Principles of Empirical Analysis

Lecture 8: Introduction to observational data and quasi-experiments

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Outline for Part II

- **In this part, we move on from using experimental data to issues we face when using observational data in estimating causal effects**
  - When and under what type of assumptions can we estimate causal effects from observational data?
- **We will familiarize ourselves with the most common quasi-experimental causal inference methods**
  - Difference-in-differences (DID)
  - Regression Discontinuity Design (RDD)
- **And, designs based on controlling for observable differences**

Outline for Part II

- **Problem sets**
  - You have one more set of exercises with a deadline in the last week of lectures
- **Reading assignment**
  - We will provide you with a list of research papers to choose from and you need to answer questions about the papers
  - We will give you more information about this next week

Outline for today

- **What is observational data and why it is so difficult to make causal claims based on observational data?**
- **What are quasi-experiments?**
    - This is the underlying theme throughout part II
- **We will continue to use neighborhood effects as the running example by illustrating the challenges in studying neighborhood effects without an experiment**
- **Quasi-experimental evidence on neighborhood effects**
    - Chyn, Eric. 2018. "Moved to Opportunity: The Long-Run Effects of Public Housing Demolition on Children." *American Economic Review*, 108 (10): 3028-56. https://www.aeaweb.org/articles?id=10.1257/aer.20161352

Observational data

Observational data

- **When experimental designs are infeasible, researchers must resort to the use of observational data from surveys, censuses, and administrative records**
  - Observational study draws inferences from a sample of a population where the independent (or treatment) variable is not under the control of the researcher because of ethical concerns or logistical constraints etc.
  - This is in contrast with experiments, such as randomized controlled trials, where each subject is randomly assigned to a treatment group or a control group (e.g. MTO)

Observational data

- **Economic theory teaches us that we should be suspicious of correlations found in observational data**
  - Correlations are almost certainly not reflecting a causal relationship because the variables were endogenously chosen by people who were making decisions, they thought were best
- **The FLEED data that you used in the earlier lectures is an example of such data**
  - E.g. education level of the people in the data is not a consequence of randomization on the part of the researcher, but of optimization on part of the individuals in the data
- **The challenges of estimating causal effects with observational data can be formidable**

People optimize

- **Consider the potential outcomes model:**
  - A treatment, in order to measure of a causal effect, must be completely independent of the potential outcomes under consideration
  - Yet, if the person is making some choice based on what she thinks is best, then it necessarily violates this independence condition
- **Economic theory predicts choices will be endogenous, and thus naive correlations are misleading!**
- **Keep in mind selection bias**

Observational alternatives to experiments

1. **Selection on observables: treatment and control groups differ from each other only w.r.t. observable characteristics**
    - Subclassification, matching, regression
2. **Selection on unobservables: treatment and control groups differ from each other in unobservable characteristics**
    - Exogenous variable induces variation in treatment – instrumental variables (IV)
    - Selection mechanism is known – regression discontinuity designs (RDD)
    - Treatment and controls are observed before and after treatment – difference-in-differences (DID)

Natural or quasi-experiments

- **Most often an experimental research design is not available**
- **Sometimes the researcher is "lucky" and a government policy or nature affects households (or firms etc.) in a way that resembles an experiment**
- **These instances are referred to as "natural experiments" or "quasi-experiments"**

Natural or quasi-experiments

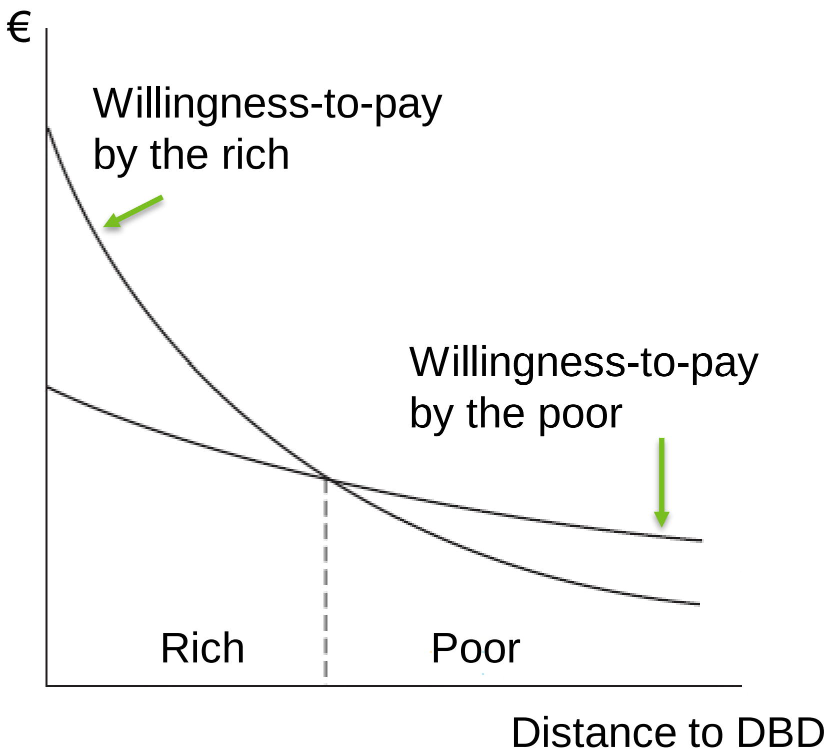
- **Most often an experimental research design is not available**
- **Sometimes the researcher is "lucky" and a government policy or nature affects households (or firms etc.) in a way that resembles an experiment**
- **These instances are referred to as "natural experiments" or "quasi-experiments"**
  - Historical episodes that provide observable, quasi- or "as if" random variation in treatment
  - These might be law changes that affect some people, but not others => control and treatment groups
  - Broad term that refers to many different situations and different situations require different research methods (IV, RDD, DID)

Quasi-experiment and neighborhood effects

Segregation in a model city

- **Let's assume that there are two residential areas in a city with a fixed supply of housing**
- **Central city**
  - Historic city center with beautiful architecture, historical monuments and easy access to jobs and variety of services
- **Suburb**
  - Far away from city center with lower quality amenities and less services
- **Let's also assume there are only two types of households**
  - Rich and poor
  - Both types work in the city center and dislike commuting

Where will the rich end up residing?



With these assumptions, the city will be segregated according to income

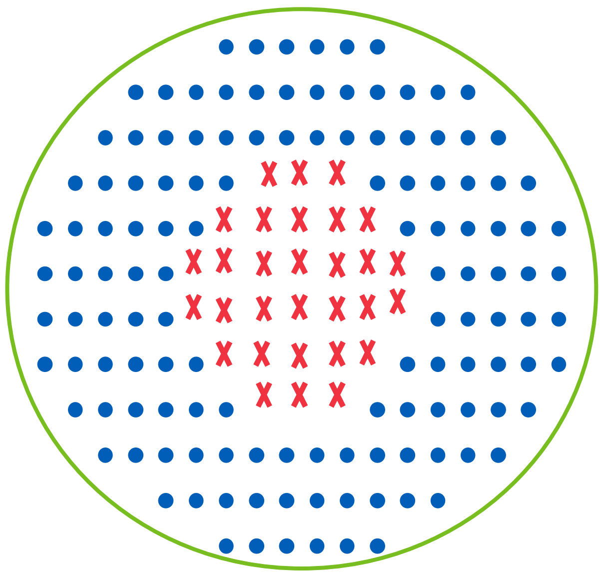
Rich live in the city center and poor in the suburbs as the poor cannot afford to live in the center given the higher prices

Segregation is the consequence of income inequality, quality differences of neighborhoods and optimizing behavior

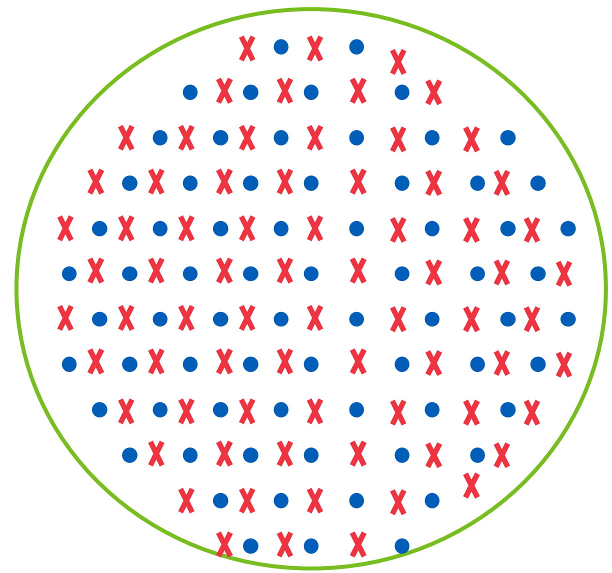
Whether this is good or bad depends on neighborhood effects

Which of these cities would be better for the citizens?

“Free market”

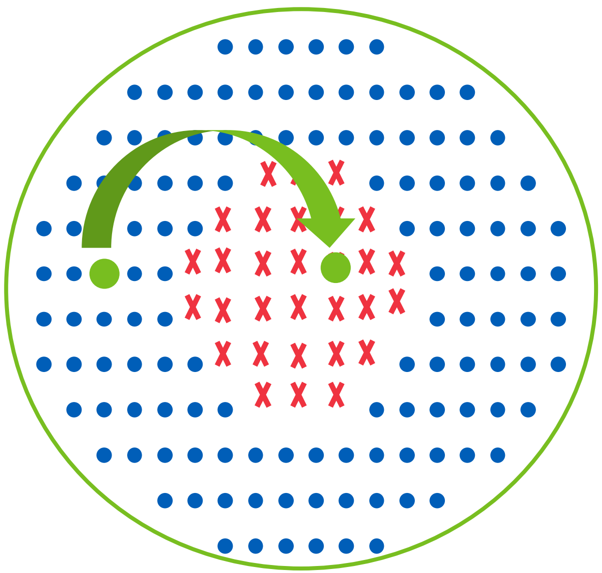


“Social mixing”



- ● Low-income
- ✗ High-income

One low-income family



- What if we provided one low-income family the resources to move to the other residential area?
  - *Neighborhood quality would increase*
  - *The children would have different role models and peers*

- Question: Would the family or the children in the family benefit if the family moved next to high-income families?

Housing market mechanism and selection bias

Parental resources

Location choice: neighborhood quality and peer group

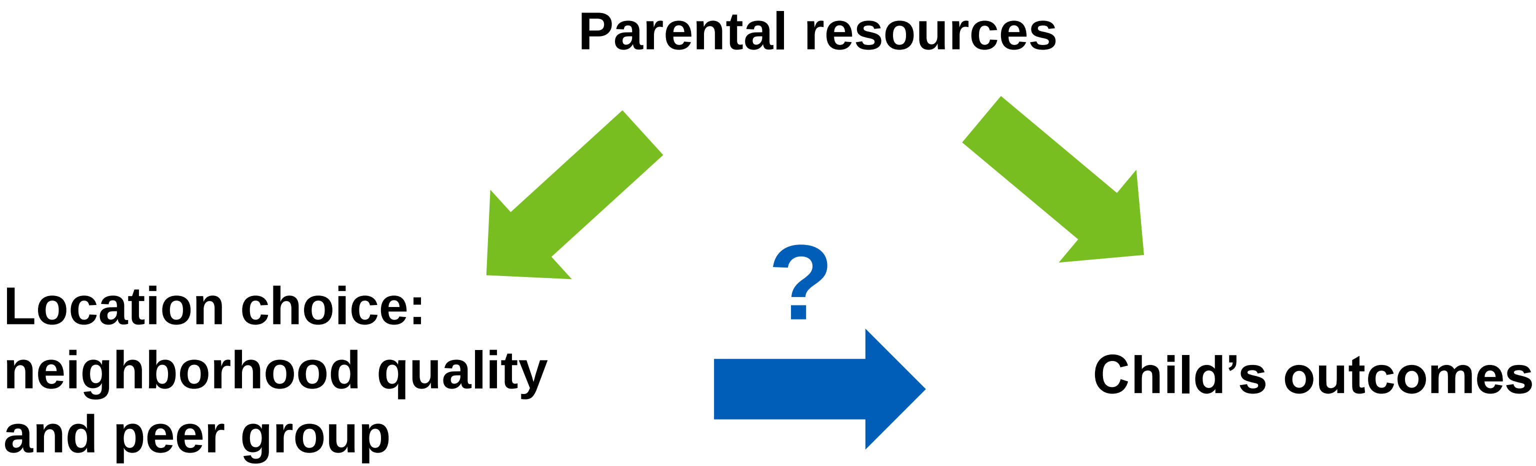
Housing market mechanism and selection bias

Parental resources

Location choice: neighborhood quality and peer group

Child’s outcomes

Housing market mechanism and selection bias



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

Controlling for observable differences?

- **One way would be to control for observable differences**
  - Compare people who are similar, have the same initial income, level of education etc., but live on in different quality neighborhoods

- **However, if families are supposed to be similar why did the families make different residential location choices?**
  - Maybe low-income parents who make the effort to move to a higher quality n'hood than observably similar parents also use more of other resources in parenting
  - Unobservable differences

Public housing demolition as a quasi-experiment

Chyn (2018, AER)

American Economic Review 2018, 108(10): 3028–3056
https://doi.org/10.1257/aer.20161352

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)

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

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- **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
  - The researcher was not involved in creating these groups
  - Quasi-experiment!

Research design

- **The research design compares the young adult outcomes of displaced and non-displaced children from the same public housing project**
  - I.e. compares the treatment and control groups
  - If these two groups of children and their households were similar before the demolition, differences in later-life outcomes can be attributed to relocation to another neighborhood

Key assumption I

- **The demolition decisions of the buildings were unrelated to the characteristics of the tenants**

- **This assumption should be valid if the tenant selection mechanism did not allow households to self-select into buildings**
  - Within a given housing project, the households were (as-good-as) randomly assigned to buildings
  - Waiting lists: there are more applicants than housing units
  - With severe need for affordable housing and few outside options, people would choose the unit they are offered

Example: Robert Taylor Homes project

Key assumption I

- **In this type of research design, one needs 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 has no effects on the children whose building was not demolished (control group)**

- **Prior research on the same demolitions shows that crime fell in the projects**
  - If crime in a neighborhood has adverse effects on children, Chyn's results might be biased toward zero (underestimates)
  - 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 covariates must be balanced across groups (balance tests)
2. **Discuss what is the treatment exactly?**
   - Everyone complies
   - Here the treatment is a combination of many things
   - See how much the neighborhood poverty rate changes
3. **Main results**
   - Heterogeneity w.r.t gender and age etc.

Table 1—Comparison of Displaced and Non-Displaced Children and Adults at Baseline (*Prior to Demolition*)

	All children		Male children		Female children		Adults	
	Control mean (1)	Difference: treated–control, within estimate (2)	Control mean (3)	Difference: treated–control, within estimate (4)	Control mean (5)	Difference: treated–control, within estimate (6)	Control mean (7)	Difference: treated–control, within estimate (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

Point estimate

Standard error

95% confidence interval = 0.81 ±1.96*0.312

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	0.656	0.011
		(0.021)		(0.020)
Only HHs with address				
Tract characteristics:				
Black (percent)	94.897	−2.801	90.042	−1.055
		(1.125)		(1.257)
Below poverty (percent)	64.208	−14.264	40.858	−2.771
		(2.729)		(2.353)
Violent crime rate	68.855	−29.522	30.801	−2.371
		(5.807)		(4.714)
Observations (HHs)		2,767		2,767
Observations (HHs with address)		2,162		1,824

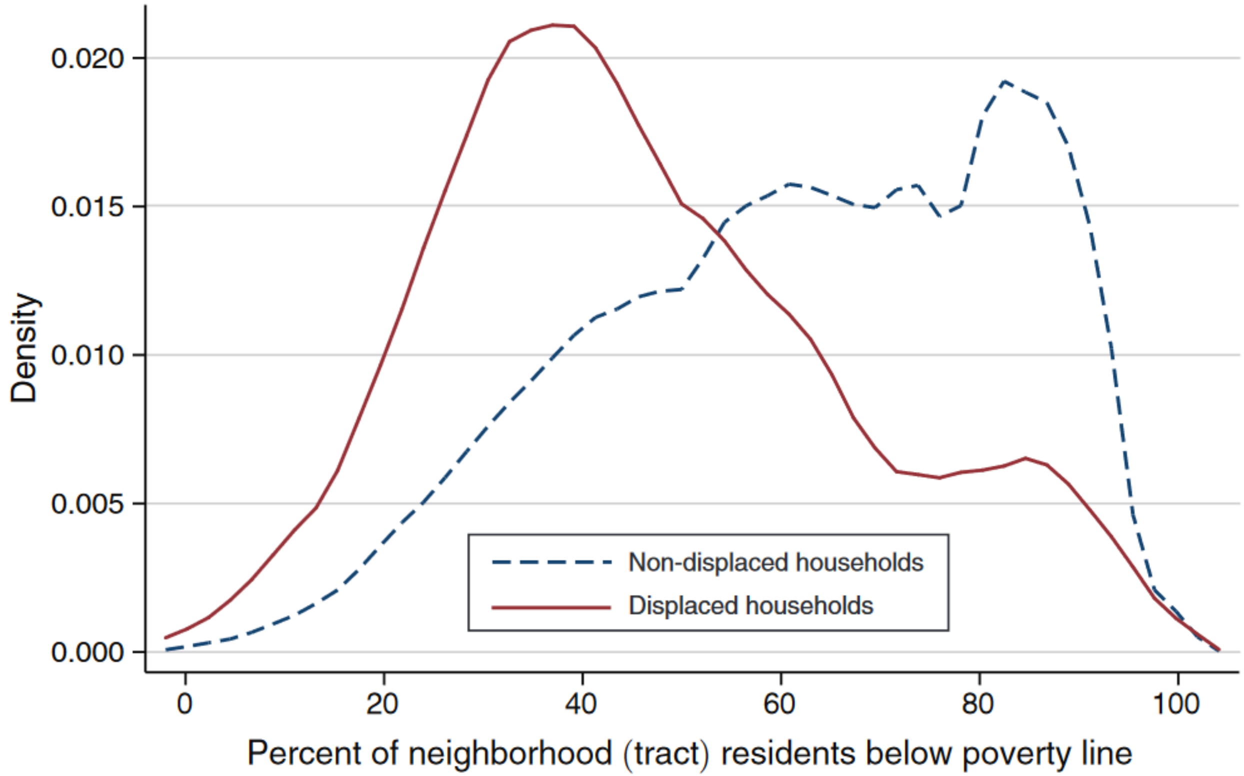

FIGURE 1. DENSITY OF NEIGHBORHOOD POVERTY AFTER DEMOLITION

Notes: The figure shows statistics for the duration-weighted average poverty rate for each household in the sample ($N = 2{,}767$). I compute the average over all locations for the household regardless of whether a child is still present.

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

TABLE 4—IMPACT OF DEMOLITION ON ADULT LABOR OUTCOMES OF CHILDREN BY SEX

	Males		Females	
	Control mean (1)	Difference: treated–control, within estimate (2)	Control mean (3)	Difference: treated–control, within estimate (4)
Employed (= 1)	0.325	0.017	0.505	0.066
		(0.019)		(0.014)
Employed FT (= 1)	0.080	0.013	0.117	0.015
		(0.008)		(0.008)
Earnings	$2,946.51	$417.46	$4,416.94	$806.22
		(236.705)		(188.520)
Earnings (> 0)	$9,055.43	$552.21	$8,739.53	$609.26
		(439.299)		(274.111)
Observations		16,876		18,506
Individuals		2,546		2,700

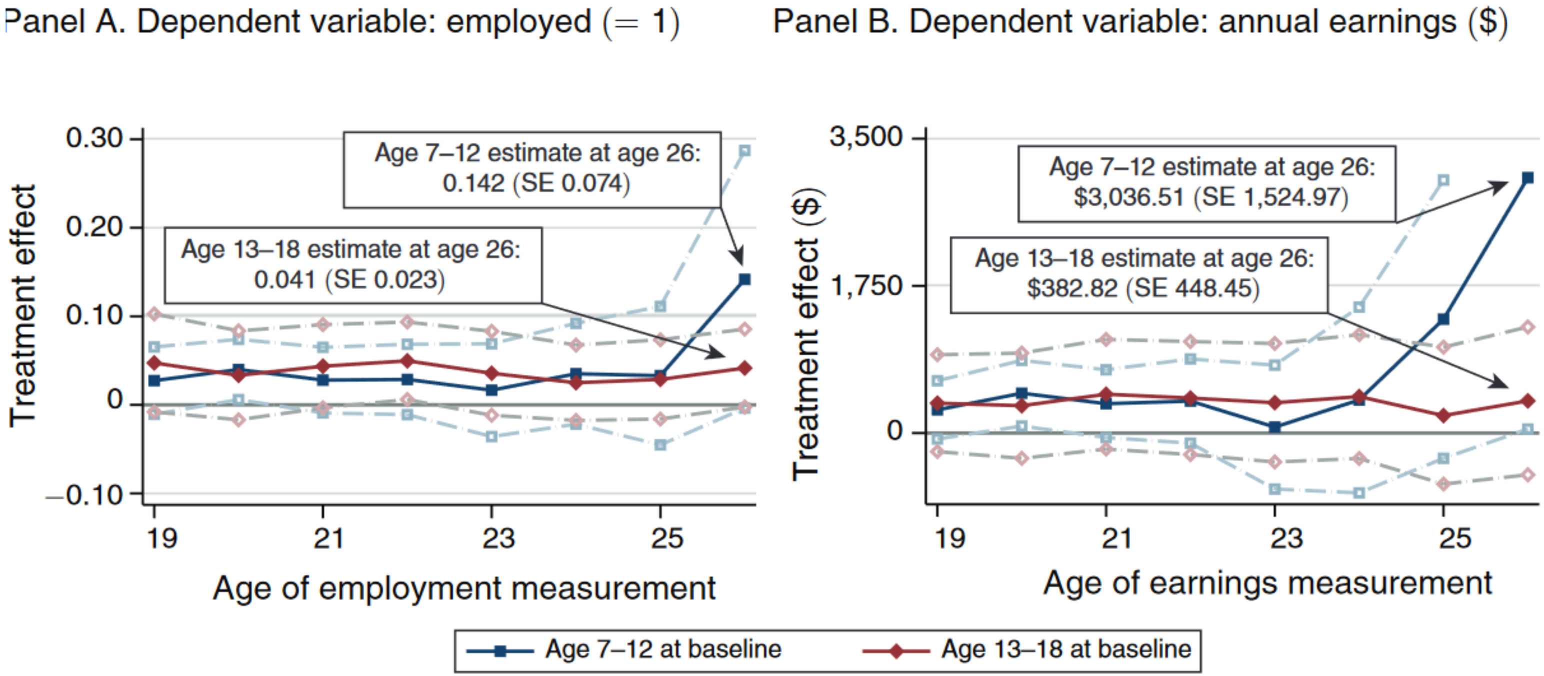

Figure 2. Impact on Employment and Earnings by Age of Measurement

Notes: Each point on the figure is an estimate from the following model:

$$Y_{it} = \sum_{j=19}^{26} \alpha_j D_{i,b} \mathbf{1}(age_{i,t} = j) + X_i'\theta + \psi_p + \delta_t + \epsilon_{it},$$

where i, t, b, and p index individuals, years, buildings, and projects, respectively. See Section IV for further details.

Reminder: Chetty et al. (2016)

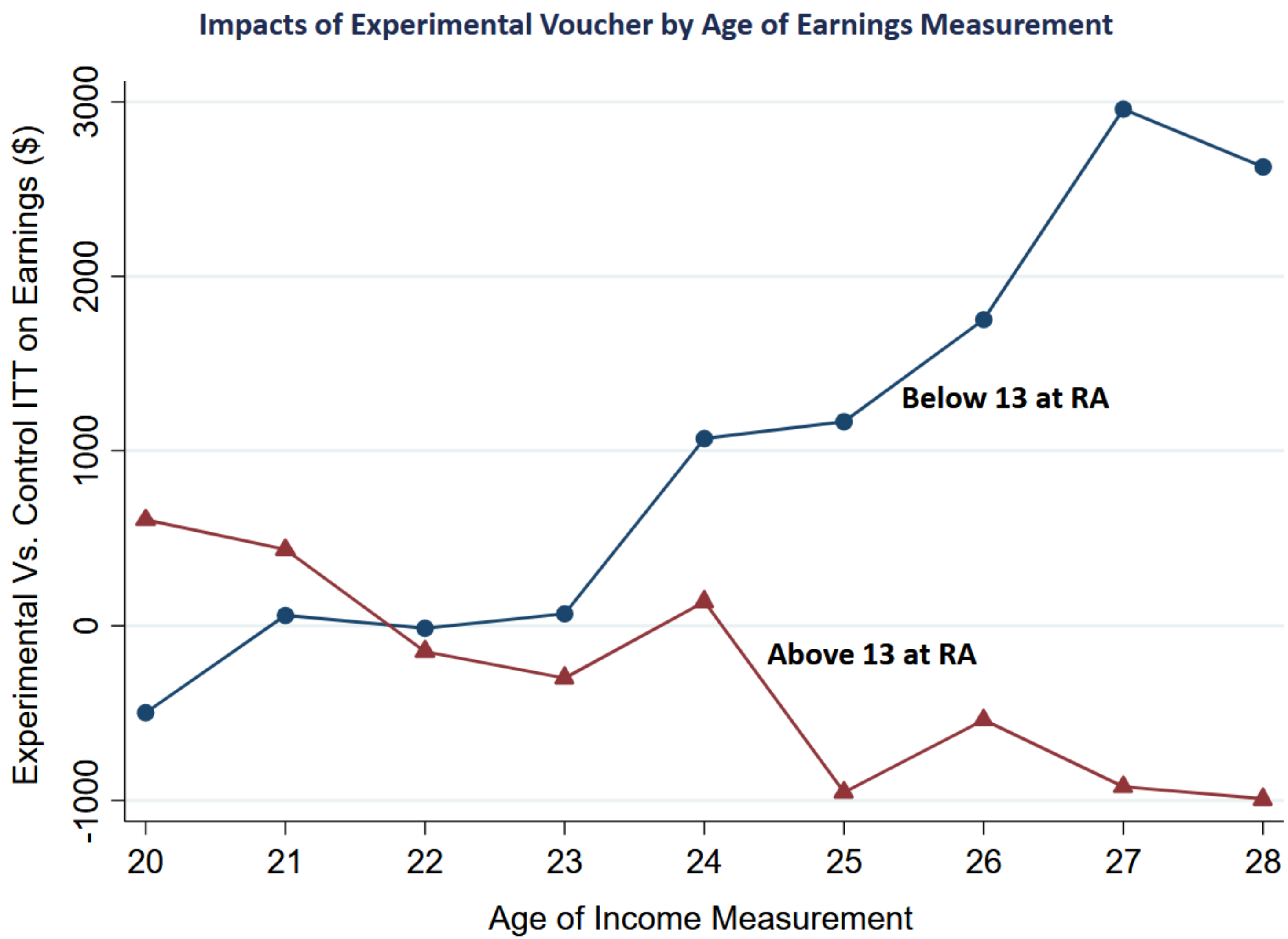

Table 5—Impact of Demolition on Crime of Children

	All		Males		Females	
	Control mean (1)	Difference: treated–control, within estimate (2)	Control mean (3)	Difference: treated–control, within estimate (4)	Control mean (5)	Difference: treated–control, within estimate (6)
Number of arrests						
Violent	0.072	–0.010	0.106	–0.017	0.039	–0.004
		(0.004)		(0.006)		(0.005)
Property	0.034	0.006	0.041	0.009	0.028	0.003
		(0.003)		(0.006)		(0.003)
Drug	0.103	–0.005	0.193	–0.016	0.018	0.005
		(0.011)		(0.018)		(0.008)
Other	0.154	–0.25	0.268	–0.037	0.046	–0.014
		(0.011)		(0.015)		(0.008)
Observations		56,629		27,246		29,383
Individuals		5,250		2,547		2,703

Notes: The control mean statistic in column 1 refers to averages for non-displaced individuals. The mean difference between displaced and non-displaced children in columns 2, 4, and 6 are computed from the regression specified in equation (1). Robust standard errors are clustered at the public housing building level.

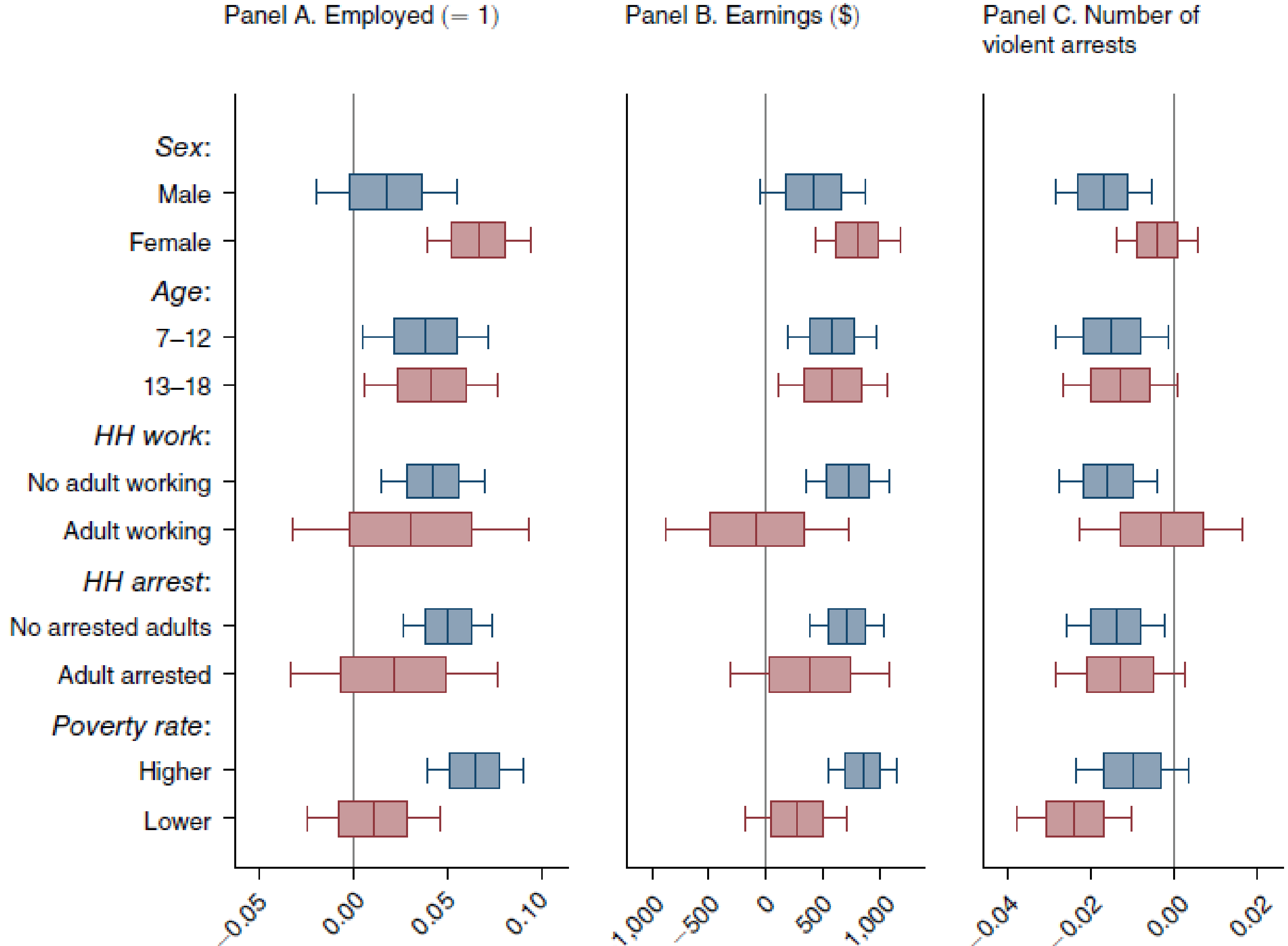

Figure 3. Impact of Demolition by Subgroup

Notes: Rows present box and whisker plots for effects estimated separately for subgroups defined by baseline characteristics. See text for further details.

FIGURE 3. IMPACT OF DEMOLITION BY SUBGROUP

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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?
  - That is, are we free of selection bias for example?

- **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

- **The challenges of estimating causal effects with observational data can be formidable**
  - Economic theory predicts that choices will be endogenous, and thus naive correlations are misleading
- **Sometimes we can make use of “natural” of “quasi-experiments”**
  - Historical episodes that provide observable, quasi- or “as if” random variation in treatment
- **In most cases, internal validity of quasi-experiments is not as strong as experimental designs**