Unpacking the Black-Box: Learning about Causal Mechanisms from Experimental and Observational Studies

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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?

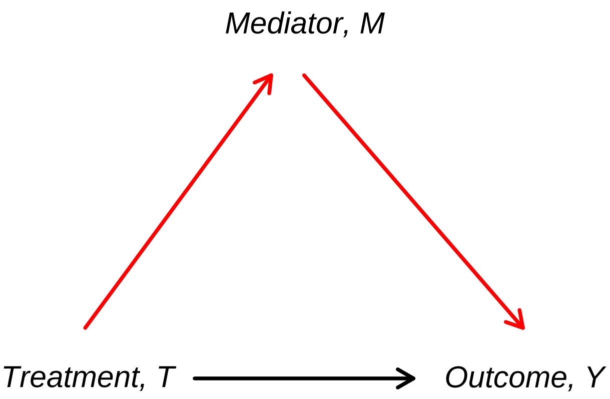
Goals of the Talk

Present a general framework for statistical design and analysis of causal mechanisms:

1. Show that the sequential ignorability assumption is required to identify mechanisms even in experiments
2. Offer a flexible estimation strategy under this assumption
3. Propose a sensitivity analysis to probe this assumption
4. Propose new experimental designs that do not rely on sequential ignorability
5. Extend these methods to 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

Project Reference

- Project Website:

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

- Papers:
  - "Unpacking the Black Box: Learning about Causal Mechanisms from Experimental and Observational Studies."
  - "Identification, Inference, and Sensitivity Analysis for Causal Mediation Effects." *Statistical Science*
  - "A General Approach to Causal Mediation Analysis." *Psychological Methods*
  - "Experimental Identification of Causal Mechanisms."
  - "Causal Mediation Analysis Using R." *Advances in Social Science Research Using R*
- Software: R package `mediation` implements all methods

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:

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

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

Identification under Sequential Ignorability

- Proposed identification assumption: Sequential Ignorability

$$\{Y_i(t', m), M_i(t)\} \perp\!\!\!\perp T_i \mid X_i = x \qquad (1)$$

$$Y_i(t', m) \perp\!\!\!\perp M_i(t) \mid T_i = t, X_i = x \qquad (2)$$

- (1) is guaranteed to hold in a standard experiment
- (2) does **not** hold unless X_i includes all confounders

Theorem: Under sequential ignorability, ACME and average direct effects are nonparametrically identified
(= consistently estimated from observed data)

Nonparametric Identification

Theorem: Under SI, both ACME and average direct effects are given by,

- ACME $\bar{\delta}(t)$

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

- Average direct effects $\bar{\zeta}(t)$

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

Traditional Estimation Method

- Linear structural equation model (LSEM):

$$\begin{aligned} M_i &= \alpha_2 + \beta_2 T_i + \xi_2^\top X_i + \epsilon_{i2}, \\ Y_i &= \alpha_3 + \beta_3 T_i + \gamma M_i + \xi_3^\top X_i + \epsilon_{i3}. \end{aligned}$$

  together implying

$$Y_i = \alpha_1 + \beta_1 T_i + \epsilon_{i1}$$

- Fit two least squares regressions separately
- Use product of coefficients ($\hat{\beta}_2\hat{\gamma}$) to estimate ACME
- Use asymptotic variance to test significance (Sobel test)

- Under SI and the no-interaction assumption ($\bar{\delta}(1) \neq \bar{\delta}(0)$), $\hat{\beta}_2\hat{\gamma}$ consistently estimates ACME
- Can be extended to LSEM with interaction terms

- Problem: Only valid for the simplest LSEM

Proposed General Estimation Algorithm

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

Need for Sensitivity Analysis

- Standard experiments require sequential ignorability to identify mechanisms
- The sequential ignorability assumption is often too strong
- Need to assess the robustness of findings via sensitivity analysis
- Question: How large a departure from the key assumption must occur for the conclusions to no longer hold?
- Parametric sensitivity analysis by assuming

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

but not

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

- Possible existence of unobserved *pre-treatment* confounder

Parametric Sensitivity Analysis

- Sensitivity parameter: $\rho \equiv \mathrm{Corr}(\epsilon_{i2}, \epsilon_{i3})$
- Sequential ignorability implies $\rho = 0$
- Set ρ to different values and see how ACME changes

- **Result:**

$$\bar{\delta}(0) \ = \ \bar{\delta}(1) \ = \ \frac{\beta_2 \sigma_1}{\sigma_2}\left\{\tilde{\rho} - \rho\sqrt{(1-\tilde{\rho}^2)/(1-\rho^2)}\right\},$$

where $\sigma_j^2 \equiv \mathrm{var}(\epsilon_{ij})$ for $j = 1, 2$ and $\tilde{\rho} \equiv \mathrm{Corr}(\epsilon_{i1}, \epsilon_{i2})$.

- When do my results go away completely?
- $\bar{\delta}(t) = 0$ if and only if $\rho = \tilde{\rho}$
- Easy to estimate from the regression of Y_i on T_i:

$$Y_i = \alpha_1 + \beta_1 T_i + \epsilon_{i1}$$

Interpreting Sensitivity Analysis with R squares

- Interpreting ρ: how small is too small?

- An unobserved (pre-treatment) confounder formulation:

$$\epsilon_{i2} \ = \ \lambda_2 U_i + \epsilon'_{i2} \quad \text{and} \quad \epsilon_{i3} \ = \ \lambda_3 U_i + \epsilon'_{i3}$$

- How much does U_i have to explain for our results to go away?

- Sensitivity parameters: R squares
  1. Proportion of previously unexplained variance explained by U_i

$$R_M^{2*} \ \equiv \ 1 - \frac{\mathrm{var}(\epsilon'_{i2})}{\mathrm{var}(\epsilon_{i2})} \quad \text{and} \quad R_Y^{2*} \ \equiv \ 1 - \frac{\mathrm{var}(\epsilon'_{i3})}{\mathrm{var}(\epsilon_{i3})}$$

  2. Proportion of original variance explained by U_i

$$\widetilde{R}_M^2 \ \equiv \ \frac{\mathrm{var}(\epsilon_{i2}) - \mathrm{var}(\epsilon'_{i2})}{\mathrm{var}(M_i)} \quad \text{and} \quad \widetilde{R}_Y^2 \ \equiv \ \frac{\mathrm{var}(\epsilon_{i3}) - \mathrm{var}(\epsilon'_{i3})}{\mathrm{var}(Y_i)}$$

- Then reparameterize ρ using (R_M^{2*}, R_Y^{2*}) (or $(\widetilde{R}_M^2, \widetilde{R}_Y^2)$):

$$\rho \quad = \quad \text{sgn}(\lambda_2\lambda_3) R_M^* R_Y^* \quad = \quad \frac{\text{sgn}(\lambda_2\lambda_3)\widetilde{R}_M \widetilde{R}_Y}{\sqrt{(1-R_M^2)(1-R_Y^2)}},$$

  where R_M^2 and R_Y^2 are from the original mediator and outcome models

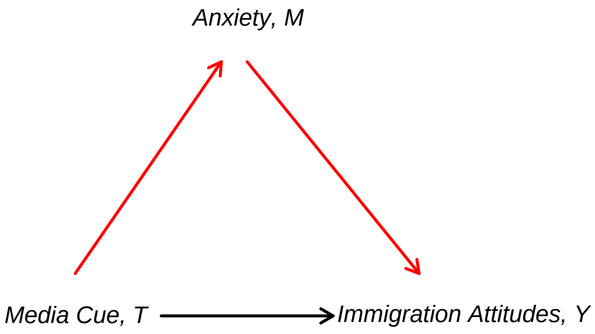
- $\text{sgn}(\lambda_2\lambda_3)$ indicates the direction of the effects of U_i on Y_i and M_i

- Set (R_M^{2*}, R_Y^{2*}) (or $(\widetilde{R}_M^2, \widetilde{R}_Y^2)$) to different values and see how mediation effects change

Example: Anxiety, Group Cues and Immigration

Brader, Valentino & Suhat (2008, AJPS)

- How and why do ethnic cues affect immigration attitudes?
- Theory: Anxiety transmits the effect of cues on attitudes



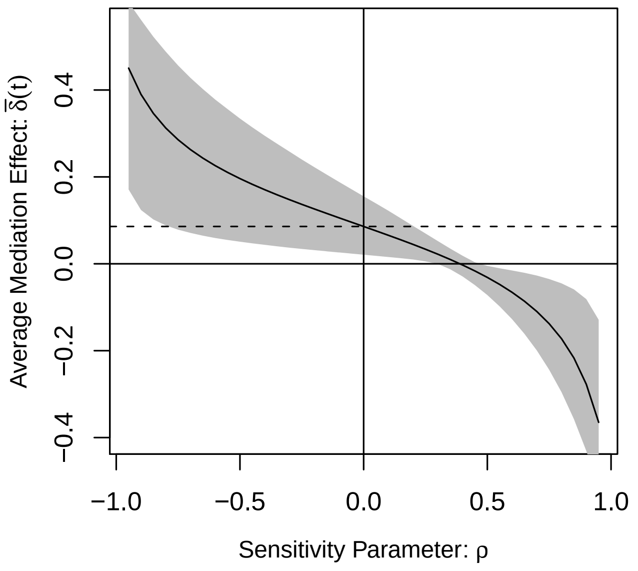
- ACME $=$ Average difference in immigration attitudes due to the change in anxiety induced by the media cue treatment
- Sequential ignorability $=$ No unobserved covariate affecting both anxiety and immigration attitudes

Reanalysis: Estimates under Sequential Ignorability

- Original method: Product of coefficients with the Sobel test — Valid only when both models are linear w/o T–M interaction (which they are not)
- Our method: Calculate ACME using our general algorithm

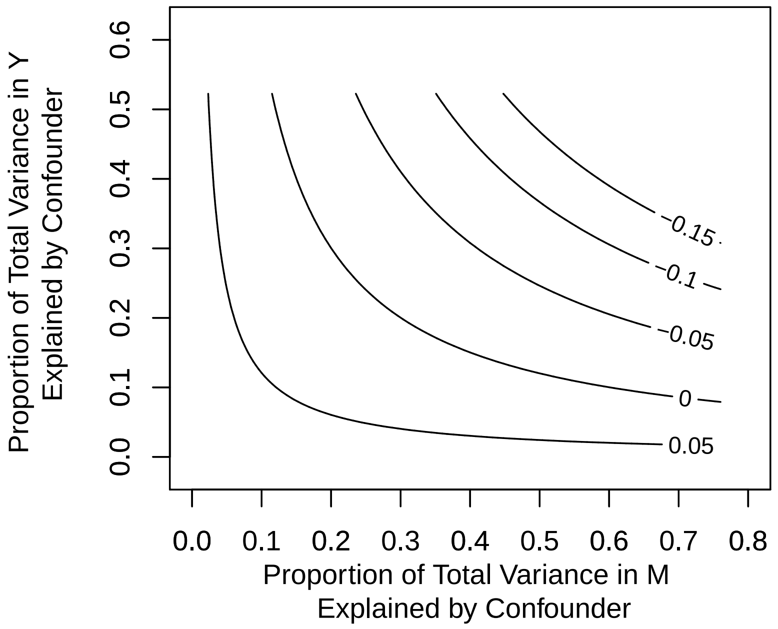
	Product of Coefficients	Average Causal Mediation Effect
Outcomes		
Decrease Immigration	.399	.089
$\bar{\delta}(t)$	[0.066, .732]	[0.023, .178]
Support English Only Laws	.287	.028
$\bar{\delta}(t)$	[0.015, 0.558]	[0.002, .078]
Request Anti-Immigration Info	.295	.049
$\bar{\delta}(t)$	[0.023, 0.567]	[0.007, 0.121]
Send Anti-Immigration Message	.303	.105
$\bar{\delta}(t)$	[0.046, .561]	[0.021, 0.191]

Reanalysis: Sensitivity Analysis w.r.t. ρ



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

Reanalysis: Sensitivity Analysis w.r.t. $\tilde{R}^2_M$ and $\tilde{R}^2_Y$



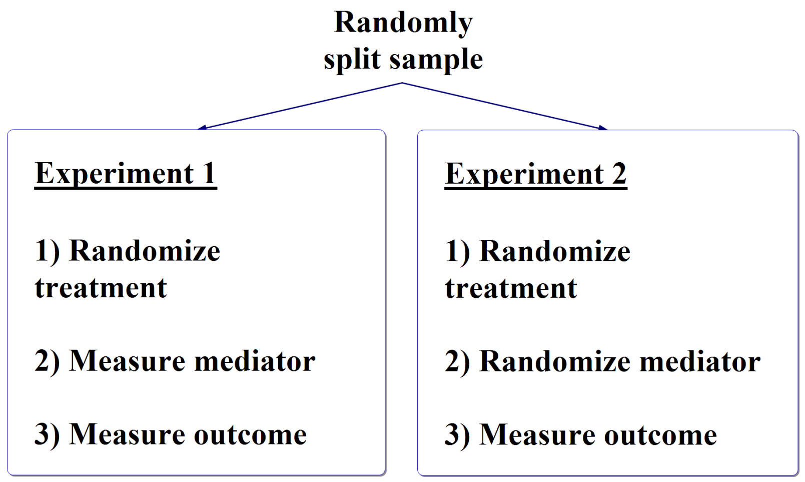
- An unobserved confounder can account for up to 26.5% of the variation in both Y_i and M_i before ACME becomes zero

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



Randomly split sample

Experiment 1

1) Randomize treatment

2) Measure mediator

3) Measure outcome

Experiment 2

1) Randomize treatment

2) Randomize mediator

3) Measure outcome

- 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

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, regressions, 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

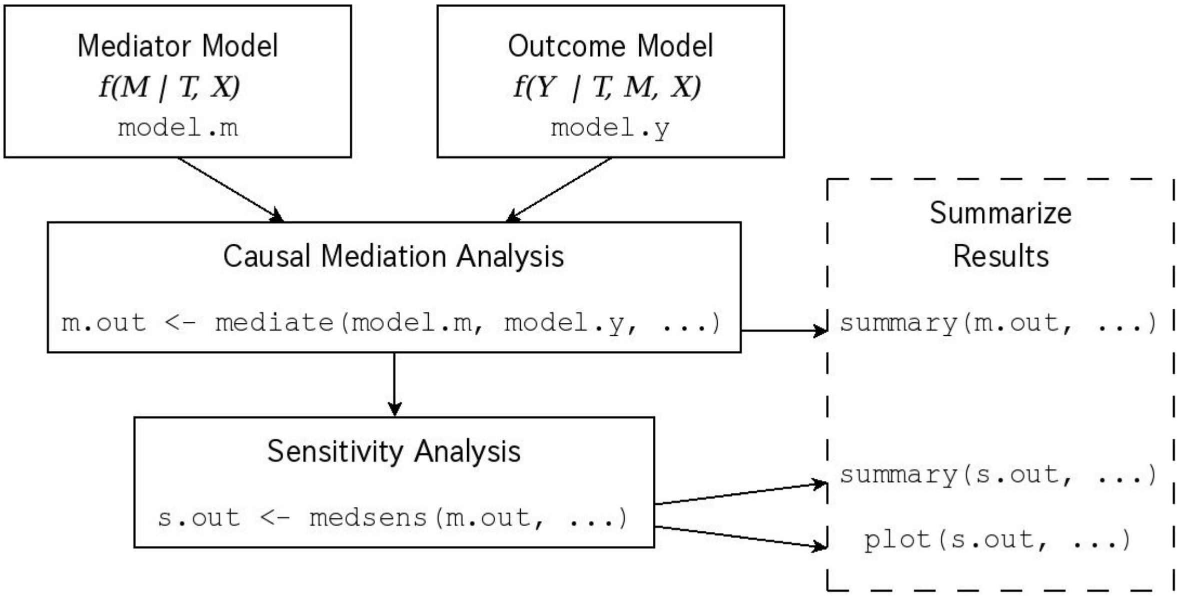
EXAMPLE Incumbency advantage

- 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

- Even in a randomized experiment, a strong assumption is needed to identify causal mechanisms
- However, progress can be made toward this fundamental goal of scientific research with modern statistical tools
- A general, flexible estimation method is available once we assume sequential ignorability
- Sequential ignorability can be probed via sensitivity analysis
- More credible inferences are possible using clever experimental designs
- Insights from new experimental designs can be directly applied when designing observational studies

Overview of **R** Package **mediation**



Mediator Model

$f(M \mid T, X)$

`model.m`

Outcome Model

$f(Y \mid T, M, X)$

`model.y`

Causal Mediation Analysis

`m.out <- mediate(model.m, model.y, ...)`

Sensitivity Analysis

`s.out <- medsens(m.out, ...)`

Summarize Results

`summary(m.out, ...)`

`summary(s.out, ...)`

`plot(s.out, ...)`

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")
```

Data Types Available via **mediation** (For Now)

- For the `mediate()` function:

	Outcome		
Mediator	Continuous	Ordered	Binary
Continuous	Yes	No	Yes
Ordered	Yes	No	No
Binary	Yes	No	Yes

- Can also deal with interaction between treatment and mediator, semiparametric regression, and quantile regression
- For the `medsens()` function:

	Outcome		
Mediator	Continuous	Ordered	Binary
Continuous	Yes	No	Yes
Ordered	No	No	No
Binary	Yes	No	No