Urban Economics

Lecture 9: Segregation and Neighborhood Effects

Spring 2024
Tuukka Saarimaa

Outline

In this lecture, we discuss neighborhood effects

- Does living in a deprived neighborhood make residents materially worse off than they would otherwise have been over the long run?

- Why is it so difficult to study these (causal) effects?

The lecture does not follow the textbook

Segregation and neighborhood effects

Causal effects of neighborhoods vs. sorting

We know from prior research that children who grow up in poor neighborhoods do less well in school and earn less as adults than children who grow up in affluent neighborhoods

However, there are two very different explanations for this finding:

1. Sorting: different people live in different neighborhoods
2. Causal effects: neighborhoods have a causal effect on children's outcomes later in life (peers, public services, role models etc.)

From a policy perspective, it is crucial to know which of these explanations is the correct one (of course both can be true)

What causes residential segregation?

Residential segregation **is the result of differences in neighborhood quality and household income in the city**

- Nice neighborhoods are scarce and access to these neighborhoods is rationed through house prices and rents

- Rich can outbid the poor for nice locations

- Sorting of rich and poor into different neighborhoods is an almost inevitable consequence of residential choices in a market system

The way residential segregation takes place is the main reason why it is so difficult to study neighborhood effects!
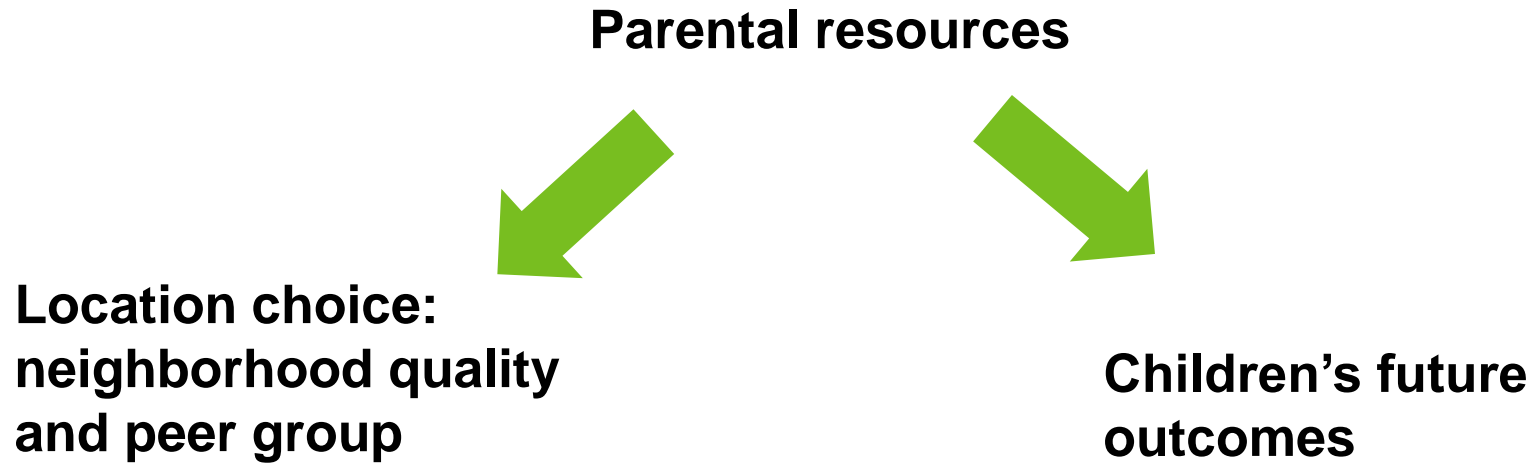
# Housing market mechanism and selection bias

Parental resources

Location choice:
neighborhood quality
and peer group

Housing market mechanism and selection bias

Parental resources

Location choice: neighborhood quality and peer group

Children's future outcomes

Housing market mechanism and selection bias

Parental resources

Location choice: neighborhood quality and peer group

?

Children's future outcomes
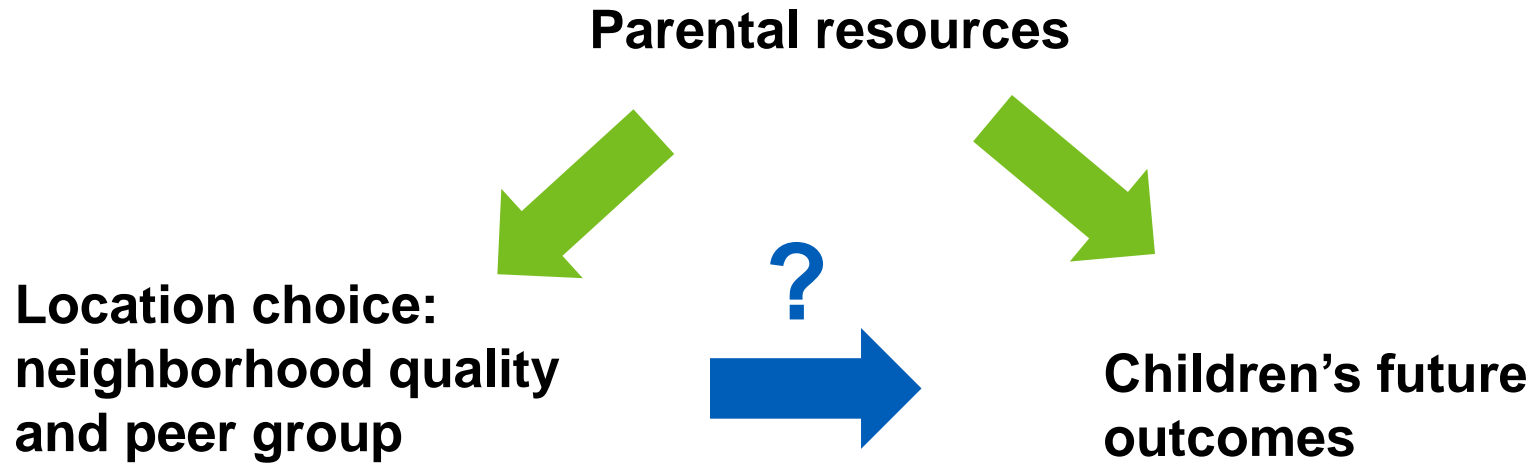
- Children who grow up in affluent neighborhoods do better later in life
- But is this just a correlation due to optimization behavior by parents or a causal neighborhood effect?

Controlling for observable differences

One way forward could be to control for observable differences

- Compare families who are similar, have the same initial income, level of education etc., but live in different types of neighborhoods
- Any problems with this approach?

Controlling for observable differences

One way forward could be to control for observable differences

- Compare families who are similar, have the same initial income, level of education etc., but live in different types of neighborhoods

If we compare similar families, why did they make different residential location choices?

- They must be different in some unobservable way

- Low-income parents who make the effort to move to a higher quality neighborhood than observably similar parents may also use more other resources in parenting

- This type of research design is unlikely to work

Experiments and quasi-experiments

Randomized controlled trials (RCT) are often considered the gold standard for causal inference

Ideal experiment: randomly assign families to neighborhoods and compare children's outcomes in adulthood

- Randomization makes sure that the groups are similar on average before the treatment
- Average differences between the groups after the treatment can be attributed to neighborhoods
- Why don't we do this?

Experiments and quasi-experiments

Randomized controlled trials (RCT) are often considered the gold standard for causal inference

Ideal experiment: randomly assign families to neighborhoods and compare children's outcomes in adulthood

- Randomization makes sure that the groups are similar on average before the treatment
- Average differences between the groups after the treatment can be attributed to neighborhoods
- Ideal from a scientific point of view, but often unethical!

Alternative, try to find a situation that approximates this ideal experiment (quasi-experiment)

Randomizing location choice?

Parents' resources

Location choice: neighborhood quality and peer group

Child's outcomes
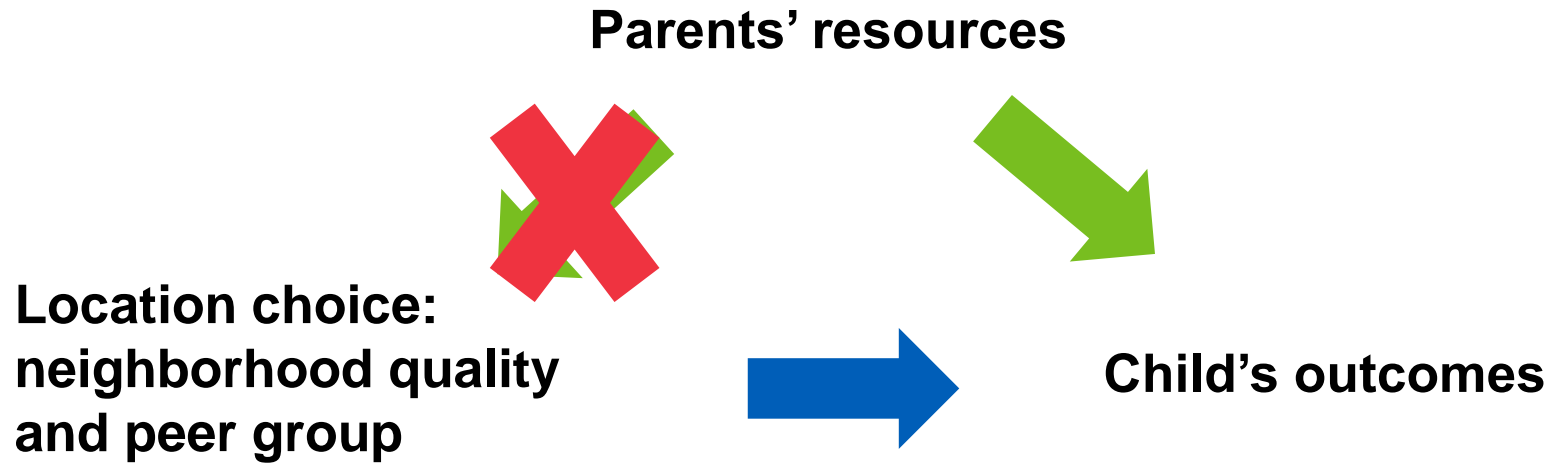
Moving to Opportunity

Chetty, Hendren and Katz (2016, AER)

The Effects of Exposure to Better Neighborhoods on Children: New Evidence from the Moving to Opportunity Experiment[†]

By Raj Chetty, Nathaniel Hendren, and Lawrence F. Katz*

The Moving to Opportunity (MTO) experiment offered randomly selected families housing vouchers to move from high-poverty housing projects to lower-poverty neighborhoods. We analyze MTO's impacts on children's long-term outcomes using tax data. We find that moving to a lower-poverty neighborhood when young (before age 13) increases college attendance and earnings and reduces single parenthood rates. Moving as an adolescent has slightly negative impacts, perhaps because of disruption effects. The decline in the gains from moving with the age when children move suggests that the duration of exposure to better environments during childhood is an important determinant of children's long-term outcomes. (JEL I31, I38, J13, R23, R38)

Moving to opportunity (MTO)

One of the most famous social experiments of all time

US Department of Housing and Urban Development (HUD) Moving to Opportunity Experiment implemented in 1994–1998

The experiment offered families living in high-poverty housing projects (public housing) housing vouchers to move to lower-poverty neighborhoods

- 4,600 families at 5 sites: Baltimore, Boston, Chicago, LA, New York
- Families signed-up for the experiment voluntarily (implications?)

Moving to opportunity (MTO)

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

Of course, the families were not forced to move, they were just offered a voucher to do so

Section 8 and control groups serve as the counterfactual

- I.e., what would have happened to the children in the experimental group had they not received the treatment (= the offer)

Moving to opportunity (MTO)

Large literature on MTO has found significant effects on, for example, adult mental health and subjective well-being

- But these older studies consistently found that the MTO had no impact on earnings or employment rates of adults and older youth

Chetty et al. (2016) revisit the MTO experiment and focus on its impacts on children of different ages when their families moved to better neighborhoods

Data

MTO data obtained from HUD

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

Link MTO data to federal income tax returns from 1996–2012

- Approximately 85% of children matched
- Match rates do not differ significantly across treatment groups

Analysis

In baseline analysis, estimate treatment effects for two groups:

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

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

- Younger children had 7 more years of exposure to low-poverty neighborhood
- Note that MTO treatments naturally changed many other features of neighborhoods besides the poverty rate

The paper

1. **Check that groups really look like they are randomized**

   - Pre-treatment characteristics must be balanced (be the same on average) across groups (balance tests)

2. **What is the treatment exactly?**

   - Compliance: How many took up the treatment?

   - Here the treatment is a combination many things

   - See how much neighborhood poverty rate changes

3. **Main results:**

   - Intent-to-treat estimates (ITT) effect of being offered a voucher

   - Treatment on the treated estimates (TOT) effect of being offered a voucher and moving

TABLE 1—SUMMARY STATISTICS AND BALANCE TESTS FOR CHILDREN IN MTO-TAX DATA LINKED SAMPLE

	< Age 13 at random assignment			Age 13–18 at random assignment		
	Control group mean (1)	Exp. versus control (2)	Sec. 8 versus control (3)	Control group mean (4)	Exp. versus control (5)	Sec. 8 versus control (6)
Linked to tax data (%)	86.4	−0.8 (1.4)	−0.4 (1.5)	83.8	1.5 (2.0)	−0.1 (2.2)
Child's age at random assignment	8.2	−0.1 (0.1)	−0.0 (0.1)	15.1	0.1 (0.1)	−0.1 (0.1)
Household head completed high school (%)	34.3	4.2* (2.4)	0.4 (2.6)	29.5	5.0 (3.1)	0.7 (3.3)
Household head employed (%)	23.8	1.0 (2.1)	−2.2 (2.2)	25.3	3.0 (2.9)	−0.4 (3.0)
Household head gets AFDC/TANF (%)	79.5	0.6 (1.9)	1.8 (2.0)	75.0	−0.8 (2.9)	−1.0 (3.0)
Household head never married (%)	65.1	−4.3* (2.3)	−3.1 (2.6)	53.0	−3.1 (3.2)	−6.3* (3.4)
Household head had teenage birth (%)	28.6	−0.9 (2.2)	−0.3 (2.5)	29.1	−3.6 (2.9)	−2.5 (3.2)
Primary or secondary reason for move is to get away from gangs or drugs (%)	78.1	−1.8 (2.1)	−4.4* (2.4)	77.7	3.1 (2.6)	−0.9 (2.9)
Household victims of crime in past five years (%)	41.3	2.5 (2.4)	0.9 (2.7)	44.8	1.3 (3.3)	−3.3 (3.5)
Household head African American (%)	66.9	−0.4 (2.0)	−1.4 (2.1)	63.9	−1.9 (2.7)	−5.9** (2.8)
Household head Hispanic (%)	29.4	−0.3 (2.0)	−0.5 (2.1)	31.1	(2.7)	0.6 (2.7)
Child susp./expelled in past two years (%)	4.9	0.7 (0.8)	0.4 (0.9)	17.6	1.0 (2.0)	0.4 (2.2)
Children in linked MTO-tax data	1,613	1,969	1,427	686	959	686

TABLE 2—FIRST-STAGE IMPACTS OF MTO ON VOUCHER TAKE-UP
AND NEIGHBORHOOD POVERTY RATES (*Percentage Points*)

	Housing voucher take-up (1)	Poverty rate in tract one year post- RA		Mean poverty rate in tract post-RA to age 18		Mean poverty rate in zip post-RA to age 18	
		ITT (2)	TOT (3)	ITT (4)	TOT (5)	ITT (6)	TOT (7)
Panel A. Children < age 13 at random assignment							
Exp. versus control	47.66***						
	(1.653)						
Sec. 8 versus control	65.80***						
	(1.934)						
Observations	5,044						
Control group mean	0						
Panel B. Children age 13–18 at random assignment							
Exp. versus control	40.15***						
	(2.157)						
Sec. 8 versus control	55.04***						
	(2.537)						
Observations	2,358						
Control group mean	0						

TABLE 2—FIRST-STAGE IMPACTS OF MTO ON VOUCHER TAKE-UP AND NEIGHBORHOOD POVERTY RATES (*Percentage Points*)
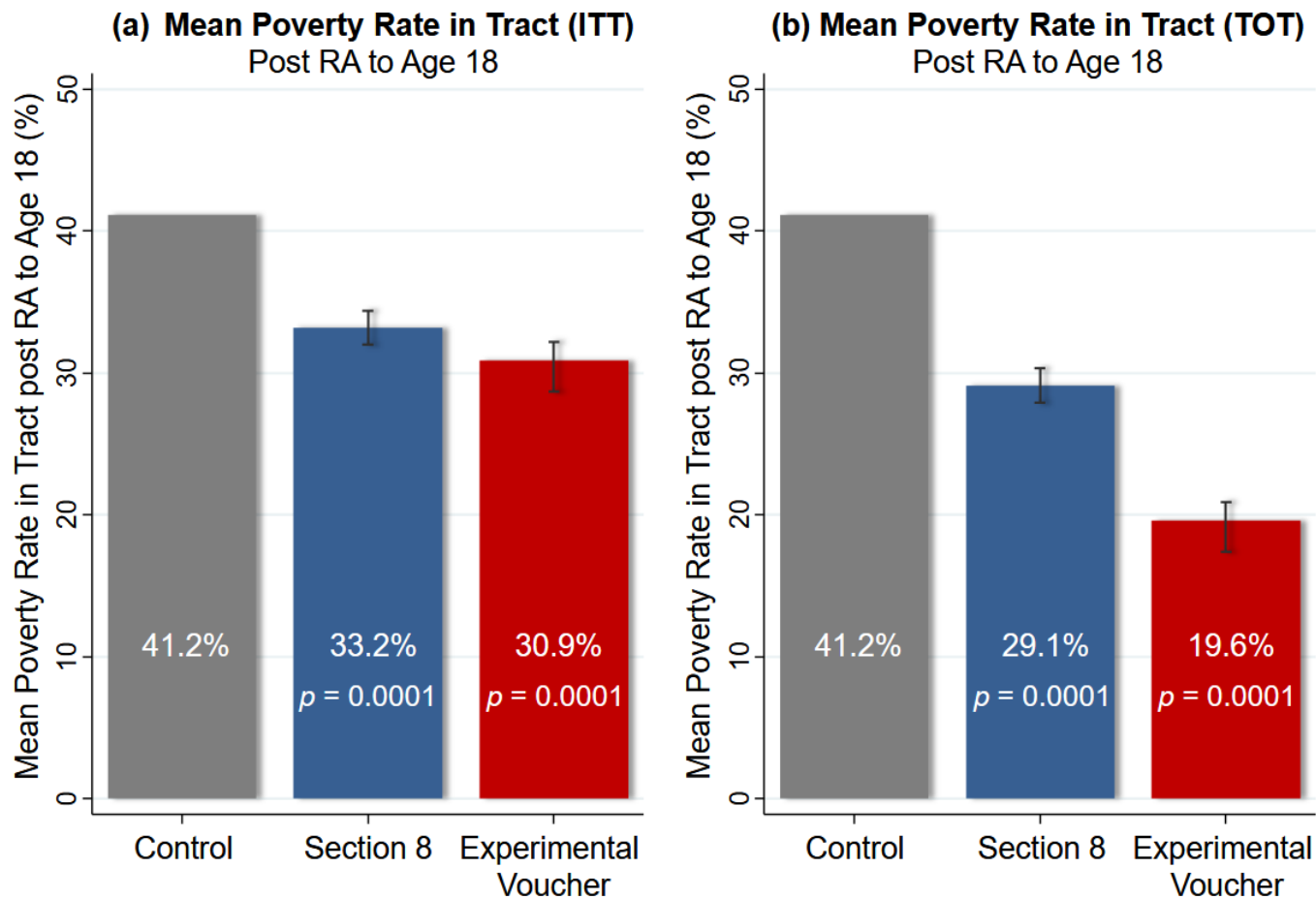
	Housing voucher take-up (1)	Poverty rate in tract one year post- RA		Mean poverty rate in tract post-RA to age 18		Mean poverty rate in zip post-RA to age 18	
		ITT (2)	TOT (3)	ITT (4)	TOT (5)	ITT (6)	TOT (7)
Panel A. Children < age 13 at random assignment							
Exp. versus control	47.66***	−17.05***	−35.96***				
	(1.653)	(0.853)	(1.392)				
Sec. 8 versus control	65.80***	−14.88***	−22.57***				
	(1.934)	(0.802)	(1.024)				
Observations	5,044	4,958	4,958				
Control group mean	0	50.23	50.23				
Panel B. Children age 13–18 at random assignment							
Exp. versus control	40.15***	−14.00***	−34.70***				
	(2.157)	(1.136)	(2.231)				
Sec. 8 versus control	55.04***	−12.21***	−22.03***				
	(2.537)	(1.078)	(1.738)				
Observations	2,358	2,302	2,302				
Control group mean	0	49.14	49.14				

Table 2—First-Stage Impacts of MTO on Voucher Take-Up and Neighborhood Poverty Rates (*Percentage Points*)

	Housing voucher take-up (1)	Poverty rate in tract one year post-RA		Mean poverty rate in tract post-RA to age 18		Mean poverty rate in zip post-RA to age 18	
		ITT (2)	TOT (3)	ITT (4)	TOT (5)	ITT (6)	TOT (7)
Panel A. Children < age 13 at random assignment							
Exp. versus control	47.66***	−17.05***	−35.96***	−10.27***	−21.56***	−5.84***	−12.23***
	(1.653)	(0.853)	(1.392)	(0.650)	(1.118)	(0.425)	(0.752)
Sec. 8 versus control	65.80***	−14.88***	−22.57***	−7.97***	−12.06***	−3.43***	−5.17***
	(1.934)	(0.802)	(1.024)	(0.615)	(0.872)	(0.423)	(0.622)
Observations	5,044	4,958	4,958	5,035	5,035	5,035	5,035
Control group mean	0	50.23	50.23	41.17	41.17	31.81	31.81
Panel B. Children age 13–18 at random assignment							
Exp. versus control	40.15***	−14.00***	−34.70***	−10.04***	−24.66***	−5.51***	−13.52***
	(2.157)	(1.136)	(2.231)	(0.948)	(1.967)	(0.541)	(1.113)
Sec. 8 versus control	55.04***	−12.21***	−22.03***	−8.60***	−15.40***	−3.95***	−7.07***
	(2.537)	(1.078)	(1.738)	(0.920)	(1.530)	(0.528)	(0.921)
Observations	2,358	2,302	2,302	2,293	2,293	2,292	2,292
Control group mean	0	49.14	49.14	47.90	47.90	35.17	35.17

Impacts of MTO on children below age 13 at RA

(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 RA

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

Control: 47.9%
Section 8: 39.3% — p = 0.0001
Experimental Voucher: 37.9% — p = 0.0001

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

Control: 47.9%
Section 8: 32.5% — p = 0.0001
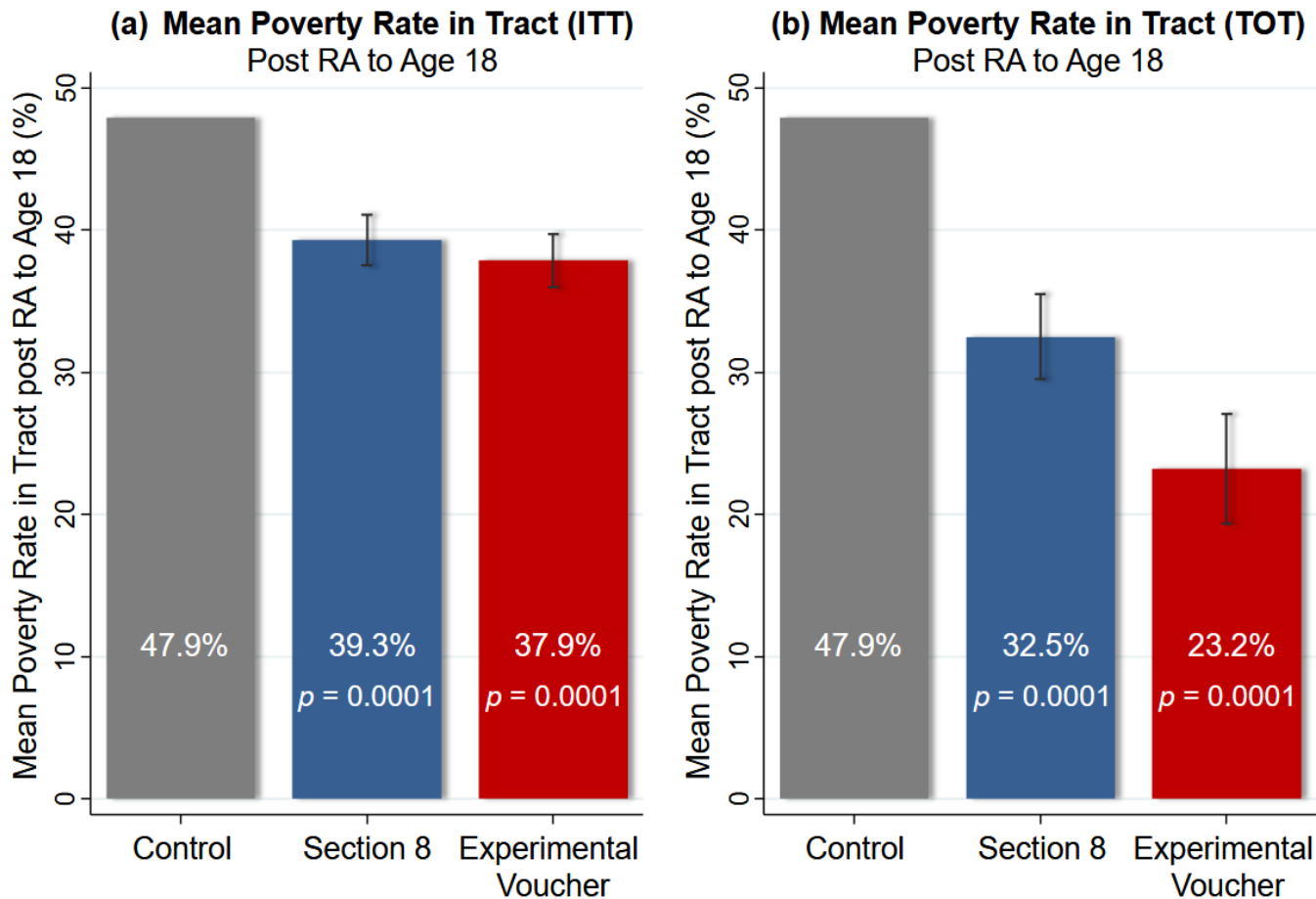Experimental Voucher: 23.2% — p = 0.0001

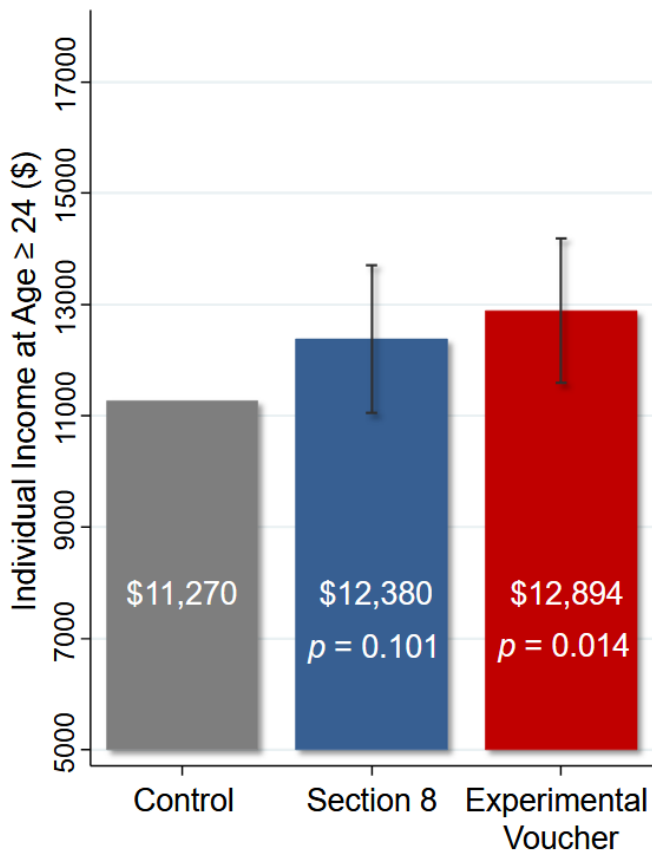## TABLE 3—IMPACTS OF MTO ON CHILDREN'S INCOME IN ADULTHOOD

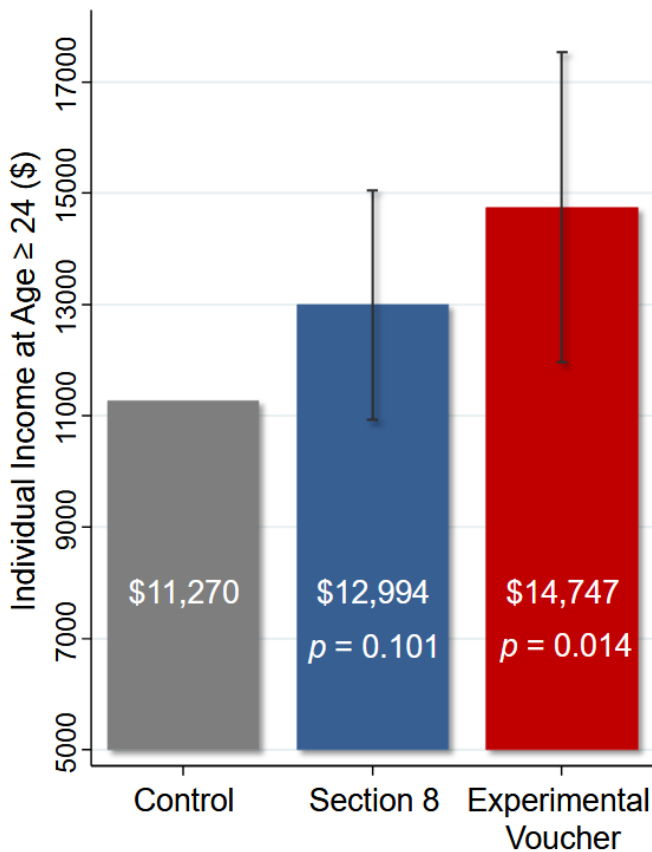|	W-2 earnings ($) 2008–2012 ITT (1)	Individual earnings 2008–2012 ($)			Individual earnings ($)		Employed (%) 2008–2012 ITT (7)	Hhold. inc. ($) 2008–2012 ITT (8)	Inc. growth ($) 2008–2012 ITT (9)
		ITT (2)	ITT w/ controls (3)	TOT (4)	Age 26 ITT (5)	2012 ITT (6)			
Panel A. Children < age 13 at random assignment									
Exp. versus control	1,339.8** (671.3)	1,624.0** (662.4)	1,298.9** (636.9)	3,476.8** (1,418.2)	1,751.4* (917.4)	1,443.8** (665.8)	1.824 (2.083)	2,231.1*** (771.3)	1,309.4** (518.5)
Sec. 8 versus control	687.4 (698.7)	1,109.3 (676.1)	908.6 (655.8)	1,723.2 (1051.5)	551.5 (888.1)	1,157.7* (690.1)	1.352 (2.294)	1,452.4** (735.5)	800.2 (517.0)
Observations	8,420	8,420	8,420	8,420	1,625	2,922	8,420	8,420	8,420
Control group mean	9,548.6	11,270.3	11,270.3	11,270.3	11,398.3	11,302.9	61.8	12,702.4	4,002.2
Panel B. Children age 13–18 at random assignment									
Exp. versus control	−761.2 (870.6)	−966.9 (854.3)	−879.5 (817.3)	−2,426.7 (2,154.4)	−539.0 (795.4)	−969.2 (1,122.2)	−2.173 (2.140)	−1,519.8 (11,02.2)	−693.6 (571.6)
Sec. 8 versus control	−1,048.9 (932.5)	−1,132.8 (922.3)	−1,136.9 (866.6)	−2,051.1 (1,673.7)	−15.11 (845.9)	−869.0 (1213.3)	−1.329 (2.275)	−936.7 (11,85.9)	−885.3 (625.2)
Observations	11,623	11,623	11,623	11,623	2,331	2,331	11,623	11,623	11,623
Control group mean	13,897.1	15,881.5	15,881.5	15,881.5	13,968.9	16,602.0	63.6	19,169.1	4,128.1

TABLE 3—IMPACTS OF MTO ON CHILDREN'S INCOME IN ADULTHOOD

| | W-2 earn- ings ($) 2008–2012 ITT (1) | Individual earnings 2008–2012 ($) | | | Individual earnings ($) | | Employed (%) 2008– 2012 ITT (7) | Hhold. inc. ($) 2008–2012 ITT (8) | Inc. growth ($) 2008–2012 ITT (9) |
		ITT (2)	ITT w/ controls (3)	TOT (4)	Age 26 ITT (5)	2012 ITT (6)			
Panel A. Children < age 13 at random assignment									
Exp. versus control	1,339.8** (671.3)	1,624.0** (662.4)	1,298.9** (636.9)	3,476.8** (1,418.2)	1,751.4* (917.4)	1,443.8** (665.8)	1.824 (2.083)	2,231.1*** (771.3)	1,309.4** (518.5)
Sec. 8 versus control	687.4 (698.7)	1,109.3 (676.1)	908.6 (655.8)	1,723.2 (1051.5)	551.5 (888.1)	1,157.7* (690.1)	1.352 (2.294)	1,452.4** (735.5)	800.2 (517.0)
Observations	8,420	8,420	8,420	8,420	1,625	2,922	8,420	8,420	8,420
Control group mean	9,548.6	11,270.3	11,270.3	11,270.3	11,398.3	11,302.9	61.8	12,702.4	4,002.2
Panel B. Children age 13–18 at random assignment									
Exp. versus control	−761.2 (870.6)	−966.9 (854.3)	−879.5 (817.3)	−2,426.7 (2,154.4)	−539.0 (795.4)	−969.2 (1,122.2)	−2.173 (2.140)	−1,519.8 (11,02.2)	−693.6 (571.6)
Sec. 8 versus control	−1,048.9 (932.5)	−1,132.8 (922.3)	−1,136.9 (866.6)	−2,051.1 (1,673.7)	−15.11 (845.9)	−869.0 (1213.3)	−1.329 (2.275)	−936.7 (11,85.9)	−885.3 (625.2)
Observations	11,623	11,623	11,623	11,623	2,331	2,331	11,623	11,623	11,623
Control group mean	13,897.1	15,881.5	15,881.5	15,881.5	13,968.9	16,602.0	63.6	19,169.1	4,128.1

Impacts of MTO on children <u>below age 13</u> at RA

(a) Individual Earnings (ITT)

Individual Income at Age ≥ 24 ($)

Control: $11,270
Section 8: $12,380, $p = 0.101$
Experimental Voucher: $12,894, $p = 0.014$

(b) Individual Earnings (TOT)

Individual Income at Age ≥ 24 ($)

Control: $11,270
Section 8: $12,994, $p = 0.101$
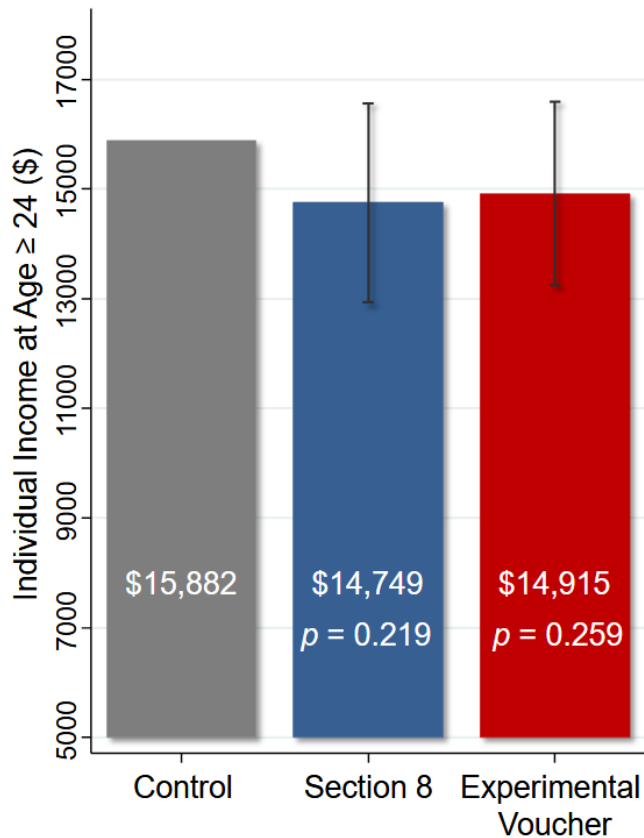Experimental Voucher: $14,747, $p = 0.014$

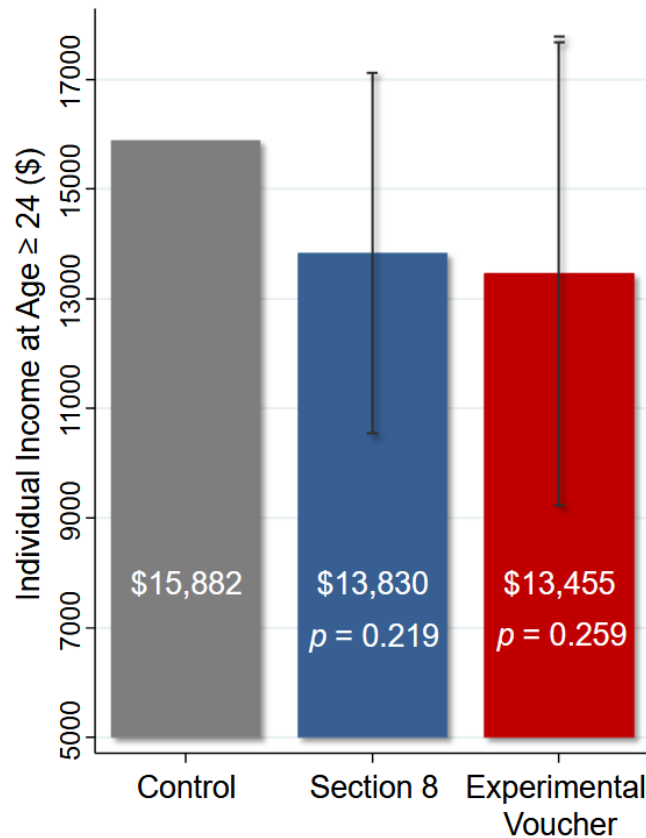## TABLE 3—IMPACTS OF MTO ON CHILDREN'S INCOME IN ADULTHOOD

	W-2 earnings ($) 2008–2012 ITT (1)	Individual earnings 2008–2012 ($)			Individual earnings ($)		Employed (%) 2008–2012 ITT (7)	Hhold. inc. ($) 2008–2012 ITT (8)	Inc. growth ($) 2008–2012 ITT (9)
		ITT (2)	ITT w/ controls (3)	TOT (4)	Age 26 ITT (5)	2012 ITT (6)			
Panel A. Children < age 13 at random assignment									
Exp. versus control	1,339.8** (671.3)	1,624.0** (662.4)	1,298.9** (636.9)	3,476.8** (1,418.2)	1,751.4* (917.4)	1,443.8** (665.8)	1.824 (2.083)	2,231.1*** (771.3)	1,309.4** (518.5)
Sec. 8 versus control	687.4 (698.7)	1,109.3 (676.1)	908.6 (655.8)	1,723.2 (1051.5)	551.5 (888.1)	1,157.7* (690.1)	1.352 (2.294)	1,452.4** (735.5)	800.2 (517.0)
Observations	8,420	8,420	8,420	8,420	1,625	2,922	8,420	8,420	8,420
Control group mean	9,548.6	11,270.3	11,270.3	11,270.3	11,398.3	11,302.9	61.8	12,702.4	4,002.2
Panel B. Children age 13–18 at random assignment									
Exp. versus control	−761.2 (870.6)	−966.9 (854.3)	−879.5 (817.3)	−2,426.7 (2,154.4)	−539.0 (795.4)	−969.2 (1,122.2)	−2.173 (2.140)	−1,519.8 (11,02.2)	−693.6 (571.6)
Sec. 8 versus control	−1,048.9 (932.5)	−1,132.8 (922.3)	−1,136.9 (866.6)	−2,051.1 (1,673.7)	−15.11 (845.9)	−869.0 (1213.3)	−1.329 (2.275)	−936.7 (11,85.9)	−885.3 (625.2)
Observations	11,623	11,623	11,623	11,623	2,331	2,331	11,623	11,623	11,623
Control group mean	13,897.1	15,881.5	15,881.5	15,881.5	13,968.9	16,602.0	63.6	19,169.1	4,128.1

Impacts of MTO on children <u>age 13–18</u> at RA
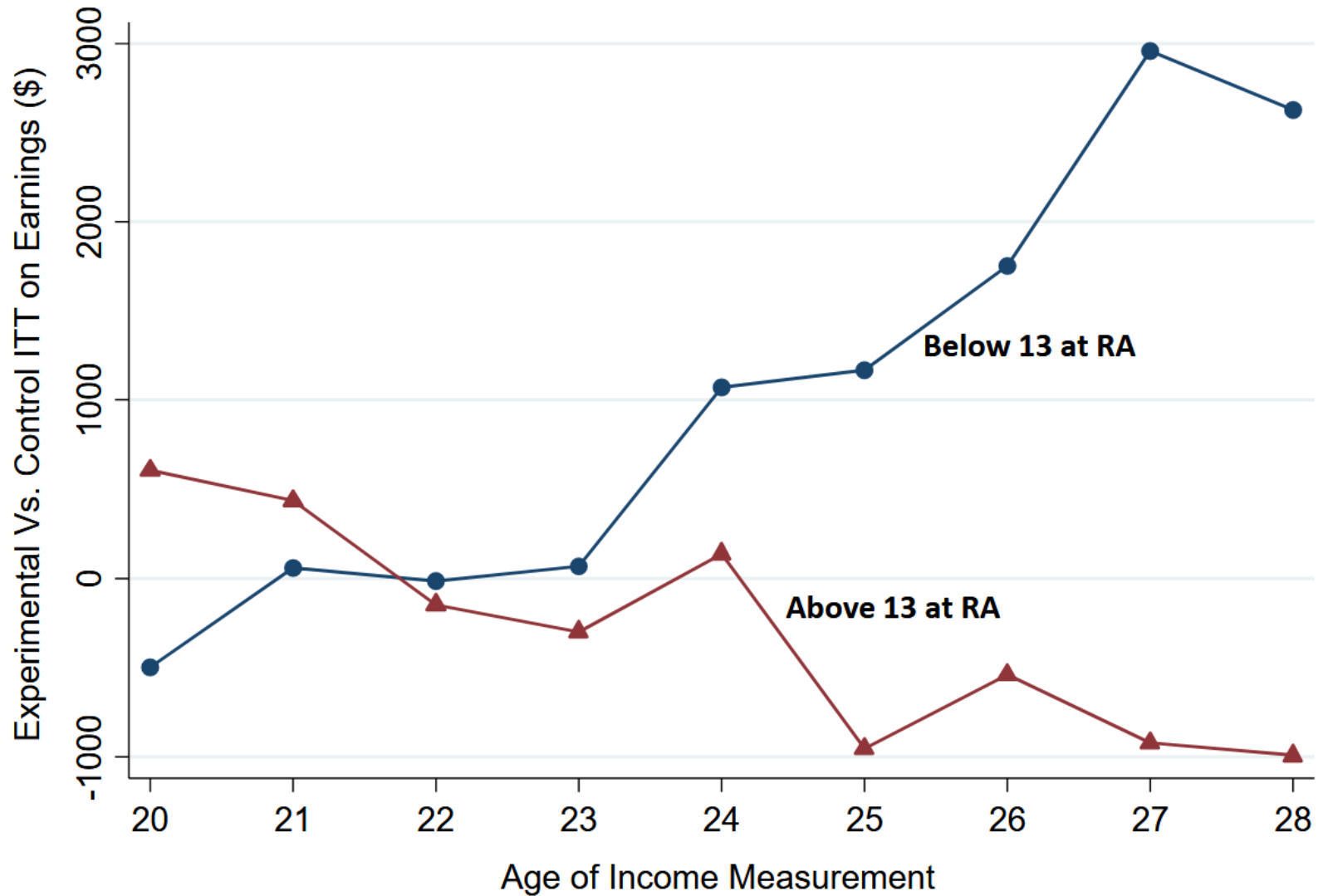

(a) Individual Earnings (ITT)

Individual Income at Age ≥ 24 ($)

- Control: $15,882
- Section 8: $14,749, $p = 0.219$
- Experimental Voucher: $14,915, $p = 0.259$

(b) Individual Earnings (TOT)

Individual Income at Age ≥ 24 ($)

- Control: $15,882
- Section 8: $13,830, $p = 0.219$
- Experimental Voucher: $13,455, $p = 0.259$

Impacts of Experimental Voucher by Age of Earnings Measurement

Below 13 at RA

Above 13 at RA

y-axis: Experimental Vs. Control ITT on Earnings ($)

x-axis: Age of Income Measurement

Other results

Children below age 13 at RA in the experimental group

- E.g. more likely to attend college, live in better n'hoods as adults

Children aged 13–18 at RA in the experimental group

- No effects on these outcomes

Adults in experimental group

- No effects on income (same finding as before)

No gender differences

The paper reports other results as well

- Opportunity Insights is a wonderful resource if you are interested in these issues: https://opportunityinsights.org/

ITT vs. TOT

Sometimes ITT is the most interesting estimate

- In the context of the MTO, it is the impact of offering housing vouchers
- This could the most relevant effect given that offering vouchers is likely to be the relevant policy (rather than forcing moves)

Sometimes TOT is more relevant

- In MTO, it is the impact of moving to better neighborhoods
- Potentially informative for policy discussion on whether we should invest in improving existing neighborhoods
- Although moving may complicate the interpretation a bit

Public housing demolition as a quasi-experiment

Natural or quasi-experiments

Most often an experimental research design is not available

- Unethical, expensive etc.

Sometimes the researcher is "lucky", and a government policy affects households in a way that resembles an experiment

These instances are referred to as "natural" or "quasi-experiments"

- Historical episodes/policy reforms that provide observable, quasi- or "as if" random variation in treatment
- Affect some people, but not others => treatment and control groups

Chyn (2018, AER)

Moved to Opportunity: The Long-Run Effects of Public Housing Demolition on Children[†]

By Eric Chyn*

This paper provides new evidence on the effects of moving out of disadvantaged neighborhoods on the long-run outcomes of children. I study public housing demolitions in Chicago, which forced low-income households to relocate to less disadvantaged neighborhoods using housing vouchers. Specifically, I compare young adult outcomes of displaced children to their peers who lived in nearby public housing that was not demolished. Displaced children are more likely to be employed and earn more in young adulthood. I also find that displaced children have fewer violent crime arrests. Children displaced at young ages have lower high school dropout rates. (JEL H75, I38, J13, R23, R38)

38

Chyn (2018, AER)

Studies the case of Chicago where the housing authority began reducing its stock of public housing during the 1990s

- The authority targeted some buildings with poor maintenance for demolition while leaving nearby buildings untouched
- Residents of buildings selected for demolition received Section 8 housing vouchers and were forced to relocate

This policy created a treatment and a control group "naturally" or by accident

- The housing authority was not planning to divide the residents into control and treatment groups for research purposes
- No researcher was involved in creating these groups

Research design

The research design compares the young adult outcomes of displaced and non-displaced children from the same public housing project

- Displaced = treatment group
- Non-displaced = control group

If these two groups of children and their households were similar before the demolition, differences in later-life outcomes can be attributed to neighborhood relocation

Key assumption I

The demolition decisions of the buildings were unrelated to the characteristics of the tenants in the buildings

This assumption is valid if the tenant selection mechanism did not allow tenants to select exact buildings

- In other words, within a given housing project, the tenants were (as-good-as) randomly assigned to buildings

Example: Robert Taylor Homes project

More applicants than housing units => waiting lists

With severe need for affordable housing and few outside options, people would choose the unit they are offered

People on the waiting list cannot influence, which unit from which building they are offered

Research design

In this type of research design, you need to carefully show that the households and children were similar in the control and treatment group prior to treatment (demolition)

- If they are similar in terms of characteristics that the researcher can observe, it is plausible that they are similar also in terms of the characteristics the researcher does not observe

- Balance tests!

Note that this is a particular type of quasi-experiment that you can analyze exactly as if it was a randomized experiment

- This is usually not the case!

Key assumption II

Demolition had no effects on the control group, i.e. those children whose building was not demolished

Prior research on the same demolitions shows that crime fell in the projects

- Why is this a problem?

Key assumption II

Demolition had no effects on the control group, i.e. those children whose building was not demolished

Prior research on the same demolitions shows that crime fell in the projects

- If neighborhood crime has a negative effect on children, the results might be biased toward zero
- Both the treatment and the control group might benefit from the demolition!

The paper

1. **Check that groups really look like they are randomized**

   - Pre-treatment characteristics must be balanced across groups (balance tests)

2. **What is the treatment exactly?**

   - Everyone complies
   - Treatment is a combination many things
   - See how much the neighborhood poverty rate changes

3. **Main results**

   - Heterogeneity w.r.t gender and age etc.

	All children		Male children		Female children		Adults	
	Control mean	Difference: treated–control, within estimate	Control mean	Difference: treated–control, within estimate	Control mean	Difference: treated–control, within estimate	Control mean	Difference: treated–control, within estimate
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Demographics								
Age	11.714	0.035 (0.159)	11.548	0.145 (0.196)	11.873	−0.070 (0.186)	28.851	0.810 (0.312)
Male (= 1)	0.489	−0.008 (0.017)					0.128	−0.001 (0.011)
Teen mom (= 1)[†]							0.371	−0.018 (0.024)
Past arrests (#)								
Violent	0.015	0.005 (0.007)	0.028	0.011 (0.014)	0.004	−0.003 (0.009)	0.185	−0.017 (0.032)
Property	0.011	0.010 (0.009)	0.018	0.015 (0.014)	0.004	0.004 (0.010)	0.156	0.016 (0.020)
Drugs	0.025	0.000 (0.013)	0.054	0.017 (0.023)	0.000	−0.018 (0.012)	0.166	0.031 (0.022)
School outcomes								
Enrolled (= 1)	0.948	0.003 (0.015)	0.946	−0.009 (0.017)	0.949	0.014 (0.016)		
Reading score (SD)	−0.443	0.024 (0.074)	−0.477	−0.045 (0.087)	−0.410	0.074 (0.074)		
Math score (SD)	−0.449	0.048 (0.061)	−0.509	0.007 (0.077)	−0.393	0.073 (0.065)		
Economic activity								
Employed (= 1)							0.173	0.006 (0.016)
Earnings[‡]							$1,493.75	−$45.91 (193.358)
Observations (individuals)		5,250		2,547		2,703		4,331

TABLE 2—IMPACT OF DEMOLITION ON HOUSEHOLD NEIGHBORHOOD CHARACTERISTICS

	3 years after demolition		8 years after demolition	
	Control mean (1)	Difference: treated–control, within estimate (2)	Control mean (3)	Difference: treated–control, within estimate (4)
HH has address (= 1)	0.777	0.014 (0.021)	0.656	0.011 (0.020)
Only HHs with address Tract characteristics:				
Black (percent)	94.897	−2.801 (1.125)	90.042	−1.055 (1.257)
Below poverty (percent)	64.208	−14.264 (2.729)	40.858	−2.771 (2.353)
Violent crime rate	68.855	−29.522 (5.807)	30.801	−2.371 (4.714)
Observations (HHs)		2,767		2,767
Observations (HHs with address)		2,162		1,824

48

TABLE 3—IMPACT OF DEMOLITION ON ADULT LABOR MARKET OUTCOMES OF CHILDREN

	Control mean (1)	Difference: treated–control, within estimate (2)
Employed (= 1)	0.419	0.040 (0.014)
Employed full-time (= 1)	0.099	0.013 (0.006)
Earnings	$3,713.00	$602.27 (153.915)
Earnings (> 0)	$8,856.91	$587.56 (222.595)
Observations		35,382
Individuals		5,246

Discussion

Discussion I

Both Chetty et al. (MTO paper) and Chyn find that younger kids benefit more

Chetty et al. even find negative effects for older kids (although not statistically significant)

Why do you think is this?

Discussion II

Internal validity

- Are the statistical inferences about causal effects valid for the population being studied?

External validity

- Can the statistical inferences be generalized from the population and setting studied to other populations and settings, where the "setting" refers to the legal, policy, and physical environment and related salient features?

- For example, can we learn something concerning Helsinki or other cities from the Chicago experience (or the MTO)?

Recap

Segregation is a consequence of differences in neighborhood quality and household income in the city

This type of residential sorting makes it very difficult to analyze neighborhood effects

- Sorting leads to correlation between outcomes of individuals and their neighbors, but these correlations do not imply causal effects

Evidence from the US using experimental and quasi-experimental research designs shows that in the context of these studies there are (causal) neighborhood effects

- However, results from the larger literature show that effects are context specific (UK, Canada, Sweden etc.)
- Also, the mechanisms still a black box

Asuinalueiden segregaatio ja naapurustovaikutukset

Essi Eerola ja Tuukka Saarimaa

Kirjoituksessa esitellään segregaation syntymekanismeja. Jos asuinalueet ovat laadultaan ja saavutettavuudeltaan erilaisia, ne eriytyvät asukkaiden tulotason mukaan. Tämä tarkoittaa, että satunnaisesti poimitun kotitalouden ja tämän naapureiden tulotasolla voidaan odottaa olevan positiivinen korrelaatio. Kirjoituksessa tarkastellaan myös sitä, millä tavoin naapurusto voi vaikuttaa asukkaisiin ja miten naapurustovaikutuksia on pystytty uskottavasti tutkimaan. Luotettavat kokeellisia ja kvasikokeellisia asetelmia hyödyntävät tutkimukset viittaavat siihen, että tietyissä ympäristöissä naapurustovaikutukset voivat olla merkittäviä. Vaikutukset ovat kuitenkin kontekstisidonnaisia, eikä vaikutusmekanismeja vielä tunneta kovinkaan hyvin. Nykyisen tutkimustiedon valossa ei esimerkiksi voida yksiselitteisesti sanoa, johtuvatko naapurustovaikutukset paikallisista palveluista vai nimenomaan naapureista tai millä maantieteellisellä tasolla naapurustovaikutukset toimivat. Kysymys on oleellinen pohdittaessa sitä, kannattaako resursseja suunnata sosiaaliseen sekoittamiseen asuntopolitiikan keinoin vai suoraan huono-osaisille perheille osoitettuina tulonsiirtoina ja palveluina.