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# Instrumental Variables and regression discontinuity designs

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Nils Droste

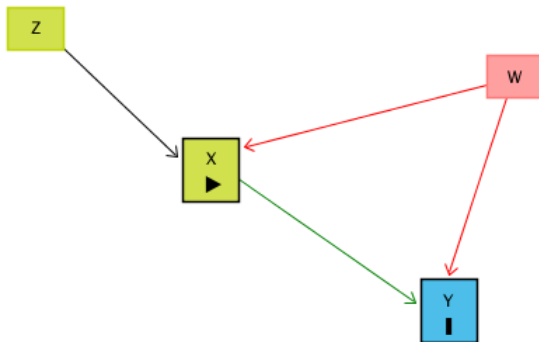
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2022 ClimBEco course



# Instrumental Variables

## Instrumental Variable (IV)



Using exogenous variation in instrument to close back-door. Image source: [Huntington-Klein 2018](#)



# Instrumental Variables

## An exemplary study

### The Impact of the Women's March on the U.S. House Election\*

Magdalena Larrebourg

Felipe González

April 10, 2021

Three million people participated in the Women's March against discrimination in 2017, the largest single-day protest in U.S. history. We show that protesters in the March increased political preferences for women and people from ethnic minorities in the following federal election, the 2018 House of Representatives Election. Using daily weather shocks as exogenous drivers of attendance at the March, we show that protesters increased turnout at the Election and the vote shares obtained by minorities, particularly women, irrespective of their party affiliation. We conclude that protests can help to empower historically underrepresented groups through changes in local political preferences.

Larrebourg and González 2021

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## Two-Stage Least Square (2SLS) estimator

1. stage: regress  $Z$  on  $X$ :

$$X_i = \alpha_1 + \beta_1 Z_i + \gamma_1 W_i + \varepsilon_{1,i} \quad (1)$$

and predict the variation in  $X$   
explained by  $Z$ :  $\hat{X} = \beta_1 Z_i$ .

2. stage: plug in  $\hat{X}$  to estimate the  
variation in  $Y$  not explained by  
confounder  $W$ :

$$Y_i = \alpha_2 + \beta_2 \hat{X}_i + \gamma_2 W_i + \varepsilon_{2,i} \quad (2)$$

# conditions

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There are important conditions to consider

- *relevance* of instrument for predicting  $Y \rightarrow E((\hat{X}_i|Z = 1) - (\hat{X}_i|Z = 0)) \neq 0$ , aka  $Z$  is correlated with  $X$ , and thus with  $Y$ .



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- *exclusion* restriction of  $Z$  being independent of  $Y$ :  $E(\epsilon_i, Z_i|W_i) = 0$ , aka no backdoor  $Z \rightarrow Y$  or endogeneity, i.e. no relation with omitted variables.



# conditions

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Now, let us see how to formulate this in the potential outcome notation.





# conditions

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Now, let us see how to formulate this in the potential outcome notation.

For this let treatment or participation again be denoted by  $D$ , now as a function of the instrument  $\rightarrow D_i(Z_i)$ , the *intention to treat*.

# notation

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## Step by step

- Imbens and Angrist (1994) formulate local average treatment effect (LATE)
  - for the *subpopulation* responding to instrument  $Z$ ,  
that is those who participate  $P(1)$  in treatment  $D$

# notation

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- Imbens and Angrist (1994) formulate local average treatment effect (LATE)
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$$E(Y_i|Z_i = 1) - E(Y_i|Z_i = 0) \quad (3)$$

# notation

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- here the LATE is given by  $P(1) \cdot E[Y_i(1) - Y_i(0)|D_i(1) = 1]$

# notation

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- here the LATE is given by  $P(1) \cdot E[Y_i(1) - Y_i(0)|D_i(1) = 1]$
- as long as participation  $P(1) > P(0)$  and  $D_i(1) \geq D_i(0) \forall i$ , aka monotonic (or  $\leq$ , respectively)

# notation

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## Why?

# Who's gonna be "treated"

Consider Angrist, Imbens and Rubin (1996)

		$Z_i = 0$	
		$D_i(0) = 0$	$D_i(0) = 1$
$Z_i = 1$	$D_i(1) = 0$	Nevertaker	Defier
	$D_i(1) = 1$	Complier	Always-taker

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# Who's gonna be "treated"

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- *"if people are more likely, on average, to participate given  $Z = w$  than given  $Z = z$ , then anyone who would participate given  $Z = z$  must also participate given  $Z = w$ " (Imbens and Angrist 1994)*





# Who's gonna be "treated"

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→ assumes existence of only one of compliers or defiers, e.g. *no one* defies



# Who's gonna be "treated"

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  - assumes existence of only one of compliers or defiers, e.g. *no one* defies
- allows valid estimate of LATE, but may not always be realistic



# Relaxing monotonicity assumption

de Chaisemartin (2017) shows IVs can be valid without strong monotonicity

- *"If there are defiers in the population, we only know that 2SLS estimates a weighted difference between the effect of the treatment among compliers and defiers"*

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# Relaxing monotonicity assumption

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de Chaisemartin (2017) shows IVs can be valid without strong monotonicity

- *"If there are defiers in the population, we only know that 2SLS estimates a weighted difference between the effect of the treatment among compliers and defiers"*
- a weak solution:  $P(C_F) = P(F)$  and  $E(Y(1) - Y(0)|C_F) = E(Y(1) - Y(0)|F)$



# Relaxing monotonicity assumption

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- a weak solution:  $P(C_F) = P(F)$  and  $E(Y(1) - Y(0)|C_F) = E(Y(1) - Y(0)|F)$
- *"is satisfied if a subgroup of compliers accounts for the same percentage of the population as defiers and has the same LATE"*



# Relaxing monotonicity assumption

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de Chaisemartin (2017) shows IVs can be valid without strong monotonicity

- *"If there are defiers in the population, we only know that 2SLS estimates a weighted difference between the effect of the treatment among compliers and defiers"*
- a weak solution:  $P(C_F) = P(F)$  and  $E(Y(1) - Y(0)|C_F) = E(Y(1) - Y(0)|F)$
- *"is satisfied if a subgroup of compliers accounts for the same percentage of the population as defiers and has the same LATE"*
- I believe this can be approached with matching, too. See Murray et al. (2021) who suggest to estimate the intention to treat  $D(Z)$  with logistic regression, providing leeway for a propensity score or other matching approach (cf. Hirano, Imbens and Ridder 2003; Rosenbaum and Rubin 1984).

# Weak instruments

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When instruments are only weakly correlated with treatment, reconsider

$$Y_i = \alpha_2 + \beta_2 D_i + \gamma_2 W_i + \varepsilon_i \quad (4)$$

$$D_i = \alpha_1 + \beta_1 Z_i + \gamma_1 W_i + v_i \quad (5)$$

A condition was relevance, i.e.  $E((D_i|Z = 1) - (D_i|Z = 0)) \neq 0$ , or

$$\text{Cov}(Z_i, D_i | W_i) \neq 0$$

- to estimate IV,  $\hat{\beta}_2 = \frac{\text{Cov}(Y_i, Z_i)}{\text{Cov}(Z_i, D_i | W_i)}$
- problematic when  $\text{Cov}(Z_i, D_i | W_i) \rightarrow 0$  as  $\Delta\beta_2$  grows large even for small variations

# Weak instruments

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A range of techniques for robust parameter estimation in weak IV 2SLS

- F-test for strong enough instruments (Stock and Yogo 2005)





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A range of techniques for robust parameter estimation in weak IV 2SLS

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- heteroskedasticity and autocorrelation robust for just identified models (Chernozhukov and Hansen 2008)



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- heteroskedasticity, autocorrelation and cluster robust in a more general setting (Montiel Olea and Pflueger 2013)



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- a more powerful test with t-ratio critical value adjustments for significance testing (Lee et al. 2020)

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- heteroskedasticity, autocorrelation and cluster robust in a more general setting (Montiel Olea and Pflueger 2013)
- a more powerful test with t-ratio critical value adjustments for significance testing (Lee et al. 2020)

→ We will look into some (basic) testing in the seminar.

# intermediate summary

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Instrumental variables allow us to

- isolate a treatment effect by looking at the outcomes of exogeneously caused treatment variation
- it is considered a very robust causal inference, but assumptions are *somewhat* crucial
- mainly it is theory and reason that make a "valid instrument"
- there is loads of tests, I do not think they alone suffice



# A first thought experiment

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Available online at [www.sciencedirect.com](http://www.sciencedirect.com)



Ecological Economics 55 (2005) 527–538

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[www.elsevier.com/locate/ecolecon](http://www.elsevier.com/locate/ecolecon)

## ANALYSIS

# Environmental pressure group strength and air pollution: An empirical analysis

Seth Binder, Eric Neumayer\*

*Department of Geography and Environment and Center for Environmental Policy and Governance (CEPG),  
London School of Economics and Political Science, Houghton Street, London WC2A 2AE, UK*

Received 7 December 2003; received in revised form 22 October 2004; accepted 14 December 2004

Available online 24 February 2005

# A first thought experiment

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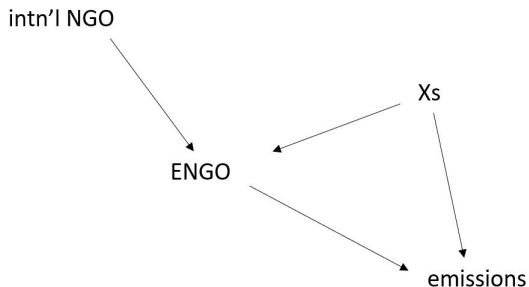
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Do you think this is a valid instrument?



reformulating Binder and Neumayer 2005: a (partial DAG)

# Regression discontinuity designs (RDD) – intuition

Suppose we believe there is an effect for which assignment is non-random, but the cut-off at which treatment is assigned is quasi-random (cf. Thistlewaite et al. 2016).

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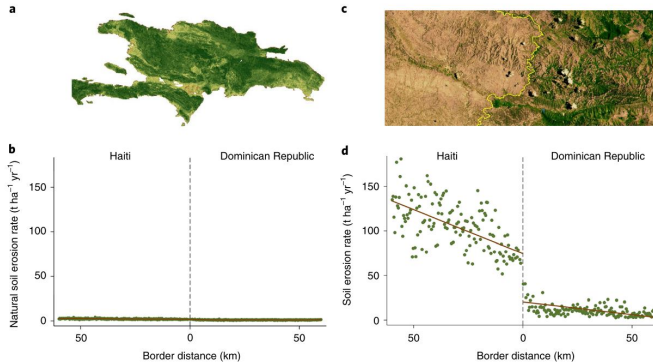
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# Regression discontinuity designs (RDD) – intuition

Suppose we believe there is an effect for which assignment is non-random, but the cut-off at which treatment is assigned is quasi-random (cf. Thistlewaite et al. 2016).



The border between Haiti and the Dominican Republic. Image source: Wuepper, Borrelli and Finger 2020



# concept

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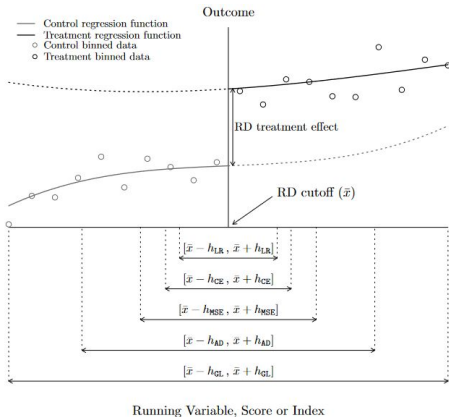
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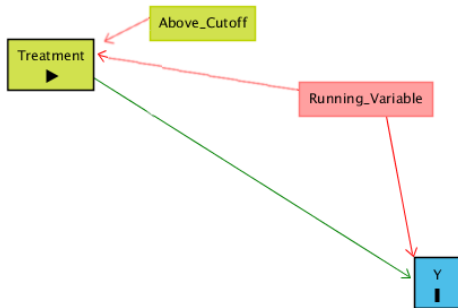
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The RDD concept and the effect of bin size choice. Image source: Cattaneo and Vazquez-Bare 2016

# DAGs

## Regression-Discontinuity-Design (RDD)

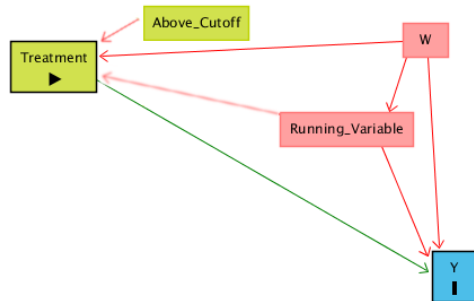
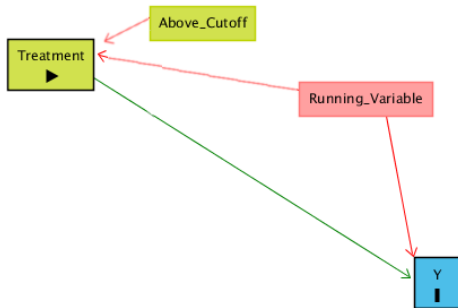


Focussing on effects just around the cutoff value. Image source: [Huntington-Klein 2018](#)



# DAGs

## Regression-Discontinuity-Design (RDD)



Focussing on effects just around the cutoff value. Image source: [Huntington-Klein 2018](#)

→ Do you see the IV in RDD?



# notation

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Let us formulate in the potential outcomes notation. Suppose there is a outcome  $(Y(1), Y(0))$  that depends on treatment  $D$  and covariate  $X$ .



# notation

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Let us formulate in the potential outcomes notation. Suppose there is a outcome  $(Y(1), Y(0))$  that depends on treatment  $D$  and covariate  $X$ .

While  $Y(X)$  is assumed to be continuous, treatment  $D$  kicks in at a quasi-random threshold of  $\bar{x}$ , such that



# notation

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While  $Y(X)$  is assumed to be continuous, treatment  $D$  kicks in at a quasi-random threshold of  $\bar{x}$ , such that

$$D_i = \begin{cases} 1 & \text{if } x_i \geq \bar{x} \\ 0 & \text{if } x_i < \bar{x} \end{cases} \quad (6)$$

# notation

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Let us formulate in the potential outcomes notation. Suppose there is a outcome  $(Y(1), Y(0))$  that depends on treatment  $D$  and covariate  $X$ .

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$$D_i = \begin{cases} 1 & \text{if } x_i \geq \bar{x} \\ 0 & \text{if } x_i < \bar{x} \end{cases} \quad (6)$$

The identifying assumption is again that treatment assignment is independent of outcomes  $E(Y(1) - Y(0) \perp D | X = \bar{x})$ .



# Sharp RDD

The cutcoff at  $\bar{x}$  can be **sharp**

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# Sharp RDD

The cutcoff at  $\bar{x}$  can be **sharp**

- in which case there is no overlap on both sides of  $\bar{x}$

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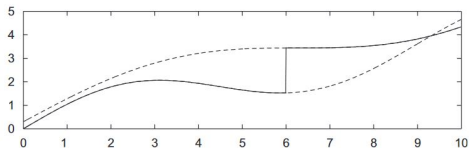
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# Sharp RDD

The cutoff at  $\bar{x}$  can be **sharp**

- in which case there is no overlap on both sides of  $\bar{x}$
- we assume the outcomes would have been smooth in the absence of treatment (aka extrapolate a "bin" beyond the threshold)



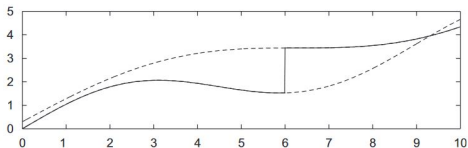
Imbens and Lemieux 2008



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The cutoff at  $\bar{x}$  can be **sharp**

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Imbens and Lemieux 2008

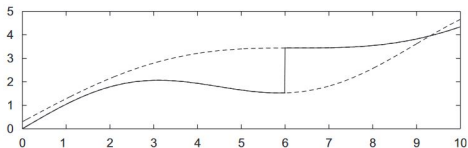
- and measure  $\tau_{srd} = \lim_{x \rightarrow \bar{x}} E[Y(1)|X = x] - \lim_{x \leftarrow \bar{x}} E[Y(0)|X = x]$



# Sharp RDD

The cutoff at  $\bar{x}$  can be **sharp**

- in which case there is no overlap on both sides of  $\bar{x}$
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Imbens and Lemieux 2008

- and measure  $\tau_{srdd} = \lim_{x \rightarrow \bar{x}} E[Y(1)|X = x] - \lim_{x \leftarrow \bar{x}} E[Y(0)|X = x]$
- $D$  is not just correlated but a deterministic function of  $x$  (once we know  $x$  and  $\bar{x}$ , we know  $D$ )



# estimator

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# estimator

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## RDD estimation

$$Y_i = \alpha_i + \beta X_{it} + \gamma t_i + \varepsilon_{it} \quad (7)$$

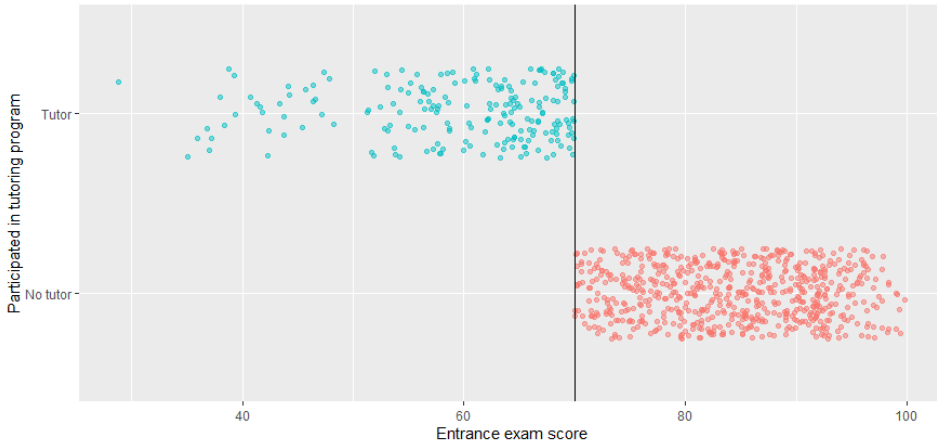
where  $t$  indicates treatment cutoff values  $\bar{x}$ :

$$t_i = \begin{cases} 1 & \text{if } x_i \geq \bar{x} \\ 0 & \text{if } x_i < \bar{x} \end{cases} \quad (8)$$

This would often include polynomial terms to allow for non-linear functional forms (but should not, cf. Gelman and Imbens 2019). Another typical approach is a local linear regression (which is displayed in the animation) or smoothing functions.

# fuzzy RDD

Suppose the data did **not** look like this





# fuzzy RDD

but rather looked like this



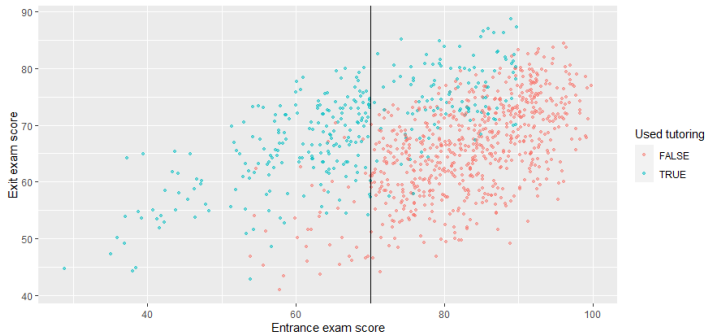
Heiss 2020

2022 ClimBEco course



# fuzzy RDD

So we need to evaluate



Heiss 2020



# fuzzy RDD

So we need to evaluate



Heiss 2020

This is literally an IV setting where a different probability on two sides of the cutoff predicts participation.



# Fuzzy RDD

The cutoff at  $\bar{x}$  is be **fuzzy**

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# Fuzzy RDD

The cutoff at  $\bar{x}$  is be **fuzzy**

- because of deniers or nevertakers etc, there is overlap on both sides of  $\bar{x}$

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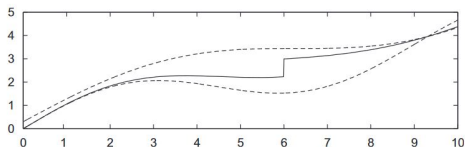
## References



# Fuzzy RDD

The cutoff at  $\bar{x}$  is be **fuzzy**

- because of deniers or nevertakers etc, there is overlap on both sides of  $\bar{x}$
- probabilities differ:  $\lim_{x \rightarrow \bar{x}} Pr(Y(1)|X = x) \neq \lim_{x \leftarrow \bar{x}} Pr(Y(0)|X = x)$



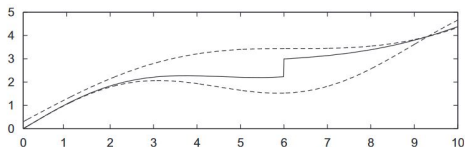
Imbens and Lemieux 2008



# Fuzzy RDD

The cutoff at  $\bar{x}$  is be **fuzzy**

- because of deniers or nevertakers etc, there is overlap on both sides of  $\bar{x}$
- probabilities differ:  $\lim_{x \rightarrow \bar{x}} Pr(Y(1)|X = x) \neq \lim_{x \leftarrow \bar{x}} Pr(Y(0)|X = x)$



Imbens and Lemieux 2008

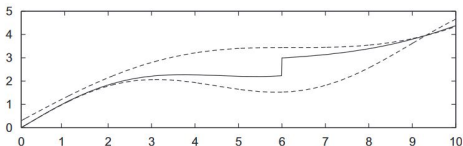
- if unconfounded,  $\tau_{frdd} = E[Y(1)|D = 1, X = \bar{x}] - E[Y(0)|D = 1, X = \bar{x}]$



# Fuzzy RDD

The cutoff at  $\bar{x}$  is be **fuzzy**

- because of deniers or nevertakers etc, there is overlap on both sides of  $\bar{x}$
- probabilities differ:  $\lim_{x \rightarrow \bar{x}} Pr(Y(1)|X = x) \neq \lim_{x \leftarrow \bar{x}} Pr(Y(0)|X = x)$



Imbens and Lemieux 2008

- if unconfounded,  $\tau_{frdd} = E[Y(1)|D = 1, X = \bar{x}] - E[Y(0)|D = 1, X = \bar{x}]$
- which we can estimate with 2SLS, predicting  $D$  in first stage, plugging estimates into second stage

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# examples

There are discontinuities in space



Wuepper et al. 2020



# examples

There are discontinuities in space

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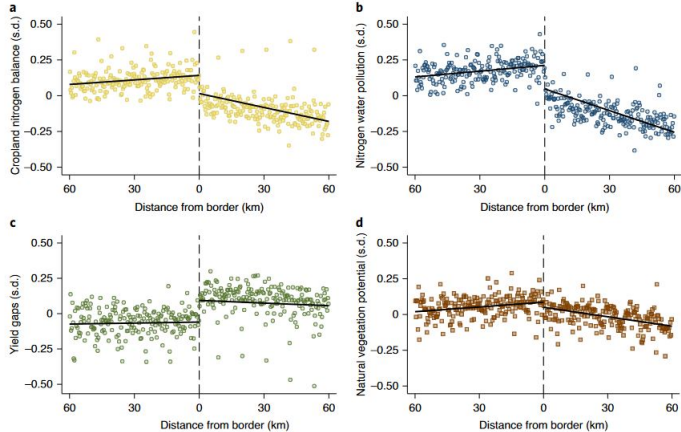
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Regression discontinuities in covariates but not in vegetation potential, Wuepper et al. 2020

# examples

## Time

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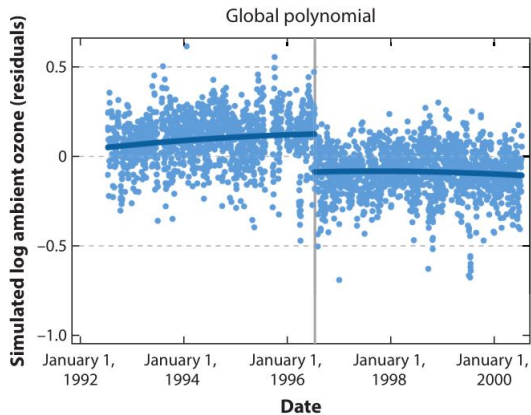
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Hausman and Rapson 2018

# examples

## Rules

### ***The Causal Effect of Radical Right Success on Mainstream Parties' Policy Positions: A Regression Discontinuity Approach***

TARIK ABOU-CHADI AND WERNER KRAUSE\*

This article investigates how the success of radical right parties affects the policy positions of mainstream parties. We do this using a regression discontinuity approach that allows us to causally attribute mainstream parties' positional changes to radical right strength independent of public opinion as a potential confounder. Making use of exogenous variation created through differences in electoral thresholds, we empirically demonstrate that radical right success, indeed, causally affects mainstream parties' positions. This is true for mainstream left as well as mainstream right parties. These findings make an important contribution to the broader literature on party competition as they indicate that other parties' behavior and not only public opinion plays a crucial role in explaining parties' policy shift.

*Keywords:* radical right; party competition; immigration.

Abou-Chadi and Krause 2018



# examples

## Rules

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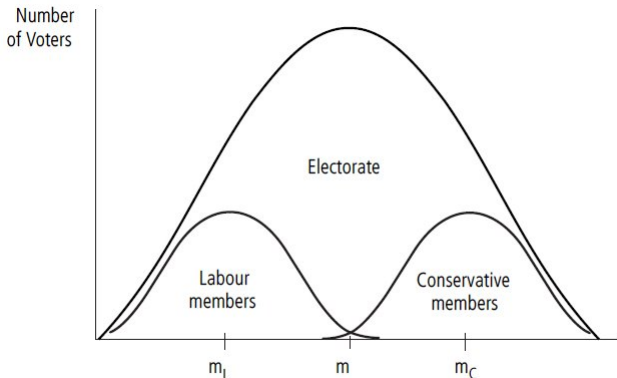
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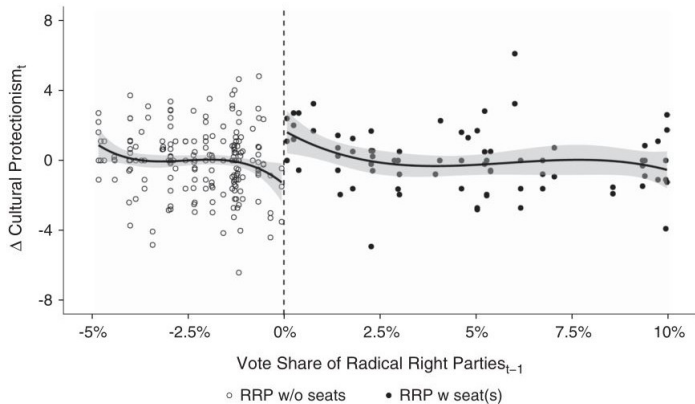


Hotelling-Downs Model of 2 Party Competition. Image Source: [Daniel Corradi Stevens](#)



# examples

## Rules

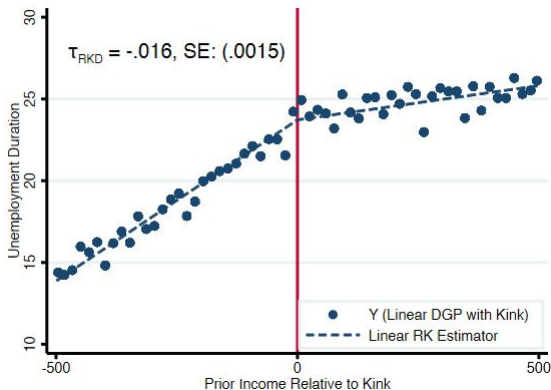


Mainstream party position on cultural position. Image source: Abou-Chadi and Krause 2018



# examples

There can be kinks, aka slope shifts

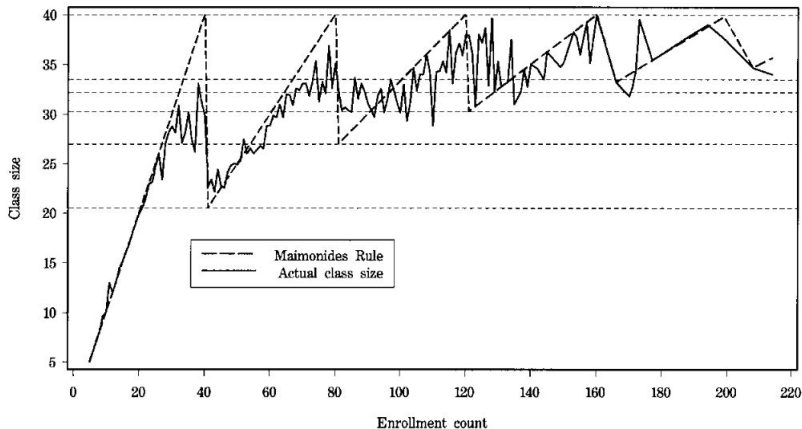


Ganong and Jäger 2018



# examples

## Multiple breaks



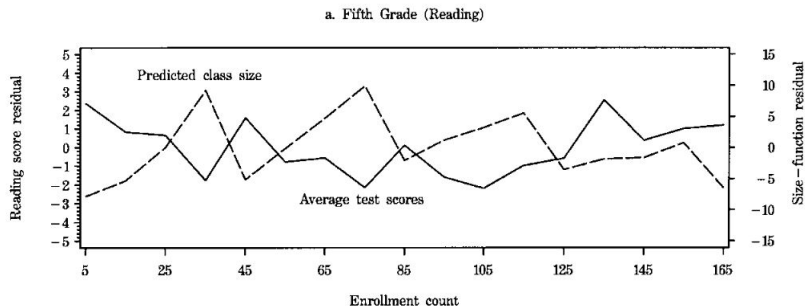
Angrist and Lavy 1999  
2022 ClimBEco course





# examples

## Multiple breaks



Angrist and Lavy 1999



# software

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## available packages

- rdd
- rdrobust
- rdlocrand
- rddensity
- rdmulti
- rdpower

# intermediate summary

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### Regression discontinuity designs

- identify a causal effect at a (quasi-)randomly occurring break point that introduces treatment
- are the youngest "classical" causal inference methods and seen as favorable
- use breaks that can occur in space, time, institutions, etc. pp.



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