Neighborhood and Peer Effects

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Labor Econ II
Report

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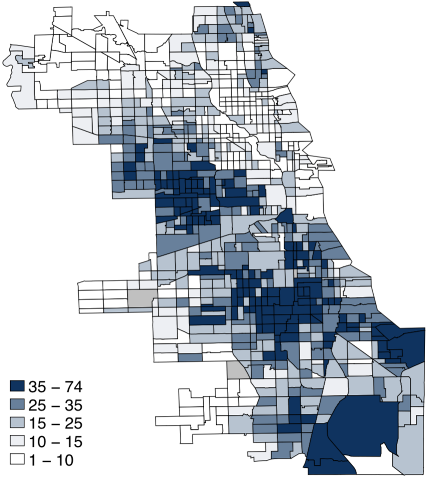
Overview

What is a neighborhood?



Gaubert, Kline, Yagan (2020)

- Geographically localized community within a city, town, suburb or rural areas.
- Outcomes are spatially correlated in the data, and so people want to understand the causes and consequences of this spatial correlation.
- Neighborhood effects as interactions between families and between families and institutions
- Influence schooling, crime, health, civic engagement, home foreclosures, teen births, leadership networks, immigration (Sampson, 2012; Durlauf, 2018)
- Effects are persistent (Ewing, 2018, Durlauf, 1996)

Neighborhoods matter

Rosenbaum-De Luca (2009) conduct interviews with 150 participants in the Chicago Gautreaux Housing Program, who moved from public housing projects to suburban neighborhoods. Interviewers asked *in what ways* moving to the suburbs benefited them and their children. They highlight:

- Feeling a sense of control over their lives
- An address in the suburbs was better for (e.g.) job applications.
- Exposure to white residents debunked stereotypes, gave them "social and cultural know-how" that helped in future interactions with white people
- Different levels of (e.g.) gang activity and violence meant mothers could worry less about their children –> more time to pursue their own goals
- Favors from neighbors (e.g. transportation/financial/watching kids if working late) as a form of capital

Ex Ante Concerns

- Broad, largely undefined concept.
- Its important to understand what makes up a neighborhood. People who refer to "neighborhood effects" might be referring to a host of different ideas.
- Formation of neighborhoods are important, effects are persistent.
- Choice of a neighborhood is correlated with a lot of other factors.
- Incentives at play to maintain neighborhood structure are important.
- Moving a few people across neighborhoods is not the same as moving many people across neighborhoods (reforming the neighborhood).
- Partial versus general equilibrium effects.
- Unhelpful (and possibly harmful) if policy targets the wrong neighborhood elements because the concept went undefined

Overview

Endogenous Stratification (Durlauf, 1996)

- Endogenize neighborhood formation process
- Income inequality emerges through a positive feedback loop where there is a tendency for families to stratify themselves endogenously into homogeneous neighborhoods
- Similar to Benabou (1996), which studies the stratification on economic growth

Endogenous Stratification (Durlauf, 1996)

Model links household income to the next generation's income through education

- Agents have utility u over their own consumption C and the expected value of their children's wealth Y

$$u(i, t-1) = \pi_1 log(C_{i,t}) + \pi_2 E[log(Y_{i,t+1}) | Info_t]$$

- Taxes for household i in time t, T_{it}, are proportional to income Y and used to fund schools

$$T_{it} = \tau_{nt} Y_{it}$$

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- Neighborhood investment in education, $T(\tau_{nt})$, the empirical distribution of income in the neighborhood, $\hat{F}_{nt}$, and individual ability, ξ_i determine a child's income.

$$Y_{i,t+1} = T_{nt}(\tau_{nt})\Theta(\hat{F}_{Y_{nt}})\,\xi_{i,t+1}$$

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$$Y_{i,t+1} = T_{nt}(\tau_{nt})\Theta(\hat{F}_{Y_{nt}})\,\xi_{i,t+1}$$

- With these preferences, solving for optimal neighborhood tax rate yields

$$\tau_{nt} = \frac{\pi_2}{\pi_1 + \pi_2}$$

Endogenous Stratification (Durlauf, 1996)

To Form a Neighborhood

- A neighborhood forms at the beginning of every period.

1. Place the wealthiest household into a new neighborhood.
2. Add households in order of wealth (high to low) until adding more households no longer increases the utility of people in the neighborhood.

- Repeat [1-2] with the remaining households left until all neighborhoods have formed

Implications

- Increasing neighbor's income increases the taxbase, raising the marginal product of investment in education, which increases the expected income of offspring
- Neighborhood stratification occurs when the income gap is higher between rich families and poor families
- Neighborhood-wide positive feedback effects, not economy wide positive feedback effects

Details on Θ, When will we see segregation?, Additional Implications

Endogenous Stratification (Durlauf, 1996)

Implications for Mobility

- Key Parameter: income growth rate among offspring of families in neighborhood n at time t where MY is average neighborhood income

$$g_{n,t} = \frac{E\left(MY_{i,t+1} \mid i \in N_{n,t}, \mathcal{F}_t\right) - MY_{n,t}}{MY_{n,t}}$$

- Stratification maximizes the expected income of the richest people in the economy, while minimizing the expected income of the poorest family's children
- This process maximizes inequality between rich and poor families
- If the economy grows, endogenous stratification results in long-run inequality
- **Intergenerational Mobility** across neighborhoods (Durlauf and Seshadhri, 2018)

$$Y_{offspring} = \alpha + \beta(Y_n)Y_{parent} + \epsilon$$

Conditions for growth, Conditions for permanent inequality, The Gatsby Curve

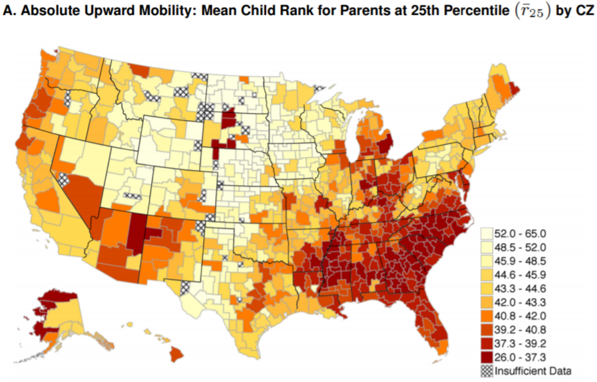
Endogenous Stratification (Durlauf, 1996)

Model Drawbacks

- Equilibrium contingent on Cobb-Douglass preferences
- Assume decentralized neighborhood. Fiscal centralization would undo the inequality.
- A neighborhood is re-formed every time period, underestimating persistence effects
- Housing is not including in the model, potentially underestimating persistence
- Newer models get around this by endogenizing housing choice (Ioannides and Zabel, 2008)

Overview

Chetty et al. (2014) - Motivation



Source: Chetty et al. (2014)

Note: The measure derived from rank-rank OLS estimate at p=25 by CZ

Causal Effects of Neighborhoods vs Sorting

- There are two different explanations to why we see these geographical differences in upward mobility
  1. Sorting: different people live in different places (education, skills, income, etc.)
  2. Causal Effects: places have a causal effect on upward mobility

Chetty and Hendren (2018a) - Causal Effects Strategy

- Given birth cohort s and CZ c, let p be the parents' percentile in the national income distribution
- Let y_i denote the child's national income rank in adulthood
- Authors assume linearity and estimate the following regression on the sample of children with permanent resident parents:

$$y_i = \alpha_{cs} + \psi_{cs} p_i + \epsilon_i$$

  where p_i is the percentile rank of child i's parent in the national income distribution
- Then, predict mean percentile ranks given c, s, and parent rank p:

$$\bar{y}_{pcs} = \hat{\alpha}_{cs} + \hat{\psi}_{cs} p_i$$

- $\bar{y}_{pcs}$ is the measure of neighborhood quality.

CH 2018a: Ideal Experiment

- **Thought experiment**: randomly assign child of parental income rank p, to new neighborhood d, starting at age m, for remainder of childhood. Estimate:

$$y_i = \alpha_m + \beta_m \Delta_{odps} + \theta_i$$

  where $\Delta_{odps} = \bar{y}_{pds}$ - $\bar{y}_{pos}$
- β_m: impact of a 1 percentile increase in the adult outcomes of permanent-d-resident relative to the permanent-o-resident on children on i's adult outcome rank
- **Exposure effect** at age m is $\gamma = \beta_m - \beta_{m+1}$, the effect on y_i of spending the year from age m to age m + 1 in the destination

CH 2018a: Quasi-experimental Design

- But, migration is not random, and estimating the above equation using observational data will yield estimates:

$$b_m = \beta_m + \delta_m$$

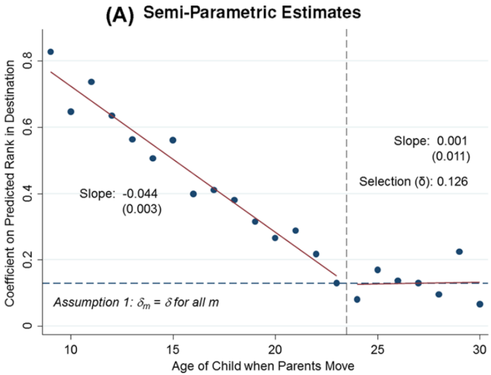
where $\delta_m = \left[\frac{Cov(\theta_i, \bar{y}_{pds})}{Var(\bar{y}_{pds})}\right]_m$

- This will bias estimated age-based exposure effects γ_m since:

$$b_m - b_{m+1} = \beta_m - \beta_{m+1} + \delta_m - \delta_{m+1}$$

- **Assumption:** selection effects do not vary with the child's age at move: $\delta_m = \delta$ for all m

CH 2018a: (Semi-Parametric) Estimation results



Source: Chetty and Hendren (2018a)

- Figure plots estimates of b_m
- We see $b_m > 0$ for $m > 24$: direct evidence of selection effects ($\delta_m > 0$)
- The degree of selection δ_m does not vary significantly with m above age 24
- Estimates of b_m decline steadily with the age at move m for m < 24
- 4% convergence in outcomes per year of childhood exposure to an area

Estimation

CH 2018a: Implications

- Place matters: people in a given neighborhood are not fundamentally different from another neighborhood (vs. sorting)
- Childhood environment matters
- Every additional year of exposure improves child's outcome

CH 2018a: Discussion

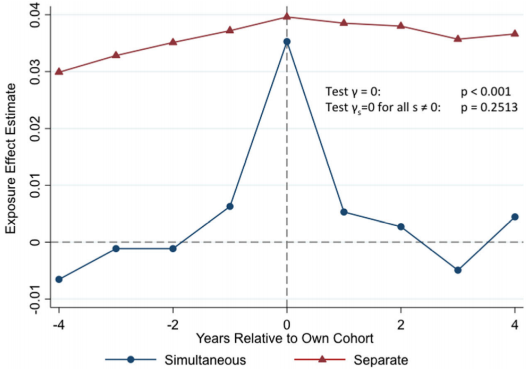
- **Key assumption**: timing of moves to a better or worse area unrelated to other determinants of child's outcomes
- This assumption may not hold for different reasons:
  1. Parents who move to better areas when child is younger may be different from those who move later
  2. Moving may be related to other factors (change in parent's job) that affect children

CH 2018a: Discussion (cont.)

How does Chetty and Hendren (2018a) tackle these two biases?

1. Comparing siblings' outcomes to control for family fixed effects FE
2. Use differences in neighborhood effects across subgroups to implement Placebo tests:
   - Birth cohorts
   - Quantiles of the income distribution
   - Child gender

CH 2018a: Birth cohorts Placebo test



Source: Chetty and Hendren (2018a)

- Exploit heterogeneity in outcomes across birth cohorts
- Add changes in permanent residents' outcome for the child's own cohort, $\Delta_{odp,s(i)}$, with analogous predictions for $s(i) + t$
- In blue analogous coefficients when all cohort-specific predictions are included
- If unobservables are correlated with exposure to a given cohort s(i)'s place effect, also correlated with exposure to adjacent cohorts t
- Supports evidence that there are causal exposure effects and they are cohort specific (peer effects)

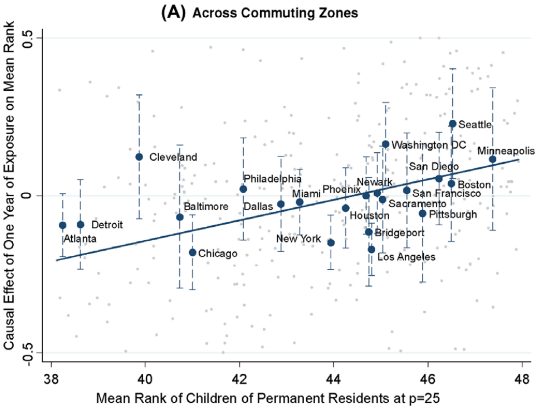
CH 2018a: Final remarks

- Alternative measures of mobility
  - Is income a proper measure of welfare of agents?
- Fundamental importance of skills (complexity, social, prosociality)
- Are children doing better than their parents?
- Further analysis when $m < 9$

Chetty and Hendren (2018b)

- Which areas produce the best outcomes? What are the neighborhood characteristics that generate good outcomes?
- Goal: identifying (approximately) 3,000 treatment effects, one for each county in the country
- Key assumption (again) required to identify counties' causal effects using this research design is that children's potential outcomes are orthogonal to the age at which they move to a given county
- Stronger assumption since it imposes 3,000 orthogonality conditions

Chetty and Hendren (2018b) - Results



Source: Chetty and Hendren (2018b)

- Figure shows CZ fixed effect estimates on children's household incomes given parents at $p = 25$, versus the outcomes of children of permanent residents in each CZ
- Every year of exposure to Los Angeles decreases the expected income rank of a child by 0.17 percentiles relative to a average CZ (95% confidence intervals are shown)
- 1 percentile increase in income translates to \$818 for $p = 25$ and mean income of children is \$26,091 (= 3.14%). Then 0.17 $\times$ 3.14% = 0.53%

Estimation

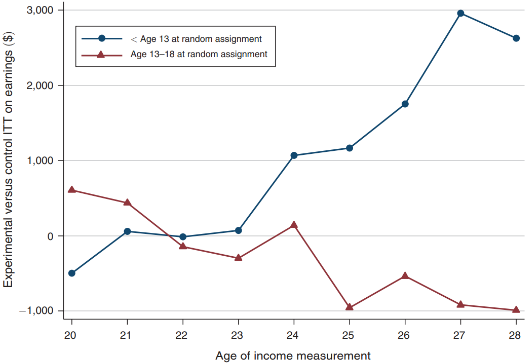
Chetty et al (2016) - MTO

- The Moving to Opportunity (MTO) experiment offered randomly selected families housing vouchers to move from high-poverty housing projects to lower-poverty neighborhoods (mid-1990s)
- Three groups: (i) an experimental voucher group that was offered a subsidized housing voucher constrained by moving to a census tract with a poverty rate below 10 percent, (ii) a Section 8 voucher group that was offered a standard subsidized housing voucher with no additional contingencies, and (iii) a control group
- Estimate the treatment effects of growing up in these very different environments by replicating the intent-to-treat (ITT)

$$y_i = \alpha + \beta_E^{ITT} Exp_i + \beta_S^{ITT} S8_i + \gamma X_i + \delta s_i + \epsilon_i$$

where *Exp* and *S8* are indicator variables for being randomly assigned to the experimental and Section 8 groups respectively, X is a vector of baseline covariates, and s is a set of indicators for randomization site

Chetty et al. (2016) - Results



Source: Chetty et al. (2016)

- MTO experimental voucher treatment substantially increased the earnings of children who were young (statistically significant results after age 25)
- The impacts of treatments on older children are somewhat negative (although not statistically significant)

Chetty et al. (2016) - Discussion

- The results show some contradictory evidence given in Chetty and Hendren (2018a)
- Authors claim that this is caused by disruption costs of moving vs not moving
- But how do disruption costs vary by age?

Identification: Evidence from Denmark

Eshaghnia (2021) explores whether or not Chetty and Hendren's identification assumption holds, using a richer data set from Denmark to compare parents of children who move at different ages.

Table 1: Childhood Exposure Effect Estimates

Dependent variable: Child's income rank at age 26 (30)

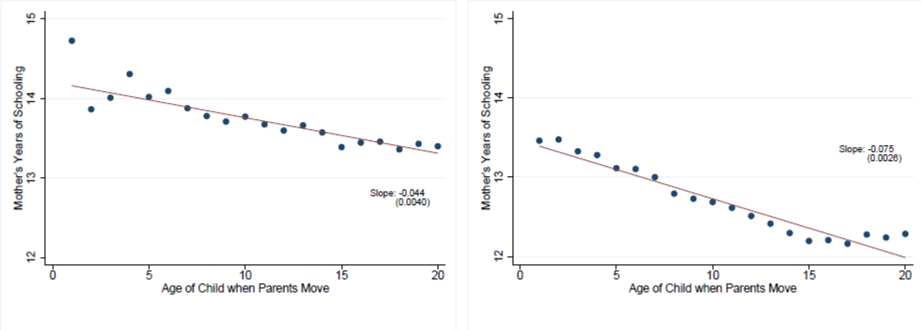
							Family FE		
Spec:	Pooled	Age <= 23	Age < 18	No cohort controls	Family Income	Child nbhd FE	Baseline	No cohort controls	Time-varying controls
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
US (γ)	0.040	0.040	0.037	0.036	0.041	0.031	0.044	0.031	0.043
	(0.002)	(0.002)	(0.005)	(0.002)	(0.002)	(0.002)	(0.008)	(0.005)	(0.008)
DK :	0.013	0.014	0.015	0.011	0.014	0.012	0.009	0.012	-0.008
	(0.003)	(0.003)	(0.004)	(0.003)	(0.003)	(0.003)	(0.009)	(0.008)	(0.016)

The annual exposure effect δ of living in a low vs. high poverty neighborhood as measured on American and Danish data under Chetty and Hendren's identification assumption. δ is the coefficent on the interaction between "years spent in one's new neighborhood" and "difference in income ranks between the two neighborhoods" in a much larger specification.

Identification: Evidence from Denmark

To test Chetty and Hendren's identification assumption, Eshaghnia tests whether various qualities of parents are in fact independent of the age of their children at the time of moving

(a) Owners (b) Renters



Average number of parent years of schooling, plotted as a function of the age of their child at the time of moving, split across homeowners and renters. Under the ID assumption, both graphs should be flat.

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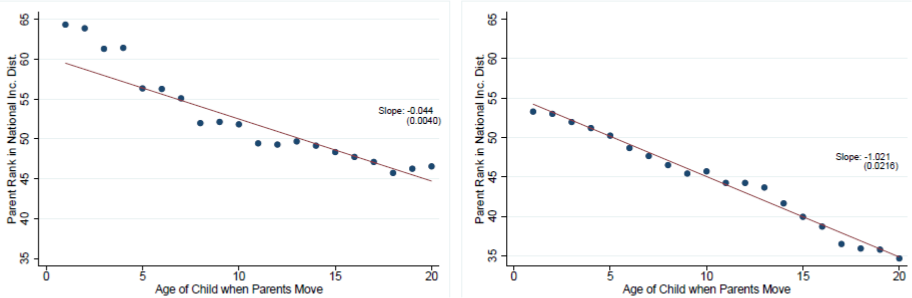

Average parent income rank, plotted as a function of the age of their child at the time of moving, split across homeowners and renters. Under the ID assumption, both graphs should be flat.

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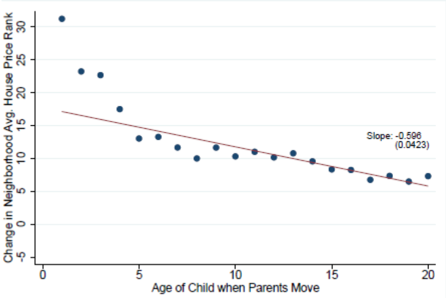
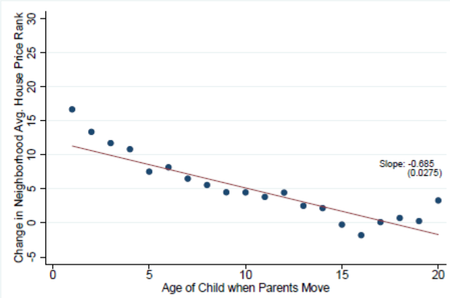


(b) Renters



Average percent increase in rank in neighborhood house prices per move, split across homeowners and renters. Under the ID assumption, both graphs should be flat.

Overview

Do Neighborhoods affect Adults?

Four perspectives on neighborhood effects on adults:

1. In many theoretical models (e.g. Durlauf 1996, Benabou 1996), neighborhood effects act on children, allowing them to obtain human capital. No mention of effects on adults
2. The sociological literature, however, leaves room for effects on adults: better addresses for job applications, access to childcare, favors from neighbors, more access to jobs and credit, network effects, etc. (e.g. Rosenbaum and De Luca 2009, Wilson 2010)
3. Observational studies tend to pick up fairly strong effects on adult income after controlling for observable characteristics (e.g. Cutler and Glaeser 1997)
4. Analyses of the recent Moving To Opportunity (MTO) experiment regularly show economically and statistically effects on children, as well as on adult physical and mental health but not on adult economic outcomes (e.g. Levanthal and Brooks-Gunn 2003, Harding et al. 2021)

Why is MTO different?

Why the discrepancy between observation and experiment? Two possibilities:

- There is a *strong* selection bias on who moves and who does not, which could explain most or all of the observational results. (Durlauf 1996, Ioannides and Zabel 2007)
- On the other hand, there are MTO-specific issues that could hide real effects:
  - Most families moved < 10 miles, children continued to attend low-achieving schools, social networks largely unchanged (Rosenbaum and DeLuca 2009)
  - Many control group members qualified for federal Hope VI program, which moved them out of large public housing projects, potentially artificially boosting their outcomes (Ibid.)
  - Estimates often combine a (presumably positive) neighborhood effect with a (presumably negative) effect of moving.
  - Blanket "treatment-on-the-treated" style analyses conflate several different treatment effects (Pinto 2020).

We will look at a study by Harding et al. arguing for the selection bias hypothesis, as well as Pinto's look at effects on different subgroups.

Harding et al. (2021): Excluding other explanations

Harding et al. (9 total authors!) look at potential non-selection-bias-related explanations for the discrepancy, including:

- Analyses using different outcome measures
- Heterogeneous neighborhood effects being lumped together
- Non-linear neighborhood effects causing misspecification
- Magnitude in the change of neighborhoods in MTO vs observational studies
- (Several others)

They systematically argue that each alternate explanation is inconsistent with the data, leaving (they argue) selection bias as the best explanation. For the sake of time, we'll only look at the first in detail.

What if MTO wasn't an experiment?

To deal with the possibility that some systematic difference in outcome measures or data sets is driving the difference between MTO and the non-experimental approaches, Harding et al. analyze the data *as if* it were non-experimental. In other words:

- While a typical analysis of MTO looks at people who were *incentivized* to end up in different neighborhoods (to avoid selection bias), the "non-experimental" econometrician doesn't know which people received vouchers.
- Instead, to match typical observational analyses, we look at all people who moved to high-poverty neighborhoods in the sample, and compare them to all people who moved to low-poverty neighborhoods.

What if MTO wasn't an experiment?

Table 3. Comparing experimental and nonexperimental estimates of the effects of low poverty neighborhoods on an economic index

	Census Tract Poverty Rates			Economic Index for High-Poverty	Estimated Effect of Low-Poverty Nbhd on Economic Index		
	Low-Poverty Group Mean	High-Poverty Group Mean	Poverty Rate Diff.		Coeff. (SE)	P-Value	N
I. Prior to trimming and reweighting							
A. Experimental estimate							
(A1) MTO exp TOT (std wgts)	0.199	0.368	-0.169	-0.229	-0.012 (0.067)	0.863	2543
B. Nonexperimental estimates (unweighted)							
(B1) PSID nonexp estimate (< 25% poverty)	0.102	0.337	-0.235	-0.262	0.101 (0.044)	0.021	4299
(B2) MTO nonexp est (< 25% poverty)	0.177	0.413	-0.236	-0.369	0.096 (0.046)	0.036	1770

Non-experimental analyses of MTO are generally similar to other non-experimental estimates, pointing to the possibility of selection bias. See Harding et al. (2021) for many more panels.

Why are MTO estimates so low?

Outcomes	$TOT(z_e, z_c)$ TSLS	
Income of Family Head	1.423	**
s.e. and p-value	0.685	0.038
Income of Head and Spouse	0.234	
s.e. and p-value	0.762	0.759
Total household income	0.538	
s.e. and p-value	0.838	0.520
Above Poverty Line	0.034	
s.e. and p-value	0.038	0.376
Employed without welfare	0.069	*
s.e. and p-value	0.041	0.092
Currently on welfare	−0.072	*
s.e. and p-value	0.037	0.053
Job tenure	0.079	*
s.e. and p-value	0.041	0.054
Economic self-sufficiency	0.024	
s.e. and p-value	0.032	0.451
Neighborhood Poverty	−28.601	***
s.e. and p-value	1.082	0.000

- Pinto (2020) notes that TOT effects of MTO vouchers on parameters of interest are often neither statistically nor economically significant.
- Citing an analysis of Kline and Walters (2016), he points out that TOT measures are conflating the effect of interest (moving from a high to a low poverty neighborhood) with smaller effects (moving from a medium to a low poverty neighborhood, staying in a low poverty neighborhood, etc.)
- Next few slides: Pinto's estimate of the actual neighborhood effect (necessarily on a smaller subgroup).

Pinto's extension of LATE

Recall the LATE model for a binary treatment D with a binary instrument Z (assume no covariates for simplicity):

- There are four types s of individual (compliers, always-takers, never-takers, defiers)
- We have four linear equations to identify propensity scores, but there is linear dependence: $\mathbb{P}[D=0|Z=z]+\mathbb{P}[D=1|Z=z]=1$. This reduces the rank to three.
- By assuming there are no defiers, we can identify propensity scores, letting us identify

$$\mathbb{E}[Y(1)-Y(0)|S=c]=\sum_{i=0}^{1}(-1)^i\frac{\mathbb{E}[Y\mathbb{1}[D=i]|Z=i]-\mathbb{P}(S=a)\mathbb{E}[Y(i)|D=i,Z=1-i]}{\mathbb{P}(S=c)}$$

Pinto's approach generalizes this to the case of MTO vouchers, which is more complex:

- There are now three values of D (low, middle, or high poverty neighborhoods) and three values of Z – no voucher, section 8 (usable in low or middle poverty neighborhoods), or experimental (only in low poverty neighborhoods for first year)
- Identification is now **much** harder: there are twenty-seven potential types of individual, and only seven linearly independent equations we can use to identify them! The LATE monotonicity condition is not sufficient to secure identification.

Pinto's extension of LATE

- Idea: use micro theory to eliminate unrealistic choices of S (responses to vouchers.)
- **Weak Axiom of Revealed Preferences**: A family revealed to prefer A to B cannot also be revealed to prefer B to A.
  - E.g. families who choose t_m under the experimental voucher, where t_m is not subsidized, will not choose t_ℓ with a section 8 voucher, where it is.
- **Normal Choice**: An increase in income does not lower consumption.
  - E.g. a family that chooses t_ℓ with no voucher will continue to choose t_ℓ with either voucher.

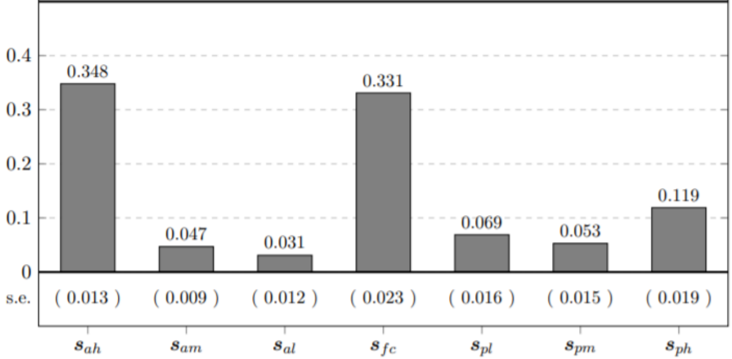
1	$T_i(z_c) = t_l$	$\Rightarrow$	$T_i(z_e) = t_l$ and $T_i(z_8) \neq t_h$
2	$T_i(z_c) = t_m$	$\Rightarrow$	$T_i(z_e) \neq t_h$ and $T_i(z_8) \neq t_h$
3	$T_i(z_e) = t_m$	$\Rightarrow$	$T_i(z_c) = t_m$ and $T_i(z_8) = t_m$
4	$T_i(z_e) = t_h$	$\Rightarrow$	$T_i(z_c) = t_h$ and $T_i(z_8) \neq t_l$
5	$T_i(z_8) = t_h$	$\Rightarrow$	$T_i(z_c) = t_h$ and $T_i(z_e) = t_h$
6	$T_i(z_8) = t_l$	$\Rightarrow$	$T_i(z_e) = t_l$
7	$T_i(z_c) \neq t_h$	$\Rightarrow$	$T_i(z_8) = T_i(z_c)$

Restriction rules implied by the two axioms

Pinto's extension of LATE

The restriction rules narrow the 27 possible response-types to just seven. Just as for LATE, we now have enough information to identify propensity scores. Most people are in either the s_{ah} group (always living in high-poverty neighborhoods) or the s_{fc} group (full compliers.)

Response-types	s_{ah}	s_{am}	s_{al}	s_{fc}	s_{pl}	s_{pm}	s_{ph}
$T(z_c)$	t_h	t_m	t_l	t_h	t_h	t_m	t_h
$T(z_8)$	t_h	t_m	t_l	t_m	t_l	t_m	t_m
$T(z_e)$	t_h	t_m	t_l	t_l	t_l	t_l	t_h



Pinto's extension of LATE

	$E(Y(t_l) - Y(t_h)\|s_{fc})$		$E(Y(t_l) - Y(t_m)\|s_{fc})$		$E(Y(t_m) - Y(t_h)\|s_{fc})$	
Income of Family Head	2.056	***	0.721		1.334	
(s.e.)	0.810		1.232		1.184	
(p-value)	0.007		0.552		0.257	
Income of Head and Spouse	0.878		1.349		−0.471	
(s.e.)	0.854		1.265		1.359	
(p-value)	0.322		0.318		0.752	
Total household income	1.902	**	2.451	*	−0.549	
(s.e.)	0.900		1.272		1.329	
(p-value)	0.047		0.073		0.698	
Above Poverty Line	0.108	***	0.044		0.064	
(s.e.)	0.041		0.065		0.067	
(p-value)	0.010		0.490		0.342	
Employed without welfare	0.113	**	0.135	*	−0.022	
(s.e.)	0.045		0.073		0.074	
(p-value)	0.017		0.095		0.763	
Currently on welfare	−0.121	***	−0.026		−0.095	
(s.e.)	0.043		0.067		0.068	
(p-value)	0.005		0.683		0.160	
Job tenure	0.088	*	0.107		−0.019	
(s.e.)	0.047		0.073		0.074	
(p-value)	0.063		0.175		0.803	
Economic self-sufficiency	0.065	*	−0.015		0.080	
(s.e.)	0.033		0.060		0.057	
(p-value)	0.057		0.777		0.167	
Neighborhood Poverty	−33.256	***	−20.387	***	−12.869	***
(s.e.)	1.008		1.808		1.955	
(p-value)	0.000		0.000		0.000	

- Effects on different variables for full compliers moving from high to low poverty neighborhoods (with intermediate changes in the second and third columns.)
- While TOT effects are small and often nonsignificant, restricting our attention to full compliers shows more tightly estimated effects.
- The usual caveats of LATE apply: this is the average treatment affect on **a** subgroup, which may or may not be relevant to a particular purpose.

Decomposition of the TOT estimate

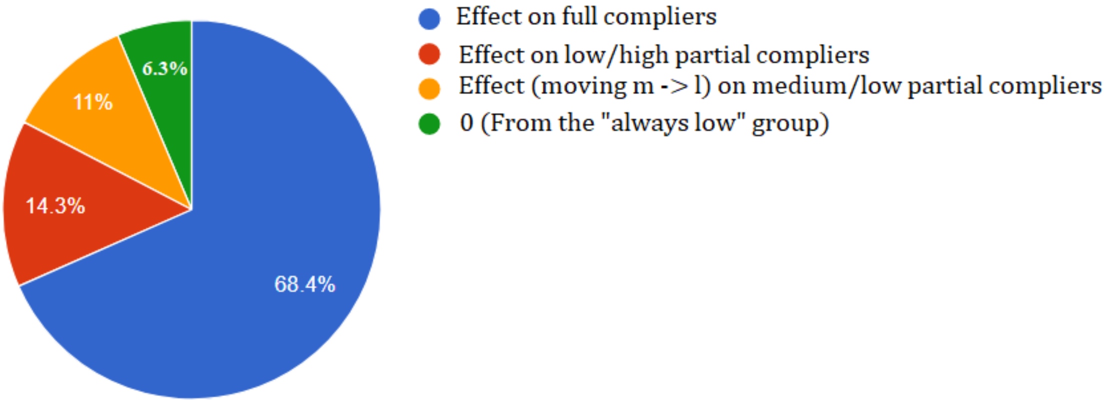


Figure: The TOT estimate is a weighted average of effects on different subgroups, shown here. The effect size for full compliers (blue) is tightly estimated, but is muted by the other three groups. The low/high partial compliers (in red) show a large (but not statistically significant) negative effect, further complicating the weighted average.

Overview

Neighborhood Choice and Housing, Ioannides and Zabel (2008)

Model Overview

- Jointly model neighborhood and housing choices.
- Decompose the neighborhood into "structural" and "neighborhood effects": structural effects might be physical properties of the space, while neighborhood properties are the influence of fellow peers who live in the surrounding area.
- Household's demand for housing depends on the mean of neighbor's demand for housing

Contributions

- Controlling for non-random sorting allows for unbiased estimate housing elasticities and allows for identification of neighborhood effects, because it partials out the effect of neighborhoods on prices.
- Mechanisms: (1) households strive for a level of housing that is on par with their neighbors (2) Households are financially motivated to maintain, renovate, repair, make additions to the house since it will increase their own assets

Neighborhood Choice and Housing, Ioannides and Zabel (2008)

In the model, households: choose what MSA, m, census tract, s, and neighborhood cluster k they wish to live in in order to maximize indirect utility V. V is a function of tract-specific characteristics, g_s, housing prices, P_{ms}, household income, I_h, their own demands, z_h, their neighbor's demand, z_k, **the idiosyncratic quality of the neighborhood,** ν_k**, their random taste parameter,** η_{mskh}**,** and a random component to utility, ϵ_{mskh}.

$$\max \ V\left(g_s, P_{ms}; I_h; z_h; \mathbf{Y}_k, \mathbf{z}_k; v_k + \eta_{mskh}\right) e^{\epsilon_{mskh}}$$

$$s.t. \ \ c_h + P_{ms} Y_h = I_h$$

Meanwhile, assume that prices are homogenous of degree one in components

$$P_{ms} = P^{\nu}_{ms,nei} P^{1-\nu}_{m,str}$$

Neighborhood Choice and Housing, Ioannides and Zabel (2008)

Apply Roy's identity to the conditional utility function V with respect to price $P_{m,stru}$ and take logs to **recover the demand for housing**

$$\begin{aligned} y_{str,mskh} = {} & \alpha + \nu p_{ms,nei} + [\mu(1-v) - v] p_{m,stru} + \delta \ln I_h + \xi z_h \\ & + \beta \Pi_y \left(\mathbf{y}_{stru,k}\right) + \gamma \Pi_z \left(\mathbf{z}_k\right) + v_k + \eta_h \end{aligned}$$

where $\Pi_y(y_k)$ is a household's behavior that depends on the behavior of her neighbors, and $\Pi_z(z_k)$ is the social effect which reflects one's taste for one's neighbor's characteristics. This reflects how that similar people like to live together.
Estimate the following equation, with and without mean neighbors' demand:

$$\begin{aligned} y_{stru,mskh} = {} & \alpha + v p_{ms,nei} + v' p_{m,stru} + \delta \ln I_h + \beta \overline{y_{stru,n(h)}} + \gamma \overline{z_{n(h)}} + v_k \\ & + E\left[\eta_h \mid s = s_h\right] + \psi_h \end{aligned}$$

Results

- The elasticity of housing demand with respect to mean neighbors' demand is 0.8504 (significant) instead of 0.7254 (significant) in the absence of the correction for neighborhood choice.

Neighborhood Choice and Housing, Ioannides and Zabel (2008)

Further Results

- Neighborhood effects – the effect of neighbor's homes on the demand for housing – are strengthened when neighborhood choice is accounted for
- Individuals tend to live with people like themselves

Potential Limitations

- Assumes that neighbors are similar to each other, and so this does not tell us what would happen if we move someone very different into a neighborhood.
- You cannot necessarily break down prices into those caused by the structure and what is caused by the neighbors: you must allow for the interaction term. (You cannot treat prices as HOD1), $P_{ms} = P^{\nu}_{ms,nei} P^{1-\nu}_{m,stru}$
- Assumes independence of irrelevant alternatives
- Instrument validity
- Strong functional form assumptions: when preferences are Cobb Douglass over consumption and expected children's wealth, taxes will be independent of wealth level
- Households limit their search to their current MSA. Thought of as a discrete choice problem

Overview

Conclusions

- The concept of "neighborhood" captures a number of different aspects of a place: the physical location, services and amenities, local schools, peer networks, etc. Sorting out which parts of a neighborhood cause a given effect is extremely hard.
- Because people select into neighborhoods, it is difficult empirically to disentangle actual neighborhood effects from selection bias.
- Theory and experimental data show economically and statistically significant effects of growing up in low-poverty neighborhoods on a wide variety of children's outcomes, as well as adult physical and mental health outcomes.
- Effects on adult economic outcomes are smaller and more difficult to measure, but do seem to exist.
- The uncertainty surrounding the root causes of neighborhood effects makes it hard to identify any particular policy prescriptions.

Appendix: Can we identify areas of opportunity?

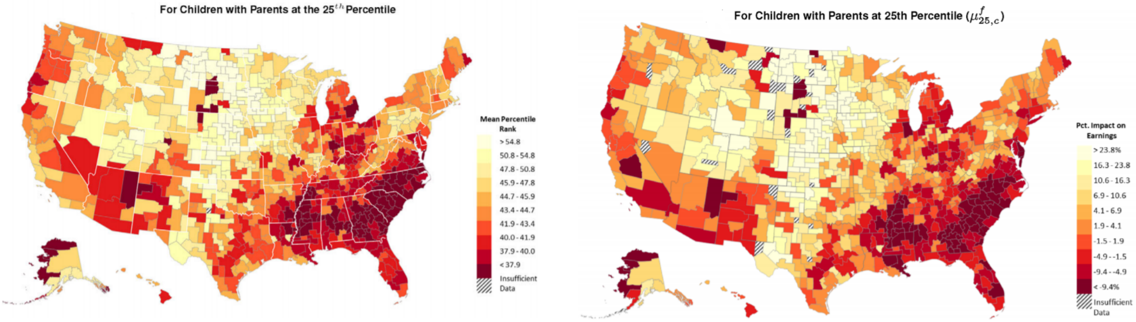
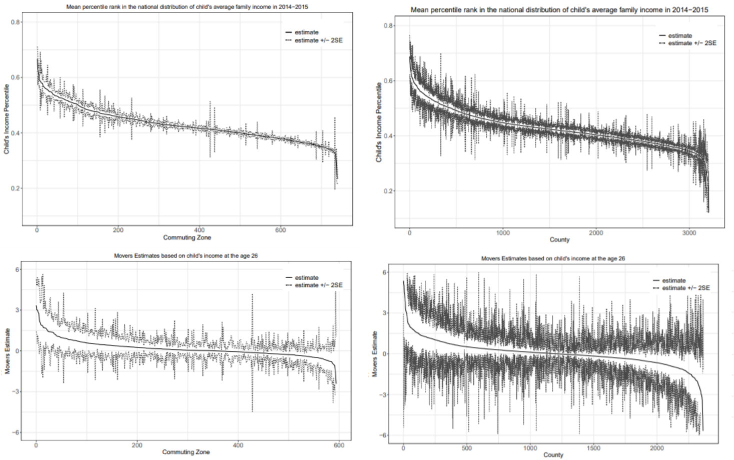


Figure: Heat maps measuring social mobility from Chetty and Hendren (2018b). The left map measures the income percentile of children born to 25th percentile parents in a given commuting zone, while the right map attempts to measure the annual exposure effect of living in a neighborhood on a child's future income.

How uncertain are the estimates these maps are based on?



Top: Adult income quantile of kids born to 25th percentile parents, by CZ (left) and county (right). **Bottom:** (Claimed) causal effect of a year in a given commuting zone (left) or county (right). All graphs from Mogstad, Romano, Shaikh, and Wilhelm (2020)

Can we really rank regions by mobility?

- **Potential Problem:** The Chetty-Hendren maps implicitly try to rank regions (e.g. commuting zones) by mobility measures, which are *estimated* quantities. If the measured differences are small relative to standard errors, these rankings could be incredibly misleading.

Mogstad, Romano, Shaikh, and Wilhelm (2020) explore confidence intervals for ranks. Assume we have estimates of a parameter of interest for a collection of regions. There are three types of confidence intervals we might be interested in:

1. What is the set of plausible ranks for a given region?
2. What is the collection of plausible ranks for the entire population?
3. How many regions could plausibly be in the top (or bottom) τ regions?

We will quickly look at how they estimate (1), and then look at how this applies to Chetty and Hendren's data.

Fix regions $P_1, \cdots, P_n$, and a parameter $\theta_\bullet$. We'd like to order the P_i based on their values θ_i, but we only observe an estimate $\hat{\theta}_i$.

- Naive Idea: assume we have enough knowledge of the distributions of the P_i to form plausible confidence intervals C_{ij} for the difference $\theta_j - \theta_i$.
  - (Either by making a parametric assumption, or by bootstrapping)
- For a given i, let p_i be the subset of $\{1, 2, \cdots, n\}$ consisting of j for which $C_{ij} \subseteq (0, \infty)$. Similarly, n_i is the subset of j for which $C_{ij} \subseteq (-\infty, 0)$.
- The resulting confidence interval for the rank of P_i is $\{|p_i| + 1, |p_i| + 2 \cdots n - |n_i|\}$.

This gives confidence intervals that are too narrow, because too many confidence intervals exclude zero due to multiple comparisons. We can resolve this using a stepdown procedure:

1. Set $s = 0$ and $I_s = \{1, 2, \cdots, n\} \setminus \{i\}$.
2. Construct confidence intervals C_{ij}^s for all j in I_s such that the probability that **any** C_{ij}^s has a sign error is bounded by your favorite α.
3. If any C_{ij}^s exclude zero, add them to n_i or p_i, let I_{s+1} include all j that did not exclude zero, and repeat step two.

Ranks of Mover Effects are Highly Uncertain

Panel A: Top 5

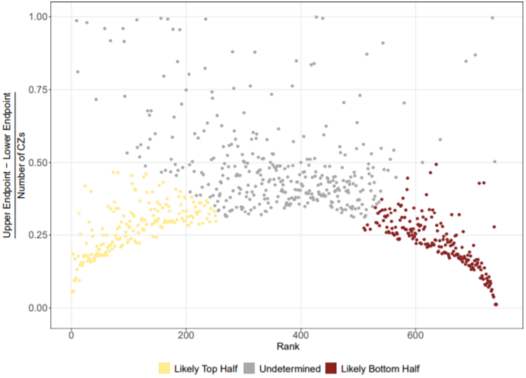
Rank	τ	Correlational CZ	$\hat{\bar{y}}_{c25}$	SE	95% CS	τ-best	Movers CZ	$\hat{\mu}_{c25}$	SE	95% CS	τ-best
1	1	San Francisco	0.457	0.001	[1, 2]	4	Seattle	0.229	0.082	[1, 38]	44
2	2	Salt Lake City	0.457	0.001	[1, 4]	4	Washington DC	0.163	0.077	[1, 41]	48
3	3	Boston	0.453	0.001	[1, 4]	5	Cleveland	0.124	0.107	[1, 48]	50
4	4	Minneapolis	0.452	0.001	[2, 5]	5	Fort Worth	0.121	0.090	[1, 48]	50
5	5	San Jose	0.449	0.001	[4, 6]	6	Minneapolis	0.116	0.120	[1, 48]	50

Panel B: Bottom 5

Rank	τ	Correlational CZ	$\hat{\bar{y}}_{c25}$	SE	95% CS	τ-worst	Movers CZ	$\hat{\mu}_{c25}$	SE	95% CS	τ-worst
46	5	Raleigh	0.369	0.001	[45, 46]	10	Charlotte	-0.248	0.096	[3, 50]	49
47	4	Indianapolis	0.364	0.001	[46, 47]	5	Port St. Lucie	-0.263	0.090	[3, 50]	49
48	3	Jacksonville	0.358	0.001	[48, 50]	4	Raleigh	-0.278	0.105	[3, 50]	49
49	2	Atlanta	0.358	0.001	[48, 50]	3	Fresno	-0.377	0.100	[13, 50]	48
50	1	Charlotte	0.355	0.001	[48, 50]	3	New Orleans	-0.391	0.111	[14, 50]	48

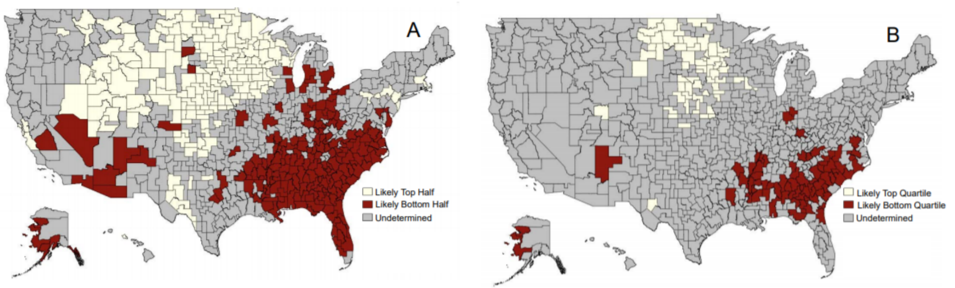
Figure: Ranks for the 50 largest commuting zones by $\hat{\bar{y}}_{c25}$, the (point estimate of the) expected income percentile of a child born to parents in the 25th percentile, and $\hat{\mu}_{c25}$, the estimated effect of an additional year of childhood in the CZ for children with 25th percentile income parents.

The distribution of uncertainty among commuting zones



- The x axis consists of all of the commuting zones, ranked by estimated correlational mobility ($\hat{\bar{y}}_{c25}$)
- The y axis is the width of the confidence interval for its rank, scaled so that 1 is the largest possible.
- A handful of commuting zones we are confident are above/below the median, but a much larger number we are uncertain about.

So can we identify areas of opportunity?



Taking the above discussion on confidence intervals into account, Mogstad et al (2020) produce more conservative depictions of areas with higher and lower correlational mobility ($\hat{\bar{y}}_{c25}$.) We can draw some broad conclusions – i.e. comparing the upper midwest to the south, but many of the specific city-level comparisons in Chetty and Hendren (2018b) are not statistically significant.

Appendix

Appendix: Durlauf (1996)

Durlaff (1996), Model
Households

- Agents have utility over their consumption and their children's consumption $U(i, t-1) = \pi_1 log(C(i, t)) + \pi_2 E[log(Y(i, t+1)|Info_t)]$
- Income, Y, is determined human capital and productivity: $Y_{i,t+1} = \phi H_{nt}\xi_{i,t+1}$, H is human capital , ξ productivity shock of a neighborhood
- $\xi_{i,t+1} = \nu_{n,t+1}\gamma_{i,t+1}$, ν is neighborhood, γ is individual
- Budget constraint: income = consumption + taxes (taxes finance education)
- $T_{it} = \tau_{nt} Y_{it}$ (τ is chosen by majority rules)
- Total expenditures (TE) = Fixed Cost + Per Student Cost * Num Student. $TE_{nt} = \lambda_1 ED_{nt} + \lambda_2 \mu(N_{nt}) ED_{nt}$, where ED is educational investment
- $H = \Theta(F)ED$, human capital = educational investment * draw from a distribution F, income distribution in the neighborhood. The wealthier the neighborhood, the larger the tax base.

Go Back

Appendix: Durlauf (1996)

Durlaff (1996), Model
Equilibrium

- For any neighborhood configuration, the tax rate is $\tau_2/(\tau_1+\tau_2)$. This is unanimously preferred to any alternative and independent of neighborhood composition.
- Utility is increasing in monotonic rightward ships of the empirical distribution over all other families in the neighborhood
- For any cross-section income distribution, there exists a core configuration of families across neighborhoods.
- Income follows $P[Y_{i,t+1}|F_t] = P[Y_{i,t+1}|F_{Ynt}, \mu(N_{nt})]$

Go Back

Durlaff (1996), Model
Equilibrium: proof of the existence of the core configuration.

Consider the following algorithm to construct the core: Given the realized income distribution at t, place the family with the highest income in the economy in neighborhood 1. If the range of incomes is open from above, introduce a family whose income closes the economy's income range from above into neighborhood 1. Add to that neighborhood the largest measure of families that maximizes the utility of the parent of the family initially assigned to neighborhood 1. This collection of families is defined up to a set of measure zero because $\Theta\left(F_{\epsilon}\right)$ is continuous in ϵ. Among the remaining families, place the highest-income family among those not assigned to neighborhood 1 into neighborhood 2 ; as before, if the range of incomes among the remaining families is open from above, introduce a family whose income closes the set from above and place it in neighborhood 2 . Add to that neighborhood the collection of families not assigned to neighborhood 1 that will maximize the utility of the first family assigned to neighborhood. Go Back

Appendix: Durlauf (1996)

Durlaff (1996), Model

Equilibrium: proof of the existence of the core configuration.

Repeat this procedure until all families are assigned to neighborhoods 1 to D. We claim that this algorithm produces a core configuration. To see this, observe that by Proposition 2 all members of a neighborhood wish to have the highest income neighbors for a neighborhood of fixed size. Hence, the relative neighborhood ranking by the richest family in the population is agreed upon by all families in the neighborhood. For families in neighborhood 1, this neighborhood is utility-maximizing among all possible neighborhoods in which they could reside.

Proceeding down across neighborhoods, the same argument applies, so the neighborhood configuration produced by the algorithm is in the core. To verify (ii), suppose that two neighborhoods have nonstratified income distributions. In this case there exists a re-allocation of families across neighborhoods, given Proposition 2, such that the richest member across the two neighborhoods lives in a community of equal size with a preferred income distribution; under this reallocation, the population will now be stratified by income. All members of the now wealthier neighborhood are also better off. Hence the original neighborhood configuration could not have been a core configuration. Finally, notice that the configuration produced by the algorithm is unique except for sets of measure zero. Go Back

The Gatsby Curve (Durlauf, 2018)

Let Y^p denote log parental income, Y^o denote log offspring income, and let β denote the mobility coefficient, the coefficient of interest. Then cannot give rise to the Gatsby curve as observed in the data

$$Y^o = \alpha + \beta Y^p + \epsilon$$

Can give rise to the Gatbsy curve

$$Y^0 = \alpha + \beta(X)Y^p + \epsilon$$

Go Back

Durlauf, 1996: Detail on $\Theta(F)$

1.) Rightward shifts in a neighborhood's income increase amount of human capital produced.
2.) Along growing income paths, the incentive for wealthy families to segregate from poorer families is not growing at the same rate as well.
3.) Continuous Go Back

Durlauf, 1996: Income segregation?

High income households benefit from large communities due to the decreasing average cost of human capital investment, T, in a neighborhood n with N families

$$ED_{nt} = \tau_{nt} \frac{\sum_{i \in n} Y_{it}}{N}$$

$$T_{nt} = \lambda_1 ED_{nt} + \lambda_2 \mu(N_{nt}) ED_{nt}$$

Poorer neighbors erode the tax base and reduce marginal product of education through human capital formation:

$$Y_{i,t+1} = T_{nt}(\tau_{nt}) \Theta(\hat{F}_{Y_{nt}})\, \xi_{i,t+1}$$

Go Back

Durlauf, 1996: Additional Implications

- Equilibrium is achieved when no family wants to withdraw from a neighborhood to form a new neighborhood, and the tax rate is supported by majority voting, and there is a core configuration of families across neighborhoods
- Either all families live in a common neighborhood or neighborhoods are stratified by wealth
- Neighborhoods are re-formed at each time period, and offspring will form this neighborhood as well Go back

Let: μ denote neighborhood size, Y denote income.
Then there exist $\bar{\bar{Y}} < \infty$ and $\bar{\mu} \leq \mu^*$
such that, if $\hat{F}_{Y,n,t}(\bar{\bar{Y}}) = 0$ and $\bar{\mu} \leq \mu\left(N_{n,t}\right)$, then

$$\frac{\pi_2}{\pi_1 + \pi_2} \cdot \frac{\mu\left(N_{n,t}\right)}{\left(\lambda_1 + \lambda_2\right)\mu\left(N_{n,t}\right)} \cdot \phi\Theta\left(\hat{F}_{Y,n,t}\right) > 1.^9$$

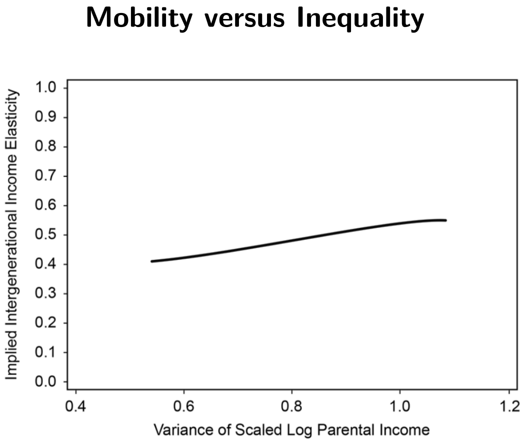
That is, the marginal change in the mean income of offspring with respect to an increase in the mean income of parents > 1. Therefor we have growth if we have sufficiently small neighborhoods.

Go Back

Durlauf 1996: Conditions for Permanent Inequality

There exist economy-wide income distributions at time t and associated population sizes such that, with positive probability, for families in some pair of neighborhoods $N_{n,t}$ and $N_{n' t}$ with $MY_{n,t}/MY_{n',t} > 1$:
(i) relative income differences are preserved over all future generations
(ii) the income ratio between rich and poor becomes arbitrarily large.
Go Back

The Gatsby Curve (Durlauf and Sheshadri, 2018)



Durlauf and Seshadri (2018)

- The graph depicts how the IGE—the marginal effect of parental income on offspring's income—responds to scaling of parental income.
- Assume that offspring income depends linearly on parental income, average and variance of tract and state income, and the interaction of parental income with these variables.
- Then predicted offspring income is regressed on scaled parental income; the regression coefficients are plotted. $Y_{offspring} = \alpha + \beta Y_{parent} + \epsilon$
- The horizontal axis displays the variance of the scaled log parental incomes.

Some more detail, Go Back

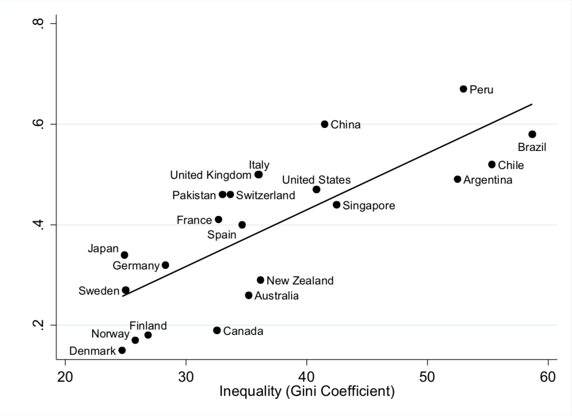
Data

- **National American Housing Survey**, 50k housing units, interviewed every two years, very detailed house-level information. Unbalanced panel. These are defined into neighborhood kernels, which are aggregated up to the neighborhood level. Use information on owner's years of schooling, whether the owner is white, marriage status, number of people in the household, household income, whether the house has changed hands in the last five years.
- **Confidential Census data**, at the MSA level. Provides demographic information, median household income, structural characteristics such as the median number of bedrooms, mobility information such as the percent of households that moved in the last five years, and tenure and vacancy statistics. There is also information on the joint distribution of some of these variables.
- Include in sample only if it: is associated with a regular occupied interview; is owner occupied; lies in a metropolitan statistical area (MSA); is valued by the owner to be at least $10,000, and is not missing any information on unit, occupant, or census tract characteristics that are included in our analysis.
- 8.3 observations per cluster (neighborhood), in about 100 MSA
- Merge the information from the 1980 and 1990 STF3s with the AHS data by census tract. Interpolate and extrapolate from the reported averages of the 1980 and 1990 STF3 data to create the tract variables for the 1985, 1989, and 1993 surveys.

Go Back

The Gatsby Curve

Mobility versus Inequality



Corak (2016)

Identification

- Impose Nash equilibrium
- Need an instrument to pull out ν
- Assume $\epsilon_{skh} \sim T1EV$, treat neighborhood choice as a discrete choice.
- IIA
- Consistency holds provided that one, independence from irrelevant alternatives holds, which is ensured by the MNL model; and two, if an alternative is included in the assigned set, then it has the logical possibility of (McFadden)

Neighborhood Choice and Housing, Ioannides and Zabel (2008)

Identification

- Assume that once neighborhood choice is controlled for, one is just left with the "structural" component of housing.
- Exogenous variation in housing prices to identify housing demand though multiple markets across time.
- Choice of housing provides a source of identification for endogenous neighborhood effect
- Identification alla Brock & Durlauf (2001)

Estimation

- Construct and estimate price and quantity of housing structure demand
- Estimate the neighborhood choice with a multinomial logit model:

  $$Prob_{s_h k_h h} = \frac{\omega_{s_h k_h} h^{\zeta_h g_{s_h}}}{\sum_{s=1}^{S_m} \sum_{k=1}^{N_s} \omega_{skh} e^{\zeta_h g_s}}$$

- Using the neighborhood choice model to correct for sample selection bias and estimate housing structure demand

$$y_{stru,mskh} = \alpha + v p_{ms,nei} + v' p_{m,stru} + \delta \ln I_h + \beta \overline{y_{stru,n(h)}} + \gamma \overline{z_{n(h)}} + v_k + E[\eta_h \mid s = s_h] + \psi_h$$

Neighborhood Choice and Housing, Ioannides and Zabel (2008)

Table 4
Multinomial logit model for choice of census tract of residence: census tract- and individual-level variables

Variable	Coefficient (1)	Std. Error (2)	Coefficient (3)	Std. Error (4)
Price	0.0009**	0.0004	0.0012***	0.0004
Median Tract Income	−0.0112**	0.0033		
Median Tract Income × Income in 1st Quartile			−0.0447**	0.0052
Median Tract Income × Income in 2nd, 3rd Quartiles			−0.0221**	0.0041
Median Tract Income × Income in 4th Quartile			−0.0030	0.0046
Median Tract Rent	−0.0011**	0.0003		
Median Tract Rent × Income in 1st Quartile			−0.0008	0.0006
Median Tract Rent × Income in 2nd, 3rd Quartiles			−0.0014**	0.0004
Median Tract Rent × Income in 4th Quartile			−0.0008	0.0006
Median Age of House	0.0048**	0.0016		
Median Age of House × Income in 1st Quartile			0.0124**	0.0029
Median Age of House × Income in 2nd, 3rd Quartile			−0.0029	0.0024
Median Age of House × Income in 4th Quartile			0.0147**	0.0030
Fraction of Vacant Units	−2.9517**	0.4040		
Fraction of Vacant Units × Income in 1st Quartile			−1.0985	0.6856
Fraction of Vacant Units × Inc in 2nd, 3rd Quartile			−3.4039**	0.5890
Fraction of Vacant Units × Income in 4th Quartile			−7.8333**	1.0788
Fraction Owners	1.9387**	0.1543		
Fraction Owners × Income in 1st Quartile			2.6062**	0.2521
Fraction Owners × Inc in 2nd, 3rd Quartile			2.0889**	0.1918
Fraction Owners × Income in 4th Quartile			1.2589**	0.2371
Fraction Non-white In Tract	0.5054**	0.1412		
Fraction Non-white In Tract × White			−0.6533**	0.1751
Fraction Non-white In Tract × Non-white			4.4078**	0.2946
Dominant Race	0.2732**	0.0873		
Dominant Race × Household Head White			−0.1862	0.1182
Dominant Race × Household Head Non-white			0.5681**	0.1893
Fraction with High School Degree in Tract	0.2863	0.2199		
Fraction with HS Degree × No HS Degree			−3.9459**	0.3479
Fraction with HS Degree × High School Degree			−0.2363	0.2642
Fraction with HS Degree × College Degree			4.0347**	0.3570
Median Number of Bedrooms	0.0772*	0.0371		
Median Beds × HH size in 1st Quartile			−0.2141**	0.0555
Median Beds × HH size in 2nd, 3rd Quartile			0.0507	0.0834
Median Beds × HH size in 4th Quartile			0.0139	0.0776
Median Beds × HH Head Married			0.2347**	0.0629
Median Age of Residents	−0.0113**	0.0035		
Med Age of Residents × Age HH Head in 1st Quartile			−0.0399**	0.0083
Med Age of Res × Age HH Head in 2nd, 3rd Quartile			−0.0197**	0.0064
Med Age of Residents × Age HH Head in 4th Quartile			0.0125$*$	0.0063
Med Age of Residents × HH Head Married			−0.0051	0.0061
Fraction Moved in Last 5 Years (FML5Y)	−0.0663	0.2035		
FML5Y × Age HH Head in 1st Quartile			0.9450**	0.3216
FML5Y × Age HH Head in 2nd, 3rd Quartiles			−0.0984	0.2619
FML5Y × Age HH Head in 4th Quartile			0.5558	0.3271
Fraction with Commute < 20 minutes	1.0514**	0.1533	1.6299**	0.2831
Fraction with Commute < 20 mins × HH Head Male			−0.5985	0.3251
Fraction Unemployed	−5.8221**	0.7005	−8.3819**	0.7578

Neighborhood Choice and Housing, Ioannides and Zabel (2008)

Table 4 (*continued*)

Variable	Coefficient (1)	Std. Error (2)	Coefficient (3)	Std. Error (4)
Fraction in Poverty	−2.3356**	0.3416	−3.4528**	0.3773
Natural Log of Tract Size	−0.0253	0.0300	−0.0146	0.0311
Observations	70,092		70,092	
Log likelihood	−14,154.6		−12,791.7	
χ^2 Significance, all Variables	0.000		0.000	
Pseudo-R^2	0.0736		0.1628	

Table 5
Estimation results for structure demand equation

Variable	1 Member per Cluster		All Cluster Members Included		
	(1)	(2)	(3)	(4)	(5)
Year is 1989	0.0275	0.0347	0.0165**	0.0231	0.0369
	(0.0246)	(0.0247)	(0.0064)	(0.0232)	(0.0224)
Year is 1993	−0.0046	0.0004	0.003	0.0007	0.0038
	(0.0226)	(0.0229)	(0.0055)	(0.0142)	(0.0141)
Mean of Observed Demand by Neighbors			0.8395**		
			(0.0141)		
Mean of Predicted Demand by Neighbors				0.8504**	0.7254**
				(0.1748)	(0.1639)
Log of Structure Price	−0.1808**	−0.1784**	−0.0644**	−0.0772	−0.1319
	(0.0284)	(0.0292)	(0.0133)	(0.0756)	(0.0714)
Log of Neighborhood Price	0.2445**	0.2086**	0.0254*	−0.0624*	−0.0443
	(0.0382)	(0.0386)	(0.0111)	(0.0312)	(0.0299)
Log of Income	0.2058**	0.2106**	0.0459**	0.0790**	0.0806**
	(0.0278)	(0.0277)	(0.0070)	(0.0073)	(0.0073)
Household Size	0.0290**	0.0292**	0.0248**	0.0243**	0.0242**
	(0.0074)	(0.0074)	(0.0017)	(0.0020)	(0.0020)
Completed High School	0.0057	0.008	0.0178*	0.0221**	0.0209**
	(0.0297)	(0.0298)	(0.0072)	(0.0078)	(0.0077)
Changed Hands in last 5 Years	−0.0365	−0.0356	0.0054	−0.0033	−0.0036
	(0.0202)	(0.0203)	(0.0049)	(0.0055)	(0.0055)
White	−0.0612*	−0.0647*	−0.0115	−0.0227*	−0.0240*
	(0.0283)	(0.0320)	(0.0095)	(0.0099)	(0.0095)
Married	−0.1209**	−0.1200**	−0.0150*	−0.0137	−0.0131
	(0.0295)	(0.0290)	(0.0069)	(0.0077)	(0.0077)
Mean of Neighbors' Log Income			0.0211	0.002	0.0609
			(0.0149)	(0.0796)	(0.0752)
Mean of Neighbors' Hhld Size			−0.0212**	−0.0188*	−0.0184
			(0.0042)	(0.0095)	(0.0095)
Pct of Neighbors Completed High School			−0.0194	−0.0195	−0.0181
			(0.0173)	(0.0389)	(0.0387)
Pct of Neighbors who Changed Hands in last 5 Years			−0.0179	−0.0148	−0.0288
			(0.0114)	(0.0301)	(0.0295)
Pct of Neighbors Non-white			0.0142	−0.0185	0.0048
			(0.0126)	(0.0348)	(0.0334)
Pct of Neighbors Married			0.0188	−0.0023	0.0003
			(0.0173)	(0.0399)	(0.0397)
Constant	−4.3003**	−4.3919**	−0.4949**	−0.2659	−0.7045
	(0.3182)	(0.3466)	(0.1044)	(0.6222)	(0.5887)
Observations	764	764	6372	6372	6372
Mean Observations per Cluster	1	1	8.3	8.3	8.3
Heckman Correction	No	Yes	Yes	Yes	No
p-value; Heckman Terms		0.0004	0.4059	0.0409	
p-value; Own Socioeconomics	0.0000	0.0000	0.0000	0.0000	0.0000
p-value; Neigh Socioeconomics			0.0000	0.3163	0.2120
R-squared Overall	0.2388	0.2652	0.6155	0.4007	0.3993
Std. Error of Random Effect				0.1355	0.1345
Standard Error of Regression	0.2434	0.2409	0.0460	0.1584	0.1586

(*continued on next page*)

Neighborhood Choice and Housing, Ioannides and Zabel (2008)

Table 5 (*continued*)

Variable	1 Member per Cluster		All Cluster Members Included		
	(1)	(2)	(3)	(4)	(5)
Pct Var due to Random Effect				0.4232	0.4197
Robust standard errors in brackets					

* Significant at 5%.
** Significant at 1%.

CH 2018a: Estimation using Observational Data

- Define $\Delta_{odps} = \bar{y}_{pds}$ - $\bar{y}_{pos}$ as the difference in mean income rank (at age 24) of permanent residents in the destination (d) location versus origin (o).
- The authors estimate:

$$y_i = \alpha_{qosm} + \sum_{m=9}^{30} b_m \mathbb{1}\{m_i = m\}\Delta_{odps} + \sum_{s=1980}^{1987} \kappa \mathbb{1}\{s_i = s\}\Delta_{odps} + \epsilon_i$$

where α_{qosm} is an (origin x parent income decile x birth cohort x age) fixed effect

- $\hat{b}_m$ is the average effect on age-24 income rank y_i, conditional on moving from o to d at age m, of a 1 percentile increase in Δ_{odps}

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CH 2018a: Estimation using Observational Data (Cont.)

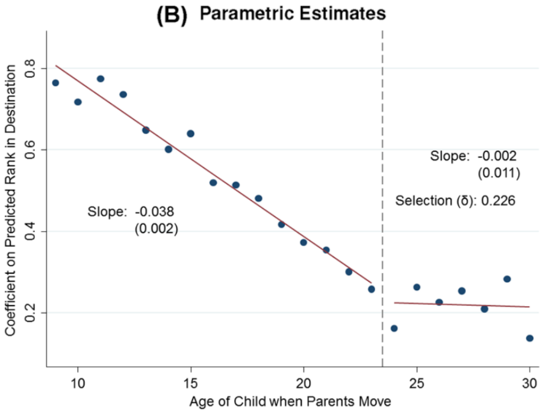
- The authors estimate:

$$y_i = \sum_{s=1980}^{1987} \mathbb{1}\{s_i = s\} \left(\alpha_s^1 + \alpha_s^2 \bar{y}_{pos}\right) + \sum_{m=9}^{30} \mathbb{1}\{m_i = m\} \left(\zeta_m^1 + \zeta_m^2 p_i\right) +$$
$$\sum_{m=9}^{30} b_m \mathbb{1}\{m_i = m\} \Delta_{odps} + \sum_{s=1980}^{1987} \kappa_s^d \mathbb{1}\{s_i = s\} \Delta_{odps} + \epsilon_i$$

  where α_{qosm} is an (origin x parent income decile x birth cohort x age) fixed effect

- Control parametrically for the two key factors captured by the α_{qosm} fixed effects: (i) the quality of the origin location, which we model by interacting the predicted outcomes for permanent residents in the origin at parent income percentile p_i with birth cohort fixed effects, and (ii) disruption costs of moving that may vary with the age at move and parent income

CH 2018a: (Parametric) Estimation results



Source: Chetty and Hendren (2018a)

- Estimation shows similar patterns as in the semi-parametric model
- Exposure effect is slightly lower (-3.8%) and selection effects are higher ($\delta = 0.226$)

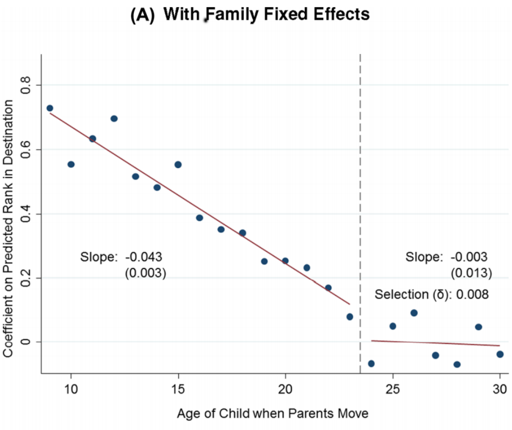
CH 2018a: Estimation using Observational Data (Cont.)

- Authors address time-varying selection possibility by adding family fixed effects:

$$y_i = \sum_{s=1980}^{1987} \mathbb{1}\{s_i = s\} \left(\alpha_s^1 + \alpha_s^2 \bar{y}_{pos}\right) + \sum_{m=9}^{30} \mathbb{1}\{m_i = m\} \left(\zeta_m^1 + \zeta_m^2 p_i\right) + \\ \sum_{m=9}^{30} b_m \mathbb{1}\{m_i = m\} \Delta_{odps} + \sum_{s=1980}^{1987} \kappa_s^d \mathbb{1}\{s_i = s\} \Delta_{odps} + \bar{\boldsymbol{\theta}}_{\boldsymbol{fam}} + \epsilon_i$$

- Regression is now estimated entirely on sample of families with 2 children. Family–level mean effects are taken out.

CH 2018a: Family FE Estimation results



Source: Chetty and Hendren (2018a)

- Similar results: children who move to a better area at younger ages have better outcomes than do their older siblings
- Selection effect falls from $\delta = 0.23$ in the baseline specification to $\delta = 0.01$ (not significantly different from 0) with family fixed effects
- Doesn't affect the slope: consistent with identification assumption

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Chetty and Hendren (2018b) - Estimation

- To estimate the causal effect of each CZ/county they start by assuming the following statistical model:
$$y_i = \alpha_{od} + e_i \cdot \mu + \epsilon_i$$
where α_{od} is a fixed effect of origin-destination; $e_i = e_{ic}$ vector whose entries denote the number of years of exposure that child i has to place c; and μ is a vector of coefficients that capture CZ exposure effect
- Assumption: conditional on α_{od} the exposure time to each place e_{ic} is orthogonal to other determinants of children's outcomes, i.e, $Cov(e_{ic}, \epsilon_i) = 0$ for all c
- Estimates for the causal effect of staying a one additional year in a neighborhood

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