ECON B253: INTRODUCTION TO ECONOMETRICS

Sebastian Anti, Assistant Professor of Economics
Bryn Mawr College

Lecture 7: Methods for Causal Inference

Motivation

- **While multivariate models are very powerful tools, saying that $\widehat{\beta}_1$ is a measure of a causal effect of X on Y is still a dicey proposition. Why?**
  - Omitted variable bias and reverse causality are very hard to rule out
- **Developments over the last 30 years have gotten us much closer to causal statements**
  - This is where the standard is in the literature
    - So it's important to know these methods

Motivation

- **Most frontier work in top econ journals at the moment has left behind simple multivariate models because of worries that the bias they leave unaddressed is too problematic**
- **Adapting multivariate models to deal with this bias is more and more the norm for the discipline**
  - This is the subject of this lecture

The Problem of Omitted Variable Bias (OVB)

- **We discussed before that OVB is a big problem**
  - If you leave out a relevant variable: $E(\hat{\beta}_1|X) \neq \beta_1$, and we've failed in our effort to accurately measure our relationship of interest
- **It is also a problem that is almost always present. Why?**
  - We are interested in socio-economic relationships
    - Many variables that belong on the RHS are ***unmeasurable***
    - Most of these will be correlated with our variable of interest

Example

- **Consider the following bivariate regression,**

$$Wage_i = \beta_0 + \beta_1 Schooling_i + \varepsilon_i \qquad (1)$$

  - What is in the error term ε_i? In other words, what else affects wage besides schooling?
    - Some ideas: (1) Parents' education, (2) Parents' socio-economic background, (3) Work experience, (4) Innate talent/ability, (5) Personal ambition
  - Are any of these correlated with schooling?
    - Of course they are!

Example

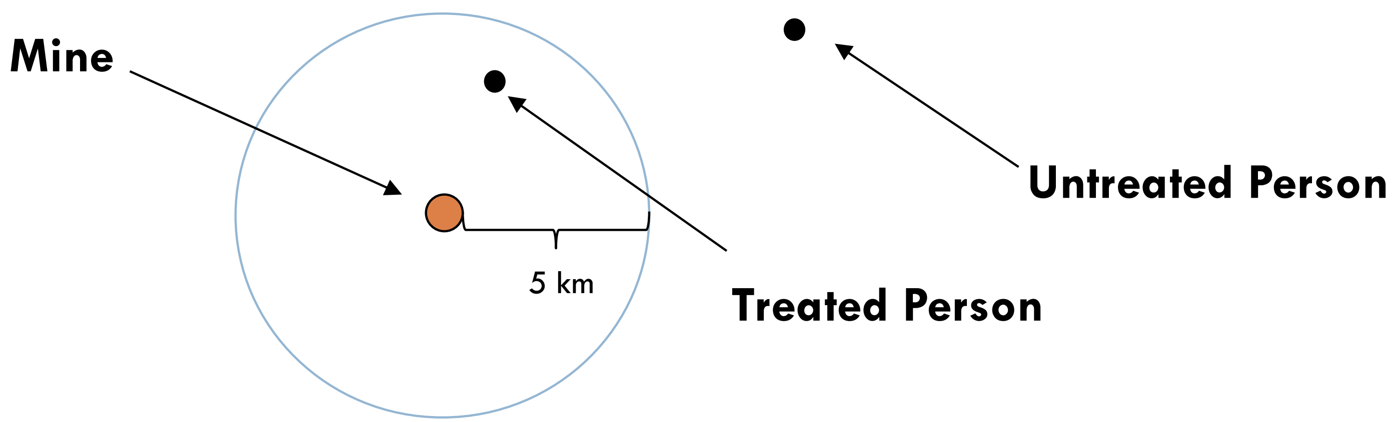
- **Okay, so now we have a problem**
  - We have omitted variables that are correlated with our variable of interest (schooling)
- **An obvious solution (called "selection on observables") that works (to an extent) is to just find measures for these omitted variables and do a multivariate analysis as follows:**

$$Wage_i = \beta_0 + \beta_1 Schooling_i + \beta_2 ParentEd_i + \beta_3 ParentInc_i + \beta_4 Exp_i + \varepsilon_i \tag{2}$$

- **But I have no choice but to leave out talent/innate ability, and personal ambition. Why?**
  - They are not measurable (arguably)

Another Example

- Let's say you want to analyze the effect of a mine opening on rural communities in sub-Saharan Africa:



- The regression would be,

$$Wages_i = \beta_0 + \beta_1 Mine(1\ if\ \leq 5km\ to\ Mine, 0\ if\ > 5km\ to\ Mine)_i + \varepsilon_i \tag{3}$$

- What's in ε_i here?

Another Example

- **Well, of course areas that receive mines will be different than those that don't. How?**
  - More likely to discover mines in areas well mapped and explored
  - More likely to develop mineral deposits in areas closer to cities and roads
  - More likely to develop mines in regions with functioning labor markets
  - Are these correlated the wages?
- **This means that those areas "selected" to have mines are systematically different than those not selected**
  - This is just a type (a special case) of omitted variable bias known as "selection bias"
    - Don't make the mistake of thinking selection bias is distinct from OVB
  - This is frequently called the "selection problem"

Another Example: The "Evaluation Problem"

- **You might be thinking, you could just compare areas near mines over time before and after the arrival of the mine to measure the effect of the mine**
- **BUT, other things are changing over time for this area unrelated to the mine**
  - General economic trends,
  - Other investment in infrastructure
  - Climate change
- **A before an after comparison will make it look like the effects of these forces are part of the mine opening**
- **Again, this is another type (a special case) of OVB**
  - This is called "the evaluation problem"

Can We Test For OVB?

- **NO! Unfortunately there is no empirical test for OVB**
  - Beware anyone who claims there is
- **So you need to use theory and social science reasoning to think critically about whether there is a reasonable worry that your regression suffers from OVB**
  - If you don't do this, then someone else will for do it for you (your boss, dissertation advisor, peer reviewers, etc.) and that is never good

Ways to Deal with OVB

- **Econometricians have thankfully devised ways to deal with <u>certain kinds</u> of OVB**
  - When these methods are applied appropriately we can still make strong statements about the (causal) relationship between X and Y
- **For some, if we have reason to believe that reverse causality is not driving the relationship between X and Y, $\widehat{\beta}_1$ can be interpreted as the "causal effect" of X on Y**

Ways to Deal with OVB

The following are the general ways to deal with OVB:

1. Signing the bias
2. Include a proxy
3. Randomized control trial (RCT)
4. Fixed effects (FE)
5. Difference in differences (DID)
6. Instrumental variable (IV)
7. Regression Discontinuity Design (RDD) (Not covered in ECON B253)

Each has limitations in what type of OVB it can control for. Also many require special situations or special data to be carried out

- An in-depth examination of these methods is really only possible over the course of a semester. Look for classes on "Impact Evaluation," "Applied Policy Analysis," or "Causal Modelling" at Penn

Ways to Deal with OVB

The following are the general ways to deal with OVB:

1. **Signing the bias**
2. Include a proxy
3. Randomized control trial (RCT)
4. Fixed effects (FE)
5. Difference in differences (DID)
6. Instrumental variable (IV)
7. Regression Discontinuity Design (RDD) (Not covered in ECON B253)

Signing the Bias

- **Approach #1 is trying to understand how the bias may affect your estimate**
  - It's possible that the bias can "work in your favor"
- **Suppose you are looking for evidence of labor market discrimination against women with the following model:**

$$wage_i = \beta_0 + \beta_1 school_i + \beta_2 male_i + u_i \quad (4)$$

- **Suppose that the coefficient on male is small and statistically insignificant with a tiny CI**
- **You conclude from your results that there seems to be little labor market discrimination against women**

Signing the Bias

- **What are potential criticisms of your conclusion?**
  - Wages depend on other productive characteristics of individuals which are omitted
    - For example, extra job training
  - Omitting this results in OVB and knowledge there is bias in the estimate of β_2
- **How might the bias affect the estimate?**
  - Let's assume for simplicity that job training is the only omitted factor here

Signing the Bias

- Wages likely increase with job training, and one might reasonably expect men to have more job training than women on average (due to labor market discrimination). Thus, this β_2 estimate is capturing the positive effect of being a man in the labor market and the positive effect of extra job training. Thus, I would therefore expect my estimate of β_2 to be ***biased upward***
- "Since I found a small and insignificant effect ***despite the likely upward bias***, I have a strong reason for concluding there is no evidence of pro-male discrimination"
  - Acknowledging the bias helps you draw an even stronger conclusion (in this particular case)

Signing the Bias

- **When you actually put together a project it is usually the case for you to suspect bias in your estimates**
  - Discussing how bias may be affecting your estimates is something that any good paper will do, unless it is the rare paper where there is no worry about OVB
  - Signing the bias, or at least trying to say for the reader how you are thinking about it, is therefore something you should take really seriously
- **Usually signing the bias only works in certain situations**
  - Results provide a strong conclusion even in the case of bias
  - The error term is simple enough that you aren't weighing multiple competing factors against each other

Ways to Deal with OVB

The following are the general ways to deal with OVB:

1. Signing the bias
2. **Include a proxy**
3. Randomized control trial (RCT)
4. Fixed effects (FE)
5. Difference in differences (DID)
6. Instrumental variable (IV)
7. Regression Discontinuity Design (RDD) (Not covered in ECON B253)

Using a Proxy

- **Sometimes even though you won't be able to measure an omitted variable perfectly, you can measure it with some level of measurement error**
- **This type of variable is called a "proxy"**
  - A variable that has measurement error but despite this is highly correlated with the omitted variable
- **If you have a proxy variable, you can include it in your model**
- **This brings it out of the error term and controls for it**

Using a Proxy

- **Examples of proxies:**
  - Binary variable = 1 if individual has started a business as a proxy for entrepreneurial ability
  - Number of decisions woman makes in in the household as a proxy for empowerment
  - Average length of time teachers have been teaching as a proxy for school quality

Using a Proxy Effectively

- **If you have an idea for a proxy, you need to argue in your paper why it is needed and why it is a good (enough) measure of your omitted variable**
  - There is really no empirical approach to validating this, it's entirely rhetorical
- **Often it's a good idea to run your estimates with and without the proxy**
  - This lets the reader decide what is more useful
- **Reading relevant literature on your topic will give you a sense of what other researchers have used as proxies and provide a guide to whether you should use them too**

Ways to Deal with OVB

The following are the general ways to deal with OVB:

1. Signing the bias
2. Include a proxy
3. **Randomized control trial (RCT)**
4. Fixed effects (FE)
5. Difference in differences (DID)
6. Instrumental variable (IV)
7. Regression Discontinuity Design (RDD) (Not covered in ECON B253)

Randomized Control Trial (RCT)

- **A randomized control trial (RCT) is a true experiment that assigns individuals or communities randomly to receive a treatment or not receive a treatment**
- **The basic idea: Remember for omitted variables to cause bias, two things must be true:**
  1. The omitted variables must help determine the outcome (Y), *and*
  2. The omitted variables must be correlated with the variable of interest (in this case being assigned to treatment or control)
- **An experiment eliminates OVB by creating a situation in which the second of these conditions does not hold**

Esther Duflo, Abhijit Banerjee, and Michael Kremer

- RCTs used to be very rare in economics
- Esther Duflo, Abhijit Banerjee, and Michael Kremer have popularized it as a method
  - They received the Nobel Prize in Economics for this

RCT Example

- **Let's look at a paper from Paul Glewwe, Michael Kremer, and Sylvie Moulin**
  - **Citation:**

Glewwe, Paul, Michael Kremer, and Sylvie Moulin, 2009. "Many Children Left Behind? Textbooks and Test Scores in Kenya" *American Economic Journal: Applied Economics* (1) 11: 112-135.

  - **Background:** Many schools in poor countries are very poor and do not have basic learning materials like textbooks
  - **Research Question:** Does providing textbooks to children in developing countries result in improved learning outcomes?

RCT Example

- Now suppose we have normal cross sectional sample of students in a poor country
- We know in the data which students have textbooks and which don't
- We can use this to run estimate the following model,

$$\underbrace{TestScore_i}_{\text{Test score for student}} = \beta_0 + \beta_1 \underbrace{TB_i}_{\text{Binary =1 if student has a textbook, =0 if not}} + \beta_2 \underbrace{Z_{i1} + \cdots + \beta_{k+2} Z_{ik}}_{\text{A bunch of control variables}} + u_i \qquad (5)$$

RCT Example

- **Why would we worry that estimating equation (4) would result in OVB?**
  - Many unobservable factors are likely both determining a student's test performance and whether they have a textbook. Like what?
    - Involvement of parents (More engaged parents means better scores and more investment in child's resources)
    - Innate motivation and drive (More drive means better study habits, more effort, and better likelihood student works and saves to buy a textbook)
- **If these are the only potential omitted variables, would we expect $\widehat{\beta}_1$ to be upward or downward biased?**
  - Upward. It will be picking up the positive effects of the child's innate drive and parental involvement

RCT Example

- **Glewwe et al. (2009) solve this issue by partnering with an NGO in Kenya to conduct the following experiment:**
  - They took a number of villages in which the NGO hoped to provide textbooks over time and randomly divided them into two or more groups
  - The "treatment" group got the textbooks immediately, the "control group" got them but after a period of delay
    - Note that the delay here was a logistical constraint faced by the NGO. It was not artificially imposed by the researchers. They simply "directed" the delay to certain villages over others in a random way

RCT Example

- Due to the randomization the unobserved components in the error term are no longer related to who received a textbook
  - In math: $TB_i \perp u_i$
- This means that the researchers can estimate the following model and achieve unbiased estimates of $\widehat{\boldsymbol{\beta}}_{\mathbf{1}}$

$$TestScore_i = \beta_0 + \beta_1 TB_i + u_i \tag{6}$$

RCT Example

- **RCTs (Glewwe et al. included) often show a table of the differences in means of various observable factors between the treatment group and the control group**
  - If randomization was effective. These means should not be different in any statistically significant way. Why?
    - If treatment is truly random then no variable determines who gets it, so $TB_i \perp Z_{ik}$
  - This is considered empirical support (although not a formal test) for the claim that the randomization of the treatment was implemented correctly
  - This is called a "balancing test" (even though it is not a formal test. Confusing!)

Glewwe et al. (2009) Results

Table 4—Impact of Textbook Program on Normalized Test Scores

Dependent variable	Normalized test score[a b] (1)	Normalized test score[b] (2)	Normalized test score minus pretest score[c] (3)	Normalized test score minus pretest score[c] (4)
Textbook school	0.023	0.020	0.018	−0.046
	(0.087)	(0.104)	(0.053)	(0.071)
Received a textbook				
Region and sex dummies	Y	Y	Y	Y
Years exposed to textbooks	1	2	1	2
Grades	3–8	4–7	3–8	4–7
Observations	24,132	12,663	11,321	7,354

Notes: Standard errors in parentheses.

Table 9—Program Impact on Normalized Test Scores, by Quintile of Pretest Scores

Years exposed	Quintile 1 (1)	Quintile 2 (2)	Quintile 3 (3)	Quintile 4 (4)	Quintile 5 (5)
1	−0.049	−0.021	0.032	0.142*	0.218**
	(0.064)	(0.069)	(0.073)	(0.079)	(0.096)
2	−0.077	−0.109	−0.089	0.021	0.173
	(0.081)	(0.094)	(0.104)	(0.101)	(0.131)

Example of a Balancing "Test"

- **Unfortunately Glewwe et al. don't provide a balancing test for us to look at. We'll look at another paper. Here's the citation,**

Blattman, Christopher, Eric P. Green, Julian Jamison, M. Christian Lehmann, and Jeannie Annan, 2016. "The Retuerns to Microenterprise Support among the Ultrapoor: A Field Experiment in Postwar Uganda." *The American Economic Journal: Applied Economics* 8 (2): 35 – 64.

Research Question: Does microfinance support improve people's welfare? If so, how much?

Some Background on Microfinance

- **If you have an innovative idea and want to start a business, what is the first thing you'll do?**
  - (1) Write up a business plan
  - (2) Find investors and borrow their money (access credit)
    - If you default on a loan, creditors can collect collateral (mitigates risk)
- **Problem:**
  - Credit is not generally available to poor people in developing countries
- **Reasons: <u>Poor people seen as risky, low return</u>**
  - Poor people lack collateral $\Rightarrow$ Higher risk
  - Poor people lack traditional credentials $\Rightarrow$ Higher risk
  - Business projects in poor economies usually low return

Potential Huge Missed Opportunity

- **But think about how many amazing ideas are being ignored by this? (TFP implications)**
  - Innovative talent not cultivated
- **Also, so many product and services not provided to poor people!**
  - Huge market of poor people not accessed (a lot of unmet demand)
- **Solution: Microfinance**
  - Idea: Provide poor entrepreneurs small loans (sometimes 15 USD) to start businesses
    - Smallness of loan mitigates risk
    - Mitigate risk in other ways through training programs and mentorship

History

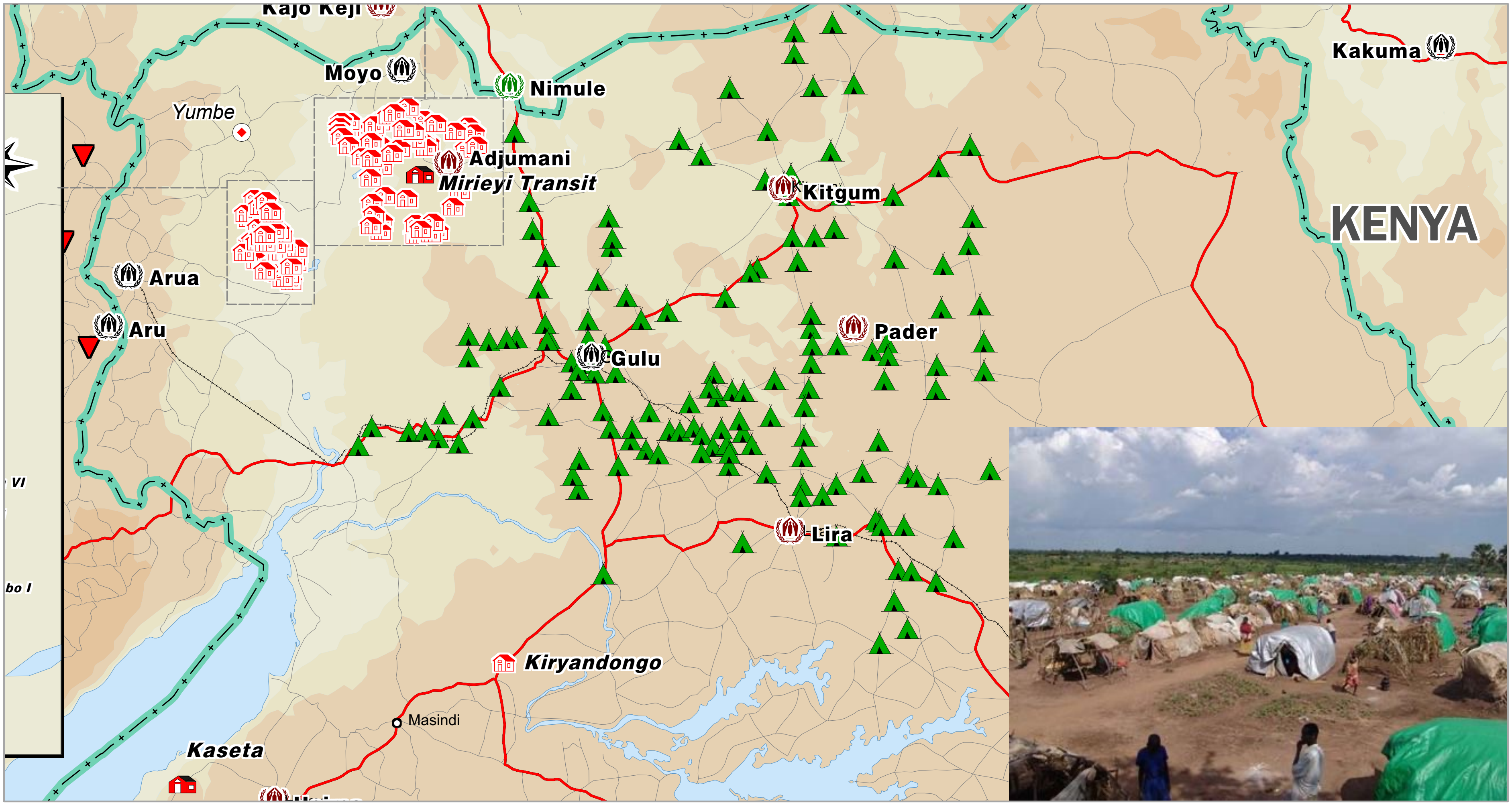
- **Usually traced back to Muhammad Yunus**
  - Economist from Bangladesh
  - Started Grameen Bank in 1982
- **Becomes Very Popular since 2000**
  - 1997: 7.6 million families with microloans, 2010: 137.5 million families with microloans (18x increase)
  - Commercial banks as well as NGOs participate
- **Yunus wins Nobel Peace Prize in 2006**
- **But does it work?**
  - Lots of anecdotal evidence it works, but is this just marketing by the banks?
  - Surprisingly this is still very controversial and a topic of serious debate

Study Context

- **Kitgum and Gulu Districts in Northern Uganda**
- **Region has "rebel bands" operating 1987-2006 conscripting, abusing and stealing from civilians**
  - 1/5 people abducted, usually for short periods of time
  - 5% forced to "marry" rebel leader
- **Government of Uganda moves <u>everyone</u> into camps**
  - Lived there for at least 3 years, losing farms and homes
- **2006 rebels defeated (mostly)**
- **In 2007, 2/3 of population live on food aid**
- **By 2009 (start of study), most people had rebuilt homes and started to farm again**

- Northern Uganda experiences an insurgency with a major secessionist movement in the North
- Study takes place around Kitgum and Gulu

Northern Uganda Map



Internally Displaced Persons Camps (Green) in Northern Uganda. Data: UNHCR

Experimental Design

- **Randomized exposure of the Women's INcome Generating Support program (WINGS) among 1,800 poor people (mostly women) in 120 war-affected villages**
  - 150 USD (~375 USD PPP) grant (30x larger than avg. monthly earning)
    - Meant to start small retail and trading businesses
  - 5 days of training in business skills, marketing, help planning
  - Ongoing supervision and mentorship
  - Half of the treatment villages received encouragement and support in setting up "self-help" groups and training in working together
    - Help forming a rotating savings and credit association

Data

- **120 villages**
- **60 randomly chosen to receive WINGS first (Phase 1) remaining 60 notified they would have to wait to get it ~18 months later (Phase 2)**
  - Within these, 30 were chosen to receive self-help group support – way to test for social network effects
- **1,800 in sample 16 months after first grant**
- **Minimal attrition (3.7%)**

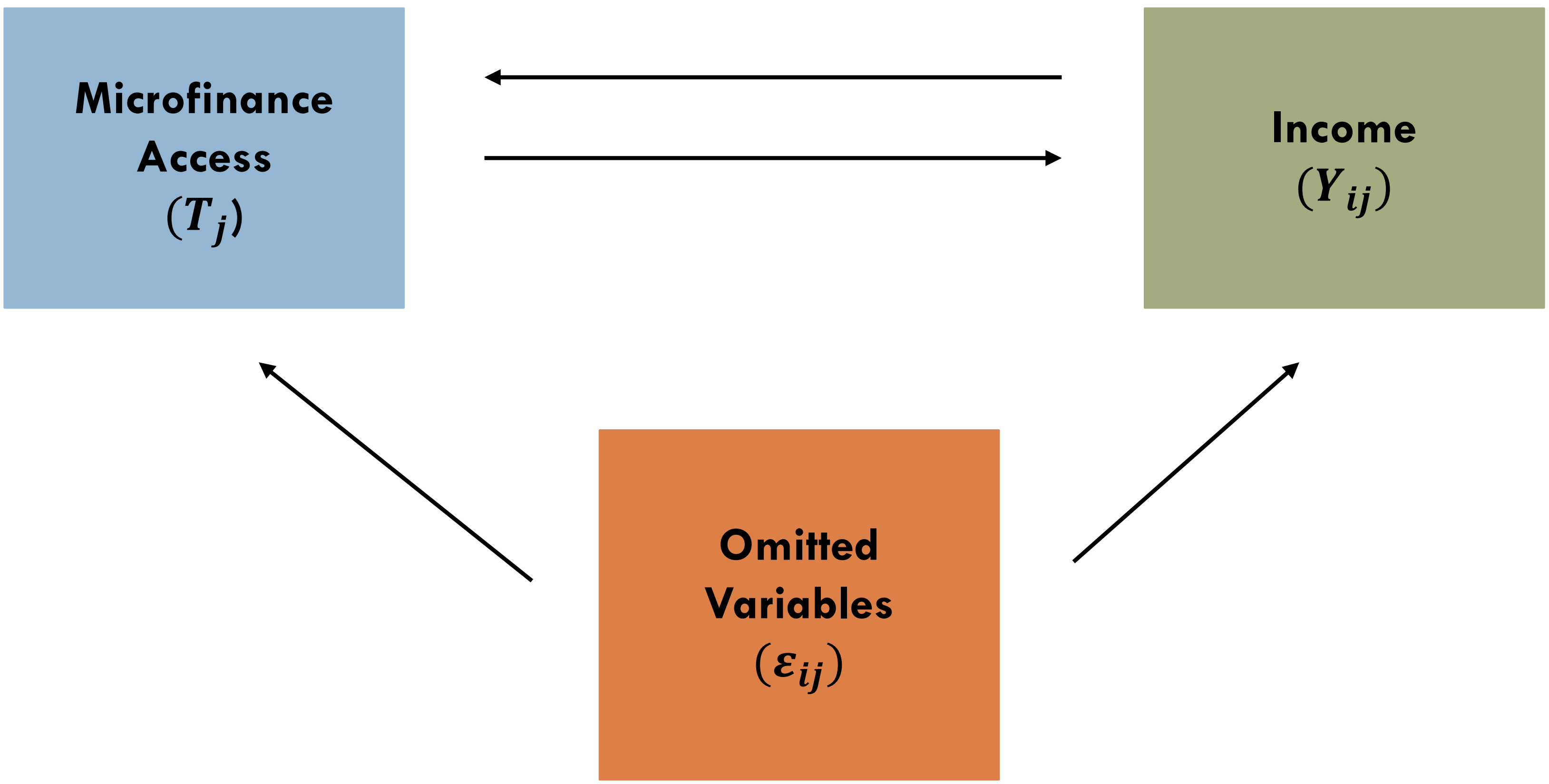
Balancing Tests

Table 1—Descriptive Statistics and Randomization Balance for Select Covariates

Variable	Means, full sample: Treatment (Observations = 896)	Means, full sample: Control (Observations = 904)	Balance test: Difference	Balance test: p-value
	(1)	(2)	(3)	(4)
Demographics				
Age	27.02	27.63	−0.62	0.17
Female	0.86	0.86	−0.01	0.72
Married or living with partner	0.46	0.50	−0.05	0.26
Single-headed household	0.51	0.47	0.04	0.17
Highest grade reached at school	2.82	2.75	0.07	0.70
Forcibly recruited into rebel group	0.20	0.25	−0.05	0.03
Carried gun within rebel group	0.03	0.04	−0.01	0.45
Forcibly married within rebel group	0.03	0.03	−0.00	0.63

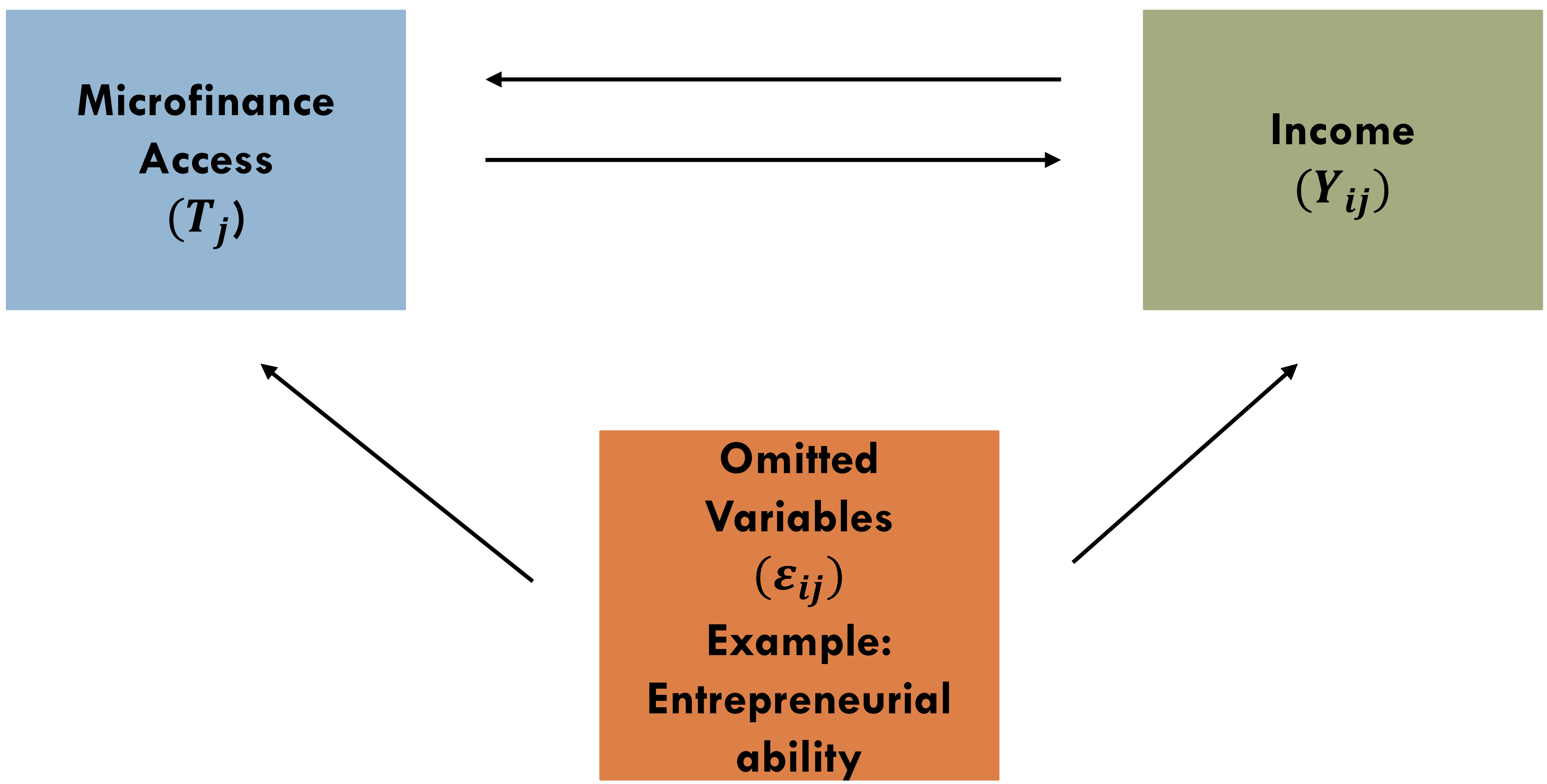
Identification Strategy: RCT

- Usual worry is that income will be affected by omitted variables



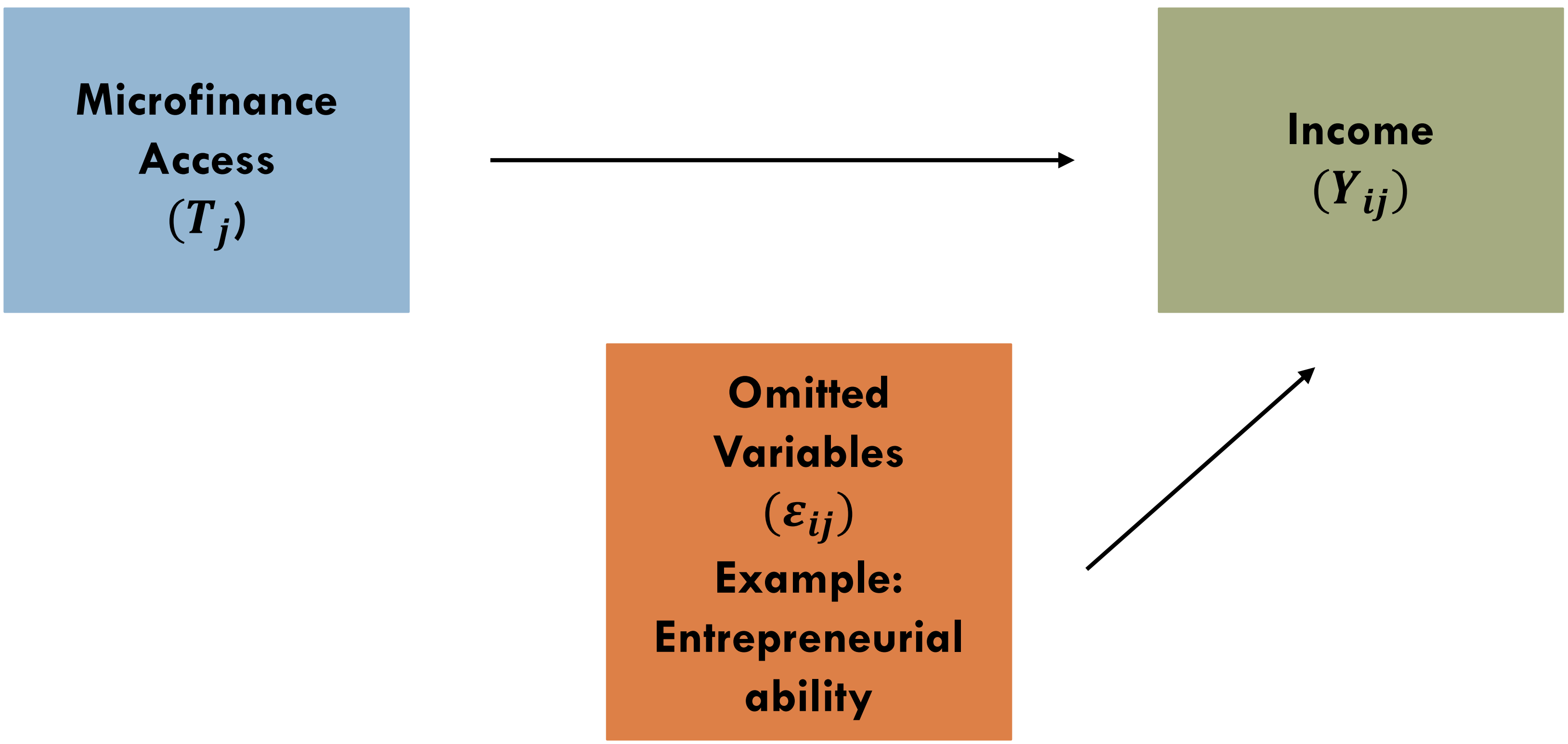
Identification Strategy: RCT

- Usual worry is that income will be affected by omitted variables



Identification Strategy: RCT

- Usual worry is that income will be affected by omitted variables



Identification

- **Regression framework:**

$$Y_{ij} = \theta T_j + \delta_T D_j^T + \delta_{\bar{T}} D_j^A + \boldsymbol{X}_{ij}\beta + \varepsilon_{ij} \qquad (s.6)$$

Outcome

Binary = 1 if in Treatment Group

Distance to treatment villages

Distance to all other villages

Control variables

Ways to control for spillover effects

***θ* is the coefficient of interest. Everything else is a control.**

It will provide a measure of the “Intent to Treat” (ITT) the effect of offering the program.

Note how this is subtly different than the effect of the treatment itself.

TABLE 3—ECONOMIC IMPACTS OF WINGS PROGRAM AND GROUP FORMATION

Outcome	Control mean (1)	ITT estimates, 16 months after grants (Observations = 1,734) No group training (2)	Group training (3)	Difference (4)
Occupational choice				
Any nonfarm self-employment	0.39	0.401	0.409	0.008
		[0.030]***	[0.033]***	[0.034]
Started enterprise since baseline	0.50	0.487	0.485	−0.002
		[0.025]***	[0.025]***	[0.018]
Average work hours per week	14.98	9.391	9.877	0.486
		[1.608]***	[1.794]***	[2.029]
Agricultural	9.52	3.496	4.002	0.506
		[1.389]**	[1.300]***	[1.524]
Nonagricultural	5.342	5.895	5.875	−0.020
		[0.893]***	[0.916]***	[1.217]
Average hours of chores per week	39.75	0.305	1.416	1.111
		[1.013]	[1.203]	[1.301]
Income and food security				
Index of income measures, *z*-score	−0.22	0.464	0.616	0.151
		[0.068]***	[0.080]***	[0.087]*
Monthly cash earnings, 000s UGX	15.53	10.372	23.390	13.018
		[3.443]***	[4.607]***	[5.512]**
Durable assets (consumption), *z*-score	0.12	0.327	0.384	0.056
		[0.067]***	[0.068]***	[0.073]
Monthly nondurable consumption, 000s UGX	107.74	31.031	33.439	2.408
		[5.010]***	[5.227]***	[6.049]
Durable assets (production), *z*-score	−0.02	0.402	0.397	−0.005
		[0.064]***	[0.058]***	[0.064]
Times went hungry, past week	0.19	−0.098	−0.084	0.013
		[0.039]**	[0.036]**	[0.043]
Usual number of meals per day	1.76	0.057	0.078	0.021
		[0.028]**	[0.031]**	[0.031]

Notes: All variables denominated in UGX and hours were top-censored at the nintey-ninth percentile to contain outliers. Columns 2 and 3 report the coefficients and standard errors on indicator for assignment to Phase 1 without and with the group dynamics component in an OLS regression of each outcome on treatment indicators, a Gulu district (strata) fixed effect, and baseline covariates. Column 4 reports the difference between the two treatment groups. Standard errors are robust and clustered at the village level.

***Significant at the 1 percent level.
**Significant at the 5 percent level.
*Significant at the 10 percent level.

Important Notes on RCTs

- **In practice, you still include the control variables when in the estimate of equation (5) to soak up noise in the data and improve precision (minimize the standard errors), but it shouldn't affect the point estimate**
- **RCTs are a great solution to the old OVB conundrum, but here are the downsides:**
  - They are time consuming
  - They are expensive
  - They require a treatment that can be randomized
    - Many phenomena we'd want to study are not able to be randomized either because they are unethical or by their nature they can't be controlled by researchers (example: refugee camp proximity)
  - Results may not be "externally valid"
    - Do Glewwe et al.'s (2009) findings hold for contexts outside of Western Kenya? Maybe? We don't really know

Worries with RCTs

- **There are still worries you should be aware of when working with RCT data or reading RCT work,**
  - **Drop outs:** If participants drop out of the treated group or control group or the researcher can't find them
    - If people are dropping out based on some unobserved characteristic related to the outcome variable then you have OVB again
  - **Imperfect randomization:** If randomization wasn't managed properly

Ways to Deal with OVB

The following are the general ways to deal with OVB:

1. Signing the bias
2. Include a proxy
3. Randomized control trial (RCT)
4. **Fixed effects (FE)**
5. Difference in differences (DID)
6. Instrumental variable (IV)
7. Regression Discontinuity Design (RDD) (Not covered in ECON B253)

(Individual) Fixed Effects (FE)

- **Recall "fixed effects" from our discussion on binary regressors**
  - If you have a group structure to your data you can control for all the time-invariant attributes of the group
    - Groups can be state of residence, occupational categories, and many others
  - Requires a series of binary variables for all but one of the groups
- **It turns out that you can do this for individuals and control for <u>individual time-invariant</u> factors**
  - But how can you have a group structure to your data at the individual level if your data are composed of individuals?
    - Answer: Panel Data!

(Individual) Fixed Effects (FE)

- **Fixed effects in this way requires the researcher to have panel data. What is that?**
- **Panel data is a dataset that contains multiple observations per unit over time. Examples:**
  - You have a dataset of 120 firms, each observed once a year over 10 years for a total of 1,200 observations
  - You have a dataset of 200 farms, each observed once a year over two separate years five years apart. How many observations would this provide in your dataset? (400)
- **Panel data can either be "balanced" and have all units observed in all time periods, or "unbalanced" and have some units observed in some time periods**
  - Fixed effects methods can use both

FE: Example of Panel Data

Individual ID	Time Period	X	Y
001	1	0.5	20
001	2	1	27
002	1	1.25	12
002	2	3	15
003	1	0.7	17
003	2	6	39

This is just like datasets we've used before, but now there are multiple observations for each individual.

Is this example "balanced" or "unbalanced"?

FE: Value of Panel Data

- **Panel data are highly valued. Why?**
  - Simple cross sections allow us to look only at how levels of outcome variables (Y) related to levels of determinants (X)
  - Panel data allows us to examine how changes over time in outcome variables relate to changes over time in determinants
  - In many cases this allows us to minimize problems related to OVB
    - Just like in group FE that we studied earlier, individual FE controls for all time-invariant characteristics, but in this case at the level of the individual. This can be very powerful
      - Note that individual FE will absorb any larger group FE that are time invariant

Implementing Individual Fixed Effects with Panel Data

- **Notation changes with panel data. Since each observation comes from an individual (or firm, etc.) i in a particular time period t, we need to add a t subscript to our model specification as follows,**

$$Y_{it} = \beta_0 + \beta_1 X_{it} + \beta_2 Z_{it} + u_{it} \tag{7}$$

- **If you have panel data with only 2 time periods, you can implement individual FE with the method of "First Differences" by taking the difference between each individual's observation. For example,**

$$\Delta X_{it} = X_{it} - X_{it-1} \tag{8}$$

And then estimating the model,

$$\Delta Y_{it} = \delta_0 + \beta_1 \Delta X_{it} + \beta_2 \Delta Z_{it} + \Delta u_{it} \tag{9}$$

Implementing Individual Fixed Effects with Panel Data

- **The best way to see how First Differences works to eliminate the bias from time invariant factors at the individual level is to first think of the error term as being composed of a time variant component ε_{it} and a time-invariant component η_i, such that**

$$u_{it} = \varepsilon_{it} + \eta_i \tag{10}$$

- **Now, take a look at the two separate models you can estimate within the panel data,**

$$Y_{it} = \beta_0 + \delta_0 + \beta_1 X_{it} + \beta_2 Z_{it} + u_{it} \tag{11}$$

$$Y_{it-1} = \beta_0 + \beta_1 X_{it-1} + \beta_2 Z_{it-1} + u_{it-1} \tag{12}$$

Implementing Individual Fixed Effects with Panel Data

- **Now, substitute equation (10) into equation (11) and the time-lagged equation (10) into equation (12),**

$$Y_{it} = \beta_0 + \delta_0 + \beta_1 X_{it} + \beta_2 Z_{it} + \varepsilon_{it} + \eta_i \qquad (11)$$

$$Y_{it-1} = \beta_0 + \beta_1 X_{it-1} + \beta_2 Z_{it-1} + \varepsilon_{it-1} + \eta_i \qquad (12)$$

- **Now take a look at what a regression of ΔY_{it} on ΔX_{it} and ΔZ_{it} results in,**

$$Y_{it} - Y_{it-1} = \beta_0 + \delta_0 - \beta_0 + \beta_1(X_{it} - X_{it-1}) + \beta_2(Z_{it} - Z_{it-1}) + \varepsilon_{it} + \eta_i - (\varepsilon_{it-1} + \eta_i) \qquad (13)$$

$$\Longrightarrow Y_{it} - Y_{it-1} = \beta_0 + \delta_0 - \beta_0 + \beta_1(X_{it} - X_{it-1}) + \beta_2(Z_{it} - Z_{it-1}) + \varepsilon_{it} - \varepsilon_{it-1} + (\eta_i - \eta_i) \qquad (14)$$

Implementing Individual Fixed Effects with Panel Data

$$Y_{it} - Y_{it-1}$$
$$= \delta_0 + (\beta_0 - \beta_0) + \beta_1(X_{it} - X_{it-1}) + \beta_2(Z_{it} - Z_{it-1}) + \varepsilon_{it} - \varepsilon_{it-1}$$
$$+ \underbrace{(\eta_i - \eta_i)}_{\text{The time-invariant component gets differenced out!}} \tag{15}$$

Remember what is in η_i. It is anything time invariant at the individual level. Note that the individual level subsumes anything time-invariant at a broader scale

- **So this is what you are doing when you estimate the model,**

$$\Delta Y_{it} = \delta_0 + \beta_1 \Delta X_{it} + \beta_2 \Delta Z_{it} + \Delta u_{it} \tag{16}$$

More on this Last Point

- **An individual's characteristics will contain <u>all</u> individual characteristics, even those shared with others**
  - This *<u>includes their group membership</u>*
    - Examples: Ethnicity, sex, state of residence, etc.
- **So when you include individual fixed effects you don't need to control for all these other factors (if they are time invariant)**
  - Indeed they'll be perfectly collinear with them and Stata will drop them from your estimates

This is the Same As Including a Binary Variable for Each Individual (Omitting 1)

- **Again, suppose we have the model:**

$$Y_{it} = \beta_0 + \delta_0 + \beta_1 X_{it} + \beta_2 Z_{it} + \varepsilon_{it} + \eta_i \qquad (17)$$

- **η_i is just the individual-specific time-invariant unobserved component of the error term**
- **If we have panel data, we can just estimate η_i for each individual by including a binary for each individual in the dataset**
  - This pulls η out of the error term and accomplishes the same thing as the first differencing (when you only have observations over two time periods in your data)
  - Adding the FE is more general than first differencing in that you can use it when $t \geq 2$

Fixed Effects more Generally

- **If you have unbalanced panel data, or you have more than one time period, you can estimate these models by simply including a binary variable for all but one individual in the data**
  - Remember you need to leave one out to avoid collinearity
  - The coefficient captures all the variables that make that individual special compared to the omitted category
- **This produces a lot of coefficients that you'll just ignore**
  - To avoid this, I recommend using the "`reghdfe`" command in Stata at http://scorreia.com/software/reghdfe/

Last Points on Fixed Effects

- **Individual-level fixed effects is extremely powerful and very common**
- **Issues to be aware of:**
  - Impossible to use if outcome variable or variable of interest is NOT time variant
  - It doesn't control for time-variant omitted variables
    - Special worry: If your "individual" data is actually some aggregate like a state or a country, it won't control for changes in the composition in the population over time (like migration)
  - It doesn't control for reverse causality
  - Requires panel data which can be hard to find
    - Special note: Don't criticize a paper for not using FE when all the researcher has to work with is cross-sectional data

Last Points on Fixed Effects

- **Most important point:** Fixed Effects only controls for time-invariant factors
  - If you have reason to believe the omitted variables causing OVB vary over time, then this approach isn't super helpful and you should be very cautious and think of other approaches that may be open to you
  - This requires a lot of thought on your part. Here are some factors that economists are generally willing to believe are time-invariant
    - Geographic characteristics
    - Innate traits
    - Institutional structure (over reasonable periods of time)

Last Points on Fixed Effects

- **Still be very cautious,**
  - Some things maybe time-invariant over shorter periods like 2-3 years, while those same things are time-variant over 10-15 years
    - If you're panel data rounds have a lot of time in between them, then you are probably going to be controlling for less stuff
      - For example, governmental policy changes over 10-15 years, but little over 2 years
      - Land quality may be another okay example here

For Students Looking to Develop Econometrics Projects with Panel Data

- **Here is a short list of good panel datasets that are accessible to researchers:**
  - The Living Standards Measurement Surveys (LSMS)
    - Household survey. Good for many things especially agriculture. Available for a subset of countries in sub-Saharan Africa
  - World Bank Enterprise Surveys (WBES)
    - Firm-level surveys available in many developing countries. Panel data available for a subset of countries
  - Penn World Tables (PWT)
    - Country-level panel for every country in the world. Great for macro questions
  - World Bank Governance Indicators (WGI)
    - Country-level panel of governance-related measures for the world
- **There are many many more (you might be surprised how much is out there)**
  - Much of the skill in becoming a researcher is knowing what datasets offer you which measures for which populations

Fixed Effects Example: Paper Intro

- **As an example, we're going to look at a a recent paper by Eleonora Bertoni, Michele Di Maio, Vasco Molini, and Roberto Nisticò. Here's the citation,**

Bertoni, Eleonora, Michele Di Maio, Vasco Molini, and Roberto Nisticò, 2019. "Education is forbidden: The effect of the Boko Haram conflict on education in North-East Nigeria" Journal of Development Economics 141: 102249.

- **Research Question: Has exposure to the Boko Haram conflict affected school enrollment in North-East Nigeria? If so, how much?**

Fixed Effects Example: Background

- **Boko Haram is an insurgency movement in Northern Nigeria**
  - It translates to "(Western) Education is Forbidden"
  - Its objective is create an Islamic state in the region
  - Waged an armed conflict against the Nigerian Government since 2009
  - Explicitly targeted the Nigerian education system, assaulting schools, students and teachers

Fixed Effects Example: Data Intro

- **Data on humans: Nigeria General Household Survey Survey Panel**
  - Panel data on individuals from 5,000 households over three waves:
    - August 2010 to April 2011
    - Sept. 2012 to April 2013
    - August 2015 to May 2016
  - Village geospatial coordinates included
- **Data on conflict: Armed Conflict Location and Event Data (ACLED)**
  - Conflict events in Africa with time and geospatial coordinate

Fixed Effects Example: Data Intro

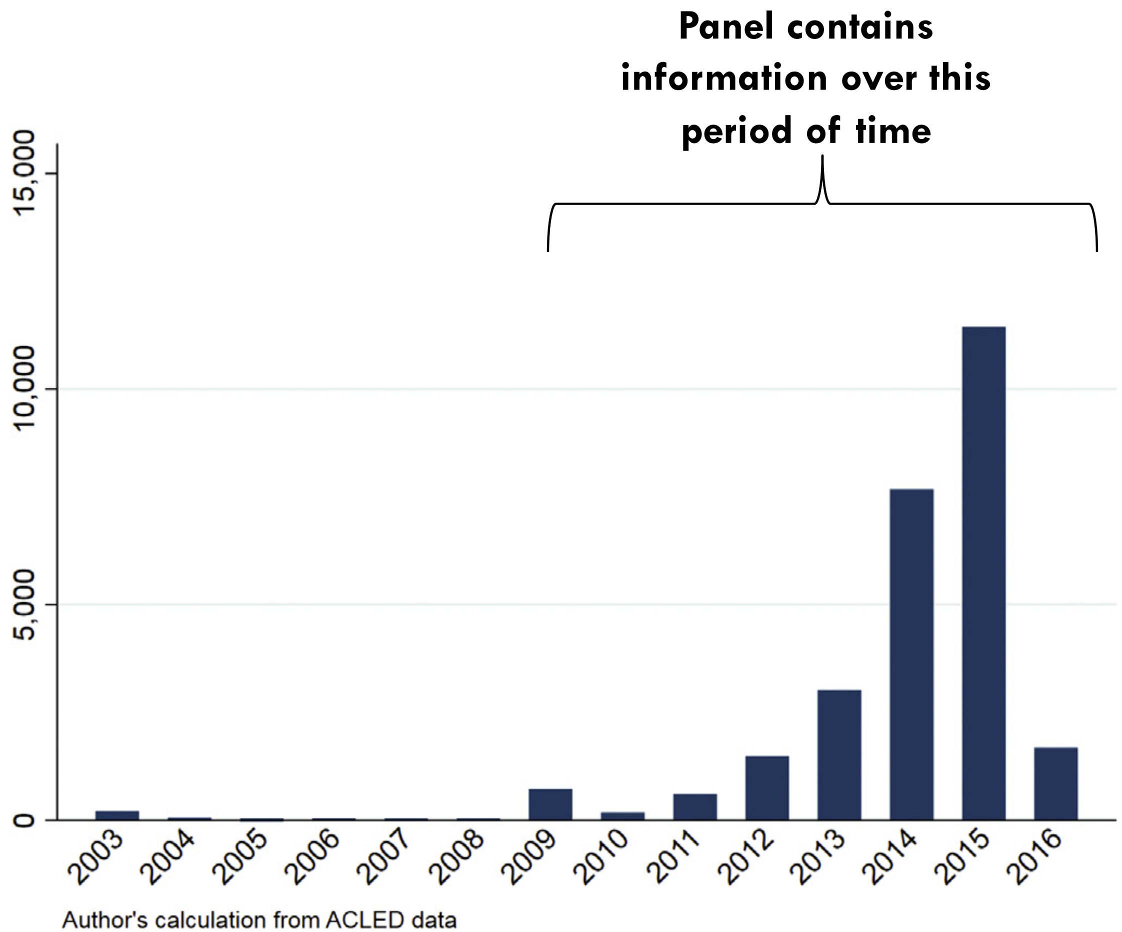
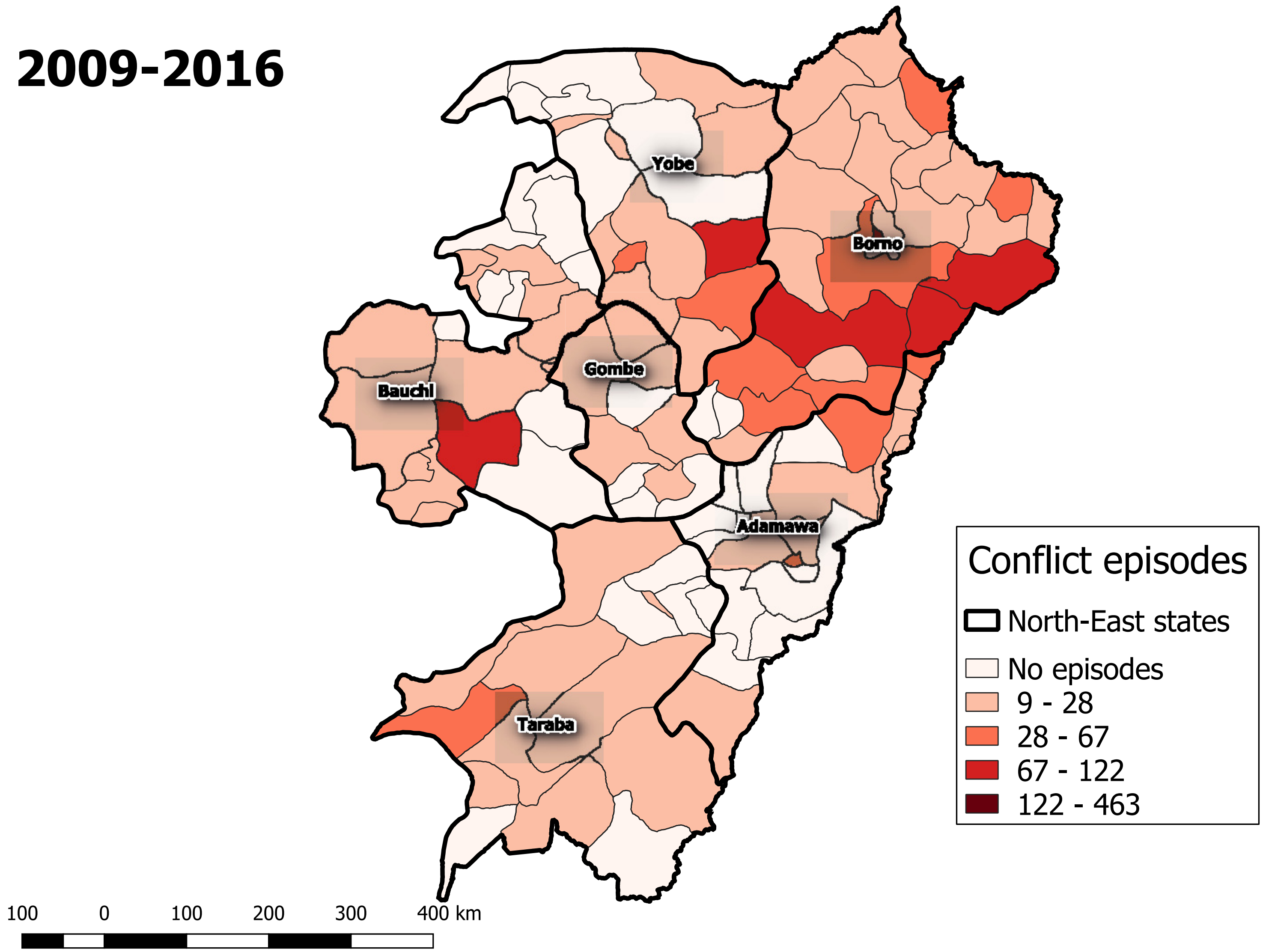


Fig. 1. Total number of fatalities in North-East Nigeria for the period 2003–2016.



Notes. The figure plots the total number of conflict events in the North-East Nigeria as recorded in the ACLED dataset (Source: ACLED).

Fig. 2. Conflict episodes in North-East Nigeria (2009–2016).

Fixed Effects Example: Model

- Here is the model that Bertoni et al. start with:

Binary = 1 if respondent i is enrolled in school in academic year t, 0 otherwise

$$Enrollment_{ijvlt}$$

Total number of conflict fatalities within 5 km of individual i's village in prior year $(t-1)$

$$= \beta_0 + \beta_1 Fatalities_{v,t-1} + X_{jvlt} + \mu_i + \theta_t + \lambda_{lt}$$

Time-variant household variables

Ind. Fixed Effects

Year Fixed Effects

$$+ u_{ijvlt} \qquad (18)$$

Linear time trends at local government area

Fixed Effects Example: Model

- **Note how this is working**
  - Enrollment can be time variant and very plausibly would change more in a conflict environment
  - Conflict fatalities is varying over time due to the conflict
- **What are omitted variables that would cause concern for bias here?**
  - Anything related to proximity to conflict fatalities and an individual’s school enrollment
    - What could these be?
    - Here’s an idea: Terrain ruggedness (more rugged $\Longrightarrow$ easier for Boko Haram to hide and more likely to see conflict, more rugged $\Longrightarrow$ school harder to get to and less enrollment)

Fixed Effects Example: Results

Table 1
Effect of the Boko Haram conflict on school enrolment.

	School enrolment		
	(1)	(2)	(3)
Fatalities (5 km)$_{v,t-1}$	-0.0003^{**}	-0.0004^{**}	-0.0009^{***}
	(0.0001)	(0.0002)	(0.0002)
Controls	No	Yes	Yes
Individual FE	Yes	Yes	Yes
School year FE	Yes	Yes	Yes
LGA-specific time trends	No	No	Yes
Clusters	55	55	55
Individuals	848	848	848
Observations	3709	3709	3709

Notes. Robust standard errors, clustered at the LGA level, are reported in parenthesis. The dependent variable, *School enrolment*, is a dummy that takes value 1 if individual i from household j living in village v located in LGA l is enrolled in school during the school year t and 0 otherwise. The main explanatory variable, *Fatalities* (5 km)$_{v,t-1}$, measures the number of fatalities occurred in the 5 km radius of the village v during the previous academic year $t-1$. The estimation sample includes individuals whose year of birth is $1998 \leq k \leq 2003$, i.e. aged 6 to 11 as of 2009, when the Boko Haram conflict started. *Controls* include: household size, percentage of female children aged 6–11 in the household, percentage of male children aged 6–11 in the household, household head employed in agriculture. * significant at 10%, ** significant at 5%, *** significant at 1%. Sources: Nigeria GHS and ACLED.

Fixed Effects Example: Results

Table 2
Effect of the Boko Haram conflict on school enrolment: Timing of fatalities.

	School enrolment		
	(1)	(2)	(3)
Fatalities (5 km)$_{v,t-1}$	-0.0009^{***}	-0.0008^{***}	-0.0008^{**}
	(0.0002)	(0.0003)	(0.0004)
Fatalities (5 km)$_{v,t-2}$		-0.0003	
		(0.0004)	
Fatalities (5 km)$_{v,t-3}$		-0.0003	
		(0.0006)	
Fatalities (5 km)$_{v,t}$			0.0012
			(0.0010)
Fatalities (5 km)$_{v,t+1}$			-0.0005
			(0.0003)
Controls	Yes	Yes	Yes
Individual FE	Yes	Yes	Yes
School year FE	Yes	Yes	Yes
LGA-specific time trends	Yes	Yes	Yes
Clusters	55	55	55
Individuals	848	848	848
Observations	3709	3709	3709

Ways to Deal with OVB

The following are the general ways to deal with OVB:

1. Signing the bias
2. Include a proxy
3. Randomized control trial (RCT)
4. Fixed effects (FE)
5. **Difference in differences (DID)**
6. Instrumental variable (IV)
7. Regression Discontinuity Design (RDD) (Not covered in ECON B253)

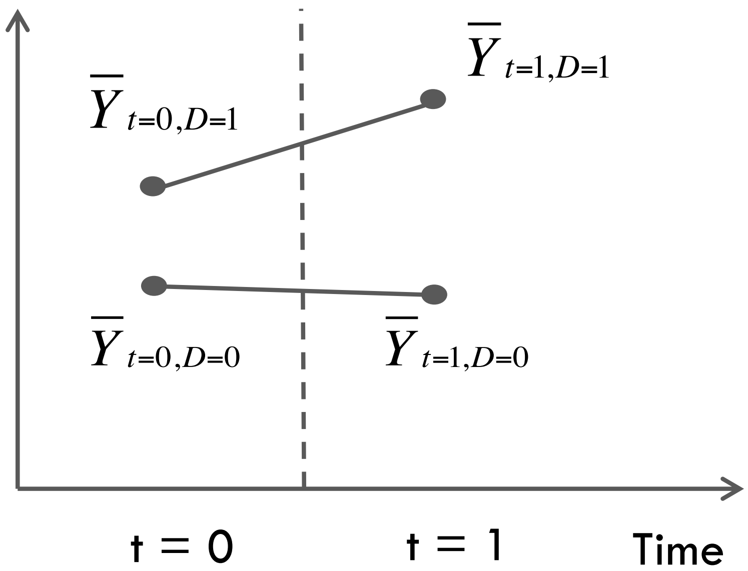
Difference In Differences (DID)

- **Difference-in-Differences is often called a "natural experiment" or a "quasi experiment"**
- **It contains three components:**
  1. **Some event that causes a change in some important RHS variable for one group (the "treatment group") and not another (the "control group")**
     - Discovery of a mine close to a village and far from another village
     - A policy change applying to one group and not the other
     - Microfinance program arrives in one village and not another

DID: Required Components Continued

2. **Data are available from both the treatment and the control groups from before *and* after the change**
   - Panel data can give you this, but repeated cross sections over time also allows for DID
3. **There is reason to believe that changes over time in omitted variables cause identical changes in average outcomes for the two groups**
   - This last point is critical. It is often called the parallel trends assumption
   - If it is true, then DID estimates a causal effect of the event mentioned in component 1
   - If you have enough data and the right kind of data, this is (sort of) testable!

DID: Graphical Depiction



The DID estimator in this set up is then:

$$\hat{\delta}_{DID} = (\overline{Y}_{t=1,D=1} - \overline{Y}_{t=0,D=1}) - (\overline{Y}_{t=1,D=0} - \overline{Y}_{t=0,D=0}).$$

We estimate the same difference (and obtain a standard error) by running the OLS regression:

$$Y_{it} = \beta_0 + \delta(D_i \times T_t) + \mu T_t + \gamma D_i + \varepsilon_{it}.$$

Graph Explanation:

- Suppose some important event occurs to just one group $D = 1$, in time period $t = 1$.
- Nothing happens to the other group $D = 0$.
- You track what happens to both groups over time. The difference in the outcome over *time* between the two groups is the DID measure of the effect of the event, $\hat{\delta}_{DID}$

DID: Why Over Time?

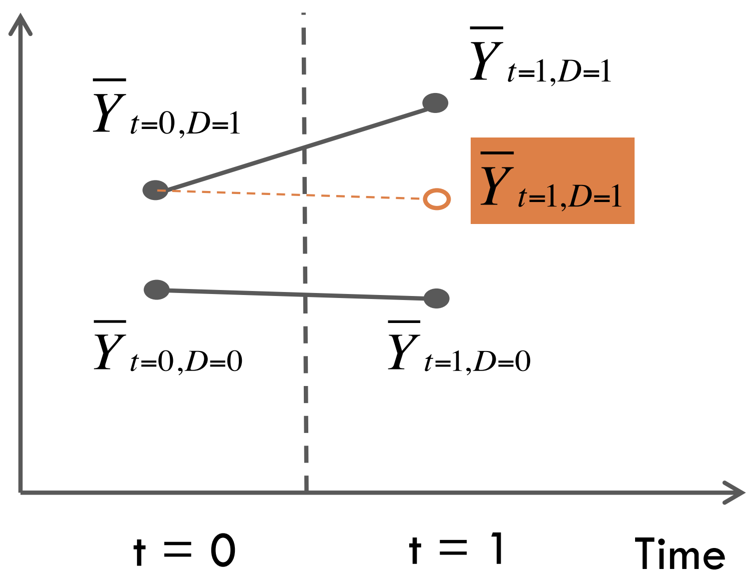
- **If you just compare differences after treatment you have OVB because of selection bias**
  - Maybe the treatment group is special in some way. In the chart their level of Y starts off higher!
  - Again, this is often called the "selection problem"

- **If you just measure the treatment group before and after the program you get OVB because you aren't accounting for other factors affecting Y that are changing over time**
  - Again, this is often called the "evaluation problem"

- **Taking the difference between the two groups address both**

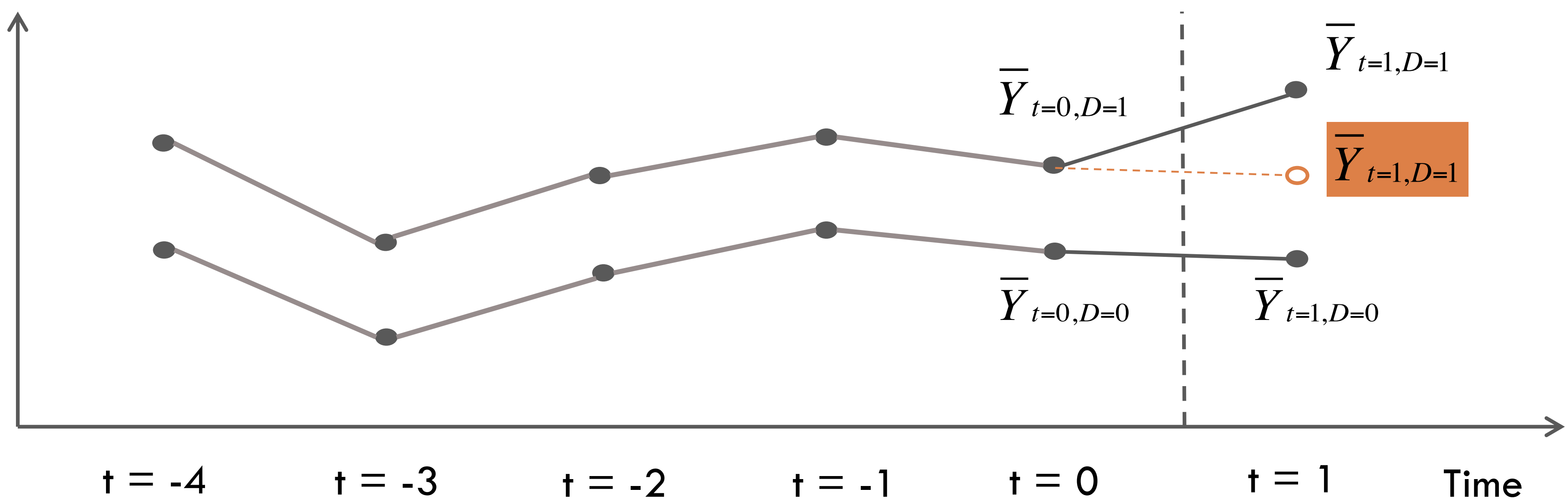
DID: Parallel Trends Assumption

- **This method only solves the evaluation problem if the trends in the outcome for the two groups would be the same in the absence of the event occurring**
  - Called the "parallel trends" assumption
  - Graphically, it is saying:
  - It is testable if you have data for Y for both groups going back in time



DID: Parallel Trends Assumption

- Test: If you have data for Y for both groups going back in time, you can look to actually see if parallel trends has held for the two groups in the past:



DID: Advantages

- **Consensus is that "parallel trends" is a relatively weak assumption**
  - If you have the data to test it and it supports the assumption your measure can be really convincing
- **It has an experimental structure with a treatment and a control group**
  - Often called "quasi-experimental" or a "natural experiment"
- **Within the regression framework, you can add time-variant controls in X or fixed effects**
  - Requires the even weaker assumption that parallel trends holds *after controlling for X and the FE*
- **Easily generalizable to a situation where treatment is occurring at multiple periods of time to multiple difference groups**

DID: Advantages

- If you have enough pre-treatment data you can test whether parallel trends had held in the past for your two groups like I showed before
- If you have enough data over time, you can look at whether the effect changed (got stronger or weaker) over time

Why Called "<u>Quasi</u>-experimental"

- **Like a true experiment:**
  - It compares a treatment and control group
- **Unlike a true experiment:**
  - The two groups are not identical by randomization
  - This means it is important to consider whether differences in the groups means different trends over time

DID: Regression Model

- **Of course the DID estimate can be calculated simply as:** $\hat{\delta}_{DID} = (\overline{Y}_{t=1,D=1} - \overline{Y}_{t=0,D=1}) - (\overline{Y}_{t=1,D=0} - \overline{Y}_{t=0,D=0})$.
  - But this provides <u>no standard error and doesn't allow for additional controls</u>
- **For this reason we usually estimate DIDs in a regression framework similar to the following,**

$$Y_{it} = \beta_0 + \boxed{\delta}\underbrace{(D_i \times T_t)}_{\text{Interaction of treatment over time compared to the non-treated groups}} + \mu \underbrace{T_t}_{\text{Control for time period (Change in time between the two groups)}} + \gamma \underbrace{D_i}_{\text{Control for Treatment group}} + \underbrace{X_{it}}_{\text{Variables that you worried my differ systematically over time between control and treatment groups}} + \varepsilon_{it} \qquad (19)$$

Coefficient of interest = $\hat{\delta}_{DID}$

Components of the DID Specification

$$Y_{it} = \beta_0 + \delta(D_i \times T_t) + \mu T_t + \gamma D_i + X_{it} + \varepsilon_{it} \tag{19}$$

- **Includes determinants that vary over time (X_{it})**
- **Allows for an intercept shift related to the group to pick up the effect of group differences that are constant over time (D_i)**
- **Allows for an intercept shift related to time period to pick up the effect of time periods that are the same for both groups (T_t)**
- **An interaction term allows for a time effect to differ between the the treatment and control group ($D_i \times T_t$)**
  - This coefficient estimates the effect of being in the treated group (δ)

DID: Example

- **The classic (two-period) DID paper is from 1994 by David Card and Alan Krueger**
- **Here's the citation:**

Card, David, and Alan Krueger, 1994. "Minimum Wages and Employment: A Case Study of the Fast-Food Industry in New Jersey and Pennsylvania" American Economic Review 90 (5): 1397 – 1420

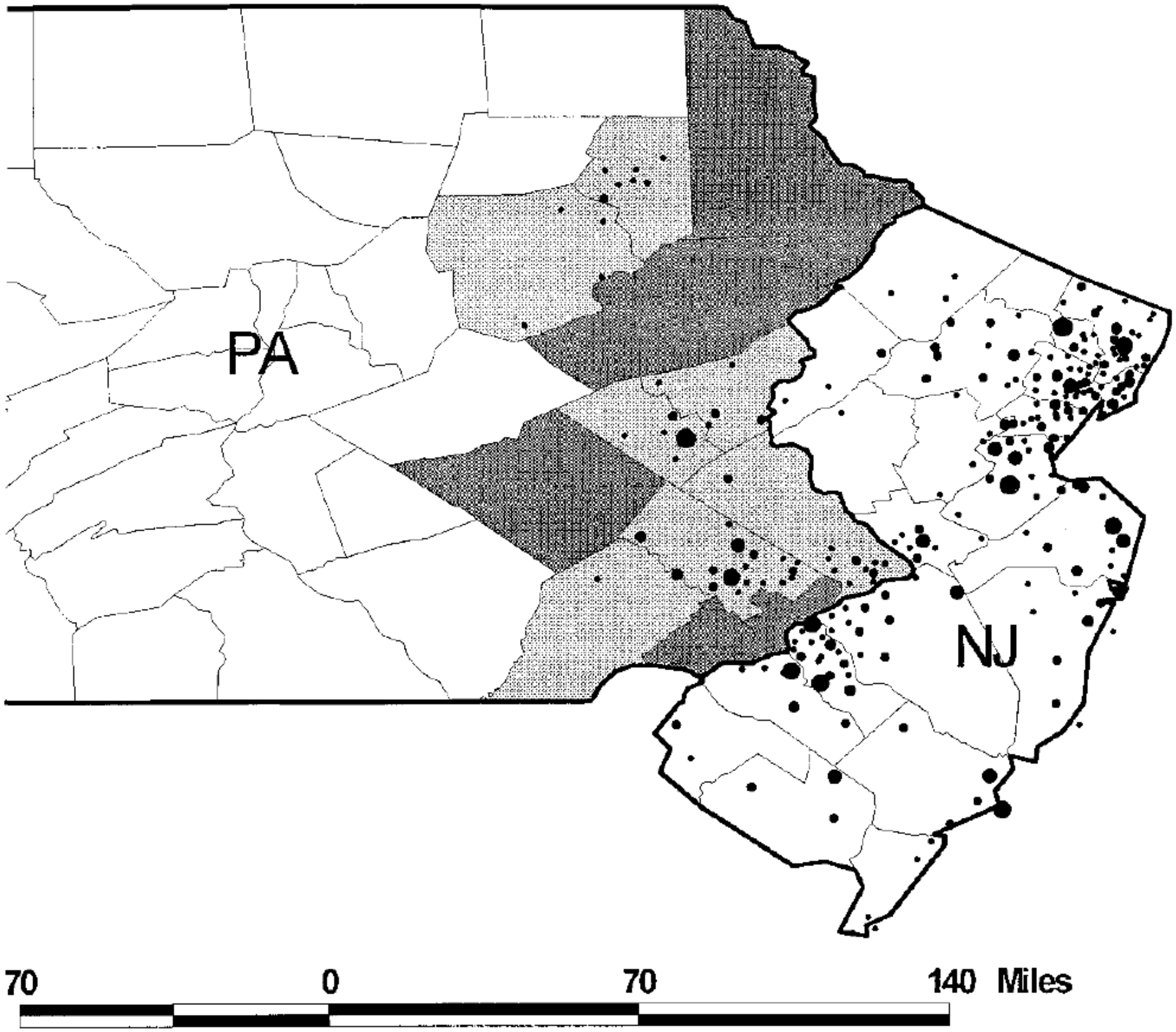
DID Example

- **Card and Krueger are interested in whether minimum wages increases lead to changes in employment**
  - Economic theory suggests higher wages requirements will lead to employers laying off workers to save on costs
- **Take advantage of fast food restaurants in PA and NJ before and after a minimum wage increase in NJ from 4.25 USD to 5.05 USD in April 1992**

DID Example

- **Challenge:**
  - Can't just compare PA to NJ after 1992. Difference will capture far more than just the effect of the minimum wage policy (selection problem)
  - Can't just compare NJ to itself over time because the difference will capture all other macro trends affecting employment in NJ over time(evaluation problem)
  - Remember, both of these are a special case of OVB
- **Solution: Use eastern PA restaurants as a control group and compare over time in a DID**

Locations of Restaurants in the Study



Data Details

- **Survey of 410 Burger King, KFC, Wendy's and Roy Roger's locations**
  - Data collected March 1992 and December 1992, before and after the minimum wage increase in April
- **Collected information employment, starting wages, prices, and other store characteristics**
- **Stores called as many as 9 times to elicit a response (over 87 percent responded)**

DID Model Adaptation

- **Here's the generic DID model:**

$$Y_{it} = \beta_0 + \delta(D_i \times T_t) + \mu T_t + \gamma D_i + X_{it} + \varepsilon_{it} \qquad (20)$$

- **Adapting this for this situation,**

$$Emp_{it} = \beta_0 + \delta(NJ_i \times Dec_t) + \mu Dec_t + \gamma NJ_i + X_{it} + \varepsilon_{it} \qquad (21)$$

- **The X_{it} stands for a series of extra control (time-variant) variables that a researcher can add if you are worried about other time-variant factors being endogenous**

Results

Variable	PA (i)	NJ (ii)	Difference, NJ − PA (iii)
	Stores by state		
1. FTE employment before, all available observations	23.33 (1.35)	20.44 (0.51)	−2.89 (1.44)
2. FTE employment after, all available observations	21.17 (0.94)	21.03 (0.52)	−0.14 (1.07)
3. Change in mean FTE employment	−2.16 (1.25)	0.59 (0.54)	$\boxed{2.76}$ (1.36)

$$Emp_{it} = \beta_0 + \boxed{\delta}(NJ_i \times Dec_t) + \mu Dec_t + \gamma NJ_i + \varepsilon_{it}$$

OLS or Differences

- **You may be wondering why we go to all this trouble to estimate DID with OLS when the simple difference in averages for the two groups is all you need to do**
- **Remember:**
  - OLS allows for statistical inference by providing a standard error (calculating differences in means doesn't)
  - OLS allows for controlling for time-variant observable variables
  - OLS allows for a more generalized form of DID where treatment can happen at different points in time for different groups (next slide)

Generalization of DID Framework to Multiple Time Periods

- **If the policy change happened to different groups at different points in time, the DID model actually simplifies to the following,**

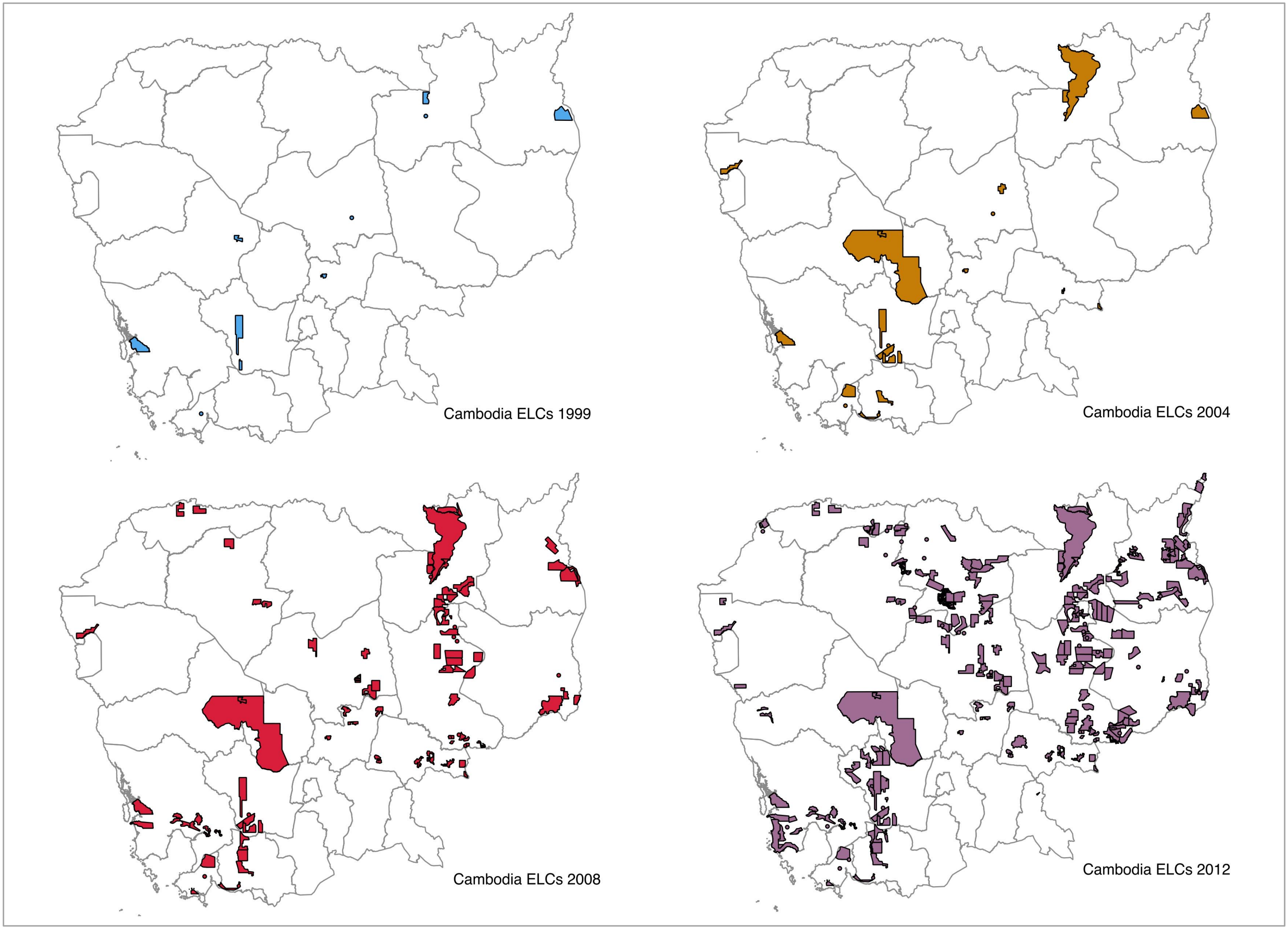
$$Y_{it} = \beta_0 + \delta D_i \times T_t + \underbrace{\gamma_t}_{\text{Time FE}} + \underbrace{\kappa_i}_{\text{Ind. FE}} + X_{it} + \varepsilon_{it} \tag{22}$$

- **These are called "two-way" fixed effects models**
  - They are extremely popular
  - In this framework, the estimate of δ will provide a DID estimate of the effect of being in the treated group

Example from My Own Research

- **Large-scale land acquisitions (LSLAs) in Cambodia**
  - Large tracts of land leased out to private companies for plantation-style agriculture
- **Research question(s):** Do LSLAs lead to local agglomeration effects? How do they affect local economies?

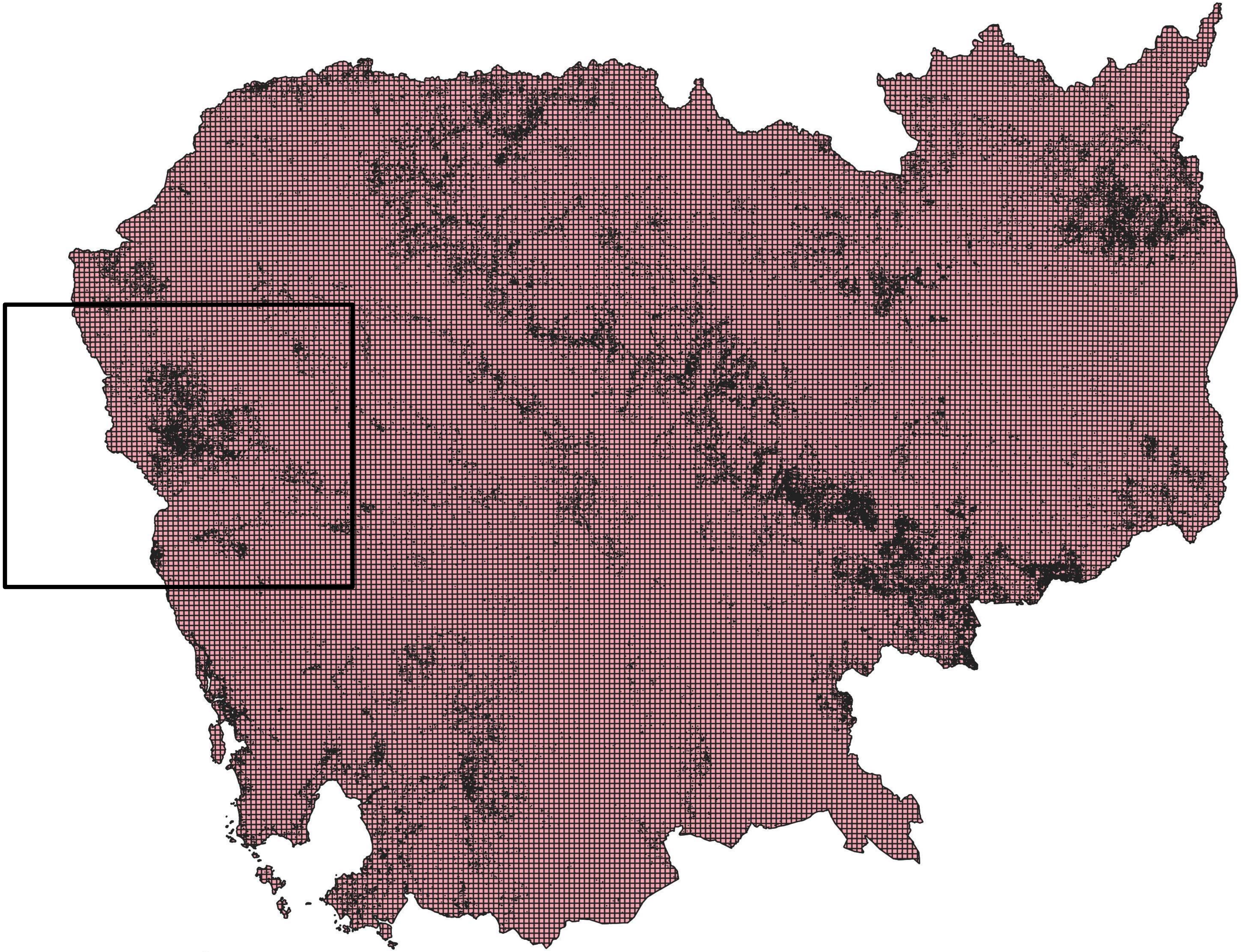
Bananalandia Plantation in Mozambique (Hammond 2014)

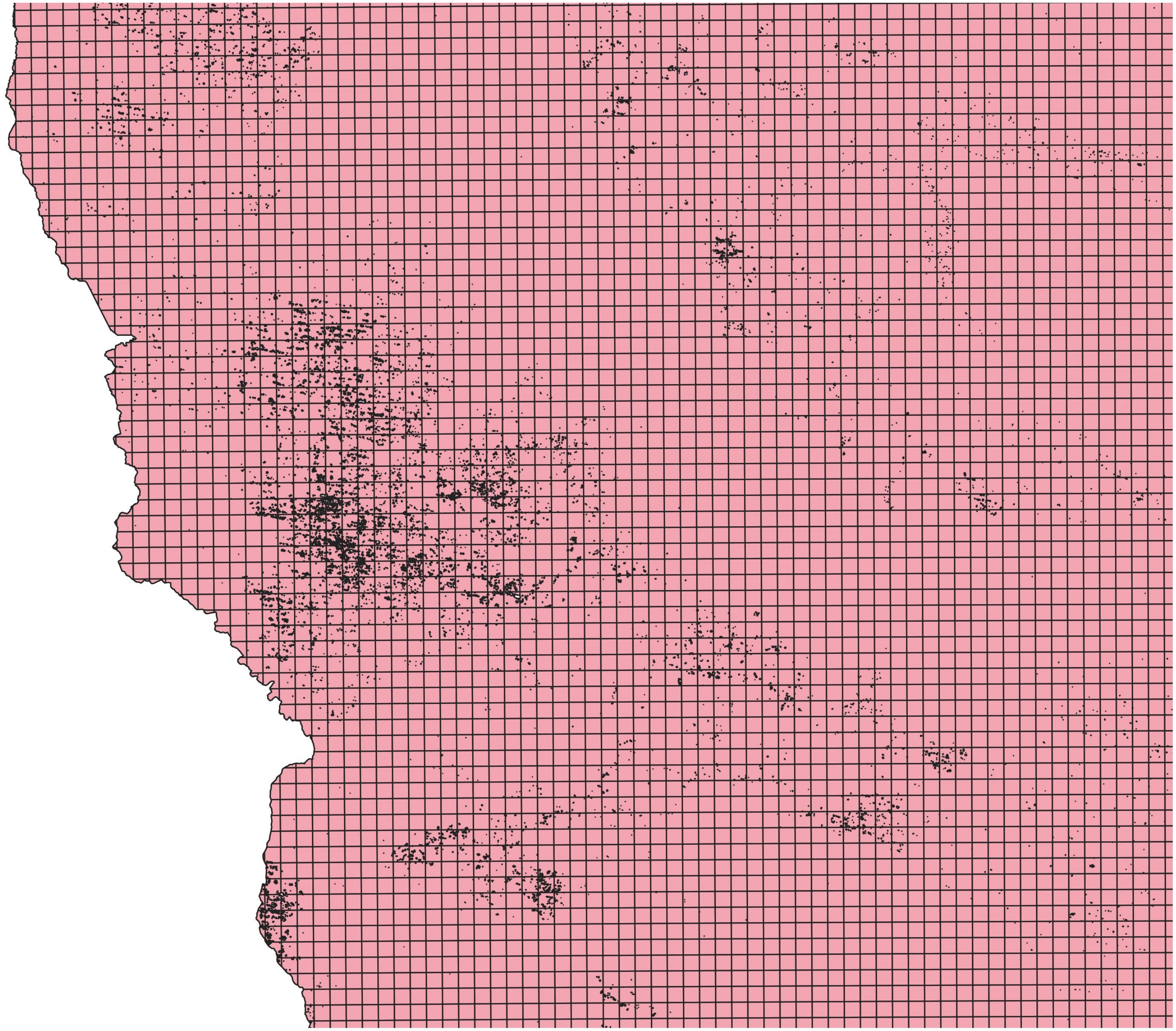


Author's calculations. Data from Open Development Cambodia.

Outcome Data

- **Measurement of economic activity:** Remote sensing data of
  - **Nighttime luminosity:** Li et al. (2020), harmonized Defense Meteorological Satellite Program – Operational Linescan System (DMSP-OLS) and the Visible Infrared Imaging Radiometer Suite (VIIRS)
  - **Population density:** Schiavana et al. (2019)
  - **Forest loss:** Hansen et al. (2013), derived from Landsat
- **I take averages using ½ km by ½ km grid cells for all of Cambodia**





DID Strategy

- **Compare grid cells close to the borders of LSLAs with those that are farther away**
  - Those that are close by are "treated"
  - Those that are farther away are the control group
  - Assumption that effect of LSLAs on economic activity dissipates over space
- **Note that the LSLAs are being leased at different times between 1996 and 2012 so this is a "multi-period DID"**

DID Model

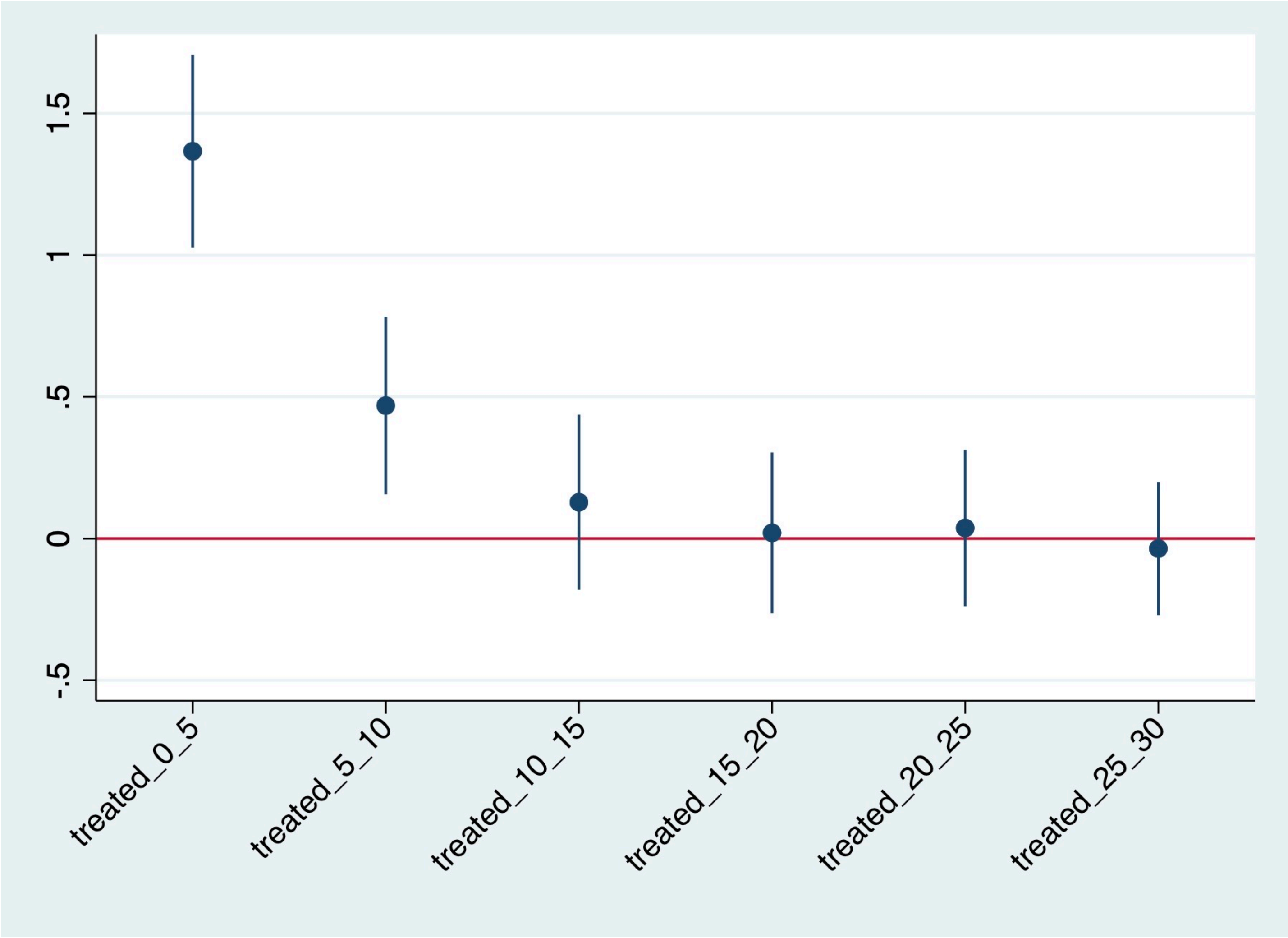
$$\underbrace{arcsine(L_{gt})}_{\text{Arcsine transformation of nighttime lights in grid cell } g \text{ in year } t} = \beta_0 + \sum_S \alpha_S \underbrace{ELC_S \times After_t}_{\text{Being within } S \text{ kilometers of an ELC (treated) over time}} + \underbrace{\gamma_t}_{\text{Year FE}} + \underbrace{\lambda_g}_{\text{Grid Cell FE}} + \varepsilon_{gt} \qquad (23)$$

Preliminary Results

VARIABLES	(1) Arcsine Lights	(2) Arcsine Cumulative Forest Loss	(3) Arcsine Population
$ELC_S \times After_t$ ($S = 0$ to $5km$)	0.211***	1.366***	0.116***
	(0.029)	(0.173)	(0.043)
$ELC_S \times After_t$ ($S = 5$ to $10km$)	0.076***	0.469***	0.058
	(0.027)	(0.160)	(0.038)
$ELC_S \times After_t$ ($S = 10$ to $15km$)	0.026	0.128	0.032
	(0.027)	(0.157)	(0.040)
$ELC_S \times After_t$ ($S = 15$ to $20km$)	0.028	0.020	0.062
	(0.025)	(0.145)	(0.044)
$ELC_S \times After_t$ ($S = 20$ to $25km$)	0.017	0.037	-0.010
	(0.024)	(0.141)	(0.046)
$ELC_S \times After_t$ ($S = 25$ to $30km$)	0.014	-0.035	-0.015
	(0.021)	(0.120)	(0.041)
Observations	17,646,510	10,278,744	1,312,006
R-squared	0.531	0.763	0.850

Robust standard errors clustered at the village level in parentheses
Coefficients for fixed effects and the constant are omitted
*** p<0.01, ** p<0.05, * p<0.1

Effects Over Space



Ways to Deal with OVB

The following are the general ways to deal with OVB:

1. Signing the bias
2. Include a proxy
3. Randomized control trial (RCT)
4. Fixed effects (FE)
5. Difference in differences (DID)
6. **Instrumental variable (IV)**
7. Regression Discontinuity Design (RDD) (Not covered in ECON B253)

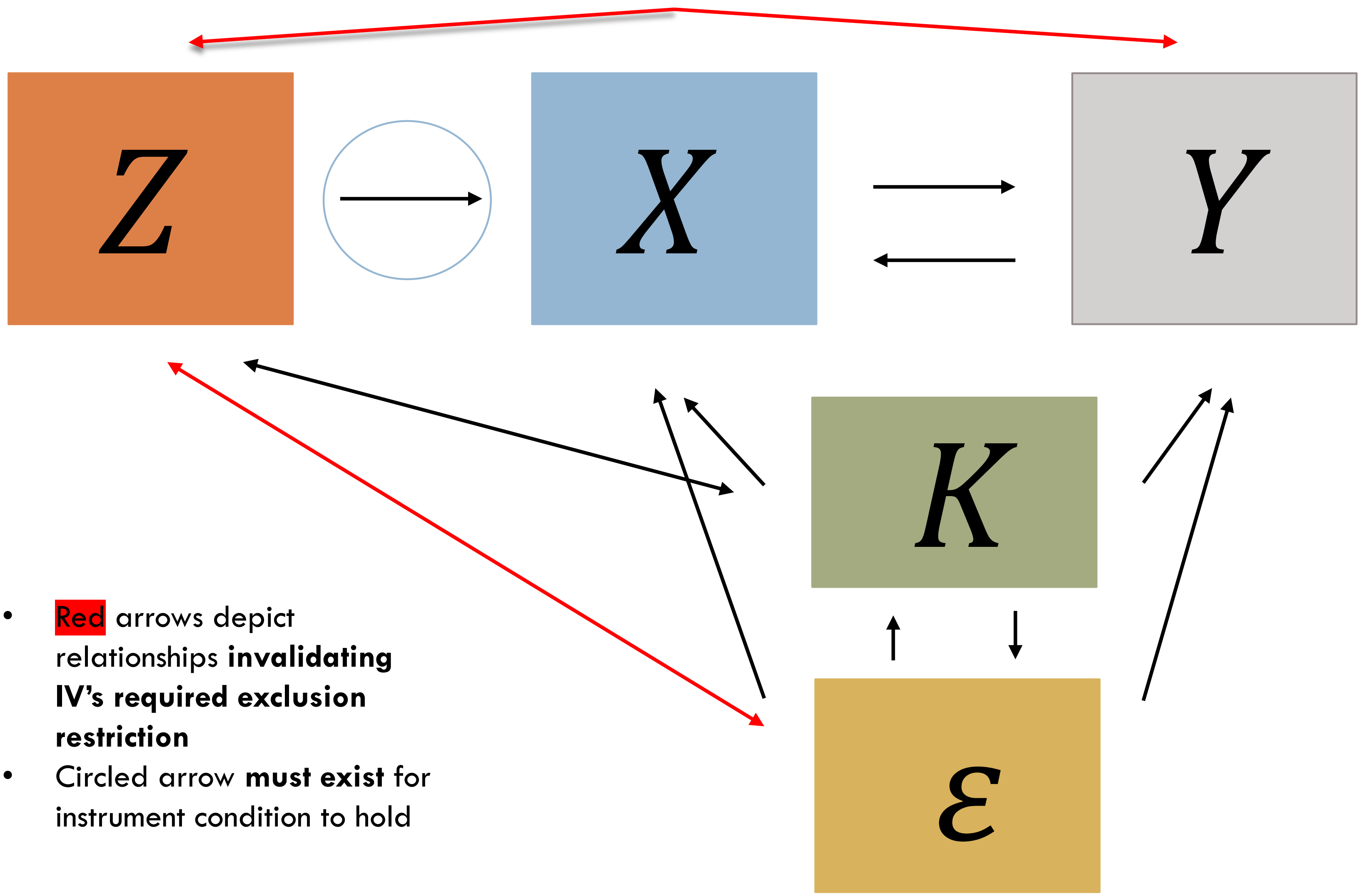
Instrumental Variable (IV)

- **This is an advanced topic but is so common you should have some exposure to it**
- **IVs are frequently used in econometric literature and are highly popular**
- **They can be very powerful, but are often misused and misunderstood (even by advanced researchers)**
- **We don't have time to do this topic justice, but I'll try to provide you with the following:**
  - Intuition behind the method
  - A sense of the technical implementation
  - A guide to assessing whether an IV application is compelling or not

IV: Necessary Assumptions

- **An instrument is a variable that satisfies three main requirements:**
  1. **Instrument Condition:** It helps to determine the value of the RHS variable of interest
  2. **Exclusion Restriction:** It has <u>no relationship</u> with the outcome variable besides its relationship with the variables on the RHS of the regression
  3. **Monotonicity:** There are no defiers
- **Not all situations have one of these! If you have an idea for an IV you're lucky!**

IV: Assumptions 1 and 2



- Red arrows depict relationships **invalidating IV's required exclusion restriction**
- Circled arrow **must exist** for instrument condition to hold

IV: Notation

- **Putting this another way, our instrument doesn't belong in our regression of interest.**

$$Y_i = \beta_0 + \beta_1 X_i + \beta_2 K_i + \varepsilon_i \tag{24}$$

But it does belong in a regression for X alone,

$$X_i = \alpha_0 + \alpha_1 Z_i + \alpha_2 M_i + \lambda_i \tag{25}$$

- **Z belongs in the X equation, but not the Y equation**
  - If it belonged in the Y equation what would it be?

IV: Basic Idea

- **We think of our variable of interest X as having two types of variation:**
  1. **Bad variation:** the variation correlated with the omitted variables, and
  2. **Good variation:** the variation not correlated with the omitted variable (possible because all sorts of things determine the level of X)

- **If you have an IV you can exploit the fact that the IV has variation related to your variable of interest that is "clean" in that it is not correlated to the omitted variables you're worried about!**
  - We can use the IV as a "washing machine"
  - It scrubs off all the variation in X that is related to the omitted variables and leaves us with clean variation

IV: Assumption 3

- **What is a defier?**
- **Imagine you have a binary instrument Z and some binary independent variable X. You think that Z and X are related in the following way: when $Z = 1$, it's more likely that $X = 1$**
  - When Z goes from 0 to 1, some people in the data are compelled by that change to change X from 0 to 1. These are "Compliers"
  - Others don't change their behavior based on the instrument. $X = 1$ for them no matter what. These people are "Always Takers"
  - Others don't change their behavior based on the instrument. $X = 0$ for them no matter what. These people are "Never Takers"
  - When Z goes from 0 to 1, some people in the data might compelled by that change to change X from 1 to 0 (against how you think Z affects X). These are "Defiers"

IV: Assumption 3

- **Here's an example:**
  - We might be interested in the effect of a woman having a third child on their wages
  - Of course, there will be a selection problem if we just compare women with third children to women with two children
    - What might be some omitted factors here?
  - **Proposed instrument:** Whether a woman's first two children have the same sex
    - Idea: Women with two children of the same sex will be more likely to have a third b/c they want the life experience of raising sons and daughters

IV: Assumption 3

- **In this context:**
  - Compliers: Women who decide to have a third child if their first two children are the same sex
  - Always takers: Women who were always going to have a third child no matter whether the sexes of the first two children were the same
  - Never takers: Women who were never going to have a third child no matter whether the sexes of the first two children were the same
  - Defiers: Women who choose ***not*** to have a third child if the sexes of the first two children are the same

IV: Implications of Assumption 3

- **If you can make a plausible case that defiers exist then you do not have a valid IV**
  - The presence of defiers is not testable. It is entirely a rhetorical argument
- **Technically, IV estimates estimate the effect of X on the compliers only**
  - This is a type of "Local Average Treatment Effect" or "LATE"
- **While important to know this, most IV papers don't discuss the monotonicity assumption**

IV: Steps in Detail

- **Step 1:** Estimate the following,

$$X_i = \alpha_0 + \alpha_1 Z_i + \alpha_2 M_i + \lambda_i \tag{26}$$

- **Step 2:** Use $\hat{\alpha}_0, \hat{\alpha}_1$, and $\hat{\alpha}_2$to obtain $\hat{X}$. Recall,

$$\hat{X}_i = \hat{\alpha}_0 + \hat{\alpha}_1 Z_i + \hat{\alpha}_2 M_i \tag{27}$$

- **Step 3:** Estimate the following,

$$Y_i = \beta_0 + \beta_1 \hat{X}_i + \beta_2 M_i + \varepsilon_i \tag{28}$$

IV: Basic Idea

- **Equation (26) is called the "first stage regression"**
  - It determines how much X varies with the instrument Z after controlling for the other RHS variables (M in our simple three-variable model)
- **Equation (28) is called the "second stage regression"**
  - It determines how much $\hat{X}$ varies with Y after controlling for the other RHS variables (M in our simple three-variable model). $\hat{X}$ is X constructed using the "clean variation" from the instrument Z
- **Due to these two stages, the IV approach is frequently referred to as "Two-Stage Least Squares"**

Ways to Evaluate an IV Application

- **The instrument condition has a test**
  - If the F-statistic (for all first-stage coefficients = 0) from the first stage (equation (26)) is greater than 10, then the relationship between X and Z is considered sufficiently strong
    - If below 10, it is considered a "weak instrument" and should be approached cautiously
    - The F-statistic from the first stage should always be reported
- **<u>The exclusion restriction has no test!</u>**
  - Many researchers get this wrong. Beware anyone claiming empirical support for this
  - Exclusion restriction validity is based entirely on theoretical and rhetorical grounds
- **<u>The monotonicity assumption has no test!</u>**
  - At this point, most papers barely discuss this issue
    - Defiers are impossible to identify

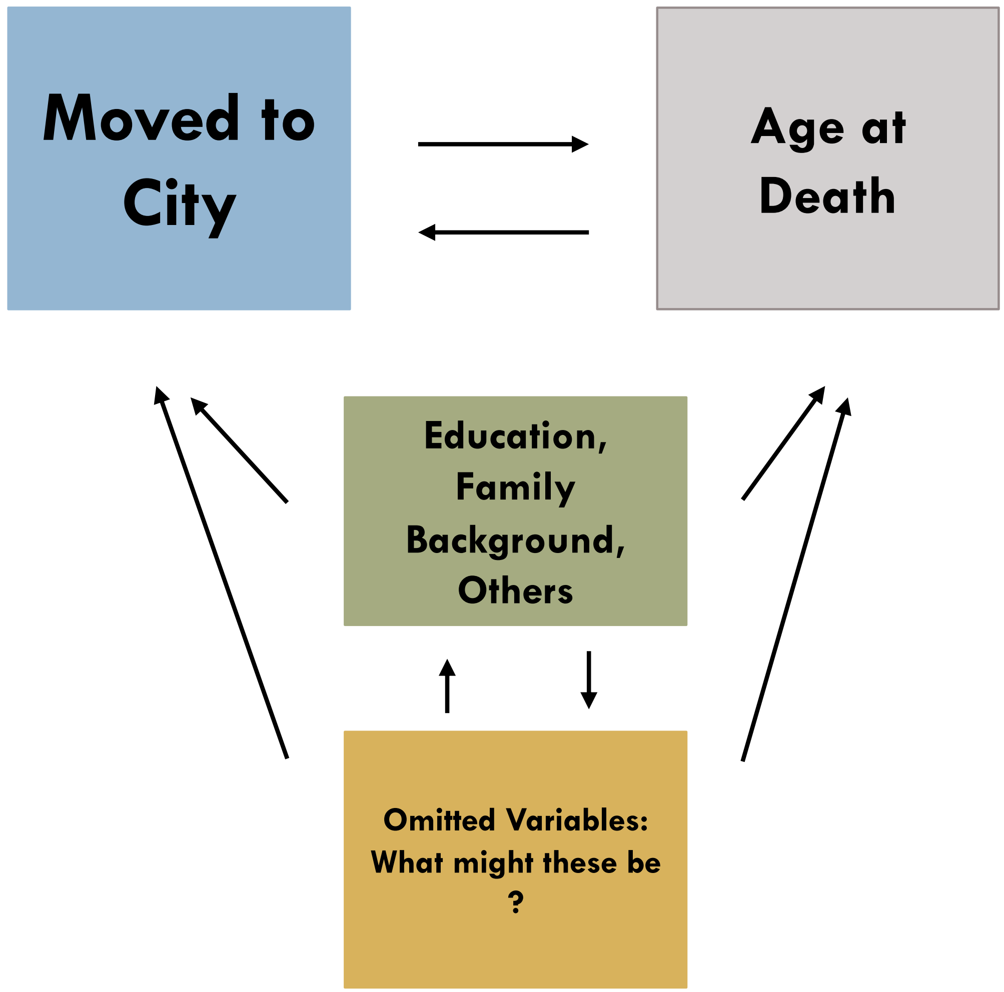
IV: An Example

- **Let's take a look at a nice paper from Johnson and Taylor (2019),**
  - **Citation:**

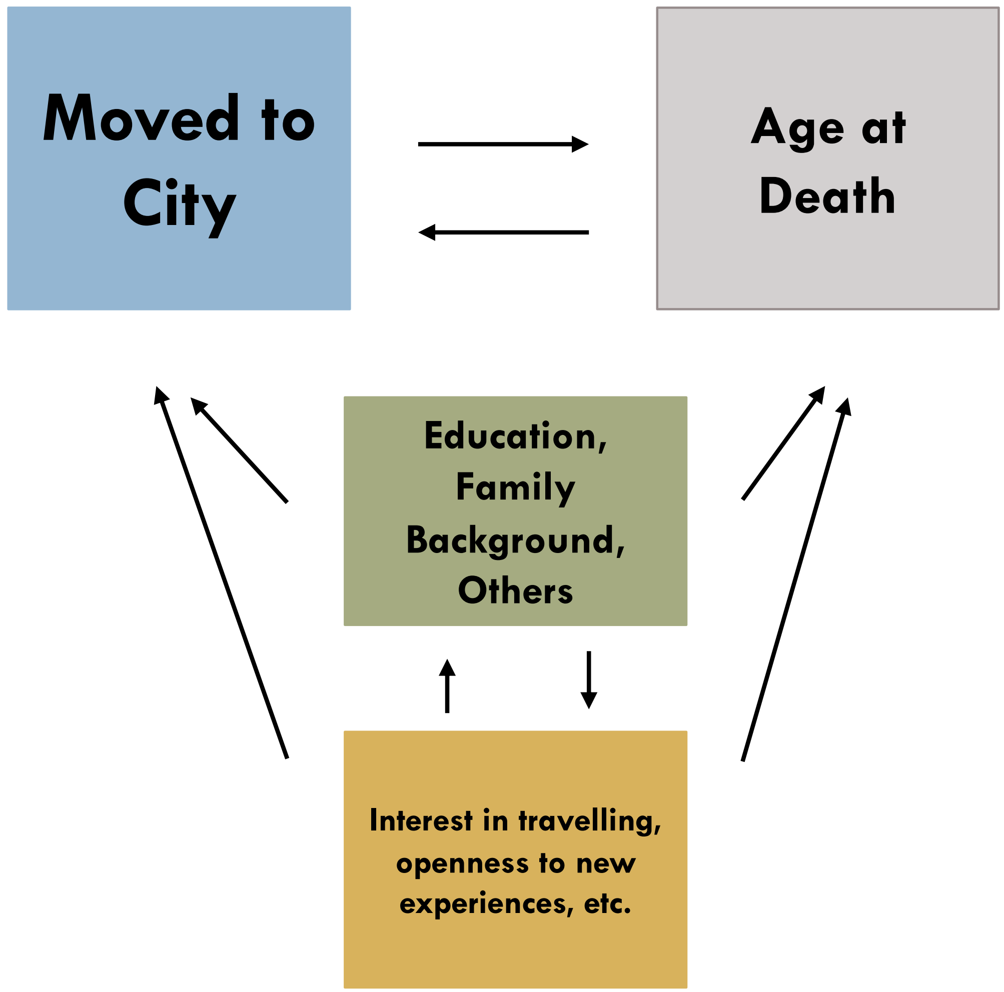
Johnson, Janna, and Evan Taylor, 2019. "The long-run health consequences of rural to urban migration" *Quantitative Economics* 10: 565-606.

  - **Research Question:** What is the effect of rural to urban migration on life expectancy?
    - Why might we expect an effect?
      - Cities offer better hospitals, better education, more diverse food sources, etc.

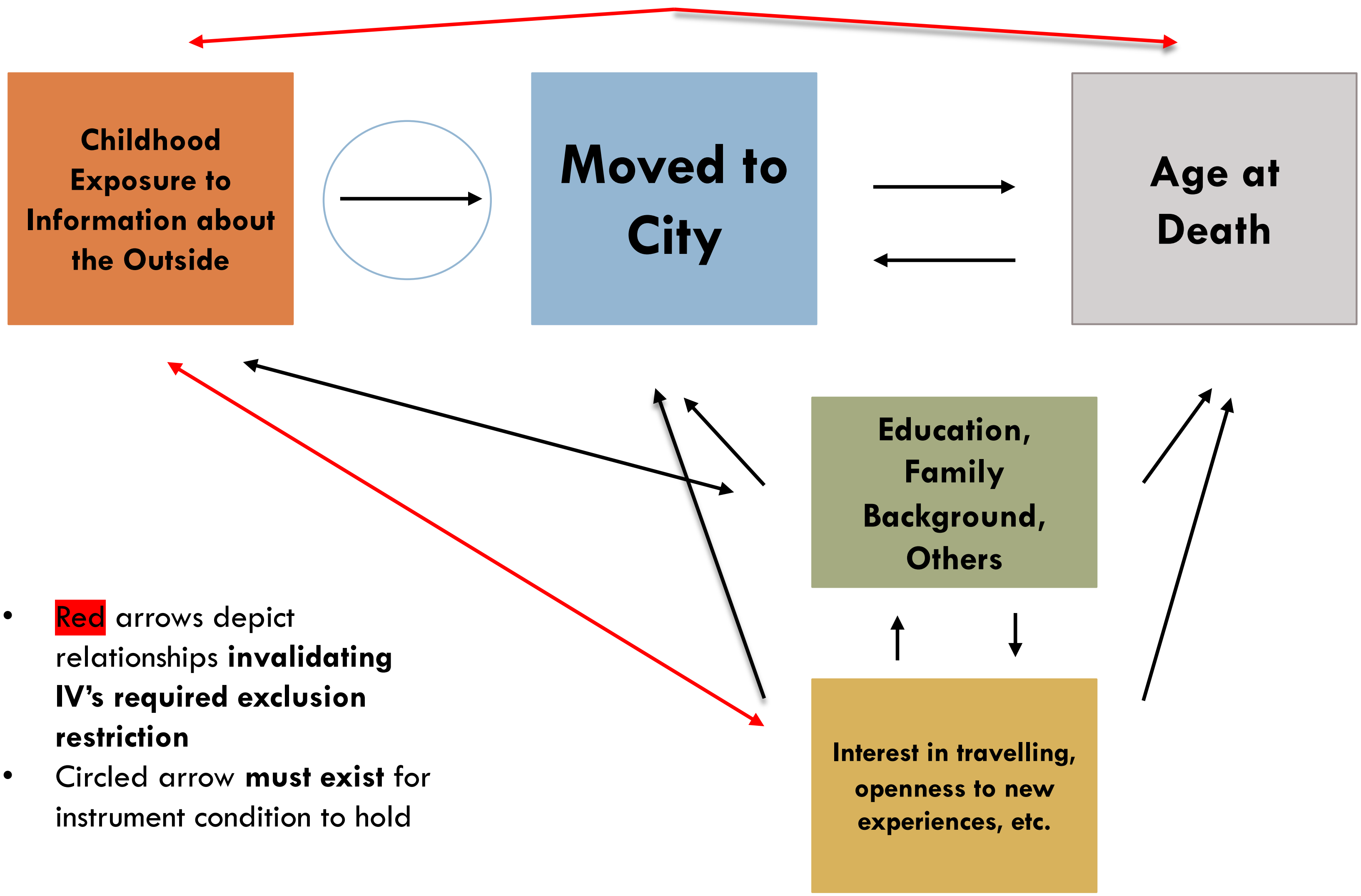
Johnson and Taylor (2019) Directed Acyclic Graph



Johnson and Taylor (2019) Directed Acyclic Graph



Johnson and Taylor (2019) Directed Acyclic Graph



- Red arrows depict relationships **invalidating IV's required exclusion restriction**
- Circled arrow **must exist** for instrument condition to hold

Johnson and Taylor (2019) Context and Data

- **Johnson and Taylor (2019) look at people from the US Upper Midwest and Great Plains born from 1916 to 1927**
  - Each observation in the data is for a person
  - They know if they migrated or not, their age when they died, the town they were born in, and a bunch of control variables
  - They need an individual's exposure to information from the outside world, they need another dataset. Two challenges:
    1. How do you measure this?
    2. Where are the data?

Johnson and Taylor (2019) Data

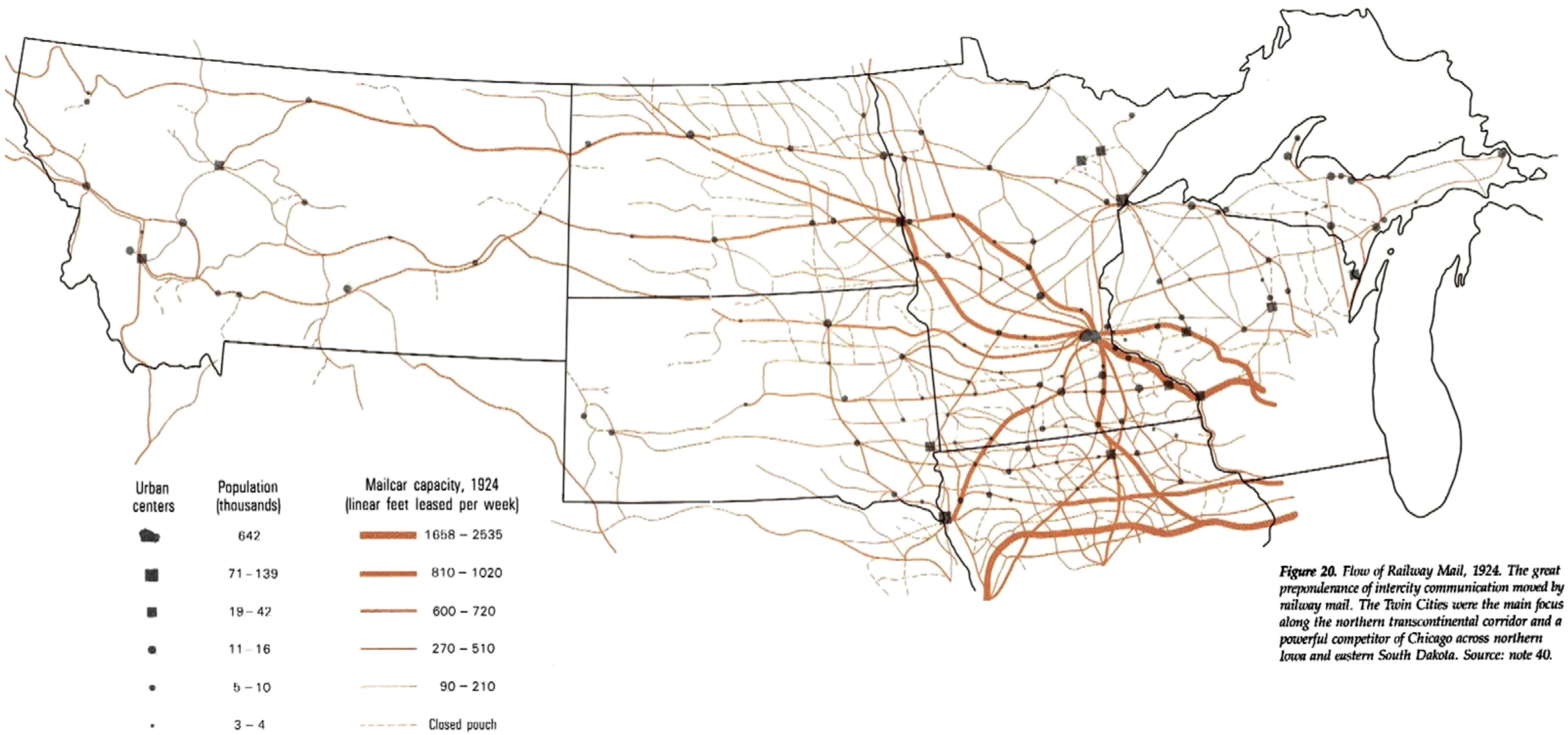


FIGURE 3. Flow of railway mail, 1924. Note: This figure is a direct reproduction of Figure 20 in Borchert (1987). Used by permission of the University of Minnesota Press from America's Northern Heartland: An Economic and Historical Geography of the Upper Midwest by John R. Borchert. Copyright 1987 by the University of Minnesota.

Johnson and Taylor (2019) Model

- Johnson and Taylor (2019) want to use this data to estimate the following model,

$$DeathAge_i = \beta_0 + \beta_1 \widehat{Mig}_i + \beta_2 Z_{i1} + \cdots + \beta_{k+2} Z_{ik} + \varepsilon_i \qquad (29)$$

$$Mig_i = \alpha_0 + \alpha_1 MailCap_i + \alpha_2 Z_{i1} + \cdots + \alpha_{k+2} Z_{ik} + \lambda_i \qquad (30)$$

- Recall,

$$\widehat{Mig}_i = \hat{\alpha}_0 + \hat{\alpha}_1 MailCap_i + \hat{\alpha}_2 Z_{i1} + \cdots + \hat{\alpha}_{k+2} Z_{ik} \qquad (31)$$

Johnson and Taylor (2019) Model

- **But they don't have continuous mail car capacity,**

$$DeathAge_i = \beta_0 + \beta_1 \widehat{Mig}_i + \beta_2 Z_{i1} + \cdots + \beta_{k+2} Z_{ik} + \varepsilon_i \qquad (32)$$

$$\begin{aligned} Mig_i \\ &= \alpha_0 + \alpha_1 MailCap90\_120_i + \alpha_2 MailCap270\_510_i \\ &+ \alpha_3 MailCap600\_720_i + \alpha_4 MailCap810\_1020_i \\ &+ \alpha_5 ClosedPouch_i + \alpha_6 Z_{i1} + \cdots + \alpha_{k+6} Z_{ik} + \lambda_i \qquad (33) \end{aligned}$$

- **Recall,**

$$\begin{aligned} \widehat{Mig}_i \\ &= \hat{\alpha}_0 + \hat{\alpha}_1 MailCap90\_120_i + \hat{\alpha}_2 MailCap270\_510_i \\ &+ \hat{\alpha}_3 MailCap600\_720_i + \hat{\alpha}_4 MailCap810\_1020_i \\ &+ \hat{\alpha}_6 ClosedPouch_i + \hat{\alpha}_6 Z_{i1} + \cdots + \hat{\alpha}_{k+6} Z_{ik} \qquad (34) \end{aligned}$$

Johnson and Taylor (2019) Results

Table 4. IV estimates of the effect of migration out of the Northern Great Plains on older-age longevity.

	OLS (1)	Railroad instruments (2)	Mailcar capacity instruments (3)	Both instruments (4)	Propensity score as instrument (5)
Migrant	$\boxed{-0.012}$	−0.118	$\boxed{-0.089}$	−0.094	−0.093
	$\boxed{(0.001)}$	(0.048)	$\boxed{(0.033)}$	(0.033)	(0.033)
Dependent variable mean	0.806	0.806	0.806	0.806	0.806
First Stage					
Closed pouch			0.041	0.039	
			(0.011)	(0.011)	
90–120			0.043	0.040	
			(0.008)	(0.009)	
270–510			0.052	0.045	
			(0.010)	(0.011)	
600–720			0.107	0.098	
			(0.015)	(0.016)	
810–1020			0.190	0.180	
			(0.021)	(0.023)	
Pre-1900 railroad		0.071		0.012	
		(0.009)		(0.008)	
Post-1900 railroad		0.043			
		(0.008)			
Partial *F* Statistic		33.07	$\boxed{24.97}$	22.89	–
Dependent Variable Mean		0.536	0.536	0.536	0.536
Observations	330,428	330,428	330,428	330,428	330,428

Assessing Johnson and Taylor

- **When evaluating IV papers, it's good to assess how plausible the three assumptions are**
  - **Is the instrument condition plausible?**
    - Yes! It seems plausible (to me) that more information about the outside world in your town through the mail would lead to higher likelihood you'd leave. The F-stat in the first stage supports this
  - **Is the exclusion restriction plausible?**
    - Yes! It seems (to me) unlikely that mail car capacity has an effect on longevity
  - **Is monotonicity plausible?**
    - Maybe… It seems possible (to me) that more information about the outside world might cause some people to stay in their towns. For some people, more information about a scary outside world might lead them to say, I'm never leaving my safe small town. If this behavior seems possible, then this IV is invalid

Other Methods for Causal Inference to be Aware of

- **Propensity Score Matching (PSM)**
- **Here's the idea:**
  - Involves using observed variables to estimate a propensity score for individuals being part of the treated group
    - You usually estimate the propensity score as a predicted probability of being in the treatment group using a Probit or a Logit model, you could also use an LPM
  - You then use the propensity score as a way to find people in the treated group and control group that are similar (in propensity scores) to make a comparison
    - Many algorithms available for deciding who have "close" propensity scores (Nearest Neighbor Matching is usually the most common)

Other Methods for Causal Inference

- **Propensity Score Matching (PSM)**
  - The propensity score is based on <u>variation in observed variables</u> so it only controls for observed variables
    - It therefore doesn't control for omitted variables (this is of course the real source of worry we have about bias)
  - What doing this allows for is we avoid a decision on the functional form of the model
    - So any bias that comes from assuming the linearity of the parameters required for OLS is taken care of
      - However, most people aren't really concerned about this

Other Methods for Causal Inference to be Aware of

- **Propensity Score Matching (PSM)**
  - Frankly, this method is outdated (popular in the 80s and 90s) and doesn't really convince many economists these days
  - However, you still see this in certain disciplines. I've seen it recently in some sociology papers
  - It can be combined with more credible approaches like DID to provide one advantage
- **Benefits of Propensity Score Matching**
  - It allows you to avoid bias from the wrong functional form decision
- **Why is PSM not that Useful?**
  - Does not control for omitted variable bias or address reverse causality
  - Usually functional form decision isn't a primary concern

Other Methods for Causal Inference to be Aware of

- **Regression Discontinuity Design (RDD)**
  - This is a great technique but typically covered in more advanced econometrics courses
  - Can provide convincing evidence of a causal effect
  - Involves a policy decision where there is a discrete cutoff for who is treated (like age limit)
    - You then compare individuals above and below the cutoff
    - The idea is that individuals around the cutoff are similar in unobserved covariates
  - See me in office hours if you want to learn about it