

# **Undergraduate Public Finance: Empirical Tools**

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# Definitions

**Empirical public finance:** The use of data and statistical methods to measure the impact of government policy on individuals and markets

e.g., how an increase in taxes affects work behavior

**Correlation:** Two economic variables are correlated if they move together

e.g., height and weight across individuals

**Causality:** Two economic variables are causally related if the movement of one causes the movement of the other

e.g., good nutrition as an infant increases adult height

# Distinction Between Correlation and Causality

There are many examples where causation and correlation can get confused (Cunningham, 2021).

In statistics, this is called the *identification problem*:

**Given that two variables are correlated, how do you identify whether one series is causing another?**

# The Identification Problem

Interpreting a correlation as a causal relationship without sufficient thought to the underlying process generating the data is a common problem.

For any correlation between two variables A and B, there are three possible explanations, one or more of which could result in the correlation:

1. A is causing B
2. B is causing A
3. Some third factor is causing both

The general problem that empirical economists face in trying to use existing data to assess the causal influence of one factor on another is that one cannot immediately go from correlation to causation.

# A Toy Example: SAT Prep Courses

Among Harvard students who took an SAT prep course, SAT scores were 63 points lower than among those who hadn't.

$$\text{SAT Score}_i = 1000 - 63 \cdot \text{Attended Prep Course}_i + \varepsilon_i$$

- Do prep courses reduce scores? (A causes B)
- Do low scores cause people to enroll in prep courses? (B causes A)
- Did some third factor cause both low scores and enrollment? (C causes A and B)

# Formalizing the Identification Problem

Given observations  $i = \{1, \dots, N\}$ , a treatment indicator  $D_i$ , and covariates  $X_i$ , researchers typically estimate:

$$Y_i = \alpha + \beta \cdot D_i + \gamma' X_i + \varepsilon_i$$

**Goal:** interpret  $\beta$  as the causal effect of  $D_i$  on  $Y_i$ .

**Key assumption:**

$$\mathbb{E}[\varepsilon_i | D_i, X_i] = 0.$$

If violated,  $\beta$  combines the causal effect and the effects of confounders.

# Omitted Variable Bias (OVB)

Suppose the true model is

$$Y_i = \alpha + \beta D_i + \delta Z_i + \gamma' X_i + u_i,$$

but  $Z_i$  is unobserved.

If we estimate without  $Z_i$ :

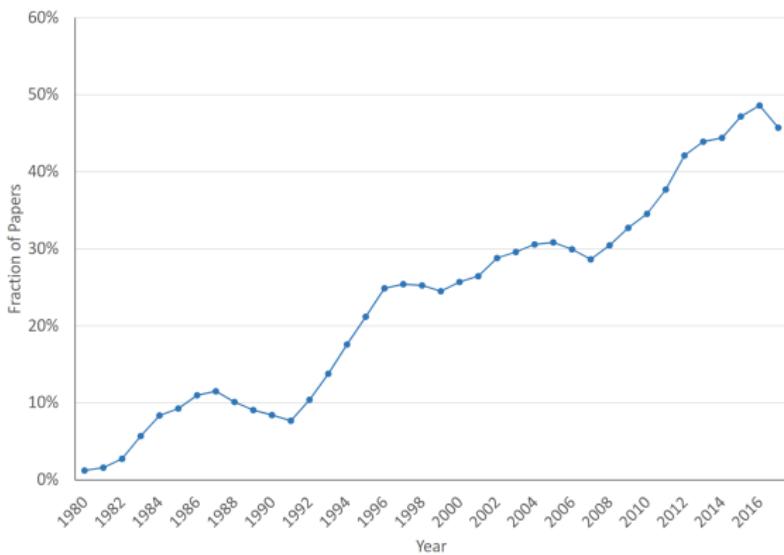
$$Y_i = \alpha + \tilde{\beta} D_i + \gamma' X_i + \varepsilon_i,$$

then

$$\mathbb{E}[\tilde{\beta}] = \beta + \delta \cdot \frac{\text{Cov}(D_i, Z_i | X_i)}{\text{Var}(D_i | X_i)}.$$

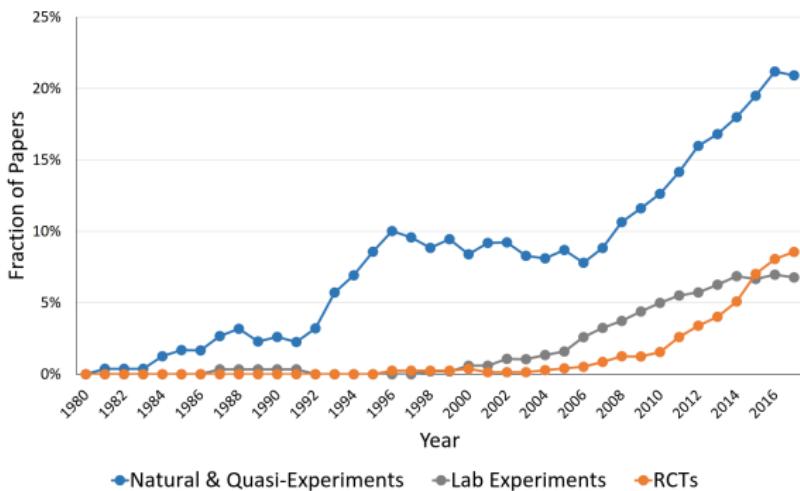
**OVB:** bias arises when  $Z_i$  is correlated with both  $D_i$  and  $Y_i$ .

# The Rise of Identification



**Notes:** The graph shows the fraction of papers that mention the word “identification” in the context of empirical identification. The sample comprises all NBER working papers 1975–2018 tagged “public economics” (4676 papers). See the original slides [here](#).

# The Rise of Experiments



**Notes:** The graph shows the fraction of papers that refer to each type of experiment. See the original slides [here](#).

# Randomized Control Trials

In an RCT, a group of individuals is RANDOMLY divided into two groups.

The treatment group receives the treatment of interest, whereas the control group does not.

Randomization effectively ensures that, in expectation, the treatment is uncorrelated to confounders.

Randomized trials have been used in medicine for many decades and have become very popular in economics in the last 30 years.

# Example: Universal Basic Income (Vivaldi et al., 2024)

## ABSTRACT

We study the causal impacts of income on a rich array of employment outcomes, leveraging an experiment in which 1,000 low-income individuals were randomized into receiving \$1,000 per month unconditionally for three years, with a control group of 2,000 participants receiving \$50/month. We gather detailed survey data, administrative records, and data from a mobile phone app. The transfer caused total individual income excluding the transfers to fall by about \$1,800/year relative to the control group and a 3.9 percentage point decrease in labor market participation. Participants reduced their work hours as a result of the transfers by 1-2 hours/week and participants' partners reduced their work hours by a comparable amount. Among other categories of time use, the greatest increase generated by the transfer was in time spent on leisure. Despite asking detailed questions about amenities, we find no impact on quality of employment, and our confidence intervals can rule out even small improvements. Treated participants broadly increase expenditures, led by spending on non-durable goods and services, with smaller increases in spending on durable goods and human capital. We observe no significant effects on degree attainment, though younger participants may pursue more formal education. Measures of subjective well-being are higher among treated participants in the first year of the transfers but then revert to control group levels. Overall, our results suggest a moderate labor supply effect that does not appear offset by other productive activities.

# Methods for Observational Data

In many settings, we cannot design RCTs to answer economic problems, and we thus rely on observational data.

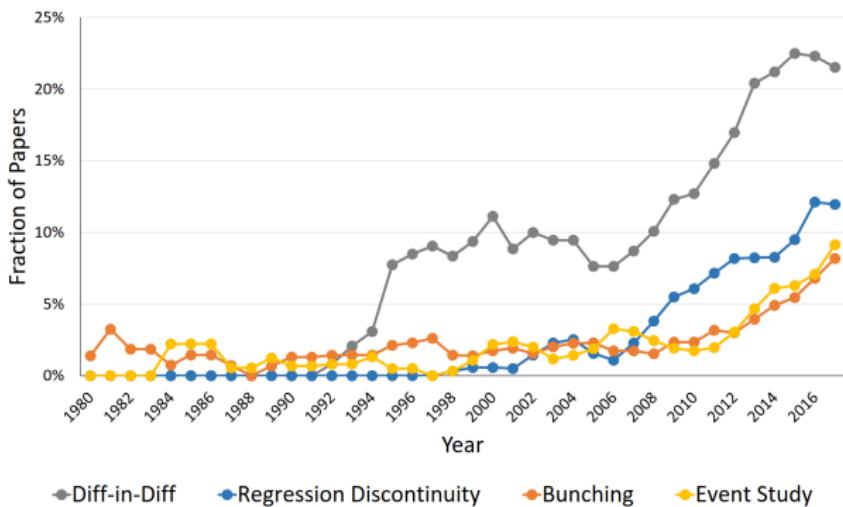
As soon as we do not have proper randomization, the identification problem is very tricky to deal with.

Thankfully, researchers have developed clever identification strategies that aim to "recreate" the random allocation of treatment.

We call those "quasi-experimental methods":

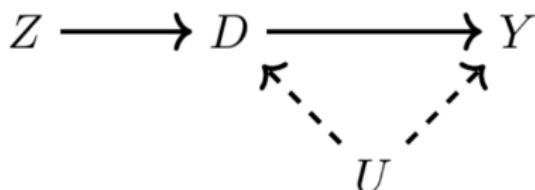
- Instrumental variables (IV)
- Regression Discontinuity Designs (RDD)
- Difference in differences (DID)

# The Rise of Quasi-experiments



**Notes:** The graph shows the fraction of papers that refer to each type of quasi-experiment. See the original slides [here](#).

# Instrumental Variables



We care about the relationship between a treatment  $D$  and an outcome  $Y$ .

There are many confounder variables  $U$  that affect  $D$  and  $Y$ .

An instrumental variable,  $Z$  predicts  $D$  (relevance) but is uncorrelated with the confounders  $U$  (exclusion restriction).

⇒ Allows for the identification of the *causal* effect of  $D$  on  $Y$

# Two-Stage Least Squares (2SLS)

**Step 1: First stage**      Regress treatment  $D$  on the instrument  $Z$  (and controls  $X$  if any).

$$D = \pi_0 + \pi_1 Z + \pi_2' X + v \quad (1)$$

⇒ Obtain predicted values  $\hat{D}$ .

**Step 2: Second stage**      Regress outcome  $Y$  on  $\hat{D}$  (and controls  $X$ ).

$$Y = \beta_0 + \beta_1 \hat{D} + \beta_2' X + \varepsilon \quad (2)$$

**Interpretation:**  $\beta_1$  is the causal effect of  $D$  on  $Y$  identified using  $Z$ .

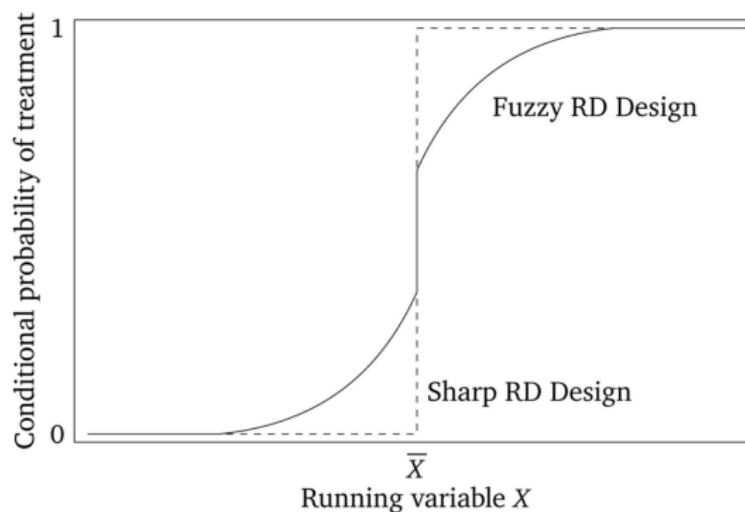
# Example: Military Service and Labor Supply

**Context:** Vietnam-era military draft in the U.S. (Angrist, 1990)

- Random lottery assigned draft priority numbers by birth date.
  - Low numbers  $\Rightarrow$  high probability of military service.
  - Instrument: Draft eligibility ( $Z$ ) predicts actual service ( $D$ ).
  - Outcome: Post-service earnings ( $Y$ ).
- Military service reduced later civilian earnings for those induced to serve (15% earnings loss for white veterans compared to non-veterans, as of around 10 years after discharge).

# Regression Discontinuity Design

RDDs exploit discontinuous jumps in the probability of treatment assignment along some running variable.



# Example: Close-election RDD (Casarico et al., 2022)

## Women and local public finance



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### ARTICLE INFO

*JEL classification:*  
E62, J16, H71, H72

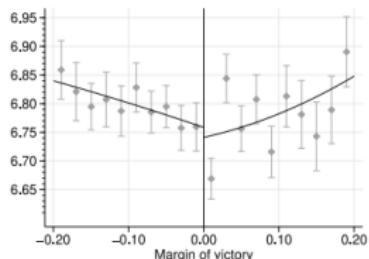
*Keywords:*  
Gender  
Municipal government  
Local public finance  
Regression discontinuity

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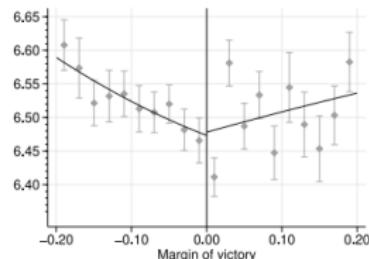
### ABSTRACT

Does the gender of the mayor affect the size and composition of public expenditures and revenues? Using a sharp regression discontinuity design in close mixed gender races for the election of mayors in Italian municipalities in the period 2000–2015, we find no significant differences in policies implemented by male and female mayors. We explore whether the result masks heterogeneity by gender composition of the local government and by electoral rules according to which a mayor is elected. We find some evidence that female mayors devote a larger share of spending to the environment when there are more women in the municipal council, whereas they reduce the amount of resources going to social spending under the run-off relative to the single round system.

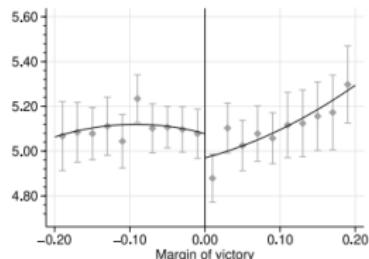
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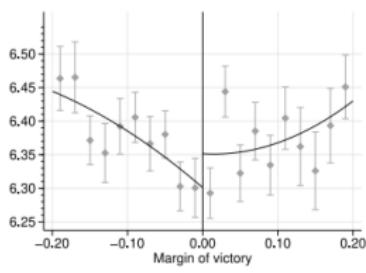
(A) Total expenditures



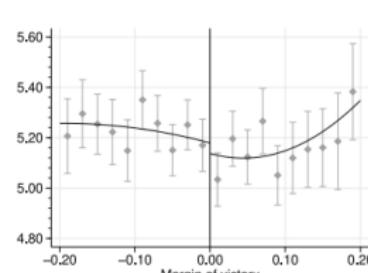
(B) Current expenditures



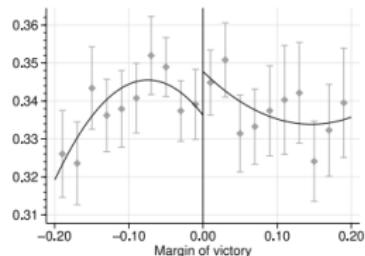
(C) Capital expenditures



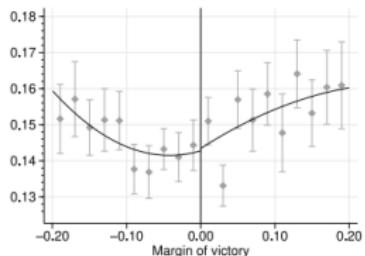
(D) Revenues from taxes and fees



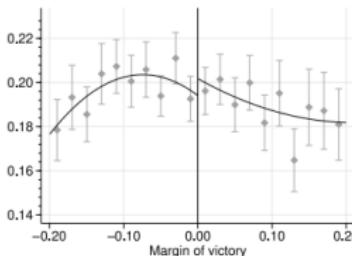
(E) Other revenues



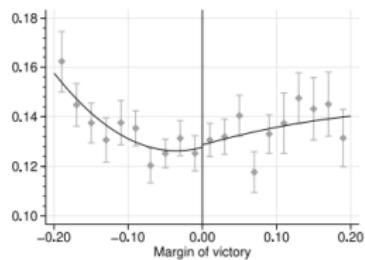
(A) Admin., Justice &amp; Police



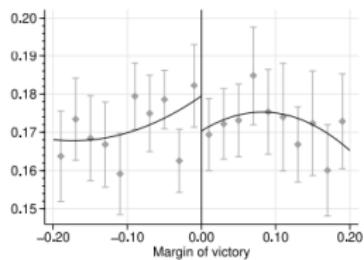
(B) Culture &amp; Education



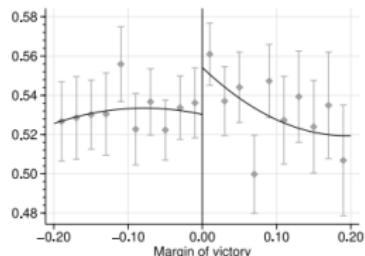
(C) Environment



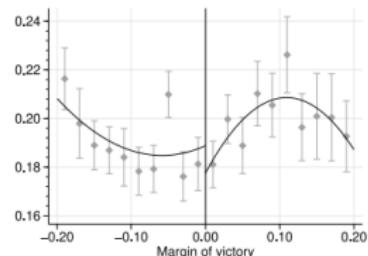
(D) Social services



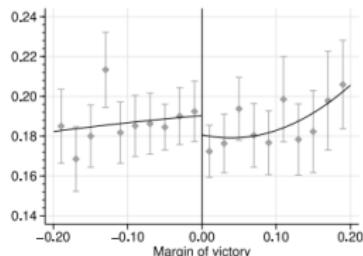
(E) Other expenditures



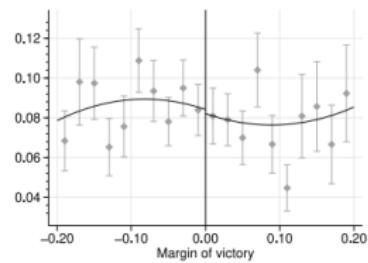
(A) Taxes



(B) Fees



(C) Alienations



(D) Loans

We are interested in estimating:

$$Y_i = \alpha + \beta g(X_i) + \delta D_i + \varepsilon_i, \quad (3)$$

where:

- $Y_i$  = the outcome variable
- $X_i$  = the running variable
- $g$  = a flexible function of  $X_i$
- $c$  = the cutoff
- $D_i$  = treatment assignment

$\delta$  is the primary quantity of interest (i.e., the “estimand”).

It represents the **Local Average Treatment Effect (LATE)** as  $X_i \rightarrow c$ .

Two types of RDDs:

- **Sharp RDD:** The treatment assignment is deterministic at the cutoff.
- **Fuzzy RDD:** The probability of treatment assignment jumps at the cutoff, but it is not a deterministic process.

In the case of the fuzzy RDD, we need to work a bit more.

Let's go ahead and define:

$$Z_i = \begin{cases} 0 & \text{if } X_i \leq c \\ 1 & \text{if } X_i \geq c. \end{cases}$$

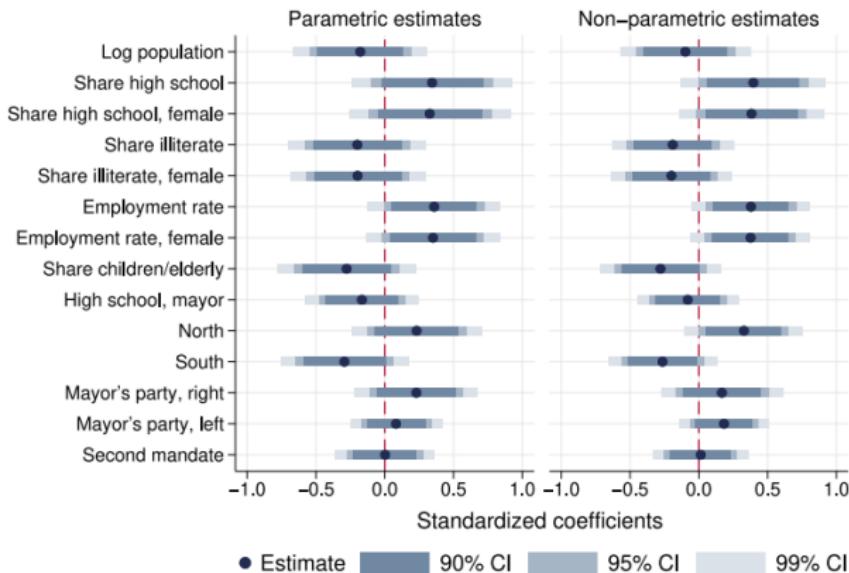
We first instrument treatment assignment  $D_i$  with  $Z_i$  and then estimate Equation 1 in the second stage.

# Limitations of RDDs

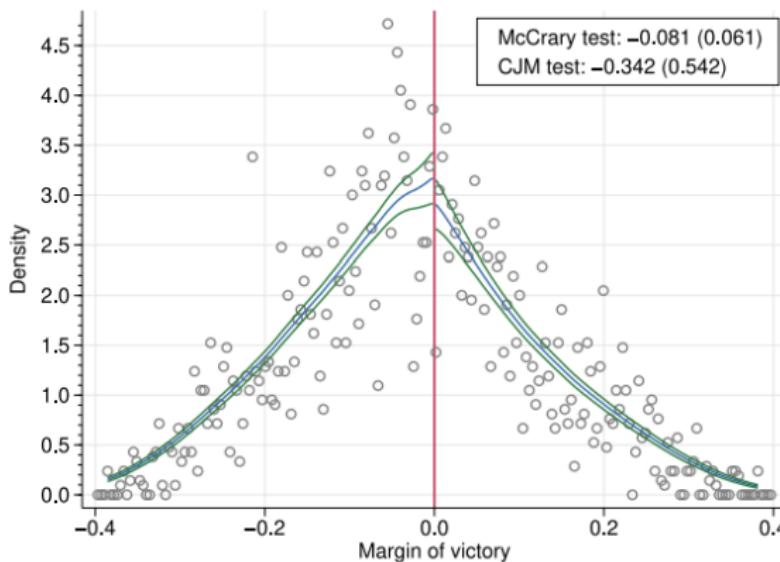
**Core assumption:** The only thing that causes the outcome to change abruptly at  $c$  is the treatment.

## Limitations/threats to identification:

- We cannot guarantee that the control and treatment group are comparable.
  - ⇒ We need to do balance tests on observables!
- There is sometimes endogenous sorting at the cutoff.
  - ⇒ McCrary density test assesses bunching at  $c$ .
- The LATE is only identified as  $X_i \rightarrow c$ , so our estimates are really based on an extrapolation exercise.
  - ⇒ The choice of  $g(X_i)$  can affect results.



**Notes:** Balance checks seem OK, comforting the plausible randomness of mixed gender close election results.



**Notes:** There is no empirical evidence of bunching at the threshold, once again reinforcing our trust in the research design.

# Difference-in-differences

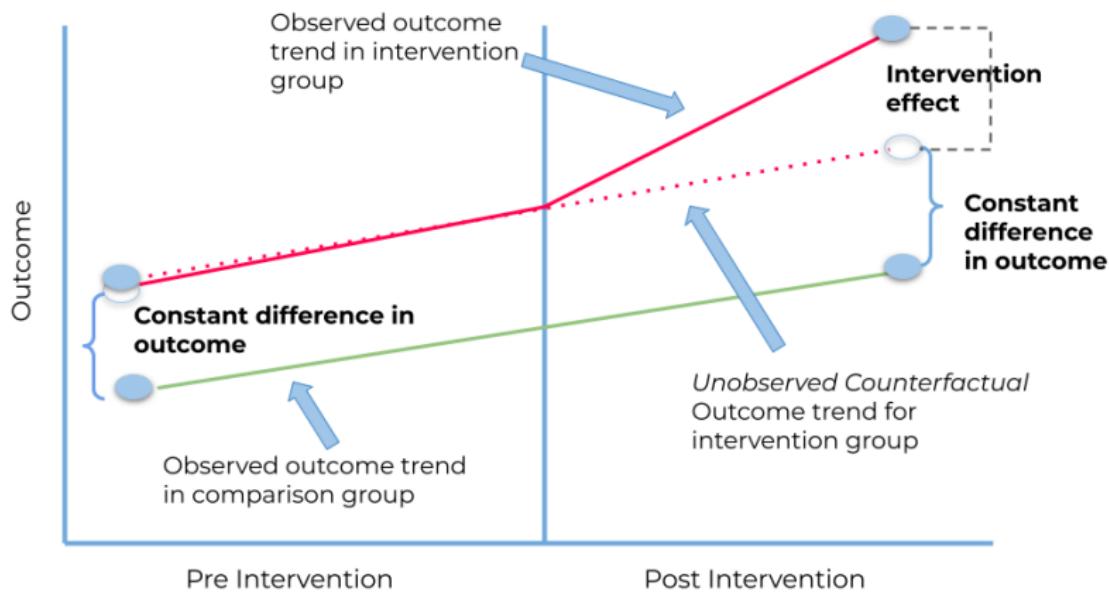
The Difference-in-Difference (DID) estimator:

$$DID = [Y^{T, \text{After}} - Y^{C, \text{After}}] - [Y^{T, \text{Before}} - Y^{C, \text{Before}}] \quad (4)$$

This measures whether the difference between treatment and control changes after the policy change.

**Parallel trends assumption:** DID identifies the causal effect of the treatment if, absent the policy change, the difference between  $T$  and  $C$  would have stayed the same.

# A Graphical Take



# Generalizing to Many Groups and Many Periods

In practice, we often observe more than two groups over many periods.

That's fine. A generalized DID estimator can be recovered by running the following regression:

$$Y_{it} = \alpha + \beta D_{it} + \delta_i + \delta_t + \varepsilon_{it},$$

where  $D_{it}$  takes value 1 if unit  $i$  is treated at period  $t$ , and 0 otherwise.

Such specifications are referred to as "**two-way fixed effects**" models.

# Can we test parallel trends? No!

We will never know if treated and untreated units would have followed the same trends absent the policy intervention.

But we can at least test whether they did display parallel trends in average outcomes *before* the policy intervention:

$$Y_{i,t} = \alpha_i + \alpha_t + \gamma_k^{-K} D_{i,t}^{<-K} + \sum_{k=-K}^{-2} \gamma_k^{\text{lead}} D_{i,t}^k + \sum_{k=0}^L \gamma_k^{\text{lag}} D_{i,t}^k + \gamma_k^{L+} D_{i,t}^{>L} + \varepsilon_{i,t}$$

,

where  $D_{i,t}^k = 1\{t - G_i = k\}$  is a variable that takes value 1 if a unit  $i$  is  $k$  periods away from initial treatment at time  $t$  and 0 otherwise.

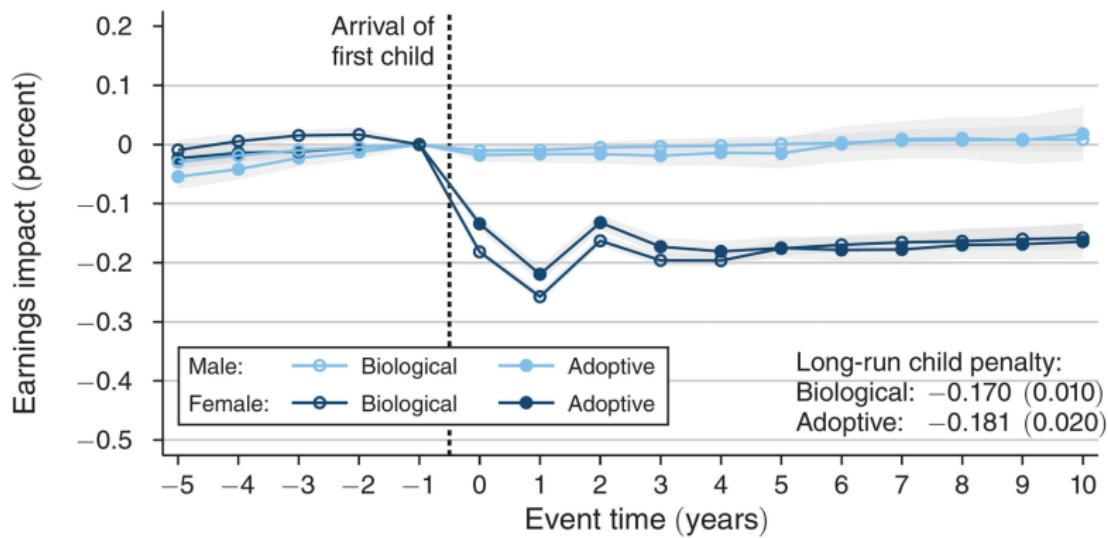
$D_{i,t}^{<-K} = 1\{t - G_i < -K\}$  and  $D_{i,t}^{>L} = 1\{t - G_i > L\}$  are defined analogously.

# Example: Child Penalty on Earnings

## Abstract

This paper investigates whether the impact of children on the labor market outcomes of women relative to men—child penalties—can be explained by the biological links between mother and child. We estimate child penalties in biological and adoptive families using event studies around the arrival of children and almost 40 years of adoption data from Denmark. Short-run child penalties are slightly larger for biological mothers than for adoptive mothers, but their long-run child penalties are virtually identical and precisely estimated. This suggests that biology is not a key driver of child-related gender gaps.

# Example: Child Penalty on Earnings



**Notes:** Estimates from Kleven et al. (2021)

# Experiments have their limitations, too.

Even well-designed identification strategies have limitations that you should pay attention to:

- Statistical power
- Selective attrition
- External validity
- Ethics

# Statistical Power

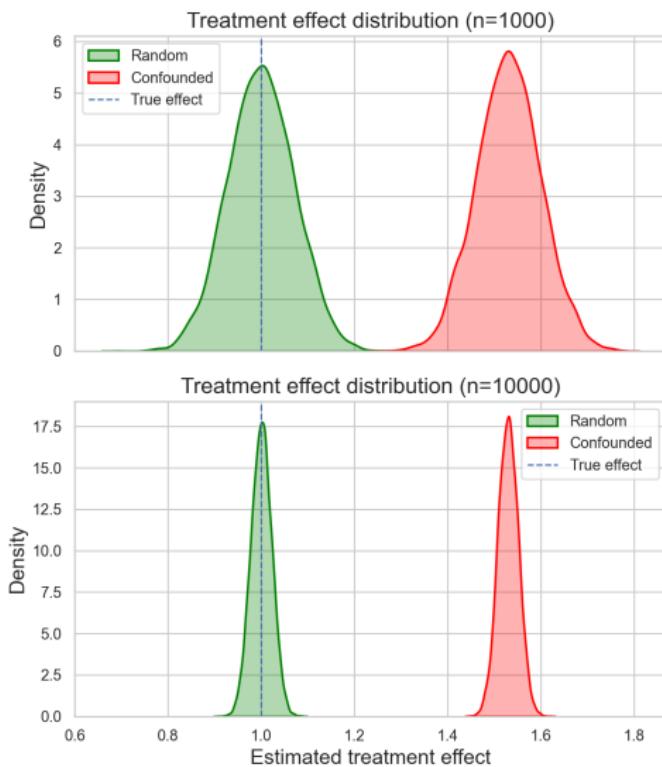
Statistical power is the probability of detecting an effect if it truly exists.

Low power increases the risk of false negatives and produces imprecise estimates.

Small sample sizes or weak interventions often lead to low power.

Researchers should plan sample sizes carefully and report minimum detectable effects.

# Simulations on Statistical Power



# Selective attrition

Participants may drop out of a study in a non-random way.

If attrition correlates with treatment, estimates become biased.

Example: Job training program where less-motivated treated individuals quit more often.

Solutions: track dropouts carefully and use bounds (Lee bounds).

# External Validity

Results from one setting may not generalize.

Treatment effects can vary across populations, locations, and time.

Often, experiments identify Local Average Treatment Effects (LATE) rather than Average Treatment Effects (ATE).

Program scale-up may change effectiveness due to general equilibrium effects (List, 2022).

Always ask: “Would this work elsewhere or at scale?”

# Example: Partial vs. General Equilibrium

Partial equilibrium: evaluates direct effect on treated individuals while holding rest of economy fixed.

General equilibrium: accounts for spillovers, price effects, and resource reallocation.

Example: Job training increases wages for participants (partial), but if many are trained, wages in the sector may fall (general).

# Ethics

Randomization may raise fairness concerns.

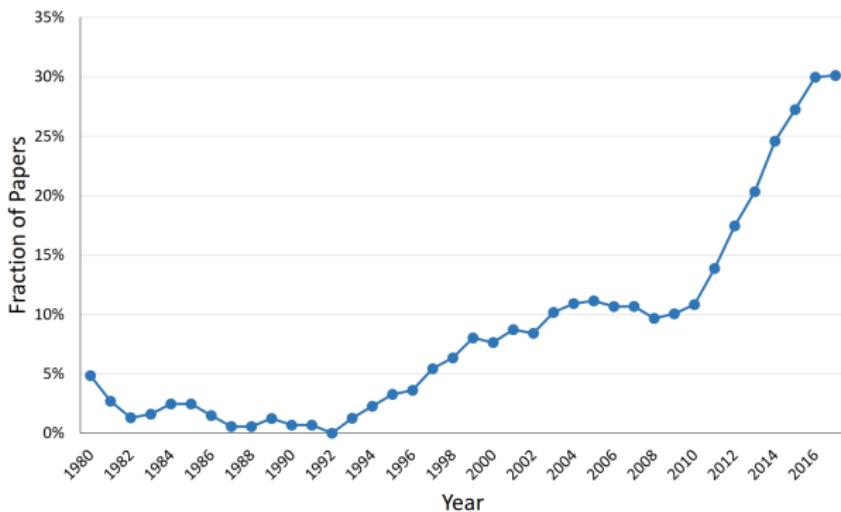
Informed consent and transparency are crucial.

Some interventions may harm participants or deny access to needed services.

Ethical review boards (IRBs) balance scientific value vs. participant rights.

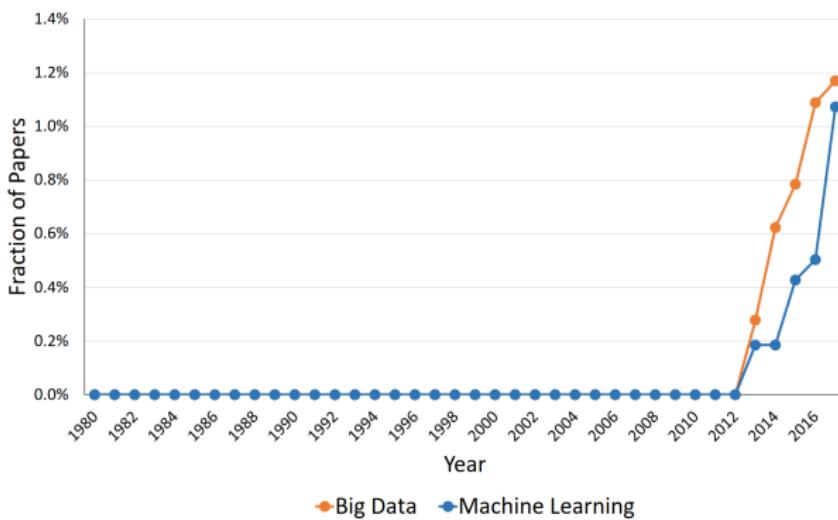
Before concluding, a glimpse of the new trends in empirical economic research...

# The Rise of Administrative Data



**Notes:** The graph shows the fraction of papers that refer to administrative data. See the original slides [here](#).

# The Rise of Machine Learning



**Notes:** The graph shows the fraction of papers that refer to machine learning. See the original slides [here](#).

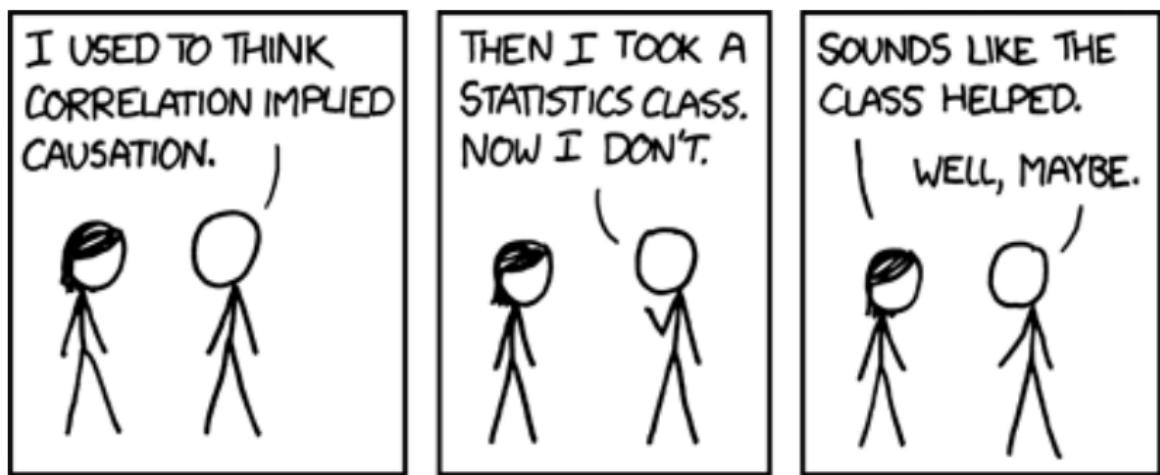
# Conclusion

The central issue for any policy question is establishing a causal relationship between the policy in question and the outcome of interest.

We discussed several approaches to distinguish causality from correlation. The gold standard for doing so is the randomized trial, which removes bias through randomly assigning treatment and control groups.

Unfortunately, however, such trials are not available for every question we wish to address in empirical public finance. As a result, we turn to alternative methods, "quasi-experiments".

Each of these alternatives has weaknesses, but careful consideration of the problem at hand can often lead to a sensible solution to the bias problem that plagues empirical analysis.



# Exercise

**You are hired by the government to evaluate the impact of a policy change that affects one group of individuals but not another. Suppose that before the policy change, members of a group affected by the policy averaged \$17,000 in earnings, and members of a group unaffected by the policy averaged \$16,400. After the policy change, members of the affected group averaged \$18,200 in earnings while members of the unaffected group averaged \$17,700 in earnings.**

**a. How can you estimate the impact of the policy change? What is the name for this type of estimation?**

**b. What are the assumptions you have to make for this to be a valid estimate of the impact of the policy change?**

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