Instrumental Variables in Action: Sometimes You get What You Need

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Introduction

Our Causal Framework

- A dummy causal variable of interest, D_i, is called a *treatment*, by analogy with clinical trials
  - *Treatment effects* can be related to health, but also to education, the labor market, and a wide range of public policies
- Potential outcomes, Y_{0i} and Y_{1i}, describe what happens under alternative treatment assignments
- We assume these are meaningful even though we only ever get to see one of them (imagine they would be revealed by a randomized trial)
- We observe

$$\text{Y}_i = \text{Y}_{0i} + (\text{Y}_{1i} - \text{Y}_{0i})\text{D}_i$$

- Covariates are denoted by the vector, X_i
- *Instrumental variables*, denoted by Z_i, provide leverage for causal inference when treatment is not randomly assigned

Our Constant-Effects Benchmark

- The traditional IV setup is a linear, constant-effects world
- With Bernoulli (dummy) treatment, we have

$$\begin{aligned} Y_{0i} &= \alpha + \eta_i \\ Y_{1i} - Y_{0i} &= \rho \\ Y_i &= Y_{0i} + D_i(Y_{1i} - Y_{0i}) = \alpha + \rho D_i + \eta_i \end{aligned}$$

- OLS is biased because D_i and η_i are correlated
- An instrument, Z_i, independent of Y_{0i} and correlated with D_i, solves the OVB problem:

$$\rho = \frac{Cov(Y_i, Z_i)}{Cov(D_i, Z_i)} = \frac{Cov(Y_i, Z_i)/V(Z_i)}{Cov(D_i, Z_i)/V(Z_i)} = \frac{RF}{1st}$$

- Example: draft-lottery estimates of the effects of Vietnam-era service . . . but are these effects constant?
- We'll shortly consider the fact that it's a heterogeneous world: $Y_{1i} - Y_{0i}$ is not the same for everyone

Sometimes You Get What You Need

- In a *design-based* framework, observational data are viewed "as if" from a randomized trial
- *Internal* and *external validity:*
  - A good instrument captures an *internally valid* causal effect: the (average) impact on a group subject to treatment manipulation
  - The *external validity* of this effect is it's predictive value in populations other than the one for which the experiment is observed
- Examples
  - Draft-lottery estimates of the effects of Vietnam-era military service
  - Quarter-of-birth estimates of the effects of schooling on earnings
  - Regression-discontinuity estimates of the effects of class size
- In each of these examples, IV captures causal effects for a well-defined subpopulation (a subset of the treated)
- With variable treatment intensity, we get effects over a limited (but knowable) range

Roadmap

1. An example: the effect of childbearing on mothers' labor supply
   - Two good instruments, two good answers
2. The theory of instrumental variables with heterogeneous potential outcomes
   - Notation and framework
   - The LATE Theorem
3. Implications for the design and analysis of field trials
   - The Bloom Result
   - Illustration: JTPA and MDVE
4. Average causal response in models with variable treatment intensity [time-permitting]
   - The ACR theorem and weighting function
   - A few more examples

Children and Their Parents Labor Supply

- A causal model for the impact of more than two children

$$\text{Y}_i = \text{Y}_{0i} + \text{D}_i(\text{Y}_{1i} - \text{Y}_{0i}) = \alpha + \rho\text{D}_i + \eta_i$$

- Dependent variables = employment, hours worked, weeks worked, earnings
    - $D_i = 1[kids > 2]$ in families with at least two children
    - Z_i = twins or same-sex sibship at second birth
- With a Bernoulli instrument and no covariates, IV is Wald:

$$\begin{aligned} \rho &= \frac{Cov(Y_i, Z_i)/V(Z_i)}{Cov(D_i, Z_i)/V(Z_i)} = \frac{RF}{1st} \\ &= \frac{E[\text{Y}_i|\text{Z}_i = 1] - E[\text{Y}_i|\text{Z}_i = 0]}{E[\text{D}_i|\text{Z}_i = 1] - E[\text{D}_i|\text{Z}_i = 0]} \end{aligned}$$

- **Results**

IV with Heterogeneous Potential Outcomes

The LATE Framework

- Let $\mathrm{Y}_i(d, z)$ denote the potential outcome of individual i were this person to have treatment status $\mathrm{D}_i = d$ and instrument value $\mathrm{Z}_i = z$.
- Note the double-indexing: candidate instruments *might* have a direct effect on outcomes
- We assume, however, that IV initiates a causal chain: the instrument, Z_i, affects D_i, which in turn affects Y_i.
- To flesh this out, we first define *potential treatment status,* indexed against Z_i
  - D_{1i} is i's treatment status when $\mathrm{Z}_i = 1$
  - D_{0i} is i's treatment status when $\mathrm{Z}_i = 0$
- The first link in the chain is observed treatment status:

$$\mathrm{D}_i = \mathrm{D}_{0i} + (\mathrm{D}_{1i} - \mathrm{D}_{0i})\mathrm{Z}_i$$

- The causal effect of Z_i on D_i is $\mathrm{D}_{1i} - \mathrm{D}_{0i}$; we assume there is one

LATE assumptions (Independence)

Independence. The instrument is as good as randomly assigned:

$$[\{Y_i(d,z); \forall\ d,z\}, D_{1i}, D_{0i}] \amalg Z_i$$

- Independence says that draft lottery numbers are independent of potential outcomes and potential treatments
- Independence means the **first-stage** is the average causal effect of Z_i on D_i:

$$\begin{aligned} E[D_i|Z_i=1] - E[D_i|Z_i=0] &= E[D_{1i}|Z_i=1] - E[D_{0i}|Z_i=0] \\ &= E[D_{1i} - D_{0i}], \end{aligned}$$

- Independence is sufficient for a causal interpretation of the **reduced form**. Specifically,

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

- The reduced form is the causal effect of the instrument on the dependent variable, but we have yet to link this to treatment

LATE assumptions (Exclusion)

Exclusion. The instrument affects Y_i only through D_i:

$$
\begin{aligned}
Y_{1i} &\equiv Y_i(1,1) = Y_i(1,0); \\
Y_{0i} &\equiv Y_i(0,1) = Y_i(0,0).
\end{aligned}
$$

- The exclusion restriction means Y_i can be written:

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

  for Y_{1i} and Y_{0i} that satisfy the independence assumption

- The exclusion restriction says that draft lottery numbers affect earnings only through veteran status; sex composition affects labor supply only through family size
- Exclusion takes us from RF causal effects to treatment effects

LATE assumptions (Monotonicity)

A necessary technical assumption:

Monotonicity. $\mathrm{D}_{1i} \geq \mathrm{D}_{0i}$ for everyone (or vice versa).

- By virtue of monotonicity, $E\left[\mathrm{D}_{1i} - \mathrm{D}_{0i}\right] = P\left[\mathrm{D}_{1i} > \mathrm{D}_{0i}\right]$
- Interpreting monotonicity in latent-index models:

$$\mathrm{D}_i = \begin{cases} 1 & \text{if } \gamma_0 + \gamma_1 \mathrm{Z}_i > v_i \\ 0 & \text{otherwise} \end{cases},$$

  where v_i is a random factor.

- This latent-index model characterizes potential treatment assignments as:

$$\begin{aligned} \mathrm{D}_{0i} &= 1[\gamma_0 > v_i] \\ \mathrm{D}_{1i} &= 1[\gamma_0 + \gamma_1 > v_i], \end{aligned}$$

  which clearly satisfy monotonicity

The LATE Theorem

Recap:

- The independence assumption is sufficient for identification of a causal effect of the *instrument*
- The exclusion restriction means that the causal effect of the instrument on the dependent variable is due solely to the effect of the instrument on D_i.
  - Exclusion is (or should be) more controversial than independence
- We also assume there is a first-stage; by virtue of monotonicity, this is the proportion of the population for which D_i is changed by Z_i

- Given these assumptions, we have:

  THE LATE THEOREM.

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

- Proof (See MHE 4.4.1)

The Compliant Subpopulation

- LATE *compliers* are subjects with $D_{1i} > D_{0i}$
- This language comes from randomized trials where Z_i is treatment assigned and D_i is treatment received (more on this soon)

- LATE assumptions partition the world:
  - Compliers $D_{1i} > D_{0i}$
  - Always-takers $D_{1i} = D_{0i} = 1$
  - Never-takers $D_{1i} = D_{0i} = 0$

- IV is uninformative for always-takers and never-takers because treatment status for these types is unchanged by the instrument (just as panel models with fixed effects capture effects only for "changers")
- Of course, we can assume effects are the same for all three groups (this is the constant-effects model)

The Compliant Subpopulation (cont.)

- From the fact that

$$\text{D}_i = \text{D}_{0i} + (\text{D}_{1i} - \text{D}_{0i})\text{Z}_i,$$

  we learn that:
  $\{\text{D}_i = 1\} = \{\text{D}_{0i} = \text{D}_{1i} = 1\} \cup \{\{\text{D}_{1i} - \text{D}_{0i} = 1\} \cap \{\text{Z}_i = 1\}\}$
- In words: {treated} = {always-takers} + {compliers assigned $\text{Z}_i = 1$}
- TOT is a weighted average of effects on always-takers and compliers
- Latent-index example:

$$\text{D}_i = 1[\gamma_0 + \gamma_1 \text{Z}_i > v_i],$$

  where v_i is correlated with potential outcomes but indep. of Z_i.
- Compliers have $\gamma_0 + \gamma_1 > v_i > \gamma_0$, so

$$E[\text{Y}_{1i} - \text{Y}_{0i} | \text{D}_{1i} > \text{D}_{0i}] = E[\text{Y}_{1i} - \text{Y}_{0i} | \gamma_0 + \gamma_1 > v_i > \gamma_0]$$

- This usually differs from TOT

IV in Randomized Trials

The *compliance problem* in RCTs: Not all those randomly assigned to the treatment group are treated

- When compliance is voluntary, an *as-treated* analysis is contaminated by selection bias
- *Intention-to-treat* analyses preserve independence but are diluted by non-compliance
- IV solves this problem: Z_i, is a dummy variable indicating random assignment to the treatment group; D_i is a dummy indicating whether treatment was actually received
- There are no always-takers (no controls treated), so LATE = TOT

THE BLOOM RESULT

$$\frac{E[Y_i|Z_i = 1] - E[Y_i|Z_i = 0]}{E[D_i|Z_i = 1]} = \frac{\text{ITT effect}}{\text{compliance rate}} = E[Y_{1i} - Y_{0i}|D_i = 1]$$

- Direct proof (Bloom, 1984; See MHE 4.4.3).

Bloom Example 1: Training

The Job Training Partnership Act (JTPA) included a large randomized trial to evaluate the effect of training on earnings

- The JTPA *offered* treatment randomly; participation was voluntary
- Roughly 60 percent of those offered training received it
- The IV setup is
  - D_i indicates those who received JTPA services
  - Z_i indicates the random offer of treatment
  - Y_i is earnings in the 30 months since random assignment
- The first-stage here is the compliance rate

$$E[\mathrm{D}_i|\mathrm{Z}_i = 1] - E[\mathrm{D}_i|\mathrm{Z}_i = 0] = .62 - .02$$
$$\cong \quad P[\mathrm{D}_i = 1|\mathrm{Z}_i = 1]$$

(about .02 of the control group received JTPA services)

- **Table 4.4.1** Selection bias in OLS (as delivered); ITT (as assigned) is diluted; IV (TOT) is . . . just right!

Bloom Example 2: Battered Wives

What's the best police response to domestic violence? The Minneapolis Domestic Violence Experiment (MDVE; Sherman and Berk, 1984) tries to find out

- Police were randomly assigned to advise, separate, or arrest
- Substantial compliance problems as officers made their own judgements in the field

Table 1: Assigned and Delivered Treatments
in Spousal Assault Cases

Assigned Treatment	Delivered Treatment			
		Coddled		
	Arrest	Advise	Separate	Total
Arrest	98.9 (91)	0.0 (0)	1.1 (1)	29.3 (92)
Advise	17.6 (19)	77.8 (84)	4.6 (5)	34.4 (108)
Separate	22.8 (26)	4.4 (5)	72.8 (83)	36.3 (114)
Total	43.4 (136)	28.3 (89)	28.3 (89)	100.0(314)

MDVE First-Stage and Reduced Forms

Table 2: First Stage and Reduced Forms for Model 1

	Endogenous Variable is Coddled			
	First-Stage		Reduced Form (ITT)	
	(1)	(2)*	(3)	(4)*
Coddled-assigned	0.786 (0.043)	0.773 (0.043)	0.114 (0.047)	0.108 (0.041)
Weapon		-0.064 (0.045)		-0.004 (0.042)
Chem. Influence		-0.088 (0.040)		0.052 (0.038)
Dep. Var. mean	0.567 (coddled-delivered)		0.178 (failed)	

MDVE OLS and 2SLS

Table 3: OLS and 2SLS Estimates for Model 1

Endogenous Variable is Coddled				
	OLS		IV/2SLS	
	(1)	(2)*	(3)	(4)*
Coddled-delivered	0.087 (0.044)	0.070 (0.038)	0.145 (0.060)	0.140 (0.053)
Weapon		0.010 (0.043)		0.005 (0.043)
Chem. Influence		0.057 (0.039)		0.064 (0.039)

Models with Variable Treatment Intensity

Average Causal Response [Skip to Summary]

Suppose that s_i takes on values in the set $\{0, 1, ..., \bar{s}\}$. There are $\bar{s}$ unit causal effects, $Y_{si} - Y_{s-1,i}$.

- A linear model assumes these are the same for all s and for all i, clearly unrealistic
- Fear not! 2SLS generates a weighted average of unit causal effects
  - Suppose a single binary instrument, z_i (say, a dummy for late quarter births) is used to estimate the returns to schooling
  - Let s_{1i} denote the schooling i would get if $\text{z}_i = 1$, and let s_{0i} denote the schooling i would get if $\text{z}_i = 0$. We observe $\text{s}_i = \text{s}_{0i}(1-\text{z}_i)+\text{z}_i\text{s}_{1i}$
- Key assumptions:
  - Independence and Exclusion. $\{Y_{0i}, Y_{1i}, ..., Y_{\bar{s}i}; \text{s}_{0i}, \text{s}_{1i}\} \perp\!\!\!\perp \text{z}_i$
  - First Stage. $E[\text{s}_{1i} - \text{s}_{0i}] \neq 0$
  - Monotonicity. $\text{s}_{1i} - \text{s}_{0i} \geq 0 \quad \forall i$ (or vice versa)

The ACR Theorem

Angrist and Imbens (1995) show

$$\frac{E[\mathrm{Y}_i|\mathrm{Z}_i = 1] - E[\mathrm{Y}_i|\mathrm{Z}_i = 0]}{E[\mathrm{S}_i|\mathrm{Z}_i = 1] - E[\mathrm{S}_i|\mathrm{Z}_i = 0]} = \sum_{s=1}^{\bar{s}} \omega_s E[Y_{si} - Y_{s-1,i}|\mathrm{S}_{1i} \geq s > \mathrm{S}_{0i}]$$

where

$$\omega_s = \frac{P[\mathrm{S}_{1i} \geq s > \mathrm{S}_{0i}]}{\sum_{j=1}^{\bar{s}} P[\mathrm{S}_{1i} \geq j > \mathrm{S}_{0i}]}$$

The weights ω_s are non-negative and sum to 1.

- The Wald estimator is a weighted average of the *unit causal response* along the length of a potentially nonlinear causal relation.
- $E[Y_{si} - Y_{s-1,i}|\mathrm{S}_{1i} \geq s > \mathrm{S}_{0i}]$, is the average difference in potential outcomes for *compliers at point s*, people driven by the instrument from a treatment intensity less than s to at least s.

The ACR Weighting Function

- The relative size of the group of compliers at point s is $P[\mathrm{s}_{1i} \geq s > \mathrm{s}_{0i}]$. This is

$$P\left[\mathrm{s}_{1i} \geq s > \mathrm{s}_{0i}\right] = P\left[\mathrm{s}_{1i} \geq s\right] - P\left[\mathrm{s}_{0i} \geq s\right]$$
$$= P\left[\mathrm{s}_{0i} < s\right] - P\left[\mathrm{s}_{1i} < s\right],$$

- By Independence, this is an observed CDF difference:

$$P[\mathrm{s}_{0i} < s] - P[\mathrm{s}_{1i} < s] = P[\mathrm{s}_i < s|\mathrm{z}_i = 0] - P[\mathrm{s}_i < s|\mathrm{z}_i = 1]$$

- Finally, because the mean of a non-negative random variable is one minus the CDF,

$$E\left[\mathrm{s}_i|\mathrm{z}_i = 1\right] - E\left[\mathrm{s}_i|\mathrm{z}_i = 0\right]$$
$$= \sum_{j=1}^{\bar{s}}\left(P\left[\mathrm{s}_i < j|\mathrm{z}_i = 0\right] - P\left[\mathrm{s}_i < j|\mathrm{z}_i = 1\right]\right) = \sum_{j=1}^{\bar{s}} P\left[\mathrm{s}_{1i} \geq j > \mathrm{s}_{0i}\right]$$

- ACR weighting is proportional to the difference in the CDFs of treatment intensity with the instrument switched off and on

QOB Estimates of the Returns to Schooling

The ACR weighting function shows us where the action is . . .

- Angrist and Krueger (1991)

- s_i is years of schooling
- z_i indicates men born in the fourth quarter
- Y_i is log weekly wage

- CDFs by quarter of birth (first compared with fourth)$\Longrightarrow$

Table 1. Compulsory School Attendance

	(1) Born in 1st quarter of year	(2) Born in 4th quarter of year	(3) Difference (std. error) (1) − (2)
Panel A: Wald Estimates for 1970 Census—Men Born 1920–1929[a]			
ln (weekly wage)	5.1485	5.1578	−.00935 (.00374)
Education	11.3996	11.5754	−.1758 (.0192)
Wald est. of return to education			.0531 (.0196)
OLS est. of return to education[b]			.0797 (.0005)
Panel B: Wald Estimates for 1980 Census—Men Born 1930–1939			
ln (weekly wage)	5.8916	5.9051	−.01349 (.00337)
Education	12.6881	12.8394	−.1514 (.0162)
Wald est. of return to education			.0891 (.0210)
OLS est. of return to education			.0703 (.0005)

Empirical Weighting Function

- For men born 1920-29 in the 1970 Census

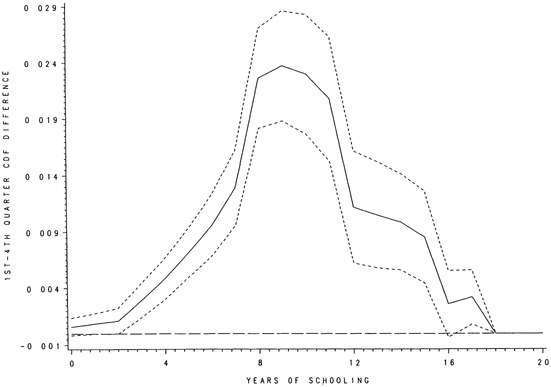

Figure 3. First–Fourth Quarter Difference in Schooling CDF (Men Born 1920–1929, Data From the 1970 Census). Dotted lines are 95% confidence intervals.

More Variable Treatment Intensities

- Returns to schooling again, identified using compulsory attendance and child labor laws (Acemoglu and Angrist, 2000)
- Class size (Angrist and Lavy, 1999; Krueger, 1999)
  - Y_i is test score; S_i is class size
  - Z_i is Maimonides Rule (regression-discontinuity) or random assignment
- GRE test preparation (Powers and Swinton, 1984)
  - Y_i is GRE analytical score; S_i is hours of study
  - Z_i is randomly assigned letter of encouragement
- Maternal smoking (Permutt and Hebel, 1989)
  - Y_i is birthweight; S_i is mother's pre-natal smoking
  - Z_i is randomly assigned offer of anti-smoking counseling
- Quantity-quality trade-offs (Angrist, Lavy, and Schlosser, 2006)
  - Y_i is schooling, earnings, etc.; S_i is sibship size
  - Z_i is derived from twins and sibling-sex composition

Summary

- IV provides a powerful and flexible framework for causal inference
  - An observational alternative to random assignment with a strong claim on internal validity
  - A framework for other observational designs; Fuzzy RD is IV (See MHE Chpt 6)
  - IV solves the compliance problem in randomized trials (the biomed RCT world has been slow to absorb this; e.g., AIDS vaccine trials)
- Of course, we can't always find a good instrument; sometimes the best we can do is run regressions
- And we might want to do that anyway, as in our recent charter study (Abdulkadiroglu, et al. 2009)
  - Here, we use lotteries to create instruments for charter attendance; this solves the selection problem . . . for some
  - But not everyone is in a lottery; this limits external validity
  - We validate regression estimates in the lottery sample; this establishes internal validity of regression, which can used in a wider sample
- Internal and external validity are complements!

Tables and Figures

TABLE 5—WALD ESTIMATES OF LABOR-SUPPLY MODELS

Variable	1980 PUMS: Mean difference by *Same sex*	1980 PUMS: Wald estimate using as covariate: *More than 2 children*	1980 PUMS: Wald estimate using as covariate: *Number of children*	1990 PUMS: Mean difference by *Same sex*	1990 PUMS: Wald estimate using as covariate: *More than 2 children*	1990 PUMS: Wald estimate using as covariate: *Number of children*	1980 PUMS: Mean difference by *Twins-2*	1980 PUMS: Wald estimate using as covariate: *More than 2 children*	1980 PUMS: Wald estimate using as covariate: *Number of children*
More than 2 children	0.0600 (0.0016)	—	—	0.0628 (0.0016)	—	—	0.6031 (0.0084)	—	—
Number of children	0.0765 (0.0026)	—	—	0.0836 (0.0025)	—	—	0.8094 (0.0139)	—	—
Worked for pay	−0.0080 (0.0016)	−0.133 (0.026)	−0.104 (0.021)	−0.0053 (0.0015)	−0.084 (0.024)	−0.063 (0.018)	−0.0459 (0.0086)	−0.076 (0.014)	−0.057 (0.011)
Weeks worked	−0.3826 (0.0709)	−6.38 (1.17)	−5.00 (0.92)	−0.3233 (0.0743)	−5.15 (1.17)	−3.87 (0.88)	−1.982 (0.386)	−3.28 (0.63)	−2.45 (0.47)
Hours/week	−0.3110 (0.0602)	−5.18 (1.00)	−4.07 (0.78)	−0.2363 (0.0620)	−3.76 (0.98)	−2.83 (0.73)	−1.979 (0.327)	−3.28 (0.54)	−2.44 (0.40)
Labor income	−132.5 (34.4)	−2208.8 (569.2)	−1732.4 (446.3)	−119.4 (42.4)	−1901.4 (670.3)	−1428.0 (502.6)	−570.8 (186.9)	−946.4 (308.6)	−705.2 (229.8)
ln(*Family income*)	−0.0018 (0.0041)	−0.029 (0.068)	−0.023 (0.054)	−0.0085 (0.0047)	−0.136 (0.074)	−0.102 (0.056)	−0.0341 (0.0223)	−0.057 (0.037)	−0.042 (0.027)

Notes: The samples are the same as in Table 2. Standard errors are reported in parentheses.

TABLE 4.4.1
Results from the JTPA experiment: OLS and IV estimates of training impacts

	Comparisons by Training Status (OLS)		Comparisons by Assignment Status (ITT)		Instrumental Variable Estimates (IV)	
	Without Covariates (1)	With Covariates (2)	Without Covariates (3)	With Covariates (4)	Without Covariates (5)	With Covariates (6)
A. Men	3,970	3,754	1,117	970	1,825	1,593
	(555)	(536)	(569)	(546)	(928)	(895)
B. Women	2,133	2,215	1,243	1,139	1,942	1,780
	(345)	(334)	(359)	(341)	(560)	(532)

Notes: Authors' tabulation of JTPA study data. The table reports OLS, ITT, and IV estimates of the effect of subsidized training on earnings in the JTPA experiment. Columns 1 and 2 show differences in earnings by training status; columns 3 and 4 show differences by random-assignment status. Columns 5 and 6 report the result of using random-assignment status as an instrument for training. The covariates used for columns 2, 4, and 6 are high school or GED, black, Hispanic, married, worked less than 13 weeks in past year, AFDC (for women), plus indicators for the JTPA service strategy recommended, age group, and second follow-up survey. Robust standard errors are shown in parentheses. There are 5,102 men and 6,102 women in the sample.

Table 4.4.2

Probabilities of compliance in instrumental variables studies

Endogenous Variable (D) (2)	Instrument (Z) (3)	Sample (4)	$P[D = 1]$ (5)	First Stage, $P[D_1 > D_0]$ (6)	$P[Z = 1]$ (7)	Compliance Probabilities $P[D_1 > D_0 \mid D = 1]$ (8)	Compliance Probabilities $P[D_1 > D_0 \mid D = 0]$ (9)
Veteran status	Draft eligibility	White men born in 1950	.267	.159	.534	.318	.101
		Non-white men born in 1950	.163	.060	.534	.197	.033
More than two children	Twins at second birth	Married women aged 21–35 with two or more children in 1980	.381	.603	.008	.013	.966
	First two children are same sex		.381	.060	.506	.080	.048
High school graduate	Third- or fourth-quarter birth	Men born between 1930 and 1939	.770	.016	.509	.011	.034
High school graduate	State requires 11 or more years of school attendance	White men aged 40–49	.617	.037	.300	.018	.068

↩

TABLE 4.4.3
Complier characteristics ratios for twins and sex composition instruments

		Twins at Second Birth		First Two Children Are Same Sex	
Variable	$P[x_{1i}=1]$ (1)	$P[x_{1i}=1 \mid \text{D}_{1i} > \text{D}_{0i}]$ (2)	$P[x_{1i}=1 \mid \text{D}_{1i} > \text{D}_{0i}]/P[x_{1i}=1]$ (3)	$P[x_{1i}=1 \mid \text{D}_{1i} > \text{D}_{0i}]$ (4)	$P[x_{1i}=1 \mid \text{D}_{1i} > \text{D}_{0i}]/P[x_{1i}=1]$ (5)
Age 30 or older at first birth	.0029	.004	1.39	.0023	.995
Black or hispanic	.125	.103	.822	.102	.814
High school graduate	.822	.861	1.048	.815	.998
College graduate	.132	.151	1.14	.0904	.704

Notes: The table reports an analysis of complier characteristics for twins and sex composition instruments. The ratios in columns 3 and 5 give the relative likelihood that compliers have the characteristic indicated at left. Data are from the 1980 census 5 percent sample, including married mothers aged 21–35 with at least two children, as in Angrist and Evans (1998). The sample size is 254,654 for all columns.