Experimental Methods: Lecture 4

Mediation

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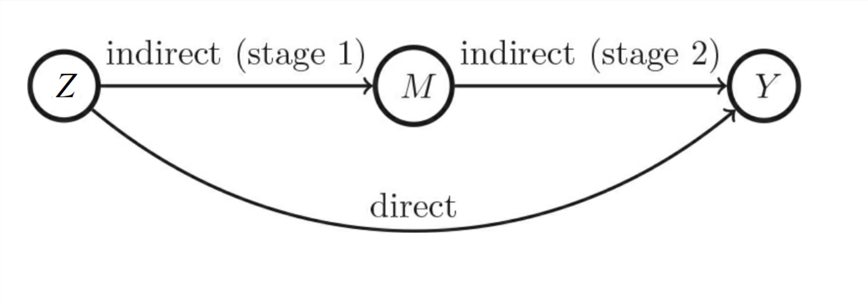
Road Map

- Mediation'

Mediation



Classic approaches to mediation

$$\text{Total Effect} : Y_i = \alpha_1 + \beta Z_i + e_{1i} \tag{1}$$

$$\text{Direct Effect} : Y_i = \alpha_2 + \gamma Z_i + \omega M_i + e_{2i} \tag{2}$$

$$\text{Indirect Effect} : (\beta - \gamma) \tag{3}$$

Mediation and Potential Outcomes

- Define $M_i(z)$ as the potential value of M_i when $Z_i = z$
- Define $Y_i(m, z)$ as potential outcome when $M_i = m$ and $Z_i = z$
- $Y_i(M_i(1), 1)$ thus expresses potential outcome when $Z_i = 1$ and M_i takes on potential outcome that occurs when $Z_i = 1$
- Total effect of Z_i on Y_i is $Y_i(M_i(1), 1) - Y_i(M_i(0), 0)$
- What is the direct effect of Z_i on Y_i controlling for M_i?
  - There is more than one definition
  - $Y_i(M_i(0), 1) - Y_i(M_i(0), 0)$ is direct effect of Z_i on Y_i holding m constant at $M_i(0)$
  - $Y_i(M_i(1), 1) - Y_i(M_i(1), 0)$ is direct effect of Z_i on Y_i holding m constant at $M_i(1)$
  - $Y_i(M_i(0), 1)$ and $Y_i(M_i(1), 0)$ are complex potential outcomes, so named because they are purely imaginary and never occur empirically

Mediation and Potential Outcomes

- What is the direct effect of Z_i on Y_i through M_i?
  - This is the effect on Y_i of changing from $M_i(0)$ to $M_i(1)$ while holding Z_i constant
  - So again, depending on Z_i, we get two definitions of the indirect effect
  - $Y_i(M_i(1), 1) - Y_i(M_i(0), 1) | Z_i = 1$ and $Y_i(M_i(1), 0) - Y_i(M_i(0), 0) | Z_i = 0$
  - Again $Y_i(M_i(0), 1)$ and $Y_i(M_i(1), 0)$ are the earlier complex potential outcomes
- Each of these four equations involve a term that is fundamentally unobservable
- True even if we assume that both indirect effects are equal
- There is thus a fundamental limitation on what we can learn from an experiment while manipulating only Z_i without making further assumptions

Example: New Drug and Blood Pressure

- FDA Evidence
  1. New Drug (Z)
  2. Blood Pressure (Y)
  3. Aspirin (M)
- Total effect of drug on blood pressure
  - $Y(1) - Y(0)$
- Total effect of drug on asprin use
  - $M(Z = 1) - M(Z = 0)$
- Total effect of asprin use on blood pressure
  - $Y(M = 1) - Y(M = 0)$
- Joint effect of drug + asprin use on blood pressure
  - $Y(11) - Y(00)$

Example: New Drug and Blood Pressure

- Effect of drug when individual forced to refrain from asprin
    - $Y(10) - Y(00)$
- Effect of drug when individual forced to take asprin
    - $Y(11) - Y(01)$

Summary

$$
\begin{aligned}
Y(1) - Y(0) &= Y(1M(1)) - Y(0M(0)) \\
&= \underbrace{Y(1M(1)) - Y(1M(0))}_{indirect} + \underbrace{Y(1M(0)) - Y(0M(0))}_{direct}
\end{aligned}
$$

$$
= \underbrace{Y(1M(1)) - Y(0M(1))}_{direct} + \underbrace{Y(0M(1)) - Y(0M(0))}_{indirect}
$$

Ruling Out Mediators

- What if the sharp null hypothesis $M_i(0) = M_i(1)$ is true?
- $Y_i(M_i(1), 1) - Y_i(M_i(0), 1)|Z_i = 1$ and $Y_i(M_i(1), 0) - Y_i(M_i(0), 0)|Z_i = 0$
- Then both indirect effects equal 0. Experiments may indicate when mediation does not occur, but sometimes difficult to do in practice:
    - Need tight estimate around 0
    - Need sharp null to be true, not just $ATE = 0$
- Although sharp null cannot be proven, we can cite evidence suggesting whether this conjecture is a reasonable approximation
- We thus learn something useful about mediation when discovering a lack of causal relationship between Z_i and proposed mediator
- Conversely, if Z_i and M_i have a strong relationship, we cannot rule out M_i as a possible mediator

Manipulating the Mediators

- A fundamental problem is that M_i is not independently manipulated via random intervention
- Could we manipulate M_i as well to build the case for mediation? $\rightarrow$ In principle, yes, but difficult in practical situations
- Example Y_i is scurvy, Z_i is lemon, M_i is vitamin C
  - We want indirect effect $Y_i(M_i(1), 0) - Y_i(M_i(0), 0)$
  - $M_i(1)$ is vitamin C level of lemon, we feed pills without lemons
  - Still not perfect: Vitamin C in lemons consumed differently from pills, pills might have other effects on Y_i
- Manipulations of M_i are therefore instructive, but ability to provide empirical estimates inevitably requires additional assumptions
- In the Bhavnani example, possible M_i are number of female incumbents, voters' sense of whether it is appropriate or desirable to have women representatives, and turnout rate in local elections

Implicit Mediation

- Consider a treatment Z_i that contains multiple elements in it
- Rather than manipulating M_i, change the treatment to isolate particular elements of Z_i (i.e. Z^1, Z^2, Z^3) whose attributes affect M_i along the way
- Focus is not on demonstrating how a Z_i-induced change in M_i changes Y_i, but on the effect of different isolated treatments on Y_i
- In particular, no attempt to estimate the effects of observed changes in M_i at all

Example: Conditional Cash Transfers

- Interest in conditional cash transfers on poor to keep children in school and attend health clinics
- Field experiments find improved educational outcomes for children in developing countries from these transfers (Baird, McIntosh, and Ozler 2009)
- What could the causal mechanism be?
  1. Cash subsidies allow greater investment in children's welfare
  2. Imposed conditions improve children's welfare
- Baird, McIntosh and Ozler (2009) designed experiment with three groups
  - Control group with no subsidy, instructions, or conditions
  - One treatment group gets cash without conditions
  - Another treatment group gets cash with conditions
  - Finding: Null hypothesis of no difference between treatment groups cannot be rejected

Benefits of Implicit Mediation

1. Simple: Never strays from the unbiased statistical framework of comparing randomly assigned groups
2. By adding and subtracting elements from treatment, this approach lends itself to exploration and discovery of new treatments
    - Facilitates the process of testing basic propositions about what works by providing clues about the active elements that cause a treatment to work particularly well
3. Can gauge treatment effects on a wide array of outcome variables
    - Allows manipulation checks for establishing the empirical relationship between intended and actual treatments
    - Example: Does discussion in the classroom improve performance? Check if treatment increases discussion

Voter Turnout Example

- Gerber, Green, and Larimer (2008) interested in the effect of communication on turnout
- U.S. has voters files, anyone know what they are?
- 180,000 Michigan households in experiment
- 100,000 in control group (no postcards), other groups 20,000 each
- **Civic duty**: "It's your civic duty to vote"
- **Hawthorne**: "It's your civic duty to vote, we're doing a study and will check public records"
- **Self**: "You should vote, here's your recent voting record"
- **Neighbors**: "You should vote, here's your neighbors' voting records and your own"

Results

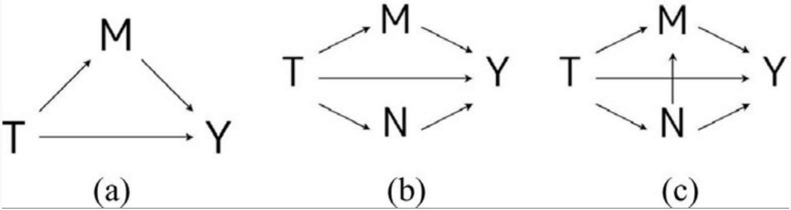
	Control	Civic	Hawthorne	Self	Neighbors
Pct Voting	29.7%	31.5%	32.2%	34.5%	37.8%
N	191,243	38,218	38,204	38,218	38,201

Anyone here know how Gerber followed up on this study?

Summary

- We are often curious about the mechanisms by which an experimental treatment transmits its influence
- Adding mediators as right-hand variables to determine this is a flawed strategy that generally provides bias in favor of mediation
- Main issue here is that the mediator is not experimentally manipulated
- In theory we could manipulate mediators experimentally, but this is difficult for two reasons
    1. We never observe complex potential outcomes
    2. Manipulation of mediators directly is often impractical
- However, two lines of inquiry seem promising:
    1. We can rule out mediators easier than we can find them
    2. We can implicitly manipulate mediators

Causal Mechanisms



Potential Outcomes

Total unit effect:

$$\tau_i \equiv Y_i(1, M_i(1)) - Y_i(0, M_i(0))$$

Indirect effect:

$$\delta_i(t) \equiv Y_i(t, M_i(1)) - Y_i(t, M_i(0))$$

Direct effect:

$$\tau_i \equiv Y_i(1, M_i(0)) - Y_i(0, M_i(0))$$

Chain Fallacy

Population Proportion on Mediator $M_i(1) - M_i(0)$ on Outcome $Y_i(t, 1) - Y_i(t, 0)$ Effect $Y_i(t, M_i(1)) - Y_i(t, M_i(0))$	$M_i(1)$ Mediator Effect Causal Mediation	$M_i(0)$	$Y_i(t, 1)$	$Y_i(t, 0)$	Treatment Effect		
0.3	1	0	0	1	1	-1	-1
0.3	0	0	1	0	0	1	0
0.1	0	1	0	1	-1	-1	1
0.3	1	1	1	0	0	1	0
Average	0.6	0.4	0.6	0.4	0.2	0.2	-0.2

General Estimator Algorithm

- Model outcome and mediator
  - Outcome model: $p(Y_i | T_i, M_i, X_i)$
  - Mediator model: $p(M_i | T_i, X_i)$
- These models can be of any form (linear or nonlinear, semi- or nonparametric, with or without interactions)
- Predict mediator for both treatment values $M_i(1), M_i(0)$
- Predict outcome by first setting $T_i = 1$ and $M_i = M_i(0)$ and then $T_i = 1$ and $M_i = M_i(1)$
- Compute the average difference between two outcomes to obtain a consistent estimate of ACME
- Monte-Carlo or bootstrapping to estimate uncertainty

Example: Continuous Mediator and Binary Outcome

- Estimate the following models:

$$M_i = \alpha_2 + \beta_2 T_i + X_i + e_{2i}$$
$$Pr(Y_i = 1) = \Phi(\alpha_3 + b_3 T_i + \gamma M_i + X_i + e_{3i})$$

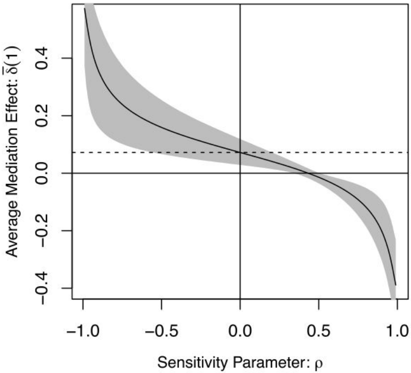
- Predict M_i for $T_i = 1$ and $T_i = 0$. This gives you $\hat{M}_i(1)$ and $\hat{M}_i(0)$
- Predict Y_i with $T_i = 1$ and $\hat{M}_i(0)$ and vice versa
- Take average of these two predictions

Media Cues and Immigration Attitudes

Brader et al. experiment:

- Subjects read a mock news story about immigration
- Treatment: immigrant in story is a Hispanic, and the news story emphasized the economic costs of immigration
- They measured a range of different attitudinal and behavioral outcome variables:
    - Opinions about increasing or decrease immigration
    - Contact legislator about the issue
    - Send anti-immigration message to legislator
- They want to test whether the treatment increases anxiety, leading to greater opposition to immigration

Sensitivity: Interpreting ρ



- ACME $>$ 0 as long as the error correlation is less than 0.39 (0.30 with 95% CI)

Sensitivity: R Code

1. Fit models for the mediator and outcome variable and store these models.

```
> m <- lm(Mediator ~ Treat + X)
> y <- lm(Y ~ Treat + Mediator + X)
```

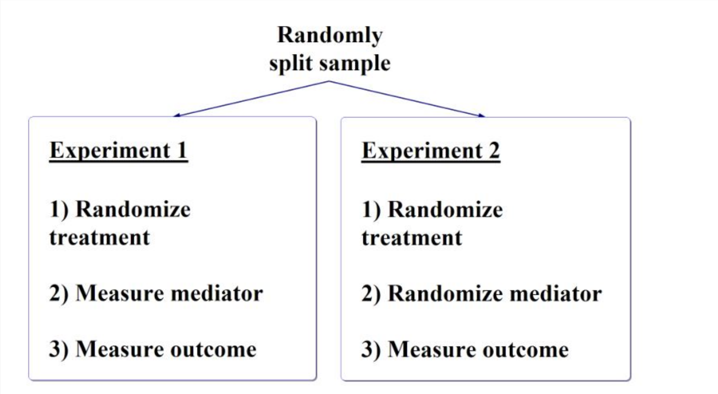
2. Mediation analysis: Feed model objects into the `mediate()` function. Call a summary of results.

```
> m.out<-mediate(m, y, treat = "Treat",
                  mediator = "Mediator")
> summary(m.out)
```

3. Sensitivity analysis: Feed the output into the `medsens()` function. Summarize and plot.

```
> s.out <- medsens(m.out)
> summary(s.out)
> plot(s.out, "rho")
> plot(s.out, "R2")
```

Parallel Design



Example from Behavioral Neuroscience

- Why study brain? Social scientists' search for causal mechanisms underlying human behavior $\rightarrow$ Psychologists, economists, and even political scientists
- Question: What mechanism links low offers in an ultimatum game with "irrational" rejections?
    - A brain region known to be related to fairness becomes more active when unfair offer received (single experiment design)
- Design solution: manipulate mechanisms with TMS
    - Knoch et al. use TMS to manipulate — turn off — one of these regions, and then observes choices (parallel design)legislator

Encouragement Design

- Randomly *encourage* subjects to take particular values of the mediator M_i
- Standard *instrumental variable* assumptions (Angrist et al.)
- Use a 2×3 factorial design:
    - Randomly assign T_i
    - Also randomly decide whether to positively encourage, negatively encourage, or do nothing
    - Measure mediator and outcome
- Informative inference about the "complier" ACME
- Reduces to the parallel design if encouragement is perfect
- Application to the immigration experiment: Use autobiographical writing tasks to encourage anxiety

Cross-over Design

- Recall *ACME* can be identified if we observe $Y_i(t_0; M_i(t))$
- Get $M_i(t)$, then switch T_i to t_0 while holding $M_i = M_i(t)$
- Crossover design:
    - Round 1: Conduct a standard experiment
    - Round 2: Change the treatment to the opposite status but fix the mediator to the value observed in the first round
- Very powerful – identifies mediation effects for each subject
- Must assume *no carryover effect*: Round 1 must not affect Round 2
- Can be made plausible by design

Example from Labor Economics

Bertrand & Mullainathan (2004, AER)

- Treatment: Black vs. White names on CVs
- Mediator: Perceived qualifications of applicants
- Outcome: Callback from employers
- Quantity of interest: Direct effects of (perceived) race
- Would Jamal get a callback if his name were Greg but his qualifications stayed the same?
- Round 1: Send Jamal's actual CV and record the outcome
- Round 2: Send his CV as Greg and record the outcome
- Assumptions are plausible

Cross-over Encouragement Design

- Cross-over encouragement design:
  - Round 1: Conduct a standard experiment
  - Round 2: Same as crossover, except encourage subjects to take the mediator values
- Example: Hainmueller & Hiscox (2010, APSR)
  - Treatment: Framing immigrants as low- or high-skilled
  - Possible mechanism: Low income subjects may expect higher competition from low skill immigrants
  - Manipulate expectation using a news story
  - Round 1: Original experiment but measure expectation
  - Round 2: Flip treatment, but encourage expectation in the same direction as Round 1