Econometrics III: Applied Methods — NovaSBE, 2026

Problem Set 2 Solutions: Instrumental Variables

Exercise 1: IV and Spillovers

Context: Labor market counseling program for unemployed. $Z \in \{0,1\}$ = random assignment to program; $T \in \{0,1\}$ = actual participation; $y \in \{0,1\}$ = found job after 6 months.

Q1. *(1 pt)* In this context, what do we call 'compliers' (C), 'always takers' (A), 'never takers' (N), 'defiers' (D)? Define them formally and intuitively.

Solution:

Define $T_i(z)$ as individual i's treatment status when assigned $Z = z$.

Type	$T_i(0)$	$T_i(1)$	**Intuition**
Compliers (C)	0	1	Enter counseling only if offered
Always-takers (A)	1	1	Would find a way to get counseling regardless
Never-takers (N)	0	0	Never enter counseling, even if offered
Defiers (D)	1	0	Enter only if *not* offered (perverse)

Formal definitions:

$$C = \{i : T_i(0) = 0, T_i(1) = 1\}$$
$$A = \{i : T_i(0) = 1, T_i(1) = 1\}$$
$$N = \{i : T_i(0) = 0, T_i(1) = 0\}$$
$$D = \{i : T_i(0) = 1, T_i(1) = 0\}$$

Q2. *(1 pt)* The following table presents the joint distribution of Z and T. Assuming no defiers, what is the proportion of compliers, always takers, and never takers?

	$Z = 1$	$Z = 0$
$T = 1$	0.22	0.00
$T = 0$	0.28	0.50

Solution:

From the table: $P(Z = 1) = 0.22 + 0.28 = 0.50$ and $P(Z = 0) = 0.50$.
Always-takers: Those with $T = 1$ when $Z = 0$. From the table:

$$\pi_A = P(T = 1|Z = 0) = \frac{0.00}{0.50} = \boxed{0\%}$$

Never-takers: Those with $T = 0$ when $Z = 1$. These are people offered the program who refuse:

$$\pi_N = P(T = 0|Z = 1) = \frac{0.28}{0.50} = \boxed{56\%}$$

Compliers: Those who take treatment when offered but not otherwise. Since there are no always-takers:

$$\pi_C = P(T = 1|Z = 1) - P(T = 1|Z = 0) = \frac{0.22}{0.50} - 0 = \boxed{44\%}$$

Verification: $\pi_A + \pi_N + \pi_C = 0 + 0.56 + 0.44 = 1$ ✓

Interpretation: The program has a 44% take-up rate among those offered. No one finds a way to enter without being offered (no always-takers), and 56% refuse even when offered (never-takers).

Q3. *(0.5 pts)* Use the following table to compute the 'Intention to Treat' parameter. Explain.

$Z = 1$	0.377
$Z = 0$	0.360

Solution:

The **Intention-to-Treat (ITT)** is the effect of being *assigned* to treatment, regardless of actual take-up:

$$\text{ITT} = \mathbb{E}[y|Z = 1] - \mathbb{E}[y|Z = 0] = 0.377 - 0.360 = \boxed{0.017 = 1.7 \text{ pp}}$$

Interpretation: Being offered the counseling program increases the probability of finding a job by 1.7 percentage points, on average. This is a "diluted" effect because:

- Only 44% of those offered actually participate (compliers)
- The ITT averages the treatment effect over compliers, never-takers (zero effect), and always-takers (zero marginal effect)

The ITT is policy-relevant: it tells us what happens when we *offer* the program at scale.

Q4. *(1 pt)* Explain in words what is the Local Average Treatment Effect (LATE), and estimate it.

Solution:

LATE definition: The average treatment effect *for compliers*—those whose treatment status is affected by the instrument:

$$\text{LATE} = \mathbb{E}[y(1) - y(0)|\text{Complier}]$$

Estimation via Wald estimator:

$$\text{LATE} = \frac{\text{ITT}}{\text{First stage}} = \frac{\mathbb{E}[y|Z = 1] - \mathbb{E}[y|Z = 0]}{\mathbb{E}[T|Z = 1] - \mathbb{E}[T|Z = 0]}$$

From the data:

- $\text{ITT} = 0.377 - 0.360 = 0.017$
- $\text{First stage} = P(T = 1|Z = 1) - P(T = 1|Z = 0) = 0.44 - 0 = 0.44$

$$\text{LATE} = \frac{0.017}{0.44} = \boxed{0.039 = 3.9 \text{ pp}}$$

Interpretation: For those who participate in counseling *because* they were offered (compliers), the program increases their probability of finding a job by 3.9 percentage points.

This is larger than the ITT because we "scale up" the diluted effect to account for imperfect compliance.

> **Q5.** *(1 pt)* We now assume there can be spillovers: the unemployed within each agency compete for available job openings. If treated individuals had better access to jobs at the expense of the non-treated, would this experiment overestimate or underestimate the relevance of the counseling policy?

Solution:

The experiment would **overestimate** the policy's effectiveness.

Reasoning:

- With spillovers, treated individuals "steal" jobs from untreated individuals in the same agency
- The control group ($Z = 0$) is harmed by the treatment of others: their employment probability falls
- This makes $\mathbb{E}[y|Z = 0]$ lower than it would be without the program
- The ITT (and LATE) measures $\mathbb{E}[y|Z = 1] - \mathbb{E}[y|Z = 0]$, which is inflated by the negative spillover on controls

Policy implication: If we scale up the program to *everyone*, there would be no control group to "steal" jobs from. The aggregate employment effect would be smaller (possibly zero if jobs are truly fixed) than what the experiment suggests.

This is the classic **displacement effect** concern in active labor market policies: helping some jobseekers may simply reshuffle who gets hired, not increase total employment.

Two-level randomization: To address spillovers, agencies were also randomized. $P = 1$ if agency participates in program (with individual randomization within); $P = 0$ if control agency (no program).

> **Q6.** *(1.5 pts)* Define counterfactual outcomes as $y(t, p)$: outcome depends on own treatment t and agency treatment p. In this design, what values of $y(t, p)$ are observed and for whom?

Solution:

There are four potential outcomes: $y(0, 0), y(1, 0), y(0, 1), y(1, 1)$.

Observed outcomes by group:

Group	**Agency**	**Treatment**	**Observed**
$P = 0$ (control agency)	Control	$T = 0$ for all	$y(0, 0)$
$P = 1, Z = 0$ (treated agency, not offered)	Treated	$T = 0$	$y(0, 1)$
$P = 1, Z = 1, T = 0$ (offered, refused)	Treated	$T = 0$	$y(0, 1)$
$P = 1, Z = 1, T = 1$ (offered, accepted)	Treated	$T = 1$	$y(1, 1)$

Key observations:

- $y(0,0)$: Observed for everyone in control agencies
- $y(0,1)$: Observed for untreated in treated agencies (never-takers with $Z=1$, and everyone with $Z=0$)
- $y(1,1)$: Observed for treated in treated agencies (compliers and always-takers with $Z=1$)
- $y(1,0)$: **Never observed**—no one is individually treated in a control agency

> **Q7.** *(1.5 pts)* Formally give the expression of the LATE estimated within treated agencies (in Q4) in terms of this counterfactual. Use π_C, π_A, π_N for population shares.

Solution:

Within treated agencies ($P=1$), the LATE from Q4 compares $y(1,1)$ vs $y(0,1)$:

$$\text{LATE}_{P=1} = \mathbb{E}[y(1,1) - y(0,1)|\text{Complier}]$$

Derivation: For $P=1$:

$$\mathbb{E}[y|Z=1,P=1] = \pi_C \cdot \mathbb{E}[y(1,1)|C] + \pi_A \cdot \mathbb{E}[y(1,1)|A] + \pi_N \cdot \mathbb{E}[y(0,1)|N]$$
$$\mathbb{E}[y|Z=0,P=1] = \pi_C \cdot \mathbb{E}[y(0,1)|C] + \pi_A \cdot \mathbb{E}[y(1,1)|A] + \pi_N \cdot \mathbb{E}[y(0,1)|N]$$

Taking the difference (ITT within $P=1$):

$$\mathbb{E}[y|Z=1,P=1] - \mathbb{E}[y|Z=0,P=1] = \pi_C \cdot \mathbb{E}[y(1,1) - y(0,1)|C]$$

Since the first stage is π_C (in our case, 0.44):

$$\boxed{\text{LATE}_{P=1} = \frac{\text{ITT}_{P=1}}{\pi_C} = \mathbb{E}[y(1,1) - y(0,1)|\text{Complier}]}$$

This measures the **direct effect** of treatment for compliers, *holding spillover exposure fixed* (everyone is in a treated agency).

> **Q8.** *(1.5 pts)* Using $y(t,p)$, define the spillover effect on untreated individuals. Suggest how to estimate it and on what populations.

Solution:

Spillover effect on the untreated:

$$\text{Spillover} = \mathbb{E}[y(0,1) - y(0,0)]$$

This compares outcomes for untreated individuals in treated vs. control agencies.

Estimation: Compare untreated in treated agencies ($P=1, T=0$) with everyone in control agencies ($P=0$):

$$\widehat{\text{Spillover}} = \mathbb{E}[y|T=0,P=1] - \mathbb{E}[y|P=0]$$

Who is in each group?

- $P=0$: All types (C, A, N) in control agencies—representative of population

- $T = 0, P = 1$: Never-takers (with $Z = 0$ or $Z = 1$) in treated agencies

Problem: In our data, $\pi_A = 0$, so the $T = 0, P = 1$ group contains only never-takers. This means:

$$\mathbb{E}[y|T = 0, P = 1] = \mathbb{E}[y(0, 1)|N]$$

But $\mathbb{E}[y|P = 0]$ includes all types. For valid comparison, we need:

$$\widehat{\text{Spillover}}_N = \mathbb{E}[y|T = 0, P = 1] - \mathbb{E}[y(0, 0)|N]$$

The latter requires identifying never-takers in control agencies, which is impossible (we only see $T = 0$ for everyone in $P = 0$).

Alternative: Use Z within $P = 1$ agencies to identify spillovers on compliers:

$$\mathbb{E}[y|Z = 0, P = 1] - \mathbb{E}[y|P = 0]$$

identifies spillover for compliers + never-takers, under additional assumptions.

Q9. *(1.5 pts)* Define the full treatment effect as $y(1, 1) - y(0, 0)$. Can we estimate this? Decompose $\mathbb{E}[y|Z = 1, P = 1] - \mathbb{E}[y|P = 0]$ into direct and spillover effects.

Solution:

Full effect: $y(1, 1) - y(0, 0)$ = being treated in a treated agency vs. being untreated in a control agency. This captures both the direct effect and spillover exposure.

Decomposition:

$$\begin{aligned}
&\mathbb{E}[y|Z = 1, P = 1] - \mathbb{E}[y|P = 0] \\
&= \underbrace{\mathbb{E}[y|Z = 1, P = 1] - \mathbb{E}[y|Z = 0, P = 1]}_{\text{ITT within } P=1} + \underbrace{\mathbb{E}[y|Z = 0, P = 1] - \mathbb{E}[y|P = 0]}_{\text{Spillover effect}}
\end{aligned}$$

Expanding by type:

For $Z = 1, P = 1$:

$$\mathbb{E}[y|Z = 1, P = 1] = \pi_C \mathbb{E}[y(1, 1)|C] + \pi_N \mathbb{E}[y(0, 1)|N]$$

For $P = 0$:

$$\mathbb{E}[y|P = 0] = \pi_C \mathbb{E}[y(0, 0)|C] + \pi_N \mathbb{E}[y(0, 0)|N]$$

The difference:

$$\begin{aligned}
&= \pi_C \big(\mathbb{E}[y(1, 1)|C] - \mathbb{E}[y(0, 0)|C]\big) + \pi_N \big(\mathbb{E}[y(0, 1)|N] - \mathbb{E}[y(0, 0)|N]\big) \\
&= \pi_C \cdot \underbrace{\mathbb{E}[y(1, 1) - y(0, 0)|C]}_{\text{Full effect for compliers}} + \pi_N \cdot \underbrace{\mathbb{E}[y(0, 1) - y(0, 0)|N]}_{\text{Spillover on never-takers}}
\end{aligned}$$

Conclusion: We **cannot** cleanly identify the full effect $\mathbb{E}[y(1, 1) - y(0, 0)|C]$ because it is confounded with the spillover effect on never-takers. The comparison mixes:

- Direct + spillover effect for compliers (who get treated)
- Pure spillover effect for never-takers (who don't get treated but are exposed)

To isolate the full effect on compliers, we would need to subtract the spillover on compliers, but $y(0, 1)$ for compliers in $P = 1$ agencies is only observed when $Z = 0$, not when $Z = 1$.

Exercise 2: LATE with Multi-Valued Treatment

Setup: Treatment $S \in \{0,1,2\}$ (e.g., years of schooling). Outcome $y(S)$ (e.g., earnings). Binary instrument $Z \in \{0,1\}$ (e.g., quarter of birth). Counterfactual treatment: S_0 when $Z = 0$, S_1 when $Z = 1$.

Assumptions:

- Independence: $S_0, S_1, y(0), y(1), y(2) \perp Z$
- Monotonicity: $S_1 \geq S_0$ for everyone

Q1. *(0.5 pts)* What does the monotonicity assumption mean?

Solution:

Monotonicity: The instrument shifts treatment in the *same direction* for everyone. With $S_1 \geq S_0$:

"No one gets *less* schooling due to being born in quarter $Z = 1$ compared to quarter $Z = 0$."

In context: If $Z = 1$ corresponds to being born later in the year (so you can drop out earlier due to compulsory schooling laws), then monotonicity says:

- Some people may get *more* schooling when $Z = 0$ (they're forced to stay longer)
- No one gets *less* schooling when $Z = 0$

Contrast with binary: In binary treatment, monotonicity rules out "defiers." Here, it rules out anyone for whom the instrument *decreases* treatment intensity.

This is stronger than in the binary case: we need $S_1 \geq S_0$ for *every* value of S, not just a single threshold.

Q2. *(1 pt)* Express y, the observed outcome, as a function of $\mathbb{1}(S \geq 1)$, $\mathbb{1}(S \geq 2)$, and the counterfactual outcomes.

Solution:

The observed outcome is $y = y(S)$ where S is the realized schooling level.

Key insight: We can write $y(S)$ as a telescoping sum of incremental effects:

$$y(S) = y(0) + [y(1) - y(0)] \cdot \mathbb{1}(S \geq 1) + [y(2) - y(1)] \cdot \mathbb{1}(S \geq 2)$$

Verification:

- If $S = 0$: $y = y(0) + 0 + 0 = y(0)$ ✓
- If $S = 1$: $y = y(0) + [y(1) - y(0)] \cdot 1 + 0 = y(1)$ ✓
- If $S = 2$: $y = y(0) + [y(1) - y(0)] + [y(2) - y(1)] = y(2)$ ✓

Compact form:

$$\boxed{y = y(0) + [y(1) - y(0)] \cdot \mathbb{1}(S \geq 1) + [y(2) - y(1)] \cdot \mathbb{1}(S \geq 2)}$$

This decomposes the outcome into a baseline plus marginal effects at each "threshold" of treatment.

Q3. *(1.5 pts)* Show that:

$$\begin{aligned}\mathbb{E}[y|Z=1]-\mathbb{E}[y|Z=0] &= \mathbb{E}\big([y(1)-y(0)]\cdot[\mathbb{1}(S_1\geq 1)-\mathbb{1}(S_0\geq 1)]\big)\\ &\quad+\mathbb{E}\big([y(2)-y(1)]\cdot[\mathbb{1}(S_1\geq 2)-\mathbb{1}(S_0\geq 2)]\big)\end{aligned}$$

What assumption did you require?

Solution:

Using the expression from Q2:

$$\mathbb{E}[y|Z=1]=\mathbb{E}[y(0)]+\mathbb{E}[y(1)-y(0)]\cdot\mathbb{E}[\mathbb{1}(S_1\geq 1)]+\mathbb{E}[y(2)-y(1)]\cdot\mathbb{E}[\mathbb{1}(S_1\geq 2)]$$

Wait—this isn't quite right because $y(j)-y(j-1)$ and $\mathbb{1}(S\geq j)$ may be correlated. Let me redo this carefully.

Under $Z=z$, the observed $S=S_z$, so:

$$\mathbb{E}[y|Z=z]=\mathbb{E}\big[y(0)+[y(1)-y(0)]\cdot\mathbb{1}(S_z\geq 1)+[y(2)-y(1)]\cdot\mathbb{1}(S_z\geq 2)\big]$$

By **independence** $(y(0),y(1),y(2),S_0,S_1\perp Z)$:

$$\mathbb{E}[y|Z=z]=\mathbb{E}\big[y(0)+[y(1)-y(0)]\cdot\mathbb{1}(S_z\geq 1)+[y(2)-y(1)]\cdot\mathbb{1}(S_z\geq 2)\big]$$

Taking the difference:

$$\begin{aligned}&\mathbb{E}[y|Z=1]-\mathbb{E}[y|Z=0]\\ &=\mathbb{E}\big[[y(1)-y(0)]\cdot(\mathbb{1}(S_1\geq 1)-\mathbb{1}(S_0\geq 1))\big]\\ &\quad+\mathbb{E}\big[[y(2)-y(1)]\cdot(\mathbb{1}(S_1\geq 2)-\mathbb{1}(S_0\geq 2))\big]\end{aligned}$$

Assumption required: Independence—the instrument Z is independent of all potential outcomes and counterfactual treatment levels. This allows us to compare $\mathbb{E}[\cdot|Z=1]$ and $\mathbb{E}[\cdot|Z=0]$ as if they come from the same distribution of potential outcomes.

Q4. *(1.5 pts)* Show that:

$$\begin{aligned}&\mathbb{E}\big([y(1)-y(0)]\cdot[\mathbb{1}(S_1\geq 1)-\mathbb{1}(S_0\geq 1)]\big)\\ &=\mathbb{E}[y(1)-y(0)|\mathbb{1}(S_1\geq 1)-\mathbb{1}(S_0\geq 1)=1]\cdot P(\mathbb{1}(S_1\geq 1)-\mathbb{1}(S_0\geq 1)=1)\end{aligned}$$

What assumption did you require?

Solution:

The term $\mathbb{1}(S_1\geq 1)-\mathbb{1}(S_0\geq 1)$ can take values:

- $+1$ if $S_1\geq 1$ but $S_0<1$ (i.e., $S_1\geq 1>S_0$)
- 0 if both indicators are equal
- -1 if $S_0\geq 1$ but $S_1<1$ (i.e., $S_0\geq 1>S_1$)

Under **monotonicity** $(S_1\geq S_0)$, the -1 case is impossible. So:

$$\mathbb{1}(S_1\geq 1)-\mathbb{1}(S_0\geq 1)\in\{0,1\}$$

Therefore:

$$\begin{aligned}&\mathbb{E}\big[[y(1)-y(0)]\cdot[\mathbb{1}(S_1\geq 1)-\mathbb{1}(S_0\geq 1)]\big]\\&=\mathbb{E}[y(1)-y(0)|\Delta_1=1]\cdot P(\Delta_1=1)+\mathbb{E}[y(1)-y(0)|\Delta_1=0]\cdot 0\cdot P(\Delta_1=0)\end{aligned}$$

where $\Delta_1 = \mathbb{1}(S_1 \geq 1) - \mathbb{1}(S_0 \geq 1)$.

The second term is zero (multiplied by 0), leaving:

$$\boxed{=\mathbb{E}[y(1)-y(0)|\mathbb{1}(S_1\geq 1)-\mathbb{1}(S_0\geq 1)=1]\cdot P(\mathbb{1}(S_1\geq 1)-\mathbb{1}(S_0\geq 1)=1)}$$

Assumption required: Monotonicity—ensures the indicator difference is non-negative, so the expectation is only over the "compliers at threshold 1."

Q5. *(0.5 pts)* Explain why $P(\mathbb{1}(S_1 \geq 1) - \mathbb{1}(S_0 \geq 1) = 1) = P(S_1 \geq 1 > S_0)$. What kind of individuals does $S_1 \geq 1 > S_0$ characterize?

Solution:

Equivalence:

$$\begin{aligned}\mathbb{1}(S_1\geq 1)-\mathbb{1}(S_0\geq 1)=1 &\iff \mathbb{1}(S_1\geq 1)=1 \text{ and } \mathbb{1}(S_0\geq 1)=0\\&\iff S_1\geq 1 \text{ and } S_0<1\\&\iff S_1\geq 1>S_0 \quad (\text{since } S_0<1\Rightarrow S_0=0)\end{aligned}$$

Who are these individuals? "**Compliers at the first threshold**"—people who:

- Would have $S = 0$ schooling if $Z = 0$
- Would have $S \geq 1$ schooling if $Z = 1$

In the education/compulsory schooling context: these are individuals induced by the instrument to cross from zero to at least one year of schooling.

Analogy to binary LATE: In the binary case, compliers are those with $T_1 > T_0$. Here, "compliers at threshold j" are those with $S_1 \geq j > S_0$—they cross the j-th threshold due to the instrument.

Q6. *(1.5 pts)* We assume (without proof) that:

$$\mathbb{E}[S|Z=1]-\mathbb{E}[S|Z=0]=P(S_1\geq 1>S_0)+P(S_1\geq 2>S_0)$$

Show that:

$$\frac{\mathbb{E}[y|Z=1]-\mathbb{E}[y|Z=0]}{\mathbb{E}[S|Z=1]-\mathbb{E}[S|Z=0]}=\sum_{j=1}^{2}\omega_j\mathbb{E}[y(j)-y(j-1)|S_1\geq j>S_0]$$

where $\omega_j = \dfrac{P(S_1 \geq j > S_0)}{\sum_{k=1}^{2} P(S_1 \geq k > S_0)}$.

Solution:

From Q3 and Q4, the reduced form is:

$$\begin{aligned}\mathbb{E}[y|Z=1]-\mathbb{E}[y|Z=0]=\ &\mathbb{E}[y(1)-y(0)|S_1\geq 1>S_0]\cdot P(S_1\geq 1>S_0)\\&+\mathbb{E}[y(2)-y(1)|S_1\geq 2>S_0]\cdot P(S_1\geq 2>S_0)\end{aligned}$$

The first stage is given as:

$$\mathbb{E}[S|Z=1] - \mathbb{E}[S|Z=0] = P(S_1 \geq 1 > S_0) + P(S_1 \geq 2 > S_0)$$

Define $\pi_j = P(S_1 \geq j > S_0)$ for $j = 1, 2$. The Wald ratio is:

$$\frac{\text{Reduced form}}{\text{First stage}} = \frac{\pi_1 \cdot \mathbb{E}[y(1) - y(0)|S_1 \geq 1 > S_0] + \pi_2 \cdot \mathbb{E}[y(2) - y(1)|S_1 \geq 2 > S_0]}{\pi_1 + \pi_2}$$

Defining weights $\omega_j = \dfrac{\pi_j}{\pi_1 + \pi_2}$:

$$\boxed{\frac{\mathbb{E}[y|Z=1] - \mathbb{E}[y|Z=0]}{\mathbb{E}[S|Z=1] - \mathbb{E}[S|Z=0]} = \sum_{j=1}^{2} \omega_j \mathbb{E}[y(j) - y(j-1)|S_1 \geq j > S_0]}$$

Note: $\omega_1 + \omega_2 = 1$, so this is a proper weighted average.

Q7. *(1 pt)* Interpret this expression.

Solution:

The IV/Wald estimator with multi-valued treatment identifies a **weighted average of marginal treatment effects**, where:

1. Each term $\mathbb{E}[y(j) - y(j-1)|S_1 \geq j > S_0]$ is the **LATE at threshold** j:

- Effect of moving from $S = j - 1$ to $S = j$
- For "compliers at threshold j"—those induced by the instrument to cross that threshold

2. Weights ω_j reflect the **relative size of complier groups**:

- $\omega_j = \dfrac{P(S_1 \geq j > S_0)}{\sum_k P(S_1 \geq k > S_0)}$
- Thresholds with more compliers get more weight
- If the instrument mainly affects the $0 \to 1$ margin, ω_1 is large

3. Interpretation: The IV estimator is a **variance-weighted average** of causal effects at different treatment margins, for the subpopulations affected at each margin.

Key insights:

- **Heterogeneity matters:** If returns to schooling vary across margins (e.g., high school vs. college), IV captures a mix
- **Instrument determines weights:** Different instruments weight margins differently based on which compliers they create
- **Not ATE:** This is not the average effect of one more unit of treatment for the whole population—it's local to compliers at each margin
- **Generalizes binary LATE:** With binary S, this reduces to the standard LATE formula

Policy relevance: If your policy operates at a different margin than the instrument, the IV estimate may not be informative. For example, if quarter-of-birth affects high school completion but your policy targets college attendance, the IV estimate weights the wrong margin.

Exercise 3: Bounding Treatment Effects with Sample Selection

Context: NSW job training RCT. Treatment (D) affects employment (S) and potentially wages (Y^*). We observe wages only for the employed.

> **Q1.** *(1 pt)* Estimate the effect of treatment on: (a) total earnings, (b) employment, (c) wages conditional on employment. Is the employment effect significant?

Solution:

```
# R code:
nsw <- read.csv("lalonde_nsw.csv")
nsw$employed <- as.integer(nsw$re78 > 0)
nsw$wage <- ifelse(nsw$employed == 1, nsw$re78, NA)

# (a) Effect on total earnings
lm(re78 ~ treat, data = nsw)
# Coefficient: $1794 (SE ~ $632), p < 0.01

# (b) Effect on employment
lm(employed ~ treat, data = nsw)
# Coefficient: 0.111 (SE ~ 0.044), p ~ 0.013

# (c) Effect on wages (employed only)
lm(wage ~ treat, data = nsw[nsw$employed == 1, ])
# Coefficient: $1341 (SE ~ $871), p ~ 0.12
```

Results:

Outcome	Treated	Control	Difference
Employment rate	0.757	0.646	0.111 (11.1 pp)**
Mean earnings (all)	$6,349	$4,555	$1,794**
Mean wage (employed)	$8,390	$7,049	$1,341

The employment effect **is statistically significant** ($p \approx 0.013$). Treatment increases employment by about 11 percentage points.

Implication: Since treatment significantly affects employment, comparing wages only among the employed yields a biased estimate of the wage effect for "always-employed" individuals.

> **Q2.** *(1 pt)* (a) Would marginal workers brought into employment have higher or lower wages? (b) Would naive wage comparison over- or underestimate the effect for always-employed?

Solution:

(a) Marginal workers likely have <u>lower</u> wages.

Workers who are unemployed absent treatment but become employed with treatment are likely more disadvantaged—otherwise they would have found jobs anyway. These "marginal" or "complier" workers probably have:

- Less experience and skills
- Weaker labor market attachment

- Lower reservation wages

(b) Naive comparison likely <u>underestimates</u> the wage effect for always-employed.
Reasoning:

- Treated employed = always-employed (EE) + compliers (NE)
- Control employed = always-employed (EE) only
- If compliers have lower wages, they drag down the treated mean
- The naive comparison includes this "compositional" effect

So the naive comparison captures both:

1. The true wage effect for EE (what we want)
2. A negative compositional effect from low-wage compliers

The naive estimate is thus biased downward relative to the effect on always-employed.

Q3. *(1.5 pts)* (a) Explain the intuition for Manski bounds. (b) Compute them.

Solution:

(a) Intuition: Manski bounds assume nothing about the unobserved outcomes. For a lower bound, we assume:

- Treated non-employed would have the *lowest* possible wages (y_L)
- Control non-employed would have the *highest* possible wages (y_U)

This makes the treatment effect as small as possible. For the upper bound, reverse the assumptions.

(b) Computation:
From the data:

- $y_L = \$45$ (minimum wage among employed)
- $y_U = \$60,308$ (maximum wage among employed)
- $P(S = 1|D = 1) = 0.757$, $P(S = 1|D = 0) = 0.646$
- $\mathbb{E}[Y|D = 1, S = 1] = \$8,390$, $\mathbb{E}[Y|D = 0, S = 1] = \$7,049$

Lower bound:

$$= (8390 \times 0.757 + 45 \times 0.243) - (7049 \times 0.646 + 60308 \times 0.354)$$
$$= 6,362 - 25,907 = \boxed{-\$19,535}$$

Upper bound:

$$= (8390 \times 0.757 + 60308 \times 0.243) - (7049 \times 0.646 + 45 \times 0.354)$$
$$= 21,006 - 4,569 = \boxed{\$16,448}$$

Manski bounds: $[-\$19,535, \$16,448]$
These bounds are **not informative**—they include zero and span a huge range. This is typical of Manski bounds without additional assumptions.

Q4. *(1.5 pts)* (a) Under monotonicity, there are no "defiers" (EN types). Explain intuitively why this helps obtain tighter bounds. (b) Is monotonicity plausible in the NSW context? (c) Compute p_0.

Solution:

(a) Why no defiers helps obtain tighter bounds:

Under monotonicity ($S_i(1) \geq S_i(0)$ for all i), there are no "defiers"—people who would be employed without treatment but not employed with treatment. This has a crucial implication:

- Among **control employed** ($D = 0, S = 1$): These can only be always-employed (EE). If there were defiers, control employed would include both EE and EN types.
- Among **treated employed** ($D = 1, S = 1$): These include always-employed (EE) and compliers (NE), but no defiers.

This helps because:

1. The control employed group is "clean"—it contains *only* always-employed individuals
2. We only need to "purify" the treated employed group by removing compliers
3. Without monotonicity, both groups would be mixtures of multiple types, making it impossible to isolate any well-defined subpopulation

In short, monotonicity ensures that one comparison group (control employed) is homogeneous, so we only need to adjust the other group (treated employed) to achieve apples-to-apples comparison.

(b) Monotonicity is plausible in the NSW context because:

- The NSW program provided job training and skills
- Skills weakly increase employability for everyone
- It's hard to imagine someone who would be employed *without* training but *not* employed with training
- The program doesn't "crowd out" other employment opportunities

So $S_i(1) \geq S_i(0)$ for all i seems reasonable.

(c) Computing p_0:

$$p_0 = \frac{P(S = 1|D = 1) - P(S = 1|D = 0)}{P(S = 1|D = 1)} = \frac{0.757 - 0.646}{0.757} = \frac{0.111}{0.757} = \boxed{0.146}$$

Interpretation: About 14.6% of treated employed are "compliers"—people who are employed only because of treatment. The remaining 85.4% would have been employed anyway (always-employed, EE).

Q5. *(1.5 pts)* (a) Explain why trimming works. (b) Derive the Lee bounds formula.

Solution:

(a) Intuition for trimming:

Among treated employed, we observe a mixture of:

- Always-employed (EE): fraction $1 - p_0 = 0.854$
- Compliers (NE): fraction $p_0 = 0.146$

We want to compare EE outcomes across treatment/control, but we can't identify individual EE types. However, if we knew whether compliers had the highest or lowest wages:

- If compliers have highest wages: trim top p_0 to get EE
- If compliers have lowest wages: trim bottom p_0 to get EE

Since we don't know which, we try both and form bounds.
(b) Derivation:
Let F be the CDF of wages among treated employed. By monotonicity, no defiers exist, so:

$$\{D = 1, S = 1\} = \text{EE} \cup \text{NE}$$

Case 1: Compliers (NE) have the *highest* wages among treated employed.
Then EE have wages in the bottom $(1 - p_0)$ of the distribution:

$$\mathbb{E}[Y^*(1)|\text{EE}] = \mathbb{E}[Y|D = 1, S = 1, Y \leq y_{1-p_0}]$$

Case 2: Compliers (NE) have the *lowest* wages among treated employed.
Then EE have wages in the top $(1 - p_0)$ of the distribution:

$$\mathbb{E}[Y^*(1)|\text{EE}] = \mathbb{E}[Y|D = 1, S = 1, Y \geq y_{p_0}]$$

Since control employed are all EE (by monotonicity):

$$\mathbb{E}[Y^*(0)|\text{EE}] = \mathbb{E}[Y|D = 0, S = 1]$$

The treatment effect on EE is:

$$\tau_{\text{EE}} = \mathbb{E}[Y^*(1) - Y^*(0)|\text{EE}]$$

Bounds:

- **Lower:** Assume compliers have highest wages (trimming top gives EE with lower mean)
- **Upper:** Assume compliers have lowest wages (trimming bottom gives EE with higher mean)

Q6. *(1 pt)* Compute Lee bounds for wages.

Solution:

Step 1: Find trimming quantiles among treated employed wages.
With $p_0 = 0.146$:

- $y_{p_0} = y_{0.146} \approx \$1,599$ (14.6th percentile)
- $y_{1-p_0} = y_{0.854} \approx \$13,690$ (85.4th percentile)

Step 2: Compute trimmed means.

- Mean (treated, trimmed top 14.6%): $\approx \$5,871$
- Mean (treated, trimmed bottom 14.6%): $\approx \$9,707$

- Mean (control employed): $\$7,049$

Step 3: Compute bounds.

$$\text{Lower bound} = 5,871 - 7,049 = \boxed{-\$1,178}$$
$$\text{Upper bound} = 9,707 - 7,049 = \boxed{\$2,658}$$

Lee bounds: $[-\$1,178, \$2,658]$
Comparison to Manski:

	Lower	Upper
Manski bounds	$-\$19,535$	$\$16,448$
Lee bounds	$-\$1,178$	$\$2,658$

Lee bounds are **dramatically tighter**—width of \$3,836 vs. \$35,983 (89% reduction).

Conclusion: Lee bounds **include zero**, so we cannot definitively conclude that training has a positive effect on wages for always-employed workers. However, the bounds are much more informative than Manski bounds, ruling out large negative or positive effects.

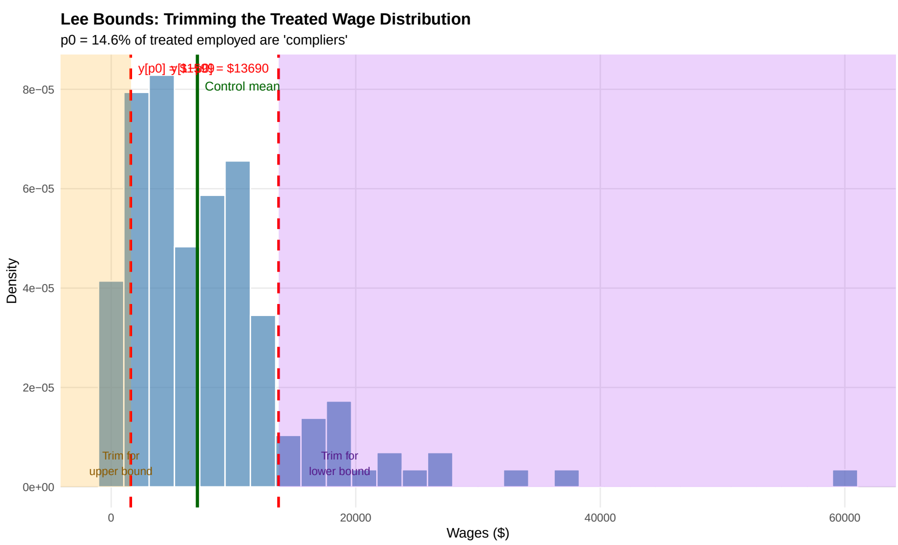
Q7. *(1 pt)* Create two visualizations: (a) histogram with trimming, (b) bounds comparison.

Solution:

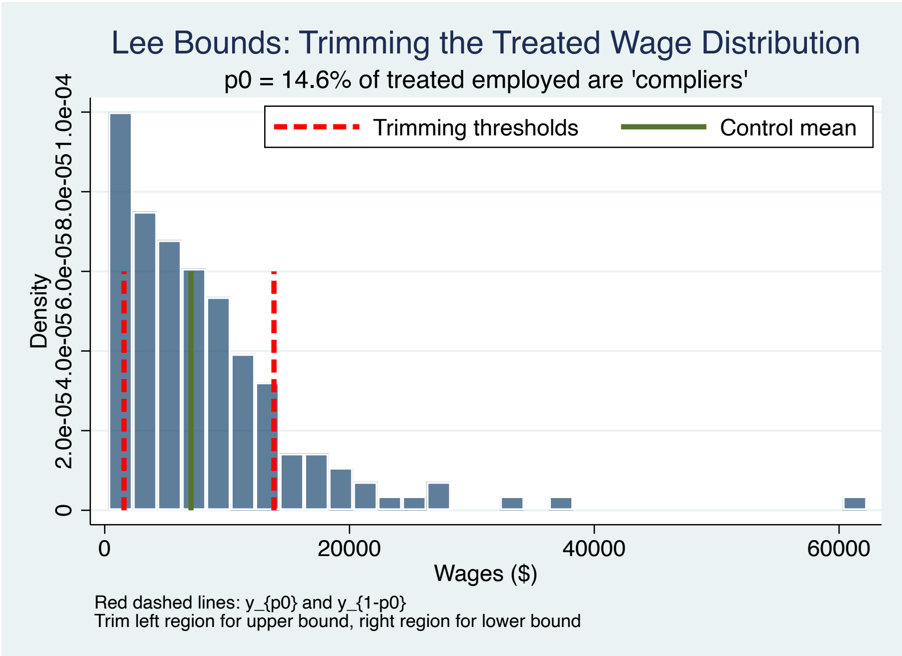
See R code in `pset2_lee_bounds.R`.
(a) Histogram with trimming:

- Wage distribution among treated employed (histogram)
- Vertical dashed lines at $y_{p_0} \approx \$1,599$ and $y_{1-p_0} \approx \$13,690$
- Shaded region on left (orange): trimmed for upper bound
- Shaded region on right (purple): trimmed for lower bound
- Solid green line: control mean (\$7,049)

This shows how Lee bounds work: we trim $p_0 = 14.6\%$ from each tail and compare to controls.
R Output:



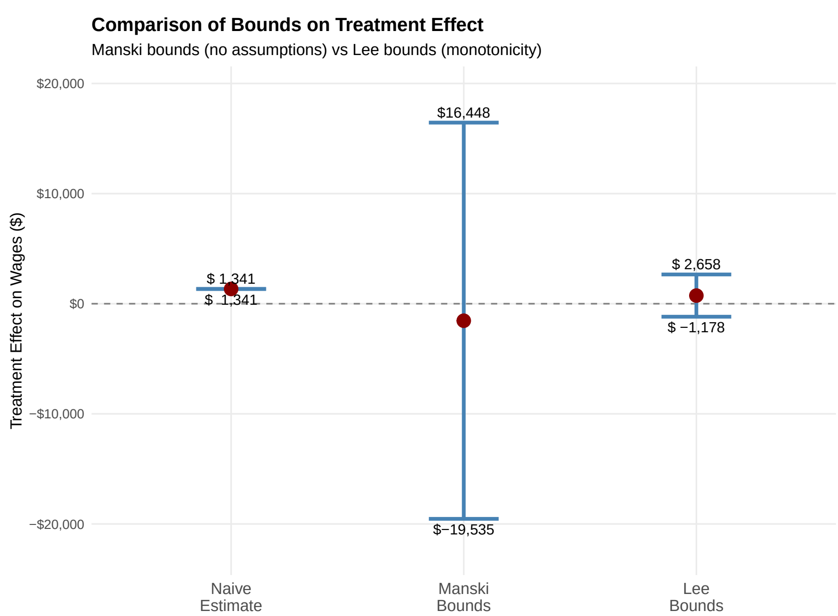
Stata Output:



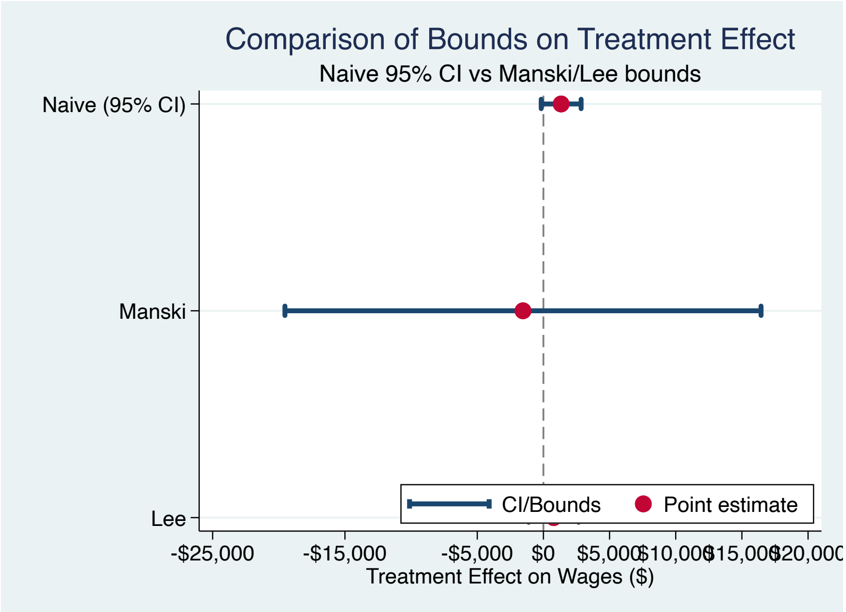
(b) Bounds comparison:
The comparison figure shows three estimates side-by-side with error bars representing bounds:

	Naive	Manski	Lee
Point estimate	\$1,341	—	—
Lower bound	—	$-\$19,535$	$-\$1,178$
Upper bound	—	\$16,448	\$2,658
Width	—	\$35,983	\$3,836

R Output:



Stata Output:



The figure dramatically illustrates:

1. Manski bounds span nearly \$36,000—almost uninformative
2. Lee bounds are 89% narrower—monotonicity is a powerful assumption
3. Both Manski and Lee bounds include zero, but Lee bounds rule out large effects
4. The naive estimate falls within both sets of bounds

Q8. *(1 pt)* Compute Lee bounds for log wages.

Solution:

Using log wages instead of levels:
Quantiles: (14.6th and 85.4th percentiles of log wages among treated employed)

- $\log(y_{p_0}) \approx 7.38$
- $\log(y_{1-p_0}) \approx 9.52$

Trimmed means:

- Mean log wage (treated, trimmed top 14.6%): ≈ 8.36
- Mean log wage (treated, trimmed bottom 14.6%): ≈ 8.93
- Mean log wage (control employed): ≈ 8.51

Lee bounds for log wages:

$$\text{Lower} = 8.36 - 8.51 = -0.15 \quad \text{Upper} = 8.93 - 8.51 = 0.42$$

Lee bounds: $[-0.15, 0.42]$
Comparison to Lee (2009) Table 5: The paper reports bounds of $[-0.083, 0.569]$ for NSW log wages. Our results are in a similar range but differ because:

- Different NSW sample (Lee uses a larger sample with additional years)

- Lee (2009) includes covariates to sharpen bounds
- Slightly different variable definitions

Interpretation: The log wage bounds include zero, suggesting we cannot rule out a null effect on log wages for always-employed. The effect is bounded between -15% and $+42\%$.