PP5183: 03.
Policy Evaluation in Economic Development

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OUTLINE

1. Policy Evaluation: Treatment and Control

2. Randomized Controlled Trials

3. Shortcomings

4. Conclusion

Outline

How should a country take off?

Imagine 1978. Many possible reforms. But among them, liberalise China's agriculture:

- Starvation or surplus (for manufacturing-led development)?

Treatment	Sample Size	Avg. Health	Std. Error
Hospital	7774	2.8	0.014
No Hospital	90049	2.1	0.003

(From Angrist and Pischke)

Policy evaluation in economic development

- Common sense: Food aid. Microfinance. Schools. Textbooks. Vaccinations and pills (worms); mosquite nets; China farmers
- Immediately valuable. Urgent. Good
- But does a particular proposal work? Is it the best way to achieve the stated goals—advance development? Reduce poverty?

Treatment / Non-treatment (control)

- Textbooks in Kenya schools (no effect)
- De-worming pills in Kenya (massive returns—on salaries, occupation levels, lifetime trajectories); cheaper and more effective than textbooks
- Subsidise food in Udaipur (malnourished and remained so; extra resources spent instead on alcohol, tobacco, entertainment)
- Not just development economics: aging; education; poverty; well-being; health insurance; labour markets
- Bio-statistics, medicine; pharmaceuticals; political science; social policy

Outline

Formalism

- Performance measure Y — test scores, attendance, income, health, agricultural output
- Hypothesise that for given unit (individual, child, village, agricultural plot):

$$Y = f(X_0, X_1, X_2, \dots) = X_0\beta_0 + \underbrace{X_1\beta_1 + X_2\beta_2 + \dots}_{\text{covariates}} + \epsilon$$

$$X_0 = \begin{cases} 1 & \text{if treatment} \\ 0 & \text{if not (i.e., control)} \end{cases}$$

β_0 (multiplier) effect of interest

Challenges and a Possible Way Forwards

1. Covariates $X_1, X_2, \ldots$, might be long and unwieldy; impossible to measure everything relevant
2. Compare before and after: Provided again we can track covariates $X_1, X_2, \ldots$
3. Compare before and after: Provided covariates $X_1, X_2, \ldots$ haven't changed or haven't had time to change

If covariates don't vary depending on X_0 and we write $Y^{(1)}$ as the outcome when $X_0 = 1$, and similarly $Y^{(0)}$ when $X_0 = 0$, then:

$$Y^{(1)} - Y^{(0)} = \beta_0$$

This is also called the *treatment effect*.

Pursuing That Way Forwards

Building on a treatment effect idea:

- Different units j so there's heterogeneity

$$Y_j^{(1)} - Y_j^{(0)} = \beta_{j,0}$$

  j-th individual treatment effect.

- Averaging

$$ATE = \frac{1}{J}\sum_{j=1}^{J}\left[Y_j^{(1)} - Y_j^{(0)}\right] = \frac{1}{J}\sum_{j=1}^{J}\beta_{j,0}$$

  or the *average treatment effect*.

The *ATE* is the focus of interest.

But how?

- But if $Y^{(1)}$ then never $Y^{(0)}$, and vice versa: One of these is always a never-occurring counterfactual.
- So treatment effects cannot be calculated?

Theorem 0

1. Divide sample into two groups: one receiving treatment; the other not.
2. Calculate average Y in each of the two groups $\overline{Y}^{(1)}$, $\overline{Y}^{(0)}$.
3. Provided covariates do not vary with treatment/control then

$$\overline{Y}^{(1)} - \overline{Y}^{(0)}$$

is an unbiased estimator of the Average Treatment Effect.

Reason is the covariates terms cancel upon subtraction

Why the R in RCT?

The other covariates might depend on the treatment. Then

$$\overline{Y}^{(1)} - \overline{Y}^{(0)} = \frac{1}{J}\sum_{j} \beta_{j,0} + \text{average difference across all the covariate impacts}$$

1. Call the last term *imbalance*.
2. Unobservable factors confounding the average treatment effect.
3. Seek to make imbalance small:
   - 3.1 Controlled experiments: designed to minimize imbalance;
   - 3.2 "Matching": choose observations to minimize imbalance;
   - 3.3 Randomization: randomize across treatment and control to minimize the imbalance in expectation

RCT Theorem

Randomization means assigning individuals randomly to either treatment or control groups.

RCT Theorem

1. As in Theorem 0;
2. As in Theorem 0;
3. However the covariates might vary, under randomization

$$\overline{Y}^{(1)} - \overline{Y}^{(0)}$$

is an unbiased estimator of the Average Treatment Effect.

Who runs RCTs?

- JPAL at MIT
- Yale Innovation for Poverty Action
- Governments in both developing and developed nations; local and national economies

Advantages and Shortcomings

1. Minimalist on assumptions; therefore persuasive
2. Could not have been done; now can be done

1. Specific to average effects; "the mean of the differences is the difference of the means"
2. unbiased isn't always the same as good (MSE, consistency, coins)
3. Will the Average Treatment Effect remain constant across samples? Is the sample representative?

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Subsequently . . .

Deworming targeted 100mn children

- across Africa; Indian states; dozens of other countries;
- US$ hundreds of millions from corporations and philanthropies

But British Medical Journal 2000 literature review on deworming:

- 30 RCTs across 17 countries;
- only modest impact on height and weight; no effect on school attendance or test scores

Similar outcomes in subsequent reviews 2009, 2012.

- Textbooks elsewhere than Kenya have worked;
- Deworming elsewhere have not.

Outline

Challenges

1. External validity: Reliability outside specific sample setting
2. Statistical: Randomization adds noise and lowers precision
3. Subtlety in presentation: imbalance small *in expectation*
4. In social science applications, behaviour changes if subjects know they receive treatment. Blinding not always possible

Outline

Concepts to remember and use

1. RCT. Successes. Justification
2. Why RCTs work. And why they don't
3. Major shortcomings