ECON 2450A
Topic 10: Tags, Place, and Intergenerational Mobility

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This lecture:

- Key empirical facts: Place matters, especially for children and intergenerational mobility
    1. Place is predictive of outcomes conditional on observables
    2. Places have causal effects on children's outcomes
- What does this mean for policy?
    - Primer: do we care about intergenerational mobility?
        - Inverse Euler Equation?
    - Akerlof Tags, applied to place and intergenerational mobility
        - Require causal effect? Or just observational variation
    - Impact of place-based policy?

Does Place Matter? Who cares?

- Proceed in two steps:
  - [Tagging]: Document wide variation in intergenerational mobility by neighborhood
    - Follow Chetty, Friedman, Hendren, Jones, and Porter (2018); See also Wilson (1987), Massey and Denton (1993), Cutler and Glaeser (1997), Wodtke et al. (1999), Sampson (2008)
  - [Causal] Do places have causal effects?
    - Chetty and Hendren (2018); Chetty, Hendren and Katz (2016)
    - Suggests potential role of two classes of policies:
      - People-based policy: change allocation of people to places
      - Place-based policy: change places

Akerlof Tags

- Suppose individuals have characteristic X that is immutable and signals having low earnings capacity
- Akerlof (1978): Redistribute to X and lower the tax rate
  - Lowers distortions in the economy
- X is correlated with lower income
  - Need it be causal?

What can potentially be a tag?

- Single parenthood (Akerlof 1978)
- Height? (Mankiw and Weinzeirl, 2010)
- Race? Gender? Rationale for anti-discrimination policies? [next week]
- Place?
  - Rosen-Roback sorting model implies place is a good tag?
  - But what about for kids?
- Purchase of Yachts? ☺
- **Tagging is about redistributing using less elastic variables than income**
  - **Weak separability?**

Tagging Children's Future Outcomes

- Many policies target areas based on characteristics such as the poverty rates
  - –Tax policies (e.g., Opportunity zones), local services (e.g., Head Start programs), …
- For such "tagging" applications, observed outcomes are of direct interest in standard optimal tax models [Akerlof 1978]
  - –Isolating causal effects of neighborhoods not necessarily relevant

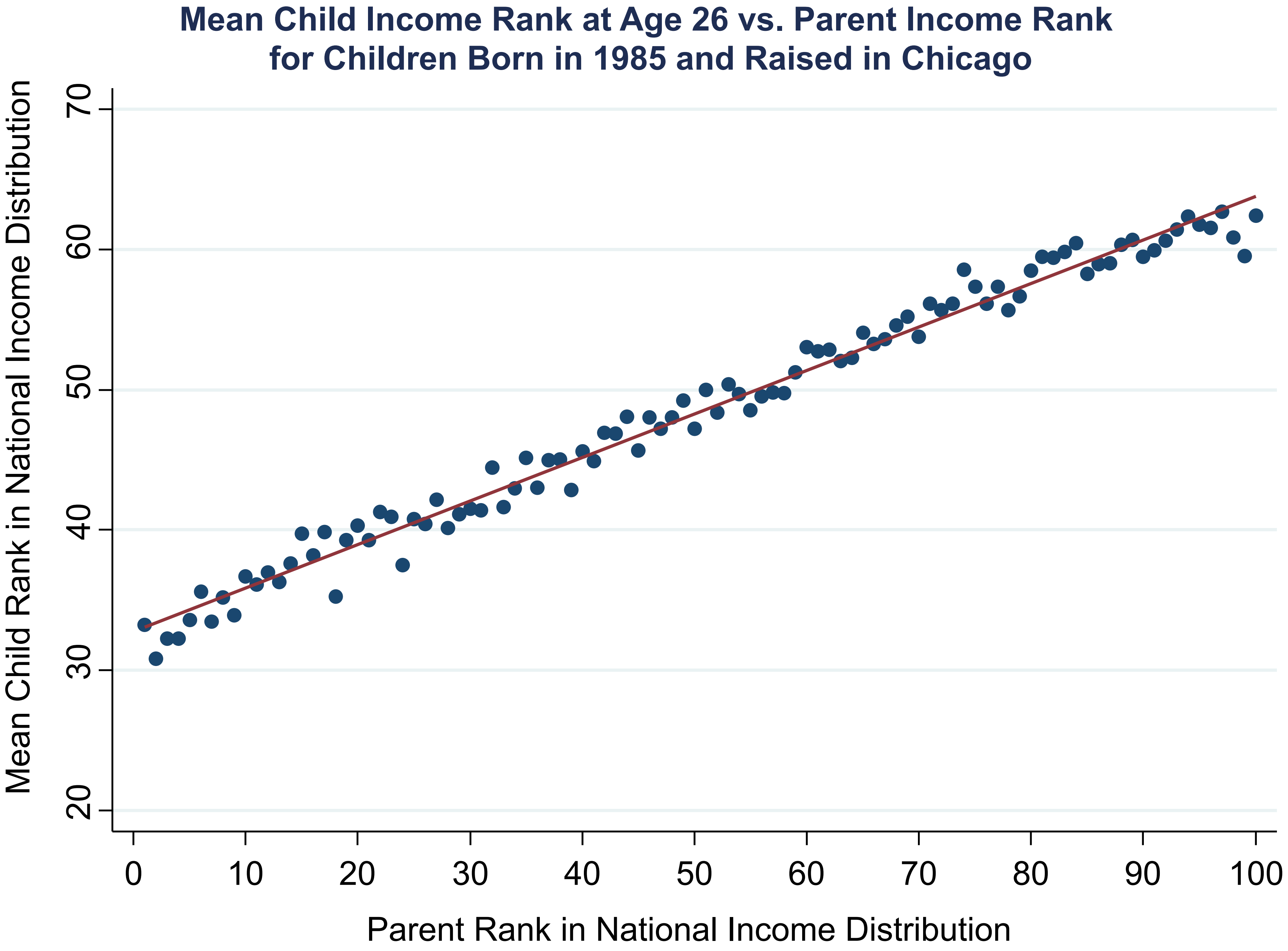
–But, need location to not respond as if it is income (i.e. violation of weak separability)

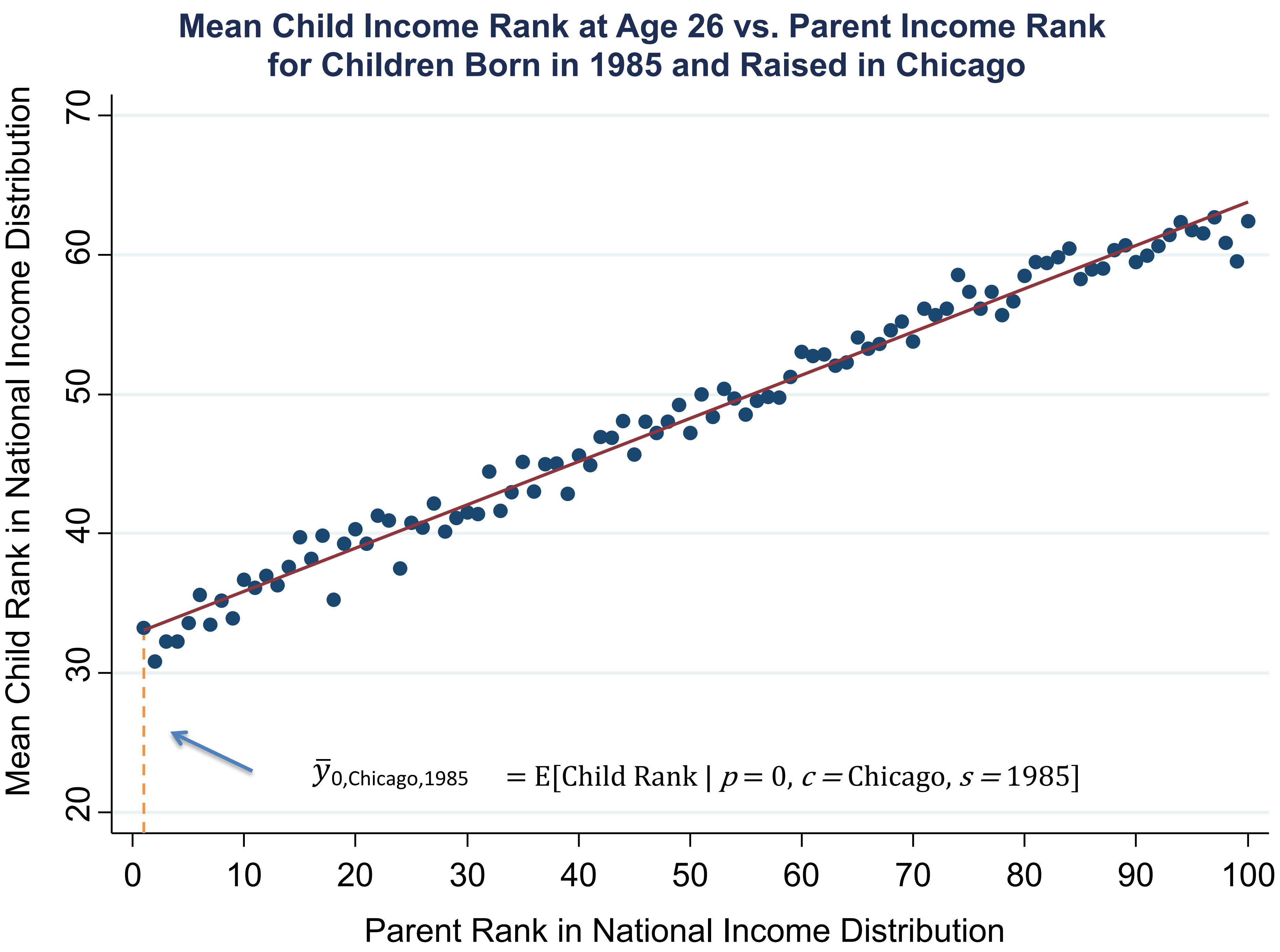
Children versus Adults

- Growing literature debating role of place in shaping outcomes for adults
  - Empowerment Zones (Busso, Gregory, Kline (2013))
  - Chinese import competition (Autor, Dorn, Hansen (2013))
  - Spatial Mismatch: Kain (1968), William Julius Wilson (1987)
- Key question: impact on place or people
  - Yagan 2017 documents impacts on people
- Here: focus on impact of place on children
- Human capital production is local; labor markets are global?

Chetty, Hendren, Friedman, Jones, Porter (2018)

- Chetty, Hendren, Friedman, Jones, Porter (2018) measure intergenerational mobility for children by childhood census tract
  - Data: Linked Census-IRS data from 1989-2015
  - Sample: Children born between 1978-83
  - Variables:
    - Parent income: mean pre-tax household income between 1994-2000
    - Child income: pre-tax household income at various ages
- Focus on percentile ranks in **national** income distribution
  - Rank children relative to others in the same birth cohort
  - Rank parents relative to other parents


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
20
30
40
50
60
70
0
10
20
30
40
50
60
70
80
90
100
Parent Rank in National Income Distribution



Mean Child Income Rank at Age 26 vs. Parent Income Rank
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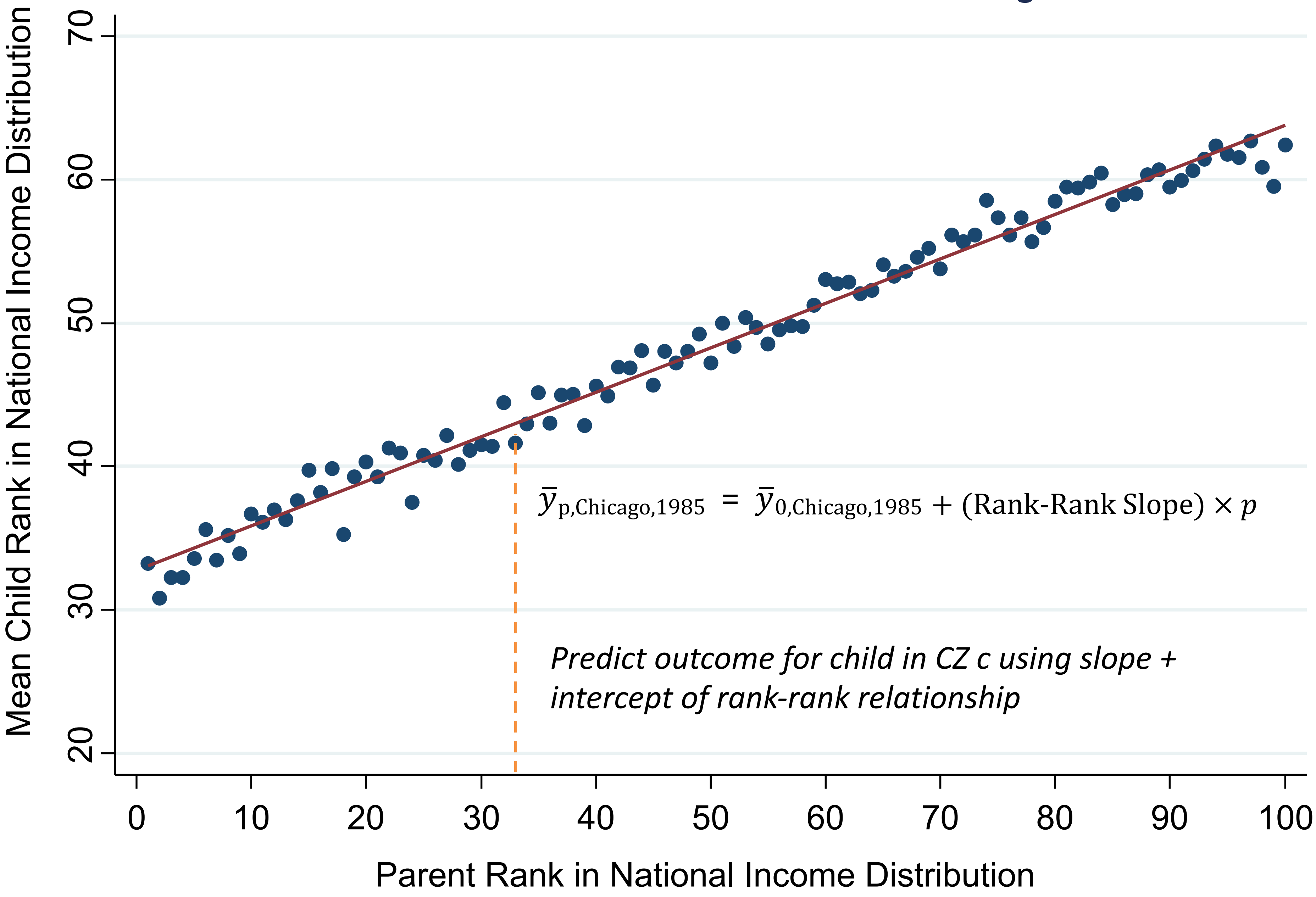
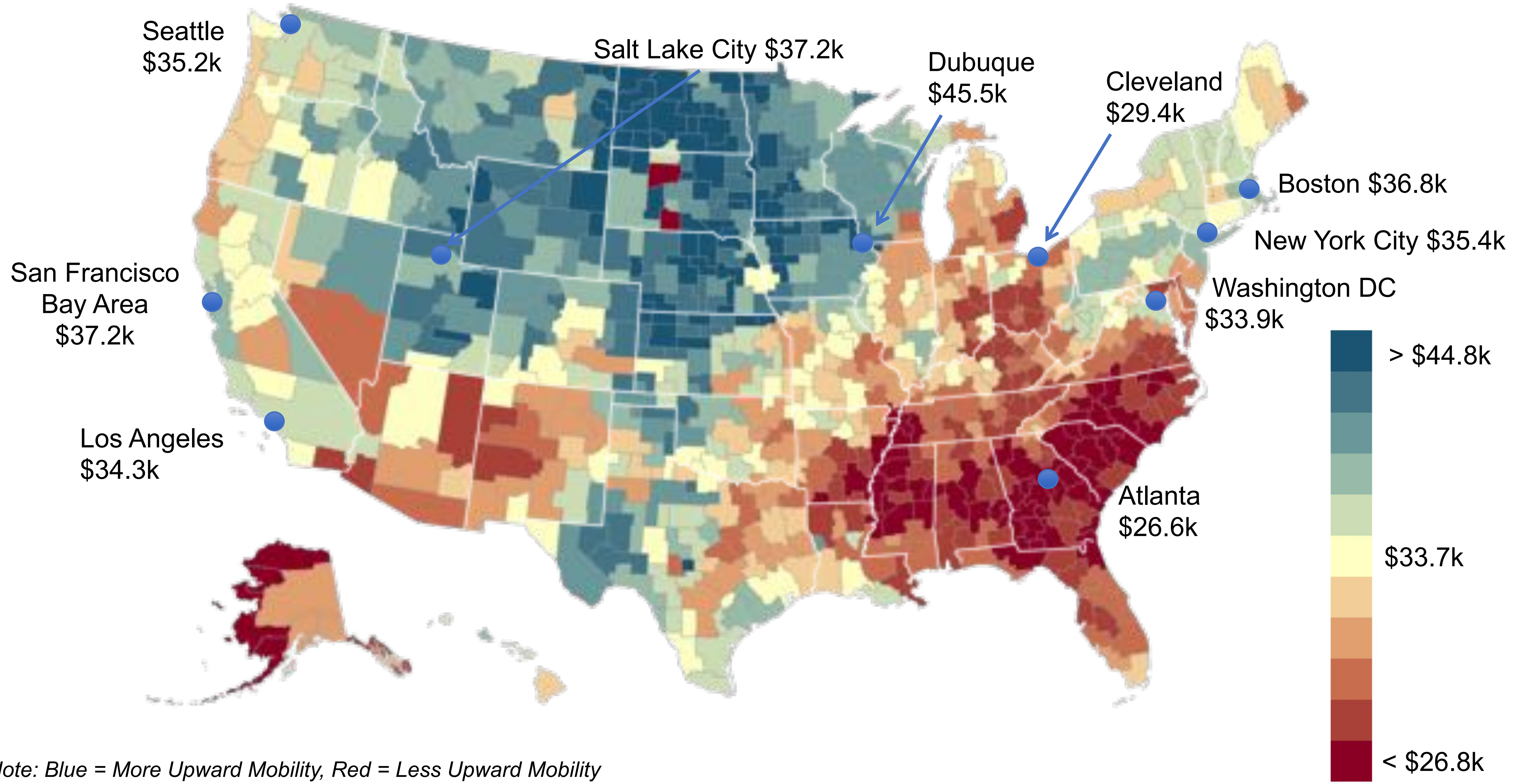
Mean Child Rank in National Income Distribution
20
30
40
50
60
70
0
10
20
30
40
50
60
70
80
90
100
Parent Rank in National Income Distribution
$\bar{y}_{0,\text{Chicago},1985} = \mathrm{E}[\text{Child Rank} \mid p = 0, c = \text{Chicago}, s = 1985]$

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



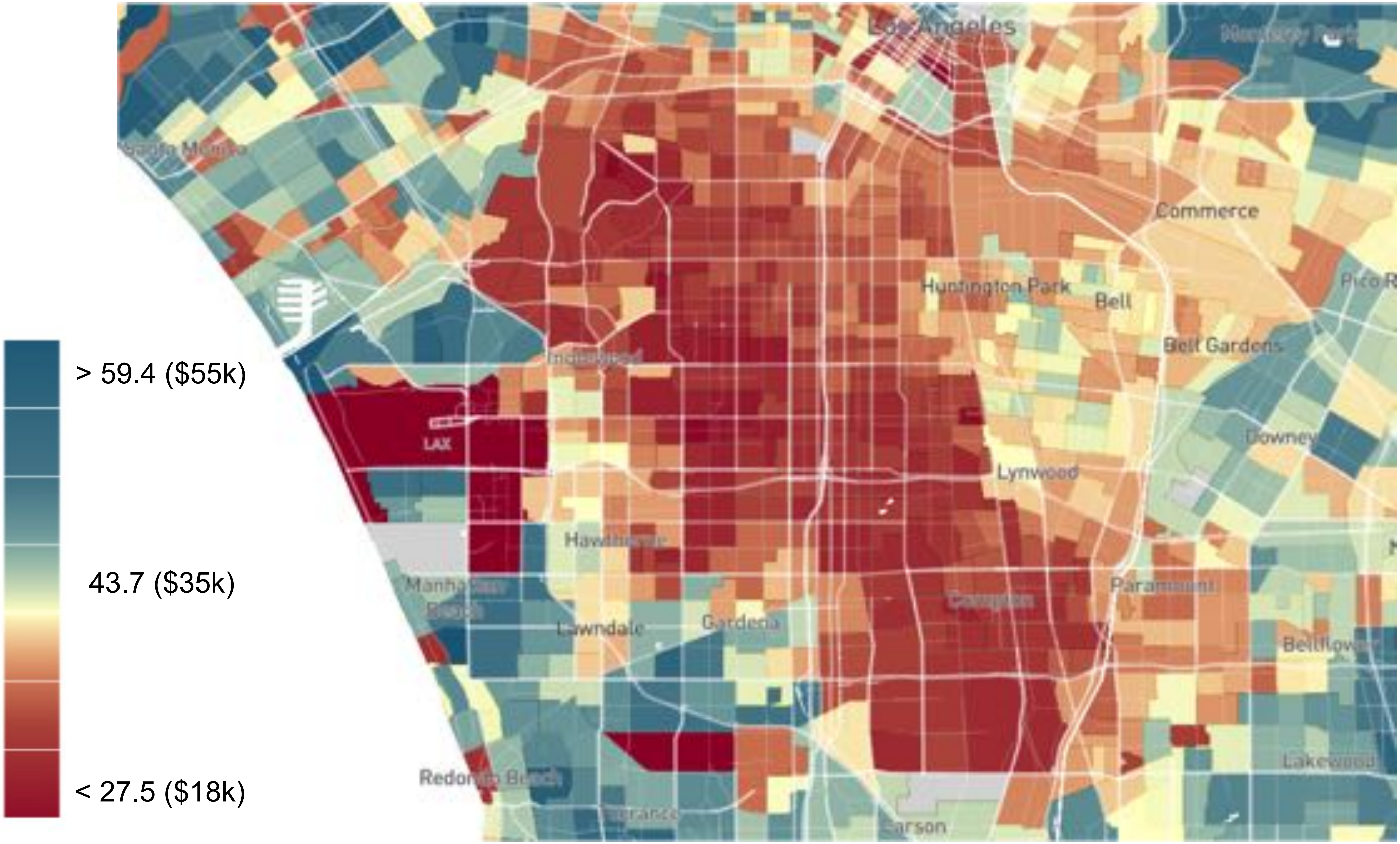
The Geography of Upward Mobility in the United States

Average Household Income for Children with Parents Earning $27,000 (25th percentile)

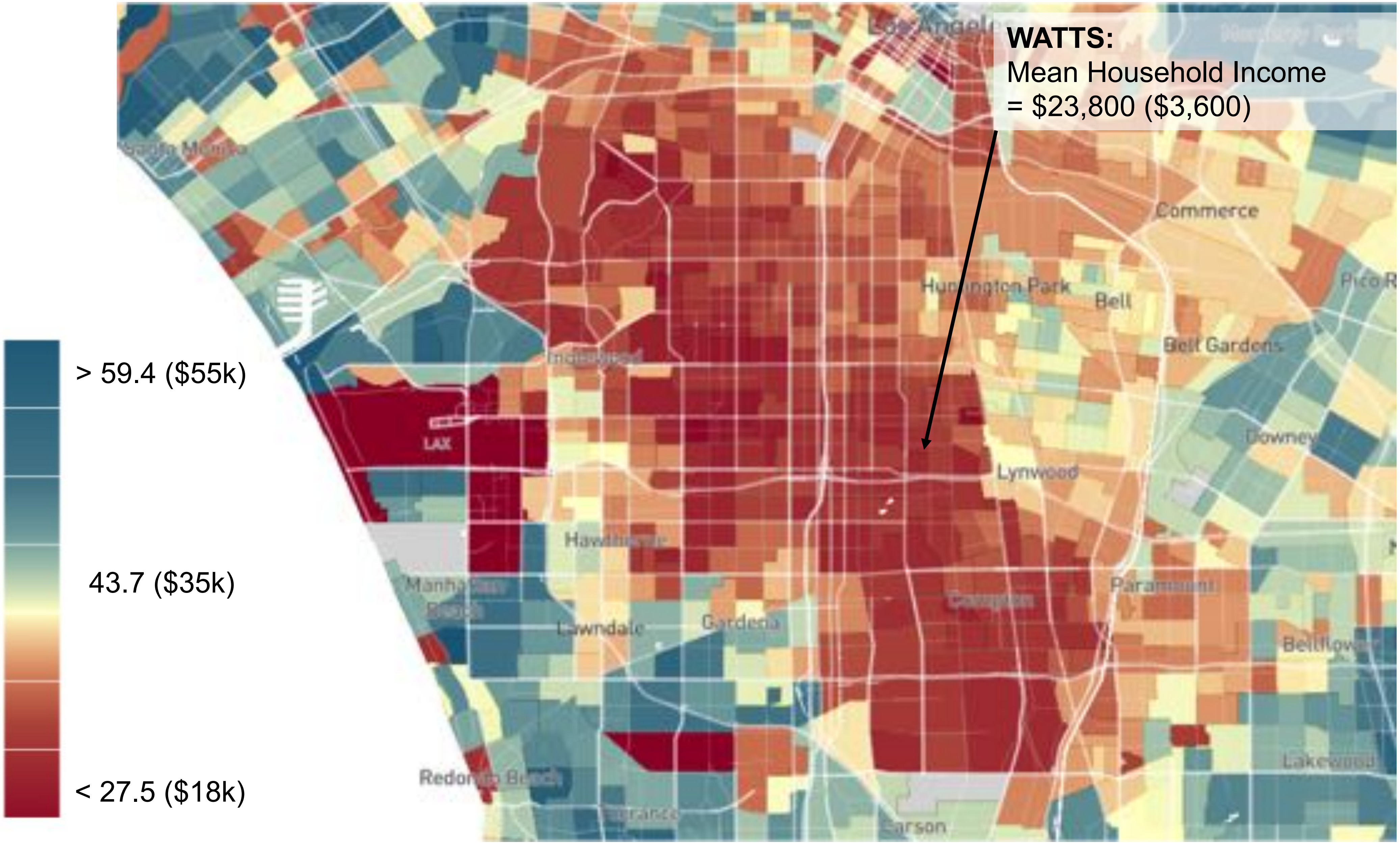


Note: Blue = More Upward Mobility, Red = Less Upward Mobility

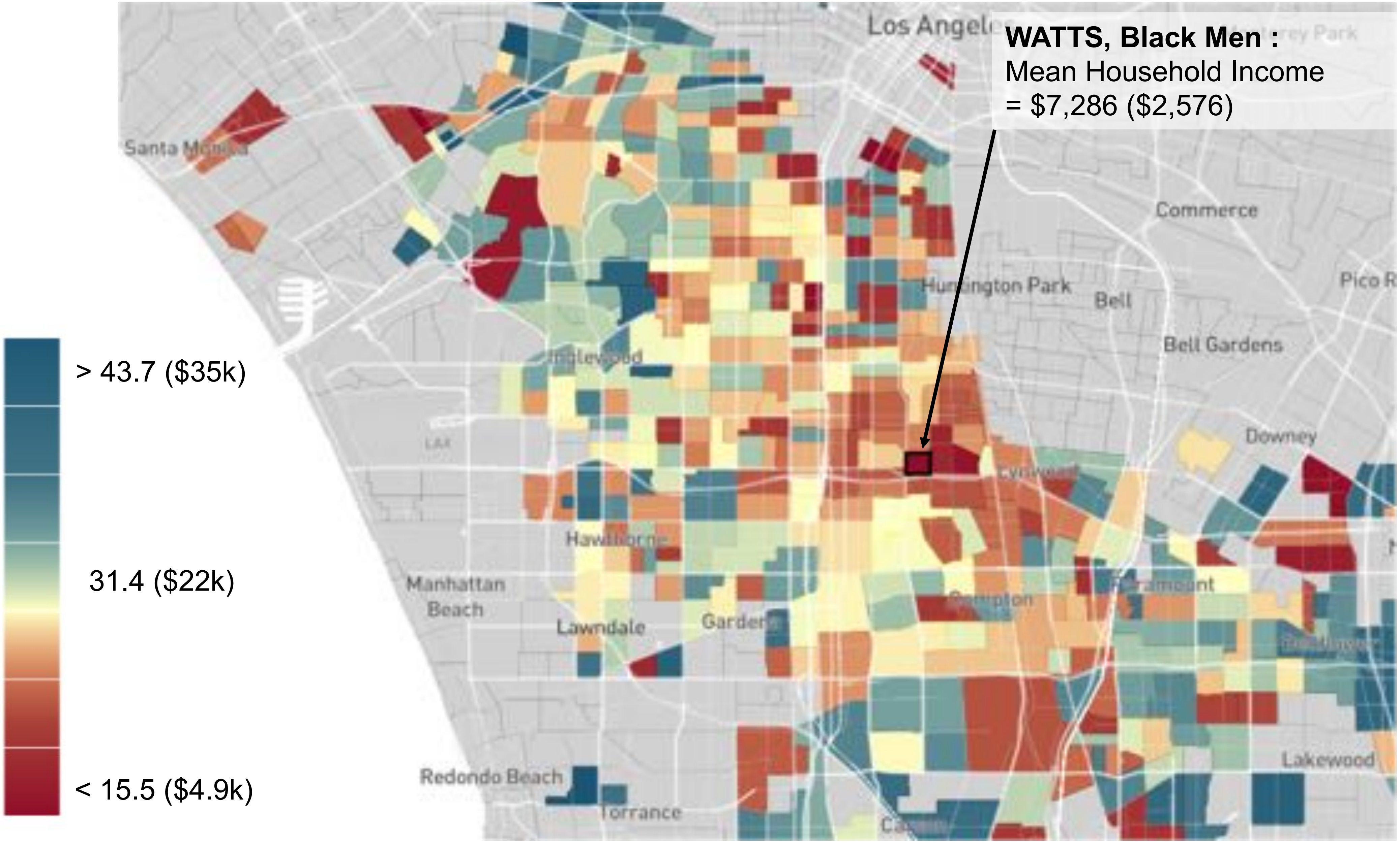
Mean Household Income for Children in Los Angeles with Parents Earning $27,000 ($25^{th}$ percentile)



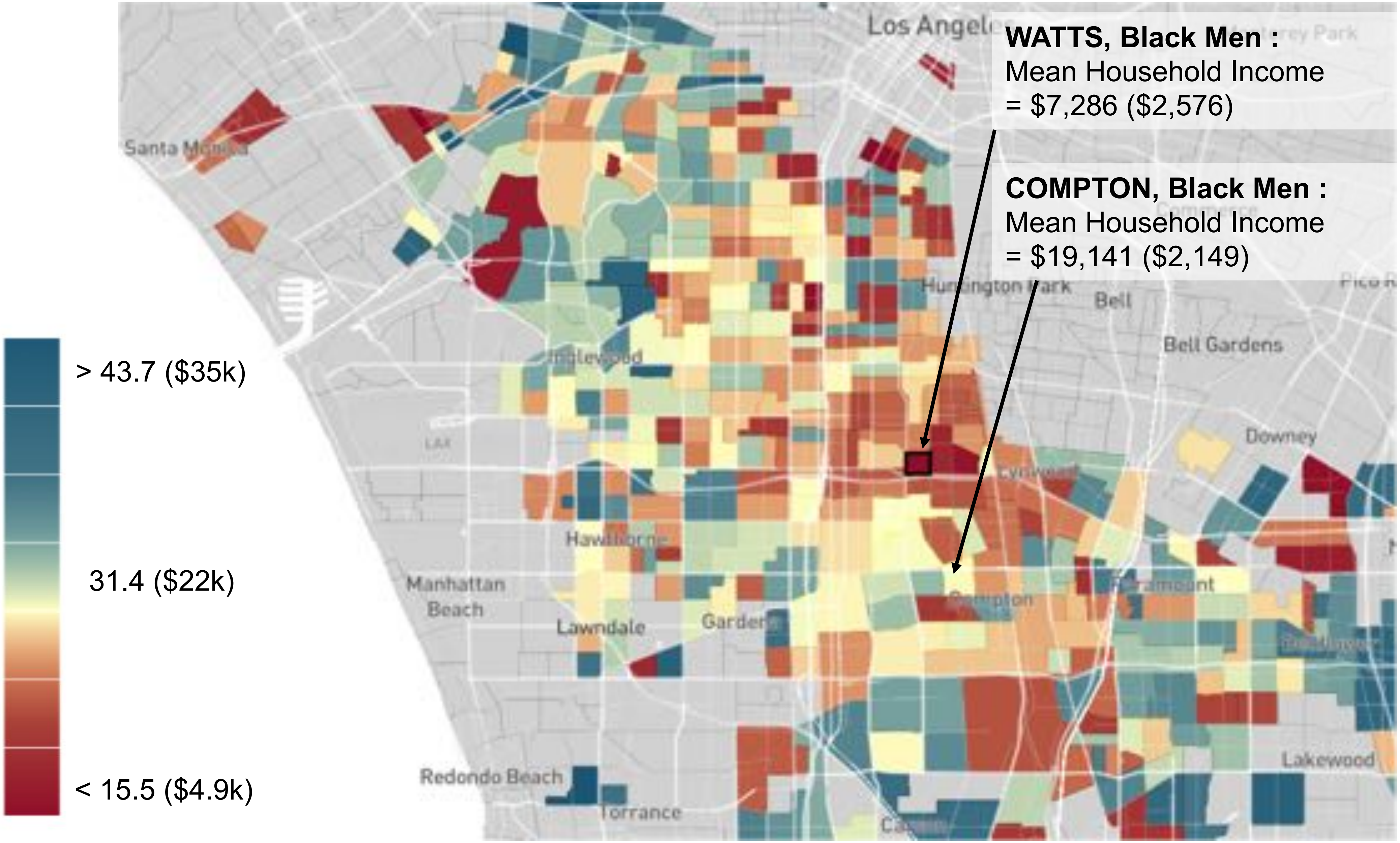
Mean Household Income for Children in Los Angeles with Parents Earning $27,000 (25th percentile)



Mean Household Income for Black Men in Los Angeles with Parents Earning $27,000 (25th percentile)



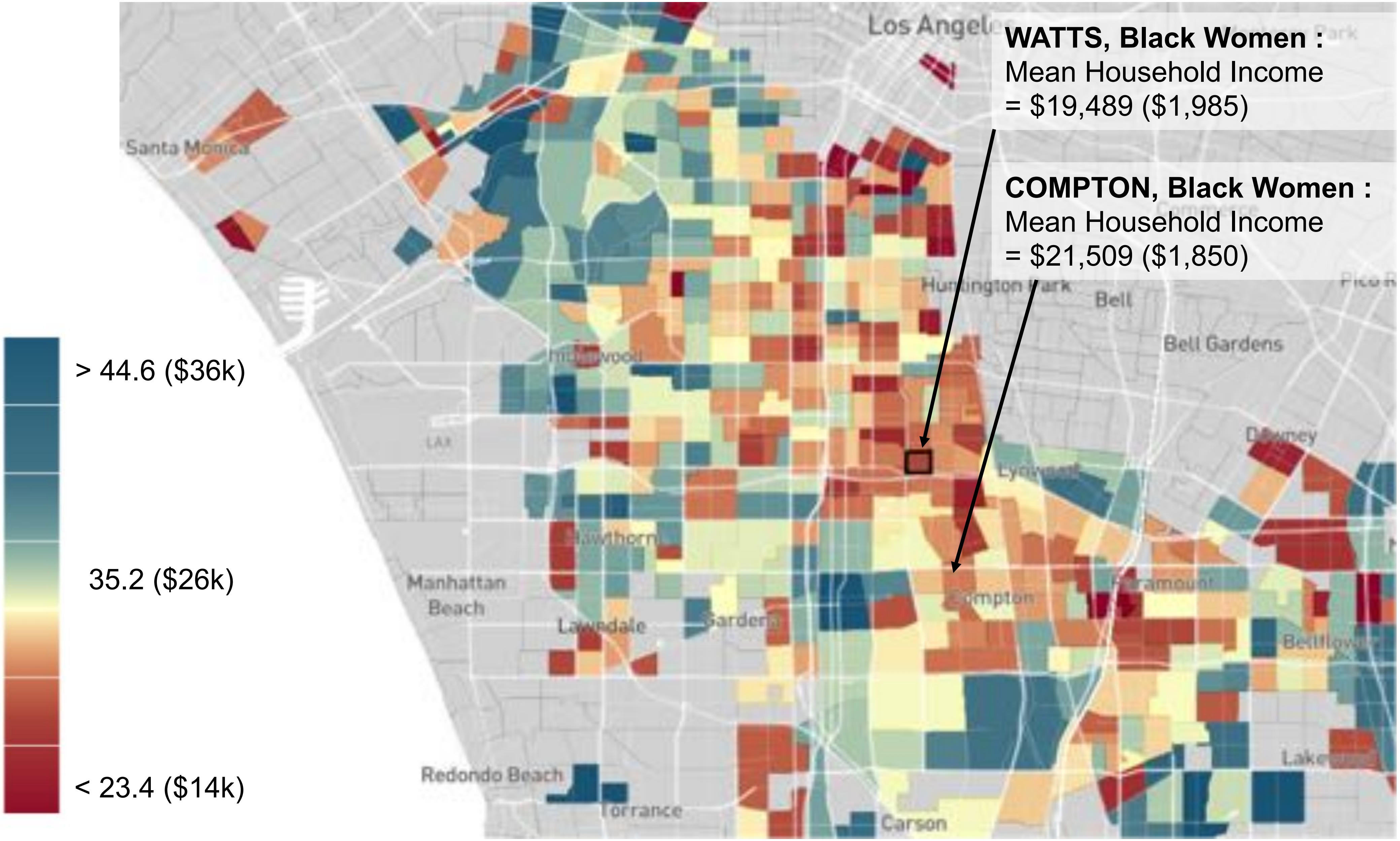
Mean Household Income for Black Men in Los Angeles with Parents Earning $27,000 (25th percentile)



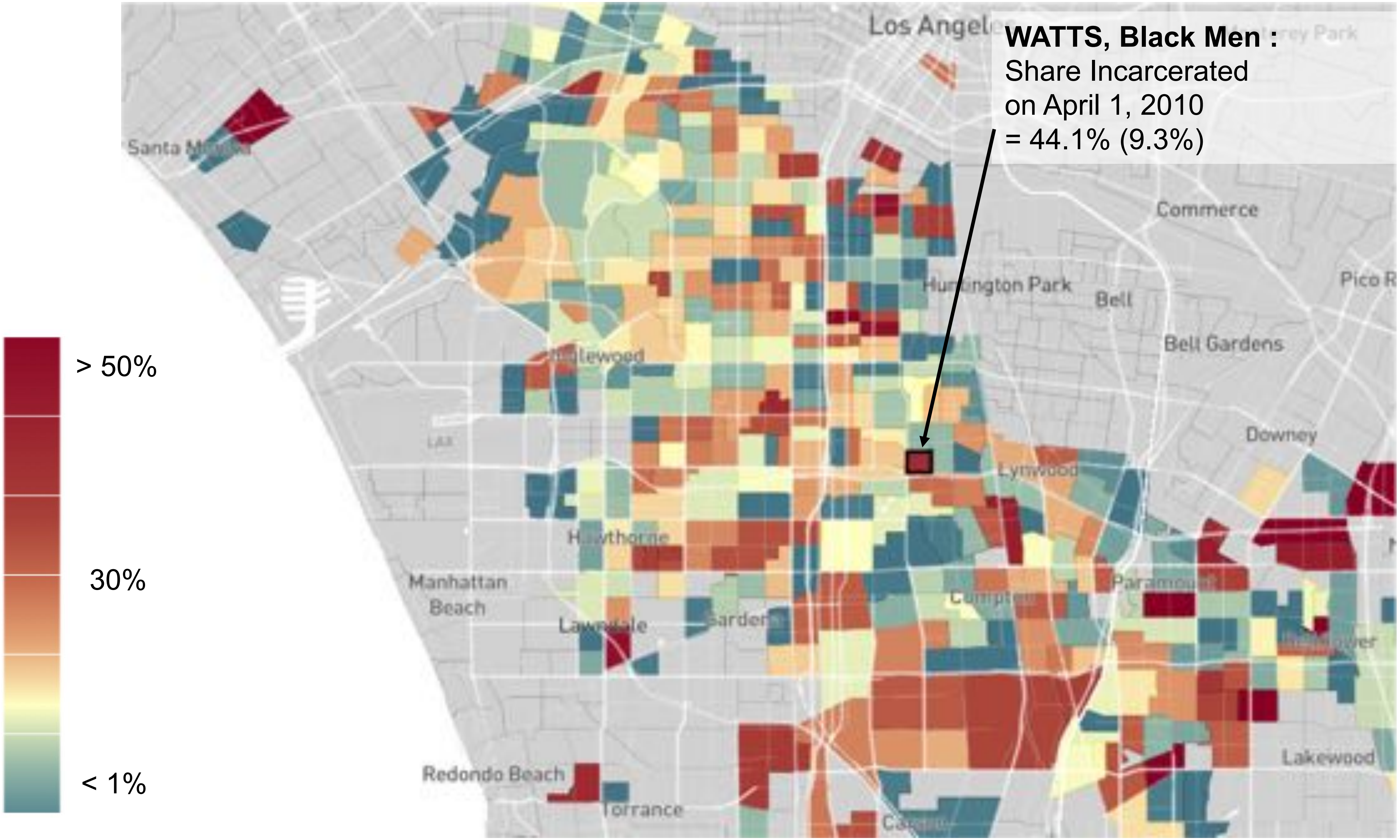
WATTS, Black Men :
Mean Household Income
= $7,286 ($2,576)

COMPTON, Black Men :
Mean Household Income
= $19,141 ($2,149)

Mean Individual Income for Black Women in Los Angeles with Parents Earning $27,000 (25th percentile)



Incarceration Rates for Black Men in Los Angeles with Parents Earning < $2,200 (1st percentile)



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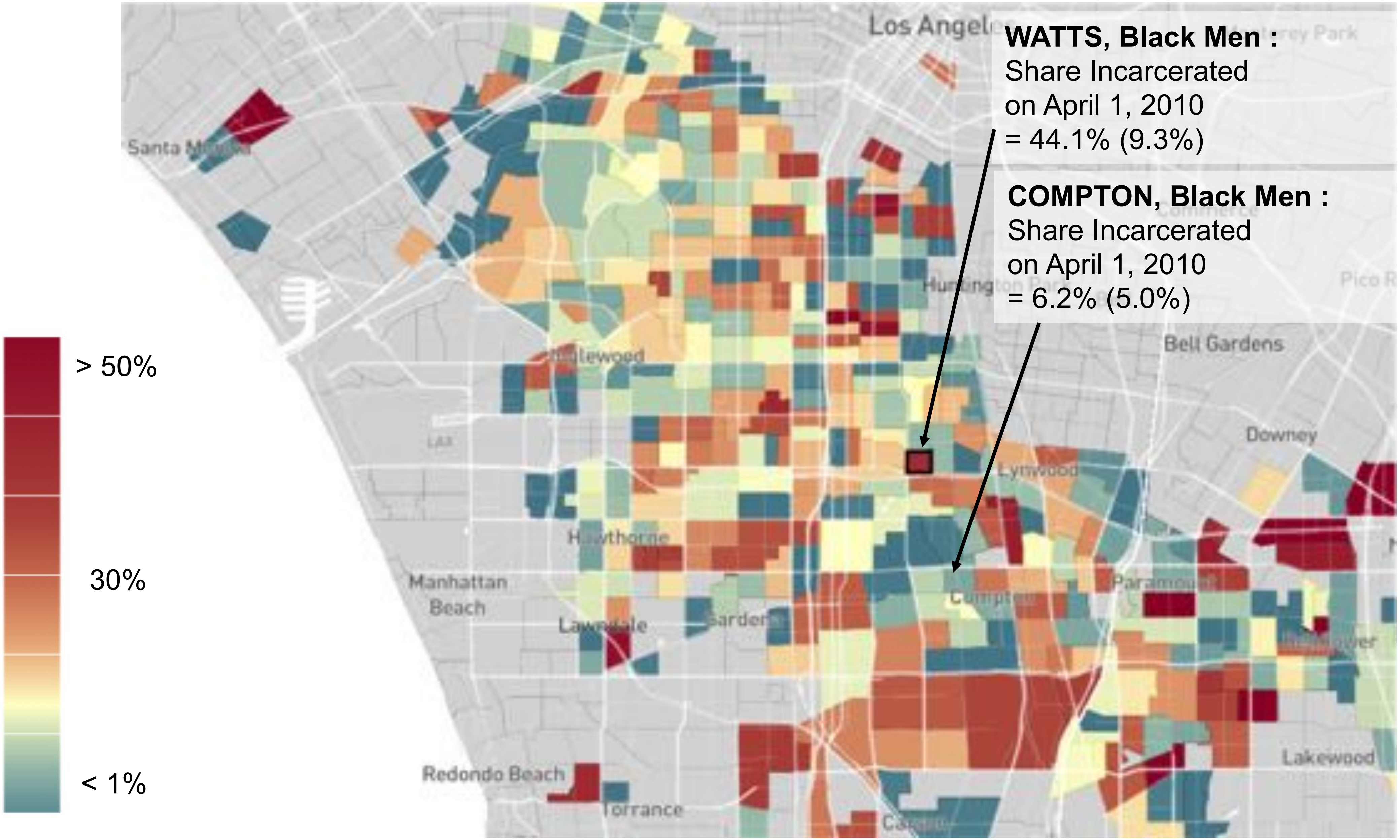
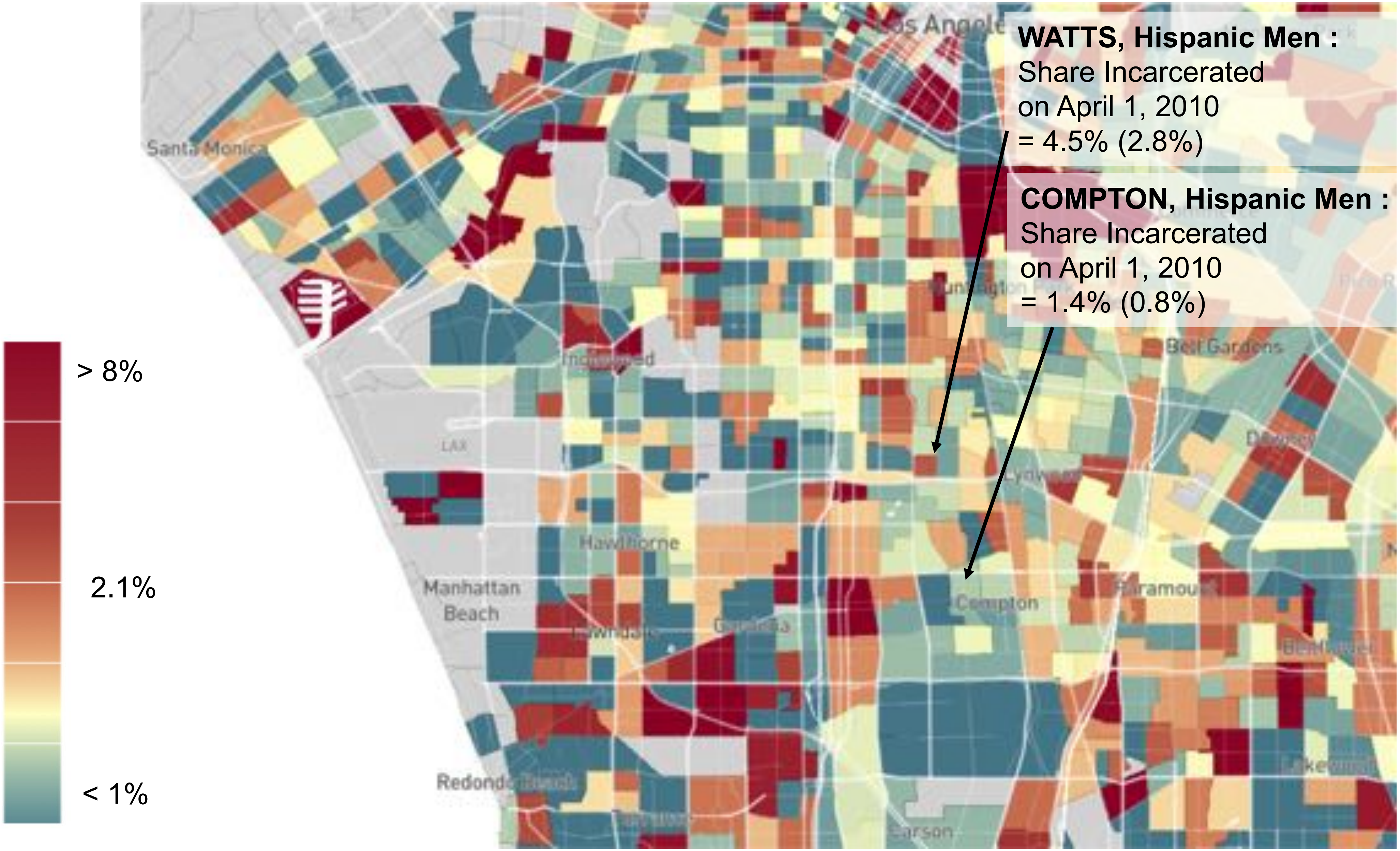


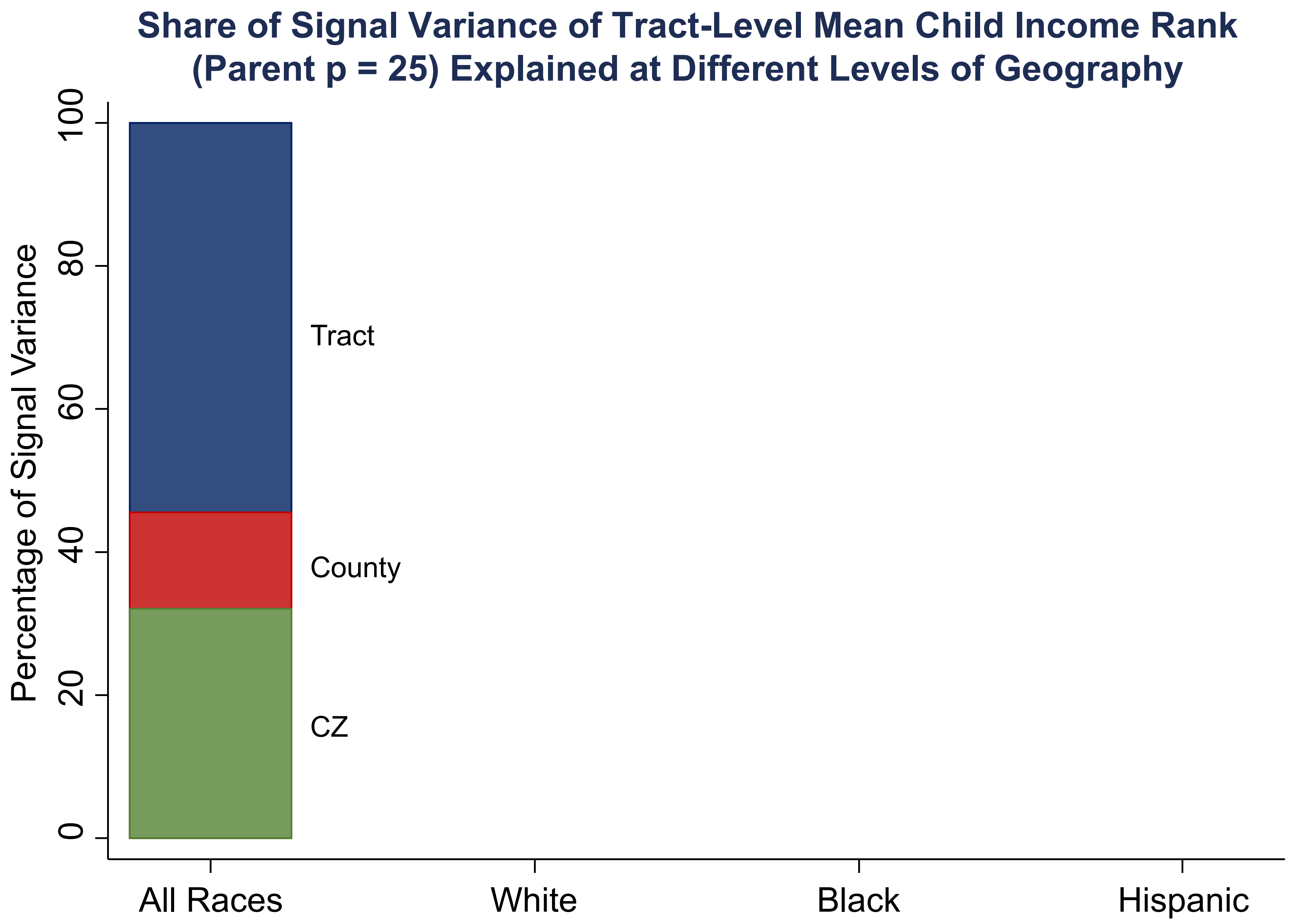
Incarceration Rates for <u>Hispanic Men</u> in Los Angeles with Parents Earning < \$2,200 (1st percentile)



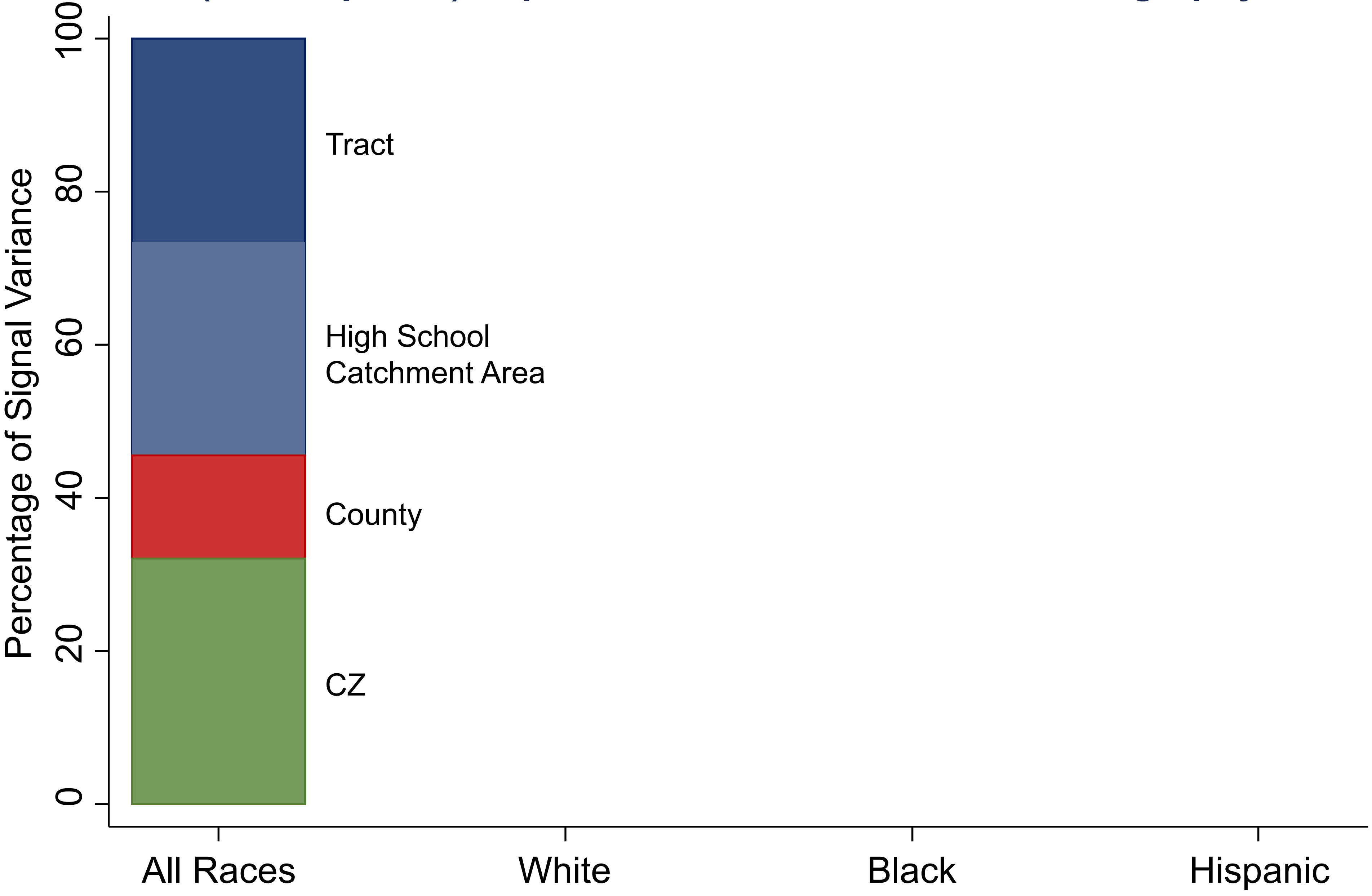
Targeting Place-Based Policies

- Example illustrates three general results on targeting:

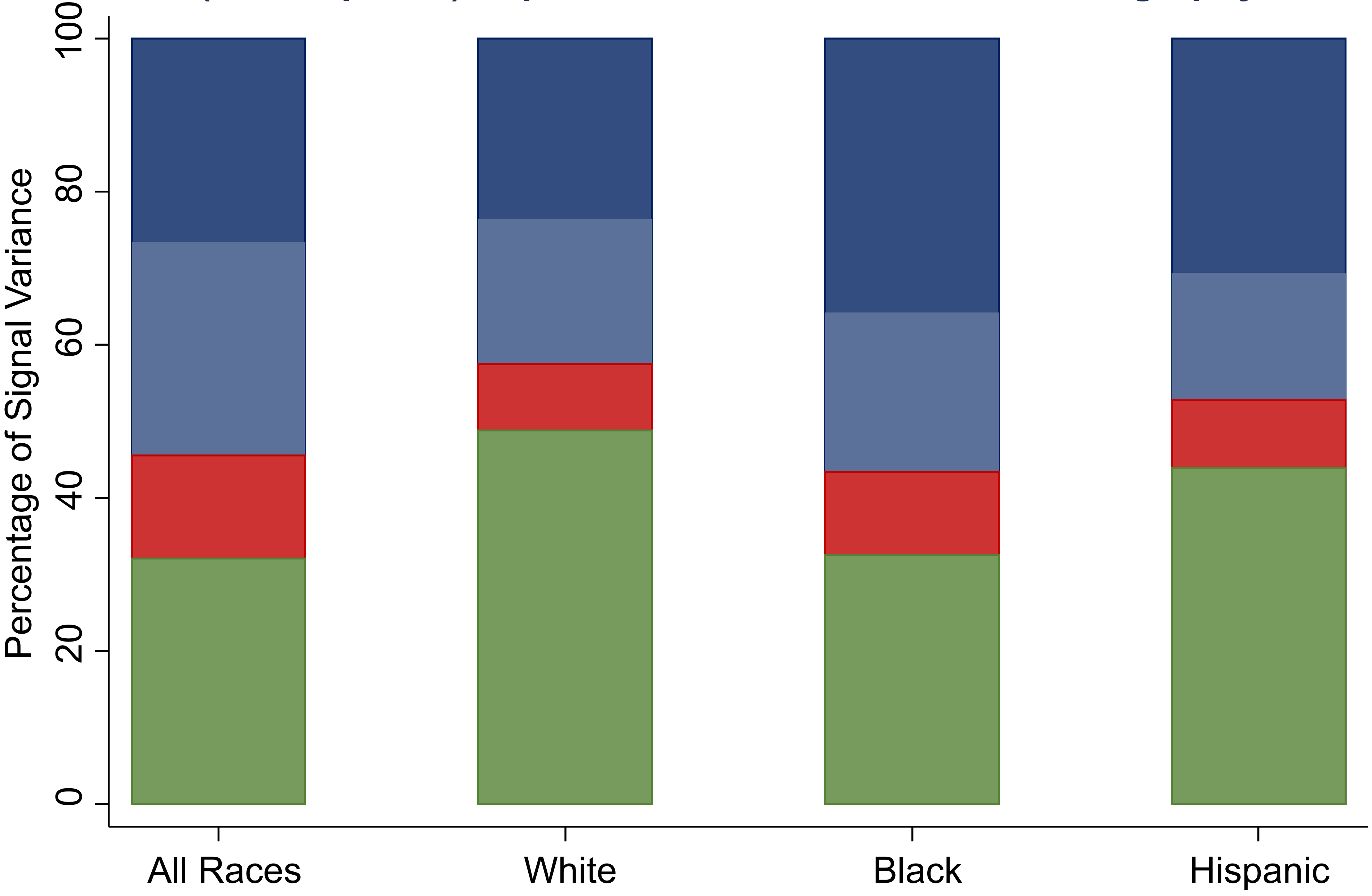
  1. Children's outcomes vary widely across nearby tracts → neighborhood where children grow up is a useful tag for policy interventions?


Share of Signal Variance of Tract-Level Mean Child Income Rank
(Parent p = 25) Explained at Different Levels of Geography
Percentage of Signal Variance
0
20
40
60
80
100
Tract
County
CZ
All Races
White
Black
Hispanic

Share of Signal Variance of Tract-Level Mean Child Income Rank (Parent p = 25) Explained at Different Levels of Geography



Share of Signal Variance of Tract-Level Mean Child Income Rank (Parent p = 25) Explained at Different Levels of Geography



Targeting Place-Based Policies

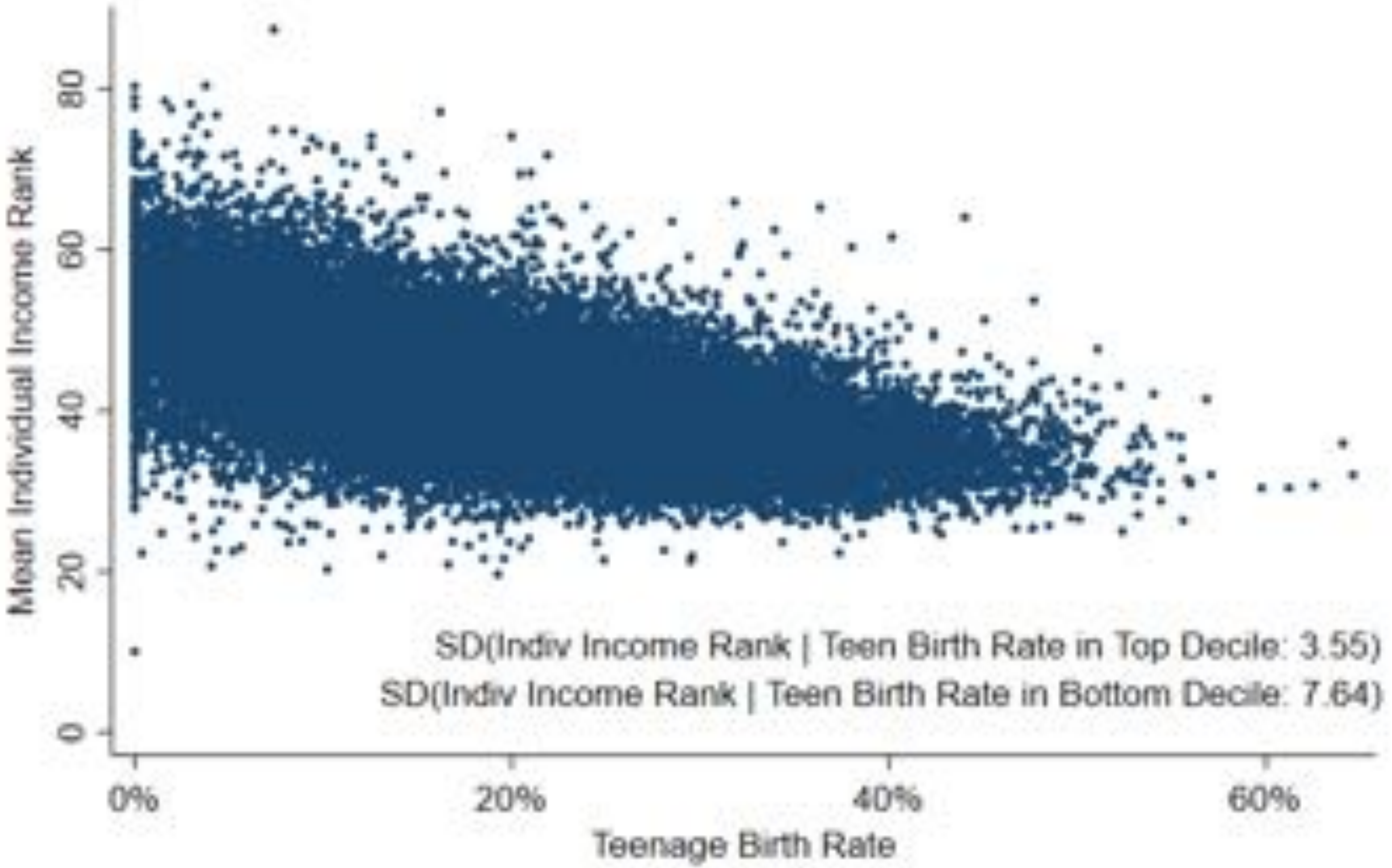
- Example illustrates three general results on targeting:

1. Children's outcomes vary widely across nearby tracts → location where children grow up is a useful tag for policy interventions

2. Substantial heterogeneity *within* areas across subgroups/outcomes cond. on parent income → neighborhoods not well described by a single-factor model

Upward Mobility vs. Teenage Birth Rates Across Tracts

White Women with Parents at 25th Percentile of Income Distribution



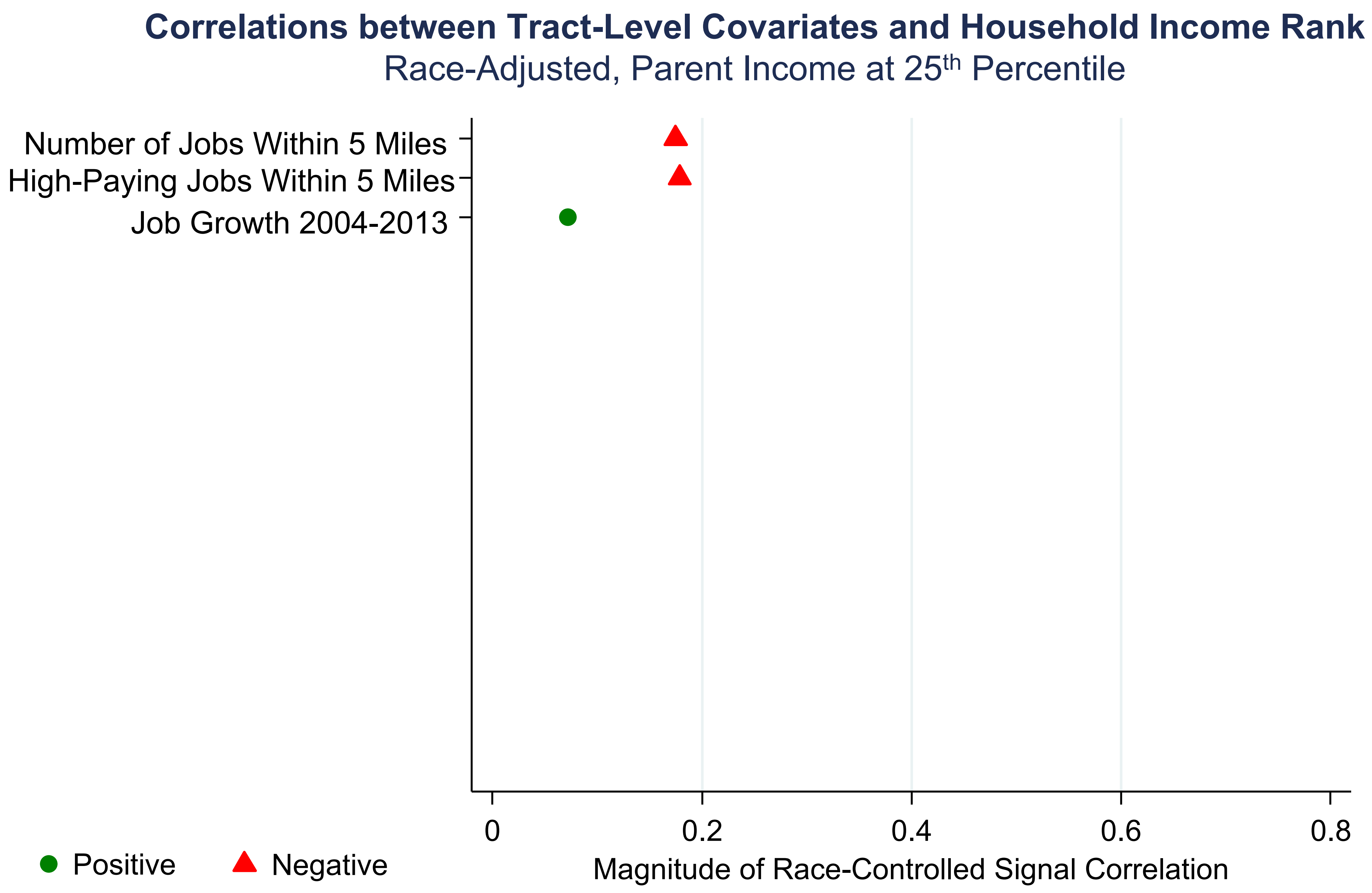
Targeting Place-Based Policies

- Example illustrates three general results on targeting:

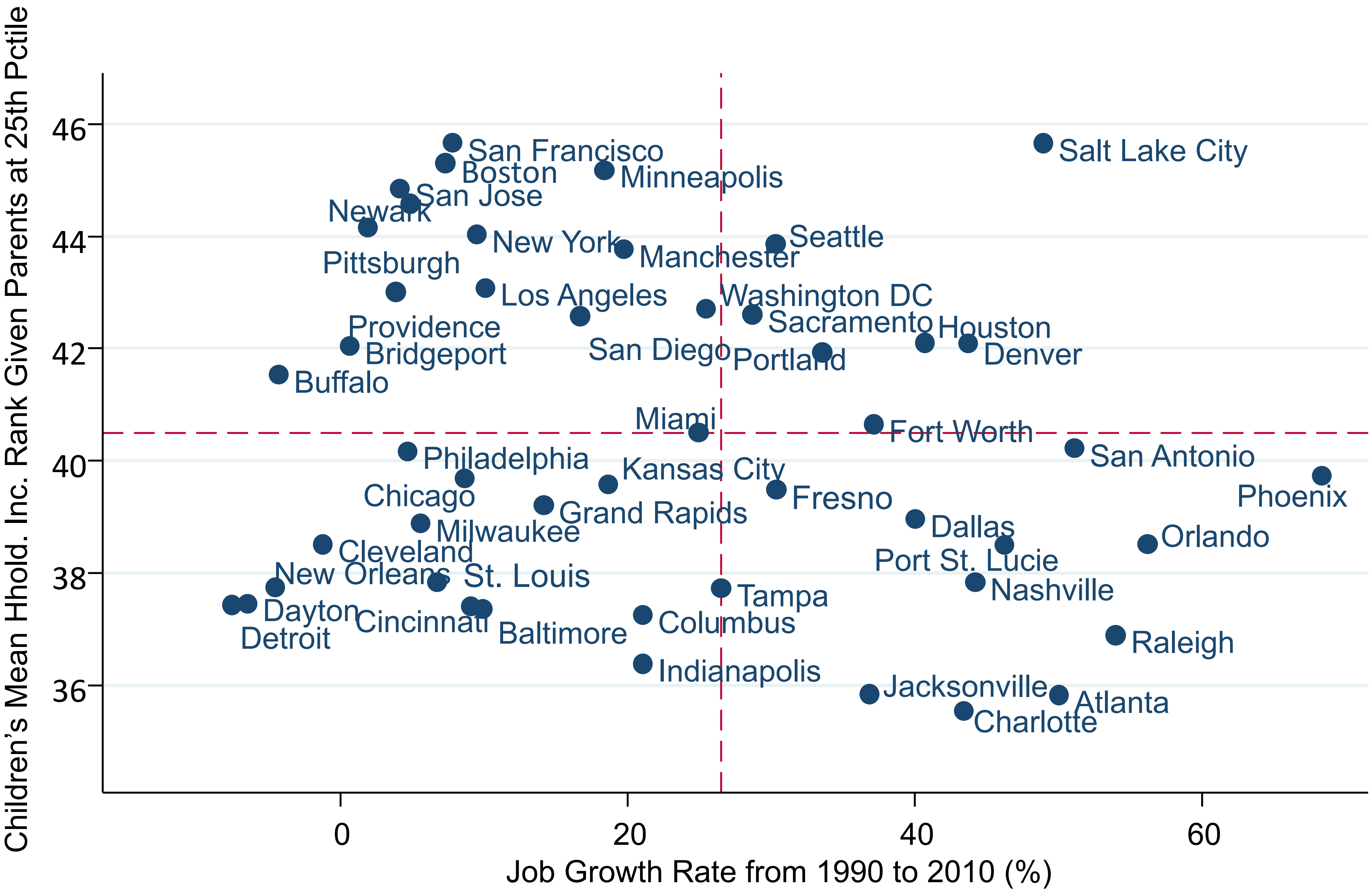
1. Children's outcomes vary widely across nearby tracts → location where children grow up is a useful tag for policy interventions

2. Substantial heterogeneity *within* areas across subgroups and outcomes cond. on parent income → neighborhoods not well described by a single-factor model

3. Outcome-based measures contain new information relative to traditional measures used to target policies, such as poverty rates or job growth


Correlations between Tract-Level Covariates and Household Income Rank
Race-Adjusted, Parent Income at 25th Percentile
Number of Jobs Within 5 Miles
High-Paying Jobs Within 5 Miles
Job Growth 2004-2013
0
0.2
0.4
0.6
0.8
Magnitude of Race-Controlled Signal Correlation
Positive
Negative

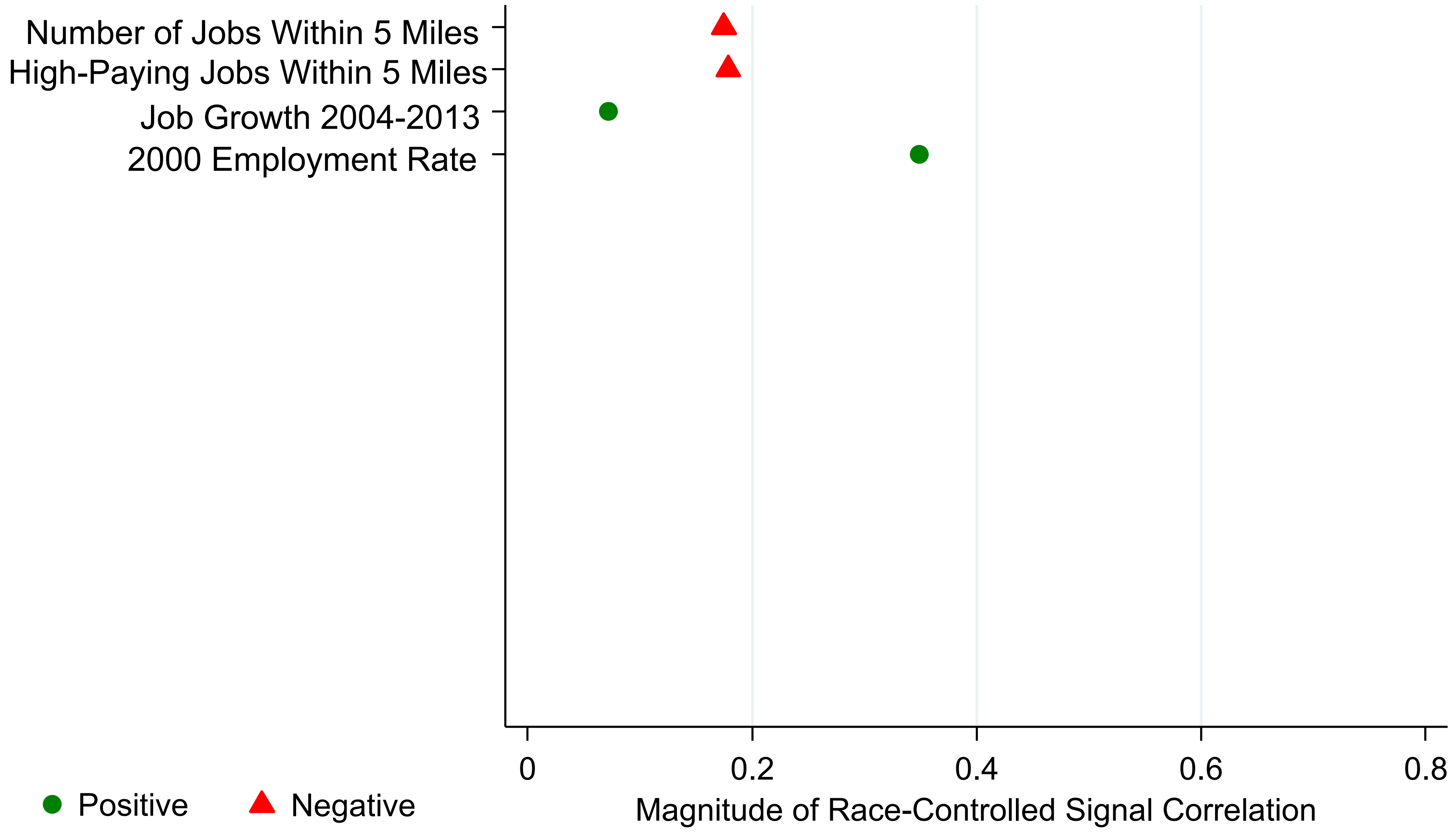
Upward Mobility vs. Job Growth in the 50 Largest Commuting Zones

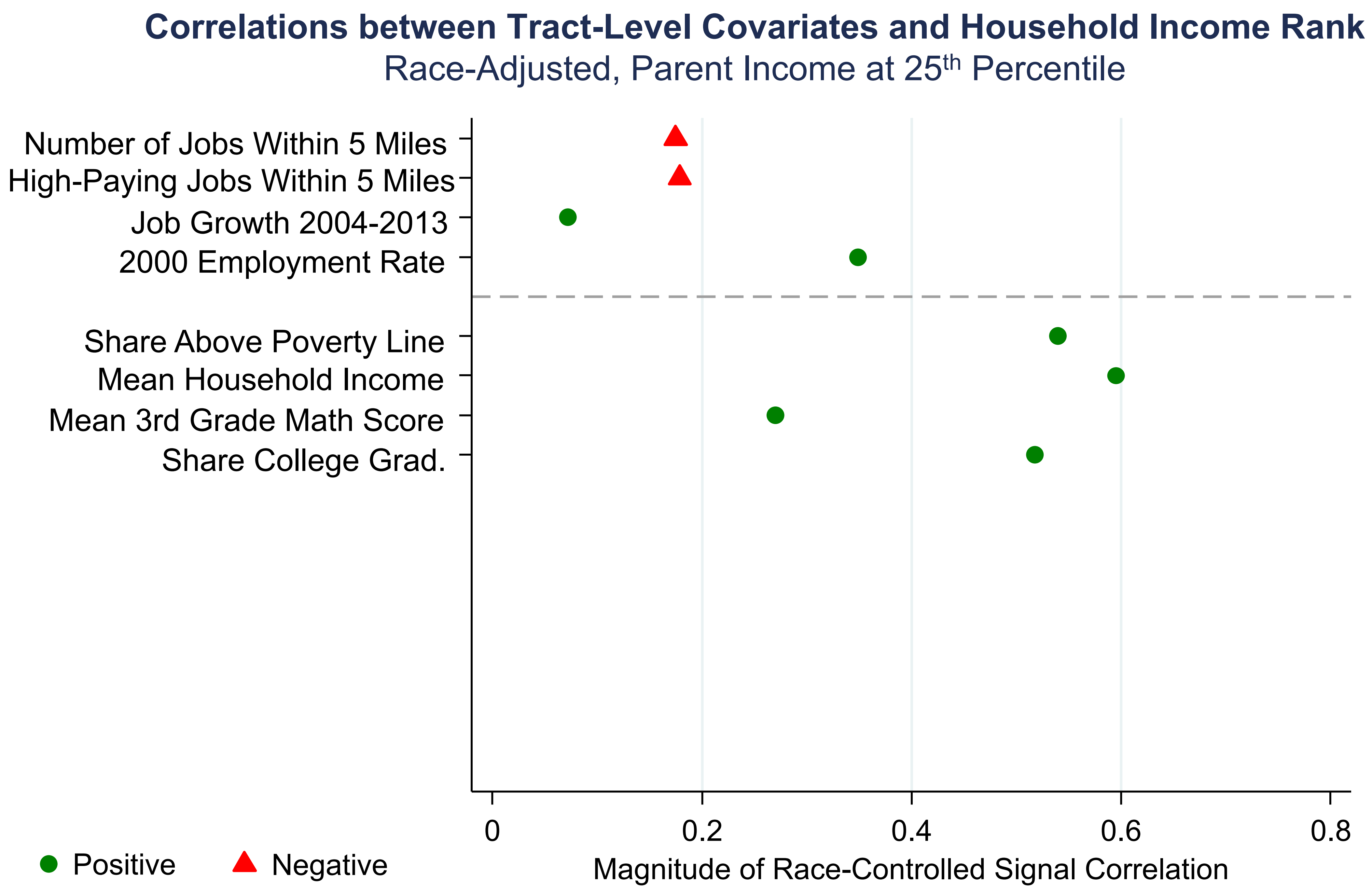


Correlation (across all CZs): -0.03

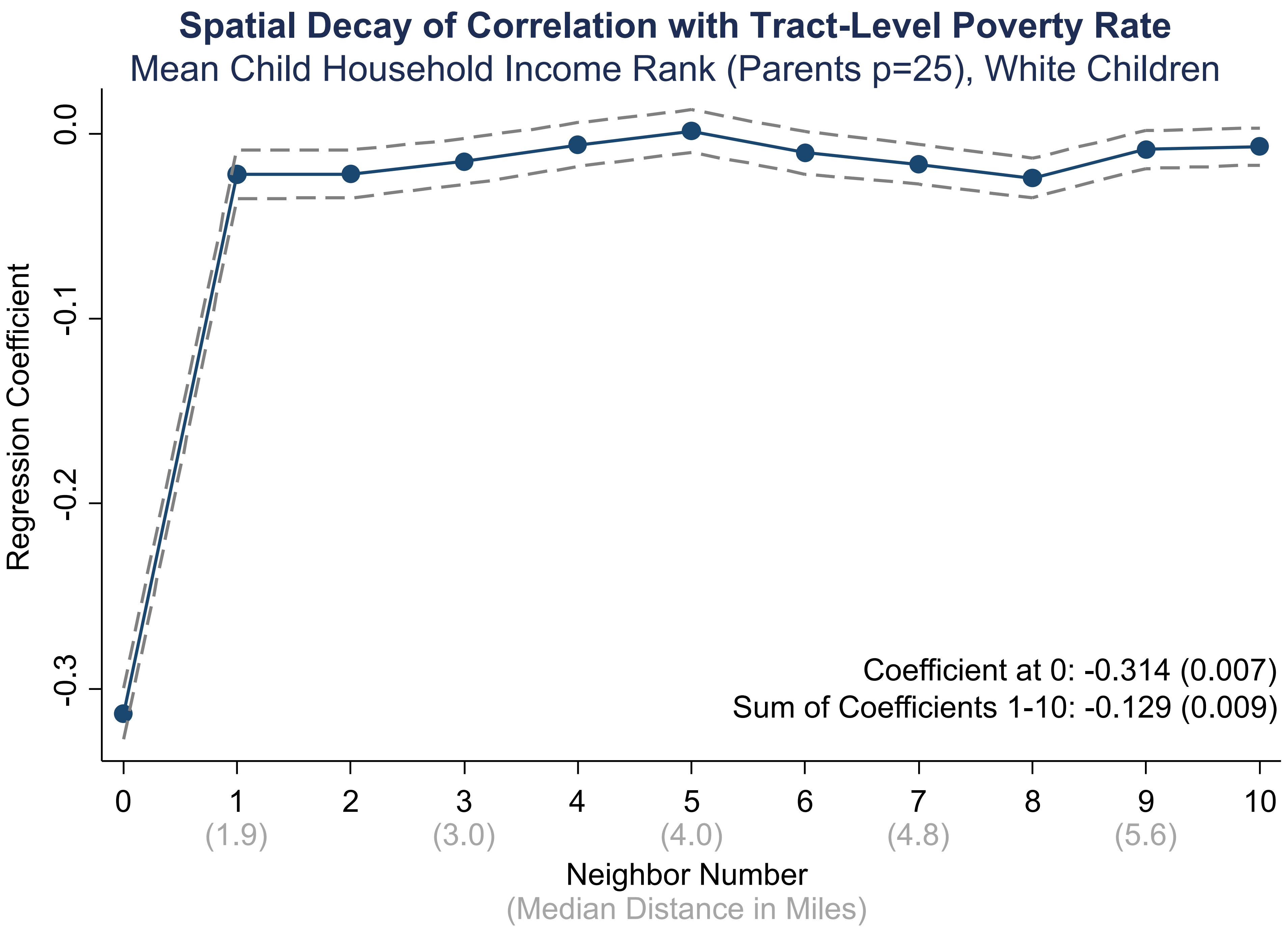
Correlations between Tract-Level Covariates and Household Income Rank

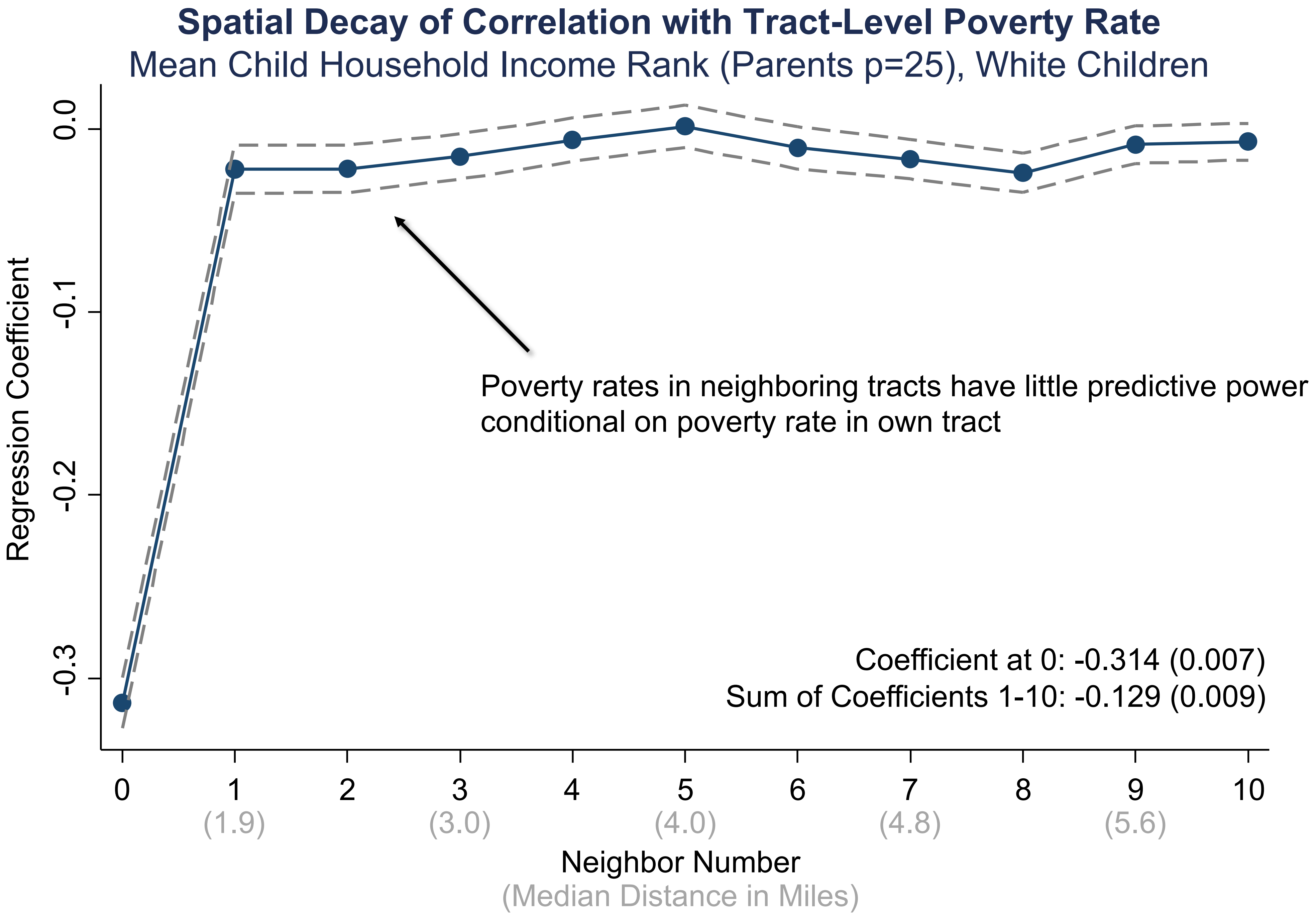
Race-Adjusted, Parent Income at 25th Percentile

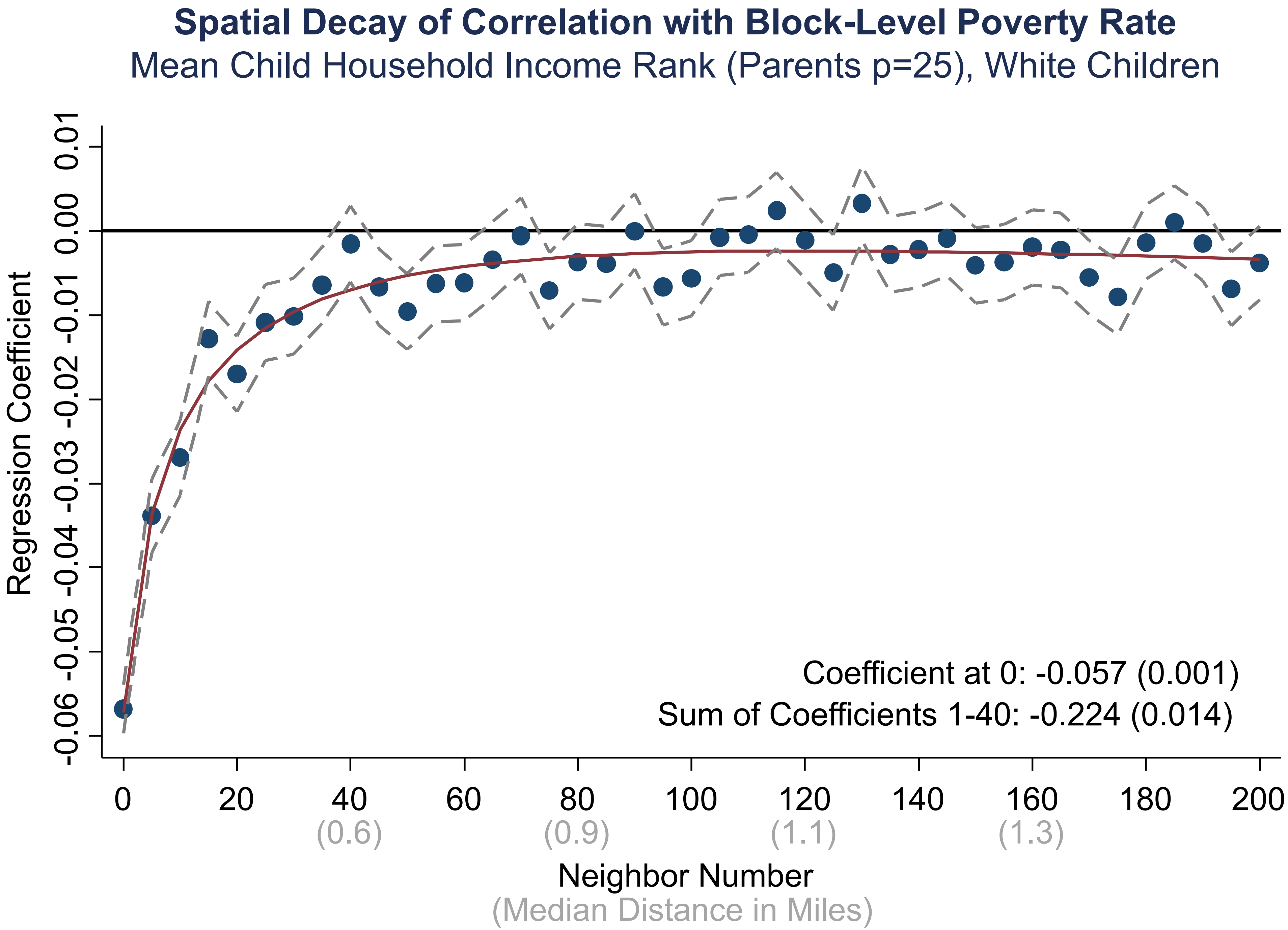




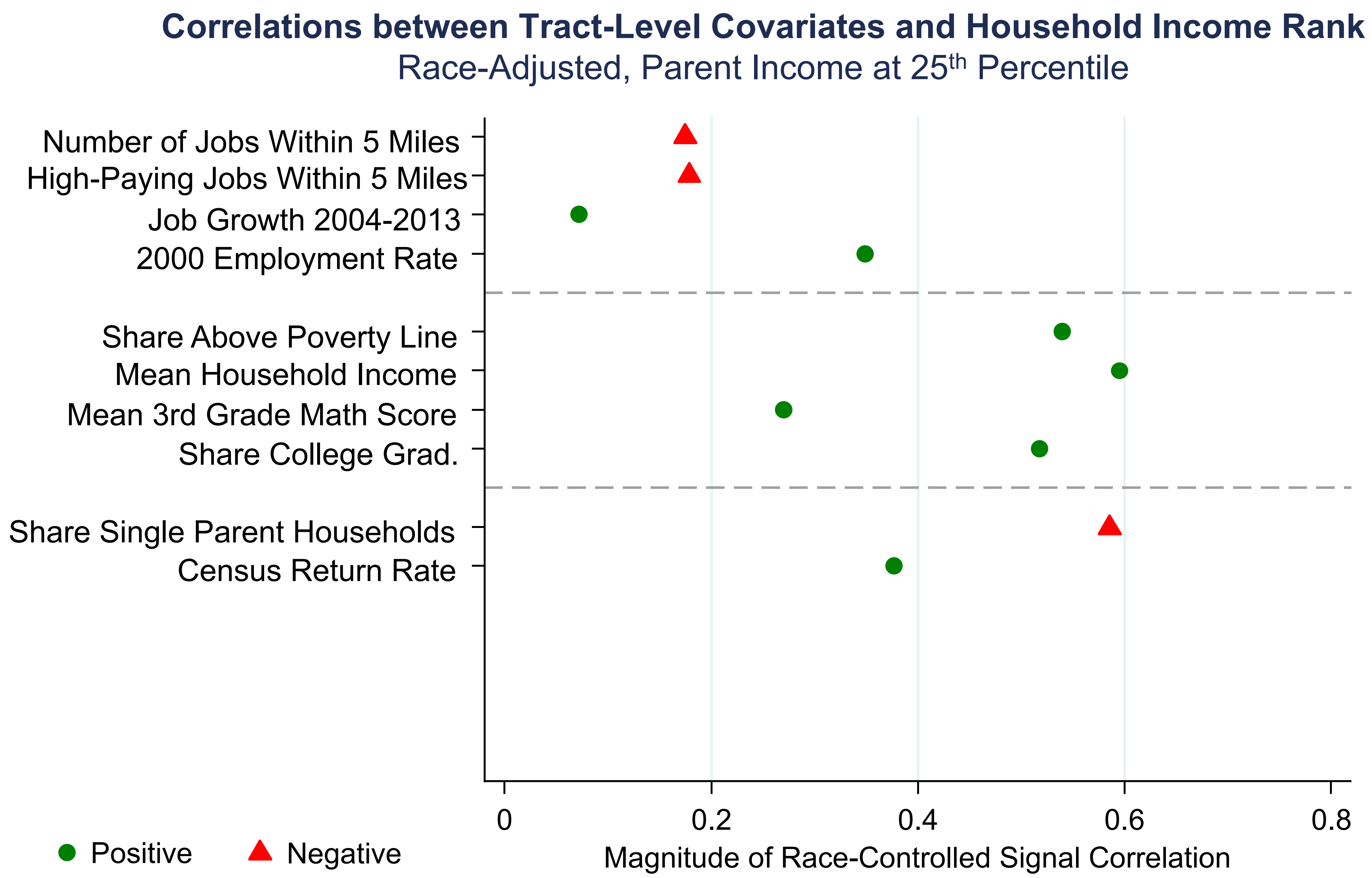
Correlations between Tract-Level Covariates and Household Income Rank
Race-Adjusted, Parent Income at 25th Percentile
Number of Jobs Within 5 Miles
High-Paying Jobs Within 5 Miles
Job Growth 2004-2013
2000 Employment Rate
Share Above Poverty Line
Mean Household Income
Mean 3rd Grade Math Score
Share College Grad.
0
0.2
0.4
0.6
0.8
Positive
Negative
Magnitude of Race-Controlled Signal Correlation

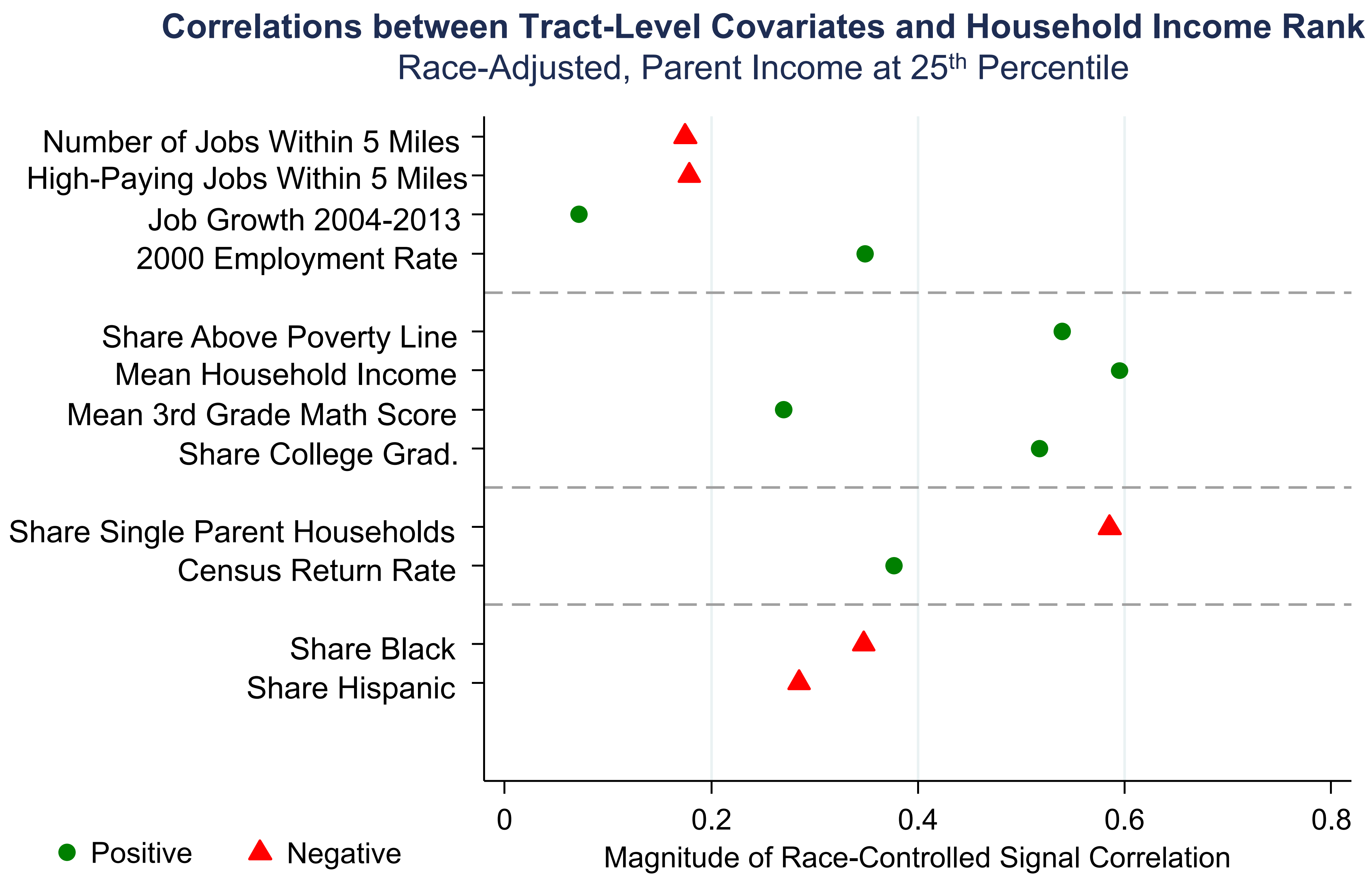

Spatial Decay of Correlation with Tract-Level Poverty Rate
Mean Child Household Income Rank (Parents p=25), White Children
Regression Coefficient
0.0
-0.1
-0.2
-0.3
Coefficient at 0: -0.314 (0.007)
Sum of Coefficients 1-10: -0.129 (0.009)
0
1
2
3
4
5
6
7
8
9
10
(1.9)
(3.0)
(4.0)
(4.8)
(5.6)
Neighbor Number
(Median Distance in Miles)


Spatial Decay of Correlation with Tract-Level Poverty Rate
Mean Child Household Income Rank (Parents p=25), White Children
Regression Coefficient
0.0
-0.1
-0.2
-0.3
Poverty rates in neighboring tracts have little predictive power
conditional on poverty rate in own tract
Coefficient at 0: -0.314 (0.007)
Sum of Coefficients 1-10: -0.129 (0.009)
0
1
2
3
4
5
6
7
8
9
10
(1.9)
(3.0)
(4.0)
(4.8)
(5.6)
Neighbor Number
(Median Distance in Miles)

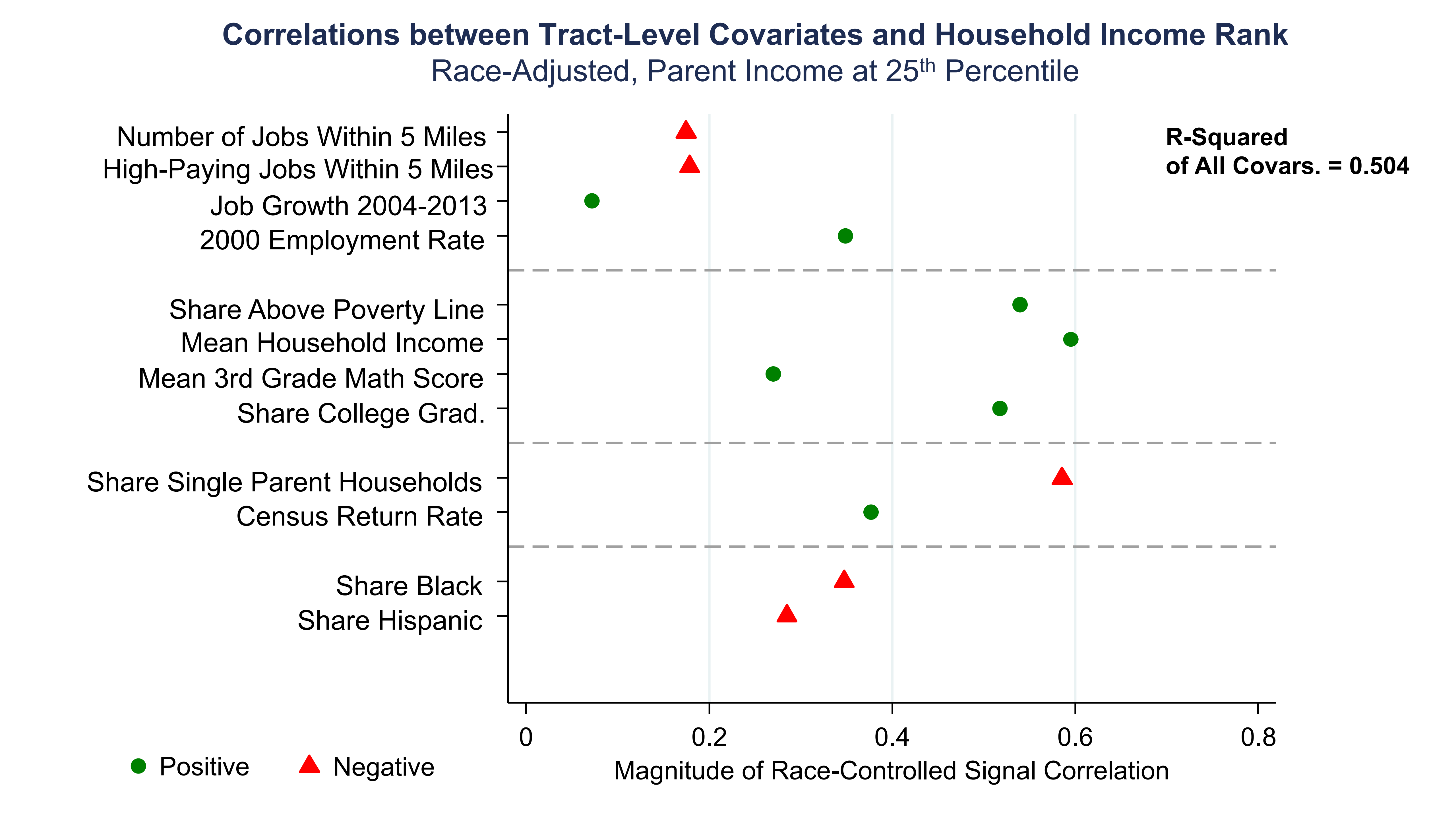


Spatial Decay of Correlation with Block-Level Poverty Rate
Mean Child Household Income Rank (Parents p=25), White Children
Regression Coefficient
0.01
0.00
-0.01
-0.02
-0.03
-0.04
-0.05
-0.06
Coefficient at 0: -0.057 (0.001)
Sum of Coefficients 1-40: -0.224 (0.014)
0
20
40
(0.6)
60
80
(0.9)
100
120
(1.1)
140
160
(1.3)
180
200
Neighbor Number
(Median Distance in Miles)

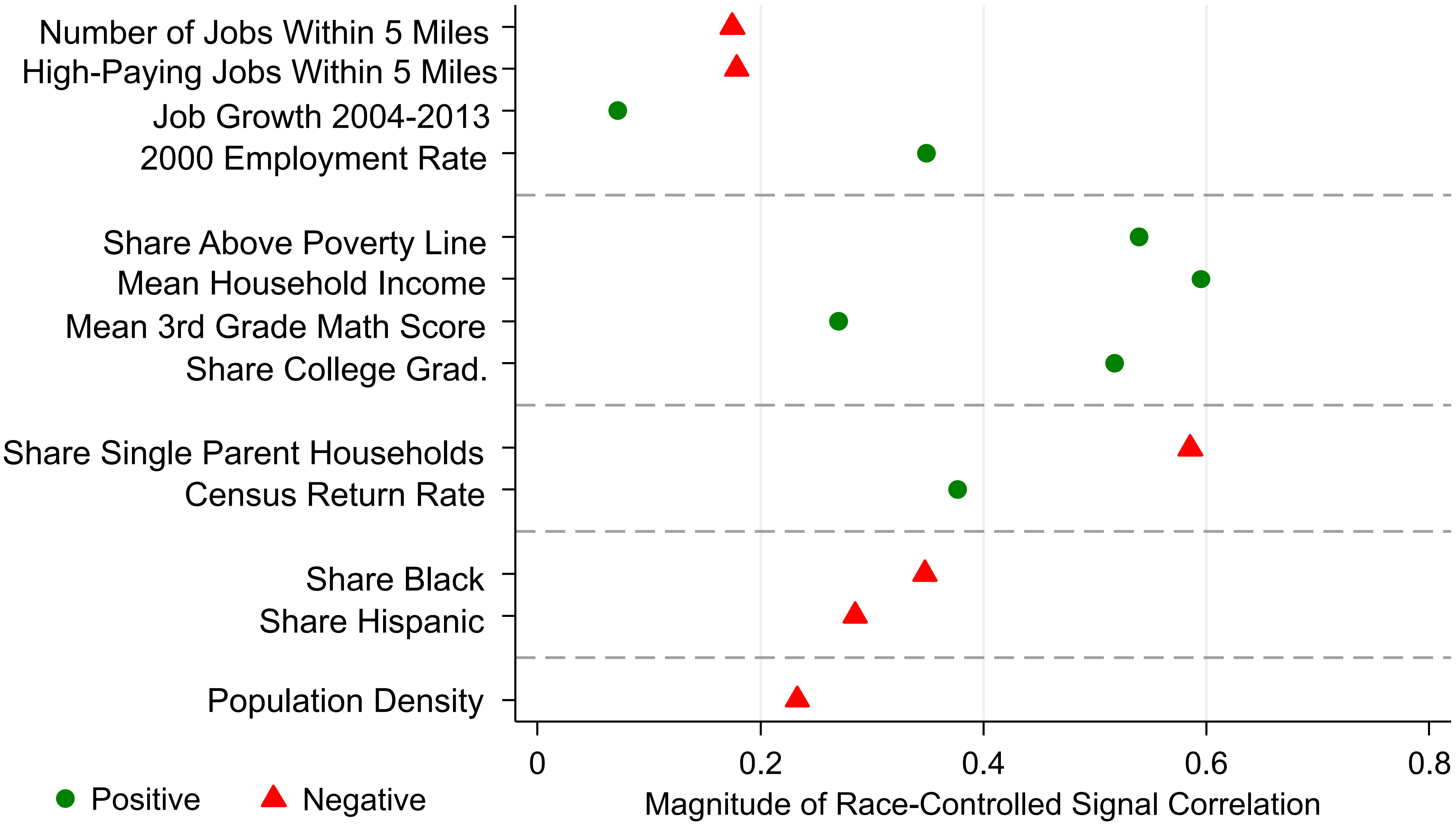

Correlations between Tract-Level Covariates and Household Income Rank
Race-Adjusted, Parent Income at 25th Percentile
Number of Jobs Within 5 Miles
High-Paying Jobs Within 5 Miles
Job Growth 2004-2013
2000 Employment Rate
Share Above Poverty Line
Mean Household Income
Mean 3rd Grade Math Score
Share College Grad.
Share Single Parent Households
Census Return Rate
0
0.2
0.4
0.6
0.8
Magnitude of Race-Controlled Signal Correlation
Positive
Negative



Correlations between Tract-Level Covariates and Household Income Rank
Race-Adjusted, Parent Income at 25th Percentile
Number of Jobs Within 5 Miles
High-Paying Jobs Within 5 Miles
Job Growth 2004-2013
2000 Employment Rate
Share Above Poverty Line
Mean Household Income
Mean 3rd Grade Math Score
Share College Grad.
Share Single Parent Households
Census Return Rate
Share Black
Share Hispanic
0
0.2
0.4
0.6
0.8
Magnitude of Race-Controlled Signal Correlation
Positive
Negative

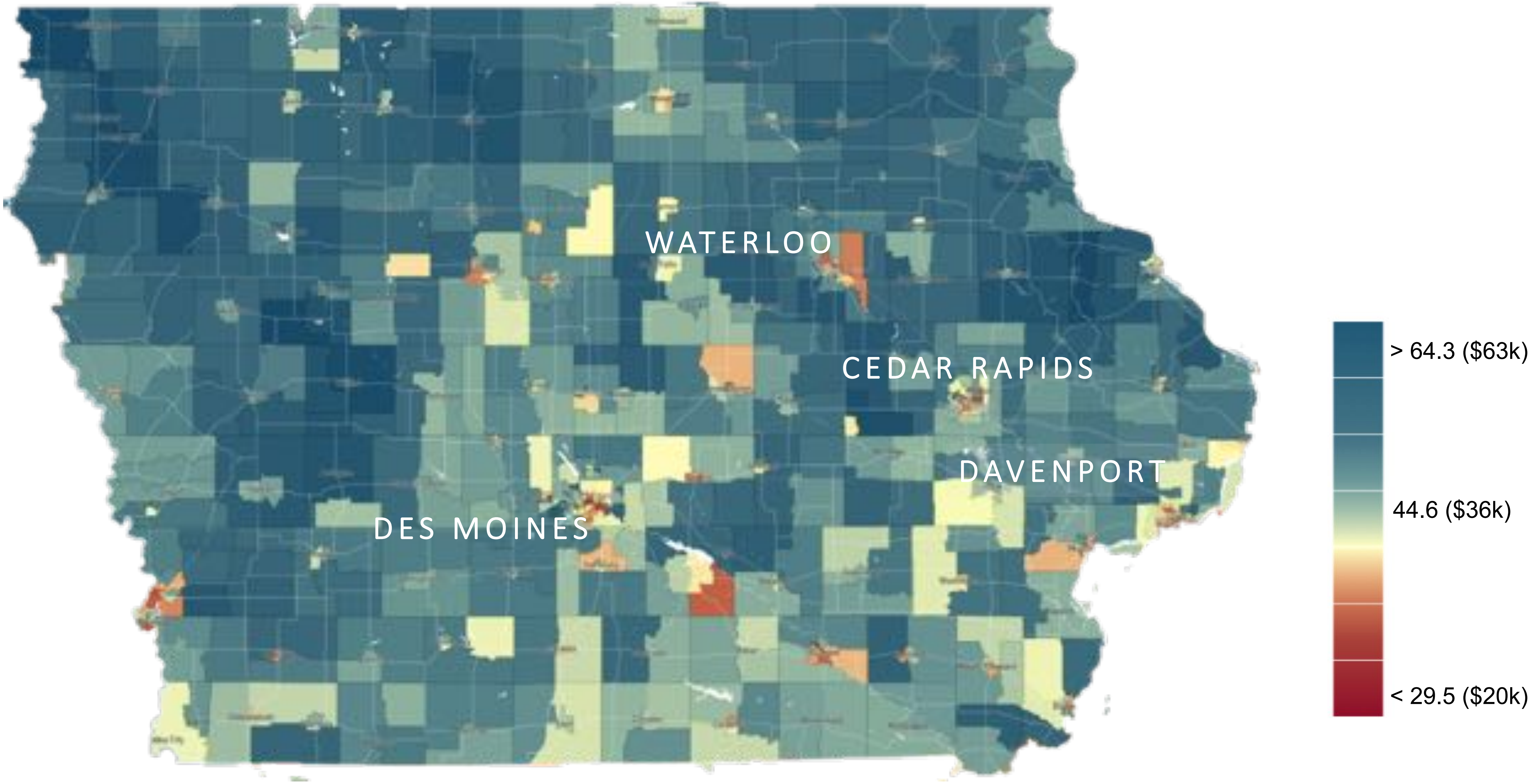

Correlations between Tract-Level Covariates and Household Income Rank
Race-Adjusted, Parent Income at 25th Percentile
R-Squared
of All Covars. = 0.504
Number of Jobs Within 5 Miles
High-Paying Jobs Within 5 Miles
Job Growth 2004-2013
2000 Employment Rate
Share Above Poverty Line
Mean Household Income
Mean 3rd Grade Math Score
Share College Grad.
Share Single Parent Households
Census Return Rate
Share Black
Share Hispanic
0
0.2
0.4
0.6
0.8
Magnitude of Race-Controlled Signal Correlation
Positive
Negative

Correlations between Tract-Level Covariates and Household Income Rank
Race-Adjusted, Parent Income at 25th Percentile



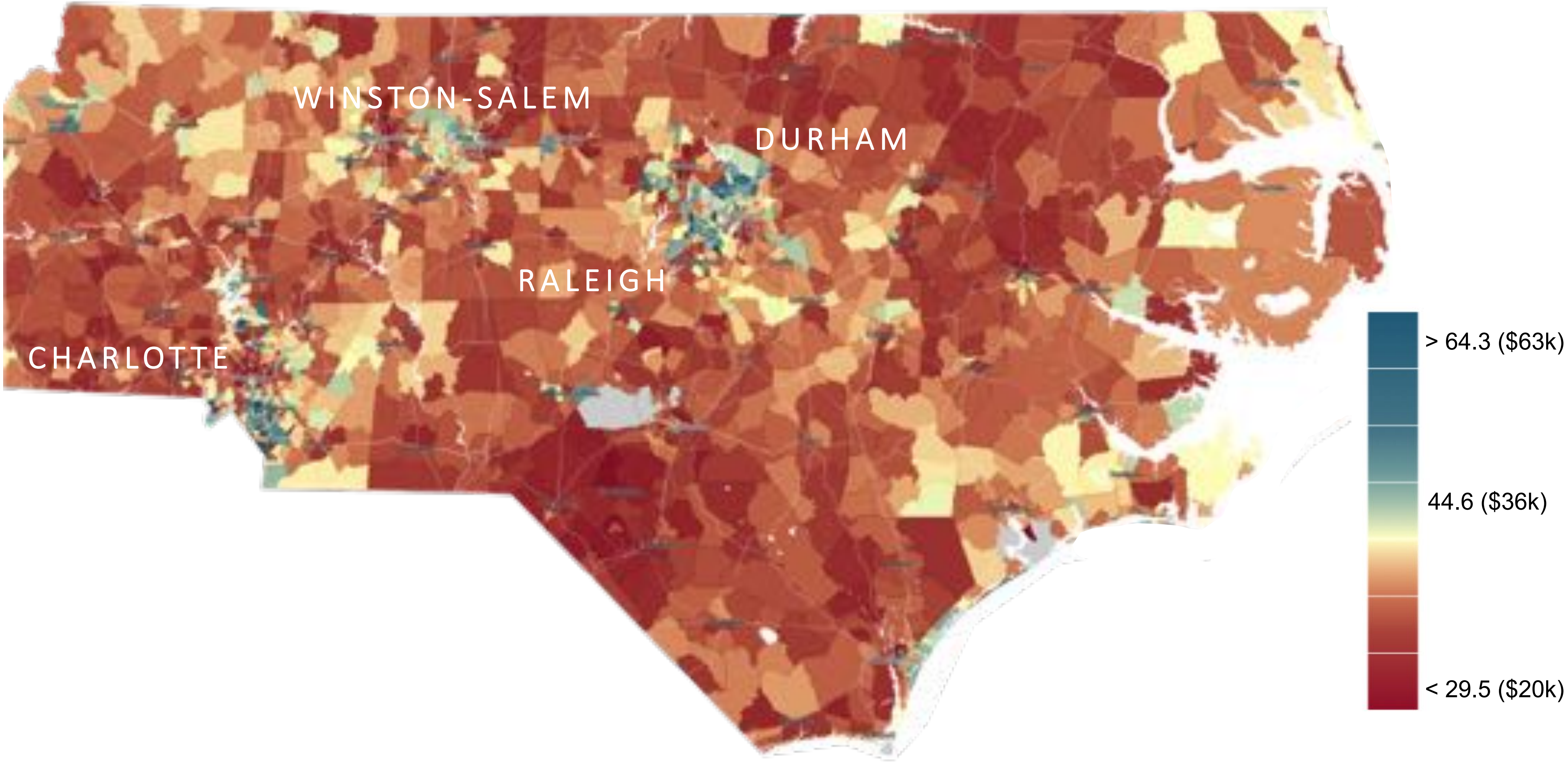
Do Cities Offer Greater Opportunities for Upward Mobility?

Average Income for White Children with Parents Earning $25,000 in Iowa



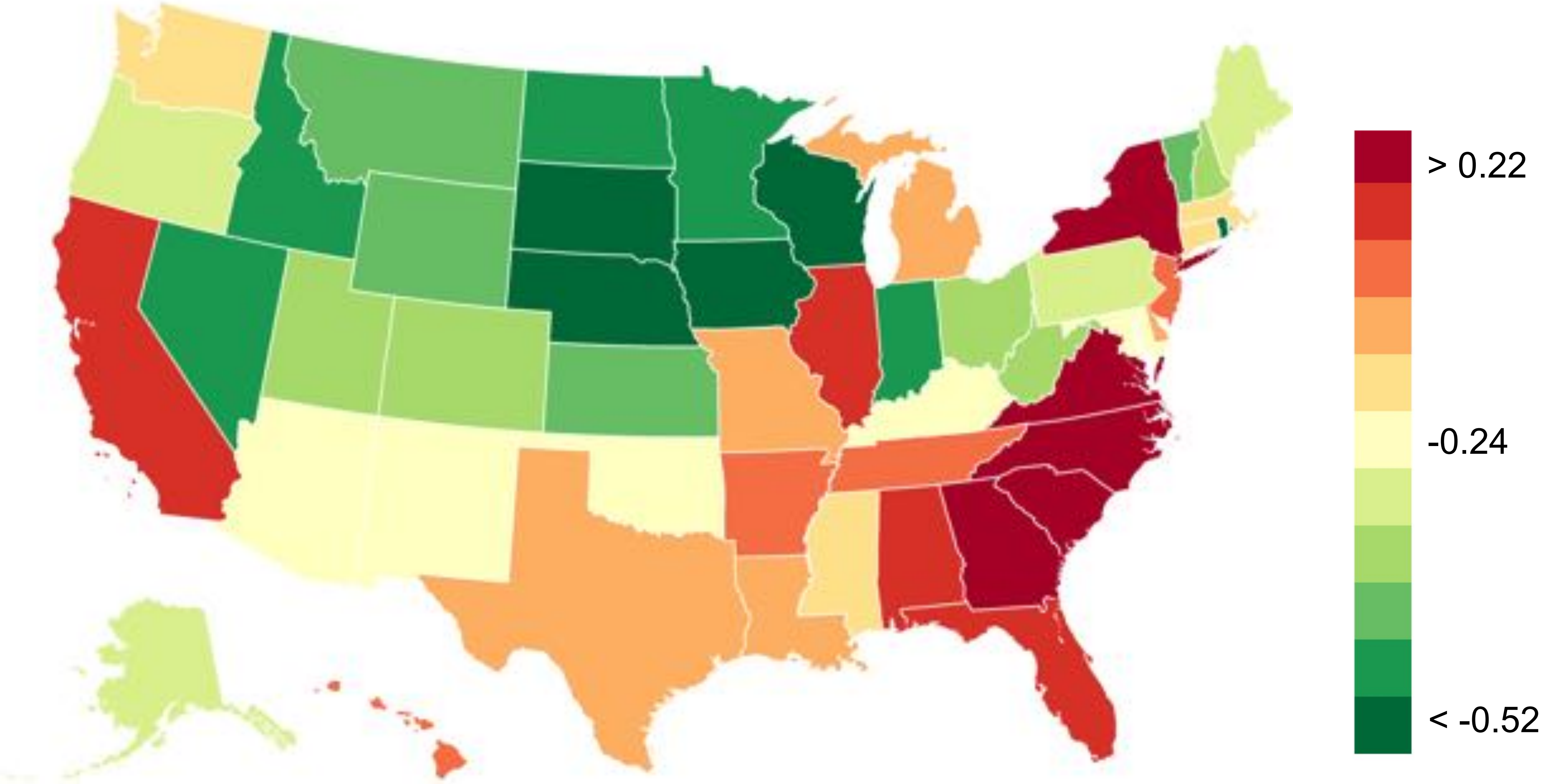
Do Cities Offer Greater Opportunities for Upward Mobility?

Average Income for White Children with Parents Earning $25,000 in North Carolina



Correlations between Population Density and Household Income Rank Across Tracts, by State

White Children, Parent Income at 25th Percentile

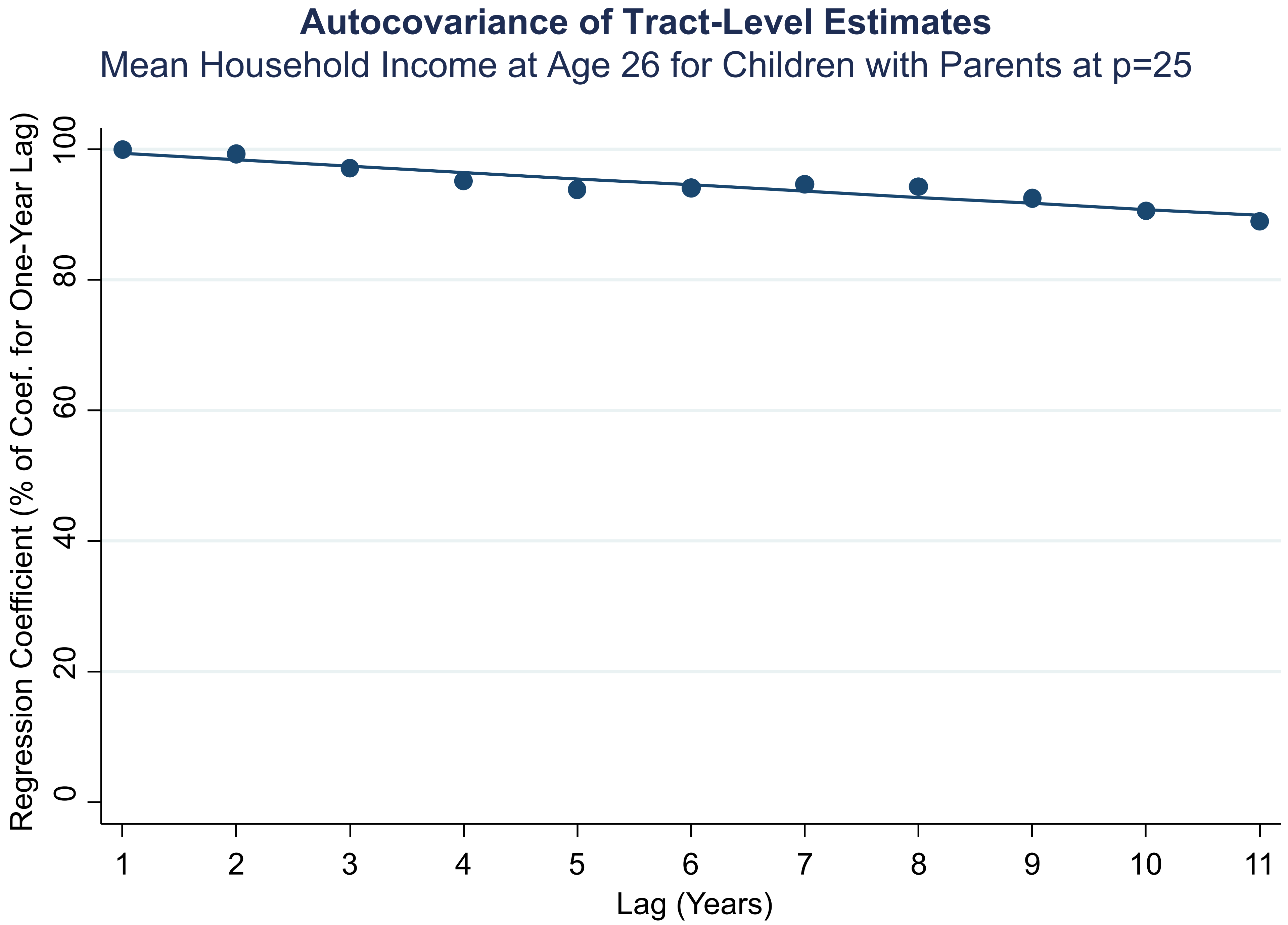


Using Location as a Tag for Policy

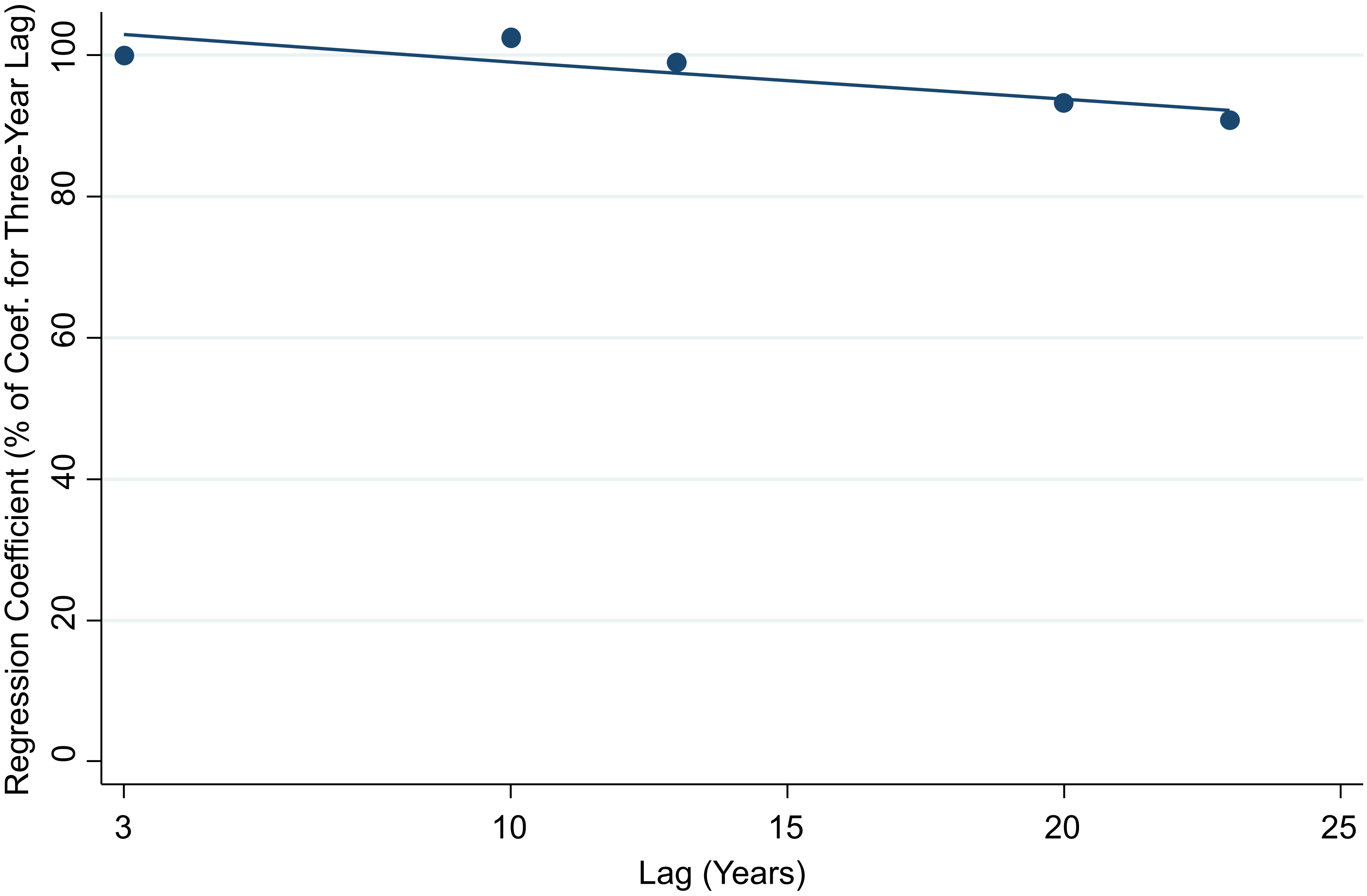
- Tract-level estimates of children’s outcomes appear to provide new information that could be helpful in identifying areas where opportunity is most lacking

- Practical challenge in using these estimates to inform policy: they come with a lag, since one must wait until children grow up to observe their earnings

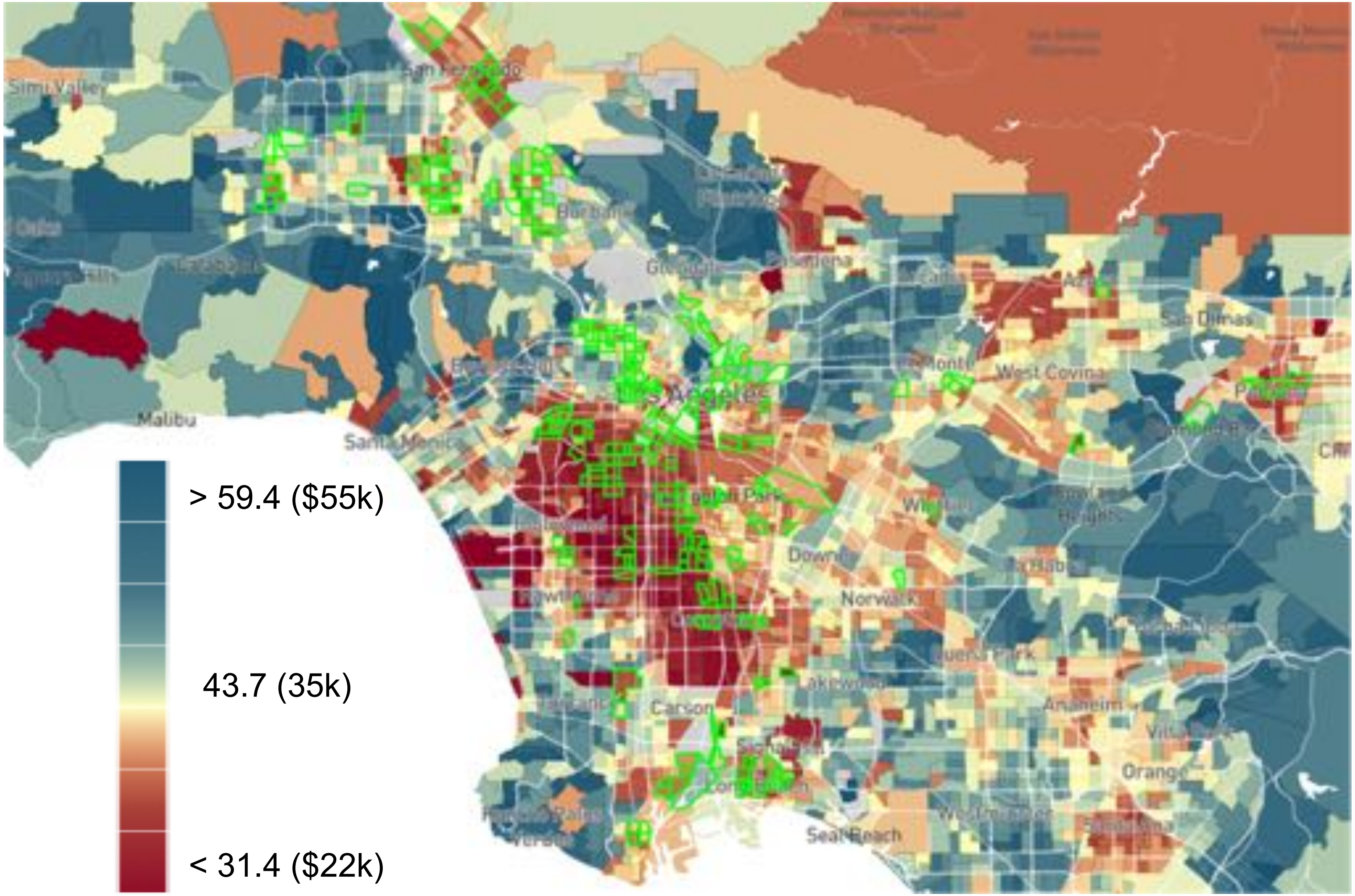
- Are historical estimates useful predictors of opportunity for current cohorts?


Autocovariance of Tract-Level Estimates
Mean Household Income at Age 26 for Children with Parents at p=25
Regression Coefficient (% of Coef. for One-Year Lag)
0
20
40
60
80
100
1
2
3
4
5
6
7
8
9
10
11
Lag (Years)

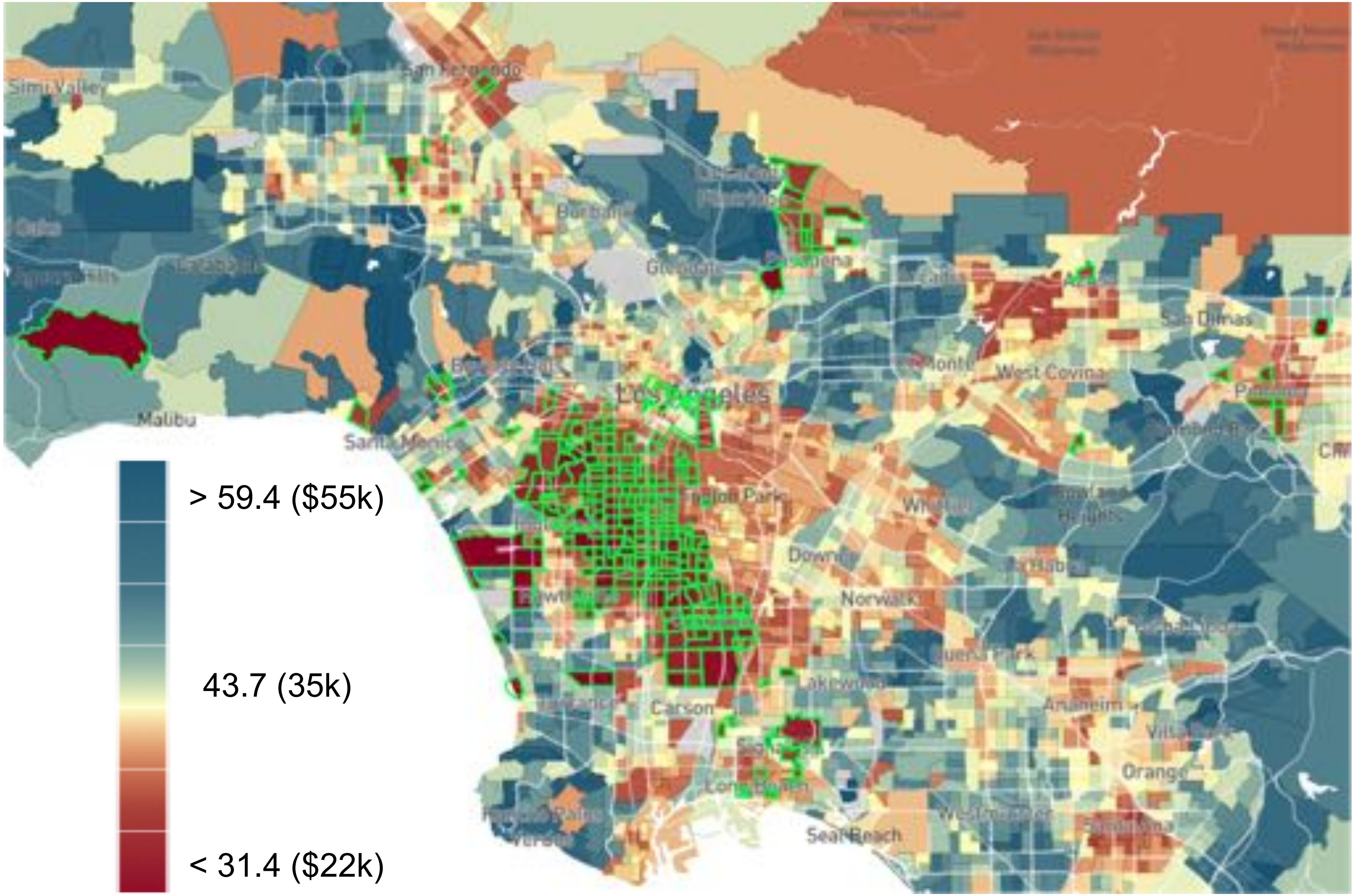
Autocovariance of Tract-Level Poverty Rates Using Publicly Available Census Data



Illustrative Application: Currently Designated Opportunity Zones in Los Angeles County



Hypothetical Opportunity Zones using Upward Mobility Estimates



Stepping Back: Rationale for Targeting by Place?

- Does this evidence provide a rationale for targeting?

- Open questions:

  - Is it actually more effective / efficient to target children by place as opposed to all disadvantaged children?

- Are there other distortions induced from place-based targeting?

  - E.g. migration?

  - Incentives for places to have lower outcomes?

Causal Effects of Place

- Do places have causal effects on outcomes?
- Can we change outcomes by changing places?
- Where might families want to live?
- Questions can be related:
  - Many affordable housing programs (e.g., Housing Choice Vouchers) have explicit goal of helping low-income families access “higher opportunity” areas

Sorting versus Causal Effects: CZ Evidence

- Chetty and Hendren (2018) 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:

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

  - Exposure effect at age *m* is $\beta_{m-1} - \beta_m$

Estimating Exposure Effects 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 (θ_i) that produces better child outcomes

- Estimating (1) in observational data yields a coefficient

$$b_m = \beta_m + \delta_m$$

where $\delta_m = \frac{Cov\left(\theta_i, \bar{y}_{pds}\right)}{var\left(\bar{y}_{pds}\right)}$ is a standard selection effect

Estimating Exposure Effects in Observational Data

- 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

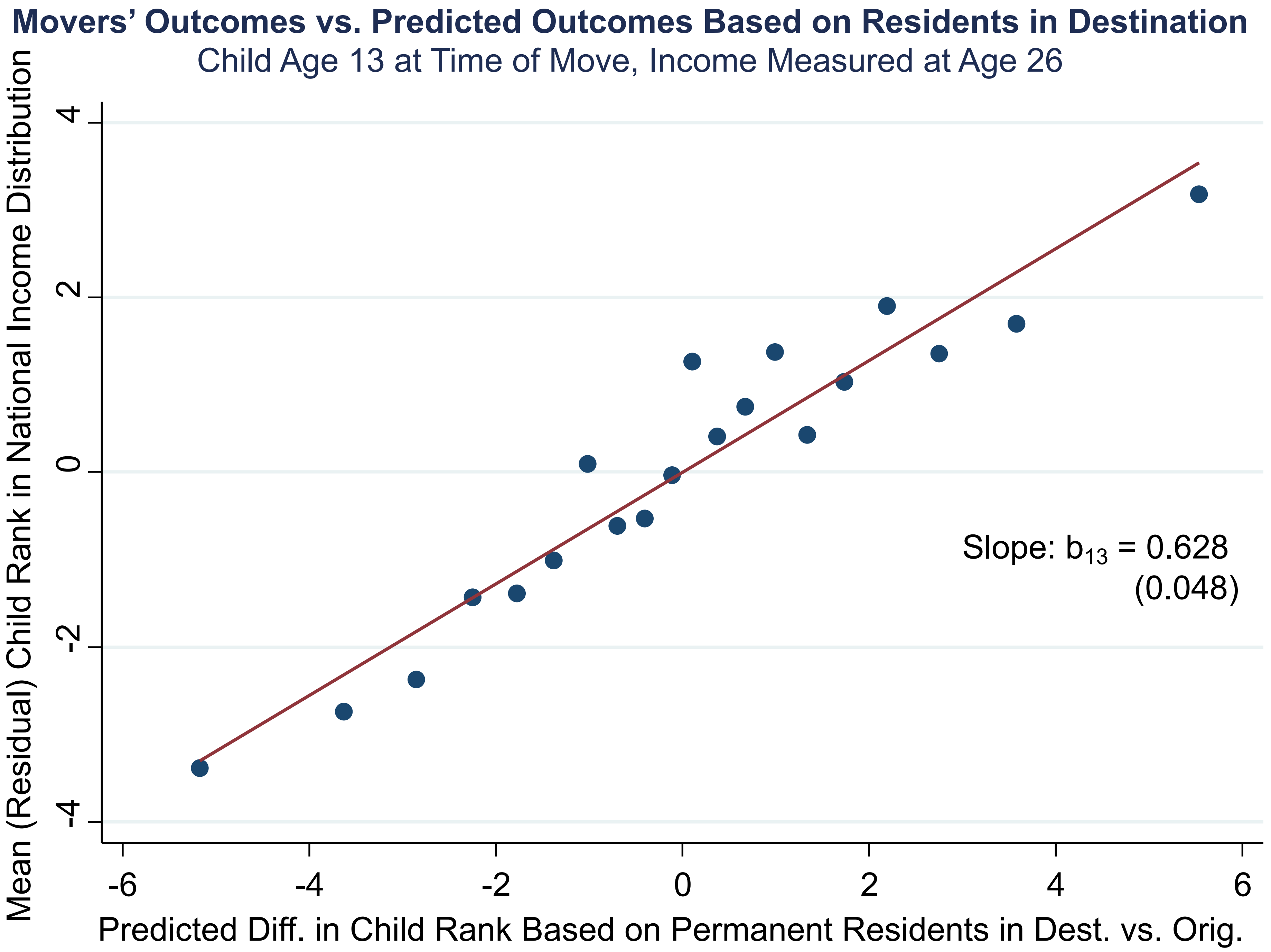
Estimating Exposure Effects in Observational Data

- 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:

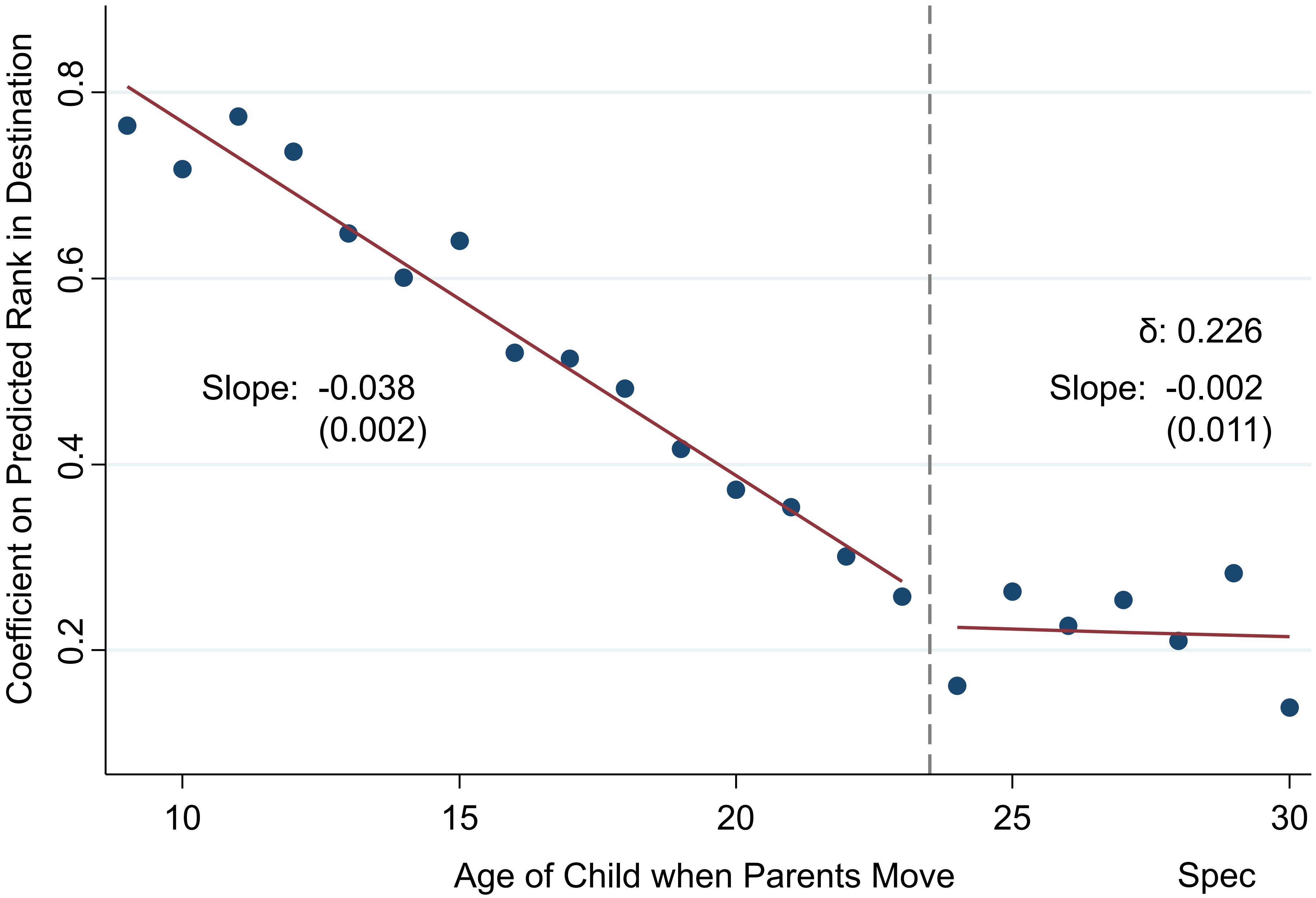
$$y_i = \alpha_{qos} + b_m \bar{y}_{pds} + \eta_{1i}$$

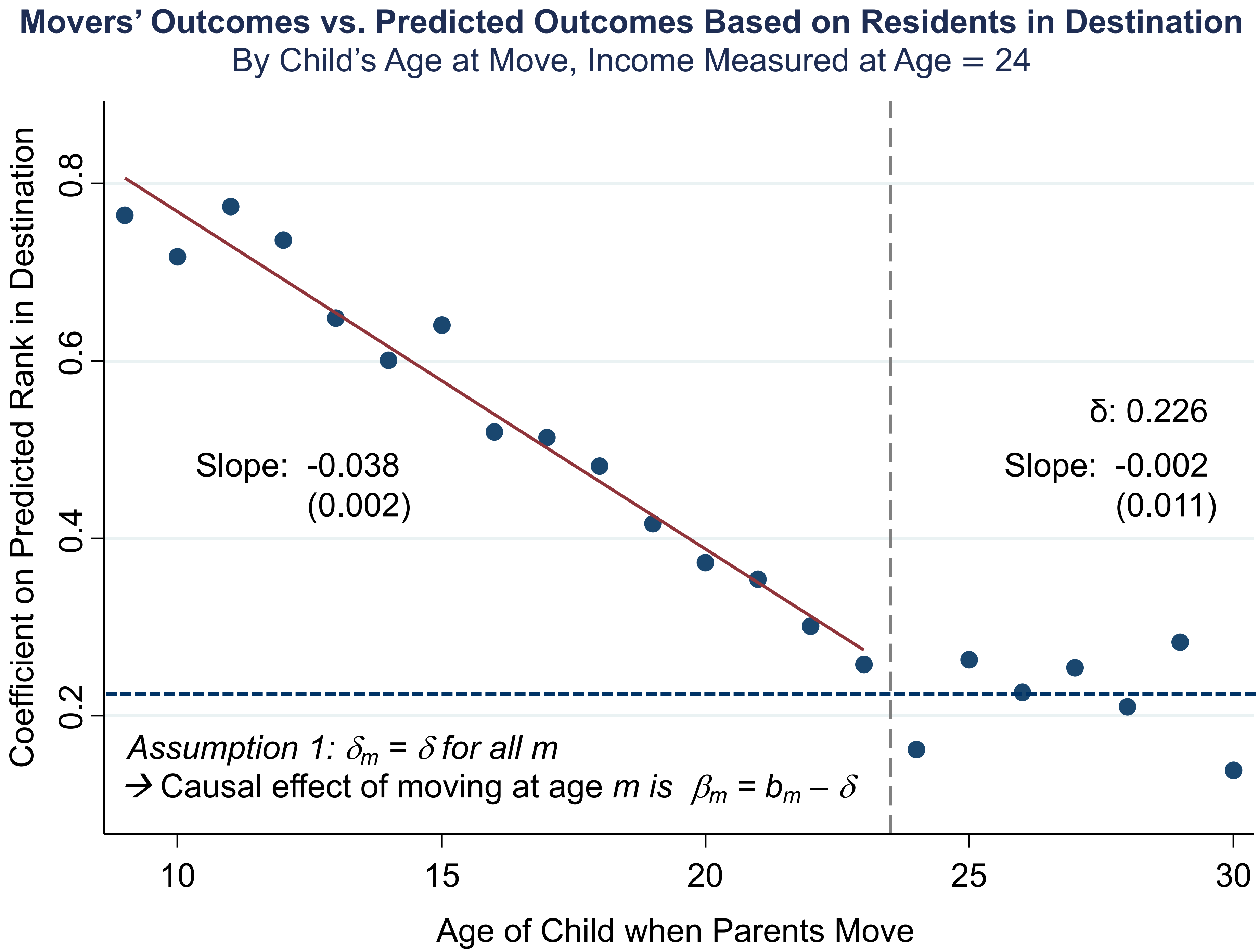
- Include parent decile (q) by origin (o) by birth cohort (s) fixed effects to identify b_m purely from differences in *destinations*



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
-4
-2
0
2
4
-6
-4
-2
0
2
4
6
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




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
0.8
0.6
0.4
0.2
δ: 0.226
Slope: -0.038
(0.002)
Slope: -0.002
(0.011)
Assumption 1: $\delta_m = \delta$ for all m
→ Causal effect of moving at age m is $\beta_m = b_m - \delta$
10
15
20
25
30
Age of Child when Parents Move

Identifying Causal Exposure Effect

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

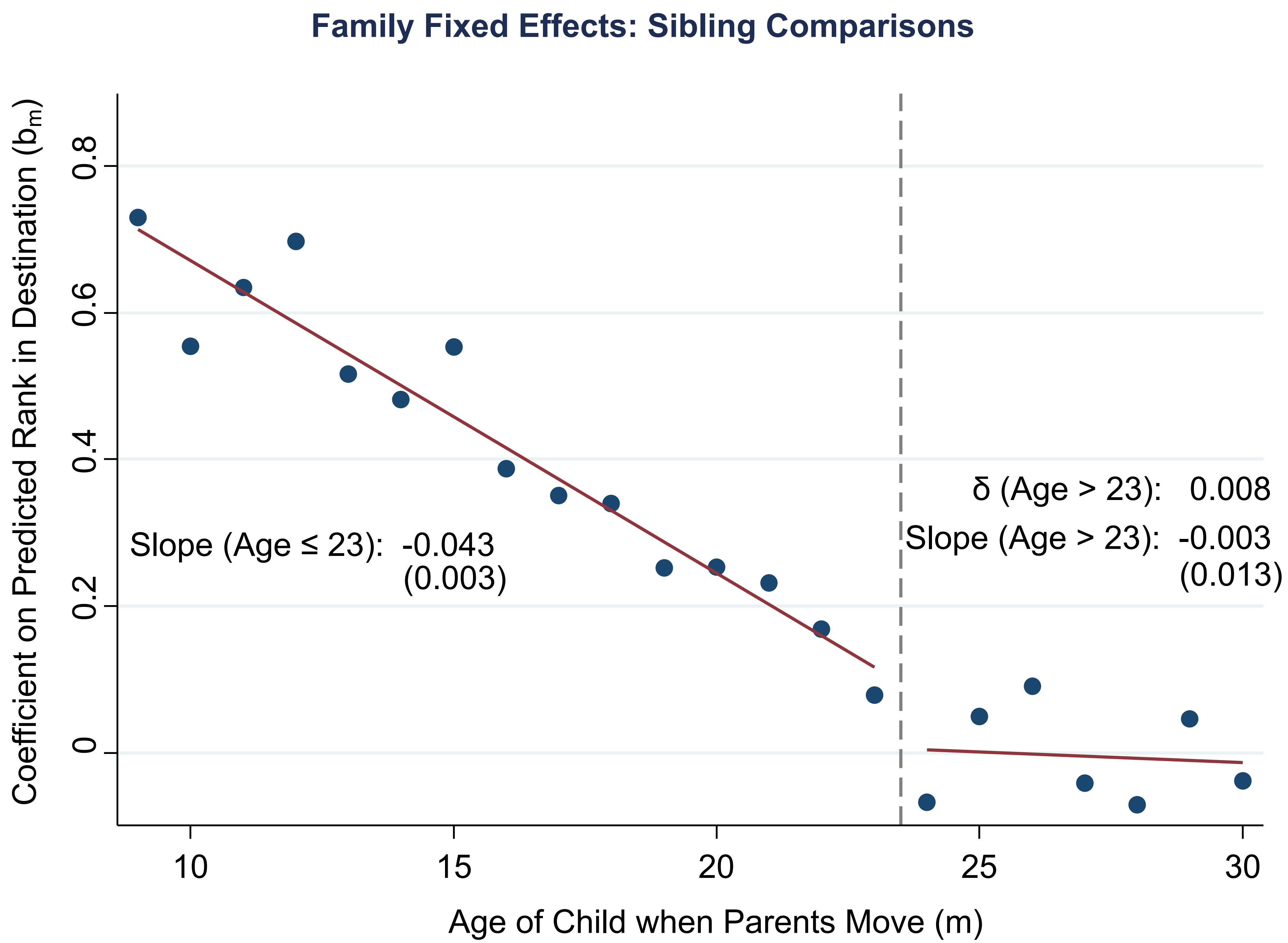
- Primary contribution of the paper is to provide evidence in support of this identification condition in observational data

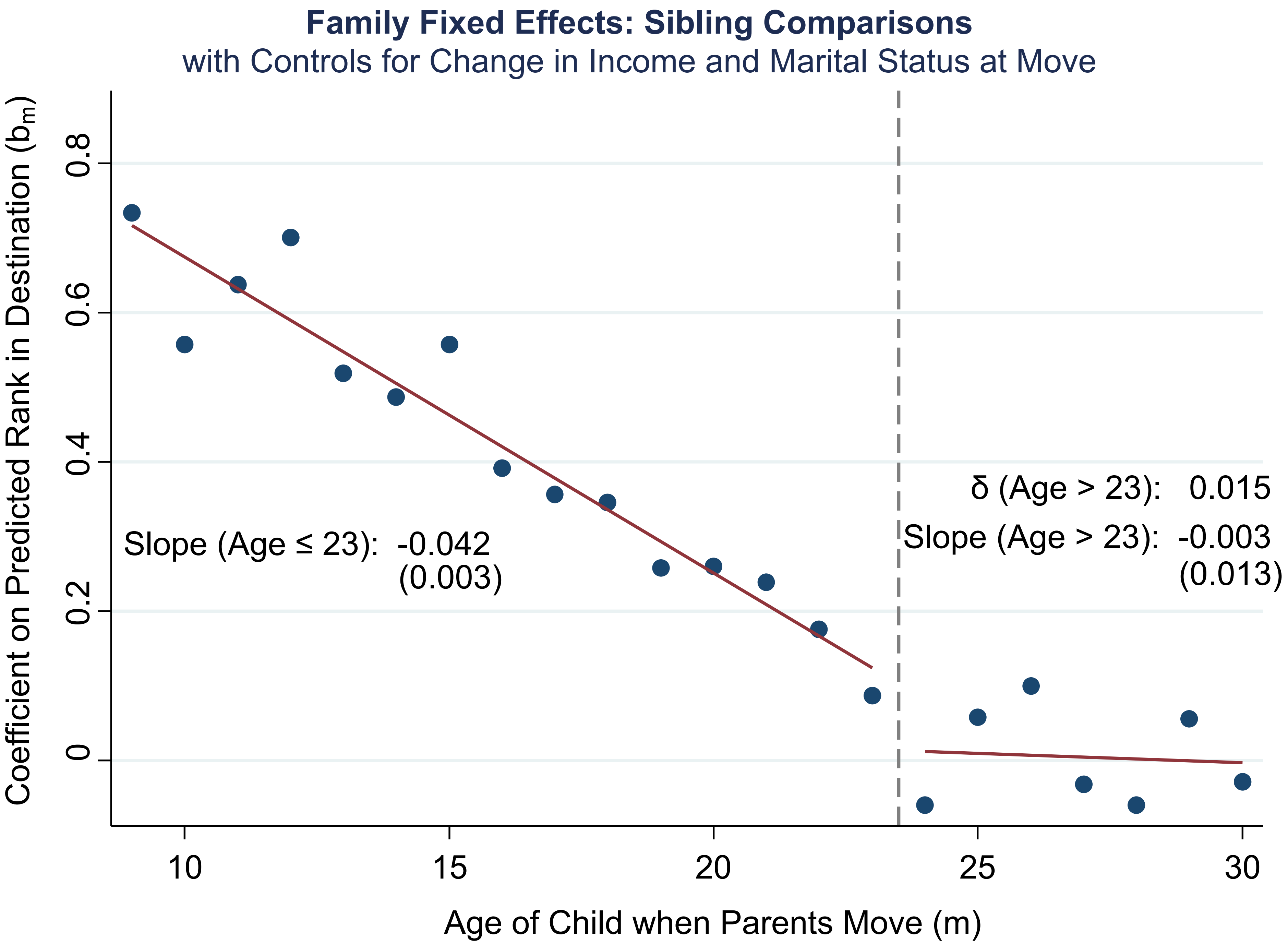
  - Without existence of an "instrument"

- 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
Coefficient on Predicted Rank in Destination (b_m)
0.8
0.6
0.4
0.2
0
10
15
20
25
30
Age of Child when Parents Move (m)
Slope (Age ≤ 23): -0.043
(0.003)
δ (Age > 23): 0.008
Slope (Age > 23): -0.003
(0.013)


Family Fixed Effects: Sibling Comparisons
with Controls for Change in Income and Marital Status at Move
Coefficient on Predicted Rank in Destination (b_m)
0
0.2
0.4
0.6
0.8
10
15
20
25
30
Age of Child when Parents Move (m)
Slope (Age ≤ 23): -0.042
(0.003)
δ (Age > 23): 0.015
Slope (Age > 23): -0.003
(0.013)

Time-Varying Unobservables

- 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]

Distinguishing Neighborhood Effects from Other Shocks

- Prior observational studies of movers define “good” neighborhoods based on observable characteristics (e.g., low poverty rates)

- Chetty and Hendren (2018) approach differs by measuring nbhd. quality based on **outcomes** of permanent residents, analogous to value-added models

  - Generates sharp predictions that allow us 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

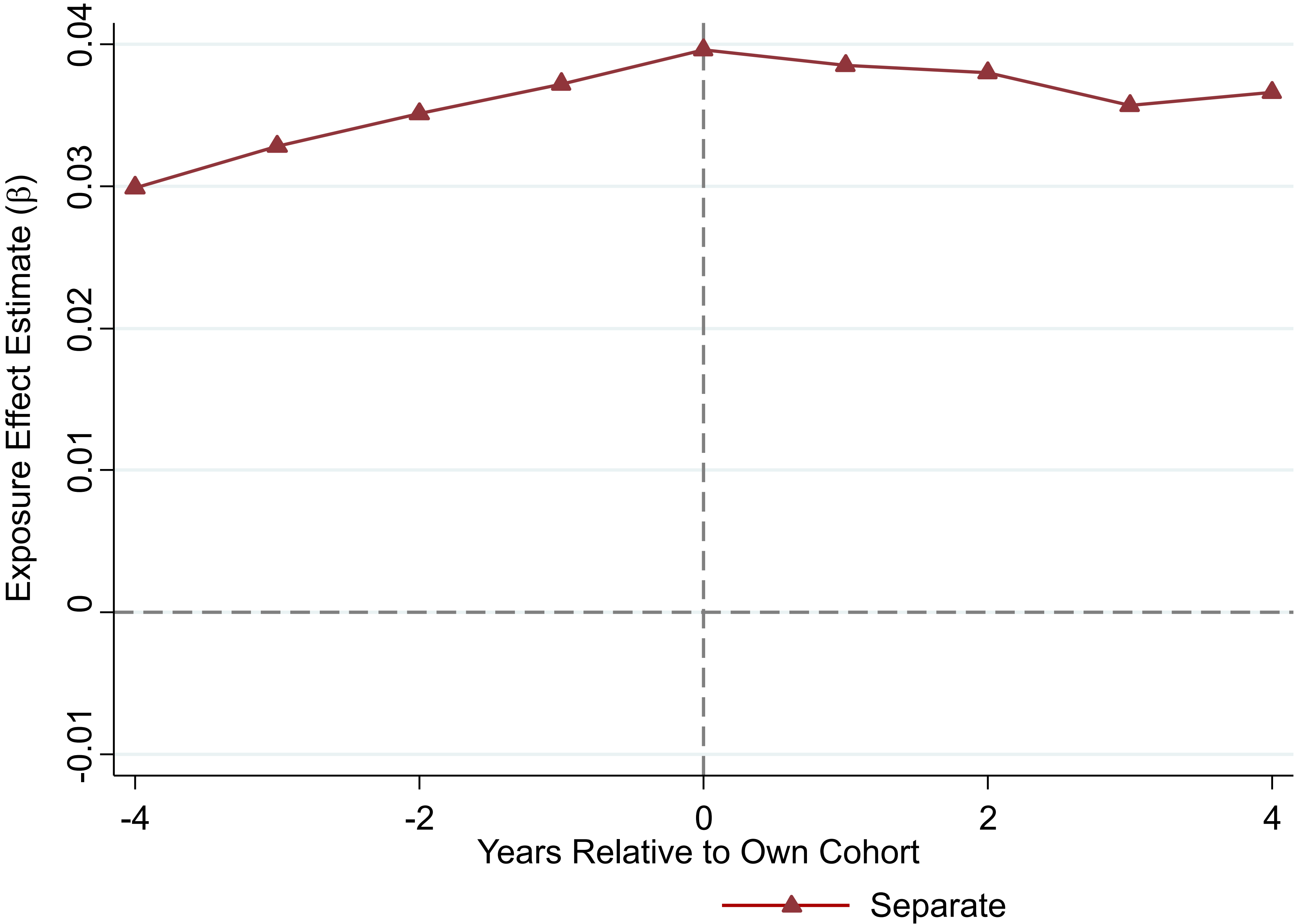
Outcome-Based Placebo Tests

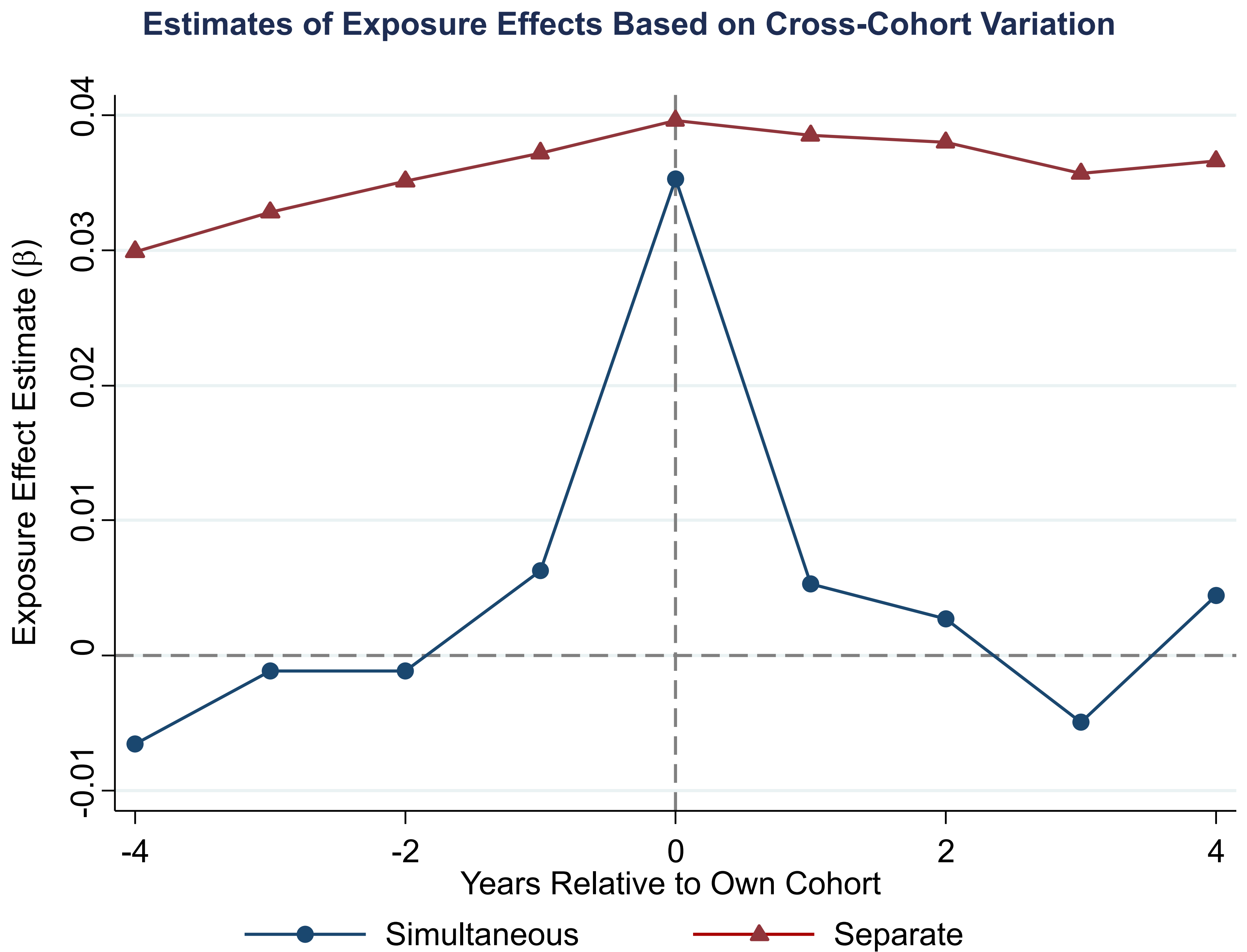
- General idea: exploit heterogeneity in place effects across subgroups to obtain overidentification tests of exposure effect model

- 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 θ_i replicates cohort variation perfectly

Estimates of Exposure Effects Based on Cross-Cohort Variation





Estimates of Exposure Effects Based on Cross-Cohort Variation
Exposure Effect Estimate (β)
0.04
0.03
0.02
0.01
0
-0.01
-4
-2
0
2
4
Years Relative to Own Cohort
Simultaneous
Separate

Distributional Convergence

- 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 25th pctile., 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 θ_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

	Dependent Variable					
	Child Rank in top 10%			Child Employed		
	(1)	(2)	(3)	(4)	(5)	(6)
Distributional Prediction	0.043		0.040	0.046		0.045
	(0.002)		(0.003)	(0.003)		(0.004)
Mean Rank Prediction (Placebo)		0.022	0.004		0.021	0.000
		(0.002)	(0.003)		(0.002)	(0.003)

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 θ_i does not covary with gender-specific neighborhood effect, can use gender differences to conduct a placebo test

Exposure Effect Estimates: Gender-Specific Predictions

	No Family Fixed Effects			Family Fixed Effects
	(1)	(2)	(3)	(4)
Own Gender Prediction	0.038		0.031	**0.031**
	(0.002)		(0.003)	(0.007)
Other Gender Prediction (Placebo)		0.034	0.009	**0.012**
		(0.002)	(0.003)	(0.007)
Sample	Full Sample			2-Gender HH

Identification of Exposure Effects: Summary

- Any omitted variable θ_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

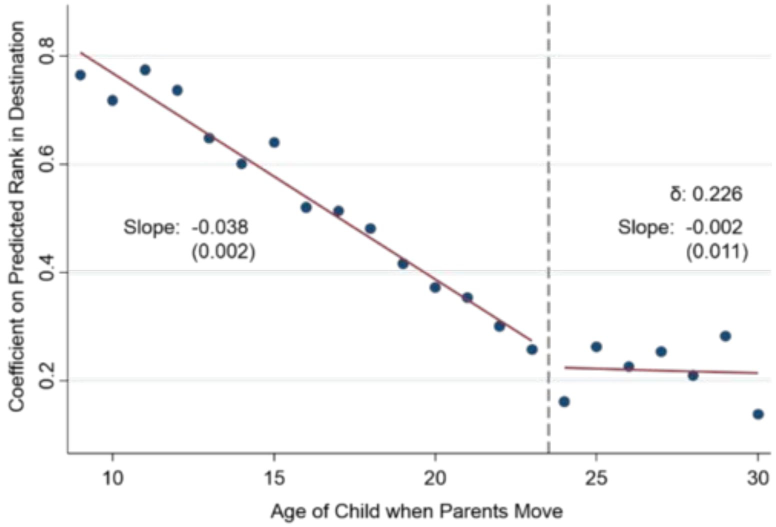
  3. Replicate prior residents' outcomes by birth cohort, quantile, and gender in proportion to exposure time *conditional* on other predictions

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

- Unlikely?

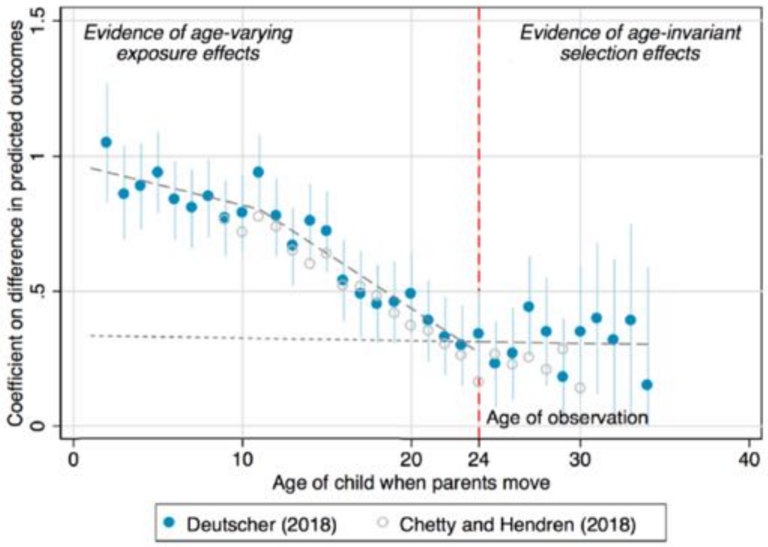
Childhood Exposure Effects: Other Evidence

United States



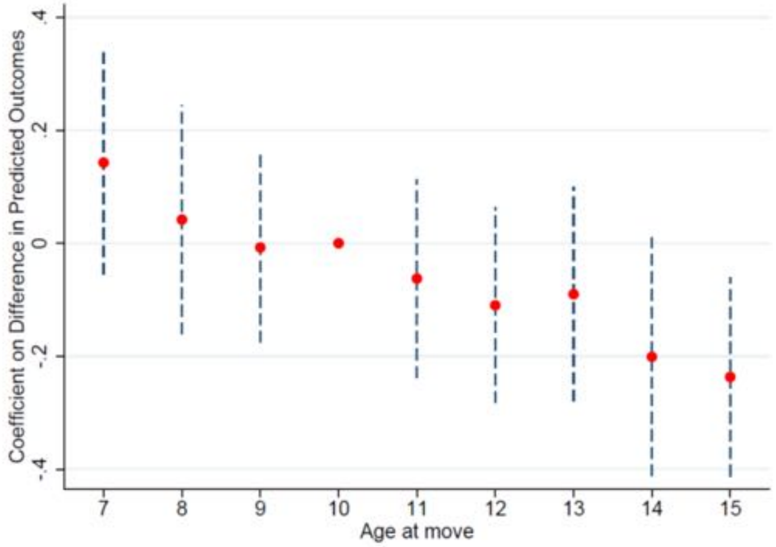
Source: Chetty and Hendren (QJE 2018)

Australia



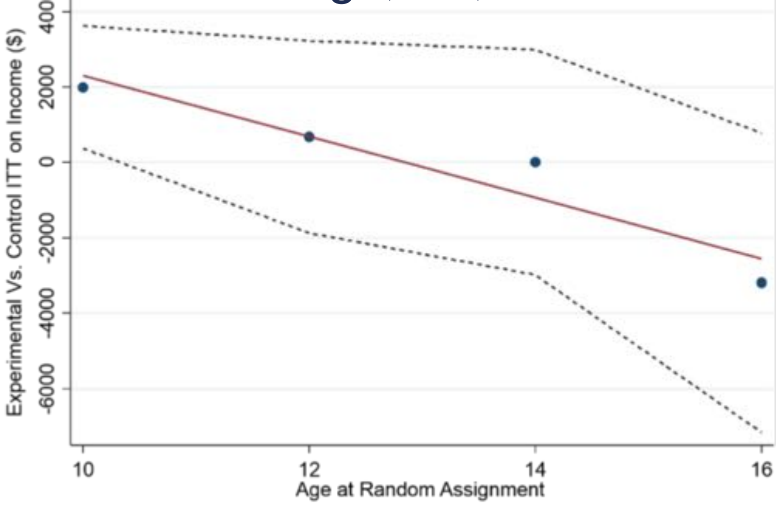
Source: Deutscher (2018)

Montreal, Canada



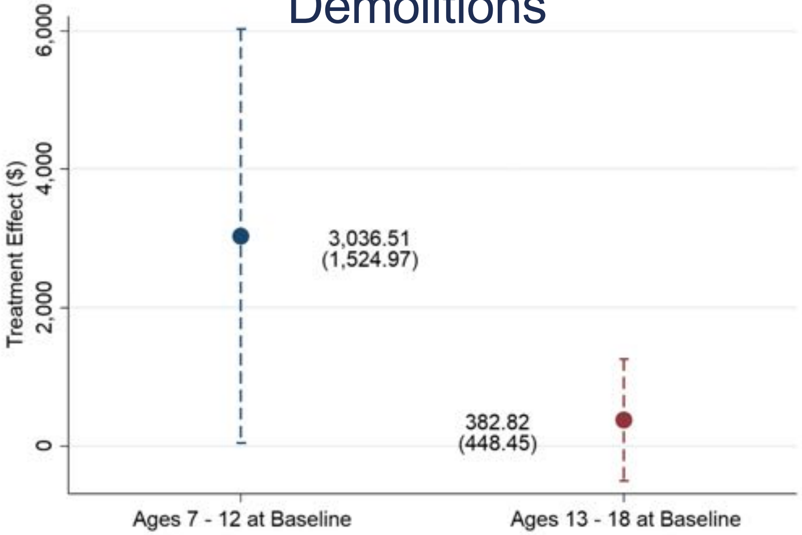
Source: Laliberté (2018)

MTO: Baltimore, Boston, Chicago, LA, NYC



Source: Chetty, Hendren, Katz (AER 2016)

Chicago Public Housing Demolitions



Source: Chyn (AER 2018)

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

Figure 1
The Harlem Children's Zone

Large Impacts on Children’s Test Scores

Table 3
Middle School Results

	Lottery RF	Lottery FS	Lottery 2SLS	Distance 2SLS
Math	0.284***	1.240***	0.229***	0.206**
	(0.050)	(0.075)	(0.037)	(0.092)
ELA	0.059	1.241***	0.047	–0.053
	(0.041)	(0.074)	(0.033)	(0.049)
Absences	–2.783***	1.260***	–2.199***	–0.220
	(0.833)	(0.079)	(0.650)	(2.544)
On Grade Level	–0.003	1.240***	–0.002	–0.011
	(0.022)	(0.075)	(0.017)	(0.036)
Observations	1449	1449	1449	41029

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

	In Zone	Out of Zone	
Math	0.201***	0.241***	0.468
	(0.051)	(0.042)	
ELA	0.067	0.039	0.577
	(0.045)	(0.037)	
Absences	−1.300	−2.601***	0.183
	(1.003)	(0.683)	
On Grade Level	0.013	−0.009	0.414
	(0.024)	(0.020)	
Observations	471	1038	

Place-Based Policy: Harlem Children's Zone

- 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

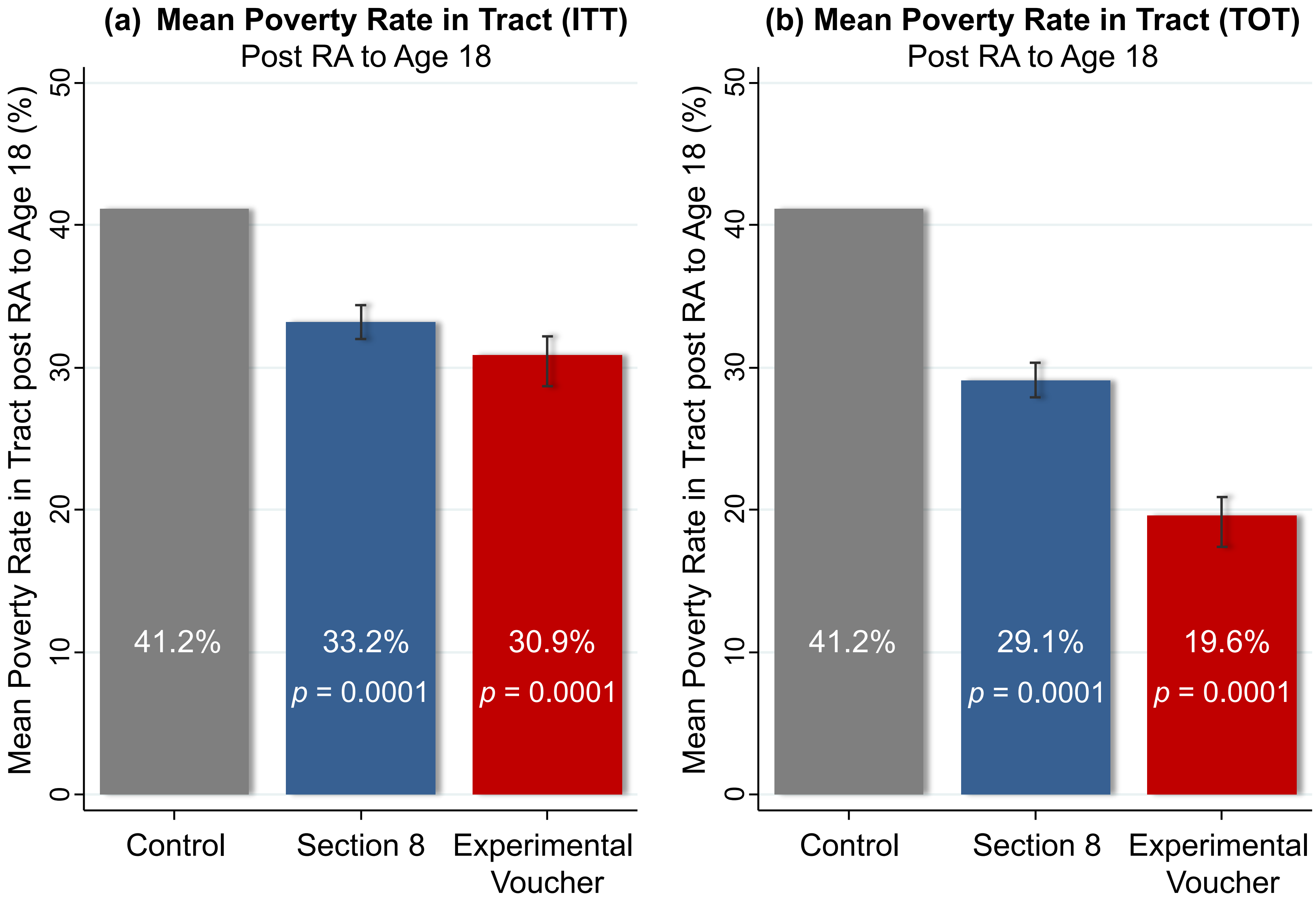
- HUD Moving to Opportunity Experiment implemented from 1994-1998
- 4,600 families at 5 sites: Baltimore, Boston, Chicago, LA, New York
- Families randomly assigned to one of three groups:
  1. Experimental: housing vouchers restricted to low-poverty (<10%) Census tracts
  2. Section 8: conventional 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

Treatment Effects on Neighborhood Poverty

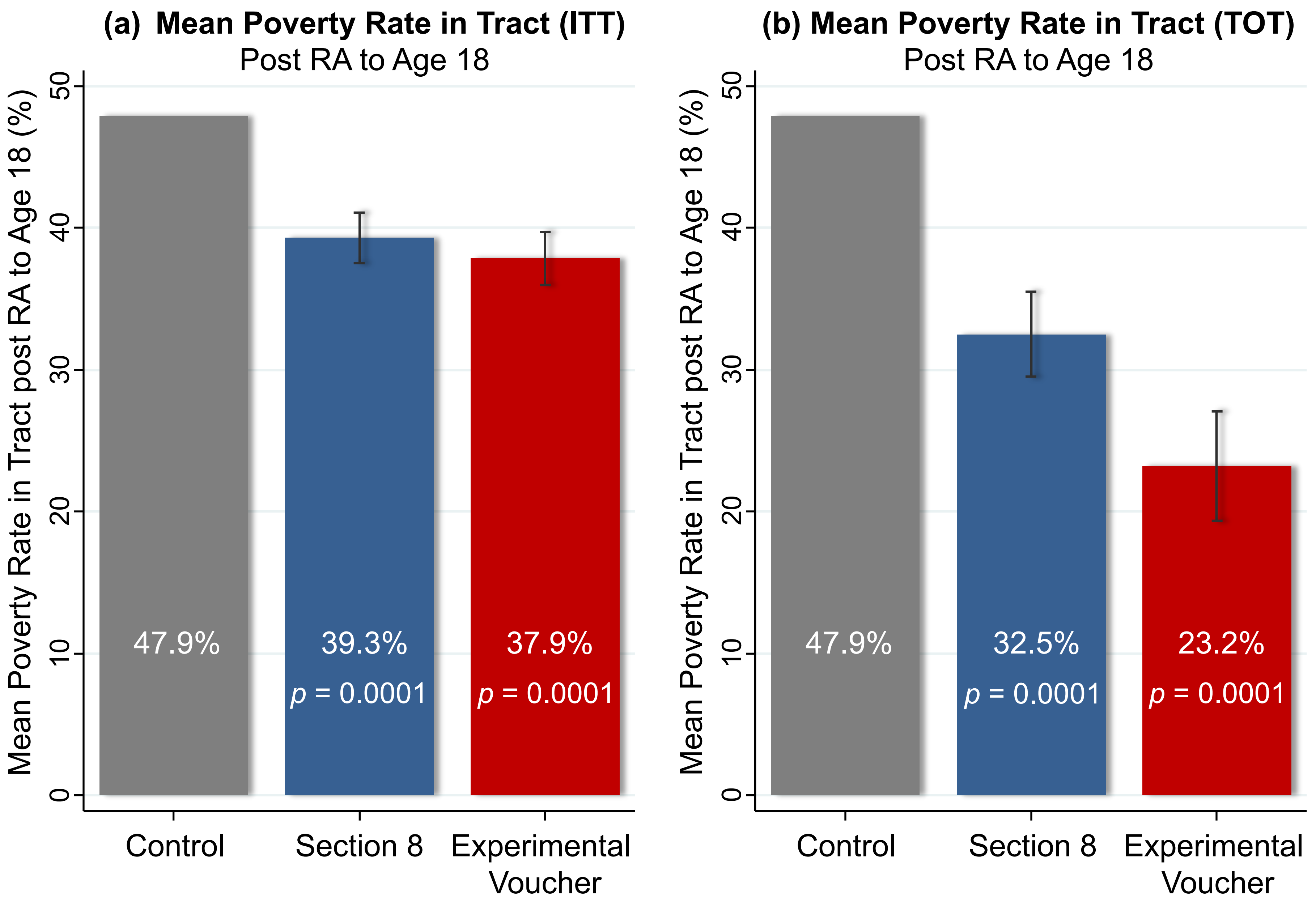
- Begin with "first stage" effects of MTO experiment on poverty rates
    - Measure mean poverty rates from random assignment to age 18 at tract level using Census data

- 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



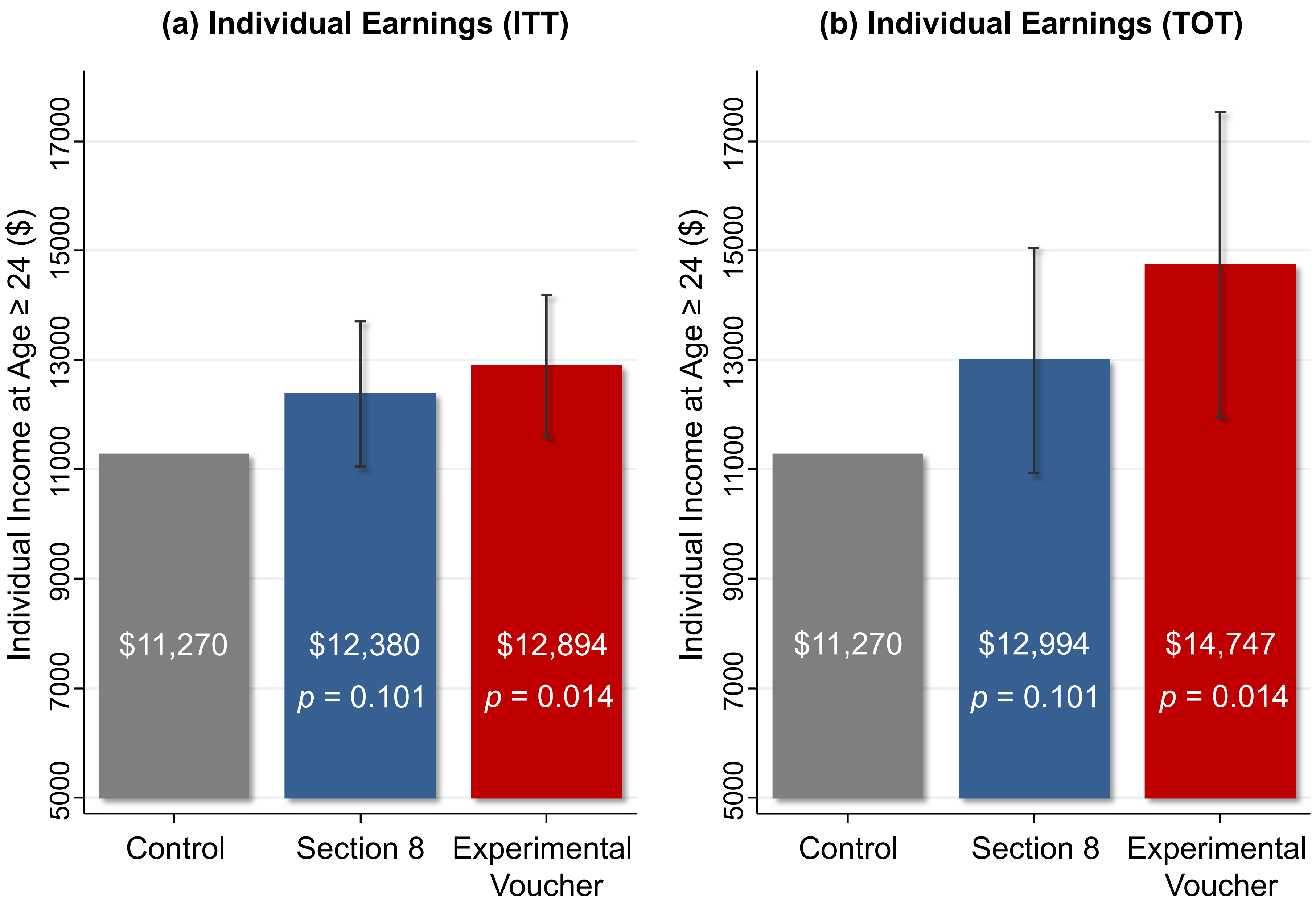
Impacts of MTO on Children Age 13-18 at Random Assignment



Treatment Effects on Outcomes in Adulthood

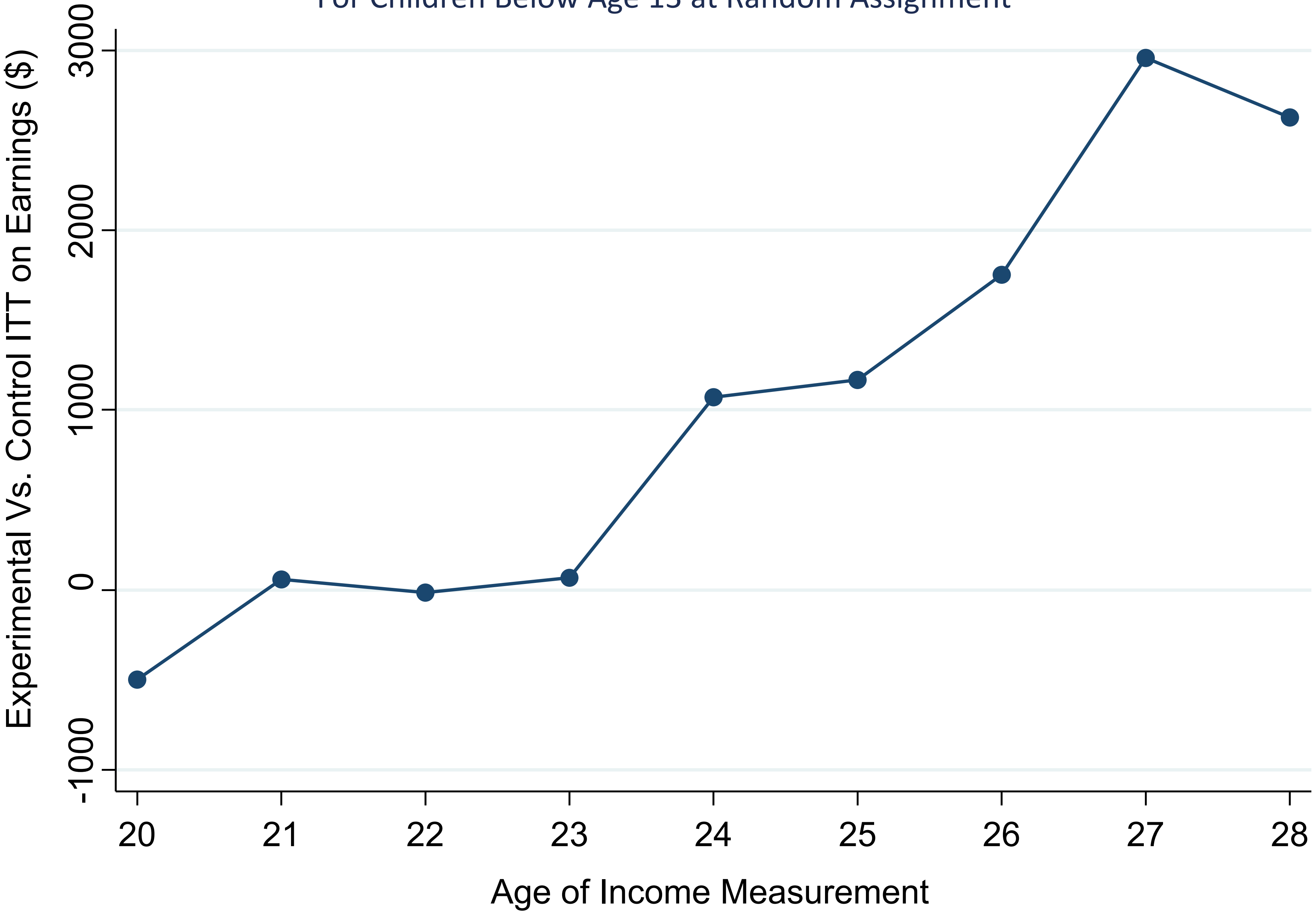
- Now turn to impacts on outcomes in adulthood
- Begin by analyzing effects on children below age 13 at RA
- Start with individual earnings (W-2 earnings + self-employment income)
  - Includes those who don't file tax returns through W-2 forms
- Measured from 2008-12, restricting to years in which child is 24 or older
  - Evaluate impacts at different ages after showing baseline results

Impacts of MTO on Children Below Age 13 at Random Assignment

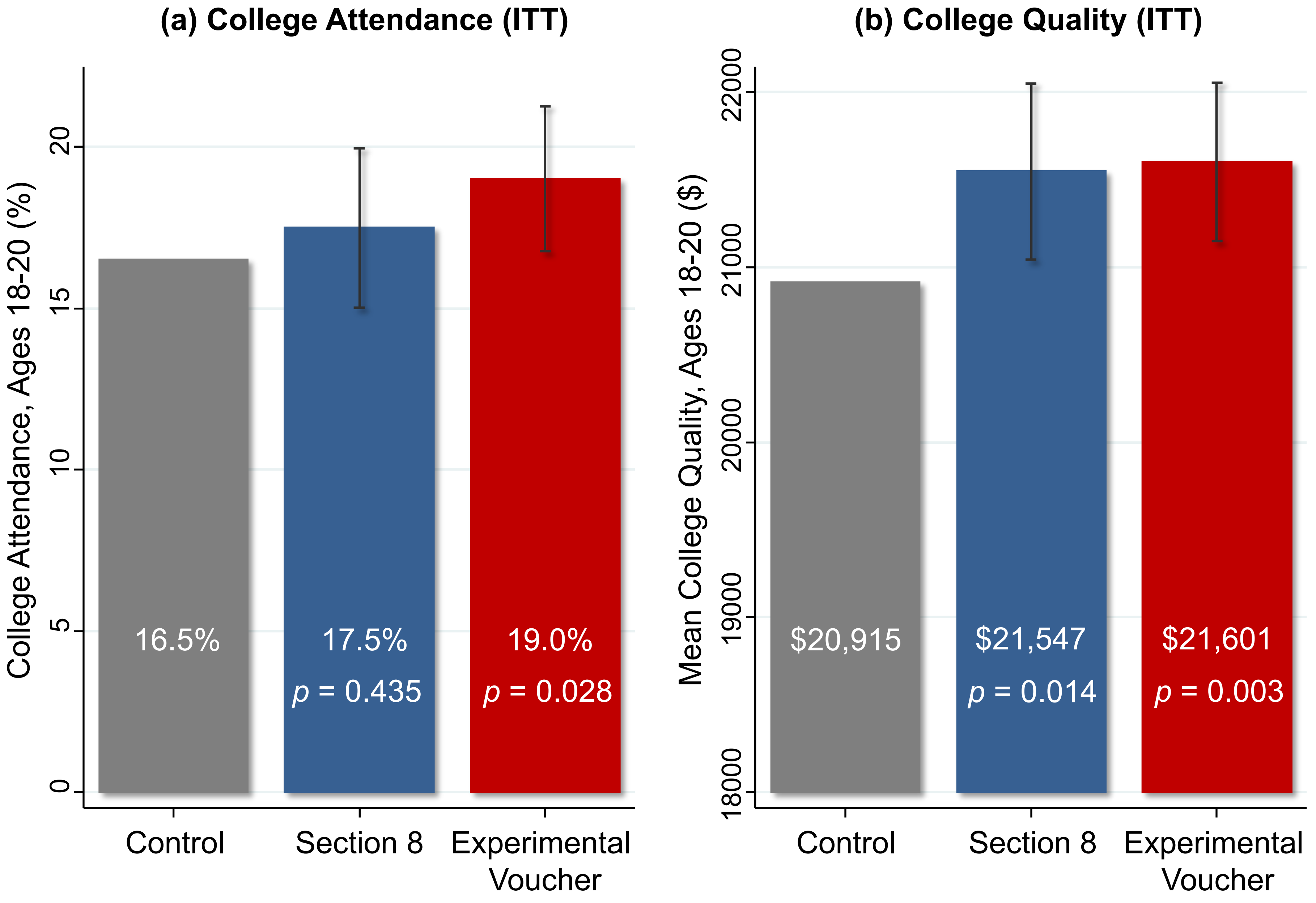


Impacts of Experimental Voucher by Age of Earnings Measurement

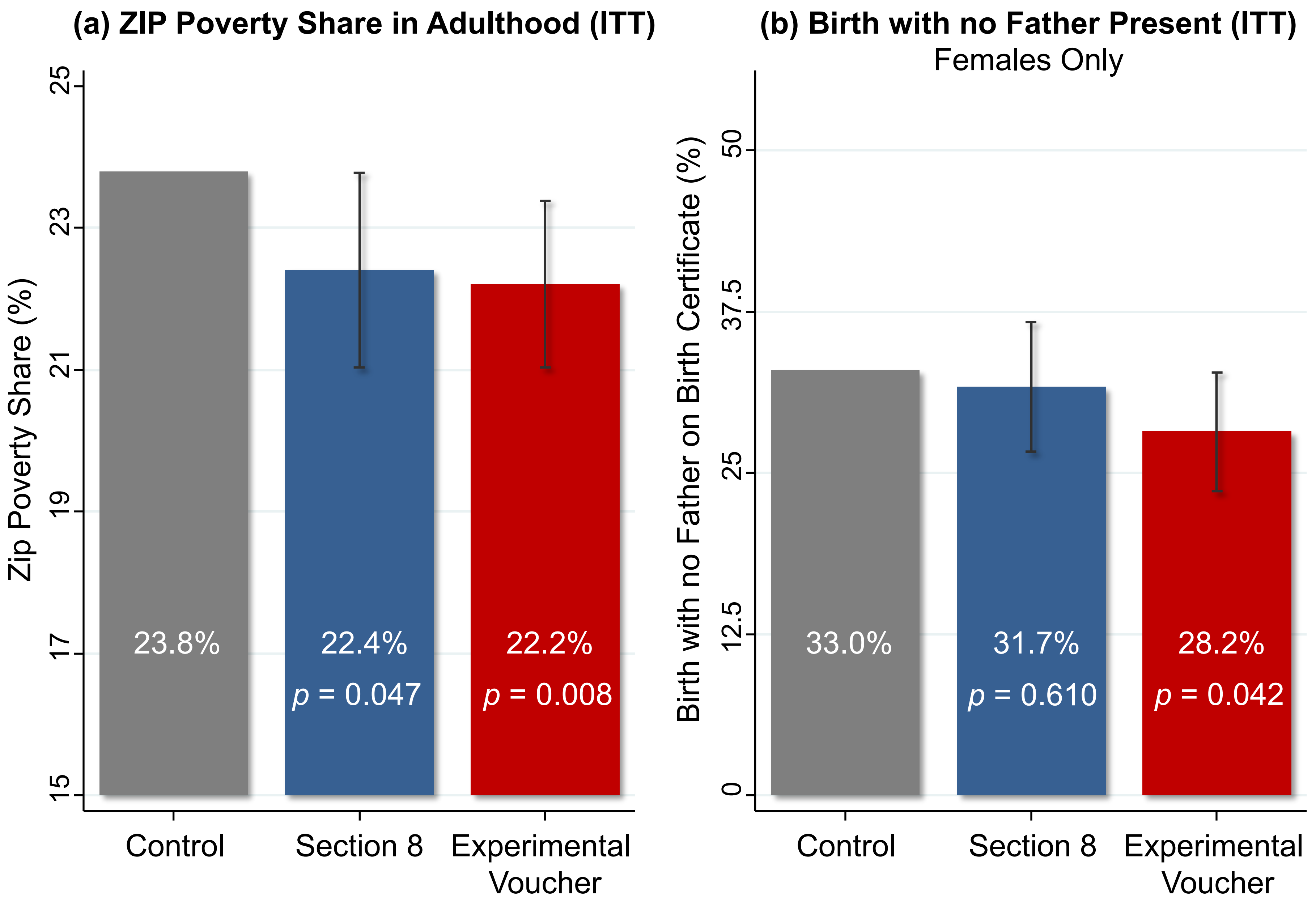
For Children Below Age 13 at Random Assignment



Impacts of MTO on Children Below Age 13 at Random Assignment



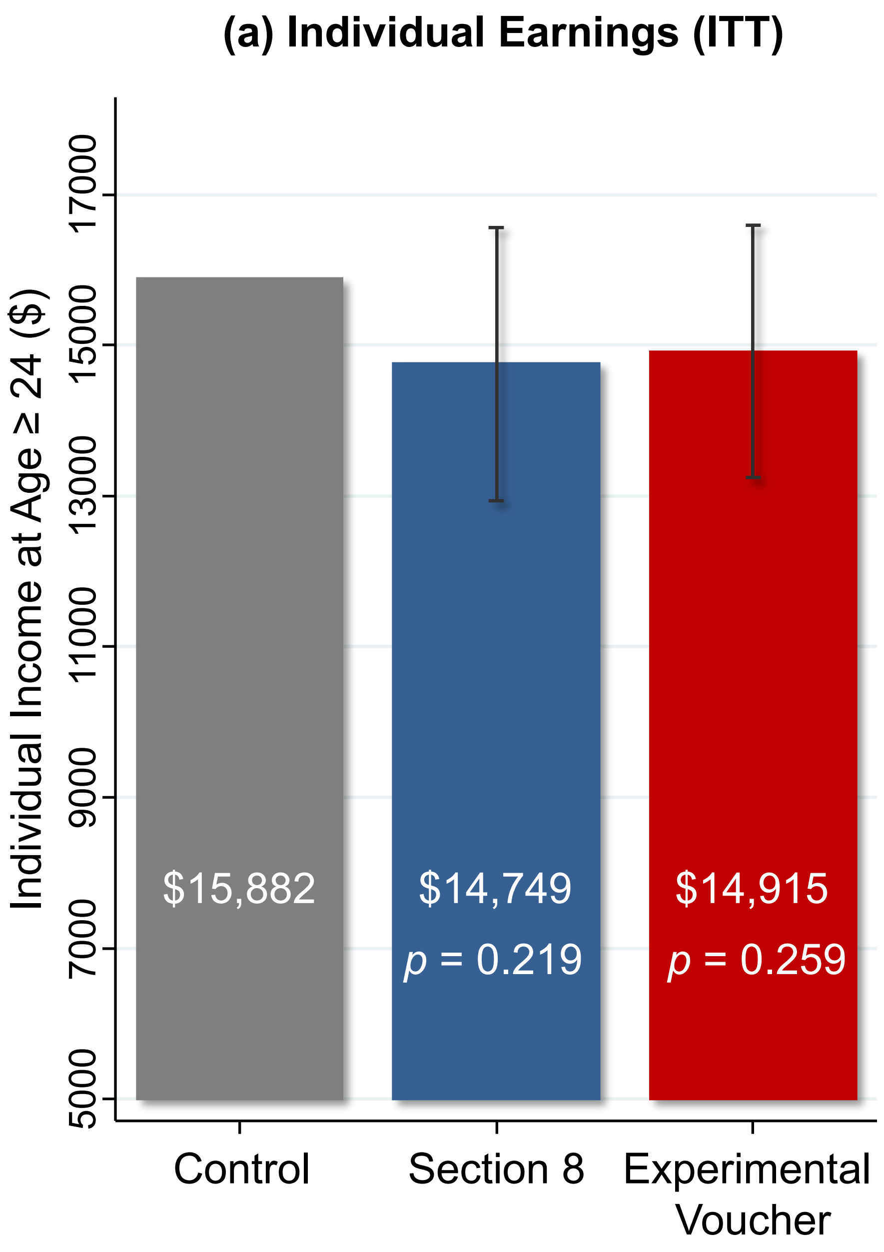
Impacts of MTO on Children Below Age 13 at Random Assignment

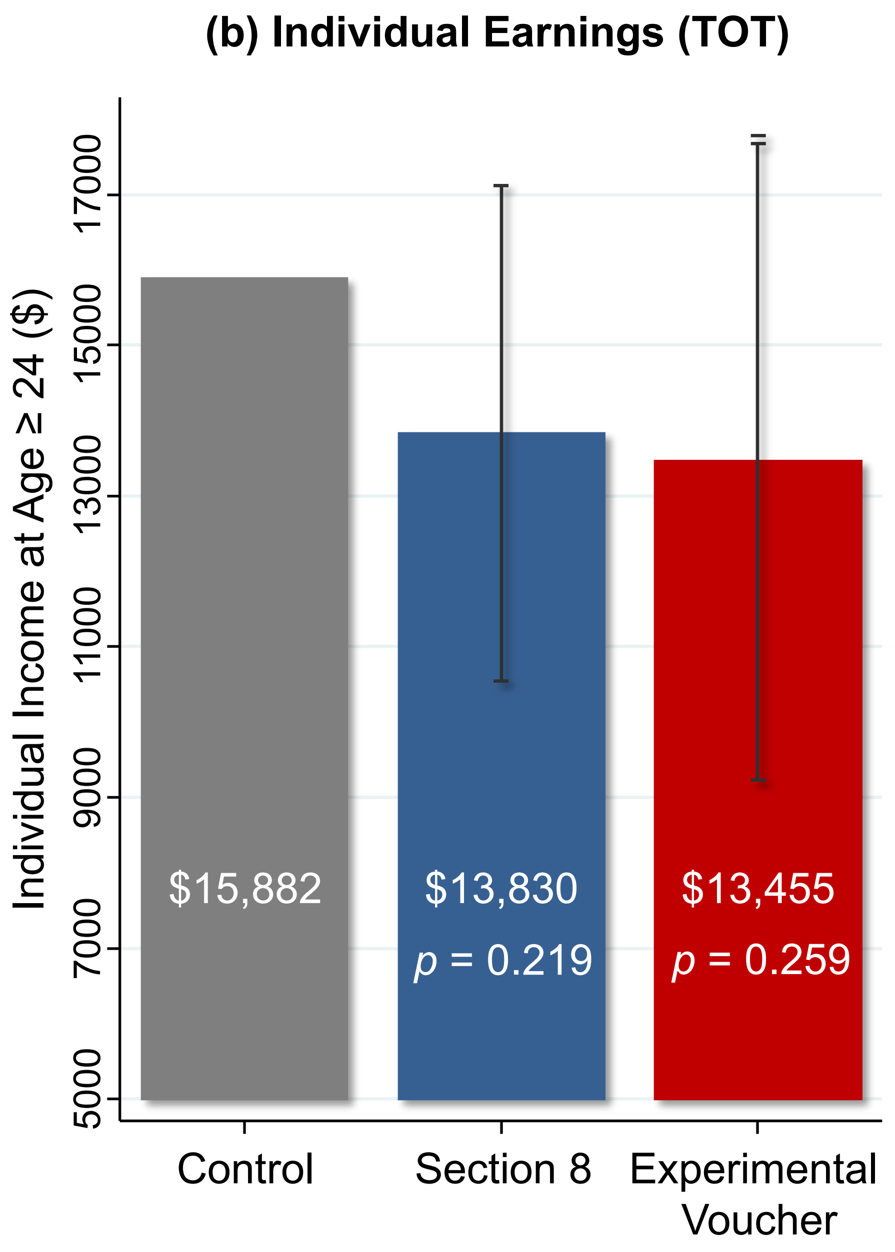


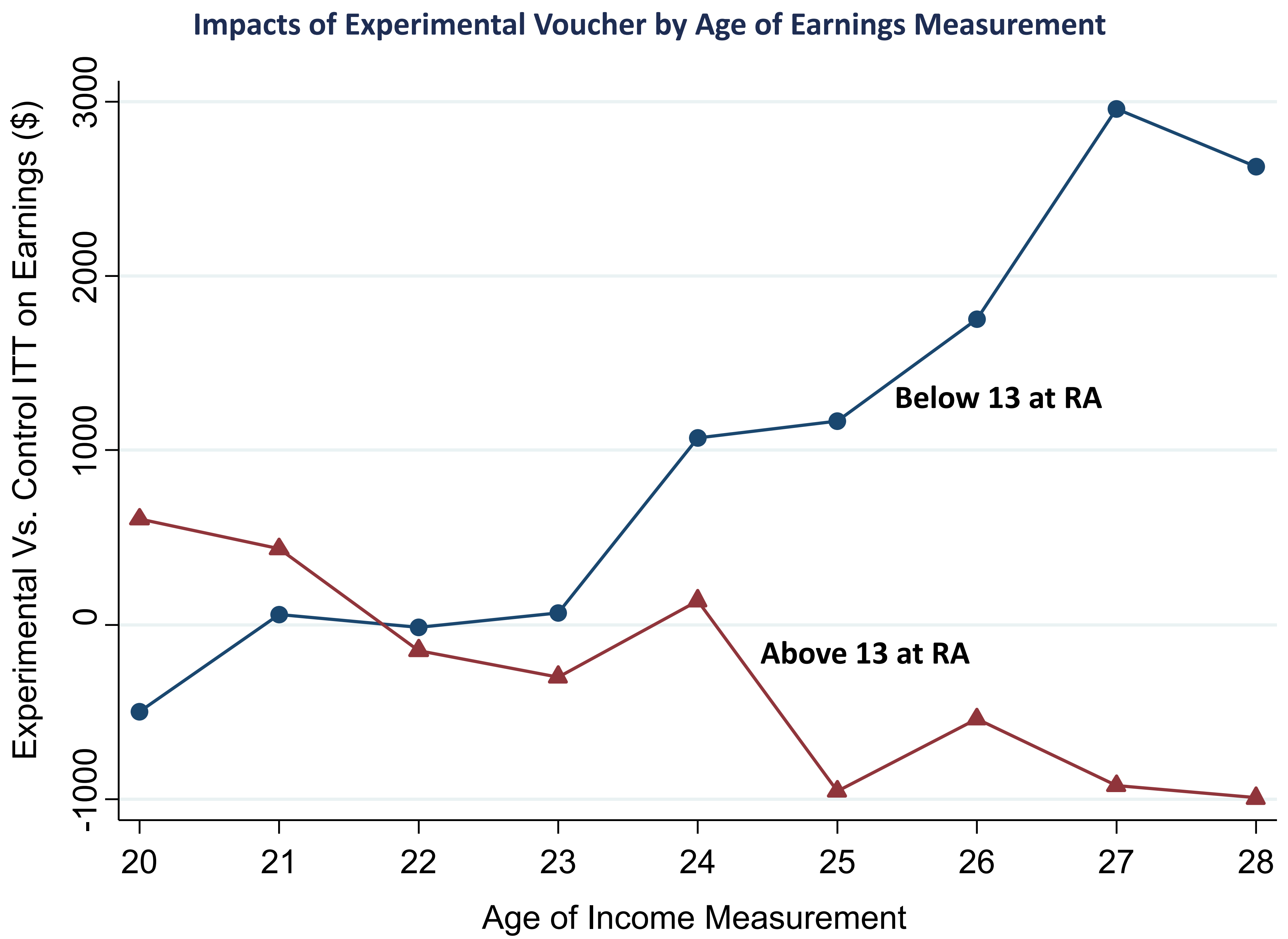
Treatment Effects on Older Children

- Next, turn to children who were ages 13-18 at random assignment
  - Replicate same analysis as above

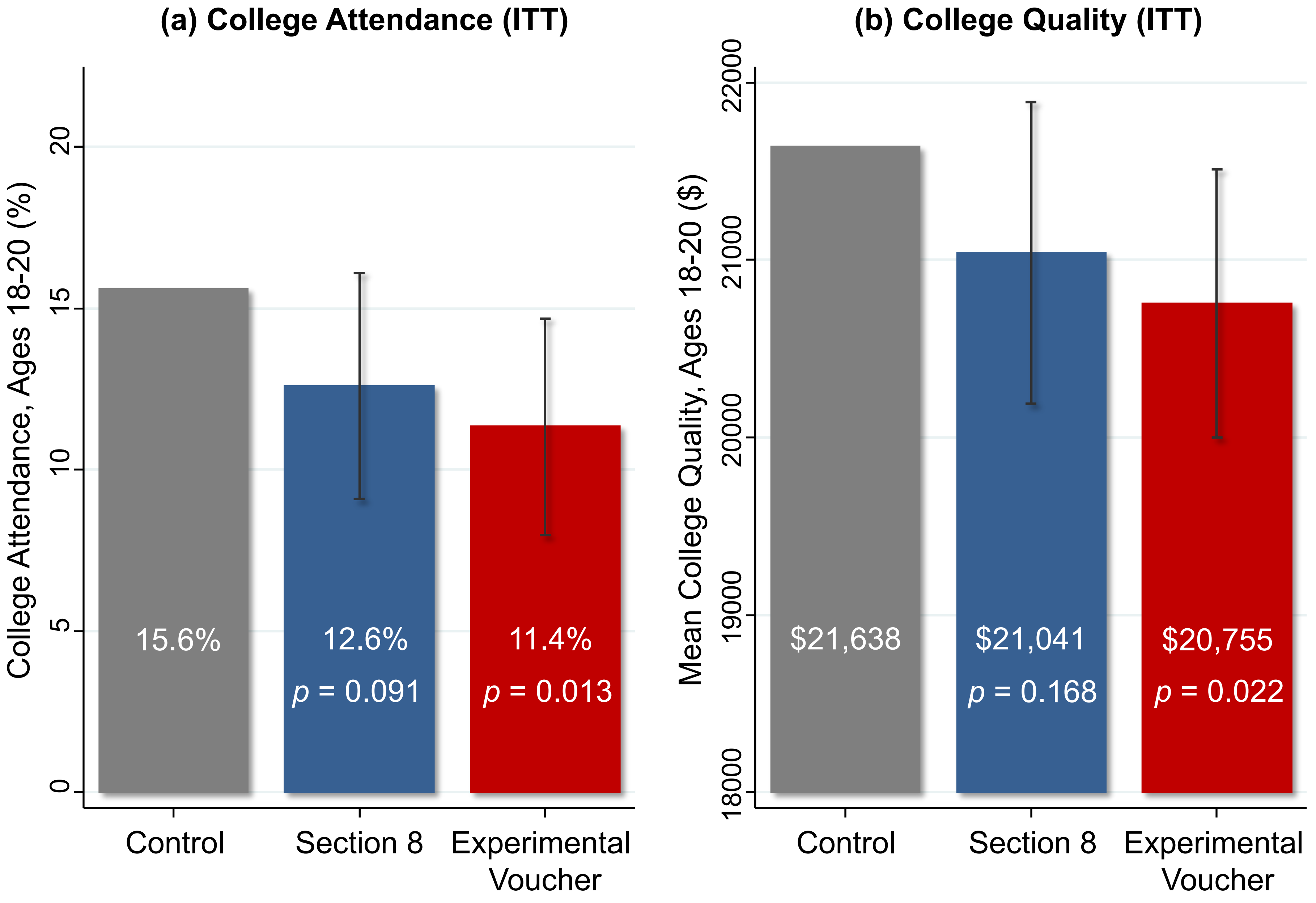
Impacts of MTO on Children Age 13-18 at Random Assignment



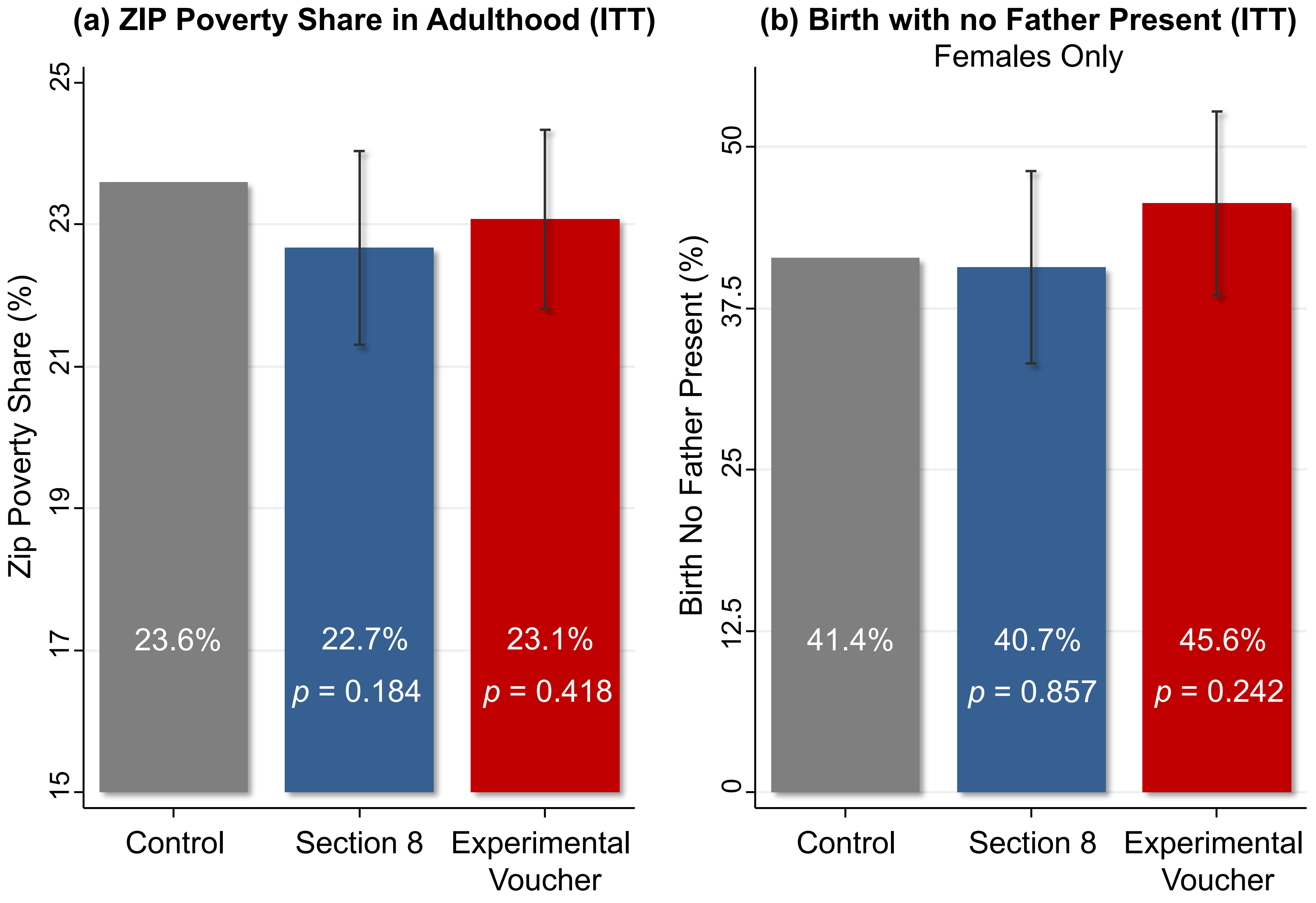



Impacts of Experimental Voucher by Age of Earnings Measurement
Experimental Vs. Control ITT on Earnings ($)
-1000
0
1000
2000
3000
20
21
22
23
24
25
26
27
28
Age of Income Measurement
Below 13 at RA
Above 13 at RA

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

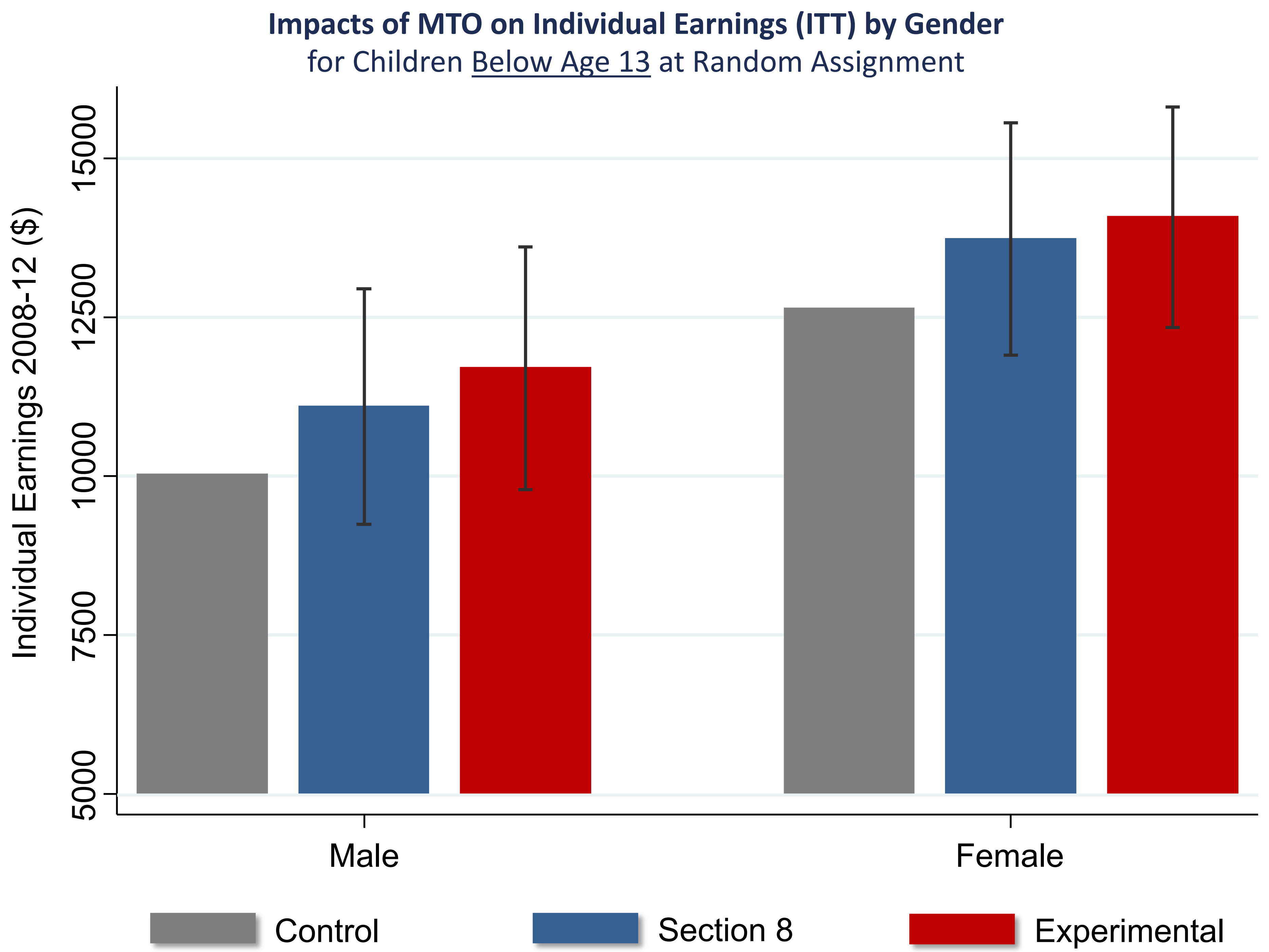


Impacts of MTO on Children Age 13-18 at Random Assignment



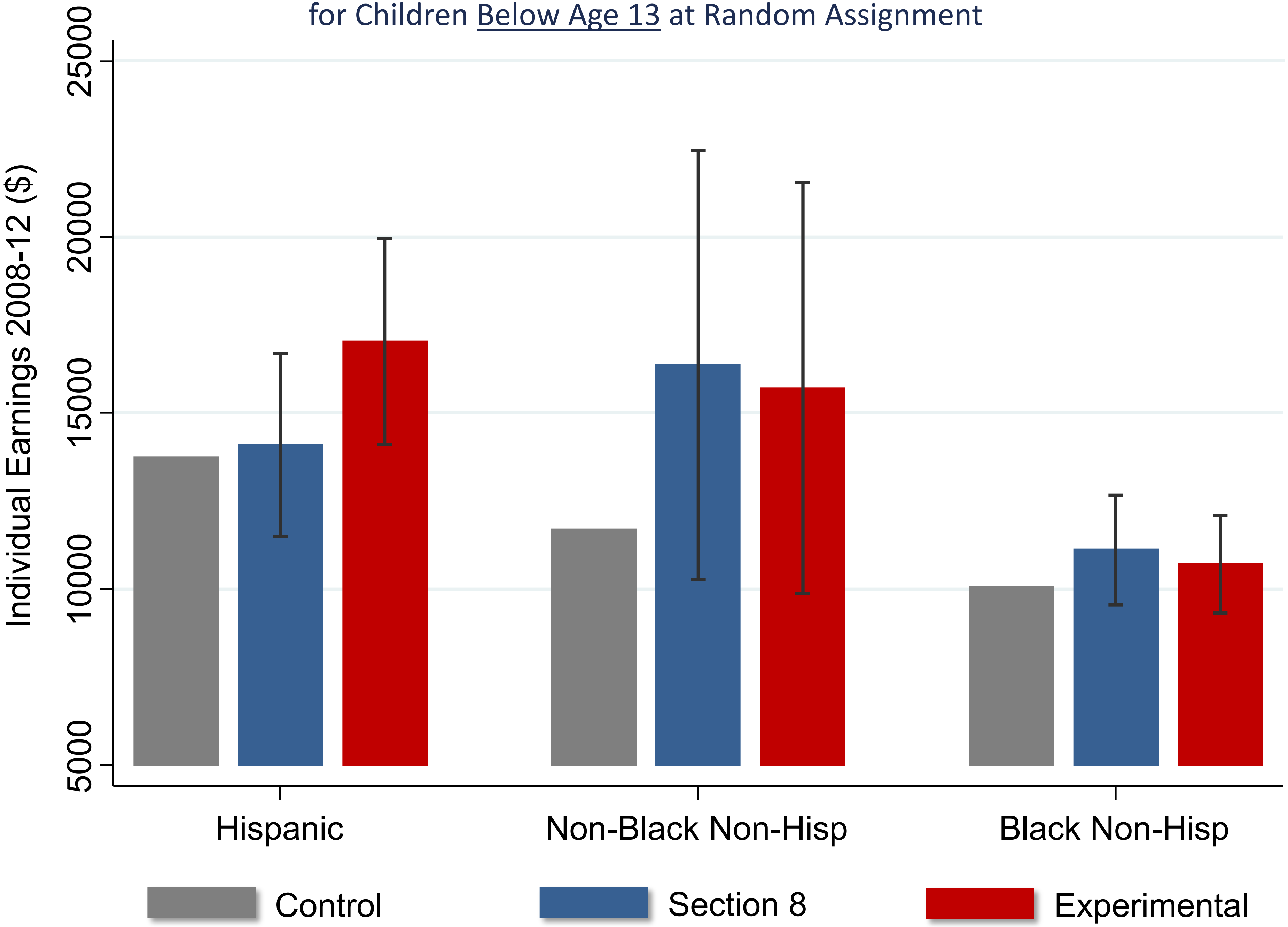
Heterogeneity

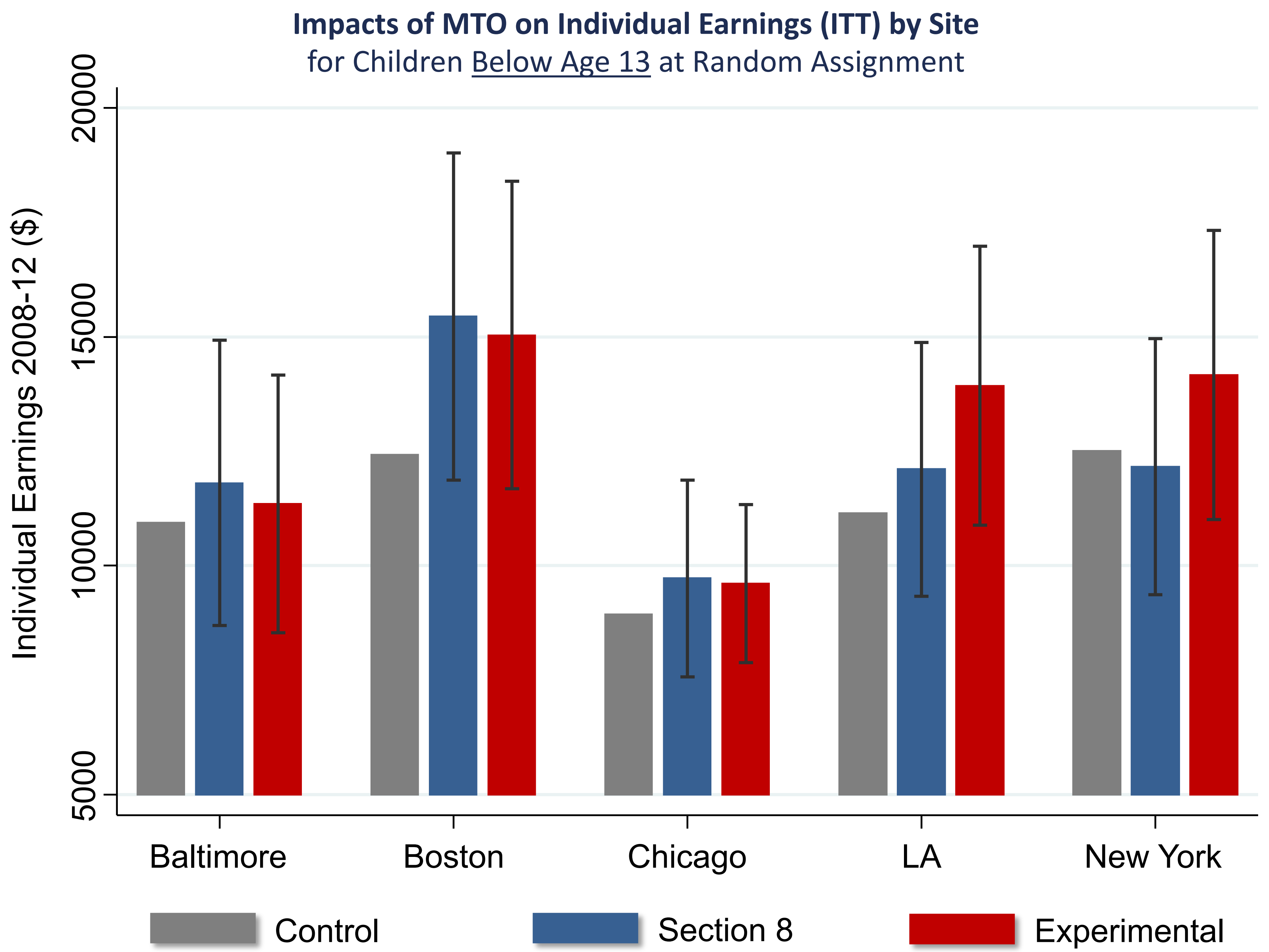
- Prior work has analyzed variation in treatment effects across sites, racial groups, and gender
- Replicate analysis across these groups for children below age 13 at RA


Impacts of MTO on Individual Earnings (ITT) by Gender
for Children Below Age 13 at Random Assignment
Individual Earnings 2008-12 ($)
15000
12500
10000
7500
5000
Male
Female
Control
Section 8
Experimental

Impacts of MTO on Individual Earnings (ITT) by Race

for Children Below Age 13 at Random Assignment




Impacts of MTO on Individual Earnings (ITT) by Site
for Children Below Age 13 at Random Assignment
Individual Earnings 2008-12 ($)
20000
15000
10000
5000
Baltimore
Boston
Chicago
LA
New York
Control
Section 8
Experimental

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

Dep. Var.:	Indiv. Earnings 2008-12 ($) (1)	Hhold. Inc. 2008-12 ($) (2)	College Attendance 18-20 (%) (3)	College Quality 18-20 ($) (4)	Married (%) (5)	Poverty Share in ZIP 2008-12 (%) (6)
Panel A: p-values for Comparisons by Age Group						
Exp. vs. Control	0.0203	0.0034	0.0035	0.0006	0.0814	0.0265
Sec. 8 vs. Control	0.0864	0.0700	0.1517	0.0115	0.0197	0.0742
Exp & Sec. 8 vs. Control	0.0646	0.0161	0.0218	0.0020	0.0434	0.0627
Panel B: p-values for Comparisons by Age, Site, Gender, and Race Groups						
Exp. vs. Control	0.1121	0.0086	0.0167	0.0210	0.2788	0.0170
Sec. 8 vs. Control	0.0718	0.1891	0.1995	0.0223	0.1329	0.0136
Exp & Sec. 8 vs. Control	0.1802	0.0446	0.0328	0.0202	0.1987	0.0016

Multiple Comparisons: Permutation Tests for Subgroup Heterogeneity

	Age		Race			Gender		Site					
p-value	< 13	>= 13	Black	Hisp	Other	M	F	Balt	Bos	Chi	LA	NYC	Min
Truth	**0.014**	0.258	0.698	0.529	0.923	0.750	0.244	0.212	0.720	0.287	0.491	0.691	0.014

Multiple Comparisons: How to Construct Permutation Tests for Subgroup Heterogeneity
EXAMPLE

	Age		Race			Gender		Site					
p-value	< 13	>= 13	Black	Hisp	Other	M	F	Balt	Bos	Chi	LA	NYC	Min
Truth	**0.014**	0.258	0.698	0.529	0.923	0.750	0.244	0.212	0.720	0.287	0.491	0.691	0.014
Placebos													
1	0.197	0.653	0.989	0.235	0.891	0.568	0.208	0.764	0.698	0.187	0.588	0.122	0.122
2	0.401	0.344	0.667	0.544	0.190	0.292	0.259	**0.005**	0.919	0.060	0.942	0.102	**0.005**
3	0.878	0.831	0.322	0.511	0.109	0.817	0.791	0.140	0.180	0.248	0.435	0.652	0.109
4	0.871	0.939	0.225	0.339	0.791	0.667	0.590	0.753	0.750	0.123	0.882	0.303	0.123
5	0.296	0.386	0.299	0.067	0.377	0.340	0.562	0.646	0.760	0.441	0.573	0.342	0.067
6	0.299	0.248	0.654	0.174	0.598	0.127	0.832	0.284	0.362	0.091	0.890	0.097	0.091
7	0.362	0.558	0.477	0.637	0.836	0.555	0.436	0.093	0.809	0.767	0.422	0.736	0.093
8	0.530	0.526	0.662	0.588	0.238	0.875	0.986	0.386	0.853	0.109	0.826	0.489	0.109
9	0.299	0.990	0.917	0.214	0.660	0.322	0.048	0.085	0.038	0.527	0.810	0.854	0.038
10	0.683	0.805	0.017	0.305	0.807	0.505	0.686	0.356	0.795	0.676	0.472	0.523	0.017

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

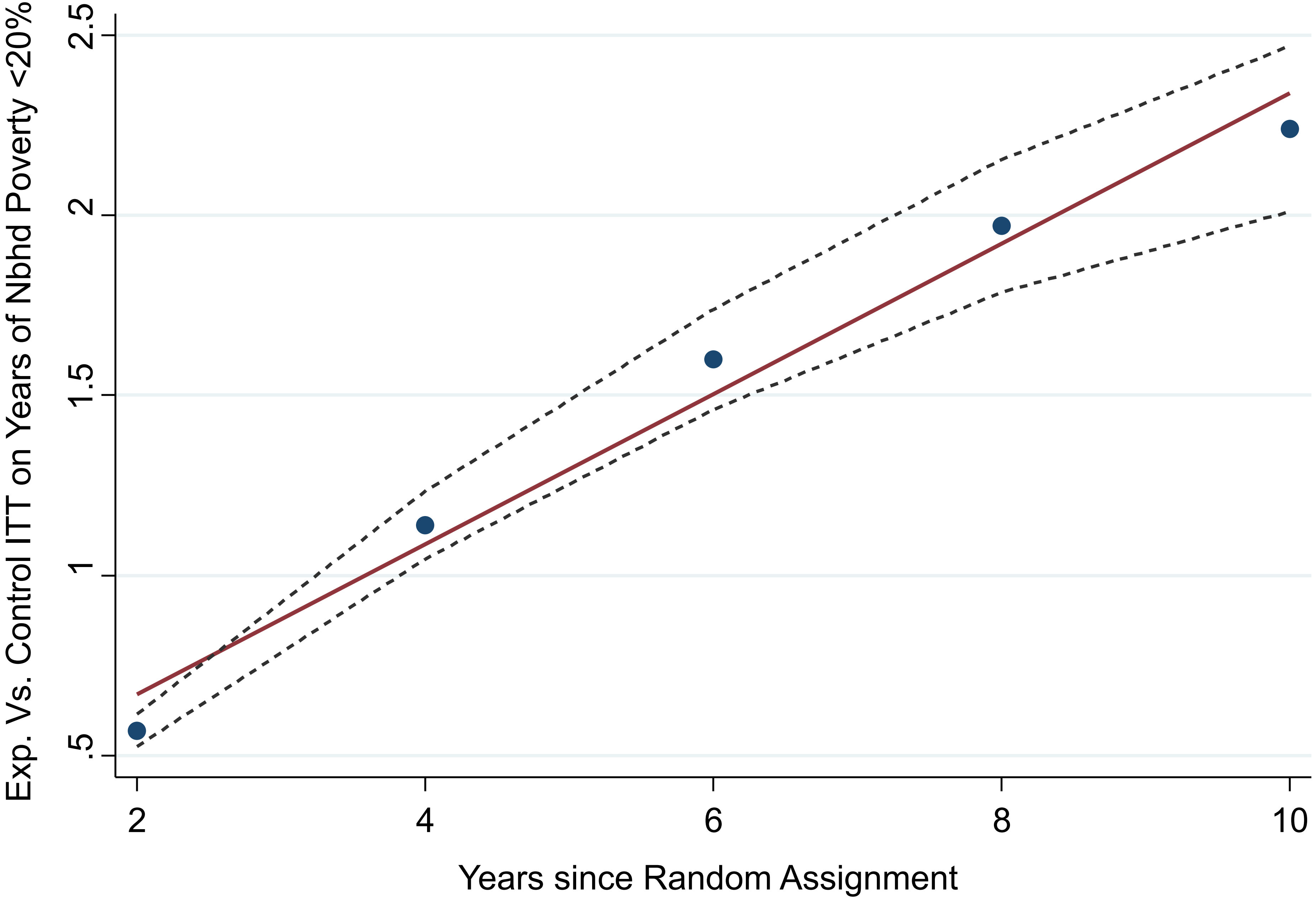
- Previous work finds no effects on adults' economic outcomes
  [Kling et al. 2007, Sanbonmatsu et al. 2011]

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

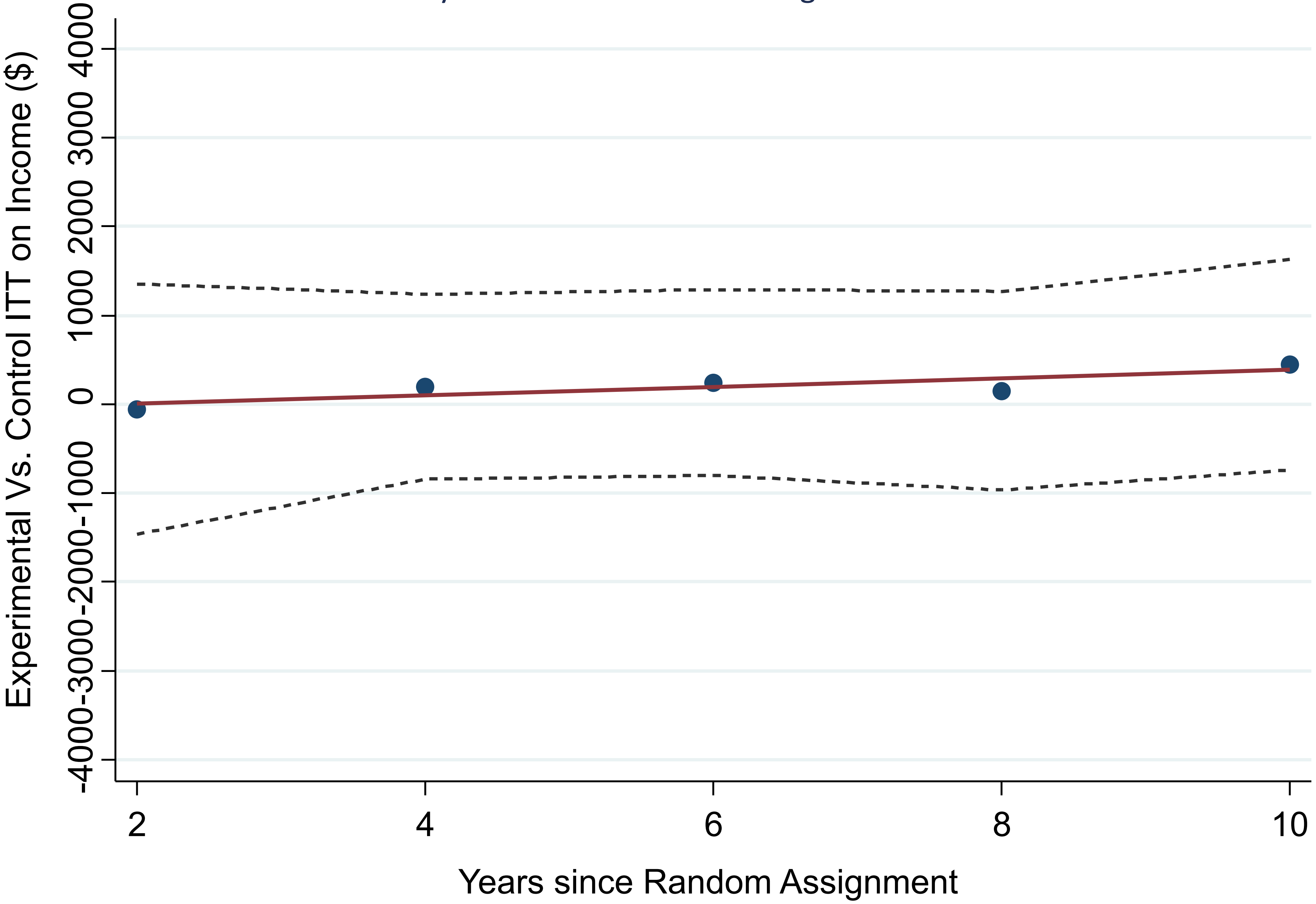
- Does exposure time matter for adults' outcomes as it does for children?
  [Clampet-Lundquist and Massey 2008]

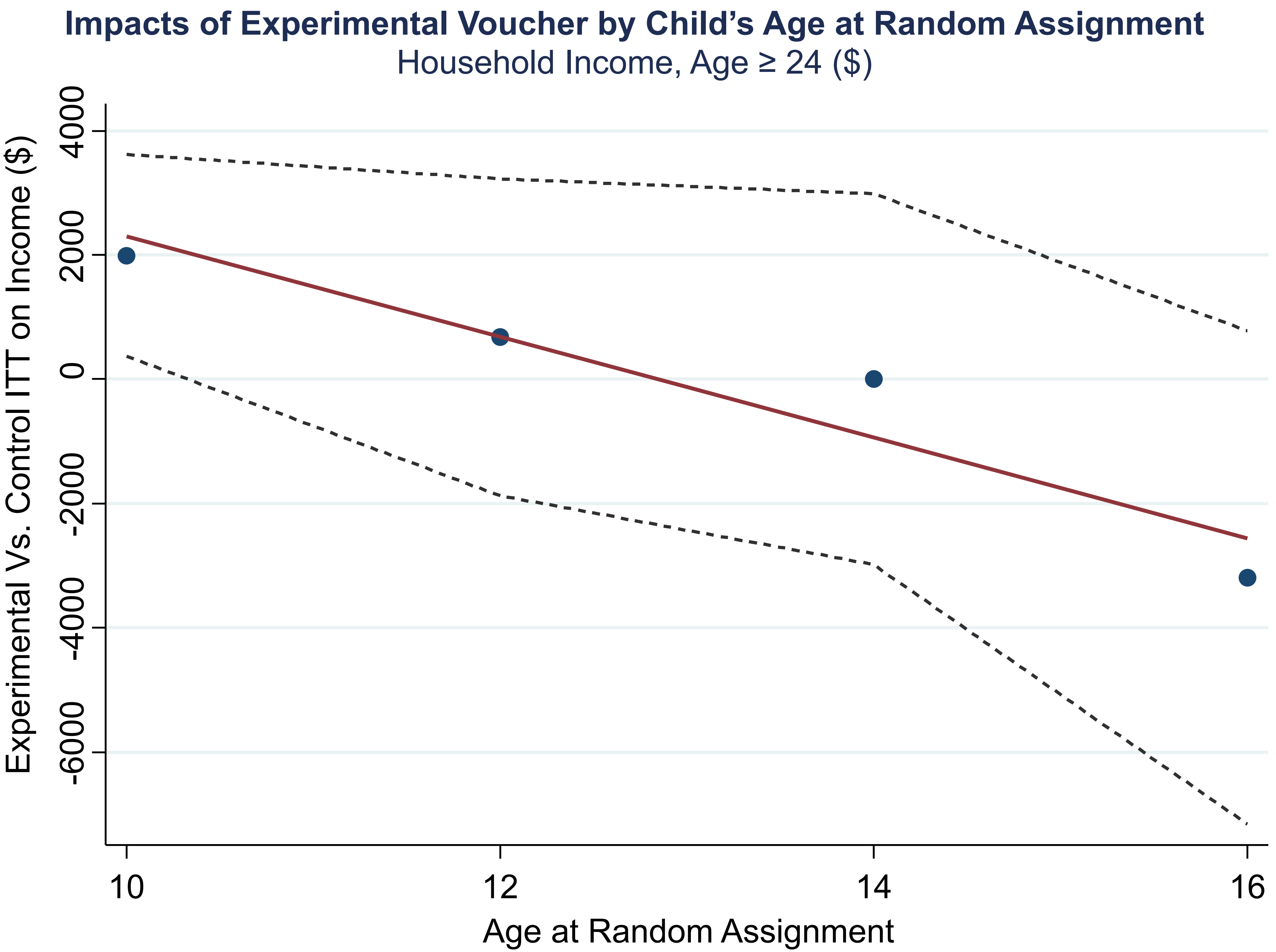
Impacts of Experimental Voucher on Adults Exposure to Low-Poverty Neighborhoods

by Years Since Random Assignment



Impacts of Experimental Voucher on Adults' Individual Earnings
by Years Since Random Assignment

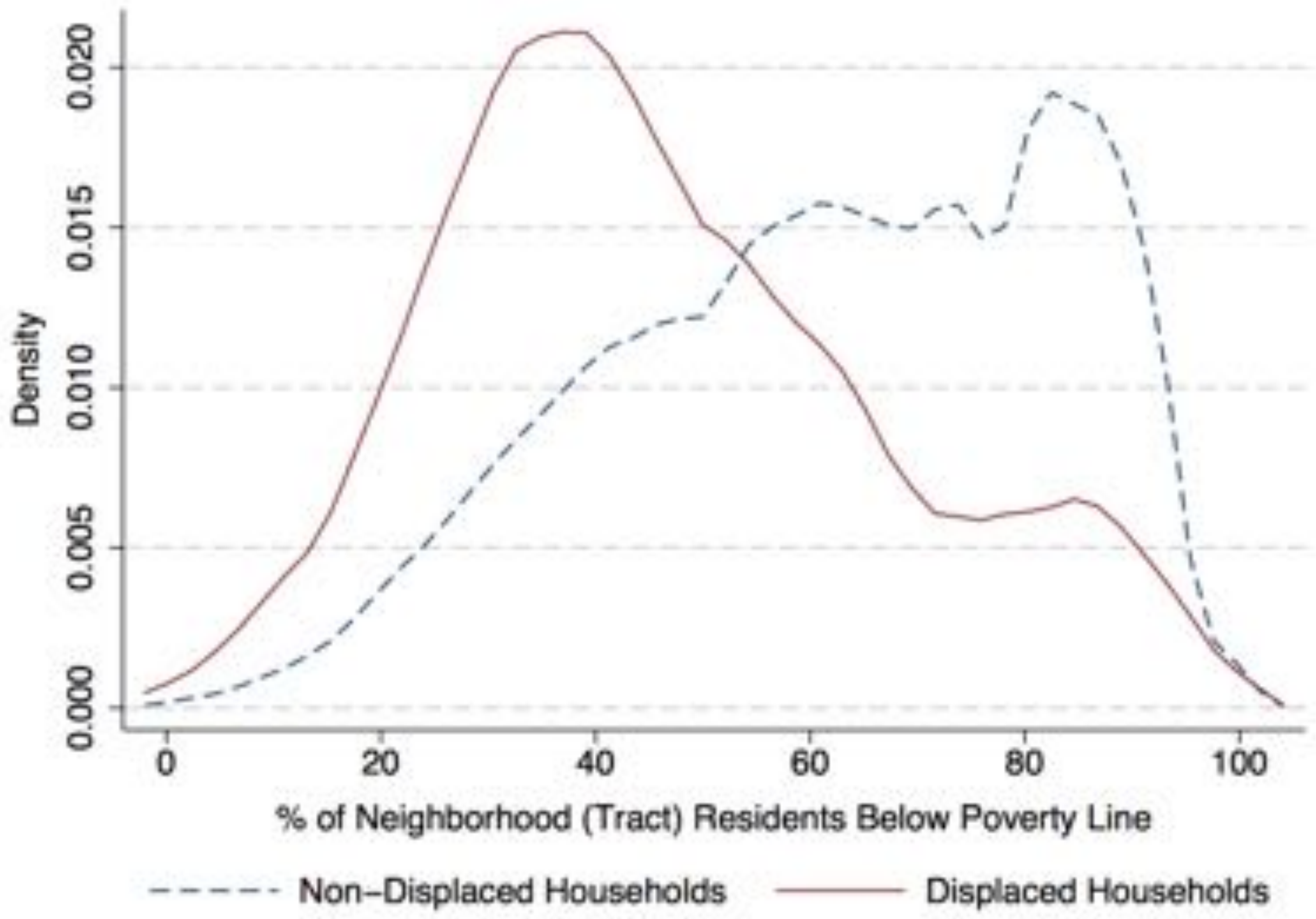



Impacts of Experimental Voucher by Child's Age at Random Assignment
Household Income, Age ≥ 24 ($)
Experimental Vs. Control ITT on Income ($)
4000
2000
0
-2000
-4000
-6000
10
12
14
16
Age at Random Assignment

Chyn (2018)

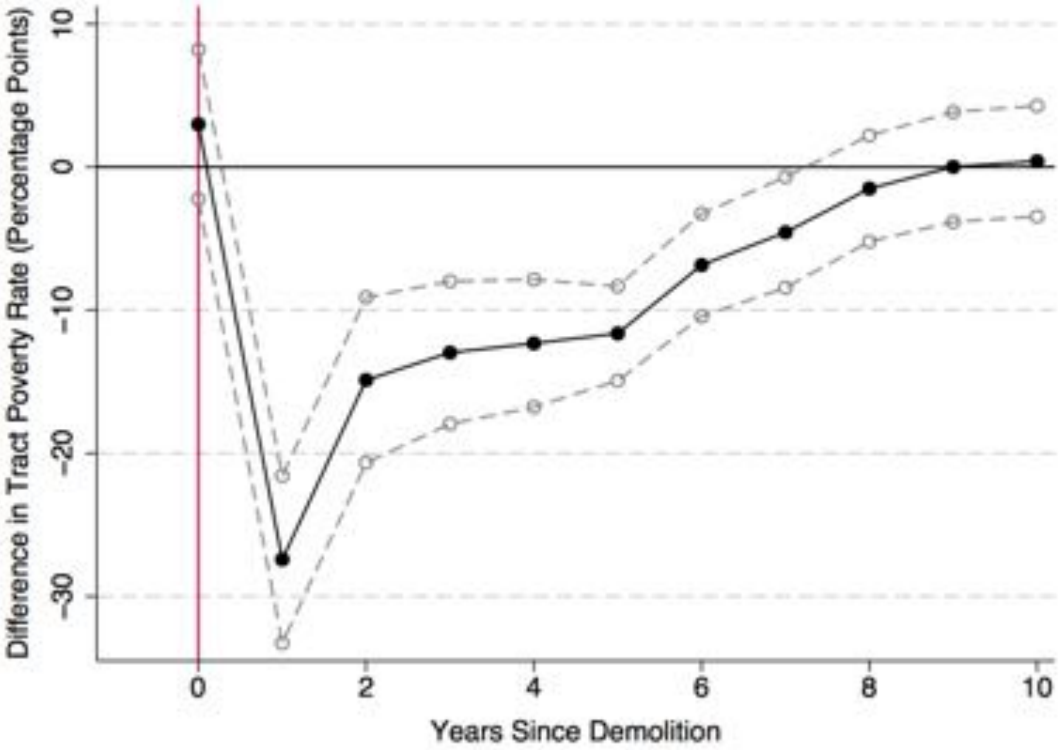
- Chyn (2018): "Moved to Opportunity: The Long-Run Effect of Public Housing Demolition on Labor Market Outcomes of Children"
    - Hope IV demolitions
    - Previous work documents impacts on test scores (Jacob 2004: "Public Housing, Housing Vouchers, and Student Achievement: Evidence from Public Housing 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



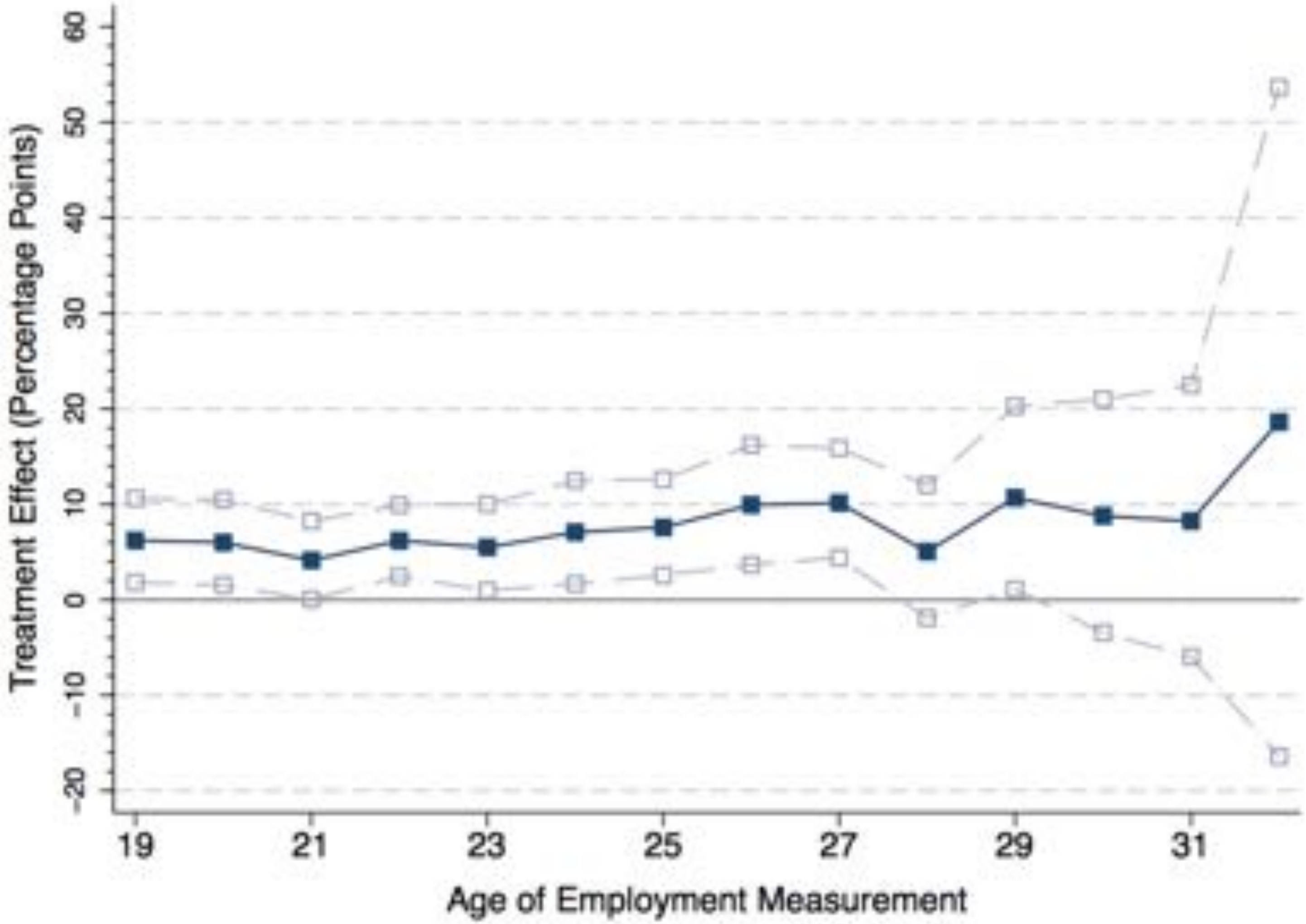
Notes: This figure illustrates the change over time in the difference in neighborhood poverty rate between displaced (treated) and non-displaced (control) households with children (age 7 to 18 at baseline). Specifically, I plot (in solid black) the set of coefficients π_y for $y \in \{0, \cdots, 10\}$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

$$pbpov_{htp} = \sum_{y=0}^{y=10} \pi_y \; treat_h \; \mathbf{1}(t - t^* = y) + \sum_{y=0}^{y=10} \delta_y \mathbf{1}(t - t^* = y) + \psi_p + \epsilon_{ht}$$

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 ψ_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 π_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)



(b) Dependent Variable: Annual Earnings ($)

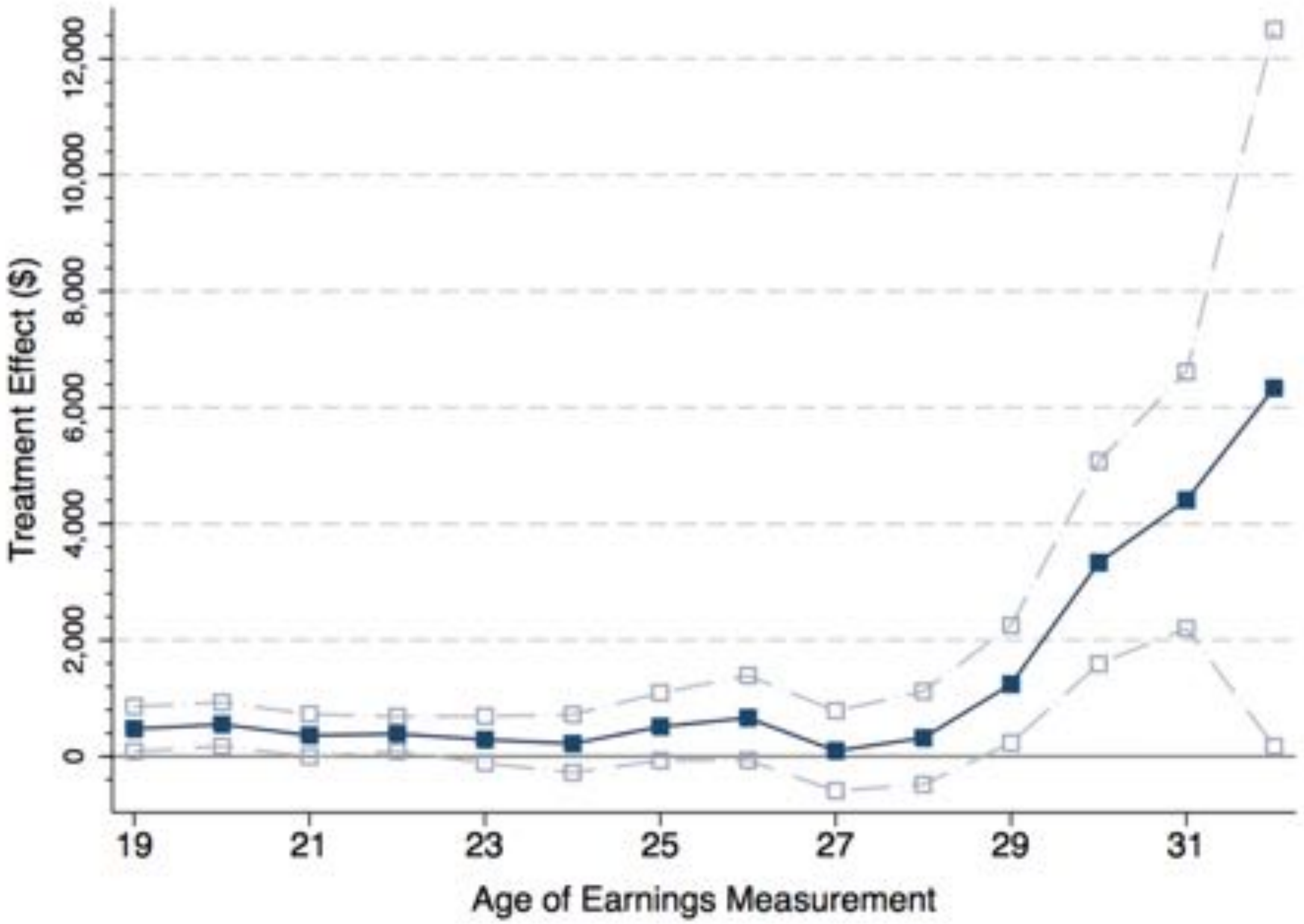
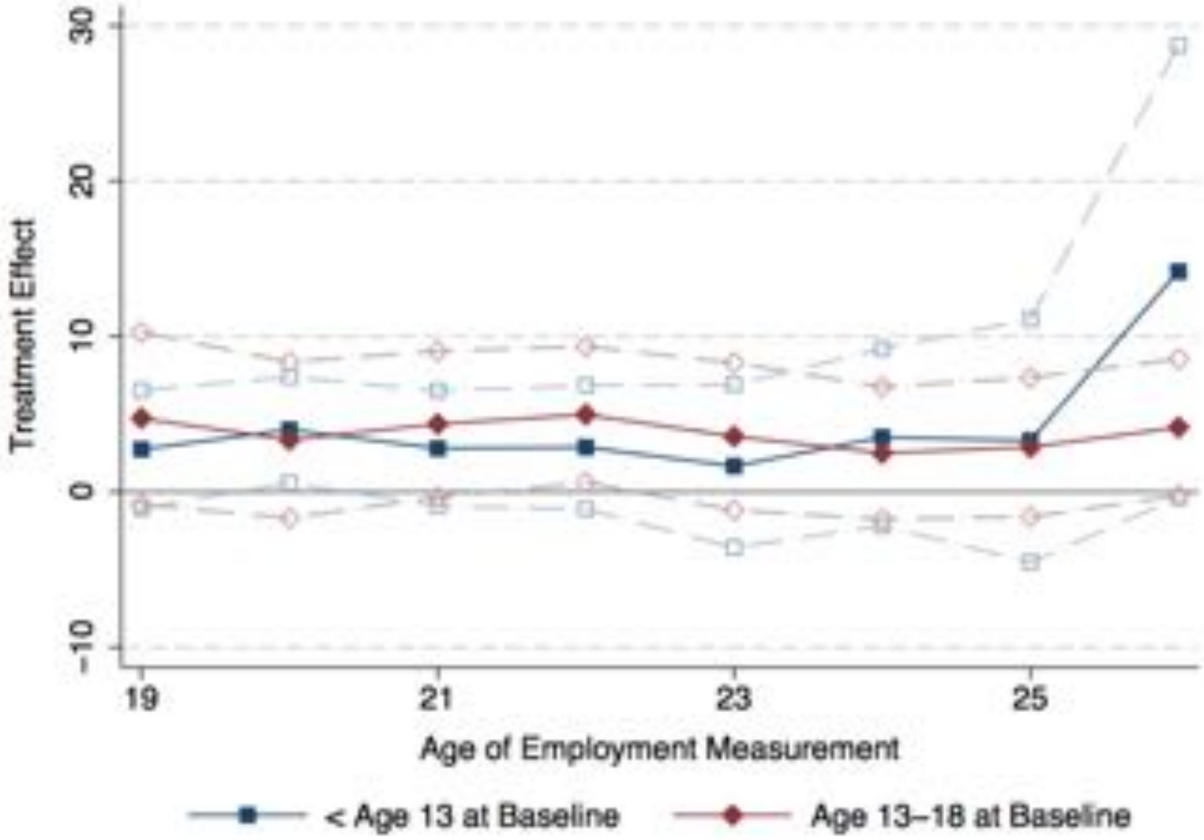
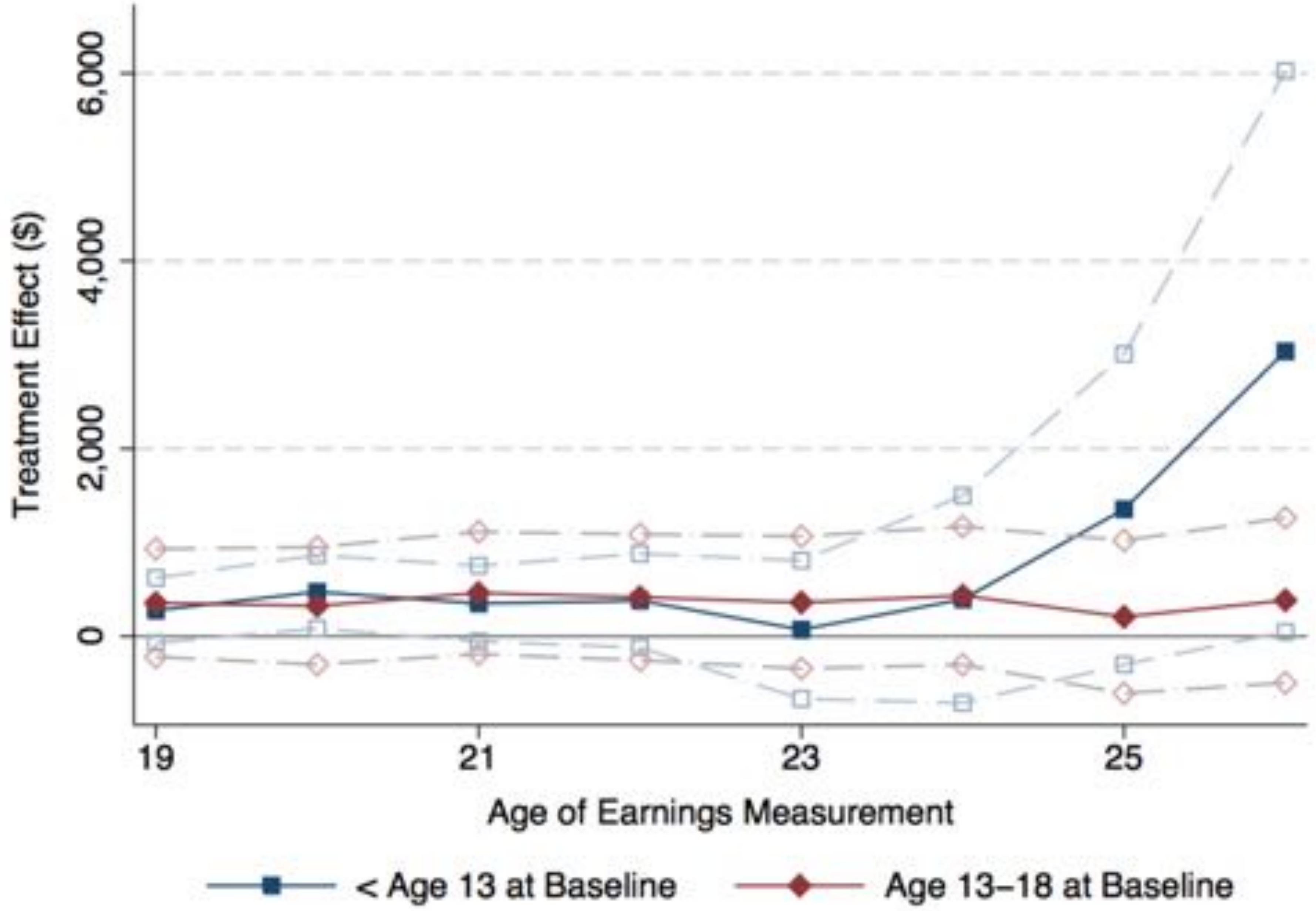


Figure 4: Younger vs Older Children: Labor-Market Treatment Effects by Age of Measurement

(a) Dependent Variable: Employed (=1)



(b) Dependent Variable: Annual Earnings ($)



Comparison to Section 8

- Chyn (2018) 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

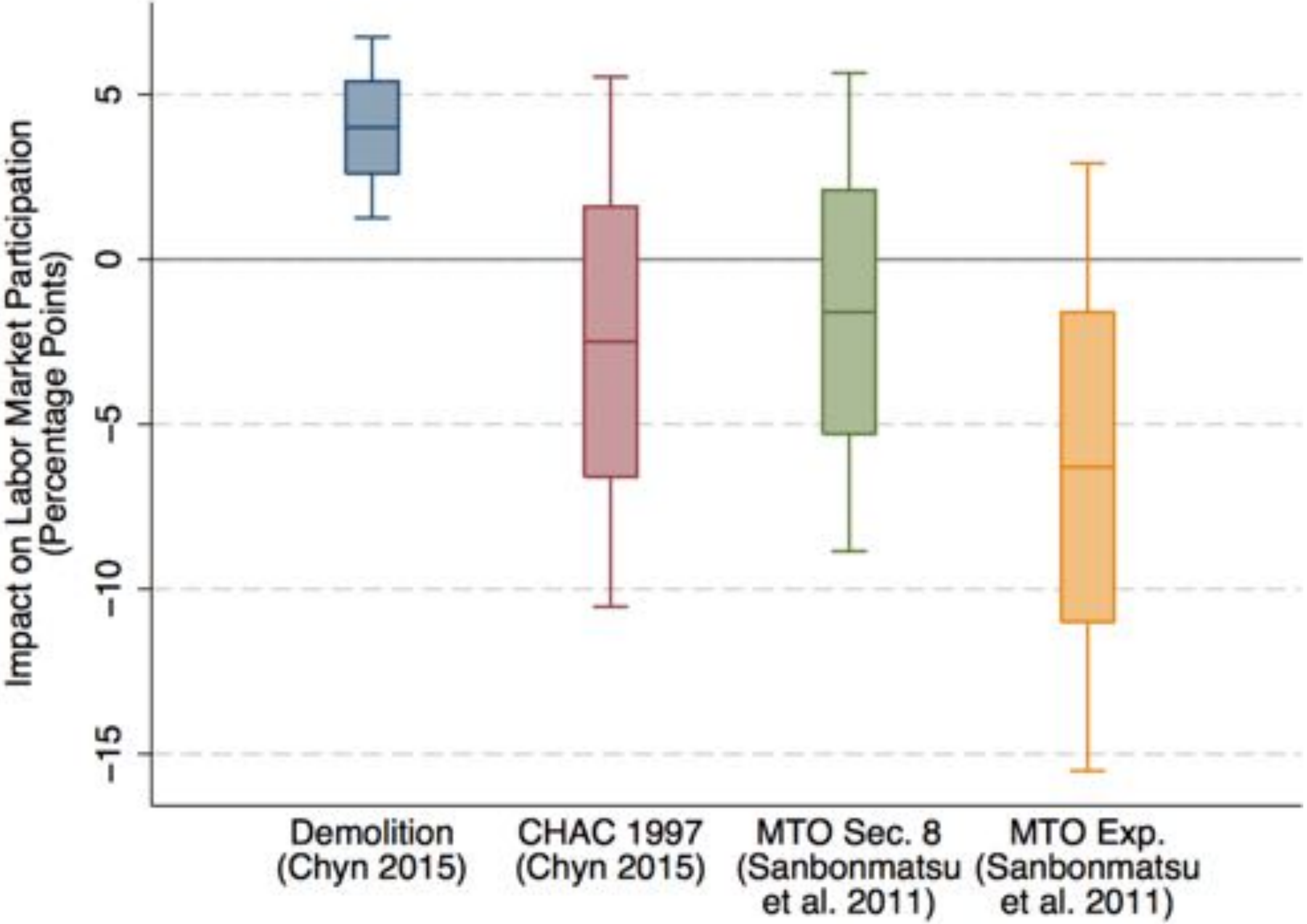
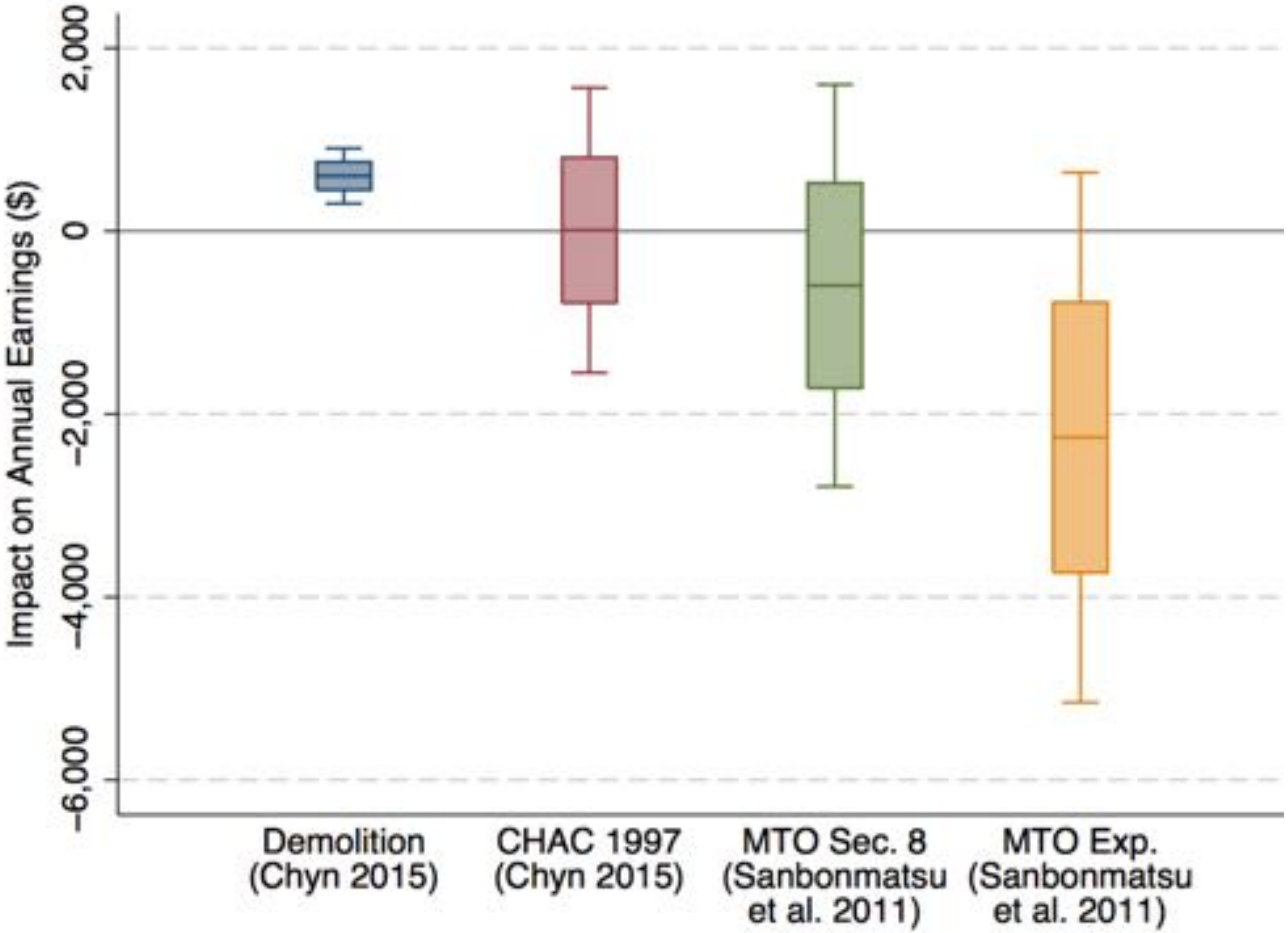


Figure 7: Effects on Adult Earnings of Children Across Studies



Housing Demolitions in Chicago

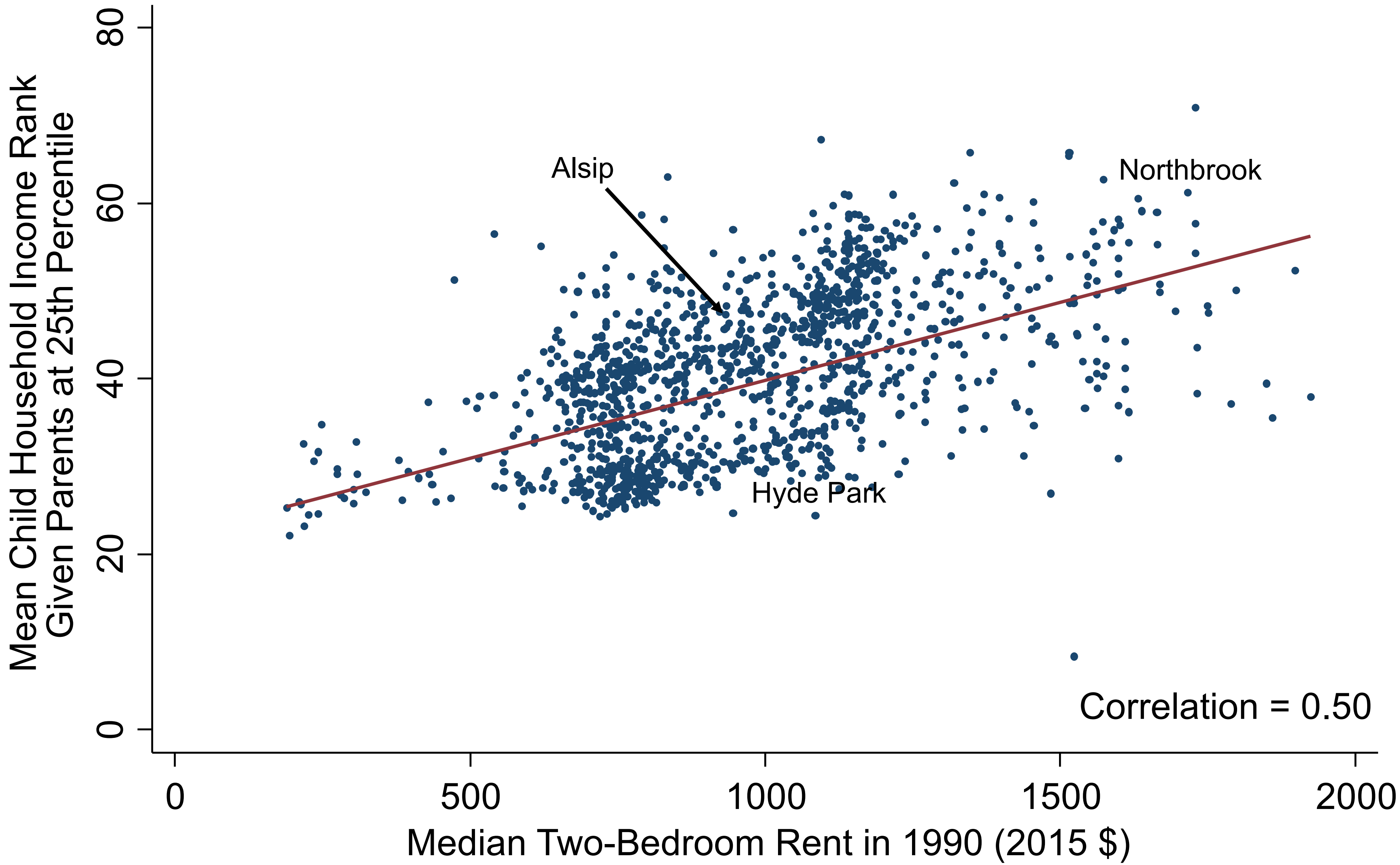
- Why no impact of Section 8 vouchers?
- Potential for "Reverse Roy" sorting model?
  - Those forced to move have higher returns than "compliers" from vouchers
  - Forcing people to move delivers larger impacts?
- Alternative story: Section 8 and demolition is a different treatment
  - Section 8 does not induce moves to better neighborhoods
  - If neighborhood quality matters, then should we expect impacts of Section 8?
  - But, suggests demolition very bad neighborhoods can improve outcomes

The Price of Opportunity

- Neighborhoods matter. Why don't people "move to opportunity?"
- Moving at birth from tract at 25th percentile of distribution of upward mobility to a tract at 75th percentile within county → $206,000 gain in lifetime earnings
- Feasibility of such moves relies on being able to find affordable housing in high-opportunity neighborhoods
- How does the housing market price the amenity of better outcomes for children?

Children's Mean Income Ranks in Adulthood vs. Median Rents in Chicago, by Tract

Children with Parents at 25th Percentile

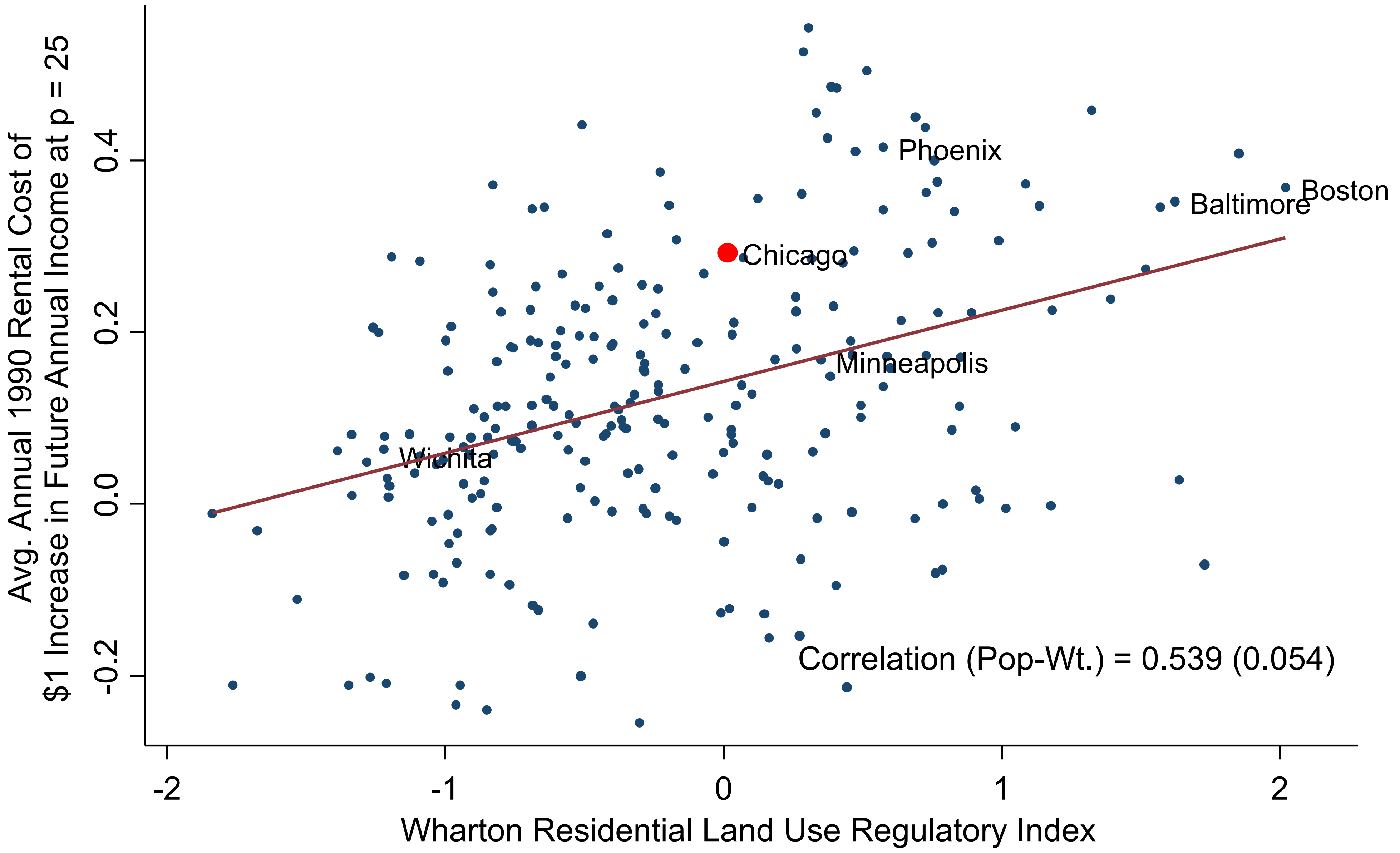


Heterogeneity in the Price of Opportunity

- Price of opportunity itself is highly heterogeneous across metro areas

- Policies such as land use regulation may play a role in determining this price in equilibrium

Price of Opportunity vs. Land Use Regulations, by Metro Area



Note: figure excludes Honolulu for scaling purposes

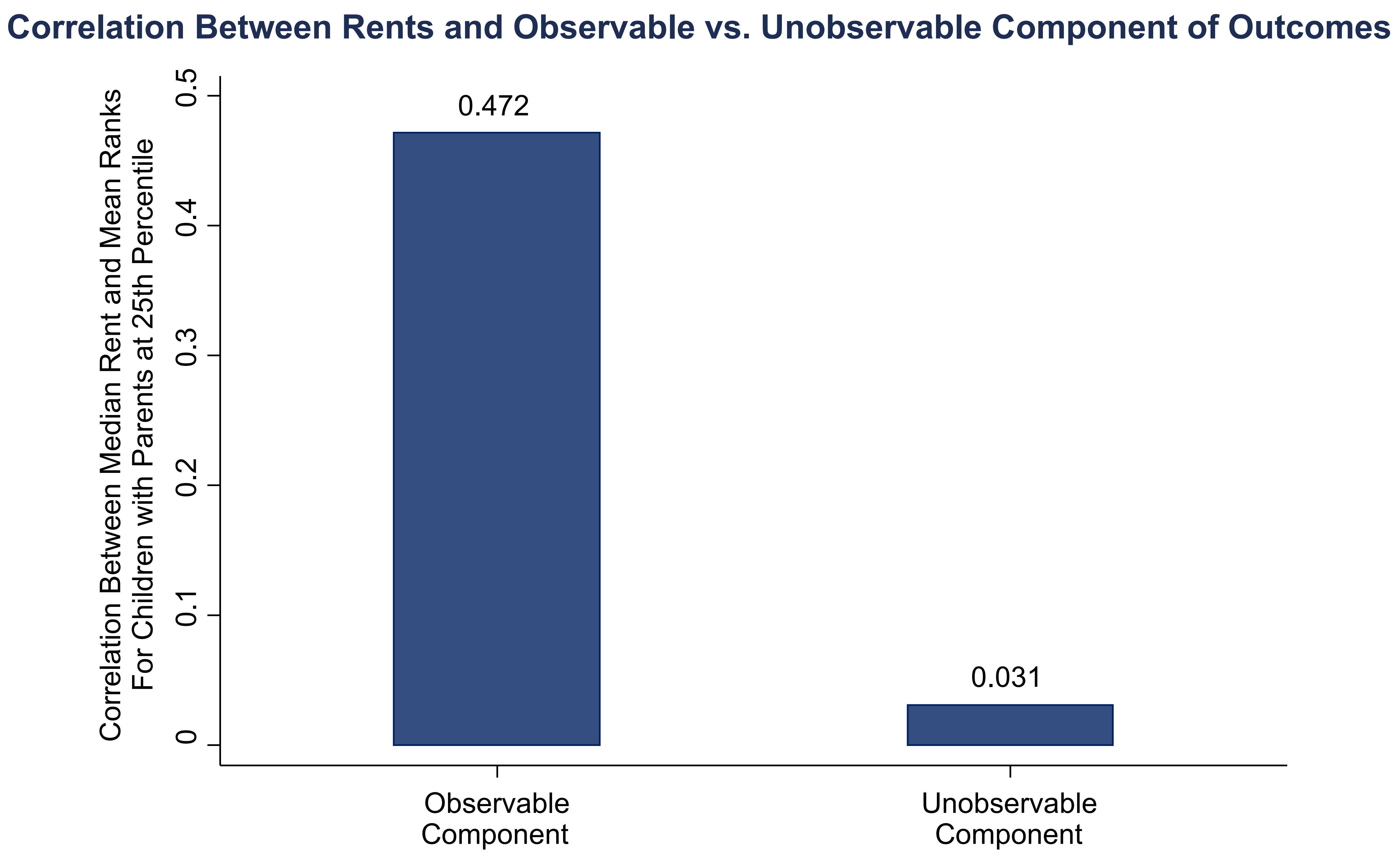
The Price of Opportunity

- Upward mobility is priced by housing markets on average, but there is still substantial residual variation in outcomes conditional on rents

- What explains the existence of areas that offer good outcomes for children but have low rents in spatial equilibrium?

  - One explanation: these areas have other disamenities, e.g. longer commutes

  - Alternative explanation: lack of information or barriers such as discrimination [DeLuca et al 2016, Christensen and Timmins 2018]


Correlation Between Rents and Observable vs. Unobservable Component of Outcomes
Correlation Between Median Rent and Mean Ranks
For Children with Parents at 25th Percentile
0
0.1
0.2
0.3
0.4
0.5
0.472
0.031
Observable
Component
Unobservable
Component

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?

Appendix: Causal Fixed Effects and Optimal Shrinkage

- What neighborhoods have the highest causal effect on children's outcomes?
- Note the observed variation across places contains both sorting and causal components
  - 2/3 may be causal, but 1/3 is still sorting

Objectives:

- Can we construct unbiased estimates of the true causal effect?
- Can we construct optimal forecasts of the place with the highest causal effect?
- Key question: Why are these objectives different?

Causal Effects of Each County

- Chetty and Hendren (2018b) 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

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

$$y_i = \underbrace{(T_c - m)}_{\text{Exposure}} \left[\underbrace{\mu_d 1\{d(i) = d\}}_{\text{Dest. FE}} - \underbrace{\mu_o 1\{o(i) = o\}}_{\text{Orig. FE.}} \right] + \underbrace{\alpha_{odps}}_{\text{orig x Dest FE}} + \eta_i$$

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

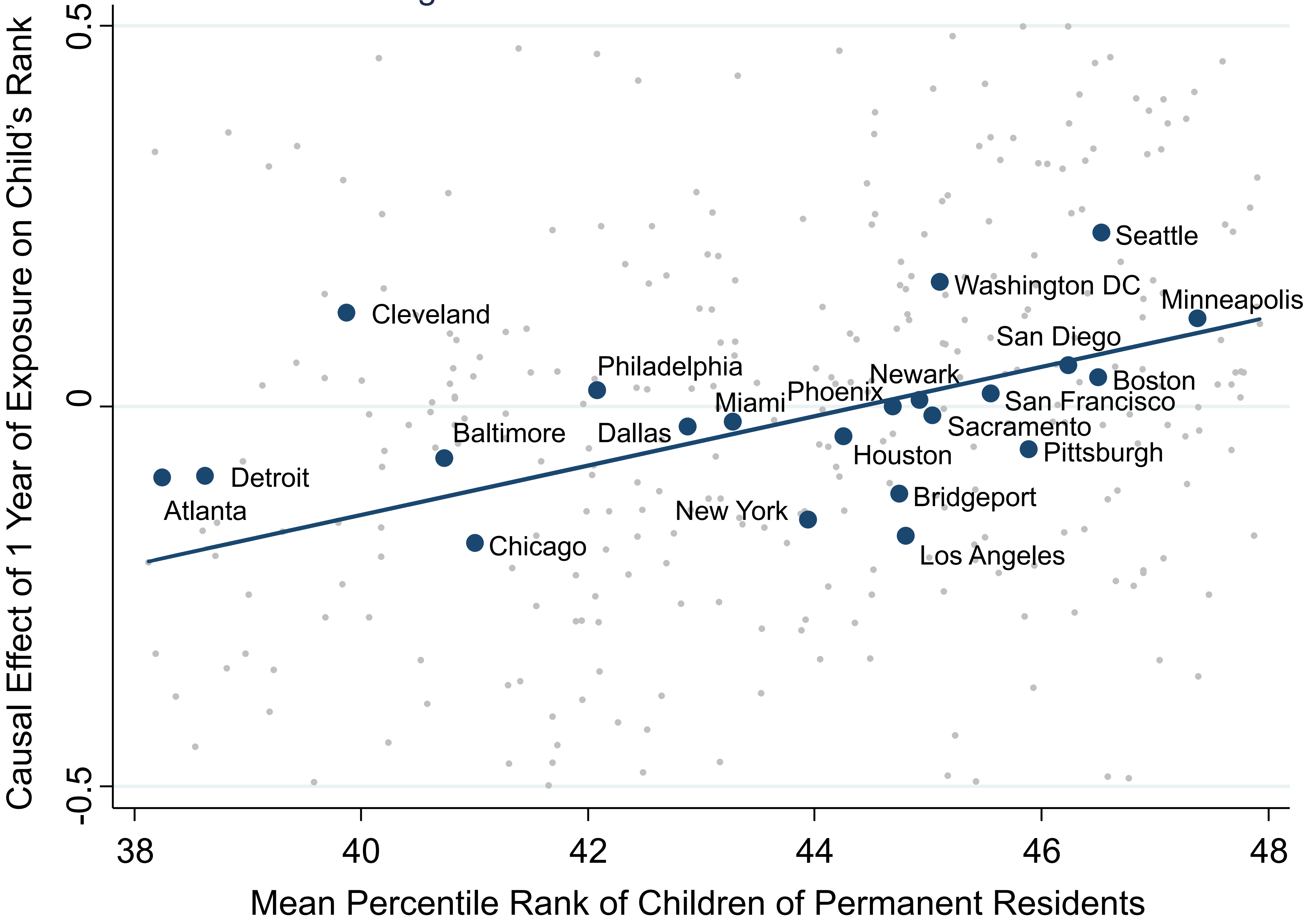
$$\mu_c = \mu_c^0 + \mu_c^P p$$

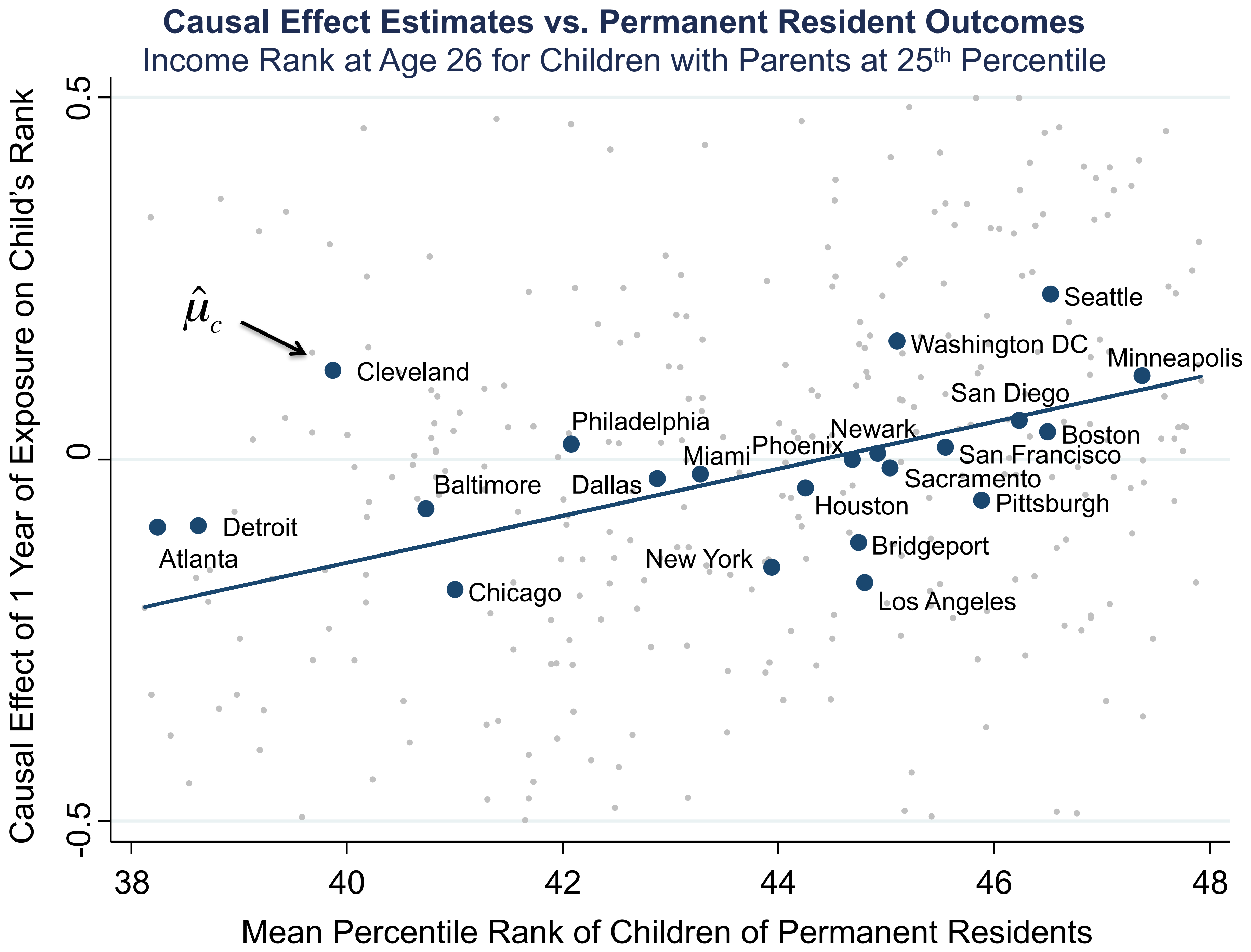
- Include origin-by-destination fixed effects to isolate variation in exposure

- What is the identification condition?

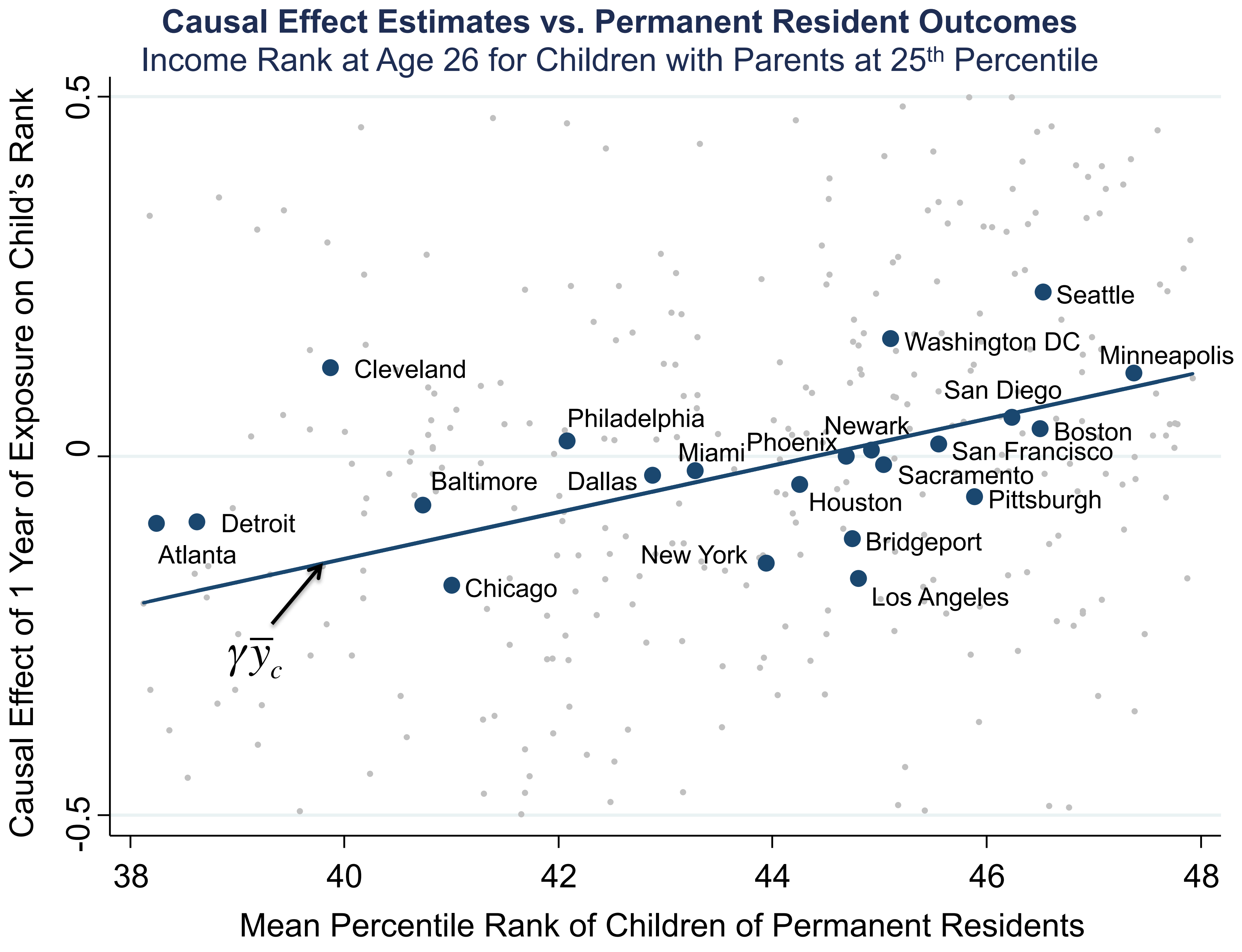
Causal Effect Estimates vs. Permanent Resident Outcomes

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



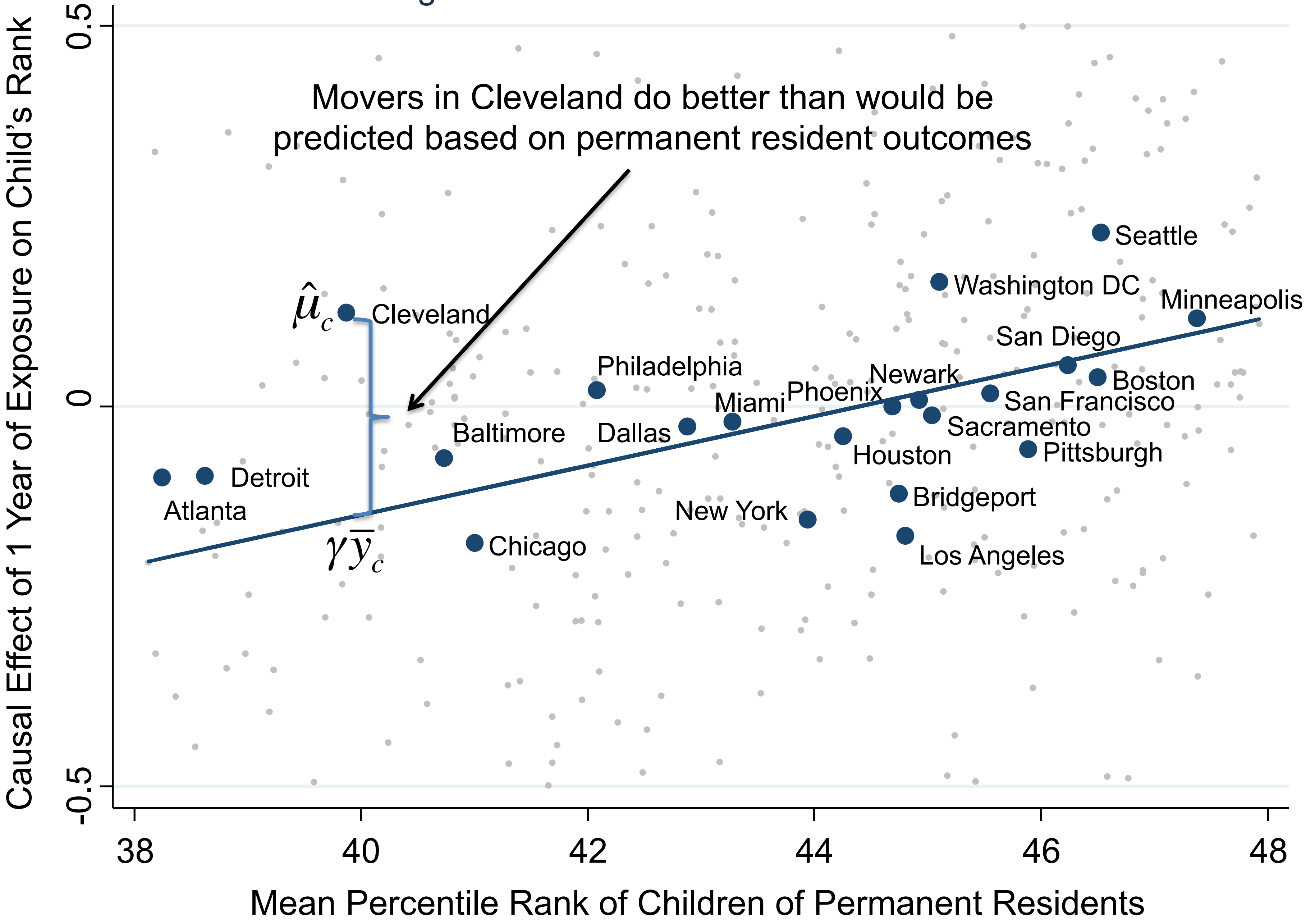


Causal Effect Estimates vs. Permanent Resident Outcomes
Income Rank at Age 26 for Children with Parents at 25th Percentile
Causal Effect of 1 Year of Exposure on Child's Rank
Mean Percentile Rank of Children of Permanent Residents
0.5
0
-0.5
38
40
42
44
46
48
$\hat{\mu}_c$
Seattle
Washington DC
Minneapolis
Cleveland
San Diego
Philadelphia
Newark
Boston
Phoenix
Miami
San Francisco
Sacramento
Baltimore
Dallas
Houston
Pittsburgh
Detroit
Atlanta
Bridgeport
New York
Chicago
Los Angeles


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
0.5
0
-0.5
38
40
42
44
46
48
Seattle
Washington DC
Minneapolis
Cleveland
San Diego
Philadelphia
Newark
Boston
Phoenix
San Francisco
Miami
Sacramento
Baltimore
Dallas
Houston
Pittsburgh
Detroit
Bridgeport
Atlanta
New York
Chicago
Los Angeles
$\gamma\overline{y}_c$

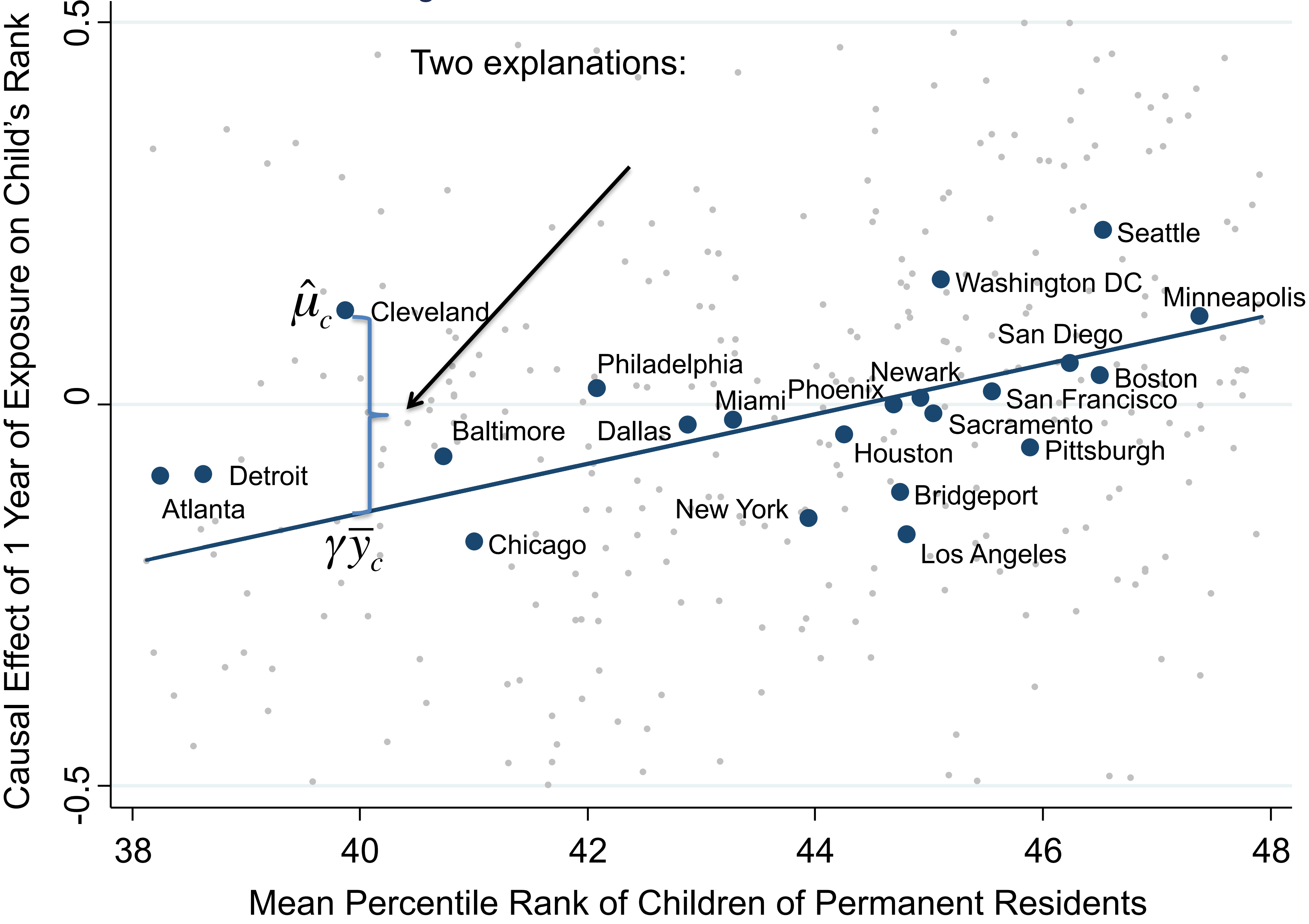
Causal Effect Estimates vs. Permanent Resident Outcomes

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



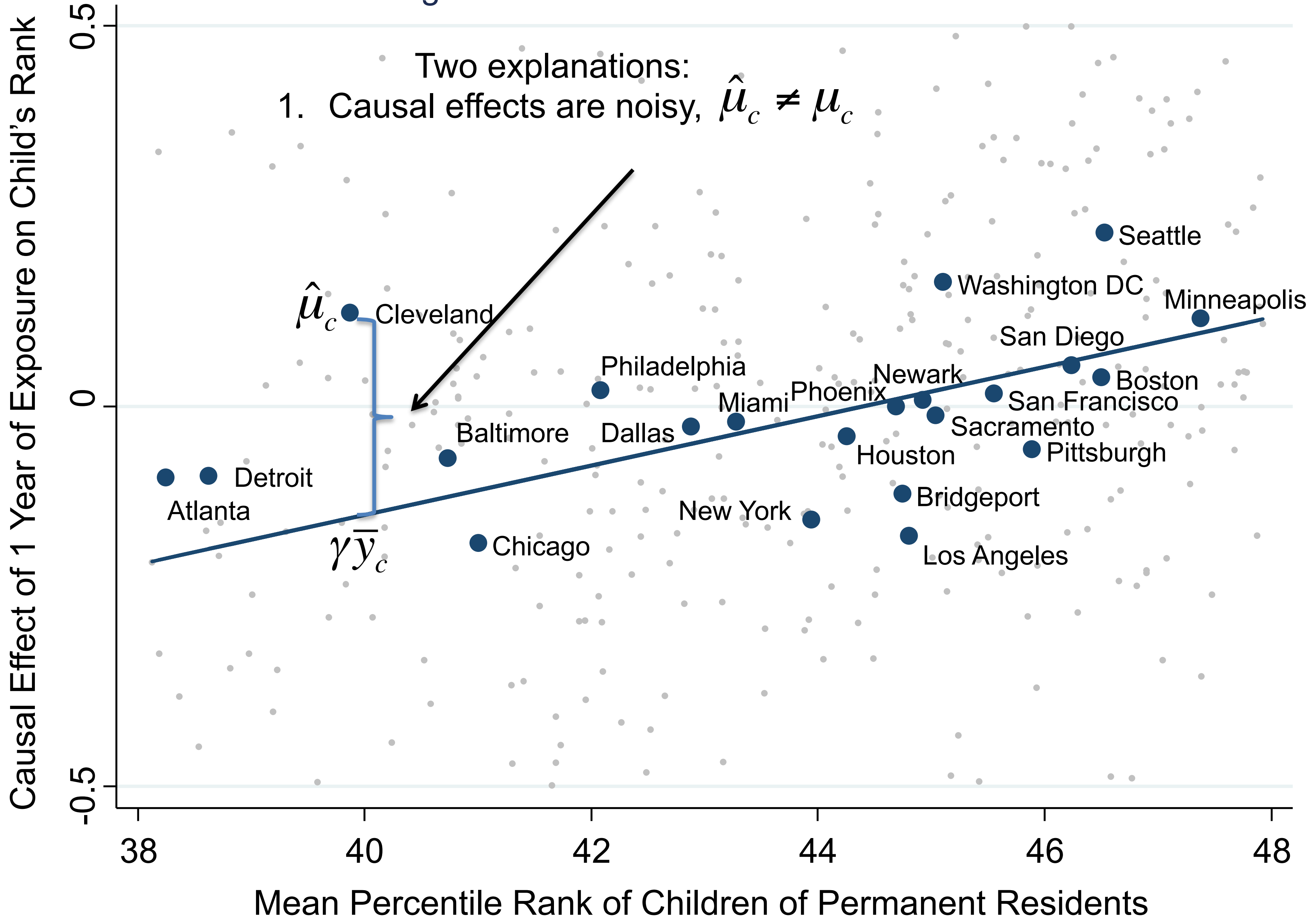
Causal Effect Estimates vs. Permanent Resident Outcomes

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



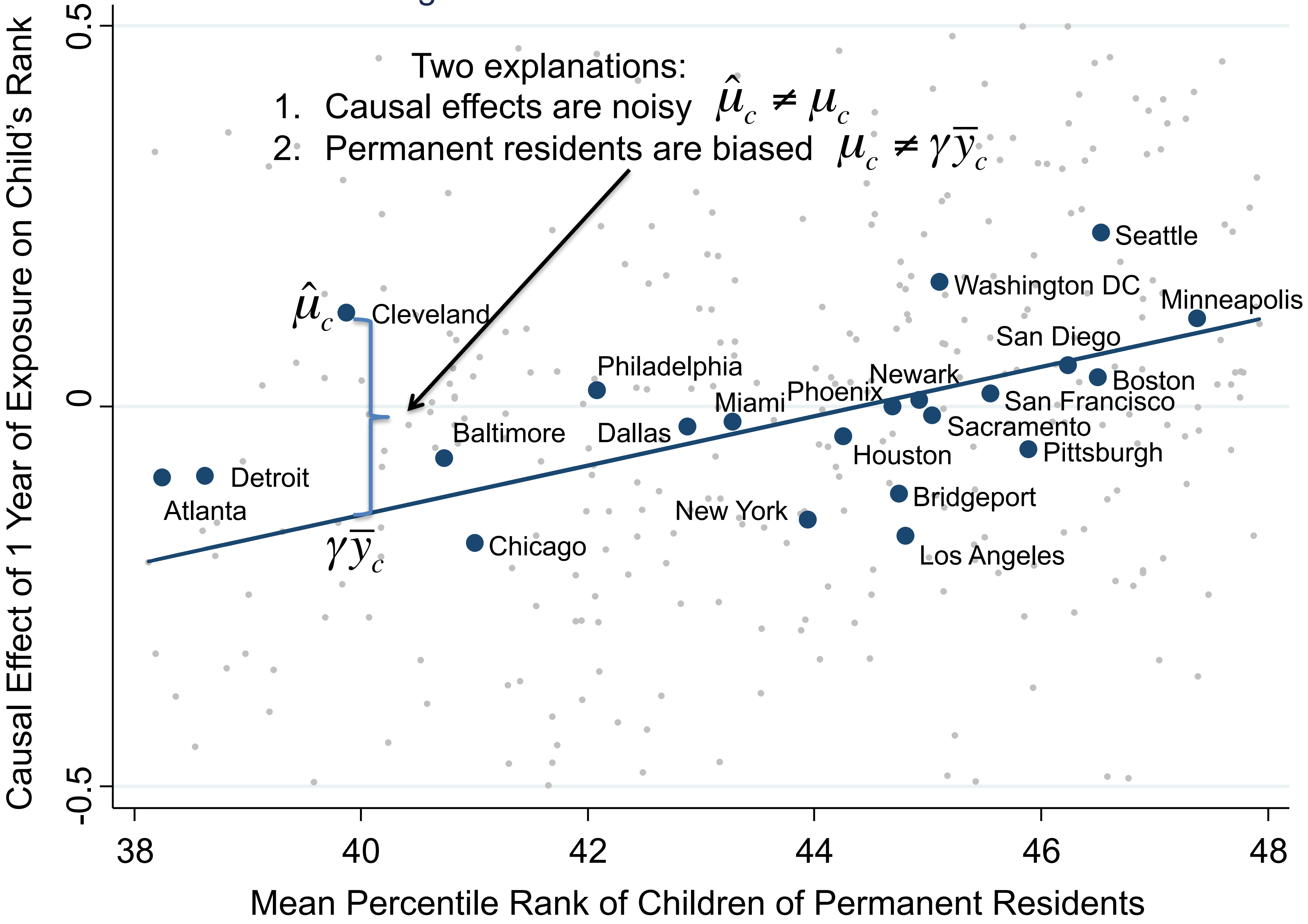
Causal Effect Estimates vs. Permanent Resident Outcomes

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



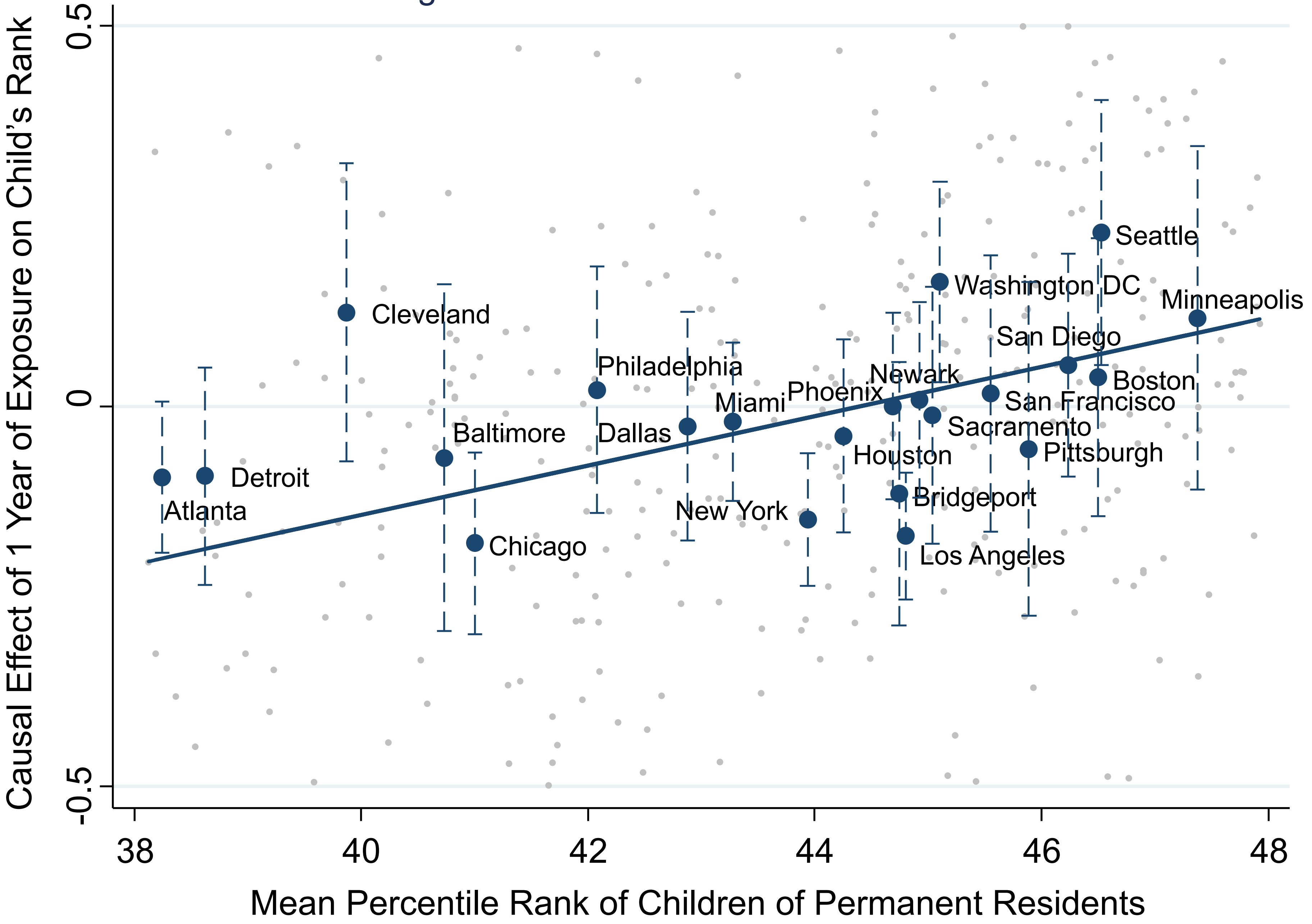
Causal Effect Estimates vs. Permanent Resident Outcomes

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



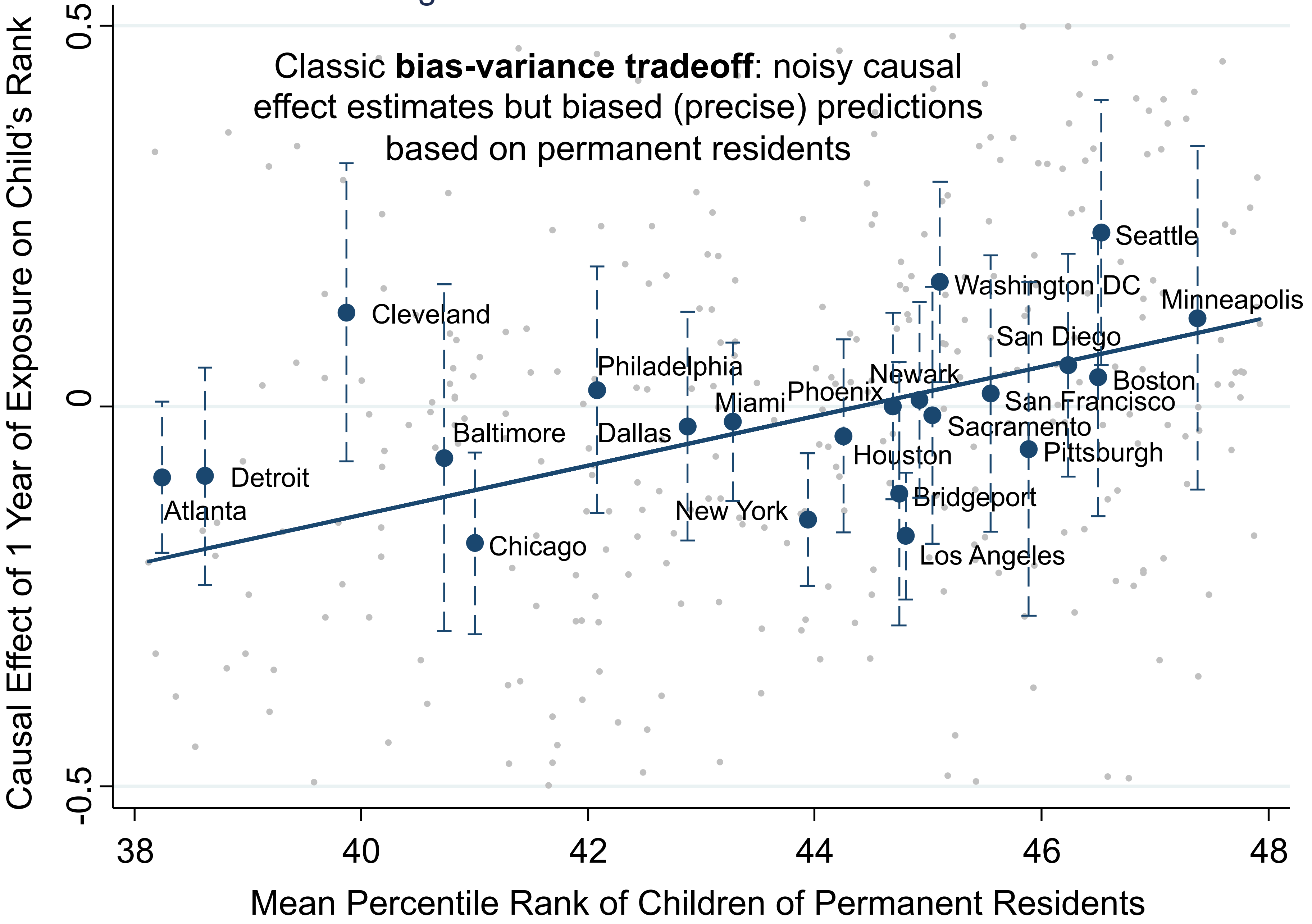
Causal Effect Estimates vs. Permanent Resident Outcomes

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



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 ε_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, $E\left[\varepsilon_c^2 \mid s_c\right] = s_c^2$
- So, $Var(\varepsilon_c) = E\left[\varepsilon_c^2\right] = E_c\left[E\left[\varepsilon_c^2 \mid s_c\right]\right] = E_c\left[s_c^2\right]$
- Variance of true place effects is given by

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

Objective 1: Magnitude of Place Effects

- Chetty and Hendren (2016) estimate across counties for parents at 25th percentile:

$$Var(\hat{\mu}_c) = 0.434 \qquad E_c\left[s_c^2\right] = 0.402$$

- 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

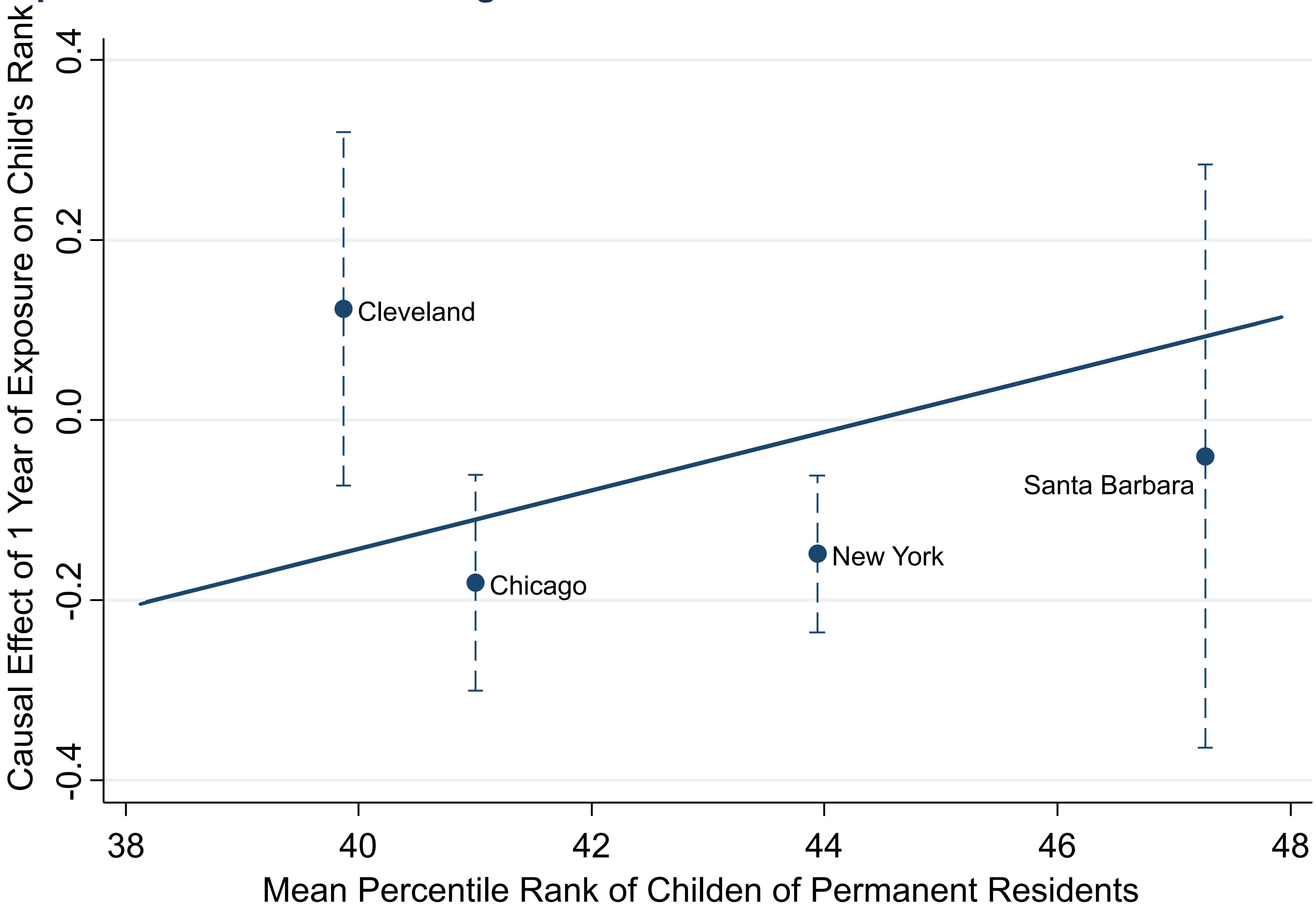
  - To interpret units, note that 1 percentile ~= 3% change in earnings

- For children with parents at 25th percentile: 1 SD better county from birth (20 years) → 3.6 percentiles → 10% earnings gain

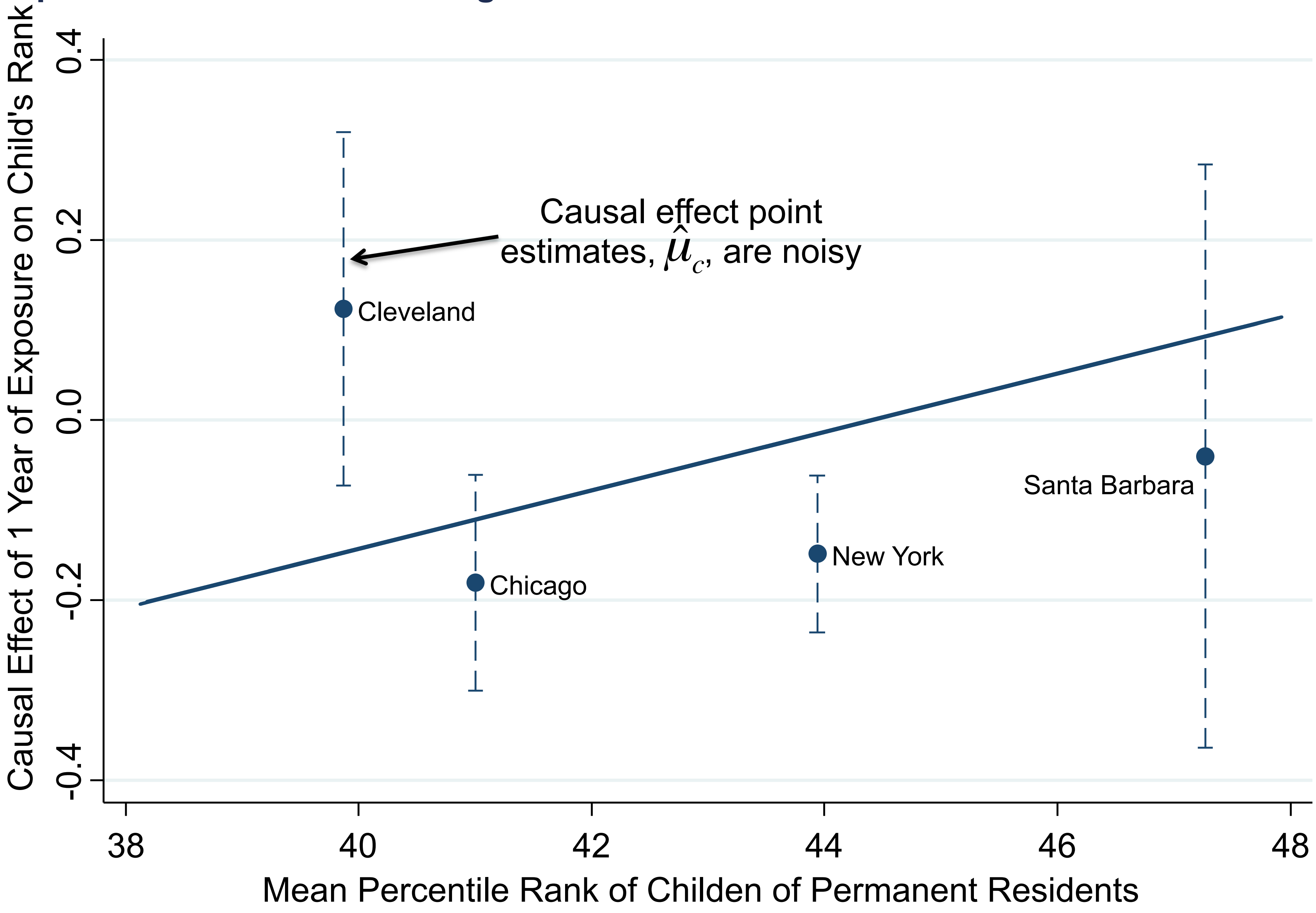
Objective 2: Forecasts of Best and Worst Areas

- 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:
  - E.g. School effects combining causal effects from lotteries with school value-added estimates [Angrist, et al. 2016, QJE: "Leveraging Lotteries for School Value-Added: Testing and Estimation]

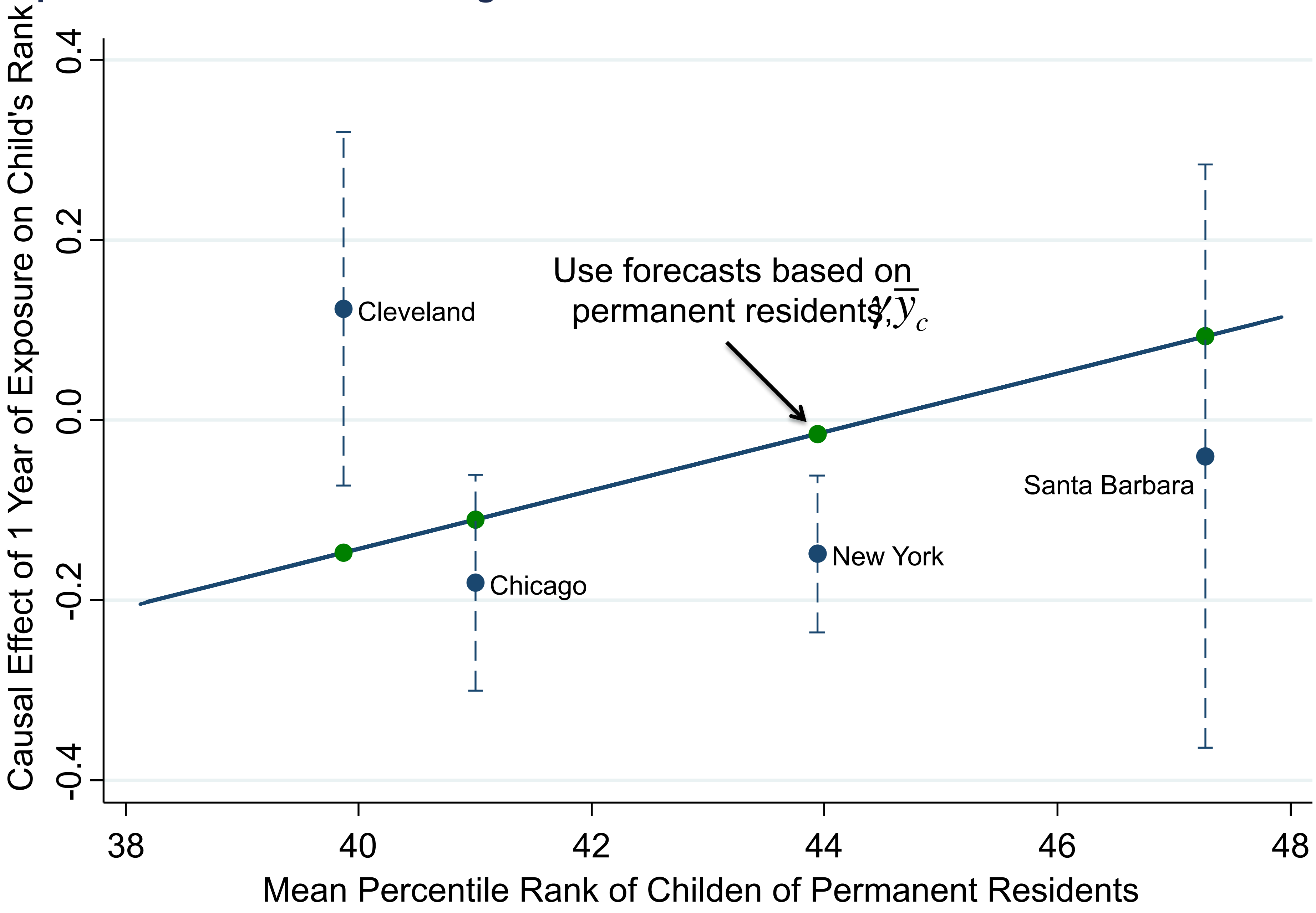
Optimal Forecasts Combining Fixed Effects and Permanent Resident Outcomes



Optimal Forecasts Combining Fixed Effects and Permanent Resident Outcomes



Optimal Forecasts Combining Fixed Effects and Permanent Resident Outcomes



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:

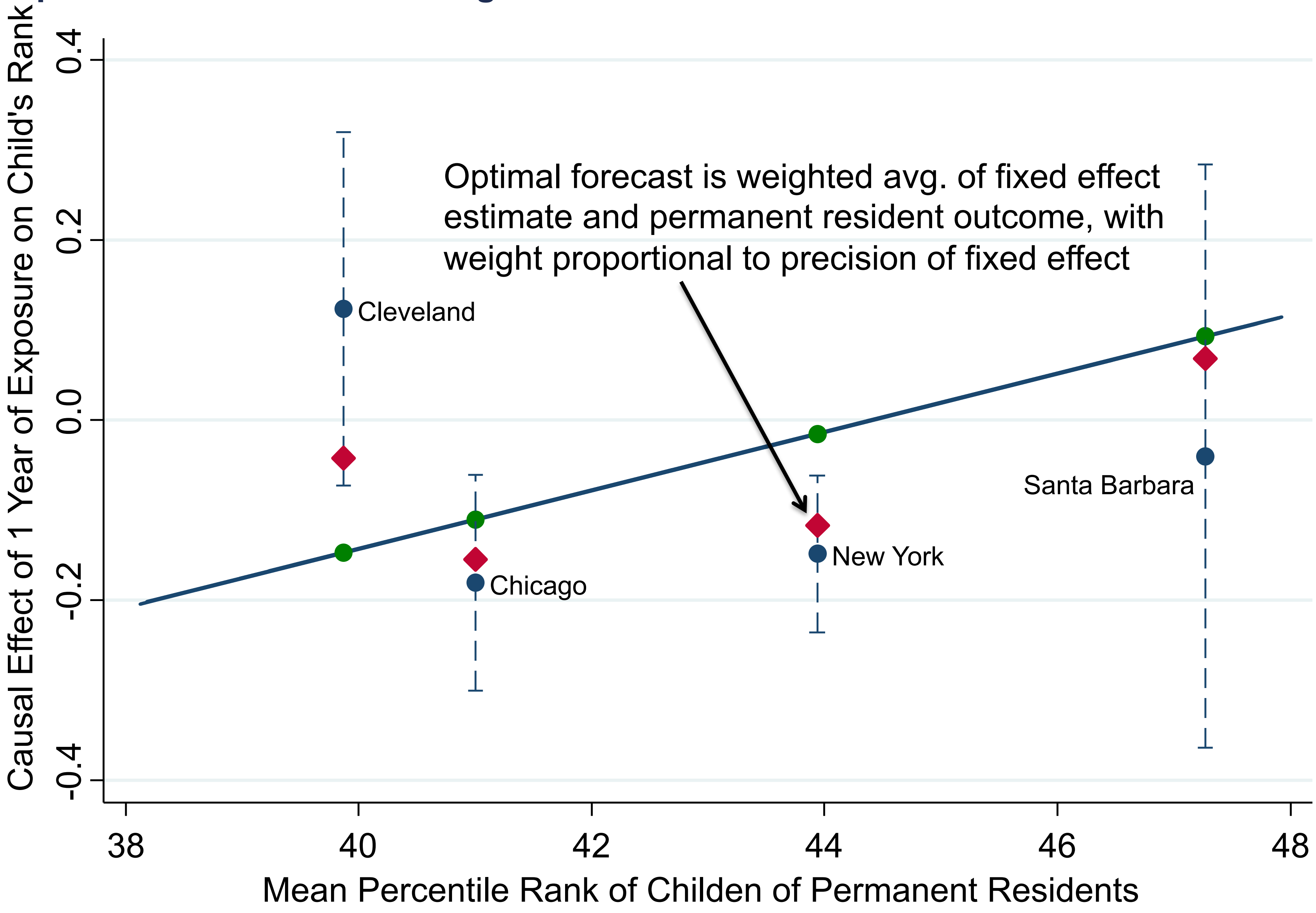
$$\rho_{1,c} = \frac{\sigma^2_{bias}}{\sigma^2_{noise,c} + \sigma^2_{bias}} \qquad \rho_{2,c} = \frac{\sigma^2_{noise,c}}{\sigma^2_{noise,c} + \sigma^2_{bias}}$$

where:

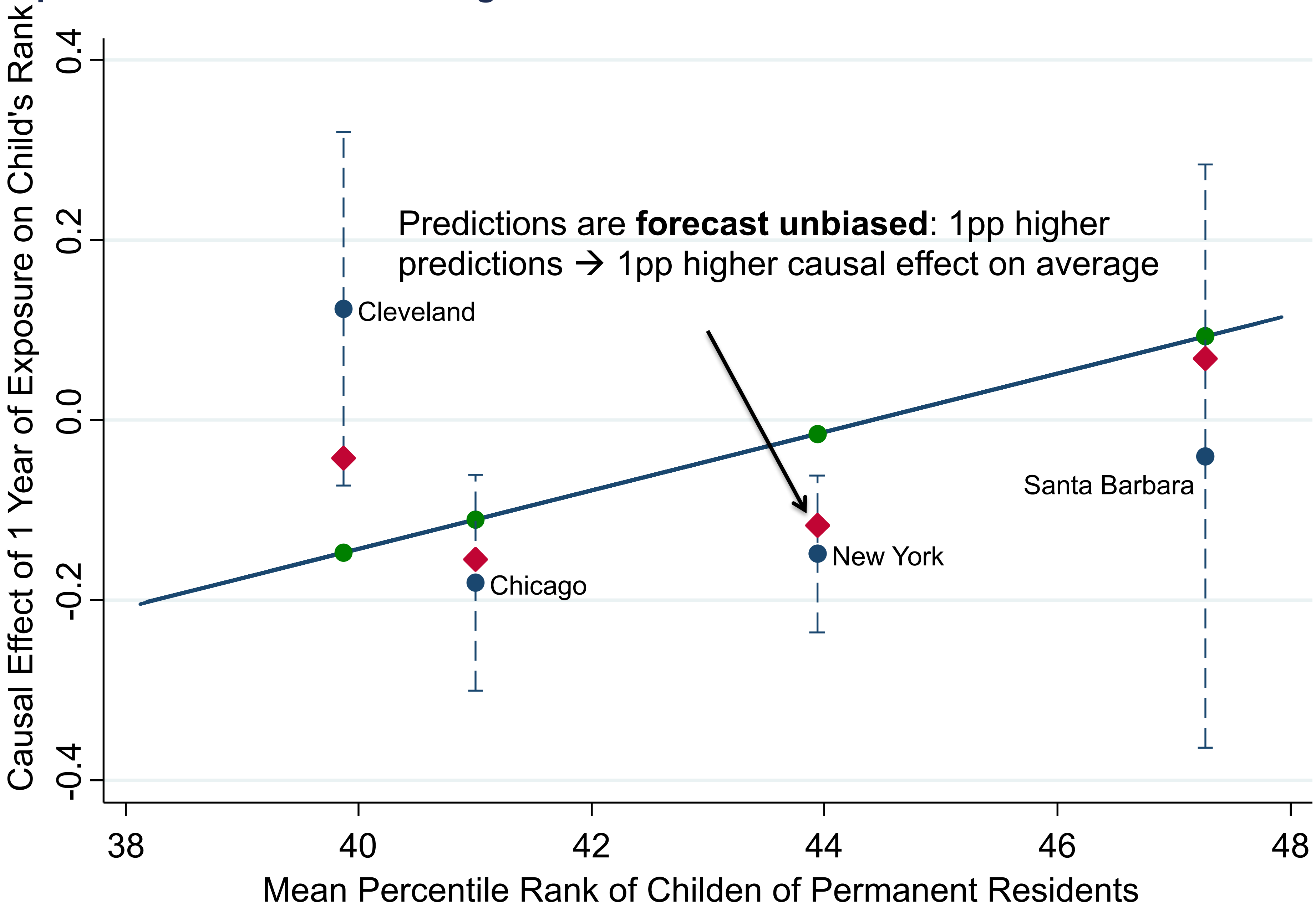
- $\sigma^2_{bias} = Var\left(\mu_c - \gamma \bar{y}_c\right)$ is residual variance of fixed effects

- $\sigma^2_{noise,c} = s^2_c$ is the noise variance of the fixed effects (=square of std error)

Optimal Forecasts Combining Fixed Effects and Permanent Resident Outcomes



Optimal Forecasts Combining Fixed Effects and Permanent Resident Outcomes



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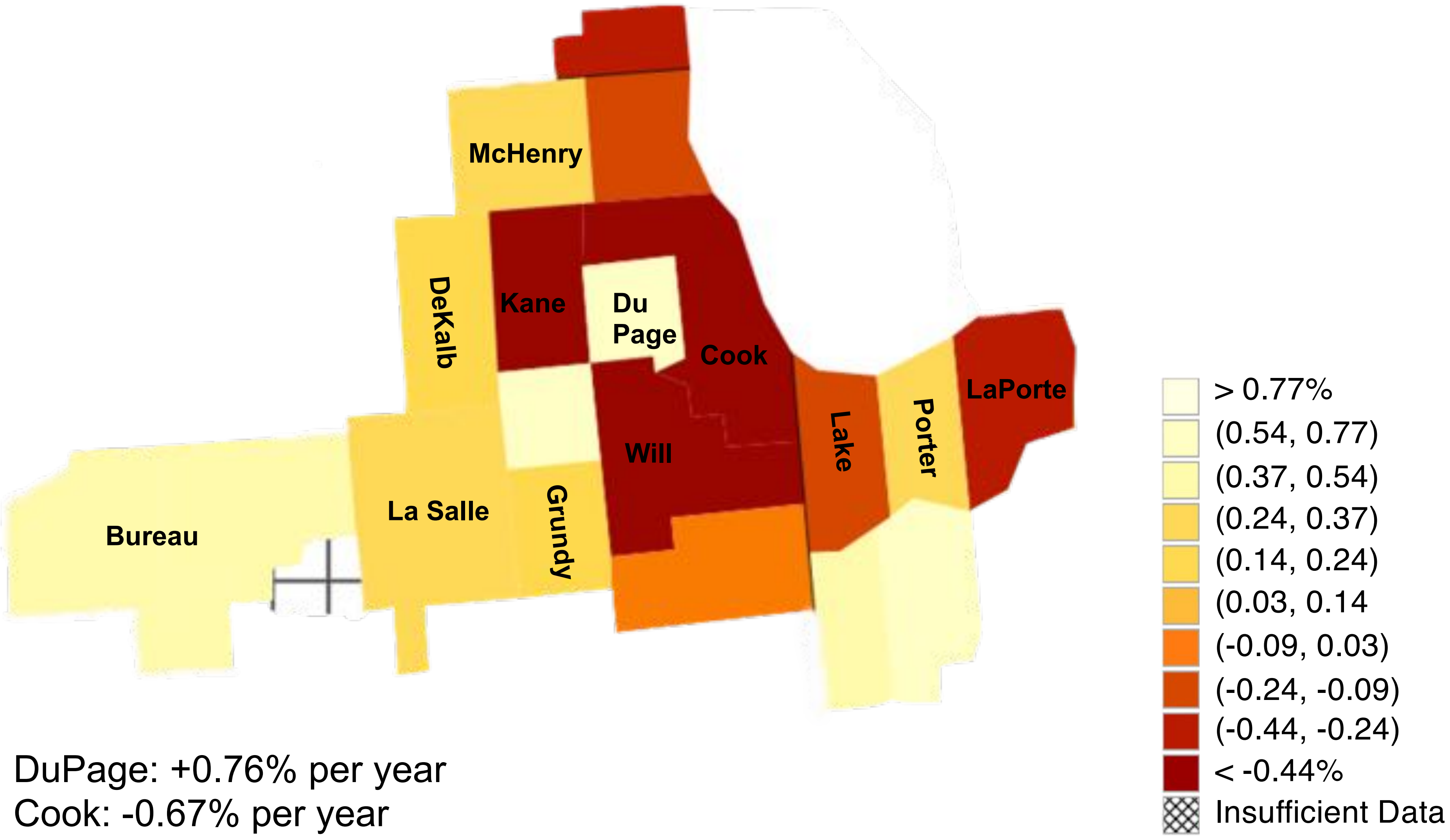
where:

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

  - $\sigma^2_{noise,c} = s_c^2$ is the noise variance of the fixed effects (varies across places)

Causal Effects of Growing up in Different Counties on Earnings in Adulthood

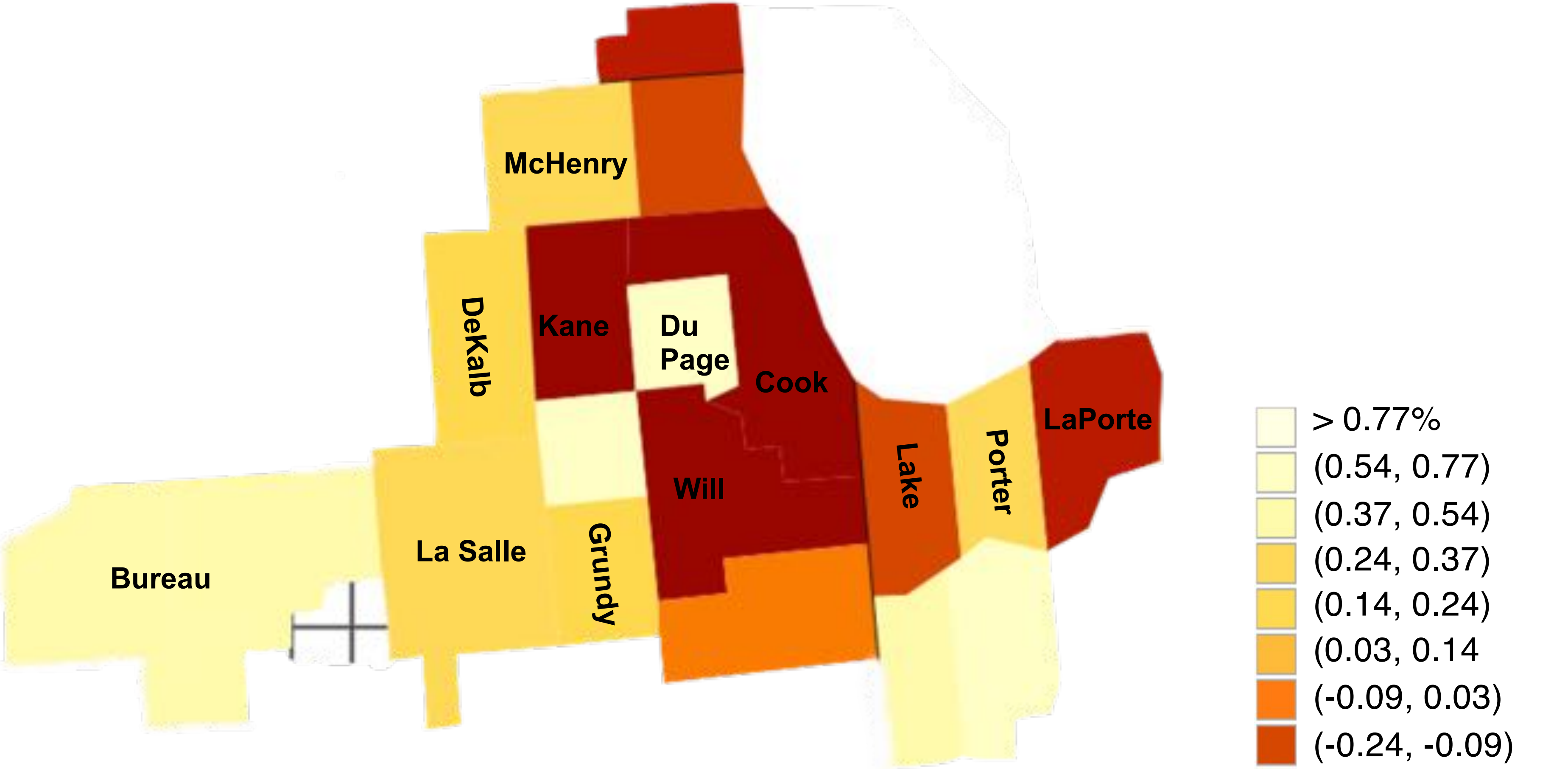
For Children in Low-Income (25th Percentile) Families in the Chicago Metro Area



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

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

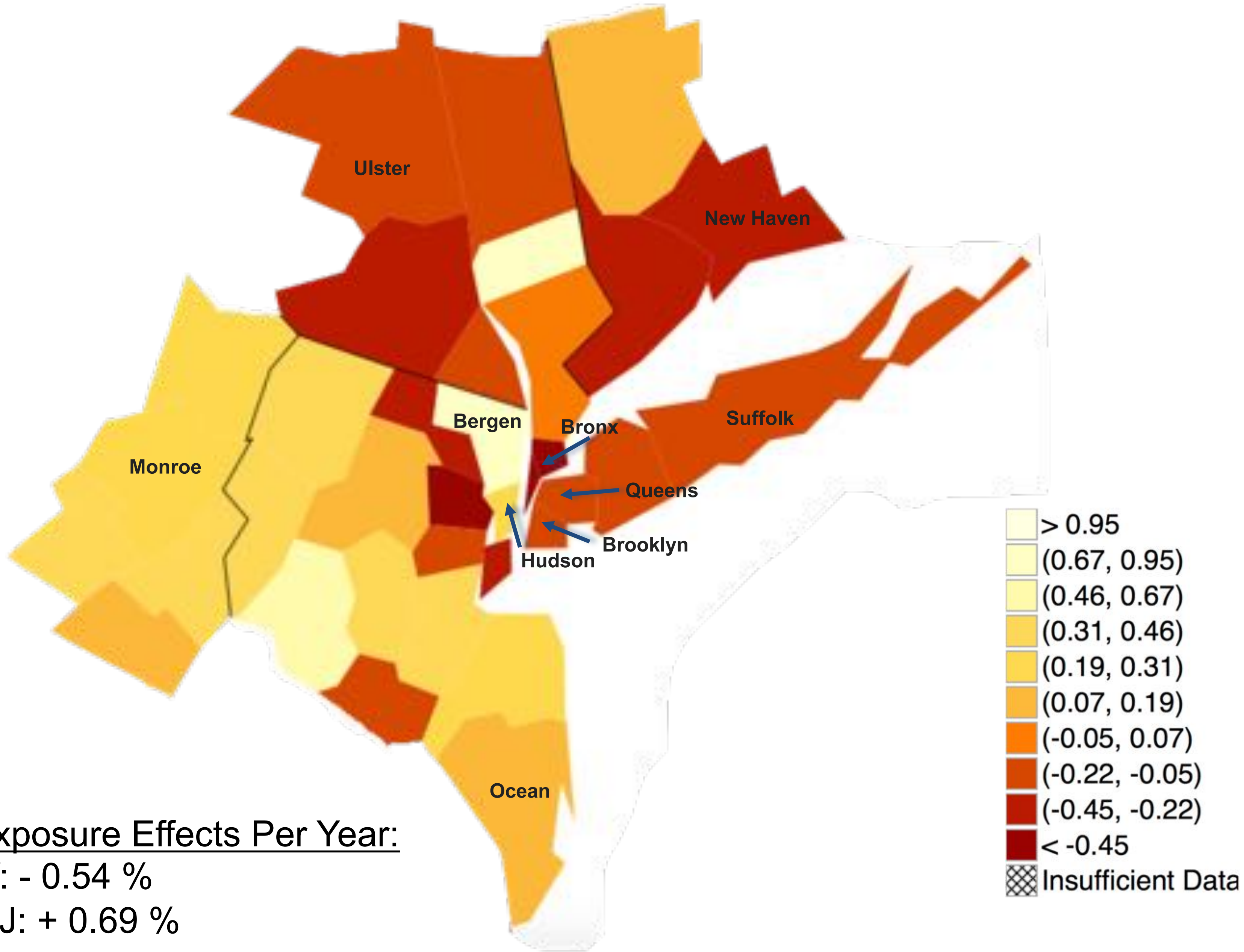


DuPage: +0.76% per year

Cook: -0.67% per year

20 Years of Exposure to DuPage vs. Cook County generates ~30% increase in earnings

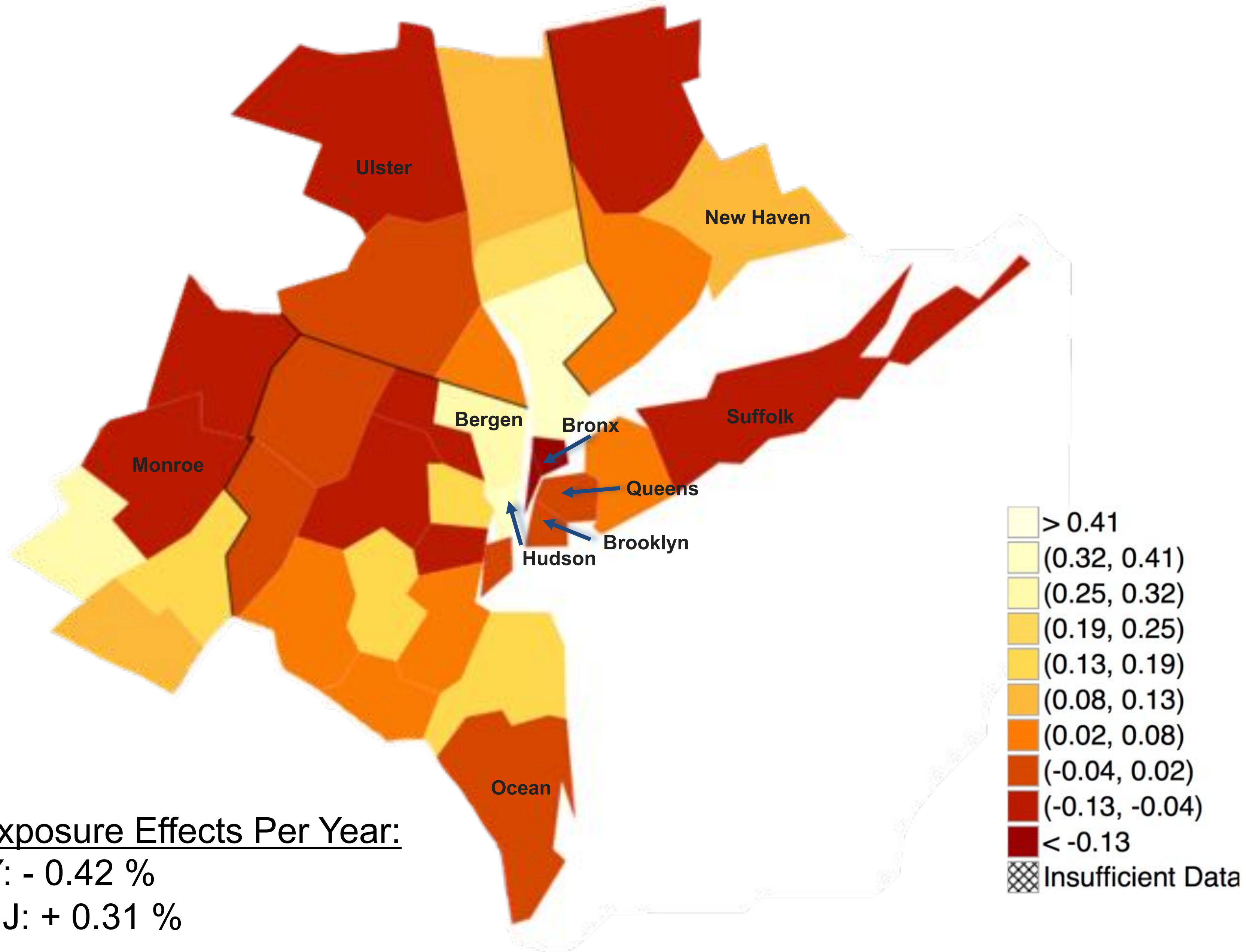
Exposure Effects on Income in the New York CSA
For Children with Parents at 25th Percentile of Income Distribution



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 75th Percentile of Income Distribution



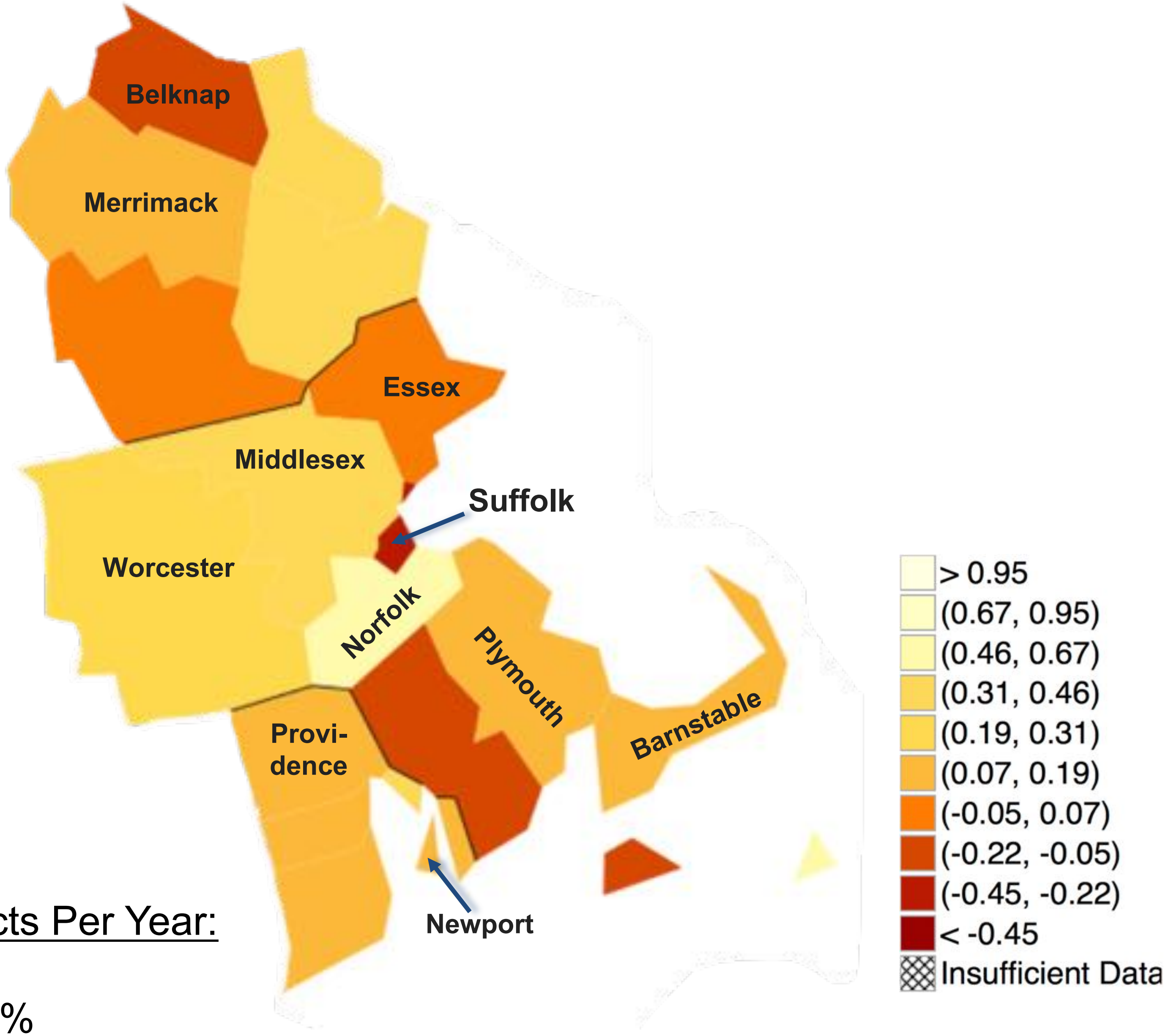
Causal Exposure Effects Per Year:

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



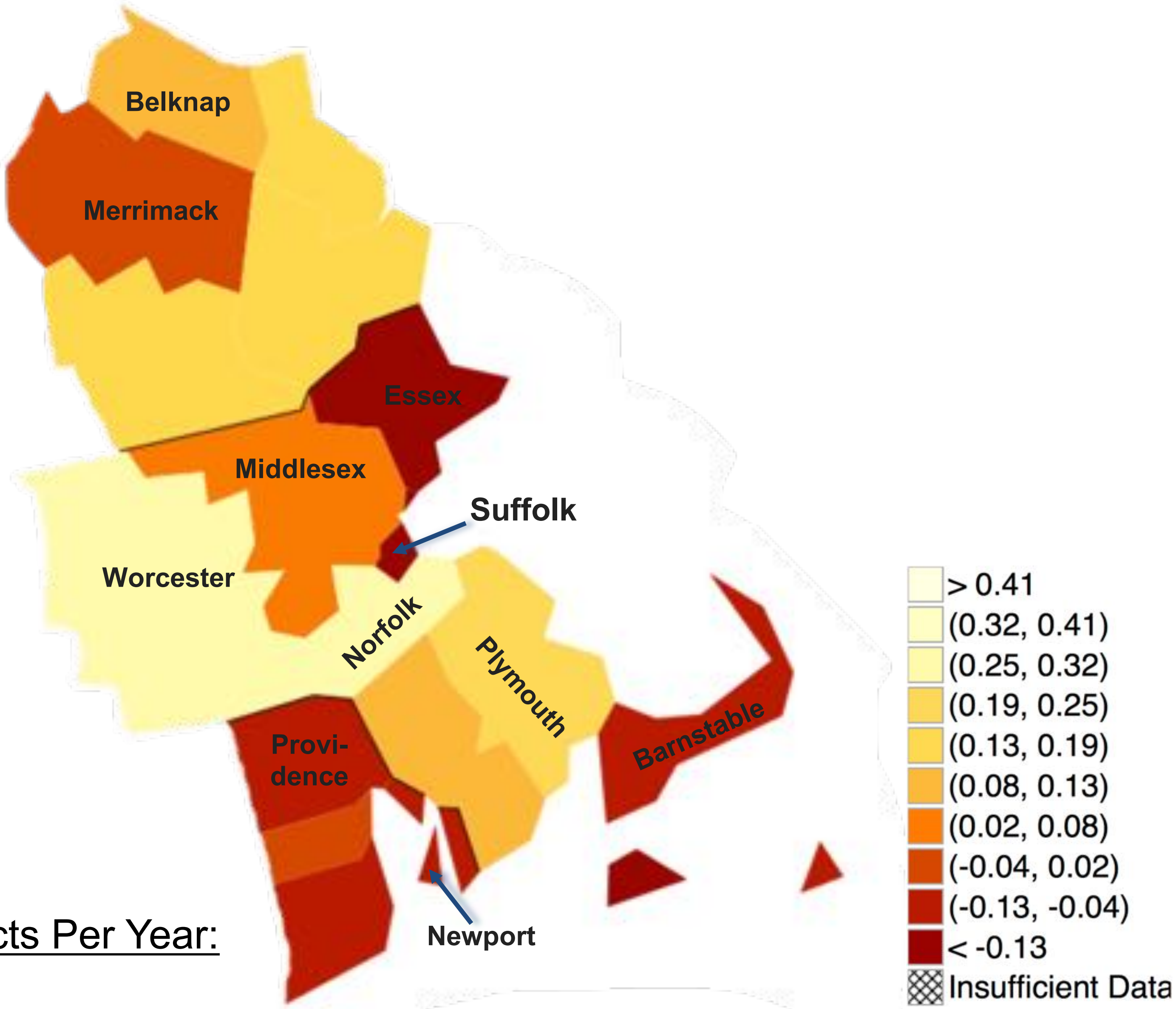
<u>Causal Exposure Effects Per Year:</u>

Suffolk MA: - 0.31 %

Middlesex MA: + 0.39 %

Exposure Effects on Income in the Boston CSA

For Children with Parents at 75th Percentile of Income Distribution



<u>Causal Exposure Effects Per Year:</u>

Suffolk MA: - 0.18 %

Middlesex MA: + 0.03 %

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.

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

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.

Top 10 Counties			Bottom 10 Counties		
Rank	County	Annual Exposure Effect (%)	Rank	County	Annual Exposure Effect (%)
1	Fairfax, VA	0.55	91	Hillsborough, FL	-0.40
2	Westchester, NY	0.34	92	Bronx, NY	-0.42
3	Hudson, NJ	0.33	93	Broward, FL	-0.46
4	Hamilton, OH	0.32	94	Dist. of Columbia, DC	-0.48
5	Bergen, NJ	0.31	95	Orange, CA	-0.49
6	Gwinnett, GA	0.31	96	San Bernardino, CA	-0.51
7	Norfolk, MA	0.31	97	Riverside, CA	-0.51
8	Worcester, MA	0.27	98	Los Angeles, CA	-0.52
9	Franklin, OH	0.24	99	New York, NY	-0.57
10	Kent, MI	0.23	100	Palm Beach, FL	-0.65

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

Top 10 Counties			Bottom 10 Counties		
Rank	County	Annual Exposure Effect (%)	Rank	County	Annual Exposure Effect (%)
1	Bucks, PA	0.84	91	Milwaukee, WI	-0.74
2	Bergen, NJ	0.83	92	New Haven, CT	-0.75
3	Contra Costa, CA	0.72	93	Bronx, NY	-0.76
4	Snohomish, WA	0.70	94	Hillsborough, FL	-0.81
5	Norfolk, MA	0.62	95	Palm Beach, FL	-0.82
6	Dupage, IL	0.61	96	Fresno, CA	-0.84
7	King, WA	0.56	97	Riverside, CA	-0.85
8	Ventura, CA	0.55	98	Wayne, MI	-0.87
9	Hudson, NJ	0.52	99	Pima, AZ	-1.15
10	Fairfax, VA	0.46	100	Baltimore City, MD	-1.39

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)

Female Children

Top 10 Counties			Bottom 10 Counties		
Rank	County	Annual Exposure Effect (%)	Rank	County	Annual Exposure Effect (%)
1	Dupage, IL	0.91	91	Hillsborough, FL	-0.51
2	Fairfax, VA	0.76	92	Fulton, GA	-0.58
3	Snohomish, WA	0.73	93	Suffolk, MA	-0.58
4	Montgomery, MD	0.68	94	Orange, FL	-0.60
5	Montgomery, PA	0.58	95	Essex, NJ	-0.64
6	King, WA	0.57	96	Cook, IL	-0.64
7	Bergen, NJ	0.56	97	Franklin, OH	-0.64
8	Salt Lake, UT	0.51	98	Mecklenburg, NC	-0.74
9	Contra Costa, CA	0.47	99	New York, NY	-0.75
10	Middlesex, NJ	0.47	100	Marion, IN	-0.77

Exposure effects represent % change in adult earnings per year of childhood spent in county