Day 1: Identification by Design

Peter Hull

Design-Based Regression Inference
Spring 2024

The Design of This Course

- This is a three-day intensive in design-based causal inference
  - Far from comprehensive: will focus on core concepts with regression/IV
  - Emphasis will be on practical lessons for applied work
  - I will assume you all have a solid foundation in the basics of causal inference (e.g. Scott's intro courses)

The Design of This Course

- This is a three-day intensive in design-based causal inference
  - Far from comprehensive: will focus on core concepts with regression/IV
  - Emphasis will be on practical lessons for applied work
  - I will assume you all have a solid foundation in the basics of causal inference (e.g. Scott's intro courses)
- 6-7 hours of lecture, two 30-minute coding demonstrations
  - Please ask questions in the Discord chat!
  - I will try to stick to the schedule but may improvise slightly
  - Code assignments will be "take home," w/solutions the following class

The Design of This Course

- This is a three-day intensive in design-based causal inference
  - Far from comprehensive: will focus on core concepts with regression/IV
  - Emphasis will be on practical lessons for applied work
  - I will assume you all have a solid foundation in the basics of causal inference (e.g. Scott's intro courses)
- 6-7 hours of lecture, two 30-minute coding demonstrations
  - Please ask questions in the Discord chat!
  - I will try to stick to the schedule but may improvise slightly
  - Code assignments will be "take home," w/solutions the following class
- Feedback/follow-up: *peter_hull@brown.edu*

Course Schedule

Monday 4/22	6:00-7:50pm	Lecture 1: Selection-on-Observables
	6:50-7:00pm	*Break*
	7:00-7:50pm	Lecture 2: Design vs. Outcome Models
	7:50-8:00pm	*Break*
	8:00-8:50pm	Lecture 3: Design-Based IV
	8:50-9:00pm	Application 1 Overview
Wednesday 4/24	6:00-6:30pm	Live-Coding Application 1
	6:30-6:40pm	*Break*
	6:40-7:40pm	Lecture 4: Negative Weights
	7:40-7:50pm	*Break*
	7:50-8:50pm	Lecture 5: Clustering
	8:50-9:00pm	Application 2 Overview
Friday 4/26	6:00-6:30pm	Live-Coding Application 2
	6:30-6:40pm	*Break*
	6:40-7:40pm	Lecture 6: Recentering
	7:40-7:50pm	*Break*
	7:50-9:00pm	Lecture 7: Nonlinear Models

The Design *in* This Course

- Design-based methods use knowledge on the assignment process of as-if-randomly assigned shocks to estimate causal effects
    - Mimic analysis of "true" experiments, w/known randomization protocol
    - Contrasts with identification strategies that model untreated potential outcomes (e.g. parallel trends) without appealing to randomization

The Design *in* This Course

- Design-based methods use knowledge on the assignment process of as-if-randomly assigned shocks to estimate causal effects
  - Mimic analysis of "true" experiments, w/known randomization protocol
  - Contrasts with identification strategies that model untreated potential outcomes (e.g. parallel trends) without appealing to randomization

- Design-based methods have several practical advantages:
  - A large class of robust estimators, which work without restricting how unobservables (e.g. potential outcomes) relate to observables

The Design *in* This Course

- Design-based methods use knowledge on the assignment process of as-if-randomly assigned shocks to estimate causal effects
  - Mimic analysis of “true” experiments, w/known randomization protocol
  - Contrasts with identification strategies that model untreated potential outcomes (e.g. parallel trends) without appealing to randomization

- Design-based methods have several practical advantages:
  - A large class of robust estimators, which work without restricting how unobservables (e.g. potential outcomes) relate to observables
  - Robust regression/IV estimation, avoiding “negative weight” issues

The Design *in* This Course

- Design-based methods use knowledge on the assignment process of as-if-randomly assigned shocks to estimate causal effects
    - Mimic analysis of "true" experiments, w/known randomization protocol
    - Contrasts with identification strategies that model untreated potential outcomes (e.g. parallel trends) without appealing to randomization

- Design-based methods have several practical advantages:
    - A large class of robust estimators, which work without restricting how unobservables (e.g. potential outcomes) relate to observables
    - Robust regression/IV estimation, avoiding "negative weight" issues
    - Clear criteria for how to pick controls and cluster standard errors

The Design *in* This Course

- Design-based methods use knowledge on the assignment process of as-if-randomly assigned shocks to estimate causal effects
  - Mimic analysis of "true" experiments, w/known randomization protocol
  - Contrasts with identification strategies that model untreated potential outcomes (e.g. parallel trends) without appealing to randomization

- Design-based methods have several practical advantages:
  - A large class of robust estimators, which work without restricting how unobservables (e.g. potential outcomes) relate to observables
  - Robust regression/IV estimation, avoiding "negative weight" issues
  - Clear criteria for how to pick controls and cluster standard errors
  - Clear(er) role of nonlinear/structural models as extrapolation devices

The Design *in* This Course

- Design-based methods use knowledge on the assignment process of as-if-randomly assigned shocks to estimate causal effects
  - Mimic analysis of "true" experiments, w/known randomization protocol
  - Contrasts with identification strategies that model untreated potential outcomes (e.g. parallel trends) without appealing to randomization
- Design-based methods have several practical advantages:
  - A large class of robust estimators, which work without restricting how unobservables (e.g. potential outcomes) relate to observables
  - Robust regression/IV estimation, avoiding "negative weight" issues
  - Clear criteria for how to pick controls and cluster standard errors
  - Clear(er) role of nonlinear/structural models as extrapolation devices
- We'll get into all of this over the next few days, building up slowly...

Outline

1. Selection on Observables
2. Design vs. Outcome Models
3. Design-Based IV

The Simplest Experimental Story

- Throughout today, we'll consider the goal of estimating parameter β in the constant-effects causal model

$$y_i = \beta x_i + \varepsilon_i$$

Here y_i and x_i are the observed outcome and treatment, while ε_i is an unobserved untreated potential outcome

The Simplest Experimental Story

- Throughout today, we'll consider the goal of estimating parameter β in the constant-effects causal model

$$y_i = \beta x_i + \varepsilon_i$$

  Here y_i and x_i are the observed outcome and treatment, while ε_i is an unobserved untreated potential outcome

  - On Wednesday: heterogeneous effects / multiple treatments

The Simplest Experimental Story

- Throughout today, we'll consider the goal of estimating parameter β in the constant-effects causal model

$$y_i = \beta x_i + \varepsilon_i$$

  Here y_i and x_i are the observed outcome and treatment, while ε_i is an unobserved untreated potential outcome

  - On Wednesday: heterogeneous effects / multiple treatments

- Suppose x_i is generated by an experiment: $x_i \mid \varepsilon \overset{iid}{\sim} G$ for known G

The Simplest Experimental Story

- Throughout today, we'll consider the goal of estimating parameter β in the constant-effects causal model

$$y_i = \beta x_i + \varepsilon_i$$

  Here y_i and x_i are the observed outcome and treatment, while ε_i is an unobserved untreated potential outcome

  - On Wednesday: heterogeneous effects / multiple treatments

- Suppose x_i is generated by an experiment: $x_i \mid \varepsilon \overset{iid}{\sim} G$ for known G
  - This means $0 = Cov(x_i, \varepsilon_i)$

The Simplest Experimental Story

- Throughout today, we'll consider the goal of estimating parameter β in the constant-effects causal model

$$y_i = \beta x_i + \varepsilon_i$$

  Here y_i and x_i are the observed outcome and treatment, while ε_i is an unobserved untreated potential outcome

  - On Wednesday: heterogeneous effects / multiple treatments

- Suppose x_i is generated by an experiment: $x_i \mid \varepsilon \overset{iid}{\sim} G$ for known G
  - This means $0 = Cov(x_i, \varepsilon_i) = Cov(x_i, y_i - \beta x_i)$

The Simplest Experimental Story

- Throughout today, we'll consider the goal of estimating parameter β in the constant-effects causal model

$$y_i = \beta x_i + \varepsilon_i$$

  Here y_i and x_i are the observed outcome and treatment, while ε_i is an unobserved untreated potential outcome

  - On Wednesday: heterogeneous effects / multiple treatments

- Suppose x_i is generated by an experiment: $x_i \mid \varepsilon \overset{iid}{\sim} G$ for known G
  - This means $0 = Cov(x_i, \varepsilon_i) = Cov(x_i, y_i - \beta x_i) = Cov(x_i, y_i) - \beta Var(x_i)$

The Simplest Experimental Story

- Throughout today, we'll consider the goal of estimating parameter β in the constant-effects causal model

$$y_i = \beta x_i + \varepsilon_i$$

  Here y_i and x_i are the observed outcome and treatment, while ε_i is an unobserved untreated potential outcome

  - On Wednesday: heterogeneous effects / multiple treatments

- Suppose x_i is generated by an experiment: $x_i \mid \varepsilon \overset{iid}{\sim} G$ for known G
  - This means $0 = Cov(x_i, \varepsilon_i) = Cov(x_i, y_i - \beta x_i) = Cov(x_i, y_i) - \beta Var(x_i)$

  $\Rightarrow$ $\beta = Cov(x_i, y_i)/Var(x_i)$, which is the population slope coefficient from regressing y_i on x_i (i.e. β is identified by regression)

The Simplest Experimental Story

- Throughout today, we'll consider the goal of estimating parameter β in the constant-effects causal model

$$y_i = \beta x_i + \varepsilon_i$$

Here y_i and x_i are the observed outcome and treatment, while ε_i is an unobserved untreated potential outcome

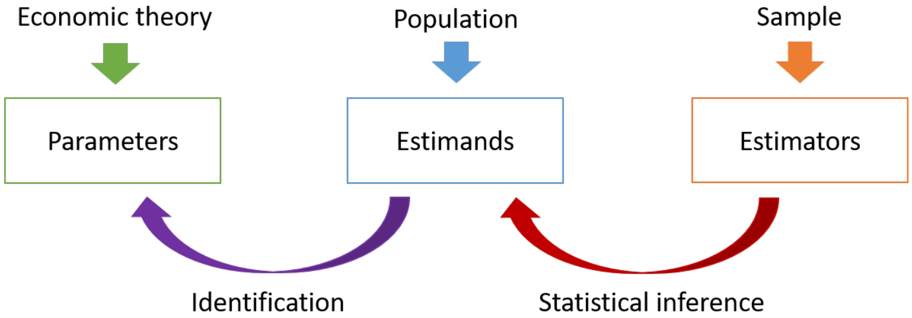
  - On Wednesday: heterogeneous effects / multiple treatments

- Suppose x_i is generated by an experiment: $x_i \mid \varepsilon \overset{iid}{\sim} G$ for known G
  - This means $0 = Cov(x_i, \varepsilon_i) = Cov(x_i, y_i - \beta x_i) = Cov(x_i, y_i) - \beta Var(x_i)$

  $\Rightarrow$ $\beta = Cov(x_i, y_i)/Var(x_i)$, which is the population slope coefficient from regressing y_i on x_i (i.e. β is identified by regression)

  $\Rightarrow$ We can estimate β by a sample (OLS) regression of y_i on x_i

Econometrics: The “Big Picture”



Always good to remember which part of the diagram you’re working on!

Stratified Randomization

- Now consider a (slightly) more complicated experimental design: $x_i \mid w, \varepsilon \overset{iid}{\sim} G(w_i)$ where $w_i = \{1, 2, \ldots, K\}$ indexes some strata

Stratified Randomization

- Now consider a (slightly) more complicated experimental design: $x_i \mid w, \varepsilon \overset{iid}{\sim} G(w_i)$ where $w_i = \{1, 2, \ldots, K\}$ indexes some strata
  - E.g. treatment is more available/rationed in across waves or cites

Stratified Randomization

- Now consider a (slightly) more complicated experimental design: $x_i \mid w, \varepsilon \overset{iid}{\sim} G(w_i)$ where $w_i = \{1, 2, \ldots, K\}$ indexes some strata
  - E.g. treatment is more available/rationed in across waves or cites
- Consider regressing y_i on x_i, controlling for strata FE, $w_{ik} = \mathbf{1}[w_i = k]$

Stratified Randomization

- Now consider a (slightly) more complicated experimental design: $x_i \mid w, \varepsilon \overset{iid}{\sim} G(w_i)$ where $w_i = \{1, 2, \dots, K\}$ indexes some strata
  - E.g. treatment is more available/rationed in across waves or cites
- Consider regressing y_i on x_i, controlling for strata FE, $w_{ik} = \mathbf{1}[w_i = k]$
  - By the Frisch-Waugh-Lovell theorm, equivalent to the regression of y_i on $\tilde{x}_i$: the residuals from regressing x_i on the FE

Stratified Randomization

- Now consider a (slightly) more complicated experimental design: $x_i \mid w, \varepsilon \overset{iid}{\sim} G(w_i)$ where $w_i = \{1, 2, \ldots, K\}$ indexes some strata
  - E.g. treatment is more available/rationed in across waves or cites
- Consider regressing y_i on x_i, controlling for strata FE, $w_{ik} = \mathbf{1}[w_i = k]$
  - By the Frisch-Waugh-Lovell theorm, equivalent to the regression of y_i on $\tilde{x}_i$: the residuals from regressing x_i on the FE
  - But notice since the auxiliary model is *saturated*, $\tilde{x}_i = x_i - E[x_i \mid w_i]$

Stratified Randomization

- Now consider a (slightly) more complicated experimental design: $x_i \mid w, \varepsilon \overset{iid}{\sim} G(w_i)$ where $w_i = \{1, 2, \ldots, K\}$ indexes some strata
  - E.g. treatment is more available/rationed in across waves or cites
- Consider regressing y_i on x_i, controlling for strata FE, $w_{ik} = \mathbf{1}[w_i = k]$
  - By the Frisch-Waugh-Lovell theorm, equivalent to the regression of y_i on $\tilde{x}_i$: the residuals from regressing x_i on the FE
  - But notice since the auxiliary model is *saturated*, $\tilde{x}_i = x_i - E[x_i \mid w_i]$
  - Hence, the regression gives:

$$\frac{Cov(\tilde{x}_i, y_i)}{Var(\tilde{x}_i)} = \frac{Cov(\tilde{x}_i, \beta x_i + \varepsilon_i)}{Var(\tilde{x}_i)} = \beta + \frac{Cov(\tilde{x}_i, \varepsilon_i)}{Var(\tilde{x}_i)}$$

Stratified Randomization

- Now consider a (slightly) more complicated experimental design: $x_i \mid w, \varepsilon \overset{iid}{\sim} G(w_i)$ where $w_i = \{1, 2, \ldots, K\}$ indexes some strata
  - E.g. treatment is more available/rationed in across waves or cites
- Consider regressing y_i on x_i, controlling for strata FE, $w_{ik} = \mathbf{1}[w_i = k]$
  - By the Frisch-Waugh-Lovell theorm, equivalent to the regression of y_i on $\tilde{x}_i$: the residuals from regressing x_i on the FE
  - But notice since the auxiliary model is *saturated*, $\tilde{x}_i = x_i - E[x_i \mid w_i]$
  - Hence, the regression gives:

$$\frac{Cov(\tilde{x}_i, y_i)}{Var(\tilde{x}_i)} = \frac{Cov(\tilde{x}_i, \beta x_i + \varepsilon_i)}{Var(\tilde{x}_i)} = \beta + \frac{Cov(\tilde{x}_i, \varepsilon_i)}{Var(\tilde{x}_i)}$$

  - Moreover, by the LIE and conditional random assignment:

$$Cov(\tilde{x}_i, \varepsilon_i) = E[(x_i - E[x_i \mid w_i])\varepsilon_i] = E[(E[x_i \mid w, \varepsilon] - E[x_i \mid w_i])\varepsilon_i] = 0$$

Stratified Randomization

- Now consider a (slightly) more complicated experimental design: $x_i \mid w, \varepsilon \stackrel{iid}{\sim} G(w_i)$ where $w_i = \{1, 2, \ldots, K\}$ indexes some strata
  - E.g. treatment is more available/rationed in across waves or cites

- Consider regressing y_i on x_i, controlling for strata FE, $w_{ik} = \mathbf{1}[w_i = k]$
  - By the Frisch-Waugh-Lovell theorm, equivalent to the regression of y_i on $\tilde{x}_i$: the residuals from regressing x_i on the FE
  - But notice since the auxiliary model is *saturated*, $\tilde{x}_i = x_i - E[x_i \mid w_i]$
  - Hence, the regression gives:

$$\frac{Cov(\tilde{x}_i, y_i)}{Var(\tilde{x}_i)} = \frac{Cov(\tilde{x}_i, \beta x_i + \varepsilon_i)}{Var(\tilde{x}_i)} = \beta + \frac{Cov(\tilde{x}_i, \varepsilon_i)}{Var(\tilde{x}_i)}$$

  - Moreover, by the LIE and conditional random assignment:

$$Cov(\tilde{x}_i, \varepsilon_i) = E[(x_i - E[x_i \mid w_i])\varepsilon_i] = E[(E[x_i \mid w, \varepsilon] - E[x_i \mid w_i])\varepsilon_i] = 0$$

  - Thus, the strata-controlled regression identifies the parameter β

Selection on Observables

- Design-based regressions in observational data appeal to such experimental ideals:
  1. Claim x_i is as-good-as-randomly assigned conditional on some w_i: formally, $x_i \mid w, \varepsilon \sim G(w_i)$

Selection on Observables

- Design-based regressions in observational data appeal to such experimental ideals:
  1. Claim x_i is as-good-as-randomly assigned conditional on some w_i: formally, $x_i \mid w, \varepsilon \sim G(w_i)$
  2. Control flexibly for w_i, such that the auxiliary regression of x_i on w_i estimates $E[x_i \mid w_i]$ ($\implies$ the controlled reg uses $\tilde{x}_i = x_i - E[x_i \mid w_i]$)

Selection on Observables

- Design-based regressions in observational data appeal to such experimental ideals:
  1. Claim x_i is as-good-as-randomly assigned conditional on some w_i: formally, $x_i \mid w, \varepsilon \sim G(w_i)$
  2. Control flexibly for w_i, such that the auxiliary regression of x_i on w_i estimates $E[x_i \mid w_i]$ ($\implies$ the controlled reg uses $\tilde{x}_i = x_i - E[x_i \mid w_i]$)

- Two steps to make such appeals convincing:

Selection on Observables

- Design-based regressions in observational data appeal to such experimental ideals:
  1. Claim x_i is as-good-as-randomly assigned conditional on some w_i: formally, $x_i \mid w, \varepsilon \sim G(w_i)$
  2. Control flexibly for w_i, such that the auxiliary regression of x_i on w_i estimates $E[x_i \mid w_i]$ ($\implies$ the controlled reg uses $\tilde{x}_i = x_i - E[x_i \mid w_i]$)

- Two steps to make such appeals convincing:
  1. Tell a clear *ex ante* story about where the $x_i \mid w_i$ variation comes from and why it is unlikely to be correlated with ε_i

Selection on Observables

- Design-based regressions in observational data appeal to such experimental ideals:
  1. Claim x_i is as-good-as-randomly assigned conditional on some w_i: formally, $x_i \mid w, \varepsilon \sim G(w_i)$
  2. Control flexibly for w_i, such that the auxiliary regression of x_i on w_i estimates $E[x_i \mid w_i]$ ($\implies$ the controlled reg uses $\tilde{x}_i = x_i - E[x_i \mid w_i]$)

- Two steps to make such appeals convincing:
  1. Tell a clear *ex ante* story about where the $x_i \mid w_i$ variation comes from and why it is unlikely to be correlated with ε_i
  2. Use *ex post* balance tests to check that x_i is not correlated, conditional on w_i, with other observables that may proxy for ε_i

Example: Dale and Krueger (2002)

- D&K estimate effects of attending a more selective college (e.g., a private school) on adult earnings
  - They have data on schooling and earnings, as well as information on which colleges individuals applied to and got into

Example: Dale and Krueger (2002)

- D&K estimate effects of attending a more selective college (e.g., a private school) on adult earnings
    - They have data on schooling and earnings, as well as information on which colleges individuals applied to and got into

- *Ex ante* selection-on-observables story:
    - Conditional on the colleges i applied to / was admitted to, the decision to go to a more elite school is unrelated to latent earnings potential

Example: Dale and Krueger (2002)

- D&K estimate effects of attending a more selective college (e.g., a private school) on adult earnings
  - They have data on schooling and earnings, as well as information on which colleges individuals applied to and got into

- *Ex ante* selection-on-observables story:
  - Conditional on the colleges i applied to / was admitted to, the decision to go to a more elite school is unrelated to latent earnings potential
  - Formally, private school attendance x_i is independent of potential outcomes ε_i given a vector of application/admission dummies w_i

Example: Dale and Krueger (2002)

- D&K estimate effects of attending a more selective college (e.g., a private school) on adult earnings
  - They have data on schooling and earnings, as well as information on which colleges individuals applied to and got into

- *Ex ante* selection-on-observables story:
  - Conditional on the colleges i applied to / was admitted to, the decision to go to a more elite school is unrelated to latent earnings potential
  - Formally, private school attendance x_i is independent of potential outcomes ε_i given a vector of application/admission dummies w_i
  - Strata controls, so the auxiliary regression estimates $E[x_i \mid w_i]$

Example: Dale and Krueger (2002)

- D&K estimate effects of attending a more selective college (e.g., a private school) on adult earnings
  - They have data on schooling and earnings, as well as information on which colleges individuals applied to and got into

- *Ex ante* selection-on-observables story:
  - Conditional on the colleges i applied to / was admitted to, the decision to go to a more elite school is unrelated to latent earnings potential
  - Formally, private school attendance x_i is independent of potential outcomes ε_i given a vector of application/admission dummies w_i
  - Strata controls, so the auxiliary regression estimates $E[x_i \mid w_i]$

- *Ex post* empirical validation:
  - Conditional on the selection controls, x_i appears uncorrelated with other baseline observables (demographics, etc)

Dale and Krueger Estimates (from MHE)

	No Selection Controls			Selection Controls		
	(1)	(2)	(3)	(4)	(5)	(6)
Private School	0.135	0.095	0.086	0.007	0.003	0.013
	(0.055)	(0.052)	(0.034)	(0.038)	(0.039)	(0.025)
Own SAT score/100		0.048	0.016		0.033	0.001
		(0.009)	(0.007)		(0.007)	(0.007)
Predicted log(Parental Income)			0.219			0.190
			(0.022)			(0.023)
Female			-0.403			-0.395
			(0.018)			(0.021)
Black			0.005			-0.040
			(0.041)			(0.042)
Hispanic			0.062			0.032
			(0.072)			(0.070)
Asian			0.170			0.145
			(0.074)			(0.068)
Other/Missing Race			-0.074			-0.079
			(0.157)			(0.156)
High School Top 10 Percent			0.095			0.082
			(0.027)			(0.028)
High School Rank Missing			0.019			0.015
			(0.033)			(0.037)
Athlete			0.123			0.115
			(0.025)			(0.027)
Selection Controls	N	N	N	Y	Y	Y

Notes: Columns (1)-(3) include no selection controls. Columns (4)-(6) include a dummy for each group formed by matching students according to schools at which they were accepted or rejected. Each model is estimated using only observations with Barron's matches for which different students attended both private and public schools. The sample size is 5,583. Standard errors are shown in parentheses.

Outline

1. Selection on Observables✓
2. Design vs. Outcome Models
3. Design-Based IV

Why are Multi-Way FE Different?

- Consider a two-way fixed effect (FE) regression estimated in a panel of individuals i observed over time periods t:

$$y_{it} = \beta x_{it} + \alpha_i + \tau_t + \nu_{it}$$

Why are Multi-Way FE Different?

- Consider a two-way fixed effect (FE) regression estimated in a panel of individuals i observed over time periods t:

$$y_{it} = \beta x_{it} + \alpha_i + \tau_t + \nu_{it}$$

Q: Can we justify this specification by selection-on-observables?

Why are Multi-Way FE Different?

- Consider a two-way fixed effect (FE) regression estimated in a panel of individuals i observed over time periods t:

$$y_{it} = \beta x_{it} + \alpha_i + \tau_t + \nu_{it}$$

  Q: Can we justify this specification by selection-on-observables?

- Note that the unit & time FE controls uniquely identify observations

Why are Multi-Way FE Different?

- Consider a two-way fixed effect (FE) regression estimated in a panel of individuals i observed over time periods t:

$$y_{it} = \beta x_{it} + \alpha_i + \tau_t + \nu_{it}$$

  Q: Can we justify this specification by selection-on-observables?

- Note that the unit & time FE controls uniquely identify observations
  - What would it mean for x_{it} to be as-if-randomly-assigned given (i, t)?

Why are Multi-Way FE Different?

- Consider a two-way fixed effect (FE) regression estimated in a panel of individuals i observed over time periods t:

$$y_{it} = \beta x_{it} + \alpha_i + \tau_t + \nu_{it}$$

Q: Can we justify this specification by selection-on-observables?

- Note that the unit & time FE controls uniquely identify observations
  - What would it mean for x_{it} to be as-if-randomly-assigned given (i, t)?

- The auxiliary regression is of x_{it} on two-way FEs (no interactions)
  - Is additivity, i.e. $E[x_{it} \mid (i, t)] = \mu_i + \gamma_t$, realistic to impose?

Why are Multi-Way FE Different?

- Consider a two-way fixed effect (FE) regression estimated in a panel of individuals i observed over time periods t:

$$y_{it} = \beta x_{it} + \alpha_i + \tau_t + \nu_{it}$$

  Q: Can we justify this specification by selection-on-observables?

- Note that the unit & time FE controls uniquely identify observations
  - What would it mean for x_{it} to be as-if-randomly-assigned given (i, t)?

- The auxiliary regression is of x_{it} on two-way FEs (no interactions)
  - Is additivity, i.e. $E[x_{it} \mid (i,t)] = \mu_i + \gamma_t$, realistic to impose?
  - Clearly can't make this specification more flexible without "dummying out" observations (there's no observed variation in x_{it} given (i, t))

Why are Multi-Way FE Different?

- Consider a two-way fixed effect (FE) regression estimated in a panel of individuals i observed over time periods t:

$$y_{it} = \beta x_{it} + \alpha_i + \tau_t + \nu_{it}$$

Q: Can we justify this specification by selection-on-observables?

- Note that the unit & time FE controls uniquely identify observations
  - What would it mean for x_{it} to be as-if-randomly-assigned given (i,t)?

- The auxiliary regression is of x_{it} on two-way FEs (no interactions)
  - Is additivity, i.e. $E[x_{it} \mid (i,t)] = \mu_i + \gamma_t$, realistic to impose?
  - Clearly can't make this specification more flexible without "dummying out" observations (there's no observed variation in x_{it} given (i,t))

- We need a different justification for this sort of regression...

Two-Way FE as Outcome Modeling

- Continue to assume a constant-effects causal model: $y_{it} = \beta x_{it} + \varepsilon_{it}$
  - Assume x_{it} is *deterministic* in the set of unit and time indicators, w_{it}: once I know the unit and period, I know the treatment status
  - No scope for selection-on-observables

Two-Way FE as Outcome Modeling

- Continue to assume a constant-effects causal model: $y_{it} = \beta x_{it} + \varepsilon_{it}$
  - Assume x_{it} is *deterministic* in the set of unit and time indicators, w_{it}: once I know the unit and period, I know the treatment status
  - No scope for selection-on-observables

- Add in a model for untreated potential outcomes: $E[\varepsilon_{it} \mid w_{it}] = \alpha_i + \tau_t$
  - "Parallel trends": units with different treatment paths have different outcome levels but common outcome changes

Two-Way FE as Outcome Modeling

- Continue to assume a constant-effects causal model: $y_{it} = \beta x_{it} + \varepsilon_{it}$
  - Assume x_{it} is *deterministic* in the set of unit and time indicators, w_{it}: once I know the unit and period, I know the treatment status
  - No scope for selection-on-observables
- Add in a model for untreated potential outcomes: $E[\varepsilon_{it} \mid w_{it}] = \alpha_i + \tau_t$
  - "Parallel trends": units with different treatment paths have different outcome levels but common outcome changes
- Putting both models together, we have:

$$E[y_{it} \mid x_{it}, w_{it}] =$$

Two-Way FE as Outcome Modeling

- Continue to assume a constant-effects causal model: $y_{it} = \beta x_{it} + \varepsilon_{it}$
  - Assume x_{it} is *deterministic* in the set of unit and time indicators, w_{it}: once I know the unit and period, I know the treatment status
  - No scope for selection-on-observables
- Add in a model for untreated potential outcomes: $E[\varepsilon_{it} \mid w_{it}] = \alpha_i + \tau_t$
  - "Parallel trends": units with different treatment paths have different outcome levels but common outcome changes
- Putting both models together, we have:

$$E[y_{it} \mid x_{it}, w_{it}] = \beta x_{it} + E[\varepsilon_{it} \mid w_{it}] =$$

Two-Way FE as Outcome Modeling

- Continue to assume a constant-effects causal model: $y_{it} = \beta x_{it} + \varepsilon_{it}$
  - Assume x_{it} is *deterministic* in the set of unit and time indicators, w_{it}: once I know the unit and period, I know the treatment status
  - No scope for selection-on-observables

- Add in a model for untreated potential outcomes: $E[\varepsilon_{it} \mid w_{it}] = \alpha_i + \tau_t$
  - "Parallel trends": units with different treatment paths have different outcome levels but common outcome changes

- Putting both models together, we have:

$$E[y_{it} \mid x_{it}, w_{it}] = \beta x_{it} + E[\varepsilon_{it} \mid w_{it}] = \beta x_{it} + \alpha_i + \tau_t$$

Two-Way FE as Outcome Modeling

- Continue to assume a constant-effects causal model: $y_{it} = \beta x_{it} + \varepsilon_{it}$
  - Assume x_{it} is *deterministic* in the set of unit and time indicators, w_{it}: once I know the unit and period, I know the treatment status
  - No scope for selection-on-observables
- Add in a model for untreated potential outcomes: $E[\varepsilon_{it} \mid w_{it}] = \alpha_i + \tau_t$
  - "Parallel trends": units with different treatment paths have different outcome levels but common outcome changes
- Putting both models together, we have:

$$E[y_{it} \mid x_{it}, w_{it}] = \beta x_{it} + E[\varepsilon_{it} \mid w_{it}] = \beta x_{it} + \alpha_i + \tau_t$$

i.e. the CEF of $y_{it} \mid x_{it}, w_{it}$ is linear, with a causal coefficient of β
$\implies$ regression identifies it

Two-Way FE as Outcome Modeling

- Continue to assume a constant-effects causal model: $y_{it} = \beta x_{it} + \varepsilon_{it}$
  - Assume x_{it} is *deterministic* in the set of unit and time indicators, w_{it}: once I know the unit and period, I know the treatment status
  - No scope for selection-on-observables

- Add in a model for untreated potential outcomes: $E[\varepsilon_{it} \mid w_{it}] = \alpha_i + \tau_t$
  - "Parallel trends": units with different treatment paths have different outcome levels but common outcome changes

- Putting both models together, we have:

$$E[y_{it} \mid x_{it}, w_{it}] = \beta x_{it} + E[\varepsilon_{it} \mid w_{it}] = \beta x_{it} + \alpha_i + \tau_t$$

i.e. the CEF of $y_{it} \mid x_{it}, w_{it}$ is linear, with a causal coefficient of β
$\implies$ regression identifies it

  - Logic clearly extends to more than two FEs, time-varying controls, unit-specific trends, or any other model for $E[\varepsilon \mid w]$

Example: Finkelstein (2007)

- Boiling multi-way FE regression specs down to simpler "diff-in-diff" comparisons can make the content of the outcome model clearer
  - Useful fact: in two periods, β in $y_{it} = \beta x_{it} + \alpha_i + \tau_t + \nu_{it}$ is given by the bivariate regression of $y_{i,Post} - y_{i,Pre}$ on $x_{i,Post} - x_{i,Pre}$

Example: Finkelstein (2007)

- Boiling multi-way FE regression specs down to simpler "diff-in-diff' comparisons can make the content of the outcome model clearer
  - Useful fact: in two periods, β in $y_{it} = \beta x_{it} + \alpha_i + \tau_t + \nu_{it}$ is given by the bivariate regression of $y_{i,Post} - y_{i,Pre}$ on $x_{i,Post} - x_{i,Pre}$
- Finkelstein is interested in estimating market-level effects of health insurance coverage, using the 1965 introduction of Medicare

Example: Finkelstein (2007)

- Boiling multi-way FE regression specs down to simpler "diff-in-diff' comparisons can make the content of the outcome model clearer
    - Useful fact: in two periods, β in $y_{it} = \beta x_{it} + \alpha_i + \tau_t + \nu_{it}$ is given by the bivariate regression of $y_{i,Post} - y_{i,Pre}$ on $x_{i,Post} - x_{i,Pre}$

- Finkelstein is interested in estimating market-level effects of health insurance coverage, using the 1965 introduction of Medicare
    - Effectively estimates $y_{it} = \beta x_{it} + \alpha_i + \tau_t + \nu_{it}$ where x_{it} is the share of elderly in market i and year t with health insurance

Example: Finkelstein (2007)

- Boiling multi-way FE regression specs down to simpler "diff-in-diff' comparisons can make the content of the outcome model clearer
  - Useful fact: in two periods, β in $y_{it} = \beta x_{it} + \alpha_i + \tau_t + \nu_{it}$ is given by the bivariate regression of $y_{i,Post} - y_{i,Pre}$ on $x_{i,Post} - x_{i,Pre}$
- Finkelstein is interested in estimating market-level effects of health insurance coverage, using the 1965 introduction of Medicare
  - Effectively estimates $y_{it} = \beta x_{it} + \alpha_i + \tau_t + \nu_{it}$ where x_{it} is the share of elderly in market i and year t with health insurance
  - Post 1965, $x_{it} = 1$ for all markets; previously far from random

Example: Finkelstein (2007)

- Boiling multi-way FE regression specs down to simpler "diff-in-diff' comparisons can make the content of the outcome model clearer
  - Useful fact: in two periods, β in $y_{it} = \beta x_{it} + \alpha_i + \tau_t + \nu_{it}$ is given by the bivariate regression of $y_{i,Post} - y_{i,Pre}$ on $x_{i,Post} - x_{i,Pre}$
- Finkelstein is interested in estimating market-level effects of health insurance coverage, using the 1965 introduction of Medicare
  - Effectively estimates $y_{it} = \beta x_{it} + \alpha_i + \tau_t + \nu_{it}$ where x_{it} is the share of elderly in market i and year t with health insurance
  - Post 1965, $x_{it} = 1$ for all markets; previously far from random
- Consider two-period version: equivalent to regressing $y_{i,Post} - y_{i,Pre}$ on $x_{i,Post} - x_{i,Pre} = 1 - x_{i,Pre}$: the pre-Medicare uninsured share in i

Example: Finkelstein (2007)

- Boiling multi-way FE regression specs down to simpler "diff-in-diff' comparisons can make the content of the outcome model clearer
  - Useful fact: in two periods, β in $y_{it} = \beta x_{it} + \alpha_i + \tau_t + \nu_{it}$ is given by the bivariate regression of $y_{i,Post} - y_{i,Pre}$ on $x_{i,Post} - x_{i,Pre}$
- Finkelstein is interested in estimating market-level effects of health insurance coverage, using the 1965 introduction of Medicare
  - Effectively estimates $y_{it} = \beta x_{it} + \alpha_i + \tau_t + \nu_{it}$ where x_{it} is the share of elderly in market i and year t with health insurance
  - Post 1965, $x_{it} = 1$ for all markets; previously far from random
- Consider two-period version: equivalent to regressing $y_{i,Post} - y_{i,Pre}$ on $x_{i,Post} - x_{i,Pre} = 1 - x_{i,Pre}$: the pre-Medicare uninsured share in i
  - Outcome model: if not for the introduction of Medicare, markets with higher/lower uninsured shares would have been on parallel trends

Example: Finkelstein (2007)

- Boiling multi-way FE regression specs down to simpler "diff-in-diff' comparisons can make the content of the outcome model clearer
    - Useful fact: in two periods, β in $y_{it} = \beta x_{it} + \alpha_i + \tau_t + \nu_{it}$ is given by the bivariate regression of $y_{i,Post} - y_{i,Pre}$ on $x_{i,Post} - x_{i,Pre}$
- Finkelstein is interested in estimating market-level effects of health insurance coverage, using the 1965 introduction of Medicare
    - Effectively estimates $y_{it} = \beta x_{it} + \alpha_i + \tau_t + \nu_{it}$ where x_{it} is the share of elderly in market i and year t with health insurance
    - Post 1965, $x_{it} = 1$ for all markets; previously far from random
- Consider two-period version: equivalent to regressing $y_{i,Post} - y_{i,Pre}$ on $x_{i,Post} - x_{i,Pre} = 1 - x_{i,Pre}$: the pre-Medicare uninsured share in i
    - Outcome model: if not for the introduction of Medicare, markets with higher/lower uninsured shares would have been on parallel trends
    - Event study version: $y_{it} = \alpha_i + \tau_t + \sum_s \beta_s (1 - x_{i,Pre}) \mathbf{1}[t = s] + \nu_{it}$; expect flat pre/post trends if the model is right...

Finkelstein Event Study

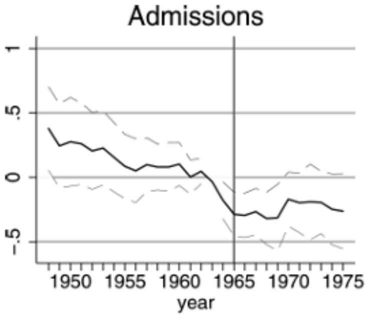


Figure II graphs the pattern of the λ_t coefficients from estimating (1) for the log of the dependent variable given above each graph. The scale of the graph is normalized so that in the reference year (1963) it is the average difference in the dependent variable between the south and west (where Medicare had a larger impact) relative to the north and northeast (where Medicare had a smaller impact). The dashed lines show the 95 percent confidence interval on each coefficient relative to the reference year (1963). Time varying state-level controls (X_{st}) in all analyses consist of eight indicator variables for the number of years before (or since) the implementation of Medicaid in state s (see text for more details).

Different Roles of Controls

- In a design-based specification, the controls can be understood as specifying *which treated/control observations* are valid to compare
  - E.g. private vs. non-private students w/same applications+admissions

Different Roles of Controls

- In a design-based specification, the controls can be understood as specifying *which treated/control observations* are valid to compare
    - E.g. private vs. non-private students w/same applications+admissions
    - Think about how the auxiliary regression isolates variation in x_{it}

Different Roles of Controls

- In a design-based specification, the controls can be understood as specifying *which treated/control observations* are valid to compare
  - E.g. private vs. non-private students w/same applications+admissions
  - Think about how the auxiliary regression isolates variation in x_{it}
- In an outcome-model-based specification, the controls can be seen as specifying *what transformations of the outcomes* are valid to compare
  - E.g. TWFE regressions compare trends in the outcome, allowing the outcome levels to be confounded

Different Roles of Controls

- In a design-based specification, the controls can be understood as specifying *which treated/control observations* are valid to compare
  - E.g. private vs. non-private students w/same applications+admissions
  - Think about how the auxiliary regression isolates variation in x_{it}

- In an outcome-model-based specification, the controls can be seen as specifying *what transformations of the outcomes* are valid to compare
  - E.g. TWFE regressions compare trends in the outcome, allowing the outcome levels to be confounded
  - Think about what "diff-in-diff" comparisons are underlying the spec

Different Roles of Controls

- In a design-based specification, the controls can be understood as specifying *which treated/control observations* are valid to compare
  - E.g. private vs. non-private students w/same applications+admissions
  - Think about how the auxiliary regression isolates variation in x_{it}
- In an outcome-model-based specification, the controls can be seen as specifying *what transformations of the outcomes* are valid to compare
  - E.g. TWFE regressions compare trends in the outcome, allowing the outcome levels to be confounded
  - Think about what "diff-in-diff" comparisons are underlying the spec
- Both strategies have *ex post* validations (balance tests / pre-trend checks), but the *ex ante* case for design is arguably easier to make
  - What ε_{it} model is best? E.g. does parallel trends hold in levels or logs?

Outline

1. Selection on Observables✓
2. Design vs. Outcome Models ✓
3. Design-Based IV

The Simplest IV Story

- Again start w/constant fx model $y_i = \beta x_i + \varepsilon_i$, now $Cov(x_i, \varepsilon_i) \neq 0$
  - E.g. x_i is enrollment in this class and y_i is later wages/happiness
  - "Endogeneity": students in this class have systematically higher ε_i

The Simplest IV Story

- Again start w/constant fx model $y_i = \beta x_i + \varepsilon_i$, now $Cov(x_i, \varepsilon_i) \neq 0$
  - E.g. x_i is enrollment in this class and y_i is later wages/happiness
  - "Endogeneity": students in this class have systematically higher ε_i
- Imagine the course was "oversubscribed"; I chose students by lottery
  - $z_i \in \{0,1\}$ indicates randomized admission to the course
  - Randomness + no direct effects of z_i on y_i implies $Cov(z_i, \varepsilon_i) = 0$

The Simplest IV Story

- Again start w/constant fx model $y_i = \beta x_i + \varepsilon_i$, now $Cov(x_i, \varepsilon_i) \neq 0$
  - E.g. x_i is enrollment in this class and y_i is later wages/happiness
  - "Endogeneity": students in this class have systematically higher ε_i

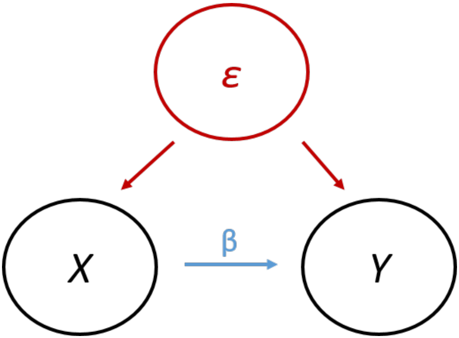
- Imagine the course was "oversubscribed"; I chose students by lottery
  - $z_i \in \{0, 1\}$ indicates randomized admission to the course
  - Randomness + no direct effects of z_i on y_i implies $Cov(z_i, \varepsilon_i) = 0$

- Plugging in the model for $\varepsilon_i = y_i - \beta x_i$, we have IV identification:

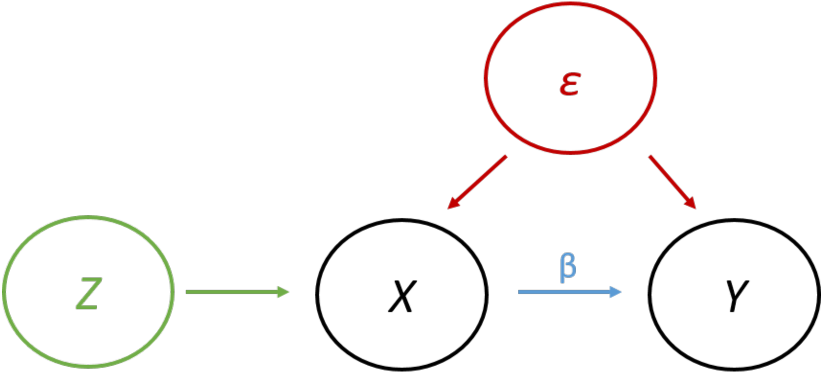
$$Cov(z_i, y_i - \beta x_i) = 0 \implies \frac{Cov(z_i, y_i)}{Cov(z_i, x_i)} = \beta$$

so long as $Cov(z_i, x_i) \neq 0$ ("relevance")

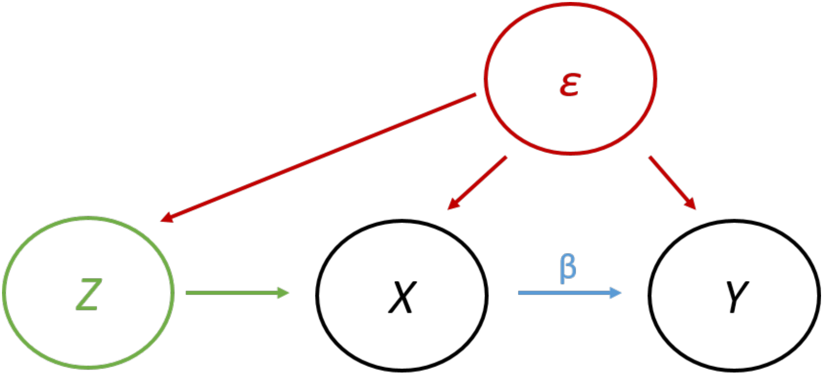
Regression "Endogeneity"



Instrument "Exogeneity" / "Validity"

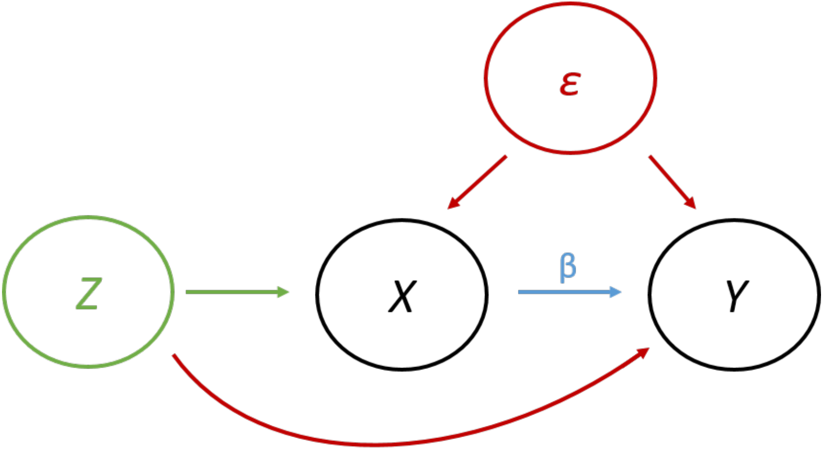


Threats to Validity: Instrument Assignment



We will later formalize this as a failure of instrument “independence”

Threats to Validity: Direct Effects



We will later formalize this as a failure of instrument "exclusion"

Adding Controls and Instruments

- Basic IV is $\frac{Cov(z_i,y_i)}{Cov(z_i,x_i)} = \frac{Cov(z_i,y_i)/Var(z_i)}{Cov(z_i,x_i)/Var(z_i)} = \rho/\pi$ from the regressions:

$$y_i = \kappa + \rho z_i + \nu_i \quad \text{, the "reduced form"}$$

$$x_i = \mu + \pi z_i + \eta_i \quad \text{, the "first stage"}$$

Adding Controls and Instruments

- Basic IV is $\frac{Cov(z_i, y_i)}{Cov(z_i, x_i)} = \frac{Cov(z_i, y_i)/Var(z_i)}{Cov(z_i, x_i)/Var(z_i)} = \rho/\pi$ from the regressions:

$$y_i = \kappa + \rho z_i + \nu_i \quad \text{, the ``reduced form''}$$
$$x_i = \mu + \pi z_i + \eta_i \quad \text{, the ``first stage''}$$

- IV with controls works similarly: ρ/π from the controlled regressions:

$$y_i = \kappa + \rho z_i + w_i'\gamma + \nu_i$$
$$x_i = \mu + \pi z_i + w_i'\gamma + \eta_i$$

Adding Controls and Instruments

- Basic IV is $\frac{Cov(z_i,y_i)}{Cov(z_i,x_i)} = \frac{Cov(z_i,y_i)/Var(z_i)}{Cov(z_i,x_i)/Var(z_i)} = \rho/\pi$ from the regressions:

$$y_i = \kappa + \rho z_i + \nu_i \quad \text{, the "reduced form"}$$
$$x_i = \mu + \pi z_i + \eta_i \quad \text{, the "first stage"}$$

- IV with controls works similarly: ρ/π from the controlled regressions:

$$y_i = \kappa + \rho z_i + w_i'\gamma + \nu_i$$
$$x_i = \mu + \pi z_i + w_i'\gamma + \eta_i$$

- Can also have multiple instruments: $(\pi' w \pi)^{-1} \pi' w \rho$ for some w
    - w governs how different RF/FS's are weighted together (e.g. 2SLS)

Adding Controls and Instruments

- Basic IV is $\frac{Cov(z_i,y_i)}{Cov(z_i,x_i)} = \frac{Cov(z_i,y_i)/Var(z_i)}{Cov(z_i,x_i)/Var(z_i)} = \rho/\pi$ from the regressions:

$$y_i = \kappa + \rho z_i + \nu_i \quad \text{, the ``reduced form''}$$
$$x_i = \mu + \pi z_i + \eta_i \quad \text{, the ``first stage''}$$

- IV with controls works similarly: ρ/π from the controlled regressions:

$$y_i = \kappa + \rho z_i + w_i'\gamma + \nu_i$$
$$x_i = \mu + \pi z_i + w_i'\gamma + \eta_i$$

- Can also have multiple instruments: $(\pi' w \pi)^{-1} \pi' w \rho$ for some w
  - w governs how different RF/FS's are weighted together (e.g. 2SLS)
- RF&FS are the nuclei of IV; the design-based approach starts w/them

Bridging the Gap

- Design-based IV applies the earlier selection-on-observables logic to z_i:
    1. Claim z_i is as-good-as-randomly assigned conditional on some w_i

Bridging the Gap

- Design-based IV applies the earlier selection-on-observables logic to z_i:
  1. Claim z_i is as-good-as-randomly assigned conditional on some w_i
  2. Control flexibly for w_i, such that regressing z_i on w_i estimates $E[z_i \mid w_i]$

Bridging the Gap

- Design-based IV applies the earlier selection-on-observables logic to z_i:
  1. Claim z_i is as-good-as-randomly assigned conditional on some w_i
  2. Control flexibly for w_i, such that regressing z_i on w_i estimates $E[z_i \mid w_i]$
- This makes both reduced form and first stage regressions causal

Bridging the Gap

- Design-based IV applies the earlier selection-on-observables logic to z_i:
    1. Claim z_i is as-good-as-randomly assigned conditional on some w_i
    2. Control flexibly for w_i, such that regressing z_i on w_i estimates $E[z_i \mid w_i]$
- This makes both reduced form and first stage regressions causal
    - As before, best to both tell a clear *ex ante* story about where the $z_i \mid w_i$ variation comes from and validate as-if-random assignment *ex post*

Bridging the Gap

- Design-based IV applies the earlier selection-on-observables logic to z_i:
  1. Claim z_i is as-good-as-randomly assigned conditional on some w_i
  2. Control flexibly for w_i, such that regressing z_i on w_i estimates $E[z_i \mid w_i]$
- This makes both reduced form and first stage regressions causal
  - As before, best to both tell a clear *ex ante* story about where the $z_i \mid w_i$ variation comes from and validate as-if-random assignment *ex post*
- New twist: have to also argue exclusion in order to interpret RF/FS
  - Can both argue *ex ante* and sometimes test *ex post*: e.g. by looking at effects of z_i on other plausible treatment channels

Example: Abdulkadiroglu et al. (2016)

- AAHP are interested in the effect of “takeover” charter schools: ones which convert a low-performing traditional public school (TPS)
  - Lots of evidence of charter effectiveness from admission lotteries, but external validity is an open question

Example: Abdulkadiroglu et al. (2016)

- AAHP are interested in the effect of "takeover" charter schools: ones which convert a low-performing traditional public school (TPS)
  - Lots of evidence of charter effectiveness from admission lotteries, but external validity is an open question

- Reduced-form selection-on-observables strategy: compare students in the TPS pre-takeover to others in similarly low-performing TPS
  - Specifically, match TPS' on baseline test score performance and control for match cell fixed effects

Example: Abdulkadiroglu et al. (2016)

- AAHP are interested in the effect of "takeover" charter schools: ones which convert a low-performing traditional public school (TPS)
  - Lots of evidence of charter effectiveness from admission lotteries, but external validity is an open question
- Reduced-form selection-on-observables strategy: compare students in the TPS pre-takeover to others in similarly low-performing TPS
  - Specifically, match TPS' on baseline test score performance and control for match cell fixed effects
- Exclusion: takeovers only affect later test scores via charter enrollment
  - Check whether there are takeover effects in the transition (pre-charter) year 0; develop a strategy to use these effects to relax exclusion

Abdulkadiroglu et al. Results

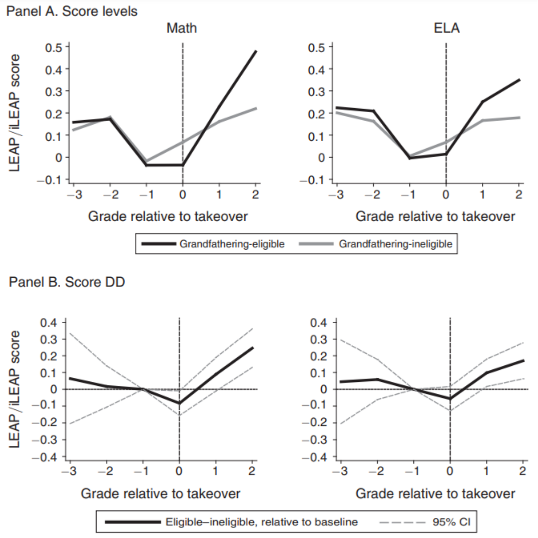


FIGURE 2. TEST SCORES IN THE RSD GRANDFATHERING SAMPLE

Notes: Panel A plots average LEAP/iLEAP math and ELA scores of students in the RSD legacy middle school matched sample. Panel B plots achievement growth relative to the baseline (−1) grade. Estimates in both panels control for matching cell fixed effects. Scores are standardized to those of students at direct-run schools in New Orleans RSD, by grade and year. Grade 0 is the last grade of legacy school enrollment.

Looking Ahead

- We've now seen the basic design-based logic for regression/IV
  - Main practical takeaway: be clear on what variation in x_i or z_i you want to use, and pick controls appropriately for extracting it

Looking Ahead

- We've now seen the basic design-based logic for regression/IV
  - Main practical takeaway: be clear on what variation in x_i or z_i you want to use, and pick controls appropriately for extracting it
- On Wednesday, we'll expand the discussion to other features of design
  1. Heterogeneous effects $\implies$ no worries about "negative weights"
  2. Inference $\implies$ clearer guidance on how to cluster standard errors

Looking Ahead

- We've now seen the basic design-based logic for regression/IV
  - Main practical takeaway: be clear on what variation in x_i or z_i you want to use, and pick controls appropriately for extracting it
- On Wednesday, we'll expand the discussion to other features of design
  1. Heterogeneous effects $\implies$ no worries about "negative weights"
  2. Inference $\implies$ clearer guidance on how to cluster standard errors
- Before then, you have the chance to play with a real-world application