Causal Data Science for Business Decision Making

Surrogate Experiments

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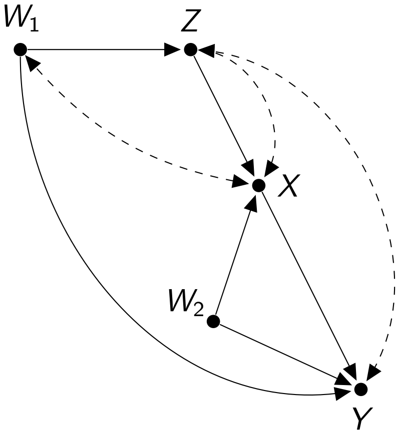
Surrogate Experiments

- Let's have a look at a causal graph like the following



Surrogate Experiments (II)

- There is no way to identify $P(y|do(x))$ via backdoor adjustment
- The set $\{W_1, W_2, Z\}$ is not backdoor-admissible because Z is a collider on the path $X \dashleftarrow\dashrightarrow Z \dashleftarrow\dashrightarrow Y$
- If we could run an experiment in which we manipulated X, we could delete all the incoming arrows into X and immediately read of $P(y|do(x))$ from the post-intervention distribution
- But what if that's not possible? Could we use experimental variation in another variable instead to get at the causal effect of interest?
- It turns out we can: In the above graph, if we are able to manipulate Z, we can transform $P(y|do(x))$ into an expression that only contains $do(z)$ (Bareinboim and Pearl, 2012)
    - Solution: $P(y|do(x)) = \sum_{w_1,w_2} P(y|do(z), x, w_1, w_2)P(w_1)P(w_2)$

Identification by Surrogate Experiment

$\mathcal{Z}$-identification (graphical criterion; Bareinboim and Pearl, 2012)

Let X, Y, Z be disjoint sets of variables and let G be the causal graph. The causal effect $Q = P(y|do(x))$ is $z\mathcal{ID}$ in G if one of the following conditions hold:

(i) Q is identifiable in G; or

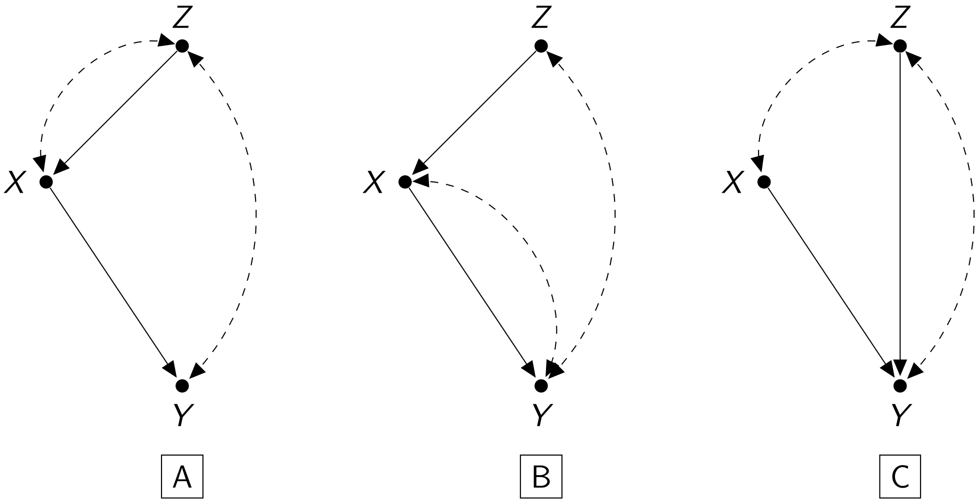
(ii) There exists $Z' \subseteq Z$ such that the following conditions hold,

  a. X intercepts all directed paths from Z' to Y, and

  b. Q is identifiable in $G_{\overline{Z'}}$.

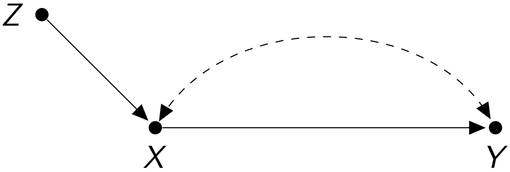
- Since the entire post-interventional distribution is identified, also other quantities such as the average causal effect are
- This graphical criterion is only a sufficient condition but not necessary for identification (i.e., there exist solutions that do not fulfill these criteria)

Test: In which causal graphs is $P(y|do(x))$ z-identifiable?



Instrumental Variables

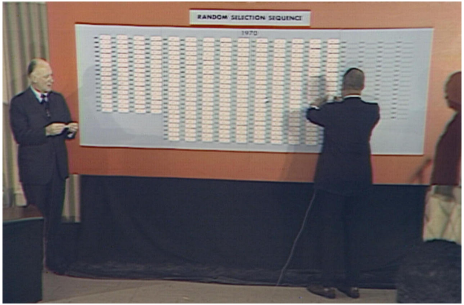
- $\mathcal{Z}$-identification does not allow for unobserved confounders that directly affect treatment and outcome
- With such a direct unobserved confounder, there is no way to identify $P(y|do(x))$ nor the average causal effect (Manski, 1990; Balke and Pearl, 1995)
- There is, however, a way to obtain some causal insights if we are willing to introduce an additional *monotonicity* assumption (Imbens and Angrist, 1994)
  - Monotonicity $\hat{=}$ every individual's treatment status X is affected by the instrument Z in the same direction



Example: Vietnam Draft

Vietnam Draft Lottery

- Conscription for serving in the Vietnam war was organized as a (somewhat macabre) lottery of birthdates for men born between 1944 and 1950
- From an urn with all dates of the year September 14 was drawn first and got assigned the number 1, the second date drawn got assigned the number 2 and so on. The first 195 birthdates drawn were eventually drafted.
- The lottery creates exogenous variation in military service, which can be used to estimate the labor market effects of veteran status (Angrist, 1990)



Local Average Treatment Effect

- For a binary instrument and binary treatment, we can divide the population in four subgroups depending on how their treatment status reacts to the instrument

$$\begin{aligned}
\text{Compliers:} \quad & X^{Z=0} = 0 \text{ and } X^{Z=1} = 1 \\
\text{Defiers:} \quad & X^{Z=0} = 1 \text{ and } X^{Z=1} = 0 \\
\text{Always takers:} \quad & X^{Z=0} = 1 \text{ and } X^{Z=1} = 1 \\
\text{Never-takers:} \quad & X^{Z=0} = 0 \text{ and } X^{Z=1} = 0
\end{aligned}$$

- Compliers only serve in the military $(X = 1)$ if they get drafted $(Z = 1)$
- Always-takers do military service $(X = 1)$ irrespective of wether they get drafted or not $()Z)$, and so forth
- Monotonicity assumption rules out the existence of defiers

Local Average Treatment Effect (II)

- If there are no defiers we can identify the causal effect of X on Y for the subgroup of compliers (Imbens and Angrist, 1994)
  - But only for this subgroup! The literature therefore calls this estimand a **"local average treatment effect"**
  - We can't say anything about the always- and never-takers, unless everyone has the same (homogenous) treatment effect, then LATE = ATE (special case)
- Problem: It's often hard to tell who the compliers are
  - Are compliers representative for the entire population?
  - The estimated LATE might thus not tell us much about the likely effect for non-compliers
- Problem 2: If the instrument doesn't effect treatment status X by much, the subgroup of compliers will be small
  - A small complier group can render effect estimates very unstable (small effective sample size), which is the so-called "weak instrument problem" stated in causal terms

Regression Discontinuity Design

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DOES A LONG-TERM ORIENTATION CREATE VALUE? EVIDENCE FROM A REGRESSION DISCONTINUITY

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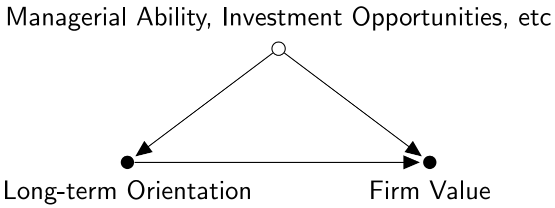
Research summary: *In this paper, we theorize and empirically investigate how a long-term orientation impacts firm value. To study this relationship, we exploit exogenous changes in executives' long-term incentives. Specifically, we examine shareholder proposals on long-term executive compensation that pass or fail by a small margin of votes. The passage of such "close call" proposals is akin to a random assignment of long-term incentives and hence provides a clean causal estimate. We find that the adoption of such proposals leads to (1) an increase in firm value and operating performance—suggesting that a long-term orientation is beneficial to companies—and (2) an increase in firms' investments in long-term strategies such as innovation and stakeholder relationships. Overall, our results are consistent with a "time-based" agency conflict between shareholders and managers.*

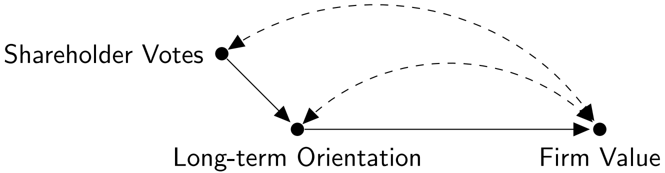
Introduction

- Research question:
  - Do companies face time-based agency problems?
  - Does the provision of long-term incentives to managers increase firm value and stimulate innovation activities?
- Confounding problem:
  - Managerial ability, investment opportunities, etc. drive both the long-term orientation and firm value
  - These confounding influences are unobserved at the firm-level



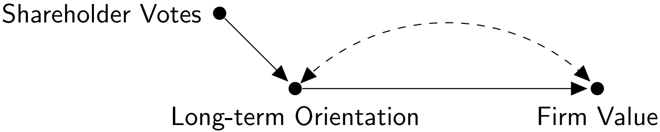
Regression Discontinuity Design (II)

- Research design:
  - Long-term executive compensation affects long-term orientation of a firm by incentivizing managers to create long-term value
  - By itself, shareholder votes on executive compensation plans are likely driven by the same unobservables though



Regression Discontinuity Design (III)

- Discontinuity:
  - Shareholders in public firms vote on executive compensation plans that incentivize long-term orientation
  - If we look at very "close call" votes, let's say between 49% and 51% for the proposal, we can reasonably assume that the respective firms do not differ systematically below and above the cutoff of 50%
  - At the same time, making the cut leads to a large impact on long-term orientation
  - I.e., in a close area around the cutoff, shareholder votes are a good instrument for long-term orientation

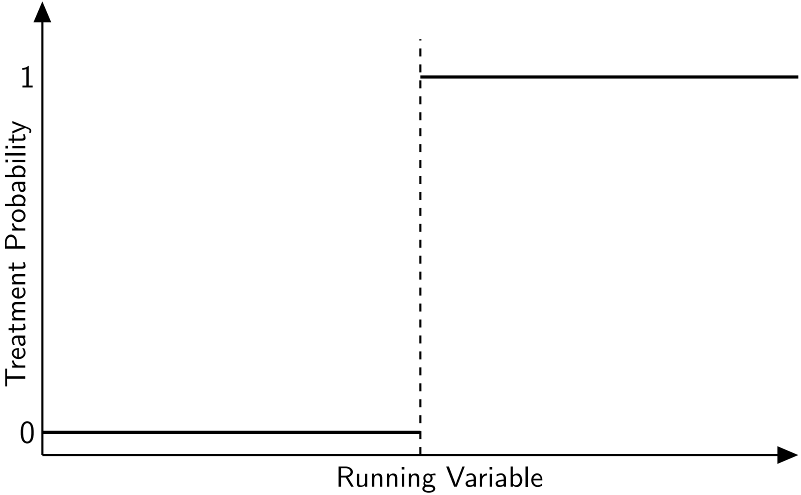


Sharp vs. Fuzzy RDD

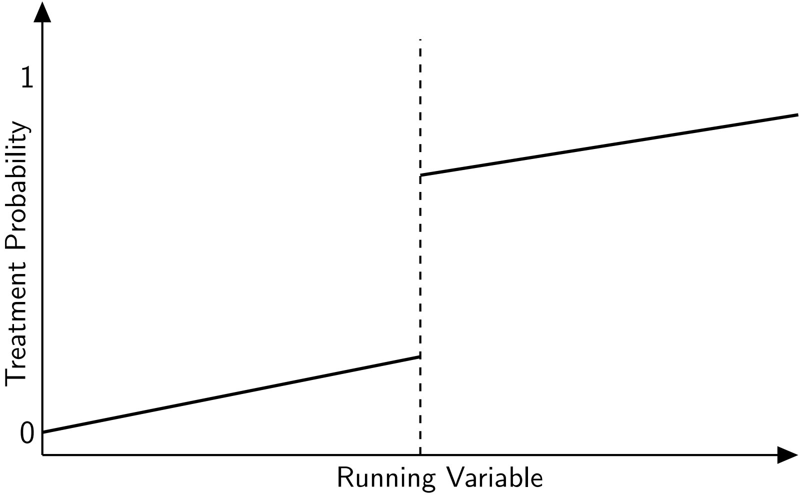
- There are two types of RDDs

1. Sharp
   - Probability to receive treatment jumps from zero to one at the discontinuity
   - Everyone above the threshold is treated and no-one below
2. Fuzzy
   - Probability to receive treatment jumps discontinuously but from a value above zero to a value below one
   - It's more likely to be treated if you're above the discontinuity, but this is not certain
   - The specification in Flammer and Bansal (2017) corresponds to a sharp RDD design

Sharp RDD



Fuzzy RDD



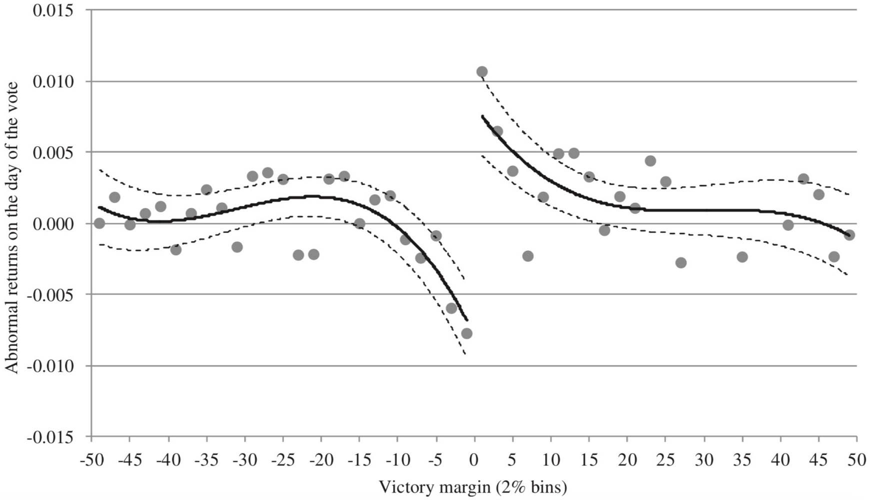
Estimation

- The basic estimation idea of an RDD could not be easier
  - Go as close to the discontinuity z_0 as possible and compare means below and above the threshold
- Problem with this approach:
  - The closer we make the window around z_0, the more data we lose, which makes our estimates unreliable
  - The wider we make the window, the more bias we possibly buy in
- Almost all practical issues with implementing RDDs revolve around this variance-bias trade-off
- Another drawback is external validity: an RDD only allows us to say something about a very specific population around the threshold
  - E.g., firms in which shareholder proposals on long-term compensation barely pass, might be very different from those where the proposal fails with zero votes in favor

Estimation (II)

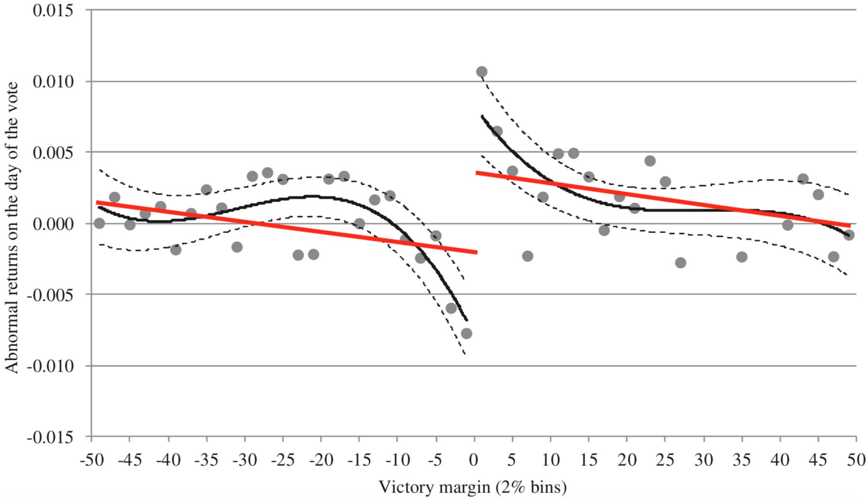
- First step in an RDD analysis is usually to plot the data to see what's going on
  - Is the jump at z_0 visually detectable?
- Instead of comparing means in a close window around z_0, we can fit two straight regressions lines below and above
  - The causal effect estimate is then just the difference between the two lines at z_0
- This functional form assumption is often too rigid though, because we can't be sure that everything is nicely linear
- Alternatively, we can fit more flexible polynomial regressions (including quadratic, cubic, etc, terms of the running variable) or use nonparametric regression techniques

Sensitivity to Functional Form Assumptions



Source: Flammer and Bansal (2017)

Sensitivity to Functional Form Assumptions

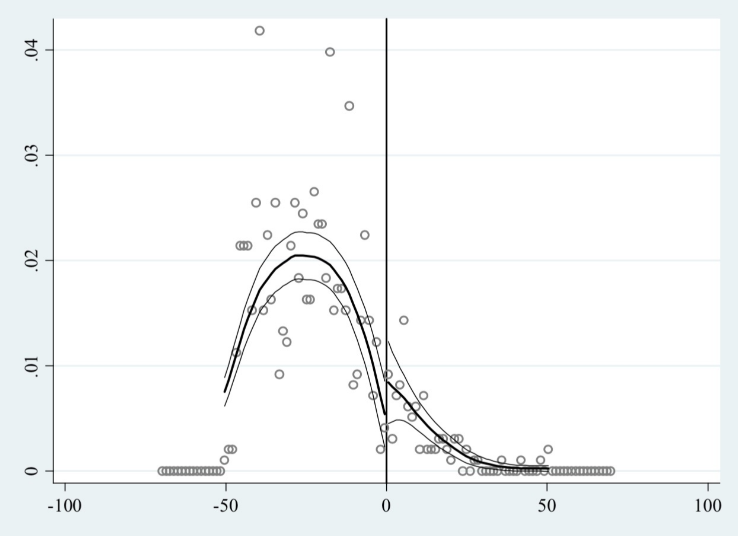


Source: Flammer and Bansal (2017)

Diagnostics

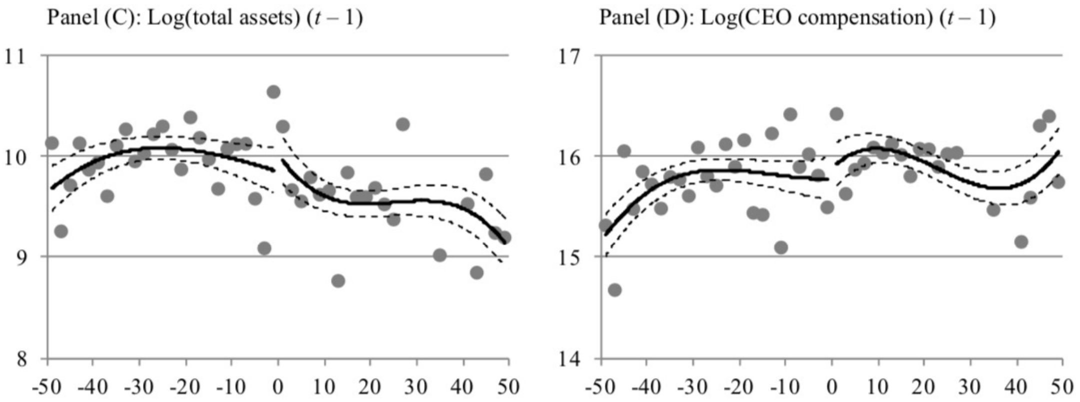
- The entire identification strategy in an RDD depends on the notion that there are no systematic differences between treatment and control group above and below the threshold
- What can go wrong?
- We might see "bunching" below or above the threshold
  - This would be an indication that individuals are somehow able to manipulate their running variable
  - Example: persuade teachers to still give minimum passing grade to go to college
  - This raises concern about self-selection: are those individuals that manage to manipulate their running variable different from the others?
- Do other covariates change discontinuously too?

McCrary Test



Source: Flammer and Bansal (2017), Online Appendix

Smoothness of Covariate Distribution



Source: Flammer and Bansal (2017), Online Appendix

Thank you

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