Week 8: Instrumental Variables II

PUBL0050 Causal Inference

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This Lecture

Instrumental Variables and Selection Bias

Solutions to OVB so far

Throughout this course we have been concerned with finding solutions to selection bias. We have so far used the following strategies:

- **Randomize treatment** $\rightarrow$ eliminate selection bias in expectation
- **Condition on observables** $\rightarrow$ eliminate selection bias by controlling for confounders
- **Condition on unobservables** $\rightarrow$ eliminate selection bias by using repeated observations over time

Instrumental Variables and OVB

- **Instrumental variables** $\rightarrow$ eliminate selection bias by finding an instrument that is (as good as) random, and only affects Y_i through D_i
- Last week: IV methods to solve non-random non-compliance in experiments
  - The instrument was random treatment assignment (or: 'encouragement' to take the treatment)
- This week: IV as a more general framework to help address selection bias
  - Instruments as (as good as) randomly occuring encouragements/nudges towards treatment
  - Much more common in social science research where only about 6% of IV designs focus on experimental data[1]

[1] Sovey and Green, 2011

IVs in observational studies

- In observational studies, we are often interested in **treatments** that are **not assigned at random**
- However, we may be able to find an **instrument** that 'nudges' units towards treatment, and is **more plausibly random** than the treatment
  - Newhouse and Mclellan (1998)

    Distance from hospital (Z_i) → Heart attack treatment (D_i) → Health outcomes (Y_i)
  - Angrist (1990)

    Vietnam draft number (Z_i) → Military service (D_i) → Income (Y_i)

Key idea

When D_i is not randomly assigned, we can estimate **an** ATE by using only the variation in D_i due to the randomly assigned Z_i.

IV intuition

1. In observational data, units are not randomly assigned to treatment and control and therefore any causal estimates will be confounded.
2. But we may be able to identify instances where the **encouragement** of units to treatment is plausibly as good as random.
3. **Assuming** units are randomly encouraged to take the treatment, we can assess downstream consequences using IV techniques.
4. The causal effects we estimate will still relate only to those units who would have had a different treatment status in the absence of the encouragement.
   - i.e. we will still be calculating a LATE

Important note

We need to employ **all** of the same assumptions as last week.

Identification with IVs in Observational Settings

The **same four**[2] assumptions need to hold in an observational setting than in an experimental one for an IV approach to be credible.

Assumption II: First stage

$0 < P(Z_i = 1) < 1$ & $P(D_1 = 1) \neq P(D_0 = 1)$
i.e. the instrument Z_i induces some variation in D_i.

Assumption III: Monotonicity

$D_{1i} \geq D_{0i}$
i.e. there are no defiers.

[2] With a minor yet crucial modification to the independence of instrument assumption.

The four horsemen of IV

In the **experimental** setting:

Assumption I: Independence of the instrument

$(Y_{0i}, Y_{1i}, D_{0i}, D_{1i}) \perp\!\!\!\perp Z_i$
i.e. the instrument is assigned at random.

In the **observational** setting:

Assumption I: *Conditional* independence of the instrument

$(Y_{0i}, Y_{1i}, D_{0i}, D_{1i}) \perp\!\!\!\perp Z_i | X_i$
i.e. the instrument is assigned as good as random **conditional on covariates** X_i.

- Note that this is equivalent to going from the *independence of the treatment* in lecture 2 to the ***conditional*** *independence of the treatment* assumption in lectures 3 and 4.

The four horsemen of IV

Assumption IV: Exclusion restriction

$$Y(D_i = 1, Z_i = 1) = Y(D_i = 1, Z_i = 0)$$

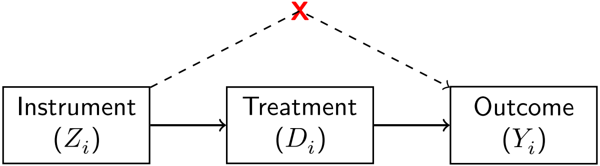
and

$$Y(D_i = 0, Z_i = 1) = Y(D_i = 0, Z_i = 0)$$

i.e. the treatment assignment **only** affects Y by affecting the treatment received.

- This is arguably the hardest one to make convincingly in an observational setting, where the instrument is not as closely logically related to the treatment

Exclusion restriction: Z_i affects Y_i only through D_i



Implications:

1. The effect of randomized encouragement can be **entirely** attributed to the subset of **units who comply** with the encouragement
   - i.e. the encouragement has no effect on the outcome for non-complying units
2. Among compliers, the effect of the encouragement **only** affects Y **via the treatment**
   - i.e. there can be no direct effect of Z_i on Y_i

Finding 'good' instruments

These are **very** demanding assumptions in many observational studies!

> *"A necessary but not a sufficient condition for having an instrument that can satisfy the exclusion restriction is if people are confused when you tell them about the instrument's relationship to the outcome...Instruments are jarring [...] because these two things (Z_i and Y_i) don't seem to go together. If they did, it would likely mean that the exclusion restriction was violated. But if they don't, then the person is confused, and that is at minimum a possible candidate for a good instrument."*
>
> *–Scott Cunningham, The Causal Inference Mixtape*

> *"So, where can you find an instrumental variable? Good instruments come from a combination of institutional knowledge and ideas about the process determining the variable of interest."*
>
> *–MHE, p. 117*

Finding ‘good’ instruments

Implications

- The exclusion restriction is an untestable assumption
- IV requires the researcher to justify their choice of instrument and persuade people that the relevant assumptions hold
- Good theory is often required to justify any instrument
- And even if both the indepencence assumption and the exlcusion restriction hold, the instrument may not be pushing that many units into treatment
  - I.e. the instrument may be weak!

Weak Instruments

Weak instruments

Assumption II: First stage

$0 < P(Z_i = 1) < 1$ & $P(D_1 = 1) \neq P(D_0 = 1)$
i.e. the instrument Z_i induces some variation in D_i.

- This assumption says that the instrument must push some units towards treatment (i.e. there are *some* compliers).
  - But what if the push is not much of a push at all?
- When the instrument has a small first-stage effect – i.e. when it explains little of the variation in D_i – it is said to be **weak**.
- Problem: IV estimates are very unstable when the instrument is weak.

The problem with weak instruments

Recall that the simple IV Estimator is given by:

$$\begin{aligned} LATE &= \frac{ITT}{\pi_C} = \frac{E[Y_i|Z_i=1]-E[Y_i|Z_i=0]}{E[D_i|Z_i=1]-E[D_i|Z_i=0]} \\ &= \frac{Cov(Y_i,Z_i)}{Cov(D_i,Z_i)} = \frac{\text{Effect of } Z_i \text{ on } Y_i}{\text{Effect of } Z_i \text{ on } D_i} \end{aligned}$$

- If $Cov(D_i, Z_i) = 0$, the instrument is **irrelevant**, and LATE is undefined
  - In other words: there are no compliers to estimate an effect for!
- Further, as $Cov(D_i, Z_i) \to 0$:
  - the 2SLS estimator is biased towards the OLS estimator
  - the standard errors of the 2SLS estimates will be too small
- Worse still, the bias of the 2SLS does not diminish in large samples, i.e. having lots of data doesn't save you from having weak instruments!

How weak is weak?

How do we assess whether Z_i is weak?

- Specify two models with the treatment as dependent variable:

$$\begin{aligned} \text{Full first stage model } (M_2)\text{:} \quad & D_i = \alpha_1 + \beta_z Z_i + \beta_1 X_{1i} + \beta_2 X_{2i} \\ \text{Restricted model } (M_1)\text{:} \quad & D_i = \alpha_1 + \beta_1 X_{1i} + \beta_2 X_{2i} \end{aligned}$$

- Use an F-test to compare the nested models:

$$F = \frac{(R^2_{M_2} - R^2_{M_1})/(k_{M_2} - k_{M_1})}{(1 - R^2_{M_2})/(n - (k_{M_2} - 1))}$$

Intuition: F is bigger when Z_i explains more variation in D_i ($R^2_{M_2}$)

Rule of thumb: If $F \gtrsim 10$, Z_i is fine.

F-test in R

```r
model_1 <- lm(D ~ X1 + X2 + X3 + X4, data = my_data)
model_2 <- lm(D ~ Z + X1 + X2 + X3 + X4, data = my_data)

library(lmtest)
waldtest(model_1, model_2)
```

```
## Wald test
##
## Model 1: D ~ X1 + X2 + X3 + X4
## Model 2: D ~ Z + X1 + X2 + X3 + X4
##   Res.Df Df      F    Pr(>F)
## 1    995
## 2    994  1 35.641 3.298e-09 ***
## ---
## Signif. codes:  0 '***' 0.001 '**' 0.01 '*' 0.05 '.' 0.1 ' ' 1
```

Two Stage Least Squares Extensions

2SLS review

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

where $\hat{D}_i$ are the fitted values from the 1^{st} stage, and $\hat{\beta}_2$ estimates the LATE.

Key assumptions:

- **(Cond.) Independence** – Z_i is (as good as) randomly assigned
- **First stage** – Z_i has some effect on D_i
- **Monotonicity** – No defiers
- **Exclusion restriction** – Z_i only affects Y_i through D_i

Extending 2SLS

We can extend the IV LATE framework with a 2SLS estimator to incorporate three extensions:

1. Multiple instruments
   - To strengthen the first stage assumption (more compliers)
2. Inclusion of covariates
   - To strengthen the **conditional** independence assumption
3. Continuous instrumental and/or treatment variables
   - When either of Z_i or D_i are continuous

Example: Family size and education

Do children from larger families receive a worse education?

What is the causal relationship between family size and the education children receive? There is a marked negative correlation between family size and educational attainment in much of the developing world, but we have little causal evidence on this question. Angrist et al (2010) use data on family size and education in Israel to address this question.

- Outcome (Y_i): Years of schooling completed by first-born child
- Treatment (D_i): Family size (number of children)
- Instrument (Z_{1i}): 1 if the second birth resulted in twins
- Instrument (Z_{2i}): 1 if the first two children were the same sex

Possible source of selection bias: The number of children in a family is clearly not randomly assigned, and parents who decide to have more children will be different from those who have fewer.

Example: Family size and education

What is the theoretical justification for these instruments?

- Z_{1i}: Whether a mother gives birth to twins is as good as random, and definitely affects the size of the family
  - Families with a twin second birth have a third of a child more than singleton second-birth families
- Z_{2i}: Gender composition of births is as good as random, and families with two same-gender children are more likely to have another child than families with two different-gender children
  - Families with two same-gendered children are about 10% more likely to have an additional child than families with two different-gendered children

Question: Are the exclusion restriction and independence assumptions reasonable?

Multiple instruments

With multiple instruments, the 2SLS estimate is a weighted average of the individual Wald estimates.

- If we have:

$$\tau^{j}_{\text{LATE}} = \frac{Cov(Y_i, Z_{ji})}{Cov(D_i, Z_{ji})}, \qquad \text{for} \qquad j \in 1, 2$$

- The 2SLS can be shown to be **(see MHE, p. 174)**:

$$\tau_{\text{2SLS}} = \pi \tau^{1}_{\text{LATE}} + (1 - \pi)\tau^{2}_{\text{LATE}}$$

  where π is a weight that is proportional to the relative strength of the first stage relationship for each instrument.

Implication:

- 2SLS with multiple instruments is a weighted average of causal effects for the (instrument-specific) compliant subpopulations
- The amount that each instrument 'contributes' to the 2SLS estimate is determined by the relative strength of the first stage

Multiple instruments example

Twins instrument

$$\begin{aligned} \text{First stage}: \quad D_i &= \alpha_1 + 0.320 Z_{1i} + \epsilon_{1i} \\ \text{Second stage}: \quad Y_i &= \alpha_2 + .174 \hat{D}_i + \epsilon_{2i} \end{aligned}$$

Same sex instrument

$$\begin{aligned} \text{First stage}: \quad D_i &= \alpha_1 + .079 Z_{2i} + \epsilon_{1i} \\ \text{Second stage}: \quad Y_i &= \alpha_2 + .318 \hat{D}_i + \epsilon_{2i} \end{aligned}$$

Both instruments

$$\begin{aligned} \text{First stage}: \quad D_i &= \alpha_1 + .449 Z_{1i} + 0.076 Z_{2i} + \epsilon_{1i} \\ \text{Second stage}: \quad Y_i &= \alpha_2 + .202 \hat{D}_i + \epsilon_{2i} \end{aligned}$$

Implications

- The 2SLS estimate is a weighted average of causal effects for the same-sex and twins instruments
- The 2SLS estimate is closer to the twins IV estimate than the same-sex IV estimate because of the stronger first stage relationship
- When we calculate the standard errors, the 2SLS is not significant

Multiple instruments

Note that the assumptions needed for multiple-instrument IV are even more demanding:

- "As if random" assignment holds for **all** instruments
- Exclusion restriction holds for **all** instruments

If these assumptions are not met for any of the instruments, the 2SLS estimate will be biased.

2SLS with covariates

We may have reason to think that our instrument is only "as good as random" **conditional** on covariates:

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

Remember, the independence of the instrument assumption here is that Z_i is independent of potential treatments and outcomes **conditional on** X_i:

Assumption I: *Conditional* independence of the instrument

$(Y_{0i}, Y_{1i}, D_{0i}, D_{1i}) \perp\!\!\!\perp Z_i | X_i$
i.e. the instrument is as good as random *conditional* on covariates X_i.

2SLS with covariates

We may have reason to think that our instrument is only "as good as random" **conditional** on covariates:

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

For example:

- Older women are more likely to have twin births
- Twin births are only as good as random, conditional on age
- $\rightarrow$ control for age in both first-stage and second-stage models

Interpretation:

β_2 is a weighted average of the covariate-cell specific LATE estimates.

Covariate or instrument?

- Think back to our example last week where we had:
  - Outcome: Opinion change (1 if changed opinion on either candidate)
  - Treatment assignment: Encouragement to watch (1 if encouraged)
  - Treatment: Watched the debate (1 if watched, 0 otherwise)
- How can we distinguish between an instrument (Z_i) and a covariate (X_i)?

	Does not affect Y_i	Affects Y_i
Does not affect D_i	**Noise** (e.g. month of birth)	**Irrelevant determinant** (e.g. post-debate campaign)
Affects D_i	**Instrument** (e.g. encouragement)	**Covariate** (e.g. gender)

Controlling vs Instrumenting, repeat

Controlling for a Confounder

```
# Regression anatomy
treat_reg <- lm(watched ~ female, data = debate)
debate$residuals <- resid(treat_reg) ## !!!!!!!!!!
resid_reg <- lm(changed_opinion ~ residuals, data=debate)
# Which is equivalent to:
long_reg <- lm(changed_opinion ~ watched + female, data = debate)

coef(resid_reg)
```

```
## (Intercept)   residuals
##   0.4470000   0.1610594
```

Instrumenting the Treatment

```
# IV
treat_reg <- lm(watched ~ encouraged, data = debate)
debate$fitted <- predict(treat_reg) ## !!!!!!!!!!
second_stage <- lm(changed_opinion ~ fitted, data=debate)
coef(second_stage)
```

```
## (Intercept)      fitted
##   0.3731314   0.2787494
```

IV and continuous treatments

- With continuous treatment variables, we can still use IV to estimate local effects, but the interpretation changes slightly.
- Imagine Y_{Di} is the potential outcome for an individual with a certain number of years of education. i.e.
  - Y_{5i} = potential outcome for i with 5 years of education
  - Y_{10i} = potential outcome for i with 10 years of education
- Assuming independence, exclusion, the first stage, and monotonicity:

$$\hat{\beta}_{2SLS} = \sum_{d=1}^{D} \pi_d E[Y_{d,i} - Y_{d-1,i} | D_{1i} \geq d > D_{0i}]$$

  - Where $E[Y_{d,i} - Y_{d-1,i} | D_{1i} \geq d > D_{0i}]$ is the ATE for compliers at treatment value d
  - And π_d is a weight proportional to the compliers at each treatment value

Implications

- 2SLS estimates a weighted average of the causal effects along the length of the treatment variable, *for those values where the instrument induces variation in treatment intake*
- If the instrument only causes people to attend, for example, an additional year of secondary education but not post-secondary education, our inferences are limited to the secondary example
- The LATE still only relates to compliers!

2SLS extensions – summary

1. **Multiple instruments**
   - 2SLS $\beta_2 \rightarrow$ weighted average of LATE for each instrument, with weights determined by strength of the first stage
2. **IV and covariates**
   - 2SLS $\beta_2 \rightarrow$ weighted average of covariate-specific LATEs
3. **IV and continuous treatments**
   - 2SLS $\beta_2 \rightarrow$ weighted average of the LATE that results from specific shifts in treatment intensity

Examples

Instruments in the wild

"Instruments that meet the requirements are hard to find, and it is difficult to marshal evidence demonstrating that one has been found. True instruments are rare, but the scientific literature is full of purported instruments."

–Rosenbaum, 2017, p. 278

Example I: Foreign media and regime support

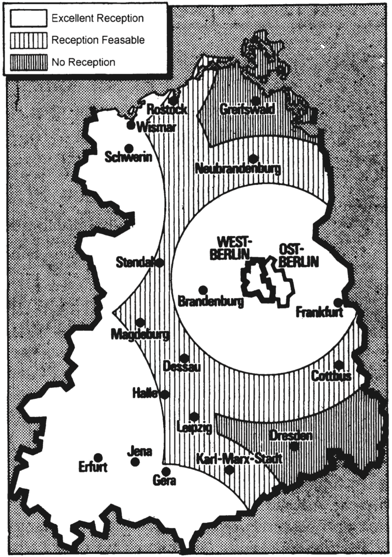
Does foreign mass media reduce support for authoritarian regimes?

Many theorists of democratization have argued that Western mass media played an important role in the fall of communism in Eastern Europe. However, we have little empirical evidence on this subject. Kern and Hainmueller (2009) use an IV design to assess whether exposure to West German TV made East German citizens less supportive of the communist regime. They study a sample of 3564 teenagers and young adults in a declassified survey run by the East German authorities.

- Outcome (Y_i): Support for the GDR (various measures)
- Treatment (D_i): 1 if unit i ever watches W. German TV
- Instrument (Z_i): 1 if unit i lives in an area with access to German TV

TV availability as an instrument

- West German TV broadcasts were not available in all parts of East Germany
- Dresden was especially cut off from Western TV
- TV signal is a function of distance and topology



LATE of TV on support for communism

Table 3 Effect of West German television exposure on regime support

Estimator	*Diff.*	*LATE*	*2SLS*	*LARF*	*2SLS*	*LARF*
Covariate set	—	—	*Limited*	*Limited*	*Extensive*	*Extensive*
Convinced of Leninist/Marxist worldview						
West German TV	−0.079	0.147	0.205	0.204	0.198	0.204
	(0.053)	(0.083)	(0.084)	(0.084)	(0.087)	(0.108)
Feel closely attached to East Germany						
West German TV	−0.013	0.217	0.258	0.255	0.256	0.251
	(0.044)	(0.067)	(0.072)	(0.075)	(0.073)	(0.090)
Political power exercised in ways consistent with my views						
West German TV	−0.014	0.158	0.193	0.191	0.186	0.185
	(0.047)	(0.078)	(0.082)	(0.083)	(0.081)	(0.106)

Identification assumptions

1. **First stage**
   - The instrument – living in Dresden – is correlated with watching West German TV
2. **Monotonicity**
   - There is no-one who would have watched West German TV had they lived in Dresden but not watched it if they had lived elsewhere
3. **Independence of the instrument**
   - Place of residence is "as good as randomly assigned' ' conditional on demographics such as age, gender, and social class
4. **Exclusion restriction**
   - The only effect of living in Dresden on political attitudes is through exposure to West German TV

How plausible are these assumptions?

1. **First stage**
   - F-statistic is about 60
2. **Monotonicity**
   - Seems very unlikely that defiers exist
3. **Independence of the instrument**
   - Residential sorting? People who want to watch West German TV might move away. This might also correlate with regime support.
   - Empirically, however, the authors show that there is very little evidence of residential movement in this period

4. **Exclusion restriction**
   - The authors show that Dresden is similar to other regions on *observed* covariates
   - However, people living in Dresden may have a multitude of different experiences compared to other regions, any of which might predict regime support
   - E.g. housing standards were, on average, worse in Dresden.

Example II: Institutions and growth

Do institutions cause growth?

Does institutional development – in terms of property rights and protection from expropriation by the state – cause higher levels of economic growth? This is a central question in political economy, and many papers have been devoted to the correlation between property rights and GDP. Whether these relationships are causal, however, is unclear because as it is quite possible that rich economies are able to afford or choose to implement better institutions. In a famous paper, Acemoglu et al (2001) assess this question using IV regression.

- Outcome (Y_i): GDP per captia, 1995 (logged)
- Treatment (D_i): Protection Against Expropriation risk, 1985 (expert survey, proxy for institutional quality)
- Instrument (Z_i): Settler mortality (death rate of settlers in early 1800s, logged)

IV justification

The justification of the settler-mortality IV is broadly as follows:

- Some colonized countries were marked by diseases – particularly yellow fever and malaria – that were lethal to Europeans
- Europeans were less likely to construct settlements in those countries
- When Europeans did not settle, they invested far less in institution building and focused on extracting resources as efficiently as possible
  - Settler colonies $\rightarrow$ representative institutions; private property rights; legal protections; capitalistic trade
  - Non-settler colonies $\rightarrow$ state administered monopolies; high tax rates; authoritarian and absolutist state power
- These institutions persisted over time, and are still reflected in modern institutional structures
- Places with weak institutions have lower levels of GDP than places with strong institutions

Intention to Treat effect

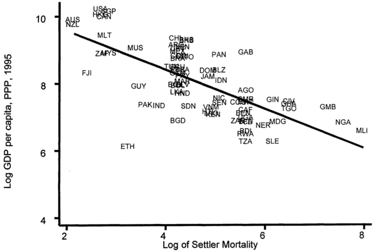


FIGURE 1. REDUCED-FORM RELATIONSHIP BETWEEN INCOME AND SETTLER MORTALITY

First stage effect

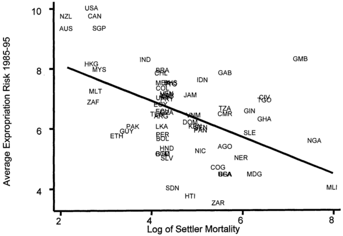


FIGURE 3. FIRST-STAGE RELATIONSHIP BETWEEN SETTLER MORTALITY AND EXPROPRIATION RISK

LATE of institutional quality on economic growth

TABLE 4—IV REGRESSIONS OF LOG GDP PER CAPITA

	Base sample (1)	Base sample (2)	Base sample without Neo-Europes (3)	Base sample without Neo-Europes (4)	Base sample without Africa (5)	Base sample without Africa (6)	Base sample with continent dummies (7)	Base sample with continent dummies (8)	Base sample, dependent variable is log output per worker (9)
	Panel A: Two-Stage Least Squares								
Average protection against expropriation risk 1985–1995	0.94 (0.16)	1.00 (0.22)	1.28 (0.36)	1.21 (0.35)	0.58 (0.10)	0.58 (0.12)	0.98 (0.30)	1.10 (0.46)	0.98 (0.17)
Latitude		−0.65 (1.34)		0.94 (1.46)		0.04 (0.84)		−1.20 (1.8)	
Asia dummy							−0.92 (0.40)	−1.10 (0.52)	
Africa dummy							−0.46 (0.36)	−0.44 (0.42)	
"Other" continent dummy							−0.94 (0.85)	−0.99 (1.0)	

Exclusion and independence

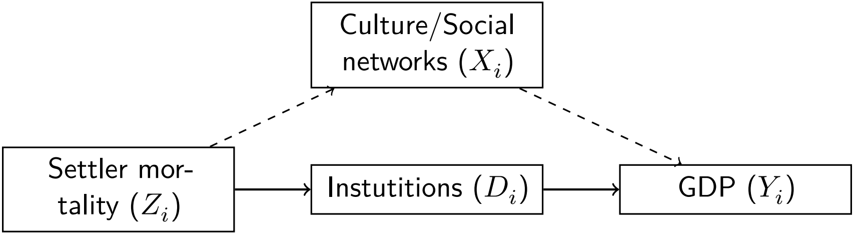
> *"The exclusion restriction implied by our instrumental variable regression is that, conditional on the controls included in the regression, the mortality rates of European settlers more than 100 years ago have no effect on GDP per capita today, other than their effect through instututional development."*
>
> *–Acemoglu et al, p. 1372*

Questions

- Do you think the instrument is as good as randomly assigned?
- Do you think the exclusion restriction holds here?

Exclusion and independence

European settlements also brought language change, culture change, and different social networks, which may also persist over time:



If these factors influence GDP, then the exclusion restriction is not valid.

Instruments, instruments, instruments...

Study	**Outcome (Y_i)**	**Treatment (D_i)**	**Instrument (Z_i)**
Newhouse and Mclellan (1998)	Health	Heart attack treatment	Distance from hospital
Angrist (1990)	Income	Military service	Vietnam draft number
Angrist et al (2010)	Education	Family size	Twins and same-sex balance
Dinas et al (2018)	Far-right support	Refugees	Distance from Turkish coast
Acemoglu et al (2001)	GDP	Instutitions	Settler mortality
Levitt (1997)	Crime rates	Number of police	Mayoral elections
Kern and Hainmueller (2009)	Communist support	Watch western TV	Live in Dresden
Madestam et al (2013)	Election outcomes	Political protest	Rainfall

Conclusion

- For observational IV design to be credible, you have to convince us of two key assumptions **(both of which are untestable)**:
  - **Independence** – your instrument Z_i is independent, i.e. conditional on covariates the instrument is as good as randomly assigned
  - **Exclusion restriction** – the only effect of your instrument on Y_i is through the treatment D_i
- In an experiment:
  - I'll grant you independence by assumption
  - I will think very hard about the exclusion restriction
- In an observational study:
  - It's harder to convince the reader that the reduced form is identified
  - It's **much** harder to convince the reader that Z_i only affects Y_i through Z_i