Lab 6: IV

Sylvan Zheng

2025-03-06

Plan

- IV Basics
- Weak Instruments and Practical Recommendations
- Nonparametrics and LATE
- Judge IV and Shift Share

IV Basics

- We worry that treatment (D) and outcome (Y) are confounded by unobservables > - But suppose we can measure an exogenous **instrument** (Z) that affects the treatment

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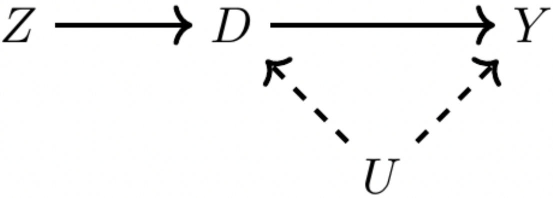

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  - First, regress the treatment on the instruments + controls to get $\hat{D}$
  - Second, regress the outcome on the $\hat{D}$
  - This yields $\frac{Cov(\tilde{Z},\tilde{Y})}{Cov(\tilde{Z},\tilde{D})}$

Manual 2SLS: Card (1995)

- Effect of schooling on wages uses student home proximity to college as an instrument for education

```
s1 <- feols(education ~ nearcollege + ethnicity + smsa + south + age, s
sr$educ_inst <- predict(s1)
s2 <- feols(logWage ~ educ_inst + ethnicity + smsa + south + age, sr)
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```

Dependent Variables: Model:	education (1)	logWage (2)
Variables		
nearcollegeyes	0.3380^{***} (0.1072)	
educ_inst		0.0926^{*} (0.0487)
Fit statistics		
Observations	3,010	3,010

IID standard-errors in parentheses
*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

IV with fixest, estimatr, ivreg

```r
# `estimatr` syntax: `y ~ x1 + x2 + ... + d | x1 + x2 + ... + z`
e.m <- estimatr::iv_robust(
  logWage ~ ethnicity + smsa + south + age + education |
    ethnicity + smsa + south + age + nearcollege,
  data = sr,
)

# `fixest` syntax: `y ~ x1 + x2 + ... | d ~ z`
f.m <- fixest::feols(
  logWage ~ ethnicity + smsa + south + age |
    education ~ nearcollege,
  data = sr
)

# `ivreg` syntax: `y ~ x1 + x2 + ... | d | z`
i.m <- ivreg::ivreg(
  logWage ~ ethnicity + smsa + south + age |
    education | nearcollege,
  data = sr
)
```

IV with fixest, estimatr, ivreg

```r
modelsummary(
  list(s2, e.m, f.m, i.m),
  keep = c("educ"), gof_map = NA, "latex"
)
```

	(1)	(2)	(3)	(4)
educ_inst	0.093			
	(0.049)			
education		0.093		0.093
		(0.050)		(0.051)
fit_education			0.093	
			(0.051)	

- Why are the SEs different?

Lal et al (2024)

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- (3) 2SLS estimates are inflated relative to OLS because of weak instruments
- Develop `ivDiag` package

IV Diag

```r
ivd <- ivDiag(
  data = sr,
  Y = "logWage",
  D = "education",
  Z = "nearcollege_num",
  controls = c("ethnicity", "smsa", "south", "age"),
  bootstrap = F
)
ivd$est_2sls %>% kable()
```

	Coef	SE	t	CI 2.5%	CI 97.5%	p.value
Analytic	0.0926	0.0503	1.8401	-0.006	0.1911	0.0658

Weak Instruments

- Suppose the exclusion restriction holds (eg, randomly assigned instrument), but extremely weak association with treatment

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- Suppose the exclusion restriction holds (eg, randomly assigned instrument), but extremely weak association with treatment
- Remember 2SLS estimate $\frac{Cov(\tilde{Z},\tilde{Y})}{Cov(\tilde{Z},\tilde{D})}$
- What happens if Z and D have a weak relationship?

Weak Instrument: Simulation

```r
n <- 1000 # Number of observations
beta_true <- 2 # True effect of the treatment on the outcome
simulate_2SLS <- function(strength) {
  # Correlated random data using mvrnorm
  sig <- matrix(c(1, strength, strength, 1), 2, 2)
  dat <- MASS::mvrnorm(n, mu = rep(0, 2), Sigma = sig)
  Z <- dat[, 1]
  D <- dat[, 2]
  Y <- D * beta_true + rnorm(n)
  ivreg(Y ~ D | Z)
}
```

Weak Instrument: Simulation

```r
set.seed(1) # For reproducibility
modelsummary(list(
  simulate_2SLS(0.02),
  simulate_2SLS(0.1),
  simulate_2SLS(0.6)
), gof_map = NA, keep = "D")
```

	(1)	(2)	(3)
D	3.273	1.724	1.978
	(3.294)	(0.209)	(0.049)

Testing for weak instruments

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```
fitstat(f.m, "ivf")
```

```
## F-test (1st stage), education: stat = 9.94613, p = 0.00
```

- F > 10 based on assumption of homoskedastic errors, hardly satisfied (Andrews, Stock & Sun 2019)

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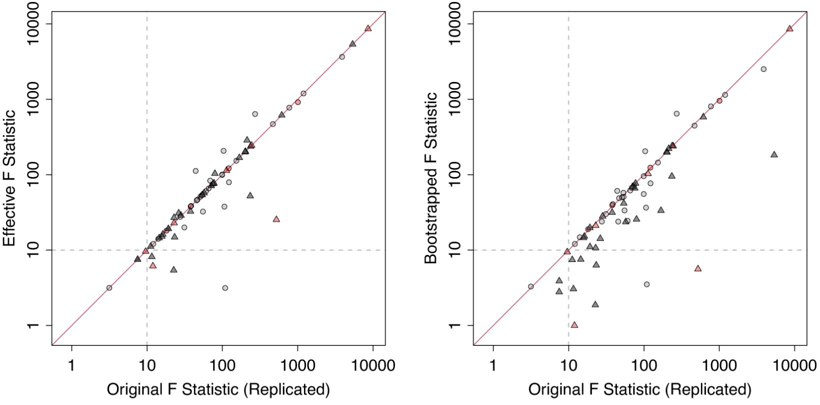
```
ivd$F_stat
```

```
##  F.standard    F.robust   F.cluster F.effective
##      9.9461      9.6316          NA      9.6316
```

Lal et al 2024 on Weak Instruments

- "To our surprise, among the 70 IV designs, 12 (17%) do not report the First-Stage Partial F-Statistic despite its key role in justifying the validity of an IV design.



(a) Original F vs. Effective F

(b) Original F vs. Bootstrapped F

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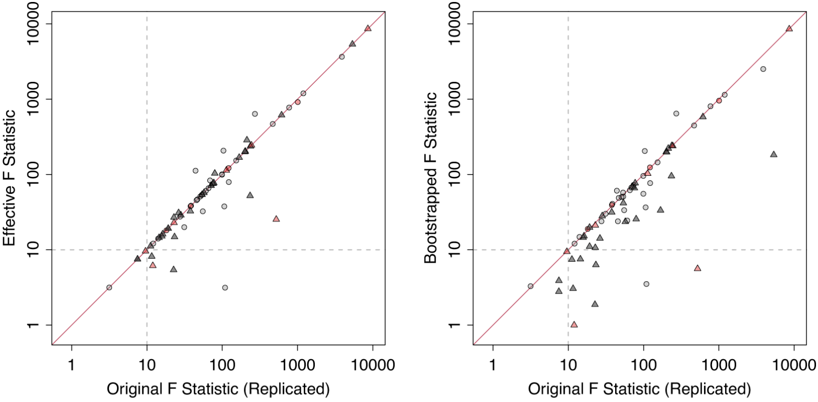
- “To our surprise, among the 70 IV designs, 12 (17%) do not report the First-Stage Partial F-Statistic despite its key role in justifying the validity of an IV design.
- Among the remaining, 16% use classic analytic SEs, thus not adjusting for potential heteroskedasticity or clustering structure.



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```
ivd$tF
```

```
##       F      cF    Coef      SE       t  CI2.5% CI97.5% p-val
##  9.6316  3.5058  0.0926  0.0503  1.8401 -0.0838  0.2689  0.30
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  - Robust to weak instruments

```
ivd$AR$ci.print
```

```
## [1] "[0.0000, 0.2660]"
```

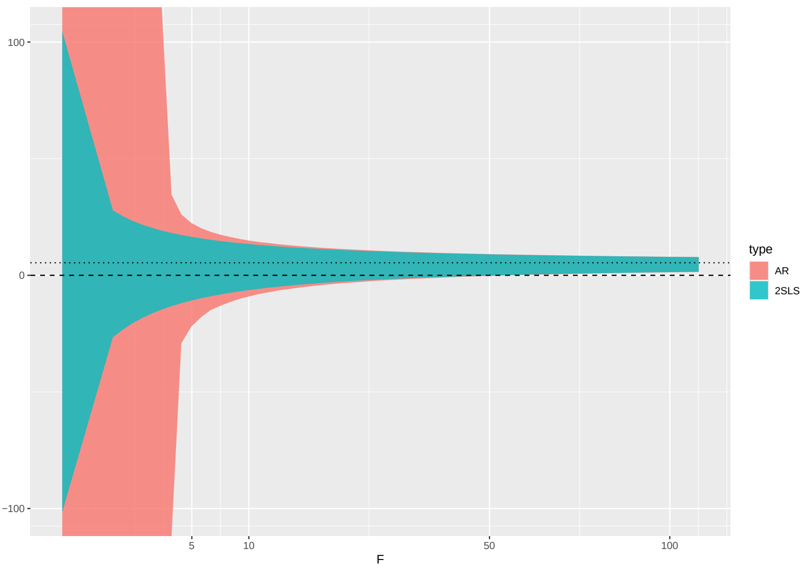
Simulation: Setup

```r
set.seed(1)
nums <- 1000
beta_true <- 0.5
simulate_2SLS <- function(strength) {
  # Correlated random data using mvrnorm
  sig <- matrix(c(1, strength, strength, 1), 2, 2)
  dat <- MASS::mvrnorm(nums, mu = rep(0, 2), Sigma = sig)
  Z <- dat[, 1]
  D <- dat[, 2]
  X <- rnorm(nums)
  # A little bit of misspecification / heteroskedasticity
  Y <- D * beta_true + rnorm(nums) + X * X
  ivDiag(
    data = data.frame(Y = Y, D = D, Z = Z, X = X),
    Y = "Y", D = "D", Z = "Z", controls = "X",
    bootstrap = F, parallel = F
  )
}
strengths <- c(seq(0.01, 0.1, 0.005), seq(0.1, 0.3, 0.01))
diags <- lapply(strengths, \(s) simulate_2SLS(s))
```

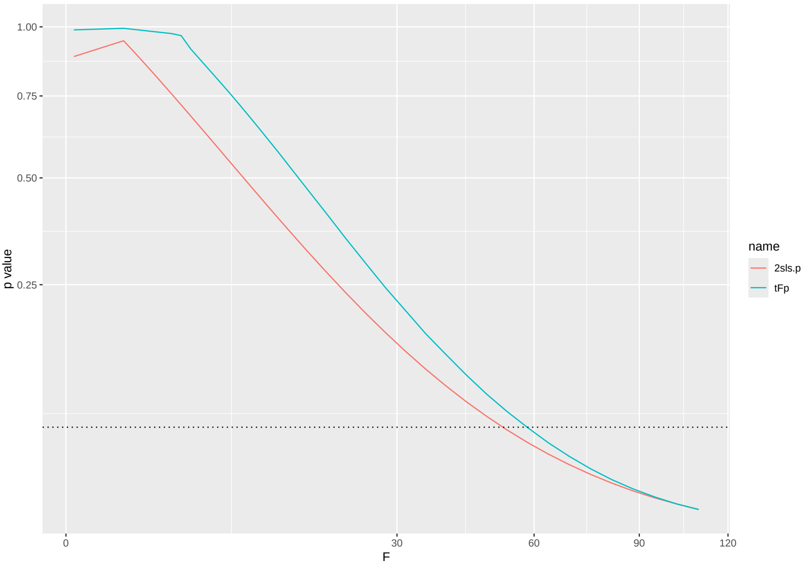
Simulation: Setup

```r
# Extract all coefficients
fs <- lapply(diags, \(d) list(
  "F" = d$F_stat["F.standard"],
  "est" = d$est_2sls[1],
  "2sls.p" = d$est_2sls[6],
  "tFp" = d$tF["p-value"],
  "AR.lo" = d$AR$ci[1],
  "AR.hi" = d$AR$ci[2],
  "2sls.lo" = d$est_2sls[4],
  "2sls.hi" = d$est_2sls[5]
)) %>% bind_rows()
```

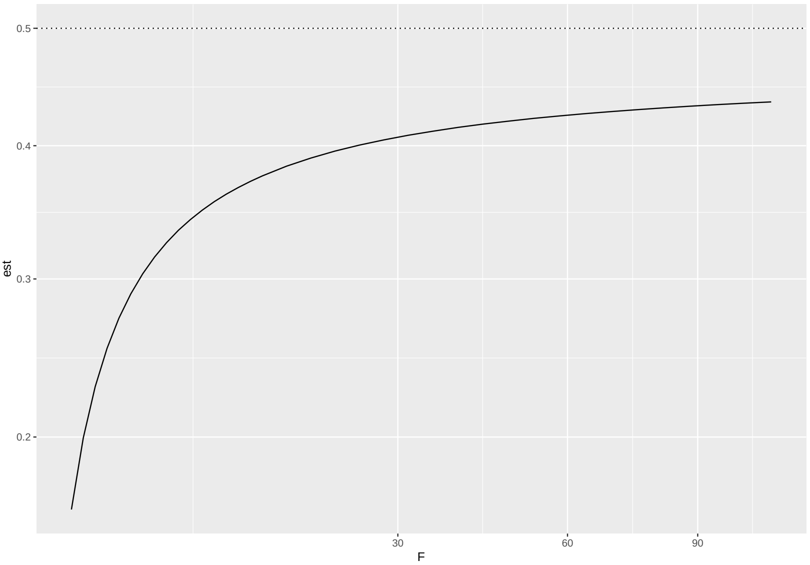
Simulation: AR vs 2SLS CIs by F



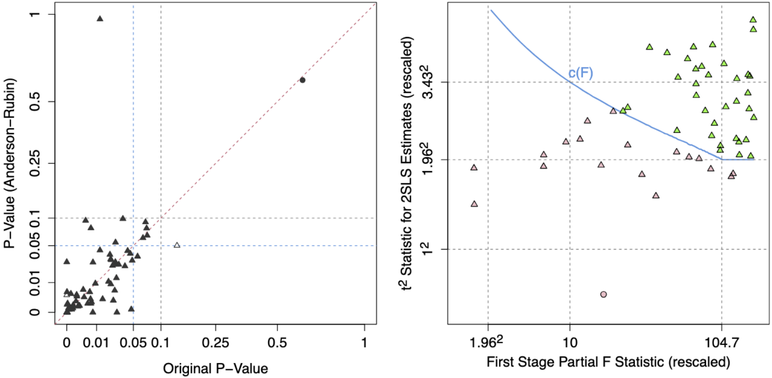
Simulation: tF vs 2SLS p values by F



Simulation: coefficient estimate by F



Lal et al 2024 on AR/tF



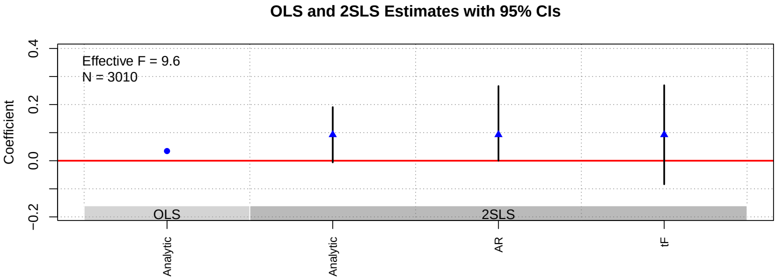
(c) Anderson-Rubin

(d) *tF* Procedure

AR/tF Takeaways

- Essential when effective F is small and especially when treatment effect is small

```r
plot_coef(ivd)
```



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- Essential when effective F is small and especially when treatment effect is small
- Use the `ivDiag` package. Also provides bootstrap CIs

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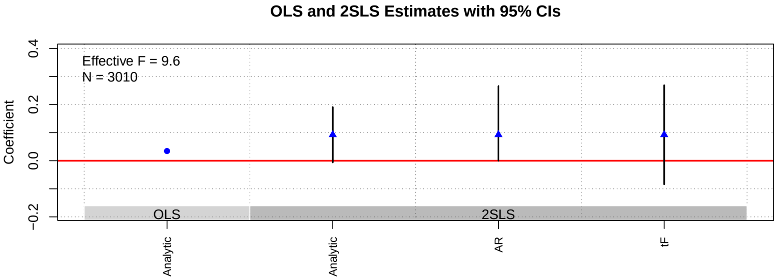

AR/tF Takeaways

- Essential when effective F is small and especially when treatment effect is small
- Use the `ivDiag` package. Also provides bootstrap CIs
- Plot all coefficients with `plot_coef`

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plot_coef(ivd)
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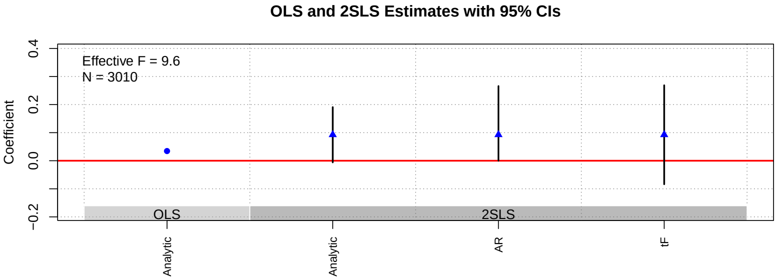

Nonparametric IV

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- Early justification for IV relied on parametric assumptions
  - eg, **homogenous treatment effect**
- However, IV can also be used in a nonparametric framework
- We allow treatment effects to vary at the individual level (potential outcomes model)

Compliance Framework

- Let Z_i be "treatment assignment"
- Let D_i be the "treatment received"
- Four types of units(or principal strata) in this setting:
  - Compliers: $D_i = Z_i$
  - Always-takers: $D_i = 1$. Z_i doesn't matter
  - Never-takers: $D_i = 0$. Z_i doesn't matter
  - Defiers $D_i = \ Z_i$.

Compliance Framework

- Even if we observe D and Z, we don't know for sure what strata the unit falls into

	$Z_i = 0$	$Z_i = 1$
$D_i = 0$	Never-taker or Complier	Never-taker or Defier
$D_i = 1$	Always-taker or Defier	Always-taker or Complier

LATE Theorem

Theorem: Under classic IV assumptions + no defiers:

$$\frac{E[Y_i|Z_i = 1] - E[Y_i|Z_i = 0]}{E[D_i|Z_i = 1] - E[D_i|Z_i = 0]} = E[Y_{1i} - Y_{0i}|D_{1i} > D_{0i}]$$

- On the right: average causal effect of the treatment among compliers

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- On the left: For binary Z, this is equivalent to IV/2SLS estimator!
- LATE theorem: the IV/2SLS estimator targets the average causal effect of the treatment among compliers

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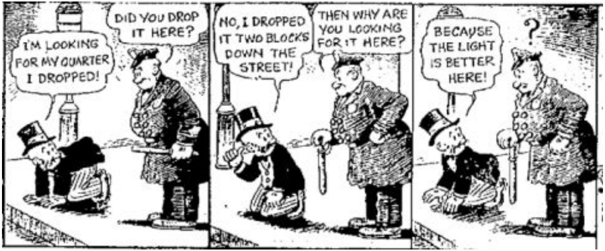
- On the right: average causal effect of the treatment among compliers
- On the left: For binary Z, this is equivalent to IV/2SLS estimator!
- LATE theorem: the IV/2SLS estimator targets the average causal effect of the treatment among compliers
- Proof in lab materials

Better LATE than never?

- Angrist, Imbens, and others: We don't get the ATT or ATE but we get something that still makes some sense (particularly for policy).



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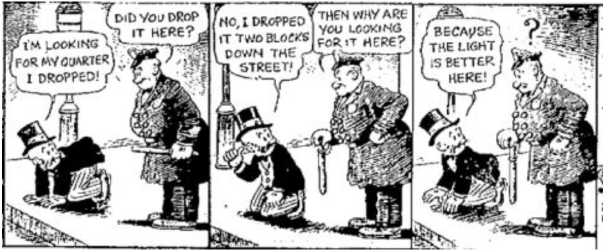
- Heckman, Deaton, and others: We don't get the ATT/ATE. How can we interpret LATE? How does the compliers framework transfer to observational settings?



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- Under parametric assumptions, IV can target the ATE/ATT.
  - If we assume that treatment effects are equal for everyone, then the LATE $==$ ATE $==$ ATT.
  - If we rule out always-takers, then LATE $==$ ATT

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- E.g., to hint at mechanisms, contextualize findings, etc

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- Then, compare these to baseline control outcomes $E[Y_i(0) | D_i = 0]$

Decomposing LATE (Angrist et al 2013)

- Angrist et al 2013 shows how urban charter schools perform better than non urban charter schools

	Urban				Nonurban			
Subject	Treatment effect (1)	$E_u[Y_0 \mid D=0]$ (2)	λ_0^u (3)	λ_1^u (4)	Treatment effect (5)	$E_n[Y_0 \mid D=0]$ (6)	λ_0^n (7)	λ_1^n (8)
Panel A. Middle school								
Math	0.483***	−0.399***	0.077	0.560***	−0.177**	0.236***	0.010	−0.143***
	(0.074)	(0.011)	(0.049)	(0.054)	(0.074)	(0.007)	(0.061)	(0.042)
N	4,858				2,239			
ELA	0.188***	−0.422***	0.118**	0.306***	−0.148***	0.260***	0.102**	−0.086***
	(0.064)	(0.012)	(0.054)	(0.049)	(0.048)	(0.007)	(0.050)	(0.030)
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- Abadie weights (Abadie 2003) and Marbach and Hainmueller (2020) for more on this

Illustration: Angrist, Hull, and Walters (2023)

	Compliers			Always-takers	Never-takers
	Untreated	Treated	Pooled		
	(1)	(2)	(3)	(4)	(5)
Female	0.506	0.510	0.508	0.539	0.463
	(0.023)	(0.021)	(0.016)	(0.024)	(0.017)
Black	0.401	0.380	0.390	0.623	0.490
	(0.022)	(0.021)	(0.016)	(0.023)	(0.017)
Hispanic	0.250	0.300	0.275	0.183	0.228
	(0.02)	(0.018)	(0.013)	(0.019)	(0.014)
Asian	0.022	0.024	0.023	0.004	0.024
	(0.007)	(0.005)	(0.004)	(0.003)	(0.005)
White	0.229	0.216	0.223	0.154	0.215
	(0.018)	(0.016)	(0.012)	(0.016)	(0.014)
Special education	0.190	0.181	0.186	0.158	0.177
	(0.018)	(0.016)	(0.012)	(0.018)	(0.013)
English language learner	0.143	0.148	0.145	0.054	0.088
	(0.015)	(0.013)	(0.010)	(0.011)	(0.010)
Subsidized lunch	0.689	0.705	0.697	0.698	0.666
	(0.021)	(0.019)	(0.014)	(0.022)	(0.016)
Baseline math score	−0.274	−0.312	−0.293	−0.394	−0.301
	(0.047)	(0.041)	(0.032)	(0.045)	(0.036)
Baseline English score	−0.352	−0.349	−0.350	−0.362	−0.299
	(0.050)	(0.043)	(0.033)	(0.046)	(0.038)
Share of sample			0.546	0.197	0.257

Illustration: Gerber, Karlan, and Bergan (2009)

American Economic Journal: Applied Economics 2009, 1:2, 35–52
http://www.aeaweb.org/articles.php?doi=10.1257/app.1.2.35

Does the Media Matter? A Field Experiment Measuring the Effect of Newspapers on Voting Behavior and Political Opinions†

By ALAN S. GERBER, DEAN KARLAN, AND DANIEL BERGAN*

We conducted a field experiment to measure the effect of exposure to newspapers on political behavior and opinion. Before the 2005 Virginia gubernatorial election, we randomly assigned individuals to a Washington Post *free subscription treatment, a* Washington Times *free subscription treatment, or a control treatment. We find no effect of either paper on political knowledge, stated opinions, or turnout in post-election survey and voter data. However, receiving either paper led to more support for the Democratic candidate, suggesting that media slant mattered less in this case than media exposure. Some evidence from voting records also suggests that receiving either paper led to increased 2006 voter turnout.* (*JEL* D72, L82)

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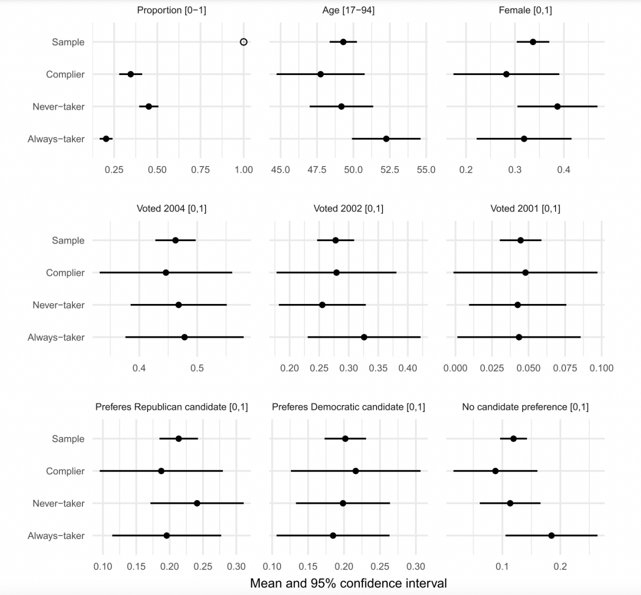

Constructed IVs (Judge, Shift Share)

- IV doesn't have to be a directly observed variable
- Some creative designs **construct** the IV

Judge IV

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- With this, have enough for IV. Need to use leave-one-out jacknife estimation in the first stage.
- Other settings: startup accelerators, admissions committees, police/emergency service dispatch . . .

Shift-Share IV aka Bartik Instrument

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  - Intuitively, "previous expected exposure to China imports"

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  - Intuitively, anything correlated with China shock exposure and employment not controlled for by “previous expected exposure” violates this assn.
  - Eg, increase in automation

Math appendix

Weak Instruments

The probability limit of the IV estimator is given by:

$$\text{plim } \hat{\alpha}_{IV} = \frac{\text{Cov}[Y, Z]}{\text{Cov}[Z, D]} + \frac{\text{Cov}[Z, u_2]}{\text{Cov}[Z, D]} = \alpha_D + \frac{\text{Cov}[Z, u_2]}{\text{Cov}[Z, D]}$$

The second term is non-zero if the instrument is not exogenous. Let $\sigma^2_{u_1}$ be the variance of the first stage error and F be the F statistic of the first-stage. Then, the bias in IV is

$$\text{E}[\hat{\alpha}_{IV} - \alpha] = \frac{\sigma_{u_1 u_2}}{\sigma^2_{u_2}} \left(\frac{1}{F + 1} \right)$$

If the first stage is weak, the bias approaches $\frac{\sigma_{u_1 u_2}}{\sigma^2_{u_2}}$. As F approaches infinity, B_{IV} approaches zero.

Abadie's Kappa (2003)

Suppose assumptions of LATE theorem hold conditional on covariates X. Let $g(\cdot)$ be any measurable real function of Y, D, X with finite expectation. We can show that the expectation of g is a weighted sum of the expectation in the three groups

$$E[g|X] = \underbrace{E[g|X, D_1 > D_0]Pr(D_1 > D_0|X)}_{\text{Compliers}} +$$

$$+ \underbrace{E[g|X, D_1 = D_0 = 1]Pr(D_1 = D_0 = 1|X)}_{\text{Always Takers}}$$

$$+ \underbrace{E[g|X, D_1 = D_0 = 0]Pr(D_1 = D_0 = 0|X)}_{\text{Never Takers}}$$

Abadie's Kappa (2003)

Rearranging terms gives us then,

$$E[g(Y,D,X)|D_1 > D_0] = \frac{E[k \cdot g(Y,D,X)]}{Pr(D_1 > D_0)} = \frac{E[k \cdot g(Y,D,X)]}{E[k]}$$

where

$$k_i = \frac{D(1-Z)}{1-Pr(Z=1|X)} - \frac{(1-D)Z}{Pr(Z=1|X)}$$

- This result can be applied to *any characteristic or outcome and get its mean for compliers by removing the means for never and always takers*.
- Standard example: average covariate value among compliers: $E[X|D_1 > D_0] = \frac{E[kX]}{E[k]}$

LATE proof

- Canonical IV assumptions for Z_i to be a valid instrument:
  1. Randomization of Z_i
  2. Presence of some compliers $\pi_{co} \neq 0$ (first-stage)
  3. Exclusion restriction $Y_i(z,d) = Y_i(z',d)$
  4. Monotonicity: $D_i(1) \geq D_i(0)$ for all i (no defiers)
- Let π_{co}, π_{at}, π_{nt}, and π_{de} be the proportions of each type.
- Implies ITT effect on treatment equals proportion compliers: $ITT_D = \pi_{co}$
- Implies ITT for the outcome has the same interpretation:

$$ITT_Y = ITT_{Y,co}\pi_{co} + \underbrace{ITT_{Y,at}}_{=0\text{ (ER)}}\pi_{at} + \underbrace{ITT_{Y,nt}}_{=0\text{ (ER)}}\pi_{nt} + ITT_{Y,de}\underbrace{\pi_{de}}_{=0\text{ (mono)}}$$

$$= ITT_{Y,co}\pi_{co}$$

- $\approx$ same identification result: $\tau_{LATE} = \frac{ITT_Y}{ITT_D}$

LATE Theorem

Theorem: Under assumptions 1 - 4:

$$\frac{E[Y_i|Z_i = 1] - E[Y_i|Z_i = 0]}{E[D_i|Z_i = 1] - E[D_i|Z_i = 0]} = E[Y_{1i} - Y_{0i}|D_{1i} > D_{0i}]$$

Proof.

Start with the first bit of the Wald estimator:

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

LATE Theorem

Proof.

Similarly

$$E[Y_i|Z_i = 0] = E[Y_{0i} + (Y_{1i} - Y_{0i})D_{0i}]$$

So the numerator of the Wald estimator is

$$E[Y_i|Z_i = 1] - E[Y_i|Z_i = 0] = E[(Y_{1i} - Y_{0i})(D_{1i} - D_{0i})]$$

Monotonicity means $D_{1i} - D_{0i}$ equals one or zero, so

$$E[(Y_{1i} - Y_{0i})(D_{1i} - D_{0i})] = E[Y_{1i} - Y_{0i}|D_{1i} > D_{0i}]P[D_{1i} > D_{0i}].$$

A similar argument shows

$$E[D_i|Z_i = 1] - E[D_i|Z_i = 0] = P[D_{1i} > D_{0i}].$$