

Neighborhood effects and housing vouchers

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Researchers and policy makers have explored the possibility of restricting the use of housing vouchers to neighborhoods that may positively affect the outcomes of children. Using the framework of a dynamic model of optimal location choice, we estimate preferences over neighborhoods of likely recipients of housing vouchers in Los Angeles. We combine simulations of the model with estimates of how locations affect adult earnings of children to understand how a voucher policy that restricts neighborhoods in which voucher-recipients may live affects both the location decisions of households and the adult earnings of children. We show the model can nearly replicate the impact of the Moving to Opportunity experiment on the adult wages of children. Simulations suggest a policy that restricts housing vouchers to the top 20% of neighborhoods maximizes expected aggregate adult earnings of children of households offered these vouchers.

KEYWORDS. Neighborhood choice, housing vouchers.

JEL CLASSIFICATION. I240, I31, I38, J13, R23, R38.

1. INTRODUCTION

We study if housing policy that was enacted to reduce housing costs of low-income households can also affect intergenerational mobility. Specifically, we consider an environment in which policy makers restrict the use of housing vouchers to a set of neighborhoods that may positively impact the earnings of children once they are adults. We

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We thank numerous discussants and seminar participants for helpful comments. We gratefully acknowledge financial support from the National Science Foundation (grant SES-1629422). The views expressed herein are those of the authors and do not necessarily represent those of the Federal Reserve Bank of Chicago or the Federal Reserve System.

investigate the extent that this change in policy affects both the willingness of low-income households to use housing vouchers and the adult earnings of children of those households.

So, why is this interesting? Chyn (2018) shows that children of families forced to relocate out of demolished public housing projects in Chicago are more likely to be employed and earn more in young adulthood than peer children of nearby public housing that was not demolished. Chetty, Hendren, and Katz (2016), hereafter CHK, evaluate the impact of the Moving To Opportunity (MTO) program on adult earnings of children. MTO was an experiment undertaken in the 1990s that randomly assigned a group of households with children eligible to live in low income housing projects in five U.S. cities to three different groups: (i) a treatment group that received a Section 8 housing voucher that in the first year could be applied only in Census tracts with a poverty rate under 10% and could be applied unconditionally thereafter, (ii) a second treatment group that received a comparable Section 8 housing voucher with no location restriction attached, and (iii) a control group that received no voucher. CHK show that children under the age of 13 from the group that received the location-restricted voucher experienced a \$3477 annual increase in adult earnings relative to the control group.

Given this evidence, it may seem reasonable to ask if public policy should steer low-income households away from neighborhoods that might be detrimental to child outcomes and towards neighborhoods that might improve child outcomes. One way to achieve this goal may be to provide counseling and assistance to both renters and landlords to reduce barriers voucher recipients face in moving to neighborhoods that improve child outcomes. This is the approach taken by Bergman, Chetty, DeLuca, Hendren, Katz, and Palmer (2019) who are currently running a large-scale experiment to this effect in Seattle. A different policy may be to simply restrict the locations in which housing vouchers may be applied. Low-income households that receive a location-restricted housing voucher would only be able to use the voucher to pay rent in a predetermined set of neighborhoods.

The Bergman et al. (2019) paper uses an experimental design to understand costs and benefits of large-scale changes to existing housing voucher programs. We use a structural approach. We estimate preferences for locations, consumption, housing, and amenities in a dynamic, forward-looking location-choice model using panel data for many different types of renting households in Los Angeles. Given these estimates, we simulate the steady-state equilibrium of our model under various counterfactual policies in which households can only use housing vouchers in Census tracts with relatively high Opportunity Atlas scores.

We believe our model-based, structural approach to analyze the costs and benefits of large changes to housing policy offers different and complementary perspectives on the impact of policy to the experimental approach advocated by Bergman et al. (2019) and others. To start, we are not limited to evaluating one or two particular policies; we evaluate a suite of policies in order to identify a policy that maximizes the adult earnings of children. Importantly, we measure general-equilibrium impacts that are not captured using current experimental methods. The relevance of general-equilibrium effects arises

because we impose a finite elasticity of housing supply in each Census tract in Los Angeles, the values of which we take from [Baum-Snow and Han \(2020\)](#), that allows both housing supply and rental prices to respond to voucher policies. In many of the counterfactual experiments we consider, the location-restricted voucher program boosts rental prices in Census tracts with high Opportunity Atlas scores. This change in rental prices induces some households that do not receive vouchers to move from high Opportunity Atlas neighborhoods to neighborhoods that may provide similar utility, but have lower rental prices and lower Opportunity Atlas scores. This relocation of households that do not receive vouchers reduces the net benefits of restricted-location voucher program. Thus, our analysis highlights the importance of understanding general-equilibrium effects in studying the distributional effects of policy as well its costs and benefits.

In the paper that is closest to ours, [Galiani, Murphy, and Pantano \(2015\)](#) use data on the location choices of the Boston participants in the Moving to Opportunity experiment to help identify the structural parameters of a location-choice model. Their approach exploits the randomization of MTO participants along with Census data on tract demographics to estimate the preference weights that households place on consumption, housing, amenities, and various neighborhood characteristics. The randomization of households into the different MTO treatment and control groups allows the preference weight on consumption and housing to be identified without an instrument for rent. The model successfully matches a number of moments summarizing the location choices of MTO participants offered location-restricted vouchers, providing out-of-sample model validation. The spirit of our paper and [Galiani, Murphy, and Pantano \(2015\)](#) are similar; however, there are a few key differences. Specifically, we focus on the location decisions of households and the implications for adult earnings of children. Additionally, we model and estimate the choices of all renters in Los Angeles (both voucher recipients and nonrecipients), enabling us to study metro-wide implications of large-scale hypothetical changes to housing-voucher policies in a general-equilibrium framework.

The rest of this [Introduction](#) summarizes our methods, details, and results.

We start by estimating the parameters of a discrete-choice, dynamic model of location choice for renters in Los Angeles. The model is in the spirit of [Kennan and Walker \(2011\)](#) and [Bayer, McMillan, Murphy, and Timmins \(2016\)](#). We use panel data on renting households from the Federal Reserve Bank of New York Consumer Credit Panel/Equifax (CCP) to estimate optimized indirect utility for each neighborhood (Census tract) in Los Angeles and the cost of moving. These data are a 5% random sample of U.S. adults conditional on having an active credit file and any individuals residing in the same household. Our estimation sample includes more than 1.75 million person-year observations of renter households living in Los Angeles. We divide the sample into 144 types of households based on observable characteristics in the first period in which we observe the household.

Next, we specify that conditional on a choice of neighborhood, each household has Cobb–Douglas preferences for consumption and housing in that neighborhood. With Cobb–Douglas preferences, the ratio of expenditures on housing to expenditures on consumption is fixed, implying that households rent smaller units in neighborhoods

where the rental price-per-unit of housing is high. The fact that households vary their housing quantity in response to changes to rental prices suggests our structural framework can be used to evaluate possible supply-side interventions if desired, for example, policies that increase the quantity of housing in high Opportunity Atlas neighborhoods.

Our specification requires estimation of one additional parameter that scales the deterministic portion of utility relative to the variance of utility shocks that are embedded in the dynamic location-choice model. This scale parameter determines how households respond to shocks that affect utility after controlling for consumption, housing, and fixed location-specific amenities. To estimate this parameter, we use the instrumental variables approach of Bayer, Ferreira, and McMillan (2007).

Finally, we determine the types of households that are eligible to receive a housing voucher and have at least one child. We use tract-level data from the 2000 Census to estimate average income and average number of children per household for each type. We identify 24 types of voucher-eligible households in our sample with children that accept a housing voucher if offered. These households are 1/4 African-American and 3/4 Hispanic, have on average 2.1 children per household, an annual income of \$18.7 thousand and spend 36% of their income on rents.

In the final sections of the paper, we combine the predictions of the estimated model with data from the Opportunity Atlas to study how various housing-voucher policies affect optimal location choices of households and the earnings of children when they become adults. The Opportunity Atlas is a data set created by Chetty, Friedman, Hendren, Jones, and Porter (2018) that, for each Census tract in the United States, predicts the percentile of a child's adult earnings in the age-26 income distribution given the percentile of the household's income in the income distribution.¹ We begin the analysis by asking if our model can replicate the estimate of CHK that the MTO voucher program increased annual adult earnings of children under the age of 13 at the time the voucher was received by \$3477 with a standard error of \$1418. We show that the model can nearly replicate this result; our model-based estimate of their statistic is \$2923.

Interestingly, holding the poverty rate constant of the chosen neighborhood, our simulations show that if MTO voucher recipients had selected neighborhoods *randomly* then expected average adult earnings of children of voucher recipients would have increased by \$5167, an increase of more than 75%. In other words, we find that MTO voucher recipients selected into neighborhoods that yield relatively low adult earnings for children. This occurs for two reasons. For neighborhoods with a poverty rate less than 10%, households accepting an MTO voucher prefer the amenities of low Opportunity Atlas score neighborhoods to high Opportunity Atlas score neighborhoods. Additionally, rental prices tend to increase with Opportunity Atlas scores across neighborhoods.

In the final part of the paper, we simulate our model under a plethora of policy scenarios to understand the extent to which a city-wide voucher program that restricts the neighborhoods in which housing vouchers can be used can increase the adult earnings

¹We will sometimes refer to the expected percentile of the child's adult earnings in the age-26 income distribution as the Opportunity Atlas "score" of the neighborhood.

of children of voucher-eligible households. In all simulations, we allow for rental prices to adjust in equilibrium in response to changes in tract-level housing demand. We consider two sets of simulations. At first, we analyze results assuming the Opportunity Atlas score of all neighborhoods is fixed at its estimated value. After that, we allow the Opportunity Atlas score of a neighborhood to adjust based on changes in the racial composition and average income of that neighborhood.

We search for a cutoff Opportunity Atlas score, such that neighborhoods with higher Opportunity Atlas scores are included in the set of acceptable locations of restricted-voucher holders, that (a) maximizes the aggregate adult earnings of all children and (b) maximizes aggregate adult earnings of the children in voucher-eligible households. In the analysis, we highlight essential trade-offs of a location-restricted voucher program: Some households decline the voucher because the set of acceptable neighborhoods is too restrictive but households that accept the voucher experience significant gains in the adult earnings of their children. We find that a voucher program that limits locations to the top 10% of Opportunity Atlas neighborhoods maximizes the aggregate annual earnings of all children of renting households in Los Angeles; and a policy that limits location to the top 20% of Opportunity Atlas neighborhoods maximizes the aggregate earnings of children of renting households eligible to receive vouchers. In either case, many children of households accepting vouchers experience enormous gains to income and children of other households experience, on average, small losses. On net, the gains outweigh the losses. We conclude policymakers can implement a location-restricted voucher program that yields aggregate gains to adult earnings of children and significantly impacts intergenerational mobility for low-income households eligible to receive housing vouchers.

2. LOCATION CHOICE MODEL AND ESTIMATES

2.1 *Model*

The first step in our analysis is to understand how household utility changes with location. To do this, we estimate the parameters of an optimal forward-looking location-choice model. The basic intuition of estimation is as follows: If we notice households moving to certain clusters of neighborhoods more frequently than others, then on average, those neighborhoods must provide higher levels of utility. In other words, viewed from the lens of the model, probabilities over location choices are directly informative of net utility of locations.

We consider the decision problem of a household head deciding where his or her family should live using a dynamic discrete choice setting. Our basic framework is somewhat standard and similar models have been studied by [Kennan and Walker \(2011\)](#), [Bishop and Murphy \(2011\)](#), and [Bayer et al. \(2016\)](#). For purposes of exposition, we write down the model describing the optimal decision problem of a single family which enables us to keep notation relatively clean. When we estimate the parameters of this model, we will allow for the existence of many different “types” of people in the data. Each type of person will face the same decision problem, but the vector of parameters

that determines payoffs and choice probabilities will be allowed to vary across types of people.

The household can choose to live in one of J locations. Denote j as the household's current location. We write the value to the household of moving to location ℓ given a current location of j and current value of a shock ϵ_ℓ (to be explained later) as

$$V(\ell | j, \epsilon_\ell) = u(\ell | j, \epsilon_\ell) + \beta EV(\ell).$$

In the above equation, $EV(\ell)$ is the expected future value of having chosen to live in ℓ today and β is the factor by which future utility is discounted. Note that the expected future value of choosing to live in ℓ today does not depend on the value of ϵ_ℓ , as in Rust (1987). We assume households solve the same problem each period, explaining the lack of time subscripts.

u is the flow utility the agent receives today from choosing to live in ℓ given a current location of j and a value for ϵ_ℓ . We assume u is the simple function

$$u(\ell | j, \epsilon_\ell) = \delta_\ell - \kappa_{\ell j} + \epsilon_\ell,$$

where δ_ℓ is the flow utility the household receives this period from living in neighborhood ℓ , net of rents and other costs. In Section 2.4, we parse δ_ℓ into utility from consumption, housing and fixed neighborhood amenities, but for now just know that δ_ℓ has the interpretation of maximized indirect utility. $\kappa_{\ell j} = [\kappa_0 + \kappa_1 * \mathcal{D}_{\ell j}] \cdot 1_{\ell \neq j}$ are all costs (utility and financial) a household pays when it moves to neighborhood ℓ from neighborhood j , which we specify as the sum of a fixed cost κ_0 and a cost that increases at rate κ_1 with distance in miles between the centroid of tracts ℓ and j denoted $\mathcal{D}_{\ell j}$; $1_{\ell \neq j}$ is an indicator function that is equal to 1 if location $\ell \neq j$ and 0 otherwise, that is, the household pays zero moving costs if it does not move; and ϵ_ℓ is a random shock that is known at the time of the location choice. ϵ_ℓ is assumed to be i.i.d. across locations, time, and people. The parameters δ_ℓ , κ_0 , and κ_1 may vary across households, but for any given household these parameters are assumed fixed over time. ϵ_ℓ induces otherwise identical households living at the same location to optimally choose different future locations. Dynamics in the model are driven by moving costs and the ϵ_ℓ shocks. The model would be static if either the idiosyncratic shocks were time-invariant or moving costs were zero.

Denote ϵ_1 as the shock associated with location 1, ϵ_2 as the shock with location 2, and so on. After the vector of ϵ are revealed (one for each location), in each period households choose the location that yields the maximal value

$$V(j | \epsilon_1, \epsilon_2, \dots, \epsilon_J) = \max_{\ell \in 1, \dots, J} V(\ell | j, \epsilon_\ell), \quad (1)$$

$EV(j)$ is the expected value of (1), where the expectation is taken with respect to the vector of ϵ . We assume each period is one year.

When the ϵ are assumed to be drawn i.i.d. from the Type 1 Extreme Value Distribution, the expected value function $EV(j)$ has the functional form

$$EV(j) = \log \left\{ \sum_{\ell=1}^J \exp \tilde{V}(\ell | j) \right\} + \zeta, \quad (2)$$

where ζ is equal to Euler's constant,

$$\tilde{V}(\ell | j) = \delta_\ell - \kappa_{\ell j} + \beta EV(\ell) \quad (3)$$

and the tilde symbol signifies that the shock ϵ_ℓ has been omitted. Additionally, it can be shown that the log of the probability that location ℓ is chosen given a current location of j , call it $p(\ell | j)$, has the solution

$$p(\ell | j) = \tilde{V}(\ell | j) - \log \left\{ \sum_{\ell'=1}^J \exp[\tilde{V}(\ell' | j)] \right\}. \quad (4)$$

Subtract and add $\tilde{V}(k | j)$ to the right-hand side of the above to derive

$$p(\ell | j) = \tilde{V}(\ell | j) - \tilde{V}(k | j) - \log \left\{ \sum_{\ell'=1}^J \exp[\tilde{V}(\ell' | j) - \tilde{V}(k | j)] \right\}. \quad (5)$$

One approach to estimating model parameters such as [Rust \(1987\)](#) is to solve for the value functions at a given set of parameters, apply equation (5) directly to generate a likelihood over the observed choice probabilities, and then search for the set of parameters that maximizes the likelihood. This approach is computationally intensive because it requires solving for the value functions at each step of the likelihood, which involves backwards recursions using equations (2) and (3). In cases such as ours, involving many parameters to be estimated, this approach is computationally infeasible.

Instead, we use the approach of [Hotz and Miller \(1993\)](#) and employed by [Bishop \(2012\)](#) in similar work. This approach does not require that we solve for the value functions. Note that equation (3) implies

$$\tilde{V}(\ell | j) - \tilde{V}(k | j) = \delta_\ell - \delta_k - [\kappa_{\ell j} - \kappa_{kj}] + \beta[EV(\ell) - EV(k)]. \quad (6)$$

But from equation (2),

$$EV(\ell) - EV(k) = \log \left\{ \sum_{\ell'=1}^J \exp \tilde{V}(\ell' | \ell) \right\} - \log \left\{ \sum_{\ell'=1}^J \exp \tilde{V}(\ell' | k) \right\}.$$

Now note that equation (4) implies

$$p(k | \ell) = \tilde{V}(k | \ell) - \log \left\{ \sum_{\ell'=1}^J \exp[\tilde{V}(\ell' | \ell)] \right\},$$

$$p(k | k) = \tilde{V}(k | k) - \log \left\{ \sum_{\ell'=1}^K \exp[\tilde{V}(\ell' | k)] \right\}$$

and thus

$$\log \left\{ \sum_{\ell'=1}^J \exp[\tilde{V}(\ell' | \ell)] \right\} - \log \left\{ \sum_{\ell'=1}^K \exp[\tilde{V}(\ell' | k)] \right\}$$

is equal to

$$\begin{aligned} & \tilde{V}(k | \ell) - \tilde{V}(k | k) - [p(k | \ell) - p(k | k)] \\ & = -\kappa_{k\ell} - [p(k | \ell) - p(k | k)]. \end{aligned}$$

The last line is quickly derived from equation (3). Therefore,

$$EV(\ell) - EV(k) = -[p(k | \ell) - p(k | k) + \kappa_{k\ell}]$$

and equation (6) has the expression

$$\begin{aligned} & \tilde{V}(\ell | j) - \tilde{V}(k | j) \\ & = \delta_\ell - \delta_k - [\kappa_{\ell j} - \kappa_{kj}] - \beta[p(k | \ell) - p(k | k) + \kappa_{k\ell}]. \end{aligned} \quad (7)$$

Combined, equations (5) and (7) show that the log probabilities that choices are observed are simple functions of model parameters $\delta_1, \dots, \delta_J$, κ_0 , κ_1 , and β and of observed choice probabilities. In other words, a likelihood over choice probabilities observed in data can be generated without solving for value functions. Our estimation approach also relies on the fact that the expected value of choosing any neighborhood in the next period does not change over time. In other words, decisions today do not affect future expected values (net of moving costs). This allows us to estimate the model with a short panel, an insight from [Arcidiacono and Miller \(2011\)](#).

2.2 Data and likelihood

We estimate the model using panel data from the Federal Reserve Bank of New York Consumer Credit Panel/Equifax (CCP). The panel is comprised of a 5% random sample of U.S. adults with a social security number, conditional on having an active credit file, and any individuals residing in the same household as an individual from that initial 5% sample.² For years 1999 to the present, the database provides a quarterly record of variables related to debt: Mortgage and consumer loan balances, payments and delinquencies and some other variables we discuss later. The data does not contain information on race, education, or number of children and it does not contain information on income or assets although it does include the Equifax Risk ScoreTM, which provides some information on the financial wherewithal of the household as demonstrated in [Board of Governors of the Federal Reserve System \(2007\)](#). Most important for our application, the panel data includes in each period the current Census block of residence. To match the annual frequency of our location choice model, we use location data from the first quarter of each calendar year. Other authors have used the CCP data to study the relationship of interest rates, house prices, and credit (see [Bhutta and Keys \(2016\)](#) and [Brown, Stein, and Zafar \(2015\)](#)) and the impact of natural disasters on household finances ([Gallagher](#)

²The data include all individuals with 5 out of the 100 possible terminal 2-digit social security number (SSN) combinations. While the leading SSN digits are based on the birth year/location, the terminal SSN digits are essentially randomly assigned. A SSN is required to be included in the data and we do not capture the experiences of illegal immigrants. Note that a SSN is also required to receive a housing voucher.

and Hartley (2017)), but we are the first to use this data to estimate an optimal location-choice model.³

We restrict our sample to individuals who (a) lived in Los Angeles County in the first quarter of any year from 1999 through 2013, (b) were observed in Los Angeles in the first quarter of the following year, and (c) never had a home mortgage, yielding 1,787,558 person-year observations. An advantage of the size of our data is that we can estimate a full set of model parameters for many “types” of people, where we define a type of person based on observable demographic and economic characteristics. We study renters to mitigate any problems of changing credit conditions and availability of mortgages during the sample window.⁴ We exclude from our estimation Census tracts with fewer than 150 rental units and tracts that are sparsely populated in the northern part of the county.⁵ The panel is not balanced, as some individuals’ credit records first become active after 1999. Appendix A in the Online Supplementary Material (Davis, Gregory, Hartley, and Tan (2021)) compares population coverage of the CCP to data from the 2000 Census for Los Angeles county; summarizing the results, coverage appears quite good.

We stratify households into types using an 8-step stratification procedure. We begin with the full sample, and subdivide the sample into smaller “cells” based on (in this order): The racial plurality, as measured by the 2000 Census, of the 2000 Census block of residence (4 bins),⁶ 5 age categories (cutoffs at 30, 45, 55, and 65),⁷ number of adults age 18 and older in the household (1, 2, 3, 4+), and then the presence of an auto loan, credit card, student loan, and consumer finance loan. We do not subdivide cells in cases where doing so would result in at least one new smaller cell with fewer than 20,000 observations. In a final step applied to all bins, we split each bin into three equally-populated types based on within-bin credit-score terciles. Our strategy for specifying types is to balance fitting location choice as flexible as possible with estimating model parameters precisely. Our target is a minimum of 6667 observations per type, giving us at least 35 observations per parameter, which we believe is large enough to get reasonably precise estimates (on average, we have 12,414 observations per type). In specifying types we prioritize race by putting that as the first variable in the subdivisions. We then further subdivide based on age, household size, and debt variables—the latter are characteristics of households captured by this panel. We prioritize credit score by doing a final split

³There are two other panel data sets of which we are aware with tract-level information on renting households in Los Angeles: Data from [Infutor.com](https://infutor.com) and confidential data from the 2000 and 2010 Censuses. The [Infutor.com](https://infutor.com) data has a larger sample but includes less information on each household. The confidential Census data are accessible only in a research data center, which limits its use for this paper.

⁴In the CCP data, renters and homeowners without a mortgage are observationally equivalent. According to data from the 2000 Census, 85% of the units without a home mortgage are renter occupied for the 1748 Census tracts of our study. Since we drop households that ever had a mortgage and follow most households over multiple years, this implies the upper bound for the percentage of homeowners in our sample is 15%.

⁵On average, each Census tract in Los Angeles has about 4000 people.

⁶We assign race based on the racial plurality of all persons in the Census block, owners and renters, where they are first observed. The mean number of households and residents at the Census-block level in our sample of 1748 tracts is 41 and 118, respectively.

⁷This refers to the age of the person in the household in the initial random sample.

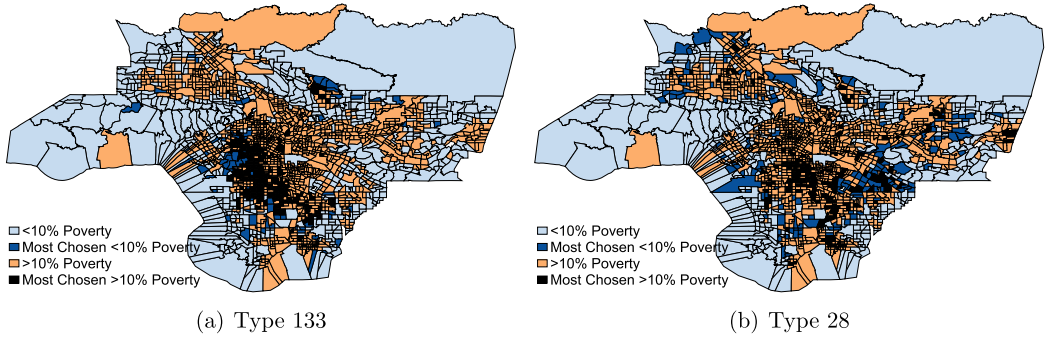


FIGURE 1. Location choices by type for tracts below and above 10% poverty rate. *Notes:* These graphs show the most frequent location choices for type 133 (left panel), an African-American household with annual income of \$12 thousand per year and a risk score less than 580 and type 28 (right panel), a Hispanic household with annual income of \$12 thousand per year and a Equifax Risk ScoreTM less than 600. The lightest-shaded areas of the map indicate tracts with a poverty rate below 10% and the second-lightest-shaded areas are tracts with a poverty rate above 10%. The darker-shaded and black areas show the most frequently chosen tracts for each type. The darker-shaded are tracts with less than 10% poverty and black are tracts with greater than 10% poverty.

of the data based on credit-score terciles within each cell. After all the dust settles, this procedure yields 144 types of households.⁸

The following figures from our data are instructive. The left panel of Figure 1 shows the typical location choices made by type 133 in our sample: A household earning \$12,000 per year with an Equifax Risk ScoreTM below 580 and first observed living in a Census block that is predominantly African-American.⁹ The lightest-shaded areas show all Census tracts with poverty rates less than 10% and the second-lightest-shaded areas show all Census tracts with higher poverty rates. The darker-shaded areas show the most chosen low-poverty Census tracts for this type and the areas in black show the most chosen high-poverty tracts. The panel shows this type predominantly clusters its location choices in one crescent-shaped area in the south-central part of the county. The right panel of this figure shows the same set of location choices for type 28 in our sample, a household earning \$12,000 per year with an Equifax Risk ScoreTM below 600 and first observed in a predominantly Hispanic Census block. Comparing the two panels, not many neighborhood choices overlap between the two types. If, counterfactually, we assumed that the vector of δ_j of the two types were the same, the model would attribute the systematic variation in optimal neighborhood choices entirely to differences in the i.i.d. utility shocks.

Households in our sample can choose to locate in one of 1748 Census tracts in Los Angeles. Allowing a separate value of δ for each tract and for each type would require

⁸We note that some of our type stratification relies on decisions households made in periods prior to the start date of our estimation sample, for example, location, and in that sense some of our sorting into types can be thought of as endogenous to the model.

⁹We discuss later how we generate the estimate of household income.

estimating more than 250,000 parameters. Conceptually, with a large enough sample we could separately estimate every δ for each type. For each type of household in our sample, we use data on approximately 1241 households followed over 10 years. For parsimony, and to exploit the fact that geographically nearby tracts likely provide similar utility, for each type we specify that the utility of location j , δ_j , is a function of latitude (lat_j) and longitude (lon_j) of that location according to the formula

$$\delta_j = \sum_{k=1}^K a_k B_k(lat_j, lon_j).$$

The B_k are parameterless basis functions. For each type, we use $K = 89$ basis functions. Additionally, we allow the values of a_k to vary for tracts above and below the 10% poverty threshold. Inclusive of the two moving cost parameters that we separately identify for each type, we estimate $2 \times 89 + 2 = 180$ parameters per type. With 144 types, we estimate a total of 25,920 parameters.¹⁰

To define the log likelihood that we maximize, we need to introduce more notation. Let i denote a given household, t a given year in the sample, j_{it} as person i 's starting location in year t and ℓ_{it} as person i 's observed choice of location in year t . Denote τ as type and the vector of parameters to be estimated for each type as θ_τ . The log likelihood of the sample is

$$\sum_{\tau} \sum_{i \in \tau} \sum_t p(\ell_{it} | j_{it}; \theta_\tau), \quad (8)$$

$p(\cdot)$ is the model predicted log-probability of choosing ℓ_{it} given j_{it} . For each τ , we use the quasi-Newton BFGS procedure to find the vector θ_τ that maximizes the sample log likelihood.

Before moving on, note that the model assumes that all households have the ability to live in any neighborhood. Of course, some landlords may be racist or discriminate against households with low income¹¹ but households in our model do not need to be able to rent from every landlord in every location; they only need to be able to rent one unit of their desired size and quality in each Census tract. In the event racism or discrimination is systemic in certain tracts, the probability that certain types of households will live in those tracts will be low and this will affect estimates of δ for those types in those tracts.¹² If discrimination in certain tracks is significant, we conjecture our framework will still be useful in predicting location choice for those tracts—and the policy experiments we discuss later will continue to be informative—as long as the degree to which

¹⁰Note that even though two adjacent tracts are likely to have similar values of δ due to smoothing, the shocks for each tract do not have to be similar as they are independent draws from the Type I extreme value distribution. We use a large number of basis functions, and in particular allow the interaction of these functions with a dummy variable for tracts below and above 10% poverty rate, to be able to fit steep changes to δ_j over a very narrow geography.

¹¹For evidence on discrimination in rental markets, see studies by Yinger (1986) and Ewens, Tomlin, and Wang (2014). Popkin, Cunningham, Godfrey, Bednarz, and Lewis (2002) and Phillips (2017) also demonstrated that landlords discriminate against rental applicants that wish to use housing vouchers.

¹²This may also affect estimates of κ , depending on the relative location of the tracts with these racial issues.

landlords are discriminatory does not systematically change as a result of any policies we consider.

2.3 Estimates and model fit

Our procedure ultimately yields estimates of δ_j , κ_0 , and κ_1 for each type to match model-predicted moving probabilities to those in the data.¹³ Due to our large number of types and tracts, it is impossible to report all parameter estimates. Instead, we summarize the estimates by examining the model's in-sample fit along a number of dimensions. By design, our model can nearly exactly match the average moving rate in the data for each type; a regression of the model-predicted average moving rate on the moving rate in the data for our 144 types has an R2 value of 0.9996. Figure 2 compares the distribution of distances moved (measured as the straight line distance between tract centroids) for all movers in the data and as predicted by our model.¹⁴ This figure shows that the model replicates the hump-shaped distribution of distances moved, with the most frequent moves around 4 miles. By having type-specific values of κ_0 and κ_1 , for each type we match the average probability of a move and the average distance conditional on a move occurring. That said, the model slightly overpredicts moves between 4 and 10 miles in length and slightly underpredicts moves less than 4 miles.¹⁵

Figure 3 shows a detailed comparison of model-predicted and actual annual migration rates for households that choose to move by poverty rate of Census tracts. The tracts from which people are moving are split into six groupings based on the poverty rate of the originating tract: 0–5, 5–10, 10–15, 15–20, 20–25, and >25. For each of these groupings, the probability of choosing a destination tract of a given poverty rate is plotted

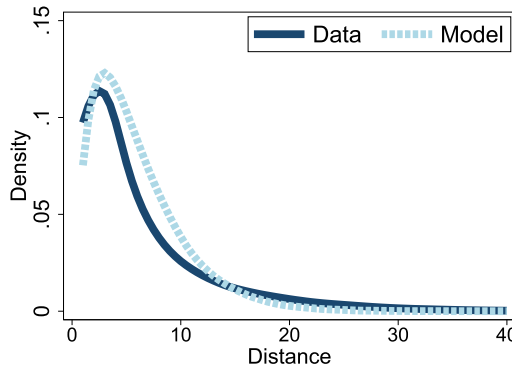


FIGURE 2. Model fit: Density of moving distance. *Notes:* This graph shows the predicted and actual density of distance moved for all types of households in our data, conditional on a move to a different Census tract.

¹³We fix $\beta = 0.95$ for all types.

¹⁴In the data, we know the Census block of residence for each household. We treat households that move within tract as if they did not move and for the remaining moves, we define distance moved as the distance between tract centroids of the sending and receiving tracts.

¹⁵The model fit is almost identical for the 24 types of low-income households with children that accept housing vouchers, the upcoming focus of much of our paper.

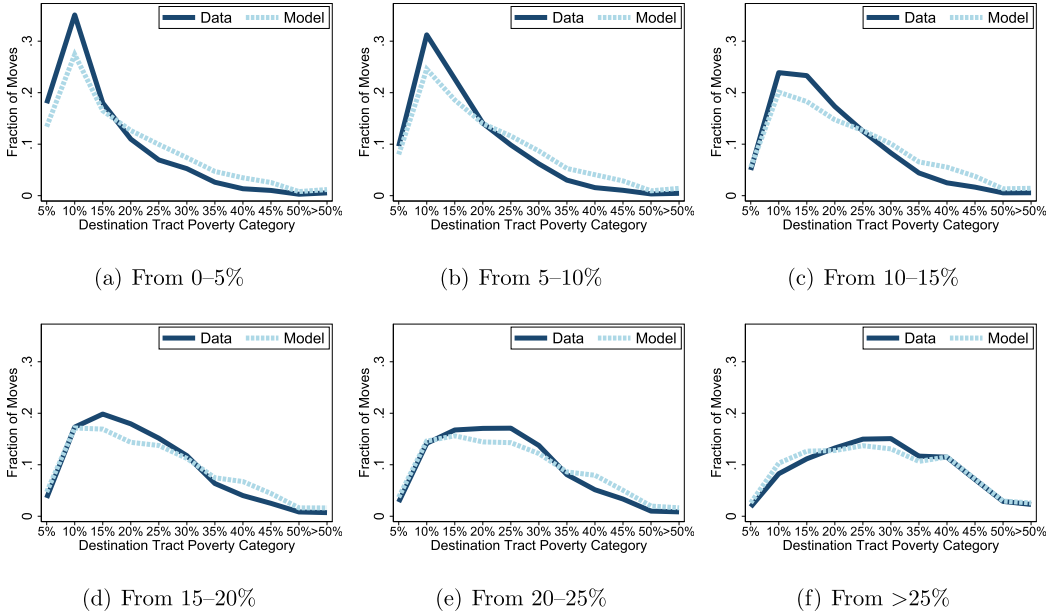


FIGURE 3. Poverty category transitions $t - 1$ to t , conditional on moving. *Notes:* These graphs show the predicted and actual density of moves in our data. Panels show outcomes for different poverty rates at the initial location. The x -axis of each panel is the poverty rate at the destination location and the y -axis is the frequency that poverty rate is chosen.

for the data (dark solid line) and as predicted by the model (light dotted line). Figure 3 shows model fit for some very low-probability moves.¹⁶ The model tends to underpredict the probability that households living in low-poverty tracts move to a low-poverty tract, conditional on a move occurring. Aside from that, in our view the model fits the data well.

2.4 Preferences for amenities, housing and consumption

We specify that δ_ℓ is the following function of consumption enjoyed in tract ℓ (c_ℓ), the quantity of housing rented in tract ℓ (h_ℓ), and type-specific amenities associated with location ℓ that are fixed over time (A_ℓ),

$$\delta_\ell = \left(\frac{1}{\sigma_\epsilon}\right) \ln A_\ell + \left(\frac{1-\alpha}{\sigma_\epsilon}\right) \ln c_\ell + \left(\frac{\alpha}{\sigma_\epsilon}\right) \ln h_\ell,$$

σ_ϵ is a parameter that effectively rescales the variance of the draws of the ϵ utility shocks and α is a parameter that determines preferences for housing relative to consumption. We will specify that σ_ϵ is identical for all households but will allow α to vary across types of households. For the purposes of exposition, we temporarily suppress all type-specific subscripts.

¹⁶For perspective, the unconditional probability of any move is less than 10%.

We assume the renting households in our sample have no savings. Households not receiving any rental assistance and choosing to live in location ℓ have the budget constraint $w = c_\ell + r_\ell \cdot h_\ell$, where w is type-specific income and r_ℓ is the quality-adjusted price-per-unit of housing in location ℓ . Given preferences and constraints, households choosing location ℓ choose optimal consumption and housing to satisfy $c_\ell = (1 - \alpha)w$ and $r_\ell \cdot h_\ell = \alpha w$. Since income is fixed for each type in our sample, and as a direct result of assumptions we have made about a lack of savings and the log-separability of consumption and housing in utility, consumption is independent of location choice. The quantity of housing consumed varies across locations in a deterministic way determined by the rental price per unit of housing. As indicated by the first-order conditions, as households move from (say) cheaper to more expensive locations, their optimal quantity of housing falls such that the expenditure share on housing stays constant.

2.5 Housing vouchers and likelihood calculations

Our assumption of Cobb–Douglas preferences is important, as it implies that the presence of households in our data that receive housing assistance in the form of vouchers does not affect our type-specific estimates of δ_ℓ . To understand why, it is useful to compare the indirect utility received in any given tract by voucher-receiving and nonvoucher-receiving households.

For this section, we continue to suppress type-specific subscripts. When households do not receive a voucher, the optimized indirect utility in location ℓ is equal to

$$\delta_\ell = \left(\frac{1}{\sigma_\epsilon} \right) \ln A_\ell + \left(\frac{1 - \alpha}{\sigma_\epsilon} \right) \ln[(1 - \alpha)w] + \left(\frac{\alpha}{\sigma_\epsilon} \right) \ln(\alpha w / r_\ell). \quad (9)$$

Computing the indirect utility of voucher-receiving households is a bit more complicated. Voucher recipients face constraints that are illustrated by Olsen (2003) and are summarized by HUD.gov (2021), which we repeat below:

A family which receives a housing voucher can select a unit with a rent that is below or above the payment standard. The housing voucher family must pay 30% of its monthly adjusted gross income for rent and utilities, and if the unit rent is greater than the payment standard the family is required to pay the additional amount. By law, whenever a family moves to a new unit where the rent exceeds the payment standard, the family may not pay more than 40% of its adjusted monthly income for rent.

In our model, utility is strictly increasing in the quantity of housing. Since households have to pay 30% of their income to receive any level of assistance, if they choose to receive assistance they should choose the maximum amount of assistance. This implies voucher recipients will never rent a housing unit with rent less than the payment standard, which we denote as *std*.

As a thought experiment, suppose (counterfactually) households are able to treat their housing assistance as if it were extra income. In this case, households would optimally choose the following expenditures on housing and consumption:

$$r_\ell \tilde{h}_\ell = \alpha(0.7w + \text{std}), \quad (10)$$

$$\tilde{c}_\ell = (1 - \alpha)(0.7w + \text{std}). \quad (11)$$

TABLE 1. Consumption and housing by α for voucher recipients.

Case	Range of α	c	h
1	0 to α^L	$0.7w$	std/r_ℓ
2	α^L to α^H	$(1 - \alpha)(0.7w + std)$	$\alpha(0.7w + std)/r_\ell$
3	α^H to 1	$0.6w$	$(std + 0.1w)/r_\ell$

However, the voucher program specifies that voucher recipients can consume no more than 70% of their income w on consumption since they have to spend a minimum of 30% of income to receive a voucher of any size up to std . This constraint binds when

$$\alpha \leq \frac{std}{0.7w + std}. \tag{12}$$

The voucher program also specifies that voucher recipients can consume no less than 60% of their income w since they are not allowed to spend more than 40% of their income on rent. This constraint binds when

$$\alpha \geq \frac{std + 0.1w}{0.6w + (std + 0.1w)}. \tag{13}$$

Define α^L and α^H as the values of alpha such that equations (12) and (13) hold with equality, respectively. Table 1 summarizes the quantities of consumption and housing for voucher recipients for ranges of α given income w and the voucher rules.

Regardless of the value of α , the difference in utility between accepting a voucher and not accepting a voucher is constant across tracts. Define $\delta_{\ell,v} - \delta_\ell$ as the difference in utility of living in tract ℓ with a voucher and without a voucher

$$\begin{aligned} \text{Case 1: } 0 < \alpha < \alpha^L & \quad \left(\frac{1 - \alpha}{\sigma_\epsilon}\right) \ln\left(\frac{0.7}{1 - \alpha}\right) + \left(\frac{\alpha}{\sigma_\epsilon}\right) \ln\left(\frac{std}{\alpha w}\right), \\ \text{Case 2: } \alpha^L \leq \alpha \leq \alpha^H & \quad \left(\frac{1}{\sigma_\epsilon}\right) \ln\left(\frac{0.7w + std}{w}\right), \\ \text{Case 3: } \alpha^H < \alpha < 1 & \quad \left(\frac{1 - \alpha}{\sigma_\epsilon}\right) \ln\left(\frac{0.6}{1 - \alpha}\right) + \left(\frac{\alpha}{\sigma_\epsilon}\right) \ln\left(\frac{std + 0.1w}{\alpha w}\right). \end{aligned} \tag{14}$$

In all three cases, $\delta_{\ell,v} - \delta_\ell$ is constant across tracts. To understand why, note that for any household std and w are constant. The only variables that vary across tracts that enter indirect utility, A_ℓ and r_ℓ , enter the expression for indirect utility of voucher and nonvoucher recipients exactly the same way; these terms cancel when taking the difference of indirect utilities of voucher and nonvoucher households.¹⁷ Thus, the probability that any particular tract is chosen does not depend on whether or not the household is receiving a voucher and our likelihood calculations are not affected by the presence of voucher recipients.

¹⁷This follows from our specification of log-separable preferences for consumption and housing.

Another way of understanding why the presence of some voucher-recipients in the data does not affect our estimates is to study $\delta_\ell - \delta_{\ell'}$ for any tracts ℓ and ℓ' . For both voucher-holders and nonvoucher holders, this is equal to

$$\delta_\ell - \delta_{\ell'} = \left(\frac{1}{\sigma_\epsilon}\right)(\ln A_\ell - \ln A_{\ell'}) - \left(\frac{\alpha}{\sigma_\epsilon}\right)(\ln r_\ell - \ln r_{\ell'}). \quad (15)$$

Thus, given estimates of α and σ , our identification of differences across tracts in amenities is unaffected by the share of each type that receives vouchers. This matters because not all eligible households in our data receive vouchers, and we do not know which households receive them.

2.5.1 Estimating type-specific w and α We wish to estimate w and α for each type, but the CCP data does not contain measures of income or rental expenditures. Instead, we estimate w and α by type using data from the Census. Starting with w , for any given Census tract ℓ we compute the average income of renters in the tract in the 2000 Census, call it \bar{w}_ℓ . We restrict our sample of tracts to tracts with at least 250 rental units, 1642 tracts in total. Denote the share of type τ renters in tract ℓ according to the CCP data in the year 2000 as η_ℓ^τ . Ideally, we could estimate type-specific income, w^τ , by regressing average tract income \bar{w}_ℓ on the full set of type shares in each tract η_ℓ^τ , that is,

$$\bar{w}_\ell = \sum_\tau w^\tau \eta_\ell^\tau + \text{error}_\ell^w,$$

where by construction $\sum_\tau \eta_\ell^\tau = 1$. That said, we wish to enforce that estimates of annual income are at least $\underline{w} = \$12,000$ for every type. To do this, we run the regression

$$\bar{w}_\ell - \underline{w} = \sum_\tau (w^\tau - \underline{w}) \eta_\ell^\tau + \text{error}_\ell^w$$

and impose that $w^\tau \geq \underline{w}$ in estimation. We estimate that 13% of our types (19 of 144 types) have income at our lower bound of \$12 thousand per year. The average income of the other 125 types is \$47 thousand per year, with a standard deviation of \$31 thousand. The largest type-specific income we estimate is \$173 thousand per year.

Our next step is to estimate a value of α for each type. Denote our estimates of w^τ as \hat{w}^τ and denote the average level of rental expenditures (paid by renters) measured in the 2000 Census in tract ℓ as $\bar{r}h_\ell$. The first-order condition of households implies

$$\bar{r}h_\ell = \sum_\tau \alpha^\tau w^\tau \eta_\ell^\tau, \quad (16)$$

where α^τ is the type-specific expenditure share on rents. We transform this equation so regressions do not place disproportionate weight fitting tracts with relatively high average rents. Define predicted average income in tract ℓ as

$$\hat{w}_\ell = \sum_\tau \hat{w}^\tau \eta_\ell^\tau.$$

Divide equation (16) by \hat{w}_ℓ and substitute our estimate of annual income \hat{w}^τ for w^τ to yield

$$\frac{\overline{rh}_\ell}{\hat{w}_\ell} = \sum_\tau \alpha^\tau \left(\frac{\hat{w}^\tau \eta_\ell^\tau}{\hat{w}_\ell} \right).$$

We run a regression of the form

$$\frac{\frac{\overline{rh}_\ell}{\hat{w}_\ell} - \underline{\alpha}_\tau}{\overline{\alpha}_\tau - \underline{\alpha}_\tau} = \sum_\tau \left[\frac{\alpha^\tau - \underline{\alpha}_\tau}{\overline{\alpha}_\tau - \underline{\alpha}_\tau} \right] \left(\frac{\hat{w}^\tau \eta_\ell^\tau}{\hat{w}_\ell} \right) + \text{error}_\ell^{rh}. \tag{17}$$

This enables us to easily enforce in estimation that $\underline{\alpha}_\tau \leq \alpha_\tau \leq \overline{\alpha}_\tau$.¹⁸ We set $\underline{\alpha}_\tau = 0.1$ and $\overline{\alpha}_\tau = 0.7$. We find that that 3 types (2%) have an expenditure share of exactly 10% and 4 types have an expenditure share of exactly 70%. For the types with α^τ strictly between these bounds, we estimate the average value of α^τ is 27% with a standard deviation of 12.2%.

Denote our estimates of α^τ as $\hat{\alpha}^\tau$. Figure 4 presents a scatterplot of $\hat{\alpha}^\tau$ and \hat{w}^τ . The small dots show the 144 type estimates and the larger diamonds show mean estimates of α^τ when we group \hat{w}^τ into 10 bins, one for each income decile. Figure 4 shows a large dispersion of estimates of α^τ , especially in the lowest-income decile; we take these type-specific estimates at face value and do not adjust them. As a check, we compute average values in each bin for both \hat{w}^τ and $\hat{\alpha}^\tau$. Although the individual type data vary somewhat, on average the expenditure share on rent falls from about 40% for households at the lowest income levels to about 20% once income surpasses \$50 thousand per year.

We acknowledge the decline in the average expenditure share on rents with income is at odds with our assumption of Cobb–Douglas preferences if all households have the same Cobb–Douglas utility function. Our assumption of Cobb–Douglas utility allows a

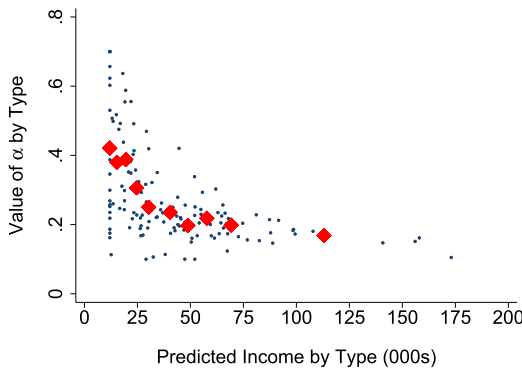


FIGURE 4. Estimates of α^τ and w^τ . *Notes:* This figure shows type-specific estimates of α^τ , the small dots, and the average estimated value of α^τ for each decile of predicted income, the larger diamonds.

¹⁸When $\underline{\alpha}_\tau \leq \alpha_\tau \leq \overline{\alpha}_\tau$, the term in brackets is always between 0 and 1.

tractable way for households in the model to consume less housing in areas with higher rent (as opposed to assuming a constant demand for housing); and, as noted earlier, this assumption allows us to ignore the presence of voucher-receiving households in our data when estimating the model. The cost to this assumption is that it is not consistent with the data suggesting the budget share on housing declines with income, unless we assume there is a distribution of α in the population that happens to be correlated with income and we have uncovered that distribution. Additionally, our assumption of Cobb–Douglas utility does not allow expenditure shares to vary with the level of rental prices; if housing and consumption are more complementary in utility than Cobb–Douglas, for example, expenditure shares increase with the level of rental prices.¹⁹

Finally, note that the presence of voucher recipients adds a small amount of error to our estimates of α^τ for the types of households eligible to receive vouchers. When some households receive vouchers, our estimate $\hat{\alpha}^\tau$ is the following function of the “true” type-specific α , $\alpha^{\tau*}$,

$$\hat{\alpha}^\tau = \alpha^{\tau*} + \rho^\tau(\aleph - \alpha^{\tau*}), \quad (18)$$

where ρ^τ is the type-specific probability of receiving a voucher and \aleph is the reported amount paid on rent as a percentage of income when receiving a voucher. Depending on how households interpret the question from the 2000 Census, “What is the monthly rent?” \aleph can be either the payment standard divided by income or it can be 30%. Later on, we show that only 24 types of households accept a housing voucher, if offered. Of these 24 types, the average expenditure share on rents is 35.8% and later we estimate that only 11.18% of households for each of these types is offered a voucher. From equation (18), if households interpret the rent question in the Census such that $\aleph = 0.30$ —this is what we predict most voucher-receiving households pay out of pocket on rent, scaled by income—we would conclude our estimates of expenditure shares of voucher-eligible types of households are too low by about 0.65 percentage points, on average ($11.18 * (0.30 - 0.358)$). If voucher-receiving households respond to the question such that \aleph is the payment standard divided by income—that is, what landlords receive in rent, scaled by income—the bias may be larger. In either case, we acknowledge that this adds some bias to our estimates of α^τ .

2.5.2 Estimating σ_ϵ Rewrite the optimized indirect utility for each type of nonvoucher household as

$$\begin{aligned} \delta_\ell &= \left(\frac{1}{\sigma_\epsilon}\right) \ln A_\ell + \left(\frac{1-\alpha}{\sigma_\epsilon}\right) \ln[(1-\alpha)w] + \left(\frac{\alpha}{\sigma_\epsilon}\right) \ln(\alpha w) - \left(\frac{\alpha}{\sigma_\epsilon}\right) \ln r_\ell \\ &= \text{const} + \left(\frac{1}{\sigma_\epsilon}\right) \ln A_\ell - \left(\frac{\alpha}{\sigma_\epsilon}\right) \ln r_\ell, \end{aligned}$$

¹⁹There is some debate among economists about the appropriate functional form for preferences for consumption and housing. For example, many authors argue the income elasticity should be less than one using micro data similar to what we have presented: See [Green and Malpezzi \(2003\)](#) and references therein. In contrast, [Davis and Ortalo-Magne \(2011\)](#) reported that median rental expenditure shares are constant across time and across metropolitan areas and are invariant to changes in income and rental prices.

where const is a type-specific constant (and we have otherwise temporarily omitted type-specific notation). Assume that log amenities include both observed \mathcal{O}_ℓ and unobserved ξ_ℓ characteristics of tract ℓ such that the above can be rewritten as

$$\delta_\ell = \lambda \cdot \mathcal{O}_\ell - \left(\frac{1}{\sigma_\epsilon} \right) \cdot \alpha \ln r_\ell + \xi_\ell.$$

The coefficient on α times log rent, $1/\sigma_\epsilon$, cannot be estimated using OLS because equilibrium rents will almost certainly be correlated with unobserved but valued characteristics of neighborhoods, ξ_ℓ . An instrument is required.

Given type-specific estimates of α from Section 2.5.1, we use a three-step IV approach to estimate $1/\sigma_\epsilon$ that is similar to the procedure in Bayer, Ferreira, and McMILLAN (2007). As mentioned earlier, we impose in estimation that $1/\sigma_\epsilon$ is the same for all types. This means that after explicitly accounting for variation in how much people value housing relative to consumption and amenities, and abstracting from differences across types in moving costs, we impose that the importance of utility shocks in household decision making is constant across types. In the first step of our procedure, we estimate $1/\sigma_\epsilon$ using two-stage least squares. We include characteristics of the housing stock 0–5 miles from tract j in \mathcal{O}_j as controls (number of rooms, number of units in the housing structure and age of structure) and use characteristics of the housing stock 5–20 miles from the tract as instruments for rent. The first-stage F-statistic is 5.35: For more details, see Appendix B in the Online Supplementary Material of Davis et al. (2021).

In the second step, we use estimates of $1/\sigma_\epsilon$ and type-specific estimates λ from the first step, call them $\widehat{\frac{1}{\sigma_\epsilon}}$ and $\widehat{\lambda}^\tau$, to construct predicted indirect utilities for each type that controls for unobserved amenities as

$$\widehat{\delta}_\ell^\tau = \widehat{\lambda}^\tau \cdot \mathcal{O}_j - \left(\frac{1}{\widehat{\sigma_\epsilon}} \right) \alpha^\tau \ln r_\ell.$$

We simulate the model using this specification for indirect utility and adjust r_ℓ for all ℓ tracts until the simulated total housing demand in any tract is equal to the observed housing demand in the estimation sample for that tract.²⁰ This procedure determines market-clearing rents in all tracts in the absence of unobserved amenities. We use these rents as instruments to estimate $1/\sigma_\epsilon$ in the third and final step with an F-statistic of 31.7. Intuitively, the F-statistic rises from 5 to 32 because the first step only uses information about the quality of substitutes for each tract individually whereas the third step uses similar information for all tracts. We estimate that $1/\sigma_\epsilon = 0.84$ with a standard error of 0.198.²¹

2.6 Estimating voucher-eligible households with children

2.6.1 Number of children

For our analysis of the MTO experiment and our alternative policy simulations, we wish to track the outcomes of households with children that

²⁰Given Cobb–Douglas preferences, type-specific housing demand in tract ℓ is $\alpha^\tau w^\tau / r_\ell$.

²¹The value of our 1st-stage estimate of $1/\sigma_\epsilon$ is 0.414 with a standard error 0.189.

are offered housing vouchers. This means we need estimates of which types have children, and for the types with children the number of children per household. In the 2000 Census, we know the average number of children by tract for *all* households, not just renting households. To estimate the average number of children by type for our sample of renting households, we invent a new type called “owner-occupiers.” We then run the regression

$$\bar{k}_\ell = \sum_{\tau} k^{\tau} \tilde{\eta}_{\ell}^{\tau} + \text{error}_{\ell}^k,$$

where k^{τ} is the average number of children per household for type τ households and \bar{k}_ℓ is the average number of children per household in tract ℓ . $\tilde{\eta}_{\ell}^{\tau}$ is the percentage of type τ households in tract ℓ (which, relative to η_{ℓ}^{τ} , explicitly accounts for the fact that there is an additional type, homeowners). As before, $\sum_{\tau} \tilde{\eta}_{\ell}^{\tau} = 1$.

To limit the influence that owner-occupiers have on our estimates of k^{τ} for renters, we restrict the estimation sample to tracts where at least 50% of the households rent.²² This restricts our sample to 1052 tracts (from 1642 tracts) with 250 or more renting households. We do not restrict k^{τ} to be an integer but we impose in estimation that $0 \leq k^{\tau} \leq 3$ for all types. After discarding the owner-occupier type, we estimate that 80 types in our sample (56% of types) have less than 0.5 children on average and 17 types (12%) have more than 2.9 children.

2.6.2 Voucher eligibility In 2000, 2-person households with annual income less than \$25,020 and 3-person households with annual income less than \$28,140 were eligible to receive a housing voucher in Los Angeles County. Given these rules, we estimate 59 types out of 144 were eligible for a voucher, 41% of households.²³ This implies that of the 1.634 million renting households in Los Angeles, 670 thousand households were eligible for a housing voucher in 2000. Only 62,487 households received a voucher, 9.3% of those eligible. Of the 59 types eligible to receive a voucher, 31 types have estimated income less than \$25,020 and 0 children ($k^{\tau} < 0.5$) and 28 types have estimated income less than \$28,140 and have at least one child ($k^{\tau} \geq 0.5$). Our estimate that 47% (28/59) of voucher-eligible households have at least one child in Los Angeles in 2000 is very close to the actual percentage of voucher households with children in Los Angeles that can be computed directly from public-housing-agency data, 52.8%.²⁴

We use the derivations in equation (14) to check if any of the 28 types of households with at least 0.5 children that are eligible for a housing voucher would choose to decline a housing voucher if offered. If the value of $\delta_{\ell,v} - \delta_{\ell}$ is less than zero for any type

²²We experimented with setting the rental-share cutoff in 10 percentile increments, from 10% to 90%. Type-specific estimates seemed to stabilize at around a rental-share cutoff of 50%. Additionally, this cutoff minimized the number of types of households with k^{τ} exactly equal to 3, the upper bound on the number of children that we impose in estimation.

²³The average expenditure share on rents of these 59 types is 37.4%.

²⁴The Housing Authorities of the City of Los Angeles, Los Angeles County and the City of Long Beach report 40,344, 16,583, and 5372 voucher units, respectively. This total of 62,299 represents almost all of the 62,487 vouchers in the county. The share of voucher units with children is 52%, 54%, and 55% in the City of Los Angeles, the County of Los Angeles and City of Long Beach, respectively. The voucher-weighted average is 52.8% of voucher units have children, 32,993 units.

TABLE 2. Differences between voucher-eligible types with at least 0.5 children.

	Reject Voucher (4 Types)	Accept Voucher (24 Types)
Average number of children	1.32	2.11
Average annual gain from accepting voucher	−\$367	\$2903
Average value of \hat{w}^τ	\$26,822	\$18,737
Average value of $\hat{\alpha}^\tau$	0.181	0.358
African-American		25.0%
Hispanic	100.0%	75.0%
Percent of Households Offered Voucher	18.4%	81.6%

Note: This table shows various economic and demographic characteristics of the 4 types of households that reject a housing voucher if offered and the 24 types of households that accept a voucher.

of household, that household would reject a voucher. We use the estimate of $\delta_{\ell,v} - \delta_\ell$ to determine the value of the voucher to households in terms of equivalent extra annual income. Specifically, we set the estimate of equivalent extra annual income from the voucher equal to annual income multiplied by $\exp[\sigma_\epsilon(\delta_{\ell,v} - \delta_\ell)] - 1$, which can be derived from equation (9).

We use the payment standard of \$9192 per year (\$766 per month) as set by the U.S. Department of Housing and Urban Development (HUD) for a 2-bedroom apartment in Los Angeles in 2000. We find that 4 out of 28 types of households should reject the offer of a housing voucher. Shown in Table 2, relative to the 24 types of households that accept a voucher, these types have fewer children, higher income, and lower expenditure shares on rents. The last line of Table 2 shows that these 4 types account for 18.4% of all households technically eligible to receive a voucher. For these four types, accepting the voucher offer would be equivalent to losing \$367 per year of income.

Figure 5 shows the income-equivalent gain from the voucher (dots) and the housing subsidy from the voucher (solid line) for all 28 types. The figure also includes a line at zero to highlight the four types that optimally do not take up the voucher. For the 24 types that accept the voucher, on average the voucher is equivalent to an increase in annual income of \$2903, shown in Table 2. The average rent subsidy, defined as the payment standard less 30% of household income, for these 24 types is equal to \$3571 suggesting a “bang-for-the-buck” (dollars of income-equivalent utility per dollar of subsidy) of 81% on average. As Figure 5 shows, for many of the types the income-equivalent is nearly exactly equal to the housing subsidy yielding a bang-for-the-buck of 100%. The average income of the 24 types that take up the voucher is \$19 thousand, explaining why the benefits of the voucher are high. The types that accept a voucher are mixed racially, 6 African-American and 18 Hispanic, and have more children (2.11) and have a higher expenditure share on rents (36%) than the types that do not take the voucher. When we use our model to simulate actual and counterfactual housing-voucher policies, we restrict the households that are offered housing vouchers to one of these 24 types.

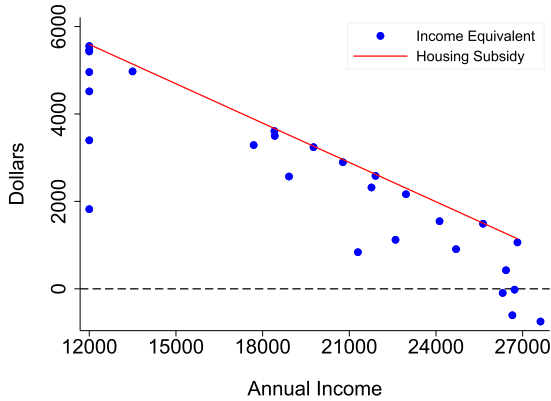


FIGURE 5. Income-equivalent value of voucher and amount of housing subsidy. *Notes:* This figure shows the income equivalent of the housing voucher, the dots, and the dollar amount of the housing subsidy provided by the housing voucher, the solid line, for each of the 28 types of households with children that are eligible to receive housing vouchers based on their household income.

3. ANALYSIS OF MTO

Moving to Opportunity (MTO) was a randomized control trial authorized by Section 152 of the 1992 Housing and Community Development Act that was designed to study the effect of low-poverty neighborhoods on child outcomes, free from selection issues associated with earlier studies on the same topic.²⁵ The MTO experiment began in the 1990s and randomly assigned a group of volunteer households with children eligible to live in low-income housing projects in five U.S. metropolitan areas to three different groups: (i) a treatment group that received housing assistance—either a housing voucher or housing certificate, with a difference we explain later—that in the first year could be applied only in Census tracts with a poverty rate under 10% and could be applied unconditionally thereafter, (ii) a second treatment group that received equivalent housing assistance with no location restriction attached, and (iii) a control group that received no voucher or certificate. Nonprofit organizations were paid as part of the MTO program to provide counseling to participating families, helping them to find appropriate units to rent. According to [Goering and Kraft \(1999\)](#), 4610 families enrolled in the MTO program and 62% of households that were offered a location-restricted voucher in Los Angeles (47% of households across all 5 MTO sites) accepted the voucher and moved to a location with less than a 10% poverty rate.²⁶ The focus on restricting the location choice of voucher recipients to neighborhoods with a poverty rate of less than 10% reflected expectations at the time that the poverty rate is highly correlated with neighborhood quality and that neighborhoods with poverty rates under 10% were likely to positively affect child outcomes.

²⁵See [Leventhal and Brooks-Gunn \(2000\)](#), [Durlauf \(2004\)](#), and [Ross \(2011\)](#) for a discussion of this earlier literature.

²⁶The take-up rate of families offered a nonlocation-restricted voucher was 75% for Los Angeles and 60% across all 5 MTO sites.

Summarizing the medium- to long-term impacts of MTO, Sanbonmatsu, Kling, Duncan, and Brooks-Gunn (2006), Kling, Liebman, and Katz (2007) and others show that on average the MTO treatment successfully reduced exposure to crime and poverty and improved the mental health of female children, but failed to improve child test scores, educational attainment, or physical health. This was surprising to many researchers given earlier work that demonstrated potentially quite large neighborhood effects on child outcomes. In later work, CHK identify important positive effects. Specifically, CHK estimate the impact of accepting an MTO voucher on the annual adult earnings of each child under age 13 at the time the MTO voucher is accepted is \$3477 with a standard error of \$1418.²⁷

We ask if simulations of our estimated model can reconcile the specific estimates of the impact of MTO on adult earnings of children with estimates from Opportunity Atlas from Chetty et al. (2018) that suggest effects of neighborhoods on adult earnings of children can be large and are correlated with poverty rates. In a way, we describe precisely in a moment, we use these Opportunity Atlas estimates to map the location choices of households receiving housing assistance to the adult wages of children in those households. The bottom line is that we can nearly match the CHK statistic (our estimate is \$2923) and we have an explanation for why the impact of MTO on adult earnings of children was not more sizable. Ultimately, poverty rates are not perfectly correlated with Opportunity Atlas scores and many households receiving a location-restricted voucher move to low poverty locations that also have relatively low Opportunity Atlas scores. In other words, when households are given location-restricted vouchers, conditioning on the poverty rate is not sufficient to ensure the adult earnings of children improve. To study the potential maximal impact of location-restricted vouchers on child earnings, in the next section we run a set of counterfactual simulations that directly require voucher-receiving households to live in high Opportunity Atlas neighborhoods.

To demonstrate the impact of neighborhood selection on the CHK result, we perform three sets of model simulations. For all simulations, we only consider the experiences of a small set of relevant households that we call “MTO Simulation Households,” the parameters of which we delineate soon. In the spirit of replicating the original, relatively small MTO experiment, we do not allow for any general-equilibrium effects on rental prices or any other variable. The three sets of simulations are:

1. *Baseline*: No household is offered any housing assistance. Household utility for type τ living in tract ℓ is δ_ℓ^τ , as estimated in Section 2.

2. *MTO*: Two-thirds of households are offered housing vouchers and one third of households are offered housing *certificates*. Households receiving a housing certificate are all governed by Case 1 of equation (14): They pay 30% of their income to receive a certificate for the payment standard, and are not allowed to spend any more of their income on rent. Any households receiving housing assistance must live in a Census tract with a poverty rate no greater than 10% in the first year. After the first year, they continue to receive the voucher and can live in any Census tract. Households that reject the initial offer of the housing assistance MTO-style voucher are not offered a voucher in the future. All households understand the full set of program rules.

²⁷See column (4) of Table 3 of their paper.

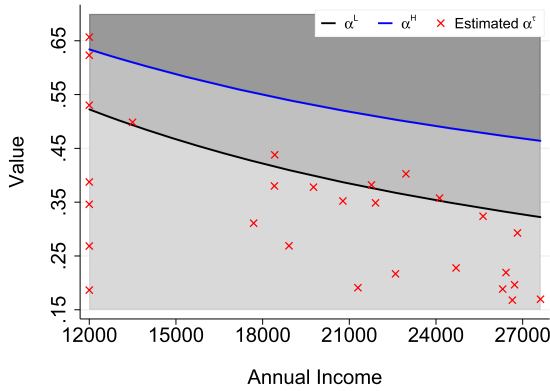


FIGURE 6. Mapping estimates of $\hat{\alpha}^\tau$ to voucher cases of equation (14). *Notes:* This figure plots type-specific estimates of $\hat{\alpha}^\tau$ for the 24 types of households that accept housing assistance and which case of the voucher program as written in equation (14) applies to that type. Case 1 is the light-grey area in the southwest; case 2 is the medium-grey area bounded by the two solid lines; and case 3 is the dark-grey area in the northeast.

We adjust δ_ℓ^τ for tracts where housing assistance can be applied in the first year and all tracts in the second and subsequent years by appropriately applying equation (14), depending on whether the household is receiving a certificate or a voucher and that household's value of α . Note that for most types of voucher-accepting households, the distinction of certificate or voucher is irrelevant. Figure 6 shows the mapping of our estimates of $\hat{\alpha}^\tau$ to cases from equation (14) for voucher-receiving households. Case 1 binds or very nearly binds for 24 of the 28 types and Case 3 only binds for 1 type of household.²⁸

3. *MTO-R*: We assign households to neighborhoods randomly according to the distribution of neighborhood poverty-rates that households are exposed to under the MTO simulations.²⁹

Note that one important difference of our MTO simulations and the actual MTO experiment is that we do not model counseling; we discuss the potential implication of this omission later. We define our MTO Simulation Households as those households that (a) are one of the 24 types of low-income agents with at least 0.5 children that are predicted to always accept housing assistance (see Section 2.6.2) and (b) who reside at the

²⁸We do not include some program details in our simulations. First, housing certificates were phased out of use by 2001 in favor of housing vouchers. Since Case 1 is binding for 24 of 28 types, we assume certificate recipients in the MTO experiment always receive certificates, even after certificates were phased out in the general population. Second, a referee has alerted us that households receiving vouchers prior to 1999 technically had the option to spend more than 40% of their income on rents. We abstract from this detail in our simulations but note that Case 3 is binding for only one type of household.

²⁹Specifically, the procedure is as follows: (1) pool the set of simulated Census tract choices in MTO and the unconditional list of sample Census tracts. (2) Estimate a probit model predicting the probability that a record comes from the simulated data using only tract-poverty-rate categories as explanatory variables, and obtain the predicted probability p_j (propensity score) that a record from tract j comes from the simulated data. (3) Draw MTO-R simulated locations from the full set of Census tract with probability $\Pr(j) = \frac{1}{J} \left(\frac{p_j}{1-p_j} \right) \left(\frac{1-p}{p} \right)$.

start of the simulation in one of 15 Census tracts with at least 250 occupied nonsenior-citizen public housing units.³⁰ While a few of the developments contain a small share of units set aside for senior citizens, these are predominately public housing developments for families with children. MTO Simulation Household types are represented in all simulations in proportion to their empirical distribution in the 15 public-housing tracts. Note that we hold income fixed for all households in all simulations.³¹

We keep track of the location of all households in all simulations and then map the sequence of locations to expected earnings of children using data from Opportunity Atlas. For each Census tract in the United States, [Chetty et al. \(2018\)](#) generate the Opportunity Atlas estimates by measuring the earnings of children given the earnings of parents using tax data from the IRS. For each tract, the Opportunity Atlas reports a child's expected percentile in the nationwide income distribution at age 26 given household income of (a) the 25th percentile and (b) the 75th percentile of the nationwide income distribution.³² We map each type's household income to the percentile of the nationwide household income distribution. Then we use the two estimates from Opportunity Atlas to produce via linear interpolation (or extrapolation) an expected percentile in the nationwide age-26 income distribution for the children of that type of household in that tract. We interpret the Opportunity Atlas estimates as causal and for the analysis in this section, we assume the estimates for each tract are fixed. In a later section, we allow the Opportunity Atlas estimates to change in response to a possibly large policy intervention that alters the average income and racial composition of each neighborhood.

Households enter the MTO intervention with children of different ages. Assumptions we make about the starting age of children in the MTO experiment and years of exposure to treatment effects once they enter the MTO experiment are detailed in Appendix C of the Online Supplementary Material of [Davis et al. \(2021\)](#). We simulate each household for 18 years, including the appropriate number of years prior to the start of the MTO intervention. For the years before the MTO intervention begins, we assign the expected Opportunity Atlas percentile of the initial tract of residence (one of the 15 tracts described previously) to the child.³³ After the intervention starts, we assign the expected Opportunity Atlas percentile for each optimally chosen location in the simulation for the number of years shown in column (3). We average all 18 percentiles and then convert the resulting average percentile to a level of income using the nationwide age-26 income dis-

³⁰We also include the Census tracts containing Avalon Gardens and Hacienda Village, which are below the 250 unit threshold but are proximate to several large developments. The MTO experiment also required the tracts to have a poverty rate of at least 40% in 1990. Of our 15 tracts, only 2 have a poverty rate of less than 40% in 2000; one tract has a poverty rate of 35.4% and the other tract poverty rate is 37.3%.

³¹A case can be made that expected household income should rise once households move to low-poverty neighborhoods, but [Jacob and Ludwig \(2012\)](#) find that households receiving housing vouchers in Chicago reduce labor supply and earnings. The MTO data show no significant impact on adult earnings.

³²The Opportunity Atlas data are for 2010 Census tracts. We use the Census Tract Relationship File from the U.S. Census Bureau to map the 2010 tracts to 2000 tracts. For the two cases in which this mapping fails, we assign an Opportunity Atlas number to those two tracts using spatial interpolation.

³³We assume that the household continuously resided in the initial tract of residence prior to the start of the simulation. Since this assumption is imposed for the baseline, MTO and MTO-R simulations, it will not affect comparisons across simulations.

TABLE 3. Simulations of MTO-style vouchers for MTO simulation households.

Type (1)	Sim. Share (2)	Demographics				Baseline		MTO			MTO-R
		w^τ (3)	α^τ (4)	Race (5)	k^τ (6)	Pov. Rate (7)	AE (8)	Pov. Rate (9)	Take-Up Rate (10)	Treated Δ AE (11)	Treated Δ AE (12)
139	0.057	12.0	0.27	B	1.30	0.44	7.9	0.35	30.6	3.65	7.09
142	0.031	12.0	0.35	B	1.29	0.39	9.7	0.23	63.6	2.74	5.70
143	0.023	12.0	0.53	B	0.53	0.40	9.9	0.19	81.4	2.77	5.80
28	0.051	12.0	0.62	H	3.00	0.42	9.3	0.21	74.0	3.62	6.32
136	0.052	12.0	0.66	B	0.96	0.42	8.3	0.21	77.2	2.47	7.00
133	0.102	12.0	0.39	B	3.00	0.45	7.3	0.24	67.5	3.78	8.10
32	0.038	13.5	0.50	H	3.00	0.39	12.4	0.24	56.9	3.19	4.19
137	0.065	18.4	0.38	B	1.82	0.43	10.4	0.37	19.7	3.14	6.29
Other 16	0.581	21.7	0.30		2.23	0.40	14.1	0.39	4.2	2.65	4.08
Avg. top 8		13.1	0.45		2.07	0.43	9.0	0.26	56.5	3.30	6.67
Overall Avg.		18.1	0.36		2.16	0.41	12.0	0.34	26.2	2.92	5.17

Note: Column (1) is a type reference number and column (2) is the share of that type in the simulated samples. Column (3) is estimated household income in \$000s, (4) is estimated value of α , (5) is assigned race (B = African-American and H = Hispanic) and (6) is estimated number of children. Column (7) is average poverty rate in the baseline simulations and column (8) is expected adult earnings in \$000s of each child, also in the baseline simulations. Columns (9)–(11) refer to the MTO simulations: (9) is the average poverty rate of everyone offered a voucher, (10) is the percentage of households that accept the MTO-style voucher and (11) is the change in the expected adult earnings in \$000s of each child relative to baseline *conditional on accepting a voucher*. Column (12) is the change in expected adult earnings in \$000s of each child for households that accept a voucher, relative to baseline, in the MTO-R simulation in which households randomly choose a tract.

tribution for individual earners.³⁴ The estimates we report are averages across simulated households of this level of income.

Table 3 reports simulation results. The table separately shows the outcomes of 8 household types that account for more than 90% of all MTO-voucher acceptances, the appropriately-weighted average outcome for the other 16 types, the overall average and the average of the top 8 rows. The top 8 rows sort types by household income (column 3) and then by housing expenditure share α (column 4). Column (2), “Sim. Share,” shows the proportion of the population that is offered an MTO voucher that belongs to this type, column (5) shows the race (B = African-American and H = Hispanic) and column (6) shows number of children. The poverty rate and level of adult earnings of children in \$000s (per child) from the Baseline simulations are shown in columns (7) and (8). Columns (9)–(11) show results from the MTO simulations. Column (9) shows the poverty rate for all households including those that do not accept the voucher; column (10) shows the fraction of households that accept the MTO-style voucher; and column (11) shows the per-child change in annual adult earnings, in \$000s, for the children of all households that accept the voucher. Column (12) shows results from the MTO-R simulations; this is the projected change in adult earnings in \$000s, per child, if households had randomly selected a tract with the same poverty rate as tracts actually chosen in the MTO simulation. The averages reported are per household except for the adult-earnings

³⁴We assume each year of exposure has the same importance at every age, consistent with the results of Chetty and Hendren (2018).

columns (8, 11, and 12), which are reported on a per-child basis such that these estimates are compatible with those of CHK.

Overall, three results are worth emphasizing. First, perhaps not surprisingly, the MTO experiment reduced exposure to poverty. The average poverty rate of the Census tract of residence falls from 41% in the baseline to 34% in the MTO simulations. The overall reduction in poverty for the top 8 types is more dramatic, from 43% to 26%. Second, our overall average simulated voucher take-up rate in the MTO simulations is only 26.2%. Recall that *all* of the simulation households are predicted to accept a location-unrestricted voucher. Our predicted take-up rate is much lower than the actual MTO take-up rate in Los Angeles of 62%. We do not include the impact of counseling in our simulations, and the additional counseling that MTO offered as noted by [Galiani, Murphy, and Pantano \(2015\)](#) likely played an important role in explaining the difference between simulated and actual voucher take-up rates. A different, possibly complementary story is that the distribution of types in the MTO experiment may be different than that in our simulations.

To see this more clearly, consider the experiences of the eight types of households that account for 90% of all households accepting a voucher in the simulations. These eight types of households are poor (average income of \$13 thousand) and mostly African-American (6 of 8 types) and have a relatively high average expenditure share on rents of 45%. Each of these types has a voucher-acceptance rate in the MTO simulations of more than 19% such that the average voucher take-up rate of these types is 56%. It may be the case that these 8 types are overrepresented in the MTO experimental data relative to the other 16 types of simulation households we consider. Since these 8 types account for almost all of the households accepting a voucher, a downweighting of the other 16 types would boost the simulated voucher take-up rate but would not affect our results on the impact on adult earnings of children conditional on households accepting a voucher.³⁵

Finally, our simulations do not quite match the reported CHK estimate on the impact of accepting the MTO voucher on the adult earnings of children under the age of thirteen. As mentioned, the CHK estimate is \$3477 with a standard error of \$1418 and our estimate is very close at \$2923. The gap could reflect simple differences in weighting of the 8 main types, as the range for these types span \$2473 (type 136) to \$3780 (type 133). Interestingly, the results from the MTO-R simulations suggest the impact on adult earnings from MTO-style vouchers had the potential to have been much greater. Had the households that accepted a voucher selected a tract *randomly* with the same poverty rate as the tract they actually chose, the expected impact on per-child adult earnings from the MTO-experiment compared to baseline would have been \$5167, an increase of more than 75%. In other words, conditional on the tract having a poverty rate of less than 10%, in the MTO experiment households negatively selected into tracts—a result that holds for every one of the 8 types we emphasize.

³⁵Of course, even if we strictly limit the simulations to only these 8 types, we would underestimate the overall take-up rate at 56% as compared to 62%. That gap may represent the impact of counseling; or it might reflect a weighting of the 8 types in the MTO experimental data that is different from the simulations as 5 of the 8 types have a take-up rate of 63% or greater.

So, why did households select into relatively low Opportunity Atlas neighborhoods when offered an MTO-style voucher? For each of the 8 types, we ran median regressions (least absolute deviation) of $(\frac{1}{\alpha}) \ln A_\ell$ on the Opportunity Atlas score for all 508 tracts in Los Angeles with a poverty rate less than 10%.³⁶ For all 8 types, the conditional median of log amenities is *decreasing* with Opportunity Atlas scores in these low-poverty neighborhoods. For six of the types, the estimated negative slope is statistically significant. Additionally, we ran a median regression of the Opportunity Atlas score on log rental prices and estimated a positive coefficient of 0.963 with a standard error of 0.20, implying that rental prices increase with Opportunity Atlas scores.³⁷ The bottom line is that MTO-voucher-receiving households negatively select into relatively low Opportunity Atlas score neighborhoods both because they prefer the amenities of these neighborhoods and because the rental prices are low.³⁸

4. LARGE POLICY EXPERIMENTS

In this section, we simulate our model to ask what would happen to the adult earnings of children of the recipients of housing assistance if the county of Los Angeles were to implement a large-scale policy in which the location choices of *all* recipients of housing assistance was restricted.³⁹ Rather than directly condition feasible location choices on poverty rates, as was the case in the MTO experiment, we assume policy-makers in Los Angeles restrict the set of neighborhoods in which recipients of housing assistance can live based on the Opportunity Atlas scores of those neighborhoods. To keep language straightforward, throughout this section we denote housing assistance in these experiments as housing “vouchers,” but to be precise the housing assistance we consider is in the form of a housing certificate in which households accepting assistance spend exactly 30% of their income on rent and choose to rent a unit with monthly rent exactly equal to the payment standard. Consumption and housing for *all* recipients of housing assistance in the policy experiments that follow is summarized by Case 1 in Table 1.⁴⁰

In each policy experiment, we restrict the set of Census tracts where vouchers can be used based on the neighborhood’s Opportunity Atlas score, its forecasted percentile in the age-26 income distribution of a child’s adult earnings conditional on the parents earning the 25th percentile of the income distribution. We specify a cut-off value such that voucher-eligible neighborhoods are restricted to the top X th percentile of Opportunity Atlas neighborhoods. We consider 10 possible cutoffs in total:

³⁶The specific score we use is the child’s forecasted percentile in the age-26 earnings distribution given parent income in the 25th percentile of the earnings distribution.

³⁷This estimate implies that for each 10-percentage point increase in the neighborhood’s impact on the child’s percentile in the earnings distribution according to the Opportunity Atlas data, log rental prices increase by 9.6%.

³⁸A different possibility that we do not consider, and that is the focus of Bergman et al. (2019), is that households faced barriers in the housing search process that impeded moves to higher Opportunity Atlas locations.

³⁹In each policy scenario and in each simulation, for each type we fully solve for the value function using value-function iteration.

⁴⁰Recall that Case 1 exactly or nearly binds for 20 of the 24 types of households that accept nonlocation-restricted housing vouchers, suggesting simulations of certificates and not vouchers will not materially affect any results.

$X = 10, 20, 30, \dots, 90, 100$. To illustrate, when $X = 10$, households receiving a voucher are only allowed to live in the top 10% of neighborhoods based on the Opportunity Atlas score of that neighborhood. When $X = 100$, voucher recipients can live in any neighborhood. We call the results from the $X = 100$ experiment our baseline, since it essentially implements current policy.

We run each experiment exactly the same way: 11.18% of each of the 24 voucher-eligible types with children described earlier, currently living in any location, are offered a housing voucher.⁴¹ The set of households that are offered vouchers is predetermined and does not change; if a household ever declines the voucher in any given period, the household may accept the voucher in a later period. We choose 11.18% such that in the baseline simulation, the number of voucher-receiving households with children is equal to 2.02% of all renter households, the same as in the data for Los Angeles County in 2000 (32,993 voucher-receiving households with children and 1,634,030 rental households in total). In the experiments where we restrict the set of neighborhoods that are voucher-eligible, the percentage of households that accept housing vouchers falls, implying total expenditures on vouchers declines. Of course, policy-makers interested in maintaining constant expenditures on vouchers have the option of boosting the payment standard or increasing the number of households offered a voucher. We do not consider these alternatives as we wish to evaluate how restricting the feasible set of location choices of a fixed set of households, with no other policy parameters adjusted, changes the voucher take-up rates and adult earnings of children of those households.

In all simulations, we compute the optimal decisions of all households, including those that are not offered a voucher, to determine the steady state distribution of types across Census tracts. In all simulations, we assume neighborhood amenities A_ℓ are unaffected by changes in public policy.⁴² Rental prices in each simulation are determined in equilibrium such that total housing supply is equal to total housing demand in each tract.⁴³ In the event, rental prices change as a result of the policy, in each tract we allow the the stock of housing to expand or contract by specifying a nonzero elasticity of housing supply that is specific to that tract.

Explaining this last point, denote \mathcal{H}_ℓ^b and r_ℓ^b as the total stock of housing and the rental price per unit of housing in tract ℓ in the baseline; and, denote \mathcal{H}_ℓ^c and r_ℓ^c as the total stock of housing and the rental price per unit in tract ℓ for a specific counterfactual experiment. We link the change in the housing stock and the change in the rental price per unit between the baseline and the experiment as follows:

$$\ln(\mathcal{H}_\ell^c/\mathcal{H}_\ell^b) = \mathcal{E}_\ell \cdot \ln(r_\ell^c/r_\ell^b), \quad (19)$$

where \mathcal{E}_ℓ is the assumed elasticity of housing supply in tract ℓ . Our estimates for \mathcal{E}_ℓ for each tract in Los Angeles are from [Baum-Snow and Han \(2020\)](#). We also consider exper-

⁴¹Restated, in our counterfactual simulations we assume the rationing of vouchers under the current system continues.

⁴²In the event households begin to understand the importance of Opportunity Atlas scores once public policy conditions on these scores, our simulations are potentially subject to a Lucas critique.

⁴³Housing demand in tract ℓ for a type τ household without a voucher is $\alpha^\tau w^\tau / r_\ell$ and is equal to std / r_ℓ for a type- τ household with a voucher.

iments when we set \mathcal{E}_ℓ for all ℓ equal to 0 and, separately, to 0.5 to understand the sensitivity of our results to assumptions about the elasticity of housing supply.⁴⁴ In our view, 0 is a natural lower bound for the elasticity of housing supply, although Baum-Snow and Han (2020) estimate about 10% of tracts in Los Angeles have a negative elasticity. We set \mathcal{E}_ℓ to 0.5 as a high value to understand the sensitivity of our results if housing supply in Los Angeles is more elastic than the data suggest.⁴⁵

Before discussing our results, we mention a few nuances in the baseline simulation. Given estimated preferences, we adjust rental prices and the housing stock in each tract from what we observe in the 2000 Census to generate a stationary distribution of types in each tract. In computing this baseline given the data, we assume a tract-level housing supply elasticity of $\mathcal{E}_\ell = 0.25$ for all tracts in Los Angeles. We use the same baseline in all experiments. We chose this value to keep the deviation of baseline rental prices from the data low, while at the same time maintaining an elasticity of housing supply within the range estimated by Baum-Snow and Han (2020) for tracts in Los Angeles. Rental prices in the baseline and in current data are very similar. When we regress log rental prices in the baseline against log rental prices in the data for the 1748 tracts in our sample, the R^2 of the regression is 0.78. The coefficient on log rental prices in the data is 1.04 with a standard error of 0.013, implying relatively expensive tracts in the data are even more expensive in the baseline. When we regress the Opportunity Atlas score for all 1748 tracts on log rents in the data and then in the baseline, the coefficients are 1.50 and 1.68, respectively. These coefficients imply that for a household to increase its Opportunity Atlas score from the 37.1 percentile to the 52.1 percentile—this is a change from the bottom 10% of Opportunity Atlas tracts to the top 10%—the predicted change in log rents in the data is 0.226 and in the baseline is 0.253.

4.1 Fixed Opportunity Atlas

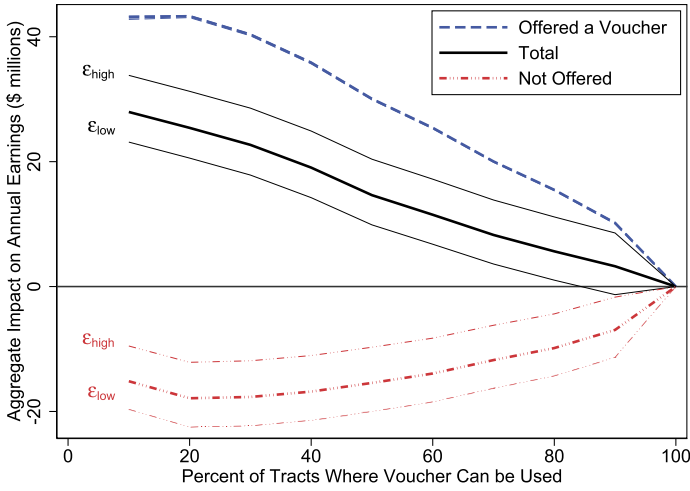
We consider two possibilities in our simulations. The first, which we discuss now, is that the Opportunity Atlas score for each tract does not change from the baseline. Later on, we allow each tract's Opportunity Atlas score to depend deterministically on the steady-state mix of types occupying the tract. Although we allow rents to endogenously adjust, for reasons we discuss later we assume that household preferences for amenities in all locations remains fixed in all simulations even when the type and racial composition, or the Opportunity Atlas score of the location, changes.

The top panel of Figure 7 shows how the various alternate voucher policies affect the aggregate average annual adult earnings of *all* children of renting households in Los Angeles in millions of dollars relative to current policy.⁴⁶ The thick lines show results

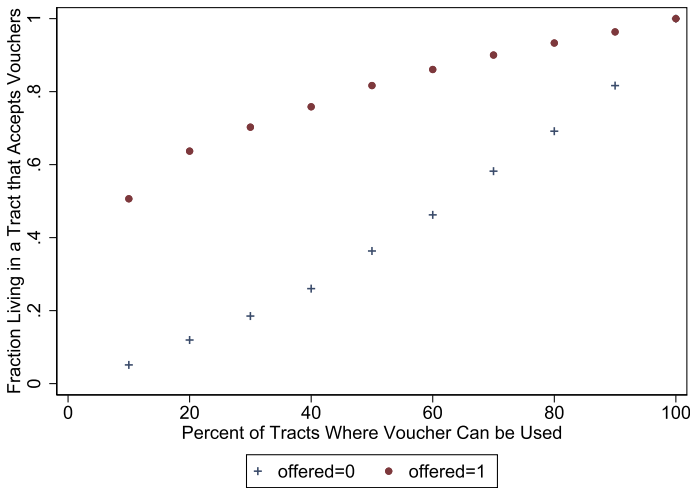
⁴⁴We are grateful to Nate Baum-Snow and Lu Han for sharing their estimates with us. We use their 2001 estimates of floorspace elasticity from the linear specification and the finite mixture model.

⁴⁵We experimented with a high value of \mathcal{E}_ℓ equal to 0.25 for all tracts, which is the 93rd percentile of tract-level supply elasticities for Los Angeles. These results are almost identical to our baseline.

⁴⁶For any given policy experiment, we know the cross-sectional steady state distribution of locations of households offered a voucher and households not offered a voucher. We use these distributions to compute the average Opportunity Atlas score of children of both sets of households. We then convert this averaged Opportunity Atlas score, which is a percentile of the age-26 income distribution, into an level of annual income.



(a) Aggregate impact, adult earnings of all children



(b) Impact on location choices, households offered voucher

FIGURE 7. Analysis of various voucher policies: Fixed Opportunity Atlas. *Notes:* For various experiments restricting where voucher recipients can live ($X = 10, 20, \dots, 100$, with $X = 10$ corresponding to the top 10% of Opportunity Atlas tracts, and so forth), the top panel of this figure shows the aggregate impact to annual adult earnings of children of households offered a voucher (top, dashed line), children of households not offered a voucher (bottom, dotted line), and all children of renting households in Los Angeles (solid line). The baseline results are for ϵ_ℓ from the Baum-Snow and Han (2020) data, the thicker lines. The results for ϵ_{low} and ϵ_{high} are for $\epsilon = 0$ and 0.5 for all ℓ , respectively. The bottom panel shows the frequency with which households offered a location-restricted voucher choose in the baseline to live in one of the acceptable locations (dots); the pluses show the frequency households not offered a voucher choose to live in one of the acceptable locations. In all experiments, each tract's Opportunity Atlas score is fixed.

evaluated at the baseline elasticity of supply and the thinner lines show sensitivity of results to this elasticity. The dashed line at the top of the figure shows the positive impact to adult earnings of children of households offered a voucher, relative to baseline; the dotted lines at the bottom shows the negative impact of the policy on children of households not offered a voucher; and the solid black line shows the net impact for all children. At $X = 100$, there are no impacts at all since this experiment replicates current policy. The policy that maximizes the aggregate earnings of all children in Los Angeles at the baseline supply elasticity is $X = 10$ which limits the voucher-eligible neighborhoods to the top 10% of all Opportunity Atlas neighborhoods. At this policy, the total net impact to adult annual earnings of children is \$28.0 million, about \$18 per year per child.⁴⁷ This net benefit reflects a positive benefit of \$43.1 million to all children of households offered a voucher and a loss of \$15.2 million to all children of households not offered a voucher. The policy that maximizes the benefit to only the children of households offered a voucher is $X = 20$. This policy yields an aggregate improvement in the adult earnings of children of households offered a voucher of \$43.3 million. There are no extra costs to the government from implementing this policy, as the number of housing-voucher offers to households is assumed to not change.

The top panel of this figure also shows how varying the elasticity of housing supply affects our results. When the housing supply elasticity is low, $\mathcal{E}_\ell = 0$ for all tracts ℓ , the relationship of total net benefits to percent of eligible falls; and when the housing supply elasticity is high, $\mathcal{E}_\ell = 0.5$ for all ℓ , this relationship increases. The intuition for these results is straightforward. The benefits of those receiving housing vouchers do not vary much with the assumed elasticity of supply; changes in tract-level rental prices are small relative to the large benefit of accepting a voucher. In contrast, the housing supply elasticity affects the location decisions of those not offered a housing voucher, the dash-dotted lines. As the elasticity falls, rental prices tend to rise in the Opportunity Atlas neighborhoods where vouchers can be used; this increase causes some households to live in other neighborhoods, presumably with lower average Opportunity Atlas scores. The migration of these households from high- to low-Opportunity Atlas neighborhoods reduces the aggregate benefits of the voucher.

The bottom panel of Figure 7 shows how various policies affect the location decisions of the 24 types of households eligible to be offered a voucher for the baseline housing-supply elasticity. The y -axis indicates the percentage of these households that choose to locate in a voucher-eligible neighborhood. The dots toward the top of the figure show the percentage for all households offered a voucher and the lower pluses show the percentage of households (given the same distribution over types) that are not offered a voucher. The gap between the dots and pluses illustrates the impact of the voucher on location choices. The figure illustrates that the policy experiments can dramatically change where households live. For example, at $X = 20$, the 24-types of households we study that are not offered a voucher live in voucher-eligible tracts only about 10% of the time, whereas the households that are offered the voucher live in these tracts more than 60% of the time—about a 50 percentage point increase.

⁴⁷The average number of children per renting household for the 1,634,030 renting households is 0.93.

TABLE 4. Policy experiments: results by type.

Demographics				Results at $X = 20$			Results at Pref. X		
Type	Race	w^τ	k^τ	Base.	Take-Up	Treated	Pref.	Take-Up	Treated
(1)	(2)	(3)	(4)	AE	Rate	Δ AE	X	Rate	Δ AE
				(5)	(6)	(7)	(8)	(9)	(10)
19	H	12.0	2.88	16.7	81	8.24	20	81	8.24
139	B	12.0	1.30	13.6	99	14.65	10	99	16.12
142	B	12.0	1.29	12.5	99	15.09	10	99	16.62
133	B	12.0	3.00	13.7	100	12.90	10	100	15.69
143	B	12.0	0.53	12.1	99	14.95	10	100	17.53
28	H	12.0	3.00	14.0	98	13.09	10	98	15.74
136	B	12.0	0.96	10.3	96	17.08	10	95	18.88
32	H	13.5	3.00	17.2	99	9.61	10	99	11.99
57	H	17.7	2.57	18.6	97	10.14	10	96	11.41
137	B	18.4	1.82	14.3	85	11.19	20	85	11.19
49	H	18.4	2.68	16.7	88	9.84	10	76	10.45
33	H	18.9	3.00	18.8	85	7.37	20	85	7.37
46	H	19.8	1.97	16.9	65	7.26	20	65	7.26
73	H	20.8	1.70	17.1	68	7.51	20	68	7.51
65	H	21.3	1.25	22.3	70	3.40	20	70	3.40
55	H	21.8	3.00	17.3	50	5.12	30	64	5.41
52	H	21.9	2.54	16.3	41	4.73	30	55	5.28
21	H	22.6	0.93	20.2	37	2.08	30	50	2.26
30	H	23.0	2.37	18.6	41	3.64	30	55	3.90
58	H	24.1	3.00	18.6	43	3.73	30	53	3.95
68	H	24.7	3.00	18.7	16	0.80	50	53	1.41
24	H	25.6	0.51	19.4	29	1.92	40	54	2.43
76	H	26.4	1.43	19.8	19	0.52	50	53	0.85
29	H	26.8	3.00	19.0	16	0.74	50	53	1.36
Average 1st 9 types				15.2	97	11.83		96	13.64
Average 2nd 6 types				17.5	77	8.06		75	8.19
Average 3rd 9 types				18.4	32	2.80		54	3.21
Average all types				17.0	67	7.38		75	8.23

Note: Column (1) is a type reference number, column (2) is assigned race (B = African-American and H = Hispanic), column (3) is estimated household income in \$000s and column (4) is estimated number of children. Column (5) is average adult earnings of children in the baseline ($X = 100$) simulations. Columns (6) and (7) refer to results from the simulation $X = 20$. Column (6) is the voucher take-up rate and (7) is the improvement in per-child adult earnings of households offered a voucher. Column (8) is the take-up rate at the type-specific value of X that maximizes per-child adult earnings of households offered the voucher, that is, the preferred X for that type. Columns (9) and (10) are analogous to columns (6) and (7).

At either $X = 10$ or $X = 20$, the policy creates enormous gains per child for the relatively few children of households offered a location-restriction voucher and fairly small per-child losses for the large number of children of households not offered a voucher. To give an illustration of the gains, Table 4 shows outcomes for each of the 24 types offered a voucher. The types are sorted by household income, column (3), and then by housing expenditure share (not shown). Column (5) shows average adult earnings of children in \$000s in the steady state of the baseline simulations, where households with vouchers

can live anywhere. Columns (6) and (7) of the table refer to results from the simulation that maximizes total adult earnings of all households offered a voucher, $X = 20$. Column (6) is the voucher take-up rate and (7) is the improvement in per-child adult earnings in \$000s of all households offered a voucher. Column (8) is the take-up rate at the type-specific value of X that maximizes per-child adult earnings of all households offered the voucher, that is, the preferred “Pref” X for that type, and columns (9) and (10) are analogous to columns (6) and (7).

For convenience, we have divided the table into three bands of types. The top band of 9 types with the lowest income almost always accepts a voucher. For these types, the average annual gain in adult earnings of children of households offered a voucher is an enormous \$11.83 thousand per child at $X = 20$, shown in column (7). This estimate includes experiences of children of households that do not accept the voucher. For reference, the average expected adult income of children of these households is \$15.2 thousand (column 5) implying the increase in income of \$11.83 thousand is equivalent to a 78% raise. At $X = 20$, the middle band of 6 types accepts a voucher with probability that ranges from 65 to 88%; for most of the types in this band, $X = 20$ is the value that maximizes adult earnings of children of households offered a voucher, shown in column (8). For these types, at $X = 20$, the average annual gain in adult earnings of children of households offered a voucher is \$8.06 thousand per child. The reduction in the benefit per-child relative to the 9 types of the top band reflects the lower take-up rate. Finally, at $X = 20$, the take-up rate of the bottom band of 9 types ranges from 16 to 50%. Since the take-up rate of these types is low, the impact of the voucher on adult earnings of children is only \$2.80 thousand per year. Shown in column (8), these types would all prefer a voucher with fewer location restrictions. Even at the preferred value of X for these types, the take-up rates are still relatively low at 54%, on average. Overall, the impact on adult earnings of children of households of the 24 types offered a voucher is maximized at $X = 20$ because the take-up rate is high and the benefits of take-up are large for the 15 types of households in the top two bands of Table 4.

One might wonder why the average voucher take-up rate is 67% in the $X = 20$ policy experiment when the simulated MTO take-up rate with the same type mix of households is only 26.2%. After all, when $X = 20$ households with vouchers can live in only one of 350 tracts, but in the MTO experiment households could choose to live in one of 508 tracts with a poverty rate less than 10%. The difference can be explained by the nature of the policy simulations. The take-up rate in the $X = 20$ policy simulation is computed based on the steady state. In contrast, we do not compute a steady state of the MTO policy experiment. Rather, any household that is offered a voucher must move to an eligible neighborhood in the first year and after that the household can live in any neighborhood. Had the MTO voucher been offered every year, presumably some households that refused the voucher in the first year may have eventually have taken it up, and given high moving costs, may have stayed in the neighborhood for quite some time.

Finally, it may seem surprising that there are any aggregate gains to adult earnings of children from a voucher policy. Consider an environment, different from ours, where (a) all households have one child, (b) each household consumes one unit of housing, (c) the housing supply is fixed in all tracts, and (d) no housing units are vacant. In this

example, any voucher policy that encourages people to move out of a “bad” neighborhood and into some other “good” neighborhood displaces existing residents out of the good neighborhood. The voucher policy yields a reshuffling of the population but since all housing units in all neighborhoods are always occupied, the aggregate impacts of the voucher policy are zero.⁴⁸

There are three reasons why vouchers in our framework may yield positive aggregate impacts to the adult earnings of children. First, some households do not have children and a voucher policy that replaces childless households with households with children in high Opportunity Atlas score neighborhoods will yield improvements to aggregate earnings of children. Second, households consume differing quantities of housing. Even if the housing stock is fixed, a voucher policy may wind up generating increases in adult earnings in the aggregate by swapping one relatively rich household with children in a high Opportunity Atlas score area for two relatively poor households also with children.

4.2 *Varying Opportunity Atlas*

A concern with the analysis of the previous section is that we hold the Opportunity Atlas scores fixed in every location while moving a possibly large number of households from one set of locations to a different set of locations. In other words, we assumed that the Opportunity Atlas score of a neighborhood does not depend on who lives in that neighborhood. In this section, we allow each neighborhood’s published Opportunity Atlas score to vary according to a simple function of the race and income of the residents of that neighborhood. Any voucher policy that changes neighborhood composition may also change the adult earnings of children of that neighborhood.

For each policy experiment, $X = 10, 20, \dots, 100$, we construct alternative Opportunity Atlas measures for each tract as follows. First, for each of the two published Opportunity Atlas scores we use in our analysis—the child’s expected percentile in the age-26 nationwide income distribution given household income of the 25th percentile and the 75th percentile of the nationwide income distribution—we regress the published score multiplied on average household income and the percentages of the neighborhood that are African-American and Hispanic.⁴⁹ We then use the regression coefficients to predict the Opportunity Atlas in each tract given the average household income and share of African-American and Hispanic households resulting from the steady state of the policy experiment. We next add the residuals from the regression.⁵⁰ Finally, one we have revised estimates of the Opportunity Atlas scores in hand for household income in the 25th and 75th percentiles in the income distribution, we use a linear interpolation procedure to impute an Opportunity Atlas score to any household given the income of that household.

⁴⁸This result also requires that the effects of neighborhoods on adult earnings of children are independent of neighborhood composition; we return to this in a moment.

⁴⁹The income and race regressors for each of the 1748 tracts are generated using data from our 144 household types. See Appendix D in the Online Supplementary of [Davis et al. \(2021\)](#) for details.

⁵⁰This ensures tract-level Opportunity Atlas scores change only when there is a change relative to the data of either household income or racial composition.

Before discussing our results, we wish to highlight important caveats. First, and obviously, correlation does not imply causation. Although racial shares and household income are highly correlated with the Opportunity Atlas scores, this does not imply that changing the racial composition or average income of a neighborhood will change the Opportunity Atlas score of that neighborhood. For example, high-income households may simply be more willing to pay higher rents that may be required to live in high Opportunity Atlas neighborhoods, thus inducing a correlation of the two series; this does not mean that moving lower-income households into a neighborhood will reduce the Opportunity Atlas score of that neighborhood.

Second, and equally importantly, we assume each type's unobserved amenities from living in a neighborhood, the $\ln A_\ell$ term in equation (9), stays constant even if the racial composition or the average income of the neighborhood changes. Households may care about fixed neighborhood amenities that may be correlated with racial composition, average household income, and even Opportunity Atlas scores in the baseline. But we assume households do not *directly care* about race or income of their neighbors or the Opportunity Atlas score. Related, if the racial or economic composition of a neighborhood changes we assume that the neighborhood amenities that households value do not change. This is a very strong assumption that we make for two reasons. First, in many models where households care about the composition of their neighbors, multiple equilibria may exist. It is unclear in our environment how to check for the presence of multiple equilibria and then how to select the appropriate equilibrium in the presence of multiplicity. Additionally, and related, households in these models need to form expectations about the composition of neighborhoods, and in rational-expectations equilibria these expectations must be consistent with outcomes, as documented by Davis, Gregory, and Hartley (2019). Computing an equilibrium is computationally very costly in our environment as it requires solving for expectation-consistent racial and economic composition of each of the 1748 tracts in Los Angeles, in addition to market-clearing rents in those tracts.⁵¹

The results of the policy experiments when Opportunity Atlas scores are allowed to vary are shown in Figure 8, which plots the aggregate improvement in adult earnings of children of households offered a voucher (top, dashed line), the aggregate loss of adult earnings of children of households not offered a voucher (bottom, dotted line), and the net aggregate gain (solid line). Restricting vouchers to be used in the top 10% of Opportunity Atlas tracts ($X = 10$) maximizes the total gain in adult earnings of children in this environment.⁵² At this policy, total net impact to adult annual earnings of children is \$33.1 million, which is *greater* than the equivalent estimate of \$28.0 million when we assumed Opportunity Atlas scores were invariant to policy. The net benefit of \$33.1 million can be decomposed into a positive benefit of \$39.2 million of all children of households offered a voucher and a loss of \$6.1 million of all children of households not offered

⁵¹Also, as discussed by Davis, Gregory, and Hartley (2019), we would need instrumental variables to estimate type-specific preferences for the Opportunity Atlas score of the neighborhood and demographic and economic composition of neighbors.

⁵²In the experiments in this section, tracts are restricted based on Opportunity Atlas scores in the baseline.

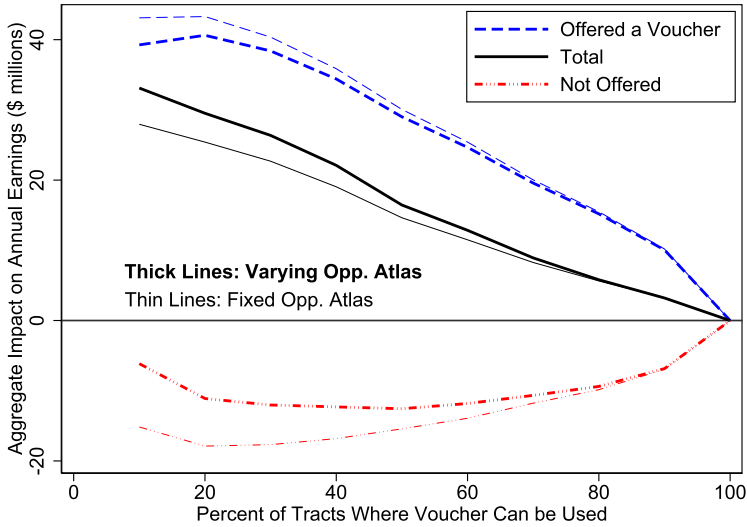


FIGURE 8. Analysis of various voucher policies: Varying Opportunity Atlas. *Notes:* For various experiments restricting where voucher recipients can live ($X = 10, 20, \dots, 100$, with $X = 10$ corresponding to the top 10% of Opportunity Atlas tracts, and so forth), this figure shows the aggregate impact to annual adult earnings of children of households offered a voucher (top, dashed line), children of households not offered a voucher (bottom, dotted line), and all children of renting households (solid line). Each tract's Opportunity Atlas score is allowed to vary depending on the income and racial composition of the tract. The thin lines copy results from the baseline housing supply elasticity in Figure 7.

a voucher. Relative to the analysis in which Opportunity Atlas scores are fixed, shown again for reference as the thin lines in Figure 8, the improvement in the net effect arises from the fact that the aggregate loss in annual earnings to children of households not offered the voucher shrinks from \$15.2 million to \$6.1 million. This occurs because the racial and economic composition of relatively low Opportunity Atlas score neighborhoods changes such that Opportunity Atlas scores in those neighborhoods improve.

5. CONCLUSION

We estimate household preferences over all Census tracts in Los Angeles using an infinite horizon, discrete-choice model that includes moving costs, and where households have preferences for consumption, housing, and location-specific amenities. We allow preferences to vary across the population of renting households by categorizing the population into 144 types. We estimate preferences separately for each type.

We then ask what would happen if Los Angeles were to convert its existing housing assistance program to one where all housing assistance is in the form of housing vouchers that can only be used in the top $X\%$ of Opportunity Atlas neighborhoods. We find that $X = 20$ maximizes the aggregate earnings of children of renting households offered location-restricted vouchers. The children of households accepting these vouchers experience substantial gains to annual income, but the children of households not offered

vouchers experience small losses on average, as some households move from locations that are most impactful on adult earnings to locations that are less impactful. On net, the gains far outweigh the losses and ultimately housing vouchers appear to have significant potential to improve intergenerational mobility.

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Co-editor Peter Arcidiacono handled this manuscript.

Manuscript received 19 June, 2020; final version accepted 25 February, 2021; available online 29 April, 2021.