

Chapter 7: Instrumental Variables (IV)

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Motivation

- We've seen how to estimate average treatment effects when treatment is as good as randomly assigned conditional on observable characteristics
- We've also seen how we can relax the assumption of conditional unconfoundedness if we have panel data and assume the selection bias is constant over time (parallel trends)

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 - We might not have panel data, or might be worried that the confounders are time-varying (so parallel trends fails)

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- But sometimes these assumptions won't be plausible
 - We might be worried about unobserved confounding variables
 - We might not have panel data, or might be worried that the confounders are time-varying (so parallel trends fails)
- What can we do then?

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- Luckily, sometimes we have a (natural) experiment that affects whether some people take up the treatment we're interested in
- If that's the case, then (under certain conditions) we can use this experiment to learn about the effect of the treatment for the “compliers” who are induced to take-up treatment by the experiment

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- The ideal situation for learning about the causal effects of Medicaid would be to randomize who gets it
 - Not possible for moral / budgetary reasons
- However, we have the Oregon Health Insurance Experiment (OHIE) which randomized *eligibility* for Medicaid for some people (i.e. those with earnings between 100 and 138% of the FPL)

Reminder - background about OHIE

In 2008, a group of uninsured low-income adults in Oregon was selected by lottery to be given the chance to apply for Medicaid. This lottery provides an opportunity to gauge the effects of expanding access to public health insurance on the health care use, financial strain, and health of low-income adults using a randomized controlled design. In the year after random assignment, the treatment group selected by the lottery was about 25 percentage points more likely to have insurance than the control group that was not selected. We find that in this first year, the treatment group had substantively and statistically significantly higher health care utilization (including primary and preventive care as well as hospitalizations), lower out-of-pocket medical expenditures and medical debt (including fewer bills sent to collection), and better self-reported physical and mental health than the control group. *JEL Codes:* H51, H75, I1.

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- Some people who win the lottery do not enroll (don't return form; no longer eligible)
- Some people who lose the lottery enroll anyway (become eligible by other criteria)

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- Say you want to estimate the effect of *Medicaid enrollment* on depression? What would you do?
- A natural estimate is to divide the effect on depression by the effect on enrollment: $-0.023/0.256 \approx -0.09 \rightarrow$ a 9 pp reduction per enrollee

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- A natural estimate is to divide the effect on depression by the effect on enrollment: $-0.023/0.256 \approx -0.09 \rightarrow$ a 9 pp reduction per enrollee
- This is called an **instrumental variables (IV)** estimate. When does it work? And how exactly do we interpret it?

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- Next, we’ll go over the assumptions we need to identify the LATE with instrumental variables

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 - Defiers are weird, and often we will assume they don't exist (called *monotonicity*)

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Key assumptions

- **Independence:** The instrument (e.g. whether you win the lottery) is as good as randomly assigned, $Z_i \perp\!\!\!\perp (Y_i(0), Y_i(1), D_i(0), D_i(1))$

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- **Relevance** Instrument affects the probability of treatment:
 $P(D_i = 1|Z_i = 1) \neq P(D_i = 1|Z_i = 0)$
 - This one is testable – we saw that it does!
- **Monotonicity:** No defiers: $D_i(1) \geq D_i(0)$ for all i
 - Seems reasonable that anyone who enrolls without winning would also enroll if they won

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- Mathematically, we have that $Y_i = D_i(Z_i)Y_i(1) + (1 - D_i(Z_i))Y_i(0)$
 - Equivalently, can write the POs as $Y_i(d, z)$, then state the exclusion restriction as $Y_i(d, z) = Y_i(d)$.

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- This implies that the outcomes for the always-takers and never-takers doesn't depend on Z_i

Why might exclusion fail?

- Intuitively, exclusion fails if the instrument (e.g. winning the lottery) can affect your outcome even without changing your treatment status

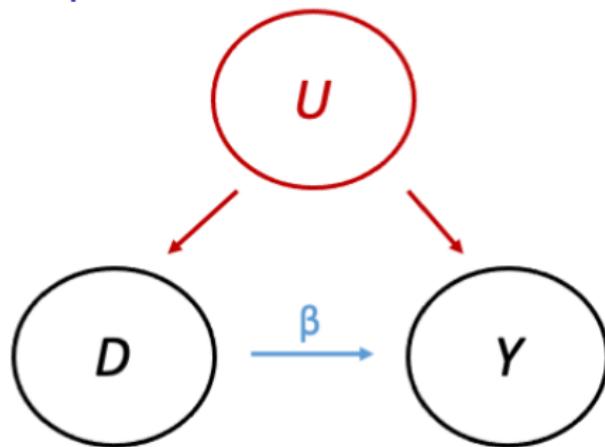
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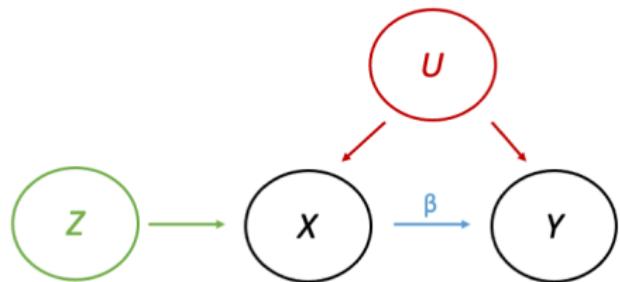
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- Yes, if wait-time for Medicaid affects your health.

Graphical Illustration



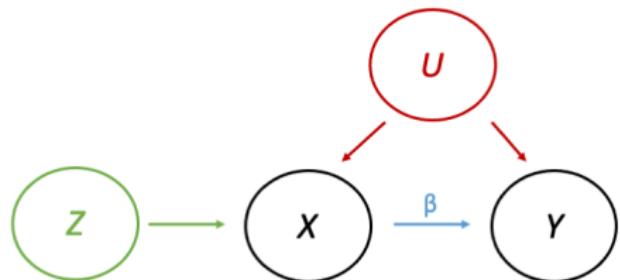
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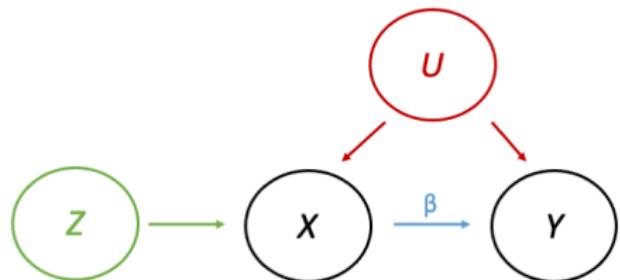
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- Unconfoundedness fails if there is an unobservable U that affects both treatment D and Y (denoted by arrows)
- We can get around this problem if we have instrument like Z
- Independence: Z is not influenced by the unobservable U (it's randomly assigned!)
- Exclusion: Z affects Y only through D (no $Z \rightarrow Y$ arrow)

The LATE Theorem

- If Independence, Relevance, Exclusion, and Monotonicity hold, then

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

or in words:

$$\frac{\text{Effect of } Z \text{ on } Y}{\text{Effect of } Z \text{ on } D} = \text{Local average treatment effect for compliers}$$

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- This result won Joshua Angrist and Guido Imbens the 2021 Nobel Prize in Economics!

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- Thus, the **numerator** will be the average effect for compliers times the fraction of compliers in the population, i.e. $LATE \times P(\text{Complier})$
- But the **denominator** is the effect of Z on D . This effect is zero for ATs and NTs, so the denominator is the share of compliers, $P(\text{Complier})$

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More formal derivation

- Remember that we can divide the population into three groups, always-takers (ATs), never-takers (NTs), and compliers (Cs) — we assumed no defiers!
- Let $\alpha_{AT} = P(AT)$, $\alpha_{NT} = P(NT)$, $\alpha_C = P(C)$ be the shares of ATs, NTs, and Cs
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Likewise, $P(AT|Z=0) = P(AT) = \alpha_{AT}$
- By similar arguments, share of NTs and Cs is the same in the $Z=1$ and $Z=0$ groups.

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where the last line uses independence

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$$E[Y_i|Z_i = 0] = \alpha_C E[Y_i(0)|C] + \alpha_{AT} E[Y_i(1)|AT] + \alpha_{NT} E[Y_i(0)|NT]$$

- Thus, the numerator in our ratio is

$$E[Y_i|Z_i = 1] - E[Y_i|Z_i = 0] = \alpha_C \underbrace{(E[Y_i(1)|C] - E[Y_i(0)|C])}_{LATE}$$

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Hence,

$$\frac{\text{numerator}}{\text{denominator}} = \frac{\alpha_C \times LATE}{\alpha_C} = LATE$$

Estimating LATE

- We've shown that under the IV assumptions, the LATE is identified as a function of population means

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

- How can we estimate LATE?

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- Plug in sample means!

$$\hat{\beta} = \frac{\bar{Y}_{Z=1} - \bar{Y}_{Z=0}}{\bar{D}_{Z=1} - \bar{D}_{Z=0}},$$

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- $\hat{\beta}$ is called the IV estimator, or more precisely, the two-stage least squares (2SLS) estimator (for reasons that will become clear soon!)

Example - Medicaid

	Treatment	Control
Ever on Medicaid	0.397	0.141
Depression symptoms	0.306	0.329

- Let's calculate the 2SLS estimator in the Medicaid example.

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$$\frac{\bar{Y}_{Z=1} - \bar{Y}_{Z=0}}{\bar{D}_{Z=1} - \bar{D}_{Z=0}} = \frac{0.306 - 0.329}{0.397 - 0.141} = -0.09$$

Two-stage least squares

- Consider the two linear regressions:

$$Y_i = \gamma_0 + Z_i \gamma_1 + \varepsilon_i \quad (1)$$

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- What are the OLS estimates $\hat{\gamma}_1$ and $\hat{\pi}_1$?

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- Equation (2) is typically called the *first-stage*, while equation (1) is called the *reduced form*

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- $\hat{\gamma}_1 = 0.306 - 0.329 = -0.023$
- So the 2SLS estimator is $\hat{\gamma}_1 / \hat{\pi}_1 = -0.023 / 0.256 = -0.09$.

IV conditional on covariates

- Oftentimes, the assumption that the instrument is as-good-as-randomly assigned will only be plausible conditional on observable characteristics
- In fact, in the OHIE, the probability of winning the lottery depended on family size!

IV conditional on covariates

- Oftentimes, the assumption that the instrument is as-good-as-randomly assigned will only be plausible conditional on observable characteristics
- In fact, in the OHIE, the probability of winning the lottery depended on family size!
- Suppose that we replace the independence assumption with conditional independence, $Z_i \perp\!\!\!\perp (Y_i(1), Y_i(0), D_i(1), D_i(0)) | X_i$
- By similar arguments as before, we can identify the *conditional LATE* by taking the same ratio within groups with the same value of x

$$\frac{E[Y_i|Z_i = 1, X_i = x] - E[Y_i|Z_i = 0, X_i = x]}{E[D_i|Z_i = 1, X_i = x] - E[D_i|Z_i = 0, X_i = x]} =$$
$$\underbrace{E[Y_i(1) - Y_i(0)|D_i(1) = 1, D_i(0) = 0, X_i = x]}_{\text{LATE}(x)}$$

Estimation for LATEs conditional on covariates

- In practice, when we have conditional independence for the instrument, people estimate a modified version of 2SLS that includes covariates.
- That is, they use $\hat{\beta}_{2SLS} = \hat{\gamma}_1/\hat{\pi}_1$, for the OLS estimates from

$$Y_i = \gamma_0 + Z_i \gamma_1 + \mathbf{X}'_i \boldsymbol{\gamma}_2 + \varepsilon_i \quad (3)$$

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- When \mathbf{X}_i is a set of dummy variables for different categories (e.g. family size), this gives a weighted average of the 2SLS estimates for each covariate value
- More generally, this will be approximately consistent for a weighted average of $LATE(x)$ when (4) is a good approximation to the CEF

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Example - Medicaid

- In the OHIE, the lottery was actually only random condition on family size
- Finkelstein et al (2012) therefore estimate 2SLS with

$$Y_i = \gamma_0 + Z_i \gamma_1 + \mathbf{X}'_i \boldsymbol{\gamma}_2 + \varepsilon_i \quad (5)$$

$$D_i = \pi_0 + Z_i \pi_1 + \mathbf{X}'_i \boldsymbol{\pi}_2 + u_i \quad (6)$$

where \mathbf{X}_i includes fixed effects for family size and some other demographic variables.

TABLE IV
HOSPITAL UTILIZATION

	Control mean (1)	ITT (2)	LATE (3)	<i>p</i> -values (4)
Panel A: Extensive margin				
All hospital admissions	0.067 (0.250)	0.0054 (0.0019)	0.021 (0.0074)	[0.004]
Admissions through ER	0.048 (0.214)	0.0018 (0.0016)	0.0070 (0.0062)	[0.265]
Admissions not through ER	0.029 (0.167)	0.0041 (0.0013)	0.016 (0.0051)	[0.002]

TABLE IX
HEALTH

	Control mean (1)	ITT (2)	LATE (3)	p-values (4)
Panel A: Administrative data				
Alive	0.992 (0.092)	0.00032 (0.00068)	0.0013 (0.0027)	[0.638]
Panel B: Survey data				
Self-reported health good/very good/excellent (not fair or poor)	0.548 (0.498)	0.039 (0.0076)	0.133 (0.026)	[<0.0001] {<0.0001}
Self-reported health not poor (fair, good, very good, or excellent)	0.86 (0.347)	0.029 (0.0051)	0.099 (0.018)	[<0.0001] {<0.0001}
Health about the same or gotten better over last six months	0.714 (0.452)	0.033 (0.0067)	0.113 (0.023)	[<0.0001] {<0.0001}
# of days physical health good, past 30 days*	21.862 (10.384)	0.381 (0.162)	1.317 (0.563)	[0.019] {0.018}
# days poor physical or mental health did not impair usual activity, past 30 days*	20.329 (10.939)	0.459 (0.175)	1.585 (0.606)	[0.009] {0.015}
# of days mental health good, past 30 days*	18.738 (11.445)	0.603 (0.184)	2.082 (0.64)	[0.001] {0.003}
Did not screen positive for depression, last two weeks	0.671 (0.470)	0.023 (0.0071)	0.078 (0.025)	[0.001] {0.003}
Standardized treatment effect	0.059 (0.011)	0.203 (0.039)		[<0.0001]

Notes. Standard errors in parentheses; per comparison p-values in square brackets; family-wise p-values in curly brackets. Column (2) reports the coefficient and standard error on *LOTTERY* from estimating equation (1) by OLS. Column (3) reports the coefficient and standard error on *INSURANCE* from estimating equation (3) by IV; for the IV estimates in column (3), the endogenous variable *INSURANCE* is defined as "ever on Medicaid" during our study period and the first stage is given in the first row of Table III. Column (4) reports the per comparison p-value and the family-wise p-value across the different measures used to create the standardized treatment effect. Standardized treatment effect reports results based on equation (2). All regressions include household size fixed effects and standard errors are clustered on the household. The regressions in Panel A include lottery draw fixed effects, and the dependent variable "alive" is measured from the notification date through September 2009 ($N=74,922$). The regressions in Panel B include survey wave fixed effects and the interaction of survey wave fixed effects with household size fixed effects, and are weighted using the survey weights ($N=23,741$).

*These questions were worded to ask about number of days health "not good" or "impaired"; we switched the sign for consistency with the other measures. See Online Appendix Figure A4 for the exact survey wording.

“Quasi-experimental” Instruments

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“Quasi-experimental” Instruments

- In the Oregon Medicaid setting, the instrument Z_i was explicitly randomly assigned
- In other settings, researchers use an instrument that is not directly randomly but may be the result of idiosyncratic factors that are potentially “as good as random”
- This expands the set of cases where we can use IV, but means the required assumptions deserve extra scrutiny!

Example – Angrist and Krueger (1991)

- AK (1991) are interested in a classic question in labor economics:
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 - Schooling is not randomly assigned! Choice of schooling may depend on many confounding factors, such as ability or family background, that would directly affect earnings as well.
- What do we need if we want to estimate the effects of schooling on earnings using IV?
- We need to find an instrument that is as-good-as-randomly assigned, and affects earnings only through years of schooling.

Compulsory Schooling Laws

- Most states have compulsory schooling laws that require people to stay in school until their 16th or 17th birthday

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- AK argue that these laws will lead people planning to drop out to have less schooling if they are born earlier in the year
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- AK argue that the part of the year in which you are born is effectively random, and therefore instrument for years of schooling with quarter of birth

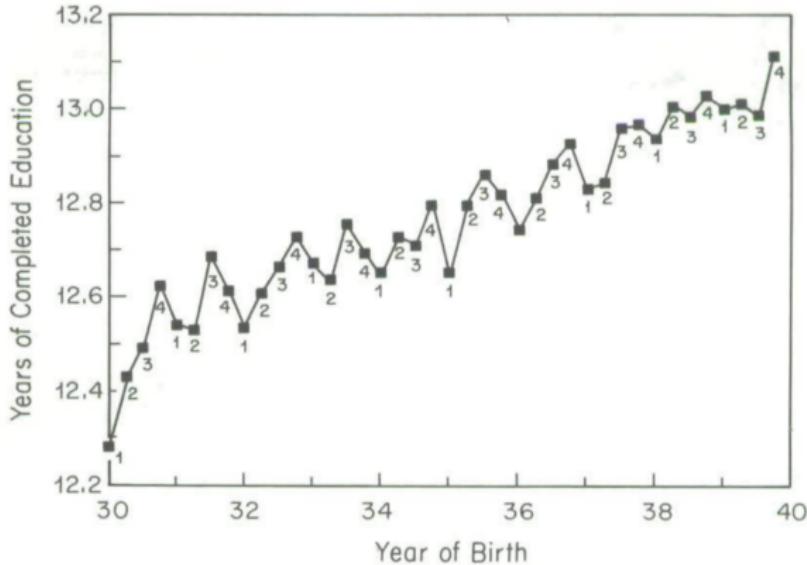


FIGURE I
Years of Education and Season of Birth
1980 Census
Note. Quarter of birth is listed below each observation.

- Indeed, AK find that people born in the first quarter the of the year tend to have less schooling on average than people born in the remaining three quarters of the same year

PANEL A: WALD ESTIMATES FOR 1970 CENSUS—MEN BORN 192

	(1) Born in 1st quarter of year	(2) Born in 2nd, 3rd, or 4th quarter of year
ln (wkly. wage)	5.1484	5.1574
Education	11.3996	11.5252

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- What is the first stage estimate?

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Education	11.3996	11.5252

- What is the first stage estimate? $11.5252 - 11.3996 = 0.1256$

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- What is the reduced form estimate?

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TABLE III

PANEL A: WALD ESTIMATES FOR 1970 CENSUS—MEN BORN 1920–1929^a

	(1) Born in 1st quarter of year	(2) Born in 2nd, 3rd, or 4th quarter of year	(3) Difference (std. error) (1) – (2)
ln (wkly. wage)	5.1484	5.1574	-0.00898 (0.00301)
Education	11.3996	11.5252	-0.1256 (0.0155)
Wald est. of return to education			0.0715 (0.0219)
OLS return to education ^b			0.0801 (0.0004)

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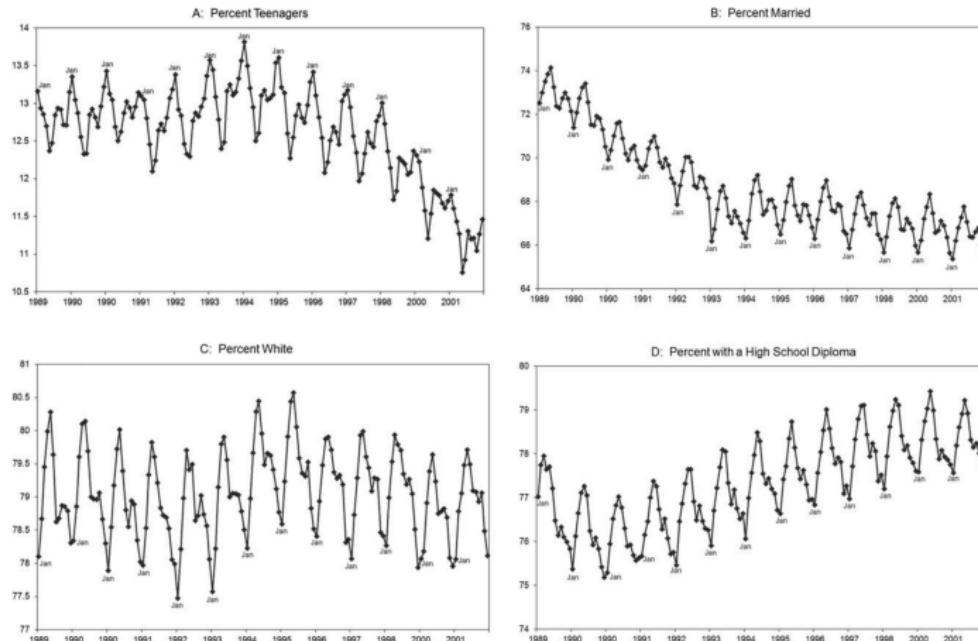
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FIGURE 1.—MATERNAL CHARACTERISTICS BY MONTH, NATALITY FILES, 1989–2001



- Quarter of birth is correlated with some demographic characteristics
- Suggests some possible violations of independence

Evaluating the assumptions...

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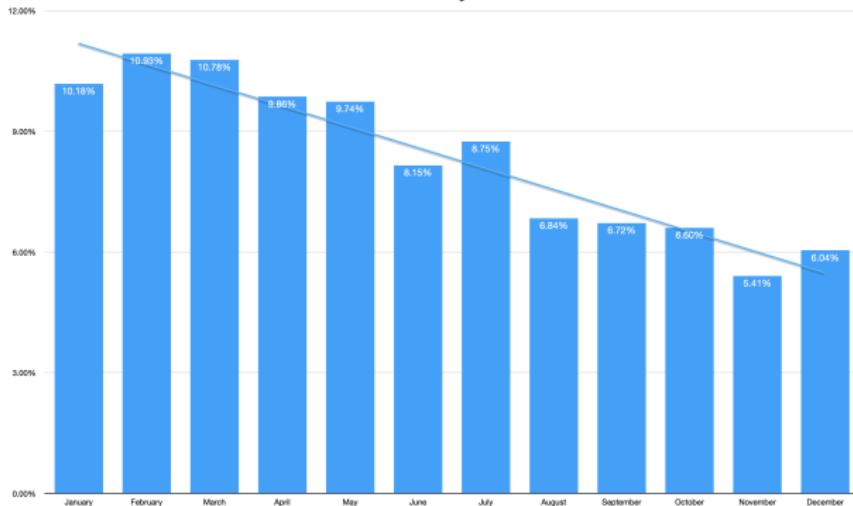
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- Seems generally plausible but... being older or younger in your grade may affect the quality of your education directly

Birth Month of NHL Players Born Since 1980

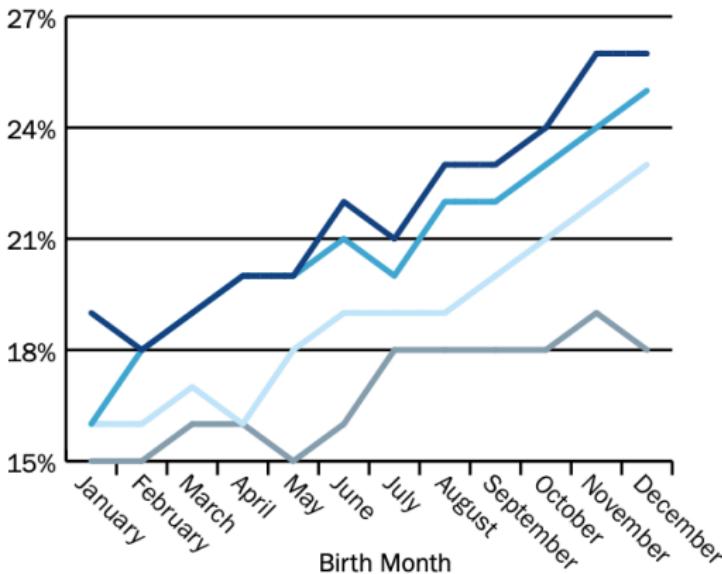


Students Born Later in the Year More Likely to Be Identified With a Disability, School Year 2017-2018

By Birth Month and Year

— 2009 — 2010 — 2011 — 2012

Percent of Students



Evaluating the assumptions

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- If some schools use a Sept 1 cutoff, it could be that being born in Q4 is even worse

TABLE II
PERCENTAGE OF AGE GROUP ENROLLED IN SCHOOL BY BIRTHDAY AND LEGAL
DROPOUT AGE^a

Date of birth	Type of state law ^b		
	School-leaving age: 16 (1)	School-leaving age: 17 or 18 (2)	Column (1) - (2)
Percent enrolled April 1, 1960			
1. Jan 1-Mar 31, 1944 (age 16)	87.6 (0.6)	91.0 (0.9)	-3.4 (1.1)
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Percent of population aged 14-17

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- Comparing states with different-aged CSLs suggested the effects of QOB on education operates through the CSL → assuages some of (but not all) concerns about independence

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 - Treatment effects could be different for people on the margin of dropping out

Multiple Instruments

- So far, we have considered binary instrumental variables (win vs lose lottery)
- Sometimes we may have an instrument that takes on multiple values (or multiple different instruments)
- Next we'll talk about how we can use similar ideas to exploit the variation from multi-valued instruments

Example – Angrist 1990

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- If we want to use IV, what do we need? A plausibly random instrument that affects earnings only through whether you enroll in the military

Angrist 1990 – Draft Lotteries

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- Angrist first considers a binary version of the instrument (eligible vs non-eligible bdays), then generalizes to multiple instruments (one for each bday)

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 - Seems reasonable

- Angrist first considers a binarized version of the instrument where he groups birthdays into eligible and non-eligible
- Here is his first stage (see $\hat{p}^e - \hat{p}^n$)

TABLE 2—VETERAN STATUS AND DRAFT ELIGIBILITY

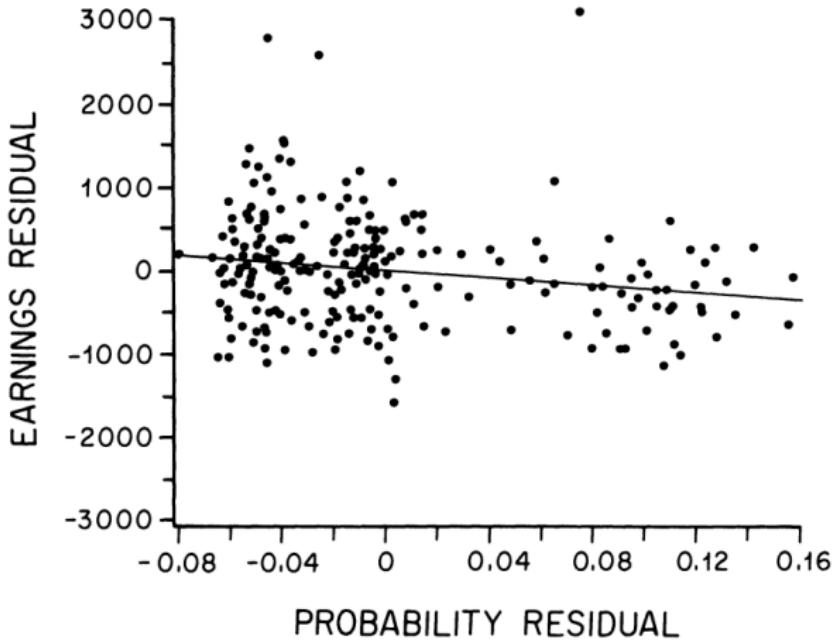
Whites						
Data Set	Cohort	Sample	P(Veteran)	\hat{p}^e	\hat{p}^n	$\hat{p}^e - \hat{p}^n$
SIPP (84) ^a	1950	351	0.2673 (0.0140)	0.3527 (0.0325)	0.1933 (0.0233)	0.1594 (0.0400)
	1951	359	0.1973 (0.0127)	0.2831 (0.0390)	0.1468 (0.0180)	0.1362 (0.0429)
	1952	336	0.1554 (0.0114)	0.2310 (0.0473)	0.1257 (0.0146)	0.1053 (0.0495)
	1953	390	0.1298 (0.0106)	0.1581 (0.0339)	0.1153 (0.0152)	0.0427 (0.0372)
DMDC/CWHS ^b	1950	16119	0.0633 (0.0019)	0.0936 (0.0032)	0.0279 (0.0019)	0.0657 (0.0037)
	1951	16768	0.1176 (0.0025)	0.2071 (0.0053)	0.0708 (0.0024)	0.1362 (0.0059)
	1952	17703	0.1515 (0.0027)	0.2683 (0.0065)	0.1102 (0.0027)	0.1581 (0.0071)
	1953	17749	0.1343 (0.0026)	0.1548 (0.0053)	0.1268 (0.0029)	0.0280 (0.0060)

TABLE 3—WALD ESTIMATES

Cohort	Year	Draft-Eligibility Effects in Current \$				
		FICA Earnings (1)	Adjusted FICA Earnings (2)	Total W-2 Earnings (3)	$\hat{p}^e - \hat{p}^n$ (4)	Service Effect in 1978 \$ (5)
1950	1981	-435.8 (210.5)	-487.8 (237.6)	-589.6 (299.4)	0.159 (0.040)	-2,195.8 (1,069.5)
	1982	-320.2 (235.8)	-396.1 (281.7)	-305.5 (345.4)		-1,678.3 (1,193.6)
	1983	-349.5 (261.6)	-450.1 (302.0)	-512.9 (441.2)		-1,795.6 (1,204.8)
	1984	-484.3 (286.8)	-638.7 (336.5)	-1,143.3 (492.2)		-2,517.7 (1,326.5)
1951	1981	-358.3 (203.6)	-428.7 (224.5)	-71.6 (423.4)	0.136 (0.043)	-2,261.3 (1,184.2)
	1982	-117.3 (229.1)	-278.5 (264.1)	-72.7 (372.1)		-1,386.6 (1,312.1)
	1983	-314.0 (253.2)	-452.2 (289.2)	-896.5 (426.3)		-2,181.8 (1,395.3)
	1984	-398.4 (279.2)	-573.3 (331.1)	-809.1 (380.9)		-2,647.9 (1,529.2)
1952	1981	-342.8 (206.8)	-392.6 (228.6)	-440.5 (265.0)	0.105 (0.050)	-2,502.3 (1,556.7)
	1982	-235.1 (232.3)	-255.2 (264.5)	-514.7 (296.5)		-1,626.5 (1,685.8)
	1983	-437.7 (257.5)	-500.0 (294.7)	-915.7 (395.2)		-3,103.5 (1,829.2)
	1984	-436.0 (281.9)	-560.0 (330.1)	-767.2 (376.0)		-3,323.8 (1,959.3)

Visualizing LATE in this example

See diagrams [here](#)



Notes: The figure plots the history of FICA taxable earnings for the four cohorts born 1950–53. For each cohort, separate lines are drawn for draft-eligible and draft-ineligible men. Plotted points show average real (1978) earnings of working men born in 1953, real earnings + \$3000 for men born in 1950, real earnings + \$2000 for men born in 1951, and real earnings + \$1000 for men born in 1952.

FIGURE 1. SOCIAL SECURITY EARNINGS PROFILES BY DRAFT-ELIGIBILITY STATUS

Formalizing 2SLS with Multiple Instruments

- We can formalize this procedure with what's called two-stage least squares with multiple instruments.
- Suppose we have a vector of instruments Z_i (e.g. Z_1 is birthday 1, Z_2 is birthday 2)

2SLS with multiple instruments

- Step 1 (first stage): Estimate $E[D_i | \mathbf{Z}_i]$ by estimating the OLS regression specification:

$$D_i = \gamma_0 + \mathbf{Z}'_i \boldsymbol{\gamma}_1 + u_i$$

E.g., estimate mean enrollment for each birthday

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- Step 2: Construct the estimate of $E[D_i | Z_i]$ for each unit i :

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- $\hat{\beta}_1$ is our estimate of the (weighted) LATE

TABLE 4—TWO-STAGE INSTRUMENTAL VARIABLES ESTIMATES

Cohort	Whites		
	FICA Taxable Earnings	Adjusted FICA Earnings	Total W-2 Compensation
Model 1			
1950	-1709.2 (946.8)	-2093.7 (1108.8)	-1895.0 (1333.1)
1951	-1457.1 (959.3)	-1983.7 (1036.1)	-2431.4 (1152.1)
1952	-1724.0 (863.1)	-1943.0 (927.2)	-2058.7 (1001.9)
1953	1223.8 (3232.1)	900.7 (3505.3)	-488.6 (3936.0)
-			

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- Where do these standard errors come from?

Normality of reduced form and first stage

- Remember that with a single instrument, $\hat{\beta}_{IV} = \hat{\gamma}_1 / \hat{\pi}_1$, where $\hat{\gamma}_1, \hat{\pi}_1$ are OLS estimates of

$$Y_i = \gamma_0 + Z_i \gamma_1 + \varepsilon_i$$

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- We've shown that OLS estimates are asymptotically normally distributed (and these convergences hold jointly), so we will have

$$\sqrt{N} \left(\begin{pmatrix} \hat{\gamma}_1 \\ \hat{\pi}_1 \end{pmatrix} - \begin{pmatrix} \gamma_1 \\ \pi_1 \end{pmatrix} \right) \rightarrow_d N(0, \Sigma)$$

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- Can we learn from this the asymptotic distribution of $\hat{\gamma}_1 / \hat{\pi}_1$?

Delta method

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Delta method

- Let $g(\hat{\gamma}_1, \hat{\pi}_1) = \hat{\gamma}_1 / \hat{\pi}_1$. For $(\hat{\gamma}_1, \hat{\pi}_1) \approx (\gamma_1, \pi_1)$ a first-order Taylor expansion tells us that

$$g(\hat{\gamma}_1, \hat{\pi}_1) \approx g(\gamma_1, \pi_1) + \nabla g(\gamma_1, \pi_1)(\hat{\gamma}_1 - \gamma_1, \hat{\pi}_1 - \pi_1)'$$

where $\nabla g(\gamma_1, \pi_1)$ is the gradient of g evaluated at (γ_1, π_1) .

- This implies that

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Delta method

- Let $g(\hat{\gamma}_1, \hat{\pi}_1) = \hat{\gamma}_1 / \hat{\pi}_1$. For $(\hat{\gamma}_1, \hat{\pi}_1) \approx (\gamma_1, \pi_1)$ a first-order Taylor expansion tells us that

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- We can estimate the variance with sample analogs (e.g. $\nabla g(\hat{\gamma}_1, \hat{\pi}_1)$).

Weak identification

- Note that $\hat{\beta}_{IV} = \hat{\gamma}_1 / \hat{\pi}_1$ is only well-defined for $\hat{\pi}_1 \neq 0$.
- As $\hat{\pi}_1 \rightarrow 0$, $|\hat{\beta}_{IV}| \rightarrow \infty$, so the IV estimator will be poorly behaved if $\hat{\pi}_1 \approx 0$.

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- But in practice, $\hat{\pi}_1$ may sometimes be close to zero (relative to its standard error).
- In this case, the normal distribution above may provide a poor approximation to the distribution of $\hat{\beta}_{IV}$. This is a problem known as *weak identification*.

Weak Instruments - Monte Carlo

Monte Carlo Simulation: No True Treatment Effect

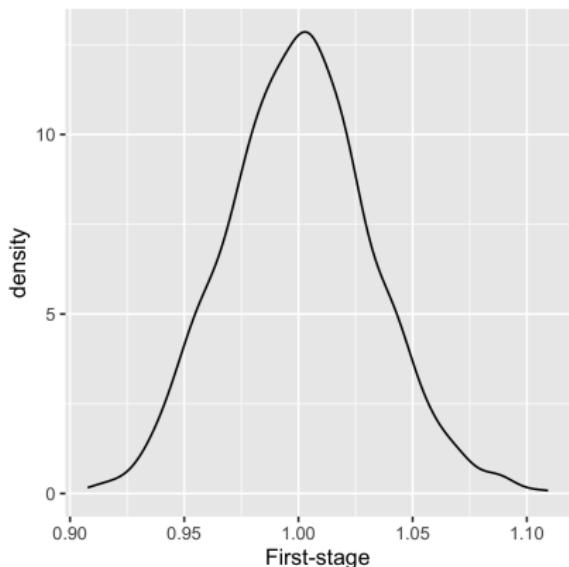
$$Y_i(d) = \eta + v; \quad D_i = \pi_1 Z_i + \eta; \quad \eta, v, Z_i \sim N(0, 1)$$

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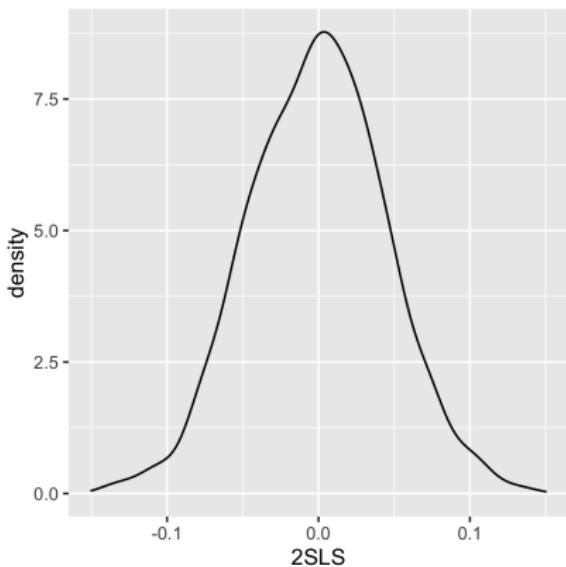
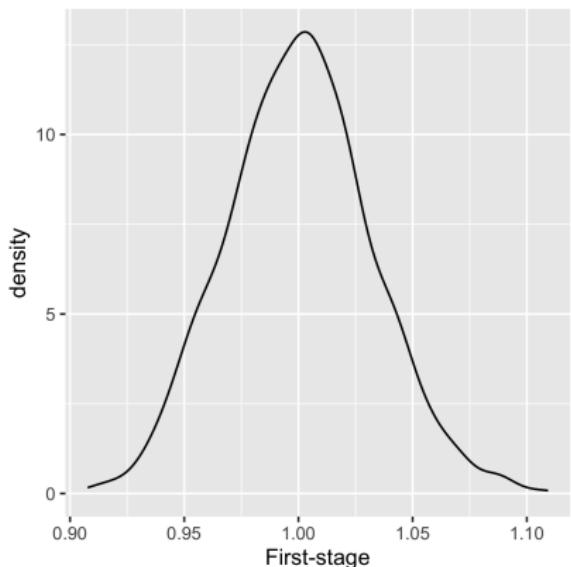


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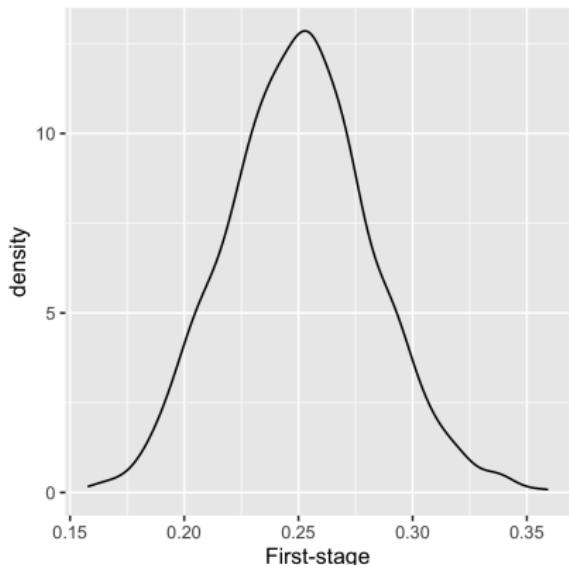


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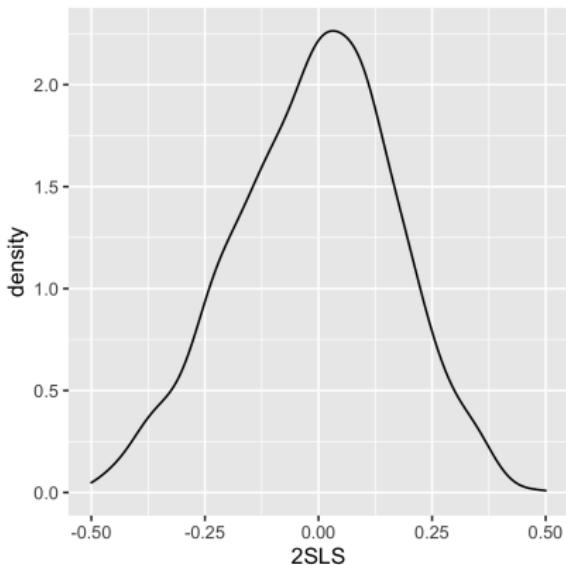
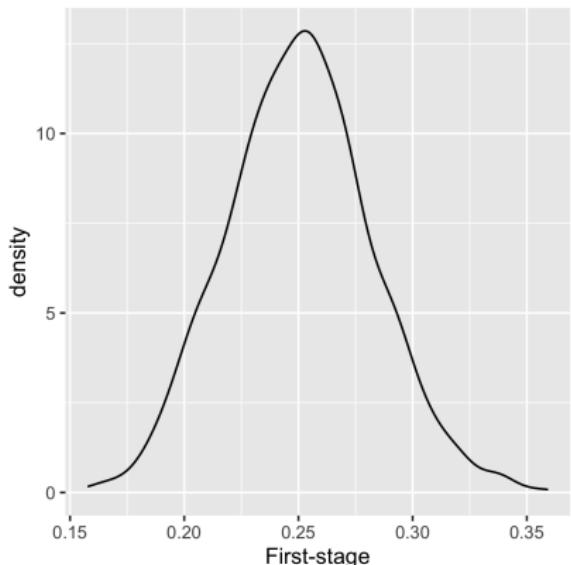


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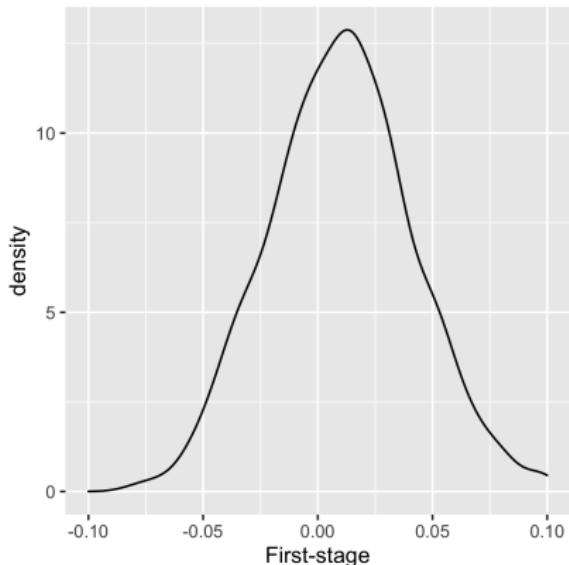


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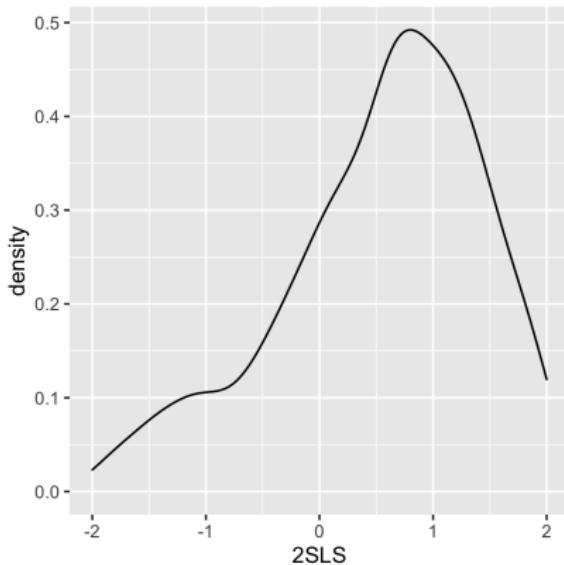
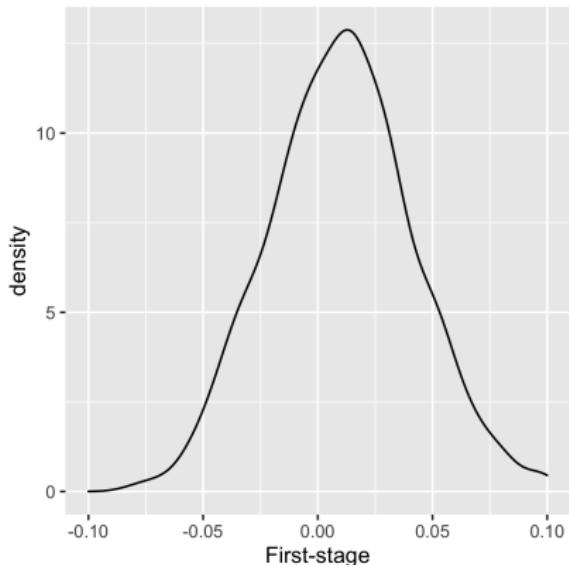


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- Imagine the extreme case, where every observation gets its own instrument (i.e. interact quarter-of-birth with individual)
- First-stage fit will be *perfect*: $\hat{D}_i = \hat{\pi}'_1 Z_i = D_i$. So 2SLS = OLS numerically
- Thus the weak instrument problem arises from “overfitting” in the first stage

Testing for weak IV

- A typical rule of thumb is to worry about weak instruments if the F -statistic on the instruments in the first stage is < 10
- That is, run the first-stage regression

$$D_i = \pi_0 + \mathbf{Z}'_i \boldsymbol{\pi}_1 + \mathbf{X}'_i \boldsymbol{\pi}_2 + u_i$$

and construct the F -statistic for the null $H_0 : \boldsymbol{\pi}_1 = 0$.

- With one instrument, the F -statistic is just the t -statistic on the instrument squared, so $F > 10$ corresponds with $|t| > 3.2$.

```
. reg education D highnum_region_D post high_intensity highnum_region highnum_regi  
> on_post highnum_region_high, r
```

Linear regression

Number of obs = 31,310
F(7, 31302) = 133.29
Prob > F = 0.0000
R-squared = 0.0282
Root MSE = 3.8458

education	Robust					
	Coef.	Std. Err.	t	P> t	[95% Conf. Interval]	
D	-.1623344	.1236149	-1.31	0.189	-.4046245	.0799557
highnum_region~D	.5457673	.1761518	3.10	0.002	.2005028	.8910318
post	.3354358	.0763198	4.40	0.000	.1858459	.4850256
high_intensity	-1.309835	.0951979	-13.76	0.000	-1.496426	-1.123243
highnum_region	-.3988454	.0838969	-4.75	0.000	-.5632866	-.2344043
highnum_region~t	-.2082078	.1109533	-1.88	0.061	-.4256806	.009265
highnum_region~h	.0932633	.1348633	0.69	0.489	-.1710742	.3576008
_cons	10.05763	.0590164	170.42	0.000	9.941953	10.1733

```
. test D highnum_region_D
```

(1) D = 0
(2) highnum_region_D = 0

F(2, 31302) = 5.53
Prob > F = 0.0040

Here, the instruments are D and highnumregion_D (confusing labels - sorry!). First-stage F is 5.53

```
. ivreg2 log_wage (education=D highnum_region_D) post high_intensity highnum_region
> n highnum_region_post highnum_region_high, r
```

IV (2SLS) estimation

Estimates efficient for homoskedasticity only
 Statistics robust to heteroskedasticity

Total (centered) SS =	13459.19497	Number of obs =	31310
Total (uncentered) SS =	4570266.616	F(6, 31303) =	276.17
Residual SS =	16731.05867	Prob > F =	0.0000
		Centered R2 =	-0.2431
		Uncentered R2 =	0.9963
		Root MSE =	.731

log_wage	Robust				
	Coef.	Std. Err.	z	P> z	[95% Conf. Interval]
education	.1856081	.0584726	3.17	0.002	.0710038 .3002124
post	-.2773642	.0197758	-14.03	0.000	-.316124 -.2386044
high_intensity	.0949207	.0819745	1.16	0.247	-.0657465 .2555878
highnum_region	-.0947804	.0319689	-2.96	0.003	-.1574383 -.0321225
highnum_region-t	-.0734455	.0165758	-4.43	0.000	-.1059335 -.0409576
highnum_region-h	.0379194	.0261998	1.45	0.148	-.0134312 .08927
_cons	10.45536	.5899054	17.72	0.000	9.299167 11.61155

Underidentification test (Kleibergen-Paap rk LM statistic): **11.057**
 Chi-sq(2) P-val = **0.0040**

Weak identification test (Cragg-Donald Wald F statistic): **5.617**
 Kleibergen-Paap rk Wald F statistic: **5.530**
 Stock-Yogo weak ID test critical values: 10% maximal IV size **19.93**
 15% maximal IV size **11.59**
 20% maximal IV size **8.75**
 25% maximal IV size **7.25**

Source: Stock-Yogo (2005). Reproduced by permission.

NB: Critical values are for Cragg-Donald F statistic and i.i.d. errors.

We can get the same *F*-stat directly from the ivreg2 command

What do about weak instruments?

- There are some ways to reducing over-fitting in the first-stage by splitting the sample (split sample or jackknife IV)
- There are also some better ways to get confidence intervals when you have weak instruments
 - Most common is what are called Anderson-Rubin confidence sets (we won't have time to cover)
- Can also try to increase the strength of the instrument by:
 - Getting a larger sample
 - Adding control variables that correlate with D (but not strongly with Z)
 - Thinking of a new instrument ;-)