

Overview of Quasi-Experimental Designs

Advanced Social Epidemiology PhD Course

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Quasi-Experiments

1. Motivation
2. Randomization and Observation
3. Quasi-Experimental Designs
4. Final Thoughts

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Stylized "forms" of questions asked in social epidemiology

What question do most studies in social epidemiology answer?

- Do individuals who are disadvantaged with respect to social position have worse health than those who are advantaged?

Other kinds of questions that could be asked:

- **Would** individuals who are disadvantaged with respect to social position have better health **if they were to become advantaged?**
- **Would** individuals who are advantaged with respect to social position have worse health **if they were to become disadvantaged?**

These are causal questions.

"Normal" etiological science in social epidemiology

1. Follow-up of individuals in different social groups for various health outcomes (incidence, mortality, risk factors)
2. Adjustment for various confounders/mediators (are inequalities "explained" by....A, B, C?).
 - "Our results demonstrate that"...we should:
 - *raise* education levels
 - *increase* economic assistance to the poor
 - *remove* noxious exposures from the environment
 - *reduce* psychosocial workplace hazards
 - *eliminate* hierarchies, and the like.
 - These statements are based on making causal inferences.

What's the problem?

- We are mainly (though not exclusively) interested in causal effects.
- We want to know:
 - Should we intervene to reduce exposure to X ?, or
 - Did the program work? If so, for whom? If not, why not?, or
 - If we implement the program elsewhere, should we expect the same result?
- These questions involve counterfactuals about what would happen if we intervened to do something.
- These are causal questions.

How to interpret statistical associations of health inequality?

We have lots of statistical associations between social exposures and health.

$$X - - - Y$$

Some possible situations *consistent* with statistical associations:

1. Causal $X \rightarrow Y$
2. Heterogeneity $X_a \rightarrow Y_a$ vs. $X_b \rightarrow Y_b$
3. Reverse causation $Y \rightarrow X$
4. Confounding $X \leftarrow C \rightarrow Y$
5. Selection bias $X \rightarrow S \leftarrow Y$

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Randomized Trials vs. Observational Studies

RCTs, Defined

RCTs involve:

1. comparing treated and control groups;
2. the treatment assignment is random;
3. investigator does the randomizing.

In an RCT, treatment/exposure is **assigned** by the investigator

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- In observational studies, exposed/unexposed groups **exist** in the source population and are selected by the investigator.
- Good natural experiments do (1) and (2), but not (3).
- Because there is no control over assignment, the credibility of natural experiments hinges on how good "as-if random" approximates (2).

Strength of randomized treatment allocation

- Recall that randomization means that we can generally estimate the causal effect *of being randomized* without bias.
- Randomization guarantees exchangeability on measured and unmeasured factors.



Randomize if you can



- Randomization leads to:
 - balance on measured factors.
 - balance on unmeasured factors.
- Unmeasured factors cannot bias the estimate of the exposure effect.
- Example from Home Injury Prevention Intervention cluster RCT
- What do you notice about Table 1?

Or maybe don't randomize?

RCT limitations

- Non-compliance.
- Attrition.
- Spillovers.
- Blinding (esp. in clinical trials).

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Other trial challenges:

- Unethical (poverty, parental social class, job loss)
- Impossible (ethnic background, place of birth)
- Expensive (neighborhood environments)
- Long latency periods (many years before outcomes are observable).
- Effects may be produced by complex, intermediate pathways.
- We need alternatives to RCTs.

Unmeasured confounding is a **serious** challenge

- We often compare socially advantaged and disadvantaged on health.
- Key problem: people choose/end up in treated or untreated group for reasons that are difficult to measure and that may be correlated with their outcomes.
- So...adjust.
 - Measure and adjust (regression) for C confounding factors.
 - Conditional on C , we are supposed to believe assignment is "as good as random" = causal.



Key issue is credibility

- If we have a good design and assume that we have measured all of the confounders, then regression can give us exactly what we want: an estimate of the causal effect of exposure to T .
- Core issue: How credible is this assumption?

SEP and CVD in Australia. Many low p-values



Why we worry about observational studies

Recent evaluation of "Workplace Wellness" program in US state of Illinois

Treatment: biometric health screening; online health risk assessment, access to a wide variety of wellness activities (e.g., smoking cessation, stress management, and recreational classes).

Randomized evaluation:

- 3,300 individuals assigned treated group.
- 1,534 assigned to control (could not access the program).

Also analyzed as an observational study comparing "participants" vs. non-participants in treated group.



Carroll, New York Times, Aug 6, 2018.

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How can quasi-experiments help?

- Quasi-experiments aim to mimic RCTs.
- "Accidents of chance" that create:
 1. Comparable treated and control units
 2. Random or "as-if" random assignment to treatment.
- Control for (some) sources of bias that cannot be adequately controlled using regression adjustment.
- More credible designs also help us to understand the relevance of other factors that may be implicated in generating inequalities.

Selection on "observables" and "unobservables"

- Observables: Things you measured or can measure
- Unobservables: Things you can't measure (e.g., innate abilities, motivation)
- Exogenous variation: predicts exposure but (we assume) **not** associated with anything else [mimicking random assignment].



Strategies based on observables and unobservables

- Most observational study designs control for *measured* factors using:
 - Stratification
 - Regression adjustment
 - Matching (propensity scores, etc.)

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- Most observational study designs control for *measured* factors using:
 - Stratification
 - Regression adjustment
 - Matching (propensity scores, etc.)
- Quasi-experimental strategies **aim** to control for some *unmeasured* factors using:
 - Interrupted time series (ITS)
 - Difference-in-differences (DD)
 - Synthetic controls (SC)
 - Instrumental variables (IV)
 - Regression discontinuity (RD)

Some *potential* sources of natural experiments



- Law changes
- Eligibility for social programs (roll-outs)
- Lotteries
- Genes
- Weather shocks (rainfall, disasters)
- Arbitrary policy or clinical guidelines (thresholds)
- Business / factory closures
- Historical legacies (physical environment)
- Seasonality

Difference-in-Differences

Difference-in-Differences: Basic Idea

In the simplest DD setting, outcomes are observed for units in two groups and in two time periods.

Treated:

- only units in one of the two groups are exposed to a treatment, in the second time period.

Control:

- Never observed to be exposed to the treatment.

Difference-in-Differences: Basic Idea



The average **change** over time in the non-exposed (control) group is **subtracted** from the gain over time in the exposed (treatment) group.

These are our two 'differences'.

Double differencing removes biases in second period comparisons between the treatment and control group that could be the result from

- permanent differences between those groups
- secular trends affecting both groups.

Visual Intuition of DD



Difference-in-Differences without Regression

DD is just differences in means! Let $\mu_{it} = E(Y_{it})$

- $i = 0$ is control group, $i = 1$ is treatment.
- $t = 0$ is pre-period, $t = 1$ is post-period.
- One 'difference' estimate of causal effect is: $\mu_{11} - \mu_{10}$ (pre-post in treated)
- Differences-in-Differences estimate of causal effect is: $(\mu_{11} - \mu_{10}) - (\mu_{01} - \mu_{00})$

Area	Before	After	Difference (A - B)
Treated	135	100	-35
Control	80	60	-20
T - C	55	40	-15

A social epidemiology example



- Evaluated impact of MA reform on inequalities in hospital admissions.
- Compared MA to nearby states: NY, NJ, PA.
- Intervention "worked": % uninsured halved (12% to 6%) from 2004-06 to 2008-09.

Evaluating pre-intervention trends



- Adds credibility to assumption that post-intervention trends **would have been similar** in the absence of the intervention.
- "Null" results help focus on alternative mechanisms linking disadvantage to hospital admissions.

Synthetic Controls

Synthetic control methods

- Inference from comparative case studies is limited if we cannot identify a control to represent the counterfactual scenario.
- Abadie and Gardeazabel (2003) pioneered the synthetic control method to examine the economic impact of terrorism in the Basque country, using other Spanish regions as control groups.
- The synthetic control method uses a data driven approach to compare the trend of an outcome in a treated unit with the trend in a synthetic composite area (the "synthetic control").

What is a synthetic control?

- A synthetic control is a weighted average of available control units that approximates the most relevant characteristics of the treated unit prior to the treatment
- The synthetic control mimics the values of the predictors of the outcome, including pre-intervention values of the outcome, for the treated unit before the intervention occurred
- The synthetic control represents the counterfactual scenario for a treated unit in the absence of the intervention under scrutiny
- Intuition: A weighted combination of comparison units (the “synthetic control”) provides a better comparison for the treated unit than any single comparison unit alone

Example of 1999 cigarette sales tax in California



- No control state looks like a good 'match'.
- SC creates a weighted control.

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- 'Synthetic' DK mostly SWE, ITA, USA, and FIN.
- Also declines in CVD mortality.
- Robustness checks:

By deliberately assigning TFA policy to wrong dates and other countries, we show the effect of the 2001 TFA policy intervention is specific to Denmark and does not appear to be driven by alternative dates



Instrumental Variables

Challenge of conventional observation study (again)

- WHO: "Educational attainment is linked to improved health outcomes."
- But what about unmeasured confounding?
Unmeasured factors such as personality traits, cognitive ability, etc. may be predictive of both education and disease.
- Failure to measure such factors will falsely attribute their effects to education.



Possible solution: Quasi-experiment

"Instrumental variable": predicts education but **not** associated with anything else [mimicking random assignment].



Non-randomized instrument creates additional issues

- In an RCT we know the treatment assignment is not associated directly with the outcome or with other unmeasured common causes.
- This assumption is less credible when our "instrument" is non-randomized.



Non-randomized examples of IV: Policies

- Does education affect cognitive functioning?
- Instrument: changes in compulsory schooling laws [mimicking random assignment].



What does a quasi- experiment look like?



Fraction left full-time
education by year
aged 14 and 15
(Great Britain)

The lower line shows the proportion of British-born adults aged 32 to 64 from the 1983 to 1998 General Household Surveys who report leaving full-time education at or before age 14 from 1935 to 1965. The upper line shows the same, but for age 15. The minimum school-leaving age in Great Britain changed in 1947 from 14 to 15 [Oreopoulos (2006)]

Average schooling
increases by exactly
half a year between
the cohorts that
were age 14 in
1946 and in 1948.



Ex: Education and HIV



Regression Discontinuity

RD: Basic Idea

- Take advantage of arbitrary thresholds that sometimes assign treatment to individuals.
- When an administrative or rule-based cutoff in a continuous variable (present in your data) predicts treatment assignment, being on one side or the other of this cutoff determines, or predicts, treatment received.

RD: Basic Idea

- Take advantage of arbitrary thresholds that sometimes assign treatment to individuals.
- When an administrative or rule-based cutoff in a continuous variable (present in your data) predicts treatment assignment, being on one side or the other of this cutoff determines, or predicts, treatment received.
- The continuous variable is called the "assignment" or "forcing" variable.
- Groups just on either side of the threshold are considered "as good as randomly" assigned to treatment.

RD: Motivating example

- Suppose we want to estimate the impact of a cash transfer program on daily food expenditure of poor households.
- Poverty is measured by a continuous score between 0 and 100 that is used to rank households from poorest to richest.
- Poverty is the assignment variable, Z , that determines eligibility for the cash transfer program.
- The outcome of interest, daily food expenditure, is denoted by Y .

At baseline, you might expect poorer households to spend less on food, on average, than richer ones, which might look like this



Under the program's rules, only households with a poverty score, Z , below 50 are eligible for the cash payment



As we approach the cutoff value from above and below, the individuals in both groups become more and more alike, on both measured and unobserved characteristics---in a small area around the threshold, the only difference is in treatment assignment



Applied example: HPV vaccine and sexual behaviors

- Does getting the HPV vaccine affect sexual behaviors?
- Vaccine policy: predicts vaccine receipt but (we assume) .red[not] associated with anything else [mimicking random assignment].



Does the cutoff predict treatment?



- Girls "assigned" to HPV program by quarter of birth.
- $\Pr(\text{vaccine})$ jumps discontinuously at cutoff

What does a
credible
natural
experiment
look like?



Note little
impact of
adjustment



Issues related to generalizability

- RD estimates local average impacts around the eligibility cutoff where treated and control units are most similar and results cannot be generalized to units whose scores are further away from the cutoff (unless we assume treatment heterogeneity).
- If the goal is to answer whether the program should exist or not, then RD is likely not the appropriate methodology.
- However, if the question is whether the program should be cut or expanded at the margin, then it produces the local estimate of interest to inform this policy decision

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Be careful, and skeptical

- Correlations between social factors and health are easy to find.
- They do not necessarily reflect causal relationships.
- Need to search hard for alternative explanations.
- Important to consider the strength of evidence in considering interventions.



Are natural experiments always more credible?

- Not necessarily, but probably.
- Key is "as-if" randomization of treatment:
 - If this is credible, it is a much stronger design than most observational studies.
 - Should eliminate self-selection in to exposure groups.
- Allows for simple, transparent analysis of average differences between groups.
- Allows us to rely on weaker assumptions.

Assumptions still matter!

- Quasi-experimental studies are still observational.
- Most credible if they create unconditional randomized treatment groups (e.g., lottery).
- Credibility is continuous, not binary.
- I worry about the cognitive impact of the "quasi-experimental" label.



Back to basics: assumptions and costs

- Major benefit of randomized evaluations are that few assumptions are needed to estimate a causal effect.
- Necessary assumptions can often be checked.
- Non-randomization means more assumptions, more possibility for assumptions to be violated.
- Should lead us to spend lots of time trying to test the credibility of these assumptions.
 - How good is "as-if random"?
 - Are there compelling non-causal alternative explanations for the observed results?
- Not all non-randomized designs are created equal.