

EEP/IAS 118 - Introductory Applied Econometrics, Section 10

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Today

- 1 Intro to impact evaluation
- 2 Potential outcomes framework
- 3 Randomized Controlled Trials (RCTs)

Impact Evaluation: Intro

What econometricians care about is **causal relationships**

- Concerns about omitted variable bias (and other problems) have prevented us from making credible causal claims so far
- We would like to observe an individual's outcomes under a given policy and in a world where the policy did not occur. But this is impossible because we can only observe one of those outcomes!
 - Cannot just compare people affected by a policy because those affected may be different from those not affected → OVB!
- Key issue in measuring impact: establishing a **counterfactual** against which the changes in outcomes for one group induced by an intervention/policy can be measured, and convincing readers that the counterfactual measures what would have happened to the beneficiaries in the absence of the intervention.

Common Challenges to Establishing Causality

- **Reverse Causality** ($Y \Rightarrow X$) means that the outcome variable (Y) actually affects the realization of X .
- **Simultaneity** ($Y \Rightarrow X$ and $X \Rightarrow Y$) means that the outcome variable causes selection into the treatment and control, but treatment status also has effects on the outcome.
- **Omitted Variables Bias** ($Z \Rightarrow X$ and $Z \Rightarrow Y$) means there is some unobserved variable Z that is correlated with both selection into treatment status and the outcome.
- All will cause bias if we simply regress Y on X to try and estimate the impact of X on Y

Potential Outcomes Framework

- We represent a program/policy/intervention with a dummy variable T that is 1 for those that receive the **treatment** and 0 for those that do not (**control**).
- Ideal situation would be to observe how the outcome changes when the same units receive both the treatment and the control. We could then estimate the effect of the treatment as $E[Y_i|T_i = 1] - E[Y_i|T_i = 0] \Rightarrow$ compare the difference in *potential outcomes*.
- Unfortunately, we only observe the outcome under treatment for units that are treated, and the outcome under control for units that are not treated.

Potential Outcomes

In the potential outcomes framework, we think of four possible conditions we could hope to observe:

$$E[Y_i^T | T_i = 1] \text{ Outcome under treatment for treated} \quad (1)$$

$$E[Y_i^T | T_i = 0] \text{ Outcome under control for treated} \quad (2)$$

$$E[Y_i^C | T_i = 0] \text{ Outcome under control for control} \quad (3)$$

$$E[Y_i^C | T_i = 1] \text{ Outcome under treatment for control} \quad (4)$$

We can observe (1) and (3) but never (2) or (4), the counterfactuals we would have expected in the treatment and control groups if they had not or had received the treatment, respectively.

Potential Outcomes and Counterfactuals

- To analyze the impact of a program, we want to estimate $E[Y_i^T | T_i = 1] - E[Y_i^T | T_i = 0]$ (or $E[Y_i^C | T_i = 1] - E[Y_i^C | T_i = 0]$)
 - i.e. the difference between the average outcomes of the treatment group *with* the treatment and what they *would have been* without the treatment.
- But simply observing differences in outcomes between people who receive the program ($T = 1$) and those who don't ($T = 0$) gives us $E[Y_i^T | T_i = 1] - E[Y_i^C | T_i = 0]$.
- Adding and subtracting $E[Y_i^T | T = 0]$ and rearranging gives us:

$$\underbrace{(E[Y_i^T | T = 1] - E[Y_i^T | T = 0])}_{\text{True effect}} + \underbrace{(E[Y_i^T | T = 0] - E[Y_i^C | T = 0])}_{\text{Selection bias}}$$

or in other words, the *true* impact of the program plus any latent differences between the two groups

Potential Outcomes and Counterfactuals

$$\underbrace{(E[Y_i^T | T = 1] - E[Y_i^T | T = 0])}_{\text{True effect}} + \underbrace{(E[Y_i^T | T = 0] - E[Y_i^C | T = 0])}_{\text{Selection bias}}$$

- When does this recover the counterfactual we would have hoped to observe?

Potential Outcomes and Counterfactuals

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- When does this recover the counterfactual we would have hoped to observe?
- Only if there are no differences (on average) between the units that did and did not receive the treatment; i.e.

$$E[Y_i^T | T = 0] = E[Y_i^C | T = 0]$$

- In other words, people who get the treatment and people who don't would be statistically identical if it weren't for the treatment.

Potential Outcomes and Counterfactuals

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- When does this recover the counterfactual we would have hoped to observe?
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$$E[Y_i^T|T=0] = E[Y_i^C|T=0]$$

- In other words, people who get the treatment and people who don't would be statistically identical if it weren't for the treatment.
- Think of the selection bias term as the OVB that would violate MLR4. There is something that affects Y that varies with treatment status

Impact Evaluation: What is an RCT?

- One way we can get rid of the selection bias term and estimate $E[Y_i^T|T_i = 1] - E[Y_i^C|T_i = 1]$ is by **randomizing** the treatment. With a large enough randomly assigned sample, we will have $E[Y_i^T|T_i = 0] = E[Y_i^C|T_i = 0]$, recovering the missing counterfactual we would have observed in an ideal world.
- Randomized controlled trials (RCTs) are the closest we can get to an “ideal” world
- To ensure that the “control” individuals are a good counterfactual for “treated” individuals, we can randomly assign some individuals to receive treatment and others not
- If randomization is properly done, the two groups should not be statistically different (in expectation) \rightarrow any difference can be attributed solely to the intervention/treatment, rather than differences between the treatment and control groups.

Impact Evaluation: Measuring causal effect

When treatment is randomized and we have confirmed no statistical difference between treatment and control, we can estimate the causal effect of treatment as:

$$Impact = \bar{Y}_T - \bar{Y}_C$$

In a regression framework, this is:

$$Y_i = a + \beta_1 T_i + u_i$$

Where T_i is an indicator (dummy variable) for treatment and β_1 is the coefficient of interest.

The impact is generally called the average treatment effect, or ATE for short.

Why is β_1 the treatment effect?

Note:

$$\begin{aligned}\bar{Y}_C &= E[Y_i | i \text{ in Control group}] \\ &= E[a + \beta_1 T_i + u_i | i \text{ in Control group}] \\ &= E[a | i \text{ in Control group}] + E[\beta_1 T_i | i \text{ in Control group}] \\ &\quad + E[u_i | i \text{ in Control group}] \\ &= a + 0 + 0 \\ &= a\end{aligned}$$

Why is β_1 the treatment effect?

Note:

$$\begin{aligned}\bar{Y}_T &= E[Y_i | i \text{ in Treatment group}] \\ &= E[a + \beta_1 T_i + u_i | i \text{ in Treatment group}] \\ &= E[a | i \text{ in Treatment group}] + E[\beta_1 T_i | i \text{ in Treatment group}] \\ &\quad + E[u_i | i \text{ in Treatment group}] \\ &= a + \beta_1 + 0 \\ &= a + \beta_1\end{aligned}$$

so

$$\begin{aligned}\bar{Y}_T - \bar{Y}_C &= a + \beta_1 - a \\ &= \beta_1\end{aligned}$$

Impact Evaluation: Key assumption

Key assumption!! If it were not for treatment, the control and treatment populations would be statistically identical, regardless of whether they are assigned to treatment/control:

$$E[Y_i | i \text{ in Treatment group}, T] = E[Y_j | j \text{ in Control group}, T]$$

or

$$E[u_i | T_i = 0] = E[u_i | T_i = 1] = 0$$

This key assumption cannot be empirically tested. Why?

Impact Evaluation: Testing key assumption

- The key assumption in the previous slide cannot be empirically tested because we never observe u_i !
- Instead, we can provide evidence that the assumption is likely to hold by checking that the **observed** characteristics (e.g., age, income, education) between treatment and control are the same on average

$$E[x_i | i \text{ in Treatment group}] = E[x_i | i \text{ in Control group}]$$

In R (for example),

```
t.test(df[df$treatment==0,]$age, df[df$treatment==1,]$age)
```


Impact Evaluation: Adding covariates

- Usually, we add covariates to a regression to prevent/reduce OVB. With proper randomization this is no longer necessary.
- However, adding covariates to the regression can serve two purposes:
 - 1 Verify, as a robustness check, that $\hat{\beta}$ is invariant to the introduction of covariates in the regression
 - 2 Add precision to the estimation regression
- We do not expect $\hat{\beta}$ to change because we do not expect the covariates to be correlated with treatment.

Impact Evaluation: Adding covariates, cont.

Adding covariates adds precision because it reduces our standard errors:

$$se(\hat{\beta}_1) = \frac{\hat{\sigma}}{\sqrt{SST_x(1 - R_j^2)}}$$
$$\hat{\sigma}^2 = \frac{1}{n - k - 1} \sum_i^n \hat{u}_i^2$$

If we include more covariates in our regression, we can reduce \hat{u}_i^2 , i.e. the unexplained variation in Y goes down.

Impact Evaluation: Heterogeneity

We may be interested in testing whether the treatment has a large impact on some groups than others → treatment heterogeneity.

We can test this by interacting these characteristics (e.g., gender, age, socio-economic status, etc.) with the treatment variable.

$$Y_i = a + \beta_1 T_i + \beta_2 x_{2i} + \beta_3 T_i \times x_{2i} + u_i$$

If the variable x_2 represents a dummy for being female for example, then β_3 gives us the differential effect of the treatment for females relatives to males.

Example Problem: Effects of Progresa CCT

- Suppose we want to evaluate the impact of the Progresa Program in Mexico
- This program was a randomly assigned conditional cash transfer: treatment households got money if their children attended school and necessary doctor visits
- Did this program actually increase school attendance among recipient households?
- (Important policy question of whether to impose conditionality on transfer programs)

Example Problem: Treatment Effect

Estimate the impact of the program on school attendance

Call:

```
lm(formula = enroll198 ~ program, data = progres)
```

Residuals:

<Labelled double>: Enrolled in school for academic year 1998

Min	1Q	Median	3Q	Max
-0.85111	0.14889	0.14889	0.22170	0.22170

Labels:

value	label
0	no
1	yes

Coefficients:

	Estimate	Std. Error	t value	Pr(> t)
(Intercept)	0.77830	0.01869	41.638	< 2e-16 ***
program	0.07280	0.02545	2.861	0.00432 **

Signif. codes: 0 '***' 0.001 '**' 0.01 '*' 0.05 '.' 0.1 ' ' 1

Residual standard error: 0.3849 on 919 degrees of freedom

Multiple R-squared: 0.00883, Adjusted R-squared: 0.007751

F-statistic: 8.187 on 1 and 919 DF, p-value: 0.004316

Interpret Results

```
Call:
lm(formula = enroll198 ~ program, data = progres_a)

Coefficients:
              Estimate Std. Error t value Pr(>|t|)
(Intercept)  0.77830     0.01869  41.638 < 2e-16 ***
program      0.07280     0.02545   2.861  0.00432 **
---
Signif. codes:  0 '***' 0.001 '**' 0.01 '*' 0.05 '.' 0.1 ' ' 1

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```

- Being in the Progres_a treatment group is associated with a 0.072808 increase in predicted school enrollment for 12-13 year olds in the year 1998.
- This is a linear probability model.
- The effect of the program is statistically at the 1% level.

Example Problem: Identifying Assumptions

What are the conditions for the validity of the method to measure the causal impact of the program?

$$E[u_i|T_i = 0] = E[u_i|T_i = 1] = 0$$

How can we be confident that this is true?

T-tests between T and C Groups on Observables

```
> t.test(progresa[progresa$program==0,]$male, progresa[progresa$program==1,]$male)
```

Welch Two Sample t-test

```
data: progresa[progresa$program == 0, ]$male and progresa[progresa$program == 1, ]$male  
t = 0.38682, df = 896.31, p-value = 0.699  
alternative hypothesis: true difference in means is not equal to 0  
95 percent confidence interval:  
 -0.05213759  0.07773457  
sample estimates:  
mean of x mean of y  
0.5117925 0.4989940
```

- Here for example, we see that the p-value for a test null of whether the genders of children in both groups are the same on average yields a p-value of 0.699. Hence we fail to reject the null at the 10% level.
- Do this for all the controls that you can, as in Section notes
- Sometimes, researchers will run a joint (F) test of whether all of the means of the observables are the same across groups jointly.

Example Problem: Adding Controls

Add variables that may influence the decision to enroll in school in the regression. Do they actually explain enrollment?

Call:

```
lm(formula = enroll98 ~ program + distsec + exp98 + hhsize +  
    h_edu + age97 + male, data = progresas)
```

Residuals:

```
<Labelled double>: Enrolled in school for academic year 1998  
      Min       1Q   Median       3Q      Max  
-0.97452  0.00730  0.12262  0.22414  0.69605
```

Coefficients:

	Estimate	Std. Error	t value	Pr(> t)
(Intercept)	2.5773471	0.2849907	9.044	< 2e-16 ***
program	0.0740452	0.0243661	3.039	0.00244 **
distsec	-0.0288689	0.0051495	-5.606	2.74e-08 ***
exp98	-0.0004827	0.0001896	-2.546	0.01106 *
hhsize	-0.0049897	0.0058742	-0.849	0.39587
h_edu	0.0123590	0.0052988	2.332	0.01989 *
age97	-0.1483166	0.0242845	-6.107	1.50e-09 ***
male	0.0800760	0.0242560	3.301	0.00100 ***

Signif. codes: 0 '***' 0.001 '**' 0.01 '*' 0.05 '.' 0.1 ' ' 1

Example Problem: Adding Controls

Do the controls affect the estimated coefficient of the program?

Call:

```
lm(formula = enroll98 ~ program + distsec + exp98 + hhsize +  
    h_edu + age97 + male, data = progresas)
```

Residuals:

```
<Labelled double>: Enrolled in school for academic year 1998  
      Min          1Q      Median          3Q          Max  
-0.97452  0.00730  0.12262  0.22414  0.69605
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Coefficients:

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hhsize	-0.0049897	0.0058742	-0.849	0.39587
h_edu	0.0123590	0.0052988	2.332	0.01989 *
age97	-0.1483166	0.0242845	-6.107	1.50e-09 ***
male	0.0800760	0.0242560	3.301	0.00100 ***

Signif. codes: 0 '***' 0.001 '**' 0.01 '*' 0.05 '.' 0.1 ' ' 1

Example Problem: Heterogeneous treatment effects

- Is the impact of the program the same for boys and girls?
- What is our coefficient of interest in the following specification?

$$Y_i = a + \beta_1 \text{program} + \beta_2 \text{girls} + \beta_3 \text{program} \times \text{girls} + u_i$$

- Do we need to do a t-test or F-test here?

Example Problem: Heterogeneous treatment effects

Is the impact of the program the same for boys and girls?

Call:

```
lm(formula = enroll198 ~ program + male + program * male, data = progres)
```

Residuals:

```
<Labelled double>: Enrolled in school for academic year 1998
```

	Min	1Q	Median	3Q	Max
	-0.88710	0.11290	0.18433	0.18474	0.26087

Coefficients:

	Estimate	Std. Error	t value	Pr(> t)
(Intercept)	0.739130	0.026657	27.728	<2e-16 ***
program	0.076131	0.036073	2.110	0.0351 *
male	0.076538	0.037261	2.054	0.0403 *
program:male	-0.004702	0.050717	-0.093	0.9262

Signif. codes: 0 '***' 0.001 '**' 0.01 '*' 0.05 '.' 0.1 ' ' 1

Residual standard error: 0.3835 on 917 degrees of freedom

Multiple R-squared: 0.01802, Adjusted R-squared: 0.0148

F-statistic: 5.608 on 3 and 917 DF, p-value: 0.0008223

We can't reject that is is at the 10% (or any conventional) level.