Causal Inference: Instrumental Variables and 2SLS Estimation

Dr. Ria Ivandić

21st February 2022

Reminders

- Please continue to wear a mask in class and test before coming to class!
- Second assignment is due this Friday on Canvas.
- If you need help, book an office hour slot with Felipe, Ken or myself.

General Feedback from Assignment 1

- Use the concepts that we have covered throughout the module and be very technical about them. For example, if you mention a concept such as omitted variable bias, state what this assumption means and what implies to the question at hand.
- Make sure to answer all the sub-questions that are within a question.
- Optional: Introduce the study that you are being asked to evaluate. Show that you know the set-up: 1) what are the treatments, 2) what is the identification strategy, 3) what are the outcomes, etc.
- Whenever you are being asked to conduct some analysis, please explain what you are doing and why you are doing it. For instance, if you are being asked to run a regression, say what is the outcome variable, what is the independent variable, if you are clustering standard errors, say at what level. In summary, walk us through your analysis.

General Feedback from Assignment 1

- **Always interpret your statistical results.** When you report a coefficient, don't just say the number. Provide the substantive interpretation with greater detail. For instance, if you got from a two-way fixed-effect model that the beta coefficient was 1.10, say that the treatment increased 1.10 percentage points on the outcome variable, controlling for time and unit fixed effects.
- Annotate your R script as much as possible. This will help us to identify the code that you used for each question.
- Round up your results using between 3 to 4 decimals. You don't report to need 6 or more decimals.
- Optional: You can report the code and the output of your analysis in each question rather than in the appendix.

Overview

Previous three weeks were about "selection-on-observables": how to estimate treatment effects by controlling for all relevant covariates, and exploiting variation in time and units in a "difference-in-differences" .

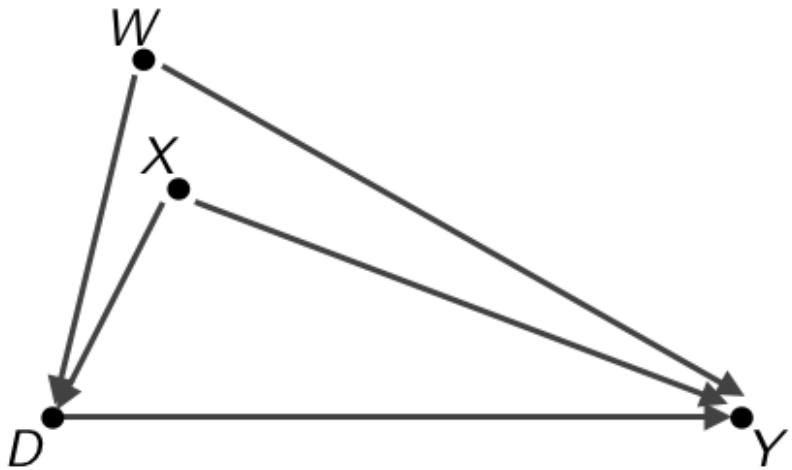
This week we consider situations where:

- Treatment depends on unobservables, i.e. CIA does not hold
- **But** treatment also depends on an as-if random variable Z_i that only affects the outcome through treatment (at least conditional on covariates).

This special variable Z_i is an **instrument**: it changes D_i, and we can use this change to measure the effect of D_i.

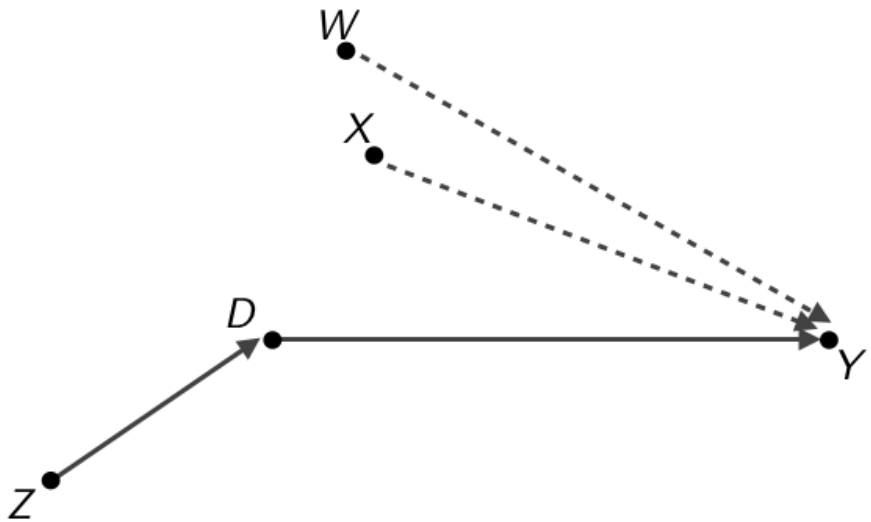
Graphical overview: selection on observables

To estimate the effect of D on Y, we must observe and control for X and W.



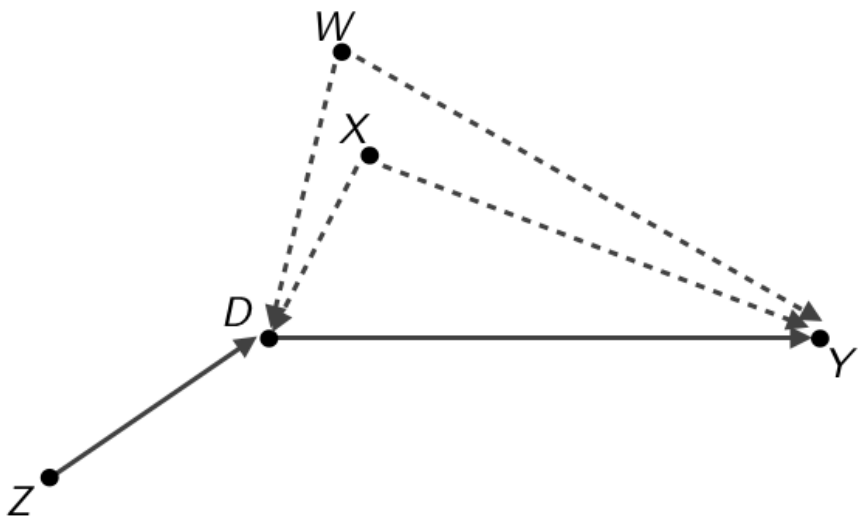
Graphical overview: randomized experiment

If D is completely determined by a randomization process Z, so we can measure the effect of D on Y, even if X and W are not observed (e.g. through DIGM).



Graphical overview: instrumental variables

If D is partly determined by random Z, and Z does not affect Y in any other way, we can measure the effect of D on Y. This is the case even if X and W are not observed (through IV techniques).
In this case, we call Z the instrument.



Treatment assigned vs. treatment received

In an experiment, we can distinguish between treatment assigned Z_i and treatment received D_i.

We previously (implicitly) assumed $D_i = Z_i$. But in practice there may be **non-compliance**:

- GOTV canvassing experiment in which some people don't answer the door We discussed this in the lecture on randomized experiments
- lottery for school places in which some lottery winners do not attend
- draft lottery for military in which some are drafted but do not serve, some not drafted but serve

One-sided and two-sided non-compliance

	Control	Treatment
GOTV canvassing experiment	No visit from canvasser	Visit from canvasser (but some people don't answer door)
Lottery for school places	No place offered at school	Place offered at school (but some people don't attend)
Military draft	Not drafted (but some volunteer and serve anyway)	Drafted (but some receive exemption or deferment)

Intention-to-treat (ITT)

Denote by $Y_{i,Z1}$ and $Y_{i,Z0}$ i's potential outcomes if *assigned* to treatment vs. control.

Intention-to-treat (ITT) effect defined as $\text{ITT} \equiv E\big[Y_{i,Z1} - Y_{i,Z0}\big]$.

If Z_i is randomized, $E[Y_i|Z_i = 1] - E[Y_i|Z_i = 0]$ is an unbiased estimator of the ITT.

If Z_i is randomized but there is non-compliance (i.e. $Z_i \neq D_i$ for some i), $E[Y_i|D_i = 1] - E[Y_i|D_i = 0]$ (DIGM) will generally not be an unbiased estimator of the ATE.
Consider the examples of lottery for private school places.

Instrumental variables methods (IV) let us use ITT (effect of treatment assignment) to estimate an ATE (effect of treatment).

Compliance types

		Assigned to control ($Z_i = 0$)	
		Not treated ($D_i = 0$)	Treated ($D_i = 1$)
Assigned to treatment ($Z_i = 1$)	Not treated ($D_i = 0$)	Never taker (N)	Defier (D)
	Treated ($D_i = 1$)	Complier (C)	Always taker (A)

- Can we identify the compliance type of an individual?
- Can we measure the proportion of each compliance type (π_A, π_C, π_D, π_N)? (Only with some further assumptions.)

Estimating compliance frequencies

Who gets treated when $Z_i = 0$? *Always-takers* and *defiers*.
Who gets treated when $Z_i = 1$? *Always-takers* and *compliers*.

Assumption: treatment assignment Z_i is random $\Longrightarrow$ compliance-type proportions same for $Z_i = 0$ and $Z_i = 1$.

$$\begin{aligned} E\big[D_i = 1 | Z_i = 0\big] &= \pi_A + \pi_D \\ E\big[D_i = 1 | Z_i = 1\big] &= \pi_A + \pi_C \end{aligned}$$

Can't estimate π_A, π_D, π_C, or π_N.

Estimating compliance frequencies (2)

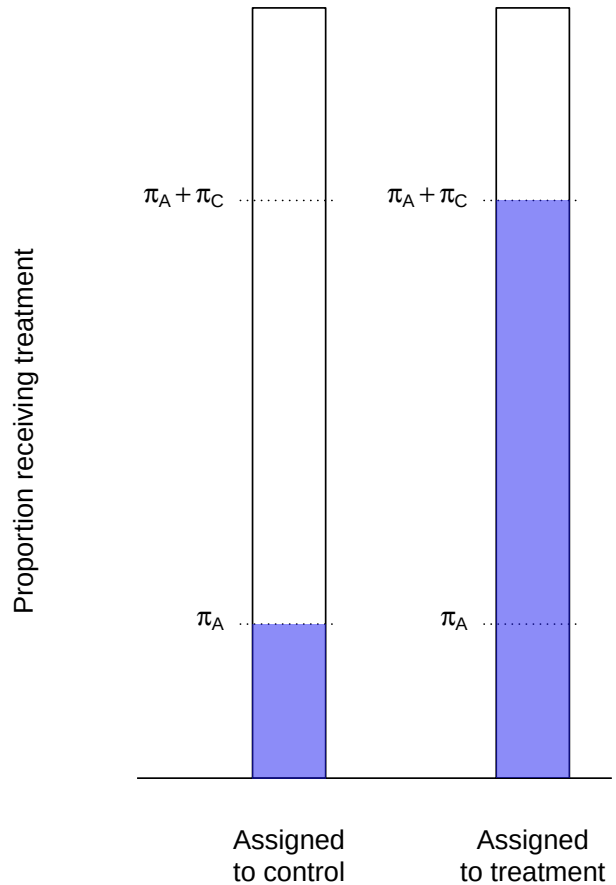
Further assumption: $\pi_D = 0$ (**no defiers**).

This implies

$$E\big[D_i = 1|Z_i = 0\big] = \pi_A$$

and

$$E\big[D_i = 1|Z_i = 1\big] - E\big[D_i = 1|Z_i = 0\big] = \pi_C$$



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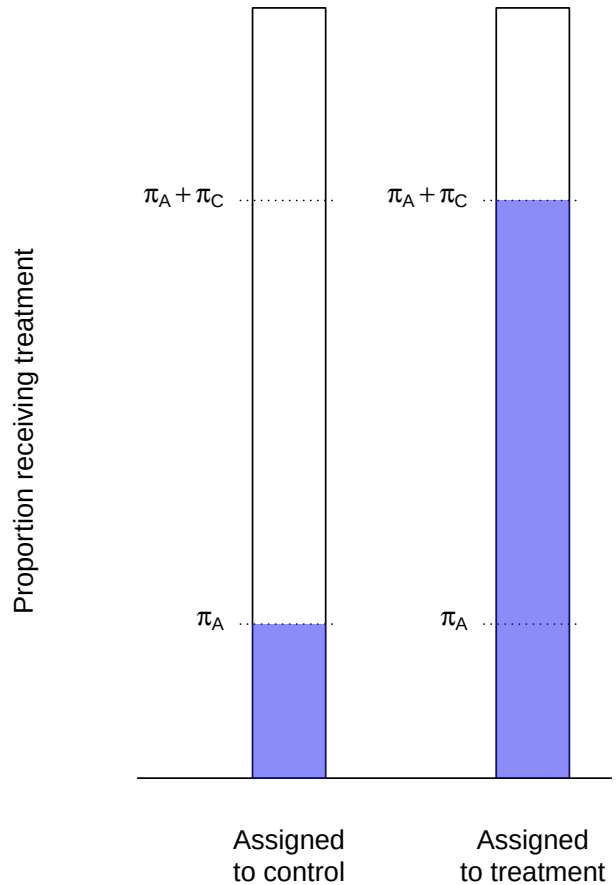

ITT decomposition

We can decompose the ITT by **compliance type**.

Let π_G and ITT_G be proportion and ITT for compliance type $G \in \{C, A, N, D\}$.

Then by definition

$$\text{ITT} = \pi_C \text{ITT}_C + \pi_A \text{ITT}_A + \pi_N \text{ITT}_N + \pi_D \text{ITT}_D \tag{1}$$

Let's assume

- **No defiers** (monotonicity).
- **Exclusion restriction:** *Treatment assigned* only affects outcomes by affecting *treatment received*.

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ITT decomposition

No defiers tells us that $\pi_D = 0$. **Exclusion restriction** tells us that $\text{ITT}_A = \text{ITT}_N = 0$. So:

$$\text{ITT} = \pi_C \text{ITT}_C + \pi_A 0 + \pi_N 0 + 0 \text{ITT}_D. \tag{2}$$

Exclusion also tells us that, for compliers, the effect of *treatment assignment* on outcomes is the same as the effect of *treatment* on outcomes:

$$\text{ITT} = \pi_C \text{CATE}_C, \tag{3}$$

where CATE_C is the conditional average treatment effect for compliers.

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LATE and the Wald estimator

Assuming $\pi_C > 0$ (**non-zero complier proportion**), the **conditional average treatment effect for compliers** or **local average treatment effect (LATE)** is

$$\text{LATE} = \text{CATE}_C = \frac{\text{ITT}}{\pi_C} \tag{4}$$

If in addition Z_i is randomly assigned, we have an unbiased estimator for the above - the **Wald estimator**:

$$\hat{\text{CATE}}_C = \frac{E\big[Y_i|Z_i=1\big] - E\big[Y_i|Z_i=0\big]}{E\big[D_i|Z_i=1\big] - E\big[D_i|Z_i=0\big]} = \frac{\text{effect of } Z_i \text{ on } Y_i}{\text{effect of } Z_i \text{ on } D_i} = \frac{\text{ITT}_Y}{\text{ITT}_D} \tag{5}$$

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LATE and the Wald estimator

Four assumptions used (not including SUTVA):

- No defiers (monotonicity)
- Exclusion restriction (Z_i affects Y_i only through D_i)
- Non-zero complier proportion
- Random assignment of Z_i

Applications of IV

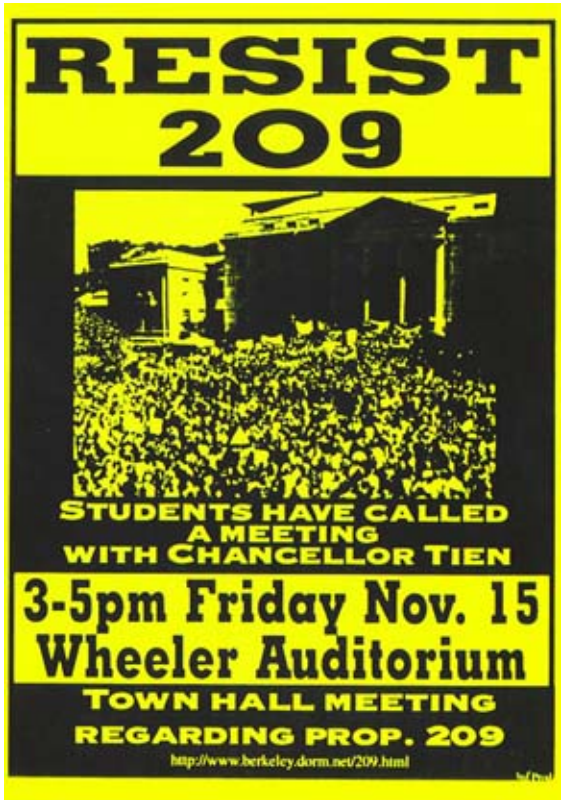
IV methods can be seen as a remedy for a **broken experiment**, i.e. failure to obtain 100% compliance.

More positively, **IV methods can be used as part of the design of using natural experiments** in which some random variation in Z_i creates some variation in D_i and then (given exclusion restriction) measures effect of D_i on some outcome Y_i.

Encouragement design example

Proposition 209: 1996 ballot proposition to end race-based preferences (affirmative action) in California government policies

Research question (Albertson and Lawrence 2009): Could watching a TV program affect citizens' attitudes toward Prop. 209?



Albertson and Lawrence 2009: Design

- Representative sample of households in Orange County, CA, interviewed by phone in October 1996
- All respondents told there will be a follow-up interview after the election
- Random subset of respondents told to watch upcoming TV debate on Prop. 209
- In follow-up, asked if they watched the debate; supported Prop. 209; felt knowledgeable about Prop. 209

In this design:

- What are Z_i, D_i, Y_i?
- What does intention-to-treat (ITT) effect mean?
- What is the exclusion restriction?
- What does the LATE (CATE_C) measure?

Albertson and Lawrence 2009: Data

	$Z_i = 0$	$Z_i = 1$	Difference
Watched TV program	0.052	0.48	0.428
Know about Prop. 209	3.251	3.293	0.041
Support Prop. 209	0.654	0.651	-0.003

- What is π_C (proportion of compliers)?
- What is the ITT?
- What is LATE i.e. CATE_C?

Taking stock

We assumed **binary treatment assignment** and **binary treatment**.

Given random treatment assignment, we can

- estimate the **intention-to-treat** effect (ITT) by comparing average Y_i among units assigned to treatment and units assigned to control
- estimate the **proportion of compliers** (π_C) by comparing average D_i among units assigned to treatment and units assigned to control
- estimate the **LATE** (CATE_C) by dividing the ITT by the proportion of compliers

Can we generalize this somehow?

- non-binary treatment (D_i)
- non-binary instrument (Z_i)
- covariates (e.g. because non-random Z_i)
- more than one instrument

Another way to get LATE

We estimated the LATE with

$$\frac{E\big[Y_i|Z_i=1\big]-E\big[Y_i|Z_i=0\big]}{E\big[D_i|Z_i=1\big]-E\big[D_i|Z_i=0\big]}=\frac{\text{ITT}_Y}{\text{ITT}_D}$$

Here is another way:

- Regress D_i on Z_i, get fitted values $\hat{D}_i$
- Regress Y_i on $\hat{D}_i$

This is called **two-stage least squares**.

Why does 2SLS work? Basic intuition (1)

Regressing Y on Z gives you ITT_Y.

Given binary Z, ITT_Y is an underestimate of CATE_C.

Wald estimator inflates ITT_Y by dividing by compliance rate $(E[D_i|Z_i = 1] - E[D_i|Z_i = 0])$.

TSLS inflates ITT_Y by replacing Z by $\hat{D}$, i.e. regressing Y on $\hat{D}$ instead of Y on Z.

Now we can generalize

Wald estimator is limited to binary D_i and Z_i:

$$\lambda = \frac{E\big[Y_i|Z_i=1\big] - E\big[Y_i|Z_i=0\big]}{E\big[D_i|Z_i=1\big] - E\big[D_i|Z_i=0\big]} = \frac{\text{ITT}_Y}{\text{ITT}_D}$$

Two-stage least squares is a much more general procedure:

$$\begin{aligned} \text{First stage:} \quad D_i &= \alpha_1 + \phi Z_i + \beta_1 X_{1i} + \gamma_1 X_{2i} + e_{1i} \\ \text{Second stage:} \quad Y_i &= \alpha_2 + \lambda \hat{D}_i + \beta_2 X_{1i} + \gamma_2 X_{2i} + e_{2i} \end{aligned}$$

where Z_i and D_i might not be binary and you can include covariates e.g. X_{1i}, X_{2i}. same covariates must be included in the first and second stage

Two-stage least squares: terminology

Terminology:

$$\begin{aligned}
\text{Reduced form:} \quad Y_i &= \alpha_0 + \rho Z_i + \beta_0 X_{1i} + \gamma_0 X_{2i} + e_{0i} \\
\text{First stage:} \quad D_i &= \alpha_1 + \phi Z_i + \beta_1 X_{1i} + \gamma_1 X_{2i} + e_{1i} \\
\text{Second stage:} \quad Y_i &= \alpha_2 + \lambda \hat{D}_i + \beta_2 X_{1i} + \gamma_2 X_{2i} + e_{2i}
\end{aligned}$$

NB: λ is the LATE. Must use same covariates in first stage and second stage.

Two-stage least squares: assumptions

Key assumptions (Wald assumptions with covariates and without "complier" terminology):

- **Non-zero first-stage**: instrument affects treatment, conditional on covariates ($\phi \neq 0$ in first stage)
- **Independence** (exogeneity, ignorability): instrument unrelated to potential outcomes, conditional on covariates (no OVB on ρ in reduced form or ϕ in first stage)
- **Exclusion restriction**: instrument only affects outcome through treatment, conditional on covariates
- **Monotonicity**: instrument's effect on treatment is weakly positive or weakly negative for all units

Colantone and Stanig (2018): Globalization and the Brexit vote?

- **Question**: Did economic globalization lead to support for the Leave option in the Brexit referendum?
- **Treatment**: Import Shock is the strength of the Chinese import shock at the regional level i
- **Outcome**: Leave Share (for Brexit) in NUTS3 region i

To consider:

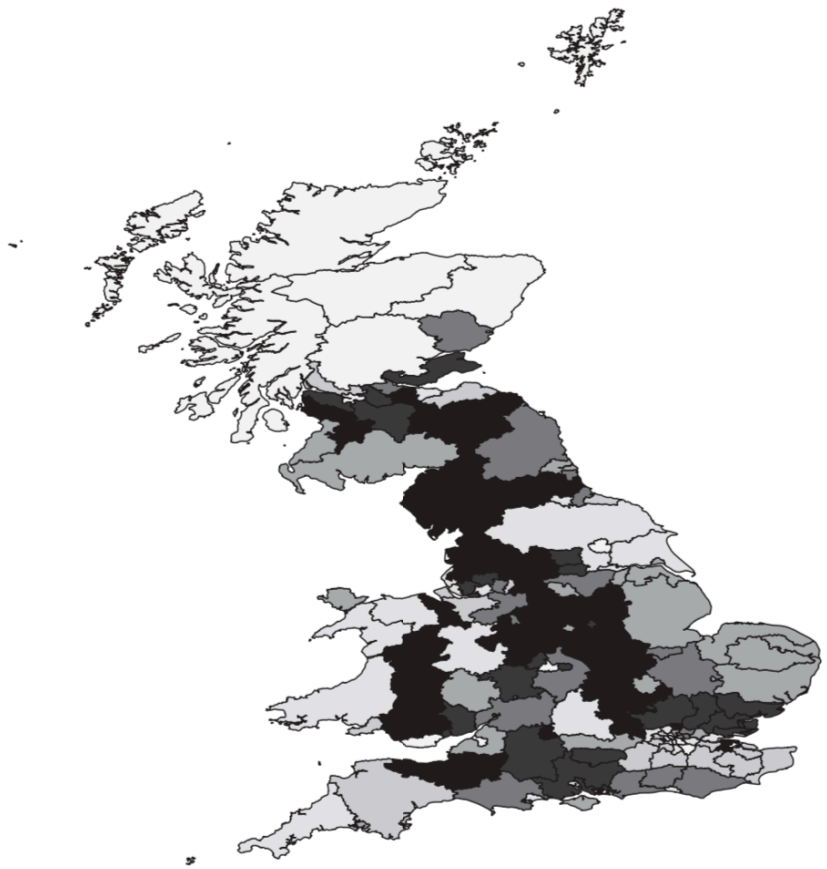
- What about just regressing outcome on treatment?
- What covariates might remove the bias in that regression?
- How might an IV approach help?

Colantone and Stanig (2018): Plausible Instrument

- The role of China in the import shock: in two decades the import share went from 1% to around 9% since China's WTO membership
- The literature (Autor, Dorn, and Hanson, 2013) has identified reasons why the surge in Chinese competition constitutes an excellent exogenous source of identification.
- **Chinese import shock**: the measure has a very intuitive interpretation: for given changes in nation-level imports per worker, the Chinese shock will be stronger in those regions in which a larger share of workers was initially employed in industries witnessing larger subsequent increases in imports from China
- They instrument the import shock using the growth in imports from China to the United States across industries, which is due to the exogenous changes in supply conditions in China, rather than to domestic factors in the United Kingdom that could be correlated with electoral outcomes.

Colantone and Stanig (2018): Plausible Instrument

FIGURE 2. Strength of the Import Shock Across NUTS-3 Regions



Note: Darker shades correspond to stronger import shock.

Colantone and Stanig (2018): Results

TABLE 1. Regional-level Results

VARIABLES	(1) Leave Share	(2) Leave Share	(3) Leave Share	(4) Leave Share	(5) Leave Share	(6) Leave Share
Import Shock	12.233**	12.225***	12.965***	12.085***	11.073***	12.299***
	[4.763]	[4.091]	[4.543]	[3.890]	[3.861]	[3.726]
Immigrant Share				−0.490***	−0.513***	−0.491***
				[0.165]	[0.155]	[0.154]
Immigrant Arrivals				−0.066	0.496	−0.058
				[0.741]	[0.801]	[0.691]
NUTS-1 Fixed Effects	Y	Y	Y	Y	Y	Y
NUTS-2 Random Intercepts	N	Y	N	N	Y	N
Observations	167	167	167	167	167	167
R-Squared	0.57		0.57	0.65		0.65
Kleibergen-Paap F Statistic			662.7			614
Number of Groups		39			39	
Model	Linear	Hierarchical	IV	Linear	Hierarchical	IV

Standard errors clustered by NUTS-2 area in all columns except 2 and 5.
***p < 0.01, **p < 0.05, *p < 0.1

Martin and Yurukoglu (2017) on impact of Fox News in USA

- **Question**: "how much does consuming slanted news, like the Fox News Channel, change individuals' partisan voting preferences?"
- **Treatment**: Minutes spent watching Fox News Channel, based on surveys
- **Outcome**: Voting in presidential election, based on aggregate zip code-level results

To consider:

- What about just regressing outcome on treatment?
- What covariates might remove the bias in that regression?
- How might an IV approach help?

Martin & Yurukoglu (2)

Instrument: channel position of Fox News on the cable lineup

Evaluate:

- **Independence** (exogeneity, ignorability): instrument unrelated to potential outcomes, conditional on covariates
- **Exclusion restriction**: instrument only affects outcome through treatment, conditional on covariates
- **Monotonicity**: instrument's effect on treatment is weakly positive or weakly negative for all units

Martin & Yurukoglu: first-stage

TABLE 2—FIRST-STAGE REGRESSIONS: NIELSEN DATA

	FNC minutes per week					
	(1)	(2)	(3)	(4)	(5)	(6)
FNC position	−0.146	−0.075	−0.174	−0.167	−0.097	−0.111
	(0.043)	(0.039)	(0.028)	(0.025)	(0.033)	(0.030)
MSNBC position	0.078	0.073	0.064	0.070	0.019	0.020
	(0.036)	(0.032)	(0.025)	(0.022)	(0.034)	(0.035)
Has MSNBC only	1.904	1.137	−3.954	−2.804	−1.220	−1.562
	(3.697)	(3.713)	(4.255)	(3.416)	(6.180)	(5.397)
Has FNC only	31.423	26.526	23.460	22.011	15.141	15.069
	(2.677)	(2.546)	(2.278)	(1.864)	(2.697)	(2.314)
Has both	24.859	23.118	18.338	16.168	15.159	14.486
	(2.919)	(2.687)	(2.361)	(1.991)	(3.216)	(2.842)
Satellite FNC minutes				0.197		0.173
				(0.013)		(0.015)
Fixed effects	Year	State-year	State-year	State-year	County-year	County-year
Cable controls	Yes	Yes	Yes	Yes	Yes	Yes
Demographics	None	None	Extended	Extended	Extended	Extended
Robust F-stat	11.39	3.72	39.02	44.7	8.86	13.43
Number of clusters	5,789	5,789	4,830	4,761	4,839	4,770
Observations	71,150	71,150	59,541	52,053	59,684	52,165
R^2	0.030	0.074	0.213	0.377	0.428	0.544

Notes: Cluster-robust standard errors in parentheses (clustered by cable system). Instrument is the ordinal position of FNC on the local system. The omitted category for the availability dummies is systems where neither FNC nor

Martin & Yurukoglu: second-stage

TABLE 4—SECOND STAGE REGRESSIONS: ZIP CODE VOTING DATA

	2008 McCain vote percentage			
	(1)	(2)	(3)	(4)
Predicted FNC minutes	0.152	0.120	0.157	0.098
	(0.056, 0.277)	(0.005, 0.248)	(−0.126, 0.938)	(−0.121, 0.429)
Satellite FNC minutes		−0.021		−0.015
		(−0.047, 0.001)		(−0.073, 0.022)
Fixed effects	State	State	County	County
Cable system controls	Yes	Yes	Yes	Yes
Demographics	Extended	Extended	Extended	Extended
Number of clusters	4,814	3,993	4,729	4,001
Observations	17,400	12,417	17,283	12,443
R^2	0.833	0.841	0.907	0.919

Notes: The first stage is estimated using viewership data for all Nielsen TV households. See first-stage tables for description of instruments and control variables. Observations in the first stage are weighted by the number of survey individuals in the zip code according to Nielsen. Confidence intervals are generated from 1,000 independent STID-block-bootstraps of the first and second stage datasets. Reported lower and upper bounds give the central 95 percent interval of the relevant bootstrapped statistic.

Why we should be skeptical of most IV designs

IV designs must convince us of two key untestable assumptions:

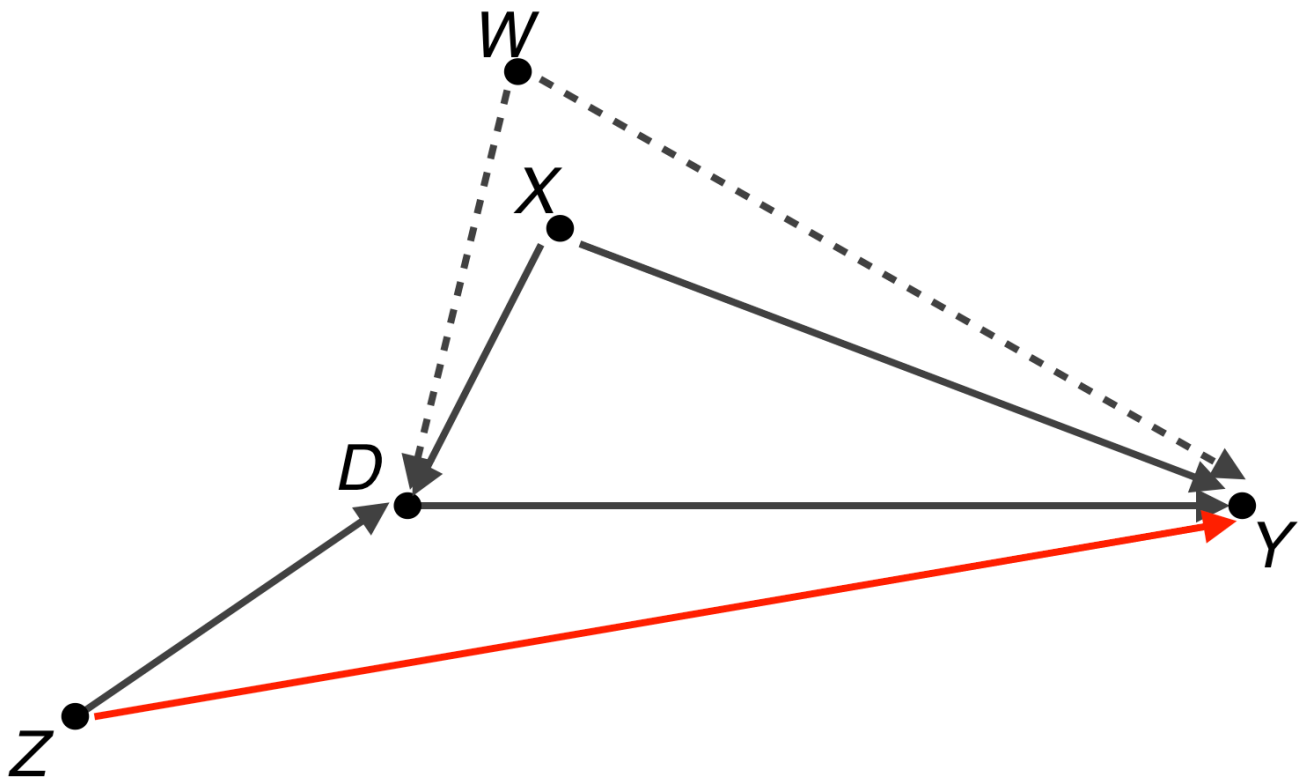
- The instrument Z_i satisfies **independence**, i.e. the CIA is met with respect to D_i and Y_i, e.g. because Z_i is random
- The instrument Z_i satisfies **exclusion**, i.e. it only affects Y_i through D_i

When Z_i is randomly determined in an experiment, it's easier to accept **independence** and think hard and discuss the **exclusion** assumption.

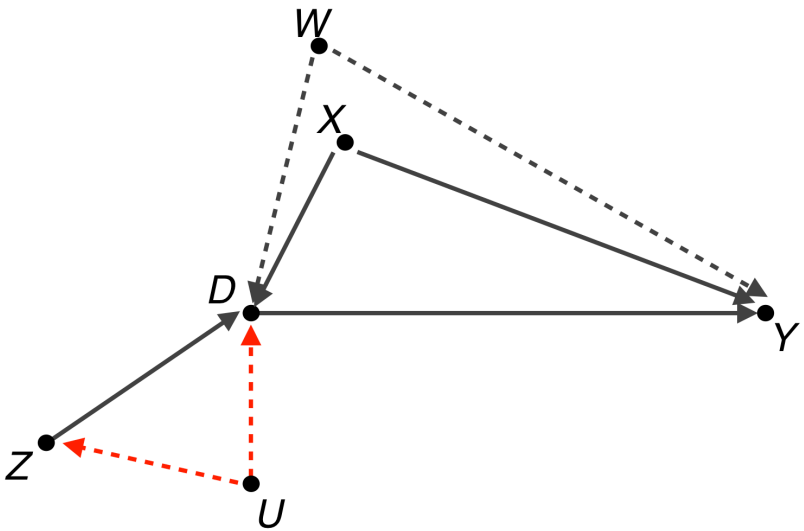
In an observational study, one should be skeptical about both.

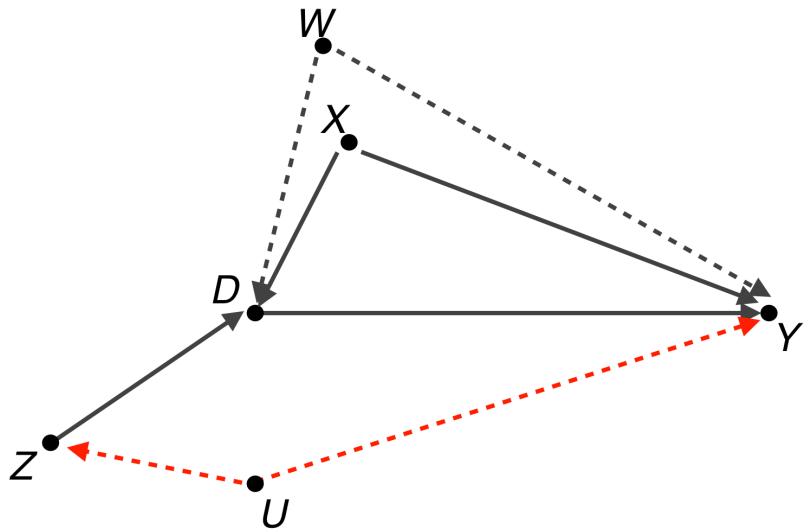
- Is the CIA really satisfied in the reduced form?
- Is D_i really the only channel through which Z_i affects Y_i?

Exclusion restriction violation DAG



Independence violations DAGs





(where U is an unobserved covariate)

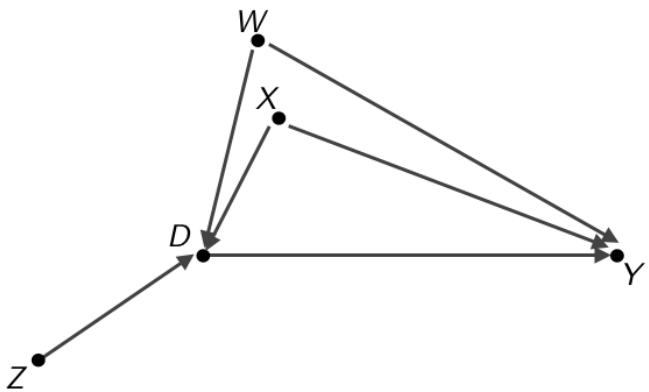
Can we test the exclusion restriction?

What about

- regress Y on D and Z
- conclude exclusion is valid if coefficient on Z is 0

Unfortunately this doesn't work, because D is also affected by X and W:

- if X and W are observed, you don't need IV to estimate effect of D on Y
- if they are *not* observed, by controlling for D you induce an association between Z and X and/or W, leading to **collider bias**



Can we test the exclusion restriction? (2)

Two main strategies:

- *placebo population test of the reduced form*: move the (reduced-form) analysis to a different population in which the instrument should not affect the treatment, show zero estimated (reduced form) effect (e.g. Acharya, Blackwell, and Sen 2016 JOP)
- *placebo outcome test of the first stage*: replace the treatment with something that should not be affected by instrument but may suffer from same OVB (e.g. lagged treatment), show zero estimated first-stage effect (e.g. Meredith 2013 APSR)

Think about applying to the Colantone and Stanig research?

Further thoughts on IV

- Could you use IVs in your own research? Examples.
- A good instrument is hard to find – another reason to start by looking for randomness.
- In observational studies, a variable that satisfies **independence** (CIA) is a rare and wonderful thing. Usually **exclusion** is doubtful, but you can measure its effect and speculate about channels.
- Now that you know about instrumental variables, you should not refer to an independent variable in a regression as an "IV": say "treatment", "control variable", "covariate", "regressor", "RHS variable"
- The recent econometrics literature is very skeptical of the internal validity of IVs, see for example: *Lal, Apoorva, Mackenzie William Lockhart, Yiqing Xu, and Ziwen Zu. (2021) "How Much Should We Trust Instrumental Variable Estimates in Political Science? Practical Advice based on Over 60 Replicated Studies."*