Challenges, advantages, and limitations of quasi-experimental approaches to evaluate interventions on health inequalities

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"Smarter Choices for Better Health", Erasmus University, 12 Oct 2018

Outline

Background

- Longstanding concerns about persistent health inequalities.
- Challenges with causal inference of social exposures.
- Much of social epidemiology focused on trying to "explain" away inequalities.
- More recent calls to think about interventions.

Policymakers' Context for Health Inequalities

- Interviews with UK health policymakers in the early 2000s were disappointing for those wanting their research to have "impact".
- The "inverse evidence law" (Petticrew 2004[1]): "...relatively little [evidence] about some of the wider social economic and environmental determinants of health, so that with respect to health inequalities we too often have the right answers to the wrong questions."
- Problem of "policy-free evidence": an abundance of research that does not answer clear, or policy relevant questions.

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- We want to know:
  - Did the program work? If so, for whom? If not, why not?
  - 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.

Randomized Trials vs. Observational Studies

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- Good quasi-experiments do (1) and (2), but not (3).
- Because there is no control over assignment, the credibility of quasi-experiments hinges on how good "as-if random" approximates (2).

Problem of Social Exposures

- Many social exposures/programs cannot be randomized by investigators:
  - Unethical (poverty, parental social class, job loss)
  - Impossible (ethnic background, place of birth)
  - Expensive (neighborhood environments)

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- Some exposures are hypothesized to have long latency periods (many years before outcomes are observable).
- Effects may be produced by complex, intermediate pathways.
- We need alternatives to RCTs.

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- Policy programs do not typically select people to treat at random.
  - Programs target those that they think are most likely to benefit.
  - Programs implemented decisively non-randomly (e.g., provinces passing drunk driving laws in response to high-profile accidents).
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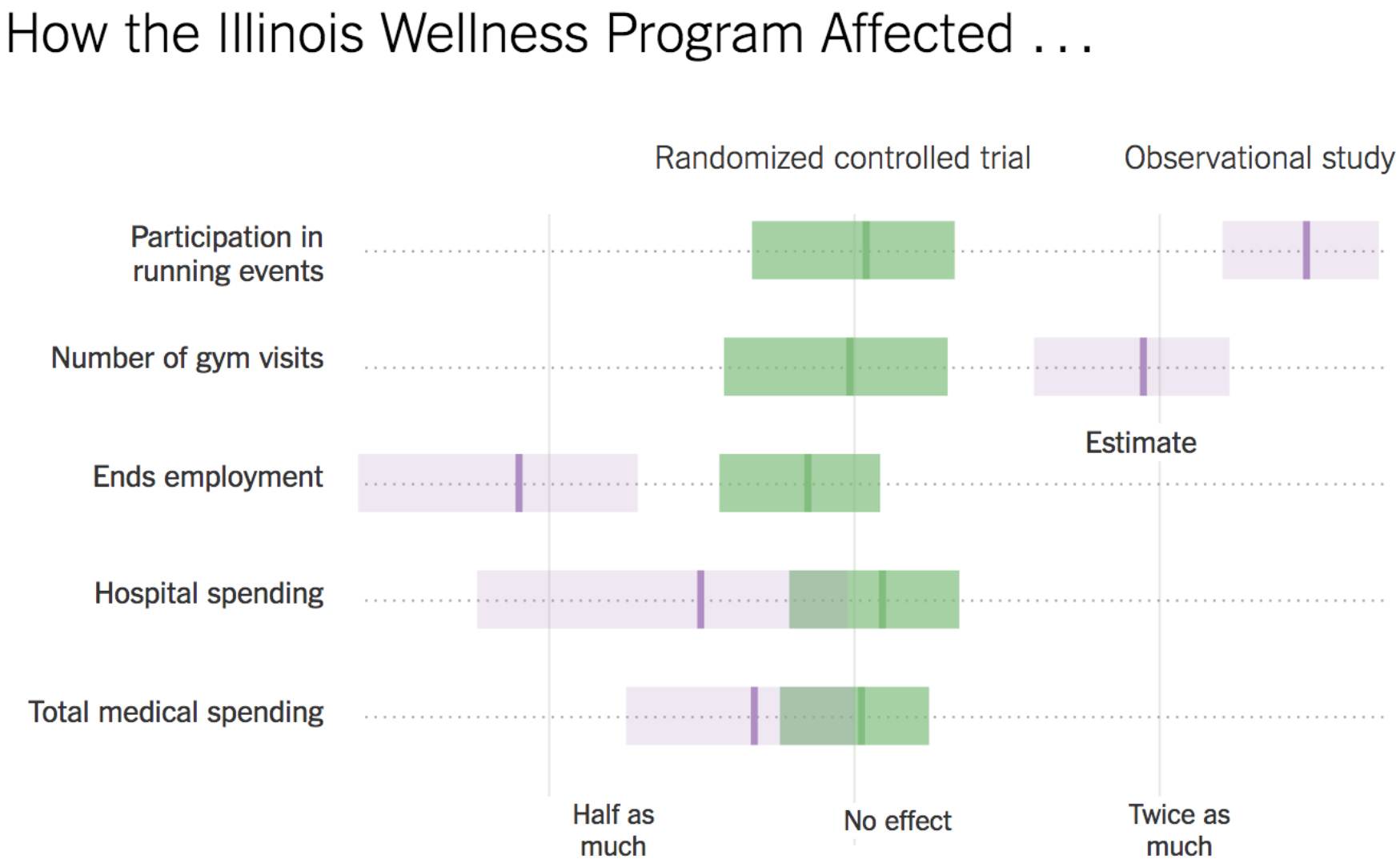
  - Governments deciding to tax (or negatively tax) certain goods.
- People do not choose to participate in programs at random.
  - Screening programs and the worried well.
  - People who believe they are likely to benefit from the program.

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.

Jones et al. 2018 [2]

Why we worry about observational studies



Carroll, *New York Times*, Aug 6, 2018.

Are observational studies getting harder to sell?

- Many observational studies show higher IQs for breastfed children.
- All generally rely on regression adjustment.
- Hard to avoid the issue of residual confounding.
    - "I would argue that in the case of breastfeeding, this issue is impossible to ignore and therefore any study that simply compares breast-fed to formula-fed infants is deeply flawed. That doesn't mean the results from such studies are necessarily wrong, just that we can't learn much from them."
- Can quasi-experiments convince a skeptic like this?

Oster (2015). http://fivethirtyeight.com/features/everybody-calm-down-about-breastfeeding/

Outline

How do quasi-experiments help?

- Quasi-experiments aim to mimic RCTs.
- Typically "accidents of chance" that create:
  1. Comparable treated and control units
  2. Random or "as-if" random assignment to treatment.
- Well-designed quasi-experiments 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.

Strategies based on observables and unobservables

- Most observational study designs select on observables:
  - Stratification
  - Regression adjustment
  - Matching (propensity scores, etc.)

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- Most observational study designs select on observables:
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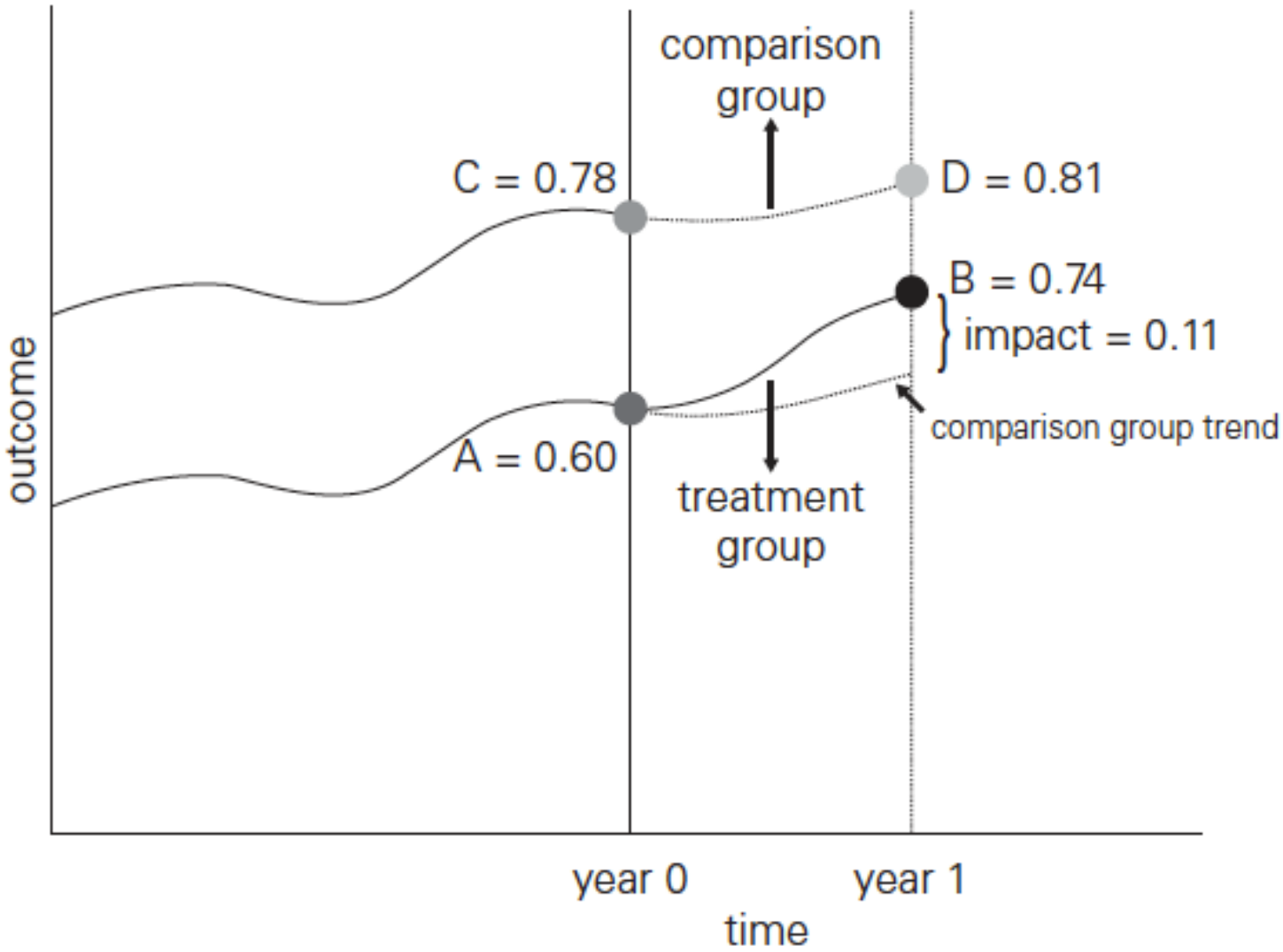
  - Regression adjustment
  - Matching (propensity scores, etc.)
- Quasi-experimental strategies that select on unobservables:
  - Interrupted time series (ITS)
  - Difference-in-differences (DD)
  - Synthetic controls (SC)
  - Instrumental variables (IV)
  - Regression discontinuity (RD)

Visual Intuition of (good) DD



Gertler (2016) [3]

The Effect of Mandatory Seat Belt Laws on Seat Belt Use by Socioeconomic Position

Sam Harper
Erin C. Strumpf
Scott Burris
George Davey Smith
John Lynch

Abstract

We investigated the differential effect of mandatory seat belt laws on seat belt use among socioeconomic subgroups. We identified the differential effect of legislation across higher versus lower education individuals using a difference-in-differences model based on state variations in the timing of the passage of laws. We find strong effects of mandatory seat belt laws for all education groups, but the effect is stronger for those with fewer years of education. In addition, we find that the differential effect by education is larger for mandatory seat belt laws with primary rather than secondary enforcement. Our results imply that existing socioeconomic differences in seat belt use would be further mitigated if all states upgraded to primary enforcement.

Harper et al. 2014 [4]

Study design

- US states pass mandatory laws at different times.
- Effect of legislation is identified by within-state changes after legislation, relative to changes in other states.
- Assumption is that the precise timing of legislation is random
- Study of legislative process suggests this is credible.

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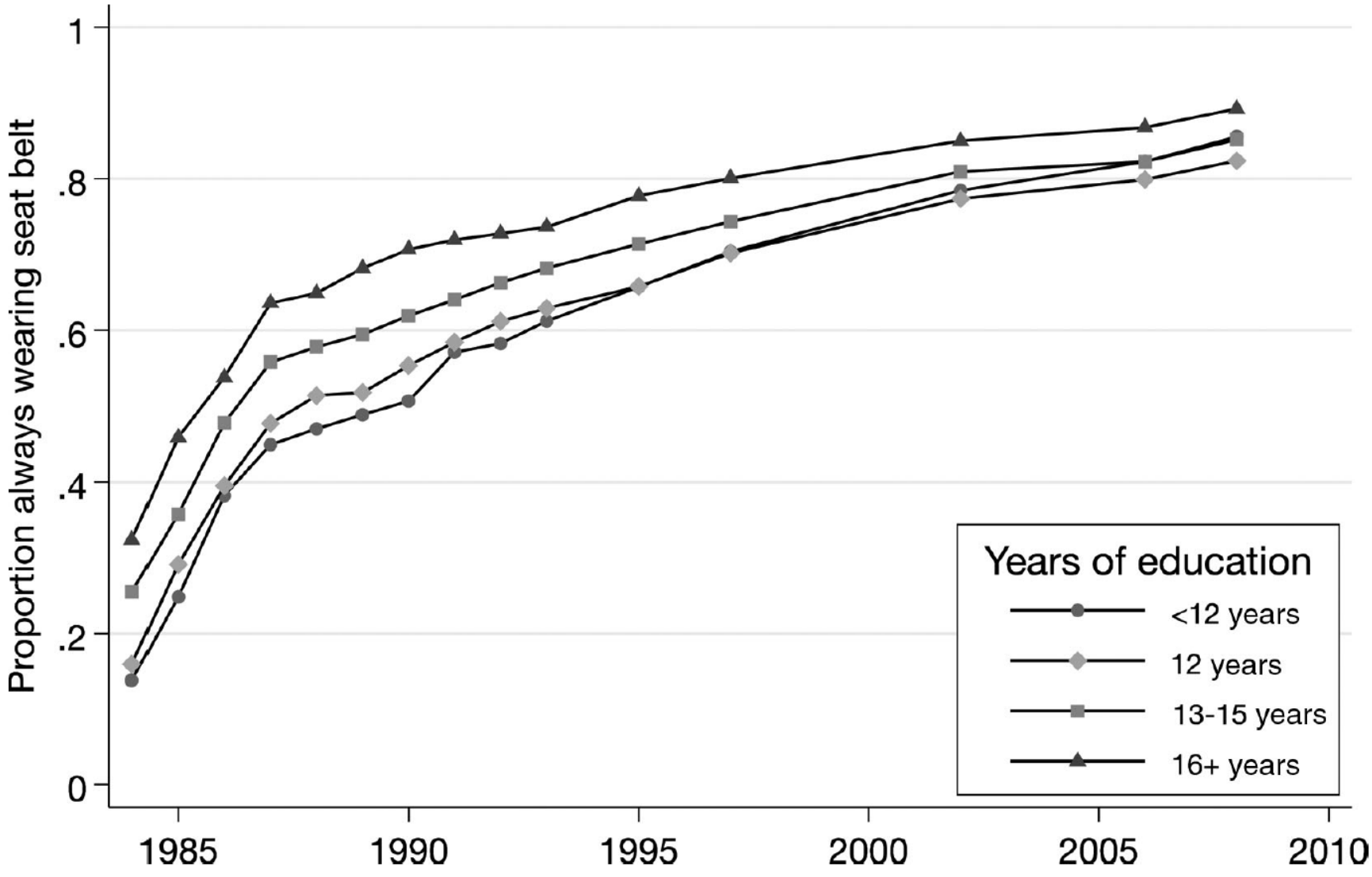
- Two things to worry about:
  - "Safer" states may pass laws and also have higher belt use.
  - Belt use increasing for other reasons (social norms).
  - Likely to lead to biased estimates of policy.
- We control for these biases using difference-in-differences.


Proportion always wearing seat belt
0
.2
.4
.6
.8
1
1985
1990
1995
2000
2005
2010
Years of education
<12 years
12 years
13-15 years
16+ years

Results for % Always Using Seat Belt

- >50% overestimation of policy impact without control for time trends.

	Impact of Mandatory Seat Belt Law (pct pts)	
Education Group	β* (95% CI)	β** (95% CI)
<12 years	37 (33, 40)	23 (17, 29)
12 years	36 (33, 39)	21 (16, 25)
13-15 years	32 (29, 35)	17 (13, 21)
16+ years	31 (28, 35)	17 (12, 22)

*Adjusted for age, age^2, sex, race, ethnicity, marital status, household income, employment, smoking, BMI, past month alcohol use, past month binge drinking, past month heavy drinking, ever driven while intoxicated, **state fixed effects**.
** Plus year fixed effects

A "null" example

Effect of Massachusetts healthcare reform on racial and ethnic disparities in admissions to hospital for ambulatory care sensitive conditions: retrospective analysis of hospital episode statistics

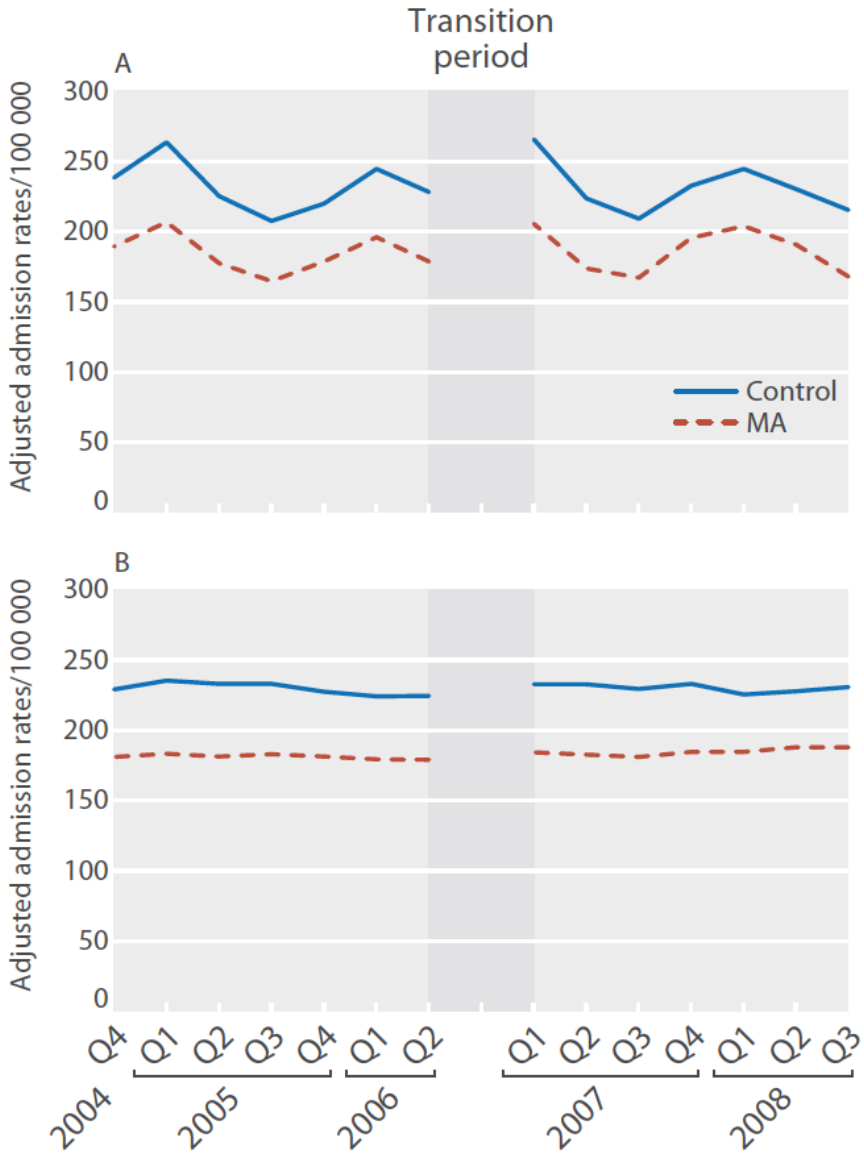
Danny McCormick,[1] Amresh D Hanchate,[2,3] Karen E Lasser,[3] Meredith G Manze,[3] Mengyun Lin,[3] Chieh Chu,[3] Nancy R Kressin[2,3]

- Evaluated impact of MA reform on hospital admissions.
- Compared MA to nearby states: NY, NJ, PA.
- Intervention "worked": % uninsured halved (12% to 6%) from 2004-06 to 2008-09.
- No change in disparities in admission rates between blacks and whites (−1.9%, −8.5% to 5.1%)

McCormick et al. 2015 [5]

Visual evidence: comparable 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.



Potential drawbacks of quasi-experimental approaches

- How good is "as-if" random? (need "shoe-leather")
- Credibility of additional (modeling) assumptions.
- Relevance of the intervention.
- Relevance of population.

Freedman 1991 [6], Rosenbaum 2017 [7]

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.
- Craig et al. [8] define natural experiments as: "any event not under the control of a researcher that divides a population into exposed and unexposed groups."

Natural experiment examining the longitudinal association between change in residential segregation and youth cardiovascular health across race/ethnicity and gender in the USA

Emily M D'Agostino,[1] Hersila H Patel,[1] Zafar Ahmed,[1] Eric Hansen,[1] M Sunil Mathew,[2] Maria I Nardi,[1] Sarah E Messiah[2,3]

Our results provide evidence that cardiovascular health outcomes can be improved for minority youth who are exposed to reduced racial/ethnic residential segregation

D'Agostino et al. 2018 [9]

Is this a quasi-experiment?

- Authors' introduction:

  We hypothesised that minority youth participating at park sites with lower residential segregation relative to their home neighbourhood would have greater improvements in cardiovascular health compared with those at park sites with the same or higher levels of residential segregation.

- Methods:

  The Fit2Play programme is a 10-month (entire school year) daily afterschool programme that takes place from 14:00 to 18:00 and is offered in 34 different sites throughout the county. Participants self-select which park site to attend.

- Is this a credible comparison?

Quasi-experimental "devices"

- Observational studies are ambiguous.
- Many potential explanations (e.g., reverse causation) that may be consistent with the observed data.
- Quasi-experimental devices (e.g., unaffected control groups, placebo tests) aim to reduce ambiguity regarding alternative explanations.

> *The devices focus attention on aspects of the data at hand that might reveal unmeasured biases if such biases are present, aspects that might distinguish an actual treatment effect from an unmeasured bias. ... A successful quasi-experiment feels like what it is intended to be: a fair minded interpretation of alternative interpretations in light of each available source of relevant evidence.*

Rosenbaum 2017 [7]

Preventive Medicine 116 (2018) 19–26

Contents lists available at ScienceDirect

Preventive Medicine

journal homepage: www.elsevier.com/locate/ypmed

Organised population-based programmes and change in socioeconomic inequalities in mammography screening: A 1992–2012 nationwide quasi-experimental study

Stéphane Cullati[a,b,c,d,*,1], Martina von Arx[d,1], Delphine S. Courvoisier[c], José Luis Sandoval[a,c], Orly Manor[e], Claudine Burton-Jeangros[b,d], Christine Bouchardy[f], Idris Guessous[a,g]

[a] Unit of Population Epidemiology, Department of Community Medicine, Primary Care and Emergency Medicine, Geneva University Hospitals, Switzerland
[b] Swiss NCCR "LIVES - Overcoming Vulnerability: Life Course Perspectives", University of Geneva, Switzerland
[c] Department of General Internal Medicine, Rehabilitation and Geriatrics, University of Geneva, Switzerland
[d] Institute of Demography and Socioeconomics, University of Geneva, Switzerland
[e] School of Public Health and Community Medicine, Hebrew University-Hadassah, Israel
[f] Geneva Cancer Registry, Global Health Institute, University of Geneva, Switzerland
[g] Department of Ambulatory Care and Community Medicine, University of Lausanne, Switzerland

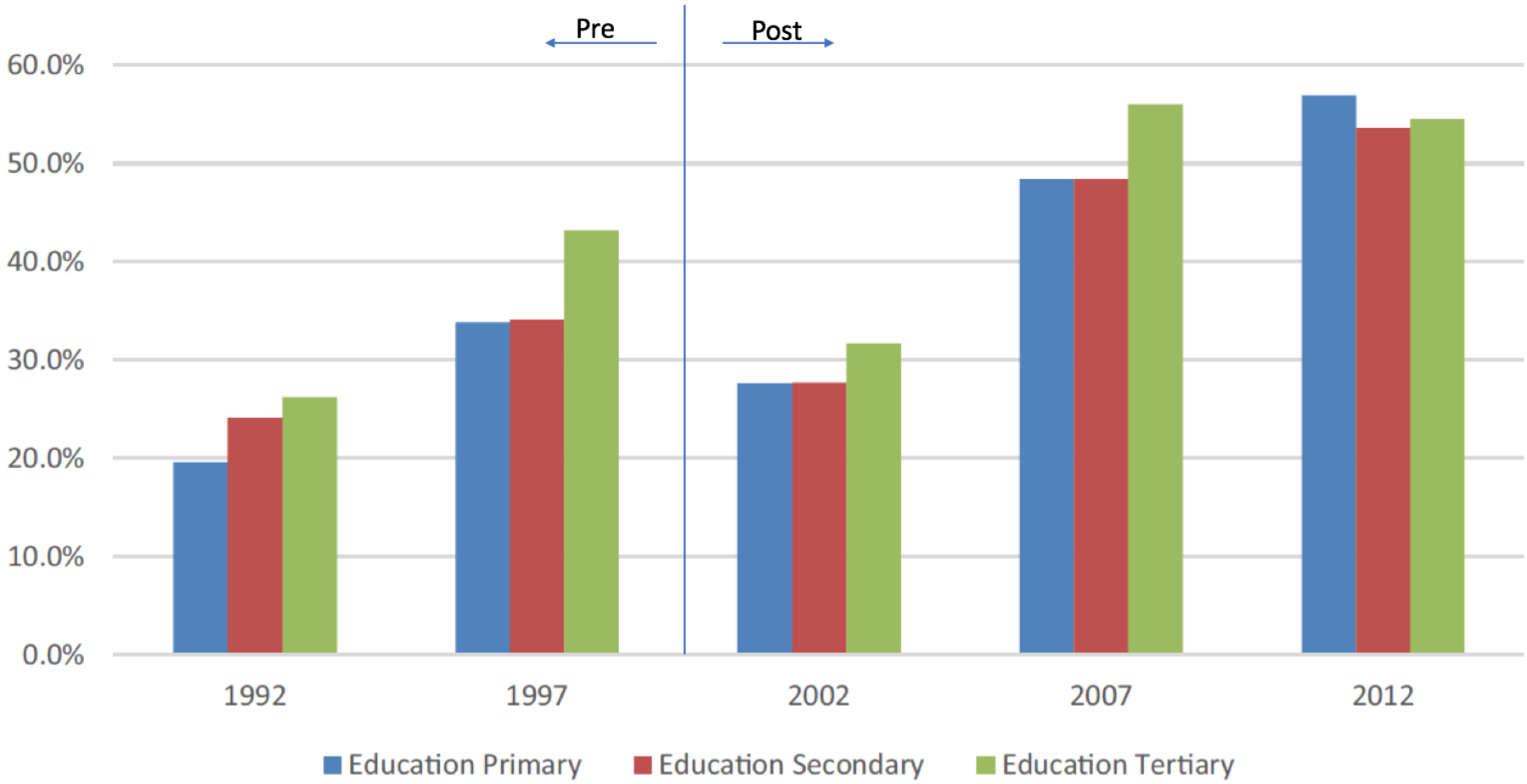
What makes this quasi-experimental?

- According to the authors :
  - "...some Swiss regions do have organised breast cancer programmes, while others still rely on opportunistic screening."
  - "This ecological quasi-experimental context allows analysing the evolution of socioeconomic inequalities in mammography screening over time in the different regions."
- No discussion of treatment assignment mechanism:
  - How do regions decide whether to implement?
  - Is it "as-if" random?
- No discussion of potential biases of the treatment effect:
  - "To assess the robustness of our findings, different coding schemes for each socioeconomic indicator were tested"

Cullati et al. 2018 [10]

Results for Education

No effect estimates on % screened but ...



And yet...

- Causal conclusions! (kind of?)

> Prevalence of mammography screening increased over the last two decades, and one driver was the progressive introduction of regional programmes. Socioeconomic inequalities decreased over time in Switzerland, but not because of the implementation of organised mammography screening programmes. Nevertheless, these programmes reversed the gap in mammography screening uptake between employed and not employed women (the latest exceeded the prevalence of employed women by 2012) and attenuated educational and income-related inequalities. Specific approaches to reach women working full time and improve their access to organised programmes may be beneficial.

Meanwhile...one year earlier....

Eur J Health Econ (2017) 18:649–665
DOI 10.1007/s10198-016-0845-7

ORIGINAL PAPER

The effects of organized screening programs on the demand for mammography in Switzerland

Mark Pletscher[1,2]

✉ Mark Pletscher
markpletscher@gmx.ch

[1] Institute of Economic Research, University of Neuchâtel, Rue A.-L. Breguet 2, 2000 Neuchâtel, Switzerland

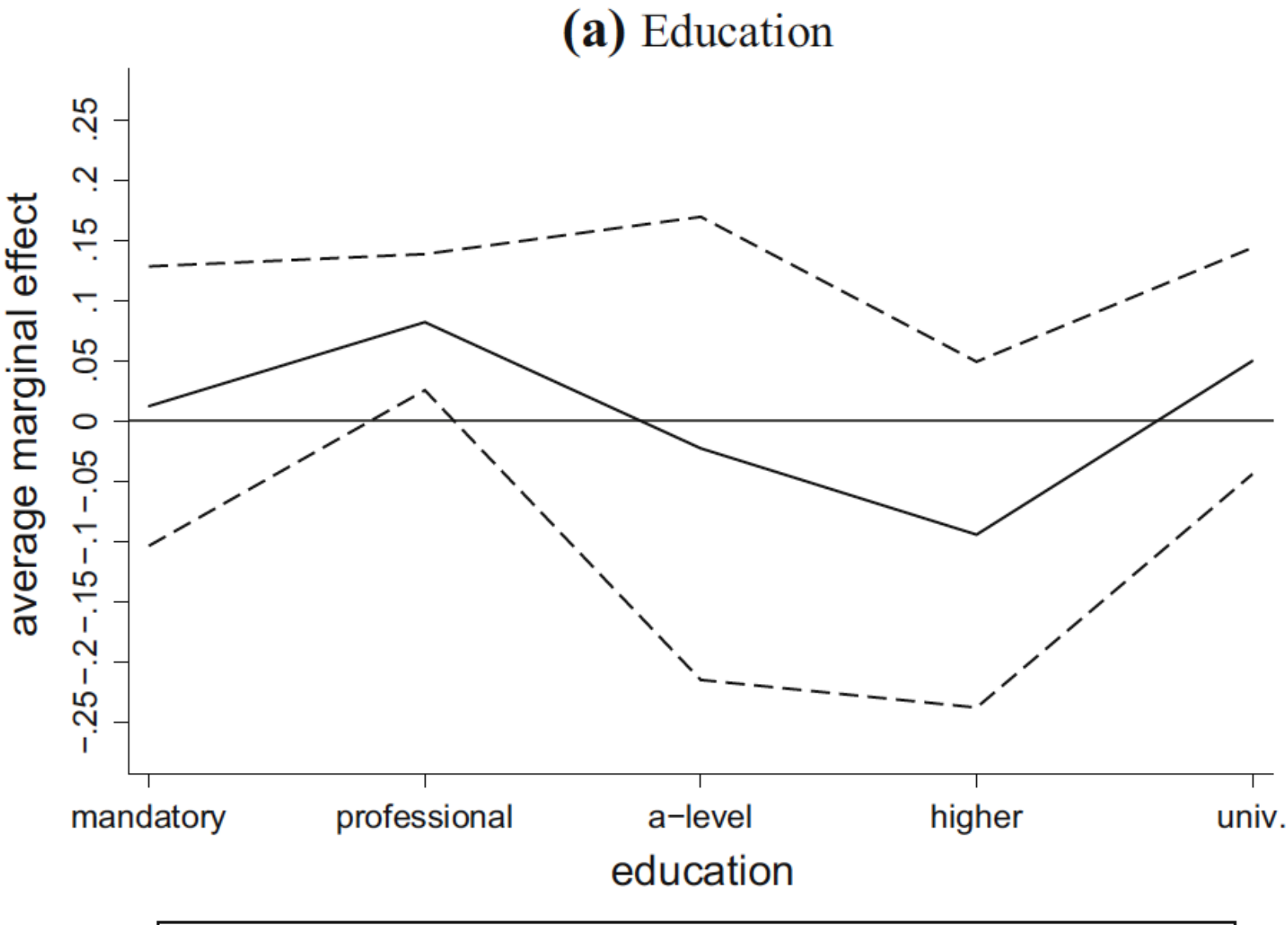
[2] Winterthur Institute of Health Economics, Zurich University of Applied Sciences, Gertrudstrasse 15, 8400 Winterthur, Switzerland

What makes this quasi-experimental?

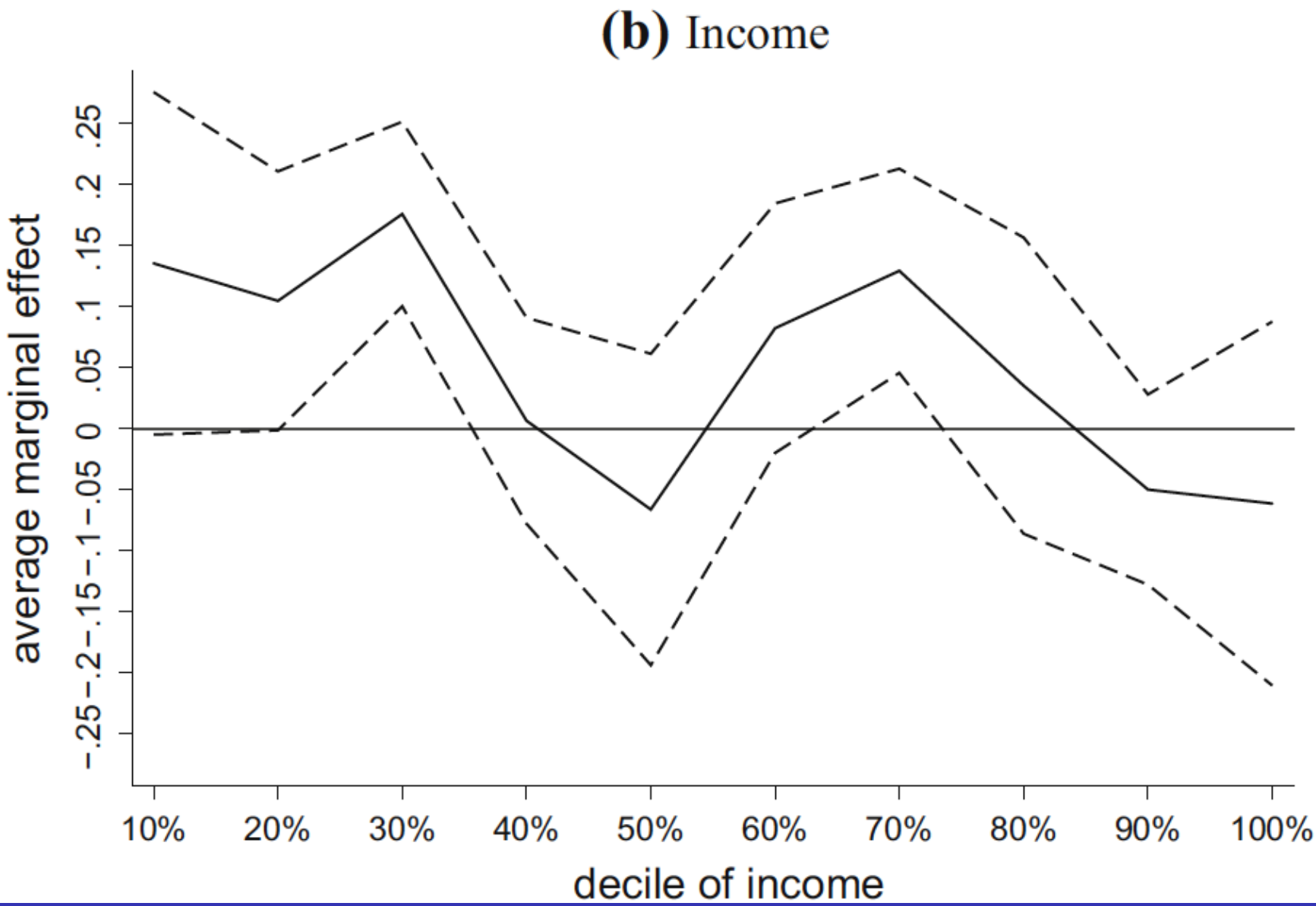
- Evaluation of identical program with same data
- Clear objective: "to estimate the effect of organized mammography screening programs on screening initiation in screening cantons."
- Concerns about identification:
  - Include region and time fixed effects
  - Functional form of model
- Evaluating alternative explanations by design:
  - Placebo tests on pre-intervention trends.
  - Triple differences model (used 40-49yo women who do not receive invitation letters and who must pay for screening mammograms themselves constitute an additional comparison group).

Pletscher 2017 [11]

Some evidence of differences by education



Stronger effects for low income



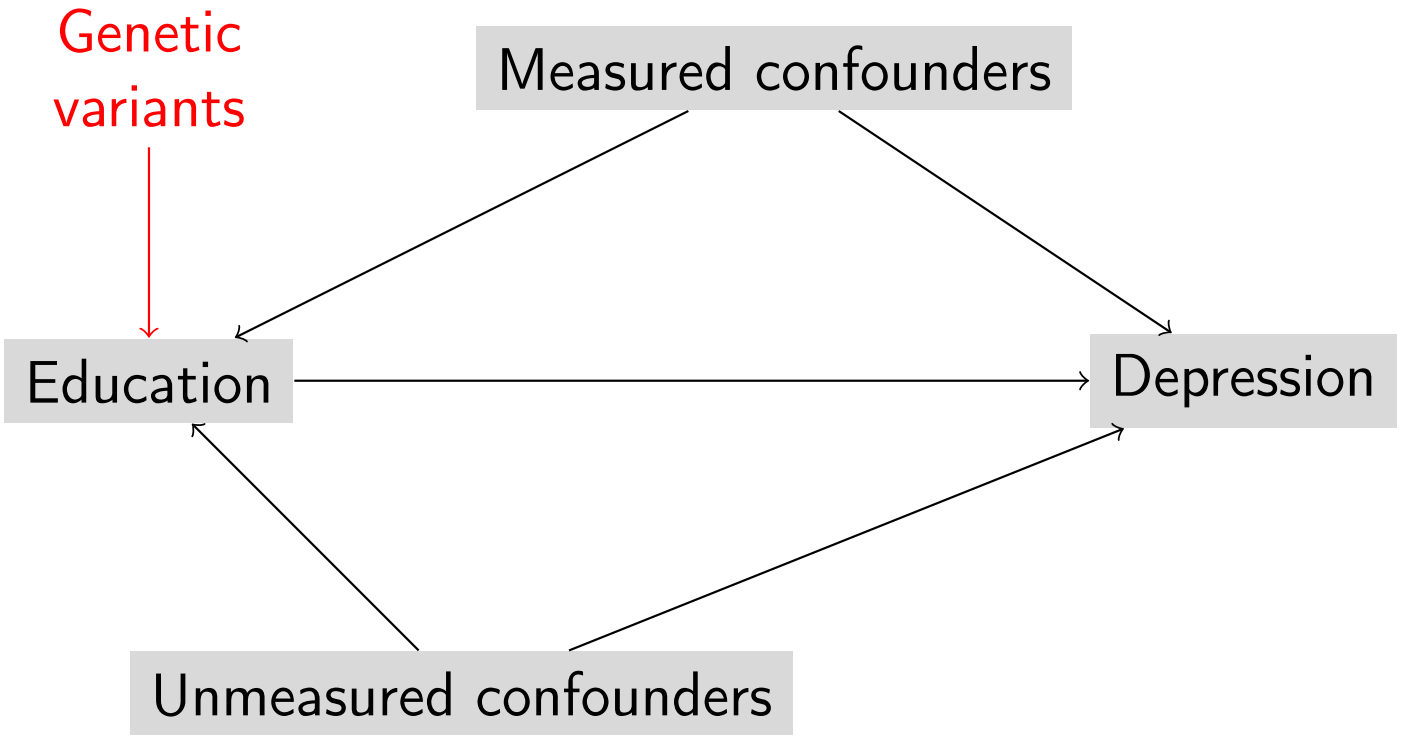
Thinking about interventions

- A good RCT is also characterized by a well-defined causal question.
  - Question and analytic approach pre-specified in study protocol.
- Most observational studies:
  - Question, methods, analysis decided after data collection.
- We should aim to emulate a target trial:
  - Eligibility criteria, treatment strategy, randomized assignment, start/end of follow up, outcomes, causal contrast, analysis plan.
- We should ask how well our quasi-experiment approximates the RCT we would do.
- Should specify a well-defined intervention.

Garćıa-Albéniz et al. 2017 [12]

Example of instrumental variable: Genes

- Does education (T) affect depression (Y)?
- **Instrument**: differences in genetic variants [mimicking random assignment].



Gage et al. 2018 [13]

Preventive Medicine 115 (2018) 134–139

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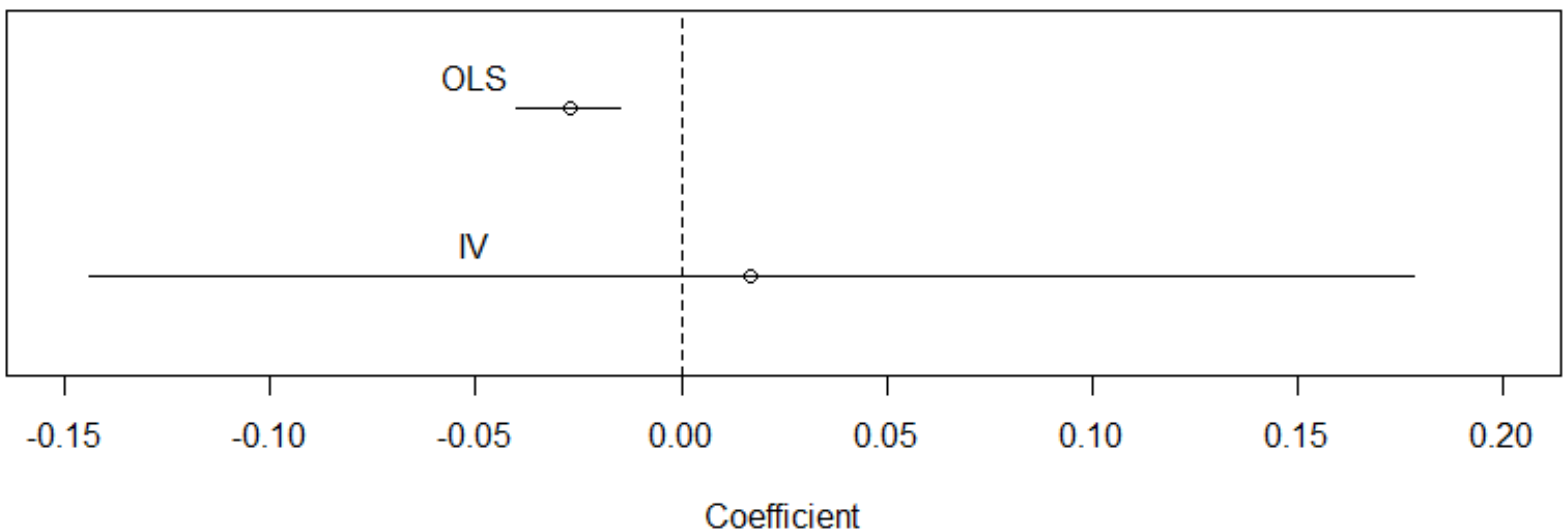
Preventive Medicine

journal homepage: www.elsevier.com/locate/ypmed

Does education protect against depression? Evidence from the Young Finns Study using Mendelian randomization

Jutta Viinikainen[a,*], Alex Bryson[b,g], Petri Böckerman[a,g,h], Marko Elovainio[c], Niina Pitkänen[d], Laura Pulkki-Råback[c], Terho Lehtimäki[e], Olli Raitakari[f], Jaakko Pehkonen[a]

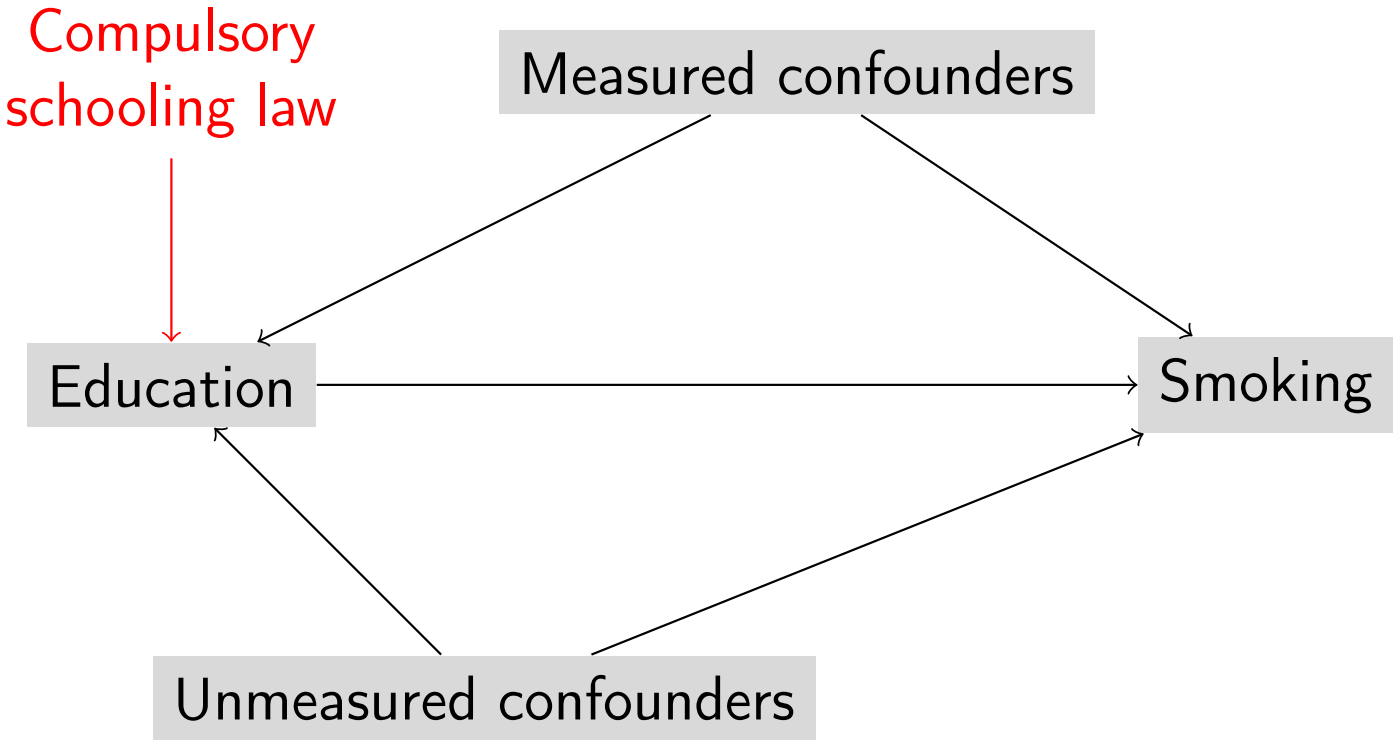
In Ordinary Least Squares (OLS) estimation years of education in 2007 were negatively associated with depressive symptoms in 2007. However, the results based on Mendelian randomization [IV] suggested that the effect is not causal. ... This suggests that education policies are not viable to address the mental health problems.



Viinikainen et al 2018 [14]

Example of instrumental variable: Policies

- Does education (T) affect smoking (Y)?
- **Instrument**: changes in compulsory schooling laws [mimicking random assignment].



Glymour et al. 2008[15], Hamad et al. 2018[16], Galama et al. 2018[17]

Contents lists available at ScienceDirect

Social Science & Medicine

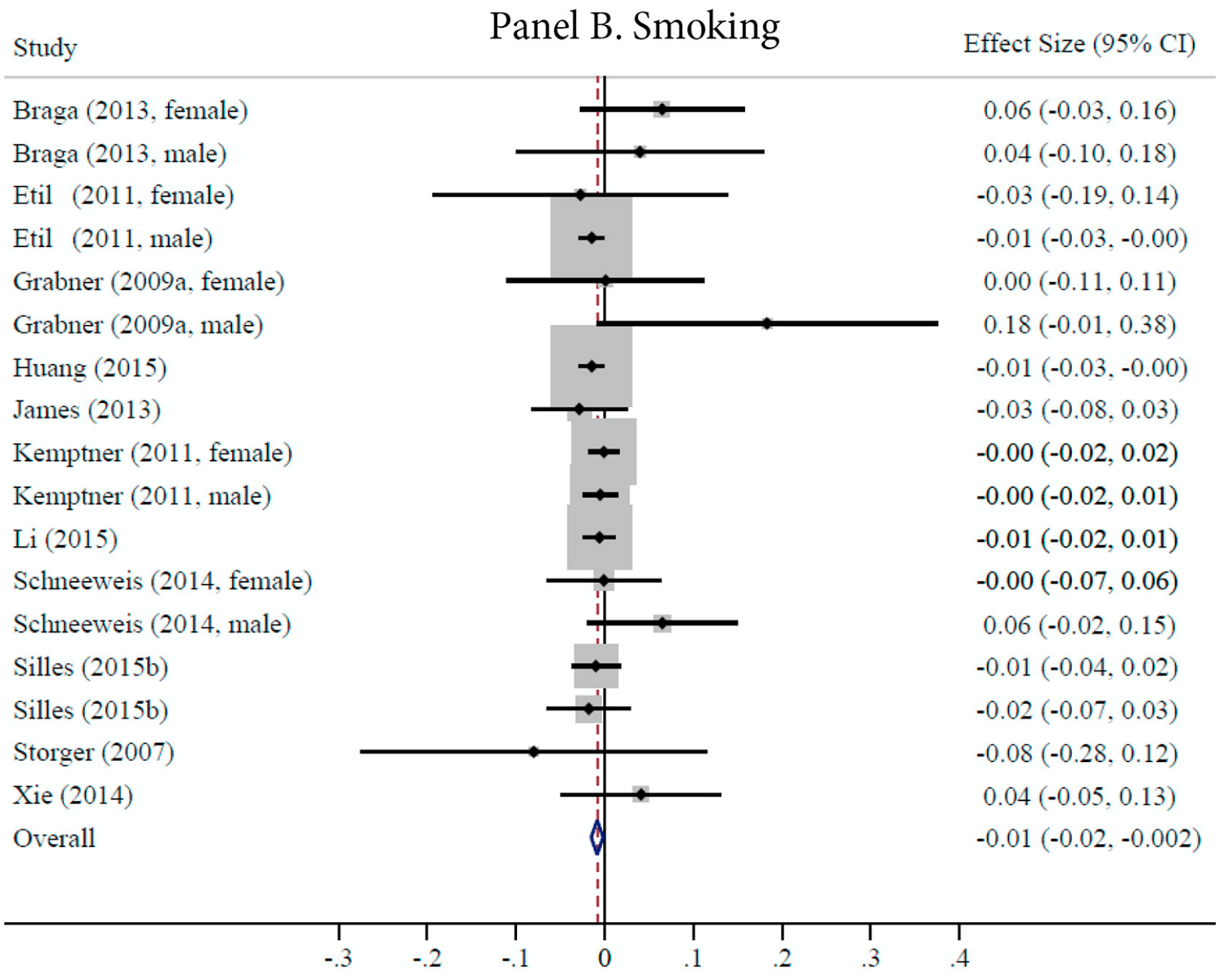
journal homepage: www.elsevier.com/locate/socscimed

Review article

How and why studies disagree about the effects of education on health: A systematic review and meta-analysis of studies of compulsory schooling laws

Rita Hamad[a,*], Holly Elser[b,c], Duy C. Tran[c], David H. Rehkopf[c], Steven N. Goodman[c]

[a] University of California San Francisco, Philip R. Lee Institute for Health Policy Studies, Department of Family & Community Medicine, 995 Potrero Avenue, Building 80, Ward 83, San Francisco, CA, 94110, USA
[b] University of California Berkeley, School of Public Health, Division of Epidemiology, Berkeley, CA, USA
[c] Stanford University, School of Medicine, Stanford, CA, USA


Panel B. Smoking
Study
Effect Size (95% CI)
Braga (2013, female)
0.06 (-0.03, 0.16)
Braga (2013, male)
0.04 (-0.10, 0.18)
Etil (2011, female)
-0.03 (-0.19, 0.14)
Etil (2011, male)
-0.01 (-0.03, -0.00)
Grabner (2009a, female)
0.00 (-0.11, 0.11)
Grabner (2009a, male)
0.18 (-0.01, 0.38)
Huang (2015)
-0.01 (-0.03, -0.00)
James (2013)
-0.03 (-0.08, 0.03)
Kemptner (2011, female)
-0.00 (-0.02, 0.02)
Kemptner (2011, male)
-0.00 (-0.02, 0.01)
Li (2015)
-0.01 (-0.02, 0.01)
Schneeweis (2014, female)
-0.00 (-0.07, 0.06)
Schneeweis (2014, male)
0.06 (-0.02, 0.15)
Silles (2015b)
-0.01 (-0.04, 0.02)
Silles (2015b)
-0.02 (-0.07, 0.03)
Storger (2007)
-0.08 (-0.28, 0.12)
Xie (2014)
0.04 (-0.05, 0.13)
Overall
-0.01 (-0.02, -0.002)
-.3
-.2
-.1
0
.1
.2
.3
.4

Are we missing the target?

- These may be credible natural experiments:
  - Usually convincing "as-if" random variation in education.
  - More exchangeable treatment groups.

Swanson and Hernan 2018 [18] , Heckley et al. 2018 [19]

Are we missing the target?

- These may be credible natural experiments:
  - Usually convincing "as-if" random variation in education.
  - More exchangeable treatment groups.
- But affect very specific group of compliers:
  - Children who would have left school earlier were it not for the compulsory law.
  - Individuals with higher education if they possess an exposure-increasing genetic variant, but not otherwise.
  - Unclear whether these map onto any actual populations or policies we may consider implementing.

Swanson and Hernan 2018 [18] , Heckley et al. 2018 [19]

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  - Individuals with higher education if they possess an exposure-increasing genetic variant, but not otherwise.
  - Unclear whether these map onto any actual populations or policies we may consider implementing.
- Potentially more relevant policy levers:
  - Early-life interventions.
  - Educational quality.
  - Changes in price, subsidies, or term length of education.

Swanson and Hernan 2018 [18] , Heckley et al. 2018 [19]

Outline

What are quasi-experiments good for?

1. To understand the effect of treatments *induced by policies* on outcomes, e.g., Policy $\rightarrow$ Treatment $\rightarrow$ Outcome:
   - Environmental exposures.
   - Education/income/financial resources.
   - Access to health care.
   - Health behaviors.

Glymour 2014 [20]

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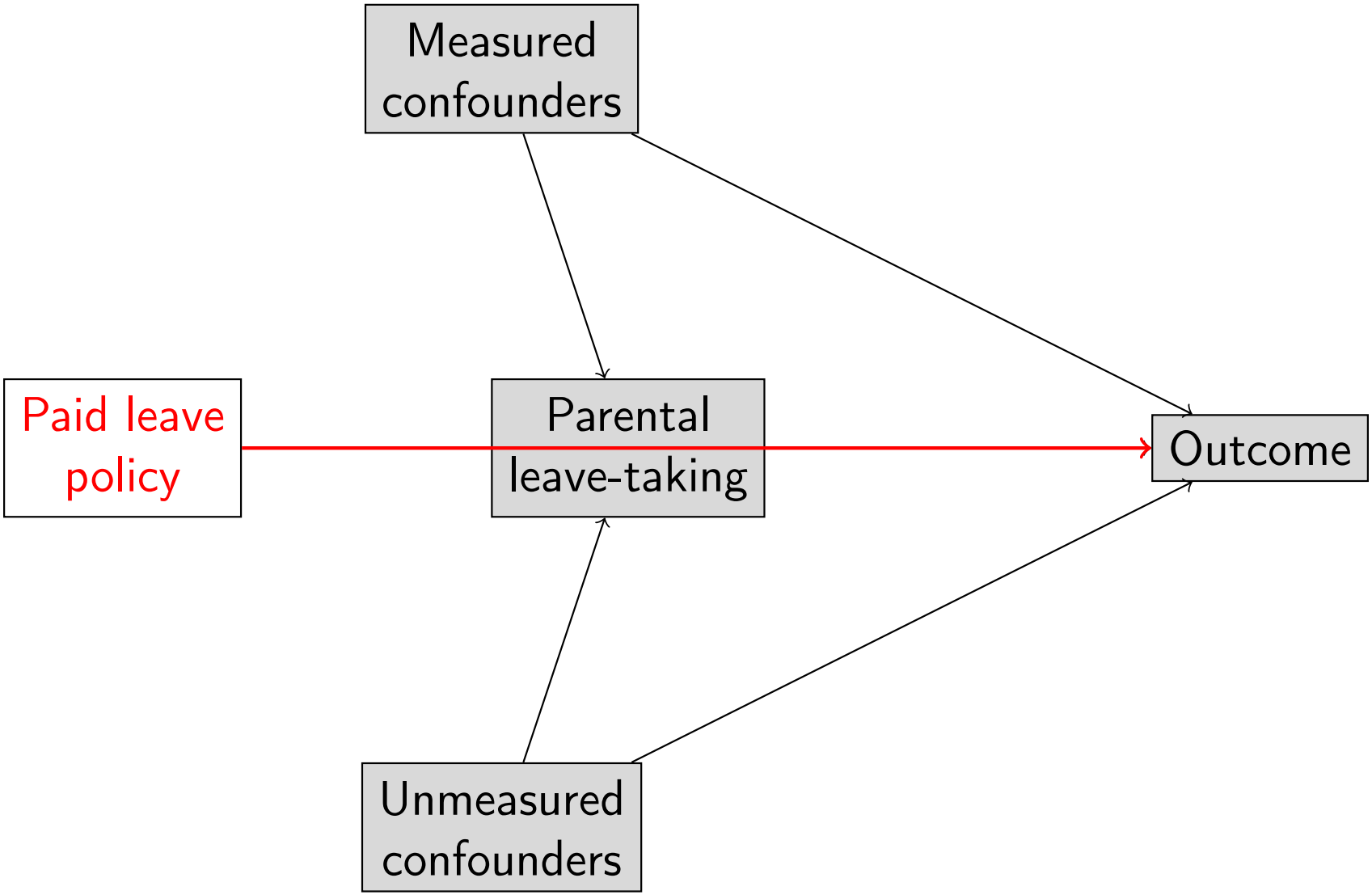
   - Access to health care.
   - Health behaviors.

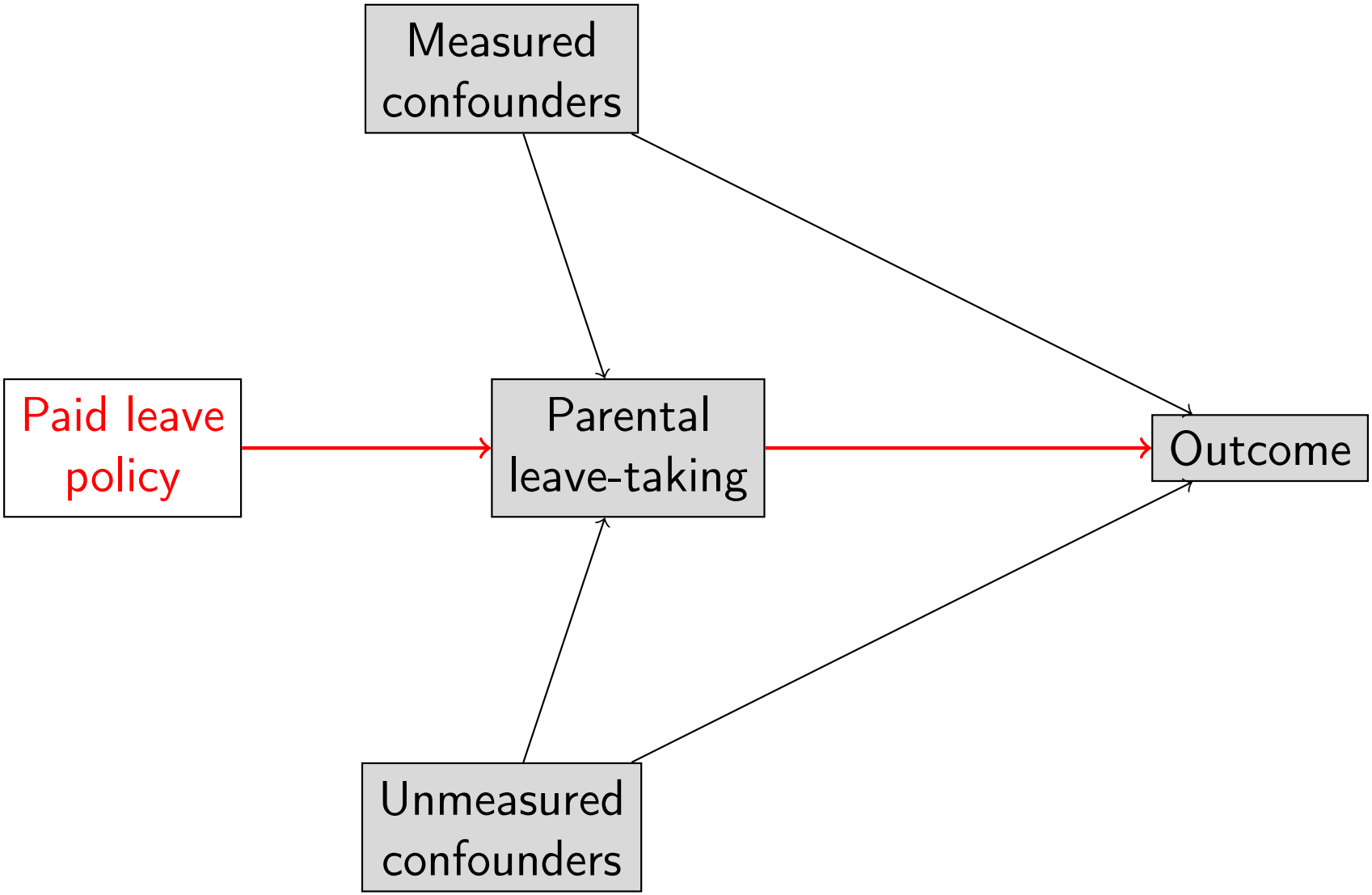
2. To understand the effect of policies on outcomes, e.g., Policy $\rightarrow$ Outcome:
   - Taxes, wages.
   - Environmental legislation.
   - Food policy.
   - Employment policy.
   - Civil rights legislation.

Glymour 2014 [20]

Are we interested in the "ITT" effect?



Or the effect of treatment



Finally, Consider experimenting!

- RCT $\neq$ Gold standard, but can be very powerful and convincing.
- We can control aspects of programs/policies to experimentally increase the probability of exposure in one group vs. another:
  - **Access**: we can randomly select which people are offered access to a program (most common).
  - **Timing**: we can randomly select when people are offered access to a program.
  - **Encouragement**: we can randomly select which people are given encouragement or incentive to participate.
- Each of these aspects can be varied for individuals or groups.

Concluding thoughts

- Quasi-experimental approaches have important strengths.
- However, difficult to find in practice.
- They are still observational: key issue is credibility of assumptions
  - Serious consideration of alternative explanations
  - Robust sensitivity analysis
- Need to think carefully about how the quasi-experiment maps on to hypothetical interventions and the target trial.
- Actual experiments may also be relevant for policy.

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Thank you!

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