Stratification, regression, etc.

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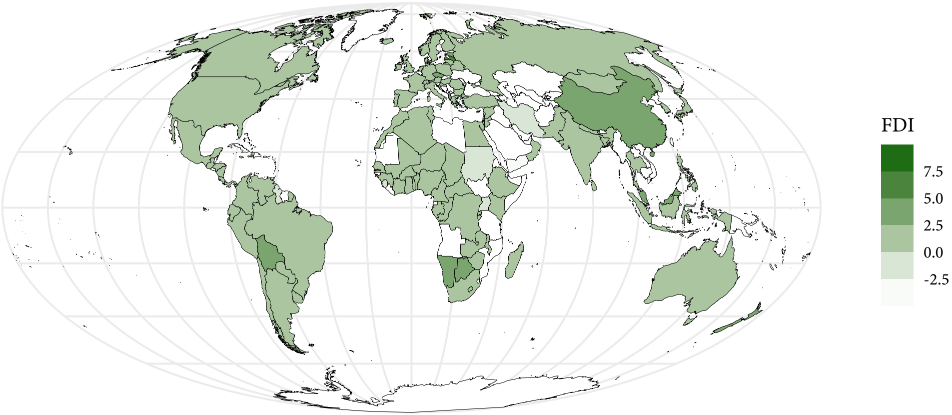
Last week

Last we thought about external validity and randomized controlled trials

This week we'll think about the observational studies and ask three questions:

- → Can I get a representative estimate from a representative sample?
- → What should I control for? (Number 10 will surprise you)
- → And how should I interpret the results?

Worldwide FDI



Foreign direct investment

Does being more democratic lead to more FDI?

According to Jensen (2003), yes. We'll look at a (simplified) version of his time series cross-sectional analysis

- → Worldwide (but with some missingness)
- → 20 years of annual data: 1975-1995
- → Regime on a 20 point scale (higher, more democratic)
- → Controls for lots of potential confounders, predictors of FDI, and country fixed effects
- → tl;dr small but significant positive effect of regime time on FDI

Foreign direct investment

Regime type is a continuous treatment, so we'll think of the causal effect of regime type as

→ the difference in expected FDI for an exogenous one unit increase in regime measure

Substantive question:

→ Do we think the effect of regime is the same on FDI everywhere?

If not, we expect *heterogenous treatment effects*

→ though we can still hope that regression will give us an ATE

Theory: Best case scenario

Possibly heterogenous additive treatment effects τ_i and sufficient covariates X:

$$\begin{aligned} Y_i^1 &= Y_i^0 + \tau_i X_i \\ (Y^0, \tau) &\perp\!\!\!\perp X \mid Z \end{aligned}$$

The average treatment effect is

$$\text{ATE} = \text{E}[\tau_i] = \text{E}[Y_i^1 - Y_i^0]$$

Jensen fits his favourite OLS regression model, controlling for all the Zs

$$Y_i = \beta_0 + X_i\beta_X + Z_i\beta_Z + \epsilon_i$$

What are we estimating with β_X with OLS?

Constant treatment effect

If $\tau_i = \tau$ then

$$\text{ATE} = \beta_X$$

Just as we hoped.

If $\tau_i \neq \tau$, that is: the treatment effects vary by case, then it's more interesting.

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Consider a different regression, predicting X using Z

$$X_i = \gamma_0 + Z_i \gamma_Z + \eta$$

If X is binary then $\mathrm{E}[X \mid Z] = p(Z)$ is the *propensity score*.

Hitting the weights

Now define a weight $w(Z_i)$ for each case

$$w(Z_i) = (X_i - \mathrm{E}[X_i \mid Z_i])^2$$

This weight is large when it's *unpredictable from the covariates* what the treatment status of the case will be, e.g.

→ If treatment is binary, it's largest when $w(Z_i) = 0.5$

→ For Jensen, it's when the residuals are large

The expected (average) value of this weight is the *variance* of treatment assignment given Z.

$$\mathrm{E}[w(Z_i)] = \mathrm{Var}[X_i \mid Z_i]$$

Finally, notice that the weights have nothing to do with outcomes.

Hitting the weights

Why would we care about these weights?

Because we can show (Aronow & Samii, 2016) that the regression coefficient we would like to be the ATE is

$$\beta = \frac{\mathrm{E}[w(Z_i)\tau_i]}{\mathrm{E}[w(Z_i)]}$$

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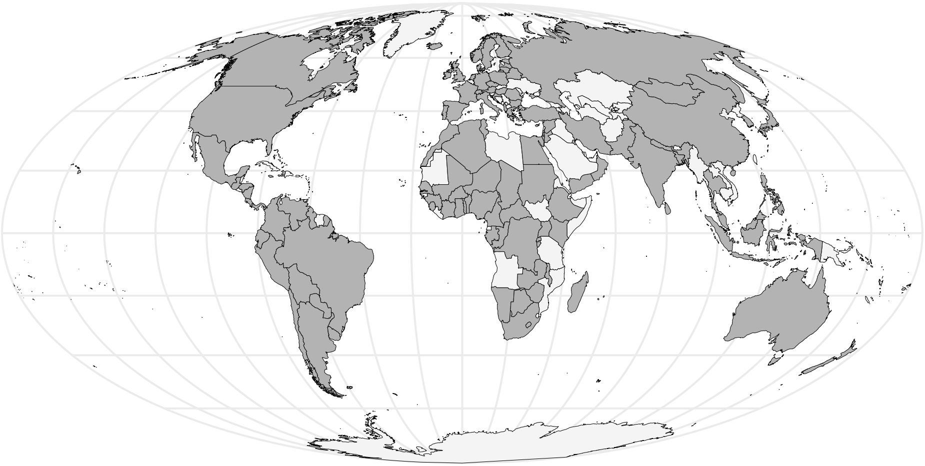
For randomized experiments, we may not care much because

$$\beta = \frac{\mathrm{E}[w(Z_i)\tau_i]}{\mathrm{E}[w(Z_i)]} = \frac{\mathrm{E}[w(Z_i)]\mathrm{E}[\tau_i]}{\mathrm{E}[w(Z_i)]} = \mathrm{E}[\tau_i]$$

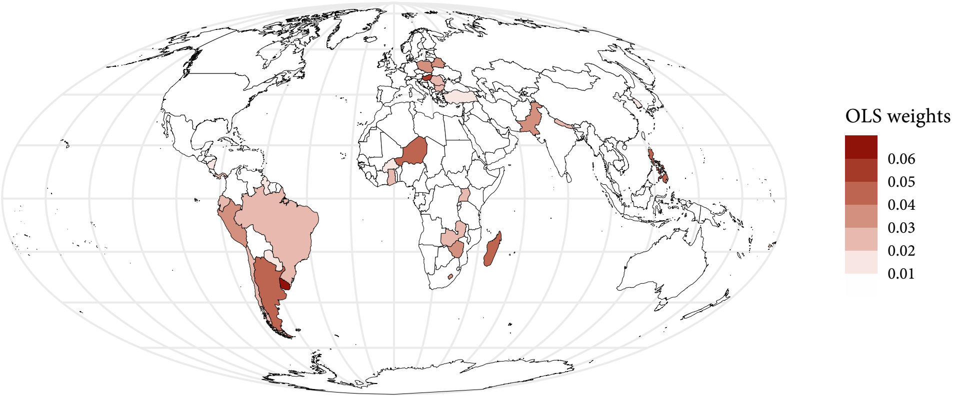
But for *observational studies* with varying treatment effects

→ Some cases matter a lot more than others

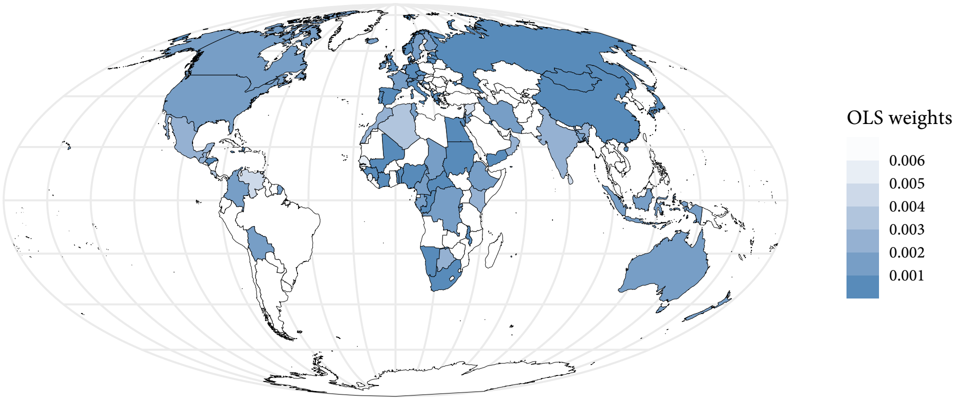
Nominal country weights



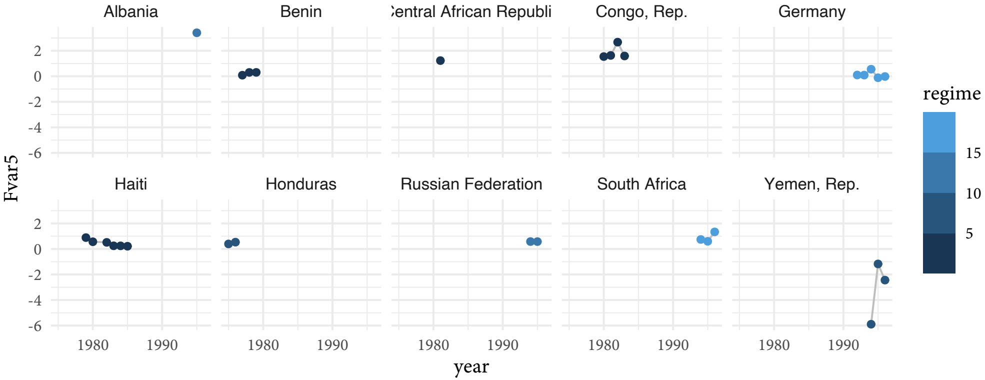
Higher country weights



Lower country weights



Lowest weighted countries



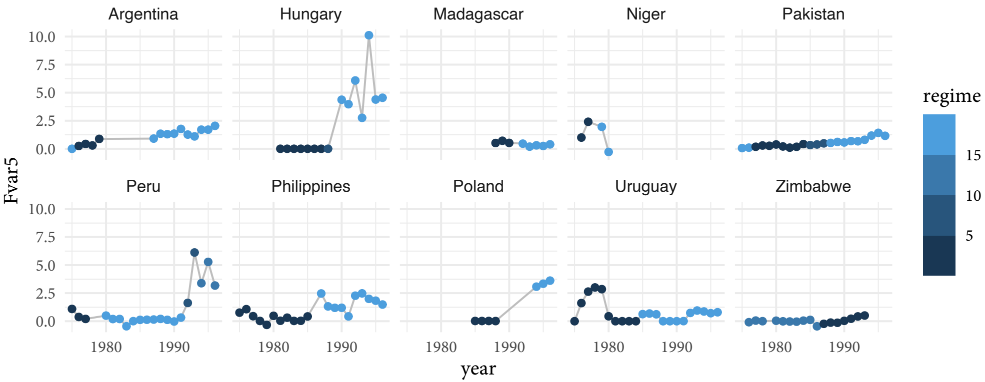
Lowest weighted countries

Countries that have few observations

- → Not much opportunity for variation
- → Well predicted by fixed effects, e.g. Germany

We have to *hope* that the treatment effects here are negligible (or the same as elsewhere)

Highest weighted countries



Highest weighted countries

Countries that have plenty of observations

→ Variation in regime and FDI

→ Badly predicted by country fixed effects

In this period there is a steady upward (democratic) trend in regime measures. Also in FDI…

→ We might want to worry about that. But not now and not here.

Modesty about regression

It *feels* like regression on representative samples ought to get us externally valid results…

- → Alas, not necessarily

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> *Estimates from a randomized experiment are only directly informative for the subpopulation whose treatment status can be manipulated by the investigator.*
>
> *Estimates from an observational study can only be directly informative for the subpopulation that exhibits some unpredictability in their treatment status after accounting for control variables.*
>
> *(Aronow & Samii, 2016)*

Alternatives

So, would this weighting happen if I used matching instead?

No, but you'd get a *different* weighting.

- → Regression: averaged over Z with weights proportional to treatment variance given the control variables
- → Matching, e.g. for the ATT: averaged over Z with weights proportional to the probability of being treated at that level of Z
- → Case less likely to be treated? Smaller weight

Matching, Regression, etc.

And back in Germany: No variation, so *zero* weight

→ As far as regression can tell, there is no possibility of regime change in Germany (because there's none in the data)

→ Assumption: *Positivity* fails / the counterfactual does not exist

More detailed comparison in ch.3.3 of Angrist and Pischke (2008) and a slightly more general framework in Hirano et al. (2003)

Control

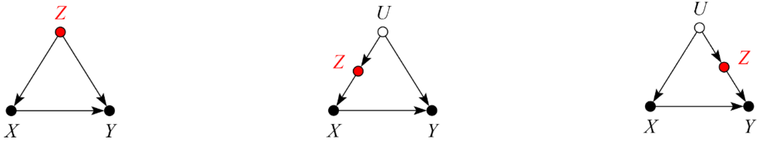
Cinelli et al. (2020) offer a typology of good and bad controls

→ Let's take a quick look

Much of this you already know, some perhaps not…

Control for confounding

Models 1, 2 and 3 – Good Controls

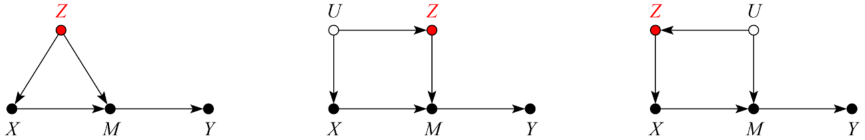


(a) Model 1 (b) Model 2 (c) Model 3

Figure 1: Models 1, 2 , and 3.

Mediators too!

Models 4, 5 and 6 – Good Controls



(a) Model 4 (b) Model 5 (c) Model 6

Figure 2: Models 4, 5 and 6.

M-BIAS

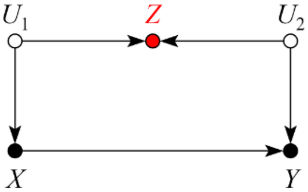


Figure 3: Model 7

Do it for the precision

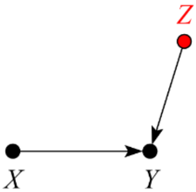


Figure 4: Model 8

OR THE IMPRECISION...

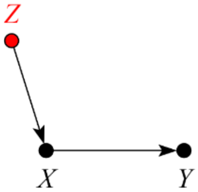


Figure 5: Model 9

Don't select on the dependent variable

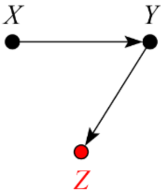


Figure 11: Model 17

OR ANY COLLIDERS

(a) Model 11 (b) Model 12

Figure 7: Models 11 and 12

OR AN INSTRUMENT

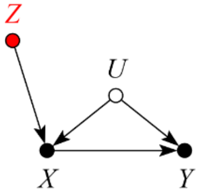


Figure 6: Model 10

OR AN INSTRUMENT

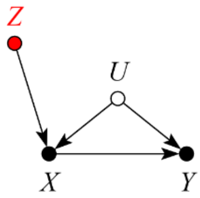


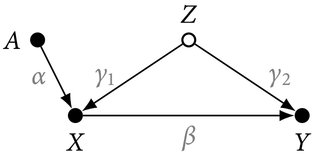
Figure 6: Model 10

Wait, what? How could this be bad?

→ It can make confounding *worse* (Middleton et al., 2016; Wooldridge, 2016)

Bias amplification

Here's a linear version with each effect marked (Pearl, 2010)



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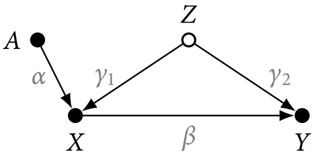

Compare the causal effect to some observational quantities

$$\tau = \mathrm{E}[Y^{X=1} - Y^{X=0}] = \beta$$

$$\tau_{\text{naive}} = \mathrm{E}[Y \mid X = 1] - \mathrm{E}[Y \mid X = 0] = \beta + \gamma_1\gamma_2$$

$$\tau_{\mathrm{A}} = \mathrm{E}[Y \mid X = 1, A] - \mathrm{E}[Y \mid X = 0, A] = \beta + \frac{\gamma_1\gamma_2}{1 - \alpha^2}$$

Control variables

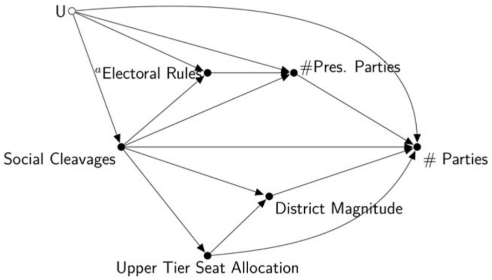

Figure 2. One possible DAG for the effective number of legislative parties. [a] Electoral rules for presidential elections.

model:

$$\begin{aligned} \text{ENPV} = \beta_0 + \beta_1 \text{Upper} + \beta_2 \text{Dist} \times \text{Mag} + \beta_3 S \times \text{Cleavages} + \beta_4 E \times \text{Rules} \\ + \beta_5 P \times \text{Parties} \end{aligned} \tag{1}$$

Which of these coefficients is causally interpretable? (Keele et al., 2020)

Control variables

Public service announcement:

→ You can't generally interpret the coefficients of control variables causally

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Public service announcement:

- → You can't generally interpret the coefficients of control variables causally

It's a very popular thing to do (Hünermund & Louw, 2020), but

- → 'It is worth noting the results of our control variables' (It's not)
- → *Nobody cares* about their signs being 'in the expected direction'

Our three questions

Can I get a representative estimate from a representative sample?

→ Sometimes? But now you know what it does behind the curtain

What should I control for in my research?

→ Now we've got a list

And how should I interpret the results (of controlling for things)

→ You shouldn't.

See, causal inference makes some things easier…

References

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