Neighborhood Effects and Housing Vouchers*

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Abstract

We use a variety of methods and data sets to evaluate the costs and benefits of housing voucher programs intended to both subsidize housing rents and increase child test scores. We start by using new, tract-level panel data on child test scores to estimate the causal impact of location on child test scores for Census tracts in Los Angeles. We find a large dispersion of effects across tracts: 13 years of exposure to the top 16% of tracts in Los Angeles improve expected scores by nearly 0.5 standard deviations. We next solve and estimate a dynamic model of tract-level optimal location choices in Los Angeles for the purposes of understanding the sensitivity of location-choice decisions to housing vouchers. The estimated model fits the data along a number of dimensions and also (correctly) predicts that Moving-to-Opportunity (MTO) style housing vouchers fail to improve child test scores. We conclude by simulating the model under a variety of possible voucher programs. When vouchers are restricted such that they can only be applied to housing units in the top 5% of tracts based on tract impact on child test scores, we compute an "optimal" voucher amount of $300 per month where the benefits to child test scores net of voucher costs are maximized. We show that at $700 per month, voucher benefits are equal to costs.

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1 Introduction

In this paper we investigate how neighborhoods affect the test scores of children, how households optimally choose a neighborhood in which to live, and how housing vouchers affect neighborhood choices and child test scores. These topics have been studied individually before, but our approach is different and our data are new. We show that some neighborhoods can significantly improve child test scores, but parents differ in their willingness to rent in these neighborhoods. We use our framework to simulate the impact of various housing-voucher policies on test scores. The policies we consider have the feature that voucher amounts vary by neighborhood, and larger vouchers are assigned to neighborhoods more likely to positively impact test scores. We conclude by discussing the costs and benefits of a targeted housing voucher that can only be applied in a small set of neighborhoods that we find substantially improve scores. For Los Angeles, the area of our study, we compute a "surplus-maximizing" voucher amount for this policy of $300 per month. At this amount, the gap between the benefits of the voucher to test scores and the costs is maximized. We also compute a "break-even" voucher amount of $700 per month in which benefits are equal to costs.

Our paper has three main sections, and the first two reflect contributions to distinct literatures. In our first section, we estimate the impact of neighborhoods, in our case specific Census tracts in Los Angeles County, on child test scores. There is a large literature in the social sciences studying these "neighborhood effects" on test scores, adolescent behavior, health, labor earnings, and other individual level outcomes. Empirical studies using observational data often find strong associations between neighborhood quality, broadly defined, and positive individual-level outcomes: See Leventhal and Brooks-Gunn (2000), Durlauf (2004) and Ross (2011) for recent surveys. While these studies typically attempt to account for selection issues,[1] the fact that individuals endogenously sort into neighborhoods leaves open the possibility of non-causal explanations for these patterns.[2]

We make two contributions to this literature. First, we use a new longitudinal dataset in estimation, the Los Angeles Family and Neighborhood Survey (LA FANS). The LA FANS data allow for a substantially richer set of controls than are typically available in observational studies of neighborhood effects. Second, we estimate the impact of neighborhoods on test scores using a "value-added approach," in which changes in test scores over time, as measured

[1] For example, Cutler and Glaeser (1997) study the impact of segregation on outcomes of African-Americans using topographical features of cities as instruments for location choice and Aaronson (1998) measures neighborhood effects by studying outcomes of siblings at least three years apart in age after a move.

[2] See Aaronson (1998) for examples of instruments used by other researchers in this field and their potential limitations.

by changes in math test scores, are regressed on neighborhood fixed effects and a set of individual-level controls including, most importantly, lagged child test scores. The value-added approach has been applied widely in assessing teacher quality, for example Kane and Staiger (2008) and Chetty, Friedman, and Rockoff (2014), but has not yet been used in the neighborhood effects literature.

The key advantage of the value-added approach for our application is that the method recovers estimates of the effect of specific Census tracts on test scores, as compared to the average effect of neighborhoods associated with particular observable characteristics such as average income level and racial composition, the typical approach in the neighborhood-effects literature. We estimate economically important variation in neighborhood value-added across Census tracts in Los Angeles County: Our findings imply that 13 years of exposure to a Census tract providing value-added one standard deviation above the mean tract, on average, boosts the level of test scores in the cross-section by one-half of one standard deviation. In support of a causal, as opposed to selection-driven, interpretation of our neighborhood value-added estimates, we show that after we have controlled for children's lagged test scores and demographics, controlling additionally for variables such as parental ability, parental demographics, and household income and assets, which are strongly predictive of child test score levels in simple cross-sectional regressions, add very little in explanatory power for changes over time in child test scores.

In our second section, we specify and estimate a dynamic model of optimal location choice using detailed micro panel data, in the spirit of Kennan and Walker (2011) and Bayer, McMillan, Murphy, and Timmins (2015). We do this to understand the sensitivity of renting households in Los Angeles to location choice decisions to rental prices and, by extension, housing vouchers. We estimate the model using panel data from the Federal Reserve Bank of New York (FRBNY) Consumer Credit Panel / Equifax. This is a 5% random sample of U.S. adults with an active credit file and any individuals residing in the same household. To our knowledge we are the first to use these data to estimate a location choice model. We restrict our sample to renters residing in Los Angeles County. We study renters to mitigate the influence of availability of credit on location choice, and we focus on Los Angeles County to match our results with estimates of the impact of neighborhoods on test scores.

Our estimation sample from the FRBNY Consumer Credit Panel / Equifax data consists of more than 1.75 million person-year observations. This huge sample allows us to estimate a full vector of model parameters for many discrete "types" of people. Our use of many types in estimation captures permanent heterogeneity in preferences for neighborhoods, as compared to an estimation framework with fewer types which would necessarily attribute more systematic variation in neighborhood choices across households to period-by-period unobservable shocks. As a corollary, our use of many types in estimation helps us better identify how households adjust neighborhood choices in response to policy changes. We find that for many types of households, utility varies greatly across Census tracts; and, for many Census tracts, the utility of living in the tract varies widely across types.

In the final sections of the paper, we overlay the results of the previous two sections to study how various housing-voucher policies affect optimal location choices of households and child test scores. We begin the analysis by demonstrating that our model can replicate the results of the Moving-to-Opportunity (MTO) experiment. We focus on MTO because it was the largest experiment of its kind, designed to understand if housing vouchers could be designed that improve the health and livelihood of voucher recipients and their children by restricting recipients to live in one of a set of pre-designated neighborhoods. The MTO experiment was a randomized control trial beginning in the 1990s that explicitly attempted to alter child outcomes of voucher-recipient households by restricting the neighborhoods in which vouchers can be applied. Households with children eligible to live in low income housing projects in five U.S. cities were randomly assigned to one of three different groups. One group received no voucher, a second group received a voucher that could be used anywhere and the third group received a voucher that in the first year could be applied only in Census tracts with a poverty rate under 10% and could be applied anywhere after that. The embedded hypothesis of the MTO experiment was that moving to a low-poverty-rate Census tract would improve child outcomes. As shown by Sanbonmatsu, Kling, Duncan, and Brooks-Gunn (2006) and others, MTO failed to improve child test scores or educational attainment.[3] When we implement a voucher program in the model much like the voucher program of the MTO experiment, with similar voucher amounts and a restricted choice set imposed on voucher recipients, simulated child test scores fail to rise – just like the actual MTO results. Test scores failed to increase because the set of voucher-eligible neighborhoods in low-poverty areas included relatively low-rent, low-value-added neighborhoods and many households receiving a voucher chose to move to one of these neighborhoods.

In the final section of the paper, we analyze the impact of housing voucher policies that vary in the dollar amount of the voucher and/or the extent to which voucher amounts

[3] Recent work by Chetty, Hendren, and Katz (2015) shows that MTO positively affected adult wages for children who were young at the time their family received a voucher. Given the finding that MTO did not affect child test scores, presumably the effects on earnings operate through a different channel. A somewhat parallel set of findings emerges from from the literature examining the impact that moves induced by public housing demolitions had on children. Jacob (2004) finds that moving out of public housing had no impact on children's test scores in the short-run, which he attributes to the fact that they moved to neighborhoods that were similar to the neighborhoods that they left. However, Chyn (2016) finds long-run positive labor market outcomes for children that were induced to move by the demolitions.

target specific sets of high-value-added tracts. The results of this section highlight the benefits of our structural-estimation approach. Once we understand households' preferences for neighborhoods and disutility from rents, and given a mapping of neighborhoods to test scores and earnings, we can use simulations of the model to quantitatively evaluate the impact of any housing voucher policy on test scores and outcomes. Section 1 of our paper uses available data on household migration patterns, rents and characteristics of the housing stock to inform us as to the desirability of various neighborhoods and the sensitivity of household choices to rents and section 2 tells policy makers which neighborhoods should be targeted by vouchers.[4] We show that vouchers that more directly target or aggressively subsidize high-value-added tracts yield larger improvements to average child value-added and adult wages. We conclude the section by considering a voucher policy in which vouchers can only be used in the 5% of tracts with the highest child value-added. As mentioned earlier, the surplus-maximizing voucher amount of this policy is $300 per month and the break-even voucher amount is $700 per month.[5]

2 Neighborhood Effects

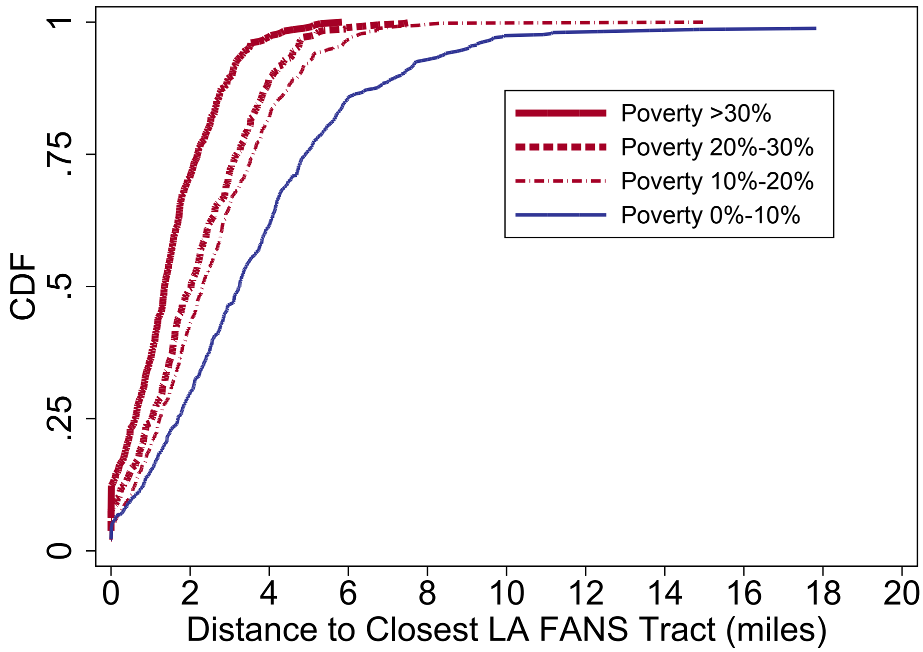
In this section, we estimate how each Census tract in Los Angeles affects expected child test scores. This information is vital for policymakers that wish to use housing vouchers to direct migration for the purposes of improving child test scores. Policy makers need to know which neighborhoods improve expected scores and which ones do not.

To uncover these estimates, we use confidential panel data from the Los Angeles Family and Neighborhoods Survey (LA FANS) to study how neighborhoods impact child test scores. The LA FANS study was designed specifically to investigate neighborhood influences on a variety of outcomes for families, adults, and children; see Pebley and Sastry (2011). The survey stratified 65 Census tracts using 1990 boundaries in Los Angeles County. Roughly 50 households in each Census tract were selected at random for inclusion in the survey. A randomly selected adult in the household was interviewed, as well as a randomly selected child. If the household had more than one child, a randomly selected sibling was also interviewed. Further, if the selected child's mother was in the household, she was interviewed as the primary caregiver. If she was absent, the actual primary caregiver was interviewed.

[4] In this sense, our paper is close to that of Galiani, Murphy, and Pantano (2015) who estimate a structural model of location choice using MTO data and run counterfactual experiments with their estimated model. Our paper is different in that we study the impact of MTO and other public policies on child well-being.

[5] Our findings are consistent with Aliprantis and Richter (2016) who find that the MTO program had positive labor market effects for the small subpopulation that was induced to move to a higher quality neighborhood.

Figure 1: Distance to Closest LA FANS Tract by Poverty Rate of Tract



The LA FANS data has the advantage of sampling by Census tract, so that we observe many households within a small geographic region. This is in contrast with other geo-coded panel datasets such as the Panel Survey of Income Dynamics or the National Longitudinal Study of Youth. The LA FANS oversamples poor neighborhoods, but the 65 Census tracts are distributed across much of Los Angeles.[6] Figure 1 shows the distance of each tract in our Los Angeles sample, as defined earlier, to a tract in the LA FANS sample. Most tracts in Los Angeles are located within a few miles of an LA FANS tract, but on average high-poverty Census tracts are closer to an LA FANS tract, reflective of the LA FANS sampling design. 3,085 households were interviewed between 2000 and 2002 (wave 1), of which 1,242 were re-interviewed between 2006 and 2008 (wave 2). New households were admitted into the LA FANS sample in the second wave. Detailed information on the housing status (rentership versus ownership), family characteristics and child outcomes were collected from respondents and Census tract information was collected in both waves.

We study the child's score on Woodcock Johnson tests as described in Schrank, McGrew, and Woodcock (2001) for applied problems ("math"), a test used in many MTO studies.[7] We

[6] We are unable to show the spatial distribution of the sampled tracts due to confidentiality restrictions.

[7] We have also studied results for passage comprehension. Our results are qualitatively and quantitatively very similar and as a result we do not discuss them in the paper.

restrict our sample to children who had valid measurements for both waves and we eliminate from our sample children with missing observations in some of our control variables.[8] This reduces our sample to 1,253, about 20 children per tract to estimate value-added. This is roughly the same sample size as studies of teacher value-added, i.e. one classroom of children. A major reason for a lack of skill measurement in both waves is the child's age. Only children under 18 were administered the Woodcock Johnson tests and thus only children who were under 18 in wave 2, i.e. aged 4 to 14 in wave 1 depending on the interview timing, are included.[9]

We compute neighborhood value-added using standard techniques in the education literature for computing teacher value-added. Following, Kane and Staiger (2008) and Chetty, Friedman, and Rockoff (2014) for example, we work with the statistical model for the production of the change in child test scores, $\Delta_{t-T}A_{i,j,t}$, between periods $t-T$ and t ,

$$\Delta_{t-T}A_{i,j,t} = Z'_{i,j,t-T}\psi + v_{i,j,t} \quad ; \qquad v_{i,j,t} = T\left[\mu_j + \epsilon_{i,j,t}\right] \ , \tag{1}$$

where i indexes children, j indexes neighborhoods, t indexes time, $Z_{i,j,t-T}$ is a vector of observable child and family characteristics measured at time $t-T$, μ_j is a causal (annualized) neighborhood "value-added" effect, $\epsilon_{i,j,t}$ is an idiosyncratic child/family effect and T is the number of years between LA FANS waves.[10]

Notice that in the absence of any control variables, μ_j would govern the average change in child test scores over time for children living in neighborhood j. Consistent with the value-added approach, splines of lagged values of a behavioral problems index as described in Peterson and Zill (1986) are included as controls. Our other controls include number of children, age, race, gender of child, parental IQ, parental education and income and assets, all measured as of wave 1. We present descriptive statistics in table 1.

The key insight to the value-added approach is that parents' optimal neighborhood choice does not have to be uncorrelated with the observable control variables, including lagged child test scores, to produce unbiased estimates of neighborhood effects on child test scores. Due to the presence of neighborhood fixed effects in equation (1), ψ is identified purely by within-neighborhood variation of $Z_{i,j,t-T}$ and $\Delta_{t-T}A_{i,j,t}$. Parents can select neighborhoods based

[8] We include all children, including those that change locations, in our estimation sample. Children that change locations between waves are assigned to the Census tract of their location in the first wave. We did not exclude movers from the sample for fear of sample selection. This choice was necessitated by the fact that LA FANS does not provide coverage for all Census tracts, including the tracts that are the destination of household moves in our sample. Our estimates can be interpreted as average annual value-added over a 5-year period for a given tract, conditional on starting the 5-year span in that tract.

[9] Additionally, new entrants to the survey would be disqualified since we only observe their test scores once.

[10] We include the T term when defining $v_{i,j,t}$ so that μ_j and $\epsilon_{i,j,t}$ are annualized.

Table 1: Descriptive Statistics, LA FANS

	Mean	S.D.	Obs.
Dependent Variables			
Change in math score	-0.009	1.034	1253
Control Variables (LA FANS Wave 1)			
Wave 1 Test Scores			
Math score	0.000	1.000	1253
Child Demographics			
Age of Child (years)	8.148	4.919	1253
Hispanic (1=yes)	0.570	0.495	1253
Black (1=yes)	0.126	0.332	1253
Male (1=yes)	0.520	0.500	1253
Parental Demographics and Education			
Number of kids	2.570	1.222	1253
Parental IQ	87.690	15.082	1253
High School dropout	0.272	0.445	1253
High School graduate	0.197	0.398	1253
Some college	0.307	0.461	1253
Bachelor degree	0.105	0.306	1253
Graduate degree	0.063	0.243	1253
*Parental Income and Assets ($000s)**			
Log income	3.799	1.174	1052
Log assets	2.404	2.005	1135

* Income and assets data are not always available for our estimation sample, explaining the smaller sample sizes for those variables.

Table 2: $R2$ Values from LA FANS data

65 tracts, 1,253 observations

Model	
1: Neighborhood Fixed Effects Only	0.09
2: + Splines in Lagged Child Scores	0.41
3: + Splines interacted w/ Child Controls	0.51
4: + Parent Ability and Demographics	0.52
5: + Lagged Income and Assets	0.52

on $Z_{i,j,t-T}$ and that will not bias estimates of ψ.[11] For an unbiased estimate of μ_j, the error term $\epsilon_{i,j,t}$ must be uncorrelated with $Z_{i,j,t-T}$. Parents can select neighborhoods based on the level of their child's test scores and/or other variables in $Z_{i,j,t-T}$, but not on the portion of expected growth of child test scores that is not forecasted by $Z_{i,j,t-T}$.

Table 2 summarizes our regression results of equation (1), showing model fit across a number of specifications. The outcome variable is the change in the standardized test score between LA FANS waves. When tract-level fixed effects are the only regressors, model 1, the $R2$ of the regression is just 9%. Once information on lagged child test scores is included as a regressor (model 2) the $R2$ jumps to 41%. Adding child controls (model 3) and parent demographics (model 4) increases the $R2$ to 52%. Adding information on parental income and assets (model 5) fails to further boost $R2$ values. Given the $R2$ value stays constant between models 4 and 5, we infer that for our results to be misleading, selection into neighborhoods based on $\epsilon_{i,j,t}$ must account for a significantly larger share of observed differences in change in average test scores across neighborhoods than selection into neighborhoods based on parental education, income and assets (Altonji, Elder, and Taber, 2005).

There are two issues we address before continuing. First, LA FANS only covers 65 tracts in Los Angeles but we require an estimate for all the 1,748 Census tracts in our sample. Second, following the teacher value-added literature (Chetty, Friedman, and Rockoff, 2014), we shrink the variance of the estimates of value-added arising from equation (1) to account for the fact that these estimates are derived from small samples and are noisy.

We perform the interpolation and shrinkage using a two-step process. To understand this process, let k (or k', as needed) denote an LA FANS Census tract. In the first step,

[11] Ioannides and Zanella (2008) estimate a model of location choice at the Census-tract level using panel data from the PSID and show that parents with young children are more likely to select neighborhoods with desirable observable characteristics used in the production of child human capital than other households.

we estimate equation (1) using the LA FANS data. Define $\hat{\mu}_k$ as the estimate of tract-k's annual fixed effect, $\widehat{\sigma}^2_\mu$ as the estimated variance of the tract-level fixed effects and $\widehat{\sigma}^2_\epsilon$ as the estimate of the variance of annual changes in child test scores after controlling for all Z variables and neighborhood effects arising from this first step. Now let j represent any tract in Los Angeles and define $\omega_{j,k}$ as a "weight" based on the physical distance between tracts j to k, a "distance" between tracts j and k in attribute space, and the number of observations in tract k, N_k. Specifically, define

$$\omega_{j,k} \quad = \quad N_k \times \phi\left(\frac{\|j-k\|_{distance}}{h_1}\right) \times \phi\left(\frac{\|j-k\|_{attributes}}{h_2}\right)$$

where h_1 and h_2 are bandwidths and $\phi\left(.\right)$ is the standard Normal density function. The term $\|j-k\|_{distance}$ is the physical distance (in miles) between the centroids of tracts j and k. The "distance" in attribute space $\|j-k\|_{attributes}$ is the difference between the value-added measures of j and k predicted by a regression of value-added on a host of observable tract characteristics.[12,13] We compute annual value-added for tract j as

$$\underbrace{\left(\frac{\sum\limits_{k} \omega_{j,k}\ \hat{\mu}_k}{\sum\limits_{k'} \omega_{j,k'}}\right)}_{\text{Interpolation}} \ \underbrace{\left(\frac{\widehat{\sigma}^2_\mu}{\widehat{\sigma}^2_\mu + \widehat{\sigma}^2_\epsilon / \widetilde{N}_j}\right)}_{\text{Shrinkage}} \tag{2}$$

where $\widetilde{N}_j$ is defined as

$$\frac{\left(\sum\limits_{k} \omega_{j,k}\right)^2}{\sum\limits_{k'} \left(\omega^2_{j,k'} / N_{k'}\right)} \tag{3}$$

The interpolation term in equation (2) is straightforward, as it is a simple weighted average. To understand the shrinkage term and why it is standard in the teacher value-added literature, consider a simplified model where Δa is the change in the next child's test score, μ is the true neighborhood effect and ϵ is a child-specific shock. Suppose that a noisy

[12] The list of explanatory variables includes tract poverty rate, median income, share receiving public assistance, crime rate, an index of transportation access, share Hispanic, and share black.

[13] We use $h_1 = 1.5$ miles, and we set h_2 to the standard deviation of the predicted value-added measures across tracts. A wide range of bandwidths (i.e. a range of relative weights placed on physical and attribute distance in the interpolation) yield nearly identical results, consistent with the high degree of spatial correlation in observable characteristics across tracts.

Table 3: Correlation of Value-Added Estimates by Tract

All 1,748 tracts after interpolation and shrinkage has occurred

Model	1	2	3	4	5
1	1.00				
2	0.75	1.00			
3	0.68	0.90	1.00		
4	0.52	0.80	0.94	1.00	
5	0.50	0.79	0.91	0.99	1.00
Ann. Std. Dev.	0.045	0.039	0.040	0.037	0.037

estimate of μ, call it μ^o, is observed

$$\begin{array}{lrcl} \text{Truth:} & \Delta a & = & 1 \cdot \mu + \epsilon \\ \text{Observed:} & \mu^o & = & \mu + \nu \end{array} \tag{4}$$

with ν being measurement error. A regression of Δa on μ^o will yield a biased coefficient of $\sigma_\mu^2 / \left(\sigma_\mu^2 + \sigma_\nu^2\right)$. Dividing estimates of μ^o by this expression will produce an unbiased regression coefficient of 1. In mapping the intuition of equation (4) to what we actually do, note that the variance of ν – the variance of the measurement error – will be a function of the sample size in the LA FANS data. The reason is that we estimate value-added as a fixed effect, which is a sample average. The greater the number of observations in each tract, the more precisely we estimate neighborhood value-added and the smaller the variance of ν. This explains the presence of the $\widetilde{N}_j$ term in equation (2). The fact that we use a weighted average of all LA FANS tracts in estimating value-added for any given Census tract leads to the functional form for sample size of equation (3).

Table 3 shows tract-level correlations of value-added estimates for the five different models discussed in table 2 after interpolation and shrinkage have occurred for all 1,748 Census tracts in our study.[14] This table reinforces the result that once lagged child controls are included as regressors (model 2), estimates of tract value-added from models that include more controls are very similar (models 3-5), as the correlations are 0.79 and above. The bottom rows report the estimated standard deviation of tract-level child value-added. In model 4, the specification we use in our counterfactual simulations later in the paper, the standard deviation of tract-level child value-added is 0.037. Note that the unconditional

[14] The results are very similar when we restrict the analysis to only the tracts with LA FANS data but still apply interpolation and shrinkage.

standard deviation of the level of the Woodcock-Johnson score is 1.0. Assuming linearly additive effects of neighborhood value-added over time, 10 years of exposure to a Census tract with a level of child value-added that is one standard deviation above the mean will cause a child's Woodcock-Johnson test scores to increase 37% of one standard deviation.

Table 4 shows regressions of our value-added estimates on measures of local public school quality, tract poverty rates and tract-level racial percentages. We use a bootstrapping procedure to compute the standard errors shown in the table.[15] The estimates of local school quality are estimates of math value-added of the nearest elementary school as produced by the Los Angeles Times.[16] The regressions show that our estimates of value-added are not simple transformations of race, poverty or public-school quality. There is considerable variation in value-added even after controlling for public school quality, tract level poverty rates and racial percentages, as the $R2$ of the regressions is only 14%.

Upon further review, a case can be made that our estimates of tract value-added are capturing an aspect of the neighborhood that is distinct from available estimates of public-school quality. Figure 2 plots our estimates of the average level of tract-level value-added by poverty rate in the top panel and the average level of public school quality as measured by the Los Angeles Times, also by poverty rate, in the bottom panel. There is considerable variation around the tract-level averages shown in figure 2 (not shown),[17] but on average our estimates of value-added decline with tract poverty rates. In contrast, the Los Angeles Times' estimates of school quality increase with tract poverty.

3 Location Choice Model and Estimates

3.1 Model

We wish to understand the value that different households receive when they live in different neighborhoods. To do this, we solve and estimate an optimal forward-looking location-choice model. The basic intuition of estimation is as follows: If we notice certain types of households moving to certain clusters of neighborhoods more frequently than others,

[15]To compute bootstrap standard errors, we draw 1,000 LA FANS samples and for each LA FANS sample we draw 1,000 samples of 1,748 Census tracts. This gives us 1 million draws in total. In each LA FANS sample, we draw from all the 65 LA FANS tracts. The number of children drawn in each tract is fixed and equal to the LA FANS sample size. The LA FANS and Census tracts samples are both drawn with replacement.

[16]See *http://projects.latimes.com/value-added/* for details on how school value-added measures are computed. We assign the elementary school that is closest in distance to the centroid of the Census Tract.

[17]Table 4 shows that the $R2$ of a regression of value-added on a set of covariates including tract poverty rates is only 14%.

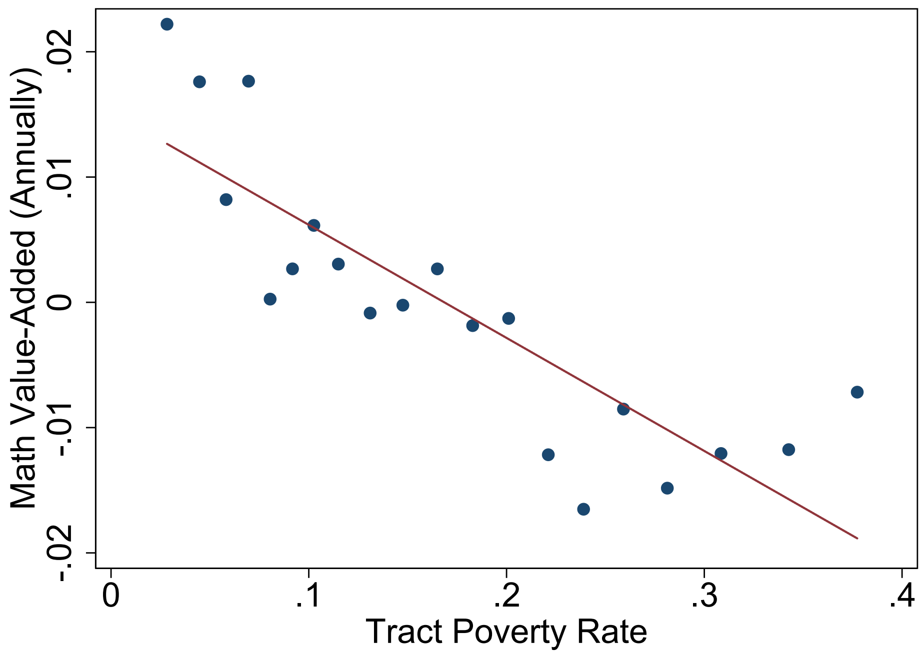
Table 4: Neighborhood Traits and Value-Added

Regr. of Value-Added Estimates on Neighborhood Covariates, 1,748 Tracts
(Bootstrap Standard Errors in Parentheses)

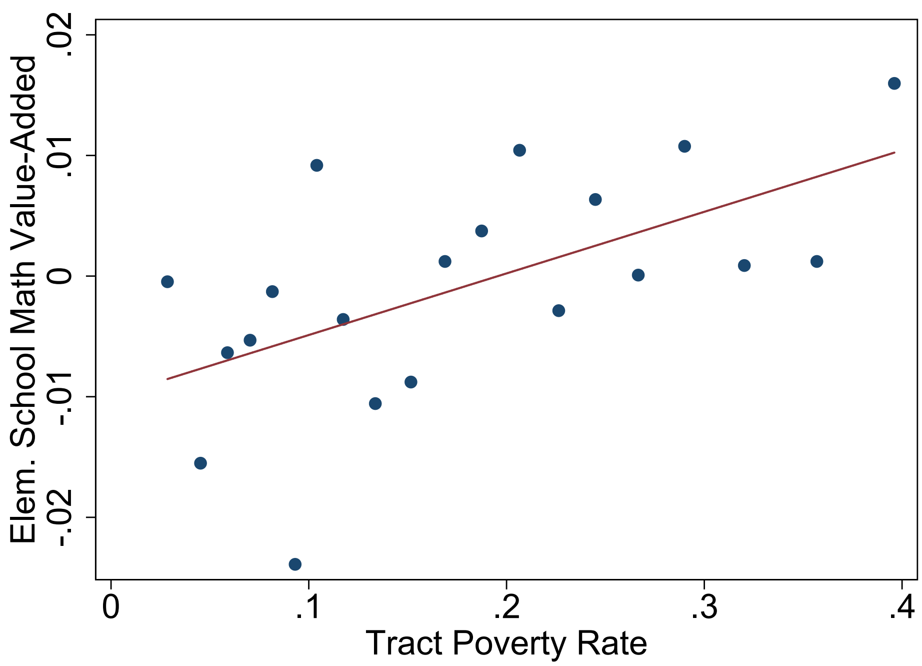
Variable	
Math School VA+	0.025
	(0.045)
English School VA+	0.064
	(0.071)
Poverty Rate	-0.003
	(0.051)
Pct. Hispanic	-0.063***
	(0.024)
Pct. Black	-0.017
	(0.025)
Pct. Hispanic x Poverty Rate	0.046
	(0.066)
Pct. Black x Poverty Rate	0.069
	(0.106)
Constant	0.032
	(0.010)
heightR2	0.141

+ LA Times Measure of Local Public Elementary School Value Add
*** Significant at a 1% confidence level

Figure 2: Tract Poverty, Value-Added and School Quality



(a) Avg. Tract Value-Added against Poverty Rates



(b) Avg. School Quality (Math) against Poverty Rates then, on average, those neighborhoods must provide higher levels of net utility (i.e. direct utility inclusive of all benefits less any costs, financial or otherwise). 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 as in Kennan and Walker (2011) and Bayer, McMillan, Murphy, and Timmins (2015). 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 family can choose to live in one of J locations. Denote j as the family's current location. We write the value to the family of moving to location ℓ given a current location of j and current value of a shock ϵ_ℓ (to be explained later) as

$$V(\ell \mid j, \epsilon_\ell) \;=\; u(\ell \mid j, \epsilon_\ell) + \beta EV(\ell)$$

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. We assume the household problem does not change over time, 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

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

δ_ℓ is the flow utility the household receives this period from living in neighborhood ℓ, net of rents and other costs; $\kappa_{\ell j} = [\kappa_0 + \kappa_1 * \mathcal{D}_{\ell j}] \cdot 1_{\ell \neq j}$ 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, i.e. 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 iid across locations, time and people as in Rust (1987). 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. Note that δ_ℓ is the type-specific indirect utility of living in neighborhood ℓ, and this utility may depend on attributes such as amenities, crime, school quality, pollution, access to public transportation and possibly child value-added, a point to which we return later.

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

$$V(j \mid \epsilon_1, \epsilon_2, \ldots, \epsilon_J) = \max_{\ell \in 1, \ldots, J} V(\ell \mid j, \epsilon_\ell) \tag{5}$$

$EV(j)$ is the expected value of (5), where the expectation is taken with respect to the vector of ϵ.

While this model looks simplistic, it is the workhorse model used to study location choice. Differences in models reflect specific areas of study and availability of data. For example, in their study of migration across states, Kennan and Walker (2011) replace δ with wages after adjusting for cost of living. Bishop and Murphy (2011) and Bayer, McMillan, Murphy, and Timmins (2015) specify δ as a linear function of spatially-varying amenities with the aim of recovering individuals' willingness to pay for those amenities. We allow the δ's to vary flexibly across neighborhoods, with the aim of realistically forecasting the substitution patterns that are likely to occur in response to government policies that change the relative prices of neighborhoods.

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

$$EV(j) = \log\left\{\sum_{\ell=1}^{J} \exp \widetilde{V}(\ell \mid j)\right\} + \zeta \tag{6}$$

where ζ is equal to Euler's constant,

$$\widetilde{V}(\ell \mid j) = \delta_\ell - \kappa_{\ell j} + \beta EV(\ell) \tag{7}$$

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 $p(\ell \mid j)$, has the solution

$$p(\ell \mid j) = \widetilde{V}(\ell \mid j) - \log\left\{\sum_{\ell'=1}^{J} \exp\left[\widetilde{V}(\ell' \mid j)\right]\right\} \tag{8}$$

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

$$p(\ell \mid j) = \widetilde{V}(\ell \mid j) - \widetilde{V}(k \mid j) - \log\left\{\sum_{\ell'=1}^{J} \exp\left[\widetilde{V}(\ell' \mid j) - \widetilde{V}(k \mid j)\right]\right\} \quad (9)$$

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 (9) 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 (6) and (7). 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 (7) implies

$$\widetilde{V}(\ell \mid j) - \widetilde{V}(k \mid j) = \delta_\ell - \delta_k - [\kappa_{\ell j} - \kappa_{kj}] + \beta\left[EV(\ell) - EV(k)\right] \quad (10)$$

But from equation (6),

$$EV(\ell) - EV(k) = \log\left\{\sum_{\ell'=1}^{J} \exp \widetilde{V}(\ell' \mid l)\right\} - \log\left\{\sum_{\ell'=1}^{J} \exp \widetilde{V}(\ell' \mid k)\right\}$$

Now note that equation (8) implies

$$\begin{aligned} p(k \mid \ell) &= \widetilde{V}(k \mid \ell) - \log\left\{\sum_{\ell'=1}^{J} \exp\left[\widetilde{V}(\ell' \mid \ell)\right]\right\} \\ p(k \mid k) &= \widetilde{V}(k \mid k) - \log\left\{\sum_{\ell'=1}^{K} \exp\left[\widetilde{V}(\ell' \mid k)\right]\right\} \end{aligned}$$

and thus

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

is equal to

$$\begin{aligned} & \widetilde{V}(k \mid \ell) - \widetilde{V}(k \mid k) && - && [p(k \mid \ell) - p(k \mid k)] \\ = & \qquad -\kappa_{k\ell} && - && [p(k \mid \ell) - p(k \mid k)] \end{aligned}$$

The last line is quickly derived from equation (7). Therefore,

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

and equation (10) has the expression

$$\begin{aligned} & \widetilde{V}(\ell \mid j) - \widetilde{V}(k \mid j) \\ & = \delta_\ell - \delta_k \;-\; [\kappa_{\ell j} - \kappa_{kj}] \;-\; \beta\,[p(k \mid \ell) - p(k \mid k) + \kappa_{k\ell}] \end{aligned} \tag{11}$$

Combined, equations (9) and (11) show that the log probabilities that choices are observed are simple functions of model parameters $\delta_1, \ldots, \delta_J$, 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 depend on the neighborhood of residence in the current period (net of moving costs). This allows us to estimate the model with a short panel, an insight from Arcidiacono and Miller (2011).

3.2 Data and Likelihood

We estimate the model using panel data from the FRBNY Consumer Credit Panel / Equifax. 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.[18] 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 Score^{TM} which provides some information on the financial wherewithal of the household as

[18] The 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.

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 FRBNY Consumer Credit Panel / Equifax data to study the relationship of interest rates, house prices and credit (see Bhutta and Keys (2015) and Brown, Stein, and Zafar (2013)) and the impact of natural disasters on household finances (Gallagher and Hartley, 2017), but we are the first to use this data to estimate an optimal location-choice model.

We restrict our sample to individuals who, from 1999 through 2013, are never observed outside of Los Angeles County and who never hold a home mortgage, yielding 1,787,558 person-year observations. We study renters to mitigate any problems of changing credit conditions and availability of mortgages during the sample window; and we study Los Angeles in particular to link our estimates of utility to measures of neighborhood effects on child outcomes we estimate for each Census tract in Los Angeles (to be discussed later).[19] 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.[20] The panel is not balanced, as some individuals' credit records first become active after 1999.

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. Previous studies of neighborhood choice such as Bayer, McMillan, Murphy, and Timmins (2015) have had access to much smaller data sets and as a result have had to restrict variation in model parameters across the population.

Table 5 compares sample statistics from the FRBNY Consumer Credit Panel / Equifax data to Census data for the tracts in Los Angeles County. This table includes data for both owners and renters. Column (2) shows the implied total population of adults ages 18-64 in the FRBNY Consumer Credit Panel / Equifax data, computed as twenty times the total number of primary individuals, and (3) shows the average population counts of adults from the 2000 and 2010 Census. The table shows that coverage in the low poverty tracts is very high, above 90%. Coverage remains high but falls for the higher-poverty tracts, either because many individuals lack credit history or do not have a social security number. Columns (5) and (6) compare the percentage of households with a mortgage in the two data sets. Not surprisingly, the percentages fall quite dramatically with the poverty rate, and generally speaking the percentages reported in the two data sets are close. The final row

[19] In the FRBNY Consumer Credit Panel / Equifax data, renters and homeowners without a mortgage are observationally equivalent. According to data from the 2000 Census, 85% percent of the units without a home mortgage are renter-occupied for the 1,748 Census tracts of our study.

[20] On average, each Census tract in Los Angeles has about 4,000 people.

Table 5: Comparison of Equifax and Census Data

Poverty Rate (%)	Avg. Population 2000-2010 Equifax[a]	Census[b]	Equifax Share	Pct. w/ Mortgage 2008-2012 Equifax[c]	ACS[d]
(1)	(2)	(3)	(4)	(5)	(6)
0-5	610,336	654,004	93.3%	61.6%	62.6%
5-10	1,395,831	1,478,114	94.4%	50.0%	50.2%
10-15	1,033,076	1,135,194	91.0%	40.5%	39.2%
15-20	751,098	870,869	86.2%	37.3%	34.9%
20-25	630,830	761,841	82.8%	30.7%	26.9%
>25	1,085,466	1,497,545	72.5%	23.9%	19.0%
Public Housing[e]	34,988	42,431	82.5%	27.0%	23.9%

Notes:
a Data are computed as 20 times the average (1999-2014) number of Equifax primary individuals ages 18-64.
b Data shown are the average (2000 and 2010) of the Census tract population ages 18-64.
c Data are the average share of households in Equifax with a mortgage, 2008-2012.
d Data are the average share of households in the American Community Survey tract-level tabulations with a mortgage, 2008-2012.
e Data shown are for 13 tracts with 250+ non-senior public housing units and above 10% poverty rate in 2000.

of table 5 compares the FRBNY Consumer Credit Panel / Equifax and Census data on 13 tracts with 250+ non-senior public housing units, the residents of which will be the focus of our counterfactual policy experiments.[21] That row shows the two data sets closely align for these tracts.[22]

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

[21] We determine the 13 tracts by using latitude and longitude data from the HUD Picture of Subsidized Housing Data for 2000 for the public housing developments with 250 or more non-senior units. We eliminate any of these developments located in a tract with a poverty rate below 10%.

[22] For these 13 tracts, we check that the proportion of the population with a mortgage and the number of residents aged 18-64 in the FRBNY Consumer Credit Panel / Equifax data align with that of the Census by regressing the Census data on the Equifax data. The point estimates are 1.05 (standard error 0.09) for mortgages and 0.78 (0.11) for population.

bins),[23] 5 age categories (cutoffs at 30, 45, 55, and 65),[24] 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. After all the dust settles, this procedure yields 144 types of households.

The benefit of working with a data set like the FRBNY Consumer Credit Panel / Equifax data is that its size allows estimates of the substitutability of neighborhoods, i.e. the vector of δ_j, to vary based on a rich set of observables, explaining why we use so many types.[25] The following figures from our data are instructive. Figure 3 shows the typical location choices made by type 133 in our sample: A 2-adult household with an Equifax Risk Score^{TM} below 580 and first observed living in a Census block that is predominantly black. The light blue areas show all Census tracts with poverty rates less than 10% and the tan areas show all Census tracts with higher poverty rates. The areas in dark blue show the most chosen low-poverty Census tracts for this type and the areas in black show the most chosen high-poverty tracts. Figure 3 shows this type predominantly clusters its location choices in one crescent-shaped area in the south-central part of the county. Figure 4 shows the same set of location choices for type 20 in our sample, a 2-adult household with a 590-656 Equifax Risk Score^{TM} first observed in a predominantly Hispanic Census block. Comparing figures 3 to 4, few of the most popular neighborhood choices overlap of these 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 experienced.

Our sample is comprised of 1,748 Census tracts. Allowing a separate value of δ for each tract and for each type would require estimating more than 250,000 parameters. Conceptually, with a large enough sample we could separately estimate every δ by type. Currently, for each type of household in our sample, we have data on approximately 2,000 households

[23] We assign race based on the racial plurality of all persons in the Census block, owners and renters, when they are first observed, which in most cases is 1999. The mean number of households and residents at the Census-block level in our sample of 1,748 tracts is 41 and 118, respectively, and Census blocks are highly homogenous by race and by tenure choice. Of the Census blocks in our sample that are at least 5% Hispanic, 26% are 75% or more Hispanic. The equivalent statistic for whites is 27%, for African Americans 9%, for Asians 2%. Similarly, of the Census blocks in our sample tracts that are at least 5% renter occupied, 25% are 75% or more renter occupied.

[24] Whenever we refer to a household "age" in the FRBNY Consumer Credit Panel / Equifax data, we are referring to the age of the person in the household in the initial random sample. We are not using the ages of any other people in the household.

[25] We experimented estimating the model using a random coefficients approach instead of a type-based approach. The computation burden was larger than our type-based approach, for what appeared to be relatively little added flexibility in the demand system.

Figure 3: Location Choices of 2-adult black households w/ <580 Equifax Risk Score^{TM}

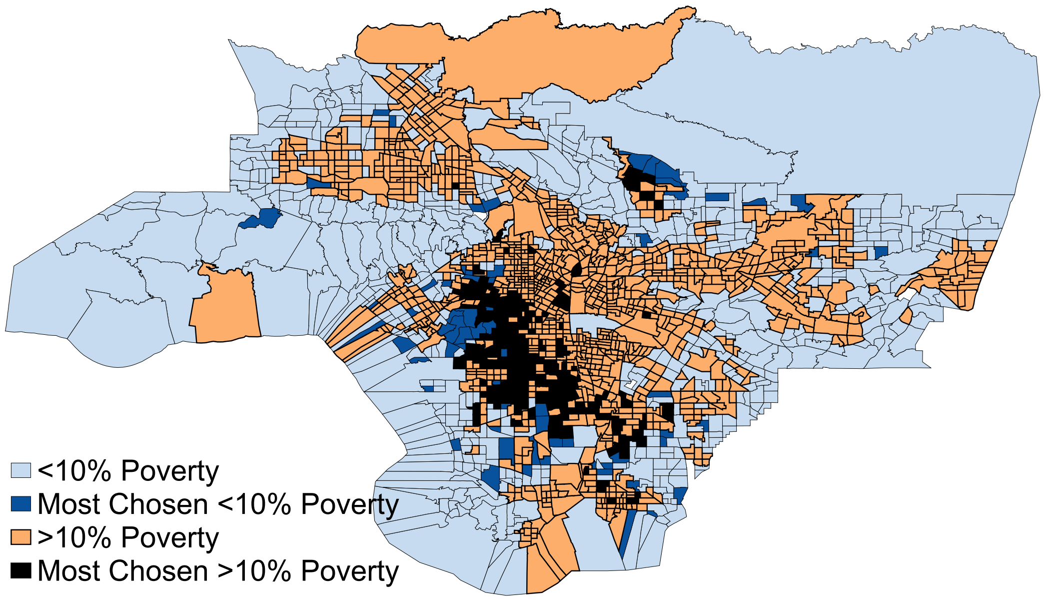
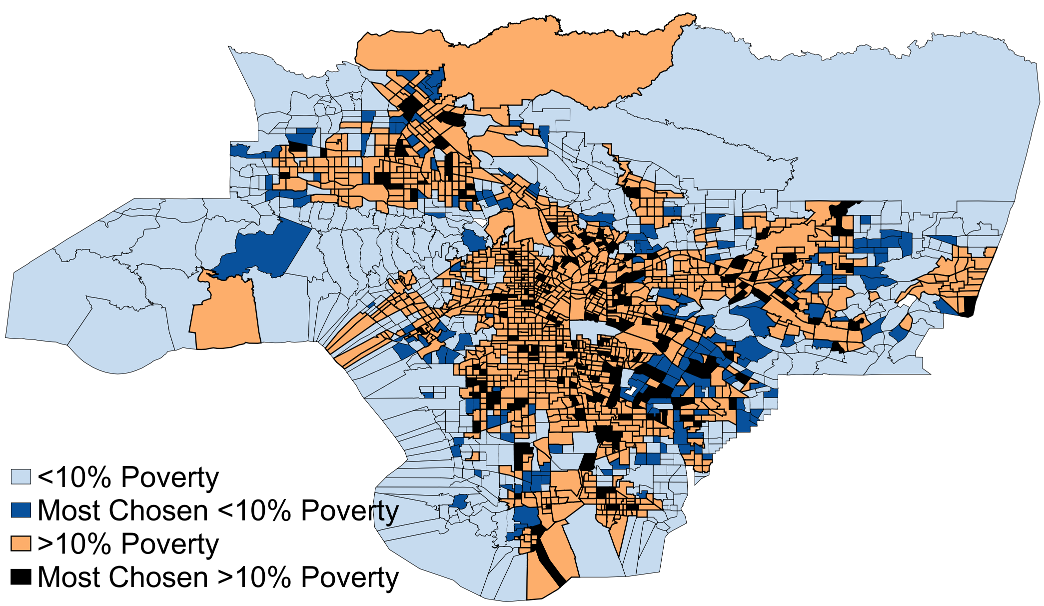


Figure 4: Location Choices of 2-adult Hispanic households w/ 590-656 Equifax Risk Score^{TM}



followed over 10 years. Therefore, 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

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

The B_k are parameter-less 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 10% poverty threshold. Inclusive of the two moving cost parameters, we estimate $2 \times 89 + 2 = 180$ parameters per type. With 144 types, we estimate a total of $25,920$ parameters.

To define the log likelihood that we maximize we need to introduce some 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

$$\sum_{\tau} \sum_{i \in \tau} \sum_{t} \quad p\left(\ell_{it} \mid j_{it}; \theta_\tau\right) \tag{12}$$

$p\left(.\right)$ 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.

3.3 Estimates and Model Fit

Our estimation 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.[26] Figures 5 and 6 show the surface of indirect utilities across Los Angeles County that we estimate for types 133 and 20, respectively, such that the model can replicate as best as possible the location choices shown in figures 3 and 4. These figures illustrate the flexibility of our specification. These surfaces are quite different, reflecting the very different optimal location choices of these types.

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. About 8.5 percent of our sample moves to a different tract in each year, and that percentage falls from just above 11 percent for those under 30 to just above 3 percent for those aged 65 and above. Our estimated model nearly perfectly matches these statistics. Figure 7 compares the distribution of distances moved (measured

[26] We fix $\beta = 0.95$.

Figure 5: Indirect Utility, Type 133

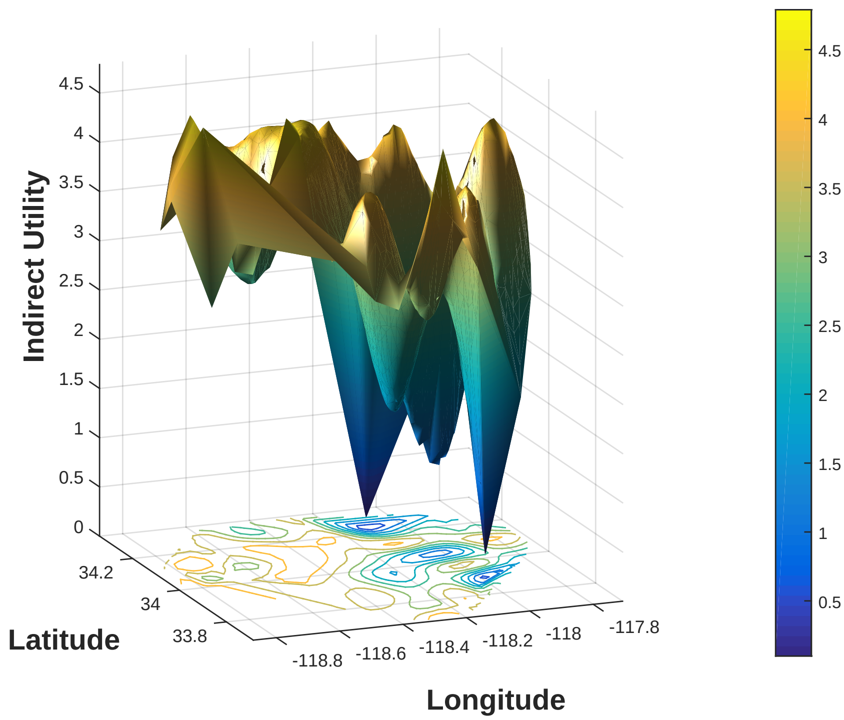


Figure 6: Indirect Utility, Type 20

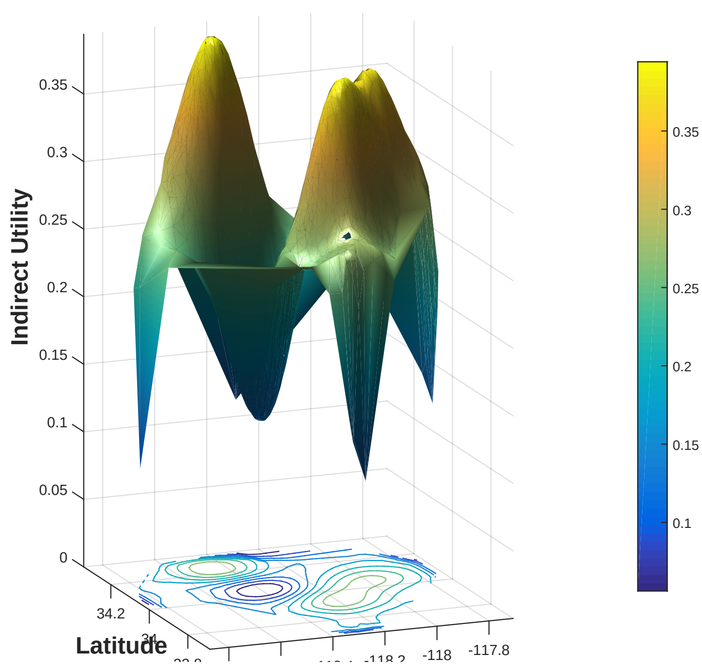
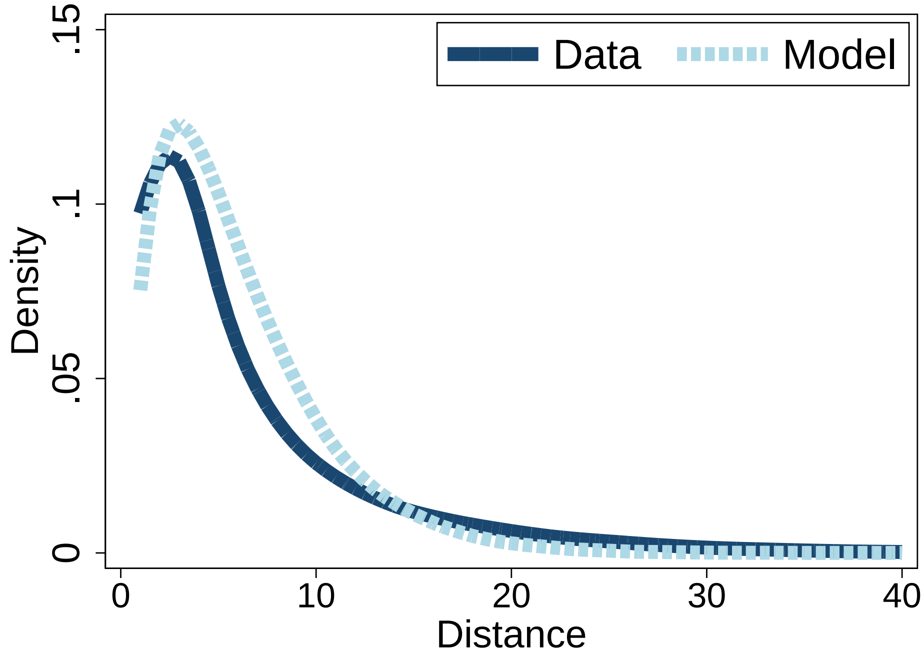


Figure 7: Model Fit: Density of Moving Distance



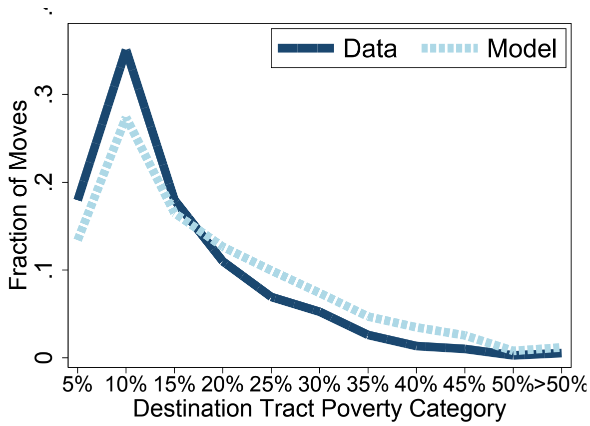
as the straight line distance between tract centroids) for all movers in the data and as predicted by our model.[27] This figure shows that the model replicates the hump-shaped distribution of distances moved, with the most frequent moves about 4 miles. The model slightly overpredicts moves between 4 and 10 miles in length and slightly underpredicts moves less than 4 miles.

Figure 8 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 for the data (dark blue solid line) and as predicted by the model (light blue dotted line). Figure 8 shows model fit for very low-probability moves.[28] The model tends to under-predict 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 along this dimension.

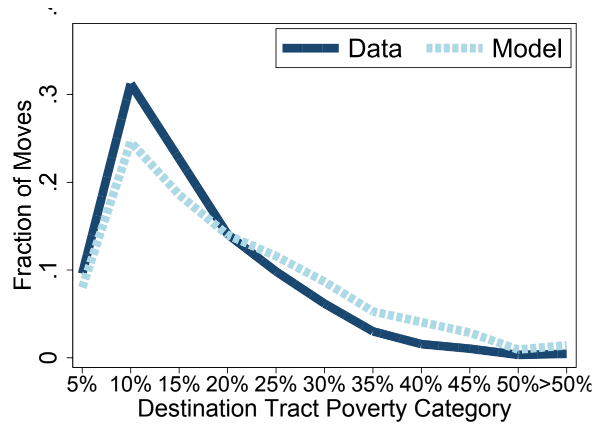
[27] In the data we know the Census block of residence for each household. We eliminate any within-tract moves and for the remaining moves, we define distance moved as the distance between tract centroids of the sending and receiving tracts.

[28] Recall the unconditional probability of any move is less than ten percent.

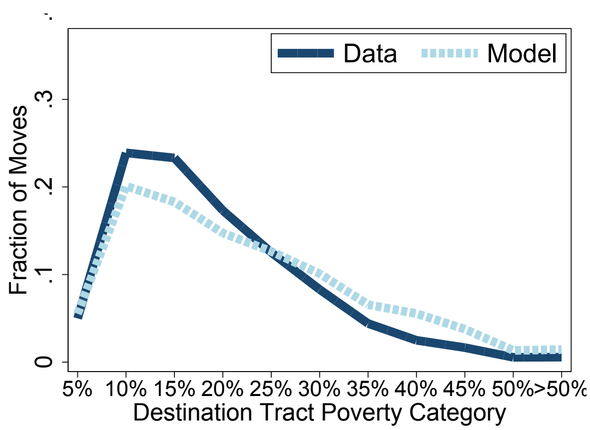
Figure 8: Poverty Category Transitions $t-1$ to t, Conditional on Moving



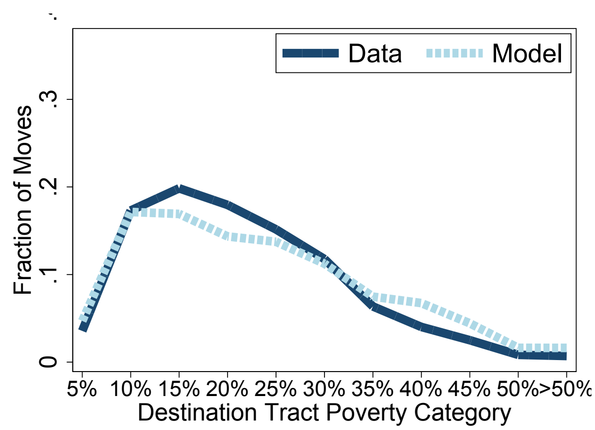
(a) From 0-5%



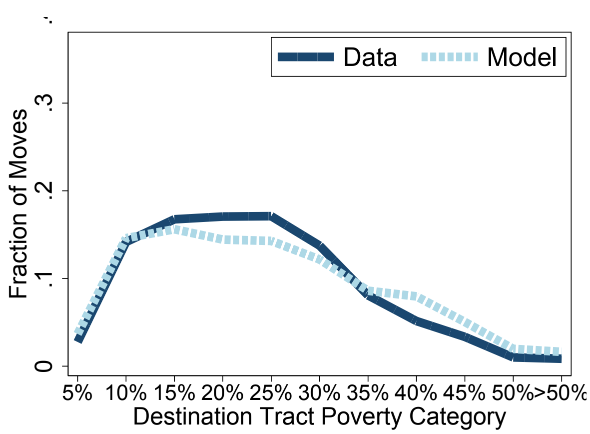
(b) From 5-10%



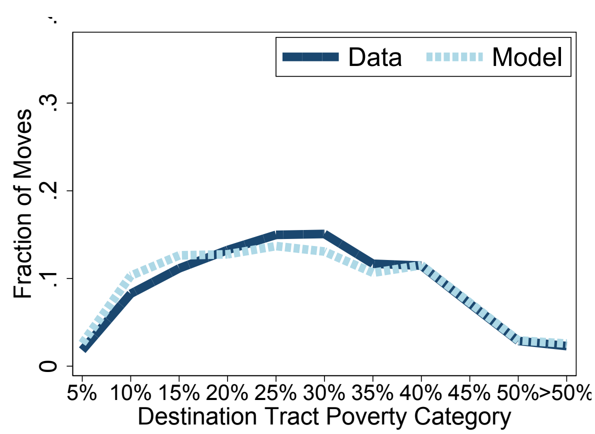
(c) From 10-15%



(d) From 15-20%



(e) From 20-25%



(f) From >25%

3.4 Type-Specific Sensitivity to Rent

Finally, to understand how vouchers might affect optimal location choice, we need to understand how utility of each neighborhood varies with rents paid to live in that neighborhood. Denote as $\tilde{\delta}_{j\tau}$ our estimate of indirect utility of neighborhood j for a given type τ. To make progress, we specify that $\tilde{\delta}_{j\tau}$ is a linear function of rent, observable characteristics of tract j, $\mathcal{O}_j$, and unobserved characteristics of tract j, $\xi_{j\tau}$

$$\tilde{\delta}_{j\tau} \;=\; -\alpha_\tau \cdot rent_j \;+\; \lambda_\tau \cdot \mathcal{O}_j \;+\; \xi_{j\tau}$$

The parameter α, the rate at which indirect utility varies with rents, cannot be estimated using OLS because equilibrium rents will almost certainly be correlated with unobserved but valued characteristics of neighborhoods, $\xi_{j\tau}$. An instrument is required. We use a three-step IV approach to estimate α that is common in the IO and Urban literature, for example Bayer, Ferreira, and McMillan (2007).

In the first step of our procedure, we estimate α_τ 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.[29] The first-stage F-statistic is 7.

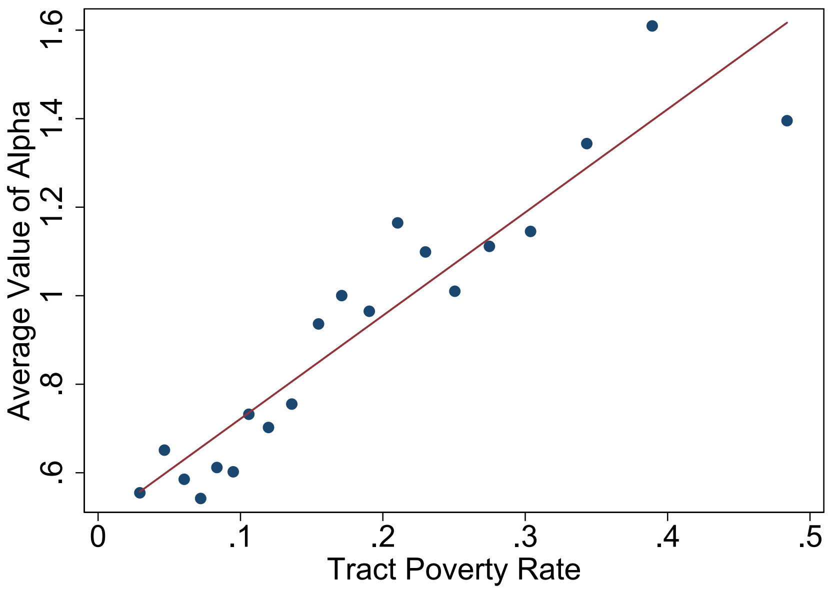
In the second step, we use estimates of α and λ from the first step, call them $\widehat{\alpha}_\tau$ and $\widehat{\lambda}_\tau$, to construct a new surface of indirect utilities for each type abstracting from unobservables as

$$\widehat{\delta}_{j\tau} \;=\; -\widehat{\alpha}_\tau \cdot rent_j \;+\; \widehat{\lambda}_\tau \cdot \mathcal{O}_j$$

We simulate the model using this specification for indirect utility and adjust $rent_j$ for all j until the simulated steady-state number of households in any tract is equal to the average number of households in our estimation sample in that tract. This procedure determines market-clearing rents in all tracts in the absence of unobserved amenities. We use these rents as instruments to estimate alpha in the third and final step with an F-statistic of 34. Intuitively, the F-statistic rises from 7 to 34 because the first step only uses information about the quality of substitutes for each tract individually whereas the third step uses similar

[29] The intuition for the validity of these instruments arises directly from the Rosen-Roback model. Consider two pairs of tracts, (A, B) and (A', B'), with A and A' providing identical direct utility and the housing stock in B' of higher quality than the housing stock in B. Assume one set of households chooses between A and B and a different set of households chooses between A' and B'. In equilibrium, A will have a higher rental price than A' because B is of lower quality than B', despite the fact that A and A' yield identical direct utility.

Figure 9: Average Estimates of α by Tract Poverty Rate



information for all tracts.

We find remarkable variation in our estimates of α by type. We summarize this variation in Figure 9 which graphs the average value of α by initial Census tract of residence for the people in our estimation sample.[30] The figure shows that people living in high poverty tracts are, on average, nearly three times more sensitive to changes in rent as people living in the lowest poverty areas.

4 Analysis of MTO

As we have noted, the goal of the paper is to understand if housing vouchers can be used to incentivize households to move to neighborhoods that positively affect child test scores. To recap, for the case of Los Angeles so far we have (a) estimated how Census tracts affect child test scores (in expectation); (b) estimated net utility provided by Census tracts for various types of households at current rent levels; and (c) estimated how exogenous changes in rents affect net utility by type of household. With this information in hand, we ask if we can understand why the single largest randomized experimental intervention program that explicitly linked housing vouchers to specific neighborhoods, the Moving to Opportunity experiment, failed to improve child test scores.

[30] The average value of α varies by Census tract because the mix of types varies by tract.

Moving to Opportunity was a randomized control trial begining 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 requirement attached, and (iii) a control group that received no voucher. Voucher amounts were set such that after applying the voucher, households spend no more than 30% of their income on rent.[31] 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, however recent work by Chetty, Hendren, and Katz (2015) demonstrates that MTO positively affected adult wages.

In our model-based analysis of the MTO experiment, we simulate optimal decisions under several policy scenarios, restricting analysis to the households in our sample likely to have been eligible for MTO had they lived in an MTO area at the time of the experiment. Our three scenarios are as follows:[32]

- (Baseline) No subsidies or vouchers.
- (MTO-A) MTO style vouchers. Households who move to a Census tract with a poverty rate under 10% at $t = 1$ receive a Section 8 housing voucher. This voucher is received in perpetuity, even if the household moves out of a qualifying neighborhood in period $t > 1$. If a type-τ household is offered and accepts a voucher and subsequently lives in neighborhood j, we set the utility of that neighborhood equal to our original estimate, $\tilde{\delta}_{j\tau}$, plus α_τ times the voucher amount. The annual voucher we use is \$6,000, which we set such that the average MTO-eligible household can rent a 2-bedroom unit costing \$766 per month after spending 30% of monthly income.[33] We assume that households receiving a voucher spend the entire amount of the voucher each period.

[31] Households that wanted to rent a more expensive unit could only contribute up to an additional 10% of their income.

[32] Our simulations target households residing at $t = 0$ in a Census tract with at least 250 non-senior citizen public housing units, 13 tracts total. 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. Note that we cannot restrict our simulations to households with children, as we do not know which households in the FRBNY Consumer Credit Panel / Equifax data have children.

[33] Our calculation is $\$6,000 \approx 12\,[\$766 - 0.30\,(\$10,000/12)]$, where \$10,000 is mean household income of the MTO-eligible population as computed by Galiani, Murphy, and Pantano (2015) and \$766 is the "payment standard" (max voucher amount) for a 2-bedroom apartments in Los Angeles in 2000.

- (MTO-B) Randomly assigned poverty reduction. We assign households to neighborhoods randomly according to the distribution of neighborhood poverty-rates that arises under scenario MTO-A. Comparisons of MTO-B and MTO-A highlight the role of neighborhood selection conditional on accepting a voucher.[34]

To summarize the expected impact on child test scores of the various MTO policies we consider, we compute an expected measure of accumulated neighborhood value-added exposure conditional on *accepting* a voucher.[35] Let i' denote a family that accepts a voucher in the MTO-A experiment, and assume there are $i' = 1, \ldots, \mathcal{I}$ such families. For any given simulation draw s, we hold this set of families fixed for each of the three scenarios (policies) we consider: Baseline, MTO-A and MTO-B.[36] We then compute the expected impact of policy p on child value-added measured over $\bar{T}$ periods (5, 10 or 18 years) as

$$\widehat{\mu}_p^{TOT} = \frac{1}{S}\sum_{s=1}^{S}\left[\frac{1}{\mathcal{I}}\sum_{i'=1}^{\mathcal{I}}\sum_{t=1}^{\bar{T}}\widehat{\mu}_{\ell(i',t,s,p)}\right] \tag{13}$$

where $\ell(i', t, s, p)$ is the location chosen by family i' in year t under policy p and for given simulation draw s and $\widehat{\mu}_{\ell(i',t,s,p)}$ is the value-added associated with $\ell(i', t, s, p)$. For each type, we run $S = 10,000$ simulations, yielding a total of 1.44 million simulations for each policy experiment. If, as suggested by Chetty and Hendren (2015), neighborhood effects are additive over time in the child test-score production function (i.e. there are no complementarities across time periods) and neighborhood quality affects children equally at all ages, then these measures will characterize actual total neighborhood contributions to child test scores. If child investments exhibit dynamic complementarities and early childhood investments are especially productive as in Cunha, Heckman, and Schennach (2010), these measures will understate neighborhoods' long-term contributions to child test scores. In either case, we view these measures as useful summaries for characterizing the impact of policy.

We compute standard errors around $\widehat{\mu}_p^{TOT}$ to evaluate if the model-predicted outcomes from the baseline, MTO-A and MTO-B are statistically significantly different. Denote the number of types in estimation (144) as $\mathcal{T}$ and the number of Census tracts (1,748) as J.

[34]Specifically, the procedure is; (1) pool the set of MTO-A simulated Census tract choices 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-B simulated locations from the full set of Census tract with probability $Pr(j) = \frac{1}{J}\Big(\frac{p_j}{1-p_j}\Big)\Big(\frac{1-\overline{p}}{\overline{p}}\Big)$.

[35]This is the impact of the treatment on the treated.

[36]We allow the set of families indexed by i' to change across simulation draws.

Referring to notation in equation (12), we estimate the following sets of parameters

$$\{\theta_\tau\}_{\tau=1}^{\mathcal{T}},\ \{\alpha_\tau\}_{\tau=1}^{\mathcal{T}},\ \mathcal{M} \tag{14}$$

where θ_τ is a vector of 180 parameters determining location choice for type τ and $\mathcal{M} = \{\mu_j\}_{j=1}^{J}$ is the vector of parameters determining child value-added in all Census tracts.

θ_τ, α_τ and $\mathcal{M}$ are assumed to be drawn independently for all $\tau = 1, \ldots, \mathcal{T}$. Denote Σ_τ^θ as the variance-covariance matrix of θ_τ, σ_τ^α as the variance of the estimate of α_τ and $\Sigma^{\mathcal{M}}$ as the variance-covariance matrix of $\mathcal{M}$. The parameters in equation (14) are assumed to be distributed with a variance-covariance matrix of

$$\begin{bmatrix} \begin{matrix} \Sigma_1^\theta & 0 & 0 & 0 \\ 0 & \Sigma_2^\theta & 0 & 0 \\ 0 & 0 & \ldots & 0 \\ 0 & 0 & 0 & \Sigma_{\mathcal{T}}^\theta \end{matrix} & 0 & 0 \\ 0 & \begin{matrix} \sigma_1^\alpha & 0 & 0 & 0 \\ 0 & \sigma_2^\alpha & 0 & 0 \\ 0 & 0 & \ldots & 0 \\ 0 & 0 & 0 & \sigma_{\mathcal{T}}^\alpha \end{matrix} & 0 \\ 0 & 0 & \Sigma^{\mathcal{M}} \end{bmatrix}$$

For Σ_τ^θ and σ_τ^α we use asymptotic standard errors and for $\Sigma^{\mathcal{M}}$ we use a bootstrap procedure where we sample from the raw LA FANS data and run the sampled data through the process described in the previous section. To compute standard errors on our policy experiments, we draw parameters from this distribution 3,000 times and compute $\hat{\mu}_p^{TOT}$ for each draw according to equation (13).

Table 6 shows our estimates of $\widehat{\mu}_p^{TOT}$. The first column shows results from the actual MTO demonstration, as reported by Sanbonmatsu, Kling, Duncan, and Brooks-Gunn (2006) and Sanbonmatsu, Ludwig, Katz, Gennetian, Duncan, Kessler, Adam, McDad, and Lindau (2011), and column 2 shows the simulated impact of MTO-A relative to the baseline. Column 1 highlights that MTO researchers found no impact of the voucher program on child test scores after 5 and 10 years and column 2 shows that our model can replicate this finding, despite not using any MTO data in our analysis. Column 4 verifies that we can not reject the hypothesis that our MTO-A results are identical to the results from the actual MTO

Table 6: MTO Demonstration vs. Simulation Experiments

Impacts on Woodock-Johnson Math Scores (sd=1)

Exposure time	MTO Demonstration TOT	Simulation Experiments MTO-A (TOT)	MTO-B (ATE of <10% Pov)	p-value H0: (2) = (1)	p-value H0: (3) ≤ (2)
	(1)	(2)	(3)	(4)	(5)
5 years	-0.019	-0.003	0.097	0.919	0.001
10 years	-0.052	-0.008	0.177	0.874	<0.001
18 years	–	-0.002	0.306	–	<0.001

data. In our view our model passes this out-of-sample fit test.[37]

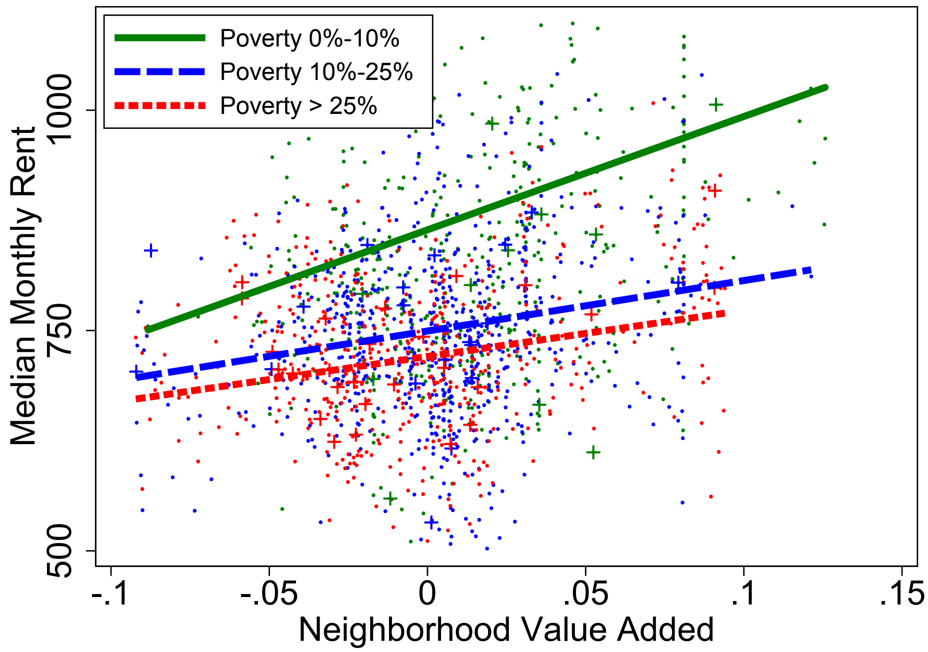
Column 3 reports the results of the MTO-B simulations. These demonstrate that when accumulated over a full 18-year childhood, the poverty reduction generated by MTO would improve math scores by 0.2-0.3 standard deviations if low-poverty neighborhoods were assigned at random to households accepting a voucher. These are substantial impacts, equivalent to closing about 20% - 30% of the black/white achievement gap according to Yeung and Pfeiffer (2009). Thus, the MTO experiment could have significantly improved child test scores if voucher-eligible tracts were chosen more selectively, motivating our work in the next section. Column 5 shows that we can reject the hypothesis that the results from MTO-B are the same as those in the MTO-A experiment.

The comparison of MTO-A to MTO-B shows that households receiving an MTO voucher selected into neighborhoods with low child value-added. Ultimately, for the types receiving housing vouchers, voucher-eligible neighborhoods with low child value-added have higher values of δ than those with high child value-added.[38] One reason this result may occur is that the types of households likely to live in public housing are most sensitive to rent (see Figure 9) and the relative price of additional value-added is high in low-poverty neighborhoods. Figure 10 shows the relationship between composition-adjusted monthly rent in 2000 and

[37]One way in which our model does not match the data is in the take-up rate of the voucher. In our MTO-A simulation, only 45% of the population eligible to receive a voucher accept it whereas the actual MTO take-up rate in Los Angeles was 67%. We attribute the difference in take-up rates to the additional counseling that MTO offered as noted by Galiani, Murphy, and Pantano (2015). We can trivially modify the model to match the MTO take-up rate by adding one parameter that reduces the moving cost associated with the initial move required when accepting an MTO voucher.

[38]This is consistent with recent work by Abdulkadiroglu, Pathak, Schellenberg, and Walters (2017) who find that parents do not value schools with high value-added in test scores.

Figure 10: Scatterplots of Rental Prices and Child Value-Added by Poverty Rate



neighborhood value-added for the 1,748 Census tracts in our study for three groups of Census tracts: Low poverty (0-10%), middle (10-25%), and high poverty (25% and above).[39] These figures show how the relative price of neighborhood quality changes with tract poverty rates. The change in rent associated with an increase in neighborhood quality is greatest in low poverty areas; that is, the slope of the green line (low poverty) is greater than the slope of the blue line (middle) and red line (high poverty). Even though neighborhoods with high value-added are relatively expensive in low poverty tracts, our analysis in the next section builds on the notion that households may be willing to pay to live in those neighborhoods if they receive a large enough rent subsidy.

5 Analyzing Other Voucher Policies

Abstracting from moving costs, the utility of living in tract j for households of type τ given a voucher of size $\mathcal{V}_j$ (for specific use in tract j) is

$$\delta_{j\tau} + \alpha_\tau \mathcal{V}_j \tag{15}$$

[39]We plot the expected rent in each Census tract for a 3-room unit built in 1960, computed as the outcome of a hedonic regression.

If policy-makers want households to reside less frequently in (arbitrary) neighborhood i and more frequently in j, they should either reduce voucher amounts to i, if any, or increase voucher amounts to j. As long as $\alpha > 0$, equation (15) shows that, holding preference shocks fixed, there is some voucher amount to live in location j such that, no matter how large the initial difference in utility between tracts i and j, households optimally accept the voucher and choose to live in j rather than i.

Our MTO-A and MTO-B simulations show that MTO-subsidized households selected into especially low value-added tracts among the set of eligible low-poverty tracts. Ultimately, this occurred because the voucher amount did not sufficiently distinguish between low- and high- value-added tracts, and the lower value-added tracts among the eligible set provided higher utility. A partial explanation is that rents are relatively high in high-value-added neighborhoods with low poverty rates (figure 10) and the types of households currently living in high poverty tract areas are especially sensitive to the level of rent (figure 9).

In the rest of this section, we consider the outcomes of a variety of possible voucher policies to see which policies are effective at inducing people to move to Census tracts with high measured value-added on child test scores.[40] Specifically, we compute the optimal location choices and child outcomes of four different policy experiments. We compute the responses and outcomes of the 10% of types (13 types) most likely to live in public housing and therefore most likely be eligible for a Section 8 housing voucher. In our baseline case, these types all receive a \$600 monthly voucher independent of where they choose to live.[41] In this section, we abstract from any general equilibrium effects. Our thought experiment is for a small, targeted voucher program that does not affect rents or housing supply in any Census tract.

Table 7 shows the distribution of characteristics for all renters in our sample (column 1), renters in tracts with housing projects (column 2) and the 10% of types in our sample most likely to live in tracts with housing projects (column 3).[42] These statistics are all derived from all years of the FRBNY Consumer Credit Panel / Equifax data. When comparing columns 2 and 3, two differences jump out. In the types we consider in our experiments (column 3), no one starts our sample in a Census block with a white racial plurality. Additionally, the types we consider are more likely to have the household member chosen by the FRBNY Consumer Credit Panel / Equifax sampling design to be over the age of 65.

In each of the policy experiments we consider, households receive a voucher of a pre-

[40]Of course policy-makers may have many objectives in mind when setting voucher policy. In this section, we focus on vouchers as a policy tool to improve child test scores.

[41]Since our framework has no wealth effects, this is equivalent to a baseline in which no one receives a voucher.

[42]See Bolton and Bravve (2012) for a description of the population receiving federally assisted housing.

Table 7: MTO Demonstration vs. Simulation Experiments

	All L.A. Renters	Population of Housing-Project Tracts	10% Most Frequent Types in Housing-Project Tracts
	(1)	(2)	(3)
Race:			
White (non-Hispanic)	32.2	8.7	0.0
Black (non-Hispanic)	7.4	12.2	33.2
Hispanic	52.2	70.0	61.2
Other	8.2	9.1	5.6
Age:			
< 30	33.3	36.6	38.3
30-44	30.4	34.6	30.4
45-54	13.7	12.9	6.0
55-64	8.0	6.5	3.1
65+	14.5	9.4	22.3
Adults in H.H.			
1	24.4	26.8	16.6
2	18.5	18.6	27.9
3	17.2	17.1	26.5
4+	39.8	37.4	29.0
Car loan:			
No	67.5	73.9	80.2
Yes	32.5	26.1	19.8
Credit card:			
No	7.7	10.5	10.9
Yes	92.3	89.5	89.1
Student loan:			
No	88.0	89.5	88.0
Yes	12.0	10.5	12.0
Consumer finance loan:			
No	71.1	69.3	64.0
Yes	28.9	30.7	36.0
Observations	1,632,696	15,486	145,974

determined amount that only depends on where they live. Denote $\mathcal{V}_{jp}$ as the voucher amount a household receives when it resides in tract j in policy simulation p. $\mathcal{V}_{jp}$ varies across tracts j in a way we describe below, but within each policy simulation $\mathcal{V}_{jp}$ is assumed to be fixed over time. Additionally, and different from the structure of the MTO experiment, in each period of the policy simulations households receive the voucher appropriate for their current tract of residence; the vouchers received are independent of history.

VA-Index Targeting

In the first two policies, we assume that policy-makers cannot directly observe tract-level value-added, but they can observe correlated characteristics and can design policies based on these characteristic values. We consider policies based on a mixture of three tract-level characteristics: The poverty rate, crime rate and accessibility to transportation.[43] For each of these characteristics, we compute for each tract a "z-score" equal to the number of standard deviations that tract's characteristic lies above (or below) the sample mean. We construct a VA-index for each tract as the z-score for transportation less the z-scores for crime and for poverty. The correlation of this index and actual value-added at the tract level is 0.22 and a scatterplot is shown in figure 11. The thinking behind this policy is that even if child value-added is only noisily targeted, the vouchers steer families currently living in public-housing toward low-crime, low-poverty, high-transportation-accessible neighborhoods.

In both policies the voucher amount for a tract with a VA-Index of 0 is set to $600 per month. In "VA-Index Targeting," changes in the voucher amount vary linearly with changes in the VA-Index. The slope coefficient is set such that the standard deviation of voucher amounts across all 1,748 tracts is identical to the standard deviation of composition-adjusted (constant-quality) rents across all tracts. This policy induces the same variation in voucher payments as a different policy that does not condition on value-added but instead simply adjusts voucher payments for observed variation in rents.[44] In "Aggressive VA-Index Targeting," we double this coefficient.

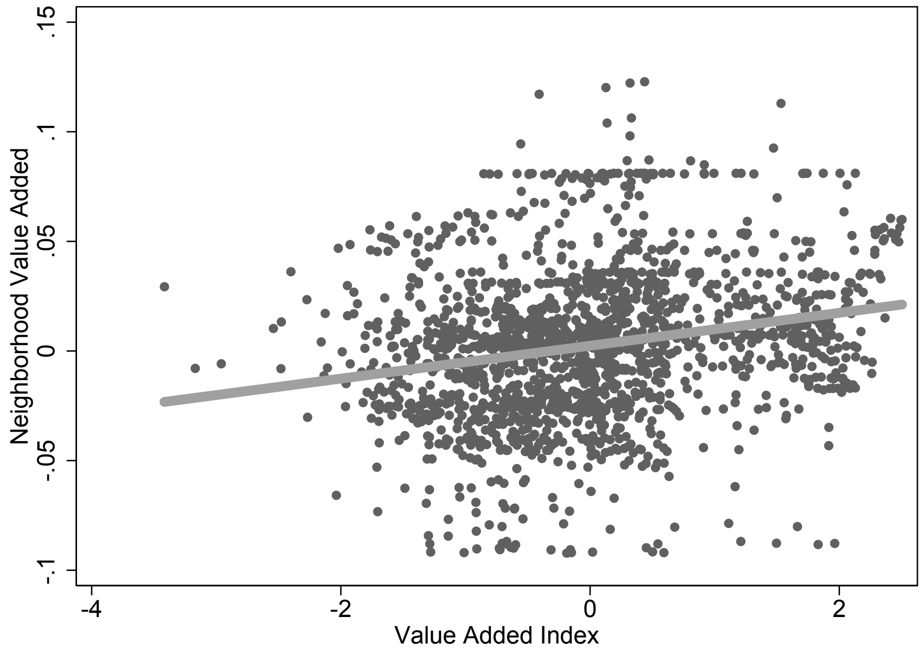
Direct VA Targeting

In the final two policies, we assume policy-makers can directly observe each tract's value-added and construct a z-score directly for value-added for each tract. As before,

[43] The poverty rate is taken from the Census, the crime rate is from Peterson and Krivo (2000) and the transportation-access data are from Ramsey and Bell (2013).

[44] While such a voucher policy sounds difficult to implement, Collinson and Ganong (2018) study a recent demonstration project in Dallas, Texas that adjusts voucher payments for observed variation in rents at the zip-code level. They argue that this type of voucher design is effective at improving the neighborhood quality for voucher recipients by inducing moves to higher quality neighborhoods.

Figure 11: Value-Added Index against Value-Added, by Tract



the voucher amount for a tract with a zero value-added z-score is set to $600 per month. In "Direct VA Targeting," the coefficient relating changes in the voucher amount to changes in the value-added z-score is set such that the standard deviation of voucher amounts across all tracts is identical to the standard deviation of voucher amounts in the VA-Index Targeting policy. In "Aggressive VA Targeting," we double this coefficient.

Table 8 reports the cumulative impact over 18 years, relative to the baseline, of each of the four policies we consider on child value-added. The first column reports the average impact across all types we simulate and the other three columns report the impact of simulated types sorted by race.[45] The overall results are disproportionately reflective of the impact to Hispanic types because they account for more than 60% of our simulated population.

As table 8 clearly shows, the policies vary dramatically in their effectiveness at altering child value-added. The most effective policy, Aggresive Direct VA-Targeting, improves average child outcomes by nearly *five* times the least-effective policy, VA-index targeting. As the VA-Index Targeting experiment shows, a noisy or mildly increasing subsidy to high value-

[45]To be clear, the columns report results for types where types are sorted based on the racial plurality of the Census block of their first residence in the sample.

Table 8: Results of Counterfactual Policy Experiments

Cumulative impact on math scores (sd=1)

Policy	All Housing-Project Types	Black	Hispanic	Other
	(1)	(2)	(3)	(4)
Baseline	–	–	–	–
VA-index targeting	0.26	0.26	0.22	0.62
Aggressive VA-index targeting	0.42	0.27	0.48	0.65
Direct VA targeting	0.77	1.15	0.55	0.90
Aggressive direct VA targeting	1.22	1.47	1.05	1.59

added neighborhoods is not sufficient to induce a large percentage of black- or Hispanic-type households to move. It is only when relatively large vouchers target higher value-added tracts that both black- and Hispanic-type households move to high-value-added neighborhoods in larger percentages. When this happens, policy shows sizable effects on child outcomes. The Aggressive Direct VA-Targeting policy has the potential to eliminate the black-white achievement gap which is equal to one standard deviation in test scores.

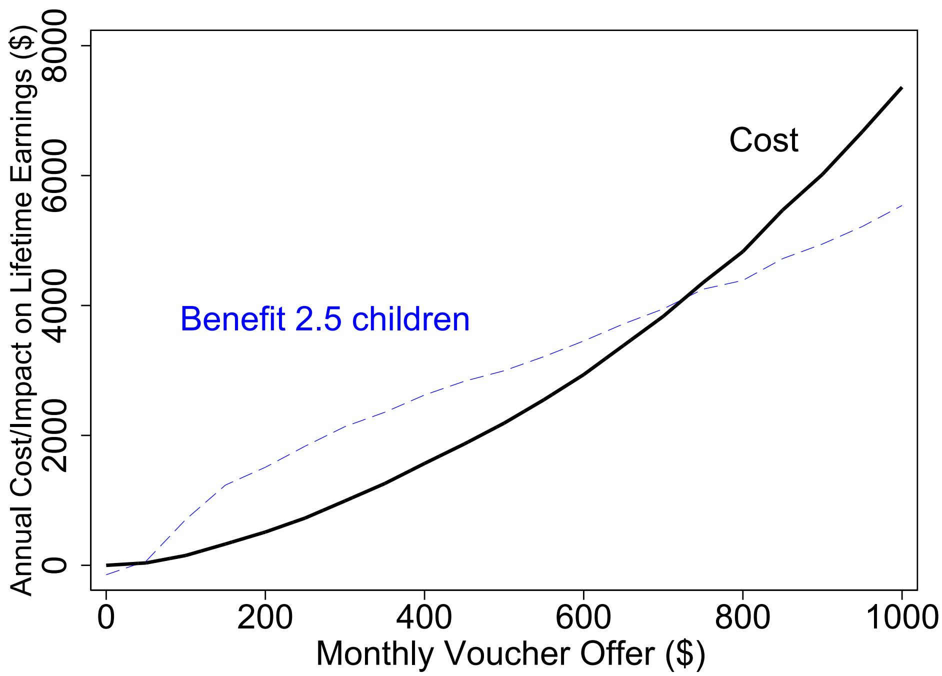
Given that we find that policies that aggressively target high value-added tracts are effective at improving child outcomes, we ask the question, "If neighborhoods can be perfectly targeted by policy-makers, what is the voucher amount that maximizes social surplus?" We tackle this question by making the restriction that vouchers may only be used in the top 5% of tracts by value-added. As with the previous counterfactual experiments, households receive a voucher only in the years in which they live in a targeted Census tract.

Figure 12 shows the costs and benefits of this voucher policy on an annual per-household basis, for various monthly voucher levels. For example, the black line shows that a monthly voucher of \$700 is associated with an annual per-household cost of \$3,830. From this, we can infer the take-up rate of the voucher among the eligible set of households is 46% = $\$3,830/(12 \times \$700)$. At this monthly voucher amount, the per-household benefit of one year of exposure to the targeted Census tracts is also \$3,830. Given a take-up rate of 46%, the benefit to those accepting the voucher is \$8,400, exactly the annual cost of vouchers paid to those accepting a voucher.[46] We compute this benefit as the total impact of one year of exposure of a household to a targeted neighborhood, assuming 2.5 children in the household,[47] to the net present value of the earnings of the children for 40 years after discounting

[46]To be clear, in these calculations we ignore other utility gains and losses from this program.

[47]For the 13 tracts in our sample with a poverty rate above 10% containing large (250 or more units) public

Figure 12: Costs and Benefits of Targeted Vouchers, by Amount of Voucher



the computed net present value by 8 years.[48] Obviously, there may be benefits to children and households from living in these tracts in addition to the impact of value-added on child future wages, and if so policymakers should adjust calculations accordingly.

As figure 12 shows, $700/month is the maximum monthly voucher amount that can be offered where the total societal benefits of the voucher are at least as large as the total costs. This is not the surplus-maximizing voucher. At the surplus-maximizing voucher amount, the change in total cost of the program for a marginal change in the voucher is exactly equal to the change in total benefit. If we denote the annual voucher amount as $\mathcal{V}$, the take-up rate

housing developments, we regress the average number of children per-household (conditional on having a child) on a constant and the share of households in the Census tract that are renters. The constant is 2.78 and the coefficient on share renter is -0.32. This gives us an estimate of 2.46 children per renter household, conditional on having a child and on living in one of these 13 Census tracts. In our analysis, we round this to 2.5.

[48]Explaining, suppose that one year of exposure in a targeted Census tract increases the Woodcock-Johnson test score by 0.0723 standard deviations for each child. Each standard deviation improvement in the Woodcock-Johnson is assumed to increase adult earnings by $4,000 per year (Chetty, Friedman, Hilger, Saez, Schanzenbach, and Yagan, 2011), implying the improvement to annual earnings from living in the targeted Census tract is $289 per-year per-child. The net present value of 40 years of $289 per-year improvement discounted at 5% is $4,964; discounting this present value again by 8 years at 5% yields $3,360 for each child. Our calculations from Census data suggest that households living in public housing with children have 2.5 children in the household, on average. Assuming 2.5 children per household gives a total benefit of $8,400.

(participation rate) of the voucher as $\mathcal{P}(\mathcal{V})$, and the benefit to the future expected wages of children living a household from living in a targeted Census tract for one year as $\mathcal{B}$, then the overall benefits of the voucher are[49]

$$\mathcal{P}(\mathcal{V})\,\mathcal{B}-\mathcal{P}(\mathcal{V})\,\mathcal{V}$$

and assuming an interior solution the optimal voucher amount $\mathcal{V}^*$ satisfies

$$\mathcal{V}^* \;=\; \mathcal{B} \;-\; \frac{\mathcal{P}(\mathcal{V}^*)}{\mathcal{P}'(\mathcal{V}^*)} \tag{16}$$

Under the assumption that the participation rate is never declining in the voucher amount, which should be satisfied given that we find $\alpha_\tau > 0$ for every type, then the surplus-maximizing voucher is always less than the benefit.

Column (1) of table 9 shows our estimate of the surplus-maximizing monthly voucher amount for the entire population in our simulations, \$300/month or $\mathcal{V} = \$3,600$/year.[50] At this voucher amount, the take-up rate is $\mathcal{P} = 28\%$, shown in column (2). Column (3) reports our estimate of $\mathcal{P}(\mathcal{B}-\mathcal{V})$, from which the benefit of one year of exposure on the net present value of children's wages can be computed as $\mathcal{B} = \$7,760$. The implied value of $\mathcal{P}'$ evaluated at $\mathcal{V}^*$ is 6.61E-5, implying a \$151 per-year (\$12.61 per-month) increase in the voucher amount increases the take-up rate of the voucher by one percentage point. Columns (4) and (5) of table 9 show the monthly voucher amount (\$700) and take-up rate (46%) of the break-even voucher, results we discussed earlier.

In the other rows of the table, we compute the surplus-maximizing and break-even voucher amounts by racial types of households. Ignoring all considerations of equity, this table illustrates potential efficiency gains from tailoring public policy by type of household.[51] The table shows there is significant variation in surplus-maximizing and break-even voucher amounts and take-up rates. Generalizing from this table, Hispanic-type households are much less likely to accept a voucher of any amount than black- and other-type households. For example, at a voucher of \$200 per month, nearly 50% of black types accept the voucher but at \$500 per month, only 22% of Hispanic types accept the voucher. This table suggests policymakers can offer relatively modest vouchers to black- and other-type households and expect to see significant participation and benefits in this program; whereas policymakers

[49]Note that these benefits exclude the monetary benefits to households from receiving the voucher of $\alpha_\tau\mathcal{V}$. Recall, in this section we assume policy-makers distribute vouchers to improve child-outcomes.

[50]We compute this using grid search, searching over \$50 increments per month.

[51]We only consider 13 types of households in this experiment; dividing the types by race seemed a natural way to illustrate heterogeneity in the population.

Table 9: Surplus-Maximizing and Break-Even Voucher Amounts, Targeted Vouchers

	Surplus-Maximizing Voucher			Break-Even Voucher+	
	Monthly Voucher Amount	Steady-state Take-up (%)	Per Household Net Benefit* per policy year	Monthly Voucher Amount	Steady-state Take-up (%)
	(1)	(2)	(3)	(4)	(5)
All Public Housing Types	$300	28%	$1,144	$700	46%
Subgroups:					
Black:	$200	47%	$3,320	$750	68%
Hispanic:	$400	18%	$152	$500	22%
Other:	$500	52%	$1,481	$750	84%

* Computed as the voucher take-up rate times the difference of the net present value of the impact on lifetime adult earnings from one year of exposure to the targeted neighborhoods and the cost of one year of vouchers paid to move to those neighborhoods. We assume households receiving a voucher have an average of 2.5 children.
+ The net benefit is zero in the break-even voucher scenario.

might need to consider a broader neighborhood choice set or perhaps a different program altogether (such as the "Aggressive direct VA targeting experiment" of table 8) to induce a majority of Hispanic-type households to accept vouchers to move out of public housing and into higher value-added neighborhoods.

6 Conclusion

In this paper, we use two new rich data sets to understand how households choose neighborhoods and the impact of neighborhoods on child test scores. We find considerable heterogeneity of the population in the utility of different neighborhoods and we show meaningful variation in the impact of neighborhoods on child test scores as measured by test scores. We also show that the utility of households residing in high-poverty neighborhoods, on-average, is much more sensitive to rental prices than the utility of households residing in low-poverty neighborhoods. This last finding helps explain the overall lack of improvement of child test scores in the MTO experiment. Counterfactual simulations of our model of neighborhood choice strongly suggest that policy-makers can significantly affect child outcomes as long as housing vouchers directly target high-value-added neighborhoods. When housing vouchers are designed to directly target these neighborhoods, our estimate of the surplus-maximizing and break-even voucher amounts are \$300 and \$700 per month, respectively. Our analysis assumes that rents and housing supply remain constant after the vouchers are introduced. We think a promising avenue for future research will be to study the general equilibrium effects arising from the large-scale adoption of any of these voucher programs.

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