Econometric Methods of Program Evaluation

Sinduja V. Srinivasan

CEPAL Summer School

August 28, 2017
What is Program Evaluation?
What is Program Evaluation? Various Types of Assessment

<table>
<thead>
<tr>
<th>Program need</th>
<th>Program design</th>
<th>Program implementation</th>
<th>Program outcomes or impact</th>
<th>Program efficiency</th>
</tr>
</thead>
<tbody>
<tr>
<td>· Define problem</td>
<td>· Concordance between logic and intention</td>
<td>· Ongoing process</td>
<td>· Estimate effect size</td>
<td>· Cost-effectiveness</td>
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Sinduja V. Srinivasan (CEPAL)
What is Program Evaluation? Various Types of Assessment

Program need
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Program need
- Define problem
- Population at risk
- Measurable effects caused by the problem
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Reduced-form
Fundamental Problem of Causal Inference

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<tr>
<th>Treatment:</th>
<th>( T_i = \begin{cases} 1 \text{ treated state} \ 0 \text{ untreated state} \end{cases} )</th>
</tr>
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<tr>
<td>Outcome:</td>
<td>( Y_i = \alpha + \theta T_i + \epsilon_i = \begin{cases} \text{estimable} \end{cases} )</td>
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Individual treatment effect: \( TE_i = Y_i^1 - Y_i^0 \)

⇒ Not estimable
Fundamental Problem of Causal Inference

Treatment:

\[ T_i = \begin{cases} 
1 & \text{treated state} \\
0 & \text{untreated state} 
\end{cases} \]
Fundamental Problem of Causal Inference

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Outcome:

\[ Y_i = \alpha + \theta T_i + \varepsilon_i = \begin{cases} 
Y_{1i} & \text{outcome for individual } i \text{ in treated state} \\
Y_{0i} & \text{outcome for individual } i \text{ in untreated state} 
\end{cases} \]
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Observe only one
Fundamental Problem of Causal Inference

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\[ \Rightarrow \text{Individual treatment effect:} \]

\[ TE_i = Y_{1i} - Y_{0i} \]

\textbf{Not estimable}
What Can We Estimate?

Average Treatment Effect:

\[ \text{ATE} = \mathbb{E}(Y_1) - \mathbb{E}(Y_0) = \mathbb{E}(Y_i | T_i = 1) - \mathbb{E}(Y_i | T_i = 0) \]

Let

\[ \mu_{T} = \mathbb{E}(Y_i | T_i = 1) = \alpha + \theta + \mathbb{E}(\varepsilon_i | T_i = 1) \]

\[ \mu_{NT} = \mathbb{E}(Y_i | T_i = 0) = \alpha + \mathbb{E}(\varepsilon_i | T_i = 0) \]

\[ \text{ATE} = \mu_T - \mu_{NT} = \theta + \mathbb{E}(\varepsilon_i | T_i = 1) - \mathbb{E}(\varepsilon_i | T_i = 0) \]

Can estimates of ATE be unbiased?
What Can We Estimate?

Average Treatment Effect:

\[ ATE = \mathbb{E}(TE_i) = \mathbb{E}(Y_{1i}) - \mathbb{E}(Y_{0i}) = \mathbb{E}(Y_i | T_i = 1) - \mathbb{E}(Y_{0i} | T_i = 0) \]
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\[
\Rightarrow ATE = \mu_T - \mu_{NT} = \theta + \{\mathbb{E}(\varepsilon_i|T_i = 1) - \mathbb{E}(\varepsilon_i|T_i = 0)\}
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\]

Let

\[
\begin{align*}
\mu_T & = \mathbb{E}(Y_i|T_i = 1) = \alpha + \theta + \mathbb{E}(\epsilon_i|T_i = 1) \\
\mu_{NT} & = \mathbb{E}(Y_i|T_i = 0) = \alpha + \mathbb{E}(\epsilon_i|T_i = 0)
\end{align*}
\]

\[
\Rightarrow ATE = \mu_T - \mu_{NT} = \theta + \left\{ \mathbb{E}(\epsilon_i|T_i = 1) - \mathbb{E}(\epsilon_i|T_i = 0) \right\}
\]

must be 0!
What Can We Estimate?

Average Treatment Effect:

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Let

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\[ \Rightarrow ATE = \mu_T - \mu_{NT} = \theta + \{ \mathbb{E} (\varepsilon_i|T_i = 1) - \mathbb{E} (\varepsilon_i|T_i = 0) \} \]

Can estimates of ATE be unbiased?

must be 0!
Randomized Control Trials (RCT)

Goal:
· Similar composition (observable characteristics) → Unobservables also evenly distributed ⇒ Avoids selection bias:

Conditional Randomization
· Treatment more effective for subgroup (women) → Randomization by subgroup
Randomized Control Trials (RCT)

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Randomized Control Trials (RCT)

Goal:
- Similar composition (observable characteristics)
Randomized Control Trials (RCT)

Goal:
- Similar composition (observable characteristics)
  → Unobservables also evenly distributed

Diagram:
```
Participant
   /  \0.5
  /    \0.5
Treated\   Untreated
```
Randomized Control Trials (RCT)

Goal:
· Similar composition (observable characteristics)
  → Unobservables also evenly distributed
⇒ Avoids selection bias: \( E(\epsilon_i | T_i = 1) - E(\epsilon_i | T_i = 0) = 0 \)
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\[
\begin{align*}
\text{Participant} & \quad \text{Treated} \quad 0.5 \\
& \quad 0.5 \quad \text{Untreated}
\end{align*}
\]
Randomized Control Trials (RCT)

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Conditional Randomization
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Conditional Randomization
- Treatment more effective for subgroup (women)
  → Randomization by subgroup
Validity of Randomization
### Validity of Randomization

#### Descriptive Statistics at Baseline for Children Ages 0-5

<table>
<thead>
<tr>
<th></th>
<th>Treatment</th>
<th>Control</th>
<th>Difference</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>Mean</td>
<td>St. Dev.</td>
<td>Mean</td>
</tr>
<tr>
<td>Ill last month (=1)</td>
<td>0.329</td>
<td>(0.470)</td>
<td>0.326</td>
</tr>
<tr>
<td>Age</td>
<td>1.628</td>
<td>(1.099)</td>
<td>1.612</td>
</tr>
<tr>
<td>Male (=1)</td>
<td>0.511</td>
<td>(0.500)</td>
<td>0.488</td>
</tr>
<tr>
<td>Father’s Years of Education</td>
<td>3.480</td>
<td>(2.746)</td>
<td>3.810</td>
</tr>
<tr>
<td>Mother’s Years of Education</td>
<td>3.612</td>
<td>(2.820)</td>
<td>3.608</td>
</tr>
<tr>
<td>Father Speaks Spanish (=1)</td>
<td>0.913</td>
<td>(0.282)</td>
<td>0.892</td>
</tr>
<tr>
<td>Mother Speaks Spanish (=1)</td>
<td>0.927</td>
<td>(0.261)</td>
<td>0.910</td>
</tr>
<tr>
<td>Own House (=1)</td>
<td>0.924</td>
<td>(0.265)</td>
<td>0.915</td>
</tr>
<tr>
<td>Electricity (=1)</td>
<td>0.646</td>
<td>(0.478)</td>
<td>0.719</td>
</tr>
<tr>
<td>Hectares of Land Owned</td>
<td>0.814</td>
<td>(0.973)</td>
<td>0.790</td>
</tr>
<tr>
<td>Male Agricultural Wage</td>
<td>23.071</td>
<td>(6.970)</td>
<td>23.494</td>
</tr>
<tr>
<td>Female Agricultural Wage</td>
<td>20.614</td>
<td>(6.821)</td>
<td>21.240</td>
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**Sample Size**  
4,444 3,259

Standard errors in parentheses  
Source: Gertler, Paul J. and Boyce, Simone (2001)
# Validity of Randomization

## Descriptive Statistics at Baseline for Children Ages 0-5

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<th>Control Mean</th>
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<th>Level</th>
<th>%</th>
<th>t-stat</th>
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<tr>
<td>Ill last month (=1)</td>
<td>0.329</td>
<td>(0.470)</td>
<td>0.326</td>
<td>(0.469)</td>
<td>0.002</td>
<td>0.6%</td>
<td>(0.220)</td>
</tr>
<tr>
<td>Age</td>
<td>1.628</td>
<td>(1.099)</td>
<td>1.612</td>
<td>(1.110)</td>
<td>0.016</td>
<td>1.0%</td>
<td>(0.630)</td>
</tr>
<tr>
<td>Male (=1)</td>
<td>0.511</td>
<td>(0.500)</td>
<td>0.488</td>
<td>(0.500)</td>
<td>0.023</td>
<td>4.5%</td>
<td>(1.970)</td>
</tr>
<tr>
<td>Father’s Years of Education</td>
<td>3.480</td>
<td>(2.746)</td>
<td>3.810</td>
<td>(2.884)</td>
<td>-0.330</td>
<td>-9.5%</td>
<td>(-5.060)</td>
</tr>
<tr>
<td>Mother’s Years of Education</td>
<td>3.612</td>
<td>(2.820)</td>
<td>3.608</td>
<td>(2.915)</td>
<td>0.004</td>
<td>0.1%</td>
<td>(0.060)</td>
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<td>Father Speaks Spanish (=1)</td>
<td>0.913</td>
<td>(0.282)</td>
<td>0.892</td>
<td>(0.311)</td>
<td>0.021</td>
<td>2.3%</td>
<td>(3.110)</td>
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<tr>
<td>Mother Speaks Spanish (=1)</td>
<td>0.927</td>
<td>(0.261)</td>
<td>0.910</td>
<td>(0.287)</td>
<td>0.017</td>
<td>1.8%</td>
<td>(2.650)</td>
</tr>
<tr>
<td>Own House (=1)</td>
<td>0.924</td>
<td>(0.265)</td>
<td>0.915</td>
<td>(0.278)</td>
<td>0.009</td>
<td>1.0%</td>
<td>(1.410)</td>
</tr>
<tr>
<td>Electricity (=1)</td>
<td>0.646</td>
<td>(0.478)</td>
<td>0.719</td>
<td>(0.450)</td>
<td>-0.073</td>
<td>-11.3%</td>
<td>(-6.860)</td>
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<td>0.814</td>
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<td>-1.8%</td>
<td>(-2.630)</td>
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<td>Female Agricultural Wage</td>
<td>20.614</td>
<td>(6.821)</td>
<td>21.240</td>
<td>(7.024)</td>
<td>-0.625</td>
<td>-3.0%</td>
<td>(-3.910)</td>
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<td>4,444</td>
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<td>3,259</td>
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Issues with Random Assignment

- Contamination/Compliance: Treatment no longer random ⇒ Intent-to-Treat (ITE)
- Attrition: Biased ATT
- External Validity: Extrapolate Results
- Randomization Bias: Randomization changes population participating
- Cost
Issues with Random Assignment

Contamination/Compliance
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$\Rightarrow$ Biased ATE
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Cost

What if we can’t randomize?
Regression Discontinuity (RD) Overview

Assignment into treatment: As Good As Random
Determined by: OBSERVED selection index $S$ with KNOWN cut-off $c$.

$Ti = I(S_i > c)$

Examples
- Treatment: Eligible to start school
  Selection rule: Age 5 by cut-off date
- Treatment: Receive program
  Selection rule: Welfare index above cut-off $S$.
Regression Discontinuity (RD) Overview

Assignment into treatment: “As Good As Random”
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\[ T_i = \mathbb{I} (S_i > c) \]
Assignment into treatment: “As Good As Random”
Determined by: OBSERVED selection index \( S \) with KNOWN cutoff \( c \)
\[ T_i = \mathbb{I} (S_i > c) \]

Examples
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  \[ \rightarrow \text{Selection rule: Reach age 5 by cutoff date} \]
- Treatment: Receive program
  \[ \rightarrow \text{Selection rule: Welfare index above cutoff} \]
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Assignment into treatment: “As Good As Random”
Determined by: OBSERVED selection index \( (S) \) with KNOWN cutoff \( (c) \)

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Examples

- Treatment: Eligible to start school
  → Selection rule: Reach age 5 by cutoff date
- Treatment: Receive program
  → Selection rule: Welfare index above cutoff
Regression Discontinuity (RD) Overview

Assignment into treatment: “As Good As Random”
Determined by: OBSERVED selection index \( S \) with KNOWN cutoff \( c \)
\[ T_i = \mathbb{I}(S_i > c) \]

Examples
- Treatment: Eligible to start school
  \[ \rightarrow \text{Selection rule: Reach age 5 by cutoff date} \]
- Treatment: Receive program
  \[ \rightarrow \text{Selection rule: Welfare index above cutoff} \]

Compare outcomes “near” cutoff
RD Motivation

\[ \hat{\theta} = E(Y_i | T_i = 1) - E(Y_i | T_i = 0) = \theta + \tilde{\epsilon}_i \]

Restrict attention to interval around \( c \)

\[ \hat{\theta} = E(Y_i | S_i = c + \delta) - E(Y_i | S_i = c - \delta) = \theta + \tilde{\epsilon}_i \]

Assumption:

\[ \lim_{S_i \to c^+} E(\tilde{\epsilon}_i | S_i) = \lim_{S_i \to c^-} E(\tilde{\epsilon}_i | S_i) \]
### RD Motivation

Difference in sample means $\rightarrow$ biased $\hat{\theta}$
RD Motivation

Difference in sample means $\rightarrow$ biased $\hat{\theta}$

\[
\hat{\theta} = \mathbb{E}(Y_i|T_i = 1) - \mathbb{E}(Y_i|T_i = 0)
= \theta + \mathbb{E}(\varepsilon_i|T_i = 1) - \mathbb{E}(\varepsilon_i|T_i = 0)
= \theta + \mathbb{E}(\varepsilon_i|S_i > c) - \mathbb{E}(\varepsilon_i|S_i \leq c)
\]
RD Motivation

Difference in sample means $\rightarrow$ biased $\hat{\theta}$

\[
\hat{\theta} = \mathbb{E}(Y_i | T_i = 1) - \mathbb{E}(Y_i | T_i = 0) \\
= \theta + \mathbb{E}(\varepsilon_i | T_i = 1) - \mathbb{E}(\varepsilon_i | T_i = 0) \\
= \theta + \mathbb{E}(\varepsilon_i | S_i > c) - \mathbb{E}(\varepsilon_i | S_i \leq c) \\
\neq 0!
\]
RD Motivation

Difference in sample means $\rightarrow$ biased $\hat{\theta}$

\[
\hat{\theta} = \mathbb{E}(Y_i | T_i = 1) - \mathbb{E}(Y_i | T_i = 0) \\
= \theta + \mathbb{E}(\varepsilon_i | T_i = 1) - \mathbb{E}(\varepsilon_i | T_i = 0) \\
= \theta + \mathbb{E}(\varepsilon_i | S_i > c) - \mathbb{E}(\varepsilon_i | S_i \leq c)
\]

$\rightarrow$ Restrict attention to interval around $c$
RD Motivation

Difference in sample means → biased $\hat{\theta}$

$$\hat{\theta} = \mathbb{E}(Y_i|T_i = 1) - \mathbb{E}(Y_i|T_i = 0)$$
$$= \theta + \mathbb{E}(\varepsilon_i|T_i = 1) - \mathbb{E}(\varepsilon_i|T_i = 0)$$
$$= \theta + \mathbb{E}(\varepsilon_i|S_i > c) - \mathbb{E}(\varepsilon_i|S_i \leq c)$$

→ Restrict attention to interval around $c$

$$\hat{\theta} = \mathbb{E}(Y_i|S_i = c + \delta) - \mathbb{E}(Y_i|S_i = c - \delta)$$
$$= \theta + \mathbb{E}(\varepsilon_i|S_i = c + \delta) - \mathbb{E}(\varepsilon_i|S_i = c - \delta)$$
RD Motivation

Difference in sample means → biased $\hat{\theta}$

$$\hat{\theta} = \mathbb{E} (Y_i | T_i = 1) - \mathbb{E} (Y_i | T_i = 0)$$
$$= \theta + \mathbb{E} (\varepsilon_i | T_i = 1) - \mathbb{E} (\varepsilon_i | T_i = 0)$$
$$= \theta + \mathbb{E} (\varepsilon_i | S_i > c) - \mathbb{E} (\varepsilon_i | S_i \leq c)$$

→ Restrict attention to interval around $c$

$$\hat{\theta} = \mathbb{E} (Y_i | S_i = c + \delta) - \mathbb{E} (Y_i | S_i = c - \delta)$$
$$= \theta + \mathbb{E} (\varepsilon_i | S_i = c + \delta) - \mathbb{E} (\varepsilon_i | S_i = c - \delta)$$

Assumption:

$$\lim_{S_i \rightarrow c^+} \mathbb{E} (\varepsilon_i | S_i) = \lim_{S_i \rightarrow c^-} \mathbb{E} (\varepsilon_i | S_i)$$
RD Motivation

All other determinants of $Y$ are continuous at $c$. 

\[ Y \]

\[ S \]

$C$
**RD Motivation**

All other determinants of $Y$ continuous at $c$

![Graph showing the continuity of determinants of $Y$ at $c$](image-url)
RD Estimation

\[ \bar{Y}_{near c} \cdot (T_i - \bar{Y}_{near c}) \cdot C \]

What is near?

Discrete variables

Parametric Model

\[ Y_i = \alpha + \theta T_i + \beta_1 S_i + \beta_2 S_i^2 + \beta_3 T_i S_i + \beta_4 T_i S_i^2 + \varepsilon_i \]

Misspecification: FIRST-ORDER issue

Non-parametric Models

Local Linear Regression (Regression around \( c \))
RD Estimation

Compare sample means near $c$: $\bar{Y}_{T \text{near } c} - \bar{Y}_{C \text{near } c}$
RD Estimation

Compare sample means near $c$: $\bar{Y}_{T \text{ near } c} - \bar{Y}_{C \text{ near } c}$

- What is “near”?
RD Estimation

Compare sample means near $c$: $\bar{Y}_{T}^{\text{near } c} - \bar{Y}_{C}^{\text{near } c}$

- What is “near”?
- Discrete variables
RD Estimation

Compare sample means near $c$: $\bar{Y}_{\text{near } T} - \bar{Y}_{\text{near } C}$

- What is “near”?
- Discrete variables

Parametric Model
RD Estimation

Compare sample means near $c$: $\bar{Y}_{T}^{\text{near } c} - \bar{Y}_{C}^{\text{near } c}$

- What is “near”? 
- Discrete variables

Parametric Model

$\rightarrow Y_i = \alpha + \theta T_i + \beta_1 S_i + \beta_2 S_i^2 + \beta_3 T_i S_i + \beta_4 T_i S_i^2 + \epsilon_i$
RD Estimation

Compare sample means near \( c \): \( \bar{Y}_{T \text{ near } c} - \bar{Y}_{C \text{ near } c} \)
- What is “near”?
- Discrete variables

Parametric Model
\[
Y_i = \alpha + \theta T_i + \beta_1 S_i + \beta_2 S_i^2 + \beta_3 T_i S_i + \beta_4 T_i S_i^2 + \varepsilon_i
\]
RD Estimation

Compare sample means near $c$: $\bar{Y}_T^{\text{near } c} - \bar{Y}_C^{\text{near } c}$
- What is “near”? 
- Discrete variables

Parametric Model

$Y_i = \alpha + \theta T_i + \beta_1 S_i + \beta_2 S_i^2 + \beta_3 T_i S_i + \beta_4 T_i S_i^2 + \varepsilon_i$
- Misspecification: FIRST-ORDER issue
RD Estimation

Compare sample means near $c$: $\bar{Y}^{\text{near } c}_T - \bar{Y}^{\text{near } c}_C$

- What is “near”?
- Discrete variables

**Parametric Model**

$Y_i = \alpha + \theta T_i + \beta_1 S_i + \beta_2 S_i^2 + \beta_3 T_i S_i + \beta_4 T_i S_i^2 + \epsilon_i$

- Misspecification: FIRST-ORDER issue

**Non-parametric Models**
Compare sample means near $c$: $\bar{Y}_{T \text{ near } c} - \bar{Y}_{C \text{ near } c}$

- What is “near”?
- Discrete variables

Parametric Model

$Y_i = \alpha + \theta T_i + \beta_1 S_i + \beta_2 S_i^2 + \beta_3 T_i S_i + \beta_4 T_i S_i^2 + \epsilon_i$

- Misspecification: FIRST-ORDER issue

Non-parametric Models

- Local Linear Regression (Regression around $c$)
Validity of RD
Validity of RD

McCrary density test
Validity of RD

McCrary density test
Validity of RD

McCrary density test

Continuity of observables
Validity of RD

McCrary density test

Continuity of observables

Falsification tests
Validity of RD

McCrary density test

Continuity of observables

Falsification tests

Including covariates
Differences-in-Differences (DID) Overview
Differences-in-Differences (DID) Overview

Non-experimental context
- Program roll-out → Treatment and Control
Differences-in-Differences (DID) Overview

Non-experimental context
- Program roll-out $\rightarrow$ Treatment and Control

![Diagram showing Differences-in-Differences (DID) method](image)
Differences-in-Differences (DID) Overview

Non-experimental context
- Program roll-out → Treatment and Control

Compare pre- and post-program outcomes for the treatment group
Differences-in-Differences (DID) Overview

Non-experimental context

- Program roll-out → Treatment and Control

Compare pre- and post-program outcomes for the treatment group: **No!**
- Ignores trends
Differences-in-Differences (DID) Overview

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Non-experimental context
· Program roll-out → Treatment and Control

Compare pre- and post-program outcomes for the treatment group: **No!**
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Compare post-program outcomes for treated and untreated
Differences-in-Differences (DID) Overview

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Compare pre- and post-program outcomes for the treatment group: **No!**
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Compare post-program outcomes for treated and untreated: **No!**
· Ignores pre-existing differences (selection bias)
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Differences-in-Differences (DID) Overview

Non-experimental context
· Program roll-out → Treatment and Control

Compare pre- and post-program outcomes for the treatment group: **No!**
· Ignores trends

Compare post-program outcomes for treated and untreated: **No!**
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DID
· First difference: Pre-post comparison **within** treatment and control groups
Differences-in-Differences (DID) Overview

Non-experimental context
· Program roll-out → Treatment and Control

Compare pre- and post-program outcomes for the treatment group: **No!**
· Ignores trends

Compare post-program outcomes for treated and untreated: **No!**
· Ignores pre-existing differences (selection bias)

DID
· First difference: Pre-post comparison **within** treatment and control groups
· Second difference: Pre-post difference compared **across** treatment and control groups
Differences-in-Differences (DID) Overview

Non-experimental context
- Program roll-out → Treatment and Control

Compare pre- and post-program outcomes for the treatment group: **No!**
- Ignores trends

Compare post-program outcomes for treated and untreated: **No!**
- Ignores pre-existing differences (selection bias)

**DID**
- First difference: Pre-post comparison *within* treatment and control groups
- Second difference: Pre-post difference compared *across* treatment and control groups
- Time trends and selection biases “differenced out”
Differences-in-Differences (DID) Overview

DID
- First difference: Pre-post comparison **within** treatment and control groups
- Second difference: Pre-post difference compared **across** treatment and control groups
- Time trends and selection biases “differenced out”
DID Estimation

\[ \hat{\theta} = (\bar{Y}_{T2} - \bar{Y}_{C2}) - (\bar{Y}_{T1} - \bar{Y}_{C1}) \]

Regression

\[ Y_{igt} = \alpha + \beta_1 t_{treated} g + \beta_2 post_t + \theta t_{treated} g \cdot post_t + \varepsilon_{igt} \]

Covariates → more precise estimates
DID Estimation

Difference in means

\[ \hat{\theta} = (\bar{Y}_2^T - \bar{Y}_2^C) - (\bar{Y}_1^T - \bar{Y}_1^C) \]
DID Estimation

Difference in means

\[ \hat{\theta} = (\bar{Y}_2^T - \bar{Y}_2^C) - (\bar{Y}_1^T - \bar{Y}_1^C) \]

Regression

\[ Y_{igt} = \alpha + \beta_1 \text{treated}_g + \beta_2 \text{post}_t + \theta \text{treated}_g \cdot \text{post}_t + \epsilon_{igt} \]
**DID Estimation**

Difference in means

\[ \hat{\theta} = (\bar{Y}_2 - \bar{Y}_C^2) - (\bar{Y}_1 - \bar{Y}_C^1) \]

Regression

\[ Y_{igt} = \alpha + \beta_1 \text{treated}_g + \beta_2 \text{post}_t + \theta \text{treated}_g \cdot \text{post}_t + \epsilon_{igt} \]

- Covariates → more precise estimates
Validity of DID

Assumption: Changes in unobservables constant over time

Falsification Tests

- Same groups, different time periods
  \[ \hat{\theta} = 0 \]

- Different Control Group
  \[ \text{same} \hat{\theta} \]

- Different Outcome
  \[ \text{same} \hat{\theta} \]
  \[ \hat{\theta} = 0 \]
Validity of DID

Assumption: Changes in unobservables constant over time
Validity of DID

Assumption: Changes in unobservables constant over time

![Graph showing changes in Treatment Group and Control Group pre-treatment and post-treatment.](image)
Validity of DID

Assumption: Changes in unobservables constant over time

Falsification Tests

- Same groups, different time period
Validity of DID

Assumption: Changes in unobservables constant over time

Falsification Tests
· Same groups, different time period $\Rightarrow \hat{\theta} = 0$
Validity of DID

Assumption: Changes in unobservables constant over time

Falsification Tests

· Same groups, different time period $\Rightarrow \hat{\theta} = 0$

Different Control Group

· Same outcome and time periods
Validity of DID

Assumption: Changes in unobservables constant over time

Falsification Tests

· Same groups, different time period $\Rightarrow \hat{\theta} = 0$

Different Control Group

· Same outcome and time periods $\Rightarrow \text{same } \hat{\theta}$
Validity of DID

Assumption: Changes in unobservables constant over time

Falsification Tests
- Same groups, different time period $\Rightarrow \hat{\theta} = 0$

Different Control Group
- Same outcome and time periods $\Rightarrow$ same $\hat{\theta}$

Different Outcome
- Same groups and time periods
Validity of DID

Assumption: Changes in unobservables constant over time

Falsification Tests

· Same groups, different time period $\Rightarrow \hat{\theta} = 0$

Different Control Group

· Same outcome and time periods $\Rightarrow$ same $\hat{\theta}$

Different Outcome

· Same groups and time periods $\Rightarrow \hat{\theta} = 0$
Instrumental Variables

\[
Y_i = \theta T_i + \epsilon_i \\
T_i = \pi Z_i + \nu_i
\]

Interested in effect of $T$ on $Y$: But $T$ endogenous

- Must impact (shift) $T$ \( \Rightarrow \pi \neq 0 \)
- Impact $Y$ only through $T$ \( \Rightarrow \text{Cov}(Z_i, \epsilon_i) = 0 \)

Estimation: 2-Stage Least Squares (2SLS)
- Estimate \( \hat{\pi} \)
- Predict \( \hat{T}_i \)
- Regress $Y$ on $\hat{T}_i$

Combine IV with RD if there is non-compliance
Instrumental Variables

Model:

\[
(1) \quad Y_i = \theta T_i + \varepsilon_i \\
(2) \quad T_i = \pi Z_i + \nu_i \\
(RF) \quad Y_i = \gamma Z_i + \theta \nu_i + \varepsilon_i
\]
Instrumental Variables

Model:

\[(1) \quad Y_i = \theta T_i + \varepsilon_i\]
\[(2) \quad T_i = \pi Z_i + \nu_i\]
\[(RF) \quad Y_i = \gamma Z_i + \theta \nu_i + \varepsilon_i\]

Interested in effect of \(T\) on \(Y\): But \(T\) endogenous
Instrumental Variables

Model:

(1) \[ Y_i = \theta T_i + \epsilon_i \]

(2) \[ T_i = \pi Z_i + \nu_i \]

(RF) \[ Y_i = \gamma Z_i + \theta \nu_i + \epsilon_i \]

Interested in effect of \( T \) on \( Y \): But \( T \) endogenous

Variable \( Z \) (instrument)
Instrumental Variables

Model:

\[ \begin{align*}
(1) \quad Y_i & = \theta T_i + \varepsilon_i \\
(2) \quad T_i & = \pi Z_i + \nu_i \\
(RF) \quad Y_i & = \gamma Z_i + \theta \nu_i + \varepsilon_i
\end{align*} \]

Interested in effect of \( T \) on \( Y \): But \( T \) endogenous

Variable \( Z \) (instrument)
- Must impact (shift) \( T \Rightarrow \pi \neq 0 \)
Instrumental Variables

Model:

\[
\begin{align*}
(1) \quad Y_i &= \theta T_i + \varepsilon_i \\
(2) \quad T_i &= \pi Z_i + \nu_i \\
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\end{align*}
\]

Interested in effect of \( T \) on \( Y \): But \( T \) endogenous

Variable \( Z \) (instrument)
- Must impact (shift) \( T \Rightarrow \pi \neq 0 \rightarrow \text{Strong first-stage} \)
Instrumental Variables

Model:

\[ Y_i = \theta T_i + \varepsilon_i \]
\[ T_i = \pi Z_i + \nu_i \]
\[ Y_i = \gamma Z_i + \theta \nu_i + \varepsilon_i \]

Interested in effect of \( T \) on \( Y \): But \( T \) endogenous

Variable \( Z \) (instrument)

- Must impact (shift) \( T \Rightarrow \pi \neq 0 \)
- Impact \( Y \) only through \( T \Rightarrow \text{Cov}(Z_i, \varepsilon_i) = 0 \)
Instrumental Variables

Model:

\[
\begin{align*}
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\end{align*}
\]

Interested in effect of $T$ on $Y$: But $T$ endogenous

Variable $Z$ (instrument)

- Must impact (shift) $T \Rightarrow \pi \neq 0$
- Impact $Y$ only through $T \Rightarrow Cov(Z_i, \varepsilon_i) = 0 \rightarrow \text{Exclusion restriction}$
Instrumental Variables

Model:

\[
\begin{align*}
(1) \quad Y_i &= \theta T_i + \varepsilon_i \\
(2) \quad T_i &= \pi Z_i + \nu_i \\
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\end{align*}
\]

Interested in effect of \( T \) on \( Y \): But \( T \) endogenous

Variable \( Z \) (instrument)

- Must impact (shift) \( T \Rightarrow \pi \neq 0 \)
- Impact \( Y \) only through \( T \Rightarrow \text{Cov}(Z_i, \varepsilon_i) = 0 \)

Estimation: 2-Stage Least Squares (2SLS)
Instrumental Variables

Model:

\( Y_i = \theta T_i + \varepsilon_i \)  
\( T_i = \pi Z_i + \nu_i \)  
\( Y_i = \gamma Z_i + \theta \nu_i + \varepsilon_i \)

Interested in effect of \( T \) on \( Y \): But \( T \) endogenous

Variable \( Z \) (instrument)
\· Must impact (shift) \( T \Rightarrow \pi \neq 0 \)
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Estimation: 2-Stage Least Squares (2SLS)
\· Estimate \( \hat{\pi} \)
Instrumental Variables

Model:

\[
\begin{align*}
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\]

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- Impact $Y$ only through $T \Rightarrow \text{Cov}(Z_i, \varepsilon_i) = 0$

Estimation: 2-Stage Least Squares (2SLS)

- Estimate $\hat{\pi}$
- Predict $\hat{T}_i$
Instrumental Variables

Model:

\begin{align*}
(1) \quad Y_i &= \theta T_i + \varepsilon_i \\
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- Estimate $\hat{\pi}$
- Predict $\hat{T}_i$
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Combine IV with RD if there is non-compliance
Propensity Score Matching

Goal: Create control group to match treatment group

Estimate propensity score for entire sample

⇒ Logistic regression

- Dependent variable: Treatment status
- Regressors: Variables associated with treatment and outcome
- Obtain propensity score: Predicted probability ($p$) of treatment

Check $p$-score balanced
→ Overlap: $0 < p(x) < 1$, $\forall x$

Match each treated to at least one control using $p$-score
→ New sample

- Exact matching
- Nearest neighbor matching
- Many other methods

Verify covariates balanced

Regression based on new sample

Combine DID with PSM to improve estimate
Goal: Create control group to match treatment group
Propensity Score Matching

Goal: Create control group to match treatment group

Estimate propensity score for entire sample
Goal: Create control group to match treatment group

Estimate propensity score **for entire sample** ⇒ Logistic regression
Propensity Score Matching

Goal: Create control group to match treatment group

Estimate propensity score for entire sample ⇒ Logistic regression
  · Dependent variable: Treatment status
Propensity Score Matching

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Propensity Score Matching

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Regression based on new sample
Propensity Score Matching

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Regression based on new sample

Combine DID with PSM to improve estimate
Conclusion

RCT may be useful, not always feasible

Quasi-experimental conditions

- Regression discontinuity
- Differences-in-Differences
- Instrumental Variables
- Propensity Score Matching

Choose carefully

- Test assumptions
- Validate

Resources

- Joshua Angrist
- Guido Imbens
- Alan Krueger
- Mostly Harmless Econometrics
- Microeconometrics Using Stata
RCT may be useful, not always feasible
Conclusion

RCT may be useful, not always feasible

Quasi-experimental conditions
RCT may be useful, not always feasible

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· Alan Krueger
· Mostly Harmless Econometrics
· Microeconometrics Using Stata
Thank you!

Sources:
Paco Martorell, UC Davis
Eirk Meijer, RAND Corporation