Examples of Instrumental Variable Analyses

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Outline

- Finding an instrumental variable
- IVs in randomized trials: Moving To Opportunity
- Answering a parallel question with a natural experiment (lottery)
- IVs from natural experiments: compulsory schooling law changes
- IVs from genes: FTO as an IV for maternal obesity

- Goals:
  1. Recognize contexts in which IV analyses might be feasible and useful
  2. Recognize the limitations and assumptions of the IV analysis

How do you find an Instrumental Variable?

1. Randomize
2. Some other possible sources of exogenous variation:
   a. Geography of city
   b. Policy variations
   c. Institutional features (banking policies, loan guarantees)
   d. Timing of newly available resources
   e. Wait lists or lotteries for subsidies
   f. Genetic polymorphisms

Randomizing is generally preferable, because the IV assumptions are more plausible and the 1st stage effects are often larger.

Example 1, Moving To Opportunity Trial

- Families with children in urban public housing developments invited and randomized to:
  - Control
  - section 8, or
  - "low poverty" section 8 (must move to neighborhood with <10% poverty)
- Once randomized:
  - 60% of section 8 group moved
  - 47% of low poverty group moved.
- This may sound bad, but compare to a drug-based trials:
  - Women's Health Initiative: "At the time the trial was stopped, 54.0% of study participants assigned to receive CEE and 53.5% of those assigned to receive placebo had discontinued use of their study medication." –Hsia 2006
  - TODAY: "Adherence to the medication regimen before the primary outcome was reached or the study was completed ranged from 84% at month 8 to 57% at month 60" -TODAY study group, NEJM 2012

Example 1, Moving To Opportunity

Multiple causal questions one might try to address with data from the Moving To Opportunity (MTO) trial:

1. Does moving from very high poverty public housing developments benefit the health of mothers or their children?
2. Does living in a low poverty neighborhood benefit the health of mothers or their children?

Randomization $\rightarrow$ Move $\rightarrow$ Live in low-poverty Neighborhood $\longrightarrow$ Child Health

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$$
\begin{tikzcd}
\text{Randomization} \arrow[r] & \text{Move} \arrow[r] \arrow[rr, bend left] & \text{Live in low-poverty Neighborhood} \arrow[r] & \text{Child Health}
\end{tikzcd}
$$

Design

- Families with children in urban public housing developments invited and randomized to:
  - Control
  - section 8, or
  - "low poverty" section 8
- Once randomized:
  - 60% of section 8 group moved
  - 47% of low poverty group moved.

This is the "first stage" estimate if you think of moving from the development as the endogenous variable.

Did the trial affect neighborhood environment?

Poverty Rate	Control	ITT (Low Poverty)	
	Mean	Difference	P-value
Baseline	53.1%	-0.4	0.41
At 1 Year	50.0%	-17.1	<.001
At 5 Years	39.9%	-9.9	<.001
At 10 Years	33.0%	-4.9	<.0001

This is the "first stage" estimate if you think of neighborhood poverty as the endogenous variable.

From ludwig 2011

IV analyses in MTO

- Standard 2-stage least squares
- In most IV analyses, we think the "treated" group includes some "always treated" people and some "compliers".
- The IV estimate refers to effect in the "complier" subgroup who received treatment because of the value of the IV.
- However, primary analyses of MTO define the endogenous variable as moving from the development *with the voucher given by the trial.*
- In this definition of the treatment, it is impossible to be treated if you are not randomized to receive a voucher.
- Therefore, everyone who is "treated" is a "complier" and the IV effect estimate = effect of treatment on the treated (TOT)

Whose Causal Effect?

Response if assigned to receive a voucher:

Response if assigned to not receive a voucher:

	Don’t Move	Move
Don’t Move	Never-Takers	Compliers
Move	Contrarians/ Defiers	Always Takers

Whose Causal Effect?

Response if assigned to receive a voucher:

Response if assigned to not receive a voucher:		Don't Move	Move
	Don't Move	Never-Takers	Compliers
	Move	~~Contrarians/ Defiers~~	~~Always Takers~~

Early Results for Behavioral Problems,

Boston 2 Year Low Poverty Group vs Controls

	Control Mean	ITT Difference (SE)	TOT/IV Difference (SE)
Boys	.326	-.090	-.184
		(.041)	(.088)
Girls	.193	-.023	-.046
		(.030)	(.056)

From Katz QJE 2001

Mid-Term (5-7 year) Results for Children’s Mental Health (K6)

	Control Mean	ITT Difference (SE)	TOT/IV Difference (SE)
Boys	-.162	.069	.167
		(.091)	(.223)
Girls	.268	-.246	-.508
		(.091)	(.060)

Trial challenges

- Mixed effects, attributable to
  - Small samples?
  - Heterogeneous effects?
- Uncertainty about the salient component of the treatment
  - Social disruption associated with moving?
  - Changes in residential environment?
  - Changes in schooling?
- Who are the compliers?

Most of these issues arise whether you use IV or ITT to analyze the data

Example 1a:

- Causal question:

Does moving from very high poverty public housing developments benefit the health of mothers or their children?

We did a trial, but do you believe the results?

Can we get more evidence?

Voucher lottery

Jacob & Ludwig 2011

- We match mortality data to information on every child in public housing that applied for a housing voucher in Chicago in 1997(N=11,848).
- Families were randomly assigned to the voucher wait list, and only some families were offered vouchers.
- Families randomized to the voucher moved to census tracts with an average of 7 points lower poverty.

Jacob & Ludwig 2011

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Jacob & Ludwig 2011

- Treatment group= children whose families were assigned a waitlist number from 1 to 18,110, and so were offered a voucher by May 2003
- Control group = everyone assigned a higher lottery number.
- OLS with a person-quarter panel dataset for 1997:Q3 through 2005:Q4
- y_{it} measures child *i*'s outcome in quarter *t*, $PostOffer_{it}$ =1 if child *i*'s family was offered a voucher prior to *t*, else $PostOffer_{it} = 0$
- X =control variables (whether the family is offered a voucher some time *after* quarter *t*, gender, splines for baseline age (kinks at 1, 2, 5, 8 and 15) and calendar time (kinks every 6 calendar quarters). Clustered standard errors.

Jacob & Ludwig 2011

- ITT:

$$y_{it} = \alpha + \beta_1 (PostOffer_{it}) + \mathbf{X}\Gamma + \varepsilon_{it}$$

- IV:

$$Leased_{it} = \alpha + \theta_1 PostOffer_{it} + \mathbf{X}\Gamma + \gamma_t + \varepsilon_{it}$$

$$y_{it} = \alpha + \pi_1 \widehat{Leased}_{it} + \mathbf{X}\Gamma + \gamma_t + \varepsilon_{it},$$

IV analyses of a housing voucher lottery

		ITT	IV
Boys Only			
Death All Causes	100.32	60.44	203.88
		(-51.52, 172.4)	(-175.12, 582.92)
Girls Only			
Death All Causes	38.16	-40.88	-130.20
		(-65.28, -16.52)	(-207.6, -50.4)

Same analytic approach to natural experiment generated by a lottery and randomized experiment. Similar message re gender effect modification. Note large CIs.

Example 2, natural experiment based on policy change

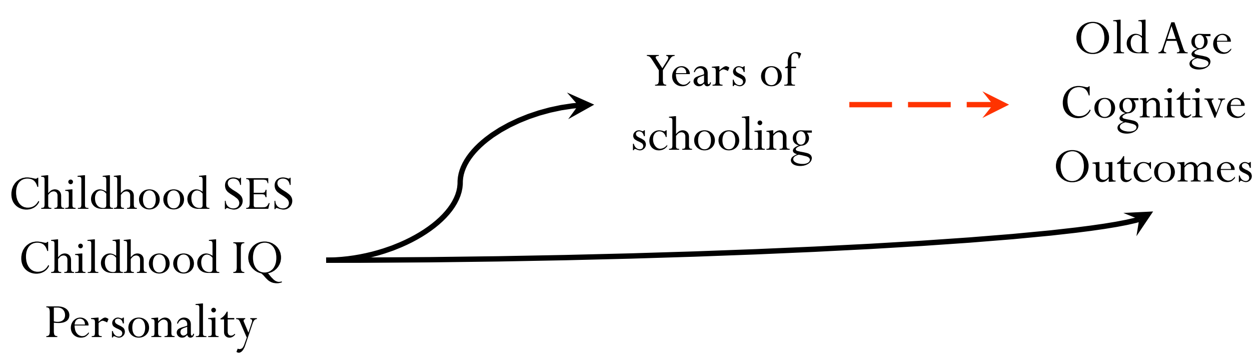
- Causal question:

Does completing additional years of education improve memory in old age?

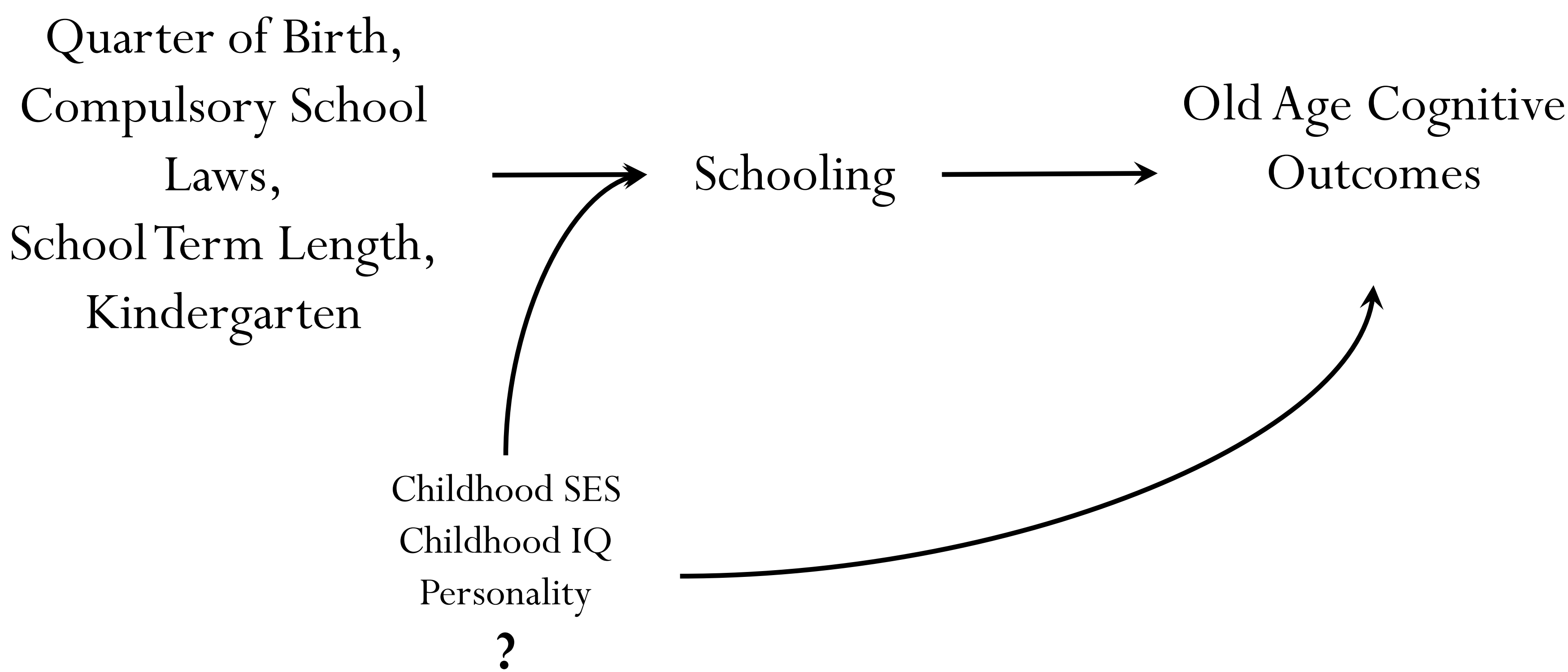
Substantive Question

Multiple studies show that years of education predicts old age cognitive function, cognitive change, and dementia.

Causality questionable.



Natural Experiments for Education



Natural Experiments:
UK Education Reform Effect on Education

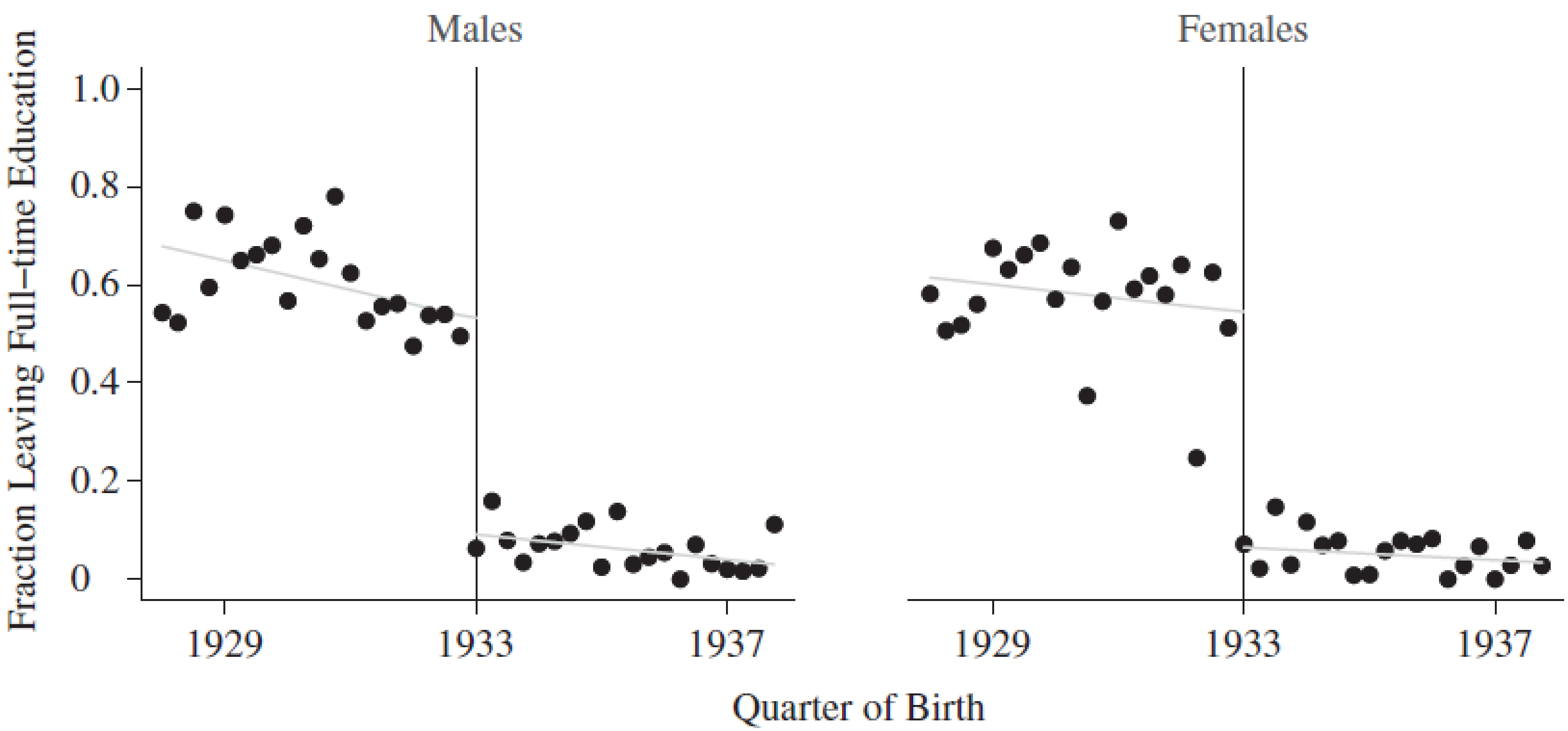
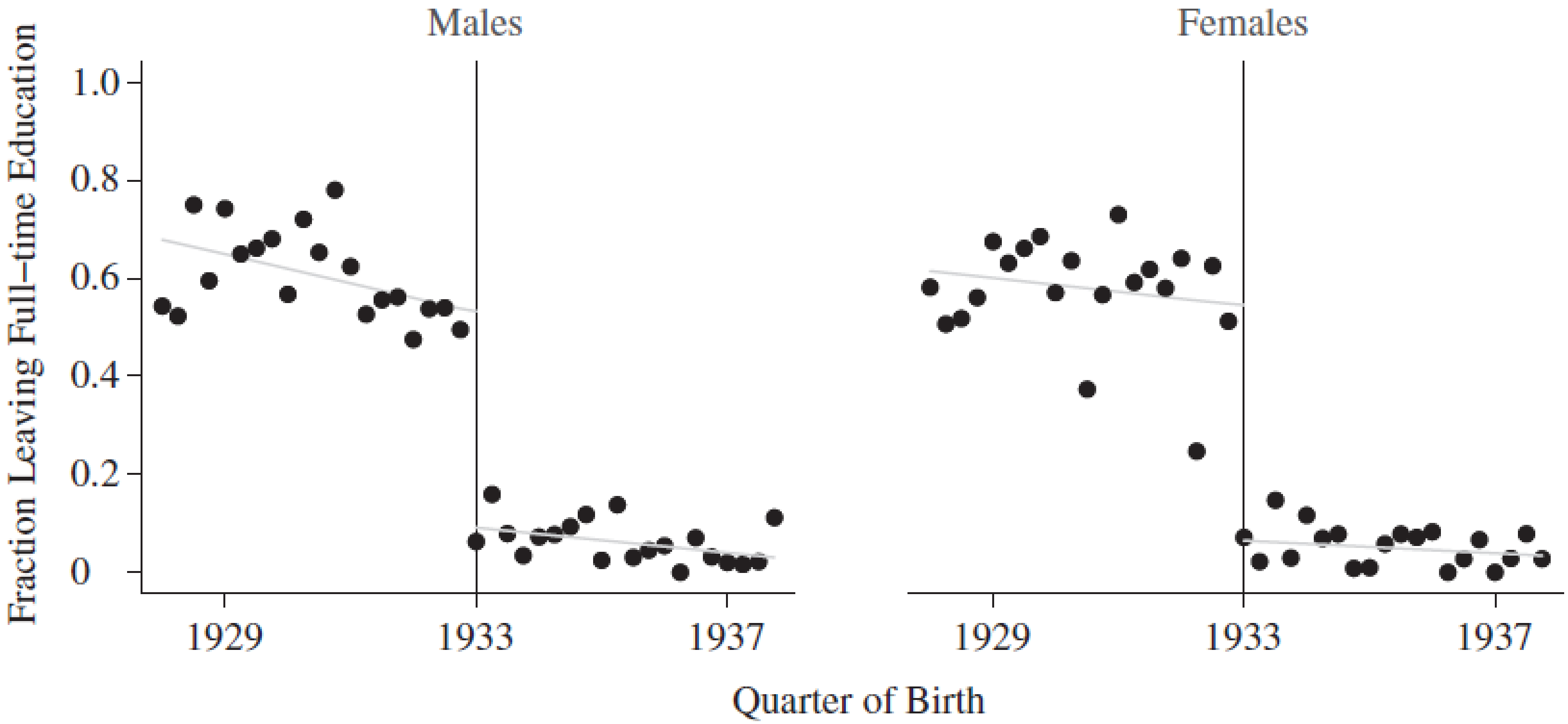


Fig. 1. *Effect of* 1947 *Reform on Fraction Leaving Full-time Education at or Before Age* 14

From Banks and Mazzona, 2012

Natural Experiments:
UK Education Reform Effect on Education

Reform had a powerful and immediate effect on about half the population of 14 years olds.



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From Banks and Mazzona, 2012

Natural Experiments:
IV Estimates for Education effect on EF

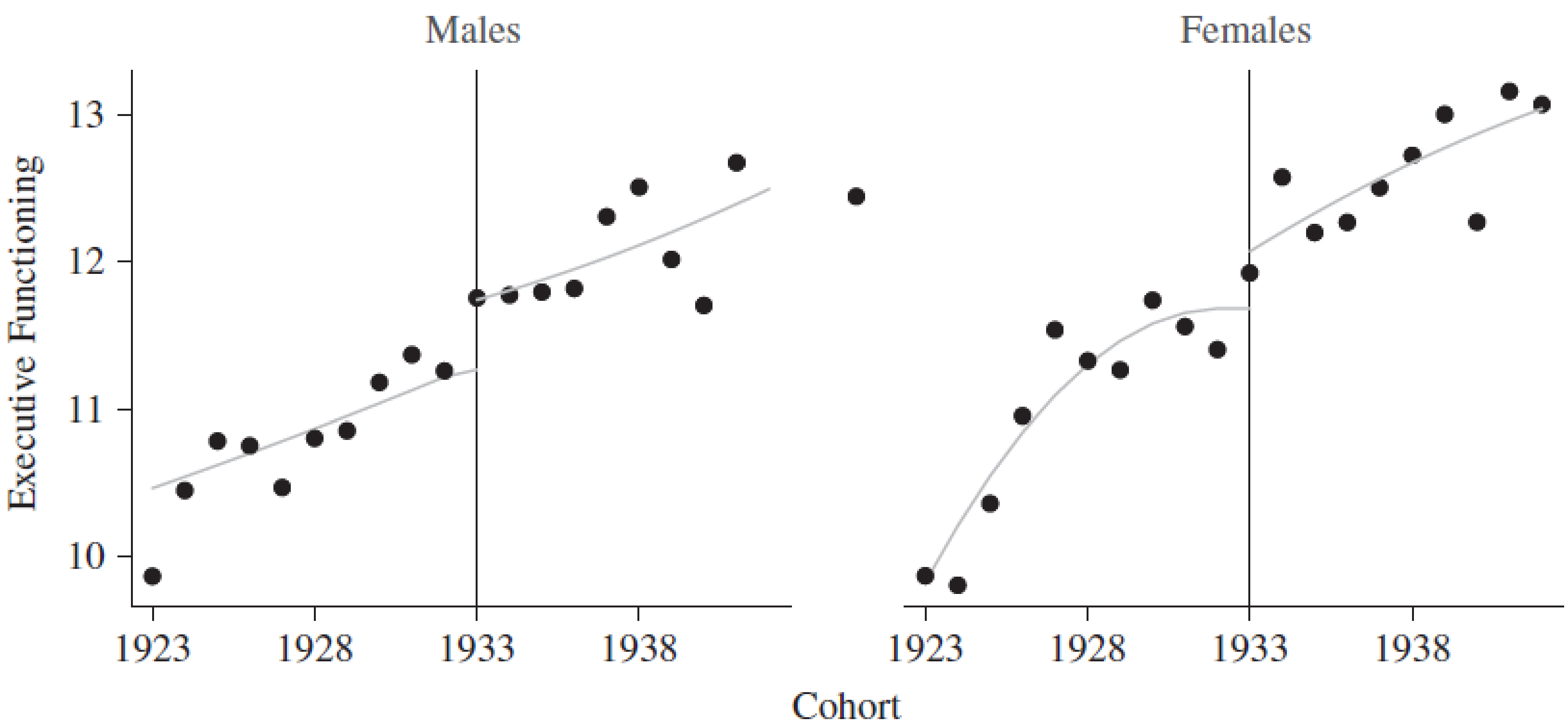


Fig. 9. *Effect of* 1947 *Reform on Executive Functioning* (*Conditional on Leaving Before* 16)

From Banks and Mazzona, 2012

Natural Experiments: IV Estimates for Education Effect on EF

Note sensitivity to model for temporal trends.

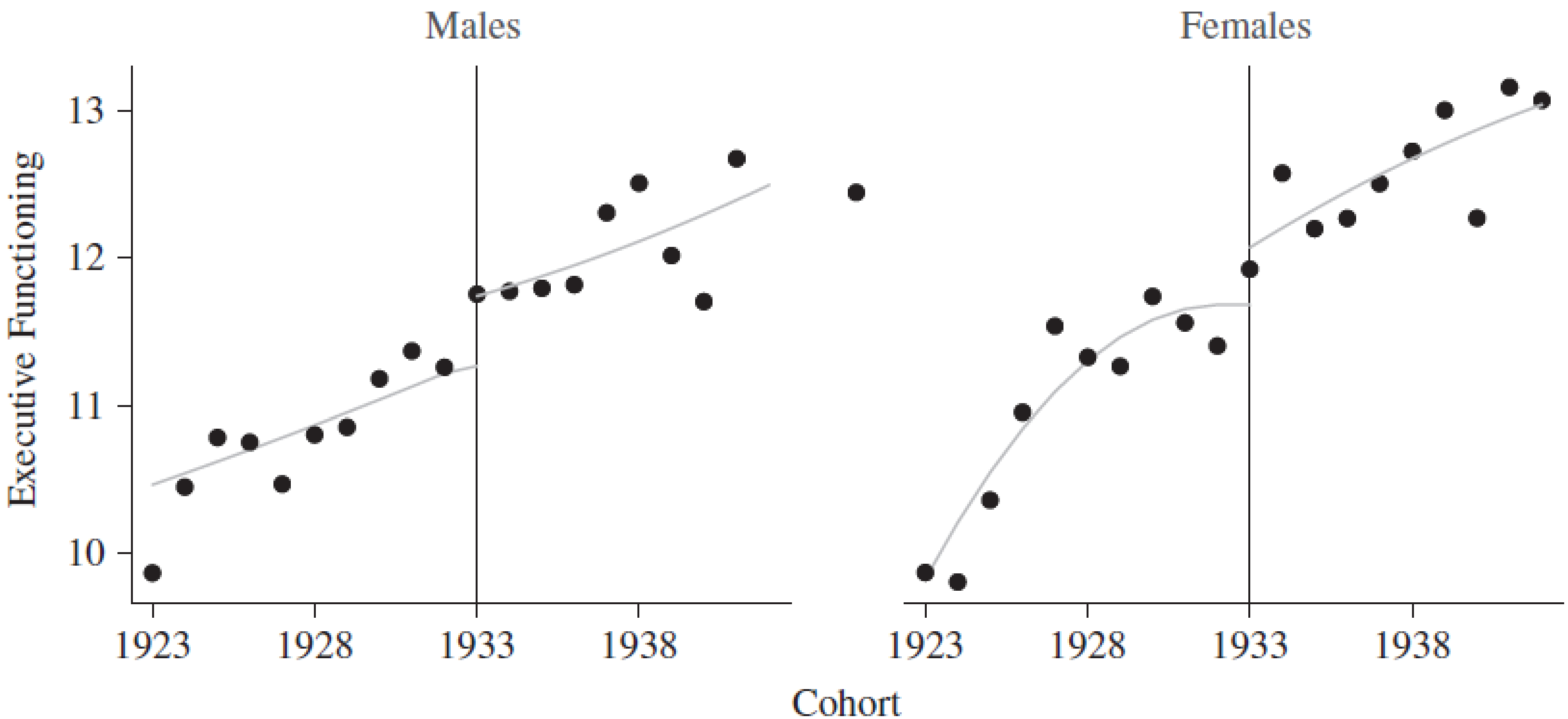


Fig. 9. *Effect of* 1947 *Reform on Executive Functioning* (*Conditional on Leaving Before* 16)

From Banks and Mazzona, 2012

Estimating the IV effect

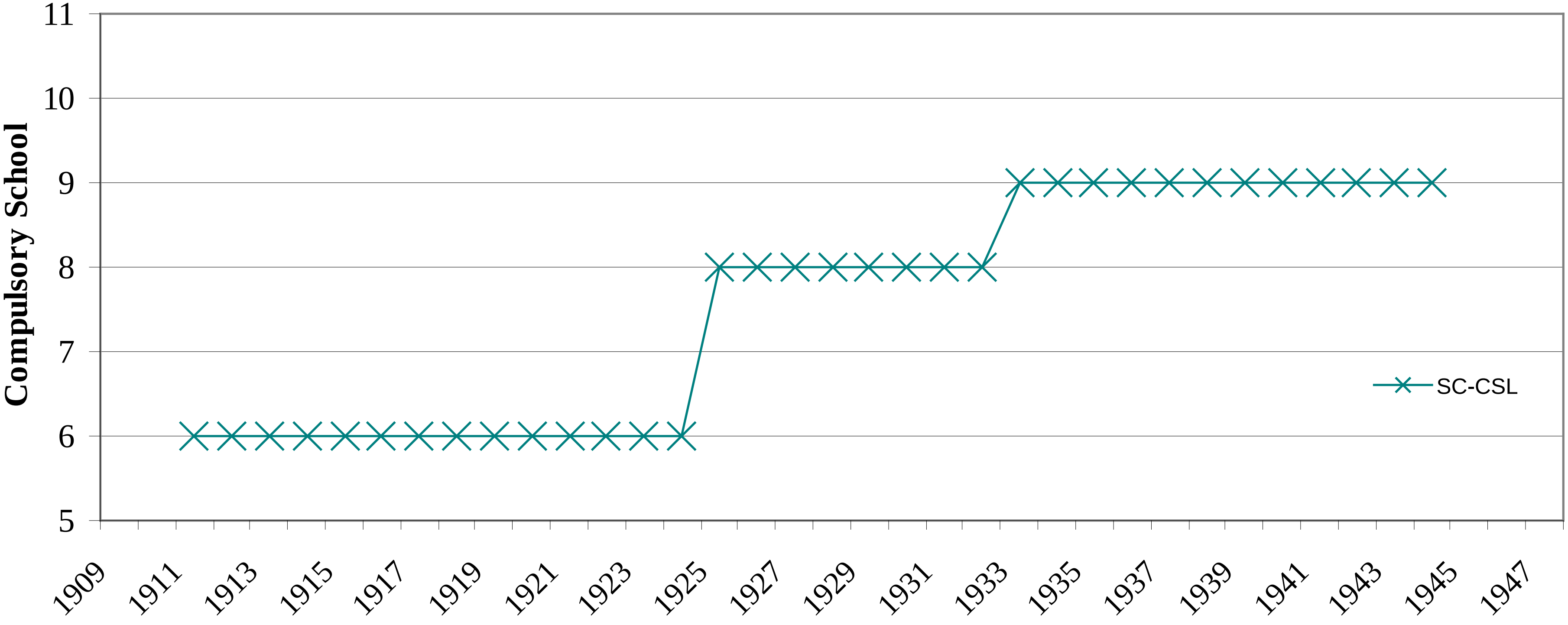
- Banks & Mazzona call this a "fuzzy regression discontinuity design" and estimate with 2SLS.

	Males			Females	
	Year band=1	Year band=3		Year band=1	Year band=3
Memory	.60 (.35)	.43 (.19)		.51 (.34)	.35 (.19)
Exec Fx	.64 (.36)	.37 (.19)		-.10 (.39)	.09 (.21)

IV Estimates Using US Policy Changes

- Banks and Mazzona replicated earlier findings in the US
- Advantage of the US context:
  - Education is decentralized, so there were more places that changed policies
  - Allows for better control of secular trends: you can rule out a sudden change in 1947.
- Disadvantage of the US context:
  - Effect of the laws was very small
  - Generally not well enforced, most people would have attended more school than required anyway
  - Complier group is small.

Early 20th Century CSL Changes



IV Analyses

- State schooling policies
  - Compulsory school to drop out (CSL) or receive a work-permit (CSL-W)
  - Based on policy in state of birth when school-age
  - 2-Sample least squares analysis
- Exposure (endogenous) variable:
  - Years of education (self-report)

Data Set: 1st Stage

- IPUMS (Census) 5% 1980 sample,
- Birth years 1900-1947
- Years of education linked to CSLs and CSL-Ws based on state of birth
- Link predictions from 1st stage regression model to individual data in the 2nd stage based on state of birth and all covariates.

Data Set: 2nd stage

- Health & Retirement Study, 1992-2000: panel enrollment by birth cohort (whites only due to evidence on enforcement)
- Cognitive assessments and state of birth on 21,041 individuals born 1900-1947
- CSLs and CSL-Ws

Two-Sample Least Squares

Sample 1:
5% Census sample.

CSLs in each state and year, 1906-1961.

Stage 1:
Regress education on CSLs, with other covariates.

Predicted education (Ê).

Sample 2:
HRS data.

Stage 2:
Regress health outcomes on Ê, with other stage 1 covariates. Regression coefficient for Ê is the IV effect estimate.

Covariates

1
- Unadjusted

- Sex

2
- Birthyear (indicators for every year)

- State of birth indicators

3

- State characteristics: age 6 % black, % urban, and % foreign born; age 14 manufacturing jobs per capita and wages per manufacturing job

4

Do the Instruments Predict Education?

First stage regression results (from IPUMS 5% sample)

	1. Unadjusted Model	**2. Birthyear and sex**	**3. Model 2 + state of birth**	**4. Model 3 + state condns**
CSLs	0.238 (0.236, 0.240)	0.110 (0.108, 0.112)	0.062 (0.059, 0.064)	0.037 (0.034, 0.040)
CSL-Ws	0.143 (0.146, 0.141)	-0.032 (-0.034, -0.029)	0.063 (0.060, 0.066)	0.044 (0.040, 0.048)
CSL-Ws UNR	-1.397 (-1.429, -1.365)	-0.282 (-0.315, -0.249)	-0.204 (-0.238, -0.17)	0.034 (0.000, 0.069)

How Strong is the 1st Stage?

	1. Unadjusted Model		2. Birthyear* and sex.		3. Model 2 + state of birth indicators		4. Model 3 + state characteristics#	
	β	95% CI	β	95% CI	β	95% CI	β	95% CI
Model r^2 without instrumental variables	0.0000		0.1080		0.1599		0.1626	
Model r^2 including instrumental variables	0.0465		0.1127		0.1613		0.1631	
Variance explained by instrumental variables	0.0465		0.0047		0.0014		0.0005	

Not technically "weak" instruments, but clear that a small violation of the IV assumptions could introduce a large amount of bias.

IV Estimates for Education: CSLs

Estimated effect of 1 year ed'n on cognitive test scores.

	Memory		Cognition	
Model covariates	β_{IV}	95% CI^	β_{IV}	95% CI^
1. Unadjusted	0.33	(0.27, 0.39)	0.19	(0.12, 0.26)
2. Birthyear, and sex	0.30	(0.14, 0.46)	0.34	(0.05, 0.63)
3. Model 2 + birth state	0.18	(0.02, 0.33)	0.03	(-0.22, 0.27)
4. Model 3 + state condns	0.34	(0.11, 0.57)	-0.06	(-0.37, 0.26)
5. OLS estimates	0.09	(0.08, 0.10)	0.15	(0.14, 0.16)

Evaluating Instruments

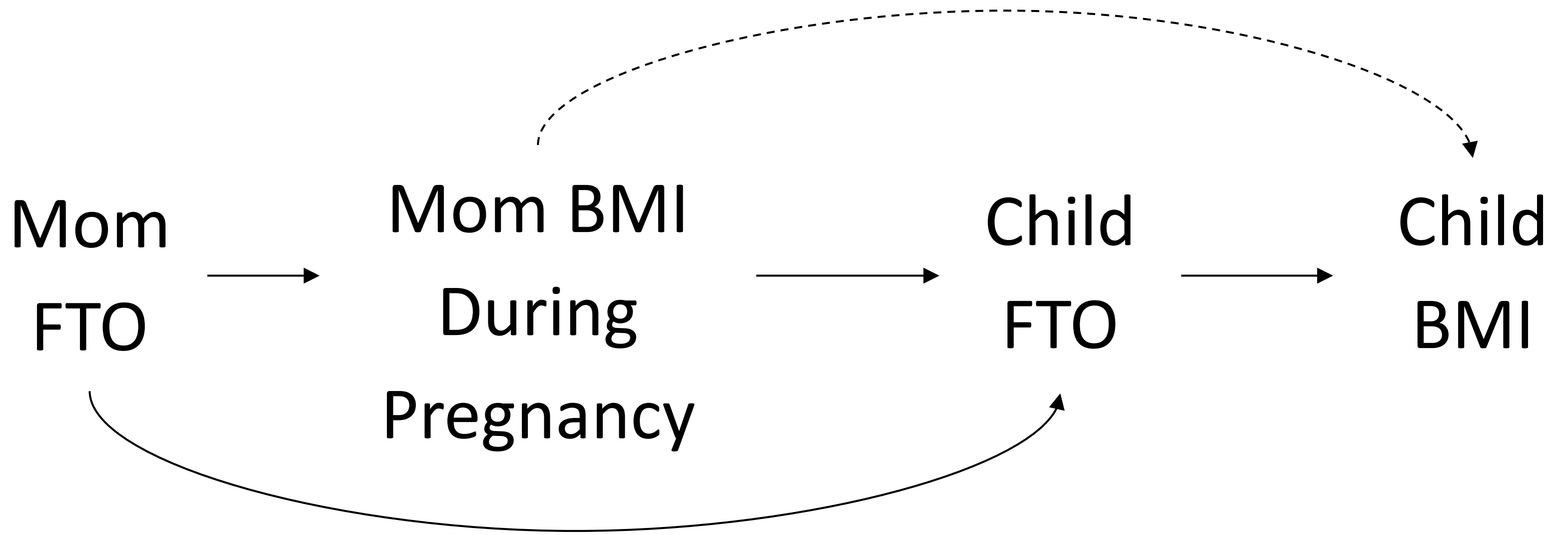
- Is the dependent variable independent of the instrument conditional on the endogenous variable?
- Over-identification tests, if you have multiple instruments
- Inequality constraints (for categorical endogenous variables)
- Evaluate the association between the instrument and the outcome across environments that modify the 1^{st} stage association

Sensitivity Analyses

- Including education >13 years
  - β_{IV} (memory, model 3): 0.15 (-0.01, 0.31)
- Restricting to education > 13 years
  - Instruments do not predict education or memory for individuals with >13 years of school
  - β_{IV} (memory, model 3): -1.04 (-3.70, 1.62)
- Inverse probability weighted for missing Memory (parental SES, self-report chronic condns at baseline)
  - β_{IV} (memory, model 3): 0.19 (0.03, 0.36)

Example 3: Maternal FTO as an IV for effect of mom's BMI on child's BMI

Goal was to test developmental overnutrition hypothesis: exposure during gestation affects child BMI



42

IV effect estimates for Maternal BMI on Offspring total fat mass

	OLS	IV	P-value for test of difference OLS vs IV
Total Fat Mass	0.26 (0.23, 0.29	-0.08 (-0.56, 0.41)	.17

From Lawlor PLoS Medicine 2008

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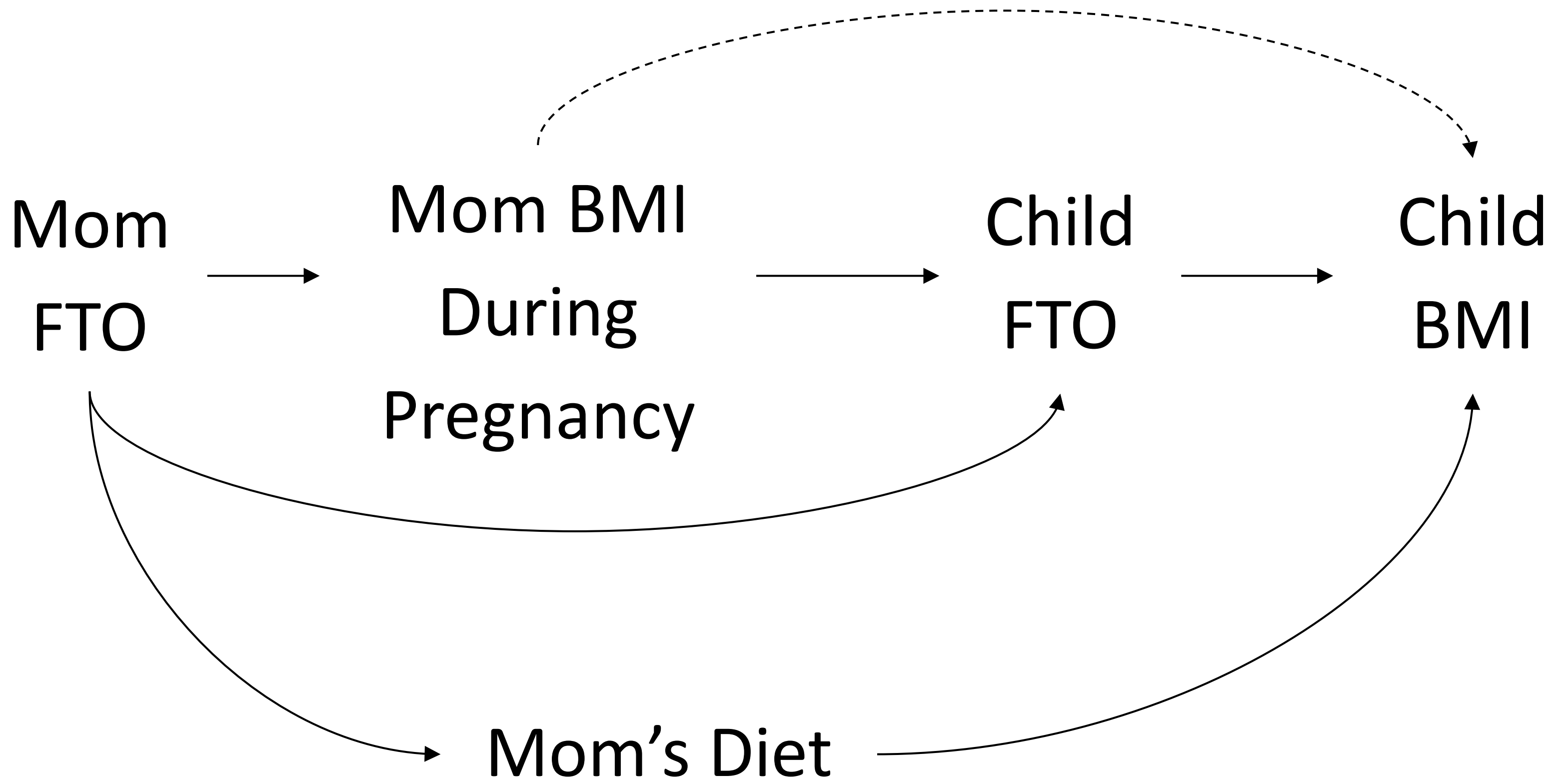

44

Doubting Instruments

- Do they have other pathways to the outcome?
  - *Quarter of birth*
- Is there a common cause of the instrument and the outcome?
  - *State of birth*
- Do they actually affect anyone's exposure?
  - *Tax policies*

Thinking of Instruments, Creating Instruments

- Often ecological
- Policy changes
- Policy discontinuities
- Differences in “expert” opinion
- Encouragement designs: randomize the incentive
- **Ask: What is the process that determines exposure? Is any part of this process arbitrary/random?**
- Content matter experts are very valuable team members

Conclusions

- Many important questions not convincingly answered with observational evidence
- Abandon the difficult questions? Or learn what we can from fraught methods?
- IV adds:
  - A way forward with observational data
  - Sometimes a parameter estimate of special interest
  - Pushes us to identify interventions that change exposures
- Not a replacement for evidence from observational research or RCTs, but a useful supplement

end