

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)

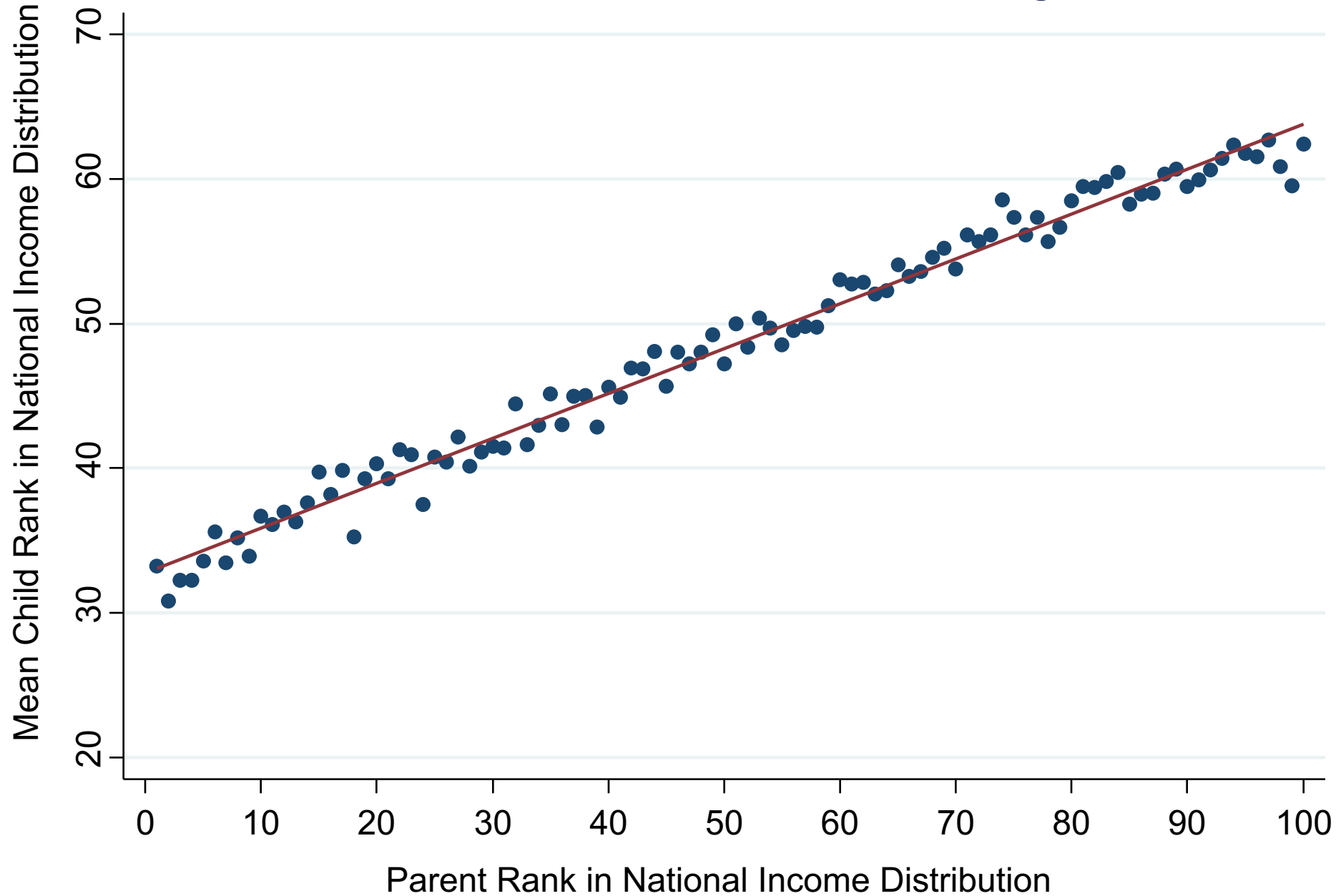
Children versus Adults

- 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
- Growing literature: impact of place on children
- Human capital production is local; labor markets are global?

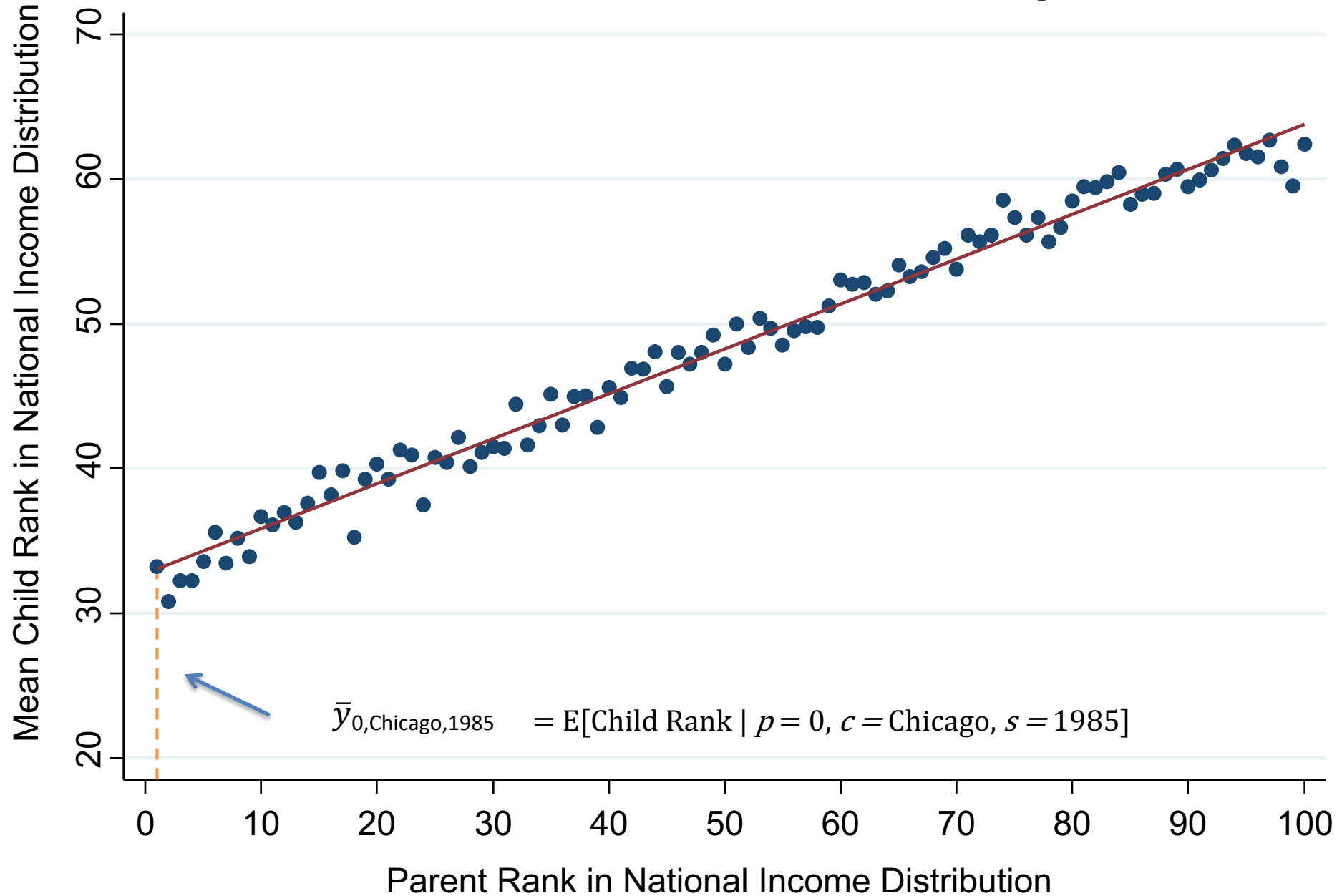
Chetty, Hendren, Friedman, Jones, Porter (2018)

- 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

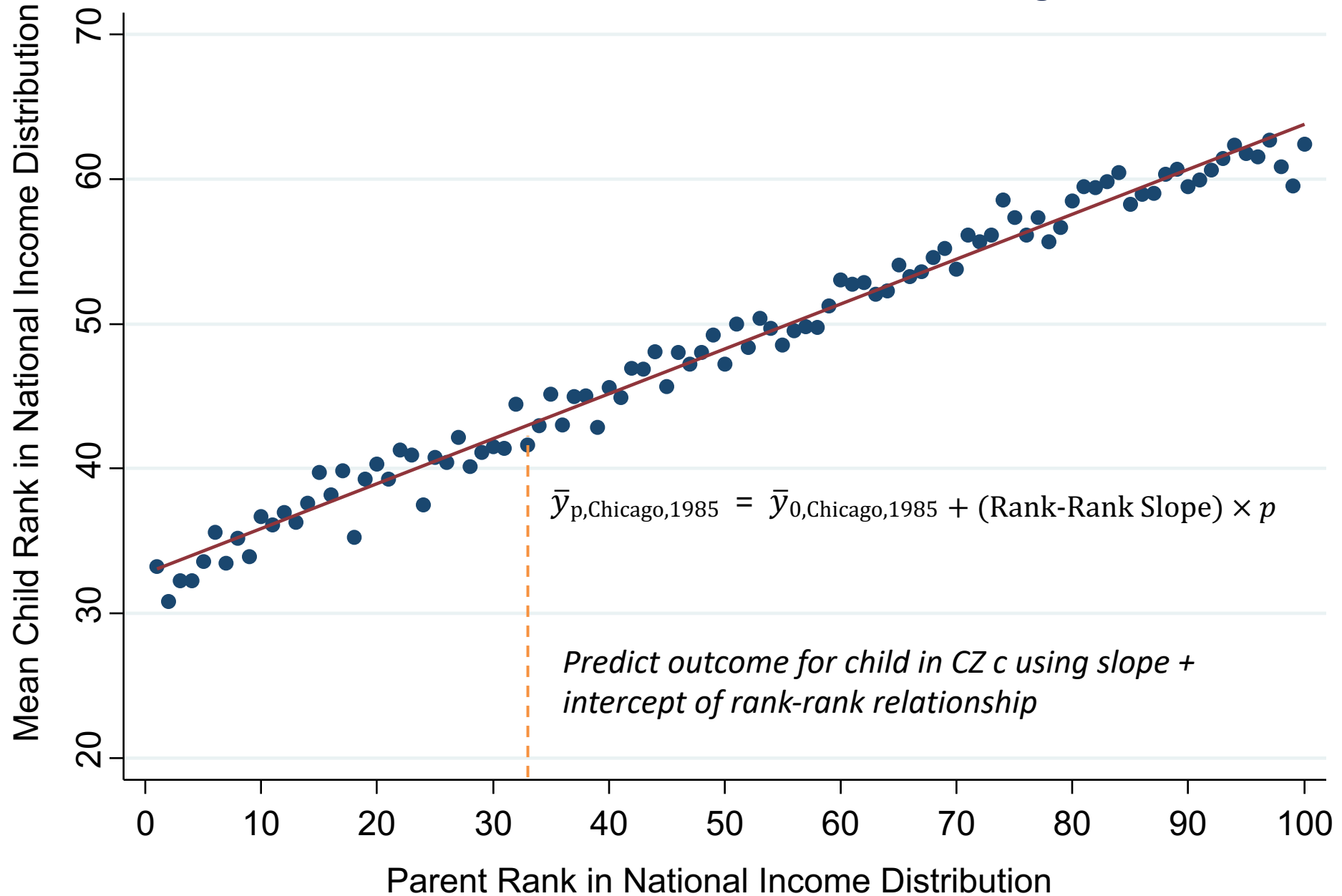
**Mean Child Income Rank at Age 26 vs. Parent Income Rank
for Children Born in 1985 and Raised in Chicago**



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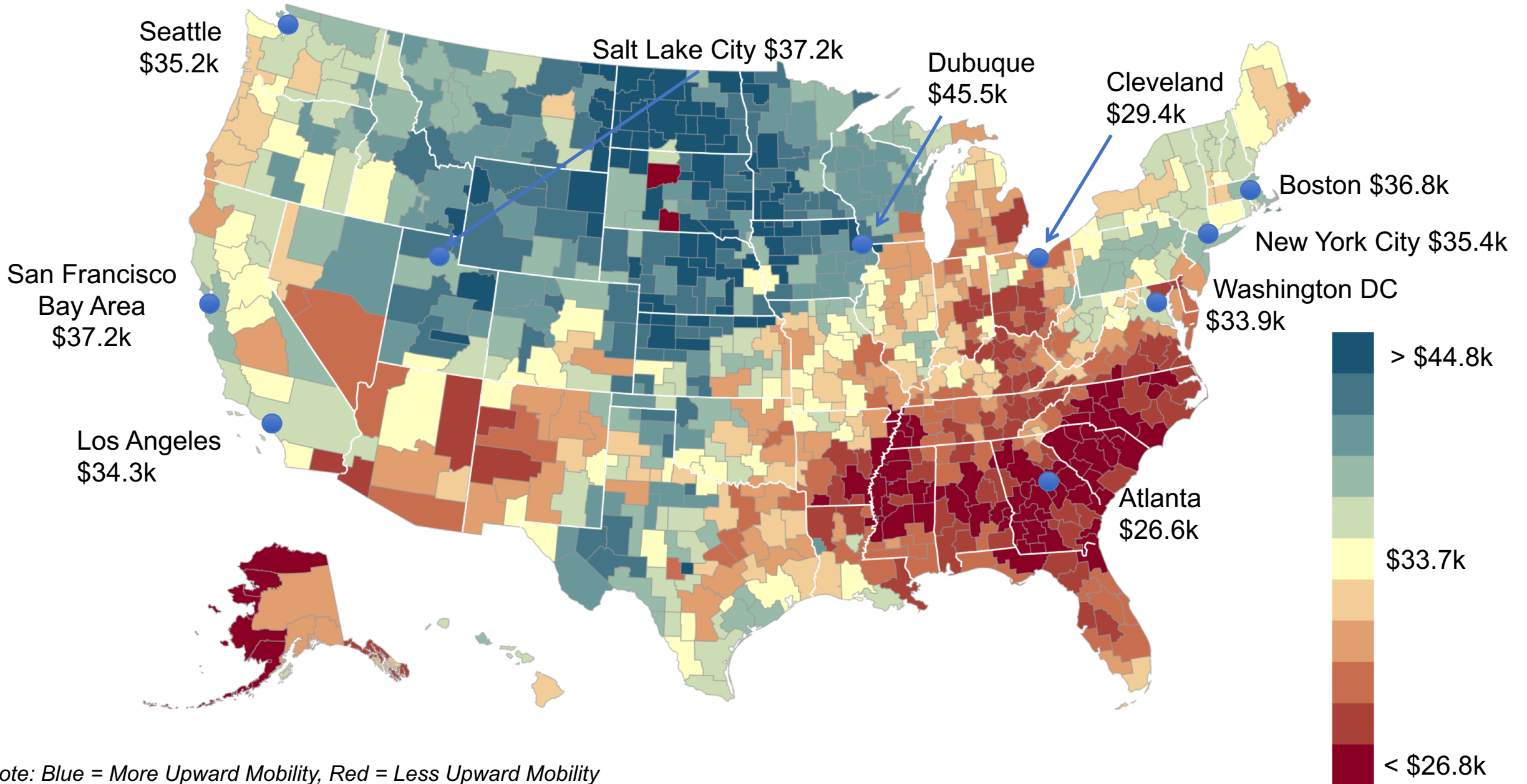


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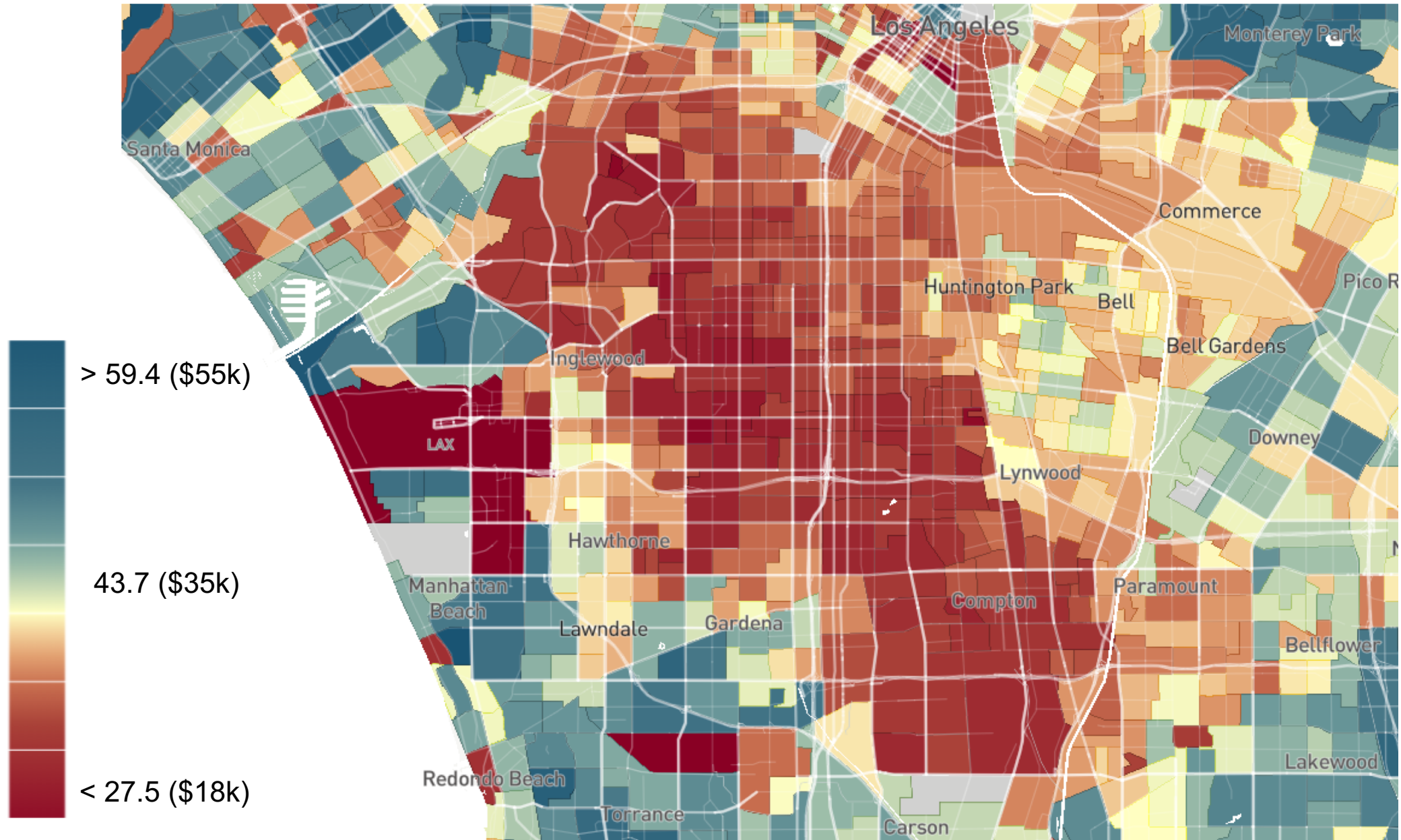


The Geography of Upward Mobility in the United States

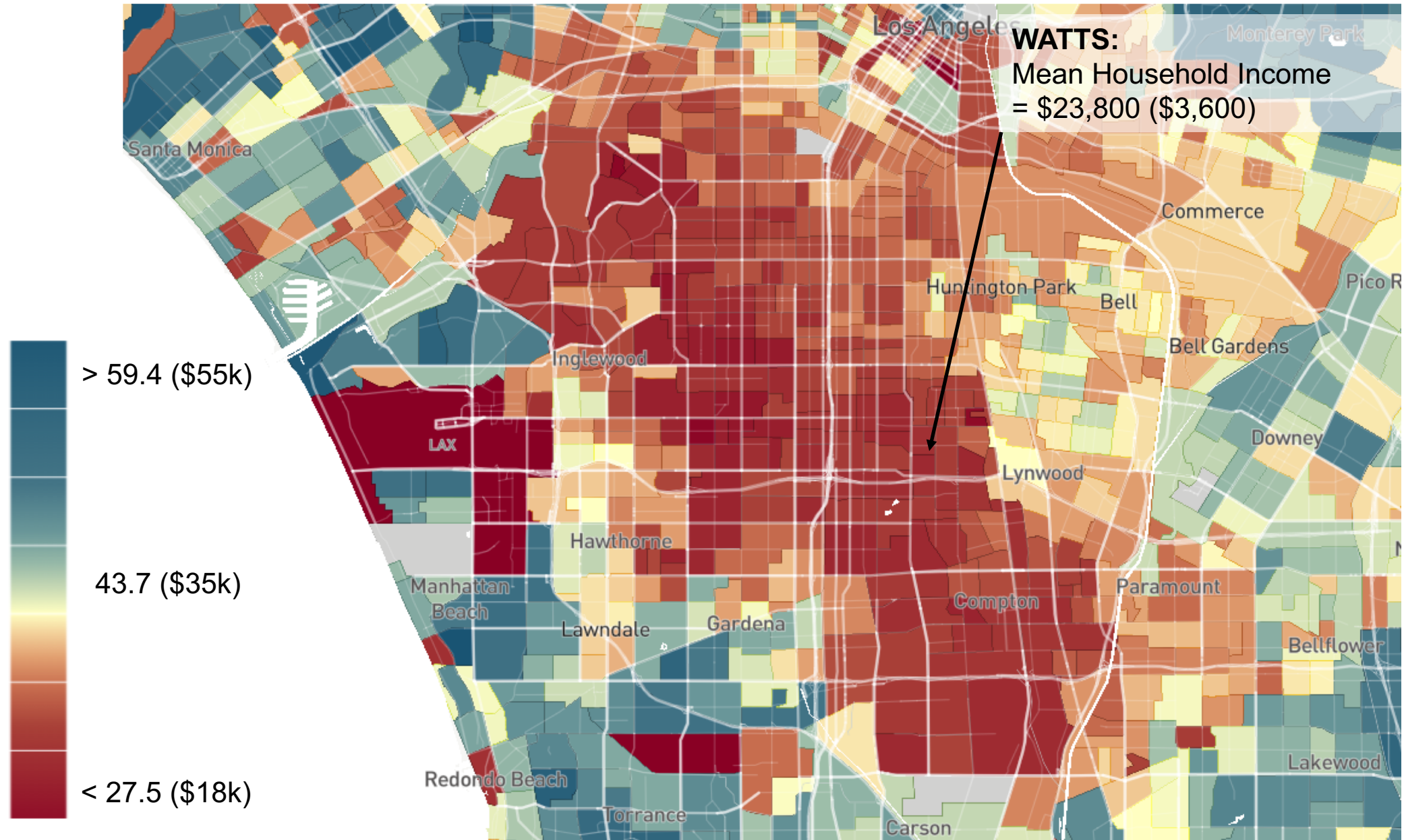
Average Household Income for Children with Parents Earning \$27,000 (25th percentile)



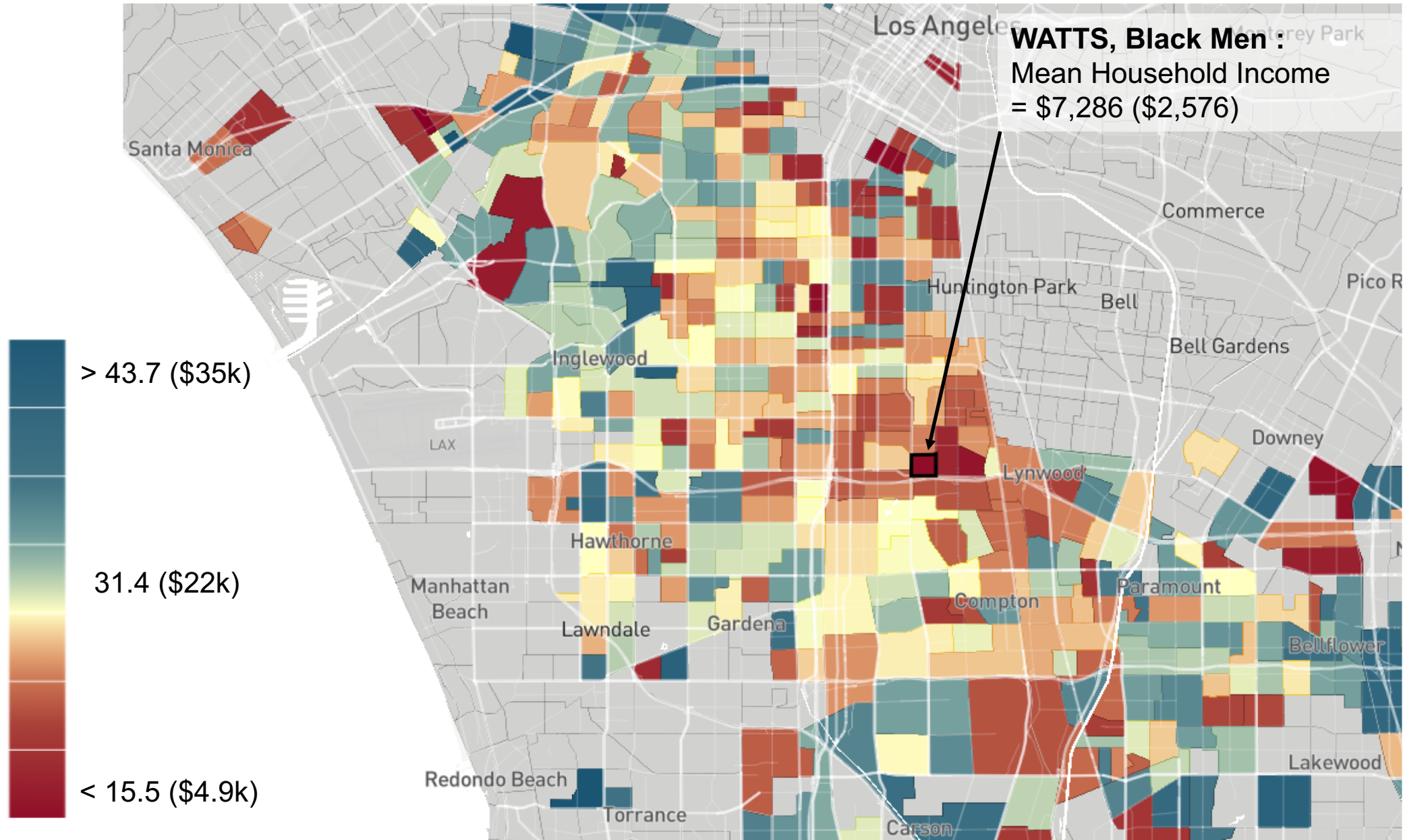
Mean Household Income for Children in Los Angeles with Parents Earning \$27,000 (25th percentile)



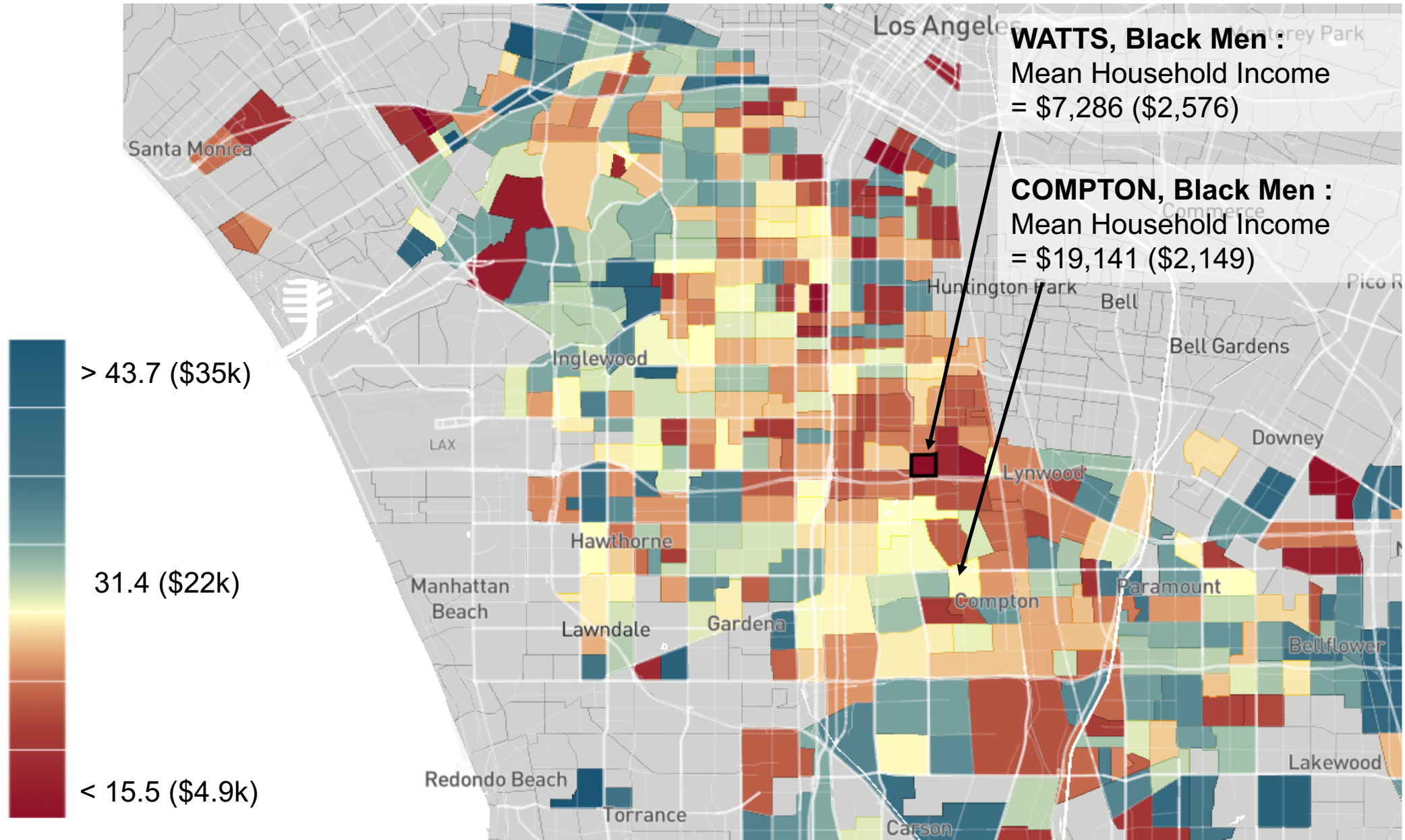
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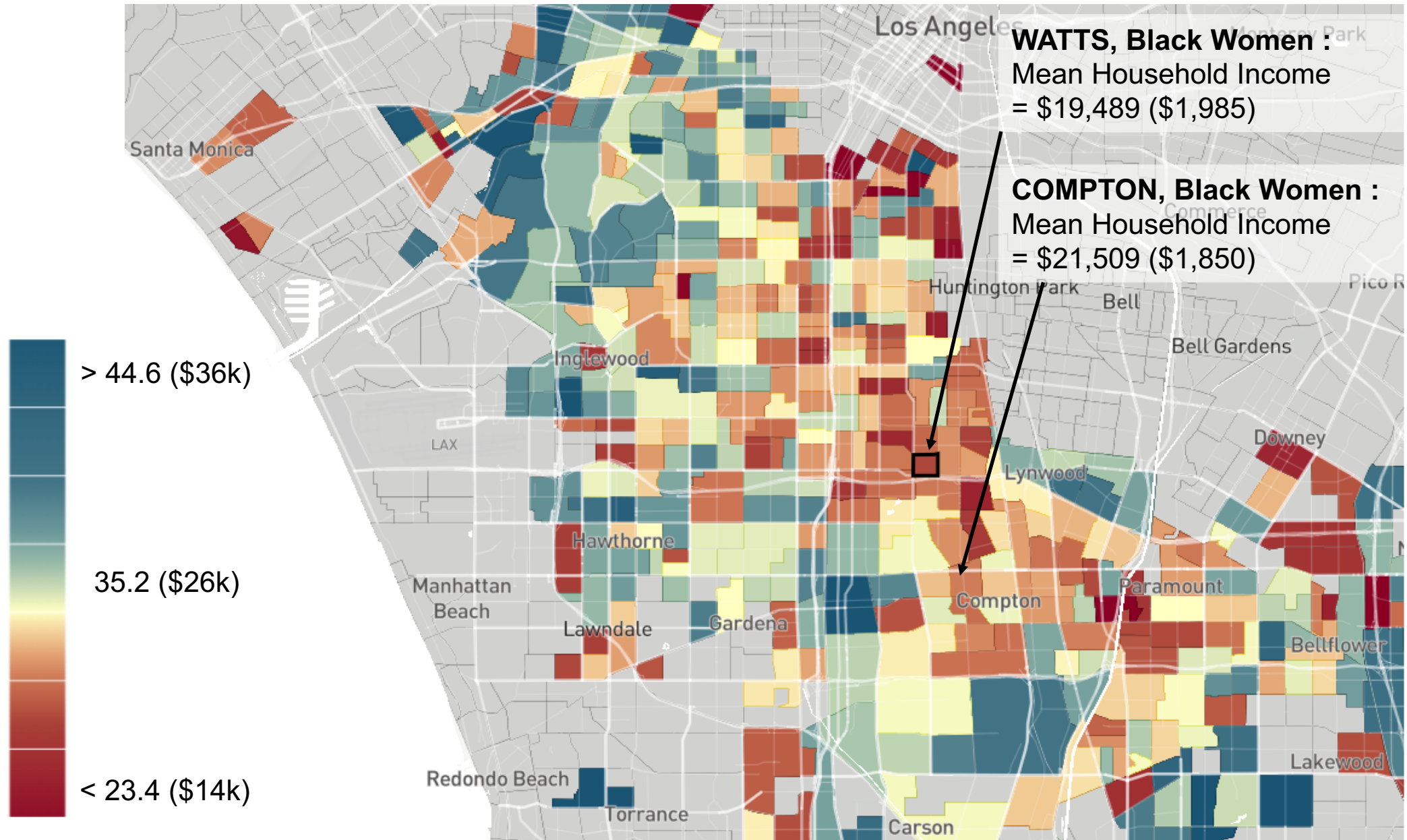
Mean Household Income for Black Men in Los Angeles with Parents Earning \$27,000 (25th percentile)



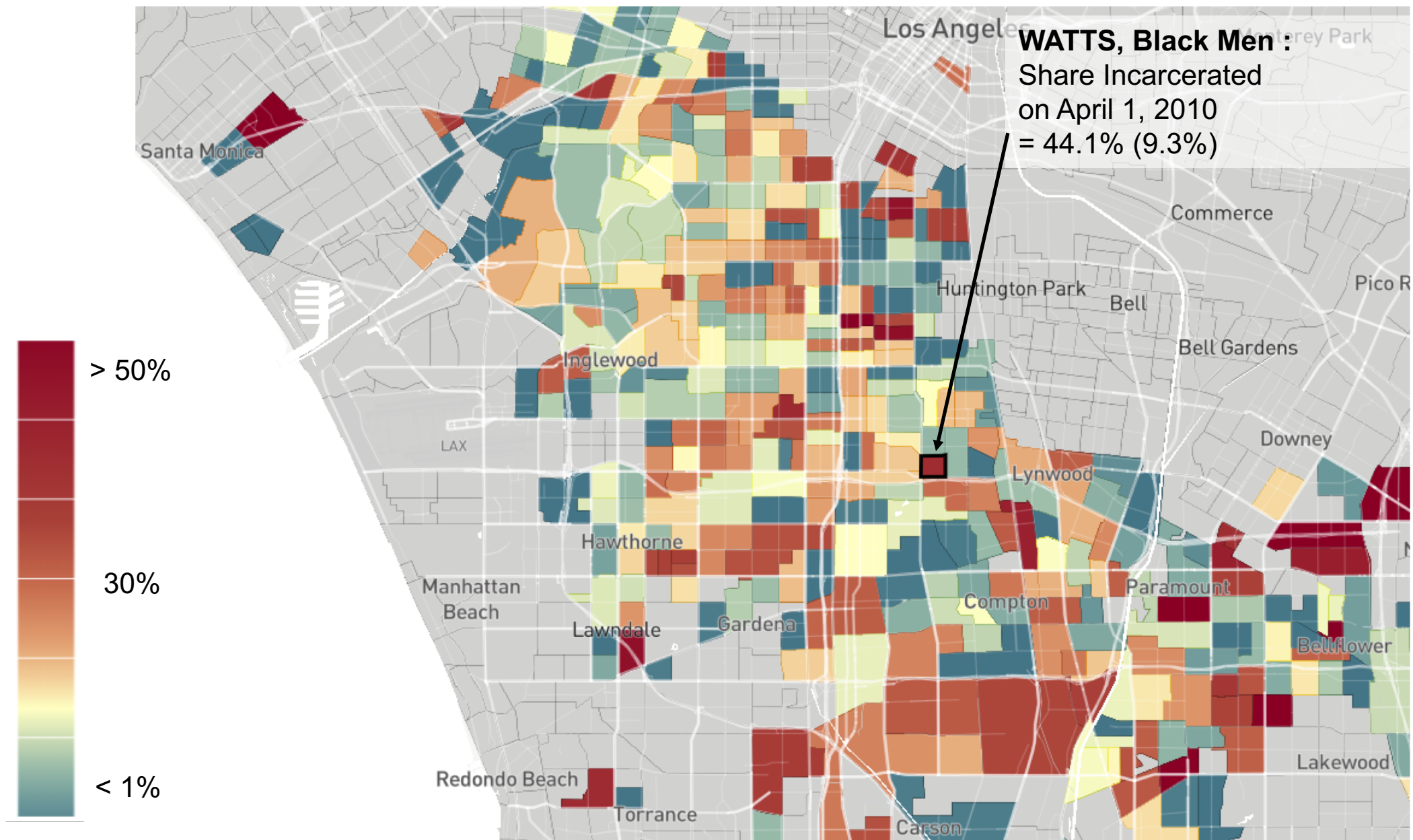
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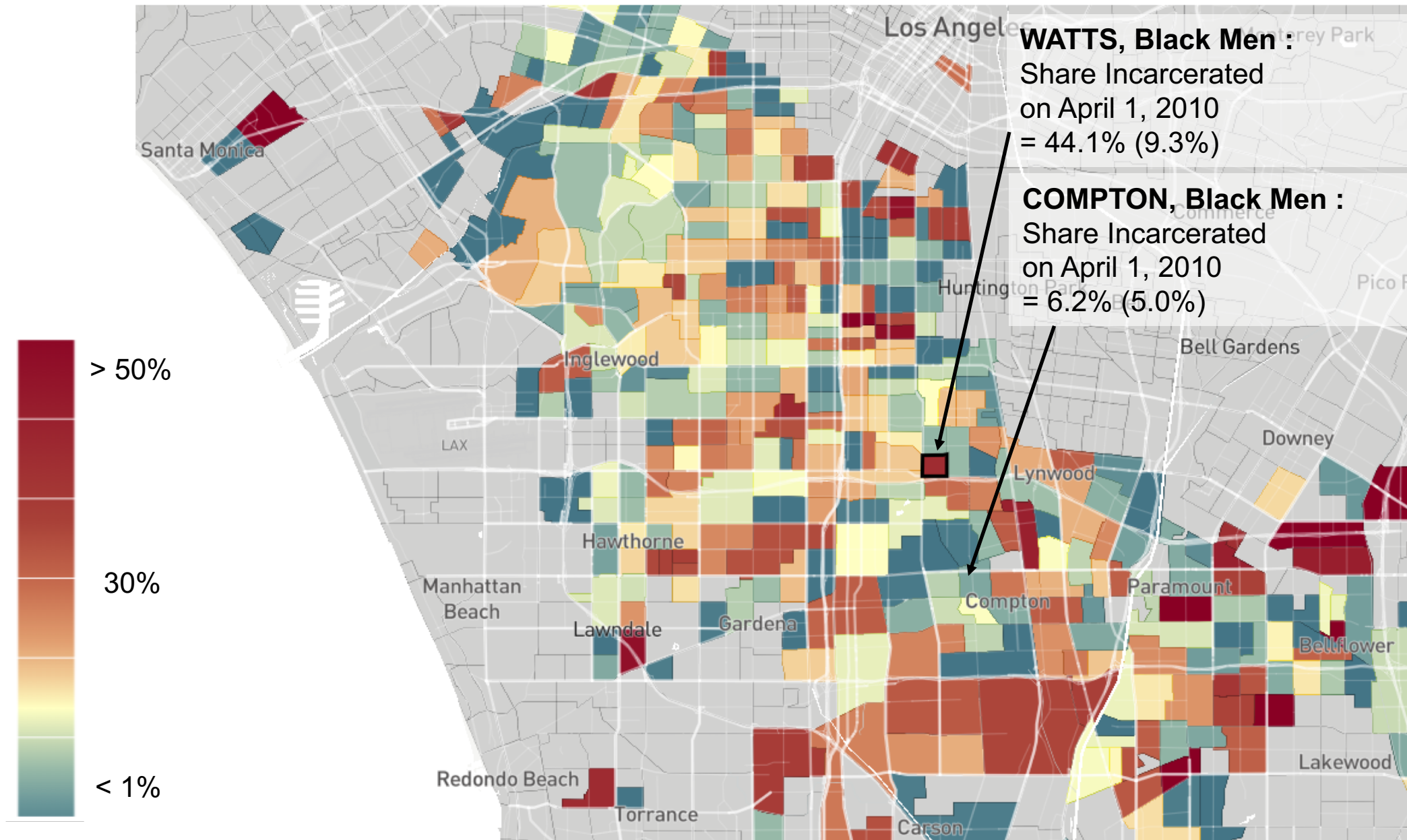
Mean Individual Income for Black Women in Los Angeles with Parents Earning \$27,000 (25th percentile)



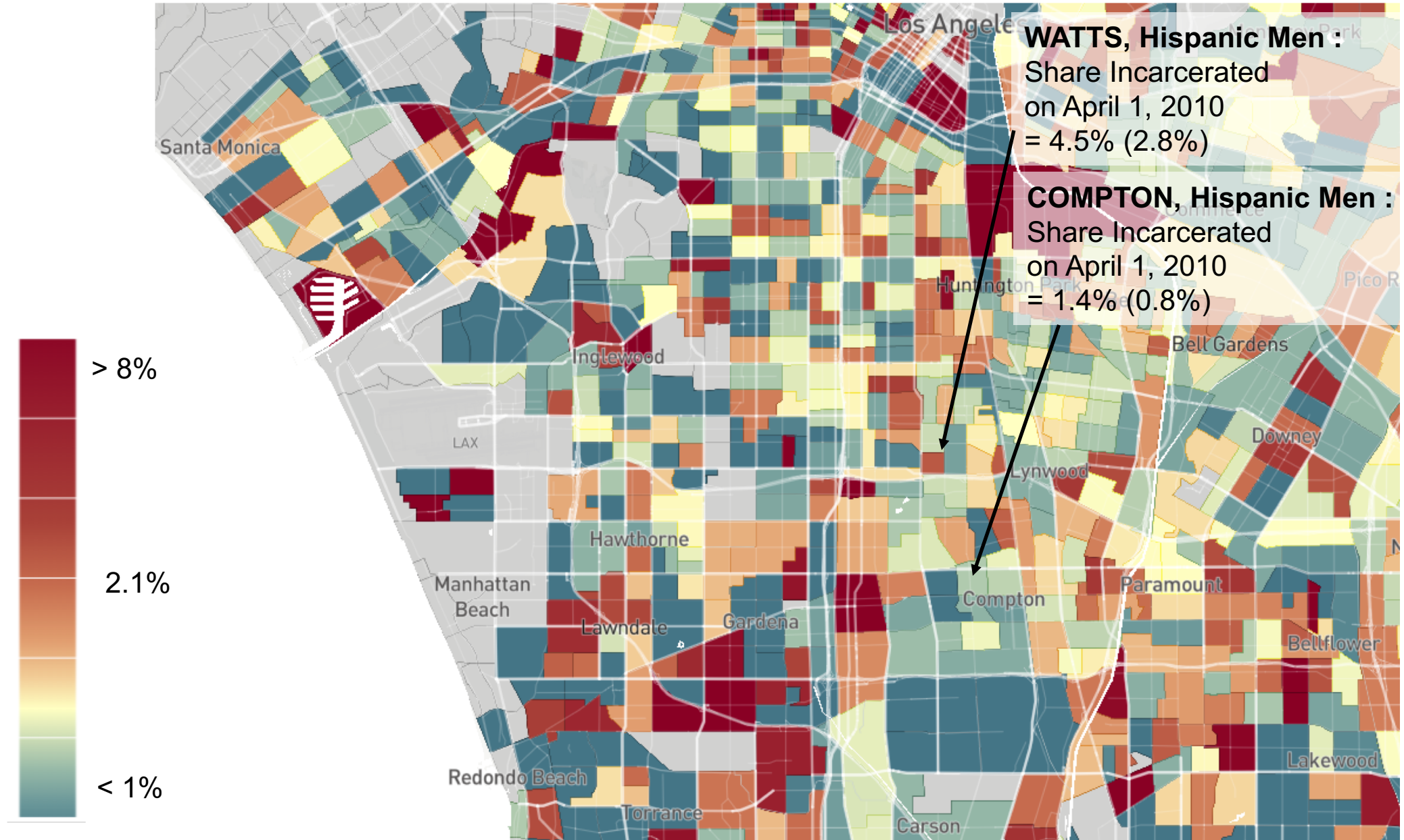
Incarceration Rates for Black Men in Los Angeles with Parents Earning < \$2,200 (1st percentile)



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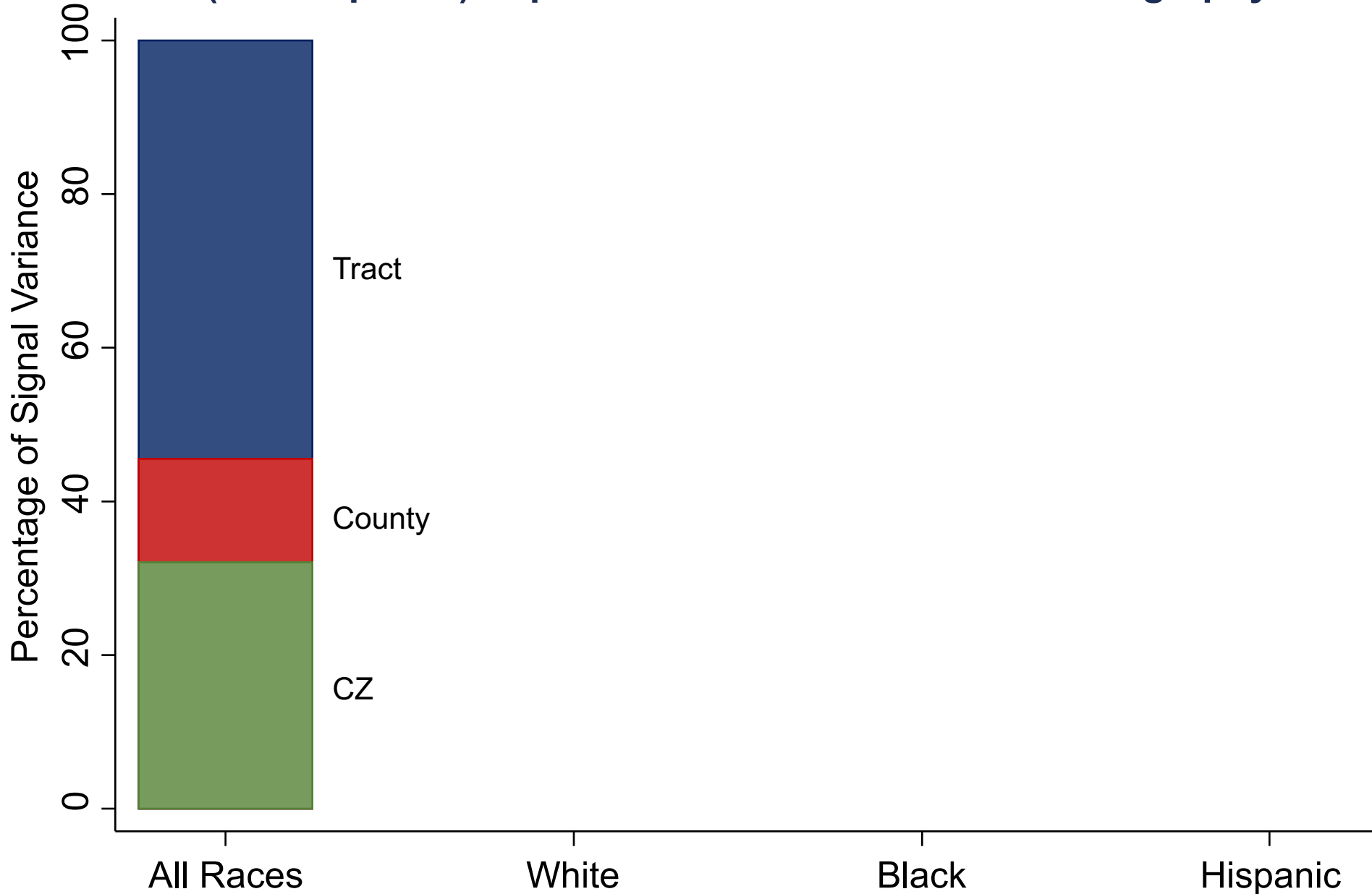
Incarceration Rates for Hispanic Men in Los Angeles with Parents Earning < \$2,200 (1st percentile)



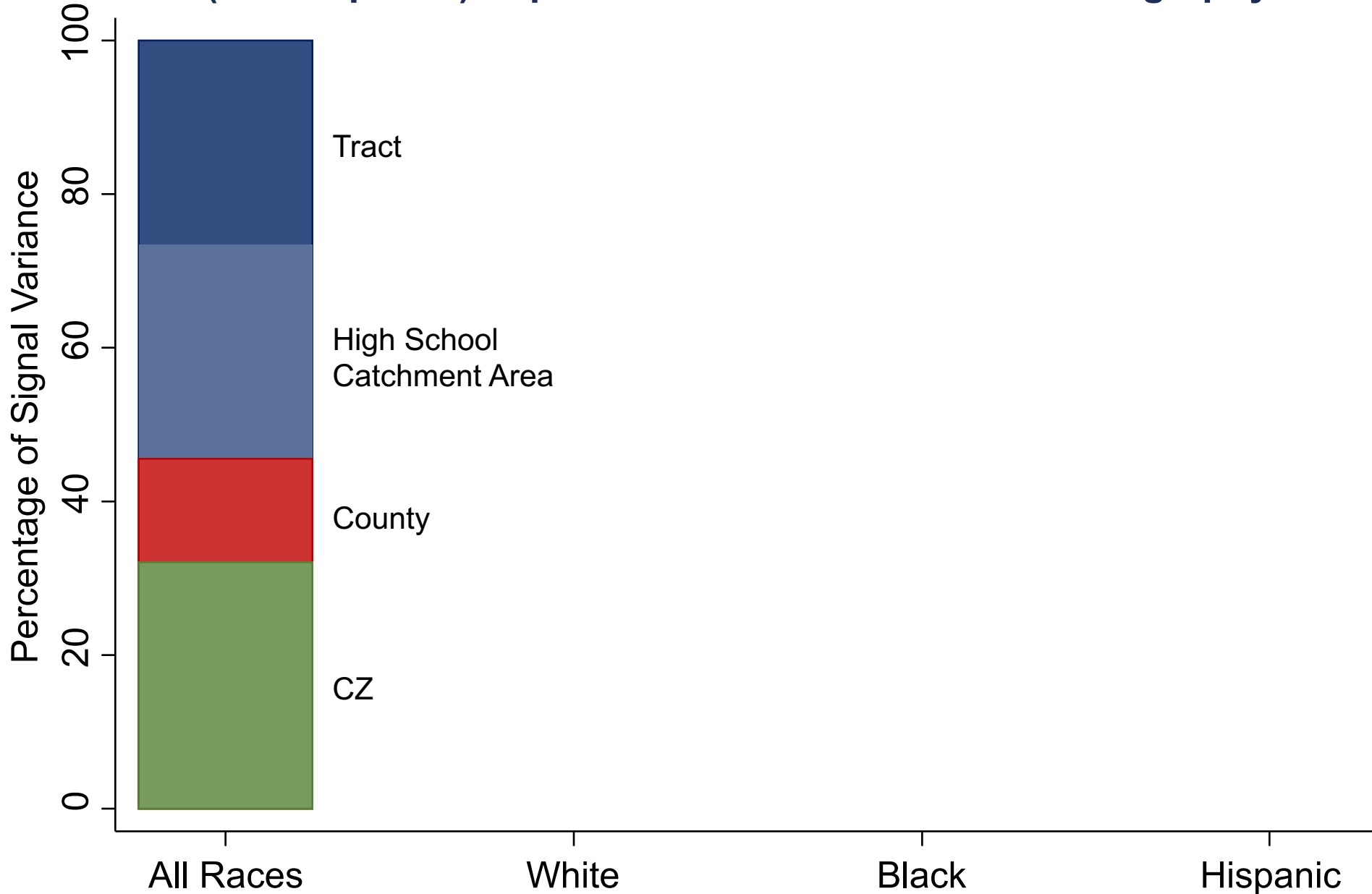
Targeting Place-Based Policies

- 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?

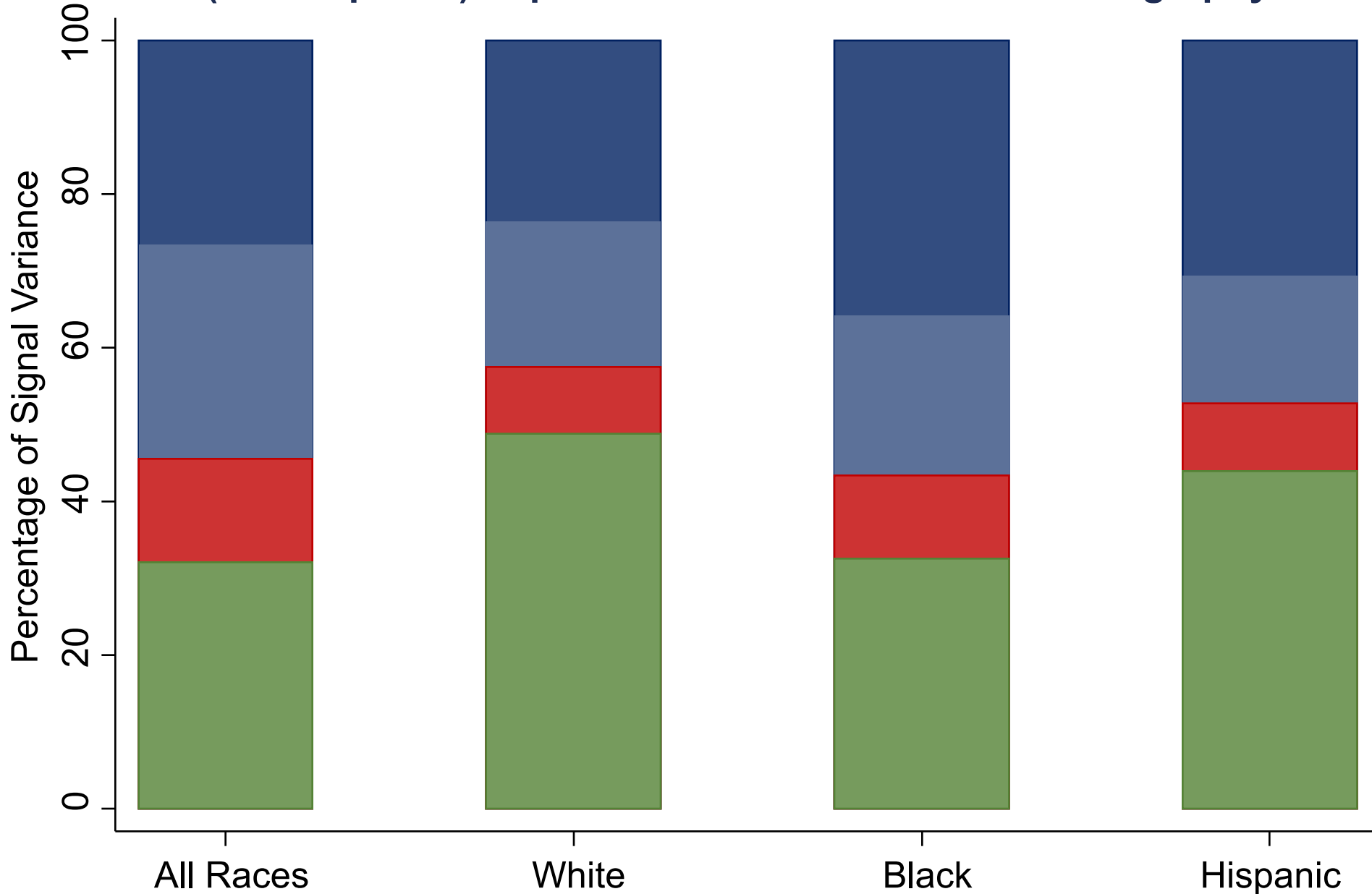
Share of Signal Variance of Tract-Level Mean Child Income Rank (Parent $p = 25$) Explained at Different Levels of Geography



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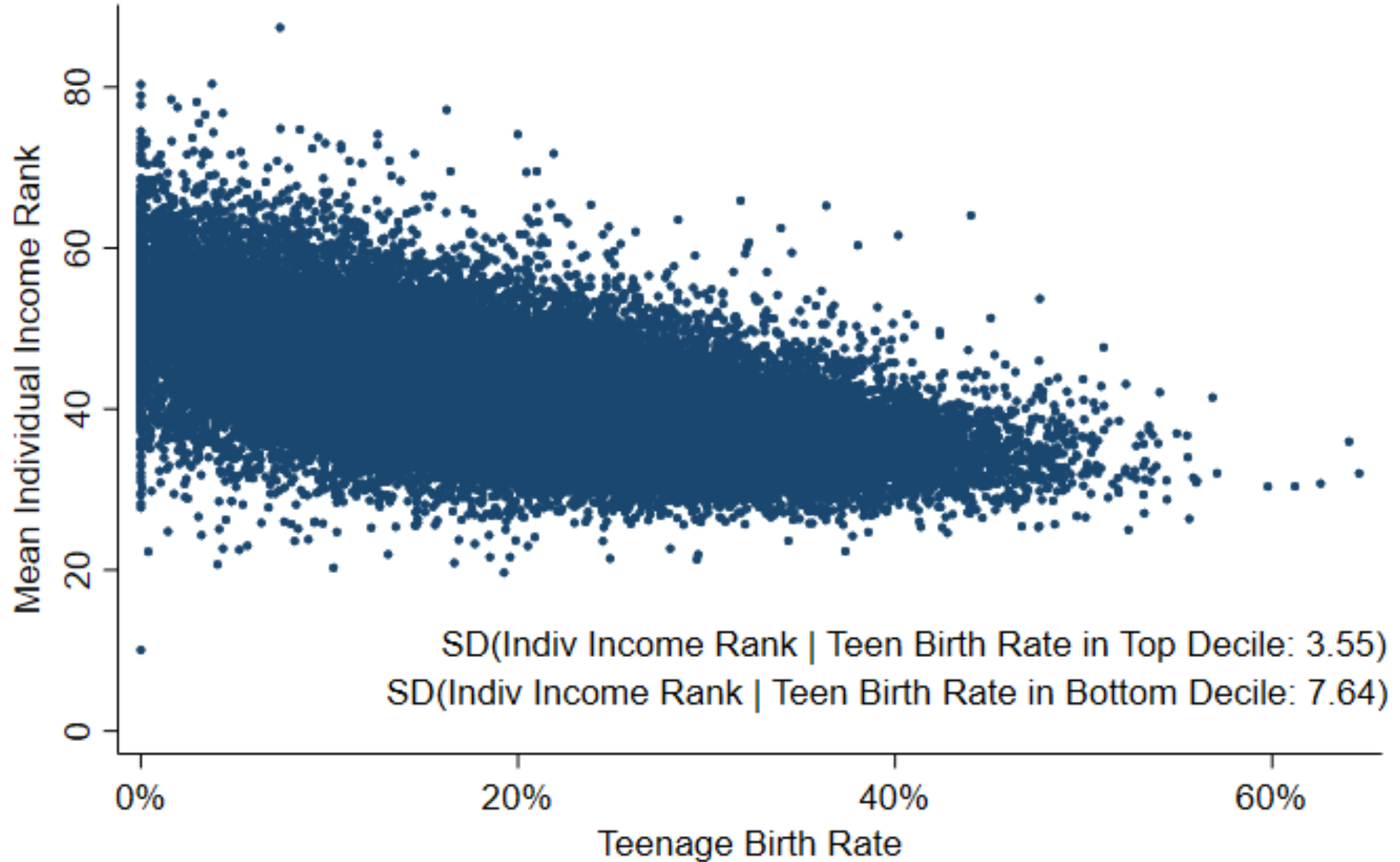


Targeting Place-Based Policies

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 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

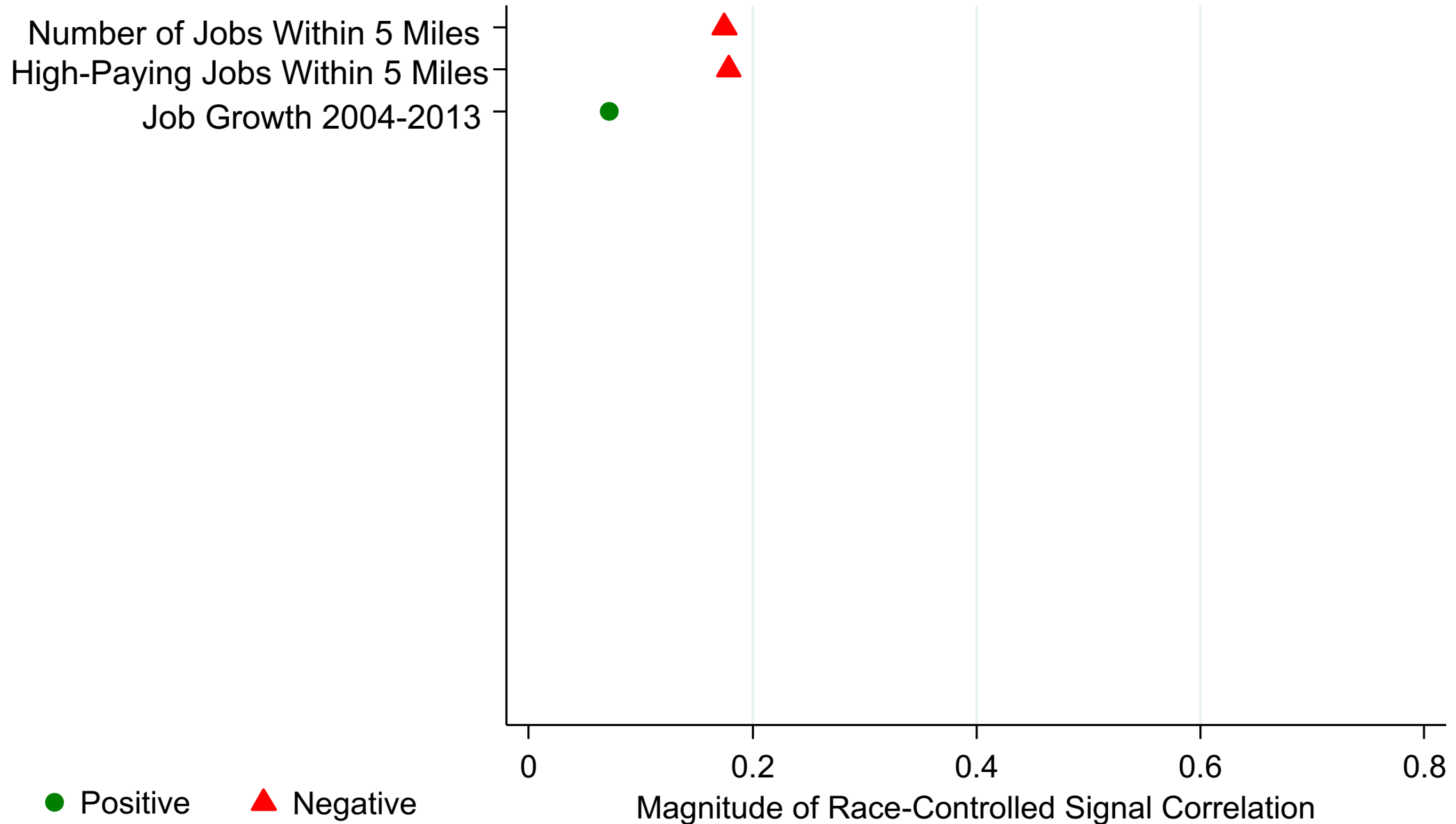


Targeting Place-Based Policies

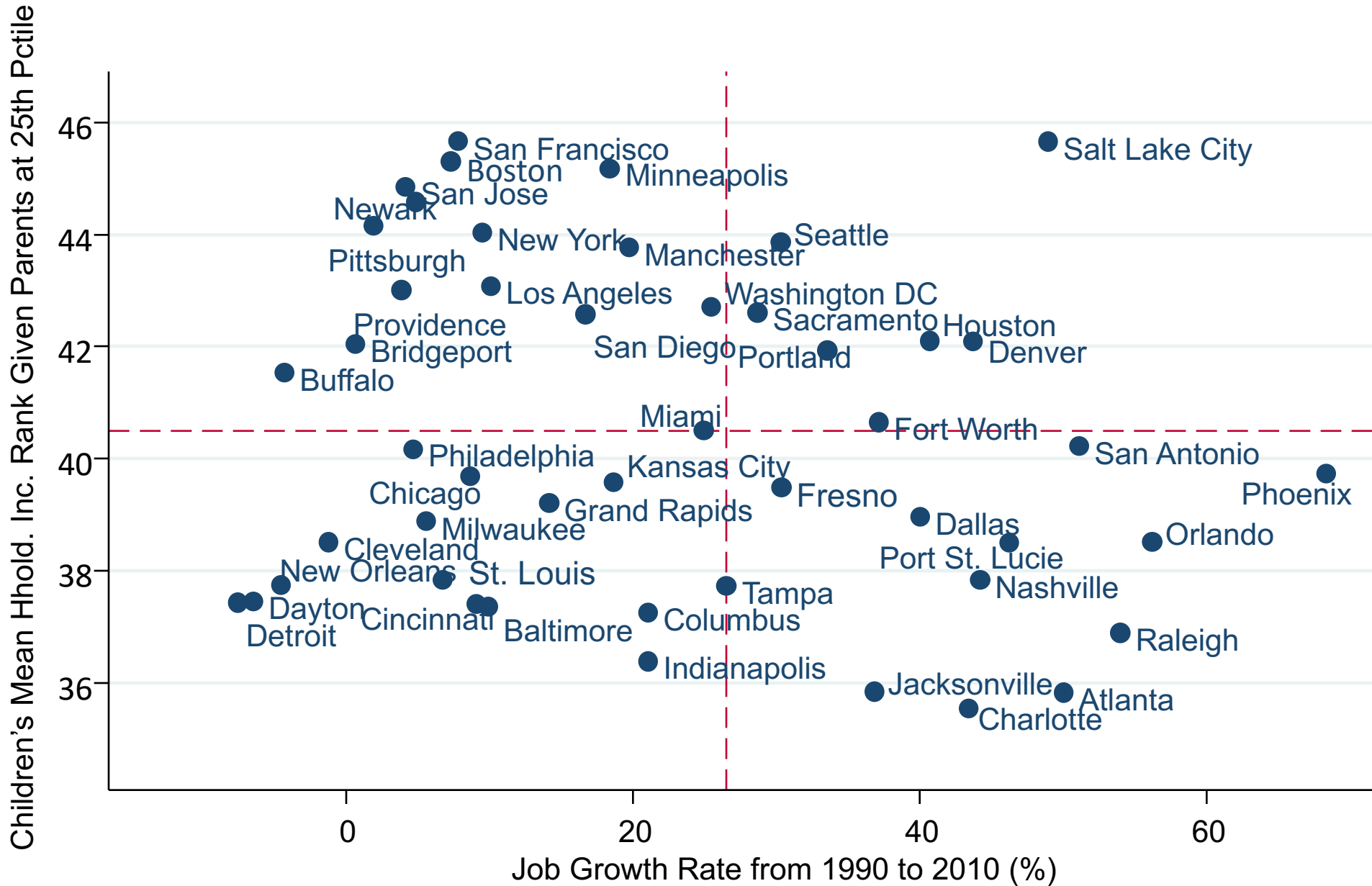
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 1. Children's outcomes vary widely across nearby tracts → location where children grow up is a useful tag for policy interventions
 2. Substantial heterogeneity *within* areas across subgroups and outcomes cond. on parent income → neighborhoods not 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

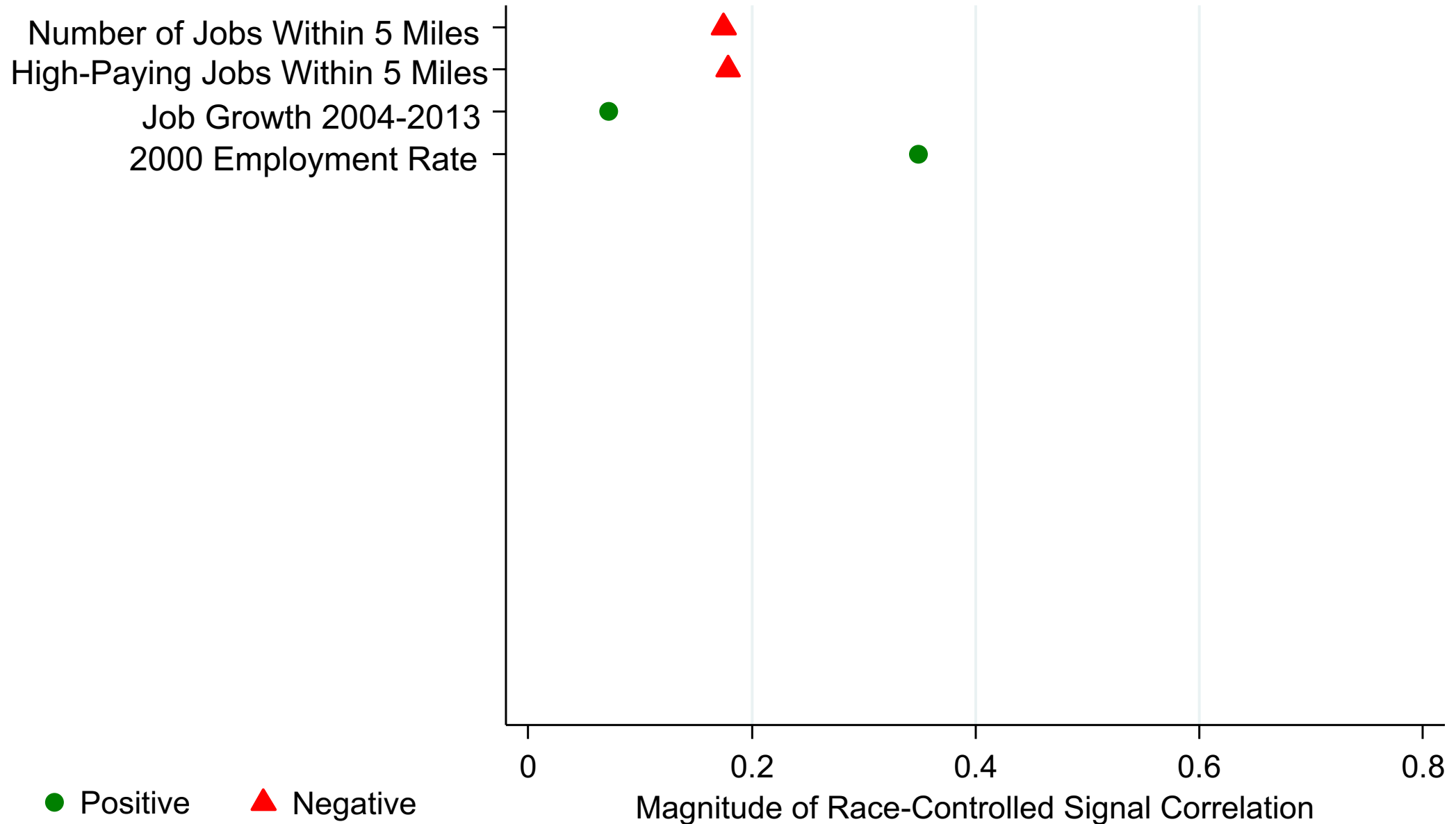


Upward Mobility vs. Job Growth in the 50 Largest Commuting Zones



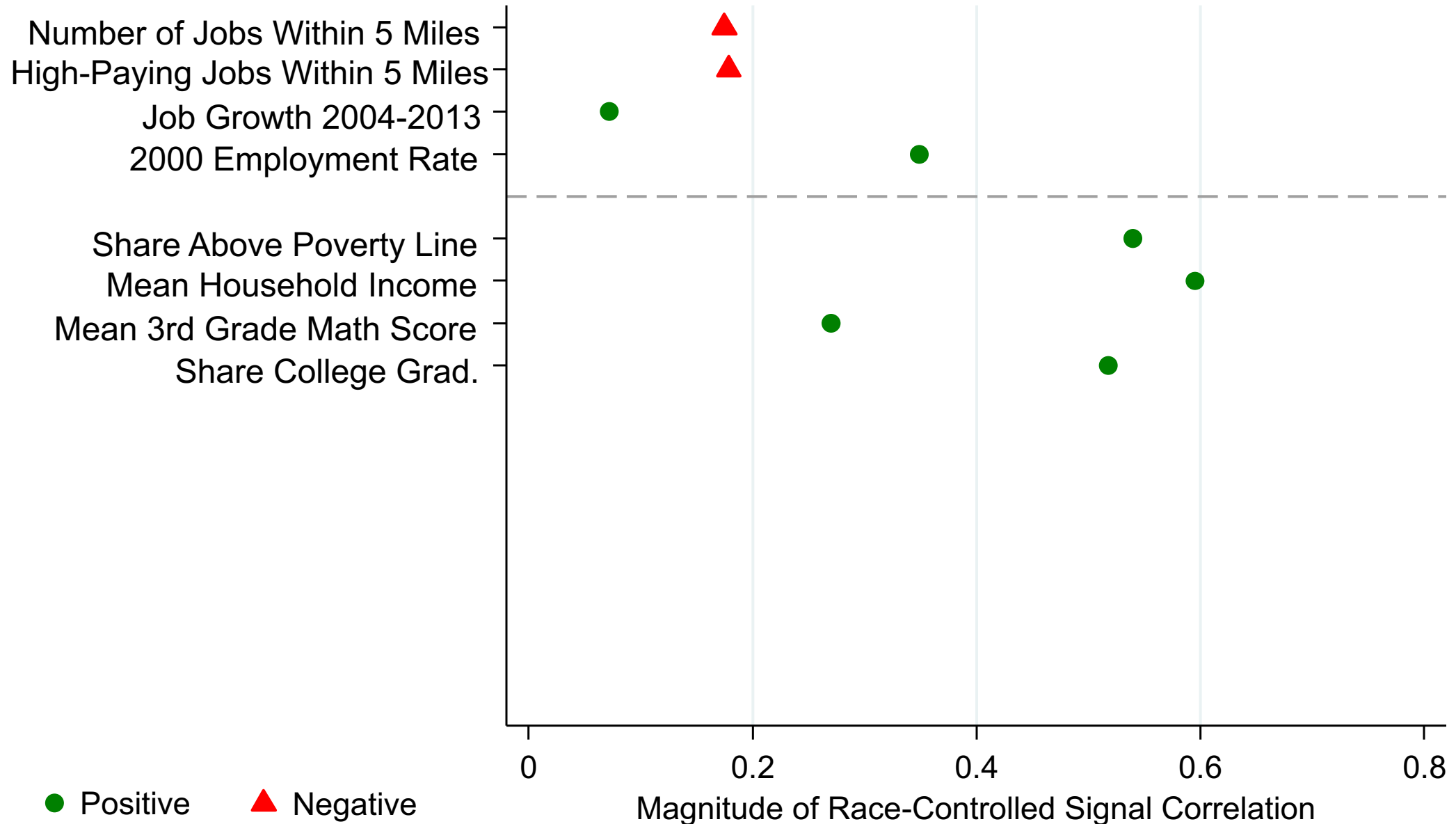
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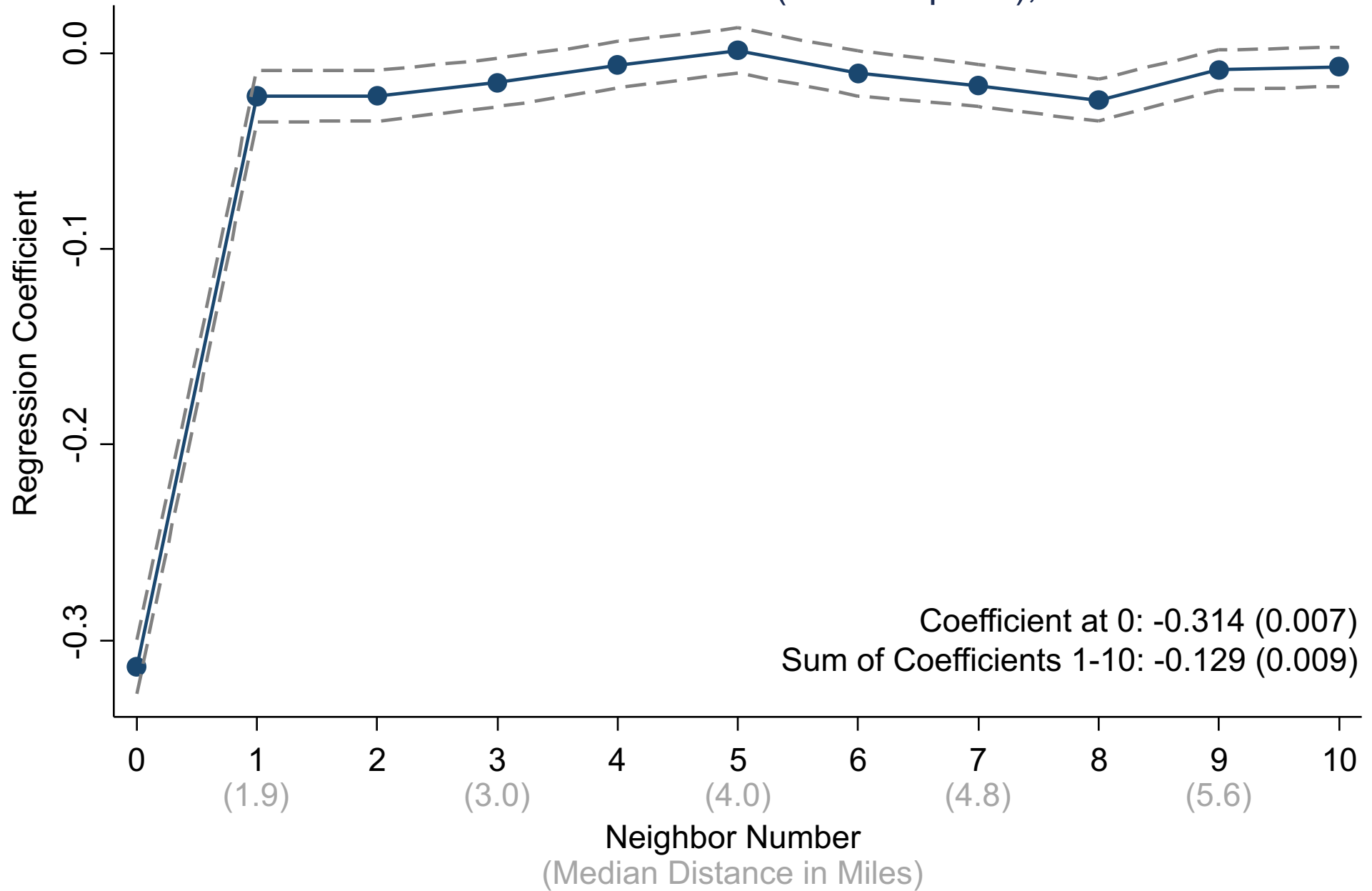
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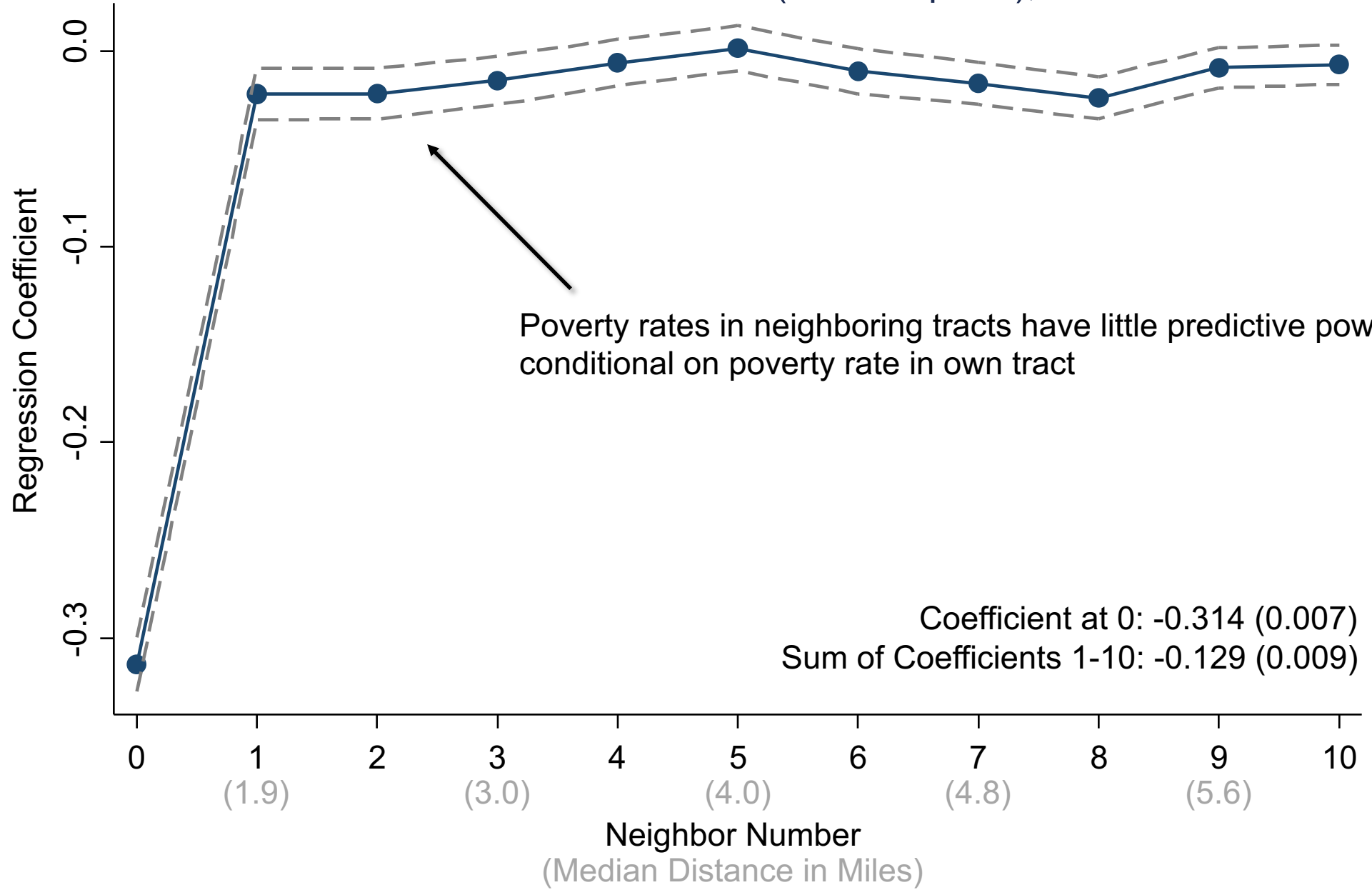
Spatial Decay of Correlation with Tract-Level Poverty Rate

Mean Child Household Income Rank (Parents p=25), White Children



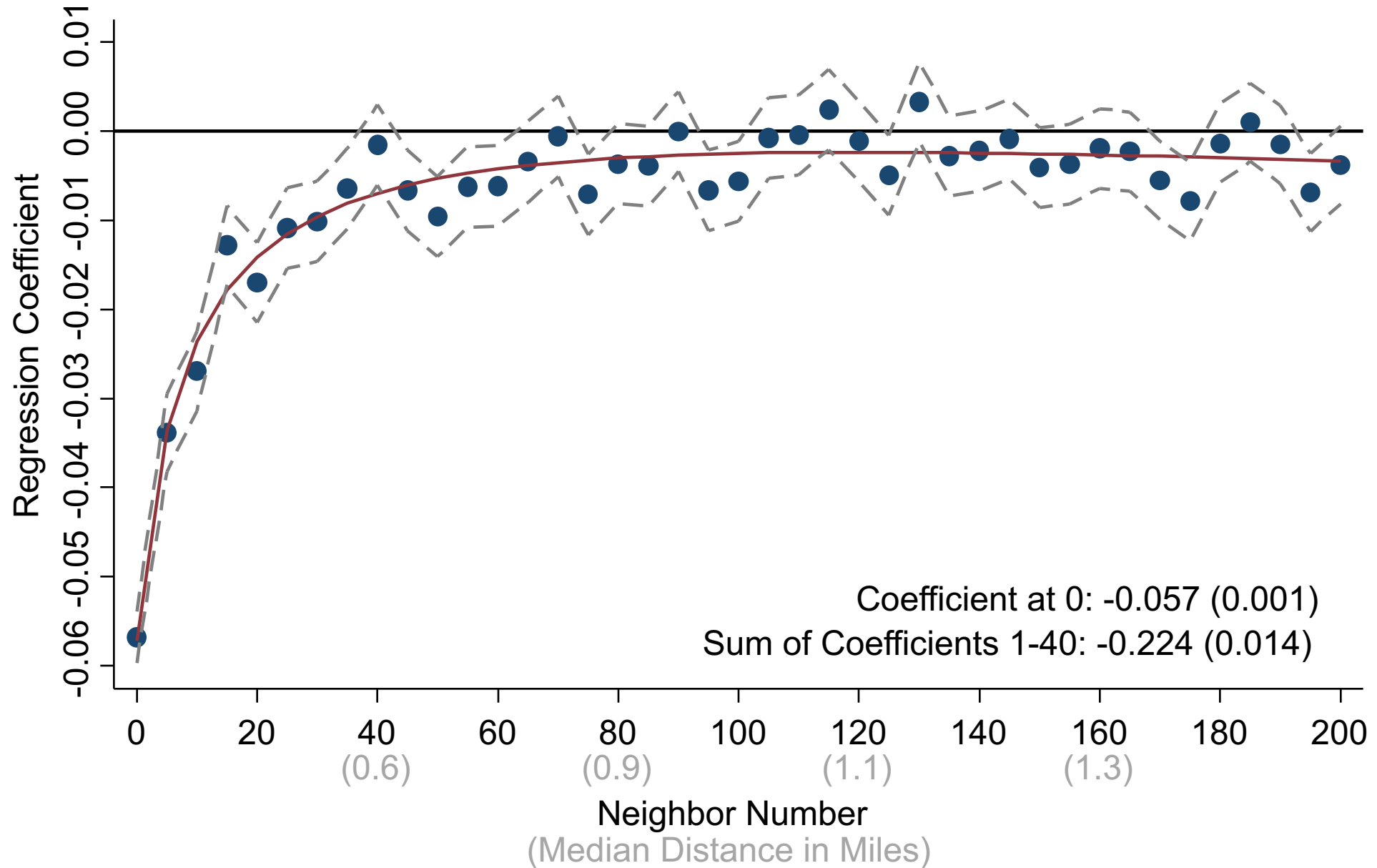
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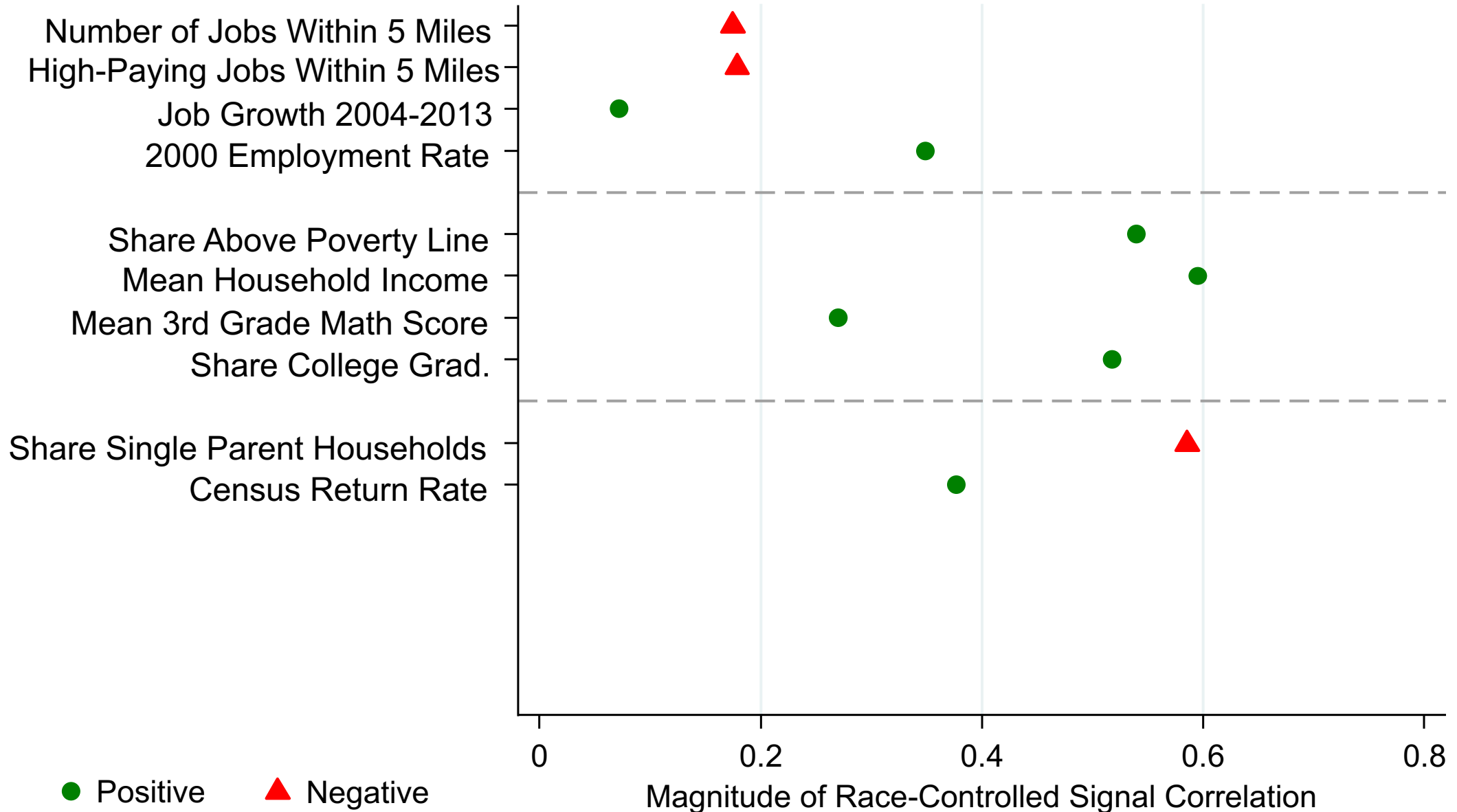
Spatial Decay of Correlation with Block-Level Poverty Rate

Mean Child Household Income Rank (Parents p=25), White Children



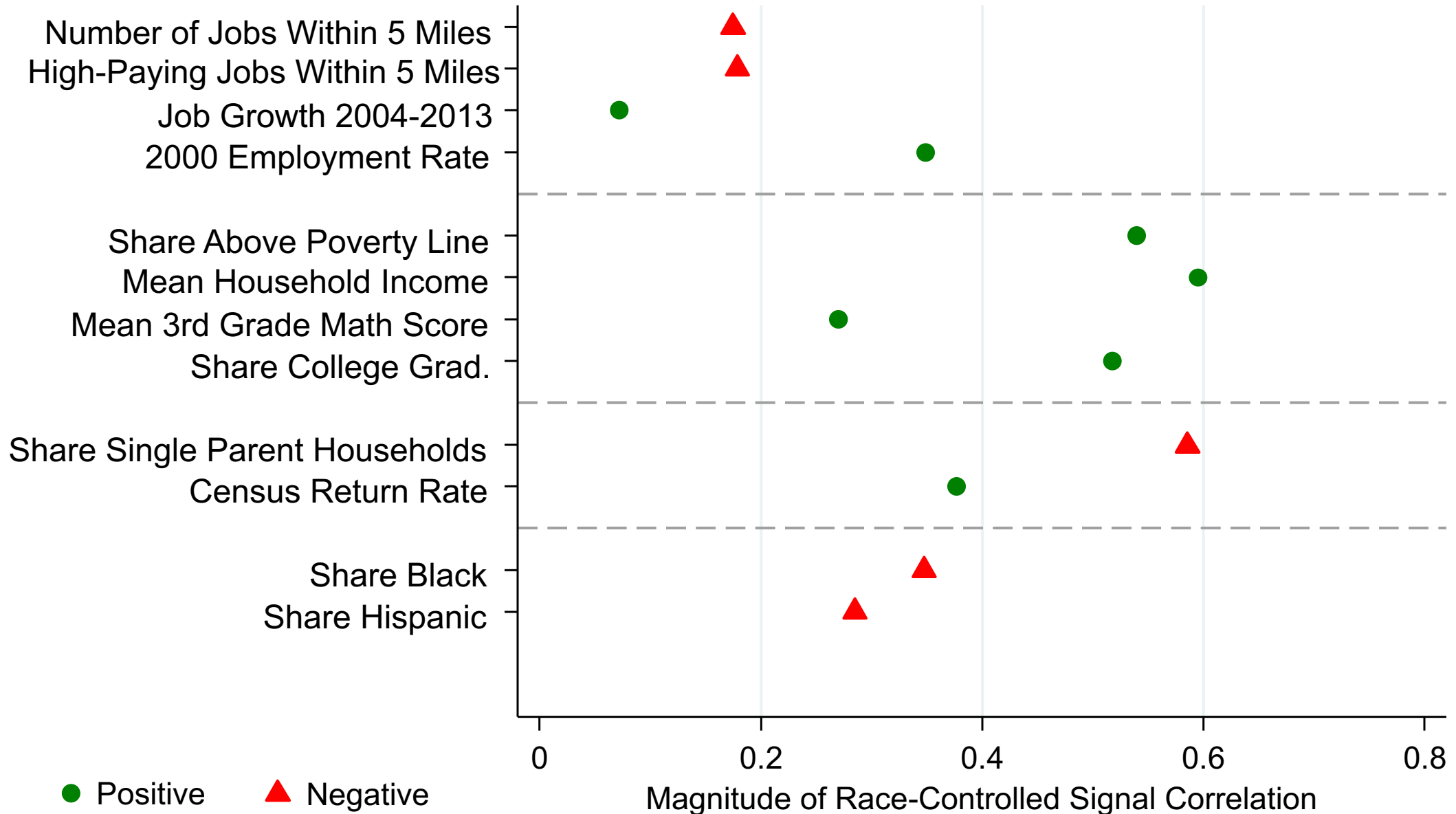
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Race-Adjusted, Parent Income at 25th Percentile



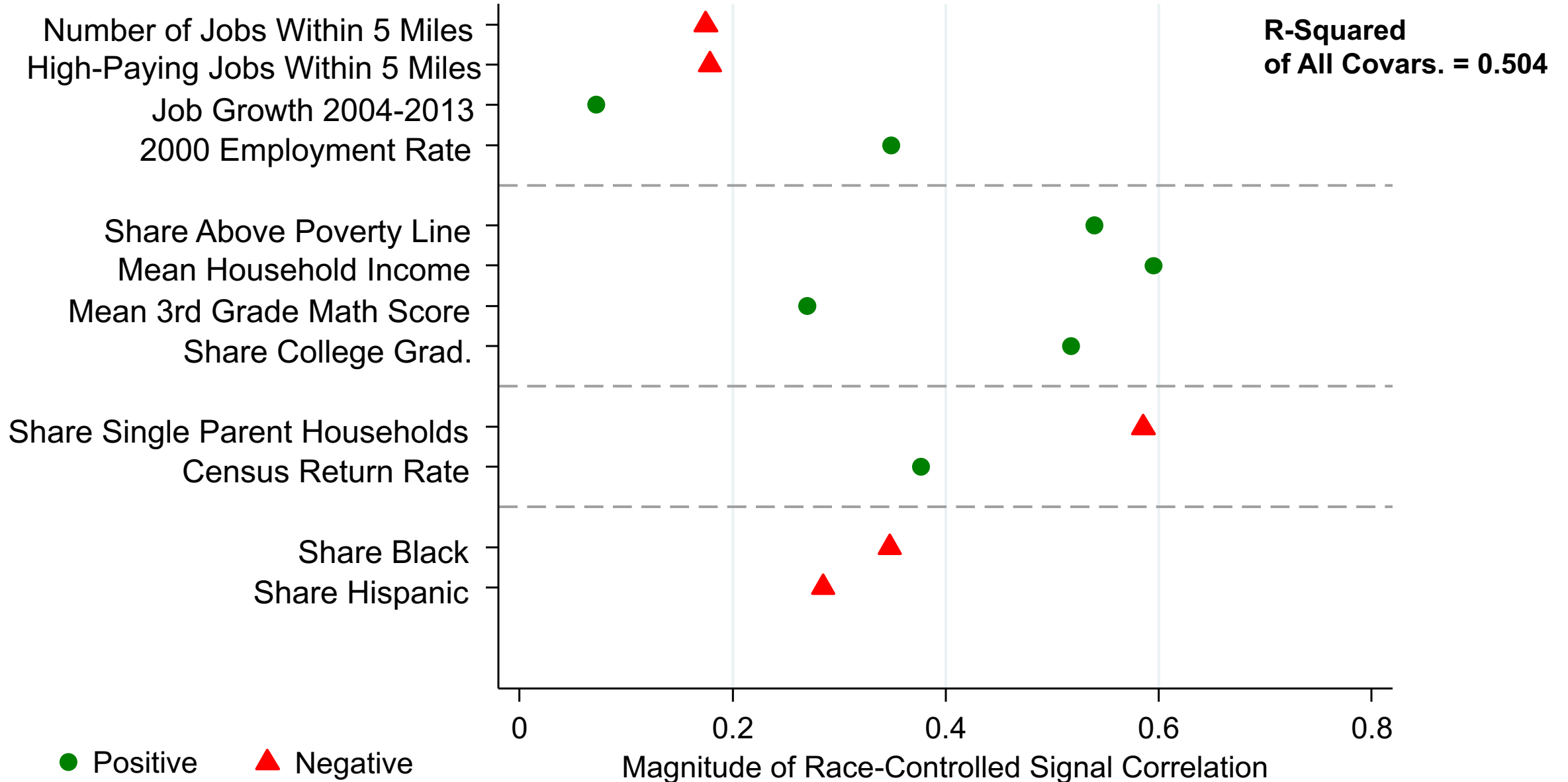
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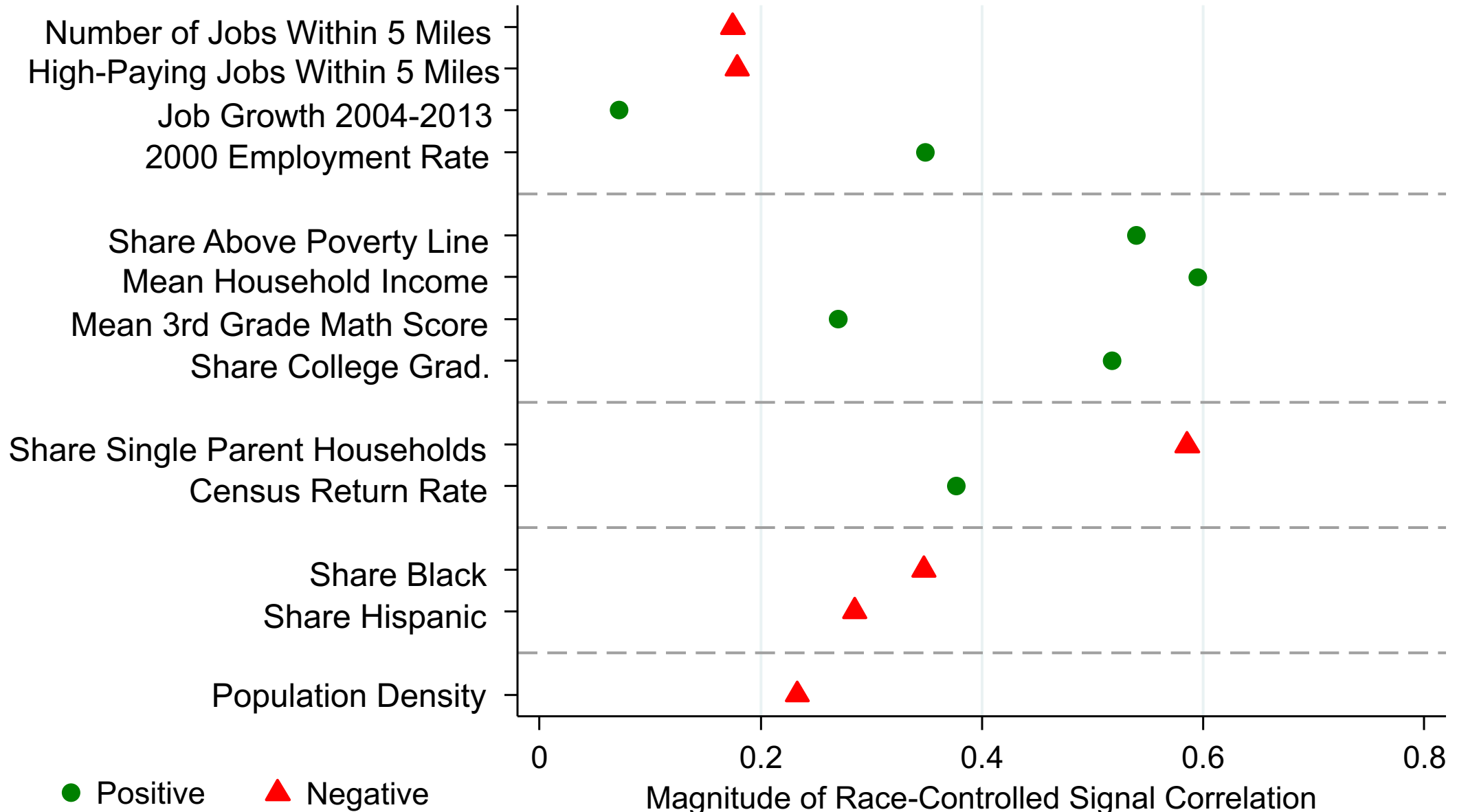
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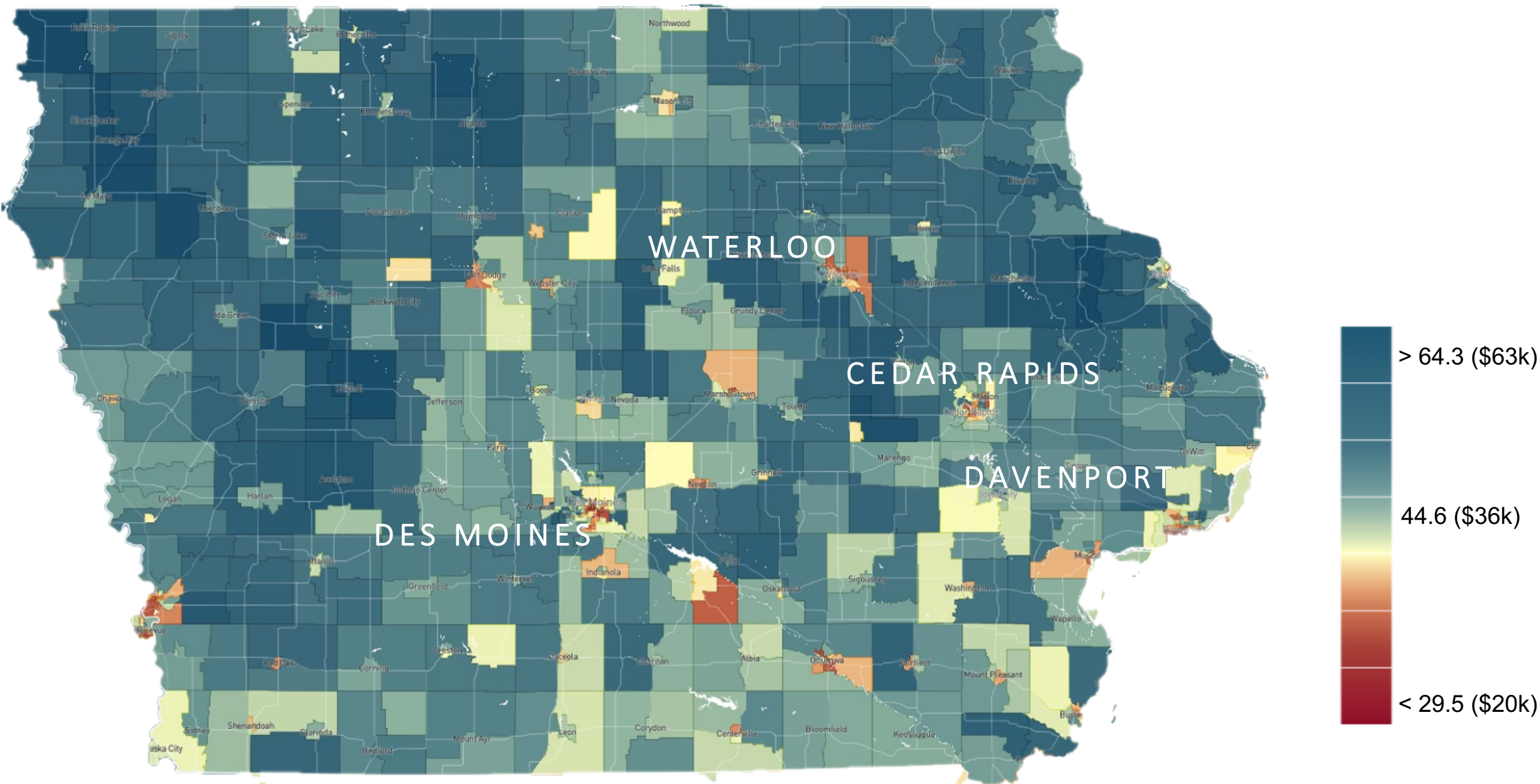
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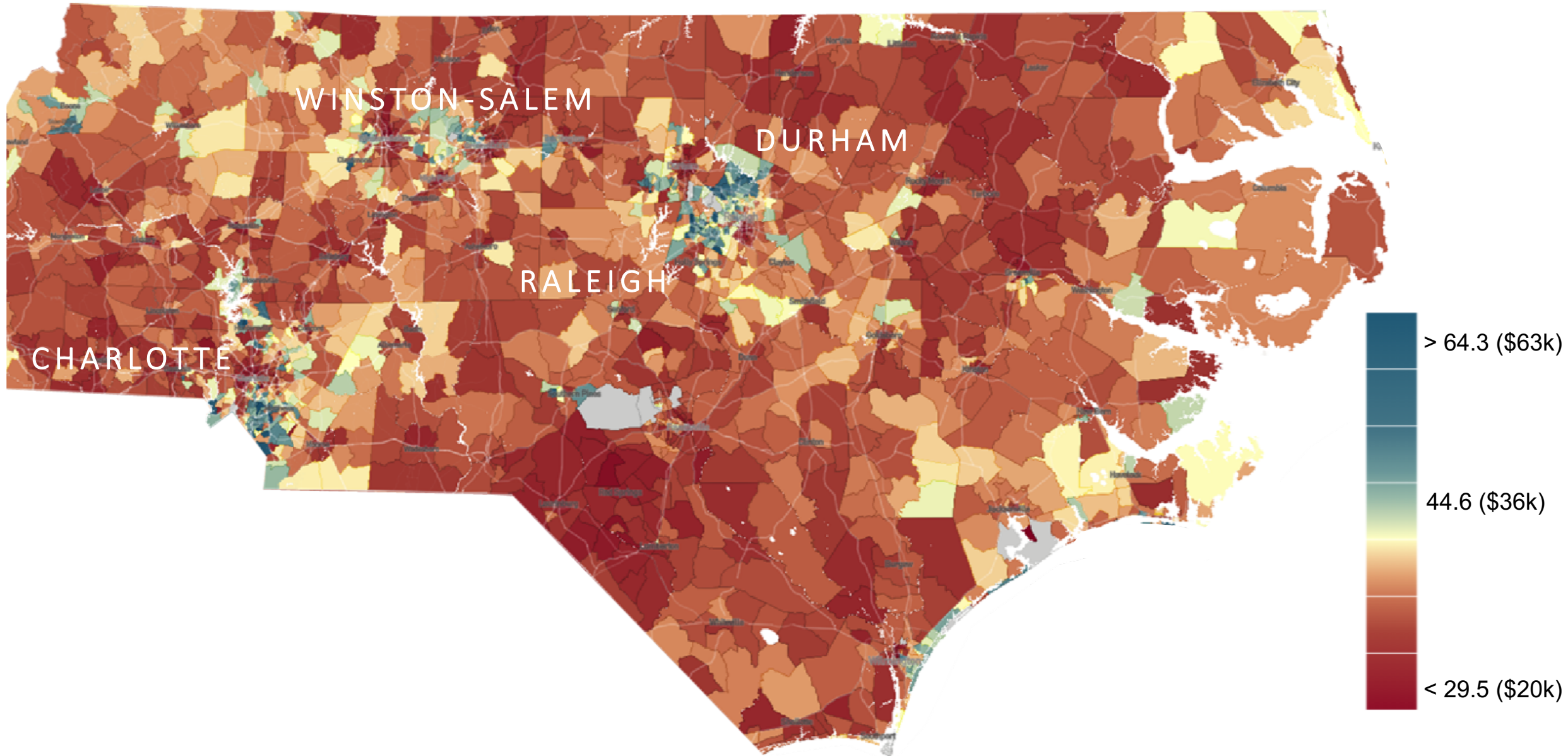
Do Cities Offer Greater Opportunities for Upward Mobility?

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



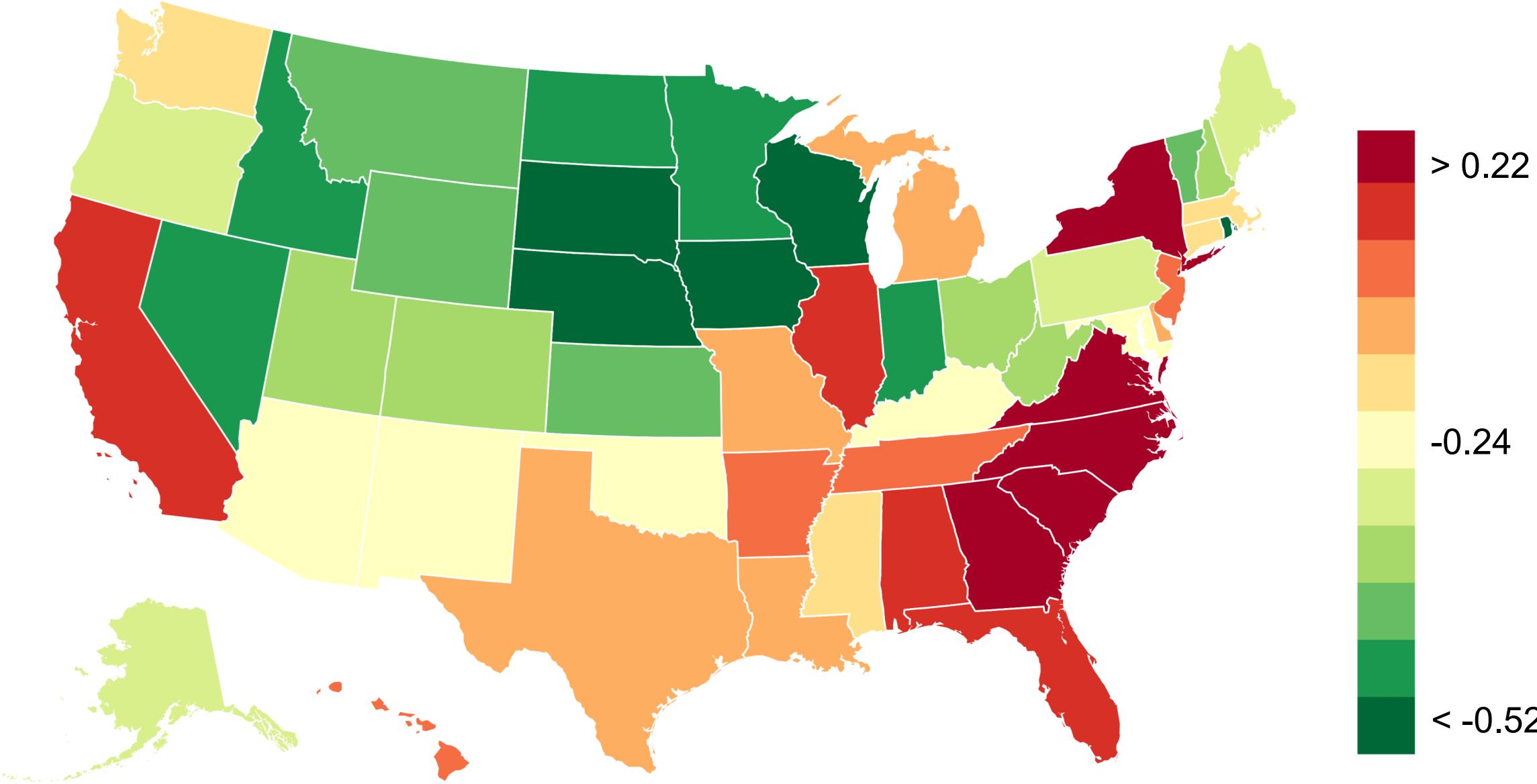
Do Cities Offer Greater Opportunities for Upward Mobility?

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

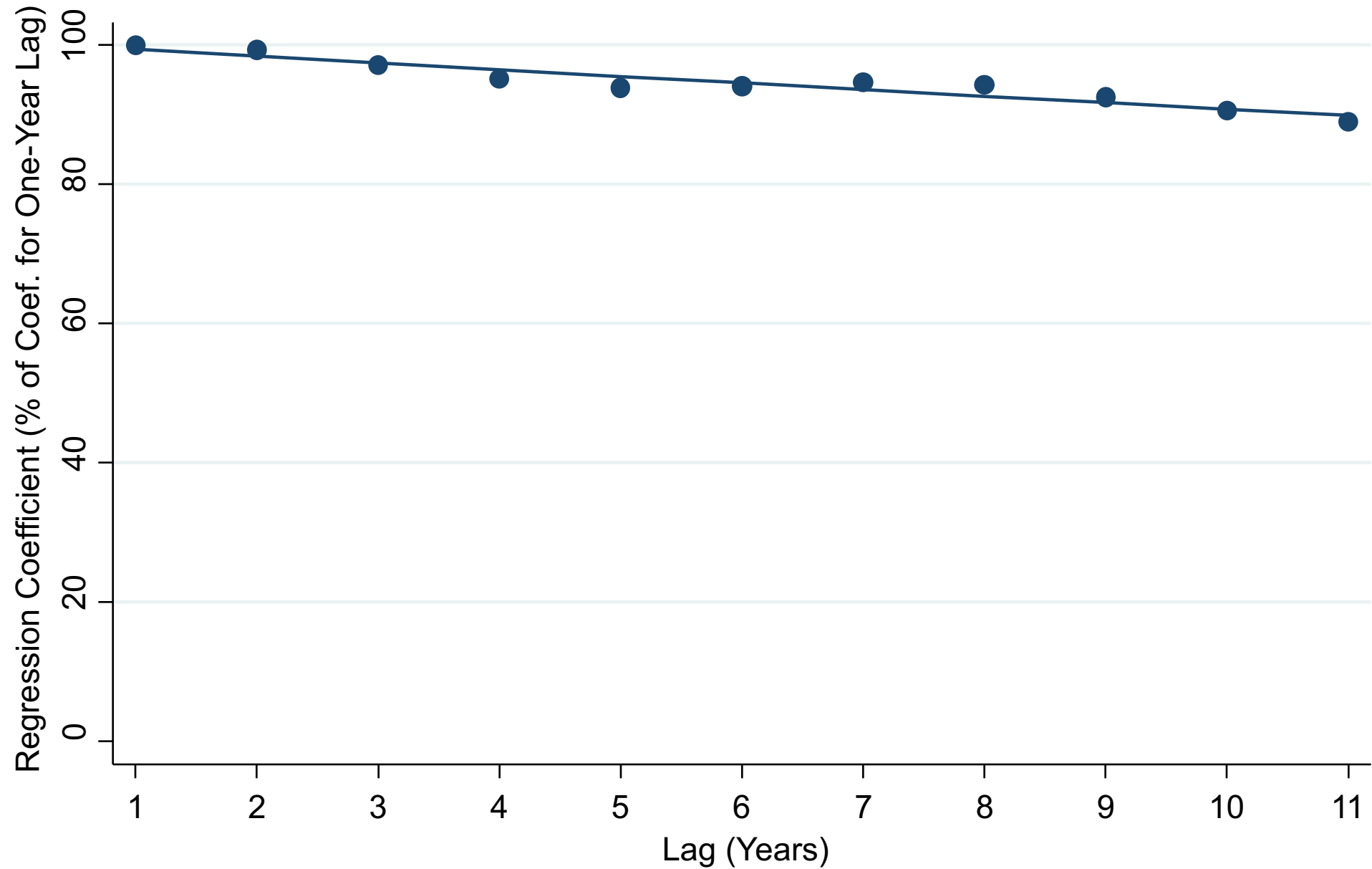


Using Location as a Tag for Policy

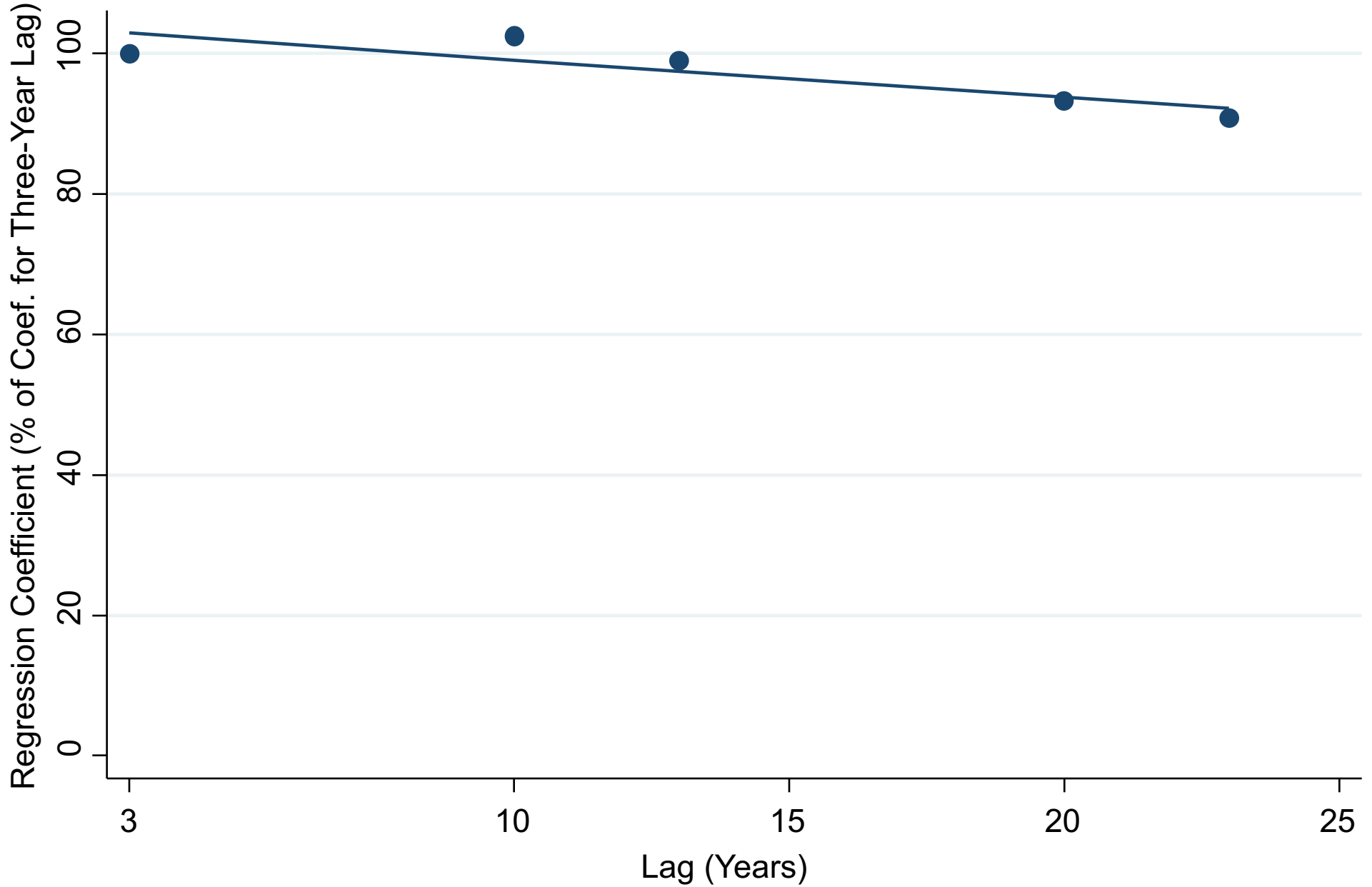
- Tract-level estimates of children's outcomes appear to provide new information that could be helpful in identifying areas where opportunity is most lacking
- Practical challenge in using these estimates to inform policy: they come with a lag, since one must wait until children grow up to observe their earnings
- Are historical estimates useful predictors of opportunity for current cohorts?

Autocovariance of Tract-Level Estimates

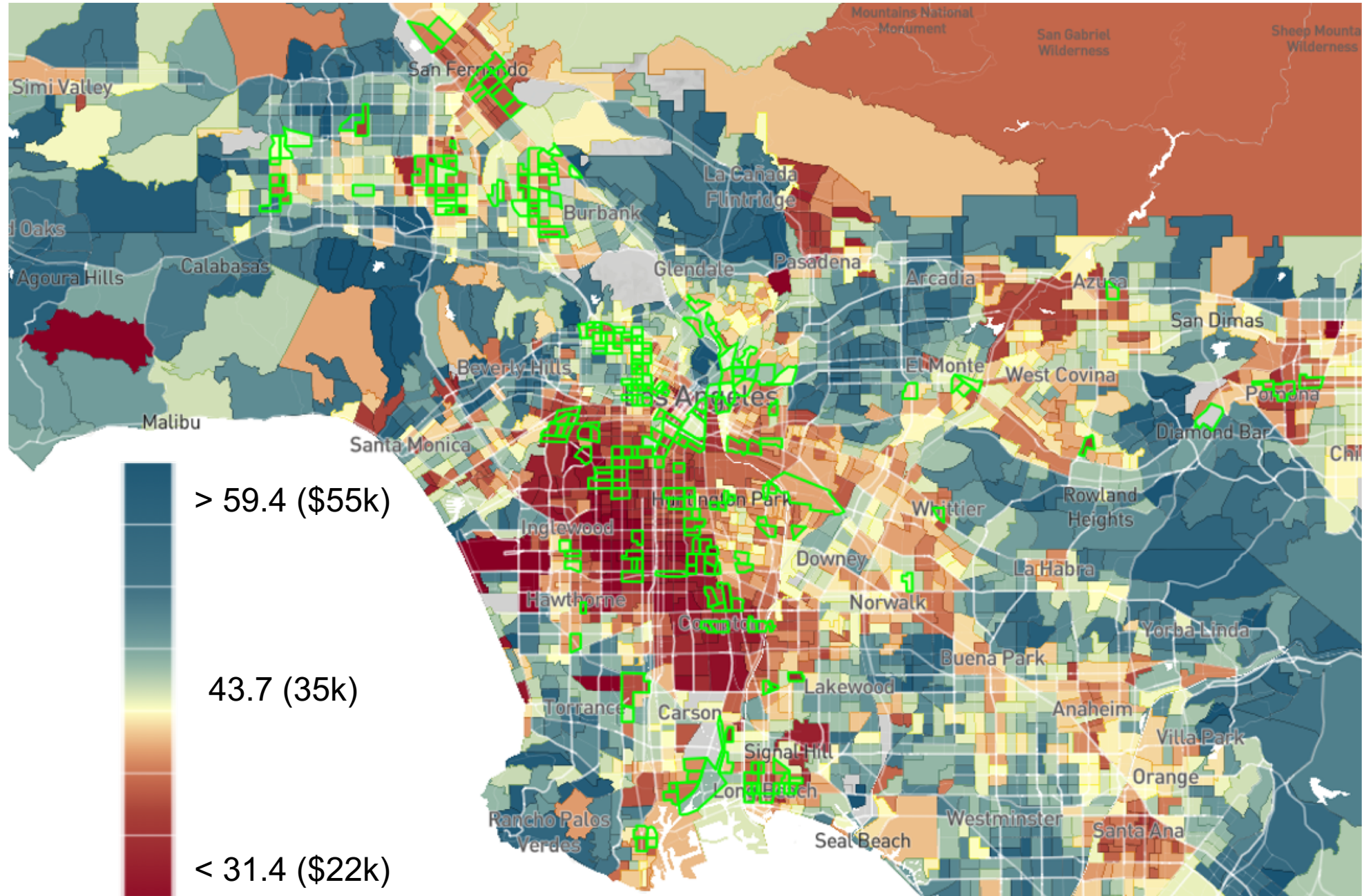
Mean Household Income at Age 26 for Children with Parents at $p=25$



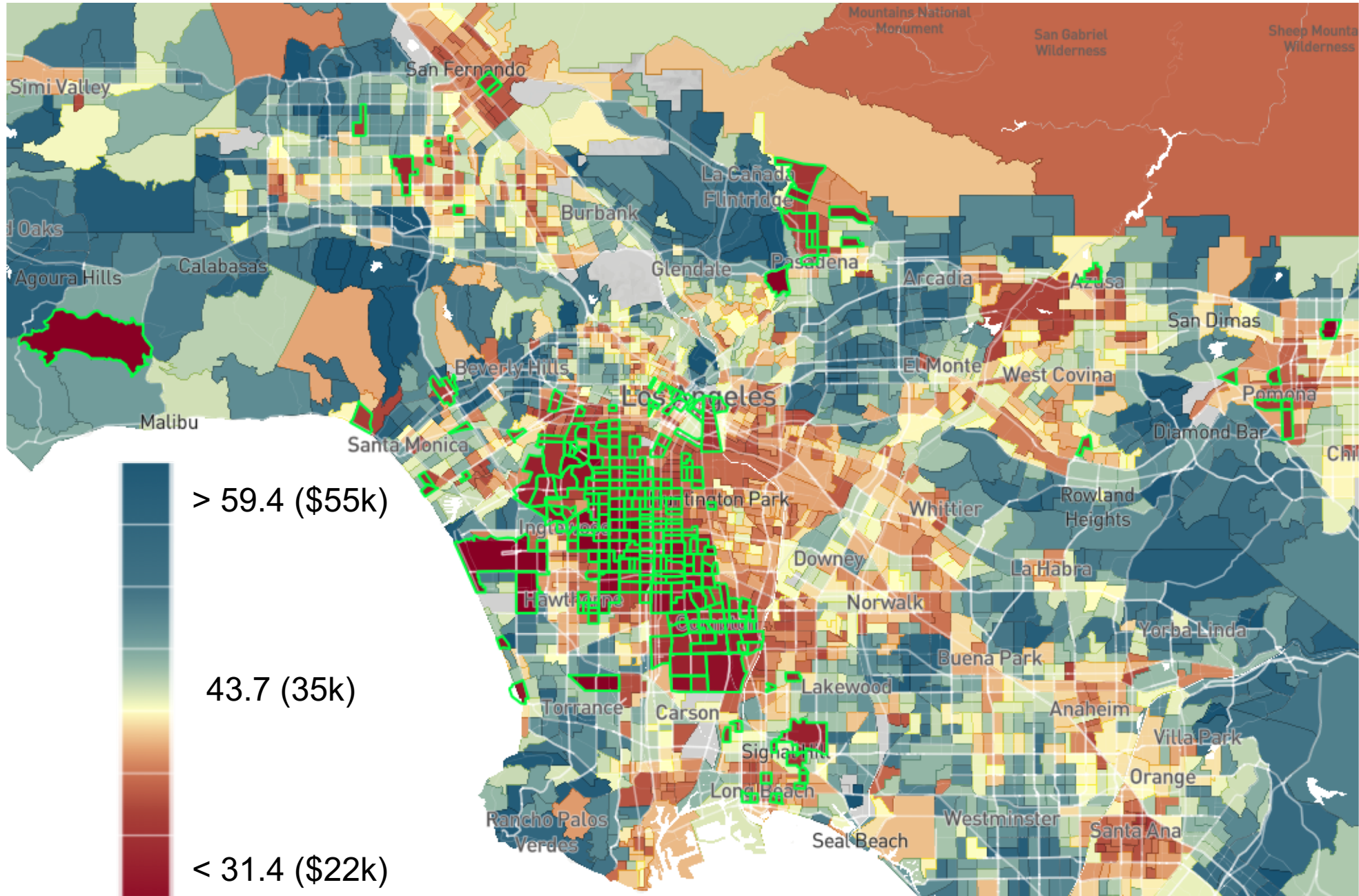
Autocovariance of Tract-Level Poverty Rates Using Publicly Available Census Data



Illustrative Application: Currently Designated Opportunity Zones in Los Angeles County



Hypothetical Opportunity Zones using Upward Mobility Estimates



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:

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

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

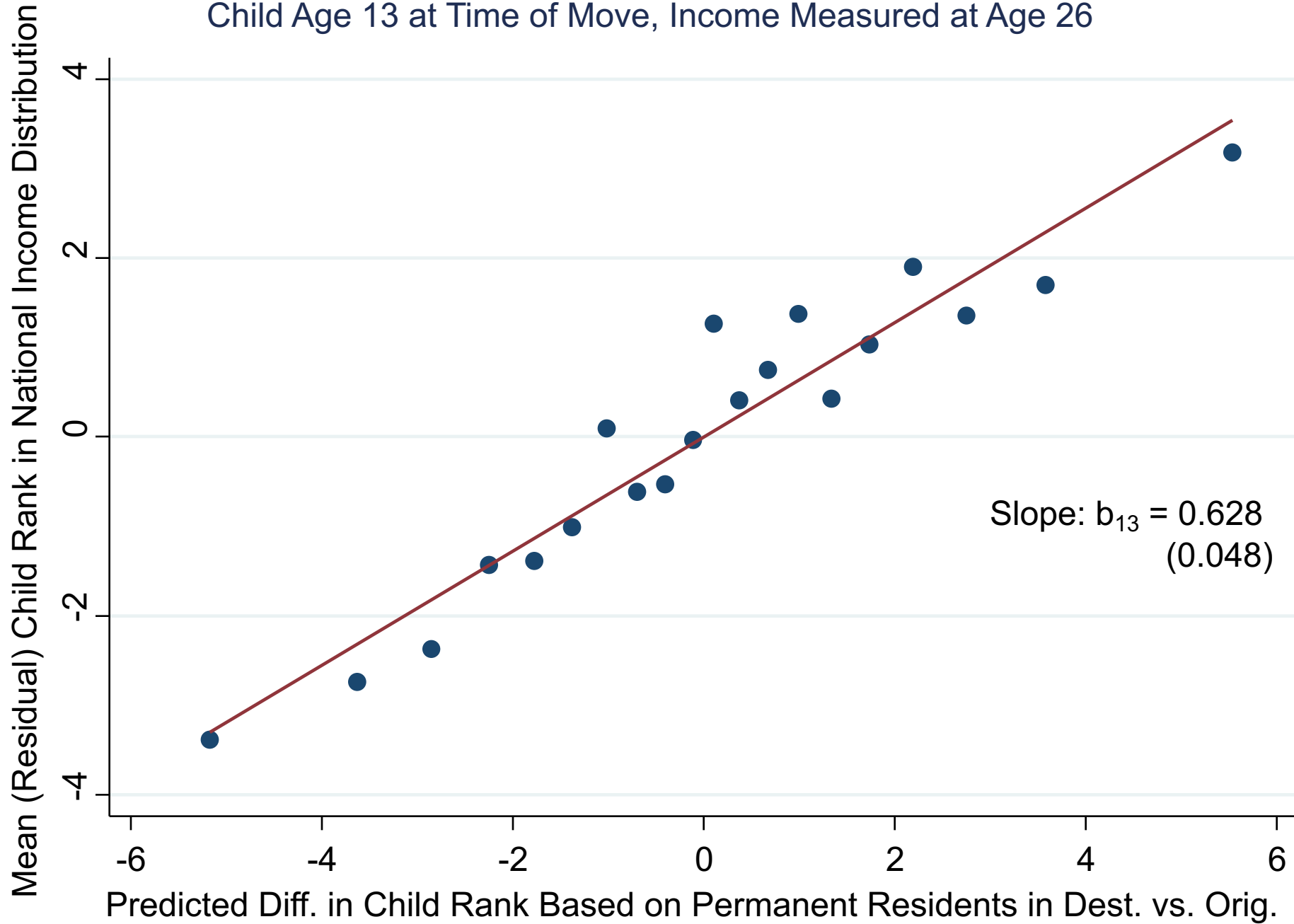
- To begin, consider subset of families who move with a child who is exactly 13 years old
- Regress child's income rank at age 26 y_i on predicted outcome of permanent residents in destination:

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

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

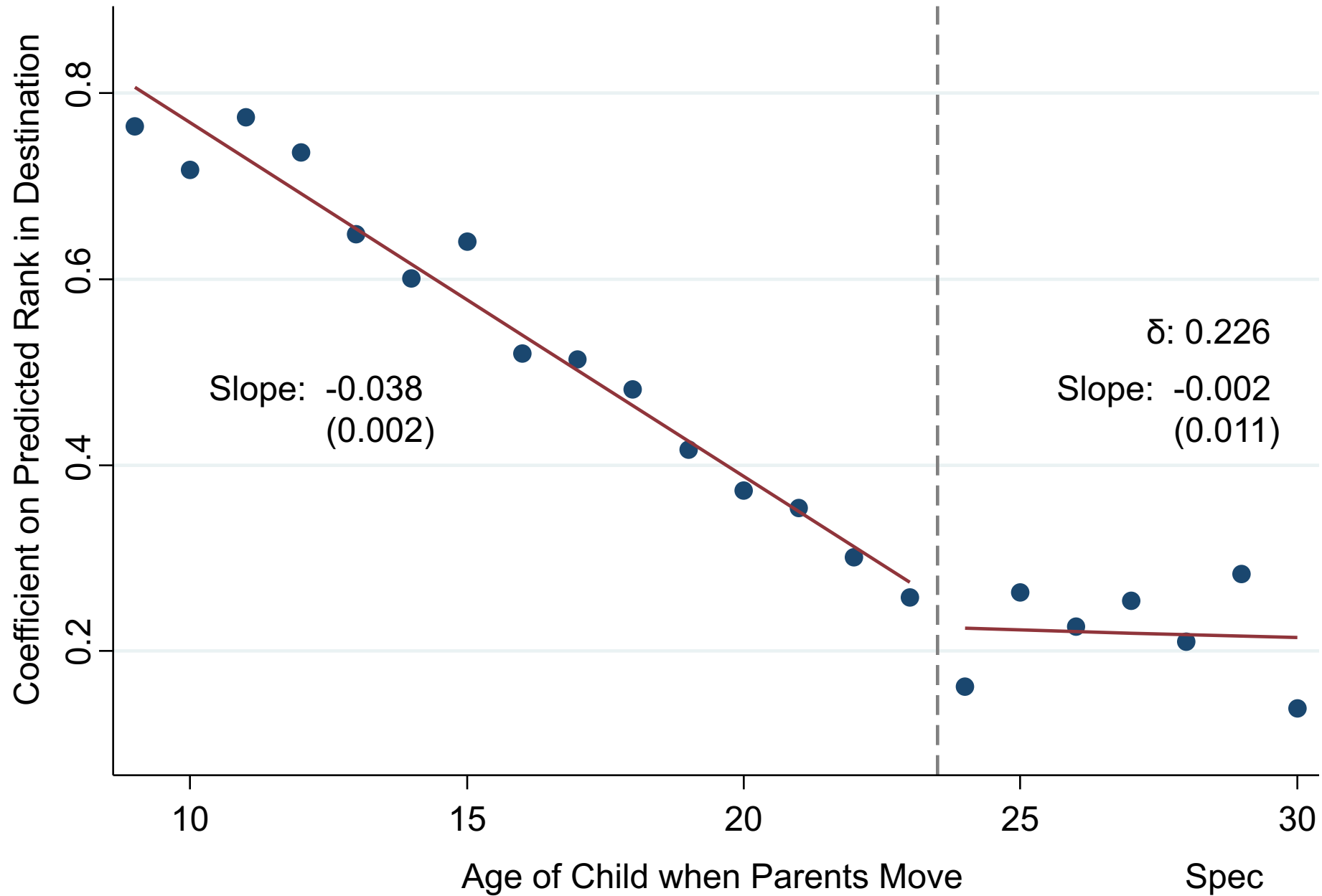
Movers' Outcomes vs. Predicted Outcomes Based on Residents in Destination

Child Age 13 at Time of Move, Income Measured at Age 26



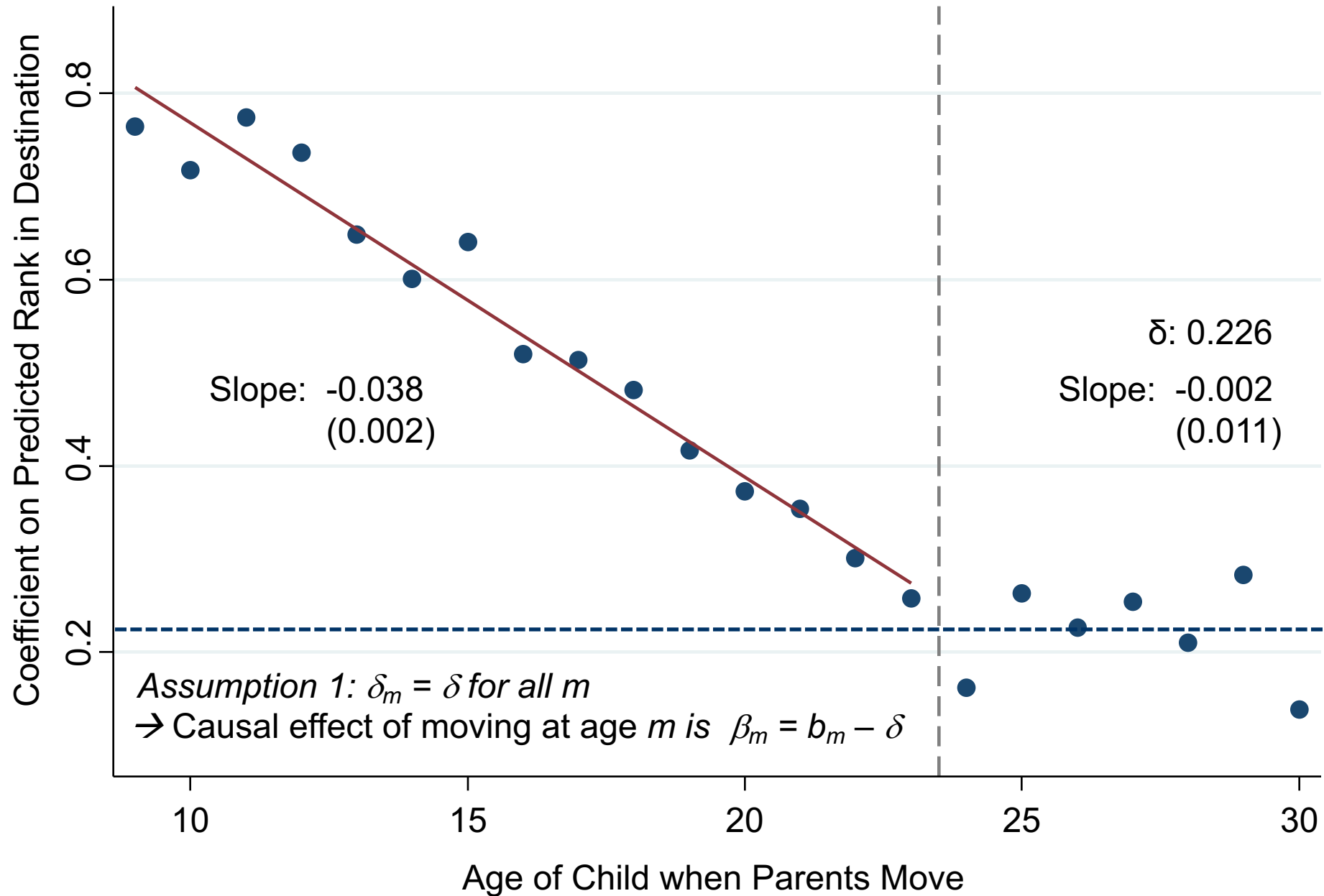
Movers' Outcomes vs. Predicted Outcomes Based on Residents in Destination

By Child's Age at Move, Income Measured at Age = 24



Movers' Outcomes vs. Predicted Outcomes Based on Residents in Destination

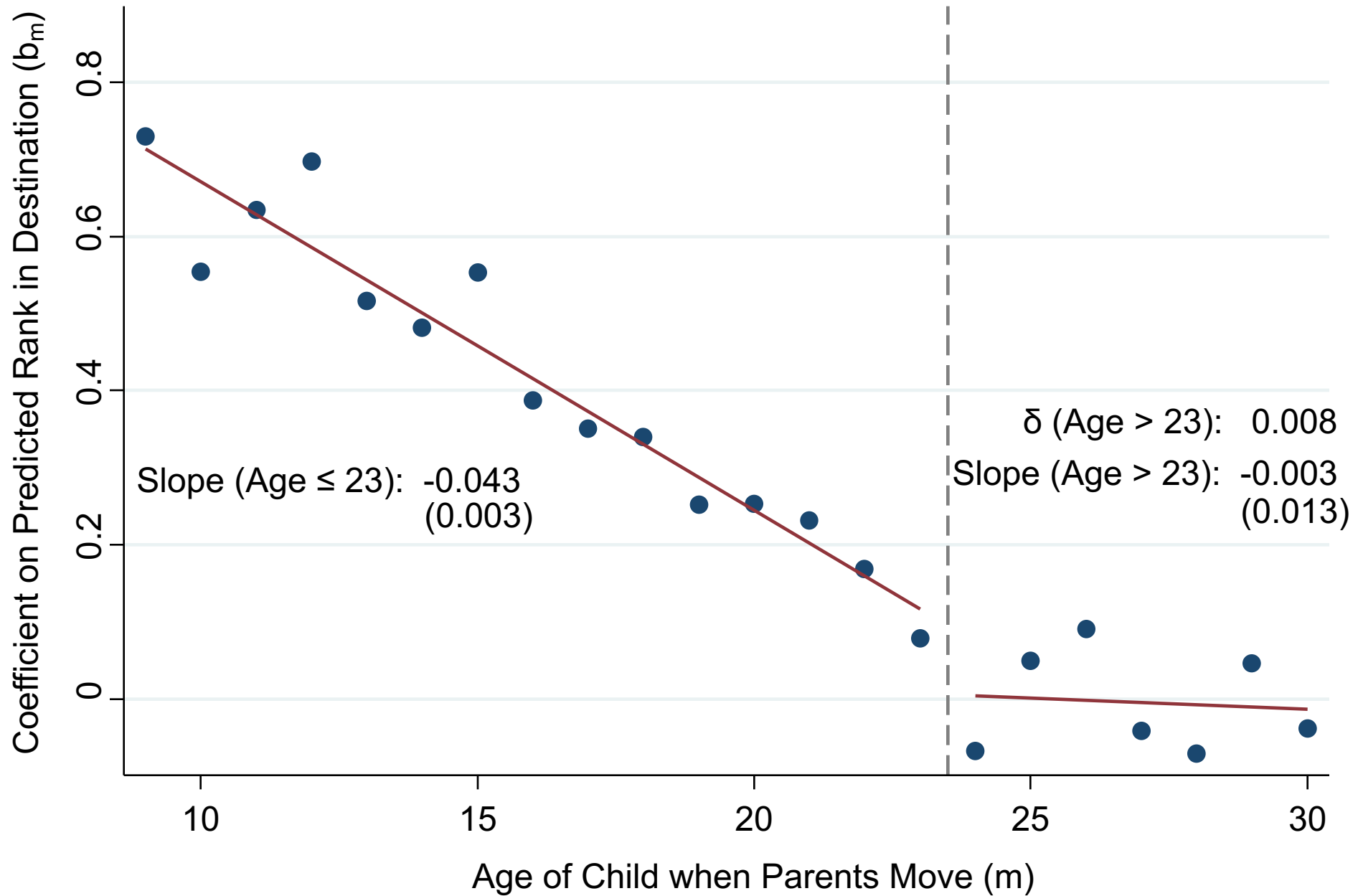
By Child's Age at Move, Income Measured at Age = 24



Identifying Causal Exposure Effect

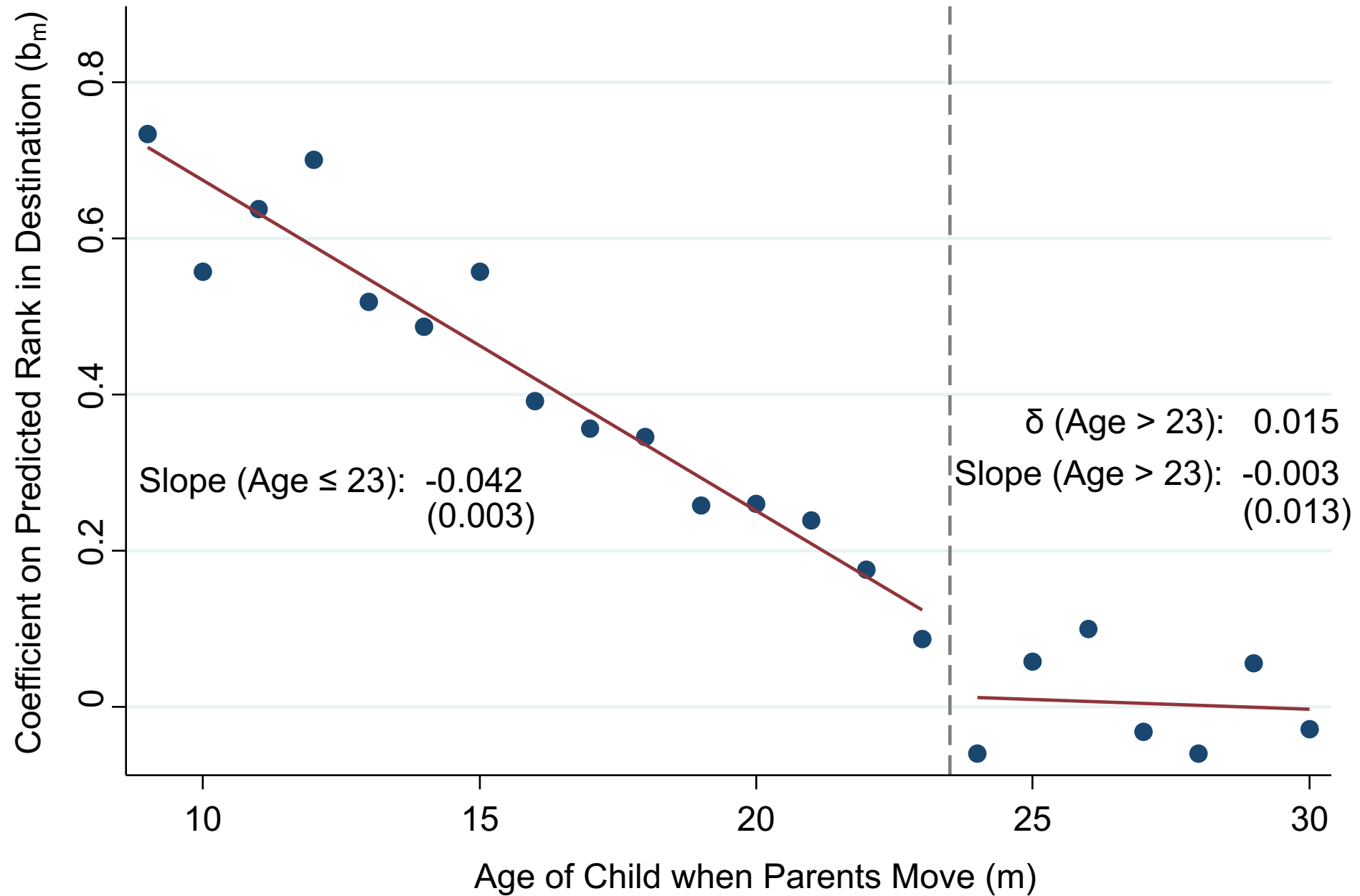
- Key identification assumption: *timing* of moves to better/worse areas uncorrelated with child's potential outcomes
- Primary contribution of the paper is to provide evidence in support of this identification condition in observational data
 - Without existence of an “instrument”
- Two main concerns (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



Family Fixed Effects: Sibling Comparisons

with Controls for Change in Income and Marital Status at Move



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

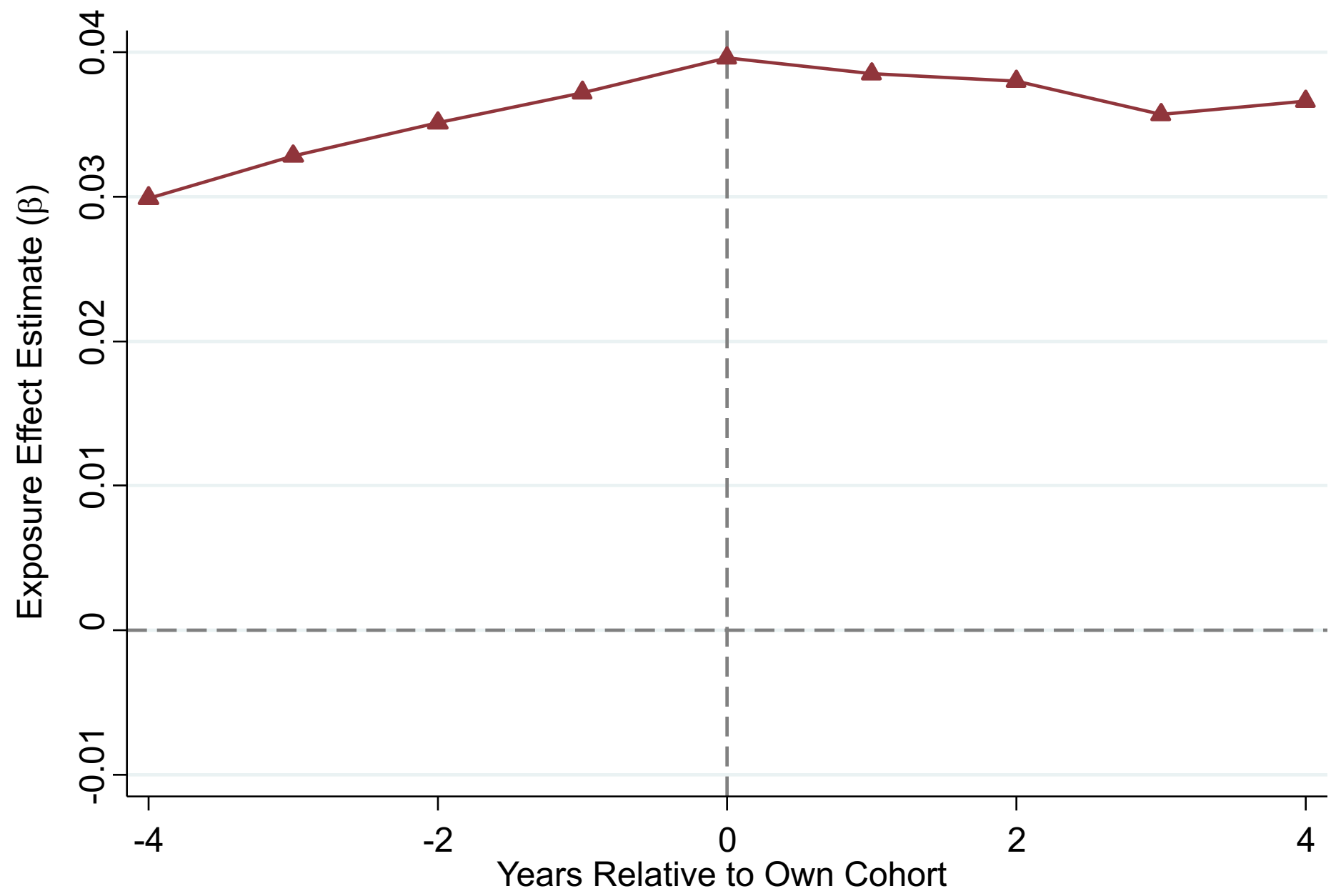
Outcome-Based Placebo Tests

- 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

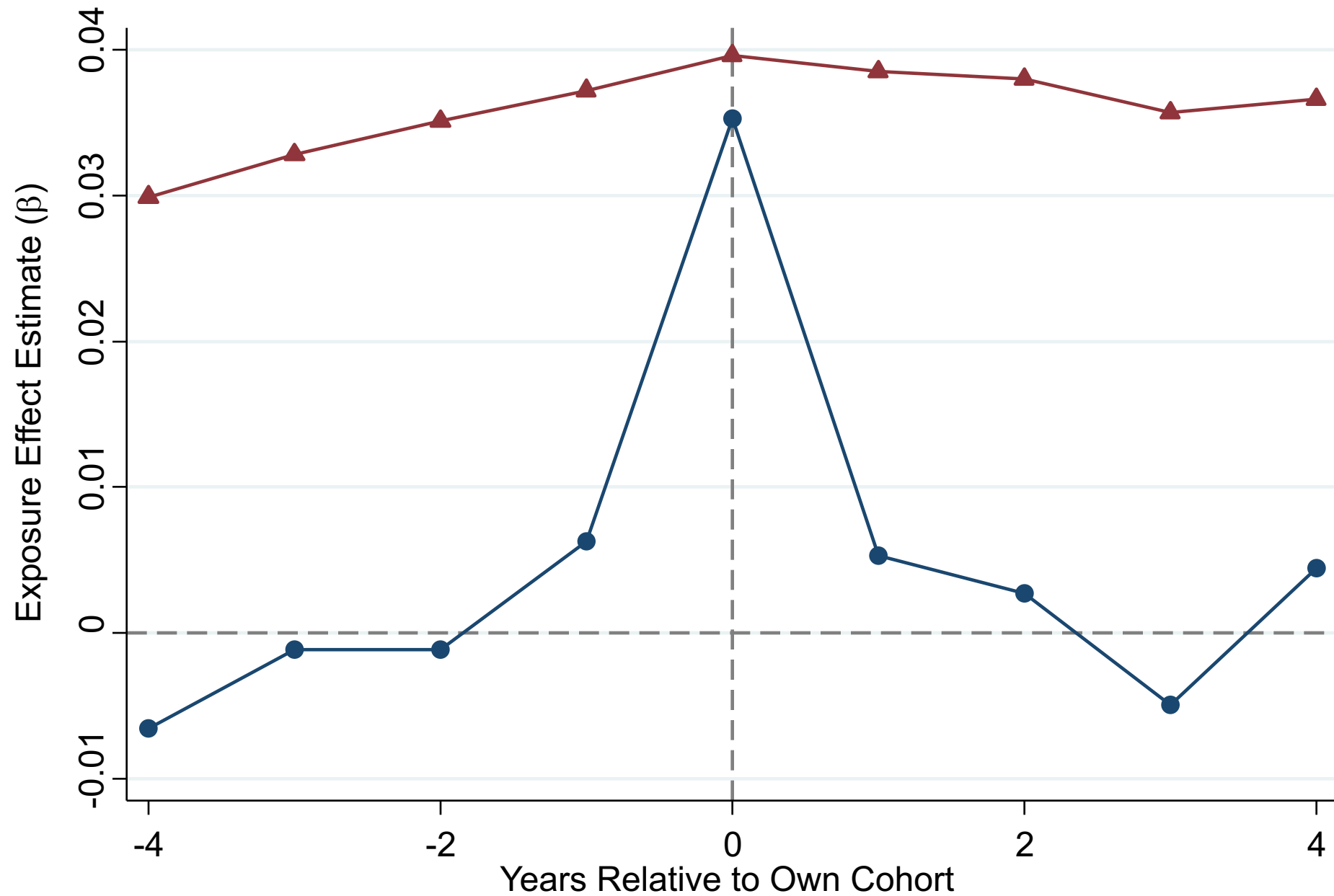
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 - Some areas are getting better over time, others are getting worse
 - Causal effect of neighborhood on a child who moves in to an area should depend on properties of that area while he is growing up
- Parents choose neighborhoods based on their preferences 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



—▲— Separate

Estimates of Exposure Effects Based on Cross-Cohort Variation



—●— 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

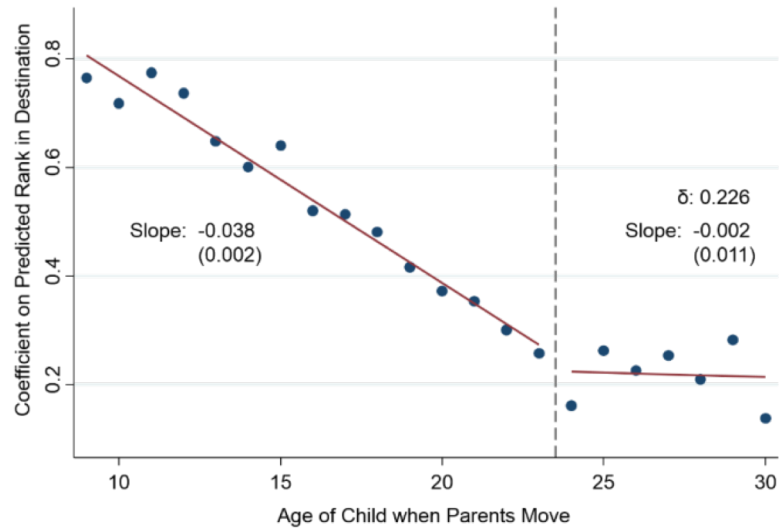
	No Family Fixed Effects			Family Fixed Effects
	(1)	(2)	(3)	(4)
Own Gender Prediction	0.038 (0.002)		0.031 (0.003)	0.031 (0.007)
Other Gender Prediction (Placebo)		0.034 (0.002)	0.009 (0.003)	0.012 (0.007)
Sample		Full Sample		2-Gender HH

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?

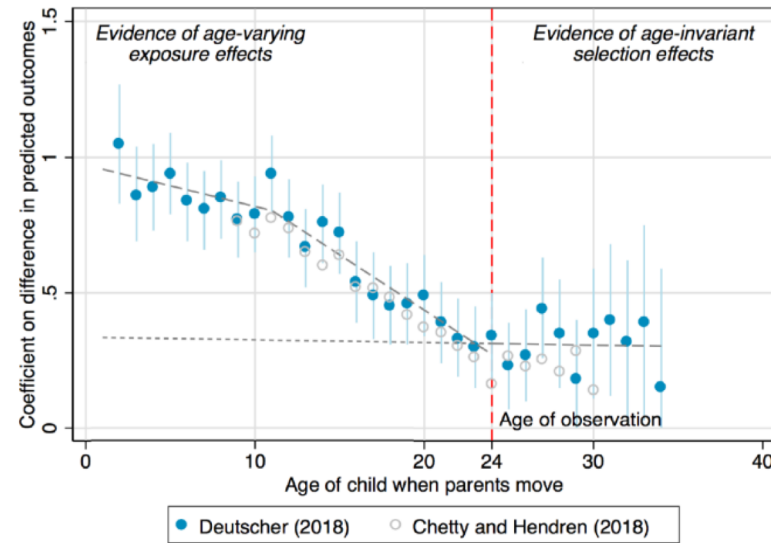
Childhood Exposure Effects: Other Evidence

United States



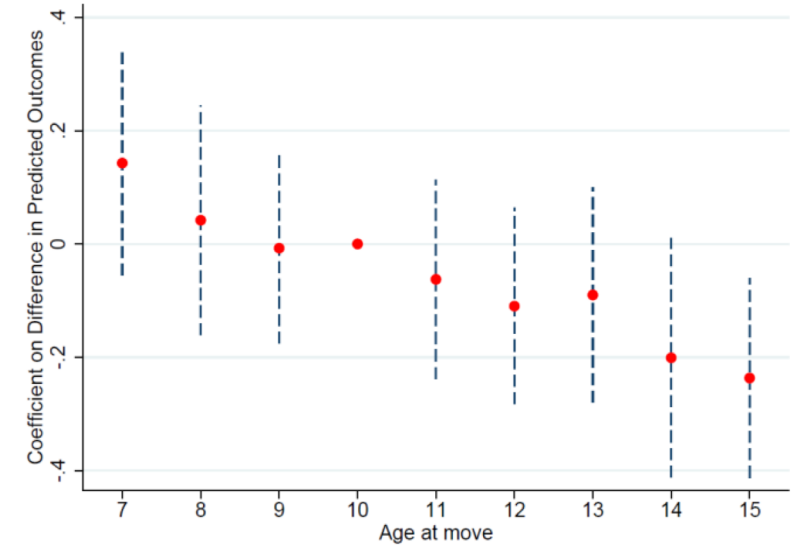
Source: Chetty and Hendren (QJE 2018)

Australia



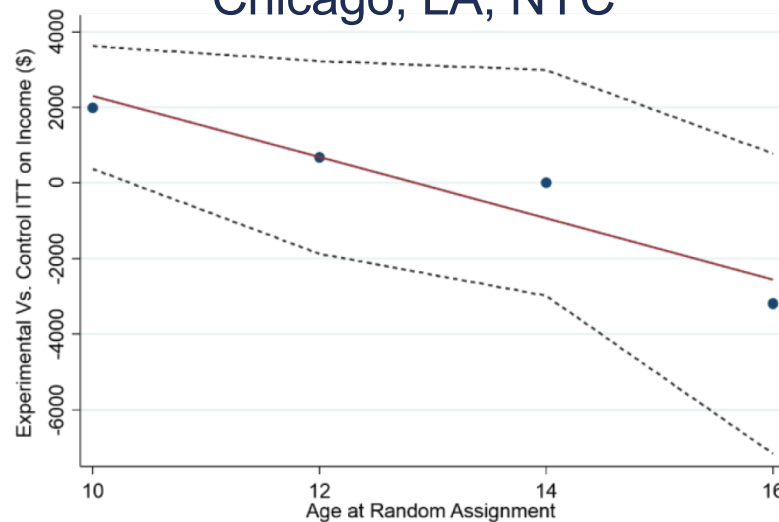
Source: Deutscher (2018)

Montreal, Canada



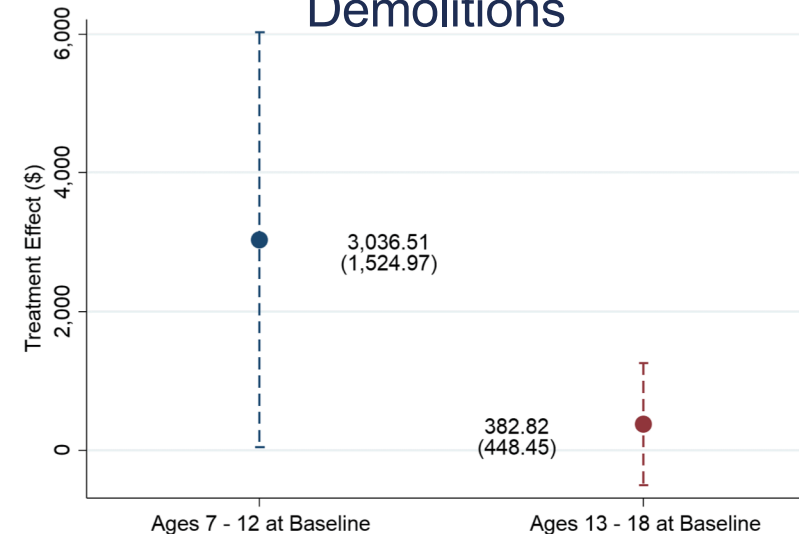
Source: Laliberté (2018)

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*** (0.050)	1.240*** (0.075)	0.229*** (0.037)	0.206** (0.092)
ELA	0.059 (0.041)	1.241*** (0.074)	0.047 (0.033)	-0.053 (0.049)
Absences	-2.783*** (0.833)	1.260*** (0.079)	-2.199*** (0.650)	-0.220 (2.544)
On Grade Level	-0.003 (0.022)	1.240*** (0.075)	-0.002 (0.017)	-0.011 (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*** (0.051)	0.241*** (0.042)	0.468
ELA	0.067 (0.045)	0.039 (0.037)	0.577
Absences	-1.300 (1.003)	-2.601*** (0.683)	0.183
On Grade Level	0.013 (0.024)	-0.009 (0.020)	0.414
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

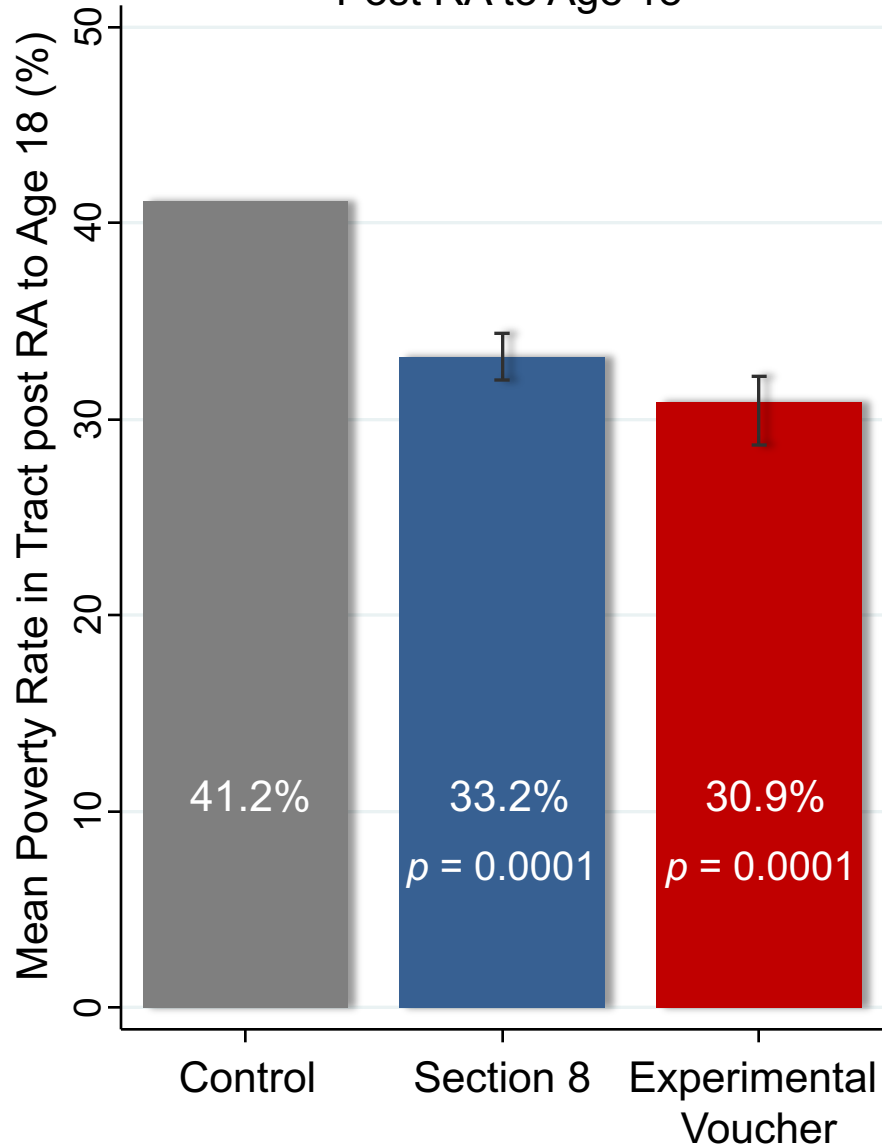
- HUD Moving to Opportunity Experiment implemented from 1994-1998
- 4,600 families at 5 sites: Baltimore, Boston, Chicago, LA, New York
- Families randomly assigned to one of three groups:
 1. Experimental: housing vouchers restricted to low-poverty (<10%) Census tracts
 2. Section 8: conventional housing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

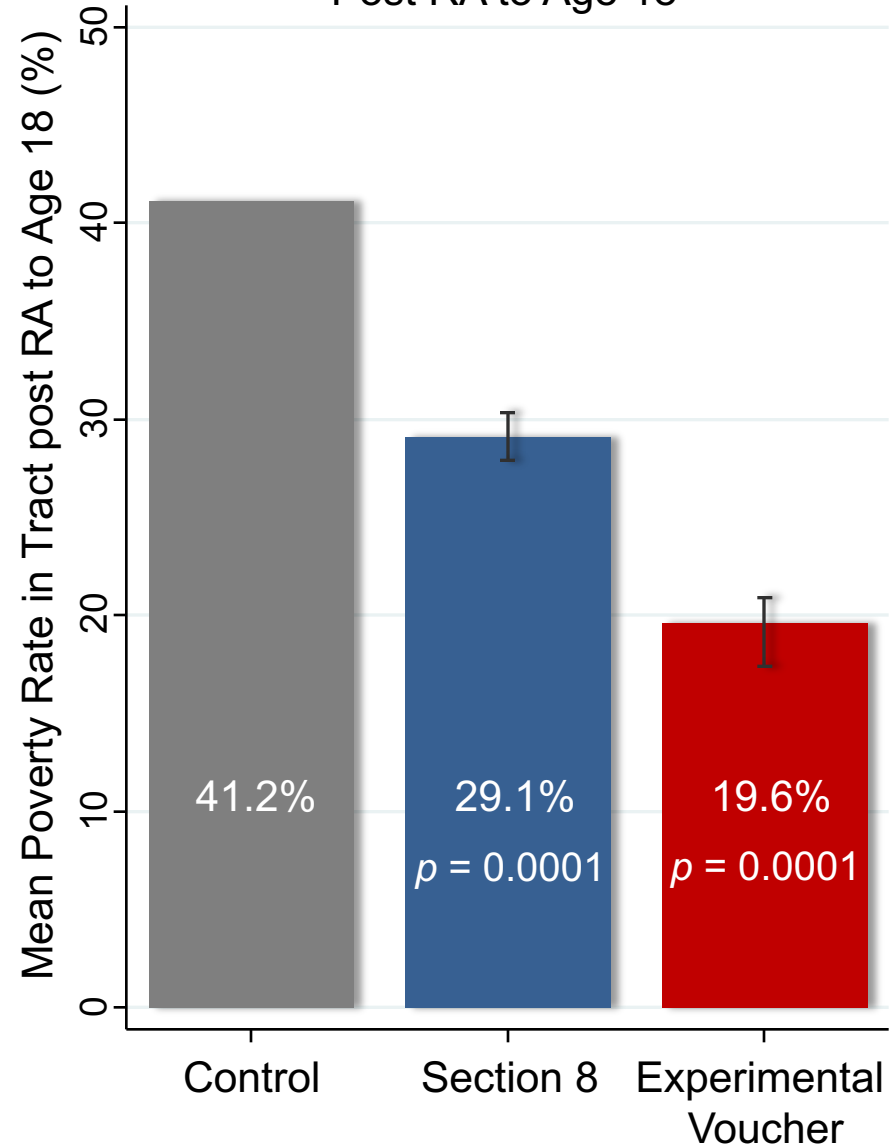
- Begin with “first stage” effects of MTO experiment on poverty rates
 - Measure mean poverty rates from random assignment to age 18 at tract level using Census data
- Use poverty rates as an index of nbhd. quality, but note that MTO treatments naturally changed many other features of neighborhoods too

Impacts of MTO on Children Below Age 13 at Random Assignment

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



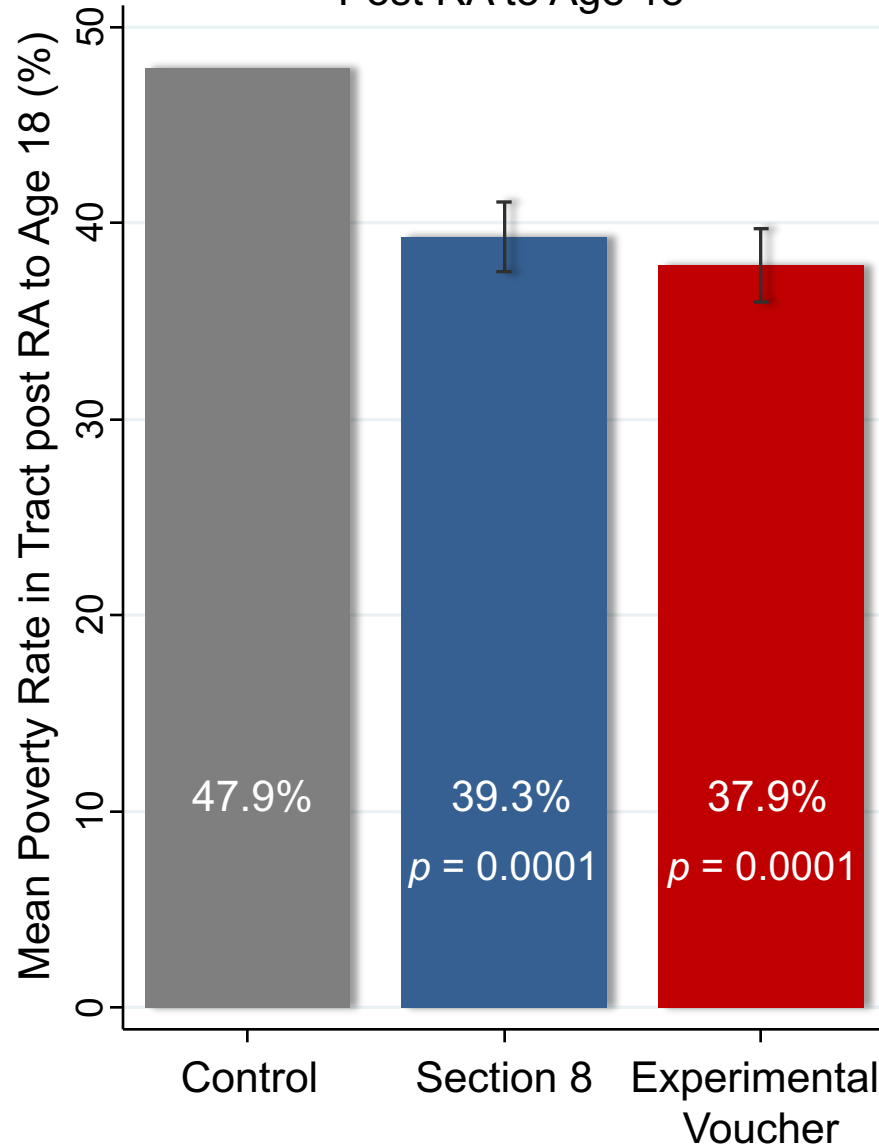
(b) Mean Poverty Rate in Tract (TOT)
Post RA to Age 18



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

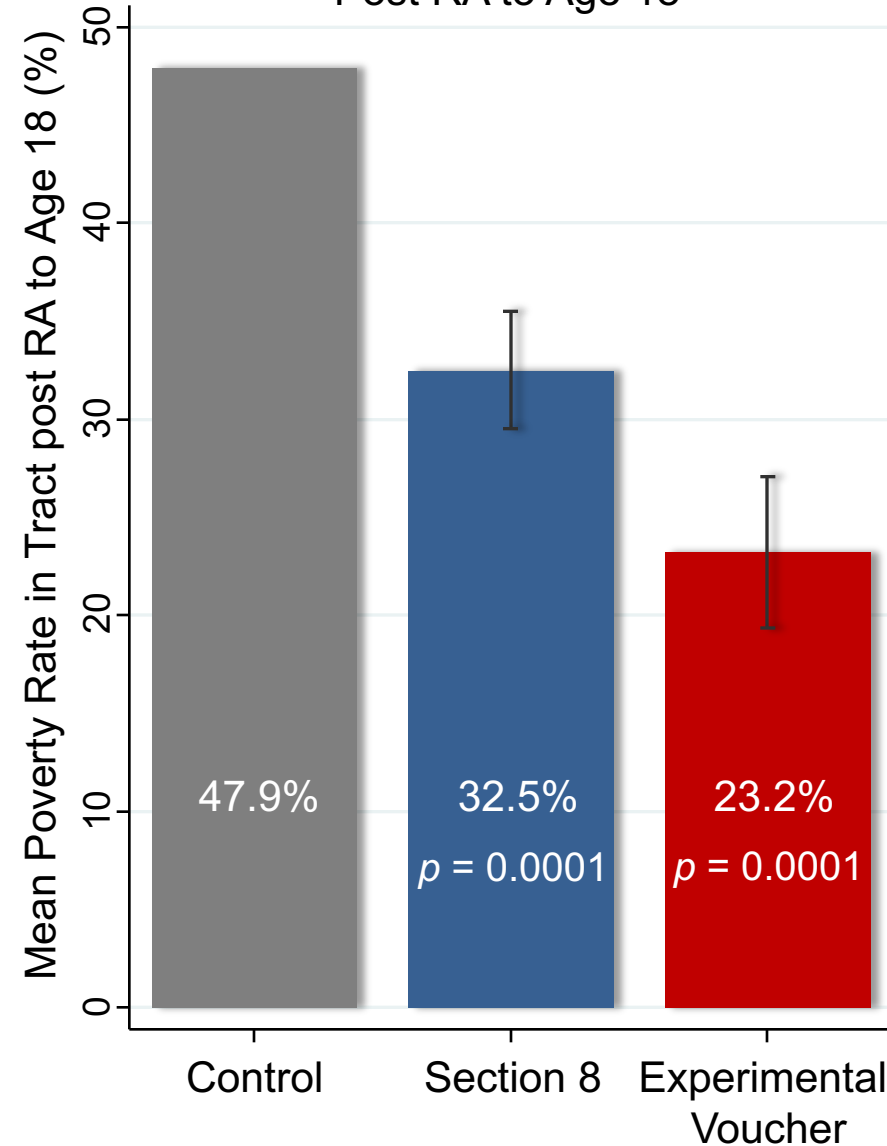
(a) Mean Poverty Rate in Tract (ITT)

Post RA to Age 18



(b) Mean Poverty Rate in Tract (TOT)

Post RA to Age 18

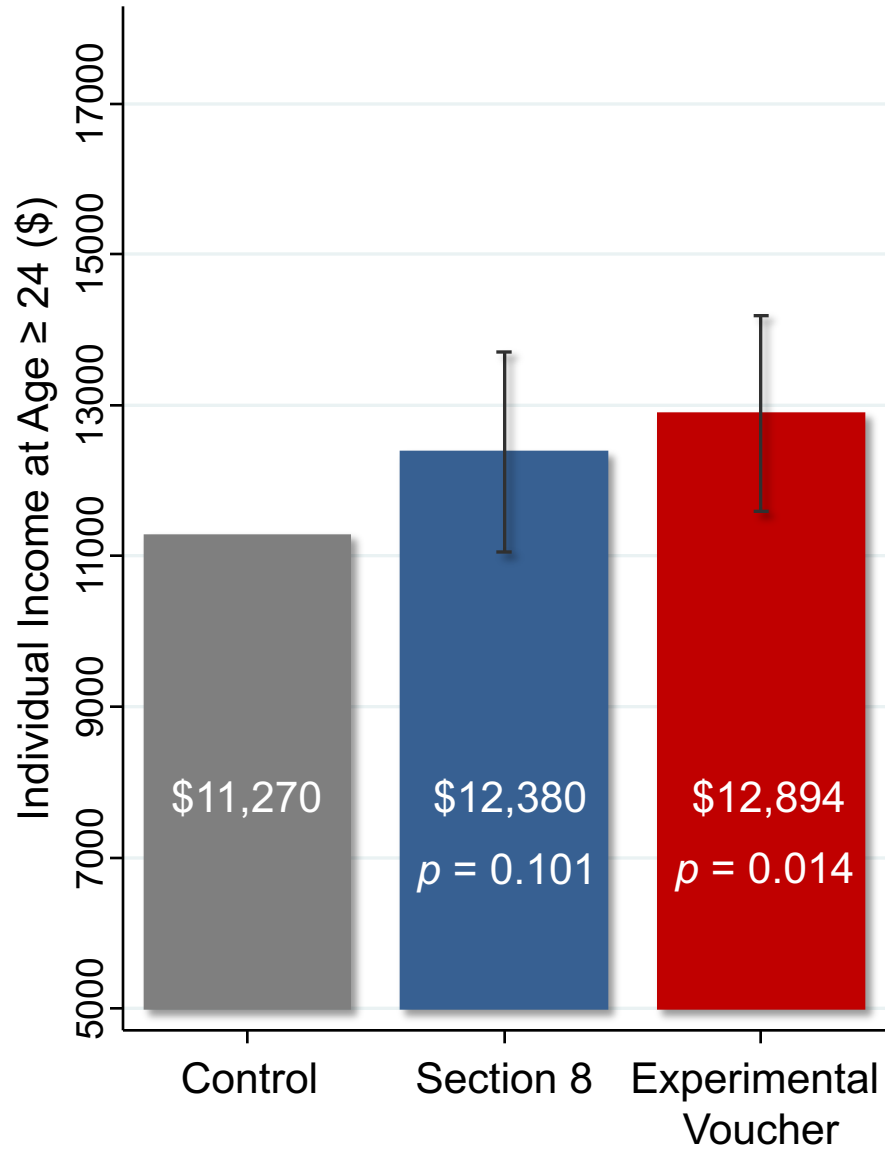


Treatment Effects on Outcomes in Adulthood

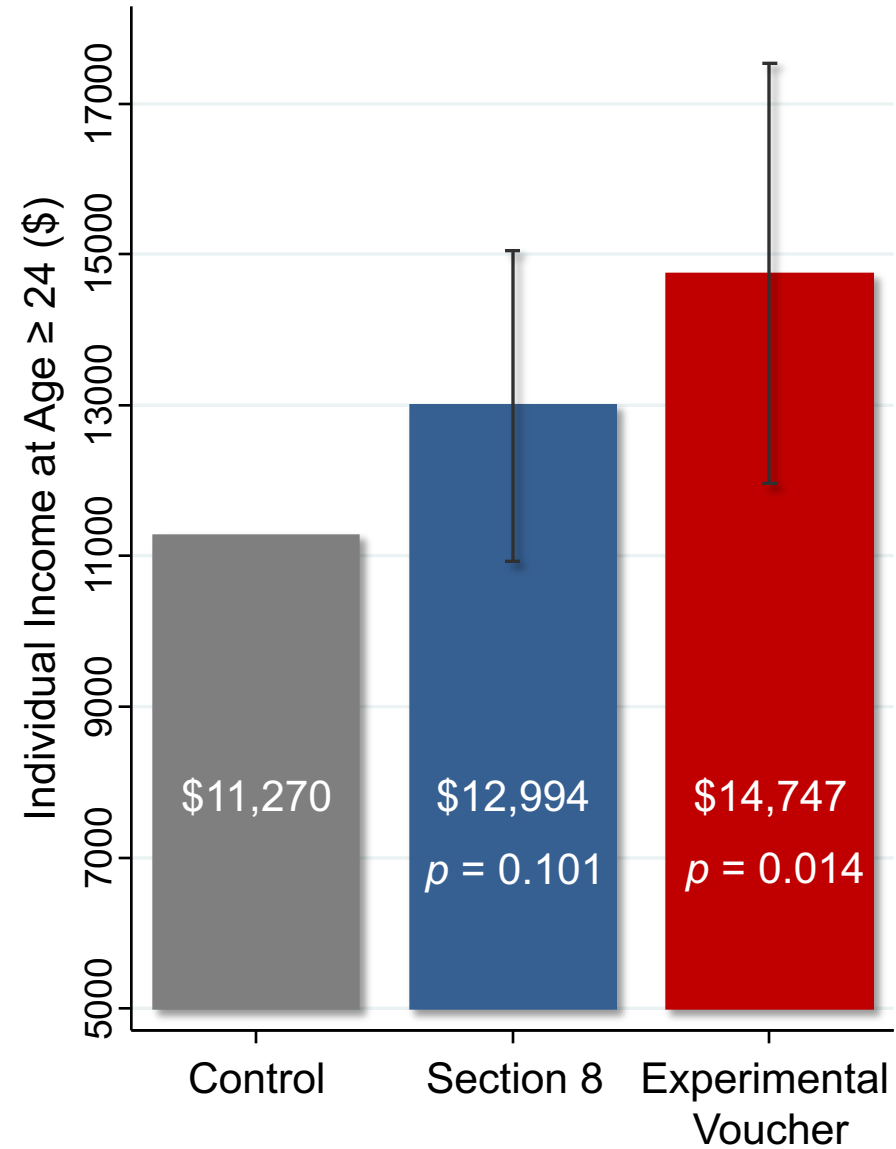
- Now turn to impacts on outcomes in adulthood
- Begin by analyzing effects on children below age 13 at RA
- Start with individual earnings (W-2 earnings + self-employment income)
 - Includes those who don't file tax returns through W-2 forms
- Measured from 2008-12, restricting to years in which child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

Impacts of MTO on Children Below Age 13 at Random Assignment

(a) Individual Earnings (ITT)

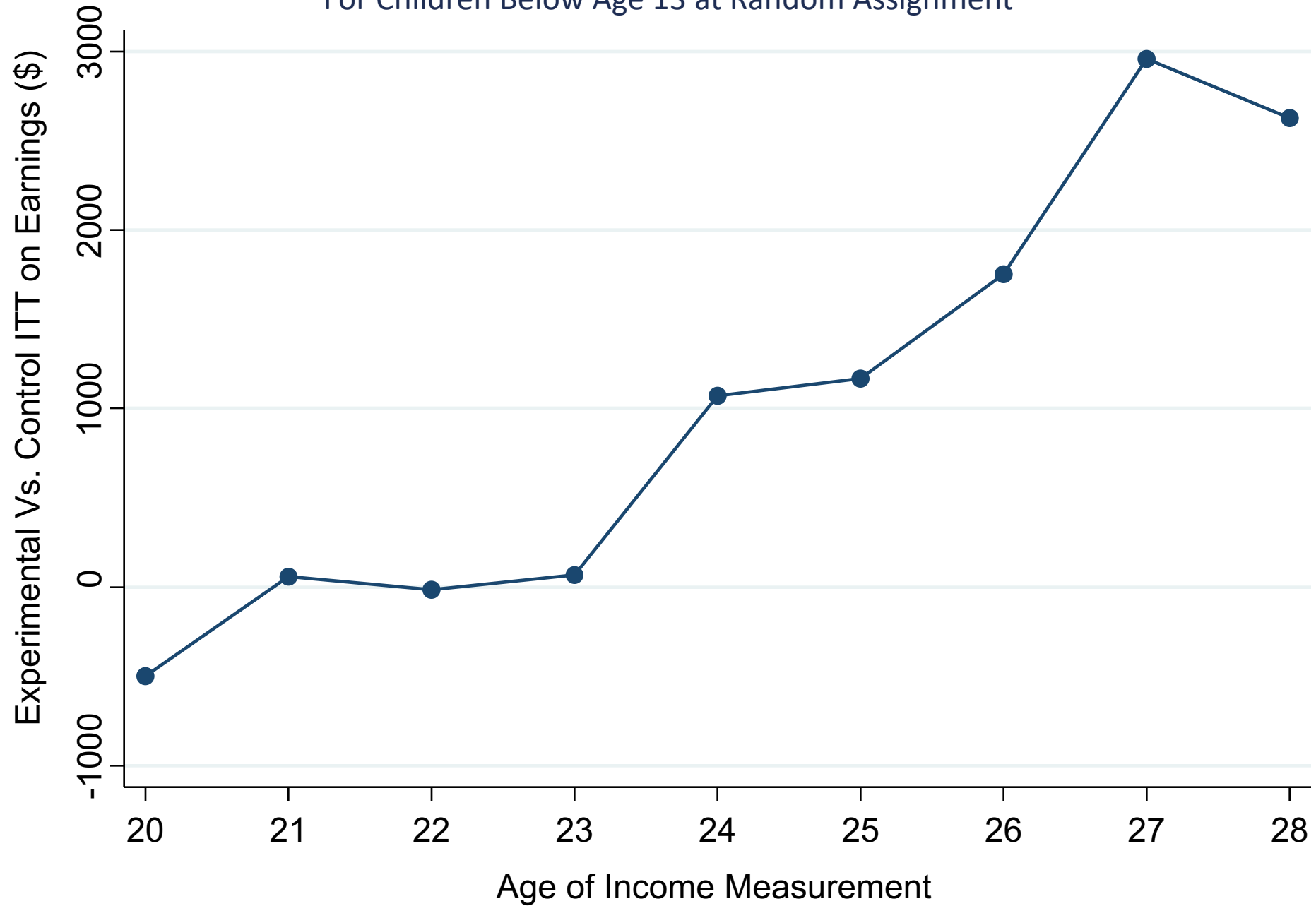


(b) Individual Earnings (TOT)



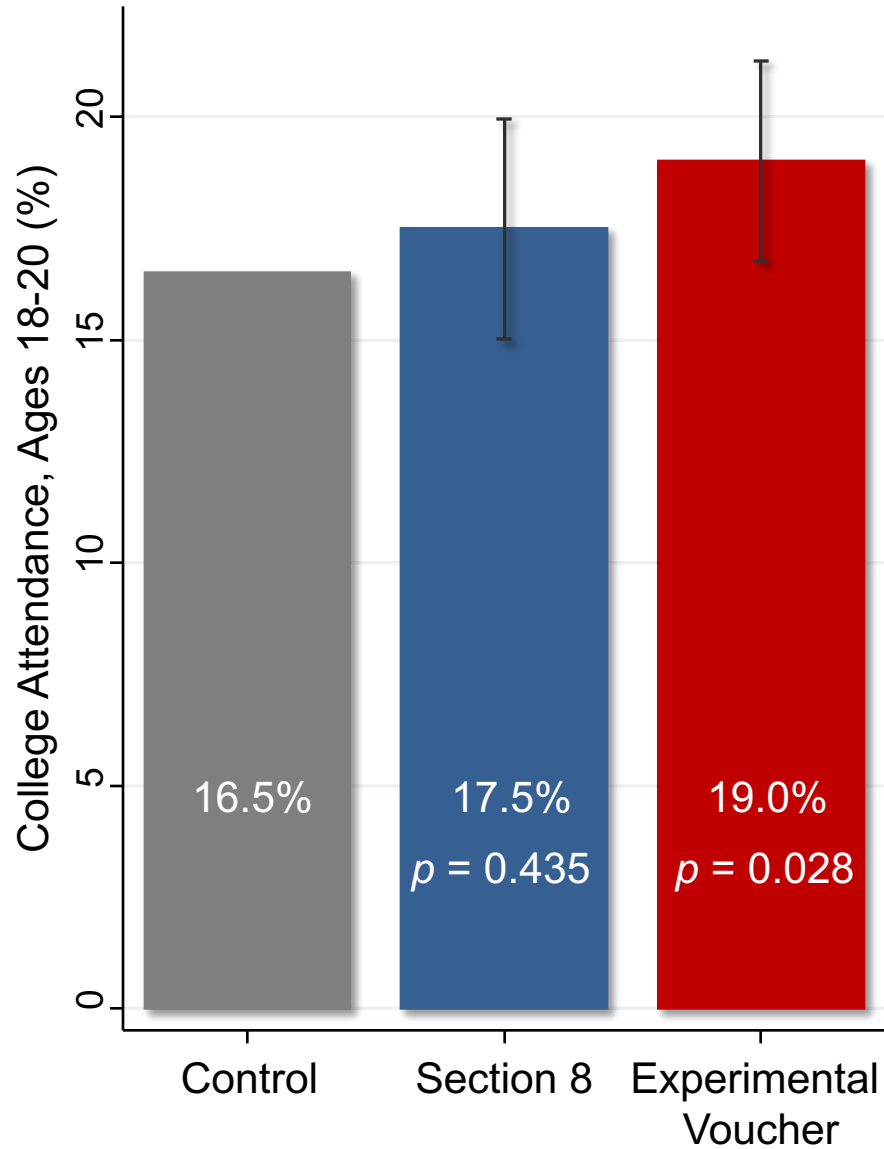
Impacts of Experimental Voucher by Age of Earnings Measurement

For Children Below Age 13 at Random Assignment

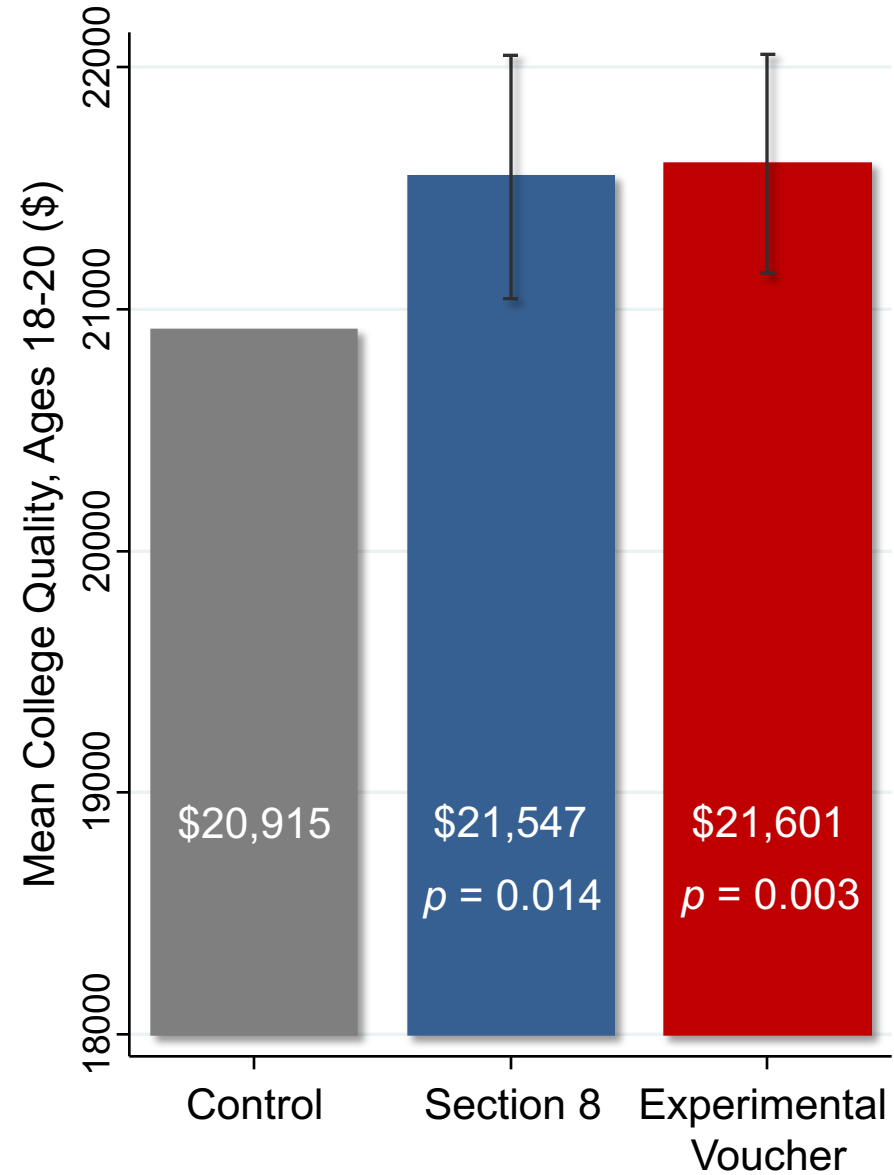


Impacts of MTO on Children Below Age 13 at Random Assignment

(a) College Attendance (ITT)

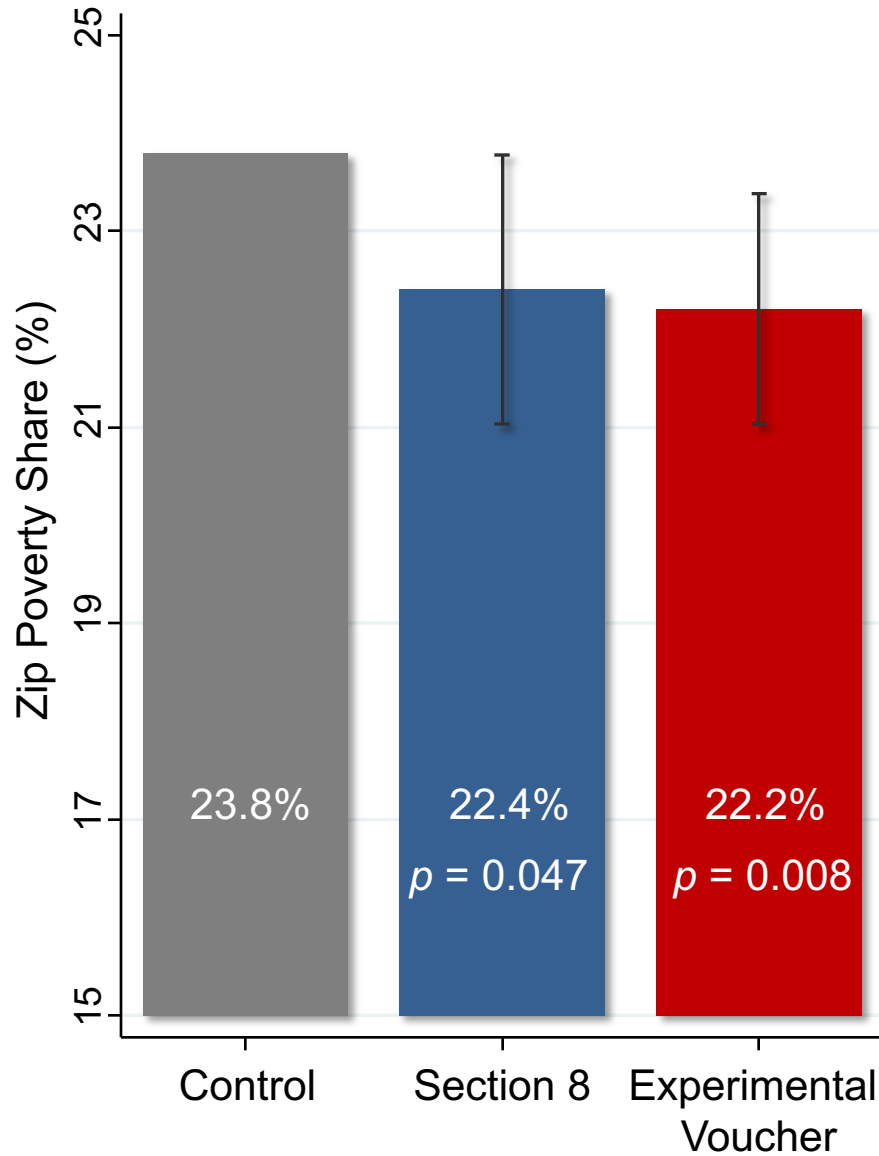


(b) College Quality (ITT)



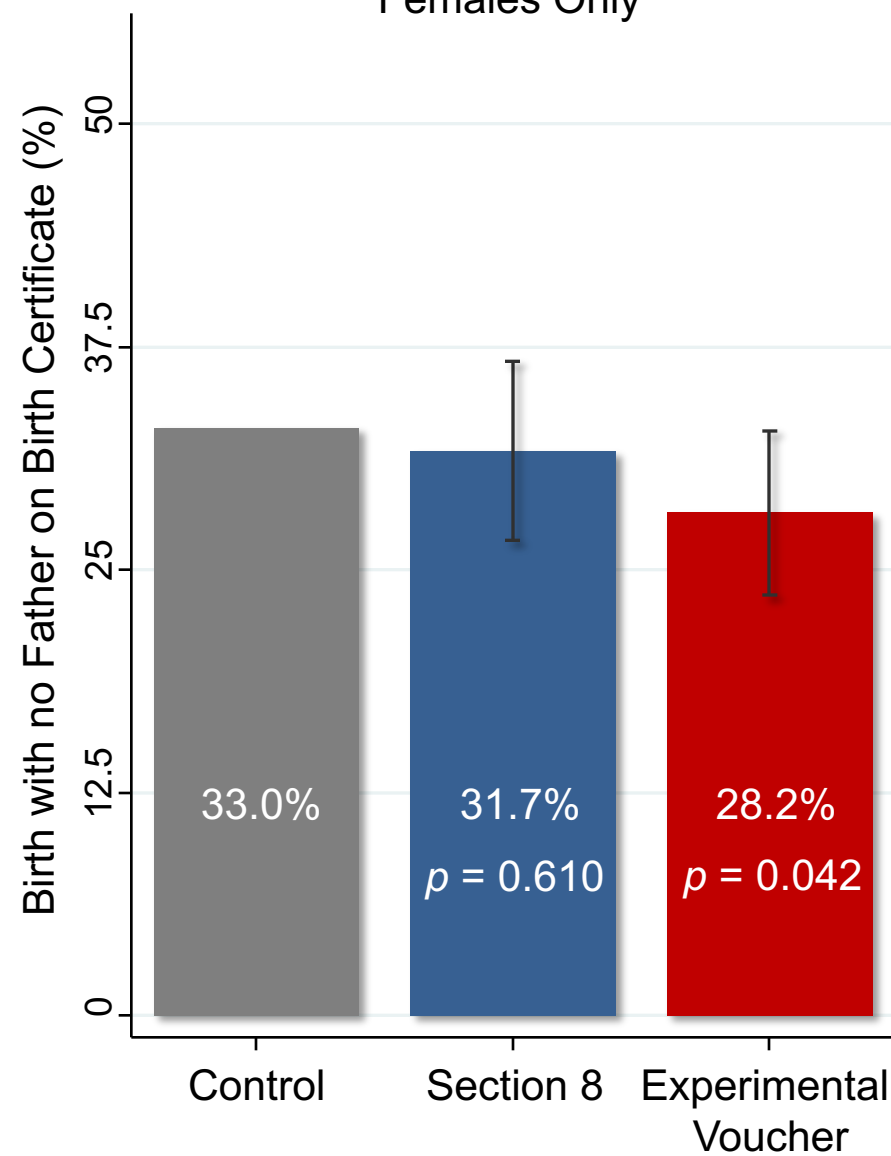
Impacts of MTO on Children Below Age 13 at Random Assignment

(a) ZIP Poverty Share in Adulthood (ITT)



(b) Birth with no Father Present (ITT)

Females Only

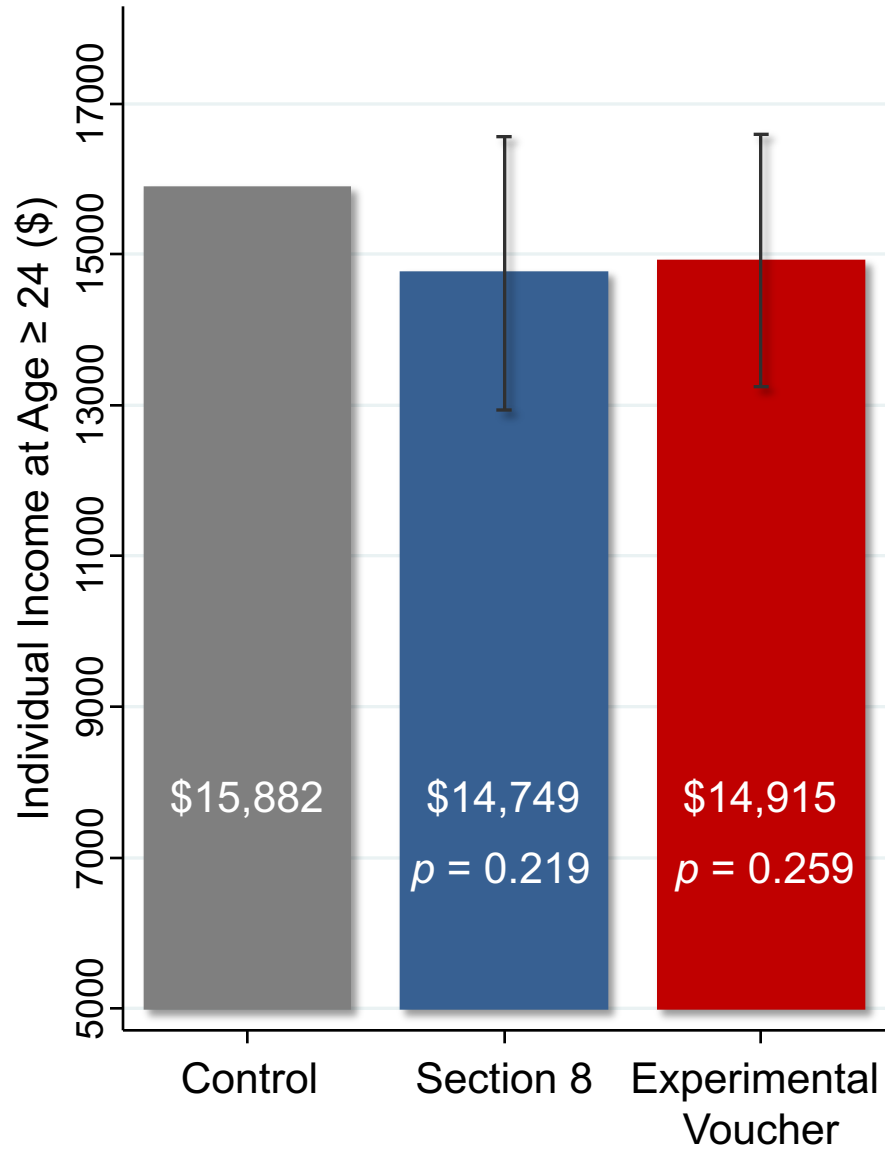


Treatment Effects on Older Children

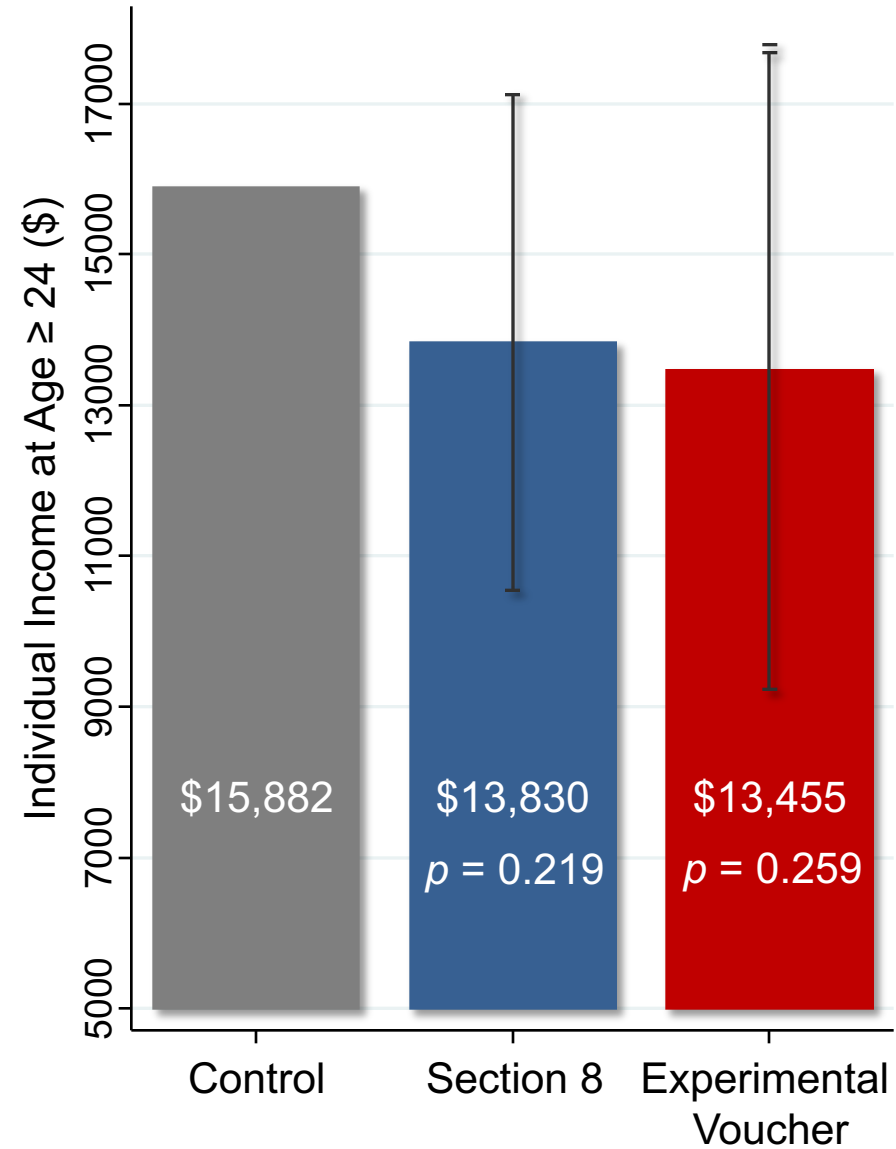
- Next, turn to children who were ages 13-18 at random assignment
 - Replicate same analysis as above

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

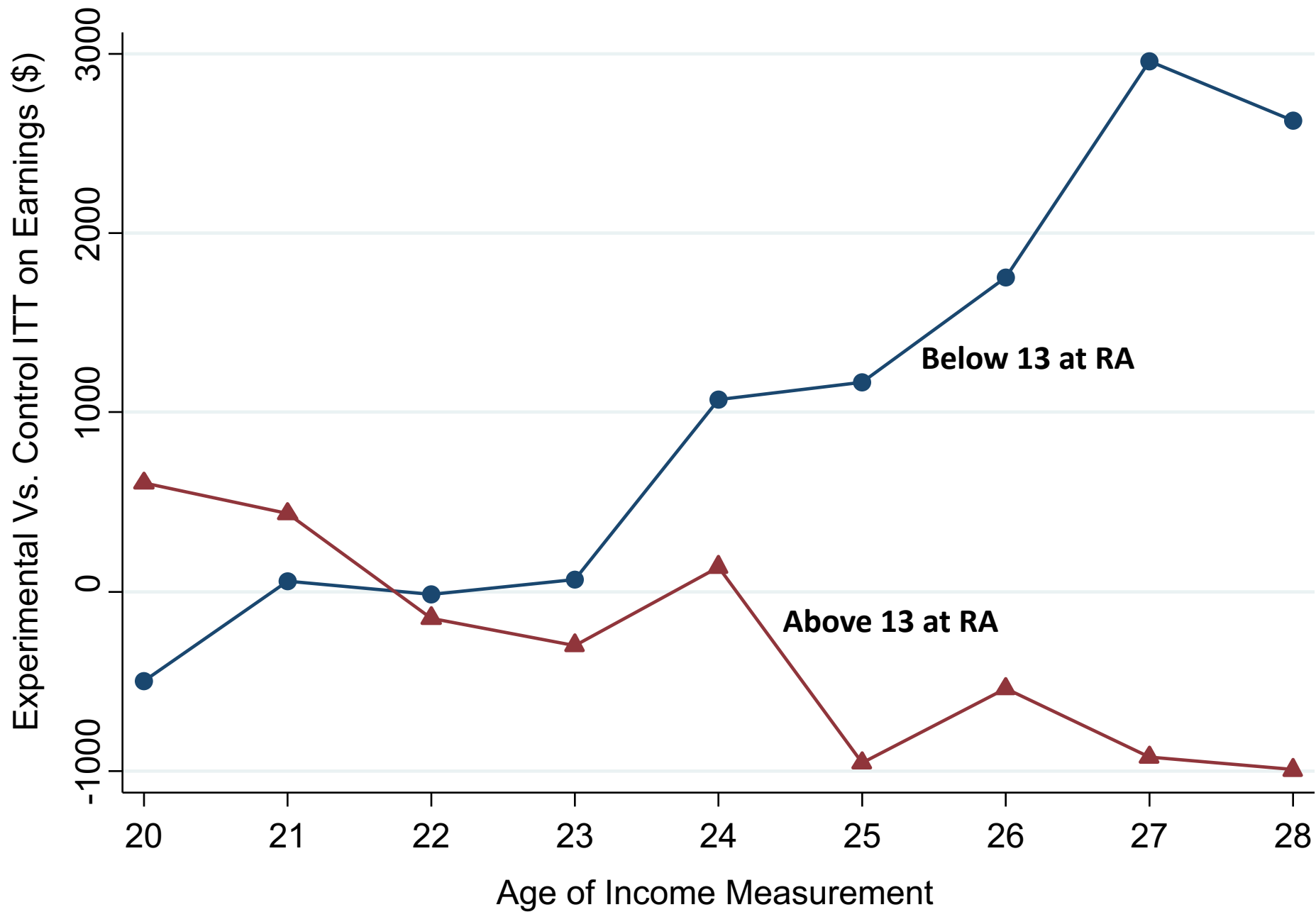
(a) Individual Earnings (ITT)



(b) Individual Earnings (TOT)

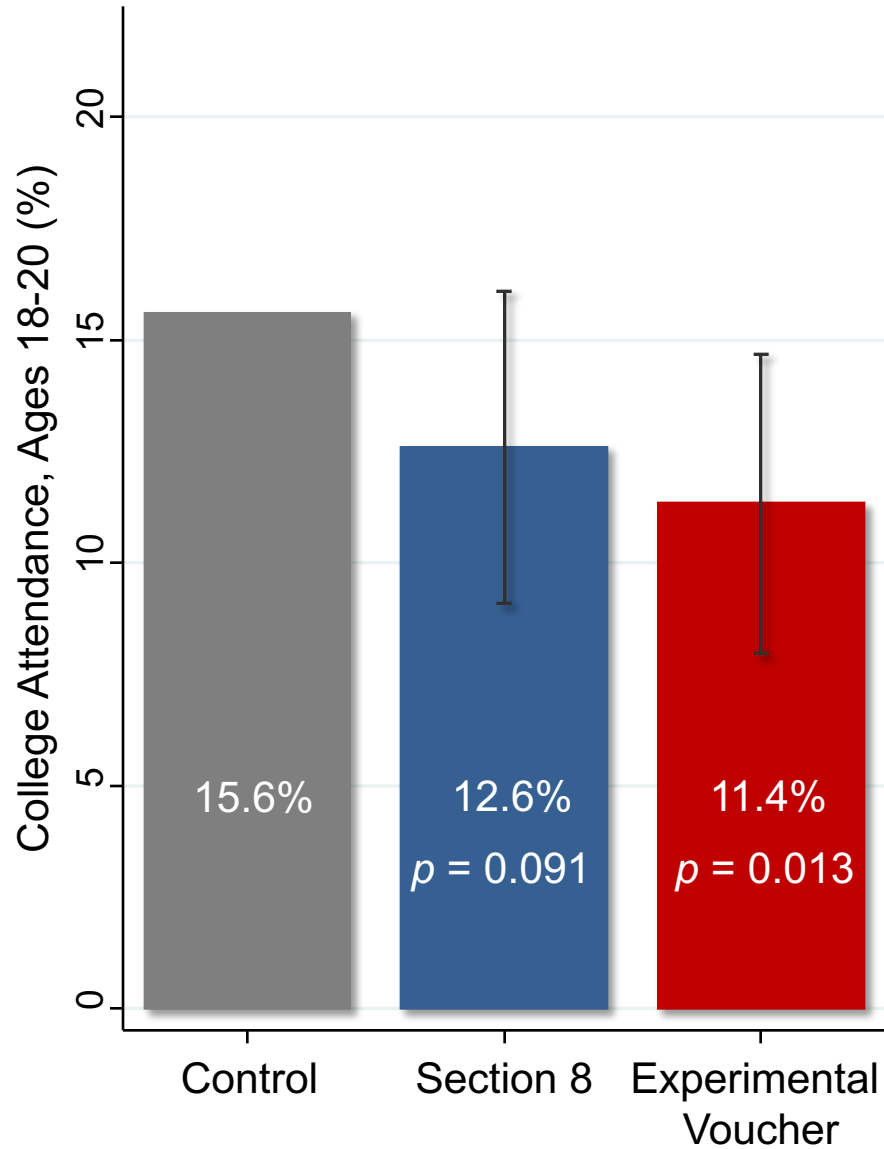


Impacts of Experimental Voucher by Age of Earnings Measurement

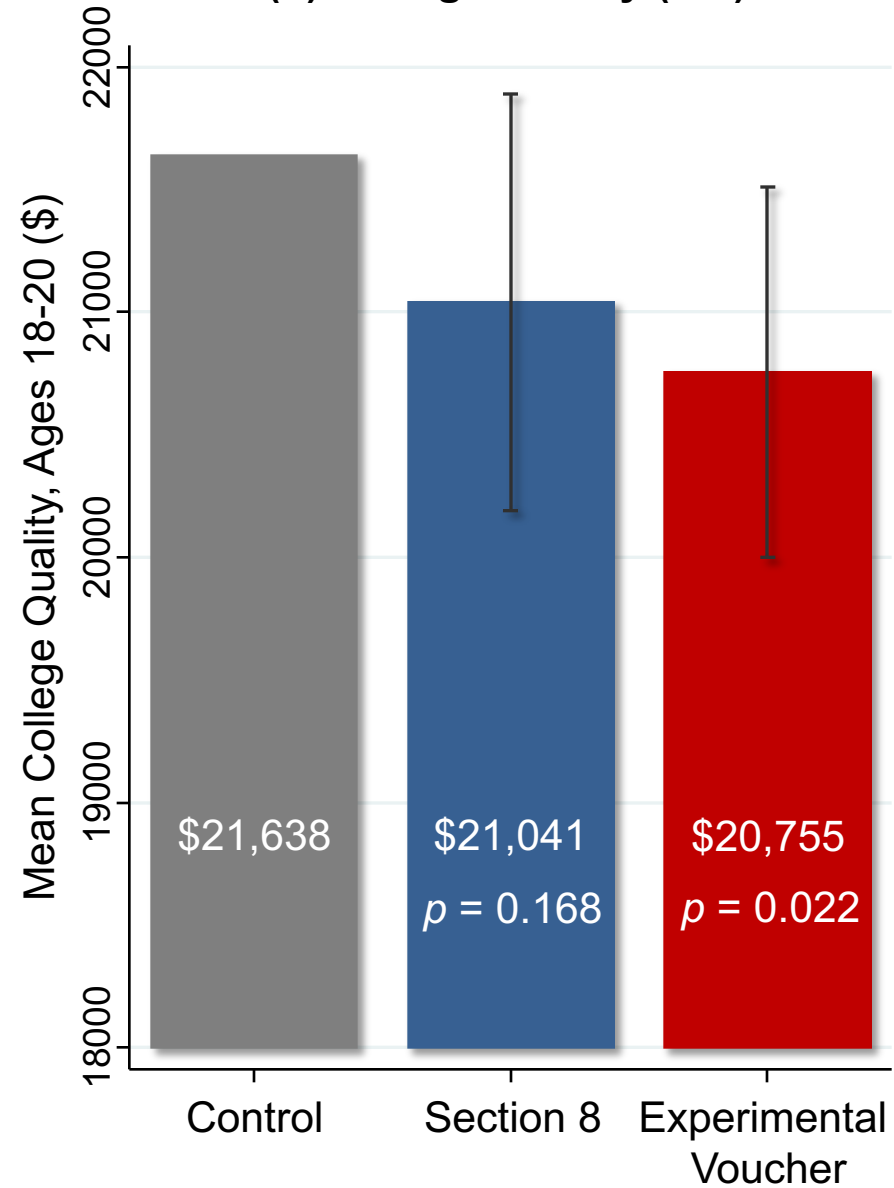


Impacts of MTO on Children Age 13-18 at Random Assignment

(a) College Attendance (ITT)

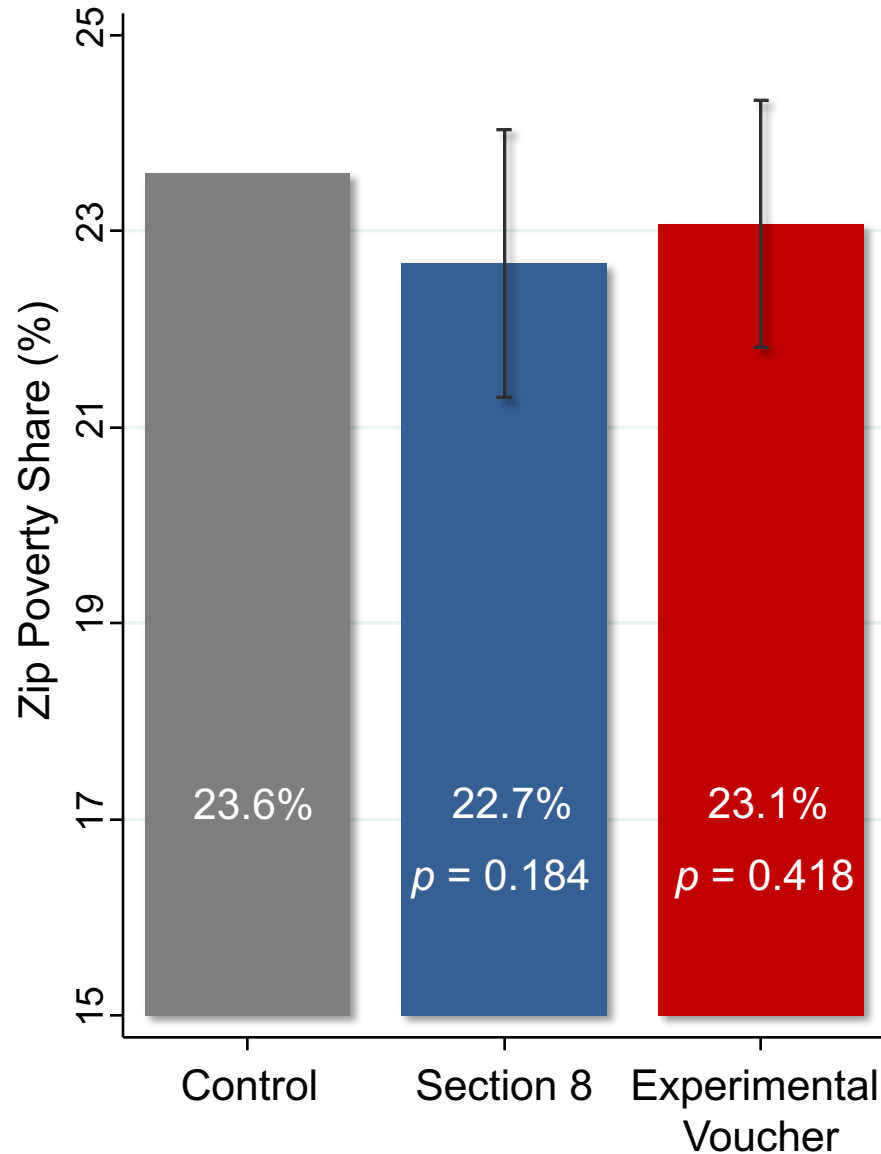


(b) College Quality (ITT)



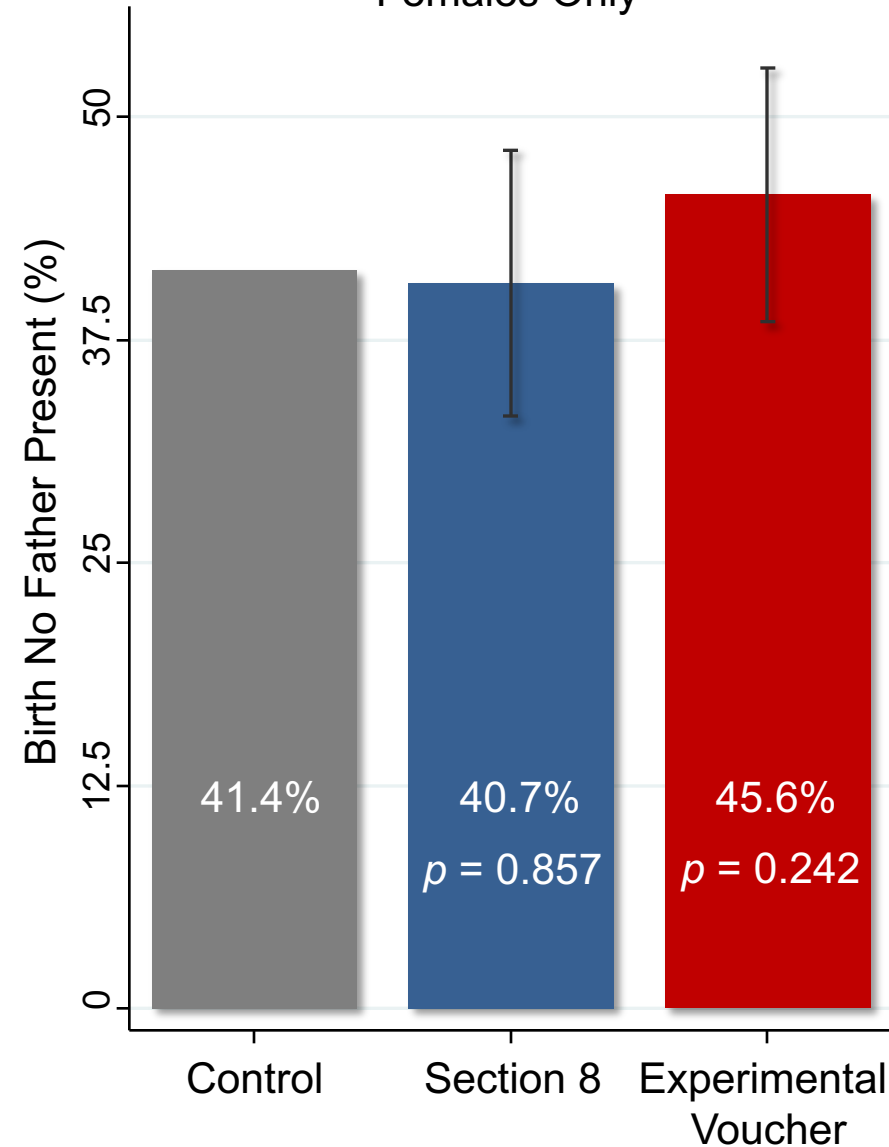
Impacts of MTO on Children Age 13-18 at Random Assignment

(a) ZIP Poverty Share in Adulthood (ITT)



(b) Birth with no Father Present (ITT)

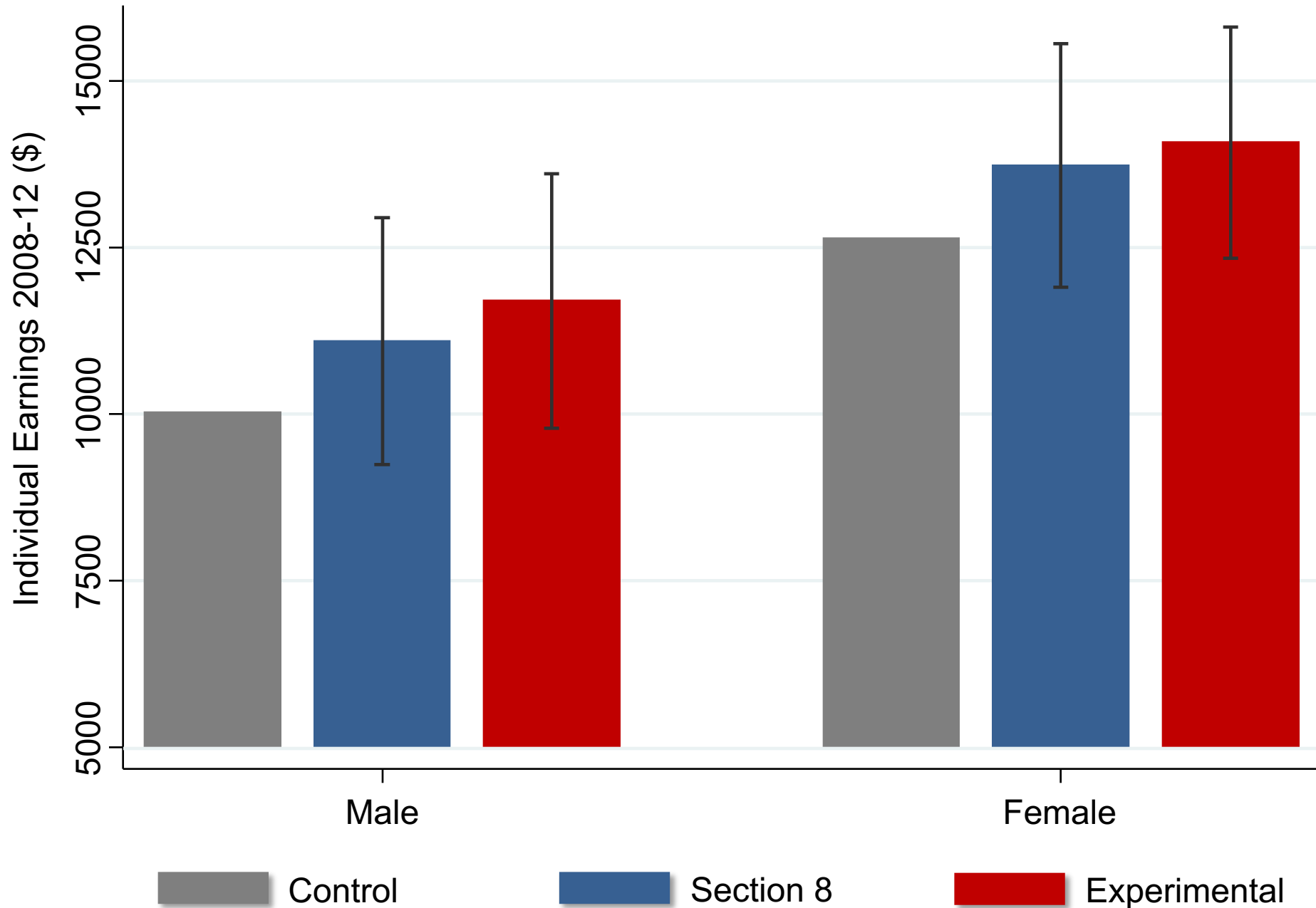
Females Only



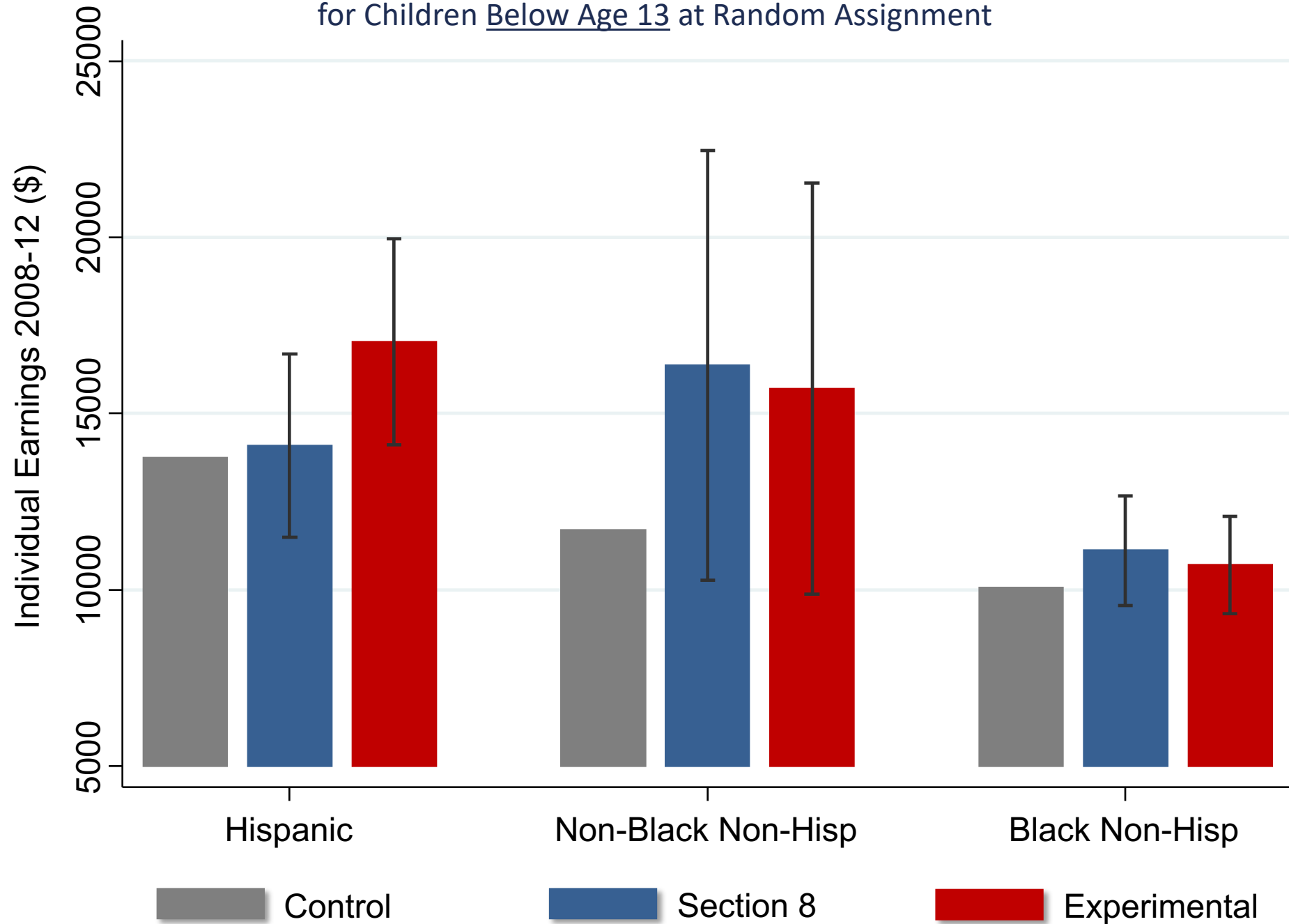
Heterogeneity

- 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

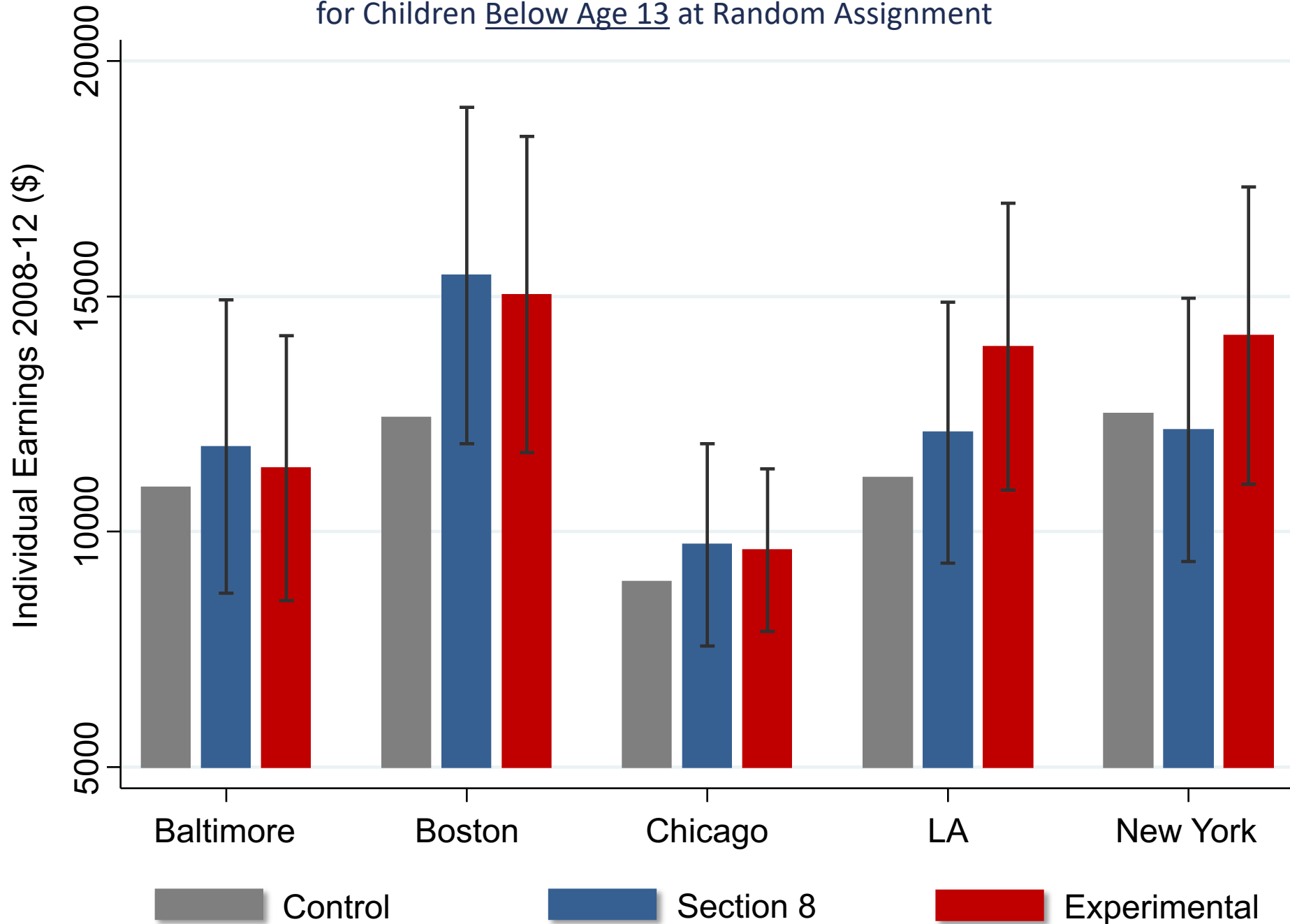
Impacts of MTO on Individual Earnings (ITT) by Gender
for Children Below Age 13 at Random Assignment



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



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)
<i>Panel A: p-values for Comparisons by Age Group</i>						
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
<i>Panel B: p-values for Comparisons by Age, Site, Gender, and Race Groups</i>						
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

p-value	Age		Race			Gender		Site					Min
	< 13	>= 13	Black	Hisp	Other	M	F	Balt	Bos	Chi	LA	NYC	
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

p-value	Age		Race			Gender		Site					Min
	< 13	>= 13	Black	Hisp	Other	M	F	Balt	Bos	Chi	LA	NYC	
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
<u>Placebos</u>													
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

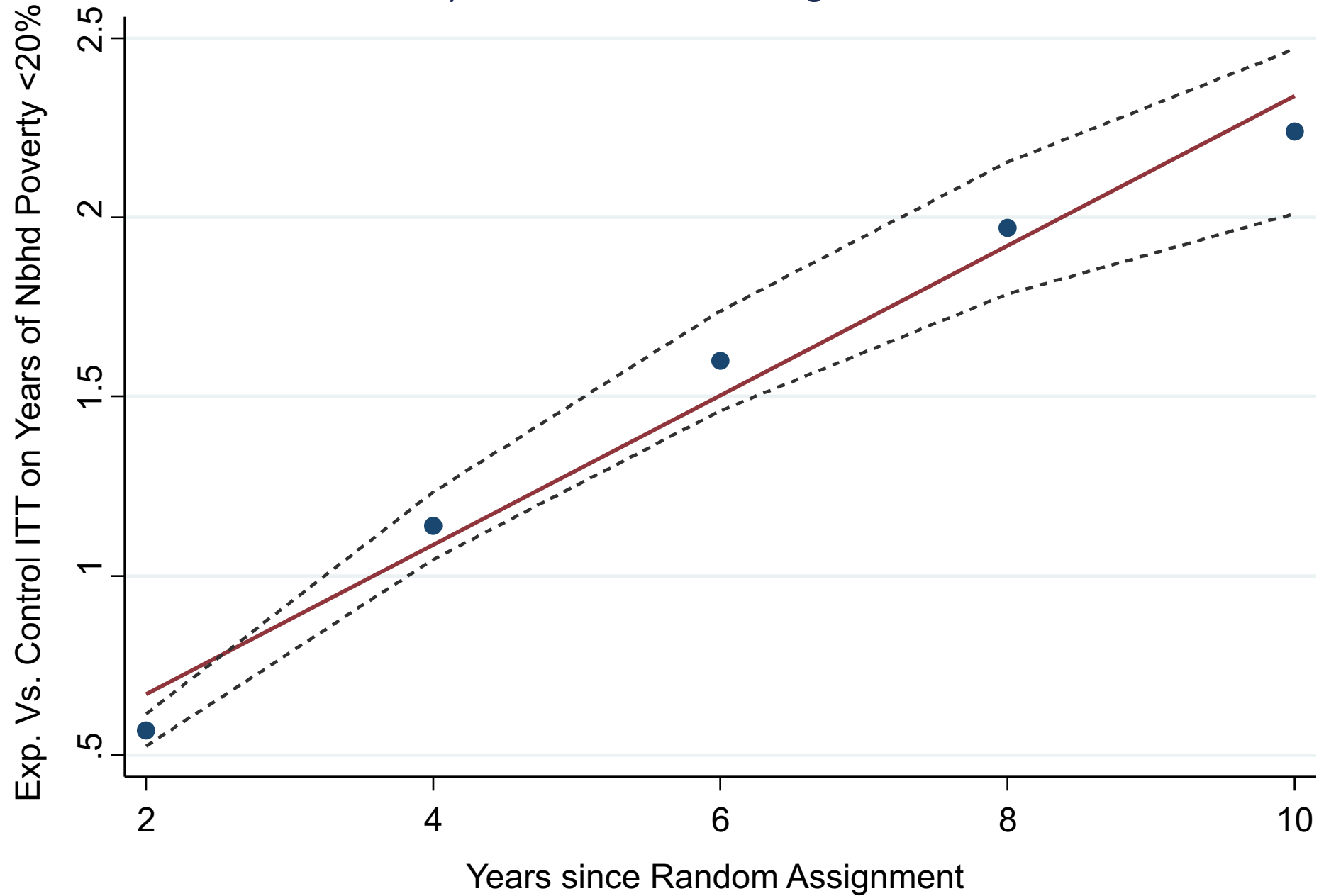
Multiple Hypothesis Testing

- 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

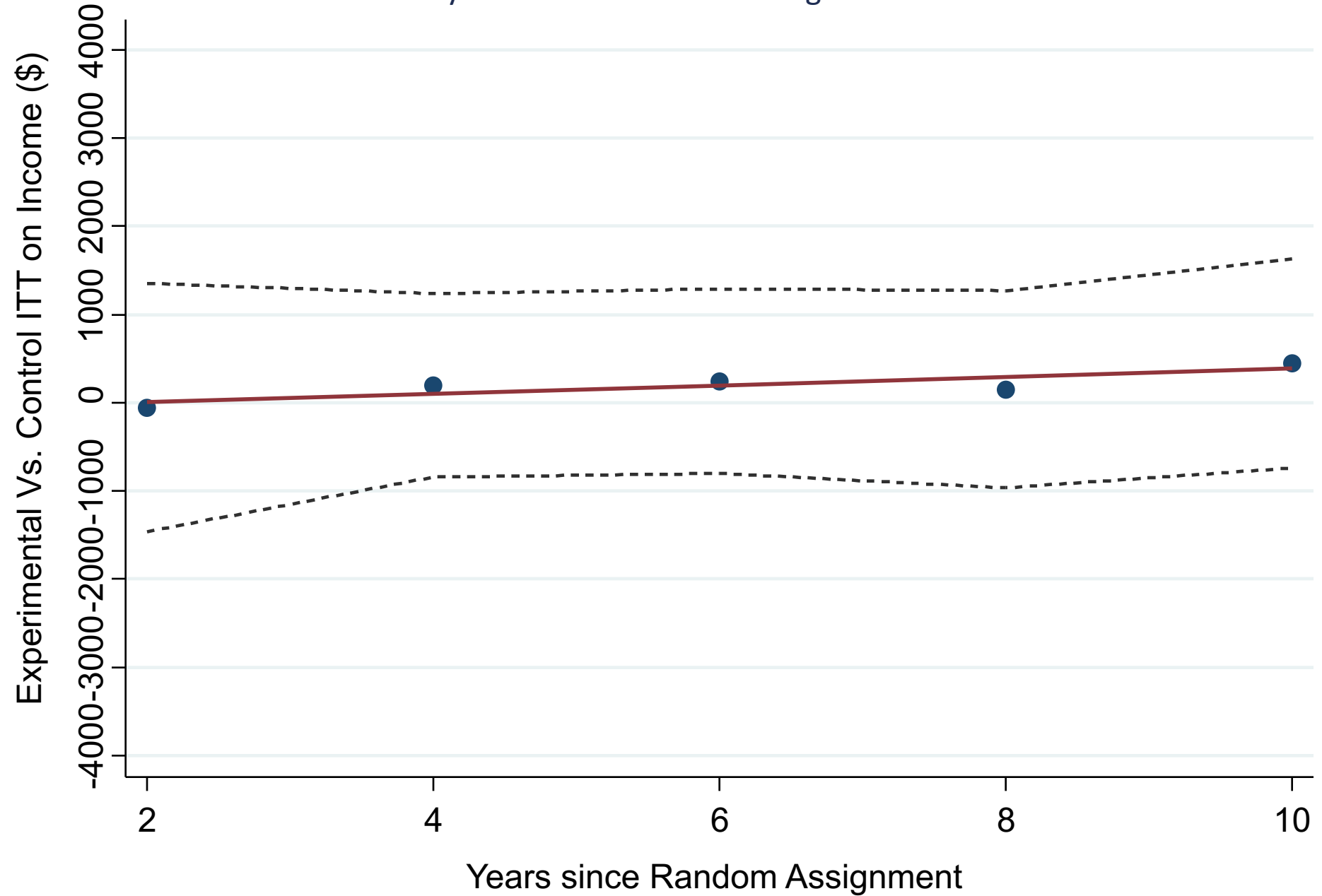
Treatment Effects on Adults

- 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

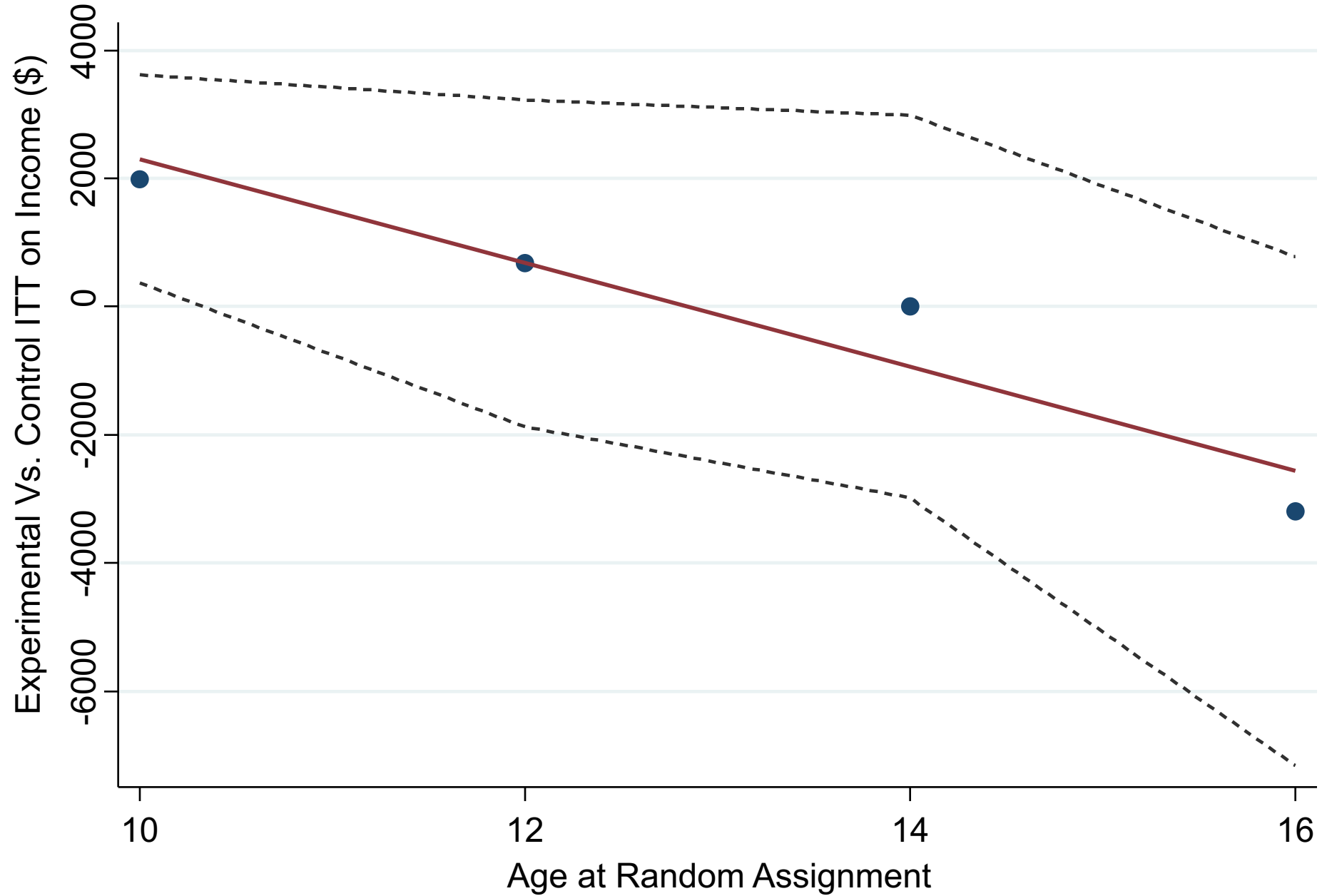


Impacts of Experimental Voucher on Adults' Individual Earnings by Years Since Random Assignment



Impacts of Experimental Voucher by Child's Age at Random Assignment

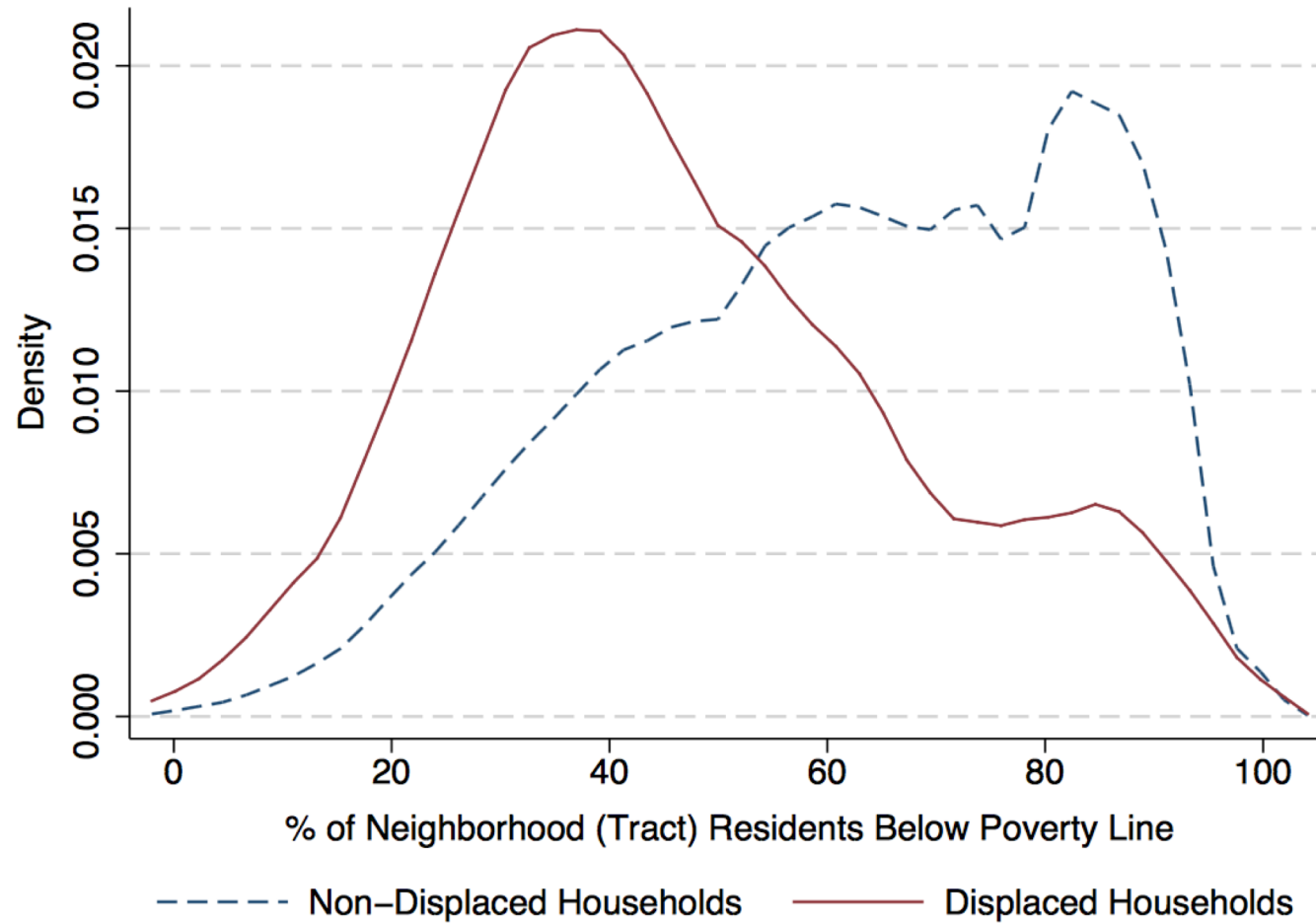
Household Income, Age ≥ 24 (\$)



Chyn (2018)

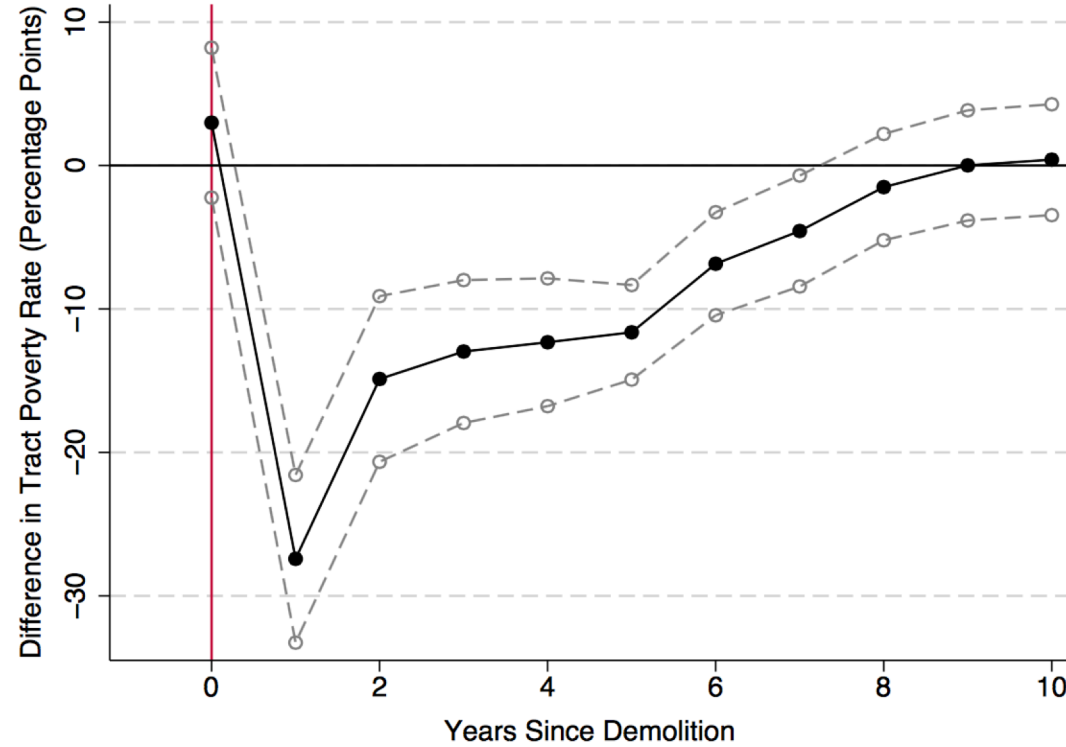
- Chyn (2018): “Moved to Opportunity: The Long-Run Effect of Public Housing Demolition on Labor Market Outcomes of Children”
 - Hope IV demolitions
 - Previous work documents impacts on test scores (Jacob 2004: “Public Housing, Housing Vouchers, and Student Achievement: Evidence from Public Housing Demolitions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



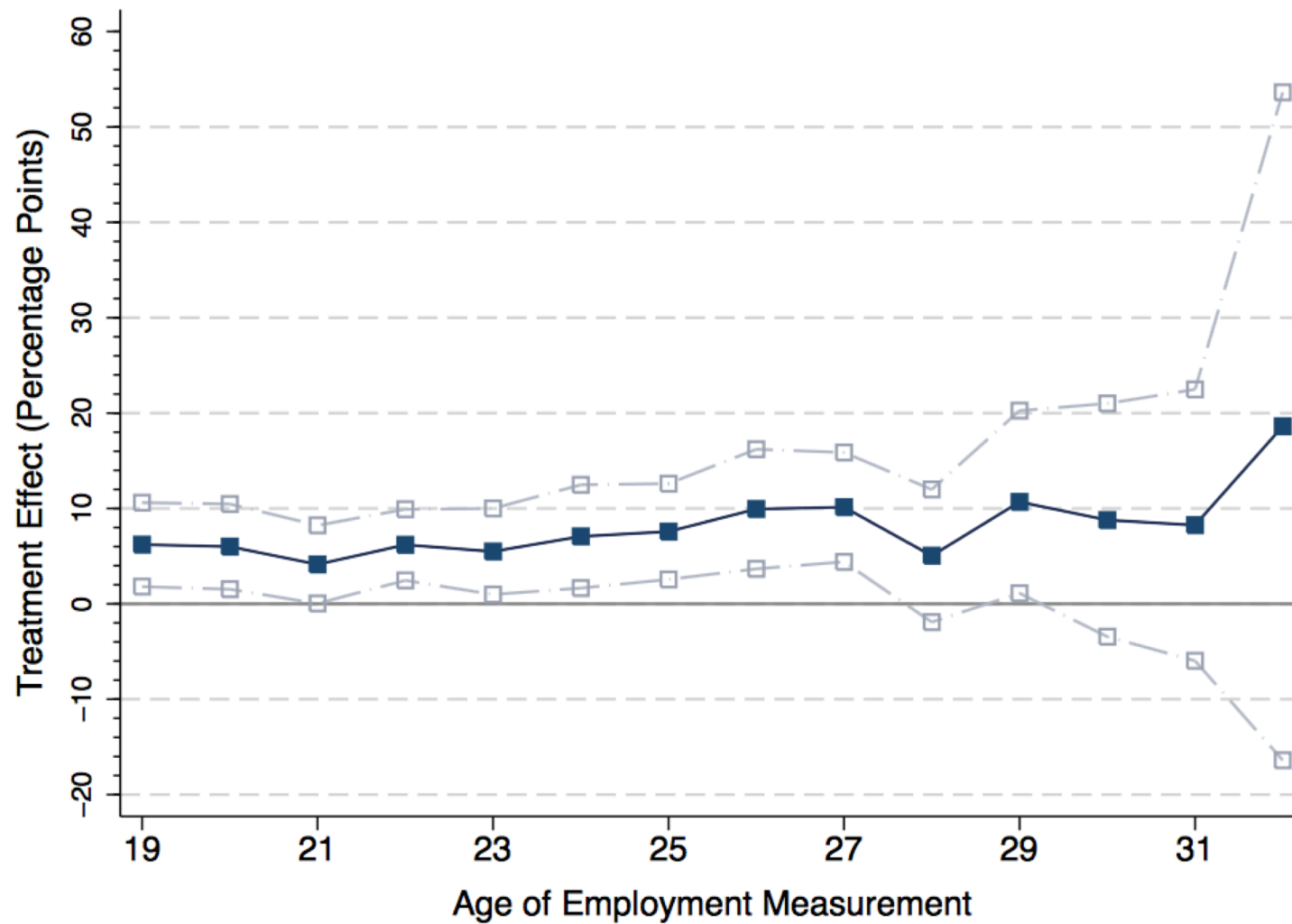
Notes: This figure illustrates the change over time in the difference in neighborhood poverty rate between displaced (treated) and non-displaced (control) households with children (age 7 to 18 at baseline). Specifically, I plot (in solid black) the set of coefficients π_y for $y \in \{0, \dots, 10\}$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

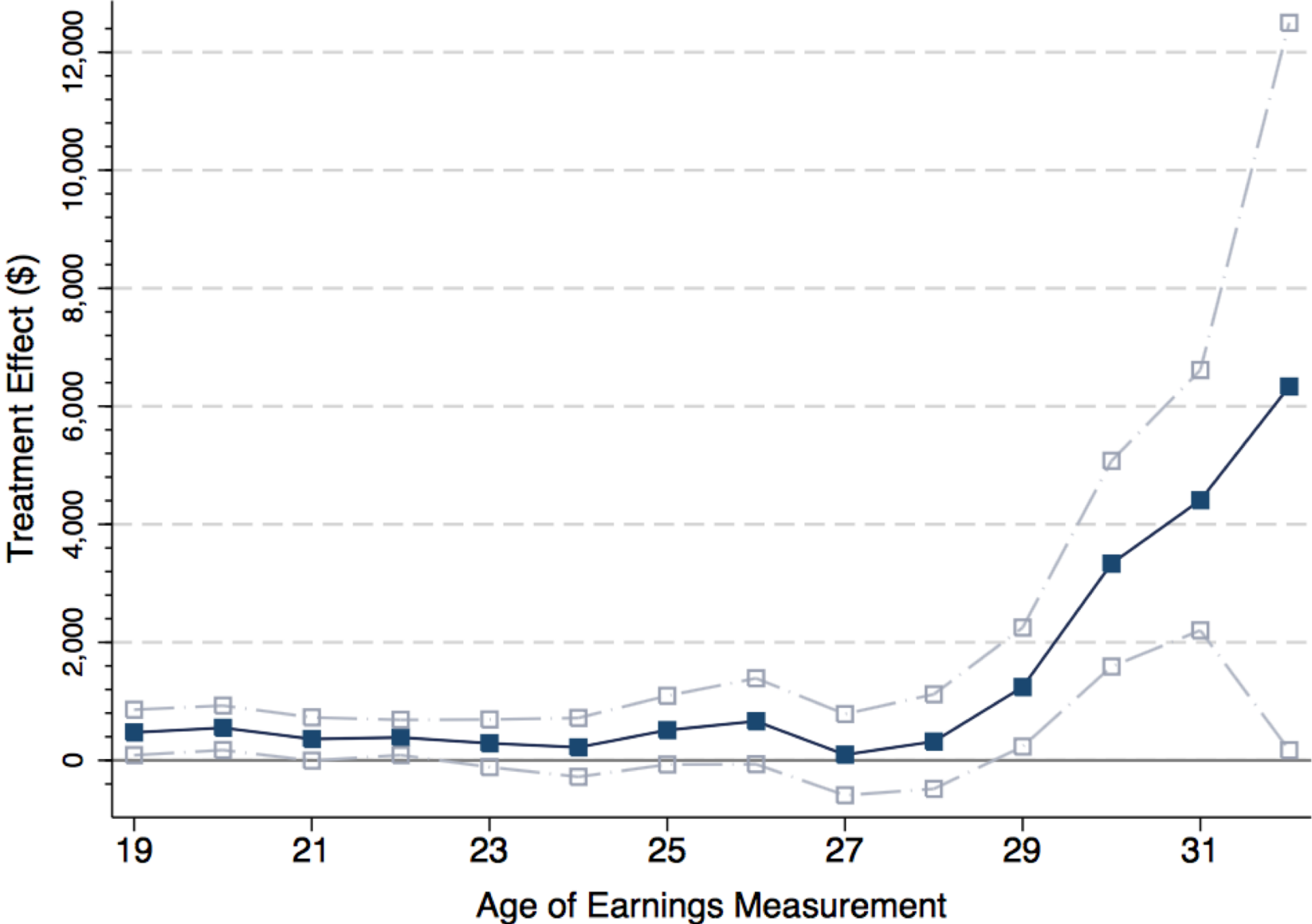
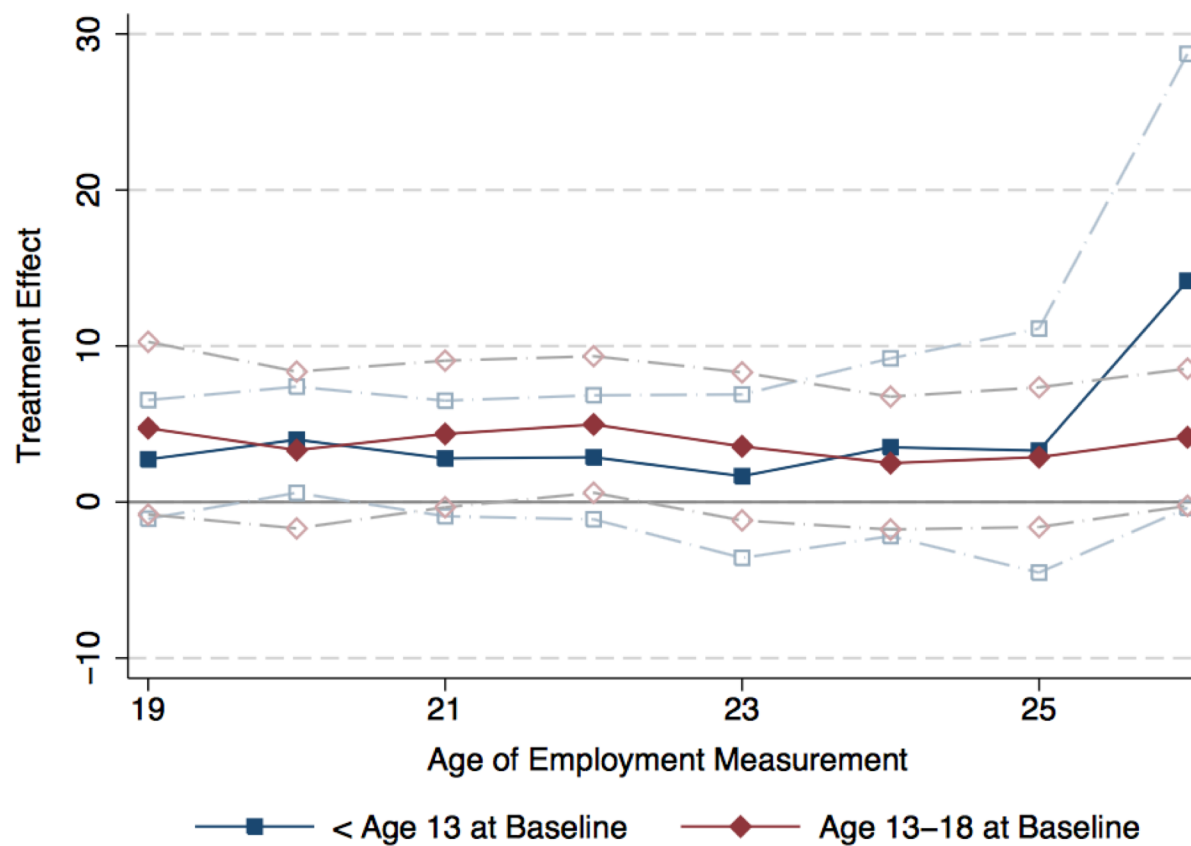
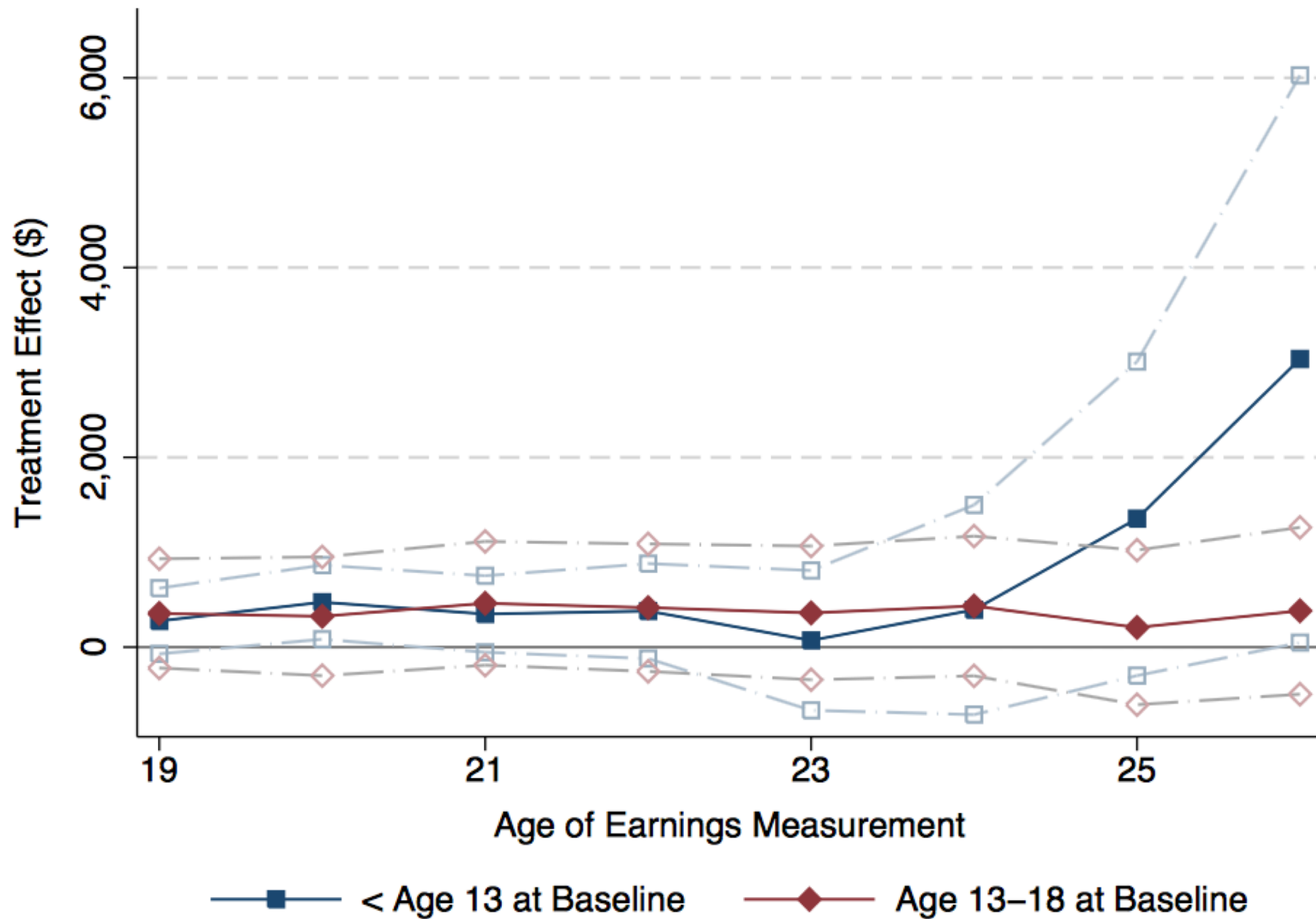


Figure 4: Younger vs Older Children: Labor-Market Treatment Effects by Age of Measurement

(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

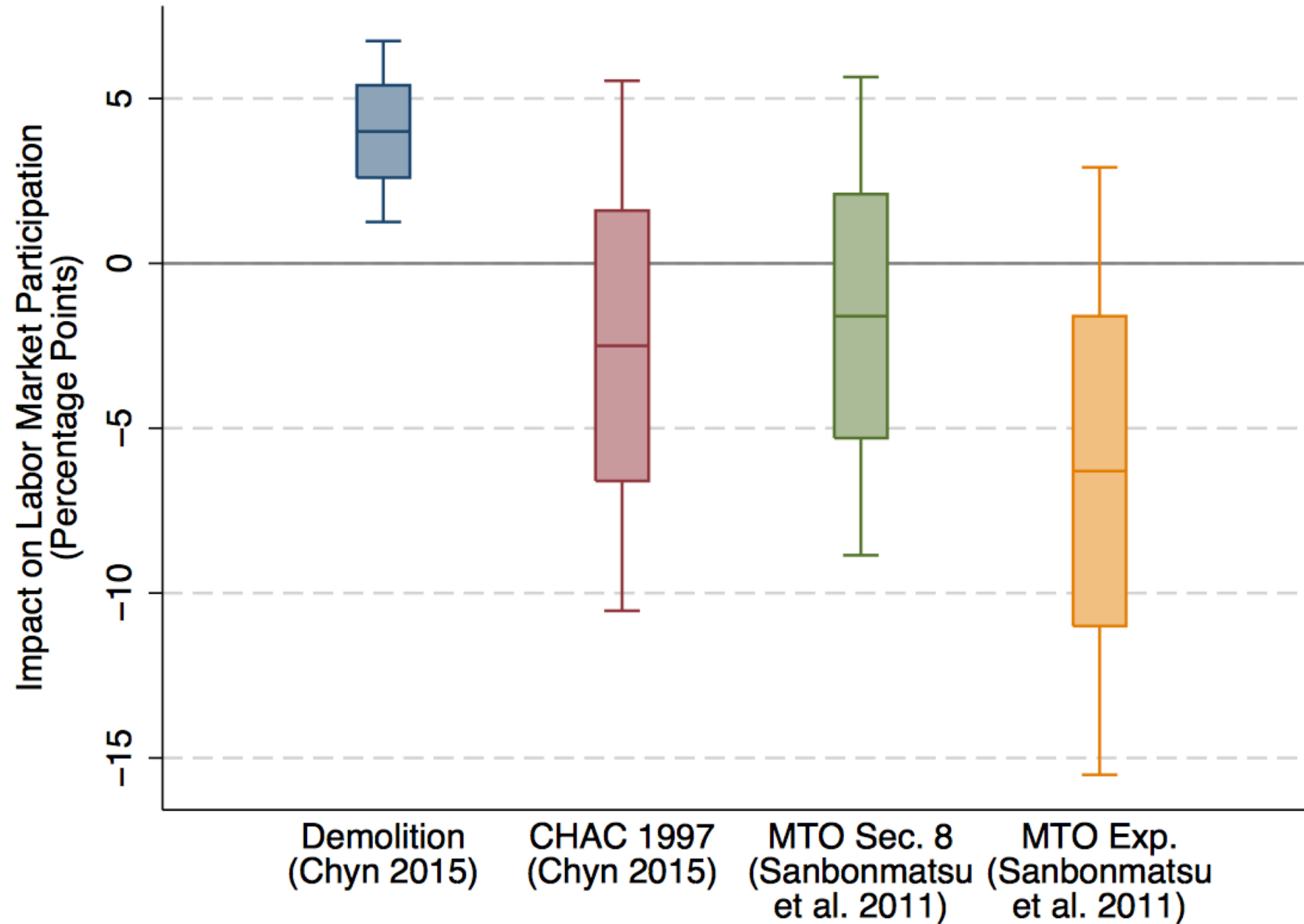
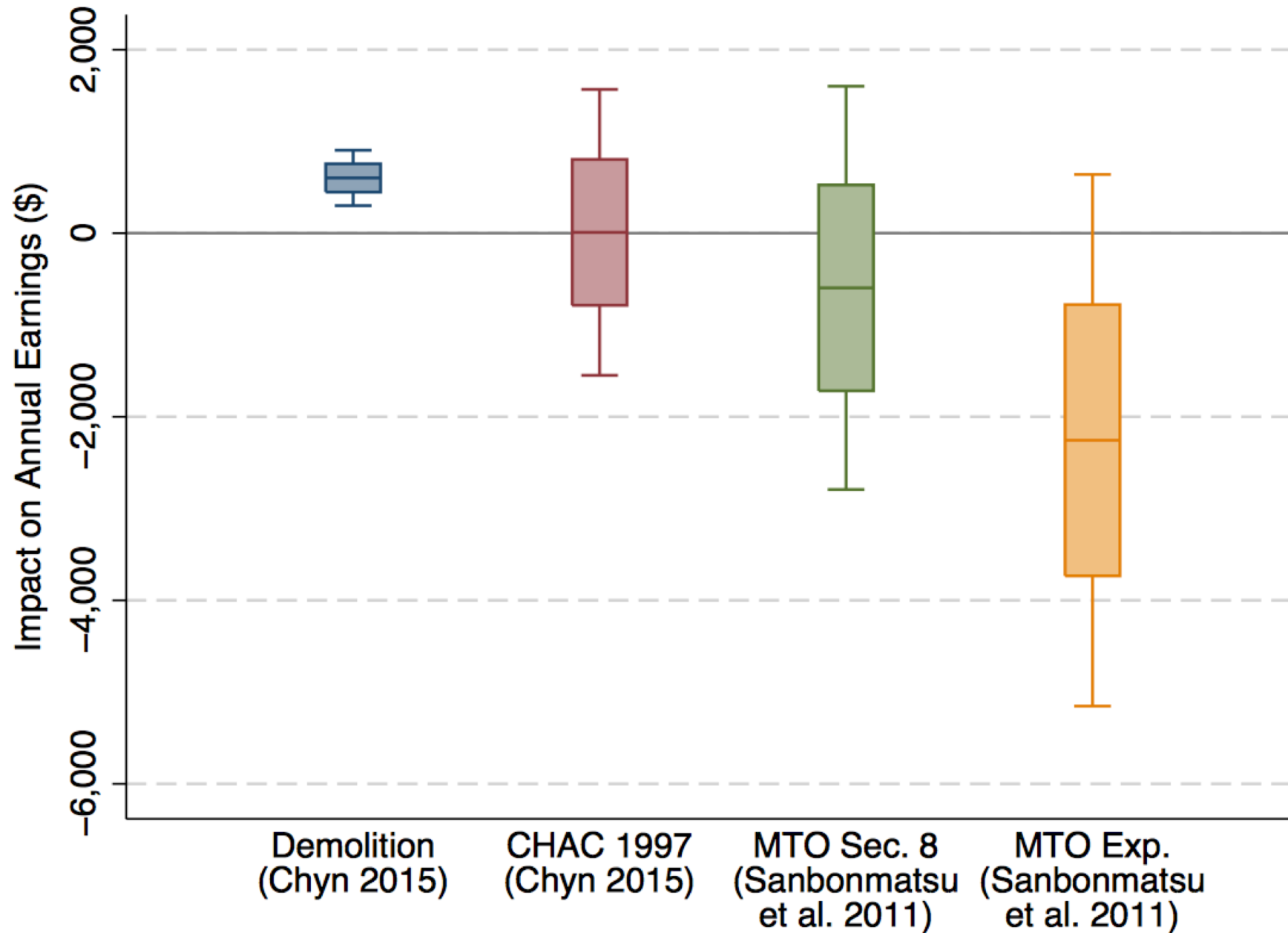


Figure 7: Effects on Adult Earnings of Children Across Studies



Housing Demolitions in Chicago

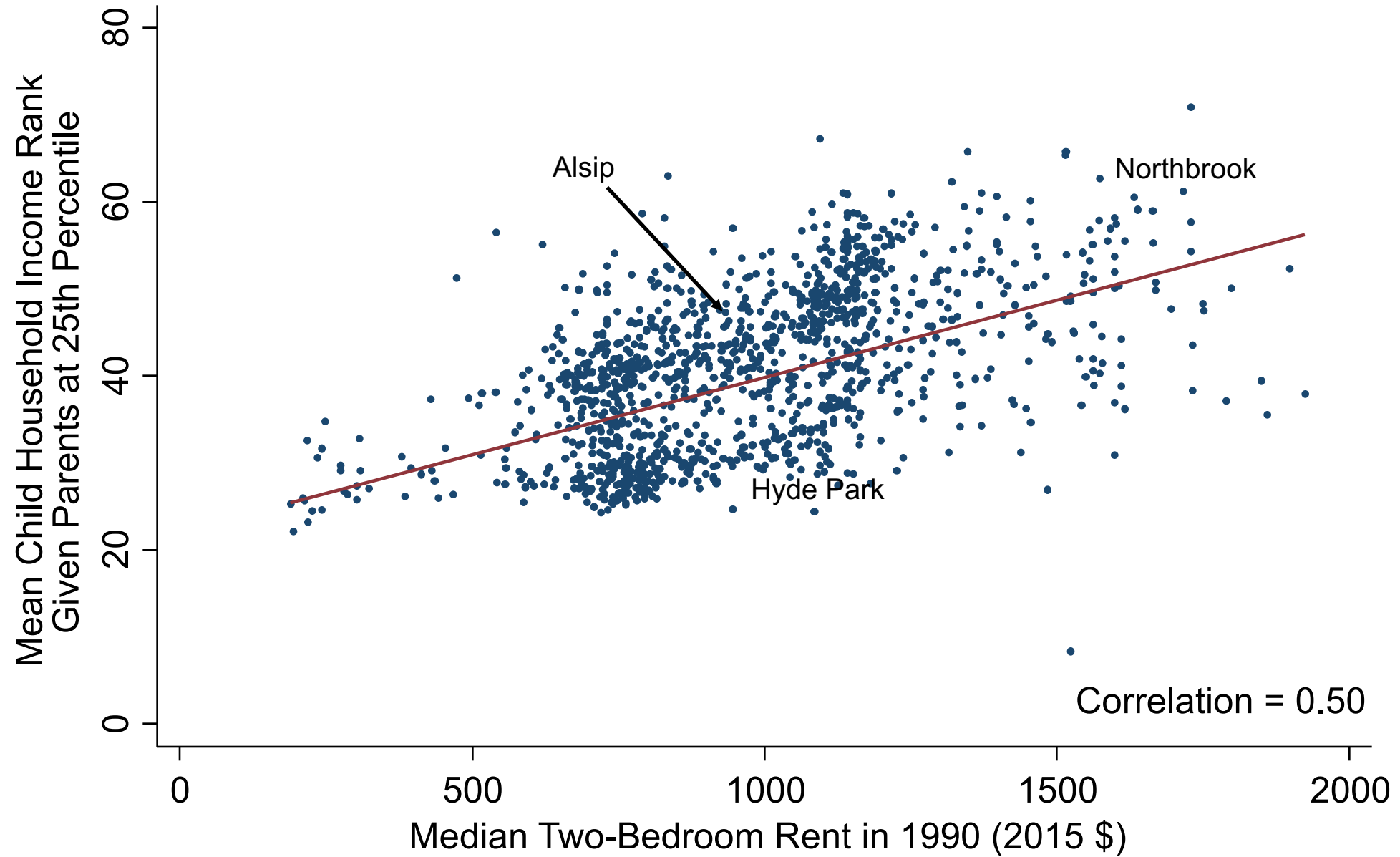
- Why no impact of Section 8 vouchers?
- Potential for “Reverse Roy” sorting model?
 - Those forced to move have higher returns than “compliers” from vouchers
 - Forcing people to move delivers larger impacts?
- Alternative story: Section 8 and demolition is a different treatment
 - Section 8 does not 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

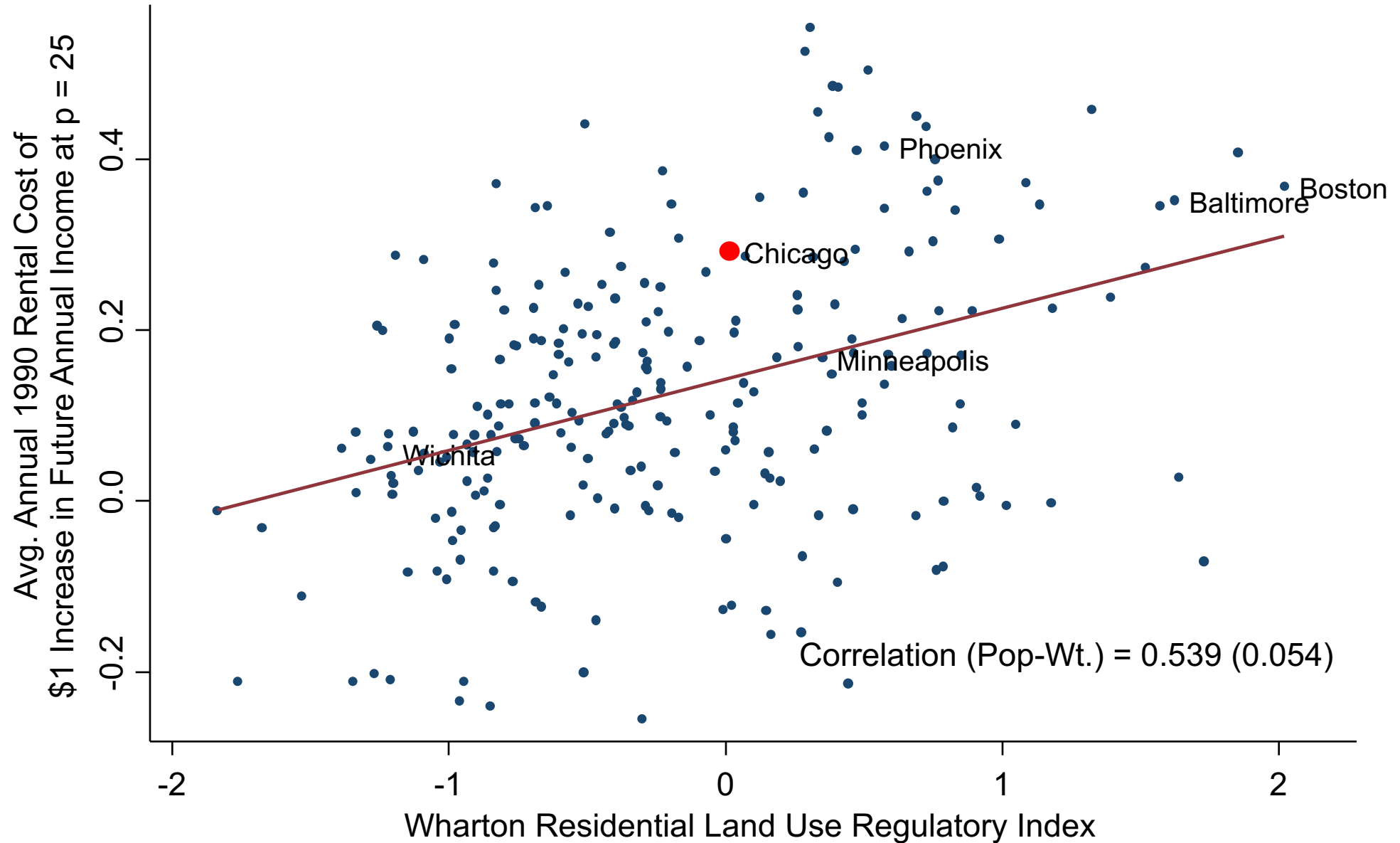
Children with Parents at 25th Percentile



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

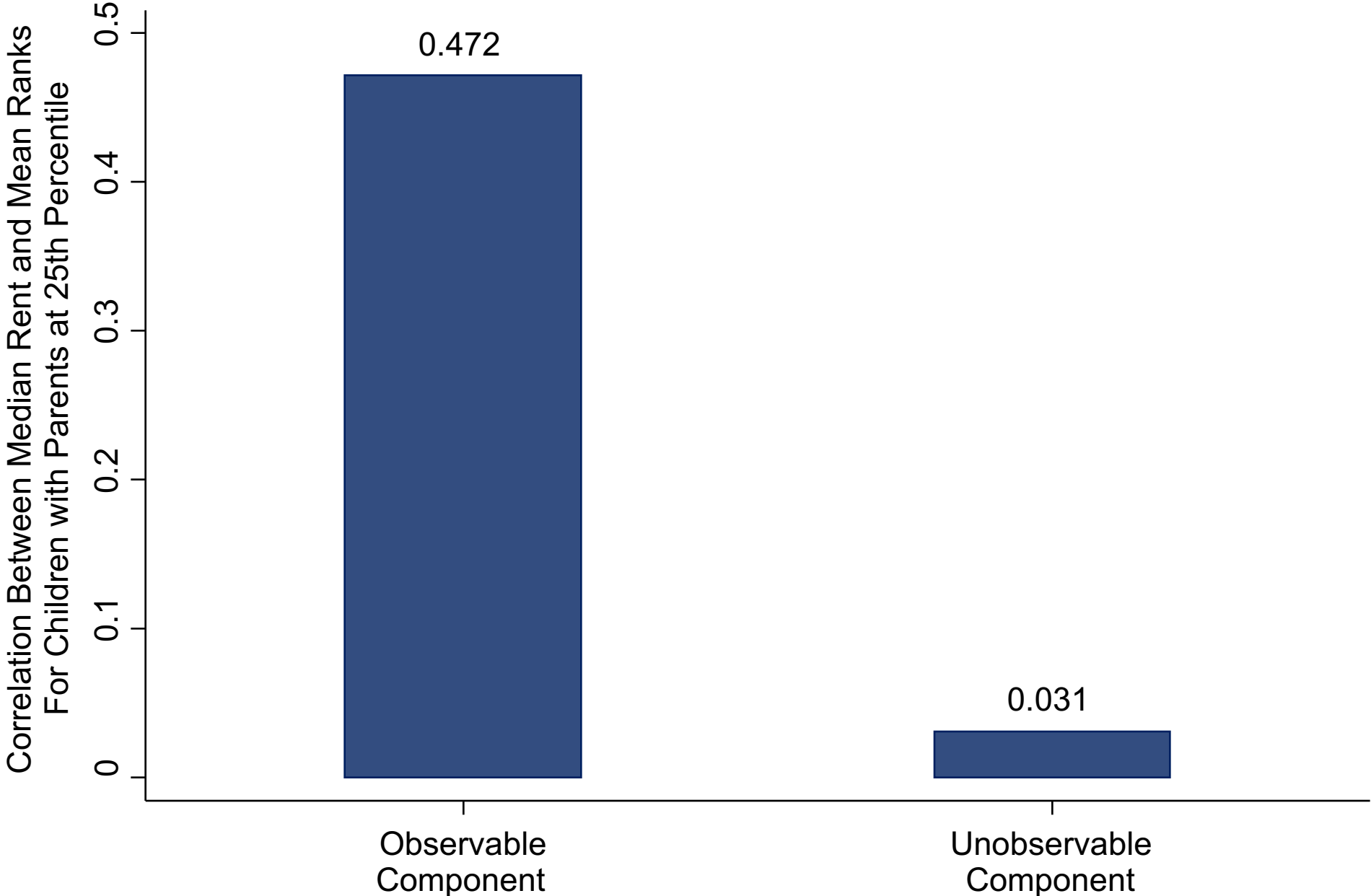


Note: figure excludes Honolulu for scaling purposes

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



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?