Experimental Identification of Causal Mechanisms

Kosuke Imai[1] Dustin Tingley[2] Teppei Yamamoto[3]

[1]Princeton University

[2]Harvard University

[3]Massachusetts Institute of Technology

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Experiments, Statistics, and Causal Mechanisms

- Causal inference is a central goal of most scientific research
- Experiments as **gold standard** for estimating *causal effects*
- A major criticism of experimentation:

  it can only determine whether the treatment causes changes in the outcome, but not how and why

- Experiments merely provide a **black box** view of causality
- But, scientific theories are all about causal mechanisms
- Knowledge about causal mechanisms can also improve policies

- Key Challenge: How can we *design* and analyze experiments to identify causal mechanisms?

Overview of the Talk

- Show the limitation of a common approach
- Consider alternative experimental designs

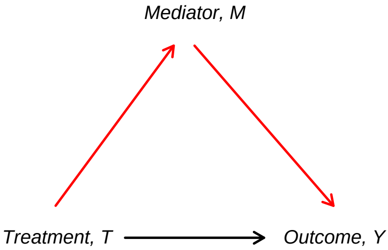
- What is a minimum set of assumptions required for identification under each design?
- How much can we learn without the key identification assumptions under each design?

- Identification of causal mechanisms is possible but difficult
- Distinction between design and statistical assumptions
- Roles of creativity and technological developments

- Illustrate key ideas through recent social science research

Causal Mechanisms as Indirect Effects

- What is a causal mechanism?
- Cochran (1957)'s example:
  soil fumigants increase farm crops by reducing eel-worms
- Political science example: incumbency advantage
- Causal mediation analysis



- Quantities of interest: Direct and indirect effects
- Fast growing methodological literature
- Alternative definition: causal components (Robins; VanderWeele)

Formal Statistical Framework 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 $M_i = M_i(T_i)$
- Potential outcomes: $Y_i(t, m)$ where $Y_i = Y_i(T_i, M_i(T_i))$

- Fundamental problem of causal inference (Rubin; Holland):

  Only one potential value is observed

  1. If $T_i = 1$, then $M_i(1)$ is observed but $M_i(0)$ is not
  2. If $T_i = 0$ and $M_i(0) = 0$, then $Y_i(0, 0)$ is observed but $Y_i(1, 0)$, $Y_i(0, m)$, and $Y_i(1, m)$ are not when $m \neq 0$

Defining and Interpreting Indirect Effects

- Total causal effect:

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

- Indirect (causal mediation) effects (Robins and Greenland; Pearl):

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

- Change $M_i(0)$ to $M_i(1)$ while holding the treatment constant at t
- Effect of a change in M_i on Y_i that would be induced by treatment

- Fundamental problem of causal mechanisms:

  For each unit i, $Y_i(t, M_i(t))$ is observable but
  $Y_i(t, M_i(1-t))$ is not even observable

Defining and Interpreting Direct Effects

- Direct effects:

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

- Change T_i from 0 to 1 while holding the mediator constant at $M_i(t)$
- Causal effect of T_i on Y_i, holding mediator constant at its potential value that would be realized when $T_i = t$

- Total effect = indirect effect + direct effect:

$$\begin{aligned} \tau_i &= \delta_i(t) + \zeta_i(1-t) \\ &= \delta_i + \zeta_i \end{aligned}$$

where the second equality assumes $\delta_i(0) = \delta_i(1)$ and $\zeta_i(0) = \zeta_i(1)$

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

Single Experiment Design

1) Randomize treatment

2) Measure mediator

3) Measure outcome

Assumption Satisfied

- Randomization of treatment

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

Key Identifying Assumption

- Sequential Ignorability:

$$Y_i(t', m) \perp\!\!\!\perp M_i \mid T_i = t, X_i = x$$

- Selection on pre-treatment observables
- Unmeasured pre-treatment confounders
- Measured and unmeasured post-treatment confounders

Identification under the Single Experiment Design

- Sequential ignorability yields nonparametric identification

$$\bar{\delta}(t) = \int\int \mathbb{E}(Y_i \mid M_i, T_i = t, X_i) \{dP(M_i \mid T_i = 1, X_i) - dP(M_i \mid T_i = 0, X_i)\} \, dP(X_i)$$

- Linear structural equation modeling (a.k.a. Baron-Kenny)
- Alternative assumptions: Robins, Pearl, Petersen *et al.*, VanderWeele, and many others

- Sequential ignorability is an untestable assumption
- Sensitivity analysis: How large a departure from sequential ignorability must occur for the conclusions to no longer hold?

- But, sensitivity analysis does not solve the problem

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

Identification Power of the Single Experiment Design

- How much can we learn without sequential ignorability?
- Sharp bounds on indirect effects (Sjölander):

$$\max \left\{ \begin{array}{l} -P_{001} - P_{011} \\ -P_{011} - P_{010} - P_{110} \\ -P_{000} - P_{001} - P_{100} \end{array} \right\} \leq \bar{\delta}(1) \leq \min \left\{ \begin{array}{l} P_{101} + P_{111} \\ P_{010} + P_{110} + P_{111} \\ P_{000} + P_{100} + P_{101} \end{array} \right\}$$

$$\max \left\{ \begin{array}{l} -P_{100} - P_{110} \\ -P_{011} - P_{111} - P_{110} \\ -P_{001} - P_{101} - P_{100} \end{array} \right\} \leq \bar{\delta}(0) \leq \min \left\{ \begin{array}{l} P_{000} + P_{010} \\ P_{011} + P_{111} + P_{010} \\ P_{000} + P_{001} + P_{101} \end{array} \right\}$$

where $P_{ymt} = \Pr(Y_i = y, M_i = m \mid T_i = t)$

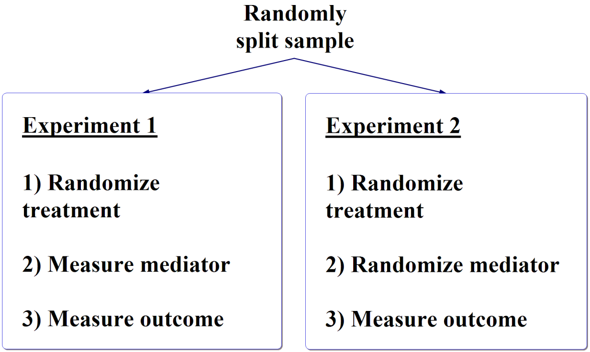
- The sign is not identified

Alternative Experimental Designs

- Can we design experiments to better identify causal mechanisms?
- Perfect manipulation of the mediator:
  1. Parallel Design
  2. Crossover Design
- Imperfect manipulation of the mediator:
  1. Parallel Encouragement Design
  2. Crossover Encouragement Design
- Implications for designing observational studies

The Parallel Design

- No manipulation effect assumption: The manipulation has no direct effect on outcome other than through the mediator value
- Running two experiments in parallel:



Identification under the Parallel Design

- 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

- $\mathbb{E}(M_i(1) - M_i(0)) = \mathbb{E}(Y_i(t,1) - Y_i(t,0)) = 0.2$, but $\bar{\delta}(t) = -0.2$

- Is the randomization of mediator sufficient? No
- The no interaction assumption (Robins) yields point identification

$$Y_i(1,m) - Y_i(1,m') \;=\; Y_i(0,m) - Y_i(0,m')$$

- Must hold at the unit level but indirect tests are possible
- Implication: analyze a group of homogeneous units

Identification under the Parallel Design

- Is the randomization of mediator sufficient? No!

- Sharp bounds: Binary mediator and outcome
- Use of linear programming (Balke and Pearl):
  - Objective function:

$$\mathbb{E}\{Y_i(1, M_i(0))\} \quad = \quad \sum_{y=0}^{1}\sum_{m=0}^{1}(\pi_{1ym1} + \pi_{y1m1})$$

    where $\pi_{y_1 y_0 m_1 m_0} = \Pr(Y_i(1,1) = y_1, Y_i(1,0) = y_0, M_i(1) = m_1, M_i(0) = m_0)$
  - Constraints implied by $\Pr(Y_i = y, M_i = m \mid T_i = t, D_i = 0)$, $\Pr(Y_i = y \mid M_i = m, T_i = t, D_i = 1)$, and the summation constraint

- More informative than those under the single experiment design
- Can sometimes identify the sign of average direct/indirect effects

An 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)

The Parallel Encouragement Design

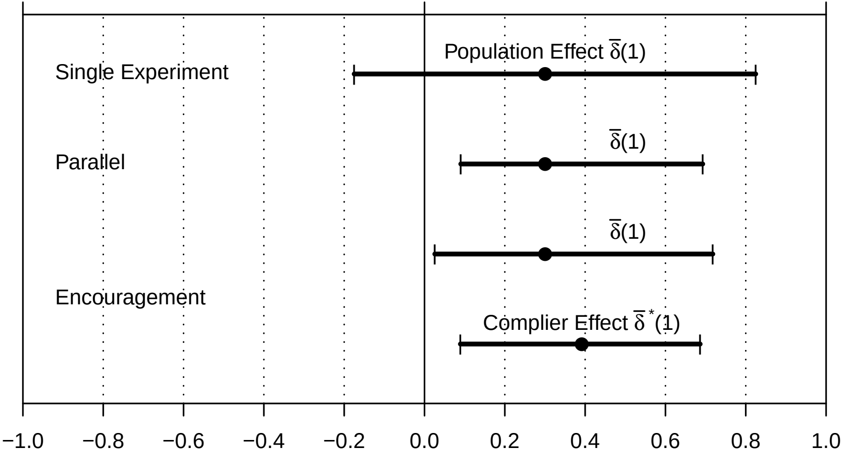
- Direct manipulation of mediator is often difficult
- Even if possible, the violation of no manipulation effect can occur
- Need for indirect and subtle manipulation

- Randomly encourage units to take a certain value of the mediator
- Instrumental variables assumptions (Angrist *et al.*):
  1. Encouragement does not discourage anyone
  2. Encouragement does not directly affect the outcome

- Not as informative as the parallel design
- Sharp bounds on the average "complier" indirect effects can be informative

A Numerical Example

- Based on the marginal distribution of a real experiment



The Crossover Design

Experiment 1

1) Randomize treatment

2) Measure mediator

3) Measure outcome

Same sample

Experiment 2

1) Fix treatment opposite Experiment 1

2) Manipulate mediator to level observed in Experiment 1

3) Measure outcome

Basic Idea

- Want to observe $Y_i(1 - t, M_i(t))$
- Figure out $M_i(t)$ and then switch T_i while holding the mediator at this value
- Subtract direct effect from total effect

Key Identifying Assumptions

- No Manipulation Effect
- No Carryover Effect: For $t = 0, 1$, $\mathbb{E}\{Y_{i1}(t, M_i(t))\} = \mathbb{E}\{Y_{i2}(t, m)\}$ if $m = M_i(t)$
- Not testable, longer “wash-out” period

Example from Labor Economics

Bertrand & Mullainathan (2004)

- Treatment: Black vs. White names on CVs
- Mediator: Perceived qualifications of applicants
- Outcome: Callback from employers

- Estimand: Direct effects of (perceived) race $\Longrightarrow$ overt racism
- 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:
  1. No manipulation: potential employers are unaware
  2. Carryover effect: send resumes to different (randomly matched) employers at the same time

The Crossover Encouragement Design

Experiment 1

1) Randomize treatment

2) Measure mediator

3) Measure outcome (optional)

Same sample

Experiment 2

1) Fix treatment opposite Experiment 1

2) Randomly encourage mediator to level observed in Experiment 1

3) Measure outcome

Key Identifying Assumptions

- Encouragement doesn't discourage anyone
- No Manipulation Effect
- No Carryover Effect

Identification Analysis

- Identify indirect effects for "compliers"
- No carryover effect assumption is indirectly testable (unlike the crossover design)

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 effect
  - Crossover encouragement: no manipulation effect, monotonicity, no carryover effect

Implications for the Design of Observational Studies

- Use of “natural experiments” in the social sciences
- Attempts to “replicate” experiments in observational studies

- Political science literature on incumbency advantage
- During 70s and 80s, the focus is on estimation of causal effects
- Positive effects, growing over time
- Last 20 years, search for causal mechanisms

- How large is the “scare-off/quality effect”?
- 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
- Assumptions
  1. Challenger quality (mediator) stays the same
  2. First election does not affect the second election

Another Incumbency Advantage Example

- 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
- Assumption: No interference between the old and new parts of the district

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. Parallel 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