

Week 6: Instrumental Variables

PLSC 30600 - Causal Inference

Last two weeks

- Identification under **conditional ignorability**
 - Treatment assignment is independent of the potential outcomes given observed confounders **X**
 - "Selection-on-observables"
- "Selection-on-observables" isn't a testable assumption
 - Relies on theory to decide which **X** to include.
 - DAGs can help here.
- Lots of estimation strategies
 - Stratify with low-dimensional **X**
 - IPTW to eliminate treatment-covariate relationship, regression to model the outcome-covariate relationship.
 - Matching to reduce model dependence.
 - Or consider more modern flexible modelling techniques for $E[Y_i(d)|X_i]$

This week

- Can we estimate a treatment effect when neither ignorability nor conditional ignorability hold for treatment?
 - Can we get rid of *unobserved* confounding?
- "Instrumental variables" designs are one way of dealing with this
- We can identify *some* average of treatment effects if...
 - There *does* exist an ignorable or conditionally ignorable **instrument** which...
 - ...has a monotonic effect on the treatment...
 - ...and has no effect on the outcome *except* through its effect on the treatment.
- What's the average? The "Local Average Treatment Effect"
 - Average effect among those who are *moved* to take treatment by the instrument

Instrumental Variables

Treatment non-compliance

- Often experiments suffer from treatment **non-compliance**
 - Participants randomized to receive a phone call don't pick up.
 - Participants randomized to wear surgical masks choose not to.
- New notation!
 - Let Z_i denote whether i is assigned to receive a treatment.
 - Let D_i denote the treatment actually *taken* by an individual.
- Can we just take the simple difference-in-means between $D_i = 1$ and $D_i = 0$?
 - No! Non-compliance affected by other factors which might also affect the outcome.
 - We're stuck with an observational design.

- Unless...

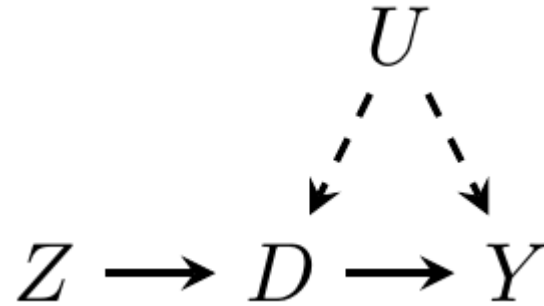
Intent-to-treat effect

- We can first just change the question - instead of the effect of **treatment**, we can make our estimand the effect of being **assigned to treatment**.
- Our estimator for the ITT is just the difference in means between the $Z_i = 1$ and $Z_i = 0$ arms

$$\hat{\tau}_{\text{ITT}} = \hat{E}[Y_i | Z_i = 1] - \hat{E}[Y_i | Z_i = 0]$$

- Identified under randomization of Z_i even if D_i is not randomized.
 - But combines two effects: the actual effect of D_i and the effect of Z_i on D_i .

Instrumental variables



- Suppose though that we're interested in the *actual* effect of receiving treatment (the effect of D_i). What can we do?

Instrumental variables

- Start by writing down potential outcomes for D_i along with joint potential outcomes of Y_i in terms of Z_i and D_i

$$D_i(z) = D_i \text{ if } Z_i = z$$

$$Y_i(d, z) = Y_i \text{ if } D_i = d, Z_i = z$$

- Observed treatment D_i is a function of treatment assignment (Z_i) - it's a post-treatment quantity (and so has potential outcomes).

Assumptions

1. Randomization of instrument
2. Exclusion restriction
3. Non-zero first-stage relationship
4. Monotonicity

Assumption 1: Randomization

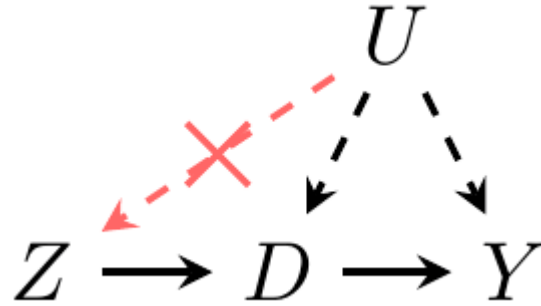
- Z_i is independent of both sets of potential outcomes (potential outcomes for the treatment and potential outcomes for the outcome).

$$\{D_i(1), D_i(0)\} \perp\!\!\!\perp Z_i$$

$$\{Y_i(d, z) \forall d, z\} \perp\!\!\!\perp Z_i$$

- We can weaken this to conditional ignorability (where Z_i is randomized conditional on X_i), which is common in observational settings.
 - But if we don't believe conditional ignorability for the treatment, why would we believe it for the instrument?
- Sufficient to identify the **intent-to-treat (ITT)** effect

Assumption 1: Randomization



- The randomization assumption eliminates any arrows from U to Z .

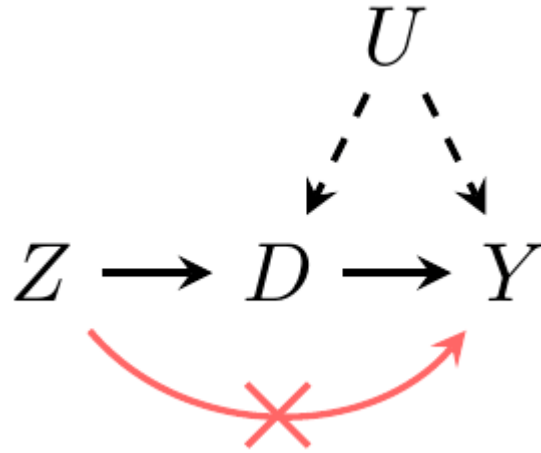
Assumption 2: Exclusion restriction

- Z_i **only affects** Y_i by way of its effect on D_i .
- In other words, if D_i were set at some level d , the potential outcome for $Y_i(d, z)$ does not depend on z .

$$Y_i(d, z) = Y_i(d, z') \text{ for any } z \neq z'$$

- **Not a testable assumption!** -- we have to justify this with substantive knowledge.
 - Easiest in the treatment non-compliance case
 - But consider what might happen in a non-blinded situation where respondents knew their treatment assignments.
- "Surprise" factor -- If I told you Z was associated with Y , would you think "that's odd"?

Assumption 2: Exclusion restriction



- The exclusion restriction eliminates any causal paths from Z to Y **except** for $Z \rightarrow D \rightarrow Y$.

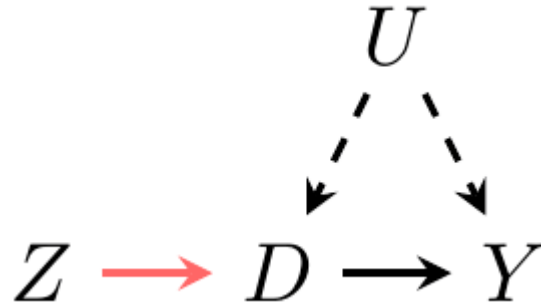
Assumption 3: Non-zero first stage

- Z_i has an effect on D_i

$$E[D_i(1) - D_i(0)] \neq 0$$

- Seems trivial, but we need this to make the estimator work.
- Magnitude matters for estimator performance - a "weak" first-stage \rightsquigarrow heavily biased IV estimator
 - IV estimators are *consistent* but not *unbiased*.

Assumption 3: Non-zero first stage



- The non-zero first stage assumption requires a path from Z to D .

Assumption 4: Monotonicity

- Z_i 's effect on D_i only goes in one direction **at the individual level**

$$D_i(1) - D_i(0) \geq 0$$

- If it goes the other way, we can always flip the direction of the treatment to make this hold
 - The key is that the instrument does not have a positive effect on D_i for some units and a negative effect for others.
- **Not a testable assumption**

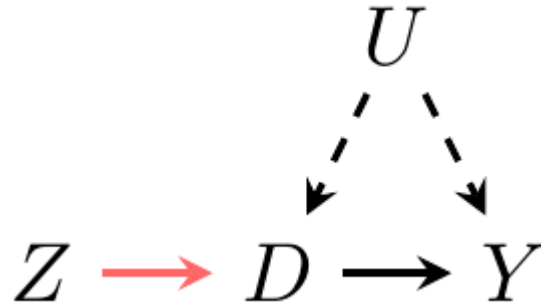
Assumption 4: Monotonicity

- In binary instrument/binary treatment world, this is sometimes called a "no defiers" assumption.

Stratum	$D_i(1)$	$D_i(0)$
"Always-takers"	1	1
"Never-takers"	0	0
"Compliers"	1	0
"Defiers"	0	1

- Under no defiers, every unit with $D_i = 1$ and $Z_i = 0$ is an always-taker, every unit with $D_i = 0$ and $Z_i = 1$ is a never-taker.

Assumption 4: Monotonicity



- Can't represent the monotonicity assumption in a DAG - it's an assumption about the form of the relationship between Z and D .

Interpreting the IV estimand

- The classic IV estimand with one instrument is a ratio of sample covariances.

$$\tau_{IV} = \frac{Cov(Y, Z)}{Cov(D, Z)}$$

- With a binary instrument, this is sometimes called the "Wald" estimand - a ratio of differences in means

$$\tau_{IV} = \frac{E[Y_i | Z_i = 1] - E[Y_i | Z_i = 0]}{E[D_i | Z_i = 1] - E[D_i | Z_i = 0]}$$

Interpreting the IV estimand

- What does the Wald estimand correspond to in terms of causal effects?

$$\tau_{IV} = \frac{E[Y_i|Z_i = 1] - E[Y_i|Z_i = 0]}{E[D_i|Z_i = 1] - E[D_i|Z_i = 0]}$$

- Under our identification assumptions:
 - The numerator is the ITT
 - The denominator is the first-stage effect

Interpreting the IV estimand

- Let's decompose the denominator first - under randomization:

$$\begin{aligned}E[D_i|Z_i = 1] - E[D_i|Z_i = 0] &= E[D_i(1)|Z_i = 1] - E[D_i(0)|Z_i = 0] \\&= E[D_i(1)] - E[D_i(0)] \\&= E[D_i(1) - D_i(0)]\end{aligned}$$

- With binary treatment/binary instrument, we can use law of total expectation to decompose by principal stratum

$$\begin{aligned}E[D_i(1) - D_i(0)] &= E[D_i(1) - D_i(0)|D_i(1) = D_i(0)] \times P(D_i(1) = D_i(0)) + \\&\quad E[D_i(1) - D_i(0)|D_i(1) > D_i(0)] \times P(D_i(1) > D_i(0)) + \\&\quad E[D_i(1) - D_i(0)|D_i(1) < D_i(0)] \times P(D_i(1) < D_i(0))\end{aligned}$$

- The first term is 0
- And by no defiers, the last term is 0 since $P(D_i(1) < D_i(0)) = 0$

$$E[D_i(1) - D_i(0)] = Pr(D_i(1) > D_i(0))$$

Interpreting the IV estimand

- Next, the numerator (the ITT). Under the exclusion restriction and randomization:

$$E[Y_i|Z_i = 1] = E\left[Y_i(0) + \left(Y_i(1) - Y_i(0)\right)D_i(1)\right]$$

$$E[Y_i|Z_i = 0] = E\left[Y_i(0) + \left(Y_i(1) - Y_i(0)\right)D_i(0)\right]$$

- The difference (with some algebra) is

$$E[Y_i|Z_i = 1] - E[Y_i|Z_i = 0] = E\left[\left(Y_i(1) - Y_i(0)\right) \times \left(D_i(1) - D_i(0)\right)\right]$$

Interpreting the IV estimand

- Conditioning on the principal strata:

$$\begin{aligned} &= E\left[(Y_i(1) - Y_i(0)) \times (0) | (D_i(1) = D_i(0))\right] \times P(D_i(1) = D_i(0)) + \\ &\quad E\left[(Y_i(1) - Y_i(0)) \times (1) | (D_i(1) > D_i(0))\right] \times P(D_i(1) > D_i(0)) + \\ &\quad E\left[(Y_i(1) - Y_i(0)) \times (-1) | (D_i(1) < D_i(0))\right] \times P(D_i(1) < D_i(0)) \end{aligned}$$

- Again, first term is zero because $D_i(1) - D_i(0) = 0$, third is zero by "no defiers" and we have

$$E[Y_i | Z_i = 1] - E[Y_i | Z_i = 0] = E\left[Y_i(1) - Y_i(0) | D_i(1) > D_i(0)\right] \times P(D_i(1) > D_i(0))$$

- The ITT is the product of a conditional average treatment effect and the proportion of compliers.

The LATE Theorem

- The IV estimand, under our identification assumptions, is a Local Average Treatment Effect (LATE):

$$\frac{E[Y_i|Z_i = 1] - E[Y_i|Z_i = 0]}{E[D_i|Z_i = 1] - E[D_i|Z_i = 0]} = E[Y_i(1) - Y_i(0)|D_i(1) > D_i(0)]$$

- The LATE is a conditional average treatment effect within the *subpopulation* of **compliers**
- If treatment effects are constant, we can generalize this to the whole sample.
 - But if effects are heterogeneous, we are not necessarily getting a "representative" treatment effect.

Better LATE than never?

- How should we interpret the LATE?
 - It's not necessarily the quantity we care about - we care about the effect of the treatment in the entire sample.
- Compliers are those compelled to take treatment by our encouragement. Would estimates generalize to those who are less encourageable?
 - The LATE is design-specific. If we came up with a different instrument, that changes the population on which we're estimating an effect!
 - What can we do?
 - We could describe the distribution of covariates among compliers vs. the population as a whole (Abadie's kappa-weighting).

Example: The effect of media on voting

- Gerber, Karlan and Bergan (2009, AEJ:AE) estimate the effect of reading the Washington Post (or Washington Times) on political attitudes and voting behavior.
 - Z_i : Random assignment to receive a free subscription to the Washington Post
 - D_i : Actually subscribing to the Washington Post (as measured by a post-encouragement survey)
 - Y_i : 2005 Turnout (measured in the survey)
- Assumptions:
 - Assignment to get the free subscription offer is ignorable/exogenous
 - Getting the free subscription offer affects actual subscriptions (non-zero first stage)
 - No one would subscribe to the Post if they *didn't* receive the offer but not subscribe if they *did*. (monotonicity/no defiers)
 - Assignment to get the free subscription offer doesn't affect voting *except through* actually subscribing to the Post (exclusion restriction)

Example: The effect of media on voting

- First, subset the data to WaPo or control observations that completed the follow-up survey

```
green <- read_dta("assets/publicdata.dta")  
wapost <- green %>% filter(treatment != "TIMES"&!is.na(getpost)&!is.na(voted))
```

Example: The effect of media on voting

- Is there a first-stage effect?

```
lm_robust(getpost ~ post, data=wapost)
```

##	Estimate	Std. Error	t value	Pr(> t)	CI Lower	CI Upper	DF
## (Intercept)	0.203	0.0189	10.73	4.06e-25	0.166	0.240	760
## post	0.341	0.0341	9.99	3.82e-22	0.274	0.408	760

- About 34 percent of the sample is a "complier" - quite substantial!

Example: The effect of media on voting

- Is there an ITT?

```
lm_robust(voted ~ post, data=wapost)
```

##	Estimate	Std. Error	t value	Pr(> t)	CI Lower	CI Upper	DF
## (Intercept)	0.72627	0.021	34.6303	1.91e-158	0.6851	0.7674	760
## post	-0.00135	0.033	-0.0409	9.67e-01	-0.0661	0.0634	760

- ITT is essentially zero.

Example: The effect of media on voting

- Compare with the naive OLS estimate

```
lm_robust(voted ~ getpost, data=wapost)
```

##	Estimate	Std. Error	t value	Pr(> t)	CI Lower	CI Upper	DF
## (Intercept)	0.703	0.0204	34.45	2.11e-157	0.663119	0.743	760
## getpost	0.066	0.0332	1.99	4.70e-02	0.000877	0.131	760

- Post subscribers are 6pp more likely to vote in the 2005 VA gubernatorial election.
 - But is this causal? No!

Example: The effect of media on voting

- Let's estimate the LATE using the Wald estimator

```
(mean(wapost$voted[wapost$post == 1]) - mean(wapost$voted[wapost$post == 0]))/(mean(wapost$getp
```

```
## [1] -0.00396
```

- Equivalent to a ratio of regression coefficients

```
coef(lm_robust(voted ~ post, data=wapost))[2]/coef(lm_robust(getpost ~ post, data=wapost))[2]
```

```
##      post  
## -0.00396
```


Example: The effect of media on voting

- We'll talk about inference later, but take note: the SE for the LATE can be much larger than the SE for the ITT

```
iv_robust(voted ~ getpost | post, data=wapost)
```

##	Estimate	Std. Error	t value	Pr(> t)	CI Lower	CI Upper	DF
## (Intercept)	0.72707	0.0368	19.7765	1.51e-70	0.655	0.799	760
## getpost	-0.00396	0.0968	-0.0409	9.67e-01	-0.194	0.186	760

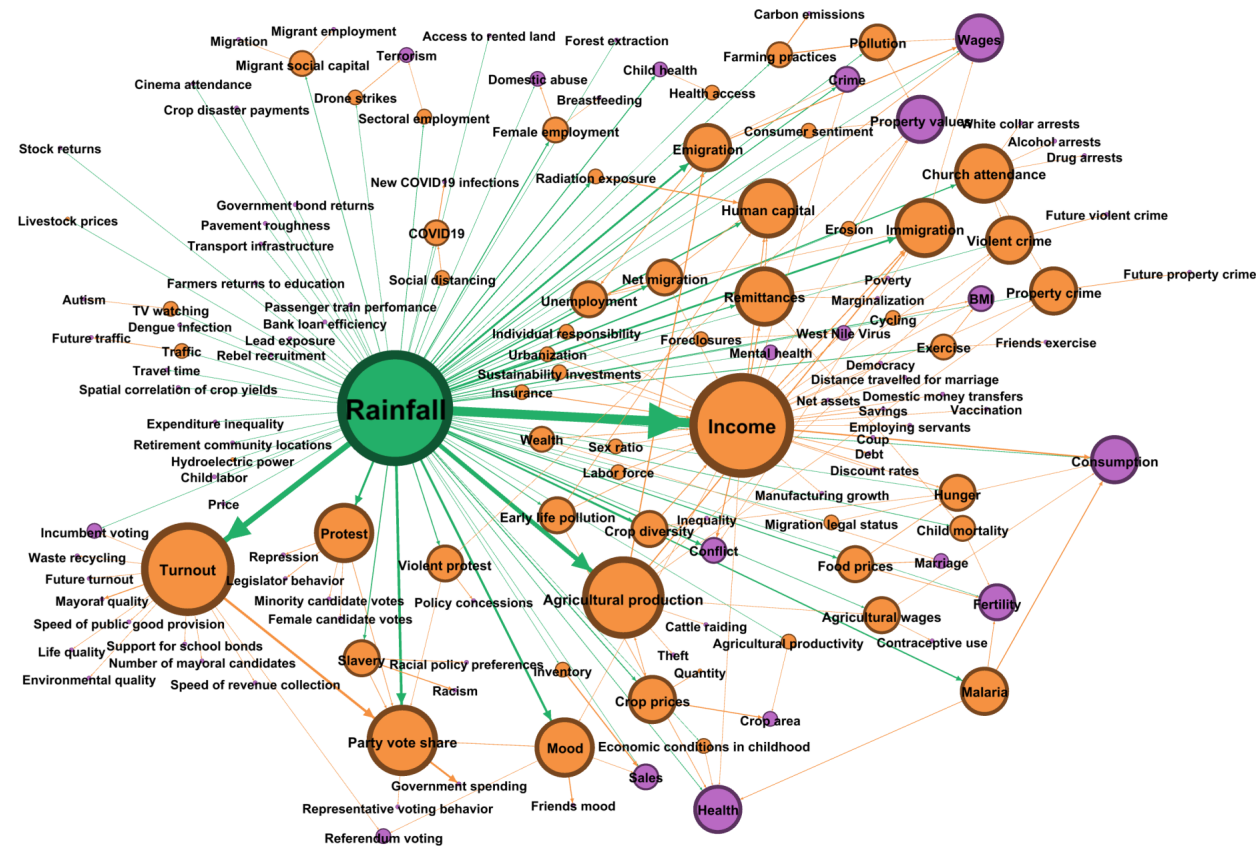
IV in observational studies

- Most applications of IV are not treatment non-compliance.
- But all follow the same underlying logic.
 - Treatment of interest is not randomized...but there exists a real or "natural" experiment that is.
 - And this natural experiment affects the outcome only through its affect on the treatment of interest.
- Examples:
 - Angrist (1990) - Vietnam draft lottery number as an instrument for the effect of military service on income.
 - Angrist and Krueger (1991) - Birth quarter as an instrument for education's effect on income.
 - Acemoglu et. al. (2001) - European settler mortality as an instrument for the effect of institutional quality on GDP per capita.
 - Kern & Hainmueller (2009) - West German TV signal strength as an instrument for the effect of watching West German TV on support for the East German regime
- Challenges
 - Exogeneity/ignorability isn't guaranteed
 - If an instrument has a large effect on your treatment of interest, it probably has an effect on other stuff that could affect the outcome as well (violating the exclusion restriction)

Discussion: The rainfall instrument

- Miguel, Satyanth, and Sergenti (2004, JPE) look at the effect of **economic growth** on civil conflict in 41 African countries.
 - Growth and conflict are confounded (e.g. by political institutions).
 - Instrument for GDP growth using the annual change in rainfall -- for heavily agrarian countries, rainfall fluctuations determine crop yields which are a large component of GDP.
 - Observe that changes in rainfall are associated with changes in GDP and negative GDP shocks (instrumented by rainfall) increase civil conflict.
- Does this satisfy the IV identification assumptions?
 - Exogeneity? Is rainfall as-good-as randomly assigned?
 - Monotonicity? Do positive rainfall shocks *strictly* boost GDP per capita?
 - Exclusion restriction? Is rainfall's effect transmitted only through the mechanism the authors define?

Discussion: The rainfall instrument



Mellon (2023) "Rain, Rain, Go Away: 195 Potential Exclusion-Restriction Violations for Studies Using Weather as an Instrumental Variable"

Estimation and inference for IV

IV with constant effects

- Let's consider a linear model for the potential outcomes

$$Y_i(d) = \alpha + \tau d + \gamma U_i + \eta_i$$

- If we could control for U_i , we could estimate the regression to get an estimate of τ

$$Y_i = \alpha + \tau D_i + \gamma U_i + \eta_i$$

- But we can't - and regressing Y_i on D_i alone will not give a consistent estimator of τ since $Cov(\gamma U_i + \eta_i, D_i) \neq 0$

IV with constant effects

- Suppose Z_i is an instrument that is exogenous and satisfies the exclusion restriction

$$\text{Cov}(\gamma U_i + \eta_i, Z_i) = 0$$

- Then, we can identify τ

$$\begin{aligned}\text{Cov}(Y_i, Z_i) &= \text{Cov}(\alpha + \tau D_i + \gamma U_i + \eta_i, Z_i) \\ &= \text{Cov}(\alpha, Z_i) + \text{Cov}(\tau D_i, Z_i) + \text{Cov}(\gamma U_i + \eta_i, Z_i) \\ &= \tau \text{Cov}(D_i, Z_i)\end{aligned}$$

- Which gives us our IV estimand

$$\tau = \frac{\text{Cov}(Y_i, Z_i)}{\text{Cov}(D_i, Z_i)}$$

IV estimator

- We can estimate τ by plugging in the sample quantities.

$$\tau_{\text{IV}}^{\hat{}} = \frac{\widehat{\text{Cov}}(Y_i, Z_i)}{\widehat{\text{Cov}}(D_i, Z_i)}$$

- This also can be written as a ratio of two regression coefficients

$$\tau_{\text{IV}}^{\hat{}} = \frac{\widehat{\text{Cov}}(Y_i, Z_i) / \widehat{\text{Var}}(Z_i)}{\widehat{\text{Cov}}(D_i, Z_i) / \widehat{\text{Var}}(Z_i)}$$

- Denominator: "First stage": Regression of D_i on Z_i
- Numerator: "Reduced form": Regression of Y_i on Z_i

2SLS - Including covariates

- What if ignorability of Z_i only holds conditional on X_i -- or we want to include X_i as predictors to improve precision.
- We'll assume a particular structure for the outcome and treatment models

$$Y_i = X_i' \beta + \tau D_i + \epsilon_i$$

$$D_i = X_i' \alpha + \gamma Z_i + \nu_i$$

- Assume the X_i are exogenous but not excluded (appear in both equations). D_i is still endogenous so we can't get the treatment effect by just regressing outcome on treatment and covariates.
- Can we get an expression for Y_i in the form of Z_i alone?

2SLS - Including covariates

- Substitute in for D_i

$$\begin{aligned} Y_i &= X_i' \beta + \tau [X_i' \alpha + \gamma Z_i + \nu_i] + \epsilon_i \\ &= X_i' \beta + \tau [X_i' \alpha + \gamma Z_i] + [\tau \nu_i + \epsilon_i] \\ &= X_i' \beta + \tau E[D_i | X_i, Z_i] + \epsilon_i^* \end{aligned}$$

- We can identify τ by regressing Y_i on X_i and the **fitted values** from a regression of D_i on X_i and the instrument Z_i .
- **Intuition** -- we want to only use the variation in D_i that is driven by the **exogenous** factor Z_i .

2SLS - Including covariates

- You can also still get the ratio form of the IV estimator

$$\begin{aligned} Y_i &= X_i' \beta + \tau [X_i' \alpha + \gamma Z_i + \nu_i] + \epsilon_i \\ &= X_i' (\beta + \tau \alpha) + \tau \gamma Z_i + [\tau \nu_i + \epsilon_i] \end{aligned}$$

- The coefficient on Z_i in the reduced form regression is $\tau \gamma$
- The coefficient on Z_i in the first-stage is γ .
- So the ratio of the reduced form to the first-stage regression is τ .

Two-stage least squares

- **First stage** - Regress D_i on X_i and Z_i . Get the fitted values \hat{D}_i

$$\hat{D}_i = X_i' \hat{\alpha} + \hat{\gamma} Z_i$$

- **Second stage** - Regress Y_i on X_i and fitted values \hat{D}_i

$$\hat{Y}_i = X_i' \hat{\beta} + \hat{\tau} \hat{D}_i$$

- The coefficient on the fitted values is the IV estimate
 - But, the standard errors will be wrong - Why?

Illustrating 2SLS

- Recall our Gerber, Karlan and Bergan (2009, AEJ:AE) experiment
 - Z_i : Random assignment to receive a free subscription to the Washington Post
 - D_i : Actually subscribing to the Washington Post (as measured by a post-encouragement survey)
 - Y_i : 2005 Turnout (measured in the survey)
 - X_i : Gender, Age
- Let's load and subset

```
green <- read_dta("assets/publicdata.dta")
wapost <- green %>% filter(treatment != "TIMES"&!is.na(getpost)&!is.na(voted)&!is.na(Bfemale)&!
```

Illustrating 2SLS

- Our first stage regresses subscription on assignment + covariates

```
first_stage <- lm_robust(getpost ~ post + Bfemale + reportedage , data= wapost)
summary(first_stage)
```

```
##
## Call:
## lm_robust(formula = getpost ~ post + Bfemale + reportedage, data = wapost)
##
## Standard error type:  HC2
##
## Coefficients:
##              Estimate Std. Error t value Pr(>|t|)  CI Lower CI Upper  DF
## (Intercept)  0.12097    0.06406   1.889 5.94e-02 -0.004786  0.24673 729
## post         0.35233    0.03482  10.118 1.31e-22  0.283965  0.42069 729
## Bfemale      -0.00435    0.03505  -0.124 9.01e-01 -0.073156  0.06445 729
## reportedage  0.00170    0.00125   1.360 1.74e-01 -0.000756  0.00416 729
##
## Multiple R-squared:  0.134 ,    Adjusted R-squared:  0.13
## F-statistic: 35.4 on 3 and 729 DF,  p-value: <2e-16
```

Illustrating 2SLS

- Let's actually run 2SLS - I like two routines: `iv_robust` in `estimatr` (does 2SLS with robust SEs) and `ivmodel` in `ivmodel` (does robust 2SLS *and* weak-instrument robust tests + other diagnostics)

```
wapo_2sls <- iv_robust(voted ~ getpost + Bfemale + reportedage | post + Bfemale + reportedage,  
summary(wapo_2sls)
```

```
##  
## Call:  
## iv_robust(formula = voted ~ getpost + Bfemale + reportedage |  
##           post + Bfemale + reportedage, data = wapo2)  
##  
## Standard error type: HC2  
##  
## Coefficients:  
##           Estimate Std. Error t value Pr(>|t|) CI Lower CI Upper DF  
## (Intercept)  0.22562    0.07334   3.0766 2.17e-03  0.08165   0.3696 729  
## getpost      0.00428    0.09086   0.0471 9.62e-01 -0.17410   0.1827 729  
## Bfemale     -0.03495    0.03354  -1.0423 2.98e-01 -0.10079   0.0309 729  
## reportedage  0.01040    0.00127   8.1879 1.19e-15  0.00791   0.0129 729  
##  
## Multiple R-squared:  0.093 ,    Adjusted R-squared:  0.0893  
## F-statistic: 23.3 on 3 and 729 DF,  p-value: 2.15e-14
```

Illustrating 2SLS

```
wapo_2sls2 <- ivmodelFormula(voted ~ getpost + Bfemale + reportedage | post + Bfemale + report  
summary(wapo_2sls2)
```

```
##  
## Call:  
## ivmodel(Y = Y, D = D, Z = Z, X = X, intercept = intercept, beta0 = beta0,  
##       alpha = alpha, k = k, manyweakSE = manyweakSE, heteroSE = heteroSE,  
##       clusterID = clusterID, deltarange = deltarange, na.action = na.action)  
## sample size: 733  
## -----  
## First Stage Regression Result:  
##  
## F=111, df1=1, df2=729, p-value is <2e-16  
## R-squared=0.132, Adjusted R-squared=0.131  
## Residual standard error: 0.444 on 730 degrees of freedom  
## -----  
## Coefficients of k-Class Estimators:  
##  
##           k Estimate Std. Error t value Pr(>|t|)  
## OLS      0.00000  0.04708    0.03247    1.45    0.15  
## Fuller  0.99863  0.00472    0.08979    0.05    0.96  
## TSLS    1.00000  0.00428    0.09060    0.05    0.96  
## LIML    1.00000  0.00428    0.09060    0.05    0.96  
##
```


The weak instrument problem

- Our ratio estimator is consistent

$$\hat{\tau}_{IV} = \frac{\widehat{Cov}(Y_i, Z_i)}{\widehat{Cov}(D_i, Z_i)} \xrightarrow{p} \tau + \frac{Cov(U_i, Z_i)}{Cov(D_i, Z_i)}$$

- Under exogeneity $Cov(Z_i, U_i)$ is zero.
- However, when there are small violations of exogeneity, a weak instrument will amplify them.
- More generally, with a weak instrument, our t-ratio hypothesis tests assuming asymptotic normality will have **incorrect** type-1 error rates.
 - Why? Distributions of ratios are poorly behaved.

The weak instrument problem

- Let's use a simulation to see how bad the bias can be in IV versus just a simple OLS regression of outcome on treatment under unobserved confounding.
- Let $U_i \sim \mathcal{N}(0, 1)$ be an unobserved confounder. $Z_i \sim \text{Bern}(.5)$ is an **exogenous** instrument.
- The probability of treatment is modeled via a logit

$$\log\left(\frac{P(D_i = 1|Z_i, U_i)}{1 - P(D_i = 1|Z_i, U_i)}\right) = \gamma Z_i + U_i$$

- γ here captures the relationship between the exogenous instrument Z_i and the treatment
- The outcome is a function of U and a mean zero error term ϵ_i only, so the true treatment effect is 0

$$Y_i = U_i + \epsilon_i$$

The weak instrument problem

- Let's see how the Wald estimator performs when we have a pretty large effect of Z_i on D_i : $\gamma = 2$ and $N = 1000$

```
## First stage effect  
mean(firststage)
```

```
## [1] 0.345
```

```
## F-statistic from the first stage  
mean(firststageF)
```

```
## [1] 157
```

```
## Bias of the naive OLS  $Y \sim X$   
mean(naive)
```

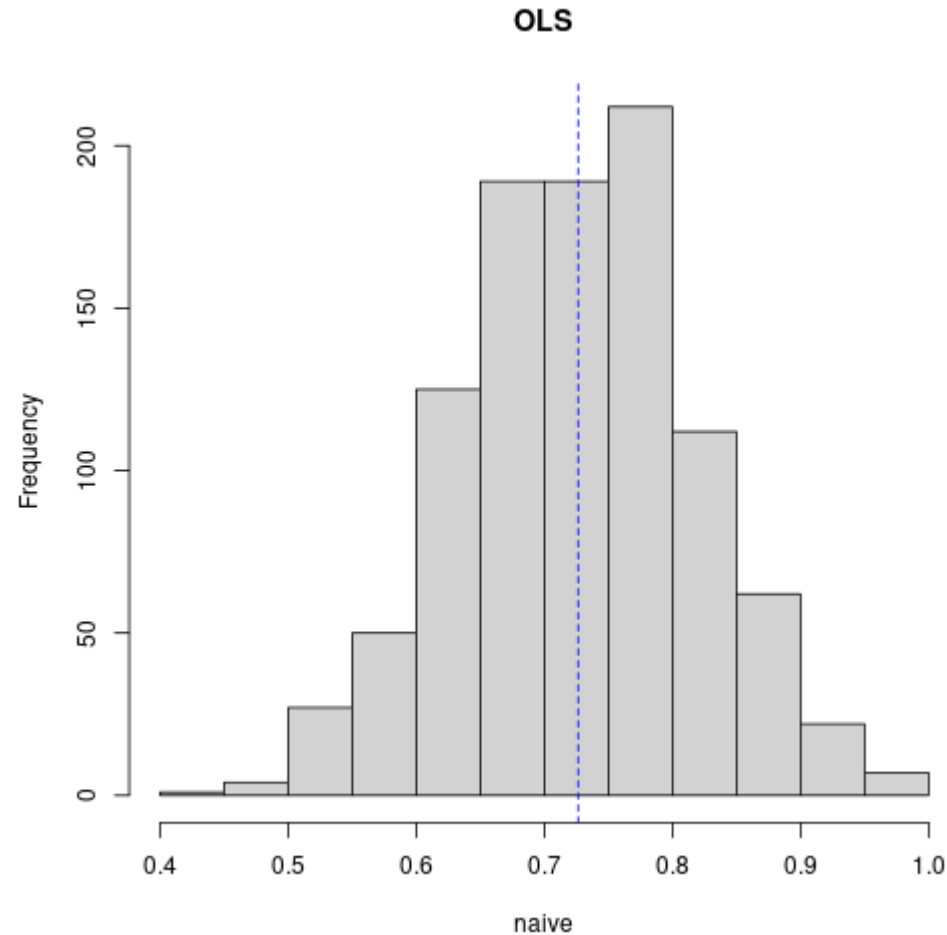
```
## [1] 0.726
```

```
## Bias of IV  
mean(IV)
```

```
## [1] -0.0196
```

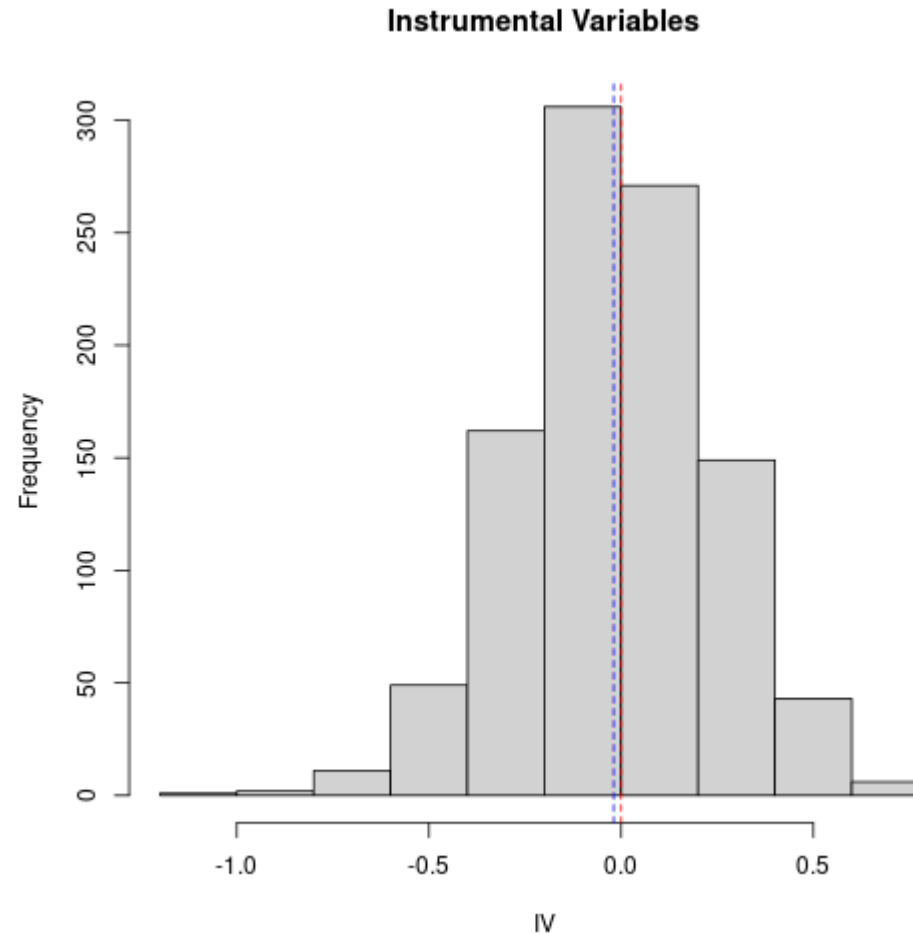
The weak instrument problem

- Sampling distribution of the naive OLS estimator



The weak instrument problem

- Sampling distribution of the IV estimator



The weak instrument problem

- Now, what happens when our instrument is weak: $\gamma = .2$ and $N = 1000$

```
## First stage effect  
mean(firststage)
```

```
## [1] 0.041
```

```
## F-statistic from the first stage  
mean(firststageF)
```

```
## [1] 2.6
```

```
## Bias of the naive OLS  $Y \sim X$   
mean(naive)
```

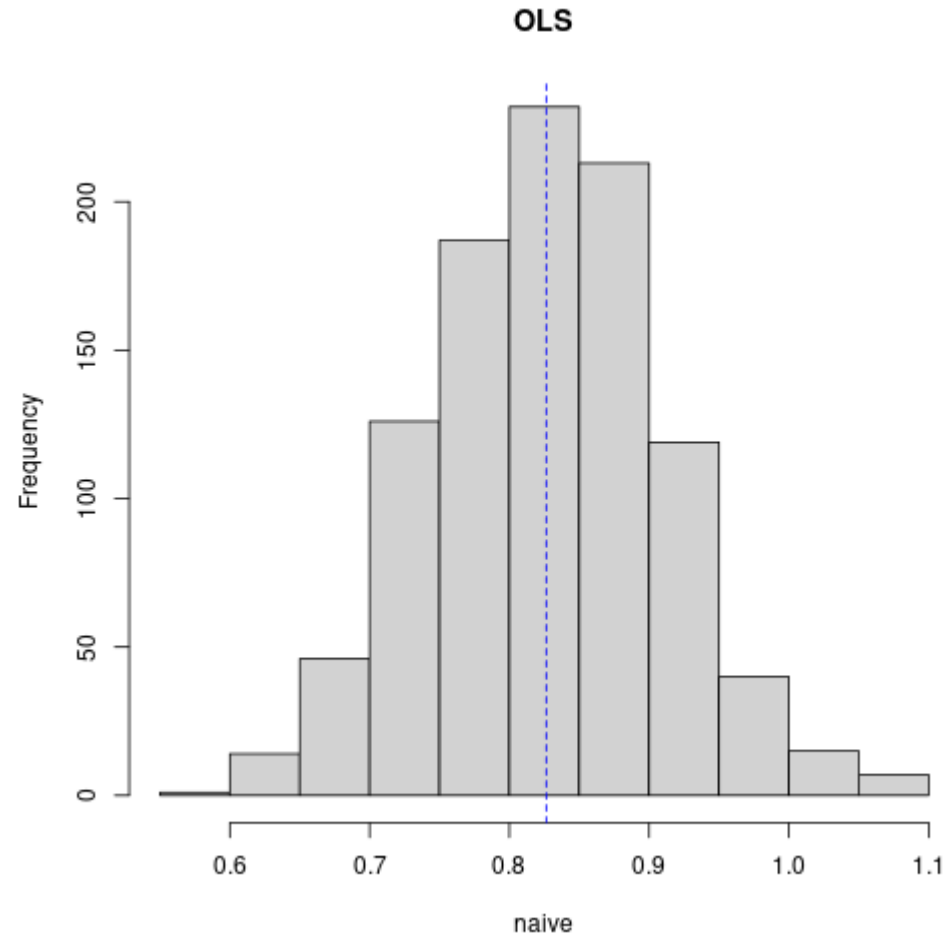
```
## [1] 0.826
```

```
## Bias of IV  
mean(IV)
```

```
## [1] -1.04
```

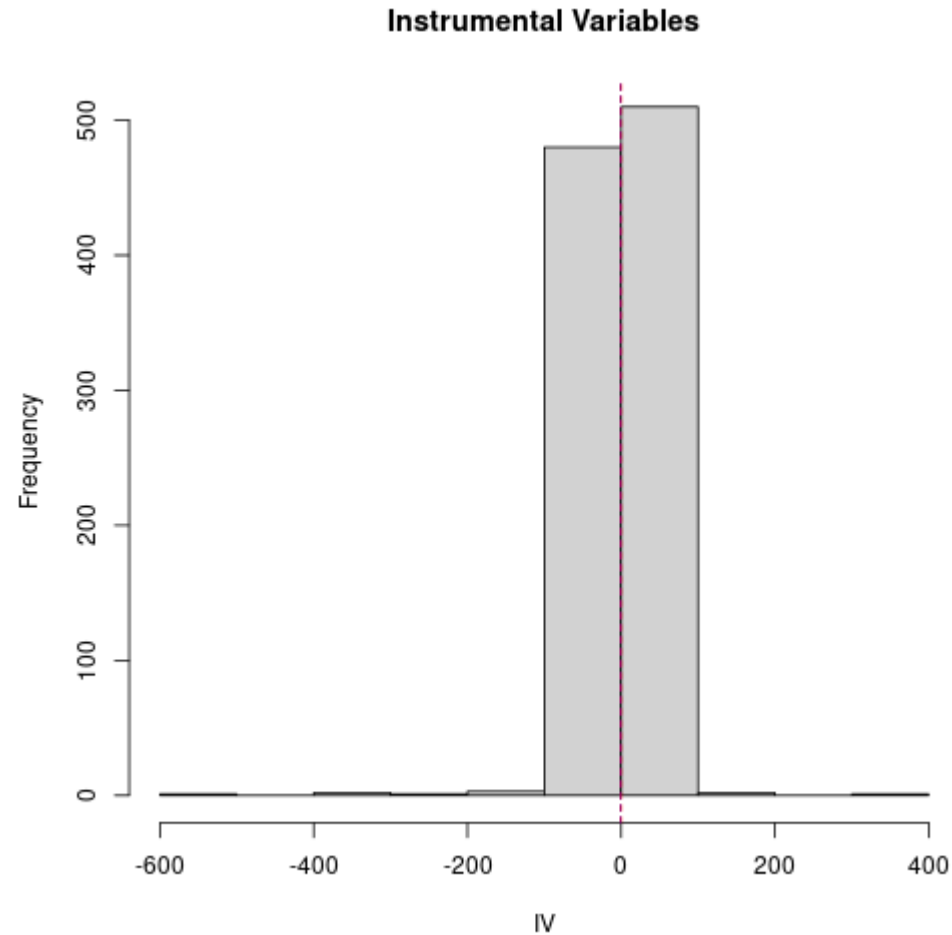
The weak instrument problem

- Sampling distribution of the naive OLS estimator



The weak instrument problem

- Sampling distribution of the IV estimator



The weak instrument problem

- When is an instrument too weak?
- **Classic result:** Staiger and Stock (1997), Stock and Yogo (2005) use first stage F-statistic thresholds
 - \rightsquigarrow heuristic of first-stage F-statistic below 10.
- **Recently:** Lee, Moreira, McCrary, Porter (2020) -- If we want to use the F-statistic as a screen then we actually need $F > 104.7$
- Suggestions:
 - Permutation tests using a test statistic that does not depend on the first stage.
 - **Anderson-Rubin (1949)** approach
 - Angrist and Pischke (2009) - with "just-identified" IV (number of instruments = number of endogenous variables) bias is usually overwhelmed by the large standard errors.

Permutation test

- When the assignment process of Z_i is known, we can construct hypothesis tests using permutation inference assuming a constant treatment effect τ (Imbens and Rosenbaum, 2005).
 - With a single, de-meaned instrument $\tilde{Z}_i = Z_i - \bar{Z}$, we can construct a test statistic based on the sample covariance between Z_i and Y_i with the effect removed:

$$T(\tau) = \frac{1}{N} \sum_{i=1}^N \tilde{Z}_i \times (Y_i - \tau D_i)$$

- If the instrument is valid, under the null hypothesis that $\tau = \tau_0$, we can get the **randomization distribution** of the test statistic by simply re-randomizing treatment according to the known assignment process.
 - Construct confidence intervals by "inverting the test" - what values of τ_0 does the test fail to reject?
- Alternative test statistics based on ranks of $Y_i - \tau D_i$ (possibly within strata) can also be used.

Anderson-Rubin Test

- Even when the assignment process is not known, the IV assumptions allow us to construct a test statistic that does not depend on the first stage.
 - This is the **Anderson-Rubin (1949)** approach - **Andrews, Stock and Sun (2019)** provide a good explanation especially for the "just-identified" case
- Let $\hat{\delta}_{\text{ITT}}$ be the **reduced form** or intent-to-treat estimate.
- The instrumental variables assumptions that the reduced form is related to the first stage π and the treatment effect τ

$$\delta_{\text{ITT}} = \pi \times \tau$$

- Assuming a particular null $H_0 : \tau = \tau_0$ implies that

$$\delta_{\text{ITT}} - \pi \times \tau_0 = 0$$

Anderson-Rubin Test

- And so we can construct a test statistic based on the difference between the estimated ITT and the estimated first stage adjusted by the null which we know is normal in large samples.

$$g(\tau_0) = \hat{\delta}_{\text{ITT}} - \hat{\pi}\tau_0 \sim \mathcal{N}(0, \Omega(\tau_0))$$

- The Anderson-Rubin (1949) test statistic is:

$$AR(\tau) = g(\tau)' \Omega(\tau_0)^{-1} g(\tau)$$

Under the null $H_0 : \tau = \tau_0$, this has a chi-squared distribution which does not depend on the value of the first stage.

- **Intuitively**: Statistical properties of **differences** in two normal random variables are well-known and easy. Statistical properties of **ratios** are much more complicated!
 - Again, invert the test to get a confidence interval
 - Can get **infinite** confidence bounds with a weak instrument - the test **never rejects** for any value of τ_0

Example: Strong instrument

```
wapo_iv <- ivmodelFormula(voted ~ getpost | post , data= wapost, heteroSE=T)  
print(AR.test(wapo_iv))
```

```
## $Fstat  
## [1] 0.0171  
##  
## $df  
## [1] 1 731  
##  
## $p.value  
## [1] 0.896  
##  
## $ci.info  
## [1] "[-0.2058034696935, 0.175035583124674]"  
##  
## $ci  
##      lower upper  
## [1,] -0.206 0.175
```

Example: Weak instrument

```
weak_iv_data <- data.frame(Y = Y, D= D, Z=Z)
weak_iv <- ivmodelFormula(Y ~ D | Z , data= weak_iv_data, heteroSE=T)
print(AR.test(weak_iv))
```

```
## $Fstat
## [1] 2.26
##
## $df
## [1] 1 998
##
## $p.value
## [1] 0.133
##
## $ci.info
## [1] "Whole Real Line"
##
## $ci
##      lower upper
## [1,]  -Inf   Inf
```

Conclusion

- Instrumental variables lets us leverage *alternative* sources of randomness to learn about an otherwise confounded causal relationship.
- An instrument:
 - Affects treatment
 - Doesn't affect the outcome except through treatment
 - Is ignorable w.r.t the outcome.
- LATE theorem: The IV estimand is the ATE among those who would take treatment due to the instrument.
 - With continuous treatment/instrument - a weighted average of LATEs (Angrist and Imbens, 1995)
 - With covariates - a weighted average of covariate-specific LATEs
 - But be careful with this interpretation when the model is not fully saturated (Śłoczyński, 2022)
- Statistical inference is tricky
 - Beware weak instruments - typical large-sample asymptotics do poorly when instruments are irrelevant.
 - Consider weak-instrument robust tests (Anderson-Rubin)
 - If it's not in the reduced form, it's not real.

