Versions of Treatment

A causal inference debate that sociologists have ignored

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Sociology Statistics Reading Group
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Introductory note for those finding these slides online

These slides were prepared for the Sociology Statistics Reading Group at Princeton. Everyone read the following paper in advance:

> Hernán, Miguel A. 2016. Does water kill? A call for less casual causal inferences. *Annals of Epidemiology*, 26(10):674–680. [link]

At times, these slides intentionally emphasize alternative positions to those presented by Hernán (2016), such as the possibility that consistency is not an assumption but is rather a consequence of the assumptions embedded in a causal DAG (Pearl, 2010). I emphasize this alternative view not because my personal position is strongly one way or the other, but because it will promote better discussion among a group that read the former but not the latter. See references at the end for further reading.

Hernán (2016): Does Water Kill?

London cholera epidemic, 1854.
John Snow deduced that the water was the cause of death.

Source: Wikimedia Commons

Hernán (2016): Does Water Kill?

Does drinking water kill?

Hernán (2016): Does Water Kill?

Does drinking **fresh** water kill?

Hernán (2016): Does Water Kill?

Does drinking **a swig of** fresh water kill?

Hernán (2016): Does Water Kill?

Does drinking a swig of fresh water **from the Broad Street pump** kill?

Hernán (2016): Does Water Kill?

Does drinking a swig of fresh water from the Broad Street pump **between August 31 and September 10** kill?

Hernán (2016): Does Water Kill?

Does drinking a swig of fresh water from the Broad Street pump between August 31 and September 10 kill

compared with drinking all your water from other pumps?

Hernán (2016): Does Water Kill?

Does drinking a swig of fresh water from the Broad Street pump between August 31 and September 10 **and not initiating a rehydration treatment if diarrhea starts** kill

compared with drinking all your water from other pumps?

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Does drinking a swig of fresh water from the Broad Street pump between August 31 and September 10 and not initiating a rehydration treatment if diarrhea starts kill

compared with drinking all your water from other pumps?

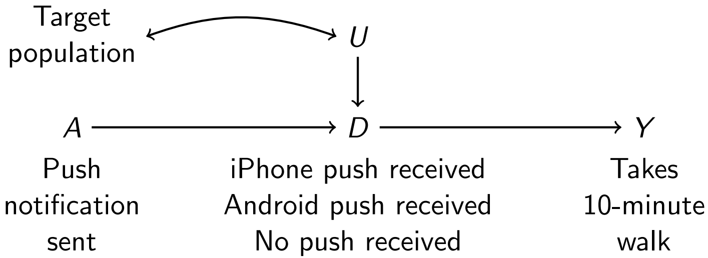
The **definition** of the causal effect is unclear without details.

Recommendation: Specify versions
"until no meaningful vagueness remains,"
(Hernán, 2016)

But some vagueness is

unavoidable

in both **experimental** and **observational** social science.


Target population
U
A
D
Y
Push notification sent
iPhone push received
Android push received
No push received
Takes 10-minute walk

Continuous treatment

Overwork

Collapsed by **researcher**

Researcher collapses continuous D

$$A = \mathcal{C}(D) = \mathbb{I}(D > 50)$$

$$D \longrightarrow Y$$

Employment hours — Hourly wage

Collapsed by **researcher**

Continuous treatment

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Researcher collapses continuous D

$$A = \mathcal{C}(D) = \mathbb{I}(D > 50)$$

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Employment hours → Hourly wage

Categorical treatment

Occupations

Researcher collapses categorical D

Class scheme: $A = \mathcal{C}(D)$

$$D \longrightarrow Y$$

Occupation → Status

	Continuous treatment	**Categorical** treatment
Collapsed by **researcher**	**Overwork** Researcher collapses continuous D $A = \mathcal{C}(D) = \mathbb{I}(D > 50)$ $D \longrightarrow Y$ Employment hours → Hourly wage	**Occupations** Researcher collapses categorical D Class scheme: $A = \mathcal{C}(D)$ $D \longrightarrow Y$ Occupation → Status
Collapsed by **respondent**	**Self-rated health** Respondent collapses continuous D 5-point scale: $A = \mathcal{C}(D)$ $D \longrightarrow Y$ Health → Lifespan	

	Continuous treatment	**Categorical** treatment
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Collapsed by **respondent**	**Self-rated health** Respondent collapses continuous D 5-point scale: $A = \mathcal{C}(D)$ $D \longrightarrow Y$ Health → Lifespan	**Returns to college** Respondent collapses categorical D Completed college: $A = \mathcal{C}(D)$ $D \longrightarrow Y$ College degree (institution and major) → Earnings

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A Causal Inference Debate Sociologists Have Ignored

1. Formalizing the **problem**
   - A) Potential outcomes
   - B) Stochastic counterfactuals
   - C) Causal graphs
2. When it matters: **Consequences** of collapsed versions
   - A) Experimental studies: Effects may not generalize
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     - — Heterogeneous treatment effects may really be the effects of heterogeneous treatments
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$$\text{Causal effect} = \underbrace{Y_i(a') - Y_i(a)}_{\text{Potential outcomes}}$$

$$\underbrace{\{Y_i(a)\}}_{\substack{\text{Potential outcomes:} \\ \textbf{Deterministic} \text{ consequence of } a}} \qquad \underbrace{Y_i^{\text{Obseved}} = Y_i(A_i)}_{\substack{\text{Observed outcome:} \\ \textbf{Random} \text{ because } A_i \text{ is random}}}$$

Imbens and Rubin (2015, p. 10):

> ... for each unit, there are no different forms or versions of each treatment level which lead to different potential outcomes.

$$A \longrightarrow D \longrightarrow Y$$

Push notification | iPhone push received, Android push received, No push received | Walks 10 minutes

$Y_i(a)$ is deterministic under either:

- Deterministic detailed treatment assignment D given A

$$\mathbb{P}(D_i = d \mid A_i = a) = \begin{cases} 1 & \text{for one value of } d \\ 0 & \text{for all other values of } d \end{cases} \qquad \forall a$$

- Treatment variation irrelevance (adapted from VanderWeele 2009)

$$Y_i(d) = Y_i(d') \; \forall \; \{d, d'\} \text{ such that}$$

$$\mathbb{P}(D_i = d \mid A_i = a) > 0 \text{ and } \mathbb{P}(D_i = d' \mid A_i = a) > 0$$

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Stochastic counterfactuals[1] allow a more plausible assumption of treatment variation irrelevance.

Under **fixed** counterfactuals

Treatment-variation irrelevance:

$$Y_i(a, d_a) = Y_i(a, d'_a) \quad \forall \quad \{d_a, d'_a\} \in \mathcal{D}_a$$

Thus can define $Y_i(a) \equiv Y_i(a, d_a)$ for any d_a.

Consistency:

$$\text{If } A_i = a, \quad \exists \ d_a \in \mathcal{D}_a \text{ such that } Y_i^{\text{Observed}} = Y_i(a, d_a)$$

[1] VanderWeele 2009, Imbens and Rubin 2015 p. 12

Stochastic counterfactuals[1] allow a more plausible assumption of treatment variation irrelevance.

Under **stochastic** counterfactuals

Treatment-variation irrelevance:

$$Y_i(a, d_a) \overset{D}{\sim} Y_i(a, d'_a) \quad \forall \quad \{d_a, d'_a\} \in \mathcal{D}_a$$

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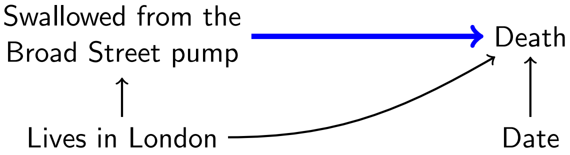
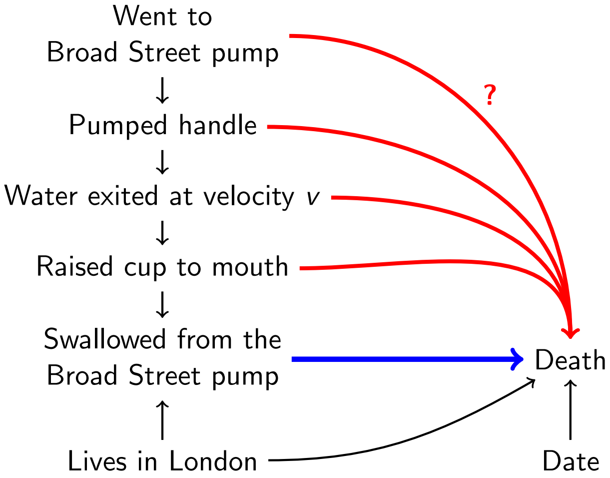
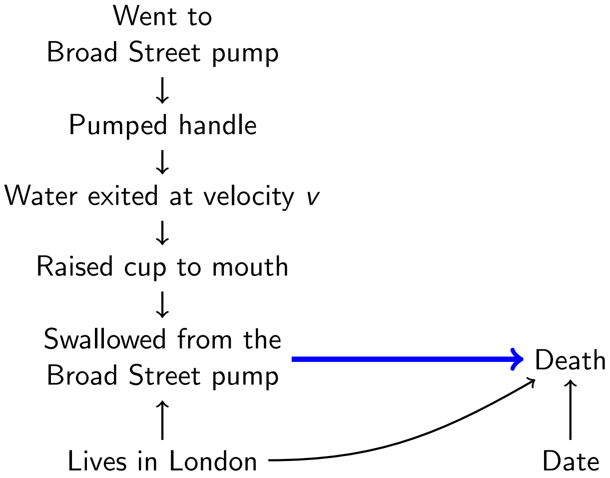
Treatment effects are defined by the DAG.

A correct DAG **implies** a well-defined effect.


Swallowed from the Broad Street pump
Death
Lives in London
Date

$$
\begin{tikzcd}
\text{Went to Broad Street pump} \arrow[d] & \\
\text{Pumped handle} \arrow[d] & \\
\text{Water exited at velocity } v \arrow[d] & \\
\text{Raised cup to mouth} \arrow[d] & \\
\text{Swallowed from the Broad Street pump} \arrow[r] & \text{Death} \\
\text{Lives in London} \arrow[u] \arrow[ur, bend right] & \text{Date} \arrow[u]
\end{tikzcd}
$$


Went to
Broad Street pump
Pumped handle
Water exited at velocity v
Raised cup to mouth
Swallowed from the
Broad Street pump
Lives in London
?
Death
Date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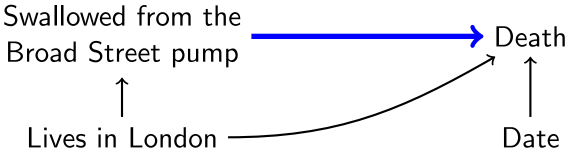
Swallowed from the
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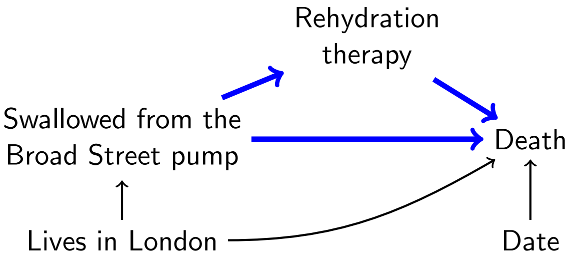
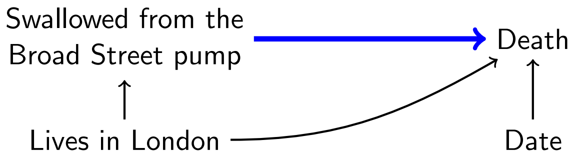
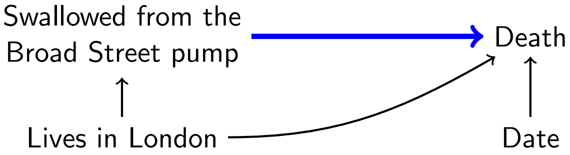
Death
Lives in London
Date

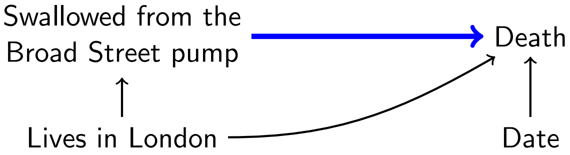

Rehydration therapy
Swallowed from the Broad Street pump
Death
Lives in London
Date


Swallowed from the
Broad Street pump
Death
Lives in London
Date

$$\mathbb{E}\left(\text{Death} \mid \begin{array}{c}\text{do(Swallowed from Broad Street pump),}\\ \text{Living in London on August 31 – September 10}\end{array}\right)$$
$$-\,\mathbb{E}\left(\text{Death} \mid \begin{array}{c}\text{do(Did not swallow from Broad Street pump),}\\ \text{Living in London on August 31 – September 10}\end{array}\right)$$



Things are vague only if the **graph** is insufficiently precise (and thus wrong).



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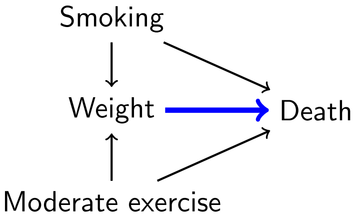
But we could just label these as confounding variables.
Not clear that consistency is an assumption.



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Experiments identify causal effects with minimal assumptions,

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Versions of treatment make generalization difficult.
(Hernán and VanderWeele, 2011)

If there is **contextual variation** in $A \to Y$, then the effect may not generalize to new contexts.

$$\begin{tikzcd}
 & & & \text{Context} \arrow[d] \\
A \arrow[rrr] & & & Y
\end{tikzcd}$$

If there is **contextual variation** in $A \to Y$, then the effect may not generalize to new contexts.

If this arises from variation in $A \to D$, then measuring D may promote transportability of inference.

$$\begin{tikzcd}
 & \text{Context} \arrow[d] & \\
A \arrow[r] & D \arrow[r] & Y
\end{tikzcd}$$

If there is **contextual variation** in $A \rightarrow Y$, then the effect may not generalize to new contexts.

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$$\begin{tikzcd}
 & \text{Hospital} \arrow[d] \\
A \arrow[r] & Y \\
\text{Surgery} & \text{Survival}
\end{tikzcd}$$

$A \rightarrow Y$ may differ across hospitals

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$$\begin{array}{ccc} & & \text{Time of day} \\ & & \downarrow \\ A & \xrightarrow{\hspace{6em}} & Y \end{array}$$

Randomized to restaurant ad on Facebook

Clicks on ad

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$$\begin{tikzcd} & \text{Time of day} \arrow[d] & \\ A \arrow[r] & D \arrow[r] & Y \end{tikzcd}$$

Randomized to restaurant ad on Facebook	Ad appears below posts about dinner	Clicks on ad

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$$
\begin{tikzcd}
 & \text{Complier} \arrow[d] \\
A \arrow[r] & Y
\end{tikzcd}
$$

Given aspirin   Headache gone

$A \to Y$ may differ by rates of compliance

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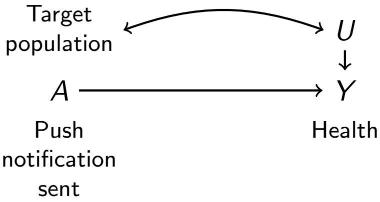
$$\begin{array}{ccccc} & & \text{Complier} & & \\ & & \downarrow & & \\ A & \longrightarrow & D & \longrightarrow & Y \\ \text{Given} & & \text{Takes} & & \text{Headache} \\ \text{aspirin} & & \text{Aspirin} & & \text{gone} \end{array}$$

$A \to Y$ may differ by rates of compliance

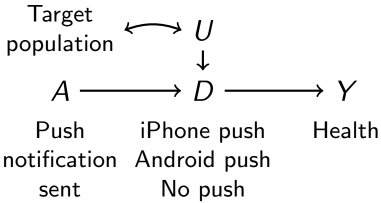
Generalizing experimental evidence

Generalization **impossible**



Generalization **possible**

by randomizing D



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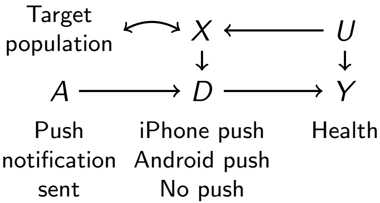
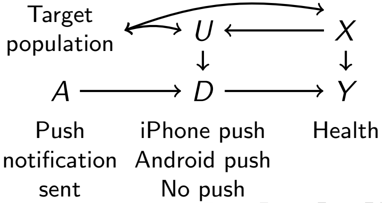


Generalization **possible**

by randomizing D and observing X



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$$D \longrightarrow Y$$

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(VanderWeele and Hernàn (2013, Prop. 8), though notation differs.)

$$\mathbb{E}(Y \mid \mathcal{C}(D) = c) = \sum_{d \in \mathcal{C}^{-1}(c)} \mathbb{E}\big(Y \mid \text{do}(D = d)\big)\mathbb{P}\big(D = d \mid D \in \mathcal{C}^{-1}(c)\big)$$

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One causal contrast

$$\mathbb{E}(Y \mid \mathcal{C}(D) = c') - \mathbb{E}(Y \mid \mathcal{C}(D) = c)$$

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Observed detailed treaments d mapping to collapsed treatment c'

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One causal contrast

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Observed detailed treaments d mapping to collapsed treatment c'	Observed detailed treaments d mapping to collapsed treatment c

$$D \longrightarrow Y$$

Because the collapsed $\mathcal{C}(D)$ is not in the causal graph, the effect of a collapsed treatment is undefined. We might examine:
(VanderWeele and Hernàn (2013, Prop. 8), though notation differs.)

$$\mathbb{E}(Y \mid \mathcal{C}(D) = c) = \sum_{d \in \mathcal{C}^{-1}(c)} \mathbb{E}\big(Y \mid \text{do}(D = d)\big)\mathbb{P}\big(D = d \mid D \in \mathcal{C}^{-1}(c)\big)$$

One causal contrast

$$\mathbb{E}(Y \mid \mathcal{C}(D) = c') - \mathbb{E}(Y \mid \mathcal{C}(D) = c)$$

Observed detailed treaments d mapping to collapsed treatment c'	Observed detailed treaments d mapping to collapsed treatment c

$\downarrow$

Random draw

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Observed detailed treaments d mapping to collapsed treatment c'	Observed detailed treaments d mapping to collapsed treatment c
$\downarrow$	$\downarrow$
Random draw	Random draw

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Observed detailed treaments d mapping to collapsed treatment c

Random draw $\longrightarrow$ Difference $\longleftarrow$ Random draw

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Random draw $\rightarrow$ Difference $\leftarrow$ Random draw

Average over many reps

With covariates. (equivalent to VanderWeele and Hernàn 2013, Prop. 8)

$$\begin{tikzcd} \boxed{X_j} \arrow[r] \arrow[rr, bend left] & D \arrow[r, blue] & Y \end{tikzcd}$$

$$\mathbb{E}(Y \mid \mathcal{C}(D) = c, \vec{X} = \vec{x}) =$$

$$\sum_{d \in \mathcal{C}^{-1}(c)} \mathbb{E}\big(Y \mid \text{do}(D = d), \vec{X} = \vec{x}\big)\mathbb{P}\big(D = d \mid D \in \mathcal{C}^{-1}(c), \vec{X} = \vec{x}\big)$$

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One causal contrast

$$\textstyle\sum_{\vec{x}} \mathbb{P}(\vec{X} = \vec{x})\Big(\mathbb{E}(Y \mid \mathcal{C}(D) = c', \vec{X} = \vec{x}) - \mathbb{E}(Y \mid \mathcal{C}(D) = c, \vec{X} = \vec{x})\Big)$$

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among $\vec{X} = \vec{x}$

With covariates. (equivalent to VanderWeele and Hernàn 2013, Prop. 8)

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Observed detailed treaments d mapping to collapsed treatment c'

among $\vec{X} = \vec{x}$

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Observed detailed treaments d mapping to collapsed treatment c'	Observed detailed treaments d mapping to collapsed treatment c

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Observed detailed treaments d mapping to collapsed treatment c'

Random draw

Observed detailed treaments d mapping to collapsed treatment c

among $\vec{X} = \vec{x}$

With covariates. (equivalent to VanderWeele and Hernàn 2013, Prop. 8)

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Observed detailed treaments d mapping to collapsed treatment c'

↓

Random draw

Observed detailed treaments d mapping to collapsed treatment c

↓

Random draw

among $\vec{X} = \vec{x}$

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$$\begin{tikzcd} \boxed{X_j} \arrow[r] \arrow[rr, bend left] & D \arrow[r, blue, thick] & Y \end{tikzcd}$$

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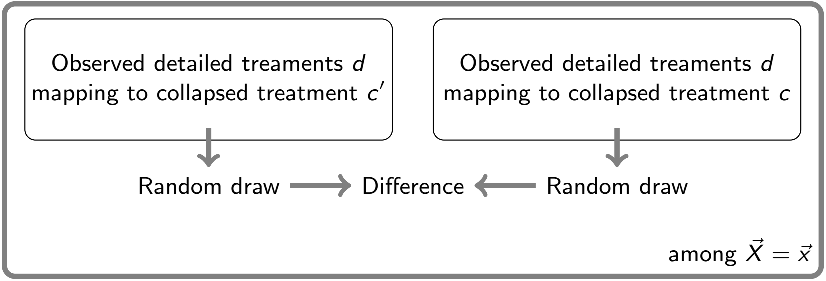

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Observed detailed treaments d mapping to collapsed treatment c' → Random draw

Observed detailed treaments d mapping to collapsed treatment c → Random draw

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Difference → Average over many reps

among $\vec{X} = \vec{x}$

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among $\vec{X} = \vec{x}$

→ Average over $\vec{X}$

Versions of Treatment

A Causal Inference Debate Sociologists Have Ignored

1. Formalizing the **problem**
   - A) Potential outcomes
   - B) Stochastic counterfactuals
   - C) Causal graphs
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What is the effect of **overwork** ($>$ 50 hours) on the **hourly wage** of those working at least 40 hours per week? Suppose **gender** is the only source of confounding.

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$$\text{Overwork} = \mathcal{C}(\text{Employment hours}) = \mathbb{I}(\text{Hours} > 50)$$

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What is the effect of **overwork** (> 50 hours) on the **hourly wage** of those working at least 40 hours per week? Suppose **gender** is the only source of confounding.

$$
\begin{tikzcd}
\text{Gender} \arrow[r] \arrow[rr, bend left] & \text{Employment hours} \arrow[r] & \text{Hourly wage}
\end{tikzcd}
$$

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Heterogeneous treatment effects

What is the effect of **overwork** ($>$ 50 hours) on the **hourly wage** of those working at least 40 hours per week? Suppose **gender** is the only source of confounding.

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Effects of heterogeneous treatments

What is the effect of **overwork** ($>$ 50 hours) on the **hourly wage** of those working at least 40 hours per week? Suppose **gender** is the only source of confounding.

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→ 3. **Recommendations:** What to do

What To Do

In randomized **experiments** aiming to generalize:

- Randomize a detailed treatment
- Theorize context-specific versions likely to remain

In **observational** studies:

- Estimate at the finest level of detail measured
  - Promotes a simple definition of the effect
  - Promotes transportability
  - Promotes clear policy implications
- If treatment remains vague, state the implied intervention.

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Appendix

Some define the **assumptions** for causal inference as:

- Ignorable treatment assignment
  - Violated if the treated would do better even without treatment
- Positivity
  - Violated if $\mathbb{P}(\text{Treated})$ is 0 or 1 for some units
- Stable Unit Treatment Value Assumption
  - Violated if there is interference
  - Violated if there are hidden versions of treatment

In this setup, **hidden versions** are the second part of SUTVA. Social scientists often focus on the first assumptions and give less thought to this part of SUTVA.

Why not use the $Y(a, d_a)$ notation?

One could state potential outcomes as a function of both treatment and treatment version (VanderWeele, 2009; Hernán and VanderWeele, 2011; VanderWeele and Hernàn, 2013).

Versions of treatment

$$A \longrightarrow D \longrightarrow Y$$

$Y_i(a, d_a)$ is unnecessary notation, though. Because only one a exists for any given d, $Y_i(d)$ carries the same information. In contrast, this is useful in mediation.

Mediation

$$\begin{tikzcd} & M \arrow[dr] & \\ A \arrow[ur] \arrow[rr] & & Y \end{tikzcd}$$

$Y_i(a, m)$ is valuable. Because A is not fixed given M, there exist multiple $\{a, a'\}$ with $Y_i(a, m) \neq Y_i(a', m)$.

Why not put A on the DAG as a consequence of D?

When the researchers take a detailed treatment D and coarsen it into an aggregate treatment A, Hernán and VanderWeele (2011) put it in the DAG as a consequence of D.

$$\begin{tikzcd} D \arrow[r] \arrow[rr, bend left] & A & Y \end{tikzcd}$$

The reasons not to do this are

1. In a DAG, it is useful to be able to conceive of an intervention to any given node. Because $D \to A$ is deterministic, it is hard to imagine an intervention to A which has no consequence for D. By the DAG, this intervention would have no consequence for D. This seems hard to swallow.
2. Perhaps A is not deterministic: it is *reported* D. But this seems like a whole different set of issues, and it is clear even without the DAG that intervening to change a report would have no consequence for Y.

What about when $A \to D$ is confounded?

When treatment precedes version, Hernán and VanderWeele (2011) also include cases like below:

$$\begin{tikzcd} U \arrow[d] \arrow[dr] & & \\ A \arrow[r] & D \arrow[r] & Y \end{tikzcd}$$

The reasons not to do this are

1. The edge $U \to A$ implies this is an observational study rather than an experiment. In observational studies, I usually do not believe the story that A is assigned first, followed by D. I think in observational studies D is typically the only variable involved.
2. We already have transportability issues from $U \to D$ alone. Omitting $U \to A$ helps to highlight these problems in the scenario when A is randomized.

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