ECON 2450B
Topic 6: Neighborhoods and Intergenerational Mobility

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Spring, 2018
Impact of Neighborhoods

- Last Lecture: Impact of Education / Schools on Children’s Outcomes
- This Lecture: Impact of Neighborhoods
- Large literature, esp. in Sociology, documents wide variation in outcomes for both children and adults across areas
  - Wilson (1987)
  - Massey and Denton (1993)
  - Cutler and Glaeser (1997)
  - Wodtke et al. (1999)
  - …
- Is this the result of different people living in different places or places having causal effects?
Part A: Does place matter? Yes.

Chetty and Hendren (2018): Variation in intergenerational mobility in the U.S. reflects the causal effect of exposure during childhood

Part B: What are the implications for place-based policies?

[Place-based] Improve places

E.g. Harlem Children's Zone (Dobbie and Fryer, 2011)

[Choice-based] Relax constraints faced by families choosing where to raise their children

E.g. Moving to Opportunity experiment (Chetty, Hendren, and Katz, 2016)
Part A: Does Place Matter?

- Key issue: separating causality vs. sorting
  - [Sorting] Do different types of people live in different places?
  - [Causal] Or, do places have causal effects?

- Illustrate this issue using impacts on children
  - Chetty and Hendren (2018) separate sorting versus causal story using cross-area movers
Data

- Data source: de-identified data from 1996-2012 tax returns
- Children linked to parents based on dependent claiming
- Focus on children in 1980-1993 birth cohorts
  - Approximately 50 million children
Variable Definitions

- **Parent income**: mean pre-tax household income between 1996-2000
  - For non-filers, use W-2 wage earnings + SSDI + UI income

- **Child income**: pre-tax household income at various ages

- Results robust to varying definitions of income and age at which child’s income is measured

- Focus on percentile ranks in **national** income distribution
  - Rank children relative to others in the same birth cohort
  - Rank parents relative to other parents
Defining “Neighborhoods”

- Conceptualize neighborhood effects as the sum of effects at different geographies (hierarchical model)

\[ \mu_{nbhd} = \mu_{CZ} + \mu_{County} + \mu_{Zip} + \mu_{Block} \]

- Primary estimates are at the commuting zone (CZ) and county level
  - CZ’s are aggregations of counties analogous to MSAs
    [Tolbert and Sizer 1996; Autor and Dorn 2013]

- Variance of place effects at broad geographies is a lower bound for total variance of neighborhood effects
Intergenerational Mobility by CZ

- Begin with a descriptive characterization of children’s outcomes in each CZ
  - CZ’s are aggregations of counties analogous to MSAs
    [Tolbert and Sizer 1996; Autor and Dorn 2013]

- Focus on “permanent residents” of CZs
  - Permanent residents = parents who stay in CZ c between 1996-2012
  - Note that children who grow up in CZ c may move out as adults

- Characterize relationship between child’s income rank and parent’s income rank \( p \) for each CZ c and birth cohort \( s \)
Mean Child Income Rank at Age 26 vs. Parent Income Rank for Children Born in 1985 and Raised in Chicago

Mean Child Rank in National Income Distribution

Parent Rank in National Income Distribution
Mean Child Income Rank at Age 26 vs. Parent Income Rank for Children Born in 1985 and Raised in Chicago

\[ \bar{y}_{0, \text{Chicago, 1985}} = \mathbb{E}[\text{Child Rank} \mid p = 0, c = \text{Chicago}, s = 1985] \]
Mean Child Income Rank at Age 26 vs. Parent Income Rank for Children Born in 1985 and Raised in Chicago

Predict outcome for child in CZ c using slope + intercept of rank-rank relationship

\[ \bar{y}_{p,\text{Chicago},1985} = \bar{y}_{0,\text{Chicago},1985} + (\text{Rank-Rank Slope}) \times p \]
The Geography of Intergenerational Mobility in the United States
Predicted Income Rank at Age 26 for Children with Parents at 25th Percentile
The Geography of Intergenerational Mobility in the United States
Predicted Income Rank at Age 26 for Children with Parents at 25th Percentile
Question 1: What happens if you move to a lighter-shade county?
Question 2: Decompose map into sorting and causal effect for each county.
Question 1: Neighborhood Exposure Effects

- Analyze childhood **exposure effects**

  - Exposure effect at age $m$: impact of spending year $m$ of childhood in an area where permanent residents’ outcomes are 1 percentile higher

- Ideal experiment: randomly assign children to new neighborhoods $d$ starting at age $m$ for the rest of childhood

  - Regress income in adulthood ($y_i$) on mean outcomes of prior residents:

    $$ y_i = \alpha + \beta_m \bar{y}_{pds} + \epsilon_i \quad (1) $$

  - Exposure effect at age $m$ is $\beta_{m-1} - \beta_m$
Chetty and Hendren (2016) estimate exposure effects by studying families that move across CZ’s with children at different ages in observational data.

Key problem: choice of neighborhood is likely to be correlated with children’s potential outcomes.

Ex: parents who move to a good area may have latent ability or wealth ($\theta_i$) that produces better child outcomes.

Estimating (1) in observational data yields a coefficient

$$b_m = \beta_m + \delta_m$$

where

$$\delta_m = \frac{\text{Cov}(\theta_i, \bar{y}_{pds})}{\text{var}(\bar{y}_{pds})}$$

is a standard selection effect.
But identification of exposure effects does not require that *where* people move is orthogonal to child’s potential outcomes.

Instead, requires that *timing* of move to better (vs. worse) area is orthogonal to child’s potential outcomes.

**Assumption 1.** Selection effects do not vary with child’s age at move:

\[ \delta_m = \delta \text{ for all } m \]

Certainly plausible that this assumption could be violated.

- Ex: parents who move to better areas when kids are young may have better unobservables

Will evaluate this assumption in detail after baseline results.
To begin, consider subset of families who move with a child who is exactly 13 years old.

Regress child's income rank at age 26 $y_i$ on predicted outcome of permanent residents in destination:

$$y_i = \alpha_{qos} + b_m \bar{y}_{pds} + \eta_{1i}$$

Include parent decile (q) by origin (o) by birth cohort (s) fixed effects to identify $b_m$ purely from differences in destinations.
Movers’ Outcomes vs. Predicted Outcomes Based on Residents in Destination
Child Age 13 at Time of Move, Income Measured at Age 26

Mean (Residual) Child Rank in National Income Distribution

Predicted Diff. in Child Rank Based on Permanent Residents in Dest. vs. Orig.

Slope: $b_{13} = 0.628 
(0.048)$
Movers’ Outcomes vs. Predicted Outcomes Based on Residents in Destination
By Child’s Age at Move, Income Measured at Age = 24

Coefficient on Predicted Rank in Destination

Slope: -0.038 (0.002)

Slope: -0.002 (0.011)

δ: 0.226
Movers’ Outcomes vs. Predicted Outcomes Based on Residents in Destination
By Child’s Age at Move, Income Measured at Age = 24

Coefficient on Predicted Rank in Destination

Assumption 1: \( \delta_m = \delta \) for all \( m \)

\[ \rightarrow \text{Causal effect of moving at age } m \text{ is } \beta_m = b_m - \delta \]
Identifying Causal Exposure Effect

- Key identification assumption: *timing* of moves to better/worse areas uncorrelated with child’s potential outcomes

- Primary contribution of the paper is to provide evidence in support of this identification condition in observational data

  - Without existence of an “instrument”

- Two main concerns (Jencks and Mayer, 1990)
  1. Sorting of families to different areas
  2. Shocks driving movement to different areas

- Begin with within-family design
Family Fixed Effects: Sibling Comparisons

Slope (Age ≤ 23): -0.043 (0.003)

Slope (Age > 23): -0.003 (0.013)

Coefficient on Predicted Rank in Destination ($b_m$)
Family Fixed Effects: Sibling Comparisons
with Controls for Change in Income and Marital Status at Move

Slope (Age ≤ 23): -0.042 (0.003)
Slope (Age > 23): -0.003 (0.013)

Coefficient on Predicted Rank in Destination

Age of Child when Parents Move (m)
Family fixed effects do not rule out time-varying unobservables that affect children in proportion to exposure time.

Wealth shocks

“Parental capital” shocks correlated with where you move.

Key challenge faced by previous observational studies that have analyzed movers to identify nbhd. effects [e.g., Aaronson 1998].
Prior observational studies of movers define “good” neighborhoods based on observable characteristics (e.g., low poverty rates).

Chetty and Hendren (2016) approach differs by measuring neighborhood quality based on outcomes of permanent residents, analogous to value-added models.

- Generates sharp predictions that allow us to distinguish causal effects of neighborhoods from other factors.
Outcome-Based Placebo Tests

- General idea: exploit heterogeneity in place effects across subgroups to obtain overidentification tests of exposure effect model

- Start with variation in place effects across birth cohorts
  - Some areas are getting better over time, others are getting worse
  - Causal effect of neighborhood on a child who moves in to an area should depend on properties of that area while he is growing up
Outcome-Based Placebo Tests

- General idea: exploit heterogeneity in place effects across subgroups to obtain overidentification tests of exposure effect model

- Start with variation in place effects across birth cohorts
  - Some areas are getting better over time, others are getting worse
  - Causal effect of neighborhood on a child who moves in to an area should depend on properties of that area while he is growing up

- Parents choose neighborhoods based on their preferences and information set at time of move
  - Difficult to predict high-frequency differences for outcomes 15 years later
  - Unlikely unobs. shock $\theta_i$ replicates cohort variation perfectly
Estimates of Exposure Effects Based on Cross-Cohort Variation

Years Relative to Own Cohort

Exposure Effect Estimate ($\beta$)
Estimates of Exposure Effects Based on Cross-Cohort Variation

Exposure Effect Estimate ($\beta$)

Years Relative to Own Cohort

Simultaneous
Separate
Next, implement an analogous set of placebo tests by exploiting heterogeneity across realized distribution of incomes.

Areas differ not just in mean child outcomes but also across distribution.

Boston and San Francisco generate similar mean outcomes for children with parents at 25\textsuperscript{th} percentile, but more children in SF reach tails (top 10%, bottom 10%).

Exposure model predicts convergence to permanent residents’ outcomes not just on means but across entire distribution.

Children who move to SF at younger ages should be more likely to end up in tails than those who move to Boston.

Again, unlikely that unobserved factor $\theta_i$ would replicate distribution of outcomes in each destination area in proportion to exposure time.
## Exposure Effects on Upper-Tail and Lower-Tail Outcomes
Comparisons of Impacts at P90 and Non-Employment

<table>
<thead>
<tr>
<th>Dependent Variable</th>
<th>Child Rank in top 10%</th>
<th>Child Employed</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>(1)</td>
<td>(2)</td>
</tr>
<tr>
<td>Distributional Prediction</td>
<td>0.043</td>
<td>0.040</td>
</tr>
<tr>
<td></td>
<td>(0.002)</td>
<td>(0.003)</td>
</tr>
<tr>
<td>Mean Rank Prediction (Placebo)</td>
<td>0.022</td>
<td>0.004</td>
</tr>
<tr>
<td></td>
<td>(0.002)</td>
<td>(0.003)</td>
</tr>
</tbody>
</table>
Gender Comparisons

- Finally, exploit heterogeneity across genders
- Construct separate predictions of expected income rank conditional on parent income for girls and boys in each CZ
- Correlation of male and female predictions across CZ’s is 0.90
- Low-income boys do worse than girls in areas with:
  1. More segregation (concentrated poverty)
  2. Higher rates of crime
  3. Lower marriage rates [Autor and Wasserman 2013]
- If unobservable input $\theta_i$ does not covary with gender-specific neighborhood effect, can use gender differences to conduct a placebo test
<table>
<thead>
<tr>
<th></th>
<th>No Family Fixed Effects</th>
<th>Family Fixed Effects</th>
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<tbody>
<tr>
<td></td>
<td>(1)</td>
<td>(2)</td>
</tr>
<tr>
<td>Own Gender Prediction</td>
<td>0.038</td>
<td>0.031</td>
</tr>
<tr>
<td></td>
<td>(0.002)</td>
<td>(0.003)</td>
</tr>
<tr>
<td>Other Gender Prediction</td>
<td></td>
<td></td>
</tr>
<tr>
<td>(Placebo)</td>
<td>0.034</td>
<td>0.009</td>
</tr>
<tr>
<td></td>
<td>(0.002)</td>
<td>(0.003)</td>
</tr>
<tr>
<td>Sample</td>
<td>Full Sample</td>
<td></td>
</tr>
</tbody>
</table>
Any omitted variable $\theta_i$ that generates bias in the exposure effect estimates would have to:

1. Operate within family in proportion to exposure time

2. Be fully orthogonal to changes in parent income and marital status over 17 years

3. Replicate prior residents’ outcomes by birth cohort, quantile, and gender in proportion to exposure time *conditional* on other predictions

4. Replicate impacts across outcomes (income, college attendance, teen labor, marriage)

Unlikely?
Part 2: Causal Effects of Each County

- Estimate causal effects of each county and CZ in the U.S. on children’s earnings in adulthood

- Estimate ~3,000 treatment effects (one per county) instead of one average exposure effect as in first paper
Estimating County Fixed Effects

- Begin by estimating effect of each county using a fixed effects model that is identified using variation in timing of moves between areas.

- Intuition for identification: suppose children who move from Manhattan to Queens at younger ages earn more as adults.

- Can infer that Queens has positive exposure effects relative to Manhattan.
Estimating County Fixed Effects

- Estimate place effects $\mu = (\mu_1, ..., \mu_N)$ using fixed effects for origin and destination interacted with exposure time:

$$y_i = (T_c - m) \left[ \mu_d 1 \{d(i) = d\} - \mu_o 1 \{o(i) = o\} \right] + \alpha_{odps} + \eta_i$$

- Place effects are allowed to vary linearly with parent income rank:

$$\mu_c = \mu^0_c + \mu^P_c P$$

- Include origin-by-destination fixed effects to isolate variation in exposure

- What is the identification condition?
Causal Effect Estimates vs. Permanent Resident Outcomes
Income Rank at Age 26 for Children with Parents at 25th Percentile
Causal Effect Estimates vs. Permanent Resident Outcomes
Income Rank at Age 26 for Children with Parents at 25th Percentile

Causal Effect of 1 Year of Exposure on Child’s Rank

\[ \hat{\mu}_c \]

Mean Percentile Rank of Children of Permanent Residents

Income Rank at Age 26 for Children with Parents at 25th Percentile
Causal Effect Estimates vs. Permanent Resident Outcomes

Income Rank at Age 26 for Children with Parents at 25th Percentile

Causal Effect of 1 Year of Exposure on Child’s Rank

Mean Percentile Rank of Children of Permanent Residents

$\gamma \overline{y}_c$
Causal Effect Estimates vs. Permanent Resident Outcomes

Income Rank at Age 26 for Children with Parents at 25th Percentile

Movers in Cleveland do better than would be predicted based on permanent resident outcomes

\[ \hat{\mu}_c \]

\[ \gamma \bar{y}_c \]

\[
\begin{align*}
\text{Movers in Cleveland do better than would be predicted based on permanent resident outcomes.}
\end{align*}
\]
Causal Effect Estimates vs. Permanent Resident Outcomes
Income Rank at Age 26 for Children with Parents at 25th Percentile

Two explanations:

Two explanations:

\( \hat{\mu}_c \)

Causal Effect of 1 Year of Exposure on Child's Rank

Mean Percentile Rank of Children of Permanent Residents

\( \gamma \tilde{y}_c \)
Causal Effect Estimates vs. Permanent Resident Outcomes
Income Rank at Age 26 for Children with Parents at 25th Percentile

Two explanations:
1. Causal effects are noisy, $\hat{\mu}_c \neq \mu_c$
Causal Effect Estimates vs. Permanent Resident Outcomes
Income Rank at Age 26 for Children with Parents at 25th Percentile

Two explanations:
1. Causal effects are noisy $\hat{\mu}_c \neq \mu_c$
2. Permanent residents are biased $\mu_c \neq \gamma \bar{y}_c$

$x$-axis: Mean Percentile Rank of Children of Permanent Residents
$y$-axis: Causal Effect of 1 Year of Exposure on Child’s Rank
Causal Effect Estimates vs. Permanent Resident Outcomes
Income Rank at Age 26 for Children with Parents at 25th Percentile

Mean Percentile Rank of Children of Permanent Residents

Causal Effect of 1 Year of Exposure on Child's Rank: 0.5 - 0.5
Causal Effect Estimates vs. Permanent Resident Outcomes
Income Rank at Age 26 for Children with Parents at 25th Percentile

Classic bias-variance tradeoff: noisy causal effect estimates but biased (precise) predictions based on permanent residents
Three Objectives

Use fixed effect estimates for three purposes:

1. Quantify the size of place effects: how much do places matter?
2. Construct forecasts that can be used to guide families seeking to “move to opportunity”
3. Characterize which types of areas produce better outcomes to provide guidance for place-based policies
Objective 1: Magnitude of Place Effects

- Can we just look at the variance of fixed effect estimates, $\hat{\mu}_c$?

- No….we can write: $\hat{\mu}_c = \mu_c + \varepsilon_c$ where $\varepsilon_c$ is orthogonal sampling error

- Total variance has two components:

$$Var(\hat{\mu}_c) = Var(\mu_c) + Var(\varepsilon_c)$$

- Let $s_c$ be the std error of the causal effect in place c, $E[\varepsilon_c^2 | s_c] = s_c^2$

- So,

$$Var(\varepsilon_c) = E[\varepsilon_c^2] = E_c \left[ E[\varepsilon_c^2 | s_c] \right] = E_c \left[ s_c^2 \right]$$

- Variance of true place effects is given by

$$Var(\mu_c) = Var(\hat{\mu}_c) - E_c \left[ s_c^2 \right]$$

Total \quad Noise
Objective 1: Magnitude of Place Effects

Chetty and Hendren (2016) estimate across counties for parents at 25\textsuperscript{th} percentile:

\[ Var(\hat{u}_c) = 0.434 \quad E_c\left[s_c^2\right] = 0.402 \]

So, \( Var(\mu_c) = 0.032 \) or \( Std(\mu_c) = 0.18 \)

1 year of exposure to a 1SD better place increases earnings by 0.18 percentiles

To interpret units, note that 1 percentile \( \approx \) 3\% change in earnings

For children with parents at 25\textsuperscript{th} percentile: 1 SD better county from birth (20 years) \( \rightarrow \) 3.6 percentiles \( \rightarrow \) 10\% earnings gain
Objective 2: Forecasts of Best and Worst Areas

- What are the best and worst places to grow up?
- Construct forecasts that minimize mean-squared-error of predicted impact for a family moving to a new area
- Raw fixed effect estimates have high MSE because of sampling error
- Reduce MSE by combining fixed effects (unbiased, but imprecise) with permanent resident outcomes (biased, but precise)
- Common approach in recent literature:
Optimal Forecasts Combining Fixed Effects and Permanent Resident Outcomes

Causal Effect of 1 Year of Exposure on Child's Rank

Mean Percentile Rank of Children of Permanent Residents

- Cleveland
- Chicago
- New York
- Santa Barbara
Causal Effect of 1 Year of Exposure on Child's Rank

Mean Percentile Rank of Children of Permanent Residents

Causal effect point estimates, $\hat{\mu}_c$, are noisy

Optimal Forecasts Combining Fixed Effects and Permanent Resident Outcomes
Causal Effect of 1 Year of Exposure on Child's Rank

Mean Percentile Rank of Children of Permanent Residents

Optimal Forecasts Combining Fixed Effects and Permanent Resident Outcomes

Use forecasts based on permanent residents, $\overline{Y}_c$.
Optimal Forecasts of Place Effects

To derive optimal forecast, consider hypothetical experiment of randomly assigning children from an average place to new places.

- Regress outcomes \( y_i \) on fixed-effect estimate, \( \hat{\mu}_c \), and stayers prediction, \( \gamma \bar{y}_c \), where \( \bar{y}_c \) is de-meaned across places.

\[
y_i = \alpha + \rho_{1,c} (\gamma \bar{y}_c) + \rho_{2,c} \hat{\mu}_c + \eta_i
\]

Part 1 shows that \( E[y_i | \bar{y}_c] = \gamma \bar{y}_c \), so that the regression coeffs are:

\[
\rho_{1,c} = \frac{\sigma_{bias}^2}{\sigma_{noise,c}^2 + \sigma_{bias}^2} \quad \quad \rho_{2,c} = \frac{\sigma_{noise,c}^2}{\sigma_{noise,c}^2 + \sigma_{bias}^2}
\]

where:

- \( \sigma_{bias}^2 = Var(\mu_c - \gamma \bar{y}_c) \) is residual variance of fixed effects.
- \( \sigma_{noise,c}^2 = \sigma_c^2 \) is the noise variance of the fixed effects (=square of std error).
Causal Effect of 1 Year of Exposure on Child's Rank

Mean Percentile Rank of Children of Permanent Residents

Optimal forecast is weighted avg. of fixed effect estimate and permanent resident outcome, with weight proportional to precision of fixed effect.
Causal Effect of 1 Year of Exposure on Child's Rank

Mean Percentile Rank of Children of Permanent Residents

Optimal Forecasts Combining Fixed Effects and Permanent Resident Outcomes

Predictions are forecast unbiased: 1pp higher predictions $\rightarrow$ 1pp higher causal effect on average
Optimal Forecasts of Place Effects

- To derive optimal forecast, consider hypothetical experiment of randomly assigning children from an average place to new places.

- Regress outcomes $y_i$ on fixed-effect estimate, $\hat{\mu}_c$, and stayers prediction, $\gamma \bar{y}_c$, where $\bar{y}_c$ is de-meaned across places.

\[
y_i = \alpha + \rho_{1,c} \left( \gamma \bar{y}_c \right) + \rho_{2,c} \hat{\mu}_c + \eta_i
\]

- Part 1 shows that $E\left[ y_i \mid \bar{y}_c \right] = \gamma \bar{y}_c$, so that the regression coeffs are:

\[
\rho_{1,c} = \frac{\sigma_{bias}^2}{\sigma_{noise,c}^2 + \sigma_{bias}^2} \quad \rho_{2,c} = \frac{\sigma_{noise,c}^2}{\sigma_{noise,c}^2 + \sigma_{bias}^2}
\]

where:

- $\sigma_{bias}^2 = Var\left( \mu_c - \gamma \bar{y}_c \right)$ is residual variance of fixed effects (constant across places).

- $\sigma_{noise,c}^2 = s_c^2$ is the noise variance of the fixed effects (varies across places).
Causal Effects of Growing up in Different Counties on Earnings in Adulthood For Children in Low-Income (25th Percentile) Families in the Chicago Metro Area

DuPage: +0.76% per year
Cook: -0.67% per year

Insufficient Data
Causal Effects of Growing up in Different Counties on Earnings in Adulthood
For Children in Low-Income (25\textsuperscript{th} Percentile) Families in the Chicago Metro Area

DuPage: +0.76\% per year
Cook: -0.67\% per year

20 Years of Exposure to DuPage vs. Cook County generates \~30\% increase in earnings
Exposure Effects on Income in the New York CSA
For Children with Parents at 25th Percentile of Income Distribution

Causal Exposure Effects Per Year:
Bronx NY: - 0.54 %
Bergen NJ: + 0.69 %
Exposure Effects on Income in the New York CSA
For Children with Parents at 75\textsuperscript{th} Percentile of Income Distribution

Causal Exposure Effects Per Year:
Bronx NY: - 0.42 %
Bergen NJ: + 0.31 %
Exposure Effects on Income in the Boston CSA
For Children with Parents at 25th Percentile of Income Distribution

Causal Exposure Effects Per Year:
Suffolk MA: - 0.31 %
Middlesex MA: + 0.39 %
Causal Exposure Effects Per Year:
Suffolk MA: - 0.18 %
Middlesex MA: + 0.03 %

Exposure Effects on Income in the Boston CSA
For Children with Parents at 75\textsuperscript{th} Percentile of Income Distribution
## Annual Exposure Effects on Income for Children in Low-Income Families (p25)

Top 10 and Bottom 10 Among the 100 Largest Counties in the U.S.

<table>
<thead>
<tr>
<th>Rank</th>
<th>Top 10 Counties</th>
<th>Annual Exposure Effect (%)</th>
<th>Bottom 10 Counties</th>
<th>Annual Exposure Effect (%)</th>
</tr>
</thead>
<tbody>
<tr>
<td>1</td>
<td>Dupage, IL</td>
<td>0.80</td>
<td>91 Wayne, MI</td>
<td>-0.57</td>
</tr>
<tr>
<td>2</td>
<td>Fairfax, VA</td>
<td>0.75</td>
<td>92 Orange, FL</td>
<td>-0.61</td>
</tr>
<tr>
<td>3</td>
<td>Snohomish, WA</td>
<td>0.70</td>
<td>93 Cook, IL</td>
<td>-0.64</td>
</tr>
<tr>
<td>4</td>
<td>Bergen, NJ</td>
<td>0.69</td>
<td>94 Palm Beach, FL</td>
<td>-0.65</td>
</tr>
<tr>
<td>5</td>
<td>Bucks, PA</td>
<td>0.62</td>
<td>95 Marion, IN</td>
<td>-0.65</td>
</tr>
<tr>
<td>6</td>
<td>Norfolk, MA</td>
<td>0.57</td>
<td>96 Shelby, TN</td>
<td>-0.66</td>
</tr>
<tr>
<td>7</td>
<td>Montgomery, PA</td>
<td>0.49</td>
<td>97 Fresno, CA</td>
<td>-0.67</td>
</tr>
<tr>
<td>8</td>
<td>Montgomery, MD</td>
<td>0.47</td>
<td>98 Hillsborough, FL</td>
<td>-0.69</td>
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<tr>
<td>9</td>
<td>King, WA</td>
<td>0.47</td>
<td>99 Baltimore City, MD</td>
<td>-0.70</td>
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<tr>
<td>10</td>
<td>Middlesex, NJ</td>
<td>0.46</td>
<td>100 Mecklenburg, NC</td>
<td>-0.72</td>
</tr>
</tbody>
</table>

Exposure effects represent % change in adult earnings per year of childhood spent in county.
Annual Exposure Effects on Income for Children in High-Income Families (p75)

Top 10 and Bottom 10 Among the 100 Largest Counties in the U.S.

<table>
<thead>
<tr>
<th>Rank</th>
<th>County</th>
<th>Annual Exposure Effect (%)</th>
<th>Rank</th>
<th>County</th>
<th>Annual Exposure Effect (%)</th>
</tr>
</thead>
<tbody>
<tr>
<td>1</td>
<td>Fairfax, VA</td>
<td>0.55</td>
<td>91</td>
<td>Hillsborough, FL</td>
<td>-0.40</td>
</tr>
<tr>
<td>2</td>
<td>Westchester, NY</td>
<td>0.34</td>
<td>92</td>
<td>Bronx, NY</td>
<td>-0.42</td>
</tr>
<tr>
<td>3</td>
<td>Hudson, NJ</td>
<td>0.33</td>
<td>93</td>
<td>Broward, FL</td>
<td>-0.46</td>
</tr>
<tr>
<td>4</td>
<td>Hamilton, OH</td>
<td>0.32</td>
<td>94</td>
<td>Dist. of Columbia, DC</td>
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<tr>
<td>5</td>
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<td>6</td>
<td>Gwinnett, GA</td>
<td>0.31</td>
<td>96</td>
<td>San Bernadino, CA</td>
<td>-0.51</td>
</tr>
<tr>
<td>7</td>
<td>Norfolk, MA</td>
<td>0.31</td>
<td>97</td>
<td>Riverside, CA</td>
<td>-0.51</td>
</tr>
<tr>
<td>8</td>
<td>Worcester, MA</td>
<td>0.27</td>
<td>98</td>
<td>Los Angeles, CA</td>
<td>-0.52</td>
</tr>
<tr>
<td>9</td>
<td>Franklin, OH</td>
<td>0.24</td>
<td>99</td>
<td>New York, NY</td>
<td>-0.57</td>
</tr>
<tr>
<td>10</td>
<td>Kent, MI</td>
<td>0.23</td>
<td>100</td>
<td>Palm Beach, FL</td>
<td>-0.65</td>
</tr>
</tbody>
</table>

Exposure effects represent % change in adult earnings per year of childhood spent in county.
### Annual Exposure Effects on Income for Children in Low-Income Families (p25)

#### Male Children

<table>
<thead>
<tr>
<th>Rank</th>
<th>County</th>
<th>Annual Exposure Effect (%)</th>
<th>Rank</th>
<th>County</th>
<th>Annual Exposure Effect (%)</th>
</tr>
</thead>
<tbody>
<tr>
<td>1</td>
<td>Bucks, PA</td>
<td>0.84</td>
<td>91</td>
<td>Milwaukee, WI</td>
<td>-0.74</td>
</tr>
<tr>
<td>2</td>
<td>Bergen, NJ</td>
<td>0.83</td>
<td>92</td>
<td>New Haven, CT</td>
<td>-0.75</td>
</tr>
<tr>
<td>3</td>
<td>Contra Costa, CA</td>
<td>0.72</td>
<td>93</td>
<td>Bronx, NY</td>
<td>-0.76</td>
</tr>
<tr>
<td>4</td>
<td>Snohomish, WA</td>
<td>0.70</td>
<td>94</td>
<td>Hillsborough, FL</td>
<td>-0.81</td>
</tr>
<tr>
<td>5</td>
<td>Norfolk, MA</td>
<td>0.62</td>
<td>95</td>
<td>Palm Beach, FL</td>
<td>-0.82</td>
</tr>
<tr>
<td>6</td>
<td>Dupage, IL</td>
<td>0.61</td>
<td>96</td>
<td>Fresno, CA</td>
<td>-0.84</td>
</tr>
<tr>
<td>7</td>
<td>King, WA</td>
<td>0.56</td>
<td>97</td>
<td>Riverside, CA</td>
<td>-0.85</td>
</tr>
<tr>
<td>8</td>
<td>Ventura, CA</td>
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<td>98</td>
<td>Wayne, MI</td>
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<tr>
<td>9</td>
<td>Hudson, NJ</td>
<td>0.52</td>
<td>99</td>
<td>Pima, AZ</td>
<td>-1.15</td>
</tr>
<tr>
<td>10</td>
<td>Fairfax, VA</td>
<td>0.46</td>
<td>100</td>
<td>Baltimore City, MD</td>
<td>-1.39</td>
</tr>
</tbody>
</table>

Exposure effects represent % change in adult earnings per year of childhood spent in county.
<table>
<thead>
<tr>
<th>Rank</th>
<th>County</th>
<th>Annual Exposure Effect (%)</th>
<th>Rank</th>
<th>County</th>
<th>Annual Exposure Effect (%)</th>
</tr>
</thead>
<tbody>
<tr>
<td>1</td>
<td>Dupage, IL</td>
<td>0.91</td>
<td>91</td>
<td>Hillsborough, FL</td>
<td>-0.51</td>
</tr>
<tr>
<td>2</td>
<td>Fairfax, VA</td>
<td>0.76</td>
<td>92</td>
<td>Fulton, GA</td>
<td>-0.58</td>
</tr>
<tr>
<td>3</td>
<td>Snohomish, WA</td>
<td>0.73</td>
<td>93</td>
<td>Suffolk, MA</td>
<td>-0.58</td>
</tr>
<tr>
<td>4</td>
<td>Montgomery, MD</td>
<td>0.68</td>
<td>94</td>
<td>Orange, FL</td>
<td>-0.60</td>
</tr>
<tr>
<td>5</td>
<td>Montgomery, PA</td>
<td>0.58</td>
<td>95</td>
<td>Essex, NJ</td>
<td>-0.64</td>
</tr>
<tr>
<td>6</td>
<td>King, WA</td>
<td>0.57</td>
<td>96</td>
<td>Cook, IL</td>
<td>-0.64</td>
</tr>
<tr>
<td>7</td>
<td>Bergen, NJ</td>
<td>0.56</td>
<td>97</td>
<td>Franklin, OH</td>
<td>-0.64</td>
</tr>
<tr>
<td>8</td>
<td>Salt Lake, UT</td>
<td>0.51</td>
<td>98</td>
<td>Mecklenburg, NC</td>
<td>-0.74</td>
</tr>
<tr>
<td>9</td>
<td>Contra Costa, CA</td>
<td>0.47</td>
<td>99</td>
<td>New York, NY</td>
<td>-0.75</td>
</tr>
<tr>
<td>10</td>
<td>Middlesex, NJ</td>
<td>0.47</td>
<td>100</td>
<td>Marion, IN</td>
<td>-0.77</td>
</tr>
</tbody>
</table>

Exposure effects represent % change in adult earnings per year of childhood spent in county.
Characteristics of Good Areas

- Are correlations documented in prior studies driven by causal effects?
  - Ex: children who grow up in “ghettos” with concentrated poverty have worse outcomes [Massey and Denton 1993, Cutler and Glaeser 1997]
  - Is growing up in a segregated area actually bad for a child or do people who live in segregated areas have worse unobservables?

- Correlate fixed effect estimates with observable characteristics of areas
Characteristics of Good Areas

- Decompose observed rank for stayers ($y_{pc}$) into causal and sorting components by multiplying annual exposure effect $\mu_{pc}$ by 20:
  
  - Causal component = $20\mu_{pc}$
  
  - Sorting component = $y_{pc} - 20\mu_{pc}$

- Regress $y_{pc}$, causal, and sorting components on covariates

  - Standardize covariates so units represent impact of 1 SD change in covariate on child’s percentile rank
Predictors of Causal Effects For Children at the CZ Level (p25)

Effect of 1 SD Increase in Covariate on Child’s Expected Percentile Rank

Racial Seg. Theil Index

Permanent Residents
Theil Index
Predictors of Causal Effects For Children at the CZ Level (p25)

- **Racial Seg. Theil Index**: Causal Correlation = -0.51
- **Gini Coeff**: Causal Correlation = -0.77
- **Top 1% Share**: Causal Correlation = -0.49
- **Social Capital Index**: Causal Correlation = 0.70
- **Dropout Rate**: Causal Correlation = -0.55

Effect of 1 SD Increase in Covariate on Child’s Expected Percentile Rank
The bar chart shows the effect of a 1 SD increase in each covariate on the child's expected percentile rank. The predictors include:

- **Racial Seg. Theil Index**: Causal Correlation = -0.51
- **Gini Coeff**: Causal Correlation = -0.77
- **Top 1% Share**: Causal Correlation = -0.49
- **Social Capital Index**: Causal Correlation = 0.70
- ** Dropout Rate**: Causal Correlation = -0.55
- **Frac. Single Moms**: Causal Correlation = -0.57
- **Frac. Foreign Born**: Causal Correlation = -0.45
- **Frac. Black Residents**: Causal Correlation = -0.51

The text notes:

- Immigrants live in worse areas but are positively selected (e.g., New York)
Predictors of Causal Effects For Children at the CZ Level (p25)

Racial Seg. Theil Index  
Gini Coeff  
Top 1% Share  
Social Capital Index  
Dropout Rate  
Frac. Single Moms  
Frac. Foreign Born  
Frac. Black Residents

Effect of 1 SD Increase in Covariate on Child’s Expected Percentile Rank

1/5 of black-white earnings gap explained by differences in the counties where black and white children grow up
Part B: Implications for Place-Based Policy

- Place matters for children’s outcomes

- Two types of potential policy implications:
  
  "Place based"
  
  - Policies that change places
    
    - e.g. investment in schools, community centers, etc.
  
  "Choice based"
  
  - Change the allocation of people to places
    
    - E.g. housing vouchers ("Section 8")
Place-Based Policy: Harlem Children’s Zone

- Enormously difficult to estimate the causal effect of place-based policy
  - Need to randomize at the place level

- Nice Example: Harlem Children’s Zone
  - Aimed to change entire neighborhood of Harlem
  - Bundle of services from birth to college (schools, community programs, …)
  - Expanded from their original 24-block area in central Harlem to a 64-block area in 2004 and a 97-block area in 2007

- Dobbie and Fryer (2011) estimate impact on test scores
  - Use lottery and distance instruments
### Table 3
Middle School Results

<table>
<thead>
<tr>
<th></th>
<th>Lottery RF</th>
<th>Lottery FS</th>
<th>Lottery 2SLS</th>
<th>Distance 2SLS</th>
</tr>
</thead>
<tbody>
<tr>
<td><strong>Math</strong></td>
<td>0.284***</td>
<td>1.240***</td>
<td>0.229***</td>
<td>0.206**</td>
</tr>
<tr>
<td></td>
<td>(0.050)</td>
<td>(0.075)</td>
<td>(0.037)</td>
<td>(0.092)</td>
</tr>
<tr>
<td><strong>ELA</strong></td>
<td>0.059</td>
<td>1.241***</td>
<td>0.047</td>
<td>-0.053</td>
</tr>
<tr>
<td></td>
<td>(0.041)</td>
<td>(0.074)</td>
<td>(0.033)</td>
<td>(0.049)</td>
</tr>
<tr>
<td><strong>Absences</strong></td>
<td>-2.783***</td>
<td>1.260***</td>
<td>-2.199***</td>
<td>-0.220</td>
</tr>
<tr>
<td></td>
<td>(0.833)</td>
<td>(0.079)</td>
<td>(0.650)</td>
<td>(2.544)</td>
</tr>
<tr>
<td><strong>On Grade Level</strong></td>
<td>-0.003</td>
<td>1.240***</td>
<td>-0.002</td>
<td>-0.011</td>
</tr>
<tr>
<td></td>
<td>(0.022)</td>
<td>(0.075)</td>
<td>(0.017)</td>
<td>(0.036)</td>
</tr>
<tr>
<td><strong>Observations</strong></td>
<td>1449</td>
<td>1449</td>
<td>1449</td>
<td>41029</td>
</tr>
</tbody>
</table>
Place-Based Policy: Harlem Children’s Zone

- Is this neighborhoods or schools?

- Exploit geographic boundary for services aside from school
  - More services in original HCZ location

- Look at heterogeneous impact of schools on test scores for those inside and outside the neighborhood boundary
Results Suggest Similar Effects for Kids Inside vs. Outside Original HCZ

Table 7
Middle School In and Out of the Zone

<table>
<thead>
<tr>
<th></th>
<th>In Zone</th>
<th>Out of Zone</th>
<th>Observations</th>
</tr>
</thead>
<tbody>
<tr>
<td>Math</td>
<td>0.201***</td>
<td>0.241***</td>
<td>0.468</td>
</tr>
<tr>
<td></td>
<td>(0.051)</td>
<td>(0.042)</td>
<td></td>
</tr>
<tr>
<td>ELA</td>
<td>0.067</td>
<td>0.039</td>
<td>0.577</td>
</tr>
<tr>
<td></td>
<td>(0.045)</td>
<td>(0.037)</td>
<td></td>
</tr>
<tr>
<td>Absences</td>
<td>−1.300</td>
<td>−2.601***</td>
<td>0.183</td>
</tr>
<tr>
<td></td>
<td>(1.003)</td>
<td>(0.683)</td>
<td></td>
</tr>
<tr>
<td>On Grade Level</td>
<td>0.013</td>
<td>−0.009</td>
<td>0.414</td>
</tr>
<tr>
<td></td>
<td>(0.024)</td>
<td>(0.020)</td>
<td></td>
</tr>
<tr>
<td>Observations</td>
<td>471</td>
<td>1038</td>
<td></td>
</tr>
</tbody>
</table>
Results:

Winning the lottery to enter the HCZ dramatically alters test scores

- Closes half the gap in white-black test scores!

Similar effects for those inside and outside original HCZ boundary

- Suggests schools can explain much of the impact

- What about baseline level differences inside and outside the zone?
Place vs. Choice Based Policy

- HCV improves children’s outcomes
  - Suggests can improve places

- Other policy: provide families opportunities to move to better neighborhoods
  - Moving to Opportunity Experiment
Choice-Based Policy: Moving to Opportunity

- HUD Moving to Opportunity Experiment implemented from 1994-1998
- 4,600 families at 5 sites: Baltimore, Boston, Chicago, LA, New York
- Families randomly assigned to one of three groups:
  1. Experimental: housing vouchers restricted to low-poverty (<10%) Census tracts
  2. Section 8: conventional housing vouchers, no restrictions
  3. Control: public housing in high-poverty (50% at baseline) areas
- 48% of eligible households in experimental voucher group “complied” and took up voucher
Most Common MTO Residential Locations in New York

- Control
- King Towers
- Harlem
- Experimental
- Wakefield
- Bronx
- Section 8
- Soundview
- Bronx
MTO Experiment: Exposure Effects?

- Existing research on MTO:
  - Little impact of moving to a better area on earnings and other economic outcomes
    - Rejects “Spatial Mismatch Hypothesis” of Kain (1968)
  - But work has focused on adults and older youth at point of move [e.g., Kling, Liebman, and Katz 2007]

- What about the young kids?


- Does MTO improve outcomes for children who moved when young?
Data

- MTO data obtained from HUD
  - 4,604 households and 15,892 individuals
  - Primary focus: 8,603 children born in or before 1991

- Link MTO data to federal income tax returns from 1996-2012
  - Approximately 85% of children matched
  - Match rates do not differ significantly across treatment groups
  - Baseline covariates balanced across treatment groups in matched data
In baseline analysis, estimate treatment effects for two groups:

- Young children: below age 13 at random assignment (RA)
- Older children: age 13-18 at random assignment

Average age at move: 8.2 for young children vs. 15.1 for older children

→ Younger children had 7 more years of exposure to low-poverty nbhd.

Estimates robust to varying age cutoffs and estimating models that interact age linearly with treatments
Replicate standard regression specifications used in earlier work [Kling, Katz, Liebman 2007]

\[ y_i = \alpha + \beta_E^{ITT} \text{Exp}_i + \beta_S^{ITT} S8_i + s_i \delta_s + \epsilon_i \]

These intent-to-treat (ITT) estimates identify effect of being offered a voucher to move through MTO

Estimate treatment-on-treated (TOT) effects using 2SLS, instrumenting for voucher takeup with treatment indicators

- Experimental take-up: 48% for young children, 40% for older children
- Section 8 take-up: 65.8% for young children, 55% for older children
Treatment Effects on Neighborhood Poverty

- Begin with “first stage” effects of MTO experiment on poverty rates
  - Measure mean poverty rates from random assignment to age 18 at tract level using Census data

- Use poverty rates as an index of nbhd. quality, but note that MTO treatments naturally changed many other features of neighborhoods too
Impacts of MTO on Children Below Age 13 at Random Assignment

(a) Mean Poverty Rate in Tract (ITT) Post RA to Age 18

Control: 41.2%  
Section 8: 33.2%  
Experimental Voucher: 30.9%  
\( p = 0.0001 \)

(b) Mean Poverty Rate in Tract (TOT) Post RA to Age 18

Control: 41.2%  
Section 8: 29.1%  
Experimental Voucher: 19.6%  
\( p = 0.0001 \)
Impacts of MTO on Children *Age 13-18* at Random Assignment

**(a) Mean Poverty Rate in Tract (ITT)**
Post RA to Age 18

Mean Poverty Rate in Tract post RA to Age 18 (%)

- **Control**: 47.9% (p = 0.0001)
- **Section 8**: 39.3% (p = 0.0001)
- **Experimental Voucher**: 37.9% (p = 0.0001)

**(b) Mean Poverty Rate in Tract (TOT)**
Post RA to Age 18

Mean Poverty Rate in Tract post RA to Age 18 (%)

- **Control**: 47.9% (p = 0.0001)
- **Section 8**: 32.5% (p = 0.0001)
- **Experimental Voucher**: 23.2% (p = 0.0001)
Now turn to impacts on outcomes in adulthood

Begin by analyzing effects on children below age 13 at RA

Start with individual earnings (W-2 earnings + self-employment income)
  - Includes those who don’t file tax returns through W-2 forms

Measured from 2008-12, restricting to years in which child is 24 or older
  - Evaluate impacts at different ages after showing baseline results
Impacts of MTO on Children Below Age 13 at Random Assignment

(a) Individual Earnings (ITT)

(b) Individual Earnings (TOT)

<table>
<thead>
<tr>
<th>Type</th>
<th>Control</th>
<th>Section 8</th>
<th>Experimental Voucher</th>
</tr>
</thead>
<tbody>
<tr>
<td>Individual Income at Age ≥ 24 ($)</td>
<td>$11,270</td>
<td>$12,380</td>
<td>$12,894</td>
</tr>
<tr>
<td></td>
<td>$11,270</td>
<td>$12,994</td>
<td>$14,747</td>
</tr>
</tbody>
</table>

\[ p = 0.101 \]
\[ p = 0.014 \]
Impacts of Experimental Voucher by Age of Earnings Measurement
For Children Below Age 13 at Random Assignment
Impacts of MTO on Children Below Age 13 at Random Assignment

(a) College Attendance (ITT)
- Control: 16.5% (p = 0.435)
- Section 8: 17.5% (p = 0.28)
- Experimental Voucher: 19.0% (p = 0.028)

(b) College Quality (ITT)
- Control: $20,915 (p = 0.014)
- Section 8: $21,547 (p = 0.003)
- Experimental Voucher: $21,601 (p = 0.003)
Impacts of MTO on Children Below Age 13 at Random Assignment

(a) ZIP Poverty Share in Adulthood (ITT)

(b) Birth with no Father Present (ITT)

Females Only

<table>
<thead>
<tr>
<th></th>
<th>Control</th>
<th>Section 8</th>
<th>Experimental Voucher</th>
</tr>
</thead>
<tbody>
<tr>
<td>ZIP Poverty Share (%)</td>
<td>23.8%</td>
<td>22.4%</td>
<td>22.2%</td>
</tr>
<tr>
<td>p</td>
<td>0.047</td>
<td>0.008</td>
<td>0.610</td>
</tr>
</tbody>
</table>

<table>
<thead>
<tr>
<th></th>
<th>Control</th>
<th>Section 8</th>
<th>Experimental Voucher</th>
</tr>
</thead>
<tbody>
<tr>
<td>Birth with no Father on Birth Certificate (%)</td>
<td>33.0%</td>
<td>31.7%</td>
<td>28.2%</td>
</tr>
<tr>
<td>p</td>
<td>0.610</td>
<td>0.042</td>
<td>0.042</td>
</tr>
</tbody>
</table>
Treatment Effects on Older Children

- Next, turn to children who were ages 13-18 at random assignment
  - Replicate same analysis as above
Impacts of MTO on Children Age 13-18 at Random Assignment

(a) Individual Earnings (ITT)

(b) Individual Earnings (TOT)

Individual Income at Age ≥ 24 ($) for Control, Section 8, and Experimental Voucher groups. The bars show the mean incomes with 95% confidence intervals.

For ITT:
- Control: $15,882
- Section 8: $14,749 (p = 0.219)
- Experimental: $14,915 (p = 0.259)

For TOT:
- Control: $15,882
- Section 8: $13,830 (p = 0.219)
- Experimental: $13,455 (p = 0.259)
Impacts of Experimental Voucher by Age of Earnings Measurement

- Below 13 at RA
- Above 13 at RA
Impacts of MTO on Children Age 13-18 at Random Assignment

(a) College Attendance (ITT)

College Attendance, Ages 18-20 (%)

Control: 15.6%  
Section 8: 12.6%  
Experimental Voucher: 11.4%  

\[ p = 0.091 \]  
\[ p = 0.013 \]

(b) College Quality (ITT)

Mean College Quality, Ages 18-20 ($)

Control: $21,638  
Section 8: $21,041  
Experimental Voucher: $20,755  

\[ p = 0.168 \]  
\[ p = 0.022 \]
Impacts of MTO on Children Age 13-18 at Random Assignment

(a) ZIP Poverty Share in Adulthood (ITT)

(b) Birth with no Father Present (ITT)
Females Only

<table>
<thead>
<tr>
<th></th>
<th>Control</th>
<th>Section 8</th>
<th>Experimental Voucher</th>
</tr>
</thead>
<tbody>
<tr>
<td>Zip Poverty Share (%)</td>
<td>23.6%</td>
<td>22.7%</td>
<td>23.1%</td>
</tr>
<tr>
<td>Birth No Father Present (%)</td>
<td>41.4%</td>
<td>40.7%</td>
<td>45.6%</td>
</tr>
</tbody>
</table>

p = 0.184
p = 0.418
p = 0.857
p = 0.242
Prior work has analyzed variation in treatment effects across sites, racial groups, and gender.

Replicate analysis across these groups for children below age 13 at RA.
Impacts of MTO on Individual Earnings (ITT) by Gender for Children Below Age 13 at Random Assignment
Impacts of MTO on Individual Earnings (ITT) by Race for Children Below Age 13 at Random Assignment

Individual Earnings 2008-12 ($)

- Hispanic
- Non-Black Non-Hisp
- Black Non-Hisp

Control
Section 8
Experimental
Impacts of MTO on Individual Earnings (ITT) by Site for Children Below Age 13 at Random Assignment

Individual Earnings 2008-12 ($)

<table>
<thead>
<tr>
<th>Location</th>
<th>Control</th>
<th>Section 8</th>
<th>Experimental</th>
</tr>
</thead>
<tbody>
<tr>
<td>Baltimore</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Boston</td>
<td></td>
<td>15000</td>
<td></td>
</tr>
<tr>
<td>Chicago</td>
<td></td>
<td>12000</td>
<td>10000</td>
</tr>
<tr>
<td>LA</td>
<td></td>
<td>13000</td>
<td>14000</td>
</tr>
<tr>
<td>New York</td>
<td></td>
<td>14000</td>
<td></td>
</tr>
</tbody>
</table>

Baltimore, Boston, Chicago, LA, New York
Multiple Hypothesis Testing

Given extent to which heterogeneity has been explored in MTO data, one should be concerned about multiple hypothesis testing.

Our study simply explores one more dimension of heterogeneity: age of child.

Any post-hoc analysis will detect “significant” effects (p < 0.05) even under the null of no effects if one examines a sufficiently large number of subgroups.

Can account for multiple tests by testing omnibus null that treatment effect is zero in all subgroups studied to date (gender, race, site, and age).

Two approaches: parametric F test and non-parametric permutation test.
## Multiple Comparisons: F Tests for Subgroup Heterogeneity

<table>
<thead>
<tr>
<th></th>
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<th></th>
<th></th>
<th></th>
<th></th>
<th></th>
</tr>
</thead>
<tbody>
<tr>
<td><strong>Panel A: p-values for Comparisons by Age Group</strong></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Exp. vs. Control</td>
<td>0.0203</td>
<td>0.0034</td>
<td>0.0035</td>
<td>0.0006</td>
<td>0.0814</td>
<td>0.0265</td>
</tr>
<tr>
<td>Sec. 8 vs. Control</td>
<td>0.0864</td>
<td>0.0700</td>
<td>0.1517</td>
<td>0.0115</td>
<td>0.0197</td>
<td>0.0742</td>
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<td>Exp &amp; Sec. 8 vs. Control</td>
<td>0.0646</td>
<td>0.0161</td>
<td>0.0218</td>
<td>0.0020</td>
<td>0.0434</td>
<td>0.0627</td>
</tr>
<tr>
<td><strong>Panel B: p-values for Comparisons by Age, Site, Gender, and Race Groups</strong></td>
<td></td>
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<tr>
<td>Exp. vs. Control</td>
<td>0.1121</td>
<td>0.0086</td>
<td>0.0167</td>
<td>0.0210</td>
<td>0.2788</td>
<td>0.0170</td>
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<td>Sec. 8 vs. Control</td>
<td>0.0718</td>
<td>0.1891</td>
<td>0.1995</td>
<td>0.0223</td>
<td>0.1329</td>
<td>0.0136</td>
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<td>Exp &amp; Sec. 8 vs. Control</td>
<td>0.1802</td>
<td>0.0446</td>
<td>0.0328</td>
<td>0.0202</td>
<td>0.1987</td>
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</table>
## Multiple Comparisons: Permutation Tests for Subgroup Heterogeneity

<table>
<thead>
<tr>
<th>p-value</th>
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<th>Gender</th>
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<tr>
<td></td>
<td>&lt; 13</td>
<td>&gt;= 13</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Truth</td>
<td>0.014</td>
<td>0.258</td>
<td>0.698</td>
<td>0.529</td>
</tr>
</tbody>
</table>
**Multiple Comparisons: How to Construct Permutation Tests for Subgroup Heterogeneity**

**EXAMPLE**

<table>
<thead>
<tr>
<th></th>
<th>Age</th>
<th>Race</th>
<th>Gender</th>
<th>Site</th>
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<tbody>
<tr>
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<td>p-value</td>
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<td>&gt;= 13</td>
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<td>0.258</td>
<td>0.698</td>
<td>0.529</td>
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<tr>
<td>Placebos</td>
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<tr>
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<td>0.683</td>
<td>0.805</td>
<td>0.017</td>
<td>0.305</td>
</tr>
</tbody>
</table>

**Adjusted p-value (example)** 0.100
Multiple Hypothesis Testing

- Conduct permutation test for all five outcomes we analyzed above

- Calculate fraction of placebos in which p value for all five outcomes in any one of the 12 subgroups is below true p values for <13 group

  Yields a p value for null hypothesis that there is no treatment effect on any of the five outcomes adjusted for multiple testing

- Adjusted p < 0.01 based on 1000 replications
Treatment Effects on Adults

- Previous work finds no effects on adults’ economic outcomes [Kling et al. 2007, Sanbonmatsu et al. 2011]

- Re-evaluate impacts on adults’ outcomes using tax data

- Does exposure time matter for adults’ outcomes as it does for children? [Clampet-Lundquist and Massey 2008]
Impacts of Experimental Voucher on Adults Exposure to Low-Poverty Neighborhoods
by Years Since Random Assignment

Exp. Vs. Control ITT on Years of Nbhd Poverty <20%

Years since Random Assignment

Impacts of Experimental Voucher on Adults Exposure to Low-Poverty Neighborhoods by Years Since Random Assignment

Exp. Vs. Control ITT on Years of Nbhd Poverty <20%

Years since Random Assignment
Impacts of Experimental Voucher on Adults’ Individual Earnings by Years Since Random Assignment
Impacts of Experimental Voucher by Child’s Age at Random Assignment
Household Income, Age ≥ 24 ($)

Experimental Vs. Control ITT on Income ($)

Age at Random Assignment

- Hope IV demolitions

- Previous work documents impacts on test scores (Jacob 2004: “Public Housing, Housing Vouchers, and Student Achievement: Evidence from Public Housing Demolitions in Chicago”, The American Economic Review)

- Link to data on earnings outcomes using administrative records

- Compare to Section 8 outcomes
Figure 1: Density of Neighborhood Poverty for Displaced (Treated) and Non-displaced (Control) Households

Notes: This figure displays the density of the Census tract-level poverty rate for households (N = 2,767) with at least one child (age 7 to 18 at baseline) affected by demolition. Poverty rates for each household are duration-weighted averages over all locations that a household lived since being displaced (treated) by housing demolition. Household location is tracked to 2009. The duration-weighted poverty rate for households that were displaced by demolition is shown in the solid red line, while households from non-demolished buildings are shown in the dashed blue line.
Figure 2: Difference in Neighborhood Poverty For Displaced and Non-displaced Households by Post-Demolition Year

Notes: This figure illustrates the change over time in the difference in neighborhood poverty rate between displaced (treated) and non-displaced (control) households with children (age 7 to 18 at baseline). Specifically, I plot (in solid black) the set of coefficients $\pi_y$ for $y \in \{0, \cdots, 10\}$ from the following specification:

$$pbpov_{htp} = \sum_{y=10}^{y=10} \pi_y \text{treat}_{h} \cdot 1(t - t^* = y) + \sum_{y=0}^{y=10} \delta_y \cdot 1(t - t^* = y) + \psi_p + \epsilon_{ht}$$

where $h$ indexes a household; $t$ represents years; and $p$ indexes projects. The dependent variable is the percentage of residents living below the poverty line in a Census tract and $\psi_p$ is a set of project fixed effects. The variable $t^*$ represents the year of demolition for a particular household. Recall that public housing demolitions occur from 1995-1998 in my sample. The variable treat$_h$ is an indicator for treatment (displaced) status. The data used with this specification is a panel for a particular household where the first observation is the poverty rate based on the household’s address at the time of demolition ($t^*$). Hence, the set of coefficients $\pi_y$ represent the difference in poverty rate between displaced (treated) and non-displaced (control) households in a particular post demolition period ($y$). There are 2,767 households in the sample. The dashed gray lines in the figure also outline the 95-percent confidence interval for the year-specific point estimates.
Figure 3: Labor-Market Treatment Effects for All Children by Age of Measurement

(a) Dependent Variable: Employed (=1)
Figure 4: Younger vs Older Children: Labor-Market Treatment Effects by Age of Measurement

(a) Dependent Variable: Employed (=1)
(b) Dependent Variable: Annual Earnings (§)

- Treatment Effect ($) vs. Age of Earnings Measurement
- Lines represent different baseline ages: < Age 13 and Age 13-18
- Treatment effect increases with age after a certain point for those who started treatment younger.
Comparison to Section 8

- Chyn (2016) also compares impact of demolition to Section 8 lotteries
- Chicago Housing Authority allocates vouchers using lottery system
- Compare lottery winners to losers
Figure 6: Effects on Adult Employment of Children Across Studies

Impact on Labor Market Participation (Percentage Points)

- Demolition (Chyn 2015)
- CHAC 1997 (Chyn 2015)
- MTO Sec. 8 (Sanbonmatsu et al. 2011)
- MTO Exp. (Sanbonmatsu et al. 2011)
Figure 7: Effects on Adult Earnings of Children Across Studies

Impact on Annual Earnings ($)

-6,000  -4,000  0  2,000

Demolition (Chyn 2015)  CHAC 1997 (Chyn 2015)  MTO Sec. 8 (Sanbonmatsu et al. 2011)  MTO Exp. (Sanbonmatsu et al. 2011)
Housing Demolitions in Chicago

- Why no impact of Section 8 vouchers?

- Chyn (2016) argues for “Reverse Roy” sorting model
  - Those forced to move have higher returns than “compliers” from vouchers
  - Conclusion: forcing people to move delivers larger impacts?

- Nathan’s take: Section 8 and demolition is a different treatment
  - Section 8 does not induce better neighborhoods!
  - If neighborhood quality matters, then should we expect impacts of Section 8?
  - Chyn paper provides no convincing evidence of reverse Roy sorting
  - But, suggests demolition very bad neighborhoods can improve outcomes
Open Questions in Place Effects on Children

- Many open questions
  - Place-based policy
    - What about a place causes low outcomes? Schools? Other?
  - Choice-based policy
    - “GE” Effects on destination and origin kids
    - Question to think about: How should people be allocated to places?
      - Role of super-modularity
  - What is more cost-effective?
    - More cost-effective relative to other redistributive programs?
Impacts of MTO on Annual Income Tax Revenue in Adulthood for Children Below Age 13 at Random Assignment (TOT Estimates)

Annual Income Tax Revenue, Age ≥ 24 ($)

- Control: $447.5
- Section 8: $616.6 (p = 0.061)
- Experimental Voucher: $841.1 (p = 0.004)