

Causal Inference

Session 9: Natural Experiments

Brad Hackinen

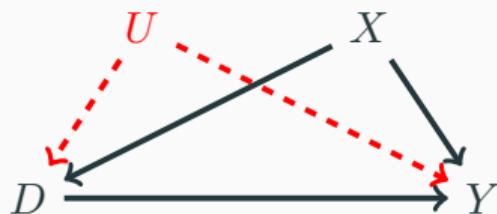
Fall 2025

Agenda:

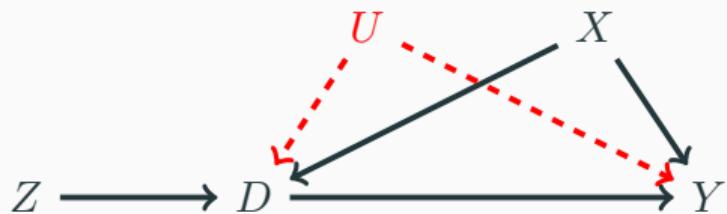
- Natural Experiments
 - Instrumental Variables
 - Regression Discontinuity Designs

Natural Experiments

So far we have focused on how to control for confounders by *conditioning on observable variables*. This is a powerful approach, but in most circumstances it is hard to be confident that there are no unobserved confounders.



We use the term “**Natural Experiment**” to refer to a situation where events that are out of the researchers control nevertheless lead to conditions that resemble randomized treatment for some specific population.



An example of a natural experiment:



VOG: Using Volcanic Eruptions to Estimate the Impact of Air Pollution on Student Learning Outcomes

by

Timothy J. Halliday, Lester Lusher,
Rachel Inafuku, and Aureo de Paula

Abstract

This study pairs variation stemming from volcanic eruptions from Kilauea with the census of Hawai‘i’s public schools student test scores to estimate the impact of particulates and sulfur dioxide on student performance. We leverage spatial correlations in pollution in conjunction with proximity to Kilauea and wind direction to construct predictions of pollution exposure at each school. We precisely estimate that increased particulate pollution leads to a small but statistically significant drop in average test scores. Then, utilizing Hawai‘i’s rich diversity across schools in baseline exposure, we estimate sharp nonlinearities - schools with higher baseline levels of pollution experience larger decreases in test scores than schools with less pollution exposure on average. At levels of particulate pollution higher than six micrograms per cubic meter ($\mu\text{g}/\text{m}^3$), we estimate that a one standard deviation increase in $PM_{2.5}$ leads to a decline in test scores of 1.1 percent of a standard deviation. Lastly, we find that *within schools* the drop in test scores is concentrated among economically disadvantaged students. The effects of $PM_{2.5}$ on student test scores are larger

Another example of a “natural” experiment:

nature human behaviour



Article

<https://doi.org/10.1038/s41562-024-01948-y>

Causal effect of video gaming on mental well-being in Japan 2020–2022

Received: 22 June 2023

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Check for updates

The widespread use of video games has raised concerns about their potential negative impact on mental well-being. Nevertheless, the empirical evidence supporting this notion is largely based on correlational studies, warranting further investigation into the causal relationship. Here we identify the causal effect of video gaming on mental well-being in Japan (2020–2022) using game console lotteries as a natural experiment.

Employing approaches designed for causal inference on survey data ($n = 97,602$), we found that game console ownership, along with increased game play, improved mental well-being. The console ownership reduced psychological distress and improved life satisfaction by 0.1–0.6 standard deviations. Furthermore, a causal forest machine learning algorithm revealed divergent impacts between different types of console, with one showing smaller benefits for adolescents and females while the other showed larger benefits for adolescents. These findings highlight the complex impact of digital media on mental well-being and the importance of considering differential screen time effects.

From the Abstract:

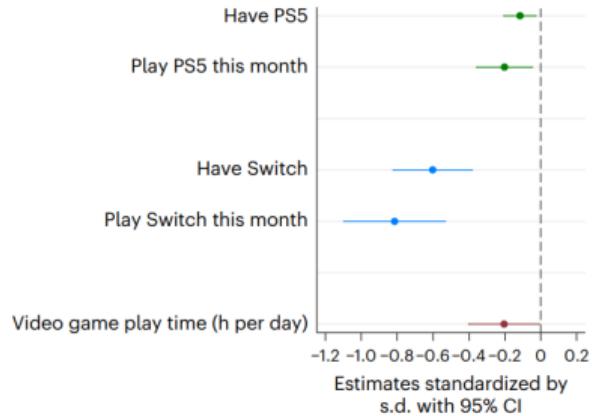
"We identify the causal effect of video gaming on mental well-being in Japan (2020-2022) using game console lotteries as a natural experiment... we found that game console ownership, along with increased game play, improved mental well-being. The console ownership reduced psychological distress and improved life satisfaction by 0.1–0.6 standard deviations."

The “Experiment”

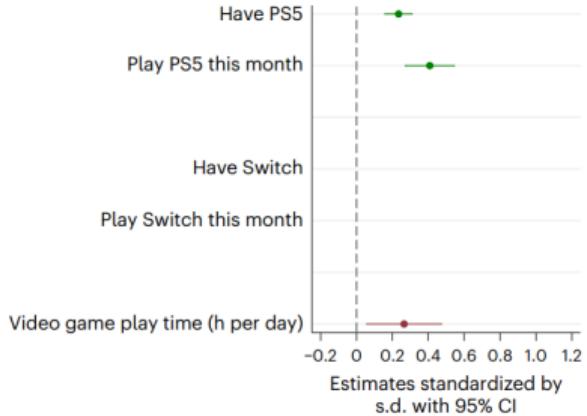
“Supply chain disruptions and surged demands during [Covid] limited the availability of two major gaming consoles: Nintendo Switch (Switch) and PlayStation 5 (PS5). To address these shortages, Japanese retailers used lotteries to assign these gaming consoles to consumers, inadvertently creating a plausibly random distribution of opportunities to play video games. Winning a lottery became the primary determinant of whether one could purchase these consoles.”

The Results

a Psychological distress (K6)



b Life satisfaction (SWLS)



Yet another example:

Digital Distractions with Peer Influence: The Impact of Mobile App Usage on Academic and Labor Market Outcomes*

Panle Jia Barwick

Siyu Chen

Chao Fu

Teng Li

From the Abstract:

"Our analysis leverages administrative data from a Chinese university of three cohorts of students over up to four years merged with mobile phone records, random roommate assignments, and a policy shock that affects peers' peers. App usage is contagious: a one s.d. increase in roommates' in-college app usage raises own app usage by 5.8% on average, with substantial heterogeneity across students. High app usage is detrimental to all outcomes we measure."

"A one s.d. increase in app usage reduces GPAs by 36.2% of a within-cohort-major s.d. and lowers wages by 2.3%. Roommates' app usage exerts both direct effects (e.g., noise and disruptions) and indirect effects (via behavioral spillovers) on GPAs and wages, resulting in a total negative impact of over half the size of the own usage effect. Extending China's minors' game restriction of three hours per week to college students would boost their initial wages by 0.9%."

The “Experiment”

“We use two sets of instruments. The first exploits the 2019 China’s minors’ game restriction . . . which directly impacted 8% of our sample students but indirectly affected all of them through their underage friends. . . . The second set of instruments exploits the launch of the blockbuster game “Yuanshen” midway through our sample period. We interact Yuanshen’s release date with students’ pre-college app usage to construct a shift-share type of instruments, while controlling for student fixed effects and other time-varying confounding factors whenever possible.”

In the extreme case, a natural experiment may randomize treatment so thoroughly and convincingly that treatment can be assumed to be independent of the outcome and the difference-in-means will be sufficient for a convincing causal estimate.

More commonly, the randomization will be imperfect in some way: Maybe it only applies to a subset of the population, or doesn't completely determine treatment. In these cases we need to use statistical estimators to isolate the random variation in treatment.

Instrumental Variables Estimators

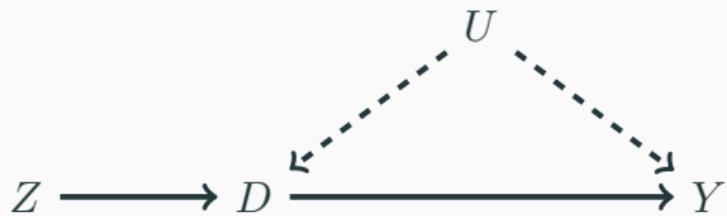
Instrumental Variables (IVs) are variables that provide information about random variation in treatment, allowing one to estimate causal effects even in the presence of unobserved confounders.

A variable must satisfy two conditions to work as an IV:

1. The variable is correlated with treatment (*relevance*)
2. The variable is independent of the outcome, conditional on treatment (*exclusion restriction*)

A simple IV

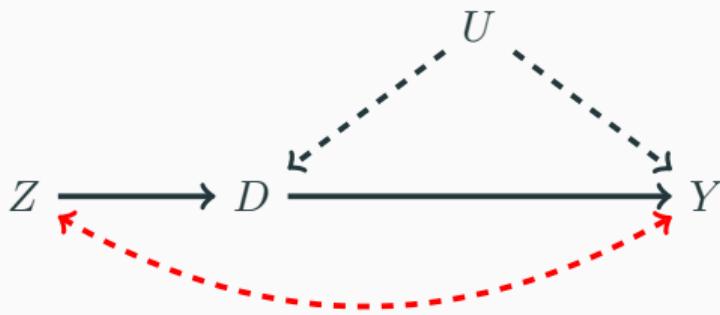
The simplest case of an IV is when we have a variable Z that affects treatment (*relevance ✓*) but has no effect on the outcome except *through* treatment (*exclusion restriction ✓*).



Then Z creates differences in treatment that are much like a randomized experiment.

Exclusion Restriction

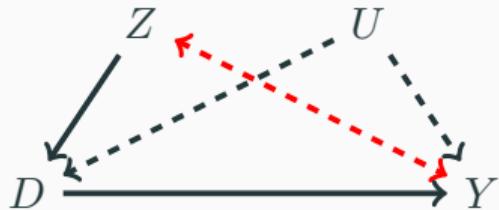
If the variable we want to use as an IV is linked to the outcome through some alternative path that does not go through treatment, then we will not be able to separate the causal effect from other correlations between treatment and outcome.



This could be the result of either an effect of Z on Y , or a confounding variable that affects both Z and Y .

The exclusion restriction is not directly testable!

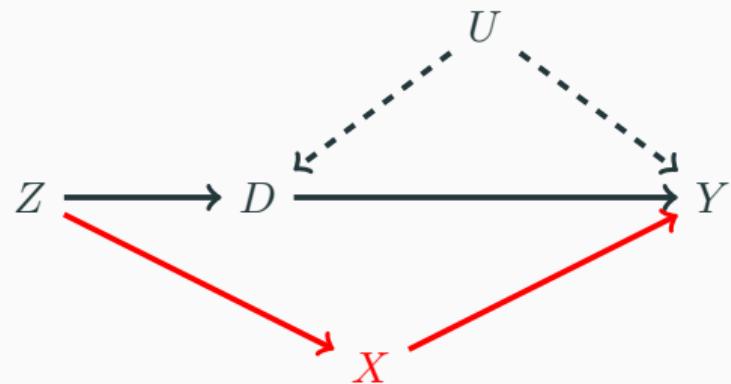
It is not possible to fully distinguish between a violation of the exclusion restriction and the presence of an unobserved confounder.



The only way to be confident that an IV satisfies the exclusion restriction is *domain knowledge*.

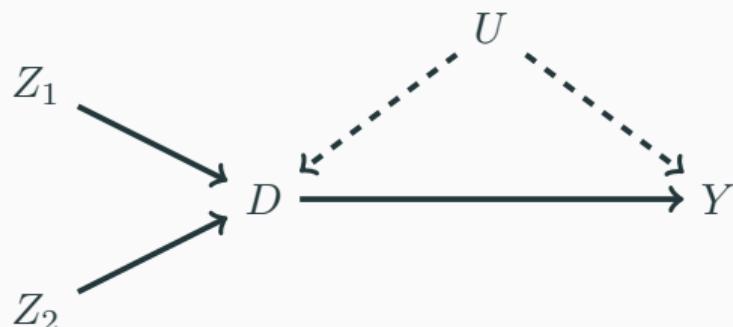
Conditional IVs

It is also possible to use IV methods when there is a violation of the exclusion restriction that can be blocked by conditioning on an observed variable:



Multiple IVs

It is also possible to use multiple instruments simultaneously (if you happen to be so lucky as to find them).



However, more instruments are not always better. Each instrument may provide additional information about treatment, but it also introduces additional noise into the first stage.

This is particularly a problem for *weak instruments*.



Contents lists available at [ScienceDirect](#)

Economics of Education Review

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The impact of online learning on students' course outcomes: Evidence from a large community and technical college system

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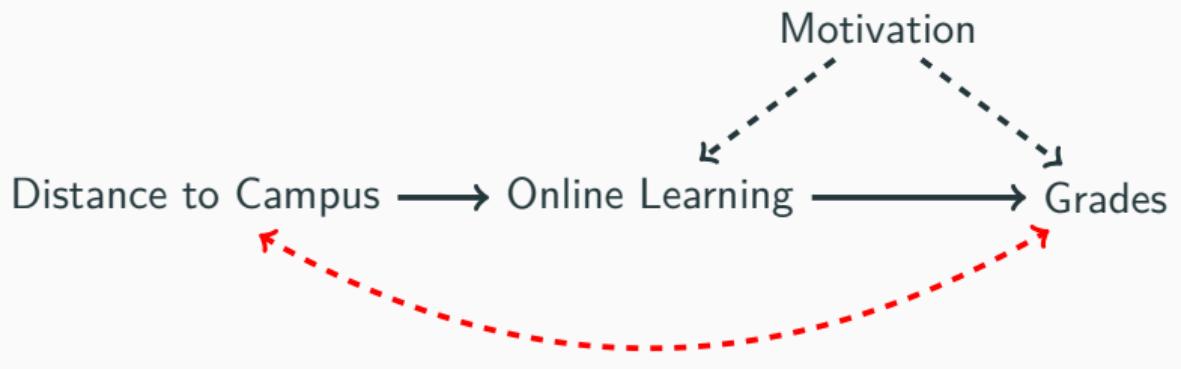
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Q:

- What is the causal effect being estimated? (treatment, outcome, potential outcomes)
- What is the potential confounding problem?
- What is the instrument employed by the researchers? What effect is it assumed to have on treatment?

"In this analysis, we used the distance from each student's home to their college campus as an instrument for the student's likelihood of enrolling in an online rather than face-to-face section. Specifically, we first identified the associated geocode for each address in the dataset, including both student home address and college address; we then used Google Maps to calculate the "travel distance" between each student's home and their college of attendance. Given that online courses offer the flexibility of off-site education, students who live farther from their own college campus might be more likely to take advantage of online courses, compared with students who live closer to their college."

Q: What does the exclusion restriction require in this setting? Do you find it believable?



IV Estimation

There are two common approaches to estimating causal effects with IVs.

1. Two Stage Least Squares (2SLS)
2. Control Functions

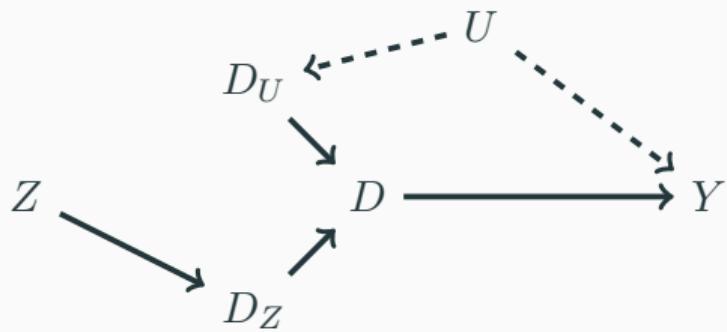
Both approaches essentially attempt to decompose D into two parts:

- D_Z : Changes in treatment driven exclusively by the instrument(s)
- D_U : The remaining variation in treatment, including changes in treatment caused by unobserved confounders.

Where $D = D_Z + D_U$

This decomposition is called the *first stage*.

Graphically, we can visualize this decomposition with the following modified DAG:

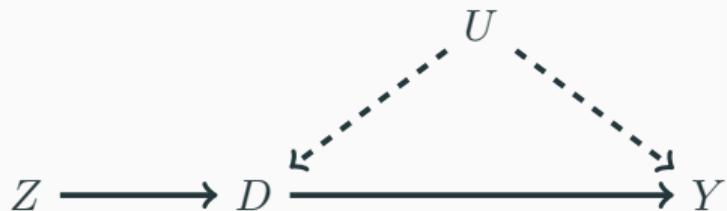


Q: What do we need to control for if we want to estimate the effect of D on Y ? What about the effect of D_Z on Y ?

The modified DAG suggests two strategies for estimating the causal effect with the new variables (the *second stage*):

1. Regress Y on $D_Z \rightarrow 2SLS$
2. Regress Y on D , controlling for $D_U \rightarrow Control\ Function\ approach$

A minimal example (one IV, no controls, linear effects)



Suppose the data is generated by the following structural model:

$$Y_i = \delta D_i + U_i$$

$$D_i = \pi Z_i + U_i$$

Two Stage Least Squares Approach

First stage:

- Regress $D \sim \pi Z + \varepsilon$
- Get predicted treatment $D_Z = \hat{\pi}Z$

Second Stage:

- Regress $Y \sim \delta D_Z + \varepsilon$

Control Function Approach

First stage:

- Regress $D \sim \pi Z + \varepsilon$
- Get residual $D_U = D - \hat{\pi}Z$

Second Stage:

- Regress $Y \sim \delta D + \gamma D_U + \varepsilon$

R Demo: NaturalExperiments_blank.Rmd

Goals:

- Use an IV to estimate a causal effect
- Illustrate IV conditions (Relevance and exclusion restriction)

Tradeoffs:

- 2SLS is most common approach for linear models. Many full-featured estimation packages exist.
- Control Functions are more flexible (e.g. can be used with non-linear first stages), but might need to be hand-coded.

Q: How did Xu & Jaggers implement their IV estimator to estimate the impact of online learning?

modified Eq. (1) to use an IV approach. Specifically, we first predicted the probability that an individual i took a particular course c online using a probit model:

$$\begin{aligned}\text{Prob}(\text{online}_{ict}) = \Phi(\alpha + \delta_1 \text{distance}_i + \delta_2 \text{distance}_i^2 \\ + \gamma X_i + \pi_t + Z_c + \mu_{ict})\end{aligned}\quad (2)$$

where Φ represents the cumulative density function for the standard normal distribution and Z_c represents college-by-course fixed effects. Consistent estimates of the relative impact of online course delivery can be then derived by using the estimated probabilities from Eq. (2) as instruments for the endogenous dummy variable online_{ict} in a 2SLS estimation process.¹⁰

A third recipe for dealing with non-linear first stage:

1. Estimate probability of treatment using a non-linear regression
2. Predict probability of treatment using fitted regression
3. Use predicted probabilities as instrument in 2SLS

This is another way to deal with a non-linear first stage.

(Note: it is *not* valid to use the coefficients from the non-linear regression directly as a first stage for 2SLS)

Table 4

Estimates of the effect of taking a course online, based on different model specifications.

	OLS/probit estimates			IV estimates		
	(1)	(2)	(3)	(4)	(5)	(6)
<i>Dependent variable: course persistence</i>						
Online format (SE)	-0.257*** (0.018)	-0.298*** (0.017)	-0.311*** (0.017)	-0.420*** (0.118)	-0.457*** (0.126)	-0.509*** (0.132)
Marginal effect (SE)	-0.036*** (0.003)	-0.041*** (0.003)	-0.044*** (0.003)	-0.054*** (0.016)	-0.058*** (0.017)	-0.065*** (0.018)
<i>Dependent variable: course grade</i>						
Online format (SE)	-0.196*** (0.018)	-0.233*** (0.017)	-0.266*** (0.016)	-0.228*** (0.089)	-0.270*** (0.098)	-0.324*** (0.110)
College and subject FE	No	Yes	Yes	No	Yes	Yes
Year-term FE	No	Yes	Yes	No	Yes	Yes
College-by-course FE	No	No	Yes	No	No	Yes

Notes: $N = 125, 218$ for the analysis on course persistence; $N = 116, 830$ for the analysis on course grade. Standard errors for all models are adjusted for clustering at the course level. Each cell represents a different regression specification. All models also include the following covariates: gender, ethnicity dummy variables, socioeconomic status dummy variables, receipt of federal financial aid, limited English proficiency, dual enrolled prior to college, ever enrolled in remedial courses, total credits taken in that term, total working hours in that term, and full-time (vs. part-time) college enrollment in that term.

*** Significant at the 1% level.

IV Challenges

IV estimators can be difficult to use in practice. There are at least three main challenges:

1. Finding a valid instrument
2. Interpreting the estimated coefficient
3. Estimating standard errors and confidence intervals

Challenge 1: Finding an Instrument

Finding an IV requires convincing yourself and others that the exclusion restriction is not violated. The only way to do this is to try to imagine plausible violations. If you can't think of any, then the IV *may* be valid.

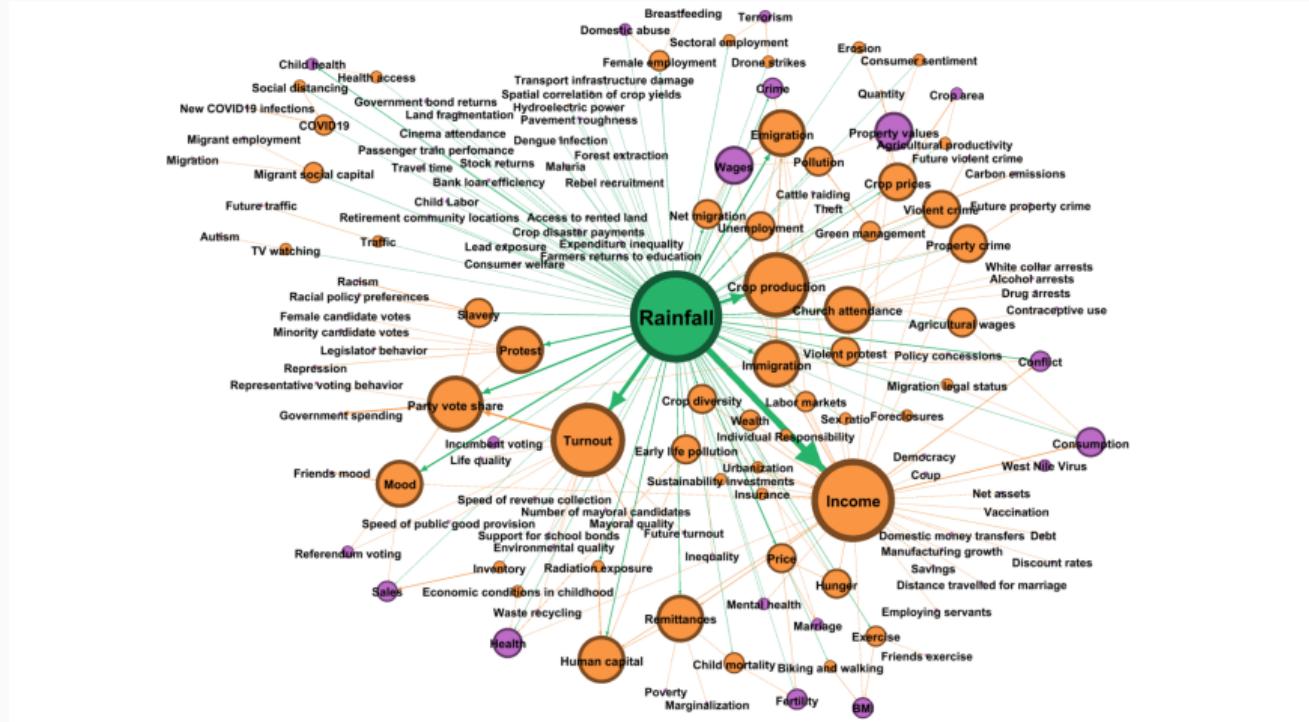
This is a lot like convincing yourself that there are no unobserved confounders.

But how can we ever be completely confident that something unobserved does not exist?

The story of IV Designs in economics

Twenty years ago, IV designs were very popular. Economists were using IVs to measure the causes of long-run growth, wages, labour-force participation, returns to schooling, conflict, political outcomes, and much more.

For example, are at least 192 studies that use rainfall as an instrument for some social or economic outcome (Mellon, 2022).



Q: Is it possible to use the same IV for multiple studies with different treatment and outcome variables?

Many older studies failed to make a convincing case that their IVs satisfy the exclusion restriction, or they suffered from weak instruments. There has been a bit of a backlash, and IV methods are currently not very popular among economists (we are as prone to fads as anyone else).

Researchers are now much more skeptical when evaluating an IV research design.

Are IV Designs ever useful?

Yes. IVs can still be very convincing if it is clear that the instrument is *randomly assigned*. This could occur either because of:

1. Deliberate randomization
2. A particularly clear natural experiment

Intent to treat designs

Sometimes we can randomize treatment assignment, but cannot enforce compliance. In these settings, units are randomly exposed to conditions that allow or encourage treatment, but not everyone who is eligible receives treatment.

For example:

- Draft lotteries
- Clinical trials where people need to take daily doses at home

IV methods (often 2SLS) can be used to estimate the effect of treatment on the eligible population.

Challenge 2: Interpreting the IV coefficient

When IVs have different effects on different individuals, then we can actually only measure a *Local Average Treatment Effect (LATE)*.

The LATE for an IV is the *average treatment effect among units that change their treatment status in response to treatment*. This group may have different characteristics than the rest of the population.

We also want to watch out for IVs that have *opposing* effect on the treatment of different units - this can bias our causal estimate.

Challenge 3: Estimating Standard Errors

Many software packages compute standard errors that are asymptotically correct for 2SLS.

However, there is an additional challenge when the instrument is *weak*.

Weak Instruments

A *weak instrument* is a variable that satisfies the exclusion restriction, but is only weakly correlated with treatment.



Weak instruments introduce a lot of noise relative to the additional information about treatment, and can decrease the precision of causal estimates.

There are special estimation techniques intended to make the best of this situation, but it is always challenge for inference.

Reduced Form

If we estimate the total effect of Z on Y , we call this the “*reduced form*” (in our simple example, we would regress $Y \sim \beta Z + \varepsilon$).

It is possible to show that:

$$\hat{\delta}_{IV} = \frac{\hat{\beta}}{\hat{\pi}}$$

Implications:

1. Estimates can “explode” in scale if $\hat{\pi}$ is close to zero
2. $\hat{\beta} \approx 0$ indicates at least one of $\hat{\delta}_{IV} \approx 0$ or $\hat{\pi} \approx 0$.

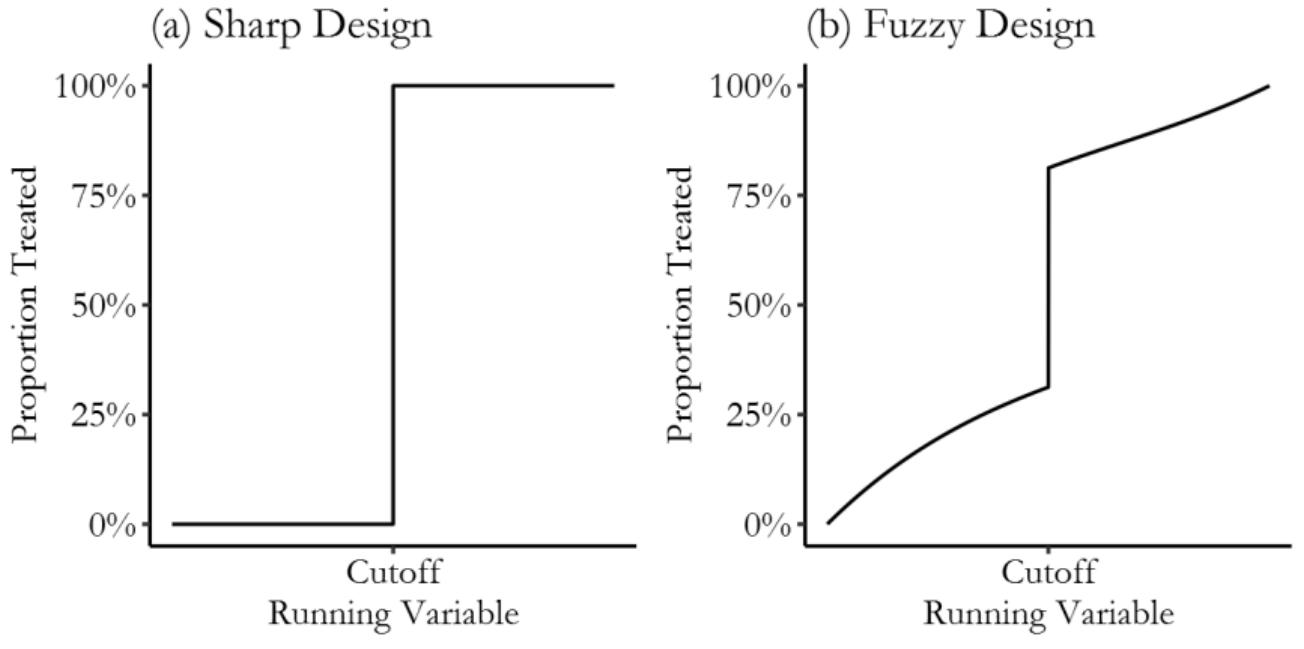
Summary:

- Natural experiments can provide random variation in treatment that can be used to estimate causal effects.
- Instrumental Variables (IVs) are variables that can isolate random variation in treatment.
- Two common IV estimation approaches are Two Stage Least Squares (2SLS) and Control Functions.
- IV designs can be challenging to use in practice: We need to find valid instruments, interpret Local Average Treatment Effects, and deal with estimating standard errors.

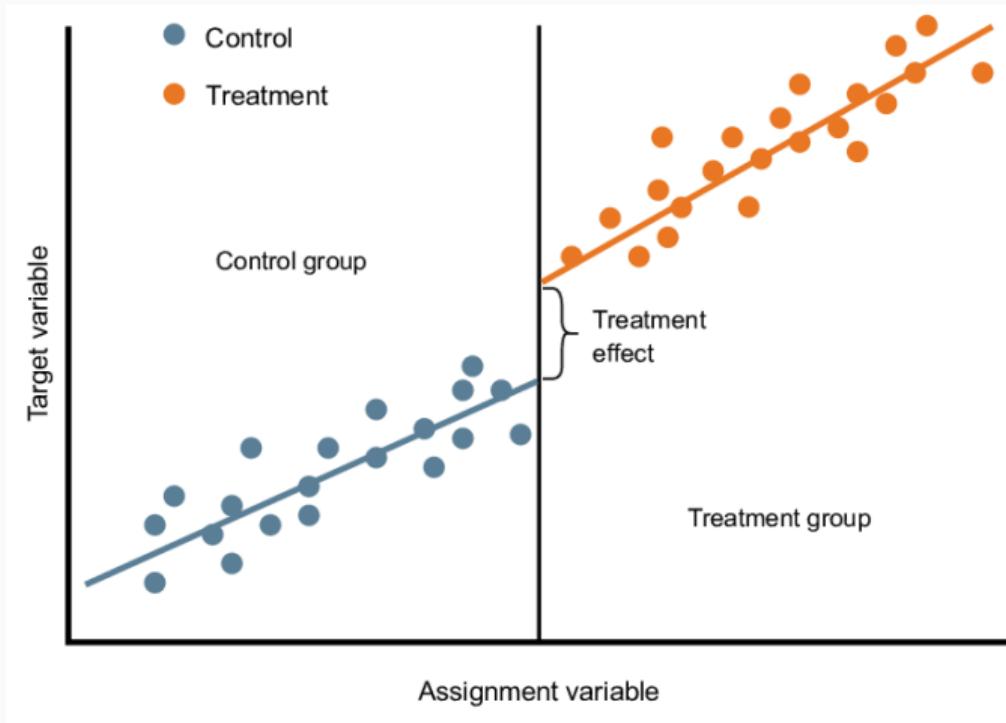
Regression Discontinuity Designs

Regression Discontinuity Designs (RDD)

RDD estimators are a special case of IV where we have a continuous “running variable” with a sharp cutoff where the probability of treatment changes discontinuously.



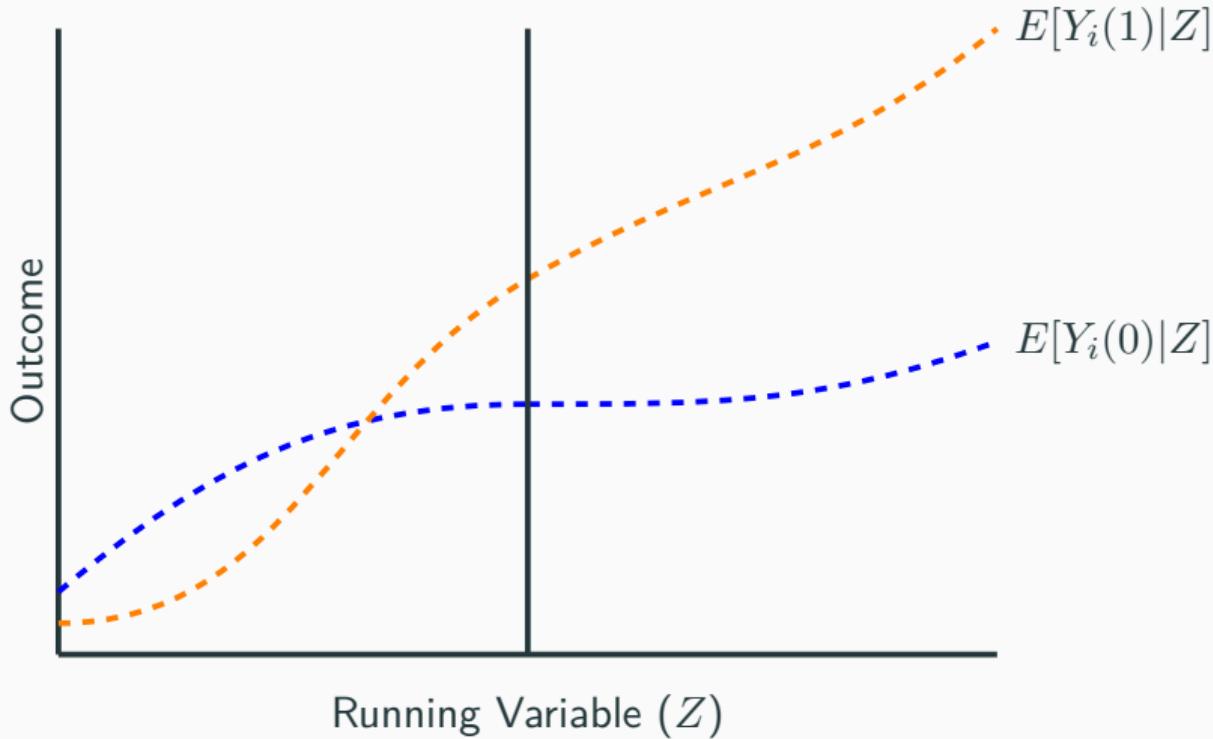
An illustration of a sharp RDD:



RDD Intuition

Even if the running variable is has a direct effect on the outcome, if the direct effect is “smooth”, then the change in outcome *right* at the cutoff will still be driven entirely by the change in treatment.

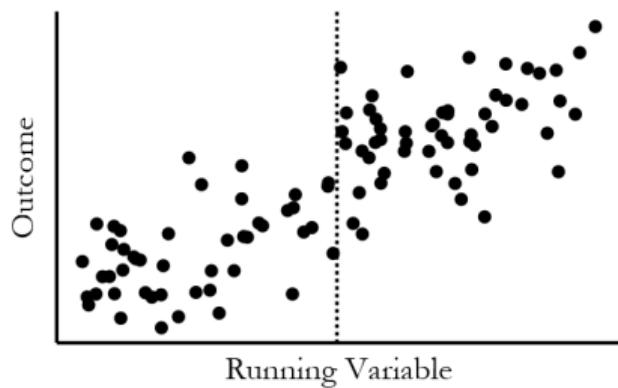
Formally, we require that the conditional expectation of both potential outcomes are continuous (smooth) at the cutoff.



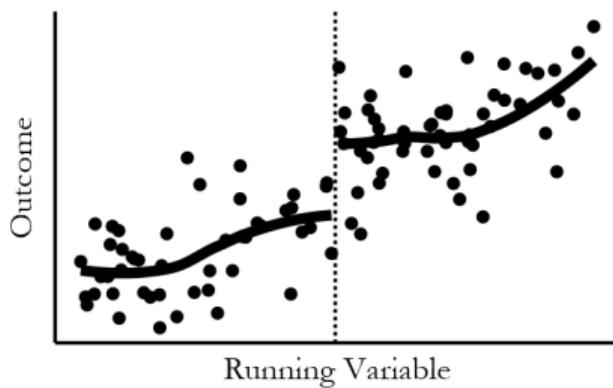
We can estimate the *Local Average Treatment Effect (LATE)* at the cutoff by discarding all the data outside of a narrow “bandwidth”.

From *The Effect*, Chapter 20:

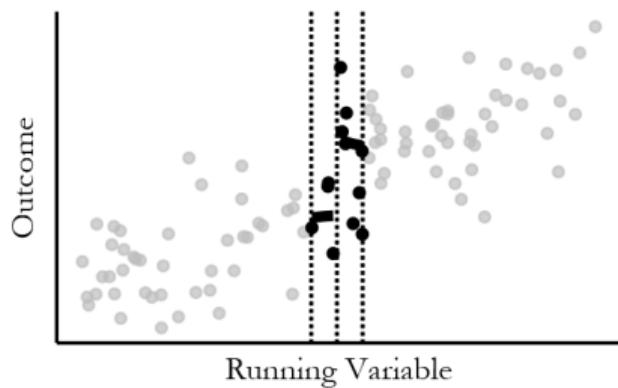
(a) Raw Data



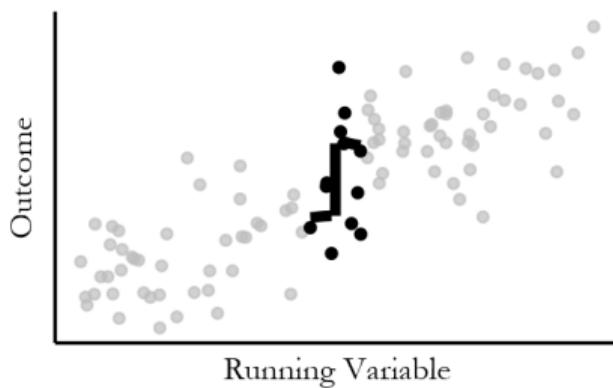
(b) Predict Values Near the Cutoff



(c) Pick a Bandwidth



(d) Estimate Jump at the Cutoff



Does Corporate Social Responsibility Lead to Superior Financial Performance? A Regression Discontinuity Approach

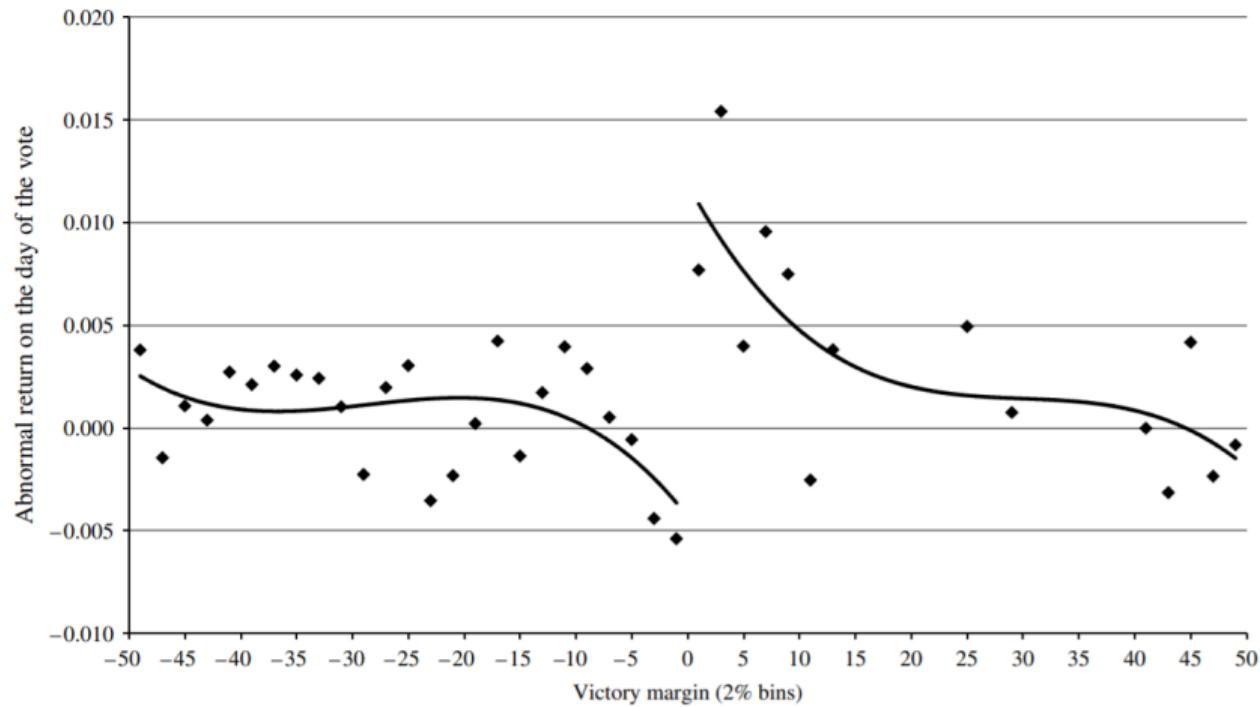
Caroline Flammer

Ivey Business School, University of Western Ontario, London, Ontario N6G 0N1, Canada, cflammer@ivey.uwo.ca

Q:

- What is the research question and setting?
- What causal effect is the author estimating? Treatment? Outcome? Potential Outcomes?
- How does the author employ RDD? What is the running variable and source of the discontinuity in treatment?

Abnormal Returns on the Day of the Vote



Q: What is this plot showing? Can you see the estimated effect?

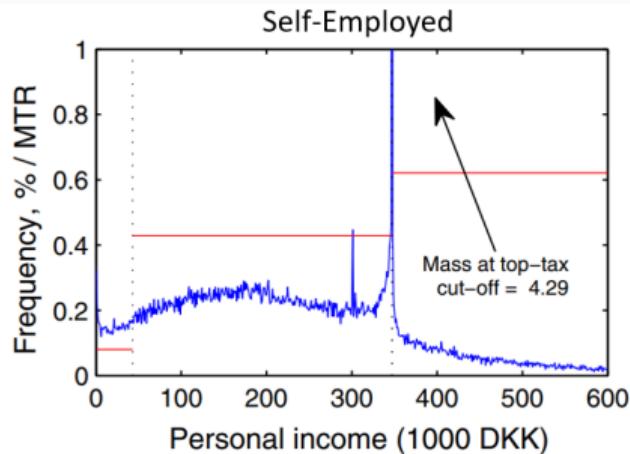
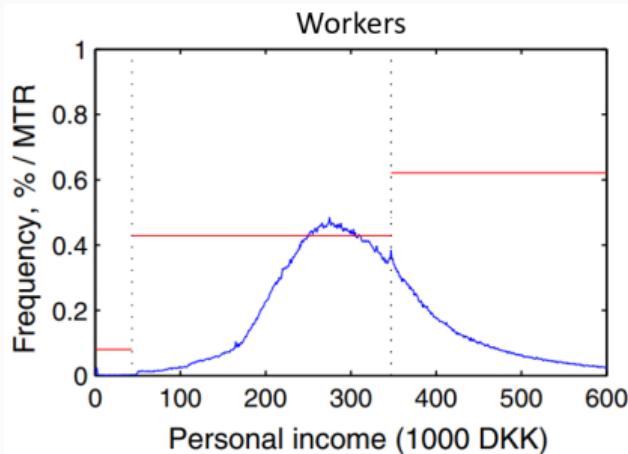
The Exclusion Restriction in RDD

Recall that the exclusion restriction requires that the IV is independent of the outcome conditional on treatment.

In RDD, we only need to worry about violations near the discontinuity (within the bandwidth). However, there are still some ways that the exclusion restriction can be violated:

- If units sort themselves to one side of the cutoff based on their expected outcome
- If there is a confounding relationship that also has a discontinuity at the same point
- If the confounding relationship is continuous, but so non-linear that it is not constant within the bandwidth

Here is some data on marginal tax rates and the distribution of reported income in Denmark in 2009 (Maire & Schjerning, 2013):



Q: Can you see any issues with using an RDD to estimate the effect of marginal tax rates on some other outcome (e.g. the choice to become self-employed)?

Bunching Estimators

When we observe units sorting themselves to one side of the cutoff, we can sometimes use this to estimate how much effort they are willing to exert to avoid the treatment. This can help estimate the causal effect of the treatment on the income or wellbeing of the units themselves.

Back to the Exclusion Restriction in RDD:

Recall that the exclusion restriction requires that the IV is independent of the outcome conditional on treatment.

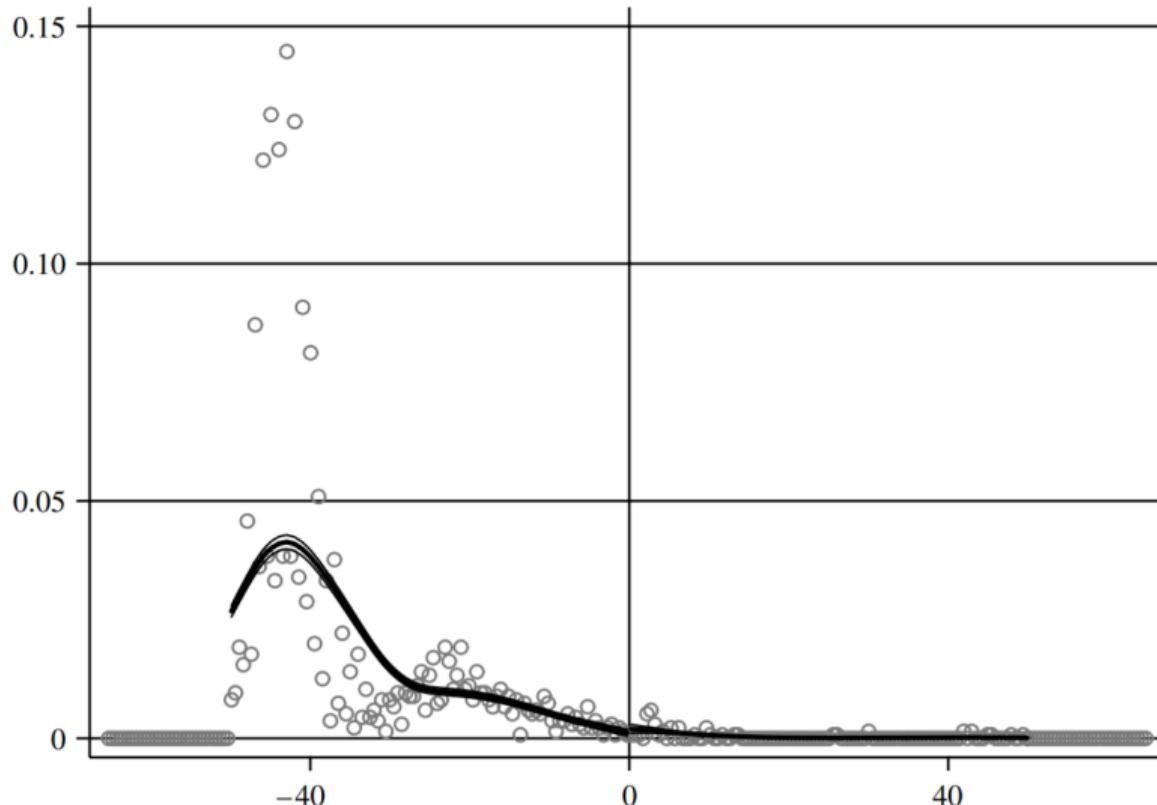
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- If units sort themselves to one side of the cutoff based on their expected outcome
- If there is a confounding relationship that also has a discontinuity at the same point
- If the confounding relationship is continuous, but so non-linear that it is not constant within the bandwidth

Q: Does it seem possible that any of these scenarios might apply in Flammer's study?

McCrory density test from Flammer (2015):

Figure 2 Visualization of the McCrary (2008) Test



Part of what makes RDD convincing is that we can use the data check for many potential problems:

- Visually, with an *RD Plot*
- Statistically, with *Specification Tests*

RDD Plots

People expect to see a good RDD plot, and will often evaluate the quality of the estimate based more on this plot than any formal statistical test.

An RDD plot has:

- The running variable as the x-axis and the outcome as the y-axis
- The discontinuity clearly labelled
- Some clear presentation of the raw data, usually local means for binned observations.
- Estimated mean outcomes on either side of the discontinuity

Specification Tests: (adapted from Imbens & Lemieux, 2007)

- Check for discontinuities in covariates
 - Conditional expectation of pre-treatment covariates should be continuous
- Check for discontinuity in observation density at threshold (McCrory test)
 - Not explicitly required for identification, but could indicate sorting
- Check for apparent discontinuities in outcome at other locations along the running variable (“placebo tests”)
 - Tests estimator performance on your real data
- Run estimator with multiple bandwidths and different covariates
 - Estimates should not be too sensitive to these changes

R Demo: NaturalExperiments_blank.Rmd

Goals:

- Use an IV to estimate a causal effect
- Illustrate IV conditions (Relevance and exclusion restriction)
- Show how RDD estimators can work even when standard IV fails