Causal Inference II

Mixtape Session

Roadmap

- Including Covariates
  - Inverse probability weighting
  - Double Robust DiD
  - Lalonde lab

Controls

- Controls can address omitted variable bias (backdoor criterion), and they can improve precision
- OLS can accommodate controls, and so we tend to include them so long as they are time varying
- But unfortunately, time varying covariates can create problems, especially if the treatment causes the covariates (bad controls, colliders)

Inverse probability weighting DiD

Abadie (2005) incorporates baseline covariates into the propensity score which are then used as weights to estimate the ATT in a simple 3-step process

1. Calculate each unit's "after minus before" (DiD equation)
2. Estimate the conditional probability of treatment based on baseline covariates (propensity score estimation)
3. Weight the comparison group's DiD equation with the IPW

Terms

- t is year of treatment which doesn't vary across units (so no differential timing)
- Y^1 and Y^0 are potential outcomes (counterfactual versus actual)
- D is 1 or 0 based on group and time
- X_b are "baseline" covariates **only** – they do not vary over time, which means propensity scores are estimated off the b period **only**

Assumptions

Kind of common for this propensity score literature to only have two assumptions. But usually the first conditional independence. Now it is parallel trends because this is DD

1. Conditional parallel trends

$$E[Y_t^0 - Y_b^0 | D = 1, X_b] - E[Y_t^0 - Y_t^0 | D = 0, X_b]$$

   (Notice the b subscript. What is that you think?)

2. Common support

$$Pr(D = 1) > 0; Pr(D = 1|X) < 1$$

   Let's see a picture of common support that I drew. Apologies it's horrible

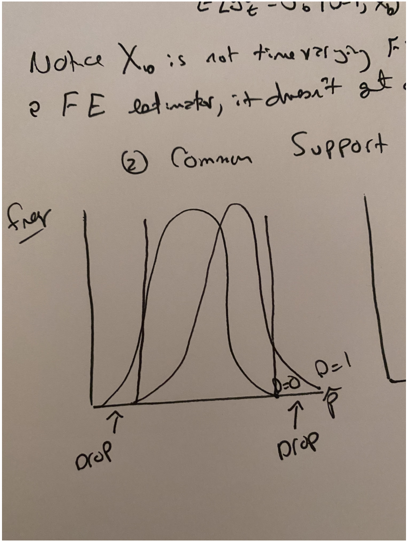
Common support

As we are identifying the ATT, we only need common support with respect to treated units

Your identify assumptions are always with respect to the missing covariates in other words and for the ATT, you are missing Y^0 for the treatment group

If we were estimating ATU, we'd be missing Y^1 for controls and need common support (Y in treatment for all ranges of control), and for ATE we'd need both

Visualizing propensity score to get common support



Definition and estimation

Defining the ATT parameter of interest

$$ATT = E[Y_t^1 - Y_t^0 | D_t = 1] \tag{1}$$

Abadie's estimator

$$E\left[\frac{Y_t - Y_b}{Pr(D_t = 1)} \times \frac{D_t - Pr(D = 1|X_b)}{1 - Pr(D = 1|X_b)}\right] \tag{2}$$

Propensity scores

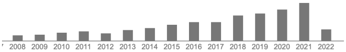
- It's common to hear people say that we don't know the propensity score; we can only estimate it. Same here – we approximate it with regressions
- Paper is titled "Semi-parametric DiD" because Abadie imposes structure on the polynomials used to construct the propensity score ("series logit")

Abadie 2005 influence

Alberto Abadie

Semiparametric difference-in-differences estimators

Authors	Alberto Abadie
Publication date	2005/1/1
Journal	The Review of Economic Studies
Volume	72
Issue	1
Pages	1-19
Publisher	Wiley-Blackwell
Description	The difference-in-differences (DID) estimator is one of the most popular tools for applied research in economics to evaluate the effects of public interventions and other treatments of interest on some relevant outcome variables. However, it is well known that the DID estimator is based on strong identifying assumptions. In particular, the conventional DID estimator requires that, in the absence of the treatment, the average outcomes for the treated and control groups would have followed parallel paths over time. This assumption may be implausible if pre-treatment characteristics that are thought to be associated with the dynamics of the outcome variable are unbalanced between the treated and the untreated. That would be the case, for example, if selection for treatment is influenced by individual-transitory shocks on past outcomes (Ashenfelter's dip). This article considers the case in which differences in observed …
Total citations	Cited by 2330



Scholar articles
Semiparametric difference-in-differences estimators
A Abadie - The Review of Economic Studies, 2005
Cited by 2330 Related articles All 12 versions

Abadie (2005) is his fourth most cited paper

Doubly Robust Difference-in-differences

- DR models control for covariates twice – once using the propensity score, once using outcomes adjusted by regression – and are unbiased so long as:
  - → The regression specification for the outcome is correctly specified
  - → The propensity score specification is correctly specified
- Sant'Anna and Zhao (2020) incorporated DR into DiD by combining inverse probability weighting and outcome regression into a single DiD model
- It's in the engine of Callaway and Sant'Anna (2020) that we discuss later so it merits close study
- One of my favorite lesser known of the new DiD papers

Patterns in econometrician reasoning

1. Define the target parameter first (as opposed to writing down a regression specification first)
2. Identification (e.g., parallel trends)
3. Estimation
4. Aggregation
5. Inference

Defining the target parameter

Major part of the new econometrics is to always start with the target parameter and build to it using estimation and identification that "works"

$$\delta = E[Y_{it}^1 - Y_{it}^0 | D_i = 1]$$

Identification assumptions I: Data

Assumption 1: Assume panel data or repeated cross-sectional data

Handling repeated cross-sectional data is possible but assumes stationarity which is a kind of stability assumption, but I'll use panel representation.

Cross-sections will be potentially violated with changing sample compositions (e.g., the Napster example).

Identification assumptions II: Modification to parallel trends

Assumption 2: Conditional parallel trends

Counterfactual trends for the treatment group are the same as the control group for all values of X

$$E[Y_1^0 - Y_0^0 | X, D = 1] = E[Y_1^0 - Y_0^0 | X, D = 0]$$

Identification assumptions III: Common support

Assumption 3: Common support

For some $e > 0$, the probability of being in the treatment group is greater than e and the probability of being in the treatment group conditional on X is $\leq 1 - e$.

Intuition of assumption 3: Called overlap or common support. Means there is at least a small fraction of the population that is treated and that for every value of the covariates X there is at least a small chance that the unit is not treated. It's called common support when it's a propensity score but it's just about the distribution of treatment and control across values of X. Very common when dealing with covariate comparisons as otherwise you're extrapolating (curse of dimensionality)

Estimating DD with Assumptions 1-3

- Assumptions 1-3 gives us a couple of options of estimating the DiD
- We can either use the outcome regression (OR) approach of Heckman, et al 1997
- Or we can use the inverse probability weighting (IPW) approach of Abadie (2005)

Heckman, et al. 1997

Petra Todd

Matching as an econometric evaluation estimator: Evidence from evaluating a job training programme

Authors: James J Heckman, Hidehiko Ichimura, Petra E Todd

Publication date: 1997/10/1

Journal: The review of economic studies

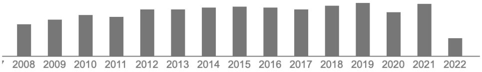
Volume: 64

Issue: 4

Pages: 605-654

Publisher: Wiley-Blackwell

Description: This paper considers whether it is possible to devise a nonexperimental procedure for evaluating a prototypical job training programme. Using rich nonexperimental data, we examine the performance of a two-stage evaluation methodology that (a) estimates the probability that a person participates in a programme and (b) uses the estimated probability in extensions of the classical method of matching. We decompose the conventional measure of programme evaluation bias into several components and find that bias due to selection on unobservables, commonly called selection bias in econometrics, is empirically less important than other components, although it is still a sizeable fraction of the estimated programme impact. Matching methods applied to comparison groups located in the same labour markets as participants and administered the same questionnaire eliminate much of the bias as conventionally …



Outcome regression

This is the Heckman, et al. (1997) approach where the outcome evolution is modeled with a regression

$$\widehat{\delta}^{OR} = \overline{Y}_{1,1} - \left[\overline{Y}_{1,0} + \frac{1}{n^T} \sum_{i|D_i=1} \left(\widehat{\mu}_{0,1}(X_i) - \widehat{\mu}_{0,0}(X_i) \right) \right]$$

where $\overline{Y}$ is the sample average of Y among units in the treatment group at time t and $\widehat{\mu}(X)$ is an estimator of the true, but unknown, $m_{d,t}(X)$ which is by definition equal to $E[Y_t|D = d, X = x]$.

Outcome regression

$$\widehat{\delta}^{OR} = \overline{Y}_{1,1} - \left[\overline{Y}_{1,0} + \frac{1}{n^T} \sum_{i|D_i=1} (\widehat{\mu}_{0,1}(X_i) - \widehat{\mu}_{0,0}(X_i))\right]$$

1. Regress changes ΔY on X among untreated groups using baseline covariates only
2. Get fitted values of the regression using all X from $D = 1$ only. Average those
3. Calculate change in this fitted Y among treated with the average fitted values

Inverse probability weighting

This is the Abadie (2005) approach where we use weighting

$$\widehat{\delta}^{ipw} = \frac{1}{E_N[D]} E\left[\frac{D - \widehat{p}(X)}{1 - \widehat{p}(X)}(Y_1 - Y_0)\right]$$

where $\widehat{p}(X)$ is an estimator for the true propensity score. Reduces the dimensionality of X into a single scalar.

These models cannot be ranked

- Outcome regression needs $\widehat{\mu}(X)$ to be correctly specified, whereas
- Inverse probability weighting needs $\widehat{p}(X)$ to be correctly specified
- It's hard to "rank" these two in practice with regards to model misspecification because each is inconsistent when their own models are misspecified

TWFE

Consider our earlier TWFE specification:

$$Y_{it} = \alpha_1 + \alpha_2 T_t + \alpha_3 D_i + \delta(T_i \times D_t) + \varepsilon_{it}$$

Just add in covariates then right?

$$Y_{it} = \alpha_1 + \alpha_2 T_t + \alpha_3 D_i + \delta(T_i \times D_t) + \theta \cdot X_{it} + \varepsilon_{it}$$

Sure! If you're willing to impose three *more* assumptions

Decomposing TWFE with covariates

TWFE places restrictions on the DGP. Previous TWFE regression under assumptions 1-3 implies the following:

$$E[Y_1^1|D=1,X] = \alpha_1 + \alpha_2 + \alpha_3 + \delta + \theta X$$

Conditional parallel trends implies

$$\begin{aligned}
E[Y_1^0 - Y_0^0|D=1,X] &= E[Y_1^0 - Y_0^0|D=0,X] \\
E[Y_1^0|D=1,X] - E[Y_0^0|D=1,X] &= E[Y_1^0|D=0,X] - E[Y_0^0|D=0,X] \\
E[Y_1^0|D=1,X] &= E[Y_0^0|D=1,X] + E[Y_1^0|D=0,X] - E[Y_0^0|D=0,X] \\
E[Y_1^0|D=1,X] &= E[Y_0|D=1,X] + E[Y_1|D=0,X] - E[Y_0|D=0,X]
\end{aligned}$$

Switching equation substitution

Last line from the switching equation. This gives us:

$$E[Y_1^0|D=1,X]=\alpha_1+\alpha_2+\alpha_3+\theta X$$

Now compare this with our earlier Y^1 expression

$$E[Y_1^1|D=1,X]=\alpha_1+\alpha_2+\alpha_3+\delta+\theta X$$

We can define our target parameter, the ATT, now in terms of the fixed effects representation

Collecting terms

TWFE representation of our conditional expectations of the potential outcomes

$$E[Y_1^1|D=1,X] = \alpha_1 + \alpha_2 + \alpha_3 + \delta + \theta_1 X$$
$$E[Y_1^0|D=1,X] = \alpha_1 + \alpha_2 + \alpha_3 + \theta_2 X$$

Substitute these into our target parameter

$$\begin{aligned} ATT &= E[Y_1^1|D=1,X] - E[Y_1^0|D=1,X] \\ &= (\alpha_1 + \alpha_2 + \alpha_3 + \delta + \theta_1 X) - (\alpha_1 + \alpha_2 + \alpha_3 + \theta_2 X) \\ &= \delta + (\theta_1 X - \theta_2 X) \end{aligned}$$

What if $\theta_1 X \neq \theta_2 X$?

Assumption 4: Homogeneous treatment effects in X

TWFE requires homogenous treatment effects in X (i.e., the treatment effect is the same for all X)

If X is sex, then effects are the same for males and females.

If X is continuous, like income, then the effect is the same whether someone makes \$1 or \$1 million.

X-specific trends

TWFE also places restrictions on covariate trends for the two groups too. Take conditional expectations of our TWFE equation.

$$
\begin{aligned}
E[Y_1|D=1] &= \alpha_1 + \alpha_2 + \alpha_3 + \delta + \theta X_{11} \\
E[Y_0|D=1] &= \alpha_1 + \alpha_3 + \theta X_{10} \\
E[Y_1|D=0] &= \alpha_1 + \alpha_2 + \theta X_{01} \\
E[Y_0|D=0] &= \alpha_1 + \theta X_{00}
\end{aligned}
$$

X-specific trends

Now take the DiD formula:

$$
\begin{aligned}
\delta^{DD} = \quad & \Big((\alpha_1 + \alpha_2 + \alpha_3 + \delta + \theta X_{11}) - (\alpha_1 + \alpha_3 + \theta X_{10}) \Big) - \\
& \Big((\alpha_1 + \alpha_2 + \theta X_{01}) - (\alpha_1 + \theta X_{00}) \Big)
\end{aligned}
$$

Eliminating terms, we get:

$$
\begin{aligned}
\delta^{DD} = \quad & \delta + \\
& (\theta X_{11} - \theta X_{10}) - (\theta X_{01} - \theta X_{00})
\end{aligned}
$$

Second line requires that trends in X for treatment group equal trends in X for control group.

Assumption 5 and 6

We need “no X-specific trends” for the treatment group (assumption 5) and comparison group (assumption 6)

Intuition: No X-specific trends means the evolution of potential outcome Y^0 is the same regardless of X. This would mean you cannot allow rich people to be on a different trend than poor people, for instance.

Without these six, in general TWFE will not identify ATT.

Why not both?

- Let's review the problem. What if you claim you need X for conditional parallel trends?
- You have three options:
  1. Outcome regression (Heckman, et al. 1997) – needs Assumptions 1-3
  2. Inverse probability weighting (Abadie 2005) – needs Assumptions 1-3
  3. TWFE (everybody everywhere all the time) – needs Assumptions 1-6
- Problem is 1 and 2 need the models to be correctly specified
- Doubly robust combines them to give us insurance; we now get two chances to be wrong, as opposed to just one

Double Robust DiD

$$\delta^{dr} = E\left[\left(\frac{D}{E[D]} - \frac{\frac{p(X)(1-D)}{(1-p(X))}}{E\left[\frac{p(X)(1-D)}{(1-p(X))}\right]}\right)(\Delta Y - \mu_{0,\Delta}(X))\right]$$

$p(x)$: propensity score model

$\Delta Y = Y_1 - Y_0 = Y_{post} - Y_{pre}$

$\mu_{d,\Delta} = \mu_{d,1}(X) - \mu_{d,0}(X),$ where $\mu(X)$ is a model for

$m_{d,t} = E[Y_t|D = d, X = x]$

So that means $\mu_{0,\Delta}$ is just the control group's change in average Y for each $X = x$

Double Robust DiD

$$\delta^{dr} = E\left[\left(\frac{D}{E[D]} - \frac{\frac{p(X)(1-D)}{(1-p(X))}}{E\left[\frac{p(X)(1-D)}{(1-p(X))}\right]}\right)(\Delta Y - \mu_{0,\Delta}(X))\right]$$

Notice how the model controls for X: you're weighting the adjusted outcomes using the propensity score

The reason you control for X twice is because you don't know which model is right. DR DiD frees you from making a choice without making you pay too much for it

Efficiency

- Authors exploit all the restrictions implied by the assumptions to construct semiparametric bounds
- This is where the influence function comes in, which those who have studied the DID code closely may have noticed
- One of the main results of the paper is that the DR DiD estimator is also DR for inference
- Let's skip to Monte Carlos

Monte Carlo details

- Compare DR with TWFE, OR and IPW
- Sample size is 1,000
- 10,000 Monte Carlo experiments
- Propensity score estimated with logit; OR estimated using linear specification

Table: Monte Carlo Simulations, DGP1, Both OR and Propensity score correct

	Bias	**RMSE**	**SE**	**Coverage**	**CI length**
TWFE	-20.9518	21.1227	2.5271	0.000	9.9061
OR	-0.0012	0.1005	0.1010	0.9500	0.3960
IPW	0.0257	2.7743	2.6636	0.9518	10.4412
DR	-0.0014	0.1059	0.1052	0.9473	0.4124

Figure 1: Monte Carlo for DID estimators, DGP1: Both pscore and OR are correctly specified

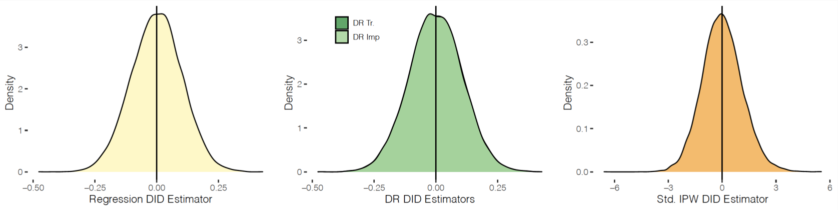
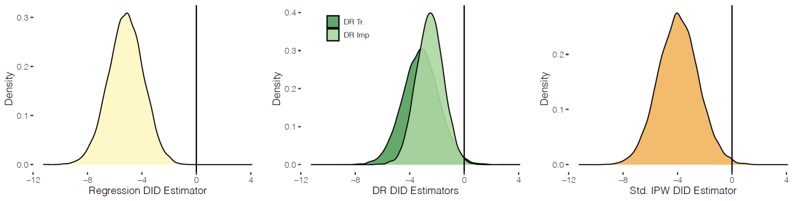


Table: Monte Carlo Simulations, DGP4, Neither OR and Propensity score correct

	Bias	**RMSE**	**SE**	**Coverage**	**CI length**
TWFE	-16.3846	16.5383	3.6268	0.000	14.2169
OR	-5.2045	5.3641	1.2890	0.0145	5.0531
IPW	-1.0846	2.6557	2.3746	0.9487	9.3084
DR	-3.1878	3.4544	1.2946	0.3076	5.0749

Figure 4: Monte Carlo for DID estimators, DGP4: Both OR and PS are misspecified



R and Stata Code

There is code in R and Stata (all DiD estimators are now beautifully arranged at a website hosted by Asjad Naqvi)

- Stata: **drdid**
- R: **drdid**

`https://asjadnaqvi.github.io/DiD/docs/01_stata/`

Remember – it's for 2x2 with covariates (i.e., one treatment group).

Application using real data

- Let’s now use a real example with real data and see how well this does
- Famous paper in AER by Lalonde (1986), an Orley and Card student at Princeton
- Found that most program evaluation did badly, but let’s revisit it with diff-in-diff

Description of NSW Job Trainings Program

The National Supported Work Demonstration (NSW), operated by Manpower Demonstration Research Corp in the mid-1970s:

- was a temporary employment program designed to help disadvantaged workers lacking basic job skills move into the labor market by giving them work experience and counseling in a sheltered environment
- was also unique in that it **randomly assigned** qualified applicants to training positions:
  - → **Treatment group**: received all the benefits of NSW program
  - → **Control group**: left to fend for themselves
- admitted AFDC females, ex-drug addicts, ex-criminal offenders, and high school dropouts of both sexes

NSW Program

- Treatment group members were:
  - → guaranteed a job for 9-18 months depending on the target group and site
  - → divided into crews of 3-5 participants who worked together and met frequently with an NSW counselor to discuss grievances and performance
  - → paid for their work
- Control group members were randomized so the same
- Note: the randomization balanced observables and unobservables across the two arms, thus enabling the estimation of an ATE for the people who self-selected into the program

NSW Program

- Other details about the NSW program:
  - → Wages: NSW offered the trainees lower wage rates than they would've received on a regular job, but allowed their earnings to increase for satisfactory performance and attendance
  - → Post-treatment: after their term expired, they were forced to find regular employment
  - → Job types: varied within sites – gas station attendant, working at a printer shop – and males and females were frequently performing different kinds of work

NSW Data

- NSW data collection:
  - → MDRC collected earnings and demographic information from both treatment and control at baseline and every 9 months thereafter
  - → Conducted up to 4 post-baseline interviews
  - → Different sample sizes from study to study can be confusing, but has simple explanations

NSW Data

- <u>Estimation</u>:
  - → NSW was a randomized job trainings program; therefore estimating the average treatment effect is straightforward:

$$\frac{1}{N_t}\sum_{D_i=1} Y_i - \frac{1}{N_c}\sum_{D_i=0} Y_i \approx E[Y^1 - Y^0]$$

    in large samples assuming treatment selection is independent of potential outcomes (randomization) – i.e., $(Y^0, Y^1) \perp\!\!\!\perp D$.
- <u>NSW worked</u>: Treatment group participants' real earnings post-treatment (1978) was positive and economically meaningful – $\approx$ \$900 (LaLonde 1986) to \$1,800 (Dehejia and Wahba 2002) depending on the sample used

LaLonde, Robert J. (1986). "Evaluating the Econometric Evaluations of Training Programs with Experimental Data". *American Economic Review*.

LaLonde's study was **not** an evaluation of the NSW program, as that had been done, but rather an evaluation of econometric models done by:

- replacing the experimental NSW control group with non-experimental control group drawn from two nationally representative survey datasets: Current Population Survey (CPS) and Panel Study of Income Dynamics (PSID)
- estimating the average effect using non-experimental workers as controls for the NSW trainees
- comparing his non-experimental estimates to the experimental estimates of $900

LaLonde (1986)

- LaLonde's conclusion: available econometric approaches were biased and inconsistent
  - → His estimates were way off and usually the wrong sign
  - → Conclusion was influential in policy circles and led to greater push for more experimental evaluations

TABLE 5—EARNINGS COMPARISONS AND ESTIMATED TRAINING EFFECTS FOR THE NSW MALE PARTICIPANTS USING COMPARISON GROUPS FROM THE *PSID* AND THE *CPS-SSA*[a,b]

Name of Comparison Group[d]	Comparison Group Earnings Growth 1975–78	NSW Treatment Earnings Less Comparison Group Earnings: Pre-Training Year, 1975		NSW Treatment Earnings Less Comparison Group Earnings: Post-Training Year, 1978		Difference in Differences: Difference in Earnings Growth 1975–78 Treatments Less Comparisons		Unrestricted Difference in Differences: Quasi Difference in Earnings Growth 1975–78		Controlling for All Observed Variables and Pre-Training Earnings
		Unadjusted	Adjusted[c]	Unadjusted	Adjusted[c]	Without Age	With Age	Unadjusted	Adjusted[c]	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Controls	$2,063	$39	$ − 21	$886	$798	$847	$856	$897	$802	$662
	(325)	(383)	(378)	(476)	(472)	(560)	(558)	(467)	(467)	(506)
PSID-1	$2,043	− $15,997	− $7,624	− $15,578	− $8,067	$425	− $749	− $2,380	− $2,119	− $1,228
	(237)	(795)	(851)	(913)	(990)	(650)	(692)	(680)	(746)	(896)
PSID-2	$6,071	− $4,503	− $3,669	− $4,020	− $3,482	$484	− $650	− $1,364	− $1,694	− $792
	(637)	(608)	(757)	(781)	(935)	(738)	(850)	(729)	(878)	(1024)
PSID-3	($3,322	($455	$455	$697	− $509	$242	− $1,325	$629	− $552	$397
	(780)	(539)	(704)	(760)	(967)	(884)	(1078)	(757)	(967)	(1103)
CPS-SSA-1	$1,196	− $10,585	− $4,654	− $8,870	− $4,416	$1,714	$195	− $1,543	− $1,102	− $805
	(61)	(539)	(509)	(562)	(557)	(452)	(441)	(426)	(450)	(484)
CPS-SSA-2	$2,684	− $4,321	− $1,824	− $4,095	− $1,675	$226	− $488	− $1,850	− $782	− $319
	(229)	(450)	(535)	(537)	(672)	(539)	(530)	(497)	(621)	(761)
CPS-SSA-3	$4,548	$337	$878	− $1,300	$224	− $1,637	− $1,388	− $1,396	$17	$1,466
	(409)	(343)	(447)	(590)	(766)	(631)	(655)	(582)	(761)	(984)

[a] The columns above present the estimated training effect for each econometric model and comparison group. The dependent variable is earnings in 1978. Based on the experimental data an unbiased estimate of the impact of training presented in col. 4 is $886. The first three columns present the difference between each comparison group's 1975 and 1978 earnings and the difference between the pre-training earnings of each comparison group and the NSW treatments.

[b] Estimates are in 1982 dollars. The numbers in parentheses are the standard errors.

[c] The exogenous variables used in the regression adjusted equations are age, age squared, years of schooling, high school dropout status, and race.

[d] See Table 3 for definitions of the comparison groups.

TABLE 5—EARNINGS COMPARISONS AND ESTIMATED TRAINING EFFECTS FOR THE NSW MALE PARTICIPANTS USING COMPARISON GROUPS FROM THE *PSID* AND THE *CPS-SSA*[a,b]

Name of Comparison Group[d]	Comparison Group Earnings Growth 1975–78	NSW Treatment Earnings Less Comparison Group Earnings				Difference in Differences: Difference in Earnings Growth 1975–78 Treatments Less Comparisons		Unrestricted Difference in Differences: Quasi Difference in Earnings Growth 1975–78		Controlling for All Observed Variables and Pre-Training Earnings
		Pre-Training Year, 1975		Post-Training Year, 1978						
		Unadjusted	Adjusted[c]	Unadjusted	Adjusted[c]	Without Age	With Age	Unadjusted	Adjusted[c]	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Controls	$2,063	$39	$ − 21	$886	$798	$847	$856	$897	$802	$662
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PSID-3	($3,322	($455	$455	$697	− $509	$242	− $1,325	$629	− $552	$397
	(780)	(539)	(704)	(760)	(967)	(884)	(1078)	(757)	(967)	(1103)
CPS-SSA-1	$1,196	− $10,585	− $4,654	− $8,870	− $4,416	$1,714	$195	− $1,543	− $1,102	− $805
	(61)	(539)	(509)	(562)	(557)	(452)	(441)	(426)	(450)	(484)
CPS-SSA-2	$2,684	− $4,321	− $1,824	− $4,095	− $1,675	$226	− $488	− $1,850	− $782	− $319
	(229)	(450)	(535)	(537)	(672)	(539)	(530)	(497)	(621)	(761)
CPS-SSA-3	$4,548	$337	$878	− $1,300	$224	− $1,637	− $1,388	− $1,396	$17	$1,466
	(409)	(343)	(447)	(590)	(766)	(631)	(655)	(582)	(761)	(984)

[a] The columns above present the estimated training effect for each econometric model and comparison group. The dependent variable is earnings in 1978. Based on the experimental data an unbiased estimate of the impact of training presented in col. 4 is $886. The first three columns present the difference between each comparison group's 1975 and 1978 earnings and the difference between the pre-training earnings of each comparison group and the NSW treatments.

[b] Estimates are in 1982 dollars. The numbers in parentheses are the standard errors.

[c] The exogenous variables used in the regression adjusted equations are age, age squared, years of schooling, high school dropout status, and race.

[d] See Table 3 for definitions of the comparison groups.

Imbalanced covariates for experimental and non-experimental samples

	All		CPS Controls $N_c = 15,992$	NSW Trainees $N_t = 297$		
covariate	mean	(s.d.)	mean	mean	t-stat	diff
Black	0.09	0.28	0.07	0.80	47.04	-0.73
Hispanic	0.07	0.26	0.07	0.94	1.47	-0.02
Age	33.07	11.04	33.2	24.63	13.37	8.6
Married	0.70	0.46	0.71	0.17	20.54	0.54
No degree	0.30	0.46	0.30	0.73	16.27	-0.43
Education	12.0	2.86	12.03	10.38	9.85	1.65
1975 Earnings	13.51	9.31	13.65	3.1	19.63	10.6
1975 Unemp	0.11	0.32	0.11	0.37	14.29	-0.26

Lab

`https://github.com/Mixtape-Sessions/Causal-Inference-2/tree/main/Lab/Lalonde`

Together let's do questions 1 and 2a-c

Concluding remarks

- So we hopefully see a few of the key elements of DiD
  - → Remember: the DiD equation and ATT equation are distinct concepts and definitions
  - → DiD designs can be implemented with OLS specifications that calculate differences in means
  - → Parallel pre-trends and parallel trends are not the same thing – the first is testable, the latter is not testable
  - → Event studies are mandatory but pre-trends are smoking guns, but can mislead nonetheless
- Including *time-varying* covariates in the canonical OLS specification requires additional assumptions
- Doubly robust and IPW incorporate covariates through propensity scores and outcome regressions (or both) using baseline covariate means only