Experimental Designs for Identifying Causal Mechanisms

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Project Reference

My talk is based on the collaborative project with L. Keele (Penn State), D. Tingley (Harvard), and T. Yamamoto (MIT)

- "Experimental Designs for Identifying Causal Mechanisms." *Journal of Royal Statistical Society, Series A* (with discussions)

Some other related papers:

- "Unpacking the Black Box of Causality: Learning about Causal Mechanisms from Experimental and Observational Studies." *American Political Science Review*
- "Identification, Inference, and Sensitivity Analysis for Causal Mediation Effects." *Statistical Science*
- "A General Approach to Causal Mediation Analysis." *Psychological Methods*
- "Causal Mediation Analysis Using R." *Advances in Social Science Research Using R*

Software `mediation` is freely available in R and Stata

Identification of Causal Mechanisms

- Causal inference is a central goal of scientific research
- Scientists care about causal mechanisms, not just about causal effects

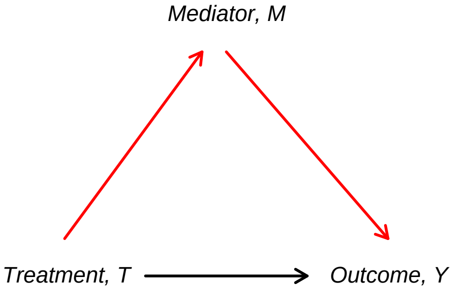
- Randomized experiments often only determine whether the treatment causes changes in the outcome
- Not how and why the treatment affects the outcome
- Common criticism of experiments and statistics:

black box view of causality

- Question: How can we learn about causal mechanisms from experimental and observational studies?

What Is a Causal Mechanism?

- Mechanisms as alternative causal pathways
- Cochran (1957)'s example:
  soil fumigants increase farm crops by reducing eel-worms
- Causal mediation analysis



- Quantities of interest: Direct and indirect effects
- Fast growing methodological literature

Potential Outcomes Framework

Framework: Potential outcomes model of causal inference

- Binary treatment: $T_i \in \{0, 1\}$
- Mediator: $M_i \in \mathcal{M}$
- Outcome: $Y_i \in \mathcal{Y}$
- Observed pre-treatment covariates: $X_i \in \mathcal{X}$

- Potential mediators: $M_i(t)$, where $M_i = M_i(T_i)$ observed
- Potential outcomes: $Y_i(t, m)$, where $Y_i = Y_i(T_i, M_i(T_i))$ observed
- In a standard experiment, **only one potential outcome** can be observed for each i

Causal Mediation Effects

- Total causal effect:

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

- Causal mediation (Indirect) effects:

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

- Causal effect of the change in M_i on Y_i that would be induced by treatment
- Change the mediator from $M_i(0)$ to $M_i(1)$ while holding the treatment constant at t
- Represents the mechanism through M_i

Total Effect $=$ Indirect Effect $+$ Direct Effect

- Direct effects:

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

- Causal effect of T_i on Y_i, holding mediator constant at its potential value that would realize when $T_i = t$
- Change the treatment from 0 to 1 while holding the mediator constant at $M_i(t)$
- Represents all mechanisms other than through M_i

- Total effect $=$ mediation (indirect) effect $+$ direct effect:

$$\tau_i = \delta_i(t) + \zeta_i(1-t) = \frac{1}{2}\{\delta_i(0) + \delta_i(1) + \zeta_i(0) + \zeta_i(1)\}$$

Mechanisms, Manipulations, and Interactions

Mechanisms

- Indirect effects: $\delta_i(t) \equiv Y_i(t, M_i(1)) - Y_i(t, M_i(0))$
- Counterfactuals about treatment-induced mediator values

Manipulations

- Controlled direct effects: $\xi_i(t, m, m') \equiv Y_i(t, m) - Y_i(t, m')$
- Causal effect of directly manipulating the mediator under $T_i = t$

Interactions

- Interaction effects: $\xi(1, m, m') - \xi(0, m, m') \neq 0$
- Doesn't imply the existence of a mechanism

What Does the Observed Data Tell Us?

- Quantity of Interest: Average causal mediation effects

$$\bar{\delta}(t) \equiv \mathbb{E}(\delta_i(t)) = \mathbb{E}\{Y_i(t, M_i(1)) - Y_i(t, M_i(0))\}$$

- Average direct effects ($\bar{\zeta}(t)$) are defined similarly
- Problem: $Y_i(t, M_i(t))$ is observed but $Y_i(t, M_i(t'))$ can never be observed
- We have an identification problem

$\Longrightarrow$ Need additional assumptions to make progress

Single Experiment Design

1) Randomize treatment

2) Measure mediator

3) Measure outcome

Assumption Satisfied

- Randomization of treatment

$$\{Y_i(t,m), M_i(t')\} \perp\!\!\!\perp T_i \mid X_i$$

Key Identifying Assumption

- Sequential Ignorability:

$$Y_i(t,m) \perp\!\!\!\perp M_i \mid T_i, X_i$$

- Selection on (pre-treatment) observables
- Violated if there are unobservables that affect mediator and outcome
- Can't condition on post-treatment confounders

A Typical Psychological Experiment

- Brader *et al.*: media framing experiment
- Treatment: Ethnicity (Latino vs. Caucasian) of an immigrant
- Mediator: anxiety
- Outcome: preferences over immigration policy

- Single experiment design with statistical mediation analysis
- Emotion: difficult to directly manipulate

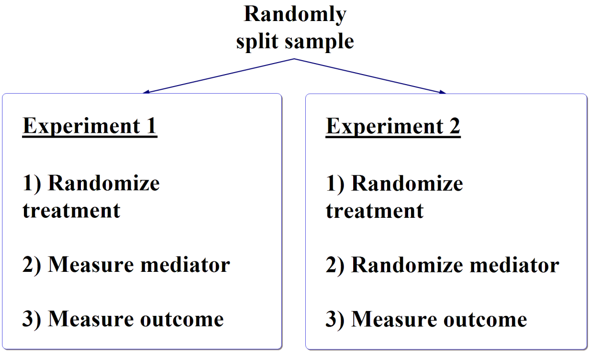
- Sequential ignorability assumption is not credible
- Possible confounding

Beyond Sequential Ignorability

- Without sequential ignorability, standard experimental design lacks identification power
- Even the sign of ACME is not identified

- Need to develop alternative experimental designs for more credible inference
- Possible when the mediator can be directly or indirectly manipulated

Parallel Design



- Must assume no direct effect of manipulation on outcome
- More informative than standard single experiment
- If we assume no T–M interaction, ACME is point identified

Example from Behavioral Neuroscience

Why study brain?: Social scientists' search for causal mechanisms underlying human behavior

- 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)

Limitations

- Difference between manipulation and mechanism

Prop.	$M_i(1)$	$M_i(0)$	$Y_i(t,1)$	$Y_i(t,0)$	$\delta_i(t)$
0.3	1	0	0	1	-1
0.3	0	0	1	0	0
0.1	0	1	0	1	1
0.3	1	1	1	0	0

- Here, $\mathbb{E}(M_i(1) - M_i(0)) = \mathbb{E}(Y_i(t,1) - Y_i(t,0)) = 0.2$, but $\bar{\delta}(t) = -0.2$
- Limitations:
  - Direct manipulation of the mediator is often impossible
  - Even if possible, manipulation can directly affect outcome
- Need to allow for subtle and indirect manipulations

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:

1. Randomly assign T_i
2. Also randomly decide whether to **positively encourage**, **negatively encourage**, or do nothing
3. 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

Crossover Design

- Recall ACME can be identified if we observe $Y_i(t', M_i(t))$
- Get $M_i(t)$, then switch T_i to t' while holding $M_i = M_i(t)$
- Crossover design:
  1. Round 1: Conduct a standard experiment
  2. 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

Crossover Encouragement Design

- Crossover encouragement design:
  1. Round 1: Conduct a standard experiment
  2. 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
- Outcome: Preferences over immigration policy
- Possible mechanism: Low income subjects may expect higher competition from low skill immigrants

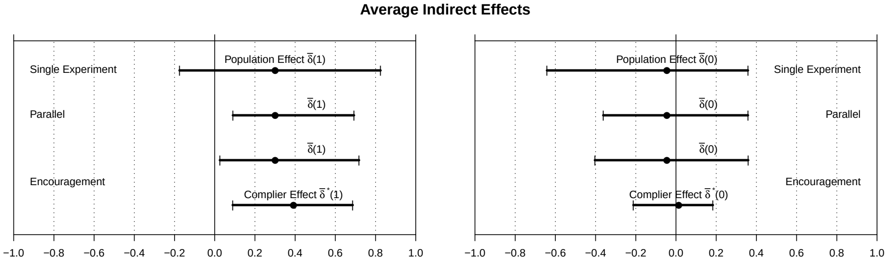
- Manipulate expectation using a news story
- Round 1: Original experiment but measure expectation
- Round 2: Flip treatment, but encourage expectation in the same direction as Round 1

Comparing Alternative Designs

- No manipulation
  - Single experiment: sequential ignorability
- Direct manipulation
  - Parallel: no manipulation effect, no interaction effect
  - Crossover: no manipulation effect, no carryover effect
- Indirect manipulation
  - Encouragement: no manipulation effect, monotonicity, no interaction (?)
  - Crossover encouragement: no manipulation effect, monotonicity, no carryover effect

Identification Power

- A numerical example based on Brader et al. (2008)
- Binary outcome, mediator, and treatment
- Sharp bounds for parallel and encouragement designs without no-interaction assumption



Designing Observational Studies

- Key difference between experimental and observational studies: treatment assignment
- Sequential ignorability:
  1. Ignorability of treatment given covariates
  2. Ignorability of mediator given treatment and covariates
- Both (1) and (2) are suspect in observational studies

- Statistical control: matching, propensity scores, etc.
- Search for quasi-randomized treatments: "natural" experiments

- How can we design observational studies?
- Experiments can serve as templates for observational studies

Example from Political Science

EXAMPLE Incumbency advantage

- Estimation of incumbency advantages goes back to 1960s
- Why incumbency advantage? Scaring off quality challenger
- Use of cross-over design (Levitt and Wolfram)
  1. 1st Round: two non-incumbents in an open seat
  2. 2nd Round: same candidates with one being an incumbent
- Assume challenger quality (mediator) stays the same
- Estimation of direct effect is possible

- Redistricting as natural experiments (Ansolabehere et al.)
  1. 1st Round: incumbent in the old part of the district
  2. 2nd Round: incumbent in the new part of the district
- Challenger quality is the same but treatment is different
- Estimation of direct effect is possible

Concluding Remarks

- Identification of causal mechanisms is difficult but is possible
- Additional assumptions are required
- Five strategies:
  1. Single experiment design
  2. Parallel design
  3. Crossover design
  4. Encouragement design
  5. Crossover encouragement design
- Statistical assumptions: sequential ignorability, no interaction
- Design assumptions: no manipulation, no carryover effect
- Experimenters' creativity and technological development to improve the validity of these design assumptions

The project website for papers and software:

http://imai.princeton.edu/projects/mechanisms.html

Email for comments and suggestions:

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