Some Counterclaims Undermine Themselves in Observational Studies

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Claims and counterclaims

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Proof by contradiction: I argue for $\mathcal{T}$ by showing that $\sim \mathcal{T}$ leads to a contradiction. The supposition that $\sim \mathcal{T}$ undermines itself.
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- A claim $\mathcal{T}$ and counterclaims to $\mathcal{T}$ may be offered by different people, say an investigator and a critic.
- Or an investigator may anticipate certain counterclaims to $\mathcal{T}$ and try to strengthen the case for $\mathcal{T}$ by refuting or rendering implausible various counterclaims to $\mathcal{T}$. 
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- Could empirical evaluation of such a counterclaim show that it fails as a counterclaim? That it does not make the original claim less plausible.
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A critic (or the investigator anticipating a critic) raises a specific counterclaim. The investigator shows that, if one were to suppose the counterclaim to be true, it would be appropriate to perform an additional, otherwise inappropriate analysis, with the finding the results are insensitive to a bias of magnitude $\Gamma_0 > \Gamma$. In this sense, the counterclaim undermines itself. It fails in its role as a counterclaim.
General structure

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Outline of talk

- A preliminary fact: In most circumstances, you should adjust for covariates, not for outcomes.
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Is what we saw in the example expected under simple models for treatment effects? (Design sensitivity and power of a sensitivity analysis.)
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- The preliminary fact: adjusting for an outcome can bias an otherwise unbiased estimate of a treatment effect.
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- If the drug worked by lowering your blood pressure so that you had the same low risk of stroke as a person with naturally low blood pressure, that might be a large effect, and you might mistakenly remove it.
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- The system records information about injuries and deaths, safety belt use, direction of impact, ejection from vehicle, and is connected to detailed information about vehicles.

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Counterclaims
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The system records information about injuries and deaths, safety belt use, direction of impact, ejection from vehicle, and is connected to detailed information about vehicles.

The system has little information about events leading up to the crash: speeds, distances between vehicles, road traction, driver performance, condition of brakes, etc, all of which affect the forces involved in the crash.
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- If people who wear safety belts drive more slowly, at a greater distance from the car ahead, etc, then people who wear safety belts may be involved in less severe crashes.
- If there are fewer deaths and less severe injuries when people wear safety belts, part of this may not be an effect caused by the belts, but rather the aggregate effect of a cautious manner of driving.
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- If there are fewer deaths and less severe injuries when people wear safety belts, part of this may not be an effect caused by the belts, but rather the aggregate effect of a cautious manner of driving.
- What can be done?
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  3. People in Volvos and Mercedes are more likely to be belted than people in Fords.

People aged 18–30 are twice as likely as older individuals to be unbelted (odds ratio 2.1). Unbelted individuals were on average 9 years younger than belted individuals.
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- The key comparison, one belted, the other unbelted, is a sliver of the FARS system, because it is atypical for driver and passenger to differ in their belt use.
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- In Evan’s comparison, many unmeasured factors are controlled: same vehicle in same crash, driver and passenger traveled at the same speed, at the same distance from the car ahead, with the same road traction.
- The risks in the driver’s seat may differ from those in the passenger’s seat, but we see both cases.
A modern version of Evan’s comparison

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- There are really 4 parallel studies, one of (ls, ls), one of (n, ls), one of (ls, n) and one of (n, n).
- Notation will describe any one of the 4 studies, so the notation is recycled.
Each person has an injury score.
Injury scores

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- \( Y_i = \text{driver} - \text{passenger} \) difference in injury scores, from -4 to 4. So a -4 means the driver was not injured but the passenger died.
Figure 1: Pair differences in injury scores, driver-minus-passenger, for a driver and a passenger in the same car in FARS 2010-2011, by restraint use. A positive difference indicates the driver suffered more severe injuries than the passenger.
Summary so far:

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- However, injury scores are lower for belted individuals when only one person is belted.
- Can’t explain this pattern with the vehicle, its speed, brake quality, driver caution, etc.

What about age? In the front seat of the same car, the mean age (driver-minus-passenger) differences are small:

- (ls, ls) is 0.36 years
- (n, n) is 0.59 years
- (ls, n) is 0.98 years
- (n, ls) is 1.34 years.

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Notation for any one of our 4 studies (e.g., (ls, n), etc.)

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- Set $i$ contains one treated with $Z_{ij} = 1$, the rest untreated controls with $Z_{ij} = 0$, so $1 = \sum_{j \in \mathcal{J}_i} Z_{ij}$ for each $i$. 

Write $Z = (Z_{11}, Z_{12}, \ldots, Z_{IJ})^T$ for the vector of dimension $n = \sum_{i \in \mathcal{I}} J_i$.

Let $Z$ be the set containing the $\prod_{i \in \mathcal{I}} J_i$ possible values of $Z$, so $z \in Z$ if $z$ is of dimension $n$ with $z_{ij} = 0$ or $z_{ij} = 1$ and $1 = \sum_{j \in \mathcal{J}_i} z_{ij}$ for each $i$.

Conditioning on $Z \in Z$ is abbreviated as conditioning on $Z$. Denote by $|A|$ the number of elements in a finite set $A$ so that, for instance, $|\mathcal{J}_i| = J_i$ and $|Z| = \prod_{i \in \mathcal{I}} J_i$. 

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- $I$ matched sets, $i \in \{1, \ldots, I\} = I$, where set $i \in I$ contains subjects $J_i = \{1, \ldots, J_i\}$, so $ij$ is a person. (In the example, $J_i = 2$ and $J_i = \{1, 2\}$ for all $i$.)

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- Write $Z = (Z_{11}, Z_{12}, \ldots, Z_{IJ})^T$ for the vector of dimension $n = \sum_{i \in I} J_i$.

- Let $Z$ be the set containing the $\prod_{i \in I} J_i$ possible values of $Z$, so $z \in Z$ if $z$ is of dimension $n$ with $z_{ij} = 0$ or $z_{ij} = 1$ and $1 = \sum_{j \in J_i} z_{ij}$ for each $i$. Conditioning on $Z \in Z$ is abbreviated as conditioning on $Z$. 
Notation for any one of our 4 studies (e.g., (ls, n), etc.)

- $I$ matched sets, $i \in \{1, \ldots, I\} = \mathcal{I}$, where set $i \in \mathcal{I}$ contains subjects $\mathcal{J}_i = \{1, \ldots, J_i\}$, so $ij$ is a person. (In the example, $J_i = 2$ and $\mathcal{J}_i = \{1, 2\}$ for all $i$.)

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- Denote by $|\mathcal{A}|$ the number of elements in a finite set $\mathcal{A}$ so that, for instance, $|\mathcal{J}_i| = J_i$ and $|\mathcal{Z}| = \prod_{i \in \mathcal{I}} J_i$. 

Rosenbaum

Counterclaims
Each subject is described by a measured covariate $x_{ij}$ and there is concern about an unmeasured covariate $u_{ij}$.
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Matching has controlled the measured covariate, so that $x_{ij} = x_{ik} = x_i$, say, for each $i, j, k$. 

Example: $u_{ij}$ is a measure of the frailty of individual $ij$, and there is concern that frail individuals are less likely to wear safety belts and more likely to suffer severe injuries or death.
Covariates

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Example: $u_{ij}$ is a measure of the frailty of individual $ij$, and there is concern that frail individuals are less likely to wear safety belts and more likely to suffer severe injuries or death.
Subject \( ij \) has two potential injury scores, \( r_{Tij} \) if assigned to treatment or \( r_{Cij} \) if assigned to control, so the observed response of \( ij \) is \( R_{ij} = Z_{ij} r_{Tij} + (1 - Z_{ij}) r_{Cij} \), and the effect of the treatment on \( ij \), namely \( r_{Tij} - r_{Cij} \) is not observed; see Neyman (1923) and Rubin (1974).
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Write $R$, $r_C$, $r_T$, and $u$ for the $n$ dimensional vectors.
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Write $R$, $r_C$, $r_T$, and $u$ for the $n$ dimensional vectors.

Each subject has a $K$-dimensional row vector of secondary outcomes, $s_{Tij}$ or $s_{Cij}$, with observed value $S_{ij} = Z_{ij} s_{Tij} + (1 - Z_{ij}) s_{Cij}$, and associated $n \times K$ matrices $S$, $s_C$ and $s_T$.
Outcomes (in each of our 4 parallel studies, e.g., (ls, n).)

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- Treated-minus-control pair difference $Y_i = (Z_{i1} - Z_{i2}) (R_{i1} - R_{i2})$ in outcomes.
Write
\[ \mathcal{F} = \{(r_{Tij}, r_{Cij}, s_{Tij}, s_{Cij}, x_{ij}, u_{ij}), \ i = 1, \ldots, I, \ j = 1, \ldots, J_i \}. \]
Write
\[ F = \{(r_{Tij}, r_{Cij}, s_{Tij}, s_{Cij}, x_{ij}, u_{ij}), i = 1, \ldots, I, j = 1, \ldots, J_i\}. \]

The subscripts \( ij \) are unique but noninformative identifiers, perhaps randomly assigned, and all information about individual \( ij \) is in observed or unobserved variables that describe \( ij \).
Randomization inference (in each of our 4 parallel studies, e.g., (ls, n).)

- If this were a randomized experiment, then we would, independently, assign treatment at random to one person in each matched set, so

\[
\Pr(Z = z \mid \mathcal{F}, \mathcal{Z}) = \prod_{i \in \mathcal{I}} J_i^{-1} = |\mathcal{Z}|^{-1} \text{ for each } z \in \mathcal{Z}.
\]
Randomization inference (in each of our 4 parallel studies, e.g., (ls, n).)

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- A test statistic \( t(Z, R) \).
Randomization inference (in each of our 4 parallel studies, e.g., (ls, n).)

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- A test statistic $t(\mathbf{Z}, \mathbf{R})$.

- In a randomized experiment, under Fisher's hypothesis of no effect, $H_0: r_{Tij} = r_{Cij}$ for all $ij$, the distribution of $t(\mathbf{Z}, \mathbf{R})$ is its permutation

$$\Pr \{ t(\mathbf{Z}, \mathbf{r}_C) \geq k \mid \mathcal{F}, \mathcal{Z} \} = \frac{|\{ \mathbf{z} \in \mathcal{Z} : t(\mathbf{z}, \mathbf{r}_C) \geq k \}|}{|\mathcal{Z}|},$$

because
Randomization inference (in each of our 4 parallel studies, e.g., (ls, n).)

- If this were a randomized experiment, then we would, independently, assign treatment at random to one person in each matched set, so

\[
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\]

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\[
\Pr\{t(Z, r_C) \geq k \mid F, Z\} = \frac{|\{z \in Z : t(z, r_C) \geq k\}|}{|Z|}, \text{ because}
\]

1. \(R = r_C\) when \(H_0\) is true,
Randomization inference (in each of our 4 parallel studies, e.g., (ls, n)).

- If this were a randomized experiment, then we would, independently, assign treatment at random to one person in each matched set, so

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\[ \Pr\{t(Z, r_C) \geq k \mid \mathcal{F}, \mathcal{Z}\} = \frac{|\{z \in \mathcal{Z} : t(z, r_C) \geq k\}|}{|\mathcal{Z}|}, \text{ because} \]

1. \( R = r_C \) when \( H_0 \) is true,
2. \( r_C \) is fixed by conditioning on \( \mathcal{F} \), and
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  \[
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  \]

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  \]

  1. \( R = r_C \) when \( H_0 \) is true,
  2. \( r_C \) is fixed by conditioning on \( \mathcal{F} \), and
  3. \( Z \) is uniform on \( \mathcal{Z} \) in a randomized experiment.
Huber-Maritz $M$-tests $t(Z, R) = \sum_{i=1}^{l} \psi(Y_i/s)$ where $s$ is the 95% quantile of $|Y_i|$, and $\psi(\cdot)$ is an odd function, $\psi(y) = -\psi(-y)$. 
Huber-Maritz $M$-tests $t(\mathbf{Z}, \mathbf{R}) = \sum_{i=1}^{l} \psi(Y_i/s)$ where $s$ is the 95% quantile of $|Y_i|$, and $\psi(\cdot)$ is an odd function, $\psi(y) = -\psi(-y)$.

$\psi_t(y) = y$ yields the permutational $t$-test
Test statistics

- Huber-Maritz $M$-tests $t(Z, R) = \sum_{i=1}^{I} \psi \left( \frac{Y_i}{s} \right)$ where $s$ is the 95% quantile of $|Y_i|$, and $\psi(\cdot)$ is an odd function, $\psi(y) = -\psi(-y)$.

1. $\psi_t(y) = y$ yields the permutational $t$-test
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1. $\psi_{t}(y) = y$ yields the permutational $t$-test
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Randomization distribution of Huber-Maritz M-tests

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- Under $H_0 : r_{Tij} = r_{Cij} \forall ij$, the difference in injury scores is $Y_i = (Z_{i1} - Z_{i2})(R_{i1} - R_{i2}) = (Z_{i1} - Z_{i2})(r_{Ci1} - r_{Ci2}) = \pm (r_{Ci1} - r_{Ci2})$. 

Rosenbaum | Counterclaims
Huber-Maritz $M$-tests $t(\mathbf{Z}, \mathbf{R}) = \sum_{i=1}^{I} \psi(Y_i/s)$ where $s$ is the 95% quantile of $|Y_i|$, and $\psi(\cdot)$ is an odd function, $\psi(y) = -\psi(-y)$.

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So under $H_0$, $|Y_i| = |r_{Ci1} - r_{Ci2}|$ is fixed by conditioning on $\mathcal{F}$, so $s$ is also fixed.
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- So under $H_0$, $|Y_i| = |r_{Ci1} - r_{Ci2}|$ is fixed by conditioning on $\mathcal{F}$, so $s$ is also fixed.

- Hence, in a randomized experiment under $H_0$, $t(Z, R) = \sum_{i=1}^{l} \psi(Y_i / s)$ is the sum of $l$ independent random variables taking the values $\pm \psi(|Y_i| / s) = \pm \psi(|r_{Ci1} - r_{Ci2}| / s)$ with equal probabilities $1/2$. 
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- I.e., the null distribution of $\sum_{i=1}^{l} \psi \left( \frac{Y_i}{s} \right)$ has a simple form.
Table: Randomization tests of no effect in 4 comparisons. n = no restraint. ls = lap-shoulder belt.

<table>
<thead>
<tr>
<th>Restraint Group</th>
<th>Restraint Use: (driver(passenger))</th>
<th>Same Use</th>
<th>Different Use</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>ls.ls n.n</td>
<td>ls.n n.ls</td>
<td></td>
</tr>
<tr>
<td>Number of Pairs</td>
<td>10996 3274</td>
<td>1412 1198</td>
<td></td>
</tr>
<tr>
<td>Mean $Y_i$</td>
<td>-0.059 0.061</td>
<td>-1.076 1.000</td>
<td></td>
</tr>
<tr>
<td>Standard error of mean</td>
<td>0.013 0.027</td>
<td>0.042 0.044</td>
<td></td>
</tr>
<tr>
<td>Standard deviation of $Y_i$</td>
<td>1.335 1.571</td>
<td>1.565 1.513</td>
<td></td>
</tr>
<tr>
<td>Randomization tests</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Huber Scores</td>
<td>P-values 0.0000 0.0241</td>
<td>0.0000 0.0000</td>
<td></td>
</tr>
<tr>
<td>Inner Trimmed Scores</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>P-values</td>
<td>0.0000 0.0374</td>
<td>0.0000 0.0000</td>
<td></td>
</tr>
</tbody>
</table>
Sensitivity to nonrandomized treatment assignment

- Model says that, in the population prior to matching, treatment assignments are independent and two subjects with the same observed covariates may differ in their odds of treatment, $Z_{ij} = 1$, by at most a factor of $\Gamma$; then, the distribution of $Z$ is returned to $\mathcal{Z}$ by conditioning on $Z \in \mathcal{Z}$. 

Rosenbaum

Counterclaims
Sensitivity to nonrandomized treatment assignment

- Model says that, in the population prior to matching, treatment assignments are independent and two subjects with the same observed covariates may differ in their odds of treatment, \( Z_{ij} = 1 \), by at most a factor of \( \Gamma \); then, the distribution of \( Z \) is returned to \( Z \) by conditioning on \( Z \in Z \).

- Equivalent to assuming that there is an unobserved covariate \( u_{ij} \) with \( 0 \leq u_{ij} \leq 1 \) such that

\[
\Pr(Z = z \mid \mathcal{F}, Z) = \prod_{i \in I} \frac{\exp(\gamma \sum_{j \in J_i} z_{ij} u_{ij})}{\sum_{j \in J_i} \exp(\gamma u_{ij})} = \frac{\exp(\gamma z^T u)}{\sum_{b \in Z} \exp(\gamma b^T u)},
\]

for each \( z \in Z \), where \( \gamma = \log(\Gamma) \geq 0 \); see Rosenbaum (2002, §4.2). For \( \Gamma = 1 \), \( \gamma = \log(\Gamma) = 0 \), this is the randomization distribution.
Model says that, in the population prior to matching, treatment assignments are independent and two subjects with the same observed covariates may differ in their odds of treatment, \( Z_{ij} = 1 \), by at most a factor of \( \Gamma \); then, the distribution of \( Z \) is returned to \( Z \) by conditioning on \( Z \in Z \).

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for each \( z \in Z \), where \( \gamma = \log (\Gamma) \geq 0 \); see Rosenbaum (2002, §4.2). For \( \Gamma = 1 \), \( \gamma = \log (\Gamma) = 0 \), this is the randomization distribution.

Distribution of \( t(Z, R) \) under \( H_0 \) is unknown for \( \Gamma > 1 \) but the degree of departure from random assignment is controlled by the value of \( \Gamma \).
Sensitivity analysis computes bounds on inference quantities for several values of $\Gamma$, for instance, bounds on $P$-values, point estimates, confidence intervals.
Sensitivity analysis, continued

- Sensitivity analysis computes bounds on inference quantities for several values of $\Gamma$, for instance, bounds on $P$-values, point estimates, confidence intervals.

- In the paired case under $H_0$, the upper bounds on the distribution of $t(Z, R) = \sum_{i=1}^{I} \psi(Y_i/s)$ is the sum of $I$ independent random variables taking the value $\psi(|Y_i|/s)$ with probability $\Gamma/(1+\Gamma)$, and value $-\psi(|Y_i|/s)$ with probability $1/(1+\Gamma)$.
Sensitivity analysis, continued

- Sensitivity analysis computes bounds on inference quantities for several values of \( \Gamma \), for instance, bounds on \( P \)-values, point estimates, confidence intervals.

- In the paired case under \( H_0 \), the upper bounds on the distribution of \( t(Z, R) = \sum_{i=1}^{l} \psi(Y_i/s) \) is the sum of \( l \) independent random variables taking the value \( \psi(\lvert Y_i \rvert / s) \) with probability \( \Gamma / (1 + \Gamma) \), and value \(-\psi(\lvert Y_i \rvert / s) \) with probability \( 1 / (1 + \Gamma) \).

- Similar for the lower bound, but with the two probabilities interchanged.
Sensitivity analysis, continued

- Sensitivity analysis computes bounds on inference quantities for several values of $\Gamma$, for instance, bounds on $P$-values, point estimates, confidence intervals.

- In the paired case under $H_0$, the upper bounds on the distribution of $t(Z, R) = \sum_{i=1}^{I} \psi(Y_i/s)$ is the sum of $I$ independent random variables taking the value $\psi(|Y_i|/s)$ with probability $\Gamma/(1+\Gamma)$, and value $-\psi(|Y_i|/s)$ with probability $1/(1+\Gamma)$.

- Similar for the lower bound, but with the two probabilities interchanged.

- Implementation for $M$-statistics in the `senm` and `senmCI` functions of the `sensitivitymult` R package.
Table: Upper bounds on $P$-values testing $H_0$.

<table>
<thead>
<tr>
<th>Restraint Group</th>
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<th>Different Use</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>ls.ls n.n</td>
<td>ls.n n.ls</td>
</tr>
<tr>
<td>$\Gamma$</td>
<td>Huber Scores without Inner Trimming</td>
<td>Inner Trimmed Scores</td>
</tr>
<tr>
<td>1</td>
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<td>0.0000 0.0000</td>
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<td>1.0000 1.0000</td>
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<td>4</td>
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<td>0.1808 1.0000</td>
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<td>5.5</td>
<td></td>
<td>0.0160 0.5058</td>
</tr>
</tbody>
</table>
A counterclaim

- Seatbelts have no safety related effects, no effect on what happens during the accident. All we are seeing is a pattern produced by the type of person who wears safety belts.
Seatbelts have no safety related effects, no effect on what happens during the accident. All we are seeing is a pattern produced by the type of person who wears safety belts.

We will see that this counterclaim undermines itself.
A counterclaim

- Seatbelts have no safety related effects, no effect on what happens during the accident. All we are seeing is a pattern produced by the type of person who wears safety belts.
- We will see that this counterclaim undermines itself.
- If this counterclaim were true, it would justify an analysis that is more insensitive to unmeasured bias than the analysis just performed.
Suppose it were true that: *Seatbelts have no safety related effects, no effect on what happens during the accident. All we are seeing is a pattern produced by the type of person who wears safety belts.*
A counterclaim analysis

- Suppose it were true that: *Seatbelts have no safety related effects, no effect on what happens during the accident. All we are seeing is a pattern produced by the type of person who wears safety belts.*

- Were this true, it would justify an analysis confined to a segment of the data, not all of the pairs but just some of them.
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- Specifically, were this true, I would be justified in confining attention to crashes in which exactly one person was ejected from the vehicle.
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- Specifically, were this true, I would be justified in confining attention to crashes in which exactly one person was ejected from the vehicle.

- Notice that I have not specified *who* was ejected, just that exactly one person was ejected.

- Will show the analysis, then explain why this analysis is licensed by the counterclaim.
Figure 2: Pair differences in injury scores, driver-minus-passenger, for a driver and a passenger in the same car in FARS 2010-2011, by restraint use, when precisely one individual was ejected from the vehicle, either partially ejected or totally ejected. A positive difference indicates the driver suffered more severe injuries than the passenger.
Table: Renalysis using only 2048 pairs in which exactly one person was ejected from the vehicle.

<table>
<thead>
<tr>
<th>Restraint Group</th>
<th>Restraint Use: (driver.passenger)</th>
<th>Same Use</th>
<th>Different Use</th>
</tr>
</thead>
<tbody>
<tr>
<td>Restraint Group</td>
<td>ls.ls</td>
<td>n.n</td>
<td>ls.n</td>
</tr>
<tr>
<td>Number of Pairs</td>
<td>222</td>
<td>782</td>
<td>522</td>
</tr>
<tr>
<td>Mean</td>
<td>-0.023</td>
<td>0.141</td>
<td>-1.540</td>
</tr>
<tr>
<td>Standard error</td>
<td>0.117</td>
<td>0.069</td>
<td>0.064</td>
</tr>
<tr>
<td>Standard deviation</td>
<td>1.748</td>
<td>1.938</td>
<td>1.455</td>
</tr>
</tbody>
</table>
Crashes with one ejection: Sensitivity analysis

Table: Values are upper bounds on $P$-values.

<table>
<thead>
<tr>
<th>Restraint Group</th>
<th>Same Use</th>
<th>Different Use</th>
<th>Huber Scores without Inner Trimming</th>
<th>Inner Trimmed Scores</th>
</tr>
</thead>
<tbody>
<tr>
<td>$\Gamma$ (ls.ls)</td>
<td>ls.ls</td>
<td>n.n</td>
<td>ls.n</td>
<td>n.ls</td>
</tr>
<tr>
<td>1</td>
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<td>0.0000</td>
<td>0.0000</td>
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<td>11</td>
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<td>0.0149</td>
</tr>
</tbody>
</table>

Rosenbaum Counterclaims
Segments of the data

- A segment consists of some of the individuals in the study.
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A segment of data \( \{ J_i, i \in I \} \) is \( \{ J'_i, i \in I \} \) where \( J'_i \subseteq J_i \) for each \( i \in I \).
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A segment of data \( \{ J_i, i \in I \} \) is \( \{ J'_i, i \in I \} \) where \( J'_i \subseteq J_i \) for each \( i \in I \).

Example: if there are \( n = 9 \) subjects in matched triples, \( J_1 = \{1, 2, 3\} \), \( J_2 = \{1, 2, 3\} \), \( J_3 = \{1, 2, 3\} \), then one segment is \( J'_1 = \{2, 3\} \), \( J'_2 = \emptyset \), \( J'_3 = \{1, 2, 3\} \).
Segments of the data

- A segment consists of some of the individuals in the study.
- A segment of data \( \{ \mathcal{J}_i, i \in \mathcal{I} \} \) is \( \{ \mathcal{J}'_i, i \in \mathcal{I} \} \) where \( \mathcal{J}'_i \subseteq \mathcal{J}_i \) for each \( i \in \mathcal{I} \).
- Example: if there are \( n = 9 \) subjects in matched triples, \( \mathcal{J}_1 = \{1, 2, 3\} \), \( \mathcal{J}_2 = \{1, 2, 3\} \), \( \mathcal{J}_3 = \{1, 2, 3\} \), then one segment is \( \mathcal{J}'_1 = \{2, 3\} \), \( \mathcal{J}'_2 = \emptyset \), \( \mathcal{J}'_3 = \{1, 2, 3\} \).
- Let \( \mathcal{S} \) be the set whose \( 2^n \) elements are the \( 2^n \) possible segments.
Nondegenerate parts of a segment

For a segment $\{\mathcal{J}_i', i \in I\}$, write $m_i$ for the random variable that counts the number of treated subjects in $\mathcal{J}_i'$, so $m_i = 0$ if $\mathcal{J}_i' = \emptyset$ and otherwise $m_i = \sum_{j \in \mathcal{J}_i'} Z_{ij}$, so $m_i = 0$ or $m_i = 1$. Write $\mathbf{m} = (m_1, \ldots, m_I)$. 
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- The contribution from $J'_i$ in segment $\{J'_i, \ i \in \mathcal{I}\}$ will be degenerate and uninteresting unless $m_i = 1 < |J'_i|$, that is, unless $J'_i$ contains the treated subject and at least one control from matched set $J_i$. 
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For matched pairs, \( |J_i| = J_i = 2 \) for all \( i \), nondegenerate part of a segment is a subset of the matched pairs.
Nondegenerate parts of a segment

- For a segment \( \{ J_i', i \in I \} \), write \( m_i \) for the random variable that counts the number of treated subjects in \( J_i' \), so \( m_i = 0 \) if \( J_i' = \emptyset \) and otherwise \( m_i = \sum_{j \in J_i'} Z_{ij} \), so \( m_i = 0 \) or \( m_i = 1 \). Write \( \mathbf{m} = (m_1, \ldots, m_I) \).

- The contribution from \( J_i' \) in segment \( \{ J_i', i \in I \} \) will be degenerate and uninteresting unless \( m_i = 1 < |J_i'| \), that is, unless \( J_i' \) contains the treated subject and at least one control from matched set \( J_i \).

- For matched pairs, \( |J_i| = J_i = 2 \) for all \( i \), nondegenerate part of a segment is a subset of the matched pairs.

- For matched sets with \( |J_i| = J_i > 2 \), a segment \( \{ J_i', i \in I \} \) may have nondegenerate parts \( J_i' \) with \( m_i = 1 < |J_i'| < |J_i| \) containing the treated subject from \( J_i \) and some but not all of the controls from \( J_i \).
For a segment \( \{ J_i', i \in I \} \), add a prime to a quantity to denote the value of a quantity confined to the segment.
Notation for a segment

- For a segment \( \{ J'_i, i \in I \} \), add a prime to a quantity to denote the value of a quantity confined to the segment.
- For instance, write \( Z'_i \) or \( R'_i \) for the vectors of dimension \( n' = \sum_{i \in I} |J'_i| \) containing, in the lexical order, the \( Z_{ij} \) or \( R_{ij} \) for \( j \in J'_i, i \in I \).
Notation for a segment

- For a segment \( \{ \mathcal{J}_i', i \in \mathcal{I} \} \), add a prime to a quantity to denote the value of a quantity confined to the segment.

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- Write \( \mathcal{Z}'_m \) for the set of possible values of \( Z' \), that is, the set of vectors \( z' \) of dimension \( n' \) with 1 or 0 coordinates such that \( m_i = \sum_{j \in \mathcal{J}_i'} z_{ij} \).
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- In parallel, write $r'_C$, $\mathbf{S}'$, etc.
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- In parallel, write \( \mathbf{r}'_C, \mathbf{S}', \) etc.

- As before, conditioning on the event \( \mathbf{Z}' \in \mathcal{Z}'_m \) is abbreviated as conditioning on \( \mathcal{Z}'_m \), and generally the conditioning will be on \( (\mathcal{Z}, \mathcal{Z}'_m, m) \) jointly.
There is a $n \times M$ matrix $W$ describing with row $w_{ij}$ describing subject $ij$. Write $\mathcal{W}$ for the set of possible values for $W$.

**Definition**

The phrase “$W$ determines the segment” means that there is a known function $S(W)$ that receives $W$ and returns a segment from $\mathcal{S}$, that is, $S : \mathcal{W} \rightarrow \mathcal{S}$. 
Using a matrix of data to determine a segment

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For instance, the values in $\mathbf{W}$ might pick out some of the pairs, or some of the people in matched sets.

Unless $\mathbf{W}$ includes $\mathbf{Z}$, a segment determined by $\mathbf{W}$ cannot make use of the identity of the treated subject.
A basic question about analysis of a segment

When can we select a segment \( \{ J_i', i \in I \} \) using \( W \), yet appropriately analyze this segment as if were an unselected data set?
A basic question about analysis of a segment

When can we select a segment \( \{ \mathcal{J}'_i, i \in \mathcal{I} \} \) using \( W \), yet appropriately analyze this segment as if were an unselected data set?

**Proposition** If the sensitivity model governs treatment assignment, if a segment \( S(W) = \{ \mathcal{J}'_i, i \in \mathcal{I} \} \) is determined by \( W \), and if \( W \) is fixed by conditioning on \( \mathcal{F} \), then

\[
\Pr (\mathbf{Z}' = \mathbf{z}' | \mathcal{F}, \mathbf{Z}, \mathbf{Z}'_m, \mathbf{m}) = \frac{\prod_{i \in \mathcal{I} : |\mathcal{J}'_i| > 0} \exp \left( \gamma \sum_{j \in \mathcal{J}'_i} z'_{ij} u_{ij} \right)}{\sum_{j \in \mathcal{J}'_i} \exp \left( \gamma u_{ij} \right)}.
\]
Counterclaims that deny effects on supplementary responses

**Proposition** If the sensitivity model governs treatment assignment, if a segment \( S(W) = \{J_i', i \in I\} \) is determined by \( W \), and if \( W \) is fixed by conditioning on \( F \), then

\[
\Pr(Z' = z' \mid F, Z, Z'_m, m) = \prod_{i \in I: J'_i > 0} \frac{\exp\left(\gamma \sum_{j \in J'_i} z'_{ij} u_{ij}\right)}{\sum_{j \in J_i'} \exp\left(\gamma u_{ij}\right)}.
\]

(1)
Counterclaims that deny effects on supplementary responses

- **Proposition**: If the sensitivity model governs treatment assignment, if a segment $S(W) = \{J'_i, i \in \mathcal{I}\}$ is determined by $W$, and if $W$ is fixed by conditioning on $F$, then

$$\Pr(Z' = z' \mid F, Z, Z'_m, m) = \prod_{i \in \mathcal{I} : |J'_i| > 0} \frac{\exp\left(\gamma \sum_{j \in J'_i} z'_{ij} u_{ij}\right)}{\sum_{j \in J'_i} \exp\left(\gamma u_{ij}\right)}.$$  

(1)

- **Corollary**: If the sensitivity model governs treatment assignment, if a segment $S(S) = \{J'_i, i \in \mathcal{I}\}$ is determined by the observed value of the supplementary responses $S$, and if the supplementary responses are unaffected by the treatment, $s_{Tij} = s_{Cij}$ for all $ij$, then the distribution of treatment assignments in the segment is given by (1).
Back to the counterclaim analysis involving ejections

- Let $S_{ij} = 1$ if $ij$ is observed in a crash one exactly one ejection, $S_{ij} = 0$ otherwise.
Back to the counterclaim analysis involving ejections

- Let $S_{ij} = 1$ if $ij$ is observed in a crash one exactly one ejection, $S_{ij} = 0$ otherwise.
- Obviously $S_{i1} = S_{i2}$ because $i1$ and $i2$ are in the same crash.
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In particular, the counterclaim says that changing $ij$’s treatment would not change whether $ij$ is ejected, that $S_{ij} = sT_{ij} = sC_{ij}$.
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- In particular, the counterclaim says that changing $ij$’s treatment would not change whether $ij$ is ejected, that $S_{ij} = s_{Tij} = s_{Cij}$.
- By the corollary, this licenses an analysis focused on the segment of crashes with one ejection.
Let $S_{ij} = 1$ if $ij$ is observed in a crash one exactly one ejection, $S_{ij} = 0$ otherwise.

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In particular, the counterclaim says that changing $ij$’s treatment would not change whether $ij$ is ejected, that $S_{ij} = s_{Tij} = s_{Cij}$.

By the corollary, this licenses an analysis focused on the segment of crashes with one ejection.

Expressed informally, the counterclaim said the unbelted individual was injured because he was frail, but switching treatment assignments (i.e., belting him) would have changed the identity of the belted subject but would have changed no safety outcomes.
So a counterclaim undermines itself. What next?

The counterclaim says: _Seatbelts have no safety related effects, no effect on what happens during the accident. All we are seeing is a pattern produced by the type of person who wears safety belts._

Rosenbaum Counterclaims
So a counterclaim undermines itself. What next?

- The counterclaim says: *Seatbelts have no safety related effects, no effect on what happens during the accident. All we are seeing is a pattern produced by the type of person who wears safety belts.*

- The counterclaim analysis says this counterclaim is hollow: to believe it is to justify an analysis that is insensitive to larger biases than the analysis that did not presume the counterclaim.
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- The critic could narrow the counterclaim to say: “yes, yes, safety belts do prevent people from being ejected from vehicles, but preventing ejections doesn’t prevent injuries.”
So a counterclaim undermines itself. What next?

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- The critic could narrow the counterclaim to say: “yes, yes, safety belts do prevent people from being ejected from vehicles, but preventing ejections doesn’t prevent injuries.”

- Depending upon the context, this concession acknowledging that the treatment does cause an effect on \((s_{Tij}, s_{Cij})\) while denying an effect on \((r_{Tij}, r_{Cij})\) may be a large concession.
The counterclaim says: *Seatbelts have no safety related effects, no effect on what happens during the accident. All we are seeing is a pattern produced by the type of person who wears safety belts.*
Another counterclaim analysis

- The counterclaim says: *Seatbelts have no safety related effects, no effect on what happens during the accident. All we are seeing is a pattern produced by the type of person who wears safety belts.*

- Another supplementary outcome is of direction of initial impact.
The counterclaim says: *Seatbelts have no safety related effects, no effect on what happens during the accident. All we are seeing is a pattern produced by the type of person who wears safety belts.*

Another supplementary outcome is of direction of initial impact.

Will look at crashes in which there was one ejection and the initial impact was not from the side. (That is, the initial impact was front or rear or unknown.)
The counterclaim says: *Seatbelts have no safety related effects, no effect on what happens during the accident. All we are seeing is a pattern produced by the type of person who wears safety belts.*

Another supplementary outcome is of direction of initial impact.

Will look at crashes in which there was one ejection and the initial impact was not from the side. (That is, the initial impact was front or rear or unknown.)

Might be the case that an important source of variation in injury is whether you are seated on the side of the initial impact.
Figure 3: Pair differences in injury scores, driver-minus-passenger, for a driver and a passenger in the same car in FARS 2010-2011, by restraint use, for all vehicle pairs, for vehicles not known to have an initial collision from the side, for vehicles with exactly one ejection, and for vehicles not known to have an initial collision from the side with exactly one ejection. A positive difference indicates the driver suffered more severe injuries than the passenger.
One ejection, not a side hit: Descriptive statistics

Table: Renalysis of differences in injury scores using only 1383 pairs in which exactly one person was ejected from a vehicle whose initial impact was not from the side. n = no restraint. ls = lap-shoulder belt.

<table>
<thead>
<tr>
<th>Restraint Group</th>
<th>Restraint Use: (driver.passenger)</th>
<th></th>
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<tbody>
<tr>
<td></td>
<td>Same Use</td>
<td>Different Use</td>
</tr>
<tr>
<td>Restraint Group</td>
<td>ls.ls n.n</td>
<td>ls.n n.ls</td>
</tr>
<tr>
<td>Number of Pairs</td>
<td>153 510</td>
<td>363 357</td>
</tr>
<tr>
<td>Mean</td>
<td>-0.072 0.133</td>
<td>-1.628 1.588</td>
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<tr>
<td>Standard error</td>
<td>0.145 0.087</td>
<td>0.071 0.067</td>
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<td>Standard deviation</td>
<td>1.789 1.961</td>
<td>1.345 1.259</td>
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</table>
One ejection, not a side hit: Sensitivity analysis

Table: Upper bounds on $P$-values.

<table>
<thead>
<tr>
<th>Restraint Group</th>
<th>ls.ls</th>
<th>n.n</th>
<th>ls.n</th>
<th>n.ls</th>
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<tr>
<td>Number of Pairs</td>
<td>153</td>
<td>510</td>
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<td>357</td>
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<table>
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<tr>
<th>$\Gamma$</th>
<th>Huber Scores without Inner Trimming</th>
<th>Inner Trimmed Scores</th>
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<td>0.6182 0.1251 0.0000 0.0000</td>
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<td>1.0000 0.9732 0.0000 0.0000</td>
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<td>11</td>
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<td>0.0129 0.0439</td>
</tr>
<tr>
<td>12</td>
<td>0.0610 0.0614</td>
<td></td>
</tr>
<tr>
<td>15</td>
<td>0.2722 0.2774</td>
<td></td>
</tr>
</tbody>
</table>
Design sensitivities under a simple model

- Design sensitivity $\Gamma$ is the limiting sensitivity to unmeasured bias as the sample size $I \to \infty$. 
Design sensitivity under a simple model

- Design sensitivity $\tilde{\Gamma}$ is the limiting sensitivity to unmeasured bias as the sample size $I \to \infty$.
- Design sensitivity $\tilde{\Gamma}$ depends on the process that generated the data (sampling model) and on the methods of analysis.
Design sensitivities under a simple model

- Design sensitivity $\tilde{\Gamma}$ is the limiting sensitivity to unmeasured bias as the sample size $I \to \infty$.
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Design sensitivities under a simple model

- Design sensitivity $\tilde{\Gamma}$ is the limiting sensitivity to unmeasured bias as the sample size $I \rightarrow \infty$.

- Design sensitivity $\tilde{\Gamma}$ depends on the process that generated the data (sampling model) and on the methods of analysis.

- Design sensitivity $\tilde{\Gamma}$ is computed under a simple model with a treatment effect and no unmeasured bias.

- Design sensitivity $\tilde{\Gamma}$ is a measure of our ability to distinguish two sharply distinct situations: (i) biased treatment assignment with no treatment effect, $H_0$, and (ii) a genuine treatment effect ($H_0$ is false) and no unmeasured bias (random assignment of treatments).
Simple model for injury and ejection, part 1

- \((s_{Tij}, s_{Cij})\) denotes ejection outcome in each of the four parallel studies (e.g., \((n, ls)\)).
Simple model for injury and ejection, part 1

- \((s_{Tij}, s_{Cij})\) denotes ejection outcome in each of the four parallel studies (e.g., \((n, ls)\)).

- \((s_{Tij}, s_{Cij}) = (1, 1)\) means ejected under both conditions, \((s_{Tij}, s_{Cij}) = (1, 0)\) means ejected only if Treated (say unbelted), \((s_{Tij}, s_{Cij}) = (0, 0)\) means not ejected in both conditions, which occur with probabilities \(\pi_{11}, \pi_{10}, \pi_{00}\), respectively, \(1 = \pi_{11} + \pi_{10} + \pi_{00}\) and \((s_{Tij}, s_{Cij}) = (0, 1)\) does not occur.
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- Injury model

\[
r_{Tij} = r_{Cij} + \tau + \beta (s_{Tij} - s_{Cij})
\]

so \(r_{Tij} - r_{Cij} = \tau\) if the treatment does not affect whether you are ejected, or \(r_{Tij} - r_{Cij} = \tau + \beta\) if the treatment (e.g., being unbelted) causes you to be ejected, \((s_{Tij}, s_{Cij}) = (1, 0)\).
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Simple model for injury and ejection, part 2

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- Then

\[ Y_i = \tau + \beta \delta_i + \varepsilon_i, \text{ where } \delta_i = Z_{i1} (s_{Ti1} - s_{Ci1}) + Z_{i2} (s_{Ti2} - s_{Ci2}) \]

\[ \varepsilon_i = (Z_{i1} - Z_{i2}) (r_{Ci1} - r_{Ci1}) \]
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Will look at this for \( \varepsilon_i \sim N(0, 1) \), and randomized treatment assignment, \( \text{Pr}(Z = z \mid \mathcal{F}, \mathcal{Z}) = 2^{-l} \) for each \( z \in \mathcal{Z} \).

Results are similar with logistic errors.
Injury model

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Then

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\]

\[
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Will look at this for \(\varepsilon_i \sim N(0, 1)\), and randomized treatment assignment, \(\text{Pr}(Z = z | \mathcal{F}, \mathcal{Z}) = 2^{-l}\) for each \(z \in \mathcal{Z}\).

Results are similar with logistic errors.

Will set \(\beta = (\frac{1}{2} - \tau) / \pi_{10}\) so that \(E(Y_i) = \frac{1}{2}\) in all cases.
Table: Design sensitivities using all pairs (All), the segment (Seg), and its complement (Comp), without or with inner trimming. The largest design sensitivities in each row are in **bold**.

<table>
<thead>
<tr>
<th>τ</th>
<th>All</th>
<th>Seg</th>
<th>Comp</th>
<th>All</th>
<th>Seg</th>
<th>Comp</th>
</tr>
</thead>
<tbody>
<tr>
<td>0</td>
<td>2.7</td>
<td>3.3</td>
<td>2.2</td>
<td>3.8</td>
<td><strong>4.9</strong></td>
<td>2.8</td>
</tr>
<tr>
<td>1/4</td>
<td>3.2</td>
<td>3.6</td>
<td>2.8</td>
<td>4.4</td>
<td><strong>5.1</strong></td>
<td>3.7</td>
</tr>
<tr>
<td>1/2</td>
<td>3.4</td>
<td>3.4</td>
<td>3.4</td>
<td><strong>4.7</strong></td>
<td><strong>4.7</strong></td>
<td><strong>4.7</strong></td>
</tr>
</tbody>
</table>

\[
(\pi_{11}, \pi_{10}, \pi_{00}) = (1/3, 1/3, 1/3)
\]

<table>
<thead>
<tr>
<th>τ</th>
<th>All</th>
<th>Seg</th>
<th>Comp</th>
<th>All</th>
<th>Seg</th>
<th>Comp</th>
</tr>
</thead>
<tbody>
<tr>
<td>0</td>
<td>3.0</td>
<td>3.8</td>
<td>2.1</td>
<td>4.0</td>
<td><strong>5.3</strong></td>
<td>2.5</td>
</tr>
<tr>
<td>1/4</td>
<td>3.3</td>
<td>3.8</td>
<td>2.7</td>
<td>4.5</td>
<td><strong>5.3</strong></td>
<td>3.5</td>
</tr>
<tr>
<td>1/2</td>
<td>3.5</td>
<td>3.5</td>
<td>3.5</td>
<td><strong>4.8</strong></td>
<td><strong>4.8</strong></td>
<td><strong>4.8</strong></td>
</tr>
</tbody>
</table>

\[
(\pi_{11}, \pi_{10}, \pi_{00}) = (1/4, 1/2, 1/4)
\]
Combining the segment and its complement

- Could test in the segment and its complement, obtaining two bounds on $P$-values.

  Truncated product of $P$-values is the product of those $P$-values $\kappa$; see Zaykin et al. (2002).

  Becomes Fisher's method for combining $P$-values when $\kappa = 1$.

  Hsu et al. (2013) evaluate the truncated product in sensitivity analyses, finding $\kappa = 0.2$ is better than $\kappa = 1$. 

Rosenbaum Counterclaims
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Simulated power of a 0.05-level sensitivity analysis

**Table:** Power of a 0.05-level sensitivity analysis at $\Gamma = 4$, using all $I = 2000$ pairs (All), the segment (Seg), its complement (Comp), and the truncated product (Tprod), $\kappa = 0.2$, based on both the segment and its complement, using inner trimming. $I_{\text{Seg}}$ is the expected number of pairs in the segment.

<table>
<thead>
<tr>
<th>Distribution</th>
<th>$\tau$</th>
<th>$I_{\text{Seg}}$</th>
<th>All</th>
<th>Seg</th>
<th>Comp</th>
<th>Tprod</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td></td>
<td></td>
<td>$\pi_1, \pi_10, \pi_00$</td>
<td>$\pi_11, \pi_10, \pi_00$</td>
<td>All</td>
<td>Seg</td>
</tr>
<tr>
<td>Normal</td>
<td>0</td>
<td>1111</td>
<td>0.01</td>
<td>0.48</td>
<td>0.00</td>
<td>0.22</td>
</tr>
<tr>
<td>Normal</td>
<td>1/4</td>
<td>1111</td>
<td>0.24</td>
<td>0.62</td>
<td>0.01</td>
<td>0.38</td>
</tr>
<tr>
<td>Normal</td>
<td>1/2</td>
<td>1111</td>
<td>0.61</td>
<td>0.40</td>
<td>0.33</td>
<td>0.55</td>
</tr>
<tr>
<td></td>
<td></td>
<td></td>
<td>$\pi_1, \pi_10, \pi_00$</td>
<td>$\pi_11, \pi_10, \pi_00$</td>
<td>All</td>
<td>Seg</td>
</tr>
<tr>
<td>Normal</td>
<td>0</td>
<td>1250</td>
<td>0.04</td>
<td>0.82</td>
<td>0.00</td>
<td>0.60</td>
</tr>
<tr>
<td>Normal</td>
<td>1/4</td>
<td>1250</td>
<td>0.39</td>
<td>0.80</td>
<td>0.00</td>
<td>0.60</td>
</tr>
<tr>
<td>Normal</td>
<td>1/2</td>
<td>1250</td>
<td>0.60</td>
<td>0.43</td>
<td>0.29</td>
<td>0.54</td>
</tr>
</tbody>
</table>
A counterclaim undermines itself if supposing the counterclaim to be true licenses an additional analysis that results in greater insensitivity to unmeasured biases than the analysis that does not suppose the counterclaim to be true.
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Summary
A counterclaim undermines itself if supposing the counterclaim to be true licenses an additional analysis that results in greater insensitivity to unmeasured biases than the analysis that does not suppose the counterclaim to be true.

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An investigator may examine potential counterclaims before they are raised by critics.

Design sensitivities and simulated powers of sensitivity analyses suggest that what occurred in the example is expected under certain simple models for an effect without bias.
Proof of the proposition

The segment \( \{ \mathcal{J}_i', i \in \mathcal{I} \} \) is fixed by conditioning on \( \mathcal{F} \); moreover, the set \( \mathcal{Z}_m' \) is a fixed set as a consequence of conditioning on \( \mathcal{Z} \) and \( m \). It suffices to consider a single set \( i \). If \( \mathcal{J}_i' \) is degenerate, then it contributes a 1 factor to distribution in the segment. Otherwise, for \( |\mathcal{J}_i'| \geq 2 \) and \( m_i = 1 \), the conditional probability that \( Z_{ij} = z_{ij}' \) for \( j \in \mathcal{J}_i' \) given \( \mathcal{F}, \mathcal{Z}, \mathcal{Z}_m', m \) is the ratio of \( \exp \left( \gamma \sum_{j \in \mathcal{J}_i} z_{ij}' u_{ij} \right) / \sum_{j \in \mathcal{J}_i} \exp (\gamma u_{ij}) \) to the sum of similar terms over \( j \in \mathcal{J}_i' \), namely

\[
\frac{\exp \left( \gamma \sum_{j \in \mathcal{J}_i} z_{ij}' u_{ij} \right) / \sum_{j \in \mathcal{J}_i} \exp (\gamma u_{ij})}{\sum_{j \in \mathcal{J}_i'} \left\{ \exp (\gamma u_{ij}) / \sum_{j \in \mathcal{J}_i} \exp (\gamma u_{ij}) \right\}} = \frac{\exp \left( \gamma \sum_{j \in \mathcal{J}_i} z_{ij}' u_{ij} \right)}{\sum_{j \in \mathcal{J}_i'} \exp (\gamma u_{ij})}
\]

as in the statement of the proposition.