



Federal Reserve Bank of Cleveland Working Paper Series

What Determines the Success of Housing Mobility Programs?

Dionissi Aliprantis, Hal Martin, and Kristen N. Tauber

Working Paper No. 20-36R

October 2022

Suggested citation: Aliprantis, Dionissi, Hal Martin, and Kristen N. Tauber. 2022. "What Determines the Success of Housing Mobility Programs?" Working Paper No. 20-36R. Federal Reserve Bank of Cleveland. <https://doi.org/10.26509/frbc-wp-202036r>.

Federal Reserve Bank of Cleveland Working Paper Series

ISSN: 2573-7953

Working papers of the Federal Reserve Bank of Cleveland are preliminary materials circulated to stimulate discussion and critical comment on research in progress. They may not have been subject to the formal editorial review accorded official Federal Reserve Bank of Cleveland publications.

See more working papers at: www.clevelandfed.org/research. Subscribe to email alerts to be notified when a new working paper is posted at: www.clevelandfed.org/subscribe.

What Determines the Success of Housing Mobility Programs?

Dionissi Aliprantis* Hal Martin** Kristen Tauber*

October 11, 2022

Abstract: This paper studies how design features influence the success of Housing Mobility Programs (HMPs) in reducing racial segregation. Targeting neighborhoods based on previous residents' outcomes does not allow for targeting race-specific outcomes, generates uncertainty when targeting income-specific outcomes, and generates bias in ranking neighborhoods' effects. Moreover, targeting opportunity bargains based on previous residents' outcomes selects tracts with large disagreements in current and previous residents' outcomes, with such disagreements predicted by sorting since 1990. HMP success is aided by the ability to port vouchers across jurisdictions, access to cars, and relaxing supply constraints, perhaps by targeting lower-ranked neighborhoods.

Keywords: Housing Mobility Program; Housing Choice Voucher Program; Opportunity Mapping; Opportunity Atlas; Neighborhood Effect

JEL Classification Codes: J15, R23, I38, H43

*: Federal Reserve Bank of Cleveland, +1(216)579-3021, dionissi.aliprantis@clev.frb.org.

***: Federal Reserve Bank of Cleveland, +1(216)774-2526, hal.martin@clev.frb.org.

*: New York University, knt2029@nyu.edu.

This paper incorporates content from a note previously circulated as “Neighborhood Sorting Obscures Neighborhood Effects in the Opportunity Atlas.” For helpful comments we thank Leah Boustan, Gregorio Caetano, Pete Cimboric, Ben Craig, Adria Crutchfield, Steven Durlauf, Bruce Fallick, Konstantina Gkritza, Richard Green, James Heckman, Jeff Lin, Ann Lott, Charles Manski, Canishk Naik, David Phillips, Arianna Rambaram, Sarah Reber, Mark Schweitzer, Chris Timmins, Dan Wilson, and seminar participants at the Baltimore Regional Housing Partnership, Cleveland Fed, Georgia, Houston, the NYU Furman Center, University of Chicago Lifecycle Working Group, the Upjohn Institute, West Virginia, the American Economic Association 2022 Annual Meeting, the American Real Estate and Urban Economics Association 2022 National Conference, the 2021 Federal Reserve System Regional Meeting, the 2021 HCEO Conference on Sorting and Segregation, the 2021 Conference of the International Association for Applied Econometrics, the 2021 Meeting of the Southern Economic Association, and the 2020 and 2021 Meetings of the Urban Economics Association.

The opinions expressed are those of the authors and do not necessarily represent the views of the Federal Reserve Bank of Cleveland or the Board of Governors of the Federal Reserve System.

1 Introduction

There is currently interest in crafting public housing policy that combats, rather than contributes to, the residential segregation in American cities. The largest federal public housing program in the United States is the Housing Choice Voucher (HCV) program, which subsidizes recipients' rental payments in the private market. There has long been suspicion that by setting a single, metro-wide subsidy cap for the value of a voucher, the HCV program effectively requires voucher holders to live in low-opportunity neighborhoods and attend low-performing schools: This is certainly what we see in the data (Galvez (2010); Lens (2013); Horn et al. (2014)). One recent policy innovation has been to allow HCV subsidy caps to rise and fall with local rents, a policy known as Small Area Fair Market Rents (SAFMRs).

While the adoption of SAFMRs has led HCV recipients to move to higher opportunity neighborhoods in some cases (Collinson and Ganong (2018)), this does not appear to be true across all sites (Dastrup et al. (2019); Reina et al. (2019); Bergman et al. (2020a)), and there is reason to question whether SAFMRs alone can generate sustained opportunity moves (Aliprantis et al. (2022)). Housing Mobility Programs (HMPs) are a potential solution to the obstacles not addressed by SAFMRs. HMPs offer assistance searching in high opportunity neighborhoods, pre- and post-move counseling, and landlord outreach. Originally designed as a remedy to past public housing policies designed to segregate cities by race (Polikoff (2006); Rothstein (2017)), HMPs are central to the US Department of Housing and Urban Development's (HUD's) reforms to the HCV program.

The success of previous HMPs in empowering moves to opportunity has varied widely. Figure 2 shows the results of two prominent HMPs in 2019. Those who moved in the Baltimore Regional Housing Partnership (BRHP) typically increased from the 5th to the 77th percentile of neighborhood poverty rates for non-Hispanic whites. Participants who moved through the Creating Moves to Opportunity (CMTO) HMP typically increased from the 23rd percentile to the 41st percentile.

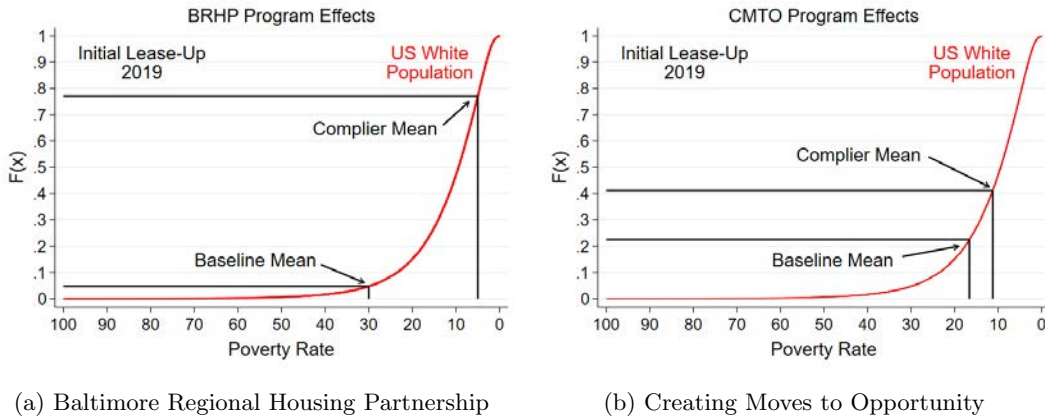


Figure 1: Program Effects on Participants' Tract-Level Poverty Rates

Note: Both figures display the Cumulative Distribution Functions of tract-level poverty for the United States' population of non-Hispanic whites in the 2014-2018 American Community Survey/NHGIS. The left panel shows the pre-program and post-move means of BRHP program participants in 2019. The right panel shows the baseline mean and treatment group complier mean for CMTO participants in April 2019, where the complier mean is calculated as the control mean plus the treatment on the treated (TOT) effect.

What drives such differences in the success of HMPs? Several factors have already been identified: Landlord outreach, pre-search counseling, housing search assistance, and post-move support (Scott et al. (2013)). Landlord outreach can broaden the supply of rental units available to program participants (Cossyleon et al. (2020)). Pre-search counseling can provide motivation and emotional support for moving (Darrah and DeLuca (2014); DeLuca and Rosenblatt (2017)). Housing search assistance can overcome information frictions and short-term financial constraints (Bergman et al. (2020a), Bergman et al. (2020b), Schwartz et al. (2017)). And post-move support can ensure that small obstacles to housing stability or community integration are addressed (Cunningham et al. (2010); DeLuca and Rosenblatt (2017)).

This paper studies how additional HMP design features affect program success. We consider whether “opportunity neighborhoods” should be targeted based on the outcomes of those who grew up in them decades ago or current residents’ outcomes, and how these targeted outcomes interact with the search for low-rent, highly-ranked “opportunity bargains.” We then consider the importance of access to cars, partnerships to support moves across PHA jurisdictions, and whether targeting neighborhoods in the middle of rankings might increase the supply of rental units available to program participants. We define HMP success in terms of reducing the residential segregation of Black and white poor residents, motivated by the Gautreaux and Baltimore HMPs having been created in response to the discrimination and violence that helped to generate racial segregation (Polikoff (2006)). Another motivation for our focus on racial segregation is that there remain large gaps in the neighborhood characteristics of Black and white residents in American cities, even conditional on income and wealth (Aliprantis et al. (2022)), and past experience with HMPs indicates they could be an effective policy for addressing this source of racial inequality.

We simulate residential locations in a reference HMP and study design effects by comparing how simulated locations would change in HMPs with alternative design features. In the reference HMP, poor residents living in neighborhoods of concentrated poverty are encouraged to move to tracts in the top third of their metro area as ranked by “neighborhood quality,” an index aggregating six rankings of current residents’ socio-economic status. While the number of moves into a given tract is constrained by the supply of rental housing units there, we assume that the maximum number of moves into a tract, 30 households, does not induce residents to leave the receiving neighborhoods. The reference HMP would move around five percent of each metro’s poor population, and receiving areas would tend to add around one percent of their original populations. If gradually implemented over the course of two decades, receiving tracts would add one or two new voucher families per year under the reference HMP.

The first design feature we investigate is the measure used to rank neighborhoods. Relative to our reference measure, an HMP could be designed around an index with information on more than the socioeconomic status of current residents, such as the Child Opportunity Index (COI), which includes information on school performance, access to public transportation, and environmental conditions (Noelke et al. (2020); diversitydatakids.org (2021)). Or an HMP could be designed around the Opportunity Atlas (OA), a new data set that uses administrative data to estimate the

adult outcomes of previous residents who grew up in a given tract years ago (Chetty et al. (2020a)).

The OA has additional variation relative to quality or the COI, and after connecting the OA's outcome estimates with causal effects of neighborhoods, Chetty et al. (2020a) interpret the additional variation in the OA in terms of neighborhood effects. We show that the additional variation in the OA could reflect bias or noise. We quantify the bias possible from interpreting observed outcomes in administrative data as potential outcomes. We find that neighborhood sorting could generate large bias from this practice.

We document that sorting over time predicts large disagreements between the outcomes of a tract's current and previous residents. This finding, that large disagreements are likely to reflect bias in the ranking of neighborhoods' effects, is important for the targeting of opportunity bargains, or highly-ranked tracts with low rents. We find that opportunity bargains identified by the OA are likely biased. If we use the OA to target opportunity bargains, the resulting HMP selects tracts with large disagreements between current and previous residents' outcomes.

In addition to conceptual uncertainty, we also document statistical uncertainty in the OA. We document major obstacles to the ambitious goal of targeting outcomes by race or income, with administrative data on neighborhoods facing the same "bias-in, bias out" problem faced by administrative data in other contexts (Mayson (2019)). We have observed very few Black children grow up in most highly-ranked tracts. And we have observed few low-income children grow up in many highly-ranked tracts.

Beyond the variable used to rank tracts, we examine additional design features such as the portability of vouchers across PHA jurisdictions. Voucher moves across PHA jurisdictions are rare in practice (Garboden (2021)), and a focus on moves across PHA jurisdictions appears to be a distinguishing design feature of the Gautreaux and Baltimore HMPs relative to the Moving to Opportunity (MTO) and Creating Moves to Opportunity (CMTO) experiments. We find that when voucher moves are restricted to stay within PHA jurisdictions, HMP success is reduced by 36 percent relative to our metro-wide reference HMP.

The third design feature we investigate is related to rental housing supply. We find that eliminating the supply constraints in highly-ranked tracts would increase program success by 54 percent while respecting the upper bound of 30 families moving into any given tract. While there are several approaches HMPs currently take to increase access to units in high-opportunity areas, we explore an alternative approach to increasing the supply of units: Targeting the middle third rather than the top third of tracts. We find that this alternative approach of targeting the middle third of tracts improves racial equality by the same amount as would completely eliminating rental supply constraints when targeting the top third of tracts. This result raises questions about which neighborhoods to target that appear unresolved since the time of Gautreaux (Rosenbaum (1995)).

A fourth design feature we consider is access to cars. We suppose that participants in our reference HMP were unable to access a car, and so were restricted to moving to tracts with access to public transportation. We find that under this HMP design, success would only be 46 percent of the success of the reference HMP.

Finally, because the US Supreme Court’s stance on race has changed over time, we consider how HMP success would change if current interpretation changed and race could be targeted directly. We find that a version of the reference HMP in which only Black households participate would improve HMP success by an additional 158 percent.

2 Previous Housing Mobility Programs

The four most-studied HMPs are the Gautreaux HMP (Gautreaux), the Moving to Opportunity for Fair Housing (MTO) experiment, the Baltimore HMP (BHMP), and the Creating Moves to Opportunity (CMTO) experiment. Figure 2 shows how residential outcomes for movers vary across different existing HMPs. Changes in neighborhood poverty rates between baseline and initial lease-up locations were largest in Gautreaux and the BHMP, both of which are regional programs. The gap between initial placement and subsequent locations is the smallest in the BHMP. The long-term success of BHMP participants’ residential outcomes is likely a function of the BHMP’s focus on landlord outreach, tenant counseling, search assistance, and post-move support. These features serve to overcome resistance to subsidized tenants among landlords and increase the effectiveness of search among those tenants, thereby increasing the reachable housing supply.

Gautreaux and the BHMP have been implemented as regional partnerships, encouraging residents to move beyond a single PHA’s jurisdiction and throughout their respective metropolitan areas. While MTO technically encouraged participants to move across jurisdictions, the program’s Section 8 certificates and vouchers were allocated to the central city PHAs at each site (Feins et al. (1996), p 1-4), and it is unclear how much the obstacles to portability across PHA jurisdictions constrained participants’ choices (Feins et al. (1996), pp 13-4 to 13-9).¹ And while CMTO was implemented by two PHAs, the Seattle and King County PHAs, it does not appear that moves across these PHAs’ jurisdictions were encouraged (Bergman et al. (2020a)).

Gautreaux is the only HMP to explicitly use an individual’s race as an eligibility criterion for program participation. Gautreaux targeted low-income African Americans in Chicago public housing (Rosenbaum and DeLuca (2008)). The BHMP does not select tenants based on their individual race or ethnicity. Although the BHMP is based on the same legal precedent as Gautreaux – that HUD has the obligation to remedy segregation by doing more than simply refraining from discriminating (PRRAC (2005)) – court rulings have disallowed race-based individual eligibility criteria.² MTO targeted residents of public housing in the lowest ranked neighborhoods in the country, and the participating population ended up being about two thirds African American (Orr et al. (2003)). The eligible population in CMTO was any household with a newly-awarded Housing Choice Voucher (HCV); this made the target population of CMTO effectively the poor residents of King County, Washington.

Participants in Gautreaux were eligible to move to neighborhoods defined in terms of racial

¹Two thirds of MTO experimental compliers’ initial lease ups were within their site’s central city (de Souza Briggs et al. (2010)) and 70 percent of this group stayed within the same school district (de Souza Briggs et al. (2008)).

²See Scott et al. (2013) and Tegeler (2009) for related discussions.

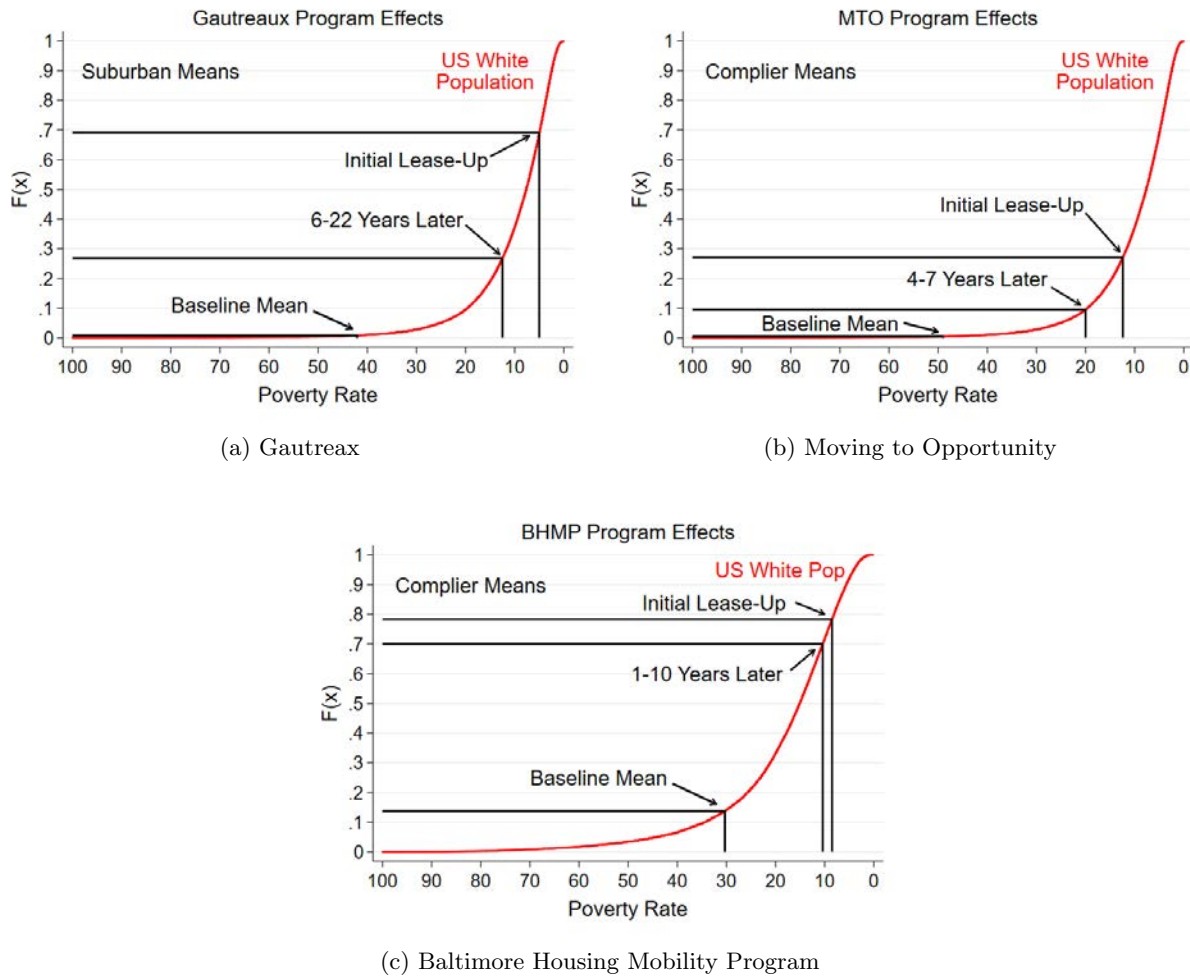


Figure 2: Residential Outcomes of Movers in Housing Mobility Programs

Note: Panel a shows mean neighborhood poverty rates in Table 1 of Keels et al. (2005) in terms of the 2000 Census distribution of the US non-Hispanic white population. Panel b shows mean tract poverty rates in the 2000 Census from Orr et al. (2003), with baseline poverty rates taken from Exhibit 2.7, complier mean in initial lease-up location taken from Exhibit 2.3, and complier mean 4-7 years after randomization taken from Exhibit 2.5. Panel c shows mean neighborhood poverty rates in the 2005-2009 American Community Survey from DeLuca and Rosenblatt (2017), with complier locations at baseline, after initial lease-up, and 1-10 years after program locations taken from Table 2 and suburban mean 1-10 years later taken from Table 4.

composition; eligible tracts were those with no more than 30 percent Black residents (Rosenbaum (1995)). In the BHMP neighborhood eligibility has changed over time. From 2002 until 2015, eligible neighborhoods were those with no more than 30 percent Black residents, no more than 10 percent poverty, and no more than 5 percent of residents receiving housing assistance. From 2015 until today, the BHMP has been administered by the Baltimore Regional Housing Partnership (BRHP), which uses a combination of 21 variables to determine tract eligibility. The eligible neighborhoods in MTO were defined in terms of poverty rates; eligible tracts were those with less than 10 percent poverty. In CMTO, eligible tracts were determined by combining the Opportunity Atlas data with several additional variables in an estimation procedure that shrinks the OA ranking toward contemporaneous variables like 2010 poverty rates and 4th grade test scores (Bergman et

al. (2020a), Appendix A).

Tenant counseling pre- and post-move have been a part of each HMP; what has varied considerably across HMPs has been the precise form of counseling provided, financial support to tenants, and outreach to landlords. For example, the BHMP makes its workshops mandatory for program participants. The BHMP and CMTO provide financial assistance toward moving costs, while Gautreaux and MTO did not. And while the BHMP actively recruits landlords in opportunity areas, contacts landlords on behalf of searching clients, and provides mediation between tenants and landlords, these services varied across sites in MTO. See Table 2 in Schwartz et al. (2017) or Cunningham et al. (2010) for further details.

The objective of an HMP that aims to dismantle racial segregation is not simply to expose Black participants to more white neighbors, as there is nothing special about living next to white neighbors per se. Instead, the goal of an HMP that aims to dismantle racial segregation is to expose Black participants to neighborhood externalities, or neighborhood effects, that will improve their economic outcomes and life-satisfaction. For example, adults' mental health improved in MTO as a result of movers' decreased exposure to toxic stress in their neighborhoods (Kling et al. (2007a); Popkin et al. (2002); Han and Madaleno (2019)). Children in Gautreaux and the BHMP had higher academic achievement after attending higher-performing schools (Rubinowitz and Rosenbaum (2000); DeLuca et al. (2016)). Children in MTO did not experience higher academic achievement, but they also did not attend higher-performing schools (Sanbonmatsu et al. (2006)). There were long-term effects on the educational attainment and labor market outcomes of children and adolescents who moved through MTO (Chetty et al. (2016)). And while labor market outcomes did not improve for all adults who moved through MTO (Kling et al. (2007a)), labor market outcomes did improve for adults who moved to higher SES neighborhoods due to MTO (Aliprantis and Richter (2020), Pinto (2019)), with the same being true for adults who moved to higher SES, less-segregated neighborhoods in Gautreaux (Mendenhall et al. (2006)).

3 Simulating a Reference Housing Mobility Program

We begin our analysis by simulating a reference Housing Mobility Program (HMP). Simulating this reference HMP will allow us to highlight data details and modeling assumptions that will be made across simulations. Results from this HMP will serve as a reference point when alternative assumptions are adopted.

3.1 Data

We consider three rankings of neighborhood-level outcomes, where we define neighborhoods as Census tracts.³ For decennial Census data before 2010, when appropriate we impute count estimates into 2010 tract boundaries using the Longitudinal Tract Data Base (LTDB) described in

³Assuming that Census tracts are the unit over which neighborhood externalities operate is a strong assumption, typically made due to data limitations. See Durlauf (2004) and Galster (2019) for broad discussions.

Logan et al. (2014), Logan et al. (2016), and Logan et al. (2021). Each ranking of neighborhoods is in terms of the national distribution of individuals, although we note the cases when we use metro-level rankings. As we discuss later, there are statistical challenges specific to these rank measures (Mogstad et al. (2021)).

The first ranking is “neighborhood quality,” an index originally used in Aliprantis and Richter (2020), which is the ranking of the first principal component of a tract’s national rankings on six socioeconomic characteristics. These characteristics, which include the poverty rate, educational attainment, and labor market outcomes, are available in the 1990 decennial census and 2014-2018 American Community Survey (ACS), which we download from the National Historical Geographic Information System (NHGIS, Manson et al. (2020)). The second ranking is the Childhood Opportunity Index 2.0 (COI). Developed at Brandeis University (Noelke et al. (2020)), the COI aggregates information from 29 items, many of which come from data sources beyond the Census, like the National Center for Education Statistics (NCES) and the Environmental Protection Agency (EPA).

The third ranking comes from the Opportunity Atlas (OA), which estimates the average outcomes for individuals born between 1978 and 1983 who spent time residing in a given neighborhood (Chetty et al. (2020a)). This birth cohort corresponds to children aged 6-11 in the 1990 Census. Unless otherwise stated, our analysis focuses on the OA ranking of neighborhoods based on the estimated average family income between ages 31-37 for children who had parents at the 25th percentile of income. We sometimes also refer to the OA rankings for high-income kids and low-income kids to denote children with, respectively, 75th and 25th percentile income parents.

Appendix A describes these measures in greater detail.

The 1990 Decennial Census directly measures the number of children aged 6-11 in each tract, as well as the number of Black or white boys aged 6-11. However, the 1990 Census does not directly measure the number of high- and low-income children aged 6-11 in each tract, and so we must estimate this number. We define quartiles of the household income distribution using the five percent sample of the 1990 Census from the Integrated Public Use Microdata Series (IPUMS-USA, Ruggles et al. (2018)). 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. We estimate the number of low-income kids in a neighborhood analogously in terms of the households at or below the 25th percentile of household income.⁴

We measure metros as Core-Based Statistical Areas (CBSAs), and when possible, as the metropolitan divisions within those CBSAs. We measure access to public transportation using General Transit Feed Specification (GTFS) data on stops that are a compilation of local transit agencies’ data, either compiled by the Bureau of Transportation Statistics’ National Transit Map (BTS (2021)) or Open Mobility Data (MobilityData IO Volunteers (2021)). We investigate some features of PHA jurisdictions using data from HUD’s estimated PHA service areas (HUD (2021a)). We utilize data on Census Designated Places (CDPs) from the NHGIS (Manson et al. (2020)).

⁴Appendix D replicates this approach for estimating the number of poor kids in a tract, and finds that this estimation approach is generally accurate when compared with the directly-measured number.

3.2 Measuring Success

As described above, the differences in previous HMP’s residential outcomes illustrate the importance of selecting features of an HMP in accordance with the program’s goals. Many PHAs today focus on economic integration by deconcentrating poverty. In the cases of Gautreaux and the BHMP, the primary goal has been to reduce racial segregation for Black residents.

Without minimizing the disparities faced by other groups, we focus this analysis on the residential segregation of Blacks and whites. Black neighborhoods have struggled to gain upward mobility in a way ethnic enclaves have not because of specific exclusionary policies, coupled with durable systemic racism and violence that have left many Black communities disinvested of the institutions that support upward mobility. While HMPs are not the only approach to addressing racial segregation, they are a means of addressing the areas of concentrated economic disadvantage that are still with us today. Figure 3a shows the remarkable clustering of poor Black residents in Baltimore in the lowest quality neighborhoods. A third of Baltimore’s poor Black residents live in tracts below the 5th percentile of the national distribution of quality.

Not all cities in the US have this type of residential segregation. Figure 3b shows that in Seattle there are no tracts in the bottom 5 percent of quality. While poor Black residents are over-represented in low-ranked tracts, poor Black residents are not clustered in the city’s lowest ranked tracts.

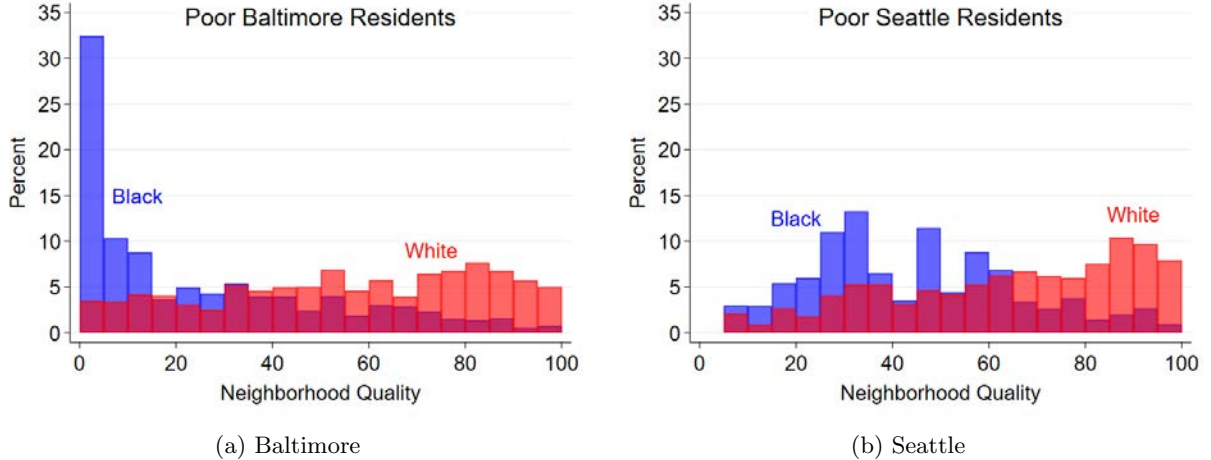


Figure 3: Racial Segregation of Neighborhood Quality

Note: These figures show the distributions of Black and white poor residents of Baltimore and Seattle using data from the 2014-2018 ACS/NHGIS. The panels display the distributions in terms of their Probability Mass Functions (PMFs). The construction of neighborhood quality is described in Section 3.1 of the text.

We measure success in terms of the racial equality of poor Black and white residents’ neighborhood characteristics. In the case of neighborhood quality q , we define the racial equality in metro m after HMP h as a function of the area between Black and white CDFs,

$$RE_m(h) = 100 \left\{ 1 - 2 \left[\int_0^{100} (F_{B,m,h}(q) - F_{W,m,h}(q)) dq \right] \right\}.$$

This measure of racial equality is equal to 100 if poor Black and white residents are equally exposed to neighborhood characteristics in metro m under HMP h (ie, if $F_{B,m,h} = F_{W,m,h}$). The measure is equal to zero if all poor Black residents live in the lowest quality neighborhood and poor white residents are uniformly distributed (ie, if $F_{B,m,h}(0) = 1$ and $F_{W,m,h} \sim U[0, 100]$). Figure 5a displays the empirical CDFs for Baltimore, for which our measure of racial equality is 38.

3.3 Description of Simulations and Assumptions

Simulating an HMP requires making assumptions along several dimensions. To organize our analysis, we make a group of assumptions about program design and behavioral responses that we will collectively refer to as our reference HMP. We will then run simulations after changing program design features one by one to look at the relative change in HMP success.

A simple description of the computational approach is that based on the housing supply in targeted destination tracts, we first determine the number of movers that can move to a new tract in an HMP of a given size. We then select that number of movers from origin tracts and move them to destination tracts. More details are provided about each of these steps in the Computational Appendix I.

Our first assumption for the reference HMP is that residents move to destination tracts located throughout their entire metropolitan division. We also assume that rental housing supply, the number of available and affordable units in a tract, is equal to 7.5 percent of the 2 bedroom and larger rental units in the tract.⁵

While the number of families allowed to move to a destination tract is an important assumption, we show that this choice is not critical for our main findings. We consider a “fully developed” HMP with the overall number of participants chosen (i) to approximately match the size of the BHMP after 20 years and (ii) to avoid sorting responses of incumbents so as to maintain current levels of neighborhood effects. We assume that if rental housing supply is greater than or equal to 30 units, then 30 families move into a given opportunity tract. If rental housing supply is $r < 30$ units, then we assume that r families move into the opportunity tract.⁶

An HMP with 30 families moving per tract makes it reasonable to assume that no incumbents in receiving neighborhoods move in response to the HMP participants’ arrival. Since we interpret the moves in our HMP as occurring over two decades or more, this assumes that every year one or two units in a tract changes to voucher holders. The evidence in the literature is that a one-time placement of 10 units per high-opportunity tract is not likely to generate a sorting response from incumbents (Davis et al. (2019); Rosenbaum (1995)). The possibility for welfare losses and sorting responses have been estimated to result from much larger one-time changes, such as building of 82 units or placing 100 housing vouchers in the same high-opportunity tract (Diamond and McQuade

⁵The number 7.5 is selected based on a conversation with BRHP staff estimating that 5-10 percent of rental units in opportunity tracts are available and affordable to their program participants. Scott et al. (2013) cite 5 percent as a typical market vacancy rate when designing an HMP.

⁶We assume 30 families is $4 \times 30 = 120$ individuals and r families is $4 \times r$ individuals. We examine some relaxations of these assumptions in our simulations.

(2019); Davis et al. (2019)).⁷ Nevertheless, if one believed that it was necessary to limit the size of our reference HMP to 10 or 20 families moving per tract to mute incumbents’ sorting responses and/or to maintain the neighborhood externalities in destination tracts (Agostinelli et al. (2020, 2021)), we show in Appendix N that our main findings are unchanged for HMPs of these smaller sizes.

Figure 4 shows the joint distribution of rental housing supply and neighborhood quality for tracts in Baltimore. We see that the rental supply constraint is binding for tracts at all levels of quality, and that the constraint is strongest for the highest quality tracts. In many of the highest-ranked tracts less than 10 families will be able to move in under our reference simulation.

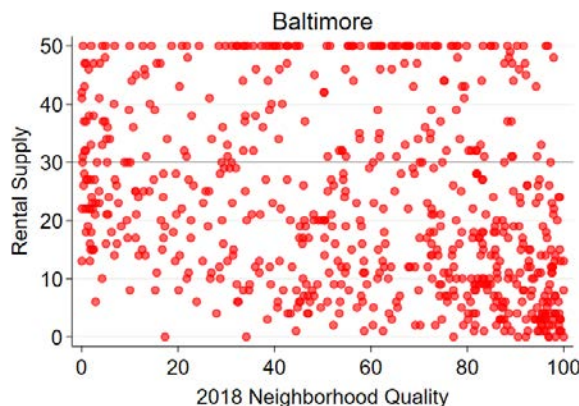


Figure 4: Supply of Rental Housing Units in Baltimore

Note: This figure shows the joint distribution of rental housing supply and neighborhood quality for tracts in Baltimore. As described in the text, we define “Rental Supply” as 0.075 times the number of 2 bedroom or larger rental units in the tract in the 2014-2018 ACS/NHGIS.

We also assume that participants move uniformly into available units in eligible tracts. This is a strong assumption about counseling and participant preferences. In the BHMP participants often initially held preferences against eligible neighborhoods (Darrah and DeLuca (2014)), and in MTO participants did not move uniformly to eligible neighborhoods (Aliprantis and Kolliner (2015); Davis et al. (2021a); Chetty et al. (2020a)).⁸

Finally, we make assumptions about how movers select into the program. To find our population of movers, we randomly select poor residents in tracts with poverty rates greater than 30 percent. While a variety of alternative criteria have been used, this is the criterion used in HUD’s current HCV Mobility Demonstration (HUD (2020)). We assume that 25 percent of eligible poor residents take up the program and move, an assumption about target population outreach and selection (Scott et al. (2013); Rosenbaum (1995)) roughly guided by the rate of selection into the MTO experiment (Orr et al. (2003)) and considerations of moving costs (Ross (2011)). In a few cases, to

⁷It is also important to consider, if difficult to model, the types of equilibrium dynamics that could result in neighborhoods (Davis et al. (2019); Aliprantis and Carroll (2018); Chyn and Daruich (2021); Caetano and Maheshri (2021)) and schools (Caetano and Maheshri (2017); Agostinelli et al. (2020); Angrist and Lang (2004)).

⁸The evidence from MTO suggests that counseling must have very strong effects on location decisions to offset poverty restrictions (Galiani et al. (2015); Shroder (2002)).

fill the eligible housing units available in destination tracts, we move on from tracts with greater than 30 percent poverty and randomly select poor residents from the bottom third of tracts.⁹

3.4 Reference HMP Simulation Results

Figure 5a shows the result of the reference HMP in Baltimore. The CDF of poor Black residents shifts to the right much more than does the CDF of poor white residents, so that our measure of racial equality increases from 38 to 48. As suggested by Figure 3, Table 1 confirms that this greater shift for Black poor residents is due to the over-representation of poor Black residents in areas of concentrated poverty. Also suggested by Figure 3 and confirmed in Table 1 is that different racial and ethnic groups will be affected depending on the metro.

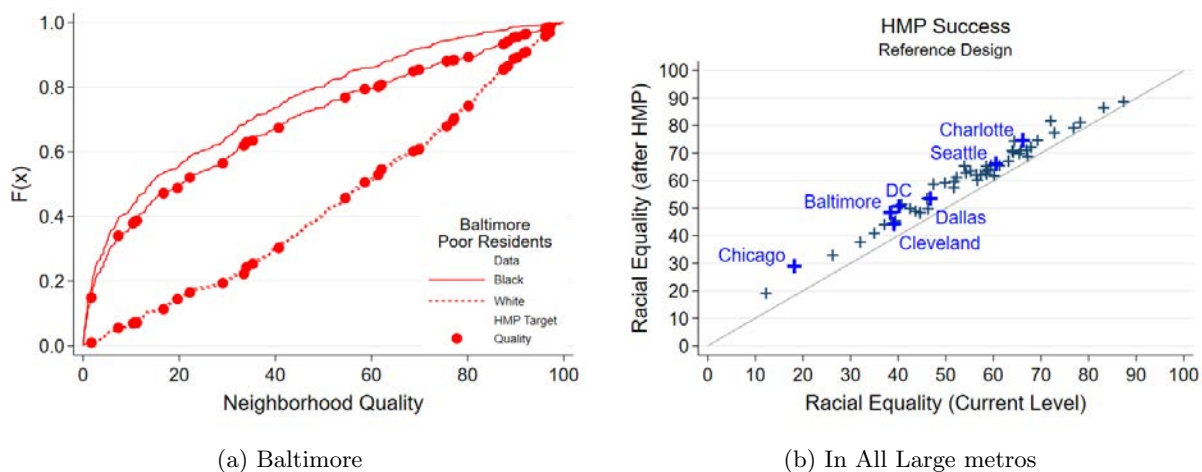


Figure 5: Results of the Reference Housing Mobility Program

Note: The left panel shows the distribution of Baltimore's Black and white poor residents in the 2014-2018 ACS/NHGIS, indicated, respectively, with a solid and dashed line. The left panel also shows the distribution of these groups before and after the reference HMP, indicated by the dotted solid and dashed lines. The right panel shows the racial equality measure for each metro as defined in Section 3.2. On the x -axis is the racial equality measure from the data, the 2014-2018 ACS/NHGIS. On the y -axis is the racial equality measure after the reference HMP is simulated in each metro.

Table 1: Black Movers by Metro (Percentage of Total Movers)

	Baltimore	Chicago	DC	Dallas	Charlotte	Seattle
Percent Black	86	70	64	40	58	20

Note: This table reports the percentage of movers who were Black in the reference HMP. The highlighted metros are chosen for their prominent HMPs or early adoption of Small Area Fair Market Rents.

Figure 5b shows the result of the reference HMP in all of the 54 large metros in our sample. The x -axis in the figure shows racial equality as measured in the current data, and the y -axis shows racial equality in the metro after implementation of the baseline HMP. All metros are above the

⁹In the reference HMP this happened in 10 cities and represented 52.2 percent of movers in those cities.

45 degree line, meaning there were increases in racial equality. A few cities are highlighted due to their prominent HMPs or early adoption of SAFMRs.

Figure 5b makes an important point about the magnitude of effects from HMPs. If HMPs are designed at a small scale to avoid endogenous sorting responses by incumbents so as to maintain current neighborhood effects (Agostinelli et al. (2020, 2021)), they will not by themselves entirely create racial equality in the neighborhoods of poor Black and white residents. HMPs are a powerful tool for generating racial equality of opportunity through neighborhood effects, but HMPs are one part of the larger changes in our society that would be required to achieve racial equality.

4 Which Ranking to Use for Opportunity Mapping?

4.1 The OA Disagrees More than Other Rankings

While the objective of exposing participants to positive neighborhood externalities is straightforward, the question of how to identify those externalities is not. There are many ways to rank neighborhoods, and here we consider making opportunity maps using three rankings introduced earlier in Section 3.1: neighborhood quality, the Childhood Opportunity Index (COI), and the Opportunity Atlas (OA) income estimates for poor children.¹⁰

Figure 6 shows the disagreement between the top third of tracts in Baltimore as identified by each ranking. While these rankings agree about the top-third status of many tracts, there are many tracts over which they disagree. The disagreement between OA and quality is considerably higher than is the disagreement between COI and quality. OA and quality disagree on the top-third ranking of 140 tracts, while COI and quality only generate this disagreement for 71 tracts, out of 671 tracts total. Which ranking should a PHA use to define the opportunity areas to which they encourage moves?

¹⁰Appendix C shows that opportunity mapping is a worthwhile pursuit: An HMP targeting the highest ranked tracts in a metro improves racial equality considerably more than an HMP that simply targets any tract in a suburb.

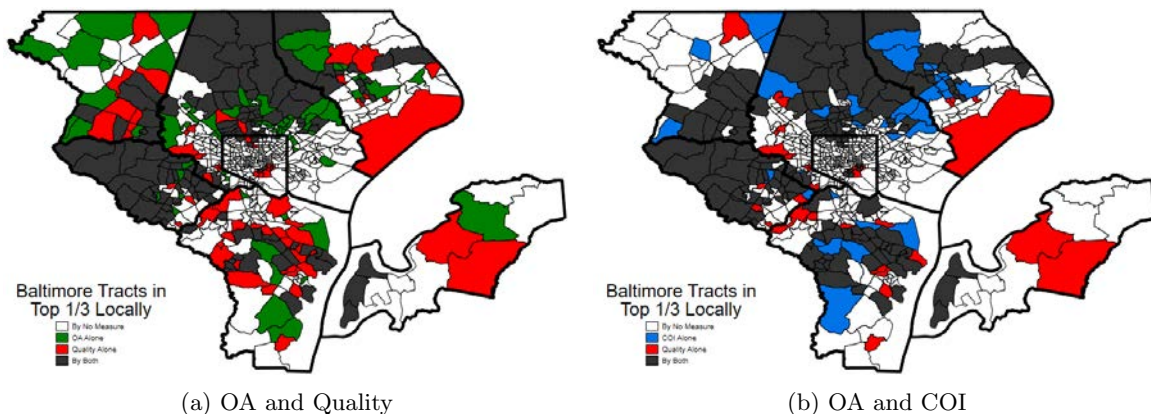


Figure 6: Overlap in Opportunity Maps

Note: These maps show the overlap in tracts ranked in the top third of the Baltimore metro by alternative rankings of neighborhoods. The left panel shows rankings based on Opportunity Atlas (OA) income estimates for poor children and neighborhood quality in the 2014-2018 ACS. The right panel shows the Childhood Opportunity Index (COI) 2.0 and neighborhood quality in the 2014-2018 ACS. Tracts where different measures agree about ranking in the top third are shaded in black, and tracts where measures agree about ranking in the bottom third are left white. Tracts where measures disagree, so that only one of the displayed measure ranks the tract in the top third, are colored in either green, red, or blue.

It remains true beyond Baltimore that OA and quality disagree more on the ranking of tracts than do COI and quality. Figure 7a plots 1,000 randomly chosen Census tracts to illustrate broad features of the joint distributions of measures. The joint distribution of COI and quality is shown with the blue dots. While there is variation in one measure conditional on the other, the measures broadly agree, and tracts ranking very high in one measure do not rank very low in the other. In contrast, the joint distribution of OA and quality, shown with the green dots, exhibits much more disagreement on the ranking of tracts. There are tracts that rank high according to quality that rank low according to OA, and vice versa.

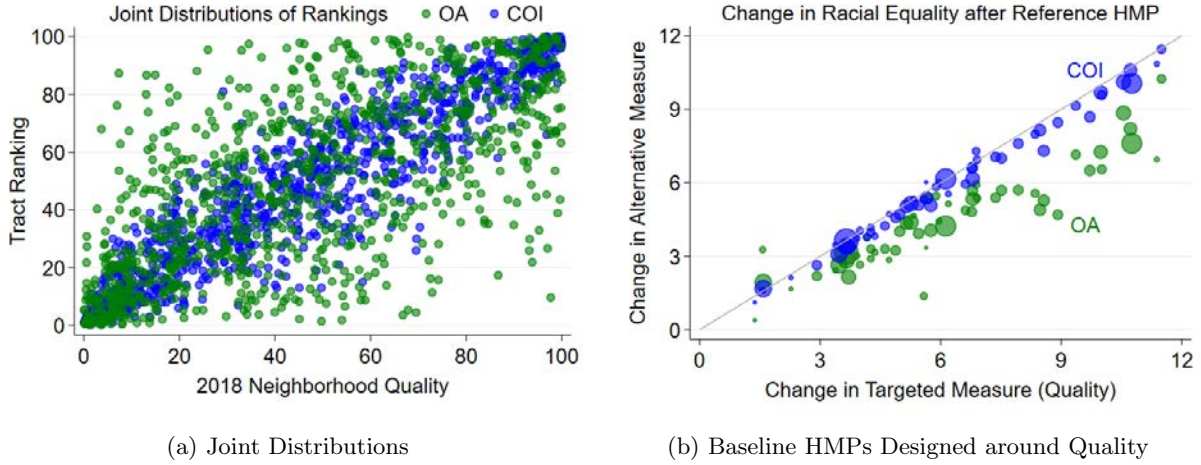


Figure 7: Additional Variation in the OA

Note: The left panel displays a scatterplot of 1,000 randomly-selected Census tracts. Green dots show the joint distribution of the OA and 2018 neighborhood quality rankings of neighborhoods, and blue dots show the joint distribution of the COI and 2018 neighborhood quality rankings of neighborhoods. The right panel shows the success of the Reference HMP in the metros in our sample where the x -axis measures success in terms of 2018 neighborhood quality, the targeted ranking of the reference HMP, and the y -axis measures success in terms of either the COI (in blue) or the OA (in green).

How much does this statistical disagreement matter for the design of HMPs? Figure 7b shows the changes in racial equality that would result for the baseline HMP designed around quality. The x -axis shows the resulting change in racial equality as measured in quality, and the y -axis shows the change as measured in either COI or OA. The fact that the blue dots representing the COI hug the 45 degree line indicates that an HMP targeting quality will also tend to improve COI; the improvement in COI would on average be 95 percent of the improvement in quality. The fact that the green dots representing the OA tend to fall below the 45 degree line indicates that an HMP targeting quality will improve OA less in a quantitatively important way; the improvement in OA would on average be 75 percent of the improvement in quality.

Since targeting quality will improve the COI, and vice versa, the question becomes: How should one interpret the disagreement between OA and contemporaneous measures? Chetty et al. (2020a) interpret this additional variation as evidence that the OA ranking better measures neighborhood externalities than other rankings like the COI or quality. We find evidence that suggests caution when giving this interpretation to the disagreement between the outcomes of a neighborhood's previous residents and the outcomes of its current residents. We also find that a key strength of the OA – the ability to rank neighborhoods by demographic groups – does not appear operational for the design of HMPs.

4.2 Bias When Interpreting Outcomes as Neighborhood Externalities

Neighborhood sorting leads to conceptual uncertainty when interpreting observed outcomes. Given the strength of sorting by race and income to be documented below, the observed Black or low-income children are a highly-selected sample in highly-ranked tracts. Thus, even when the sam-

ple of such children in a neighborhood is large enough to reliably estimate their outcomes without statistical uncertainty, those outcome estimates still contain conceptual uncertainty. Outcomes in a highly-ranked neighborhood could reflect either neighborhood effects or bias from neighborhood sorting.

The Opportunity Atlas (OA) has been used to interpret observed outcomes as potential outcomes.¹¹ Whether using the outcomes of prior or current residents, this interpretation is unorthodox. Indeed, the central goal of the neighborhood effects literature has been to find sources of randomness in the neighborhood sorting process so that observed outcomes coincide with potential outcomes.¹² One approach is to assume that neighborhood effects are increasing in the outcomes of current residents, and then to use sources of random variation to map out the precise magnitudes of those effects. Given the paucity of data from scenarios with random neighborhood sorting, researchers still have much to learn about how neighborhood characteristics contribute to potential outcomes.¹³

How large could the bias be from neighborhood sorting when interpreting observed outcomes as potential outcomes? And what are the consequences of this bias for choosing between measures to rank tracts when designing an HMP?

Appendix E defines neighborhood effects in a Rubin Causal Model (RCM) where the outcome variable Y_i is child i 's adult income, there are four levels of treatment for the neighborhood externality D_i , and the observed covariate X_i is parental income percentile. Within the model we define the bias that sorting on unobservables would create when using observed outcomes as estimates of potential outcomes. We then calculate the bias under assumptions on the degree of sorting on unobservables, while assuming constant potential outcomes across neighborhood treatment levels (Landersø and Heckman (2017); Heckman and Landersø (2021)), and where the distribution of potential outcomes is estimated on data from the 2015 and 2016 IPUMS ACS using a simple log-normal parameterization of income.

To quantify the magnitude of the bias, we scale these simulated levels of bias by the effect of moving to a new neighborhood implied by interpreting OA estimates of observed outcomes as potential outcomes. Figure 8a reports this measure, which is the difference between potential and observed outcomes in top quartile neighborhoods (as ranked by quality) divided by the difference

¹¹While Chetty et al. (2020a) is somewhat more conservative in its claims, the project's public policy tool [opportunityatlas.org](https://www.opportunityatlas.org) suggests such an equivalence: "Which neighborhoods in America offer children the best chance to rise out of poverty? The Opportunity Atlas answers this question using anonymous data following 20 million Americans from childhood to their mid-30s."

¹²Some sources of randomness used in this literature include public housing policy (Oreopoulos (2003), Edin et al. (2003), Jacob (2004), Kling et al. (2007a), DeLuca et al. (2010), Galster et al. (2016), Chetty et al. (2016), Chyn (2018), Aliprantis and Richter (2020)); neighborhood change experienced by non-movers (Dastrup and Ellen (2016), Baum-Snow et al. (2019)); geographic and administrative boundaries (Ananat (2011), Aaronson et al. (2021), Caetano and Macartney (2021)); the thinness of the housing market (Bayer et al. (2008), Schmutte (2015), McCartney and Shah (2021)); and employer location decisions (Boustan and Margo (2009), Miller (2021)). Additional research designs use a model to account for sorting (Hellerstein et al. (2011), Hellerstein et al. (2014), Wodtke et al. (2016), Altonji and Mansfield (2018), Pinto (2019), Davis et al. (2021b)).

¹³For example, a recent literature review concludes that "there remains substantial scope to conduct studies whose primary aim is simply to test for the presence, and measure the magnitude of, neighborhood effects. There is not yet even a loose disciplinary consensus on the rough importance of neighborhoods on life outcomes" (Graham (2018)).

in observed outcomes between top quartile (87th percentile) neighborhoods and lower-ranked (n^{th} percentile) neighborhoods:

$$\text{Bias}(n) = 100 \times \frac{\text{Bias}(d_i = 4, x_i = 25)}{\mathbb{E}[Y_i|q_i = 87, x_i = 25] - \mathbb{E}[Y_i|q_i = n, x_i = 25]}.$$

Figure 8a shows that interpreting observed outcomes as potential outcomes can lead to large bias in neighborhood effect estimates. Even in our weakest case of correlated unobservables, where the correlation between unobservables in the outcome and selection equations is $\rho_4 = -0.1$, bias is nearly always 100 percent or larger of the neighborhood effect implied by observed outcomes.

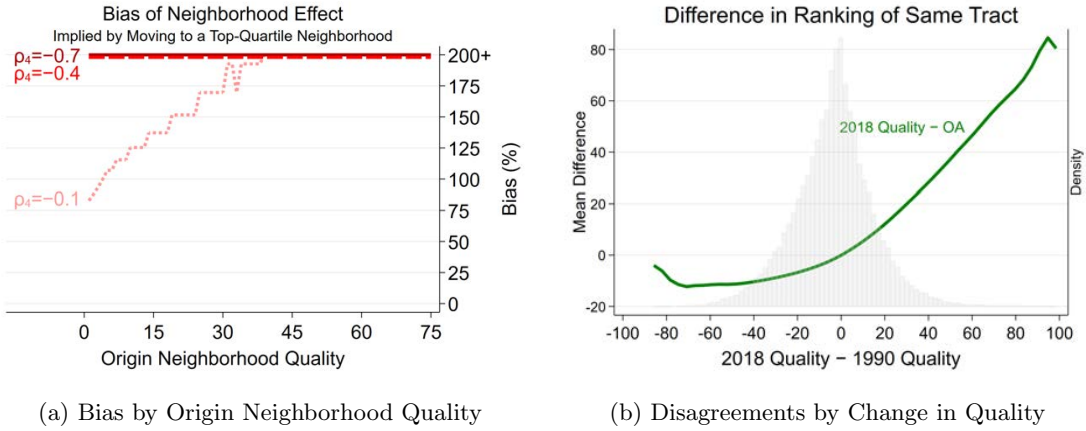


Figure 8: Bias and Disagreement in Neighborhood Rankings

Note: The left panel 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 and Appendix E for details on model specification and parameter estimation. The right panel plots a local linear regression of the mean difference in a tract's 2018 neighborhood quality ranking minus its OA ranking.

In addition to neighborhood sorting by income, another known obstacle for interpreting the OA rankings as unbiased estimates of neighborhood externalities is that the rankings use outcomes for children who grew up in each tract decades ago. One reason to worry about neighborhood change affecting the OA is that the OA is silent on how neighborhood externalities are produced. While the other measures take stands on the inputs driving externalities or their proxies, if neighborhood externalities change over time due to changing inputs, OA will miss due to a 20-25 year lag between treatment and outcomes. However, there are also reasons to doubt that neighborhood change would affect OA rankings of tracts today, since the ranking of tracts within metros tends to be stable over time (Malone and Redfearn (2018)) and neighborhood poverty is relatively stable over time (Chetty et al. (2020a)). Figure 8b shows, perhaps surprisingly then, that changes in quality over time are highly predictive of disagreements between neighborhood quality and OA rankings. Appendix Figures 35 and 36 show similar results in terms of population growth.

4.3 Targeting Opportunity Bargains

Opportunity bargains are neighborhoods with strong, positive effects on residents while also having low rents. How should we think about using alternative rankings to identify such neighborhoods? Chetty et al. (2020a) argue that the OA is the best measure to target opportunity bargains when implementing HMPs. Below we find evidence that suggests caution]when using the OA to target opportunity bargains: The bias creating low-rent, highly-ranked tracts in the OA is likely in the ranking, not the rent.

We consider an HMP that has 1/4 the number of vouchers as the reference HMP, and while moving residents to the top third of a given metro’s tracts, begins by moving participants to the lowest cost tracts. We find that targeting low-rent tracts would have a much more detrimental effect on HMPs targeting outcomes of tracts’ previous residents (ie, the OA) than doing so would have on HMPs targeting outcomes of tracts’ current residents (ie, quality).

Low-rent tracts that are ranked highly according to the OA tend to be ranked much lower by neighborhood quality. Figure 9 shows data from Chicago to illustrate this point. Figure 9a shows the distribution of rental units over 2018 neighborhood quality for tracts in the top third of Chicago when ranked by either the OA or 2018 quality. Because Chicago has many highly-ranked tracts relative to the national distribution, the top third of tracts in Chicago as ranked by quality are nearly all in the top quintile nationally. In contrast, the OA has a long left tail, with about a quarter of tracts in the top third of the OA being below the median in terms of quality. Recall that Figure 8b showed that this disagreement is predicted by neighborhood sorting over time, and therefore is likely to reflect bias.

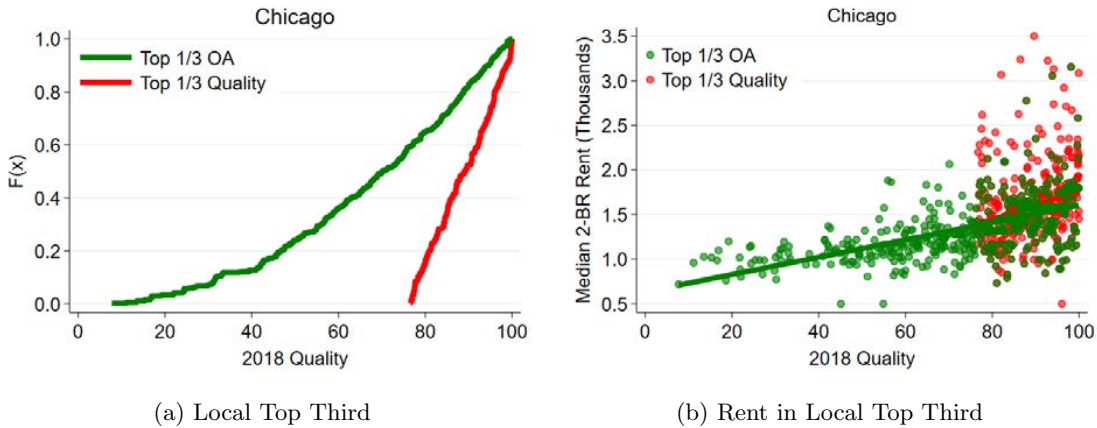


Figure 9: The Top Third of Tracts in Chicago

Note: The left panel plots the Cumulative Distribution Functions (CDFs) of the 2 bedroom and larger rental units in the top third of tracts in Chicago as ranked by either the OA or 2018 neighborhood quality. The right panel plots the joint distribution of median 2 bedroom rent and 2018 neighborhood quality for tracts in the top third in Chicago as ranked by either the OA or 2018 neighborhood quality, as well as lines fitted by Ordinary Least Squares (OLS).

While targeting the top third of tracts as ranked by the OA will allow for tracts that are low-ranked in terms of quality, additionally targeting low-rent tracts will select for these low-ranked

tracts. Figure 9b shows that there is a positive relationship between rent and quality in the top third of tracts in Chicago as ranked by the OA. The left tail of quality is over-represented in the low-rent group of OA top third tracts. Appendix K shows that this same pattern is found across metros.

Figure 10 explores the consequences for the success of HMPs of selection by rent. Each color represents an HMP targeting a different set of tracts, where the overall number of vouchers is $1/4$ the size of the reference HMP. By hugging the 45 degree line, the red dots show that targeting low-rent tracts in the top third of quality has little impact on success relative to the reference HMP randomly targeting top third tracts. Targeting low-rents while sticking with quality as the criterion for being top third only decreases HMP success to 96 percent of the $1/4$ -sized reference HMP.

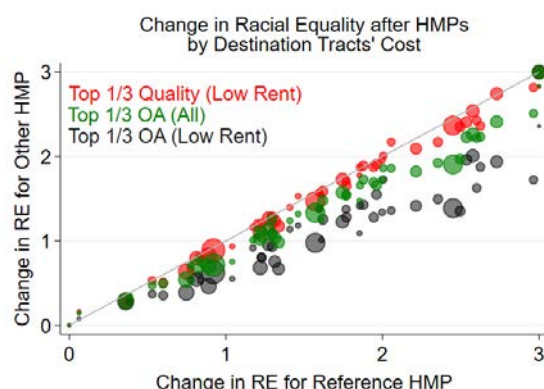


Figure 10: HMP Success by Targeted Measure and Rent

Note: This figure shows the success of HMPs that all have $1/4$ the number of vouchers as the reference HMP. The x -axis shows the quarter-sized reference HMP, while the y -axis shows HMPs with some change. The red dots show an HMP targeting the lowest rent tracts in the top third of quality. The green dots show an HMP targeting the top third of OA tracts, but where success is measured in terms of quality. And the black dots show an HMP targeting the lowest rent tracts in the top third of OA, where success is measured in terms of quality.

Ignoring rents, and instead targeting the top third of OA tracts would have a larger negative impact on the success of HMPs than targeting low-rent top-third quality tracts. This fact is shown by the green dots being slightly below the red dots. Now, the success of HMPs would on average be 84 percent of the $1/4$ -sized reference HMP.

Designing an HMP around not just the top third of OA, but also the lowest-rent tracts within that group, would have an even larger effect on the racial equality resulting from the HMP. The black dots show that the increase in racial equality resulting from this HMP would fall considerably, to 67 percent of the $1/4$ -sized reference HMP. These simulation results confirm the intuition gathered by looking at the distributions in Figure 9: Targeting low-rent tracts in the top third of OA rankings selects tracts that disagree considerably with quality rankings. Section 4.2 describes reasons to suspect these large disagreements reflect bias from sorting over time.

4.4 Targeting Neighborhood Outcomes

Given the just-described obstacles to using the OA for improving the targeting of neighborhood externalities, we now consider a less ambitious goal of targeting outcomes rather than externalities. While we are ultimately interested in targeting neighborhood externalities, targeting outcomes specific to the demographic characteristics of HMP participants might still be an improvement over targeting outcomes that are aggregated over all types of individuals.

4.4.1 Can Race-Specific Neighborhood Outcomes be Targeted?

Targeting outcomes specific to demographic groups could be especially important in the case of race, where the experience of being “Black in white space” could generate different experiences for Black and white individuals occupying the same physical space (Anderson (2020), Harriot (2019)), a mechanism that has been noted by HMP participants (Lott (2021)) and that could be perpetuating residential segregation at all levels of income and wealth (Aliprantis et al. (2022)).

Unfortunately, race-specific outcomes estimates are simply not available for the most relevant tracts when designing an HMP. Residential sorting by race is so strong in the US that we have not observed Black children growing up in most high-opportunity neighborhoods. Consider Black boys: In the 1990 Census, the median tract in the top half of neighborhood quality had 2 Black boys in the OA sample age range (6-11).

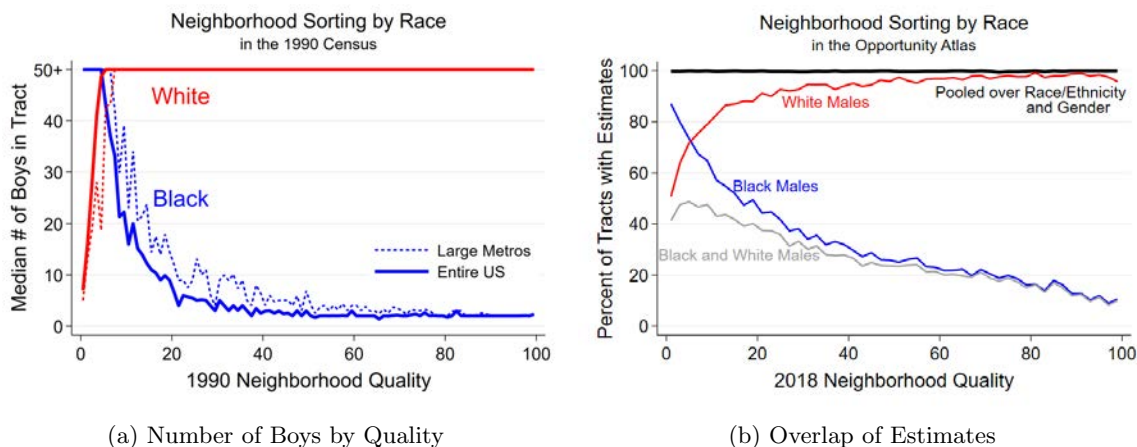


Figure 11: Sample Sizes and Common Support by Race

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.

Figure 11a 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. The dashed lines in the figure show that this sample selection is not reflective of Black boys being concentrated in urban areas; the same pattern holds in metros with populations of at least 1 million inhabitants.

Figure 11b 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.¹⁴

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. (2020b)). We interpret the documented patterns of sorting by race as evidence that using administrative data to study neighborhood effects faces the problem of “bias in, bias out” common to the use of administrative data sets in other settings (Mayson (2019)).¹⁵

4.4.2 Can Income-Specific Neighborhood Outcomes be Targeted?

As above in the case of race, neighborhood sorting also leads to both statistical and conceptual uncertainty in the case of measuring income-specific outcomes. Section 4.2 already focused on conceptual uncertainty and bias, so here we focus on statistical uncertainty.¹⁶ Nevertheless, there is again an interaction between conceptual and statistical uncertainty.

Figure 12a shows that neighborhood sorting by income generates many highly-ranked tracts with very few children. In 1990 almost half of high-quality tracts had less than 30 low-income children aged 6-11. What does this strong sorting by income mean for estimating the income-specific outcomes of a tract’s previous residents? Figure 12b shows that the disagreement between OA and quality is the largest in tracts with very few children. The green dots in Figure 12b plot the R^2 of a regression of the OA ranking of a tract on its 1990 neighborhood quality ranking conditional on being within a given percentile in the distribution of children in the 1990 Census. We see that the R^2 can rise above 0.7 for tracts with many children, but that the R^2 starts below 0.1 in tracts with the fewest children. The R^2 is particularly low in tracts with the low sample sizes sorting by income generates in high-quality tracts. Given the nature of sorting by income, this sorting generates the greatest statistical uncertainty in precisely the highly-ranked tracts where HMPs would tend to move low-income children.

¹⁴Chetty et al. (2020a) 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. (2020b) Online Appendix Figure XIVa excludes tracts with fewer than 50 Black or white boys.

¹⁵Some of the other contexts in which this problem presents itself is occupational sorting by gender when predicting job performance (Ajunwa (2019)); access to care when predicting illness (Obermeyer et al. (2019), Noor (2020)); judicial decisions when predicting recidivism (Kleinberg et al. (2017)); or police decisions to interact with civilians when predicting police bias (Fryer (2019), Durlauf and Heckman (2020), Knox et al. (2020)).

¹⁶See Manski (2015) for a definition of statistical and conceptual uncertainty.

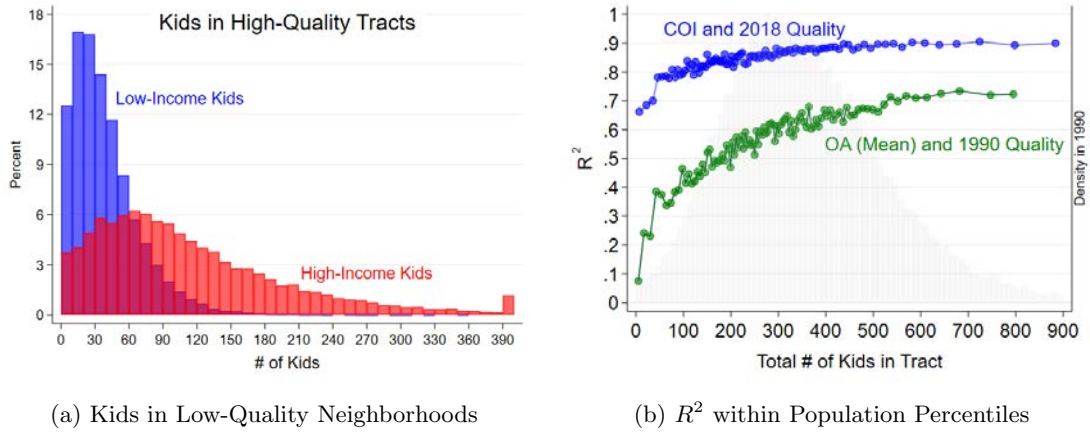


Figure 12: Sample Sizes

Note: The left panel shows in blue the distribution of the number of low-income children (with parental income at the 25th percentile or below) in blue and in red the distribution of the number of high-income children (with parental income at the 75th percentile or above), both in top quartile tracts. The right panel reports the R^2 from population-weighted regressions within single percentiles of the total number of children aged 6-11 in the tract in the 1990 Census. The green dots show the R^2 for regressions of OA (mean) ranking on 1990 neighborhood quality and the blue dots show the R^2 for regressions of COI ranking on 2018 neighborhood quality.

4.5 Interpreting the Disagreement between Rankings

When looking at the local ranking of tracts by the OA and neighborhood quality, large disagreements become rare when we plot rankings that do not include tracts with large changes in quality between 1990 and 2018 or small sample sizes. Figure 13 shows the implications of sorting over time in Chicago for interpreting disagreements between the outcomes of a tract's current and previous residents. Appendix J.1 shows that a similar pattern holds in other cities, with the joint distribution of local quality and OA rankings looking more like the joint distribution of quality and COI when these outliers are removed.

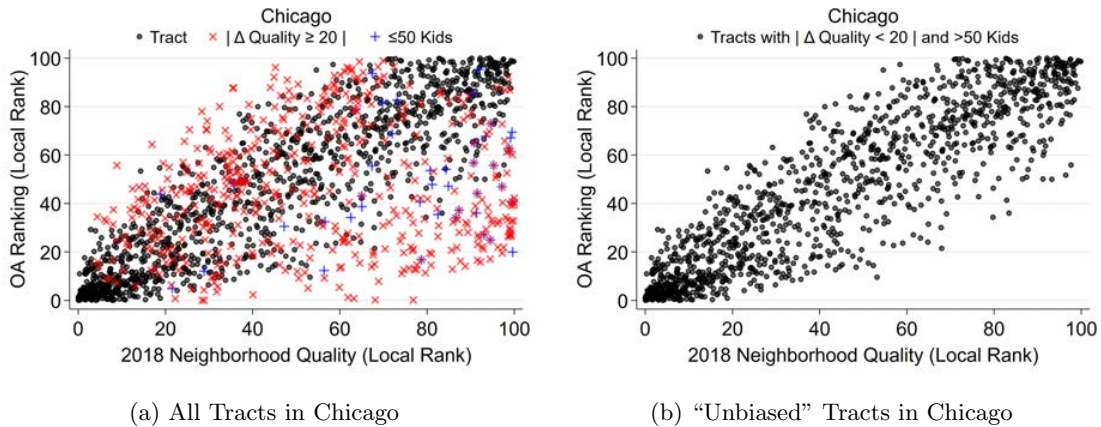


Figure 13: Disagreement in Rankings in Chicago

Note: These figures show the joint distribution of the local ranks of tracts in terms of 2018 neighborhood quality and mean family income pooled over race/ethnicity and gender as estimated in the Opportunity Atlas (OA). The left panel flags tracts that either experienced a change in quality (in the local distribution) between 1990 and 2018 of at least 20 percentile points, or else that had less than 50 children in the OA sample age range in the 1990 Census. The right panel shows only those tracts that are not subject to these two sources of uncertainty in OA estimates. Appendix J.1 shows similar figures for other metros.

In practice, PHAs are likely to make discretionary changes to move rankings toward those implied by current outcomes when drawing an opportunity map (See Chapter 3 of Scott et al. (2013), Appendix A of Bergman et al. (2020a), or Weismann et al. (2020)). The evidence on uncertainty and bias just presented suggest that such adjustments are important for the neighborhood externalities experienced by HMP participants.

5 Regional Partnerships for Porting Vouchers across Jurisdictions

A central difference between Gautreaux or the BHMP as opposed to MTO or CMTO is that Gautreaux and the BHMP have been implemented as regional partnerships, encouraging residents to move beyond a single PHA’s jurisdiction and throughout their respective metropolitan areas. Because voucher holders rarely port their vouchers across PHA jurisdictions (Garboden (2021)), it is likely that obstacles to portability across PHA jurisdictions could have constrained participants’ choices in MTO in ways that were not experienced in Gautreaux or the BHMP. Consistent with PHA jurisdictions constraining mobility, residential moves in MTO were shorter distances and more likely to be within city and school district boundaries than those experienced in Gautreaux or the BHMP. For MTO experimental compliers, two-thirds of initial lease ups were within their site’s central city (de Souza Briggs et al. (2010), p 150) and 70 percent of initial lease ups were within the same school district (de Souza Briggs et al. (2008), p 61), with less than 5 percent of the experimental group moving more than 10 miles from their baseline address due to MTO (Kling et al. (2007b)). Nearly all suburban movers in Gautreaux changed school districts (Rubinowitz and Rosenbaum (2000)), and the mean suburban mover was in a location 18 miles from their original address 6-20 years after initial lease-up (Keels et al. (2005)). Only twelve percent of BHMP participants stayed within the city (DeLuca and Rosenblatt (2017)), with three quarters of suburban movers leaving city schools (DeLuca et al. (2016)) and average initial moves of 11 miles (DeLuca and Rosenblatt (2017)).

Given the potential importance of regional partnerships allowing for voucher holders to easily port their vouchers across PHA boundaries, we simulate a version of the baseline HMP with no porting across PHAs. Measuring PHA jurisdictions is difficult, as the HCV program is administered by about 2,200 state and local PHAs (HUD (2021b)). Additionally, it is not clear how many small PHAs would fit into an HMP. This issue is especially pronounced in the central counties of metros, which often have separate PHAs for cities or incorporated areas.

Appendix Figure 49a shows this issue for several metros, including the metropolitan division of Chicago, using HUD data on the boundaries of PHA jurisdictions (HUD (2021a)). In light blue is the city of Chicago, in dark blue is the remainder of Cook County, Illinois, and in white inside Cook County are small cities with their own PHAs: Park Forest, Cicero, Oak Park, and Maywood. In many metros it is not clear from the data alone how jurisdictions overlap.

We resolve the issues of measurement and realism of HMP implementation by making the following assumptions. First, we assume that each county has its own PHA. Second, within the

central county of each metro, we check whether there is a separate PHA serving a central city within that county. If so, then we use boundaries on Census Designated Places (CDPs) to delineate the boundaries of the central city, and we assume that the central city PHA serves the central city CDP and the central county PHA serves the remainder of the central county outside the central city. Figure 49b shows how these assumptions create our assumed PHA boundaries in a few metros. Appendix L lists the information we used about PHAs in central counties.

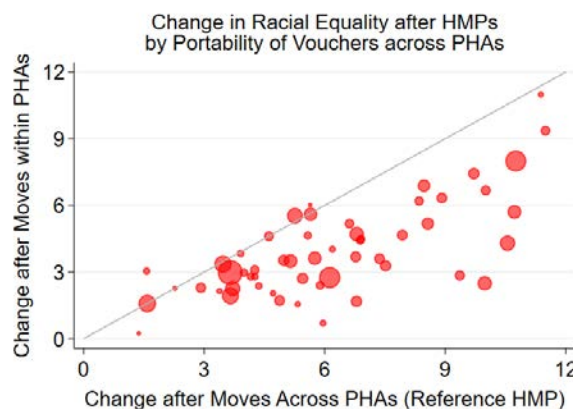


Figure 14: No Porting HMPs

Note: This figure shows the results of the reference HMP on the x -axis along with a “No Porting” HMP on the y -axis. In the reference HMP moves are allowed across PHA jurisdictions, and in the “No Porting” HMP moves are only allowed within PHA jurisdictions.

We consider a “No Porting” HMP that changes the baseline HMP by targeting neighborhoods with greater than 30 percent poverty and those in the bottom third of tracts, in that order, but now only within each PHA’s jurisdiction. Likewise, the “No Porting” HMP only moves participants to the top third of tracts within each PHA’s jurisdiction. Figure 14 shows the results of the “No Porting” HMP. We see that in many metros the effectiveness of the HMP falls considerably. These results quantify the benefits of forming regional partnerships (Scott et al. (2013)).

6 Constraints and Preferences Related to Household Choice

6.1 Rental Housing Supply

The supply of rental housing is one of the most important constraints HMP practitioners face. Polikoff (2006) describes how supply constraints emerged early on in the 1970s as a major issue for the implementation of Gautreaux (Chapter 5.3). Today the supply of rental units in high-opportunity areas is an eligibility criterion facing PHAs applying for HUD’s HCV Mobility Demonstration (HUD (2020)).

Supply constraints can be especially binding for voucher holders due to barriers that limit supply like search costs, information frictions (Bergman et al. (2020b)), and landlord avoidance of voucher holders (Phillips (2017); Aliprantis et al. (2022)). Some PHA-provided services can therefore relax

supply constraints, with housing search assistance and landlord outreach being two key services provided by the BHMP (Cossyleon et al. (2020)). But such services are not always given priority when implementing housing programs (Popkin et al. (2003)). Should they be? And are there any other strategies for expanding the supply of rental housing available to HMP participants?

Figure 15a shows the results of the reference HMP altered so that families move uniformly to all targeted neighborhoods, regardless of each neighborhood’s supply of rental housing. The metros all being located well above the 45 degree line indicates that relaxing the supply constraint has a major effect on the success of the HMPs.

Figure 15b shows an approach to relaxing supply constraints; targeting lower-ranked tracts with a greater supply of rental units.¹⁷ We find that targeting the middle third of tracts as opposed to the top third of tracts actually reduces racial inequality much more than the baseline HMP; generating as much improvement as eliminating the supply constraint. Table 2 helps explain the results of this HMP. In most cities, the share of supply-constrained tracts is much higher in the top third of tracts than it is in the bottom third of tracts. The resulting supply of units in the middle third of tracts is higher than in the top third of tracts.

The results from HMPs targeting the middle third of tracts raises a host of questions that appear unresolved since the time of Gautreaux (Rosenbaum (1995)). Are original residents in middle third tracts more likely to move out in response to program participants moving into the tract (Caetano and Maheshri (2021))? What is the marginal improvement in the externalities in top third of tracts versus the middle third (Weinberg et al. (2004))? And might program participants be more successful in finding stable housing or social integration in middle third neighborhoods?

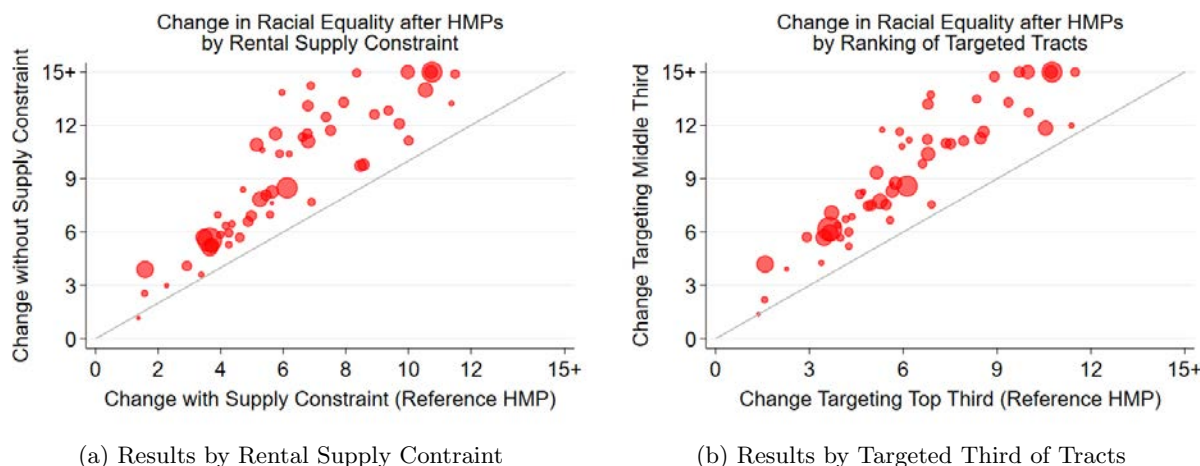


Figure 15: Relaxing Housing Supply Constraints

Note: The left panel compares the Reference HMP, which constrains the number of families moving to a targeted tract based on the supply of rental housing in the tract, with an HMP that moves 30 families to targeted tracts, regardless of their rental housing supply. The right panel compares the Reference HMP, which targets the top third of tracts in each metro, with an HMP that targets the middle third of tracts in each metro.

¹⁷Recall that in each of these HMPs, the size of the program is being constrained not by the number of vouchers, but by the ability of receiving tracts to absorb no more than 30 vouchers per tract.

Table 2: Rental Housing Supply

MSA	Share Supply- Constrained by Third		Avg. Rental Supply by Third	
	Middle	Top	Middle	Top
Baltimore	63	84	28	16
Chicago	67	78	26	19
DC	65	79	25	20
Dallas	47	67	36	26
Charlotte	58	65	32	27
Seattle	55	59	32	31

Note: This table reports statistics on the rental housing supply in select metros. The rental supply in a tract is 7.5 percent of its rental units that are 2 bedrooms or larger, and a tract is supply constrained if this number is less than 30; Section 3.3 provides relevant discussion. The highlighted metros are chosen for their prominent HMPs or early adoption of Small Area Fair Market Rents.

6.2 Access to Transportation

In general, geographic mobility may be a less important spatial friction than information and referral networks (Schmutz and Sidibé (2019); Heise and Porzio (2021); Miller and Schmutte (2021)). However, in the context of low-income Americans residing in neighborhoods of concentrated poverty, access to transportation, especially an automobile, could play a relatively large role in accessing economic opportunity (Gobillon et al. (2007)). Jobs in American cities have suburbanized (Miller (2021)); transit mode is a key determinant of the commute times that low-wage employers consider in hiring decisions (bunton et al. (2022); Phillips (2020)), and access to a car improves labor market outcomes (Gurley and Bruce (2005), Baum (2009), Ong (2002)).¹⁸

Car ownership has represented a central barrier to suburban moves in previous HMPs and consent decree programs (Polikoff (2006), Popkin et al. (2003)). In MTO, less than 40 percent of program participants had access to a running vehicle (Kling et al. (2007b), Table F12). Residential outcomes in MTO could be explained by the lack of access to a car, much as the need for access to public transportation can explain the urbanization of the poor in the US (Glaeser et al. (2008)). The neighborhood effects on adult labor market outcomes observed in MTO could also be explained by access to a car (Blumenberg and Pierce (2014); Pendall et al. (2016); Aliprantis and Richter (2020); Pinto (2019)).

We assess the importance of access to a car by conducting an HMP that modifies the reference HMP to only target tracts with access to public transportation. We use data from The National Transit Map, which contains GTFS data collected by the Bureau of Transportation Statistics (BTS (2021)), augmented by additional GTFS data downloaded from Open Mobility Data. We define a tract as having access to public transit if its centroid is within half a mile of a transit stop.¹⁹ Figure

¹⁸The finding that positive income shocks are often spent on cars (Aaronson et al. (2012); Imbens et al. (2001); West et al. (2021)) could therefore represent access to economic opportunity for low-income households.

¹⁹Appendix M describes the construction of our transportation data set and provides some descriptive statistics.

16a shows that access to public transportation tends to be negatively correlated with neighborhood quality, and that there is considerable variation across metros in the strength and levels of this relationship.

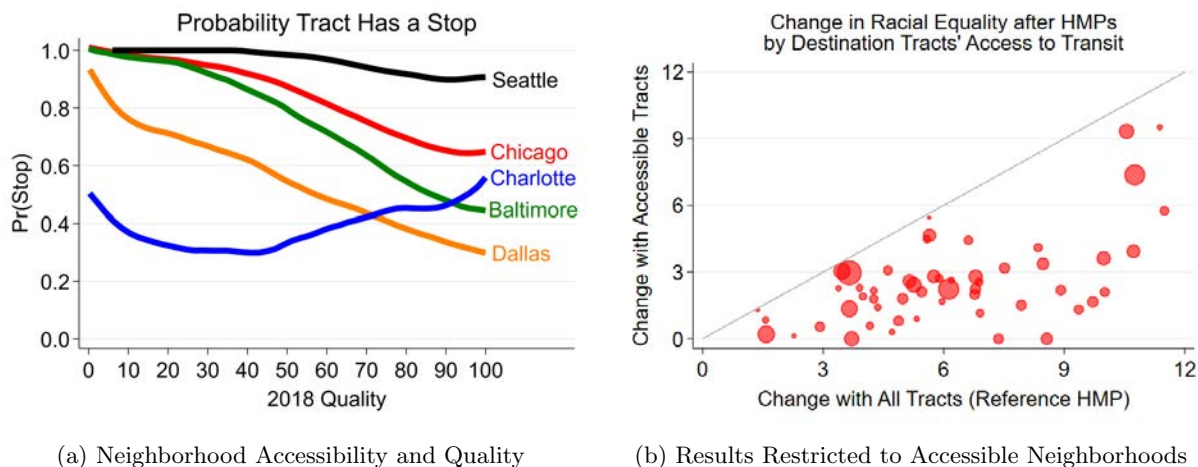


Figure 16: Access to Public Transportation

Note: The left panel shows local linear regression estimates of metro-level linear probability models summarizing the probability that a tract's centroid is within half a mile of a public transit stop as a function of neighborhood quality. The right panel shows the results of the Reference HMP plotted against the results of an HMP in which participants only move to tracts with access to public transportation. Access to transit is defined as a tract's centroid being within half a mile of a transit stop as measured in the National Transportation Map.

Figure 16b shows that the effect on racial equality plummets when HMP participants only move to transit-accessible tracts. Given the massive decline when only moving to accessible tracts, along with the fact that it is not clear that access to public transportation is sufficient for access to employment (Sanchez et al. (2004), Smart and Klein (2020)), suggests that supplementing program design with access to cars could improve the success of HMPs (Pendall et al. (2016)).

7 Constraints on Program Design

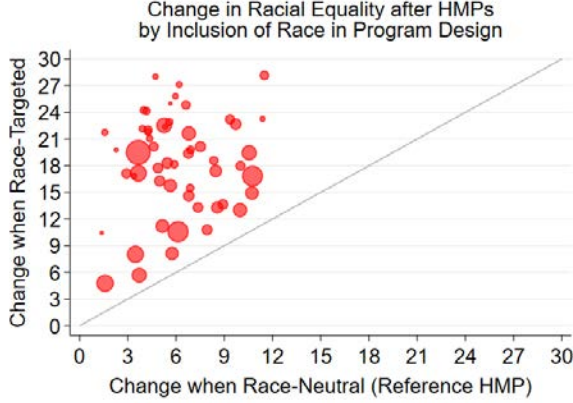


Figure 17: Race-Targeted Program Design

Note: This figure shows the results of the reference HMP plotted against the results of an HMP in which the only participants are poor residents who are Black.

Table 3: Black Movers by HMP
(Percentage of Total Movers)

MSA	HMP	
	Reference	Race-Targeted
Baltimore	86	100
Chicago	70	100
DC	64	100
Dallas	40	100
Charlotte	58	100
Seattle	20	100

Note: This table reports the percentage of movers who were Black in the reference HMP and in an HMP targeting Black poor residents. The highlighted metros are chosen for their prominent HMPs or early adoption of Small Area Fair Market Rents.

The US Supreme Court’s interpretation of the US Constitution is constantly evolving, and the history of interpretation with respect to race is full of major changes. Thus, while the current legal precedent would indicate that designing an HMP that targeted only Black participants would be unconstitutional, one could imagine a change in interpretation that would render such a policy legal. What would be the implications of an HMP designed explicitly around race and Black participants?

Figure 17 shows that an HMP targeting only Black participants would have a substantially larger impact on racial equality than the race-neutral reference HMP. Table 3 characterizes this HMP’s additional success as being a function of its more directed focus on Black residents.

8 Summarizing Results

We discuss here the differences in results for the alternative HMP designs explored above. To summarize our results across metros, we compute the change in racial equality resulting from reference HMP h relative to the racial equality found in the data in each metro m . Then we sum across metros weighting by π_m , the share of the Black poor in all 54 metros that resides in metro m . We then use this change to normalize the analogous population-weighted average change in racial equality resulting from alternative HMP h' relative to the racial equality found in the data. This allows us to report results in terms of the average improvement from an HMP as a percentage of the reference HMP:

$$\text{Average Improvement (\% of Reference HMP)} = 100 \times \frac{\sum_{m=1}^{54} [RE_m(h') - RE_m(\text{data})] \times \pi_m}{\sum_{m=1}^{54} [RE_m(h) - RE_m(\text{data})] \times \pi_m}.$$

Recall that the reference HMP is one in which poor residents living in neighborhoods of concentrated poverty are encouraged to move to tracts in the top third of their metro area as ranked by “neighborhood quality,” and a supply constraint binds in receiving neighborhoods.

Figure 18 plots the results for the reference HMP along with several HMPs examined earlier that differ in one design feature relative to the reference HMP. Many receiving neighborhoods have little access to public transportation; if transit is important to movers, this may seriously impact the effectiveness of an HMP. An HMP where participants only move to tracts with access to public transportation is just 46 percent as successful as the reference HMP at improving racial equality. Access to a car, implicitly assumed in the reference HMP, appears to be a major factor in HMP success.

HMPs administered by regional partnerships are likely to be more successful at combating racial segregation than are HMPs administered by individual PHAs. An HMP administered by individual PHAs in which participants cannot port their vouchers across PHA jurisdictions is 64 percent as successful as the reference HMP.

Across our simulations, an HMP does not move more than 30 families to a single census tract. Most models also impose a constraint that caps neighborhoods to receive no more families than would occupy 7.5 percent of rental units that are at least two bedrooms in size. An HMP that permits up to 30 movers to occupy as many as 100 percent of available rental units in receiving neighborhoods would generate 154 percent of the success of the reference HMP. An HMP might broaden access to existing units through landlord outreach and tenant counseling during search.

We also test the impact of targeting more marginal moves in neighborhood quality by targeting the middle third of tracts rather than the upper third. On the one hand, such moves do less to improve racial equality than more dramatic moves to high-opportunity neighborhoods by definition. On the other hand, the rental housing supply may be larger in such neighborhoods, enabling more moves in the aggregate. In our simulations, the increase in housing supply from targeting this set of receiving neighborhoods outweighs the effect of mechanically smaller changes in quality. Such an HMP would result in similar improvements in racial equality to that which relaxes the supply constraint in high quality neighborhoods. And an HMP targeting the middle third of tracts in a metro, rather than the top third, would generate 153 percent of the success of the reference HMP.

Figure 19a highlights the change in racial equality that would result from designing an HMP around the alternative rankings of neighborhoods considered in our analysis, the OA and the COI. For HMPs that reach all eligible receiving neighborhoods, the difference due to changes in the chosen ranking are considerably smaller than the differences due to changing other design features. An HMP targeting the OA or COI would generate, respectively, 86 and 92 percent of the success of the reference HMP targeting quality, as measured by quality. As discussed in Section 4, it is unclear that disagreements between these measures capture differences in neighborhood externalities rather than simply bias or statistical noise.

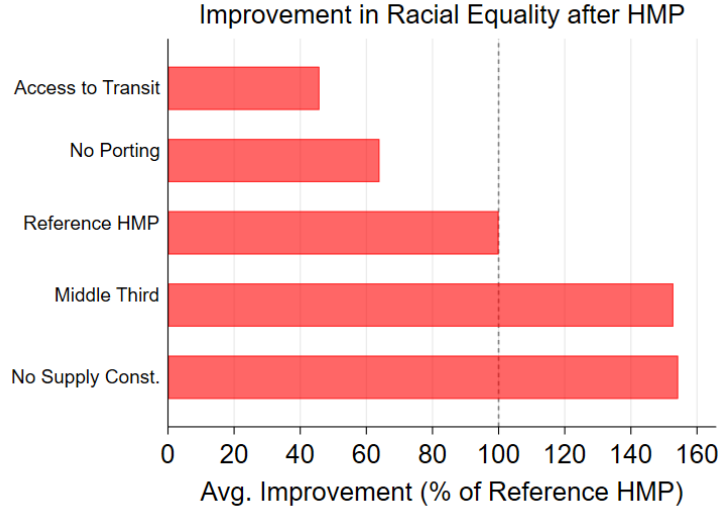


Figure 18: Average Improvement in Racial Equality across All Metros

Note: This figure shows the average improvement in racial quality across all metros for the reference HMP along with several HMPs that change one design feature. Each of the HMPs is described in detail in the main text, as is the measure of racial equality and associated measure of average improvement.

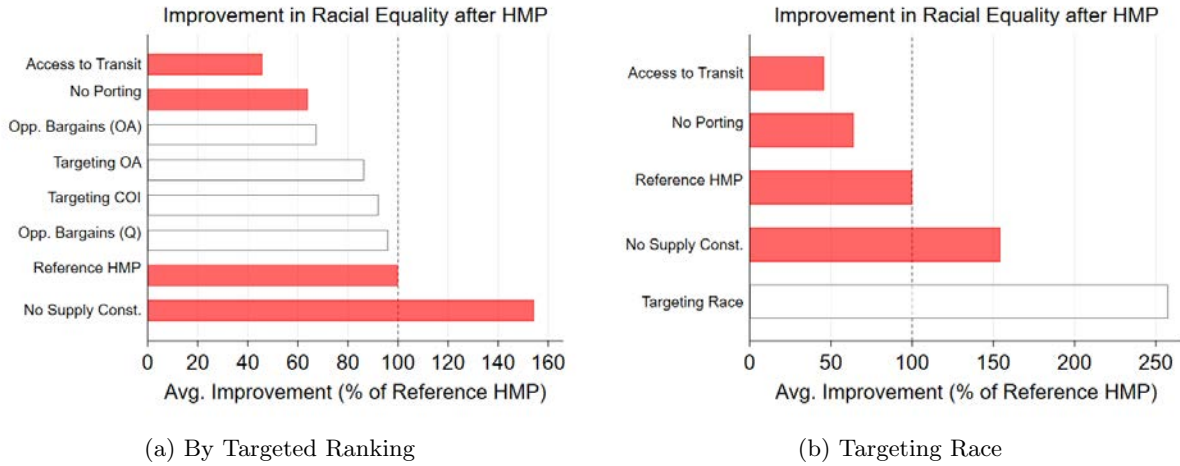


Figure 19: Average Improvement in Racial Equality across All Metros

Note: The left panel shows the average improvement in racial quality across all metros for the reference HMP along with several HMPs that change one or two design features, highlighting HMPs that change the targeted ranking or the rent in targeted tracts. The opportunity bargain HMPs are 1/4 the size of the reference HMP, and their average improvement is scaled relative to a 1/4-sized version of the reference HMP. The right panel shows the average improvement in racial quality across all metros for the reference HMP along with several HMPs that change one design features, highlighting an HMP targeting race. Each of the HMPs is described in detail in the main text, as is the measure of racial equality and associated measure of average improvement.

The targeted ranking matters quite a bit, though, when targeting opportunity bargains, which are the least expensive among the top third of tracts according to a given ranking. We show this by shrinking the scale of the HMP and preferentially filling the least expensive opportunity neighborhoods first. While targeting opportunity bargains identified by quality has little impact

on HMP success, at 96 percent of the reference HMP, targeting opportunity bargains as identified by the OA has a large impact on HMP success. The strong negative selection of low ranked tracts among OA-identified opportunity bargains results in an HMP generating only 67 percent of the success of the reference HMP. This suggests that opportunity bargains as identified by the OA are neighborhoods which disagree particularly with the quality measure, and highlights the need to understand what drives differences in these rankings when selecting one for housing mobility programs.

Finally, 19b shows that, were HMPs able to specifically target Black residents, the resulting programs would generate more than two and a half times the success as the reference HMP, which is race-blind.

9 Conclusion

Public policy in the United States has often acted as a force increasing residential segregation by race (Polikoff (2006); Rothstein (2017)). A desire to find public housing policies reversing these effects is driven by the view that residential segregation is a central contributor to maintaining racial inequality, through the means of limiting social interactions across racial groups, segregating schools, and separating job referral networks (Bayer (2019); Pew Research Center (2019); Meyer (2000); Miller and Schmutte (2021)). If barriers to residential integration are not only financial (Aliprantis et al. (2022), Bergman et al. (2020a)), then perhaps the design features of previous Housing Mobility Programs (HMPs) offer lessons for both what barriers prevent residential integration, as well as how we can design future HMPs to achieve this goal. The variation in design features and residential outcomes in previous HMPs suggests this to be a fruitful direction for research.

This paper studied how design features lead to residential outcomes in HMPs, with a focus on quantifying the success of HMPs in increasing the equality of the neighborhoods in which Black and white poor residents of large metros reside. We first considered the variable used to rank tracts as a feature of HMP design. We do not know the best way to rank neighborhoods to leverage their effects on HMP participants. The reason is the fundamental problem of neighborhood effects research, which is that the endogenous sorting of residents into their neighborhoods can make the observed outcomes in a neighborhood a biased estimate of the causal effects of the neighborhood (Graham (2018)). We show that the problem of bias remains when using administrative data on the outcomes of tracts’ previous residents, such as those estimated in the Opportunity Atlas.

We investigate other potential strengths of using administrative data to rank and target neighborhoods, and find that the structure of our society tends to limit their usefulness, an issue with administrative data often labeled “bias in, bias out” (Mayson (2019)). For example, while we might want to target the outcomes of a tracts’ previous Black residents, we simply have not observed Black children grow up in most high-SES tracts. Similarly, income sorting generates both conceptual and statistical uncertainty when trying to target the outcomes of the children of the

low-income residents of a tract. Finally, using tracts’ previous residents to target opportunity bargains, or highly-ranked, low-rent tracts, tends to select tracts that are low-ranked in terms of today’s residents.

While we found limitations to the promise of using administrative data on neighborhoods’ previous residents for the sake of designing HMPs, we did find design features with major implications for HMP success. There are several ways that HMPs might expand the set of neighborhoods available to HMP participants. Consistent with the residential outcomes observed in previous HMPs like Gautreaux, the BHMP, MTO, and CMT0, regional partnerships facilitating the porting of vouchers across PHA jurisdictions appear central to HMP success. Ensuring HMP participants have access to cars would make a range of neighborhoods available that do not have access to public transportation. And targeting neighborhoods that are in the middle of the distribution, rather than at the top, would increase the supply of rental housing available. Although such a design raises questions about implementation, if the goal of HMPs is to address the issue of poor Black residents’ hyper-concentration in the lowest ranked neighborhoods, then this design could prove effective.

References

- Aaronson, D., S. Agarwal, and E. French (2012). The spending and debt response to minimum wage hikes. *American Economic Review* 102(7), 3111–39. DOI: 10.1257/aer.102.7.3111.
- Aaronson, D., D. Hartley, and B. Mazumder (2021). The effects of the 1930s HOLC “redlining” maps. *American Economic Journal: Economic Policy* 13(4), 355–92. DOI: 10.1257/pol.20190414.
- Agostinelli, F., M. Doepke, G. Sorrenti, and F. Zilibotti (2020). It takes a village: The economics of parenting with neighborhood and peer effects. *NBER Working Paper 27050*. DOI: 10.3386/w27050.
- Agostinelli, F., M. Lufade, and P. Martellini (2021). On the spatial determinants of educational access. *HCEO Working Paper 2021–042*. <https://ideas.repec.org/p/hka/wpaper/2021-042.html>.
- Ajunwa, I. (2019). The paradox of automation as anti-bias intervention. *Cardozo L. Rev.* 41, 1671.
- Aliprantis, D. and D. Carroll (2018). Neighborhood dynamics and the distribution of opportunity. *Quantitative Economics* 9(1), 247–303. DOI: 10.3982/QE785.
- Aliprantis, D., D. Carroll, and E. Young (2022). What explains neighborhood sorting by income and race? *Journal of Urban Economics*. Forthcoming.
- Aliprantis, D. and D. Kolliner (2015). Neighborhood poverty and quality in the Moving to Opportunity experiment. *Federal Reserve Bank of Cleveland Economic Commentary*. DOI: 10.26509/frbcec-201504.

- Aliprantis, D., H. Martin, and D. Phillips (2022). Landlords and access to opportunity. *Journal of Urban Economics* 129, Article 103420. DOI: 10.1016/j.jue.2021.103420.
- Aliprantis, D. and F. G.-C. Richter (2020). Evidence of neighborhood effects from Moving to Opportunity: LATEs of neighborhood quality. *The Review of Economics and Statistics* 102(4), 633–647. DOI: 10.1162/rest_a_00933.
- Altonji, J. G. and R. K. Mansfield (2018). Estimating group effects using averages of observables to control for sorting on unobservables: School and neighborhood effects. *American Economic Review* 108(10), 2902–46.
- Ananat, E. O. (2011). The wrong side(s) of the tracks: The causal effects of racial segregation on urban poverty and inequality. *American Economic Journal: Applied Economics* 3(2), 34–66. DOI: 10.1257/app.3.2.34.
- Anderson, E. (2020). *Black in White Space*. University of Chicago Press.
- Angrist, J. D. and K. Lang (2004). Does school integration generate peer effects? Evidence from Boston’s Metco program. *American Economic Review* 94(5), 1613–1634. DOI: 10.1257/0002828043052169.
- Baum, C. L. (2009). The effects of vehicle ownership on employment. *Journal of Urban Economics* 66(3), 151–163.
- Baum-Snow, N., D. A. Hartley, and K. O. Lee (2019). The long-run effects of neighborhood change on incumbent families. *Federal Reserve Bank of Chicago Working paper 2019-2*. DOI: 10.21033/wp-2019-02.
- Bayer, P. (2019). The enduring legacy of our separate and unequal geography. In I. G. Ellen and J. P. Steil (Eds.), *The Dream Revisited: Contemporary Debates about Housing, Segregation, and Opportunity*. Columbia University Press.
- Bayer, P., S. L. Ross, and G. Topa (2008). Place of work and place of residence: Informal hiring networks and labor market outcomes. *Journal of Political Economy* 116(6), 1150–1196.
- Bergman, P., E. Chan, and A. Kapor (2020). Housing search frictions: Evidence from detailed search data and a field experiment. *NBER Working Paper 27209*.
- Bergman, P., R. Chetty, S. DeLuca, N. Hendren, L. F. Katz, and C. Palmer (2020). Creating Moves to Opportunity: Experimental evidence on barriers to neighborhood choice. *NBER Working Paper 26164*. DOI: 10.3386/w26164.
- Blumenberg, E. and G. Pierce (2014). A driving factor in mobility? Transportation’s role in connecting subsidized housing and employment outcomes in the Moving to Opportunity (MTO) program. *Journal of the American Planning Association* 80(1), 52–66. DOI:10.1080/01944363.2014.935267.

- Boustan, L. P. and R. A. Margo (2009). Race, segregation, and postal employment: New evidence on spatial mismatch. *Journal of Urban Economics* 65(1), 1–10.
- BTS (2021). *National Transit Map [dataset]*. Washington, DC: United States Department of Transportation, Bureau of Transportation Statistics. <https://www.bts.gov/national-transit-map/national-transit-map-data-maps-and-apps>.
- bunton, d. m., E. Fu, L. Rolheiser, and C. Severen (2022). The problem has existed over endless years: Racialized differences in commuting, 1980-2019. *Mimeo., Federal Reserve Bank of Philadelphia*.
- Caetano, G. and H. Macartney (2021). What determines school segregation? The crucial role of neighborhood factors. *Journal of Public Economics* 194, 104335.
- Caetano, G. and V. Maheshri (2017). School segregation and the identification of tipping behavior. *Journal of Public Economics* 148, 115–135.
- Caetano, G. and V. Maheshri (2021). A unified empirical framework to study segregation. *Mimeo., University of Georgia*.
- Chetty, R., J. N. Friedman, N. Hendren, M. R. Jones, and S. R. Porter (2020). The Opportunity Atlas: Mapping the childhood roots of social mobility. *Mimeo., Opportunity Insights*. DOI: 10.3386/w25147.
- Chetty, R., N. Hendren, M. R. Jones, and S. R. Porter (2020). Race and economic opportunity in the United States: An intergenerational perspective. *Quarterly Journal of Economics*. DOI: 10.1093/qje/qjz042.
- Chetty, R., N. Hendren, and L. F. Katz (2016). The effects of exposure to better neighborhoods on children: New evidence from the Moving to Opportunity experiment. *American Economic Review* 106(4), 855–902. DOI: 10.1257/aer.20150572.
- Chyn, E. (2018). Moved to opportunity: The long-run effects of public housing demolition on children. *American Economic Review* 108(10), 3028–3056. DOI: 10.1257/aer.20161352.
- Chyn, E. and D. Daruich (2021). An equilibrium analysis of the effects of neighborhood-based interventions on children. *Mimeo., Dartmouth College*.
- Collinson, R. and P. Ganong (2018). How do changes in housing voucher design affect rent and neighborhood quality? *American Economic Journal: Economic Policy* 10(2), 62–89. DOI: 10.1257/pol.20150176.
- Cossyleon, J. E., P. M. E. Garboden, and S. DeLuca (2020). *Recruiting Opportunity Landlords: Lessons from Landlords in Maryland*. Washington, DC: Poverty & Race Research Action Council and Mobility Works. <https://www.prrac.org/recruiting-opportunity-landlords-lessons-from-landlords-in-maryland-june-2020/>.

- Cunningham, M. K., M. Scott, C. Narducci, S. Hall, A. Stanczyk, J. O’Neil, and M. Galvez (2010). *Improving Neighborhood Location Outcomes in the Housing Choice Voucher Program: A Scan of Mobility Assistance Programs*. Washington, DC: What Works Collaborative.
- Darrah, J. and S. DeLuca (2014). “Living here has changed my whole perspective”: How escaping inner-city poverty shapes neighborhood and housing choice. *Journal of Policy Analysis and Management* 33(2), 350–384. DOI: 10.1002/pam.21758.
- Dastrup, S. and I. G. Ellen (2016). Linking residents to opportunity: Gentrification and public housing. *Cityscape* 18(3), 87–108. <https://www.huduser.gov/portal/periodicals/cityscpe/vol18num3/article4.html>.
- Dastrup, S., M. Finkel, and I. G. Ellen (2019). The effects of small area fair market rents on the neighborhood choices of families with children. *Cityscape* 21(3), 19–48. <https://www.huduser.gov/portal/periodicals/cityscpe/vol21num3/ch1.pdf>.
- Davis, M., J. Gregory, and D. Hartley (2019). The long-run effects of low-income housing on neighborhood composition. *Mimeo., Chicago Fed*.
- Davis, M. A., J. Gregory, D. A. Hartley, and K. T. K. Tan (2021a). Neighborhood effects and housing vouchers. *Quantitative Economics*.
- Davis, M. A., J. Gregory, D. A. Hartley, and K. T. K. Tan (2021b). Neighborhood effects and housing vouchers. *Quantitative Economics*.
- de Souza Briggs, X., K. S. Ferryman, S. J. Popkin, and M. Rendón (2008). Why did the Moving to Opportunity experiment not get young people into better schools? *Housing Policy Debate* 19(1), 53–91. DOI:10.1080/10511482.2008.9521627.
- de Souza Briggs, X., S. J. Popkin, and J. Goering (2010). *Moving to Opportunity: The Story of an American Experiment to Fight Ghetto Poverty*. Oxford University Press. DOI: 10.1093/acprof:oso/9780195393712.001.0001.
- DeLuca, S., G. J. Duncan, M. Keels, and R. M. Mendenhall (2010). Gautreaux mothers and their children: An update. *Housing Policy Debate* 20(1), 7–25. DOI:10.1080/10511481003599829.
- DeLuca, S., A. Rhodesa, and P. M. Garboden (2016). *The Power of Place: How Housing Policy Can Boost Educational Opportunity*. Baltimore: Johns Hopkins University/The Abell Foundation. <https://eric.ed.gov/?id=ED570914>.
- DeLuca, S. and P. Rosenblatt (2017). Walking away from *The Wire*: Housing mobility and neighborhood opportunity in Baltimore. *Housing Policy Debate* 27(4), 519–546. DOI: 10.1080/10511482.2017.1282884.

- Diamond, R. and T. McQuade (2019). Who wants affordable housing in their backyard? An equilibrium analysis of low-income property development. *Journal of Political Economy* 127(3), 1063–1117. DOI:10.1086/701354.
- diversitydatakids.org (2021). *Child Opportunity Index 2.0 database*. retrieved from https://data.diversitydatakids.org/dataset/coi20-child-opportunity-index-2-0-database?_external=True.
- Durlauf, S. N. (2004). Neighborhood Effects. In J. V. Henderson and J.-F. Thisse (Eds.), *Cities and Geography*, Volume 4 of *Handbook of Regional and Urban Economics*, Chapter 50, pp. 2173–2242. Elsevier. DOI: 10.1016/S1574-0080(04)80007-5.
- Durlauf, S. N. and J. J. Heckman (2020). An empirical analysis of racial differences in police use of force: A comment. *Journal of Political Economy* 128(10), 3998–4002.
- Edin, P.-A., P. Fredriksson, and O. Åslund (2003, 02). Ethnic enclaves and the economic success of immigrants-evidence from a natural experiment. *The Quarterly Journal of Economics* 118(1), 329–357. DOI:10.1162/00335530360535225.
- Feins, J. D., M. J. Holin, and A. A. Phipps (1996). *Moving to Opportunity for Fair Housing Demonstration: Program Operations Manual*. Cambridge, MA: Abt Associates. <https://www.huduser.gov/publications/pdf/mto-prog-manual.pdf>.
- Fryer, R. G. J. (2019). An empirical analysis of racial differences in police use of force. *Journal of Political Economy* 127(3), 1210–1261. DOI:10.1086/701423.
- Galiani, S., A. Murphy, and J. Pantano (2015). Estimating neighborhood choice models: Lessons from a housing assistance experiment. *American Economic Review* 105(11), 3385–3415. DOI: 10.1257/aer.20120737.
- Galster, G., A. Santiago, L. Stack, and J. Cutsinger (2016). Neighborhood effects on secondary school performance of Latino and African American youth: Evidence from a natural experiment in Denver. *Journal of Urban Economics* 93, 30–48.
- Galster, G. C. (2019). *Making Our Neighborhoods, Making Ourselves*. Chicago: University of Chicago Press. DOI: 10.7208/chicago/9780226599991.001.0001.
- Galvez, M. M. (2010). *What Do We Know about Housing Choice Voucher Program Location Outcomes? A Review of Recent Literature*. Urban Institute. <http://www.urban.org/sites/default/files/publication/29176/412218-What-Do-We-Know-About-Housing-Choice-Voucher-Program-Location-Outcomes-.PDF>.
- Garboden, P. M. E. (2021). You can’t get there from here: Mobility networks and the Housing Choice Voucher Program. *Journal of Planning Education and Research*, 0739456X211051774. DOI:10.1177/0739456X211051774.

- Glaeser, E. L., M. E. Kahn, and J. Rappaport (2008). Why do the poor live in cities? The role of public transportation. *Journal of Urban Economics* 63(1), 1–24. DOI:<https://doi.org/10.1016/j.jue.2006.12.004>.
- Gobillon, L., H. Selod, and Y. Zenou (2007). The mechanisms of spatial mismatch. *Urban Studies* 44(12), 2401–2427.
- Graham, B. S. (2018). Identifying and estimating neighborhood effects. *Journal of Economic Literature* 56(2), 450–500. DOI: 10.1257/jel.20160854.
- Gurley, T. and D. Bruce (2005). The effects of car access on employment outcomes for welfare recipients. *Journal of Urban Economics* 58(2), 250–272. DOI:<https://doi.org/10.1016/j.jue.2005.05.002>.
- Han, J. and M. Madaleno (2019). Neighborhoods, well-being, and families. *Mimeo., Loyola University Chicago*.
- Harriot, M. (2019, June 28). The racist Nextdoor. *The Root*. theroot.com/the-racist-nextdoor-1835939264.
- Heckman, J. and R. Landersø (2021). Lessons from Denmark about inequality and social mobility. *IZA Discussion Paper 14185*.
- Heise, S. and T. Porzio (2021). The aggregate and distributional effects of spatial frictions. *NBER Working Paper 28792*. DOI: 10.3386/w28792.
- Hellerstein, J. K., M. J. Kutzbach, and D. Neumark (2014). Do labor market networks have an important spatial dimension? *Journal of Urban Economics* 79, 39–58.
- Hellerstein, J. K., M. McInerney, and D. Neumark (2011). Neighbors and coworkers: The importance of residential labor market networks. *Journal of Labor Economics* 29(4), 659–695.
- Horn, K. M., I. G. Ellen, and A. E. Schwartz (2014). Do Housing Choice Voucher holders live near good schools? *Journal of Housing Economics* 23, 28–40. DOI: 10.1016/j.jhe.2013.11.005.
- HUD (2020, July 29). *Housing Choice Voucher Mobility Demonstration Notice*. Washington, DC: US Department of Housing and Urban Development (HUD). FR-6191-N-01.
- HUD (2021a, August 12). *Estimated Housing Authority Service Areas [dataset]*. Washington, DC: Department of Housing and Urban Development: Office of Policy Development and Research. <https://hub.arcgis.com/datasets/HUD::public-housing-authorities>.
- HUD (2021b). *Fiscal Year 2021: Budget in Brief*. Washington, DC: US Department of Housing and Urban Development (HUD). https://www.hud.gov/sites/dfiles/CFO/documents/BudgetinBrief_2020-02_06_Online.pdf.

- Imbens, G. W., D. B. Rubin, and B. I. Sacerdote (2001). Estimating the effect of unearned income on labor earnings, savings, and consumption: Evidence from a survey of lottery players. *American Economic Review* 91(4), 778–794.
- Jacob, B. A. (2004, March). Public housing, housing vouchers, and student achievement: Evidence from public housing demolitions in Chicago. *American Economic Review* 94(1), 233–258. DOI: 10.1257/000282804322970788.
- Keels, M., G. J. Duncan, S. DeLuca, R. Mendenhall, and J. Rosenbaum (2005). Fifteen years later: Can residential mobility programs provide a long-term escape from neighborhood segregation, crime, and poverty? *Demography* 42(1), 51–73. DOI: 10.1353/dem.2005.0005.
- Kleinberg, J., H. Lakkaraju, J. Leskovec, J. Ludwig, and S. Mullainathan (2017, 08). Human Decisions and Machine Predictions*. *The Quarterly Journal of Economics* 133(1), 237–293. DOI:10.1093/qje/qjx032.
- Kling, J. R., J. B. Liebman, and L. F. Katz (2007a). Experimental analysis of neighborhood effects. *Econometrica* 75(1), 83–119. DOI: 10.1111/j.1468-0262.2007.00733.x.
- Kling, J. R., J. B. Liebman, and L. F. Katz (2007b). Supplement to “Experimental analysis of neighborhood effects”: Web appendix. *Econometrica* 75(1), 83–119. DOI: 10.1111/j.1468-0262.2007.00733.x.
- Knox, D., W. Lowe, and J. Mummolo (2020). Administrative records mask racially biased policing. *American Political Science Review* 114(3), 619–637.
- Landersø, R. and J. J. Heckman (2017). The scandinavian fantasy: The sources of intergenerational mobility in denmark and the us. *The Scandinavian Journal of Economics* 119(1), 178–230. DOI:https://doi.org/10.1111/sjoe.12219.
- Lens, M. C. (2013). Safe, but could be safer: Why do HCVP households live in higher crime neighborhoods? *Cityscape* 15(3), 131–152. <https://www.huduser.gov/portal/periodicals/cityscpe/vol21num3/ch1.pdf>.
- Logan, J. R., B. Stults, and Z. Xu (2016). Validating population estimates for harmonized census tract data, 2000-2010. *Annals of the American Association of Geographers* 106(5), 1013–1029. DOI: 10.1080/24694452.2016.1187060.
- Logan, J. R., Z. Xu, and B. Stults (2014). Interpolating US decennial census tract data from as early as 1970 to 2010: A longitudinal tract database. *Professional Geographer* 66(3), 412–420. DOI: 10.1080/00330124.2014.905156.
- Logan, J. R., C. Zhang, B. Stults, and T. Gardner (2021). Improving estimates of neighborhood change with constant tract boundaries. *Applied Geography* (132). DOI: 10.1016/j.apgeog.2021.102476.

- Lott, A. (2021, September). *The Racial Wealth Gap and Access to Opportunity Neighborhoods*. Federal Reserve Bank of Cleveland and The Inclusive Communities Project.
- Malone, T. and C. L. Redfearn (2018). Shocks & Ossification: The durable hierarchy of neighborhoods in US metropolitan areas from 1970 to 2010. *Regional Science and Urban Economics* 69, 94–121. DOI: 10.1016/j.regsciurbeco.2018.01.002.
- Manski, C. F. (2015, September). Communicating uncertainty in official economic statistics: An appraisal fifty years after Morgenstern. *Journal of Economic Literature* 53(3), 631–53. DOI: 10.1257/jel.53.3.631.
- Manson, S., J. Schroeder, D. V. Riper, T. Kugler, and S. Ruggles (2020). *IPUMS National Historical Geographic Information System* (15.0 [dataset] ed.). Minneapolis, MN: IPUMS. DOI: 10.18128/D050.V15.0.
- Mayson, S. G. (2019). Bias in, bias out. *Yale Law Journal* 128(8), 2218–2301.
- McCartney, W. B. and A. M. Shah (2021, November). The economic importance of neighbors: Evidence from hyperlocal social influence effects in mortgage markets. *Journal of Urban Economics*, 103409. DOI: 10.1016/j.jue.2021.103409.
- Mendenhall, R., S. DeLuca, and G. Duncan (2006). Neighborhood resources, racial segregation, and economic mobility: Results from the Gautreaux program. *Social Science Research* 35(4), 892–923. DOI: 10.1016/j.ssresearch.2005.06.007.
- Meyer, S. G. (2000). *As Long as They Don't Move next Door: Segregation and Racial Conflict in American Neighborhoods*. Lanham, MD: Rowman & Littlefield.
- Miller, C. (2021). When work moves: Job suburbanization and Black employment. *Review of Economics and Statistics*.
- Miller, C. and I. M. Schmutte (2021). The dynamics of referral hiring and racial inequality: Evidence from Brazil. *NBER Working Paper 29246*. DOI: 10.3386/w29246.
- MobilityData IO Volunteers (2021). *OpenMobilityData*. Canada: MobilityData IO. [Database].
- Mogstad, M., J. P. Romano, A. Shaikh, and D. Wilhelm (2021). Inference for ranks with applications to mobility across neighborhoods and academic achievement across countries. *NBER Working Paper 26883*. DOI: 10.3386/w26883.
- Noelke, C., N. McArdle, M. Baek, N. Huntington, R. Huber, E. Hardy, and D. Acevedo-Garcia (2020). *Child Opportunity Index 2.0 Technical Documentation*.
- Noor, P. (2020). Can we trust AI not to further embed racial bias and prejudice? *British Medical Journal* 368. DOI:10.1136/bmj.m363.

- Obermeyer, Z., B. Powers, C. Vogeli, and S. Mullainathan (2019). Dissecting racial bias in an algorithm used to manage the health of populations. *Science* 366(6464), 447–453.
- Ong, P. M. (2002). Car ownership and welfare-to-work. *Journal of Policy Analysis and Management* 21(2), 239–252.
- Oreopoulos, P. (2003). The long-run consequences of living in a poor neighborhood. *The Quarterly Journal of Economics* 118(4), 1533–1575.
- Orr, L., J. Feins, R. Jacob, E. Beecroft, L. Sanbonmatsu, L. Katz, J. Liebman, and J. Kling (2003). *Moving to Opportunity for Fair Housing Demonstration Program: Interim Impacts Evaluation*.
- Pendall, R., E. Blumenberg, and C. Dawkins (2016). What if cities combined car-based solutions with transit to improve access to opportunity? *Washington, DC: Urban Institute*. <https://www.urban.org/research/publication/what-if-cities-combined-car-based-solutions-transit-improve-access-opportunity>.
- Pew Research Center (2019, May). *Americans See Advantages and Challenges in Country’s Growing Racial and Ethnic Diversity*.
- Phillips, D. C. (2017). Landlords avoid tenants who pay with vouchers. *Economics Letters* 151, 48–52. DOI: 10.1016/j.econlet.2016.11.036.
- Phillips, D. C. (2020). Do low-wage employers discriminate against applicants with long commutes? Evidence from a correspondence experiment. *Journal of Human Resources* 55(3), 864–901.
- Pinto, R. (2019). Noncompliance as a rational choice: A framework that exploits compromises in social experiments to identify causal effects. *Mimeo., UCLA*.
- Polikoff, A. (2006). *Waiting for Gautreaux*. Northwestern University Press.
- Popkin, S. J., G. C. Galster, K. Temkin, C. Herbig, D. K. Levy, and E. K. Richer (2003). Obstacles to desegregating public housing: Lessons learned from implementing eight consent decrees. *Journal of Policy Analysis and Management* 22(2), 179–199.
- Popkin, S. J., L. E. Harris, and M. K. Cunningham (2002). *Families in Transition: A Qualitative Analysis of the MTO Experience*. Washington, DC: Urban Institute.
- PRRAC (2005, February). *An Analysis Of The Thompson v. HUD Decision*. Washington, DC: Poverty & Race Research Action Council. <https://prrac.org/analysis-of-the-thompson-v-hud-decision/>.
- Reina, V., A. Acolin, and R. W. Bostic (2019). Section 8 vouchers and rent limits: Do small area fair market rent limits increase access to opportunity neighborhoods? An early evaluation. *Housing Policy Debate* 29(1), 44–61. DOI: 10.1080/10511482.2018.1476897.

- Rosenbaum, J. E. (1995). Changing the geography of opportunity by expanding residential choice: Lessons from the Gautreaux program. *Housing Policy Debate* 6(1), 231–269. DOI: 10.1080/10511482.1995.9521186.
- Rosenbaum, J. E. and S. DeLuca (2008). What kinds of neighborhoods change lives: The Chicago Gautreaux housing program and recent mobility programs. *Indiana Law Review* 41, 653–662.
- Ross, S. L. (2011). Social interactions within cities: Neighborhood environments and peer relationships. In N. Brooks, K. Donaghy, and G. Knapp (Eds.), *Handbook of Urban Economics and Planning*. Oxford University Press. DOI: 10.1093/oxfordhb/9780195380620.013.0010.
- Rothstein, R. (2017). *The Color of Law*. New York and London: W. W. Norton & Company.
- Rubinowitz, L. S. and J. E. Rosenbaum (2000). *Crossing the Class and Color Lines: From Public Housing to White Suburbia*. University of Chicago Press.
- Ruggles, S., S. Flood, R. Goeken, J. Grover, E. Meyer, J. Pacas, and M. Sobek (2018). *IPUMS USA: Version 8.0 [Dataset]*. Minneapolis, MN: IPUMS. DOI: 10.18128/D010.V8.0.
- Sanbonmatsu, L., J. R. Kling, G. J. Duncan, and J. Brooks-Gunn (2006). Neighborhoods and academic achievement: Results from the Moving to Opportunity experiment. *Journal of Human Resources* 41(4), 649–691. DOI: 10.3368/jhr.XLI.4.649.
- Sanchez, T. W., Q. Shen, and Z.-R. Peng (2004). Transit mobility, jobs access and low-income labour participation in us metropolitan areas. *Urban Studies* 41(7), 1313–1331. DOI:10.1080/0042098042000214815.
- Schmutte, I. M. (2015). Job referral networks and the determination of earnings in local labor markets. *Journal of Labor Economics* 33(1), 1–32.
- Schmutz, B. and M. Sidibé (2019). Frictional labour mobility. *The Review of Economic Studies* 86(4), 1779–1826.
- Schwartz, H. L., K. Mihaly, and B. Gala (2017). Encouraging residential moves to opportunity neighborhoods: An experiment testing incentives offered to housing voucher recipients. *Housing Policy Debate* 27(2), 230–260. DOI: 10.1080/10511482.2016.1212247.
- Scott, M. M., M. K. Cunningham, J. Biess, J. L. O’Neil, P. Tegeler, E. Gayles, and B. Sard (2013). *Expanding Choice: Practical Strategies for Building a Successful Housing Mobility Program*. Washington, DC: The Urban Institute/Poverty & Race Research Action Council. <https://www.urban.org/research/publication/expanding-choice-practical-strategies-building-successful-housing-mobility-program>.
- Shroder, M. (2002). Locational constraint, housing counseling, and successful lease-up in a randomized housing voucher experiment. *Journal of Urban Economics* 51(2), 315–338. DOI: 10.1006/juec.2001.2247.

- Smart, M. J. and N. J. Klein (2020). Disentangling the role of cars and transit in employment and labor earnings. *Transportation* 47, 1275–1309. DOI: 10.1007/s11116-018-9959-3.
- Tegeler, P. (2009). The future of race conscious goals in national housing policy. In M. A. Turner, S. J. Popkin, and L. Rawlings (Eds.), *Public Housing and the Legacy of Segregation*. The Urban Institute. https://www.prrac.org/pdf/Tegeler_Urban_Institute_Chapter.pdf.
- Weinberg, B., P. Reagan, and J. Yankow (2004). Do neighborhoods affect hours worked? Evidence from longitudinal data. *Journal of Labor Economics* 22(4), 891–924. DOI:10.1086/423158.
- Weismann, G., N. Rolfe, P. Kye, and B. Knudsen (2020). *Housing Mobility Programs in the US 2020*. Washington, DC: Poverty & Race Research Action Council and Mobility Works. <https://prrac.org/housing-mobility-program-in-the-us-2020/>.
- West, S., A. C. Baker, S. Samra, and E. Coltrera (2021). *Preliminary Analysis: SEED’s First Year*. Stockton, CA: The Stockton Economic Empowerment Demonstration (SEED). https://www.stocktondemonstration.org/s/SEED_Preliminary-Analysis-SEEDs-First-Year_Final-Report_Individual-Pages.pdf.
- Wodtke, G. T., F. Elwert, and D. J. Harding (2016). Neighborhood effect heterogeneity by family income and developmental period. *American Journal of Sociology* 121(4), 1168–1222. DOI:10.1086/684137.

Appendix to “What Determines the Success of Housing Mobility Programs?”

Dionissi Aliprantis Hal Martin Kristen Tauber

October 11, 2022

A Strengths and Weaknesses of Each Ranking

As a measure of neighborhood externalities, the strength of each ranking we consider tends to be a weakness of the other rankings. Table 5 summarizes the strengths and weakness of each measure.

The strength of neighborhood quality as a ranking of neighborhood externalities is that (Q-i) it can be easily calculated from timely Census data that is made publicly-available by the NHGIS. (Q-ii) While only negligibly more difficult to calculate than neighborhood poverty, neighborhood quality captures many of the additional variables thought to determine how a neighborhood affects its residents above and beyond poverty alone. Weaknesses of quality are that (Q-a) it may not capture all relevant neighborhood characteristics; and (Q-b) the characteristics included may not affect outcomes in a linear way or in a way that is homogeneous across individuals of different demographic groups.

Two strengths of the COI as a ranking of neighborhood externalities are that (COI-i) it can be calculated from timely data; and (COI-ii) it incorporates even more neighborhood characteristics thought to affect residents’ outcomes, like school district outcomes and pollution, from disparate datasets that contain information not available in the Census. Strength COI-ii helps to address weakness Q-a of quality by doing more to capture all relevant neighborhood characteristics, but this comes with the tradeoff of (COI-a), that the COI is more difficult to calculate, requiring the assembly of multiple datasets. Another weakness is a holdover from quality: (COI-b) the characteristics included may not affect outcomes in a linear way or in a way that is homogeneous across individuals of different demographic groups.

Two strengths of the OA as a ranking of neighborhood externalities are that it allows us to (OA-i) measure outcomes conditional on individual characteristics like race/ethnicity and gender; and (OA-ii) measure a wide variety of outcomes like incarceration, teenage pregnancy, and marriage. Three weaknesses of the OA as a measure of neighborhood effects are (OA-a) neighborhood sorting by individual-level demographic characteristics X_i resulting in small sample sizes; (OA-b) neighborhood sorting on unobservables resulting in bias; and (OA-c) neighborhood sorting over time resulting in bias. Weakness OA-a is an issue because OA rankings are estimated. Thus, to capture strength OA-i, we may be concerned for cases where sample sizes are small enough to make estimates noisy. Strength OA-i is particularly exciting because it could allow us to address weaknesses Q-b and COI-b. However, weaknesses OA-b and OA-c makes strength OA-i difficult

to gauge. Measuring outcomes does not overcome the fundamental issue in neighborhood effects research, neighborhood sorting. We do not know if realized outcomes reflect neighborhood effects or neighborhood sorting.

Table 4: Rankings of Neighborhoods

Neighborhood Quality: Aliprantis and Richter (2020)’s tract-level neighborhood quality index

Years: 2014-2018, 2013-2017, . . . , 2005-2009, 2000, 1990, 1980, 1970

Area: All census tracts

Sources: American Community Survey (ACS) from 2005–; Decennial censuses from 1970-2000; Longitudinal Tract Database (LTDB)

Construction: Created by using principal components analysis to combine tract-level ranks of six neighborhood characteristics into a single tract-level ranking of neighborhoods. Those six characteristics 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 Ratio 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. LTDB is used to interpolate earlier years into 2010 census tract boundaries.

COI: Brandeis University’s tract-level [Child Opportunity Index 2.0](#)

Years: 2013-2017 and 2008-2012

Area: All census tracts

Sources: 29 indicators from numerous sources including the American Community Survey (ACS), National Center for Education Statistics (NCES), Stanford Education Data Archive (SEDA), GreatSchools (GS) proprietary data, US Department of Education EDFacts, US Department of Education Office for Civil Rights Data Collection (CRDC), Environmental Protection Agency Risk-Screening Environmental Indicators (EPA RSEI), Centers for Disease Control and Prevention (CDC), Opportunity Atlas, RW Johnson Foundation 500 Cities Project

Construction: Created by combining tract-level measures of many neighborhood characteristics into a single tract-level ranking of neighborhoods.

OA: [Opportunity Atlas](#) tract-level income estimates

Years: 1978-83 birth cohorts

Area: Census tracts with sufficient observations

Sources: Census 2000 and 2010; Federal income tax returns in 1989, 1994, 1995, and 1998-2015

Construction: Estimate child’s expected income conditional on their parents’ household income; parameterize income distribution within a tract using a functional form estimated on the national income distribution.

Table 5: Strengths and Weaknesses of Neighborhood Rankings

Neighborhood Quality Strengths

Q-i: Measure can be easily calculated from timely census data that is made publicly-available by the NHGIS

Q-ii: Measure captures 6 key variables thought to determine neighborhood effects

Neighborhood Quality Weaknesses

Q-a: Measure may not capture all relevant neighborhood characteristics

Q-b: Linear effects of variables/homogeneous by X 's?

Q-c: Linear preferences over variables?

COI Strengths

COI-i: Measure can be calculated from timely data

COI-ii: Measure captures many measurable variables thought to determine neighborhood effects

COI Weaknesses

COI-a: Measure is more difficult to calculate than poverty or quality

COI-b: Linear effects of variables/homogeneous by X 's?

COI-c: Linear preferences over variables?

OA Strengths

OA-i: Measure can be made conditional on individual characteristics like race/ethnicity, parental income, and gender

OA-ii: Measure can be made for a wide variety of outcomes beyond income, like incarceration, teenage pregnancy, and marriage

OA Weaknesses

OA-a: Neighborhood sorting by individual characteristics resulting in small sample sizes

OA-b: Neighborhood sorting on unobservables resulting in bias

OA-c: Neighborhood sorting over time resulting in bias

B Variation in Neighborhood Rankings

Table 6: Variation Explained

Neighborhood Ranking	Independent Variable	R^2
2018 Quality	2018 Poverty	0.74
COI	2018 Poverty	0.70
OA	2018 Poverty	0.35
OA	1990 Poverty	0.33
OA	1990 Quality	0.39
2018 Quality	1990 Quality	0.67
2018 Quality	COI	0.86

Note: The top three rows are the relationships shown in the figure on the right. All regressions are weighted by the population at the time of measurement for the independent variable. The OA rank is in terms of the income estimates pooled over race/ethnicity for children from parents with 25th percentile incomes.

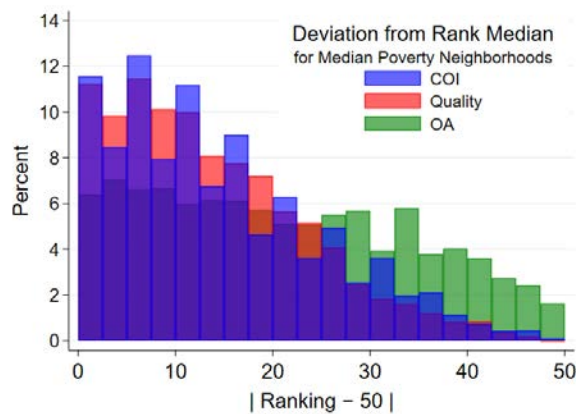


Figure 20: Variation in Other Rankings for Median Poverty Tracts

Note: The figure shows tracts that are between the 47.5th and 52.5th percentiles of the individual-level distribution of tract-level poverty rates in 2014-2018.

C Does Ranking Neighborhoods Improve HMP Success?

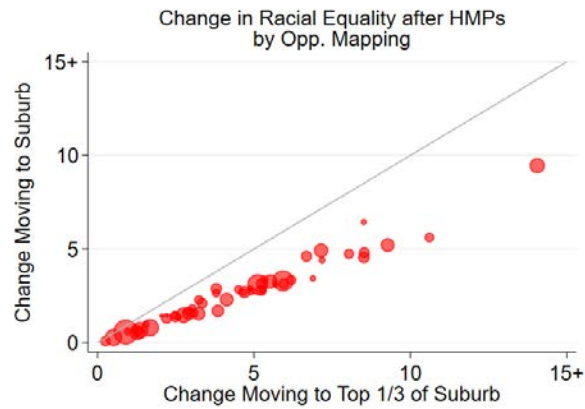


Figure 21: Anywhere in the Suburbs Versus the Top Third of Suburbs

Note: This figure shows a comparison between HMPs targeting any suburb (on the y -axis) versus HMPs targeting the top third of tracts in a metro's suburbs (on the x -axis). We define suburban tracts as any tract outside the metro's central county.)

D Measuring Kids by Poverty Status

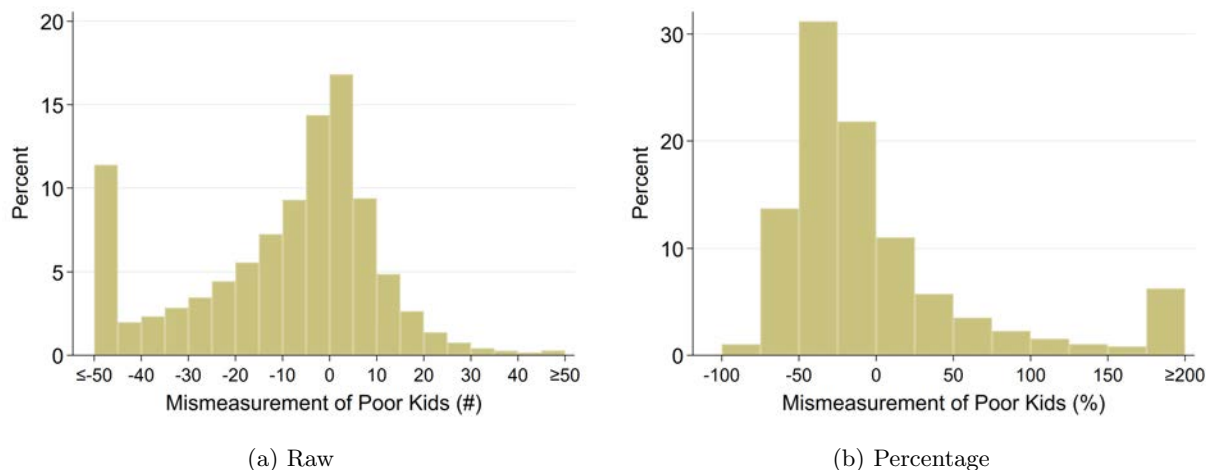


Figure 22: Examining Mismeasurement using Kids in Poverty

Note: We estimate the number of poor children in a tract, \hat{p} , as the share of a tract's families that are poor times the number of kids age 6-11 in the tract. In the NHGIS publicly-released 1990 Census data we can observe the true number of children aged 6-11 who are poor, p . In the left panel we compute mismeasurement as $\hat{p} - p$, and in the right panel we compute mismeasurement as $100 \times \frac{\hat{p} - p}{p}$. Note the asymmetric tails of mismeasurement: Poor children in a tract imply poor adults in a tract, but poor adults in a tract are not necessarily accompanied by children.

E Quantifying the Implications of Neighborhood Sorting

E.1 Model

A joint model of neighborhood sorting and potential outcomes, the two fundamental ingredients in a Rubin Causal Model (?) of neighborhood effects, allows us to express the bias from estimating potential outcomes using observed outcomes. Consider the ordered choice model developed in ? 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.²⁰

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 as

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

and

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

²⁰We 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 (?, ?, Agostinelli et al. (2021)). We also abstract from social interactions in selection, often referred to as the Stable Unit Treatment Value Assumption (SUTVA, ?), an assumption known to have major implications for identification in this setting (?, ?). This model also abstracts from dynamics; see Aliprantis and Carroll (2018) for a dynamic general equilibrium model of neighborhood effects. See ?, Graham (2018), ?, and Durlauf (2004) for literature reviews including more general models of neighborhood effects.

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

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

and

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

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

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

and

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

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. This is the identification problem that arises from neighborhood sorting. 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 .

E.2 Empirical Analysis

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). For the 1990 decennial Census data, when appropriate we impute count estimates into 2010 tract boundaries using the Longitudinal Tract Data Base (LTDB).²¹

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. (2020a)). 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 (?).²² Figure 23 presents US Census data characterizing this sorting in 1990. Figure 23a 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 23b 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.

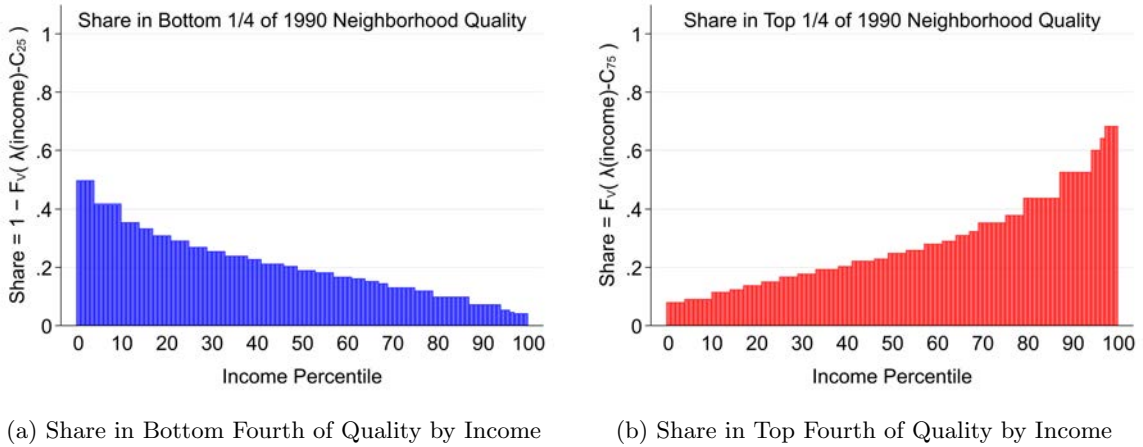


Figure 23: Sorting into Neighborhood Quality by Household Income

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.

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

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

²¹See descriptions in Logan et al. (2014), Logan et al. (2016), and Logan et al. (2021).

²²Trends in income sorting are more difficult to discern than levels (? , ? , ?), and patterns can vary by scale of observation (? , ?).

where

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

For our simulations we use assume $M = M_1 = M_4$ and $\sigma = \sigma_1 = \sigma_4$, so that differences across neighborhoods are driven by sorting (Landersø and Heckman (2017); Heckman and Landersø (2021)) and there are two parameters M and σ^2 in the model

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

We 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. (2020a)'s sample restrictions. Figure 24 shows the fit of the estimated model. The estimated model produces tails that are larger than the tails in the data, but broadly mimics the skewed distribution in the data.

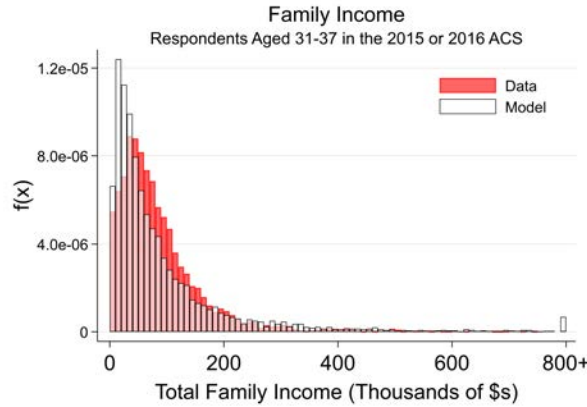


Figure 24: The Family Income Distribution

Note: This figure shows the family income distribution in the data and from the parameterized, estimated distribution used for simulations. See the text just above for an explanation of the parameterization and estimation.

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 Ratio 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. (2018)).

Given parental income x_i , we calculate

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

using

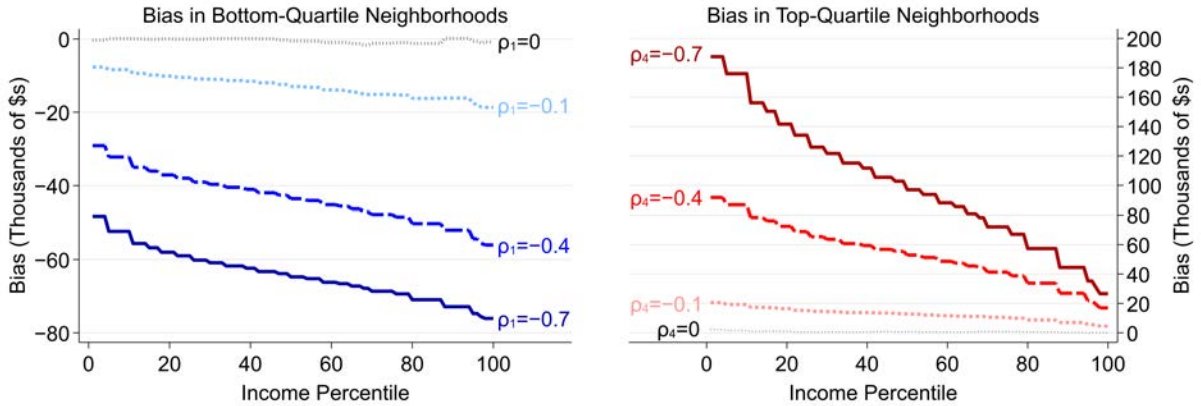
$$\mathbb{E}[Y_i|v_i > v_1^*(x_i)] \quad \text{and} \quad \mathbb{E}[Y_i|v_i < v_4^*(x_i)]$$

where the cutoffs $v_1^*(x_i)$ and $v_4^*(x_i)$ are obtained from the data displayed in Figure 23.

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 25 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 25: 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 25 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.

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 26 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:

$$\text{Bias}(n) = 100 \times \frac{\text{Bias}(d_i = 4, x_i = 25)}{\mathbb{E}[Y_i | q_i = 87, x_i = 25] - \mathbb{E}[Y_i | q_i = n, x_i = 25]}.$$

Figure 26 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.

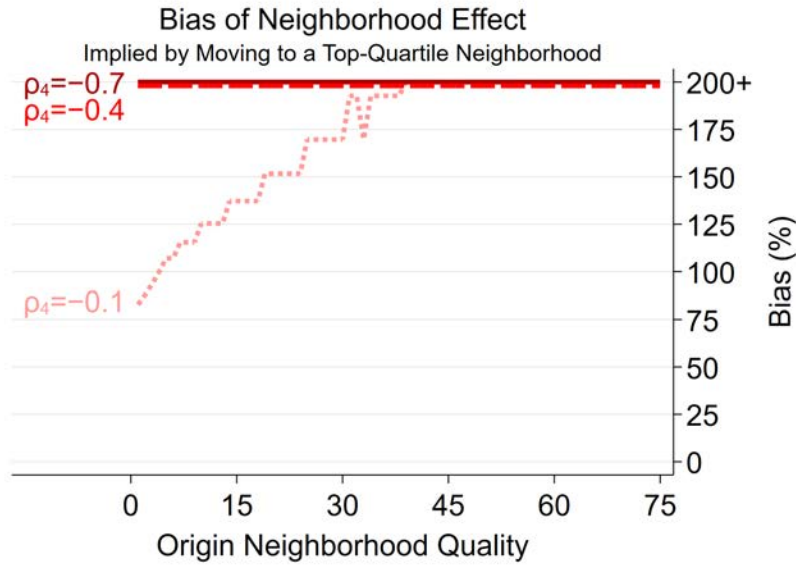


Figure 26: Bias by Origin Neighborhood Quality

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.

F Discussion of Chetty et al. (2020a)’s Approach to Sorting

Chetty et al. (2020a) conclude that approximately 30 to 40 percent of the variation in the OA estimates reflects neighborhood sorting using two research designs. The points we raise below, like those in the text, align with the concern that the variation from sorting may be bias rather than just noise.

F.1 Moving to Opportunity

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.

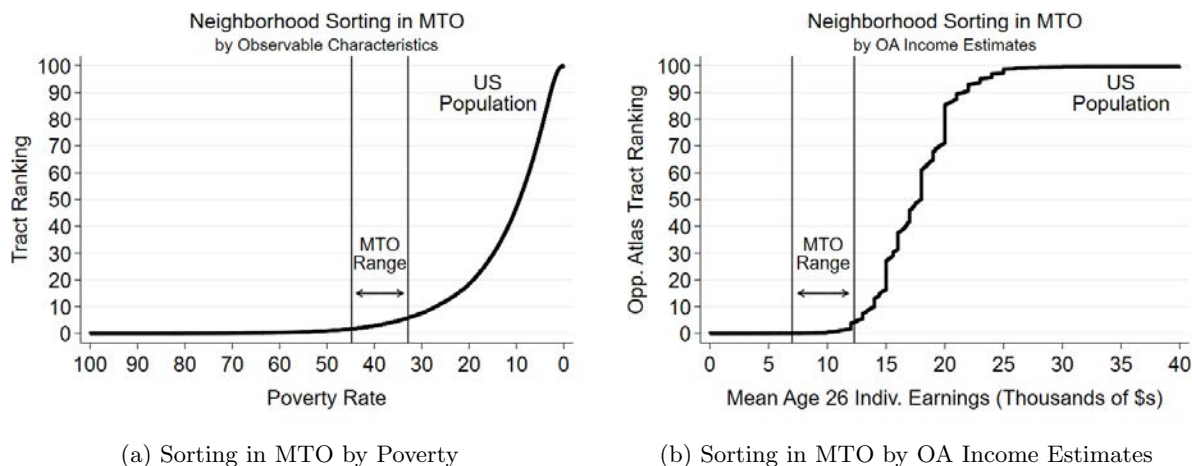


Figure 27: Neighborhood Sorting in Moving to Opportunity (MTO)

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. (2007a). 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. (2020a) Figure X.

Figure 27a 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. (2007a), ?, ?). 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 having moved participants within racially-segregated areas that are still likely to be disconnected from the mainstream economy (?, ?, ?).

Figure 27b shows that the dichotomy in interpretations of MTO based on tracts’ current residents’ outcomes extends to the interpretation of MTO based on the OA’s estimates of tracts’ previous residents’ outcomes.²³ 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.

Figures 28 and 29 revisit the OA dichotomy shown in Figure 27 to show that evidence on neighborhood effects from MTO is limited to low-ranked neighborhoods. Aliprantis and Richter (2020) specifies and estimates a model of neighborhood effects that account for the ways household choices result in non-random sorting across neighborhoods in MTO. Figure 28b shows the effects on those identified by the model to change (“Movers”) and not change (“Stayers”) neighborhood quality due to MTO. Figure 28a shows that these effects are from small changes in neighborhood quality.

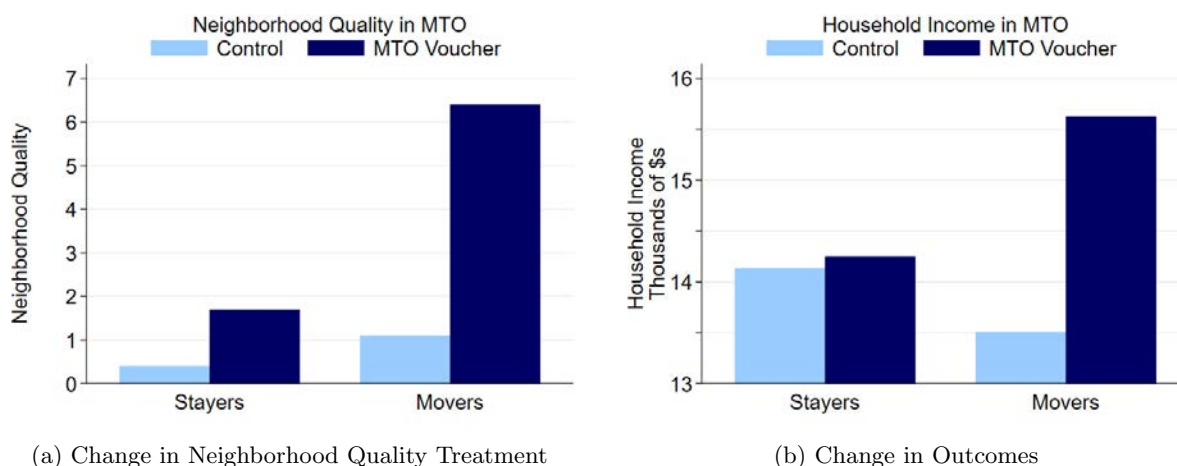


Figure 28: Modeling Potential Outcomes with the MTO Data

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. (2020a) 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. (2020a) 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.

Similarly, Figure 29a shows that when extrapolating based on tracts’ raw outcomes across the range in Chetty et al. (2020a) 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. We conclude that any exercise using the MTO data, including the robustness analysis in Chetty et al. (2020a), is necessarily confined to a highly-selected set of neighborhoods.

²³Predictions based on current and previous residents’ outcomes do not always match in MTO (? , ?).

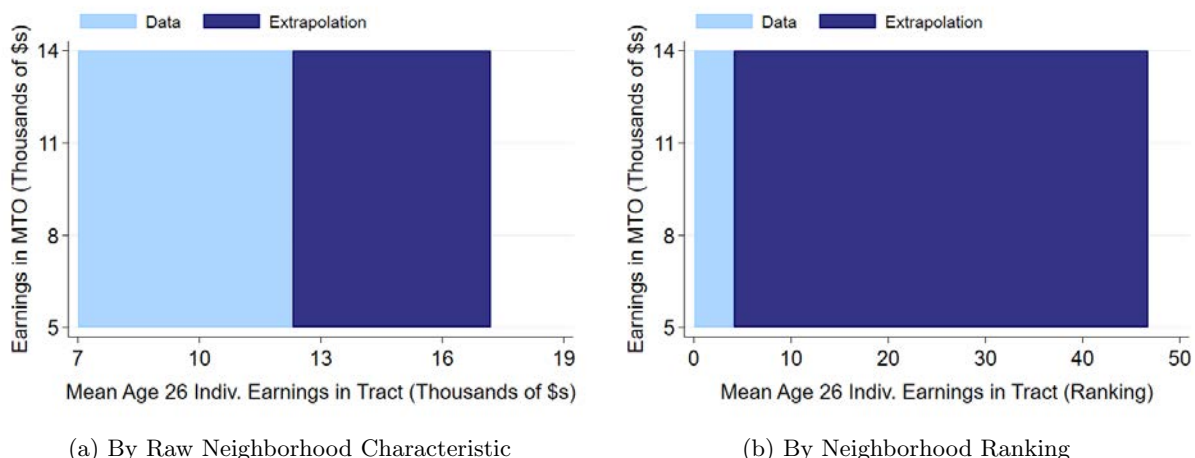


Figure 29: Modeling Potential Outcomes with the MTO Data

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. (2020a) 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. (2020a) 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.

F.2 Household Mobility

Here we focus on the second design used in Chetty et al. (2020a) to decompose the share of OA measures of neighborhood outcomes into neighborhood sorting and neighborhood effects. The second design uses household mobility over the life-cycle.

The results of the household mobility analysis provide compelling evidence on the heterogeneity of effects by age at move. There is little for us to add about the effects found, aside from how to appropriately extrapolate those effects to the larger set of neighborhood effects.

The moves examined in the household mobility analysis are selected with respect to endogenous household choice and distance. There is evidence that the degree of endogeneity of school moves, for example, matters for effects on academic performance (?). Perhaps more importantly, the endogeneity of the moves under investigation means that the identifying variation is primarily coming from small changes in neighborhood characteristics. Chetty et al. (2020a) note that when children move, they tend to move to an area very similar to the one in which they previously lived. This raises the possibility that results could be driven by extrapolation and/or a select set of moves to significantly different locations.

With respect to distance, the Chetty et al. (2020a) analysis only includes moves over 25 miles, and there is evidence of heterogeneous effects on academic performance by distance of move (Schwartz et al. (2017)). The degree of selection into moves of 25 miles or greater is significant, as these moves represent less than 7 percent of the OA sample (Chetty et al. (2020a), Appendix Table II). The highly-selected sample for which neighborhood effects are identified in the OA is analogous to the highly-selected sample for which neighborhood effects are identified in MTO (Aliprantis and

Richter (2020)).

While the household mobility analysis in Chetty et al. (2020a) provides valuable evidence on neighborhood effects, the details above point to some caveats: First, the select sample to which this evidence applies renders it far from conclusive about other samples. For example, there are many subpopulations and/or types of moves for which there could be large neighborhood effects on *adult* labor market outcomes. Interpreting the evidence from the OA as evidence that there are no neighborhood effects on adult labor market outcomes *in general* requires strong assumptions that are open to debate. In a similar context, interpretations of the evidence from MTO on adult labor market outcomes vary depending on what one assumes about selection (?).

Second, the heterogeneity of effects of moves less than and greater than 25 miles is notable (Chetty et al. (2020a), Appendix Table III). Chetty et al. (2020a) propose that the shorter moves are affected by measurement error when children, particularly in single parent households, continue to access their prior neighborhood. Taken at face value as evidence of neighborhood effects, this finding poses practical caveats for families looking to maximize the value of the OA as a navigating tool: while some high externality neighborhoods appear close to low-externality ones, this finding suggests families may need to forego nearby possibilities in order to maximize the effect of their new neighborhood.²⁴

It is not clear one should take these results at face value, however, if the families that spontaneously move at least 25 miles are unobservably different from those that move shorter distances and who do not move at all. One might view such effect heterogeneity in the context of the sorting model just presented. Families who move more than 25 miles are likely to have the lowest unobserved costs to mobility (i.e., the lowest V s), and therefore are unlikely to produce unbiased counterfactuals for those families who do not move or who move short distances (ie, those with high V s).

F.2.1 Targeting Neighborhood Outcomes in Dimensions Beyond Poverty

One lesson learned from MTO is that defining opportunity neighborhoods in an HMP in terms of a single poverty cutoff could allow for moves that do not generate large changes in the school or neighborhood externalities experienced by program participants (de Souza Briggs et al. (2008); ?). Chetty et al. (2020a) highlight this point by showing that an HMP with the same budget as MTO could have relocated participants to much higher-ranked tracts according to the OA. Section 4.3 examines cost considerations and the targeting of opportunity bargains, so here we focus on investigating whether the low-poverty (ie, less than 10 percent of residents in poverty) definition of opportunity neighborhoods in MTO also left considerable room for improved targeting

²⁴This would be consistent with the evidence from housing mobility programs. On average in the Gautreaux housing mobility program, suburban communities were 25 miles away from baseline locations, compared with seven miles for city moves (DeLuca et al. (2010)). The smaller program effects experienced in MTO could be explained by the fact that receiving an experimental voucher induced only 5 percent of participants to move more than 10 miles from their baseline address by the time of the interim evaluation (Kling et al. (2007b), Table F2). In the main text we hypothesize that such mobility patterns in MTO could be due to the difficulty of porting vouchers across public housing authorities.

of neighborhood quality and COI.

Within low-poverty neighborhoods in MTO cities in 2000, there was considerable variation in other neighborhood characteristics. Tracts to which MTO participants moved were negatively selected relative to characteristics other than poverty (Aliprantis and Kolliner (2015); Davis et al. (2021a)), likely due to the fact MTO moves were often made from and to Black neighborhoods (??). To generate negative selection into neighborhoods, we consider a “Pessimistic-MTO” HMP in which we move participants to the lowest-ranked tracts in their metro area that were also low-poverty in the 2014-2018 ACS. We then consider an “Optimistic-MTO” HMP in which we move an MTO-scaled number of participants to the highest-ranked tracts in their metro area while respecting supply constraints. Figure 30 shows the results for HMPs ranking tracts by OA, quality, and COI. The “Pessimistic-MTO” HMP on the x -axis results in average changes in each measure much lower than the average changes experiences by the “Optimistic-MTO” HMP on the y -axis moving participants to the highest ranked tracts.

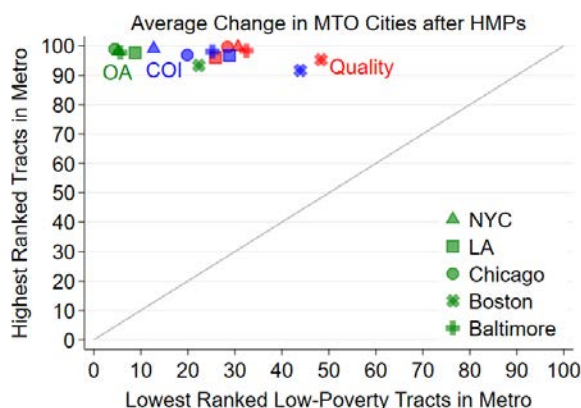


Figure 30: Targeting Neighborhood Characteristics other Than Poverty

Note: This figure compares “Optimistic” and “Pessimistic” versions of the MTO HMP. The “Optimistic MTO” simulation moves the number of MTO participants into the highest ranked tracts in the relevant metro, and is displayed on the y -axis. The “Pessimistic MTO” simulation moves the number of MTO participants into the lowest-ranked tracts that also met the low-poverty cutoff in MTO, less than 10 percent.

The possibility for better-targeting OA, quality, and COI is not due to complete disagreement between these rankings and poverty. Recall that Figure 27 shows that the OA provides a nearly-identical characterization of mobility in MTO as provided by poverty in Figure 2b. Instead, the possibility for better-targeting rankings other than poverty is due to the broad definition of “low-poverty” neighborhoods in MTO, which allowed for strongly negative selection into tracts along other dimensions. Figure 31 illustrates this point by showing that when the baseline HMP is designed to target the top third of metros’ tracts based on poverty, and when program participants move uniformly to those tracts, the resulting improvement in racial equality as measured by other rankings does not suggest the type of extreme sorting just shown, with quality resulting in 97 percent of the improvement obtained with poverty, COI resulting in 95, and OA resulting in 78.

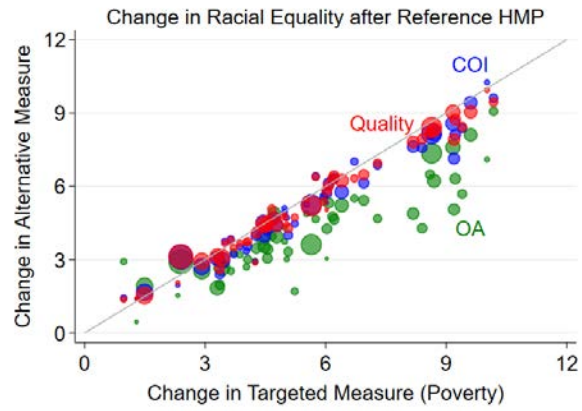


Figure 31: Simulated Effects of HMPs Targeting Neighborhood Poverty

Note: This figure shows the change in racial equality that results from the reference HMP changed to target poverty rather than quality. The x -axis measures racial equality in terms of poverty, and the y -axis shows results when measuring racial equality in terms of the COI, the OA, or neighborhood quality.

G Conceptual Uncertainty and Statistical Uncertainty

Relevant for understanding the magnitude of conceptual uncertainty in outcome estimates, Figure 32 shows that there was strong sorting by income and race in 1990.

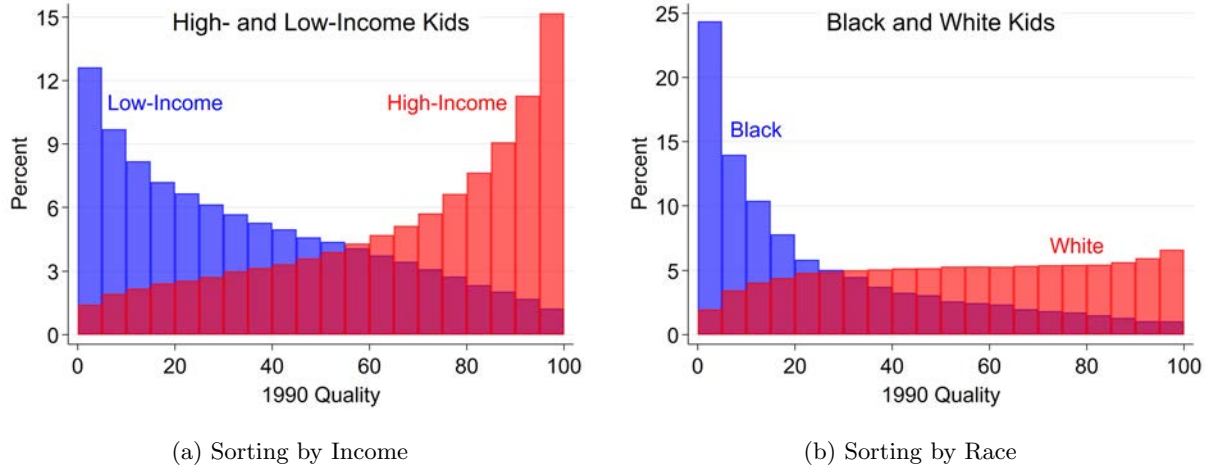


Figure 32: Sorting into Neighborhood Quality by Household Income and Race

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.

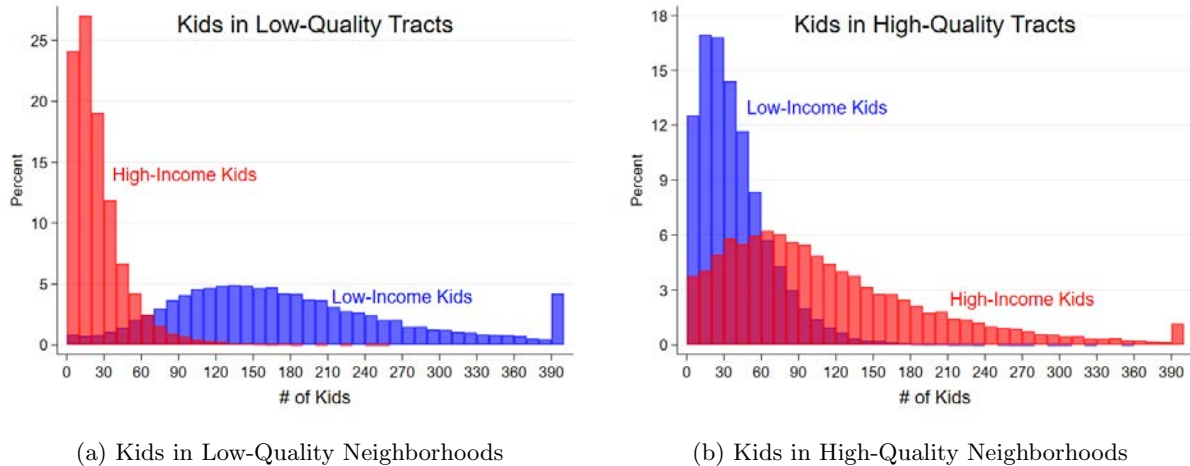


Figure 33: Sample Sizes by Income

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 1990 Census with parents in the top and bottom quartiles of household income residing in tracts in the top quartile of 1990 neighborhood quality.

Figure 33 shows that, in addition to the conceptual uncertainty documented above, neighbor-

hood 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. 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. While the statistical uncertainty created by small sample sizes is different from the identification problem posed by biased estimates, this uncertainty adds to the threat of inaccurate estimates of potential outcomes in exactly the places where accurate estimates are most valuable to policymakers.

Figure 34 displays data from Shaker Heights, Ohio, an inner ring suburb of Cleveland well-known for its efforts at racial and economic integration (, , Galster (2019), ,). The top number in the figure is the OA estimate of average individual income at age 31-37 for children who grew up in each tract with parents whose income was at the 25th percentile of income. The bottom number is the number of children aged 6-11 in the 1990 Census with parents at or below the 25th percentile of income. Highlighted in white are two tracts that are in close physical proximity to one another, have a considerable difference in outcomes, and have relatively few children on which to estimate outcomes. It is difficult to judge the statistical uncertainty in this difference in outcomes because the standard errors reported in the OA data set are in terms of percentile rankings, which generates obstacles to inference (Mogstad et al. (2021)).

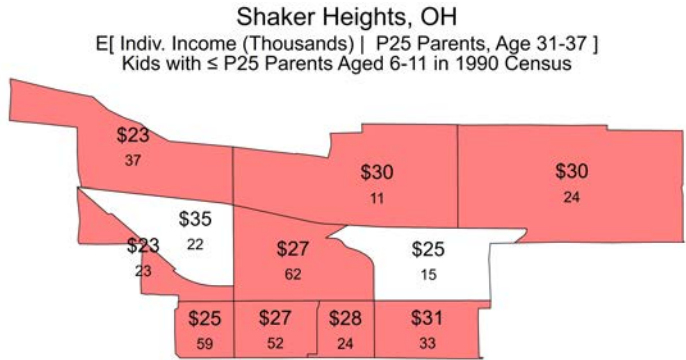


Figure 34: Shaker Heights, Ohio
 Note: This figure shows OA estimates of individual income for low-income children together with the number of low-income children in the OA sample age range in the 1990 Census.

H Changes over Time

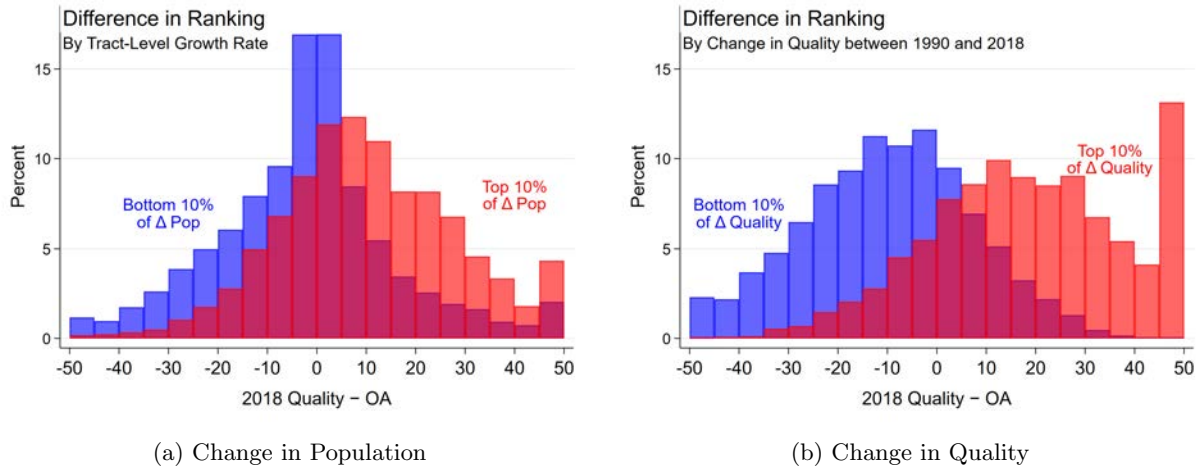


Figure 35: Predicting Disagreement in 2018 Quality and OA Rankings

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.

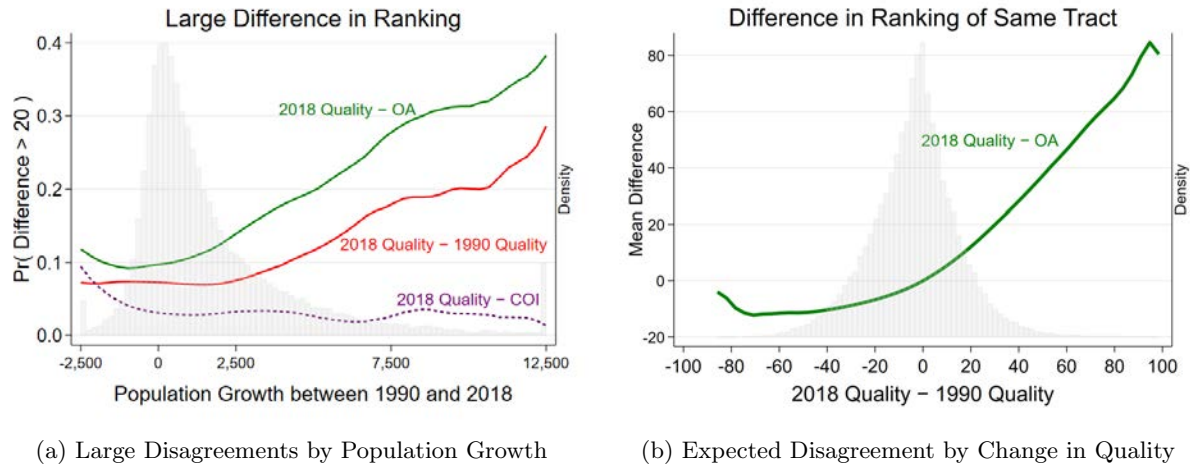


Figure 36: Predicting Large Disagreements in 2018 Quality and OA Rankings

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.

I Computational Appendix

I.1 Reference HMP

Step 1: Determine M_m , the total number of movers M in each metro m given f , the maximum number of families that can move to a new tract.

Step 1.1: Determine the total number of movers per tract in receiving tracts, or those in the top third of the metro's tracts as ranked by quality:

- a. If a tract has housing supply equal to or greater than f , then assign $f \times 4$ as the number of movers to the tract.
- b. If a tract has housing supply $h < f$, then assign $h \times 4$ as the number of movers to the tract.

Step 2: In origin neighborhoods, select individuals to move from original tracts.

Step 2.1: Identify tracts that have poverty rates greater than 30 percent or are otherwise in the bottom 1/3 of the metro's distribution of quality.

Step 2.2: Identify the number of poor persons in these tracts that are compliers and will move (25 percent of the total number of poor persons in the tract). Let hp_j represent the number of compliers in high-poverty tract j and bt_k represent the number of compliers in bottom third tract k . Then define the sums in metro m as $HP_m = \sum_{j \in \{1,2,\dots,J_m\}} hp_j$ and $BT_m = \sum_{k \in \{1,2,\dots,K_m\}} bt_k$. $HP_m + BT_m = C_m$, the total number of compliers in metro m .

- a. If $HP_m \geq M_m$, we will only move from high-poverty tracts.
- b. If $HP_m < M_m$, HP_m people are moved from high-poverty tracts and $M_m - HP_m$ people are moved from tracts that are in the bottom third of their neighborhood measure and not high-poverty tracts. This will still be M_m total movers.
- c. If $C_m < M_m$, we will move C_m people.

Step 2.3: Create a vector or vectors of the eligible moving tracts. Let php_j represent the number of compliers in high-poverty tract j and pbt_k represent the number of compliers in bottom third tract k . Then define the sums in metro m as $PHP_m = \sum_{j \in \{1,2,\dots,J_m\}} php_j$ and $PBT_m = \sum_{k \in \{1,2,\dots,K_m\}} pbt_k$. $PHP_m + PBT_m = P_m$, the total number of compliers in metro m . Let p_j represent the number of poor persons in tract j .

- a. If $HP_m \geq M_m$, the vector will have the tract number of high-poverty tract j repeated php_j times, where the vector is a stacked vector of length PHP_m .
- b and c. If $HP_m < M_m$ or $C_m < M_m$, there will be two vectors. One will have the tract number of each high-poverty tract j repeated php_j times. The other vector will have the tract number of each bottom third tract k that is NOT a high-poverty tract repeated pbt_k times. Both vectors are again stacked vectors of length PHP_m and PBT_m respectively.

Step 2.4: Randomly select tract numbers to represent the number of people who are going

to move from each tract.

- a. If $HP_m \geq M_m$, we randomly draw M_m tract numbers from our one vector.
 - b. If $HP_m < M_m$, we randomly draw HP_m tract numbers from our first set of vectors and $M_m - HP_m$ tract numbers from our second set of vectors.
 - c. If $C_m < M_m$, we randomly draw HP_m tract numbers from our first vector and BT_m tracts from our second vector.
- Note: Let the number of times tract j is chosen be denoted by n_j such that in cases (a) and (b) above $\sum_{j \in \{1,2,\dots,J_m\}} n_j = M_m$ and in case (c) above $\sum_{j \in \{1,2,\dots,J_m\}} n_j = C_m$.

Step 2.5: Randomly select n_j poor individuals from each tract j to move.

- a. For each tract j , create a matrix ip_j that is $p_j \times 4$, that will contain indicator variables representing the number of poor persons of each race in tract j . The first vector will be all ones, representing all poor persons, regardless of race, in tract j . There will then be three race vectors with ones indicating whether the poor person is of race black, white, or other. The race vectors will be created in a way so that the first column is a linear combination of the other three, so in each row there will be exactly two columns with ones in them.
- b. Randomly draw n_j persons from matrix ip_j . This allows to keep track of how many poor persons of each race are moving from tract j . A poor person is "moved out" of a tract by changing the ones to zeros in that row.

I.2 HMP Focused on Access to Public Transportation

Change Step 1: Determine M_m , the total number of movers M in each metro m given f , the maximum number of families that can move to a new tract.

Step 1.1: Determine the total number of movers per tract in receiving tracts, or those in the top third of the metro's tracts as ranked by quality:

- a. If a tract has housing supply equal to or greater than f , then assign $f \times 4$ as the number of movers to the tract.
- b. If a tract has housing supply $h < f$, then assign $h \times 4$ as the number of movers to the tract.
- c. If a tract does not have access to public transportation, then assign 0 as the number of movers to the tract.

I.3 HMP with No Porting Across PHA Jurisdictions

Change Steps 1 and 2: Instead of indexing the HMP to metro m , conduct Steps 1 and 2 at the level of each PHA p , and then sum across PHAs in metro m to obtain metro-level outcomes.

I.4 HMP Targeting the Middle Third of Tracts

Change Step 1: Determine M_m , the total number of movers M in each metro m given f , the maximum number of families that can move to a new tract.

Step 1.1: Determine the total number of movers per tract in receiving tracts, or those in the middle third of the metro's tracts as ranked by quality:

- a. If a tract has housing supply equal to or greater than f , then assign $f \times 4$ as the number of movers to the tract.
- b. If a tract has housing supply $h < f$, then assign $h \times 4$ as the number of movers to the tract.

I.5 HMP without Housing Supply Constraints

Change Step 1: Determine M_m , the total number of movers M in each metro m given f , the maximum number of families that can move to a new tract.

Step 1: Set the total number of movers per tract in receiving tracts, or those in the top third of the metro's tracts as ranked by quality, to $f \times 4$, regardless of the rental housing supply in the tract.

I.6 HMP Targeting Another Ranking of Tracts

Change Step 1: Determine M_m , the total number of movers M in each metro m given f , the maximum number of families that can move to a new tract.

Step 1.1: Determine the total number of movers per tract in receiving tracts, or those in the top third of the metro's tracts as ranked by either COI or OA:

- a. If a tract has housing supply equal to or greater than f , then assign $f \times 4$ as the number of movers to the tract.
- b. If a tract has housing supply $h < f$, then assign $h \times 4$ as the number of movers to the tract.

Change Step 2: In origin neighborhoods, select individuals to move from original tracts.

Step 2.1: Identify tracts that have poverty rates greater than 30 percent or are otherwise in the bottom 1/3 of the metro's distribution of either COI or OA.

J Uncertainty in Measuring Neighborhood Effects

J.1 Two Sources of Disagreement

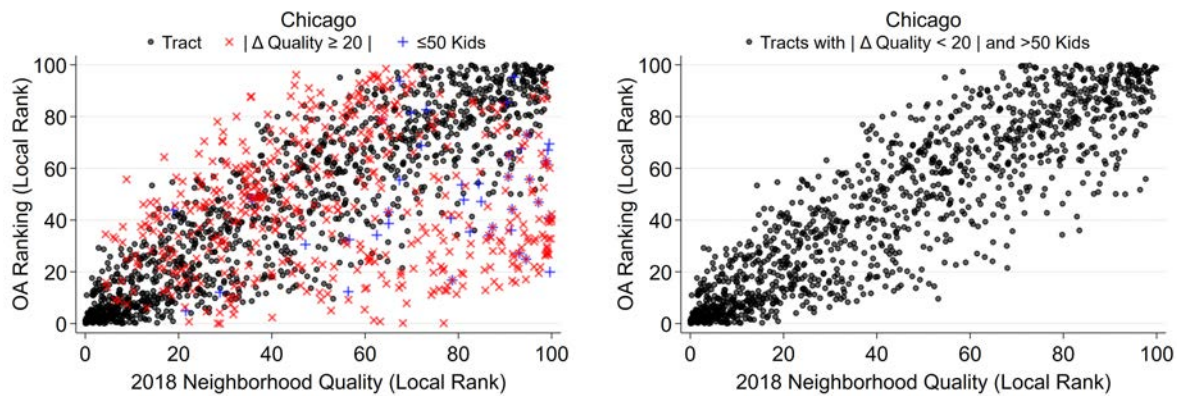


Figure 37: Chicago

Note: See note to Figure 42.

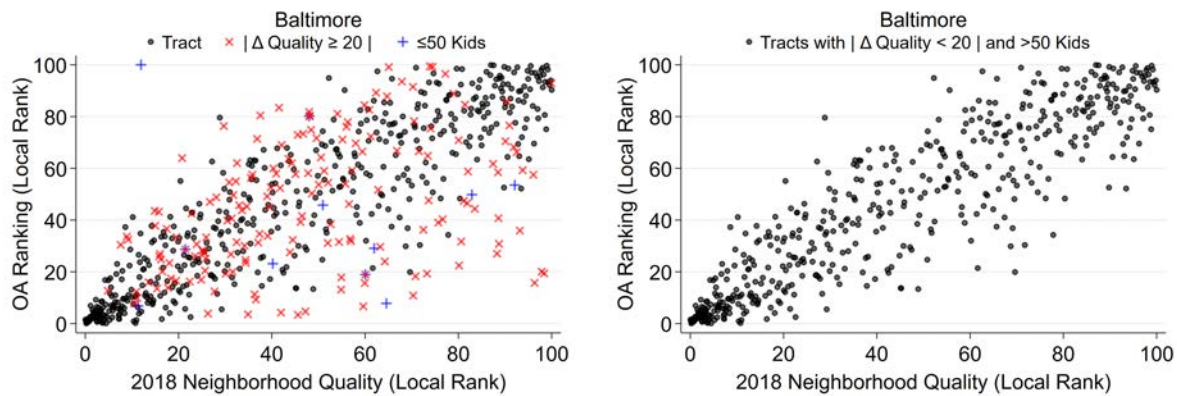


Figure 38: Baltimore

Note: See note to Figure 42.

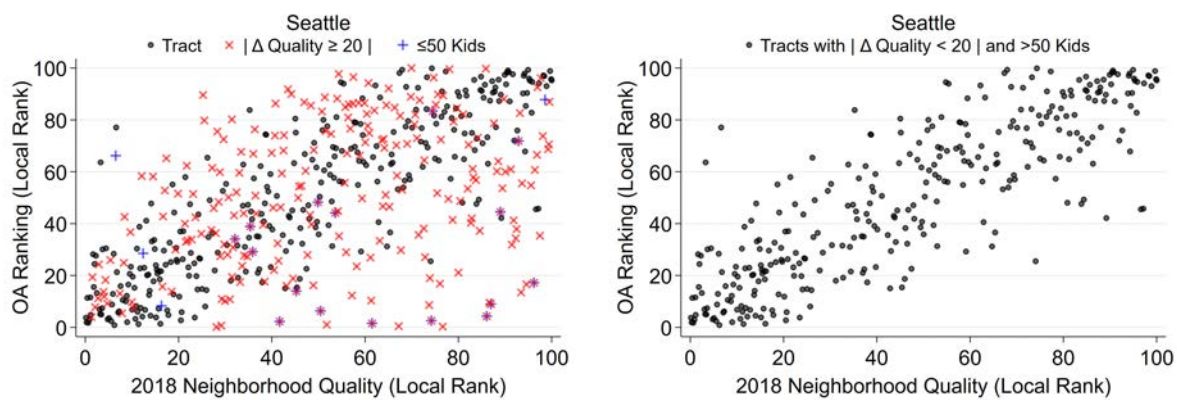


Figure 39: Seattle

Note: See note to Figure 42.

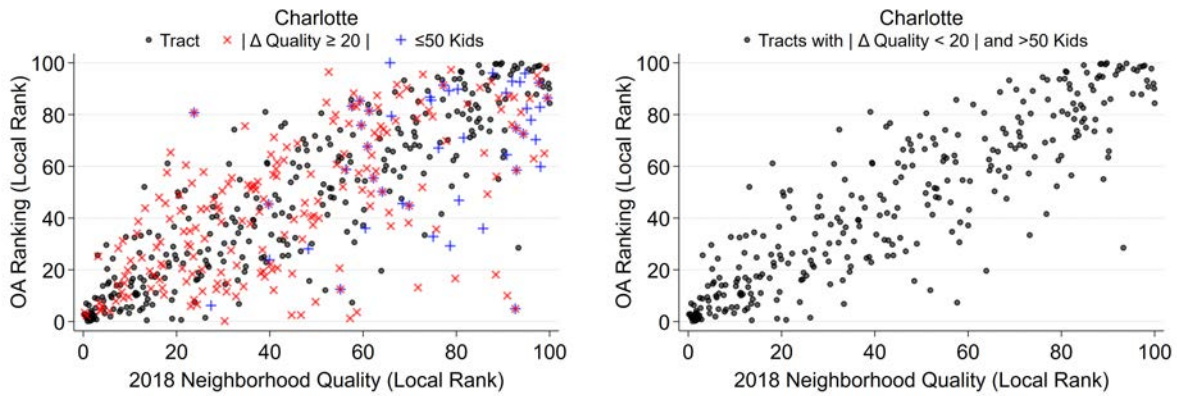


Figure 40: Charlotte
Note: See note to Figure 42.

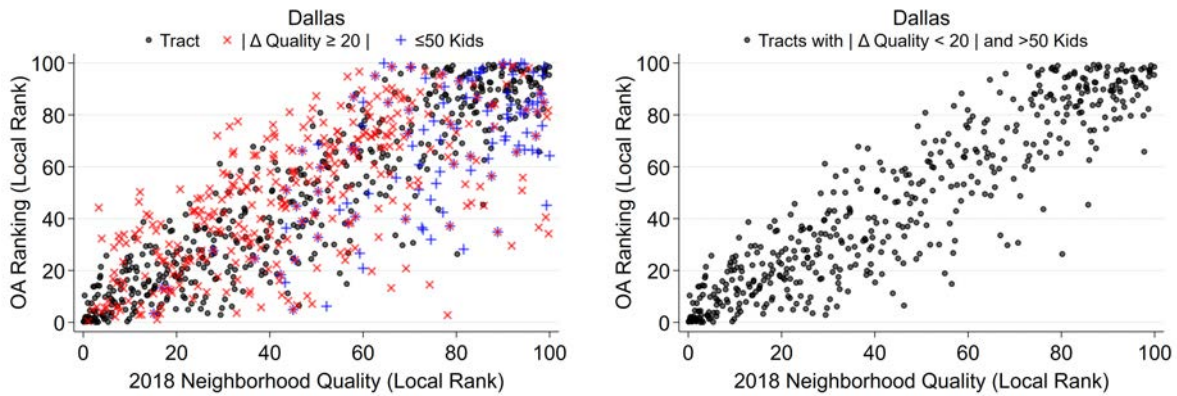


Figure 41: Dallas
Note: See note to Figure 42.

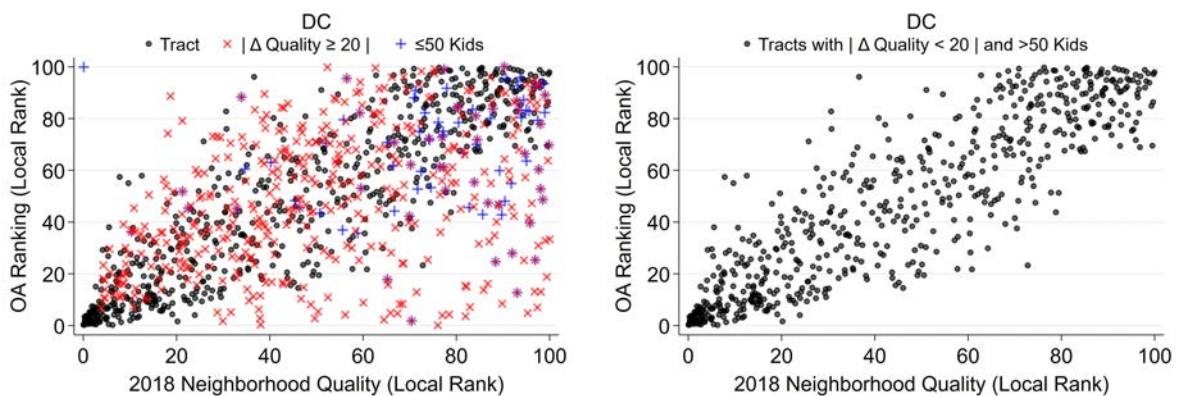


Figure 42: DC

Note: These figures show the joint distribution of the local ranks of tracts in terms of 2018 neighborhood quality and mean family income pooled over race/ethnicity and gender as estimated in the Opportunity Atlas (OA). The left panel flags tracts that either experienced a change in quality (in the local distribution) between 1990 and 2018 of at least 20 percentile points, or else that had less than 50 children in the OA sample age range in the 1990 Census. The right panel shows only those tracts that are not subject to these two sources of uncertainty in OA estimates.

K Opportunity Bargains and Cost

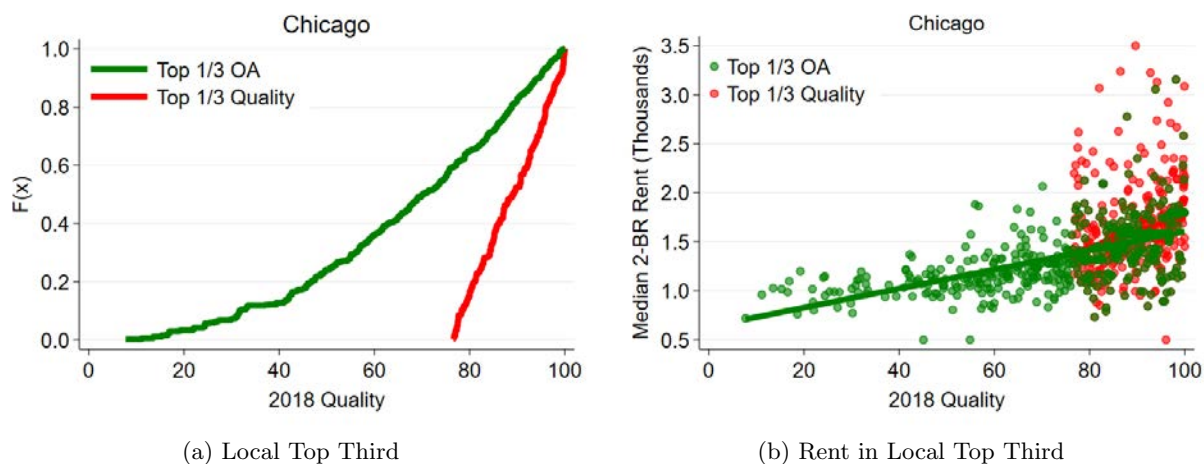


Figure 43: The Top Third of Tracts in Chicago

Note: The left panel plots the Cumulative Distribution Functions (CDFs) of the 2 bedroom and larger rental units in the top third of tracts in the metro as ranked by either the OA or 2018 neighborhood quality. The right panel plots the joint distribution of median 2 bedroom rent and 2018 neighborhood quality for tracts in the top third in the metro as ranked by either the OA or 2018 neighborhood quality, as well as lines fitted by Ordinary Least Squares (OLS).

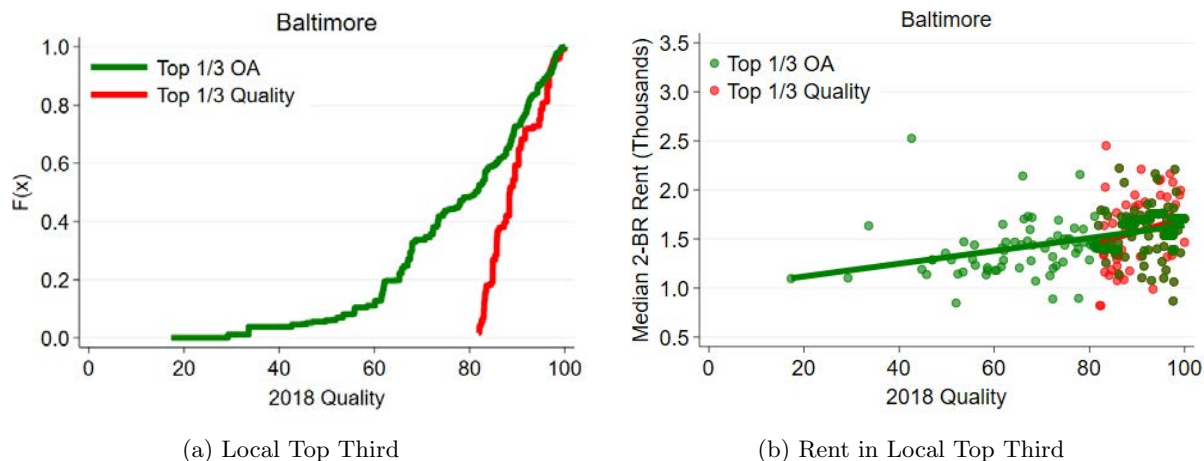


Figure 44: The Top Third of Tracts in Baltimore

Note: See note to Figure 43.

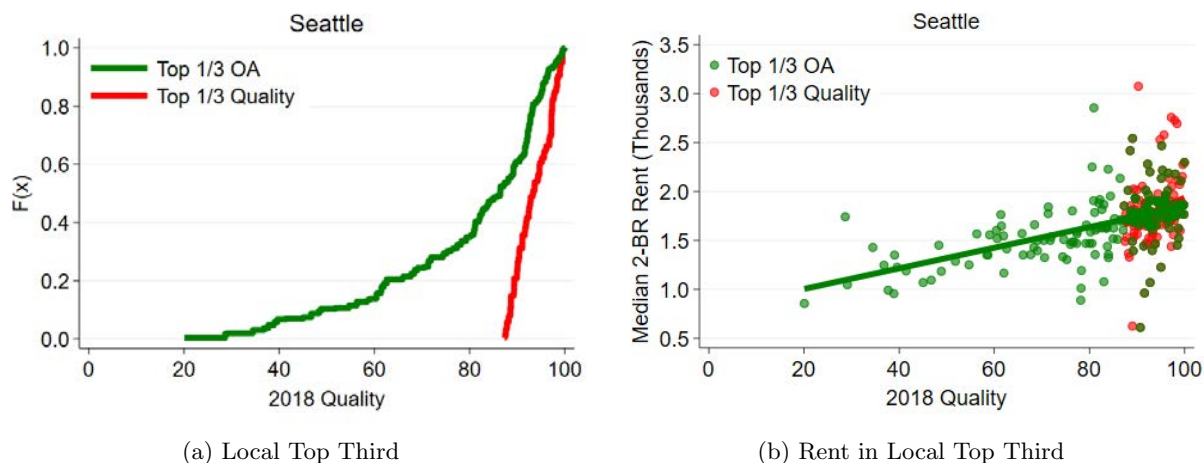
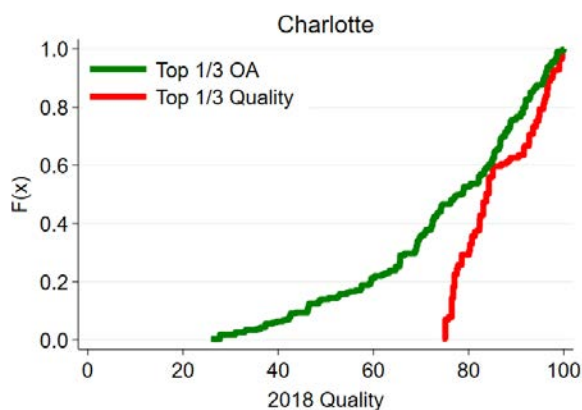
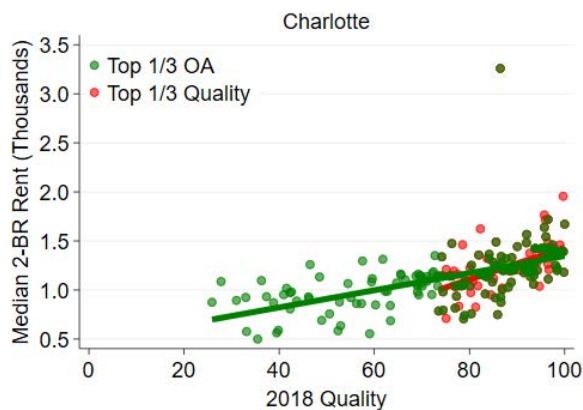


Figure 45: The Top Third of Tracts in Seattle

Note: See note to Figure 43.



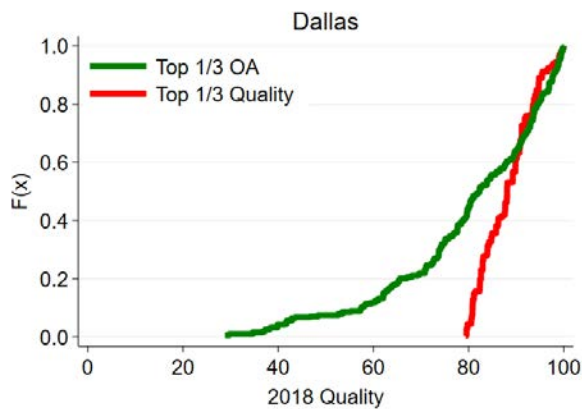
(a) Local Top Third



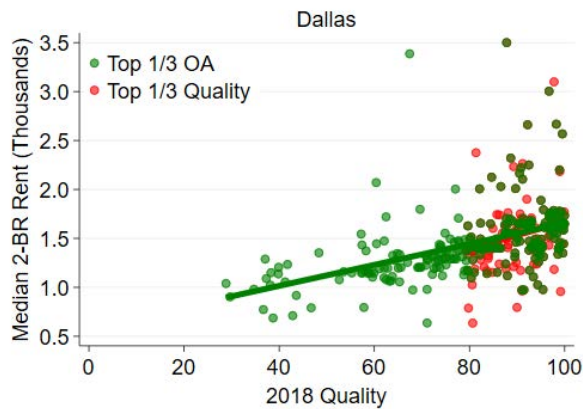
(b) Rent in Local Top Third

Figure 46: The Top Third of Tracts in Charlotte

Note: See note to Figure 43.



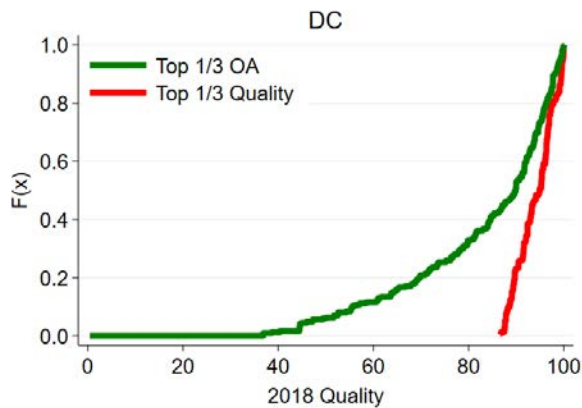
(a) Local Top Third



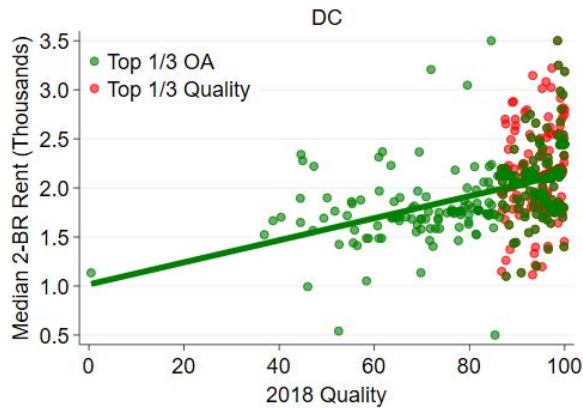
(b) Rent in Local Top Third

Figure 47: The Top Third of Tracts in Dallas

Note: See note to Figure 43.



(a) Local Top Third



(b) Rent in Local Top Third

Figure 48: The Top Third of Tracts in DC

Note: See note to Figure 43.

L Measuring PHA Jurisdictions

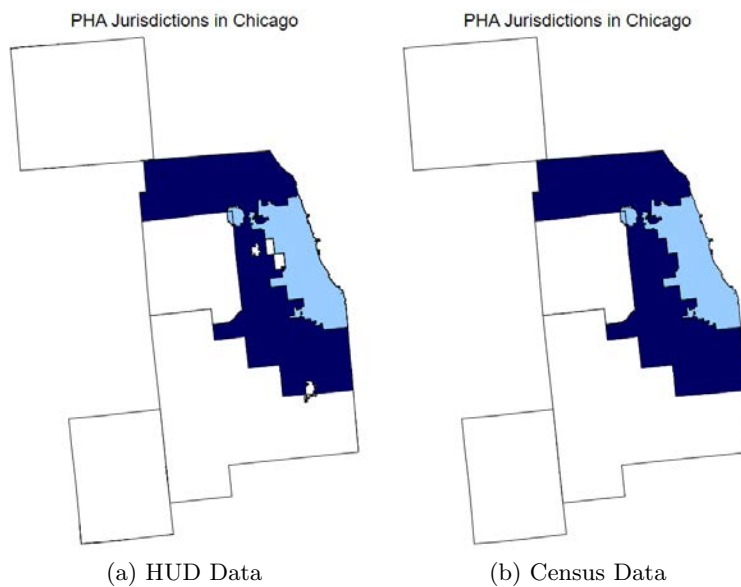


Figure 49: PHA Jurisdictions in Chicago

Note: This figure shows PHA jurisdictions in Chicago, with the central county highlighted in blue. In light blue is largest city in the central county, and in dark blue is the remainder of the central county. In the left panel, small areas of white indicate where the HUD data report the jurisdictions of small PHAs within the central county. In the right panel, the dark blue filling in the white areas from the left panel indicates how the assumptions we make about the boundaries of PHA jurisdictions maps into the Census data used to determine jurisdiction boundaries.

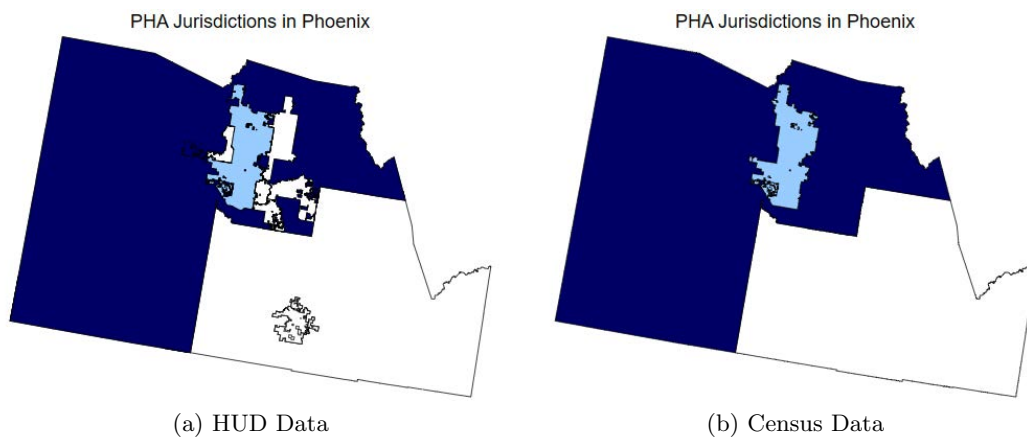


Figure 50: PHA Jurisdictions in Phoenix

Note: See note to Figure 49.

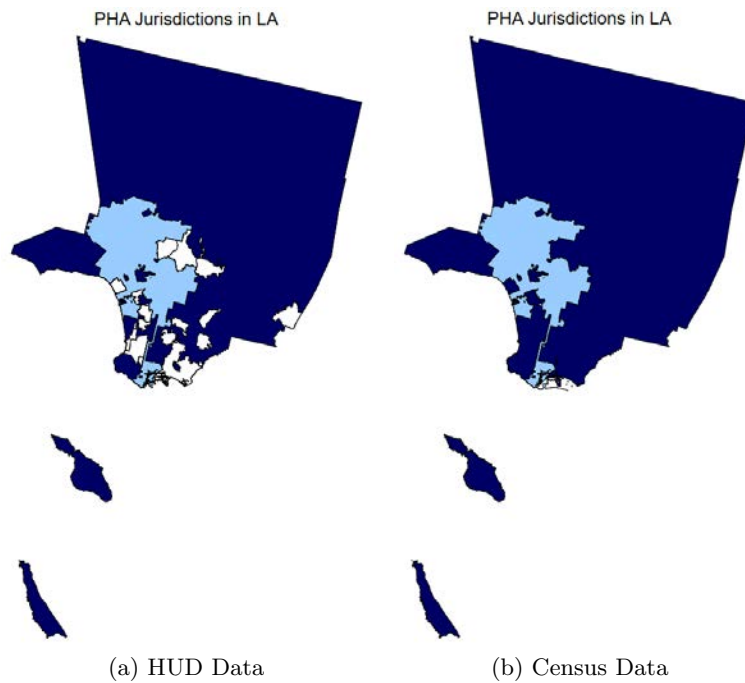


Figure 51: PHA Jurisdictions in Los Angeles
 Note: See note to Figure 49.

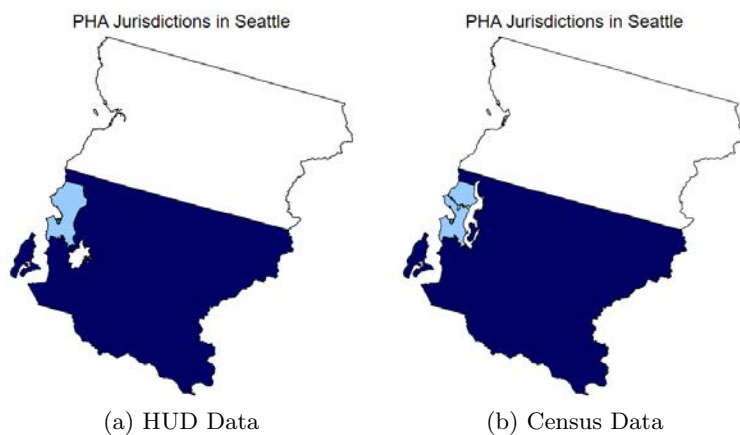


Figure 52: PHA Jurisdictions in Seattle
 Note: See note to Figure 49.

Table 7: Central City and County PHA Jurisdictions

	Central City is Central County	Separate Central City and Central County PHAs	Exclusionary Central County PHA Jurisdiction
New York	Yes	No	No
Los Angeles	No	Yes	Yes
Chicago	No	Yes	Yes
Dallas	No	No	No
Houston	No	Yes	Yes
DC	Yes	No	No
Miami	No	Yes	No
Philadelphia	Yes	No	No
Atlanta	No	Yes	Yes
Boston	No	No	No
Phoenix	No	Yes	Yes
San Francisco	No	No	No
Riverside	No	No	No
Detroit	No	No	No
Seattle	No	Yes	Yes
Minneapolis	No	Yes	Yes
San Diego	No	Yes	Yes
Tampa	No	No	No
Denver	No	No	No
Baltimore	Yes	No	No
St. Louis	Yes	No	No
Charlotte	No	No	No
Orlando	No	Yes	No
San Antonio	No	Yes	No
Portland	No	No	No
Pittsburgh	No	Yes	No
Sacramento	No	Yes	No
Las Vegas	No	No	No
Cincinnati	No	No	No
Kansas City	No	Yes	Yes
Austin	No	Yes	No
Columbus	No	No	No
Cleveland	No	No	No
Indianapolis	No	No	No
San Jose	No	Yes	No
Nashville	No	No	No
Virginia Beach	No	No	No
Providence	No	Yes*	Yes*
Milwaukee	No	Yes	No
Jacksonville	No	No	No
Oklahoma City	No	No	No
Memphis	No	No	No
Raleigh	No	Yes	Yes
Richmond	No	No	No
Louisville	No	No	No
New Orleans	Yes	No	No
Hartford	No	Yes*	Yes*
Salt Lake City	No	Yes	No
Birmingham	No	Yes	No
Buffalo	No	Yes	No
Rochester	No	No	No
Grand Rapids	No	Yes	No
Tucson	No	No	No
Tulsa	No	No	No

* There is no county PHA, all cities have separate PHA jurisdictions

Exclusionary PHA jurisdiction means the central county explicitly states it does not serve the central city.

M Access to Public Transportation

We have the latitude and longitudes of transit stops from the General Transit Feed Specification (GTFS) data, taken from the National Transit Map (NTM) of the Bureau of Transportation Statistics, or Open Mobility Data if the NTM had missing or insufficient data. Table M shows the source of transit access data by metro.

Table 8: Data Source on Transit Stops, by Metro

Metro	Data Source	Metro	Data Source
New York	National Transit Map	Philadelphia	Open Mobility Data
Los Angeles	National Transit Map	Atlanta	Open Mobility Data
Chicago	National Transit Map	Phoenix	Open Mobility Data
Dallas	National Transit Map	Tampa Bay	Open Mobility Data
Houston	National Transit Map	Charlotte	Open Mobility Data
DC	National Transit Map	Orlando	Open Mobility Data
Miami	National Transit Map	San Antonio	Open Mobility Data
Boston	National Transit Map	Sacramento	Open Mobility Data
San Francisco	National Transit Map	Las Vegas	Open Mobility Data
Riverside	National Transit Map	Cincinnati	Open Mobility Data
Detroit	National Transit Map	Indianapolis	Open Mobility Data
Seattle	National Transit Map	Louisville	Open Mobility Data
Minneapolis	National Transit Map	New Orleans	Open Mobility Data
San Diego	National Transit Map	Rochester	Open Mobility Data
Denver	National Transit Map	Grand Rapids	Open Mobility Data
Baltimore	National Transit Map	Virginia Beach	Not Found
St. Louis	National Transit Map	Memphis	Not Found
Portland	National Transit Map	Richmond	Not Found
Pittsburgh	National Transit Map		
Kansas City	National Transit Map		
Austin	National Transit Map		
Columbus	National Transit Map		
Cleveland	National Transit Map		
San Jose	National Transit Map		
Nashville	National Transit Map		
Providence	National Transit Map		
Milwaukee	National Transit Map		
Jacksonville	National Transit Map		
Oklahoma City	National Transit Map		
Raleigh	National Transit Map		
Hartford	National Transit Map		
Salt Lake City	National Transit Map		
Birmingham	National Transit Map		
Buffalo	National Transit Map		
Tucson	National Transit Map		
Tulsa	National Transit Map		

N HMPs by Program Size

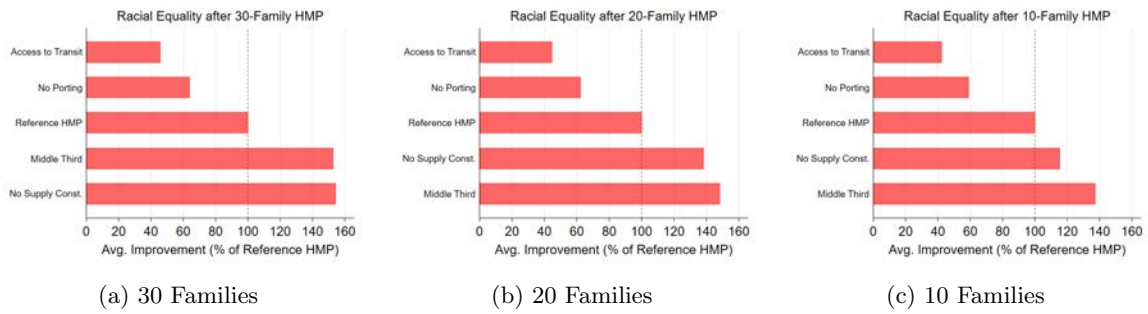


Figure 53: HMP Results by the Maximum Number of Families per Destination Tract

Note: This figure shows how changes in HMP design affects the overall improvement in racial equality due to the HMP, depending on the maximum number of families eligible to move to destination tracts.