

# When is TSLS actually LATE?

## When is TSLS weakly causal?

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# Outline

## 1 Introduction

|                                  |   |
|----------------------------------|---|
| 1.1 The Common TSLS Recipe ..... | 2 |
| 1.2 Motivation .....             | 5 |

## 2 Binary $T$ and $Z$

## 3 What is Weak Causality?

## 4 When do we have WC?

## 5 Practical Solutions

|                          |    |
|--------------------------|----|
| 5.1 The New Recipe ..... | 22 |
| 5.2 Intuition .....      | 23 |

## 6 Implementation

|                                      |    |
|--------------------------------------|----|
| 6.1 Card (1995) .....                | 26 |
| 6.2 Nunn and Wantchekon (2011) ..... | 28 |
| 6.3 MacDonald et al. (2018) .....    | 29 |

## 7 Discussion

## References

# 1.1 The Common TSLS Recipe

## Selection on unobservables

We have an outcome variable  $Y$ , a discrete, ordered treatment  $T \in \mathcal{T} = \{t_0, t_1, \dots, t_J\}$ , a discrete instrument  $Z \in \mathcal{Z}$ .

### First Stage:

$$T = \alpha_1 + \beta_1 Z + v$$

### Second Stage:

$$Y = \alpha_2 + \beta_{\text{tsls}} \hat{T} + \varepsilon$$

We assume:

- Relevance, i.e.  $\text{Cov}(Z, T) \neq 0$ ,
- Exclusion, i.e.  $\text{Cov}(Z, \varepsilon) = 0$ .
- Monotonicity, i.e. no defiers.

$\beta_{\text{tsls}} = \frac{\text{Cov}(Y, Z)}{\text{Cov}(T, Z)}$  identifies LATE of compliers. There is nothing wrong with this classical setup.

# 1.1 The Common TSLS Recipe

## Selection on unobservables & observables

We have  $Y, T \in \mathcal{T} = \{t_0, t_1, \dots, t_J\}$ ,  $Z \in \mathcal{Z}$ , and an observed covariate matrix  $X$ .

### First Stage:

$$T = \alpha_1 + \beta_1 Z + \gamma_1 X + v$$

### Second Stage:

$$Y = \alpha_2 + \beta_{\text{tsls}} \hat{T} + \gamma_2 X + \varepsilon$$

We additionally assume **exogeneity (EX)**: conditional on  $X$ , the assignment of  $Z$  is independent of both the potential treatment and potential outcomes, i.e.  $(\{T(z)\}_{z \in \mathcal{Z}}, \{Y(t)\}_{t \in \mathcal{T}}) \perp Z \mid X$ .

**Typically,  $\beta_{\text{tsls}}$  does not have a causal interpretation and is not LATE.**

## 1.1 The Common TSLS Recipe

Angrist and Pischke, 2009:

“TSLS with covariates produces an average of covariate-specific LATEs.” – under saturated covariates **and** instrument (Saturate and weight, SW).

| <b>IV Paper (n=122)</b>    | <b>N</b> | <b>%</b> | <b>TSLS + Covariates (n=99)</b> | <b>N</b> |
|----------------------------|----------|----------|---------------------------------|----------|
| Used TSLS                  | 112      | 92       | Followed SW                     | 1        |
| TSLS + Covariates          | 99       | 81       | Saturated in covariates         | 4        |
| TSLS + Covariates, as LATE | 30       | 25       | Not saturated in covariates     | 94       |

General critiques on IV-LATE: (Robins and Greenland, 1996; Heckman, 1997; Angrist and Imbens, 1999; Deaton, 2010; Imbens, 2010; Swanson and Hernan, 2014)

- complier-specific, misleading for population analysis;
- vulnerable to heterogeneous treatment effects;
- assumptions like EX are hard to empirically justify.

## 1.2 Motivation

### Def. Saturate and weight (SW):

Given a treatment variable  $T$ , let  $X$  take values in  $\mathcal{X} = \{x_1, \dots, x_L\}$ , and  $Z$  in  $\mathcal{Z} = \{z_1, \dots, z_K\}$ . The TSLS model takes  $C$  as covariates and  $I$  as instrument, where:

- $C = [1, \mathbb{I}(X = x_l) : l = 2, \dots, L]'$ , i.e., one coefficient for each possible  $x$  on its entire support;
- $I = [\mathbb{I}(Z = z_k), \mathbb{I}(Z = z_k)\mathbb{I}(X = x_l) : l = 2, \dots, L; k = 2, \dots, K]'$ , i.e. the instrument vector consists of each  $Z$  value and the interaction term between each specific  $z, x$  on their supports.

SW is **not possible** if any of the variables takes continuous values, and is rarely feasible for empirical studies that include many covariates.

SW may also incur **many instruments bias** due to the excessive instruments generated from  $\mathbb{I}(Z = z_k)\mathbb{I}(X = x_l)$ , biasing it towards the OLS estimator.

## 1.2 Motivation

Practical relevance:

- TSLS with covariates is popular, with  $\beta_{\text{tsls}}$  often interpreted as LATE of compliers;
- SW is a highly theoretical setup, and is not favoured by empirical studies;
- Without SW, does the LATE interpretation still apply?

Unsolved problem:

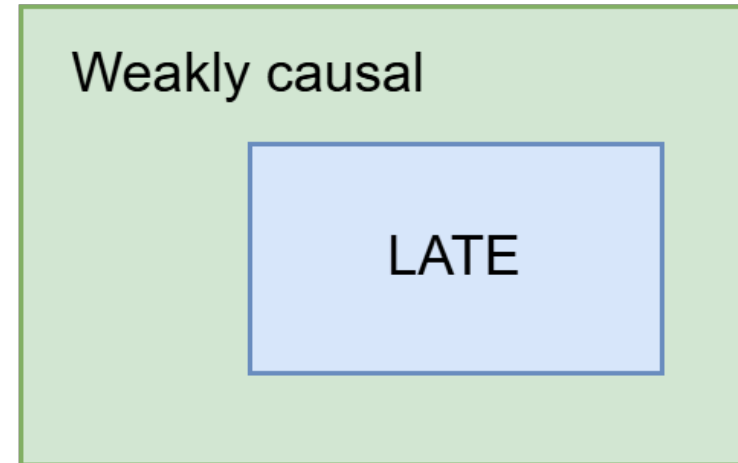
- Angrist and Krueger, 1999: [With IV estimates in models with unsaturated covariates], it seems reasonable to assume that some sort of approximate weighted average is being generated, but we are unaware of a precise causal interpretation that fits all cases.

Related effort:

- Słoczyński, 2020, 2024: focuses on incorrect monotonicity and thus the impact of negative weights.
- Evdokimov & Kolesár, 2018: on treatment effect heterogeneity, and sufficient conditions for LATE.
- Abadie, 2003: theoretical refinement under constant treatment effect assumption.

## 1.2 Motivation

$$\beta_{\text{tsls}} \notin$$



When is TSLS actually LATE?

When is TSLS weakly causal?



# Outline

## 1 Introduction

|                                  |   |
|----------------------------------|---|
| 1.1 The Common TSLS Recipe ..... | 2 |
| 1.2 Motivation .....             | 5 |

## 2 Binary $T$ and $Z$

## 3 What is Weak Causality?

## 4 When do we have WC?

## 5 Practical Solutions

|                          |    |
|--------------------------|----|
| 5.1 The New Recipe ..... | 22 |
| 5.2 Intuition .....      | 23 |

## 6 Implementation

|                                      |    |
|--------------------------------------|----|
| 6.1 Card (1995) .....                | 26 |
| 6.2 Nunn and Wantchekon (2011) ..... | 28 |
| 6.3 MacDonald et al. (2018) .....    | 29 |

## 7 Discussion

## References

## 2 Binary $T$ and $Z$

How education affects income? From Card (1995), adapted.

- $N = 3010$  individuals
- $Y$ : continuous, log hourly wage
- $T$ : binary, received education  $> 12$  yrs
- $Z$ : binary, presence of college nearby
- $X$ : 4 binary covariates: work experience  $> 8$  yrs, 3 indicators for race and location of residence.

We assume:

1. Exogeneity:  $Z \perp (Y(0), Y(1), T(1), T(0)) \mid X$ ;
2. Monotonicity:  $\mathbb{P}[T(1) \geq T(0)] = 1$  (no defiers).

## 2 Binary $T$ and $Z$

Since  $T, Z, X$  are binary, we can do SW.

```
1 # OLS
2 lm(Y ~ D + X)
3 # IV without saturation:
4 ivreg(Y ~ X + D | X + Z)
5 # IV with saturated covariates:
6 Z_int <- cbind(Z, Z * X1, Z * X2, Z * X3, Z * X4, Z *
7 X1 * X2, Z * X1 * X3, Z * X1 * X4, Z * X2 * X3, Z *
X2 * X4, Z * X3 * X4, Z * X1 * X2 * X3, Z * X1 * X2 *
X4, Z * X1 * X3 * X4, Z * X2 * X3 * X4, Z * X1 * X2 *
X3 * X4)
7 ivreg(Y ~ X1*X2*X3*X4 + D | X1*X2*X3*X4 + Z_int)
```

| Method            | Coefficient on $D$ | SE    |
|-------------------|--------------------|-------|
| OLS               | 0.185 ***          | 0.016 |
| TSLs, no covar.   | 1.317 ***          | 0.232 |
| TSLs, unsaturated | 0.778 *            | 0.353 |
| TSLs, SW          | 0.412 *            | 0.179 |

RESET-test on `lm(Z~X)`:  $p = 0.000$ , i.e. no rich covariates.

Overestimated causal effect:  $\frac{\beta_{\text{tsls}} - \beta_{\text{sw}}}{\beta_{\text{tsls}}} = 47\%$ ?

Overestimated selection bias:  $\left| \frac{\beta_{\text{tsls}} - \beta_{\text{sw}}}{\beta_{\text{ols}} - \beta_{\text{sw}}} \right| = 161\%$ ?

Consider many instruments bias.

## 2 Binary $T$ and $Z$

We divide the population into groups according to potential treatment:

$$G := (T(0), T(1)) = \begin{cases} (0, 1) =: \text{CP} \\ (1, 1) =: \text{AT} \in \mathcal{G} \\ (0, 0) =: \text{NT} \end{cases}$$

Denote the conditional average treatment effect as  $\text{CATE}(g, x) := \mathbb{E}[Y(1) - Y(0) \mid G = g, X = x]$ .

We introduce one of the key findings, [Prop.2](#), in a minimalist style.

**Proposition 2.** Assume potential outcome is linearly predicted by  $X$ .

Then, the  $\beta_{\text{tsls}}$  can be decomposed into the sum of weighted average of CATEs of CP, AT, and NT. That is:

$$\beta_{\text{tsls}} = \sum_{g \in \mathcal{G}} \mathbb{E}[\omega(g, x) \cdot \text{CATE}(g, x)]$$

where for AT and NT,  $\omega(g, x) = \mathbb{E}[\tilde{Z}|X] \mathbb{P}[G = g|X] \mathbb{E}[\tilde{Z}T]^{-1}$ , while  $\omega(\text{CP}, x) > 0$ .

## 2 Binary $T$ and $Z$

- The signs of weights on AT and NT are determined by  $\mathbb{E}[\tilde{Z}|X]$ .
- $\tilde{Z}$  is the residuals from the regression  $Z \sim X$ .
- If and only if  $\mathbb{E}[\tilde{Z}|X] = 0$ , a condition defined as having **rich covariates**,  $\beta_{\text{tsls}}$  only reflects the positively weighted treatment effects on compliers, and is thus LATE.
- If without rich covariates,  $\beta_{\text{tsls}}$  reflects all three groups. As  $\mathbb{E}[\tilde{Z}|X]$  is possible to take negative values for any data, AT and NT can be negatively weighted, thus biasing the direction of treatment effect interpretation.
- Negative weighting on some treatment groups is a violation to what the authors define as **weakly causal**.

# Outline

## 1 Introduction

|                                  |   |
|----------------------------------|---|
| 1.1 The Common TSLS Recipe ..... | 2 |
| 1.2 Motivation .....             | 5 |

## 2 Binary $T$ and $Z$

## 3 What is Weak Causality?

## 4 When do we have WC?

## 5 Practical Solutions

|                          |    |
|--------------------------|----|
| 5.1 The New Recipe ..... | 22 |
| 5.2 Intuition .....      | 23 |

## 6 Implementation

|                                      |    |
|--------------------------------------|----|
| 6.1 Card (1995) .....                | 26 |
| 6.2 Nunn and Wantchekon (2011) ..... | 28 |
| 6.3 MacDonald et al. (2018) .....    | 29 |

## 7 Discussion

## References

### 3 What is Weak Causality?

Assume throughout that we have  $T \in \{0, 1\}$ ,  $Z \in \{0, 1\}$ .

**Def. Weakly causal (WC):** an estimand  $\beta$  is WC iff :

$\forall g \in \mathcal{G}$ , and every  $x \in \mathcal{X}$ , if  $\text{CATE}(g, x) \geq 0$ , then  $\beta \geq 0$

$\forall g \in \mathcal{G}$ , and every  $x \in \mathcal{X}$ , if  $\text{CATE}(g, x) \leq 0$ , then  $\beta \leq 0$

Intuitively, if taking the treatment always gives higher potential outcome for every group, then the estimator should also be positive. A WC estimator is not systematically wrong in its direction.

Under this weak criterion, the trivial estimand  $\beta = 0$  is also WC.

We want to further investigate some parametric or semi-parametric settings that translate into weak causality.

## 3 What is Weak Causality?

The decomposition in [Prop.2](#) can be rewritten with a different definition of weights as:

### Proposition 3.

$$\beta = \sum_{g,x} \overbrace{\omega_0(g,x) \mathbb{E}[Y(0)|g,x]}^{\text{baseline level w/o treatment}} + \sum_{g,x} \overbrace{\omega_1(g,x) \text{CATE}(g,x)}^{\text{diff. in outcome w. treatment}}$$

where  $\omega_0(g,x) = \mathbb{E}[\tilde{Z}T]^{-1} \mathbb{E}[\tilde{Z}|g,x] \mathbb{P}(g,x)$ , and  $\omega_1(g,x) = \mathbb{E}[\tilde{Z}T]^{-1} \mathbb{E}[\mathbb{I}(T=1)\tilde{Z}|g,x] \mathbb{P}(g,x)$

**Proposition 4.** Given **BF**, **EX**, and suppose further **GR**, then  $\beta$  is WC iff

1. (Non-negative weights):  $\forall g, x, \omega_1(g,x) \geq 0$ ;
2. (Baseline irrelevance):  $\forall g, x, \omega_0(g,x) = 0$ .

Intuitively,

- Each choice group is strictly non-negatively weighted;
- The baseline level of outcome where the treatment is absent does not flow into  $\beta$ .



# Outline

## 1 Introduction

|                                  |   |
|----------------------------------|---|
| 1.1 The Common TSLS Recipe ..... | 2 |
| 1.2 Motivation .....             | 5 |

## 2 Binary $T$ and $Z$

## 3 What is Weak Causality?

## 4 When do we have WC?

## 5 Practical Solutions

|                          |    |
|--------------------------|----|
| 5.1 The New Recipe ..... | 22 |
| 5.2 Intuition .....      | 23 |

## 6 Implementation

|                                      |    |
|--------------------------------------|----|
| 6.1 Card (1995) .....                | 26 |
| 6.2 Nunn and Wantchekon (2011) ..... | 28 |
| 6.3 MacDonald et al. (2018) .....    | 29 |

## 7 Discussion

## References

## 4 When do we have WC?

For binary  $T$  and  $Z$ .

**Assumption EX:**  $Z \perp (Y(0), Y(1), T(1), T(0)) \mid X$

**Def. Rich covariates (RC):** Let  $\mathbb{L}[Z|X = x]$  be the population fitted values at  $X = x$  from the linear regression  $Z \sim X$ . An IV specification has RC if:

$$\forall x \in \mathcal{X}, \mathbb{L}[Z|X = x] = \mathbb{E}[Z|X = x] \Leftrightarrow \forall x \in \mathcal{X}, \mathbb{E}[\tilde{Z}|X] = 0$$

**Saturation** guarantees **RC**. If we have **RC** without saturating, it's called “correctly specified”.

**Assumption MN:** Monotonicity.  $\forall x \in \mathcal{X}, \mathbb{P}[T(0) \leq T(1) \mid X = x] = 1$ .

Intuitively, receiving the instrument does not decrease potential treatment, for all  $x$ .<sup>1</sup>

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<sup>1</sup>Strong monotonicity. For results under weaker restrictions see Blandhol et al. 2022 (pre-revision).

## 4 When do we have WC?

**Theorem 1.** Suppose **EX** and **MN** are satisfied, then  $\beta_{\text{tsls}}$  is WC if and only if **RC**.

Intuitively, **Th.1** states that without **RC**, then baseline-irrelevance in **Prop.4 & 5** does not hold, and the estimator is not WC.

Recall **baseline irrelevance**, which requires:

$$\forall g, x, \omega_0(g, x) = \mathbb{E}[\tilde{Z}T]^{-1} \mathbb{E}[\tilde{Z}|G = g, X = x] \mathbb{P}(g, x) = 0$$

From **EX**, we have  $\tilde{Z} \perp G \mid X$ . Therefore,  $\mathbb{E}[\tilde{Z}|G = g, X = x] = \mathbb{E}[\tilde{Z}|X = x]$ . With  $\mathbb{P}(g, x) > 0$ , this holds iff  $\mathbb{E}[\tilde{Z}|X = x] = 0$ , i.e. **RC**.

The  $\tilde{Z}$  problem stems from the practice of interpreting the linear TSLS estimator through yet a nonparametric setup. The linear-regression-based TSLS was meant to serve linear IV models.

## 4 When do we have WC?

If **MN** fails, **RC** does not imply an WC estimator. We have to additionally restrict treatment effect homogeneity.

**Assumption CLE:** constant, linear effects. Order  $T$  into  $\{t_0, t_1, \dots, t_J\}$ . There exists an  $\delta \in \mathbb{R}$ , such that  $\mathbb{E}[Y(t_j) - Y(t_{j-1}) | g, x] = \delta(t_j - t_{j-1})$  for every  $j \geq 1, g \in \mathcal{G}, x \in \mathcal{X}$ .

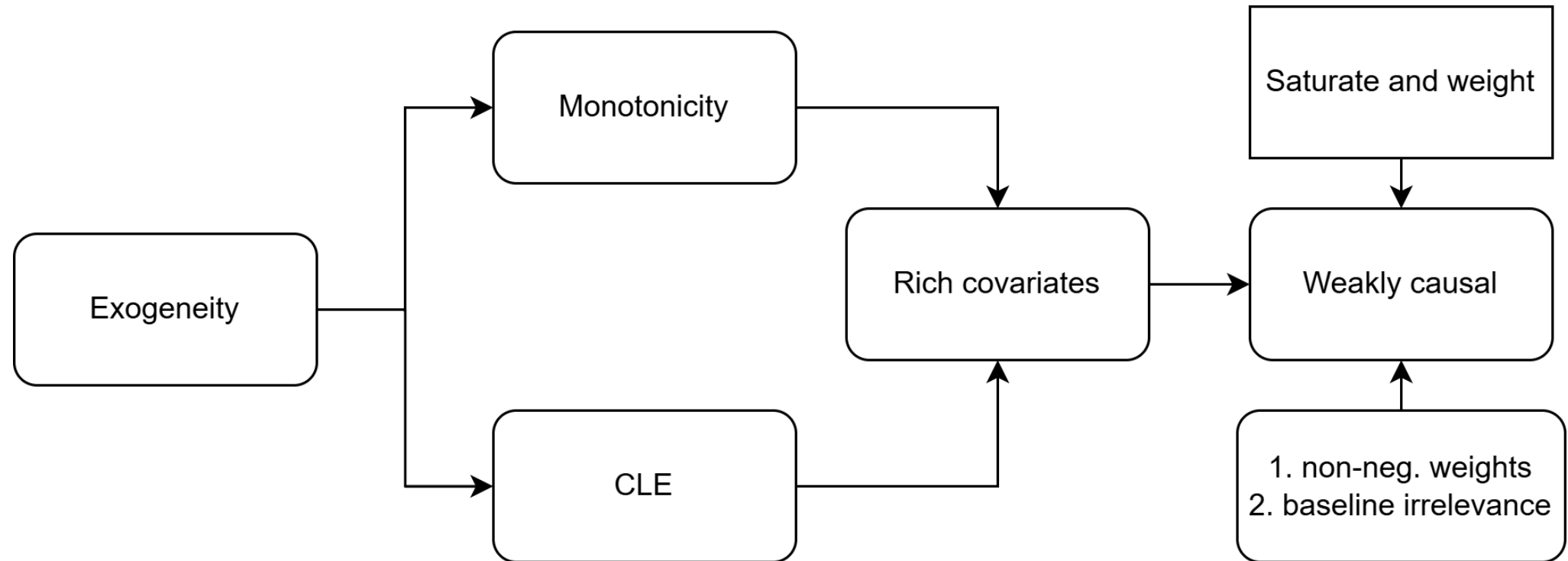
Intuitively, the mean difference in potential outcome is linearly proportional to the difference in treatment levels, for all groups and all  $x$ .

For binary  $T, Z$ , this simplifies to  $\forall g, x, \text{CATE}(g, x) = \delta$ .

**Proposition 5.** Suppose **EX** and **CLE** are satisfied, then  $\beta_{\text{tsls}}$  is WC iff **RC**.

## 4 When do we have WC?

Wrap-up:



# Outline

## 1 Introduction

|                                  |   |
|----------------------------------|---|
| 1.1 The Common TSLS Recipe ..... | 2 |
| 1.2 Motivation .....             | 5 |

## 2 Binary $T$ and $Z$

## 3 What is Weak Causality?

## 4 When do we have WC?

## 5 Practical Solutions

|                          |    |
|--------------------------|----|
| 5.1 The New Recipe ..... | 22 |
| 5.2 Intuition .....      | 23 |

## 6 Implementation

|                                      |    |
|--------------------------------------|----|
| 6.1 Card (1995) .....                | 26 |
| 6.2 Nunn and Wantchekon (2011) ..... | 28 |
| 6.3 MacDonald et al. (2018) .....    | 29 |

## 7 Discussion

## References

## 5.1 The New Recipe

1. If covariates are not essential for justifying instrument exogeneity, then do TSLS without covariates.
2. If covariates need to be included, think twice on including **which** covariates. The more included, the more likely that the specification is not rich in covariates.
3. Use RESET-test (Ramsey, 1969) **lmtest::resettest()** to examine covariate richness.
4. In general, the linear TSLS estimator can be complemented with:
  - DDML-PLIV, which produces  $\beta_{\text{pliv}} \approx \beta_{\text{rich}}$ .
  - If  $Z, T$  are binary, DDML-LATE identifies LATE of compliers.

## 5.2 Intuition

We have discussed that  $\tilde{Z}$  became a problem because we tried to impose the linear IV model onto an unknown data structure which, in practice, is hardly linear.

Instead, we consider the following **partially linear IV model (PLIV)**:

$$Y = \beta_{\text{pliv}}T + f(X) + \varepsilon, \quad \mathbb{E}[\varepsilon|X, Z] = 0$$

$$Z = g(X) + v, \quad \mathbb{E}[v|X] = 0$$

That is, we only assume linearity on  $T$ , and try to approximate the non-linear functions  $f, g$ . A reasonable way is to do **machine learning**.

Problems: causal interpretation is vulnerable to overfitting, regularization bias and estimation error. **DDML** (Double/debiased machine learning) uses orthogonalization and data splitting to solve these.



## 5.2 Intuition

DDML also applies a group of base learners, e.g. OLS, LASSO, XGBoost, RF, and then outputs the  $\beta_{\text{pliv}}$  estimate from an ensemble model.

It can be proven that  $\beta_{\text{pliv}}$  approximates  $\beta_{\text{rich}}$  under correctly specified linear IV, without additional parametric assumptions.

If we have  $T \in \{0, 1\}$  and  $Z \in \{0, 1\}$ , then a three-stage framework will approximate the LATE estimator for compliers  $\beta_{\text{late}}$ .

Implemented in the R-Package **ddml** (Chernozhukov et al., 2018; Ahrens et al., 2024).

Trade-offs:

- Correctly specified TSLS might be rare and hard to justify.
- Saturation is generally infeasible, or entails large variance if high-dimensional.
- DDML-PLIV is precise, but seems to have a slight systematic bias.

# Outline

## 1 Introduction

|                                  |   |
|----------------------------------|---|
| 1.1 The Common TSLS Recipe ..... | 2 |
| 1.2 Motivation .....             | 5 |

## 2 Binary $T$ and $Z$

## 3 What is Weak Causality?

## 4 When do we have WC?

## 5 Practical Solutions

|                          |    |
|--------------------------|----|
| 5.1 The New Recipe ..... | 22 |
| 5.2 Intuition .....      | 23 |

## 6 Implementation

|                                      |    |
|--------------------------------------|----|
| 6.1 Card (1995) .....                | 26 |
| 6.2 Nunn and Wantchekon (2011) ..... | 28 |
| 6.3 MacDonald et al. (2018) .....    | 29 |

## 7 Discussion

## References

## 6.1 Card (1995)

We extend the previous adapted Card (1995) experiment.

```
1 library(ddml) r
2 set.seed(123)
3 # Estimate the PLIV using short-stacking with base
  learners ols, rlasso, random forest, and xgboost.
4 learners_multiple <- list(list(fun = ols), list(fun
  = mdl_glmnet), list(fun = mdl_ranger), list(fun =
  mdl_xgboost))
5 pliv_fit_short <- ddml_pliv(Y, D, Z, X,
6   learners = learners_multiple,
7   ensemble_type = c('nnls', 'singlebest', 'average'),
8   shortstack = TRUE,
9   sample_folds = 10, cv_folds = 10)
```

| Method            | Coefficient on $D$ | SE    |
|-------------------|--------------------|-------|
| OLS               | 0.185              | 0.016 |
| TSLs, no covar.   | 1.317              | 0.232 |
| TSLs, unsaturated | 0.778              | 0.353 |
| TSLs, SW          | 0.412              | 0.179 |
| TSLs, sat. in $X$ | 0.913              | 0.439 |
| DDML-PLIV         | 0.912              | 0.432 |
| DDML-LATE         | 0.662              | 0.325 |

- Speculated many instruments bias: 55%
- Overestimated causal effect:  $\frac{\beta_{\text{tsls}} - \beta_{\text{pliv}}}{\beta_{\text{tsls}}} = -17\%$
- Overestimated selection bias:  $\left| \frac{\beta_{\text{tsls}} - \beta_{\text{pliv}}}{\beta_{\text{ols}} - \beta_{\text{pliv}}} \right| = 18\%$

When is TSLs actually LATE?

When is TSLs weakly causal?

## 6.1 Card (1995)

The original Card (1995) data:

- $N = 3010$  individuals
- $Y$ : cont. log hourly wage
- $T$ : cont. years of education
- $Z$ : binary, presence of college nearby
- $X$ : cont. years of experience, and indicators on race, location of residence, etc. (14 co-variates)
- RESET-test on  $\text{lm}(Z \sim X)$ :  $p = 0.000$

| Method            | Coefficient on $D$ | SE    |
|-------------------|--------------------|-------|
| OLS               | 0.075              | 0.003 |
| TSLS, no covar.   | 0.188              | 0.026 |
| TSLS, unsaturated | 0.132              | 0.054 |
| TSLS, SW          | -                  | -     |
| TSLS, sat. in $X$ | -                  | -     |
| DDML-PLIV         | 0.138              | 0.050 |
| DDML-LATE         | 0.068              | 0.043 |

- Overestimated causal effect:  $-4.5\%$
- Overestimated selection bias:  $9.5\%$
- Relatively low bias among other works.

## 6.2 Nunn and Wantchekon (2011)

Effect of the slave trade on modern day level of trust in Africa. The data<sup>1</sup>:

- $N = 16679$  individuals
- $Y$ : cat. level of trust in neighbors
- $T$ : cont. log of total slave exports
- $Z$ : cont. distance from the nearest coast
- $X$ : 93 covariates.
- RESET-test on  $\text{lm}(Z \sim X)$ :  $p = 0.000$

| Method            | Coefficient on $D$ | SE    |
|-------------------|--------------------|-------|
| OLS               | -0.203             | 0.033 |
| TSLS, no covar.   | -0.190             | 0.111 |
| TSLS, unsaturated | -0.271             | 0.088 |
| TSLS, SW          | -                  | -     |
| TSLS, sat. in $X$ | -                  | -     |
| DDML-PLIV         | -0.071             | 0.091 |
| DDML-LATE         | -                  | -     |

- Overestimated causal effect: 73.8%
- Overestimated selection bias: 151.5%
- The authors' key finding was invalid!
- Cited by 2800+.

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<sup>1</sup>Result of this table is currently cited from Table 3(2), Blandhol et al. 2025.

## 6.3 MacDonald et al. (2018)

Increased human-forest interaction = more Lyme disease incidence? The data<sup>1</sup>:

- $N = 514$  counties, USA
- $Y$ : cont. log Lyme disease incidence
- $T$ : cont. population% living near forest
- $Z$ : cont, land use regulation score
- $X$ : year, forest%, forest edge density, average forest patch area in county (4 cont.)
- Binary:  $T > \text{median}$ ,  $Z > 0$ .

|                   | Original      | Binary        |
|-------------------|---------------|---------------|
| OLS               | 0.008 (0.004) | 0.332 (0.262) |
| TSLS, no covar.   | 0.037 (0.009) | 2.995 (0.967) |
| TSLS, unsaturated | 0.037 (0.014) | 3.473 (1.910) |
| DDML-PLIV         | 0.030 (0.009) | 2.872 (1.522) |
| DDML-LATE         | -             | 0.922 (0.693) |
| RESET p-Value     | 0.579         | 0.0001        |

- Careful theoretical justification
- Ignored saturation, but had RC (by chance?)
- Interpreted as in simple linear relationship, not directly mentioning LATE or causality.
- Systematic bias of DDML is visible.

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<sup>1</sup>Only complete cases, used cluster-robust standard errors with contiguous counties as clusters.

# Outline

## 1 Introduction

|                                  |   |
|----------------------------------|---|
| 1.1 The Common TSLS Recipe ..... | 2 |
| 1.2 Motivation .....             | 5 |

## 2 Binary $T$ and $Z$

## 3 What is Weak Causality?

## 4 When do we have WC?

## 5 Practical Solutions

|                          |    |
|--------------------------|----|
| 5.1 The New Recipe ..... | 22 |
| 5.2 Intuition .....      | 23 |

## 6 Implementation

|                                      |    |
|--------------------------------------|----|
| 6.1 Card (1995) .....                | 26 |
| 6.2 Nunn and Wantchekon (2011) ..... | 28 |
| 6.3 MacDonald et al. (2018) .....    | 29 |

## 7 Discussion

## References

# 7 Discussion

## Contributions

- Extended Angrist and Krueger 1999, by providing a precise interpretation for the TSLS estimate with covariates that is generally applicable.
- Proved that TSLS with covariates, if not under strong assumptions, cannot provide weakly causal interpretation, and thus cannot identify LATE.
- Extended previous efforts from, e.g. Słoczyński and Kolesár, by combining covariate richness, treatment effect heterogeneity, and monotonicity under a more holistic investigation with more general setups.
- For empirical researchers, it showed TSLS with covariates is a common practice, urges caution when doing so, provided a guided recipe on choosing IV specifications, and also discussed a few alternative methods to linear IV.



## 7 Discussion

### Limitations

- It is clear that  $\text{LATE} \Rightarrow \text{WC}$ , but how WC relates to LATE needs to be specified. At least we know, empirically,  $\beta_{\text{rich}} \neq \beta_{\text{late}}$ . The weights are empirically inaccessible.
- Similarly, the estimator DDML-PLIV is generally hard to interpret. Only in binary cases, we get DDML-LATE for compliers. Further research is needed.

### Notes

- Further theoretical and empirical investigation could be done on analogous methods, e.g. 2SRI. Consider a more thorough survey on previous empirical results in diverse fields.
- Call for good practice: Discussed results from 13 papers, selected based on publicly available data and reproducible results, out of 99 surveyed in total. Itself does not publish code.
- My codes are available on GitHub: [https://github.com/Yc-Han/Causal\\_TSLs](https://github.com/Yc-Han/Causal_TSLs).

# Outline

## 1 Introduction

|                                  |   |
|----------------------------------|---|
| 1.1 The Common TSLS Recipe ..... | 2 |
| 1.2 Motivation .....             | 5 |

## 2 Binary $T$ and $Z$

## 3 What is Weak Causality?

## 4 When do we have WC?

## 5 Practical Solutions

|                          |    |
|--------------------------|----|
| 5.1 The New Recipe ..... | 22 |
| 5.2 Intuition .....      | 23 |

## 6 Implementation

|                                      |    |
|--------------------------------------|----|
| 6.1 Card (1995) .....                | 26 |
| 6.2 Nunn and Wantchekon (2011) ..... | 28 |
| 6.3 MacDonald et al. (2018) .....    | 29 |

## 7 Discussion

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