Causal Machine Learning

Post-Double-Selection Procedure

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Reference

Belloni, Chernozhukov, and Hansen (2014): "High-Dimensional Methods and Inference on Structural and Treatment Effects", Journal of Economic Perspectives, 28 (2), pp. 29-50, download.

Outline

Selection Bias

Selection-on-Observables Identification Strategy

Power of Conditioning

Multivariate Regression

Post-Double-Selection Procedure

Impact Evaluation

- Impact evaluation is a fascinating field of econometrics.
- It allows to make policy recommendations and business decisions.
- It enables to answer questions like:
  What is the causal impact of variable D on variable Y?
- Examples include:
  - What is the causal effect of one additional year of education on wages?
  - Do micro-finance programs reduce poverty in developing countries?
  - What is the causal effect of value added taxes on customer purchases?
  - How large is the incumbency advantage in elections?
  - What is the causal effect of a marketing campaign on revenues?

$\Rightarrow$ The ability to conduct and/or interpret an impact evaluation study is useful in (almost) every field of economics and management!

Causal Effect

- Lets say we want to analyse the causal effect of participation in a job search assistance course on earnings
- D is a dummy indicating the participation in the job search assistance course
  - $D = 1$ under participation
  - $D = 0$ under non-participation

  $\rightarrow$ We often call this the treatment dummy
- Potential outcomes:
  - Y^1 denotes the potential earnings under participation in the job search assistance course
  - Y^0 denotes the potential earnings under non-participation in the job search assistance course
- The expected causal effect for a randomly selected individual from the population (Average Treatment Effect, ATE) is

$$\text{ATE} = E[Y^1] - E[Y^0]$$

Stable Unit Treatment Value Assumption (SUTVA)

$$Y = D \cdot Y^1 + (1 - D) \cdot Y^0 \tag{1}$$

1. This assumption states that there are only two types of treatment (participation and non-participation),
   - The job search assistance course is always the same (no heterogeneous treatments, e.g., the duration of the course should not vary)
   - There are no alternative treatments (e.g., there should be no substitute for job search assistance for non-participants)
2. It excludes general equilibrium effects of the job search assistance course
   - Spillover effects could occur if course participants would inform non-participants about the course contents
   - Crowding-out effects could occur when course participants get jobs that would be devoted to non-participants in the absence of the course
   - Large courses could have externalities on the business cycle

$\Rightarrow$ We refer to (1) often as the "observational rule" (OR)

Selection Bias

- We assume that $0 < Pr(D = 1) < 1$
- Naive estimation strategy:

$$
\begin{aligned}
E[Y|D=1] - E[Y|D=0] \overset{OR}{=} & E[Y^1|D=1] - E[Y^0|D=0] \\
= & \underbrace{(E[Y^1|D=1] - E[Y^0|D=1])}_{\text{ATET}} \\
& + \underbrace{(E[Y^0|D=1] - E[Y^0|D=0])}_{\text{Selection Bias ATET}} \\
= & \underbrace{(E[Y^1|D=0] - E[Y^0|D=0])}_{\text{ATENT}} \\
& + \underbrace{(E[Y^1|D=1] - E[Y^1|D=0])}_{\text{Selection Bias ATENT}}
\end{aligned}
$$

- ATET: Average Treatment Effect on the Treated
- ATENT: Average Treatment Effect on the Non-Treated

Law of Iterative Expectations (LIE)

- Law of Iterative Expectations:

$$E[Y] = E[E[Y|X]] = E_X[E[Y|X]] = \int E[Y|X] f_X(x) dx$$

- Special case for dummy variables:

$$E[Y] = Pr(D=1) \cdot E[Y|D=1] + Pr(D=0) \cdot E[Y|D=0]$$

Average Treatment Effect (ATE):

$$\text{ATE} \stackrel{LIE}{=} Pr(D=1) \cdot \text{ATET} + Pr(D=0) \cdot \text{ATENT}$$

Randomised Experiments

- Randomised experiments are often called the "gold standard" of impact evaluation
- Under random assignment (RA), the potential outcomes are independent of the treatment, such that $(Y^1, Y^0) \perp\!\!\!\perp D$ is satisfied
  - ATET:
    $$E[Y^1|D=1] - E[Y^0|D=1] = E[Y^1] - E[Y^0] = \text{ATE}$$
  - ATENT:
    $$E[Y^1|D=0] - E[Y^0|D=0] = E[Y^1] - E[Y^0] = \text{ATE}$$
  - Selection Bias ATET:
    $$E[Y^0|D=1] - E[Y^0|D=0] = E[Y^0] - E[Y^0] = 0$$
  - Selection Bias ATENT:
    $$E[Y^1|D=1] - E[Y^1|D=0] = E[Y^1] - E[Y^1] = 0$$

Some Disadvantages Experiments

- Minimal social acceptance:
  - Would you agree to randomize the years of schooling for your children?
  - Would you agree to randomize police interventions to combat domestic violence?
- Randomisation technically impossible or impractical:
  - We cannot randomize climate change, gender, and incumbency.
  - Randomizing the Fed rate or value added taxes on the unit level is impractical (or even impossible).
- Costly and time consuming:
  - Poverty programs can be randomized, but the randomization can cause welfare losses during the experimental period.

Some Disadvantages Experiments

- External validity:
  - Are experiments carried-out with a small group of economic students externally valid?
- Imperfect compliance
  - We can randomize the offer to participate in training programs, but not everybody participates.
  - We can randomize phone calls of get-out-the-vote (GOTV) campaigns, but not everybody answers the phone.

$\Rightarrow$ There is need for alternative empirical strategies!

Outline

Notation

- D: Binary treatment dummy which can have values $d \in \{0, 1\}$
- Y^1, Y^0: Potential outcomes under treatment and non-treatment
- $Y = D \cdot Y^1 + (1 - D) \cdot Y^0$: Observed outcome with support $\mathcal{Y} \subseteq \mathbb{R}$ (we assume SUTVA throughout)
- X: K-dimensional vector of exogenous pre-treatment control variables which can have values $x \in \mathcal{X}$ (with $\mathcal{X} \subseteq \mathbb{R}^K$ being the support of X). The first element of X is a constant term.

Notation

- $\mu_d(x) = E[Y^d|X = x]$: Conditional expectation of the potential outcome Y^d (for $d \in \{0, 1\}$) when control variables have values x
- $p(x) = Pr(D = 1|X = x)$: Condition probability that $D = 1$ when control variables have values x (propensity score)
- We assume to observe i.i.d. (independent and identically distributed) data on the triple (Y, D, X) throughout

Individual Causal Effects

$$\delta_i = Y_i^1 - Y_i^0$$

for observation units $i = 1, ..., N$ (e.g., individuals)

- Most of the time we omit the subscript i for ease of notation. We only use it when needed for clarity.
- Here the subscript makes clear that we allow for heterogeneous effects of each observation units.
- However, individual causal effects can only be identified under unrealistic assumptions

Parameters of Interest

- Average Treatment Effects (ATE):

$$\delta = E[Y^1 - Y^0] = E[\delta_i]$$

- Average Treatment Effects on the Treated (ATET):

$$\theta = E[Y^1 - Y^0|D = 1] = E[\delta_i|D = 1]$$

- Average Treatment Effects on the Non-Treated (ATENT):

$$\rho = E[Y^1 - Y^0|D = 0] = E[\delta_i|D = 0]$$

- Conditional Average Treatment Effects (CATE):

$$\delta(x) = E[Y^1 - Y^0|X = x] = E[\delta_i|X = x] = \mu_1(x) - \mu_0(x)$$

Identifying Assumptions

Assumptions for non-parametric models:

1. SUTVA (or observational rule, OR)
2. Conditional Independence Assumption (CIA):

$$(Y^1, Y^0) \perp\!\!\!\perp D|X = x \text{ for all } x \in \mathcal{X}$$

3. Common Support (CS) Assumption:

$$0 < p(x) = Pr(D = 1|X = x) < 1 \text{ for all } x \in \mathcal{X}$$

Interpretation of Assumptions

Conditional Independence Assumption (CIA):

- Potential outcomes Y^1 and Y^0 are independent of the treatment D conditional on the covariates X.
- Implies that we have to control for all covariates that have a joint impact on the treatment and the potential outcomes.
- All covariates X have to be exogeneous (typically determined pre-treatment).
- The CIA is an untestable assumption. We have the use application specific economic arguments to justify this assumptions.

Common Support (CS) Assumption:

- Requires that we observe for each treated observation unit a comparable (in terms of covariates X) non-treated observation unit.
- The CS assumption can be tested.

Identification of ATEs

Under Assumption 1-3, we can identify δ from observable data (Y, D, X):

$$
\begin{aligned}
\delta &= E[Y^1 - Y^0] = E[Y^1] - E[Y^0] \\
&\overset{LIE}{=} \int (E[Y^1|X = x] - E[Y^0|X = x]) f_X(x) dx \\
&\overset{CS,CIA}{=} \int (E[Y^1|D = 1, X = x] - E[Y^0|D = 0, X = x]) f_X(x) dx \\
&\overset{OR}{=} \int (E[Y|D = 1, X = x] - E[Y|D = 0, X = x]) f_X(x) dx \\
&= E_X[E[Y|D = 1, X = x] - E[Y|D = 0, X = x]] \qquad \square
\end{aligned}
$$

Outline

Power of Conditioning

- Y: Earnings (in Euro).
- D: Dummy for participation in a job search assistant program ($D = 1$ under participation, $D = 0$ under non-participation).
- X: Gender dummy ($X = 1$ for women, $X = 0$ for men).
- We observe a sample (Y, D, X) with $N = 100$.
- Observations:

		Participants $D = 1$	Non-participants $D = 0$
Women	$X = 1$	$N = 10$	$N = 30$
Men	$X = 0$	$N = 40$	$N = 20$

Power of Conditioning

- Observable expected earnings:

	$E[Y^1 \vert D=1, X=x]$ $= E[Y \vert D=1, X=x]$	$E[Y^0 \vert D=0, X=x]$ $= E[Y \vert D=0, X=x]$
Women $(X=1)$	4000	3000
Men $(X=0)$	5000	5000

- Counterfactual expected earnings (unobservables are in **red**):

	$E[Y^0 \vert D=1, X=x]$	$E[Y^1 \vert D=0, X=x]$
Women $(X=1)$	3000	4000
Men $(X=0)$	5000	5000

True Causal Effects

- Average Treatment Effect on the Treated (ATET):

$$
\begin{aligned}
ATET =& Pr(X=1|D=1)\cdot(E[Y^1|D=1,X=1]-E[Y^0|D=1,X=1]) \\
&+Pr(X=0|D=1)\cdot(E[Y^1|D=1,X=0]-E[Y^0|D=1,X=0]) \\
=& \frac{10}{50}\cdot(4000-3000)+\frac{40}{50}\cdot(5000-5000)=200
\end{aligned}
$$

- Average Treatment Effect on the Non-Treated (ATENT):

$$
\begin{aligned}
ATENT =& Pr(X=1|D=0)\cdot(E[Y^1|D=0,X=1]-E[Y^0|D=0,X=1]) \\
&+Pr(X=0|D=0)\cdot(E[Y^1|D=0,X=0]-E[Y^0|D=0,X=0]) \\
=& \frac{30}{50}\cdot(4000-3000)+\frac{20}{50}\cdot(5000-5000)=600
\end{aligned}
$$

- Average Treatment Effect (ATE):

$$
\begin{aligned}
ATE =& Pr(D=1)\cdot ATET+Pr(D=0)\cdot ATENT \\
=& \frac{50}{100}\cdot 200+\frac{50}{100}\cdot 600=400
\end{aligned}
$$

Naive Estimator

- Expected earnings of participants:

$$\begin{aligned} E[Y|D=1] =& Pr(X=1|D=1) \cdot E[Y|D=1, X=1] \\ &+ Pr(X=0|D=1) \cdot E[Y|D=1, X=0] \\ =& \frac{10}{50} \cdot 4000 + \frac{40}{50} \cdot 5000 = 4800 \end{aligned}$$

- Expected earnings of non-participants:

$$\begin{aligned} E[Y|D=0] =& Pr(X=1|D=0) \cdot E[Y|D=0, X=1] \\ &+ Pr(X=0|D=0) \cdot E[Y|D=0, X=0] \\ =& \frac{30}{50} \cdot 3000 + \frac{20}{50} \cdot 5000 = 3800 \end{aligned}$$

- Naive estimator:

$$E[Y|D=1] - E[Y|D=0] = 4800 - 3800 = 1000$$

Is the CIA Valid?

- Conditional mean independence assumption for potential outcome under treatment (Y^1):

$$E[Y^1|D=1,X=x] = E[Y^1|D=0,X=x]$$
$$4000 = E[Y^1|D=1,X=1] = E[Y^1|D=0,X=1] = 4000$$
$$5000 = E[Y^1|D=1,X=0] = E[Y^1|D=0,X=0] = 5000$$

- Conditional mean independence assumption for potential outcome under non treatment (Y^0):

$$E[Y^0|D=0,X=x] = E[Y^0|D=1,X=x]$$
$$3000 = E[Y^0|D=0,X=1] = E[Y^0|D=1,X=1] = 3000$$
$$5000 = E[Y^0|D=0,X=0] = E[Y^0|D=1,X=0] = 5000$$

Average Treatment Effect on the Treated (ATET)

Under Assumptions 1-3,

$$
\begin{aligned}
E[Y^1 - Y^0|D=1] = & E[Y^1|D=1] - E[Y^0|D=1] \\
= & E[Y^1|D=1] - Pr(X=1|D=1) \cdot E[Y^0|D=1, X=1] \\
& - Pr(X=0|D=1) \cdot E[Y^0|D=1, X=0] \\
= & E[Y^1|D=1] - Pr(X=1|D=1) \cdot E[Y^0|D=0, X=1] \\
& - Pr(X=0|D=1) \cdot E[Y^0|D=0, X=0] \\
= & E[Y|D=1] - Pr(X=1|D=1) \cdot E[Y|D=0, X=1] \\
& - Pr(X=0|D=1) \cdot E[Y|D=0, X=0] \\
= & 4800 - \frac{10}{50} \cdot 3000 - \frac{40}{50} \cdot 5000 = 200
\end{aligned}
$$

$\rightarrow$ Positive bias ($= 1000 - 200 = 800$)!

Average Treatment Effect on the Non-Treated (ATENT)

Under Assumptions 1-3,

$$
\begin{aligned}
E[Y^1 - Y^0|D=0] =& E[Y^1|D=0] - E[Y^0|D=0] \\
=& Pr(X=1|D=0) \cdot E[Y^1|D=0, X=1] \\
& + Pr(X=0|D=0) \cdot E[Y^1|D=0, X=0] - E[Y^0|D=0] \\
=& Pr(X=1|D=0) \cdot E[Y^1|D=1, X=1] \\
& + Pr(X=0|D=0) \cdot E[Y^1|D=1, X=0] - E[Y^0|D=0] \\
=& Pr(X=1|D=0) \cdot E[Y|D=1, X=1] \\
& + Pr(X=1|D=0) \cdot E[Y|D=1, X=0] - E[Y|D=0] \\
=& \frac{30}{50} \cdot 4000 + \frac{20}{50} \cdot 5000 - 3800 = 600
\end{aligned}
$$

$\rightarrow$ Positive bias ($= 1000 - 600 = 400$)!

Average Treatment Effect (ATE)

- ATE:

$$
\begin{aligned}
E[Y^1 - Y^0] =& Pr(D=1) \cdot E[Y^1 - Y^0 | D=1] \\
& + Pr(D=0) \cdot E[Y^1 - Y^0 | D=0] \\
=& \frac{50}{100} \cdot 200 + \frac{50}{100} \cdot 600 = 400
\end{aligned}
$$

$\rightarrow$ The average effect of participation in job search assistance on earnings is 400 Euro.

Simpson's Paradox

- Suppose we investigate the gender wage gap.
- We observe the following average wages of 100 women and 100 men in management and non-management positions:

	Women	Men
Non-management	1581.65 Euro ($N = 87$)	1507.59 Euro ($N = 59$)
Management	2796.22 Euro ($N = 13$)	2659.91 Euro ($N = 41$)

- In the sample, 13 women and 41 men have a management position.
- How large is the gender wage gap?

Simpson's Paradox

- On average women earn less in this example:

$$\underbrace{\left(\frac{13}{100} \cdot 2796.22 + \frac{87}{100} \cdot 1581.65\right)}_{\text{Average Wage Women}} - \underbrace{\left(\frac{41}{100} \cdot 2659.91 + \frac{59}{100} \cdot 1507.59\right)}_{\text{Average Wage Men}} = -240.50$$

- Without conditioning on management position, women earn on average 240.50 Euro less than men.

Simpson's Paradox

- But in each sub-category women earn more than men:
  - Management: $2796.22 - 2659.91 = 136.31$
  - Non-management: $1581.65 - 1507.59 = 74.06$
- The gender wage gap after conditioning on management position is:

$$\frac{13+41}{200} \cdot 136.31 + \frac{87+59}{200} \cdot 74.06 = 90.87$$

- After conditioning on management position, women earn on average 90.87 Euro more than men.

Simpson's Paradox

⇒ **What is the correct gender wage gap?**

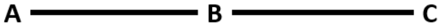
⇒ **Do we need to control for management position or not?**

- The seemingly contradicting results of the conditional and unconditional estimator are called Simpson's Paradox.
- The correct answers depends on the (typically untestable) assumptions we impose.

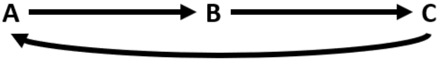
⇒ **Let us elaborate more on this on the next couple of slides using the DAG framework.**

Directed Acyclic Graphs (DAGs)

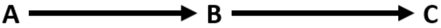
- Undirected graphs:



- Directed cyclic graphs:

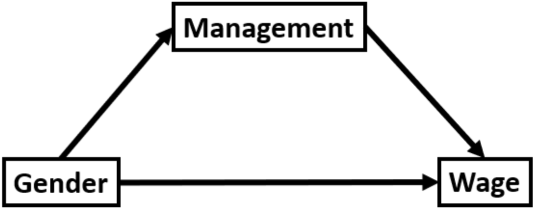


- Directed acyclic graphs:

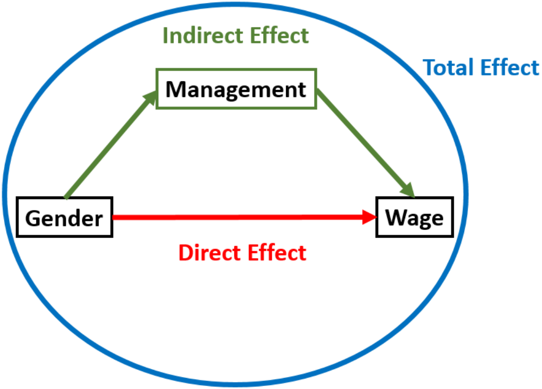


Gender Wage Gap

→ We are interested in the effect of gender (left variable) on wages (right variable). → gender wage gap



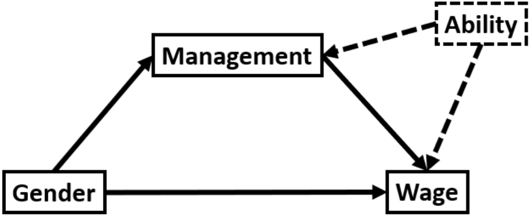
Gender Wage Gap



- Total effect is identified when we do not condition on management.
- Direct effect is identified when we do condition on management.

Gender Wage Gap

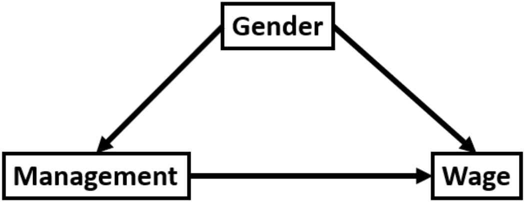
→ Now we consider additionally the unobserved variable ability (dashed means unobserved).



- We can only identify the total effect.
- Controlling for management would introduce an omitted variable bias.

Manager Wage Premium

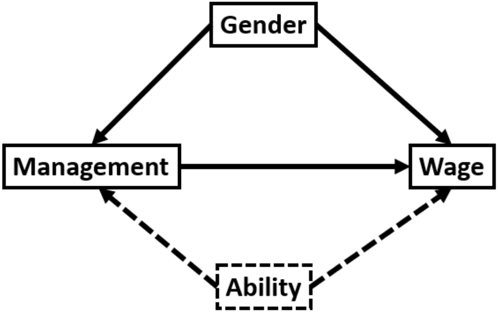
→ We are interested in the effect of management position (left variable) on wages (right variable) → manager wage premium.



- We must control for gender, otherwise we have an omitted variable bias.

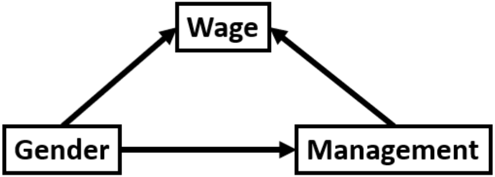
Manager Wage Premium

- But unobserved ability could still cause an omitted variable bias, even after controlling for gender!
- Then the manager wage premium is not identified.



Glass Ceiling in Career Ladders

→ We are interested in the effect of gender (left variable) on management position (right variable) → glass ceiling in career ladders



- Do not control for wage!
- It is an outcome variable and accordingly endogenous (even if we ignore the unobserved ability).

Overview

Conditional Expectations of Potential Outcomes

- We saw on Slide 19, that

$$\delta = \int (\underbrace{E[Y^1|X=x]}_{=\mu_1(x)} - \underbrace{E[Y^0|X=x]}_{=\mu_0(x)}) f_X(x) dx$$

- We can identify $\mu_1(x)$ and $\mu_0(x)$ from observable data

$$\mu_1(x) = E[Y^1|X=x] \overset{CS,CIA}{=} E[Y^1|D=1,X=x] \overset{OR}{=} E[Y|D=1,X=x]$$
$$\mu_0(x) = E[Y^0|X=x] \overset{CS,CIA}{=} E[Y^0|D=0,X=x] \overset{OR}{=} E[Y|D=0,X=x]$$

T-Learner

- Using the sample analogy principle, an estimator for ATE is

$$\hat{\delta} = \frac{1}{N} \sum_{i=1}^{N} (\tilde{\mu}_1(X_i) - \tilde{\mu}_0(X_i)) \tag{2}$$

where $\tilde{\mu}_1(X_i)$ and $\tilde{\mu}_0(X_i)$ are the estimated conditional expectation of the potential outcome for observation units with characteristics X_i

Regression Model

- There are many possible ways how we can estimate $\tilde{\mu}_1(X_i)$ and $\tilde{\mu}_0(X_i)$
- A very simple way is to use OLS regressions
- We can estimate $\tilde{\mu}_1(\cdot)$ and $\tilde{\mu}_0(\cdot)$ in two separate empirical models

$$\tilde{\mu}_1(X_i) = X_i\tilde{\beta}^1 \text{ in the sample of participants with } D = 1$$
$$\tilde{\mu}_0(X_i) = X_i\tilde{\beta}^0 \text{ in the sample of non-participants with } D = 0$$

- After we have estimated the coefficients $\tilde{\beta}^1$ and $\tilde{\beta}^0$, we can calculate $\tilde{\mu}_1(X_i)$ and $\tilde{\mu}_0(X_i)$ for the entire sample (since X_i is observed for all units $i = 1, ..., N$)
- Accordingly, we have all ingredients to estimate (2)

Additional Assumptions

- For the regression model we have to make additional parametric assumptions:

  1. **Linearity:**
     We have to assume that the linear functional form is correct for $\tilde{\mu}_1(X_i) = X_i\tilde{\beta}^1$ and $\tilde{\mu}_0(X_i) = X_i\tilde{\beta}^0$
  2. **No Perfect Multicollinearity:**
     We have to assume that the design matrix has full rank, otherwise the objective function of the OLS estimator has multiple solutions

- However, both additional assumptions can be relaxed:
  - We can add many non-linear and interaction terms in X to allow for more flexible functional forms
  - We can use linear machine learning estimators (e.g., Lasso, Ridge, Elastic Net) instead of OLS, which make it easier to handle very flexible models and can even deal with perfect multicollinearity

Alternative Representation

- The fully interacted empirical model (interacted with the treatment dummy) is a more general representation for the conditional expectations of the potential outcomes

$$\tilde{\mu}_d(x) = \tilde{E}[Y^d|D=d, X=x] = x \cdot \tilde{\beta}^0 + d \cdot x \cdot \underbrace{(\tilde{\beta}^1 - \tilde{\beta}^0)}_{=\tilde{\gamma}} \quad (3)$$

  where $\tilde{\gamma}$ is a K-dimensional vector of coefficients
- We can rewrite the T-Learner as

$$\hat{\delta} = \frac{1}{N}\sum_{i=1}^{N} X_i \tilde{\gamma}$$

Proof that Alternative Representation is Correct

$$
\begin{aligned}
\tilde{\mu}_d(x) =& \tilde{E}[Y^d|D=d, X=x] \\
=& \tilde{E}[Y^0 + d \cdot (Y^1 - Y^0)|D=d, X=x] \\
=& \tilde{E}[Y^0|D=d, X=x] \\
& + d \cdot (\tilde{E}[Y^1|D=d, X=x] - \tilde{E}[Y^0|D=d, X=x]) \\
\overset{CIA}{=}& \tilde{E}[Y^0|X=x] + d \cdot (\tilde{E}[Y^1|X=x] - \tilde{E}[Y^0|X=x]) \\
=& \tilde{\mu}_0(x) + d \cdot (\tilde{\mu}_1(x) - \tilde{\mu}_0(x)) \\
=& x \cdot \tilde{\beta}^0 + d \cdot (x \cdot \tilde{\beta}^1 - x \cdot \tilde{\beta}^0) \\
=& x \cdot \tilde{\beta}^0 + d \cdot x \cdot (\tilde{\beta}^1 - \tilde{\beta}^0) \\
=& x \cdot \tilde{\beta}^0 + d \cdot x \cdot \tilde{\gamma}
\end{aligned}
$$

Effect Homogeneity

- We assume additionally that the treatment effects do not vary with regard to the characteristics X, such that $X\beta^1 = X\beta^0 + \alpha$, where α is a scalar
- Under effect homogeneity, the empirical model (3) simplifies to

$$\tilde{\mu}_d(x) = \tilde{E}[Y^d|D = d, X = x] = x \cdot \tilde{\beta}^0 + d \cdot \tilde{\alpha} \tag{4}$$

  and the T-Learner simplifies to $\hat{\delta} = \tilde{\alpha}$

- **Note that the canonical model in (4) is used very often to estimate ATEs, even though this model makes unnecessarily strong assumptions about linearity and effect homogeneity**

Exclusion Restriction and Common Support

- **Exclusion Restriction:**
  - In the undergraduate studies you learned that the exclusion restriction $E[u|D,X]=0$ is an important assumption to identify models like in (4)

$$Y = X\beta^0 + D\alpha + u$$

  - The exclusion restriction is stronger than the CIA, but it would be sufficient to assume $E[u|D,X]=E[u|X]$ if we are only interested in consistent estimates for α and do not care so much about the estimates of β^0
- **Common Support:**
  - If the functional forms $X\tilde{\beta}^1$ and $X\tilde{\beta}^0$ are correct, we can relax the common support assumption, because we can extrapolate out of support.
  - But too much extrapolation might lead to overfitting. Accordingly, we should be careful about common support violations even in OLS regressions!

Overview

Estimation Target

- Multivariate Linear Regression Model:

$$\begin{aligned} Y_i &= D_i\delta + X_i\beta_g + U_i && \text{(structural model)} \\ D_i &= X_i\beta_m + V_i && \text{(selection model)} \end{aligned}$$

- Parameter of interest: δ
- Nuisance parameters: β_g and β_m

- X_i contains $p \gg N$ covariates.
- We assume controlling for $K \ll N$ covariates is sufficient to identify δ.
- Controlling for too many irrelevant covariates may reduce the efficiency of OLS.

Types of Covariates

Relation between covariates and outcome (for some $s_g > 0$):

- $|\beta_{gj}| > s_g$: covariate X_j has a **strong association** with Y_i
- $0 < |\beta_{gj}| \leq s_g$: covariate X_j has a **weak association** with Y_i
- $\beta_{gj} = 0$: covariate X_j has a **no association** with Y_i

Relation between covariates and treatment (for some $s_m > 0$):

- $|\beta_{mj}| > s_m$: covariate X_j has a **strong association** with D_i
- $0 < |\beta_{mj}| \leq s_m$: covariate X_j has a **weak association** with D_i
- $\beta_{mj} = 0$: covariate X_j has a **no association** with D_i

$\rightarrow$ All covariates are standardised

Types of Covariates (cont.)

	$\beta_{gj} = 0$	$0 < \|\beta_{gj}\| \leq s_g$	$\|\beta_{gj}\| > s_g$
$\beta_{mj} = 0$	Irrelevant	Irrelevant	Irrelevant
$0 < \|\beta_{mj}\| \leq s_m$	Irrelevant	Unclear?	Weak Confounder
$\|\beta_{mj}\| > s_m$	Irrelevant	Weak Confounder	Strong Confounder

- $|\beta_{gj}| > s_g$ and $0 < |\beta_{mj}| \leq s_m$: "Weak Outcome Confounder"
- $|\beta_{mj}| > s_m$ and $0 < |\beta_{gj}| \leq s_g$: "Weak Treatment Confounder"

Naive Approach I: Structural Model

Apply Lasso to the structural model

$$\min_{\beta_g}\{E[(Y_i - D_i\delta - X_i\beta_g)^2] + \lambda\|\beta_g\|_1\}$$

without a penalty on δ and estimate a Post-Lasso model using all covariates with non-zero β_g coefficients.

Covariates that are weakly associated with Y_i could be dropped.

$\rightarrow$ Potentially we drop "weak treatment confounders"

Covariates that are strongly associated with D_i could be dropped.

$\rightarrow$ Potentially we drop "strong confounders"

Naive Approach II: Selection Model

Apply Lasso to the selection model

$$\min_{\beta_m} \{ E[(D_i - X_i \beta_m)^2] + \lambda \|\beta_m\|_1 \}$$

and estimate a Post-Lasso structural model using all covariates with non-zero β_m coefficients.

Covariates that are weakly associated with D_i could be dropped.

$\rightarrow$ Potentially we drop "weak outcome confounders"

Double Selection Procedure

1. Apply Lasso to the reduced form models

$$\min_{\tilde{\beta}_g}\{E[(Y_i - X_i\tilde{\beta}_g)^2] + \lambda\|\tilde{\beta}_g\|_1\}, \tag{5}$$

$$\min_{\beta_m}\{E[(D_i - X_i\beta_m)^2] + \lambda\|\beta_m\|_1\}, \tag{6}$$

   with $\tilde{\beta}_g = \delta\beta_m + \beta_g$.

2. Take the union of all covariates $\tilde{X}_i$ with either non-zero β_m or $\tilde{\beta}_g$ coefficients and estimate the Post-Lasso structural model

$$Y_i = D_i\delta + \tilde{X}_i\beta_g^* + u_i.$$

Double Selection Procedure (cont.)

Potentially (6) omits "weak outcome confounders"

$\tilde{\beta}_{gj} \approx \beta_g$ when $0 < |\beta_{mj}| \leq s_m$, such that the missing "weak outcome confounders" are likely selected in (5).

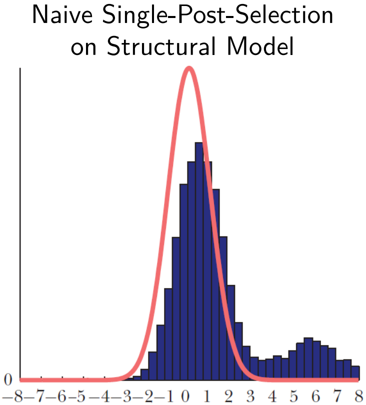
Disadvantages:

→ Potentially we omit "very weak" confounders with $0 < |\beta_{gj}| \leq s_g$ and $0 < |\beta_{mj}| \leq s_g$.

→ All procedures potentially include irrelevant variables.

Simulation Exercise

Distribution of Estimators



Source: Belloni, Chernozhukov, and Hansen (2014)

Asymptotic Results

- Consistency and asymptotic normality

$$\sqrt{N}(\hat{\delta} - \delta) \xrightarrow{d} N(0, \sigma^2).$$

- Model selection step is asymptotically negligible for building confidence intervals.
- Optimal penalty parameter $\lambda^* = 2c \cdot \Phi^{-1}(1 - \gamma/2p)/\sqrt{N}$ (e.g., $c = 1.1$ and $\gamma \leq 0.05$) for "Feasible LASSO"

$$\min_{\beta} E[(Y_i - X_i\beta)^2] + \lambda^* \|\beta\|_1.$$

Reference: Belloni, Chernozhukov, and Hansen (2014)