on the outcomes of the entire 1978-1983 birth cohort in the US. The OA estimates conditional expectation functions for each neighborhood in terms of parental income, race/ethnicity, and gender using the outcomes of the children who grew up in each neighborhood. Neighborhood sorting, however, also poses an obstacle to measuring neighborhood effects using the outcomes of prior residents, as the OA does. Conceptually, observed outcomes identify neighborhood effects so long as residents' neighborhood choices are exogenous to the factors that affect outcomes. But parents make great efforts to select neighborhoods for factors they anticipate will positively affect their children's outcomes, and the selection process is highly opaque to the researcher. In the presence of such sorting, there is a risk that the potential outcome for a child deviates substantially from the observed outcomes of endogenously selected children; that is, sorting may bias these estimates of neighborhood effects.

This note formally states the identification problem under which neighborhood outcomes become biased estimates of neighborhood effects in the presence of neighborhood sorting.[3] Focusing on sorting by income, we express the bias that sorting will generate if the unobservable contributors to

[1] Appendix Figure 6 provides an example for which seemingly innocuous decisions about which characteristics of a place to measure, and at what geographic scale, fundamentally alter our inference about a neighborhood effect.

[2] Neighborhood effects are the changes in an individual's outcomes that result from residing in one neighborhood rather than another. We do not use the phrase *neighborhood opportunity* because it is unclear to us whether the phrase is synonymous with *neighborhood effects*, *neighborhood outcomes*, or both.

[3] We use the phrase *identification problem* as defined in Manski (2009) as an obstacle to inference that cannot be solved by collecting more data from the same data generating process.

neighborhood selection and outcomes are correlated. We then empirically characterize the sorting by income observed in the data: The high-income children observed in disadvantaged neighborhoods are a highly selected group, as are the low-income children observed in advantaged neighborhoods. We present simulation results to quantify the magnitude of the potential bias, and we show that sorting by race and over time are also threats to identification.

2 Identifying Neighborhood Effects

A joint model of neighborhood sorting and neighborhood effects, the two fundamental ingredients in a Rubin Causal Model (Imbens and Rubin (2008)) of neighborhood effects, allows us to express the bias from estimating potential outcomes using observed outcomes. Consider the ordered choice model developed in Heckman et al. (2006) and applied to identifying neighborhood effects in Aliprantis and Richter (2020): Suppose there are four discrete ordered neighborhoods, $D \in \{1, 2, 3, 4\}$, corresponding to the quartiles of positive neighborhood externalities in ascending order. We suppose household i has income x_i, and we specify an ordered choice model where the discrete neighborhood selection is determined by the rule

$$D_i = j \iff C_{j-1} < \lambda(X_i) - V_i < C_j$$

where $C_0 = -\infty$, $C_4 = \infty$, $\lambda(X_i)$ represents the costs of residing in a higher-ranked neighborhood for households with observable characteristics X_i, and V_i represents such unobserved costs to household i. Because the four treatment levels are defined in terms of quartiles, we will also refer to C_1 as C_{25} and C_3 as C_{75}. Potential outcomes are

$$Y_i(D) = \mu_D(X_i, U_{Di}).$$

The model represents a static, partial equilibrium.[4]

To identify neighborhood effects

$$\mathbb{E}[Y(D) - Y(D')]$$

we are interested in identifying and estimating potential outcomes in each neighborhood. Suppose that the joint probability density function of V, U_D, and X exists for each level of $D \in \{1, 2, 3, 4\}$ and can be written as $f_D(v, u_D, x)$. Assume that the marginal pdf of V exists and can be written as $f_V(v)$. Then the expected potential outcomes in the bottom and top quartiles can be expressed

[4] We do not consider a more general model in order to focus on the most fundamental identification problem. Assuming fixed levels of D is a partial equilibrium assumption because in reality treatment will be determined in part by social interactions with the residents who endogenously sort into a given neighborhood (Manski (1993)). We also abstract from social interactions in selection, often referred to as the Stable Unit Treatment Value Assumption (SUTVA, Angrist et al. (1996)), an assumption known to have major implications for identification in this setting (Sobel (2006), Manski (2013)). This model also abstracts from dynamics; see Aliprantis and Carroll (2018) for a dynamic general equilibrium model of neighborhood effects. See Graham (2018), Durlauf and Ioannides (2010), and Durlauf (2004) for literature reviews including more general models of neighborhood effects.

as

$$\mathbb{E}[Y(1)] = \int_0^{100} \int_{-\infty}^{\infty} \int_{-\infty}^{\infty} \mu_1(X_i, U_{1i})\, f_1(v, u_1, x) dv du_1 dx \tag{1}$$

and

$$\mathbb{E}[Y(4)] = \int_0^{100} \int_{-\infty}^{\infty} \int_{-\infty}^{\infty} \mu_4(X_i, U_{4i})\, f_4(v, u_4, x) dv du_4 dx. \tag{2}$$

Due to neighborhood sorting, the observed outcomes observed in the data are

$$\mathbb{E}[Y|d_i = 1] = \frac{\int_0^{100} \int_{-\infty}^{\infty} \int_{\lambda(x)-C_{25}}^{\infty} \mu_1(X_i, U_{1i})\, f_1(v, u_1, x) dv du_1 dx}{\int_{\lambda(x)-C_{25}}^{\infty} f_V(v) dv} \tag{3}$$

and

$$\mathbb{E}[Y|d_i = 4] = \frac{\int_0^{100} \int_{-\infty}^{\infty} \int_{-\infty}^{\lambda(x)-C_{75}} \mu_4(X_i, U_{4i})\, f_4(v, u_4, x) dv du_4 dx}{\int_{-\infty}^{\lambda(x)-C_{75}} f_V(v) dv}. \tag{4}$$

For a given level of income x_i, Equation 3 highlights that the households sorting into the lowest quartile of neighborhoods are the households with the largest values of V_i, and Equations 4 highlights that the households sorting into the highest quartile of neighborhoods are the households with the smallest values of V_i.

Observed outcomes are unbiased estimates of potential outcomes when the unobserved determinants of neighborhood selection and the unobserved determinants of outcomes are independent. That is, if V and U_D are independently distributed conditional on X for each D,

$$V \perp\!\!\!\perp U_D \,|\, X,$$

then observing a selected sample of V_i will not bias estimates. In that case, for a given x

$$\int_{-\infty}^{\infty} \int_{-\infty}^{\infty} \mu_1(X_i, U_{1i})\, f_{V,U_1|X}(v, u_1) dv du_1 = \frac{\int_{-\infty}^{\infty} \int_{\lambda(x)-C_{25}}^{\infty} \mu_1(X_i, U_{1i})\, f_{V,U_1|X}(v, u_1) dv du_1}{\int_{\lambda(x)-C_{25}}^{\infty} f_V(v) dv} \tag{5}$$

and

$$\int_{-\infty}^{\infty} \int_{-\infty}^{\infty} \mu_4(X_i, U_{4i})\, f_{V,U_4|X}(v, u_4) dv du_4 = \frac{\int_{-\infty}^{\infty} \int_{-\infty}^{\lambda(x)-C_{75}} \mu_4(X_i, U_{4i})\, f_{V,U_4|X}(v, u_4) dv du_4}{\int_{-\infty}^{\lambda(x)-C_{75}} f_V(v) dv}, \tag{6}$$

so that $\mathbb{E}[Y(1)|x] = \mathbb{E}[Y|d = 1, x]$ and $\mathbb{E}[Y(4)|x] = \mathbb{E}[Y|d = 4, x]$.

As long as V and U_D are not independent, though, Equations 5 and 6 will not hold and observed outcomes will be biased estimates of potential outcomes. The magnitude of the bias at an given level of income x – the difference between the left- and right-hand sides of Equations 5 and 6 – will depend on the strength of neighborhood sorting together with the joint distributions of V and the U_D. In the next section we explore these details to understand the possible magnitudes of bias: We empirically characterize the strength of neighborhood sorting and quantify bias in simulation exercises under various assumptions about the joint distributions of V and the U_D.

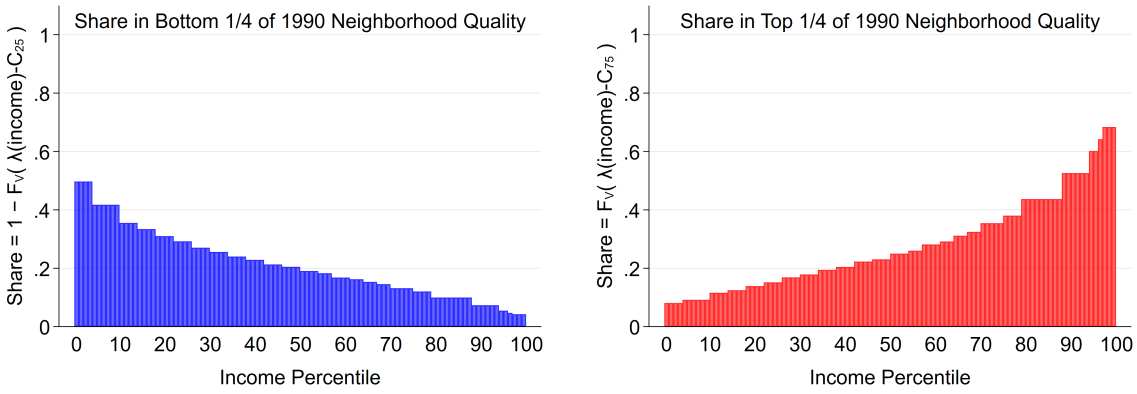
3 Neighborhood Sorting

3.1 Sorting by Income

For our empirical exercises we define neighborhoods as Census tracts and rank neighborhoods in terms of their socioeconomic status as measured by the neighborhood quality index used in Aliprantis and Richter (2020). This index is the ranking of the first principal component of six socioeconomic factors available in the 1990 decennial Census and 2014-2018 American Community Survey (ACS).[5] For the 1990 decennial Census data, when appropriate we impute count estimates into 2010 tract boundaries using the Longitudinal Tract Data Base (LTDB).[6]

We also use a ranking based on the Opportunity Atlas (OA) estimates of the average family income for children with parents at the 25th percentile of income (Chetty et al. (2020)). We often focus on the 1990 decennial Census because the OA sample is for individuals born between 1978 and 1983, and this birth cohort's age range of 6-11 in the 1990 decennial Census is likely when neighborhoods most influence children's outcomes relative to the alternative age ranges for decennial Censuses of 0-1 or 16-21.

It is well known that there is strong sorting by income in the United States (Owens (2020)).[7] Figure 4 presents US Census data characterizing this sorting in 1990. Figure 1a shows sorting by income was strong in the lowest quality tracts. Only 4 percent of highest-income households were in the bottom quarter of tracts in terms of neighborhood quality, while 50 percent of lowest-income households were in such tracts. Figure 1b shows sorting by income was slightly stronger in the highest quality tracts. Only 8 percent of lowest-income households were in the top quarter of tracts in terms of neighborhood quality, while 68 percent of highest-income households were in such tracts.



(a) Share in Bottom Fourth of Quality by Income

(b) Share in Top Fourth of Quality by Income

Figure 1: Sorting into Neighborhood Quality by Household Income
Note: The left panel shows the percent of households residing in the bottom quartile of neighborhoods ranked by 1990 neighborhood quality, conditional on household income percentile. The right panel shows the analogous percent of households residing in the top quartile of neighborhoods.

[5] See Appendix B for data details.

[6] See descriptions in Logan et al. (2014), Logan et al. (2016), and Logan et al. (2020).

[7] Trends in income sorting are more difficult to discern than levels (Logan et al. (2018), Logan et al. (2020), Reardon et al. (2018)), and patterns can vary by scale of observation (Andreoli and Peluso (2017), Ioannides (2004)).

To quantify the magnitude of the bias under various assumptions about the independence of V and U_D, we simulate child i's potential outcomes for family income in adulthood as

$$Y_i(1) = \mu_1(X_i, U_{1i}) = M_1 \exp(U_{1i}) \qquad \text{and} \qquad Y_i(4) = \mu_4(X_i, U_{4i}) = M_4 \exp(U_{4i})$$

where

$$(V_i, U_{1i}) \sim \mathcal{N}\left(\begin{bmatrix} 0 \\ 0 \end{bmatrix}, \begin{bmatrix} 1 & \rho_1 \\ \rho_1 & \sigma_1^2 \end{bmatrix} \right) \qquad \text{and} \qquad (V_i, U_{4i}) \sim \mathcal{N}\left(\begin{bmatrix} 0 \\ 0 \end{bmatrix}, \begin{bmatrix} 1 & \rho_4 \\ \rho_4 & \sigma_4^2 \end{bmatrix} \right)$$

We assume $M = M_1 = M_4$ and $\sigma = \sigma_1 = \sigma_4$, and estimate $\widehat{M} = \$52,962$ and $\widehat{\sigma} = 1.12$ via maximum likelihood, using the family income of respondents aged 31 to 37 in the 2015 and 2016 IPUMS ACS to follow Chetty et al. (2020)'s sample restrictions (Appendix Figure 7 shows the fit of the estimated model.). Given parental income x_i, we calculate

$$\mathbb{E}[Y_i | d_i = 1, x_i] \qquad \text{and} \qquad \mathbb{E}[Y_i | d_i = 4, x_i]$$

using

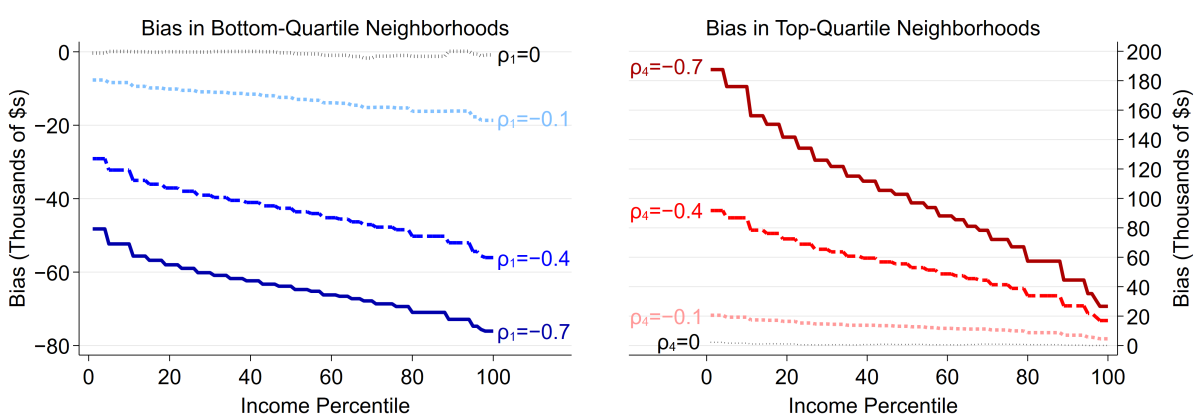
$$\mathbb{E}[Y_i | v_i > v_1^*(x_i)] \qquad \text{and} \qquad \mathbb{E}[Y_i | v_i < v_4^*(x_i)]$$

where the cutoffs $v_1^*(x_i)$ and $v_4^*(x_i)$ are obtained from the data displayed in Figure 4.

Under various assumptions on ρ_1 and ρ_4, we then calculate bias as the raw gap between potential outcomes and observed outcomes,

$$\text{Bias}(d_i = 1, x_i) = \mathbb{E}[Y_i(1)|x_i] - \mathbb{E}[Y_i | d_i = 1, x_i] \quad \text{and} \quad \text{Bias}(d_i = 4, x_i) = \mathbb{E}[Y_i(4)|x_i] - \mathbb{E}[Y_i | d_i = 4, x_i].$$

Figure 2 shows the results when ρ_1 and ρ_4 take values of 0, -0.1, -0.4, and -0.7.



(a) Bias in Bottom Fourth of Quality by Income

(b) Bias in Top Fourth of Quality by Income

Figure 2: Bias by Household Income

Note: This figure measures bias as the difference between the observed and potential outcomes of the model parameterized as described in the text.

The patterns of bias are as expected. The larger is the magnitude of the correlation between unobservables, the larger is the bias. In the left panel we see that the bias is largest for high-income parents in low-ranked neighborhoods. And in the right panel we see that the bias is largest for low-income parents in high-ranked neighborhoods.

The right panel of Figure 2 displays the bias for one of the central parameters of interest when designing a housing mobility program, one of the key policy use cases of the OA: How will the children of low-income parents do when growing up in high-ranked neighborhoods? The bias is greatest for this combination of families and neighborhoods, the very combination in which policymakers are most interested when designing a housing mobility program (Aliprantis et al. (2020), Bergman et al. (2020)).

In what sense is the bias above large? One way to answer this question is to represent the bias above as a percent of the effect of moving to a new neighborhood implied by interpreting OA estimates of observed outcomes as potential outcomes. Figure 3 reports such a measure, showing the difference between potential and observed outcomes in top quartile neighborhoods divided by the difference in observed outcomes between top quartile (87th percentile) neighborhoods and lower-ranked (n^{th} percentile) neighborhoods:

$$\text{Bias}(n) = \frac{\text{Bias}(d_i = 4, x_i = 25)}{\mathbb{E}[Y_i|q_i = 87, x_i = 25] - \mathbb{E}[Y_i|q_i = n, x_i = 25]}.$$

Figure 3 shows that interpreting observed outcomes as potential outcomes can lead to large bias in neighborhood effect estimates. Even in the case of weakly correlated unobservables, where $\rho_4 = -0.1$, bias is nearly always 100 percent or larger of the neighborhood effect implied by observed outcomes.

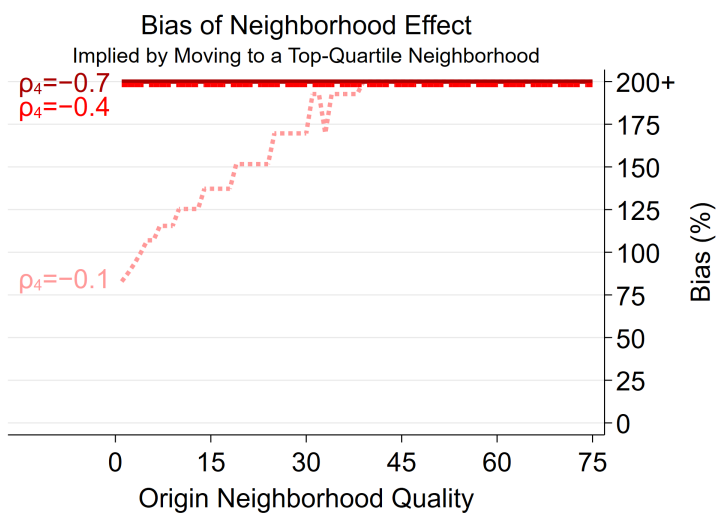


Figure 3: Bias by Origin Neighborhood Quality
Note: This figure plots the difference between potential and observed outcomes in top quartile neighborhoods as a percentage of the difference in observed outcomes between top quartile neighborhoods and lower ranked neighborhoods. See the text for more details.

Appendix Figure 9 shows that, in addition to the conceptual uncertainty documented here, neighborhood sorting by income generates statistical uncertainty when estimating neighborhood outcomes of low-income children in high-ranked tracts or of high-income children in low-ranked tracts.[8] Almost half of high-quality tracts have less than 30 low-income children aged 6-11. And half of low-quality tracts have less than 20 high-income children. The identification problem posed by small sample sizes is different from that posed by biased estimates, but it adds to the threat of inaccurate estimates of potential outcomes in exactly the places where accurate estimates are most valuable to policymakers.

3.2 Sorting by Race

Residential sorting by race is also strong in the US. The problem of small sample sizes is even more prominent along this dimension. By way of example, we focus on young Black males: in the 1990 Census, the median tract in the top half of neighborhood quality had 2 Black males in the OA sample age range (6-11) (hereafter boys). Figure 4a shows how quickly the median number of Black boys in 1990 Census tracts falls as 1990 neighborhood quality increases. At the lowest levels of quality, most tracts have 50 Black boys or more with which to estimate outcomes. But once quality gets out of the bottom decile, the number of Black boys is already too low to reliably estimate outcomes in many neighborhoods. Outside the bottom third of tracts, most neighborhoods simply do not have enough observations to reliably estimate how a sample of Black boys – even a selected sample – performed in adult outcomes after residing there.[9]

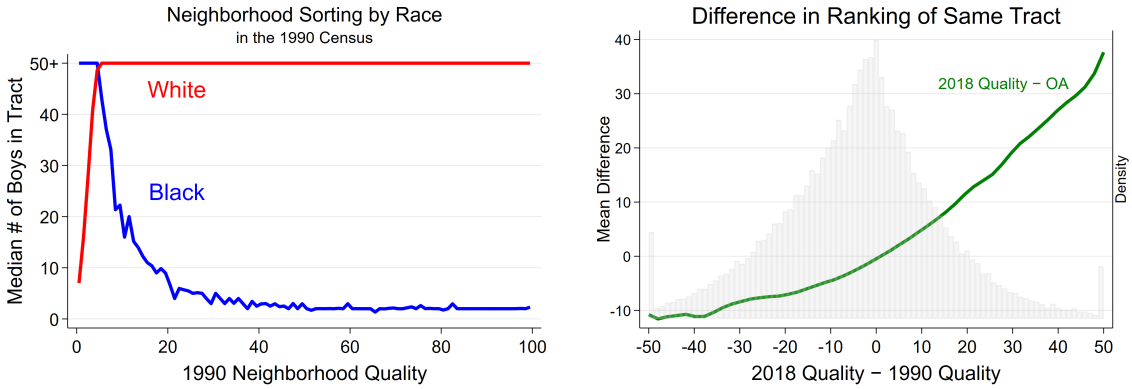
The strong neighborhood sorting by race in the 1990 Census creates both conceptual and statistical uncertainty in the OA. Appendix Figure 10b shows how this neighborhood sorting by race in the 1990 Census passes through to the Opportunity Atlas. The share of tracts with publicly-reported outcome estimates for Black males drops rapidly as 2018 neighborhood quality rises. In the top half of tracts, 21 percent of tracts have estimates for Black males.[10]

Estimates of the observed outcomes of Black boys who grew up in high-ranked neighborhoods are based on a highly-selected sample of boys and neighborhoods; in addition to the results just presented, see also Appendix Figure 8b. And as we showed earlier, observed outcomes are themselves likely to be biased estimates of potential outcomes. Thus, the strength of neighborhood sorting by race has implications for how we interpret potential outcomes estimated via regressions of Black boys' outcomes on parental income and tract- or block-fixed effects (Chetty et al. (2020)).

[8] The typology of conceptual and statistical uncertainty are from Manski (2015). While standard errors are reported along with the OA estimates, the appropriate way to use those standard errors to quantify the statistical uncertainty around point estimates appears to be an open question (Hall and Miller (2009), Xie et al. (2009), Klein et al. (2020)). Mogstad et al. (2020) provide the most relevant analysis on quantifying statistical uncertainty in estimated rankings.

[9] Appendix Figure 10a shows that this is not a matter of Black boys being concentrated in urban areas; the same pattern holds in metros with populations of at least 1 million inhabitants.

[10] Chetty et al. (2020) report a sample size cutoff of 20 observations for publicly releasing a tract's estimate, and the distributions of within-tract gaps shown in Chetty et al. (2020) Online Appendix Figure XIVa excludes tracts with fewer than 50 Black or white boys.



(a) Number of Boys by Quality

(b) Disagreements by Change in Quality

Figure 4: Sorting by Race and Disagreement over Time
Note: The left panel plots the median number of Black and white boys in the OA sample age range, 6-11, in Census tracts within each percentile of 1990 neighborhood quality. The right panel plots a local linear regression of the mean difference in a tract's 2018 neighborhood quality minus its OA ranking.

3.3 Sorting over Time

A known obstacle for the OA rankings is that they use outcomes for children who grew up in each tract decades ago. There are reasons to doubt that this would affect OA rankings of tracts today, since the ranking of tracts within metros tends to be stable over time (Malone and Redfearn (2018)). Figure 4b shows, perhaps surprisingly then, that changes in quality over time are highly predictive of disagreements between neighborhood quality and OA rankings.

Figure 4b shows a local linear regression of the difference in ranking between 2018 neighborhood quality and the OA ranking using age 29 family income as a function of the difference between 2018 and 1990 neighborhood quality rankings. Changes in neighborhoods over time predict large differences between the neighborhood quality ranking based on current inhabitants and the OA ranking based on previous inhabitants. Appendix Figures 11 and 12 show this finding in terms of population growth as well.

3.4 Sorting in Moving to Opportunity

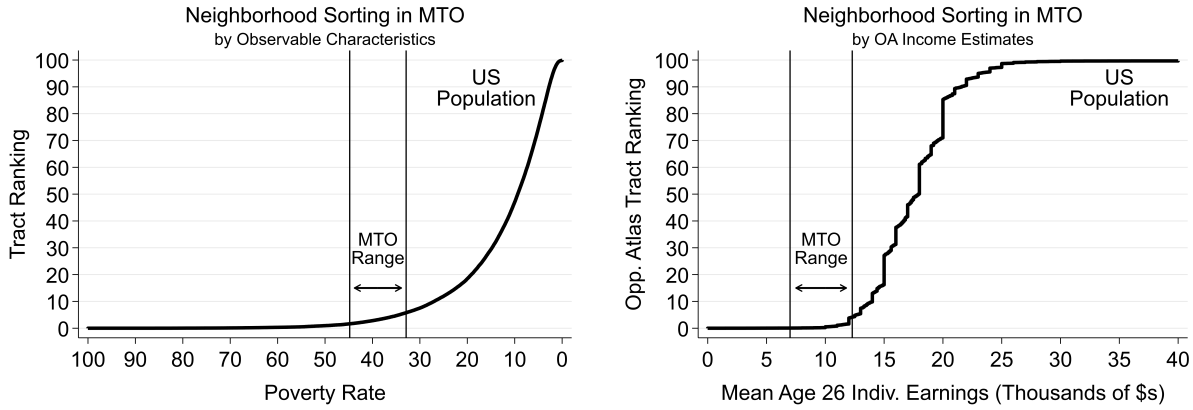
Is there any way to judge the concern raised in this note that OA measures of neighborhood outcomes may reflect biased estimates? Chetty et al. (2020) provide evidence on this question using two research designs. Here we focus on the first design, which uses the experimental variation in the Moving to Opportunity (MTO) housing mobility program. Between 1994 and 1998, MTO awarded public housing assistance to households in some of the poorest neighborhoods in the US. The experiment was conducted in five cities, and randomized the locations where households were eligible to move with housing assistance.[11]

[11] It is important to recall that randomly assigning housing vouchers is not the same as randomly assigning neighborhoods, because households still make choices after receiving a voucher. Aliprantis and Richter (2020) and

Figure 5a illustrates that MTO can be given two distinct interpretations based on observable socioeconomic characteristics. When viewed in terms of changes in the raw poverty rate, shown on the x-axis of the figure, MTO can be interpreted as having induced large changes in participants' neighborhood poverty (Kling et al. (2007), Fryer Jr and Katz (2013), Ludwig et al. (2008)). When viewed in terms of changes in the distribution of poverty, shown on the y-axis of the figure, MTO can be interpreted as having induced small changes in participants' neighborhoods. Race is an important factor in interpreting MTO: The latter view is consistent with MTO merely moved participants around racially-segregated neighborhoods that are still likely to be disconnected from the mainstream economy (Sampson (2008), Clampet-Lundquist and Massey (2008), Aliprantis (2017a)).

Figure 5b shows that the dichotomy in interpretations of MTO based on observable characteristics extends to the interpretation of MTO based on the OA.[12] If viewed in terms of the raw change in mean individual income in a tract, the change induced by MTO was large. If viewed in terms of the OA ranking of neighborhoods in terms of mean individual income, the change induced by MTO was small.

Our interpretation of Figure 5 is that any exercise using the MTO data is necessarily confined to a highly-selected set of neighborhoods. This is true if one relates observed outcomes in the OA to observed outcomes in MTO, or if one extrapolates from the MTO data (Appendix Figure 13).



(a) Sorting in MTO by Poverty

(b) Sorting in MTO by OA Income Estimates

Figure 5: Neighborhood Sorting in Moving to Opportunity (MTO)
Note: The left panel presents data from the 2000 US Census along with MTO treatment and control group means 4-7 years after randomization as reported in Kling et al. (2007). The right panel uses those aged 26 in the 5% IPUMS-USA sample of the 2004-2008 American Community Survey, when the OA sample was aged 26, to compute the percentiles of the individual-level earnings distribution. OA tract outcomes for children with 10th percentile parents estimated and reported in terms of these percentiles are linked with the US population in the 2000 Census to then provide an OA ranking of tracts in terms of mean age 26 individual earnings. The MTO treatment and control group means are taken to be $7,000 and $12,289 for mean individual earnings for children with parents at p=10 in the Opportunity Atlas based on Chetty et al. (2020) Figure X.

Pinto (2019) specify and estimate models of neighborhood effects that account for the ways household choices result in the unobserved component of potential outcomes observed in MTO varying across tracts.

[12] MTO results based on rankings using observable neighborhood characteristics do not always match results based on the OA outcome-based ranking of tracts (Kaestner (2020), Chetty et al. (2020)).

4 Conclusion

The Opportunity Atlas offers an innovative approach to estimating neighborhood effects. Unfortunately, neighborhood sorting represents a significant barrier to identifying neighborhood effects using outcomes, a problem common to previous efforts to quantify neighborhood effects. We find that the estimates in which policymakers are likely to take greatest interest are those estimates for which this barrier applies most strongly (e.g., identifying high-externality areas for low-income Black residents).

The OA provides excellent direct evidence on intergenerational mobility and the spatial patterns of variation in that mobility. Use cases of the data that do not depend on the observed outcomes to be unbiased estimates of potential outcomes (that is, ones that seek a measure of intergenerational mobility *conditional on neighborhood sorting*) can ignore the central issue of sorting we raise here.

Additionally, policymakers may find the trade-offs of using biased estimates of neighborhood effects worthwhile given the urgency of the problems they face. Some contexts may be more forgiving than others; Aliprantis et al. (2020) explores just such a situation, where housing mobility programs looking to identify target neighborhoods find broad agreement between those neighborhoods indicated by the OA compared to more traditional measures. Policy often cannot wait for perfect understanding. When the time comes to evaluate programs that rely on the OA estimates, however, careful attention should be paid to how the identification problems raised in this note might translate to results.

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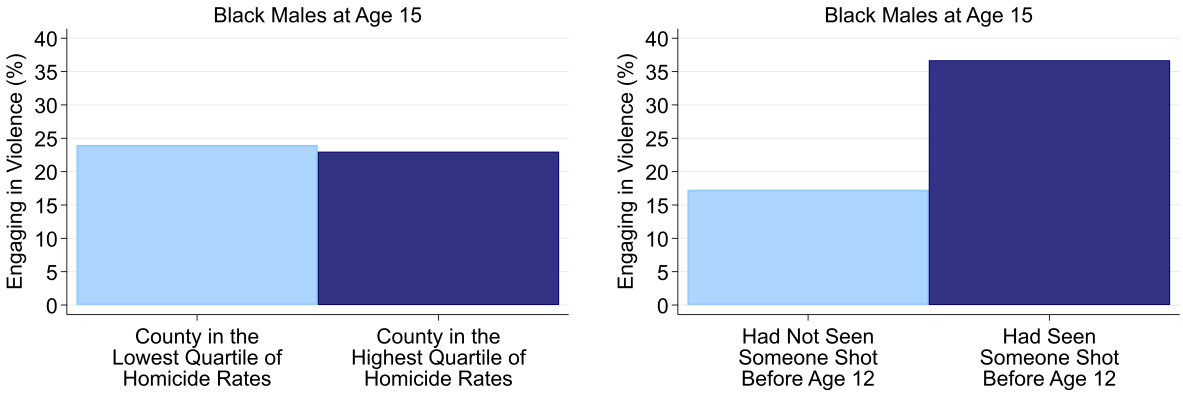
A Exposure to Violence



(a) Violence as County Homicide Rate

(b) Violence as Seeing Someone Shot at

Figure 6: Black Young Males Engaging in Violent Behavior, by Exposure to Violence

Note: This figure displays data from Aliprantis (2017b). The left panel displays data from the National Longitudinal Survey of Youth 1997 (NLSY97) and the Federal Bureau of Investigation's Uniform Crime Reporting (UCR) Program. The right panel displays NLSY97 data alone. Engaging in violent behavior is a one-zero indicator for a youth attacking someone, carrying a handgun, or belonging to a gang in the past year. The left panel illustrates that Black boys are equally likely to engage in violent behavior whether living in counties with the highest or lowest rates. The right panel illustrates a very different pattern when measuring exposure to violence at the much finer and idiosyncratic level of witnessing a shooting. Black boy are twice as likely to engage in violent behavior at age 15, even conditional on observable characteristics, if they witness a shooting before age 12.

B Data Details and Simulation Results

Our analysis uses tract-level data from the National Historical Geographic Information System (NHGIS, Manson et al. (2020). The six characteristics used to calculate neighborhood quality are the poverty rate, the share of adults 25+ with a high school diploma, the share of adults 25+ with a BA, the Employment to Population Ration for adults 16+, the labor force participation rate for adults 16+, and the share of families with children under 18 with only a mother or father present. To calculate the percentage of households in a given neighborhood type conditional on income, we use the NHGIS tract-level data reporting the number of households in each tract in income bins. We convert these bins of raw income into bins of income percentiles using the household income distribution in the five percent sample of the 1990 Census from the Integrated Public Use Microdata Series (IPUMS-USA, Ruggles et al. (2020)).

For our simulations we use parameters M and σ^2 from the model

$$Y_i(D) = Y_i = \mu(X_i, U_i) = M \exp(U_i) \qquad \text{where} \qquad U_i \sim \mathcal{N}\left(0, \sigma^2\right)$$

estimated via maximum likelihood on the family income of respondents aged 31 to 37 in the 2015 and 2016 IPUMS ACS (This sample is chosen to follow Chetty et al. (2020)'s sample restrictions.). Figure 7 below shows the fit of the estimated model with $\widehat{M} = \$52,962$ and $\widehat{\sigma} = 1.12$. The estimated model produces tails that are larger than the tails in the data, but broadly mimics the skewed distribution in the data.

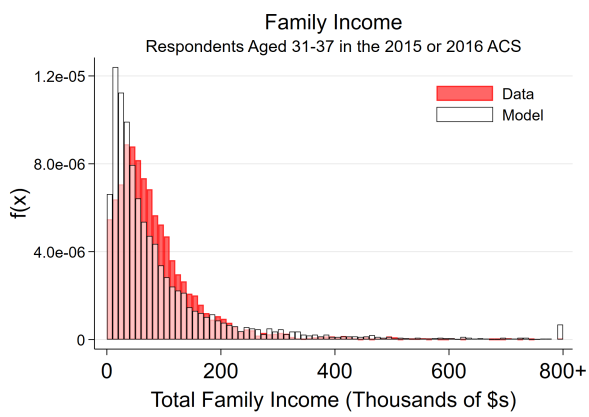
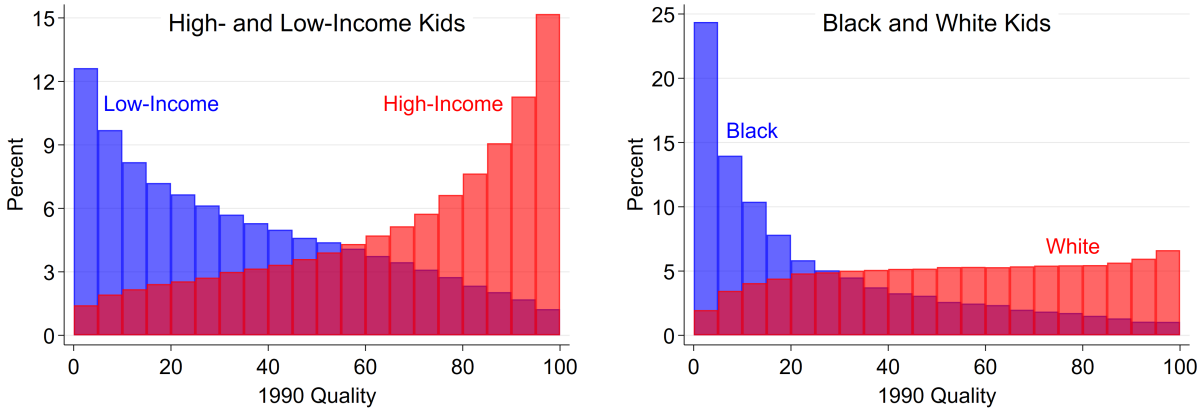


Figure 7: The Family Income Distribution
Note: This figure shows the family income distribution in the data and from the parameterized, estimated distribution used for simulations in the main text. See the Appendix text just above for an explanation of the parameterization and estimation.

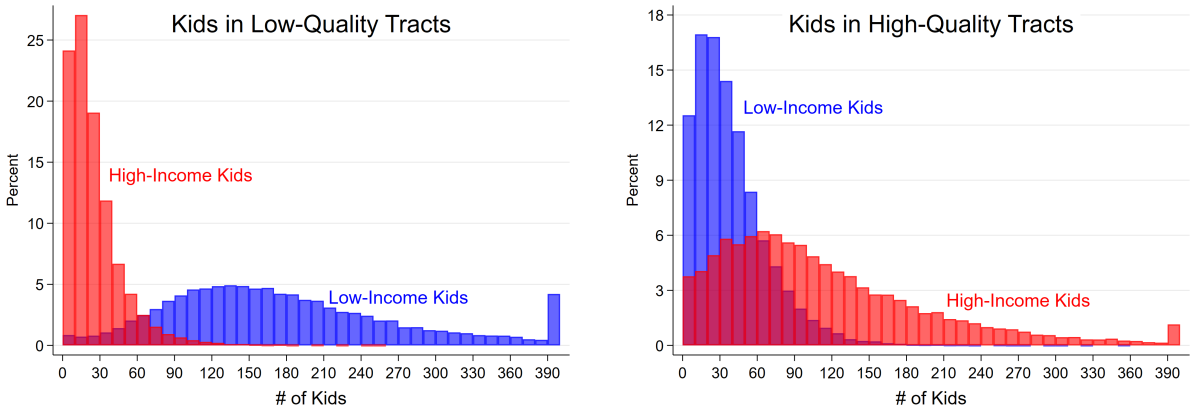
C Conceptual Uncertainty and Statistical Uncertainty



(a) Sorting by Income

(b) Sorting by Race

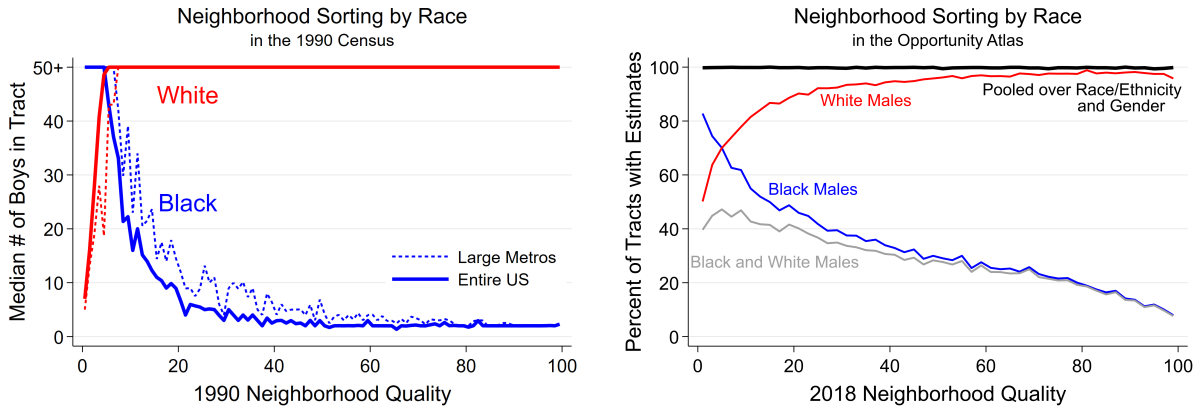
Figure 8: Sorting into Neighborhood Quality by Household Income and Race
Note: The left panel displays the distributions of children aged 6-11 in the 1990 Census with parents in the top and bottom quartiles of household income residing in tracts in the bottom quartile of 1990 neighborhood quality. We estimate the number of high-income kids in a tract as the share of the tract's households that are at or above the 75th percentile of household income times the number of children aged 6-11, and we estimate the number of low-income kids in a tract analogously. The right panel displays the distributions of Black and white children aged 6-11 by the 1990 neighborhood quality of their tract of residence.



(a) Kids in Low-Quality Neighborhoods

(b) Kids in High-Quality Neighborhoods

Figure 9: Sample Sizes by Income
Note: The left panel displays the estimated number of children aged 6-11 in the 1990 Census with parents in the top and bottom quartiles of household income residing in tracts in the bottom quartile of 1990 neighborhood quality. We estimate the number of high-income kids in a tract as the share of the tract's households that are at or above the 75th percentile of household income times the number of children aged 6-11, and we estimate the number of low-income kids in a tract analogously. The right panel displays the estimated number of children aged 6-11 in the 19990 Census with parents in the top and bottom quartiles of household income residing in tracts in the top quartile of 1990 neighborhood quality.



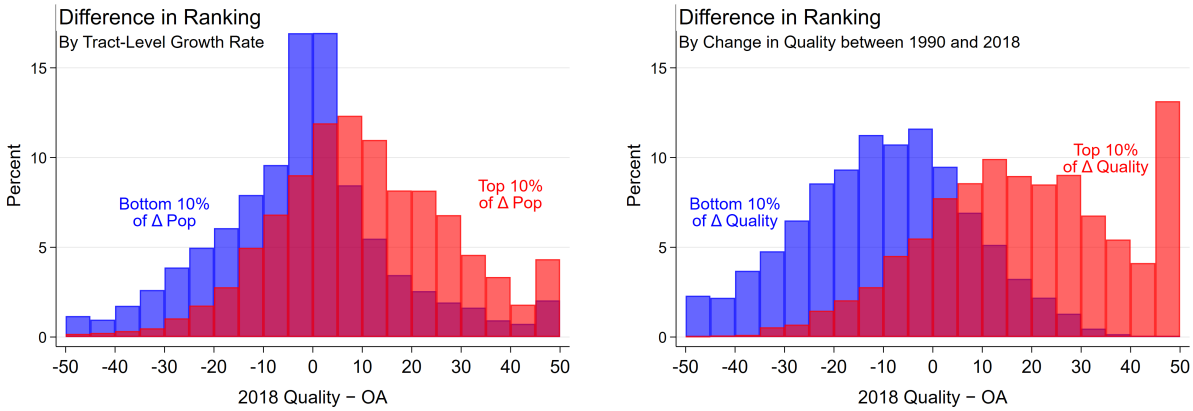
(a) Number of Boys by Quality

(b) Overlap of Estimates

Figure 10: Sample Sizes and Common Support by Race

Note: The left panel shows the median number of black and whites boys in a tract conditional on being in a given percentile of 1990 neighborhood quality. The dashed lines show the medians when calculated only for tracts in the 54 largest metros in the 2017 American Community Survey, with each metro having at least 1 million inhabitants. The right panel shows the percent of tracts with OA estimates of conditional outcomes for Black males and white males at each percentile of neighborhood quality.

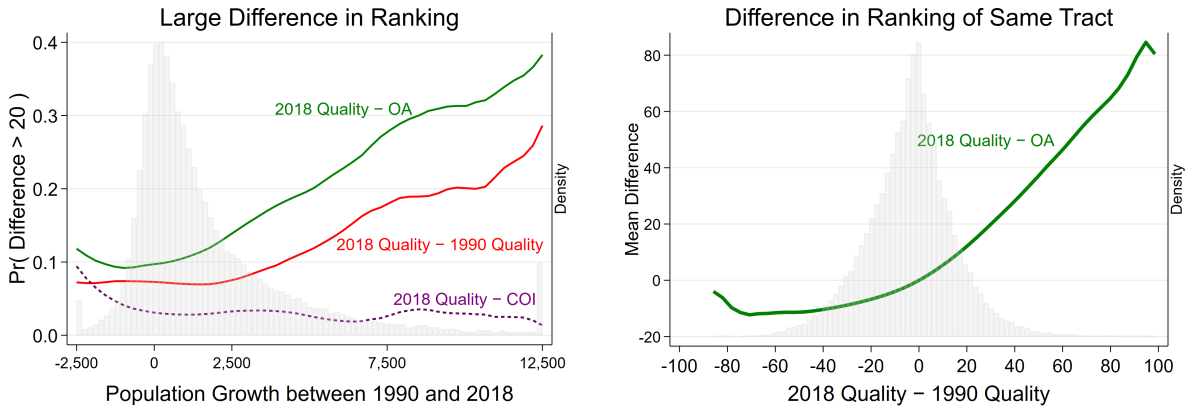
D Changes over Time



(a) Change in Population

(b) Change in Quality

Figure 11: Predicting Disagreement in 2018 Quality and OA Rankings
Note: The left panel shows the distributions of disagreement between the 2018 quality and OA rankings of tracts for those tracts in the top and bottom 10 percent of population growth between 1990 and 2018. The right panel shows the distributions of disagreement between the 2018 quality and OA rankings of tracts for those tracts in the top and bottom 10 percent of the change in quality between 1990 and 2018.



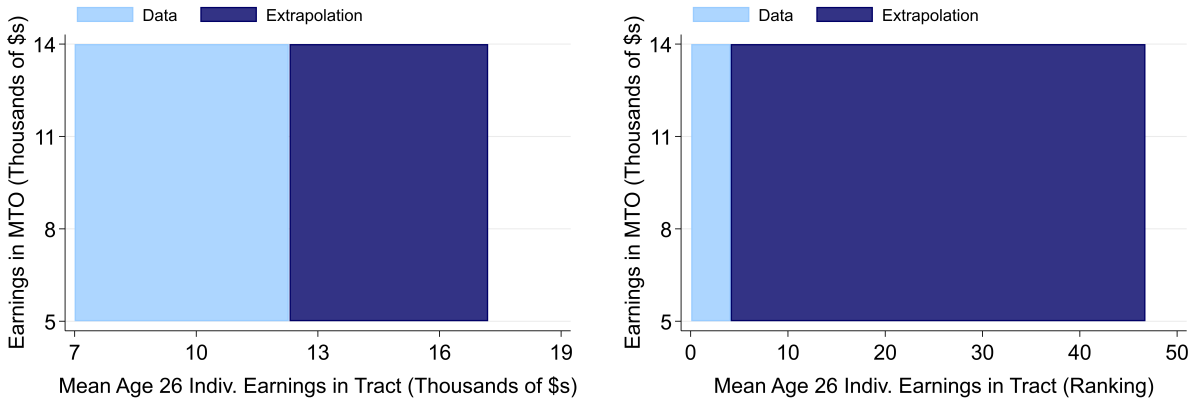
(a) Large Disagreements by Population Growth

(b) Expected Disagreement by Change in Quality

Figure 12: Predicting Large Disagreements in 2018 Quality and OA Rankings
Note: The left panel shows local linear regressions of the probability that 2018 quality ranks a tract at least 20 percentile points higher than another measure as a function of population growth in the tract between 1990 and 2018. The other rankings shown are OA in green, 1990 quality in red, and the Childhood Opportunity Index 2.0 (COI, Noelke et al. (2020)) in purple. The right panel shows the mean difference in 2018 quality and OA rankings of a tract as a function of the change in quality between 1990 and 2018.

E Moving to Opportunity

Appendix Figure 13 revisits the OA dichotomy shown in main text Figure 5 to show the dichotomy's implications for extrapolating results from MTO using the OA data. The left panel shows that when extrapolating based on tracts' raw outcomes across the range in Chetty et al. (2020) Figure XIV, the support of the data is large relative to the support of extrapolation. The right panel shows that when extrapolating based on tracts' rankings, the support of the data is small relative to the support of extrapolation.



(a) By Raw Neighborhood Characteristic

(b) By Neighborhood Ranking

Figure 13: Extrapolating with the MTO Data

Note: Both panels assume MTO treatment and control group means of \$7,000 and \$12,289 for mean individual earnings for children with parents at p=10 in the Opportunity Atlas based on Chetty et al. (2020) Figure X. Both panels assume extrapolation to \$17,207 for neighborhood mean individual earnings for children with parents at p=10 in the Opportunity Atlas based on Chetty et al. (2020) Figure XIV. The left panel shows the support of the MTO data in light blue and the range of extrapolation in dark blue in terms of the OA estimates of raw neighborhood outcome. The right panel shows the support of the MTO data in light blue and the range of extrapolation in dark blue in terms of the OA ranking of neighborhoods.