Sensitivity Analyses

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Sensitivity

What is “sensitivity”?

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When inputs change, do outputs change?

- With different variables in model, does parameter of interest change?
- With different assumptions about error structures, does causal mediation estimate change?
- With different data collected, would causal conclusion change?

Sensitivity to Model Specification

Should we trust our model?

Estimating all possible regressions

Idea

Example 1

- “Great Recession” following global financial crisis of 2008-2009 (“subprime mortage crisis”)

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Example 1

- “Great Recession” following global financial crisis of 2008-2009 (“subprime mortage crisis”)
- Two big bills in US Congress to shore up US auto industry
  - Auto Bailout:
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Moore, Powell, and Reeves (2013) estimate relationship between presence of auto factories and Congressional votes these 2 quasi-private, particularistic bills.

Claim: Local econ interests at least on par with corporate campaign contributions, corporate lobbying, corporate public positions.

Moore, Powell, and Reeves (2013)

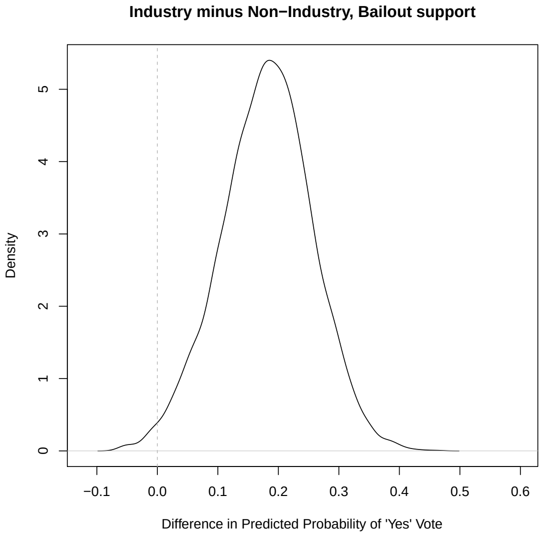

Figure 1: First differences for predicted probabilities of member supporting the auto bailout, comparing member from industry

Moore, Powell, and Reeves (2013)

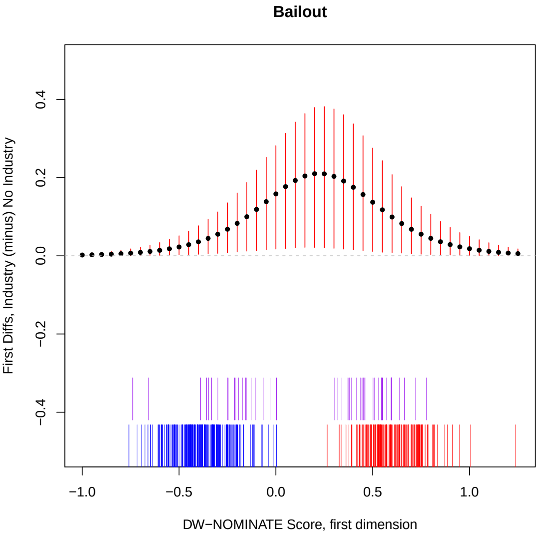

Figure 2: First differences between industry and non-industry district members' probabilities of supporting the bailout remain positive at

Moore, Powell, and Reeves (2013)

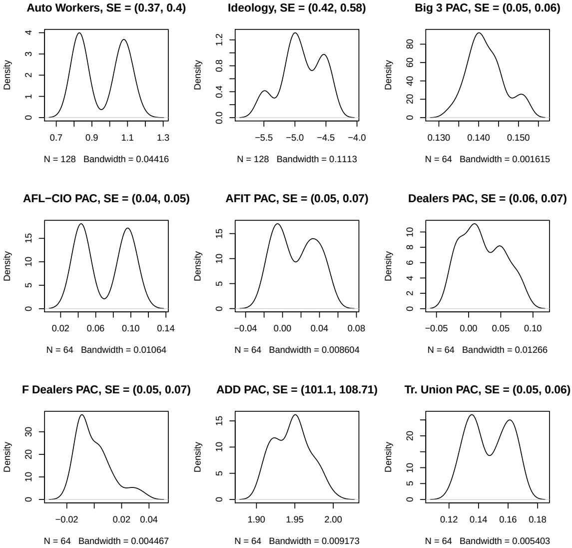

Figure 3: Industry presence coefficient always positive in Bailout vote logistic regressions. Coefficient densities with industry presence and

Moore, Powell, and Reeves (2013)

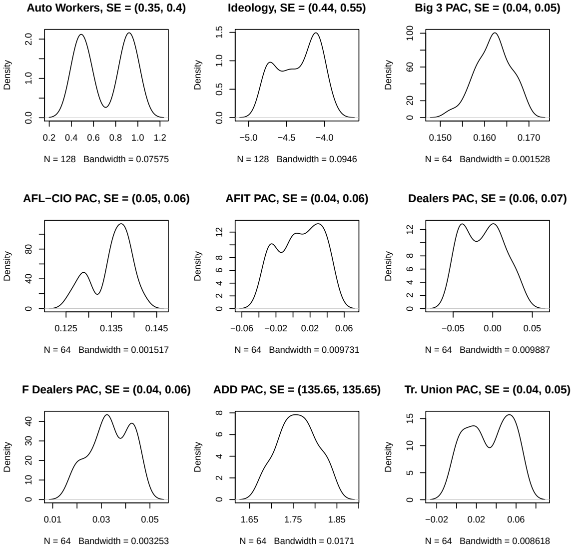

Figure 4: Industry presence coefficient always positive in Cash for Clunkers vote logistic regressions. Coefficient densities with industry

Implementation

Hebbali (2024)

```r
library(olsrr)
```

Example 2

```
library(qss)
data(social)

social <- social |> mutate(
  age = 2006 - yearofbirth,
  age_c = age - mean(age),
  messages = fct_relevel(messages, "Control")
)

head(social)
```

```
     sex yearofbirth primary2004   messages primary2006 hhs
1   male        1941           0 Civic Duty           0
2 female        1947           0 Civic Duty           0
3   male        1951           0  Hawthorne           1
4 female        1950           0  Hawthorne           1
5 female        1982           0  Hawthorne           1
6   male        1981           0    Control           0
```

Example 2

```r
lm_out <- lm(primary2006 ~ messages + sex + age_c +
               primary2004 + hhsize, data = social)

all_lm_social <- ols_step_all_possible(lm_out)$result
```

Example 2

```r
all_lm_social_coefs <- ols_step_all_possible_betas(lm_out)

all_lm_social_coefs
```

```
   model         predictor        beta
1      1       (Intercept)  0.2966383083
2      1 messagesCivic Duty  0.0178993441
3      1  messagesHawthorne  0.0257363121
4      1  messagesNeighbors  0.0813099129
5      2       (Intercept)  0.3059095493
6      2           sexmale  0.0126509479
7      3       (Intercept)  0.3122445777
8      3             age_c  0.0041515670
9      4       (Intercept)  0.2508820413
10     4       primary2004  0.1528795252
11     5       (Intercept)  0.3763534949
12     5            hhsize -0.0293482475
13     6       (Intercept)  0.2902800648
```

Example 2

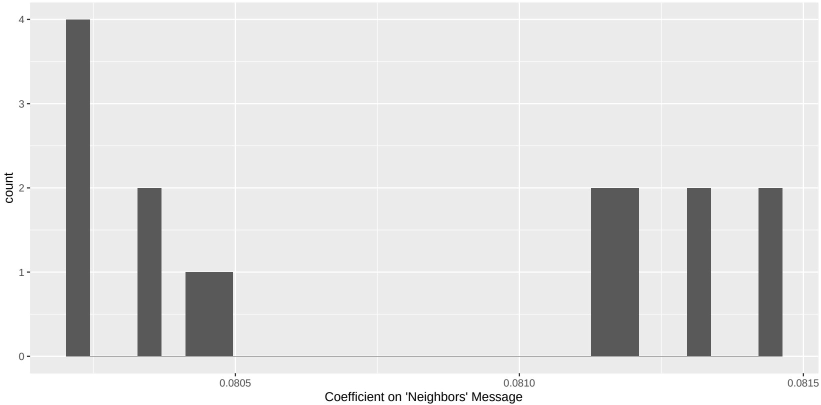

Figure 5: Coefficients from All Possible Regressions

```
   Min. 1st Qu.  Median    Mean 3rd Qu.    Max. 
0.08023 0.08032 0.08081 0.08080 0.08122 0.08145 
```

All Coefficients

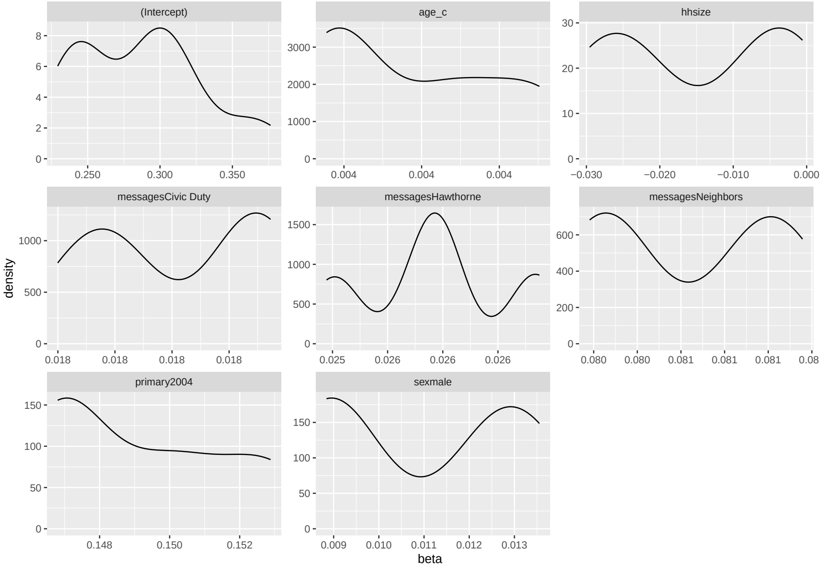

Figure 6: **?(caption)**

Matching as Preprocessing

- minimize effects of model-based adjustment (subclassify, match)

"model-based adjustments ...will give basically the same point estimates"

Matching as Preprocessing

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"model-based adjustments ...will give basically the same point estimates"

What does this mean?

Ho et al. (2007)

"Matching as Nonparametric Preprocessing for Reducing Model Dependence in Parametric Causal Inference"

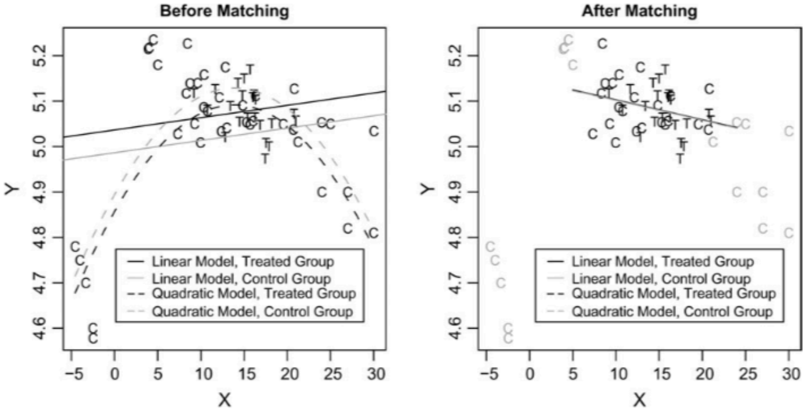


Figure 7: Here

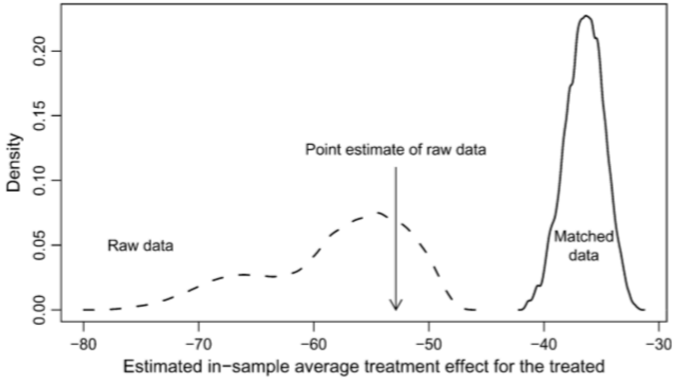


Fig. 2 Kernel density plot (a smoothed histogram) of point estimates of the in-sample ATT of the Democratic Senate majority on FDA drug approval time across 262,143 specifications. The solid line

Figure 8: Here

How to Identify Problem?

Different distributions; non-overlap

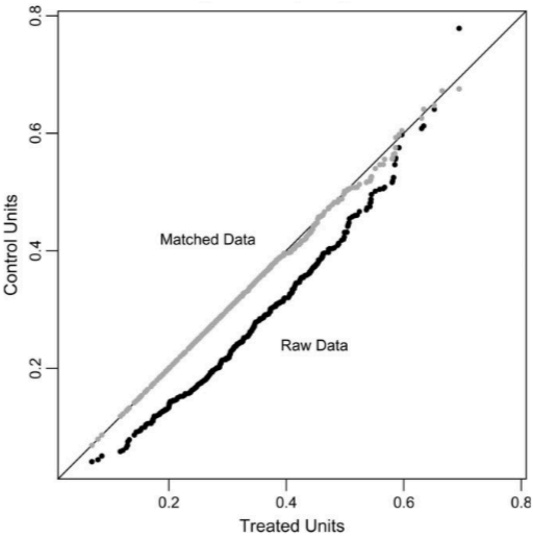


Fig. 3 QQ plot of propensity score for candidate visibility. The black dots represent empirical QQ

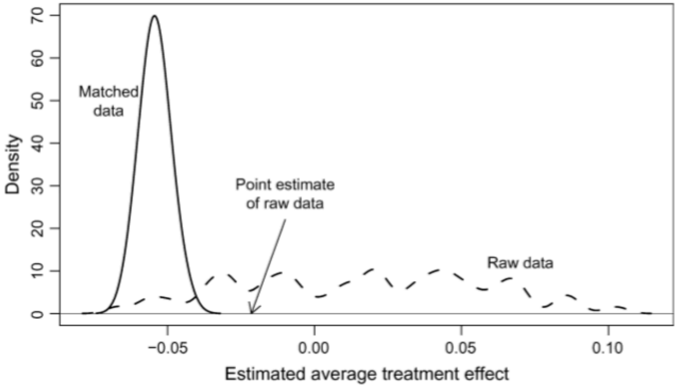


Fig. 4 Kernel density plot of point estimates of the effect of being a less visible male Republican candidate across 63 possible specifications with the Koch data. The dashed line presents estimates for

Paradox of Regression for causal inference?

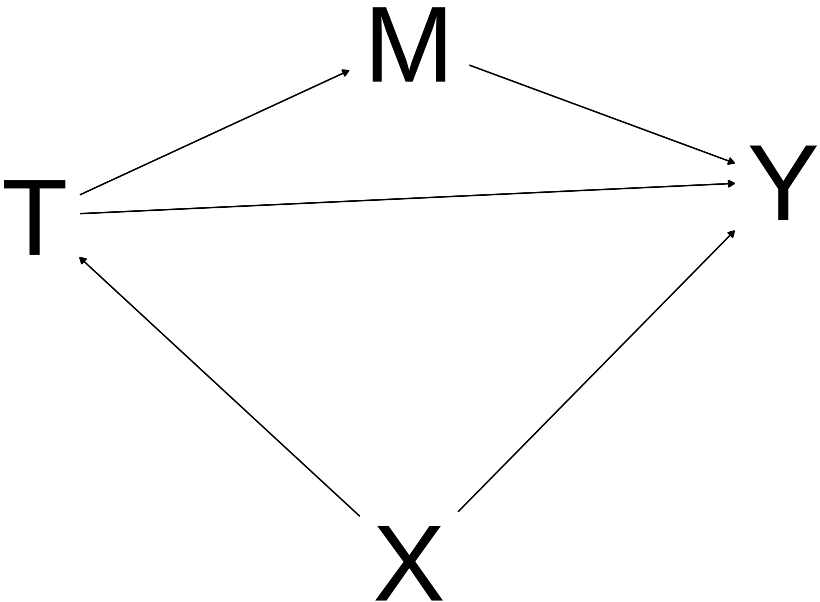
- If diffs large, regression not enough, very sensitive
- If diffs small, regression won't matter much
- Ho et al. (2007)

Matching as Preprocessing for Dynamic Treatment Regimes

Blackwell and Strezhnev (2022)

Sensitivity to an Unidentifiable Parameter

Confounding in Observational Studies



Mediation Effects

- If interest is $M \to Y$, seek experiment-like M

Mediation Effects

- If interest is $M \to Y$, seek experiment-like M
  - random M

Mediation Effects

- If interest is $M \to Y$, seek experiment-like M
  - random M
  - subclassify/match for M

Mediation Effects

- If interest is $M \rightarrow Y$, seek experiment-like M
  - random M
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- In *mediation*, interest is $T \to M \to Y$

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- In *mediation*, interest is $T \to M \to Y$
  - (and maybe $T \to (\neg M) \to Y$)

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Condition on /control for M?

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- No: how to estimate $M \to Y$?

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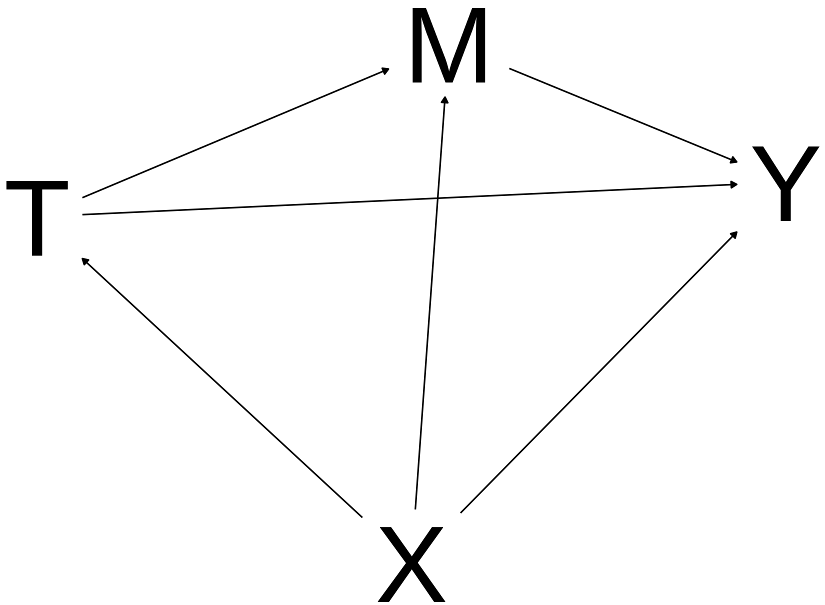
- And if $X \to M$, too?

Mediation Effects

Condition on /control for M?

- No: how to estimate $M \to Y$?
- Yes: induces *post-treatment bias* in estimate of $T \to Y$

- And if $X \to M$, too?
- Even worse ...


M
T
Y
X

Addressing Confounding

To break confounding,

- can't break $X \rightarrow Y$

-> -> -> -> -> -> ->

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Addressing Confounding

To break confounding,

- can't break $X \to Y$
- break $X \to T$
- but $X \to M$ may still remain!

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Notation

–> –>

–> –> –> –> –> –> –> –> –> –> –> –> –> –> –> –> –>

–> –>

- $M_i(t)$: value of the mediator (function of treatment)

Notation

–> –>

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- $M_i(t)$: value of the mediator (function of treatment)
- $Y_i(t, m)$: potential outcome under some combination of t, m

Notation

–> –>

–> –> –> –> –> –> –> –> –> –> –> –> –> –> –> –> –>

–> –>

- $M_i(t)$: value of the mediator (function of treatment)
- $Y_i(t, m)$: potential outcome under some combination of t, m
- $Y_i(T_i, M_i(T_i))$: potential outcome under
  - $T_i = t$
  - M_i you would get with $T_i = t$

Notation

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- Quiz:

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  - $T_i = t$
  - M_i you would get with $T_i = t$
- Quiz: In news/anxiety/attitude example,
  - what's $Y_i(1, M_i(1))$?
  - what's $Y_i(0, M_i(0))$?
  - what's $Y_i(1, M_i(1)) - Y_i(0, M_i(0))$?
  - what's $Y_i(1, M_i(0))$?

Notation: Causal Effects

–> –> –> –> –> –> –> –> –>

For individuals:

- $Y_i(1, M_i(1)) - Y_i(0, M_i(0))$: Total effect

Notation: Causal Effects

–> –> –> –> –> –> –> –> –>

For individuals:

- $Y_i(1, M_i(1)) - Y_i(0, M_i(0))$: Total effect
- $Y_i(0, M_i(1)) - Y_i(0, M_i(0)) \equiv \delta_i(0)$: Indirect/Mediation effect under Co
- $Y_i(1, M_i(1)) - Y_i(1, M_i(0)) \equiv \delta_i(1)$: Indirect/Mediation effect under Tr

Notation: Causal Effects

-> -> -> -> -> -> -> -> ->

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- ACMEs: $\bar{\delta}(1)$ and $\bar{\delta}(0)$

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- ACMEs: $\bar{\delta}(1)$ and $\bar{\delta}(0)$
- $Y_i(1, M_i(0)) - Y_i(0, M_i(0)) \equiv \zeta_i(0)$: Direct effect of Tr on Y, under mediator value as if control
- $Y_i(1, M_i(1)) - Y_i(0, M_i(1)) \equiv \zeta_i(1)$: Direct effect of Tr on Y, under mediator value as if treated

Notation: Causal Effects

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- ADEs: $\bar{\zeta}(1)$ and $\bar{\zeta}(0)$

-> -> -> -> -> -> -> -> -> -> -> -> ->

-> -> ->

->

Are 2 Experiments Enough for Mediation CEs?

- Exp. 1: Randomize T_i, measure M_i, get "ACE of T on M"

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- Then, combine somehow, get ACME/Indir. effect of T on Y via M?

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  - Unbiased estimate

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- Then, combine somehow, get ACME/Indir. effect of T on Y via M?
- But, this doesn't get you
  - Unbiased estimate
  - Sign of ACME
  - Informative bounds for ACME!

Usual (Best-Case?) Way to "Combine Somehow"

"Baron & Kenny Procedure"

$$
\begin{aligned}
M_i &= \alpha_1 + aT_i + \epsilon_{i1} && (1)\\
Y_i &= \alpha_2 + cT_i + \epsilon_{i2} && (2)\\
Y_i &= \alpha_3 + dT_i + bM_i + \epsilon_{i3} && (3)
\end{aligned}
$$

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(Can add $+\mathbf{e}_1 X_i$, $+\mathbf{e}_2 X_i$, $+\mathbf{e}_3 X_i$.)

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(Can add $+\mathbf{e}_1 X_i$, $+\mathbf{e}_2 X_i$, $+\mathbf{e}_3 X_i$.)

Then, call effect of

$$\begin{aligned} T \to M &= a \\ T \to Y &= c && \text{(Total)} \\ T \to Y &= d && \text{(Direct)} \\ M \to Y &= b \\ T \to M \to Y &= c - d = ab && \text{(Mediation)} \end{aligned}$$

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Problem: This doesn't work.

Why Aren't 2 Experiments Enough?

-> -> -> -> -> -> -> -> -> -> ->

TABLE 1. The Fallacy of the Causal Chain Approach

Population Proportion	Potential Mediators and Outcomes $M_i(1)$	$M_i(0)$	$Y_i(t, 1)$	$Y_i(t, 0)$	Treatment Effect on Mediator $M_i(1) - M_i(0)$	Mediator Effect on Outcome $Y_i(t, 1) - Y_i(t, 0)$	Causal Mediation Effect $Y_i(t, M_i(1)) - Y_i(t, M_i(0))$
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

Notes: The left five columns of the table show a hypothetical population proportion of "types" of units defined by the values of potential mediators and outcomes. Note that these values can never be jointly observed. The last row of the table shows the population average value of each column. In this example, the average causal effect of the treatment on the mediator (the sixth column) is positive and equal to 0.2. Moreover, the average causal effect of the mediator on the outcome (the seventh column) is also positive and equals 0.2. And yet the average causal mediation effect (ACME; final column) is negative and equals −0.2.

Why Aren't 2 Experiments Enough?

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TABLE 1. The Fallacy of the Causal Chain Approach

Population Proportion	Potential Mediators and Outcomes				Treatment Effect on Mediator	Mediator Effect on Outcome	Causal Mediation Effect
	$M_i(1)$	$M_i(0)$	$Y_i(t, 1)$	$Y_i(t, 0)$	$M_i(1) - M_i(0)$	$Y_i(t, 1) - Y_i(t, 0)$	$Y_i(t, M_i(1)) - Y_i(t, M_i(0))$
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
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\begin{aligned}
T \rightarrow M &= a = 0.2 \\
M \rightarrow Y &= b = 0.2 \\
T \rightarrow M \rightarrow Y &= ab = 0.04
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$$

But, true $\bar{\delta}(t)$, ACME, $= -0.2$!

What Else Do You Need?

Consistency assumption: $T_i = t$, $M_i = m$ have same effect regardless of how they came to have those values.

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(Using lottery to estimate effect of income on attitude requires **lottery income** to have same effect as **regular income**.)

The ACME, e.g., is an estimate of the effect of changes in M due to changing T (but without changing T).

(Other manipulations of M rely on consistency.)

What Else Do You Need?

Big picture: to get more detailed estimates from same data, need more assumptions

Assumption 1 [Sequential Ignorability (Imai, Keele, and Yamamoto 2010)].

$$\{Y_i(t', m), M_i(t)\} \perp\!\!\!\perp T_i \mid X_i = x, \tag{3}$$

$$Y_i(t', m) \perp\!\!\!\perp M_i(t) \mid T_i = t, X_i = x, \tag{4}$$

where $0 < \Pr(T_i = t \mid X_i = x)$ and $0 < p(M_i = m \mid T_i = t, X_i = x)$ for $t = 0, 1$, and all x and m in the support of X_i and M_i, respectively.

What Else Do You Need?

- Eqn 3: Conditional independence of PotOut's from Tr, given X (pretreatment!)
  - Ok, for random T, or balanced obs design. T as good as random, exog., etc.
  - (t' is just saying, for each $t = 0, 1$, must have Y's from both $t = 0, 1$ must be indep.)
- Eqn 4: Hard. Mediator is as good as random, given particular Tr status
- Problem: can't randomize *both* T and M in same experiment
  - (if want effect of T through M)
- You're getting 2 different QoI's if you randomize both: $T \to M, Y$ and $M \to Y$.
  - Showed can't combine those into $T \to M \to Y$

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Sensitivity for Mediation Effects

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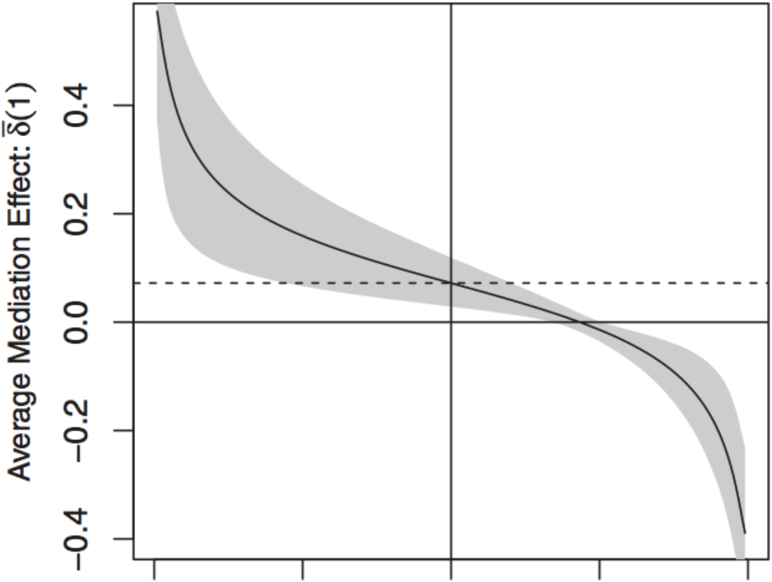

Summary

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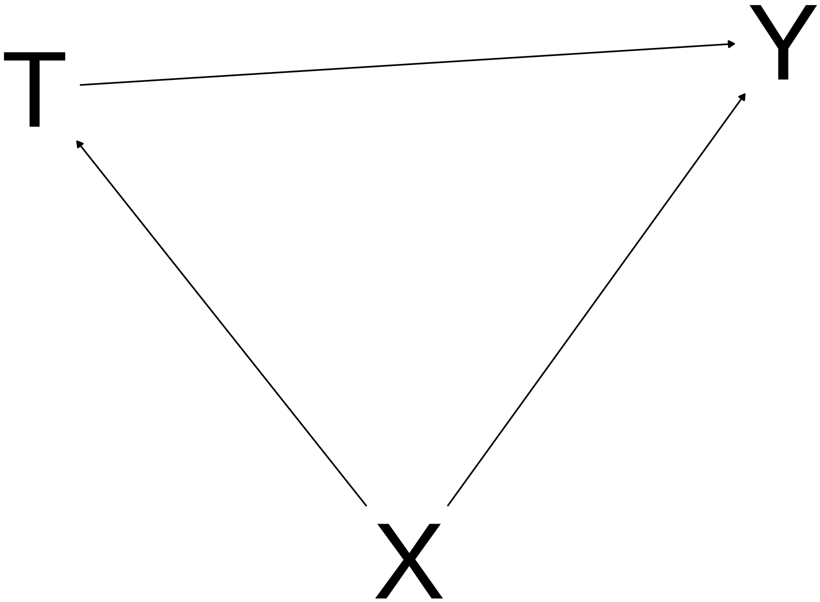
Summary

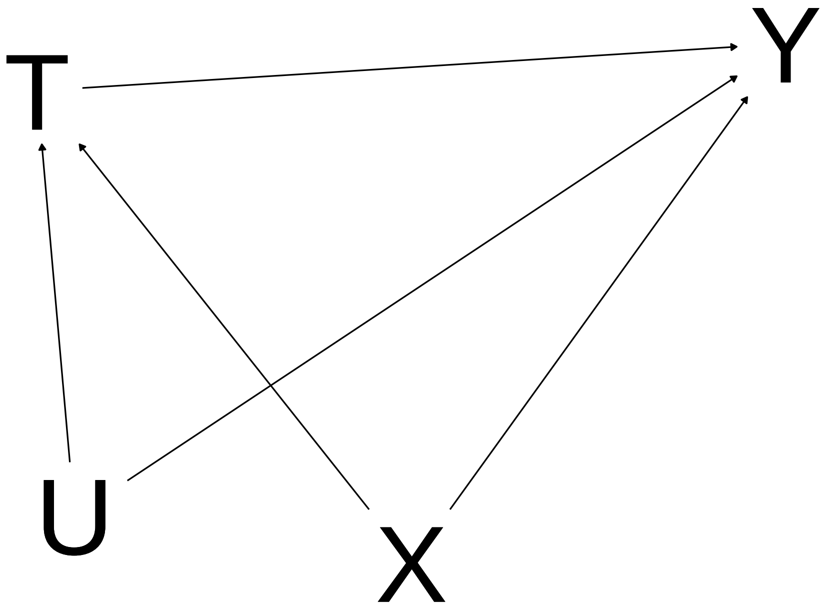
- Be careful. If you estimate, you must do sensitivity.
  - A serious case of "don't just get an answer"
  - (Do `plot(lm_out)`, too ...)
- Imai et al. (2011) thorough on assumptions, when trouble, when sensitivity is OK, when identification can be done
- From Bullock, Green, and Ha (2010):

> a cumulative enterprise. Persuasive conclusions about mediation are difficult to reach under any circumstances, but they are most likely to be reached when they derive from an experimental research program that addresses the particular challenges of mediation analysis—challenges that we describe here.

Sensitivity to an Unobserved Covariates

Confounding in Observational Studies




T
Y
U
X

Addressing Confounding

To break confounding,

- can't break $X \to Y$
- break $X \to T$
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(Of course, if no causal effect of $U \to Y$, no problem.)

Hidden Bias

Where there is $U \to T$ and $U \to Y$, there is *hidden bias*.

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but are different in prop score:

$$\pi_i \neq \pi_j$$

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Sensitivity: how strong must sociability be to invalidate inference about phone calls?

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	% Below Pov Line			% Above Pov Line		
	B	W	$\frac{B}{W}$	B	W	$\frac{B}{W}$
t_1	90	80	1.1	10	20	0.5

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Clearly, huge absolute improvements.

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- (Easy to produce examples of OR's same and AbsDiffs slightly diff.)
- (Diffs betwn groups real, importnt, but how we meas. changes is tricky)

King's Conjecture

17 October 2012

Modeling Hidden Bias

Odds of treatment for i and j:

$$\frac{\pi_i}{1-\pi_i}, \frac{\pi_j}{1-\pi_j}$$

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$$\frac{\pi_i}{1-\pi_i}, \frac{\pi_j}{1-\pi_j}$$

OR of i versus j:

$$\begin{aligned} OR &= \frac{\pi_i}{1-\pi_i} \div \frac{\pi_j}{1-\pi_j} \\ &= \frac{\pi_i(1-\pi_j)}{\pi_j(1-\pi_i)} \end{aligned}$$

Modeling Hidden Bias

Let Γ be upper bound on OR of treatment.

$$\frac{1}{\Gamma} \leq \frac{\pi_i(1-\pi_j)}{\pi_j(1-\pi_i)} \leq \Gamma \qquad \forall i, j \text{ s.t. } \mathbf{x}_i = \mathbf{x}_j$$

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By what factor does the odds of treatment differ? (No more than Γ)

Modeling Hidden Bias

Rosenbaum (2020) shows that this is same as

$$\begin{aligned}
\log\left(\frac{\pi_i}{1-\pi_i}\right) &= \kappa(\mathbf{x}_i) + \gamma u_i \\
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s.t. $0 \leq u_i \leq 1$.

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s.t. $0 \leq u_i \leq 1$.

Interpretation: first rewrite

$$\log\left(\frac{\pi_j}{1-\pi_j}\right) = \kappa(\mathbf{x}_i) + \gamma u_j$$

Exponentiate:

$$\begin{aligned}
\left(\frac{\pi_i}{1-\pi_i}\right) &= e^{\kappa(\mathbf{x}_i)+\gamma u_i} \\
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Exponentiate:

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Calculate OR:

$$\begin{aligned}OR &= \frac{\pi_i(1-\pi_j)}{\pi_j(1-\pi_i)} \\ &= \frac{e^{\kappa(\mathbf{x}_i)+\gamma u_i}}{e^{\kappa(\mathbf{x}_i)+\gamma u_j}} \\ &= e^{(\kappa(\mathbf{x}_i)+\gamma u_i)-(\kappa(\mathbf{x}_i)+\gamma u_j)} \\ &= e^{(\gamma u_i-\gamma u_j)} \\ &= e^{\gamma(u_i-u_j)}\end{aligned}$$

Interpreting Γ

$$OR = e^{\gamma(u_i - u_j)}$$

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Shows $\Gamma = e^{\gamma}$.

TABLE 4.1. Sensitivity Analysis for Hammond's Study of Smoking and Lung Cancer: Range of Significance Levels for Hidden Biases of Various Magnitudes.

Γ	Minimum	Maximum
1	< 0.0001	< 0.0001
2	< 0.0001	< 0.0001
3	< 0.0001	< 0.0001
4	< 0.0001	0.0036
5	< 0.0001	0.03
6	< 0.0001	0.1

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4	< 0.0001	0.0036
5	< 0.0001	0.03
6	< 0.0001	0.1

- Groups: smokers/nonsmokers
- Outcome: lung cancer
- Something must increase smoking by $6\times$ to change inference.
- If exists, maybe it's that factor, not smoking directly.

(Bias from $U \to T$; effectively, $U \to Y$ nearly perfect.)

Γ	Minimum	Maximum
1	≤ 0.0001	≤ 0.0001
2	≤ 0.0001	0.0018
3	≤ 0.0001	0.0136
4	≤ 0.0001	0.0388
4.25	≤ 0.0001	0.0468
5	≤ 0.0001	0.0740

Table 4.2: Signed-Rank Statistic p-value Sensitivity for Lead in Children's Blood

- Groups: parents occupationally exposed/unexposed
- Outcome: children's levels
- Something must increase parents' exposure by $5\times$ to change inference.
- If exists, maybe it's that, not parental exposure directly.

Γ	Minimum	Maximum
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(one-sided)

Γ	Minimum	Maximum
1	15	15
2	10.25	19.5
3	8	23
4	6.5	25
5	5	26.5

Table 4.3: Point Estimate Sensitivity for Lead in Children's Blood

Γ	Minimum	Maximum
1	15	15
2	10.25	19.5
3	8	23
4	6.5	25
5	5	26.5

Table 4.3: Point Estimate Sensitivity for Lead in Children's Blood

- HL point estimate: 15 (median of all $m \times n$ possible matched pairs)
- With confounding, wider range of possible effects.

Γ	95% CI
1	(9.5, 20.5)
2	(4.5, 27.5)
3	(1.0, 32.0)
4	(-1.0, 36.5)
5	(-3.0, 41.5)

Table 4.4: Confidence Interval Sensitivity for Lead in Children's Blood

Γ	95% CI
1	(9.5, 20.5)
2	(4.5, 27.5)
3	(1.0, 32.0)
4	(-1.0, 36.5)
5	(-3.0, 41.5)

Table 4.4: Confidence Interval Sensitivity for Lead in Children's Blood

- Inverted NHST CI's
- If something increases parental exposure by $4\times$, negative estimates of parents on children are reasonable.

(two-sided)

Implementation

Packages

- Frank et al. (2013): `konfound`
- Keele (2022): `rbounds`
- `sensitivitymw`
- `sensitivitymv`

Example

```r
anes <- read_csv("../data/anes_pilot_2016.csv")
dim(anes)
```

```
[1] 1200  594
```

```r
anes <- anes |> mutate(age = 2016 - birthyr,
                       pid_rep = as.numeric(pid3 == 3),
                       pid_dem = as.numeric(pid3 == 1))
```

```r
lm_out <- lm(turnout12 ~ pid_rep, data = anes)
summary(lm_out)
```

```

Call:
lm(formula = turnout12 ~ pid_rep, data = anes)

Residuals:
    Min      1Q  Median      3Q     Max 
-0.3395 -0.2451 -0.2451 -0.2451  1.7549 

Coefficients:
            Estimate Std. Error t value Pr(>|t|)    
(Intercept)  1.24512    0.01868  66.641  < 2e-16 ***
pid_rep      0.09435    0.03320   2.842  0.00456 ** 
---
Signif. codes:  0 '***' 0.001 '**' 0.01 '*' 0.05 '.' 0.1 '

Residual standard error: 0.535 on 1198 degrees of freedom
Multiple R-squared:  0.006695,	Adjusted R-squared:  0.005
```

```r
library(konfound)
konfound(lm_out, pid_rep)
```

```r
library(konfound)
konfound(lm_out, pid_rep)
```

```
Robustness of Inference to Replacement (RIR):
To invalidate an inference, 30.959 % of the estimate would
have to be due to bias.
This is based on a threshold of 0.065 for statistical
significance (alpha = 0.05).

To invalidate an inference, 372 observations would have to
be replaced with cases
for which the effect is 0 (RIR = 372).

See Frank et al. (2013) for a description of the method.

Citation: Frank, K.A., Maroulis, S., Duong, M., and Kelcey,
B. (2013).
What would it take to change an inference?
Using Rubin's causal model to interpret the
robustness of causal inferences.
```

```r
lm_out <- lm(turnout12 ~ pid_rep + age, data = anes)
summary(lm_out)
```

```

Call:
lm(formula = turnout12 ~ pid_rep + age, data = anes)

Residuals:
    Min      1Q  Median      3Q     Max 
-0.5825 -0.3388 -0.1711  0.0301  1.9831 

Coefficients:
             Estimate Std. Error t value Pr(>|t|)    
(Intercept)  1.678649   0.045960  36.524  < 2e-16 ***
pid_rep      0.082685   0.031870   2.594  0.00959 ** 
age         -0.008943   0.000873 -10.244  < 2e-16 ***
---
Signif. codes:  0 '***' 0.001 '**' 0.01 '*' 0.05 '.' 0.1 '

Residual standard error: 0.5132 on 1197 degrees of freedom
```

```r
konfound(lm_out, pid_rep)
```

```
Robustness of Inference to Replacement (RIR):
To invalidate an inference, 24.379 % of the estimate would
have to be due to bias.
This is based on a threshold of 0.063 for statistical
significance (alpha = 0.05).

To invalidate an inference, 293 observations would have to
be replaced with cases
for which the effect is 0 (RIR = 293).

See Frank et al. (2013) for a description of the method.

Citation: Frank, K.A., Maroulis, S., Duong, M., and Kelcey,
B. (2013).
What would it take to change an inference?
Using Rubin's causal model to interpret the
robustness of causal inferences.
Education, Evaluation and
```

```r
cor(anes[,c("pid_rep", "turnout12", "econnow")])
```

```
            pid_rep   turnout12     econnow
pid_rep   1.00000000 0.081825966 0.141257803
turnout12 0.08182597 1.000000000 0.008599061
econnow   0.14125780 0.008599061 1.000000000
```

```r
lm_out <- lm(turnout12 ~ pid_rep + age + econnow, data = ar
summary(lm_out)
```

```
Call:
lm(formula = turnout12 ~ pid_rep + age + econnow, data = ar

Residuals:
     Min       1Q   Median       3Q      Max 
-0.60257 -0.33748 -0.17138  0.04458  1.96702 

Coefficients:
              Estimate Std. Error t value Pr(>|t|)    
(Intercept)  1.6290966  0.0565381  28.814   <2e-16 ***
pid_rep      0.0755031  0.0322095   2.344   0.0192 *  
age         -0.0091496  0.0008833 -10.358   <2e-16 ***
econnow      0.0202398  0.0134633   1.503   0.1330    
---
Signif. codes:  0 '***' 0.001 '**' 0.01 '*' 0.05 '.' 0.1 '
```

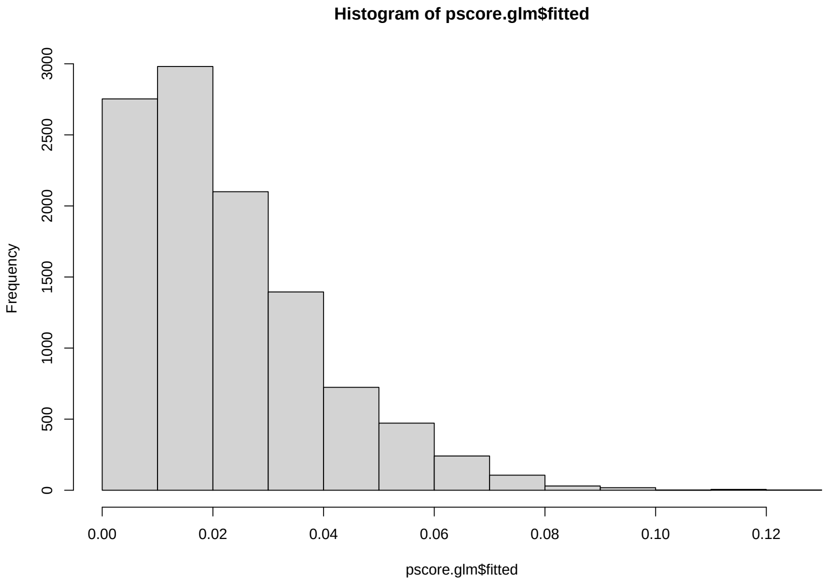
```r
konfound(lm_out, pid_rep)
```

```
Robustness of Inference to Replacement (RIR):
To invalidate an inference, 16.303 % of the estimate would
have to be due to bias.
This is based on a threshold of 0.063 for statistical
significance (alpha = 0.05).

To invalidate an inference, 196 observations would have to
be replaced with cases
for which the effect is 0 (RIR = 196).

See Frank et al. (2013) for a description of the method.

Citation: Frank, K.A., Maroulis, S., Duong, M., and Kelcey,
B. (2013).
What would it take to change an inference?
Using Rubin's causal model to interpret the
robustness of causal inferences.
Education, Evaluation and
```

Implementation in rbounds

```
library(Matching)
data(GerberGreenImai)

# Estimate Propensity Score
pscore.glm <- glm(PHN.C1 ~ PERSONS + VOTE96.1 +
                    NEW + MAJORPTY + AGE + WARD +
                    PERSONS:VOTE96.1 + PERSONS:NEW +
                    AGE2, family = binomial(logit),
                  data = GerberGreenImai)
```

```r
hist(pscore.glm$fitted)
```



Implementation in rbounds

```
# Match - without replacement
m.obj <- Match(Y = GerberGreenImai$VOTED98,
               Tr = GerberGreenImai$PHN.C1,
               X = fitted(pscore.glm), M = 1, replace = FAL

summary(m.obj)
```

```
Estimate...  0.08502
SE.........  0.039918
T-stat.....  2.1299
p.val......  0.033182

Original number of observations..............  10829
Original number of treated obs...............  247
Matched number of observations...............  247
Matched number of observations  (unweighted).  247
```

Implementation in `rbounds`

```r
library(rbounds)

# Sensitivity Test
# binarysens(m.obj, Gamma = 2, GammaInc = .1)
```

Implementation in `rbounds`

```
#hlsens(m.obj, Gamma = 5, GammaInc = 1)
```

Thanks!

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