Introduction to Impact evaluation: Methods & Examples

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Outline of presentation

1. The Evaluation Problem & Selection Bias
2. Solutions to the evaluation problem
   - Cross- Sectional Estimator
   - Before and After Estimator
   - Double Difference Estimator
3. Experimental Designs
4. Quasi-Experimental Designs: PSM, RDD.
5. Instrumental Variables and IE
6. How to implement an Impact evaluation

1. Evaluation Problem and Selection Bias

How to assess impact

- What is beneficiary's test score with program compared to without program?

- Formally, program impact is:

$$E\ (Y \mid T=1) - E(Y \mid T=0)$$

- Compare same individual with & without programs at same point in time
- So what's the Problem?

Solving the evaluation problem

- Problem: we never observe the same individual with and without program at same point in time
  - Observe: $E(Y \mid T=1)$ & $E(Y \mid T=0)$ ➔ NO!
- Solution: estimate what would have happened if beneficiary had not received benefits
  - Observe: $E(Y \mid T=1)$ ➔ YES!
  - Estimate: $E(Y \mid T=0)$ ➔ YES!!

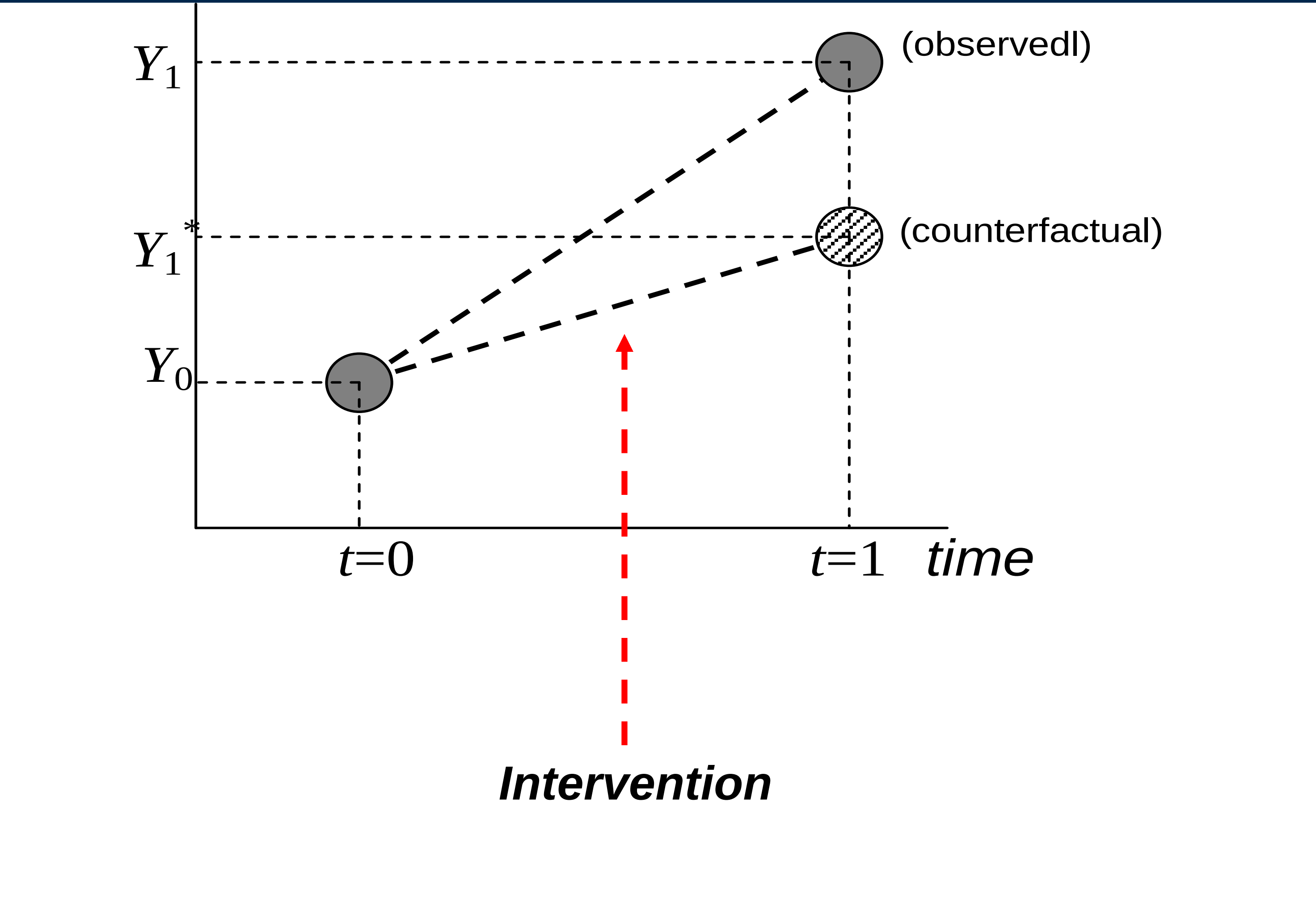
Solving the evaluation problem

- Counterfactual: what would have happened without the program
- Estimated impact is difference between treated observation and counterfactual
- Never observe same individual with and without program at same point in time
- Need to estimate counterfactual
- Counterfactual is key to impact evaluation

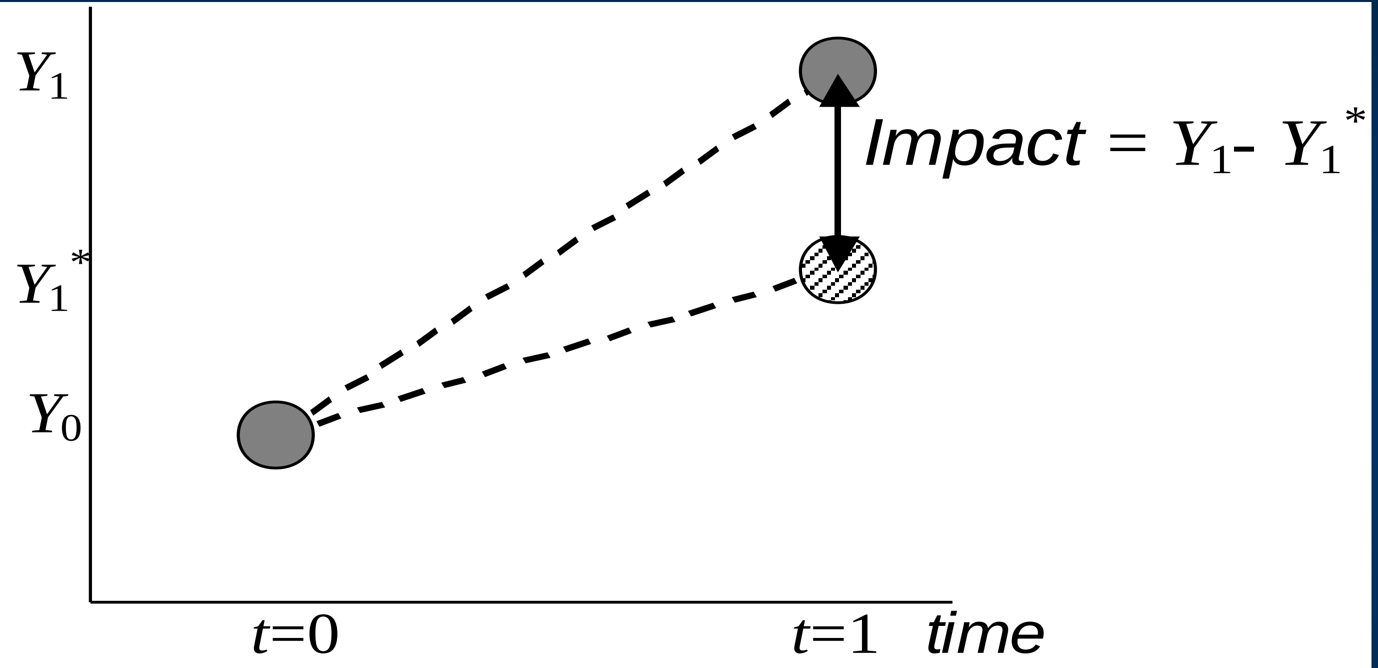
Finding a good counterfactual

- Treated & counterfactual
  - have identical characteristics,
  - except for benefiting from the intervention
- No other reason for differences in outcomes of treated and counterfactual
- Only reason for the difference in outcomes is due to the intervention

Having the "ideal" counterfactual……



allows us to estimate the true impact



Comparison Group Issues

- Two central problems:
  - Programs are targeted
    - Program areas will differ in observable and unobservable ways precisely because the program intended this
  - Individual participation is (usually) voluntary
    - Participants will differ from non-participants in observable and unobservable ways (selection based on observable variables such as age and education and unobservable variables such as ability, motivation, drive)
- Hence, a comparison of participants and an arbitrary group of non-participants can lead to heavily biased results

Archetypal formulation

Outcomes (Y) with and without treatment (D) given exogenous covariates (X):

$$Y_i^T = X_i \beta^T + \mu_i^T \ (i=1,..,n)$$
$$Y_i^C = X_i \beta^C + \mu_i^C \ (i=1,..,n)$$
$$E(\mu_{0i} | X_i) = E(\mu_{1i} | X_i) = 0$$

Gain from the program: $G_i \equiv Y_i^T - Y_i^C$

ATE: average treatment effect: $E(G_i)$

conditional ATE: $E(G_i | X_i) = X_i(\beta^T - \beta^C)$

ATET: ATE on the treated: $E(G_i | D_i = 1)$

conditional ATET:

$$E(G_i | X_i, D_i = 1) = X_i(\beta^T - \beta^C) + E(\mu_i^T - \mu_i^C | X_i, D_i = 1)$$

The evaluation problem

Given that we cannot observe Y_i^C for $D_i = 1$ or Y_i^T for $D_i = 0$, suppose we estimate the following model?

$$Y_i^T = X_i \beta^T + \mu_i^T \text{ if } D_i = 1$$
$$Y_i^C = X_i \beta^C + \mu_i^C \text{ if } D_i = 0$$

Or the (equivalent) switching regression:

$$Y_i = D_i Y_i^T + (1 - D_i) Y_i^C = X_i \beta^C + X_i(\beta^T - \beta^C) D_i + \varepsilon_i$$
$$\varepsilon_i = D_i(\mu_i^T - \mu_i^C) + \mu_i^C$$

Common effects specification (only intercepts differ):

$$Y_i = (\beta_0^T - \beta_0^C) D_i + X_i \beta^C + \varepsilon_i$$

The problem: X can be assumed exogenous but, without random assignment, D is <u>endogenous</u> => ordinary regression will give a biased estimate of impact.

Alternative solutions 1

Experimental evaluation ("Social experiment")

- Program is randomly assigned, so that everyone has the same probability of receiving the treatment.
- In theory, this method is assumption free, but in practice many assumptions are required.
- Pure randomization is rare for anti-poverty programs in practice, since randomization precludes purposive targeting.
- Although it is sometimes feasible to partially randomize.

Alternative solutions 2

<u>Non-experimental evaluation</u> ("Quasi-experimental"; "observational studies")

One of two (non-nested) conditional independence assumptions:

1. *Placement is independent of outcome given X*

   → single difference methods assuming conditionally exogenous placement

   OR placement is independent of outcomes changes

   → Double difference methods

2. *A correlate of placement is independent of outcomes given D and X*

   → Instrumental variables estimator

Generic issues

- Selection bias
- Spillover effects

Selection bias in the outcome difference between participants and non-participants

Observed difference in mean outcomes between participants (D=1) and non-participants (D=0):

$$E(Y^T|D=1) - E(Y^C|D=0) =$$

$$E(Y^T|D=1) - E(Y^C|D=1)$$

ATET=average treatment effect on the treated

$$+E(Y^C|D=1) - E(Y^C|D=0)$$

= 0 with exogenous program placement

Selection bias=difference in mean outcomes (in the absence of the intervention) between participants and non-participants

Two sources of selection bias

- Selection on <u>observables</u>

  Data

  Linearity in controls?
- Selection on <u>unobservables</u>

  Participants have latent attributes that yield higher/lower outcomes

- One cannot judge if exogeneity is plausible without knowing whether one has dealt adequately with observable heterogeneity.
- That depends on program, setting and data.

Spillover effects

- Hidden impacts for non-participants?

- Spillover effects can stem from:
  - Markets
  - Non-market behavior of participants/non-participants
  - Behavior of intervening agents (governmental/NGO)

- Example 1: Poor-area programs
  - Aid targeted to poor villages+local govt. response
- Example 2: Employment Guarantee Scheme
  - assigned program, but no valid comparison group.

Even with controls…

OLS only gives consistent estimates under conditionally exogenous program placement

- there is no selection bias in placement, conditional on X
- or (equivalently) that the conditional mean outcomes do not depend on treatment:

$$E[Y_i^C | X_i, D_i = 1] = E[Y_i^C | X_i, D_i = 0]$$

Implying:

$$E[\varepsilon_i | X_i, D_i] = 0$$

in common impact model.

controls

Regression controls and matching

OLS regression

Ordinary least squares (OLS) estimator of impact with controls for selection on observables.

Switching regression:

$$Y_i = D_i Y_i^T + (1 - D_i) Y_i^C = X_i \beta^C + X_i (\beta^T - \beta^C) D_i + \varepsilon_i$$

$$\varepsilon_i = D_i (\mu_i^T - \mu_i^C) + \mu_i^C$$

Common effects specification:

$$Y_i = (\beta_0^T - \beta_0^C) D_i + X_i \beta^C + \varepsilon_i$$

Randomization

"Randomized out" group reveals counterfactual

- As long as the assignment is genuinely <u>random</u>, mean impact is revealed:

$$E(Y^C | D=1) = E(Y^C | D=0)$$

- ATE is consistently estimated (nonparametrically) by the difference between sample mean outcomes of participants and non-participants.

- Pure randomization is the theoretical ideal for ATE, and the benchmark for non-experimental methods.

- More common: randomization <u>conditional</u> on '*X*'

2. Impact Evaluation methods

Differ in how they construct the counterfactual

- Cross sectional Differences
- Before and After (Reflexive comparisons)
- Difference in Difference (Dif in Dif)
- Experimental methods/Randomization
- Quasi-experimental methods
  - Propensity score matching (PSM) (not discussed)
  - Regression discontinuity design (RDD)
- Econometric methods
  - Instrumental variables/Encouragement designs

Cross-Sectional Estimator

- Counterfactual for participants: Non-participant n the same village or hh in similar villages
- But then:

Measured Impact = E(Y | T=1) - E(Y | T=0)= True Impact + MSB

where MSB=Mean Selection Bias = MA(T=1) - MA(T=0)

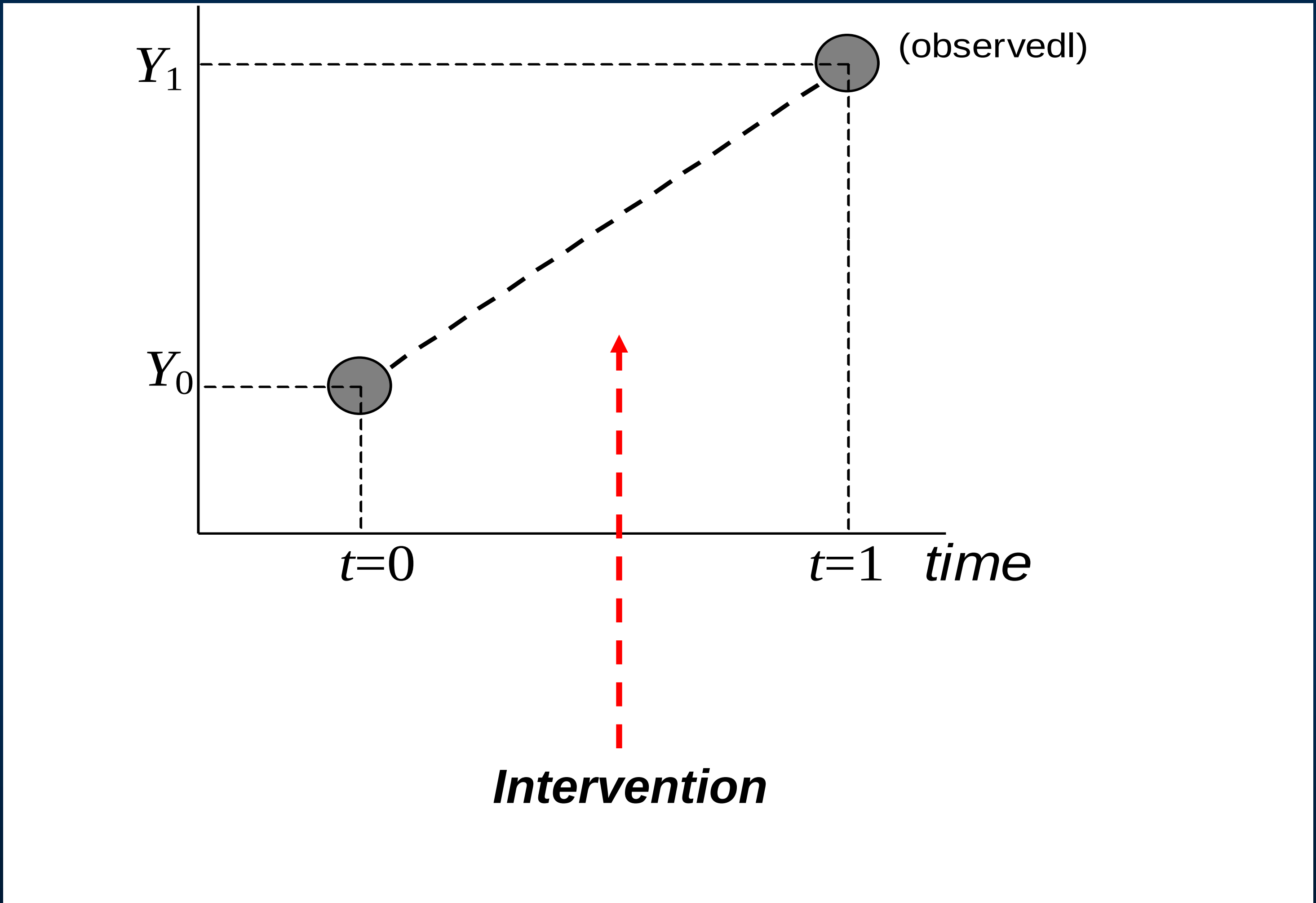
- If MA(T=1) > MA(T=0) then MSB>0 and measured impact > true impact
- Note: An Experimental or Randomized Design
  - Assigns individuals into T=1 and T=0 groups randomly.
  - Consequence: MA(T=1) = MA(T=0) →MSB=0 and
  - Measure Impact = True Impact

Before and After Estimator

- Counterfactual for participants: the participants themselves before the start of the program
- Steps:
  - Collect baseline data on potential participants before the program
  - Compare with data on the same individuals (villages) after the program
  - Take the difference (after – before) or use a regression with a dummy variable identifying round 2 obs

- This allows for the presence of selection bias assuming it is time invariant and enters additively in the model

Before and After Estimator



Shortcomings of Before and After (BA) comparisons

- Not different from "Results Based" Monitoring
- Overestimates impacts
- Measured Impact = True Impact + Trend
  - Attribute all changes over time to the program (i.e. assume that there would have been no trend, or no changes in outcomes in the absence of the program)

- Note: Difference in difference may be thought as a method that tries to improve upon the BA method

Difference-in-difference (DiD):

- Counterfactual for participants: Observed changes over time for non-participants
- Steps:
  - Collect baseline data on non-participants and (probable) participants before the program.
    - Note: there is no particular assumption about how the non-participants are selected. Could use arbitrary comparison group
  - Or could use comparison group selected via PSM/RDD
  - Compare with data after the program.
  - Subtract the two differences, or use a regression with a dummy variable for participant.

- This allows for selection bias but it must be time-invariant and additive.

Difference-in-difference (DiD): Interpretation 1

- Dif-in-Dif removes the trend effect from the estimate of impact using the BA method
  - True impact= Measured Impact in Treat G (or BA)– Trend
- The change in the control group provides an estimate of the trend. Subtracting the "trend" form the change in the treatment group yields the true impact of the program
  - The above assumes that the trend in the C group is an accurate representation of the trend that would have prevailed in the T group in the absence of the program. That is an assumption that cannot be tested (or very hard to test).
  - What if the trend in the C group is not an accurate representation of the trend that would have prevailed in the T group in the absence of the program?? Need observations on Y one period before the baseline period.

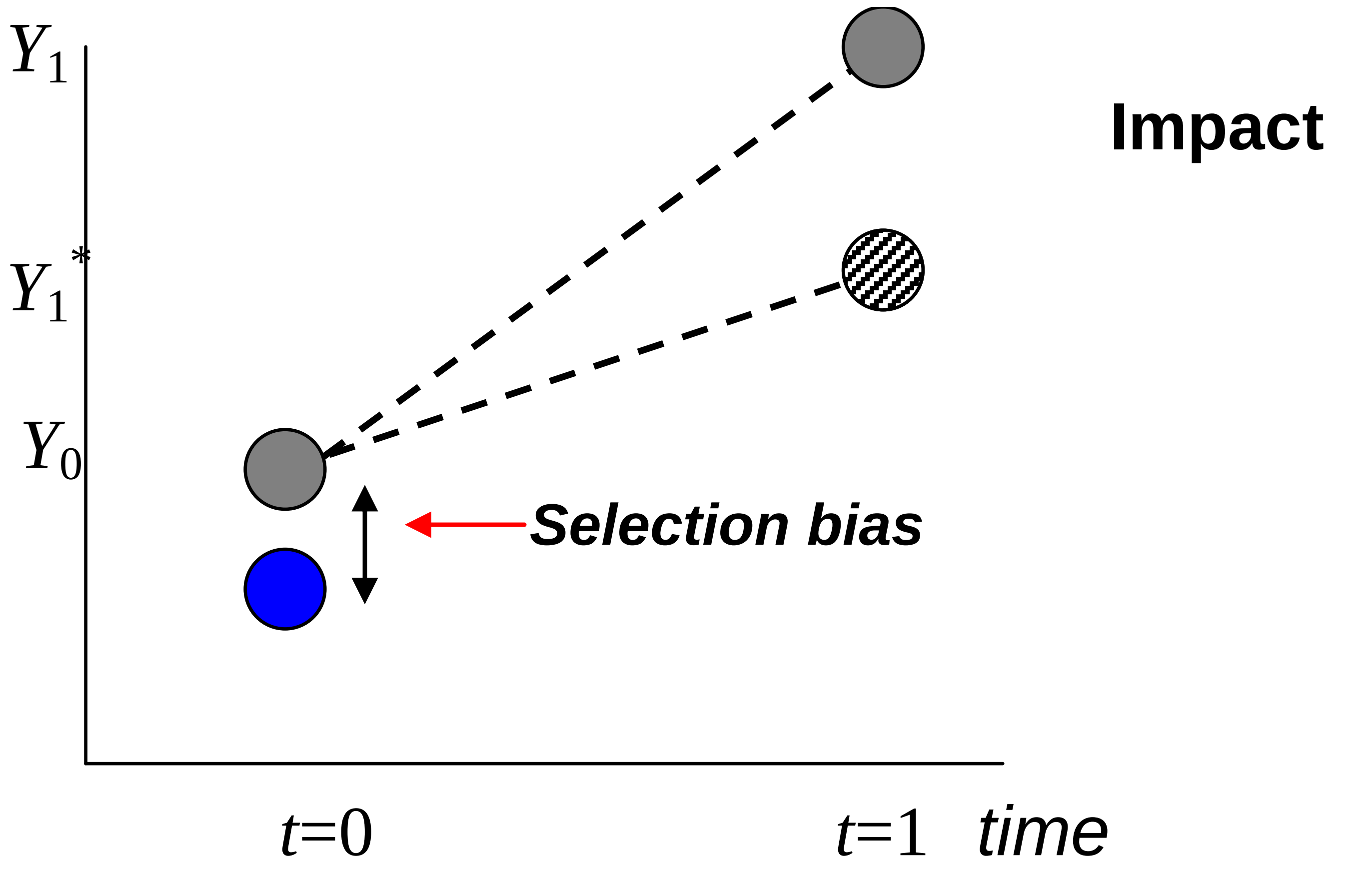
$$\left(Y^T - Y^C\right)_{t=1} - \left(Y^T - Y^C\right)_{t=0} = \left(Y^T_{t=1} - Y^T_{t=0}\right) - \left(Y^C_{t=1} - Y^C_{t=0}\right) = \text{Measured Impact} - \text{Trend}$$

Difference-in-difference (DiD): Interpretation 2

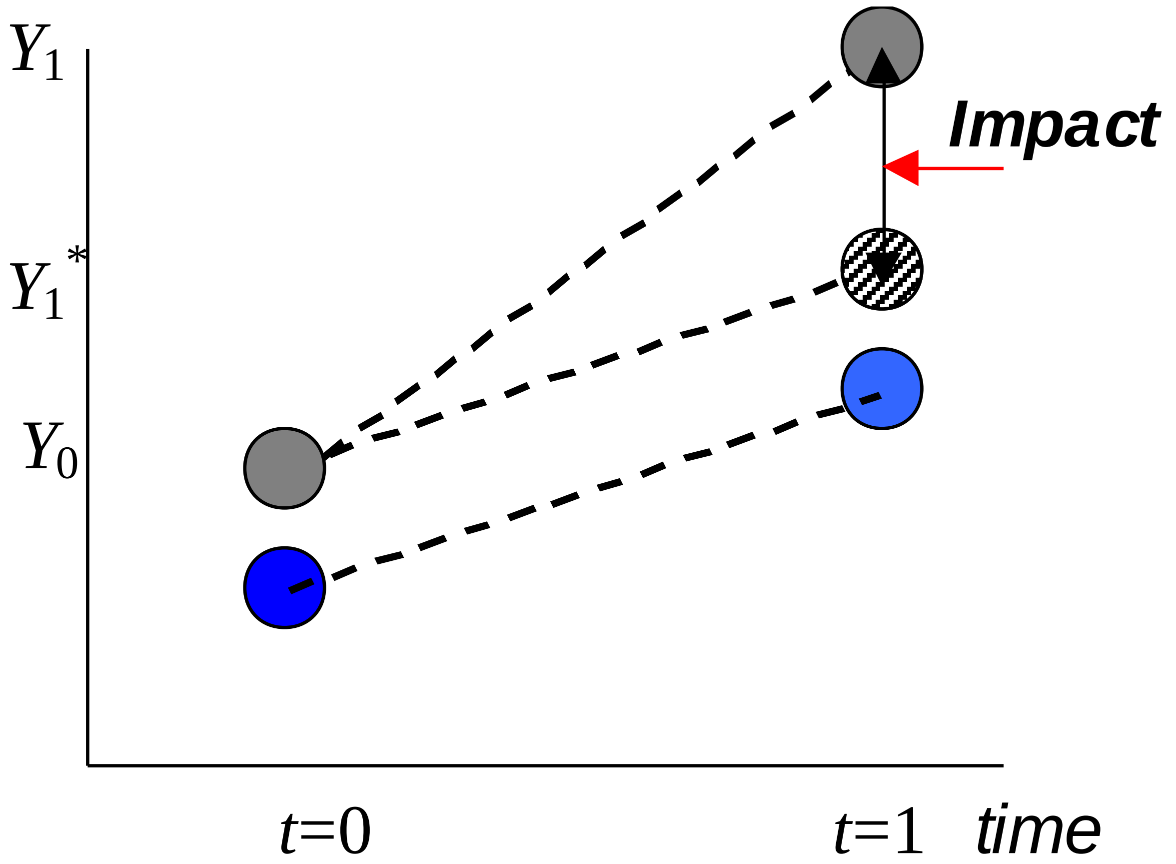
- Dif-in-Dif estimator eliminates selection bias under the assumption that selection bias enters additively and does not change over time

$\left(Y^T - Y^C\right)_{t=1} - \left(Y^T - Y^C\right)_{t=0}$ = True impact - $\left(MSB_{t=1} - MSB_{t=0}\right)$. The latter term drops out if $MSB_{t=1} = MSB_{t=0}$, i.e. MSB is time invariant

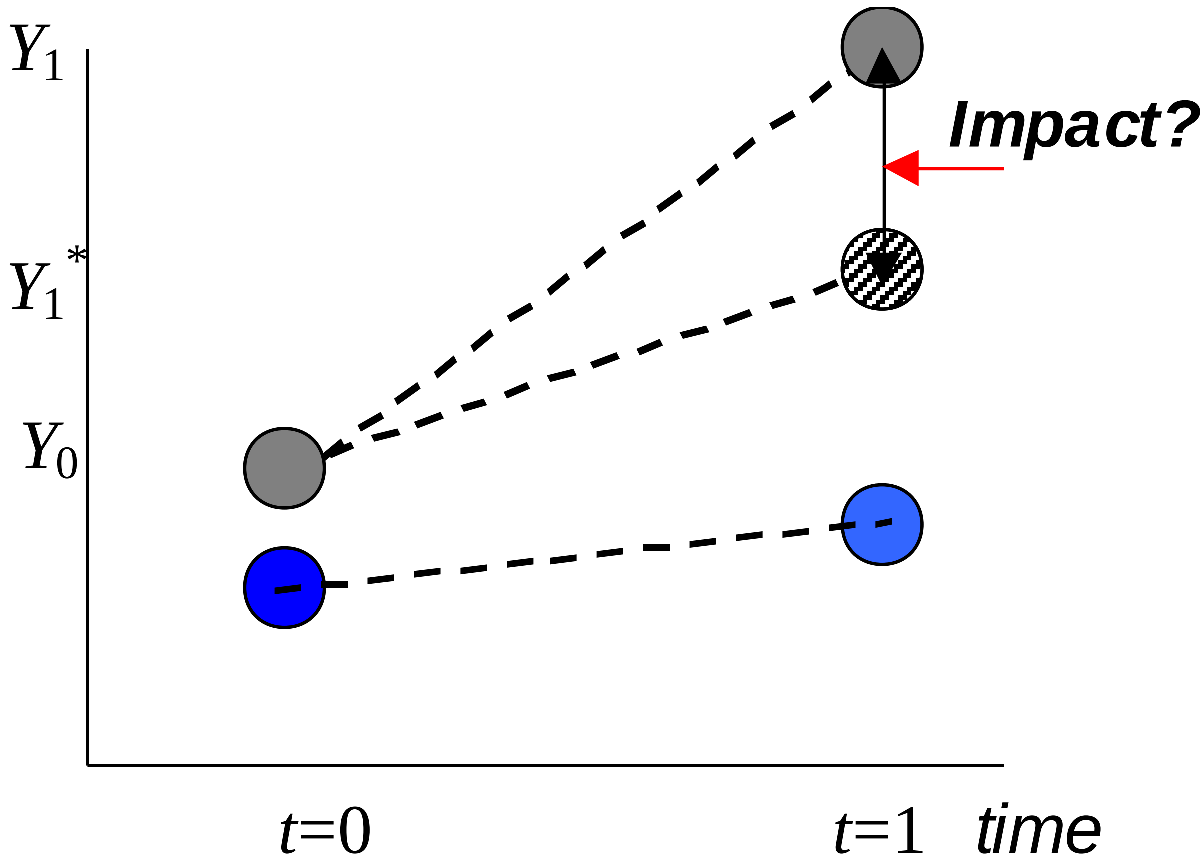
Selection bias



Diff-in-diff requires that the bias is <u>additive</u> and <u>time-invariant</u>



The method fails if the comparison group is on a different trajectory



3. Experimental Designs

The experimental/randomized design

- In a randomized design the control group (randomly assigned out of the program) provides the counterfactual (what would have happened to the treatment group without the program)
- Can apply CSDIFF estimator (ex-post observations only)
- Or DiD (if have data in baseline and after start of program)
- *Randomization equalizes the mean selection bias between T and C groups*
- Note: An Experimental or Randomized Design
  - Assigns individuals into T=1 and T=0 groups randomly.
  - Consequence: MA(T=1) = MA(T=0) → MSB=0 and
  - Measured Impact = True Impact

Lessons from practice--1

Ethical objections and political sensitivities

- Deliberately denying a program to those who need it and providing the program to some who do not.
  - Yes, too few resources to go around. But is randomization the fairest solution to limited resources?
  - What does one condition on in conditional randomizations?
- Intention-to-treat helps alleviate these concerns
- => randomize assignment, but free to not participate
- But even then, the "randomized out" group may include people in great need.

=> Implications for design

- Choice of conditioning variables.
- Sub-optimal timing of randomization
- Selective attrition + higher costs

Lessons from practice--2

Internal validity: Selective compliance

- Some of those assigned the program choose not to participate.
- Impacts may only appear if one corrects for selective take-up.
- Randomized assignment as IV for participation
- *Proempleo example*: impacts of training only appear if one corrects for selective take-up

Lessons from practice--3

External validity: inference for scaling up

- Systematic differences between characteristics of people normally attracted to a program and those randomly assigned (“randomization bias”: Heckman-Smith)
- One ends up evaluating a different program to the one actually implemented

=> Difficult in extrapolating results from a pilot experiment to the whole population

PROGRESA/Oportunidades

- What is PROGRESA?
  - Targeted cash transfer program conditioned on families visiting health centers regularly and on children attending school regularly.
  - Cash transfer-alleviates short-term poverty
  - Human capital investment-alleviates poverty in the long-term
  - By the end of 2004: program (renamed *Oportunidades*) covered nearly 5 million families, in 72,000 localities in all 31 states (budget of about US$2.5 billion).

CCT programs (like PROGRESA) Expanding

- Brazil: Bolsa Familia
- Colombia: Familias en Acción
- Honduras: Programa de Asignación Familiar (PRAF)
- Jamaica: Program of Advancement through Health and Education (PATH)
- Nicaragua: Red de Protección Social (RPS)
- Turkey
- Ecuador: Bono Solidario
- Philippines,
- Indonesia,
- Peru,
- Bangladesh: Food for Education

Program Description & Benefits

- Education component
  - A system of educational grants (details below)
  - Monetary support or the acquisition of school materials/supplies

(The above benefits are tied to enrollment and regular (85%) school attendance)

  - Improved schools and quality of educations (teacher salaries)

PROGRESA/OPORTUNIDADES: Evaluation Design

- EXPERIMENTAL DESIGN: Program randomized at the locality level (Pipeline experimental design)
- IFPRI not present at time of selection of T and C localities
- Report examined differences between T and C for more than 650 variables at the locality level (comparison of locality means) and at the household level (comparison of household means)
- Sample of 506 localities
  - 186 control (no program)
  - 320 treatment (receive program)
- 24, 077 Households (hh)
  - 78% beneficiaries
  - Differences between eligible hh and actual beneficiaries receiving benefits
  - Densification (initially 52% of hh classified as eligible)

PROGRESA Evaluation Surveys/Data

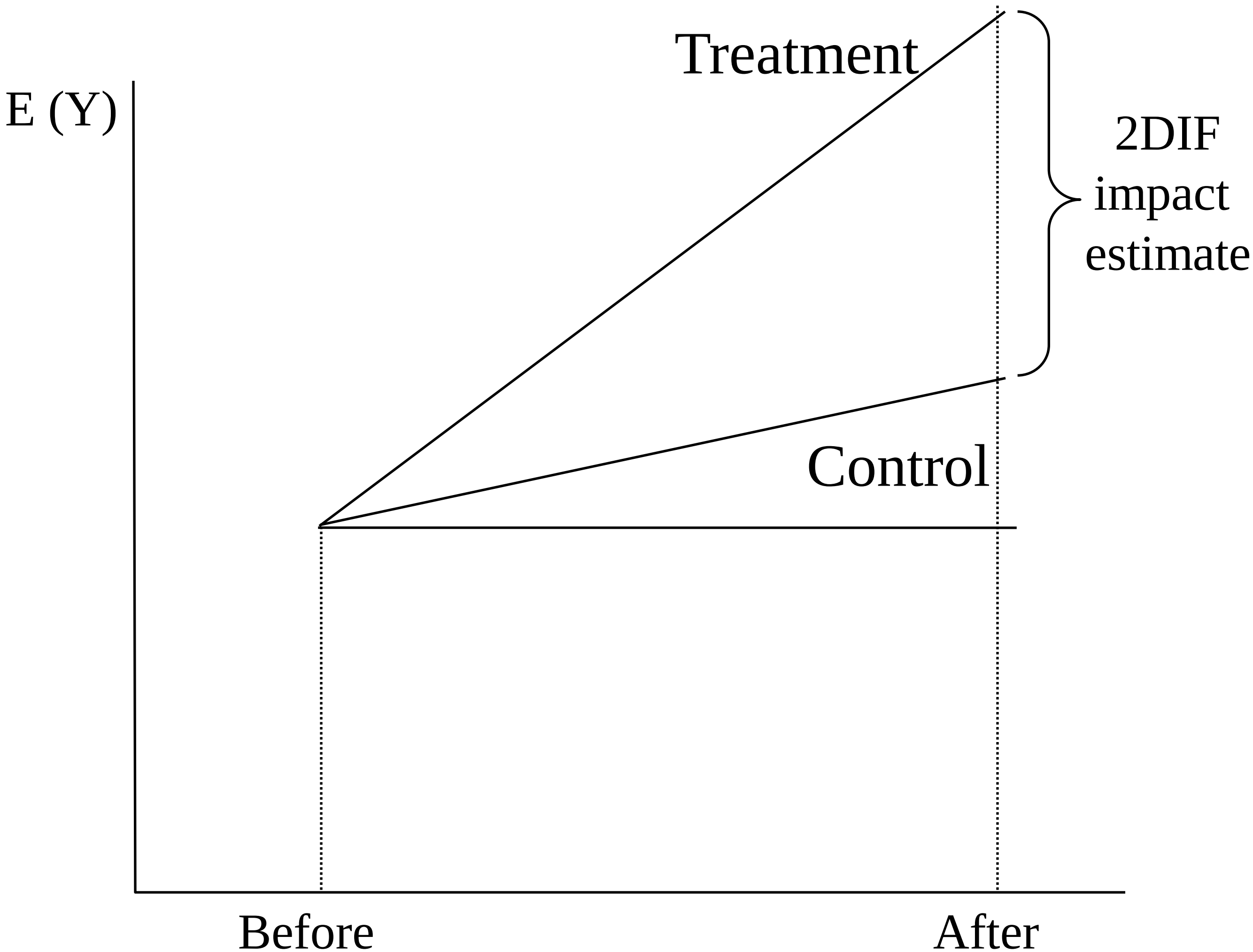
- BEFORE initiation of program:
  - Oct/Nov 97: Household census to select beneficiaries
  - March 98: consumption, school attendance, health

- AFTER initiation of program
  - Nov 98
  - June 99
  - Nov/Dec 99

  Included survey of beneficiary households regarding operations

Table: A Decomposition of the Sample of All Households in Treatment and Control Villages

Household Eligibility Status	Discriminant Score (‘*puntaje*’)	Localities: 320 Households:14,856 TREATMENT LOCALITY where PROGRESA is in operation (*T*=1)	Localities: 186 Households: 9,221 CONTROL LOCALITY where PROGRESA operations are delayed (*T*=0)
Eligible for PROGRESA benefits (*B=1*)	Low Below Threshold	**A** B=1, T=1	**B** *B=1, T=0*
Non-Eligible for PROGRESA benefits (*B=0*)	Above Threshold High	**C** B=0, T=1	**D** *B=0, T=0*


Treatment
E (Y)
2DIF
impact
estimate
Control
Before
After

Using regressions to get 2DIF estimates:

Limit sample to eligible households in treatment and control and run regression:

$$Y(i,t) = \alpha \; + \beta_T T(i) + \beta_R R2 + \beta_{TR} (T(i) * R2) + \sum_j \theta_j \; X_j + \eta \; (i,v,t)$$

- Y(i,t) denotes the value of the outcome indicator in household (or individual) i in period t,
- alpha, beta and theta are fixed parameters to be estimated,
- T(i) is an binary variable taking the value of 1 if the household belongs in a treatment community and 0 otherwise (i.e., for control communities),
- R2 is a binary variable equal to 1 for the second round of the panel (or the round after the initiation of the program) and equal to 0 for the first round (the round before the initiation of the program),
- X is a vector of household (and possibly village) characteristics;
- last term is an error term summarizing the influence random disturbances.

$$CSDIF = \left[E\left(Y \mid T=1, R2=1, \mathbf{X}\right) - E\left(Y \mid T=0, R2=1, \mathbf{X}\right)\right] = \beta_T + \beta_{TR}$$

$$BADIF = \left[E\left(Y \mid T=1, R2=1, \mathbf{X}\right) - E\left(Y \mid T=1, R2=0, \mathbf{X}\right)\right] = \beta_R + \beta_{TR}$$

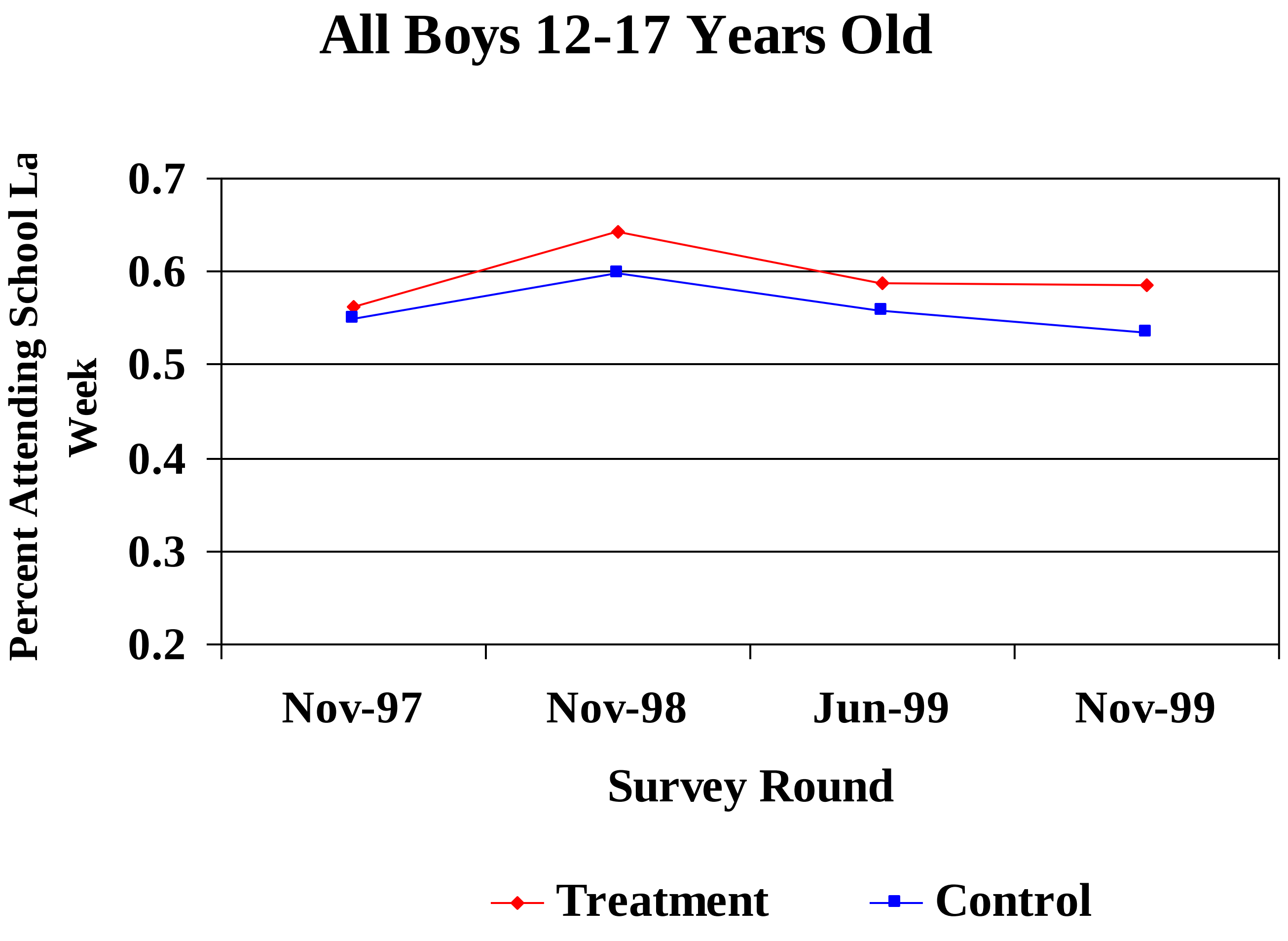
$$2\,DIF = \beta_{TR} =$$

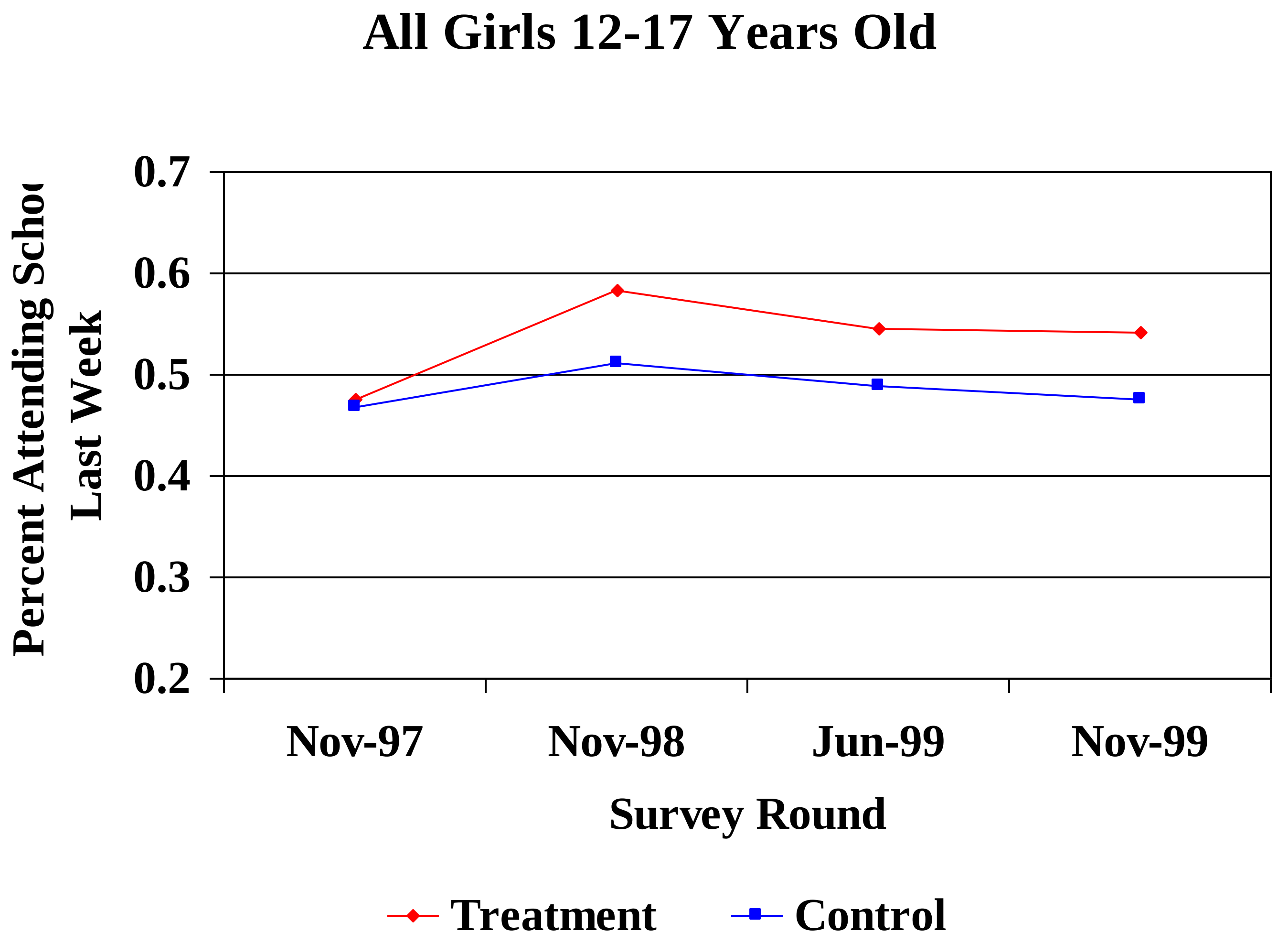
$$\left[E\left(Y \mid T=1, R2=1, \mathbf{X}\right) - E\left(Y \mid T=1, R2=0, \mathbf{X}\right)\right] -$$

$$\left[E\left(Y \mid T=0, R2=1, \mathbf{X}\right) - E\left(Y \mid T=0, R2=0, \mathbf{X}\right)\right]$$

Evaluation Tools

- Formal surveys
- (Semi)-structured observations and interviews
- Focus groups with stakeholders (beneficiaries, local leaders, local PROGRESA officials, doctors, nurses, school teachers, promotoras)


All Boys 12-17 Years Old
Percent Attending School La
Week
0.7
0.6
0.5
0.4
0.3
0.2
Nov-97
Nov-98
Jun-99
Nov-99
Survey Round
Treatment
Control



All Girls 12-17 Years Old
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Percent Attending Scho
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Nov-97
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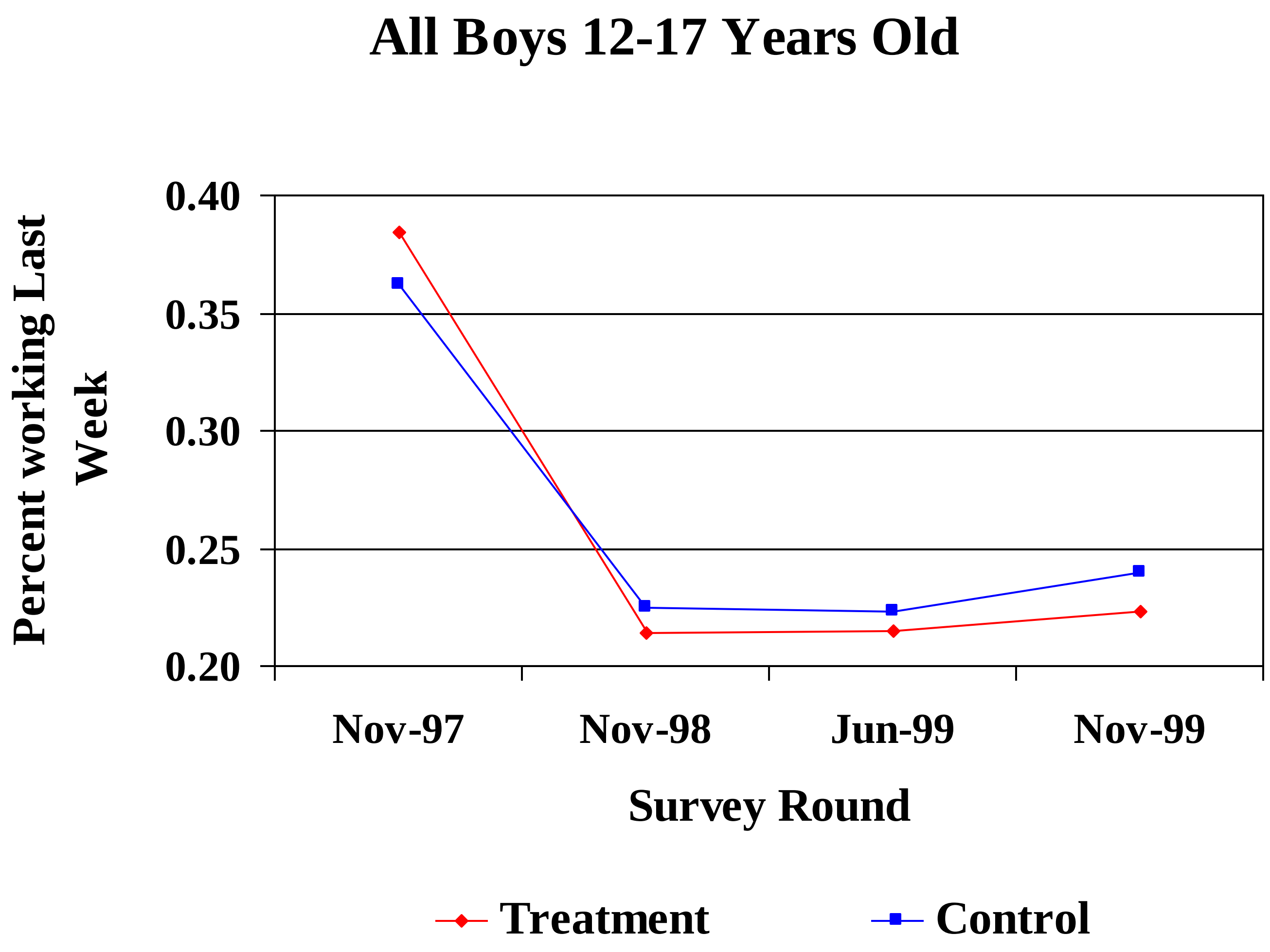
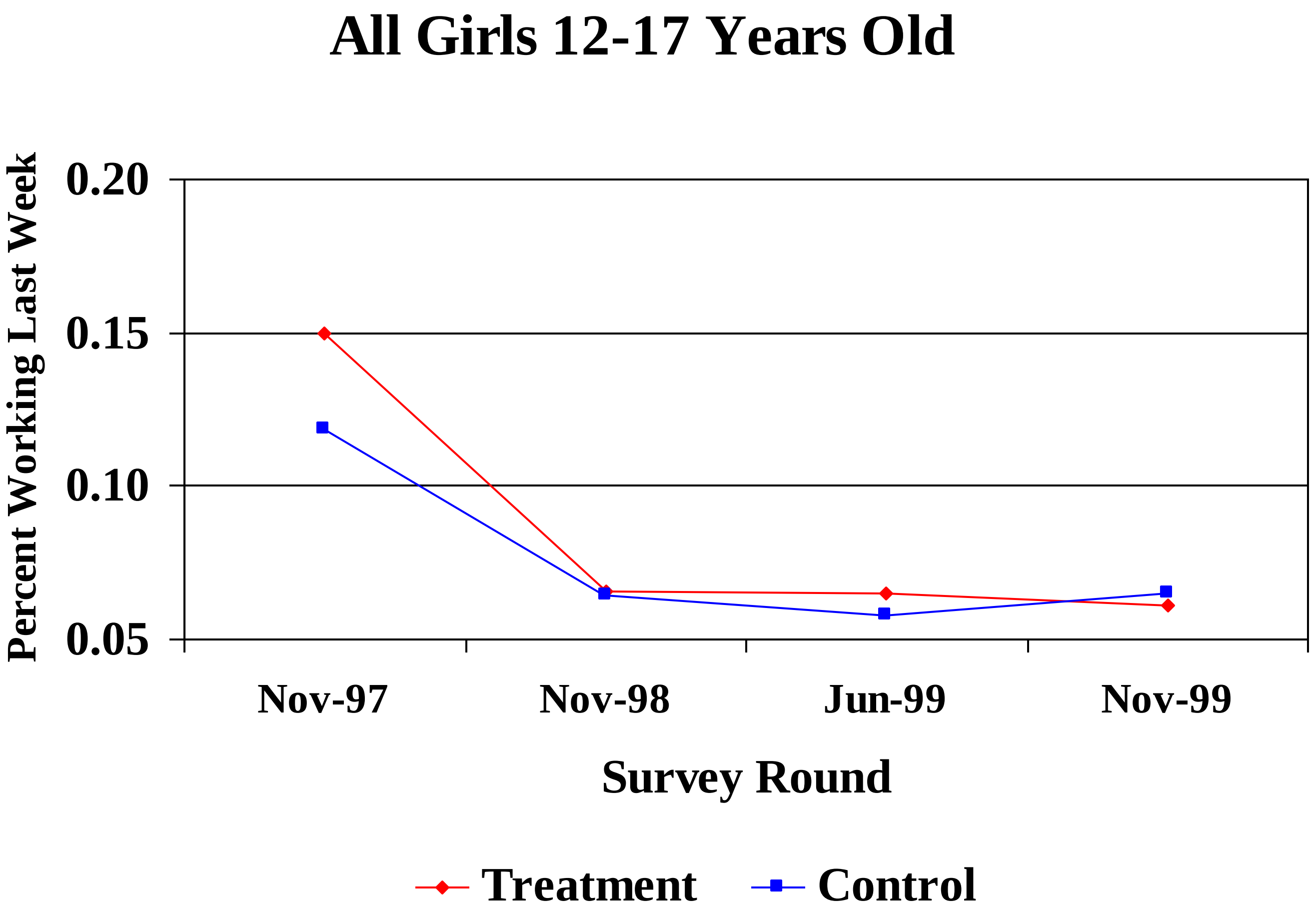

All Boys 12-17 Years Old
Percent working Last Week
0.40
0.35
0.30
0.25
0.20
Nov-97
Nov-98
Jun-99
Nov-99
Survey Round
Treatment
Control



All Girls 12-17 Years Old
Percent Working Last Week
0.20
0.15
0.10
0.05
Nov-97
Nov-98
Jun-99
Nov-99
Survey Round
Treatment
Control

4a. Quasi-Experimental Designs:
Propensity Score Matching-PSM

Introduction

- By consensus, a randomized design provides the most credible method of evaluating program impact.
- But experimental designs are difficult to implement and are accompanied by political risks that jeopardize the chances of implementing them
  - The idea of having a comparison/control group is very unappealing to program managers and governments
  - ethical issues involved in withholding benefits for a certain group of households

Propensity-score matching (PSM)

- Builds on this fundamental idea of the randomized design and uses it to come up a control group (under some maintained/untested assumptions).
- In an experimental design a treatment and a control have equal probability in joining the program. Two people apply and we decide who gets the program by a coin toss, then each person has probability of 50% of joining the program

Propensity-score matching (PSM):

Match on the probability of participation.

- Ideally we would match on the entire vector $\boldsymbol{X}$ of observed characteristics. However, this is practically impossible. $\boldsymbol{X}$ could be huge.
- PSM: match on the basis of the ***propensity score*** **(**Rosenbaum and Rubin) =

$$P(X_i) = \Pr(D_i = 1 | X_i)$$

- This assumes that participation is independent of outcomes given $\boldsymbol{X}$**.** If no bias given $\boldsymbol{X}$ then no bias given $\boldsymbol{P(X)}$**.**

Steps in score matching:

1: Representative, highly comparable, surveys of the non-participants and participants.

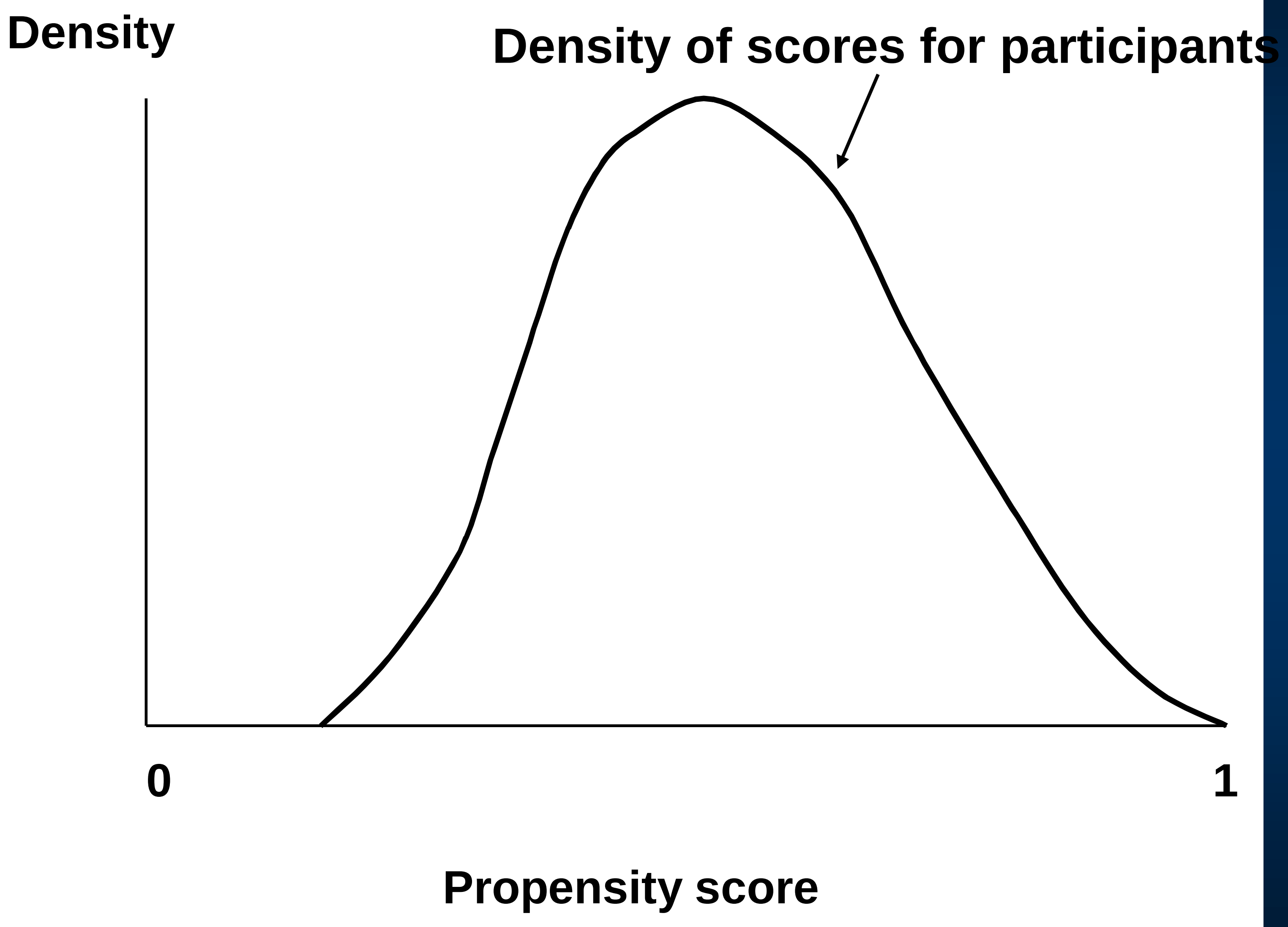
2: Pool the two samples and estimate a logit (or probit) model of program participation. Predicted values are the "propensity scores".

3: Restrict samples to assure <u>common support</u>

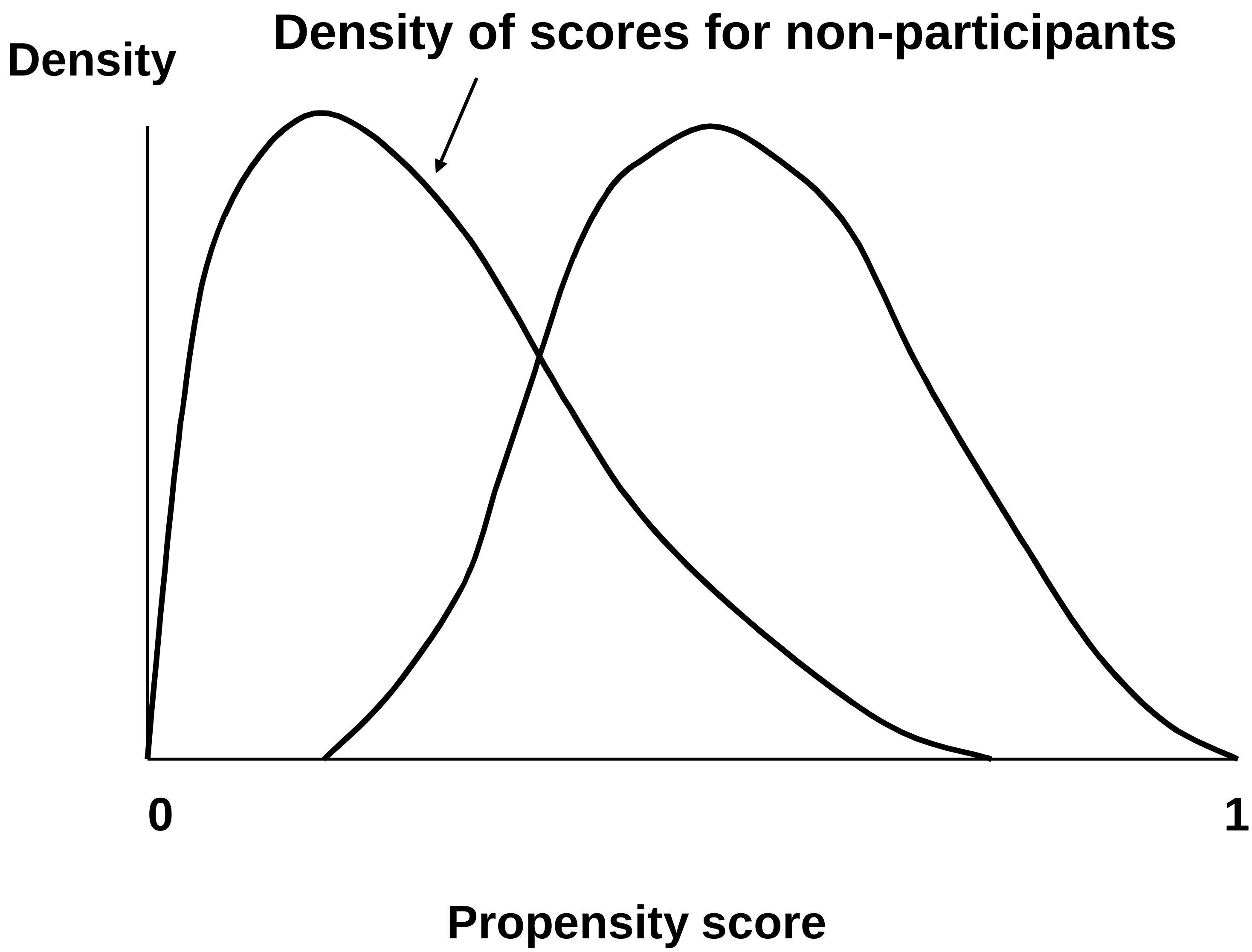
Failure of common support is an important source of bias in observational studies (Heckman et al.)

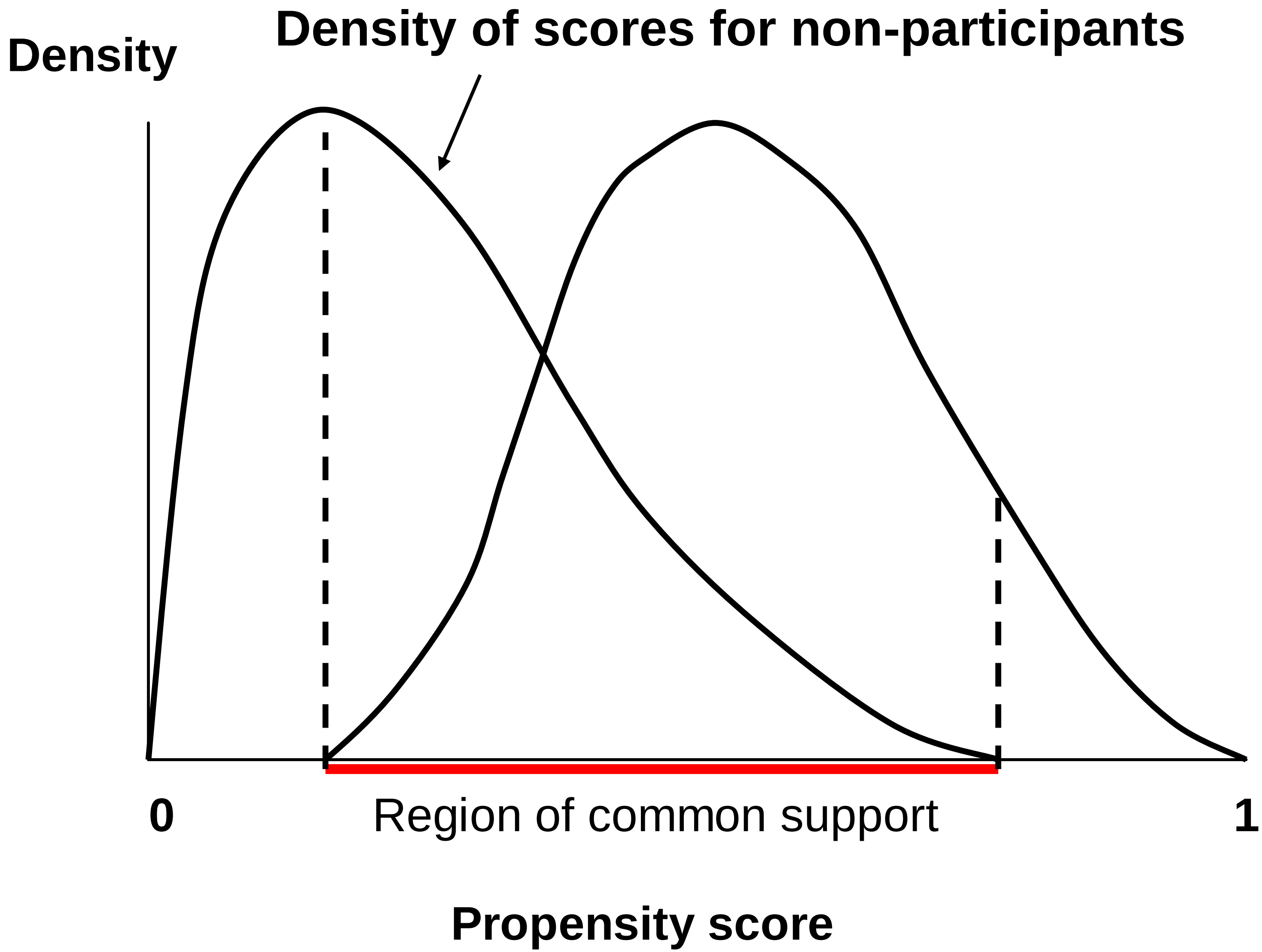
Propensity-score matching (PSM)

- You choose a control group by running a logit/probit where on the LHS you have a binary variable =1 if a person is in the program, 0 otherwise, as a function of observed characteristics.
- Based on this logit/probit, one can derive the predicted probability of participating into the program (based on the X or observed characteristics) and you choose a control group for each treatment individual/hh using hh that are NOT in the program and have a predicted probability of being in the program very close to that of the person who is in the program (nearest neighbor matching, kernel matching etc).
- Key assumption: selection in to the program is based on observables (or in other words unobservables are not important in determining participation into the program).



Density
Density of scores for participants
0
1
Propensity score


Density of scores for non-participants
Density
0
1
Propensity score


Density of scores for non-participants
Density
0
Region of common support
1
Propensity score

Steps in PSM cont.,

5: For each participant find a sample of non-participants that have similar propensity scores.

6: Compare the outcome indicators. The difference is the estimate of the gain due to the program for that observation.

7: Calculate the mean of these individual gains to obtain the average overall gain. Various weighting schemes =>

The mean impact estimator

$$\overline{G} = \sum_{j=1}^{P} (Y_{j1} - \sum_{i=1}^{NP} W_{ij} Y_{ij0}) / P$$

Various weighting schemes:

- Nearest k neighbors
- Kernel-weights (Heckman et al.,):

$$K_{ij} = K[P(X_i) - P(X_j)]$$

$$W_{ij} = K_{ij} / \sum_{j=1}^{P} K_{ij}$$

Propensity-score weighting

- PSM removes bias under the conditional exogeneity assumption.
- However, it is not the most efficient estimator.
- Hirano, Imbens and Ridder show that weighting the control observations according to their propensity score yields a fully efficient estimator.
- Regression implementation for the common impact model:

$$Y_i = \beta D_i + \varepsilon_i$$

with weights of unity for the treated units and $\hat{P}(X)/(1-\hat{P}(X))$ for the controls.

How does PSM compare to an experiment?

- PSM is the observational analogue of an experiment in which placement is independent of outcomes
- The difference is that a pure experiment does not require the untestable assumption of independence conditional on observables.
- Thus PSM requires good data.
- Example of Argentina's *Trabajar* program
  - Plausible estimates using SD matching on good data
  - Implausible estimates using weaker data

How does PSM differ from OLS?

- PSM is a non-parametric method (fully non-parametric in outcome space; optionally non-parametric in assignment space)

- Restricting the analysis to common support
  => PSM weights the data very differently to standard OLS regression

- In practice, the results can look very different!

How does PSM perform relative to other methods?

- In comparisons with results of a randomized experiment on a US training program, PSM gave a good approximation (Heckman et al.; Dehejia and Wahba)
- Better than the non-experimental regression-based methods studied by Lalonde for the same program.
- However, robustness has been questioned (Smith and Todd)

Lessons on matching methods

- When neither randomization nor a baseline survey are feasible, careful matching is crucial to control for observable heterogeneity.
- Validity of matching methods depends heavily on <u>data quality</u>. Highly comparable surveys; similar economic environment
- Common support can be a problem (esp., if treatment units are lost).
- Look for <u>heterogeneity in impact</u>; average impact may hide important differences in the characteristics of those who gain or lose from the intervention.

4b. Quasi-Experimental Designs:
Regression Discontinuity Design-RDD

Exploiting program design

Pipeline comparisons

- Applicants who have not yet received program form the comparison group
- Assumes exogeneous assignment amongst applicants
- Reflects latent selection into the program

Lessons from practice

- Know your program well: Program design features can be very useful for identifying impact.
- Know your setting well too: Is it plausible that outcomes are continuous under the counterfactual?
- But what if you end up changing the program to identify impact? You have evaluated something else!

Introduction

- Alternative: Quasi-experimental methods attempting to equalize selection bias between treatment and control groups
- Discuss paper using PROGRESA data (again)
  - one of the first to evaluate the performance of RDD in a setting where it can be compared to experimental estimates.
  - Focus on school attendance and work of 12-16 yr old boys and girls.

Regression Discontinuity Design: RDD

Discontinuity designs

- Participate if score $M < m$
- Impact=

$$E(Y_i^T | M_i = m - \varepsilon) - E(Y_i^C | M_i = m + \varepsilon)$$

- Key identifying assumption: no discontinuity in counterfactual outcomes at m

Indexes are common in targeting of social programs

- Anti-poverty programs → targeted to households below a given poverty index
- Pension programs → targeted to population above a certain age
- Scholarships → targeted to students with high scores on standardized test
- CDD Programs → awarded to NGOs that achieve highest scores
- Others:
  - Credit scores in Bank lending

Advantages of RDD for Evaluation

- RDD yields an unbiased estimate of treatment effect at the discontinuity
- Can many times take advantage of a known rule for assigning the benefit that are common in the designs of social policy
  - No need to “exclude” a group of eligible households/individuals from treatment

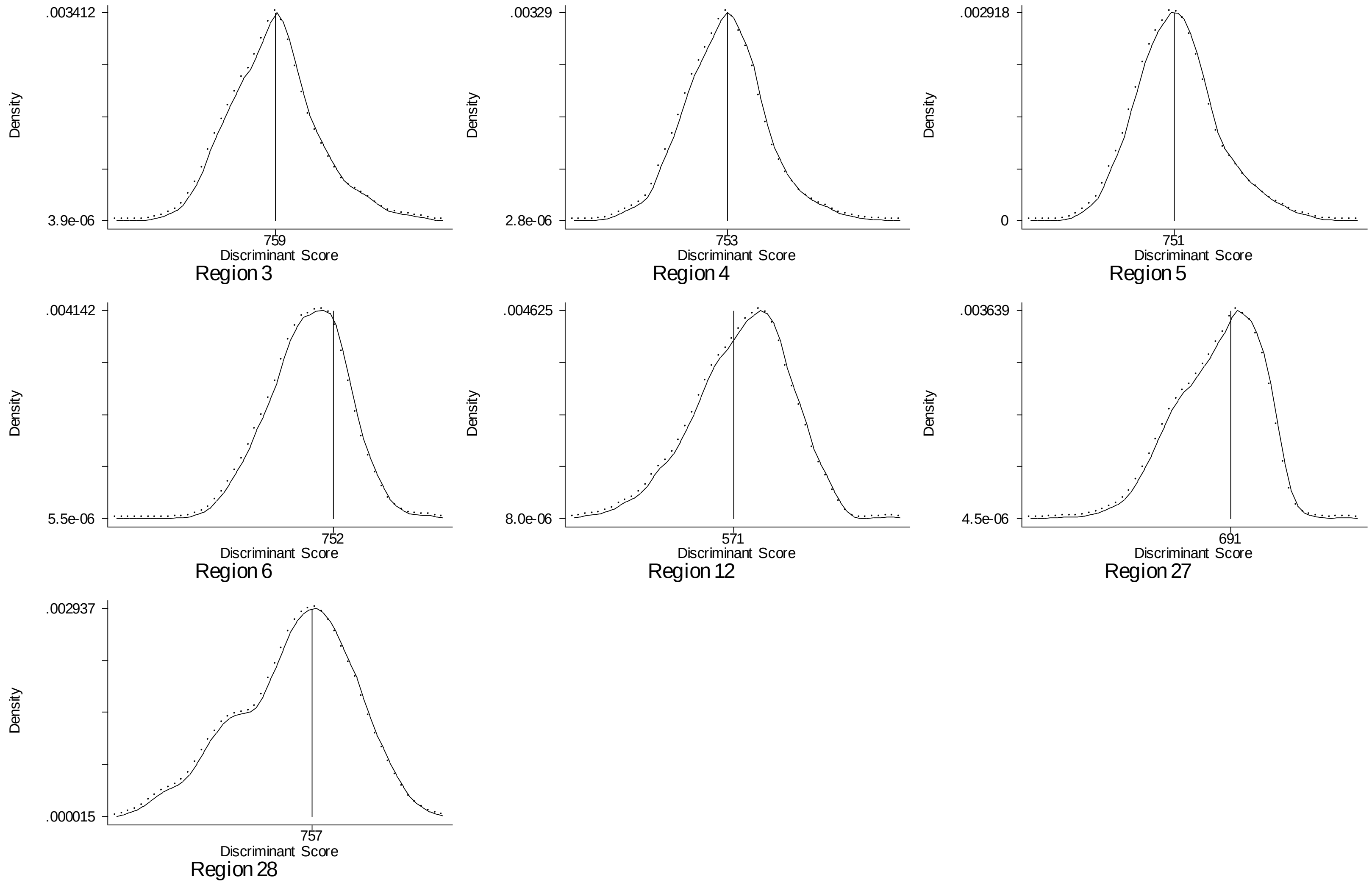
Potential Disadvantages of RD

- Local treatment
  - effects cannot be generalized (especially if there is heterogeneity of impacts)
- Power:
  - effect is estimated at the discontinuity, so we generally have fewer observations than in a randomized experiment with the same sample size
- Specification can be sensitive to functional form: make sure the relationship between the assignment variable and the outcome variable is correctly modeled, including:
  - Nonlinear Relationships
  - Interactions

Some Background on PROGRESA’s targeting

- Two-stage Selection process:
  - Geographic targeting (used census data to identify poor localities)
  - Within Village household-level targeting (village household census)
    - Used hh income, assets, and demographic composition to estimate the probability of being poor (Inc per cap<Standard Food basket).
    - Discriminant analysis applied separately by region
    - Discriminant score of each household compared to a threshold value (high DS=Noneligible, low DS=Eligible)
  - Initially 52% eligible, then revised selection process so that 78% eligible. But many of the “new poor” households did not receive benefits

Figure 1: Kernel Densities of Discriminant Scores and Threshold points by region

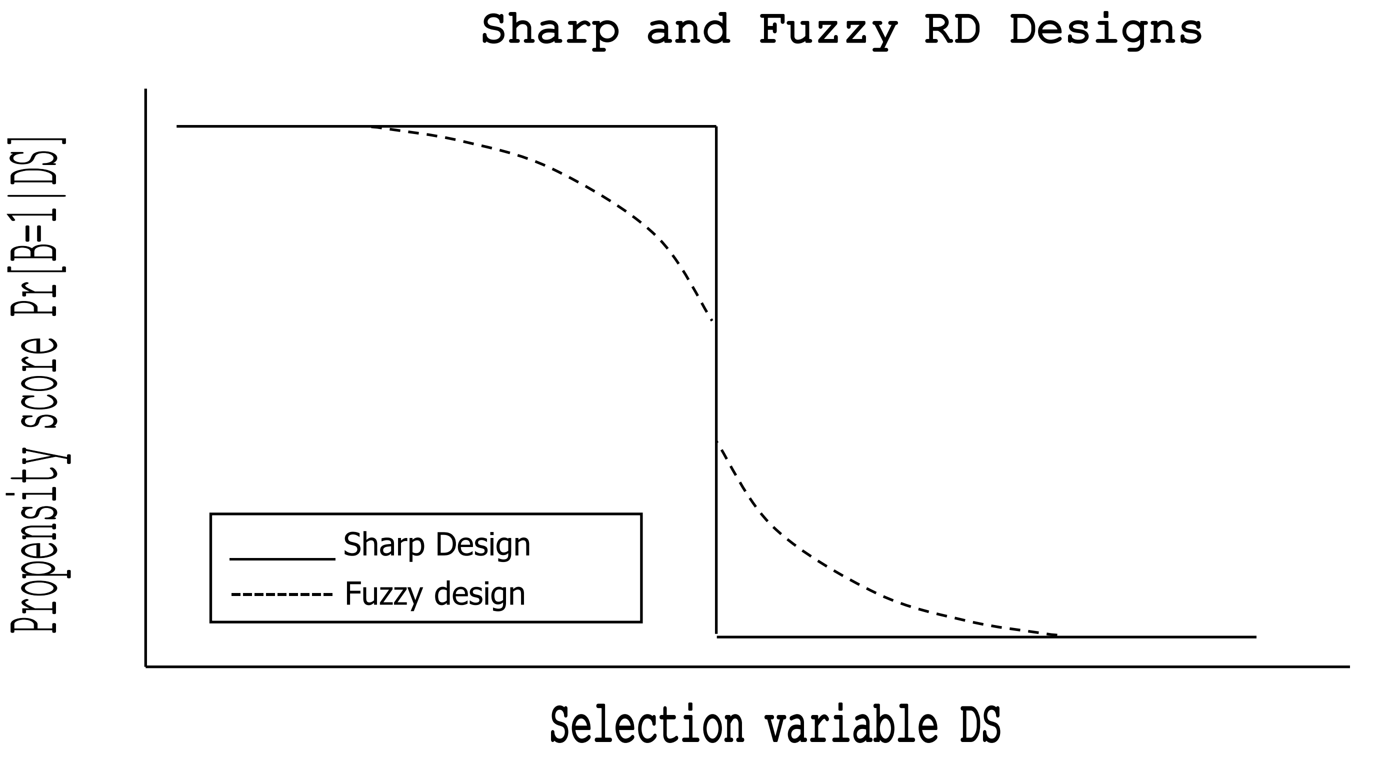


The RDD method-1

- A quasi-experimental approach based on the discontinuity of the treatment assignment mechanism.
- Sharp RD design
  - Individuals/households are assigned to treatment (T) and control (NT) groups based solely on the basis of an observed continuous measure such as the discriminate score DS. For example, *B* =1 if and only if DS<=COS (*B*=1 eligible beneficiary) and *B*=0 otherwise . Propensity is a *step function* that is discontinuous at the point DS=COS.
  - Analogous to selection on observables only.
  - Violates the *strong ignorability* assumption of Rosenbaum and Rubin (1983) which also requires the overlap condition.

The RDD method-2

- Fuzzy RD design
  - Treatment assignment depends on an observed continuous variable such as the discriminant score DS but in a stochastic manner. Propensity score is *S-shaped* and is discontinuous at the point DS=COS.
  - Analogous to selection on observables and unobservables.
  - Allows for imperfect compliance (self-selection, attrition) among eligible beneficiaries and contamination of the comparison group by non-compliance (substitution bias).



Regression Discontinuity Design; treatment assignment in sharp (solid) and fuzzy (dashed) designs.

Kernel Regression Estimator of Treatment Effect with a Sharp RDD

$$\tau(COS) = Y^- - Y^+ = \lim_{DS \uparrow COS} E(Y_i \mid DS_i = COS) - \lim_{DS \downarrow COS} E(Y_i \mid DS_i = COS)$$

where

$$Y^- = \frac{\sum_{i=1}^{n} Y_i * \omega_i * K(u_i)}{\sum_{i=1}^{n} \omega_i * K(u_i)} \quad \text{and} \quad Y^+ = \frac{\sum_{i=1}^{n} Y_i * (1-\omega_i) * K(u_i)}{\sum_{i=1}^{n} (1-\omega_i) * K(u_i)}$$

Alternative estimators (differ in the way local information is exploited and in the set of regularity conditions required to achieve asymptotic properties):

Local Linear Regression (HTV, 2001)
Partially Linear Model (Porter, 2003)

TABLE 3a

Estimates of Program Impact By Round (BOYS 12-16 yrs old)

	Experimental Estimates			**RDD Impact Estimates using different kernel functions**					
	2DIF	**CSDIF**	**CSDIF-50**	**Uniform**	**Biweight**	**Epanechnik**	**Triangular**	**Quartic**	**Guassian**
SCHOOL	**(1)**	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**	**(7)**	**(8)**	**(9)**
Round 1	n.a	0.013	-0.001	-0.053	-0.016	-0.031	-0.018	-0.016	**-0.050**
st. error		0.018	0.028	0.027	0.031	0.029	0.031	0.031	0.021
Round 3	**0.050**	**0.064**	**0.071**	0.020	0.008	0.010	0.008	0.008	0.005
st. error	0.017	0.019	0.028	0.028	0.034	0.031	0.033	0.034	0.022
Round 5	**0.048**	**0.061**	**0.099**	0.052	**0.072**	**0.066**	**0.069**	**0.072**	**0.057**
st. error	0.020	0.019	0.030	0.028	0.032	0.030	0.032	0.032	0.021
Nobs	16331		4279						
R-Squared	0.21		0.25						
WORK									
Round 1	n.a.	0.018	0.007	0.012	-0.016	-0.004	-0.013	-0.016	0.025
st. error		0.019	0.029	0.027	0.032	0.029	0.031	0.032	0.021
Round 3	-0.037	-0.018	-0.007	0.007	-0.004	0.002	0.001	-0.004	0.005
st. error	0.023	0.017	0.029	0.024	0.028	0.026	0.028	0.028	0.019
Round 5	-0.046	-0.028	-0.037	-0.031	-0.029	-0.030	-0.029	-0.029	-0.028
st. error	0.025	0.017	0.025	0.024	0.028	0.026	0.027	0.028	0.019
Nobs	16331		4279						
R-Squared	0.16		0.19						

NOTES:

Estimates in **bold** have t-values >=2

Treatment Group for Experimental & RDD Estimates: Beneficiary Households in Treatment Villages (Group A)

Comparison Group for Experimental Estimates: Eligible Households in Control Villages (Group B)

Comparison Group for RDD Estimates: NonEligible Households in Treatment Villages (Group C)

TABLE 3b

Estimates of Program Impact By Round (GIRLS 12-16 yrs old)

	Experimental Estimates			RDD Impact Estimates using different kernel functions					
	2DIF	**CSDIF**	**CSDIF-50**	**Uniform**	**Biweight**	**Epanechnik.**	**Triangular**	**Quartic**	**Guassian**
SCHOOL	**(1)**	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**	**(7)**	**(8)**	**(9)**
Round 1	n.a.	-0.001	0.000	-0.027	-0.025	-0.026	-0.025	-0.025	-0.035
st. error		0.020	0.030	0.029	0.036	0.033	0.034	0.036	0.023
Round 3	**0.086**	**0.085**	**0.082**	0.038	0.039	0.041	0.039	0.039	**0.054**
st. error	0.017	0.020	0.029	0.030	0.036	0.033	0.034	0.036	0.024
Round 5	**0.099**	**0.098**	**0.099**	**0.078**	**0.114**	**0.097**	**0.107**	**0.114**	**0.084**
st. error	0.020	0.019	0.028	0.031	0.036	0.033	0.035	0.036	0.025
Nobs	15046		3865						
R-Squared	0.22		0.23						
WORK									
Round 1	n.a.	**0.034**	0.000	0.033	0.026	0.027	0.027	0.026	**0.030**
st. error		0.017	0.024	0.019	0.022	0.020	0.021	0.022	0.015
Round 3	**-0.034**	0.000	0.001	0.005	0.001	0.003	0.002	0.001	-0.008
st. error	0.017	0.009	0.016	0.015	0.018	0.016	0.017	0.018	0.012
Round 5	**-0.042**	-0.008	-0.025	-0.019	-0.034	-0.029	-0.033	-0.034	-0.025
st. error	0.019	0.009	0.018	0.015	0.018	0.017	0.018	0.018	0.013
Nobs	15046		3865						
R-Squared	0.05		0.07						

NOTES:
Estimates in **bold** have t-values >=2
Treatment Group for Experimental & RDD Estimates: Beneficiary Households in Treatment Villages (Group A)
Comparison Group for Experimental Estimates: Eligible Households in Control Villages (Group B)
Comparison Group for RDD Estimates: NonEligible Households in Treatment Villages (Group C)

Main Results

- Overall the performance of the RDD is remarkably good.
  - The RDD estimates of program impact agree with the experimental estimates in 10 out of the 12 possible cases.
  - The two cases in which the RDD method failed to reveal any significant program impact on the school attendance of boys and girls are in the first year of the program (round 3).

5. Instrumental variables/Encouragement Designs

5. Instrumental variables

Identifying exogenous variation using a 3rd variable

Outcome regression:

$$Y_i = \beta D_i + \varepsilon_i$$

(D = 0,1 is our program – not random)

- "Instrument" (Z) influences participation, but does not affect outcomes given participation (the "exclusion restriction").
- This identifies the exogenous variation in outcomes due to the program.

Treatment regression:

$$D_i = \gamma Z_i + u_i$$

Reduced-form outcome regression:

$$Y_i = \beta(\gamma Z_i + u_i) + \varepsilon_i = \pi Z_i + \nu_i$$

where $\pi = \beta\gamma$ and $\nu_i = \beta u_i + \varepsilon_i$

<u>Instrumental variables</u> (two-stage least squares) estimator of impact:

$$\hat{\beta}_{IVE} = \hat{\pi}_{OLS} / \hat{\gamma}_{OLS}$$

$$Y_i = \beta(\hat{\gamma} Z_i) + \nu_i$$

Predicted D purged of endogenous part.

Problems with IVE

1. Finding valid IVs;

- Usually easy to find a variable that is correlated with treatment.
- **However, the validity of the exclusion restrictions is often questionable.**

2. Impact heterogeneity due to latent factors

Sources of instrumental variables

- Partially randomized designs as a source of IVs
- Non-experimental sources of IVs
  - Geography of program placement (Attanasio and Vera-Hernandez); “Dams” example (Duflo and Pande)
  - Political characteristics (Besley and Case; Paxson and Schady)
  - Discontinuities in survey design

Endogenous compliance: Instrumental variables estimator

D =1 if treated, 0 if control

Z =1 if assigned to treatment, 0 if not.

$$D_i = Z_i\pi_1 + \eta_{1i}$$ ***Compliance regression***

$$Y_i = Z_i\pi_2 + \eta_{2i}$$ ***Outcome regression***
("intention to treat effect")

2SLS estimator (=ITT deflated by compliance rate)

$$\frac{\hat{\pi}_2}{\hat{\pi}_1}$$

Essential heterogeneity and IVE

- Common-impact specification is not harmless.
- Heterogeneity in impact can arise from differences between treated units and the counterfactual in latent factors relevant to outcomes.
- For consistent estimation of ATE we must assume that selection into the program is unaffected by latent, idiosyncratic, factors determining the impact (Heckman et al).
- However, likely "winners" will no doubt be attracted to a program, or be favored by the implementing agency.
- => IVE is biased even with "ideal" IVs.

Stylized example

- Two types of people (1/2 of each):
  - Type H: High impact; large gains (G) from program
  - Type L: Low impact: no gain
- Evaluator cannot tell which is which
- But the people themselves can tell (or have a useful clue)

- Randomized pilot:
  - Half goes to each type
  - Impact=G/2

- Scaled up program:
  - Type H select into program; Type L do not
  - Impact=G

IVE is only a 'local' effect

- IVE identifies the effect for those induced to switch by the instrument ("local average effect")
- Suppose Z takes 2 values. Then the effect of the program is:

$$\beta_{IVE} = \frac{E(Y \mid Z=1) - E(Y \mid Z=0)}{E(D \mid Z=1) - E(D \mid Z=0)}$$

- Care in extrapolating to the whole population when there is latent heterogeneity.

Local instrumental variables

- LIV directly addresses the latent heterogeneity problem.
- The method entails a <u>nonparametric regression</u> of outcomes *Y* on the propensity score.

$$Y_i = f[\hat{P}(Z_i)] + \pi X_i + \varepsilon_i$$

- The slope of the regression function $f'[\hat{P}(Z_i)]$ gives the marginal impact at the data point.
  - This slope is the <u>marginal treatment effect</u> (Björklund and Moffitt),
  - from which any of the standard impact parameters can be calculated (Heckman and Vytlacil).

Lessons from practice

- Partially randomized designs offer great source of IVs.
- The bar has risen in standards for non-experimental IVE
  - Past exclusion restrictions often questionable in developing country settings
  - However, defensible options remain in practice, often motivated by theory and/or other data sources
- Future work is likely to emphasize latent heterogeneity of impacts, esp., using LIV.

6. How to Implement an Impact Evaluation

Timeline

		Timeline							
		T1	T2	T3	T4	T5	T6	T7	T8
Program Implementation	Treatment			Start of intervention					
	Control						Start of intervention		
Impact Evaluation	SURVEYS	Baseline				Follow-up			Follow-up

- Baseline survey must go into field before program implemented
- Exposure period between Treatment and Control areas is subject to political, logistical considerations
- Follow up survey must go into field before program implemented in Control areas
- Additional follow up surveys depend on funding and marginal benefits

Prepare & plan evaluation at same time preparing intervention

- Avoid conflicts with operational needs
- Strengthen intervention design and results framework
- Prospective designs:
  - Key to finding control groups
  - More flexibility before publicly presenting roll-out plan
  - Better able to deal with ethical and stakeholder issues
  - Lower costs of the evaluation

Use Phasing for Control Groups

- Limited budget and logistical ability means almost always phase in program over time
  - Those who go first are treatments
  - Those who go later are controls
- Who goes first in rollout plan?
  - Eligibility Criteria → defines universe
  - Cost minimum efficient scale → defines unit of intervention
  - Transparency & accountability: criteria should be quantitative and public
  - Equity: everyone deserves an equal chance

Monitoring Data can be used for Impact Evaluation

- Program monitoring data usually only collected in areas where active
- Start in control areas at same time as in treatment areas for baseline
- Add outcome indicators into monitoring data collection
- Very cost-effective as little need for additional special surveys

Countries already regularly collect

- Vital statistics
- Electricity, water & sanitation, transport company administration information
- School, health clinic MIS
- Industrial surveys
- Labor force & household budget surveys
- Demographic & Health
- National & local budgetary data

Can these other data be used?

- Critical issues
  - Do they collect outcome indicators
  - Can we identify controls and treatments
    - i.e. link to intervention locations and/or beneficiaries
    - question of identification codes
  - Statistical power: are there sufficient sample sizes in treatment and control areas
  - Are there baseline (pre-intervention data)
  - Are there more than one year prior to test for equality of pre-intervention trends
- True for census data & vital statistics
- Usually not for survey data

Special Surveys

- Where there is no monitoring system in place or available data is incomplete
- Need baseline & follow-up of control & treatments
- May need information that do not want to collect on a regular basis (esp specific outcomes)
- Options
  - Collect baseline as part of program application process
  - If controls never apply, then need special survey

Sample Sizes

- Should be based on power calculations
- Sample sizes needed to statistically distinguished between two means
  - Increases the rarer the outcome (e.g maternal mortality)
  - Increases the larger the standard deviation of the outcome indicator (e.g. test scores)
  - Increases the smaller the desired effect size
- Need more sample for subpopulation analysis (gender, poverty)

Staffing Options

- Contract a single firm
  - Easy one stop shopping & responsibility clear
  - Less flexible & expensive
  - Few firms capable, typically large international firms only ones with all skills
- Split responsibility
  - Contract one for design, questionnaire content, supervision of data collection, and analysis
  - Contract another for data collection
  - More complex but cheaper
  - Can get better mix of skills & use local talent

Staffing

- In-country Lead coordinator
  - Assists in logistical coordination
  - Based in-country to navigate obstacles
  - Can be external consultant, or in-country researcher
  - Must have stake in the successful implementation of field work
- Consultants (International?)
  - Experimental Design and analysis
  - Questionnaire and sample design
- Local research firm
  - Responsible for field work and data entry
  - Local researchers (work with international?)

Budget: How much will you need?

- Single largest component: Data collection
- Cost depends on sample size, interview length & measurement
  - Do you need a household survey?
  - Do you need a institutional survey?
  - What are your sample sizes?
  - What is the geographical distribution of your sample?

Consultants

- Money is well spent on consultants for design, sampling, and analysis
- Are there local researchers?
  - Can they do it themselves?
  - Partner with international experts (OPORTUNIDADES Model)?
  - Save money if can access local consultants
- Long-term need to build local capacity

Monitoring the Intervention

- Supervise the program implementation
  - Evaluation design is based on roll-out plan
  - Ensure program implementation follows roll-out plan
- In order to mitigate problems with program implementation:
  - Maintain dialogue with government
  - Build support for impact evaluation among other donors, key players

Building Support for Impact Evaluation

- Once Evaluation Plan for design and implementation is determined:
- Present plan to government counterparts
- Present plan to key players (donors, NGOs, etc.)
- Present plan to other evaluation experts

Operational messages

- Plan evaluation at same time plan project
- Build an explicit evaluation team
- Influence roll-out to obtain control groups
  - Use quantitative & public allocation criteria
  - Randomization is ethical
- Strengthen monitoring systems to improve IE quality & lower costs
- Sample sizes of surveys drive budget

Thank you